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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

JOINT MEETING OF THE

MATERIALS AND METALLURGY, THERMAL-HYDRAULIC

PHENOMENA AND RELIABILITY AND PROBABILISTIC

RISK ASSESSMENT SUBCOMMITTEES

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THURSDAY

NOVEMBER 15, 2001

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ROCKVILLE, MARYLAND

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The ACRS/ACNW Joint Subcommittee met at Nuclear Regulatory Commission, Two White Flint North, T2B3, 11545 Rockville Pike, at 8:30 a.m., William J. Shack, presiding.

SUBCOMMITTEE MEMBERS:

WILLIAM J. SHACK	Chairman, ACRS
DR. THOMAS S. KRESS	Co-Chair, ACRS
DR. DANA POWERS	Member, ACRS
MR. MARIO V. BONACA	Member, ACRS

ACRS STAFF PRESENT:

Mr. Michael T. Markley, ACRS

ALSO PRESENT:

Mr. Ralph Meyer

Mr. Steve Bajorek

Mr. Norm Lauben

Ms. Carolyn Fairbanks

Mr. Alan Kuritzky

Ms. Mary Drouin

Mr. Tom King

I N D E X

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

8:31 a.m.

CHAIRMAN SHACK: The meeting will now come to order, the Joint Meeting of the Subcommittees on Human Factors and Safety Research Program, previously scheduled for today, has been postponed. This is a joint meeting of the Advisory Committee on Reactor Safeguards, Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena and Reliability and Probabilistic Risk Assessment.

I am William Shack, Chairman of the Subcommittee on Materials and Metallurgy. Graham Wallis, Chairman of the Subcommittee on Thermal-Hydraulic Phenomena and George Apostolakis, Chairman of the Subcommittee on Reliability and PRA were unable to attend this meeting, and we are proceeding with this meeting on their behalf.

The Subcommittee members in attendance are Mario Bonaca, Thomas Kress and Dana Powers. The purpose of this meeting is to discuss the status of NRC staff and industry initiatives to risk-inform the technical requirements of 10 CFR 50.46 for emergency core cooling systems for light water nuclear power reactors.

The subcommittees will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full Committee. Michael T. Markley is the cognizant ACRS staff engineer for this meeting.

The rules for participation in today's meetings have been announced as part of the notice of this meeting previously published in the Federal Register on November 6th, 2001. A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. We have received no written comments or requests for time to make oral statements from members of the public regarding today's meeting.

I guess we're going to hear an update from the staff on some of the technical work they've been doing to support the rulemaking for changes, and I see Tom King is in attendance here, and I'd like to note this is probably one of the last times we're going to get to see Tom at a subcommittee meeting, since he's going on to retirement.

We certainly enjoyed having you for many extended discussions.

MR. KING: Thank you, but you know, you may see me again. So don't be too optimistic over there.

DR. POWERS: Well, in light of his advancing years, should we get him a bottle of Geritol or something like that?

CHAIRMAN SHACK: Well, that's one of the politer bottles that you buy.

MR. KING: I'll take a bottle. It doesn't have to be Geritol, though.

(Laughter)

CHAIRMAN SHACK: Since I don't see Mark Cunningham, we'll assume that Tom King is going to speak to --

MR. KING: Yes. We were going to provide the status report on where we stand at this point. You know, the SECY paper was out. We've not received an SRM yet from the Commission, but we're proceeding as if we're going to go forward with the rulemaking, that the Commission's going to approve proceeding.

We're doing the technical work to see what would that rulemaking look like. Today's meeting is to provide you a status report on the options and issues that we're dealing with in doing that technical work and try and solicit some at least informal feedback from the subcommittee on questions, views, concerns you may have.

So with that, I'll turn it over to Mary and the others who are going to give the presentation.

DR. POWERS: Tom, is there anything going on that you might call Option 4, which would be the complete reexamination of the regulations?

MR. KING: The short answer is, yes, there is. There's a meeting this afternoon where NEI is going to come in and give us their views on this clean sheet of paper approach for -- particularly directed toward future plants.

DR. POWERS: Yes.

MR. KING: We owe the Commission a paper in June of '02 with our recommendations on whether or not to proceed to do that, and if so, what are the options. And if we can, what's our recommended option for doing that.

So there is some work underway. This is clearly as a policy question. The Commission's going to have to make a decision and our target date is June to get them something.

DR. POWERS: I think we might want to alert the Planning and Procedures Committee that it sounds like in the April, May, June time frame that we ought to try to help staff where we can on the development of that paper.

MR. KING: Yes. I think I -- in fact, I ran into Med in the elevator and I said we need to sit down and schedule several meetings with subcommittees and the full committee, PBMR, GTMHR.

DR. POWERS: Is it the type of thing, Tom, where we ought to have sort of an ad hoc committee? I mean, it doesn't really fit within any of the existing subcommittee structures. What I'm fishing around for is how to be most helpful to you and not a pain in the neck on this, because this is not a lot of time to prepare that paper.

MR. KING: Yes. The idea of the paper is to look at the pros and cons, look at the options, give the Commission a recommendation. But assuming the recommendation is to go forward, we also want to give them at least a conceptual idea of what this new clean sheet of paper approach would look like, so at least they know what they're being asked to approve.

DR. POWERS: Yes.

MR. KING: So there is some technical work that goes along with this.

DR. POWERS: Sure; sure. And we may want

to pursue that a little bit in December just to line out schedules and things like that, from our own part.

MR. KING: Okay.

MS. DROUIN: We ready?

CHAIRMAN SHACK: Yes.

MS. DROUIN: My name's Mary Drouin, with the Office of Research.

MR. KING: Who?

(Laughter)

MS. DROUIN: Cute, Tom.

DR. POWERS: Are you related to the outstanding individual that produced the ITE Insights Report that has had such a tremendous impact?

MS. DROUIN: Yes.

DR. POWERS: Okay.

MR. KURITZKY: We'd just like to get you off to a good start.

(Laughter)

MS. DROUIN: And I greatly appreciate that; really, I do. Before we get started we'll go around and let everyone at the table introduce themselves.

MR. MEYER: I'm Ralph Meyer, from Research.

DR. POWERS: Are you new in this?

MR. MEYER: What?

DR. POWERS: Are you new in this organization?

MR. MEYER: Lots of jokes this morning.

MR. KURITZKY: I'm Al Kuritzky. Work with Area Branch.

MR. BAJOREK: Steve Bajorek, Research.

MS. FAIRBANKS: Carolyn Fairbanks, Research with the Materials Engineering Branch.

MR. LAUBEN: Norm Lauben. I'm very new, about 30, 40 years.

CHAIRMAN SHACK: Yeah, I know; I know.

DR. POWERS: Well, you know, as a rookie trainee, maybe you'll listen to these experienced hands and, you know, get some insights here.

MS. DROUIN: Okay. I think the last time that we were in front of the subcommittee on 50.46, the Option 3 part, was back in the summer as we were writing our SECY paper.

And at that time, you know, we made the commitment to maintain contact throughout the program and solicit input and feedback from the committee as we move forward. There are a lot of issues associated with risk-informing 50.46.

So we did want to come in at this point, since we have been proceeding with the technical work. We had said in the SECY to the Commission that we would not wait on the SRM to continue with the technical work. We were going to move forward.

The rulemaking aspect, though, is contingent upon when the SRM comes out. So we've had about four months behind us in proceeding forward, and we thought it was very timely at this point to give you, you know, our status, what our -- kind of our early thinkings are and issues we may have come across.

So I'm not going to spend a lot of time going through the background. We'll just quickly, you know, refresh your memory of what our changes were, what our recommended changes were to the Commission on 50.46.

We primarily are going to focus on the technical work that we've been doing in support of the recommended changes we made to the Commission and then quickly go over what our schedule is at this point.

Again, primary purpose for being here is to solicit feedback from the committee and comment, one, on our overall approach because we're still -- have not quite solidified our approach. We are converging on it and we thought, again, this would be a good time.

We are encountering some, you know, technical and implementation issues that we wanted to bring to your attention and still, on some of our recommended changes, whether we still feel that they're feasible or not.

At this point, of course, we're not requesting any letter from the committee. On the background, you know, starting way back with SECY-300, which instigated the program, 264-R Plan, 86 and 198 were two different updates of the framework.

198 gave our recommendations for 50.44, and then our most recent SECY 133, which provided our recommendations for risk-informing 50.46. Now, I'll personally use the term "50.46," and when I use it, I use it loosely.

It encompasses Appendix K and also GDC 35. This here shows in a figure, an overview of 50.46, plus Appendix K and GDC 35. I think it's a good breakdown of the regulation in terms of how the different requirements are grouped.

When you come over from the right -- or from the left to the right, sorry. I still haven't learned my left from my right yet. And you look at the requirements, they tend to be divided up into what we call these four functional groups.

The first one we're looking at the ECCS reliability. Now, of course, when you look at the 50.46 and Appendix K, you're not going to see the word "reliability" there, but for example, when you look at GDC 35 and you look at the single failure criteria requirement and the LOCA/LOOP, what that in essence does in an indirect way of sitting with the reliability of the ECCS as is.

So we have those requirements and this box actually says what the technical requirements are. The next LOOP tend to deal with the acceptance criteria of the emergency core cooling system. The next group is the evaluation model.

And finally, the last one is dealing with the LOCA break size. And it's those different groups that in 133 that we made recommendations to. Now, when we go back to 133 we had two sets of recommendations.

We had some that we called short-term considerations and we had those that we considered long-term. The short-term considerations dealt with those first three boxes, looking at the ECCS reliability, looking at the acceptance criteria and the evaluation model.

In terms of the acceptance criteria in the evaluation model, those we were going in and making a recommendation to put a permanent change right into the rule. That change would, though, be voluntary. So it would be entered in through like an "or" gate.

Then an alternative to that would be an alternative on the reliability side, and that is dealing with GDC 35. We thought we could do those in the short-term, and the technical work that we had proposed on the short-term would be finished in the April and July time frame of 2002.

On the long-term considerations we felt there was still a lot more work done even to determine if the feasibility was doable. And so we were looking at the outside, two to three years to just finish the feasibility study.

And again, in doing anything that -- in terms of looking at Option 3, part 50, we have our framework document which sets the guidelines and rules of how we make the decisions that we make.

So at this point we're going to get right into the technical work we were at, and we're going to start first with the acceptance criteria in the evaluation model, and so I'm going to turn it over to Steve Bajorek.

MR. BAJOREK: Okay. Thank you, Mary. Want to just slide over?

MS. DROUIN: Want us just to change it for you?

MR. BAJOREK: Well, no. Let me --

MS. DROUIN: Okay.

MR. BAJOREK: -- let me just kind of introduce what we're going to do next. The next segment should be in a package that we just handed out. We have three presentations in this. We're going to start off with Norm Lauben.

He's going to talk about revisions to the decay heat standard, how we deal with the uncertainties. I'm going to talk about the use of the evaluation models and the impact that may have on how we do those analyses.

And Ralph Mayer is going to talk about the 50.46 performance-based criteria, and I think also deal with a couple of cladding-type of issues that have been brought up before. But Norm.

MR. LAUBEN: Yes. Let's see.

MR. BAJOREK: How's the --

MR. LAUBEN: I was happy to sit here, if you want to change the slides for me.

MR. BAJOREK: Give me your --

THE REPORTER: Mr. Lauben, can you use the microphone there?

MR. LAUBEN: Yes. I'll tell you what. Why don't you move over a chair. Then I can just use this microphone. And as long as I don't hit anyone in the eye, I can use my laser beam.

MS. DROUIN: High tech. You might set a fire.

MR. LAUBEN: Yes, right.

Carolyn, you may want to get out of the way.

(Laughter)

MR. LAUBEN: Okay. Let's see. Yes, that's -- let's see. This is what we're talking about. That's who you are and that's who I am. So we can go to the next slide. The real context for this is going to be in Steve's presentation.

So mostly what I'm going to talk about are the decay heat standards and their uncertainties and some of the issues that we have uncovered with respect to them, and how that all fits. But the context in terms of conservatism and so forth is really, really in Steve's presentation.

But so the hard part is to come after my presentation, I hope, unless you all have questions that I'm not expecting. Okay. This is just kind of a review, first of all.

10 CFR 50.46 and Appendix K was promulgated in '74, required the use as a draft, '71 ANS decay heat standard with a multiplier of 1.2 and the assumption of infinite operating time for use in ECCS evaluation models.

It's very simple. The '71 standard did virtually have a curve with a table of uncertainties that we chose 1.2 out of, and the assumption of -- you could have finite operating time if you wanted to do summation calculations, but we chose infinite operation, which made the '71 standard very simple to implement.

It was not -- there was no difficulty at all in that. Anyway, number one, the research and analysis since 1973 has shown that the most significant conservatism in Appendix K is the decay heat requirement.

The 1988 ECCS rule change allowed use of a realistic evaluation model analysis option with an uncertainty evaluation. In other words, instead of using the conservative Appendix K, now you can use the best estimate option.

So there's always two choices. One is the best estimate option that was allowed since 1988. The other was the Appendix K conservative option which was the only option from 1974 to '88. But then after '88 it was still grandfathered. So licensees still have a choice today of whether they want to use the best estimate option or the Appendix K option.

Regulatory Guide 1.157, which accompanied the '88 rule change, declared the acceptability of using the '79 ANS decay heat standard for a realistic option.

It said there's a few more physical things that you need to consider, such as neutron absorption efficient products and things of that sort. So it says it's now become a little bit more complicated.

The '79 option now has three isotopes -- fissionable isotopes that you have to worry about, not just one. So it's now more complicated. It has three different ways of applying the standard. You can use summation calculations -- excuse me.

Yes. You can use a summation calculation for groups of decay products and you can also use some integrated values for post-fissions, or you can use integrated values for infinite decay heat and a summation methodology where you can change that infinite irradiation into finite irradiation.

So there's lots more choices that you have to use when you start to use the '79 or then eventually the '94 option. But okay. Regulatory Guide 1.157 only applies to the best estimate option.

That Regulatory Guide does not apply to Appendix K. Appendix K is self-standing. There's no regulatory guides associated with it today or in the past. It's just -- it's self-standing. You abide by those rules and that's it. So 1.157 applies to the best estimate option.

There's nothing to prevent a licensee or an applicant from using all or part of an even newer standard, the 1994 decay heat standard today, if you want to, because there's no -- there are -- there's really no requirements to -- as it says in the second bullet.

The only technical requirement in the realistic option has to do with the things that Mary has addressed, i.e., the break spectrum in GDC 35. Otherwise -- and those things apply to both the best estimate and the realistic option. Okay. Back to --

CHAIRMAN SHACK: The current best estimate models have been improved and they're really based on the '74 guide or --

MR. LAUBEN: No. No. No. No. No. No.

No. No. No. No. Oh, excuse me. No. No. No. What I said was that Reg. Guide 1.157, that's the guidance as to what's acceptable to the staff with respect to the best estimate option.

The best estimate option in 50.46 doesn't say very much. It says, do a best estimate with a high degree of certainty that your peak clad temperatures won't exceed the limit. That's really what it says, or that the criteria won't be exceeded with a high probability. That's really all it says.

DR. KRESS: Does it explicitly call out 95.95?

MR. LAUBEN: No, it does not. That's called out -- the 95 percent probability is called out in the Regulatory Guide.

DR. KRESS: Yes, okay.

MR. LAUBEN: Not in the rule. The rule doesn't say anything about that. The rule just says, high probability that the criteria won't be exceeded. You have to go to the Reg. Guide before you first see the words, "95 percent" used.

DR. KRESS: So if somebody wanted to, they could come in with less confidence level if they could justify it?

MR. LAUBEN: If they could justify it.

DR. KRESS: Had a reason for it?

MR. LAUBEN: That's true of any regulatory guide. You don't have to abide by a regulatory guide. You can -- it's something that's acceptable to the staff, but if you want to do something else, risk the prolonged review that would be required for something that's not in a regulatory guide, you can do it. That's the rules.

DR. KRESS: Okay.

MR. LAUBEN: So at any rate, the Regulatory Guide's been in place since '88 for best estimate option. And it did say that the '79 standard was acceptable. That's because the Regulatory Guide came out in '88.

The '94 standard wasn't available at the time, obviously. Okay. The last bullet. The '94 ANS-5 standard is potentially more accurate and less conservative than the '71 draft standard, but requires more choices to be specified by the user, as I mentioned.

