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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
(ACRS)
REACTOR FUELS SUBCOMMITTEE

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WEDNESDAY
OCTOBER 9, 2002

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. Mario V. Bonaca, Chairman, presiding.

COMMITTEE MEMBERS:

DANA A. POWERS	Chairman
MARIO V. BONACA	Member
F. PETER FORD	Member
GRAHAM M. LEITCH	Member
STEPHEN L. ROSEN	Member

ACRS STAFF PRESENT:

MEDHAT EL-ZEFTAWY

OTHER NRC STAFF PRESENT:

SUDHAMAY BASU

RALPH MEYER

JACK ROSENTHAL

HAROLD SCOTT

UNDINE SHOOP

JARED WERMIEL

EPRI REPRESENTATIVES PRESENT:

ROSA YANG

ROBERT MONTGOMERY

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

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

(8:32 a.m.)

CHAIRMAN POWERS: Let's come into session here. This is the ACRS Subcommittee on Reactor Fuels. I'm Dana Powers, Chairman of the Subcommittee. The ACRS Members in attendance are Mario Bonaca, Graham Leitch, Jack Seiber, Steve Rosen and Peter Ford.

Before I get into the introduction to the meeting, I do have an announcement of interest perhaps to the Members of the Subcommittee, is that Jessie Delgado is inviting you all to attend the Fourth Annual Hispanic Month Dinner, which is being organized by the Hispanic Employee Program Advisory Committee in celebration of Hispanic Month. It will be held at On The Border Restaurant, 1488 Rockville Pike at 6:30. The cost is $20 which includes meals, dessert, and a non-alcoholic beverage. I understand Chairman Meserve and Commissioner Diaz will be there. If you'd like to attend this dinner, see Jessie before noon so she can get you a menu selection and give you information on how to get to the restaurant. I think all of you will find that an enjoyable experience.

Today's meeting has a lot of stuff that has to go on the record for format sake. First, I'll note that Med El-Zeftawy is our Cognizant ACRS Staff

Engineer. The rules for participation in today's meeting have been announced as part of the notice of the meeting previously published in the Federal Register on September the 23rd, 2002. A transcript of this meeting is being kept, and will be made available, as stated in the Federal Register notice.

It is requested the speakers first identify themselves, and speak with sufficient clarity and volume so they can be readily heard. We've received no written comments or requests for time to make oral statements by members of the public.

What I'd like to do is a little introduction on the strategy that we want to pursue here. We're going to talk today about the Reactor Fuels Program and some of its results, focused primarily on the behavior of high burn-up fuels under design- basis accident conditions. We're not going to discuss reactor fuels pertinent to the advanced reactors, per se.

Consequently, this discussion would not be part of our research report, so we need to discuss whether we want to prepare a letter to the Commission about this particular research program or not, so bear that in mind as we progress through the discussion, especially this afternoon when we hear about the research program per se.

I think the other things that we're not going to discuss are high burn-up fuel in beyond design-basis accidents. That's another aspect of the program that's not being presented here today because that work is in some early stage of development and cooperative research. Be aware that there is - I'm looking at high burn-up fuel that goes well beyond design- basis accident considerations.

WE also need to consider what information needs to be presented to the Full Committee about these programs. High burn- up fuel has an influence in quite a number of issues that come before the Committee, beyond just the fuel research program itself. Certainly, we're going to hear about high burn-up fuel in consideration with transport casks and on-site storage.

We've already had discussions of high burn-up fuel in connection with power uprate program where there's reasonable confusion in my mind on exactly what is being used as the enthalpy limits on the fuel. So as we progress through today's presentations, the Members should think about advising me on what it is that we want to present to the Full Committee so we keep them up to speed on what's going on in the world of high burn-up fuel, because it impacts a lot of things we discussed.

Today's program requires some introduction, if you're not intimately familiar with what all has gone on in connection with high burn-up in the past. I think everybody understand that licensees have a tremendous economic incentive to use fuel to as high level burn-up as safely possible. It's important also to recognize there is a tremendous societal incentive to use fuel at high levels of burn-up. I mean, quite frankly, the less fuel one uses, the less spent fuel there is that one has to store on-site, the less fuel that has to be disposed in some geological repository, if it ever gets constructed. So the question is, how far can we take the fuels that we have safely in the current generation of reactors?

And it probably comes as no surprise to you that the limits to which we've allowed fuel to be burned up have quickly exceeded our empirical database in understanding how fuel behaves under upset conditions. The limitations on that understanding has been brought to our attention abruptly by a series of tests that have been conducted in Japan, in France, and even in Russia on the responses of fuel to reactivity insertion.

As a result of those experimental results, the agency has put a limit on the level of burn-ups that it will allow fuel to go without some further justification, and an agency-wide research program was initiated to confirm that, in fact, this limit still preserve the public health and safety, and that really is the research program that we're looking at.

We're also going to get to hear some discussions of analyses of these reactivity insertion events that -- reactivity insertion tests that have been done that led to this consideration. We're going to get some perspective on this from both NRR and EPRI who have spent an enormous amount of time looking at these tests in some detail to try to understand what their implications are on the behavior of fuel in actual nuclear power plants.

The focus in the presentation of the research program itself, however, is going to evolve for looking at high burn-up fuel under LOCA conditions, and probably maybe even some stuff on ATWS conditions.

With that little bit of introduction, I'm going to turn to the rest of the agenda, and we're going to begin with a presentation by Undine Shoop.

I think most of the members know Undine. She worked with us on some of the steam generator tube rupture stuff. She's going to give us an overview of the NRR Staff's view on the high burn-up issues. Undine, are you ready?

MS. SHOOP: Yes.

MR. WERMIEL: Before Undine, I just have a couple of words to --

CHAIRMAN POWERS: Would you tell us who you are.

MR. WERMIEL: Sure. My name is Jared Wermiel. I'm Chief of the Reactor Systems Branch in NRR. I wanted to just make a couple of introductory remarks and point something out to the Committee that they may not be aware of. When we met with the Staff, the ACRS last May, we agreed to come back and talk about the issues that Dr. Powers already delineated in his remarks.

Today's presentation, as he pointed out, is divided into basically two parts. This morning NRR is going to provide some background and discussion of its current efforts to review new guidance that was provided to us via an EPRI topical report from the industry to justify future burn-ups beyond the current limit of 62 gigawatt days per metric ton.

Undine is going to provide some background, and following her background, EPRI will discuss the topical report itself, and then Undine will give you a little status of where we are with that review at this time.

This afternoon, the Office of Research will update you on their efforts to gather data and address the issues that are identified in the 1998 burn-up fuel program plan.

I'd like to point out that that program plan is somewhat data and we are currently, NRR is currently working with research on an update of that program plan. We hope to complete the update, and put it into the form of a memorandum to the Commission some time by the end of the year, if all goes well. And that's all I had. Undine, if there's no questions, you can proceed.

CHAIRMAN POWERS: Well, I guess a question comes to my mind, a little bit puzzling to me. Maybe none of my business, but I'll ask the question anyway.

MR. WERMIEL: Sure.

CHAIRMAN POWERS: It seems to me I got a notice that said NRR had felt it had no users need for the RES Program, and now you tell me that you're working to help them revise their program plan.

MR. WERMIEL: We view the program plan in maybe a different light than just the matter of identifying user needs, Dr. Powers. We felt the program plan was important because it communicated to the Commission and other interested stakeholders the entire status of the agency's efforts and activities related to fuel.

If there is a user need, we will work out with research exactly what it is. The Office of Nuclear Reactor Regulation needs, by way of the work that research is undertaking. If we don't identify a user need, we still believe it's important that the program plan reflect the current efforts that are ongoing properly.

At this time, I don't know that we've identified a "user need" per se, but we're still discussing this with research, and we haven't made a definitive determination yet.

CHAIRMAN POWERS: Well, it goes without saying that the ACRS proper has been confused by this user need business, and I don't know that we need to go into that.

MR. WERMIEL: We can, if you want.

CHAIRMAN POWERS: Well, I don't want.

MR. WERMIEL: Okay.

CHAIRMAN POWERS: I'd rather get on with the discussion of the technical work right now.

MR. WERMIEL: That's fine.

CHAIRMAN POWERS: Okay. I guess the floor is your's, Undine.

MS. SHOOP: Thank you, Dana. I'd like to talk today about the EPRI topical report on reactivity initiated accidents. First of all, I'd like to go over the history of RIA criteria. That way we can bring everyone up to speed and we're all on the same page for discussing this issue.

Then we're going to have a presentation by EPRI to provide you information about what they are proposing in their own words. And then I'm going to come back and share with you the preliminary review plan of how we plan to address this topical.

RIA criteria history started off back in May, 1972 with Reg. Guide 1.77. This is the original Reg. Guide that had the criteria of 280 calories per gram, and then later in 1993 when the industry wanted to get a higher burn-up. At that time, they were at 30 to 40 gigawatt days per metric ton Uranium, and they wished to go to 60 to 62 gigawatt days per metric ton. And at that time, the Office of Nuclear Reactor Regulation wrote a letter to the Office of Research asking them to evaluate fuel failure thresholds for normal operation and RIA conditions, because we wanted to make sure that as we extended the burn-up, that we had the knowledge to be able to do that type of assessment.

MEMBER LEITCH: I think I missed that number, because I was writing instead of listening. What was the original limit, gigawatt days per metric ton?

MS. SHOOP: Back in 1993, they were at 30 to 40 gigawatt days.

MEMBER LEITCH: Thirty to forty. Okay.

MS. SHOOP: Yeah. And then they wanted to go to 60 to 62.

MEMBER LEITCH: Thank you.

MS. SHOOP: And then in 1997 we wrote a memorandum to the Commission. Basically, we had seen some low enthalpythial bows in the CABRI and NSSR programs, and we were a little bit concerned about it. So one of the things we did is industry came in and they did a generic assessment.

They used a more representative model. They used 3-D analysis rather a current 1-D analysis that's used, to be able to better demonstrate what would actually happen in one of these events. At that time, they showed that with the 3-D analysis they were all well below the 100 calorie per gram limit that had been proposed by research. And because they were under the 280 calorie per gram, and they all demonstrated that they used this more representative analysis that they would meet the lower limit, we determined that they were okay on that basis.

CHAIRMAN POWERS: This always a little bit confuses me. We had a 280 calorie gram limit that became a 225 calorie per gram limit for PWR fuel, and there's a different one for PWR fuel. And that was borne of some tests done a long time ago in a land far, far away.

Then people come in and they say well, we've done these better neutronics, and they say that the power input is much less than that. I have never understood what that has to do with what the limit the fuel will take itself.

MS. SHOOP: Okay. The limit of what the fuel will take it based upon testing criteria that says these are the boundaries at which the fuel can withstand. The more representative analysis that the industry does is an analysis to demonstrate in a real reactor, loaded, with control rod works that are realistic, what will the fuel actually experience?

And what they demonstrated through these analysis is that what the fuel will experience is much lower than the 280 calories.

CHAIRMAN POWERS: And that's fine, and they have to do that. It still has nothing to do with what the criteria are.

MS. SHOOP: Okay. Let me back up.

CHAIRMAN POWERS: Unless you're going to make criteria that's a function of time and impulse shape. Instead, you've got a criteria that's strictly number of calories per gram.

MS. SHOOP: Yes, we do. Okay. So back in 1998, research had provided an information letter, and in that information letter, they proposed changes to the RIA criteria, and they proposed 100 calories per gram. That's what feeds back into our Commission memorandum, that the industry did the representative studies and demonstrate that they could meet that.

WE got together in 1998 between the two offices, and we put together an agency program plan for high burn-up fuels. At this time, the industry mentioned that they would like to go beyond the 60 to 62 gigawatt days per metric ton, and we did an analysis. We determined that with our declining budgets, we would not be able to support all the research needed to be able to do that, so in this agency program plan, we put down that the industry would have to provide the criteria, the database and the models for burn-ups above 62 gigawatt days per metric ton Uranium. That means, in essence, they would have to perform the research to support developing the database to be able to get the information to support extending the burn-ups.

In that agency program plan, we also said that research would still confirm the criteria for burn-ups less than 62 gigawatt days per metric ton, and that feeds back from our user need letter of 1993 when we originally asked them to do that.

The industry responded to our program plan. One of the things that they did was the EPRI Robust Fuels Program, included an objective of being able to develop industry-wide criteria, data, analysis, and models to be able to support the higher burn- up.

This topical report that they're going to present on today is the first topical report that they are presenting that they have given to the agency to be able to address higher burn- up, and to be able to support the criteria development for higher burn-up use.

Their approach is consistent with the current Reg. Guide 1.77 in that it has a coolability limit, and it has a radiological release criteria, so it's still a two-tier approach, which is consistent with our current criteria, and that's what we would be looking at when we review this topical. That's all I have. I'd like to bring on EPRI next.

CHAIRMAN POWERS: Let me ask you just another question about these multi-dimensional kinetics, and I'm quickly getting out of my depth here. It seems to me that in discussing the energy impulses delivered to the fuel by a reactivity event of some sort, a lot of attention has been focused on the differences in the speed with which that energy is delivered to the fuel in reality versus the test.

Now the reality, unfortunately, is a reality that's kind of -- it's an interesting reality. It's not an experimental reality. It's a code calculational reality with these multi- dimensional kinetics models.

On the other hand, I've seen some work at Penn State that says that as the amount of Plutonium in the fuel builds up, that these impulses narrow, and that the calculations that show them remaining wide, are because of some errors in the treatment of the delayed neutrons. Can you comment on any of that?

MS. SHOOP: I have not seen the Penn State reports. I'm not familiar with them. If you could provide a reference to that, I would definitely appreciate it.

CHAIRMAN POWERS: I believe I can.

MS. SHOOP: And with that information, I'd be more than happy to get back to you after I can look at it and intelligently address it.

CHAIRMAN POWERS: I mean, it seems to me you have to look at that because no matter what criteria you say, the licensee is going to have to come in and say well, see, I'm always below that for any hypothesized accident.

MS. SHOOP: Correct.

CHAIRMAN POWERS: And they don't do that by saying see, I've run my reactor and put this impulse into it, and here's the measured data on this. They do this with a calculation.

MEMBER ROSEN: Would you prefer that they run them?

CHAIRMAN POWERS: Well, I would very much prefer to see some experimental data on the impulses in light of the questions that have been raised. I mean, I'm a naive soul here, and a very trusting soul and, you know, these people present me these computer codes where things are calculated out to four or five significant digits, you know. I have great confidence in that until some very smart people from Penn State tell me I shouldn't have confidence in that, and then I'm not sure what I have confidence in.

MS. SHOOP: I think the pulse width may change, but I think that our ability to determine reactor physics and the equations that go into them, and the uncertainties into them are very low. And, therefore, the analysis, as long as you have the right input as far as what the pulse width is, and that's what these tests determine, that the actual analysis is very well defined and well-known.

CHAIRMAN POWERS: Well, of course, that's what the smart people at Penn State are telling me I should be suspicious of.

MS. SHOOP: And that's why I'd like to get those papers, please.

CHAIRMAN POWERS: Okay. I guess we're ready to listen to Rosa Yang.

MS. YANG: My name is Rosa Yang from EPRI. What I'd like to do today, the industry represented by EPRI, the Robust Fuel Program -- there are two parts of the presentation. Like Dr. Powers said, there's tremendous incentive for going to higher burn-up, not only economic incentive but the societal incentive, so this work that will be presented this morning by us is part of our effort in going to higher burn-up.

As I outlined it here, what I'd like to do is to first talk about some of the industry effort related to the topical report that you'll be hearing from Robby Montgomery later on. And he's going to go into the detail, and which may address some of the questions, Dana, that you raised regarding the mechanism of reactivity initiated accident, the impact of pulse widths, temperature, and other stuff.

What I would like to do is to address a couple of the points related to this topical. One of the points I'd like to address is some of the experimental effort, and analytical effort that has been put into this area by the Robust Fuel Program in the industry. And specifically, I'd like to highlight two points raised by this group, particularly the RepNa-1 test. And talk a little bit about the future, which is the CABRI Water Loop Project, to put those two issues into the context related to the submittal of the topical. But I will not address the topical itself, so for the detailed question related to the mechanism and stuff like that, that will be the next presentation. Next slide, please.

Of course, Dana alluded to that the RepNa-1 test from France, which was run in November of 1993. This is the famous test that started it all. It raised a concern about the high burn-up failure limit for reactivity initiated accident may not be conservative enough. And one of the -- let me just get to the test result directly.

The failure limit is 30 calories per gram, as contrasting 170 calories for the failure limit that you'll see later on in Robby's presentation, which is what Undine calls radiological limit, so 30 is much lower than 170. So it raised the question about are we conservative enough? And more importantly, fuel dispersal occurred on this test, so that kind of started the whole thing.

A bit background on that test, and the material is an O-type of cladding, Zircaloy-4, and the burn-up is 64,000. The corrosion thickness on the outside of the cladding is 80 microns, with extensive spallation, the oxide peeling off. The test was run with a very narrow pulse in the sodium loop. Next test.

Tremendous amount of number of tests and effort has gone into in this area to look at this reactivity initiated accident. I just give you some of the effort. This is really just from the experimental side. There's eleven CABRI tests run in France at the CABRI reactor. Thirty-six NSRR tests run in Japan. This number may not seem very large comparing to light water reactor, we have 50,000 rods in one single reactor. However, each of these tests are highly instrumented, and they're fairly expensive. It's on the order of three to five million dollars per test, so these are tremendous amount of effort, and tremendous amount of data being accumulated.

But I think what is more important is not only the data being obtained, but a considerable amount of post-test analyses, and mechanical property measurement, the various laboratories, organizations have been analyzing all these data. And the current situation is, there's a fairly good understanding and agreement what the failure mechanisms are. And in general, most people -- by the way, one thing I want to point out is, NRC has run a PIRT Program, that some of you may be familiar with. And one of the PIRT panel was on RIA, and the conclusion of that PIRT panel was very consistent with what you're going to be hearing later on in terms of the failure mechanism, so I think there's a good understanding of what caused these rods to fail. And, you know, later on you'll see a lot of data which seems seemingly random. But if you consider the cladding ductility of the rods that are being tested, the temperature of the test conditions, the pulse width, you'll see they're actually telling you a very consistent story.

Because of these variables involved that many of the organizations have used analytical tools trying to analyze it, not only to analyze it but trying to link that to the light water reactor condition. The one you're going to hear from us is using FALCON. The French have SCANAIR and NRC have FRAPTRAN.

CHAIRMAN POWERS: You tell me that the data are consistent if we taken into account these factors that you listed down here. I presume there are some others.

MS. YANG: Right.

CHAIRMAN POWERS: But, you know, I have never seen a plot that says okay, your data here are calculations, and notice that they all fall in a 45 degree slope or something like that.

MS. YANG: I think you will see that in our report in terms of predicted versus measured. And you will see some of the -- quite a lot of the data supporting what we're proposed by Robby in a minute.

MEMBER FORD: In your first bullet, the implication is that the RepNa-1 results are, as you said, outliers.

MS. YANG: Right.

MEMBER FORD: They're of no significance. However, of the 47 tests that were done in France and Japan, were any done under exactly the same conditions, Zircaloy-4 oxidized, et cetera, et cetera, to those which were done at RepNa?

MS. YANG: No.

MEMBER FORD: So, in fact --

MS. YANG: There was nothing exactly.

MEMBER FORD: So, in fact, the RepNa results may be relevant. They may not be applicable, but they are relevant. They are relevant data.

MS. YANG: Yes.

MEMBER FORD: It wasn't badly controlled.

MEMBER ROSEN: I think let me help with the question, because I think I have the same sort of question. If you had put a heavily spalled piece of Zircaloy-4 into one of those tests, the 47 tests, which was hit with a nine and a half millisecond is that pulse, would you -- do you think that that rod under those conditions in one of those 47 tests would have failed like in RepNa-1?

MEMBER FORD: That's exactly my point.

MS. YANG: Thank you. I understand the question. Since we -- I'm a scientists. Since we've never done that experiment, I can't tell you what the outcome would be. But based on my judgment, it would not.

MEMBER FORD: Now is that what the --

MS. YANG: And that's why I'm going to give you a little detail on why it wasn't done, and why I think it's an outlier.

MEMBER FORD: But you then go on and say you have some analytical tools.

MS. YANG: Yes.

MEMBER FORD: Would those analytical tools predict the RepNa-1 results?

MS. YANG: No. That's why, if you'll bear with me, that's in my next couple of slides exactly. I'm trying to address your question.

MEMBER FORD: Okay.

MS. YANG: And you're quite right, and I forgot to mention that. I'm probably too nervous. One more thing I forgot to say --

MEMBER ROSEN: Why are you nervous?

MS. YANG: This is an August group.

CHAIRMAN POWERS: These are all sweethearts here. Don't you worry about these guys. They are just -- they're gullible, believe everything that's said.

MS. YANG: You know, I'm very naive, but not that naive. But what I want to say if we have to prepare the presentation, but we have worked in this area since 1994, so we have considerable amount of information on the computer. So, you know, if you don't want to hear any of these, just tell us go through it fast, and then we'll talk about whatever you're interested in. So that's what I meant to say in the beginning, but let me say that now.

So I'm going to tell you why RepNa-1 is so unique. Next slide. Sorry. Let me just sort of finish my thought, and then I'll come back. Because RepNa-1 is so unique, and we formed a RepNa-1 task force to look at all the unique features of it, and that's what I want to spend a few minutes to tell you about. But let me kind of just give you a little bit background about the industry effort in the RIA area in general, not limited to RepNa-1.

There was, as you see, the 1993 RepNa-1 report created all the concerns, and the industry has evaluated all the data, and has created a report that we did not necessarily submit to you, and we did not submit to NRC because there was no regulatory action or licensing activity at that time. However, we did the analysis to ensure ourselves that this is not a concern for the current licensing limit, and we have produced a report, which recognized the core coolability of 230. And if you want to know the difference between 230 and 280, we'll talk about that later. And what is important is, we recognize that there should be a burn-up dependent failure limit, so in --

CHAIRMAN POWERS: Yeah. I have to say that that's something that everybody ought to understand, is that your report recognizes a burn-up dependence.

MS. YANG: Yes.

CHAIRMAN POWERS: Which heretofore has never been recognized in the regulatory process, and that is the biggest take-home lesson I got out of the 1996 report.

MS. YANG: And what we -- at that time, we didn't think we have enough understanding, so we didn't really do too -- although we have analyzed the data extensively, but we didn't use the analytical tool to propose the criteria. What we did was, we kind of proposed a region of success, which is basically bounding all the non-failed data point. Can you go to the next slide? Which is this limit, this dashed line, which is what we call region of success.

I know right now they are not supported by data, but you'll see from Robby's presentation, all the data below here are non-failed. Could we go back? Thank you.

Since that report was issued, several countries have kind of adopted that failure limit, because there's a very conservative approach, supported by the relevant tests. And from 1996 to now, we have gained a considerable knowledge base. As I said, those analytical and experimental, and we have used our code to develop the failure limit, which you will hear later. And we have adopted the no incipient melting to ensure coolability. Next slide.

And I just want to kind of give you the schematic without developing how we -- without really presenting how we developed this, so we have two limits. And as you can see, the analytical developed limit isn't that different from the region of success line that was developed in 1996.

Now let me talk about RepNa-1 now. Next slide, please.

MEMBER BONACA: Could you tell us just one word about FALCON? I mean, what is -- is it a neutronic code, is it three dimensional?

MS. YANG: It is a thermal mechanical fuel performance code. Is it three dimensional? It's probably two dimensional. It addressed the LOCA, in fact, circumferentially. And, of course, the axial dimension, as well.

MEMBER BONACA: So really, it's for purpose of comparing the test with --

MS. YANG: Yes. I'm sorry. I should have said also, is the steady-state in the transient code. The transient part is used to analyze the test and compare the test.

MEMBER BONACA: Thank you.

MS. YANG: And there are quite a few features unique to RIA have been incorporated in the code.

MEMBER LEITCH: Could you define the fuel rod failure, and coolability limits? In other words, what does fuel rod failure look like? What does that mean? Is that a perforation in the fuel?

MS. YANG: It is a breach of the cladding, yes.

MEMBER LEITCH: A breach of the cladding.

MS. YANG: Yeah, that's what failure. And that limit is used to calculate the radiological consequence.

MEMBER LEITCH: Okay. And then the coolability --

MS. YANG: And then the safety limit is the coolability limit.

MEMBER LEITCH: Okay.

MS. YANG: It has to maintain the core geometry.

MEMBER LEITCH: Thank you.

MEMBER FORD: Excuse me, Rosa. I --

MS. YANG: And by the way, Robby is going to talk about that a bit too. I'm sorry.

MEMBER FORD: Okay. Would you mind going back to the previous graph?

MS. YANG: Sure.

MEMBER FORD: I, also, am learning about this. I'm assuming, therefore, that the fuel rod failure --

MS. YANG: Which is this blue line.

MEMBER FORD: That blue line.

MS. YANG: -- and the current limit is the burn-up independent limit of 170 calories per gram, which is saying if 170 calorie per gram was put into fuel, the fuel rod will not fail.

MEMBER FORD: And so the -- any analytical code that you develop for that will have inputs, such as the mechanical properties of the fuel cladding, the degree of hydriding of the fuel cladding. There are parameters in that which take into account.

MS. YANG: Yes.

MEMBER FORD: And the coolability algorithm analysis will have thermo hydraulics criteria.

MS. YANG: Yes.

MEMBER FORD: Heat input criteria into the fuel. Is that right?

MS. YANG: You mean how we developed it?

MEMBER FORD: No. What parameters would be in the algorithm that would define that red line? What sort of parameters?

MS. YANG: How do we define the red line?

MEMBER FORD: No, I'm not interested in -- could you just give me a feeling of the physics. What sort of inputs to the algorithm that define that line? There's an algorithm, an equation that defines that line?

MS. YANG: The current regulatory limit is a straight line 230, burn-up independent straight line.

MEMBER FORD: Okay. So it's defined by policy, isn't it?

MS. YANG: Yes, and some experimental data.

MEMBER FORD: But it's experimental, not analytical. There's not a thermo hydraulic --

MS. YANG: No.

CHAIRMAN POWERS: The upper criterion is one that was invented based on some tests, I guess they started in the 60s actually.

MS. YANG: Yes.

MEMBER FORD: Okay.

CHAIRMAN POWERS: And like sensibly negligible levels of burn-up, imaginative tests, some of them within cladding. It was a long time ago.

MEMBER FORD: Okay.

CHAIRMAN POWERS: Okay? That's really not -- the physics you're looking for really lies in the lower lines.

MEMBER FORD: Okay.

CHAIRMAN POWERS: Not in the upper lines.

MEMBER FORD: Okay. Fine.

MS. YANG: Okay. Now let me address some of your questions about - next slide, please - about

RepNa-1, and what have we done with RepNa-1 is. It's such an outlier or several characteristics. It is a much lower failure limit, enthalpy level comparing to the other RepNa test. Can you go to the next slide?

CHAIRMAN POWERS: In fact, Rosa, correct me if I'm wrong about this, the enthalpy input, integrated input may have been 80, I mean 30 calories per gram, but the failure actually occurred during the power ramp-up, so it actually occurred at even lower enthalpy input.

MS. YANG: Yeah. The total energy input or enthalpy input for this particular test is what, 120 or 110? Something like that.

MR. MONTGOMERY: Robert Montgomery. The answer to that is 100, the energy input is 100.

MS. YANG: Yeah. Right. Thank you. The total energy input is 100. The rod failed at 30 at the peak power location. However, the most intriguing aspect, at least to me as a material-type of person, is the failure did not initiate at the peak power location. In fact, it is very much down below at the rod, and I have a picture to show you in a minute.

Then you ask yourself, what is there that caused the failure? The power level at that location is much lower than 30, maybe something like 26 or 27 or so, so it's not the peak power location. A failure initiated there, according to the organization running the test. And, of course, none of the codes -- you ask can the code explain? The code can explain every other test, except this particular test.

There are other concerns raised about this test. There's a pre-existing defect that was identified after the refabrication. These rods that were tested were from a French power reactor, and they're long, of course. And in order to test it, they cut them short, and then put in end-plugs, and other stuff. And after the refabrication of this particular test, they found an artifact.

CHAIRMAN POWERS: Let's see now. The artifact you're discussing had to do with attaching the ends on this, or was it something that was in the cladding that they cut out?

MS. YANG: In the cladding that were to be tested, not at the end, but at the cladding.

CHAIRMAN POWERS: So it's not an artifact. I mean, it's something that exactly existed in the clad.

MS. YANG: Well, they didn't see it before the refabrication, but they saw it after the refabrication.

CHAIRMAN POWERS: Well, the question is, did they look?

MS. YANG: They did look. According to their report, it was not there. But let me just show you the test. I don't want to make a big deal out of it. I don't think this is the smoking gun, but that's one of the concerns.

CHAIRMAN POWERS: One of the questions that persist in coming up in this is, we say gee, this particular test had spalling clad, it had a pre-existing defect. The question I ask is, well, is that different than the fuel that we would have in the reactors after it had been taken to some elevated level of burn-up? And quite frankly, the databases that I have available for high burn-up fuel never answer that question for me. Some of the fuel seems to be in pretty good shape, but I never get any kind of detail to say over the length of this rod, which can vary from 12 to as much as 14 feet nowadays --

MEMBER ROSEN: In some states.

CHAIRMAN POWERS: -- do we have anything that looks like what you've called here a pre-existing defect? Do we have any evidence of spallation?

MS. YANG: We certainly don't have pre-existing defect. The outcome is that pre-existing defect is a part of the refabrication process, so we don't have that in the reactor. We don't know exactly how those -- I'll show you a picture in a minute. But regarding to the spallation, this is Zircaloy-4 cladding, and when we talk about burn-up extension to 70-75,000, I don't think anybody would use Zircaloy-4 cladding to go there. They're probably mostly looking at advanced alloys, and that's what is pretty much widely used in the industry. So I don't anticipate this kind of material in our burn-up, in our live water reactor high burn-up.

MEMBER ROSEN: Rosa, when you say advanced alloys are you talking about ZIRLO?

MS. YANG: ZIRLO and M5. And as many of you know, corrosion is a temperature driven affect. Some of the low duty plant, they probably could still using the improved Zircaloy-4, which is sometimes called low-tin Zircaloy-4, but it's improved more than just lowering the tin content.

CHAIRMAN POWERS: Of course --

MS. YANG: They're all better than this cladding, is what --

CHAIRMAN POWERS: Well, the problem is it's better on paper. We just don't have any data for reactivity insertion accidents at high burn-up with these improved alloys, do we?

MS. YANG: We will have this year.

CHAIRMAN POWERS: But will and have are two different things.

MS. YANG: Right. I agree. We will have, and they're in the pipe.

MEMBER FORD: Could there not also be a relationship between the pulse geometry as a function of time and the strain rate?

MS. YANG: Yes.

MEMBER FORD: Imposed strain rate. And would not the failure and the clinical failure of Zircaloy-4 change strain rate? Is this not somewhat of an expected result, failure on the forward part of the pulse?

MS. YANG: Yes.

MEMBER FORD: High strain rate pulse.

MS. YANG: It's really not even high strain rate. The whole pulse is very narrow, but at the beginning of the pulse, the rate isn't that high.

MEMBER FORD: No, but where you said it curves, it would be a high strain rate part during the pulse, would it not?

MS. YANG: Not yet. Not at the time of the failure. See, it failed at extremely low power condition.

MEMBER FORD: Okay.

MS. YANG: Let me go on to some of the concerns. Pre- existing defect, instead of going back and forth, I'll show you the picture in a minute. But most importantly, this is the first time 10 millisecond test was run. And when we started looking into the data, we find that, you know, of course the time of failure, the energy input of the failure and all that is dependent on the signals. And they are microphone signals, flow analysis. Bear with me and I'll get into that detail in a minute.

Because the pulse is so narrow and is in the beginning phase, so a very small difference in the uncertainty of the signal interpretation, or the recording time would cause a big difference. And so that's one concern that I'm getting back to.

Another concern was raised by Dr. Hee Chung of Argonne, is talking about this particular rod, because it's a first test. They preconditioned it somewhat differently, at slightly higher temperature, so that could have caused the embrittlement of the cladding. There's another material aspect I'm getting into, so because of all these clouds, if you may, centered around this test, the RepNa-1 task force was formed within the CABRI International Water Loop Project in October 2000.

As you can see, this is kind of a difficult task. On one hand, people outside asking the validity of the test, but you do need the cooperation of the group, the organization conducting the test in order to fully investigate that. I'm personally chairing that group. We have been at this now for two years, and it's a lot of effort, and it's very difficult because we're looking at something that happened ten years ago. Next slide, please.

This is just some table list of RepNa-1 comparing to another sibling test, which is RepNa-10, which is exactly the sibling of RepNa-1. It failed at about 80 calories per gram. And most importantly, there is no fuel dispersal. It failed, but no fuel dispersal. The rods are spalled. The other difference you said has exactly the test been done? No, it was done at 30 milliseconds, because it was recognized that 10 was not representative. Next slide, please.

MEMBER ROSEN: So pardon me, would you go back to that. So I would conclude if those were the only two tests that you had, the big difference was the pulse width.

MS. YANG: Yes.

MEMBER ROSEN: The pulse width at ten milliseconds is simply too much for this fuel. Thirty-one milliseconds is not.

MS. YANG: Yes. Well, there are other narrow pulses done, because one of the speculation, if you may, is the ten millisecond pulse create a gas dynamic loading on the cladding. Thank you. In this one, this particular test was high burn-up, as well, ten milliseconds. The difference is the oxide thickness are different, so it's very good cladding. There are no failures. It goes up all the way to 113 calories per gram, no failures. And one of the reason I list one percent strain is if there's such tremendous dynamic gas loading, you would expect a large strain on the cladding. The result is normal strain, so that's why, you know, I'm not quite convinced about the gas dynamics.

