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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
(ACRS)
SUBCOMMITTEE ON REACTOR FUELS

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THURSDAY,
JULY 28, 2005

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS PRESENT:

DANA A. POWERS, Chairman
RICHARD A. DENNING, Member
THOMAS S. KRESS, Member
WILLIAM J. SHACK, Member

ACRS/ACNW STAFF PRESENT:

RALPH CARUSO, Designated Federal Official

PANELISTS:

DAVID MITCHELL, Westinghouse
ROBERT MONTGOMERY, EPRI
ROSA YANG, EPRI

NRC STAFF PRESENT:

FAROUK ELTAWILA, RES/DSARE
RALPH MEYER, RES
HAROLD SCOTT

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P R O C E E D I N G S

(8:30 a.m.)

CHAIRMAN POWERS: Let's come into session.

This is the second day of the meeting of the ACRS' Reactor Fuels Subcommittee. Today we're going to focus on the issues of RIA, sometimes known as reactivity initiated accidents and sometimes known as reactivity insertion accidents.

Do any of the members have comments they want to make in the opening? Dr. Shack is in full voice today. Dr. Kress is with us and Professor Denning.

I'm Dana Powers, Chairman of this subcommittee, and since we have no opening comments, I will turn to Dr. Meyer to start us out on this subject.

Pat, did you want to say anything?

PARTICIPANT: No.

MR. SCOTT: Okay. And while Ralph is getting ready, let me say that we prefer reactivity initiated accident.

CHAIRMAN POWERS: Oh, I know that. You will opine that you get a better hearing if you state your name for the record.

MR. SCOTT: Harold Scott from research

staff.

CHAIRMAN POWERS: Thank you, Harold.

So you don't want to parse those words too carefully because it's hard to believe that the reactivity actually initiated the accident.

(Laughter.)

DR. MEYER: Well, it was in late 1993 and early 1994 when the tests were run in France and Japan that showed cladding failure accompanied by some fuel dispersal at energies well below the 280 calorie per gram value we've been using for many years. At that time, our pulse reactors in the U.S. that we had been using for this work had been shut down for ten years, and so we were dependent on others for data.

Altogether we've accumulated data from the Cabri reactor in France, the NSRR reactor in Japan, two reactors in programs run by the Russians, the IGR reactor and the BIGR reactor, and also we have included earlier data taken in the U.S. at the SPERT reactor and the PBF reactor.

So I want to express my appreciation to IRSN in France, to JAERI in Japan, the Kurchatov Institute in Russia who cooperated with us and made their data available to us, and also to an earlier generation of researchers at the Idaho National

Laboratory for work we're still using.

As with local work we discussed yesterday, we don't have nearly as many data points as you would like, and we don't understand everything, but a picture has emerged here as well, and the method we've used to analyze the data is one that I developed several years ago during the expert panel discussions we referred to as PIRTS. That's PIRTS.

I outlined this method for you at our last meeting in September of 2003, and I'll go over it in more detail today. Using this method we've interpreted the RIA data independently, and as before there are differences of opinions.

I want to describe our methods and our conclusions for you now, and I'm sure we'll discuss some of these different opinions before the day is over.

So just to summarize what we're doing, we have data from six test reactors. I mention them all by name. In each case there is some atypicality about the test condition because we're not able to simulate the conditions in a power reactor accident.

These atypicalities have introduced some biases, some of which we recognize, some of which maybe we don't recognize, but we've made an attempt to estimate the magnitude of the biases using analysis, and then to adjust the data with those estimates, identify a failure threshold and using that failure threshold, then to go off and look at the energy deposit that is likely in an accident of this type in a power reactor and compare it with that failure threshold.

After we do this, we find that it's very unlikely that there would be enough energy deposited in this accident to fail the cladding, and all of the conclusions that we hope to reach follow from that, and this is the study that I hope to tell this morning.

DR. KRESS: Is there a probability associated with that that it's not likely?

DR. MEYER: No, there's not. We have not done any frequency estimates. I use this --

DR. KRESS: I was looking for a conditional problem, given the RIA. You know, to me it would be the overlap of the --

DR. MEYER: A long time ago we did some estimates of the probability of the accident. In our original program plan, Brookhaven helped us with estimates of the probability of the accidents, and that's been a very long time ago. What I recall clearly is that when we looked at LOCA, PWR rod ejection, and BWR rod drop, they were in that order of importance, and the BWR rod drop had such a low estimated frequency that we decided not to spend much time looking at the BWR rod drop accident, but to focus on the PWR rod ejection accident for the reactivity transient.

So we did go that kind of scoping work, and that's documented in a program plan that we wrote in the summer of 1998.

DR. SHACK: But Tom's question is whether the RIA is unlikely and therefore cladding failure is unlikely.

DR. MEYER: No.

DR. SHACK: Or if you have the RIA --

DR. MEYER: Given an RIA, cladding failure is very unlikely.

DR. KRESS: Actually what I was looking for was a distribution of enthalpy.

DR. MEYER: No.

DR. KRESS: And a distribution of a failure criteria, and the overlap of those two is a failure probability. That's actually what I was looking to see.

DR. MEYER: What we have is a paper in the literature that surveys a large number of rod worths in power reactors in the U.S., and from that distribution, if you take the highest rod worths that you find and compare them with the failure threshold, which is a lower bound, a threshold by nature is a lower bound. It doesn't reach the lower bound.

DR. KRESS: Okay.

DR. MEYER: So the two distributions don't appear --

DR. KRESS: In essence, that's pretty much what I was saying.

DR. MEYER: Yeah. That's not to say that all of the information or this paper encompasses everything in the world or that our threshold is 100 percent accurate, but they don't overlap.

DR. DENNING: Ralph, let me ask a slightly different question that gets a little closer to what Bill said, and that is these reactivity initiated accidents that you look at, are they all within the design basis envelope of the plant?

DR. MEYER: Yes.

DR. DENNING: They are?

DR. MEYER: Yes.

DR. DENNING: I think there's been very little PRA work that goes beyond this class of accidents just because their initiating frequencies are so low, but I'm not aware of work, but I'm curious as to whether there are accident that are outside of the design basis.

I mean, clearly there is at some lower level of probability, there's something that can rupture the clad, and I'm just kind of curious where that boundary is and whether people have really looked at these kind of really extraordinary accidents.

DR. MEYER: I can tell you exactly where the boundary is because we are looking at the design basis accident, and it's the ejection of a single control rod cluster. That's it. So it is the one that's analyzed in Chapter 15 of the safety analysis report.

CHAIRMAN POWERS: Ralph, this survey of the control rod worth or the design worth?

DR. MEYER: Say again?

CHAIRMAN POWERS: When you mentioned the paper in the literature --

DR. MEYER: Yes.

CHAIRMAN POWERS: -- survey the control rod worth, these are the design worths?

DR. MEYER: Can you help me with that, Harold?

I don't really know. David Diamond has done all of the neutron kinetics work for this. This work was done a year and a half ago, and I don't think David is here today, and I certainly can't answer a question like that. Maybe Harold can.

MR. SCOTT: Let's wait until this afternoon whenever Westinghouse talks. I think they're going to talk about this, but the paper itself was a bank worths, and so you know, a bank might have four, five, six control rod assemblies, and you'd have to assume they would either equal or whatever. So --

CHAIRMAN POWERS: The question is whether it's what their intention is what they actually got.

MR. SCOTT: I think this paper was measurements. So these weren't just --

CHAIRMAN POWERS: Okay.

MR. SCOTT: As Ralph Diamond says, there's not a lot of uncertainty anymore in the calculations. What might be uncertain is, you know, for a brand new high burn-up core with different axial power distributions than you might have expected before what it would look like, but let's wait until later, and I think you'll get a good picture of this.

DR. MEYER: Okay. So just to start from the beginning, the rod drop accident from the cladding's point of view is quite different from the loss of coolant accident. You have a very large power pulse that for an order of magnitude, 100 calorie per gram pulse that we would be interested in, the peak linear heat rate might be 5,000 times full power. It's very, very high, but very short period, a few milliseconds on the width of the pulse.

The cladding temperature is fairly cold during the time that the power is high, and it heats up later on. Consequently, if you have cladding with low ductility, you can get pellet cladding mechanical interaction, PCMI failures, at low cladding temperatures, and if the ductility is high and there's enough plastic deformation available to accommodate the thermal expansion of the pellet, then if the energy is high enough, the cladding temperature may still be high enough to damage the cladding and even cause some oxidation that those temperatures that may end up looking something like the LOCA specimens.

We're going to be interested primarily in the PCMI failures that occur at low temperature because these are the ones that embody the high burn-up effect, again, through the corrosion process which puts hydrogen into the cladding.

This is just some wallpaper here to give you a visual image of what a rod might look like that has low ductility and has failed during one of these tests. This is a rod from the test reactor in Japan. The cladding is a long longitudinal split, and fuel was lost during the pulse.

Here is a picture. We may have seen this same picture yesterday in Mike's presentation. This is what --

CHAIRMAN POWERS: It's a utility picture.

DR. MEYER: Well, this is what the cladding looks like under the microscope as irradiated. This is before any transient, and what you notice, this is a piece of high burn-up fuel from which this cladding was taken, and you see an oxide layer, and then you see this dense hydride layer, and you see a lot of other hydrides throughout the cladding. The hydrides tend to be long, stringy things, and they line up circumferentially, and in this direction, just as a sort of rough image to have, think of rebar and concrete. They can help up to a point, but then when you get too many of them, they become brittle.

If they were to turn and line up radially, it would be really bad. The brittleness, the cladding would fracture along those very, very readily, but that doesn't normally happen.

CHAIRMAN POWERS: Well, what is the uniformity, say, of the cladding microstructure along the length? What I'm asking about is clearly you have some minor thermal discontinuities at locations caused by, first of all, ridge spacers.

DR. MEYER: Yes.

CHAIRMAN POWERS: Second of all caused by just the interface between two pellets, things like that.

DR. MEYER: Right.

CHAIRMAN POWERS: If we were to look at this microstructure in the vicinity of those things, how were they different.

DR. MEYER: Yeah, how was it different? First of all, from the bottom of the core to the top of the core in a PWR, the temperature increases. So you have higher corrosion at the top of the fuel rod than at the bottom of the fuel rod, and consequently you have more hydrogen and more hydrides.

So we will tend to choose specimens from the upper part of the core in order to capture the worst location.

Now, the next thing you would see is at the grid locations where you have a little cooler temperature, you'll get some hydrogen concentration there because the hydrogen likes to run ot the cooler temperatures.

We generally avoid the grid locations in our test specimens because you see the same kind of effect at the pellet interfaces. So you can go along in a very heavily corroded rod and at the pellet interfaces, you can see little spikes in the hydrogen concentration.

So you do have that degree of nonuniformity. The test specimens are eight to 15 inches long, many pellet lengths long. So you have those discontinuities within the test specimen. We generally choose test specimens so that the burn-up along the length is flat, and so what we're looking at are not strictly irradiation effects, but the corrosion effects.

CHAIRMAN POWERS: So none of our tests encompass this grid location, but they do look at multiple pellets.

DR. MEYER: Yeah, yeah.

Okay. If you look at the raw data, the picture is very confusing, and so this is like a strawman, and I'm going to knock him over, but what I've plotted here is the peak fuel enthalpy in every test, not the failure enthalpy but the peak fuel enthalpy of the test versus the burn-up of the fuel rod.

So if you were to think those were the principal variables, I would say that there's not a good choice. A much better choice is to look at the oxide thickness and the maximum fuel enthalpy change, where in this plot for all of the filled symbols which represent cladding failures during the test, the enthalpy change is the fuel enthalpy at the instant of failure minus the fuel enthalpy at the beginning of the test.

And so now you see a much more uniformed trend. I'll show you in particular the IGR and the BIGR data points which are here. Had very low corrosion, had five microns of corrosion on it, but they had a fairly high burn-up. So in the previous slide they were way out here.

And burn-up just isn't the big actor here. It's the corrosion related process. So the first thing we do is to replot the data this way, and you saw this when we were here 18 months ago.

Now, I don't want to dwell too much on this. I pulled this out of MacDonald's paper to identify the mechanisms. Pellet cladding, mechanical interaction is where the pellet expands thermally more rapidly than the cladding, and it pushes against the cladding, and then these other mechanisms. All of the mechanisms are present in the database, unlike yesterday where we tried to sort of put a fence around all of them.

I'm going to focus on the pellet cladding mechanical interaction and looked out there at high burn-up where you tend to have high corrosion, but I'm not going to ignore the others. They're going to be there, but I'm just going to focus on the PCMI failures.

Now, I mentioned before that the test conditions were not always correct and that we believe that this led to biases in some of the data. Here are some of the atypicalities. I've covered myself by saying others. I'm sure there are some others, but the testing temperature is not always correct.

The specific accident risk that we're talking about here is a hot, zero power control rod cluster ejection, and so we're looking for an initial test temperature of about 300 degrees Centigrade. Some of the testing is done at room temperature. This could have a big effect on the result. We all agree on that. We try and estimate that effect.

The pulse width is not always correct. I'm going to show you in just a minute the relation between pulse energy and pulse width. There is a natural relation to those two, and where the pulse width is not correct in the test we try and adjust for that.

The coolant type is not always correct. In terms of the cladding failure itself, not what happens after failure like a fuel-coolant interaction which would be very dependent on the coolant, this event is so fast that I think most of us have convinced ourselves that doing tests in sodium as has been done in the Cabri reactor is not a bad thing because if you're just looking at the cladding failure process itself, and so we don't do anything about that in our assessment of the data.

Coolant flow may not be that important for the fast transient. I mean, these are ten millisecond transients. So I don't know how much flow takes place in ten milliseconds, but again, we don't dismiss test programs that were run in stagnant capsules just because they didn't have flow.

So we're going to try and assess these. Here's the relation between pulse width and energy. There's been a lot of controversy about whether the tests in the Cabri reactor should have a narrow pulse width or a broad pulse width. To put that question aside for a moment, I don't think there has been any major controversy over the fact that as the pulse energy increases, the width of the pulse gets narrower.

There's an analytic expression, Nordheim-Fuchs equation, that in a closed form solution shows this same behavior, and these code calculations have been benchmarked with that.

CHAIRMAN POWERS: Do you have a citation for these calculations?

DR. MEYER: I think so. I'm sorry.

CHAIRMAN POWERS: Do you have a citation for these calculations that resulted in this plot?

MR. SCOTT: There's a reference in the paper, I believe, for this, a Diamond paper.

CHAIRMAN POWERS: Okay. You're going to have to tell me what the paper is.

MR. SCOTT: We'll get that for you.

DR. MEYER: Okay. Now let me try and outline the scaling method that we use. It's fairly simple. We use a code called FRAPTRAN. It's a transient version of the FRAPCON code that I think you're familiar with. We run several calculations in order to estimate the effect that we're looking for.

The first calculation that we run is a calculation for the test pulse, exactly as the test pulse was run. So we input the exact shape of the test pulse. It was double hump. We put in double hump. We put in all of the initial conditions that correspond to the test, and we run the calculation and we get some output, and we look at the output at the experimentally measured time of cladding failure.

Surprisingly, all of the experimenters are able to identify rather accurately at what time during the ten to 30 millisecond pulses the failure took place. So we will then go to that time in the output and note what the stress and permanent strain values are, and we will then call those the failure stress and failure strain.

These are calculated by this code. It doesn't mean that those numbers are absolute correct values because maybe the code isn't calculating everything perfectly, but we're going to turn around using exactly the same code, and I'll tell you what input changes we made and the rerun the calculation under PWR conditions and go in and find those what we call failure stress and failure strain values and see what fuel enthalpy they correspond to.

And then the difference between the fuel enthalpy in the second calculation and the fuel enthalpy in the first calculation is the adjustment.

Now, in the process of doing that, we recognize that the failure stress or failure strain values might be altered because of temperature differences between the test condition and the PWR accident condition. So we make an attempt to adjust those values which were deduced from the first calculation by a temperature effect, which we get from experimental data on mechanical properties of --

DR. SHACK: Is that really an "or" or an "and"? I mean, do you do the calculation looking at the stress and then you look at the strain and pick the minimum or maximum?

DR. MEYER: Yeah, it's an "or" or an "and." It's one of the two. I'm not sure, but what I tell you is I know exactly how we do it. For the test cases where the experimental observation was little or zero plastic strain, we'll use the stress because this is a matter of strength. It has failed somewhere in the elastic region.

Where there has been plastic strain, we'll use the strain. We use only one or the other, but I think in any calculation you could still identify that stress and strain and call it the failure stress or strain. So I didn't try and make that distinction too clearly.

Okay. I already said this.

So here is just a sort of textbook diagram of terms that we're using, and it's necessary now to talk a little bit about uniform elongation and total elongation.

Uniform elongation is the -- the way we will talk about it is the plastic strain that has occurred by the time that you start to get some nonuniform deformation usually in the form of necking. And that occurs up here at a stress we call the ultimate tensile strength, and then the total elongation is the actual elongation of the specimen at the time of failure.

Now we're going to argue that we're not going to make any temperature correction for the cladding temperature change during the transient for properties that depend on some sort of diffusional material flow because the transient is only ten milliseconds long, and you don't have time for any significant migration to take place.

So we're going to assume that the failure strains or the fracture toughness don't change because of temperature changes during the transient. However, we will look at the effect of the difference between conducting a test at room temperature when we're interested in an accident at 300 degrees C.

That's a huge change. There's ample time for things to readjust, and the properties would change. So that's the temperature effect that we're looking for. We're not going to chase around the little temperature changes that occur during the transient.

Now we have a couple of choices because the parameters that are reported in mechanical tests that would be of interest to us are these two plastic strain values, the uniform elongation or the total elongation.

So we've pursued both of those. We didn't make an immediate decision on which one we wanted to use, and in the temperature range from zero to about 300 degrees Centigrade, the temperature difference that we're interested in, when we look at uniform elongation data, we don't see a strong temperature dependence. So we say there's no temperature dependence for uniform elongation.

When we look at total elongation data, we do see a temperature dependence, and it's fairly strong.

So we ran all of our calculations both ways, but in doing it, we noted that the total elongation is really a not very well mannered property because it depends very strongly on the gauge length of the test that you're doing. Whereas the uniform elongation is more like a true materials property.

So right away we're kind of biased in favor of the uniform elongation. Our codes calculating uniform elongation, not total elongation. So that's another reason that we favored the uniform elongation.

When we ran the cases for total elongation, we got very large changes, and the changes were both in the -- well, no, I'm not going to say that, but the fact that the changes were very large seemed very undesirable because now your result is going to be dominated by your code calculation. So we have to wonder are we sure we want to use total elongation, which is really going to have such a huge influence.

Besides that, it took the results and it made them less consistent instead of more consistent, whereas using the uniform elongation temperature dependence made all of the data come into alignment that are in it. It just seemed there.

So in the end, we have stuck with the temperature dependence of the uniform elongation, which is nil or the temperature range of interest. So we made no change to the deduced failure strain in the first calculation in going to the second calculation, and we let the code take care of the elastic properties.

The elastic properties are going to respond instantly to temperature changes. They are related more to the atomic forces and not to diffusional properties.

So when we get down to the end, and I will point out to you which data set was affected most by our assumption of uniform elongation, and you can put a question mark around what we've done to that data set.

Now, the devil is in the details sometimes, and when we got into these calculations, we noticed that we were not able to reproduce some of the measured test data as well as we wanted to in order to go through with this scaling method, and so we had to make two changes to the code, one of which has become a permanent change in the code that has now been issued and published by Carl Beyer & Associates at

Battelle Northwest, and the other was the use of some unusual input values for the cold gap when we ran the case, and I'm going to talk about both of these things so that you can see what we did to the code.

And this was the starting point. We took all of the non-failure data from the Cabri, the French program Cabri, and the Japanese program, NSR, and in the cases that did not fail they measured strain. In all the cases that failed up until very recent times nobody was able to measure strain. We don't have any in the database where we have measured permanent strain values for the cases that had failures, but we do have all of that data for all of the cases in which failure did not occur.

And let's look first at the Cabri data points. I wish I had colored these, but they're the diamond shaped one, and they're clustering rather nicely around that freehand line I've drawn. There are two other points down here that I'm not allowed to draw on there because the data haven't been released yet, but they fall in line with that plot anyway.

Now, what does this mean? This is permanent strain, cladding hoop strain, but it's the permanent strain, what you measure with micrometers at the end of it.

So what this means is that you have to get something like 60 calories per gram into the fuel before it starts creating any permanent deformation on the cladding at all. During that time you are closing the cold gap, the gap between the pellet and the cladding, and you are going through the elastic region of the deformation. So only after you do that do you start giving some permanent strain to the cladding.

We've found that before we modified the thermal expansion algorithm that, in general, we couldn't get as much strain as was being measured in these data.

The other thing, I've got two subjects going here, and I'm going to try and introduce both of them and then come to them one at a time, but the other thing we noticed is a strangeness in the data. This is not a strangeness with anybody's code.

Now, look at the HBO series of tests in the Japanese reactor. That's these pluses. So here they are. They're clustering fairly nicely along this solid line. The Japanese tests were run from room temperature. They should have a larger cold gap than the French tests which were run at high temperature, but they appear to have a smaller cold gap. This is backwards from intuition.

Now, within the Japanese data sets there is some logic to HBO being lower than the other PWRs, being lower than the BWR rods, being lower than the ones that were irradiated in test reactors, all having to do with bigger gaps in BWRs, less creep down in BWRs, no creep down in the test reactors. So there are real fabrication or as irradiated gap differences there, but it's difficult to understand why the PWR strains are higher at lower energies in the cases with the colder gap.

Okay. The first thing I'm going to address is not that flip-flop of the Cabri and the NSR, but the general inability to get high enough strains. We looked at the thermal expansion algorithm, and the typical thermal expansion algorithm in anybody's code is like the one on the left, and it's this way because normally we're accustomed to some parabolic temperature distribution in the fuel pellet where the center temperature is higher than the temperature at the outside.

And so if you're modeling this up in a nodal scheme, the inner node is the hottest one. It expands a little more. The next node expands a little less, and the outer nodes expand a little less, and whether you model it explicitly or not, what you're assuming is that these ceramic pieces are cracking, and you just add up the delta Ds to get your total delta D.

That doesn't work for a zero power transient with high burn-up fuel where you have a lot of fissile material at the surface just from burn-up effects, and now what you have here is you have the outer rim being hotter than all of the other rims. So it tends to run away from the ones on the inside, and if you now simply add up the delta Ds, you get an answer that's too small. You need to take a delta D corresponding to one or several rings at the outside.

Now, certainly, you know, they don't have enough strength. If you want to get down to the mechanisms of this, you're going to need something more sophisticated than just taking the thermal expansion of the outer ring, but I think we took the thermal expansion of the second ring or something like that and put that in the code and immediately made a big improvement on matching -- getting in the right ballpark for the measured strains.

And you would still use this model if you had a temperature gradient like this. So it doesn't screw up the overall validation of the code.

Now, the next thing we did was probably

the most controversial, was we used input, cold gap sizes that modelers don't normally use, and we did that so that for the Cabri data, that we would start observing plastic deformation at about 60 calories per gram.

So we have manually adjusted the gap in order to fit the large body of data for unfailed rods, and then we're going to use that gap to analyze the rods with failures, and we did the same thing for the HBO cases.

And so we have a very large, 95 micron cap for the Cabri case. We use the same 95 microns for the PWR because it's the same temperature, and a small gap for this, and we have a hypothesis why this is necessary, and it has to do with -- I mean, it's related to preconditioning. We all know about preconditioning. If you're going to change power levels in a BWR, even in a PWR, you want to go up there gently before you start changing power rapidly so that you don't crack the fuel.

And it all has to do with letting the gaps in the pellet relax and relax the stresses on the cladding. I think I call it chips and fines. When these rods are prepared, they're taken in a hot cell. They're sawed and they're drilled, and they're pounded on, and if you'd just imagine what's in there, in all cases you're going to have loose pellet pieces because they're now cold. The gap is open, falling in the cracks, and in the Cabri or the PWR cases where you're going to take it up to 300 degrees and hold it there for a day or two, all of that is going to equilibrate, and you're going to reestablish your gap.

In the NSRR test, you just, bang, shoot the test right from that cold condition, and it acts as if the gap was nearly closed. That's a hypothesis. I'm not going any farther with it, but the manual adjustment of the gap allows us with our code to track rather accurately the plastic strain that develops in the measured cases, and then we use those for the calculations where they didn't measure the strain, and this is the bottom line result.

So we did this calculation for three of the Cabri tests. These are all three of the failures. There was a fourth failure, REP-Na1, and we and the U.S. industry disregard REP-Na1 as a flawed test. Preconditioning in our opinion has caused radial hydrides and some other problems.

CHAIRMAN POWERS: I believe we've discussed that test at length.

DR. MEYER: What?

CHAIRMAN POWERS: We've discussed that test in this committee at length.

DR. MEYER: Okay. So we tossed that one out. These are the three that remain with failures, and then we just did two of the Japanese tests, one of the HBO series and one of the TK series.

And what you see over here is a remarkably small effect from pulse width, and a fairly substantial effect from the test temperature. So the Cabri data points are slightly non-conservative because they were conducted with pulses that are too broad, and the Japanese test points are overly conservative by a substantial amount because they were run from room temperature instead of from a high temperature. This is also intuitively what you would expect.

So that's in your handout. You can read those data.

And here we plot up the result. These are all of the failure data where we now have taken all of the round dots, all of the NSRR failures and added 25 calories per gram to them. So those are 25 calories per gram higher up on the plot than they were originally.

The three Cabri data points, this one,

that one, and that one were moved down by exactly the amount that we calculated. It was slightly different in each case, just a couple of calories per gram out here, a few more.

CHAIRMAN POWERS: Why if you take the average of two Japanese 23 and 27 that you did each one of the Cabris individually?

DR. MEYER: Well, it's simply because we just were not able to analyze all of the Japanese points. There were too many of them, and we picked two that I don't know if I can identify HBO-1, but we picked two from different test series. It's just all that we had time to do, and they were close together. The adjustments were close together. So we just averaged them.

You don't want to look at any of this with too sharp a pencil point, but that's the reason that we did it. We would have --

CHAIRMAN POWERS: Well, I mean it seems like you do in some cases and you don't in others.

DR. MEYER: You know, as we started running the calculation, you could see that if we kept repeating these calculations we're going to get the same answers.

These were different enough that in the

test energies and pulse widths. I mean, all of the Japanese tests have pulses on the order of five milliseconds. It's barely a factor of two off of what we thought they should have.

Ten milliseconds is about right for a 100 calorie per gram pulse. This one was 30 milliseconds. This one was 75 or 80 and had a double hump. It was really weird, and this one was 40 milliseconds. This was a MOX test. So we had MOX properties put into that one. So that's --

DR. KRESS: You need to explain to me what you're plotting there.

DR. MEYER: What's the what?

DR. KRESS: What is this oxide thickness?

DR. MEYER: I didn't understand.

DR. KRESS: What is your oxide thickness?

DR. BILLONE: What's the corrosion thickness that you're plotting?

DR. MEYER: This is the thickness of the corrosion layer as irradiated.

DR. KRESS: So it's the measured value that you --

DR. MEYER: It's a measured value.

DR. KRESS: Before you even started the test.

DR. MEYER: That's correct. It's measured in each and every case, and it, in turn, is a measure of the amount of hydrogen that's inside. So if you had a hydrogen measurement, which we don't have, you would find that a ppm hydrogen up here, probably 800 to 1,000 ppm, and in here it might be 200 ppm.

DR. KRESS: So you would expect that oxide thickness just like your correlation --

DR. MEYER: Yes.

DR. KRESS: That's something like the Cathcart-Pawel time thing.

PARTICIPANT: Only it's real oxide thickness.

DR. MEYER: This is low temperature. This is not Cathcart-Pawel high temperature oxidation.

DR. BILLONE: He's using an analogy.

DR. MEYER: Yeah.

DR. KRESS: So you would expect that to have an effect after you change it.

DR. MEYER: Yeah, yeah.

DR. KRESS: Okay. Now I understand what you're talking about.

DR. DENNING: And the line is supposed to be your best estimate of a threshold?

DR. MEYER: The line is a freehand

threshold. Now, why didn't I fret more over the shape of the line? It's because I'm trying to see if we have a problem or not, and with this line, I can convince myself that the energy available in a power reactor is not enough to get up to that line, and so if I can't get up to the line, I don't have a problem, and I don't have to worry too much about --

CHAIRMAN POWERS: Well, presumably if you drew the line off the top of the paper then you'd be very safe.

(Laughter.)

DR. MEYER: I drew the line to bound all of the failure cases.

CHAIRMAN POWERS: But you didn't.

DR. MEYER: It's a threshold.

CHAIRMAN POWERS: You don't.

DR. MEYER: Huh?

CHAIRMAN POWERS: You don't bound all of the failure cases.

DR. BILLONE: REP-Na1.

DR. MEYER: That's REP-Na1. We agreed to cross it out.

CHAIRMAN POWERS: Okay. I just assumed you didn't even plot it.

DR. MEYER: Sorry. I should have either

left it off or labeled it.

CHAIRMAN POWERS: One or the other.

DR. MEYER: Okay.

CHAIRMAN POWERS: Because it detracts badly from the plot because the problem is your 110 is a failure. It could have failed when it was only 80 microns thick. Okay? I mean that's the problem you have when you don't label NEP-Na1.

DR. MEYER: Okay.

CHAIRMAN POWERS: You could come out here and argue that that's a block at the end.

DR. MEYER: Okay. Now, I have some more slides where I want to examine some of these points, but let's just dwell now on the Japanese points. The round points here are the ones that are affected by that assumption of uniform elongation versus total elongation.

Since all of the others here out here were run at the right test temperature, it's the Japanese point. So had I used uniform elongation, these points would slide way up here and up so high that you would then question whether they would fail by a mechanical interaction or not. If they were able to get up to that temperature without failing, then they probably had enough ductility to give during the PCMI phase and fail by some high temperature mechanism.

They were PCMI failures. I mean, you can look at them microscopically and you can tell that. So if that is the case, then you basically would throw these points away and say, you know, they failed by PCMI, but the temperature effect was so large that it effectively ruined them.