Instead of three fissionable isotopes, the '94 standard has four fissionable isotopes. Still a lot more than the one that was implied by the curve that was in the '71 standard. So there's much more to be -- choices that you have to make when you're using the more modern standards.

More choices in '94 than there was in '79, and certainly, many, many more choices than you had to make from '71 standard. In fact, the '71 standard had so few choices that the options could be contained in two or three sentences in Appendix K.

Okay. If NRX makes it -- okay. Here's the problem. You have a lot of choices now that you have to make with the '94 standard, if you want to use that. So the question is, who's going to make the choices?

If NRC makes the choices ahead of time, that may make life easier, but it also means that we would have to make choices that would conservatively bound any number of things that you have to consider when you're applying the decay heat standard.

So let's see. What is -- let's see. If NRC makes a choice -- yes, right. Okay. Anyway, if NRC makes the choices, however, it's likely to make the process more predictable and stable.

That is, if the choices are made at a time by the NRC, no one argues with them, then there's no -- there's very little potential for review, now, when somebody comes in and says, I'm applying the '94 standard and here are the things that I choose to implement out of that standard.

If each applicant or licensee selects the options, then obviously, there's a lengthy review process involved. Okay. Now, here are --

CHAIRMAN SHACK: Is there really a lengthy review process? I mean, is it --

MR. LAUBEN: Yes, there could be. It depends on -- it would depend on how the licensee or applicant came in and decided to implement the standard. He may be very -- he may want to get a lot out of this, so he may be very tight in how he defines his operating cycles, because that's one of the things that you have to choose in here.

Or he may want to do a bounding histogram that he could have, in which case it might not be. It just depends on how much margin he's trying to shave by using it, and that's the point of us making some of these choices first.

And I can go through -- these are the six that I identified as being the most important choices that you have to make. Operating time. Well, in the old standard infinite operating time is easy.

That's conservative. It's easy to use. It actually reduces -- it reduces the complexity of your uncertainty analysis. It makes life very easy and it's obviously a conservative assumption, too. But like I say, if you wanted to use a bounding histogram of operating cycles, you could do that.

But if it was tightly bound then you may run the risk of it doesn't apply to the next cycle of operation or something like that. And that's part of the problem, how tightly do you want to do this?

You going to leave it up to the individual licensees or are you going to leave it up to -- or should the NRC decide ahead of time? Part of the reason I'm bringing this up is that -- and we didn't mention this, but there was a petition by NEI to use the '94 standard, just use it.

Well, they didn't say how they would use it. They just said use it. So that -- the implication is not clear. Do you mean for the NRC to make choices ahead of time, or do you choose to come in and make your own choices?

And if you're going to make your own choices and each licensee makes a different choice, then it does increase the potential for review.

CHAIRMAN SHACK: Let me put the question a different way. You know, suppose you're just using this to determine that decay heat --

MR. LAUBEN: That's all --

CHAIRMAN SHACK: -- that seems to me one question. Well, is in fact -- I mean, do you feel that your calculation of decay heat is now covering some other non-conservatism somewhere else? I mean, that would seem to me --

MR. LAUBEN: Yes.

CHAIRMAN SHACK: -- the difficult thing to assess when you're trying --

MR. LAUBEN: Indeed

CHAIRMAN SHACK: -- to trade these off.

MR. LAUBEN: Indeed.

CHAIRMAN SHACK: Just looking at the decay heat --

MR. LAUBEN: Yes.

CHAIRMAN SHACK: -- by itself I would think --

MR. LAUBEN: Yes. Yes.

CHAIRMAN SHACK: -- looking at these would be a relatively straightforward thing.

MR. LAUBEN: Relatively straightforward thing. I agree. And Steve's going to address the other part.

CHAIRMAN SHACK: Right. Okay.

MR. LAUBEN: I left the hard stuff for him. But indeed, you're right. It should be relatively straightforward, but the point is, even -- there are still some issues that need to be addressed, just to do the straightforward part.

And that's what I'm attempting to address here, that in the petition there was no mention of how you even deal with the straightforward part. So I bring these issues up here. I've brought them up in a couple public meetings in the past, but there didn't seem to be as much interest then, however. Okay.

Okay. Second one, fission fractions per isotope. Well, this requires some -- okay. Well, like I said, the '71 standard assumed ^{235}U only. Three additional isotopes in the '94 standard; fission fractions vary with time and space.

You need a physics calculation to determine what those fission fractions are for each isotope. They vary with time. They vary with space. They're burnup dependent and enrichment dependent.

So it's not -- you know -- it's not a straightforward thing. You can make simple choices, simple bonding choices like all ^{235}U. That's a simple bonding choice. Okay.

Neutron capture. This effect was added in '79 and '94. The effect is burnup dependent and adds to the decay heat. There's also some uncertainty in it, although the amount is low until you get up to beyond the times of interest that I think we're interested in, like 10,000 seconds or so.

However, it's still something that needs to be considered and addressed, and you can choose times that are very high and then, you know, you're conservative, because it's a T to the fourth effect in the equation. It's there for neutron capture.

Okay. Fission energy. Each fissionable isotope has different recoverable fission energies. The standard in the past has always been to shoot 200 MEV per fission, because that's conservative. You can actually reduce conservatism by using higher values because that number appears in the denominator.

So you can, if you can justify it, choose other numbers for other fissionable isotopes. So that's another choice you have to make, or somebody has to make, either the NRC ahead of time or the licensees or whoever.

Okay. Actinide heavy element decay. The same basic equations are in all three standards for actinide decay. However, required ^{235}U fission yield is not specified and is burnup dependent. It was not even specified in the '71 standard -- excuse me -- it wasn't even specified there.

It was assumed I think that the value was .7, but I don't -- you can -- I can't find the documentation of it anywhere. In the examples in the '79 and '94 standard I think it was like .7. There are codes that use a default value of one. But the point is, it's another choice to be made.

Okay. Tabular data. As I think I mentioned earlier, there are three tables now for each fissionable isotope, four fissionable isotopes. That means you have 12 tables. You have 12 tables you can go to and that depends on your method that you choose to evaluate the decay heat.

That's quite different than just one table that you had in the previous '71 standard. Okay. So those are the key choices you have to make. At least -- like you say, they're not -- they don't have to be that difficult.

They can be chosen in a bounding way, but if you want to reduce your conservatism you may not want to make them quite as bounding as somebody else might want to make them. Okay. So that's it. I guess we can go to the next slide, then.

Okay. The issue here is uncertainty and conservatism. Well, as we all know, now that we have a decay heat standard that has many more variables in it, your uncertainty analysis becomes a different issue than it did before.

There are RMS equations or something like that, that you have to go through and use to determine your overall uncertainty in this. There are uncertainties with two of the table types, the post-fission type and the infinite irradiation tables, that could be used.

They're different because they come out with different values when you do this. But in addition to the uncertainties in those tables for those methods, you now have to look at other uncertainties like uncertainties in power, or any of the other variables that we're talking about, uncertainties in fractions of fissionable isotopes.

So all these things now have uncertainties, uncertainties in neutron absorption. What if -- although the standard says, don't bother doing that because we've picked conservative values for you. So but the point is how to deal with uncertainties now becomes an issue.

It wasn't an issue before, but how to combine them and deal with them does become an issue. There are equations in the standard, however. Let's see. Let me see. I don't want to get ahead of myself here.

Okay. Oh, yes, okay. Let me not get ahead of myself then. Okay. Bullet number 3 here. Use of the '94 standard with nominal inputs and uncertainties could result in a substantial reduction of overall conservatism in the Appendix K analysis.

And number 4, thus the magnitude of one or more non-conservatisms is too large. If it is, the appropriate overall conservatism may be in jeopardy. I think this is Steve's presentation. I'm jumping the gun a little bit.

But the point is here, is that if you now reserve the conservatism in your analysis you now have to worry about those other things that create uncertainties in your analysis that you didn't have to worry about before, because you don't have a bounding, conservative -- it may be in jeopardy.

Let me just say that. It may be in jeopardy. Okay. The current version of Appendix K makes no break size distinction concerning application of the decay heat requirement. Longer transients, such as small breaks, would derive substantially larger benefit from a reduction in decay heat, compared to faster large breaks.

Large breaks, some of them, depending on

-- depending on a particular plant that you're looking at, some large breaks can be over so quickly, peak clad temperature can turn around so quickly, that it's virtually a stored energy issue and not a decay heat issue.

So large breaks that turn around quickly are going to be -- are not going to be decay heat dominated the way some small breaks that may be uncovered, albeit later, for a longer period of time become much more decay heat dominated.

Among the required features of Appendix K, decay heat is the only one, except for the ones that Mary talked about, i.e., the break size and the GDC 35 types of things. The only thing that really applies to small breaks in Appendix K specifically is decay heat.

Or let me put it this way, largely. There are some other things that can influence it, too, but I mean it's by and large virtually the entire predominant feature of Appendix K, is decay heat for small breaks.

Okay. RES is evaluating -- okay. Number 5 -- or 6. This may be somewhat new. We are evaluating potential errors in the uncertainty methods in the '79 ANS and '94 standards. Therefore, previous sensitivities may not be appropriate.

Some of our assessments before -- the uncertainties -- in other words, some of the equations, I don't want to say for certain that they're wrong, but I've had a number of physicists and statisticians tell me that they are.

But wrong may not be a good adjective. It may be that they were -- that they used methods that were designed to enhance the uncertainty, and that it has to be looked at more carefully. I think in fact we've talked with the ANS Standards Subcommittee on this subject and they -- those members agree also.

They believe that this should be fixed. So our choice is either to convene a group of experts under ANS and work this through or make the exceptions ourselves. I tend to -- would tend to prefer to work this through the ANS committees.

I think that's a better way, get a new standard out which everyone agrees on these methodologies and so forth. So that's something, like you say, not necessarily that difficult to do, but still something that you sort of need to establish your baseline by doing these straightforward things first.

And I think that's what we -- so that's

what we mean when we say, number 7, we're going to do some additional work. We need to get some of these things straightened out.

And now, as I said a couple times, number 8 there, the context of the decay heat work and the -- is really the basic subject of Steve's presentation. So I don't know if there are any other questions or not. Okay. Thank you.

MR. BAJOREK: Those go back to Norm. Thank you, Norm. Where I'm going to pick up now, then, is if we start to change the decay heat model, going from '71 or '79 to something that's technically better, what are the consequences that we're going to see in two different evaluation models that have been presented to the staff, the classic Appendix K model and how this might impact best estimate analyses, as well.

Our directive to do this comes from the SECY-01-133, and what I've summarized here in the first three or four bullets are some of the items that specifically discuss this.

What basically 01-133 asks us to do is to take a look at Appendix K, identify those models which are unnecessary conservatisms, come up with revisions to them, but also keep in mind that Appendix K may not cover everything.

We do have new knowledge in front of us and there may be issues related to Appendix K where there may be non-conservatisms involved. So we've been asked to look for the unnecessary conservatisms, as well as look at features that may not be appropriately counted for.

DR. KRESS: Is there such a thing as a necessary conservatism?

MR. BAJOREK: I think there is, and I think Norm hinted on it. We see that the '71 decay heat model is very overly-conservative technically, compared to what it should be to an accurate correlation.

But that excess conservatism has been used in evaluation models to account for other things. It forgives a lot of sins, okay, uncertainties in other models, processes where you may have questions, but from a regulatory viewpoint you may feel comfortable with because you know there's so much conservatism in the Appendix K, as well as in things like the single failure criteria.

I'm going to talk about a few things that we would consider as non-conservatisms in a couple of minutes here. Principally, the focus of the efforts in our branch have been by the norm to take a look at the decay heat standard itself, what its implementation should be, and then to look at options to deal with incorporation of a revised decay heat standard and how we should deal with some of these things that we're terming as non-conservatisms.

Well, I think the first question is, well, what is this thing that you would refer to as a non-conservatism? And I would say that there are three potential sources. First, there may be models in Appendix K that even though they're intended to be conservative, later information has shown them not to be.

Now, the only example that we're aware of is in the case of the Dougal-Rohsenow Model for post-critical heat flux heat transfer. Information that was uncovered in the '70s and '80s showed that it was non-conservative.

And in the '88 rule change there were restrictions placed on the Dougal-Rohsenow Model. It could only be grandfathered and if there was a change to the analysis, you had to justify its conservatism.

And to my understanding, most evaluation models have basically replaced that at this time. So that's not really an issue anymore. However, Appendix

K, while it gives prescriptions for several thermal-hydraulic models, it doesn't account for everything.

And there has been the concern that these models, which have not been specifically discussed by Appendix K, may have a large uncertainty when they're applied in an evaluation model. This was the focus of another SECY paper, 86-318, that actually looked at an issue very similar to what we're doing right now.

The premise for 86-318 was reduce the decay heat by changing to an updated standard. I think they were looking at the '79 standard at that time. And the conclusion at that time was that, no, that was not a good thing to do unless you accounted for uncertainties in the other thermal hydraulic models.

Now, it having been written in 1986, you can see that this was basically a formulation for the 1988 rule change and the best estimate rule that required people to actually address these uncertainties.

But it still remains a concern that if we start to do things with Appendix K, we still have to do something to look at those uncertainties and assure ourselves that there is some conservatism remaining in that type of an evaluation model.

The third source of potential non-conservatism are things that have arisen out of thermal hydraulic test programs that were conducted in the late '80s and the early '90s. These are processes -- we'll talk about those in the next overhead.

These are processes that were identified in the test programs that Appendix K didn't know about in 1971, and had basically fallen through the cracks. SECY 133 is asking us to identify what some of these processes are and make sure that they are at least compensated for by any revision in the decay heat standard, plus some adequate multiplier.

A couple of examples that are considered non-conservatism, and these are large break models. The first one to refer to is downcomer boiling, and this is something that was seen in the 2D/3D Program, CCTF, SSTF that were run in Japan and also UPTF that was run in Germany in the last '80s, early '90s.

They noted that some of the heat transfer that occurred in the core wasn't as good as they anticipated it to be. They thought the reflood rate was going to be a little bit higher. In looking at some of that data, part of it was attributed to things that had gone on in the downcomer.

Later in the transient the fluid begins to boil, takes awhile to get that energy out of the walls. This voids part of the downcomer and two things go on. One, you reduce your gravitational head that drives fluid into the core.

And secondly, when that downcomer fluid frosts up, part of it gets pushed back out the break. So your result is a lower collapsed level in the downcomer than you would have if you had made the assumption it didn't boil and you were full up to the cold leg.

Now, typically Appendix K evaluation models don't really account for this. There are two reasons. One, the models themselves are based on equilibrium models that stem from work in the '60s and early '70s.

They assumed that both the vapor and the liquid were at the same temperature. Boiling does not begin until you brought everything up to the saturation temperature. Subcooled boiling, however, is well-recognized to begin while you still have some subcooling remaining in your bulk fluid.

So we know it will begin earlier. This is also complicated by the simplified nodalization that's used in many of these Appendix K evaluation models. They lump everything in the downcomer together, as opposed to allowing a thermal stratification to occur, which is put in most of the realistic codes like RELAP or TRAC or COBRA.

Now, the most vulnerable plants are ones that have relatively high power, low containment pressure. The large break transient extends for a fairly long period of time. So you're depending on your downcomer driving head during -- after your accumulator period, okay, when boiling may occur to start to recover your core.

So the longer this transient proceeds, the larger this defect is going to be. The other one --

CHAIRMAN SHACK: Would this be a problem even with the current best estimate models? Would they account for this?

MR. BAJOREK: They account for it. Most of the realistic codes have what they will call mechanistic models for subcooled boiling. And what this allows you to do is to boil in the downcomer well in advance of the time when everything is at saturation.

In fact, this is how this issue started to arise, because in the realistic calculations people were noticing, hey, there's not as much margin here as everyone had hoped, and in looking at those calculations the cause was this downcomer voiding that was going on in the calculations.

Let me show you an example of what goes on. This figure is from the calculation using COBRA/TRAC for a combustion engineering system 80 plant. The plant in this case is at 3800 megawatts, sort of a standard 3800 system 80 design.

What's shown in the figure is the upper curve, the saturation temperature, and the lower, which is the liquid temperature in the downcomer. Now, early in time at 50 seconds or so, the accumulators come on.

In this plant the accumulators are enormous with respect to other types of designs. So you get a very high amount of subcooling early in the transient. Well, eventually enough heat comes out of the vessel wall and core barrel, so at roughly 180, about 180 seconds or so into the transient the downcomer is effectively saturated.

