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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

REACTOR FUELS SUBCOMMITTEE

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MONDAY,

SEPTEMBER 29, 2003

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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. Dana A. Powers, Chairman, presiding.

COMMITTEE MEMBERS:

DANA A. POWERS	Chairman
F . PETER FORD	Member
THOMAS S. KRESS	Member
VICTOR H RANSOM	Member

ACRS STAFF PRESENT:

RALPH CARUSO

ALSO PRESENT:

MIKE BILLONE

YOVAN LUKIC

RALPH MEYER

JACK ROSENTHAL

JEFF SCHMIDT

JOHN VOGELWEDE

I-N-D-E-X

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

(8:32 a.m.)

CHAIRMAN POWERS: Let's bring the meeting to order now.

This is the meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Reactor Fuels.

I'm Dan Powers, Chairman of the Subcommittee. Subcommittee members in attendance are Tom Kress, Vic Ransom, Peter Ford.

The purpose of today's meeting is to discuss ongoing activities in the Office of Research related to reactor fuel and to hear from the industry about methods to produce crud on reactor fuel and lots of other things, I hope, too.

Tomorrow we'll hear from the Electric Power Research Institute about the robust fuel program. The Subcommittee will hold discussions with representatives and the NRC staff and with industry regarding these matters. The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions, as appropriate, for deliberation by the full Committee.

Ralph Caruso is the designated federal

official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of the meeting previously published in the Federal Register on September 15th, 2003. Portions of tomorrow meeting will be closed for discussion of proprietary information.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice.

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

We have received no request from any member of the public for time to make an oral statement.

What I will caution the members about is one of the primary objectives of today's session is to really understand where the fuel program is going, not just for the next year, but the future. So when it says in the agenda that we'll have members' discussions, I think it says that specifically on Tuesday's session, but I guarantee you at the end of this session I'm going to be

asking the members to not only tell me what their thoughts are, but to volunteer to write up proposed positions on those thoughts.

Okay. So you might be prepared for a little bit of discussion at the end of the day, and that we may have to decide if we do additional leg work in order to get things ready for the report on reactor fuels in the research program.

Any members have the opening comments they'd like to make about this?

(No response.)

CHAIRMAN POWERS: I will say that the reactor fuels meetings that we have about once a year do have a reputation for being technical meetings with lots of exchange. So I encourage members of the Committee, the Subcommittee, and members in the audience to feel free to participate.

The one ground rule for participation is you have to speak to a microphone, and you have to tell me who you are and speak with sufficient clarity and volume so that you can be heard by me, and as I get old, that means you have to speak with a lot of clarity and volume, but do feel free to participate. The Committee is anxious to understand where we're going.

We don't have Subcommittee meetings for the fuels program very often. So having an understanding, making sure that we understand things clearly is very important to us at this time.

Well, if there are no other comments to be made, I'll turn to Jack Rosenthal to give opening remarks and a status report.

MR. ROSENTHAL: Jack Rosenthal. I'm the Branch Chief of the Safety Margin Systems Analysis Branch, the Office of Nuclear Regulatory Research.

In 1998, the staff provided the Commission with a program plan which identified the issues that are shown in the one slide on the wall. That was -- I'm sorry. And then this chart is right out of the August 21st, 2003, updated of the program plan which was provided to the Commission.

I just want to point out some salient points. We're on, I think, a reasonably fast track for resolving the reactivity insertion issues and LOCA for high burn-up Zircaloy clad, Zirc-2, Zirc-4 clad fuel, with reactivity insertion position coming from research to NRR at the end of this year.

About a year ago when we were looking over the data or the few data points that we'll get from Cabri and many data points from the Japanese,

NSRR, which are not for fuel temperature conditions, we realized that we would not be able to just put data points down on a piece of paper and draw a line through them for the purposes of reactivity insertion events, but that we would have to adjust the data points to some common basis.

And that means that we had to develop an analytic method, and Ralph Meyer will be telling you about his thoughts about how he could move to points around to a common basis, which is new.

And we had to extensively use FRAPTRAN, our fuel transient code, to help us with that effort.

LOCA, we're proceeding with testing of Zirc-2 and Zirc-4, and I think that that program is well underway, and there's been first of a kind ever testing of high burn-up fuel, and we should be proud of that.

In the future, most of the clad will be ZIRLO or M-5, and we'll leave --

DR. KRESS: When you say high burn-up fuel, what exactly? Seventy, 65?

MR. ROSENTHAL: Sixty-two megawatt days per metric ton is our target. The actual fuel is a few megawatts higher, 70.

DR. MEYER: This is Ralph Meyer from the Research staff.

Let me just clarify. When we say "high burn-up fuel," what we're talking about is anything above about 40 gigawatt days per ton. Now, we have a current limit on the approvals that have been given by NRC that sits at 62 gigawatt days per ton average for the peak rod (phonetic). There are efforts underway to extend that out to about 75 gigawatt days per ton average for the peak rod.

And in general, the data that are being taken in these programs cover a range that's sufficient to go up to the 75, although some of our activities are specifically limited to 62. I'll try and make that distinction a little later on.

DR. KRESS: Okay. When a core ends up having that kind of burn-up, it will only occupy maybe one third of the core at any time at that level, something like that?

MR. ROSENTHAL: We think three or four batch fuel, right?

DR. KRESS: Yeah.

MR. ROSENTHAL: Okay. Just to pick up the flow, so my point was that for ZIRLO and M-5 clad, future clad to be tested in out years, that

will be a major effort, and we will surely need cooperation with industry to achieve that.

We've done some work on dry storage, which although may seem mundane, putting the stuff -- pressurizing it and heating it and leaving it for a while and looking at strain, in fact, that work is very, very important for dry storage campaign because it's showing that a fuel stored after 15 years and taken out has seen virtually no degradation, and we briefed the ACNW on that plan. They were quite pleased to see some data.

It's for 15 years of storage, but, it's very encouraging. And what's so nice is that it puts it on an experimental basis rather than on --

DR. KRESS: Did you skip the source term and the core melt progression item?

MR. ROSENTHAL: I did.

DR. KRESS: It says it's resolved, as best I can read the slide. What does that really mean?

DR. MEYER: Yeah, it's Ralph Meyer again.

What that means is that for burn-ups up to 62 gigawatt days per ton, the staff has taken the position that the source term in NUREG 1465 is

adequate. That's what "resolved" means in that case.

Now, you'll see the footnote or the asterisk on this table. In most or all of these areas where specific issues as they were identified have been resolved, there still is some ongoing work in order to either improve the accuracy, move burn-ups further, or something of that sort.

DR. KRESS: Okay. That was basically what I was interested in hearing.

CHAIRMAN POWERS: Is this resolution written down?

MR. ROSENTHAL: Yeah. Well, we published. In 1965 we published the program plan.

DR. MEYER: A summary of everything that I just said is in the recent Commission paper. It's August 21.

CHAIRMAN POWERS: That is where this resolution in the source term is written down?

DR. MEYER: It summarizes that resolution in that document.

CHAIRMAN POWERS: Does that resolution show that, indeed, the accelerated release that has been seen in some experiments of volatile fission products is consistent with the timing in 1465?

DR. MEYER: No. This document does not go in that level of detail.

CHAIRMAN POWERS: And where do I go to find the thinking that went into saying 1465 is, in fact, good for 62 gigawatt days per ton?

DR. MEYER: I believe we have cited adequate references for you to track that down. I hope that's --

DR. KRESS: Was this resolution based on the PIRT?

MR. SCOTT: Yes.

DR. KRESS: And the PIRT documents are published?

MR. SCOTT: Yes. The answer is yes.

DR. MEYER: Yeah, sure. It's based on the PIRT.

MR. ROSENTHAL: So while we're proceeding well on reactivity insertion events, and I think we have a program in place, LOCA, and we will ultimately have to come up with performance based criteria that we would recommend for use in future LOCA analysis, the ATWS analysis is lagging behind the two other accidents.

For ATWS, what we need to do is to be able to predict transient fuel temperatures as a

function of time in what we believe would be a period of rapid changes and oscillations.

Step 1 is to get TRACE working, which I think we've achieved.

Step 2 is to get a 3D kinetics model coupled to TRACE, which we call PARCS, as modular TRACE, and that's been achieved.

And the next step would be to couple a fuel code into that suite of codes for the module of the code or couple codes, and with that capability, which we should start on next year, we should be able to look at the ATWS oscillations in some specificity.

Though I just want to make another couple of points. This work is very expensive, and it's highly leveraged where participating with Cabri we have agreements with the Japanese. We participate with Halden, and we think that our participation in these programs is giving us on the order of perhaps $30 million worth of worldwide research.

Our cost is roughly three FTE and five million a year, and we would expect a similar, although a somewhat declining level, to continue on, and that's it. That's it.

I have a handwritten note to mention the EPRI cooperation. Clearly, in the Argonne fuel program, the fuel has been provided by EPRI to us and providing and shipping with fuel is roughly equal in cost to the program. So it's roughly a 50-50 partnership with industry.

EPRI also participates in Cabri.

With that I think that we're ready for the first presentation.

DR. FORD: I had a question about the last item, the high enrichment which is deferred.

MR. ROSENTHAL: Right.

DR. FORD: There's no discussion of this in your August 21st plan as to the risks associated with deferring it versus the commercialization plans. What sort of risk are you taking by not addressing this?

MR. ROSENTHAL: We're going to see high burn-up -- I'm sorry -- high enrichment in IRIS, the proposed IRIS design, which is out some time into the future. I think to prepare our plans, these are mostly physics calculations to calculate neutrons and specifically cross-sections and cross-section sets applicable to the high enrichments, and we can do that reasonably fast.

MR. ELTAWILA: This is Farouk Eltawila from Research.

The reason for the deferral, there is no industry initiative to go above five percent enrichment right now. The infrastructure is not existing in the country. So there is no reason to pursue research in this area.

DR. KRESS: There's one school of thought that says the higher enrichment if you don't go too far is probably a safer condition rather than a more risky one because of the neutronics associated with it and associated with loss of coolant and the ability to -- actually in order to make the Chernobyl reactor safer, they increased the enrichment in it.

MR. ROSENTHAL: Well, they just wanted to achieve --

DR. KRESS: Just to get rid of the positive void coefficient or help make it smaller.

MR. ROSENTHAL: So they want to achieve a negative void coefficient.

DR. KRESS: Yeah.

MR. ROSENTHAL: But I think at least in my mind is the assessment that we know how to go about this work; that it's dominantly physics work;

and that we would do the actual work when there was a need.

DR. KRESS: When you say "physics," it's mostly --

MR. ROSENTHAL: Neutronics.

DR. KRESS: -- yeah, neutronics.

MR. ROSENTHAL: We have to -- you have to generate cross-section sets that are applicable.

MR. ELTAWILA: Nobody is pursuing the --

MR. ROSENTHAL: No. So what I'm saying is that we're able to do it, and we anticipate when there's a need that we would be able to do it. So in my mind the risk is small because I think we know how to go about it.

CHAIRMAN POWERS: I guess two issues come to the fore there. We need, to the extent available or possible, here in the next couple of days to understand better what physics capability NRC needs to have in its research program.

We've gotten some material on that sent to the Committee about what, three or four months ago? It looked like a very useful and reasonable program that you have for this physics work.

And if that's appropriate, just tell us because we are aware of that sort of thing.

The other thing I'd like to know a little more about is how do the activities connected with risk informing 50.46 and the Code of Federal Regulations impact what you do in your loss of coolant accident program here.

MR. ROSENTHAL: As I mentioned earlier, we're going to have to come up with performance based criteria, and I think if we just wait for the appropriate presentation we'll hear about that.

CHAIRMAN POWERS: Good.

MR. ROSENTHAL: And later in the day we can just sneak in -- well, not sneak in -- just give you five minutes on the physics probably --

CHAIRMAN POWERS: Yeah.

MR. ROSENTHAL: -- to tell you what our plans are. I'll do that.

CHAIRMAN POWERS: We just need to know -- I'm particularly interested in that area in knowing what the magnitude of activities that you anticipate you need to maintain just to meet reasonably foreseeable obligations of the agency in that area.

And, again, you've sent us stuff on this earlier, and we're aware of that material.

MR. ROSENTHAL: I can take a minute now

if you'd like.

CHAIRMAN POWERS: Sure.

MR. ROSENTHAL: Actually much of this is spurred on by our mixed oxide program where we're assuming that we need a quite rigorous position on our ability to do independent calculations, independent order calculations for mixed oxide.

For that purpose, we need to develop cross-sections for the ability to calculate power distributions, the ability to do kinetics.

For that purpose we're developing a code call NEWT at Oak Ridge National Laboratory, which will give us cross-sections. We're continuing with our work on PARCS, which will let us do spatial and time dependent calculations, and as I said earlier, that's coupled to the thermal hydraulic code.

And we're benchmarking this work to St. Laurent critical experiments. We have a good experimentally based program, and there's also quite a fair amount of UO2 data out there to also benchmark against.

And we will have the capability to independently go from evaluating nuclear data file, Brookhaven, six or seven cross-sections right through to doing a reactor calculation, and that's a

capability that we haven't had, an independent capability that we haven't had in the past.

So that work, it's ongoing. We have some capability. We're actually applying that capability at Brookhaven because we find it healthy when we actually move a code from where it was developed to still another location for application. The bumps and warts come out of it.

When we get the theory down right, this is higher order SN calculations themselves. Then the next thing will be to develop a more automated scheme to apply it because, after all, what you want for your integral calculations is cross-sections as a function of moderator temperature, moderator density, fuel temperature, burn-up, et cetera. So it's a lot of crunching.

I think we know how to go about doing it, that there isn't some theoretical hurdle, but that it's a fair -- it's just plain a fair amount of work.

CHAIRMAN POWERS: Okay.

MR. ROSENTHAL: Okay. With that, why don't we return to the agenda? And John Vogelwede is the first presenter.

MR. VOGELWEDE: Good morning. My name

is John Vogelwede. I'm with the NRC Research staff, and I'll be talking to you this morning about fuel codes and how they're used at the Nuclear Regulatory Commission.

Fuel codes have had a long history at NRC, dating back to the early 1970s. They're used to calculate things like fuel temperatures, fission gas release, dimensional changes in the fuel and cladding, and these feed into different regulatory criteria.

The first one on there, stored energy, is perhaps the best known. In 10 CFR 50, Appendix K, there's a fairly prescriptive description of how fuel codes should be used. It's quite old, and it's probably the most prominent place for use of these codes, which is to calculate fuel temperatures or stored energy of the code.

A little bit later, in the same part of the regulations, it says that in the review of the LOCA calculations, one has to accommodate other things in the analysis as well. These variables start getting very complicated.

I don't know whether you can see this clearly, but it gives you an idea of the number of parameters that go into calculation of fuel

temperature. All of these things have been done for some time.

For thermal performance --

DR. FORD: Sorry. Could you go back to that? Being somewhat new at this game for fuels, yeah, I can understand such a diagram, the concept behind such a diagram, and you say you have codes that relate to all of these interactions?

MR. VOGELWEDE: That's correct.

DR. FORD: Are those codes benchmarked against data?

MR. VOGELWEDE: Yes, and I will be showing that.

DR. FORD: And you'll be showing that?

MR. VOGELWEDE: Yes.

CHAIRMAN POWERS: This is one of these plots that Professor Apostolakis is probably particularly fond of. It does not excite me the least little bit because I believe I could take that same plot and put it on a fairly hierarchical structure with a great deal more simplicity.

DR. FORD: You see this in similar diagrams for cracking phenomena. Some of those must be high impact items --

MR. VOGELWEDE: Oh, yes, of course.

DR. FORD: -- unless you can forget about essentially.

MR. VOGELWEDE: To draw a parallel, there's roughly a subroutine in our codes to do each one of these effects that's shown in a box up here. Some of them dominant fuel temperatures. Some of them are second or third order clearly.

DR. FORD: Okay, and we'll see those algorithms.

MR. VOGELWEDE: Yes.

DR. FORD: Good.

MR. VOGELWEDE: And I will focus on the dominant ones.

DR. FORD: Good.

MR. VOGELWEDE: Both traditionally and in practice the dominant consideration has been fuel temperatures, not other things like mechanical performance. You establish a boundary condition for fuel temperatures with the coolant temperature, which is used to calculate the fuel temperatures as one goes in.

The major uncertainties in that are gap conductants. It's for a radial distribution, a one dimensional distribution that is a parabolic. At the center of the fuel because the gradient has to

be zero at the center line, that is one of the boundary conditions. The cladding coolant is the other. You can see here that there is a -- for an open gap that may not have a very good conducting gas medium in it, there's a big jump there.

In addition to that, fuel materials or ceramics are not very good conductors, and you get some fairly big temperature changes going from the coolant into the center line of the fuel.

Some of the second order effects are fission gas release. For regulatory analysis one wants to know how much release there is from the fuel to the plenum or the fuel rod into the fuel cladding gap.

Normally fuel is pre-pressurized with helium. That becomes contaminated with the noble gases that are released and degrades the conductivity. Fuel densifies when it's put in.

Years ago the densification effect was very pronounced. These days it's usually less than a percent. There's also a creep of the cladding. There is usually an over pressure from the system coolant, and it tends to creep down to the fuel.

CHAIRMAN POWERS: I noticed that you have on your slide associated with the creep also

the formation of hydrides. Do your codes calculate local hydride formation or is it just all kind of a uniform hydride?

MR. VOGELWEDE: Uniform. Local hydride formation is much more difficult. We don't get into that level of microscopic formation of hydrides, although it's very clear that they exist, and Ralph will go in, when he talks about fuel failures, to how that is taken into consideration.

CHAIRMAN POWERS: Okay, good. Dr. Kress will be particularly interested in that issue.

DR. KRESS: Thank you.

MR. VOGELWEDE: Here's some typical --

DR. KRESS: I was going to ask the same question.

MR. VOGELWEDE: -- temperature predictions from our fuel code. You can see that temperatures start fairly high. There's a slight upswing at the beginning where the fuel densifies and the gap reaches its maximum size very early in life.

Cladding then creeps down. Eventually the gap is closed so you have the best conduction between the fuel and the cladding.

Later, as fission gas releases

contaminate the gap, the fuel tends to rise. Now, in this particular case, this was done at a constant linear power rating out to about two thirds of the scale, and then the power rating was dropped down.

It's practically impossible to run a fuel out to extremely high burn-up at the same power rating. After the first two cycles, one tends to shift the burden of producing power to the fresher assemblies.

DR. FORD: Now, you said earlier on that -- this is obviously a calculation --

MR. VOGELWEDE: That's correct.

DR. FORD: -- that's crucial to where we go from here. Are there data to confirm that those calculations are correct as a function of, for instance, fuel cladding characteristics, corrosion rates, et cetera?

MR. VOGELWEDE: Yes, there are, and I'll show you some data later in the presentation where experimental data is taken the reactor from fuel with center line thermocouples for a variety of conditions, and the predictions are actually quite good.

DR. FORD: Now, when you say "quite good," in the American sense of "quite," within one

percent?

MR. VOGELWEDE: Slightly bigger than that, but relative to other predictions made in the world, I think that NRC codes hold their own quite well.

DR. FORD: What's the risk when you say slightly greater than one percent? Say ten percent. What was the risk impact for that?

MR. VOGELWEDE: For ten percent, it's not terribly bad because for LOCA analysis you normally do this for a lead rod. So you want a lead point in the code where temperatures are maximum. So there is a fair amount of conservatism built into the regulatory analysis so that the uncertainties are adequately covered.

DR. FORD: Now, will you be discussing this question, the margins and uncertainties later on?

MR. VOGELWEDE: Not very much. In the research standpoint, we tend to focus on best estimate calculations, and I'll show you some uncertainties in fuel temperature calculations, but not on the overall calculations involved.

Here's an example of some medium burn-up fuel. This is a cross-section from fuel taken from

the Surry reactor. I believe that the burn-up is about 36,900 megawatt days per metric ton here.

You can see that because it is a ceramic material, it tends to crack very, very quickly because of the thermal stresses imposed on it, surrounded by a zirconium based alloy cladding.

For higher burn-up, this is from H.B. Robinson. You tend to accumulate more fission gases. You get more stratification across the radius and the center line where the fuel is hotter. You get bubble link-up, more grain growth, and things like that.

But it's still a non-homogeneous matrix with cracks, so that in many cases the material properties that we're talking about are a surrogate for the composition including cracks and other things.

Here's some of the parameters that we need to calculate fuel performance. The dimensions and so forth of the fuel. Material properties, which are most often dependent on temperature, burn-up and other things.

We have a compendium of material properties called MATPRO that is used not only for these fuel codes, but for other codes used in

regulatory analysis as well.

CHAIRMAN POWERS: You've listed down here MATPRO, Rev. 2, and there's a flood of data coming in since 1981, and in particular, you get things coming out of the Halden program on these extended burn-ups and whatnot. Can you explain how that is recognized in the code and whatnot?

MR. VOGELWEDE: We've incorporated these data as they become available directly into the codes. We haven't done an update to MATPRO in some time.

You are correct and, I think, I will be correct for some time in the future as the new cladding alloy data becomes available, the high burn-up stuff that comes from Argonne that Mike Billone will be talking about as well.

So we incorporate this directly into the code. The only reason I'm mentioning MATPRO here is it's some kind of a baseline.

CHAIRMAN POWERS: It's a standard that's used by a lot of people --

MR. VOGELWEDE: Yes.

CHAIRMAN POWERS: -- outside the agency and within. Is there a plan to issue a Rev. 3 on MATPRO?

MR. VOGELWEDE: Not at this time.

CHAIRMAN POWERS: Is there a reason not to issue an update?

MR. ROSENTHAL: Do we intend at some point to update MATPRO? Yes, surely. And then it's just a question of competing for budget resources. Most compelling is the RIA and the LOCA work, and you just are going to compete for resources.

CHAIRMAN POWERS: We clearly understand that, but I have never seen on any planning document that says, okay, here's MATPRO update competing. I mean, maybe I've seen it and just not recognized it, but so it's not competing very well.

DR. MEYER: This is Ralph Meyer.

We did a couple of years ago actually plan for the upgrading MATPRO and developed a sort of revolving scheme where you would have MATPRO-10, MATPRO-11, MATPRO-12, which you'd keep a historical record of these because codes couldn't upgrade their validation every time you change the parameter.

And as Jack pointed out, this simply gets pushed back in favor of the more pressing needs, and right now we're running on rapid schedules on the two subjects he mentioned, and this is just getting pushed off.

CHAIRMAN POWERS: Yeah, it's one of those things that's really easy to put off because, I mean, it's not absolutely crucial at any time. But you're getting a little long in the tooth here. I mean, 24 years is probably long enough to wait for an update.

DR. FORD: If I could just follow up on, materials properties, of course, is not only the fuel, but also the fuel cladding.

MR. VOGELWEDE: That's correct.

DR. FORD: And corrosion properties.

MR. VOGELWEDE: Yes.

DR. FORD: And how they affect conductivity.

MR. VOGELWEDE: And to respond to both your question and Dr. Powers', the updates are made continuously to the code itself. The issue that he raised is reflecting this back in some kind of a comprehensive document like the MATPRO manual.

DR. FORD: Now, I read in the August 21st plan, and I can't put my finger on it exactly right now, but there is an inference that the physical model upon which the code was originally based has changed. I don't know. I can't put my finger on that particular incident.

CHAIRMAN POWERS: Which code are you speaking of?

DR. FORD: On this August the 21st.

CHAIRMAN POWERS: Oh, no. We've been discussing MATPRO, and I'm wondering what code are we discussing.

DR. FORD: I know, but I'm about to come onto this because it relates --

CHAIRMAN POWERS: Tell me what code you're talking about.

DR. FORD: The materials properties will be relevant to a specific physical failure phenomenon that you're proposing. Now, what happens as I seem to remember in this document, the physical failure phenomenon has changed. You no longer believe the original one.

Okay. I'll defer the question, and I'll look for this particular item.

MR. CARUSO: I think the question he's asking is the materials change over time. We now have ZIRLO --

DR. FORD: Well, exactly.

MR. CARUSO: -- ZIRLO-2, I'll call it, and we have M5. Do those materials get reflected in MATPRO?

MR. VOGELWEDE: Not at this time, Ralph, but they do into the codes that we're using to make these calculations. So --

MR. CARUSO: MATPRO is not a code. It's?

MR. VOGELWEDE: MATPRO is not a code in the sense that you're talking about. It is a series of articles about what material property behavior should be.

And originally we started with subroutines reflecting each one of those, and they were incorporated into the codes at that time. Those subroutines changed in the codes, and the documentation for MATPRO did not keep up to date with that.

MR. CARUSO: So it's the documentation for MATPRO that has not been updated, but the code --

MR. VOGELWEDE: That's correct.

MR. CARUSO: -- the codes themselves have been updated.

MR. SCOTT: John will get -- this is Harold Scott from Research.

When John gets to the slide that shows the reports for FRAPCON and FRAPTRAN, those

documents contain all of the information about the material properties. So it is documented. It's kept up to date as we go along, and he'll come to that slide shortly.

MR. CARUSO: Let me just get this clear in my mind. There is a MATPRO-11 document, dated 1981.

MR. VOGELWEDE: Big.

MR. CARUSO: Just a document, and that --

CHAIRMAN POWERS: It's huge. It's about that thick.

MR. CARUSO: Right. And it contains physical material properties, but it hasn't been updated, although the codes that use the information in that document have been updated to reflect new data that has been received.

MR. VOGELWEDE: That's correct.

MR. SCOTT: And that document is new NRC whatever.

MR. CARUSO: Which is the code documentations themselves.

MR. VOGELWEDE: That's correct.

DR. RANSOM: You mean they're in house?

MR. ROSENTHAL: I mean, there are

subtleties because MATPRO is used other places, like one of the thermal hydraulics codes, but this is a fuel meter.

MR. VOGELWEDE: I'm sorry?

DR. RANSOM: Is this report being done in house or do you have contractors? You mentioned Brookhaven applying the codes. Are there other people that maintain and are doing this upgrade work or is this internal?

MR. VOGELWEDE: Yes, and I'll get to that in a moment.

DR. RANSOM: Okay.

MR. VOGELWEDE: What I wanted to say is that input parameters that one uses for these fuel codes is, for example, power history has to come from neutronics or actual in core data, and these are not stand alone operations.

This is kind of an interesting one. We found that at least three quarters of all of the problems that we've had with running the fuel codes tend to be errors that are made in the input. The codes aren't that friendly at the moment.

But the typical problem is somebody attempting to put in a fuel dimension of eight millimeters and actually has eight meters, and

things don't work out correctly.

NRC has two major fuel codes, FRAPCON, which does steady state analysis. Here's the documents that are used for that. They're fairly up to date, just in some cases about a year and a half old, and FRAPTRAN, which does our steady or transient analysis.

These codes at the moment are maintained and supported by Pacific Northwest National Laboratories. We also have a number of international users who use the codes, and we've documented input from them as well where they've made suggestions and updates on their own.

We have a fairly extensive peer group program supported by a Web site, annual meetings, and formal reports.

FRAPCON 3.2 is our current steady state full performance code. It calculates fuel performance that can be measures in hours, days, weeks, months, things like that, even years. It's basically a best estimate code. In addition to temperatures, it does do fission gas release, mechanical analysis, and things like crud build-up.

FRAPTRAN is our transient code. It does a lot of things in parallel. It's used for things

that are minutes, seconds, milliseconds in duration. A good example is the reactivity initiated event.

We also do other things, fuel performance during the loss of coolant accident. FRAPTRAN has a fairly sophisticated cladding, ballooning, and rupture model in it.

Here's an example of an RIA, which a little bit complicated. The red line represents the power which is a few tens of milliseconds in duration. You can see the fuel surface temperature, which is the green line actually peaks and is higher than the center line for a short period of time.

So rather than this profile that I gave you originally, which showed the maximum fuel temperatures at the center line, this can change during transient analysis.

Here's a number of models which are common to both codes. Both of them do fuel temperatures. We have sort of one and a half dimensional temperature analysis.

The radial analysis is the most detailed, but also we can do temperatures up and down the length of the cladding. This is mostly a function of the axial power profile.

DR. RANSOM: Do these codes include this

effective ratcheting that they used to talk about? The clad locks up with fuel and --

MR. VOGELWEDE: It does. It does, but the ratcheting model is mostly driven by thermal expansion of the fuel once the fuel and the clad have locked up.

There is experimental data for both circumferential strains and for axial strains as well, from in-pile data that we attempt to model, and some of that is shown in our integral assessment reports.

DR. FORD: Could you go back one slide, please? I found the reference to what I was referring to earlier on. If I could just quote from your August 21 thing, this relates to RIAs. "Test results have shown that cladding damage in high burn-up zircaloy fuel occurs in a partially brittle manner as a result of the mechanical expansion pellets rather than by dry out and over heating of the cladding as addressed by the current criteria."

That is to what I was referring. A different physical phenomenon giving rise to the failure, are the materials properties currently needed reflected in that change of understanding of the degradation mode?

That was the reason for my question.

DR. MEYER: Well, this is Ralph Meyer.

The answer is a little bit yes and a little bit no. The code isn't capable of doing a straight up calculation for the failure of all of these, but we're using the code in a roundabout way to accomplish this, and that's really the subject of my presentation which follows this.

DR. FORD: Okay.

DR. MEYER: So you can bring this up again when we're talking about the details.

DR. FORD: Okay. Thank you.

MR. VOGELWEDE: Here's a number of sources of data that we use. This can include both in and out of pile data. Here's an example for fuel center line temperatures. All of the data shown on this particular slide are from the Halden reactor in Norway. It's all instrumented fuel assemblies. So there's a center line thermocouple. All of these are mixed oxide.

The results are as good or better than what everybody else does in the world using the same openly available data. Now, you can see it at higher power ratings, which is --

CHAIRMAN POWERS: Are Halden data really

openly available?

MR. VOGELWEDE: Eventually, yes. For participants in the program, usually it's released to them first, but ultimately most of the information becomes publicly available and can be used.

We're reasonably pleased with this level of uncertainty, although it may seem larger. At higher power ratings, between ten and 12 kilowatts per foot, it becomes more and more difficult to do this, but this is as well as anybody else does.

DR. KRESS: Is this FRAPCON predictions?

MR. VOGELWEDE: This is FRAPCON.

DR. KRESS: And the colors are different burn-ups?

MR. VOGELWEDE: The colors are different assemblies.

DR. KRESS: Different assembles.

MR. VOGELWEDE: Different experiments in Halden.

DR. KRESS: What burn-up level do these get to?

MR. VOGELWEDE: Harold, do you know on this one? I believe they went out to about 25 or 30,000 megawatt days per metric ton.

MR. SCOTT: John, this is Harold Scott again.

Another technique that Halden uses is they take rods that come out of reactors. They then drill a hole down the pellet and put a thermocouple in it. So there may be a few data points there that are higher than 40 or higher than 25 for MOX. I think they actually have a couple of assemblies that were previously irradiated.

DR. KRESS: Well, how is it they vary the center line temperature? They vary the power of the reactor?

MR. VOGELWEDE: Yes.

DR. KRESS: Just where they put the assembly?

MR. VOGELWEDE: Yes, and not all of the data points are shown here. You get data points that were ten minutes or weeks on end. So it's fairly easy to accumulate a large amount of information.

I'm not sure that you can see this very well, but this is the radial power distribution for both codes. It has a fairly sophisticated flex depression model in it based on experimental data. In this particular case, they use neodymium as a

tracer to determine the burn-up across the radius. It's very, very sharply peaked at the outside radius of the fuel, often two to three times the pellet average.

So for an RIA event where you dump a lot of power into the fuel, it's preferentially dumped into the periphery, this outside rim of the fuel, and becomes a very strong effect for accident analysis, but again, this is experimental data compared to that particular module in the code.

And this is also another case of something that we put into the code and is fairly well documented, but did not show up in MATPRO in its original incarnation.

Research is not the only office that uses the fuel codes. NRR uses the code for auditing in some of its reviews. NMSS uses the fuel codes to determine end of life rod pressures and void volumes. You do this by running the code out following its power history in the reactor and then cooling it down to room temperature and pressure conditions.

We also tried to encourage this in our Office of Research. Recently we held a two-day training session for NMSS and NRR to teach them how

to use these codes. I see several training participants in the room today.

Internationally, we have 29 member organizations in our user group. There's 15 countries represented. We have fairly extensive peer review of these codes, a lot of nice feedback.

We have periodic meetings. Our most recent one was at Argonne in July. We have a Web site use URL is given on this page.