So if you want mentally to say, "I'm going to discard these points," I will argue with you that it's a bad choice, but I can't say with great confidence that I'd be right and you'd be wrong.

MR. SCOTT: I think you meant total elongation. If you assume total elongation, they would move up a lot.

DR. MEYER: Right. If you assume total elongation, if you assume the temperature dependence of experimentally measured total elongation data and applied that temperature dependence to the uniform elongation calculation you're doing in your code, then these points would jump way up and then the next thing you would conclude is that those points are of no value.

So just keep that in mind. I think that they're in a good position here.

I haven't quite finished the story before we start doing some pathological examination of data points, but here is David Diamond's results from a study on the maximum fuel enthalpy change. Those are exactly the units of interest to us. So this contractor did it right.

(Laughter.)

DR. MEYER: As a function of the ejected control rod worth, and now if you recall where that threshold line was in the middle of the range, it was at 80 calories per gram, and if you were to go out to 80 calories per gram and take the worst case, you would come down at 2.2 dollars. So you're going to see that number, 2.2 dollars, come up.

The lowest level out with the high oxidation and high hydrogen concentrations was at 55 calories per gram, and that would come in at 1.7 dollars.

This study was run on a single core. It's not universally applicable, but I did discuss this with David Diamond, and he said that he thought that plus or minus ten percent would probably cover other cores.

So you can take plus or minus ten percent on the control rod numbers and nobody would argue with you.

DR. DENNING: I don't understand the parameter. The data, is this beta factor? I mean, when it says end of cycle, 120 percent beta, what is that saying?

DR. MEYER: Beta is the delayed neutron fraction.

DR. DENNING: Yes, and what does 120 percent of it mean?

DR. MEYER: Harold, can you?

MR. SCOTT: That's the uncertainty. If you think beta is .006, then for those diamonds that he assumed it was .006 times 1.2 or times .7 for the 70 percent.

DR. DENNING: So that's, in effect, the beta effect in a sense. It's the modification to beta of whatever reality is, and then it effects what we mean by rod worth?

DR. MEYER: Please answer him, Harold.

MR. SCOTT: Sine we don't know the value of beta exactly and it can be different depending on exactly the burn-up or something else, he picked a range which he thought covered. So from .7 to 1.2 multiplier would seem to cover that particular parameter. As you can see, it is sensitive to that number.

DR. DENNING: I understand.

DR. MEYER: Okay. So if the control rod worth are less than approximately these amounts, then you wouldn't have enough energy to reach the cladding failure threshold.

David did look in the report. This was a report in Nuclear Technology in December of 2000 written by a group of Westinghouse authors. So you can find it if you need to.

So that was our conclusion. Now, I want to proceed now a little and talk about some of the other data points. We've talked a little bit about the NSRR data points and some uncertainties in the way that we treated those. There are others that have been questioned and --

DR. DENNING: Can we go back to your conclusions? I just wanted to go back to your conclusions, there. The fourth bullet, "without cladding till your energetic" --

DR. MEYER: Yeah.

DR. DENNING: Would you also say that there's a substantial margin there relative to this as well?

DR. MEYER: Oh, I think there is margin. I don't know how much margin there is.

Here is the difference between high burn-up fuel and fresh fuel in this regards. With fresh fuel, when you insert a big power increase. The phenomenon that can cause fuel dispersal, I mean, fuel getting outside of the cladding into the coolant, small pieces finely divided, coherently ejected in time so that you set up conditions for a fuel coolant interaction, it's molten fuel. Nothing else in our experience would do it except molten fuel.

That's not the case for high burn-up fuel. High burn-up fuel, you have all of this fission gas on the grain boundaries, all through the fuel pellets. So you have little gas bubbles, and if you heat it up high enough, it tends to blow the pellet apart. And in many of the test cases, fuel expulsion with associated power pulses were recorded.

Now, these power pulses were -- I'm not going to try and quantify them. From the experimenter's point of view, these were big power pulses. From a structure analyst's point of view these are not big power pulses.

DR. DENNING: When you said "power pulses" you meant pressure pulses.

DR. MEYER: I'm sorry. Pressure pulses, pressure pulses. Thank you.

I think it's clear without proof that there is some regime above cladding failure at which the energetics of fuel dispersal would be rather benign. I haven't made -- we haven't studied that and tried to discover where that is, and the reason is that from the outset we believed that we would reach this outcome that you couldn't even get to the cladding failure level.

The industry has told us for years and years in all of our discussions about test planning that the maximum enthalpy you're going to get in a power reactor in this event is about 40 calories per gram based on their work, and that looks to be about right.

Now, I have forgotten some of the slides that I put in, but here are some milestones.

CHAIRMAN POWERS: Let me go back to the previous. I'm dying to know why you've got a MIL spec on your planning document, but I mean, there's a qualitativeness about this.

DR. MEYER: Got a what on a?

CHAIRMAN POWERS: A qualitativeness about this. You tell me look at these rod worths. Say they're ten percent inaccurate.

DR. MEYER: Yeah.

CHAIRMAN POWERS: And I say okay. Fair enough. I have no idea how you came up with ten percent, but I'll take them as ten percent.

Then there's a real overlap between available rod worth and your critical rod worth that is such that you'd want to quantify that I would think.

I look a little bit to Dr. Denning because he knows all about these things, but I mean, if you tell me that you've got uncertainty bands of ten percent on these numbers, they overlap, and so the third conclusion, that it is not likely may be entirely accurate. It just depends on your definition of "likely."

DR. MEYER: So you're saying if we increase 1.5 by ten percent, we get 1.7.

DR. BILLONE: Or 1.65, which is approximately --

CHAIRMAN POWERS: And if I decrease 1.7 by ten percent I get 1.55. I mean, there's an overlap here that begs for quantification here.

DR. MEYER: Okay.

CHAIRMAN POWERS: To reach the third statement.

DR. MEYER: Yeah. Well, here's where also from the beginning of this we have taken the point of view that it's not necessary in analyzing this event to put two sharp a point on the pencil because it is a very low probability accident to begin with.

We're talking now about the threshold for cladding failure, and we all agree that there is some margin above that. We haven't quantified it, but I think you stack all of these things together and can reasonably come to the conclusion that this is a good enough estimate and that we should put our effort on some other problems.

CHAIRMAN POWERS: I mean that's an accurate statement of your beliefs. I mean, I assume. It's not a defense of the conclusion. It's just a statement of belief.

DR. MEYER: Okay. But I mean, what I've told you is, I think, an accurate description of the way we've approached this. When we come to these areas of uncertainty, we haven't pursued them because we just didn't think it was important.

Now, the industry is going to tell you that what I have up here is hopelessly conservative, and I want to disabuse you of that view if I can get to my next slides.

CHAIRMAN POWERS: Press on, and you can

tell me why you have a MIL spec on your slides.

PARTICIPANT: It's ML, not MIL.

DR. MEYER: Yeah. I happen to know what a MIL spec is, but that's just a coincidence.

These numbers are the Adams succession numbers where you can find the documents if you like to search for them that way. So we documented this work in March of last year, and NRR has made some use of this in their review of the entry topical report, and they issued a letter in March of this year.

One of the test series that I referenced is not well documented at the present time. There's only one paper in an obscure OECD conference that was held in France a few years ago, but we are in the process of getting this documented in a NUREG international agreement report, and that will be done in the next year, and eventually, we plan to revise Regulatory Guide 1.77, which has the limit values for fuel enthalpy in them, but we don't have this on a fixed schedule at this time, and we're kind of waiting to see how this debate plays out between us and the industry on the technical issues.

So I didn't plan to go into any more detail on this.

Okay. Now, I wanted to look at several

data points or groups of data points that have been questioned. One is the REP-Na7 test, which was conducted with a MOX fuel rod instead of the UO_2 fuel rod. One was an old SPERT test, and then there's a two REP-Na8 and REP-Na10 test from Cabri that resulted in cladding failure.

I want to start by talking about the MOX text first. Let me consult my notes.

There have been --

DR. SHACK: You mentioned with the use total elongation those Japanese tests.

DR. MEYER: Yes.

DR. SHACK: Does it move up when you use strain energy density?

DR. MEYER: I've got to think about this a minute. You would have, I think, exactly the same situation because the strain energy to failure you would assume is temperature dependent. You'll have to relate that to something, and so would you take the strain energy for uniform elongation, you know, integrate the curve out to uniform elongation or would you integrate it out to total elongation?

I think in the end you come to exactly the same dilemma. We've thought about -- in fact, our code calculates this strain energy density if we wanted to, and we've done some calculations like that, but I prefer to do it the way we did because we can separate the cases that fail in the elastic region from the ones with plastic strain and treat them differently.

With strain energy density, you just multiply stress and strain together and you smear that together. We just prefer to do it the other way.

DR. DENNING: Ralph, I'm missing something again, and that is why is there so much emphasis on -- there are all examples of where there was failure.

DR. MEYER: Yes.

DR. DENNING: Yeah?

DR. MEYER: Yes.

DR. DENNING: Why is there so much emphasis on the cases where there's failure as opposed to the cases where there's non-failure?

And I was, you know, thinking about your limiting curve there. If you're going to have confidence in the limiting curve, don't you really want to look heavily at the cases of non -- I mean, clearly they have failed cases below that, but if I looked at your non-failure cases, would they give me confidence that, indeed, that you've defined that threshold boundary well?

DR. MEYER: First of all, I don't know how to do the scaling adjustment for a case that doesn't fail. Maybe I could. We didn't.

The only other thing that I think of that might be helpful to say is that certainly if you look at the non-failure cases along with the failure cases you get a real estimate of a large uncertainty in this whole business, and it is large.

So by taking a bound on the failure cases, I think we've somehow tried to bound that uncertainty, but you know, if you run the same test twice, you're not going to get the same answer exactly.

DR. DENNING: So if you did plot to the extent that you could, your non-failure cases, they would well overlap. They would fill in well up to that curve, but they'd also well overlap.

DR. MEYER: Well, if we go back to slide number three, and I don't know how to adjust the open symbols, but you would take all of the black round symbols and move them up 25 calories per gram, and the others are not much different than where they're plotted, except for this Cabri MOX point. It's down about 20, and that's the picture that you're asking for.

And there remains a lot of non-failure

tests at fuel enthalpies higher than ones you had failure in, and I think that's a real measure of the scatter or uncertainty in the data, and I don't know how to handle that.

So we simply try and bound the failure cases.

Okay. MOX. There's been floating around for a number of years a hypothesis about a dynamic fission gas effect, and I wish I had put a slide in to illustrate this, but if you take the fission gas that's residing in these small gas bubbles on the grain boundaries, and if you release that inside of the cladding enclosure in the open spaces in the effective gap of the fuel rod, the gas pressure that occurs during the transient is very small, just a couple percent of the yield stress.

So it won't do anything. It won't make any significant contribution to the stress applied to the cladding by thermal expansion, but it's postulated that if you keep then gas in the little bubbles and allow them to act as wedges pushing grains apart because the pressure in the gas bubble goes at two gamma over r; you have extremely high pressures in the gas bubbles, but these things can contribute in a substantial way to the stress applied to the cladding during the transient.

Now, in EPRI's case, you'll hear today they do not apply that model to UO_2 fuel, but it is argued by some that it should be applied to MOX fuel, mixed oxide fuel, because there are some differences in the location of the gas and the microstructure of a MOX fuel pellet and a UO_2 pellet.

A UO_2 pellet would have more of the gas at the rim, and the MOX pellet would have more of the gas in the little plutonium clusters distributed throughout the pellet.

Okay. So the first thing I want to point out, and just look at this for entertainment while I'm talking, is that the gas bubbles in UO_2 are gas filled voids in UO_2. They're not like soap bubbles with thin surface membranes that can expand at will, and I don't think you can get bubble expansion at all or to any significant degree during a ten millisecond pulse when what you're requiring is for the UO_2 pore to swell.

So first of all, I think there's a logic problem here in trying to imagine that that gas can expand instantaneously and push on something.

CHAIRMAN POWERS: Let me understand a little better. The pressure inside a pore here is a function of the thermodynamic pressure and the surface tension pressure.

DR. MEYER: Yeah.

CHAIRMAN POWERS: That's your two gamma over r term.

DR. MEYER: Un-huh.

CHAIRMAN POWERS: When that heats up, that two gamma/r hardly changes at all. Surface temperature and energy is a little bit dependent on temperature, but not very dramatic. It's only the thermodynamic pressure that's going to go up in response in the increase in temperature, right?

DR. MEYER: I think I follow you.

CHAIRMAN POWERS: I mean, you agree that that's the term that's going to increase with temperature, right? It's just the PVORT's term.

DR. MEYER: It's the increase in temperature, yeah. That's all.

CHAIRMAN POWERS: And that's usually small compared to the surface energy term.

DR. MEYER: Yeah. I don't think it can change.

CHAIRMAN POWERS: Yeah, it changes. There's no question about it, but it's small compared to two gamma over r term, which is kind of fixed.

DR. MEYER: I'm a little slow. I'm not

sure I followed you, but I'm sure you're right.

CHAIRMAN POWERS: Well, I mean, if you do the equation that's stated, the gas in the pore, the pressure term that you put in is a thermodynamic pressure.

DR. MEYER: Yes.

CHAIRMAN POWERS: Plus the surface temperature.

DR. MEYER: Yes.

CHAIRMAN POWERS: Or surface energy. It's the two gamma over r term.

DR. MEYER: Yeah.

CHAIRMAN POWERS: The two gamma/r isn't dependent on temperature at all really.

DR. MEYER: Okay.

CHAIRMAN POWERS: I mean, it's flat. Whereas the thermodynamic pressure, I mean, it's vibration of molecules. They respond almost instantly to the pressure. That must go up, but it's small compared to -- I mean, you don't have a scale on that. Those irritating little voids you've got there are microns in size, submicron in size.

DR. MEYER: Submicron, yeah.

CHAIRMAN POWERS: I mean, typically those grains are what, 12 microns, something like that. So it looks like the little voids must be a couple of microns. The surface energy is on the order of what, 600 ergs?

DR. YANG: Of the temperature increases.

CHAIRMAN POWERS: Yeah, but it would have to be screaming.

MR. MONTGOMERY: This is Robert Montgomery from Anatech.

Yeah, you have a temperature effect, and it is fairly high. I don't know about screaming, but this is a pretty complicated process, and there's another factor that you have to keep in mind, which is as you notice, decorated along these grain boundaries you can see in this picture a number of grains, and you see a number of gas bubbles along the grain boundaries.

Grain boundary cohesion is one of the factors that plays a role in this as well, and the grain boundary strength or tension capability is much less than the surface tension effect of a pore within the grain itself. So you're looking at a number of different factors that come into play here, and it's not just simply the surface tension effect that will be restricting the growth of the bubbles.

DR. MEYER: If I can leave this point, I'd like to make a second point about the MOX fuel, and that is that because you now have more of the gas located in the plutonium clusters on the interior of the pellet, you're putting it into that part of the pellet that can't expand fast enough to keep up with the outer rim.

So you're putting it into a region of the pellet where it's going to be ineffective even if it could expand because it's the thermal expansion driving the hot outer rim that's going to control here.

So my conclusion is that this MOX effect, this dynamic gas effect doesn't exist. Sometimes we look at these series of tests to see if we can see any trends, and I don't think you see a trend. The first two entries here, REP-Na2 and 9, were at fairly high fuel enthalpies. These strains indicate that these have surpassed the PCMI range, and so what you're seeing here is the result of gas pressure and temperature which could be different.

These three are clearly PCMI, in the PCMI range, and if you simply go from the highest enthalpy to the next highest enthalpy to the next highest enthalpy, they're in the order of the strains going from the highest value to the next highest to the next highest. I don't think there's any MOX effect indicated by these data.

So my conclusion is I don't think there's a MOX effect. I think we're dealing with thermal expansion predominantly. The thermal expansion of mixed oxide fuel is virtually identical to the thermal expansion of UO_2 fuel, and I think that that's a good data point to keep in the database.

Now, there's also some questions about another data point that we use here. This is a SPERT data point where the oxide thickness is in question. This very old set of data, all of the information that you'd like is not recorded. Some years ago we had one of the original experimenters working with us, Mack McCardell, and his estimate from the early data was that this was somewhere around 70 microns of oxide thickness.

I believe there were probably some errors in his calculation, notwithstanding Carl Beyer and Harold, I think have looked at the irradiation history of this rod in the ATR reactor, run the FRATCON code and calculated the expected oxide thickness and found that it was not greater than this amount.

So there is some perhaps substantial uncertainty of what the oxide thickness is for that

SPERT data point, but it's just one of several data points, and I don't think it has too big an effect on our bottom line.

Now, I want to talk about the last two data points which there is some discussion about. The specimens for REP-Na10 and REP-Na8 -- I think I got them in the right order -- in the Cabri reactor had spalled locations on the oxide. Some of the oxide had spalled off during normal operation. It always flakes off during the test, but these had flaked off pieces of oxide during normal operation.

When this happens during normal operation, you get a little better cooling in the location where this insulating oxide has flaked off, and when you get a little better cooling in that location, the hydrides, which are accumulating, will tend to congregate in that spot, and you can end up with what we call hydride blisters in the cladding in the locations where the spalling took place.

And, indeed, these rods had some small hydride blisters in them. So it is argued that the hydride blisters would act as failure sites and cause early failure in rods that had spalled oxide compared to cases that might not have spalled oxide.

We have seen some rods out in this region with the same amount of corrosion on them that didn't have spalling. The Robinson rods have corrosion levels up to 100, and I don't know if there's any spalling, but there's certainly not much spalling on the Robinson rods.

So sometimes there's some spalling and sometimes there isn't some spalling. If you have this much oxide, you'll always have a lot of hydrogen.

In REP-Na10, the better of the two tests because of the pulse shape, there was only one cross-section taken for metallography. They attempted to take this at the location of the initiation of the through-wall crack. Once you initiate the crack, it can run pretty easily. So it's kind of important to figure out where it initiated.

Now, they couldn't estimate the location with that much precision. They do this from their on-line instruments, and they can pinpoint the location plus or minus four or five centimeters, and so they went right in the middle of that, right where they thought it was, and they did this radial cut, and they found two blisters. They're thin, and they're not associated with the through-wall crack.

Here is the through-wall crack, and you'll notice it's adjacent to a crack in the pellet. Now, the crack in the pellet looks huge, and that's an artifact, I think, because the test in sodium, before they get it out and cleaned up, the sodium etches away some of the fuel material. So I think this has been artificially enlarged, but there was a crack adjacent to that.

In the other specimen, in REP-Na8, and I don't have the pictures included, in REP-Na8 they did the same thing. They took a cut right where they thought the failure initiated, and they took a couple of cuts, one on each side of that.

In all cases, you see the through-wall crack lined up with the pellet crack in or near the expected location, and you don't see it -- we couldn't find any blisters in those cross-sections. I don't know whether that was because we weren't looking at the right cross-sections. You have to etch them in a certain way to make the blisters stand out.

So I don't think the blisters were associated with the initiation of the cracks in either of these test runs, and there's a little bit more information that supports that view, and this is work at Penn State where they did a study on blisters in zirconium sheet. They happened to use sheet instead of tubing, and compared that to the strain. They were looking at strain to failure for Zircaloy sheet material in which they had artificially put hydride blisters, and they compared that with some tubing cases where there was a uniform hydride rim without hydride blisters.

But the rim thickness, the rim is pretty thick and what you see here is for depth of either the hydride blister or the hydride rim; that the strain to failure is reduced pretty much the same as you increase either the depth of the rim or the blister.

And in fact, in their database they see the rim being a little more severe than the individual blisters, and they argue that point. They made a presentation a couple of years ago at NSRC on this, and this is in a recent paper that --

DR. DENNING: Those articles are above --

DR. MEYER: Yes, yes.

DR. DENNING: Throw away that for a second and just look at the rest of that curve and look at the trends of open circles versus closed circles. And if I were objective and I didn't know what's going on here, I would see no trend at all. Honestly, if I look at that out there, it's not at all clear to me that as oxide thickness goes that there really is a trend of where you get a failure, where you don't get a failure.

If I objectively look out here in the range of 80 or something like that and say, well, suppose I had run tests down at ten calories, would I have gotten failure? You know, if I didn't know a lot of other stuff, I can't look at that data and have confidence that there is a threshold that's at the place that you put the boundary line.

DR. MEYER: yeah.

DR. DENNING: Based upon that, I know whether other people from the committee have a similar comment, but I don't see a strong basis for where you draw the line based upon what I see.

I recognize there's been adjustments and stuff like that. They're kind of minor relative to the comment I'm making.

DR. MEYER: That's exactly why we went through this exercise, because we felt strongly that these black round circles were too low, because the tests were run at room temperature, and we're interested in an event that starts at 300 degrees Centigrade.

DR. DENNING: And I agree, and you raised them a bit.

DR. MEYER: Yeah.

DR. DENNING: But I'm saying look at here --

DR. MEYER: Maybe not enough. Those are the most uncertain. I mean, they dominate this whole part of the plot. These are the NSRR tests, and they're the most uncertain of all because for this PWR accident, they're all conducted at the wrong temperature, and the temperature variation may, in fact, just actually spoil those results.

I'm not adamant that these belong up only 25 calories per gram. I'm simply reluctant to push them up any higher because I might be wrong.

DR. DENNING: But isn't what's really controlling out here as we get the higher oxide thicknesses? I mean, from your curve certainly the lowest parts of that curve are out here in high oxide thicknesses.

DR. MEYER: Yeah.

DR. DENNING: And you drew some comfort that there's margin there relative to 40 calories per gram in your conclusions, yes?

DR. MEYER: Well, it's a little tight. I agree, but there's some margin.

DR. DENNING: The test looks data-sparse to me to be able to draw the conclusion. That's my feeling.

DR. MEYER: I'm sorry, Rick?

DR. DENNING: It just doesn't look like there's enough data out there to draw much of a conclusion.

DR. MEYER: What can I say? It would be wonderful if these programs were searching for specimens with high corrosion limits in order to get data out there. That's where the dearth of data exists.

Now, let me comment about this group. These failures here are almost all high temperature failures, and the ration test in IGR and BIGR, in my opinion, were beautiful confirmation of this old 170 calorie per gram value that we use for the high temperature failures, but if you subtract off 18 calories and you look at these, in IGR they had a pulse width of 700 milliseconds. In BIGR they had a pulse worth of three milliseconds, not 300, three, and it got exactly the same answer. The failure level was just about 155, 160 calories per gram.

And so what I feel confident in is that when you start the transition from high temperature into PCMI, you start it from about 155 calories per gram. I'm also fairly confident in that data point, that data point, and that data point.

So I'm reasonably certain that we're dealing with something like this. I'm not so confident in the Japanese data points and the old SPERT data point. While the complaints have been in the uncertainty in the oxide thickness, I think I will help the opposition here. You should complain about that was run at room temperature, and we didn't adjust it. So it probably should get pushed up.

The data recorded were simply not sufficient to adjust that point, and we left it right there.

There is some sentimental attachment to that figure. For decades we ignored that data point. We've known about this data point since the '70s, and we ignored it because we thought it was an outlier, that it was waterlogged, and we subsequently found that that is not true, and the people that ran that test were still living and breathing when we became reinterested in this, went back and looked at everything they could.

It's a good data point, but it was taken at room temperature.

DR. DENNING: I've known the truth of that particular -- not the truth, but the non-truth of that so long that, you know, it's buried into your unconsciousness.

DR. MEYER: But I'm reluctant to throw it out again. We threw it out once, and that was a mistake.

(Laughter.)

CHAIRMAN POWERS: Any other questions for the speaker?

Well, I bet we get a chance to come back to this when done.

In that case we'll take a break until 25 after the hour.

(Whereupon, the foregoing matter went off the record at 10:09 a.m. and went back on the record at 10:27 a.m.)

CHAIRMAN POWERS: Let's come back into session.

Let's see. Our agenda calls for some opening comments from Rosa Yang, but I want to just touch on a little bit of committee business here.

As the members of the subcommittee are aware, the is an 800 pound gorilla facing us, which is the MOX facility scheduled to come down. It looks to me like we can take that off the immediate agenda and delay our planning on that for at least a year. So that's a little relief for us.

Let's continue our discussions of the reactivity insertion or initiated accident, as you will, and hear from you, Rosa.

DR. YANG: Okay. Can you hear me without the microphone?

CHAIRMAN POWERS: It's not us hearing you.

DR. KRESS: It's not us.

DR. YANG: All right.

CHAIRMAN POWERS: Just sit down and pull one of those microphones towards you.

DR. YANG: Okay. It's just I'm not properly dressed.

PARTICIPANT: No necktie.

DR. YANG: Yeah, I'm sorry about that.

CHAIRMAN POWERS: Yeah, it's a little sexism on the part of the committee.

DR. YANG: Just everywhere.

Well, good morning, everybody. My name is Rosa Yang from EPRI.

The industry presentation today will be three parts. I'll give an overview which I'll mostly focus on what we have done, you know, in this area, and then Robbie Montgomery will get into the technical details of what we have done and our response to what

Ralph just presented.

And then Westinghouse will give a perspective about how this issue can affect the industry. Then we will wrap up.

Let me just start before the presentation to say I think Ralph gave a good presentation, set the stage for most of our talk, and I think from his presentation you can see the industry, not just the U.S. industry, but this is something internationally, and there is a pretty good understanding of the mechanism.

The plot that he has presented, there are over 100 data points, and each of the data, depending upon where it is done, cost on the order of tens of millions of dollars from running the test to the end.

And Ralph has made a proposal in the RIL, and I think just to kind of preface on what I'm going to say is we think the approach is very conservative and the failure limit extremely low, and as you heard earlier, he agrees that the methodology is fairly crude, and that the adjustment could be more.

But most importantly, I guess what I'm going to focus on is the collapse of the coolability limit to the failure limit, in fact, for an accident that all of us agree that will not happen, you know.

We talked a lot about it, that the overlap between what the failure limit would be and what the real world would be, there is, I guess, a word Ralph used, "comfortable gap."

But as Mr. Chairman pointed out, there's an overlap. It's really not comfortable. There is an overlap of what was proposed in the RIL between 1.5 dollars and 1.7 dollars. We're awfully close. To implementing that, it's going to cost the industry a lot of resources in terms of core design, in terms of methodology.

And what I would like to point out to you today and with the three presentations that are to follow is there is tremendous work that has been done. It's unlike LOCA. This issue started in 1993, and from 1993 till now, we have spent a tremendous amount of resources, and there's very good understanding of the mechanism, as Ralph alluded to earlier, and there are pretty mature technology codes that have been developed and can be used to avoid a lot of the awkwardness that you have raised the questions about earlier.

You know, there's really no need to use an unbenchmarked code to adjust different data points using different criteria and different approaches.

You know, the whole data set as you will see later on in Robbie's presentation, the data set, as Professor Denning pointed out earlier, is more than just oxide. There are a lot of other important factors there, and there's good understanding of that.

So hopefully we can try to answer some of these question. So let me start. How do I start? Just click on it?

Okay. So the outline of my presentation would be I will give you a bit of the historical perspective of this issue and talk about the industry effort and approach, and also I'd like to share with you about what some of the other people, other regulators, other people are doing in terms of understanding the mechanism and how they use their understanding to either promulgate criteria or proposed criteria.

And then I want to just kind of summarize our major difficulties with the RIL.

As pointed out earlier, there was a test in late 1993 actually, REP-Na1 in 1993 and then HBO-1 followed shortly, that raised big concerns about this high burn-up fuel would fail at much lower enthalpy. This particular REP-Na1 as Ralph's chart already indicated failed at fairly low enthalpy and with some fuel dispersal.

I will just commenting very briefly on that later on.

As a result of that, it raised a lot of concern among the international community, and what happened in this country is NRR has assessed the situation to look at the safety significance, and their conclusion was summarized in a memo from the then EDO to the NRC Commissioners, and the conclusion of that was that there's no significant impact on public health and safety because of the low probability of the occurrence and, more importantly, because of high burn-up rods. There's just not enough reactivity in the high burn-up rods. So the reactivity input would be small on high burn-up rods.

And they also concluded that there's no concern for core coolability with the disbursal of solid fuel particles, which Ralph also agreed earlier on.

However, they do recognize that because -- remember these two data points are extremely low and there are problems with them later on, as we recognize, but at the time they did recognize because of failure, enthalpy could be lower for high burn-up fuel. Therefore, you know, there will be higher radiological consequences.

And the industry at that time has performed assessment of the impact of low enthalpy failure, and in the letter submitted by NEI in December of 1994, it shows the investigation at the time. Earlier there was a question about the probability. It's less than one times ten to the minus six per reactor year, and that particular number was for the PWR rod ejection accident.

And as Ralph also indicated earlier, for BWR rod drop, the probability is even lower. And even looking at that low enthalpy failure, the plant will be able to meet the off-site dose requirement as required in the 10 CFR Part 100 limit.

So, therefore, the industry confirmed there is no immediate safety concerns, pretty much like the NRC conclusion.

However, even with no immediate safety concern, the failure criteria were needed to be revised for high burn-up to reflect the experimental data that we have produced over this time frame, and also there's significant understanding we have gained since 1994, and I just want to share with you what we have done.

Around right after the REP-Na1 test,

obviously the international community is quite concerned about this, and the industry was asked by NRC to jointly sponsor a project which was proposed by IRSN in France. It was an international project, and a lot of the data that Ralph presented which he calls Cabri data came from this.

But this particular one was really to look at the conversion from that sodium loop to the water loop, and we have spent over $4 million since 2000, closer to $4 million, but that's really just a participation to that project. Later on you'll see we have spent a considerable amount of effort trying to understand this mechanism.

As a result of that participation, there were two tests that were run recently in the sodium loop, and they were at the highest burn-up achieved at 75,000 -- 75 gigawatt days per metric ton, one ZIRLO rod at corrosion level of 85 microns and MP of 15 micron, and both were ramped to about 90 calories per gram, and neither of the rods failed.

And, by the way, that's the maximum energy you can put in for that level of burn-up from that reactor.

So that's the experimental part of it, but as you have gathered earlier, this phenomenon is relatively complex. You know, you're shooting in a very short pulse and the fuel heat-up tremendously, and the rod may fail under certain conditions.