In other tests which were done, unfortunate -- with an even worse cladding spalled, and unfortunately, this one is 75 milliseconds. But again, no fuel dispersal. The rod failed at about the same level as that, so we quite often think these two tests are very similar, and both have no fuel dispersal.

MEMBER LEITCH: But those failure rate, those failure enthalpies are still well below your previous blue curve. Right?

MS. YANG: Yes, because they are spalled, and we -- the proposal that we have does not include spalled rods.

MEMBER LEITCH: I see. Okay.

MEMBER FORD: Can you have pulse widths of the order 10 milliseconds occurring in the reactor?

MS. YANG: No.

MEMBER FORD: It's physically impossible.

CHAIRMAN POWERS: It could, not from a control rod ejection, but I can create a pulse for you, if you want.

MEMBER ROSEN: In a real reactor?

CHAIRMAN POWERS: If you let me borrow the reactor for a while.

MEMBER ROSEN: No, no, no. I'm not going to do that. No, I mean in a real reactor, Dana, is a 10 millisecond pulse at all credible?

CHAIRMAN POWERS: Not for the -- no, not for a natural event.

MEMBER ROSEN: No. So I guess that was the issue.

CHAIRMAN POWERS: I mean, there is this

question that's been raised by Penn State about as you build Plutonium in, the pulses do become narrower.

MEMBER ROSEN: Narrower, but that's a MOX Fuel plant.

CHAIRMAN POWERS: Well --

MEMBER ROSEN: That's a whole nother ball game.

CHAIRMAN POWERS: It's challenging to tell the difference between a MOX Fuel plant, and a high burn-up fuel. You build in quite a lot of Plutonium.

MS. YANG: Well, the particle size --

CHAIRMAN POWERS: Particle size.

MS. YANG: Yeah. So let me say something to you about the RepNa-1 task force. First I want to say, our evaluation is not complete. WE're close, but we're not complete, and so what I'm presenting here is kind of work in progress to show why we did not include it in our evaluation.

CHAIRMAN POWERS: Let me ask you just an opinion here. I mean, you knock yourself out trying to explain one test result, and whatnot, but isn't the really substantive thing that's coming out of all these programs, is that you have a burn-up dependence?

MS. YANG: Yeah.

CHAIRMAN POWERS: And really, that's where we ought to be focusing our attention.

MS. YANG: I agree. I absolutely agree. In fact, you concluded mine for me in saying there is one outlier, and there are so many other good tests, do we really need to really put a lot of effort in --

CHAIRMAN POWERS: I mean, the RepNa-1 is useful for me when I want to badger Ralph Caruso a little bit, but quite frankly, the real issue is, we see a burn-up dependence that we never recognized before.

MS. YANG: And we have a consistent data set, and then we know why they're so consistent. It's really the bottom line I want to leave with you.

MEMBER BONACA: I have a question I'd like to ask you. You showed us a table with comparisons, and we talked about the basis for comparison. On the previous slide, you had a list of concerns regarding RepNa-1.

MS. YANG: Yes.

MEMBER BONACA: Okay. Could you go back to that and tell me how those concerns apply to tests RepNa-5, 8 and 10, versus the number 1?

MS. YANG: Yes.

MEMBER BONACA: Perhaps understanding there is a modifier there, or if you try to -- or if you're addressing the same microstructure, the same conditions and so.

MS. YANG: Yes. In fact, in the report we're going to address all of that. But let me just very quickly -- and again, let me emphasize, we don't have -- we have found several smoking guns. We haven't found the smoking gun. We haven't satisfied ourselves --

MEMBER BONACA: Yeah. I'm trying to understand if we are comparing apples and oranges.

MS. YANG: Okay. This is the first test done, so there's considerable more uncertainty and lack of experience in terms of identifying exactly when the failure occurred. This one, I think they have gained enough experience. All the other are much wider pulse. There's just inherent experimental difficulties in dealing with a very, very narrow pulse like 10 milliseconds.

Now in terms -- this is the only one that we found artifact, and this is the only one that did not fail at the peak power location. All these failed at pretty much near the peak power location.

MEMBER BONACA: The first and second tests, were they -- did they have the same pre-conditioning conditions?

MS. YANG: No. This is the only one that has -- can I go to my next one? That will really answer your question about the pre-conditioning.

MEMBER BONACA: All right.

MS. YANG: Actually, it's the one after that. Can you go to the next slide, please? Maybe just go to the next slide, and let me answer Mario's question.

The artifact, I already talk about it. Go to the next one. I think that's where the picture is. This is where the artifact is. It's like a crater with a depression. This is a crater. There's a depression in it. It's not throughwall. What they did is they found it. They didn't know how it happened. They made an impression of it, and they were able to see the depths of it. There are people arguing, you know, when you make an impression you really don't go deep enough, but that's what was done ten years ago. So this was this artifact, and I'll show you where it is in terms of the rod. This is a real picture.

MEMBER ROSEN: Before you go away from that, can we look at it together for just a second more. The artifact -- to me, there are two artifacts there. There's a scratch also.

MS. YANG: Oh, the scratch. Oh, that is -- rods have scratches. That's not --

MEMBER ROSEN: Yeah, but rods have scratches because when you put the rod into the grid --

MS. YANG: Yeah, exactly.

MEMBER ROSEN: -- they scratch.

MS. YANG: Yeah, you should ignore -- I don't think this is that significant, because most rods have scratches.

MEMBER ROSEN: Have scratches. Okay.

MS. YANG: Yeah.

MEMBER FORD: But you don't think that when you do the pulse there's -- that is the -- that could be the defect --

MS. YANG: That's what we -- let me kind of --

MEMBER ROSEN: I want to understand Peter's point.

MS. YANG: That's a speculation at this point.

MEMBER ROSEN: Peter, did you just say that you think it's possible that the defect that caused the failure is the scratch, not the crater?

MS. YANG: Oh, the scratch? No, no, no.

The scratch is very shallow, and all the rods have scratches, and the scratches pretty much run along the rod.

MEMBER FORD: From that rather shallow delve, can't be very high.

MS. YANG: No. Oh, you mean the --

MEMBER FORD: Yes.

MEMBER ROSEN: From the scratch.

MEMBER FORD: The value for that must be very small.

MS. YANG: Yes.

MEMBER FORD: That is, even if you have a shallow scratch, sharp scratch, which that looks like, and it's a long scratch.

MS. YANG: Yes.

MEMBER FORD: Then during the heat-up, the pulse, then the high strain rate condition -- I'm hypothesizing these things --

MS. YANG: Yeah.

MEMBER FORD: During the high strain rate, a portion of the pulse, during the pulse width you could exceed K1C, G1C for that.

MS. YANG: I don't think so, because all the other rods have scratches.

MEMBER FORD: Okay.

MS. YANG: I would -- all the rods have these scratches because when you pull the rods, you always have the scratches, and they're very, very shallow.

MEMBER FORD: Okay.

MS. YANG: This is the artifact, and if you -- let me, since I'm on the artifact, let me go to the next slide. The artifact is here. The peak power location is about here. The artifact is here, and the IRSN, the organization running the test said that the failure occurred about here. Okay? And this is a peak power location. There is where they think the failure occurred. This is where the artifact is. And by the way, this is a schematic of how the rod looked like after the test. You have tremendous amount of material lost. This is the, you know, the loop, so that's just to give you a sense about what the -- roughly what the location is like, if you can go back to the last slide. One more.

There's an artifact. I showed you that, and I'm not sure. I'm not saying that's a smoking gun. I'm not sure. WE're evaluating it, because there are very -- they took a lot of cut after the test, but they couldn't find it. But the rod was so badly cracked as a result of the test, so it's hard.

Another thing is that they didn't make a good indication of the azimuthal orientation, so they don't know where to look for it, azimuthally. They know roughly where to look axially, but they didn't know how to look -- so the artifact was not found. So that's one of the concerns that we're chasing.

The other concern we're chasing is the pre-conditioning of RepNa-1. Because it's the first test, and Hee Chung has a hypothesis that because this particular test was done at higher temperature, 380 comparing to 310 for 14 hours, and all the RepNa tests were conditioned at lower temperature for a slightly shorter time, so he hypothesized it may have embrittled the cladding. And we're evaluating that, and I don't want to talk yes or no on that hypothesis, because we're in the middle of the evaluation. And it's so controversial, and I'm not done with our task force.

And we're also comparing, as I said, we think the RepNa-8 and 10, although they were somewhat different pulse widths, but they are sibling rods, they are spalled, and we're looking at the ductibility of the cladding and the failure mode, so that's on the microstructure, which is one part of the investigation. The other part, which I think is equally important, is when the rod fail, if you can go back, I think just one slide, which is on the signal analysis, which is really even more interesting that we found quite a few things. You know, these are highly instrumented tests, as I said earlier.

There's microphone, which is basically used to indicate when the failure occurred. They had microphone from the top and bottom based on the different --

MEMBER ROSEN: What are they listening for?

MS. YANG: The sound.

MEMBER ROSEN: Yeah, I know. The sound of what?

MS. YANG: The sound of -- that's exactly a relevant point. The sound of failure, they think.

MEMBER ROSEN: What does it sound like?

CHAIRMAN POWERS: Crack.

MEMBER ROSEN: But you have a test. Is there flow going through this rod?

MS. YANG: Yes.

MEMBER ROSEN: There's flowing liquid metal, actually.

MS. YANG: Right.

MEMBER ROSEN: And so it makes some -- you have a background noise.

MS. YANG: Right.

MEMBER ROSEN: And you sit there, and you listen, and you hear shhh. Right?

MS. YANG: Yeah.

MEMBER ROSEN: And then you do this test, and you hear something different.

MS. YANG: Right. You're absolutely right.

MEMBER ROSEN: What is it you're hearing?

MS. YANG: It's whatever you're hearing, and the expert -- you know, that's why in this one, I'm relying a lot on experts who are familiar with the signal to interpret it, because there are a lot of noise involved, and have to sort of find the relevant signal.

CHAIRMAN POWERS: You're listening to the propagation of a crack.

MS. YANG: Yeah.

MEMBER FORD: A ping.

CHAIRMAN POWERS: Yeah.

MS. YANG: I'm going to tell you, not just the crack would make the sound. The crack initiation could make sound. The oxide cracking could make sound. In fact, we have actual experience that shows the sound come from other stuff, as well.

MS. YANG: Okay. So they look at different -- they also have flow meters that look at flow change as a result of rod failure. Sorry. The expansion of the cladding, and after the failure there are material dispersed, so that changed the flow, and the pressure sensor. So they have all these recorded. And, of course, the organization running the test are the expert in interpreting these.

The very low value is based on the microphone signal. And exactly answer your question, does microphone only listen to failure, or it could listen to others? In fact, there was a test that they heard three microphone signals, and after a lot of analyses and all that, they concluded that some of this microphone signal they heard earlier was not failure indication, but rather maybe oxide cracking, or whatever. So they actually, they themself did not rely 100 percent on the microphone signal.

Another, to me, maybe even more disturbing situation which shows uncertainty is the flow meter signal and the pressure sensor. The flow meter, we're dealing with 1cc difference in the flow, and --

MEMBER ROSEN: One cc per second, per what?

MS. YANG: One cc total difference between the flow meter from the top and the bottom, as a result of fuel change -- fuel rod change in the dimensional.

MEMBER ROSEN: Flow is typically in terms of a mass flow rate, or a volume flow rate, not a

MS. YANG: It is, yeah.

MEMBER ROSEN: What do you mean when you say a cc, a cubic centimeter without a time?

MS. YANG: Well, the flow will change once the -- it will change as a result of fuel expansion, and it will change after the rod fail.

MEMBER ROSEN: Well, it changes, I agree, and flow rate -- you're saying the flow rate changes, because the flow channel is obstructed. I agree with that.

MS. YANG: Yeah.

MEMBER ROSEN: But when you say 1cc, I don't know you mean. Is it 1cc per second, 1cc per minute, 1cc per hour? The flow rate change, I'm trying to get a sense of --

MS. YANG: It's been a while since I looked at it.

MEMBER ROSEN: -- how big the flow rate change was.

MS. YANG: Do you know what is the --

CHAIRMAN POWERS: Can you tell me what flow rate we're talking about?

MEMBER ROSEN: Flow through the --

MS. YANG: It's the flow rate of the sodium in the channel of the --

MEMBER BONACA: Actually, the delta would give you the flow rates.

MS. YANG: Yeah. It's the delta.

MEMBER ROSEN: You put this rod in the channel and you establish flow. You know what it is. And then when you fail a rod, the flow changes. Typically, it goes down. Pressure goes - - Delta P goes up, the flow rate goes down. And you say 1cc. I say okay, 1cc per what?

CHAIRMAN POWERS: No, I think it's just a volume change that you have.

MEMBER ROSEN: Well, why don't we -- Rob, do you know the answer to that question?

MR. MONTGOMERY: I think I can help you answer that question. The 1cc that Rosa's referring to is at the instant of failure indicated by the flow meters. The difference in the inlet flow meter and the exit flow meter was 1cc at the time of failure.

MS. YANG: But they'd still have a unit

though. Is that per second?

MR. MONTGOMERY: Well, it's integrated -- it's at a particular point in time. Yeah, the fuel rod expanded at that particular point in time.

CHAIRMAN POWERS: And you had a volume displacement.

MR. MONTGOMERY: And basically, at that point in time, it displaced 1cc of sodium, as determined by the difference in the inlet flow meter and the exit flow meter.

MEMBER ROSEN: So essentially, instantaneous.

MR. MONTGOMERY: Instantaneous.

MS. YANG: Yeah.

MR. MONTGOMERY: At the point of --

MS. YANG: Basically, you're looking at very small differences, because what you are looking at is when the failure occurred that makes enough of a difference in the flow rate, and since the magnitude is so small, that it's hard to compare with another point. And a new point was, they have different recording systems. You know, they have three different recording systems to record the time zero for the flow meter, for the flow rate. And the different recording systems give you somewhat of a conflicting time. And during this two years we've been back from A system is the best, to B, and back to A, and then back to B, so we've been flip-flopping quite a bit.

In one of those systems, that would give you a value which is like 60 or 70 calories per gram, very similar to RepNa-8 or 10. And the other would confirm that it should be about 30, so because of all these conflicting things, and we've been flopping back and forth during the two years of our investigation, and the difficulty is, it has been -- most of the data were just stacked in the drawers during all this time. And most of the people running the experiment were not there, so we're not sure we'll ever get to the bottom in terms of signal analyses, because it's so complex, and then we're not sure we have all of the data available.

So at the last meeting, we kind of just throw up our hands and say we've done this enough. Let's call it quits. Instead of arguing is it 30, is it 50, is it 60, let's draw a range saying that's the uncertainty of the test. Kind of what Dana said, hey, do we -- how much effort do we want to spend on a single test that may not be representative. So if you go --

MEMBER ROSEN: So you have victory is what you're saying. You declared victory.

MS. YANG: Well, I'm a scientists, Steve. I'm trying to get to the truth.

MEMBER ROSEN: Well, not through the -- you're a scientist, and I grant that. And you've been trying to get truth, and I grant that. But you're not trying to get to the truth through RepNa-1. And it's not necessary that you get to the truth through RepNa-1.

MS. YANG: I'm glad to hear that, but there's always people ask what about RepNa-1? So that's why we've gone through this trying to --

MEMBER ROSEN: The industry has supported a tremendous amount of effort to try to understand RepNa-1, and what you've concluded is that RepNa-1 probably demonstrates a failure for all these conflicting reasons, between 30 and 50 calories.

MS. YANG: Right. Right.

MEMBER ROSEN: Good enough.

MS. YANG: And we just want to put it in proper perspective for all the -- but I want to say is during this whole exercise, we have a much better understanding of how to record the signals better, to interpret the signal better. We have a much better understanding about the microstructure different among the various tests which were the data were there, but because of this exercise, we have a much better understanding of the failure mechanism, I believe.

MEMBER FORD: You didn't say too much, or I didn't hear you say too much about the microstructure. Was it hydrided?

MS. YANG: It was.

MEMBER FORD: You mentioned the oxide thickness, but presumably that relates to hydriding?

MS. YANG: If you would allow me just to escape that, because that's the most sensitive issue right now, and there's just tremendous debate about it. I would rather not say it until we come to the conclusion. There's significant hydride on the material, so that's kind of where I think all of you pretty much already concluded for me that the RepNa-1 is probably not a representative test. And it is okay not to include it in this analysis. And more importantly, we are going to M5, ZIRLO low- tin cladding for those conditions.

MEMBER ROSEN: But I won't let you escape that slide without talking about the bottom line. Typical PWR pulse is around 30 milliseconds.

MS. YANG: Right.

MEMBER ROSEN: What do you mean? Is that typical in a reactor?

MS. YANG: No. I mean, obvious -- thank God, we never have a rod ejection rod drop accident. Typical in the licensing framework.

MEMBER ROSEN: In the licensing framework.

MS. YANG: With conservative licensing calculation, typically -- I mean, we have some maybe 20, 25, but typical range.

MEMBER ROSEN: People who do calculations in support of licensing of these kinds of fuel assemblies use a pulse that's about 30 milliseconds, even though they know there's really no way to get to that fast a pulse in the real reactor.

MS. YANG: Yes. Thank you, Steve. Thank you for pointing that out. That's exactly the truth. You really have to stack up conservative assumptions in order to get a pulse. That's why it's called licensing calculation. And because of that, and this is kind of an agreement among the various group, and I'm not saying it's unanimous, but most of the CABRI test has been run at this pulse width, and from now on will be pretty much run at that pulse width.

Now if you could -- I'm going to direct my to some recent industry effort related to supporting the topical, my next slide. I know I'm not supposed to be here talking to you about the Robust Fuel Program, but that's something near and dear to my heart, so I have to say a few words about it.

The Robust Fuel Program, RFP is what we call it, was formed in 1998, and some of the people in the room actually as a champion for forming this program. It's really a utility initiative trying to keep our fuel safe and economically operating. Operating economically is -- here are some of the objectives that we're driving at, is no operational surprises. We want fuel to perform as advertised. No regulatory surprises, because right now we have some of these surprises, so we want to get rid of those surprises. And that's why we're proactively supporting the RIA evaluation, which is an important aspect of the focus of the Robust Fuel Program.

And after we kind of address our current problems, our interest is in burn-up extension. Here's a little cartoon that was drawn for our program.

CHAIRMAN POWERS: Rosa, let me ask a question. I know you're not -- we didn't give you any time to talk about this Robust Fuel Program, but I'm willing to bet that the Subcommittee and even the

ACRS as a whole, would be very interested in your program. When would be an appropriate time for you to come talk to us about this program, or maybe would you please keep in mind that we'd like to hear about the program, and suggest to us a time when you know.

MS. YANG: Be happy to. Any time.

CHAIRMAN POWERS: Any time.

MS. YANG: Yeah.

MEMBER ROSEN: This I think, Rosa, just for the benefit of some of the Subcommittee Members who may not know about it, is a very expensive program that has gone on for many years. It's the utilities' money. Well, like I think it was like --

MS. YANG: It's all utility money. Right now it's about $10 million per year.

MEMBER ROSEN: Per year. And it's been going on for how many years now?

MS. YANG: Since 1998, about four, five years.

MEMBER ROSEN: So it's $50 million already been spent on this. It's not a small thing, so I think the Committee would be interested in it.

MS. YANG: And it's worth every penny of it.

CHAIRMAN POWERS: Well, I think -- I mean, I think that our interest would be most peaked when they get to the burn-up extension portion of it. Clearly, operational surprises and regulatory surprises are of interest, but I think the burn-up extension is probably where we're most interested in it.

MEMBER ROSEN: Some of the operational and regulatory surprises have been cured, like with sticking rods, that sort of thing.

CHAIRMAN POWERS: Sure. Sure. Yeah, I think we ought to try to interact with Rosa, and find a time when she can come talk to us about this, get an idea of whether we should do it Subcommittee-wise or Full Committee, because I'm sure the Full Committee would be interested. Maybe some time after the first of the year.

MS. YANG: Sure, that's good.

MEMBER FORD: Rosa, could I ask also the question. In the planning for this program, you obviously had in mind the current light water reactor fleet. Is there any part of this plan that takes into account advanced light water reactors?

MS. YANG: No, but from every document -- no, because from every document I read about advanced light water reactor, they usually just say they use the fuel at the time, so there's, you know -- not really that I see, a lot of work that goes into different fuel.

MEMBER FORD: There's no different.

MR. SIEBER: No, light water reactor is light water reactor.

MEMBER FORD: But do the advanced light water reactors, part of the strategy is to go for extended burn-up periods.

MR. SIEBER: Then you need research like this to do that.

MS. YANG: Yeah.

MEMBER FORD: But there's no difference than if you go to MOX fuels, no change?

MR. SIEBER: Yes, there is.

MS. YANG: MOX will be different. The program was formed by the U.S. Utilities, as you know, in the U.S. Only Duke Power is interested in MOX, so this program has not addressed MOX.

MR. SIEBER: Other than particle size, all fuel becomes MOX fuel, so you're going to learn about it anyway. I do have a question though. All these tests were run with sodium as a coolant. Right? And so you have to take into account when you apply that light water reactors, the difference in the cooling fluid.

MS. YANG: Yes.

MR. SIEBER: How is that done, other than to say well, we know, you know, what the heat transfer is and flow rates, but you don't know the interaction between the sodium and the clad, and obviously, velocities are different. And, you know, there's a lot of impacts there, and maybe you could say a couple of words about that.

MS. YANG: I'll say a couple of words, but if it could wait until Robby's presentation.

MR. SIEBER: Fine.

MS. YANG: We believe that sodium tests are relevant and conservative, because the sodium apparently are more efficient in conducting the heat away than water, so it would keep the cladding temperature cooler. And in terms of cladding mechanical property at lower temperature, the cladding is more brittle.

MR. SIEBER: Right.

MS. YANG: So we think the tests are relevant and conservative. Next slide, please.

For burn-up extension, as Undine alluded to earlier, that NRC has mandated that the industry does the work for the burn-up extension. The industry proposed a consistent set of criteria, proposed data to develop the criteria, and to demonstrate the compliance. So with that mandate, there are three major focus. The Robust Fuel Program focus on full burn-up extension.

The first one is industry guide, which is the framework for burn-up extension, is to say what type of criteria are needed, what type of data are needed for burn-up extension. The RIA which is culminated in the work of the topical that will be presented later. The LOCA, and I think Ralph probably will talk some of the joint effort in the LOCA area. And this is a little bit of a commercial for just saying, you know, the Robust Fuel Program is not just off-set type condition type of thing. We do do a lot of work that confirms the steady-state operation, high duty fuel designs, but the same set of data are the basis for burn-up extension, so the type of work we do are poolside inspection at the power plants, hot cell examinations, laboratory tests, laboratory testing included both in test reactors in the laboratories to provide the data. Next slide, please.

Let me just give you a quick sense about the type of poolside and laboratory tests - sorry, poolside and hot cell. I'm not going to talk about laboratory tests at all today.

The BWRs we have two campaigns, one at 57,000 which is below the current licensing limit. The other is for burn-up extension at 70,000, and noble metal chemical addition is the current practice for BWRs, and we will compare the impact of that on fuel performance.

For the PWRs, we look at two advanced alloys, both at 70 or a little bit above 70, 000 burn-up, and we'll be looking at fuel properties, cladding properties, and all the other stuff.

MEMBER ROSEN: Now help me understand, Rosa, how these plants got to these very high burn-ups. I thought 62 was the limit.

MS. YANG: Yes, these are LTAs.

MEMBER ROSEN: Lead Test Assemblies.

MS. YANG: Lead Test Assemblies.

MEMBER ROSEN: Where you're allowed to go beyond the limit --

MS. YANG: Yes.

MEMBER ROSEN: -- for a few rods.

MS. YANG: Right.

MEMBER ROSEN: Okay.

MR. SIEBER: Well, actually the whole assembly.

MS. YANG: For fuel assembly. Right. Thank you. Of course, these rods, some of them -- especially the Limerick rods are currently in the Argonne hot cell for the LOCA test. Next slide, please.

I'm running out of time, so I'm going to run through very quickly about the CABRI Water Loop Project, because --

CHAIRMAN POWERS: Rosa, let me worry about the time. You worry about making sure the Committee understands.

MS. YANG: Okay. Because Robby really has a very good presentation.

CHAIRMAN POWERS: Fine. You let me -- I will worry about the time, and you guys worry about presenting understandable materials.

MS. YANG: All right. For the RIA, we have submitted the topical, and that's the purpose of the presentation later. We have -- another effort is the CABRI International Water Loop Project. This project, by the way, is a $62 million project. It will run 12 tests, so that gives you a sense about the magnitude of this type of test. And, of course, they will be run. The difference here is they want to run it in a prototypical water loop under the PWR conditions.

Some of the special feature of that test is they will run advanced alloys, which I think this is the most interesting to the Robust Fuel Program. They will run two tests in 2002, one M5, one ZIRLO. They will run tests with very high burn-up fuel, about 70 or 80. They will show the fuel coolant interaction because this is water, so you can get the fuel cooling interaction after the rod failed.

They will also run tests to show some mechanistic understanding of the mechanisms, in fact, the pulse width, grain structure or whatever. And the reason I say whatever is because some of the tests are not clearly defined at this moment, and which is appropriate.

MEMBER ROSEN: Now, Rosa, are they on schedule to get all this done in 2002, which is fast coming to an end?

MS. YANG: Sorry. Only two tests are run. Next slide, please, then you'll see. Only two tests, which is what we call CIP. CIP means CABRI International Project, and they have six series. And two of the tests will be run this year, which is a little bit behind schedule. It was supposed to --

MEMBER ROSEN: In the sodium loop.

MS. YANG: In the sodium loop. And then they are going to do the -- you see there's a -- I'm not good at using the pointer. You see there is a three year gap here. That's when they're going to take out the sodium loop, convert to the water loop. And then they're going to run a qualification test to make sure thing go well, and then they're going to run tests in the water loop in 2006, to sort of parallel the test run in sodium to sort of bridge the gap.

MEMBER ROSEN: To really answer Jack's question about, you know, what's the difference between sodium and water?

MS. YANG: You'll see that comparison in 2006. And to answer your question

CHAIRMAN POWERS: Mark your calendar.

MEMBER ROSEN: For four years.

MS. YANG: Okay. So they're going to run some high burn-up tests. They already talk about mechanistic understanding, MOX fuel to be defined. So that's coming. Next slide, please.

The two tests that's most interesting to the industry are these what we call CIP-0 Tests. They will be run, one in October, in this month. In fact, the 17th of October, and the other will be run next month. The first one will be run is this advanced alloy called M5, which is used mostly in France, but now in the U.S., as well. This particular cladding, the oxide has always been low, about 20 micron, and you can see at such high burn-up.

CHAIRMAN POWERS: When you have very thin oxides on the M5 clad, do you pick up a lot of hydrogen in the --

MS. YANG: No. In fact, the characteristic of the M5 is the hydrogen pickup fraction is lower than Zircaloy-4, so they not only have low corrosion, they have low hydrogen pickup. These are from literature, and we have -- the hot cell program will confirm that in our program later on.

CHAIRMAN POWERS: It seems to me that I saw a report from Canada on its Calandria tubes which are made out of M5, reporting some, not all, but some of those tubes show an elevate level of Deuterium pickup. Do we understand that?

MS. YANG: I'm not familiar with that, Dana. If you could tell me more about it. Based on what --

MS. SHOOP: Actually, Dana --

MS. YANG: Sorry?

MS. SHOOP: Could I interject something in here? Framatone has recently shared with us some plots of the M5 hydrogen pickup versus the Zircaloy hydrogen pickup, so we'll have to share them with you to show what their results have been.

CHAIRMAN POWERS: I mean, what I could derive from this report from the Canadians was that many of their tubes -- they went to the M5 to reduce the Deuterium pickup. And on a few of their tubes, they saw an anomalously high Deuterium pickup and, of course, you know, what I was seeing was a report on the theory of why something should have an anomalously high Deuterium pickup. And quite frankly, it didn't persuade me, but I'm not that smart, so maybe other people know things about this.

MS. SHOOP: We'll have Framatome address that, but they have shown us the plots of that.

CHAIRMAN POWERS: Uh-huh.

MS. YANG: Okay. So the test will be performed in a week or so, and it will be done with 30 millisecond pulse. And the energy that can be injected is 95 calories per gram, because that's the highest they can put in for such high burn-up rods with this facility. You know, the new facility will be better, but for this, that's what we get.

For the ZIRLO rod, this particular ZIRLO rod is from Spain. It has very high corrosion. What

I listed here is the maximum corrosion of the rod, but the test section will be a little bit lower, at 85.

CHAIRMAN POWERS: Let's say an important thing to understand better, when you quote these oxide layer thicknesses, do you have a feeling for what the uncertainty is in those? And the reason I ask is, I see things in your topical reports correlating things against oxide thickness, and Least Squares Fits against oxide thickness. And yet, where the oxide thickness is taking a precisely known value, and whatever they're correlating against is assumed to have some scatter in it. Whereas, it seems to me that both the dependent and independent variable have a substantial amount of scatter. And that ordinary Least Square Fitting is not the appropriate technique.

MS. YANG: Yes. Robby have slides that will show the sensitivity as a result of the uncertainty. And let just address your questions about uncertainty. Yes, the uncertainty of these measurements are, I would say about 10 to 20 micron also, maybe 10 micron is what it would be. And another thing to point out is these are the maximum thickness of the whole rode, as there's azimuthal variation, and there's tremendous axial variation.

When we do the RIA test, we usually pick the top section for a couple of reasons. One, this is the most brittle section because of the highest oxide thickness in the reactor, and the other is for the PWR rod ejection, the energy is dumped mostly in the upper portion of the rod.

CHAIRMAN POWERS: One of the phenomena we've seen is that as people go to high burn-up fuel, of course, is a tendency for some deposition of Boric Acid on the upper sections of the rods. I noticed that you had test plans in which you're going to look at what this noble metal chemistry did to the surface of the rod. Are you also going to look at what this Boric Acid absorption, or have we gotten rid of that by going to the M5 cladding?

MS. YANG: Oh, boy. You have several questions. First, let me answer yes, we are looking at Boric Acid deposition on the upper portion of the PWR rod, which we refer to this anomaly as axial offset anomaly. Now that from our current understanding is the result of CRUD deposition on the upper span of the fuel rods. M5 is better in terms of corrosion between the cladding material and the coolant, so if the duty of M5 is high enough, I think we would have similar problems, like the CRUD deposition and the resulting --

CHAIRMAN POWERS: CRUD comes from the piping system, not from the clad.

MS. YANG: Yes, from steam generators, pipes, so that the corrosion in terms of oxide may be low, but the CRUD is still there.

MR. SIEBER: I think CRUD deposition is a cycle phenomenon, rather than a life-time phenomenon, because of what you do when you shutdown, is to borate the system heavily, which loosens a lot of CRUD, which you then remove, and so you go through these peaks and valleys in operational --

MS. YANG: We get rid of a lot of the CRUD that way, but those we don't get rid of in our program, we also developed a technique to clean it.

MR. SIEBER: Right.

MS. YANG: To ultrasonically clean off the CRUD.

MEMBER ROSEN: Which, by the way, you should show the Committee when you return next year.

MS. YANG: Okay. Is one of the reason we spend $10 million a year. Okay.

MEMBER ROSEN: Pretty neat.

MS. YANG: Pretty neat. Right. Where am I? So this ZIRLO have 100 micron very high burn-up, and the test will be performed a month from now, again at 30 milliseconds with about the same energy level. There's not a big difference between M5 and ZIRLO. It's whatever maximum you can get.

Now there a couple of new parameters involved in these two tests. The most important one is the first time we test advanced alloy. Dana, you asked about that. Yes, we will confirm this test for advanced alloy, is the higher burn-up than our current experience database from 63-73,000 burn-up.

So let me conclude my short presentation with, we submitted the topical, and I think, you know, there are tremendous databases supporting this submittal. There are over 80 RIA simulation tests using irradiator rods rather than unirradiated rods. And more importantly, we have a very large corrosion database, and couple that with the mechanical property test, because Robby will outline for you, it's not the burn-up, but rather the condition of the cladding that determines if the rod will fail, or not. And he'll also show you some analysis and experiments on fuel coolant interaction.

Now the test to be performed later this year, in fact, this month and next month, will just confirm the conservatism in the proposed criteria. And if the fuel suppliers want to use those data to develop higher values for the advanced alloys, they can do that. But in our point of view, we just want to use that to confirm the conservatism in our proposed criteria.

We do not think we need the water loop in order to draw conclusions from the RIA topical, because as I answered one of the questions earlier, the sodium test results are very conservative, because you have lower cladding temperature. And, you know, we already have 80 some good tests, another six, another half a dozen because some of them are in sodium, some of them are comparison. Another six tests is not really going to change the picture.

Now one of the concerns is DNB. What about DNB-induced failures? I made some broad statements saying they're not expected at this proposed value. I know that's a broad statement, and Robby is going to address that, because that's part of our entire submittal. So if you have any questions, I'll answer them. Otherwise, I think we should turn to the --

MEMBER LEITCH: I have one question. I guess you -- I'm coming away with the conclusion that RepNa-8 and 10 are still considered to be valid tests. But if I go back to your curve of enthalpy versus burn-up, the colored curve, if I plot that --

MS. YANG: They're below.

MEMBER LEITCH: They're well below.

MS. YANG: Yes.

MEMBER LEITCH: The blue curve, for example.

MS. YANG: Yes.

MEMBER LEITCH: And I don't understand why that is the case.

MS. YANG: Okay.

MEMBER LEITCH: Why wouldn't the blue curve be done through the RepNa data?

MS. YANG: Let me give you a short answer, and Robby will give you a long answer.

MEMBER LEITCH: Okay.