We have extensive international use of the codes, and the reason I've listed these names here is in most of these cases we have reports that have been issued either cooperatively with the NRC or by the member organization on use of the code, suggested improvements and things like that.

DR. FORD: Before you go on, this is a question that has come up, the use of other codes, some hydraulic codes, et cetera. You have a code which is being used by quite a few people, and yet EPRI has another code and NMSS had another code. Who's to say which code is correct? Is it strictly a question of how well it predicts the observations?

MR. VOGELWEDE: In many cases, yes, that is correct.

DR. FORD: And so there's an exam, is

there?

MR. VOGELWEDE: We tend to try to encourage the case where NRC can use a code when it's doing regulatory review of another one. So, for example, EPRI's FALCON code, which is currently in for review now, we have an NRC code which can be used to double check.

DR. FORD: But it does come down to a question as to which predicts the observation the best.

MR. VOGELWEDE: I think so, yes.

DR. FORD: Is there a situation when a FALCON code is better than the NRC code?

MR. ROSENTHAL: It's under review.

DR. FORD: Okay.

MR. VOGELWEDE: To get to your point of whether or not NRC's codes are good or not so good, we came up with this report card for our codes, on the left-hand side for the steady state version, on the right-hand side for the transient version, and we arbitrarily assigned letter grades to things.

So, for example, for steady state thermal performance, we have an A or we have given ourselves an A for this because we believe that our ability to predict experimental data is pretty good,

as good as anybody else.

In some of the other areas, let's go down to the bottom. For fuel assembly and channel effects, this is a single rod fuel code which doesn't have the capability to do that modeling-wise.

So it isn't a question of whether or not it does good or bad. It can't do it at all. Because of this, we have through a cooperative agreement with Finns, have incorporated a single channel code called GENFLO, which we use with FRAPTRAN to simulate some of these effects. So using the two codes in tandem helps us to accommodate that.

In the same sense, we don't have the ability to do neutronic type effects, and Jack already talked to you about Research's efforts to use other codes in combination with one another so that they could do all of these calculations.

DR. FORD: You showed a very complex interaction diagram very early on, and you also indicated just previously that fuel and cladding chemistry was an F or D. Is that a fatal flaw?

MR. VOGELWEDE: I don't believe so. It is important for some of the newer things that we're

looking at, such as cladding failure, but for fuel temperatures and fission gas release, which are the traditional end products of these codes, it is not. So it depends on how the code is being used.

DR. FORD: And yet you say in one of your documents that partial brittle failure of the cladding is one of the prime reasons for an IRA failure, and I would have thought hydrogen embrittlement would, therefore, have played a large part.

MR. VOGELWEDE: Yes, and Ralph will get into that in his presentation.

DR. FORD: Okay.

CHAIRMAN POWERS: Let me ask you a couple of questions about that slide. I see in the literature a lot of discussion about directed diffusion of gas bubbles along vacancy gradients. Do you model that in FRAPTRAN?

MR. VOGELWEDE: No. The fission gas release is fairly straightforward. We're looking for an inventory and release from the overall structure. How this is handled as far as migration to either grain boundaries or something like that is an effort that is done, for example, in the ANC subcommittee, which we participate in, but that's

not yet incorporated into the codes.

So for general releases, I don't think it's a major issue, but for transient analysis, we're going -- now, I think your question led to things like the rim effect, how it behave during the transient.

There's a lot of work on that. We don't have that in our codes.

CHAIRMAN POWERS: What I was really driving at is I think your codes for on the area of fission gas release are crude relative to the level of understanding that's evolving --

MR. VOGELWEDE: Yes.

CHAIRMAN POWERS: -- about this, and what I was driving or ultimately going to drive at is the technologies that you've adopted in these codes are the product of an era that's perhaps 20, 25 years old now, and you've upgraded them to account for high burn-up effects, such as the rim effect and whatnot, but you've held that structure. The computational structure, the phenomenological structure is being held fixed, and basically what you're doing is updating some features of it.

And what I wanted to ask is, okay, is there a point at which you say, "Fine. That was

good and it worked well, but now we'll go to a different phenomenological formulation altogether"?

MR. VOGELWEDE: Yes, I would agree with you there. The tradition for these codes, as you point out, is decades old. It has been primarily focused on traditional transient and accident analysis used in the safety analysis reports and not in some of the newer regulatory applications that we're talking about now and I agree with you on.

CHAIRMAN POWERS: Let me come to another one. The topic is fuel clad materials properties. I have received a copy of a letter from NEI to Ashok, in essence, questioning the methods by which --

MR. VOGELWEDE: Yes.

CHAIRMAN POWERS: -- we collect data on the structural properties of alloys, et cetera. Can you comment on that?

MR. VOGELWEDE: We have received the letter. We'll be talking about how that data is currently collected and the impact of the EPRI letter later on in today's presentations.

CHAIRMAN POWERS: Okay.

MR. ROSENTHAL: Yeah, at the time of the ECCS rulemaking the Commission settled on a non-

ductility, no ductility criteria, and the question before us right now is: should we change our fundamental thinking and go to a toughness criteria in the proposed test?

We'll be discussing it at length in the course of the day, and I think that I'd say our mind is still open about how to proceed.

MR. VOGELWEDE: Any other questions? I'm finished.

CHAIRMAN POWERS: Any other questions for the speaker?

(No response.)

CHAIRMAN POWERS: Well, thank you. You gave us a good introduction to the issues of FRAPCON and FRAPTRAN.

Dr. Meyer, you're going to discuss RIA issues.

Dr. Meyer, I just can't avoid commenting that the last time you put up the paintbrush plot in one of these Subcommittee meetings it precipitated about two hours of discussion.

(Laughter.)

CHAIRMAN POWERS: And I thought you had vowed never again to put that slide up, but I notice that it's in the package again.

DR. MEYER: It's there. It's there.

CHAIRMAN POWERS: Should I anticipate another two hours of discussion?

(No response.)

DR. MEYER: Okay. Help. I've got it. Okay.

All right. So I want to move now from the very general to the very specific and talk about how we're attacking the RIA problem with an empirical method to determine the cladding failure threshold, and to use that failure threshold to demonstrate that we can avoid losing coolable geometry or generating big pressure pulses, which are the main objectives in surviving this accident in a benign way.

Is there a lapel mic? I'm sorry. I'm taking just a few minutes to get going.

CHAIRMAN POWERS: Perfectly okay. I'm not agonizing over the schedule because it's a Subcommittee meeting.

DR. MEYER: Yeah. This presentation will probably take a little longer than scheduled. We've trimmed back in some other areas. I think we'll come out okay at the end of the day.

CHAIRMAN POWERS: If there's a logical

break in it, Ralph, you might want to just signal me about that, and we'll take a break in the middle of it if it's going to run very long.

DR. MEYER: Okay. Now, I think I'm going to stand up and try and do this.

And do you have a pointer?

CHAIRMAN POWERS: Well, you're just very demanding. That's all there is to it, Ralph. You're a high maintenance individual here.

(Laughter.)

DR. MEYER: Okay.

CHAIRMAN POWERS: And now you want batteries, too.

DR. MEYER: Okay. So this is the outline, and I'm sure you've read that by now.

The issue is that there has been a change in failure mechanism as we move from unirradiated to irradiated and particularly heavily corroded material. The initial database was taken on very low burn-up fuels and irradiated materials. It presumed that the failure mechanism was related to high temperature and oxidation.

And based on that, we had arrived at a 280 calorie per gram limit. We acknowledged two decades ago that that was nonconservative by 50

calories per gram. Because of a mistake in interpretation, it should have been 230 calories per gram, but it really didn't matter because, in fact, we believe the real achievable fuel enthalpies were down under 100 calories per gram. So we didn't bother to make any correction.

When we now look at data from high burn-up fuel in test reactors, principally in France in the Cabri reactor and Japan in the Nuclear Safety Research reactor, NSRR, we see cladding failure at a much lower enthalpy than that, and in many cases those cladding failures are accompanied by a prompt disbursal of fuel particles into the coolant, which can lead to some undesirable effects.

So we saw a need to make a change in this 280 calorie per gram number, and in particular, the issue that we described in the earlier high burn-up plan was to make some confirmatory assessment that was good up to at least 62 gigawatt days per ton, the current limit, to show that everything was okay in operating reactors at that time, if indeed that was the case.

And we believed that was the case, and we still believe that was the case, and we're going to do that.

Now, I'll get to the paintbrush slide in a minute, but there are problems with the database, and the problems boil down very simply, are that the two machines that are generating data are not producing conditions that are sufficiently like PWR conditions, and so they're giving biased results, and our goal with this scaling method and in this presentation is to show how we're going to accommodate that.

In the Japanese test reactor, you have a natural pulse width of the machine that's about half the pulse width that we expect for this range of energies in the PWR, and also a test temperature that is way off. The NSRR tests to date have been done in room temperature capsules. They are building a high temperature, high pressure capsule. In 2005-2006, we'll start taking some data at high temperature.

So you've got two things wrong. You've got a pulse width that's only half what it ought to be, and you've got a test temperature that for PWRs is way off.

And the Cabri reactor, that's a very controversial subject, and members of the controversy are right here in this room. But they

have unfortunately chosen to broaden a perfectly good pulse and make it a perfectly no good pulse. And so they are now taking a nine and a half millisecond pulse, which would just be great, and artificially broadening it to 30 milliseconds based on a misunderstanding that we all had a few years ago, but which has subsequently been corrected.

So that's the problem. So we've got a database that has some atypical conditions, and I think I can deal with that using our code and some --

MS. YANG: Excuse me. Can I make a comment?

DR. MEYER: If the Chairman wishes to entertain it.

CHAIRMAN POWERS: Anxious to hear what you have to say.

MS. YANG: Thanks, Dana -- Mr. Chairman.

Can I back to your last slide, please, Ralph?

I want to say for the PWR condition, the rod ejection accident is a hypothetical event, and even give the most conservative calculation, we don't get ten millisecond pulse. The PWR typical pulse is greater, a lot greater, than 30

milliseconds.

And I think at the last ACRS meeting a year ago we have talked about that. I think that was well documented in the transcript of the meeting. I think that's the PWR condition.

That's why with the international community debate and very thorough discussion, the Cabri test reactor pulse was changed to greater than 30 millisecond to better represent the PWR condition.

DR. MEYER: Let me give you a couple of numbers. In a PWR, a pulse with an energy of 20 calories per gram will have a pulse width of about 40 milliseconds. A pulse width energy of about 40 calories per gram will have a pulse width of about 20 milliseconds.

And as you go on up to 100 calories per gram from 40 calories per gram, you go from 20 milliseconds down to ten milliseconds. I don't think there's any debate about the accuracy of that number, give or take a few calories per gram.

The debate is whether it's appropriate to test up near the failure level of the cladding, which is in the vicinity of 100 calories per gram where the pulses would be narrow, or whether you

want to test down at the energy of the expected pulses in a PWR, which may be 20 or 30 calories per gram with broad pulses.

And in fact, what Cabri is doing right now is a bastardized approach of using half and half. They're using a high energy and a broad pulse.

CHAIRMAN POWERS: I guess, I mean, this is a common controversy that comes up, and the question of where you test. I mean, oftentimes what you get into is the debate of do I do a very prototypic test or do I test my codes.

And I'll offer the opinion that the best thing to do is to test your codes because nothing you can do with the Cabri or the NSRR, there is no conceivable thing that you can do to make those completely prototypic machines. You're always going to have to be taking data out of one machine and analytically transforming it to make it look like a reactor accident.

Now, where do you come in on this? I mean, where do you stand on this?

DR. MEYER: Okay. We have not attempted to put failure models into our code so that we can do straight up predictions. It's very difficult,

and so we have chosen to stay closer to an empirical database, and I'm going to show you a method which allows us to make some adjustments to the data to account for these variations in pulse width and test temperature so that we can then rely directly on the empirical database without relying on the code so much.

Now, we will rely on the code to make the comparative calculations, and my claim is that in doing comparative calculations, a lot of mistakes that we make will cancel out, and that's the basis for the method.

And I'd like to show it to you. It's a little detailed. I'm not skilled at giving this presentation yet because the method is fairly new, and I haven't had too many opportunities to describe it.

So if you'll bear with me, what we have here is a -- we have a broad pellet in a test, a narrower pellet in some cases. Well, let me back that up.

We have a pulse in a test with a certain width. We have a pulse in a PWR with a certain width, and the width of the pulse is going to affect the temperature, and the temperature, in turn, is

going to affect mechanical properties and some other expansion.

So here are two things that happen, particularly in the Japanese test. You have the initial coolant temperature, which is obvious, but in the case of the pulse width, you're going to see that a broad pulse will lead to a higher cladding temperature at the time of a certain drain occurrence than will a narrow pulse.

I'll show you pictures of this, and it is this temperature difference then that will affect the mechanical properties and also the thermal strain in the calculation so that there will be a tendency for a broad pulse to -- for two things to happen. First of all, for the cladding to be more ductile at the instant that the critical stress is applied, and also for the cladding to try and run away from the pellet, if you will.

The picture to keep in mind is that you're dumping thermal energy into the pellet, which is expanding more than the cladding, and it pushes on the cladding and it strains the cladding.

What we're going to be looking at is the plastic strain in the cladding. Now, there are several components of strain in the cladding. One

of them is what we call a thermal strain. It's just a thermal expansion of the cladding.

So to the extent that the cladding can thermally expand, you don't have to stretch it, and so there is a component of thermal strain that is effective as well. It's not too big, but it's definitely there.

We're going to use the FRAPTRAN code to do the calculations. For today's discussion I'm going to guess at the mechanical properties and their temperature dependence. I'm just going to make some assumptions about these. I'm not going to try and convince you that my assumptions are correct, but just want to illustrate the method.

I'm going to do two numerical examples, one for HBO-1, a test from Japan, and one for REP-Na10, a test from Cabri.

Now, there's a major difference in the mode of failure in these two cases. In the Japanese test, HBO-1, the cladding was clearly beyond the elastic region. It was in a regime where it was experiencing plastic strain, and the opposite is true in REP-Na10. REP-Na10 appears to have failed while it was still in the elastic region, just at the end of that elastic region.

DR. FORD: That difference is understood, given the difference in temperatures, one at room temperature and the other at 280 degrees centigrade?

DR. MEYER: I'm sorry. What was the question?

DR. FORD: Just your reference to the Cabri failure was due to brittle failure, elastic strains, whereas at the lower temperature in the Japanese reactor it is by plastic deformation, necking (phonetic), do you remember? It seems opposite to what you'd expect.

DR. MEYER: It is opposite to what you'd expect. I don't understand it. I'm going to show you some data that I don't fully understand yet, why the Japanese seem to see more strain in the test conducted at lower temperature.

Now, one thing is --

MR. SCOTT: Ralph, this is Harold Scott.

Don't we think that the Cabri tests have lots of corrosion and a lot more hydrogen than the Japanese test? So that's one possible reason why the failure mode is different, is because they have different amounts of embrittlement.

DR. MEYER: That's a good point.

MR. SCOTT: I think when you said before that we knew, the way we sort of know whether it was plastic or elastic is partly by looking at the micrographs of the fracture.

DR. MEYER: Well, and also by looking at strain measurements, and I've got some strain measurements in here. So kind of hang onto the question, and we'll come back to it, but I was thinking about our analytical predictions, and we don't hit the Cabri predictions as well as we hit the Japanese predictions.

So let me start off first with the Japanese one. Here was the test. These are measured values now. They had a total energy input of 93 calories per gram. This was reconned at some time like 1.2 second. They determined the time of failure by looking at the instruments, and so they report a failure time on an arbitrary scale. The pulse had a width of 4.4 milliseconds, and the coolant temperature was room temperature, about 291.

Those were measured test values. These are our calculated results. So we now calculate at the time -- at the reported time of failure, the fuel enthalpy that we calculate is 60 calories per gram, which by the way is exactly the same number

that Jerry has reported.

So they calculated the same thing that we calculated quite independently.

At the time of failure, we look at the cladding permanent hoop strain in our calculation, and we get .62 percent, and we're going to say that this is the failure strain. In this test, .62 percent average plastic strain was all it could take and it failed, and at that time of failure, the cladding temperature was 338 degrees.

Okay. That's just put in for your reference to define the terms that I use. I don't want to spend any time on that.

Here is a plot of measured permanent hoop strain. This is plastic strain in the whole HBO series.

Now, in the HBO series, they measured strain on tests that didn't fail. They didn't measure strain on tests that did fail, and so here we were able to plot the measured strain values as a function of the peak fuel enthalpy in the HBO series, and you see that it intercepts the axis somewhere around 30 to 40 calories per gram.

So if you're in the range of 60 calories per gram, which is where our calculation said was

the failure, then you should have a permanent hoop strain of about .7 percent based on these measured data, and we calculated .62 percent.

So so far our calculation looks credible and we go on.

CHAIRMAN POWERS: Ralph, just a question on experiment here. When you have the horizontal axis here, peak fuel enthalpy increase, how accurately do you know that?

DR. MEYER: These are reported numbers, but they were calculated numbers because any time you're dealing with the enthalpy, you're dealing with heat loss.

CHAIRMAN POWERS: There's not much in these short pulse.

DR. MEYER: John or Harold, do you want to give me a plus or minus on the peak fuel enthalpy?

MR. SCOTT: We just said ten percent to each other.

DR. MEYER: Plus or minus ten percent.

CHAIRMAN POWERS: And, Harold, where do you think that uncertainty is coming from? Is it from just the reactor characteristics?

MR. SCOTT: Yes.

CHAIRMAN POWERS: Because there's not much loss in a four millisecond pulse. You're getting most of it in the fuel pretty easily.

DR. FORD: Again, just on experimental detail so I can understand it, this test, HBO-1 --

DR. MEYER: Yes.

DR. FORD: -- that was on a fuel that had a certain degree of burn-up. What about the cladding?

DR. MEYER: Yes.

DR. FORD: Had that been exposed to lithiated water at 288 degrees Centigrade or whatever the temperature was?

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

DR. FORD: Before you did the test.

DR. MEYER: I don't know about the water chemistry, but HBO-1 had about 40 microns of corrosion. I don't know the hydrogen level. It had a burn-up of about 60, 65 gigawatt days per ton in the length of specimen that was tested.

DR. FORD: I'm inferring from your remark earlier on, I think it was, that this had not been exposed to any degree of corrosion, corrosive environment, lithiated water beforehand.

MR. SCOTT: You said 40 microns.

CHAIRMAN POWERS: Yeah, I mean, there's 40 microns of corrosion on it. I'm not sure what you're asking.

DR. FORD: I'm just trying to sort out in my own mind the degree of corrosion, and I take your point.

DR. MEYER: Okay. Just a moderate level of corrosion.

DR. FORD: Right.

DR. MEYER: It's certainly not a heavy level of corrosion.

All right. So here is the four and a half millisecond pulse in the test reactor, and here is the ten millisecond pulse. Here is a ten millisecond pulse with the same energy.

Okay. Now, in the calculation that we ran with this pulse, we get the failure somewhere over at this time, right about here, and that failure occurred at .62 percent plastic strain.

So now the game is to go on this curve and look for the time at which the plastic strain is .62 percent. Well, I've got to tell you right up front that it won't be exactly .62 percent because it's temperature dependent. The failure strain is going to -- we expect it to be temperature

dependent.

Now, here is a comparison of cladding temperature as a function of fuel enthalpy for those two pulses, and you can clearly see here that for any enthalpy value which you can think about as the amount of pellet displacement, because to a first approximation, the amount of enthalpy in the fuel is the amount of thermal expansion, and the pellet is hard. The cladding is not so hard. It pushes on the cladding.

So for given amount of pellet displacement, you see that the cladding temperature in the narrow pulse is significantly less than it is in the broad pulse.

So now what we have to do is take that temperature difference into account in the failure strain that we're going to associate with that .62 calculated number.

Okay. I've said those words. I want to skip this slide for now.

Okay. This is an assumption. Now, what I've plotted here is total elongation as a function of temperature. The failure strain is a total longation, but total longation is not a fundamental materials property. It's affected by geometry, by

the gauge length and dimensions of the specimen, and so if we look at total elongation data from tests that may have been on axial specimens or on ring specimens with different gauge geometries, then the effective gauge geometry of this non-uniform deforming cladding, we're going to see a temperature dependence.

Now, frankly, I looked at EPRI's plotted data, and this is not quite as bit a slope as EPRI has in their report, but it's a ballpark number, and so I'm just going to use this number to illustrate the method.

Now, in effect, what we're doing is we're going to assume that the total elongated -- the failure strain in the specimen, which is a total elongation, is going to have the same temperature dependence as this. So we just ratio the two.

In effect, what I'm doing is drawing a different line that would be right down around there somewhere, which is going to be the locus of failure points in this particular specimen.

So this is what I just said in words, and so we're trying to find a new failure strain at a different temperature, and we need a temperature and a strain combination that are on that adjusted

line.

This is the part that I haven't figured out how to explain clearly yet, but from the nods, I think that you --

CHAIRMAN POWERS: Know exactly what you mean.

DR. MEYER: -- understand what I'm trying to do.

CHAIRMAN POWERS: I understand.

DR. MEYER: Anyway, when we go through this, we find that the new failure strain is .75 percent at a cladding average temperature of 380K. So the PWR pulse in this case is or the wider pulse is at a higher cladding temperature. There's a little more ductility. So you get a little higher failure strain, and the corresponding fuel enthalpy at that time is 69 calories per gram.

So in this example, a nine calorie per gram increase as a result of pulse width, just pulse width. I haven't altered the test temperature yet. That's going to be a bigger deal than this, but I wanted to look separately at these two effects for the HBO case.

So the next thing we did then was to -- let me back up. I need to talk just a minute about

this detail.

And this is fascinating, and it's also difficult for me to explain. But now I'm going to go back one, two, three, to this figure. This is the ten millisecond pulse here, ten millisecond pulse.

And I have plotted on this same figure the permanent hoop strain and the cladding temperature. Now, the peak fuel enthalpy occurs right about here. The enthalpy peaks out because heat losses then are as big as the heat input in the tail of the pulse, and when the fuel enthalpy peaks out, you don't get anymore cladding hoop strength.

But the cladding temperature continues to rise. Okay. You back up. Somewhere around here is what I call a point of no return. If you don't have enough strain to fail it, if you haven't reached the failure strain at this point, you're not going to reach it up here because the cladding temperature is starting to increase more rapidly than your strain value is increasing.

So the point is if you had done a test, say, with a peak fuel enthalpy of 75 calories per gram and observe the cladding failure at 60 calories per gram, if you go back now and run a test with a

60 calorie per gram peak fuel enthalpy, it won't fail that cladding because here's where the 60 is going to occur, and it's happening too slow.

So we had, in fact, to increase the energy in the deposited pulse in order for the cladding strain to keep up with the temperature increase.

So we were not able to find an adjusted failure strain in the ten millisecond pulse without increasing the energy in that pulse. We increased it incrementally ten percent, 20 percent, 30 percent. The ten percent didn't do it. Twenty percent did it. Thirty percent did it and gave the same answer as 20 percent.

And now if you have some feeling for that concept, now you will understand that when we try and account for this huge difference in test temperature from room temperature up to nearly 300 Centigrade, that we need a large increase in pulse energy in order to find that failure strain at the right temperature in a reasonable pulse.

So the pulse that we used had twice the energy in it as the original pulse. So this is the original NSRR pulse, and this is the ten millisecond wide pulse with twice the energy, and in that pulse,

we now find an adjusted failure strain of 1.71 percent. There's the temperature at 100 calories per gram.

Now, if you go back to the measured failure strain for HBO, you'll find that 1.7 -- that at 100 calories per gram, 1.7 is just about on the line. So this is a credible number.

The combined effect of pulse width and test temperature with the temperature dependence that we assumed is 40 calories per gram. It's huge. That means on the paintbrush slide that those NSRR points are going to have to be moved up about 40 calories per gram.

If we used the larger temperature dependence that EPRI used, it would go up further, and it's now up into the range where you have to wonder whether it would fail at all by a cladding mechanical interaction or whether it would go into DNB and fail by a high temperature mechanism up around 160 or 170 calories per gram.

Okay?

CHAIRMAN POWERS: Back to your step, do you, in fact, do a step-wise conversion? The way you presented it, Ralph, was first you made a correction without correcting for the water

temperature, and then you corrected for the water temperature.

Do you, in fact, when you actually sit down and do it, do those things all at once?

DR. MEYER: Yeah. The second one was done all at once. I didn't do the second one with a four and a half millisecond pulse. So the second calculation that shows the 40 calories per gram is the sum of both.

So in this case order of magnitude was you got ten calories per gram from the pulse width and another 30 calories per gram from the test temperature, giving you about 40 calories per gram.

CHAIRMAN POWERS: Okay, but that was for pedagogical purposes that you did that. When you really do it --

DR. MEYER: Yeah.

CHAIRMAN POWERS: The difficulty I have in your way of presenting is when you did the first step, you did it for a ten millisecond pulse, but the lower energy. Okay?

DR. MEYER: Yeah.

CHAIRMAN POWERS: Whereas in the reactor, you would actually have a broader pulse if you did a low energy pulse. Okay?

Whereas in the second step where you did both corrections, you had what's appropriate for a reactor pulse.

DR. MEYER: In the first case, we increased the power 20 percent. In the second case, we increased it 100, and it included the pulse width effect.

CHAIRMAN POWERS: Yeah, you did two things. In the first example, the step that you showed, you increased the pulse width, and you increased the energy.

DR. MEYER: Yes.

CHAIRMAN POWERS: Okay, but the increase in the pulse width is not reflective of the width you would get in a PWR if you did a pulse of the energy magnitude that you did.

DR. MEYER: Oh, yeah. Yeah, it is because this curve is really flat. Once you get to 60, 70, 80 calories per gram, it's asymptotically going to ten milliseconds.

CHAIRMAN POWERS: Okay.

DR. MEYER: So it doesn't make much difference, but you're right. When we do this, we will incorporate that dependence in it, but it's a small thing.

DR. FORD: Could I ask a question again on your specific methodology? Your approach for correcting for the pulse width seems dependent very much on the interaction between the average cladding temperature and the hoop strength.

DR. MEYER: Yes.

DR. FORD: What is the uncertainty of that, given, for instance, that the failure strains will change dependent on the amount of corrosion?

If you're going to apply it to BWRs, you might be talking about barrier fuel cladding. All of these are going to be interactive. So there's some uncertainties in these very precise 1.71 percent cladding, a lot of uncertainty in that. What degree of uncertainty are we talking about because of these other material property changes which we don't know?

DR. MEYER: Okay. I've got to make a distinction between two types of uncertainties. One, the uncertainty in the material --

DR. FORD: Yeah.

DR. MEYER: -- and the properties and the amount of corrosion and random defects and things like that.

DR. FORD: Right.

DR. MEYER: It is completely outside of this scaling method because what we're doing is looking at test HBO-1, that test run specimen, and asking: what if I took that exact same specimen and tested it with a PWR shaped pulse to failure?

DR. FORD: Yes.

DR. MEYER: So right away all of the material variabilities are not involved because I'm assuming that I'm still working on the HBO-1 specimen. The uncertainty in this parameter is going to be determined by two things. I think by far the largest is the uncertainty and the temperature dependence of the mechanical properties. They're poorly known at this time. EPRI's figure has a nice average line, but the data scatter is very large.

We are hoping to narrow this down quickly in our program at Argonne, and so we hope to make some improvements on that, but even within these large uncertainties, you can now begin to get an order of magnitude feeling for what it does to the data.

I'm going to skip this slide for a minute, and now this is the second example. This is REP-Na10, and these are the test parameters, real

numbers: 170 calories per gram total energy input; measured time of failure; 31 millisecond pulse; and 553 Kelvin initial coolant temperature. Those are all measured values.

So we run the calculation for that pulse, for those exact conditions, and this time we don't get quite as good agreement as we had before. If we take their reported time of failure, we get 68 calories per gram fuel enthalpy at the reported time of failure.

IRSN reports 61 calories per gram at the time of failure. IRSN also reports that there is no plastic strain in their calculation for this specimen. At the time of failure we get a little bit of plastic strain.

So what we did just for the purpose of illustrating the example is to move the failure time back a very small amount so that we were still in the end of the elastic region. So this is an artificial example, but it's still pretty close to REP-Na10.

So we moved it back till it was right at the end of the elastic region, and at that new assumed failure time, we had 59 calories per gram, which is uncannily close to their 61 calories per

gram.

Now, since we're talking about failure in the elastic region, strength becomes the important parameter and not ductility. So we now want to look at the stress on the cladding, and so the hoop stress at that new assumed time of failure is 450 megapascals, and the cladding average temperature at that time is 743 Kelvin.

CHAIRMAN POWERS: When you say that IRSN reported no plastic strain in the specimen, is that they saw no evidence of plastic strain or they calculated no --

DR. MEYER: No, it's calculated. In the Cabri tests, the Cabri tests are in sodium.

CHAIRMAN POWERS: Right.

DR. MEYER: And they cannot measure accurately the strain on a rod that has failed because you get a sodium interaction with the O2 and the swelling, and so they can't go in after the fact and measure the strain on the failed rods.

I'm going to show you some data though, and that's one slide that I skipped over, and it will indicate that we're sort of in the crossover point, and I don't know which would be correct, some strain or no strain.

CHAIRMAN POWERS: What you're also saying here de facto, I believe, is that whatever calculational tool the French are using, it's not getting results that are wildly different than what's your calculational tool is giving.

DR. MEYER: That's correct. That's correct.

Okay. So now in this case, this is the Cabri pulse, 31 calories per gram, and here is a ten millisecond pulse with the same energy, and so we're now going to look at the failure stress for the pulse as we calculated it, and then we're going back on this ten millisecond calculation and look for that same failure stress adjusted for temperature changes.

So it's exactly the same scenario as you had for the strains, except now we're dealing with stresses.

Don't ask me to explain this, but Mike Billone is here. He can explain this if there are any questions, but this is a plot of fracture toughness versus temperature, and fracture toughness is related to the fracture stress, and we're out in a temperature region up here.

Actually I had already just assumed a 25

percent reduction in failure stress, in macroscopic failure stress for a 100 degree temperature reduction. This curve shows about 35 percent reduction.

If you take a temperature here and you go down 100 degrees, it's about a 35 percent change on this figure. We took 25 percent in our calculation.

So, again, it's an assumption, but it's in the ballpark, and so here are the calculated results. The 450 megapascal failure stress came down to 350 megapascal because we're now nearly 100 degrees lower in temperature, and the failure stress will be lower.

And this lower stress occurred at a time where the fuel enthalpy increase was 40 calories per gram instead of the 60 calories per gram that we had calculated. So in this example, the REP-Na10 number would be adjusted downward by 20 calories per gram.

And if I can go back, these are measures strain values from the REP-Na series. These are all of the tests that did not fail, and they're a mixture of several things, and I don't think we know quite how to sort them out yet, but there are two MOX tests in here. The MOX results might be

different than the UO2 results, and there are narrow pulses. Here's a couple of nine millisecond pulses, three of them, and broad pulses, 75, 35, 34.

These lines are not statistical fits. These are just drawn to help aid the eye. Somewhere in the range of 50 to 80 calories per gram is where you should leave the elastic region and enter the plastic region in the REP-Na test.

And we were at 60 and calculating a small amount of plastic strain. IRSN had calculated no plastic strain. So, again, the result is reasonable, but there's not a very sharp point on the analysis yet.

So here are the conclusions. Both pulse width and testing temperature affect the results, and the amount of that effect depends strongly on the temperature dependence of the mechanical properties, The mechanical properties aren't well known. They're under investigation. We hope to make improvements. The effect of pulse broadening in Cabri, in our example, was large, about 20 calories per gram. The effect of pulse atypicality in NSRR was modest, about ten calories per gram, but the effect of low test temperature in NSRR was very large, about 30 calories per gram, and these two get

added together. So it's a big adjustment. And I've already said that.

Okay. Now, I have one other slide. I have one other conclusion that's not on this slide. If there was ever a Friday night calculation that was reported on Monday morning, this is it, but it occurred to me in looking at the plot that I had of permanent hoop strain and cladding temperature on the same graph where there was this what I call the point of no return, and I've said it already, you cannot fail a rod with a peak enthalpy in the pulse that's the same as the enthalpy number in the failure that was determined from a little larger pulse. You've got to have a little extra.