So we have analyzed all of these experimental data points. I would refer to them as RIA simulation tests because we have spent a considerable amount of effort, have obtained another type of data, which is really cladding mechanical property because there's really two prongs to this approach.

First, you need to understand the mechanism, and as many of you'll see later, and I think Ralph alluded to that, this is PCMI type of failures. So cladding ductility is the one that really determines if the cladding can hold the type of loading that was put on the cladding during the simulation test.

So we have performed a considerable amount of mechanical property test data, and by putting the two together into a model call FALCON, which is our approach, and the others have different codes. Ralph talked about FRAPCON and there's another industry code called SCANAIR, and we have combined that knowledge and have looked at a proposal, have put together a criteria that Robbie will talk about in extensive detail later on and submit it to NRC in April of 2002.

And right now we're still considering continuing this benchmarking effort with EDF to look at how robust the approaches are and how the code compares with each other.

I just talked about the effort in the U.S., but there are, you know, considerable efforts elsewhere. I think the two most famous places are France and Japan, and they are like together 100 data points simulation type of tests have been generated. Fourteen of the tests are from Cabri.

As you will see, both Ralph and us kind of favor the Cabri test more because they are well instrumented. They're detailed, characterized and maybe more importantly is that they are the closest to the PWR rod ejection accident condition.

The Japan tests are very, very conservative mainly because the temperature is so low and the pressure pulse is very narrow, is four milliseconds, is much narrower than any code would calculate, and of course, the lower the pressure pulse, the impact is more aggressive.

CHAIRMAN POWERS: Several comments have been made about these Japanese tests and their biases, and I wondered if I had a Japanese speaker here, would he be so critical of his own tests.

DR. YANG: I don't know I would use the word "critical." I think he would agree that it's a very, very demanding and conservative condition. What is really very, very interesting is the community works very close together. We have meetings quite frequently, and you have the Japanese and the French, the Americans, the Germans, the Spanish, and I'll share with you some of these data.

We meet frequently, and there's really a lot of communication. I think I should have mentioned this in the very beginning. I see LOCA, which we talked about yesterday, I see LOCA being the situation where we were maybe five, eight years ago for RIA. You know, RIA when it first happened, you can see, you know, the tests were done in late '93, and actually it was presented to everybody in April '94, and then everybody scrambled trying to find out the safety significance.

And we have done a lot of LOCA data, but if you look at what we presented yesterday, a lot of the data were hot off the press. In RIA, we've been at this so long I really would like to -- one key message I'd like to communicate to you is that there are extensive publications, and I will -- actually I was halfway making the list, you know, to show that a lot of the papers said sort of like summary of the understanding of the RIA test or the mechanism of the RIA failures.

So there's tremendous knowledge in this area, and this particular Japanese, Dr. Toyo Fuketa, whom we know very well, and I think he would agree that the Japanese tests are very, very conservative.

In fact, I'll show you even their criteria are not as conservative as what is proposed in the RIL.

CHAIRMAN POWERS: Yeah, I would like to see how their -- it would be of interest to see how they're interpreting their own tests.

DR. YANG: Yes. Actually, to give you a short summary, they don't. They basically don't interpret any data point at all. That's the Japanese approach.

They don't, but you'll see they have their way around it. So I'll talk about that in a minute.

As I said, there's tremendous work in the international community, and I think, you know, I'll be happy to provide the list to this committee, and the list is pretty long. And if you look at it, there is a good consensus about what the mechanisms are. I think Ralph would agree what the mechanisms are, and particularly, there are some really good summary papers from France and Japan about their particular interpretation of their experimental data, and they're pretty coherent and consistent.

You can't just plot it for burn-up, and you know, you can't plot it for oxide either because those are only part of the picture. The picture is that you have to look at the cladding ductility. You know, temperature, pulse width are very important, as Ralph pointed out, but cladding ductility are important.

High burn-up cladding, which started this whole thing about, gee, this is a high burn-up effect, high burn-up cladding are very robust under prototypical I probably should say rod ejection accident rather than RIA accident. You just have a hard time to fail them. Usually the reactor doesn't have enough energy to fail the rod as long as it is not spalled, and that's one key point I will try to illustrate in a minute -- not in a minute. Maybe in a few minutes.

And there's really no so-called high burn-up effect, and I think Ralph probably agreed to that, if I understand him. All of these gas bubbles you're talking about at high burn-up, they really don't produce additional loading as you go to high burn-up, and the reason, we probably mentioned this to you, is because that was one of the key debates within the industry.

As you go to high burn-up all of these gas bubbles, do they really produce more loading; therefore, you would fail lower? That's not it. What is really important is the cladding ductility.

And having said all of that, I think we all recognize I think the awkwardness of making these adjustments. So the best way is really to develop an analytical tool to fully understand the mechanism and then try to translate it, and there are these codes available to do that.

Some may be more benchmarked than others, but codes are available.

And a lot of these mechanisms were also discussed in this PIRT process that NRC conducted. They basically reached the same conclusion to say what the failure mechanisms are.

And all of these studies, experiments also confirm what NRC's early evaluation that there is no immediate safety concern. However, we all recognize that the failure criteria that is in the current regulation, which is 280 for coolability and 170 for failure, are probably not conservative enough looking at the data because, you know, we all talk about the oxidation, the hydrogen accumulation at higher burn-up. That definitely degraded the cladding ductility.

So some revisions are required, and I think what we're debating here is what is a proper revision.

I talked about earlier many -- because of all of this tremendous amounts of work, many of the regulatory agencies have promulgated new criteria, and particularly interesting is when the REP-Na1 tests were first reported. Switzerland and Sweden, they all immediately just look at the data and dropped their failure limit to very, very low, and it's so low it's really affecting core design and burn-up extension in those countries.

So they recognized the problem and they have very recently revised those earlier very conservative criteria, and I'll show you how they revised it to.

I don't think I'm going to dwell on this too much because Ralph already talked about that. There are really two different types of mechanisms, one at low burn-up level when your gap is not closed and the cladding have very good ductility.

And I'm sorry I used the word "burn-up" because it really should be something more related to ductility, but just for explanation purposes it's sort of a surrogate for that because as typical as you go to higher burn-up, the corrosion tends to be higher, but it's not a one-to-one and not a linear relationship.

So there are very different mechanisms, and I think Robbie is going to point out later on some of the corrections that were made in the RIL is by not properly addressing the two different mechanisms that are operating in the data, but because this is a high burn-up issue, so the rest of the study that we're going to focus on in Robbie's presentation is really going to be looking at pellet cladding/mechanical interaction. So this whole picture is when you heat up the fuel, which mostly on the rim of the pellet, and that provides a loading on the cladding. So how strong the cladding is and how bad this impact is is what determines the failure limit.

This picture, I just want to show you this is a paper from a recently presented at a couple of places by a Swedish organization, including the Swedish regulator, and this is the logo of the organization that presented the paper. I forgot the name. It's a Swedish firm, but they basically said exactly the same thing.

So I just want to use this picture to illustrate the point that although the data scatter quite a bit because of the test condition, but the fundamental understanding, there's very, very good consensus among the industry.

And I think this plot, Professor Denning, is somewhat different from what Ralph presented because this is just showing the burn-up. I think the plot that Ralph later on is transformed, some of the points to the left side for oxide purposes.

But I just want to use this plot to show you where the current regulations are and the need, therefore, because some of these data points are, indeed, at a lower level, and there is a need to make that adjustment.

But another point I just want to point out, you can see the data. Not just the failures are going down as you go to the right-hand side, but the non-failed point. You see, a lot of non-failed points are kind of trending downwards as well, and that is a result of the nature of the situation. Because when you go to high burn-up, there is just not enough reactivity for the reactor to move these points high enough to fail it, and that's actually the struggle that we face at Cabri.

We just couldn't put in enough energy to fail good cladding. If you have good cladding, it will be a challenge to bring it high enough to fail it,a nd that's kind of what we all talked about earlier, this non-overlap or overlap.

I think I talked about this enough and Ralph talked about it enough in that not all data are equal. The temperatures are different. The pulse widths are different. The cladding materials are different, but more importantly is all of these are simulations, and what you want to note is what is in the lightwater reactor.

And you can see the key parameters are closest for Cabri and very different for NSRR, and the tool is really needed. You can't just, you know, adjust things because you inevitably get into an awkward situation when you just make adjustment. Let me very --

CHAIRMAN POWERS: Yesterday we were shown data that demonstrated fairly persuasively that there was a change in measured ductility in clad as you went from room temperature just to 135 degrees Centigrade, a relatively modest temperature change.

And the question was posed: why is that? And the answer was basically God made it that way.

If that's the level of our understanding, how do you make a tool to compensate for this temperature effect?

DR. YANG: Well, first of all, the temperature difference here is considerably bigger. It's from room temperature to about 300 degrees C. So the temperature range difference, and I don't know that God makes that way is a good thing, but there are lots of experimental data to show the temperature effect. I mean, it is --

CHAIRMAN POWERS: So we have the empirical data that will allow us to account for the change in ductility that go from 25 to 300 degrees Centigrade?

DR. YANG: Well, we have data. You know, you always can want more data. I think what is amazing -- I'm just about to show you -- is the robustness of this approach. You know, we somewhat use something different. As long as you use a good benchmark code, that seems to give you the same answer.

So, you know, we do have --

CHAIRMAN POWERS: I mean, the one code

that I know will always give you the same answer is one that's very, very bad. I have just not seen these data that would show us what the measured ductility for a given kind of irradiated clad when the ductility measurement was made at various temperatures from room temperature up to 300 degrees C. I just haven't seen that.

DR. YANG: Okay.

CHAIRMAN POWERS: It would be nice.

DR. BILLONE: Dana, there are a lot more data relevant to this, relevant to LOCA.

DR. YANG: Relevant to this than LOCA, I think, yeah.

DR. BILLONE: I mean, they can be presented.

CHAIRMAN POWERS: You're telling me that there are no data between 25 and 135, but from 135 to 300 we're data rich.

DR. BILLONE: No, no, no. As irradiated material which is in the alpha phase, it has been studied quit a bit, and there's a lot of data, whereas post LOCA material has not been studied.

PARTICIPANT: Okay. That's where the problem is.

DR. BILLONE: That's where the difference is.

CHAIRMAN POWERS: I understand. Good. Thank you.

DR. YANG: Thank you.

Let me say a few words about REP-Na1. I know we have talked about it at this committee.

CHAIRMAN POWERS: What amazes me is that we spent a full subcommittee meeting deciding the REP-Na1 was an outlier and pretty much came away all agreeing with that, but you guys all put it on your plots. I mean, you love this point.

DR. YANG: Well, we are trying. No, no, no, we're trying to be honest. We plot everything.

CHAIRMAN POWERS: This brings tears to your eyes when you think of this point.

DR. YANG: Well, it really gets tears in your eyes because that's what prompted all of us to spend this tremendous amount of resources to address it. We would not have a Cabri water loop. We would not have a lot of these tests. We would not have spent millions of dollars to develop the code had it not in REP-Na1.

CHAIRMAN POWERS: As I understand it, the Japanese program was underway in parallel with the others.

DR. YANG: Yes.

CHAIRMAN POWERS: So maybe that's not a good prognostication. Instead of being REP-Na1, it might have been HBO-1.

DR. YANG: Yeah. Well, I don't want to tell you REP-Na1 is an outlier. That's my title. What I want to tell you is because IRSN, which is the organization that produced the data in lists on REP-Na1 is a valid data point.

As a result, we formed a task force to evaluate it, and this is like what, five years, six years after the test was done? Your colleague, Dr. Hee Chung, presented a paper saying, hey, this preconditioning, this very, very unique preconditioning of REP-Na1.

You see, the first time the Cabri --

CHAIRMAN POWERS: Oh, he was persuasive.

DR. YANG: Well, --

CHAIRMAN POWERS: The committee endorsed it. You guys are the ones that put it on the plot.

DR. YANG: Yes and no, yes and no. Well, you know, Ralph explained the preconditioning. Before we do any rim tests, we precondition it to make sure there's no artifact, no chips and fines, no shards or anything. So we precondition it.

And REP-Na1 was preconditioned at a much higher temperature than the rest of the test. IRSN recognized the deficiency of it. All of the layer tests were not preconditioned at that temperature.

CHAIRMAN POWERS: Now, the stumbling block in that discussion, as I recall it, was, oh, we go to elaborate lengths to select our specimen here so that it doesn't have all these defects in it, meaning that there must be those defects in fuel rods so that you avoid them.

Gosh, are we doing the tests on specimens that are predestined not to be susceptible to failure?

DR. YANG: No. This is a very unique test, and the reason I wanted to --

CHAIRMAN POWERS: I'm not asking about this test. I'm asking about all of the rest.

DR. YANG: I don't understand your question.

CHAIRMAN POWERS: Having sent many things to be cut in a hot cell and say, "Get me a sample out of this," I know that nobody sawing a specimen is going to send me back a specimen with a flaw in it unless I ask for it explicitly. Okay? They just avoid it. It's a pain in a neck to cut one with a flaw in it.

And what I'm asking is are we selecting these segments that we put into this test to be those that are best in nature and don't have these fines, don't have flaws in the clad and whatnot.

DR. YANG: No, no. I'm not sure what you mean by "flaw," but this --

CHAIRMAN POWERS: Well, I mean whatever you mean.

DR. YANG: Yeah. No, I mean, when you handle a fuel rod, you inevitably create some fragments, but that's why we need to precondition it, and we have ran this type of test many, many times. That's not the issue.

I think this particular test has many doubtful characteristics, and the reason I want to spend a few minutes on that is not so much to say it's an outlier, but we really learned a lot from this whole exercise because we have -- that's what I was getting at.

I think all of us are ready to just discard it, except IRSN, and because of Hee Chung's paper, and what we did is we convened a group of people, really industry experts, and the head of this group is Dr. Herman Rosenbaum. Some of you may know him. He's a very good metallurgist so that there was some questions about the metallographic evidence of this particular test. It's very, very unique, and there are some instrumentation things.

So we investigated both. I'm just going to focus on that because that's relevant to some of the real conclusions here.

I think what is really useful from this whole exercise is not so much to say, "Hey, let's discard the test," other than that itself is very useful.

If we uncover a tremendous amount of data that weren't even reported, weren't even available, they were just sitting around in the lab, you know. It was done and then it was not really properly recorded and published to the outside community.

So there's a tremendous amount of data recovered, and it gives us a lot of insight. In fact, Robbie is going to present some of the findings to show that some of the cracks that we think were in the laboratory were formed during the test, were really formed in the laboratory. There's ample good evidence to show you that sodium introduced those cracks.

So I'll let Robbie address that, but the most important finding that there are like six or seven experts looking at this whole thing was uncovering new data with new calculations because it's just wonderful, you know. We have Robbie and Herman on one side, Hee Chung, and then the IRSN on the other.

Neither side wants to really admit they're wrong, and we perform data; we investigated. But one thing that everybody agreed on -- this is the agreement among all the experts -- is that if you have large blisters, like the REP-Na1 and 8 and 10 and 1, of course, that were used in real to anchor this very low limit that was presented earlier, if you have large blisters, you're going to have low failure enthalpy. That's something all of the experts agree, and they published all of these findings.

If you have good, robust, low corrosion rods, you cannot fail the rod. And another very interesting thing is that they do agree with Hee Chung, although they don't agree with Hee Chung's whole analysis; they do agree with him that this very unique heating, pre-transient heating, you know, you would think it's isothermal. Therefore, there should be no stress on the cladding.

But if you sit down and calculate it, just because of thermal expansion of the fuel and the cladding are different, it does create a hoop stress on the cladding, and that hoop stress is going to redistribute and reorient the hydride that as you all remember showing their predominant amount of hydride in there.

However, what it really hinges on is how deep, how bad the blisters are in REP-Na1 that caused this very, very low enthalpy failure, and our calculations show that blister has to be like 80, 90 percent. The IRSN calculation showed it had to be greater than 70 percent.

And of all the REP-Na tests, this is the test we have the most amount of metallography, and we could not see blister anywhere near 70 percent.

So it's hard to really believe there is a blister that you don't detect. So the other authors feel that it's a suspicious test because in addition to the very large blisters, the preconditioning is the problem, and there also is some eddy current signals on these rods that were never investigated and different thing. I won't go into detail.

But two and a half years of work resulted in a very comprehensive report which is about -- I don't know -- three, four inches thick, and in that document all of the metallographies, calculations, investigation I think is a wonderful book if you want to know anything about RIA failure.

So that report, in addition to the report, the report will be published later this year. I would be happy to provide a copy to this committee, and there are papers presented.

But what is most interesting, as Mr. Chairman said, is nobody have used the data although there are lots of criteria being developed. Nobody have used the data, although for one reason or another, still plot it just to be honest, I guess, with all of the data ever produced.

CHAIRMAN POWERS: Just a nostalgia.

DR. YANG: Nostalgia.

CHAIRMAN POWERS: I still want to come back to this question, and I'm posing it to everybody in general, is what assurance do we have that segments selected for testing aren't preferentially being selected to be the segments most immune to failure during a reactivity insertion event. Just by the natural inclinations of a technician working in a hot cell, I believe he would select segments that are most immune to failure, I mean, to the extent that he could.

DR. YANG: Most immune or most prone to?

CHAIRMAN POWERS: Most immune.

DR. BILLONE: But the technician doesn't pick the segment. I mean the principal investigator and the program manager and the sponsors and all of the partners choose the locations.

DR. YANG: Yeah. In fact, Dana, we tend actually choose the most prone to because we tend to try to bound the situation. So we tend to choose the most corroded rods. This particular rod and the sibling of it, which is REP-Na8 and 10 that were used in the RIA, were spalled, then inserted into the reactor for the next cycle. So it severely spalled. It's more spalled than any lightwater reactor fuel would be.

So this is, indeed, a bounding situation or more than bounding.

You know, we continue to do the RIA test, although Cabri has kind of stopped, but if you look at the burn-up level and everything, it's way outside our operating experience.

CHAIRMAN POWERS: I mean, is there someplace where there's protocol for the selection of the rod for tests, say? The most numerous ones seem to be the Japanese test. It's written down that says -- I mean, I'm just not sure how you do it. You've got a rod here. You know, something about it. You certainly know the external aspects of it. You can roll it around and whatnot. I assume you can autoradiograph it or something like that. You know something about the fragmentation pattern inside.

DR. BILLONE: You can also use eddy current to determine the oxide, the corrosion thickness.

CHAIRMAN POWERS: Thickness and then --

DR. YANG: We always pick the highest corrosion, almost.

CHAIRMAN POWERS: And then it says, okay, from this rod we picked this. I mean I can go read this, and I can understand how it was picked?

DR. YANG: Yeah, I think so. I think so. I'd be happy to provide you something. You try to do the minimum number of tests to bound the most. So given that assumption, you tend to select the most limiting conditions so that you don't have to spend, you know, $20 million to run another test.

DR. MEYER: You should acknowledge, Rosa, however, that there is consideration given to whether or not the rods selected for testing are rods with typical, average or high corrosion levels because some of the rods that have been chosen for this test, although you might have selected the upper grid span that had the highest level of corrosion on that rod, the rods themselves had very low corrosion.

DR. YANG: Yes. That's just the nature of corrosion level.

DR. MEYER: And you could have selected rods with higher corrosion.

DR. YANG: Sorry? Yes, your point is that the corrosion are not even on the rod. So we --

DR. MEYER: No, that's not my point. My point is that some of the rods are selected because they are more typical rather than --

DR. YANG: There are those, and there's always, Mr. Chairman, there always is debate about do you get more data from failed rod or do you get more data from sound rod (phonetic), and there are different camps, and so you know.

CHAIRMAN POWERS: I think Dr. Denning has raised an interesting question that we look like crazy at the rods that have failed and send the ones that didn't fail off to archive, I guess.

DR. YANG: That characterize it, and so you learn a lot, and we use it to benchmark our codes. The sound rods are much more useful to benchmark your codes, to make sure you fully understand the mechanism.

CHAIRMAN POWERS: Yeah. It would just be interesting to see a table that says, okay, this segment was picked for this reason and whatnot.

DR. YANG: I don't know the restriction of the Cabri water loop project, but we have -- almost every meeting we've gone through the rationale do we pick this rod or that rod. If possible, like investigate, if possible, or provide you what some of the rationales we consider in choosing the rod for the test because the Cabri water loop is a $62 million program, and only 12 tests. So we select them carefully.

CHAIRMAN POWERS: Well, more tests because your cost per test was down with every additional test you did.

I'm sorry, Farouk. I'm spending your money for you.

DR. BILLONE: Dana, can I make an experimental point? Sometimes high burn-up rods that you get are rods that are atypically shifted around, reconstituted subassemblies, and there might be some selection there of what rods you're going to test that are more prototypic than if you had started with a single subassembly and burned it all the way to 62 gigawatt days per metric ton.

DR. YANG: Yeah.

DR. BILLONE: I know we've made that.

DR. YANG: I think just your --

CHAIRMAN POWERS: Yeah, I suspect if we look closely at your selection criteria that you're doing pretty much what the speaker has said, that you're trying to find the bounding rod, but your understanding of what is bounding keeps changing on you, and so there's probably not a consistency over time, and it's probably an evolution in time.

DR. YANG: Yeah. You know, another factor, Mr. Chairman, is availability and the willingness of that particular utility to let you take rods. So it's --

CHAIRMAN POWERS: Rather than to --

DR. YANG: -- a juggling act.

So okay. Now that we disregarded the REP-Na1, let me just quickly go through what other countries are doing in terms of the criteria. As I indicated earlier, the Swiss one earlier has a very, very conservative limit, and now they have since I think about a year ago or less than a year ago, they have promulgated this new limit which have separate failure and coolability limit.

As I indicated earlier, our biggest

problem with the RIA is the collapse of the two limits for such type of accident. So I'm trying to show which country have separate limits.

Switzerland have separate failure and coolability limits. In fact, they have pretty much adopted the limit that we have proposed to NRC for UO_2, and our report and our submittal to NRC only addressed UO_2 since we don't use MOX in the U.S.

In Switzerland, they have developed a lower limit for MOX.

In Germany they have separate failure and what they call rod fragmentation limit. The burn-up threshold is the function of burn-up and oxide thickness. I'm going to show you graphically what each of these limits are graphically, just going through them.

The French have a slightly different approach. In earlier days they have kind of an empirical what they call safety domain. They don't want to say as a criteria, but it's sort of an interim safety domain, and it's bounded by some experimental parameters like the cladding oxide's thickness, the enthalpy input, the pulse width, the cladding temperature. So it's totally based on experimental data developed in the late '90s.

However, they are taking, with a better understanding now, they are taking a more analytical approach, very similar to our approach, and they are ready to submit it to their regulators later this year, and I'm going to show you a preliminary sketch of that as well.

We talked about the very conservative approach in Japan earlier. What they did, they have two limits, too, and as I indicated earlier, they do not analyze their data. They don't do the kind of adjustment that Ralph discussed earlier, and they acknowledge it's very conservative because of the low temperature.

And they also analyze water logging, analyze pressure pulse, and Japan is the place where they do the most tests to look at what happened after fuel failure, look at the fuel dispersal, and they look at the fuel coolant interaction, and they try to analyze it.

And as a result of it, they have what they call rod fragmentation threshold, and it's based on limiting the fuel melting. They don't want fuel to melt because that's when you have the most energy between fuel and coolant interaction, and they actually use pretty much the same approach as what we have used, and they actually come out with slightly higher value than what we have proposed.

In Spain, they are looking at our report, and they are considering we don't know what they have done.

Sweden is the one, I think, I'd like to spend a little bit of time talking about because they looked at our approach. Then they said, "Hey, I want to see how robust that this approach is, how important it is." You have to use FALCON code to develop this approach. FALCON is our code.

So they have closely evaluated our approach for robustness and conservatism, and here's, again, I'm quoting their paper. Present study is the Swedish study. That's the black line, and the red curve is what Robbie is going to talk to you about later. That's what the U.S. industry's submittal to NRC.

The blue curve with dotted line is Battelle's approach. As you probably know, Battelle is a contractor to U.S. NRC. So it's their approach.

As you can see, these are the failure limits, and you can see they are pretty close to each other.

Let me just jump to this. This shows

there are different codes used in development of the failure criteria. In our case we used FALCON code and we used strain energy density, which I'm not going to spend much time because some of you are familiar with and Robbie is going to talk about it, but we use strain energy density.

In Sweden, they use FRAPCON code and this French code, and they look at the strain to failure as their yardstick for their analysis.

PNL is the NRC contractor. FRAPCON is the steady state code. That's why there are two codes listed, and they used strain to failure as their yardstick.

EDF in France using the French SCANAIR code, but they use strain energy density, and I'm just about to show you, you know, there's a tremendous amount of variation here in terms of code use, assumptions use, and the methodology used, and they all came to fairly similar answer, and of course, the NRC RIL is quite different from everything else.

And this is the chart. There are going to be two charts, this one for failure, next curve for coolability. I'm going to just graphically show where everybody is.

The EPRI proposed one is a dotted line,

and the Sweden one, which I show you some of the graphic with their logo, is this. I don't know how you call it. Brown one? Okay, and the Germany is orange one, and EDF is this magenta type of curve. And the RIL is this one, and you can see the Japanese one, which is the green curve, which is what we call room temperature, no adjustment whatsoever of the data. They directly just use the data as is.

And Professor Denning asked about where are the non-failed point. These are the non-failed point from Cabri. They are what we think are the most relevant ones. As you can see, they are conservatisms in almost all the other countries' approach, and obviously they're conservatism in RIL. I think too much conservatism.

CHAIRMAN POWERS: Now, I guess I've been sensitized over the last day and a half and certainly this morning that burn-up isn't the issue.

DR. YANG: Yes.

CHAIRMAN POWERS: It's something to do with ductility, perhaps reflected adequately by plotting it versus oxide thickness. If we replotted these data against oxide thickness, would the plot look the same?

DR. YANG: I think you would draw exactly the same conclusion. First, my apology for plotting as burn-up. That's not the crucial parameter. I said it, and I acknowledge, it, and the reason I plotted it this way is because that's the current licensing criteria, which is burn-up independent as a function of burn-up at 280 and 170. You know, 170 is right here.

See, the current criteria is like that, and what we all have done is recognizing the degradation of the ductility as you have more oxide on there. So burn-up is kind of a -- "surrogate" may be even too strong for oxide, but in Robbie's presentation, he's going to tie the two together for you very, very nicely because in most of our licensing approach we can tie the corrosion thickness to the burn-up. There is a one-to-one correlation on that, but not a linear one, but there is a correlation.

So I guess what I'm trying to show with this picture is almost everybody are pretty much together, and all of these curves are using codes to make the translation to the lightwater reactor condition meet these ones, and this one is what you heard earlier. This is direct data. Just draw a line underneath the low temperature data.

For coolability you can see even much

better agreement because everybody is trying to prevent fuel melting.

DR. MEYER: Excuse me a minute. Could I interrupt and could I ask the Chairman if we could go back to the previous slide for just a moment?

CHAIRMAN POWERS: Sure.

DR. MEYER: I just need to point out that these calculations of failure invariably involve a failure model, which in almost all of these cases, I believe in all of these cases, are crude empirical correlations that have the same degree of difficulty as fit into the data.

For example, you cite the PNL failure curve, which is work supported by NRC. The failure assumption in those calculations is two percent strain.

We see test data with failures in the elastic region. It's a very simplistic result that has been shown up there, and I just don't think it's a fair comparison to show all of these calculations.

We can all calculate stresses and strains pretty much the same.

CHAIRMAN POWERS: Speak for yourself, sir.

DR. MEYER: -- fail is a different matter.

DR. YANG: Well, what I want to say is we didn't show PNL data here because, you know, with all due respect we don't think FRAPTRAN is properly benchmarked. That's probably one of the key difficulties with the RIL. It's not properly benchmarked.

DR. MEYER: Well, it was the previous slide. You showed it, and you showed it right up --

DR. YANG: I know. Yeah, but in fact, that's the problem, using the same code that your own contractors come up with very different answers.

DR. MEYER: Okay. The EPRI model has a failure model in it, CSED. It's an empirically determined failure curve, and so, you know, it's there. It's just hidden behind all of this analysis. You come back to a set of data and empirical determinations about when you think failure happens, and there's nothing fundamentally different in dealing with those kind of test data and doing a calculation than dealing directly with the integral test data.

DR. YANG: Ralph, I think you give me a very good lead-in for my next slide if there are no other questions on this chart.

There are different approaches. We all know that not all data are the same. They need to be adjusted, and there are different ways of adjusting it, and I think there is a big difference between the different ones. If you remember earlier days, this is one of Ralph's favorite charts, and it was in high burn-up program planning.

CHAIRMAN POWERS: Well, he was cured of liking this chart the last time he appeared here. He hates this chart now.

DR. YANG: He now hates it?

CHAIRMAN POWERS: He never wants to see this chart again.

DR. YANG: I thought he was -- well, how can that be? He call it the paint brush approach, and I copied this from the high burn-up program plan. This is Ralph's chart.

And as time goes on, like everybody else, Ralph recognized so in the RIL, it adjusts effects, and we heard the detail in the adjustment. You adjust for temperature. You adjust for pulse width, but there is one thing that we all talk about is the cladding ductility, and that's the key parameter, and you need to adjust that one properly.

And that's one thing. That's one of the big problems that I think with the RIL, and the other problem -- I'm ahead of myself a bit -- is the adjustment of these things need to be made based on the physical process and experimental data, and that's what some of these approaches. We call it the mechanistic approach.

There are two key words here. One is mechanistic. Mechanistic can mean different things to different people. To me it means you need to look at the physical process that's operating in there.

Another is benchmark codes. You know, all of our nuclear design, the fuel design is based on codes, and the codes need to be benchmarked, and I hope with all of the time I'm trying to convince you is there is good understanding. There is good consensus among the industry, and some of the codes are benchmarked.

Not all of the codes are benchmarked. Some of the codes are well benchmarked, and there's documentation of those benchmarking effort, and you're going to hear about the FALCON benchmarking effort.

And if you recall this point I made, most of these benchmarkings use codes, like SCANAIR, which is the French code, and they're based on the on-line instrumentation, based on the post irradiation characterization.