MS. YANG: The simple answer is, those two rods are heavily spalled. And the criteria that we have developed is for high burn-up, and we do not think we will use spalled rods for high burn-up. So in our database we clearly separate those rods that have spalled, and those rods that have not. So the criteria that we proposed are not for spalled rods, so your observation is quite correct. They are below the curve, and he'll show you that we show the mechanical property of spalled rods, are considerably worse --

MEMBER LEITCH: But in the operating reactor, there are some spalled rods.

MS. YANG: Right now, yes, but not as we go to advanced alloys. Yes, you're quite right. Some of the rods have spalled, but is very small number of rods, and we are talking about a very local phenomenon here.

MEMBER LEITCH: Okay.

CHAIRMAN POWERS: Are there other questions for Rosa? Rosa, I have a question on your proposed test matrix for the CIP Program. I don't think your slide intended to lay out a detailed test matrix, would indicate just the general types of test. But one of the things that I know about tests of this nature is, if I could do exactly the same test twice, I would not get the same answer, because there are -- though you might try to control a lot of the variables that affect the rest results, it's physically impossible to control them all.

Do you plan in that program to have a test in which you attempt to measure the magnitude of the experimental layer, essentially doing the same test twice? And if not, why not?

MS. YANG: Dana, let me first say it's not my test matrix.

CHAIRMAN POWERS: I understand.

MS. YANG: It's a test matrix proposed by IRSN, the French safety authority who will run the test, and it's being discussed and debated among all the participants, and we are just one of them.

MEMBER ROSEN: Which includes the agency.

MS. YANG: Which includes the agency. In fact, they and EDF funding the major share, the lion's share. Two-third of the program are funded by the French, so they're a little bit more equal than the rest of us.

MEMBER ROSEN: But there's U.S. government money, particularly from the NRC in this.

MS. YANG: Yes.

MEMBER ROSEN: And there's utility money, as well.

MS. YANG: Yes. So we do have a seat at the table, and we do try to argue as strongly as we can, but we're just one of the participants. Among others are the Germans, the Spanish --

CHAIRMAN POWERS: Regardless of the nationalities involved, understanding the magnitude of experimental error seems to me a critical factor.

MS. YANG: Yes, I agree with you. And that very issue has been debated a lot within the program. And we will continue the deliberation of this, but most people do not really want to spend $5 million, or $3 million, whatever the number is, just to duplicate the test. They think a lot of the experimental uncertainties could be gleaned from others. And if you look at - - one thing, Dana, I would agree with that a little bit. I mean, there's always a lot to be said about duplicating exactly the same experiment. But if you look at the whole data set, run at such vast different conditions, they're very consistent.

CHAIRMAN POWERS: I would be intrigued to hear a statistician justify that position.

MS. YANG: Okay.

MEMBER ROSEN: These are wealthy statisticians. Very wealthy statisticians.

CHAIRMAN POWERS: Well, quite frankly, I have taken the position, I think I am willing to defend the position that when you have a few expensive tests, it's more critical than ever to measure the experimental error.

MS. YANG: You can --

CHAIRMAN POWERS: If I have a lot of easy tests to do, I can get away with not measuring the experimental error. If I have only a few and they're very expensive, I should focus on measuring the experimental error.

MS. YANG: I think you are right, Dana. And like I said, we can discuss and debate that within the CABRI Water Loop. What I want to point out is, maybe it will be very clear from Robby's. At the end of his presentation, we are not using these tests in a statistical sense to develop the criteria. We're trying to understand the basic mechanism of reactivity-initiated accident, and how the failure occur. With that understanding, then we look at how consistent the data are, so the understanding is eventually benchmarked by these simulation tests. So these simulation tests give us a lot of information, because it's not just a go/no-go. It give you the emission gas release, it give you the strain on the cladding, it give you, you know, some of the microstructures, so you really have a wealth of data coming from a single test. I think, you know, it is -- they should not be treated in a statistical sense. I think --

CHAIRMAN POWERS: The problem is that you get all these data, and you do not understand how much of the variability that you see is a function of uncontrolled parameters in the test. And I guarantee there are some.

MS. YANG: Uh-huh.

CHAIRMAN POWERS: And without having that understanding, you can be fitting noise, you can missing the most important affect, you can end up spending millions of dollars for finding a code to account for an anomaly in the experiment, where you would be knocking yourself out on understanding something like oh, maybe RepNa-1.

MS. YANG: Yes, it's possible. I think the RepNa-1 Task Force investigation have produced quite a lot of some of this uncertainty information you're talking about, and I briefly mentioned some of those in terms of timing, in terms of the magnitude. So I'm not trying to disagree with you. I'm just mainly pointing out some of the considerations that has been discussed during the CABRI Water Loop Project.

CHAIRMAN POWERS: Yeah. Quite frankly, I hear it on all expensive test programs. I heard the same stories, and I will reiterate --

MS. YANG: That's one of your frustrations. I understand.

CHAIRMAN POWERS: Well, you have this, literally a hundred years of people understanding how to design experiments efficiently and whatnot, consistently coming back and saying you have to measure the experimental layer, and for some reason, we blow that all off, and say we will neglect a hundred years of people saying here's how to design efficient experimental programs, and not measure experimental layer because it's too expensive. And quite frankly, it's too expensive not to measure the experimental layer.

MS. YANG: I agree. Just for you maybe a little bit comfort is CIP0, and CIP0-1 are, in a way, kind of a duplicated test, if you ignore the coolant conditions, which I think is reasonable to ignore. But they are sibling rods, and they'll be duplicated.

CHAIRMAN POWERS: Good. Any other questions for Rosa? I propose that we go ahead and take a break here for 15 minutes. Unless there are people with airplane connection problems, I'll be kind of easy on when we end, and I'll let it run until we're done and whatnot.

MS. YANG: Okay.

CHAIRMAN POWERS: Okay. Let's take a break until 25 of the hour.

(Whereupon, the proceedings went off the record at 10:19 a.m., and resumed at 10:38 a.m.)

CHAIRMAN POWERS: We're going to now have another presentation that Rosa has set put for us with Robbie Montgomery. He's going to walk us through some technical bases here. Robbie has, of course, appeared before the Committee before. He takes the heat so that Joe Rashid doesn't.

(Laughter.)

Joe's gotten chicken or wise in his old age, I'm not sure which.

(Laughter.)

The floor is yours, sir. And, again, let me worry about the time, you go ahead and worry about communicating well.

MR. MONTGOMERY: Okay. Thank you. Thank you. I'd like to thank everyone for letting me come talk today. As Rosa mentioned, I'll be talking about the technical bases that were used to support the fuel failure and the core coolability acceptance criteria that she presented in the previous presentation.

Just a brief outline, I'll just familiarize everybody with the regulatory bases for the reactivity accident. Typically, that would be a control rod ejection accident from a hot-zero power or hot-full power bed. Then I'll go over some discussion about the database of the RIA simulation tests. Rosa alluded to a few of those tests, and I'll go through and show you some of the characteristics of the test and some of the test conditions and try to familiarize everybody with the terminology of what we talk about when we discuss RIA tests. And then I'll go through a discussion of the technical bases that we've used to establish the fuel rod failure threshold.

I'll go through some of the cladding failure mechanisms, both at low burnup and high burnup. I'll talk a little bit about the development of the cladding failure model that we've used to understand and interpret the experiments and then discuss the revisions that we're proposing with regards to the failure threshold limit used for those calculations. And then I'll go on into the safety limit and core coolability limit, talk about some of the issues related to that, how high burnup fuel influences those issues and then discuss the methodology and the revised limit for the core coolability. And then, finally, I'll try to go through a short summary of what I've said.

So it's a lot of material, but I'll try to move through it. Please, as you guys have done already, you're going to ask me lots of questions, I'm sure.

The regulatory background, Undine mentioned briefly the background. Here we have the two limits or the two criteria. One is the coolability limit in red there. It's been defined in the Reg Guide 1.77 as 280 calories per gram, and that's a radially averaged fuel enthalpy, and I'll get to what that means in a minute. It's basically set up to address the GDC, the General Design Criteria, 28. Typically, nowadays, most people use a lower value in their licensing submittals, so generally around 200 to 230 are the values that are used.

Cladding failure threshold is used for meeting dose requirements -- radiation release requirements. It's defined in a number of different places, SRP 4.2 for BWRs and Reg Guide 1.77 for PWRs, and it has a number of values or parameters are used to define fuel rod failure. For BWRs, 170 calories per gram radially averaged fuel enthalpy used. For BWRs and hot-full power BWR events -- PWRs, I'm sorry, PWRs and hot-full power BWR events, DNB is typically used to define fuel rod failure. At this point in time, in the current regulatory base, they're burnup independent, so that's how they're shown here.

CHAIRMAN BONACA: Just one point I would like to make.

MR. MONTGOMERY: Sure.

CHAIRMAN BONACA: You mentioned that typically they submit that like 230 calories per gram. I think one of the reasons, however, is that they use very conservative methods which have been approved 20 years ago and because the limit is going anyway, they don't want to invest money. I mean they also neutronics calculations that show much lower values. They simply don't want to license those codes for economic reasons oftentimes, and so the documents show very much higher limits. I'm just mentioning this because we saw certain data down in the 100 range and below, then we see the values in the FSAR 280 and we think there is such a disparity. I don't think there is that much a disparity, okay? When they do calculate this peak clad temperature with the neutronics codes, three dimensional codes, the get much lower results.

MR. MONTGOMERY: Certainly. Certainly, that's correct.

CHAIRMAN BONACA: They don't need to document them in the FSAR because they were documented a long time ago and they're still below 280. So just to precise that.

MR. MONTGOMERY: Thank you. Now, when we

look at the database here, I'm plotting a reduced set of the database. This is primarily all the data that has been tested for radiated material. As was talked about this morning, there's a large database of unirradiated tests that have been done. I've included a half a dozen or so at the zero burnup line, but there's actually hundreds of rods at the zero burnup line, I didn't include them all. What I've shown here in the database is the 80 or so tests that have been done on rods or rodlets that have been pre-irradiated in either a commercial reactor for a good number of these or in some sort of test facility, the SPERT facility, for example -- not SPERT, but the CDC, the driver core, for example. Some of those have been irradiated there. Some of them have been radiated in a Japanese test reactor called the JMTR reactor.

You have -- okay, so I've indicated here which test programs they come from. NSRR would be the Japanese program, CABRI would be the French program, you've heard something about that this morning already, PBF, the Power Birth Facility at Idaho, and then the older CDC SPERT tests. And I've only included a small sampling of those tests.

What I'm showing here is the radially averaged peak fuel enthalpy versus the segment burnup for the segment that was tested. These tests range from six-inch tests. Most of these are six-inch segments, six to eight inches. That would be the square NSRR program typically uses a six-inch section. The CDC program is about the same, about a six-inch section. Those are indicated in red. The CABRI program typically use 50 centimeters, so you'll have to do the math in your head about how long that is, about a foot and a half. Here is the CABRI program primarily.

You see a generally downward trend with the data, but that's indicative typically of the fact that these test facilities can only put so much energy into the rod or reactivity into the rod. And as a consequence, with burnup increasing, the reactivity of each rod generally drops. So the downward trend is indicative of how hard the test facility can test those particular samples.

Interspersed here, there are solid symbols. The solid symbols indicate that those are tests that had cladding failure during the pulse or following the pulse in each of these. So you see that there are some failures interspersed amongst some of the ones that did not fail, the survivors we call them. This tells us that burnup is probably not the parameter to correlate this data against, because we see that there is no clear separation between the failures and the non-fail tests.

So let me just briefly just show you a comparison, and I should point out too that in this database there's a variety of pulse widths. They vary from as low as four milliseconds to as high as 70 milliseconds. They are a variety of coolant temperatures and conditions. There's stagnant ambient water at 25 degrees C, and there's flowing sodium at 280, 290 degrees C. There's flowing water in some of these tests. The PBF were in flowing water, 1000 Psi, approximately 280, 250 degrees C. So you have quite a bit of mixture in there and the type of test conditions as well.

So here's just an example of a RIA-type pulse. We have a nine-millisecond pulse here, typical of a CABRI-type test. You have a 40-millisecond pulse, more consistent, say, with a typical PWR rod ejection event. And then even some wider pulses. And it's showing you the magnitude. And the area under the curve, the amount of deposited energy for each of these pulses is the same.

MEMBER ROSEN: And, again, a 40-millisecond is not a true in-plant event --

MR. MONTGOMERY: Correct.

MEMBER ROSEN: -- it's a value that's chosen to represent conservatively an in-plant event.

MR. MONTGOMERY: Yes. Just briefly, a schematic to show some of the terminology that I will refer to and have already referred to today. We have three curves on this plot. Again, I'm plotting time along the X axis and then power or energy or enthalpy along the Y. The pulse is here. Typically, what we mean by the pulse width is the full width at half the maximum value. Not all the pulses are Gaussian-shaped in the experiment. Some of them are double-humped, some of them have some nuances. So when you hear someone give a range of a pulse width, for example, RepNa-8, it has a pulse width range between 65 and 75 milliseconds, it's because it's a little difficult to define exactly where the full width half max is for a double-humped pulse.

The consequence of this pulse is an energy deposition, and that's this curve here which gives us the energy deposition as a function of time. And it's just simply the integration of the area under the power time curve. And typically we refer to this in terms of calorie per gram as well. So you may hear terminology like the test experience 100 calories per gram deposited energy. So that would be a value out here. The maximum deposited energy, that would be the integrated energy of the power time curve.

And then you have the enthalpy curve. That would be the solid curve here. And this is the response of the energy deposition. And this is a integration of the temperature, stored energy in the fuel as a function of time. And typically we call it radially averaged, so we're taking the average across the radius of the stored energy.

MS. SIEBER: The downward slope at the end, I take it, indicates the fuel is being cooled?

MR. MONTGOMERY: Correct, correct. So, generally, you have a maximum radially averaged fuel enthalpy that occurs during the power pulse or shortly thereafter, because depending on the width of the pulse heat conduction effects can begin to drive it downward.

The fuel enthalpy may start out at a non-zero value depending on the test conditions. For tests done at room temperature, the enthalpy's essentially zero, the initial enthalpy. And then at elevated temperatures, say in the CABRI facility where you're at 280 degrees C or at a hot-zero power state, you have some initial enthalpy which is typically on the order of 15 to 17 calories per gram. So let's see, we've talked primarily about that.

We generally look at the tests in terms of their radially averaged fuel enthalpy, and so the database that I was referring to here this is the radially averaged peak fuel enthalpy, and it's been determined by a number of different methods. Some of them take into account the heat conduction effects, some of them do not. So in and amongst this data, there is some uncertainty with regard to the fuel enthalpy when you first look at it. Okay.

Here, as a result of an analysis for one of the RIA experiments, what I wanted to illustrated here just to give an example of the fuel temperature profile across the pellet at different points in time during a power pulse. So what I have shown here is the fuel temperature as a function of radial position. And this is for a burnup of 65,000 and a pulse width of 9.5 milliseconds. And I've indicated here the range, the pellet is given here out to just a little over four millimeters. And then the cladding is this outer half millimeter range. At the early part -- in the early part of the pulse, during the upsweep, when there hasn't been very much energy deposition, you see a fairly cool central part of the pellet, and because of the radial peaking due to the plutonium build-in at the pellet periphery, you'll see there's a temperature peaking region here in the pellet periphery. At that point in time, the cladding really doesn't know what's going on yet. It's still sitting there very innocently minding its own business.

And then later on in the pulse, near the peak power, typically, depending on the pulse width, you'll reach the maximum temperature, and that will occur out near the pellet surface, generally 100 to 200 microns inside the pellet surface because of heat conduction effects. And then cladding now begins to feel some of the heat as heat conduction begins to move some energy from the fuel into the cladding.

And then as the pulse progresses, heat conduction begins to become more dominant, and then approximately two to three seconds after the pulse is over, you'll then develop -- the fuel will then develop a more characteristic parabolic temperature distribution that we're all familiar with, and the cladding is now heated up.

So as I said, the test database that we have on reactivity accident tests is pretty much summarized here on this table. We have a variety of different initial temperatures, different types of coolant conditions, different types of pressure conditions, they're pretty similar, though, quite a variety of pulse widths and a variety of energy depositions. In the early SPERT programs, they tested up near 350, 400 calories per gram. The more current programs have really focused on ranges more like less than 200.

Comparing that to light water reactor conditions, there's some differences, there's some similarities, but in all there's enough differences that it really is difficult to apply the data coming from these test programs directly to a light water reactor. So there's a need for using analytical tools to assess the test results, interpret them and then compare back and translate them back to LWR conditions.

MEMBER ROSEN: Well, hold on just a second. that 25 to 90 in the RI column is what your estimate is of the real pulse width in a reactor now?

MR. MONTGOMERY: Again, these would be based on --

MEMBER ROSEN: If you have a full rod ejection.

MR. MONTGOMERY: -- full rod ejection, licensing-type analyses where you've made conservative assumptions on the parameters of control rod worth. This would be the range of pulses that you would expect to see.

MEMBER ROSEN: So the 40 you saw before, the 40-millisecond pulse you saw before you said was not typical of a LWR. Did you say that because of the 90 value?

MR. MONTGOMERY: No. I said it would be typical.

MEMBER ROSEN: Oh, you did. I misunderstand.

MR. MONTGOMERY: I'm sorry, I must have misspoke then. Yes, the 40-millisecond pulse that I showed in the previous slide would be representative of -- this pulse here would be representative -- in the range of a licensing-based --

MEMBER ROSEN: Of what could really happen if in a PWR a rod was fully ejected.

MR. MONTGOMERY: Right. That's correct.

MS. YANG: No, no. The best estimate we did not get a pulse. That's a conservative licensing calculation, as Robbie said several times. The 40 millisecond we call representative is representative in the licensing calculation, but you are asking question about if you have a rod ejection in a PWR.

The best estimate does not show any pulse. The best estimate doesn't show a pulse, but you have to use conservative assumptions in order to get a pulse, because we're dealing --

MEMBER ROSEN: Why does it show no pulse if the rod is ejected? Is it so slow?

MS. YANG: Yes.

MEMBER ROSEN: If you actually had a rod ejected, it would be so slow that there wouldn't be a pulse, you're saying.

MR. WERMIEL: We'll talk about this some this afternoon, so -- we could talk it about now, but let Ralph, when he comes up this afternoon, say some more about this.

CHAIRMAN BONACA: Just a question. From any conditions? Those are from, for example, have zero power? I mean we assume all rods inserted and you're pulling out one? I mean I would expect to see an effect there.

MR. MONTGOMERY: Well, there is an effect but it generally is not a prompt event. You have to have -- I'm not a neutronics expert so I'll try not to get too -- I'm going to get in over my head real quick -- but it's the addition of all the -- assumption of all the parameters that go into calculating a rod worth that gives you the prompt event. And it difficult to -- unless you assume very conservative values for things like neutron lifetime, Doppler coefficients and all the parameters that go into rod worth, it's difficult to make it a prompt event. You'll get an event, you'll get generally a fast rise to power, but you won't have a prompt pulse. It will go to some power level very fast, but you won't have a pulse because it won't be prompt, you'll be less than a dollar.

MS. SIEBER: And you don't have damage in short-term unless you have a prompt event.

MR. MONTGOMERY: Yes. The prompt event then gives you -- obviously, it gives you the rapid rise in the fuel enthalpy because you get this, in effect, an adiabatic type of energy deposition. It needs to be on the order of less than a second to deposit energy faster than the fuel conducted out.

MEMBER ROSEN: I'll wait for later, but I think I'm beginning to understand. We'll hear more about it later.

CHAIRMAN BONACA: Yes. Except that this goes counter to a lot of physics calculations. So it will be interesting to hear more about that there isn't any pulse.

MR. MONTGOMERY: But given a licensing-based approach where the assumptions that go into the calculation of rod worth used in a multi-dimensional physics calculation would generally give you pulse widths that are in this range, and it really depends on the rod worth and these sorts of things.

Now, what have we learned from this database? What we've learned is that the cladding failure response -- I'm going to talk initially about cladding failure, then I'll come back and talk about coolability and fuel rod geometry effects and that discussion. So with regard to cladding failure mechanisms, what we've learned from the database is that there are essentially two failure processes or mechanisms that are active in a fuel rod during a reactivity accident.

The first one generally occurs at low burnup, and that's a high temperature failure response caused by post-DNB operation, and when you go into post-DNB operation you get the cladding temperature excursion which initiates oxidation effects and possibly ballooning effects, and that is generally what happens at low burnup. At low burnup, the pellet cladding gap is generally fairly wide, and the cladding ductility is good. And it can survive any sort of pellet cladding mechanical interaction that goes on at low burnup. But once you get into post-DNB operation there's potential for cladding failure due to the oxidation processes or ballooning type processes.

At high burnup, where now we have -- the gaps tend to have closed or become quite small and the effects of oxidation and hydriding and irradiation damage have all combined together to decrease the cladding ductility, then the failure process is transitioned from a high temperature response to, I don't want to use the word "low temperature," but cooler temperature response where the cladding hasn't seen much heating to failure by cladding ductility processes.

CHAIRMAN POWERS: Let me ask you a question, Robbie. On one of the previous slides, you showed the database, and in that database you quoted the pressure at which the tests were run. And all the tests were at relatively modest pressures with fuel rods that had been reconstituted, yet the accidents of interest are at high pressure. And whereas we probably don't worry about the pressure effect when we're on the left-hand side of this current plot, the low burnup side, it seems to me that pressure becomes a concern when you're on the high side where your failure is due to pellet clad mechanical interactions. why don't we worry about the pressure at which these tests are run?

MR. MONTGOMERY: The primary effect of temperature is the pressure differential, and in the experiments that the pressure differential is set up through the re-fabrication process, and generally the pressure is equal to or less than the external pressure in the experiments that have been done on pre-irradiated material. There have been tests done where the pressure differential is positive and looked at the ballooning effects. At high burnup, we don't expect rod pressure to be a real dominant mechanism because the pressure differential is negative still at hot-zero power, because the fuel is a bit cooler and we license generally to pressure levels that are equal to system pressure at power conditions. So the pressure differential is negative, if you will, it's coming from the outside instead from the inside.

And then, secondly, at elevated burnup, the axial gas communication is quite restricted because of the closed gap and the tight condition between the fuel and the cladding. So the pressure, which is generally -- a majority of the gas is resident in the plenum doesn't have the time in the time frame that we're talking about, less than a second, to migrate to these regions and to contribute to any additional PCMI loading. I'm not sure if that answers your question, but those are the --

MS. SIEBER: I'd like to ask a question that would display my ignorance. If in a practical reactor with a best estimate calculation you can't achieve reactivity insertion that would give you a prompt pulse, then why don't we concentrate on making sure that the mechanics of reactivity insertion will not provide a prompt pulse rather than do all these experiments on what happens to the clad after you get one?

MR. MONTGOMERY: That's a good question. Unfortunately, I don't have an answer for you.

MS. SIEBER: Is this a political question?

MR. MONTGOMERY: Are there any more questions regarding this?

(Laughter.)

MEMBER ROSEN: You mean there's no one in this room who would venture an answer to Jack's question?

MR. ROSENTHAL: Rosenthal. I'm the Branch Chief of the Safety Margins and Systems Analysis

Branch, and we have discussed that at the conclusion of all of this really the free variable is the core design since the rod patterns and the rods are fixed in an existing reactor and that one could design such that you limit the rod worths, and then the rod worths, in turn, determine the pulse widths and, in turn, the enthalpy deposition. So that when you're all said and done, from a very practical reload standpoint where you have to do analysis every 18 months, you might come up with a surrogate in terms of rod worth that ripples through. So we have had those discussions, but I think at this point we're trying to still understand the underlying phenomenology. But, yes, you're right, pragmatically that's where you may end up.

MS. SIEBER: Well, I'm listening to discussions on how much all this costs. On the other hand, part of the solution to this gets back to Dana's comment of an hour ago, which says you ought to really know the experimental and calculational uncertainties to be able to really put your arms around what's going on and what's important and what is not important from a practical phenomena standpoint. And, you know, I'm all for learning everything about everything, and you can make a career out of that, but, you know, once you can establish that an event is precluded, then that changes the focus of where you want to spend your resources, I would think.

MR. MEYER: Ralph Meyer from Research. I think the practical answer to the question is that in the past licensing calculations have been predominantly done with point kinetics models --

MS. SIEBER: Right.

MR. MEYER: -- which are grossly conservative and they give big numbers.

MS. SIEBER: Yes, they do.

MR. MEYER: And so they give energy depositions, fuel enthalpies that are in the range of 100 or more calories per gram. Now, everybody now has --

MS. SIEBER: And they're fictitious, right?

MR. MEYER: -- 3-D kinetics models and nobody has -- well, the models have been submitted, but as far as I know we are not routinely reviewing results of those to the point where we could address this issue. I know at least in the context of this generic issue that the industry has not come forward with 3-D calculations that could be reviewed by NRC that say we're way out of the ballpark on this subject.

CHAIRMAN BONACA: And the reason is that you've kept the limit at 280. I can tell you for a fact, being from the other side for a long time and being involved in this. And the reason is that there is no motivation for a vendor to come in and modify its methodology and have it qualified and accepted, modified and validated, when they can still use the point kinetics combined with a PDQ peak 2 average and can stay well below 280. So what's the point? I mean some of the analysis on the dockets go back to 1968, '70.

MEMBER ROSEN: If George Apostolakis were here, he would go right through the ceiling because he would say it's exactly the same reason that licensees don't do better PRAs. There are no real requirements.

CHAIRMAN BONACA: Well, but I think it's important to understand that from the perspective of the vendors and the owners they are aware that the results are much less severe than what is in the FSAR. You just simply don't go in and change an FSAR if it is a bounding value that is still there. I mean how many of those values in the FSAR go back to 1970?

CHAIRMAN POWERS: I mean I think what you're seeing is a statement on the state-of-the-art that preceded 1983 --

CHAIRMAN BONACA: That's right.

CHAIRMAN POWERS: -- that a high licensing criteria was set that could be easily met with conservative analysis methods. The general belief of all concerned, regulator and licensee, was that nothing would ever approach that in a conceivable core design. There was no incentive to change the criteria, there was no incentive to improve the analysis. What upset that was in fact the RepNA-1 test.

CHAIRMAN BONACA: Absolutely.

CHAIRMAN POWERS: And we should all hail RepNA-1 for having awakened us to the fact that fuel is important and whatnot and let it go at that and move on.

(Laughter.)

I will comment that we're spending most of this morning dealing with RIAs, and certainly that was where this thing started. This afternoon, we're going to deal with other aspects of high burnup fuel, LOCA, ATWS, things like that, which are also important. With that, I'll give it back to you, Robbie.

CHAIRMAN BONACA: One last note I would like to make then is that this is an example of where because of those licensing constraints, maybe we have failed to learn something here that has imposed enormous conservatism and maybe enormous regulatory burden, but the industry has accepted it in place of itself, because we didn't go forward, we understand these issues. If in fact you can convince me that you're not going to have any pulse resulting from a rejection from any conditions, then I can tell you how many places there are where those kind of previous commitments are a burden to the utility.

MEMBER ROSEN: Well, beyond burden, Mario, which I agree with, what concerns me about this in a very general and broad sense is that it diverts attention from the really risk-significant accidents that could occur and their enthalpy deposition parameters.

CHAIRMAN POWERS: It's one of the fundamental flaws of the design basis accident concept, which you and I have decried for advanced reactors.

MR. MONTGOMERY: Okay. Well, back to the cladding failure processes that we were talking about before. Effectively, there are two processes. Just to remind everybody, we have a low burnup -- a process that's primarily active at low burnup and that's the post-DNB response due to high temperature mechanisms, such as oxidation, induced embrittlement or ballooning response. And then this typically occurs after the power pulse when energy's had time to conduct from the pellet to the cladding and initiate the post-DNB heat transfer processes. And then as burnup proceeds and we changes induced in the rod as a consequence of burnup, either through -- well, both through pellet cladding gap closure and changes in material ductility. it's possible to induce failure for a PCMI, pellet cladding mechanical interaction, process during the power pulse. If in fact it's possible to survive in some way, either through improved material ductility, the power pulses at high burnup -- then the post-DNB operation could become effective or active.

So just to reiterate a few points. Cladding mechanical failure mechanism is PCMI resulting from the pellet expansion and fission product matrix swelling in the pellet. The controlling factor or the key factor is the material ductility, the cladding ductility. This conclusion is consistent with the PWR PIRT that was done a couple years ago, a year and a half ago.

The burnup is not really a key factor. It does influence the gap closure processes and initiating of PCMI, but it's really the field duty that drives the corrosion and hydriding process that define the residual ductility. We know that spalled rods, which we've talked about briefly and I'll talk a little bit more, has significantly less ductility than the non-spalled rods. And we see that at high burnup, for rods that have no spallation, no oxide spallation, but still high, on the order of 80 to 100 microns but without any spallation, they have not failed up to now.

MEMBER ROSEN: Can you zero in on that for me that last statement, that spalled rods have significantly less ductility than non-spalled rods. Spallation is a surface phenomena on the outside of the rod -- of the oxide layers on the outside of the rod surface. The ductility is a property of the remaining un-oxided, non-oxided cladding.

MR. MONTGOMERY: Correct.

MEMBER ROSEN: So how are these tracks connected?

MR. MONTGOMERY: How are they connected? That's a very good question. During the oxidation process, certain fraction of the hydrogen is produced due to the chemical reaction. It's absorbed into the cladding and is resident in the Zircaloy matrix material. If the cladding oxide is rather uniform, then the temperature distribution generally azimuthally and axially is rather uniform, and the hydrogen stays rather uniformly distributed. There's some gradience through the thickness that occur because of the temperature grading across the thickness of the clouding. But azimuthally and axially, the hydrogen stays rather uniform.

Once spallation happens, and the spallation process is the local loss of oxide cracking and falling off the oxide layer, you get local perturbations in the cladding wall temperature. Either they're hot because there is an insulating layer of oxide and steam that's ingressed in a crack between the oxide layer before it's fallen off. You might have a local hot spot. Once the oxide has fallen off and exposed either bare metal or a thinner oxide, maybe it's gone from 100 microns to ten microns, then you have a cold spot. These local temperature variations induce thermal gradients that drive hydrogen to move and become non-uniformly distributed. And you get localized areas where hydrogen concentration is elevated. That can increase to pure zirconium hydride levels and be on the order of several thousand ppm locally. And this hydrogen is what influences the material ductility. And it's the non-uniform distribution of the zirconium hydrides that have the biggest impact on the material ductility.

MEMBER ROSEN: So once a piece of oxide spalls, it cools off the cladding in that region and hydrogen moves into this cooler region of the cladding?

MR. MONTGOMERY: Correct.

MEMBER ROSEN: Creating lower ductility in that region.

CHAIRMAN POWERS: What you're making an argument is that you get the hydride precipitation following a spalling event. I could have gone through the same argument and said that it's the hydride nodule that causes the spalling event. And I mean the argument would go along something like this: That when I look at a detailed stress/strain analysis of the oxide growth process, I find that the compressive stress in the oxide imposes a tensile stress on the underlying metal. And that as long as that metal is ductile, everything is fine. As soon as it embrittles, then I get a separation at the interface causing the spallation event. That loss of ductility could come from the formation of a hydride.

MR. MONTGOMERY: Well, I haven't really gone into the details of exactly what drives the spallation process. The spallation process is very complex process. It obviously is one process that could lead to the spallation. But we have seen from micrographs of non-spalled material with very thick oxides, 80 to 100 microns, generally the hydrogen is rather uniformly distributed around the azimuthal dimension. There is generally a gradient through the thickness. There's local deposition -- or precipitation of hydrides near the outer surface of the cladding due to the thermal grading and stress grading that you point out. These have an effect on the ductility but not a dramatic effect as what arises from spalled material.

The spallation process where the oxide falls off and creates cold and hot spots is what leads to the non-uniform hydride distributions. Local hydride, sometimes we use the word "lenses" or "blisters" to define a region of maybe three or four clad thicknesses in azimuthal angle, a few degrees, ten- to 15-degree angle, where you have a very high concentration of hydride. This results from the spallation process and generally is not observed when you have a uniform hot side.

CHAIRMAN POWERS: Well, I mean it's a question of cause and effect. I mean the problem, of course, is that you only see after the spallation event where a spallation has occurred. But it's not obvious to me that you can immediately conclude that the hydride precipitation that you see there followed the spallation event and didn't precede it.

MR. MONTGOMERY: Well, yes, we don't always see exactly what has caused the spallation event. We do see end rods that have spalling. There are regions that don't have spalling because it's a very local phenomenon. So the micrographs are available a few inches above or a few inches below where you have a uniform oxide layer and you see these fairly uniform hydrogen distributions, but when you move up into the spalled region, then you see these non-uniform hydride distributions. You're correct, we don't know --

CHAIRMAN POWERS: I will argue that in every case where we've seen a spall and looked at the underlying material, there's something unusual down there. And that something unusual could have led to the hydride formation and the hydride led to the spalling rather than the spalling leading to the hydride.

MS. YANG: I think whatever the cost -- well, we don't know. Actually, we don't know --

CHAIRMAN POWERS: You're going to have to be on the record or we'll never know what bit of wisdom you gave us.

MS. YANG: Oh, no, I wouldn't go that far.

CHAIRMAN POWERS: Well, you can't talk unless you're on the record.

(Laughter.)

MS. YANG: I think the mechanism is not very important here. There are different -- it could be hydride to drive the corrosion --

CHAIRMAN POWERS: Oh, Rosa, let us have some fun discussing science instead of all this practicality stuff.

(Laughter.)

MS. YANG: Okay. In that case, we can debate the mechanism. What I want to point out is when you have spallation you have hydride lenses form depending upon the degree of spallation, and sometimes the lens could be very thick into the cladding. What I was drawing on the picture is what Robbie just said, that in the right-hand side which is a regular PWR rod that you have some hydride on the cooler part of the cladding and that's a normal condition. When you have spalled rods -- it needs the spalled rods and we don't know which, chicken first or egg first, but you have these spallation, you have these hydride lenses and that's what really causes the cladding to behave quite differently. And he'll show you some mechanical property data that clearly shows the two types of cladding behave rather differently.