How can I say this? About the last ten calories per gram aren't going to cause a cladding failure, and so here is some free margin that I don't think anybody recognized before. When we calculate peak fuel enthalpy and compare it to something with the neutronics calculation, you know, we do a neutronics calculation and we calculate a peak fuel enthalpy, that peak fuel enthalpy has to be something on the order of ten calories per gram higher than the failure enthalpy in order to actually cause the failure.

And I know this isn't said very clearly, but there is some margin in here that we didn't recognize before that we can take credit for. It's on that order. It may be five; it may be 15. Hopefully in a few months of working on this we'll be able to say what it is with confidence and use it in our final assessment.

Now, how we're going to wrap this up is we're going to do the best job we can by the end of this calendar year, and we're going to put it out. This is a never ending thing. We can do mechanical properties measurements and calculations for years and years, and we've been going on a long time on this one.

We have a program in place to do mechanical properties. I'm going to turn the mechanical properties part over to Argonne and say, "Give us your best temperature dependence by the end of the year."

The analytical part, John and Harold are going to work on that. We're going to do the best that we can, and then we're going to write it up and try and define this cladding failure boundary empirically as a function of oxide thickness with just these adjustments.

We're going to use the cladding failure boundary as the de facto limit in our assessment. I think it's going to work. I believe we will be able to show from the neutron kinetics analyses that have been done to date that for reasonable control rod worth we cannot generate enough fuel enthalpy in a PWR rod ejection accident to reach the cladding failure boundary.

I think that's going to be the result based on preliminary evidence. If that's the case, we can still with this cladding failure boundary and say this is plenty adequate because if you don't fail the cladding, you're not going to get any energetic fuel coolant interactions. You're not going to lose fuel particles and have questions come up about is it coolable.

And we're going to do this all by the end of the year and issue it as a research information letter.

DR. FORD: When you look at the second bullet on your previous graph or slide, the mechanical properties are not well known. That seems to me a kind of fairly fatal or high risk item because when you look at all of the variables, strain rate, degree of hydriding, whether you have

barrier fuel cladding or not, there's a lot of variables, degree of plastic constraint.

But are all those mechanical properties going to be available by the end of the year?

DR. MEYER: No, but in our method, the mechanical properties are already imbedded in the test result, and so this is a second order. It's the second order effect, the correction that's going to be affected by how well or how poorly we know the temperature dependence of these mechanical properties.

And I think it's only the temperature dependence that we need to get a handle on. The biggest uncertainty in doing a laboratory test is in adequately representing the condition of the stress applied on the cladding, which is probably a plain strain stress, which is very hard to replicate in a test.

Now, we can do it. We have a plain strain specimen design that can approximate that, and we will try and do that. There will be uncertainty in it, but I think it's a big uncertainty and a second order effect can be tolerated.

DR. FORD: In your program plan, the

August 21st program plan, you also mention with respect to this particular problem the Vitanza multi-variable algorithms. Could you say something about that?

DR. MEYER: I'm sorry. Try --

DR. FORD: Carlo Vitanza has come up with a multi-variable --

DR. MEYER: Oh, yeah. In the plan we mentioned three possible approaches to this, and we said we thought we could get one of them to work.

DR. FORD: Right.

DR. MEYER: One of them is a straight up calculation. We do not have a failure model in our code. We can calculate strain energy density. It's in the code now, but we don't have a good failure model, and we are not pursuing that.

I think, John, have you looked further at the Vitanza type approach? And I've got to ask John if we're actually doing anything on that.

My own approach is this empirical method, and I don't know whether we have made any further progress on the Vitanza type approach.

MR. VOGELWEDE: This is John Vogelwede again.

Vitanza's correlation is well known to

us. It has a fairly significant pulse width and corrosion effect in it. We have investigated whether or not we could use something like this to do the same transformation that Ralph is talking about here.

The only thing that we've done so far is to adopt something that he did in his correlation where he goes from a relative to an absolute enthalpy to account for the NSRR data from Japan. The calculations are not too bad. He's published that already.

DR. MEYER: So I think the answer is that we're going to use one of the three approaches.

CHAIRMAN POWERS: It seems to me, Ralph, that in this empirical approach that you've created here you're now creating a vulnerability to the selection of specimens that have been tested.

DR. MEYER: yeah.

CHAIRMAN POWERS: And so what do you do about that? I mean, there's a natural bias to pick specimens that hold together well and look nice when you do the testing. What do you do about that?

DR. MEYER: Well, fortunately, there have been some selections made that don't fit that pattern, and I think those turn out to be key tests.

REP-Na10 was heavily corroded. REP-Na8 was also corroded, but it has a really squirrely pulse width, and that one is going to be a little more difficult to deal with. It had a double hump pulse at 75 milliseconds across.

CIP01, the ZIRLO rod, which probably did not fail, is a good rod, and it will give us a good, non-failure point. I think we can treat the adjustment to the non-failure point just like this one. We just say it was a non-failure.

I'm not quite sure how we do it, but I think from looking at CIP01, I think CIP01 was right at the point of no return, just past the point of no return because it gave some signals, and yet it still seems to have sufficient gas in the plenum, and we haven't gotten reports yet on the pressurization test to know whether it really failed or not failed, but I would say it didn't fail at this state of understanding.

CHAIRMAN POWERS: I guess what I'm asking is: do we know enough about fuel rods coming out of the reactor to know that we have a representative or at least a conservative sampling of the fuel rods?

DR. MEYER: Yeah, I actually think we

do, and particularly when you recognize that the newer alloys, the M5 and ZIRLO or Alloy A. What is the next one coming down the line?

These claddings don't corrode very much, and I don't think the reactivity accidents are going to challenge those claddings. I think you're going to have a lot of -- even the Russian E110, which we describe in not very favorable terms for its LOCA behavior, sails through these tests. Of course, they only collect five or ten microns of oxide on them, but we have never seen a PCMI failure in an E110 rod yet. They're all high temperature ballooning and rupture things.

So I really think that's the situation for M5 and next generation ZIRLO at least, if not this generation ZIRLO, the way it's operated in this country with lower corrosion. That is, you know, as soon as you get down below 60 or 50 or 60 microns of corrosion, I don't think you're going to have any problem at all.

CHAIRMAN POWERS: Rosa.

MS. YANG: I think I just want to make two comments. One -- sorry. This is Rosa Yang, EPRI -- one of them, I just want to remind everybody, especially the last year, October 9th,

this particular meeting there was a very detailed presentation on the RIA methodology that we have submitted for review that was presented by Robbie Montgomery on the methodology.

I think Ralph Meyer here presented something, and I think he correctly called it the scaling method, and I think it's very interesting, but I think as some of the questions already alluded to, that this is a highly complex and nonlinear phenomenon. It is difficult to really just look at one parameter and scale it to the light water reactor or the PWR condition.

I think the correct way to do it is to really model the phenomena as best as you can, and then try to benchmark that with measured parameter like the cladding strain, like the temperature, like different phenomena that you can model, and that's what we have attempted to do in this submittal.

The intent is to model the NSRR data, the Cabri data, and try to benchmark with measured parameter, and then from there trying to make the link from the test condition to the PWR condition.

I think, you know, given the complexity of the issue, that's probably the only way you have a chance of success, and that might address this

issue, Mr. Chairman, you're asking about have we tested the relevant material, you know. Have we covered enough of the variable so that that is an attempt that we have tried.

And one other comment that I wanted to make was at the last year's meeting, I think the conclusion was we have a good understanding of this phenomenon, and given the light water and PWR condition, there's probably sufficient -- not probably -- I guess there is sufficient margin that this is an area that maybe we're getting to a point of diminishing return; that we shouldn't spend a lot of resources trying to sharpen the pencil further.

And I think that's consistent with a comment just made that REP-Na10, which failed at, you know, 70 or 80 calories per gram, and is highly spalled rods, and given the advanced alloys that are being used in the industry, that corrosion is much lower. And I think we didn't discuss in detail, but one of the key phenomena that's important for the failure threshold for the RIA type of thing is the cladding mechanical properties. So advanced alloys should behave much better than REP-Na10 being talked about here.

CHAIRMAN POWERS: Well, it seems to me,

responding to your comments in order, if I can remember them all, is that Ralph has linearized this phenomenon to do his empirical process, and the detailed phenomenology approach that Robbie presented at our last meeting, in fact, he invented the phenomenon in developing his model, and that's the one that's the source of controversy there, is whether you actually have a dependence that's hypothesized or not.

And I guess we'll eventually hear CA review of that phenomenology or phenomenological report that NRR is coming out. Do we know when? Did you see that review?

I think I'm getting an answer to my question.

MS. SHOOP: This is Undine Shoop with the Office of Nuclear Reactor Regulation.

We're currently planning to complete that review by next summer based on getting the information from Ralph Meyer and being able to also assess that information as part of our process.

CHAIRMAN POWERS: Okay. So in the next maybe a year from now we'll get that.

And then as I understand what Ralph presented, what he is saying is that had we done

REP-Na10 in a completely prototypic test, in this hypothetical test it would not have failed it at 61 or 69, depending on how you look at it, but in fact would have failed at 40 calories per gram.

DR. MEYER: That's what I'm saying, yes. That's correct.

With regard to the mechanical properties and the linear relation that I'm using, EPRI is using a linear relation for this.

CHAIRMAN POWERS: Yes.

MS. YANG: It wasn't linear. No, no, no, it wasn't linear.

DR. MEYER: Yeah, it was. It has got a --

MS. YANG: What is linear?

DR. MEYER: -- A plus BT equation right on the graph.

MS. YANG: No.

MR. OZER: Can I make a comment, Mr. Chairman?

CHAIRMAN POWERS: Sure.

MR. OZER: This is Odelli Ozer, EPRI.

I think what we see as far as the failure criterion is concerned is that the rods fall into two categories. The rods that are heavily

spalled have a lower failure line than the rods that are not heavily spalled.

The rods that are heavily spalled are pushed well beyond their design corrosion levels, and to use a single correlation that folds in the spalled rods as well is really not fair for the rods that we will be seeing in the future or the rods that are operated within their limits.

DR. MEYER: I'm not sure how far you want to go down this path, but this is --

CHAIRMAN POWERS: Oh, a little ways.

DR. MEYER: -- this is an interesting point because, frankly, we don't believe that the two populations are separable. Spalling, the occurrence of spalling, it doesn't instantly lead to bad mechanical properties. It eventually leads to local hydride blisters, and as these local hydride blisters grow and get thicker and thicker, they have a deteriorating effect on the mechanical properties, and it, in fact, has been tested as a function of blister thickness at Penn State, and the transition from zero thickness to basically through the wall is a nice, smooth, uniform transition.

So we tend to think that these are all part of one population and treat it in that way.

CHAIRMAN POWERS: Well, I guess what I sense the issue is is this. You've got a database now of a bunch of empirical tests done in modestly non-prototypic conditions, and you've come up with a methodology here that you would like to correct all of those data for effects that you think you understand in a fairly linearized way. Okay?

You're not seeing such high nonlinearity here that it precludes that, and you will do so. And most of those experiments that you are going to make that correction for are zircaloy clad rods.

DR. MEYER: That's correct.

CHAIRMAN POWERS: And now you get a curve out, and you say, "Okay. If you're using zircaloy, please show me in your design basis analysis that you don't have any accidents that will give you an energy input greater than this threshold here."

Okay. The concern that comes about says, "Gee, I'm not using zircaloy. I'm using M65," or whatever the next. Maybe M16 is what they want to use. I don't know.

(Laughter.)

CHAIRMAN POWERS: And they're saying, "Hey, don't constrain me with that curve and invent

me some other curve."

And so the question I put to you is: who invents that second curve?

DR. MEYER: Yeah. We, of course, want to generate those kind of data. That would require high burn-up rods clad with ZIRLO and M5, which we are proposing, and we hope the industry will cooperate with us and allow us to do that in the future.

We don't plan to hold this issue open until that's done because we have some other clues to go on, and ironically one of the most advanced set of clues that we have is from our Russian program where they have measured mechanical properties on unirradiated and irradiated E110, compared that to zircaloy.

And I don't know if Mike wants to say any more about that, but they don't see big effects of the irradiation process or big differences from the zircaloy properties.

So you know, I think most of the action is in the corrosion. Whether zircaloy, ZIRLO, or M5, the dominant factor is going to be how much hydrogen have you allowed into that cladding as a consequence of the corrosion process.

And we also are studying pre-hydrided materials, and in fact, are proposing for the extended work on high burn-up ZIRLO and M5 that we study the efficacy of using pre-hydrided specimens as a surrogate for burn-up for these mechanical properties tests.

And I think if we can go that distance, then we'll have Zirconium-10, zirconium niobium, and zirconium with a mix of 10 and niobium. We'll have three alloys at high burn-up, and we have the ability to do pre-hydrided work. We have a new program starting at Penn State on the mechanisms of this, and so the beginnings of a nice way of wrapping this all up, confirming our guesses that we're going to make this year and next year, and developing a methodology which will allow us to do a lot of testing on pre-hydrided, unirradiated specimens and avoid the expense of going to a hot cell for all of this.

CHAIRMAN POWERS: Other questions to the speaker?

I'll pose a couple of issues for members to think about. One issue is this question of where we test, prototypic or whether we're challenging codes, and the second issue to think about is the

question of who draws the second curve. Is it the NRC's responsibility to draw out failure curves for advanced alloys that the industry brings forward or is it the industry's responsibility to develop that database and have the NRC review it?

And with that, I will recess until ten after the hour.

(Whereupon, the foregoing matter went off the record at 10:52 a.m. and went back on the record at 11:13 a.m.)

CHAIRMAN POWERS: Let's come back into session.

Dr. Meyer, there's no relief for you. You have to do this session as well.

DR. MEYER: Well, I want to shift gears now to the loss of coolant accident, and as Jack mentioned this morning, this is one where we're trying to make some definitive progress by next summer. So this is still a fairly fast track item at this point.

Now, there are really three problems that we're addressing. One of them that we've been talking about for several years, and that is that for high burn-up fuel the ductility of the cladding is affected by burn-up and corrosion, and this may

have some impact then on the embrittlement criteria that are in 10 CFR 50.46.

A second problem that we're looking at is about one of the evaluation models. This is an Appendix K type thing rather than a 50.46 type thing. The oxidation kinetic models, which are used both for calculating the oxide thickness and the metal water reaction heat, may be affected by burn-up and corrosion, and we need to check that out.

And then the third problem that we're addressing now is the fact that the rule as it's currently formulated only provides criteria to be used by two cladding types, and we would like to see some change made so that the rule can apply to all cladding types and not put us in a situation where we have to use a lot of exemptions from the rule.

So I'm going to try and describe how we intend to fix all of this. So we're going to, in fact, we're in the process of generating a database on high burn-up fuel. We have high burn-up zircaloy, Zircaloy-2 and Zircaloy-4, and we have unirradiated M5 and ZIRLO in the lab, along with some other cladding types.

And so we're working on an appropriate database with those rods. Mike Billone will talk

about that extensively in the next presentation. Mike and I decided that we would put my applications presentation before his data presentation so that you could see where we're trying to go with the data, and then have an idea of the focus that Mike should have in his program and keep the discussions a little more focused on the job that we have.

Now, I want to make a little distinction between the confirmatory check on the current licensing analysis and developing a basis for a more inclusive role, two separate steps.

One is to make a demonstration that the way we're doing business now for the operating reactors is okay, and then the second thing is to try and fix up the rule so that it won't be restricted to any particular alloy type.

And the form of the results of all of this will be, first of all, a research information letter summarizing the laboratory results, and then perhaps in the same rulemaking procedure, a confirmation or modification, if necessary, if the grandfathered rule and a new performance based option.

Now, what do I mean, "the grandfathered rule"? Currently the -- I think I have it on the

next -- no, I don't have it on the next slide. Well, this is the 17 percent, 2,200, and I've got it on a slide a couple down to show some of the fine points in the application that's currently being made.

And if we can demonstrate that these are all adequate, then we can keep them in the rule as an option. So the rule as envisioned would have the option of using the old 17 percent, 2,200 method or the new method.

And what we're trying to do now is with those goals in mind, to generate a database that will allow us to support those kind of changes.

So we look back at the basis for the current requirements and actually have gone back and studied the documents, particularly the Commission opinion of 1973 at the end of the ECCS hearing. I don't know how many people -- not many people here remember the ECCS hearing of '72 and '73. There's one at least. Norm Lauben back here was involved in that.

But this, I think, was the longest hearing the NRC, AEC at that time, had ever had that produced the rule in 50.46 and Appendix K.

And so we've gone back and looked at the

Commission's discussion of their conclusions, and for the embrittlement criteria, specifically the peak cladding temperature and the limit on oxidation, those were defined to maintain a coolable geometry, and the way you maintain a coolable geometry in the Commission's view was to keep the fuel pellets inside the cladding, and the way you did that was to keep the cladding from fragmenting or breaking into several pieces.

And to accomplish that the Commission said, "I want some ductility."

They had looked at arguments about stress, loads, flexibility, and other considerations, surviving quench, and very succinctly said that the stress calculations, the measurements of strength and flexibility of oxidized rods, and the thermal shock tests are all reassuring, but their use for licensing purposes would involve assumption of knowledge of the detailed process taking place in the core during a LOCA that we do not believe is justified.

And for that reason they said that they wanted some non-zero ductility when this LOCA was all over, and that is the basis for the current rule, and it is that basis that we're pursuing in

order to develop a database to simply use the same basis and go forward.

That doesn't mean you have to use the same basis, but it was our judgment that if we did this, that we hopefully would avoid another big hearing because we were sticking to the principles that were established in the original hearing. And that's the foundation for what we're doing in the research program at this time.

Now, these are the embrittlement criteria: don't exceed 2,200 degrees Fahrenheit heat cladding temperature, and don't exceed 17 percent oxidation of the cladding thickness.

There are three fine points here that may not be as well known as the original numbers. One is that this determination is, in fact, done in the ballooned region of the cladding. Just to refresh your memory, during the LOCA the cladding heats up. At somewhere around 800 degrees Centigrade it not only goes through a phase change, but it balloons and it ruptures, and then at about 900 degrees Centigrade, it starts oxidizing rapidly, but below that temperature the oxidation rate is so low that it's not significant.

So this oxide is all taking place at

high temperatures after you formed a ballooned region. That means that some of the oxidation can occur on the inside of the balloon because it's open. And so the original rule and directions in Appendix K provided that you should assume two-sided oxidation for one and a half inches in either direction from the location of the rupture and do this calculation.

It wasn't said in the rule, but if you look at the derivation of the 17 percent number, it was done using the Baker-Just oxidation correlation. In other words, in determining the 17 percent number from the data, the data did not include measured values of oxidation. They were calculated, and they were calculated with the Baker-Just correlation.

So if you don't use the Baker-Just correlation to go backwards when you're doing your analysis, then the analysis will be off by a few percent.

Also, recently NRR has clarified the interpretation of total thickness or total oxidation. It says total oxidation in the rule, and we clarified that to include the corrosion that takes place during normal operation.

Now, so this including the corrosion

during normal operation is what we're now doing in a rough approximate way to accommodate the effects of burn-up, and you can think of arguments why this is overly conservation or arguments why it's not conservative enough.

Certainly during the corrosion process at low temperature, oxygen is not getting into the center of the material, of the metal which is going to end up being this so-called prior beta phase, which contains all of the strength and ductility.

So the oxygen isn't going to get in there, but the hydrogen is going to get in. And hydrogen was not included in the original understanding of oxidation embrittlement, and there's a fair amount of hydrogen that gets into the cladding metal due to this corrosion process.

So it's a guess, and we all agreed it was a good guess, and so that's the way we're handling high burn-up effects now, and our confirmatory activity is to do real tests on real high burn-up specimens and see if these approximate methods, in fact, did the job adequately.

To accomplish all of this, we have several types of tests that are going on at the laboratory. One are the ductility tests. We're

using ring compression tests to determine the dependence of ductility on corrosion alloy types. This is similar to the original approach.

We have checked out the ring compression test for adequacy in determining the point at which you lose ductility, and it's a good method. We've checked it against ring tensile tests. We've checked it against three point bend tests. Some of this checking is still going on, but the early indications are that the ring compression tests are quite an adequate method of screening to tell where the zero ductility point is.

DR. FORD: Ralph, I seem to remember that in the past there's been a fair amount of discussion about the state of stress in these various tests, mechanical testing procedures. That has now been resolved to everybody's satisfaction, I'm assuming and that this ring compression test satisfies --

DR. MEYER: The state of stress for --

DR. FORD: Yeah, plain strain, plain stress, orientation of hydrides, et cetera.

DR. MEYER: Well, this is a -- Mike Billone is going to bail me out here on all of these technical questions, but this is quite a different

arrangement than in the reactivity accident, where you have basically an expanding mandril --

DR. FORD: Yes.

DR. MEYER: -- setting up a plain strain condition. What we're now talking about is a fuel rod which is not being -- the cladding which is not being pushed out by the fuel pellets at all because, in fact, the cladding is getting hotter than the pellets, and we're talking about some external load that might cause a high stress on the cladding, and the ductility test actually sets up tensile loads in several places, and so those are the ones that we measure.

Now, Mike, do you want to clean this up in some way?

MR. BILLONE: No, that's fine. Basically if you're going to stick with the idea of ductility and not strength and failure stress, then you could do a bending test, which is an axial load. You can do a ring compression, which is bending in the circumferential direction, and to the extent that you get similar answers in terms of when they go to zero ductility, the ring compression tests would be fine for that purpose.

So there are a variety of tests

included. The Japanese do axial tensile tests. We're proposing bending tests followed by ring compression.

DR. FORD: The reason why I bring it up is that I seem to remember several years ago a lot discussion on these various testing techniques.

MR. BILLONE: Exactly.

DR. FORD: I'd hate for us to be in a year's time having someone turned around and says, "But all of these tests are useless. You should know that."

DR. MEYER: Yeah.

DR. FORD: A, B, C, and D. We're not in that situation?

MR. BILLONE: Not for the LOCA criteria.

DR. FORD: Okay.

MR. BILLONE: It is very applicable to the RIA analysis.

DR. FORD: Okay.

DR. MEYER: I think initially there was a natural reaction when we discovered these ring compression tests on the Russian cladding that were done in the early '90s by a guy name Boemert (phonetic) in Germany, and the first thing that you ask is, "Oh, well, was his testing technique

adequate?"

And what we found is, first of all, this is the same technique that Hobson used in the early '70s on which this whole thing was done. Boemert's work was repeated in Prague. It was repeated in Budapest. It was repeated in Moscow and Dmitrovgrad, and they always got the same result.

And then we started testing it, and we started comparing it with these other types of testing, like the three point bend and the tensile tests. Now, I don't want to overstate how much of that with the other methods has been done because it has been rather limited, but nevertheless, the ring compression test is a screening test for determining at what oxidation level you lose ductility. It appears to be quite good.

DR. FORD: So what I'm hearing you saying is that there is no one in the technical world who is going to turn around and say in a year's time all of this is useless because it's an irrelevant test. That's no longer the case.

DR. MEYER: Well --

MR. BILLONE: Well, there will always be somebody.

DR. FORD: I don't know the answer to my

question.

MR. BILLONE: Well, there will always be somebody that might say that, but --

DR. MEYER: There are other ways of doing business than ductility testing, and you're going to find a chorus of people who might want to do that otherwise.

DR. FORD: But this test is crucial to -- there we go.

MR. ELTAWILA: This is Farouk Eltawila from Research.

I think Ralph alluded to it, said that's our test plan at this time. The issue of testing has been raised again internally here at NRC and by the industry, and we are planning to convene a meeting with the experts in this area to see if we're still doing the relevant testing or not, and so that will be an issue.

We will be reporting to you later, but just to be fair to everybody, this issue keeps coming up again, and finally we're going to have that meeting and try to resolve that issue.

DR. FORD: Thank you.

DR. MEYER: I think the issue though is not so much about testing technique, but about what

approach you take to demonstrating coolable geometry because in the traditional approach you do that by demonstrating that you have ductility.

Another way of doing that which was not taken originally, but could be taken, is to demonstrate that you have adequate strength so that you don't fail the rods under the loads that are expected during a LOCA.

And I think when you examine the industry proposal and the approach that we're taking, you will see that they depart right here, and for retaining ductility, I don't think there is much of an argument about the adequacy of the ring compression test, but there is another way of doing it.

CHAIRMAN POWERS: Suppose that I came along and I said, "Gee, what I read the Commission is saying is that they want to keep the fuel rod. That's what they really wanted to do."

DR. MEYER: Right.

CHAIRMAN POWERS: And so I calculate a bunch of loads on the fuel rod, and through some magic say, "Well, these loads are such that the fuel rod stays intact even at 50 percent oxidation," say.

What experimental database is there

available to me to show that I have the loads calculated correctly?

DR. MEYER: Well, I think that is the $64 question. That's where this discussion will come down, and that's the point that I believe the Commission sidestepped initially when they said, "We don't believe that we understand the details of the LOCAL process enough to do that."

CHAIRMAN POWERS: They said that in '73, and there has been a lot of water flowing over the dam.

DR. MEYER: That's a long time ago. That's right. That's right.

CHAIRMAN POWERS: And we're getting better and better calculational methodologies developed. The question is: do we know that those calculational methodologies are any good?

I mean, they're fancier, and the LOCA described in Appendix K is a stylized LOCA.

DR. MEYER: Yeah.

CHAIRMAN POWERS: So you would have to know a lot more about the range of LOCAs you could have, wouldn't you?

DR. MEYER: The only thing I can say in answer to that is that in NRC's research program, we

have not investigated the loads or developed analytical methods to analyze the loads. Now, I think the industry has done some of that. We have not.

DR. KRESS: Are the loads mostly thermal expansion? Because you've already failed the -- you don't have the internal pressure anymore. That's gone.

DR. MEYER: That's correct. That's correct.

DR. KRESS: And you have the weight of the fuel and the thermal expansion and the constraints. The flowing steam is not anything. So is it mostly just thermal expansion loads we're talking about?

DR. MEYER: Well, not entirely. You can imagine axial loads from constraints within the fuel bundle. The Japanese have done tests where they apply axial loads. Many of us think that the axial loads that they apply are excessive, but in the extreme what they will do is allow the rod to go through its ballooning, bursting, heat-up and get up to its maximum temperature, and then grab it in an Instron machine and hold it.

DR. KRESS: Hold it? Okay.

DR. MEYER: And then quench it, and now it snaps. If the oxidation percentage is as low as eight or nine percent, if they don't grab hold of it, it survives the quench with the oxidation levels as high as -- I don't know -- 28, 30 percent, even more than that.

I don't know. The fuel is ballooned. Its neighbors are ballooned. We assume that it's not coplanar. They're going to be interlocked in some way. All of that corrosion is taking place during the transient. The grids are probably going to corrode also.

DR. KRESS: I see.

DR. MEYER: From NRC's side, I think we are unprepared to say anything quantitatively about those loads and have thus planned to go along the path where we don't have to answer those questions and hope that it's the past of least resistance and will get us to a revision of the rule that is in many respects just a refined image of the original rule.

But it's not the only way that the job could be done.

CHAIRMAN POWERS: Do you have to answer the question of what is enough ductility?

DR. MEYER: Well, you may force me to answer that question. Originally the answer was just not zero, but I'm going to move on now and show you where even trying to use that concept we run into a problem.

CHAIRMAN POWERS: Okay.

DR. MEYER: So let me finish this slide and I'll get right to the subject that I think you're interested in. So we're going to do the ductility test, the integral test.

Now, the integral tests are where we take fueled segments of high burn-up rods. They're about 15 inches long. The fuel is inside. We weld the end plugs on them, pressurize them to an appropriate level, heat them up through a stylized transient. They balloon; they rupture; they oxidize; and then they're cooled and quenched in what we believe is a typical manner.

Now, we presume they're going to survive the quench at the oxidation levels that we are using, and so we're going to take those surviving specimens, turn them sideways in a four point bend apparatus and break them.

DR. KRESS: Does the quench somehow model the injection of the ECCS?

DR. MEYER: Model the what?

DR. KRESS: The ECCS injection.

DR. MEYER: In the sense that if we go to 1,200 Centigrade -- I'm flipping to Centigrade now -- if we go to 1,200 Centigrade that we will cool down to 800 Centigrade slowly and then quench, which I think is about the right way to do it.

It turns out that cool-down period is important because it will affect the way that hydrogen re-precipitates and aligns itself as hydrides in the cladding as it comes on down.

So then we're going to do these four point bend tests. Now, there are a limited number of the integral tests. We'll do dozens and dozens of ring tests on undeformed sections of de-fueled cladding. The integral tests are very difficult and very expensive. So we'll do maybe a half a dozen integral tests with Zircaloy-4 and a half a dozen with Zircaloy-2.

Now, oxidation tests are separate from those, and we've done quite a lot of those already where we take specimens and do isothermal anneals in steam to measure, to map out the oxidation kinetics.

DR. FORD: Could you just go through that sentence? I'm having trouble deciphering what

it means. Oxidation tests, you're measuring oxide thickness as a function of burn-up. Corrosion, oxidation is corrosion.

DR. MEYER: Now, are we talking about this one?

DR. FORD: Yes, yes.

DR. MEYER: Okay. So this is high temperature oxidation at a fixed temperature during a hypothetical LOCA, and we're going to do this at several temperatures because you want to map out the temperature dependence. So we'll do some tests at 1,200 Centigrade, some at 11, some at ten, maybe some at 1,300, and now we can do this on specimens that have different burn-ups, different corrosion levels with the same burn-up, and get the effects of these variables on the oxidation.

DR. KRESS: This is just to expand on Baker-Just or Cathcart Pawel or --

DR. MEYER: Mike, you're going to show some of these?

MR. ROSENTHAL: Well, I think what was very nice is the side benefit from this program was that the data points were lying right up on top of Cathcart Pawel, very well, and that's what's used in the best estimated ECCS calculations. That gives

you some inferment (phonetic).

And, in fact, that's what we would advocate. That wasn't the original intent of the program, but it's a very nice side benefit.

MR. BILLONE: Mike Billone from Argonne.

Just to clarify, the term "oxidation" is referring to high temperature steam oxidation. Corrosion refers to the low temperature phenomenon in the reactor. So it's all oxidation, but the terminology is different.

CHAIRMAN POWERS: Jack, let me follow up on something. You probably didn't have anything to do with what you were saying when you said, gee, all of the data points are falling on Cathcart-Pawel, and then I read the report coming out of the Quench workshop.

MR. ROSENTHAL: German work.

CHAIRMAN POWERS: Yeah, that says something about using Prupach or Klett (phonetic). That's for higher temperature work?

Okay. But they're okay with Cathcart Pawel at these temperatures?

DR. FORD: One of the things, you say "alloy type." What about ranges of composition within an alloy type? Fabrication procedures,

they're going to affect the kinetics. Are they covered?

(Laughter.)

DR. MEYER: They're covered perhaps not in a systematic manner, but we have unirradiated materials. We have quite a range of unirradiated materials in the lab up at Argonne. We've got Zircaloy-2, Zircaloy-4, M5, ZIRLO. We also have E110, several varieties of the Russian E110, and so we have tested all of those, and you're going to see -- I guess you're going to show some of the birchbark stuff. You're going to see some wild differences in which some do appear to be related to fabrication, but perhaps not the things that might jump to mind, like cold work and things like that; more perhaps related to impurities or the source of the ore or the reduction process, the chemical reduction process that's used because they leave different kinds of impurities in the metal.

And so we do see some of those, but if you avoid getting into this, it's like good oxide and bad oxide. You know, we've got the good oxide is black, tetragonal, adherent stuff that keeps hydrogen out pretty well, and as long as that forms, Cathcart-Pawel seems to work.

DR. FORD: Right.

DR. MEYER: And even with the E110, the Russian cladding, when you're at very low oxidation levels, the kinetics look like Cathcart-Pawel, but with the E110 cladding, you get to a point pretty soon where the oxide form changes, and you start developing a white oxide that has a lot of cracks, lets a lot of hydrogen, and its rate goes --

DR. FORD: And aren't those outliers the ones that we should be really worried about rather than the best estimate average?

DR. MEYER: Well, we are worried about them, but we think that the original Commission wanted to retain --

DR. FORD: From a risk point of view, is that not one you're really worried with?

DR. MEYER: Our expectation is that we can figure out what they did that caused it to be that way and make sure we don't do that.

It looks to us like that the products that are being used in this country right now have a manufacturing process results in a robust, black, protective oxide coating at high temperature.

DR. FORD: But we're hearing comments about BWR fuel currently if you've got some

corrosion problems because of outliers, but aren't those the ones we should be worried about in this particular relationship? No?

MR. BILLONE: The BWR problems you're hearing about are at operating temperatures.