So what is different here is you can't just look at the failure. You've got to look at the sound. You've got to look at what all of the data are telling you, before the test, during the test, and after the test, and your code ought to be able to explain all of the things coherently instead of sort of shifting gears as you explain different things.

So coming back to what I started with, our two key problems with the RIL is there's no basis to collapse a coolability limit to the failure threshold for an event that everybody recognized will not occur, and by doing that, you're severely limiting what the plant can do, what the fuel designers can do, and I'll get into that in a minute.

And then you collapse it to a failure. This failure threshold is overly conservative, and the reason it's overly conservative I think Ralph acknowledged. Maybe those points should be higher. If you properly address those key parameters, let me just expand on this point.

The first one is you collapse the coolability limit from 280 calories, which is the current limit, now is collapsed to 55 for high burn-up, a factor of five reduction. You're prohibiting cladding failures for a postulated accident, which is significantly deviating from the current planned licensing basis, and by doing that there's no demonstration of the benefit, of the safety benefit, and it is certainly not consistent with the risk of the event.

The two data points that Ralph used in developing the RIL, they were high burn-up data, and they failed. He used them because they failed. They failed, and the crack was so tight that they don't even know if they failed for a long time until they did the detailed examination.

So they lost the gas, but they certainly did not lose any of the solid particles. So to collapse to say -- to put the safety limit where failure is, it's not even consistent with the experimental condition, and now you talk about fuel dispersal. There are some tests that showed fuel dispersal, and there are even some pressure pulse reported, but you have to remember these are little capsules or loops, you know, and one of the famous dispersal cases is REP-Na1, and some of these are capsules.

So I'm going to let Robbie discuss the detail later, but the potential is very low, and in his presentation and in the Westinghouse presentation, you can see this whole thing is very local. It's limited to a very small number of assemblies, and we also talk about the corrosion thickness. It's only high at the upper span. So to think about that would generate a pressure pulse that would threaten the core is a stretch.

The fuel dispersal, if you collapse the true limit and you're not allowing any fuel dispersal, although limited fuel dispersal is within the safety boundary, you know, we have this separate limit of failure limit to calculate the radiological consequence, and there are limits for that.

So we strongly believe that as long as you don't have molten fuel you can address this issue by calculating the radiological consequence. If we adopted the RIL as is, as proposed, it would certainly be the most conservative limit worldwide. This is a picture that I have shown earlier. It will be certainly way different from the others.

And certainly I talked about that and consider the key mechanisms. One of the key ones is his use of spalled rods in developing the criteria, and the other is really, I think, it was mentioned earlier that, you know, for high burn-up rods if you look at any metallography, any metal codes, they say the gap is closed between the fuel and the cladding, and in the RIL development they have to use a gap size which is slightly bigger than the as fabricated gap size, when the gap really should be zero and, in fact, what is even more awkward is you use different assumptions for different gap size depending upon what set of data you need to look at.

And like I said, the same codes were used by a different organization and come up with quite different answers. I don't know the detail on why, but it may have to do with assumptions and the code.

So I just want to show a picture of the difference between spalled and non-spalled rod for people who are interested in material properties.

This picture was shown, I think, or something similar by Ralph earlier. This is the typical PWR rod with fairly high corrosion rim, but not spalled, and there is the oxide here. Well, when the oxide gets too thick, as you can see here, this is the oxide, and you're missing and it's a cold spot as shown by Ralph earlier, and you create this what we call blisters, which is a local defect or whatever, and this is a very important difference because the rod, that's where the failure initiation occurs.

You know, you have a very brittle failure right at the blister, if you can see the blister. It's kind of hard here, and then the rest of the cladding is still ductile.

We will address your point. Don't worry.

But so when you have a blister like this, quite often because the oxide is so thick, so it's not just a small LOCA situation. Actually sometimes it has expanded axially as well, and Robbie is going to talk about this data in greater detail.

We have pretty good characterization of spalled and non-spalled rod. This is just one example of it, which shows the ultimate tensile strength with the hydrogen content.

CHAIRMAN POWERS: Can we go back?

You showed us the REP-Na8 failure at a hydride blister location. Earlier Dr. Meyer showed us a REP-Na10 with two hydride blisters and a failure that didn't occur at a blister location. Am I to conclude that blisters are just totally no never mind?

DR. YANG: Well, certainly not no never mind. As I indicated earlier, there are just lots and lots of publications to quantify the blister effect. What Ralph showed earlier, and there are more pictures that Robbie can show earlier. Let me just give you a short answer.

What the crack that Ralph said at the blister location, that particular crack actually was a result of -- that crack was an extension of the original crack in the hot cell. As you remember, all of these REP-Na tests were done in the sodium loop.

So after the rod failed, they were stored in the hot cell, and there's sodium inside, and sodium and UO_2 interact, and we have, as a result of the REP-Na1 task force, we have found that the crack lines actually grew during the hot cell storage by a factor of ten.

CHAIRMAN POWERS: I mean, that's fine, but he went through a fairly elaborate explanation of saying, well, gee, they wanted to find out where the crack initiated, and they made their best effort, and I look at it, and it looks like it failed at some location in fairly pristine looking cladding when it had opportunity to fail at a blister.

DR. YANG: Yeah.

CHAIRMAN POWERS: Here you show me a crack obviously at a blister, but it doesn't seem to have much to do with a blister, but it's in the blister, and I'm trying to figure out what the point is.

DR. YANG: The point, I think, well, this certainly degrades the mechanical property. I guess the key difference I want to say is Ralph's picture was actually taken at a crack position that was the artificial grown in the hot cell rather than the initiation point.

What this is finding, that the laboratory, IRSN, has found during the REP-Na1 investigation -- nobody recognized that crack would grow so much. So, you know, the hot cell experiments were done at different times and actually they were able to go back an reconstruct what the initial crack must be like since we didn't look at it, and then they have grown.

They look at it for both REP-Na8 and REP-Na10, and they have since concluded that the picture in Ralph's presentation was the crack that he has shown was the crack that was grown in the hot cell.

Unfortunately that's the only metallography they have ever taken. That's just the nature of --

CHAIRMAN POWERS: I'm still struggling to understand.

MR. MONTGOMERY: Well, I'll try to show in my presentation, Dana --

DR. YANG: There are data.

MR. MONTGOMERY: -- a little bit more of a map of the hydride blisters in REP-Na10 and how the crack grew, but the bottom line is that you don't know where the crack initiated. What you see is how it propagated in the hot cell, and the propagation process is different in the hot cell than the crack initiation process during the test itself.

CHAIRMAN POWERS: Well, you're not going to have to work very hard to persuade me of that. What I can see if that there are two hydride blisters here that did not initiate cracks.

MR. MONTGOMERY: Right, right. And when we do --

DR. YANG: Not all of them initiated a crack obviously.

MR. MONTGOMERY: Right, and we do know that the bigger the hydride blister, the easier it is to initiate a crack.

CHAIRMAN POWERS: We know that?

MR. MONTGOMERY: Yeah, we know that.

CHAIRMAN POWERS: Why do I know that? I mean, why should I believe that? I don't know that.

MR. MONTGOMERY: Oh, we know that. I'm sorry.

DR. YANG: We have done lots of experiments.

MR. MONTGOMERY: I'll show some information on that.

DR. YANG: Yeah, we have done lots of

experiments.

CHAIRMAN POWERS: I mean, it looks to me -- I mean, I look at this and you say, well, this is not the right -- well, apparently it must have been initiated at a hydride blister. I don't know. I look at this now. Did this grow in the hot cell?

DR. YANG: No, not this one.

CHAIRMAN POWERS: This didn't grow in the hot cell.

MR. MONTGOMERY: It grew a little, but we know from the characteristics of this crack that this is not a crack that was grown in the hot cell, this particular crack at least at this location. It did get longer in the hot cell, but at this location it is not and we know that primarily by the shape of the crack.

You see that there's a -- I don't want to get into too much detail.

DR. YANG: Well, we can construct how the crack -- well, in fact, it was very fortunate. They took pictures at different times of the rod, and then they didn't realize some of the cross-sections were taken at a time that the crack didn't exist during the experiment, but there were very good data that actually you'll find it fascinating that shows on this day the crack is this much and then it grew progressively. Robbie has it.

We didn't realize that was what Robbie -- Ralph's was going to show. So we just put that. That was part of the study of the REP-Na1 task force.

MR. WAECKEL: May I make one comment, a short one?

CHAIRMAN POWERS: Please.

DR. YANG: Say your name.

CHAIRMAN POWERS: State your name and speak clearly.

MR. WAECKEL: Nicolas Waeckel from EDS.

I was participating to the test in Cabri, and what I would like to mention, first I would like to confirm exactly what was said about REP-Na10. The crack was very tight in the beginning in the test itself, and everything grew afterwards tremendously in the hot cell during the storage.

And when you look at the pictures, when the crack is always going through one blisters. So we don't know exactly why it was initiated, but we will have to mention to see CIP-01. CIP-01 is another test with no blisters at all, but how you had what counts in REP-Na10, you had something like 1,000 ppm of hydrogen, but no hydride blisters, and it didn't fail.

So if this is correct, CIP-01 would have failed because its REP-Na10 failed away from the blister. In a region where the hydride content is something like 650 or 700 ppm it would have failed.

DR. YANG: So you have a rod that have higher hydrogen concentration without blister and --

MR. WAECKEL: That didn't fail.

CHAIRMAN POWERS: And forgive me, but I look at your database, and I say the fact that a rod did not fail under one particular test means almost nothing to me. I think you have to do about five or six tests and show me that it consistently doesn't fail to be very persuasive because I look at your database and it's all over the map.

DR. YANG: Well, all over. Do you mean the simulation test?

CHAIRMAN POWERS: No, no. If I look at these, at your scatter plots, especially when you plot them against --

DR. YANG: Burn-up, yeah.

CHAIRMAN POWERS: -- well, just about anything, but if you plot them against burn-up --

DR. YANG: That's what I call simulation tests, yes, because --

CHAIRMAN POWERS: Plot against burn-up.

It seems to me that the definitive conclusion is that a rod failed. When a rod doesn't fail does not mean that if you ran the test over again that it would still not fail.

DR. YANG: Well, maybe I didn't explain well. I mean there are over 100 simulations of these type of tests, and there are lots of organizations have analyzed them, and I think the conclusion is PCMI is the failure mechanism, and if you properly account for the differences in the test, I agree with you they are scattered.

That's why you need a well benchmarked code, to differentiate the scatter.

CHAIRMAN POWERS: What you need is a way of being able to plot the data so that the failures are in one group and the non-failures are in another group definitively, and the closest I've seen to that Nirvana is this plot against oxide thickness.

I wonder if it wouldn't be better if it were plot against hydrogen content, but I don't know. I mean, it may suffer from the fact that you just don't have that data, but so far I have not seen anything better than that.

DR. YANG: You are quite right. Oxide is much better than burn-up, but as indicated earlier, there is still scatter there. That's what the code is trying to explain, the scatter, if you will.

You know, when you have an animal which is multi-dimensioned, when you plot it in two dimension, it will have certain scatter, but like you said, you want to separate failure from non-failure. So if you propose something, then you look at the data. You go, "Gee, are all of the failures above the data point and the non-fails are below?" And that's really the proof of the pudding, I guess.

CHAIRMAN POWERS: Okay, but so far I've seen no alternative that's better than plotting it against oxide thickness.

DR. YANG: Well, that's --

CHAIRMAN POWERS: Am I correct? I don't know.

DR. YANG: Well, --

CHAIRMAN POWERS: Okay. So we should be plotting against oxide --

DR. YANG: Mr. Chairman, I think maybe it's not so much just the plot is what I'm trying to get to. You really need a code to --

CHAIRMAN POWERS: If I have a code --

DR. YANG: -- to do it justice. A paint brush, a semi-adjustment, it's too crude. They're too crude to do what you want, I think.

CHAIRMAN POWERS: That's good. There should be some way to plot the data that says all that's on this side of it failed. All that's on this said has not failed or something like that.

DR. YANG: Okay. Maybe Robbie can --

MR. MONTGOMERY: I'll try to demonstrate.

DR. YANG: -- demonstrate that after me.

DR. SHACK: You have to agree on which failures you're going to include, right?

DR. YANG: No, no, I think that's pretty much agreed on here.

DR. SHACK: Well, I heard some disagreement here, but on the REP-Na8 and 10.

DR. YANG: Oh, I see. What you're include. Okay. There.

DR. DENNING: It's quite possible that there really is a stochastic element to this, that even if you had a very mechanistic code, and I think Ralph has pointed out that the codes that we've seen so far have some or are more empirical in nature or they have elements of empiricism rather than true fundamental modeling of the processes that are going on.

But if you're going to head down the --

from what I've seen for far, if you're going to head down a path towards mechanistic analysis -- I'm sorry -- code-based analysis, it looks to me like from what we've seen so far that there's a stochastic element that has to be taken into account.

DR. YANG: Well, probably you take into account by some conservatism in it because, you know, you try to be conservative when you're in doubt.

To summarize, I think we have spent a tremendous amount of effort. I would say we spent more effort as an industry on RIA than on LOCA. I think that, you know, maybe with a limited time we haven't done this justice, and hopefully Robbie can remedy that situation, but I think one way to remedy that is to provide some references to this committee on what has been published.

You know, there's really a pretty good understanding, I think, if you ask the experts within the industry. I think most would agree. I just show you a couple of examples of what other labs are doing. They are no means all of the examples. There are a lot more.

We think what we have proposed is fairly conservative, and it's pretty consistent with what other countries have done independently, and the key point is really the low probability event, and the consequence is very local and is very limited, to use a very low criteria and different one. You know, we are shifting from calorie per gram to dollar worth type of criteria where proposed. It is going to cost the industry significant resources to comply.

Right now there are no 3D methodology, and we need to develop the 3D methodology, get them licensed to show that, indeed, we can comply with this very low limit being proposed, and this very low limit will limit the core design flexibility and any of the new core designs that were developed.

We have submitted topical report, and we would like -- the industry would like to see NRR continue its review of the topical report.

Thank you.

CHAIRMAN POWERS: Okay. We've got a few minutes.

I have deliberately let the speaker go beyond the allotted time because I thought the information was important to the committee to hear, and we'll figure out how to amend tomorrow -- I mean this afternoon.

Are there any direct questions to the speaker?

DR. YANG: There might be because there are two following my presentation. Robbie is going to give the --

CHAIRMAN POWERS: We've got a lot to go yet.

DR. YANG: Yes.

CHAIRMAN POWERS: But I'm going to break for lunch here in a few minutes, and we'll pick up on that later.

With respect to the topical report, the last time we met there was some question on the way of parameterizing the model. You were using a least squares methodology. Did you ever sort that out?

DR. YANG: The strain energy density, you mean?

CHAIRMAN POWERS: Yeah.

DR. YANG: Yeah. We have sorted some of the stuff out.

CHAIRMAN POWERS: I don't remember the details, but I remember the controversy.

DR. YANG: Yeah. They are --

CHAIRMAN POWERS: It looked like you were fitting an outlier, is what it looked like. Now, you were certainly using linearally squares where both of your variables were uncertain. Things like that came up.

DR. YANG: Maybe that would be better addressed --

MR. MONTGOMERY: The answer to that question is that the data that I'll show you today, which was continuing to use the least growth fit method or best fit method, but we are evaluating and have looked at other ways to look at the data, either using lower bound to try to address some of these uncertainties.

CHAIRMAN POWERS: Okay. We'll see that later.

Any other questions on this apparent difference of opinion that we have here?

Ralph, you look like you want to make a comment other than you're hungry.

DR. MEYER: I'll make my comment later.

DR. ELTAWILA: No, because we are planning to meet with the industry on the review of that issue. So I think we would like the exchange to be between the committee and EPRI at this time.

CHAIRMAN POWERS: Okay. That's fine.

DR. MEYER: My comment had nothing do with that.

CHAIRMAN POWERS: That's fine.

DR. MEYER: It's very short. I just wanted to point out that REP-Na7 was a failure with no spalling.

DR. YANG: That's a MOX.

DR. MEYER: It's a MOX, right. We discussed that, but just so you don't forget that there was a test that failed in Cabri without spalling.

DR. YANG: That's a MOX. You wanted to say something?

CHAIRMAN POWERS: Please. We'll have you sit at the table there and it can be easy.

(Laughter.)

MR. WAECKEL: Nicolas Waeckel from EDF.

Just to follow up on Ralph's comment about the MOX fuel, he demonstrated this morning that MOX is equivalent to UO_2. Maybe he's right in terms of dynamic fission gas swelling effect, but you have to say one thing that is a fact. In the experimental on-line measurements, when you look at the REP-Na7, also other MOX fuel, they behave quite differently in terms of kinetics.

When you measure the displacement of the volume of sodium is in channel, it grows much faster for MOX fuel than for UO_2 fuel. So that means we don't know exactly what does it come from. It just comes from the thermal properties of the MOX fuel, which is maybe different from UO_2. When inject energy, the response is faster.

And something is going on, and the response of the MOX fuel is different from the UO_2 from the very beginning of the transient.

CHAIRMAN POWERS: We've suspected, and certainly the VEROCRS tests confirm, that we get a little faster and earlier fission gas release and fission vapor release from the MOX fuel. I mean those are certainly --

MR. WAECKEL: Yeah, we have noted that with everything, but we do know that the MOX is a very different animal from UO_2. So to plot --

DR. YANG: To mention the manufacturing, they have these clusters of high concentration.

MR. WAECKEL: Yeah, we have many classes. The gas is distributed in different locations, and it's very dependent on the size of the classes and all of the gas is around these classes. So it's a very different animal, and I don't think it's fair to plot under the same plot MOX and UO_2 fuels.

DR. YANG: Because when MOX are made, they have these little MOX clusters that have very high burn-up within the cluster.

CHAIRMAN POWERS: It depends a little bit on who's making it because certainly --

DR. YANG: Yes.

CHAIRMAN POWERS: -- certainly the modern MOXes have rather little --

DR. YANG: Yes, you're quite right.

CHAIRMAN POWERS: -- concentration. They do a much better job in homogenizing the plutonium. But like I say, we do know the MOX behaves in its release a little different, and that's not surprising. The question comes about, and I came away from Dr. Meyer's presentation persuaded when you're looking at a clad effect, clad ductility effect, I don't really care. All you're doing is changing a little bit the driving force and not so much whether it's going to crack and fail or not.

And so I came away as saying, okay, put it on the plot.

MR. MONTGOMERY: Do you want to address that Joe or do you want me to address it?

DR. RASHID: I will address that. Joe Rashid, Anatech.

Unfortunately the MOX was introduced in this meeting. We have done a significant amount of work on modeling the MOX effects and under RIA conditions, but we didn't present any of it in this meeting. We disagree with Ralph's description of the effect of the gas release on the MOX RIA response, and as a result of that one evidence of it, of course, in the data he presented for REP-Na9 -- I'm not sure if its REP-Na9 or 6 -- the strain measurements is more than twice that that can be predicted by any other code, and the only explanation to that is the effect of the gas swelling enhancement for PCMI.

So there's not enough information being discussed regarding the MOX. Unless we address all of the work that's being done on the MOX I don't think we should be even evoking the effect of MOX in this meeting.

Thank you.

CHAIRMAN POWERS: Well, it looks to me, if I look at the plot, that the MOX data point could be sacrificed and wouldn't change the conclusion one iota.

MR. RASHID: Regarding UO_2, I agree with you, yes, regarding UO_2. I think UO_2 stands on its own, and MOX has a separate effect altogether, and we ought to address it separately and examine all of the work that's being done on it.

CHAIRMAN POWERS: Boy, you have no idea how reluctant I am to do that.

MR. RASHID: We'll be glad to have a separate meeting on MOX, and we can show the work that we've done on it.

CHAIRMAN POWERS: I suspect you've almost guaranteed we will.

Any other comments?

(No response.)

CHAIRMAN POWERS: Well, hearing none, we will recess for lunch until one o'clock.

(Whereupon, at 12:00 noon, the meeting was recessed for lunch, to reconvene at 1:00 p.m., the same day.)

AFTERNOON SESSION

(1:01 p.m.)

CHAIRMAN POWERS: Let's go back into session.

Rosa gave us an introduction on this subject and made all kinds of promises that Robbie was going to fulfill. I don't know whether Robbie knew about these promises beforehand or not, but we're going to hold him to it.

So the floor is yours, sir.

MR. MONTGOMERY: Thank you.

Let me see if I can find my presentation here.

CHAIRMAN POWERS: Yeah, the dog ate it, right?

MR. MONTGOMERY: Yeah. We have to go home now.

(Laughter.)

MR. MONTGOMERY: Well, when you get to the controversial points, Robbie, you can just say, "Well, Rosa covered this all."

MR. MONTGOMERY: Thank you, Mr. Chairman, for a nice introduction of what I have to do today.

As Dr. Yang pointed out or mentioned several times, we have developed a methodology for analyzing and understanding our experiments, and then we have gone the next step, and that is to use that methodology to take that understanding and develop reactivity initiated accident acceptance criteria, and my presentation will cover how we went over that and how we developed those.

I guess before I get started, I have a fairly long presentation and I know we're running late. So will you just kind of keep me track of the time and let me know how much time I have.

CHAIRMAN POWERS: Sure.

MR. MONTGOMERY: Okay.

CHAIRMAN POWERS: You've got about two hours.

MR. MONTGOMERY: I'll try to keep it in that time period.

All right. Kind of an overview of my presentation. I will just briefly make some comments on our objective of what we're trying to do here and then really get into the meat of the presentation, and that is to discuss how we're dealing with fuel rod failure, how we've developed an understanding of fuel rod failure mechanisms through evaluating the experimental data, and then how we developed a methodology to construct a failure threshold, taking into consideration the understandings that we've gained from the experiments.

And then I'll briefly show you the failure threshold and go over the characteristics.

In addition, the approach that we took in evaluating RA safety criteria was to develop not only a failure threshold, but a core coolability or fuel coolability limit.

So I'll briefly go over the core coolability issues, the issues of fuel dispersal, how we think we can deal with those, and the methodology that was used to develop the core coolability limit.

And then finally I'll go over the failure or the core coolability limit and talk a little bit about some of its characteristics as well.

And then I have a summary, and in that summary I'll basically talk about some of the points again, kind of elaborate on some of the points that Rosa brought up with regards to differences in the approach used by the industry and the differences and the methodology that's been described in the RIL that Ralph Meyer summarized this morning.

As I said, really our objective is to understand the fuel rod failure mechanisms and the processes that come about after failure, and then to use those to develop criteria, and that's what that slide is saying. I won't go through that, but basically I'll just jump right into the real work here.

All right. Our approach was to first try to develop an analysis methodology for reactivity initiated accidents, and to do that we basically constructed a big database of all the relevant RA experiments or as many as we can get our hands on anyway.

We used those experimental results and accompanied analytical evaluations, code calculations, to try to gain as much insights and understanding in the fuel rod behavior exhibited in these experiments, things like evolution of fuel temperatures, evolution of cladding temperatures, evolution of fuel and cladding strains, elongations, that sort of thing, and try to really develop an understanding of the failure mechanisms and the processes that are ongoing inside a fuel rod bearing these rapid energy insertions.

Once we were able to develop that methodology or that understanding, we went in and began really to validate as best we could an analysis methodology using these experiments. Once we understood them, we were able to model them and develop codes that could represent the deformations and temperature responses of these rods. We went and started to compare our calculator results with the measured data developed an ability to differentiate between failed and non-failed rods.

And I think that's a fairly important process in the whole approach, is to be able to understand the failure mechanisms and be able to differentiate between failed and non-failed rods.

Thirdly, by doing that we were able to identify what the failure mechanisms are, what the rod deformation processes are, and we can develop a cladding integrity model that incorporates all of these understandings based on less expensive, more readily available data, and that is mechanical property data or separate effect tests, if you want to call it that.

And these mechanical property data are an attempt to be as representative as possible of the failure mechanisms that we understand that are active in the fuel rod during these types of events.

All of these Steps 1, 2, and 3 basically are focused at, first, understanding experiments and being able to gain the insights from these experiments to direct us in determining what a failure threshold should be for lightwater reactor conditions, and that's Steps 4 and 5, is to develop a failure threshold that's applicable to lightwater reactor conditions, whereas most of these experiments have only a remote representation of lightwater reactor conditions, and Rosa highlighted some of those differences in terms of coolant temperature, pulse width, and things like that.

So in developing the failure threshold, the approach is to first try to account for all of the important mechanisms that affect fuel rod behavior. Those are temperatures, loading conditions, fuel rod geometries, pulse width, things like that.

And once we've incorporated these mechanisms into our approach, we're able to use the validated analysis methodology that was based on these experimental results to transfer that understanding into developing a failure threshold for what I would call an ID life or bounding reactivity accident condition.

All right. We've seen several plots today with regards to the RA simulation test results, tests that are out there. This is one way to plot it. Dr. Meyer showed us the data this way, plotted this way this morning, and that is we're plotting it as a function of oxide thickness, and the question was raised this morning about what does oxide thickness mean. That's a good question because in some of these experiments we have peak oxide. In some of them they're kind of a sample average. You have a variety of oxide thicknesses because we know oxide thickness is not all created equal.

They're being plotted and what we're plotting here is the maximum fuel enthalpy change or you could think of it as the fuel expansion process if you want to, the fuel pellet expansion process, and we're using fuel enthalpy or fuel enthalpy changes, the parameter to represent that as a function of this oxide thickness.

What I'm plotting here are the failed rods that came out of these various programs from the early SPERT programs all the way through to the more recent tests in Cabri and NSRR. We have the Russian data here which is primarily at low oxide, and what we see is that there's failures, that low oxides tend to be at higher energy, and failures at higher oxides tend to drop off a little bit, and that gives some idea that maybe oxide thickness is one of the parameters.

But then if we superimpose on those, the non-failed rods, which are all of the open symbols, one of these open dot symbols, these are all of the non-failed rods. We see that there's some interdispersion. There's not a clear separation in terms of oxide thickness or energy where you can say, well, there's a threshold.

So there's other parameters involved. That cladding oxide thickness is not the only parameter. You saw this morning when Rosa showed her slides or Dr. Yang showed her slides, that in burn-up spaces it's not clearly delineated as well. So there's a variety of factors or variables that are in play here.

So we're back to this plot again, and so what we do is if we look at these data a little bit more closely we see that these data -- if we look at the failure mechanisms about how these different rods failed or some characteristics of these rods, we see that these rods represent data or rods that have experienced high fuel enthalpies , exceeded the departure from nuclear boiling regime, have gone up to cladding temperatures of 11, 1,200 degrees C. possibly, sometimes higher. They all failed by a ballooning burst mechanism or an oxidation induced embrittlement mechanism. That would be high temperature oxidation induced or even clad melting.

There's a variety of failure processes going on in this data set.

If we look a little closer, there's the MOX fuel rod here. So these failures are starting to become a little evident that there's something unique about each one of these and they're not all equal. We have rods here that were failed that have oxide spallation and accompanying hydride lenses. We saw a little bit about that this morning.

We have rods here that have -- not well characterized. So they have an unknown oxide thickness with them. It's kind of difficult to know exactly where to plot them on this plot.

In addition, it should be pointed out that these were very early rods fabricated in the early 1960s, and they were specially designed for the CDC SPERT reactor. So they are very narrow rods, very thin fuel rods with thin cladding, which, again, may exacerbate the problem.

And then finally we have the rods from NSRR that were all tested at room temperature, and so we have a mixture mode here. We have room temperature tests. We have a 300 degree C. test, and so there are some differences there, and so not all of the failures are created equal, and as a consequence, we must really develop a clear understanding of what the processes are that lead to the failures so that we can determine how relevant these failure mechanisms are to lightwater reactor fuel.

So we have a database of something like 100 experiments or so that span burn-up ranges from very low burn-up to 75, 74 gigawatt days, oxide thickness from five to 130 microns, pulse width from four to 75 milliseconds, maybe even three. We heard three for the BIGR test. Fuel enthalpy levels from 65 all the way up to more than 200 calories per gram.

And then we have quite a bit of information. A lot of these experiments had end pile (phonetic) instrumentation that was used with them. So they had thermocouple measurements or they have rod deformation measurements, coolant temperature measurements, rod internal pressure measurements. They also have quite extensive post test examination results, measuring deformations, fission gas release, things of that nature.

Of these somewhat like 100 rods, we've selected a little more than 30, 31, 32 rods, for detailed analytical evaluations using FALCON. This is to first validate and develop a methodology to analyze these experiments, and that's what this step number two is, is to really validate a good methodology for analyzing these experiments.

We do that by comparing to these end pile measurements and post test examination results, and I'll show you quite a bit of those comparisons. Of these 30 rods, nine of these rods were failed. So we have really more non-failed rods in the database because we can learn a lot about the behavior of fuel under RA conditions from the non-failed rods because you get much better post test examination results.

But we do have nine rods that we included in there that failed at fuel enthalpy levels, a failure type range between 60 and 86 calories per gram. So relatively low.

And then I should point out that this validation using FALCON is one of the largest validation efforts or most extensive assessments of a fuel code for RA experiments. There are several other codes out there. The SCANAIR you've heard is a good code, but its focus has been primarily on Cabri and doesn't have as much NSR validation as we have used in our code, and conversely, the Japanese codes have primarily focused on the NSR experiments and not so much on Cabri.

We have used as much of the information as available to try to give as large a validation base as possible.

DR. ELTAWILA: Dana, can I ask a question, please?

CHAIRMAN POWERS: Please.

DR. ELTAWILA: Rosa mentioned the nominal identification and ranking table that would be developed for the fuel, and it addressed both the experimental data and the analytical model. Does the FALCON code include models for every high ranked phenomena identified by the PERT panel?

MR. MONTGOMERY: I would think so, yes. I would think so. I don't have that long list in my brain to do the entire check-off, but I believe it would.

All right. Here's an example of some results we would get and some comparisons to experimental data. I have here, and I apologize for the quality of the graph, but you see a representation of a real power pulse used in the Cabri facility here. It is power versus time, and you can see that this is about 50 milliseconds. This is an approximate ten millisecond pulse. So you can see the pulse is really over in about 30, 35 milliseconds.