This figure shows the collapsed level in the downcomer. Now, early on at 50 seconds or so, that's when the accumulators are active. They just fill the downcomers all the way up to the spray nozzles and up into the cold legs.

At by about 100 seconds, the level in that downcomer is about to the bottom of the cold light. So they're effectively filled. Liquid is still subcooled at that time. If you recall from the last figure, saturation occurs at about 180 seconds.

And what you notice in the collapsed liquid level is now a fairly significant decrease, dropping from cold level to two or three meters lower in this calculation. Eventually, your pressure decreases, the pump's safety ejection comes on and you recover your downcomer level and subsequently the core.

Now, on the system 80 3800 megawatt plant, which this calculation is for, there's not a real big concern. This shows the peak cladding temperature in the core. The reflood peak is reached shortly after the accumulators inject.

Turned around, the core quenches by 140, 150 seconds. So with regards to downcomer boiling it's basically no harm, no foul. Downcomer boiling doesn't take place until after you've gotten the energy out of the core, not much of a concern.

Well, now let's try to uprate the unit. Now, to do this I took calculations that had been performed for a system 80+, effectively the same geometry. There is a difference in downcomer injection location, but if I compare the transients, I basically see that same accumulator effect.

We fill the downcomer, okay, and the downcomer again boils around, oh, around 150 seconds, a little bit earlier, because now we're looking at a plant 3800 to 3914 megawatt thermal. Now, the energy has not been removed from the core by the time downcomer boiling begins.

And you see that rather than the peak cladding temperature decreasing, as it would have at 150, 160, it starts off and reaches a second peak, in this case at about 400 seconds. Quench of the core because of the reduced reflood rate does not occur out until roughly 900 seconds or so.

The transient is significantly prolonged because of the downcomer boiling process. We'll get back to the net PCT effect in a second, but let me mention the other non-conservatism that has been talked about for several years, and this one's referred to as fuel relocation.

During the transient the clad will swell at several locations, usually just below your peak temperature location. Experiments that have been run in this country, in France, in Germany, have found that upon ballooning, the pellet fragments above the ballooned location relocate and move into this balloon zone.

Now, if you're doing a calculation and you don't account for fuel relocation, you have a situation where you have a stack of pellets with the balloon clad. The clad effectively behaves like a fin. Ballooning in some ways is good for the clad because you remove it from the heat source, push it off into the fluid where it gets effectively good cooling.

That's why you really shouldn't see your PCT location at the burst or the balloon location. However, the concern from these experiments is that upon ballooning you don't maintain a concentric stack of pellets.

The pellets are fragmented due to prior operation. They come down, increase the local power and increase the pellet to clad gap conductance, which would significantly increase your PCT. Now, one of the things that we've been working on has been attempting to get estimates of what does all this mean.

If we change the decay heat from '71 to '79 or '94, how does that impact the analysis and how does these non-conservatisms stack up? Decay heat, we've gotten estimates from two different sources. Westinghouse had a meeting with the staff about a year ago and they were asked their estimate of what would happen if they took their Appendix K model and reduced the decay heat to the '79 standard.

Their estimates were 250 to 340 degrees, and there had been some calculations done to substantiate those numbers. We also had one of our contractors modify a RELAP to do a similar calculation. They looked at an older vintage combustion engineering unit.

I think it was Millstone or Calvert Cliffs.

MR. LAUBEN: Millstone.

MR. BAJOREK: And they estimated that it was 372, so more or less consistent with the Westinghouse estimate, 3- to 400 degrees due to the decay heat relaxation. Both also estimated what would be the benefit of going from Baker-Just to Cathcart-Powell for the metal heat reaction.

Smaller in effect, less than 100 degrees, and you can see the estimates of 50 and 75 degrees. Now, for downcomer boiling and relocation we've gone to information that the vendors have provided us, information that has been obtained publicly.

We have three estimates for the downcomer boiling. Westinghouse took a look at their calculations for a four-LOOP ice condenser unit and they estimated that the penalty by accounting for downcomer boiling in your calculation relative to ignoring it was roughly 400 degrees.

ISL did some RELAP calculations for us, again taking a look at a system 80+ unit. This is similar to what I just showed you, but their interest there is looking at the Korean next generation, which is even at a higher power than the Palo Verde and the powers that we're looking at for a system 80+ in this country.

Now, their estimate was 700 degrees, or I should say that's our estimate in taking a look at the first reflood peak and the second reflood peak. What I think you should gather from that is a few hundred degrees.

I don't think it's 700 degrees. I think that's a RELAP problem. The interfacial drag is too high. We've seen that consistently in other RELAP calculations. Once you get some bubbles it pushes up far too much liquid into the downcomer.

Okay. So I think that number's too large. The calculation that I showed you there in the last four figures, if I subtract away a power effect and look at the difference in PCTS and attribute that to the downcomer boiling, my estimate was slightly over 300 degrees on that one.

So for at least for the downcomer boiling we're seeing three separate organizations using different relisting codes, all basically agreeing that downcomer, it's not a 10 or a 20-degree effect. It's something larger.

And those values, three, four -- three or 400 degrees are basically on the same order of the decay heat change that would be envisioned for Appendix K models that do not account for this process correctly.

Fuel relocation. A technical paper was written by the French and they took a look at the experiments. They estimated some filling fractions of the balloon region, did some CATHARE, which is considered a realistic code for relocation versus not accounting for relocation.

Their estimate -- or I should say our estimate comparing their numbers translates to 313 degrees by accounting for this fuel relocation. Those are the two that we see public information, we've heard talked about at technical meetings and the vendors have made us aware of.

They know that some of these are going on. It's a question on how you should really deal with those. We don't want to say that we're clairvoyant and we k now everything with respect to the non-conservatisms.

So I made a couple of phone calls to a couple of university professors; what would you consider a non-conservatism. We got a few things. Some fit into Appendix K. Some really don't. They're more plant condition issues.

An example may be, do you account for secondary to primary leakage during a LOCA. We know that during steady-state operation there is a amount of leakage allowed from the primary to the secondary side.

Well, if you account for that during an analysis you would be increasing the amount of steam binding, okay, and potentially having a penalty. I guess my point on the final one there is even though we have a list of several things that we would account for, we would consider non-conservatisms, we still feel we want to do a little bit more work, not going on a witch hunt, but trying to make sure that we are at least informed on things that are recognized as these major non-conservatisms.

DR. POWERS: You have looked or reported some examinations of a comparison between Cathcart-Powel and Baker-Just for the parabolic reconstants. Has anybody looked at what effect would happen if we had deviations from parabolic?

MR. BAJOREK: I'm not aware of it, but Ralph is best to answer that one.

MR. MEYER: No, I don't have that.

DR. POWERS: It has always struck me that one of the best justifications for using Baker-Just in the face of several examinations that took place later that suggested it was quite conservative was the fact -- a couple of things.

One, you really don't know the surface area that you're oxidizing, and the second loophole is that we usually calculate these things in a fairly stylized fashion and don't calculate the epitaxial stresses that arise in cylindrical coordinates that might cause delamination of the oxide, locally if not globally, especially around things which cause deformations of the ballooned region around grid spacers and the like that would cause a deviation from strictly parabolic kinetics, and that because those things were challenging to do you just used Baker-Just to cover your ignorance there.

But I don't know that anybody has ever gone through and looked and said, how much ignorance are we covering.

MR. BAJOREK: Right. Ralph.

MR. MEYER: Well, we are going to look at that in the work that we're doing at Argonne right now. I would say that based on the early results that are now coming out of the program, I don't expect to find much here because what we have found in the last couple of months for the BWR high burnup rods that have undergone oxidation kinetics measurements, the oxide layer doesn't seem to have any protective effect in altering the rate of oxidation.

And our results for the high burnup cladding appear to be virtually identical to results for fresh tubing. Now --

DR. POWERS: Well, I guess the question I would ask is, have you ballooned that cladding around a grid spacer and can you come to that conclusion?

MR. MEYER: Have we -- well, so far we haven't made measurements with balloon cladding, but we are going to make measurements with balloon cladding. Now, you're asking about, is the location of a balloon close to a grid spacer and I can't tell you the answer.

What I can tell you is that we are discussing right now whether the upper fasting point for the fuel rods should be shaped like a grid so that we would get any grid effects on this.

But I think the best answer we can give, and I think it's an adequate answer, is that we are in the process of testing under conditions that are just as prototypical as you can imagine, and we would be able to detect any deviation in the oxidation kinetics that results from the deformation and related processes, like flaking off of the oxide, because we will have at the outset very careful measurements of oxidation kinetics on undeformed irradiated tubing in order to compare with the more integral tests that we're going to do.

DR. POWERS: Have you tried to determine the conditions by say modeling or some sort that would optimize the conditions for delamination of the oxide? I know that the French have set up what looks to me to be a relatively impressive model of those epitaxial stresses.

I have never taken their model and tried to say, okay, now, what kinds of things lead to high strains and stresses at the interface that would cause delamination? But it looks like they have one that's sophisticated enough so that you could do that sort of thing.

MR. MEYER: Well, we don't have a model with that level of sophistication, but I don't see why you couldn't get that information more reliably from a test rather than a model.

DR. POWERS: Well, I mean, the problem is -- I mean, maybe you could if you're fairly imaginative in the testing capabilities. It's just that I worry that you can't test all -- I mean, I'm just not smart enough to participate.

MR. MEYER: No, that's true, but the indications right now are that the oxide isn't going to affect the oxidation rate, that large amount of oxide that has accumulated from corrosion and is present at the time of this ballooning deformation.

Now, remember, the ballooning deformation occurs at a relatively low temperature. So you haven't gotten into the high temperature oxidation region where you're going to build up 17 percent.

The ballooning deformation is over with before you ever start accumulating the large amounts during the high temperature portion of this transient. So you're really only talking about the spallation of oxide that's on there from the corrosion process during burnup operation.

In that part of it there's already a preliminary indication it doesn't make any difference.

CHAIRMAN SHACK: Well, of course, that BWR cladding has a relatively thin oxide --

MR. MEYER: It has a relatively small amount of oxidation, that's absolutely true. It has only seven to ten microns of oxide on it, and we have PWR cladding that we're going to test soon that has 100 microns and even more than that in some locations.

So you know, I can't say that this is a general observation, but there is an expectation that even the heavier corrosion layer thicknesses, that it's cracked and it's pervious. Is that the right word? The opposite of impervious.

DR. POWERS: I mean, the challenge one faces in this is that when we look at analog systems with fluoride structure oxides on metal surfaces, the analog systems that come immediately to mind are things like cerium metal, uranium metal and plutonium metal.

In every one of those cases they suffer catastrophic delamination of the oxide at very thin levels, and zirconium just doesn't do that.

MR. MEYER: Yes.

DR. POWERS: And hafnium even more doesn't do that. And you keep saying why, and what is it that will cause delamination of this oxide? Is it so extreme that it's outside the range of things that you can have during a reactor upset condition?

And I don't know the answer to these things but I keep struggling with it, because I can't keep my oxides on plutonium, so why are you guys with zirconium so successful at keeping your oxides intact?

MR. MEYER: Well, I haven't suggested that it won't spall. What I have suggested, that it won't make any difference if it does spall. And I think we will be able to see both, whether it flakes off during the deformation process and if it makes any difference.

CHAIRMAN SHACK: Steve, just --

MR. BAJOREK: Sure.

CHAIRMAN SHACK: -- when I look at this kind of one at a time thing it sort of suggests to me that I'm not going to get anything from a best estimate calculation, that everything's going to wash out. That doesn't seem to jibe with experience.

MR. BAJOREK: The penalties that you see for the realistic calculations are relative to what you would have gotten from that calculation without conservatism boiling. For this one, the peak cladding temperature was probably around 1900 degrees.

So there was a benefit in there. I mean, it came due to relaxing the decay heat, okay, but if you had ignored the boiling processes in the downcomer you would have been dealing with a peak cladding temperature 15 -- you know -- 1500 degrees or so.

So yes, there is a reduction, but the net reduction isn't as large as what had been anticipated. These numbers, by the way, are for large break. Now, I've got some numbers, some estimates here for small break, but the situation there is a lot more nebulous, quite likely because most plants are large break limited, or I should say, the vast majority are vast break limited.

There hasn't been a tremendous amount of work looking at the sensitivities with regards to small break. Estimates that we have gotten from -- basically one of our contractors looked at the issue and did some of their calculations, found some information from Combustion Engineering and Westinghouse that basically estimated close to 1,000 degrees, 800 to 1,000.

Their calculations, based on RELAP, were showing another range, five to 1,000 degrees by going from '71 to the '79 standard. I don't have an estimate for the metal water reaction.

If you were to do that separately, my guess it would be larger than what you would see for a large break, because it occurs over a much longer period of time, but we're not aware of any unique sensitivity studies at a high enough temperature whether it would an effect.

MR. LAUBEN: It is about the same. I did some with -- for the 2700 megawatt CE plant and it was about the same.

MR. BAJOREK: Okay.

MR. LAUBEN: The same 50 to 75 degrees.

MR. BAJOREK: Okay.

MR. LAUBEN: Because intransient's usually a little bit slower.

MR. BAJOREK: Well, you have lower decay heat at the time.

MR. LAUBEN: Right.

MR. BAJOREK: So it's --

MR. LAUBEN: Right.

MR. BAJOREK: Okay. Let's go. Now, some of the things that we might want to consider as non-conservative issues, we've seen some cases with nodalization, where whether you uniquely look at cross-flow into the hot assembly.

I think, Norm, you have these RELAP calculations that show you get a 600-degree effect. And this kind of goes back to the idea that these codes were written '60s, '70s. You were -- had them on CRAYS, 7600s, and you tended to want to simplify your nodalization compared to what you could do so.

So by incorporating more complexity into your model you start to see more variations, simply because of the number of processes that lie cross-flow that you would take into account.

Now, one that has been recognized in the past has been the consequences of operator action during a small break. Right now, you don't have to worry about it as much in small break if you have the loss of off-site power, the pumps trip on reactor trip.

If you have off-site power available, which is another one of the avenues that is being pursued under risk-informed regulation, now you have to depend on the operators to trip the pumps according to their EOPs.

This usually calls for them to recognize that the rods are on the bottom, you've got a safety injection pump and that you've lost, by looking at your monitors, lost some cooling into your hot leg. Once you recognize that the reactor pumps are tripped.

Now, this is going to depend on how quickly they go through the EOPs, their recognition of these various signals while there's a lot of confusion going on. Calculations that had been performed in the mid-80s looking at this, found that there were periods of time where the operator could trip them while you had a plant at an elevated pressure and lost inventory.

Then if you tripped the pumps you collapsed the froth over core -- over the core, you're still at high pressure, meaning you weren't getting as much safety injection into the system, and could get a very high peak cladding temperature.

So it's one of the things that may need to be considered. The other two have more to do with model and correlation uncertainty. In taking a look at the decay heat sensitivity, the contractor noted that, hey, being off just a couple of inches in your level swell, where your froth level is in the small break, can result in several hundred degree increase or decrease in what your peak cladding temperature is.

LOOP seal clearance refers to the effect out in a small break when steam starts to slip through one or more of the LOOP seals of a plant. This redistributes the fluid. Some goes out the break.

Some goes to the vessel; some stays in the LOOP seal. And what happens then is you get a different two-phase hydraulic loss through the LOOPs. If that loss is high you tend to suppress the core level much more than if you had a nice, clean blow in that LOOP.

People who have tried to model this in codes have had a fairly difficult time doing this. I don't put a basis down on this because I'm talking a little bit more from personal experience in developing a small break evaluation model.

This was a very complex issue when we saw hundreds of degrees of variation. We have also seen some experimental tests that have raised it as a concern, mainly due to LOOP seal replug. Some of the ROSA tests said that you've got very good heat transfer in your steam generator.

What that means is later in the transient you can put enough condensate into the LOOP seal to replug it, force it to blow again. So in terms of the non-conservatism, it's something that we feel would at least have to be looked at in terms of the consequence of reducing decay heat if we're going to be relaxing the amount of conservatism that we see now.

The next thing that I want to move into are options that we're currently looking at. And I want to emphasize that we have not reached a staff consensus on which option should be pursued.

MR. BONACA: I have a question on one of the thoughts just --

MR. BAJOREK: Sure.