DR. FORD: Yes. That's the corrosion --

MR. BILLONE: But that may be a fuel cladding interaction based on special fuel pellets that were developed.

DR. FORD: Okay. Just pushing a little bit.

MR. BILLONE: There's nothing wrong with the alloy, the Zircaloy-2 alloy that they're using. There's a special problem that may have to do with the fuel.

DR. MEYER: Okay. Now, here is the sort of challenging situation that we've observed. So we're trying to preserve ductility. We think that we've retained ductility everywhere in the ballooned region because they have set up the regulation to apply the calculation double sided in the region of the balloon, and when we look carefully, we find some places in the balloon where even within the current regulatory constraints you may not have non-zero ductility, and --

DR. KRESS: Now, is that a local thing or is it circumferentially all around or --

DR. MEYER: I don't have a picture for this.

DR. KRESS: You know, i can conceive of a circumferential ductility --

DR. MEYER: Okay.

MR. BILLONE: The answer is both.

DR. MEYER: Yeah, it's both. It's both. Above the burst and below the burst, more or less symmetric locations, you have peaks of high hydrogen concentration. These come about from ID steam oxidation, release of hydrogen which can't get swept away because it's inside a stagnant area, and so it goes up to where it's a little colder, and it sits there, and you get these bands of very high hydrogen concentration in those two locations.

MS. YANG: Can I just add the clarification? That's been observed for low burn-up fuel or for unirradiated material. What is not clear is if it will appear in high burn-up fuel when the fuel pellet and cladding bounding are so tight that you may not have such a phenomenon. So that's something that needs to be --

DR. MEYER: That's true.

MS. YANG: -- to be demonstrated first.

DR. MEYER: And we'll find that our real soon.

And the other place where you have zero ductility must have been known originally, although the hydrogen wasn't, and that's just around the rim of the burst opening because the formula in the rule for calculating the oxidation limit is to take at the midplane of the burst the average cladding thickness, which you get from taking the cross-sectional area and dividing by a circumference.

Well, if you look at the cross-sectional area, it gets knife-edge thin as it comes right down to the opening, and it's 100 percent oxidized.

DR. KRESS: What temperature do they use for that?

DR. MEYER: What temperature? Well, this would be true at any of the temperatures where you -- suppose you're right at the --

DR. KRESS: Well, the clad is probably at the coolant temperature at that point.

DR. MEYER: We're talking about the high temperature. The burst occurs around 800, and then this thing goes on up to nine, ten, 11, 1,200 degrees Centigrade and comes back down. So the

burst is open that whole time, and this knife-edge thin region is oxidizing, and if you're anywhere close to 17 percent average, you're 100 percent in the thin edge, and it's fully brittle, and you've now got a nice place to start a crack that will run.

Mike will show you. Let's see. I may even have the picture myself.

Mike did in his hands a couple of four point bend tests, and this is one where the opening of the balloon was pointed towards him, and then he went like this, not touching the ballooned region, and it broke. A crack went down here and found the high hydrogen brittle region, and it broke cleanly in that region.

DR. KRESS: Which is upstream and which is downstream?

DR. MEYER: Huh?

DR. KRESS: Which part of this is upstream and which is downstream?

MR. BILLONE: For this test it doesn't matter, but the break is upstream.

DR. KRESS: It's upstream.

MS. YANG: And this is, of course, unirradiated material.

MR. BILLONE: Yes.

DR. FORD: Your final bullet says we're expecting integral tests, sure the fuel loss will be minimal.

DR. MEYER: Yeah.

DR. FORD: Now, if you go back to your previous picture, why do you say that the fuel loss will be minimal?

DR. MEYER: Okay. Here's what we're counting on. It's a nice, clean break. The balloon is not shattered. There is a lot of ductility in a lot of the surface area of the balloon. There is no ductility right there. There is no ductility here, and there is no ductility here.

You are not going to find this entire section smashed up into little pieces like a piece of glass because back in here you have non-zero ductility, and we're going to do tests like this. These are only crude, preliminary tests. But if you have -- now, we're not saying that the loads are large enough to do this, but if the loads would be large enough to break the cladding, you're probably going to get a clean break there or a clean break here, and in the constraint of the balloon, fuel pellets can't come raining out of that down onto the core plate.

DR. FORD: But why can't they come to the left, just come streaming out from the left?

MR. ROSENTHAL: Remember that I have a sea of fuel rods in a fuel bundle with the bridge spacers above and below, with failures that are not coplanar, and I think at least my middle model is that we end up with a coolable geometry when we're done, and it surely won't look pristine. It will be broken up, but that's okay, as long as we can insure coolable geometry.

MS. YANG: Yeah, and again, this is an unirradiated rod. So you get this rim for high hydrogen, and that's where the guillotine break occurred, and like we said earlier, we're not sure you will get that for high burn-up rods.

DR. MEYER: Well, I wouldn't count on not getting it because we've ruptured two high burn-up rods already, and what we found was that the balloon for all practical purposes looked exactly the same as it did in the unirradiated tests, and furthermore that the axial gas transport through the rod during the LOCA was essentially unimpeded, and we expected it to be throttled down, and we didn't see that.

So, I mean, it looks quite clear that

you're going to get ID oxidation. Now, whether there's some -- well, I just --

MS. YANG: We'll just wait and see.

DR. MEYER: Well, we're speculating now, and we're going to run the tests, and we're going to know pretty soon.

CHAIRMAN POWERS: Let me ask a question that simply reflects the fact that my memory is shot. I think the French came in and made a presentation to us, and didn't they show us -- I don't know whether they were X-ray or tomographic results that showed that when you got this ballooning, you had fuel pellets collapsing, not pellets, but fragments collapsing down into the ballooned region?

MR. BILLONE: That was a hypothesis.

MS. YANG: Yeah.

CHAIRMAN POWERS: I thought they showed us actual results of some of the early Phebus experiments. I mean, they were either tomographic or X-rays. I'm not sure which.

MR. ROSENTHAL: I believe that's tomography.

MS. YANG: I think I didn't see the presentation. It must be very low burn-up. I don't

think any so-called fuel relocation being observed for high burn-up fuel. High burn-up means even greater than 30 or 30,000.

I think when you have a large gap between the fuel and the cladding, it's conceivable you could have some kind of settling or the relocation, but I think what we're trying to demonstrate here is for higher burn-up rods. When you have very tight fuel and cladding bounding, I'm not sure you will have fuel relocation or even this hydrogen.

I think we need to wait and see. That's what most of these experimental programs are trying to find out.

CHAIRMAN POWERS: Again, I don't want to place a great deal of faith in my memory, but it seems to me that what they spoke of was a swelling of the cladding over some substantial length, and maybe it was like this, and they would have a somewhat larger ballooned region down here, but over the entire length things would fall down into this region. I mean, that's what it looks like.

DR. MEYER: We're well aware of the hypothesis, and we are looking for evidence of that in these tests. We also are trying to help design

the Halden test specifically to look for that relocation process.

You know, when you start dealing with random orientation of granules of stuff, then you start talking about packing fractions, and you've got to open up a pretty large balloon in order to get the same mass of randomly oriented particles that you had in the pellets.

And I think that that number is somewhere in the range of 65 to 70 percent strain on the balloon in order to get the break even point.

Now, we didn't see quite that much strain on our balloon specimens. We had 40 to 50 percent, and so I don't know. That's part of the mix, part of what we're trying to study, and I guess there's a lot of skepticism about whether it really can exist or not.

What we have found that wasn't expected was that we lose a little fuel from the ballooned area during the test. The blow-down seems to push out some finds, and that we might experience some cracks or severing of the fuel rod that probably won't shatter the rod, and it might let out some additional small pieces of fuel.

CHAIRMAN POWERS: The loss of a little

fuel finds can't be a surprise to you. I mean, that's been known since Malinowskus' (phonetic) work.

DR. MEYER: Yeah, okay. Well, I guess this is going to be the hardest part of the whole thing, is that at the end of the day we don't have a pure situation. We don't have ductility everywhere. We can't flatly say that it won't break.

Okay. What can I say here?

CHAIRMAN POWERS: Well, here you say something different than what you've been saying up till now. Here you say specifically "sufficient ductility," whereas up till now you've been very careful to say --

MR. BILLONE: "Some."

CHAIRMAN POWERS: -- "some."

DR. MEYER: Yeah.

CHAIRMAN POWERS: Non-zero.

DR. MEYER: Yeah, but I actually don't know the difference. Sufficient ductility in my mind as I wrote this was that that band of high hydrogen was not so big that it knocked a big section out of the tube or that the rim of heavily oxidized material produced a shattering, gaping hole in the side.

If the test results show that it's fairly clean and tight, then I would say that's sufficient ductility, and that's all I meant there.

Okay. So as I mentioned before, we're going to try and demonstrate with high burn-up zircaloy and with unirradiated ZIRLO and M5 and sort of put it all together and see if it looks like that the current way of doing business is sufficient, and that would give us a basis for leaving that in the rule as an option without change other than the database that we're generating should be applied to the grandfather part of the rule because we've got M5 in the laboratory.

CHAIRMAN POWERS: More importantly, you have it in the reactor.

DR. MEYER: And we have it in the reactors. The performance based criterion would be an option, and the current thinking is to simply specify a ductility test, and perhaps describe the details of this in a regulatory guide, and from this ductility test, a licensee would then generate the temperature limits and oxidation limits that would correspond to the zero ductility point in the test.

This, in fact, could then turn loose the peak cladding temperature from its 2,200 degree

limit right now because it's quite easy to imagine getting a ductility criterion at 2,300 Fahrenheit and 14 percent oxidation or something like that, and so it might be necessary to rethink the peak cladding temperature limit.

It's a curious situation, the peak cladding temperature limit that's in the rule because it was ostensibly put in the rule as part of the embrittlement criteria. It was known that if the oxidation had taken place at a temperature much above 2,200 Fahrenheit or 1,200 Centigrade that the diffusion of oxygen into the prior beta region would be higher and you'd get more oxygen in the part of the metal that was giving you your ductility.

But the dependence on temperature was not very apparent in the original data. I guess Hobson's data at 2,400 Centigrade showed some enhanced hydrogen in the prior beta region, and in principle everyone agreed that the effect would be there, but it was not like you had plots of embrittlement criteria as a function of temperature and at 2,200 degrees the correlation fell apart.

There was, in fact, another consideration, and the other consideration that was discussed in the Commission opinion was one of

excessive metal water reaction in relation to run-away temperatures, and we've looked at that, and we've looked at the Cathcart-Pawel correlation, which appears to work well for everything we've studied if it doesn't develop the bad oxide in comparison with the Baker-Just correlation, and just by coincidence the metal water reaction heat, like Cathcart-Pawel at 2307 is the same value that Baker-Just has at 2,200.

Norm Lauben has done a lot of RELAP calculations to look at the margin that you have to where the heat balance gets unfavorable and the temperatures run away, and so it looks to us from the preliminary work that we've done that if you allowed temperatures as high as 2,300 degrees Fahrenheit that you might be preserving the same margin to run-away that the Commission would have thought they had initially.

That's just a reference point, but if one finds that the embrittlement criteria are coming in with temperature limits higher than 2,200, you might have to think through the metal water reaction arguments a little bit and perhaps put some additional limit on it.

Now, I think that's all I had. So I'm

finished.

CHAIRMAN POWERS: Good.

MR. ROSENTHAL: I just want to reiterate that we're sharing with you our thoughts on the way we might go. There is not uniformity amongst the staff yet or any sort of decision yet on how we might go.

We also have stakeholder input to consider, and so this is where we are in our thinking at this time, and we really would appreciate; it would be a very timely time for ACRS to provide this.

DR. MEYER: I want to underscore that and say that the reasons for even discussing things as specifically as we have is that we're trying to generate a database to support something, and you need to have a concept of what the something is that you're trying to support. So we make up the mental models of what the something is and plan the program to support that.

CHAIRMAN POWERS: Good. Well, we'll ask by the end of the day.

Okay. Thank you, Ralph.

MS. YANG: Mr. Chairman, can we give a short presentation just to describe what the

industry position in terms of the type of data that should be generated?

I don't really want to have a debate here, but I just thought it might be helpful at this point to at least briefly describe what an alternative suggestion here is.

CHAIRMAN POWERS: You've got 12 hours tomorrow.

MS. YANG: Okay.

CHAIRMAN POWERS: I want to move on with Mike talking about the LOCA test results.

MR. BILLONE: Are you guys okay with lunch? It's going to take me an hour.

CHAIRMAN POWERS: You've got an hour.

MR. BILLONE: All right. I'm going to take you back a few years. I'm going to use the viewgraph projector.

CHAIRMAN POWERS: Oh, good man.

MR. BILLONE: And I also have some chalk for demonstration.

Okay. Ralph, do you still have that pointer?

DR. MEYER: yes.

CHAIRMAN POWERS: Thanks. One that works. I usually point it at someone's eyes.

All right. I have one presentation, and I have a bunch of back-up slides in case I've anticipated your questions correctly. We'll see.

I also like to move around. I hope that doesn't cause a problem.

In the handout you have, it's rather long, and I think the way to approach it -- first all, we have to get rid of --

CHAIRMAN POWERS: Yeah, we have to get rid of --

PARTICIPANT: Ralph, how do we get rid of this thing?

MR. BILLONE: You could always shut it.

PARTICIPANT: Well, the question is how to turn it off.

MR. BILLONE: You just rotate it.

CHAIRMAN POWERS: No, it's up here on the projector.

MR. BILLONE: Oh, I'm sorry. I'm sorry.

CHAIRMAN POWERS: Just go ask Aaron to come help us. Charge ahead, Mike. We'll read them off the handouts if nothing else.

MR. BILLONE: Okay.

CHAIRMAN POWERS: The first one tells us your name.

MR. BILLONE: Yeah.

CHAIRMAN POWERS: And even the date, which is always useful for me because I never know what day it is.

MR. BILLONE: Okay, all right. So we're going to talk about LOCA test results generated at the Argonne program -- oh, this is a nightmare -- and I'd like to acknowledge my colleagues, Yung Yan and Tanya Burtseva. They like to work. They don't like to talk. I like to talk. So I'm here. Okay.

CHAIRMAN POWERS: You might twist the knob there and get us a little bit in focus or I'll think it's me.

MR. BILLONE: Oh, good. Thank you. Thanks a lot.

All right. In this morning's presentation I'm going to talk about our LOCA relevant research. I'm going to pick up the dry cask storage in a later presentation. I'm going to his our advanced alloy post-quench ductility testing of unirradiated material, steam oxidation of high burn-up Zirc-2 and Zirc-4 cladding, LOCA integral tests with fuel, boiling water reactor, and PWR cladding. That's to be followed by post-quench ductility of high burn-up LOCA integral test

specimens, and we've also had proposed several ramp-to-burst tests with varying the heating rate and the internal pressure in the program.

Let's hold off on these two until later this afternoon. Let me just give you an idea of the materials we have at Argonne. I'll go through this list quickly.

We have a variety of Zirc-2 designs, eight by eight, nine by nine; ten by ten is to be provided; a variety of Zirc-4, normal Zirc-4 archived to our Robinson cladding, and low tin 17 by 17 provided by Westinghouse. Framatome is also going to provide us with some.

We have ZIRLO provided by Westinghouse, M5 provided by Framatome, and a variety of the E110 claddings. The focus of our program is really the alloys used in the United States. The E110 is here to try to understand why it behaves the way it does and make sure that none of these alloys are on the edge of some kind of cliff.

I'll show you the table of the irradiated fuel rod segments we have at Argonne. Some of these are for dry cask storage, and we'll come back to it, and on this table, would you please correct a wonderful typo? You've got an 1888 for a

discharge date for the Surry reactor. So would you please make it 1981 for me?

But we have for PWR cladding, we have the Robinson, which has primarily for the LOCA program 64 to 67 gigawatt days per metric ton averaged over the whole fuel column. It gives you an enrichment, Zirc-4, and gives you a discharge date.

Limerick is the BWR cladding, which I'll show you some results for. The pins that we're testing are 56 to 57 gigawatt days per metric ton, and this is lined cladding. About ten percent of the wall thickness is zirconium, low alloy zirconium on the ID of the cladding, and this is about .7 millimeters in thickness.

So for LOCA we're just going to be talking about these. I'll come back and pick up these other two when we talk about dry cask storage.

All right. The nice thing about some of the variables of the LOCA test, if we go to the Limerick test, you have very little oxide, something over ten microns, but some tenacious crud, and because you have very low oxide and it doesn't vary axially very much because your coolant temperature is pegged at about 288 degrees C., you only have

about 70 ppm of hydrogen that you picked up from in reactor corrosion.

If you contrast that with the Robinson, which is more typical of a pressurized water reactor with an increase in cooling rate as you move along, you've got up to 110 microns of oxide, and as far as what we measured, up to 800 wave parts per million of hydrogen.

So Robinson is very interesting because if you want to study the effects of hydrogen, you could go to gridspan four with high hydrogen content. You can go to gridspan two with low hydrogen content, all with the same irradiation conditions.

So, again, these two would be for our LOCA relevant program. Okay. Let me just summarize where we are in each of these.

For the advanced alloy post-quench ductility study, we received cladding over a period of time. We did extensive validation, looking at temperature responses, metallography, hydrogen pick-up, oxygen pick-up, and our test matrix calls for tests at 1,000, 1,100, 1,200, 1,260 degrees C.

We've completed the results for all alloys oxidized at 1,000 degrees C. and 1,100

degrees C., up to a calculated ECR of 20 percent.

We've also measured the ECR by measuring the oxygen pick-up. So when I give you results, I give you results versus measured ECRs.

We completed our E110 study as far as we can go, with emphasis on oxidation at 1,000 degrees C. The alloy is particularly challenged at 1,000 degrees C.

By "completed," I mean we've oxidized the samples and done all of the ring compression tests. We intend that each one of these temperatures in the single ECR to do a four point bending test of a balloon and burst sample of the advanced alloys. We would call our LOCA integral test followed by LOCA ring compression test.

That's our current plan, and that's subject to input from the interested parties as to what other tests might be done.

All right. For those oxidation tests of unirradiated alloys, this is the kind of temperature history. We have a fairly rapid ramp-up to about 100 degrees C. from our gold temperature, slowing down so that we don't overshoot. We hold for a certain amount of time, depending on how much oxide you want, oxidation you want.

Slow cool to about 800 degrees C., and what's not shown here is the rapid quench. We have the water hit the sample, and the sample temperature is about 800 degrees C.

So that's what we expose small samples, 25 millimeter samples to, and then we proceed to do ring compression tests on those and look at oxygen and hydrogen pick-up on those samples. So that's for our advanced alloy program.

Let me give you a quick summary of where we are on the LOCA program. Of course, we do oxidation kinetic studies. The Limerick has been completed. The Robinson is about to start.

Let me go down here because this is more the emphasis of my talk. Our LOCA integral tests currently are pegged at the 2,200 F., the 1,204 degrees C. peak temperature, and for a time range of one to five minutes.

Five minutes turns out to give us a Cathcart-Pawel calculated ECR of about 20 percent peak in the burst region. We're measuring somewhere around 18 to 19 percent. So this would be an over test relative to the criteria, but an interesting test relative to phenomena.

We're coasting along last year. We had

completed a Limerick ramp-to-burst test. That's an actual irradiated fuel segment, and then ramp-to-burst followed by oxidation for five minutes at 1,204 degrees C. That was about a year ago.

Then we lost about a year because our hot cells were essentially shut down for major maintenance, and so we were back to where we were last year, and I'll show you where that is.

When we looked at these two samples in detail based on nondestructive results -- that means looking at profilometry of diameter changes and photography, we saw more similarities than differences between the unirradiated Zirc-2, which had zirconium pellets in it tested out of cell, and the irradiated with fuel tested in cell.

We're in the process -- and Rosa brought up this point -- of determining axial profiles of hydrogen pickup and oxygen pickup, and the only thing it might save you -- I'm sorry. That's too dramatic. I'm supposed to present data. I'm not supposed to be melodramatic.

The issue of whether you pick up hydrogen inside the high burn-up rod is not so much the fuel cladding tight bonding because the cladding is going to expand away from the fuel. It's the

oxide layer that you pick up in the reactor on the ID of the cladding, and the question is we have evidence we know it's not protective against steam oxidation. It will oxidize just the same as air cladding, but does it prevent pickup of hydrogen? That's what we're in the process of determining.

We're hoping to run the Limerick test with quench this month, and then initiate the Robinson test, the PWR test with the high oxygen and hydrogen levels in the fall of 2003.

Let me show you where we are with this Limerick test. And off line, if someone wants to know what we've been doing with our hot cells, I'll tell you. I don't want to start that story because it sounds like a sob story of complaining.

This is our stylized -- I never knew that term "stylized LOCA" -- this is our stylized LOCA. What we have run is at room temperature pressurizing the top of the sample, having pressure transducers at the top and the bottom, and measuring permeability or time response to the bottom transducer, which was much higher than we thought, meaning that the pressure equilibrated much quicker than we thought for high burn-up fuel.

Then we depressurized, went up to 300

degrees C., really 300 even if it doesn't look like it; repressurized, did the same test, and got high permeability; introduced steam, ran up to burst, and actually in this first test we didn't have steam. We had argon. We ran up to burst and then stopped. That was the first test.

The second test went through this sequence of five minutes. A program cooled down three degrees per second, and then we quenched in the cell a year ago. So we did slow cooling, but that test was -- those two tests were completed a year ago.

And what we're shooting for now is this same sequence, only with the quench hitting the sample at 800 degrees C.

Okay. There was a tremendous amount of movement in our hot cells and moving radioactive material away from half the hot cells so that the shield window could be repaired, trying to move it back. Equipment got damaged, and we need to test out all of our sample preparation techniques, which we were doing very quickly, as well as the LOCA apparatus.

This is the particular Limerick rod we're working on right now. It's called J4. This

is a gamma scan. It gives you a rough idea of the burn-up profile, and we've just cut these three new samples from this rod. These are two good samples that we'll use in our testing. This sample which is in the down slope of the power profile or the burn-up profile we're using to practice removing fuel from about half to one inch from each end in the welding end caps, and that's going on today.

Hopefully that's successful. We'll move on to these two this week and we'll have two samples ready to go.

Let's skip that one. I'm going to skip some slides as we go along.

A quickie. Let's go back now and do some details on the advanced alloy program and the high burn-up program. So we'll get into details now.

Basically our approach, we know very well that alloys like M5 and to some extent E110 have this unusual behavior at 1,000 degrees C. where they oxidize at much less than Zirc-4 and the rest of the alloys. What we're going to do is use a calculated Cathcart-Pawel time to set our test matrix, which means we're going to go up to 20 percent calculated ECR, and we'll also, as I said,

measure the ECRs.

That means that these corresponding temperatures or double sided oxidation, these are the maximum times that we're going to oxidize these samples. This is close to an hour down to minutes, depending on what the peak temperature is.

What's interesting, as you go up in temperature, you're increasing the oxide solubility in your ductile layer, and eventually if you keep going up, that ductile layer will become embrittled by oxygen.

So we determined the measured ECR based on weight gain. In the process of doing this, we want to look at the oxidation kinetics because we're generating the samples by oxidizing. It's useful data, as well as the post quench ductility data, and the approach is to compare the results for ZIRLO and M5 to Zirc-4 and Zirc-2 data when we get the appropriate Zirc-2.

There seemed to be some sensitivity on the part of the vendors who gave us the cladding that these two alloys not be compared directly on the same graph. So I will show you graphs of ZIRLO compared to Zirc-4 followed by M5 compared to Zirc-4, as opposed to one nice, simple graph, and I'm

going to respect that sensitivity.

We've explored factors that may contribute to E110 behavior. We certainly confirmed that it's very poor post quench ductility performance at low test times, particularly at 1,000 degrees C.

We've explored the effects of surface roughness and surface chemistry on oxide instability and got some interesting results in being able to delay the instability by smoothing the surface.

And we've done some characterization of both chemistry, metallography, SEM, and some TEM. The moral of this story is there's more than one reason why E110 will behave the way I show you it behaves, and some of the things that we could do, we don't manufacture E110. All we could do is work from the outside and play with the surface. It may delay the instability, but it doesn't eliminate the instability.

Okay. Very quickly in terms of apparatus, I don't want to get into too much with apparatus. Basically, this is a 25 millimeter long sample. This looks like overkill. This is a quartz tube, and steam enters from the bottom. It's held in place with Inconel holders, and isolated from

those holders by something that we'll see in the next section.

We have the thermocouples coming down through here, through the top, and steam exiting at the bottom. Let me show you how we get double sided oxidation out of this with the next slide. This is just an enlargement of that test section.

Basically we have the steam -- well, I can tell the thermocouples are head to the top. So I know this is the bottom. We have steam flow within the quartz tube coming this way. We have three or four holes substantially, a bottom for steam to get in. This is hollow. Steam could continue on, but it's too long of a path, and it gets cool. So steam would condense. So we put holes for steam exit there.

Our sample is here protected from the Inconel with aluminum spacers and zirconia washers, and that's our basic set-up. We only run one sample at a time for each of the alloys under each of the conditions.

Okay. Let's talk about good oxide and bad oxide, and let's put some fancy words to it, and let's show some pretty pictures. Protective oxide layers. This is under high temperature steam.

Generally in appearance they're lustrous black. They're a particular phase of the material called tetragonal, and they are ZrO two minus X. They're hypostoichometric. They're slightly under the two to one ratio.

You need this at temperatures at 1,100 degrees C. and below because this form of oxide is not thermodynamically stable in 1,000 degrees C. or 1,100 degrees C. However, it is stable under compressive stress and that forms under compressive stress, and it's stable for the hypostoichometry.

So you rely on those two things to give you the good oxide. If you have that, how can you lose ductility? Protective means protective against hydrogen pickup, and it means that oxidation is diffusion control.

Well, if you keep going in time, you will bend the effective ductile layer as you increase time at temperature or weight gain and ECR. If you increase temperature, go to 1,260 and beyond, you will increase the oxygen content in that ductile layer, and it will become brittle.

Also, there's a chance that obviously with high burn-up you could have the effects of hydrogen causing embrittlement from in reactor

corrosion, and I'll show you what happens during LOCA ballooning and burst of unirradiated cladding in terms of hydrogen pickup.

So these are mechanisms in which eventually you're going to go to zero ductility.

There is also not so good oxide, and this is classical break-away oxidation which we've observed for Zirc-4 and M5. We would observe it for ZIRLO if we tested ZIRLO, but it's something that happens at very, very high, long times, like three hours at 1,000 degrees C. We're not studying this because we don't think it's LOCA relevant. We could study it, but it would be of more academic interest.

What we have looked at is what happens to E110 because this classical break-away oxidation, after your oxide grows big enough, it's something that happens from the outside layer and moves in. E110 seems to develop an instability right at the metal oxide interface, and we see local enhancement of the oxidation rate, local enhancement of hydrogen uptake at 1,100 degrees C., and then -- let me do this with pictures rather than words. That's too many words.

Okay. Top picture. Good, lustrous -- well, it's hard to get lustrous black to show up.

That is lustrous black, and believe it or not, that's Zirc-4 after about 870 seconds, which measures out to about 18 percent ECR in steam at 1,100 degrees C. It only picked up eight weight parts per million of hydrogen. It was fabricated with five and it only picked up eight during this process. That's very low.

E110 looks the same after you ramp it for 75 seconds up to 1,000 degrees C. and you only hold it for five second. It kind of looks like this until you look under high magnification. You see these very small white spots. These white spots will grow. So the point is they form during the temperature ramp, and they will go very unstably as shown in the next picture, almost the next picture, not quite.

Let's look at the good stuff first. One of the things we did was we looked at metallography for a couple of reasons. We want to make sure things are going okay. In other words, we're growing an OD oxide and an ID oxide of about the same.

We know this is brittle. We know that the high oxygen alpha phase, the white stuff you're looking at, is even more brittle. So from a post-

quench ductility point of view, you throw this away, you throw all of the white stuff away, and what will give you ductility is this gray stuff. In this picture it turns out gray. That's what's called the prior beta layer.

As long as this is not loaded with hydrogen and as long as you didn't ramp the temperature up too high so it's loaded with more oxygen, that's where your ductility comes from.

So if I took this sample and exposed it to a ring compression test -- hopefully that's my next slide -- traditionally in the ring compression test you get four snaps, four breaks. It breaks into four points, and this is the load that you're applying to the ring. This is the displacement, and this is the methodology we use. This is the effective elastic part which we're not interested in. It's this part here: do you have any ductility?

And from that previous picture you should. You had enough gray stuff in that picture and it was low in hydrogen, and this comes out to if you divided this by about -- if you multiply this by ten, you get percent coincidentally. So this is about three percent plastic deformation that you get

before you start this cracking.

It's probably a little bit more in that this may not be a through-wall crack. To get four cracks, this might be one through-wall crack. This might be a second through-wall crack, a third, and then a fourth.

But the point is that previous picture does have ductility, and I want to make the point that we don't simply rely on this picture to tell whether or not we have ductility or not. We use this offset method to determine plastic deformation that's classical with ductile materials. We look at the metallography to make sure we have ductile materials, and we measure the hydrogen content to make sure we have an embrittlement with hydrogen.

That's the good stuff. All right. Let's go to the stuff that's still kind of a mystery to us, but this is E110 at 1,100 degrees C. In this sample you can see those white spots have grown. They've cracked. They've interlinked a little bit, and you've picked up some hydrogen at each of these cracks, but only about 200 weight parts per million.

It turns out that this sample with the oxygen and the hydrogen is brittle. If you cut this underneath the white spots, you will see the

enhanced nodular oxidation. This is all oxide.

Under the black spots, you'll see the thinner oxide. So this is not what is treated by a Cathcart-Pawel model or any of the other models. This is an instability.

So we're calling white bad and black good, reversing the process. That's 1,100 degrees C. The alloy is not too bad at 1,100. It's better at 1,200. It's a disaster at 1,000 and probably worse at 950.

So let's take E110 for a very small time, 300 seconds, and then a longer time, 1400 second at 1,000 degrees C., double sided oxidation, and if you look at the surface of this, it's ugly. I mean, all of this gray or white stuff is the kind of oxide that cracks and allows hydrogen pickup, and it has picked up about 120 ppm of hydrogen at this very low calculated ECR.

And if you look underneath this gray area and take a cross-section, you can see that it's actually cracked and delaminated, and that allows steam to come in direct contact with the metal, but let's go on in time.

This is 1,400 seconds at 1,000 degrees C., and you have a mess, but you can actually

describe it. All of these areas interlink and this whole thing becomes essentially white oxide. It cracks, it spalls, it delaminates. It picks up 4,000 weight parts per million hydrogen. You don't even have to test this. This is brittle.

So what we did is we explored the transition between this picture, and it turns out this is ductile. It's very high ductility, but a couple of hundred seconds later it has got zero ductility because it's going to continue to pick up hydrogen. So when it gets to about 400 ppm of hydrogen, a little more oxygen, then it does go brittle.

So somewhere around 500, 600 seconds is when E110 goes bad at 1,000 degrees C., but really keep in mind that the seeds of all this were right at the beginning when you were starting up the high temperature. Those tiny white spots accrued.

I'm not going to show you much on E110. So let me just say that we were able to delay this significantly by simply polishing the surface of E110 because a rough surface can disturb the compressive stresses. As a matter of fact, the ends of the sample can disturb it from E110. Welding a thermocouple on it can disturb it.

There's something else causing this instability, but you can as a catalyst, think of it as a catalyst. Roughened surfaces, certain surface chemistries, discontinuities will all make this happen much, much sooner.

MR. CARUSO: The picture on the right, is the black area fuel? Is that fuel pellets or is that just an underlying --

MR. BILLONE: No, no, no. This is epoxy.

MR. CARUSO: No, no, no. On the right.

MR. BILLONE: This?

MR. CARUSO: Yes.

MR. BILLONE: This is E110 cladding. I mean it starts out like this with no fuel in it.

MR. CARUSO: I'm trying to understand the scale. Is that the same scale as the one on the left?

MR. BILLONE: Approximately. These are approximately the same scale.

MR. CARUSO: So it looks like it has shavings that have come off?

MR. BILLONE: Yeah. It spalls. I mean, if you look at this at low time and you keep going on in time, this eventually will -- well, this is a

little bit of spalling, but it will eventually -- I'm sorry -- delaminate. This is delamination. It separated from the base metal. It will eventually spall off, and then you will grow new oxide. It will also be bad. It will spall off.

So all of this is oxide that you're looking at.