When we talk about pulse width, we're

typically talking about the full width, half max as that parameter.

Up here in this left corner we have fuel and cladding temperatures. This would be the fuel pellet region here, and this is the cladding region here. The calculated fuel and cladding temperatures is a function of radio position and as a function of time during the pulse and shortly after the pulse. So you can see that early in the pulse down here, at a fairly low inserted energy of about 30 calories per gram, that's typically in the first third of the pulse. The temperatures are peaked and not too high with the temperature peak at the pellet periphery and then the cladding temperature shown here is fairly flat. It doesn't even know that there's much going on yet.

Then we get to the max temperature location, which is near the tail end of the pulse. You can see that the peak temperatures reach something like 2,500 degrees K. for a 100 calorie per gram pulse, and a semi-temperature of something like 1,500 K. And you start to get some clad heat-up, and then as you move later in the pulse, the clad even heats up more and the fuel temperature begins to drop down, and after a few seconds, you reach -- a cosine temperature distribution starts to become more evident, and these are the types of things that we calculate in FALCON.

Here's a comparison. I've put the arrow here after the pulse is over. Post test examination data here for cladding deformation. This happens to be radial displacements of the cladding as a function of axial position, and the blue line is the code calculator results, and the red line are the measured data and some of the scatter in the measured data for that particular rod.

Here's another example of some measurement data that's on-line measurement data. We have here axial growth of the fuel rod as a function of time during the energy insertion period. This is what, approximately 200 milliseconds here? And we can see that at the initiation event once the gap closes the fuel rod extends fairly rapidly and the symbols are the data and the FALCON results are the line, the blue line.

This is for REP-Na5, and this one happens to be for CIP-01. Again, we have the fuel rod growth and a range in the measured data and the calculated results.

So you can see the type of validation that we have going on for this kind of --

CHAIRMAN POWERS: What I don't understand from your presentation is there's a systematic discrepancy between the code calculations, the data in some cases, and what I'm grappling with is is that at all significant.

MR. MONTGOMERY: Well, I think the next set of slides will try to do that. I've just shown you a couple of snapshots of the performance. I wasn't able to show you everything.

This is a little bit better summary of the overall code performance and what we have here are your classic predicted versus measured curves. This one happens to be for residual hoop strains, and this one happens to be for peak cladding or fuel rod elongations, and again, predictive elongation versus measured elongation.

The solid line here represents the 45 degree perfect agreement line, and what I have here are some of the results for the Cabri and NSRR tests, both B and PWR tests shown in here, and you can see that for the Cabri facility where DNB is not an issue because you have a sodium loop; you don't have departure from nuclear boil and the cladding temperatures remain relatively straightforward to calculate and predict.

We get pretty good agreement with the experimental data. I show some uncertainty here, the variation in experimental data because there's ovality and some things are going on in the measured data. So you have some uncertainty or some range of measured data showing there, but you can see that for where the cladding temperature is reasonably well controlled we show pretty good agreement with experimental data.

For tests in NSRR where, again, NSRR has a capsule test. The tests are run in a stagnant water and ambient conditions, so atmospheric pressure, room temperature coolant water, and these tests experience DNB a lot of times.

And the way I kind of picture the NSR experiments is if you have a hot rod and you quench it in a bucket of water and that violent heat transfer that goes on in that pool boiling condition is what's going on in NSR facility, except you just don't stick the rod in. The rod starts out there and you heat it up really fast with the pulse.

So we don't have a good way to model that violent boiling under stagnant water conditions in our code. There may be other capabilities out there. So we don't get a good representation of the DNB and the cladding temperatures, and what you see is that when the rods start to experience DNB, the cladding deformations tend to grow larger, and they grow larger for a number of reasons. They grow larger because of the rod internal pressure. Inside is pushing out and so you get some plastic deformation and that way you get some expansion of the pellet that drives that, but the cladding strains tend to be a little bit larger and can go out to even 15, 20 percent in some cases.

Here we have the elongation values. You see pretty good agreement there. What we can conclude from this is that -- and I think Dr. Meyer concluded with this this morning as well -- is that the primary mechanism leading to deformations is pellet thermal expansion. That's the PCMI loading, especially during the power pulse. There are mechanisms that come in after the power pulse is over when cladding temperatures begin to heat up. DNB may become active where you can get some additional cladding deformations, but those are not driven by the pellet. They're driven by other secondary mechanisms.

And in terms of PCMI failure, the initial power pulse conditions are the major area of concern.

All right. So we've kind of looked at the non-fail test. Let's look at the failed test and see what we can learn from the analysis of these rods.

The first thing we notice is that all of them failed at fairly low energies, and correspondingly, the calculated cladding strains are rather low, typically less than one percent, elastic plus plastic strain, which would be the total strain. These are calculated.

These are representations of the peak fuel temperature, and you can see that there's no real correlation between the temperature peaking and cladding failure. It appears to be more related to the PCMI load or the cladding ductility more than the PCMI loading.

But you do need PCMI loading to induce cladding strains. So what we can learn from this is that the cladding failures are primarily driven by PCMI. In UO_2 fuel there's no driving factor coming from the pellet that's strongly burn-up dependent.

So essentially what we have is a fuel rod failure, and high burn-up UO_2 fuel is controlled by PCMI, which is a loading process and cladding ductility which is the ability of the cladding to accommodate the pellet expansion process.

And here I put the slide you saw earlier on here where we have gap closure going on. When the gap is wide and the cladding ductility is high, cladding failure is not driven by PCMI. It's driven by high temperature processes. When the gap is small and pellet cladding contact can occur, when the pellet heats up, then depending on the cladding ductility, failure by PCMI become possible.

So hopefully at least in a very quick, short way I've shown that we have a pretty good understanding of the processes that go on inside the fuel rod during a power pulse and that we understand the failure mechanisms sufficiently to be able to develop a cladding integrity model or failure model.

And we have done that in this activity, and we have elected to use strain energy density and critical strain energy density as the parameter of choice for analyzing the mechanical response of the cladding and determining its failure potential.

There are other methodologies out there, cladding strain. You heard this morning cladding strain is one, and there are others.

So for those that may not fully be familiar with the strain energy density concept, what we have here is that the strain energy density, which is calculated by the fuel performance code is a measure of the loading intensity on the cladding and it's really just an integration of the stress and strain curve. So we have the stress plotted here, schematic stress here and strain here, and we're basically getting the area under the stress/strain curve.

In the calculation process because we have the constitutive model built into, say, FALCON or SCANAIR or the codes. It can incorporate things like strain rate, temperature and stress by axiality all in calculating the SED parameter.

Then we need to have a way to judge failed versus not failed or failure potential, and that's done by the critical strain energy density, which is derived from mechanical property tests, and the critical strain density as a measure of the cladding failure potential or the residual ductility of the cladding, and again it's an integration of the stress/strain curve now extracted from mechanical property tests, and it depends primarily on hydrogen content, zirconium hydride distribution, temperature, things that affect the cladding microstructure and ability to form.

Cladding failure, in our methodology, cladding failure occurs when the SED calculated by FALCON reaches the CSED for a given cladding material.

Just to give you a kind of brief overview of the database that's used for the CSED model development, this busy slide shows you that. We have a variety of different test types, test conditions from burst tests to ring tension tests to axial tension tests.

This is our irradiated material properties table. So we have different fuel types, burn-up levels, fluence levels, and oxide thicknesses from very low oxide thicknesses to as high as 110, 120 microns, including some spallation, spalled oxide rods, and temperatures ranging from nominal operating temperatures like 588K all the way down to room temperature 298 and 313K type temperatures. So although the database is primarily focused on operating type temperatures and most of the data is there, we do have a subset of the database that is at colder temperatures, and a variety of strain rates from low strain rates representative of nominal operation to higher strain rates more typical of what you would expect at the loading rate of an RA event.

Here we have a representation of the critical strain energy density mechanical property data plotted now as a function of oxide thickness to cladding thickness ratio, and I plotted and kind of identified the different test types here from ring tension tests, axial tension and so forth.

The data can really be looked at in two different data sets, subsets. We have the data that's non-spalled and the data that's from spalled rods, and we have elected to fit a best fit curve through both the non-spalled data to come up with a non-spalled curve and then through the spalled data to have a spalled curve in terms of allowing us to interpret the experimental test.

You'll notice that there's some scatter in the data. Maybe that's an understatement, but most of this scatter is really related to the different types of test conditions used, the different specimen geometry.

DR. SHACK: How did you get this best fit curve?

MR. MONTGOMERY: Well, we just fit all of that data.

DR. SHACK: Just plunked it in.

MR. MONTGOMERY: Just plunked it in.

MR. CARUSO: Is it a linear equation?

MR. MONTGOMERY: It's an exponential equation.

MR. CARUSO: How did you determine the form of the --

MR. MONTGOMERY: I'm sorry. The form? We just tried to use as simple as we could. We knew linear would just basically go like this. So we didn't really want to do linear.

MR. CARUSO: Do you use a parabola?

MR. MONTGOMERY: Well, parabola, we'd end up with some --

MR. CARUSO: -- equation?

MR. MONTGOMERY: Yeah.

MR. CARUSO: What did you choose that for?

MR. MONTGOMERY: Well, we do understand that the cladding ductility as represented by CSED, cladding ductility does decrease as the cladding oxide thickness or, more importantly, the cladding hydrogen content increases. So we wanted a curve that does decay downward, but we didn't want it to necessarily decay to zero. So it has some flattening off shape.

I mean a linear curve wouldn't be a lot different, but it would just be more difficult to extrapolate.

DR. DENNING: It's least square fit. Is that true?

MR. MONTGOMERY: Yes.

DR. DENNING: It looks like it doesn't -- it's not really pinned very well to the right-hand side, and that may be a limitation of using an exponential rather than some other form that would have been freer.

DR. BILLONE: But he's not trying to fit the solid red point.

MR. MONTGOMERY: Yeah, the solid red point --

DR. DENNING: You're excluding those points anyway.

MR. MONTGOMERY: These are all excluded, and that's a separate fit.

DR. DENNING: Yeah, yeah. So I'm wrong.

MR. MONTGOMERY: But you do have this one point out here and obviously there's -- actually there's two points, and you can't see one of them.

CHAIRMAN POWERS: Dr. Denning, you're not wrong. You're just misled by the speaker.

(Laughter.)

DR. SHACK: That's right. If Rob threw away that data point, that curve wouldn't move.

MR. MONTGOMERY: Not by very much. Right. I was agreeing with him because the curve is really driven by this big set of data over here, and --

DR. SHACK: And the exponential form.

MR. MONTGOMERY: And the exponential form, and we've added, you know, a few more data points in this data set since this graph was actually developed, and it hasn't shifted very much.

Okay, but let's -- are there any other questions?

DR. BILLONE: Yeah, Rob, I want to emphasize your point number one about the scatter because you remember our round robin with the ring tests in which we all tested the same material at various labs, and if you're talking about total elongation, which is what you use, you integrate to the end. We got numbers at room temperature between eight percent and 40 percent for the same cladding lot, depending on the details of the testing technique, and that's one of the possible problems with plotting all data on one plot.

So I support your point number one.

MR. MONTGOMERY: Yeah, and I think you might support number two in a way that there is an effort, albeit not as much as I would like, but there is an effort to go in and prove the test designs in terms of the specimen geometries and try to get some standardization going on to try to reduce some of this scatter as well, but it's difficult because there are a number of factors involved.

CHAIRMAN POWERS: I guess maybe I don't understand. There's a guarantee I don't understand.

The blue solid line, what exactly is the significance of that other than it's an improper fit to the data?

MR. MONTGOMERY: That will be my next slide.

Can we go to the next slide?

Do you mean in terms of how we're going to use the blue line?

CHAIRMAN POWERS: Yeah, yeah.

MR. MONTGOMERY: Okay. Let's go to the next slide. Okay?

So this blue line represents a threshold between the loading needed to survive or the loading needed to fail to cladding. A crack would form. Okay?

So we'll go to the next slide.

Okay. What we have here, again, we're reproduced the blue line now from that previous plot, and what we're now plotting, again, the blue line would be CSED versus oxide thickness, declining thickness ratio. The symbols now represent the analysis results for the various RA experiments conducted at -- these are for 280 degrees C. for all the Cabri rods that are UO_2. And the open symboled are rods that did not fail, and more importantly, they did not have -- they're blue. They're labeled blue, and that means they're non-spalled rods. They came from rods that did not have a spalling oxide.

CHAIRMAN POWERS: Those are Democratic points. We have blue points and red points.

MR. MONTGOMERY: They have their stuff together. Okay? These are the rods that have their stuff together, but they're really blue because they're to be compared against this blue line, and none of these rods failed and they fall below the blue line. We would expect them if they were to have failed to fall above the line. Okay?

Now, we also have two failed points. These are the solid symbols here, and they're labeled in red, REP-Na8 and REP-Na10, and they're from rods that we've talked about a little bit this morning, had a spalled oxide, and I'll talk a little bit more about that later, but they do have spalling, and that is flaking of the oxide off.

And as I showed in the previous slide, there was a red curve on there, and we really didn't talk about that much, but there's a red curve down here that's fed to samples that are identified to have oxide spalling and hydride lenses, and we have developed a CSED fit for those as well, and that's this line here, and we can see that they both reside above this line, which is that they should fail. If they were residing below this line, they would be expected not to fail.

CHAIRMAN POWERS: I guess I'm really confused. If I go back to your previous plot, there are points below the red dashed line that are filled in.

MR. MONTGOMERY: Right.

CHAIRMAN POWERS: And then there are points above the blue dashed line or blue line.

MR. MONTGOMERY: Yes.

CHAIRMAN POWERS: I mean, you've lost me just a little bit.

MR. MONTGOMERY: Okay. Now, these are not experimental RA test results. I should say these are mechanical property tests, and they're a variety of different types of tests. Like I said, there are bursts tests, which are gas -- not gas, but primarily oil loaded tests where the cladding tube primarily had been defueled. Some of them are done fueled, but most of them are done defueled, were pressurized up to failure.

You have ring stretch specimens in there. That would be the ring tension test where they're loaded up to failure. Although these are cladding mechanical property tests, they don't exactly represent PCMI loading conditions in a fuel rod. There are some variations. Loading with oil is not the same as loading with a pellet. The frictional effects aren't there, for example.

Ring test --

DR. YANG: We have two type of data, Dr. Powers. We have these, which are cladding mechanical property tests, which are trying to find out how strong the claddings are. Remember his modeling approach. He's trying to -- he has his model and he's trying to assimilate the RA test, and then one of the elements of it is how strong the cladding is to take the RIA loading. So these group of data are mechanical property tests of the same type of cladding that we may later on or before -- subject to the RIA test.

And the next slide, Robbie, if you can go to, these are the actual RIA simulation tests that look at, you know, if his modeling prediction -- no, I'm sorry. The blue and the red curves are mechanical property tests. What is involved in this curve is that there are some model calculation of what is the loading, those REP-Na1 -- I'm sorry. Not one -- 2, 3, 5, CIP-04, you k now, that type of thing. So this is kind of a sort of what we call RIA simulation test. It's kind of an integral test, where the plot before is mechanical property separate effects test, and we rely on both type of tests in our analytical approach.

CHAIRMAN POWERS: So you found some curves that said, okay, the failed tests fall below this. They don't have spalling, and if they are above this, they do have spalling.

DR. YANG: Those kind of confirm the model is pretty good, pretty consistent with the data.

MR. MONTGOMERY: What this shows is that a fair amount of additional energy in depositing these rods above what was able to get in the test to have them have higher loading on the cladding and exceed this curve, then they would fail, and we haven't really had that for non-spalled rods. Like I say, or as Rosa kind of indicated this morning, it's hard to fail a non-spalled rod. So there's a void region here where we don't have any failures that would kind of complete the picture a little bit.

CHAIRMAN POWERS: As a result you don't know what the significance of the curve is other than the whole point is it could fail and fall below it.

MR. MONTGOMERY: It could be that this curve could drop some, yeah. I mean, there is that issue. Now. --

DR. DENNING: Now, isn't it a little bit of a surprise that the curve works that well? If you go back to the previous curve, other than the fact that I think that -- I think the reality is that a number of those points that are above that line are more valid than the ones below it, and that's why. I mean, if you look at that, you'll see a curve with a lot of variance.

MR. MONTGOMERY: Yeah, there is some of that.

DR. DENNING: You would have thought if that really is the best estimate curve, if you'd gone over to the next one, you have expected a large number, if it is really a best estimate curve.

MR. MONTGOMERY: And I think that you'll see that. I understand your point, and I guess before I leave this slide because I'm going to go -- we actually did the same thing for room temperature data, for -- I guess I shouldn't say "room temperature." For lower temperature data, below 150 degrees C., we have a much less populated database. Instead of some hundred-odd points we have something like maybe 20 or 30 mechanical property at room temperature, and we've developed the same curve, and you'll see that it works, but there's a little bit more scatter, as I'll show or a little bit more of what you would expect of a best fit curve.

But I do want to point out that what I would like to say here is that mechanical property tests done under these conditions don't necessarily represent the way cladding actually is going to fail exactly in fuel rods, and as a consequences we've shown through analysis, and I can give you a paper on that, Dana, but these test conditions tend to exaggerate the failure capability of the cladding a little bit, and that's one reason why you have some of this separation here.

But let's go to the next slide, and this, I didn't show you the development of it, but this is the similar CSED curve now, but as derived for low temperature. We do have a database, the subset of database for lower temperatures, 150 degrees C. or less, and now what we're plotting here is, again, the SED for the various experiments that we've analyzed in the room temperature test and comparing them to the curve, and now you see what we talked about. You see, since it's a best fir curve, you see some non-failures above the curve. You see some failures a little bit below the curve.

But what you see is that most of the failures are kind of agglomerated around the curve. I put in here basically the variation on the samples of the oxide thickness so you can see oxide thickness if not a single number. It varies over some range.

I should point out that HBO-3, HBO-6, and HBO-7 all had incipient cracks. So that gives you some confidence that this failure line here is reasonably close. You have HBO-5 that failed, HBO-1 that failed here. Again, they're pretty close to the line.

Here you have tests that developed a fairly high SED, but they also went into DNB as well, and so you have a temperature effect going on where the cladding temperature is changing during the experiment and so since this curve we know is temperature dependent, and if you'll notice just for example the low temperature curve starts at about 15 megajewels per meter cubed. The high temperature, the 380 to 280 degrees C. test starts at 40. So you have a 15 versus 40. So you can see that there's a temperature effect going on here.

And we know that the temperature evolved during these experiments so that this curve is actually changing with time over the experiment from the lower temperature value and as the cladding heats up to the higher temperature value.

So there's some explanation of why these may be above the line, and also they're partly above the line because of the --

DR. SHACK: In your big scatter plot, you treated everything between 280 and 400.

MR. MONTGOMERY: That's right.

DR. SHACK: I was just wondering why you didn't -- I mean is there any systematic variation with temperature here that I would see if you actually sorted this data?

MR. MONTGOMERY: If I added some temperature? There may be a little bit for the 400 degrees C. data, and there are a few data -- the database is primarily 280 to 350 with a few points at 400, and there is a separation between the 400 and a little bit.

But unfortunately, we don't have enough really data to really develop clear temperature dependencies.

DR. BILLONE: Rob, go to your third slide from here, the --

MR. MONTGOMERY: This one.

DR. BILLONE: Yeah. Could you explain how you got such good agreement on that and such poor agreement on predicted strain? I thought for the low temperature tests one of your earlier graphs showed that you under predicted strain considerably.

MR. MONTGOMERY: For these specimens here, yeah, which went into DNB.

DR. BILLONE: I thought there were more than that. Okay. I'll let you come back to it later.

MR. MONTGOMERY: Okay. All right.

CHAIRMAN POWERS: If I could come back to your original fit of mechanical property data, and I would do this in a validated or a proper way of doing it. I believe that for the non-spalled data and even for the spalled data I probably would get a constant, and the reason I do that is because your oxide cladding thickness ratio is this very broad range, and consequently you can't ignore the variance in what you treated as the independent variable relative to the variance that you have in your dependent variable.

I mean, I think if you hypothesized in a decaying exponential you'd come up with a zero exponent in that if you took into account the variance and what you've treated is the independent variable.

DR. YANG: Can I add one thing? There are two points that need to be made. I think this graph was what we submitted earlier. I think there are two things. One is that the spalled and non-spalled really should not be plotted on the same curve because in this region you cannot spall. You know, when the oxide is very thin, you just don't spall.

So maybe only this area you have the possibility of either spall or not spall.

CHAIRMAN POWERS: Doesn't speak to the issue. Mine is strictly a mathematics issue.

DR. YANG: So my point is that you shouldn't mix it.

CHAIRMAN POWERS: I'm not mixing those.

DR. YANG: Okay.

CHAIRMAN POWERS: I am strictly speaking to the process by which you've found the slope of your decay.

DR. YANG: Okay. Let me make --

CHAIRMAN POWERS: Now, if I come in and say, "Gee, there's just no dependence here on the oxide thickness ratio for these elevated temperature tests, doesn't that throw all of the things that we've heard about, embroiling of the prior beta phase into a -- well, it's not the prior beta phase now. It's the alpha phase -- into some disarray here?

DR. YANG: This is in the lower temperature. This is a much lower temperature.

CHAIRMAN POWERS: These are all 400 degrees. Okay? And it seems to me what it's saying is the critical strain energy density just doesn't depend on the oxide cladding thickness ratio.

MR. MONTGOMERY: Well, there's a number of reason why you have the scatter that's there.

CHAIRMAN POWERS: And I accept every one of them, but the problem is that there's scatter on what are treated as the independent variable, and if you take that into account in the derivation of the equations you use for calculating a least squares line, I don't know, but I'm willing to bet that line comes out to be a flat constant.

DR. BILLONE: Rob, I think Dana is making two points, and you'll probably get both of them, but let me try to articulate it so I understand. One is all of those solid points there could be best fit with a horizontal line with a slope of zero. You could try that.

MR. MONTGOMERY: Sure.

DR. BILLONE: Point number two, there

should be an increase in average hydrogen content away from the blister as you go up in oxide ratio thickness, and if you had a horizontal line it would be independent of that increase in hydrogen content with the increase in oxide layer thickness, which may make sense if you've got a blister of the same size at two different levels of oxide thickness. I'm not sure about that.

But, Dana, is that anything to do with your point?

MR. RASHID: Mr. Chairman, may I make a point?

CHAIRMAN POWERS: Sure, Joe.

MR. RASHID: Joe Rashid.

We're ignoring a very important consideration here in these tests. First of all, when you look at spalled versus unspalled cladding or spalled or unspalled test sample, you are not measuring the local conditions. You are measuring the average conditions. Take pressurized tests, for example. You have hydride blister in that pressure sample specimen, but what you do measure, you measure the stress resulting from PR overdue (phonetic), which is the burst stress, and you measure the average strain around the circumference.

If you were to go and measure the local conditions for these, you will have a different kind of plot. Okay? So all of these are the plots of the data coming out of the material tests, material property tests, cast in the way that would be comparable to the data coming out of the RIA test. In other words, under RIA conditions, you are not measuring local effects. You are measuring average effects.

In RIA test logged, what you measure is the PCMI force which is axisymmetric average force, axisymmetric strains, okay, and maximum stresses, and so the whole thing is comparable, consistent between the data set for material data and for the structural test. RIA tests are structural tests. This is material failure tests.

But there are some averaging processes taking place and we are not looking at local effects because we don't know what the local effects are in the RIA test. So we're trying to be consistent with that.

The common denominator between the RIA tests and the material failure test is the energy deposition, and that's what combines the two conditions, but the average energy deposition in the rodded failure should be equated to the average energy deposition in energy property tests.

So you can take any kind of fitting you would like, and power variations, exponential terms, linear regression or what have you and you will come up the final result will be the same.

DR. YANG: Let me add to one other thing that we were not prepared to talk about it, but let me just give it to you in a qualitative sense because the work will be published in a couple of months in the journal, is following what was said earlier, some of these tests, especially some of the ring tests, there's some artifacts involved, and they need to be corrected, and there are detailed analyses. I think many of the mechanical experts would have agreed how to correct them, and that process is ongoing.

The preliminary results indicate that tremendously reduces scatter, actually shifts most of the data upward. So that work is ongoing and we'll be ready to talk about it as soon as the paper is published.

So let me just say we recognized there are scatter in the experiments, and some of the experiments may not be very relevant because if you think about PCMI loading the axial tension tests are not very relevant. So if you take those out, you know, recognizing some of the artifacts introduced in the test, that would tremendously reduce the scatter.

But we do recognize the scatter in the data, and we took a best estimate. There are possibilities to say, okay, what if I take the lower bound. You know, we can look at that.

DR. SHACK: Just to disagree with Dana a little bit, I mean, if I look at Slide 16, my uncertainty in the oxide ratios about plus or minus .01 to .01, his scatter in his properties are very much larger than his scatter in his oxide. It really is the way the mechanical property test is conducted that's contributing to his uncertainty.

MR. MONTGOMERY: And this value here, this parameter here is really trying to get at the hydrogen content. We didn't know the hydrogen content for some of these samples. So it wasn't possible to really derive this same curve as a function of hydrogen content.

But we have done a little bit of looking at that and you do see a kind of a change in the data, but the shape of the curve stays the same. I mean, what happens is as some of these points move around a little bit, primarily these points move way out here and then this red line basically becomes kind of an extension of the blue line if you push it out far enough in terms of the hydrogen content.

But we're still trying to look at some of these variables.

DR. BILLONE: May I make one more comment and I really will let you go on? I know from interacting with CEA through this mechanical properties expert group that you're on and Joe's on, they are still refining their analysis of their old data. They're still improving it by looking at the effects of friction on the ring when they try to expand the ring. They're not finished, and initially they published the raw data, sort of their engineering mechanical properties data. They're still working on their finite element analysis to determine a stress-strain behavior from those tests. So there is work still going on on old data.

That's my last comment.

MR. MONTGOMERY: All right. Well, I thought by now that I would have hopefully convinced you or at least demonstrated to you that our methodology is fairly sound. We understand the processes that lead to fuel rod behavior under RA conditions. We understand the processes that lead to cladding deformations and finally cladding failure under RA conditions, and given that understanding we can make a next logical step, and that is to try to construct a failure threshold for RA conditions.

So that's what I'm going to do next. So what we've done is try to develop a failure threshold that is consistent with current licensing approach, and what I mean by that is that we're going to look at radially averaged fuel enthalpy at failure as a function of rod average burn-up. It just kind of ties into what the licensees do now in terms of calculating radial average fuel enthalpy in their system analysis codes and typically working in terms of burn-up space.

Some assumptions that are going to be used in deriving this failure threshold is that first is we're going to use as our cladding integrity model the best fit CSED versus oxide thickness for non-spalled Zirc-4, which is about applicable to temperatures of 300 degrees C. We feel that this bounds B and PWR cladding behavior.

I should have said early on that all of that data we were talking about is Zirc-4 cladding data.

Secondly, I'm going to show you a conservative Zirc-4 corrosion versus burn-up correlation, which is going to be able to relate. We've been talking in kind of a corrosion space, relate corrosion with burn-up in a conservative manner.

And finally, just a bullet that says basically what we're going to do is in terms of the reactivity initiated accident event or the rod ejection accident event that we're going to analyze here, we're going to assume that the peak power, the peak burn-up, peak corrosion, all occurred at the same location on the rod, and in reality that's not the case, but in terms of developing the criteria or the failure threshold that's what we're going to do.

I'll skip that slide and just go to this slide here. This kind of summarizes our approach. Here's a schematic that we've spent a lot of time on already. This is the cladding ductility of CSED versus oxide thickness, and we're going to derive an oxide thickness versus burn-up based on data, and we're going to combine these two together to end up with a cladding ductility as a function of burn-up. It's going to be in terms of CSED.

And then finally, we're going to run that using our fuel performance codes, analytical codes using SCANAIR, FALCON or FRAPTRAN. We're going to use

FALCON for this. We're going to calculate the fuel enthalpy or the fuel enthalpy rise needed to reach this CSED versus burn-up curve as a function of burn-up, and we're going to end up with a fuel enthalpy that causes cladding failure as a function of burn-up.

This is a database of approximately 4,400 oxide measurements for Zirc-4 cladding, primarily low tin Zirc-4 cladding. This is the maximum oxide thickness versus the rod average burn-up, and essentially we've taken this very conservatively bounding curve as the curve to use to derive a relationship between ductility and burn-up through oxide thickness, and here we're plotting oxide thickness and burn-up.

The approach is to use this approach for fuel assemblies or fuel rods that are targeted for high burn-up operation and for newer and more current cladding designs, and so we expect that the fuel will operate within this envelope, and we've capped it at 100 microns kind of as a limit to say, okay, we're going to operate in this range, and we don't need to look at spalled rods where spalling becomes more of an issue for higher oxide thicknesses.

So when we combine all of that together and we run the analysis, essentially what you end up with is a curve that looks like this. Rosa showed you this black curve already. What we have here is radial average peak fuel enthalpy as a function of rod average burn-up, and the failure threshold here, and I've just shown you a couple of different fuel designs. We looked at a variety of fuel designs, thicker wall, thinner wall claddings and pellet diameters and things like that.

And we've taken the lowest bound fuel design as the curve to use. You can see the curve is basically made of two points. One is a flat line, a flat line at 170 calories per gram out to a burn-up of 36, and then a curve.

The flat line represents the region where it's not really possible to fail the cladding by PCMI, and that really failure is driven by high temperature processes, and what happens when you exceed this line here, this threshold here is that the cladding temperatures get high enough due to departure of a nuclear boiling that failure has become more likely.

And the second part is really driven by the PCMI failure response, driven by cladding ductility changes as burn-up increases, and those cladding ductility changes are being driven by the oxidation growth that occurs throughout the lifetime of the rod.

If you notice here before I leave this, we've actually formulated this threshold as a function of rod average burn-up. The next slide I'll be comparing the failure threshold to the Cabri experiments on non-spalled rods from the high temperature sodium loop.

Again, radial average fuel enthalpy and now I'm looking in terms of rod peak burn-up because these segments typically represent the peak burn-up for that particular fuel rod, and we've just done a translation in terms of peaking factor.