CHAIRMAN POWERS: Well, see, the difficulty is this: That one could come along and say, okay, we can take this fuel up to high burnups as long as you don't see any spallation in the course of going up there, because that will lead to hydrides. Well, if the hydrides come first, then that criterion is no good anymore.

MR. MONTGOMERY: Okay.

MEMBER FORD: Robbie, does barrier fuel cladding come into the equation, this disconnect between non-barrier fuel cladding and barrier fuel cladding?

MR. MONTGOMERY: Barrier fuel cladding, if you're referring to the type of fuel cladding that's used in BWRs --

MEMBER FORD: Correct.

MR. MONTGOMERY: -- the oxidation response in BRWs is generally considerably less than PWRs.

MEMBER FORD: No, no. I was really driving at the fact that cladding ductility is a key determining factor.

MR. MONTGOMERY: Yes.

MEMBER FORD: If you have zirconium barrier on the ID of the tube, then that must affect the overall mechanicals in plants.

MR. MONTGOMERY: It does some.

MEMBER FORD: It does.

MR. MONTGOMERY: I mean that's generally included -- when we measure mechanical properties of barrier cladding, it's inherent in that database because we generally don't separate that out. We don't separate the barrier. When cladding with a barrier is tested for the mechanical properties, it's tested as a unit. The barrier is included. And so whatever effect the barrier has on the material properties is inherent to that data. Do you understand what I'm saying?

MEMBER FORD: Correct. We'll bring it up as you go on.

MR. MONTGOMERY: Yes.

MEMBER FORD: Because if you want to use a barrier fuel cladding, then you could well not have any mechanical failure because of the interaction between the --

MR. MONTGOMERY: Oh, I see what you're saying now.

MEMBER FORD: If the barrier fuel cladding came out because of PCMI problem.

MR. MONTGOMERY: Right. And what we're talking about here is not really stress corrosion cracking induced failure, this is really a bulk material response. So the PCMI that I'm referring to here is really being controlled by the entire cladding wall thickness and not the inner surface. The barrier liner was set up to limit localized stress effects and other things, which --

MEMBER FORD: No, I wasn't really talking about ID as being the final failure mode.

MR. MONTGOMERY: Right.

MEMBER FORD: I was talking about the zirconium barrier is purely a compliant layer between the fuel, expanding fuel, the fission gas, and the relatively unductile Zircaloy-2 in this case. But the same principle should apply to Zircaloy-4 because it wasn't compliant there. I take it that hasn't been done. There hasn't been done the same tests on Zircaloy-2 as has been on Zircaloy-4.

MR. MONTGOMERY: No. There have been some

RIA tests on Zircaloy-2 material with barrier material.

MEMBER FORD: Oh, there has.

MR. MONTGOMERY: Yes, there has.

MEMBER FORD: Okay.

MR. MONTGOMERY: In order to understand the high burnup cladding failure process, we needed to develop a cladding failure model, so a cladding failure model based on PCMI conditions is what I'm going to talk about next. And the model is based on strain energy density concept or parameter.

We looked at the -- generally, when a mechanical property test is done, you get parameters such as stress and strain, yield stress, ultimate tensile stress, uniform elongation and total elongation type parameters. If one integrates the stress/strain curve from the mechanical property test, you end up with a strain energy parameter, called the strain energy density. And, generally, that's the critical strain energy density if you carry that integration out to the point of failure in the mechanical property test where you're measuring things like yield stress and ultimate tensile stress. We call that the critical strain energy density.

The strain energy density is just simply the integration of the stress/strain response. What we're talking about here, in the analysis of a reactivity initiated accident test, an RIA test, a code such as FALCON, it was referred to earlier, a field performance code that would calculate that response would calculate the stress and strain evolution in the cladding, and that would be what we call the SED. This concept or approach addresses the effects of strain rate brought up earlier, temperature and the stress condition by axiality, tri-axiality stress conditions. And it's a measure of the loading intensity on the cladding.

The CSED, which we determine from mechanical property tests, it brings in the material characteristics such as the hydrogen content, the temperature, the hydrogen morphology and distribution, and it is used as the parameter to define the point of failure. The cladding is calculated to fail an analysis -- if the SED from the response of the fuel during the power pulse exceeds the CSED, then it would be --

CHAIRMAN POWERS: Robbie, I guess I don't understand how your strain energy density takes into account the strain rate.

MR. MONTGOMERY: Because here in the

calculated strain energy density, you're calculating the response of the cladding as a consequence of the energy deposition. So the response of the cladding is going to become a function of how fast the energy is deposited in the fuel.

CHAIRMAN POWERS: And it's because of the way that you're going to incorporate the properties of the cladding into the calculation.

MR. MONTGOMERY: Yes. And also in the CSED material database, these mechanical property tests are tested with certain types of strain rates. So the constuitive law that you have here that drives the stress/strain law incorporates it as well.

MEMBER FORD: But the CSED will also get some sort of strain rate.

MR. MONTGOMERY: It could be, yes, it could be. The database that we have so far that I was just about to show has a range of strain rates in there. Now, in analyzing in this data, we didn't find a strong dependency of strain rate in this database. This is a database of medium to high burnup fuel cladding properties that we had available to us to use to develop this type of model. We have burnup ranging from about 25, 30 out to 63,000, with fluence ranges from about five to 12 ten to the 21. These oxide thicknesses range from rather low, on the order of ten to 15 microns, up to 110, 115, 120 type range with oxide spallation in some cases. Like testing temperatures range from room temperature all the way up to operating temperature type conditions. And then the strain range was all from very fast strain rates, on the order of five per second, all the way down to ten to the minus five per second. So quite a variety of strain rates.

Just to kind of point to a question or a comment that, Dana, you made earlier, in these oxide thickness ranges that I'm talking about here, these are generally the measured oxide on the sample that was tested in the mechanical property test. There are a variety of different tests that are done here. we have

CHAIRMAN POWERS: The question I'm going to ask you eventually, so you can think about it, you don't have to answer it right now --

MR. MONTGOMERY: Okay.

CHAIRMAN POWERS: -- is I see -- you know, I see in this topical report that you're going to develop critical strain energy density correlation as a function of the oxide thickness, and you're going to that with the Least Squares method, okay? And you're going to do that taking this oxide thickness or its ration to the clad thickness as a well-known parameter, yet the previous speaker said that there was substantial uncertainty in that oxide thickness, approaching 100 percent, as you got down to the lower thicknesses that you have here. Okay? And when you've got that situation where your independent variable is uncertain just as much as your dependent variable in your correlations, you can't use normal Least Squares fitting methods, you tend to overemphasize the slopes when you do that.

MR. MONTGOMERY: Okay. Thank you. I will think about that and try to answer it after lunch if we get that far.

Okay. Just to point out that generally the oxide thicknesses that I have reported in this table, and that we used in the next plot, were measured on the sample. Now, I did not get into the details of the error associated with the measurements themselves, but these are very local, as I was about to say. The ring tension specimens are generally a quarter of an inch in height. They're a ring and they're tested by pulling with some sort of dye device on the inside surface, maybe a double-D set pull. Axial tension tests are generally short four- to six-inch segments that are pulled axially. And then burst tests are generally six- to eight-inch specimens that are pressurized with either primarily oil but some of them are gas pressurization systems. Some have been included -- removed all the fuel, some of them have only removed part of the fuel. But you have a variety of different tests that we get the information from.

The next page gives us a flavor for a subset of this data. This is data all applicable to 300 degree C range. You see from 280 to 400 degrees C. What I've plotted here is the critical strain energy density which, in effect, is an integration of the stress/strain curve coming from the experiment, plotted as a function of the sample oxide thickness to cladding thickness ratio. We picked that particular parameter because in most of these samples the hydrogen concentration in itself is not measured. In some they are, but a good fraction of them they're not. And we know that really it's the hydrogen that's the variable that we want on the X-axis but since we don't have access to it, the oxide to thickness ratio was a parameter that, in effect, represents the hydrogen impact.

We have a variety of testing conditions. We've got axial tension test, ring tension tests, we have burst tests. We also have separated out the solid symbols are the data from samples that have spalling oxide layers on them. The samples themselves may not have come exactly from a spalled area or have exactly spalling on them, but they came from regions that had spallation. And that would be the solid symbols here. And you do see a separation between samples that were oxidized but without spalling and then those that are oxidized with spallation. So there is some separation of the data.

You see some scatter here on this plot, but a good part of that scatter is related to the test conditions. We're mixing different temperature ranges, we're mixing different testing conditions. We've tried to use biaxiality correction factor to bring together the burst data and the uniaxial type tests, so there has been some, it's been talked about in the topical, a correction factor that brings into the biaxiality effect between a burst and an axial test -- or a uniaxial test.

There is some scatter due to design effects. There's some bending effects that come into play in the ring specimens, for example, so there's some test artifacts that it will add some scatter to that.

Currently, I'm showing here a best fit of all the open symbols and non-spalled data and the a best fit of the spalled data. And you may wonder why we selected to use a best fit as opposed to some other lower bound or some other type of fit, and I'll talk about that in a minute about how we justified that by --

CHAIRMAN POWERS: See here's where the question comes up, is that you fit this with ordinary -- and yet your independent variable in the fitting process is just as uncertain as your dependent variable. And you should not do that. You should use something like a min-max sort of process, because otherwise you're going to overestimate slopes.

UNKNOWN: You eventually take a logarithm of this and do it with a linear by a Least Squares fitting.

CHAIRMAN POWERS: But you've got uncertainty in both variables.

MR. MONTGOMERY: I understand.

CHAIRMAN POWERS: And we can't use them in the ordinary linear Least Squares fitting.

MR. MONTGOMERY: Certainly, your point is well taken and we will go back and look at if we added error bars in the X direction on these, how big they would be with respect to what we did the fitting for. I'm not fully convinced yet that it's large enough to have a significant impact on the fitting process.

CHAIRMAN POWERS: Rosa told me that the oxide thickness measure in uncertainty are quite large, especially as you move toward thin oxides.

MR. MONTGOMERY: Thinner oxides. Now, a lot of these oxides were measured destructively, and what Rosa's referring to may be a non-disruptive poolside examination technique. There is a lot bigger variability in poolside examination techniques as opposed to destructive examinations. Here, primarily these were determined through destructive examinations, because the samples are defueled and tested in a hot cell and through metallography it's fairly straightforward to get the oxide thickness from the specimen, but not in all cases.

CHAIRMAN POWERS: I mean the problem is you can measure it at one location to three significant figures, but if in fact you have azimuthal and --

MR. MONTGOMERY: Azimuthal variations, yes.

CHAIRMAN POWERS: -- axial variations, that's what you really want.

MR. MONTGOMERY: Right.

CHAIRMAN POWERS: You want some volume with --

MR. MONTGOMERY: And that's what we -- I would go back -- taking your input, I would go back and look, what would be the variability for each sample? And we'd have 100 samples here and I'd go back and try to determine is that 50 plus or minus five or is that 50 plus or minus 25?

CHAIRMAN POWERS: Right.

MR. MONTGOMERY: That's what I would try to do.

MS. YANG: Robbie, I thought you had done analysis to show the uncertainty bar, how the effects the criteria.

MR. MONTGOMERY: Well, I'll --

MS. YANG: You can go into that later.

MR. MONTGOMERY: -- go into the uncertainty, but that's the next slide is that I've looked at different fitting approaches. Instead of doing a best fit, a lower bound fit to this database and then limiting the amount of data we used to look at just the burst data, so it fit just the burst data, some people would argue that's the most applicable to a PCMI stress state would be the burst data. So I've done that.

MS. YANG: Robbie, if I could just add one more thing, if you'd go back to your slide. I'd just say the uncertainty of ten microns that's at the poolside. If you ask the person using the eddy current technique, they probably would quote something like a couple micron that's the technique, but I think ten is a reasonable number. But for very think oxide, let's say the oxide is ten or 20 microns, the cladding ductility is so high it probably doesn't make much of a difference if you're talking about ten micron or 30 micron.

CHAIRMAN POWERS: It makes a huge difference when yo do Least Squares methods.

MS. YANG: Yes.

CHAIRMAN POWERS: Then you're waiting just as much on that end as you are on this end, and you shouldn't be doing it, it will flatten your curve. It's giving you a slope which may not exist.

MS. YANG: You are right about the fitting, but this curve is the data that we develop the CSED, but when we develop the criteria that we propose in the topical, we're taking an upper bound curve. So in that case, the uncertainty in the oxide thickness is not very important. I'm giving away a little bit of what Robbie is going to say, but I just want to point out the difference in the data when we develop the criteria, which we really take the upper bound of the corrosion thickness, so that in the case the uncertainty in the measurement of the oxides are not relevant. So we can come back to that when he presents the --

CHAIRMAN POWERS: I'll be stunned.

MR. MONTGOMERY: Okay. So I didn't put all the data on this but the blue line is the same as the previous slide where you saw the data scattered about. And in addressing the uncertainty question that we've -- and the data scatter question that has been raised before, we also looked at a number of other ways to look at the data, and that was with fitting just the burst data and ignoring the other data from ring and axial, and then also taking a lower bound of the ring and burst data and arguing that the axial data, since it's not in the direction of PCMI, we could not look at that. So I will come back to this with regard -- well, I think the next slides shows it. Okay.

Now, if we then go back and analyze each of the experiments from CABRI that we've done here, these are the UO2 tests, with -- we used FALCON, you could use SCANAIR, which is the French version of FALCON, or FRAPTRAN, and calculate what the maximum strain energy density is during the pulse event. And that's what I have plotted here is the strain energy, and you can think of it in strain or stress if you want but I'm using strain energy density here, for each of the experiments. So we've gone and analyzed the pulse, given the appropriate boundary conditions and burnup levels and oxide thickness, et cetera, et cetera, taken that into account and calculated for the actual experiment pulse what the SED would be for that cladding. And we've put those points on here, and that's what the symbols mean, as a function of the maximum oxide thickness divided by the cladding thickness ratio for that test specimen.

MEMBER FORD: Just for interest, where would Rep-1 be, just for interest?

MR. MONTGOMERY: In terms of oxide thickness ratio, it's right here, and in terms of the calculated SED at failure, it's about right here, just about a half, little less than a half. So it went way down here.

Now, if we now superimpose on these tests, and I should just point out that these two tests, RepNa-8 and RepNa-10, as Rosa talked about this morning, they did fail with a cladding crack.

MEMBER FORD: So just to follow up on that, I apologize for destroying your train of thought, based on that, Rep-1 is not crazily out of your model. Assuming that your red line is correct, and there's some assumptions in that, and given the variance you have on either side of that line, it's not out of line, especially if you put importance on any stress intensification, either because of that pit or because of the scratch. It's not so out of line.

MR. MONTGOMERY: Yes. it sits down in this range, and we would have to look and see what would be necessary in terms of stress intensifications or some other factors that would either move this line down or move it up if we were to do a local effects calculation.

MS. YANG: It's below the curve.

MR. MONTGOMERY: It's well below the curve. It's down in this range, approximately a half. Okay.

So I get the sense that at least some in the room are understanding what I'm trying to do here. So if we then take the previous curves, the CSED curves, and compare them, this is the best fit for the non-spalled material and this is the best fit for the spalled material. We see that for the failures, they reside above the spalled CSED so they would be predicted to fail by the analysis process. The non-spalled specimens, 2, 3, 4 and 5, all reside below the best fit. They survived without failure, and that's what this process would indicate.

Now, if we were to go to instead of the best fit, the best fit of the burst data, non-spalled again, we see that it would basically give almost the same answer as the blue line except that RepNa-2 would be predicted to fail. And then if we went to the lower bound of the data, we see that that curve would predict that RepNa-2 and 3 failed when in fact they did not. So you can see there's some justification -- the strongest justification for using a line more like this one is the fact that it does reproduce the experiment results.

And we've done this for the tests done in sodium, which is elevated temperature, 280 degrees C. And the process is similar when we -- I didn't show you the CSED data for that, but we've done it also for the room temperature tests. So with mechanical property data for temperatures less than 150 degrees C, we've derived a similar curve through another database, albeit not quite as large as the other one, and then analyzed some of the -- these are tests out of -- all these are from the Japanese program. The Japanese program is done in atmospheric condition in water, so you're starting at 25 degrees C. The SPERT-CDC test is the same way.

We see a similar correlation where the failures are near or above the line of the CSED, and those that did not fail are below the line. There are two that reside very near the line or on the line, which in post-test examinations they found part-wall cracks. So they were very near failure. They did not fail, but they were very near failure.

MEMBER FORD: And the physical argument is purely difference between those two cards is difference in temperature and therefore the ductility of the Zircaloy-4 with a given amount of hydride.

MR. MONTGOMERY: Yes.

MEMBER FORD: Hydriding being --

MR. MONTGOMERY: Yes, correct.

MEMBER FORD: -- with the oxide fitness.

MR. MONTGOMERY: Correct. So the primary difference between these two curves is the temperature effect on ductility. The hydrogen effect, which is influenced by temperature because of solubility considerations, drives the -- is the mechanism that drive the difference between those two lines.

So in the previous set of slides, I've established an analysis methodology that has been able to reliably reproduce the results of the experiments conducted on irradiated fuel material. And given this basis of understanding, now we understand the processes that go into cladding failure under power pulse condition. We can use that to now establish the licensing threshold for fuel rod failure. And so we've done that and that's in the topical report, and we did that to construct something that's consistent with the licensing approach. And what that means is we're going to derive a radial average fuel enthalpy at failure as a function of rod average burnup. There are other ways that it could be done, but this one is much more consistent with the approach where coming out of the 3-D neutronics calculation is generally a radial average fuel enthalpy, and so if we provide a threshold for which they can compare this coming out of the 3-D neutronics, that -- or the neutronics calculations, not necessarily 3-D, neutronics calculations, that now is a function of burnup. Before it was burnup-independent. So it's consistent with the methodologies that are established out there for licensing.

To address the uncertainties involved in the analysis methodology and the approach in general, we have elected to use a corrosion versus burnup correlation which has some conservatism built in. And that gives us a relationship between the cladding oxidation and the rod average burnup. And since we know the cladding ductility is a function of cladding oxidation, we can now have a ductility versus burnup relationship. And that's illustrated here.

So, in essence, what we've done to develop the fuel rod failure threshold is illustrated on this slide schematically. You've seen a bit about the CSED versus oxide thickness to clad wall thickness ratio. That's the data we have here. I'll show you in just a minute we have oxide thickness versus burnup data. We can combine these two together to give a ductility parameter CSED as a function of burnup now for different material conditions. I've illustrated here schematically for different alloys, potentially. And then given an analytical bases to calculate the fuel enthalpy and the cladding response, we can then determine what fuel enthalpy level is needed to reach this CSED as a function of burnup. And that then derives the threshold that you saw a few minutes ago that Rosa presented.

CHAIRMAN POWERS: Let me come back to the plots that you were doing beforehand. I just glanced through your topical report and I did not find a tabulation of the data you used to prepare those plots of strained energy density versus the ratio. Would it be possible to get those tabulations?

MR. MONTGOMERY: We're working on putting that together.

CHAIRMAN POWERS: I'd appreciate getting a copy of that.

MEMBER FORD: Actually, I've done the same -- I'm trying to follow your argument because you're going back. On this plot here where you plot strain energy density versus oxide, in order to get to that plot and to put on the data points that you have for Rep numbers, you also need the relationships between burnup and enthalpy and strain energy density. Those are all separate algorithms you need to get to how you place those --

MR. MONTGOMERY: Yes. Correct.

MEMBER FORD: -- points on that plot. You haven't shown those, have you?

MR. MONTGOMERY: No, I did not go into details of that.

MEMBER FORD: Okay.

MR. MONTGOMERY: But I'll briefly explain it. We take a fuel transient behavior code, FALCON is the one we use, and we analyzed each one of these experiments, providing as input the power pulse shape, the burnup conditions, so we have to do a steady state analysis up to each burnup. The burnup ranged here from 30,000 to 65,000 depending on which experiment we're looking at here. So we defined the initial conditions of each experiment which brings in the burnup from the post-test examinations, the pre-test examinations as well. All that is brought into initialize the transient analysis. The transient analysis with FALCON is done, and that value of SED that's plotted there comes from that analysis.

MEMBER FORD: But each of those calculations there's got to be a certain amount of uncertainty, uncertainty in terms of the validation of the various codes against data. And is it possible that the reasonable correlation you have there between the data and the theory, or the computation, is luck? Is that all being too cruel?

MR. MONTGOMERY: I would like to not say that it was luck. I haven't gotten into details of the code of the validation base of the code and the numerical bases of the program. The approach that we're using here has been replicated by others. The French, using SCANAIR, have done something similar and the results are very consistent. I'm not showing those, but I can get you that information.

MEMBER FORD: Okay.

MR. MONTGOMERY: So I don't believe there's a large element of luck in here. There may be a small element of luck in here, but I don't believe there's a large element of luck.

MS. YANG: If I can add, I think Robbie there published a paper that shows the comparison between what the code predicted in terms of the deformation, in terms of measured deformation and predicted deformation, and I think that answers your question.

MEMBER FORD: So there is experimental validation for those --

MS. YANG: Yes.

MEMBER FORD: -- algorithms that go into --

MS. YANG: Yes.

MEMBER FORD: -- it and make it that way.

MS. YANG: Yes.

MR. MONTGOMERY: Primarily for the rods that did not fail they have measured post-test examinations for things like cladding strain deformation, radial strain and hoop strain and axial. So they have those types of data that I have not shown which we have --

MS. YANG: Have been published.

MR. MONTGOMERY: Have been published and the code comparisons to it are reasonably well.

MEMBER FORD: I'm sorry, also I'm just flipping through your charts. You're going to go into how you're going to use this --

MR. MONTGOMERY: Yes.

MEMBER FORD: -- from this point on. Would you mind going back two more plots to the one that you have the "night sky." The reason I call it "night sky" from the cracking world we have a lot of "night sky" plots look like this. The presumption here is that there is a unique relationship between crack strain energy -- or critical strain energy density and oxide cladding thickness and that there's just one relationship, that's that line. But in fact there's got to be more than just a single parameter relationship.

MR. MONTGOMERY: Well, we know the temperature for sure.

MEMBER FORD: The temperature and the

strain rate. Even though you say strain rate is not a big thing, it will be. Physically, it must be an input to the model.

MR. MONTGOMERY: In looking at this data under a variety of strain rates, we didn't find a strong strain rate dependency. Now, we have included in this a strain rate dependency, so there is a -- the biaxiality factor that we used to relate the axial and ring tension has a strain rate effect. So we have that. There is some inherent strain rate built in.

MEMBER FORD: I guess the reason I'm bringing it up is we see a lot of plots like this out in literature and the correlation factors must be very low on that blue line. And yet it's the basis for all of your subsequent analysis and the use of that analysis, and it just makes me feel uncomfortable that we have no way of knowing how to normalize or collapse that to correct, if you like, those data points even though there are experimental errors on each data point, how you correct those data points to move it down towards that blue line if that blue line is correct.

MR. MONTGOMERY: Well, the only thing that we have done, as I said, we have gone through and looked at this various looking at the data to try to bound it, to try understand the uncertainty and impact of uncertainty. So we've looked at this. We see in this slide where that -- how that uncertainty could influence at least the validation process.

MEMBER FORD: Okay.

MR. MONTGOMERY: And then, as I'll go into later on, in the application, we've also looked at this uncertainty variation on the result of the application and we come up with a threshold and how big of an impact this variability would be on the threshold that's derived in application of the methodology. So we recognize that there is clearly scatter inherent in that data that adds some uncertainty into the process that we're implementing. And we tried to address it through this evaluation. And I'll talk at the end and show that at low burnup where the oxide thickness is lowest and you see the biggest impact, the effect is there but it's not that large. It can be on the order of ten calories per gram or so, but here in the area where these all tend to converge because the data is getting tighter together the impact is much smaller.

MEMBER FORD: Okay.

MR. MONTGOMERY: Okay. Let's see, where was I now? We're talking about how we use this methodology, combined with the data, to come up with the threshold value. Let's see, so a part of this process is the requirement of an oxide thickness versus burnup relationship. So we've collected several thousand poolside examination measurements on oxide thickness and looked at the data and there's clearly a trend in the data that as the burnup increases the oxide is increased. Now, there's a lot built into that, there's duty effects, the temperature of the plant effects, many things other than burnup, but we've boiled it down to burnup for this application.

And in looking at the scatter and the variability in the oxide thickness versus burnup, we elected to take a very conservative approach and just take a trending line that mirrors, to some degree, the relationship of burnup versus -- oxide versus burnup so that we can bound some of these higher points and then prescribe a limit of 150 microns to preclude the possibility of oxide spallation. We know that above 100 microns the propensity for oxide spallation tends to increase because of the internal stress effects and other effects that influence the spallation process.

So in our application of the methodology, we're applying this very conservative oxide thickness versus burnup curve. It's anticipated strongly with advanced alloy materials for the cladding, as I said, designed to go to high burnup that you'll fall well below that curve. So you'll be in this -- well below the curve and the envelope of operation down in here.

So here's the bottom line. I'm sure you're going to have lots of questions of how I got there. But, essentially, the result of all this process is a radial average peak enthalpy that is essentially 170 calories per gram out to a burnup level and then becomes a function of burnup after that. So from about 36,000 on it's now a function of burnup. Below, it's burnup-independent. The 170 calorie per gram limit comes from the DNB failure process. Experimental data from tests show that below 170 calories per gram the cladding temperatures do not exceed that necessary to induce high temperature failure processes. So the failure would only occur above this line and appears where you get to the very high temperatures needed to fail the cladding.

PCMI, because of changes in the ductility function that we've used, combined with the gap closure effects, begins dominant after 36,000 and then begins to saturate out as you reach the 100 micron level.

MEMBER ROSEN: This is excellent, because what this is, as a utility guy, I can run to 100 gigawatt days per metric ton because it saturates out.

CHAIRMAN POWERS: No. It seems to me that there's some flaw here that he comes up and he says, all right, at 40 gigawatt days per ton I don't want the material to spall and I know that oxides do get spalling, so I'm going to cap my correlation. Then he calculates this curve. His curve should come up to 40 gigawatt days per ton and then stop. He should say you have to stop at 40 gigawatt days because there's the potential of spalling and you switch to a different curve then.

MR. MONTGOMERY: We're saying that the oxide is below this level, and we are going to draw at envelope at which you're below. We're not saying that because --

CHAIRMAN POWERS: Starting at 40 gigawatt days, that philosophy disappeared.

MR. MONTGOMERY: That becomes the envelope. As long as you're below 100 microns --

CHAIRMAN POWERS: You now switch to a different criterion. As soon as you cross 40 gigawatt days per ton, you're saying, "Oh, yes, but in addition to this, you have to stay below 100 microns."

MEMBER ROSEN: But that below 100 microns, and we have reasonable assurance of that. That's pedal to the metal all the way up to however many gigawatt days per ton I want, right?

CHAIRMAN POWERS: Ten to the sixth, as a matter of fact.

MR. MONTGOMERY: It's a straight line after this.

CHAIRMAN POWERS: It's a straight line, not because of what the fuel is doing, but because of his capping the outside parameters.

MR. MONTGOMERY: Well, inherent in this there's a burnup effect coming from the fuel pellet, but the cladding ductility saturates and that's the reason that the PCMI loading still remains the same. And it's fairly asymptotic, yes.

MEMBER ROSEN: This is crucial. I mean what this work is saying is that as long as you keep oxide below 100 microns, you can go to practically anywhere it's willing to support.

CHAIRMAN POWERS: As long as there's no change in the physics, which is not demonstrated here.

MR. MONTGOMERY: Well, that's the next slide. I'm trying to demonstrate that through the experimental database. We have, again, the experimental database for the conditions for which this curve's applicable, which is 300 C, 280 C or above, we only have these data points that have not failed -- or that are not spalled, okay? None of these had spalled oxide. They had oxides up to 100 micron but they did not have spalling.

We have tests out to 63, 64,000 that are very near our curve and did not fail. We have this one that's above our curve that did not fail. And then we have this one that's well above our curve, and this one is a bit of -- I don't want to call it anomaly, but in a sodium reactor you're not going to post-DNB heat transfer conditions, so you don't really -- can't really say that that's -- that the failure could be moved that high, it's just that PCMI is not active at that level of enthalpy to cause failure.

MEMBER ROSEN: So here in this curve you've actually -- you've drawn out to 90 gigawatt days per ton.

MR. MONTGOMERY: Well, yes. Just note -- now, be careful here.

(Chatter.)

MR. MONTGOMERY: Be careful. Let me point out something. I was obscuring it in my standing of the -- where I was standing. Since heat is short segments over 50 centimeters or so, they represent a peak burnup per se, so this curve has been moved from a rod average burnup to a peak burnup, and that's where there's a shift to a higher burnup. So this would be the largest -- the peak burnup, the peak nodal burnup. For a rod at 75 average would be about 85, 86, 87 type number. Eighty-eight is what's plotted here. That depends on the peaking factors of the plant, the axial power shape. So that's the difference between the two curves. This one is on rod average basis, and this one is on rod peak basis.

MEMBER FORD: Just to make sure, Rep-5, 11 and 4 are no failure?

MR. MONTGOMERY: All of these have no failure. And I should point out, just for clarification, is that RepNa-11 is a UO2 rod but it has the M5 cladding, it's got the more advanced cladding, so it's oxide is really low, like 30 microns.

MEMBER FORD: But in Rep-2 not failed?

MR. MONTGOMERY: It did not fail because at this low of burnup the oxide is rather low and the ductility cladding is sufficient to accommodate the loading from the pellet. It was tested in sodium so you don't get the high temperature mechanisms of oxidation-induced embrittlement that would occur if you were to test this same type of test in water. So that's why it did not fail. Okay.

All right. So this is what I have to say about the failure threshold criterion that has been established. I will now -- unless there's some questions about this, I will move into the coolability discussion and talk about core coolability.

MEMBER FORD: Just to make sure I understand, if you had oxide scoring, then at around about 50 what you'd see is that you'd have a discontinuous curve and it should just drop down to a value.

MR. MONTGOMERY: Yes. The spalling curve would be down here.

MEMBER FORD: Down here, and it would presumably loop up to join that main curve.

MR. MONTGOMERY: Yes, loop up here. Because when the spallation process kicks in, it's a fairly -- there's a step almost change between the ductility between spalled and non-spalled.

MEMBER ROSEN: So one way to supercondense this discussion for us laymen is to say the transition to advanced cladding materials is done to make sure that you don't get thick oxide layers, so that you don't have a potential for spalling, so that you don't get hydride mobility which can lead to low ductility.

MS. SIEBER: And that protects you against prompt pulses which you'll never get.

MEMBER ROSEN: That's right. All of that work is to protect you against something you'll never get. But if you did, if you could imagine it, you would be okay anyway.

MS. SIEBER: You could do it but you've got to put a tunnel in there to get it in there.

MEMBER ROSEN: All you got to do is just ten percent more and you get the 100 megawatt days per ton, which is where --

MS. SIEBER: Just bigger paper. Once you draw beyond the data, it becomes a matter of how embarrassed you are.

(Laughter.)

MEMBER ROSEN: And for those of us who are never embarrassed about anything?

MR. MONTGOMERY: Now we have a couple of pieces of data that are going to come in in this range right here, right, Rosa, for this step 1 and step 2 test. On M5 cladding, they'll come in on this range in the next coming months.

Up to now we've been talking about failure and the threshold required to define when it's necessary to start counting for radiological releases to meet the dose requirements. So the next subject that I'll move to now is the coolability concern, which really represents the safety limit with regards to maintaining the core geometry.

The database is a bit smaller in that regard than for the failure database. The past experiments in the U.S. and Japan early on focused on enthalpy generally above 280 calories per gram. Their primary objective was to look at molten fuel, dispersal kinetics and the mechanical energy generation from fuel coolant interactions. They really wanted to see what was happening at very high energies to understand the real safety consequences.

Recent experiments we've had in France and Japan generally have been below the 220 calorie per gram limit. You saw one point that I had from CABRI that was about 215, and we have a couple from NSRR that are on the order of 210 or so. And some of these cases and for those that experience failure, some of them have dispersed a small amount of finely fragmented solid materials generally coming from the pellet periphery.

And in some of these cases, there is a

measurable amount of mechanical energy generation, and maybe I should just talk briefly about what I mean about that. Particularly in Japan, they use a stagnant water system where the fuel segment, again a six-inch segment, sits in a canister with a -- in a pool of water. And at the top of the water they generally put a float device, and the float device has a magnetic sensor system on it so when the float bumps up and down they can measure the velocity and how far that float moves up and down. And what we mean then by mechanical energy generation is that in the process of conducting a test if they measure that float moving with some significant velocity and have some upper movement and the height that it moved to, they can then determine from the energy, mechanical energy generation from that process. So that's what I mean by mechanical energy generation.

The fuel dispersal is an issue. It occurs generally at burnups greater than 40,000 due to the rim effect. The increase in local burnup and fission density in the outer rim influences the temperature and the local effects that go on in this area and when the cladding fails can promote some material to be dispersed from the fuel rod through the cladding out to the coolant.

Some of the issues that are raised as a consequence of fuel dispersal is if you can get significant amount of material out, could it result in loss of low blockage or loss of raw geometry such that you can't maintain cooling? These are geometrical type effects. And then more of pressure vessel integrity issue is that you could get pressure pulse generation that could effect either the core geometry, again from a cooling point of view, or the vessel integrity itself. So this is something we need to look into. And so we've looked at the data and what we see is that the potential for fuel dispersal is a function of how much energy is deposited after the cladding has failed and also the pulse width.

