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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)
SUBCOMMITTEE ON MATERIALS, METALLURGY
AND REACTOR FUELS

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FRIDAY,

JANUARY 19, 2007

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The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. J. Sam Armijo, Chairman, presiding.

SUBCOMMITTEE MEMBERS PRESENT:

J. SAM ARMIJO	Chairman
WILLIAM J. SHACK	ACRS Member
MARIO V. BONACA	ACRS Member
DANA A. POWERS	ACRS Member
JOHN D. SIEBER	ACRS Member
SAID ABDEL-KHALIK	ACRS Member
J. SAM ARMIJO	ACRS Member

NRC STAFF PRESENT:

JENNIFER UHLE
RALPH MEYER
HAROLD SCOTT

ALSO PRESENT:

MIKE BILLONE
BERT DUNN
ROBERT MONTGOMERY
MITCH NISSLEY
DAVID MITCHELL
PAUL CLIFFORD
ODELLI AMANPOUR
CHUCK PATTERSON

C O N T E N T S

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

(8:34 a.m.)

CHAIRMAN ARMIJO: Good morning. The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Materials, Metallurgy and Reactor Fuels.

I am Sam Armijo, Chairman of the subcommittee. Subcommittee members in attendance are Mario Bonaca, Dana Powers, Bill Shack, and Jack Sieber and Said.

The purpose of the meeting today is to discuss the results of activities in the Office of Research related to the development of revised LOCA criteria for reactor fuel. The subcommittee will hold discussions with representatives of the NRC staff, their contractors, and representatives of the nuclear industry regarding these matters.

The subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee.

Ralph Caruso is a designated federal official for this meeting.

The rules for participation in today's

meeting have been announced as part of the notice of this meeting previously published in the Federal Register on December 21st, 2006.

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

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

We have received requests from various industry representatives to speak, and they are included in the agenda for the meeting. We have received no other request for oral presentations.

Our first speaker of the day will be Jennifer Uhle from the Office of Nuclear Regulatory Research.

DR. SHACK: Mr. Chairman, I should note that I have a conflict of interest since Mike Billone and I work at the same great national laboratory.

MR. CARUSO: One other comment. We have a speaker phone set up with a bridge line. So I would ask people to be especially attentive to speaking up so that the people on the phone can hear you.

CHAIRMAN ARMIJO: All right. Could you let us know who is on the phone?

MR. CARUSO: Is there anyone on the phone right now? If you are, could you introduce yourselves?

MR. OZER: Odelli Ozer, EPRI.

CHAIRMAN ARMIJO: Okay. EPRI is on the phone.

Okay. Well, Bill, you're welcome to stay. I don't know what your limitations are, but I'm sure you do. Okay. Thank you very much.

Jennifer.

MS. UHLE: Thank you.

Good morning. My name is Jennifer Uhle. I'm the Deputy Division Director for Materials Engineering in the Office of Research at NRC.

On behalf of the office, I'd like to thank the subcommittee for taking the time to meet with us today to discuss the results of our high burn-up fuels research program. We'll be talking, as you have just indicated, we'll be talking about the revised emergency core coolant system acceptance criteria for 5046.

As most of you know, loss of coolant accident is one of the design basis accidents that licensees are required to analyze as part of the regulatory process. Some of you in the audience, as well as on the committee, have invested a lot of time in the thermal hydraulic models required to analyze this accident. Yet in the end we mainly rely on two figures of merit to determine the adequacy of the ECCS systems, and that's primarily peak cladding temperature, maximum clad oxidation.

So for more than 30 years we've been relying on the famed 2200, 17 percent values as to determine the appropriateness of or the adequacy of the ECCS systems. About ten years ago we realized that these values may not be appropriate for the higher fuel burn-ups that have been licensed.

So we began a research effort in 1996 to determine these burn-up effects, and today we'd like to present those results to you.

From the beginning this program has been done in cooperation with our international partners, as well as the U.S. industry. At the same time we maintained our neutrality by analyzing the results independently. We have faced some difficulties in obtaining irradiated fuel rods and access to hot cells in which to do the testing. We are aware that more data would be desirable, but we feel we have adequate database to support a modification to the rule, and we'd like to go forward.

In fact, we have all of the data that we said we would have in our research plan that the subcommittee and committee had previously reviewed.

There is a need for a rule change since we know that the regulation does not directly consider high burn-up fuel effects. Although we are managing that issue through our regulatory process, the rule should be more specific with regard to the burn-up effects.

In addition, the current rule is written in a very material, specific manner such that licensees wishing to use newer and better fuel clads must receive an exemption from renewal. Fortunately, this additional burden has not inhibited the licensees from adopting the new materials, but we believe that it's best to move the rule forward in a more performance based manner so that this inhibition does not exist.

In our first presentation, Dr. Ralph Meyer from the Office of Research will summarize the research results and propose a way in which these results could be incorporated into the regulatory process.

And then Mike Billone from Argonne National Lab, Dr. Billone, will then present the results in detail supporting Dr. Meyer's presentation.

I want to stress that we are not presenting the final rule language. That is the purview of NRR. We will be trying to summarize our research results in a manner that will facilitate everyone's understanding so that we have put in terms of some numbers to give you an idea of how perhaps this will go forward in the future, but this is not the final rule language.

And we're hoping that at the end of the meeting today, after we hear from the committee or hear questions and your comments, as well as the industry's presentations and member of the public questions and comments, that the subcommittee will go forward to the full committee supporting our belief that we can move this research result over to NRR in the form of a research information letter to support the proposed rulemaking for the revised criteria.

We're hoping to send over the RIL in March 31st, 2007. So we're looking forward to pursuing that goal.

So with that I'd like to turn the meeting over to Dr. Meyer from the Office of Research.

DR. MEYER: Good morning. Do we have a laser pointer here?

So it was 33 years ago that the Atomic Energy Commission had the biggest hearing that I think it ever had to come to these rules for emergency core cooling system, ECCS, performance. It involved the peak cladding temperature and the oxidation limit that Jennifer mentioned.

Two things were clear from the Commission's opinion, which was written and issued on December 28, 1973. One was that they wanted to retain ductility in the fuel rod cladding after the LOCA transient, and two was that they adopted very simple temperature and oxidation limits, notwithstanding the fact that the phenomena were very complex, and they understood the complexity of the phenomena at that time. It involved diffusion in the metal. There were discussions of adopting the square root of DT limits, criteria based on the alpha thickness, the beta phase thickness. All of these things were understood and developed, and in the end they settled on these very simple empirical criteria.

So we've used the same approach in trying to accommodate burn-up and alloy effects. There are additional complexities from the burn-up process and from using the niobium alloy additions. They are complex effects. We understand them only approximately, but I believe we can characterize them empirically using a simple form very similar to what was done originally.

DR. POWERS: Ralph, you talk as though there's something good about preserving these surrogates for ductility. Is there a reason to write the rule that says preserve ductility post LOCA or post design basis accident event?

I mean, why have these figures that are related to ductility? Why not just write the rule that says, "Preserve ductility"?

DR. MEYER: The question is why did we use ductility instead of some other measure? Is that the question?

DR. POWERS: No.

DR. MEYER: I'm sorry. I have a hearing problem which I'm going to get fixed soon, but it's not today. I'm not sure I really understood the question.

DR. POWERS: Well, we put in 2,200 degree clad temperature, 17 percent oxidation, and those have become the goals and people worry about them and agonize about them. They've forgotten largely that the idea was to preserve ductility in the post LOCA environment.

DR. MEYER: Right.

DR. POWERS: And so what I'm asking is why continue to do that. Why not just say we want the cladding to have some ductility when you recovered from an event, a design basis event, and please show us that that is the case.

Why put in more parameters or a different set of parameters or things like that?

MS. UHLE: Can I answer that? This is Jennifer Uhle from Research.

Again, we're not going to be presented rule language here. Even if the ultimate rule were completely performance based as you've indicated, the Agency would still have to determine an appropriate set of values by which to ascertain that a particular ECCS system was adequate to maintain the ductility, and so if it goes in that direction to be completely performance based, then these values then would be in some form, in a reg. guide or something like that.

So if we can perhaps postpone your question to a later date when NRR has absorbed the information we are giving them and interactive with OGC, as well as the public, about the best way for the rule language, I think you'll hear today what we believe are the phenomena behind what is necessary to maintain ductility.

Does that answer your question?

DR. POWERS: Nope.

MS. UHLE: Come on. That was a perfect answer.

(Laughter.)

DR. MEYER: The purpose of the 2,200 limit and the 17 limit, they were derived specifically to insure that there was ductility, and the test at the time showed that if you remained below those limits that you would have ductility after a LOCA temperature transient in the steam environment.

DR. POWERS: Yes.

DR. MEYER: Okay? I still don't understand the nature of your question.

DR. POWERS: It's a philosophical question, and you only get yourself locked up into a material specificity as long as you put these parameters in the rule. If you'd say, "Here's what the rule is intended to do. Please do this," and tell me the details during the evaluation, and I've got this body of research behind me that tells me the things to look for, you get out of that problem.

DR. MEYER: Well, I think you'll find that that is about what is going to happen because what we're going to show you are some criteria that work. They involve parameters that are measured for any alloy in this zirconium, tin, niobium family, and then you figure out limits, which would be reviewed.

How much of the details go into the actual regulation and how much of the details go into the reg. guide have not been established, but certainly this regulation would have to have a reg. guide to describe the kind of tests that need to be done in order to get the right characterization of the materials property.

DR. POWERS: I mean, the reason for asking the question is you look at the research program in a different fashion. If you look at it as a body of information that the regulator has, and the licensee as well since it's public information, that tells him the kinds of effects to look for in the system, that's one thing.

If you look upon the body of research as the justification for particular parameters and things like that, that's quite a different thing. And I would argue that the size and quality of the database is different between the two.

Well, I think, in fact, that we have done both. We will identify the phenomena that need to be looked at because we believe we know the major phenomena that affect the ductility, and we have made enough measurements that we believe we can adequately characterize the three cladding types that are currently forming the majority of the cladding used in U.S. plants.

So I think we have both of that in here, and I think you'll see it that way when we present it.

DR. POWERS: Good.

DR. MEYER: Shall I go on?

DR. POWERS: Please.

DR. MEYER: Okay. Let's start by just reviewing the basics, and we'll move quickly.

So during a loss of coolant accident, the cladding will go through a temperature transient. It goes up, and it comes down after the reflow occurs from the ECCS systems.

If you're looking at a location on the rod close to a balloon, you will see that somewhere in the vicinity of 800 degrees Centigrade, the cladding softens. It swells up. It pops; it ruptures, and because of the larger surface area, there is a small reduction in the temperature because of the extra cooling, and then the temperature can then continue to go on higher. So we know about ballooning and rupture. We know about oxidation that generally takes place after ballooning and rupture, and we know about quenching, which eventually comes in and cools it all.

What I really want to focus on are these other processes that go on just about at the same time. Just about at the same time you get ballooning and rupture, you get a phase change in this material. It goes from a hexagonal crystal structure which we will call alpha phase -- it's always called the alpha phase -- and it changes to a bi-center cubic structure, the beta phase.

Now, oxidation is going on on the surface, but at the very same time and at the very same temperature region, diffusion of oxygen in the metal becomes significant, and so you now have oxygen beginning around here to diffuse in substantial quantities into the metal, and it is that oxygen that ends up giving you the dominant effect on embrittlement.

And then eventually when you quench, you go back through the phase change and it all changes back to an alpha form.

This is the regulation that's currently on the books and a clarification that occurred in 1998 in an information notion. So we've already talked about the temperature limit of 2,200 degrees Fahrenheit, which is 1,204 degrees Centigrade, and we'll switch and talk for the rest of the time in Centigrade degrees, and we'll probably slough over the four degrees and just call it 1,200 degrees Centigrade.

You really calculate temperature and time during a LOCA calculation. So you basically have a time limit that corresponds to the accumulation of 17 percent oxidation on the surface of the cladding.

The oxidation is calculated. We call it an ECR, equivalent cladding reacted. That's just a measure of percent oxidation that's calculated in the safety analysis, and you assume that there's one-sided oxidation pickup away from the balloon. This is oxygen moving in from the oxide that's built up on the surface, and two-sided oxidation in the balloon because after the balloon ruptures, steam gets inside and oxidizes the ID of the cladding in the vicinity of the rupture.

So that's the way the analysis is done today. As of 1998, we informed the industry that 17 percent should be understood to be the total oxidation, that is, the sum of the corrosion that took place during normal operation and the amount that occurs during the transient.

So, in effect, you're subtracting from 17 percent the corrosion thickness, which can be significant.

So the heart of the subject that's going to have to do with where the oxygen is, and here is a schematic of the oxygen concentration on and in a piece of Zircaloy that has been oxidized in steam. You have an oxide layer that builds up on the surface. This is in weight percent, but it's not drawn to scale. The oxygen diffuses into the metal, which is beta phase at high temperature.

As the oxygen diffuses into the metal, it quickly exceeds the solubility limit in the beta phase and forces that beta material to convert back to an alpha phase, and we call it the oxygen stabilized alpha phase. So at high temperature, you have a small region of oxygen stabilized alpha and a beta phase out here.

And as time goes on, the oxygen stabilized alpha layer grows and the beta layer shrinks. Now, if you cool it back down to room temperature, you can see these phases in the microscope, and I'll show you that on the next picture, but that's why we've labeled this prior beta, because after you cool down it has all gone back to alpha, but you can still see what was in the beta phase at high temperature.

So here you have on the surface an oxide layer. You have this oxygen stabilized alpha layer. And you have the bulk of the material in the prior beta phase.

So keep that in mind as we go through the rest of the discussion.

Now, these are the equations that we'll be using, and I won't show a lot of equations in the presentation, but the oxidation rate, the weight gain goes as the square root of time. So these are parabolic kinetics. The K term here is temperature dependent, and like a lot of these high temperature processes, it's thermally activated with an activation energy. R is the gas constant. This is a very common Arrhenius type temperature dependence.

ECR, equivalent cladding reacted, is a sort of artificial term that has been used from the beginning in analyzing the accidents. You assume that all of the oxygen consumed, that is, all of the weight that has been gained, is in the oxide, ZrO_2, ignoring what has diffused into the metal.

So if you just use that construct, then the ECR is simply a geometrical relation between weight gain and the thickness of the clad, and we will most frequently be using a two-sided oxidation equation, and that's the equation that we use.

Cathcart and Pawel have the coefficients A and Q to plug into this, and when you put it all together and boil it down, this is the practical equation that we normally use.

Now, that's for weight gain. Cathcart and Pawel had other equations. They had equations for oxide layer thickness, alpha layer thickness, and for the sum of those. All of them have the same parabolic kinetics. The same Arrhenius type temperature dependency coefficients are a little different, and we've stuck with the weight gain just as was done before to try and keep this in the same formalism as we had in the original rule.

So now what I want to do is to talk about the different mechanisms that cause embrittlement, and they're related to this oxygen diffusion into the metal. The first two mechanisms were understood originally when the rule was written, and they have to do with the beta layer, embrittlement by the oxygen, and beta layer thinning. You can see this on the next slide where I've shown that the -- well, just for starters, the outside layer and the oxygen stabilized layer are brittle.

So you get no ductility out of those at all. You get ductility only out of this prior beta layer, and the prior beta layer itself is only ductile when the oxygen concentration is below about .6 of a percent. So there can be a portion of the prior beta phase that is brittle, and so this is what we have to deal with.

So it was found empirically after discussing all of these things that if you kept the temperature below 2,200 Fahrenheit and the oxidation below 17 percent, that you would have a large enough prior beta region with a low oxygen content so that the sample would behave in a ductile way.

And that's where the 2,200 and 17 percent numbers come from.

Now, around 1980, Chung and Kassner at Argonne and then later Letsuga and co-workers at the Geri (phonetic) Lab in Tokai found that there was some special behavior that was going on inside the balloon beside the simple oxidation process.

Now, as you know, when you oxidize zirconium in steam the zirconium takes the oxygen, but it releases hydrogen. They have some free hydrogen, and usually a small percentage of that hydrogen is also absorbed into the zirconium.

It turns out inside of the balloon a lot of hydrogen was being absorbed in the balloon region because that released hydrogen in there wasn't getting swept away very effectively by the steam. So you developed very high bands of hydrogen concentration. These are 3,000 ppm of hydrogen above and below the center of the balloon, and here you see at the center of the balloon the oxygen concentration calculated as an ECR using average values as would be done according to the regulation. This had about 18 percent oxidation.

So here's a case, and these are real data. These are data from high burn-up BWR rods where you have kept the oxidation in the balloon no higher, well, infinitesimally higher than specified by the regulation. Yet you have this huge amount of hydrogen that gets absorbed into the cladding. This leads to embrittlement of the balloon, and we see this over and over again, and it occurs even if you're living inside of the regulatory limit.

So we discussed this the first time with this committee in 2003, referred to it at that time as a ductility singularity because the balloon, even when you follow all of the rules, the balloon is not going to be ductile.

Now, the next mechanism of cladding embrittlement that really is the main burn-up effect has to do with hydrogen enhanced beta layer embrittlement. We'll just go ahead and look at the data while I talk.

These data are fresh zircaloy for -- and you can see this is ductility. It's going to zero, somewhere around 14 percent. We can talk about zero being zero or being one percent or two percent.

Once we get down to about one or two percent on a mechanical test, the material has lost its ductility.

And here is for the very same cladding type, high burn-up PWR cladding type from the H.B. Robinson plant. You can see the ductility is being lost much lower than 14 percent, somewhere down around eight percent.

What is happening here is that the hydrogen that was absorbed into the cladding during normal operation has affected the diffusion rate and the solubility of oxygen into the beta phase. The fusion rate through the alpha phase and the solubility limit in the beta phase are both probably affected by the hydrogen and result in higher oxygen concentration in the beta phase and, therefore, more loss of ductility.

Another embrittlement mechanism is related to the oxidation break-away process. We had shown this committee pictures of the Russian cladding at an earlier time. We came across this effect a few years ago in the Eastern European literature. We got some of the Russian E110 cladding into the laboratory, tested it, and found that the oxide that was forming during steam oxidation would break up and then it would allow hydrogen to be absorbed very rapidly into the cladding, and this hydrogen, in turn, would have the effect that I just talked about. It would accelerate the oxygen embrittlement process.

Now, what's happening here is that ZrO_2 can exist in several crystalline forms. The one that normally forms on the cladding surface is black and shiny, and it's relatively protective. So hydrogen can't go through it very readily. It's a tetragonal phase, and it's what we normally see when we oxidize in steam at high temperatures.

Another form that is actually thermodynamically stable at some of these temperatures is gray when it first appears, and it's cracked, and it's a monoclinic form, and it lets the hydrogen in.

Let me show you some data. These are old data from Leistikov and Schanz in the 1980s. They did this work in Karlsruhe. It's a huge study of oxidation of Zircaloy 4 in steam. I know this is a little bit fuzzy, but they went from temperatures ranging from 600 degrees Centigrade all the way up to 1,600 degrees Centigrade, and they went out to 25 hours in their annealing time.

And what you see here is as you look at each of these weight gain versus time curves is that a time comes when the rate increases rather abruptly. This is when the break-away process takes place and where the oxidation accelerates rapidly.

Along with that you would have rapid absorption of hydrogen. When you get up to 1050 -- let me see where 1050 is. Ten, fifty is right here. That's the last one where you see a little bit of break-away, and at 1100 you see no break-away anymore, and I think that the tetragonal phase is thermodynamicly stable above 1,100 degrees Centigrade, and it just won't switch to the monoclinic phase, but below that it can switch.

And whether it switches or not depends on some things that we know about now, but weren't known a few years ago. What we've done is taken the times that correspond to the break-away process and plotted them on this next slide.

And so for Zircaloy 4, you'll see that the break-away process is starting as low as about 25 minutes at a temperature around 1,000 degrees. There's another broad minimum that's almost that low, over around 750, 800 degrees, and then this strange temperature around 900 where it seems to be fairly stable and we don't understand why that takes place.

But what we do know is that all of the zirconium alloys that we've tested, and we tested a lot of them, they all show this break-away phenomenon eventually at one time or another. The worst one that we've tested is the Russian E110 cladding, which had a break-away at about less than ten minutes, and most of them were higher than this.

This, by the way is old zircaloy. It's zircaloy from the 1980s which had a rough surface and which broke away at an earlier time than the later stuff did.

The factors that we found mostly from studying the Russian alloy that affects this are the surface condition and the allow ingot impurity content. What you're dealing with here basically is crystal growth on a substrate, and it's sensitive to what the substrate looks like. If the surface is very rough, it tends to grow the monoclinic form much easier than if the surface is smooth.

There may be some surface contaminant effects as well. We've found that if you etch smooth cladding that it will tend to make it worse and probably picking up something from the etchings on the surface.

The other things that seems to make a difference is the impurity content in the starting material.

The Russians were using an electrolytically refined zirconium which is very pure, and they were getting this effect. But they had bought an ingot from Sayzus in France that had the standard spun zirconium process to get the zirconium alloy, and they made tubing out of that. And that tubing behaved substantially better than the standard E110 tubing.

This was done in a program that Kerchottof ran for us under the joint program that the NRC ran with IRSN in France and eventually with additional support from Tuvell, the Russian manufacturer.

And we have some general understanding of why that takes place, and it has to do with in the niobium alloys, niobium like zirconium has a very high free energy formation, and so the niobium is much more prone to get into the oxide than the tin is. And you put niobium into ZrO_2, and it has got the wrong cation valence.

And what we think is that the calcium that comes in from the CROL process probably counterbalances that and so that it behaves more normally when you have the good impurities present, and so the trick seemed to be to use spun zirconium and have a nice poly surface on the cladding and the break-away process is suppressed.

So that was a fairly interesting episode, but it did tell us that we need to be careful to look out for break-away because it is possible within the normal range of manufacturing things to either get an ingot that you might think is really nice because it's very pure or to do something to the surface that could affect us.

So it's easy to test to see the result, and we don't have to get involved with any manufacturing processes.

And then finally, the sixth thing that we realized was that you can get oxygen diffusing into the metal from the inside diameter even away from the balloon in high burn-up fuel because keep in mind you have a huge oxygen source inside the cladding, the fuel, UO_2. And uranium, plutonium, zirconium oxides, you know, they're all about the same location on an Ellingham diagram, and so the oxygen can move fairly freely.

We found some earlier test data that showed that you could get this kind of pickup from the UO_2 fuel if you had contact, good, intimate contact between the fuel and the cladding.

Now, what happens at high burn-up is that after a while you get an interaction between the fuel and the cladding, and you get bonding, and the bonding layer that develops is nearly pure ZrO_2. You can see it in the samples. We see it in our high burn-up samples. When you run them through a high temperature transient with steam on the outside only you get an alpha layer on the inside that's just about as big as the one that you had on the outside.

And so it's quite clear that what we're dealing with is like a diffusion couple where you have the metal on the inside sandwiched between oxide on the OD from steam and corrosion and oxide on the ID from the bonding layer and any fuel that's bonded to it, and now you heat this up during a high temperature transient, and oxygen diffuses in from the ID at approximately the same rate that it diffuses in from the OD.

And so the indication is very strong that we should be accounting for ID oxygen ingress even away from the balloon region.

So here are a set of criteria that are close in form to the original criteria, but which take into account most of the phenomena that I just discussed. These work in the laboratory and would be the criteria that we would recommend be looked at for possible use in the regulation.

We'd stick with the same temperature limit that's in the current regulation. We have indications from our data that once you get above about 1,200 degrees Centigrade the oxygen solubility in the beta phase increases pretty significantly, and so since that number is well ingrained in current regulatory process and sine it's related to something that we want to avoid, we just stayed with that number and used the time limit to account for the rest of the variables.

So what works for us is to make sure that the calculated oxidation is less than the measured oxidation corresponding to the transition from ductile to brittle behavior, and subtracting from that something a little larger than the measured or expected corrosion thickness on the high burn-up fuel expressed also as an ECR value.

CHAIRMAN ARMIJO: What's the basis for the 1.2? Why do you do that? Is that hydrogen?

DR. MEYER: It's simply empirical. Mike is going to show you the data and show where this came from.

It depends most strongly on the temperature transient, on heat-up rates and cool down rates. You could have several different -- I mean you can imagine a number of different transients that will have different heat-up rates and cool-down rates, and so this is sort of a middle of the road value.

CHAIRMAN ARMIJO: That's not alloy dependent at all?

DR. MEYER: I can't say it's not alloy dependent because we don't have high burn-up data yet on the other alloys, but I'm going to show you some examples where I think its alloy dependence is less important than you might initially suspect.

Okay. So we're going to calculate the ECR with the Cathcart-Pawel equation all the time. I'll come back to this matter of the two-sided oxygen pickup in a minute, and then we've also measured this minimum break-away time, and we're quite sure that if you don't exceed that time for the temperature that you're above 650 that you won't have a break-away problem.

So we can come back to any of these you want to, but let me first give you some examples just to show how this plays out.

So here is ZIRLO, the Westinghouse cladding. This is the current stuff, 17 by 17 belt-polished, standard ZIRLO. We measured the unirradiated transition, the transition from brittle to ductile, from ductile to brittle behavior at 19 percent, just a little higher than the 17 percent that's in the rule.

And this is just an example. We took an arbitrary case where we had 40 microns of corrosion for the 17 by 17 geometry. That's about four percent in ECR terms, and we multiplied the four percent time 1.2, subtract it from 19, and get 14.2.

Okay. I'll come back to that thought in just a minute.

This number is just in here for calibration purposes. Had this transient gone immediately up to 1,200 degrees, stayed there for 190 seconds, and come back down, you would have gotten the

14.2 percent. I just wanted to give you that number in reference to something on a subsequent slide.

And then here is the measured break-away time for that material.

Here is the numbers for M5, and you know, it's the same current material, the same arbitrary case of 40 microns. It's just a very minor difference in the measured ECR for the unirradiated material. The limit in this case would be 15.2, and here is the break-away time.

Now, this one is going to be a little bit more interesting. This is zircaloy, real material, current material, 17 by 17 belt-polished, low tin zircaloy measured in the laboratory right at 17 percent.

Now, we have a de facto corrosion limit that's used in safety analyses of 100 microns, and zircaloy can get that much corrosion on it if you push it hard enough. And so I've taken this example right at the limit. So this would be what I call a worst case zircaloy example, and the 100 microns is about ten percent ECR, and you multiply that by 1.2, subtract the 12 from 17, and you get five percent, a fairly small number.

Also look at this. If you could take that cladding up to 1,200 degrees instantaneously and leave it up there, it would only be up there for 24 seconds to get to five percent. With this limit, you would never quite get up to 1,200 degrees Centigrade in a real reactor transient because they don't go up instantaneously.

DR. POWERS: Let me ask you a question about this --

DR. MEYER: Yeah.

DR. POWERS: -- Cathcart-Pawel.

DR. MEYER: Right.

DR. POWERS: Which figures prominently in all of your discussions.

DR. MEYER: Yes.

DR. POWERS: When I look at this report from Argonne of a variety of experiments, they're very nice, and they say, "Well, we measured this and then we compared it to what we would have calculated using Cathcart-Pawel.

DR. MEYER: Yeah.

DR. POWERS: And in some cases there's extraordinary agreement. In some cases there is extraordinarily systematic under prediction --

DR. MEYER: Yes, exactly.

DR. POWERS: -- by Cathcart-Pawel.

DR. MEYER: Yeah.

DR. POWERS: When I think about Cathcart-Pawel, what he did, they did, was they measured a bunch of stuff. They took their measurements, converted them into some sort of parabolic rate constants, took the logarithm of those, plotted them in a straight line, fitted it to a straight line.

That means that there's some uncertainty in their predictions. What I don't know is what the magnitude of that uncertainty is. Do we understand that?

DR. MEYER: I don't want to answer that question. Maybe Mike will answer that question. What I want to say is that we're using Cathcart-Pawel equation simply as a variable transformation for time, and we're fully aware that for some of these alloys there are substantial differences between the true weight gain and the calculated weight gain. We are simply using it as a parameter against which to correlate empirically the ductility measurements.

For example, as 1,000 degrees Centigrade, the Cathcart-Pawel oxidation predictions are much higher than the measured values for oxidation of M5, for example.

Now, you might not want to use that

Cathcart-Pawel equation in calculating the metal water heat for your thermal hydraulic analysis, although I think in all cases the Cathcart-Pawel would be conservative. So it would not be nonconservative to use it.

But for our purposes this discrepancy between the calculated weight gain and the true weight gain does not in itself lead to any error in what we're doing.

MS. UHLE: This is Jennifer Uhle from the staff. Ralph, you had on your I think fourth or fifth slide an indication that provided you are using the same oxidation kinetics reaction to reduce the data, and in the calculation of the percent ECR, and that's why it doesn't enter into our adding a specific error.

DR. MEYER: I don't think I have a slide on this, but the situation was almost exactly the same originally --

MS. UHLE: Ralph, you do.

DR. MEYER: -- with Baker-Just. The Hobson's data were analyzed with calculated weight gains, not measured weight. He didn't measure them at all, and so if you reduce the data using Baker-Just and then when you turn around and make a calculation, you should use Baker-Just. So Baker-Just was written into the original rule in Appendix K of Part 50 for that reason.

And it's the same here. We are analyzing the data that way, and they should be used with the Cathcart-Pawel equation when you turn around and make a prediction.

MS. UHLE: The slide I'm referring to is Slide 8.

(Pause in proceedings.)

DR. MEYER: Should I go on?

CHAIRMAN ARMIJO: Sure.

DR. MEYER: Now I want to point out something that is kind of neat. This isn't a picture of a real test, but I just want to use this as an example just as a side calculation here.

In a typical LOCA you would expect that the maximum oxidation would occur in the balloon node in the calculation, in your safety analysis calculation of the LOCA. You would expect the maximum oxidation to occur in the balloon node because here you have oxygen in the calculation coming in from both sides. So you assume two-sided oxidation in a licensing calculation.

And, furthermore, the cladding is thin, and you take that into account in running the analysis.

At a different location you would probably find the peak cladding temperature node because you had additional cooling down here. So generally you find the maximum oxidation in the balloon and the peak cladding temperature somewhere outside of the balloon.

And so I've taken an artificial case where I've said let's put the peak cladding temperature at about the limit and put the maximum oxidation at about the limit and just assume for the purpose of the example that the balloon is running 50 degrees cooler than the hot spot.

Okay. So now if I run the calculation at 1,150 degrees Centigrade until I get 17 percent ECR, it takes 366 seconds. If I now slip up on the rod here and ask what has happened to the ECR in 366 seconds, it has gotten 15 and a half percent, providing I've assumed double-sided oxygen pickup, which would be appropriate for this rod, which is a high burn-up rod.

So 15 and a half percent, 17 percent are fairly close numbers, and the point that I want to make is that whether you're doing two-sided oxygen pickup away from the balloon in the peak temperature node or whether you're doing two-sided oxygen pickup in the balloon where you have a slightly lower temperature, you're going to get about the same number, about the same answer.

With the information that we now have about the loss of ductility in the balloon due to the additional hydrogen absorption, it seems to me that continuing to do detailed calculations in the balloon is not particularly meaningful because we can run the calculation and the balloon is going to have lost the ductility anywhere.

So if you were to give us a piece of a high burn-up fuel rod taken out of a reactor, I think we could make a very good prediction of whether it would be brittle or ductile after running it through a given LOCA transient just by looking at the two-sided oxygen pickup away from the balloon and forgetting the balloon.

Now, okay. Let me go on and give you my last slide and then we'll see if we want to go back and dig into any of these things again.

So now we're going to say that we believe that the current operating reactors are safe with regard to the LOCA analysis even though the 50.46 limits on the books are not doing the job well because of several things.

First of all, temperature limits are the same. So there's no change there. We are subtracting the corrosion thickness from 17 percent in response to the information notice in 1998. So even though it's not in the rule, it's being done.

Seventeen percent turns out to be conservative for all temperatures below 1,200 degrees Centigrade, and I think this was known originally. If you look at the transition ECR at 1,200, 1,100, 1,000, the transition slips up a few percent each time. So particularly for cases like that worst case zircaloy example that I put up where you had a low limit of five percent, if you're making the subtraction and not even getting up to 1,200 degrees Centigrade in the transient by staying under the limit, there would be a little conservatism there.

And now finally, although we are not for the present licensed cases, we are not accounting for oxygen that diffuses from the inside diameter of the cladding from the bonded fuel away from the balloon. We are doing the calculation in the balloon, and as I showed you in the example, you get about the same answer whether you're doing the calculation with two-sided oxidation in the balloon or outside the balloon.