MR. BONACA: You just made a pretty strong case for some of the conservatisms that you have in Appendix K, I mean, in the tradeoffs. And in the best estimate, when you do best estimate calculations, do you have -- there is no modeling of downcomer boiling in best estimate testing.

MR. BAJOREK: In best estimate you do.

MR. BONACA: You do?

MR. BAJOREK: Yes.

MR. BONACA: Okay. That's one of the -- what I --

MR. BAJOREK: The two fluid codes would take a look at non-equilibrium.

MR. BONACA: Okay. So yes, all right.

MR. BAJOREK: Phases that allow voids to develop. The RELAP, as we noted --

MR. BONACA: Yes, that's right.

MR. BAJOREK: -- in the calculations we think isn't doing it very well, but it's doing it very conservatively, which gave that 700 number. We think that the COBRA formation is maybe doing that in a more kinder, gentler fashion, but it's still significant, three to 400 degrees.

MR. BONACA: I guess the point I'm making, it would be interesting to have a comparison of these effects also for the best estimate so we could have an understanding of what tradeoffs have already occurred.

And now in the best estimate modeling do you still -- are most -- what's happening to the decay heat curve? Which one is being used?

MR. BAJOREK: Usually, the '79.

MR. BONACA: Seventy-nine.

MR. BAJOREK: Yes.

MR. BONACA: Not '94 effect.

MR. BAJOREK: No. In fact, what I wanted to point out with this overhead -- this is not in your package -- but to point out the work that we need to do in coming up with an option for Appendix K and a revision to the decay heat, just to make sure that that revision satisfies a new option for the Appendix K, but also addresses some of the issues in a best estimate model.

And I think as you noted and as Norm noted, for a best estimate evaluation model, Reg. Guide 1.157 simply says for decay heat, calculate in a best estimate manner. It considers by way of a reference that the '79 decay heat is acceptable.

Now, you could take that Reg. Guide at this time and use the '94, but it's certainly not clear to anyone that goes through when we're developing a model. Perhaps even a little bit more cloudy is the metal water reaction.

Again, the Reg. Guide says to calculate it in a best estimate manner and it cites Cathcart, et al., Cathcart, Powel and who else may have been on that, their data is acceptable and doesn't even cite the correlation.

It just says "that data is acceptable," and leaves it go at that point for the licensee and the review process to sort out what is truly a best estimate model.

CHAIRMAN SHACK: What have people actually done in the best estimate models today for the metal water action? Do they use Cathcart-Powel?

MR. BAJOREK: Yes. Yes. They've been using Cathcart-Powel. There is an uncertainty in the application that I'm aware of in how it's applied. So they're -- Westinghouse is using Cathcart-Powel. There is an uncertainty about that calculation.

CHAIRMAN SHACK: In my simple-minded view of this thing, you know, the thing I'm normally looking at when I have a conservative calculation, you know, when I decide how conservative it is I go out and I get a better calculation and I compare the two.

Well, you know, now I've got a better calculation. It would seem to me that, you know, I look at all my best estimate calculations and I go off and I do my simple Appendix K calculation, it would seem to be relatively straightforward to do.

You know, suppose I change my Appendix K calculation with the decay heat and I look at my best estimate calculations, you know, and --

MR. BAJOREK: We have that. That was discussed in the meeting last year. There's a figure. I'm not sure if it's proprietary or not. That's why I didn't -- that's why I wanted to stay with stuff that I knew was public.

Those calculations showed that the Appendix K evaluation model, okay, with '71 decay heat, gave a peak cladding temperature that was just larger than best estimate plus uncertainties. Okay.

When they reduced the decay heat, the Appendix K calculation gave values that were closer to the best estimate, but without the uncertainties. It's somewhere in the middle. Now, you have to take it a bit with a grain of salt, because I think the plant types were slightly different and there were -- it was more of a apple versus a different type of apple.

CHAIRMAN SHACK: Pear.

MR. BAJOREK: Yes. So it wasn't straightforward, but the calculations suggested that if you reduced the decay heat in that -- for that plant in that Appendix K evaluation model, the PCT would not -- would be more favorable than what you would be getting out of a best estimate methodology.

And that raises some concerns going back to the SECY paper 86-318, which says, hey, there are models which can result in a fairly large uncertainty and you should account for those in your overall peak cladding temperature and your analysis methodology.

MR. LAUBEN: But you're right about -- as long as you have the standard of a best estimate calculation to compare with the Appendix K calculation, you can do it. But if you don't have that standard, what do you do?

And for some plants, some vendors, some plants, you have the standard with which you can compare, and the calculation Steve has was actually done by the same vendor, that he was able to compare one to the other.

So that was an apple -- as close as you can get to an apple and an apple. But it's not as easy to do if you don't have a best estimate standard by which you can compare to the existing Appendix K calculation. That's --

CHAIRMAN SHACK: Well, I guess I just don't have a feel whether we have enough best estimate results --

MR. LAUBEN: Yes.

CHAIRMAN SHACK: -- available now to be able --

MR. LAUBEN: Good.

CHAIRMAN SHACK: -- to make the benchmark.

MR. LAUBEN: Yes.

MR. BAJOREK: One of the problems that occurs --

MR. LAUBEN: Good question.

MR. BAJOREK: -- is because they are complex analyses to perform, you know you're getting margin. So immediately what you want to do is to use that margin, okay, for an operating enhancing the core peaking factors.

So if you do the work you don't want to do it at the levels that you already have the Appendix K calculation. And because of that you always wind up in this apples versus oranges type of comparison.

Three options that we are looking at right now, and as I mentioned in this previous figure, we feel that in coming up with an alternative option to Appendix K we have work that needs to be done.

We also need to do work on what I'll call the realistic option, to clarify the use of the '94 standard for decay heat. How you would use Cathcart-Powel, if that's to be recommended for metal water reaction?

What is the difference and how should we deal with uncertainties and conservatism in either of these analyses? Option A, as I'll refer to it. In the realistic option -- and this is going to be true in the two or three options that we'll discuss -- we would revise Reg. Guide 1.157, clarify that you can, perhaps should use the '94 decay heat standard, take the work that Norm is doing to recommend how it should be implemented into those decay heat questions, which we have work ongoing.

Recommend a specific metal water reaction that should be used, and clean up. Perhaps, the more nebulous part of that Reg. Guide is how you deal with these uncertainties. This is why you need to quantify the accuracy.

You need to deal with the uncertainties, and then you were sort of left to the winds on how to do that. And part of the difficulty in the application is coming up with an appropriate statistical method to account for those uncertainties.

And it's been one of the things that has driven up the difficulty in that analysis. Now, in this particular option the Appendix K revision would involve replacing the ANS '71 standard with '94, plus some uncertainty, okay, that would account for the experimental uncertainty in the decay heat.

Okay. One, two, three sigma, something along those order. It would address solely the decay heat model uncertainty. We would ask licensees to take a list that we would propose and they could augment to address recognized non-conservatisms, things like the downcomer boiling, fuel relocation, other things that we may identify.

We think that the approach is consistent with what was requested in the 0-133. That relax where it is clearly non-conservatism -- overly conservative decay heat would account for conservatisms.

DR. KRESS: When you say "consider non-conservatisms" what does that actually mean?

MR. BAJOREK: They would be required to account for those in their Appendix K evaluation model.

DR. KRESS: Okay.

MR. BAJOREK: Okay. We would envision -- and this would depend on NRR, on how they wanted to deal with this -- the licensee coming in with basically an alternative approach to Appendix K, a new evaluation model, which would have reduced decay heat, but those Appendix K evaluation models would have to have features to account for downcomer boiling, fuel relocation and in the case of small break, the issues that we would have to identify for that.

MR. BONACA: More and more that would look like the best estimate.

MR. BAJOREK: Well, we're -- you're jumping ahead just a little bit, but I want to let -- what I want to say. I want to lay this out because as a staff we have not reached a consensus on this. I want to summarize what we're looking at and lay out the pros and cons of each one.

And you raise a good point and we're going to point that out. Now, one of the obstacles we see, that this approach would result in a new methodology and we think that it would be very likely that it would require a review.

NRR would have to expend resources for vendors. The licensees would have to deal with these issues. They aren't straightforward and simple to deal with. They would have to come up with new models for those.

In some cases, experimental information to address those may not be entirely satisfactory right now. There's some 2D/3D data, UPTF, CCTF, that points out the effect. I think it's questionable right now whether it has the right range of conditions by which you might want to develop a new model for.

So I think there are some questions there that need to be answered and we are going to take a look at that. We have to come up with a list of all recognized non-conservatisms. We have a few.

I guess our fear is once we get this list, if something else crops up or is recognized, there's a difficulty in getting it in, okay, without violating some type of a back-fit rule that we might want to keep on ourself.

Because of the difficulty in coming up with new models, licensing those and dealing with the potential uncertainties in the remaining models, we start to think that this may not be that attractive to vendors and licensees.

Our fear is that when you start to look at expenses to come up with this, make Appendix K look more realistic, but still be conservative, deal with modeling uncertainties, you start to tip the balance close enough to best estimate, there may not be an advantage to going this way right now.

On a philosophical point, one of the stones that we would throw at this option is that this would effectively delay the transition from codes that were developed in the '60s and early '70s to more modern thermal-hydraulics codes.

Okay. We would be instituting codes that people have objected to because of their ad hoc models and implications in the past. The second option is one that has been suggested by NRR.

It retains many of the features of Appendix K -- excuse me -- of Option A that we talked about. We would still deal with Reg. Guide 1.157 as we had in the previous overhead.

The idea here is to replace Appendix K, take out the '71 model, replace it with '94, and apply a conservative multiplier, not one that just accounts for uncertainties in decay heat, but now has additional conservatism built in, but sufficient to cover the uncertainties that are observed in the realistic calculations.

DR. KRESS: That sounds just like the current Appendix K, only with a little better quantification.

MR. LAUBEN: Right.

MR. BAJOREK: That's pretty much it.

CHAIRMAN SHACK: Well, and to come up with the right multiplier you still have --

DR. KRESS: Yes.

CHAIRMAN SHACK: -- to do everything they do.

DR. KRESS: You really have to do the --

CHAIRMAN SHACK: I mean, it's magic when you're done, but --

MR. BONACA: It's one added superficiality --

(All talking at once)

MR. LAUBEN: A lot of print.

MR. BAJOREK: It's a tough row to hoe.

CHAIRMAN SHACK: Yes.

MR. BAJOREK: Jump ahead on here. If we were to do this, our estimate is that this would take the staff something on the order of ten man years, because we would need to, one, make sure that we get realistic codes behaving the way we want to.

We're fairly close on that. We still have work to do. But we need to satisfy ourselves that they're handling fuel relocation, downcomer boiling appropriately. Okay. That's an issue in itself.

In the past, we haven't developed evaluation models here at the staff. So we would have to take our realistic code, revise it, change the decay heat, put Baker-Just back in, prevent rewet during blowdown, change the steam cooling models, change this and the other thing to make it mimic an evaluation model.

DR. KRESS: Let me ask you another question about that. If you did that, including the uncertainties, and then you ended up with your answer at the end, and you took the 95 percentile and used that to get your multiplier on your decay heat curve, how is that any different at all than just the best estimate approach?

MR. BAJOREK: You would not know what the uncertainties are here. You would be basing your conclusions based on another set of calculations and hoping those are mimicked by the Appendix K evaluation model. Where things --

DR. KRESS: But you would have to make Appendix K conservatism enough to cover all plants is what --

MR. BAJOREK: That's right.

DR. KRESS: -- I think you're saying.

MR. BAJOREK: We jumped ahead.

DR. KRESS: Yes.

MR. BAJOREK: Where we say it may not be technically achievable is that if we do this strictly as it was proposed, come up with a multiplier. Well, we could look at the worst plant.

DR. KRESS: Yes.

MR. BAJOREK: Like that one I showed you earlier that has a very long transient.

DR. KRESS: Yes.

MR. BAJOREK: Treat it as an evaluation model, put a multiplier on it; look at some other issues, things that we might want. Well, I don't think it takes a big stretch of the imagination to see that you can wind up with a multiplier based on that plant that when you apply it to lower power units you're going to have something that's even more restrictive than the Appendix K as it is today.

DR. KRESS: Yes. I am --

MR. BAJOREK: I'm aware of one attempt, not in this country, to do something like that. Their approach was to take a realistic code, say we want to stay away from all this uncertainty calculation, but let's make our heat transfer conservative.

Let's make our plant initially conservative and do it for a range of plants. Well, they want through the exercise and they eventually went back to a realistic methodology, because the answers they were getting when they applied them for all of the units were now even worse than what they had been getting in Appendix K.

That's where the work comes in, because to make this any benefit, we think, to industry we would have to break this down into a plant-specific type of multiplier.

And when you start looking at the different types of BWRs, PWRs, BNWs, CE units, large break and small break, the magnitude of number of calculations that you have to do and get right becomes very large, and that's what propagates into this.

I estimated a ten-year effort. I've been told that I was too low. It's substantial. Go to the last bullet. One of the reasons we've noticed downcomer boiling as a potential issue has been due to plants being uprated.

In that first figure I showed you, the no harm, no foul, that was primarily due to the relatively low power of that unit. It quenched before the downcomer boiled. As we start to uprate the units, the transient link must get larger because you have more decay heat, okay, to remain.

So as we start to uprate units beyond what they are now, the multiplier, even if it captured the downcomer boiling in today's power levels, may not necessarily capture that effect if that plant is uprated by another five or ten percent.

The margin is not going to go unused. It will likely be absorbed in another power uprating. So our fear is that even if we came up with multipliers, they would be invalidated once the plants start to deviate from the present-day operation.

Also been notified by some of the staff at NRR, they said, well, even if you come up with multipliers using TRAC or RELAP in our versions of evaluation models, we've recognized over the years that the sensitivity of evaluation models that we see from Combustion, from Westinghouse, from GE and BNW aren't necessarily the same from one to the other, much less how the staff's models would behalf.

They approximate things, but the power sensitivity could be different. Nodalization can have effect, as well. So as a result they said, well, even if you spend your ten staff years coming up with a set of multipliers, we're still going to have to go back to the vendors to either verify or come up with equivalent multipliers for their codes, because they may behave significantly different.

Okay. Third option, and this is one where we see it as perhaps an opportunity to move ahead technically. And we see this as an option that says, rather than continue to sink more resources into Appendix K, maybe this is a point to say, let's put the best technology into the best estimate rule.

Let's put those resources into revising 1.157, clarify how we would use the decay heat; what's an appropriate way to apply it; what's an appropriate model for the metal water reaction?

Pursue the other 50.46 risk informed criteria, because there's a tremendous amount of true margin that can be gained by relaxing plant boundary condition assumptions, break size, loss of off-site power.

But retain in the realistic option of a way of analyzing it to at least a conservative fashion or at least to a fashion by which we know what the true margin is.

So in Option C, by focusing our attention on the best estimate rule, making it easier to use, easier to apply, we feel that we'll at least maintain the present-day margin in Appendix K, okay, and if we go to a realistic type of calculation we'll know what that margin is.

I think it's been said in some of the ACRS meetings that safety is better served by having to quantify measure of the margin, rather than some nebulous amount of conservatism that we don't know the extent of.

We already have clear guidelines for the review. We would have to clarify those further in the Reg. Guide 1.157. NRR wouldn't be able to apply their reviews as they currently perform those. In the long run, we feel that this would encourage vendors to continue to develop and use realistic models and more advanced thermal-hydraulic tools.

Westinghouse currently has an approval for best estimate. Siemens-Framatome has submitted one several weeks ago. NRR says that in about a year they think they can get approval for that. We've been told that General Electric would be coming in, in a realistic local methodology early next year.

We're seeing most of the vendors already going down this path.

MR. BONACA: They already had Safer-Gester, right, so.

MR. BAJOREK: Safer-Gester, my understanding is that it's more consistent with an inter-methodology. There are some -- it's not a true best estimate and now they're ready to go the rest of the way.

MR. LAUBEN: It has a 600 degree penalty associated with its use, too. So it's not truly best estimate. So it's TRAC-G that they're coming in with to get approval.

MR. BAJOREK: Now, the cons of doing this. Reduction in regulatory burden is probably minimal. There would be benefit in clarification of the best estimate rule and how you do this, but it's not a tremendous leap. There's still a lot of work involved there.

The expectations of SECY-01-133 may not be met in going this approach. I think you go through there, there was an expectation that we would make a definite revision to the Appendix K. So that -- we would need to make I think a very strong argument on why we wouldn't think that the Appendix K revision is necessarily the right way to go as part of supporting this option.