And you can see that the curve here really bounds these survivors, non-failed rods in the Cabri facility, and we've pointed out already there are no failed rods that reside in this space yet. For a number of reasons I just haven't been able to get there from the test facilities.

This rod resides above the curve. If it would have been done in a lightwater reactor condition, which would have been water, DNB would have occurred and this rod could have failed, but since it was in sodium, it didn't fail.

All right. What's kind of the box or parameter, range of applicability for the failure threshold? Essentially it has been designed to be applicable to both PWR and BWR, hot zero power reactivity accidents, a rod ejection accident and a PWR control rod drop accident BWR. It can be applicable to cladding material, Zirc-4 and Zirc-2, and we feel that it's bounding for ZIRLO and M-5 because of the much improved corrosion characteristics of those cladding alloys.

It's applicable to UO_2 fuel or UO_2 fuel with burnable absorbers and out to rod average burn-ups of 75 gigawatt days per ton.

And, finally, as I said before, it's really limited to cladding that contains oxide thicknesses less than 100 microns, and without any surface spallation large enough to affect the cladding mechanical properties.

All right. What I'd like to do now is shift gears and move into the coolability limit.

DR. BILLONE: Rob, before you shift gears, just is it okay with industry that you're doing this as a function of burn-up, which penalizes something like M-5, which has a low oxide thickness and a low hydrogen content as compared to Zirc-2 or Zirc-4, or are you just applying this to Zirc-4 right now?

MR. MONTGOMERY: We're applying it to

Zirc-4, and we're saying that it's bounding for --

DR. BILLONE: Bounding. Okay.

MR. MONTGOMERY: -- for the other alloys. It is obviously the different fuel vendors or licensees have the option to modify it in some way for their cladding alloy.

DR. BILLONE: Okay, all right.

MR. MONTGOMERY: If there are no questions, anymore questions on the failure threshold, I'll move on to the coolability limit.

What we've done here is to look and see what the consequences are above cladding failure and address the energy deposition beyond cladding failure and try to establish a limit that would insure that the reactor remains in a coolable core geometry. So we call it the coolability limit.

The approach that I'll be describing is based on establishing a limit to preclude incipient pellet melting. We see -- and I'll talk a little bit about that -- that dispersal of molten material can lead to some important fuel coolant interaction processes and generation of mechanical energy that, if large enough, could end up threatening the reactor vessel and the coolability of the reactor core. So we want to certainly stay below that.

Again, to be consistent with the failure threshold and the way the license methodologies conduct these calculations, we're going to develop it as a fuel enthalpy limit as a function of burn-up, and it's going to be based on both experimental results and analytical valuations.

There are programs underway to evaluate the consequences of fuel dispersal for high burn-up fuel, and I'll talk a little bit about that, but there are programs within the Japanese work of JAERI and the Cabri water loop project to try to determine what happened, the consequences of dispersing fuel after cladding failure.

There have also been engineering evaluations that are underway to look at the consequences associated with solid fuel dispersal. If there is a pressure pulse, you know, what would be the consequence of that derived from looking at the experimental results and trying to translate them to lightwater reactor conditions?

It's our opinion that the results of these programs will confirm that the consequences of solid fuel dispersal are well within the safety boundaries.

DR. KRESS: Ten CFR 100?

MR. MONTGOMERY: Not CFR, not in terms of dose consequences. In terms of the general design criteria of maintaining a coolable core geometry.

DR. KRESS: I see.

CHAIRMAN POWERS: This always a little bit confuses me. So bear with me. It seems to me that as soon as we dropped out of a couple of hundred calories per gram we've pretty much limited the idea of disbursing molten fuel. I don't care how you get the energy in, it takes a certain amount of energy to melt fuel.

MR. MONTGOMERY: Right.

CHAIRMAN POWERS: And it's a lot.

MR. MONTGOMERY: Yes.

CHAIRMAN POWERS: But on the issue of coolability, however, it has to do with things like particle size and things like that.

MR. MONTGOMERY: Well, what we're looking at here is gross core distortion and loss of pressure vessel integrity, not necessarily -- keeping particles cool in the core is fairly easy. It's insuring that the core remains in a coolable geometry and that the pressure vessel is not compromised in some way. That's what we're looking at here.

Now, how you insure that in terms of the consequences of disbursing fuel will be a function of particle size and things like that.

Maybe I didn't understand your comment.

CHAIRMAN POWERS: Worse than that, maybe I don't understand where you're going to. I mean, if what you're worried about is just a fuel coolant interaction leading to a loading on the pressure vessel head, I think we can skip over that.

If what you're worried about is long-term coolability, then you've got to deal with particle size distributions.

MR. MONTGOMERY: We're worried about the former. I mean, the latter, of course, is also in there. We think that disbursing a small amount of fuel will always remain coolable when it's in a solid form. What we're talking about here is the generation of mechanical energy and the generation of pressure pulses that could compromise the pressure vessel.

CHAIRMAN POWERS: Okay. I mean, you run into a problem with feasibility. You just can't get enough energy in to melt the fuel, and if you can't get enough energy to melt the fuel, it's going to be very difficult to get a pressure pulse here.

MR. MONTGOMERY: Well, I think the point is I guess this kind of leads into this slide, and, yeah, the safety limit of 280 calories per gram, which is the existing enthalpy limit used today, is primarily based on molten fuel dispersal kinetics and the mechanical energy generation coming from fuel-coolant interaction.

Recent tests in France and Japan though have shown that at fuel enthalpy levels below 200 or 220 calories per gram you do get some fuel dispersal; a small amount of pellet material comes out. It's solid form.

You do get measurable mechanical energy generation, but it's small. Now, the question is should the coolability limit be set up to preclude the dispersal of pellet material in solid form or should it be established to preclude dispersal of pellet material that's in a molten form.

The approach that was used here, the industry effort, is to preclude the conditions of dispersing molten material, but there is a small possibility of dispersal of solid material, a small amount of solid material.

So I can just briefly just try to go over this as quick as I can. The potential for dispersing non-molten particles coming out of the fuel pellet increases because there are changes in the fuel pellet that occur during irradiation that promote this process, and Dr. Meyer referenced that a little bit today.

Part of that is the fact that you get more temperature peaking right at the pellet periphery. When the crack forms, that fuel gets very hot. It increases the potential for it to be expelled out.

Factors that influence that are pulse width, energy deposition, and the burn-up, and what we see is that there really has not been any field dispersal for tests greater than ten milliseconds. Pulse width is one of the variables, but you do get fuel dispersal with tests that are below ten milliseconds.

I should just point out that what I'm plotting here is energy deposition after failure, cladding failure, versus the pulse width of the experiment, and what we see here is that those tests that had pulse widths greater than ten millisecond did not disperse fuel. That with less than ten milliseconds did disperse a small amount of solid fuel material.

The amount of material that's release is relatively small. It's usually ten percent or less of the test specimen, and it typically comes from the pellet periphery region. It's relatively small, but it's greater than ten microns in size primarily.

As a consequence of the amount and the size of this material, the mechanical energy conversion rations are small, but not comparable to molten fuel material.

Another thing, we saw a picture this morning on a test that did disburse some material, and it's really hard to interpret these experiments because the amount of material that's dispersed is influenced by the sample geometry.

For example, the test we saw today did exhibit some fuel dispersal, but that's because the lower end plug fell off during the experiment because as the short six inch specimen started to crack, the crack reached the end plugs and ran around the end plugs and the end plug fell off.

Well, that's an experimental artifact and not really representative of what would happen in a 12 foot long fuel rod.

In addition, the tests that have been done that have generated mechanical energy, it has been shown by calculation and by experiments that the amount of fuel volume to water volume that's used in these experiments tends to exaggerate that process as compared to PWR conditions.

Based on all of this information, we determined that the energy deposition is very localized. The limited amount of materials is going to be dispersed for several reasons. One is that it's very localized both axially along the rod and radially within the fuel rod core and within the fuel rod pellet.

I just want to show quickly a picture here. This is the axial position along a fuel rod here, and on this side we're looking at the burn-up. So this is the burn-up shape of a fuel rod. This happens to be a rod with rod average burn-up of a little over 50, 54, 55, 50 maybe about the average and 55 is the peak, and what we see here is the normalized relative power distribution during the peak power point in a reactivity accident, and what you can see is that it's very localized power just near the top part of the core. This would be the top part of the core and this would be the bottom of the core, and we can see that it's localized over a fairly narrow region axially within the fuel rod, and most of the fuel rod does not experience the reactivity or the power pulse resulting from the reactivity insertion.

So we have a limited amount of material that's available to be dispersed.

CHAIRMAN POWERS: Excuse me. I mean, it seem like to me a very significant point here. Let me make sure I understand it. What you're saying is most of the power is deposited in whatever that end of the core was.

MR. MONTGOMERY: Yes, the top. This would be the top. I'm sorry.

CHAIRMAN POWERS: But when we do our tests, were we sampling from the top or we were sampling just any old where?

MR. MONTGOMERY: Most of the samples are taken from the flat part of the burn-up shape primarily from the top, and at NSR they took from various regions, and in Cabri they have done as well both from the bottom to the top. Typically low oxide samples can come from the bottom or the top. The peak oxide, the ones with larger oxides generally come from the top of the rod.

DR. YANG: I guess another point to look at it is the test, the what we call simulation test, they're just that. They're simulating. So they take a small piece of that flat part usually and then they subject it to the energy which for this curve would be on the top part of the energy. So it's just a simulation. The simulation test is never intended to simulate this shape, what Robbie present.

MR. MONTGOMERY: Yeah.

DR. YANG: And that's the point he's trying to make, is that we take a fail/no fail test and we apply to a situation which occur in the lightwater reactor in a very limited and local condition or area, maybe not condition.

MR. MONTGOMERY: Very local area.

DR. YANG: Yeah, local area.

MR. MONTGOMERY: And as will be shown in the next presentation, not only is it limited axially, but within the core itself is very limited. So there's only very local response within the core. Not all assemblies see the same response, and you'll see that a majority of the fuel assemblies hardly even know that there are an event happened.

CHAIRMAN POWERS: I know that. My question is one of sampling.

DR. YANG: You're exactly right about sampling. Sampling, we don't do that. We just take a very small segment, maybe roughly a block.

MR. MONTGOMERY: Yeah, for the Cabri it's about a block and for the NSR it's one third of a block, yeah, six inches.

DR. YANG: And then you subject the power which is not that shape, not the purple shape.

MR. MONTGOMERY: It's primarily flat with a slight peaking, depending on the test reactor.

DR. YANG: Basically you just hit it with the maximum energy that that purple curve shows.

MR. MONTGOMERY: Yeah, yeah. We're always testing at this location in terms of the power.

CHAIRMAN POWERS: Please continue.

MR. MONTGOMERY: Okay. All right. So based on the limited amount of fuel that's dispersed and the size of the disburse, we don't expect any possible coolant flow blockage that can lead to some coolability concern.

In addition, again, because of the limited amount of material we have a limited thermal to mechanical energy conversion. This would produce pressure pulses typically less than 200 psi from the calculations that we have done and the data that we've analyzed.

And it's really not possible to generate, develop damaging pressure pulses. Now, that's in terms of solid dispersal.

Now, this slide basically says that the major issue here is disposal of molten fuel. So we don't want to get to molten fuel conditions. So we're going to basically skip this slide real quick and just say that what we're going to do is we're going to calculate the conditions necessary to reach incipient melting in the pellet, and we're going to use that as the basis for defining the coolability limit.

We'll see that in one test, JMH-5, which is a Japanese test, it was tested up to about the melting point locally in the fuel. It had some incipient melting, and I'll show you a picture of this rod and it looks very normal, no consequences of this incipient melting.

If we look at the temperature distribution in here we have a little schematic. We see that by limiting this temperature here, the peak temperature to the melting temperature, very conservative because most of the fuel will never be at melting temperature. Well, below the melting temperature. So the majority of the fuel is rather cool and the cladding remains in a solid state. It's not molten.

This limits the mechanical energy conversion. So what we do to determine this coolability limit is to use the melting temperature as a function of burn-up. The data we have I didn't really talk about that, but we have data that shows the melting temperature is a function of burn-up, and then we super impose on that the burn-up distribution and the power distribution across the pellet. We combine those together, again, using analytical capability, and we calculate the enthalpy needed to reach melting as a function of burn-up.

And it incorporates the burn-up effects through lowering the melting temperature and increasing the local burn-up.

This is a nice picture I thought we might like to see. These are three dimensional plots of the temperature within the fuel pellet. We have the pellet radius here starting in the center line going to the surface. We have time here. This is the time evolution, and then we have temperature along here.

What I wanted to point out is the very local effect, and what we're limiting is this peak temperature in here, and you can see that in terms of both spatial dependency, as well as time dependency, that the peak temperature is very, very localized, and the pellet is only there for a few milliseconds, and then the heat conduction begins to drop everything away.

So what we're limiting is this peak point in all of these different plots as a function of burn-up. So what we end up with --

DR. MEYER: Excuse me just a minute. Could I ask? Dana, could I ask just for clarification on something?

CHAIRMAN POWERS: Please, Ralph.

DR. MEYER: Generally, we have always characterized this event by the radially averaged enthalpy, not a local peak. Are you, in fact, talking about establishing a local limit, not a radially averaged limit?

MR. MONTGOMERY: No, we're looking at the radially averaged limit, but the temperature is limited in a local way. We're not limiting the average temperature, which is defined by the radially averaged enthalpy. We're limiting the local temperature, but we're determining the average temperature to give you that local.

DR. YANG: Which is preventing melting at any place within the --

MR. MONTGOMERY: So we're finding the radially average enthalpy that gives you that local temperature that reaches the melting point.

Okay. So the red line here represents the enthalpy needed to reach incipient melting. Again, I'm plotting it as a radially averaged fuel enthalpy as a function of rod average burn-up. This is the result of our methodology.

I'm also comparing it here to -- I'll come back to the Japanese limit in a minute -- some experimental data we have. Most of the data at very high enthalpy is at low burn-up or zero burn-up. There are a few points that are at burn-ups up to about 40 gigawatt days. Okay?

And what I've done is I've separated these into three groups. One is rods that remain in a rod geometry. You look at it, and you say, "That looks like a fuel rod."

Those that had some partial melting, you look at it and say, "Oh, there's some melting on the cladding here in spots."

And this one, where you almost can't tell it was a fuel rod,a nd essentially the test that resulted in total loss of rod geometry are up here in these very high enthalpies, where you know, the whole pellet melted and the rod just became a bunch of little pieces, and rods that remained as a rod in a rod geometry are down here, just at or below the curve that came about, and I'll show you just kind of an example of one of these.

Here, again, here's some unirradiated tests. There are rods that remained in a rod-like geometry. This rod had some melting. I apologize. You can't really see it.

As the enthalpy is increasing we get up to enthalpies where loss of rod geometry becomes dominant, and eventually you get to the point where it looks like a bunch of rubble, and we want to certainly avoid this region here. We want to stay in this area.

Now, if we look at a rod here that's been irradiated to 30 gigawatt days and tested at 220 calories per gram, it effectively looks unaffected except for a crack in it. The cladding did fail. There was some dispersal. It was about five or six percent of the fuel was dispersed out, but you can hardly tell it, notice it in these pictures.

And it effectively looks just like one of these rods, if you can use your imagination a little bit, and we can conclude from this that irradiation has very little impact on the fuel rod appearance at high energy depositions, and that is it looks like a fuel rod. It's not falling apart and is not difficult to keep cool.

All right. So that was one of these points over here that I was just showing the picture. Okay? And we also have for comparison purposes the Japanese coolability limit. That's also based on incipient melting, and then I've shown here just for illustrative purposes the real 0401 failure threshold that's being uses as a limit as proposed by Dr. Meyer.

I guess I'll finally just point out that we do see a number of these points that tend to reside well above this curve.

CHAIRMAN POWERS: Let me ask you a question in these various power inputs that you put in, what's the fission product release associated with those?

MR. MONTGOMERY: Fission gas release during RA events typically vary between five percent and 30 percent fission gas release.

DR. YANG: Are you talking about failed rods or --

CHAIRMAN POWERS: Well, these --

MR. MONTGOMERY: These rods we don't have measurements for. Well, I think we do for a couple of these because they didn't fail, but the ones that failed, of course, you lose the gas, but --

DR. MEYER: Those that you were just talking about, those were failures?

MR. MONTGOMERY: Not all of them.

DR. MEYER: From NSRR?

MR. MONTGOMERY: Not all of them.

DR. MEYER: Just that group of five?

MR. MONTGOMERY: No, not all of these are are failures. Some of these are non-failed rods.

DR. MEYER: And so those are probably low corrosion.

MR. MONTGOMERY: Most of these rods are either lower corrosion or just didn't fail.

DR. YANG: They're just experimental data.

MR. MONTGOMERY: Right. I didn't indicate which ones fail and didn't fail. I guess I should have done that, but I didn't.

DR. MEYER: Well, it just doesn't seem like you have any data out at high burn-up where you could study the loss of material. I mean, all of these other data points, the --

MR. MONTGOMERY: Some of these did fail, of course. That's this one. Well, for example, this one right here, it's a failed test. You can see the crack. You can see the crack here. This is a cross-section. Through the crack you see some fuel material here. You see the crack. This dark line is a crack. That's the crack where the fuel came out.

DR. MEYER: That figure looks like MacDonald's figure from 1980. Are you sure those are NSRR tests?

MR. MONTGOMERY: This or this?

DR. MEYER: No, on the left.

MR. MONTGOMERY: This is NSRR tests as well. It's from 1980. It's from the same --

DR. MEYER: Are you sure that's not a review of MacDonald's SPERT test results?

MR. MONTGOMERY: No, I think it's a review of the Japanese test results at that time.

DR. MEYER: I don't think they ever tested that high.

MR. MONTGOMERY: Oh, yeah. It could be a combination of both. That's where I got the figure from though, is that --

CHAIRMAN POWERS: I'll have to admit it looks an awful lot like MacDonald's figures.

MR. MONTGOMERY: I think they all used each other's figures.

CHAIRMAN POWERS: That could be.

MR. MONTGOMERY: That's the reference that I got the figure from.

MR. MITCHELL: It is NSRR tests. Those are. This is from NUREG CR02 -- this is David Mitchell.

This same figure is in NUREG CR0269, and there's a diagram of Figure 4, test photographs of

NSRR STD rods tested in the NSRR.

DR. YANG: So it is NSRR. It's just in the MacDonald's report.

MR. MITCHELL: Well, he put everything together in his report, all the known data at that time.

MR. MONTGOMERY: And this test was done 15 years-odd later. This was probably mid to late 1990 type test, 1997, '98. I don't know the exact date.

DR. YANG: Robbie, why don't you address some of the points earlier?

CHAIRMAN POWERS: I would definitely like to move on to the questions about the scaling analysis because somehow melting fuel with these pulses just leaves me cold.

MR. MONTGOMERY: Okay. Well, all right. So some specific issues with the scaling method. I've got four points here.

The first one is that we feel that there has been an incorrect characterization of the cladding failure mechanisms and the changes in cladding ductility. Really the effect of hydrogen content and hydride distribution on the cladding, the idea that the spalled rods did not fail as a consequence of the spalling and the hydride localization.

In addition, the effect of temperature on cladding ductility and the assumption that there is no temperature effect or that the temperature effect doesn't apply during RA condition, we have some questions about that as well.

DR. SHACK: But didn't you do the same thing? I mean, you use a CSD.

MR. MONTGOMERY: CSED?

DR. SHACK: It's independent of temperature.

MR. MONTGOMERY: Yes, we do. We do do that. In terms of what Ralph is doing, his temperature dependency really comes from going from room temperature to 300 C. He didn't take into account that effect, but his effect really comes from the post width, but no changes in the material capability from room temperature to 300 degrees C.

Remember he said his uniform elongation is temperature independent?

DR. SHACK: But I thought he started at his base temperature. That's what I interpreted him to say, that he used the 300 C. temperature for a test started at 300 C. He used 175 for a test that started at 175.

MR. MONTGOMERY: But in terms of

translating from room temperature to 300 degrees C. he used the same parameters at room temperature as 300 degrees C.

DR. MEYER: I did not adjust, make an adjustment to the deduced failure of strain for the difference between the test temperature at 20 degrees C. and the PWR temperature at 300 degrees C. because the uniform elongation data that were examined by Rob Daum up at Argonne in that temperature range showed no significant temperature dependence.

When I analyze it with a temperature dependence from a total elongation data, I got a big effect. Now, I did it both ways, and I told you in the end why I preferred to go with the uniform elongation. So I have done it both ways.

MR. MONTGOMERY: Okay. The second point. Consideration of factors in --

CHAIRMAN POWERS: Maybe e can explore this first one because I'm left a little confused.

MR. MONTGOMERY: I'll go into that in great detail.

CHAIRMAN POWERS: Yeah, that's what I'm afraid of. I'm going to lose where we're going. Maybe we can walk through each one of them or something like that.

DR. YANG: Maybe we can go through each one rather than go through --

MR. MONTGOMERY: Sure. Two, three, and four, in the interest of time, wasn't going to be addressed.

DR. YANG: Go through those.

MR. MONTGOMERY: Okay. Then we won't go to two, three, and four.

Okay. The first one is within the characterization of the failure mechanisms, the effect of hydrogen content and hydride distribution on the cladding ductility, that's really the spalled rods that were used in the RIL effectively generalizes the behavior of spalled rods to non-spalled rods. The limit is based on REP-Na8 and 10.

But what we do know from mechanical property tests is that the mechanical performance of cladding with spalled oxide layers is worse than non-spalled, but highly oxidized material, and that comes about because of these localized hydride lenses that accompany the spalling, and they impact the overall material strength and ductility, and I'll show some slides on that.

Now, we've seen that in both two burst tests. Rosa showed you some of that test this morning, and I'll go through that in a little bit more detail, and then also in these ring tension tests.

Specifically, --

CHAIRMAN POWERS: I mean, just a question. There are lots and lots of defects in a rod after it has been irradiated for a while, and presumably lots of those defects will have some impact on the mechanical properties of it. Maybe the experimental program hasn't parsed it down, but unless we can do something that says we will only have reactivity initiated accidents around specified types of flawed rods, don't you want to at this level of resolution kind of average those defects into your database?

DR. YANG: I think maybe, Dr. Power, spallation is a rare phenomena.

CHAIRMAN POWERS: Yeah. Well, it's rare nowadays. It didn't used to be.

DR. YANG: Yeah, a long, long time ago maybe you have more spalled rods.

CHAIRMAN POWERS: Well, in fact, last year we had some spalled rods pulled. I can't remember what reactor it is, but I mean, these things come to me every once in a while. I mean, it happens.

DR. YANG: With all of the current cladding we're using, we don't see spallation, and just to remind what I said, these test rods, they were the first test, and we want high burn-up rods. So although we recognize they spalled, they were put back into the reactor for one more cycle of irradiation.

So you know, it isn't phenomena that we recognize it degrade the mechanical property and we have done our best to avoid, to eliminate this problem.

And I think Robbie is going to show that the mechanical property, indeed, are significantly different.

CHAIRMAN POWERS: Yeah, but you just do not want to come in here and end up with a requirement that says, okay, here's what the energetics you can take, but thou shalt not have spalled rods, because there's just now way to guarantee that you won't have a spalled rod.

DR. YANG: Well, you could say if I had spalled rods I would apply a different criteria. You could do that, and in fact, that's what Ralph and --

CHAIRMAN POWERS: You could always end up applying -- that would become the limiting criterion, but you could never guarantee you wouldn't have spalled rods unless you pulled the rod every day and check it. I mean, that's the problem you'd get into.

You just don't want to do that.

DR. YANG: Yeah. Well, we are not doing that for normal operation, and this is a Class IV event. I mean, I recognize --

CHAIRMAN POWERS: I just don't know how you'd write an FSAR that did say, "I will never have spalled rods in this reactor."

I mean, I could write it, but why would anybody believe you? If we knew how to predict spallation all that accurately, we'd probably get rid of it, and I'll admit they've done a good job.

DR. YANG: -- got rid of it.

MR. MONTGOMERY: We've gotten rid of it. There are a number of different ways.

Okay. Well, let's just talk about -- we'll come to that point. Your point about defects is a good one, and we'll come to that point when I talk about the mechanical properties, which is just a few more slides, where all of the samples that we have are from irradiated rods. They're from real rods, and they will contain the defects that are in them.

And what we've identified is that there are defects in these samples, but the ones that are the dominant defects is the spalled and hydrided rods.

CHAIRMAN POWERS: Yeah, but see the

trouble is it's not clear to me that they are the dominant defects. They're the defect that you picked on and focused on probably because it's very detectable, but I suspect that if I went in and looked, if I was very, very perspicacious and went and looked, I would find that every single one of the cracks formed at something that could be attributed by someone as a defect.

MR. MONTGOMERY: And the point that I'm trying to make is that the mechanical property database includes the best representation of the defects that we can because we've gone in and taken samples out of rods, lots of different rods and done mechanical property tests on them, and they have whatever defect there was in them, small, large, incipient cracks, noninsicipient cracks or whatever.

But when you look at the data set, you can separate the data out into two data sets, those that behave consistently one way and those that behave consistently another way. And those that behave consistently in the lower range, as I'll show in a minute, can be directly correlated to spalled rods and hydride lenses.

The rest of the data set has its defects, and whatever they are.

CHAIRMAN POWERS: And I say I can go in there, and if I was willing to spend time, I could probably parse that data set even further.

MR. MONTGOMERY: And that's a possibility. That is a possibility.

CHAIRMAN POWERS: Do you really want to do that or do you want to say, "Okay. I'll live with what I've got"?

MR. MONTGOMERY: All right. Well, let's just talk a few minutes about REP-Na8 and 10 and the effect of spalling. I'm not sure this is exactly where we want to go, but that's what we're going to do. It's what's in the slide.

What we see is that pretest neutron radiographies were done on both of these rods, and there are a number of hydride lenses that are identified through that process, and they were able to actually create maps of the hydride lenses,a nd what you see is that there are a number of them, and especially in the peak power region during the following on experiment when it was run.

In addition, there's post test examination. Rosa showed you probably the best picture of that that indicate the declining cracks initiated at hydride lenses. However --

CHAIRMAN POWERS: Well, that was singularly nonpersuasive because --

DR. YANG: Because of Ralph's chart.

CHAIRMAN POWERS: -- I've got another plot, another figure that if it weren't labeled, I would be hard-pressed to distinguish it from the REP-Na8 that it was argued didn't initiate it, a hydride lens.

MR. MONTGOMERY: That's my next point, and that is that it's really difficult to interpret these results. You have to really dig deeply because of the fact that these cracks grew quite a bit in the hot cell, and that's what the next slide shows, is that what you have here is a plot of the crack opening displacement. They just looked at the rod in the hot cell, and they said, "Oh, the crack is X wide," you know, so many percent, so many centimeters or millimeters wide, and they plotted this as a function of the axial position along the rod, and this test was done in roughly June-July time frame of 1998.

So approximately five, six months after the test they saw a crack in this range, this red range. Okay? The crack tips ended here and here, and it was so wide.

And then come back in about six months

later, and now it's grown, and now here it is. It's going from here to here, and then another year later, I guess another eight months later, here it is. This is how wide it is, and this is how much it grown.

So it basically more than probably ten -- it grew ten times more than it started out when it was right at the completion of the test or during the test.

What I have here is a map of the hydride lenses that were observable in the neutron radiography, again plotted kind of as a function of axial position here, and this dimension here is asmuthal (phonetic) positions. You can kind of consider this going from zero to 360 or 180, something like that.

And you can see that there are hydride lenses in this region. We can't really qualify them as hydride lenses exactly. Maybe they're hydride localization in these dark spots here, here, and in this region here and here.

The metallography sample that was taken was taken right in this area that Ralph showed, and it turns our that it was very near where the crack initiated, but it wasn't at the crack or the initiation site.

There's more details of that than a lot of metallograph and analysis that was performed to look at that particular sample, and it was fairly easily concluded that that sample does not represent the crack initiation process.

DR. YANG: I was going to say IRSN.

MR. MONTGOMERY: It is an IRSN assessment, yeah.

CHAIRMAN POWERS: Well, I mean, I look at the picture and it looks to me like the crack is as far away from the hydride blisters as it can possibly be.

DR. YANG: It's a three dimensional.

MR. MONTGOMERY: Right, right, right.

CHAIRMAN POWERS: Well, the picture is two dimensional.

DR. YANG: Yeah, our picture, but the rod is three dimensional.

MR. MONTGOMERY: Three dimensional. So we don't know if there are -- the resolution of a neutron radiograph can, of course, be questioned. It can exceed that small of a hydride lens that's on that picture. So they're not even evident here, or is it seeing bigger and smaller? We don't know= exactly. So what we do know is that it initiated somewhere other than that crack, other than that location. It more than likely initiated in a hydride blister, but when it began to grow, it grew outside of that hydride blister and just continued to grow in the virgin material, and if you look very -- you can't see it in that, but IRSN has gone in and done more image analysis of that, and there's radial hydrides that formed during the post test cool-down of this experiment, and you see lots of radial hydrides in the region of where that crack is, and the only way that those radial hydrides could be there would be if the crack wasn't there during the cool-down phase.

And then once they formed and they cracked during the sodium ingress phase and relieved the stresses, but you had to have stress to form the radial hydrides that are in there. You can't see them in that picture. I didn't come prepared to talk about that unfortunately. So I don't have a plot of that with me, but I can get you that information.

All right. Rosa showed you this slide already. This one is a slide showing the ultimate tensile strength plotted as a function of sample average hydrogen content, which could be related to the oxide thickness in a way, and we have the non-spall material and the spall material.

I can point out that the metallographic images of most of these samples find that the hydride lens is about 50 percent of the wall in these samples, and that's consistent with about a factor of two decrease in the ultimate tensile strength.

If we look at plastic elongation from ring tension tests versus sample average hydrogen content, again, we see a separation between those samples that did not have hydride lenses and oxide spalling and those that did, about a factor of four to five in the elastic or the plastic capability of elongation.