MR. CARUSO: But the black area in the middle --

MR. BILLONE: The black is sort of a dull black oxide between this and the base metal that has grown.

MR. CARUSO: How much of the base metal did you lose to those shavings? What percentage?

MR. BILLONE: This our Russian colleagues measure for us. We lost so much of it the measurement was meaningless, but somewhere around ten percent of the zirconium was oxidized to cause this picture, somewhere around ten percent.

But really five, six, seven, eight -- between seven and eight percent is where you went completely brittle, long before you got to this picture.

There's no fuel here. This is all ugly cladding basically.

DR. FORD: Mike.

MR. BILLONE: Yes.

DR. FORD: It concerns me that, you know, you're doing a lot of correlation between the damage, the burst, and the fracture of the zircaloy cladding, and the appearance of the oxide, and yet I haven't heard once anyone talk about the relationship, the well known relationship between nodule oxidation which you're showing there and general oxidation and the fabrication procedures for the cladding and the compositions.

And you're only looking at four or five specimens. Is there anywhere in your methodology that you look at the past history of the last ten years for the development of optimum cladding, compositions, and how you can fill in the experimental program that takes into account the variability that you will have in these alloys as far as composition is concerned?

MR. BILLONE: Well, we did a lot of probing because in some of our tests the inner surface oxidized a little different than the outer surface. We had to ask the question: is there a different treatment?

I mean, there's etching and there's

polishing, and there's all kinds of variables, and essentially we were able to track over the last ten to 20 years the evolution, and the evolution is such that where they used to etch as a final step, remove as much as 25 microns from the OD, they don't do that any more. Their final steps are polishing.

And when we play around, we did etching and an oxidation, and we got some strange results. We did polishing and oxidation, and we got some very good results.

They seem to be going in the -- they seem to have arrived in the right direction long before we discovered the importance of these variables, we at Argonne.

DR. MEYER: This is Ralph Meyer.

Could I comment on this, too? Because I think I know the itch you're trying to scratch.

DR. FORD: Yeah.

DR. MEYER: In the BWR nodule or corrosion, it was related substantially to the distribution of the particles and to the beta quenching and the temperature controls subsequently.

There's a parallel program going on through Kurchatov Institute in Moscow, which is working very closely with us, and they are also

doing some of the same things that we're doing at Argonne, but they have different cladding specimens available to them.

And what they've found was that there are other features that seem to be controlling this not necessarily related to the beta quench. I'm not saying that we've ruled out the beta quench, but one thing that they found. They had a batch of tubing that was made with a western ingot of zirconium, and they claim they put that through the same tube fabrication process as standard E110, and they got a product that did not show this white oxidation like you see here. It's called G110.

So now this raises the possibility that the impurity content which you would expect to be different between the electro-refined Russian zirconium metal and the chemically refined Western zirconium ingot might be different.

So at the present time we're aware of several things that seem to affect this. Second phase particle size is one of them. Source material is another one. Surface condition is another one.

Mike is not able to investigate all of these at Argonne because he doesn't have the variety of materials that are available in Russia.

DR. FORD: Right.

DR. MEYER: But we're able to get more of those varieties into the test program in Russia and have come down to that point.

We will get an update on the Russian work at the Nuclear Safety Research Conference near the end of next month.

MR. BILLONE: Okay. Sorry.

DR. FORD: And another thing. Again, skipping through your graphs, I see no mention of the Zircaloy-2 from Limerick, which was presumably barrier fuel.

MR. BILLONE: No, no.

DR. FORD: There's no barrier fuel in this?

MR. BILLONE: I have the Limerick Zirc-2.

DR. FORD: Oh, you do?

MR. BILLONE: As a matter of fact, the next picture is Limerick Zirc-2, not the high burn-up. So let me get to the next picture.

DR. FORD: Okay.

MR. BILLONE: Let me try to be clear when I'm talking about Limerick Zirc-2 in these pictures.

DR. FORD: Right.

MR. BILLONE: So okay. I want to show you the results of what we've done, which is the ring compression tests. They're to be followed by four point bend tests, and based on our experience with Limerick Zirc-2 unirradiated, their potential failure locations under four point bend tests and modes in uniform bending are the burst region, which is thin, flawed cladding, high ECR, and oxygen embrittlement, and the neck regions which are thick, and an unclogged cladding. Most of those things are good. Low ECR, but very, very high hydrogen.

And there's a transition here which may render the whole burst region basically lacking in ductility, and we'll see what we mean by that.

Let me go to that picture now. We'll come back to it because it really wasn't part of this high burn-up program -- I mean, sorry, it wasn't part of the advanced alloy program. And Ralph and Rosa, who have seen this picture, we've added more points. Odelli, we keep adding more points.

Basically what I'm going to give you is distributions of hydrogen, and this is really an oxygen distribution converted to ECR, starting at

the burst center and moving below and, well, above. Okay. This is a distance above the burst center.

So this is going towards the top of the specimen. This is going towards the bottom of the specimen. And really we get about a 158 to 170 millimeter balloon in our samples, but what you see is in the burst region. Of course, you have the highest oxygen pickup relative to the thickness. It's the thinnest material, and this is averaged over the circumference.

And then as you move away, this is still in the balloon region. You haven't gotten to the neck region. Your hydrogen for the unirradiated material which has room to pick up hydrogen, it has zirconium pellets inside. These hydrogen contents are so high that this is guaranteed to be brittle. It might be stronger in this region, but it's definitely lacking in ductility.

And even as you go -- let me work on this side -- as you go to decreasing hydrogen, you're going to increasing oxygen, and so in terms of ductility within the balloon region, let's just say that this whole area has the potential for acting in a structural sense like a brittle material if you're going to subject to bending, and we'll

come back and --

DR. FORD: This is Zircaloy-2 from Limerick?

MR. BILLONE: This is Zircaloy-2 from Limerick, unirradiated, unirradiated.

DR. FORD: Right.

MR. BILLONE: And so what we're doing right now with the tests we ran last year --

DR. FORD: I guess I haven't given my concern.

MR. BILLONE: Okay.

DR. FORD: If it's from Limerick, presumably it's a barrier fuel cladding, i.e., it's got zirconium on the ID.

MR. BILLONE: Right.

DR. FORD: Zirconium is going to oxidize like crazy, is it not?

MR. BILLONE: No. There's no difference in the high temperature oxidation of zirconium, Zircaloy-2, Zircaloy-4, da-da-da-da-da-da-da-da. The temperatures of 1,100, 1,200 degrees C.

DR. FORD: Okay.

MR. BILLONE: And what I'm suggesting to you is this is not particularly Zirc-2. This is well known phenomenon that demonstrated Zirc-4 in

1981. Only the magnitudes weren't as great, and all the cladding alloys to some extent will have this qualitative picture when tested in the unirradiated condition.

That's my prediction, but that's what we're in the process of doing, is testing all of the alloys under the balloon and burst condition.

But I want to show you this now and then I want to come back to it because my demonstration tests and my pictures all pertain to something that looks like this in terms of oxygen and hydrogen. That's why I wanted to hit it early. I'll hit it again soon.

Okay. In my back-up slides I have a lot of graphs. I'm not going to do the graphs. I'm going to try to do it this way.

When we look at the data results for 1,100 degrees C. oxidation temperatures, and that was up to 1,100 seconds coincidentally, Zirc-4 and 5 and ZIRLO data are all in agreement with the Cathcart-Pawel prediction. I think I do have a graph of that. I just didn't identify the points, meaning within plus or minus ten percent.

So 1,100 degrees C., the oxidation kinetics are very similar for these three alloys.

We could not get meaningful data from the as received E110 because of the oxide instability. The oxide flaked off.

However, if we polished and machined it or at least polished it, we could delay the instability and basically the E110 data polished prior to instability behaves the same as these three alloys up here.

Things start to change when you go to 1,000 degrees C. Zirc-4 and ZIRLO are in very good agreement, as published previously by Westinghouse. They're very similar weight gain kinetics. As published by a variety of groups, M5 is significantly lower at this particular temperature. It picks up less oxygen during the same period of time. Whereas at 1,050 and 950 it's about the same, at 1,000 it's different.

Again, we could not get meaningful data for E110 unless we polished it, and basically M5 and E110 both behaved the same in terms of weight gain kinetics. The Zirc-1 niobium alloys at 1,000 degrees C. pick up less oxygen than the Zirc-10 alloys.

We're in the process of preparing tests at 1,200 and 1,260 degrees C., and during our

studies basically if you don't pick up hydrogen, we saw no effects of quench at 800 degrees C. on the weight gain. We also saw no effects on the post-quench ductility, but we'll hold that until the next slide.

All right. This is my compromise with the vendors. That's all the alloys that I just mentioned at 1,100 degrees C., and we're comparing the Cathcart-Pawel correlation to the measured weight gain. The alloy that falls off a little bit is the E110.

And in terms of the alloys we're interested in, they're all in excellent agreement at 1,100 degrees C., and most likely we'll get the same results as 1,200 degrees C.

It's 1,000 degrees C. where we start seeing alloy differences.

CHAIRMAN POWERS: Do I read it correctly that you have a consistent bias to underpredict the amount of weight gain in ZIRLO?

MR. BILLONE: I'm sorry?

CHAIRMAN POWERS: Do you consistently underpredict the weight gain in ZIRLO with Cathcart-Pawel?

MR. BILLONE: No.

CHAIRMAN POWERS: It seems like that's what you have with the plot.

MR. BILLONE: I'd have to dig for the ZIRLO plot. Let me show you. I have a table with results at about 20 percent ECR, predicted versus measured for two temperatures. Definitely not 1,000 degrees C. A thousand degrees C., Cathcart-Pawel predicts more than is measured for ZIRLO.

Actually our Zirc-4 should match Cathcart-Pawel because it was done with Zirc-4, and our Zirc-4 tends to be a little bit high, the measured values.

Okay. We have detailed results at five percent ECR, ten percent ECR, 15, 17, 20. I'm just going to show you 20. Basically you're not comparing the alloys. You don't notice there's a comparison, but at 1,100 degrees C. oxidation temperature and 20 percent calculated ECR, well, the Zirc-4 came out okay, and I just contradicted myself. The ZIRLO is a little bit higher, but not significantly higher. That's five percent, and the M5 is a little bit lower.

So at 1,100 degrees C. this is all about 20 percent measured ECR. These are the offset displacements converted to strains by dividing by

the diameters. They all indicate that you have some plasticity still left in these samples after 20 percent ECR.

We went ahead and measured the hydrogen pickups, and they are low, consistent with the fact that you have ductility. We'll look at the metallography to do the third confirming factor. At 1,100 degrees C. if all you're doing is picking up oxygen and no hydrogen, you're not going to embrittle within the ECR range that you're interested in.

Add these to your table because I had this in progress. This is, again, Friday night. With M5 you can see the clear decrease in weight gain compared to the other alloys for the same test time, but you don't see any increase in ductility, which is kind of interesting because the oxygen pickup is much less. There's hardly any hydrogen pickup for these two, and the ZIRLO for some reason picks up about 110 weight parts per million of hydrogen.

Having just gotten this Friday night, I do not have an explanation for why that alloy behaves differently. As I say, we'll have metallography on all of these for you to back them

up, but basically at these temperatures, these three alloys test out as being ductile in tests where you don't have ballooning and burst. These are just undeformed rings that you're oxidizing on both sides. This is basically consistent with what's published in the literature. They're good up to at least this ECR without hydrogen.

Okay. Let me try to do the summary of the E110 results very quickly as far as we could take it. Clearly, the alloy is more challenged at 1,000 degrees C. than 1,100 degrees C. and then at 1,200 degrees C. The farther away you get from that phase equilibrium temperature for the good oxide, the more chance for instability in the development of the white monoclinic oxide.

But there is a difference. At 1,100 degrees C. basically these white nodes stay pretty much separate, and they lead to a combination of oxygen and hydrogen embrittlement. That sample that I showed you had 200 ppm of hydrogen and it was brittle. At 1,000 degrees C., you have delamination and spallation of the oxide at least at very high hydrogen embrittlement, at fairly low weight gains or ECRs.

We ran a couple of tests at 950 for the

same times as 1,000, and the samples at least look worse than they did at 1,000. So, I mean, definitely there's a problem in that ramp-up and in the hold time.

We found that the surface roughness, the grooves in the material, welding TCs in the material, the ends, they're all initiation sites for oxide transitions and instability, and for one thing they definitely would disturb the compressive stress field that you need.

There's something else disturbing the chemistry that you need to keep it as ZrO two minus X. There's something dragging a little extra oxygen in there, pushing you towards that white oxide phase.

Okay. Surface polishing significantly improves the E110 behavior. Etching, especially with HF, degrades. As said here, "etching as received E110 significantly degrades the initial oxide due to the fluorine pickup."

This work is in progress, and all we can find is in looking at a tiny, tiny spot with TEM, it's an indication of nonuniform distribution of niobium particles in comparing E110 to M5.

So that's where we are with the E110.

That work is being continued by our Russian colleagues.

CHAIRMAN POWERS: Mike, if you're going to move to the LOCA integral tests now --

MR. BILLONE: Yeah. Do you want to break?

CHAIRMAN POWERS: Yeah, let's break for --

MR. BILLONE: Thank you.

CHAIRMAN POWERS: -- lunch until, say, 1:45.

MR. BILLONE: That would be wonderful.

CHAIRMAN POWERS: Okay. We're recessed until 1:45.

(Whereupon, at 1:01 p.m., the meeting was recessed for lunch, to reconvene at 1:45 p.m., the same day.)

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

(1:47 p.m.)

CHAIRMAN POWERS: Let's come back into session to continue hearing about the testing going on at the Argonne program from Mike Billone.

MR. BILLONE: Okay. While people are gathering, let me just summarize what I presented already on advanced alloy from one slide and then we'll move on to the LOCA high burn-up stuff.

As I talked about with our current oxidation quench study, and as we see cladding and basically for Zircaloy-4, ZIRLO, and M5, you're looking at oxygen induced embrittlement. These are short rings that we're oxidizing. They don't pick up any hydrogen with the exception of that last ZIRLO point, which is about 100 weight parts per million, and that's not enough to embrittle it.

All three alloys retain ductility at the two temperatures we've completed, up to 20 percent ECR calculated, and that's based on three things: the load flexion curve, the hydrogen pickup, and the metallography that we're making this statement.

For E110 it's hydrogen and oxygen induced embrittlement. What's in progress are the LOCA integral tests for ballooning and burst for

each of the alloys at each temperature, and that would be one test, one ECR each temperature, each alloy, followed by four point bend tests.

And we've talked about issues associated with hydrogen concentration. I think you'll see those in all of the unirradiated alloys.

So let's move on to our work with high burn-up Limerick fuel, and all of this from now on will be pertaining to Limerick Zirc-2.

I showed you our temperature history, and I'll show it to you again. Basically we stabilize at 300 degrees C. We pressurize. Pick your units by 8.3 megapascals.

This will only rise to about 8.6 during the test. It's almost a constant pressure test.

So as we ramp from five degrees C. per second, there's not a huge change in pressure through ballooning and burst at 1,204 degrees C. For our unirradiated materials we've held from one to ten minutes. Ten minutes is too aggressive. That's about 30 percent Cathcart-Pawel ECR, about 1.3 times that Baker-Just.

Cooled to 800 degrees C. at three degrees C. and quenched. We've done detailed profilometry, metallography, hydrogen and oxygen

determination. Our samples, and in progress are four point bend tests and ring compression tests.

So far results of post-quench ductility tests, these are demonstration tests that I did with my hands in front of various audiences just to learn something about it, and you all have a concept of brittle versus ductile, and I haven't done this in 20 years, but they don't make chalk the way they used to.

Chalk we know is basically brittle. It fails with no plastic flow, and it fails straight across based on maximum principal stress. This metal, on the other hand, is highly ductile. It will bend excessively. You probably can't even get it to break unless you fatigue it.

So we have a sense of ductile versus brittle. This happens to be a fluorescent tube, which is not quite glass, and we had to do it this way, but this is a four point bend test, and you could get shattering with the glass or you could get a clean break.

If you score it, if you put a little scratch on it, then you'll get a clean break across, and it's basically low fracture toughness material.

So what we're interested in is as a

structure, the four point bend test, does this material behave like the chalk in the glass or does it behave like this or somewhere in between, and we're going to find out it's a little more complicated than that because as pointed up earlier, we don't have a uniform degree of embrittlement.

Okay. I'm sorry you have a black and white copy of this, but let me try to -- okay.

If we compare our companion out of cell test, and this would be ramp-to-burst and then cooled in argon. So there's no oxidation of these tests. If we look at the change in diameter starting from the top going to the bottom of the specimen, basically we find for the unirradiated with zirconia pellets inside slightly higher average burst strain and a wider balloon, and you're following the blue and the green, and a much more concentrated balloon region, slightly less ballooning strain if you average these two numbers together. This is 30, so approximately 40 percent average strain for ballooning for that.

CHAIRMAN POWERS: Mike, if I did any one of the tests 500 times and plotted them up there, would there be any significant difference?

MR. BILLONE: What we find is there's a

little shift on where --

CHAIRMAN POWERS: Yeah, what I'm asking is is that little shift significant?

MR. BILLONE: Not in terms of the parameters we're looking at, which is what is the extent of the ballooning region, what is the maximum, and what does the cross-section look like.

Yeah, we would get slightly different results each time we insert a test strain and run the test.

DR. FORD: When you do this four point bend test as a measurement of the ductility, how does that relate to the actual strain or the straining mode that you will have in a post --

MR. BILLONE: Well, you do out of cell. You do the test in an Instron.

DR. FORD: I recognize that.

MR. BILLONE: Yeah.

DR. FORD: But what sort of -- are you going to have bending stresses on this structure, too?

MR. BILLONE: Yes. Let me get to that when I get to the -- I mean, I have a nice demonstration sample, but it failed during transport because it was too brittle in the high hydrogen

region, but --

(Laughter.)

MR. BILLONE: -- you won't get the theatrics of a live demonstration.

Let me go through what's similar between high burn-up fuel and unirradiated fuel, and some of the details of what the cross-sections look like for the two.

Basically for Limerick we found more similarities than differences, except in the burst shape hopefully, and then I'll get to the demonstration samples.

So you saw the diameter profiles, and this would be the fuel high burn-up sample. These two burst at about the same temperature during the ramp. This would be unirradiated Zirc-2 out of cell; irradiated high burn-up Zirc-2 in cell with fuel limit.

They burst at about the same temperature and about the same pressure. I just showed you the burst strains, which are a little bit different. The main difference is the shape of this opening. This is more of a dog bone shape, and this is more of an oval shape.

If I go to bend this sample with this

region under tension, the stress concentrations will be a little bit different than that. That is the one difference we found between the high burn-up and the unirradiated. We expected to find more than that.

That's based on nondestructive testing. I'll mention something about destructive, but it's not too hard to guess what's going to happen. If you take the unirradiated Zirc-2, just burst it and then cool down with no oxidation and look at the thickness variation as you go around, this is 180 degrees from burst. Obviously this region, as Ralph was saying, steam enters here. You're going to get essentially 100 percent oxidation here. It's going to drop off to maybe 13 percent here, and there's a nice algorithm explaining how you determine what this average thickness is and do you ECR calculation.

But what you're going to have is after oxidation I'll show you the picture. You're going to have a gradient this way in which you're going to have almost completely 100 percent brittle material here, transitioning to a locally ductile material there, and the question is: how does that behave in a structural test? And what does "some ductility"

mean?

Actually I'm not supposed to answer that question. I'm just supposed to respond.

If you go to the neck cross-section, obviously this is without oxidation, without hydrating, you obviously haven't -- your circular structure only is six percent reduction in wall thickness, and that's a fairly strong and ductile sample at this point in time. This is just at burst.

Later when we look at some of the pictures, we'll find out that we do get some bending during the ejection of gas from the rod, and clearly at zero percent ECR, you have ductility with these two pictures that I've shown.

All right. Okay. I showed you the profilometry with no oxidation, and now let's look at five minutes of oxidation, and this gets back to Dana's point. We're getting the ballooning and burst for the unoxidized sample. It's nice for looking because they don't overlap. This is the unoxidized sample. It has moved up a little bit towards the top in terms of where the ballooning and burst occurred.

You have to realize in response to

Dana's question, too, ballooning and burst is an instability phenomenon, and if you happen to have a uniform temperature, which we don't have, over about 100 millimeters, exactly where that ballooning and burst is going to initiate, once it is initiated, it takes off on you. It's an instability phenomenon that could occur anywhere within this region.

So our in cell test has about the same for strain; again, a little more narrow in terms of burst length, and we'll look at -- we'll do some cuts here and some cuts here and look at what the cross-sections look like because the question with the high burn-up fuel is do you have full double sided oxidation with the fuel in there. Do you have the hydrogen pick-up with the fuel in there.

Okay. By the time we took a photograph of this picture, we had lost most of the fuel from this section. If you look at a cross-section of the fuel before we start, the cracks are such that if you have an opening, .3 millimeters, it's large enough for fuel particles to come out of here.

And so we lost about less than a pellet initially, and then with further handling we lost more.

This strain's shape, which looked a

little better in the previous picture, we got a little more bending. I'm sorry. I don't have a -- out of plane bending this way. So this side went in and this came out a little bit, and you ended up with this kind of burst opening, but you're looking at the picture after five minutes of oxidation and steam, and it's clearly ductile at the time of burst, and the question is: is it ductile at this point?

DR. FORD: I thought someone said earlier on that you would not be using pellets.

MR. BILLONE: No, I'm sorry. This sample with fuel in it, the whole thing is like 300 millimeters, 12 inches. That will be subjected to a four point bend test with fuel in it.

DR. FORD: Yeah.

MR. BILLONE: Let's assume it breaks here or it breaks here. In the regions that are essentially circular, we would cut eight millimeter rings, defuel them, and then subject them to ring compression tests because they should be essentially brittle if the hydrogen is high.

So the idea is you subject them to ring compression tests. If you happen to get zero ductility, no ductility, then you measure the

hydrogen and you correlate the two. So the ring compression test would be with the fuel.

Okay. This is the picture I wanted to show that we've been alluding to. Even in the cutting of this in cell, you've lost the tips which were 100 percent oxidized. I've put this in terms of ECR. It's really oxygen pickup relative to the thickness, and this 36 percent goes to essentially 100 percent.

But although this region here -- and the only thing keeping you ductile -- I don't know if you can see it -- is this region from here to here. That's the prior beta layer. It's essentially missing from here. It's 100 percent brittle.

This region, based on our ring compression tests and our other program, this really should be ductile, locally ductile, and how this sample is going to behave depends on how you bend it. If you bend it with this under tension, you're going to rapidly initiate a crack, which is going to go across that cross-section, and you may miss whatever ductility you have.

If you do the reverse, something interesting would happen depending on whether pellets are left inside or not. Those are some of

the results I wanted to show you.

All right. That was unirradiated. For the irradiated, which is harder to get the metallography in cell, basically what you're looking at is a similar type cross-section. This is our in cell high burn-up test. Ignore this wide opening. It's just going to put pieces together, but essentially the oxide layer is dark. So you're not looking at that, but you're seeing essentially the same structure, very thin tips going around to thicker regions.

And we've looked at the detailed micrographs of the oxide layer. It is double sided oxidation all the way around here, the same as you would get in an unirradiated test. We don't expect this region to pick up any hydrogen. So we're not measuring hydrogen in that region.

So what is the influence of the fuel on the oxidation? It's zero. You've expanded about 40, 50 percent away from the fuel. Even if you had fuel particles in there, it doesn't protect you against the steam.

All right. Let's go back to the unirradiated graphs real quickly because I've gotten failures in both of these regions in bending. If

you move 22 millimeters, close to an inch, above the burst center, you're still in the balloon region. The ECR is 16 percent, but the hydrogen is 2,500 weight parts per million.

This really should be brittle, and you're still in the balloon region. You haven't hit the neck region yet.

It looks okay. I mean, you've got a nice, thick prior beta layer, but it's loaded with hydrogen. As a matter of fact, one of our bending test failures did occur there.

And as you get closer to the neck, when you get to the neck region, you essentially have one sided oxidation, very little oxidation on this side. I mean, ignore this. This is from the epoxy.

So your ECR drops way down low, but your hydrogen peaks to 3,500, and this is close to two inches above the burst center. So you have a gradation of thin, weak, oxidized cladding in the burst region, which may look brittle in the tests, and then as you move, you continue to have what may be brittle for unirradiated material.

What we're in the process of doing at this location for the irradiated tests, we're measuring the hydrogen here and in the previous

picture to find out if this secondary hydriding, which is all picked up from the inner surface, and the question is that oxide layer that you form, that fuel clouding bond that you form during or up to high burn-up irradiation, is it protective against hydrogen? It's certainly not protective against oxygen and steam oxidation.

Okay. We've seen this picture. So let's take this picture now and let's take several samples with this kind of picture. Let's expose it to four point bending which essentially at all of these locations you're exposing it to the same bending moment, and where it fails. We're interested in two things. Where does it fail? Here, here, here or in between? And how does it fail? What kind of failure mode do we have?

Let me do this with pictures because I don't want to take up too much of your time. All right. I was going to physically show this to you.

But this is the sample prior to the test. This is after five minutes of oxidation at 1,200 degrees C. You can see a slight bend to the sample that occurred during burst, and clearly the sample was ductile at that point in time. It has got permanent plastic deformation.

The idea of the four point bend test -- and this was the first one I performed in June, at our June meeting -- this is the burst region. I'm going to put that under tension, and this ductile region is under compression.

For this test I left the pellets in, and the pellets were supposed to be left in for the test I was going to do, but these are 2,500 millimeter long, 100 percent dense zirconium pellets. They're very, very, very stiff, and when you try to bend, they add to the stiffness of it.

Fortunately it didn't affect -- the thing failed before I got too far into the bending, and in this particular test it failed right at the center of the burst, and it failed with a snap.

And, again, I'm doing this by hand. It's not an Instron. I don't have a bending moment versus deflection curve, but it failed more like the chalk than like this. That's just a qualitative description.

And it also fails basically straight across. We're in the process of measuring. Even though this was a reject sample we weren't interested in, because the failure is interesting we're measuring the oxygen content right here to see

what the peak ECR is for this particular sample.

All right. At this point I thought I knew everything, and so a month later when we had our international meeting I figured I would just take this sample, a different sample, turn it 180 degrees C., and put the good side under tension and the bad side under compression and try to control it to get bending before a break.

That was being a little too cocky. So that's what I was trying to demonstrate. Essentially I've turned the sample upside down, and so this good side is under tension -- did I do this right? -- and this bad side is under compression.

I mean, it was an interesting test because I did it very slowly, and I did it with a lot of witnesses, and what I was foiled by is the sample Ralph showed you, and I'll pass it around. That's the one I just broke today.

As you can see what happened on the compressive side, again, I'm trying to bend the other side of this, and what happened is this burst area fragmented. Cracks started growing in all different directions, and the axial crack grew here and grew down here.

When the axial crack hit the high

hydrogen region, it snapped across the high hydrogen region. So depending on how you do the test, I mean, that determines the location of failure, and it's obviously much more complicated when you put this burst section under compression and get these cracks growing all over the place.

MR. CARUSO: And these are without the zirconium pellets inside?

MR. BILLONE: This test was without the zirconium pellets, and so I was intrigued by the results, but my pride was hurt. So I came back here on August 18th and left the pellets in and repeated the test because I was convinced I could get the good side to show ductility.

So if you leave the pellets in and just do the same test, the pellets stabilize this region. It's not a great picture, and I apologize. You do get cracking in the burst region, and the cracks go in all directions.

But on the ductile side which is under tension, I don't know if you can see it. This is a pellet that's wedged in there, and essentially you're bending with very high ductility the 180 degrees from burst part, which is at about 13 percent ECR, around that pellet, and it took a lot

of force to get this bending.

So, again, what does "some ductility" mean? This is consistent with the metallography in the sense that that back side has ductility, but in every test that I'm familiar with when you talk about fracture toughness or you talk about the ability of a material to withstand loads, you never perform a test this way. You always put the flawed region under tension and you look at how that crack grows.

And if it grows rapidly with very little plastic deformation in a structure sense, you call it brittle. Then there's mixed mode, which we're really in, and then there's ductile behavior where you get bend before break.

DR. FORD: I'm assuming that these are wasted samples. These are just --

MR. BILLONE: These were all reject samples.

DR. FORD: Yeah, got you.

MR. BILLONE: There's little oscillations in the temperature history. We didn't like them for the --

DR. FORD: But the controlled experiments will be presumably done at different

strain rates, different temperatures.

MR. BILLONE: Yeah, most definitely out of reactor.

DR. FORD: Yeah, with the fuel in.

MR. BILLONE: With the fuel in it, but I would choose to do the burst opening always under tension. That would be my choice.

DR. FORD: But is that necessarily --

MR. BILLONE: Well, if this thing bends, I mean, I'm not supposed to be relating this to an actual reactor event, but if --

DR. FORD: Well, why not?

MR. BILLONE: -- if you had a seismic event and you got an aftershock after the quench, you would induce some bending.

DR. FORD: Sure.

MR. BILLONE: So, I mean, it's not just going to bend one way. It's going to bend both ways. So I'm just trying to be consistent with all testing that I'm familiar with.

If you're going to take a flawed sample and test it for fracture toughness, which is not what we're doing, we won't get a fracture toughness out of this, and honestly, this was a nice impact sample with pellets in it. I was going to do some

kind of tricks with it, and it failed between the hotel and here. I don't know how it failed, but it failed in the high hydrogen region, and I have no idea of the loads inside the tube.

But basically, the idea is with the burst opening --

DR. FORD: All I'm questioning is you had some peculiar results using your samples which didn't go according to what your intuition told you. So, therefore, should you not be doing your controlled tests, not necessarily --

MR. BILLONE: That's the next slide. It's the next slide, but my intuition was bordering on hubris because I thought I knew the answers and that is not how you do research.

Okay. All right. We already know the observations. Skip that, skip that. I am winding down now.

Okay. I think it's two slides and we're done.

Comparing our out of cell results with our high burn-up results, we saw a lot of similarities. Pressurization rate, meaning permeability, when you pressurize from the top and you measure gas at the bottom, and depressurization

rates at bursts at least down to the three megapascals of pressure are all similar.

Maximum circumferential strain and burst region are more similar than different. Length and maximum opening of the burst were similar. Extent of double sided oxidation in burst region and maximum ECR appear to be similar.

Differences are the shape of the burst region which will affect the stress concentrations and response to bending tests, and of course, the axial extent of the burst region was much less for the high burn-up fuel than for the unirradiated.

And the second and extent of secondary hydriding we know is very, very high for these unirradiated. We're in the process of determining it for the irradiated.

Expectations as we move to the Robinson HBR cladding, again, all of this is work done with low hydrogen content, high burn-up Zirc-2.

As we move to the Zirc-4, the hydrogen content, we hope to take samples from the 400 weight parts per million regions and the 800 weight part per million regions. These contents will have an effect, a significant effect on ballooning and burst, as the JAERI results will show, because the

hydrogen does lower this transition from one phase to the other phase.

And we've been purposely bursting in the alpha phase to get the largest balloon we could produce, and essentially in order to do that, we're going to have to increase our pressure to get the same kind o results for hydrided Zirc-4. So that's one effect we know that we saw in the results of the JAERI test, is hydrogen will affect the phase transition temperature, which will, in turn, affect the ballooning size. Okay.

CHAIRMAN POWERS: Is the length of your balloon region and the size of the opening a function of the material or the furnace you're testing it no?

MR. BILLONE: We just completed -- we wanted to rebenchmark our in-cell apparatus. So we put a fresh two sample in cell in the same place that the high burn-up was, and we got the same result.

So for the first order I would say no, meaning that unirradiated material without fuel in it tends to give us a longer burst region and a different shape to the burst opening than the fuel high burn-up when tested in the same apparatus.

CHAIRMAN POWERS: I guess I'm confused then. Put your slide back up.

MR. BILLONE: Okay. I never showed you an apparatus. We have an apparatus out of cell and then right in cell we have a duplicate apparatus and we have common instrumentation in between.

CHAIRMAN POWERS: But here you're saying the similarities.

MR. BILLONE: Right.

CHAIRMAN POWERS: The length and the maximum opening of the burst, and what I'm asking you: is that a function of the materials or is that a function in the way you're testing it?

In other words, if I put a different furnace in there --

MR. BILLONE: Oh, I'm sorry.

CHAIRMAN POWERS: -- will I get a different length and a different maximum opening?