Where we plan to go from here, we feel that we need to get a -- agree on a list of non-conservatisms, lay that out in a little bit better fashion. We've talked about a few. Let's try to make that list complete.

We want to go back and look at the experimental data, because if we have to start asking people to look at these non-conservatisms, we should be well aware of whether the current database supports development of those models.

The reason I suspected we may have a problem in doing that, I did a kind of a quick scaling evaluation CCTF. In taking a look at a parameter that relates the energy that would be available in the downcomer and core barrel walls, versus the energy that would be required to raise the entire downcomer to saturation.

In kind of a very crude fashion, looking at energy available versus energy that would be necessary. For the PWR, if you'll look at the energy that's available in the core barrel in the vessel wall, it's about seven and a half times the amount of energy that would be required to raise this bulk of fluid in the downcomer to saturation.

That's a lot of stored heat. CCTF where we did see evidence of downcomer boiling -- you don't have to get the whole thing up to saturation, just part of it -- we're looking at something closer to one.

So this is why I said, well, when we look at the experimental data, we need to take a look at the tests versus what we were expecting in the PWR, because when we go down this path now of treating these non-conservatisms for Options A or B, the folks who want to go down that are going to have to demonstrate that the experimental data is adequate to come up with models for that. That may require them to participate in new test programs.

Third, once we lay this out it's probably advisable for us to hold a public meeting to discuss what this alternative approach to Appendix K would look like, and I think as someone pointed out, well, are you getting so close to best estimate now that you're going to throw a party and no one's going to show up.

And if we get that word from the vendor then maybe what's recommended in 0133 should be revised somewhat. That's all I have, but we're interested in your comments.

MR. BONACA: You made a case again for the fact that in Appendix K you have tradeoffs that you car calling for.

MR. BAJOREK: I couldn't hear you. I'm sorry.

MR. BONACA: Yes. I'm saying that you made a case for the fact that there are tradeoffs in Appendix K right now that are an impediment to simply moving on to 1994 ANS standard.

MR. BAJOREK: Right.

MR. BONACA: But the industry has requested it and the way it came out was almost as if in fact those issues were not there. Is there consensus on the part of the industry, the technical community, regarding these tradeoffs, these issues? Or do you have to go to this public meeting before that will be surfacing?

MR. BAJOREK: I think we have to go to the public meeting to really surface that.

Norm, it was the vendors that came to you last year and pointed some of this out.

MR. LAUBEN: Well, we have -- let's see. We have the questions that we've -- you know -- they came with the request for rulemaking. We have proposed some questions to them about their proposal.

But as Steve said, it isn't clear what venue it's -- addressing those questions would take. Would it be a public meeting? Would it be publishing the questions and then having them respond to it, or what I think -- I think eventually it has to be some kind of a public meeting so that all interested parties get a chance to address their concerns about this.

So I don't know. Last -- let's see. A couple of years ago we did ask -- informally now. This was not formally at all. At some public meetings we asked questions about, you know, similar to the ones that I had on decay heat.

How would you -- you know -- this is not as simple as it was, but we -- at that time there was no apparent interest in the decay heat change. And so --

MR. KURITZKY: Yes. I think in all fairness to the industry and that --

MR. LAUBEN: Yes.

MR. KURITZKY: -- those public meetings were focused on --

MR. LAUBEN: Right.

MR. KURITZKY: -- on all the different options.

MR. LAUBEN: Yes, right. Right. Right.

MR. KURITZKY: And industry was really interested in the larger picture, local redefinitions --

MR. LAUBEN: Right.

MR. KURITZKY: So they really didn't want to spend time looking at other types of changes. So.

MR. LAUBEN: Right. So they're interest in decay heat is relatively -- is subsequent to those meetings. And I think we have to somehow get, you know, stakeholder involvement in this, right.

MR. BONACA: The other question I had was, you made a statement that typically, best estimate results plus uncertainty, comes quite close to the Appendix K, and that's the experience I've had, too, I mean, in looking at that.

And that's -- and of course, the point of comparison you used was PCT, peak core temperature, okay. I'm trying to understand if there are other measures of merit that you're using in these comparisons to see what is appropriate to make reductions in what is not.

Or rather than appropriately, what is convenient or not convenient. Is PCT the only criteria you use in there to compare the two approaches and --

MR. BAJOREK: We should probably look at clad reaction. I think in the shorter transients usually that the equivalent clad reaction is not as limiting as PCT. That may not be the case as we get out to fairly long duration transients. It probably should get looked at, because we haven't done that yet.

MR. BONACA: And one last question I have is, again, I mean, if I have the best estimate, which typically, I mean, it has certainly conservatism built in plus uncertainty, and I come up with the results very close to the Appendix K and typically pretty close to 2200 degrees fahrenheit, I mean, typically, these plants don't have a lot of margin there, really, what is the opportunity for margin reductions or for reducing regulatory burden?

MR. BAJOREK: It would probably be in -- I think reduction in break size would certainly amount to --

MR. BONACA: Well, that -- yes, that was something we didn't want to -- I mean, that's a different issue, talking about purely that we walk down this path with the belief that there were opportunities purely in the artificiality of Appendix K, but then, you know, this comparison you're referring to, it's a solid one.

I mean, I've seen it many times for different plants and it's there.

MR. BAJOREK: Well, I guess, you know, some of that -- they wouldn't necessarily wind up in the Reg. Guide -- well, they could wind up in the Reg. Guide. And when we developed the best estimate for Westinghouse, in a number of cases the range over which you addressed the uncertainty bounded all of the data.

Appendix K doesn't have to do that. You just have to be conservative relative to the mean. So what happens when you go to best estimate under that type of a regulatory requirement, Appendix K really gets an advantage.

If the Reg. Guide were revised relative to treatment of the experimental data that you have to bound 95 percent or within two sigma, and made your uncertainties smaller, first, you would make it clear for people developing new pools what they had to come up with.

And secondly, I think there would be a fair amount of margin gained by, you know, getting away from the wings of some of this experimental data. I can think of several models where you would get a lot of benefit in taking that approach, but that's something that would have to come from probably a Reg. Guide as opposed to a regulation.

That, again, goes back to -- as I mentioned on that one slide, the problem with 157 is the discussion and treatment of the uncertainties. It kind of leaves it too wide open, and if that were clarified there would probably be a fair amount of benefit in that.

CHAIRMAN SHACK: I'm not sure I understand that last argument. Are you just saying you just cut the uncertainty analysis at the 95th percentile? That would certainly help.

MR. BAJOREK: I don't have a pen.

MR. BONACA: What you seem to say, that you have more opportunities in the best estimate than you have in Appendix K, of course.

MR. LAUBEN: Here's one that works on slides.

DR. KRESS: Just do it on the screen.

(Laughter)

MR. BAJOREK: Are you telling me to?

DR. POWERS: He's hoping somebody else will to --

DR. KRESS: Yes.

MR. BONACA: To be the second.

DR. KRESS: I hate to be the only idiot in the crowd.

MR. BAJOREK: If we take a look at an uncertainty distribution in some model, we may see some bias away from perfection. We've got everything on here. But we also see a scatter in how well you get a prediction to the experimental measurement. It may take some distribution.

Well, if you want to range the uncertainty for that model, well, you have to make a decision, do I range it over best to worst, or do I say, hey, I don't necessarily have to address the wings out in here.

Most specifically, can I get away from some of these worst situations? And in the calculations that I see, what drives your 95th percentile PCT frequently comes from this part of the distribution, then an experimentalist may say it's a bit of an outlier and isn't representative of the bulk of the data.

But right now, the Reg. Guide is not very clear on where you draw that limit.

DR. KRESS: Well, it looks to me like there's not a very lot to be gained by changing the Appendix K part of the rule. It looks like what we didn't know before was that the non-conservatisms were pretty much balanced out by the conservatisms.

And you know, that's -- you maybe don't have them all quantified exactly right, but it's a good guess that it's getting close.

MR. BAJOREK: Whoever picked out the 1.2 --

DR. KRESS: Yes, did a pretty good job.

MR. BAJOREK: -- did a pretty good job.

DR. KRESS: Yes. So since it would be a big deal to change it and you have to worry about back-fits, I guess, because it could require some plants to redo their analysis and do things over, my leaning right now is for your Option C.

But I would encourage you to continue on with this action plan, because it does two things for you. One, it bolsters your case because it does give you a better look at what these non-conservatisms are compared to conservatism, and it gives you information that could be very insightful when you go into evaluating the best estimate models.

MR. BAJOREK: Right.

DR. KRESS: So that's kind of my inclination right now. I don't know how these other guys feel about it.

MR. BONACA: Absolutely. I totally agree with that. It seems to me Option C is the one that has some opportunities.

CHAIRMAN SHACK: Well, clearly, there's a difference of opinion, because somebody submitted a petition to change it. So --

MR. BAJOREK: Yes.

DR. KRESS: Yes, but weren't they mostly interested in changing the large break LOCA definition in that? Or did they want --

MR. LAUBEN: No. No. I mean, this petition is strictly for the decay heat.

MR. BAJOREK: Strictly decay heat.

MR. BONACA: Well, that's why I asked the question about the technical community, because I mean, the case you made today would discourage a change without your investigation of this -- and you have data that says that in fact you have -- you may not be able to support the intention of that multiplier literally.

DR. KRESS: And anyway, you have the best estimate option which --

MR. LAUBEN: Right.

DR. KRESS: -- let's them do what --

MR. BAJOREK: It's there, now.

DR. KRESS: It's there now.

MR. BAJOREK: It's there now. It's not clear and there are things that could be done to make it perhaps less onerous.

DR. KRESS: Yes. You have -- yes. That might be the place to focus your attention, I think.

MR. BONACA: I mean, any use of these changes would require a new analysis, anyway.

MR. BAJOREK: Yes.

MR. BONACA: And you know, Appendix K may be less expensive one. I don't know.

MR. BAJOREK: Generally, it is.

CHAIRMAN SHACK: Well, I guess just to follow along here.

MR. BAJOREK: Okay.

CHAIRMAN SHACK: I guess it seems to me we have realistic analyses, you know. People can make comparisons, you know. Your case is reasonably convincing in terms of discussion, you know, but I just have to see more of these comparisons.

Now, Mario says he's looked at them and, you know, they're there, but it seems to me that really is the thing. You have best estimate estimates. You have other estimate you can really get a much more concrete comparison of what the effect would be.

MS. DROUIN: I was going to say, Steve covered the evaluation model. We now have the acceptance criteria. I did notice, though, that on the agenda you had a break at this point, whether you want us to go ahead and get into the acceptance criteria, or do you want to take a break now?

CHAIRMAN SHACK: Ralph's discussion looks reasonably short. I think I'd just as soon keep on going and then take the break.

MR. MEYER: Looks short, but the discussion may be -- looks short, but may be deceptive. All of the discussion so far has been on analytical methods for calculating the peak cladding temperature which are laid out in Appendix K.

There are in fact five acceptance criteria specified in 50.46, not just peak cladding temperature. These are the speed limits, so to speak, and they are listed here on this slide.

The objective in examining the acceptance criteria in 50.46 for possible modification is to see if we can remove some or all of the prescriptive nature of these criteria, which are related specifically to zircaloy cladding and to ZIRLO, which are written into the present rule, and take them out so that the rule could apply generally to any zirconium-based alloy that's used for fuel rod cladding.

I think that this can be done simply by removing number 2 on this list, the maximum cladding oxidation, which is specified at 17 percent, and I want to discuss that. So I have in fact just one option here.

It's either do it or don't do it. You could, I guess, make some variations on this, but this seems like a logical approach, relatively simple in procedure, that would solve the problem.

DR. KRESS: What does it change, Ralph?

MR. MEYER: What?

DR. KRESS: It just changes the -- it's a perceptions change.

MR. MEYER: What you would do here is to take the 17 percent equivalent cladding reactive limit out of 50.46 and replace it with a performance-based requirement that says simply that you should retain some post-quench ductility in the cladding.

DR. KRESS: Yes, but isn't the 17 percent a surrogate for that?

MR. MEYER: That's -- well, 17 percent was a measure of that for zircaloy.

DR. KRESS: I see. It may not be the same surrogate for other things.

MR. MEYER: That's correct. It may not be the same, for example, for M-5. Even ZIRLO wasn't tested carefully against this limit, although the rule was changed to include it.

DR. KRESS: And if you made the change, the licensee would have to come in, if you had a different clad, and show you the database.

MR. MEYER: That's correct; that's correct.

DR. KRESS: I think that'd be a good change. That would clarify a lot of things.

MR. MEYER: Yes. Most of this is on the next slide, but before you move to the next slide, let's --

DR. POWERS: Before we get too excited about making this change.

MR. MEYER: Yes.

DR. POWERS: What you want to do is to preserve some ductility so that you can cool this core and keep it cool and not have it fall apart on you.

MR. MEYER: Right.

DR. POWERS: We test for ductility in a variety of fashions and we get different results when we test in different ways.

MR. MEYER: Yes.

DR. POWERS: How do we know that the test that we propose to use for ductility is the one that's applicable for the core and the post-quench environment?

MR. MEYER: Could you say a little more so I understand what's in your mind a little better?

DR. POWERS: Okay. What I know is if you test it one way it says there's lots of ductility. Test it a different way there's not so much ductility, okay. That's laboratory tests of remaining ductility.

Okay. Now, what we want to have is the core not fall apart after we have gone through the ECS injection or something like that and we've got -- everything's cooled down, and it doesn't because there's some ductility there.

How do we know that the ductility we

derive from whatever test we endorse is ductility that actually exists in the clad under the conditions of the post-quench environment?

MR. MEYER: Well, the -- first of all, the testing that was done back in the early '70s and late '60s was in fact done under a post-quench environment. That is, the cladding pieces were taken through a high-temperature steam oxidation.

They were cooled down. They were quenched and then they were tested at a relatively low temperature. And there -- while it's true that you could use other methods than the ring compression method that was used back in the '70s, and there would be some scatter in the result, from reviewing what had been done earlier, it still appears to be a reasonable approach.

And in fact, we are at the present time in an ad hoc expert group that has participation from a number of international groups. We are exploring several different test methods for determining ductility.

All you're trying to do here is to have a screening test where you can differentiate between fully brittle material and material that has some residual ductility. From what I've seen so far, I think the ring compression test will continue to be a good way of doing this.

You can do it with a hardness indentation. You could do it with some sort of plastic extrusion method of providing the loading on the rings. But it's materials property, and basically, any way you test it except for some variations introduced by the testing method, you're going to get about the same answer.

So I guess the answer to your question is, we are aware of the concern about the appropriateness of the test. We have an effort underway to see if some other procedure would be better than the one that was used in the early '70s.

At the present time the general configuration of ring compression test still appears to be a good approach, and the details of the test method that would be used for this would be laid out in a regulatory guide.

And I guess we can just -- you're jumping to the bottom line here and going --

CHAIRMAN SHACK: Going at it a different way because I think Dana's coming from -- there's two problems here. One, given a given temperature strain history I end up with a certain condition of cladding, and to determine the ductility --

MR. MEYER: Yes.

CHAIRMAN SHACK: -- then it really is an experimental problem of what is the right test.

MR. MEYER: Right. Right.

CHAIRMAN SHACK: And that's one we can address.

MR. MEYER: Yes.

CHAIRMAN SHACK: I think in a fairly straightforward way. I think Dana's concern is with temperature strain history you put the clad through --

MR. MEYER: Oh. Oh. Oh. Okay.

CHAIRMAN SHACK: -- before you get to the test.

MR. MEYER: Okay. Okay. Well, we're also poking into that and the way -- we haven't completed this, and you'll see that the last column on this second slide here is that we will not have done enough work to actually put this thing through a -- its paces for one or two more years because we haven't finished the work yet.

But we know pretty much now how this would play out. You would use a temperature -- you would simulate the several of the high-temperature LOCA transients. You would have a slow temperature rise up to some temperature at which you would soak it for a period of time to accumulate the oxidation.

You would cool it at some steam cooling rate. I forget the number, but I think it's on the order of five or ten degrees a second, down to 800 degrees centigrade, at which point you would then flood it and quench it.

And we are exploring the effect of different heating and cooling rates and the effect of different temperatures at which you hold the specimen. And it's likely that one would want to prescribe tests at a series of temperatures, not just at a single temperature, up to and including the peak cladding temperature of 2200 fahrenheit, which is 1200, 1204 degrees centigrade.