These are just some examples of mechanical property tests. This one happens to have a hydride lens residing here. It fractured and then broke. We see here a sample from the same fuel rod but in a region where there wasn't oxide spalling and hydride lens formation, and we see that there's about a factor of four in the elongation difference between these two. You can see the necking here in this region and the thinning of the wall here. It's pretty significant classic deformation capability.

I'm just contrasting. Then you saw a little bit of this picture earlier. Rosa showed this REP-Na8. Again, we have a hydride lens region here, the fuel pellet and a crack, a brittle type crack here and then a ductile sheer failure there, fairly consistent with the mechanical property tests.

This is just -- I won't go through this in the interest of time, but these are some excerpts from various researchers' papers that talk about the effects of spallation and hydride lenses on mechanical properties and cladding failure potential.

DR. SHACK: Rob, would you agree that the total elongation is sort of a specimen property as much as a material property?

MR. MONTGOMERY: In a way, yes.

DR. SHACK: Then do you think a ring tension test is a reasonable thing to use to represent the deformation you're going to see in this?

MR. MONTGOMERY: The ring tension test has its pluses and minuses in terms of using the mechanical properties.

DR. SHACK: Wouldn't uniform elongation be much more of a material property?

MR. MONTGOMERY: In terms of uniform elongation and its reference to a material property, it also is a bit dependent on the specimen geometry and specimen design, and it really comes from the stress-strain curve in an engineering space, and it happens to be the point of maximum stress, but in terms of a material property, if I'm going to derive a constitutive law, I don't ever have a uniform elongation point because if I mull in the material very accurately and derive that, the stresses are just increasing.

But so we see in terms of application, and you know, this question about uniform elongation, total elongation has been around for quite a while, that if you look at the experimental data and if you're using a uniform elongation approach, not mechanical property, but if you go to fuel rod data, RA experiments, you see that the cladding exceeds the uniform elongation under RA conditions and very large power ramps in test reactor conditions fairly well without failing if it has ductility in the material.

So using uniform elongation as a failure parameter doesn't necessarily represent what's going to happen in the reactor necessarily. Once material becomes extremely brittle like here, you see very little difference. That's uniform elongation.

DR. SHACK: That's totally different.

MR. MONTGOMERY: There's no difference, but when you get to this reality, failure is somewhere between the uniform elongation value which is here and this total elongation value there, and designing a mechanical property test to bring those together that represents a fuel rod is the difficulty we have.

DR. SHACK: Well, one of the things that seems to indicate is you've lost your dependence on hydrogen content again. I mean, unless I assume an exponential curve again and fit it.

MR. MONTGOMERY: And the ring compression test because of the very local behavior that you have, it has only got a slight --

CHAIRMAN POWERS: It's going to be --

MR. MONTGOMERY: There's a very slight dependency on hydrogen.

CHAIRMAN POWERS: There's no dependency at all on that.

MR. MONTGOMERY: Okay.

CHAIRMAN POWERS: That's a variant with hydrogen.

MR. MONTGOMERY: Okay. I don't know if I addressed your question or not.

CHAIRMAN POWERS: Well, I'm getting more confused, I think.

DR. BILLONE: No, Bob, I understand why you use total elongation, but Bill has a point. It's something that's highly dependent on your test apparatus, test geometry. It's not even a mechanical engineering material property.

But if you don't have true stress-strain from the test, if all you have are these engineering parameters, one sometimes argues that total elongation with trends in it might mirror the trends or be a lower bound on what the true plastic strain is localized at the failure point.

MR. MONTGOMERY: The total elongation or uniform elongation.

DR. BILLONE: Well, the total is closer. I mean, the total is -- you're averaging over a gauge length.

MR. MONTGOMERY: Right.

DR. BILLONE: And you're localizing, and so where you're localizing you're getting a higher strain than what you were averaging over the whole gauge length, and total elongation is supposed to be somewhat indicative of that.

However, as we pointed out, you can go from specimen geometry to specimen geometry, lab to lab, and you can get variations on the order of 30 percent for a nonirradiated material in that. So it's far away from a material property. It's used for convenience because if that's all you have, that's all you have. And its true uniform elongation is too conservative. With displacement control loading you can certainly go displacement control loading.

MR. MONTGOMERY: Yeah.

DR. YANG: Which is PCMI.

MR. MONTGOMERY: PCMI loading.

DR. BILLONE: You can go beyond.

MR. MONTGOMERY: Beyond uniform elongation. Okay.

DR. SHACK: That's the best argument I've heard, is that we really are in a displacement controlled loading situation.

DR. BILLONE: I just had to talk a long time to get to it.

MR. MONTGOMERY: I should have said that. That's what I meant when I was talking about it.

DR. SHACK: That makes a big difference.

MR. MONTGOMERY: Yeah, yeah, in terms of displacement control loading, which is the PCMI, that's what I meant by fuel rod conditions. You can go beyond uniform elongation. Thank you, Mike.

DR. BILLONE: That just took me a long time to get there. I'm sorry.

MR. MONTGOMERY: You got there. I didn't.

Okay. Let's talk about this for a few minutes in terms of that declining temperature as a factor on fuel rod behavior and understanding it. I think we've gone over most of these point. There are differences between the hot test in Cabri that are done at 280 and the NSRR tests that are done at 200. We must acknowledge those and try to use that information to interpret experiments.

What we do know is that temperature does affect mechanical performance during PCMI loading. Typically you have fairly low material ductility at low temperature for a number of reasons: lower hydrogen solubility, and you also have less ductile zirconium hydride platelets in irradiated material. You have some hydrogen in there.

The NSR tests are done at room temperature, and not only that. Well, that leads to less ductility, but also, the very narrow pulses in these tests allow for very little heat-up of the cladding. So the cladding is at failure generally right around room temperature because of the four millisecond pulse width.

In order to understand these cold temperature tests, we need mechanical properties and a failure model to be able to interpret the rests first and then translate to higher temperatures. And we don't think that the RIL did that in an appropriate manner.

Here's some data done by Penn State. Again the plain strain test where we're looking now at the fracture strain as a function of hydride blister depth. We saw that a little bit this morning, just a representation of the hydrogen content or the hydrogen morphology, and we have two different trend lines, one with 300 degrees C. and one at room temperature. So you see an improvement of the fracture strain between these two of about a factor of one and a half or two.

And another example of an effective temperature. These are some interesting test specimens that are done at Studsvik. These are expansion due to contraction tests, where they take a piece of cladding tubing sample and use a polymer plunge where they compress the plunger. The plunger expands against the cladding and loads the cladding and is able to load it in a way that's somewhat PCMI related.

And we have here plotting the maximum hoop strain versus test temperature here, and we're starting out near room temperature going through to about 150 C., 160 C., and then we see an improvement in the material ductility as we go beyond that temperature range, and what we see here is that at low temperature you typically get a fracture and fairly low strain.

The expansion process has a limit that can't expand forever, and here the material expands up to 20 percent with out fracture in these higher temperature tests.

Again, showing the effect of temperature on material ductility.

DR. BILLONE: I think that only applies to radiated BWR cladding. I don't remember that ever being seen with the PWR.

MR. MONTGOMERY: This happens to be for BWR cladding. I don't know if they've done these tests yet for PWR cladding.

MR. MITCHELL: Yes, we have.

MR. MONTGOMERY: You have?

MR. MITCHELL: This is David Mitchell from Westinghouse.

We had four EDC tests done at Studsvik on irradiated ZIRLO. It has about 550 ppm hydrogen in it, and we were unable to --

MR. MONTGOMERY: Oh, that's right. You were unable to fail.

MR. MITCHELL: -- fail at temperature.

MR. MONTGOMERY: Yeah, at these

temperatures, they were unable to fail them.

MR. MITCHELL: And at room temperature we did fail them.

MR. MONTGOMERY: Yes. Those are about to be published, I guess.

Okay. Just a point here in terms of PCMI loading. The type of loading we would expect, the maximum, just kind of a frame of reference is we need this kind of strain capability to accommodate 180 calorie per gram energy input roughly. It's the rule of thumb

MR. MONTGOMERY: All right. I'm at the end here. So just a quick slide.

In terms of our differences or our issues with the RIL with respect to the coolability limit, in RIL 0401, coolability limit is based on precluding fuel dispersal. No failures are allowed during an RA event, Category IV event. That's pretty restrictive and unprecedented.

We see that there are many tests that maintain raw geometry. They reside well above the limit that has been discussed by Dr. Meyer in the RIL. We feel it's an unrealistic lower bound and overly conservative by at least a factor of two, and as I'll show in the next couple of slides I may have to skip, I think they're there. They certainly could impact plant operations and will require improved neutron kinetics methods.

I've already gone over this. The industry proposal is to limit the enthalpy based on fuel melting, develop a coolability limit that way. It represents the high energy tests that have been conducted in Cabri and NSRR and is in agreement with what others are doing out there.

The next slide here is just a quick comparison. These are 3D neutron kinetics calculation, comparing the code calculated results for the neutronics to the various type of methodologies that are out there from the RIL up to the proposed industry thresholds and limits.

That's for four loop Westinghouse plant. We have a similar --

DR. DENNING: Quick question, and that is from a regulatory viewpoint is the only thing that matters coolability limit? Does failure threshold enter into like a 10 CFR 100 analysis?

MR. MONTGOMERY: Yes. Yes, it does.

DR. DENNING: And so the number of pens that you fault affects the efficient product release. So is it an important one or is --

MR. MONTGOMERY: The failure threshold is used for, as you said, dose calculation, and this is less of importance in terms of plant restriction or restriction of operation as this one. This is the more restrictive one.

Similarly, for a three-loop plant.

So I'm done here. It's a summary. I think I've demonstrated that we have the revised threshold that are the function of burn-up that include the controlling factors that control cladding that kill the corrosion and hydriding and how it evolves with burn-up and also the burn-up effects on UO_2 melting.

The criteria have bene defined in terms of radial average peak fuel enthalpy as a function of burn-up. That's applicable to hot zero power conditions. We feel it can be directly used and core reload design calculations, pretty consistent with the current practices that are used out there.

Just a small point. For hot, at power conditions DNB still remains the limit for or the threshold for failure. These two slides basically say what I've already said. So I'm effectively done.

CHAIRMAN POWERS: Let me see if I have come away with a correct perception here. The perception is the last time we met we persuaded ourselves the REP-Na1 could be discounted from the database. Now we meet and the perception is that the two tests with spalled oxide, if not excluded from the database should be separately categorized; that the two tests from SPERT are of dubious value; that the MOX data point ought not be included in the data set.

And if I do that, is not the upshot of this that we have no acceptable data for high burn-up fuel?

MR. MITCHELL: You have survivors.

MR. MONTGOMERY: We have all of the survivors that are out there.

Thank you, Dave.

The database includes both failed and non-failed rods. We've learned many lessons from the failed rods, as well as the non-failed rods. I don't have the plot as a function of burn-up. So if I could bring --

DR. YANG: It's in my chart.

MR. MONTGOMERY: There are many rods that are at high burn-up.

CHAIRMAN POWERS: Okay. What you, in fact, have is two data points as I count them from the Cabri program that survived in the high burn-up range.

MR. MONTGOMERY: Well, it depends on what you define by high burn-up, but you have three rods above 60, REP-Na4, REP-Na 5 and REP-Na11, and CIP-01, CIP-02 above 70.

Let me bring up Rosa's slides for a second. Her's are still here.

CHAIRMAN POWERS: Well, I've got them over here.

MR. MONTGOMERY: Yeah.

CHAIRMAN POWERS: So we have no failed rods in the high burn-up region and some surviving rods.

MR. MONTGOMERY: They were here at this slide. What was that, five?

CHAIRMAN POWERS: Like I say, I've got them over here.

MR. MONTGOMERY: Just so that everybody else has them.

So what we're talking about here are these. You have still all of this data out here. You have rods from NSRR. What we're saying is that these failure mechanisms that define these failure have to be evaluated and understood and determined in terms of relevancy with regards to establishing a failure threshold.

DR. MEYER: Please keep in mind that by plotting these data points as a function of burn-up that you're throwing out there in the high burn-up regions some specimens that had very low oxide levels on them and some -- well, you see the test energy. So some of those are fairly small, but if you do look at the --

MR. MONTGOMERY: That's real life.

DR. YANG: Because you cannot in more energy. We are struggling how to fail the rod in the Cabri water loop. That's the maximum energy input.

DR. MEYER: If you would narrow the pulse to the appropriate width, you could get more energy in it.

DR. YANG: In 2011 we may do that.

CHAIRMAN POWERS: Well, now, let me ask Professor Denning's question. If we looked at those survivors that remained out there as intensively as we looked at the non-survivors, would we find reason to exclude them from the database?

MR. MONTGOMERY: Most of these are in the validation base. They're not -- I think they're equally scrutinized, but you probably could find to say --

CHAIRMAN POWERS: I guarantee you I could find something.

MR. MONTGOMERY: You could find something.

CHAIRMAN POWERS: I can always find something.

MR. MONTGOMERY: That one has a wart in the wrong spot, you know.

CHAIRMAN POWER: Yeah, I mean it's just more difficult.

MR. MONTGOMERY: But they're about as representative as rods. Again, they've been pulled from high powered rods that have operated for --

CHAIRMAN POWERS: Yeah, but if you use that criterion, then you end up putting the others back in because they're from high powered rods that are pulled from reactors and things like that.

MR. MONTGOMERY: Well, yeah, and not necessarily in a way, particularly of REP-Na8 and 10. They were from a program that was looking at fuel behavior and didn't really -- wasn't so interested in cladding behavior. So the cladding was sacrificed in regards to give burn-up. So trying to define if those rods represent all rods that operate the 65 gigawatt days' burn-up needs to be questioned.

DR. YANG: There was a cladding made maybe 20 years ago to get that --

MR. WAECKEL: This is Nicolas Waeckel from EDS.

I confirm that these rods are coming from EDS power plants, and this is part of the high burn-up fuel program back to the '80s, and the target of these experiments were mainly to study the true behavior as a pellet. The microstructure changes with burn-up, not at all to cladding.

We did know when we reloaded this fuel rod that the cutting was spalled. We knew that, but we took the risk to put them back just to have the amount of burn-up we wanted to reach with the pellet.

So it turns out afterwards that they were the only rods available at that level of burn-up to be testing Cabri. That was a mistake. So cladding itself was a very high tin content cladding, a very old design, not the right heat treatment and surface finish and so on.

So these rods and the set-back and we've brought that so many times. I'm not the representative of any current fuel rod design.

MR. DUNN: Dr. Powers, this is Bert Dunn from Framatome. I'd like to add a comment.

CHAIRMAN POWERS: Please.

MR. DUNN: Thank you.

You asked a question earlier about why wouldn't we just subsume spallation into the database in order to not have to predict it.

CHAIRMAN POWERS: True.

MR. DUNN: Well, the vendors are out there making a very strong effort today to develop cladding materials that will not be subject to spallation.

CHAIRMAN POWERS: They are.

MR. DUNN: And we are post irradiation testing those claddings to show that they aren't subject to spallation. So I believe it would be possible to show that we won't have spallation with post radiation examination. We may not have as much data as we'd like today, but, for example, on our cladding we have not seen any as of yet.

In terms of the criteria we're talking about here or the PET, it has an opportunity to impact the cycle design, the fuel design, the fuel handling or the way the fuel is burned. It could be difficult in that way for the utility to get there. So we'd like to avoid it for that.

It's going to be difficult to match the real proposal for the fuel damage. So we'd like it up a little bit. In particular, we'd really like to make sure we get the core coolability limit for those plants that want to go ahead and do a dose calculation.

And I would only add one other thing is that when we start limiting the ability of the cycle design in terms of rod worth to the extent it will be necessary here, we have the opportunity of coming up against other safety goals than the NRC may be wishing to do, and we should be very careful in doing that.

The thought I have, and I can't prove this one way or another today, is that we may -- something like pressurized thermal shock where we're definitely trying to prevent fluence out on the reactor vessel and we are trying to peak activities in the core may come into a rod worth situation. We might wind up against that some time.

Thank you.

CHAIRMAN POWERS: Good points.

Any other questions for the speaker?

(No response.)

CHAIRMAN POWERS: Robbie, as always, highly informative, data filled presentation. I enjoyed every minute of it.

MR. MONTGOMERY: Thank you very much, Mr. Chairman.

CHAIRMAN POWERS: Let's see. I think --

yeah, why don't we go ahead and do that? Take a break.

Robbie as usual overloads me with information. It gives me too much for me to absorb all at once, and so we'll take a break. Let's go to 3:30.

(Whereupon, the foregoing matter went off the record at 3:12 p.m. and went back on the record at 3:34 p.m.)

CHAIRMAN POWERS: Let's come back into session.

I think, Mr. Mitchell, you're going to clarify all of this stuff for us, right? Straighten it all out.

DR. KRESS: Clear it all up.

MR. MITCHELL: Or maybe give you an added source of confusion.

CHAIRMAN POWERS: If that's all you're proposing to do, you can sit down right now. We have reached saturation.

MR. MITCHELL: Okay.

CHAIRMAN POWERS: All you can accomplish in doing is shifting our confusion around in different areas. You can't add to it.

MR. MITCHELL: Okay. I did this in Adobe

Acrobat because every time I change the machines my fonts run off.

Anyway, what I'm going to talk about this afternoon is our comments on proposed reactivity insertion accident criteria. And I'm David Mitchell. I'm an engineer with Westinghouse down in Columbia, South Carolina, and Charlie Beard is here who's from Pittsburgh, and we'll be going over our areas of concern.

And when we looked at some of the proposed criteria, both here and some other sources, we issued a letter to the NRC which addressed these specific areas, and some of these you've already heard about, of course. One is the use of the objective rod worth as a limit, collapse of the fuel coolability limit on the cladding failure limit, the probability of high energy RA events, the use of local oxide thickness to set general RIA limits, and the reliance on NSRR data.

Now, in these two areas here, I'm going to later present a sample analysis. We're basically going to go through an actual reload core and show you where the impact of the worst case ejected rod would be, and what the resulting energy depositions would be.

Our first comment was on the use of

ejected rod worth as a limit. Now, we agree that the ejected rod worth is the key parameter associated with the reactivity insertion accident. However, the limit should be based on physical phenomena of the event as related to safety, and we believe the existing criteria on the fuel enthalpy is a more appropriate parameter that encompasses the effect of the ejected rod worth, along with a number of other parameters that are also important.

And obviously one of the big ones you have seen is the use of corrosion or the translation of corrosion in to equivalent burn-up that's been presented.

The other thing that we believe is that the criteria should provide for differentiation between the fuel failure limit and the coolability limit and provide for the calculation of an appropriate dose based on the amount of possible fuel failures.

The ejected rod accident, which is the accident of merit that we use, is a Condition 4 event, and so the criteria should be similar to other Condition 4 events where you do not necessarily preclude fuel failure, but you calculate the off-site dose based upon the accident.

DR. KRESS: When you calculate this dose, what do you assume about containment, that it's leaking at its design leak rate?

MR. MITCHELL: It would be based upon whatever was the analysis of record.

CHAIRMAN POWERS: I mean, that's nearly always going to be the design basis.

MR. MITCHELL: Yeah, the design basis accident.

And in order to do these, it basically requires a limit on the local fuel parameters, not a global core parameter, such as ejected rod worth.

Now, we'll reiterate on this point a little bit. The collapse of the single RA limit based on cladding failure threshold is proposed in the reel, and the basis for this proposal was the assertion that failure of high burn-up fuel could result in fuel dispersal with adverse system impact, including a pressure pulse.

Now, Westinghouse believes this is not justified. We went back and looked at the experimental results in the NUREG CR-0269, and what we saw is, you know, EPRI developed this limit with the industry based on fuel melting, but just strictly looking at the experiments that were done, we said there's only small pressure pulses from energy depositions of less than 170 calories per gram.

And we also looked in a commercial PWR. The core volume that is within 80 percent of the peak RA energy is small. It's less than one percent of the total core volume. So you're going to have a small impact on system pressure.

When you look at these experiments and look at where basically the capsule is relatively small compared to the rod being tested and it was a very small pressure pulse in that, and then you look in a commercial PWR core where the overall volume that's going to be at high energy is going to be small, your impact, the pressure pulse that you're going to get from that is also going to be quite small and well within the ASME faulted limits for the pressure vessel.

Now, this is taken from NUREG CR-0269, and they had a term in there -- we kept this term -- called "pellet surface energy deposition," because they looked at it for a lot of failure phenomena and pressure phenomena, and it's just basically a measure of the energy available at the pellet's surface, which is to be transferred either to the clad for clad melting or into the coolant for pressure effects.

So we kept that same terminology. Now, roughly the correlation is 170 calories per gram radial average deposition, is approximately 250 calories per cubic centimeter pellet surface energy deposition, and so that's, you know, right there.

And here you have the capsule pressure, 100, 200, 300, 400, 500 psi. So we see we've got less than 200 psi in these experiments at reasonably high energies. Obviously we're not near the melting point of the fuel or anything, but even under these conditions, and this would be something that we could live with as a coolability limit and somewhere in this area. You have a very small effect.

Now, the question comes up, is those experiments were not done with extremely high burn-up fuel. So the question is: what is the impact of burn-up on the pressure pulse?

We've obviously seen earlier the decrease in fuel melting temperature results in lower energy for the onset of fuel melting. Coolability drop with burn-up, and that was documented in the industry topical and a similar set of criteria was developed by the Japanese.

But let's look at what really goes on in the commercial PWR. When you look at it, your most limiting fuel in terms of possible pressure pulse is likely the mid-burn-up, and this isn't the middle in terms of the thing. I'm really talking about second cycle fuel. So it's you get two large burn-up increments in the first and second cycle, and then what fuel is reinserted for a third cycle gets maybe 10,000 megawatt days in its third cycle.

So fuel in this range that at the end of cycle has between 50 and 55 gigawatt days burn-up on it, this is when you're likely to get the greatest rod worth. And I'll show you some of that later from the sample case. This is when you have the most rod worth, and you'll also get the higher energy deposition in a rod ejection accident.

So this fuel still has enough peaking factor to reach significant energy levels at end of cycle in a rod ejection accident, and it's going to have some dropoff in cladding capability due to corrosion.

However, if you look at where the peak energy pulse occurs on the fuel, your burn-ups aren't that high. At that burn-up you have very little rim formation at the peak energy location. The rod internal pressure, which is something we worry about that it will increase with burn-up, and we're allowed to operate above system pressure, but the rod internal pressure at hot zero power is typically going to be well below system pressure because you're operating at zero power as opposed to being up at five kilowatts per foot rod average, whereas you'd be calculating a no clad liftoff type of criteria.

So you're going to have a delay in the heat-up of the plenum with a rod ejection accident, and you've got some physical distance from the plenum to the cladding failure. And so all of this would basically tend to delay fuel expulsion, any large scale fuel expulsion.

The result is what you'd have is a limited ejection of very high temperature material with clad failure in a rod ejection accident and a small pressure pulse. The highest burn-up fuel is limited at peaking capability. At the end of cycle where you're going to have your highest rod worth and your highest deposited energy, the highest burn-up fuel is pretty dead. Basically it has given its all.

So that leads us into the next one, and that's the probability of high energy rod reactivity insertion accident event. The reactor's conditions needed to obtain the worst case energy depositions in an RA are very limited. You have to have a certain control rod insertion at hot zero power needed to achieve the maximum energy deposition, and actually would not be expected in typical operation of hot zero power conditions.

However, in a typical reload analysis we have to analyze for these conditions. We have to look at the limits of rod program and the limits of the control bank insertion and say what would happen in this case. But actually in a typical reactor you may never operate at those limits, but they are part of your tech spec limits, and so you have to account for them.

The other thing is, once again, only a very small volume of the core is within 80 percent of the peak enthalpy, and that also brings us into proposals where the use of local oxide thickness to set general RIA limits.

In a lot of our modern core designs,we're operating in ow leakage, fairly long cycles. The interior of the core is basically one and two cycle fuel, and the third cycle fuel that would have the maximum oxide thickness is placed on the core periphery, and in those type of core designs, the energy deposition in an RIA event would be relatively small, and you k now, even accounting for changes in the capability with burn-up or, you know, burn-up being used as an equivalent of oxide and high cladding hydrogen would be well within its capability.

And so to show you this, we'll go through a sample analysis. Now, we have a topical that you'll see on the end of this there's references. So we performed this using our approved topical on 3D rod ejection accident with our methods, with realistic core design methods. This is a three-loop core, 157 fuel assemblies in the core, a 17-by-17 OFA rod array, and what that is is the rod diameter is .360 inches. So it's smaller than the original .374 inches used in the 17-by-17 array.

We looked at the worst case ejected rod at hot zero power, and then we looked at the relative energy deposition throughout time in cycle at various control bank insertions as a function of core position both in the radial direction and in the axial direction.

Now, here's the core map that we use, and here you see where there's a letter in the assembly. This is where a control cluster sits. Now, the SB and SA, these are the safety banks. These are pulled all of the way out. Obviously if you ejected one of those rods, you still have a serious accident, but you're not going to put any energy. That rod is basically parked out. It's part of the scram system for the reactor safety controls.

The D bank has the highest worth and the D bank in this location has the highest worth here. Notice we're sitting on the core periphery. This is sitting in a fresh fuel assembly, and then the fuel assemblies next to it, adjacent, these are third burn assemblies out here.

Now, here what we have in this plot is a normalized ejected rod worth. So we have everything is taken to the maximum rod worth. That's set equal to one.

Now, in this actual case, the maximum rod worth was about $1.70, but we have cases where we've looked at that go up to about $2.25. But this shows basically this is beginning of cycle here. This is the end of cycle here. You can see everything increases towards the end of cycle. Your maximum rod worth occurs at the end of cycle, with the lead bank inserted all the way in core, and that makes sense.

Basically the further in core the rod is inserted before it's ejected, the more worth it has, and so you can see here a lot of the operating space, you're going to have very low rod worth, and this is where you're typically going to be operating, out in that type of area.

But if you look at the tech spec limits, if you look at the conditions we have to analyze for the core design, in very narrow areas you can have quite high rod worths even though a given plant might never operate there during a cycle, but we have to make provisions for them to be able to look at that particular set of circumstances.

Now, if we then eject that rod at the end of life, so we've rejected the rod there, and we've made this a little bit -- we're renormalized this. We've set this entire assembly here equal to one. Actually this assembly average here I think is about .92 compared to the worst case rod, but basically to be able to show this conveniently, we've normalized this to one.

And so you can see even though this is at one, the third burn assembly next to it is at basically 61 percent of the energy of this one. And you can see the high power assemblies here. These are the five assemblies within 90 percent of the peak energy there, of this one.

And so it's very, very localized in the radial direction, and it's even more localized in the axial direction, which I'll show you in a little bit here.

And you see as you get away from it you're basically much reduced, and of course, the other side of the core barely sees anything.

And this is a power census here. Basically this is a radial census. So that's looking at it there. That's basically those five fuel assemblies, in that range there.

But you also look at this same -- this is a fraction of the core. So basically about less than four percent is within 90 percent in the radial direction.

But we have a similar type of distribution when we look at the axial direction, and you saw that in Rob's presentation earlier, where you basically only have about out of 24 axial nodes, you only have four of them that are, you know, within 90 percent of the peak energy, and you can see that distribution here.

And when you look at these two together, you've got less than one percent of the core is within 80 percent of the peak energy.

Now, I'll go through this because this is reiteration of what Rob talked about earlier. We're talking about a lot of the data used to anchor the low allowable energy deposition levels from the recent NSRR tests, and we believe those don't represent commercial reactor conditions.

You have the short pulse width, you know, compared to what you would get in a commercial PWR; the low temperature conditions; and the low pressure environment.

And we believe the translation of that that was done in the RIL was unduly conservative, and we believe the treatment that was done in the industry topical was more appropriate.

Our summary is basically that the RA limits should be based on the more relevant parameter of fuel enthalpy, and we look at this as a Class IV accident. You know, basically what you're looking at, you have to keep in mind there's only a small core volume is near the peak energy deposition, and based on that and the treatment of other Condition IV accidents, separation of fuel failure and coolability limits is appropriate.

The conditions needed to obtain the maximum control rod worth and, thus, maximum deposit energy is very limited, and there's low impact on the high burn-up fuel on the core periphery.

And then there is just the references there.

CHAIRMAN POWERS: Any questions for the speaker?

That's extraordinarily useful actually. I appreciate that.

MR. MITCHELL: Okay.

CHAIRMAN POWERS: Rosa, are you going to wrap us up here?

DR. YANG: Sure.

CHAIRMAN POWERS: Promising no additional confusion.

DR. YANG: I will try my best.

CHAIRMAN POWERS: Just tell us when you're going to get us data on fuel failure and high burn-up.

(Laughter.)

DR. YANG: Okay. Let me wrap it up.

CHAIRMAN POWERS: How much of your budget are you going to devote to getting some data points on high burn-up fuel failures?

DR. YANG: A lot. So let me kind of wrap up.

I think we spent a lot of time today focusing on two different approaches to derive the fuel failure limit. On one hand, you have this RIL approach that the author itself characterized as a not a very sharp pencil. It's a little bit sharper than paint brush, but not very sharp, and basically it's empirical.

Then on the other hand, you heard a lot of what we talked about based on the understanding phenomena, data, and you focus on it. We need more data. We need more mechanical property data.

There are scatter in the data, and we are in the effort of reducing it, and we'll continue that. And we can discuss with NRC, you know, the failure limit. Maybe we can make certain adjustments. We can look at different ways. Rather than best estimate, maybe there are other ways of looking at it. We're willing to look at that.

But the most important point, I think for the industry is not so much the difference between 100 calories as we propose versus 55 that was proposed in the RIL. The most, most paramount of importance for us is not allow fuel failures. It's the collapsing of the two limits that's absolutely going to create a lot of burden on the industry.

You heard a good presentation from Westinghouse on some of it, and I'm sure we can provide more of that.

See, to us there's just no justification to do that, and that's exactly what we were concerned about since 1993, and that's why we don't want this sharp/not so sharp pencil approach, and not to just collapse the limit.

There are data out there, lots of data, and we didn't examine most of the data for you today,b ut there's just not justification considering the risk, considering this a Class IV event, the type of accident for LOCA.

We allow the clad balloon and failure. We allow the failure. This is totally unreasonable, and we allow failure in normal operation. I just don't want to get into trouble for saying we allow failure. I mean we try --

(Laughter.)