So we've come up with a slide here that shows the data on high energy tests that have had cladding failure and post-failure energy deposition. So we have along the Y-axis here is the energy deposition after the cladding has failed, and plotted along the X-axis here is the pulse width. And you see that for most of these tests that were tested below ten milliseconds there is some fuel dispersal that occurs, and that's separated by the points on this side of the dash line all had some sort of fuel material -- solid fuel material dispersal, a few grams, generally, or less.

And then the tests on this side of the line, although they failed, had developed a crack in the cladding, none of the fuel was released from the -- cut from cladding and into the coolant. So that there is some -- you can see that there's some effect of pulse width and then effect of energy deposition after failure.

This very busy schematic illustrates the processes that are controlled by pulse width that can influence the dispersal process. Here in this illustration is the narrow pulse and in a narrow pulse, as I showed earlier, we get these temperature distributions where the peak temperature occurs in the outer pellet periphery region. As a consequence of the rapid energy deposition rate, the heat transfer conditions are slower so you don't have as much heat transfer. So you generally end up with higher temperatures in that region and stepper gradients in that region.

Combine that with the fission gas distribution and content and the pellet in that region, you can end up then with higher gas pressures and higher thermal stresses as a consequence of these gradients, and end up with the fuel tending to fragment apart and what we call grain boundary decohesion, resulting in fission gas release and also now that the fuel is fragmented a bit, it has the potential to be dispersed.

And this micrograph is a micrograph from RepNa-5. It's a sermography here. This is the fuel pellet, this would be towards the center of the pellet, and this is the pellet periphery. The cladding would be just over here. It's kind of hard to see, but there's kind of a gap right here. But what we see is that you see individual grain boundaries that are decorated, and you see there's a crack here, there's a number of cracks here and here too. And you can see that the grain boundaries are very evident, and that indicates that the grain boundaries have more than likely separated off and the fuel is almost cracked up into approximately grain-size segments or bigger, on the order of ten to 20 microns.

MS. SIEBER: There's a marked difference in density in the right third of that micrograph.

MR. MONTGOMERY: Here versus over here?

MS. SIEBER: Yes.

MR. MONTGOMERY: Yes. This is the rim region.

MS. SIEBER: It looks like a straight line. Could you tell me what that is?

MR. MONTGOMERY: Right here?

MS. SIEBER: No, over to the left.

MR. MONTGOMERY: Right here?

MS. SIEBER: Right there.

MR. MONTGOMERY: That's an artifact of the etching more than likely. This is the rim region where the grain size has decreased some, and when the etching is done to generate this micrograph usually that region where the rim is comes out stronger, showing a stronger etching. So this is where the rim generally is, and you get a finer grain density in that area. And it's a pretty sharp transition between the two. It could be a photograph artifact as well.

On the other hand, if we have a wider pulse, generally on the order of 20 milliseconds or greater, there's time for a heat transfer so the temperatures tend not to go quite as high, the gradients are smaller. These combine together to have less of effect on the local gas pressure and the bubbles and on the grain boundaries, and limits the cracking fragmentation and the possibility of fuel dispersal. And you can see here that these -- again, this is RepNa-4, had a wider pulse, and we don't see quite the level of cracking and grain boundary separation in this micrograph. Again, this is the pellet surface region and that's going towards the center. We don't see quite the level of grain fragmentation.

MEMBER ROSEN: And, again, that's the artifact that Jack was talking about in RepNa-4, the photograph.

MR. MONTGOMERY: I'm trying to see.

MEMBER ROSEN: One side's very light and one side's very dark.

MR. MONTGOMERY: These could be two photographs here. Yes, that's a montage. Although this is really not a montage here. It looks much better on my screen.

So back to this prototypical pulse width, for prototypical pulse widths no fuel dispersal is expected. However, at high energy after failure, it's possible that a small amount of non-molten pellet material may be dispersed, but it's impact is low. We have experimental data to support that. In a test at NSRR approximately ten percent of the pellet was released, and in that case the fuel rod maintained the geometry.

I have a little slide I added that I

wanted to show you just to give a feel for what I'm talking about. I don't have it in a handout, I'll be happy to give it to you if you want it. On the top here is a rod that was tested up over 200 calories per gram, it developed an axial crack, you can see here, and then in further post-test examinations you can see the crack in the cladding. Here's the fuel pellet. It was pre-irradiated to about 30-something thousand, 30, 35,000, and you can see that there's some material lost right in the vicinity of where the crack is that some material has been released out. You can see a little bit of loss in this region here as a consequence of the test. And this test lost about ten percent of the fuel material was -- left the cladding and was found in the coolant. But the rod still looks like a fuel rod, and it's still maintaining a geometry that is coolable and contains a majority of the fuel material.

This is just a picture, I know you can't see this very well, but that's just a picture of the material that was found outside the fuel rod. You see small pieces. The key point here is that none of it looks molten. This test was done almost to the melting temperature, and it clearly did not reach that, and the material that left the fuel rod was not molten. The difference would be is if this was molten material, it would look like a bunch of BBs, pellets, you know, round balls almost.

Okay. Generally, what we see is that for any fuel coolant interaction that results in pressure pulses is that the tests exhibit rather low mechanical energy conversion primarily because the material temperature is low and molten material, so it has less stored energy. And the heat transfer kinetics aren't as energetic. Secondly is that there's a limited amount of material, generally just a small amount of rim material is what's been released.

So to establish the coolability limit we elected to use an enthalpy limit that would preclude incipient melting so that if in the off chance some material is dispersed it would not be molten. As I said, the data show that dispersal molten material generally produces higher thermal-to-mechanical energy conversion ratios. I'll show that in the next slide. The test that I just showed you, this JMH-5 which was tested up at 200 calories per gram, showed no adverse impact on rod geometry. Even though it dispersed a small amount of material, it maintained a rod-like geometry. And that there would be no impact on the pressure vessel because the pressure pulse -- the mechanical energy generation would be low.

Using fuel incipient melting as a precursor for the coolability limit is very conservative in the sense that we really are limiting most of the fuel to well below the melting temperature. If we define the peak temperature here to be below the melting temperature, a majority of the fuel is well below that because of the peaking effects at high burnup fuel. It also limits such that the cladding does not reach melting, so we maintain rod geometry in that fashion as well. And, finally, it limits the thermal-to-mechanical energy conversion.

And that's shown here where we're looking at -- this is a subset of the data done from the early Japanese and CDC SPERT tests where they tested the fuel up in the molten area. So they're all tests done about 320 calories per gram or higher. And you can see that I'm plotting here mechanical energy conversation ratio versus the particle size when they look at the particles after the test. And we see that the data shows a dependency on the particle size and can get up to one percent energy conversion when the material is molten.

If we go to non-molten material tests, that's these two, and this is the real important test because these are powder tests, these are done with powder, special tests done to simulate powder being dispersed, we see that the conversion ratio is about an order of magnitude less and the total energies that are generated are even larger than that between the two, if you look at the energy generated in this versus the energy generated in that. So there's quite a bit of difference between non-molten and molten material. And the dependency on particle size is much less. This generally has about a square root dependency on particle size, and this has about a linear dependence on particle size.

So in order to establish a limit on the enthalpy to preclude incipient melting, we need to determine what enthalpy would be necessary to reach the melting temperature. So to do that, we did an analysis again where we combine data on the UO2 melting temperature as a function of burnup, combine that with the radial burnup and power distribution information that we know to give us the local burnup and the local temperature, melting temperature. This gives us the local burnup and the local melting temperature, and then through an analysis, using a pulse width of 20 milliseconds, we calculated what the enthalpy would need to be to reach the melting temperature locally in the fuel and then define that as the maximum enthalpy level as a function of burnup. And we did this through the burnup range.

And the answer is shown here where we have, again, maximum radial average fuel enthalpy versus rod average burnup. This is the result of the analysis. A limit was placed on -- this curve actually goes up to about 250 or so, but I went ahead and just capped it at 230 because that's kind of where the licensing base of today is anyway. And what we see is I plotted out some of the data here from the zero burnup tests that have been done where we've had some maintained rod geometry here, we have clad melting in this range, partial clad melting, and then total loss of rod geometry, as indicated by these symbols. And then I've overlaid the few tests that are in this energy range of interest where they've been tested up to about 200 calories per gram or so. That's where this database is. And all those maintained rod geometry. So that's the data compared to the limit line.

So I'm getting close to being finished up here. You saw this curve before, Rosa showed it. What we have here is the failure threshold as a function of burnup and the core coolability limit as a function of burnup. They incorporate the effects of burnup through the material properties and melting temperature.

To summarize, we've proposed acceptance criteria, fuel rod failure threshold and the core coolability limit, that as a function of burnup we think that these acceptance criteria include the key controlling parameters, that is the corrosion and hydriding evolution with burnup that affects the material ductility and failure and the burnup impact on UO2 melting. These criteria are given in terms of radially averaged peak fuel enthalpy. This is consistent with the current reload design methods where the neutronics calculations generally calculate this parameter as one of their outputs. Currently, it's applicable to hot-zero power RIA events. At this point in time, we feel that DNB remains the limit, the appropriate failure criterion for at-power rod ejection events.

The failure threshold is based on integral tests from RIA simulations, mechanical property tests and analytical methods. It's certainly based on the corrosion kinetics that we used. It certainly represents a lower bound for modern, low corrosion cladding. And, as I said, it tends to bound the data for the non-spalled Zirc-4 rods.

For coolability limit, we don't expect any fuel dispersal to occur during LWR conditions, but if there is, we've put a limit on the peak fuel temperature or the enthalpy -- put a limit on the enthalpy to preclude incipient fuel melting. This is now a function of burnup. And it is supported by data from the database that we have on both loss of rod geometry, mechanical energy release. We feel this is conservative and in general we get much less than ten percent of the fuel material that's going to come out. And then there's a large margin between peak burnup that we assume in this calculation and generally the location of the peak energy deposition, and that's given in the next slide.

This is the result -- superimposing a burnup distribution from a high burnup rod, you can see that the burnup is about 55,000. And superimposed on that this is burnup on this axis versus axial position. And we have superimposed on that the axial power shape during a rod ejection event, and in a rod ejection event the axial power shape is very much peaked in the top of the core because of the characteristics of the event. And we see that for this case the axial peak power, which we assume in our analysis, this would be the radial average peak.

(Whereupon, the proceedings went off the record at 12:31 p.m. and resumed at 1:32 p.m.)

CHAIRMAN POWERS: Let's come back into session. Undine, you are going to tell us about what you are going to do about all this good stuff we have heard about, right?

MS. SHOOP: Absolutely. You are going to be dazzled and impressed. Okay. I would like for the second part of this presentation --

CHAIRMAN POWERS: We are always dazzling and impressive.

MS. SHOOP: I won't go there. I would like to talk to you how we are actually proposing what kind of plan we have come up with, a preliminary plan actually, to review this topical report.

The purpose of generating a plan to begin with is that we can focus our resources to appropriately provide the detailed review, and identify all the elements up front so that we make sure that we are not missing anything, and that we have a complete review and that there are no surprises.

This is a team effort. There is myself, Shi-Lang Wu, and Ed Kendrick on the NRR team; and then we are also working with the Office of Research, and also Carl Beyer from PNNL, our contractor, provides support for this.

The elements of the review plan currently include data verification. As you have seen, there is a lot of databases, and there is a lot of databases from a lot of different tests.

And what we need to do is make sure that all of the data is applied in a manner consistent with the way that it was generated. It is applied and there is a correct application of the methodology, and any time that you get more than one task, you always have uncertainties.

So we need to make sure that all of the data is in line.

CHAIRMAN POWERS: Do you mean to tell me that with one test that we have no uncertainty?

MS. SHOOP: You can talk to Rosa about that.

MEMBER ROSEN: With two points, you have a straight line, and with one point, you have the answer.

CHAIRMAN POWERS: You know, I never thought of that. You may have established a new principle of science there. Don't expect Stockholm to call too soon though.

MS. SHOOP: Okay. So you know that statistics is always our favorite thing, and so we are going to look at that as well. In the SED/CSED theory and model, we need to investigate, and come to terms, and verify ourselves that the SED/CSED model is an equivalent of Rice's J/Jc formulation, which was the inter-role of the strain.

And then we are going to code the SED/CSED formulation into the NRC FRAPTRAN code. That way we can do an independent verification of the analysis results that EPRI has presented.

In the fuel rod failure thresholds, we are going to have to validate the application, and we are also going to have to review it for applicability to the current future and proposed fuel types just to make sure that everything is bounding.

In the core pool ability limit, we have to do application verification. As we have seen today, there is some limited data, and then some of it is from analytical methods.

And we need to make sure that that is rigorously addressed and appropriate. The FALCON code. EPRI uses the FALCON code in the development of this methodology, and that is a code that the staff has not seen, nor have we ever reviewed it.

And EPRI has graciously agreed to provide us with a copy of the code. That way we can look at it and review it. The data dispersal --

CHAIRMAN POWERS: Will they be giving you things like users manuals, and models and correlations, and things like that?

MS. SHOOP: Yes. They are exceptional gracious. They are providing us training with the code, and they are providing all of the V&D, users manual, and the theory manual, as well as the source code.

CHAIRMAN POWERS: Are you going to share it with us?

MS. SHOOP: What are you guys going to run it on?

CHAIRMAN POWERS: What do you mean? I have access to a computer with 3,000 processors, 1 gigabit, 1 gigahertz each node. Is that enough?

MEMBER ROSEN: That ought to be enough.

MS. SHOOP: I think I am going to log into that machine. In the area of field dispersal, where you have to review the data for applicable at each of the phenomena of the proposed safety limits, there again do the validation and verification.

For the uncertainty and conservatism, you know, we always have to look at the uncertainty, and we have to verify that the conservatism is appropriate and bounding. But for the limitations of the criteria, you have to review the data for where it is applicable, and make sure it is applicable for the full range that we anticipate it being used for.

And then we also have our safety evaluation conditions of acceptability. We always have those. And what type of fuels are applicable to, and is there any sort of core design limitations, or anything like that. We will of course always look into that.

This is also going to entail revising the Reg Guide, Reg Guide 1.77, and also there is three SRPs that all reference this limit. And they will all have to be verified.

So of course we will be coming back to see your smiling faces to show you the reg guides and the SRPs, and get your weigh in after all of this is all done. Okay.

CHAIRMAN POWERS: This is the highlight of your schedule, right?

MS. SHOOP: Yes.

CHAIRMAN POWERS: That's good.

MS. SHOOP: Yes. Coming down to see you guys is always a highlight. Since this is a team effort, we will as I alluded to before, we will be asking the Office of Research for some assistance.

The Office of Research is very familiar with the data, and with the testing mechanisms, and so we will definitely need their assistance with verifying that the application methodology is applied, and all data is used consistently.

The Office of Research also has a contract for the FRAPTRAN computer code. So incorporating the CSED/SED model into the FRAPTRAN code will entail getting their assistance in that respect.

And I should actually back up, because DPRI is looking a little worried. That will be a proprietary version of the FRAPTRAN code and that will not be a publicly available one.

Fuel dispersal. We are going to also ask for their assistance with the applicability of the data to the proposal for the fuel dispersal mechanisms. And my last slide.

Our offices, since this is a preliminary review plan, we are planning on coming up with an office agreed upon final review plan, and we anticipate having that by December of this year. Do you have any questions on our review elements or our proposal? Rosa has a question.

MS. YANG: May I ask just for clarification, because we were given a schedule of the review, and does this bullet mean that you may revise that schedule?

MS. SHOOP: The schedule may actually -- you know, there were some interim dates in that schedule, and they may move as -- you know, some of those we discussed with the Office of Research, and then some of them we have since gotten input.

So we need to dialogue between our offices and see if any of those interim bullets need to move.

MEMBER FORD: This is saying that you must have already done many of these tasks.

MS. SHOOP: No, this is just saying that we are going to come up with a final plan on how we are going to review this topical by December.

MEMBER ROSEN: No. That's not the finish date.

MS. SHOOP: We have started to review, but that basically will lay out the elements of the review.

MEMBER FORD: That would be wonderful if we could have it by the end of the year.

MS. SHOOP: And it will be in a laid out plan that both offices agree to.

MEMBER FORD: So when will the final review be done?

CHAIRMAN POWERS: Whenever the -- it says on December 31st that they will answer that question. Okay. Well, thank you, Undine.

MEMBER FORD: I am putting my mouth in EPRI's foot, or my foot in EPRI's mouth, but I would imagine that they would want this to be done fairly quickly. Is there any way of pushing it up, or is it not high on the prioritization, or what?

MS. SHOOP: There are a number of components, and yes, and any licensee who comes in here with a licensing application wants it done quickly. That is just a given. With this particular plan, this is one of a series of different topicals that will have to be submitted to support high burn-up.

And high burn-up is of interest to the agency, and it is important to the agency, and therefore we need to make sure that we take the appropriate time and resources to do a thorough review to have all of our ducks in a row to approve it.

MEMBER FORD: This also means that you

will be doing therefore on the high burn up fuel, which is contrary to the message that we were getting before.

MS. SHOOP: We will be doing work in the areas of reviewing what the industry has provided.

MEMBER FORD: Yes.

MS. SHOOP: Keeping abreast ourselves of what is going on in the international community, and to see how that all relates. However, we are not as I said in the Agency's 1998 plan, we have said that the onus of coming up with the criteria database and the methodology will be the industry's responsibility.

MEMBER ROSEN: And they have done it.

MS. SHOOP: Yes.

CHAIRMAN POWERS: Okay. Thank you. Well, now we are going to switch gears a little bit and move to the question of the RES program. And I guess we are going to start with Jack, who is going to give us --

MR. ROSENTHAL: My name is Jack Rosenthal for the record again. I just wanted to say that this is a very good time when we welcome coming here, and we are trying to generate test data relative to LOCA, and Argonne.

We have finished some Surry creep data

that is important in the waste arena, again with some data, and so after many years of promising, we are finally seeing some results. So it is a very good time to brief you on where we stand.

And Ralph Meyer will go over what the promises were from 1998, in terms of the plan, and what we have accomplished, and where we are. And then you will hear more detailed presentations mostly on our experimental program.

CHAIRMAN POWERS: Ralph, I'm sure that you are going to say this, because I have looked at your slides, but I want to reiterate that to this subcommittee that we have looked in great depth at the reactivity insertion accident aspect of high burn-up fuel.

There are many other aspects of high burn-up fuel impacting issues of safety, and I am sure that Ralph will touch upon at least some fraction of those. Ralph.

MR. MEYER: Yes. Actually, we have four hours of presentation prepared, and we will shorten it up, and quit before the sun goes down or something. Anyway --

CHAIRMAN POWERS: Who imposed this sundown criterion? This Committee is used to being here until

7:30 or 8:00 o'clock at night.

MEMBER ROSEN: It is normal ACRS practice to say when someone who says they have four hours to give them 40 minutes.

CHAIRMAN POWERS: Go ahead, Ralph.

MR. MEYER: Okay. We are in fact working on a revised, or you could think of it as a new high burn-up program plan that would cut across the offices.

CHAIRMAN POWERS: So we have heard.

MR. MEYER: In addition to the plan for a review of a particular licensing topical report, there is a broader update in progress, but we are not finished with that.

So what I thought I would do would be to roll back to the 1998 plan, and tell you where we are on the issues that were identified in that plan, because the new plan will obviously pick up and go forward in some manner on these or other issues.

So here is the original list of issues, and just to identify, there were nine of them; cladding integrity, control rod insertion problems, reactivity accidents, which we have talked about all morning; loss of coolant accidents, the power oscillations in BWR associated with an anticipated transient without SCAM.

Our computer codes for fuel rod behavior, and neutron kinetics; a source term for high burn up fuel, transportation and dry storage issues related to high burn-up fuel, and high enrichments.

Now, in 1998, we said that Issues 1, 2, and 9 were essentially either resolved or we didn't need to talk about, and so I am not going to talk about them today. I am going to concentrate 3 through 8 --

MEMBER ROSEN: It is a good thing you have four hours, because let's talk about some of those. And I want to talk about nine in the context of the advanced reactor research plan that we are working on here.

If we are really serious about -- if the agency is serious about writing research, an advance reactor research plan that considers the introduction of fast reactors, either gas cooled or liquid metal cooled, or in any, you are going to need enrichments greater than five percent.

MR. MEYER: Yes.

MEMBER ROSEN: So there is some sort of something going on here and I don't know what the -- I am just rolling out the rope here.

MR. MEYER: We have work in place to look at advanced reactor fuels. We have an advanced reactor research plan that has been developed that includes both fuels, and therefore, would include higher enrichments.

But in the context of high burn-up fuel, the industry has decided that it would like to make additional steps in increasing burn-up, but that they would not need to go beyond 5 percent enrichment in current light water reactors in order to do that.

So in terms of a program plan that is looking at high burn-up fuel in current reactors, it is pretty much off the table for us.

MR. MONTGOMERY: We did provide an advanced reactor research plan. You know a draft plan to the ACRS, like I think two days ago. So you ought to find it appearing in the in-boxes shortly.

And the big thing was to add ESBWR and ACR 700 to that plan, and they are not high enrichment. The ESBWR, for example, uses modern boiler fuel. IRIS is out in the distant future, and at one time we thought that that would involve high enrichment fuel when they were talking about multi-year cycles.

As of Thursday, last Thursday, at a presentation that they made here at the NRC, they indicated that at least for now that they did not plan to go above the five percent enrichment value. So that is where we stand right now.

MEMBER ROSEN: Well, we will duly note that, and the advanced reactor research plan comments that this committee will offer some time. It is pretty clear that you can't get there from here for that comprehensive list of things that are apt to be in the plan, or apt to be on the table.

At least they are in the Gen IV list, and in the international and near-term deployment list. They may not be in the domestic near-term deployment though. There are enough concepts in those lists that will require enrichments beyond five percent and that somebody in the agency ought to be thinking about, rather than just dismissing it out of hand.

I understand that in this case that you are dismissing it out of hand because this is a plan for the current light water reactors.

MR. MEYER: Yes.

MEMBER ROSEN: And I agree that nobody is talking about greater than five there.

MR. MEYER: Shall I go on?

MEMBER ROSEN: Yes.

CHAIRMAN POWERS: Please.

MR. MEYER: Okay. Now I want to talk about several of the issues, including the reactivity initiated accidents. I plan to do this by way of an introductory presentation and then a revisiting of the issues in the subsequent presentations, and to go into a little more detail about work that we have actually done.

So it is somewhat of an artificial split, and there is likely to be some interest in jumping into the second presentations right now, and we can do that if you want to, or not do that.

CHAIRMAN POWERS: The subcommittees are controllable, but we will try and kind of constrain ourselves and get a quick overview, and then delve into the details.

MR. MEYER: Okay.

CHAIRMAN POWERS: So just feel free to say stop, and I'll tell you about this later.

MR. MEYER: Okay. Well, the issue was described well this morning, and it has to do with a regulatory guide number that we don't believe applies to high burn-ups. I am going to show you in a diagram on the next slide the method that we are going to use, or the methods that we are going to use to address this.

But before even doing that, I want to point out the schedule that we are working on. As Rosa mentioned, there are 2, or perhaps 3, CABRI tests in the sodium loop coming up in September. I'm sorry, in October, November, and perhaps a follow-up test early next year. It's not clear.

These are tests on ZIRLO and M5, and at the Argonne National Laboratory, we will be completing a series of mechanical properties test next year on high burn-up Zircaloy-4.

And there is a test in Japan that we are looking forward to, to try and get a handle on the temperature effects. You saw this morning that the Japanese tests were run at approximately 25 degrees centigrade, which is not the right temperature for the accident that we are thinking about.

And the Japanese have or are constructing a high temperature-high pressure capsule, which they expect to start testing in in 2004. And so our plan for providing a confirmatory assessment for Zircaloy clad fuel at 62 gigawatt days per ton is to wait for these tests, and give ourselves two years to get it all together, and in early 2005 come out with an assessment document that gives a story on why everything is okay with regard to reactivity accidents for the current zircaloy fuel in operating reactors up to the burn-ups that are licensed at this time.

CHAIRMAN POWERS: And this again is just reactivity accidents here?

MR. MEYER: That's right. I have different schedules for different things. But we are now down to the point where we are talking about fairly finite periods of time and definite schedules, and definite activities.

CHAIRMAN POWERS: Now, there are a series of CABRI tests scheduled to begin somewhat after or in late 2005, I think?

MR. MEYER: Yes.

CHAIRMAN POWERS: And so how do they figure in? Are they confirmatory of confirmatory?

MR. MEYER: Yes. In fact, that is the way that we are looking at them. The program has been delayed and they water loop tests themselves don't get underway until late 2005 or 2006. So I think we and EPRI have pretty much decided that we want to make our decisions without waiting for that, and hope that everything pans out according to those confirmatory tests.

It took too long to hold things up for that, and I think we are learning enough that we can go ahead and get much of the job done before then. This is the same bunch of data that you saw before, plotted in a different way.

This our so-called paint brush slide, and you won't be surprised to learn that we have a somewhat different view of the data and the implications of the data than EPRI has.

So the picture that I am going to describe is a little different than you heard this morning. The first thing to notice is that we have plotted this as a function of oxide thickness rather than as a function of burn-up.

Obviously, the enthalph increase that a fuel rod can withstand before the cladding breaks is a function of several variables. You have talked about them. They re temperature, and rates which are related to pulse widths, oxidation, hydride, and burn-up.

I think the oxide thickness has a stronger effect than the burn-up has, because it directly affects the cladding properties. And so we have chosen to look at the data as a function of oxide thickness, which does not have burn-up directly associated with it.

So in that sense there is no limit out

here at the end in burn-up. This brings the scatter down a little bit, but clearly doesn't remove the scatter.

Now, there are certain bodies of data in here whose personalities we know a little bit about.

The Japanese data points probably should be shifted upwards because the test temperatures were too low. These CABRI data points should have probably shifted downward because the pulse whip was too large.

And we will talk about this in the second presentation in a little more detail. What I want to say about this slide right now is that at the low corrosion end of the plot, which is the low burn-up end of things, the original correlation did indeed have a relation to incipient melting.

The enthalpy for melting UO2 is 267 calories per gram, and if you do that on a radial average, 230 calories per gram, is about where you start melting fuel somewhere inside the rod.

There is a large volume increase going from solid UO2 to liquid UO2, and this provides a mechanism for breaking the cladding and expelling fuel out of the rod which is what you saw or what we saw in the earlier data at the higher enthalpy levels.

Now, 230 would be somewhere in here, and you do see some cladding failures below that point, but you didn't get fuel dispersal in those cases because there was no mechanism for getting the fuel outside of the cladding. The cladding just broke open in some splits.

There is a big difference when you get to the higher corrosion rates which correspond to a higher burn-up, and there is definitely a correlation between burn-up and corrosion and Rosa showed one, or Rani did int heir presentation.

When you get the high burn-ups, and you heard this this morning, but I will just repeat it, you have this gassy grain structure in the fuel pellets. So now when you have a sudden temperature increase from the reactivity insertion, you have a rapid gas expansion, and you have a mechanism built in to disperse fuel if you can crack the cladding and produce some opening in the cladding.

CHAIRMAN POWERS: Let me ask you a question that might be better directed towards one of your subsequent speakers, and if so, I will be glad to wait. But when they do these tests, they cut out a section of irradiated fuel, and they put some fancy things on the end of it, and they may even repressurize it.

But when they cut it, they clearly lose the gases that were in the nominal fuel clad gap, and that was in the plenum. How much of the gas do they lose out of this gassy structure at the perimeter of the fuel that you are talking about?

MR. MEYER: Well, I don't think that they lose any of that gas, because what we are talking about is what we think of as non-released fission gases, which are accumulated in tiny little bubbles that attach themselves ot the grain boundaries.

And in high burn-up, you get so much of that that it actually causes the grain boundaries to subdivide a little bit. So you have got a relatively fine grain material that has got a lot of these gas bubbles on the grain boundary.

And I don't think you lose much or any of that during the refabrication process.

MEMBER ROSEN: Now that or those gas bubbles, micro bubbles, they don't form at the grain boundaries exclusively do they?

MR. MEYER: The fission gases are not soluble in the matrix and so they precipitate into little bubbles, and the bubbles move around. And when the bubbles get to a grain boundary, they share surface area, and it is a lower energy position, and so they stay there.

So what you predominantly see is all this gas gets attached to the grain boundaries. So a large inventory of the fission gases that have been generated end up on the grain boundaries.

There is some still in the grains trying to make their way out, but this is where they accumulate.

DR. SIEBER: And some go to the plenum, correct?

MR. MEYER: Some, not a lot, because the -- well, 50 percent.

DR. SIEBER: So, 3 to 5 inches, and it goes to a pressure increase of about a hundred pounds over a 12 foot --

MR. MEYER: Yes.

MEMBER ROSEN: Tell me again why does the gas form within the grains, and migrate to the grain boundary?

MR. MEYER: It is just a random process.

MEMBER ROSEN: It is a random process?

MR. MEYER: Yes.

CHAIRMAN POWERS: There is in fact -- there is a thermal chemical driving force, two of them. One is the temperature of the radiant, and the other is --

MEMBER ROSEN: It is a random process, and the gas migrates around and when it gets to the grain boundary, it stays there?

CHAIRMAN POWERS: It's not random.

MEMBER ROSEN: Any more.

MR. MEYER: When the first gas atom gets in there, it moves randomly. It meets another one, and they get together, and when you get a double, it is not a random process any longer because the temperature gradient gets involved. This is an old story.

And we see them, and we believe they can have this effect of pushing the fuel out through the cracks in the cladding, because we have seen this kind of dispersal in a number of the tests.

Now, this morning, you heard that typical pulses in a PWR would be 30 milliseconds or bigger, and they showed some data that showed that you only saw this dispersal when the pulse widths were 15 milliseconds or less. Do you remember that slide? Okay.

Every PWR pulse that has an energy high enough to fail the cladding will have a pulse width of 10 milliseconds or thereabouts. They will not be broad.

The broad pulses that were spoken of this morning are pulses with energies that are very low; 25 or 30 calories per gram or less.

If you get in the range where you can do damage to the cladding, you already have narrow enough pulses, except in a test reactor, where you can contrive to make them broad, to expel fuel.

DR. SIEBER: Well, have you done any research to decide what the pulse width will be in an RIA in a reactor?

MR. MEYER: Yes. I will show you that in the third presentation.

MEMBER ROSEN: And I thought you were going to finish that sentence, Jack, in a real what?

DR. SIEBER: In a real reactor?

MR. MEYER: Yes. The answer is yes, and I have a --

DR. SIEBER: And is this a realistic calculation or a licensing calculation?

MR. MEYER: That is a realistic calculation.

DR. SIEBER: And you are going to show me them?

MR. MEYER: I am going to show you them in the third presentation.

DR. SIEBER: Okay.

MEMBER ROSEN: The third presentation? I have to wait for that.

MR. MEYER: You have got to wait.

DR. SIEBER: Yes. Tell us when.

MR. MEYER: Okay.

MEMBER FORD: But the value of this plot, paint brush thing there, is that your technical basis for a specification of some sort?

MR. MEYER: No. This is just to guide your eyes along roughly where the fuel failure level is seen in these data. Now, for a PWR, we -- let me get my story straight. Because of the potential for a fuel dispersal here, we have chosen to take cladding failure as the coolability limit.

In other words, whereas at low burn-up, we recognize that cladding failure did not cause fuel dispersal, and therefore, any consequences of fuel dispersal.

So we worked with two different limits; a coolability limit at a higher enthalpy, and a cladding failure limit at a lower enthalphy for the purpose of doing some dose calculations.

At high burn-up, we have chosen to

collapse this and to work on the cladding failure limit as the cladding failure threshold as the coolability limit. And I am confident that this is going to work because we are going to end up with cladding failure enthalpies in this range, somewhere in the 80 to 100 calorie per gram range, which is roughly twice as high as the enthalpy that you can deposit with a PWR experiencing a rod ejection accident.

And so it is a success path in my opinion, and I am going to show you how we are going to put it all together on the next slide. So we are searching for a curve that looks something like this, and what we are going to do with that is to do some plant calculations, which we can do, with a nice 3-D neutron kinetics code called PARCS.

And we are going to do some generic calculations looking at road works that are necessary to get you up to this cladding failure limit, and then by comparison with rod worths that are known from cordizines for commercial reactors to show that you don't have enough worth to fail the cladding.

And therefore none of the consequences that we are concerned about will take place, and that will be the end of our confirmatory demonstration.

Now --

MEMBER FORD: There is a big assumption, and that is that your logic tree coming up with the results, and the big assumption is that the oxide thickness is the predominant metric of fuel cladding failure.

And if you can show that, great, but that is a dominant one, and burn-up has got nothing at all to do with it, and strain rate has got nothing to do with it.

MR. MEYER: Well, we are not -- I don't think we are constrained to saying that this is the only variable involved. Just as EPRI was not constrained to say that burn-up was the only variable involved when they plotted burn-up along this line.

In fact, we are going to be using correlations and codes that take many variances into effect in getting there. Maybe it won't look exactly like that two years from now, but this is the concept.

And we have actually three approaches, or maybe it is 2-1/2 approaches, to arriving at this -- at such a correlation. One of them is strictly empirical. You look at the data, and look at the various parameters, and try and correlate them.

Now, there is now for the past year a

correlation out there that we intend to work with. Carlo Vitanza has developed a correlation based on the CABRI data and the NSRR data, and I will show you a little bit about that this afternoon. Not a lot.

So we intend to work with Carlo and utilize a straight empirical approach from the data to try and get some a correlation. We also have a fuel rod behavior code, and can do in fact exactly the same type of calculation that EPRI is doing with FALCON.

We can right now calculate the strain energy density. It is just the integral of the stress strain curve, and we can calculate stresses and strains. Now, we are not as good at it as EPRI yet, and we don't have a big an effort as EPRI or the French have in the analytical area.

But we have got some improvements that we are looking forward to soon, and that is one approach that we can take. Another approach is just to look at the individual data points themselves and move them around on that plot based on things like temperature variations.