Now, there's some urgency about revising the criteria now because you can imagine some cases where if the rule were interpreted strictly or somebody forgot to take into account one of the things that's being done up here, you could have a case that satisfied the rule and did not produce the desired result.

We have two cladding types that have been approved for use in U.S. reactors which at the present time require license exemptions because the language of the present rule does not cover those two, and we'd like to fix that situation.

It is not a stretch of the imagination to think of manufacturing changes that could be made, particularly in this time when there is concern about the security of supply, where manufacturers are setting up alternate fabrication areas and buying materials from all over the world on the market that you could get a cladding that had poor break-away performance if you weren't looking for it. And there is a test in our criteria that takes account of that.

And also, I know there's a desire the committee itself has talked about trying to get these criteria resolve before moving forward with the 50.46(a) rulemaking, which involves the transition break size definition.

We're painfully aware that we don't have as many data as we would like to have. It is simply not going to be possible within the next couple of years to get a lot more data. We do have a few pieces of high burn-up ZIRLO and high burn-up M5 which we are going to test within the next weeks before the report is finished. They have fairly low corrosion on them. So I don't expect there's going to be any surprising result from those tests.

We do not have available more heavily corroded high burn-up samples of ZIRLO or M5, nor do we have a hot cell at the present time where we could do those tests. So we are limited in that way, but on the other hand, we have learned an awful lot from the work that we have done, and I think that we could make a rule change now that would be infinitely better than the rule that's on the book, still have some uncertainties in it which could be confirmed in three to five years in the following phases of the local work that would continue.

That's al I planned to present. If you have any further questions, I'll take them before we move on.

CHAIRMAN ARMIJO: Any questions from the sub committee?

DR. SHACK: In your current operating reactors, your 17 percent is being calculated by Baker-Just.

DR. MEYER: Not all the time because the 1988 rule change that allowed the best estimate calculations, the reg. guide for that actually mentions Cathcart-Pawel. So generally Cathcart-Pawel or Baker-Just are being used right now rather than any alloy specific best estimate models.

CHAIRMAN ARMIJO: Okay. If there's no more questions, let's move on. Our next speaker is Mr. Billone, ANL.

DR. BILLONE: I'm Mike Billone from Argonne, and do we have any experts in getting my presentation up? Let's see.

Okay. I'm going to present the data we've generated and then try to close the loop with what Ralph just presented, maybe not as definitively.

And so I've divided the presentation up into three parts. You perhaps have two parts by now. The first part will be our data on as fabricated and prehydrided cladding alloys. I'd like to acknowledge Dr. Yuen Yan who generated most of this data and Tania Burtseva for the careful characterization she's done pre-test and post test on this material.

Okay. What we looked at are the effects of high burn-up, mainly hydrogen, the effects of hydrogen on post quench ductility. And to perform a baseline we did a lot of work with as fabricated cladding alloys. Our based alloys for the program are Zry-4, Zry-2, modern Zry-4s or 2, ZIRLO and M5.

We did work on prehydrided cladding alloys, specifically Zry-4, focusing on the most embrittling temperature was 1,200 degrees C. as a surrogate for high burn-up effects, and then we did testing of actual high burn-up Zry-4, 1,200 degrees C., with plans to test ZIRLO and M5, and these are de-fuel cladding tests in the temperature range of 1,000 to 1,200 degrees C.

At a lower temperature range, in particular, 800 to 1,000 degrees C., based on the schematic Ralph showed you, there's break-away oxidation eventually at some time, hydrogen pickup and embrittlement. So we have kind of divided our work up into 1,000 degrees to 1,200 degrees C. for classic embrittlement, and then the embrittlement due to break-away oxidation.

Now, what's new since I was here last July -- well, not last July; July 27th, 2005 -- and had a long presentation, we discussed the importance of small break LOCAs and data on break-away oxidation as a possible limit, more stringent than the kind of ECR limits we were placing based on high temperature.

So we generated break-away oxidation and for as fabricated cladding alloys and what is reported is Zry-4 and ZIRLO in the draft that you have. Since that time we've also done Zry-2 cladding, and I'll present those results.

So we have finished three out of the four claddings we had planned. M5, there is published data on break-away oxidation for M5.

Another issue that came up was the effects of cooling rate and quench temperature and possible enhancement in ductility just depending on how you cool the sample from your peak temperature, and we looked at the effects of quench temperature in the range of 600, 700, 800 degrees C. versus just cooling without quench at all, and those effects on ductility. So that's new data since that summer.

We refined our interpretation of data for prehydrided and high burn-up Zry-4. Our colleagues at CEA in conjunction with AREVA and EVF did some very excellent scientific work showing in quite vivid detail what happens to hydrogen and oxygen within that prior beta layer during slow cooling versus rapid quench, which helped us to understand our phenomenological results a lot better.

We generated a draft LOCA NUREG report. It's still in draft form. We're still adding data in different points, and that data is January 9th, 2007. That's in ADAMS. It also has an ANL number. It will eventually have a NUREG number.

Another big change since this date, actually precedes it by one day, is effectively our alpha-gamma hot cell facilities where we were doing this work with fuel for defueled cladding has been closed to programmatic work effective July 26th. That was a temporary closure, and then January 13th, 2006 was the more permanent.

The word "more permanent" sounds like an oxymoron, but it's currently not available to us. So we've moved our operations to a beta-gamma cell, which means we can't bring fuel in there, but we can still test defueled cladding as we've been doing the ductility.

It has been a lot of work to move everything out of the alpha-gamma hot cell facility boundaries. If you think of this room as the actual hot cells and then you move out to the empty spaces between the hot cells and the boundaries of the hot cells and then you go upstairs, which shares a common nitrogen system, an air conditioning system, a fire alarm system, the alpha-gamma hot cell facility boundaries are much, much larger than the cells themselves, and we basically had to move all of our labs and all of our equipment out of those boundaries.

So we've made a lot of progress, but it has taken time, in reestablishing our capabilities for sample preparation of high burn-up fuel, pre and post test characterization, and actually testing of defueled cladding samples, and that's just coming together now.

So that's what's new or what's different. Let me talk about the structure of the report, and I'll try to parallel that in my presentation. Section 1 is introduction. Ralph gave you some of the heart of the introduction.

Two is a description of the cladding alloys, geometry, chemistry, surface roughness as best we know them that we've used in our program, and I tried to give you a fairly detailed description of our testing methods, how we oxidized samples, how we quenched samples, how we performed the ring compression test, how we interpret data.

There's a lot in there about temperature control and temperature monitoring, which is extremely important.

In Section 3, we present our results for as clad fabricated cladding alloys. Actually as far as the program is concerned, the modern Zry-4 17 by seven Zry-4, ZIRLO, and M5, along with the ten by ten Zry-2, which we finally got around to testing, formed the heart of our program. We added E110. You need a bad alloy in this bunch. Otherwise if your screening tests don't screen out the bad alloy, there's something wrong with your screening tests.

And we also wanted to try to understand why M5 behaves so well at 1,000 degrees C. oxidation and E110 was so poor.

Now, our high burn-up fuel is not of this particular design. We have Limerick high burn-up BWR fuel, which is nine by nine, slightly large diameter and thicker wall than the ten by ten Zry-2. So we have done several testing on that s to generate baseline data for what the high burn-up fuel is that we have.

We've done a lot of work with 15 by 15 Zry-4, which is the design for the H.B. Robinson high burn-up Zry-4 that we have.

We also have some 15 by 15 M5 not for data generation, but for kind of validation. So if you get results for thin wall 17 by 17 M5, you want to check and make sure that wall thickness doesn't somehow alter the results because you're accounting for wall thickness. So you run a few quick tests with M5, 15 by 15 thicker wall at the same conditions as the 17 by 17.

Again, in two temperature ranges we looked for high temperature embrittlement in the range of 1,000 to 1,200 degrees C., along with the effects of quench. The new studies of break-away oxidation. That matrix of tests, I don't know how long Leistikov and Schanz had to do that work, but the '80s were a glory period, and it probably was cladding of the 1970s. The test matrix gets really extensive if you want to cover the whole temperature range of 800 to, let's say, 1,025 degrees C.

What they did was they went in increments of 50 degrees C. When you look at the unusual behavior and the two minimums, if they studied 950 and 1,000, you don't know if 975 is worse or not. Plus break-away is an instability phenomenon. You could run one test and the material looks good. You can run it under the same conditions and the material will pick up hydrogen.

So you get into a lot of tests. So we kind of focused our range on where the oxidation rate would be highest, where the hydrogen generation rate would be highest, and once you hit break-away, hydrogen pick-up would be relatively rapid, and that's 950 to 1,025, and then just to confirm whatever minimum time we got up here, we ran at 800 to make sure that we didn't pick up any hydrogen at that time, the 800.

So we did not study every degree C. in this range. That was the approach we took.

That's all in Section 3. We did look at the effects of surface conditions because of our work with E110, which is an unstable alloy. So if it's scratched, it behaves worse. If it has got a rough surface it behaves worse, and we're just conscious of the possibility, and we found that for the modern cladding alloys that we looked at the scratches seemed to have very little effect, scratches as deep as 20 microns, 25 microns; had very little effect on the break-away time of stable alloys, which was encouraging. It was something I was concerned about.

Section 4 is prehydrided alloy --

DR. POWERS: Let me ask you --

DR. BILLONE: -- 17 by 17 Zry-4, until we ran out of that material, and then a lot of results for the Robinson baseline material, the 15 by 15 Zry-4.

DR. POWERS: Mike, let me ask a question.

DR. BILLONE: Yes.

DR. POWERS: More out of curiosity than any substance here, as long as I've got you.

You spend a lot of time in the report discussing about belt sanded, belt sanded, belt sanded.

DR. BILLONE: Right.

DR. POWERS: But I don't recall ever seeing any of the details about what belt sanded means.

DR. BILLONE: What belt polished means.

DR. POWERS: It must mean some grit was used at some size, and what is that grit?

DR. BILLONE: I think it varies from vendor to vendor, and it's more appropriate for the vendor to answer. I think it's silicon carbide in some cases. Typically alumina or silicon carbide are acceptable candidates for belt polishing.

I don't know the details as to whether there's a slight change in surface chemistry when you do that or whether there's some cold working --

DR. POWERS: Oh, I'm sure there's some cold working.

DR. BILLONE: I know, but I don't have -- I basically have what we can measure, is we can measure the surface roughness.

DR. POWERS: Right. Good. The other thing that arises as you read through your report, especially when you're talking about irradiated fuel, is you say, gee, you know, fuel at the top of the core is different than fuel at the bottom of a core.

DR. BILLONE: Right.

DR. POWERS: And PWR fuel likes to do weird and perverse things with boric acid. Does that affect your results at all?

DR. BILLONE: Basically what we look for is certain corrosion layer thicknesses and hydrogen content because we knew in the end we were going to try to interpret our results that way.

For the BWR fuel that we had, other than having some tenacious crud still attached to the OD, we found very little variation. We basically looked from the core mid-plane for our test samples, and we found very little variation in corrosion layer thickness. In this case I'm talking about the oxide layer plus the crud layer. It was fairly uniform because the coolant temperature is uniform.

You get the biggest variation -- and again, we only have one type of high burn-up fuel from one plant. So we were not able to look at variations from plant to plant. But, of course, with PWR fuels, Zry-4 anyway, you get quite a significant gradient in oxide thickness and hydrogen.

Beyond that I don't know.

DR. POWERS: Okay. So you don't know of small levels of boron incorporated in the oxide will do anything to you.

DR. BILLONE: No, I suspect not, but I don't know.

DR. POWERS: I have no reason to think so either.

DR. BILLONE: We test what we get. Okay.

DR. POWERS: Similarly in your report --

DR. BILLONE: Go ahead.

DR. POWERS: -- you propose your prehydriding. It was a rather fascinating discussion in your difficulties in getting uniform hydride, but you develop a work-around, but you never comment very much. I mean, I suspect that high temperatures, hydrogen is fairly mobile, and yet in your experiment you couldn't homogenize the hydride very much. Did you ever sort that out?

DR. BILLONE: Yeah, there's two parts to your question. One is in the actual prehydriding operation, which is done at the lower temperatures, like 360 degrees C. to 400 degrees C., and that's very much of an art, and what you're reading and what I wrote is about all you're going to read because everybody else's method is proprietary basically.

DR. POWERS: Well, I was fascinated by it.

DR. BILLONE: But the decision I had to make was do I live with the non-homogeneity in the prehydrided material or do I homogenize? In the alpha phase it's easy. At 400 degrees C. for 72 hours you can homogenize that hydrogen. But you're usually in an argon atmosphere with some oxygen impurity, moisture impurity, nitrogen impurity. So you're going to be changing the surface, fine oxide and maybe nitriting of the surface depending on your impurity levels in that annealing process.

So I chose to go with the samples which had variations in hydrogen content. That's part of the question. Given that those samples had axial and circumferential variations, I thought taking it up to close to 1,200 degrees C. I know across the radius of the cladding -- I wasn't talking about across the radius. Beta has a very high affinity for hydrogen, and that hydrogen essentially homogenizes in the beta phase rapidly across such a short distance, which is like .6 millimeters.

But during a test that's 100 seconds because there wasn't enough time for the hydrogen to homogenize in a circumferential direction and the axial direction, and I know some people are very upset about that data. They don't like the data, but I think rather than criticizing the data, we should find an explanation for why hydrogen diffuses slower in the presence of increased oxygen rather than to say the data are impossible.

So it was interesting. It was a side discovery maybe. What it has significance for with high burn-up fuel, of course, you have a hydride rim near the outer surface of the cladding, and the hydride density decreases as you move to the inner surface. That's across the radius and you expect that to homogenize when you form the beta phase, but you have edge rods. You have corner rods. You have rods which do not have a symmetric temperature distribution around them. So you could get gradients, and we do find gradients in our H.B. Robinson rods and hydrogen, and it's just interesting to note that during short time tests, 100 seconds, 150 seconds that hydrogen may not homogenize. So if you have 400 ppm here and 700 ppm there, that sample may be very brittle.

Okay. Section 5 is post quench and post oxidation ductility. We didn't quench all of the samples. I'll explain why, of high burn-up Zry-4.

Six, I know we end up with the conclusion that there's no way of preserving ductility in the balloon region, and I show a lot of results here that are of interest, but the only ones I'll show today are basically the hydrogen pickup after I think back last July when I was here and brought two samples that we had, interval samples that are ballooned and burst. One was ramped and held for one second at 1,204 degrees C., nd it picked up about three -- at the time I didn't know that, but it picked up about 3,000 ppm of hydrogen.

And the other sample was held for two minutes or 120 seconds and then picked up a lot of hydrogen, al from this secondary hydriding. So this is more confirmation of why we don't think we can preserve ductility in the balloon region.

However, there is some discussion, and I know -- I mean, strength is not a dirty word. The fact is that if you are at a low oxidation level, you have a very significant fracture toughness of the material. It's brittle if you were going to do a ductility test, but if you whack it with a hammer as I did then, that material is very resistant to any kind of fragmentation.

If I let the oxidation continue in the presence of the hydrogen, then the strength of the material goes down. The toughness of the material goes down, and I can take it in my hands and just easily snap it or I could tap it with a hammer.

So you have to think beyond ductility in the sense that you may still want to limit the oxidation level in the balloon region to retain some strength, but you don't have a chance at retaining ductility.

Now, this is all we really initially promised, and so the heart of the data is Section 3 through Section 6. We thought in Section 7 that we would -- again, you have to realize that our data goes out independently to industry and to licensing or research and licensing and NRC, and for them to independently assess the data and the application of the data.

We thought we would take a shot at this in modifying what was in Information Notice 9829, really testing it. Actually the word is "testing." Does it really work? Is it adequate? Does it mean that F factor times the corrosion layer to make our data?

So that's really the spirit in which we did the empirical criteria for embrittlement, plus included the -- beyond this, we included the break away oxidation, but up to this point is data generation with some mechanistic explanation of why the data behaves the way it does, and I'm still revising this part.

Okay. Ralph gave you some good background. Let me try to elaborate on it. As far as post quench ductility for cladding oxidized in the higher temperature regime, Ralph's schematic shows oxide alpha and beta layers. I've got the same schematic.

Following quench the beta goes to alpha. We call it prior beta. Embrittlement, al of our testing, we either test it at room temperature. If we had adequate ductility, we said we were done up to high UCR values. If we found embrittlement at low ECR values, we retested 135 degrees C., which is kind of where the reactor is just following quench of the core.

And embrittlement at that temperature, if you're going to squeeze a ring, for example, at 135 degrees C., it seems to occur in zircaloy for a beta layer oxygen content somewhere around .55 weight percent. You can call it .6 if you'd like, and if you look at the solubility, how much oxygen can the beta layer handle?

This is just for understanding. If you take as fabricated Zry-4, the oxygen solubility is low. It's .24 at 1,000 degrees C., which means you can oxidize that for a long period of time and you're never going to get that beta layer brittle. At 1,000 degrees C. you'll probably break away and pick up hydrogen before you embrittle the beta layer with oxygen.

And it goes up to about .57 weight percent for 1,200 degrees C. So this is just right at the border where if you oxidize to about 17 to 20 percent oxidation level, you will get embrittlement at 135 degrees C.

Now, we're interested in hydrogen because of high burn-up. If you look at prehydride in Zry-4, the oxygen solubility at 1,200 degrees C measured increased from about .6 to 1.1 percent, which is well above that with 600 weight parts per million hydrogen. It's not that much different with 300 weight parts per million hydrogen.

So hydrogen, what it's going to do is -- let me go to my figure. This is the figure Ralph is showing -- hydrogen will do two things. Right at this boundary is the solubility limit of oxygen in the beta layer. If you add hydrogen to the material and all hydrogen in this material will go to the beta layer, which has the high affinity, then this value will go from about .57 to about 1.1.

At the same time, the long time solution will go from this level to this level. Basically, the hydrogen will increase your concentration gradient, drive oxygen into that beta layer faster and the long time solution will be significantly higher oxygen level.

So that's what we know about the effects of it. In addition to increasing the oxygen content, the rate at which we pick up oxygen and your steady state rate of oxygen, your solubility limit.

We do notice that there's an intrinsic hydrogen induced embrittlement above and beyond just increasing the oxygen. It's low, but it's very significant. It takes us from samples that we thought should be brittle to just above the ductility criterion.

So embrittlement rates the concentration gradients in that beta layer will increase with temperature, which will drive that boundary condition up and with hydrogen, and the diffusivity of oxygen in the material loss will also increase with temperature.

So in terms of embrittlement, increasing the temperature, increasing the hydrogen content, all push you in that direction.

We switched to break-away oxidation. Ralph mentioned that at high temperature the oxide is black by appearance in the tetragonal phase. It transitions to the low temperature oxide monoclinic gray phase, which is what you see in reactor fuel rods that you pull out of the reactor.

The weak monoclinic oxide cracks give an increase in oxygen and hydrogen pickup, and it's strange. The tetragonal phase is I'll call it meta-stable below 1,150 degrees C. because you can grow this phase and it will appear without any monoclinic oxide as low as 950 and even lower.

DR. POWERS: Well, you call it meta-stable. I don't think it's meta-stable. It's stabilized by the non-stoichiometry.

DR. BILLONE: It's stabilized by the non-stoichiometry, and it's also stabilized by growing under compressive stress.

DR. POWERS: Okay. So it's an epitaxial.

DR. BILLONE: Yeah. I'm thinking of a very layman term rather than a -- usually when we talk about ZrO_2, it's just very hard to measure the oxide phase transformation temperature because it's very hard to form it and grow it without some non-stoichiometry and without some stress.

So that's why in the literature you'll see 1,100 to 1,150 as a phase transformation temperature, but the statement is there's a lot of uncertainty. The important thing for our work is if you just forget this here. Initially at high temperature you will grow the tetragonal phase. As the temperature gets lower, below this limit, you get farther away from that, and as you drive the material more towards ZrO_2 stoichiometric, then you're driving it towards the monoclinic phase.

But what seems to be important is how this oxide layer breaks up. The precursor to break-away stability is a very wavy boundary that I'll show you between the oxide and the metal. That means you've got regions of alternating tensile and compressive stress, and that precedes the break-away, the transition to the weak monoclinic phase and the cracking, and so the instability starts at the metal oxide surface and then propagates outward and then propagates axially and circumferentially.

Okay. This picture Ralph showed. Let me go back a second. Let me try this. This came out a little too dark. Sorry about that.

These are two samples just for fundamental understanding. These are two samples of HBR type 15 by 15 low ten, Zry-4 oxidized at the same temperature to the same oxidation level. One is as fabricated, and these were quenched at 800 degrees C; the other, that's 600 weight parts per million hydrogen.

This sample without hydrogen as expected at well below the saturation level of oxygen, this sample is extremely high ductility at room temperature and 135 degrees C. The sample with the hydrogen is extremely brittle.

The metallography doesn't tell you much. The oxide layer is about the same. The alpha layer is about the same, and the thickness of the beta layer is about the same.

What's different about these two materials is this one has a lot more oxygen inside here due to the hydrogen that you've added to the material.

CHAIRMAN ARMIJO: Can you discern any

hydrides from your metallography of these?

DR. BILLONE: You never can see hydrides in the beta phase. Our French colleagues have shown hydrides under certain slow cooling situations. If you rapidly quench, you kind of freeze the hydrogen --

CHAIRMAN ARMIJO: Too small?

DR. BILLONE: -- you freeze the hydrogen in.

CHAIRMAN ARMIJO: In solution?

DR. BILLONE: In solution basically. If you slow cool to room temperature, like from 800 degrees C. to temperature, that gives time for very small hydrides to precipitate, and those have been observed at CEA.

So basically the quenching process kind of freezes the hydrogen into sort of the low oxygen beta phase at that temperature and then it's frozen and you don't see that it's hydrides.

Okay. To support my point as to one of the things we do beyond just taking pictures, this is extremely brittle. If it's brittle it should show up in micro hardness, which is making small indents across the radius and correlating that with a diamond point hardness.

So just for fundamental understanding, I'm just showing you the beta phase of the as fabricated material, and the lower the hardness valued, the higher the ductility. So it makes sense that this material, which only goes up to at this boundary about .57 weight percent of oxygen, that this would be ductile, whereas the red curve is for the prehydrided sample. The oxygen content is gone from about .57 to 1.1. The hardness has increased significantly, and then you've driven in more oxygen through the concentration gradient and the overall hardness has increased.

These are room temperature values.

CHAIRMAN ARMIJO: You said the oxygen gradient follows those curves?

DR. BILLONE: Approximately, yes.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: But basically this would be another way of looking at the effect of what the hydrogen does to the material.

CHAIRMAN ARMIJO: But I thought you said that the hydrogen would be uniform across the --

DR. BILLONE: Hydrogen is uniform across the material, but you have oxygen diffusing from this side and oxygen diffusing from that side. So the long time solution would be flat at the solubility limit.

You get embrittlement way before that.

Okay. This is really more conceptual because I don't have enough data to do this plot, but basically we have data from Chung and Kassner, 1979, on what the oxygen solubility limits in the beta phase are. Those are the numbers I gave you, .24 at 1,000 degrees C., .38 at 1,100 and .57 at 1,200 degrees C.

If we say that when the average oxygen concentration gets up to about .55, that represents this slide. So the point there is in order to embrittle as fabricated materials, you've really got to go to about 1,200 degrees C. to eventually embrittlement. It may take a while to get it.

However, if you add hydrogen to the material, and this is some CEA-AREVA-EDF data, based on a very different technique than this one, and this is 320 weight parts per million, 320 weight parts per million hydrogen, and this is 600, the only two data points I really have.

And so basically the oxygen content does increase; the solubility limit does increase with hydrogen, and so I've just given you kind of a cosign function fit to that.

CHAIRMAN ARMIJO: There are no other data in the literature to help you fill out that curve?

DR. BILLONE: No published data yet. What I don't have is data at 1,100 degrees C. and 1,000 degrees C. So I just basically assumed the same difference just for illustrative purposes.

So certainly 1,100 degrees C. is benign as far as embrittlement. As fabricated eventually if you put enough hydrogen in it, you know, it's going to become brittle at a lower ECR value, and a little later, the 1,000 degrees C. would come in. So this is just supposed to help you get a feeling for, again, this solubility limit is your boundary condition for diffusion, and the higher you go in hydrogen content, the higher that boundary condition, the faster the diffusion rate early on.

And it's your long time solution of what you would flatten out to do this.

Okay. Break-away oxidation is kind of fun. It's tedious, and it's easy to see. Again, this is darker than what I am looking at on my screen, but if you take a rough surface, HBR type cladding at 1,000 degrees C. oxidation and steam, if you go for 3,600 seconds you essentially pick up about 60 ppm of hydrogen. You see a black tectragonal oxide layer, which is protective. This sample is not in break-away. If I take it another 30 minutes to 5,400 seconds, the sample is completely gray. I've picked up 2,100 weight parts per million hydrogen, and you're way past the break-away oxidation time.

It took us a surprisingly long time, and I'll show you the results, to find out at 3,800 second was the break-away time. We started here. We started backing up. Then we started going forward, but anyway, we were fortunate to catch this sample just prior to break-away.

And what it looks like, what I wanted to show you is just prior to break-away the break-away time is 3,800 seconds. Is it my eyes or is it this? Oh, well, I will proceed. It looks much better on the computer screen than it does up here.

But 3,600 seconds, I did want to show you the wavy boundary between this is the alpha layer. This is the oxide layer, which is too dark to see, unfortunately, and it's this kind of waviness that's a precursor to break-away, and so 200 seconds later, this oxide layer out here actually did break away.

DR. POWERS: That was one of the really intriguing things in your data report, because you have excellent photographs in there --

DR. BILLONE: Right.

DR. POWERS: -- of the waviness.

DR. BILLONE: Much better quality than was showing on the screen.

DR. POWERS: And so you're naturally provoked to say, gee, when you get a waviness it's an instability of some sort. What's driving the instability?

DR. BILLONE: Well, I think I can explain it after the waviness occurs, which I'm calling the precursor. If you look at the peaks and valleys of that waviness, again, you need compressive stress and hypostoichiometry to stabilize the tetragonal phase at this temperature. And I forgot how it goes, but in part of the curve you have compressive stress. In the other part -- I'm sorry. In part of this region you have compressive stress at the metal surface and then you're developing tensile stresses, which in the other part of the surface. You have alternating tensile and compressive stresses.

The oxide is growing with a higher volume than the metal, and it tends to cause a compressive stress at that boundary if it's a flat boundary. If it's a wavy boundary, you're going to get alternating tensile and compressive stresses.

And eventually that waviness will turn into monoclinic oxide formation at that surface which will propagate throughout the material, but as far as explaining the precursor, that I can't do.

DR. POWERS: Well, I mean, it sounds like --

DR. BILLONE: Going from the flat surface, the flat interface or the smooth interface to the wavy one, I don't know.

DR. POWERS: Sufficient compressive stress on it, it's relieving itself by buckling.

DR. BILLONE: Yours is as good as mine. There are experts that maybe can explain it better than I can.

CHAIRMAN ARMIJO: Mike, I'm going to have to -- you've got 36 slides here and you're on Slide 6. We're supposed to take a break around 30 minutes.

DR. BILLONE: All right. I'll do it. I'm going to skip some of those slides.

Just very quickly, break-away transition from modern cladding alloys, belt polished, 15 by 15, Zry-4, 95 degrees C. is about 5,000 seconds. You can hardly see it here, but there's a gray streak in the black matrix, which is the beginning of the break-away.

And for ZIRLO, I think you can see the yellow patch that forms on the outer surface.

All right. I have a speech on weight gain, ECR, and post quench ductility. Let me hold that for questions in the interest of time.

I do want to show you one slide as to why the CP ECR, or any kind of ECR or any kind of weight gain, is really a measure of what you're throwing away. It's a measure of your embrittled oxide layer, and it's a measure of your alpha layer.

If we do a calculation of isothermal conditions, like 1,200 degrees C. or 1,100 degrees C., and we look at the function of increasing weight gain or ECR, if we look at the build-up of average oxygen content in the beta layer, this would be what you would start with as fabricated. This is your solubility limit. So this is the equilibrium solution over a long time.

And if you look at the fraction between the average oxygen minus the initial oxygen divided by that, there seems to be for Zry-4, anyway, a correlation with the ECR up until you get to high ECR values, and then you're slowly approaching saturation. So it may be not a total coincidence of blind luck that when we correlate our post quench ductility data, this parameter, for Zry-4, that in a way we are also correlating to the embrittlement mechanism, which is the oxygen build-up.

Okay. One important point is to -- if I poke you with the laser I'm sorry -- one important point as to why we use CP ECR, there are alloys, in particular the M5, which have lower weight gain, and we just got data on the ten by ten Zry-2. Zry-2 behaves the same as Zry-4, but the Zry-2, ten percent of the inner wall is a zirconium alloy. It's not Zry-2, and that oxidizes much slower than the outer wall, giving you a net decrease in weight. So the open circles are the new data we've got for Zry-2.

CHAIRMAN ARMIJO: Whoa, whoa, whoa, whoa. You're saying the liner on the inside oxidizes slower?

DR. BILLONE: Yes, and that was actually pointed out to us by EDF and Nicholas in the back room, circa 2000 or 1999. I can't remember when, yes.

CHAIRMAN ARMIJO: Pure zirconium generally doesn't.

DR. BILLONE: Well, at 1,200 degrees C. it's about the same. At 1,000 degrees C., and I don't have the details of what's in there. There's some small amount of oxygen in the zirconium liners, some iron, but it's really a different alloy, and we notice a dramatic difference.

The point about these slow -- this really means that your oxide layer is growing very slowly, and as a matter of fact, the difference in time between this point and this point where there was actually a decrease is about 600 seconds, and the reason we don't correlate to measured weight gain is that you may have almost no change in weight gain over a five, six, seven, 800 seconds, whereas you do have diffusion of oxygen into the beta layer. You have continued embrittlement.

So we correlate much better, our ductility data correlates much better to the temperature time calculated value than this measured parameter here.

All right. The trouble with ECR -- and then I'll move on to actual data. I apologize for the long introduction -- is ECR and weight gain are totally insensitive to how much hydrogen you have in the material. The red points are prehydride in materials with that level of hydrogen. Basically oxide growth doesn't care what hydrogen is in the metal, a very limited effect on the alpha layer and the beta layer thicknesses and oxygen contents.

And so we have to keep that in mind if we're going to use that as a metric for embrittlement. We have to be a little bit careful because these blue points are ductile, and these red points are highly brittle, and so we need to be a little bit clever in how we handle this because ECR does not include the effects of hydrogen.

Okay. Let's skip test methods unless you have questions and get into data.

CHAIRMAN ARMIJO: I did have a question on your test methods.

DR. BILLONE: Which is probably the best way for me to proceed is to ask.

CHAIRMAN ARMIJO: but you compress these little ring specimens --

DR. BILLONE: Right.

CHAIRMAN ARMIJO: -- after oxidation, and I know that historically that's what was done.

DR. BILLONE: Not the way we do it, but go ahead.

CHAIRMAN ARMIJO: Well, you do it better, but --

DR. BILLONE: Oh, of course we do it better.

CHAIRMAN ARMIJO: But I wanted to know is there a length effect on your samples? Do you use these eight millimeter wide rings?

DR. BILLONE: Our Russian colleagues have studied that and we've studied that. In the range of maybe seven to 20 millimeters the answer is no.

CHAIRMAN ARMIJO: Okay. So it's fairly insensitive to that.

DR. BILLONE: The maximum load that you would reach obviously --

CHAIRMAN ARMIJO: Sure.

DR. BILLONE: -- is the stiffness. It obviously is, but the ductility that seems to be independent of the length within that range. So we haven't found a length effect, but again, doing screening tests we are very careful to keep as much the same as possible, and what you're varying is oxidation level or alloy. So we fix it at the eight millimeters and try not to have that as a variable.

CHAIRMAN ARMIJO: And you cut your sample to avoid edge effects, you know, oxidation from the sides of your --

DR. BILLONE: No, we don't have the end effects in the samples.

Okay. So in the interest of time I'm going to run through these quickly, but again, you mentioned the ring. That's too dark. Well, that was easy.

CHAIRMAN ARMIJO: I didn't mean to do that.

DR. BILLONE: No, no, that's Chapter 2, and I will go back if you have any questions.

Basically the two types of mechanical tests that we run are the ring tests that we talked about just now, and then a limited number of bend tests with the balloon and burst cladding, where there's some information and data on that in Chapter 6 of the report.