DR. YANG: -- we try to avoid it, but in normal operation in very small, low, on the order of ten to the minus four and minus five range, we have failures, and that's during normal operation. We allow that.

It's totally unreasonable to say in a Class IV event, which everybody agrees there's no possibility to happen, with very, very conservative calculations which, by the way, are not licensed, that we may get into this very, very unlikely event of $2.00, and by just collapsing the coolability limit the failure limit you're going to severely limit the core design and may be some of the intended consequence that Areva described very eloquently.

CHAIRMAN POWERS: Let me just ask you a question about this. I think this coolability is -- quite frankly, I was ignoring you for a long time on the coolability. You're casting this coolability criterion in terms of fuel melting. For the life of me, I don't see how in high burn-up fuel you get enough energy in to melt some fuel.

DR. YANG: You're absolutely right.

CHAIRMAN POWERS: Okay. It seems to me that the issue is not of coolability in the sense of avoiding this pressure pulse, but rather coolability in keeping the particles of fuel within the rod and not letting them come out into the coolant stream.

DR. YANG: Well, that's a conservative approach. If you --

CHAIRMAN POWERS: I'm a very conservative guy. Trust me.

I mean, why even worry about melting fuel? I mean, in your opening presentation one of the first things that you said is, "Gee, we look at the data.

Corrections have to be made. We can't have this 225 number anymore. As soon as you dropped off that, you got out of the possibility of really melting very much fuel at all.

DR. YANG: That's true.

CHAIRMAN POWERS: Okay. And so it's just not operative anymore.

DR. YANG: Go ahead.

DR. DENNING: Let me help you.

DR. YANG: Go ahead.

DR. DENNING: I'm not sure I want to help you in that sense, but I do think that the nice thing about fuel melting is that it's one --

CHAIRMAN POWERS: There's nothing nice about fuel melting.

DR. DENNING: I take it back.

CHAIRMAN POWERS: Any more than it's nice to allow clad ballooning and rupture, Rosa.

DR. YANG: That's a postulation.

DR. DENNING: It is though comparatively easy for us to determine at what point that would happen, and if one assures oneself that one stays away from fuel melting -- and you gave reasons why there might be some natural reasons that would absolutely assure that -- regardless of that, fuel melting is a convenient criterion, and it's one where if the industry does some additional work, I think that they can justify that that really is the point where you would start to worry about the potential for events that could lead to damage to the vessel.

So I think that that is a reasonable limit. As far as fuel particles getting out into the flow stream in this very low probability event, I have very little concern that they would then lead to a condition that would result in melting of the core.

Now, perhaps that still has to be demonstrated, but if you look at the condition of TMI, which turned out to be --

CHAIRMAN POWERS: That step of putting them out into the flow stream and then letting -- that's one that you --

DR. DENNING: Introduces uncertainties.

CHAIRMAN POWERS: No. You just confused me. I mean, I don't understand how that would lead to. I think the concern is that if you put a substantial amount, but it's on the order of an assembly's worth of particles, that they would accumulate in a low flow and you cannot cool them.

DR. DENNING: And you took the whole core from TMI when you did that to it?

CHAIRMAN POWERS: No, no, no.

DR. DENNING: I'm sorry.

CHAIRMAN POWERS: An assembly's worth of fuel roughly.

DR. DENNING: Well, I don't think we're talking an assembly's worth of fuel anyway, but even if you did, you're concerned that they would go around the system someplace and collect someplace in the system?

CHAIRMAN POWERS: That's right. That's right.

DR. DENNING: And be uncoolable.

CHAIRMAN POWERS: And they're uncoolable.

DR. DENNING: With full flow of the --

CHAIRMAN POWERS: Well, they accumulate someplace where there isn't full flow, but it doesn't matter. I mean, if they accumulate, the particle size distribution is such that it's very difficult to cool.

That would be the coolability limit I would worry about. Now, clearly that requires a more energetic disruption of the assembly, but it is about an assembly's worth. Maybe it's a little more.

DR. DENNING: Well, if there are debris beds of that type that you can conjecture that have to be analyzed, you know, that would be part of the challenge that industry would face. I personally doubt it.

CHAIRMAN POWERS: Why do you doubt this?

DR. DENNING: Well, partly when I saw what happened to TMI. In part because of what I saw what happened to TMI, and it turned out to be coolable.

CHAIRMAN POWERS: No, it turned out to be noncoolable.

DR. DENNING: It turned out to be ultimately coolable. All we needed was flow.

PARTICIPANT: Yeah, flow helped.

CHAIRMAN POWERS: While it was a debris bed it was not very coolable. Twenty-six tons of it were definitely not coolable. But the two particle size distributions, of course, are radically different. In TMI you had roughly pellet sized particle distributions. I don't know what they are, but I've certainly seen the particle size distributions that came from the old SPERT tests, and those were darn fine.

DR. DENNING: Well, and they have a hard time settling out uniformly anyplace under that --

CHAIRMAN POWERS: I think it always surprises me how easy it is. Did we or did we not have fuel settled out in the TMI reactor?

DR. DENNING: Well, we had a lot of molten material that went into --

CHAIRMAN POWERS: We had about three tons of it that was settled out in the piping system.

DR. DENNING: Well, did it melt through the piping system?

CHAIRMAN POWERS: It spread all over the place.

DR. DENNING: Did it melt through the piping system?

CHAIRMAN POWERS: Well, do you know the answer to this question or --

DR. DENNING: Well, I thought it was a rhetorical question.

CHAIRMAN POWERS: Well, I was blunting your rhetoricism here.

(Laughter.)

CHAIRMAN POWERS: My question remains the same. It seems to me this melting thing, I mean, okay, you can't melt much fuel. I will grant you that it's possible to get surface melting. I will not grant you that surface melted material will engage in a pressure pulse.

DR. DENNING: I don't think that's the argument that it will. It's a threshold. It's a threshold for that kind of behavior.

CHAIRMAN POWERS: Well, then I can set a threshold for the total vaporization of the core. I Mean, it's a threshold that's just not very operational.

DR. DENNING: That's a good threshold for that, too.

CHAIRMAN POWERS: I mean it's just not an operational threshold.

DR. ELTAWILA: May I say something?

CHAIRMAN POWERS: Please, Farouk.

DR. ELTAWILA: I thought the original criteria was intended so when you melt the fuel and you expel it out of the fuel rod, it will have potential for fragmentation causing coolant interaction.

CHAIRMAN POWERS: I believe that was --

DR. ELTAWILA: The fuel is already fragmented in high burn-up. So you don't need really to melt it. If you correct the cladding, the fuel itself is so hot, very hot. It will get out, and you will get a pressure pulse, not necessarily a steam explosion, but you still can get a large pressure pulse.

So the concern is still the same.

The other point is that the potential of this material, hot material, moving in the primary system and going into the steam generator and the blocking it and causing steam generator fuel failure or something like that, that's why you try to prevent the fuel from coming out of the cladding because of the complication in the system into areas that we really cannot analyze.

CHAIRMAN POWERS: I think Dr. Eltawila is exactly right. There was originally a concern about a fuel coolant interaction. We have a huge amount of experience that suggests to us you can never get a shock wave generated from solid particles interacting with water. That does not mean we can't get a steam pulse that could propagate some damage.

But the real issue is just particulate, particulate getting out of the clad, and Lord knows what that's going to do, and that seems to me to be much more interesting and useful coolability criterion to look at.

MR. DUNN: Dr. Powers.

CHAIRMAN POWERS: Yes.

MR. DUNN: This is Bert Dunn again. Could I add a comment?

CHAIRMAN POWERS: Please, kick in here,

Bert.

MR. DUNN: Well, I think you've got one Class IV accident here, which is a rod ejection, and you're worried about particles from a small amount of the core, the top foot of the core and maybe two or three percent of the fuel assemblies in the core, and that debris going around the system and floating out.

Another Class IV event that we allow this to happen for is a loss of coolant accident, and on a design basis Appendix K, not realistic LOCA, but on a design basis Appendix K, we can talk about a substantial portion of a core over, again, perhaps this time maybe three inches being exposed to getting out of the cladding perhaps in the fine structure of high burn-up fuel because the pellet has already cracked, and floating around the system, and we're not worried about cooling that so much.

Now, that would collect, I guess, in the upper plenum as opposed to over in the steam generators or in the lower plenum maybe fall back down through the core.

CHAIRMAN POWERS: Yeah, I would worry about the lower plenum and not the upper plenum.

MR. DUNN: Yeah, but we don't have the full force of the real flow.

It seems to me like they're relatively equivalent there, and maybe worrying about the rod ejection where it's a much smaller portion of the core isn't balanced.

Thank you.

DR. YANG: Yeah, I think that's really the key. It's very limited. It is a local event and is very limited, and it's limited to the upper portion of the fuel rod.

CHAIRMAN POWERS: The question I'll pose to you because I certainly do not know the answer is: how much fuel does one have to have as particulate to create a problem when it's an uncoolable bed of particulate?

Now, are there stagnated regions in the core? Yes, there are. My perception is it's about an assembly's worth.

DR. YANG: I think the Japanese have done this study extensively, and what they have used is to look at what is the maximum amount of material, and what is the interaction of that, and they assume really no containment and what sort of pressure pulse was created that would jeopardize the pressure value and different things.

And we can certainly come back and look at those things.

CHAIRMAN POWERS: You're trying my patience.

MR. MONTGOMERY: I should also point out that that Japanese study looked at the coolability of a bed of particles of this nature using, I think, a Lipinski model to look at how big a bed would be coolable if it was all collected in one spot. So we can go back and look at that and collect that information.

CHAIRMAN POWERS: Or you can just tell me about it.

DR. YANG: Yeah, they did look at all of that, and then at the end they decided that the melting is not necessarily a coolability limit, but if you can prevent yourself from molten fuel -- and I think as Dr. Denning indicated, that is a criteria -- if you don't violate that, then you won't get into a very uncomfortable situation. It is not necessarily the limit.

CHAIRMAN POWERS: I'd certainly like to see that argument because it's not transparent to me immediately. I mean, I'm certainly willing to listen. It's a convenient barrier, but the problem you have is this sustain enough energy to get you to melting.

Okay?

Now, maybe there's not enough energy to get you to fragmentation either, but it seems to me that's just a matter -- I've interrupt you. Please continue.

DR. YANG: No, I think that's the key point I want to make. I think there are a lot of technical details that we will be ready to discuss when NRR is ready to continue the review, and the failure criteria is not that important, you know.

I mean, you can look at the data to say, "Okay. I'm going to look at the most conservative approach. I'm going to take a super licensing approach."

You can do all of that, you know, and we spent the majority of the time talking about that. that's fascinating, and not just us. I think the whole industry has spent a fascinating amount of time, amount of resources to look at failure, and one of the key challenge for the Cabri water loop project, when and if that comes back on line, beyond 2009 and 2010, is how do you fail this cladding that we are using today.

On one hand we want good cladding, which ZIRLO and M-5 that represent what we will all be using in 2010, and there's just not enough energy in a reactor which is not a lightwater reactor, which is a Cabri reactor, which is designed for this type of accident, you know, to simulate this type of accident.

There's just not enough reactivity to put in, and we're looking at how do we may be jack up the temperatures start the flow, the water flow so that we create a failure, so that we can look at particle fuel-coolant interaction.

So failure -- I think there is enough data that we all understand it, and I think we have good understanding. We can take a more conservative approach. We're willing to discuss the discrepancy between 155, but that's not the real issue.

The real issue is to collapse the coolability limit, and when we have -- you know, on one hand, you have lots of data that when the rod failed, you don't even know it failed until you look at it in the hot cell. These are very high burn-up rods. So you know failure is --

CHAIRMAN POWERS: But you will definitely know it failed in a reactor accident because you will just get a huge xenon signature coming through.

DR. YANG: Sorry. Say it again.

CHAIRMAN POWERS: You will definitely know that it failed in a reactor accident.

DR. YANG: If it failed.

CHAIRMAN POWERS: Because you will get this nice xenon signature coming through.

DR. YANG: Yes, yes, yes, but luckily we don't have an RIA accident in the reactor. So the next best thing is in a test reactor to simulate it, and you know, if you have a good cladding like we have today, it's a challenge to fail the cladding.

You know, in a way it's a bit ironic in my view with this for more than ten years, is what started it all. Then we discarded it. You know, what started out as REP-Na1, and now everybody agrees an outlier.

But, again, we shouldn't focus on the failure. We should focus on it's unreasonable to collapse the coolability to failure when there's a huge distance between the two phenomena.

CHAIRMAN POWERS: The trouble is I don't know what the distance is between the phenomena that I would worry about, which is not melting in failure. It's some other phenomenon. I don't know how to articulate it any better than I have.

Now, let me ask you another question here. In Dr. Meyer's presentation, he confronted the same problem that you confronted and said, "Gee, I've got data, and I don't really like my data, but it's the data I've got and how do I fix it?"

And he went through and made some arguments that resulted in him moving some data point hither and yon and whatnot, and he set up a criterion in there.

You confronted largely the same problem, but the upshot that I understand is that you reclassed some of the points out of the database, and he didn't give me a very transparent way of moving the others, maybe because they were nonoperational, but the upshot of it is that you end up with a criterion that is way above where I would plot the data.

Is there a way that I can understand how you've moved these data points without appealing to a computer code that I don't begin to understand?

With Meyer's approach, I can do it on the back of the envelope. I can see exactly how he moved them. With your approach, it seems to rest on reclassing some points out of the database, mostly the failure points, and then moving things with a computer code by mechanisms I'm not quite sure I understand.

DR. YANG: Let me response slightly differently. I guess I wouldn't quite call we encountered the same problems or difficulty. I would like of say we both recognize the need to translate the experimental data to a more representative lightwater reactor condition because, after all, that's what we care about, is lightwater reactor condition.

And you know, similar to LOCA, we spent so much time yesterday. Thank God we don't have a LOCA. So you're trying to simulate and you want to make sure you capture the key characteristics, and the key characteristics, I think the international community pretty much agree, and Ralph agreed to two out of the three key ones.

So I think it's a big improvement over just look at the data and use a brush approach. So I think I see that as a very positive forward.

Now, can I try to persuade you to move some of these data without a code? Basically you're saying, "I don't believe in the code."

CHAIRMAN POWERS: No, you're too strong.

DR. YANG: I'm sorry.

CHAIRMAN POWERS: I believe a lot in the code. I just want to understand transparently what moved what by about how much, enough understanding so that I can say, "Okay. They moved them enough that I can do a back-of-the-envelope calculation and say, yeah, that's about right."

DR. YANG: I think if you give us more time, I think we could. And I think Robbie has tried to do that, and you know, these kind of things, I like to use an analogy like where we're trying to understand a complex phenomena. It's a bit like, you know, a blind person trying to feel an elephant, and maybe sometimes we try to focus the trunk. So you get the impression it's a trunk, and that kind of thing.

I think it doesn't quite do the ten, 12, 15 years' work justice to try to cramp it in in two, three hours, and maybe we haven't been successful to represent it, but I think if you look at the tremendous amount of literature, data, you will agree there is a good consensus among the international community, you know. Maybe in your conference next year we will convince you, but there are these type of conferences, and you go to them, and some of the data I presented was directly coming from the conferences a month ago.

It's incredible. I don't know if any of you have experience with the earlier PCI days. You know, you can always argue about the smaller differences. You won't agree, but the overall sort of

I would call the key branches, I think most people agree, and I think if we can agree on that, I think we can convince you how we move those data.

And another thing I want to point out is whatever curve we end up with and I think wherever appropriate, wherever data available, we try to demonstrate that those curves are supported by data. They are not just theoretical calculation.

I guess another thing I want to point out is this is a fascinating field and I have only been somewhat involved. I'm certainly not an expert in the field. You can always sharpen your pencil further, but I think given the risk we're talking about and the consequence we're talking about, it just seems to me we ought to focus more on things like LOCA rather than continue to spend the tremendous amount of resources as we have been.

And we are committed to the Cabri water loop project. We're going to continue monitoring, but that won't have any data, 2010 and plus, and we're going to see some data in Japan, and some of them are going to be very telling in six months. You know, either we won't be able to fail the rod at all and then I think, you know, we probably should all go home and sleep comfortably at night.

So I'm --

CHAIRMAN POWERS: Boy, am I hoping for that outcome.

DR. YANG: I'm just saying this is the field. I think the understanding is fairly mature. The mechanical property data given a lot of the experts here, there's just this intrinsic difficulty in measuring data, but I think we have come a long way, like Mike indicated. Some of the data we used were preliminary, with data several years ago.

DR. BILLONE: Raw.

DR. YANG: Raw. Sorry. Raw is the right word, and these things need to be analyzed, and as I indicated, two months from now we are going to EDF. Actually not "we." EDF and CEA are going to generate the data, going to publish their paper which you can see the data are going to be moved higher, and the scatter are going to be considerably reduced.

So I think we can convince you if you give us more time now.

CHAIRMAN POWERS: Well, I'm an easy dealer. I'm easy to convince compared to those not interested in being convinced, which is some people in the public, and a more transparent discussion of the physics than moves things probably would help.

But let me not in any way denigrate what I thought was a tremendous amount of information presented today.

DR. YANG: A tremendous amount of work.

CHAIRMAN POWERS: And yeah.

DR. YANG: And you ask me about money. A lot of money.

CHAIRMAN POWERS: I certainly appreciate the amount of work, and I know that translates into dollars.

Any other comments? Any other questions for the speaker?

(No response.)

CHAIRMAN POWERS: Well, thank you, Rosa.

DR. YANG: Thank you.

CHAIRMAN POWERS: Dr. Meyer.

DR. MEYER: Thank you.

I just want to make three small technical comments for the record and then perhaps a more general comment.

The first one, I simply want to point out that EPRI with the FALCON code had exactly the same problem that we did with the FRAPTRAN code on the Japanese data, and we addressed that by manually adjusting the gap size and went on. So I just wanted to emphasize that the reversal of the trends, the sort of unexpected reversal of trends in those data were in the data, not in the codes. We both had the same problem and we dealt with it by manually adjusting the gap size to a value that you wouldn't have otherwise expected.

I don't want to say anything more about that, but just to make that observation.

The second point that I want to make has to do with the critical strain energy density, the CSED value. You saw all of those data and had a lengthy discussion on it. It just seemed to me that the data from material with spalling would have fit in that population just as well as the other data, and one could have fitted it with a very simple curve, and the result would have then been much closer to the result that we got in the real. Just an observation.

The third minor point that I'd like to make right now is about REP-Na8 and REP-Na10 and the hydride blisters that were seen. There were a total of four radial cuts between those two tests, one in REP-Na10, which I showed, which was taken from the approximate location of the failure initiation. There were three cuts in REP-Na8. One of those cuts exhibited a hydride blister. Robbie showed that one.

I simply want to point out that that cut was taken well away from the point that had been identified as the failure location. It was a good ten centimeters away from the point at which they had deduced the initiation of the failure from the online instrumentation.

The final comment that I'd like to make is a little more general, but it won't be long or lengthy. It has to do with the RIL and the intent of the RIL.

The RIL was done more or less -- I think of it as a sufficiency analysis. The goal was to see if we had a safety problem with the operating reactors with regard to this unlikely accident, and as we went along we found that the failure level that was emerging from our analysis was higher than the possible enthalpy values that we had been told about by Westinghouse and General Electric and EDF for the past ten years, and so we felt quite comfortable that there was no need to push for less conservative or nonconservative levels and performed an analysis that I thought we could stand behind and be confident that it would not be nonconservative.

On the other hand, switching now to the subject of that failure level and the melt related coolability limit, this, of course, is the same limit that was adopted 30 years ago and makes no accounting for the new mechanism of fuel loss or fuel dispersal that appears in high burn-up fuel where you have the fission gas and its ability to push fuel outside of a crack that might otherwise be benign.

So I think somewhere in between these regions might be a reasonable place to land for a limit, but we thought with the RIL that from the information available in the published literature, that we were quite okay and that it was an adequate demonstration that cladding failure and, therefore, any consequences could move toward loss of coolability would be avoided.

So that's all I wish to say right now.

CHAIRMAN POWERS: Let me ask you this question on that. Are you saying that you're open to considering a difference between a failure on the coolability limit here?

DR. MEYER: I said during my presentation, and I believed this all along, that there is probably some domain in energy above the failure limit in which the failures will still be benign.

DR. KRESS: Do you have any way to go about deciding what that level would be?

DR. MEYER: That's the part that's difficult, and that's the part that we did not attempt to do because we thought what we were doing was sufficient. Now, you might be able to sharpen your pencil a little bit and get the failure level up if you look more closely at things like, for example, if we get a test from Japan in the high temperature, high pressure capsule or a couple of tests, it looks like there are two good tests planned within the year, one in March and one in June of 2006.

If we should happen to get some really good data points, it might convince us that the entire population of NSR data points is not appropriate for the PWR hot condition, and these two new points were. That could go a long way.

You remember I addressed it both ways. I took a conservative, no temperature dependence, and what I thought was a fairly strong temperature dependence and did the calculation both ways. So we did not prejudge this situation and the result was the more conservative of those two which appeared to be sufficient.

CHAIRMAN POWERS: Farouk, did you have something?

DR. ELTAWILA: No, I think he covered it.

CHAIRMAN POWERS: He covered it for you, and we're not going to let Mike say anything. He doesn't know anything about --

DR. ELTAWILA: I think the only thing I just want to emphasize one thing. We really don't want to have -- I think you struck a nerve with me when you talked about the overlap between the criteria and the net worth when you consider the uncertainty. So we really need to go back and look at the conservatism in Ralph's approach and try to identify that conservatism and see where it can be realized, and I think he committed to do that.

CHAIRMAN POWERS: Members have any other questions that they would like to ask?

Farouk, are you looking for anything from us on the near term?

DR. ELTAWILA: No. I think that's for information right now. I think what we would like to do is -- I'm speaking for NRR. Tell me if I'm wrong -- I think we would like to have NRR and EPRI start resuming the review. We are going to work independently on the RIL and help NRR as deemed as necessary.

So I don't think an ACRS letter at this time is needed.

CHAIRMAN POWERS: Well, I think anything we'll comment on the actual RES research program will probably show up in the research report. I don't prejudge what the subcommittee or the ACRS will say, but we're not counting on any presentation on this subject on the September meeting.

DR. ELTAWILA: Correct.

DR. DENNING: Even though there isn't anything, would we plan for it at some later time? Certainly it's extremely interesting, and I think it will be of high interest, even though there's nothing moving forward.

I know that we're really busy in September and maybe also in October, but I don't know what your feeling is.

How often would we provide an update on this type of information?

CHAIRMAN POWERS: Are you asking for the full committee or for the subcommittee?

DR. DENNING: No, the full committee. I mean, is there a reason that we would go to the full committee just to bring them up to speed on status?

CHAIRMAN POWERS: I think what we will do is with something significant to resolve here. The committee has been briefed fairly often on the salient features of this. I'm not anxious to spend some time with the full committee right now just because I know how time equates. I mean I just don't see any urgency to move forward with them until we have something substantive for them to look at.

DR. DENNING: You mean like 2010 or 2011.

CHAIRMAN POWERS: Gosh, that sounds like a very inviting time.

Well, maybe we should walk around and ask if there are any opening comments. Dr. Shack, do you have any comments other than those you've made?

DR. SHACK: No.

CHAIRMAN POWERS: Dr. Kress?

DR. KRESS: Well, I'm glad to see that the NRC people are at least open to the thought of separating the coolability limit, but I think that's going to be a difficult problem because what I think you have to do is just what you said. You have to determine how much solid fuel gets ejected at what size and what it does to the primary system. Will it cause a LOCA by itself by being noncoolable at some position?

It would be nice to see some analysis along that line, but I'm not concerned. I think the industry was correct in saying that the pressure pulse is likely to be too small, but even that I'd like to see the evaluation. That will depend on how much fuel and what size the particles are that get ejected.

You won't get a real extremely explosion, but you may get enough pressure pulse to fail some steam generator tubes.

CHAIRMAN POWERS: I mean, the only way I see of getting a pressure pulse is to get something like this criticality that the Finns calculated in one of their events.

DR. KRESS: Yeah, well, I'm not too worried about that.

CHAIRMAN POWERS: Well, I know Ralph isn't. He probably cringes every time I bring it up. They do have a published paper in which they get at criticality, but I take it we've largely discounted that possibility.

DR. KRESS: But the other thing is -- two other things. I don't think I'd be so anxious to throw out the Japanese data. That was done at room temperature. I would be anxious to see these better tests and to compare the corrections to the results.

And I'd also like to see the corrections made with the code like theirs.

The other thing that struck me was on the mean curve through the energy density failure criteria, that strikes me as not being a place to put a mean. I would have looked for some lower bound on that, some one sigma or something.

I don't know how you decide one sigma or two sigma, but with the mean curve through the thing, you're going to be raw at least half the time. So you know, I wouldn't have used that curve as my failure criteria curve.

And I do think Dana is right. When you do your least square fit, he didn't quite do it right and he probably wouldn't even get that mean curve if he did it correctly.

But those are my initial reactions to it.

CHAIRMAN POWERS: Mr. Caruso.

MR. CARUSO: No.

CHAIRMAN POWERS: No comments?

MR. CARUSO: No comments.

CHAIRMAN POWERS: Uncharacteristically silent, sir.

Professor Denning.

DR. DENNING: Just a couple of more things, and that is I think based upon what I've heard today is I think there's so little real data out there at high burn-ups that I think both of these approaches that we've seen have very questionable assumptions and draw conclusions from some questionable treatment of data in both cases.

So I don't mean to be critical because I think that there's a tremendous amount of good work that has been done and these experiments are so expensive, but the reality is that we just have very little data out there to really -- and there obviously is an awful lot of variability that either we don't understand or it's just in the nature of the beast here.

So I think there are areas of concern in both of those treatments. Now, certainly from the RES viewpoint here of tying coolability to fuel damage, that certainly is a very conservative approach.

When you look at what was done in terms of fuel coolability -- I'm sorry --

CHAIRMAN POWERS: You mean tying it to clad damage?

DR. DENNING: Into clad failure, into fuel failure, in tying fuel ability to fuel failure. That's a conservative thing to do clearly. You can be pretty confident that as long as the fuel doesn't fail that you're cool.

But as far as the treatment of the data

was concerned and the plots against the percent oxidation, reality is if you look at those curves objectively, they don't tell the story that RES is -- that is pulling out of those curves. I think that there just isn't enough data out there ot really do it.

So I think it's still very open as to what the reality is of when high burn-up fuel is going to fail. I think our understanding is not --

CHAIRMAN POWERS: I mean, it seems to me that the easy conclusions everybody has made, that is, yeah, the 225 are just not applicable as your burn-up fuel, and so we have to worry some about burn-up fuel, and the problem we're running into, I think -- well, there are two problems we're running into. One of them is that you really, really, really would like to be able to run fuel up to higher burn-ups. I mean industry would like to do it maybe for economic reasons. Society would like them to do it. I mean, there's just a whole lot of good reasons to run up to higher burn-ups, and we don't have a good understanding of what goes on.

We now know that there are some physical transformations of the fuel. It seems to me that the big step that has been made here in our understanding is that we've got a clad problem and not so much of a fuel problem, and I think there's general agreement on that.

Now you're coming in and I struggle to think of a field where somebody says, "Gosh, we just don't need anymore data. We've got it all."

There are some, but I struggle to remember them right how. So now what can you do with the data you've got and make a persuasive case?

And at least I'm gaining an understanding of how to do it, and yet I would just like to see -- you know, I'm always fascinated with Robbie's presentations. They're just great because they've got more data, things moving around, and big uncertainty bars, and I just love that sort of stuff, but I don't really follow how things are moving.

I'm a little concerned about when I throw data out of a sparse database and saying I can't get it all grouped in here together. It's just different. There may be sound reasons for doing that, and I certainly appreciate the comments that were made about what the fuel vendors are trying like crazy to do is to get rid of this fuel spalling problem, but it still reminds me that somewhere something came across my desk about some spallation events occurring in a reactor, but when I think about it, it may not be a reactor in the United States.

You know, I worry how we handle these flaws and the fact that we're doing all of our tests on this much of a fuel rod, and this is one of those games where failure anywhere counts, and so that's why I asked lots of questions about preferential sampling and things like that, and things to worry about, but I think we've got to make some progress with the database in the short term. 2010, I think, maybe is a long time to wait for this because somehow we have to recognize the regulations, these things that we all agree on, is that burn-up does change these criteria and whatnot.

I'm very sympathetic with Ralph. I, too, when I looked at your criteria said, "Oh, yeah, those are all below, but the 3D kinetic rod worths were full sizes, and so everybody is going to be happy with this limit."

Then I say, "Maybe not." Maybe not.

Okay. Any other comments people would like to make?

DR. ELTAWILA: I would like to thank you for accepting to take on this job in a very short period of time. We really appreciate the ACRS --

CHAIRMAN POWERS: Oh, back to the LOCA stuff.

DR. ELTAWILA: It was very important for us. So thank you very much.

CHAIRMAN POWERS: Yeah. I think we've got some interesting things to discuss there, and that will be an interesting session.

DR. ELTAWILA: Thanks.

CHAIRMAN POWERS: Now I'll put in a little plug here. You've got to put in a plug now. I am trying to organize a session at the Reno American Nuclear Society meeting in this general area addressing both the LOCA and the RIA, and I think everyone should consider contributing a paper to this. I think we could have an interesting session to address this.

I think my intention is just to acquaint the larger American Nuclear Society membership with all that has gone on and their thinking here. Quite frankly, a lot of the members I talk to are operating on hearsay and bits and snippets of information, and I don't think they have a good appreciation of all that's going on.

So I really encourage you to think about submitting a paper to that and any advice you would like to make to me about also having a panel session where we could discuss things with the membership.

If there are no other comments, I'll bring this meeting to a close, and once again, just praise all of the speakers and the investigators and their support staff who did not attend. I think every other subcommittee would be just green with envy at the quality of work, the quality of presentation, the quality of science that's going into this area. I think you all have a right to be very proud of yourself.

And I'll close it with that.

(Whereupon, at 4:45 p.m., the meeting was concluded.)