I will talk a little bit about that. I think we can make some progress doing that. So we are going to try 2 or 3 ways of coming up with an enthalpy, a failure enthalpy curse, to be used in order to make our assessment, our confirmatory assessment.

MEMBER ROSEN: Now, Ralph, I have been thinking about what you said, and it seems to me aren't you getting a little ahead of yourself by saying that it is a success path?

Because you could go through all of this, assuming that you can do it effectively, and end up with rod worth limitations that are so stringent that nobody could design a cycle.

MR. MEYER: Well, you know, if we didn't have a clue as to where we were going that would be the case. But I can tell you right now that it looks right now like the rod worths that you need to get to cladding failure are about two dollars.

MEMBER ROSEN: Right.

CHAIRMAN POWERS: Jack, you had a question?

DR. SIEBER: Yes. I'm trying to think about the practicality of the box on that drawing. There is a correlation that EPRI put forward, and perhaps it is an amorphous to some extent that equates oxide layer thickness to burn-up.

And I am thinking as a plant operator saying I really don't know what the oxide thickness is of my core, nor do I really have the means to measure it during a refueling. One thing I know is what the burn-up is.

So it seems to me that from a practical standpoint that I would like to calculate burn-up related to the oxide thickness, and then use that correlation to determine whether I am in bounds or out of bounds.

And to me it is a more practical approach and one which EPRI has chosen, too.

MR. MEYER: Well, we are not proposing this for industry use. If you recall, we accepted an obligation for the NRC to do confirmatory assessment for current plants 62 gigawatt days per ton.

DR. SIEBER: Yes.

MR. MEYER: And what the industry does to go from 62 to 75 is to be determined. I mean, there is a proposal on the table, and a review under way.

DR. SIEBER: Yes, but with this methodology, even at 62, you have got to make the relationship between the oxide layer thickness, which to me would vary from plant to plant, depending on how the plant is operated, to the burn-up.

MR. MEYER: Well, we know that the oxide thicknesses aren't much more than a hundred, and we have got data out to like 130 in the database. And we also have burn-ups in the database up to and a little higher than 62. So I think we have covered the range of the population of plants that we are trying to address.

And so we will just go way out here to where we think it is not any higher, and do the calculation, and all indications are that we are going to have ample margin to show that everything is okay.

CHAIRMAN POWERS: Yes, your objective is to show that the decision to limit burn-ups to 62 gigawatt days, as opposed to 55 gigawatt days, still provides adequate margin.

MR. MEYER: Yes. I would probably phase it a little bit differently.

DR. SIEBER: Or 75, or 80.

MR. MEYER: But the approval is up to 62, and for those approvals, we can demonstrate that we have adequate margin for this accident.

DR. SIEBER: That's right, and in order to approve that burn-up level though, somebody somewhere has to make that correlation.

MR. MEYER: Well, it has already been approved. This is after the fact. The 62 gigawatt a day burn-up is approved.

DR. SIEBER: Well, what if you want to go to 75? You still have to use the correlation to get there.

MR. MEYER: Yes. Okay.

CHAIRMAN POWERS: Well, no, your correlation is only good to 62.

MR. MEYER: Our correlation is as good as their correlation. It is the same database, but we are only attempting to apply it up to 62. I wouldn't say it was no good above 62.

CHAIRMAN POWERS: You have got no information.

MR. MEYER: What?

CHAIRMAN POWERS: You have got no information right now above 62, or 65, or somewhere in there.

MR. MEYER: Well, we are going to have a couple of tests at 73 in a couple of months.

CHAIRMAN POWERS: But it is not your obligation to defend a proposal to go to 75?

MR. MEYER: No, it is not.

CHAIRMAN POWERS: That's right.

DR. SIEBER: Well, I would suggest that you go on while I ponder what you have said, and how it fits into my working and thinking.

MR. MEYER: Okay. Oops. So now let me move on to the loss of coolant accident. Here we wondered if the embrittlement criteria in 10 CFR 50.46 and the associated evaluation models either in Appendix K or whatever ones are being used, are effected by burn-up, because in fact most of the models and the criteria were based on data from low or unirradiated materials.

Now, I have got several slides that I will show in a minute that talk particularly about the embrittlement criteria. So just to back up, and keep in mind that there were embrittlement criteria. So, 2200 degrees fahrenheit, peak cladding temperature limit, and 17 percent cladding oxidation limit. Those are the embrittlement criteria.

Then in Appendix K, or in a licensee's evaluation model, are some fuel related models. Oxidation kinetics, and a correlation for the occurrence of rod bursts, and a correlation for the amount of deformation in a ballooned section, and a correlation for a flow area reduction.

So these are the models and criteria that we are looking at to see what if any effect there is of burn-up. And we have work under way right now at Argonne National Laboratory, and harold Scott is going to talk about that in the second presentation.

We have Zircaloy-2 and Zircaloy-4 fuel rods with high burn-ups up at the laboratory, and the tests are under way, and it is our hope that in two years from now that we will have enough tests completed to be able to say something definitive about any changes in those criteria or evaluation models that we might have to make to accommodate the burn-up effects.

Now, I want to talk a little bit about the embrittlement criteria, the 17 percent oxidation limit and the 2200 degree fahrenheit cladding temperature limit. They arose as a pair of numbers, and they were related to some ring compression tests that were done by Hobson at Oak Ridge back in the late '60s and early '70s.

And the concept of a ring compression test was to take a piece of tubing, unirradiated tubing, cut some rings from that, and -- well, I'm sorry. First, oxidize a length of tubing.

And you oxidize it at some temperature for a period of time to accumulate a certain amount of oxidation on it. Now, what are the temperature ranges? During a LOCA transient, you heat up the fuel rod somewhere around 750 or 800 degrees centigrade.

The cladding balloons then burst, and then at a somewhat higher temperature, around 900 or 950 centigrade, the oxidation rate picks up, and as you go up from -- let's say 900 to 1200, which is 2200 fahrenheit, and up to 1200 degrees centigrade, now you are picking up a lot of oxidation rapidly.

So it is the amount of oxidation and the temperature at which that oxidation is accumulated that ends up becoming the embrittlement criteria. So in order to do that, you do a lot of ring compression tests on specimens that have been oxidized at different temperatures in that range, and accumulated at different levels of oxidation in that range.

And you find that as log as the oxidation temperature was not much above 1200 centigrade, and the total amount of oxidation is less than 17 percent calculated by the Baker-Just correlation, then you have ductility left int he ring specimen.

And if it is at a higher oxidation level, you don't have ductility left, and this is the way those two numbers were developed. So we are going to try and replicate this process with high burn-up fuel.

But there is a little problem that came up with all of this about a decade later, and that has to do with some unexpected enhanced hydrogen absorption on the inside of the cladding.

So here is a sketch of a rod that has ballooned and ruptured, and what was found was that steam got inside of the ballooned area and caused oxidation on the ID.

Well, that is taken into account in the regulation. We require ID oxidation to be calculated. So far, so good. The thing that wasn't so good was that the steam that was reacting with the zircaloy on the ID wasn't flowing. So the hydrogen that was released wasn't swept away.

And so a higher traction of the hydrogen that was generated on the ID got absorbed into the cladding, and now if you took a ring specimen from near the ballooned region, actually I am told that the effect is a maximum actually out of the balloon region and up in here.

And if you take a ring from that location, you find that at 17 percent, calculated by Baker-Just, it may be brittle when it was supposed to be ductile. Okay. There was some work done, and I just hasten to say that there was some additional work done at Argonne with pendulum impacters to show that in fact you still had ductility remaining for these specimens at the 17 percent level.

And so everything was under control, and this was all done back in 1980, 1989 or 1980, but we have to understand this, and we are going to go back through the process and do this kind of testing with a high burn-up fuel.

MEMBER FORD: What is the justification for doing the testing at 135 degrees centigrade?

MR. MEYER: What is the justification for doing the testing?

MEMBER FORD: At 135.

MR. MEYER: Oh, oh, oh, yes. Now I am going to forget exactly where the -- well, this is the temperature at the end of the transient when you come back down.

The Commission wanted ductility remaining. There were big arguments about whether thermal shock would fragment the fuel rods, and at the end of this long hearing the Commission came down and said those are all good arguments, but the only way to be sure that we don't lose the geometry of the fuel rods is that after this transient is all over, to have some ductility left.

So this is the temperature at which it is all over. These tests have been done at room temperature as well, but I think 135 centigrade is about where you expect the plant to be at the end of a LOCA, a terminated LOCA accident. So that is where they were done.

We probably are going to do them at 135 and at 20. But instead of doing pendulum impact tests to examine the effect of this enhanced hydrogen absorption, we are planning to do four-point bending tests on ballooned segments.

This is a fairly ambitious idea, but I think we will be able to do it. What this means is that we take a section of fuel -- of high burn-up fuel rods, with the fuel intact, and we sit it vertically in a channel, flow steam, and it is pressurized, and it heats up and we run it through a LOCA type transient.

And it balloons, and it ruptures, and it quenches, and it comes back down. Then we take the specimen out and we lay it down, and bend it in a four-point bend test, with the suspect region in the middle, and we let mother nature tell us where the weakest point is, and if there is any ductility left in this.

MEMBER FORD: The actual stress is in the component, and the stress would be highly biaxial, and highly anisotropic microstructure. And I am assuming that someone has taken all of these into account, all these aspects? You are applying a different stressing mode to a --

MR. MEYER: This is not -- I would like to just say yes and hope that we went away from this, but this is in fact about the right stress mode to apply here, because you if have fuel rods experiencing vibrations or seismic accelerations, they are going to be lateral, and the fuel is going to be bending and putting tensile stresses along the bowed out parts of the parts of the road.

So I think it is just about right, although I would say that we have a lot of work going on looking at the biaxiality ratios, and anisoptropies. These things tend to disappear at high temperatures and at high burn-ups. So it is definitely real critical.

DR. SIEBER: It seems to me when you get down to about 130, or even as far down as 20, blowdown loads are minimal.

MR. MEYER: Oh, blowdown loads aren't really part of it, because the blowdown load is over before the high temperature transient. What we are thinking about are --

DR. SIEBER: Seismic?

MR. MEYER: -- seismic and just vibrations,
and --

DR. SIEBER: From what?

MR. MEYER: Well, unknown bumps in the night. I mean, this is where the Commission in debating this in 1972 and 1973, after discussing possible loadings and talking about the actual magnitudes of the loadings, decided that they couldn't handle that analysis, and they said just give us some ductility when it is all over, and then we will be happy.

MEMBER FORD: I think the argument goes, Jack, that we will abuse this material as much as we can, and then we will further stress it at the worst possible temperature range, and see if anything happens to it. It is a correlation to what could really happen.

CHAIRMAN POWERS: That sounds like something for PRA.

MR. MEYER: What we are trying to do is we are trying to follow the spirit of the regulation as it was originally defined, and simply investigate the effect of burn-up on that, without trying to reinvent the whole procedure.

This is a sketch of the flow of the experimental work. You can think of another diagram over here that is just a furnace with a piece end for the oxidation kinetics measurements.

We have already completed a large series of oxidation kinetics measurements, and Harold will show you some of that later. It is just done in a furnace. These are -- the furnace that we used is a quadelliptical radiant heating furnace, heated from the outside.

So ring compression specimens would be prepared in a furnace, and we do the ring compression tests, and we look at the results, and we can do actual oxygen measurements, oxidation measurements afterwards, and look at the fracture surfaces to see if it is ductile or brittle.

Then in separate tests, we would do what we would refer to as a integral LOCA test, and from those specimens after doing a profilometry to look at the burst dimensions, then we can turn it over and do the ring compression tests.

And again look for the hydrogen, and do metallography on the fracture surfaces. So this is a general layout of the work that is going on. This work has not started, but is expected to start very soon.

And we have done a substantial number of preliminary tests with unirradiated material, and in the last two months have completed two tests with high burn-up fuel rods.

And there is some rather interesting information coming, and just immediately coming from those tests which Harold will show you this afternoon.

The BWR power oscillations, this is not a design basis accident, and so what we are interested in here is whether or not a fuel rod that has gone into this situation and had the oscillations terminated in a way that the ATWS rule would specify so that you would consider this successfully terminated.

And so in a successfully terminated ATWS with power oscillations, do you have benign behavior, or do you have non-benign behavior. I mean, in the PRAs up to now, it is always assumed that if you stop the oscillations in time that the behavior is benign.

But that was based on some analysis done by General Electric, where they used this 280 calorie per gram number, which is now not going to service well for high burn-up fuel. So it raises the question as to whether that assumption that we make in the PRAs is correct.

CHAIRMAN POWERS: Well, even the process by which you get out of the oscillation is based on the assumption that there is ductility in the rods at sufficient level to withstand the dropping of the level, and remixing the boric acid when you have to, and things like that.

So, I mean, there is quite a lot that is involved here.

MR. MEYER: Let me try and -- oh, that one is in another one. I don't have any more on it in this presentation. I have a little more on this later. This work is going very slowly, and I tell you this every time. This is not our top priority, and we don't have a lot of horsepower working on this, but we have made some forward motion on it in the last year, and we will tell you just what little progress we have made in that presentation.

CHAIRMAN POWERS: Maybe this comes up later, but for us this is -- for the ACRS, and not for this committee or subcommittee, but for the ACRS as a whole, this is a great deal of interest to us because we are going through approving power uprates for boiling water reactors.

And when we come to the PRA and say what is the risk significance of this, this is what -- the issue that comes up, and the issue that comes up is how shortened is the time available for the operator, and if we go to high burn-up fuels as we well might with uprated power reactors, are we getting into regimes that are not.

So I guess for us it is maybe a higher priority than you see it in the fuel programs.

MR. MEYER: Okay. One of the next issues on that list was our computer codes for fuel rod and neutron kinetics, and if you read the wording for the issue, and what the issue was, it was related to the fact that when we started reviewing applications for high burn-up, our codes were not adequate for doing audit work, because they had not been updated for high burn-up analysis.

And so we said as soon as we can get these codes updated for high burn-up work, then this is not an issue for us any more. It doesn't mean that we are not going to do any work on our codes anymore, but it means that this particular issue would be resolved, and it is now resolved.

The FRAPCON code was updated in 1997, which is actually before the plan was issued, and the FRAPTRAN report, the transient code, that was finished in -- well, just about a year ago, August-September of 2001 is when we issued documentation on the FRAPTRAN code with the high burn-up updates.

And in late 1998 the PARCS code was documented, and we are usually it routinely. So I think for the purpose of this plan that we have achieved our objectives for these code improvements, and this particular issue then ought to be considered resolved.

Source term for high burn-up fuel. I am almost afraid to say anything to this group about source term on high burn-up fuel. But the question was could we use the NUREG-1465 source term above 49 gigawatt days per ton, because in that report it said may not be applicable above.

There is sort of a bottom line on this, and the sort of bottom line is that we have met with a group of experts, and that elicitation was documented earlier this year, and if I could say it in a word, I would say that the 1465 source term is probably okay above 40 gigawatt days per ton, to at least 62, where we are using it now, with the provision that it would be nice to make some improvements in checks and things with data that are being generated in Japan and France.

And so we have agreements in place for these data, and we have a Reg Guide that may be revised to take all of this into account, but it is at this point that the schedule kind of breaks down because I really don't know exactly how we are going to wrap this up.

But I think in essence it is kind of wrapped up and maybe Dana has another perspective.

CHAIRMAN POWERS: Well, I like what you have said here better than I did on your first slide on the nine topics, because you said it was resolved there.

MR. MEYER: Oh.

CHAIRMAN POWERS: And the experts got in and looked at the thin little data that we have, and said, gee, the biggest changes in 1465 actually come because since 1465 was written, we have more understanding of fission product behaviors, and I would characterize that most of their changes is being changes to the fission product phenomenology, rather than the high burn-up effects if I were to characterize them.

The database says, gosh, volatile fission products look like they are released a little faster from high burn-up fuel than from medium burn-up fuel, but since they are high percent released, and 1465 is an integral release, maybe it changes the timing and there are some relatively minimal changes in timing were made.

And the more significant observation has come out of the PHEBUS program where we are seeing a lot more molybdenum release than we had in previous tests at any burn-up, and that seems to only get worse as you go up in burn-up.

And those are the big changes that I recall on this sort of thing. The database is thin, and there are lots of things that we don't understand. The VERCORS data and the PHEBUS data don't really agree entirely on some fission products, notably barium.

There are lots of things, but to your general conclusion that 1465 isn't completely orthogonal to high burn-up fuel is probably a pretty fair assessment of the situation.

MR. MEYER: Okay. And the final issue that I want to mention is the one having to do with fuel behavior during dry storage and transportation. In the original plan, we said this is something for the future that we don't have to worry about.

Well, the future arrived since then, and there is now a need to renew some cask licenses and license some new casks for burn-ups that are higher than the 45 gigawatt day per ton that I believe was the previous limit that had been approved.

We actually started working on dry storage and transportation issues with some Surry fuel that was medium burn-up fuel that had been in storage in a demonstration out in Idaho for the last 15 years.

And as soon as that work came out of the creep furnaces up at Argonne, in July of this year, we inserted some creep specimens from the Robinson rods, which are high burn-up PWR rods.

So at this time, we have high burn-up fuel rods sitting in the creep furnaces, and these tests go for something on the order of six months, nine months, to a year.

And during the next year we will also do the isotopes measurements and other things that are needed. So this is a fairly short range effort that will give us a chunk of data that are needed for the cask licensing and we will probably deliver that in a research information letter to NMSS in 2004.

Now, there are a number of other factors that affect their guidance, their review guidance documents, and so it is not clear at this time whether they are going to immediately make some revision, or just hold that information until they are ready to make other changes.

But I believe the research part of this will be done in one year. So with that, I would like to stop, and --

DR, LEITCH: I just have one question back on the reactivity-initiated accidents. Did I understand you to say that you were making the coolable geometry limit co-incident with the cladding limits?

MR. MEYER: Yes, you did.

DR, LEITCH: I missed exactly what you said in that.

MR. MEYER: That's exactly right.

MEMBER LEITCH: And what was the rationale for that?

MR. MEYER: And the rationale for that was that at low burn-up or zero burn-up, where the original criteria were developed, and where we had two different criteria, you did not have a mechanism for dispersing or expelling fuels out of a cladding split until you got up to a higher temperature, where you started getting incipient melting in the fuel pellets.

And at that time, you now had a mechanism for dispersing fuel, and breaking up the fuel rod itself, and so we started out with this two-tiered structure.

We end up with a situation where the high burn-up fuel has a built-in mechanism for dispersing fuel, and as soon as you open up the cladding, it can cut fuel out of the opening.

MEMBER LEITCH: Okay. Yes, I understand.

MR. MEYER: Okay. So, Harold Scott is now going to tell you about some of the work at Argonne, and some of it is very recent, and I think you will find it quite interesting.

MR. SCOTT: Let me just emphasize some topics that I am going to want to emphasize as we go through in these experiments at Argonne in the hot cells. We did see a fuel loss, and I will show you some pictures and they will be in your handout of the balloon fuel rods and the fuel actually -- and little pieces of fuel coming out through that burst.

The other thing that we have observed in two tests that were done with the radiated rods is that we get the same approximate shape and size of the balloon and the burst.

At one point, we thought, well, this is irradiated cladding, and it has got oxidation on it, and it may give little tiny bursts, and maybe just little cracks. It will look a lot different. But so far we get sort of the same kind of balloons and cracks that we got previously.

And there is some data on irradiated tubes. The Germans ran some tests 20 years ago and so we are not completely blind in terms of irradiated tubing. We also thought that with the high burn-up and the fact that the gap is closed that maybe the gas flow would be different, and therefore, maybe you wouldn't balloon, because the gas in the plenum wouldn't be able to get down and make much of a balloon.

Or as soon as it ballooned a little bit, the pressure difference would go away, but it seems that the gas communication was at least good enough to give us balloons, and I will show you some of that information.

We also find that the rupture temperature was about what we expected, and so when we say, okay, let's put a certain amount of pressure on a rod, it balloons and bursts at about the temperature on the way up that we would have assumed from the information previously.

Okay. This is our largest program from a financial standpoint, and so the boss, he spends most of his money on my program here. Yuan Yan is the principal scientist on this program, and Dr. Billone is the principal investigator.

As we mentioned before in previous meetings, EPRI was instrumental in getting these limerick rods and the Robinson rods. These are Lee test assembly rods, and the limerick rods are 9-by-9 design.

The Robinson rods were made by Framatome and its predecessor, Siemens, and they are 15-by-15 rods. And as I mentioned before, they do have this typed fuel cladding bond and they might even be in some of the cuts.

It looks like the cladding and the fuel are sort of stuck together and they just don't fall out like they would as a bunch of pellets in a new cladding. So these are the kinds of effects that we would expect to be looking at.

So now I will talk about the kinds of effects that we are going to have. The main item in these oxidation kinetic studies was that the question had to do with whether the corrosion layer on the cladding to start with would make some difference in the following high temperature steam corrosion.

And whether, of course, as the previous slide showed, whether the fluence had some effect. So this 1204C is the 2200 degrees F, and we are doing the same kind of experiments primarily that they did -- that Cathcart and Pawel did, and other people have done over the years a long time ago.

You oxidize them, and then you go in and measure these thicknesses. Then we have the LOCA tests, the integral tests, which are sort of unique and new, and some of these tests have never been done before when we go through the whole sequence, because once again we have taken the rod and cut a piece out of it, and didn't really disturb the pellets in the middle of the section.

So we are looking again at this criteria. This equivalent cladding reacted is just a simple function of the weight gain, divided by the clad thickness. The trick of course is what is a clad thickness. As it gets thin, and as the rod balloons, you need to take that into account, and that is the way that this is defined on how to do that.

Ralph already showed you these little pictures of how we are going to do the bend and ring compression tests.

DR. SIEBER: I have a question about the

details. Robinson fuels, 15-by-15?

MR. SCOTT: Yes.

DR. SIEBER: And it came from Framatome, who evolved from Siemens, and Siemens evolved from Exxon, right?

MR. SCOTT: Yes, and this was probably made by Exxon in --

DR. SIEBER: Now, Exxon autoclaved their clad, which is a different process than General Electric and Westinghouse, and a bunch of others, to try and reduce the surface oxidation.

Did you find a benefit from that autoclaving, and do you think that the fact that that fuel clad was manufactured differently than other brands that it had an impact on the data that you have?

MR. SCOTT: Well, I think I show -- what was the oxide thickness? It was almost a hundred. Didn't we have that?

DR. SIEBER: That's pretty bad.

MR. SCOTT: Yes, and so we have -- these rods had like many cycles, and so they started out and were in a couple of cycles, and they took them out, and they reconstituted them, and put them back in. So the fact that it may have been sort of protected cladding for an extent, the fact that they were in there a long time to get up to this burn-up, because they weren't particularly high enrichments, they had to reload them into the assembly with other driver rods.

DR. SIEBER: Right.

MR. SCOTT: So that they could reach that and once again the maximum amount is this hundred. So some of them may have 80 and some may have 90. I don't really know whether anybody said that autoclaving technique helped these or not.

DR. SIEBER: Well, could I conclude that it makes no difference as far as the data that you are --

MR. SCOTT: You mean is this oxide a little bit different maybe than some other oxide?

DR. SIEBER: Yes. Could I draw that conclusion and then we could just move on, or is there a difference and did you look at it?

MR. SCOTT: I don't think we looked at that.

DR. SIEBER: Okay. So let's just move on then.

MR. SCOTT: Okay. I'm up to some of the results here on --

MEMBER FORD: And so the same point if you are going to oxidize the inside. I am assuming that Limerick rods are not barrier fuel?

MR. SCOTT: They are barrier fuel, yes.

MEMBER FORD: So the inside is severely oxidized aren't they? When you blow steam through the --

MR. SCOTT: Well, no, for these oxidation kinetics tests, they are OD oxidation only.

MEMBER FORD: Oh, okay.

MR. SCOTT: So we put them in the furnace, and we plug up the ends so that nothing really -- and we removed the fuel, and so we are just taking mineral specimens that don't have any -- and we are only doing the oxidation.

So there was no difference in the weight gain between the unrated and rated. I will show you a graph in a moment that shows this comparison for the unrated. The Russians in our joint program with them are working on this alloy, and it has niobium in it, as do these two here.

And it turns out that Cathcart-Pawel does pretty well on all of these alloys.

DR. SIEBER: Now, the Robinson fuel is Zirc-4?

MR. SCOTT: Yes. And it was called -- it wasn't really low-tin, but it was not the regular 1.6 old. It was maybe 1.5 or some other number. It was a better version, or maybe selected from an ingot that was a little bit -- because at that point they realized that the more they got the tin down, the better off they were.

DR. SIEBER: Yes, it had fewer car bumpers in it.

MR. MEYER: Now let me show up --

CHAIRMAN POWERS: What you are saying is that the low level alloy agents don't make very much difference in these oxidation kinetics, right?

MR. SCOTT: That's what we seem to find.

CHAIRMAN POWERS: That's terrific.

MR. SCOTT: Okay. Here is an unirradiated, and this is what we have dated all over the place with this kind of stuff, but not too much data now for the radiated.

But this is about -- and here is a scale under this 250, and so this is about 120, and this is -- it is hard to see the edge here, but this outer zirc-oxide is about a hundred microns.

DR. SIEBER: Now, whose fuel is that?

MR. SCOTT: This is the Limerick, the G.E.

Limerick fuel.

DR. SIEBER: Okay. Thank you.

MR. SCOTT: So I don't think you can see it in here, but the barrier would be on this side, and it had maybe 10 or 20 microns of original corrosion on the outside, and so in this case here for the irradiated, you can't see it here, and I don't think you can even distinguish it metallographically.

But this outside corrosion layer would be here, and then the steam oxidation would have kept eating away the zircaloy and forming zirc oxide at this point.

And you have a nice boundary here for these unirradiated, but in the irradiated, it is going to be sort of tough to measure this thickness.

DR. SIEBER: That's your fault.

MR. SCOTT: That's how you get this. We call this prior beta because the temperature here was up to 2200 F. a long time, and this material changed phase, and then as it cools back down, it comes back maybe slightly different HCP than it was originally.

And there is some oxygen in here -- and I will talk a little bit more about the movement of that.

MR. MEYER: I think we ought to emphasize that we have not done the oxidation measurements on the Robinson fuel yet. So in everything that Harold is going to talk about, we don't yet have a high burn-up fuel that was heavily corroded in this database. That will be started this fall, I believe.

DR. SIEBER: All right.

MR. SCOTT: That is a good point. This data point would be, say, five minutes at the -- and here is one at maybe about 10 minutes, and here is 20 minutes. So it seems to fit.

They took the old data out of the Cathcart-Pawel report, and put it here, and then this is like in cell, and so this is irradiated, and these are unirradiated archive specimens from the GE cladding.

This is some other material that we had in hand in Zirc-4, hut it is unirradiated. So it is just these ones that have an i (phonetic) that are the irradiated.

CHAIRMAN POWERS: I am fascinated by your vertical access. It says measured weight gain from metallography.

MR. SCOTT: How do I get that? Okay. Well, I can measure back on that graph that I had before, and I go in here and I measure this thickness, and I compute the weight gain from that thickness.

CHAIRMAN POWERS: When I look at that layer and cross-section, it looks like the -- it is topographic in its nature. And so it kind of takes a measurement of the thickness. I mean, do you measure it 50 times and take an average?

MR. SCOTT: Yes.

CHAIRMAN POWERS: Okay.

MR. SCOTT: And there is some -- and this is Zr-02, and there is weight gain from this layer, and this layer is maybe Zr-0.1, or .15 or something like that. And there is a concentration grading across it a little bit.

MR. MEYER: Tell them of the several methods that are used to get the weight gain, because that is not the only method that the lab uses.

MR. SCOTT: Besides just weighing it at some point.

MR. MEYER: They weigh it.

MR. SCOTT: Okay. Remind me a little bit.

MR. MEYER: Well, they have like three different methods. The third one is escaping me at the moment, but they do weight measurements, but this metallographic technique turns out to be the most accurate, and here they have done repeated checks to see what their error has been, and this technique seems to give them the best accuracy of all, and so that is the one that we chose for this plot.

MR. SCOTT: And we get rather uniform -- some of the tests that we did early on, this was a variable, and we decided that there was something wrong with the test, because most people who have done these kind of tests all the way around, you should get uniform thickness.

The temperature in the furnace, we know it is uniform all the way around. Here is a plot now that compares with the Cathcart-Pawel correlation, and these different alloys. So what we did is we said okay, compute at this temperature with the Cathcart-Pawel model, and it would then give a number right along this line here.

So here is Baker-Just, and as we know it gives more oxidation. The Leistikow is this one, and this is the other one that probably has world acceptance, in addition to Cathcart-Pawel.

The Urganic measurements were primarily high temperature measurements, but they did enough measurements at lower temperatures that they have a correlation.

This is the GE cladding. These are

unirradiated, of course. We did some at Argonne, and then we gave some of the same specimens to France and they did them. So here is their answer here, and here is our answer here, and so the point about this is that it is sort of the same specimen, but done slightly different.

They did double-sided oxidation, and they have a different technique maybe of getting the answer than we do, and so you can see the variability, talking about some variability for the same duplicate specimens.

So the fact that these lines and these points don't all come down there, you are not going to expect that because here is two materials that are exactly the same.

This is M-5 data from the literature, and this is not anything that we have measured yet, but we expect to, and once again then this is the Russian data that our colleagues in Moscow have -- well, here is one here, plus one over there.

So it looked to me from this type of figure that all these alloys -- we don't have any ZIRO on here, but they are going to give -- you know, Cathcart-Pawel can be used for all of those.

Okay. This is now my last slide on the

kinetics, and I wanted to go back I think to my figure, but now I don't remember why. Oh, okay. This item here about why does there seem to be some difference here on the cooling, or on the heating and cooling rates.

If the old data were taken with fast heat-up, fast quench, but in these experiments, since we cool them down slowly, this layer, its thickness can change depending upon how fast or how your cooling down this material, and we cool that rather slowly.

Which is the case in the LOCA. I mean, it goes up to its peak, and it may not get to 2200 F., but it takes several hundred seconds sometimes to get back down to 800 Centigrade. Now we will go on to the integral tests, because these are the ones that we have had the most interest in.

The main idea of doing the oxidation test was to make sure that if it turned out that for irradiated material that Cathcart-Pawel was not the right number, or was not the right correlation then we would have to have a unique correlation to do these, because the idea is to oxide these specimens in the LOCA such that we get close to this 17 percent criteria or something like that.

And we have to include the prior corrosion thickness. So these specimens are about a foot long, and once again this is the 2200 F. number that we are going to shoot for. This takes about 3 minutes to go from --

CHAIRMAN POWERS: Your next line, temperature ramps relevant to small-break and large-break LOCAs, is one that I am interested in. When you say that those ramps are relevant to those particular accidents, presumably for which you calculate for those rather, is it the -- what usually gets shown in connection with a small-break or a large-break LOCA is the maximum temperature at any point in the core as a function of time.

Did you use a ramp appropriate for a particular rod in a core as a function of time?

MR. SCOTT: These kind of calculations would be done -- this is now during the -- not during this initial CHF blowdown type thing, but later on, at least for the large-break LOCA.

We looked at data and the calculations. So sort of like the hot-rod or --

CHAIRMAN POWERS: If you look at a particular rod, or did you look at the temperature, which is the hottest location in the core, which changes as the transient goes on.

MR. SCOTT: I understand that, and I am saying that we have used a variety so that not everything that we looked at was the maximum power rod that was running up the fastest rate.

We also looked at some rods that were running up at a slower rate. Now, it may turn out that some rods might go slower than this, and if they don't go very high, and if they only go from 400 C. to 800 C., I don't really care what the rate was for that.

CHAIRMAN POWERS: I am more interested in the rod that goes up and kisses 2200 degrees F., and drops back down, and then comes back up again.

MR. SCOTT: I guess I haven't seen any calculations that do that.

CHAIRMAN POWERS: There were some that were presented to us some years ago showing exactly that kind of behavior. I don't know how general that is.

MR. SCOTT: So these are the main -- this one here is -- let me just say that this is assuming that some rod in the core that had its maximum gas release, and was driven hard, could have a pressure of, say, 2900 psig in it, and so under LOCA conditions of atmospheric pressure there might be a delta p that high across that clad.

But for the GE BWRs, this number is maybe 1250 psi. This is now a picture of the test train and that we have one of these thermal couplers on here will be the one that is controlling the furnace lamps from.

There is a pressure transducer at the bottom, and one at the top. This distance between these spacers was like 18 inches, and I sort of want to point that out, because visually in your mind now, turn this up so that when or after it has gone through its temperature, its high temperature, I am going to put water in at the bottom here, and the water is going to sort of come up in here.

And at some point here, it will start to boil and bubble, and throw or drop this up, and then this part of the rod will quench. Now, these are the parameters that we were using for heating up and cooling down the rod.

And I thought I would for you thermal-hydraulic guys, don't try to take this number and think about the FLECHT correlation or something in inches per second, because we have a different flow area, and our idea was to just get some water in there and start to get it cooled down.

And after it reaches this temperature, we would push the button and the water comes in. Once again, we are looking for this equivalent cladding reacted, and it would make a difference of about -- well, wall thinning as Ralph showed when you get steam on the ID and hydrating, and we are going to see all of those.

So the first few tests that we are doing go for five minutes, and if we go for more than five minutes, we would get way above the 17 percent oxidation criteria.

So we have three specimens, and so we are doing in three different times, we are going to make these different experiments. So we did this first test in August. These are the in-cells, and so these are our first in-cell BWR tests.

We did quite a few out-of-cell tests with unirradiated material to check everything out, and we can then do comparisons between exactly the same sort of set-up. So this one was done without steam, the very first one.

The next one, we did this one then in September, and in this one we have steam and we take it up here, but then we don't put any water in. So it just cools itself down. We turn off the furnaces at

800 C.

So the next test we will do, probably in November, will be the complete sequence. So when we do this test here, this will be the first one with fuel, irradiated fuel, and the fuel inside and undisturbed, and through the whole sequence of quench.