That's where we got the idea that if you hold for one second at 1204 degrees C. given our LOCA temperature history that we use, it takes about I think it's 21 Newton meters to break the sample as a bending moment, and then if you hold it for an extra two minutes and oxidize more, it drops down to about 12, and if you held it longer, that bending moment keeps decreasing.

Okay. Let me try to go through some of the data. I showed it in July and I'll show the data points that we've added.Basically, our goal if we'd look at 1,000 degree C. was to oxidize two-sided up to 20 percent ECR, and what we found is that at 1,000 degrees C. as long as you haven't gotten into break-away, what I said was true, that we don't embrittle the alloys. The Zry-4 levels off at about three percent offset strain. Zry-2 we only took up to 17 percent ECR, but that was 14 percent offset strain, and again, the ZIRLO and the M5 are ductile up to those levels.

The E110 tubing due to break-away we couldn't take the break-up very high. We couldn't take it beyond seven percent ECR without hydrogen embrittlement of material.

So 1,000 degrees C. is benign. We don't have hydrogen in it. It's benign if you don't do break-away oxidation. You just can't pump enough oxygen into that beta layer to embrittle the material.

Alloys oxidized at 1,100 degrees C. are even more benign. At 1,100 degrees C. you don't experience break-away oxidation. So as long as you don't have hydrogen, this is all as fabricated alloys. Basically sine you don't have break-away you can't pump enough oxygen in to embrittle. All you can do is let it go for a long time until that beta layer almost disappears or thins, and that's way beyond 20 percent ECR.

CHAIRMAN ARMIJO: What's your strain where you could find brittle or non-brittle? Is it two percent?

DR. BILLONE: It's two percent and based on -- there's two kinds of measurements we do. One is a -- and sorry about the scale. This is load and this is displacement. If you take as fabricated material and you just compress the ring to get plastic displacement, and in these kinds of tests, this is the initial loading curve or thickness of the ring, and usually you don't have the luxury of stopping a test because you run it to failure and you get a big load drop.

But if we stop with about two percent plastic strain -- maybe it's 20 percent plastic strain. Sorry -- if we stop at this point and actually program the machine to unload, you unload at a less stiff or more compliant rate, lower rate, than we would if we just took this green line and moved it over here.

So what is the error? What is the error involved in using this initial loading curve, which is all we're going to know for the tests that we're going to run from now on.

We took every cladding material we had, and we compressed them all two millimeters, and we found that the difference between actually measuring the change in diameter and determining it from this type of approach is .2 millimeters or less. So .2 millimeters divided by about ten millimeters outer diameter gives you your two percent uncertainty.

So below two percent offset strain we're not saying the material is necessarily brittle. We're saying we're in the region which we can't tell, and most of our tests we stop after the first significant load drop, get through-wall crack, and then measure what we call the permanent strain, which is literally the change in diameter of the material. So you don't go through this uncertainty.

So we do both approaches. So for permanent strains, our criterion is one percent. Below one percent material is brittle. Let me say it another way. One percent or above, the material is ductile for direct measurement, and if we're going to use the indirect approach of the load displacement curve, we say above two percent, greater or equal to two percent strain is ductile. Below two percent we're not sure.

Okay. I did want to emphasize this again about measured versus calculated ECR since we went through this before, but this is our offset strain data determined from the load displacement curve. This is measured ECR. Everything is fine with Zry-4 when the measured and calculated weight gains are about the same and, therefore, the ECR is about the same.

The oxide layer on M5 at 1,000 degrees C. grows very slowly and almost stops at 1,000 degrees C., and so the weight gain, even though I'm increasing my time, these tests are run at the same. These data points correspond to roughly the same time. This point here and that point there at the same time, but the weight gains are very different.

So between these two data points you have about six to 700 seconds with almost no change in weight gain. So if I look at the ductility, it looks like, oh, my God, I hit a wall, and it suddenly becomes instantly brittle. The fact is there's a lot of test time at 1,000 degrees C. between this point, this point, this point and that point.

And so what I say correlates better, although Harold has criticized me. He says that correlates. It is what it is. What correlates better is to ignore the physical weight gain and just use the Cathcart-Pawel predicted ECR, and when you plot Zry-4 and M5 on the same scale, then they come down to about the same high temperature strain.

And that precipitous drop is deceiving that you see on the previous page. It's just that the physical weight gain there doesn't really reflect for

M5 the oxygen diffusion into the metal. It's literally just the oxide layer slows down in growing.

Okay. Eleven hundred degrees C. when I said it was more benign, the Zry-4 is the black curve. The ZIRLO is the green curve, and it does what I said it should do. When you get saturation of oxygen in that beta layer, then as you increase time, the beta layer just starts to thin, but it's still thick enough, and there's almost no change in ductility of the material, and the ZIRLO behaves comparable to that.

M5, we had one point which when you get to a point like this you want to double check. It looked like it dropped to a brittle level at about 18.8 percent ECR. We ran a test for a higher temperature time, a higher time. We tested it at room temperature, and we also stopped the test right after the crack, tested at elevated temperature, and the material is brittle.

So this data point here is a lot higher quality than that data there. So M5 does get out to 20 percent ECR at 1,100 degrees C.

I have some D110 results. We got some pretty good results when we polished machine and polished E110, and we got results that are almost comparable to the allied results that you had at 1,100 and 1,200.

We couldn't save the alloy at 1,000 degrees C. It was too vulnerable to break away oxidation, but in the report there are some E110 data.

All right. If we go to 1,200 degrees C., again, we are doing all of our tests and most people in the world do their test at room temperature. It's easy, but the way we set the program up is if we got low ductility values of like nine percent to 11 percent at room temperature we would retest at 135 degrees C.

One of the alloys we tested at room temperature, 100 and 135, but in general for 1,200 degrees C. you very quickly generate your first data set and say, well, forget this. We're going to go to the more prototypic temperature, which the criteria are based, and it doesn't take that much more time.

So if I look at our database and I take the minimum ductility points, not the average, and then that's where whoever receives the data are free to come up with their own numbers. The curves I'll show you flatten out, but basically for the materials we studied, the ECR for modern materials, the ECR ranges from 17 percent to 20 percent, and the new data would be the Zry-2 data, ten by ten. Zry-2 is not in the report yet or maybe it is. Maybe it is on the Web version, but you're in the range of 17 to 20 percent ECR, with the exception of this cladding of the 1970s, '80s, and '90s, which doesn't behave as well.

We took E110 up to 13 percent ECR, and it was quite ductile, and that's machined and polished E110.

Okay. This is just the enhancement with temperature. This is the room temperature data for offset strain versus the 135 degree C. data, and you can see there is a significant enhancement that's worth testing at that temperature.

Just to show you that we also have permanent strain, which is the direct measurement of the change in diameter of the material before the test and after the test, and that's really what we use to determine the transition ECR.

So if I go to 17 percent I've got essentially data points right at about one percent strain, which is our limit, and if I go to a higher ECR I've got one above and one below, and actually this has got two data points above and one below. So about 17 percent is what we picked, but really more based on permanent strain even though we show a lot of offset strain data.

Okay. Zry-4 is Zry-4 is Zry-4. Well, not true, not in terms of what we're measuring. So if we compare modern 17 by 17, belt polished, smooth surface, modern 15 by 15, these are different vendors, materials, and then the older, rough surface, and the rough surface may have nothing to do with why it behaves poorly. You could see that the older material embrittles much more, a lower ECR, about 14 percent. This is the 17 by 17 Zry-4 data, and actually that comes down pretty close to the 15 by 15 Zry-4, but at lower ECR, this material has more ductility, and Zry-2 that we have just tested falls in between these two curves and come s out to about 19 percent.

Okay. That is again this is ZIRLO room temperature versus the elevated temperature, 135 degrees C., and permanent strain. We have already hammered that one.

Okay. The idea was to compare the modern alloys to Zry-4 in our study. So always we'll have a plot of the Zry-4 data and, for example, ZIRLO, and you can see basically that the ZIRLO has higher ductility than the Zry-4 and comes down kind of close, but it's the difference between 17 percent and about 18 or 19 percent in terms of transition ECR, and this is our two percent criterion.

M5, we already saw this. That's the temperature relation. What I want to look at is this one. This is M5 compared to Zry-4, and M5 at 20 percent has exactly one percent permanent strain, and a little higher than two percent offset strain. So M5 is ductile, right at the limit of 20 percent ECR.

Okay. From a metallurgical point of view, all of these samples are quenched at 800 degrees C. That was our test protocol. There really should be no difference in as fabricated alloy behavior whether you quench at 800 or you cool to room temperature if oxygen is basically immobile, below 800. The fusion is so slow there's nothing really that's going to happen. Your phase changes have already happened. Oxygen doesn't move around. So basically we did a limited number of tests that we varied our procedure and instead of quenching at 800, we slow cooled.

The results are kind of interesting in that at 17 percent ECR, this is one data point; this is another data point. Slow cooled are the open circles, and there's another one you can't see in here.

Within the scatter of the data, there was actually no effect of slow cooling versus quench.

There is a possible enhancement at 13 percent ECR, but this curve is changing so rapidly that if you put enough data points on it, this may have no effect whatsoever.

So possible enhancement at lower ECR values, Zry-4 by sow cooling. The ZIRLO data points were slow cooled. We don't understand why, but they fell below the trend curve. They still were ductile, but when you got up to 17 percent ECR, the slow cooled and the quenched samples were all in the same range.

And for M5 we saw absolutely no effect of slow cooling versus quench. It's hard to distinguish the points. This would be slow cooling, and these two would be quench.

So the issue is cool rate and quench temperature really comes up for the hydrided materials and for as fabricated materials, it doesn't seem to matter. Our CEA colleagues actually quench from the oxidation temperature very rapidly. We cool at a certain rate to 800 and then quench, or we cool and then continue to cool at room temperature. It doesn't seem to be a significant difference in databases.

CHAIRMAN ARMIJO: In doing this work did you look at varied strain rates to see the sensitivity to strain rates?

DR. BILLONE: We only did one test where we varied the strain rate for one material. We increased the strain rate by a factor of ten and we saw a decrease in ductility with a factor of ten increase in strain rate, and at the time when we presented this the sponsors and the partners were more interested in, given the size of the test matrix, fixing the strain rate, fixing the temperature at the 135 degrees C. and then getting into studying the effects of hydrogen as soon as possible.

CHAIRMAN ARMIJO: Was there a basis for picking the strain rate that says, "Hey, this is closer to the kind of strain rates or loadings we might see in" --

DR. BILLONE: No, we're not trying to simulate any kind of loadings. This is closer to Hobson's strain rate back in 1973.

CHAIRMAN ARMIJO: So just sticking with that.

DR. BILLONE: He did slow strain rate and very fast strain rate tests, and the Commission only used the slow strain rate data. They do not use the high strain rate data. So it's partly tradition.

DR. ABDEL-KHALIK: Let me just try to understand one thing here. You have a lot of data on measured ECR, and yet you elected to set all of that aside in favor of using this empirically predicted value.

DR. BILLONE: Right.

DR. ABDEL-KHALIK: And that's presumably biased by what? By the need to retain the 17 percent rate?

DR. BILLONE: Oh, no, no, no.

DR. ABDEL-KHALIK: Or what?

DR. BILLONE: First of all, for Zry-4 there's hardly any difference between the measured and the predicted.

DR. ABDEL-KHALIK: I understand.

DR. BILLONE: In the back of our minds we know that vendors who are doing these calculations don't have the luxury of measuring weight gain. I mean, basically they calculate it, and ultimately for a hypothetical LOCA you need to come up with a scheme in terms of something you can calculate.

DR. ABDEL-KHALIK: Well, why not correlate your own data based on the actual measurements?

DR. BILLONE: We did. We got severely criticized the last time we came here in July 2005 because we preferred weight gain at that time for a particular alloy like Zry-4 because if there's variations from test to test and the test strain, it works a little bit. The temperature changes a little bit. That would be captured by the measured ECR, which would reflect the temperatures.

But the message was that ultimately have this material be usable by vendors and by NRC. You needed really to present it in terms of something you can calculate.

DR. ABDEL-KHALIK: You have sort of really valuable information.

DR. BILLONE: It is, but then when we ran into the M5 example where knowing that weight gain was really a measure of what you're kind of throwing away as brittle, it's a measure of the oxide layer thickness and to some extent the brittle alpha layer thickness, and when we saw the dramatic difference with M5 in the fact that we extend the test 700 seconds at 1,000 degrees C., yet no change in measured weight gain and get a significant decrease in ductility. It no longer made sense to continue to use measured weight gain under all conditions.

So the idea of switching to this is diffusion processes like oxygen diffusion into the beta layer and through the beta layer. It has got an exponential of minus Q over RT, and it goes to the square root of time, and this parameter on the bottom also has a square root of time and an exponential --

DR. ABDEL-KHALIK: But clearly, there must be something else going on whereby your measured ECR for M5 is significantly different than what this empirically predicted value is.

DR. BILLONE: Yeah. What's going on seems to have nothing to do with the mechanism of embrittlement, which as long as you can keep the oxygen concentration a the edge of the beta layer up around .57, .6, you will drive that much oxygen into the material over time. As time increases, you can drive more oxygen. It doesn't matter whether the oxide layer is 30 microns or 40 microns. It doesn't matter that it has stopped growing and it may be 30 microns.

So there's a disconnect. For Zry-4 it's all in harmony, but for other alloys, even for Zry-2 with the zirc liner it doesn't work as well to correlate to measure ECR. It's all in the report, and the point is you're all free to assess the quality of the data using that because within an alloy class like within one lot of Zry-4, there should be a correlation between the predicted and the measured.

As a matter of fact, when that gets more than ten percent different, we build a new test train. We use that as a metric for the quality of our test train and our benchmarking.

DR. ABDEL-KHALIK: Thank you.

DR. BILLONE: Okay. I'm not moving fast enough, but let's see where we are.m

CHAIRMAN ARMIJO: Yes, we're way behind schedule, but we're going to eat into the break a little bit.

DR. BILLONE: We don't want me to be responsible for that.

CHAIRMAN ARMIJO: Yeah, I'd like you to move along.

DR. BILLONE: I think this is important. So I do want to pause a little bit. Ralph, gave you some background data from Leistikov and Schanz, and what I think was 1970 standard means rough surface, may have had etching involved in processing the material. A very extensive study, and they found minimum break-away oxidation time at 1,000 degrees C.

Before we started our program we did the literature search, and that break-away was about 1,600 seconds based on weight gain and about 1,800 seconds, which are much lower numbers than I am going to be showing you for more modern alloys, but based on picking up 200 ppm of hydrogen, it was a little bit later.

There is published data by AREVA, CEA, EDF, Mardon, et al., on modern low tin for 17 by 17. That's belt polished, and they got very high break-away times at 1,000 degrees C. It showed it in terms of hydrogen pickup that it was about 5,400 seconds.

So you're talking 1,800 seconds for '70s cladding in this particular test and significantly higher for the more modern cladding.

There was also results presented for 17 M5, which showed break-away times very large relative to what we think of as LOCA relevant times, and this was significant to us because we put M5 last on our list of priorities because there was always some data that showed extremely high break-away times.

we had our own results for E110 tubing and cladding and the ANL modified E110 as background, and based on the results I already showed you at 1,000 degrees C., although all the samples were ductile at 20 percent ECR and 1,000 degrees C., we did notice that the ZIRLO picked up 100 weight parts per million of hydrogen. It turns out the inner surface was just into break away oxidation. We got some hydrogen pickup, but 100 ppm did not affect the ductility at all.

So that's the background going into our study. Again, Ralph showed you this curve meaning e would have to search a little bit. This is a curve for Zry-4 based on their data. If we wanted the minimum oxidation time, we'd have to be kind of clever about it in searching, and the cleverness is basically you start at 1,000. You find out what your minimum time is, drop your temperature ten or 15 degrees C. If the sample is black and there's no hydrogen pickup at that test time then you're okay and drop it a little further.

And as you keep getting lower minimums, you keep using that as your metric for the next test. So at best we explored between about 1,025 and 950 and also ran the test at 800.

Okay. This is the sample that we had tested for about 3,400 seconds. It's not part of our break-away oxidation test, but this is our ZIRLO sample. You can see the outer surface has got the precursor to break away as most alloys do. It has got the weighted boundary.

The inner surface, if you've got a better view of it. You can actually see gray on the surface of the inner surface and some cracked oxide. You don't pick up much hydrogen, but you pick up about 100 ppm, and again, 100 ppm at room temperature was not enough to affect the ductility.

So anyway, a couple of things about our reported numbers. We're reporting times from the beginning of the temperature ramp starting at 300 degrees C. at the end of the hold time. The ramp time is about 75 seconds, which is trivial to numbers like 4,000 and 5,000 seconds.

The cooling time is only about 20 seconds from 1,000 degrees to 800 degrees C. We're not including that.

As I say, our approach was to try to find the minimum break-away time at 1,000 degrees C., move up a little and then down a little to see whether the alloy was okay at those temperatures at that particular time.

Now, our criterion for break-away oxidation you can argue with, but I want to point out a couple of things. We chose 200 weight parts per million hydrogen pickup. It's very hard to exactly hit 200 because when break-away starts, it's an instability phenomenon and your hydrogen takes off on you.

So break-away is an instability phenomenon with rapid hydrogen pickup following break-away. Generally in life you learn to stay away from instability phenomena, and the embrittlement is definitely due to the hydrogen pickup. You don't pick up enough oxygen to embrittle that material all by itself.

And we also confirmed that the ductility is retained for these alloys when you test it at 135 degrees C. when you do have 200 ppm.

Now, for every test that we ran, we had rings cut to run ring compression. So samples that pick up 500 ppm hydrogen, 1,000 ppm hydrogen, we didn't test those, but if someone would like to contest the 200 ppm criterion and say, well, gee, I think it's brittle at 300, let us know and we could just squeeze those rings so that it would be very trivial.

So the testing sequence for us was to start with the Zry-4 because we had the baseline data for that. We had no data for ZIRLO published in the literature of break-away oxidation, and then we moved to the ten by ten Zry-2. That work has been essentially completed, and then five would be last on our list just based on the published data being so high for break-away.

Okay. Just quickly time. We call this rough surface material. We don't know the surface chemistry of this material. It's our baseline material for a high burn-up Zry-4. We don't know why it breaks away earlier. We do know it has a much rougher surface. The surface chemistry might be different, but at about 985 degrees C. long time temperature it breaks up at about 3,800 seconds, which is better than Leistikov's material but low compared to the values I'll show you.

We wanted to compare a modern 15 by 15 Zry-4. We also ran out of 17 by 17 Zry-4. So we had no choice. So we used the 15 by 15 Zry-4, which is belt polished, and again, at the same temperature we get a break-away time of 5,000 seconds. That's comparable to the Mardon results at 5,400 seconds.

So the belt polishing seems to have an effect, and whatever subtle changes in chemistry that may have been made from 1970s to the modern day seem to help on this.

We also explored temperatures in this range with a second test train.

So that's our Zry-4 results, and if I want to plot it, you can see for the rough surface cladding at 3,600 seconds we got almost nothing. At 3,800 seconds we got nothing for one sample and got high hydrogen content for another, and then, of course, if you go beyond that, you get extremely high hydrogen pickup.

The point about break-away oxidation, and I think it's a no brainer, you kind of want to stay away from this, and what's good enough if you're below the 200 ppm hydrogen pickup, you at least know you have ductility. You may have ductility at 250, but who's talking? Who wants to quibble about 100 seconds out of 5,000 seconds or 4,000 seconds?

The belt polish material did pick up hydrogen more slowly, and this is about the 5,000 second point.

Okay. Quickly, we're fortunate enough our furnace has a window. So instead of just arbitrarily running test times, you can look for patches of gray that may form, which means the transition has already started, and for this particular belt polished material at break-away, 5,000 seconds, we cut a two millimeter ring for hydrogen analysis in the center or wherever this gray region is, and we cut eight millimeter rings on either side for ring compression.

But the outer surface is -- I can't see it. I don't know what you can see -- is cracked. The inner surface is a precursor to break-away, but it's not in break-away. So those are the kinds of results that we're looking for.

Okay. So let's summarize. All right. For ZIRLO, we had a test matrix where we started at 1,000. We went up to 1,015 and then we went down. What we found was that 970 degrees C. plus or minus five -- that's the variation circumferentially around our sample -- we got what we think is the minimum break-away time from about 3,000 seconds. So it actually breaks away earlier at this temperature than it does at 1,000 degrees C.

What was encouraging though is we machined a 20 micron deep scratch in the material, and that scratch did cause local break-away oxidation to occur in the region of the scratch, but the hydrogen pick-up was negligible.

So at 2,600 seconds with a 20 micron scratch the material was good and without the scratch at 3,000 seconds it had already picked up exactly 200 ppm of hydrogen.

And the sample was ductile, but again, we're stopping at that 200 ppm point. It did better at 950 and it did better at 800 degrees C., but all we did was basically one test at each of these to make sure that we didn't have a lower minimum time.

Okay. Metallography confirmed that we had outer surface break-away oxidation, and one of the ricks that picked up about 175 ppm of hydrogen with a lot of variation of hydrogen, as much as 440 ppm locally, that had five percent ductility at this test temperature.

In all, we conducted 17 tests, and so rings are available with this range of hydrogen content for ring compression ductility tests if someone is interested.

Again, too dark. I'm not going to show metallography today.

Okay. We just finished our testing at 1,000 degrees C. and we're up to 5,000 seconds for Zry-2 with the zirconium inner liner, and we don't have any break-away that we've observed, no hydrogen pick-up for the Zry-2. We've got other tests planned to investigate it, but we really don't expect any surprises with this material.

CHAIRMAN ARMIJO: Do you have any estimate of what the break-away would be if it was just Zry-2 with no liner?

DR. BILLONE: No, we don't have any such -- well, we could do that. We could weld the material.

CHAIRMAN ARMIJO: I'm just saying that, you know --

DR. BILLONE: We do.

CHAIRMAN ARMIJO: -- there's some older material around.

DR. BILLONE: No, no. I'm sorry. We do. In 2001 we did a whole series of one-sided oxidation tests of Zircaloy-2, high burn-up and as fabricated. So the liner wasn't involved. It was just outer surface, and we didn't know we were doing break-away oxidation tests at the time, but basically I can tell you the outer surface based on that data is certainly greater than 3,000 seconds and less than 6,000 seconds.

CHAIRMAN ARMIJO: They're similar.

DR. BILLONE: Similar. Okay. All right. I don't know if you want to take a break right now. I want to switch to prehydrided material. A coffee break?

CHAIRMAN ARMIJO: Probably we should do that. Let's take a --

DR. BILLONE: And then I'll figure out how to.

CHAIRMAN ARMIJO: Yeah, because you've got a lot of material in the next session. We're going to try and wrap up the test procedures. Let's get back here at 11:15.

DR. BILLONE: Okay.

CHAIRMAN ARMIJO: Okay.

(Whereupon, the foregoing matter went off the record at 11:05 a.m. and went back on the record at 11:21 a.m.)

CHAIRMAN ARMIJO: Okay. Let's come to order.

DR. BILLONE: Okay. I think I can go faster with my jacket off.

CHAIRMAN ARMIJO: Okay. Roll up your sleeves, Mike, and let's go.

DR. BILLONE: We're into Chapter 4 of the NUREG report, which is prehydrided materials, and again, everything I have presented to you so far is directly applicable to fresh cladding put in the reactor. It also serves as a baseline for everything else I'm going to show you in terms of baseline data.

So now let's look at prehydrided Zry-4 where before we oxidize the samples we're going to put hydrogen in them and then we're going to do post quench ductility tests at 135 degrees C. Our target temperature is 1,200 degrees C.

Again, Dana mentioned this point. With prehydriding we had an initial campaign. God, I think it was like June 2004, in which the samples had circumferential and axial gradients in hydrogen. The furnace had small gradients, and we took those samples and decided to fix the oxidation level and cut samples with different hydrogen contents. So we did something that no one else has done. Instead of having every sample at 600 ppm, we had a variety of hydrogen levels, and we just fixed the oxidation level so we can map out the embrittling effects of hydrogen at a fixed oxidation level.

But, again, during the short time tests of 100 to 150 seconds, the gradients did remain in those directions, not through the wall of the cladding, which we couldn't measure, but in these larger, longer directions circumferential and axial.

In the second campaign, which was more recent, we reduced the gradient significantly, but we did generate a hydride rim where we went above about 300 ppm of hydrogen.

So we haven't mastered this art, but we take what we can get out of each sample.

Let me summarize the results for Zry-4, and again, this is going to be data that we're going to use to plan the in-cell test and also to help interpret the in-cell test.

The data I'll first give you is all samples are standardly quenched to 800 degrees C. At five percent fixed ECR for the H.B. Robinson type Zry-4 you don't quite get to 1,200 degrees C. and I'll show you in my next plot. You get to in one case 1,180 and 1,190. It means you're picking up the five percent in the ramp, and most of your oxidation is occurring at less than 1,200 degrees C.

However, from 600 ppm of hydrogen and five percent ECR with this peak temperature, you embrittle at that level of hydrogen. So lower hydrogen levels, you're ductile; higher hydrogen levels, you're brittle.

All right. If you increase the six percent ECR, we only had one set of samples at 450 weight parts per million. The sample was brittle at 450 weight parts per million and six percent ECR. So we just increased the oxidation level by one percent, and the hydrogen level was decreased by 150 ppm and the sample was still brittle with quench.

When you go up to 7.5 percent, we had data sets for modern cladding and the older cladding, and both seem to embrittle at about 375 plus or minus 25 weight parts per million hydrogen at seven at a half percent CP ECR. When I show you the data, maybe the 17 by 17 is more like 400 and maybe the 15 by 15 is more like 350, but within the scatter we can't really tell.

And then this cladding which is a better cladding in the as fabricated condition we took up to ten percent ECR, and we get embrittlement at about 300 ppm of hydrogen, and that's all with quench. I'll just show you the results graphically. Stop me if something is not clear.

But this, as I say, is an unusual study. This is the 15 by 15 at cladding at five percent ECR where you're less than or equal to 1,190 seconds throughout the transient, and the effects of hydrogen are essentially nothing as fabricated, nothing about 250 ppm, and then when you get above 300 ppm, you start to see the effects of the hydrogen.

So interpolating these two values, brittle, ductile, you come up with about 600 ppm as the transition.

DR. POWERS: You show curves on all of your data plots.

DR. BILLONE: Trend curves. They're not calculated by some program.

DR. POWERS: They're just French curve --

DR. BILLONE: Artistic.

DR. POWERS: Artistic.

DR. BILLONE: Renditions. We're trying to get the trend which we think is kind of a reverse S shape where oxygen has a little effect and then a more dramatic effect and then levels off. They're not best estimate and they're trend curves we've put on there because we want to add data to these curves for slow cooling and quench and other temperatures to get comparison.

Okay. I hope the blue shows up. The open blue circles are the 17 by 17 Zry-4 which based on as fabricated data should be a lot better than the 15 by 156, and of course, it is at the intermediate hydrogen contents. We don't have enough data, but by about 420 ppm of hydrogen that Zry-4 is brittle. This is a big extrapolation between this point and this point, but you come up by about 375.

So is it better? I don't know. We would need more data. We're out of material. We can't answer that question, but certainly increasing the oxidation level decreases the hydrogen at which embrittlement will occur.

So you increase the oxidation level.

Yeah, I know how to say that, but I'm not going to say "right."

And then we did ten percent, which is this curve here. I've also plotted the seven and a half percent for the same cladding, which is 17 by 17 Zry-4, and if you follow the trend of this data, this is brittle. This is ductile. So about 300 ppm is the crossover.

This is kind of useful if you're thinking of the effects of hydrogen as a function of burn-up because I show you fresh cladding results. If I just show you high burn-up results, 600 weight parts per million hydrogen, that's sort of the end point, and you learn nothing about the transition. So this kind of helps give you a clue.

If you map the corrosion layer growth as a function of burn-up and the associated hydrogen as a function of burn-up, you have some idea of what the effects of that hydrogen is from the beginning of operation to end of operation.

All right. At the last ACRS subcommittee meeting, it was mentioned the effects of quench temperature or quench temperature may have a significant effect on ductility following quench. So we ran two types of tests. We did a lot of tests where we cooled to room temperature without quench for samples oxidized in this range, five to seven and a half percent.

I'll show you in the graphs. There was a small but very significant enhancement in ductility. A couple of percent is enough to kick you from brittle to ductile, and the results were very, very flat in the sense that they didn't -- they nosedived, but they nosedive and level off at a ductile level, and basically there was an increase or a fixed ECR. The increase in hydrogen to cause embrittlement was about 180 weight parts per million. That's a lot.

So there's something that's happening during slow cooling where the extra hydrogen doesn't seem to contribute to the embrittlement.

CHAIRMAN ARMIJO: Now, what is the reason why someone would say, well, you ought to slow cool versus quench? Is there a system issue?

DR. BILLONE: Oh, no, it's not that. It's a practical issue in that our in-cell tests, we have to run several slow cooled tests to map out the ductility because we're not guaranteed more than one quench before the thermocouples pop off.

So the motivation for running the slow cooled test is to help us interpret the in-cell test with high burn-up cladding.

CHAIRMAN ARMIJO: But I read in one of the handouts or some of the earlier material though that the CEA had an issue. They tested --

DR. BILLONE: I've got their graph.

CHAIRMAN ARMIJO: Right, and they seem to prefer a slow cooling compared to a quench. That's what I got.

DR. BILLONE: When I get to their graph, why don't I explain that?

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: And this is an experimental issue, but it also relates to a LOCA issue in the sense that calculated wetting temperatures -- I'm using "quench" synonymous with when the water actually wets the cladding surface and you get rapid cooling, and I'm just talking about at what temperature does rapid cooling occur, and does it matter in terms of ductility?

It shouldn't matter in terms of oxygen moving around, but it turns out it has a huge impact on what hydrogen does, and so you're bounding. You've got two cases. You've got a CEA case where they initially quench from like 1,200 degrees C. rapidly, argon quench from 800 degrees C., and then the other extreme is just don't quench at all.

And since that time we've been looking at intermediate quench temperatures because usually, I mean, calculated ones I don't have -- I'm not an expert in thermal hydraulics, but you see cases shown with 400 degrees C. wetting temperature, 500 degrees C., and so where the 800 came from, it looks like a reasonable upper bound of LOCA quench temperatures, but it may not -- doing all of the tests with 800 degrees C. quench may not allow you to take full benefit of some of the not annealing, but some of the good effects that could happen during cooling.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: So we're trying to quantify this. Okay. The six percent sample that was brittle with 800 degrees C. quench was ductile. We got a little more hydrogen with slow cooling, and the seven and a half samples, the embrittlement hydrogen content increased from about 375 to 550 weight parts per million. So that's quite a significant swing in how much hydrogen it takes to embrittle a sample when you slow cool it.

We also looked at 600, 700 and 800 because the CEA results which I'll show you showed a very significant enhancement in ductility if you quench at

600 or 700. However, those results I'll show you are from extremely slow cooling rates from 800 to 600 to 700. So when I end up showing you the argon cooling rate and the CEA cooling rate, basically what you have are two bounds. Ours is faster than a LOCA would cool. The results are very much slower.

So for testing these three temperatures, we did seven and a half percent ECR, but we used a very aggressive heating rate, a very fast heating rate. We allowed it to go to 1,216 degrees C. for a few seconds, and that turned out to have a very embrittling effect on the material.

So 300 weight parts per million hydrogen we expected to be ductile at all of these quench temperatures based on previous stuff, but just because of the aggressive heating rate and the higher temperatures, it was brittle for the two quench samples, 700 and 800, and ductile for no quench or slow cooling.

Six percent we talked about, and we already know it was brittle for 800 degrees C. quench. Seven hundred degrees C. quench, new data. Six hundred degrees C. quench, all brittle. Something is happening between 600 degrees C. and maybe 200 degrees C. in terms of hydrogen coming out of the solution such that the slow cooled sample was ductile, but the quench at these temperatures were brittle.

So in our work with our cooling rates, which are relatively fast, we have seen no enhancement of ductility for quench temperatures greater than or equal to 600 degrees C. I expect that the difference between these tests and the slow cooling with no quench means that somewhere below 600 degrees C. there should be an enhancement in ductility. We haven't mapped that out.

Let me show you the CEA results, and let me remind you -- well, let me show you some results and get off the words. First of all, in terms of temperature, this is our typical temperature history of heating very, very fast to about 1,100 degrees C., and then to avoid overshoot, we slow it down to a couple of degrees C. per second on the average for the last 50 degrees C. to get to 1,200.