And so we are doing those kind of tests in the near future on a high burnup cladding and the archive under-radiated fresh material that corresponds to that to try and map out what these effects are and what would be the best rates and temperatures to conduct this temperature history for the ductility test.

So that would all be set out in the Regulatory Guide. Now, in the relatively near term we could set out in a draft guide the conditions that we in fact are planning to use in the laboratory.

But if you rush this through before we're able to actually do those tests in the laboratory and see if the result appears satisfactory, then you run the risk that we might have to change something.

MR. MEYER: Now, there is -- may I move to another point? There is one thing that I wanted to emphasize here, and that is that the peak cladding temperature of 2200 degrees and the cladding oxidation limit of 17 percent really arose as a pair of numbers originally, and these both came from these ductility tests, the ring compression tests.

The 17 percent -- okay. So you have some flexibility if you want to move away from these precise numbers you could say, well, let's work with 2300 degrees fahrenheit and maybe we would get 15 percent for zircaloy.

So there would be some flexibility in working with both of those numbers, but you'll notice that I've suggested that we keep the 2200 degree fahrenheit number, and I've suggested that for what I think is a good reason.

And that is during the ECCS hearing this was the most contentious part of the debate about the acceptance criteria. And in fact, a second line of concern was raised about the peak cladding temperature, and that had to do with rapid oxidation at higher temperatures.

And so the Commission reached a decision to limit the temperature a relatively low value so that you didn't have the concern of rapid oxidation. And at the same time it fit in with the -- with Hobson's ring compression test data on the ductility.

And so I think that if you were to alter that temperature that you would probably open this up to a lot of contention. And I don't believe there's a need to change that because we can work with that as a fixed number, and then let the maximum cladding oxidation figure vary in order to capture the effects of both cladding alloy variations and burnup effects.

And so it might go up or it might go down. And it might be different for high burnup, low burnup, different cladding alloys. And so you pull that out. You put it into a Regulatory Guide. Everything else can stay fixed and then the -- in 50.46.

And the 50.46 would not be pegged to zircaloy or ZIRLO and could be used for all zirconium based alloys.

CHAIRMAN SHACK: Why not just pull them out to the Reg. Guide?

MR. MEYER: Well, you could --

CHAIRMAN SHACK: Use the coolable geometry and the long-term cooling as the fundamental requirements, which they really are.

MR. MEYER: Yes.

CHAIRMAN SHACK: And then how you assure that, put that in the Reg. Guide because if you get new data some day --

MR. MEYER: Well, you could do that. I think that you would also need to address the question of rapid oxidation at higher temperatures. And to me this would open up the possibility of litigation unnecessarily.

There's no reason that we couldn't work with that 2200 figure on the embrittlement criteria. Leave it fixed. It was, you know, a hard fought number in the beginning and it does not cause, as far as I can see, any problems with the technical adequacy of an embrittlement criterion that you would derive with that as a fixed number, because you've got two parameters to work with. So we can do it all with the other one.

CHAIRMAN SHACK: It's just that, you know, you don't really have the database on M-5 or even ZIRLO. I'm not sure that people were worried about it as much.

MR. MEYER: No. It hasn't been worried about too much in the past, but we're worrying about it now. And in fact, just for your interest, I could say that we've made excellent progress in our discussions with Framatome about an agreement to begin testing their M-5 cladding.

And we're now down to the point of some legal language in a memorandum of understanding with all the basic issues having been agreed upon between us and Framatome. So I think the time is coming soon that we will begin to test, first, the Framatome M-5 cladding on irradiated material at first, and then hopefully, Westinghouse, their low cladding, although those negotiations are simply on hold waiting the outcome of the negotiations with Framatome.

DR. KRESS: You're fairly confident, though, that the 2200 will keep you below a runaway oxidation.

MR. MEYER: Yes.

DR. KRESS: That's well enough below it that it's safe.

MR. MEYER: Yes. I'm not aware of anything that would be significantly altered by making these small alloy changes. I mean, it's still based

--

DR. KRESS: Just not enough that has changed.

MR. MEYER: -- and zirconium and zirconium oxide.

CHAIRMAN SHACK: I mean, 2200 is more than runaway oxidation. It really is sort of oxygen pickup that --

MR. MEYER: Sure. 2200 first of all is part of the embrittlement criteria.

CHAIRMAN SHACK: Yes.

MR. MEYER: And how it came about -- put my backup slide. I've got my backup slide to make me look smart here. When you ran a piece of zircaloy cladding through a high temperature transient and brought it back down, if you -- you're looking at the outer surface on the left and the inner surface on the right, it went through a phase change.

It was hexagonal close-packed in its alpha phase at normal temperatures. And some of it changed to a body center cubic structure at high temperatures, and then you quenched it and brought it back down.

And you could tell what had been body center cubic and it turns out that the body center cubic phase is the one that provides your strength and ductility. And so what you really are interested in is maintaining a ductile prior beta region in the cladding.

And the gross amount of oxidation correlated pretty well with the thickness of a ductile prior beta layer. Above 1200 degrees centigrade, 2200 degrees fahrenheit, you've got additional oxygen diffusion into this prior beta region that pretty much upset that handy little correlation.

And so that was the reason that you didn't go above 1200 degrees centigrade, because your use of gross oxidation as a surrogate for this one layer fell apart. Now, you could deal with that by backing down the total amount of oxidation in say 15 percent or 14 percent, and let the temperature go up.

But the Commission did not do that. They stuck with that number and then they said, and by the way, we don't want to make it any higher because there is this consideration of rapid oxidation at higher temperatures and we don't have much information on that.

And it was a huge -- that was a huge part of the hearing. It was a huge part of the Commission opinion, and it seems like the sensible thing to do is to leave it alone and to work with the oxidation thickness for the embrittlement criteria.

CHAIRMAN SHACK: Any additional questions for Ralph? If there are none, this seems like a good place for a break and I suggest we come back at 11:05.

(Whereupon, a recess was taken at 10:48 a.m. until 11:09 a.m.)

CHAIRMAN SHACK: So in that quandary, we can start again.

MS. DROUIN: Okay. He has it on. It's not working.

CHAIRMAN SHACK: Time to change bulbs.

(Pause)

MS. DROUIN: Okay. We just have one slide here to bring in the status of what's happening on the rulemaking side with NRR. Unfortunately, Sam Lee couldn't be here today. There was a petition that was sent in, in September by NEI.

The primary purpose was, as we saw with 50.44, if there's a part that can be -- that appears to be -- that can -- that appears -- man, I just can't get these words out of my mouth -- that can be separated out and move on a faster track, they like to see that be done that way.

And so they have submitted a petition to separate out the decay heat part and put that on a faster track and make a separate rulemaking activity out of that. We had noted in our SECY that all of these things could be one rulemaking or several rulemakings, and that would be decided as we move forward.

But in their particular petition, you know, it would allow the licensees optional adoption of the latest standard and allow adoption by the licensees of any subsequent revisions to the standards that are endorsed by the NRC as we go forward in time.

Right now, the staff is currently evaluating the petition. It's in the normal process. Okay. Now, today so far we have talked about the status on the evaluation criteria and the evaluation model.

Those were two very important parts because we are right now deviating on the recommendations that we had made on the evaluation criteria and the evaluation model. We had made one set of recommendations, and now, as we move forward in the technical work we are now proposing somewhat different things.

On the reliability part, we've still got a lot more work to do here, and as we've seen in the criteria in the evaluation, we're coming up against some technical issues that we had not anticipated. So with that, I'll turn it over to Alan.

MR. KURITZKY: Okay. As Mary said, the previous discussion dealt with the proposed changes in SECY 133 dealing with the ECCS evaluation model and the acceptance criteria.

We also proposed changes in the near-term to the reliability requirements, particularly those that are included in, you know, GDC 35, dealing with the loss of off-site power requirement and also the single failure criterion.

In the SECY what we recommended was a risk-informed alternative to those ECCS reliability requirements. The idea was that we would replace the existing requirements of GDC 35 with requirements that were more risk-informed and more realistic.

Particularly, we would be deleting the -- oh, we'll call it the requirements or the assumption that you have a loss of off-site power when you have a LOCA, and also the need to model the single worst additional failure.

Instead, we would be offering two performance-based options that would get at -- that would help assure ECCS reliability. A first option would be a generic -- would be something that was done in a generic fashion by plant type that the NRC staff would do ahead of the game.

We would put together, we would define by plant group or plant type what minimum ECCS equipment would be required for that group or type, and we would -- that would also include whether or not you need to consider the loss of off-site power for -- to prevent accidents.

And the equipment requirements themselves would be tied to different groups of accidents. You may have one set for large LOCA ones and for small LOCA, et cetera. The idea under Option 1 is that's something that the NRC staff would do ahead of time so that if a licensee wanted to implement it they would not have to do any technical analysis.

It would be pretty much cut and dried. They can choose to go with it. They don't have to do any analysis and nor does -- do any review, and it's -- it'll go quickly.

However, if a licensee decides that they feel they are not getting as much unnecessary burden reduction as they feel they could get, you know, doing a more detailed analysis, a more plant-specific analysis, they don't like the group they're in, they feel some bad actor was dragging their group down, they will have the option to go ahead and do a plant-specific analysis and that will be based on guidance that we would include in a Regulatory Guide.

We would give them an ECCS function reliability threshold that would be derived from, well, what we're envisioning is probably something derived from the core damage frequency threshold that's in our framework, our Option 3 framework, the qualitative guidelines we have there.

And then the licensee would go through and do analysis using their own data, you know, whatever analysis they want to do analysis mix they want, their own PRA, and try and justify some -- they would have to meet some reliability threshold for the ECCS function, and it could be with whatever equipment they have at their plant, whatever set they feel is necessary.

And again, that would also cover whether or not they would need to consider the simultaneous loss of off-site power assumption for different acts and classes. To kind of explain that a little bit better I have -- this just shows you the --

CHAIRMAN SHACK: Now, your own Reg. Guide would also give them some way to calculate LOCA frequencies, right?

MR. KURITZKY: Well, what the Reg. Guide wold do would give them guidance on LOCA frequencies, exactly. That's going to be one of the main things. You'll see as we get to the technical issues, that's one of the main things we are still wrestling with. But that's -- yes, you're right. It would give them guidance or --

CHAIRMAN SHACK: Or a set of numbers or something.

MR. KURITZKY: Yes. It could give them a set of numbers or it could --

THE REPORTER: Excuse me. Would you pull your microphone up.

MR. KURITZKY: It can give you -- it would give you a set of numbers or it could tell you things you have to consider when you want to calculate your own numbers. They say that latter part about things you have to consider, some of that stuff may have to, you know, I don't know whether it's something we'll do now in the short-term, whether it's something that would have to wait till we get to the long-term thing of looking at the spectrum of LOCAs.

Just to kind of clarify a little bit about what we're looking at from coming from these two options, for Option 1 we're envisioning that we would have matrices that we have produced, and the matrices would have plan group or plan type along one side.

You know, it'd have different acts and classes along the other side. And it would delineate what minimum system requirements you would have or equipment requirements you would have for the ECCS function.

And these -- the purpose of specifying that equipment is if a plant finds out that they have more equipment than the minimum required, it would give them fuel or additional justification for making some kind of operational relaxation, whether it be in technical specifications or whether or not it would allow some kind of design change.

That would be up to us to decide, you know, the NRR to decide in the implementation phase. But in addition, there would be a second matrix that would identify the actual sections that are used in the ECCS thermal-hydraulic performance calculations.

And specifically, it's the GDC-35 requirements of the single additional -- single worst additional failure, and considering both with or without off-site power available.

This matrix would again be the same thing, plant type and accident type on the other side, and it would specify whether or not you do need to consider a loss of -- a conditional or a current loss of off-site power with that particular accident class, and also whether or not -- what failures you'd need to consider.

It could be a single failure. It could be multiple failures. It would also allow you to address passive failures. It would give us the opportunity to finally try and resolve the footnote that's been in Appendix A to Part 50 for -- since I was a small boy, because all that would fall into this reliability threshold. And so --

DR. POWERS: It seems to me that the assumption is the ECCS requirements right now were installed in response to the possibility of some stochastic event during normal operations, and you're trying to address that. That doesn't seem to me, then, to span the entire spectrum of reasons for having an ECCS.

MR. KURITZKY: Well, as far as I understand, the ECCS -- we're looking at all the different types of acts and issues that you could have at the plant.

DR. POWERS: No, you're not. You're not looking at all of them. You're not looking at any kind of sabotage. You're not looking at any kind of external threat whatsoever here.

MR. KURITZKY: External threat. You're referring to a sabotage threat or external events like seismic activity?

DR. POWERS: Clearly, I'm talking about sabotage.

MR. KURITZKY: Sabotage, yes. Yes. Sabotage isn't --

DR. POWERS: Well, doesn't that -- I mean, doesn't that make you -- I mean, why can you exclude that?

MR. KURITZKY: That's an interesting question. I mean, as I understand it -- I'm not privy to all that's going on in the Agency on that topic. There's a lot of work going on there and that's going to impact -- I assume that's going to impact a lot of the work that the Agency does.

It can impact a lot of the regulations. I don't know how that's all going to fall out. I would say I wouldn't want to hold up everything else waiting to see how that falls out. So what we're going to do is based on current risk insights.

And unfortunately, as we all know, they do include sabotage as an initiator. I mean, it does not include sabotage is -- that's my answer.

DR. POWERS: I mean, it seems to me that until you can establish that the only reason we have any CTS in each plant is for stochastic events that are covered by the PRA kinds of analyses, you can't go around doing this.

MR. KURITZKY: Well, I don't know. I guess my opinion is that I don't necessarily agree with that. I feel that we have enough knowledge that we can propose some changes based on what we feel are reasonable events.

I think you make a good point and that's one thing that just has been kind of overlooked by PRAs, and it's not a question of something that's overlooked because of the frequency is so low, which we can make some probabilistic argument why we don't need to worry about it, but it's one that obviously we can't make that argument about. I guess that may be, you know, policy --

MS. DROUIN: I'm confused by your -- I wasn't even sure what your question was in all of that data. But what I'm more confused by is your concern doesn't seem to be addressed by the current set of the way the regulation is written right now anyway.

DR. POWERS: Why not?

MS. FAIRBANKS: I guess if I can interrupt, I was going to say that later when I had a slide come up that we were going to be providing from our branch some support to PRAB. And one of the things that we were trying to look at, too, was some of these indirect causes.

And we had actually considered potentially sabotage or maybe that would be the subset of something like an indirect crane hit to piping, which could cause a large break LOCA. But to fully risk inform --

DR. POWERS: Well, I think you -- I mean, I don't think there's an analogy, a good analogy there, because one of the reasons for saying -- you got a large break LOCA and a simultaneous loss of power, it is clearly a, gee, this is a deliberate sabotage event and I want to be able to respond to that with my system.

And I don't think you can do both of them with a stochastic event. I mean, that's why he's interested in dropping out the simultaneity, because it's hard to come up with a finite, nonvanishing probability, is to have both at the same time. But you can when you come to sabotage.

MS. FAIRBANKS: Yes, you're right.

MS. DROUIN: If you give the sabotage a probability of 1.0 -- the frequency --

DR. POWERS: No, I don't think I have to do that. I have to say that in any event even those of a vanishingly small probability can in fact occur, and if they do occur I want something to protect me.

MS. DROUIN: So regardless of how small the occurrence is, I mean, to me that's not being risk-informed, and that's where we are right now. We have two events there that are extremely low frequency.

MR. KURITZKY: And I think also in the off-site thought, Mary's point is that -- or to address more your question, Dr. Powers, is that the -- if we do make some of these changes, it does not mean that -- you mentioned you'd like to have something to protect you in case that event does occur.

And it's not that these changes that we're going to make here are necessarily going to strip away all that protection. Your reliability may be somewhat reduced, but it doesn't strip that away.

So then it's the question of that reliability times the probability or the frequency of a sabotage event that results in the loss of off-site power and large break LOCA, plus your residual mitigated capability, is that frequency -- previously something that gives us heartburn in that -- without a quantification of sabotage frequency, I can't say one way or the other.

DR. POWERS: I guess I'm wondering if suppose somebody even figured out how to calculate the sabotage frequency, would I want to give up that protection?

MR. KURITZKY: Well, what protection specifically are you giving up?

DR. POWERS: Be able to cool the core.