MR. BILLONE: The answer is yes and no. Yes, you would get different answers, but you'd still get the same -- I think you'd still get the same relative similarity between irradiated and unirradiated.

In other words, we're getting about a half inch burst length.

MR. ROSENTHAL: Why don't you put up the slide that has the burn-up, the high burn-up fuel burst above and the unirradiated below, you know?

MR. BILLONE: Oh.

MR. ROSENTHAL: One is taken through the window.

MR. BILLONE: Yeah.

MR. ROSENTHAL: You know, the yellow, and then if you could find that, then people could stare at that and decide whether the characteristics of those two bursts are similar or different.

CHAIRMAN POWERS: Well, that might be an interesting exercise, but it doesn't yield results that are very useful to me. The result that I'm interested in is you get this kind of a burst in your test.

MR. ROSENTHAL: Right.

CHAIRMAN POWERS: What I'd really like to know is what kind of a burst do I get in the reactor.

MR. BILLONE: Ah, okay. I tell you one thing that will be different is, since our relatively uniform heating zone is about 125 millimeters, about five inches, we're not going to get a balloon longer than that, and that's test

specific.

The strain that we get is pretty much -- will be different for different internal pressures that you start with, and we're shooting for something like 60 percent. We get something between 40 and 60, which varies from test.

That's really up to modelers or whatever you want to say to translate this data, these data -- sorry -- into reactor relevant conditions. We're looking for phenomena that are different between high burn-up fuel and regular fuel when tested under the same conditions, and that translation will be made separately by EPRI and by NRC to how relevant this is to reactors.

So we never intended to run tests that would directly be applied to a full length rod and a bundle. We're more humble than that.

MR. SCOTT: This is Harold Scott. Let me just mention just thinking about all of the tests that they did at Oak Ridge and in Germany and in other places with unirradiated and irradiated rods, the balloons were always relatively short except for the ones they did in England, and those had a particular reason why they did that, and these were bundled tests. They had long, heated zones.

So from a material property, as Mike said before, you're going to find one little place that goes first, and so it's almost impossible to get a long length balloon.

Now, maybe they'll have slightly different shapes. I think that fish mouth thing may look different in the same apparatus or from different apparatus, but in general, the total length of the balloon is always going to be short.

MS. YANG: Can I just add one more thing?

MR. BILLONE: Yeah, Rosa.

MS. YANG: I think in terms of uniform temperature this is probably more uniform here than in the reactor, so tend to promote the balloon size.

And another difference between this and the reactor is these tests are heated from the outside on the cladding. So, in fact, the cladding temperature is hotter than the fuel, while in the LOCA in the reactor the temperature of the cladding comes from the fuel. So if anything, this particular test is more conservative in terms of promoting the balloon because of the way the experiment is heated.

MR. BILLONE: Okay. I'm going to --

CHAIRMAN POWERS: I'm puzzled with that one a little bit. Why does this lead to a more --

MR. BILLONE: I'm not responding.

CHAIRMAN POWERS: -- a longer balloon than in the reactor? Because assuredly I have seen in reactor tests with balloons that were that long. So I'm going to have to think about that a little bit.

MR. BILLONE: All right. Let me just tell you where we're going, and then I'll sit down. I promise, I promise, I promise.

What I would like to do, what we can do easily out of cell in an Instron, which has just arrived this week, a new tabletop model just for this purpose, is as I mentioned before, we know at zero ECR we can see the specimen bend. We know it has got plastic deformation from a structural point of view.

All of the tests we've been conducting up till now have been at a 20 percent calculated ECR. It's very inexpensive to just march down. These are hold times, and so just from the ramp alone, you're at three percent ECR, and as you go up in time one minute, two minute, three minute, four minute, five minute, you will probably recapture

more and more ductility in that balloon inverse region because we know before we even oxidize and we're at zero ECR we're ductile. We think we know, but we're going to put it in an Instron to find out, that this essentially would look like a brittle material under bending.

And there will be an ECR, and again, these are calculated with Cathcart-Pawel models. So this is like the Baker-Just 17 percent, somewhere around two minute test.

CHAIRMAN POWERS: Now, what would I learn from this?

MR. BILLONE: What would you learn from this? You'd get a better feeling of what some ductility meant and what ECR it corresponded to. In other words, it would be completely ductile prior to the oxidation and may appear brittle here and may appear quite ductile here.

All I have is two extremes. I have what the shape of the LOCA test specimen is after burst, which has got some permanent bending in it, plastic bending, and I have hand demonstrations at this level which suggest that from a structural point of view it behaves in a brittle manner.

All right. These would all be done in

an Instron, and you would get a bending moment versus deflection curves, and you'd look and see whether you got --

CHAIRMAN POWERS: Well, I guess I can certainly see why it might be useful to do one at 20 and one at 16. It's the nine and the three that I don't understand at all.

MR. BILLONE: Well, we'd start here and work back. See, what the problem is -- okay. I'll tell you. Now I know what the nine and the three is. That hydrogen pickup occurs very early in the process. It's not correlated with absolute ECR. So as I make the balloon region stronger and more ductile, do I just simply shift the failure load to --

CHAIRMAN POWERS: Oh, okay. Now I understand.

MR. BILLONE: I forgot. I forgot why I did it. So you mentioned it. All right, but that's something you can do easily out of cell.

Let's end it with that. We're working very hard to do the in cell quench test as soon as possible. With the Limerick, we may do one more Limerick, a total of two quench tests, and then move on to the Robinson.

CHAIRMAN POWERS: How do your efforts in the quenching relate to the quench program in Germany?

MR. BILLONE: How do they relate? Someone remind me. Are these low burn-up fuels, old program?

CHAIRMAN POWERS: I think it's no burn-up fuel.

MR. BILLONE: It's got to be old.

MR. SCOTT: A severe accident, right? They take them up to 2,800 C. and watch how much hydrogen comes out, then quench them.

CHAIRMAN POWERS: Well, I think that in their international standard problem they were actually doing a quench for a DBA; that they do do tests. I know Quench 7 and Quench 9 are definitely severe accidents, but I think the international standard problem is intended to be a LOCA DBA.

MR. SCOTT: They did burn some at lower. That's true.

CHAIRMAN POWERS: Yeah. I believe that to be the case, but I'm asking you guys. I'm not supposed to answer that question.

MR. BILLONE: Harold has to answer that one for me. I'm not familiar with those tests.

CHAIRMAN POWERS: I mean, they're clearly out of pile tests, but the interesting feature of them, of course, is that they're bundles and not --

MR. BILLONE: right.

CHAIRMAN POWERS: -- and not single rods.

And so that leads me to the next question. What do you need to know about fuel bundle behavior that you're not going to learn from single rod tests?

MR. BILLONE: Just about everything. As Ralph mentioned, with a fuel bundle, you're going to have bursts at different locations unless they're going to be coplanar, and I guess some of the issues are -- and I'm making this up as I go along -- if you have any vibrations and you have these balloon regions, the whacking against the neighboring rod, or if the bending during a LOCA event is not perfectly in phase for every rod, you're going to have not only bending loads, but you're going to have some impact loads.

And I think -- well, plus, you don't have an infinite room to balloon burst, and you're going to hit the next rod. So you're --

CHAIRMAN POWERS: And does that do anything to you?

MR. BILLONE: Well, I don't think it's going to affect your core coolability, but I'm --

CHAIRMAN POWERS: Gee, I would think so. You're not going to cool the two parts to the touch.

MR. BILLONE: Well, no, but you'll have a lot of -- that's somebody else's area. That's my take, the core coolability versus --

CHAIRMAN POWERS: Could we --

MR. BILLONE: -- not an issue per bundle.

DR. MEYER: This is Ralph Meyer.

MR. BILLONE: Jack, can you help me out? Ralph?

DR. MEYER: Let me say that this really was a modest program. We did not set out to readdress questions that might not have been answered satisfactorily about single rod versus multi-rod or bundled tests. We set out only to look at burn-up effects, which I think we can do adequately with single rod tests.

Now, that may not answer multi-rod tests, questions about multi-rod behavior that you might have, but we really never attempted to do

that.

We did not at the outset have any multi-rod questions that we thought were burning, and so it's just not in the scope of things. Although this program is expensive in terms of current budgets, this is a very, very modest program compared to the amounts of money that were put in during the days of multi-rod burst tests, and I just don't think we can answer those, any of those questions.

CHAIRMAN POWERS: Well, it's a question that the Subcommittee has got to answer.

DR. MEYER: I'm sorry?

CHAIRMAN POWERS: It's a question the Subcommittee has to address.

DR. MEYER: Yeah.

CHAIRMAN POWERS: I mean, the question actually is pretty succinct. Are we getting anything out of these tests with just a single rod, or do we have to go to multi-rod tests, and the single rod tests are just interesting academic exercises?

I mean that's the question that the Subcommittee has to address.

DR. MEYER: Well, I think you have to ask the question in two parts. One is do you have

to go to multi-rod tests in order to see the effects of burn-up, and then the other part is do you have to go to multi-rod tests in order to answer questions that you never thought were adequately answered before.

MR. ROSENTHAL: The program addresses the former but not the latter.

MR. BILLONE: I think in terms of our focus, which is to address ductility, post-quench ductility of high burn-up, I think what we're doing is okay. There is a broader question that you're asking. It's not just academic to learn whether high burn-up fuel picks up 4,000 ppm of hydrogen or zero hydrogen on the other surface, and it's not academic to learn that it has permeability that allows gas to flow to that balloon region and sustain it and keep it going. These are unknown, totally unknown questions that are addressed by modeling prior.

So there's a lot about fuel and cladding behavior that we're able to learn that will teach us something about a single rod. Putting that together into a bundle is another world for me.

Does anyone have my sample that I passed around or did it get -- okay. Thanks.

All right. Shall we go on?

CHAIRMAN POWERS: Wait, wait.

MR. ROSENTHAL: Dana, let me just say, you know, in terms of my own thought process, I think that in overall LOCA activities we're going to be emphasizing small break LOCAs over large break LOCA, and we have to look at small break LOCA phenomenology as some sort of design basis, and that's not to say that once we define some break size we'll still look at bigger LOCAs, but we'll look at those through the lenses of severe accident.

So that when we do that exercise we'll stack up what we think we don't know without being -- because I think in the past we've been what I call large break LOCA-centric. So then when we restack for the future risk informed LOCA rules within that small break LOCA context with the severe accident stuff with the bigger breaks, I don't know where the multi-rod tests will come out against all of the other phenomenology that we'll be interested in.

But that would be the context that I would love to put it in.

CHAIRMAN POWERS: I understand what you're saying.

MR. OZER: Mr. Chairman, this is Odelli

Ozer.

May I read a passage from NUREG 1230 relating to the coolability issue where there are multiple rods or the coolability in reactor? May I?

CHAIRMAN POWERS: If you think I'll learn something from it. I have no idea what NUREG 1230 is.

MR. OZER: This says that research conducted since the ECCS hearings has in general yielded two important results. The first is that total blockage is nearly impossible to attain -- and this is based on a reference from BNL -- even if the 2,200 and 17 percent ECR criteria are closely approached or exceeded.

A second result is that even cases with large blockages remain coolable. In fact, a number of experimental cases in which the blockage actually enhances local cooling, this has been documented.

MR. LAUBEN: Excuse me. Dana, NUREG 1230 is a compendium of ECCS research that was published in about 1980 --

MR. OZER: 1988, yeah.

MR. LAUBEN: And I think that you're --

MR. CARUSO: Get a mic.

CHAIRMAN POWERS: You have to come to a

microphone.

MR. LAUBEN: You're talking about ECCS coolability in there. It's not necessarily talking about the phenomenology of clad ballooning and rupture, and most of the ballooning and rupture experiments that were done with cooling were done with fairly prescribed geometries for the ruptured and swollen region.

Not to say that they were wrong. Some of them were even flat plates in the early days, but others were more typical of ballooned regions. However, I don't know how those tests would have to do with the typicality of ballooned regions based on the -- you know, for those kind of tests.

MR. OZER: I thought the question was of interference between adjacent rods, when you have ballooning not just in one rod, but in multiple rods.

CHAIRMAN POWERS: The question was explicitly what is it that we need to know about real reactor behavior that we're not going to get from single rod tests.

The answer was nearly everything, which was a distressing answer, but perhaps an honest and true one, and I'm a bit at a loss because I know

the Committee has to address this.

The question had been posed a little differently to us. The question had been posed: are we going to learn so little from the single rod tests that there's no point in carrying them out?

I think that what we've learned today is enough to dispel that particular version of the question, but the modified version, is there more needs to be done, is still a little open to me.

DR. MEYER: This is Ralph Meyer.

Let me --

CHAIRMAN POWERS: Let me --

DR. MEYER: -- address your question before --

CHAIRMAN POWERS: Let me first of all --

DR. MEYER: Okay.

CHAIRMAN POWERS: -- tell you that Dr. Kress is going to take over chairing the session because in about 15 minutes I'm going to run up and talk to the boss man.

DR. MEYER: Okay. When the multi-rod tests were done earlier. Harold can help me out if I oversimplify this too much, but it seemed to me that there were really only two substantial conclusions from the multi-rod tests, and that was

that the burst sizes and appearances were about the same as you saw in the single rod test, and that the burst locations were not coplanar.

So there was not a lot of detail that came out of the multi-rod test in terms of what you need for a safety analysis. Now, if that's an oversimplification, then somebody will correct me, but as we moved into high burn-up effects, there was nothing that came to our mind about bundle effects that would be raised by high burn-up effects. It all looked like we could address the burn-up questions by looking at single rods.

CHAIRMAN POWERS: Well, about two years ago -- when did the French talk to us? About two years ago we had a presentation from --

DR. MEYER: Alan Myatt (phonetic).

CHAIRMAN POWERS: Myar (phonetic), who came in and showed us some interesting pictures and whatnot, and he argued fairly passionately that there was a bundle effect here.

Since the time I have seen some calculations on really basically dealing with heat transfer of single rods versus bundles which says, well, on heat transfer effects I just don't learn anything from single rod tests. So I really have to

go to bundles, and even multi-bundle to understand the heat transfer.

The question we're struggling with now is a modified question. Is there more we need to do to understand what goes on in the reactor accident?

DR. MEYER: Does Rosa want to comment on this? I don't have anything to say right now.

MS. YANG: I think the bundle one -- I forgot the name of the test -- I think you have summarized it quite well.

The only other thing I remember was these ballooned regions were all in the midspan. None of them are really close to the grids. So sort of confirming what you said earlier, the axial constrain effect is not big.

I think what Alan Myar (phonetic), at least the presentation I heard when he was promoting the Phebus program, was more on the fuel relocation. I haven't heard him make any really argument, even argument -- forget about convincing --

CHAIRMAN POWERS: Yes.

MS. YANG: -- to say there's any really bundle effect, except his test is a five-by-five array.

So I thought because of that he since

has changed his emphasis to more focus on source term in addition to LOCA.

DR. MEYER: Yeah, I had the same understanding, that Myatt's main concern was the axial relocation, which is going to be looked at as carefully as we can in the out of reactor tests at Argonne, and also specifically in the Halden test.

The Halden tests are designed almost exclusively for that purpose.

CHAIRMAN POWERS: Okay. You may go ahead, Mr. Chairman. Charge ahead, Ralph.

DR. KRESS: I'm already here. Go ahead.

DR. MEYER: Okay. So I thought I would tell you a little about what I know about the fuel damage at the Paks Nuclear Power Plant in Hungary. I'm not going to attempt to give you a detailed description of the chronology of events and things like that.

What I want to do is just to go quickly over what happened and then to inform you of an interest that NRC has in cooperation with CSNI in some possible cooperative effort to examine the damaged fuel that are in this cleaning tank.

So the background is that after chemically cleaning some steam generator tubes in

the Paks Unit 2, that they had a crud build-up on a lot of fuel elements, and they had hired Siemens from Germany, which is now part of the Framatome organization, to come in and clean the fuel in a special cleaning machine that they had.

So they had a big tank. They could put 30 fuel assemblies in this tank at one time. Now, these are VVER fuel assemblies. They're small, hexagonal array assemblies with a flow shroud around them, and they had used this successfully on five batches of fuel and were cleaning the sixth batch of fuel when, because of the unavailability of a crane one evening, they left the fuel in the tank overnight to be moved out of the tank the next morning.

Now, in this cleaning tank there were three circulation pumps. There was a large pump which they used during the cleaning operation, which had been completed. So they had put the oxalic acid in and removed the crud and taken samples, and they were satisfied that it was done, and they had flushed it, and they had turned off the main coolant pump and left running a smaller pump.

There was also a back-up smaller pump in case of some failure, but there was no failure in

the pump. The small cooling pump which they thought would be adequate to keep it cool overnight was left running.

It was not adequate. There was a problem with the circulation, and so there was overheating. They believe there was as steam bubble that formed in the top of this tank, and there was some release of fission products. Noble gas activity was detected several places in the plant.

This is a picture of the cleaning tank. I'm really not going to do much with this picture, but it's fairly large. Here you see one of the 30 assemblies. There's this upper grid structure, and a lower grid structure. There are, in fact, some bypass flow holes in the shroud which may have figured into the inadequacy of the cooling. There was also the possibility of some misalignment of the nozzles in the lower plate.

The details of this are unimportant from our point of interest here now, and so I just show you this. This tank is submerged in an area between the reactor and the storage pool, and it has interfered with further operation of the plant. So the plant is shut down at this time.

So all 30 fuel assemblies are badly

damaged. We've seen some pictures. If you visit over there they'll show you some pictures, but they won't give you anything to take away. So I don't have any pictures that I can show you. I'll try and describe some of the damage a little bit.

From the activity releases we were able to just make an estimate. Well, we were told that roughly 20 percent of the gap activity was released. This is based on detector measurements, and from that estimate it seems to us that some of the fuel got kind of warm, but it didn't really get hot. If you had gotten above 2,000 Centigrade, you'd probably start seeing more than gap activity, and they didn't see anything more than gap activity.

So this was our inference about the possible temperature limits, which, in fact, are consistent with calculations that have been done in Hungary and in Germany on this.

Now, I've seen pictures of some of this. The shrouds, many of them are broken just below that upper grid area. It's a strange looking geometry that's left. Many of the fuel assemblies are intact. Many of them have the top broken and are just laying askew.

There are pieces of the channel box, of

the shroud wall that maybe are ten or 12 inches long and several inches wide that are just missing.

You can look inside of these open places in the shroud and see fuel rods. So there are a lot of fuel rods that are left intact in the bundle array.

And now I wish I didn't have to tell you this, but we see long sausage balloons in some places.

CHAIRMAN POWERS: Why do you not want to tell me that?

DR. MEYER: Because we just told you that all of these balloons were short.

CHAIRMAN POWERS: But I didn't believe you when you said that anyway. So I mean, we know we can get long sausage balloons. We've done it before. Coming in and telling me that you --

DR. MEYER: Well, Ed Hindle did it in a big muffle furnace where he had creamy smooth, uniform temperatures, and we never saw that kind of behavior with internally heated test runs.

The thing here is that you've now been shut down for a period of weeks. The heat generation rate is extremely low, and within this shroud there are obviously some areas of very

uniform heating which is not the result of any significant heat flow from the fuel where local variations in gap can --

CHAIRMAN POWERS: If you're trying to persuade me that we'll never see long sausage balloons in reactor accidents, give up now while you're ahead.

DR. MEYER: Well, there's one other --

(Laughter.)

DR. MEYER: Well, I haven't told you the other thing, which is that the sausage balloons, insofar as I can remember seeing them, were relatively small in diameter and so far none of the long balloons were seen to be ruptured. They did see a number of balloons that were ruptured, and they were all short.

So we don't understand all of this, but the fact that there are ballooned rods which have not been "rubbleized" still inside of these flow shrouds I think makes this much more interesting for pathological examination than if it had just been a rubble pile.

CHAIRMAN POWERS: I mean that's all a very fair statement, but where I run into trouble is saying X or Y can never happen. Simply because

you've never seen it in an experiment you've done with one foot sections, that's where I have real trouble.

MS. YANG: Ralph, did they estimate how long they were left at high temperature time-wise? Was it overnight?

DR. MEYER: Well, yeah, it was overnight that it was left like that.

PARTICIPANT: Didn't they stop the main pump in the afternoon and then they noticed something in the evening, something like that?

DR. MEYER: Yes. It was fairly late in the evening.

PARTICIPANT: It was like nine o'clock, and they noticed something at like 11.

DR. MEYER: And then about an hour later they started noticing some pressure increase and then some activity.

I don't -- I didn't prepare to give you a chronology of this, but I can tell you that Ann MacLachlan wrote a real nice summary of this in the May 8th Nucleonics Week. So if you want a good summary of the overall event, that's one of the best places to look for it.

Now, what we did was to discuss the

possibility of some cooperative effort to examine the fuel, and there was a meeting to discuss this in Budapest just a couple of weeks ago, and the participants there were from NRC. There were two of us from NRC, one guy from IRSN in France, two guys from GRS in Germany. So this was not Siemens. This was another part of the German population, the GRS Institute.

Of course, in Hungary you had the Atomic Energy Authority, the personnel from the power plant and also the research institute, KFKI.

The Russian team was interesting. Just two days before the meeting, the Russians had been awarded the recovery contract, and the contract went to TVEL. They call it TVEL. It's T-V-E-L, and so TVEL was there, and they had a team for this recovery effort, and the team included the Bochvar Institute, which is sort of -- TVEL is the manufacturer. Bochvar is sort of the design institute. Kurchatov, which is an independent institute, and then I can't remember whether the other fellow was from Dmitrovgrad or not, but Dmitrovgrad, the reactor. Russian Institute of Atomic Reactors was the fourth partner in this consortium of Russian institutes and companies, and

that's where the hot cells are.

DR. KRESS: Ralph.

DR. MEYER: Yeah.

DR. KRESS: Do you have any idea of what burn-up this fuel had been taken to?

DR. MEYER: I just don't recall. I'm sure we can find out, but I don't recall. I don't recall.

MS. YANG: Probably not very high. They're cleaning it and then putting it back in.

DR. KRESS: Yeah, that's what I would have thought.

DR. MEYER: Right. So it wasn't fresh, and it wasn't ready to be discharged. In between.

So anyway, we discussed this possibility of cooperative effort, and there was sort of agreement in principle to continue considering this possibility. There were no major decisions made at the meeting.

There were, of course, two organizations there that had concerns about this. One was the Paks Power Plant people because they don't want anything done that might slow down the recovery of the plant, and then TVEL, the Russian organization, didn't want anything that might increase their costs

or slow up their part of the recovery effort.

But notwithstanding, the value of doing this seemed to be pretty widely recognized. There was interest coming from CSNI. Mr. Thadani is the current chairman of CSNI, and so it's kind of an NRC-CSNI interest. Carlo Vitanza, the staff person from NEA, was there, and he now has the assignment of preparing a written proposal which will, I believe, be first reviewed by CSNI and then presented to the Hungarians for consideration.

Now, all of this has to happen reasonably fast because the recovery contract calls for completion of that in six months. So the Russians are going to move in and move fairly fast to get this tank defueled and moved out of the way because it's blocking traffic right now.

DR. KRESS: Since this is a Russian firm, would these -- I presume these tests have to be done in a hot cell.

DR. MEYER: Well, now --

DR. KRESS: Would they be done in Russia?

DR. MEYER: Now, that's interesting, and I shouldn't speculate too much on this, but you see, from my point of view and the fuels research program

at NRC, we have a group in Russia who are working in this very area, and they are Kurchatov and RIAR, but they're not exactly the same people.

The Kurchatov people that were part of the TVEL team were not the nuclear safety institute that we deal with.

DR. KRESS: I see.

DR. MEYER: But they're in the same big institute. So I don't know how this is going to play out. We have our Russian colleagues who we've been working with on oxidation studies who are knowledgeable in this area and placed in the right organizations.

And then you have TVEL with the recovery contract who will want things to run smoothly, and I don't know how the pieces will fit together, but I just thought it might be of interest for you to know that there was this effort going on to try and secure -- probably we would like to get one complete fuel assembly. Maybe the top is broken off of it, but this would give us some highly damaged fuel, some not so damaged fuel, and some intact balloons to look at.

DR. KRESS: And what would you look for?

DR. MEYER: Well --

DR. KRESS: The degree of oxygenation?

DR. MEYER: There are several obvious things to look for. The first one to look for, in my opinion, is why did the side of the flow shrouds fall out, just pieces, just big chunks, you know, football size, cross-section areas missing. And it is likely to be from severe hydriding because this is a closed, bottled up system which had oxidized a lot of zirconium, and so you built up a high partial pressure of hydrogen, which also has gone into the zirconium somewhere.

And so I think the first thing of interest is going to be to look at hydrogen absorption and effects on the materials.

I also think examining these balloon sections will be of value, particularly if what we thought were long, extended balloons are truly long, extended balloons. It will be interest to look at those and see what we can understand from that.

I guess going into this our expectations are modest. There's no burning question that we have that we think would be answered by this, but it's certainly an intriguing event. It involves the kind of phenomena that we're studying actively for LOCA behavior and also for spent fuel behavior, and

it would just seem to be a shame not to go in and have a look at an event that has preserved some very interesting looking specimens.

DR. KRESS: As long as you can get a bunch of people to cooperate and it doesn't cost you too much, it might be well worth it.

MS. YANG: May I ask what are the materials for the cladding and for the shroud?

DR. MEYER: It's E110.

MS. YANG: E110? Okay.

DR. MEYER: It's the standard VVER cladding. Of course, there are varieties of VVER cladding. I mean of E110. There are varieties of E110, oxidized, annodized.

DR. KRESS: Does that make it less attractive to you?

DR. MEYER: No, not really, because you know, E110 is zirconium one percent niobium made by a different company, and it has some very different behavior characteristics, and we're still interested in figuring out what is causing this.

I'm sure a lot of people are interested besides us. So it's a very intriguing possibility.

DR. KRESS: I guess whenever you get this proposal in late October we might get a look at

it?

DR. MEYER: I can't say how this is going to go. The negotiations are somewhat delicate because the whole situation is in litigation over the responsibility for this, and we have to make sure that we don't interfere with normal processes of plant recovery and whatever financial recovery is involved.

We have to just stay clear of that, and for that reason, some of these things may be done diplomatically and a little discretely. I simply don't know.

DR. KRESS: It's not one of the things that this Committee normally looks at anyway when you get into these cooperative programs.

DR. MEYER: We're simply asking the Hungarians to let us have an opportunity to look, and we have to be patient and polite about it.

DR. KRESS: Sure. Okay.

DR. MEYER: I'm finished.

DR. KRESS: I guess we're at the dry cask storage conditions. We'll hear from Mr. Billone again.

We're also scheduled to take a break at this time. Do you guys feel like this would be a

good time to take a 15 minute break?

MR. BILLONE: It's a good time to take a break before you let me start talking.

DR. KRESS: Yeah, let's do that. Okay. I'm going to declare a break for 15 minutes, and be back at 3:15.

(Whereupon, the foregoing matter went off the record at 3:01 p.m. and went back on the record at 3:18 p.m.)

DR. KRESS: Could we please come to order and resume the meeting?

MR. BILLONE: All right. We're going to switch subjects to dry cask storage, and you'd better let me get started so that you can get to supper tonight.

There's two aspects of our program. One is dry cask storage license renewal, and let's call it low burn-up fuel less than 45 gigawatt days per metric ton by this world. Our work has been documented in a NUREG report, CR-6831, which is coming out the end of this month. We are at the end of this month so it should be out now.

That's work with Surry PWR fuel rods at 36 gigawatt days per metric ton. We're fortunate to have those. They were dry cask storage for 15 years

in a helium environment.

We took 12 of those rods out of one of the subassemblies, and we did profilometry to look at any interesting possible changes in cladding diameter due to creep while they were in storage. We saw none. All 12 rods looked pretty much identical, and they looked pretty much like they would look as they would come out of a reactor going into the wet pool.

We did fission gas analysis on four of the rods. This was done at Argonne West. Fission gas release is half to one percent, which is typical of this kind of rod at this burn-up, and three of the rods were sent to Argonne East -- that's us -- for destructive examinations. I'll show you some results on those.

We did thermal creep studies from 360 to 400 degrees C. to see what kind of residual creep life was left in these samples.

The purpose of this is twofold. One, this work was sponsored by EPRI, NRC and DOE-RW. So one purpose was if these rods had gone in at higher fission gas pressure, would they have had residual creep lag to make it the first 20 years.

DR. KRESS: Are those typical

temperatures in a dry cask?

MR. BILLONE: Typical temperatures now, everything is under 400 degrees C. for the whole process. That's the recommendation of ISG-11, Rev. 2.

So this would be typical of starting temperatures, and we picked those temperatures because we're in a laboratory framework with a limited amount of time. We can't run 15-year tests. So this would be typical of the upper bound temperature.

The second purpose of doing this was for DOE-RW because at the end of storage, these assemblies will be reconstituted -- not reconstituted -- reconsolidated and put in a repository site with an elevated temperature.

So at the end of 15 years for Surry, it would have started at something like 350 degrees, 355 degrees C., ended at something like 150 to 200 degrees C. So that temperature would go up for a while in the repository and come down again.

We also have axial tensile tests in progress, room temperature to 400 degrees C. We got interested in radial or reorientation and axial redistribution of hydrides and what those effects

might be, and we've also proposed and have samples for post-storage post-creep, bending tests, and there's been a request for some kind of impact test to represent possible transportation loads, particularly after the storage when you're going from the dry cask storage to the repository.

The second part of our program is high burn-up spent nuclear fuel behavior issues, and for that we're using the Robinson rods. Several of the rods were selected for this part of the study.

In progress is fuel actinide and fission product concentration measurements and burn-up analysis. This is for our code people and for burn-up credits, which I'm not an expert on. So I won't elaborate.

DR. KRESS: Is this for behavior in spent fuel pools or in dry cask?

MR. BILLONE: No, no.

DR. KRESS: This is dry cask?

MR. BILLONE: This would be in dry cask.

DR. KRESS: Okay.

MR. BILLONE: However, the DOE-RW is also interested in this kind of analysis. You have to do a criticality analysis and see how tightly you can pack everything.

All right. We chose essentially the same matrix of 360 to 400 degrees C. and tensile test, room temperature to 400. These rods had gone through not the traditional process, but they had seen temperatures as high as 415 degrees C. during vacuum annealing or during vacuum really, being in a vacuum environment.

These rods came to us out of the wet pool. So they haven't seen that kind of treatment. So in addition to thermal creep, we're interested in looking at annealing and reorientation, redistribution of hydrides, particularly during the vacuum drying process, and effects of these things, annealing and hydride orientation, on mechanical properties. And by "mechanical properties" I'm including creep in that.

So, again, same picture. We need to do something post-storage, post-creep. We're proposing bend tests. Our creep samples would be ideal for three point bend tests.

There's no universal agreement on what is the best test to do or series of tests to do following storage such that you can safely handle these things. They're not going to shatter on you, and you can transport them.

And so, again, some kind of impact tests. There's a couple that we could do. The question is: what do you do with the data?

We can generate some data, Charpy impact type data or even pendulum data. Again what do you do with the data?

I don't think that's been completely resolved, but it's in our test plan to do something.

Let's go back to the earlier slide I showed you; only now let's just focus on those rods which we're going to use in this program, and a lot of the data we're generating here is also going to apply to the mechanical properties data we need for RIA. As you'll see, we're basically going to be using two strain rates, one moderate and one fast, and those data will be useful to both programs.

So the Surry rods we'll talk about first, and then we'll talk about the Robinson rods. We do have the TMI-1 rods, thanks to EPRI and Rosa, that we use to benchmark the mechanical properties. But if you look at the next slide, you'll see that we have an interesting range of hydrogen contents and fast fluences, and those are the things that you correlate models to.

So we're actually very fortunate. If

you ignore the storage at Surry, which appears to be benign, you've got fast fluences in these units from seven to nine to 14. So it's a factor of two and a potential hardening mechanism and embrittling mechanism due to neutron damage.

And significantly, forget the oxide content. That's not something we correlate to. We correlate to what's inside the metal, and what's inside the metal is for Surry less than 300 weight parts per million, up to 300 weight parts per million of hydrogen. The TMI is a little bit lower, and then up to at least 800 weight parts per million hydrogen in the Robinson.

So we expect differences in mechanical properties and even creep properties and ductilities between those two. So it's actually a nice matrix of materials to work with.

Let's start with Surry, and then everything that we study, the metallography, the hydride orientation that I'll show you, everything seemed relatively benign.