We cool faster than a LOCA. If you take the temperature here and the temperature there and the time, there's two things that are different. A LOCA cooling would have a different curvature, but on the average, if you're interested in how much time you spend, and this is an average of 13 degrees C. per second cooling rate. The CEA cooling rate is less than a tenth of a degree C. per second.

So the amount of time that the CEA samples spend at these temperatures is much, much greater than the amount of time that our samples are, and it seems to matter most in going from 800 to 700 to 600.

Okay. Quickly, that's the effects of slow cooling, and it's better shown on permanent strain, which is what we use, but I'll show it on offset strain.

We had some high, in the 700s, 720 to 780, weight part per million hydrogen samples, which I insisted were going to be brittle. They had to be. It's too much hydrogen. But when you cool them without quench, they have this. These are all ductile, and with your eye back there, you say, "Well, gee, there's not much difference between these two."

But it's the difference between dropping well below our criterion and being a couple of percent above.

Okay, and this is the higher ECR in which we had no noticeable difference for triangles versus the circles, meaning quench at 700 to 800, but again, you see the same trend. The slow cooled samples are all essentially ductile when you give hydrogen time to come out of solution.

CHAIRMAN ARMIJO: Is there pretty good agreement on the mechanism that's going on?

DR. BILLONE: Yeah, it's just I don't feel free to show CEA results. We had a June meeting -- yes, yes. I'm sorry. The simple answer is yes, but it's not Argonne data. It's CEA data.

CHAIRMAN ARMIJO: What is the mechanism?

DR. BILLONE: Well, based on observation, hydrogen does two things when it's frozen into this beta layer. First of all, at temperature it allows more oxygen to get in and causes more hardening. There seems to be a hardening above and beyond that based on hydrogen in solution in the beta phase. So if you cool rapidly and freeze that hydrogen in solution, the observation is your ductility drops off very rapidly with increase in hydrogen content.

If you cool without quench and you keep slowing down the lower the temperature and you get enough time, some of that hydrogen certainly can diffuse short distances and leave regions of ductile material and then they form on the edges between the brittle and the ductile material. It doesn't matter.

They're not big hydrides. I mean, they're very, very, very, very small, and it's just simply my impression is if they come out of solution --

CHAIRMAN ARMIJO: Like a precipitation hardening phenomenon?

DR. BILLONE: Let's call it that.

CHAIRMAN ARMIJO: And then coarsening and getting less brittle.

DR. BILLONE: That would be the best way of doing it. I'm just basing this on the excellent imaging that CEA has done to --

CHAIRMAN ARMIJO: Which you'd see by transmission microscopy. You'd look for these things and the function of quench rate and stuff like that.

DR. BILLONE: They've used it all.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: Basically.

CHAIRMAN ARMIJO: Okay. As long as --

DR. BILLONE: But it's their data. It's not Argonne data. So I'm not going to show it, but the mechanistic understanding comes from the work done in France.

Okay. So I don't know if you could see it, but this is their data. Again, they go up much faster than we do in temperature between 1,100 and 1,200 degrees C. They're going up at ten degrees C. per second, and so they're essentially isothermal in terms of very fast ramp, and they're one-sided oxidation tests, different cladding, thinner wall, 17 by 17, but essentially they put 600 ppm of hydrogen in it, which is their standard. Usually it means 550 to 650, but you always get the nominal value reported.

And they held it for 50 seconds at 1,200 degrees C. Their standard procedure is after holding it at 1,200 degrees C., they rapidly cool, and when they do that, they get direct quench. They get extremely brittle. This is offset strain versus weight gain, but the weight gain is close to the ECR. This tells you more.

The important thing is when they rapidly cool from 1,200 degrees C. It's brittle. When they rapidly cool from 800 degrees C., even though this time is very long and their big furnace has 1,200 to 800 samples are still brittle. So this is the argon quench temperature, but at a much longer time and it seemed to have no effect at this level of hydrogen.

Where they noticed the biggest effect, and again, the cooling rate is slowing down even more, is C and D are where they quenched, and again, you're not picking up anymore oxygen at this point. You're too low in temperature. So the only thing we can think of is hydrogen and/or something else is moving around, but they got significant enhancement in ductility with very, very slow cooling and intermediate quench temperatures of 600 and 700.

So, in summary, they didn't see any difference whether they did it their way, which is to quench from 1,200 rapid cool or our way, quench from 800. They saw a huge difference when they allowed it to sit between 800 and 700 for a long period of time and then quench, and then an even longer period of time between here and here.

So those would be your data extremes. The Argonne data would tend to be conservative because of the cooling rate. This would tend to be optimistic. The real answer is going to lie in between, but in terms of us trying to bound data, I think we've got a reasonable bound of data if your quench temperature is 600 degrees C. or above.

There remains that question if the quench temperature is 400 or 300 do you get a little extra margin to work with.

Well, I must have had something profound to say. That ends the section on the prehydrided cladding, and we're going to go into the high burn-up cladding.

I have some very good metallography in this section, but it's not showing up very well on the screen. So you may only see the SEM results.

All right. The second part is the work we did with high burn-up Zry-4, which is oxidation, cooling or cooling with quench, and ring compression, and then the LOCA interval tests we did with Zry-2, high burn-up Zry-2.

Okay. Let me remind you of something. The pretest data we had available for planning these tests, which were conducted in January 2005, of course we had the temperature history for as fabricated and prehydrided materials. We had the embrittlement thresholds of 600 at this ECR level and 375, and that's a quench. We had no idea of the effects of slow cooling at the time we ran these tests.

So in picking our high burn-up samples, we had a preference for testing high burn-up samples with 550 to 600 weight parts per million. We felt if we went above that level, we wouldn't have any chance of both getting the 1,200 degrees C. and preserving any ductility.

And when you run a set of tests, you'd like to get ductile to brittle transition. So we had to be very careful in planning these tests to try to make sure the combinations of hydrogen and oxidation level were such that we could bracket the transition.

You learn something if every sample is brittle, but you don't learn enough to justify the cost of running the test. So we were very careful in selecting fuel rods, and within a fuel rod, the location for sectioning samples. We had already characterized some edge right next to the corner rods, which have more water on one side than another is one way of saying it, and the fuel mid-plane looked good. It was about 70 micron corrosion layer for these rods and about the right hydrogen level.

And if we go .7 meters above the mid-plane of the fuel column, you're up to your 100 microns for these test rods and about seventy-fifty weight parts per million hydrogen, which we felt was too high. We didn't think we'd see any ductility at that level.

There were also atypical regions as you go around the circumference, which was very, very dense hydrides and very high hydrogen concentrations over part of the cross-section. Usually you don't see that.

We were worried about that because the prehydrided results, hydrogen did not homogenize. It started out highly nonuniform and would end up highly nonuniform in relatively short time tests.

So instead of edge rods, we selected

interior rods, slightly lower burn-up, but burn-up is not significant at all. These are all the same exposure time to the coolant, roughly the same hydrogen pickup in the corrosion layer. So the difference between 64 and 67 is trivial.

So we selected those, and we sampled sections from near the mid-plane, and we also wanted to do some sampling sectioning for a look at integral sample, which was about 300 millimeters long. So we cut some pieces for testing and characterization, .3 millimeters and .7 millimeters roughly above the mid-plane, but we're focused mainly on the mid-plane.

And we targeted three to ten percent CP ECR with an embrittlement expected at five to six. We also had an extremely short window to conduct the in-cell tests. Forty year old hot cell equipment breaks down. You have to pick the moment that it's working. You also have auditors coming in as we found out who can close you out at any day.

So we could not do the scientific thing where you run one test, take six months to analyze it, decide on the next test. This had to be a one-shot deal. You pick your ECR levels, boom, boom, boom. You oxidize, boom, boom, boom. You recompress, boom, boom, boom. You do your metallography.

I'm trying to explain why I didn't pick the perfect test matrix, but basically going into it, this is the kind of temperature history I've showed you. These are the kind of ECRs. The three percent is now shown, but clearly you are not going to be hitting 1,200 degrees C. at three percent and, you know, just probably make it a five.

So that's what we had for bare cladding going into these tests. We're not testing bare cladding. We're testing corroded cladding, as I'll show you in a minute, and we had these results that we've already looked at. So we don't need to look at them again.

So let me tell you what we tested. Let's look at the corrosion layer and the hydrogen content at the mid-plane. For the first rod, for the two-sided oxidation test about 70 microns, which was consistent with corrosion layer.

It didn't change that much when you went up about .3 meters. It was only 74 microns, but the big jump was between .3 and .65 meters in which we jumped from 74 to about 95. Round this off to 100 if you'd like.

So if you think of Ralph's presentation where you need to account for this corrosion in the

ECR, I'm just letting you know we're talking about samples of 5.3, 5.6, 7.1 percent, different than his numbers because this is very thick walled, 15 by 15 cladding. The thicker the wall the less the percent oxidation for a fixed oxide thickness.

We looked at the fuel cladding bond. We do metallography generally with the fuel in place. Then we de-fuel, and then we test. In some cases early in the program we do metallography after we defuel to make sure none was still there.

But if you go around the circumference and over the length, we looked and there was complete bonding of the cladding inner surface around the circumference. It was on the order of ten microns, and based on our experience with Limerick BWR and early TMI-1 PWR, the oxide bond definitely remains after nitric acid defueling of cladding. Nitric acid basically dissolves the UO_2 and other stuff in there and it leaves the ZrO_2 bond as is.

The bond is mostly ZrO_2 with some UO_2 in solid solution. And we got what we wanted. We got normal distribution of hydrogen. So we felt pretty good at this point in going about our test.

Useless. I don't know if you can see in your package.

CHAIRMAN ARMIJO: Let's see if we can see it.

DR. BILLONE: Well, I don't know what happened here, but anyway, this is the fuel cladding bond. This all came out too dark.

CHAIRMAN ARMIJO: This is awful.

CHAIRMAN ARMIJO: Did you find any cesium in your bond layer?

DR. BILLONE: Any cesium?

CHAIRMAN ARMIJO: Yeah.

DR. BILLONE: We found regions of cesium, but not dominant.

CHAIRMAN ARMIJO: Okay, but that --

DR. BILLONE: Mostly at the end of cracks, radial cracks in the pellets like in BWR fuel. There was a build-up of fission products and bond, and that's where we found the cesium.

That's great metallography, but you'll have to either get it from the report or something. It's not showing up well on the screen.

DR. POWERS: It comes out pretty well on the viewgraphs.

DR. BILLONE: Okay. This is the typical hydride morphology with concentrated hydrides near the oxide surface, and then this would be the higher elevation. Hopefully you can see it in your package, but it's a denser hydride structure.

CHAIRMAN ARMIJO: How much hydrogen was that at the higher elevation?

DR. BILLONE: Pardon?

CHAIRMAN ARMIJO: How much was that?

DR. BILLONE: About 740 weight parts per million, as opposed to 550.

CHAIRMAN ARMIJO: Yeah, it makes a difference.

DR. BILLONE: I'm glad I spent all of that time pasting those metallography --

CHAIRMAN ARMIJO: Well, the handouts are pretty good.

DR. BILLONE: Okay. Good. All right. When we presented these results on February 10th, 2005 in this room, it was strongly suggested and very wisely by EPRI that we really needed to verify our temperatures. We were basing our temperature history on their cladding with no oxide layer, and yet we were testing cladding which had a 70 micron corrosion layer on the outside, ten micron bond.

What that does is it slows down the heat of oxidation initially when you're heating up. So whatever boost in temperature you were getting from the heating rate of oxidation, since this is an exothermic process, you're losing some of that. And that was very wise.

So this is a case where we did the thermal benchmarking we needed probably five months after we ran the test and then helped us interpret the data.

So what are the effects of the bond then corrosion layers? As I said, they slow down the exothermic reaction. You expect a decrease in temperatures during the heat ramp.

We did run a thermal benchmark where we welded two thermocouples to the bare surface. We ended up 1,200 degrees C., grew 37 micron oxide layers, cooled down, and then ramped up again with the same parameters. So the first ramp is bare cladding. The second ramp is with let's round it off and say 40 microns oxide layer on the ID and OD.

And I don't want to go into it, but if you go through the thermal analysis, this really gives a lower bound than what your ramp temperatures are because it slows down the net heating rate, meaning that surface with the ten micron layer on the ID has a lot more heative (phonetic) reaction and would dominate.

So we feel like we've at least got a

reasonable lower bound the water temperature is in, and what happens is that initial high temperature peak decreased by about 40 degrees briefly, and then our long time temperatures, time to reach hold temperature was not affected. It was a small decrease, about five degrees C. in hold temperature.

So mainly what this affected -- and these are the benchmark results where the blue is the bare cladding. We took it up to here to grow the oxide layers, and then we cooled, and then we went back and then we re-ramped with the same control parameters, and so that 40 degrees is right here.

The net effect was a shift downward of about .7 percent in ECR. Instead of five we're at 4.3, and that was the primary effect.

So this was all done with a different test train, and so basically correcting for small differences in test trains. This is the thermal history we deduced which we think is a reasonable lower bound in here and excellent over here. This is basically what we ran our in-cell tests at.

This is what we used to recalculate ECR for our test times and to interpret data, and you notice the same rapid cooling rate.

Okay. For two-sided tests we had samples of this that were cooled without quench. We're trying to map out the ductility now knowing that that would be an upper bound. So if one of those samples were brittle, there was no sense repeating the test for quench. You want to quench something that's ductile to see the transition.

So in this range basically we have high ductility for the low ECRs. We had low but significant ductility for intermediate ECRs, and then for the 9.3 percent, the sample was brittle, and if you interpolate the ductile to brittle transition, it's 8.3 percent. If you're modest, you call it eight percent because you know you don't have that much accuracy in your interpolation.

We did compare our results with prehydrided samples. One of the points of this is does every vendor have to go test high burn-up cladding when he has a new alloy. That would be very, very expensive. So how good is prehydriding as a surrogate?

And we looked at the prehydrided samples. I'll show you. I got excellent agreement between the prehydrided and high burn-up samples for the slow cooled samples.

As far as oxidizing and quenching, we did have one sample left. We took it to seven and a half percent ECR. It had 700 and surprise, surprise. It was extremely brittle. We didn't know the hydrogen content at the time we did it.

So we at least know for Zry-4 highly corroded, you just can't go for that. You have to be below seven and a half percent with quench at 800 degrees C., and the results are consistent with those for prehydrided material, but you don't have a whole range of results for high burn-up.

If I have to take all of this data and give you a best estimate, it would be that if our high burn-up samples have been quenched at 800 degrees C., they would have embrittled at about five and a half percent CP ECR. It means I'm pretty sure they would be ductile at five percent. I know they'd be brittle at six percent. So pick a number in between, and it's five and a half.

Those are the words. This is a test matrix. Okay. Graphs are worth showing. Basically this sample and this sample were highly ductile. This one didn't fail at all. So that's why I'm drawing an arrow up.

This one was very ductile, and it wasn't until we got from 4.3 percent to about 6.4 percent that we saw a significant decrease in ductility, but surprisingly it kind of flattened out, and I really expected this sample here to be brittle.

This is one that is quenched. This one is slow cooled.

So where does the eight percent come from? It comes from 8.3 and 9.3, sort of a linear extrapolation of where do you cross the two percent line.

Okay. Let me just talk about one point because I hadn't showed you low displacement curves. This point is very difficult to tell how ductile it is, and this sometimes happens when we test. You don't get that sharp load drop in a highly ductile material.

So we first ran this test, and I was convinced that the sample would be brittle. So I had the operator stop the test right here and it was not a through wall crack. We have another sample. We ran it all the way through the end. You could hear a crack at the end, but in fact, it could have cracked anywhere, gradually cracked, anywhere along the way.

So maximum would be 53 percent offset strain for that point, which is kind of our test limit, and there is a little load drop. Minimum would be somewhere up here, and if I do take a 50 percent load drop, which most of our load drops are when it actually cracks, that's how I come up with the 37 percent.

The point is the sample is highly ductile, and without the sharp load drop I can't be precise about --

CHAIRMAN ARMIJO: Is that possibly a crack right at that --

DR. BILLONE: Well, the outside is brittle and the corrosion layer is brittle. You can get a crack part way. Yeah, it is a crack.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: It just wasn't through wall. It wasn't all the way through the wall. It wasn't all the way through the wall, and it will initiate under the inner surface, at the inner surface under the loading plate basically.

Okay. When I want to compare prehydrided samples to high burn-up samples, I should be conscious, we should be conscious that there is a difference in the heating rate, and that if anything, we're being more aggressive in heating, the blue curve which is the prehydrided samples.

Obviously, if you were looking at long

ECR, long times, it wouldn't be so important. If you're down where we're going to do the comparisons between five and seven and a half percent, if anything, I would expect the blue, the prehydrided to be a little worse, be a temperature higher for a longer period of time, pick up more oxygen.

But for the slow cooled samples, the agreement I would call remarkable. Basically now I've combined data between 470 and 720 weight parts per million hydrogen because it seems they have a gradual effect when you slow cool, and then our high burn-up samples you've seen. So these are two high burn-up samples. I'm showing the real stuff which is permanent strain. It's a more accurate measure, and one percent is our limiting criteria, and basically this is the agreement for the slow cooled between the high burn-up stuff and the prehydrided stuff.

So you can be your own judge. I call that excellent, and if anything, if you want to draw a line here and say, "Gee, it's a little bit higher," just remember that the blue samples were exposed to a higher temperature for a longer time.

So that's encouraging. Well, metallography, we're not going to do that.

CHAIRMAN ARMIJO: We can look at the

handout.

DR. BILLONE: Okay.

CHAIRMAN ARMIJO: You can just explain it.

DR. BILLONE: Basically there was a lot of interesting things learned about the behavior of high burn-up cladding and particularly oxidation behavior. We started out with a 70 micro corrosion layer. By the time we examined it, it wasn't 70 microns. Some of that corrosion layer actually dissolved in the metal. A lot of it flaked off during cooling and during handling. So this is not 70 microns that you're looking at.

This scale is only 30 microns here, but with SEM we could distinguish the remaining corrosion layer. The high temperature steam grown oxide layer, and we notice that this is about, oh, ten to 12 microns less than you would predict for bare cladding, and ten to 12 microns less than what the inner surface oxide layer is.

So it clearly shows that as far as oxide layer growth and heat of oxidation, this corrosion layer is not transparent. It does slow down the growth of this layer. However, the alpha layer, which is more important is about the right thickness, the same thickness as for bare cladding.

As a matter of fact, if you took this corroded sample and you heated it in a vacuum, the metal would steal oxygen from the oxide and reduce the oxide, and you would grow the alpha layer and you have enough to do it.

So whether there's team here and a bare surface, steam in a corroded surface, I don't know about the next statement, but vacuum and a corroded surface like that, you will get a growth of an alpha layer, which in turn means that you're pumping oxygen into the beta layer probably at the same rate for this picture as for bare cladding.

So, again, if you're talking measured weight gain, it's not going to be meaningful. If you're talking what's important.

Okay. That was SEM. This is the same type of picture in terms of metallography, and you get to see a little more of the corrosion layer.

Okay. The inner surface, again, the ID oxide is close to what you would predict. There was a ten micron oxide layer, bond layer here. It is gone by this time. It's most likely dissolved in the metal. The alpha layer is about the same as on the OD, and this is what you would kind of predict for bare metal.

Okay. So those are our two-sided tests. There's only a couple of points I want to make about the one-sided test that we ran. After generating those, the request was that, gee, you guys need to get to 1,200 degrees C. faster so that you're really running a 1,200 degree C. test.

So the way to do that is to plan one-sided oxidation tests where five percent ECR you actually are at 1,200 degrees C. for a longer period of time, whereas two-sided you never even reach 1,200.

We had run tests back in 2001 like that for different purposes and they served our purposes at that time. We did a much better job of benchmarking bare and preoxidized cladding before we went in cell to run the test.

However, what we learned, we should have known, but we relearned, that the only thing that would -- if you get steam leakage inside the sample and the oxidation near the ends results in hydrogen generation, that is why we flowed some argon purge through. The problem is once you lose that semi-protective bond layer, once it dissolved, then you lose hydrogen from the sample into the purge.

And so basically the tests weren't perfect in that we had hydrogen loss from high burn-up Zry-4 through the inner surface following reduction of the fuel cladding bond layer. The inner surface did form a visible alpha layer. The oxide was reduced by the metal during oxide reduction. As a matter of fact, when we compressed the rings, they cracked from the inner surface to the outer surface. There was enough embrittlement from the bond layer at the inner surface not to embrittle the whole cladding, but to initiate the crack which then grew through.

And basically it taught us that we really need to account for oxygen pickup from the cladding inner surface. These were defueled samples. So we're just looking at the effects of the ten micron oxide bond, and that ten microns of oxide bond, the oxygen definitely ends up in the metal.

As far as how much fuel would contribute to that, that's a more complicated issue.

Okay. This is our thermal benchmark, but what I want to do is just compare heating rates. This is our one-sided test to get to about five percent ECR. You're here on the temperature curve for the one-sided test, and for the two-sided test at about six percent ECR you got there at lower temperatures. So that was the point of these tests, and we ran them from about five percent ECR.

The most useful information we got out of it is the following. What's happening to the hydrogen doesn't affect the oxide layer growth, and so if I look at one-sided tests, we're now able to grow much thicker oxide layers.

If I look at the outer surface oxide layer, which is the only one I'm really growing, it, again, never seems to catch up with what the predicted value is for bare cladding. It's always lagging by about ten to 12 microns in this range. If you go to very, very high times, it might catch up.

So the corrosion layer, you know, it's protective, partially protective. It decreases the oxide growth rate initially. You lose something, and then it probably continues at the same rate as you would expect, but when you integrate over that rate, you're still about 11 to 12 microns behind.

Okay. I can actually see this. So maybe you can. I'm not showing you the oxide layer. This is the alpha layer on the OD of the cladding of that test sample. Again, there was a fuel cladding bond layer here. We, with help from our colleagues from IRSN predict about 100 seconds that bond would be completely reduced. So this is a test time. This is the 174 seconds of test time.

So we don't really know. The alpha layer could have been bigger at the 100 seconds because what happens is that a limited oxygen source, you grow the alpha layer. The oxygen continues to diffuse into the beta layer, and eventually the alpha layer disappears, as everyone knows who has prepared this material.

Okay. We had two data points that I never showed you before that were from a lower grid span that actually ended up with 550 weight parts per million from the one-sided test, and these are the results you've seen for the two-sides with slow cooling. These are the results at the -- now, this is by calculating CP ECR assuming that you only have oxygen on the OD of the cladding, and there is an indication certainly that the results are lower in terms of ductility. As a matter of fact, at seven percent you're brittle.

And there are two reasons for this: that you picked up additional oxygen from the inner surface that is not accounted for in this parameter, and also you have to allow for the fact that you heated at a faster rate to get there.

So that's what we learned from our one-sided test. I think I can do the balloon region very quickly because we're going to try to say there's no hope for saving ductility in the balloon region.

Basically without showing the curves, our LOCA program is that you heat at five degrees C. per second from 300 degrees C. to burst. These are pressurized samples, or you continue on generally to 1,200 degrees C.

You hold at 1,200 degrees C. for one to 300 seconds, and you control the cooling. This is different than the oxidation test. This is a higher thermal mass sample.

Control cooling at three degrees C. per second from 1,200 to 800, and then you quench at 800 or you slow cool without quench.

Again, we're going to look mainly at hydrogen. You have a burst opening. You have steam leakage in. The steam oxidizes with cladding away from the burst opening. Hydrogen is released. Hydrogen is trapped. Hydrogen is absorbed by the metal.

How fast does it happen? No one seems to know. My answer is fairly fast because if I take just this ramp up, this ramp down and essentially no hold time, at the thinnest region of the cladding, eight percent ECR and the hydrogen peaks are already at 2,800 weight parts per million. If I hold for 120 seconds, which is a higher ECR, the hydrogen peaks are about the same, and if I hold for 300 seconds, they are about 4,000, all extremely high.

And this is for out of cell, unirradiated cladding. Embrittlement curls relatively early, but again, bending strength decreases as the oxidation level decreases. Also, whacking you with a hammer, that's much more liable to shatter at high oxidation levels. It's very hard to whack it and make it shatter at the low oxidation levels.

We did run four in-cell tests for high burn-up Zry-2. They're all conducted with fuel cladding and high burn-up, cladding and fuel.

We were able to confirm in the balloon and burst region you do get two-sided steam oxidation. You have an ID oxide layer and OD oxide layer, but again, the big news was whatever bond layer you had on there was long gone in the balloon region, and so you picked up about 300 ppm due to secondary hydriding.

So there is a difference here, and let me just go quickly through this.

DR. POWERS: Mike.

DR. BILLONE: Yes.

DR. POWERS: Let me ask a question. In a real reactor accident --

DR. BILLONE: Yeah.

DR. POWERS: -- you would balloon. Steam would enter the balloon region. Then you would get a competition between the reaction of that steam with the clad and with the fuel.

DR. BILLONE: I'm not sure at that temperature you'd get much with the fuel at 1,200 degrees C. I'll have to check it.

DR. POWERS: I think, yeah, you would.

DR. BILLONE: I mean, it's not my area of expertise. That's why I'm not certain. Go ahead.

DR. POWERS: The reason I think that there might be some is that Oak Ridge did some experiments primarily for fission product release.

DR. BILLONE: Right.

DR. POWERS: And whatnot, and they seemed to get quite a lot of expansion of the fuel. They'd drill a hole in the clad. They didn't balloon it. They'd just drill a hole.

DR. BILLONE: Right.

DR. POWERS: And, you know, the oxidant would enter into that region and things would foam up on them and whatnot. So they were getting, you know, quite a lot of reaction with the fuel.

DR. BILLONE: Yeah, it was hard to tell

because our ballooning strain was like 50 percent, 40 percent. We had a lot of volume in there, and either --

DR. POWERS: Well, you may be getting so much volume the fuel reaction just --

DR. BILLONE: The fuel reaction and the cladding, you couldn't see it.

DR. POWERS: Yeah, you couldn't see it, and that's probably a good point.

DR. BILLONE: As a matter of fact, the ballooning strains were almost identical for the as fabricated cladding with no fuel, just zirconium pellets and the real stuff. It was remarkable, the ballooning -- not remarkable. There's an explanation, but that same internal pressure. They burst at about the same temperature with about the same maximum strength.

So I don't know about the steam fuel reaction. It just didn't show up in our test with such a large --

DR. POWERS: Yeah. I mean, you're probably right. It could be able to be compared to the volume you'd get.

DR. BILLONE: Okay. Just quickly showing you profiles, the red is oxygen content. So at the center of the burst region you get the most oxygen. Approximately multiply this by two and a half to get ECR, but it's 2.85 actually, but these are the hydrogen peaks, and again, this is a one second hold time and a fairly low ECR. So you only spent one second at 1,204 degrees C. Most of your oxidation -- it doesn't matter, but most of your oxidation was at a lower temperature. Released steam was picked up by the metal.

CHAIRMAN ARMIJO: This is hold time after --

DR. BILLONE: There's a five degrees C. per second temperature ramp, and then almost instantaneously you cool it at three degrees C. per second and quench.

CHAIRMAN ARMIJO: I just wanted to know how much time after the balloon bursts.

DR. BILLONE: Oh. The balloon bursts at about 750 degrees C.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: So you take 1,200 minus 750 divided by five.

CHAIRMAN ARMIJO: I can do the math.

(Laughter.)

DR. BILLONE: About 800.

Yeah, there's enough time. I mean, it's basically between the 800 and the 1,204 that you're getting all of your oxidation, but it's not much. I mean, we thought maybe this was a long time effect, and then if --

CHAIRMAN ARMIJO: The longer you hold them.

DR. BILLONE: At a 300 second hold time we did get a little bit more, but not significantly more.

All right. This is the same curve, only now I'm showing you hydrogen pickup where I've renormalized to kind of the weight of the cladding after burst but prior oxidation, and I converted oxygen to ECR. I think maybe Ralph showed a plot like this, but again, the same hydrogen peaks.

I should make the point that essentially the center of this burst region is embrittled by oxygen. It had got very low hydrogen, but these intermediate regions are also brittle by a combination of oxygen and hydrogen. They seem to be stronger, but they are brittle.

Now, the reason for high burn-up fuel, we didn't get a near zero minimum. So for high burn-up fuel, again, we try to keep the blue as the same. It's the hydrogen. It's about 2,000 ppm at the center of the burst region, which was a surprise to us.

So this material really is from edge to edge. It's about minus 50 to plus 50, is the extent of the ballooning where the strain is above two percent, and it's pretty much all brittle in that region.

And, again, Ralph showed you this picture that we like to show. We got a lot of that data in this region here and some of it in here for the hydrogen analysis, but the fact is in handling the room temperature the sample broke in three places. Two of them we wanted to cut out anyway.

So the only place we got to section it was D. Mother Nature and handling it broke it at those locations.

Okay. So that takes me through Chapter 6 quickly, and Chapter 6 has got a lot more -- Section 6. I'm sorry. Section 6 has a lot more information about ballooning strains and what burst openings look like.

Bottom line is we don't think we can really preserve any ductility there. The best you can do is have some kind of limit if you want it to have some strength, and also I should mention out Japanese colleagues pull in the axial direction, and what we've done is some bend tests, and it always tends to be a clean break across the weakest part, which is usually the center of the burst region, sometimes a little above and below the burst region, but within basically the balloon.

So if we're worried about fuel dispersal and you've got a bunch of these rods next to each other and there's really no place for them to go, if there's just a clean break across here, it's just something to think about. It's hard to imagine what would -- even if it were a three inch long balloon what loading would actually smash and fragment all three inches.

Okay. I have a short presentation coming up on Section 7. It's short because I wrote it this morning, but that's where we try to sit back and look at the data we generated and test the existing criteria for embrittlement and just apply an F factor because the current criteria gives you slightly too much margin.

And so let's --

DR. ABDEL-KHALIK: Can I ask a question?

DR. BILLONE: Please ask me a question before I do that because that's --

DR. ABDEL-KHALIK: If you put up Slide No.

40 that shows the oxygen and hydrogen concentration distributions. The one before that. Right there.

DR. BILLONE: Yeah.

DR. ABDEL-KHALIK: Is there anything we can learn from this spatial spacing of the oxygen and hydrogen peaks?

DR. BILLONE: Sure. This is where the cladding is the thinnest, and so it's supposed to steam for the same amount of time as this thicker cladding. The percent oxidation is less.

So the oxide layer may be the same here on the ID and OD, the oxide layer, as it is here, but this is much thinner because of the large ballooning strain.

DR. ABDEL-KHALIK: I'm talking about the location of the hydrogen peak.

DR. BILLONE: The hydrogen peak is simply --

DR. ABDEL-KHALIK: Relative to the center of the balloon.

DR. BILLONE: In the report, preceding each of these is a picture of the ballooning strain, and essentially these hydrogen peaks occur just outside the ballooning region where steam can't get to but hydrogen can.

DR. ABDEL-KHALIK: So it's just related to the geometry of the ballooning.

DR. BILLONE: It's just related to what happens to the hydrogen. Hydrogen is a much smaller molecule and it can travel further up and down the rod in any gap area, and the gap area would be created by -- when I say "outside the balloon region" I mean strains of less than two percent. It's not like it's zero.

So, yeah, it's depending on where the steam can get to, and the steam can obviously -- by here the steam is just oxidizing the outer surface of the cladding also, but this oxidation generated hydrogen that's migrated there, and as long as it's bare surface, it can get in easily.

DR. ABDEL-KHALIK: Thank you.

CHAIRMAN ARMIJO: Mike, when you go through this part, somewhere along the line either yourself or Ralph, if you would just kind of present your ideas of how someone would use -- if someone wanted to come in with a new alloy, brand new alloy, and he said, "Okay. Look. What do I have to do to meet these criteria from a test standpoint and from an analysis standpoint?" I'd like you just to kind of somewhere in your talk just to go through the steps that you would see would be required and what ductility numbers would have to be satisfied.

DR. BILLONE: Yeah, I'm going to.

DR. MEYER: I would like to do that from one of the next to last slides that I showed. So I could do that --

CHAIRMAN ARMIJO: Yeah.

DR. MEYER: -- probably when he's finished. Maybe we could go back to that.

CHAIRMAN ARMIJO: Okay. Great.

DR. BILLONE: Yeah, but as far as this phenomenon, I can't see an alloy that would be resistant to it.