MR. KURITZKY: Well, what I'm saying, I don't envision that what we would be changing would necessarily give up your ability to cool the core.

DR. POWERS: I am --

MR. KURITZKY: You still have low pressure injection pumps that are most likely going to be on the -- so it's not -- you know -- there is a mitigative capability remaining in the plant.

MR. BONACA: I think this is somewhat of a broader issue of how the -- you know -- the issue of security has always been dealt with. I think that the fact that there is a security has always been eliminating consideration of sabotage as initiator for this.

I mean, it's an issue but it's broader than this specific one, I think.

MR. MARKLEY: I think the point here that you're driving toward is that it was built on internal threats.

MR. BONACA: Right.

MR. MARKLEY: And not external threats.

MR. BONACA: Correct.

MR. KURITZKY: Okay. So as I was mentioning, we have these two types of matrices that would really -- that we're looking at, we're envisioning would come from this: the one we specified with the minimum equipment, ECCS equipment that the plant -- or plant type or plant group would have to have for each different type of accident initiator category.

And the second matrix would specify the assumptions, failure assumptions to be used in doing the thermal-hydraulic calculations by the types of failures we'd have to consider, and also what equipment -- also whether or not you'd have loss of off-site power or not.

For Option 2 we would be producing a Regulatory Guide. I guess I should have used the words "contain the requirements," since there are requirements, but I guess it would provide the guidance to licensees for performing a plant's specific analysis like I had discussed previously where they would essentially be going through the same things that we're going to be going through, trying to come up with the option on matrices.

The same issue that we are going to wrestle with we would have to lay out in that Regulatory Guide, at least to get to know how -- at least one way that we would approve them in doing the analysis.

And that would allow them, like I said before, to try and get additional margin or additional unnecessary burden reduction if they feel that the first option didn't give them -- or didn't get them where they wanted to go.

CHAIRMAN SHACK: Now, what -- you know, when we redo this, what changes do you see the licensees actually making in response to this? Is it tech spec requirements on the --

MR. KURITZKY: I think one thing is probably tech spec requirements; allowed outage times, maybe some relaxation of allowed outage times. I think the LOCA/LOOP assumption may allow them to extend the -- or relax the diesel generator start time that -- the tech spec requirements for the ten-second diesel start time.

Those are the two main things that I can envision, relaxation of tech specs which includes, of course, the diesel start time. What's not clear is that if they are -- if they can do the calculation to show that they can extend the diesel start time, whether they would in fact push for a change on that start time or allow the diesels to start later, or whether or not they would just keep that margin for some other usage or find that margin for some other usage.

But that would be up to the licensees to decide. So we will just have to make sure that whatever that chance could entail, we're happy with that they would do with it.

MS. DROUIN: You go back to the issues and when we come back if they want to hear about that.

(Pause)

MS. DROUIN: We were going to skip the next slide in and get into -- because it just seems to go more along better with the discussion -- the issues that we're encountering.

MR. KURITZKY: It's off the slide 13.

Okay. In doing the technical work and pursuing the technical work since we submitted the paper, SECY 133, we have encountered a number of issues that we have to wrestle with, technical issues, implementation issues.

We're also looking out for policy issues. Some of the issues may border or be on some fine line between technical and policy. But right now we have a quite a long list, maybe 20, 30 issues that we've come up against.

Some of them were relatively easy to resolve and we've already resolved them internally, or have those resolutions to them internally. Others we still are wrestling with. We'll require more broad-based discussions and probably public stakeholder input.

And certainly, we welcome and one of the purposes of this meeting is to get ACRS input on some of these issues. The three of them that I have listed on this slide are three of the more important ones that we're wrestling with.

The first one we all were just starting to discuss a little while ago was the LOCA scope and frequency. We are planning -- or at least at this point the most up-to-date column, state of the art LOCA frequency numbers are in NUREG CR 57.50, and that's our starting point for getting LOCA frequencies for this effort.

However, the new NUREG CR 57.50 LOCA frequencies are just for pipe break LOCAs. They don't consider nonpipe break failure methods, such as steam generator man-way, or heavy load drops.

So we have to determine some way to include those other types of LOCA initiators or LOCA causes into our calculations. In addition, we need to address the methodology in NUREG CR 57.50. We need to determine whether or not it adequately addresses aging effects and other unknown -- mechanisms that may show up sometime in the future that would serve to undermine the service data that is the basis for the NUREG 57.50 numbers.

We are planning to have meetings with contractors from the engineering folks, the probabilistic factual mechanics experts meeting with people involved with NUREG 57.50 to try and internally come up with something that may be acceptable to all parties, at which point we also want to go out to the public and get their input on how we're going to address the LOCA frequencies.

And obviously, it's a big driver. It's going to drive both pieces that I was talking about previously, the LOCA/LOOP and the reliability. Both -- the LOCA frequency is a parameter in the equation for both of them.

So it's obviously a very important issue that we need to come to some resolution on. Another issue that's very important that we need to resolve deals with the conditional loss off-site power probability given a LOCA, going to that LOCA/LOOP assumption.

And unfortunately -- but fortunately, there is no data on LOCA/LOOPs. We're happy about that, but as analysts it makes it a little trickier. We have, instead, had to use as surrogates for an actual loss of off-site power conditional on LOCA, we've had to use just regular reactor trip events, and also, ECCS actuations.

Now, ECCS actuations more closely resemble the conditions you would have, at least electrically, electrical load-wise, from a LOCA. However, again, there's very few ECCS actuation events.

There's more readily available data on reactor trips. The problem we have with reactor trips is that the electrical loading conditions aren't nearly as severe or not as severe as you would have if you had a LOCA, because you don't have the ECCS loads coming onto the safety buses.

And that concern is further exacerbated by the fact that we have a situation now where there are plants sometimes operating with a degraded grade of voltage, and when that condition occurs, given that we have the degraded voltage relays or trip relays on the safety buses, you can run into a situation where just an extra load from the ECCS pump starting could be enough to trip those relays, and even though power may still be available on the grid, for all practical purposes the plan is experiencing a loss of off-site power because it's going to separate from the grid and have to run on the diesels.

So that again is one of the data limitations we have right now that we're working with. And the third issue that we have up here involves giving credit for non-safety grade -- non-ECCS systems in the calculation.

The reliability threshold that we're basing this on is probably going to come from a CDF threshold from the framework. The CDF threshold is based on values from a PRA.

PRAs and doing the core damage frequency calculations do credit non-ECCS equipment for serving a function, or a RCIC pump or in a BWR you could have a service -- cross-tie, or you know, a fire, a fire protection pump.

So what we have to determine is what credit are we going to give for non-ECCS systems in this -- in our calculations or in trying to come up with these matrices?

We don't want to get up in a situation where a licensee may try and meet the entire reliability threshold with non-safety grade systems and then say, okay, I can have a lot of relief on my true ECCS systems because I have all this reliability from my -- you know -- additionally, my other nine ECCS systems.

So we may have to come up with a -- as we mentioned -- a sub-threshold, which would at least assure a minimum reliability of the pure safety-grade ECCS systems. So that's just another one of the issues that we're wrestling with.

MR. BONACA: This is mostly on BWRs, right?

MR. KURITZKY: Exactly.

MR. BONACA: And those which rely heavily on the procedures that you have in place on how integrated those systems are in the procedures?

MR. KURITZKY: Yes, that's the thing.

Certain systems -- like I think, I don't know, maybe the RCIC system, my understanding, may have a little better pedigree than some of the other systems. But again --

MR. BONACA: You know, if they're trained and they're used that way and there is a real hardened use for that, I think is different than purely if you have some hypothetical, you know, ideas that was implemented in the PRA but is not supported by procedures.

MS. DROUIN: Yes, I mean, because the -- a lot of these it's not hypothetical, but particularly when you look at boilers, they give a lot of credit to systems whose primary function, you know, is not --

MR. BONACA: Not ECCS.

MS. DROUIN: -- is not the core coolant.

MR. BONACA: Oh.

MS. DROUIN: You know, the service for the cross-tie, the fire water system, enhanced CRD. And they do have procedures in place at the plants for using these systems in those, you know, extreme cases.

But they aren't there for ECCS -- they are not ECCS systems. And with boilers in particular, they would have a very difficult time if we did give them credit for meeting any kind of CDF threshold value.

But as Alan said, on the other hand, you don't want them to come in and have so much credit that we'd back off on the ECCS.

MR. BONACA: Right.

MS. DROUIN: And have a reliability of -- that's unacceptable to us.

MR. KURITZKY: And I guess one of the other things that will come up when we -- or that we have to consider when we look at the non-ECCS systems is that right now the ECCS performance calculation is just to look at the safety where you need the actual ECCS systems.

And so when you make sure you meet your 20-200 DUEF (phonetic) threshold you have, relying on just those ECCS systems. If we're going to credit RCIC or service or cross-tie or something like that, you know, there are at present no calculations demonstrating that they can meet 20-200.

MR. BONACA: Right.

MR. KURITZKY: So we wouldn't want to have the calculations need to be run. So we would want to credit systems where it would be fairly obvious that the function could be accomplished. So that's -- those are three of the biggest issues that we're facing.

MR. BONACA: And I guess the same would be under the fit for PWRs.

MS. DROUIN: Yes. Yes.

MR. BONACA: How credible is it that you complete the fit, first of all, for the given plant and then what credit do you give the function so that you don't degrade the reliability of the ECCS?

MR. KURITZKY: Okay. So

CHAIRMAN SHACK: How would you -- or how do you propose to address the LOCA frequency for the non-pipe break LOCAs? Is this a database thing again, you would look at experience and try to do estimates on that?

MR. KURITZKY: That's kind of up in the air right now. As I mentioned, we're going to try and get some meetings together with some of our -- the engineering folks and the PRA folks to kind of has some of this out.

It may not -- you know -- it may be some kind of bounding. We may put some kind of bound on the numbers from 57.50 to try to account for some of these other mechanisms. Some of them specifically may have some data on them.

For instance, seismic LOCAs, you know,

seismically induced pipe breaks, okay, or at least not data, but the analyses show that they're very low contributors. On the other hand, seismically indirect -- you know -- seismic indirect LOCAs where you've failed the supports on something and that falls and breaks a pipe, well, the models show that to be on the order of what you're getting from just the pipe break LOCAs.

So in some of them we have some kind of -- you know -- we don't call it data, but we have some kind of models that give us some feel for what kind of contribution they're going to make. Other ones are really kind of floundering right now.

The shut-down conditions, drain-down events, we really don't have very good weight of data for that. There's not very many studies that are out there, at least not -- and not in this country particularly.

So it's -- right now we don't know exactly how we're going to address that. That's why that issue is out, you know, we're taking input from anybody who wants to give us input on issues like that. I don't have a proposal right now. Okay. Well, let me just --

MS. DROUIN: No, let's keep going, timewise.

(Pause)

MR. KURITZKY: Okay. So let me just move on to the other piece that we had in SECY 133 was the long-term piece looking at the possibility of redefining large-break LOCA or the spectrum of breaks that would be considered in the 50.46 analyses, and Carolyn Fairbanks.

MS. FAIRBANKS: Yes. We've just really initiated the work on this long-term objective out of risk-informing 50.46. The objective here, which would be at a time line of about the end of three years would be to have a tech basis developed for redefining the large-break LOCA.

We've developed a program approach here. The approach that we were taking was really trying to parallel the work that's being done to revise the PTS rule, pressurized thermal shock rule. We wanted to do this and we've had a meeting with industry a couple months ago to relay that and to say that this is our objective in following this example, to have a level of rigor in our approach that's equivalent to that that's being pursued with PTS.

So far we are doing some work on tasks one and tasks three. There are some existing codes on probabilistic break codes, probabilistic fracture codes. We're adding at this point some sub-critical crack growth modules.

As Alan described, there are some issues as far as indirect failures that --

DR. POWERS: I'm just a little confused when you say the word "codes." You're not talking about the SME code or anything like that?

MS. FAIRBANKS: No. No. No. This would be programs.

DR. POWERS: Computer programs.

MS. FAIRBANKS: Programs, computer programs.

DR. POWERS: And --

MS. FAIRBANKS: Modeling.

DR. POWERS: -- and so which ones are those?

MS. FAIRBANKS: We're starting off initially, we're just about done I think adding some subcritical crack growth modules to the squirt code, which is a probabilistic break code. There are a number of other codes, P-squirt, PROLBB, and we're not really sure --

DR. POWERS: I'm wondering --

MS. FAIRBANKS: Pardon me?

DR. POWERS: -- I'm wondering just which of these codes predicts the cracks like in plants like Summer and places like that.

MS. FAIRBANKS: We have to add that in. Jumping a little bit ahead, the --

DR. POWERS: How about the cracks that we have not seen today but will appear next year?

MS. FAIRBANKS: Jumping a little ahead again, those are things that are we going to consider, and they are difficult to do. A year ago nobody would have thought we would be having to add in some BWSCC modules; Summer occurs, and we're going to be doing that.

We're not far along, not far enough along yet to have that done, and if we look back historically, we do see that new degradation mechanisms do arise, some more significant than others, but there is a history.

You know, we have a little interesting plot, about every seven years there's something new.

DR. POWERS: Yes.

MS. FAIRBANKS: And so I don't think anyone would feel comfortable in risk-informing this without accounting for some possible future degradation mechanism. How we're going to approach that we haven't quite settled yet, but that's certainly one thing that's going to be incorporated into this work.

MR. KURITZKY: And I think also the point that -- of course, we definitely need to focus on, but also, each of these clear mechanisms shows up, you know, I think one time Mike Mayfield showed a slide that showed like every seven years a new mechanism shows up that they hadn't thought of before.

But in all cases these mechanisms that have shown up, we don't have a LOCA so far, knock on wood, and we'd identify the mechanism and then we do some analysis of it. We come up with some type of response or something that's implemented to try and control it.

And so we've managed to avoid having a LOCA. So in fact, yes, we do need to consider that there could be other mechanisms that are going to crop up and get us as time goes on.

But we also, you know, keep in mind that once that mechanism -- unless that mechanism shows up and it acts fast enough that we end up with a LOCA condition right away -- we'll evaluate it and try and address it so that, you know, corrective actions will be taken to minimize its impact on the LOCA frequency.

That's just one other thing we need to consider.

MS. FAIRBANKS: That's something that we really think, is considering our input, too, with LOCA frequency for the LOCA/LOOP.

DR. POWERS: It sounds like, if I follow your logic, we should get rid of these things. I mean, you say we got a lot of band-aids, and we don't really need band-aids, because nothing ever results in a LOCA. So why worry about it?

MR. KURITZKY: Like I say, with the band-aids, your band-aids is what -- the corrective actions that come up when we discover these things, we need those band-aids. That's what keeps the LOCA frequency low.

If we didn't put these -- if primary -- crack and popped off and we just said, well, that's a new mechanism, okay, very -- that's good, let's keep going the way we're going, then I would expect to see an increase in that LOCA frequency.

I'm not saying we wouldn't see one now anyway because of this mechanism, but the idea that we find it, now we address it, we put in corrective actions to minimize it.

So you wouldn't expect to see, even if there is going to be some delta in the LOCA frequency, it wouldn't be of a magnitude as if you just let the thing go off on its own and didn't address it. So there's some kind of self-correcting or self-modulating type of mechanism there.

MS. FAIRBANKS: Also shoes that as we're approaching this we're looking at all of the different type of crack growth modules to incorporate axial, circumferential and surface cracks. They will all be incorporated into the modeling.

We had a -- as I said earlier, we had a meeting to convey this approach to industry, and right now we're trying to support PRAB to the best we can with our knowledge and our plants, that we're using it in development of our break frequency and pipe size diagrams to support the --

DR. POWERS: What is PRAB again?

MR. KURITZKY: PRA -- it's probable --

MS. DROUIN: That's us. What Carolyn is getting there is that --

DR. POWERS: I thought it was a code for a second.

MS. DROUIN: -- our technical work on the reliability side, we're supposed to be finished, you know, early in the spring time frame. We don't want to move forward with a recommendation and then say a month or six months later they come in on their long-term work and the frequency numbers that we're using are totally not in agreement with them.

So we're trying to have some kind of convergence here between what they're doing on the large pipe break size versus what we're trying to do on the reliability.

MS. FAIRBANKS: And it is a complex issue, because in the approach for this we're even at the initial point of deciding which pipe break sizes we're going to look at, which lines those would be on, what is the -- what are the operating conditions, what are the environments for that size of diameter, which degradation mechanisms would be potentially involved in those.