There's only one mildly interesting thing, and the question is with this long rod and the axial temperature profile over 15 years, does hydrogen move from the hot inner regions or midpoint

region of the rod to the colder ends.

So there's a temperature profile and storage, and so we had commissioned to do three hydrogen measurements, a midplane, a half a meter above and one meter above, and everything was going fine and the oxide increased the way it was supposed to. The hydrogen increased the way it was supposed to until we got to the last reading, and it decreased.

This location happens to be just where you start the down slope in temperature. So what we have in progress is going higher to one and a half meters and then the plenum region, and the only issue here is do you get hydrogen accumulation at the colder ends that would tend to embrittle the colder ends.

What's nice about having the Surry rods is DOE-RW happened to have a lot of money this year for sabotage considerations and dry cask, and so they want a little bit of the midplane of this third Surry rod, and they will pay a lot of money for characterization. So we'll get oxide thickness, a couple of hydrogen readings and isotopics at two locations, actinides, and fission products.

And TBM means to be measured. That

means the money is not there and is not paid for by NRC, but NRC and EPI get the data from that. And they only want a little bit to make rodlets and Sandia is going to shoot shaped charges through them and wee what kind of aerosols come off.

I'm not involved in that part of the program.

DR. KRESS: I was wondering what you were going to do with that.

MR. ROSENTHAL: Wait, wait, wait. I think if we start saying more we're going to have to go into closed session.

DR. KRESS: Yeah, okay. We'll leave it at that.

MR. BILLONE: Sorry.

DR. KRESS: That's okay.

MR. BILLONE: My only point is there's more characterization data that will be made available.

Okay. I want you to get a good mental image of the hydride distribution and the Surry cladding. This is the OD oxide you're looking at. It's basically circumferential, and at this hydrogen concentration, almost all of this hydrogen would have been in solution at 415 degrees C. during the

early period of time where this was in vacuum.

However, the stresses were low. They were no more than 50 megapascals hoop stress, and under those conditions when you start cooling you don't get the hydrogen reoriented in a radial direction. So essentially it reprecipitated where it was, maybe with a little bit of extra precipitation here.

So at some point early in the history of dry cask storage prior to the actual storage time when they were doing thermal benchmark tests, most of this hydrogen was in solution. It precipitated out in a benign fashion.

And let's keep this aside because I want to come back to that because a couple of our creep tests we shut down under very high pressure and stress and got quite a different picture than that.

Okay. So we ran a series of creep tests on the Surry cladding, all basically in the range of 250 to 300 weight parts per million hydrogen. Temperatures ranged from 360 to 400, and characteristically our stresses are 160, 190, 220.

In this particular test we got as high as six percent creep strain, hoop creep strain without any failure, and we're saving this sample

for a bend test, and the idea is you take the surry cladding prior to running it through creep, and then you take the Surry cladding after this, and how much damage was accumulated? Is damage due to creep additive to sort of plastic flow or the kind of damage you get from a tensile test or a bend test?

Also C8 got to one percent creep strain, and we're saving that for a bend test. These are the two that were very low strains, and it wasn't much advantage based on the creep rates of keeping them going.

We shut those down under pressure, under stress and looked at the hydride distribution for those particular samples to see if we got reorientation.

DR. FORD: Presumably measuring the strain in real time is not just a grab sample, is it? You are measuring.

MR. BILLONE: The strain is measured periodically by shutting down, depressurizing first, and then cooling to room temperature and measuring the strain. It wasn't measured on line.

DR. FORD: Okay.

MR. BILLONE: Oh, I'm sorry, and I'll show you the histories. These are just the end of

life values.

All right. With Surry creep tests everything behaved the way it was supposed to behave. I mean, you're supposed to have stress dependency. It's nonlinear. You're supposed to have temperature dependency which is nonlinear.

So if you look at a fixed hoop stress and two different temperatures, you see 20 degrees C. temperature difference makes quite a bit of difference in the creep rate, at least a factor of five in the creep rate, and I'll summarize that at the end.

So that's --

DR. KRESS: Now, is this a (pause) --

MR. BILLONE: These are three inch long pressurized tubes.

DR. KRESS: These are the test data you got.

MR. BILLONE: This is test data. So we've taken Surry, which has already gone through 15 years of storage --

DR. KRESS: Yeah.

MR. BILLONE: -- and we're asking ourselves how much residual creep does it have.

DR. KRESS: Yeah, okay.

MR. BILLONE: And we're trying to add to the general overall database for irradiated hydrided materials for creep rates. It's something that's useful for the modelers.

So 20 degrees C. Particularly, these 400 degrees C. temperatures become interesting. As I go on in my presentation, it's becoming more and more interesting because that's set as of August 2002. That was the recommended upper limit for beginning of dry cask storage and all of the other processes, and that's part of the reasons why we're concentrating initially on that.

Again, temperature dependency at a higher stress level, 380 degrees C. down to 360. That 20 degrees makes a huge difference in creep rate.

I don't know if you saw these last year. Some of them were available. So I'm going to go through them quickly until I get to the Robinson, which that's a stress effect of 30 megapascals. Interesting, but let me get on.

Okay. Four hundred degrees C. The red curve is new data, and the test, I'll explain why it was terminated at this point in time. It didn't fail, but at this point in time we do not see as

strong a stress dependency as we expected at the 400 degrees C. level. We'll get into that a little bit later.

Okay. Let's try to go through the Surry stuff fast because most of it is in the NUREG report, except for some data.

We were able to determine secondary creep rates or stead state creep rates, and we've got a range of more than a factor of 100 in creep rates by varying these temperatures. The 400 degrees sample at 190 megapascals after it accumulated one percent strain, we jacked up the stress to 250 megapascals, and that's what took us -- we were creeping too slowly, and we wanted to get up to higher strains. So this took us up to about six percent strain and about five times ten to the minus third.

All right. Two of the tests we shut down, and again, let me show you this. This is what you start with before you run the creep test. This is what happens when you shut down under fairly high stress, and this is what should happen because the critical stress for hydride reorientation, we think, is lower than this, but basically the hydrides -- and you don't see all of the hydrides when you etch,

but basically you've got reorientation in the radial direction, some in that direction, and you broke up the concentration of hydrogen at the boundaries.

And one question that we would like to address is what effect does this have on the mechanical properties. Do these effectively act like locations for cracks to easily grow through the radius of the material?

How detrimental is hydride reorientation is one questions, and, two, under what stress and cooling conditions does it occur?

Those two samples I just showed you were in the process of remeasuring the hydrogen to make sure it didn't actually move out of our sample.

That's Surry. Let's move on to the high burn-up Robinson, and again, TBM means to be measured. I've got to be careful here, but basically most of our work is with two of these rods, and that's the fuel and cladding metallography, OD oxide thickness measurements, hydrogen isotopics and burn-up analysis, again, to be measured.

The same with BO-1. This is a gamma linear rod, and the interest in giving it to us was to do the isotopic and burn-up analysis of the gamma

linear rod. Again, one extra rod will get data at the midplane from this unnamed source of funding.

All right. Let's look at the gamma scan for one of the rods and where we've done most of our destructive examination and where creep samples come from. This happens to be rod A02. These are grid spacer locations, and this is the expected profile. These dips are not real. These come to us in approximately a little less than one meter segments, and so what you see here is just the end of the segment, and we're piecing these curves together. So ignore these particular dips.

At these locations, roughly the core midplane and roughly .7 meters above the core midplane, that's where our metallography, hydrogen samples, and our burn-up and isotopic samples were taken from these locations. So you had a complete picture.

When we get back to this we'll take samples from down here in the lower hydrogen region for the same kind of analysis. So most of our creep samples that I'll show you results from came from these locations.

Okay. There's a lot more hydrogen in the Robinson cladding, and the question is how does

it affect the mechanical properties, how does it affect the creep behavior, but this would be roughly 650 weight part per million hydrogen in this particular location.

At the midplane we got roughly 600, 580 at the midplane, and 750 about .7 meters above the midplane. This is not the maximum. If you keep going up, you would measure more hydrogen than this, but our samples are taken from this regime, and oxide thicknesses is from 70 at the midplane to about 100 at .7. This might go up another ten to 20 microns as you go up the rod.

And the hydrides, again, are all circumferentially oriented.

Let's save that picture because I want to come back to it.

This is more of an RIA issue, but just for those who want to know what the fuel looks like, if you put this in a dry cask, basically this is the fuel rim which is porous and very fine grained, and this is an interaction layer of fission products between the fuel and the cladding. It doesn't really eat away at the cladding or deteriorate the cladding, but it does exist, and it would have a bearing on the response for an RIA, and again, it

would have some bearing on the LOCA response depending -- I mean, some of this is oxide, and the question is is any of this protected when hydrogen gets inside and tries to get into the cladding.

Okay. We started some creep tests at 400 degrees C. Actually I'll be reporting four of the creep test results, two at 400 degrees C. and two at 380 in two different stress levels.

DR. KRESS: How do you do these creep tests? Do you pressurize the inside or do you pull them in tension or --

MR. BILLONE: No, we pressurize. We have one open end connected to a -- well, it's bound.

DR. KRESS: Yeah.

MR. BILLONE: So we actively control the pressure.

DR. KRESS: So it's creep in the radial.

MR. BILLONE: It's basically creep in the radial, almost no axial contraction. So it's all hoop creep strain.

One advantage of our system is we can change the stress and pressure at any time during the test.

DR. KRESS: It would be easy.

MR. BILLONE: All right. So far that's very valuable information written down. I can see my signature over here, and there's a cost code number. So hopefully that didn't come across in your slide.

At 400 degrees C. and at 380 degrees C., the same stress level, we got expected behavior, meaning that 20 degrees C. difference in temperature made a significant difference in creep. I'll explain why this starts curving up on us soon.

So that result was expected. When we compare the higher hydrogen and higher fast fluence, higher neutron damage, Robinson to Surry, at 380 and 220 megapascals, we got the expected result, that both hydrogen and additional radiation hardens the material more. Everything was fine at this point.

And then we went to 400. Funny things started happening at 400 degrees C. This is the Surry sample at 190 megapascals and 400 degrees C., and the Robinson sort of starts like the Surry, and then it takes off on us, almost as if it's going through some annealing during the test time at 400, whereas the Surry did not appear to do that.

These are two different samples. C-14, we were trying to see how far in strain we could go,

and C-15, we were trying to get good secondary creep.

We stopped C-14 after we got an average strain of 3.6 percent, and if you move 15 millimeters above the average point, we've got as much as five percent peak strain.

So Robinson, like Surry, even though it's higher hydrided, seems to have the same creep capacity. What's not clear is why this takes off in our tests and also in some of the French tests at this particular temperature and stress level.

Just to give you some idea of the temperature sensitivity which is not explained by any of the models which have creep as an erroneous function of temperature, if we take the one sample and just look at three different locations separated by 15 millimeters apart, we have a very small axial temperature gradient. This would be towards the bottom of the furnace, about 402 degrees C. This is 401 degrees C., and these are the differences in local creep rates observed at different locations of the sample corresponding to different temperatures, and this kind of temperature sensitivity, as I say, cannot be explained by any of the existing creep models. It's much, much, much too high and much

higher than we expected. So we'll call this an interesting result.

DR. FORD: Surely if those areas where you're measuring those temperature are fairly close, you're going to have constraint from the adjacent --

MR. BILLONE: Then there will be constraint, but the constraint means that these differences would be even larger. In other words, this material here is partially constraining that material.

DR. FORD: Yes. Okay.

MR. BILLONE: All right. I'm showing you C-15 because we got very cavalier with this sample and things were going extremely well here, and we got to this point in time, and we happened to shut it down under pressure to study hydride reorientation, totally convinced that it would be benign to shut it down under pressure.

You can't get too cavalier when you're doing research.

MR. CARUSO: I'm just curious. You've drawn all sorts of nice curves that look like maybe swine between these points. Why haven't you done any sort of least squares fit? Why have you drawn the curve?

MR. BILLONE: Why do we connect the points?

MR. CARUSO: Well, you don't have --

MR. BILLONE: Only for your eye.

MR. CARUSO: Well, I mean, I look at them and they're not straight lines between the points. They're curves.

MR. BILLONE: They're not straight lines because the material seems to be annealing or going into tertiary creep. What we were looking for was straight lines to determine secondary creep. We never got in that regime. We went from primary creep to a transition, to like a tertiary creep.

There's no advantage to doing least squares fit of this because all I'm trying to do here is show you temperature sensitivity of one single sample. So you're not talking about sample to --

MR. CARUSO: Is there an error associated with the hoop strain that was measured?

MR. BILLONE: The error is very slight. What we do is we measure diameters at 16 locations around one axial location, and then we measure a number of different axial locations.

MR. CARUSO: So there's no error bar

associated with each of these points?

MR. BILLONE: The error bar is too small to see if we're talking about precision in terms of one sigma deviation from the average. So if I take one location and I measure 16 different diameters to get this point over here, there's very little variation. It's small.

What's much larger than the error bars is this temperature sensitivity. That may not be the best answer in the world because I don't think I'm addressing your question.

Error bars, I would rather -- if we repeated this test ten times and --

MR. CARUSO: If.

MR. BILLONE: I said if we did.

MR. CARUSO: Yes.

MR. BILLONE: Then I would show you what you want to see, which is the error bars. The measurement error is very small, but to do what would be useful is to run a number of different tests and then show the spread and results as a one sigma variation.

One single test, one single location, you're not going to see it.

So the purpose of that is to show you

temperature sensitivity. Another way of showing you temperature sensitivity is at fixed times let's just look at the axial profile of strain, and again, the temperature, this is our benchmark temperature, looking at this scale, which is very expanded, and at the end of -- when we stop the C-14 sample, this is the strain profile, and we have a constraint on this end and we have a constraint on that end.

Again, the only thing different as you go along the sample basically is the temperature difference. So what I'm saying is at 400 degrees C. for the Robinson rods there is a very, very, very high temperature sensitivity, and when you have guidance like we're going to limit such operations to 400 degrees C., you usually don't worry about 401, 399 or 402.

DR. FORD: I'm just trying to interpret this graph here.

MR. BILLONE: Okay.

DR. FORD: Does that mean you've got a balloon forming?

MR. BILLONE: No, that is an exaggerated scale. I mean, I wouldn't call that a balloon. Our balloons were 60 percent strain, but you have a peak in strain.

DR. FORD: At a certain position..

MR. BILLONE: Well, this gets hotter as you go down here, and the only reason it comes down is you're approaching the end, which is welded and constrained.

We did not think we needed perfect temperature control to get a flat profile, and of course, when you start out at low strains, you don't see that, but being a spin doctor, what I want to tell you is for a single test we're able to get multiple data points that are very useful to study temperature dependence. That's what a spin doctor would tell you.

This was not planned.

Okay. Let's go with our cavalier shutting down of C-15, which temporarily shut down our creep program. C-15 developed a rupture during the final shutdown, which involved cooling from 400 degrees C. under full pressure, intentional. The old hydride reorientation data, the maximum hoop stress was about 205 megapascals. It started at 190 with wall thinning due to creep. The stress would have gotten up to about 205.

And I'll show you a picture of that shutdown. Again, one of the things that's happening

during shutdown under these conditions is you are reorienting the hydrides, the radial and you've got them under significant stress, although the stress is maybe one fourth of the yield stress of irradiated material. So it is not huge.

At the end of the run at temperature, the sample was intact. It held pressure very nicely for a total of 2,440 hours. Rupture occurred when temperature decreased at 205 degrees C. under full pressure. This is a temperature plot with the scale. This is the pressure plot, and boom.

And the rupture was very significant because even though it expanded into the test chamber and the volume, it went through our whole system, wiped out our HEPA filter, blew out the oil in the tank at the end, and contaminated by hot cell standards -- and this is a beta-gamma hot cell -- spread a lot of alpha and beta contamination all over that particular cell.

So it was not a pinhole failure. We ere designed for a pinhole failure because that's what you're supposed to get in creep.

So the status of that particular sample, the rupture caused substantial contamination of the particular beta-gamma hot cell in spite of the

following.

We had de-fueled the sample with boiling nitric acid to get as much of the stuff out from the inside. We minimized the volume, the gas volume, of the sample by filling it with zircaloy pellets. We had an in-line pinhole in pressurization system to restrict gas flow, a solenoid valve to shut off gas pressure when it sets decrease, and we had a downstream HEPA filter.

Unfortunately, with the level of contamination we have to do some clean-up of the cell before the lab will allow us to inspect that sample, open up the furnace, and there's two possibilities.

With welding and plugs, there's always a possibility that you blew an end plug weld and got that huge pressure release. If that's the case, then the sample would still be interesting from a hydride reorientation point of view, but not as interesting as if this happened, the second one, rupture due to hydride reorientation, the second possibility.

So we're very eager to view this sample. I have to spend some money and some time to clean up the cell before we can view it, and I put off that,

but I've been told I have to do it now.

Okay. Let me go quickly through this. Basically that cell which has got low value equipment; the furnaces and stuff are low value equipment. it's too contaminated to salvage. We very much need to retrieve this sample to see where it failed, along with our other samples in there that were either being tested or about to be tested.

And we need to view the test chamber to see whether or not it bulged or any problems occurred because of the size of this pressure pulse.

In a different building we also have beta-gamma cell that we're using. We have the identical system built in that cell ready to resume creep tests.

Again, the system is designed for pinhole leaks and shutting off the pressure. That's no problem to redesign for large pressure pulses, but we have to see whether or not we have to redesign the test chamber depending on the inspection of the test chamber up here.

So this is setting idle until we can resolve this issue. We'll never be able to convince a safety committee that we won't have a large pressure pulse once we had the large pressure pulse,

even if we promised to never ever cool under pressure again.

Let me give you an idea of what we're talking about. This is a test chamber that sits inside the furnace. The sample, this is the sample here, three inches long. This leads to the active gas pressurization system.

We purge it with an inner atmosphere so that you're not oxidizing the sample. So when the sample blew down in here, expanded into this volume, shut out the purge outlet and then did a lot of contamination damage downstream. Live and learn.

Let me give you a little footnote of what could happen, although I don't think this happened. I haven't shown this yet.

Prior to this, we had another Robinson sample where the endcap wasn't that well welded. It had gone for about 400 hours, a much shorter time, very small strain, had roughly similar conditions only lower temperature, same pressure, by the way. It was maintained during the next run, 236 hours. We shut down the sample. It held pressure. We depressurized first and followed that by cooling the room temperature, and during inspection we saw a crack in the weld region that obviously occurred

during the cooldown.

So let's look at that picture because that could be what C-15 looks like, although this is a very different example of something that occurred very early in life, and we've separated these two pieces a little bit so you could see.

This is our active creep sample. This is just a hose clamp to keep the weld affected zone from ballooning out on us. This is the end plug and the weld, and this happened at the bottom, the hottest part of the furnace. So it's not hydrogen migrating to the cold region causing this.

So that's a possibility for what C-15 looks like.

All right. Let's move on to the subject of annealing. We've done some I would call them preliminary annealing tests where we've taken the Robinson samples at about 600 weight parts per million hydrogen. This is a no stress type annealing. We're looking at annealing out of radiation damage, and we did tests from 420 to 500 degrees C.

The reason we didn't do 400 degrees C. is because we were running creep tests at 400. We figured they'd give us the information.

And these are short time tests designed for the vacuum drying process and the length of time of vacuum drying, and these are temperatures that were relevant at the time before the ISG-11, Rev. 2, fixed that temperature at 400 degrees C.

We did post-annealing micro hardness and hydride morphology determinations, and let's see what they look like.

I'm going to skip a few slides.

Okay. This is a matrix of the hardness, and again, for non-irradiated starting material, the hardness in these units, the micro hardness is about 200 for the irradiated material that hasn't been annealed. It's about 250. So that's sort of the range of hardening that you get with irradiation.

And we're looking at the decrease in this number versus time and temperature, and you can look at 500 degrees C. for about 48 hours. You're essentially to your unirradiated conditions, and obviously 420 degrees C. you're essentially there. You're essentially back to midway.

So we've converted this to percent in the traditional way, percent recovery or percent of annealing, and that top formula is the standard way of doing it. And as I say, 500 degrees C., you

recovered about 94 percent of your irradiation damage.

So given time, significant recovery will occur at temperatures greater than about 420 degrees C., and this was all done not under stress.

If you look at the hydrogen morphology, and again, why don't you keep that picture in mind as it came out of the wet pool, out of the reactor into the wet pool, and what we're going to find is under no stress and time at temperature, essentially you will make the hydrogen distribution a lot more homogeneous, which is no big mystery.

So this is the 500 degree C., 48 hours, and hydrogen is much more homogeneous. This is what you started with. So this is one possible effect of vacuum drying if under the old vacuum drying conditions where you were going to more elevated temperatures than the current practices are supposed to be.

So you do have a lot of rods in dry cask storage that have gone through treatments like this.

Okay. That picture would be essentially what you would get if your stress is below the threshold it takes to change the orientation of the hydrides. We don't know this answer. We have data

on unirradiated material. We have a couple of argon data points that need to be put on this plot, but this is the best that existed prior to the start of our data.

This would be the stress that you're cooling under, and this would be the starting temperature that you're cooling from. And if you go to 400 degrees C., you see the critical stress is about 100 megapascals, and we shut down that C-15 sample at 190 megapascals. So it's no mystery that we would have gotten hydride reorientation, although we haven't looked at it yet.

And another sample was at 360. We shut it down at 220 and saw significant hydride reorientation. That's no mystery.

So we need to kind of improve on this curve. Most of it is based on unirradiated data or very low burn-up data. We try to find out not only a boundary for when you start reorienting hydrides, but what percent of the hydrides have been reoriented.

And then finally mechanically, how much have you weakened the cladding by doing that?

Okay. Here's what we are going to do. We've kind of redirected our program a little bit

based on the needs as expressed by NMSS and SBO and RES.

It is not a good idea to do these studies in our beta-gamma cell with our creep apparatus because of the contamination issue. It's better to do it in the alpha-gamma hot cell where the contamination would be trivial. It wouldn't be an issue if these ruptured at all.

And so what we'd like to do -- also, what we don't want to do is extreme tests because you don't have that high of a pressure constant during cooling. Just due to the ideal gas law you're going to have a decrease in pressure as you cool an actual rod.

So we're going to seal pressurized capsules at 400 degrees C. initially in a range of stresses just below what they think is critical for reorientation and just above. I will use a controlled cooling rate, and it will be a corresponding pressure decrease. We're developing under our other funding technology for laser welding pressurized capsules in the hot cell, and the idea is to conduct the test in the alpha-gamma hot cell. It circumvents dose related issues, worker dose issues, and moving samples, and it mitigates all of

the contamination issues. There are no contamination issues in the alpha-gamma hot cell.

Reorientation during dry cast storage. We have the option of letting these samples cook at pressure sealed, 40 degrees C. to get creep, and then control the cooling and decrease the pressure correspondingly with the cooling.

This is something we're working on, and the only thing holding us up is that this is new technology for us, and we're developing that this fall.

But that's how we proposed to study the idea of hydride reorientation, and you could follow that with metallography of the hydrogen, and you could also follow that with micro hardness tests.

Let me say a couple of words on mechanical properties and then close. We have three kinds of specimens that are relevant to RIA testing. The most relevant for dry cask storage is the uniaxial test looking at axial hoop properties, and this is an axial sample with the machine gauge section about 25 millimeters long, and this happens to be after it fails. This is before it has been stretched.

We also have rings with machine gauge

sections to get hoop properties, and as Arthur can elaborate tremendously, we have what I call the PSU, plain string ring stress specimens. These specimens are designed to give you a biaxial loading state in this region here and may be the most applicable RIA type analysis for limits in strain.

So for our combined RIA-dry cask storage, all of these samples are relevant. At the moment the dry cask storage people are only interested in the axial tensile tests and not in the hoop properties. I'm not saying they should be or shouldn't be, but that's what exists at the moment.

I should have Arthur explain this slide because this is the result of Penn State work, but basically this is the Robinson Zirc-4 hydride distribution. Please do not get confused. This is the oxide layer. It's not a dense hydride rim, and this is a pre-hydrided sample unirradiated, and this is just to show you some of the similarities between what you can do in the laboratory without irradiation and what occurs naturally with irradiation.

And the study was to determine ductile versus brittle behavior based on certain criteria. This is one percent strain as a function of the

thickness of this dense hydride rim.

Basically if you are to the left of this curve, you're brittle, and to the right of this curve you're ductile.

So the hydrides are not 100 percent ductile in all temperatures. As you get up above 300 degrees C., even the zirconium hydrides have some ductile behavior. So this tells you that somewhere around 100 microns dense rim of hydrides will embrittle material unless you go up to higher temperatures, and then the material behaves more ductile.

And it's usually a mixed mode failure if you look at the details of that. So it's good to have those results because those results are for unirradiated hydrided samples. Our results will be for a combination of irradiation and hydrogen.

Okay. Let's skip this slide.

Basically we've cut a number of samples, both Surry and Robinson, and more in the process of being cut. I don't know what this strange symbol is.

These are our axial tensile specimens again, and we'll skip this. This is our machine for operating them. That's not the slide I wanted.

What I wanted is we thought we were on a roll back a year ago, a year plus. July 2000 we ran our first ring test with TMI cladding, and we got the kind of hardening we would expect and the losses of strain hardening capability we would expect. This is an engineering stress versus strain diagram.

Unfortunately, there was enough alpha contamination on the ID of this tiny ring sample to cause serious problems with the Instron contamination.

That led to the building of an elaborate glove box, which is supposed to be more like a Chevy, and it turned out to be Cadillac. So this has been completed.

This is the glove box encasing the Instron. This is a smaller glove box with an automatic indentation system so that we can index samples and measure strain directly.

And we passed all of the hoops and hurdles of that. We're in the process of validating this whole system, and we're trying to move as fast as possible to the irradiated Zirc-4, which would be servient (phonetic) Robinson this month. This month starts tomorrow, October.

So we kind of lost a year with various

committees and change in climate and concern about ALARA, and talking to other hot cells, when I complain and moan, they complain more. So I guess it's a generic problem.

Let's summarize. Thermal creep tests, we completed five Surry tests, initiated an additional one, but we didn't get far enough, and we have one more to go.

We completed two Robinson tests, one intact and one not intact. We initiated two at 380 degrees C. and there are six more planned tests.

Testing will resume this fall after we can inspect the C-15 sample test chamber. Axial tensile tests, we're doing baseline properties of unirradiated Zirc-4 right now. This would be a Robinson design, room temperature to 400 degrees C., two different strain rates, .1 percent per second and 100 percent per second, and we'll do a couple of Surry tests, and we're hoping to initiate both of these in the month of October.

The only thing holding us back is some problems with the plant facilities in terms of the fans that draw through the glove boxes.

All right. Let me continue with what's planned and where we run into sort of a question.

We're ready to do pre- and post-creep three point bend tests at room temperature. We have two Surry samples available, one Robinson sample available, and here's the question:

What do we do about impact tests? Impact tests are really high strain rate, three point bend tests. You whack something in the middle. It's supported at two ends. I don't have something long enough to demonstrate, but you have a sample supported at two ends. You either come down with a guillotine in the middle. That makes it a three point bend test at very high strain rate.

Usually you groove the opposite side or you swing a pendulum and you whack it and you look at the difference in absorbed energy between the initial energy of the pendulum and the final energy.

So our proposal had been for normal Instron three point bend tests. There seems to be a concern that that's not enough and that we should be doing some impact tests.

There is a question. Well, we can do impact tests. There's a question of how the data are to be used because this is not a traditional sample of impact tests, such as a Charpy sample where you purposely put a known flaw in and study

the crack growth.

So are such tests meaningful for unflawed pre- and post-creep tubes? And how will the data be used?

We can do the tests. We would like to sort of pursue this further discussion as to how one would use the data because that could dictate what kind of tests we choose to do.

So I would call this an area that requires further discussion between the people who need the data and the people who generate the data.

And let me end on that note.

CHAIRMAN POWERS: Any questions for the speaker?

(No response.)

CHAIRMAN POWERS: I don't see a lot of questions appearing. Thank you.

MR. BILLONE: You're welcome.

CHAIRMAN POWERS: We're now scheduled to hear from Mr. Lukic.

MR. LUKIC: Lukic.

CHAIRMAN POWERS: Lukic.

(Pause in proceedings.)

MR. LUKIC: Good afternoon. While we're waiting, it's a pleasure to come over here.

THE REPORTER: Sir, a microphone.

CHAIRMAN POWERS: You can use either a clip-on or sit, one or the other.

MR. LUKIC: Before starting, it's a pleasure to be here. This is a civilized type of climate, not like Arizona where it's 105 degrees as we left. They tell us it's dry heat, but after 13 years we still don't believe it.

CHAIRMAN POWERS: Well, in Phoenix, there's no such thing as dry heat, and I thought Arizona was now referred to as Eastern California.

(Laughter.)

PARTICIPANT: That's probably pretty accurate.

CHAIRMAN POWERS: You're just the Californians that don't get to vote. That's it.

(Laughter.)

PARTICIPANT: Without ocean front property.

CHAIRMAN POWERS: Wait till the earthquake.

PARTICIPANT: Then we'll all have beach property, yes.

CHAIRMAN POWERS: We're looking for technical support here.

MR. LUKIC: But this is work that has been developed all at Palo Verde. It has to do with our own particular design. I've been asked a question just about an hour ago. We are not going to talk here about boilers. We are not going to talk here about pressure at Westinghouse, pressurized reactors. We are just strictly talking about our particular design today.

Jeff Schmidt has been instrumental in coming up with the lattice redesign that has evolved from having a correlation, a model that can predict crud deposition, and hence, his work was optimizing the lattice design to make possible to deal with crud, in fact, to minimize crud.

Oh, thank you very much. I appreciate that. Do you want to handle this? How many engineers does it take to run a presentation, I guess, huh? Sounds like a California joke.

Okay. Where did you put next slide? And then the next slide?

Okay. About six years ago APS has transitioned to a more efficient design philosophy. This transition was driven, in particular, for a desire for a larger capacity factor, as well as cross-reduction pressures that most energy

manufacturers have to face.

The transition, the shift was from a traditional checkerboard core design to one of a couple of ring type of designs that make it more efficient, in particular, the ring of fire and the Saturn core designs.

Next slide.

The effects of this transition were pretty well established once we had an inspection. It was quickly seen that there is a crud build-up, something that had not been seen before in the checkerboard core designs.

You're probably aware, but crud has some pretty negative characteristics. For one, it inhibits heat transfer. As a result of inhibiting heat transfer, there is a raise in clad temperature, and also there is an oxide layer growth rate increase.

Furthermore, it is believed that crud concentrates lithium and enhances it. It is postulated to increase corrosion.

Lastly, crud may lead to boron deposition within its own matrix, and that is a precursor of AOA. All of these things are pretty negative.

CHAIRMAN POWERS: Let me ask a question.

MR. LUKIC: Yes.

CHAIRMAN POWERS: You indicated that the crud increases the local clad temperature just because it inhibits the heat transfer, and that in itself will be enough to increase the corrosion, but you said there's an additional effect due to lithium?

MR. LUKIC: yes.

CHAIRMAN POWERS: Do we know why that is?

MR. LUKIC: This has been postulated. It has been postulated that there is some concentration of the lithium and that may cause itself some clad corrosion, some damage to the actual cladding.

CHAIRMAN POWERS: I'm wrestling with trying to understand how the cation affects the corrosion.

MR. SCHMIDT: This is actually postulated to be a LOCA pH increase due to lithium, maybe a lithium borate of some type that is postulated to occur at the crud-clad interface, and that pH effect could enhance corrosion.

MR. CHENC: Maybe I should add a little

bit on that issue. When you increase the --

CHAIRMAN POWERS: Would you like to use the magic microphone? Those are the ground rules around here.

MR. CHENC: Thank you.

CHAIRMAN POWERS: We'll listen to anything, but it has to be by microphone.

MR. CHENC: My name is Bo Chenc from EPRI.

I think there's a lot of testing of zircaloy in this condition when lithium is somewhere like seven ppm by itself without boric acid. You see an increase in the rate of corrosion of zircaloy.

When you have boric acid, then it will be neutralized. Even with 100 ppm or 200 ppm of lithium in water, as soon as you add enough boric acid, there is no effect on the corrosion rate of zirconium model.

So it depends. You know, you have to have a solid separation of lithium to cause the corrosion enhancement, but as long as in the PWR core, because you already have substantial boric acid, you know, 800,000 ppm, the effects of lithium tend to be very small.

CHAIRMAN POWERS: Okay. Thank you.