CHAIRMAN ARMIJO: I can't either, but I'm just saying, you know, what kind of --

DR. BILLONE: I mean, this is like whatever criteria you write, we could certainly give criteria for the bulk of the rod. The three inches or less of ballooned area is not something that can be protected by ductility criteria.

CHAIRMAN ARMIJO: Unless you can stop ballooning, this game is over.

DR. BILLONE: You have to stop burst.

CHAIRMAN ARMIJO: Stop the burst, right.

DR. BILLONE: Some countries, right?

CHAIRMAN ARMIJO: That's tough to do.

DR. BILLONE: All right. Now I have a brief section, believe it or not.

Okay. Humbly, I have to repeat again up through Section 6 of the NUREG really what we committed to do is providing all of you with the data. This is, as I say, an attempt on our part to do what you all will be doing, and this is just some of our ideas on what might work in applying the data.

And we've got to get the slide show, don't we?

Okay. Let's do break-away oxidation first because if you have comments, this is a good time to make the comments.

Break-away oxidation, again, tends to be a phenomenon. Everyone studies 1,000 and 1,050 and no one studies 1,025. So I'll pick 1,025. It's a phenomenon that occurs less in 1,025 and all alloys we've tested except E110. Russians have done extensive testing at 1,100 degrees C. It breaks away. So put old fashioned E110 off to the side.

So during minimum time, a performance based test, this is just to determine minimum time for break-away oxidation. It's not so easy. We believe it should not be based on the weight gain increase but the minimum time to pick up 200 ppm of hydrogen because we know ductility is retained for that level of hydrogen for these low temperatures.

We also know break-away is an instability phenomenon. You don't want to mess with it too much and push it too high. It's most likely dependent on the alloy, and not every Zry-4 made by everybody is going to break away at the exact time, and certain Zry-4 versus M5 versus ZIRLO are different.

On impurity content, particularly near surface impurity, and surface finish, we can never really separate these two because when you polish and when you machine and polish, you're sort of polishing out impurities as well as smoothing the surface.

CHAIRMAN ARMIJO: You don't have a variable of material heat treatment, for example, whether it's stress relieved, solution annealed?

DR. BILLONE: We have a variety of material, but generally the details of the fabrication rare not provided to us.

CHAIRMAN ARMIJO: Okay. So if somebody wanted to do that, he'd check with his --

DR. BILLONE: Zircaloy-2 which we've tested is recrystallized annealed.

CHAIRMAN ARMIJO: Right.

DR. BILLONE: Zircaloy-4 is --

CHAIRMAN ARMIJO: Stress relief.

DR. BILLONE: So we have that weighing. The M5, I believe -- and please correct me -- I believe that's stress relieved and the ZIRLO, I can't find it published, but I -- I'm sorry. M5 is solution, recrystallized anneal, and the ZIRLO I don't know, but I assume it's made like Zry-4. I just don't know the answer.

So it's not a variable in our test. Each material is --

CHAIRMAN ARMIJO: Whatever was delivered.

DR. BILLONE: Right. So, again, let's summarize the results because the idea is you do performance based tests. Initially we thought you just do it 1,000 degrees C. It's the worst temperature, and then you're finished, and now I'm not so sure because we've got a significant difference.

Anyway, for the old fashioned Zry-4, I remind you it's 3,800 seconds. They're belt-polished. We got improvement to 5,000. With Zry-2 we've only tested at 1,000 EC and it's greater than 5,000 seconds. Belt-polished ZIRLO break-away times are greater than or equal than 3,000 seconds at 970 degrees C. and for M5, Argonne tested up to 4,100 seconds previously at 1,000 degrees C, and CEA tested and found break-away at 6,400 seconds.

So if we tested M5, we would be in between this and probably close to 6,000 seconds, and the E110, depending on how you count time, counting the ramp time would be 700 seconds.

CHAIRMAN ARMIJO: Was there a time and temperature that you would pick as saying, okay, this is what you have to pass?

DR. BILLONE: I thought I could do that by just picking 1,000 because I thought it was most susceptible temperature for break-away and oxidation is higher and so hydrogen generation is higher, but it's alloy dependent, and it's dependent on a lot of variables.

CHAIRMAN ARMIJO: So you're going to have to find that for each different alloy, explore?

DR. BILLONE: Maybe one could pick a more limited number of tests within the range of 950 to 1,000 and one test at 800 to confirm. Since it is more complicated than I originally thought, the original idea was to go in and do tests at 1,000 degrees C.. They were going to be fine, but you needed to double check above and below, and that didn't work out because 1,000 degrees C. wasn't always the minimum time for break-away.

So I don't know how this will be worded, but it's certainly performance based tests and it would be alloy dependent.

I didn't mention it, but there were two valleys, minimum break-away times for Zry-4. They are pretty close to phase transition temperatures, although I can find no relationship between phase transition and break-away oxidation. Coincidentally they are, and that's why you've got to tell the person doing the LOCA calculation; you've got to tell them when you start the clock in counting time.

We picked 650 for EC as a reasonable time because all phase transitions are completed basically and all of the alloys by 650 degrees C. Oxidation is extremely slow. We know it's 650 if you hold it for a long time. It takes something like four hours to get up to 200 ppm of hydrogen. So we figured it was behind.

Now, could it be 700? Could it be eight? I don't know if it makes a difference.

DR. MEYER: Could I jump in here a minute?

DR. BILLONE: Sure.

DR. MEYER: Because at the last minute I put in the slide of the old Leistikov and Schanz paper, and I did that and you'll notice that there were some -- now, you won't see the yellow lines on the black and white copies, but up on the slide I had highlighted 600 and 700 degrees, the 600 and 700 degree traces, and 650 is the number that we had chosen and we realized that this might be somewhat arbitrary.

However, when you go up to 700 degrees and you look at that accelerated oxidation after the break-away, you see that the oxidation rate is pretty high, and by 25 hours, which I know is too long, but in the time period of the testing it was catching up with some of the oxidation levels for the much higher temperature test.

So I think this goes back to the statement that at 650 and below the oxidation rate is so low that you don't worry about it. Above 605 after break-away, the oxidation rate picks up pretty substantially, and so somewhere not far above 650, you're into this region where you're both susceptible to break-away and where the oxidation rates are rapid enough that the hydrogen absorption would be quite high.

So that's the best we can do, I think, on this choice of 650 at the moment, and perhaps the committee or someone else can help us refine that number, but it looks like a number if you stay below it, you're okay.

DR. ABDEL-KHALIK: Would it make sense to tie this number to an operator action specified by the emergency operating procedures? I mean, for PWRs, operators are instructed to enter into the C1 procedure at 1,300 F, which is about 700 C., and that way you have a criterion that you can not only check with codes, but you can also check in terms of response of operators on the simulator.

DR. BILLONE: It goes way beyond me.

DR. MEYER: Well, somebody else can maybe jump in here, but is it 12? I think some of the PWRs have operating procedures that kick in at 1,200, which is 650 and where they take actions to reduce temperature in the secondary side and thereby shorten the period of time that these temperatures would hang up in a small break LOCA.

If you need further discussion on that, it will have to come from someone who really understands what I just said.

CHAIRMAN ARMIJO: Well, I'm still trying to figure out are we trying to determine the minimum time and the minimum temperature where break-away oxidation occurs? And you've said 650 is probably nothing will happen in a lifetime.

DR. MEYER: Four hours.

CHAIRMAN ARMIJO: Yeah, and so somewhere between 650 and 1,025, there could be break-away as a function of time.

DR. MEYER: You don't want break-away to occur during the LOCA transient at a temperature that's high enough that you will get rapid loss of ductility at times and temperatures below the normal limits for --

CHAIRMAN ARMIJO: So the alloy developer has got to check that to make sure that he doesn't have a material that has a lo break-away temperature or a short break-away time for any given temperature. So in order to qualify a new material to meet these criteria, he's got to explore that by testing or something. That's what I'm trying to get at.

How could I use -- what would I have to do to --

DR. BILLONE: That would have to be part of the performance testing. Being more specific is challenging.

CHAIRMAN ARMIJO: I'm kind of jumping ahead to when somebody is writing a rule. We're getting into very complicated --

DR. MEYER: Well, we have a good counter-example in this Russian E110 cladding because it breaks away 500 to 700 seconds, which is a very short period of time, and that won't protect you from the consequences of break-away, the embrittling consequences of break-away during a typical PWR small break LOCA. And so that's not good.

CHAIRMAN ARMIJO: Okay.

DR. BILLONE: And you can certainly easily screen out a bad alloy, but, yeah, we haven't gotten far -- I haven't gotten far enough to thinking through how to conduct the tests. I mean, we found a way to do it economically at Argonne because we were short on time, this searching method starting at 1,000.

CHAIRMAN ARMIJO: Okay. Well, it's something to think about.

DR. BILLONE: You come out with a totally new alloy that I've never -- I wouldn't know what to say about that. It's a phenomenon. It happens. You want to stay away from it. One way of doing it is this way: determine the minimum time for break-away oxidation and the local relevant temperature and stay below that. That's one way to do it. It may be-- as I say, is it too --

CHAIRMAN ARMIJO: But those kinds of instructions would be more like a reg. guide rather than a rule.

DR. BILLONE: Yeah, yeah.

CHAIRMAN ARMIJO: Because it's way too complicated for a rule.

DR. BILLONE: yeah, that's Ralph and his role.

CHAIRMAN ARMIJO: Well, we'll talk about it later.

DR. BILLONE: Let me get in trouble with the next slide. Okay. All right. Let's go to the higher temperature regime. In terms of trying to determine -- okay. I'm okay. This is fresh cladding. I'm not in trouble yet.

What we basically recommend is performance based tests are exactly what we did, determined ductile brittle transition at 1,200 degrees C. I recommend two-sided steam oxidation tests because these are controlled. The temperature is easy to measure, the weight gain with reasonable heating and cooling rates and quench at 800, which we found to be not important, but we might as well do it.

Limit LOCA time, temperature such that the CP ECR during the transient is less than or equal to whatever limit you find out.

The approach is conservative for Tmax, but the LOCA Tmax being less than 1,200 degrees C. So that's another performance test, and these are the examples of the results that we got for the claddings we tested.

I mean, one could ask for more and one could ask for 1,000 and 1,100, but 1,200 would be the most. Whatever your limit is, it would be the most embrittling condition to test for.

So with the oxygen and hydrogen induced embrittlement, we want to test the subtraction of the corrosion layer recommended, and information notice 9,820. We doubt strongly that it will be so brilliant back in 1998 that they're going to hit it right on the nose. So we include an F factor times the corrosion layer just to see what empirically that would turn out to be.

And we needed it to account for cooling rate effects, quench temperature, and possible alloy effects to determine what margin, what transient ECR are you allowed.

So the idea is with this approach and testing it is whatever your calculated local temperature history is, you do what you already do.

You calculate a transient ECR, assuming bare cladding, taking no credit for slowing down the oxide, and you do it with the Cathcart-Pawel model. If you do a Baker-Just you would even be more conservative.

You've already determined this for fresh cladding. This is a straightforward calculation based on your corrosion level, and F is the undetermined factor. So we're going to use the complete data set for HBR cladding, ductilely dated for high burn-up Zry-4 with 70 micron corrosion level to try to determine what F is for this corrosion level, and the results we're going to use, and actually we're showing you the fresh cladding is only good up to 14 percent ECR. The corrosion level varied a little bit from sample to sample, 5.3 to 5.6.

Unfortunately, it makes a difference whether you use the 14.3 or the 5.3 or the 5.6 in determining the F factor.

So what are you allowed? Well, if you slow cooled, you're allowed about eight percent for the transient ECR because that's where high burn-up samples went brittle. I mean, your LOCA is not going to be slow cooled, but just to give you an example.

As I said, if we had quenched the high burn-up samples, I know the answer would be greater than five, and by the way, what's in parentheses here is since we don't include cooling for our ECR calculations as generally small, it's .2 percent ECR of cooling.

So this five percent that we've been quoting is up to the end of the heating ramp, and for very rapid cooling, there's only a small increase in ECR.

So I know the answer is between five and six, 5.2 and 6.2, which means we're going to test this model assuming that if we had quenched the high burn-up samples and we had only gone to 5.5 percent ECR, we would just cross the threshold from ductile to brittle, and that's pretty much the best I can narrow that down.

So what do you get for F for all of these conditions, including slow cooling? You get F of 1.3 plus or minus .3, assuming your quench is at less than or equal to 800 degrees C.

All right. Let's take advantage of some conservatism in our calculations, and I'm missing an important figure. I forgot to put it in this morning, but is the 1.5 to 1.6 too conservative, which comes directly out of our data, for quenching 800 degrees C.?

The LOCA cooling rates that we've looked at are slower than the ANL cooling rates. Additional oxidation, meaning oxide layer growth, occurs during cooling with little change in average oxygen content in the beta layer. That means little change in embrittlement. It implies that you actually get a little boost from cooling slower than we do. You get a higher ECR value, and you would get a decreased F value.

So let me give you examples and see if they fit in with those of you who do thermal hydraulics.

If I take from the peak temperature down to about 800 degrees C. and I cool at five degrees C. per second, I get an increase in the total amount of ECR to calculate. My F factor drops down to 1.4. It's beautifully shown in the graph, which is on the Web, but it's not on the machine right now, and this is supposed to be three degrees C. per second. Please make this correction for me. It was like the last thing I typed before I rushed over here. Three degrees C. per second and the F factor would drop down to 1.3.

So depending on where you quench, if you're going to quench at less than 600 degrees C. and your cooling rate is in this range of three to five degrees C. per second, then I would expect 1.2. That's where the 1.2 comes from, plus or minus .2. In other words, it would include the five degrees C. per second cooling value of the 1.4, the slow cooling value, which is about one, and that's about the best I can nail that down right now.

I do recommend that new alloys, in particular, that prehydrided allies, the oxidize and quench, determine if the F factor is alloy dependent. I mean, it's a personal recommendation.

Before we do our in-cell test with ZIRLO and M5, certainly with ZIRLO, we definitely want to prehydride it, run the same test, baseline data, see where we are and to help us plan the in-cell test.

CHAIRMAN ARMIJO: For Zircaloy-2 and Zircaloy-4, can you say that the F factor is pretty much the same?

DR. BILLONE: I can say it's the same because it won't matter because the Zircaloy-2 BWR cladding grows like a ten micron oxide layer. So it's a low number times 1.5 or 1.4, and it doesn't matter.

CHAIRMAN ARMIJO: It doesn't matter, yeah.

DR. BILLONE: It's the same. It's really the BWR cladding that it matters.

DR. MEYER: I think that's an important point, and it also applies to the M5 and the ZIRLO. The real critical one is the Zircaloy-4 with the very heavy corrosion layer, but as you go to fuel cladding that corrodes less and less, the subtraction factor is smaller and smaller, and the errors in the F term become less and less important.

So I think --

DR. SHACK: Unless somebody wants to drive their cladding harder and harder.

DR. BILLONE: Yeah, I mean, the Spanish drives ZIRLO very hard. So it's got 80 to 100 microns.

CHAIRMAN ARMIJO: They will.

DR. BILLONE: I would assume they would.

CHAIRMAN ARMIJO: They will.

DR. BILLONE: But one of my last comments, again, let me emphasize that particularly sine I've seen data for prehydrided M5. I haven't seen it for ZIRLO. I wouldn't want to go into the test of the high burn-up material, which is very precious. We have a limited supply without a fundamental baseline

That will help us pick up our test times on ECR to try to bracket the ductile and brittle transition, and that pretty much is the end of my presentation, and thank you for your patience because it was long.

CHAIRMAN ARMIJO: Ralph, did you want to make some wrap-up comment?

DR. MEYER: Well, the only thing I would do if you insist is to go back to your question about what would be the test that one would perform and do you want me to put that slide back up?

CHAIRMAN ARMIJO: Yeah, we gained 15 minutes, and I want to use it productively.

(Pause in proceedings.)

CHAIRMAN ARMIJO: This is it, huh?

DR. BILLONE: Sorry.

DR. MEYER: Okay. I think the answer to your question is what do you do to test a new alloy to see if it's LOCA behavior is great or not so good.

CHAIRMAN ARMIJO: Or, Ralph, let's say some manufacturer discovers a wonderful new heat treatment and he's really happy with it. Maybe it's messing up something else, and how would he check to make sure that he hasn't shot himself in the foot with regard to LOCA?

DR. MEYER: Okay. With regard to the LOCA, I think it's fairly simple. You need to test for the embrittlement threshold at 1,200 degrees C., 1,204 degrees, 2,200 degrees Fahrenheit.

CHAIRMAN ARMIJO: That's a test, not just an analysis.

DR. BILLONE: That's a test. You go in the laboratory and you measure it. So the way that we would try and apply these criteria for a new alloy that we haven't seen before, as long as it's within the family of zirconium-tin-niobium and those other things that we put in there, I think the first thing you do is you go in and measure the embrittlement threshold of 1,200 degrees in fresh material, and you get a number, and we've shown you numbers for all the current cladding types, and it's going to be a number somewhere around 17 to 20 percent.

Then you need to know what your corrosion behavior is during irradiation, and you know that because that's --

CHAIRMAN ARMIJO: You shouldn't be in the business unless you've got that.

DR. MEYER: -- because you do that in the normal process of developing an alloy.

And then the only other thing that you need to do is to check for break-away oxidation and to get a number for the minimum time to break away oxidation, and if that minimum time is significantly longer than the time you're going to hang up during a small break LOCA, then you won't have a break-away problem.

Now, there are some fine structure that could be seen with the prehydrided tests which Mike has suggested. I personally thank you for just checking, just for a screening test that you wouldn't have to have the prehydrided results to look at either the break-away or the adjusted oxidation limit.

So those are the three tests. We have a bad apple in the basket with the old Russian cladding, and they pick it up right away, and I think they give you the benchmarks that you need for all of these.

There's somebody behind you who wants to comment.

MR. MONTGOMERY: I'm just waiting for you to be done, Ralph.

DR. MEYER: I'm done.

MR. MONTGOMERY: Mr. Chairman.

CHAIRMAN ARMIJO: Yes, please.

MR. MONTGOMERY: I have a question.

CHAIRMAN ARMIJO: You have to identify yourself. Say your name.

MR. MONTGOMERY: Oh, I'm sorry. My name is Robert Montgomery. I'm with EPRI and a tech.

The question here is you didn't mention anything about the 1.2 factor.

DR. MEYER: The 1.2 factor. Yeah. Well, Bill Shack raised a good point. My presumption was that the only cladding that was going to develop high corrosion, that is, up near a ten percent ECR which we see at 100 microns of corrosion on the thin cladding, if you're going to have that much corrosion, you might want to look more carefully at the F factor.

It's hard for me to imagine that you're going to develop new alloys that will have that kind of corrosion behavior, but if you had an old one that you're pushing and it's not --

CHAIRMAN ARMIJO: You should trade to cell?

DR. MEYER: -- then you might want to look more carefully at the F factor.

MR. MONTGOMERY: Well, the F factor brings in the effects of the transient time history. It also brings in the effects of the alloy and things of that nature and the effect of hydrogen.

DR. MEYER: Yes. Definitely there's room for refining this process with regard to the F factor and the transient time history. I, frankly, don't know how to do it since we're trying to develop criteria that you can apply broadly to any small break or large break LOCA, but I would say that it's a possibility.

MR. MONTGOMERY: So you don't expect then as an outcome of this process to have a procedure established that would define how one would get to an F factor for a particular cladding alloy.

DR. BILLONE: Could I comment on that?

DR. MEYER: I don't know the answer. I guess it depends on the kind of comments that we get.

DR. BILLONE: No, but if you run -- I mean, obviously we're trying to find out what the transient ECR that you're allowed. This is Mike Billone from Argonne.

You could run a series of prehydrided tests to directly determine the allowable transient ECR for different hydrogen levels, correlate that back to what your corrosion layer thickness is and the corresponding hydrogen content, and you could either -- a hole different approach than Ralph is showing you -- you could just stay with the prehydrided data and forget the subtraction or you can use it to determine an F factor for your alloy without ever testing irradiated material because you know the oxide layer thickness for your operating material.

You should have a hydrogen pickup that you've already determined for that material.

So then you go in the lab with prehydrided material. That is one way to do it for new alloys.

DR. MEYER: We actually have a small difference of opinion --

DR. BILLONE: We disagree on this.

DR. MEYER: -- about the hydriding because I think when you look at the figure where he compared the prehydrided results with the real thing that the difference was -- it was small but it was significant, and it is significant enough that it could make a difference on the F factor.

The thing that sticks in my mind is that we have been regulating for the last eight years with basically a subtraction with an F factor of one, and we have good information on the cladding type that develops the most corrosion in our U.S. plants. That factor of one is not quite good enough, and we also have unlimited data set and we all know the reasons that we haven't been able to move faster with that.

So my opinion is you pick a general number like 1.2 and you stick with it until you can develop a better database. It's certainly better than 1.0.

DR. ABDEL-KHALIK: Excuse me. The two

quantities on the right-hand side, you indicated that both of them are experimentally measured.

DR. MEYER: That's correct.

DR. ABDEL-KHALIK: Unirradiated ECR.

DR. MEYER: Well, one of them is measured. The corrosion thickness is measured from your end reaction.

DR. ABDEL-KHALIK: How about the unirradiated ECR?

DR. MEYER: And this one you measure from tests in the laboratory.

DR. ABDEL-KHALIK: If that's the case, doesn't that penalize the M5 alloy right off the bat?

DR. MEYER: Why is that?

DR. BILLONE: No, it helps it.

DR. ABDEL-KHALIK: I mean, based on the data that was shown earlier, the measured ECR for M5 was considerably lower than the calculated value.

DR. BILLONE: Oh, I'm sorry.

DR. MEYER: You're looking back at the corrosion layer. It's a hard concept to explain adequately, but the embrittling process is a consequence of diffusion in the metal, not of oxidation process on the surface. Give it some oxygen on the surface and it will diffuse into the metal according to its own kinetics.

Now, it turns out that those kinetics are in the same ballpark as the kinetics that describe the oxidation process, and so we just use one of those equations instead of using straight time. It simplifies things.

DR. BILLONE: Excuse me.

DR. MEYER: It's similar to what was done in the past, and it doesn't penalize M5 at all int hat respect.

DR. ABDEL-KHALIK: Did you want to say something?

DR. BILLONE: Yeah, I just wanted to clarify that that last term goal, the ECR corrosion, you're not using any correlation at all, like Cathcart-Pawel. It's just --

DR. ABDEL-KHALIK: No, I'm talking about the ECR unirradiated.

DR. BILLONE: That's about 20 percent.

DR. MEYER: And the fact that we use the Cathcart-Pawel weight gain equation, which for M5 over predicts corrosion at lower temperatures, but it's only used as a correlating parameter.

DR. SHACK: Well, I think it could work. I mean, you could give with one hand and taketh away with the other. I mean, if you did it on measured ECR, then his limit would be eight percent instead of 20 percent.

DR. BILLONE: Right.

DR. MEYER: That's true.

DR. SHACK: And then he could correlate against measured and he'd have a limit of eight percent. He can correlate with Cathcart-Pawel and have a limit of 20 percent. It changes the numbers, but, in fact, the limits on the fuel or the cladding and its real use haven't changed at all.

DR. MEYER: Sam, someone else wants to comment.

CHAIRMAN ARMIJO: Oh, yes.

MR. DUNN: Bert Dunn, AREVA.

I think we should recognize that what we're talking about here is an irradiated ECR determined at 1,200 degrees C and at last for that thick the implied cladding there isn't a difference in the kinetic at 1,200 degrees C.

DR. BILLONE: That's right.

MR. DUNN: The difference in the kinetic is at 1,000 degrees C.

So that helps M5. The point is still valid for some other alloy, you know, that we haven't dreamed up yet maybe.

Thank you.

CHAIRMAN ARMIJO: Any other questions?

Well, one o'clock. I'm really proud of you guys. We'll recess and come back after lunch at 2:00 p.m.

(Whereupon, at 1:00 p.m., the meeting was recessed at 2:00 p.m.)

AFTERNOON SESSION

(2:02 p.m.)

CHAIRMAN ARMIJO: Ladies and gentlemen, we're ready to start, and we're going to resume the session.

The order of the speakers is going to be changed a little bit from the handout. The first speaker will be Mr. Robert Montgomery of Anatech, followed by Mr. Dunn of AREVA, Mr. Nissley of Westinghouse, and Mr. Patterson of GNF. Robert, if you're here. Where's our first speaker?

MR. MONTGOMERY: Thank you, Mr. Chairman. Sorry I'm late. I had a battery problem.

Okay. I'm beginning to echo like you did, Ralph. I can hear myself.

All right. Well, first I'd like to thank the subcommittee for giving me the opportunity to present the industry comments on the ANL NUREG report and on the NRC research proposal on their revision of the embrittlement criterion 10 CFR 50.46.

My name is Robert Montgomery. I am a consultant to EPRI. I work for a company called Anatech Corporation, and I will be summarizing the industry comments on what we saw this morning.

As the Chairman indicated, there will be also presentations made by AREVA, Westinghouse and GNF summarizing their individual comments as well.

A short outline of what I plan on talking about today is first I'd like to give some background on the industry support that we've provided to NRC and Argonne in the program, and that was summarized this morning. Then I'll go through some of our concerns with the proposed technical basis to revise the 10 CFR 50.46, Paragraph B criteria.

Then briefly, I'll kind of summarize some of the data we think is needed to try to understand the phenomena and to support more relevant criteria development, and then finally some conclusions.

The industry has been collaborating or working with NRC on the testing of high burn-up fuel under LOCA conditions. It has primarily been through a group that EPRI manages called the Fuel Rod Reliability Program. The Working Group 2 that is a subgroup of that program that is made up of industry representatives, both from the utilities as well as the fuel vendors, and basically that's the focal point for the interactions with NRC on fuel licensing issues.

We've been doing that for regulation related issues. We go through NEI, and then directly with Research on research topics like LOCA and RIA. And I said, the members include both U.S. and international utilities, fuel vendors, and NEI.

Specifically, the program has been involved with the Argonne testing since 1998. A little parenthetical there. We've pretty much had limited input since about 2005, but primarily the input has been on providing the fuel. You can see that the industry has provided the irradiated fuel to the program that has been used, as Mike talked about today, the H.B. Robinson fuel for PWRs, the Limerick fuel, and even the M5 and ZIRLO cladding that's been provided or will be provided.

In addition to providing the fuel, we've also provided analytical support to the program, helping define some of the test conditions, particularly related to the LOCA integral test, and also help understand the data to some degree in the mechanical property area.

With regards to the specific objectives of the program to develop a technical basis to modify or revise the embrittlement criteria, the industry overall is supportive of that effort and the objectives of that to revise it. We feel that the revision to a performance based concept or performance based approach is a good one, and we like the concept, and we also feel that by revising the existing regulations we can start to look at ways to get new cladding advances taken care of without the need for a rule exemption, so that we can go through that process.

So those are the benefits we see of going through the revision. Now, in 2005, there was a status review and discussion of the technical basis to support the revision, and at that time in 2005 we were supportive of that technical basis. However, it was a qualified support. We felt that the data still wasn't complete enough to really support the approach that was being discussed at that time and really to talk about it here today as well.

And so we echoed those at a full ACRS meeting in September, that we were supportive of it. However, there were areas where we thought that more data was needed to really give our full support. And these areas were we thought that there needed to be more data that supported the approach. So completing some of the ANL tests that were talked about at that time, both recompression tests and integral tests on LOCA samples, the LOCA integral tests, and also some clarification on how these data would be used, some of the questions that came up this morning. What would be the test conditions one would use to test different types of cladding? And then how also it would be applied.

Specifically, some of the questions that we had or some of the areas we thought was needed is summarized here, and those included, like I said, recompression tests, and we had requested at that time the recompression tests be completed on irradiated samples from H.B. Robinson that had been quenched.

And that actually didn't turn out to be the case. However, Argonne was able to add some additional prehydrided samples evaluating the effect of quench. So there is some information that came about. It still may not be fully complete yet though.

In addition to the recompression tests on the H.B. Robinson cladding, we were also expecting to see recompression tests on prehydrided and irradiated ZIRLO, M5 and Zry-2 material. That was not completed at this time and has not been completed at this time.

Integral LOCA tests on irradiated PWR cladding, to confirm the overall LOCA behavior, that was part of our qualified support that hasn't been completed at this time yet.

At the 2005 ACRS meeting, we requested

that some justification be provided to support the balloon region, and now that has been provided. It's a strength based justification to disposition the balloon region.

As I said, we were looking for test definitions and protocols on how to go about establishing the criteria for particular alloys. Those procedures and protocols have not been defined at this time.

And then finally, the confirmation that using in-reactor corrosion as an adequate surrogate for the effect of hydrogen. More data was developed in the last year, but it still appears to be incomplete because of the number dependencies that I'll talk about here in a few moments.

So that was kind of a summary of what we were expecting to see in the new report that was just issued. Some specific concerns I'd like to talk about are the F factor and then the double-sided oxidation.

As we saw this morning, the effect of the radiation through the hydrogen and corrosion is now being addressed by a factor of the F factor, and the F factor basically is a multiplier that multiplies on the corrosion thickness to account for the effects of hydrogen content, cooling path, quench temperature and phased transformation temperature and other variables that we may not actually know at this time on the post quench ductility behavior.

Our feeling is that the F factor is really not a complete factor that can address all of the impacts of these variables in a single factor, that it's a much more complex factor than a single number.

In addition, it's really not clear how to apply a single factor for a wide variety of LOCA transients that might have different cooling path, different heating path, different quench temperatures, et cetera, et cetera.

So the feeling is that trying to use one single F factor would be too bounding or too conservative, and as a result, we need more data.

DR. POWERS: Let me ask you a question. Are you casting this in terms of an F factor embodied in the rule or an F factor in a regulatory guide that says, "Do it this way and the staff is happy, or come to us with a different way"?

MR. MONTGOMERY: Let me see if I understand your question. Are we concerned about an F factor in the rule versus an F factor in the reg. guide?

DR. POWERS: If the F factor, just

exactly as it has been portrayed, recognizing that they haven't exactly solidified on that, but suppose it was just exactly that way and the regulatory guide said, "This is a mechanism of analysis acceptable to the staff, but if you want to bring to us something else, we'll be glad to look at it."

MR. MONTGOMERY: What we're expecting is a recipe or procedure by which one would come in. I think Dr. Armijo mentioned it earlier. There needs to be some sort of specification of how an alloy would be tested in some fashion.

DR. POWERS: I mean this would be the staff saying, "Here is a recipe. You can use this or use something else. If you use this, we understand it, and it's acceptable to us, but if you want to use something else, we'll certainly listen to you."

MR. MONTGOMERY: I believe we're receptive to that approach in general. The specifics of defining the F factor, I think, still need to be better defined, but if it's in the reg. guide where deviation can then be accepted --

DR. POWERS: What I'm asking is it's defined exactly as it is now, the 1.2 or maybe it's the 1.3 plus or minus .3. It's exactly that way, no more, no less, in a regulatory guide.

MR. MONTGOMERY: Oh, the F factor is specified in the regulatory guide.

DR. POWERS: Yeah.

MR. MONTGOMERY: I see.

DR. POWERS: And the staff says you can use this or you can come to us with some other definition and we'll look at the technical basis for it.

MR. MONTGOMERY: I think in general we would -- I mean, it's hard to say at this point, but in general it would be acceptable of that.

CHAIRMAN ARMIJO: As compared to being in a rule where there's no --

MR. MONTGOMERY: Right. If the options are in a reg. guide or a rule, we certainly want it in a reg. guide, not in a rule.

MR. NISSLEY: Mr. Chairman.

CHAIRMAN ARMIJO: Comment?

MR. NISSLEY: Yes. I'm Mitch Nissley. I'm with Westinghouse

Dr. Powers, I think that's a reasonable suggestion. I guess the one point I would like to make is that in terms of practical applications when you go into a licensing arena and you know what is expected and what is acceptable, you have a small risk. If you have a position that one feels is technically sound but has not yet been run through the mill, if you will, you have assumed a significant regulatory risk, and if you're trying to get a new alloy introduced into the marketplace that improves the performance in our plants, you may elect to take an unnecessarily conservative single factor rather than running the risk of delaying the introduction of your material.

So I just wanted to make a comment on the practical matter related to that.

DR. POWERS: It's true, but you see, this is an unbounded thing. We could have 15 feet worth of topical reports from Argonne describing tests, and I could still stand up and say we need additional data. It's an easy thing to say and --

PARTICIPANT: It's true.