I am going to now go to some of the out-of-cell tests, and then I will come back and talk some more about these in-cell tests. So here is this test sequence.

At room temperature, we do some permeability tests and I will show you some graphs for that. Then we raise it up to 300 C. and do some more. The steam comes on, and off we go for this 3 minute ramp up to the temperature, and we are holding up here for 5 minutes.

But later on, we will do some longer tests, and it then cools down and we control the furnace to let it cool down at this rate. At this point then, the water in this next test that we haven't done yet is the one.

And it turns out from the out-of-cell tests that we have done that this sort of continues to come on down here. And even though we are adding water, and then all of a sudden it will drop down pretty much like this. It really quenches quickly once it gets to the point where there is enough water.

CHAIRMAN POWERS: Did I understand you correctly that you have not done any quench up until now?

MR. SCOTT: That's right. Well, out of pile we have. We have sort of two set-ups. One set-up is right outside the hot cell that uses unirradiated tubing.

We have done all these sequences with quenching with water, but not in-cell yet. So we have done (a) and (b), and we are going to do (c). So here again is the -- these 9-by-9s as I said are the GE-11 design with with the liner.

This is once again -- well, we chose this 1250 psig to try to give us a ballooning at about a temperature that would give us a large balloon if we could.

And if you go back to the old Oak Ridge burst curve, they had a temperature and engineering hoop stress, and there was maybe a high ramp rate curve on there.

So this number turns out to be like 9.5 ksi. So you could look up on -- and that is what I mean before about if I go back to that old Oak Ridge unirradiated curve, and look at the KSI, and come over to the burst temperature, and I see it would maybe be 750, lo and behold this test gave me about the same number.

And once again we are getting 50 percent strain for this one, and this one is maybe a five inch. Now, this doesn't mean that the balloon was this big for five inches.

It just means that if I look along the profile, you begin to see a diameter increase over this distance. So I will come to that picture in a moment.

But these are two more of the out-of-cell tests.

For instance, this one here would sort of be the equivalent to this Phase A test that we did in-cells, since there was no steam involved with that one. And this is part of our idea for deciding how to get started with some of the experiments.

So here is a picture of this number three, and I think in one of these viewgraphs before it talked about a dog bone shaped burst. So that is what they meant, the fact that it looked sort of a little bit like dumbbells at the end.

And this seems to be -- this may have sort of collapsed back down in, and I can't tell from the way that looks. And if you look and see, it was for 10 minutes. So this one has really high equipment clad reacted.

You would almost have expected this one to -- well, if we did a ring test on it, it ought to just crack in little pieces. But it did survive cooling down and handling.

Now, the next one did not happen and it broke. Do you want a colored picture, Med, of the one that I just showed you?

CHAIRMAN POWERS: The zirconium oxide material is white and the stoichiometric material is black, and color doesn't help very much.

MR. SCOTT: Okay. This is one that survived the quench, but later then broke. The story is that the guys were all done and they went off to lunch, and when they came back, it had cracked apart.

CHAIRMAN POWERS: Well, they should have taken it to lunch was the problem.

MR. SCOTT: It maybe better on your handouts, but this is a shadow down here. They have got a light up here, and so this is a shadow that you are looking at here.

These thermal-couples are a little -- you can't see it on here, but there is a little spot here.

They stayed on during the experiment and during the cool down and everything, and then just came off when they handled it, the same way this one over here came off.

And this one tested -- and this one again had a high --

MEMBER ROSEN: This is the one that I want to see the color on.

MR. SCOTT: I think I have that one, but I don't know what I did with it.

MEMBER ROSEN: There is color on the screen.

MR. SCOTT: I didn't bring one of those with me. Now let me go off to these burn-up ones. This is a fuel mid-plane, and so I am thinking high up in the rod here, and then another one. This one is going to be maybe between .8 and 1.2, and 1.1 space.

And we have seven of these rods, seven 12-foot. What they did was that they shipped the rods from the Limerick reactor out to Valecido, and they come them into little pieces for us, and then we got back all those segments.

And so we have a number of segments to look at, and part of the idea is -- and maybe it is not so much in Limerick, but when we get around to the

Robinson rods, the higher up you are on the rod, the more oxidation you have. So that may be a factor with Robinson as to what grid span the sample came from.

CHAIRMAN POWERS: Just a question on nomenclature here. This Phase A, B, and C that you have under Limerick has nothing to do with the Region A, B, and C, and your heating cycles? You previously showed us a chart of your LOCA integral test sequence, and you have an A sequence, and a B sequence, and a C sequence.

MR. SCOTT: This one?

CHAIRMAN POWERS: Yes. Those A, B, and Cs, don't have anything to do with the A, B, and C under Limerick?

MR. SCOTT: Yes. What I am saying is the first Phase A test that we did, we went up to here, and stopped, and came back down in Argonne. The Phase B test was one that went through this sequence, and this steam oxidized and came over here, but was not quenched and just cooled down.

CHAIRMAN POWERS: Okay.

MR. SCOTT: The Phase C test that I am going to do in November is going to follow this path. So A, B, and C all followed this part, but A stopped here, and B stopped here, and C then follows the whole.

CHAIRMAN POWERS: I understand now it does.

MR. SCOTT: Okay. So, yes. This is not October. This is the out-of-cell test. So here is the one where we were talking about it being a dog-bone shape that we saw before.

So these are some out-of-cell tests like I said before compare quite well with so far the two in-cell tests that we did. The shape looks the same, and the length looks the same, and the amount of strain is looking the same. So nothing seems to be out of the ordinary for these rods.

And we are hopeful that we will have to wait to do the Robinson rods. We won't be doing them until 2003. Now I am going to have some plots, and this first one and the next one are sort of at the beginning, and then later on just to show you what the pressure and the temperature do.

So we go up to -- this is the temperature up here, and so we are going to go up to 1200 C., and then come back down, and this would be this one here, right?

MR. MEYER: That is the temperature.

MR. SCOTT: Well, here is my ramp here at the end here. Sorry. So, 300 C., and up to -- this was this Phase A test that was in Argonne, and it stopped after it ballooned, and was turned off.

And this is a comparison between that in-cell test and an out-of-cell test. What we wanted to notice here is this in-cell test at the tail end, and notice how the pressure takes a while to fall down.

So what it means is that if I have large delta p's, the gas can flow pretty well up and down in the rod. But if I get to a place where I don't have much delta p, then the gas doesn't flow very well.

And we sort of expected that from the fact that these are high burn-up rods.

MR. MEYER: May I say this differently, because here is a case of looking at a glass that is half-full or half-empty. I think the main thing to get from this slide is that the pressure took a nose-dive immediately in the in-cell test, just as it did in the out-of-cell test.

Had there been a lot of axial flow resistance, the pressure in the plenum would have fell off slowly, but it didn't, and so the gas is obviously flowing easily from the plenum into the balloon area, and depressurizing the whole rod quickly until you get down to very low pressures, and then you begin to notice this difference, because there is some flow resistance, but it is not effective at the high pressure differentials in really slowing down the movement of gas from the plenum to the balloon.

DR. SIEBER: Now, the pressures that are in there in a real situation would be the differential pressure between internal rod pressure and the reactor coolant system, correct?

MR. SCOTT: Yes.

MR. MEYER: That's --

DR. SIEBER: And the pressure is determined by the heating that is going on inside the fuel element, and the cooling is taking place due to ECCS or whatever else is taking the heat away. So you aren't going to follow these curves. That the phenomenon would occur as it is shown here in the text; is that correct?

MR. MEYER: This is internal rod pressure,a nd it would be like this is a real situation, because you do have a plenum.

MR. SCOTT: At the top of the bundle --

DR. SIEBER: Whether it bursts or not has to do with the differential pressure across the board.

MR. MEYER: Correct, and we have that set up about right, and this is showing that when it does burst that the pressure in the plenum falls off very quickly, which means that the gas is getting out from the plenum and going through the fuel, and out the opening.

MR. SCOTT: Remember that in our case that we are not too far away. The plenum is only a few inches away from the burst, because we had data from Haldan for high burn-up rods when they changed gases, and they do these kinds of experiments that show the permeatability is rather low, and they try to measure the hydraulic diameter of the gas, and it is almost zero.

And so we sort of thought maybe that the gas can't flow very well. But as Ralph says, this quickly depressurizes until -- and we don't think that the ballooning is affected by gas flow, because we get this pressure change normally.

DR. SIEBER: Well, that is consistent with your statement that the gaseous fission products are distributed throughout the wall or the rod, and held in the matrix end grain boundaries.

But it does not require flow from the plenum down to the point of ballooning the rupture in order to get a rupture.

MR. SCOTT: Well, I don't know that the --

well, it is true that the -- well, okay.

DR. SIEBER: It doesn't require much gas.

MR. SCOTT: But these in-cell tests, and the boundary, and the outer rim of the fuel, it is true that they have not been at this 2200 degrees F. I mean, the pellet rim region would be at 300 C., or 400 C., and not way up at 1200 C.

DR. SIEBER: Right.

MR. SCOTT: So there is some -- I have put a temperature transit on part of the fuel pellets, and so there might be some gas release, but we didn't see a big bump in the pressure. This is because the top of the rod is heating up, and therefore the plenum is heating up.

It is not from some gas release coming out of the pellets. Now let me show you some of the strains of these, and comparing the in-cell and the out-of-cell. I may have marked on your handouts since they are not in color, but this is the out-of-cell.

The zero degrees is where the balloon, and so they turn the rod, let's say, with the balloon up, and then they measure how tall it is. Then for the 90 degrees, they turn it over 90 degrees and measure the height once again at a difference.

So you can see that it has swollen some, and all around the rod, and then these are the in-cell ones and they once again come up to about the same amount, and here is this nine degree one.

So this one and this one sort of compare and these two compare. And if this is 44 original, half of that, 22, and 66, and so this is about a 50 percent swelling. And here is a good picture of how it is now.

So at the bottom here, this is an unirradiated one, and it had like zirconium pellets or something inside it just so we didn't have an empty tube. But you can see these little fuel particles in there, and some of them fall out.

And once again these, because they were in the reactor, I think this sort of reddish color is the color that these rods have because they have that oxide layer, corrosion layer, on the outside. Here is now a picture, and we can see the balloon itself up close.

This is the one that now went up burst and no steam, and then was cooled back down. So that is the kind of balloon and burst that we would expect for a rod, whether it is radiated or unirradiated.

Now the thing that is also sort of new that we didn't expect was this deposit. I don't have the -- maybe I can go to the next one and then come back to this one.

Before you saw that the fuel train was inside of that quartz tube, and so here is the tube again, and this is like a rag or something in the background, and so forget that.

But here are these little fuel pellet particles that have come out, and here is a black deposit on the inside of this tube, and it turns out that this is -- that inside the tube, this is where the burst was.

So something came out of that burst and pasted itself on the inside of that tube. We are going to take that to a gamma scanning device, and see if we can't see what it is.

You say a lot of moly comes out of these fuel rods?

CHAIRMAN POWERS: That's at much higher temperatures than what you have. You have not even gotten close yet.

MR. SCOTT: Is cesium the only one that would be sort of volatile at 2200 F.?

CHAIRMAN POWERS: Yes. These particles may or may not be cesium.

MR. SCOTT: Well, okay. We will see what

they are.

CHAIRMAN POWERS: I remember that Dick Laurentz, in his tests, reported in the burst test a release of particulate and vapor cesium.

MR. SCOTT: These are the VI tests.

CHAIRMAN POWERS: No, these were tests that he did on bursting rods many years ago.

MR. SCOTT: Oh, before that.

CHAIRMAN POWERS: But it turns out that those things are extraordinarily important to the transportation folks, because that's is their -- to them that is the source term.

MR. SCOTT: So like I said, this is the one that -- this is not a lot, less than a pellet's worth of pieces. And now I am going to come back to the burn-up case again here.

CHAIRMAN POWERS: I am going to face a rebellion from my committee. I promised to allow them to take a break and get some coffee before they close downstairs. So could we take a 15 minute break here and come back to the second test after that break.

MR. SCOTT: Okay. Thank you.

CHAIRMAN POWERS: It is a little bit of a disruption to your presentation, but I think everybody is following what you are doing pretty closely here.

MR. SCOTT: All right.

CHAIRMAN POWERS: So I will resume at a quarter-of.

(Whereupon, at 3:29 p.m., the meeting was recessed, and resumed at 3:48 p.m.)

CHAIRMAN POWERS: Let's come back into session. I remind everybody that Harold Scott is discussing the LOCA tests, the first we've seen of actual tests. We've seen lots of plans, but not results. And Harold, I have to say that up to this point I've got the overwhelming sense that qualitatively not much has changed by going to the higher radiation.

MR. SCOTT: That seems to be from the information we've seen so far, but as we mentioned, the Robinson rod was substantially thicker, corrosion oxide may make a difference. We'll have to wait until we get the first few tests from those.

CHAIRMAN POWERS: And I'll also say that I remain concerned with exactly what you've got up here nicely for me. You're a great straight man. With this heating schedule that you put up here because my perception, rightly or wrongly, is that this reflects the hot spot of the core kind of analysis whereas especially for the Robinson test, I think we're really interested in what an individually rod perhaps at some point the hottest rod, but not always the hottest rod, is actually experiencing, which may not be monotonically heating up and monotonically cooling down, but rather going through wild gyrations.

MR. SCOTT: But remember that we don't really get any oxidation cooking until we get up into here.

CHAIRMAN POWERS: But remember, that you've already got it oxidized.

MR. SCOTT: The outside is.

CHAIRMAN POWERS: The external oxide and especially with Robinson fuel for your roughly 100 microns, it's getting very close to the point where that oxide becomes very susceptible to thermal shock induced spalling.

MR. SCOTT: But also, the kind of data we have from various ballooning and burst tests, this isn't particularly too critical. This is 3 or 8. It's going to come up here and I wouldn't think we'd get too much effect on -- what we will get some effect on is the fact that this material is irradiated may change this what we call alpha-beta transition temperature which will depend upon -- Hee Chung says some of these bursts look like -- they're a little bit different than the ones he would have expected at that temperature because they've sort of crossed over into another crystallographic -- I put this up to remind you again because I'm going to show some vu-graphs. We're doing a permeability test, a gas flow test at -- down here and then at 300 C and this was the A one. Then the B one goes through the high temperature oxidation.

MR. MEYER: While you're changing slides, let me comment to Dana.

I think we will get the information that you're interested in from the integral tests where we will be looking at the oxidation level in the balloon region where it has deformed and broken up any heavy oxide that was in that region, so even though we don't jerk it up and down in temperature, there is going to be a big balloon deformation taking place that will mechanically shake up the oxide and where we will look in great detail.

MR. SCOTT: Yes, we haven't had a chance to do any metallography on these specimens yet. So this is the low temperature, this one and the next one showing you how well this upper pressure transducer and the lower pressure transducer track each other and as we just mentioned before, Ralph reminded us that this one tracks very well, right here at the beginning. So that's sort of what's critical, the fact that it pales off here a bit here probably doesn't have too much difference in the behavior.

So now we're going to go to Phase B which steam when through the 5-minute oxidation. This was the pattern here and then we ramped up to -- and I think I've got some graphs here I may have shown before. We got this little pressure peaking because the plenum heats up again.

Here's the burst temperature. All of these, you'll notice, A, B and the out of pile, out of sale tests all had for this same gas pressure had about this 750 C. In fact, I think I noticed that before here. One of them -- they're the right order. If the pressure was a little bit higher, it -- the burst temperature was a little bit lower which is what you'd expect.

So how we're at the higher temperature permeability and it looks about the same. Once again, it's a little lag in this lower pressure transducer to see the pressurization, but then it quickly catches up. And I have the downlay side.

Now we're off to the test, the most recent test we did in September. This is this little heat up here where the pressure goes up. This is the bursting here and then it goes on up and starts to oxidize.

MR. MEYER: If you wonder why we don't show the lower pressure transducer at high temperatures, it always fails. We've got -- the steam is doing it in and we have to do something about that.

MR. SCOTT: In the first test, it didn't hardly work at all and the second test, it works, but wasn't reliable.

Once again, here's this 50 percent strain. We haven't gone back and subtracted out oxide thickness, so these numbers will in the report might be slightly different and once again, the shape of the burst opening was sort of like what we saw in the underrated experiments.

Here's now a plot of those. I was just talking during the break, Robbie Montgomery, his code will calculate, he can see this shape here. My code doesn't do that. I get this number and this number, but I don't -- I'm not able to calculate the shape of this.

We don't know how you do it, but -- actually, we do.

Okay, now I'm up to a picture here of

these -- of the one I just showed you here, the ballooning and you can see -- this is this zero degree, if I measure from here up to here, that's the so-called zero degree. If I roll it over, then I get a 90-degree measurement here and this is when I said before like this is the from maybe here to here is the amount of ballooning that I get.

And part of this point is before when Ralph showed you that schematic about how's he going to cut a ring near the balloon, but it's still got that high hydrant, so we think up in here, there's going to be, you can get rings up here that we don't think will have much hydrant because it's not much weight for that extra steam and hydrogen to get all the way up here. But in this part here, we can get several of these rings out of here and then we can say from here over to here and do this 4 point bin test.

CHAIRMAN POWERS: I take it you are lobbying heavily to convince Argonne that they ought to become metric?

MR. SCOTT: Weren't all these metric, centimeters and C.?

MEMBER FORD: An inch?

(Laughter.)

CHAIRMAN POWERS: You can explain to him

these modern measurements.

MR. MEYER: The hot cell was built in the 1950s. This ruler has been in there every since.

(Laughter.)

DR. SIEBER: It's probably a little over waste by now.

MR. SCOTT: Okay, I mentioned before this dark deposit on the tube that occurred. The other question we sort of had was does that deposit now affect the temperature behind it since the lamp was trying to send entry through and I was told that they were going to check that out and do some tests with -- put a device inside of that tube that has a deposit and see if it can -- if that shadow actually makes any difference.

But we were told it was rather thin, so I think it's not going to make much difference. Once again, we're talking about the amount of fuel that came out. We put a little basket at the box so we can catch the fuel if it falls out during the test and then later if when they take this thing over to some table, but they try to keep the brake up so nothing else falls out. But a pellet is maybe 10 grams.

We're only getting maybe half. I wasn't really able -- I've not seen these and you can't see it in one of these pictures here. I'm going to show a little bottle. Here's these pieces. They're small, but I don't know if they look like shards or if they -- the size of a bb size, but we'll characterize those.

The other thing that came up is in preparation of these specimens are we doing anything to the fuel rod and the pellets inside that maybe would make a difference in the answer. Is our experimental technique affecting our answer? So we have these specimens that we cut and we can look in there and say okay, if I drill out the top of the rod to put a little plenum in it and a cap on it, have I somehow vibrated and cracked the pellets six inches away, so we're going to do some -- and so far we don't see that. We can head it off and it looks just normal.

Once again, this dark deposit, to see if we can see what it is. We'll calculate more exactly the equivalent cladding reacted and we'll look at the -- we have a hydrogen determinator device that if you take a specimen, we'll be able to see what the PPM to hydrogen was at the various locations.

Then we have the -- as I said before, we've done these out of sale tests with the quench.

We'll take that quench system and put it in the cell so in November we can do this phase C test that's the full LOCA sequence. And that's the end of mine.

Are there any questions?

CHAIRMAN POWERS: Any other questions for Harold?

MEMBER LEITCH: I guess I just want to make sure I'm coming away with the right conclusion here. I guess what I'm hearing as far as this high burnup fuel is concerned from the work that you've done so far, and I know you're anxious to see the results of the Robinson test, but from the results of high burnup fuel at Limerick, we don't see that much unexpected or different than you would have expected.

Is that a correct assessment?

MR. MEYER: let me answer that and say that's a correct interpretation of what we showed you, but keep in mind that what we're looking at are embrittlement criteria and evaluation models and we've now looked at data relevant to three of the evaluation models, the oxidation kinetics, the rupture temperature pressure conditions and the ballooning strain.

Those three at least on the low corroded BWR fuel look unaffected, qualitatively unaffected by burnup. We have not get looked at the embrittlement which is the one that is most likely to be affected by burnup because it should be directly affected by hydrogen absorption and there's going to be more hydrogen in the burnup specimens than in the fresh fuel.

So we've looked at three important models, but not at the criteria.

MEMBER LEITCH: Then, of course, the Robinson work will be very interesting because they have thicker oxidation.

MR. SCOTT: And it's hydrogen levels are substantial. When you have 80, 90 microns of oxide, you get substantial hydrogen.

MEMBER LEITCH: Yes, okay, thank you.

MR. SCOTT: We had a paper that Argonne issued, about 10 pages, back in June. It's in ADAMS. I gave Med a copy if anybody wants to get it. It sort of shows some of the graphs I've showed and describes more details of these ECR calculations and the fact that we get sort of a similar oxidation for the different alloys.

MR. MEYER: So now I'm going to come back to the RIA and ATWS situations and just hopefully demonstrate a little bit of progress in the last year, but I don't think we'll reach too many conclusions that you haven't heard before.

I'd like to summarize the pulse width situation to show this Vitanza correlation and then to describe briefly the method for making temperature corrections.

This is just two typical cases that were run with the PARCs 3D neutron kinetics code for rod worths that are reasonable, about $1.20. And in fact, they produce relatively low energy pulses. This is a plot of the power for these, a beginning of side and an end of cycle. They're different. The calculation takes the plutonium build up into account and other things.

And here is the enthalpy, the fuel pellet enthalpy for those two cases and you can see, indeed, that the enthalpy peaks rather slowly and it's a low value on the order of 30 to 35 calories per gram, but it started at 18 calories per gram, so the increase was only 15 to 20 calories per gram.

Now based on a fairly large number of cases and I think you've seen this slide before, Brookhaven has used that code to look at pulse width as a function of the change in fuel pellet enthalpy and they've done that for a lot of different assumptions.

Not on this slide, but on a similar slide, have been placed results from other people's calculations, from some of the vendor calculations and except for quibbling a little bit about the exact value, there has really never been any serious criticism of this finding. It also checks well with the Norhung-Fuchs equation, so there's analytical basis that doesn't rely on big codes and then there's big codes and there's other people's big codes. And the bottom line is that if you have low energy pulses that are broad, if you have high energy pulses that are narrow, and this morning EPRI, talking about pulses that have pulse widths no greater than about 30 milliseconds and if you, I'm sorry, no less than about 30 milliseconds and if you look on the chart you will see that those energies then are all less than 30 calories per gram.

Now let's see what I have next. If you are interested in running a test at a low energy that is comparable to what you would predict for a PWR in this accident, then you would want to run a test at maybe 30 calories per gram and 30 milliseconds pulse width. But if you're interested in exploring the energy range where the cladding is going to fail, which is up in the range of 60, 80, 100 calories per gram or maybe higher than that, the pulse widths should be around 10 calories per gram because in a PWR you just could not, it --

CHAIRMAN POWERS: You mean 10 millisecond pulses?

MR. MEYER: Ten millisecond pulses. Did I misspeak?

CHAIRMAN POWERS: You said 10 calories per gram.

MR. MEYER: Ten millisecond. It's getting late.

So this is a point that we've been making over and over again in our discussions with the industry and with the CABRI Technical Advisory Group as they plan future tests, because they continue to plan these tests with a 30 millisecond width.

Brookhaven has also looked at boron dilution events to look at the power level, the pulse widths and I have a few of those slides. I think I'll sort of rush through them in order to save a little more time for Sud. I won't skip them all together, but two cases are illustrated here, one with pumps on them and one with pumps off. This is the power and boron concentration and it shows these spikes.

This event is very reminiscent of the BWR power oscillations from the worm's eye view, from the fuel point of view. It looks very similar. And what you see is you see peak fuel enthalpy from the first pulse is very low. So you can quickly get in a little bit of fuel enthalpy and then you can get in more which is also the case in the BWR oscillations, but it happens more slowly and during that time you get heat transfer and the cladding heats up.

The cladding is then able to take it to expand, to deform and so it appears in this case to me just at first blush as it did to the PIRT members look at ATWS that probably the PCMI is not going to be the big challenge for the fuel, but rather the temperature excursion.

MR. SIEBER: How do you get a fuel dilution, a boron dilution that fast? What's the phenomenon in the plant that would take you from --

MR. EL-ZEFTAWY: Actually this was GSI 185 and it's being reviewed. We spent a whole day with the Thermal-Hydraulics Subcommittee, but what's postulated is that you have a small break LOCA and in a BNW plant. You've effectively distilled water. You now have a slug of unborated water in the -- down by the loop seal and then you do one of two things. You either through natural phenomena, natural circ. restarts which is slower, or the operators start the pumps. And their procedures tell them not to, but that's how you could get these sort of events.

MR. SIEBER: But that's well beyond the design basis, right?

MR. EL-ZEFTAWY: It looks like something that almost happened. I'm sorry.

MR. MEYER: There's an error in this label. This is peak fuel enthalpy, peak fuel enthalpy. This is the other case, natural circulation, the power and boron. Well, I guess it's just the power curves and the enthalpy curves, labeled correctly.

Now moving on from boron dilution to BWR rod drop, we've not focused much on BWR rod drop because in our risk perspective, we thought that the power oscillations were more important to look at than the rod drop. The rod drop has a lower probability than the rod ejection in the PWR, so we haven't spent much time on it and we still haven't spent much time on it. Brookhaven had done some earlier calculations. They went back and had a look and it appears, I will just say it appears that the pulse width for the boiling water reactors are indeed broader than for the pressurized water reactors, but the pulse width for the boron dilution pulses look right in line with the rod ejection pulses.

I'm not sure if this conclusion about the boiling water reactors is well examined or -- but it kind of makes sense that there could be a difference and it sort of -- the characteristic of a core and --

CHAIRMAN POWERS: Still, I think the way you went about a decision to drop the explicit consideration of the rod drop accident of the BWRs was appropriate use of risk-informed decisionmaking, guiding your research program.

MR. MEYER: You can do that or if we solve the problem for the PWRs, then the BWR analysis --

CHAIRMAN POWERS: It might like falling off a log, right.

MR. MEYER: Right, right.

MEMBER LEITCH: In the BWR, have you given credit for the velocity limiter or is this just an instantaneous --

MR. MEYER: No, no, no, no. The velocity limiter is taken into account.

MEMBER LEITCH: Thank you.

MR. MEYER: I'm going to come back to that. I didn't put the slides in the order that I wanted to have them in. And I'll just talk a minute about the temperature effect related to pulse width.

Well, pulse width, we imagine has several effects. One is through temperature and one is through dynamic fission gas expansion. We have not done any examination of the dynamic fission gas expansion hypothesis and I don't think EPRI has modeled that. It's a hypothesis that's out there and it might account for some of the scatter in the data, but certainly, we ought to be able to handle the temperature effects and I just want to make a few simple comments about it. We haven't done it yet, but we're beginning to work on it.

Here are three results from three calculations that are kind of illustrative. The three cases that we took resembled NSRR pulse, a PWR pulse and a CABRI pulse. All three of these pulses have a total fuel enthalpy of increase of about 100 calories per gram.

What we did was plotted cladding temperature as a function of fuel enthalpy, rather than temperature. And the picture to have in mind when thinking about this is we have a reactor that can give us 100 calories per gram fuel enthalpy increase and the cladding that's going to fail at 80 or 90 calories per gram. So we want to look in the range of 80 or 90 calories per gram. This is the time at which the cladding is going to let loose. It's going to fail and find out what the cladding temperature was at that time. And then try to relate that to some mechanical properties or something.

So here are the cladding temperatures. Now, the NSRR temperature is very low because it started low. It started at 25 degrees instead of 300 degrees Centigrade. Had it started at 300 Centigrade it would be very close to the 10 millisecond line.

And the 30 millisecond pulse at a given fuel enthalpy out in the range where you might expect failure is about 70 degrees too high.

Now one of the things that we think we notice from the data are that the total plastic strain in the case with the real broad pulse, the 30 millisecond pulse was a little less than the total plastic strain was in the 10 millisecond pulse. Now the fuel enthalpy was the same, so the pellet expansion should be the same and the difference that we think is there and I spoke to Rob about this earlier and we're not sure of it, but we'll look at it, is that the cladding is going to increase its diameter just from thermal expansion. And since it's hotter, it's trying to run away from the pellet and it's able to run away a little bit better when it's hotter.

So I think there are really two effects to look at here. One is thermal expansion. The other is the mechanical properties, the temperature effect of the mechanical properties. And so here is a plot of a collection of data that we have that shows total elongation as a function of temperature almost in the right temperature range. It doesn't quite go high enough, but you see here exactly the same kind of spread that you saw in the CSED curve, because the CSED curve is a reflection of the total elongation measurements.

And so we will in trying to use this, we will experience exactly the same kind of difficulties that EPRI experiences with data like this, but you know, you can say the temperature effect is between zero and this and we can look at that parametrically.

From thermal expansion and from the tensile data, we can then get a strain increment that is related to the temperature difference. You could call it strain, I guess on the thermal expansion and then we can relate that to the enthalpy chain. So this is simply the -- for that 10 millisecond pulse, the cladding strain is the function of enthalpy increase. And so we can convert the delta Ts to delta strains to delta Hs and then take them back to the paintbrush slide and move the data around and claim that we have made a correction for temperature, although we make no claim about any other effects like the dynamic fission gas or perhaps some pellet lock up or something like that.

So that's the method. Hopefully, you see a little progress from a year ago where we are. I'm going to try and go back now and pick up those other slides.

Okay, so I mentioned that we were also trying to use an empirical correlation and this is Vitanza's correlation and I only show this to indicate that the failure level in the correlation is dependent on a number of parameters, on the burnup, on the mechanical properties of the cladding, on the pulse width, on the oxide thickness and on the cladding wall thickness.

Vitanza compared his correlation with the failures in the CABRI data sets and there's one more point on this then. EPRI has showed this is the MOX data point, so he predicts the RepNa-8, RepNa-10 and I think it's the RepNA-7, the MOX failure quite well, but like everybody else, can't predict RepNa-1.

And I agree with Rosa and EPRI that RepNa-1 is an outlier. I'm probably, less diplomatic and more conclusive in my view because it's our contractor who has said that the preconditioning temperature soak has probably caused the embrittlement of this and has written a number of detailed descriptions of his observations of severed hydrides and all kinds of things to support that position.

So I am inclined to believe Hee Chung from Argonne National Laboratory that that is the main reason that this test result is not reliable. The other factors that Rosa mentioned are also legitimate areas for looking into and I think this whole thing will be wrapped up in another few months. Hopefully, we'll get that behind us.

CHAIRMAN POWERS: I'm just not sure of the Japanese data.

MR. MEYER: No, it doesn't. I don't recall whether -- did he --

MS. YANG: No, I think Carlo Vitanza only looked at high temperature data. He basically just took the CABRI data and fed it into the equation that you presented.

MR. MEYER: I don't think this correlation is ready for service, but it is interesting. It's moving in the right direction. I've spoken to Carlo about it and he's not only willing, but eager to work with us on this and I'm hoping that we can work with him to develop this correlation a little more broadly.

CHAIRMAN POWERS: Correlations of a strictly empirical type like that suffer needlessly when you try to extrapolate it and of course, you're trapped in extrapolation here because you're doing your tests in situations that people can find a litany of fault where your data base is coming from.

A phenomenological understanding is always much better, but when I did experimental work I always said well, let's get an empirical fit of the data first and then we'll work on the phenomenological. Sometimes that didn't work out. So it may have some virtue to it, a less desirable outcome than Hee Chung talking about hot short metals and things like that.

MR. MEYER: Well, as I mentioned this morning we really have a multiple approach to this, one of which is a code calculation which involves the mechanical properties in a manner that's similar to EPRI's.

I frankly think that in the end we don't need exquisite accuracy on this thing because I think we're going to have a margin of a factor of 2 on a failure limit that is clearly conservative. If you don't crack the cladding, you can't have bad things happen.

So if it works that way and we have some uncertainty in this correlation, in my opinion that would be tolerable.

Okay, on the BWR power oscillations, we talked to you a year ago about the implications that we drew from the PIRT elicitations and I have those on the next two slides and I don't intend to read through those. I just want to tell you about two new steps forward on this.

From the PIRT implications, there was a conclusion that the repeated power pulses would probably not cause PCMI failures and that in the end this would be a high temperature transient and that the temperature would be the damage mechanism. So what we have from Japan now are two tests in which they did repeated pulsing. And let me see if -- they used BWR rods, two of them, with modest burnup, so 25 gigawatt base to turn 56 and they found that the mechanical interaction didn't enhance, wasn't enhanced by cyclic loads which was one of the things we were worried about, sort of a ratchet effect.

This is a slide of their data which they will be presenting at the Nuclear Safety Research Conference in a couple of weeks and it shows the cladding elongation which is pretty small, what am I looking at here? The relative rod power is not on the scale and then you have the temperature which is -- ah, here is the cladding elongation and these are the temperatures and this will be explained at the NSRC conference in a couple of weeks.

The other thing that we have done is we've signed an agreement with STUK in Finland for cooperation in the analysis area. They had a little thermal hydraulic code called GENFLO which they coupled to FRAPTRAN and used that to try and analyze the rather active feedback that goes on between the hydraulic conditions and the fuel rod conditions in this transient.

This code was actually installed out at Battelle just almost a month ago now and we've run the code on some sample cases and are going to plan our attack, our analytical attack in the next year.

So with that I think we're ready for Sud Basu who will talk about the fuel behavior under dry storage conditions.

CHAIRMAN POWERS: Thank you. Are there

any additional questions for Ralph on his final presentation or anything that's gone before, I suppose?

Seeing none, we'll proceed.