MR. LUKIC: In 1999, there was a Unit 2 outage inspection and fuel inspection, and that fuel inspection indicated the presence of tenacious crud deposits. Peripheral pins of high duty assemblies appear to be most affected.

Now, responding to concerns raised by the fuel inspection, there was plans to put together a detailed thermal hydraulic of the selected high duty assemblies. The objective was to try to establish a correlation between localized thermal hydraulic variables to measure crud thickness.

And such a correlation it was felt if it could be developed would be a useful adjunct to lattice redesign that will allow us to preclude the type of thermal hydraulic conditions that leads to enhanced crud deposition.

During the Unit 2 visual inspection, it was revealed that crud deposits occurred, as I said now, mostly on peripheral rods, such as the assembly P2K410. And so following the inspection, the P2K410 was taken apart and selected rods were subjected to eddy current testing to basically gain a trace of the crud and oxide thickness.

The measurements that were performed

with eddy current testing confirmed that the crud deposits were mostly at Spans 7 to 9. That's pretty much towards the top of the core, and they occurred predominantly in the peripheral rods. That's the first and the second row, and to a much lesser degree, significantly lesser degree, on the interior.

This figure basically shows the five rods of the P2K410 starting from the bottom of the reactor to the top of the reactor. A5, rod A5 happens to be a peripheral rod in the first row. We'll shortly see where, and the Spans 7 and 8 and 9 show the combination of the composite of oxide and crud.

A 353 subchannel, four quarter assembly pH model was developed. Axial and radial power distributions for this model were developed using the SIMULATE-3 code, and that data was entered into the VIPER2 code, along with the other extensive required data.

In parallel to developing a model, we did analysis of the eddy current test data analysis, test data that was collected, and in order to quantify crud thickness of the selected rods at axial locations that were identical to the thermal

hydraulic model.

That basically provided us with all of the information that we need to quantify the thermal hydraulic model.

This is a transverse cross-section of that four quarter assemblies that show the northwest corner of P2K410 in the lower right position. Also, I don't know whether you will be able to see it, but this is the position for Rod A5, which is the peripheral rod. B4 would be on the second row. D3, the one right next to the instrument guide tube, that did not show any deposits at all, and there is an E7 and H5.

That gave us a very good cross-section about locations and differences in thermal hydraulic characteristics to be able to quantify an appropriate model.

One interesting thing out of this. I did mention to you that fuel pin D3 did not show any evidence of tenacious crud build-up. In fact, during the inspection, the visual inspection, the people who were inspecting, it just showed a very shiny rod as opposed to where tenacious crud was present, which is dark brown and fairly obvious.

So we used this fact that D3 did not

appear to contain any crud deposition. So to first approximation we assumed that it was affected by oxide only.

That allowed us to calculate the inferred crud thickness for the other pins, and these were obtained by subtracting the D3 oxide thickness from the other rods.

Next slide.

This information here is the basis for the regression model that we use. Once we have stripped the oxide information from this data, what remains over here is just a trace of the crud. It's an inferred crud thickness because of all of the assumptions that we made with regard to D3.

Again, A5 in blue and B4 in violet are the peripheral rods, and E7 and H5 are interior rods.

Go back one more time. Right there. Okay.

Again, this is Span 7, 8, and 9, and this is where the deposits were observed predominantly.

Next slide, please.

This is a formula. This is the regression model that we used. We went through many

iterations and lengthy evolution. In fact, we decided that this is the appropriate equation.

Data here represents the crud thickness and the subsequent J at various different burn-up intervals. Other important variables here is W sub I, which is the weighting coefficient for a given burn-up interval, I, which is a burn-up interval steaming rate, and chi survived the burn-up interval correction factor.

These variables contain, in fact, the parameters for which we do regression analysis.

CHAIRMAN POWERS: What is the coefficient that you're optimizing herd?

MR. LUKIC: Okay. We are not showing this because it's proprietary in nature, but the burn-up interval weighting coefficient has one parameter that is being fitted. Psi I has three parameters. The burn-up interval correction factor, chi of I, contains one parameter, and the last parameter will be C bar, which is cycle averaged crud concentration.

We'll be happy to expand on that in a closed meeting, but we felt that it would be most appropriate if we did not show the details.

Next slide, please.

CHAIRMAN POWERS: And you also have a summation over J or something.

MR. LUKIC: Yes. W sub I is the summation of the weighting coefficient across all of the burn-up intervals, is equal to one. That is on the next slide.

Keep going one more.

Again, it has five parameters. The summation of the weighting factors adds up to one, and that's kind of a forced fit when you do the regression analysis.

Next, next.

It's very interesting to point out that traditionally steaming rate is calculated by subtracting the convective heat flux from the total heat flux. We have gone through that approach initially, but we found that we had some inaccuracy in the prediction, and so we went and did a more untraditional approach and actually did fit the steaming rate parameters as required, let it flow and let the nonlinear regression take care of that.

CHAIRMAN POWERS: So your steaming rate is a determined quantity?

MR. LUKIC: I'm sorry?

CHAIRMAN POWERS: Your steaming rate is

--

MR. LUKIC: Yes. The variables that entered into it --

CHAIRMAN POWERS: How do you assure that in a fitting process that you get anything?

MR. LUKIC: Yes.

CHAIRMAN POWERS: You must have to constrain it some way.

MR. LUKIC: I'm sorry?

CHAIRMAN POWERS: In just a fitting process you're going to have to constrain that variable to keep it in a rational regime.

MR. LUKIC: They are in a rational regime, and I can assure you of that, and we'll be happy to go over that after the meeting if you're interested in it.

DR. FORD: I was about to ask the question a slightly different way.

MR. LUKIC: Sure.

DR. FORD: Knowing crud deposition, you can explain it in terms of potentials of zero charge, et cetera. This algorithm you've got here doesn't take into account what is happening physically on the surface, or is it just purely fitting to some data?

MR. LUKIC: Actually, yes, I think it does. I think it allows us -- I mean there are certain principles. In this case it's the steaming rate. As you steam, you're depositing the crud, which is in solution, and you know, that adheres to the cladding with a particular efficiency, given the rate of process.

So, yes, we are taking care.

DR. FORD: Okay.

MR. LUKIC: And we found that empirically determined steaming rate provides better results, and hence, that's what we used. And we'll be showing some comparisons between this model where we allow certain variables within the steaming rate to float, and one that we take a hardball approach and define the steaming rate as traditionally is usually used.

Next slide.

This figure is a comparison. The blue is the inferred value of the crud, and the whatever color this is, the red, is the calculated one. These are span averaged crud thicknesses. This is for Rod A5, and Span 9 has the largest amount of crud deposits. Span 8, we simply go down all the way to Span 6. It becomes a minimum.

This is another peripheral rod on the second row, Rod B4, that experienced somewhat less crud deposition than the entirely peripheral rod, but nevertheless, does experience.

Rod E7 and Rod H5 are interior rods, and they experience minimal crud deposition.

DR. FORD: So could you tell me what the difference between inferred and calculated? Inferred is observed?

MR. LUKIC: That's how we use the word "inferred." We made an assumption regarding D3, Rod E3, that it was only affected by the oxide. So when we subtracted the oxide trace from D3, we were left with a level of crud.

Now, that's why we call it inferred, because of the subtraction of D3. I'm saying "measured" because that would be a direct measurement. So we tried to keep that straight so that it's understood.

MR. OZER: You also have something that all of the rods oxidize at the same rate.

MR. LUKIC: Yes, yes. The temperature is fairly close in that particular high duty assembly area, and it's a first approximation. Now, you can go and further refine this with additional

iterative type of analysis, and we have done that, but --

MR. OZER: So if the temperatures are the same, why did D3 have no crud?

MR. LUKIC: I'm sorry?

MR. OZER: If the temperatures among all the rods are the same --

MR. LUKIC: Ah, okay.

MR. OZER: -- why did D3 have no crud?

MR. LUKIC: The bottom of this is, again, the steam rate. If you had rods that did not experience steam rate at the time when the crud concentration is the largest, which is at the beginning of the fuel cycle, those would not see very much deposits, and I will be showing shortly the assembly P2K410, and you will be able to see the cross-section of all the fuel pins and crud deposition. I think you'll become clearer.

CHAIRMAN POWERS: When you do your fitting process, how do you monitor auto correlation in your residuals? They sure look auto correlated to me.

MR. LUKIC: I'm sorry?

CHAIRMAN POWERS: Do you look for auto correlation errors in the residuals when you do a

fitting process on this regression formula?

MR. LUKIC: You do a fitting process basically, yes. This is done --

CHAIRMAN POWERS: Do you look for auto correlation?

MR. LUKIC: I'm sorry?

CHAIRMAN POWERS: Well, it looks to me --

MR. LUKIC: Auto correlation is typically for signals. When you do nonlinear regression analysis, you are basically searching for the absolute minimum in that multiple dimension curve.

CHAIRMAN POWERS: Well, the problem is that you get a parameterization that makes your residuals auto correlated, and that's usually the mark of your physical phenomena just aren't being reflected in your formula.

And when I look at what you put up there, it looked like they were auto correlated. So I wondered, do you monitor something like a Durban-Watson statistic or something?

MR. LUKIC: Yes. Well, you monitor. Once you develop the model, once you quantified through regression analysis the model, then you

compare it with data that he hasn't seen.

CHAIRMAN POWERS: But do you have a Durban-Watson statistic?

MR. LUKIC: Oh, absolutely.

CHAIRMAN POWERS: And what does that number run around?

MR. LUKIC: I don't remember right offhand, but I mean, that's certainly something that's available. That plus rho squared, which will just give you --

CHAIRMAN POWERS: That's just a global measure. It hardly tells you anything.

MR. LUKIC: Exactly, exactly.

This slide, in response to your question, this slide shows the northwest corner of P2K410, and it shows that crud, span average crud deposition. This will be the peripheral rods together with these. This will be rod A5, and this will be rod B4. Rod D3, if I can point out, this will be rod D3, and rod, let's see, E7 would be here, and what's the next one? H5 would be here.

The important thing to see from all of this is that these calculated values of the crud match very well what was observed during the inspection. The interesting part is that there is

hardly any deposition at all around instrument guide tubes, and this was the first indication about the mechanics of how the model worked.

Okay. Next slide.

This is an interesting slide. On the left, we see the APS crud correlation. The error bands are 90 percent confidence level, five percent on the bottom, 95 on the top. It shows a reasonably good fit on the average between the measured crud thickness and the calculated crud thickness with the model.

We have performed as a comparison; we have performed -- we calculated the values of the correlation that is strictly based on the steaming rate, and then displayed effectively the same data that we have here.

It can very easily be noted that for measured thickness, low measured thickness, the correlation that's based on steaming rate alone tends to show higher values than it should.

Likewise for larger measured thicknesses, it really under evaluates the magnitude of the thickness. It should be here, and yet it is showing here. In fact, it seems like it is stuck at the level of ten. So it doesn't show any value

higher than ten, which is not a problem over here on the other one.

I hope that maybe begins to shed some light with the motivation for modifying it.

Beyond the one quarter assembly, we developed a lump subchannel one-eighth core T-H model, and the objective for this was to be able to quantify crud deposition on the assemblies. If you used the four one-quarter model, you are just limited to finding out what happens in four adjacent assemblies.

But if you have a one-eighth core model, then you can pretty much identify what is your lead assembly, the assembly that produces the most crud, and then if you're interested, you can go in more detail with the four one-quarter assembly T-H model and develop information on a rod basis.

The first model, one-eighth core model, that we developed was consistent with a resolution of the traditional lattice. Traditional lattice recall is what we used before we redesigned the lattice, and that effectively had the interior -- was pretty much dead as far as crud deposition.

Once we started moving that crud, spreading it evenly across the lattice, it become

more important what happened on the inside, and hence the second model of the eighth core model provides the enhanced resolution of the assembly interior. That has been very, very useful.

However, the price, this is a comparison. This is the original one-eighth core T-H model with very gross resolution in the center, and here is a very detailed model.

Now, everything comes at a price, and the quantification of the T-H model like that takes about ten times as much CPU time as the other one, but well worth the time.

Next slide.

A computer program was written to read VIPER output data. The program calculates crud assembly deposited on -- crud deposition on all the assemblies, as well as the core. It helps us identify assemblies with the highest crud deposits. These are the lead assemblies, and then, again, as I mentioned earlier, if we need more detail, we go to the four one-quarter assemblies to obtain that kind of detail.

Now, as far as crud model application results, the crud model has been fully integrated into the core design process. In fact, it has been

used so far in six reloads, and there has been no evidence of AOA or crud induced failures, which we feel is not a direct measure because we have not measured the crud on any of these. It's a very costly proposition, but an indirect indication has been that we do not have crud induced failures or AOA.

The model application has been a real success. It helped prevent crud deposition. It eliminated potential for crud induced corrosion in AOA, and as we feel, as we like to think about it, it prevents the cause and avoids having to treat the symptoms.

Jeff will continue from here on the lattice redesign that basically is an evolution of what we were doing before. By modifying the lattice design, we can really take advantage of the ability to measure the crud and optimize the lattice such as to spread the crud and otherwise minimize the crud level in the entire core.

So Jeff Schmidt.

MR. SCHMIDT: Good afternoon. I'm Jeff Schmidt, like Yovan said, section leader of Nuclear Analysis Group.

And I want to talk today about basically

the application of Yovan's model to a lattice redesign to try to basically reduce crud deposition.

As Yovan mentioned, six years ago Palo Verde made a transition in in-core fuel management. That transition was driven by a desire to increase the plant capacity factors while maintaining or managing fuel costs. The basic transition was a switch from a checkerboard loading pattern to a ring or a pre-type loading that Yovan mentioned.

Here are some examples of that. On the left you'll see a traditional checkerboard pattern. The blue assemblies are the feed locations.

Let me get this together here. I didn't get your laser pointer.

MR. LUKIC: Oh, my laser pointer didn't work.

MR. SCHMIDT: Given defective material. There we go. All right. Go back to the pictures, please.

MR. LUKIC: Okay.

MR. SCHMIDT: Okay. We see here the traditional checkerboard loading pattern. Basically the dark blue are the feed assemblies, and they're surrounded basically on four of adjacent faces by burned assemblies.

We transitioned to a ring type design where, again, the blue assemblies, the darker blue assemblies are the feed assemblies. You can see basically two pronounced rings, an inner and an outer ring surrounding the interior checkerboard, and that placement of fuel led to increased crud on peripheral assemblies or filled pins on the assemblies.

So following the fuel inspections, when we transitioned to a ring pattern, as Yovan mentioned, basically it was a deposition on peripheral pins with the high duty assemblies.

The contributing causes were basically the highest pin powers and the lowest flow locations in the assembly, and degrading thermal hydraulic conditions due to conservatively plugging steam generator tubes. What's happening basically is we are plugging tubes, and flow was reducing, and over time that contributed to the enhanced crud deposition.

Effective fuel management. Basically the current Palo Verde designs are limited by crud deposition and not traditional peaking factors, such as DNBR, linear heat rate.

Crud deposition has led to AOA and fuel

failures at some locations or some plants, as you're aware of.

Basically after we observed the crud induced AOA, we created an integrated fuel performance plan, which was to address in a global perspective the crud deposition that we were seeing.

One of the key components of that plan was to evaluate the current lattice design and its performance in these ring type loadings. Our current lattice design has two intra-assembly enrichments, which are basically a high and a low pin enrichment. The low enrichment pins are typically surrounding the guide tubes and the corner pin of the assembly, and then the high pins or higher enrichment make up the rest of the assembly design.

This enrichment split in our design effectively pushes power to the peripheral pins of the assembly, and that's aggravated when you load them in feed, face feed location.

That is further exaggerated when you load additional erbia. Erbia is our burnable poison. In a ring type design you're loading more erbia, which again enhances the push toward the peripheral pins.

Why don't we go ahead and show a picture? Go ahead, Yovan.

Here is a typical, fairly high erbia, which is our burnable poison type quarter of assembly. This is the northwest quadrant of an assembly. This would be the center of the assembly. That's a quarter of the center guide tube. Here's your full guide tube.

As you can see, the box marks where the max relative pin power is, and if you examine this slide, it's pretty much predominantly along the face of the assembly is where power is being pushed. All of the rest of the assembly locations are pretty low in relative power, and this is at beginning of cycle, and that's important to know.

The goal basically of the redesign was fairly straightforward. It was to avoid any plant operational challenges or pin integrity challenges due to crud and try to attempt to reclaim some of the efficiencies in ring type loading.

Redesign aims to reduce basically total crud mass and also for the crud that remains is to homogenize the crud within the assembly so that we don't have it localized all on certain surfaces to yield a very thick crud layer.

The redesign was basically we looked at enrichment, changes in enrichment and splits and burnable poison locations because, one, that we felt was the best understood and lowest risk, while at the same time being quickly available to implement instead of doing other design changes.

The redesign effort consisted of three phases basically, as I mentioned: examining the current lattice design in a ring type loading, which was very interesting; perform calculations to modify the intra-assembly enrichment to see if we can improve or reduce the crud deposition; and then also kind of modify the burnable poison locations for that same result.

And then once we had some candidate lattice designs, is go ahead and throw them into various test core designs and see what the crud deposition yielded.

Basically two approaches or two design philosophies were used in the design of the lattice. One is to lower early cycle peak pin powers. We felt that deposition curve primarily early in cycle, and that if we delayed higher pin powers to later in cycle, that would have a reduce crud benefit, and even if we did have crud deposition, there would be

less boron to have an AOA. So that was kind of one of the design philosophies.

The other one was just simply to match, better match assembly flow, basically subchannel flow to pin powers.

Basic steps were as I mentioned, is to modify the two. First we started with what we could do with the two enrichment, say, limitation on the assembly. So we used that. We designed difference splits of enrichment. Then when we found something that looked reasonable or would lead us in the right direction, we would modify burnable poison placements to fine tune it.

And then really one of the early tests is is the BOC beginning a cycle pin power distribution roughly equal to what you would see when the erbia burned off and you got a mid-cycle peak. Because we didn't want to artificially reduce BOC and then pay the penalty later on in middle cycle or end of cycle. We just didn't want to move the problem basically.

And then step four was once we got some candidate loading patterns is to go ahead and set up some core design models and actually design various core designs and predict the crud deposition, and

then basically we extended -- we usually work from like a pallet of 16 to 20 lattice designs where the differentiation is the number of erbia pins.

So then we basically concentrated on ones we typically use in a design. Once we found the designs we liked there, then we expanded it to the whole range of erbia loadings basically from zero to some number.

Basically we had very good success. We got to step three, and we had two two enrichment designs with different burnable poison placements that yielded some significant crud deposition.

When we further studied those designs though, we decided to implement a third enrichment to fine tune the design, and that's really where we ended up with our final lattice design. That extra degree of freedom we were able to tailor the power distribution to the flow a little better.

Here's a picture of a fairly heavily poisoned assembly. What you have here is a relative peak pin power of the assembly versus burn-up and EFPD. The top line up here that starts high and goes low is our current lattice design, and the what we're calling the Lattice F -- it was my F try -- was basically very similar, BOC peaks to MOC peaks.

So we weren't --

CHAIRMAN POWERS: Pretty good if you can get it by try F, presuming you started at A or did you start at Z?

MR. SCHMIDT: No, no. I started at A, but there were probably A1s, A2s, A3s before --

CHAIRMAN POWERS: Oh, I see.

MR. SCHMIDT: No, I think Lattice F was mainly to -- kind of branched off into the third enrichment. The other ones were the two enrichment designs.

Here's another representation of that. It's basically comparing our current design with 72 erbia to our new design with 76. We don't have a one-to-one comparison. This is the closest we could do.

You basically see along the outer edge is about a three percent reduction in pin powers, and that was really what we were looking for.

What's also important here is that we didn't -- even though we reduced powers along the face, we didn't really peak it up at least at BOC significantly anywhere because the max location, which is this red box for the redesign pattern, is almost identical to that similar pin in the current

lattice design.

So we were able to reduce this edge face where we were seeing the crud deposition, but we didn't put a big pin power somewhere else that we'd just basically be moving the problem.

At MOC, you have still a reduction in the outer row of pins, but what you're seeing now is that you're seeing a pin power increase towards the center of the assembly or really towards the -- this is the center down here, but this would be the guide tube locations.

But if you look at the absolute value of the new lattice, it's still very low relative to the BOC pin powers of the current lattice. They were about six percent, seven percent. We're still talking four percent here. It's a seven percent increase, but as Yovan noted, we're seeing almost no crud deposition around the guide tubes, and there's a reason for that. That's our highest flow location within the assembly. So that's really where we wanted to push the power to.

Phase three of the design was take our pallet of new redesigned lattices and put it into our Unit 2, Cycle 12, which is our up rate cycle. Unit 2, Cycle 12, is a three percent power up rate,

but a degree and a half inlet temperature increase, and with new steam generators.

So we basically took a parallel design pass, saying, okay, we take our new lattices and we take our current lattices and make the best designs possible out of each one and see where we end up in crud deposition.

Go ahead, Yovan.

We compared those to Unit 3, Cycle 9, where we had mild localized AOA in the high powered assembly. So that was kind of considered our threshold. Do not go past that mild AOA.

Here's the results. You basically have Unit 3, Cycle 9, which is our benchmark. Unit 2, Cycle 12, with the C stands for the current lattice, and this is the revived or redesigned lattice. Here you basically have maximum pin -- that should be crud thickness. Sorry about that. There should be "crud" in there, and that's basically a span average crud thickness.

And so you can see that with our best design on our current lattice, we were going to go over our three/nine threshold. So we felt we had some risk associated with that.

And the revised lattice, we had a

significant reduction in crud deposition. So we felt pretty comfortable with that.

This is just another way of looking at it. This look at total core crud. That was kind of a localized maximum even though it's a span average, but it's kind of a criteria of potential for pin failure if you got too much crud.

This is kind of a global AOA indicator we tend to use it as. Again, you can see three/nine here. The current lattice did pretty well in terms of current, and the revised lattice did substantially better.

These, I should mention that the two-12 designs have different design assumptions than the three/nine. Because we're getting new steam generators, we have to assume an increase in source term, basically the crud concentration coming off the bare metal of the new steam generator before it is basically pacified.

So what we did is for the two/12 designs we assumed basically a source term or a crud concentration of twice that would be assumed in the three/nine design, and because we don't know how basically the RCS crud concentration or nickel and iron will fare with time, we assume an equal

weighting of deposition all through the cycle. So that adds conservatism to the two values represented for the cycle 12 designs.

The redesign lattice is predicting significantly reduced crud deposition, as you saw. Palo Verde has decided to implement the redesigned lattice in all future core designs.

And then we have a multi-cycle fuel inspections plan for multiple cycles of Unit 2 coming up to further validate the crud model and to make sure the revised lattice is behaving as predicted.

Just a couple other points that Yovan had talked about was we have been using this crud prediction model for six cycles now, and we have had an opportunity to look at one assembly visually that was a high powered feed assembly, and that fell -- you know, visually it's tough to tell, but we did not see the tenacious crud that we had been seeing on prior visual inspections of our fuel. So that's another indication that we seem to be moving in the right direction.

So we have had some data. The Unit 2 data is going to be -- excuse me. I'm not used to talking this much. The Unit 2 data will include

oxide thickness measurements and basically visual inspections to, you know, further validate the model. So that's going to be ongoing.

Do you have any questions?

DR. KRESS: I have a question about one of your earlier slides.

MR. SCHMIDT: Okay.

DR. KRESS: The one on your crud thickness regression model.

MR. LUKIC: Do you want to know the formula?

DR. KRESS: Yeah. It doesn't reproduce very well.

CHAIRMAN POWERS: Here we go. We've got it all written out here.

DR. KRESS: Okay. That takes care of my question.

MR. CARUSO: We copied the formula from the paper. Is that the same formula?

MR. LUKIC: Yes, absolutely. Would you like to keep the disk?

MR. CARUSO: Sure, that's fine.

MR. LUKIC: I'd be happy to give it to you.

MR. SCHMIDT: For some reason it didn't

print out when we went to print out this.

MR. CARUSO: That's right.

CHAIRMAN POWERS: That always seems to happen.

Any other questions for the speakers?

Are we going to have any data on how this new core load behaves?

MR. SCHMIDT: Sure, if you want. Absolutely. Be happy to.

CHAIRMAN POWERS: I mean if nothing else, send us a note some time and tell us how it works, I mean, what the outcome is. This is like one of those mystery stories. I'm waiting for who done it here.

MR. LUKIC: And if you could invite us for when the cherry blossoms are on, that would be even better.

CHAIRMAN POWERS: Oh, yeah. We would love to do that, except they carefully schedule ACRS meetings so that that doesn't happen. We work for the government. So you've got to suffer. It's one of the requirements of the job here.

MR. SCHMIDT: Just as an aside, we'll be looking at ZIRLO performance as well.

CHAIRMAN POWERS: Oh, okay.

MR. SCHMIDT: But we will be looking at a lot of things coming up, including oxide and crud deposition and ZIRLO performance. So we're going to get a lot of data out of it basically. In the next three cycles we are planning on fuel inspections.

CHAIRMAN POWERS: Okay, yeah. I think I would enjoy hearing how it all comes out and get --

MR. SCHMIDT: I will, too, if it comes out well.

(Laughter.)

CHAIRMAN POWERS: No, see, I'm interested regardless, but you only --

MR. SCHMIDT: Maybe somebody else will be up there if it doesn't come out well.

(Laughter.)

CHAIRMAN POWERS: Well, thank you very much.

MR. LUKIC: A pleasure.

CHAIRMAN POWERS: Let me now walk around the committee and see if people have any first thoughts here. I'll keep doing this throughout the meeting in order to give you a chance to revise your thoughts.

Peter, any thoughts on what all you've heard here?

DR. FORD: You've said there are two questions.

CHAIRMAN POWERS: Well, I've got about five here.

DR. FORD: The whole topic of the structure and temperature and fuel cladding, there's a complex interaction diagram shown, and I'm concerned that there was nothing related to the primary and secondary interactions to distinguish them. We only heard about the RAI, the LOCA, and the transportation cost or the plan. Your question, Dana, was how complete is the plan. We only had about three of them.

We didn't hear any about ATWS for the BWRs.

I was concerned that FRAPCON and FRAPTRAN do not predict corrosion and hydrogen embrittlement effects very well, and yet the embrittlement of the fuel cladding is a prime variable, and yet the FRAPCON and FRAPTRAN does not take into account corrosion effects.

As far as the RAI aspects are concerned, there's obviously some disagreement with EPRI about the question of the pulse, the size. That concerns me because it seems to me two of the experts

disagree, and we don't know which one is --

CHAIRMAN POWERS: They're a hell of a lot closer now than they were last time.

DR. FORD: Yeah, true. I was a little bit concerned that in the plan we were talking about three approaches to the RAI, and yet it now looks as though because of the stretch of time coming up to resolving this by the end of this year, that we're only going with one, which was really to modify the paintbrush data using modifications of pulse width aspect. It seemed as though they're shoving the Vitanza multi-parameter code to one side, and maybe that's a mistake, but that's what I thought I had heard.

I think it's going to be optimistic that we're going to have a believable modification by the end of this year, 2003.

As far as the LOCA is concerned, my first question really was or concern was are we absolutely sure the compression ring test is the right test to do. I am not a mechanical engineer, but I keep hearing these murmurs that maybe it's not the right one to use, and yet the whole approach depends on that particular test.

I was puzzled somewhat to see how from

the basis of some of the pictures we saw, how the pellets were going to be contained in the ruptured -- if the tube does rupture and swings around in a somewhat chaotic, thermal hydraulic and mechanical condition, how the pellets are going to be contained.

The LOCA thing I thought was a very ambitious program which I think can be done by the summer of next year, which is what I had heard. I don't know how the gaps in the questions that obviously still abound, how they were going to be answered by the other cooperative partners that you have. You mentioned the Russians and the fact that they had corrosion aspects for E110. I don't know the specifics of those interactions.

I will write this all down, Dana, for you, but your final question was, I believe, how much should be done by NRC versus other parties, primarily industry. If you remember in our last year's research report, we made a case for crucial areas, such as neutronics codes and fuels, NRC must have an independent research capability in the crucial area of fuels.

I tend to agree. However, looking at the number of questions that still abound, I can't

see how they're going to be resolved without there being some sort of cooperative arrangement with industry. I'm not quite sure that exists currently.

I'll write this all down, Dana, but those are my first --

CHAIRMAN POWERS: That's good. You've been thinking hard. We'll report to Bonaca that you didn't dally around in this meeting; that you worked diligently.

DR. FORD: Thank you.

CHAIRMAN POWERS: And he'll undoubtedly give you a gold star.

Dr. Kress?

DR. KRESS: Well, let me address the RIA, and I'll the initials in the right order.

DR. FORD: Oh, I got them wrong?

DR. KRESS: First, we did see basically two approaches, one by the staff in readjusting their basically empirical paintbrush model in order to come up with a boundary for the failure insertion rate and one by EPRI, which I haven't seen the details of yet, but I understand it's a look at the methodology of failure due to the loads and the stresses and the material properties and getting some sort of failure rate.

I see no reason why both of those wouldn't work. I mean there's no reason both of those in principle couldn't work. I think the paintbrush approach we heard from Ralph will very nicely settle the issue of do we have the right regulatory bound on the reactivity insertion and will the calculations show that we're below that bound for the ones.

I think it will handle that for the existing clads and fuels. I think though that if we're going to look at new clads and new fuels, that you are either going to have to have a lot more data to do that process, and I worry that you may miss some of the fundamental issues.

So I think I'm leaning towards both approaches. I like the EPRI approach for the new materials, and I think the staff's approach to show that the current regulatory level is okay is the way to go.

So I like both approaches. I think in order to extrapolate this to the different materials you're going to have to go with EPRI's approach because I think it will take too much new data to get a new paintbrush curve for the new materials.

May I'm wrong there, but I think I would

look very strongly at the EPRI approach and review their stuff when it comes in. I haven't really seen it in detail yet.

With respect to the LOCA stuff, I think I like all of the fundamental data they're getting on the materials properties and the effects of hydrides and oxides on the strength of the material and on its ductility. It seems to me there is a missing step, and that's how you convert that into what would sufficient -- the word "sufficient," I guess, is in there.

I didn't really see that step being closed yet, and I think some more is needed on that.

I guess I thought all of that work done by Argonne was good work and nice stuff to have and have no real complaints about it.

There is this issue that you brought up about single rods versus bundle behavior, and I don't know how to deal with that right now. I think it's still an issue and will have to be dealt with at some time.

That's about it, I guess.

CHAIRMAN POWERS: Good. Thanks.

Vic.

DR. RANSOM: Mine is going to be pretty

minimal. This is kind of a new area for me, but from what I heard it certainly sounded like the models were the right way to go to extrapolate the data, and I guess I sort of felt like there wasn't an awful lot of difference in the high burn-up compared to the normal fuels as far as the least failure criteria were concerned.

And I would say that uncertainty was brought up a couple of times, but not really addressed very well, and any of this modeling, and I think in general that should be addressed in either approach.

That's about all I would have to say.

CHAIRMAN POWERS: You make the point; you and Dr. Kress both made the point that modeling is the way to extrapolate. What I would pose to you is a question that you don't have to answer right now, but it's a question we have to think about, is can we do on the unconstrained modeling extrapolation or do we have to have some sort of benchmarks again of those models, and how big does that modeling database have to be?

When we look at the database we have, you can see that one data point as a benchmark could be either wildly optimistic or wildly pessimistic.

The tests have a natural scatter to them of some magnitude. So what's the magnitude of database that you have to have to benchmark your models, if indeed you think you have to have a database to benchmark your models? And I would be stunned if you didn't think that, but I'm always willing to be stunned.

DR. RANSOM: Well, I think the problem also would be similar if you simply tried to take the empirical approach. You've got to prove that you have enough data to evaluate the uncertainty associated with any prediction you made from that.

CHAIRMAN POWERS: I guess the point I'm trying to make is that the two are not different in the magnitude of the data.

DR. RANSOM: True, but I guess from my own personal point of view, I tend to -- if you have a model, you know, that involves the phenomena that you pretty much know are present and does explain the trends of the data, I would tend to trust that more than simply an empirical model.

CHAIRMAN POWERS: We used to have a model, right up until REP-Na1 was done.

Okay. On that note, I guess we'll recess and resume again tomorrow at 8:30. So we are recessed.

(Whereupon, at 5:18 p.m., the Subcommittee meet was adjourned, to reconvene at 8:30 a.m., Tuesday, September 30, 2003.)