DR. POWERS: -- it's true. It would be accurate. Nobody would criticize me. The question is: at what point do you say, okay, I'm not going to have your new alloy? I have no samples of it. In fact, I have a hard time getting samples of alloys that we do have, and it's okay. I don't have a hot cell to test them in anyway.

You know, at what point do you stop

collecting data and say, "Okay. Here's a method that's acceptable to the staff"?

Now, if you've got a specific thing that you want to look at in detail, you know, and it requires some modification of this F to be 1.12321 pi over 16 or something like that. Then they can do the in depth experimentation there, but otherwise it's unbounded. I mean, there are too many alloys or too many things to worry about and insufficient capacity to carry it out.

MR. NISSLEY: This is Mitch Nissley again.

I think what we should do is defer to a later slide that Mr. Montgomery unless you want to --

MR. MONTGOMERY: It's the next slide.

MR. NISSLEY: Okay.

MR. MONTGOMERY: No, it'll be just a minute. Sorry.

Mitch is going to point out that there's a second slide in here that talks a little bit more in detail about our concerns about the F factor, and how we think the data is still incomplete in terms of trying to define that.

The second concern is with regards to the double-sided observation away from the balloon region or fuel rods that have had a closed gap, that is, irradiating fuel at higher burn-up.

The major impact on that is that it will reduce the allowable transient time by about a factor of two, and there really doesn't appear to be a sufficient technical basis to make that at this point, based on the limited amount of data that Argonne has provided us to date.

We feel that we need additional post quench ductility data from interval LOCA experiment samples which have had fuel in them, which have had the possibility of ID oxygen uptake, and then we can then evaluate the consequences or the effects of the ID oxygen uptake.

Now, let's just talk a few minutes about --

CHAIRMAN ARMIJO: Robert, specifically what types of fuel rods that need to be tested that haven't been tested?

Now, we have integral tests on BWR rods.

MR. MONTGOMERY: Yes.

CHAIRMAN ARMIJO: Do we have integral tests on the PWR rods? Are those the ones that are closed gap that you're talking about?

MR. MONTGOMERY: Those would be more applicable because they had the bonding effect that was talked about this morning. The LOCA experiments on the BWR fuel, the bonding wasn't there, and so you didn't see the effect that strongly, and there were no recompression or post quench ductility tests done on those samples to quantify the effect.

So even though that process may have occurred in the Limerick samples in terms of some oxygen uptake, we don't have any mechanical property tests to tell us the consequences.

CHAIRMAN ARMIJO: But what you're calling for is for tests that were in the original plan that have just been delayed because of hot cell problems and things like that?

MR. MONTGOMERY: Yes.

CHAIRMAN ARMIJO: Okay. Not new tests that were never\ considered in the plan.

MR. MONTGOMERY: In the planning process we've talked about a number of different types of tests. We talked about ballooning, tests that were single sided oxidized which were balloon tests that were like the ones that Mike talked about this morning, the LOCA integral tests.

And we also discussed tests where they didn't pressurize them. They just ran them through an oxidation train. You have to look and see if the ID oxygen uptake effect would occur and the consequences of it. We talked about that in the 2005 meeting even.

CHAIRMAN ARMIJO: Okay.

MR. MONTGOMERY: So there is an appearance in the way the approach is being carried out here that -- let me find out where I'm at here -- there's an appearance of stockpiling of some conservatisms with regards to the embrittlement criteria that we're considering, the revision that we're considering.

First, there's some historical conservatisms already built in the existing criteria as defined, and those are the requirement to retain post accident or post quench ductility after the accident, some sort of ductility.

A body of data --

DR. POWERS: Why is that a conservatism?

MR. MONTGOMERY: Well, there's a body of data that's out there now that shows that the ECR limit to retain ductility is about half the detail needed to survive quench and also the post quench loads that occur, say, after the accident. So there's data out there to suggest that ECR limit based on ductility is conservative with regards to its strength, maintaining sufficient strength during the accident to survive the accident, any loads that would occur after the accident.

So that we feel that there's some conservatism in that particular requirement.

Secondly, defining ductility based on ring compression tests, those are very aggressive tests, especially empty ring tests. They're very aggressive tests in terms of mechanical property definition, and there is some conservatism built into this particular testing technique as well that enhances or enforces the ductility requirement, to define the ductility requirement.

So these are historical. We accept them. They're there, and already imbedded into the existing criteria.

Now, with the new proposed revisions, there are some new conservatisms being added in. First is that we're looking at oxidation of high burn-up fuel at 1,200 degrees C. High burn-up fuel by definition if you're going to have less reactivity work and as a consequence have less peaking factor. Its operation at 1,200 degrees C.; you won't see operation at 1,200 degrees C. during a LOCA accident.

So defining criteria at that temperature level adds some conservatism.

The second additional conservatism is the application of this F factor for hydrogen effects which high temperature quench, and I'll talk about that in a little bit more detail.

And, thirdly, the double-sided oxidation away from the balloon region is a conservatism. The data isn't -- strongly suggests that the oxygen uptake from the ID is as embrittling as the corrosion process, the high temperature corrosion process from the ID.

So treating that equally is a conservatism.

CHAIRMAN ARMIJO: But you don't deny that it happens, right? I mean, you see it's happening. The oxygen is being --

MR. MONTGOMERY: There is some oxygen uptake, yes, and I'll talk about that, but is it happening at the same rate and the same amount as the OD? Because in double-sided oxidation you're assuming they're the same. So that's the question.

So as I said already, the F factor is a function of the hydrogen content, the fine design, which is the alloy basically. The accident time-temperature history -- and that time-temperature history, what I mean by that is the heat up and cooling rates and the quench conditions.

The Argonne data showed that at quench temperatures below about 600 C. you do get an improvement in F factor, and there's also data that suggests slower cooling rates below one degrees C. per second or so, lower than what Argonne has used, also improved the F factors.

In addition to the uncertainty or the difficulty in using a single value based on the complicated function that comes in from the hydrogen effects and the temperature effects, there's also how does one define an F factor from prehydrided material. There has to be a conversion factor from hydrogen to corrosion to basically estimate the equivalent steady state ECR for a given hydrogen content, and in doing that, that becomes now dependent on the alloy hydrogen pickup fraction, the Pilling-Bedworth ratio for that particular alloy in terms of how the oxide rose. There is some dependency on that density, the outside density for different alloys, and also the oxidation uncertainty. We know there's some uncertainty with regard to the oxidation rate.

So the consequence if we look at the F factors that are available in the literature, most of these are from F factors that are derived from the data presented in the Argonne report. Some of these are also beta available in the literature from CEA primarily, the french data from experiments conducted in their labs.

And in doing that, I've used the same definition of F that was presented this morning by Dr. Billone, and that is that the F is defined as the unirradiated ECR limit, and that is basically the fresh cladding material, that 17 percent type number, then looking at a 20 percent type number, minus the ductile brittle transition ECR limit for the irradiated or prehydrided material, depending on which type of material we're talking about, divided by the steady state pre-trained in corrosion ECR, and that has been converted by -- for the irradiated samples, there are two irradiated samples on here. Those are the ones that Mike presented this morning, and that is the quenched sample and the slow cooled sample here, but the rest of these are all for prehydrided material. So in defining the ECR steady state for prehydrided material, there was an assumption made that that was used on what the pickup fraction was for that cladding alloy and what the Pilling-Bedworth ratio is, and that then goes into the calculation of F.

And so we have here plotted F factor

versus hydrogen content and some data here. This data includes cladding that is being cooled down and quenched at very high rates or rates faster than one degrees C. per second and quenching temperatures of 800 C. or samples that have been quenched directly from 1,200 C. In addition samples that have been cooled very slowly, less than one degrees C. per second either then quench or just allowed to cool all the way to room temperature, and we had different alloy types. Our basically wall thickness, these are all Zry-4, 17 by 17 and 15 by 15, and then as I pointed out, there's two samples on here that are from irradiated specimens.

And what we see is that if you put all of the data on here, you basically have a nice fruit salad. You have some apples, oranges, bananas, grapes. There's really no clear trend here in terms of behaviors, but you do see, as was pointed out, there is an effect of the quenching temperature at quench temperatures that are high and what we've kind of defined as unrealistic, you have higher F factors.

And then for slow cooling condition, this is all a group of data from the slow cooled condition, you have lower F factors.

CHAIRMAN ARMIJO: Is this al Argonne data that you're plotting or does that include some French data?

MR. MONTGOMERY: It does include some French data. The Argonne data or I should -- it's easier for me if I point out the French data. The French data are all of the purple symbols. Everything else is argonne data.

CHAIRMAN ARMIJO: The blues are also low. Are those Argonne data?

MR. MONTGOMERY: These are Argonne slow cooled, prehydrided specimens. Our one is slow cooled and one is quenched. No, they're both slow cooled, both slow cooled.

Now, you see some error bars on there.

DR. POWERS: Yes.

MR. MONTGOMERY: The error bars in terms of the hydrogen content, that's what was the range of the measurements that were quoted for the different samples, and if you see for the CA data they want -- I was not provided any variation in the hydrogen content. So I did not put an error bar on there for that.

The variation in F is an estimate. They're not one standard deviation or anything, but they're an estimate based on what uncertainty I have in either the steady state ECR value for the prehydrided specimens or the measured transient ECR. There's some uncertainty. You saw Mike show this morning that he measured the pretransient or the transient threshold by about half a percent, .5 or five percent plus or minus a half of a percent.

So that goes into this variation here in terms of the variation in the F factor. It's just to give you a rough idea of how these can vary for any one point. It's not a hard and fast one sigma variation at this point. It's just estimated based on what information was available in terms of the uncertainty for the different measurements or the uncertainty in estimating the pretransient corrosion.

CHAIRMAN ARMIJO: Just to make sure I understand that chart, you say it's tested at unrealistic quench temperature. You really mean after an unrealistic quench temperature, but it was tested at 135, and everybody agrees that that is a good test temperature.

MR. MONTGOMERY: That's correct, Sam. That's correct, Dr. Armijo. That is it was quenched at an unrealistic quench temperature.

CHAIRMAN ARMIJO: Right, and so we have Argonne data and French data on both sides of the curve. So there's general agreement that the cooling rate is very important or the quenching temperature is very important.

MR. MONTGOMERY: Yes. Quenching temperature and cooling rate has an effect, and you can see it appears to be somewhat important.

DR. POWERS: Of course, I don't understand that at all. First of all, I don't know what a realistic quench temperature is.

CHAIRMAN ARMIJO: Well, that was where I'm getting to. Which one can you count on? I mean, do you know enough bout the LOCA event that you can say I'm really sure that for this plant, approximately quench temperatures and cooling rates are down here or up there. It's a system issue.

MR. MONTGOMERY: Yes, and there will be a presentation after me that will talk a bit about the quench temperature assessment and what would be a more realistic quench temperature versus what we see in the data here.

DR. POWERS: Well, I thought earlier we had seen a discussion that said it really doesn't matter whether I let the furnace cool down to 800, 700 or 600. I get kind of the same number. It's only when I let it drift down for very long times that I get any difference at all.

MR. MONTGOMERY: I heard that as well. This is the database though. So I think the point here is that in a very complicated function of these various test variables and the amount of data we heard this morning suggests compared with this data, it suggests that we need more evaluation and understanding of the data before we make any global statements about what the dependencies are.

DR. POWERS: Well, the next thing that I heard is, yes, everything you've said, you've brought it to your Zry-4 data and all of this is true. But the fact of the matter is that the modern alloys all have a very big term, if multiplied by the 1.4. So why do I care? C times F.

MR. MONTGOMERY: F times -- yeah. Yes.

DR. POWERS: If the C is small, why do --

MR. MONTGOMERY: As long as we're looking at -- as long as we look at this, and F is a very complicated function, it's hard to determine which one of these goes down faster. You see?

DR. POWERS: No, that --

MR. MONTGOMERY: Which one of these is going down quicker. Is the bottom, the denominator or the numerator going -- decreasing to --

DR. POWERS: Ralph is going to tell me 1.2. Mike is going to tell me it's 1.3, and I think what I've learned is that if I have a decent alloy I want to introduce, I don't care. That's one of them. I'll take whichever one will make them happy because what I multiply them by is ten microns. I don't care.

I care if it's 100 microns. Then I get really agitated over whether it's 1.2 or 1.3.

CHAIRMAN ARMIJO: You wouldn't want that alloy anyway.

MR. MONTGOMERY: But if we looked at an alloy that has a different hydrogen pickup rate.

DR. POWERS: You haven't told me anything to do about hydrogen pickup, right? They told me to use F times how much corrosion I get during the --

MR. MONTGOMERY: Well, as long as F is 1.3, then I think, yeah, the debate is a difficult one. I mean, if we're down here --

CHAIRMAN ARMIJO: You're arguing about units.

MR. MONTGOMERY: Yes, but the question is is the database sufficient to really say that F is down here or could we end up in this area. That's the question that we still haven't answered and that we're looking to try to answer.

DR. POWERS: The regulator is happy with 1.2. I don't have a very big, normal operation corrosion problem. Why am I not happy with 1.2? If a regulatory is happy with two and I have low amount of corrosion during normal operation, I'm still happy. I mean, if I'm trying to sell Zircaloy-4 in the marketplace, I may have a little more heartburn here, but if I'm selling M5, I'm real happy and if it's M-7.232, I probably am just ecstatic. They can pick ten if they want to.

MR. MONTGOMERY: Okay. Well, one of the things that I should point out is that coming into this meeting this morning F was 1.5 and not 1.2. So we're learning a little bit here today also.

CHAIRMAN ARMIJO: Well, keep negotiating because it sounded like we're arguing over something that doesn't mean much. That factor may be technically something that should be in there, but it has no impact on the development of a material or the qualification of --

MR. MONTGOMERY: It depends, I think, partly on the procedure that's defined. If it's defined that F is going to be 1.2 or is it a recipe that gets defined?. I think that's part of the issue.

CHAIRMAN ARMIJO: Could be, could be.

MR. NISSLEY: This is Mitch Nissley from Westinghouse.

The second presentation after this I'll be giving is focused on the practical implications applying this method in the industry today, and I think you will see that there are some rather substantial consequences. Even at the end of the day there's no real impact on safety margins, but there are practical implications that are quite costly.

CHAIRMAN ARMIJO: Okay. Well, let's go on and we'll hear that later.

MR. MONTGOMERY: All right. So changing gears in terms of the F factor, now I want to talk a few minutes about the double-sided oxidation requirement. We feel that this exaggerates the whole of the ID oxygen uptake. First let's just talk about the fact that we're going to assume the weight gain is the same on both the ID and the OD.

Without an ID oxide formation, basically really the ID oxide dominates the weight gain. It's about 85 percent of the weight gain is from the formation of the oxide, and we're not forming an oxide on the ID. We're just absorbing oxygen into the metal from an alpha layer, a preexisting ID oxide layer that's forming an alpha layer.

The question then becomes is there sufficient amount of ID oxide layer to warrant a double-sided consideration throughout the entire transient. We saw data. Mike showed some data this morning that said that there is a limited oxygen source out there, and it could stop creating or putting oxygen in the system as the ID oxide is absorbed and then that alpha layer could actually go away after some period of time.

To have additional oxygen coming from the fuel, there will have to be fuel cladding contact, and that's data that's been presented and talked about in the Argonne report, basically looked at from the late '70s and early '80s. There is some data created that suggests tat, and the MAP crews actually has a correlation that tries to account for the oxygen uptake into the cladding from the reaction of the uranium and zirconium alloys.

The data that's been presented so far is not really clear on what conditions we can expect during a LOCA. The information from the three LOCA integral tests on the Limerick fuel don't show a clear indication that this is actually happening and that would warrant using double-sided oxidation requirement.

CHAIRMAN ARMIJO: But I don't understand that. The only time you're going to have double-sided oxidation is if you have a burst of a balloon, right?

MR. MONTGOMERY: No. That's not what has been talked about this morning.

MR. SIEBER: That's not what he said.

CHAIRMAN ARMIJO: That's not what you said.

MR. MONTGOMERY: That's right.

CHAIRMAN ARMIJO: So your issue is if --

MR. MONTGOMERY: Say I'm one meter away from the balloon region.

CHAIRMAN ARMIJO: Okay.

MR. MONTGOMERY: Which is where the peak cladding temperature more than likely is going to be in this event now because of the cooling effect of the balloon. With that position the proposal on the table is to use double-sided oxidation at that point, considering that the ID oxygen source is the ID oxide pretransient corrosion that's there from the bond layer.

CHAIRMAN ARMIJO: And your argument if the gap is closed, the only source of oxygen is either this bond region or the fuel, but there will always be a an oxide on the ID.

MR. MONTGOMERY: Yes.

CHAIRMAN ARMIJO: It's a very thin oxide.

MR. MONTGOMERY: But the very thin oxide. So does that warrant double-sided, considering double-sided oxidation throughout the entire accident, which is what we're talking about, the proposal this morning.

CHAIRMAN ARMIJO: Okay. I understand.

MR. MONTGOMERY: Okay. I would just point out that during a LOCA event it's more than likely that there's a gap between the fuel and the cladding because the thermal contraction of appellate, the cladding also retracts away because the pressure has dropped, you know, on the outside. So there could be a gap open. So considering that the fuel is a source of oxygen, may not be appropriate because there is a gap there and it's not in intimate contact with the cladding necessarily.

That's why we feel that as the next slide has, we feel that currently the results from the integral tests that have been conducted to date are inconclusive and that we need some additional data from integral tests where fuel and cladding are in the same sample together, to give some indication, to give some information on the possibility of this effect.

So like I said, the results from the current data set that's been provided from the Argonne program is inconclusive. There doesn't appear to be a clear indication that inner surface oxygen uptake in the balloon region is going on sufficiently to warrant a double-sided oxidation requirement.

There may be some data that we can get out of the NSR irradiated test. We should go and try to look for that, and currently there's been no test, no ring compression test or post quench ductility test on these high burn-up LOCA integral specimens to investigate what could be happening to the post quench ductility due to the effect of ID oxygen uptake.

DR. POWERS: I understand the ID oxygen uptake. The concept as I understood it, and I have to admit I kind of glanced and glossed over that whole thing, is that we have got a hyperstoichiometric fuel with some saturated -- with a molybdenum oxide equilibrium going on, and that if we get hot enough on the order of 1,000 degrees Centigrade, that we will get certain partial pressure of oxygen in there, and that oxygen will react with the inside of the cladding. Am I correct in that?

CHAIRMAN ARMIJO: No, I don't think that's what I heard. I heard that all of the oxygen of interest was coming from the bonded layer, and that was more than enough oxygen.

DR. POWERS: Sam, you've got to create the bond.

CHAIRMAN ARMIJO: Well, that happened during normal irradiation and high burn-up.

DR. BILLONE: Normal irradiation, and Dana is correct on that. During normal irradiation you're burning up uranium. So your oxygen to metal ratio is increasing. Even if you had stoichiometric UO_2 and pressed it against zirconium metal, zirconium has such a high affinity for oxygen. It will take some of that oxygen away, but in terms of normal operation and how the bond forms, it's depending on linear heat rating and it's dependent on burn-up as to when you get a complete bond, but the mechanism is --

DR. POWERS: Whether or not we get some oxygen in there.

CHAIRMAN ARMIJO: You get oxygen. There's no question.

DR. BILLONE: Just one comment on Robbie's remarks. What hasn't been demonstrated or what we have seen so far in our local integral test is the bond is very strongly attached to the cladding. It moves out with the cladding, and it takes some of the fuel with it.

Now, how much fuel does it take? It's hard to quantify. It could be -- we've seen chunks of fuel attached to the bond, and we've seen particles of fuel. So it's difficult to quantify, and I think Robbie and I could agree that assuming double-sided oxidation would bound that effect, it would be conservative. The point is it's too conservative.

MR. MONTGOMERY: That's right.

DR. BILLONE: But we don't have it quantified.

CHAIRMAN ARMIJO: But we all know at least --

MR. MONTGOMERY: We need some data to justify it.

DR. MEYER:

While you're on this subject let me point out that the fuel particles that Mike talked about being stuck to the bonding layer have all been etched away before these tests were run. So if these tests show some limitation of the oxygen supply, that doesn't necessarily say that if the fuel particles were still present that you would have such a limit.

I've seen four sources of data that are

very relevant to this and in all cases when you have bonding, you see an alpha layer, an oxygen stabilized alpha layer that is roughly the same size on the OD as the ID, and I think that's clear evidence that it's the same and not some conservative bound.

MR. MONTGOMERY: I don't think we disagreed that you need fuel in NO_2, zircaloy contact to have oxygen uptake. That's not the argument here. The question is: is there sufficient amount of contact during a LOCA event where the fuel is retreating from the cladding and the cladding is moving away from the fuel to support a double-sided oxidation requirement throughout the whole event. Is there sufficient oxygen coming from these sources to support that?

If the oxygen is less, significantly less, then this would be bounded. If it's not the data suggests that it's not a -- if there's sufficient oxygen, then the data does suggest that double-sided oxidation is reasonable.

CHAIRMAN ARMIJO: I guess I heard Mr. Billone say that the oxygen source was the bonded material that stays with the cladding and is attached to the cladding. He wasn't claiming oxygen coming from any other source.

MR. MONTGOMERY: But he hasn't run those tests yet. So we don't know the answer.

CHAIRMAN ARMIJO: He has run one, one test, two tests.

DR. BILLONE: No. Most of the data I showed you are from cladding samples that have the bond where the fuel was etched out. So it wasn't even present. We've run three tests, LOCA interval tests with the fuel in it, and we wanted to run a fourth. It was proposed at the last meeting, I guess, the same day the hot cells got shut down while I was sitting here, but that test will be run, and that's basically the worst case is to take a one foot long -- excuse the units -- sample filled with fuel and weld the ends and expose it to steam oxidation, and then do ring compression and metallography and micro hardness to see how much oxygen you pulled in from the ID.

It can be done. We can also use the samples we've already generated to do that from Limerick. It would have to be done at Oak Ridge.

MR. MONTGOMERY: You can do that from the Limerick samples?

DR. BILLONE: Yes. It's just that we can't cut them right now in our hot cells, but they can be cut at Oak Ridge, and we can take your proposal and actually do that.

CHAIRMAN ARMIJO: Well, I haven't looked at a lot of PWR fuel, but I have looked at a lot of BWR fuel, and the bonding is definitely there. The ID oxide is definitely there. The bonding is highly variable at least in the BWR. So it's kind of hard to say 100 percent of that would clearly be conservative, but I don't think it's necessarily accurate.

DR. BILLONE: No, for the Limerick BWR fuel we tested it was 57 gigawatt-days per metric ton, and the bonding was not -- there were gaps, areas in which the fuel cladding gap so that you could run the circumference n other areas of complete bonding.

CHAIRMAN ARMIJO: Right.

DR. BILLONE: And that's primarily due to the lower system pressure. You're not squeezing the cladding of the fuels fast.

MR. MONTGOMERY: And in those samples, there wasn't a strong and there wasn't really an alpha layer at all on the ID of those samples.

DR. BILLONE: No, because they were run for 300 seconds, and by that time you've gone past that point.

MR. MONTGOMERY: And basically indicating that there was a limited amount of oxygen on the ID, and if we had assumed double-sided oxidation in that case, you would have well over estimated the amount of oxygen that would have been absorbed in that sample.

DR. MEYER: Not necessarily, because there were fuel particles attached to the bonding layer before he etched them off of there.

MR. MONTGOMERY: Well, the alpha layer was gone though. So that meant the --

DR. MEYER: Because he started out with a limited --

MR. MONTGOMERY: The oxygen source stopped.

DR. BILLONE: The integral tests, we didn't etch any fuel. So we've got to separate the discussion.

CHAIRMAN ARMIJO: Okay. These are the issues.

MR. MONTGOMERY: Yeah. Okay. So I'm just about near the end here. Our overall concerns are kind of a lack of data, as you've kind of got the picture, and also we feel that there has been a lack of opportunity for the industry and the public to really review and evaluate the data. We feel that we need more time to review and understand the Argonne test data and their evaluations of that data in relationship with other data and experience that we have out there available.

And I've pointed out already that we feel that there's some additional need for tests and data acquisition. Data acquisition would be things like oxygen distribution measurements or how to do distribution measurements to try to help us answer some of these key technical questions that we talked about today.

In addition, there does appear to be an appearance of a rush to rulemaking without cause on this issue. We don't see that there's any safety concern here with our current evaluation methods that are used in analyzing the LOCA, as well as in the design basis of existing operating plants.

We feel that there's a possibility here that we could impose by going forward with the current technical basis, we could impose criteria we've had with excessive conservatisms that end up becoming costly to implement on the industry. That's kind of our overall concerns.

I've indicated that we would like to see additional data. Here's some examples of the type of data that we think is needed to help us move forward. Again, we're talking about additional recompression tests to help us understand this F factor or whatever other variables we might need to use if that is not the appropriate one on all the different alloys.

Looking at quench temperatures, including relevant quench temperatures, cooling rates, and hydrogen content in the range of irradiated fuel.

In addition, we'd like to see some integral LOCA tests on PWR cladding. We have some data on BWR claddings, but we don't have any on PWR claddings to confirm the overall LOCA fuel behavior for that type of fuel, and from these, we also feel that we can use these types of tests, also include looking for the effects of the double-sided oxidation and try to assess the consequences of using or requiring double-sided oxidation away from the ballooned area.

Just to give a feel for what else other people are going out there, here is kind of a time line schedule of additional data that have become available, and I won't talk much about the top line because that's really the Argonne program continuation, but we also have the integral LOCA tests that can go on in Oak Ridge, but they're probably not going to start until about 2008.

Is that still current, Mike?

DR. BILLONE: 2007.

MR. MONTGOMERY: Okay. So the last quarter of 2007.

We also have data coming from the Halden program. There's a LOCA program going on in Halden looking at the effects of high burn-up fuel behavior during a simulated LOCA event. There will be an additional test later this year and a second test in the end of 2008 that should provide more information for us to understand these different phenomena.

In addition, there will be some LOCA quench tests in the Japanese program on advanced alloys, the M5 and ZIRLO. Those will be starting in the middle of this year and running for about two years where we'll be getting some data on the LOCA behavior of the quench and the quench behavior of advanced alloys.

And finally, the CA program has separate effects tests going on looking at post quench ductility as well as quench behavior of advanced alloys and Zry-4 looking at the effects of quench, quench temperature, cooling rates. That will also become available in the next two years.

CHAIRMAN ARMIJO: Are the data from these tests available to the NRC, all of them, or you know, if somebody holds them proprietary they're not going to do --

DR. BILLONE: Of free lines, yes.

MR. MONTGOMERY: I think everything but the bottom line, and the bottom line, some of that would become available. I can't speak to the CEA for sure, but the rest of that would all be available.

CHAIRMAN ARMIJO: Who's paying for the Halden work? Is that --

MR. MONTGOMERY: The Halden group.

DR. POWERS: U.S. is a partner in Halden.

CHAIRMAN ARMIJO: U.S. is. Yeah, I know that.

MR. MONTGOMERY: The Halden program.

CHAIRMAN ARMIJO: So NRC gets that.

MR. MONTGOMERY: yes.

DR. POWERS: What's remarkable is I don't see anything from the U.S. industry on this.

CHAIRMAN ARMIJO: Well, there's some U.S. people who are members of the Halden program, some manufacturers in the U.S.

MR. MONTGOMERY: Yes. We're providing the fuel for the top two lines. So there is contribution to the Argonne program. The M5 and ZIRLO is U.S. industry fuel. The Oak Ridge test will be performed on fuel that the U.S. industry provided, and as Dr. Armijo said, the industry does participate in the Halden program. EPRI is a member, as well as the fuel vendors.

The NSR, the Japanese test, I can't speak to you right now, the source of the M5 and the ZIRLO. Some of that could be provided from the U.S., and the CA data is being sponsored by the AREVA as well as EDF.

MR. NISSLEY: This is Mitch Nissley.

Westinghouse has provided some additional data on ZIRLO performance in the 800 to 950 degrees C. range to the NRC this week at their request. This was some data we already had available. We've provided that informally.

We're also planning to conduct additional testing here in the coming months. As you'll see, industry would really like to work with the NRC to reach a consensus on the additional data needs, and Westinghouse does plan to perform additional testing and supply that data to the NRC.

MR. MONTGOMERY: My last slide here is our conclusion is based on what we've heard this morning, that there's really more work that's needed to support a possible revision of the embrittlement criteria.

It's a bit maybe premature to be thinking about exactly how to make that revision at this time. The technical basis is not complete yet.

Finally, we feel that in support of this we would like to suggest a series of stakeholder workshops where the industry would come ready to talk in more detail about the types of tests we feel are necessary and needed to help move forward in this area, and so we're willing to talk about that at a future date.

DR. POWERS: What I still don't quite understand -- does the industry want to come forward and discuss how a reg. guide should be written or how a regulation should be written?

MR. MONTGOMERY: I think we're ready to do both.

DR. POWERS: I mean, I think I can write a regulation right now, and I guarantee you I don't need anymore data to write a regulation.

CHAIRMAN ARMIJO: But that regulation probably wouldn't include that level of detail on the F factors and things like that.

DR. POWERS: Of course not.

CHAIRMAN ARMIJO: It would be a general statement.

DR. POWERS: It would be a statement of what I consider safe, okay, as part of defense in depth. Now, you see the NRC has the option of saying, "Okay. Here's our safety criterion. You meet it. And, by the way, here's a method that's acceptable to us to meet it."

You're free to use that if you want to or do it any way you want to.

MR. MONTGOMERY: Well, our efforts here are to try to insure that as a safety criteria that first it meets the requirements of protecting the health and public safety, but then second is one that is also meetable, that can be met without too much burden.

CHAIRMAN ARMIJO: Well, this is anticipated burden. We haven't heard what the actual burden would be, and I guess AREVA or Westinghouse --

MR. MONTGOMERY: I believe there is some discussion about the burden coming up after I speak.

CHAIRMAN ARMIJO: Okay. Any further questions?

DR. POWERS: Well, I mean, I think it's somewhat important for people to understand we're on a pathway to revise 50.46. this is one part of it. I can almost assure you that there is absolutely no interest in going through the pain twice.

And so there's some reason to get on with revising the regulation. The reg. guides, you know, they percolate around and get changed regularly. They're much easier to change. That overstates the ease with which they can -- they are easier to change than regulations. They are not easy to change.

And so when you put up 2009 before we get data, 2011 before we understand that data, those are essentially saying, okay, this part of the regulation is going to remain an anachronism while all of the rest of it changes, and that's probably a fairly unpalatable thing right now.

CHAIRMAN ARMIJO: Well, you know, if it's meeting or if its doing its job, the existing regulation is doing its job right now or else we should be doing something else.

DR. POWERS: It's not.

CHAIRMAN ARMIJO: Well, the question is the fuel out there in an unsafe -- potentially likely to fail in an unanticipated way? I don't think so.

DR. POWERS: The problem is, Sam, that the regulation is written for zircaloy and ZIRLO and people want to use other alloys, and so they have to come in for an exemption request.

CHAIRMAN ARMIJO: That's a price they pay in lieu of --

DR. POWERS: It's a price the agency also pays because they have to take manpower aside and look at the thing and say, "Yeah, here we've done all of this stuff," and whatnot.

MR. MONTGOMERY: But there would also be a price to pay both in the industry and within the agency with regards to criteria if we end up with criteria that are overly burdensome in some fashion.

CHAIRMAN ARMIJO: Okay. Well, we've heard those views. Let's move on to the next speaker.

MR. MONTGOMERY: Thank you.

CHAIRMAN ARMIJO: It will be Mr. Dunn, Bert Dunn of AREVA.

MR. DUNN: Okay. My name is (pause) --

DR. POWERS: You need the viewgraphs to figure that out, huh.

MR. DUNN: Okay. My name is Bert Dunn, representing AREVA.

I have a presentation that was going to talk about the expected transient of a LOCA, the variation in the famous F factor that we've got here now, et cetera. There's been a couple of questions come up that I'd like to offer some of my insights into.

And I guess the first one would be on rule versus reg. guide. In this area, we will need both at about the same time. If you wrote a rule that just said you need to maintain post LOCA ductility -- that was the 50.46 rule -- you would then need some time for implementation, and you would have to have fairly quickly a reg. guide that would be this is what we want to do.

Now, it might be a little bit easier to do that, but I don't know that we gain too much by doing that. The reason we have 50.46 is that we were all getting hauled all over the place in the early '70s talking about this on this plant, this plant, this plant, this plant, this plant, and so it does serve the utility and the vendor and the people trying to get here to have fixed numbers that you can count on. The reg. guide is a little less fixed than the rule.

But by the same token you don't really complicated things within the rule. So that's that.