MR. BASU: So at the end of the day I will talk about some old stuff and I mean literally old stuff. We'll talk about spent fuel rods which were in the reactor for about three years. Then they had a residence time in a wet pool for another five years. They were taken out. Went through vacuum drying and they were stored in dry casks for about 15 years. Back in 1999, they took some assemblies out, did some observation on their behavior and that's what I'm going to talk about.

The scope of the program is looking at the post-storage and by that I mean 15 years of storage in dry cask. When we took them out, post-storage characterization of these spent fuel rods. I'm going to actually focus more on the creep testing of fuel rods and I'll touch upon --

(Pause.)

I'm going to, as I said, emphasize the creep testing of fuel rods and I'll make some comments on the post-creep mechanical properties.

We are looking at and we have actually

looked at Surry fuel rods with a medium burnup, less than 45 gigawatt day per metric ton. We are currently looking at, we have started the campaign on high burnup cladding. Ralph alluded to that.

The focus of this presentation is on Surry rods because we had results to share with you. These rods which we have actually sampled from the dry casks have an actual burnup of 36 gigawatt day per metric ton. As I said, they spent in wet pool for about five years and in dry storage since 1985.

Now why are we interested in this stuff, this old stuff? The rods that are stored in dry casks are the dry casks, actually the dry casks are coming up for license renewal as early as 2004, not all of them, obviously, but the population of dry cask will be coming up for license renewal.

MEMBER ROSEN: How long were they licensed for originally?

MR. BASU: Twenty years, original license period is 20 years. They're coming up for license renewal. They'll be submitting license renewal application. About this time, they'll probably submit a couple of them and they'll be submitting more and more and there's a two year period between the application and the ramping of up renewed licenses or what we call certificate of compliance.

In order to issue that certificate of compliance, we have to assure that these casks can go up to another 20 to say 100 years. That's the license renewal period. And of course, in order to assure that, we need to assure ourselves that the fuel rods which are stored in dry casks are in good condition to be restored. So that's the incentive that is driving the medium burnup work.

And there is the incentive for the high burnup work that's the new licensing. We want to be able to verify the validity of the efficacy of Part 72 rule and how that transfers to tech specs in the Spent Fuel Project Office. So that's the incentive for doing the high burnup creep studies that Ralph alluded to and I'll touch upon that as time permits.

Part 72 says that the spent fuel in dry casks must be protected from degradation that leads to gross ruptures. That's a very broad definition. That definition has been translated in the technical specification, if you will, of the staff guidance work as cladding that should not have or must not have more than 1 percent creep strain over the period of the life in dry cask.

It certainly must not have crumbling or

you know total loss of geometry, so to say. And then of course for -- we need to look into mechanical properties of these rods so that during restorage or transportation that these rods do not lose their geometry or do not lose their strength, so to say.

So we need the creep and mechanical properties data and that's what the focus of this, the work that I'm going to present. I'm going to go very quickly through the post-storage characterization part because that's kind of an uninteresting part in terms of observations that we made.

What we did was we took 12 rods from an assembly that we recovered from an open cask and we did the peripherometry of these 12 rods to see the diameter changes and what we found is that the diameter changes first of all are pretty uniform and they're about .6 percent. Very little variation azimuthally or axially and what that transfers to is a thermal creep during that 15 year of storage life to less than .1 percent. Very little. Very little.

Then what we did is we took 4 of these 12 rods and we did -- we punctured holes and we did some gas analysis, fission gas analysis using fission gas analysis -- well, laser puncture technique so that we can do fission gas analysis. What we found is that fission gas release is about 1 to 41 percent which is well within the range that you would expect from these rods stored in dry casks for about 15 years.

And of course that again translates to some internal gas pressure of 3.5, around that, which is then within the range, so that's why I said these are all uninteresting results and that's -- there's nothing exciting about what we found. It's all expected results.

We did metallography. Not all four of these rods or not all four segments, but we chose two rods out of these four rods. Again, these were so uniform in every respect that we didn't have any problem choosing any two rods from the inventory. We chose two rods. We cut up segments and we did metallography and what we found is the rod thickness varies from 20 to 40 microns, about that.

The hydrogen content varies from 200 to 300 PPM. No hydride reorientation, not that we expected any hydride reorientation, but we wanted to be sure there's no hydride reorientation during the vacuum drying period and during the external storage period and that's what we found.

We did also some microhardness testing and what we found is the microhardness is about 240 DPH which tells you that there is no annealing that took place during the storage period. Again, nothing unexpected.

MR. MEYER: What is DPH? I'm sorry.

MR. BASU: DPH stands for Diamond Perimeter Hardness.

MR. MEYER: Okay.

MR. BASU: That's a hardness testing measure. You use a diamond cone, diamond shape. You indent the surface and you see how much deformation of the surface. That's what it is.

MR. MEYER: Thanks.

MR. BASU: As I said I'll be focusing more on the creep test because that's what we really want to know how much creep these rods have gone to in 15 years of dry storage life and how much residual creep lies ahead. So we came up with the metrics and these are seven tests. Five of these tests have already been conducted. The two that you see at the bottom, 6 and 7, have not been done and I'll come to those in a little while.

The conditions for the creep tests, the conditions were selected to represent pretty much the temperature that you would expect in the beginning of life storage, dry storage of 360 to 400 degrees.

That's where these temperatures come from.

If you take that temperature and you try to run a creep test within a finite time frame, you're not going to see any creep whatsoever. So what we did, in order to do some creep studies, we had jacked up distress to about twice or a little more than what you would experience what these rods experience in the beginning of dry cask environment.

Again, the purpose of these creep tests are multifold. We want to, of course, know what is the residual creep life in these rods. Do they have 10 years left, 50 years left so that that will give us an idea of whether we can really renew the cask license, but of course, also to generate the primary and secondary creep data so that we can use the date to develop correlations or to verify correlations that are in the code and in the model.

I'm just going to go through very quickly because these are standard creep tests. There's not much to explain here. The 3-inch specimens, the cladding segments for the fuel and then refuel with zirc pellets and the specimens were pressurized with Argonne gas. The pressurization system has the capability to pressurize up to 6000 psi which translates to something on the order of 330 megaPascal hoop stress. Okay?

Excuse me. My throat is drying up.

It's a fancy regulated system that Argonne has which can regulate pressures up to Class 1 of 10 psi.

CHAIRMAN POWERS: I'm impressed.

MR. BASU: It is impressive. Unfortunately, I don't have a picture to show you here, but it looks fancy.

I have a picture to show for the specimens loading in furnaces to do concurrent creep testing, so we can do more than one at a time. By way of measurements, we of course to the temperature, the temperature and pressure measurements as the control parameters and in terms of the measured parameters to derive the strain and strain red. We did the diameter measurements at multiple axial and azimuthal locations. We also did length measurements to verify whether or not there is anistropy in the creep process.

Again, the dry data from the diameter measurements of hoop strain and the strain rate and strain time history --

MEMBER FORD: Why did you put it in zirconium pellets?

MR. BASU: Oh, this is to stimulate the pressure inside what is representative of what you would expect if you were actually doing testing with fuel inside.

MEMBER FORD: Even though you filled it with argon -- pressurized it with argon?

MR. BASU: Yes, because some of the energy will be absorbed in the pellet as opposed to putting all the energy to cladding. That would not be representative.

Okay, here is the 3-inch specimen that I am talking about. This is a 3-inch specimen. It's the end cap and this is the pressurization system, welded to it. It's going to an argon chamber to mitigate any contamination due to -- if this was an air or open space there would be oxidation, perhaps, so to mitigate that we have this chamber and this is the creep system or the assembly of furnaces. These are smaller furnaces which can accommodate 1 sample leech, and this is the largest furnace which can accommodate 3 samples. You can pressurize these samples at different pressures, but of course, here, the temperature for all three samples would be the same.

Okay, here's the photograph of the

diameter measurements using laser profilometry again. We have the spindle here. The sample is taken out directly from the furnace. Put in a spindle here and then you can actually move this axially in this direction to get axial measurements. You can also rotate this to get the azimuthal measurements at 20 degree increment. So you get, what is it, 18 measurements for each axial location and you get much more axial measurements.

Okay, so what do we get from that layer of performing measurements are these diametral data and what these circles, the perfect circles, if you will, show, showing are the diametral marker, one for 8 inch. One for 9, one for 20, etcetera, and these are the before diamond if you will, constructed from 20 azimuthal measurements.

And as you can see that kind of progress is the creep time progresses.

These are, of course, the average over the length of the segment, length of the specimen, and I'm going to show you what the variation of the length of the specimen, it's really not much. So we have a 10, 2, 3, 5, 7, 9 data points around the axial direction and what we do is we average the five middle ones and just discard the 10, 4, 2 on each side. So that's how you generate the data that you saw in the previous plot.

Okay, so those are in terms of measurement and some kind of data reduction and then we come up with results in terms of what we can relate to for creep and that's the average strain. That's in percent. And the strain rate that we can construct from average strain measurements.

There was no failure in all five tests that we conducted.

CHAIRMAN POWERS: Yes, but did you take No. 4 out quickly so it didn't?

MR. BASU: No. That's a good question. What happened was we kind of tricked a little bit. Didn't mean to trick you guys. Within No. 3, that's No. 3 at 400 degrees, 190 MPa for this length of time and we saw an average strain of 1.03. Of course, no failure. And then we said really, that's a very small strain and it's been a fairly long duration, so we took that out, put it back. We jacked up the stress. So what you see here for this duration, the additional strain that you accumulated is this 5.83-1.03 or about 4.8 percent, by just jacking up that stress.

If you want, it's 3A and 3B experiments. So, all right?

MEMBER FORD: Those strain rates are the average strain rates? Those strain rates are the average?

MR. BASU: Yes, based on the average strain.

MEMBER FORD: Because it's not a logarithmic creep log, decreasing the time, presumably.

MR. BASU: Yes.

MEMBER LEITCH: What would failure have been in this test, excessive strain rate? Or what would you have construed as failure?

MR. BASU: Obviously, one definition would be it pops open, but what we were obviously looking at.

MEMBER LEITCH: Obviously that, but I was wondering if you had a lower threshold of failure?

MR. BASU: Yes. If it had gone from a secondary creep or the secondary creep regime to tertiary creep regime would the strain have gone substantially up. We would consider that to be at least close to the failure.

And that we don't have.

CHAIRMAN POWERS: It sure looks like No. 4 was getting close.

MR. BASU: I can give you the bottom line. Everything is fine and dandy and nothing happened with these rods, but let me just go through a couple of slides here to show you in terms of plots, some interesting observations.

Returning to 400 C. at 198 MPa and 380 degrees C., so that's a matter of 20 degrees difference. There was obviously a significant difference in hoop strain all the time. Likewise, if you go from 190 MPa to 220 MPa, we saw again significant difference in hoop strain and again, nothing unexpected. This is what you would expect by increasing the temperature or by increasing the stress.

What was obviously not obvious to us is that by increasing 20 degree temperature, that you're going to see that much difference in hoop strain.

Then, if you combine, make some combinations of stress and temperature, you can actually get the same kind of strain for both combinations and in this case we're showing that 380 to 220 MPa is very similar to 400 degrees C., 190 MPa, similar kind of strain.

Again, what that tells us is that in the laboratory environment we can actually keep one of the two parameters, the temperature or stress, very representative of what would be the beginning of life of dry storage condition and then we can artificially increase or decrease the other parameter, but then we can come back to analytically to what we would expect to see in terms of the real parametric changes.

Where am I?

Okay, this one is what Dana, you asked me and I tried to explain what we did is we basically ran that 190 degree and -- I'm sorry, 190 degree, what is that? 400 degree, 190 MPa test up to 1870 hours or so. It was not much happening in terms of strain accumulation. Then we jacked the stress up to 250 MPa and you can see that it is going really fast. But still in the steady state regime.

This plot, only to show that the average strain that we have been talking about all along is actually pretty close to the outer diameter strain that we also measured.

Okay?

What are the conclusions? Significant creep, residual creep strain is demonstrated, even up to 15 years of dry storage. So one implication is that you can go on for another extended period of time without accumulating a lot of strain and realize, of course, the temperatures are now actually up to 15 years, even much lower than beginning of light temperatures and the pressure also.

The creep ratio, strong temperature and stress dependency and the regime tested, we haven't tested tertiary regime. We haven't been able to take anything to tertiary regime as yet, but in the steady-state regime that's the dependence.

Now coming back to the, let's see, ah, No. 6 and 7 which have not been done yet. It's not complete yet. What we do want to do 400 C., and different stress level because our Spent Fuel Project Office is in the midst of revising the interim Staff Guidance 11. The original guidance has a temperature limit of, I believe, 380 C. or 360 C. and they're looking into the prospect of actually describing a 400 C. temperature instead of 360 or 380. There's no one here now from the Spent Fuel Project Office, so I'm not sure if I'm -- I think I'm representing them okay in terms of their intent, but they can verify that.

CHAIRMAN POWERS: It seems to me that you're generating a data base with the sufficiency which you could accommodate a licensee coming in and saying well, I want to run it at 400 or some range of temperatures and you can say well, that's okay.

MR. BASU: Yes, that's what this data is showing at the moment. Of course, the other thing with the 400 degrees is the fact that at 400 degrees as we have seen from the vacuum-drying process that you have some hydrogen that will go into solution and then they will again reprecipitate and whether or not in that process there is any reorientation. We haven't see, of course, at Surry, but with Robinson rod campaign that's probably another story.

So let's see, have I gone through that? There it is. High burnup. In essence, it is very similar to the Surry campaign that we had concluded. We're going to do the fuel and cladding characterization. We have already started that. We're going to do isotopic analysis. Ralph alluded to that. We have performed annealing tests to again see whether or not there have been some annealing that took place already.

We have put a lid test specimen in the furnace for tunnel creep test and that's in July, back in July.

CHAIRMAN POWERS: You have a ways to go yet.

MR. BENNETT: That's right, a ways to go, that's right. We'll do some mechanical properties test. The material is Robinson, as I said, and 67 gigawatt day, burnup, 2.9 percent enrichment. The oxide thickness is between 60 micron and 110 microns. The hydrogen content is anywhere from 600 to 750, 800, perhaps.

The status is the analysis isotopic analysis in progress, the characterization in progress. Annealing test completed and I'll show you some results.

Creep test matrix developed. Now, when we were about to start Surry creep testing, we came up with a creep test matrix. We have done a kind of peer review of that. In fact, we had two peer reviews of that test matrix in terms of its progression to come up with the final test matrix and a lot of that actually depended on what we predict as going to be the creep's trend based on some model, some correlation and what we actually observe as we started this creep testing and then we changed or modified our course.

And this is what we have to do in terms of the development of the Robinson test metrics. The lead tests started and the mechanical testing plan.

What am I showing here? Oh, this is the annealing test results which we completed and all this table shows that there is irradiated rods with 600 PPM hydrogen. The peak DPH number that we came up with is 252 which is -- what it says is it's very close to not having any annealing. That's what it says.

What is the testing strategy? We're going to conduct two lead tests, two duplicate Surry tests which show that everything is in order. We're going to -- one has started, as I said already. Then we're going to establish test methods based on the lead test results to see whether or not we are getting the kind of strain that the models are predicting and we're going to emphasize 400 degrees. I mentioned the reason earlier and we're going to duplicate the testing technique.

I am giving you the last slide a preliminary creep test matrix. It doesn't really do justice here. It does give you the temperature, indicating that we are focusing on 400 degrees C. and of course a couple of tests around 400 degrees C. We are focusing on stress where we think that we are going to have reasonable and measurable creep strain. We don't know about the duration that we're going to subject this test to. That will be determined based on the lead test results and of course we can predict creep trend based on the current model, current correlation. We just don't know how good the prediction is going to be so this is still to be computed.

And that's about it.

MEMBER LEITCH: The Robinson rods have not been in a cast for 15 years.

MR. BASU: That is correct. That is correct.

MEMBER LEITCH: This is like a baseline?

MR. BASU: Well, if you look at the -- if you are actually wondering what the creep test matrix in this case, the beginning of life temperature is probably going to be similar to what I have shown. That's not going to change.

Now when we took these Surry rods out, after 40 years in dry storage or 50 years, the temperature was something on the order of 150 degrees, rather than 360 degrees, but our tests are based on beginning of life.

CHAIRMAN POWERS: Now the Surry specimens have been in dry cask storage for 15 years. So they've seen a fair amount of creep already.

MR. BASU: You saw the amount of creep they saw which is less .1 percent.

CHAIRMAN POWERS: Because of the stress,

presumably.

You've seen the logarithmic creep --

MR. BASU: Yes, of course. Lower stress than what the trends that we tested at, yes. Absolutely.

RZ: I heard Carl Papariello lecturing and he was outright eloquent and what he said was look, this stuff is going to go into an ISFI, it's going to be there for some indeterminate number of decades and some days some future generation of engineers is going to open this thing up and take this stuff out, without saying what one might do with it at that point.

And he didn't want it to fall apart on them. You're going to get some hook or clamp or something and pull it out and it shouldn't fall apart. We shouldn't leave a problem for another generation and he said it better than I just did, but that's the goal. And I think what we're generating is some sound data putting this on a data base. Things are okay. And there's nothing wrong with a good news story.

MR. BASU: May I have just the last word?

CHAIRMAN POWERS: Sure.

MR. BASU: This program was co-sponsored by EPRI and DOE, Office of Civilian and Radioactive Waste Management. So this is a joint program and I'd like to acknowledge EPRI. EPRI representatives are here. DOE is not here, but DOE was an equal partner in this program.

CHAIRMAN POWERS: Golly darn. Thank you. Are there any other questions for Dr. Basu?

I'm starting to lose the ability to talk and I haven't even been speaking. Sud's comment prompts me to ask did I mention that the LOCA work at Argonne was done in cooperation with EPRI.

CHAIRMAN POWERS: You did.

CHAIRMAN POWERS: Good.

MEMBER ROSEN: And that cooperation with the utilities who fund EPRI. EPRI has no money.

CHAIRMAN POWERS: I take it that this is a paid political announcement here.

MEMBER ROSEN: The preceding was a factual --

CHAIRMAN POWERS: Statement of fact.

RZ: We have a full Committee summary and if you have some direction.

CHAIRMAN POWERS: Give me time. We'll get to that. First of all, I'd like to thank all the speakers for an extremely informative sessions, excellent presentations on the part of all and it filled the Committee with information.

It comes time now for the Committee to work and I had said that the Committee should think about two questions. One is what should be presented to the full ACRS and I will cast out a preliminary agenda.

Our focus in discussions with the Committee is, in fact, on the RES research program and we have in our second question a debate on what we actually want the ACRS as a whole to do here, but I would suggest that any presentation to the Commission focus heavily on that RES program as it stands now because that's the issue that we confront right now.

I would suggest the following that we -- that I begin with an opening summary of the general issue in which I can give a thumbnail sketch of the presentations that EPRI made in this. It is not because I didn't think the EPRI work is excellent. It's that the ACRS as a whole does not have to confront that particular issue until NRR comes back with their SER on the issue.

If we tell them all this wonderful stuff that we heard today at the meeting, they will simply forget. By the time the SER comes, because as we heard from Undine, there's a fairly deliberate program to review that material underway. And I think when that evaluation report comes from NRR that would be the appropriate time for EPRI to present the material to the full Committee and perhaps even remind this subcommittee of all the material because I'm sure there would be more and better understanding that will come along at that time.

So it's just that the press of things will mean that the ACRS will simply forget and so there's no real need to do that whereas they're focused very much on the research program. Then we would ask Ralph who taking as a springboard his opening presentation to us and perhaps augmenting with synoptic presentations of some of the new results you've gotten in the area of LOCA, some of your new thinking about how to approach the RIA and some of your thinking about the ATWS, you can take a bulk of the time to get the Committee up to date on where you stand in your research program.

I think in the course of that I forgot to mention this business that you're reworking the program plan. I know you're not in a position to say what that rework program plan is, but you're going to have to mention that that's going on and give us some hint on when we will know when the new program plan becomes available.

Now this is my proposal to the Committee and you guys are free to augment this. And then it seems to me that following Ralph's program might be the appropriate time for Undine to give us a description of what you're planning to do on the review of the EPRI work. I mean I would -- you had a fairly succinct presentation of that plan that you presented here and I think that's about an appropriate level of detail which I have to give a little bit more introduction on the issue, just so they can put it in the context.

That's my proposal.

MEMBER FORD: You will give a synopsis of the EPRI program to start?

CHAIRMAN POWERS: I will start the Committee off with getting them back up to speed on what the overall issue is and in the course of that, I will -- in connection with the RIA, give a capsule summary of the approach that you outlined in the analysis, the ductility approach that you've taken and the separation you have between the coolability and the rupture limits there. Does that sound fair?

MS. YANG: Sounds fair.

CHAIRMAN POWERS: So I'll take a little more time in the introduction than is common, but I think it's approach to do so because the Committee loses track of where this issue is and in addition, as Peter will be glad to tell you, there are several new Members who haven't had the benefit of all the history in this program and what not. So I'll take a little more time to begin.

MEMBER FORD: Rosa, did you want to say something else?

MS. YANG: I would just -- maybe some clarification. There's an inconsistency in what we proposed and what Ralph talked about. From what you just said, Dana, you don't think tomorrow is the place to acknowledge that inconsistency.

CHAIRMAN POWERS: That's right.

MS. YANG: I agree with that.

CHAIRMAN POWERS: It think it's going to be difficult for me to avoid saying there's an inconsistency, but I don't want to try to highlight that right now. I want to say you guys have done a detailed analysis and an approach on this problem, given an outline of what it is that you've done and I'll say at the end of the day's presentation, Undine will talk about what NRR is doing to review that. But I don't think Ralph wants to contest what you said right now. You certainly didn't today. He simply has a different approach and he gets a little more time to outline his approach, but his is still a work in progress and that's what the Committee needs to know about.

We're in the business of advising the Commission on the viability of this and I think the time to try to get a common view on that is when we have the NRR review, the work.

MR. SIEBER: They're not necessarily inconsistent.

CHAIRMAN POWERS: They're not necessarily inconsistent.

MS. YANG: The only thing I want to point out, I agree they're not necessarily inconsistent in many aspects of it, but one of the aspects which is extremely critical to the industry which is a separation of coolability and fuel failure limit because for fuel failure you calculate the dose and we all know how to do that and we have done that. But coolability is the safety limit and that's the most important limit. And I just don't think there's any discussion yet.

Our comment on what Ralph proposed, and we have gone through a lot of discussion regarding our coolability limit, so I'm a little bit concerned about to bring that issue too much forward in the limited time because that point is of major importance to us.

CHAIRMAN POWERS: I think the fact that it's a major point, it's unavoidable for me saying that to the Committee. I just don't think I can avoid saying that, but I don't think I want to resolve it here.

MEMBER ROSEN: I don't think the Committee will have any interest in trying to resolve it either.

CHAIRMAN POWERS: They're going to draw a blank.

MEMBER ROSEN: That's right, but it will be necessary for you to say this is difference in approaches and that the significant impact of that difference.

MR. SIEBER: I don't think it's resolvable in the time that we have, number one because you're going to have to get into a lot of detail to do that and I don't think anybody is really prepared, maybe EPRI is, but I don't think the rest of us are prepared to deal with that issue to finality at this point.

MS. YANG: No.

CHAIRMAN POWERS: If we're going to get -- some time in December, we're going to know a schedule of when NRR is going to have an in-depth review and I think it's once that review comes forward that we're in a position to discuss the nitty-gritty of those issues and right now we're really working on the design of the research program subject, of course, to whatever comes out of this revised program plan that we've done not too much about, but I mean I think it will still have the same elements that we're going to hear about, RIA, LOCA and ATWS. There may be a different emphasis across that board and of course I left out the spent fuel work, but that seems to be progressing along at a nominal pace.

RZ: It would be really good if we could declare something done. And just programmatically, if had my druthers, I would finish the 1998 program and work out a new program and call it a new program. I'd go into advanced field -- and then I could say this would be a great value to us.

CHAIRMAN POWERS: I think that's great. You're stuck with the fact that these tests with irradiated fuel don't conform well to management's schedule. And I think we have to live with that. I think the Committee's interest in knowing what's going on -- by the way, Ralph, the Committee will be very interested in the CABRI test matrix. You didn't put it up in your presentation, but I think Rosa had a very nice slide in her presentation, something akin to that.

Perhaps when you're discussing what's going to be accomplished at the end of 2003 with an analysis in 2004, and then you can show the follow-on confirmatory tests and what not. I think the Committee is very interested in this because we did years ago write a letter endorsing that cooperative agreement and like to know where they're coming along.

My proposal. Now the second question is whether we should write a letter here and at this point I'll turn to Peter and say you have an alternative to writing a letter on the research program at the end of this meeting.

MEMBER FORD: Yes. You started off the meeting, Dana, by saying that there would be a letter because the assumption was that this particular topic would not be in the scope of the ACRS report on the RES plan for advanced reactors.

In writing out the scope of that report, I put it that we really should be looking at where we will be in 20 years time in terms of the reactor fleet. My guess is we'll have our current reactor fleet upgraded, obviously, and license renewed. In all likelihood from the risk perspective, advanced light water reactors coming potentially on line and maybe we might have a gas cool reactor. That's a real stretch in my view.

But regardless in the time period that we have in 2003-2004 working period, if we just look at the time lines, you've got a huge gap. You've got an overlap. The research that you guys have got to do with respect to some of the advanced light water reactors and especially gas reactors, and then the industry has to make some commercial decisions.

So in that time period, the fuels, for instance, high burnup fuels, MOX fuels have kind of limited my experience with this, but there must be some areas which are on-going in our current programs and the advanced reactor program which have to be done on a priority basis right now as it impacts where we will be in 2020, 2025.

MR. SIEBER: Well --

MEMBER FORD: Just to finish up, Jack, I think that's why some of this project that we talk about, high burnup fuels, is relevant to the advanced reactor coolant. That's my suggestion being that some of this work is appropriate for the ACRS report on the advanced reactor program.

MR. SIEBER: Maybe I could comment a

little bit on a couple of things. If I looked at future reactors, it seems to me the work is being done now for the current fleet is applicable to advanced light water reactors. This appears that way to me.

Gas cooled reactors is not clear to me whether they'll be deployed or not and if I look at the roadmap for June 4, deployment is 25 years in advance and so starting something next fiscal year for any of those concepts is probably premature.

On the other hand, I think that we have to recognize that they're out there and be prepared at least with some conceptual plans as to what research should be about to put our arms around any one of those concepts.

I'd like to get back to the issue of what gets said to the Committee. One of the artifacts that has been laying around for several months is RepNa-1 test data which caused some excitement and I think it would be worthwhile to say a sentence or two or at least consider saying it that the data that came out of that was, isn't considered to be an outlier and I think that there is a firmer basis to establish conservative limits without saying that this is a valid data point.

And I think you can take it or leave it, but we made a fuss about it at one time and I'm sure that it will come to others' minds if it comes to mind.

CHAIRMAN POWERS: Yes. I mean, it seems to me that in the EPRI presentation that Rosa made there was a discussion of rather elaborate efforts that they'd been going to try to understand this test. I would certainly bring that up in a summary presentation.

MR. SIEBER: Great.

CHAIRMAN POWERS: And I would say their conclusion is that this is probably an outlier or difficult to explain.

Ralph, in your presentation you might want to think about putting in just a slide or two, say a slide or at least a line on a slide that outlines Hee's point, Hee Chung's point and you indicated that you, too, are prepared to say that this is an outlier, that doesn't have to fit all the correlations here and I agree with Jack. There are two things that have impressed me today as take home lessons. One is there is a burnup dependence to the enthalpy the fuel will take and that there seems to be an agreement that RepNa-1 is a peculiar test. That seems to be a point of agreement that is significant to my mind.

MEMBER ROSEN: I have one other thing that I think I can take away and that is the information in the dry cask storage. I think that is something that should be mentioned.

CHAIRMAN POWERS: Yes, I've left that out. Sud, did you want to say something to the Committee?

MR. BASU: I want to remind you that this was the medium burnup work. I think all of your other presentations were high burnup, so I did not know how you plan to -- I don't know how you plan to couch the medium burnup work, but I think there is a value to this work in the sense that we are going to follow the same procedure, same testing methods and the campaign would be pretty much the same. So we are going to generate some high burnup data soon.

MEMBER ROSEN: But notwithstanding the fact that you've got to go on and do high burnup work. I think the results you presented today are valuable for the Committee to know that there has been an organized look at some fairly long stored medium burnup fuel and that the results are nominal.

MR. SIEBER: Just make the cask 10 inches longer --

CHAIRMAN POWERS: The Committee, the Planning and Procedures Committee has only given us an hour and 25 minutes and I'm trying to avoid having people racing up here like scared deer --

MR. BASU: Dana, I don't have to make a statement in the meeting.

CHAIRMAN POWERS: Could you perhaps arm Ralph with two or three vu-graphs so that he could just give a capsule statement on the existence of the work and indicate that it's going on.

MR. MEYER: I have those already captured.

CHAIRMAN POWERS: Maybe that will be useful to begin because I agree very much with Steven. That's not a usual thing for the two of us to agree.

MEMBER ROSEN: I promise not to do it again.

(Laughter.)

CHAIRMAN POWERS: And what you're essentially coming back to so far -- okay, Ralph, I can count on you capturing that because I agree with Steve, that that's a significant point.

Are there other comments to be made?

MEMBER FORD: I still didn't hear a conclusion about whether we have a letter or not.

CHAIRMAN POWERS: Here's what I would propose the Members of the Subcommittee to do. I'll ask you to think about it tonight and give me some advice tomorrow on whether we'll write a letter and regardless of what your position, if a letter is to be written, any points you think are to appear in it, what not.

My tendency is to go ahead and write a letter on this program, because I think it has some visibility with the Commission. I think there's been some substantial investment in it. I think that it merits comment.

Right now, I think those comments are fairly benign in the sense that they say progress has been made and is being made and stay tuned. I don't think I have outstanding advice to give the researchers on how to do their work better. I don't think that there are any major changes in the direction here, but I have a tendency to think that this has -- there's enough money invested in this program that has enough visibility because it's a highly cooperative international program that we ought to tell the Commission something about it, so that they're aware of it. That's my general feeling.

If it seems appropriate to add more material into the overall research program, I think we can do that.

MEMBER LEITCH: Just a couple of points

that I had and I think in your synopsis of the EPRI presentation, certainly discuss that there's a burnup dependent failure limit. I guess what I think I heard today is that it may actually be more correct to say there's an oxide film thickness dependent failure limit, but burnup is more easily measurable circuit for that perhaps.

CHAIRMAN POWERS: Well, I think it's a ductility argument that's being advanced and in truth, I think that's why Jack sees there's not a great controversy between the two because I see Ralph talking about things that smack of ductility here as well.

MEMBER LEITCH: The other thing I heard that was interesting. There was an allusion to a future presentation on the Robust Fuel Program. I hope that doesn't get lost in the shuffle some place. I think we need to --

CHAIRMAN POWERS: Rosa and I have agreed that some time after the first of the year, but we'll talk on the phone.

MEMBER LEITCH: Okay.

CHAIRMAN POWERS: There are two things, it seems to me, I think there's a lot in that program and so I'm wondering if it shouldn't have a subcommittee meeting to hear all about it, some time immediately before a full Committee and give the full Committee a synoptic picture of that whole program.

MEMBER ROSEN: I think it merits a subcommittee meeting all by itself.

CHAIRMAN POWERS: It's a big program that's been going on and I know Rosa is not very enthusiastic about it and never thinks very much about it, but I will implore to come give us a few words.

MEMBER ROSEN: She also knows if she comes to speak about the Robust Fuel Program for a whole day she can bring some supporting cast. She doesn't have to do it all by herself.

CHAIRMAN POWERS: I was going to see her do it by herself.

MEMBER LEITCH: And just one other comment, maybe it's more in the form of a question for Ralph, your second slide was titled "Original List of Issues." And I'm not sure of the research plan that you're working on. Are there different issues or are we just further refining the resolution of these original issues? It's not clear to me whether they're new issues related to high burnup fuel that are going to surface.

MR. MEYER: I don't think they are new

issues of that nature. There are, of course questions about alloy effects for M5 and ZIRLO which are not addressed in the current wrap up of the old issues, but which are to some extent being planned in the program. And those haven't been laid out in terms of just what are we going to do and what are the schedules for that. So that will constitute part of the new program plan, but not necessarily represent any new issues.

MEMBER LEITCH: So there might be an additional issue or sub-issue related to cladding materials?

MR. MEYER: Yes, related to the cladding materials. Yes.

CHAIRMAN POWERS: Well, I think we just have to stay tuned for this new program plan. I got the impression in the opening remarks that this is very much a work in progress and maybe the progress has just been initiated or something like that.

MR. WERMIEL: It hasn't just been initiated, Dr. Powers, but it is a work in progress.

There's been discussion between the two offices, actually three offices, because it is going to include the NMSS piece as well and those discussions have been going on for several months at least, but we still have certain things that we're trying to clarify and clear up.

CHAIRMAN POWERS: I think it's just premature for the ACRS to try to inject itself into this debate.

MR. WERMIEL: I think so, too.

CHAIRMAN POWERS: Any other comments people would like to make?

Again, I really want to emphasize to all the speakers that the presentations were excellent. They were filled with information and I envy you all. It looks like fun work and challenging work to sort these things about.

I have to admit that I was just stunned at the amount of work that must have been done, the EPRI work because Robbie would get up there and say well, here's a point and we did this with multiple computer code calculations and things like that and he had 85 points like that, data. So I know there's a huge amount of work there. Similarly, Ralph, you and Harold, I know that each of your data points is obtained with a great deal of pain and frustrations and problems, so I very much appreciate you sharing with us and Undine, I wish you well on your review plan.

(Laughter.)

And with that I think we can adjourn this subcommittee meeting with the imposition that all the Members should think about the points that should be raised in the letter on the research program and your advice on whether it's appropriate to write one or not. With that, I'll adjourn this meeting of the subcommittee.

(Whereupon, at 5:32 p.m., the meeting was concluded.)