Someone said earlier today that the lack of having a generic cladding within the rule has not blocked any plants from going forward with a preferable or a better cladding by one of the advanced claddings. That's not really true. The utilities, at least one that I can tell for sure, and I believe there's two, have made the decision not to pursue the advanced cladding until such time as they're not at risk for having to go in with an exemption.

What the utility doesn't like about an exemption is that an exemption opens it -- when you get an exemption, you wind up open to additional regulatory activity that you hadn't planned on. So it's a risk event.

Some utilities view it as a high risk and utilities view it as a relatively low risk. What we've been able to get to move to M5 cladding, for example -- that is the cladding I'm talking about -- are utilities where that has been relatively important for them to get the low corrosion rate. We may see a few more come on in a couple of years.

We would like to have everybody on M5 at this particular time.

A test procedure for doing the break-away oxidation. It's been a long time since I've worried about how much it costs to build a heater rod, but one consideration could be something like a small break LOCA test or a boil-down test or perhaps a level of water already established. You may have here it could be Hoprider's rig at Penn State. You would get all of your information at one time because if you do a boil-down test, every portion of that rod is going to be hotter and hotter and hotter as you go up. There could be some set-up difficulties, transient difficulties that might do that, but you know, so I think those can do.

AREVA is doing very well with testing with prehydrided cladding, et cetera, like that. Now, we've got a substantial program to deal with M5 in that fashion, and while I'm on that subject, I did want to introduce or there have been questions about cooperation and stuff like that. We've had two or three meetings with Mike. We've brought the people from the CEA program on the cladding to Argonne. We've sat down. We've spent an informal day.

The purpose of those meetings was to give Mike the information that we had so that he could learn what he needed for his program, so that he could then ask us specifically for it, and then we would go forward and see whether or not or try to get that released, and we have provided a lot of information to the program.

So I think it's an international company. It's not the U.S. -- we're not owned in the U.S., I guess, and it's certainly not the Lynchburg office that's providing most of the source of information, but AREVA has been providing a great deal of information, and we will continue to do that.

It may surprise things, but AREVA wants the rule to be right and the reg. guide, too. Some people don't give vendors credit for that. Some people just say vendors want to keep things cheap.

Okay. Finally, the way I measure things in terms of are we there yet is to look at a learning curve, and I ask myself how frequently am I learning something? If I'm learning something new, I'm not sure I want to stop the process and say I've done it all. Okay?

Two years ago I stood up here and said we're still learning some things that are new, and we need to slow down a little bit and make sure we get the tests.

Okay. Well, here we are a year and a half later. There's two new items that we didn't know about or weren't considering two years ago. That's the ones Robbie mentioned, the ID source of oxygen migration into the prior beta region and the potential effect of quench temperature or LOCA transient on the thing.

So knowing what I want to say, don't write a rule, but if you do start writing the rule now or if you do start going on this process, let's go slow so that we have an opportunity to figure out whether we've got all of the phenomena we need to toss in there or maybe we take Dr. Power's recommendation: write the rule very general and give ourselves a lot of time to finish a reg. guide and let the reg. guide develop more slowly. That may be better.

Now I'll stop with the general stuff and go on. What I want to do is talk about making a rule. If we pursue it now, and this is colored by the fact that we're afraid that the one F factor will get picked. Now I'll show you a little bit about that.

We're afraid that it will be too specific and not general enough. It would rely on, I think, immature data. Some of this information has just hit. It hasn't been out there for peers to talk about it a lot. We haven't fought over it, you know, and you know, we need to do that. Most likely it would embody excessive conservatism.

Now, obviously there I just say retain a little bit of ductility. So I'm talking here about reg. guide or rule.

Okay. The value of this normalization for the existing corrosion or for the hydrogen, one thing.

As mentioned in Ralph's equation, that ECR corrosion is today a surrogate for hydrogen. Okay? There can be a few other effects in there, but the best we would understand it right now is that it's just serving for hydrogen.

I want you to know that because I'm going to make a request later on that will differ from Dr. Meyer.

F is going to vary with the importance of hydrogen to the material. Cooling rate, quench process, possibly. We have Argonne and we have CEA getting different results. We don't know exactly why. And the phase change kinetics. We're talking about transients that take 100, 200, 300, 400 seconds. The last time I looked at phase change kinetics, it's on the time frame of ten to maybe as much as 100 seconds.

DR. POWERS: Maybe even longer.

MR. DUNN: Yeah. So we're messing around with what the beta fraction is in the cladding at the same time we're doing this. We ties all three of those. I'm not sure we understand that well enough yet.

Okay. There is the proposal, and I'm going to talk about some experimental results that I did. That's going to be on the quench, not that I did. A survey that I did. That's going to be on the cool down and quench rates.

And then I've done some calculations of some F factors to show you what the variation might be.

Okay. Mike presented this slide. I'm not going to go through it again in too much detail, but we have very rapid or 800 degree quench data, and then in this area we have stuff that was slow cooled down to 700 degrees and then quenched, and we have some stuff up here that was slow cooled down to 600 degrees and then quenched, and then finally we got a couple of data points out here where it was slow cooled all the way down to room temperature.

Now, if you take this data and you draw some reasonable lines through there with a French curve or the same type of line that Mike was drawing, you can kind of compute what the F factor might be for, and I did that.

Okay. From the CEA data, if we do 1,200 or 800 degree quench, we would come up with an F factor of 1.6. Now, that's been tossed around fairly effectively this morning. I think most people agree with that.

If we do a room temperature quench, we

come up with a factor of .9. We do a six or 700 or 500 degree quench, we're not exactly sure where the lower bound is going to be, but it would be .7. So there's quite a variation from just the EA data.

Now, if we do Mike's stuff, you won't get that much variation. Okay? CEA cools at less than a degree per second on the way down to the quench temperature. Mike cools at 13.

In a few minutes I'll show you my recommendation, which is six. Now, six gets us in that time frame where we can start talking about the kinetics of the base range becoming important.

Someone asked about the material. If I take M5 as we know it today based on the Argonne data, apply a hydrogen correction to that that's a linear interpolation of what Zry-4 does, but just use the M5 hydrogen content, which will give you an idea. We have never measured hydrogen over 80 ppm in M5 from operations. The F factor would be .7 for that material. So it can be a fairly strong dependence, material dependence on this type of a factor.

Okay. It's made up of the reduced corrosion. M5 does have corrosion. We typically would measure maybe 20 or 25 microns of corrosion.

DR. SHACK: But isn't that directly

accounted for in the ECR corrosion term?

MR. DUNN: No. I'm getting there. We get low corrosion, but then if we did that, if we just took the corrosion and compared that to Zry-4, for example, the two corrosions, we would get one fourth, one third what Zry-4 develops, which should put us in the 200 to 300 ppm range.

Our pickup fraction is about one-third of what Zry-4 is. That's what gets us under M2 about 80. All right. And here I'll say if we can design a clad that has a pickup fraction of one-third of Zry-4 and do that for a certain reason, we can also design a clad that has a pickup fraction of twice what Zry-4 had.

Now, that's one of the things that I want to do here is make sure that we get the flavor that there are some changes around in here, and we are going to have to be testing, if you will, to decide which way those go because of reg. guide, the rule or what have you.

Finally, just again, the main point here is that it's all over the place. This is Mike's public H.B. Robinson number or one of the H.B. Robinson numbers.

DR. POWERS: I think his number as I

recall was 1.3 plus or minus .3, right?

DR. BILLONE: Yes. The plus is a plus 1.3, which would be Baker data as it is. I'm sorry.

The 1.6 factor can come out of the calculation for our heating and cooling rates, and then went on to argue that if the heating rate were slower, you can go from 1.6 to 1.4 to 1.3 to one.

But, yeah, taking our data as is without using any rationalization, the number is 1.5 to 1.6.

MR. DUNN: Well, we asked the question what is the appropriate quench temperature. Okay? This question first got raised, I think, last summer at Argonne when we first saw the CEA data and people were saying, "Well, we need to know what the quench temperature is. Tell us what the cooling rate and the quench temperature is."

Well, the cooling rate is anywhere from zero to almost infinity, depending on what part of the transient you're talking about. As we start to turn down on any LOCA, it's cooling at zero. We've just matched the ability to keep up with the decay heat, and it gradually increases as the plant refloods or refills with water, and you have this type of a culture in general.

If we look at the calculations, they'll

vary a little bit over the place. If I go look at loft, it will jump around and vary and then we have our quench here, but we can make some argument by maybe looking at that whole process or looking at what the rate is down in this area.

It changes with the type of transient you're looking at. Whether you're looking at a large break or a small break. It changes with whether I'm trying to calculate things. This is a problem for maybe the analysts as to whether it's a best estimate work or deterministic work and what I might force to consider in doing it.

So I've got two large breaks up there. One is a plant that cools relatively early and quickly. That might be representative of VMW decision plant with vent valves in place. So vent valves enhance the flooding rate, and we get a lot of water in there really quick.

The verpa (phonetic) light line would be perhaps representative of a four loop plant. A standard Westinghouse four loop or a combustion plant or something like that, and it's going to take a little bit longer to get there.

I worked all of this in a -- and then the yellow in here, the small break -- I worked all of this into a clad, quench and cooling rate predictor, and it is not just based on prediction. It's a survey of experiments, and I'll get that to the source data in just a minute.

I suggested a cooling rate of six degrees C. per second, with the highest quench temperature of 600 degrees C. and the shortest cool down time being at 200 seconds. And the thing is I've got an equation that goes with this that you can plug these numbers into and you can actually see the transient, see the caricature of the transient. It's a caricature.

For the large break LOCA, small break LOCA we give 250 degrees C. for the quench, and here we're about 1,000 seconds. Now, that 250 degrees C. is nominally the range of saturation temperatures that we see because what happens with the small break is a mixture level is in the reactor, and it's just gradually rising, and so you're going to go from just a little bit. You're going to build up some steam cooling, and then where you're really going to hit the temperature drop is as you get very close to that quench frame -- I mean the mixture, mixture height.

Now, this is what I looked at when I looked at NUREG 1230, which is a compendium of data and testing and supported best estimate LOCA. That included FLECHT, FEBA, SCTF and CCTF and deterministic and best estimate LOCA predictions. It also supported, the work was supported by a paper from Kansas State from -- is Steve still here? I guess he has left. Steve Bajorek and Mr. Peterson.

There, in that paper, they were talking about quench. We were talkinga bout material differences and corrosion differences, and I allowed for that.

Now, you get about potentially a 100 degrees effect in going from stainless steel to Zry-4, and I actually wasn't able to observe quench temperatures above 500 degrees C., but most of those tests are done not with a zirconium based alloy.

I also had some work from Penn State that helped in looking at that.

Just a couple of real quick -- this is FLECHT, three different test temperatures, and you can see everything is down below the -- well, this one does have a couple at 600 there, but most of these are down in that 500 range. So I guess I lied to you a few minutes ago when I said I never observed anything was over 500.

DR. POWERS: Have you looked at all of the quench experiments that have been underway at

Karlsruhe?

MR. DUNN: AT where?

DR. POWERS: At Karlsruhe.

MR. DUNN: No, I did not look at those.

I want to add one more consideration here, and a great deal of these experiments -- I think everything I looked at or a lot of it does not have the appropriate amount of stored energy inside the heater element. So it's quenching quicker than it should.

If we actually did nuclear pellets there, we would get a lower temperature quench.

And here's, again, some stuff. This is CCTF. Now we can see the quench temperature running along in here. Now, in this case these quench temperatures up here at 700 or so, that's actually K. So that's really down at about 450.

But I just wanted to show you that I looked at a fair number of things. Okay, and now it's time to get off the podium.

The conclusions are that F can be a complicated item. It is going to vary with alloy. It's going to vary with hydrogen, and I don't think we understand how it interacts relative to the cooling rate. We should resolve the difference in cooling effect between CEA and Argonne, and I think we're trying to do that. There's work going forward to look at that, but it's not there yet.

The adequacy of corrosion to correlate the hydrogen effect on ductility should be confirmed. This is where I'll differ with Dr. Meyer. We should do a prehydrided testing to confirm that, in fact, for this particular alloy, the corrosion is the appropriate surrogate. We shouldn't just assume one number for F times the corrosion because there's differences in pickup fractions. There may be differences in how the hydrogen interacts with the material.

ID action and uptake, I just think that's a little bit early. Ralph said four tests. I don't know where those four are. I had thought this morning that there was one test. Well, I thought two days ago there was one test, and yesterday we were talking about there's apparently a couple others where we think the interior alpha layer went away.

I think we ought to make that a little bit more robust, and we ought to answer this question of is it just 1,100 degrees Fahrenheit or more. Okay? There's a paper somewhere that say sit can't happen for cladding temperatures below 1,100 degrees.

Okay. The amount of conservatism to achieve one number suitably bounded may very well be excessive, and Mitch is going to show us a bit more on that.

And I guess I'm just making a plea for a little bit more data, a little bit more time to let thoughts mature, let people bounce things back and forth between their ears and get these things, until the learning curve starts to go down a little bit.

Thank you very much for your time.

CHAIRMAN ARMIJO: Okay.

DR. POWERS: You caution prudence and aging these things in a cask is one of the things that I think ought to be done a lot with these data. So I can't argue with you. The problem I keep coming back to is you've got a fudge factor called F here, which is the function of a lot of things that we think we know and presumably another raft of things.

I mean, in a previous life I used to worry about the tactic phase transformations of these obscure things that exhibit hysteresis and undoubtedly that has some bearing on these things.

And I'm sitting here saying I could make a lifetime of several people to sort out this F factor, which is probably not the right even functional form for this dependence and whatnot. But I ask you a question. How accurately do you have to know it in a generic sense since we don't have a specific?

MR. DUNN: I would answer --

DR. POWERS: Hugh may have a specific alloy. Ralph doesn't.

MR. DUNN: My biggest concern is not necessarily F or picking a number for it. I would like very much for F to be picked in a reg. guide, for example, if we're going to go with that. Our biggest concern though is have we discovered the phenomenon. Have we put that all together?

Two years ago we wouldn't have said quench rate was important, and we see the CEA stuff. Quench rates are important to CEA. The cooling rate is wrong.

You know, I just believe that we do need to not be learning quite as rapidly as we're learning today, and I'm not talking about small stuff. It's the ID source of our oxygen. We need to look at that. We need to find out and understand it.

I don't disagree there isn't a source there. I mean the process of getting down in the clad, there's a creep-down process. It's going to change from one type of fuel designer to another designer. We vary internal pressures in the fuel. So one guy is going to creep down at 35 gigs. Another guy is going to creep down at 45 gigs. The creep rate of the cladding is a little bit different depending on ZIRLO and M5 are not going to have the same creep rate. The build-up and the return to offset is not going to occur at the same time, so what have you.

CHAIRMAN ARMIJO: There are a lot of variables in the system, and I was kind of surprised that the system, the cool-down rates from the events that you showed weren't used to guide the experiments so that they were done without -- getting the conclusion of quench from high temperature, of very fast quench, is totally unrealistic.

MR. DUNN: When we go back to Chung and Kassner, for example, we see that, you know, the beta quench -- I call it a beta quench. It's probably wrong -- but the high temperature quench down there is established as conservatism, and you know, when we justified M5, we just did quench tests.

And with quench tests, you're going to come in at 25 or 30 percent ECR before you shatter the cladding, or actually the way we do them is we test them with about a two psi over pressure afterwards to see if they leak gas.

And you'll get 25, 30 percent ECR before you do that. So we had plenty of room. So it was fine to quench right off the top. Why after this summer we didn't switch to something like 600 or so or get some criticism of that work that I put together, I can only tell you it's out there. I sent it to people, and we haven't had a tag -- of course, we haven't done it a lot since last summer because we got our cells. Everybody has been worried about where we're going to do the test and stuff. I'd like to see it considered. I didn't do it for nothing.

Mike?

DR. BILLONE: Just historically, this 800 C. number was, as I showed you for as fabricated materials, it didn't matter. We're only discovering this for hydrided materials, high burn-up materials, and it was proposed a long time ago with EPRI and partners, and we also sent out a request. It had different names, fuel reliability group for input for small break LOCAs, and we decided that 800 was bounding, and no one was concerned at that time. So we picked 800.

CHAIRMAN ARMIJO: That's before people knew about hydrogen and people knew about the quench.

DR. BILLONE: Right.

MR. DUNN: Well, I think we knew about hydrogen, but we won't worry about the quench interaction because I --

DR. BILLONE: Yeah. We didn't realize that you could get a little bonus. You can get some extra ductility by quenching hydrided or high burn-up samples at lower quench temperatures.

We have done some work since. We had a meeting in Argonne, a very good meeting in June of our industrial partners. NRC was a program review meeting, and that's where the slide that Bert showed that the CEA data was presented. You have to keep in mind that they have a large furnace, as I said, and when they say less than one degree C. per second cooling, it was less than a tenth of a degree C. per second from high temperature, 800, and it got slower and slower and slower and slower.

CHAIRMAN ARMIJO: Well, maybe that exaggerates the benefit of the cooling. Is that what you're saying?

DR. BILLONE: Yes, yes.

MR. DUNN: It might. It might. It's clearly going to mix us up in that phase change kinetics a little bit more with a more realistic cool down temperature.

DR. POWERS: One thing I'm sure you do not want to do is provoke the thermal hydraulicists to get involved in defining what your cool-down rate it because this cooling is extraordinarily complicated because as you bring water in it, you get little droplets that go flying up there, and life gets very complicated very quickly on this cooling process.

Now, there are some experiments, quench experiments, that I've written some notes here to go back and look at because I didn't pay any attention at the time and what the cool-down rates are and things like that.

MR. DUNN: Can I have your note afterwards?

DR. POWERS: What did you say?

MR. DUNN: Will you share your note with me?

DR. POWERS: Oh, yeah, absolutely, yeah.

MR. DUNN: Okay. Thank you.

DR. POWERS: Yeah. I mean, the guys that do that are very free with those data.

MR. DUNN: Yeah, that Penn State paper is worth looking at, too.

DR. POWERS: Yes.

MR. DUNN: It's about that thick.

DR. POWERS: Well, the type of report that sits around in draft is about this thick. But he does have all of his data in there.

CHAIRMAN ARMIJO: Okay. Well, thanks a lot.

MR. DUNN: Okay.

CHAIRMAN ARMIJO: Our next person up is going to be Mr. Nissley from Westinghouse.

(Pause in proceedings.)

MR. NISSLEY: I'm Mitch Nissley. I'm with Westinghouse Electric Company. I'm going to follow up with Westinghouse's contribution to the industry comments.

There's been a lot of questions over the last hour or so about if I can get out of town with a 1.2 F and my corrosion is relatively low. Shouldn't I declare victory and go home?

The main pat of my presentation is to try and portray some of the realities of the consequences of fully embracing the proposal as written. I will say that the proposal has actually evolved somewhat from what was published on ADAMS. What industry has been talking about back and forth over the last two or three weeks, we've had a number of telecoms in the

EPRI working group. We had an all day preview yesterday, and we've had some preconceptions of what would be presented today based on what was in that draft reg guide, and we acknowledge that there have been adjustments made, in particular, the F factor has been reduced from what we thought it was going to be. We have not had time as an industry group to fully talk through what that means. It's certainly in the direction we would like it to go, but we have not really had a chance to talk through that.

I'm also going to discuss our positive outcomes of the Argonne program; additional concerns Westinghouse has. They're similar to other ones, but I'm going to present them with a slightly different spin.

Essential consequences to the industry. I think this is really what the question has been over the last hour, is what's the big deal. I'm going to address that from a PWR perspective. Dr. Patterson from GNF will have some slides following mine where he will address the BWRs.

I have a few slides on consideration of realistic reactor physics. This is really aimed at looking at the physical burn-down of UO_2 fuel, and its ability to produce power as a function of burn-up and what the consequences of that are for its ability to achieve the high kinds of temperatures that have been tested to date in the high burn-up test programs.

And I have a few risk informed comments at the end of the presentation. It comes with a program. Argonne and the NRC have essentially confirmed that this interim use of IN 98-29 for the last eight years is a suitable LOCA criterion for irradiated fuel. It has closely approximated the effects of hydrogen on embrittlement in Zircaloy type claddings.

Going to the future, I think it is a very important enabler for the introduction of new and advanced alloys. If we would have been required to obtain high burn-up claddings and test them prior to introducing them into the reactor, number one, there's some risk that while you think the alloy will perform well at high burn-up, it might not. But I think that NRC has done substantial work to show that you can approximate high burn-up effects by prehydriding the cladding, and that has come up with an effective screening technique for advanced alloys so that you can really assess the acceptability from the LOCA perspective very early in the development program and not wait ten years to get meaningful how each cell works. So I think that's a significant contribution.

These are the concerns I said I would comment on with a little different twist. The first one is similar to the calculation Mike Billone showed this morning or I believe, no, it was Ralph Meyer. Sorry.

Based on the draft NUREG that we saw, we anticipated an F factor on the order of 1.5 or 1.6, and I went through and did a shorthand calculation just to show this was similar to Dr. Meyer's use of the 100 micron Zry-4 design limit. One hundred microns or four mils is effectively a design limit at least for Westinghouse fuel for all of our cladding types.

For such a design limit and with our 17 by 17 cladding thickness, which is 22 and a half mils, and if we use the theoretical Pilling-Bedworth oxide to metal ratio of 1.56, you go through the MAP and you get 17 percent oxidation at a design limit acceptable corrosion level without any transient.

So I wanted to put this simple calculation here just as an example. If you use a large F factor, you've got no room to work with with curb design limits on fuel.

Another thing that hasn't been discussed in a lot of detail. I do think that the two-sided oxidation is real if you have good, hard contact, and if you're at 1,200 degrees C. I think the data supports that.

However, one of the back-up references that was cited, this Hoffman and Politis ASTM paper, in there they concluded that you needed a temperature of at least 1,100 degrees C. and power cladding bonding in order to get the two-sided oxidation. At 1,100 degrees C. their paper cited that with the thin oxide layer they could go an hour before that layer would even start to dissolve.

If you got much above 1,100 degrees C. that layer dissolved very quickly. So in that paper, now, that's another area of uncertainty, but what that reference cited is that if you are below 1,100 degrees C., which I tend to present you would be for high burn-up fuel with pellet cladding bonding, you will not be anywhere close to 1,100 degrees C, and therefore there's no need to account for double-sided reaction.

Essential consequences to the industry. I think this slide -- that we might want to have a little back and forth on this -- is part of my presentation was aimed at supporting that there's really no safety issue here. The plants are fine today. I think Dr. Meyer did an effective job of getting that out of the way very early in the day.

So I think what we have here is a situation where it's recognized that there should be some changes to the regulations, but if we make them wholesale and if we make them quickly, that there will be a significant impact on the entire industry.

Now, that sounds maybe over dramaticized, but there will be a significant resource diversion and cost, and I really don't think there's any real safety benefit.

For the vendors, we will have to relicense our evaluation models. Our evaluation models currently do not address IN 98-29 within the framework of the approved evaluation models. The assessment of 98-29 is done outside of the evaluation models in accordance with the NRC guidelines.

So once this becomes a rule, it would appear to me that you would have to make that an inherent part of your evaluation model so that you would have to relicense it.

The use of Cathcart-Pawel is also not in all evaluation models. So the use of Cathcart-Pawel to do ECR calculations will need to be relicensed. Wherever we end up on two-sided observation and based on my prior comment, I don't think we really need to go there. Again, it would require a relicensing action.

So the vendors will have to relicense all of our evaluation models as I see it today unless there's some kind of a really creative regulatory spin on this that's not apparent to me.

I put in here a potential need for additional hot cell testing. I put that in because it was not clear to me from the information in the draft NUREG what was going to be required to derive F. I think the proposal was put forth this morning that I think more or less eliminates that as a concern, but that was not clear from the draft NUREG that was made available to industry.

Licensees, I think, have the lion's share of the work here. Okay? I don't see any way around this. If you have a new 50.46 criteria, if you're to demonstrate compliance, which is what you're required to do by law, you will have to use these relicensed evaluation models and every PWR in the country will have to have updated analyses. They will have to update their technical specifications to reference these new evaluation models, which are part of the bases, and they will have to update their FSARs.

A lot of cost. In my opinion, no real safety benefit.

There's a potential for increased fuel cycle costs. I showed on that previous calculation that this was really coming from the F factor point of view, but that F factor is too large, and you have design limits of four mils or 100 microns. You may be discharging at the discharge fuel in the future, but right now it will be perfectly acceptable due to the conservatisms that are contained in the proposal.

If we discharge fuel assemblies that have plenty of capability left in them, we're increasing the spent fuel also, which is really not a good idea in anybody's mind.

The next one is also related to fuel cycle economics. While our PWRs currently use ZIRLO for those who are supplied by Westinghouse. There are instances during a fuel shuffle where an assembly may be damaged or something like that, where you have to do what's called an emergency core redesign. What you're doing is you're coming up with a revised loading pattern. You can't afford to wait till the fuel fabricator makes another assembly and ships it out to you. You do the best with what you have, and I mean, this is really done in real life.

You look for the Zry-4 assembly or the whatever in your spent fuel pool that has the most reactivity left in it, and you'll come up with a new loading pattern that will support your energy requirements for that cycle, and that's not something that' done on a wholesale basis, but that can really save your skin and save your outage time if you have damaged assemblies during shuffling.

If there's a need to introduce -- I'll show you in a minute about this physical burn-down effect. Currently that's really not generally in the plant technical specifications as something that is a limit as a function of burn-up. It's just recognized, you know, that that exists and you can take credit for the high burn-up.

I believe that with the new requirements that you will have to do these explicit analyses as a function of burn-up, and in doing so you will have to a explicit burn-down credits at certain times in life, and once you've done that, that will be part of your licensing basis, and there's a potential there that that will put in overly conservative peaking factor limits that may reduce operational flexibility.

I'm not saying this is a guarantee, but one of our vendor participants in the working group meetings pointed that out as a real concern on his part.

And finally we have the NRC. Who's going to review all of this? Okay. This is going to be every PWR in the U.S., every PWR EM in the U.S., and they're going to have to review and approve all of this.

Okay. Now, I'm not saying this isn't the right thing to do at some point in time, but to do the rule change quickly without, if you will, a roll-out plan or a phased in way of accomplishing this, this is a huge amount of work to get to the same place where we are today. But I really can't interpret it any other way.

DR. SHACK: Well, half of those bullets at least would be true for any change in 50.46(b), you know, whether it's the one that's currently proposed. Any change would involve most of these.

MR. NISSLEY: If we codified the IN 98-29, for example, we'd have this.

DR. SHACK: You'd have that.

MR. NISSLEY: And we'd have this.

DR. SHACK: And you'd have the review.

MR. NISSLEY: Yeah, we'd have this.

DR. SHACK: Now, you're going to trade the penalty off with the benefit of being able to license your new advanced fuel and thoughts and potential value of one versus the other?

MR. NISSLEY: Well, I made the comment earlier about creative regulatory, you k now, ways of rolling this out. There's a way to grandfather plants, and as I thought through that, I thought, well, if you grandfather them where they are today, what if I got somebody who wants to do an extended power up rate in 2009? Is it still grandfathered or does he have to not do all of this?

I'm not a regulator, but that looks to me like quicksand a little bit. So I wish I had a sharp answer to that.

The next few slides are going to focus on consideration of realistic reactor physics. Current fuel in the U.S. is limited to five weight percent Uranium 235. In practice, you really don't want to build it any higher than 4.95 because of the manufacturing problems and things like that.

If you limit you fuel to five weight percent, it runs out of gas, and it starts to run out of gas around halfway through life, around 30 gigawatt days per metric ton.

Once it starts to burn down in terms of

its achievable power levels, achievable peak cladding temperatures and the corresponding transient oxides drop off dramatically, and that comment is valid for all break sizes, both large and small breaks.

The important conclusion from this, high burn-up fuel limited to five weight percent cannot approach 1,200 degrees C. without violating the limits for the fresher fuel in the core, and let me show you some core physics calcs and a sample LOCA calc to back that up.

This is depletion calculations for a free loop PWR to support an extended power up rate. The plot shows achievable tin power versus burn-up. The first cycle fuel are the curves that go through or the clouds that go, of course, from zero burn-up out here to about 25 or about 20, 25.

The ones that start here and go down are second cycle fuel, and in this case the third cycle fuel you can see is way down here with a maximum relative power of 0.8. Okay? This fuel is pretty big. It has been way around that.

We talked about, again, I want to come back to the idea of the two-sided observation. The Hoffman and Politis paper cited a threshold of 1,100 degrees C., that below that it did not really appear that it could occur due to the thin oxide layers.

The draft NUREG cites incipient bonding in PWRs at about 30 gigawatt-days and complete bonding at about 50 gigawatt days.

Okay. If I go out to 30 gigawatt-days, I already have a good ten percent reduction in achievable tin power by the time I reach incipient bonding. Ten power in power is about 100 degrees C. worth of reduction in ECT. So you're already well on your way to getting out of town on ECT.

If I go out to 50 gigawatt-days, on this particular plot or this particular loading pattern, I've got -- I don't know -- more than 50 percent reduction. I put in here 20 percent reduction for one simple reason. This is a typical loading pattern. I'm not trying to say I'm bounding everything. Okay?

And the 20 percent is based really on where these rods were. If I had a different loading pattern where I was pushing the fuel harder, even harder, I could shift the second cycle assemblies a little further out here, and so I didn't want to go for this whole benefit. I was really only taking credit for the reduction down to about here.

CHAIRMAN ARMIJO: What would be your typical just ballpark hydrogen numbers for the second cycle fuel at the end of a -- on the chart, and also for your highest burn-up? Would it be a couple of hundred ppm?

MR. NISSLEY: I'd like to ask Dave Mitchell to comment on that please.

MR. MITCHELL: Well, at the end of the second cycle, you're going to be running probably around 40 microns.

MR. NISSLEY: Your name, again.

MR. MITCHELL: David Mitchell, Westinghouse.

At the end of the second cycle, typical ZIRLO fuel would be running maybe around 40 to 45 microns.

CHAIRMAN ARMIJO: Oxide.

MR. MITCHELL: Of oxide thickness. So that would be probably around 400 ppm of hydrogen or so.

CHAIRMAN ARMIJO: And then the highest burn-up would be in the 600 range? Would that be reasonable?

MR. MITCHELL: Six to 700, somewhere in that range.

CHAIRMAN ARMIJO: Okay, but it couldn't get hot. That's your argument.

MR. MITCHELL: Yes.

CHAIRMAN ARMIJO: Okay. So from a temperature standpoint, if you try to integrate temperature and hydrogen in the cladding, it's not clear that the highest risk from a LOCA or largest amount of loss of ductility might come from the second cycle fuel rather than the high burn-up fuel.

MR. NISSLEY: I believe that to be true. I think if you get out past -- if you get out past about 50,000 megawatt days or so, it's bad, and you know, we talked about extending perhaps out to 70 or 75,000. People have done fuel cycle cost calculations. I mean you can put at most a couple of assemblies in there that are going to -- you can't put a lot of assemblies in play and have to get there or you're not going to meet your energy requirements. You just can't do it.

One last point. You see a curve here, which I haven't talked about. That curve was put in on this plant for the purposes of doing the IN 98-29 assessment. Okay? And what we did was we put in our core reload process, which is something we do every cycle to check our safety limits and things like that, to make sure that our limits remain applicable.

You want to build in a lot of cushion

there. So if I have a different cycle design, net cycle, if I take that right down to where these numbers are, I might have to do a reanalysis or something like that.

So in a high burn-up LOCA analysis, you're not going to really want to take credit for this. You want to leave yourself a lot of fat so you don't -- nobody wants to do cycle specific LOCA analysis except maybe a vendor.

Okay. What I wanted to illustrate here is this is a little different spin on the power capability versus burn-up concept. This is a typical four-loop PWR, core to core symmetry, the way these things are usually calculated and unless you've really done something bad to your core design.

Fresh fuel is indicated as the dark red, and I'm probably color blind. So if I get the colors wrong forgive me. This color here is also fresh fuel. What you can see is you've got a very limited number of fuel assemblies that are leading a core at this point in time with relative powers in the order of 1.4 to 1.5, and that's assembly power relative to core average.

The other fresh assemblies as in the range of 1.1 to 1.4. Okay? That's these guys.

The highest power of the once burned assemblies is at a relative power of 1.2. Okay. So you can see the fresh assemblies are really carrying the load for this cycle.

My once burned assemblies that are out near core periphery due to leakage considerations and things like that are going to have relatively low powers, and in this case my twice burned assemblies are all on the periphery, and they have relative powers less than 0.4. Okay? So this stuff is effectively dead

It is helping you out from reduced -- you know, if you're going to have leakage, you want these assemblies to be absorbing as much of those neutrons coming out as possible. You don't want fresh fuel out here that's doing the leakage.

The other thing is these assemblies are dead enough. You don't want to put them inside in the inner region to the core or you're really ruining your fuel cycle economics. You don't want dead assemblies towards the core.

CHAIRMAN ARMIJO: That would b ea misloading error, but if you did, how much power could you get out of it, if you put the twice burned surrounded by the highest reactivity fuel?

MR. NISSLEY: If you put it, say, right there, that would be -- well, number one, you'd create massive gradients across these assemblies.

CHAIRMAN ARMIJO: Right.

MR. NISSLEY: I would imagine early in the cycle you could get that up to something around this level or maybe a little higher. I don't think you could hit a one.

CHAIRMAN ARMIJO: Nobody would do it on purpose.

MR. NISSLEY: But you can destroy these assemblies.

MR.SIEBER: In terms of your control

MR. NISSLEY: Yeah, you'd be wasting fuel right and left.

MR. SIEBER: You're just wasting neutrons when you do it.

DR. POWERS: You just don't like any rod to have a high work. You move some in there.

MR. NISSLEY: Okay. Now, in current LOCA analyses, you know, you have to consider peaking factors up to the tech spec limits, and there's all kinds of requirements on how you perform calculations even with realistic methods.

My next slide, I'm going to show you a

refresh assembly, one of these guys, but with the power bumped up to be consistent with tech spec limits. I'm going to show another assembly, which will be a once burned assembly with a relative power in this range, and I'm going to show another assembly that is once burned out here near the periphery and has a relative power right about this number. I think it's about a .6 or something like that.

Did I lose that figure? Oh, there it is.

Okay. Two points here. One is this is out of a design basis accident analysis. In this particular methodology we look at variations in axial power distribution. This is a relatively well behaved shape. It's not extremely -- so it's a reasonable axial power shape like you would have in base load operation.

However, that hot assembly has been bumped up to a higher level than calculated in that core physics distribution. This is the average type assembly once burned that I talked about, and this is a once burned peripheral assembly with a relative power of about .6.

Okay. Even with a power of .6, you saw all of those twice burned assemblies were less than .4. This is more or less a mono-transient. There's almost no heat up here. Even with the relative power of 1.1, you're under 600 degrees C. It's only that hot assembly with the power bumped up that starts to get up relatively high.

Now, again, this is a relatively favorable because in this particular analysis or this particular case I've used a well behaved shape like you would get in a normal core depletion. This is like a realistic axial power shape but with the peaking factors bumped up to that noticeable margin for the hot assembly up to the limit.

The core average fuel once burned is way down here, and I used once burned on a the periphery, and it's very, very benign.

A few comments on risk informed considerations. I know there's currently a fair amount of uncertainty in what's going to happen with 50.46(a). I know the committee sent a letter to the Commissioners and the staff tat they are currently trying to figure out how to deal with.

The large break LOCA as we currently think of it is beyond the transition break size in that draft rule.

The current PCTs and observations that we get for large break LOCAs would be dramatically reduced if one implemented the optional 50.46(a). The reason for that is that you do not have to take the single failure. We can use realistic power shapes. You do not have to take loss of off-site power assumptions. So any plants that are currently close to the LOCA limits with the fresher fuel, recognizing that the high burn-up fuel is very, very, very benign, you would generate even more margin with a realistic considerations that are embodied within the draft 50.46(a).

With regard to small break LOCA in the U.S. for PWRs, all the current small break evaluation models still use Appendix K requirements. Nobody has licensed a realistic analysis yet for small break LOCA.

One of the main features of Appendix K from a small break perspective is the decay heat requirement, the 1971 plus 20 percent. There was a petition for rulemaking back in the early part of the decade that was looking at relaxation and the Appendix K decay heat requirement.

The NRC put out a regulatory information letter, 0202, and it had a number of attachments to it with supporting information, and they looked at the reductions in PCT and oxidation for small break that would be realized just from a change to a realistic decay heat, and they were getting estimates on the order of 500 to 1,000 degree reduction.

From a realistic LOCA response, what you currently see in an FSAR for design basis analyses have huge amounts of margin in them, and I believe I showed you in that one example large break calculation that even using more or less an upper bound for the high burn-up fuel in terms of relative power, it was more than 1,000 degrees, less limiting than the fresh fuel.

I think the real message here is we've done a lot of testing at 1,200 C. with high burn-up fuel. The double-sided reaction is also a limit that I k now of to a very high temperature like that, and you just can't get there.

So in summary, yes, we believe the test program and the NRC's interpretation. The data has yielded positive results for NRC and the industry. They've confirmed some of the NRC interim requirements in the information notice. That was a positive finding.

They also have put out a framework for moving forward with advanced claims. We voiced some concerns and we've had some dialogue about what do you do in regulatory space with this.

I hope I've given some insights into some of the real consequences of what this will mean to the industry if we move forward in a rapid basis and impose these recommended changes without some careful thinking of how to roll this out in a way that industry, the vendors, utilities, and the NRC, how we can handle this.

The day this becomes law, it would seem to me people have to be in compliance. How are you going to prepare to do that and recognizing the fact that there's really no significant safety benefits from doing this.

So this is, you know, what we've been struggling with and why we've been, you know, raising questions on why we're moving fast so forward. Do we understand this sufficiently to move forward at this time?

CHAIRMAN ARMIJO: Okay. Any questions?

DR. BILLONE: Mitch, just help me out on one thing, on fuel management.

MR. NISSLEY: Yes.

DR. BILLONE: How you reload the core. There's nothing -- NRC doesn't regulate fuel management. The utility can juggle fuel around any way it wants as long as it meets its tech specs; is that correct?

MR. NISSLEY: Correct.

DR. BILLONE: So in principle, you could take a one or twice burned rod and put it in the center of the core. I mean, there's nothing that would stop you from doing that.

CHAIRMAN ARMIJO: Oh, yeah.

MR. CLIFFORD: Paul Clifford, NRR.

I was just noting that there's a few mechanical design limits that would often limit how you burn a high burn-up assembly towards the analyte.

DR. BILLONE: Thank you.

MR. NISSLEY: I'd like to recognize Odelli Amanpour.

MR. AMANPOUR: Odelli Amanpour, Southern Nuclear Core Analysis.

Typically the way fuel management is done, you have to have the burn-up limits met. In other words, there was a question earlier whether or not a fuel assembly with a higher burn-up can be placed inside the fuel on the periphery. Yes, that is possible, but you still have to meet the burn-up, and if you put the fuel assembly in that are of the core, that has higher peaking factor. It would definitely push that fuel assembly to go outside the 60 to 062 thousand megawatt-day.

So there are speed breakers built into the --

CHAIRMAN ARMIJO: Right. The only point I was trying to make, you wouldn't do -- it's physically possible to do it in error, a mistake, a big mistake, or if you were doing an experiment and you went to the NRC; somebody was interested in driving fuel to high burn-up, and that was an idea. You'd have to review that, approve it, get exemptions for a lot of your tech spec limits, and maybe you would do it. You know, it doesn't make any sense, but it's feasible to do it, but it would be, my guess, mostly a mistake. So I just don't think there'd be any incentive to do it.

MR. NISSLEY: I would agree.

CHAIRMAN ARMIJO: Okay. If there are no other questions, we'll have Mr. Patterson from GNF.

MR. PATTERSON: I am Chuck Patterson from G.E., Global Nuclear Fuel.

What I'd like to do is discuss the potential effects of the change on BWR. We typically are a bit less sensitive to LOCA events than a PWR in most of the reactor types. There is one type, a BWR-2 that is sensitive to LOCA loadings, but we can build around that by the way the fuel is designed, how much void space is put in and how the fuel is operated with the power limits.

The proposed changes appear -- and this is based on what was available as of last week -- that they will affect the BWR-2s and depending on what's assumed for F factors and possibly the ID oxidation, it might also affect some of the other types of reactors.

Our assessment is that this effect is due not so much to the method, but into the conservatisms that are inherent to it, inherent to the proposal.

Here is a summary of where we are with the current regulations and what we show here is the calculated ECR in a LOCA event based on the number of plants of a given type. So we have the BWR-6s with on the order of one percent ECR; the same for the fives and the fours. The threes are here, and the BWR-2s are here. These right now are designed to comply with the 17 percent clad oxidation and the 1,200 C. peak cladding temperature, and so they're already affected.

Here is a specific example of a BWR-2 where this is exposure on the X axis and the Y axis is the maximum linear heat generation rate, and early in life through around 30 to 40,000 megawatt days per ton, the operation is constrained by the peak cladding temperature.

At this point a ballooning and perforation is then calculated to be a limit, and so the two-sided oxidation becomes the dominant factor, and so the power is reduced to keep this below the 17 percent. As burn-up increases, the factors that affect the operation in the threes and the fours and the other reactor types dominate, and the power comes down due to the nuclear, thermal, and mechanical constraints.

And so the actual operating power then is the locus of the lower bounds of these curves, and what we think will happen is that if the F factor goes up, this will come down and the total energy under this curve will also go down.

In the region that is limited by cladding temperature, as I mentioned earlier, the expected effect depends on what is assumed for the F factor. Now, earlier on we talked about the oxidation being on the order of ten microns, which is not a bad assumption, ten to 20 microns on a best estimate basis. But when this is translated into licensing space, all of a sudden we're working at the 95th percent, and the 95th confidence bounds and the 95th percentile, and it's more like 50 to 70 microns.

So we really are sensitive to the values that are assumed for the F factor.

We're a bit concerned about the F factors for the Zry-2 because the work that was done is primarily on the Zry-4 with almost ten times the amount of hydrogen. This is the material that was used in test was 70 and the Zry-4 was 600 to 800.

the other thing is that the quench temperature -- and I almost hate to mention this because if you ask me a question I won't be able to go in deeper. We'll need a thermal hydraulicist, but the argument is that the minimum temperature for film heat transfer is around 600, as during the cooling phase the temperature change will be slowed to 600 and then at that point the rapid punch can occur.

And so the 800 C. quench temperature on which the F values are based is bounding, but it also bounds the region in which the base transformations are happening and the diffusion rates are still pretty high. So we think that overstates what the effect might be.

Knowing this range here, the ID oxidation is really not a factor because a BWR is a semi-free standing design. You just don't have the hard pellet clad contact, and we don't have the ID oxidation.

As you move to the higher burn-up region, the oxidation becomes limited, and until the F is one, we're looking at a half to one kilowatt per foot, depending on where the exposure, one kilowatt per foot towards the higher burn-ups and a half towards the point of the oxidation becomes dominant.

The potential effect here, I don't know whether it's going to affect us or not. We already are considering in the twos the effect of two-sided oxidation. We are somewhat concerned on the threes. We don't know how -- on the fours -- we don't know how that will behave. We're concerned because the rods that were tested with fuel in them, which were down at the point 57,000 megawatt days per ton lot average, which was almost equal to the point of assuming full bonding, really didn't show any stabilized or any of the alpha on the inside. There were nodules of alpha, but it wasn't the same as what you get on the water side.

So we think that this might also be a conservatism. We really don't know how it will affect us.

Now, we talked about this. The ductility limits seemed to be based on or they were based on the

Zry-4. Mike showed us earlier today some Zry-2 results which were encouraging. We mentioned the lower hydrogen concentration. A large concentration for us is on the order of 200 ppm, maybe 300, and as I mentioned, this is something that has really not been observed.

We do have bonding, but even after the pellet is in placed, the amount of oxygen is transferred into the rods that were tested really didn't lead to an ID oxide phase comparable to what happened on the outside surface.

There are some Zry-2 tests in here, but the thing that really wasn't stated in the discussion of how an F factor is derived is you need a test of irradiated material with the hydrogen and all the other things that contribute to the loss of ductility on the assumption that zirconium based alloys are all the same or maybe the 1.2 is okay, but this is a conservatism that may come to hurt us.

I think that the biggest factor for us is that the change in hydrogen which leads to the F factor for BWR is really not a monotonic function. It's not a scaler times burn-up or oxidation. We find that we can have oxide that follows a normal parabolic relationship and hydrogen that increases at a faster rate. And so we're concerned that we can probably live with the 1.2, but maybe that another method was a better approach where hydrogen is explicitly treated in the formulation independent.

That's what I had to present. I deliberately didn't put a summary or conclusion slide. As I said, I think we have margin. I think we're going to be effective, and the conclusion is really in the details of what will emerge.

Any questions?

CHAIRMAN ARMIJO: Any questions from the committee?

DR. BILLONE: May I make one comment? I think the big difference between an 800 degrees C. quench and, say, 500 degrees C. quench is not so much all of the phase changes you people are talking about. I think it's the redistribution of hydrogen --

MR. PATTERSON: Yes.

DR. BILLONE: -- in the material. I think most of the phase change for Zry-4 is completed by 800 to 750. That's Zry-4. It doesn't include all of the alloys, and I don't think what you're seeing in these CEA and Argonne test results, comparing no quench to quench is really phase changes in the material. I think it's hydrogen migration, based on the results that CEA has presented.

It doesn't mean that there isn't a difference. I just want to make a point that --

MR. PATTERSON: Oh, that's one complement. The other is the rate at which things move around. Going from 800 to 600 can have a big effect on your mobility.

DR. BILLONE: No, but oxygen is much larger than hydrogen, and oxygen diffusivity and mobility is extremely slow, below 800.

MR. PATTERSON: That's my point.

DR. BILLONE: Yeah, okay.

MR. PATTERSON: Thank you.

CHAIRMAN ARMIJO: Dr. Meyer?

DR. SHACK: How about a break?

CHAIRMAN ARMIJO: Well, we could have a break. All right. Why don't we take a break for about 15 minutes? So it will e 4:35.

(Whereupon, the foregoing matter went off the record at 4:21 p.m. and went back on the record at 4:38 p.m.)

CHAIRMAN ARMIJO: Okay. Are we ready to resume our meeting?

Ralph, would you care to?

DR. MEYER: Yes. I have several comments

that I would like to make with reference to the industry presentations that have just been made.

First of all, regarding the status of the research, we have had a formal research plan for this work dating back to 1998, updated in 2003, and frequently reviewed by both the committee and our industry partners. Work like ring compression tests on prehydrided and irradiated ZIRLO and M5 integral tests with PWR rods and the testing of adequate surrogates has always been planned for what we refer to as Phase 2 of this program, most of which requires an alpha-gamma hot cell, which as you know is unavailable to us now, and we hope to do in the Oak Ridge Laboratory in a few years.

It has always been our plan and openly so to try and bring the ductility information in early so that we could address the issues with the criteria in 50.46.

There was another slide given early on and echoed in part in subsequent presentations by the industry about stockpiling conservatisms, and I want to address each of the three areas that were listed in that slide.

One was oxidation at the 1,200 limit, and I simply want to point out that this is not different from what has always been done. The 17 percent number was derived from data at that temperature limit, and the numbers which we have current measured, which, by the way, are in almost all cases a little bit higher than 17 percent, are done at the same basis.

A second concern about conservatisms has to do with the F factor. The F factor of 1.2 that we have suggested is said to introduce more conservatism into the analysis, and I want to point out that that is really only true for Zircaloy-4 for which we have a full set of data.

If you look at the example that I presented for ZIRLO, for example, the current regulatory procedure would take, for 40 microns of corrosion, would take the 17 percent limit and subtract four percent, giving you a limit of 13 percent.

In the proposed limit for ZIRLO, you would start with 19 percent, subtract 4.8 percent and get 14.2 percent. So, in fact, when these numbers are applied to ZIRLO, you get a slightly higher limit instead of a lower limit.

Similarly, for Zircaloy-2, if you had -- gee, I don't know what number. I think I took the -- I'm sorry. I've made a slight mistake in doing my own hand calculation here, but the point is you would be subtracting a number from -- you currently subtract a corrosion based number from 17 percent, and say that were a big number like ten percent. If you then subtracted 1.2 time ten percent from the current number, which we now get as 19 percent, you come out at the same seven percent.

So it's basically a wash in some of these cases, and although for Zircaloy-2 it is not.

The third item which most everybody talked about as introducing additional conservatism was the matter of double sided oxygen penetration away from the balloon region, and this, in fact, has a very small impact on the analysis because everyone is already doing doubled-sided oxidation penetration in the balloon.

And you saw the example I presented. The balloon is slightly cooler, but it has a thinner wall, and put that together and compare it to a two-sided calculation away from the balloon and you get very similar results. So I don't think there is any impact of significance on doing the two-sided calculation.

What this does, in fact, is bring realism back into the analysis. So instead of running a very complicated calculation in the balloon where you have to take the wall thinning into account and replacing it with a calculation outside of the balloon where you don't have to do that and you don't have to worry about such other things as axial fuel relocation, you get an answer that is approximately the same and would not result in a major impact on the plants.

Now I want to talk just very briefly about the F factor itself and the magnitude of it. You've seen data that showed that the F factor could be 1.3 plus or minus .3. In other words, it could be one or ranging up to 1.6. An F factor of one corresponds to the current regulatory position using the information notice from 1998, and after considering the various heating and cooling rate effects that might occur, we selected 1.2 as a good average number, which I think is very reasonable and not overly conservative.

And one final comment then is about the two-sided oxygen penetration, and the hydrogen absorption. It's a very good point, that some of the cladding materials absorb less hydrogen for a given amount of steam oxidation, and this term is a hydrogen surrogate.

It is also possible that the sensitivity of hydrogen might be different in these alloys. We have not tested it. I would be very reluctant to base that test on prehydrided material without confirming it as an adequate surrogate, but nevertheless, we do have the one set of data, and that's all we have to go on at the present time.

There was one other technical comment about the data of Hoffman and Politis and this threshold of 1,100 degrees Centigrade, with the suggestion that you would not get any oxygen pickup on the idea of the cladding at lower temperatures.

The Hoffman and Politis test involved putting unirradiated pellets of O_2 inside of unirradiated tubes of zircaloy and then annealing them under pressure. So you did not have in that case an intimate bond between UO_2, which was the oxygen source in the cladding, until you created that bond in the short time of the experiments.

In the case of real fuel rods, you have that bond already created and intimate contact between the fuel and the cladding, and in the four tests that I referred to, one of them was the Hoffman and Politis work. One of them was Hobbin's (phonetic) work on the power coolant mismatch test in PBF, and then one of the observations was on the Limerick integral test that we've done at Argonne, and the fourth observation was on the Robinson one-sided test. In each of these cases you saw significant stabilized alpha layers on the ID of the same magnitude as the alpha layers on the OD.

So those are the comments that I would make, and I would now like to ask if Mike Billone has anything he would like to say, unless there's some discussion of this, of my comments.

CHAIRMAN ARMIJO: Is there any discussion? Go ahead. Just give your name.

MR. MONTGOMERY: Robert Montgomery.

Just one comment, Ralph. You noted that in the test plan, the original test plan, that integral PWR tests were planned. My recollection was that they were planned back in 1998, 1999, 2000 time frame, and we actually were planning on doing those types of tests even as late as 2005 with regards to looking at the double-sided oxidation effect.

So I just should point out that, yes, that it is correct that the ZIRLO and M5 data was an additional program that was not part of the original plan, but that the integral PWR tests were par of the original plan.

CHAIRMAN ARMIJO: Thank you.

Mike.

DR. BILLONE: I have just one slide I'd

like to show that was missing in the morning if that's okay.

CHAIRMAN ARMIJO: Sure, sure. Is this in a handout?

DR. BILLONE: No, it's the one that was missing. It should have been my very last slide.

CHAIRMAN ARMIJO: Oh.

DR. BILLONE: Okay. I just wanted to show the examples about to what degree are test results conservative. How would they be relaxed if you had different heating rates?

And where I got these numbers of F going down from 1.6 to 1.4 to 1.3, depending on the cooling rate, and now Bert has suggested six. So we might do that.

And these are experiments that we can actually do, but I'm going to tell you what my opinion is right now of the outcome, that this is the experimental heating rate we used, which seems to be reasonable for a large break LOCA. It seems to be at least upper bounding type rate. Most, if not all, of your embrittlement and oxygen pickup is occurring during this heating rate.

What happens during the rapid cooling is you only get an increase in oxidation level of .2

percent, and however, in the beta layer where you're looking for ductility basically what's happening is phase changes. You're not getting extra oxygen in there. You're getting some alpha stabilized by oxygen that's precipitating out as recooling, and you keep some beta. You're in a mixed phase regime.

So my contention is that this is faster obviously than a LOCA cooling rate, and it calculated ECR for the transient is 5.7 percent. If you went down at five degrees C. per second my contention is you'd pick up no extra oxygen in the beta phase. You just have a little more redistribution. The calculation would be 6.4 just to do this cooling effect, and if you cooled it three degrees per second, you'd get 6.9.

So the 1.6 F factor is just taking the data literally as it is, not applying it to realistic LOCA and saying, well, gee, you know, to make this correlation work I need a factor of 1.6.

If I take into account what I think are more realistic cooling rates, and again, I haven't taken into account the conservatism in the high quench temperature. I'm just looking at the calculation that's involved. This corresponds to an F factor of 1.4. This corresponds to an F factor of 1.3.

If I take into account that your quench temperature might be below six hundred, and it seems like most of the calculations are, I allow more hydrogen to come out of this beta phase, precipitated small hydrides, and I got the word "precipitation hardening" wrong when I agreed. This is actually allowing the material to soften by losing hydrogen.

CHAIRMAN ARMIJO: No, the way I was talking is when you quench very rapidly, you'll form very tiny GP zones, if you will, very tiny zones, and if you then cool slowly, they'll grow and you'll get less hardening by slow --

DR. BILLONE: Okay.

CHAIRMAN ARMIJO: So we're in agreement.

DR. BILLONE: Okay. Sorry. I was afraid I agreed to the wrong terms.

Anyway, this was the point, and the point is that the reason that we have such fast cooling is that we don't have very much thermal mass. We have a 21 millimeter sample. There's no fuel in it. If we went to a three inch long sample with zirconium pellets in it, we with our controller would control the cooling rate to whatever you want. It's not so bad to be linear as opposed to where we have -- the issue is the amount of time that you're spending at temperatures.

MR. SIEBER: These plots though are the are plots of the way you conduct your test.

DR. BILLONE: Only this is the only plot of how we had --

MR. SIEBER: As opposed to the plot of what the real LOCA temperature response is, which to me would be more convincing if I saw the results of the LOCA curve and then fit your test onto that curve at the right rate. I think that that's one of the areas at least in my mind where the disconnect sort of is, is figuring out whether what you're testing and what you're doing corresponds to what is actually happening in the core, and arguments and your data and your plots seem to reflect what went on during the tests as opposed to what goes on in the core.

DR. BILLONE: May I just comment on that?

MR. SIEBER: Yes.

DR. BILLONE: I think in the spirit in which we're generating these data we want to make sure we're at least giving you a lower bound, realistic lower bound on how much time we can spend at high temperature or what oxidation level you can tolerate as a function of burn-up or hydrogen content.

So I'm more concerned with the fact that we received initial criticism that we weren't rising to 1,200 degrees C fast enough to test the criteria. It turns out that I am very pleased that we weren't because I don't see any LOCAs going up instantaneously to 1,200 degrees C. It makes this part more relevant and probably I would call it bounding.

This is too fast, and all I care about here is 800 degrees C. is the largest quench temperature that you can calculate in any of your calculations in the world. then at least I know 800 degrees C. quench I'm being conservative. I'm going to come out with a lower number. I can certainly quench. We can quench at any temperature, but you might want to think whether you want to end up analyzing every rod in the core or whether you want some bounding results.

The point is that these kinds of rates, what's important here is how much time are you spending at higher temperatures to allow the hydrogen to at least go below this point.

So I'm just saying that the speculation is that if you want some relaxation with 1.6, if Bert's six degrees C. per second is really bounding in terms of how you're coming down towards 800 degrees C., then you're somewhere in here. This is five and this is three.

If a lower temperature is more realistic than 600 degrees C., you guys can call it bounding. We can run the test. It's very easy to do with prehydrided material, and it's easy for us to control these ramp rates, which are more relevant than this ramp rate. This is a bounding, upper bound, 13 degrees C. per second cooling rate on the average from here to here, which is much faster.

MR. SIEBER: Pretty fast.

DR. BILLONE: Which is much faster.

MR. SIEBER: Really fast.

DR. BILLONE: I'm just pointing out that in deriving the F factor, this cooling effect is important.

And the last comment is there's no discontinuity of discrepancy between CEA data and ANL data. ANL data is for 13 degrees C. per second. CEA data is for less than a tenth of a degree C. per second, slowing down to .01 degrees C. per second. In other words, it's the same exponential curve, but just stretch it way, way out.

So that maybe they pick up two and a half percent ECR during their high temperature phase, and they're up to six percent ECR in the cool.

MR. SIEBER: Those rapid cool-downs and quenches are for large break LOCAs.

DR. BILLONE: Yes, yes. Well, as a matter of fact, when we run our tests for break-away oxidation, we don't even quench. We were trying to thinking of applying it this morning.

So, I mean, is this point clear, that when I give numbers like 1.2 plus or minus .2, I'm sort of taking this curve, assuming that I've quenched at lower than 600 degrees C. and trying to give you an estimate. If I don't want to play this game, I just way, okay, if you want to use that corrosion ECR directly from the data, the F factor is 1.6 and I quit.

But I know that there's some things that happen during slower cooling that we use and during lower quench temperatures. So I'll continue to elaborate that in Section 7 in my report. I just wanted to make this point clear, where I'm coming out with these different numbers.

CHAIRMAN ARMIJO: Mike, what additional things do you have to do to issue your report? Do you have any other tests in progress, or is it just basically editing?

DR. BILLONE: No. There's short time

tests, tests that won't take us much time that we just never got to. We just finished a batch of two tests which in the report have the squiggles and it says "data to be inserted here." So let's not get into those.

We haven't done prehydrided tests on ZIRLO, which we've started prehydrided ZIRLO, and we haven't done prehydrided tests on M5, but really what everyone wants is the tests on the high burn-up ZIRLO and M5.

So by this March 31st date, I'm fairly confident that we could get some prehydrided data in there which would add to Section 4. We're trying very hard to get some high burn-up data. It would probably only be one of the two alloys, which would just be added to Section 5.

CHAIRMAN ARMIJO: Okay. So this report will be held up until you finish the irradiated tests. Is that your plan or are you planning to issue it --

DR. BILLONE: I think the plan is whatever we have before the end of March. The plan is to finalized the report for the end of March.

MR. SCOTT: Could I interrupt? This is Harold Scott.

Since I'm the manager of the program, it's more a function of what we in research are willing to do and not to do than him. If Ralph and I can convince our boss on my right to let us delay, then we'll do these Studsvick (phonetic) irradiated specimens, or if Mike is luck and everything works fine at the lab, he can get them done in time, but we haven't talked about this before. So I'm hesitant to go too far.

It's milestone constrained, not technically constrained. Is that unfair?

MS. UHLE: Well, this is Jennifer Uhle from the staff and I am one of the many managers in research, and I would say that it's a function of it's not milestone restrained. It's a function of technical adequacy and technical sufficiency, and that comes in part from our opinion here in research. I t comes in part from NRR's opinion. It comes from the public's opinion as well as ACRS' opinion and, you know, all of our stakeholders.

So if there's the decision that everyone comes to consensus that there's a need for more data, then we'll simply inform the Commission of that, but I think that the research staff feels that there is sufficient amount of data, and that is the research position, but again, it depends on the consensus of everyone involved.

CHAIRMAN ARMIJO: Okay. I understand.

DR. BILLONE: I guess my last comment is what you have in front of you edited is a better version on ADAMS and even better version than I have.

CHAIRMAN ARMIJO: Yet to come.

DR. BILLONE: It's really a quantum leap from what we had in 1973, and obviously we're going to continue to generate data after this report is issued. We're going to continue to generate data from now until the data report is issued, but I yield to Jennifer and Ralph and Harold and HRR on what is enough.

MS. UHLE: This is Jennifer Uhle again.

I mean, the decision as far as the rulemaking schedule is ultimately the Commission's decision and, of course, the staff is going to propose various options and certainly what the staff feels is the best path forward, but also there is an acknowledgement that the current regulation does not consider burn-up effects as we've talked about earlier, about the need to move forward. It's just a matter of when exactly the appropriate time is.

CHAIRMAN ARMIJO: Okay. Thank you.

MR. DUNN: Mike, don't disconnect.

This is Bert Dunn, AREVA.

Could I just ask one question?

DR. BILLONE: Yes, sure.

CHAIRMAN ARMIJO: Go ahead.

MR. DUNN: On the Studsvick samples, were they cleaned so well that we won't learn anything about ID sources from them or is there still enough dirt on --

DR. BILLONE: They most definitely have an oxide bond on the ID. They weren't cleaned very well at all, and we have to do additional cleaning to get rid of the rest of them, the remnants of the fission products. Those rates are very high. When we finish cleaning, additional nitric acid cleaning, you will always have the bond.

MR. DUNN: You're going to destroy the --

CHAIRMAN ARMIJO: You are going to have mixed feelings about taking more of that uranium off, don't you?

DR. BILLONE: No mixed feelings at all because we're in a beta-gamma hot cell where there isn't supposed to be any. So no.

CHAIRMAN ARMIJO: From that standpoint, but from an experimental -- experimentally, to Ralph's point, if you take all of the uranium off that's bonded, you take away a mixed source of oxygen.

DR. BILLONE: I have no mixed feelings because what you're left with is really residue, various compounds from the nitric acid, uranium dioxide interaction. It's not true fuel when you do the defueling. You do the defueling of nitric acid and you want to get rid of all of the fuel.

There are other tests that we have planned in which we will leave the fuel in. Those tests will be conducted at Oak Ridge, and there will be no nitric acid treatment. That would be just as received, and we'd be testing high burn-up as we receive the fuel.

But for where we have to do these tests, which is a beta-gamma hot cell with very strict limitations on what you can bring in there, what you can leave in there, it will be the fuel and the fuel cladding bond.

CHAIRMAN ARMIJO: Well, just to make sure, will you consider those tests definitive if you've etched off the uranium, the UO_2?

DR. BILLONE: Well, they're definitive in the same way the results I presented are. In other words --

CHAIRMAN ARMIJO: No more definitive.

DR. BILLONE: No more definitive. It

would be a new alloy, and M5 and ZIRLO are new relative to our testing program, and it would be very, very valuable to have a companion data set for for those alloys to go along with our zirc data set, but also to really complete the picture, you really would like to have the prehydrided data to scope out where you should test. You have a limited number of tests. You pick the test time and you pick the test temperature. To know where you are in terms of embrittlement, it's really nice to have the prehydrided data available.

DR. MEYER: I don't think your expectations should be too high on the small pieces of ZIRLO and M5 that we have because they have fairly low corrosion on them. Could you say what the corrosion levels are, Mike, on the pieces that we got from Studsvick?

DR. BILLONE: The corrosion on the M5 weren't given, but they're expected to be -- Bert, help me out. Is it ten microns or 20? Is it 17, 15? Something on the order of 15 to 20 microns, small.

MR. DUNN: I think we can confirm that. I'd need to go back to Lynchburg.

DR. BILLONE: But the ZIRLO, we got four specimens in the first shipment. There was another shipment that you've arranged at Studsvick waiting for us to receive it for the LOCA program with the higher corrosion levels on the order of 50 microns. We do have material coming from ZIRLO that's higher.

MR. DUNN: One comment back to Mike -- I mean back to Ralph. If we look at our data for corrosion, the highest we're getting is in the 20 to 22 micron range. There was maybe three spots that are up at 30, and those are out of German reactors, which are very high duty reactors. They're not U.S. reactors, and other than that, what you got out of Studsvick is what the material does.

Now, the stuff from North Anna will be about the same.

DR. BILLONE: Yes.

MR. DUNN: You can't create something that doesn't exist.

CHAIRMAN ARMIJO: Unless there's some really pressing new issue --

DR. BILLONE: No, no.

CHAIRMAN ARMIJO: -- I'm going to now close these discussions, and now I'd like to ask the committee members for their comments and also some --

PARTICIPANT: You want to go off the record at this point.

CHAIRMAN ARMIJO: Yeah, I think. I don't know how to go off the record. Well, I'm going to go off the record if I bang this thing, but I don't want anybody to leave. That's what I'm afraid of, but I'll do it anyway.

Off the record.

(Whereupon, at 5:10 p.m., the subcommittee meeting was concluded.)