And so you're not necessarily going to get a curve when you're looking at the break size versus frequency. I think that is really just wrapping up where we are at this point. We are pursuing this --

MR. BONACA: Just to understand a little better, just to know.

MS. FAIRBANKS: Okay.

MR. BONACA: For example, clearly, I mean, you have a whole issue of crack growth and development in two different size breaks, depending, you know, I think there is a time parameter there. I mean, you have a crack initiation result.

Now, are you looking also at how a seismic event could result in a certain break size, given that you have an identified crack?

MS. FAIRBANKS: Yes.

MR. BONACA: For example, there is some event where you have a crack or multiple cracks and now you do have a seismic event, and so you will be looking at that?

MS. FAIRBANKS: Yes. We'll be looking at all the modes.

CHAIRMAN SHACK: It's more difficult than it is in the PTS case, though, where, you know, you sort of assume that the only flaws you have to worry about are due to fabrication. You know, when you allow flaws to suddenly appear --

MR. BONACA: Right.

CHAIRMAN SHACK: -- and to grow, life gets a great deal more difficult than the --

MS. FAIRBANKS: And repair welds, many things are going to come --

CHAIRMAN SHACK: Yes.

MS. FAIRBANKS: -- up to complexity.

CHAIRMAN SHACK: But I guess in your case, though, I mean, the frequency you need to get rid of a large break LOCA as a design basis accident is really probably a whole lot lower than you need -- although I guess you want to use the most realistic.

I mean, it's not as though you're going to just stick a sort of a conservative estimate, because you can move on. But in reality, you could have a fairly conservative estimate for the large break LOCA and still be able to obtain some relief, I would think, in the simultaneous LOOP, wouldn't you?

MR. KURITZKY: The thing -- difference being is that right now, since we don't have LOCA frequencies by pipe size, which is something that we idealistically hope that we can maybe get in the longer-term, right now we have to deal with the large-break LOCA category from a PRA stance, which is essentially something six inches per PWR, every six inches and above.

So we have to make it hold for everything six inches and above. In the future we may be able to show that the ultimate frequency you need to get rid of a design based axiom, yes, we need to be lower.

But you may be able to do it with, say, a 14-inch pipe or a 12-inch pipe, and that frequency may be much lower, while the six-inch break, you know, you may give up something. So you're right. You can definitely get a much lower frequency to get it out of the design basis.

But because we can't do it by a pipe size right now, we have to do it for everything that's, say, six inches and above that we can't afford to have too many layers of, you know, conservatism piled on there to, you know, address various uncertainties before we would start to run into a -- we lose our flexibility also.

MR. BONACA: Although you mentioned before you are going to include consideration of heavy loads, for example, in heavy loads then the frequencies associated with a procedural violation of some type is a measured one, perceive that, you know, it's -- I think it's more likely that you have that happening than certainly you have just a mechanistic double-ending break. So that may drive up your frequency quite a bit.

MR. KURITZKY: Well, except that it's not just a question of having heavy load drop. It has to be -- the operations have to be taking place during power. You have to be doing something that's inside the containment where you're over --

MR. BONACA: I'm talking about maybe

you're -- you know -- you're in shutdown conditions.

MR. KURITZKY: Yes, for the -- oh, for a shutdown.

MR. BONACA: You haven't removed the head yet. You have the fuel there and whatever. You're moving some heavy load and, you know, a heavy load have enough to do damage.

MR. KURITZKY: Yes. In fact, at shutdown, that's one of the -- like I mentioned before -- that's one of the things we're really struggling with right now because of all types of -- the drained elements or LOCA initiates that could have occur at shutdown.

MR. BONACA: You know, that could drive your frequency there, because I mean, that's a procedural violation of some type.

MR. KURITZKY: Something we have to consider.

MS. FAIRBANKS: I think we can move along.

MS. DROUIN: Move along?

MS. FAIRBANKS: No other questions.

MS. DROUIN: Okay. Well, at this point we've gone through the four different areas covered by 50.46 and tried to give you an update and status of where we are at. Just going back to what we had said in the SECY in terms of our schedule, we had for the evaluation model and the acceptance criteria that that technical work would be done, you know, on or before July 2002.

That work right now is still on schedule, and as you've seen, you know, we're deviating a little bit from what we said in the SECY in terms of what we would do there. On the risk-informed alternatives to the reliability requirements, our work there is due in April.

It's going to be very tight to us trying to meet that schedule, a lot of issues there we're still trying to --

MR. KURITZKY: Oh, that's 2002? I thought it was 2003.

(Laughter)

MS. DROUIN: On the definition of the break size.

DR. POWERS: I just have to interject. One of the commissioners has bet me that he will not have to vote on this during his term of office.

MR. KURITZKY: How many years do they have left?

DR. POWERS: If I told you that I'd reveal which commissioner it was, but several.

MS. DROUIN: You mean, for the break size?

DR. POWERS: He said he would not have to vote on any revision to 50.46 in his term of office.

MS. DROUIN: Well, if he doesn't vote, then he, you know, we can't go forward with that break size. I mean, he's kind of set the -- he's stacked the deck -- yes.

CHAIRMAN SHACK: He said it would never come to him to vote. Looks like -- I mean, we're still working on the SECY. The vote is a long way down the road.

(Laughter)

MS. DROUIN: Yes.

DR. POWERS: I think I'm going to owe him some money here.

CHAIRMAN SHACK: But the most substantive thing that's changed since 133 is the skepticism about the possibility of relief for the K curve.

MS. DROUIN: Yes.

CHAIRMAN SHACK: I mean, you were more optimistic in 133 than you are --

MR. KURITZKY: Right.

MS. DROUIN: Correct.

CHAIRMAN SHACK: -- from today's presentation.

MS. DROUIN: Correct.

CHAIRMAN SHACK: But everything else is more or less following, which you laid out --

MR. KURITZKY: Right.

CHAIRMAN SHACK: -- in 133.

MS. DROUIN: That's correct. But you know, our technical work on 50.46 is not tied to any commissioner's vote. It's the rulemaking. So -- but we plan to have all of our technical work done as we laid out in 133. The public has been very interested in this program, followed it closely.

We have had a lot of meetings with the various stakeholders. We can -- we plan to continue having a lot of meetings with the stakeholders. We just have listed a couple there that's happened back in August on the LOCA frequencies.

We had another one on the LOCA and new frequencies. We just had a second one in October. We plan to have another one at the end of November. So I mean, almost on a monthly basis you can see we're meeting with the stakeholders on this program.

They've been following it very closely, giving us a lot of good data, also working with us. So on that note, that kind of sums up where we're at on the status for 50.46.

CHAIRMAN SHACK: You want to make any

comments? Okay.

MS. FAIRBANKS: No comments.

SECRETARY HORN: We were scheduled to have a presentation from NEI, Tony -- Mr. Angelo, but he's had other commitments and so can't make it today. And I think we're done with the formal presentations. Final comments from members of the subcommittee?

MR. BONACA: I think it was an informative presentation, particularly when it came down to the Appendix K tradeoffs that are really in the Appendix K model, and then for which we need the credit, really, from the K-8 curve.

I mean, that was quite a bit of information. I think that that was quite valuable. I don't have any other specific comments, except it may be a long time before we have that relief, and this work seems to be --

CHAIRMAN SHACK: Where's that low-hanging fruit?

MR. BONACA: Yes.

MR. KURITZKY: Drying up.

MR. BONACA: And I thought it was in general a very good presentation.

DR. KRESS: I thought so, too, and RP --

it looks like Option C on that Appendix K is probably the best one, plus continuing with the action plan to get that additional information. I worry about using frequencies to eliminate the LOCA/LOOP combination, because I've been considering this a defense in depth type of approach.

For sabotage and other things, I think there are other ways you can get LOCA/LOOPS at the same time, but I haven't thought enough about it to get a firm position on that.

MS. DROUIN: One of the things I didn't say and we tend to forget about our framework document, but we do follow the framework very closely. And the framework does not allow us, regardless of what the numbers say, to violate those six elements of defense-in-depth.

DR. KRESS: That's true, yes.

MS. DROUIN: So you know, we tend to talk a lot about the numbers and we tend to, you know, not verbalize so much the framework, but everything does pass through that framework and you cannot violate one of those defense-in-depth elements, regardless of what the numbers tell us.

DR. KRESS: I guess we're supposed to decide on what to do about the full committee?

CHAIRMAN SHACK: No. I don't think we had --

DR. KRESS: We don't.

CHAIRMAN SHACK: -- planned to have a presentation to the full committee.

MR. MARKLEY: Just a Subcommittee report.

CHAIRMAN SHACK: Just a subcommittee report.

DR. KRESS: Okay. Good.

MR. BONACA: You know, just to interject on the issue of LOOP. I mean, I agree that's an issue of defense-in-depth that makes you uneasy when you think about eliminating it if you really want to cover all the bases.

But one of the big challenges of the licensees is really the frequent start of the diesels, you know, cold and the wear of the diesels, and also the very strict and demanding requirements imposed on a lot of systems like, you know, HVAC and so on and so forth.

I mean, does it mean that you -- I mean, could we possibly have still a design capability for LOCA and LOOP with less demanding requirements imposed on the equipment from a perspective of testing?

Couldn't you look at the risk significance of reducing some of the tests imposed on the equipment that really are the driving force right now for the licensees on requesting some relief?

MR. KURITZKY: Well, I think in regards to that, I mean, most plants' technical specifications have already been changed so that that fast start of the diesel --

MR. BONACA: Yes.

MR. KURITZKY: -- only has to be done -- used to be done once a month, but it's down to, I think, once every six months.

MR. BONACA: That's true.

MR. KURITZKY: Assuming you pass it. So there has been some relief in that regard already. What it comes down to is do you still have to have your diesel designed and your equipment designed to come up at a certain time.

And that's governed by what kind of flow rates -- you know -- what kind of flow rates have to break and how quickly you have to get water back in the core cylinder. And how big a break you're going to consider and what the frequency of that event is, you know, determines the time when that diesel has to come on board.

But as far as the testing, I think most

plants I think already have managed to reduce that, you know.

MR. BONACA: I've not seen, but some of the systems like the HVAC system and the time you need to draw a vacuum in the enclosure buildings, I mean, that's stuff that is tremendously demanding on the plants and on the equipment.

And so anyway, but that's just a thought that one could also determine the risk increase associated with the relaxation of some of the requirements, and they may find that you can still have a high expectation of success of a LOCA and LOOP, you know, coping with both of them by imposing less restrictive requirements.

I agree with the -- on the start of the diesel. That's one thing that has been done. But again, many of the other systems have not been relaxed at all, their testing.

MR. KURITZKY: That's something we can look into.

CHAIRMAN SHACK: Well, I guess, you know, when I -- I'm not as concerned as Tom is about the defense-in-depth, because when I looked at the proposed changes I don't see that those really affect your defense-in-depth very much.

You know, if you were proposing to remove some equipment, but the kind of changes I foresee, at least from this part, I think eliminating the large break LOCA as a design basis accident could have a more substantive impact than what one would consider defense-in-depth. But this part doesn't seem to me to impact it as much.

MS. DROUIN: Well, you may have some PWRs out there who would like to get rid of their accumulators, and they could.

CHAIRMAN SHACK: Under this.

MS. DROUIN: Under this.

MR. KURITZKY: Well, so far we have to wait and see, but it doesn't even look like they're going to get too much in that regard, because most of the IPEs or the PRAs are going to correct the accumulators for the smaller breaks, also, what's called medium breaks.

And so therefore, you may not -- from a reliability point you still may not make the grade. But it's possible that maybe for A, if you have a spare accumulator, you know, that you may be able to relax the allowed out of time on it.

But again, whether or not somebody takes something out of a plant or whether this gets relaxation in their technical specifications, I think we're going to have to decide, the staff will have to decide whether or not -- you know -- even if the numbers come out that show that you could do that, we have to decide whether or not that meets the defense-in-depth filter or whether or not we're just comfortable with that.

And we may want to put some kind of limitations on that, some kind of restraints.

CHAIRMAN SHACK: Dana?

DR. POWERS: I guess I opened the Pandora with the sabotage concern, the defense-in-depth, and I think I still wrestle with defense-in-depth and what we're -- where it is and how it is in the regulations and how far it goes.

I come back to the earlier presentation, I look forward to seeing what comes out of these studies on the oxidation of clads and your circumstances. And it looks to me like what -- after a lot of complications of phenomenological discussions -- that the basis idea that you will only go after the 17 percent oxidation and replace it with an embrittlement criterion seems like a pretty good idea to me.

I mean, I feel sorry for those that have to demonstrate that they have retained ductility, because I think that's a much harder analytic tool than just showing that you don't hit 17 percent oxidation using Baker-Just kinetics than in a fairly simple temperature transient.

But I think it's a much more defensible thing and the challenge that the industry would face in showing that the oxidation kinetics of every type of clad that came along was bounded by Baker-Just might prove as much of a challenge as the calculation of ductility.

But I guess it's -- I mean, I think before you finalize on this we just have to wait for the experimentalists to get their act together and get some data, and it takes a lot of data.

CHAIRMAN SHACK: Other comments?

MS. DROUIN: I have a question. You know, we were asked last time, you know, to keep the ACRS informed, you know, of the status. I look down the road to the future of, you know, where our next milestones are and, you know, the technical work on the reliability parts to be done at the end of April, the acceptance criteria and evaluation model in July.

My question is to, when would you all be interested in seeing us again, and would it be at the

Subcommittee level, the full committee level?

DR. POWERS: You know, my initial reaction, of course, Mary, is the committee is always delighted when you want to come talk to us because you always have something interesting to tell us, but I think that really it's up to you.

And I think you've got a good strategy here that you're trying to pursue and I think it's when you think you have enough substance in there to get some opinions instead of some speculations and questions --

MS. DROUIN: Okay.

DR. POWERS: -- that we want to hear. And I would suggest this is before the full committee because everybody has a stake in this. And so you want to ask not only for a time, but enough time so that you can -- you're not rushed in getting your points across.

So don't let them short-change you on time, in other words. But I think it's really up to you because you know your schedule and the various challenges. It's when you think you've got enough to -- this to mark some point on it, rather than any -- rather than judging on the calendar list, judge based on the progress of the work.

MS. DROUIN: No. No. I agree and I didn't know if there was --

DR. POWERS: Sure.

MS. DROUIN: -- something here in particular that you wanted to hear back again on, but absolutely, when we feel we've got --

DR. POWERS: Your current milestones sort of suggest something like April or May --

MS. DROUIN: Yes.

DR. POWERS: -- when you've finished the reliability. But obviously, if you're not there, you're not there.

MR. BONACA: And the other thing, the other criteria I would use is if you feel there is some surprise for us. For example, today I think was a very valuable presentation because at least for me I thought that the decay heat curve was a low-hanging fruit, and now I've been, you know, educated on that.

And I think that was an important time to hear about that. Otherwise, we would be still proceeding in our mind with the thought that, here it comes, you know.

MS. DROUIN: And that's why we felt it was important to come now.

MR. BONACA: Yes.

MS. DROUIN: Because we were deviating from what we had in the SECY, and what we had told you guys in our letter.

DR. POWERS: I guess one of the real problems that the NRC will face, Mario, is just that lots of people think that decay heat curve is a "gimmee." I mean, I've even characterized it as the "gimmee."

MR. BONACA: Yes.

DR. POWERS: And there's going to be -- they're not going to have had the benefit of the discussions that we went through and so they're going to persist in taking that.

I'm wondering if maybe you shouldn't think seriously about formulating that into a paper that you could give to -- before the ANS or some body like that and try to socialize the opinion to at least in some sense take the weight off your back, because you're going to have a lot of people says, ah, NRC, they'll never change this.

I mean, you're just -- it's going to be conventional wisdom and maybe you ought to reach out a little bit to the -- at least the technical community and advertise that position.

MR. BONACA: I think that's an excellent

recommendation because I thought that the presentation that you provided, Steve, it was outstanding in that sense. I think it was very pointed.

It showed some of the effects and really was a good -- you know -- I mean, you come out of it, you can ask some pointed questions to that, but then you come up with an understanding of what the issues are.

And I think that that would be valuable because I think there is -- in my judgment there is a widespread belief that that's an easy tradeoff and how come the NRC's not moving on this.

MR. BAJOREK: By the way, a lot of those figures that I did show, we're intending to put that in a OCONEE paper, submitting that at the end of the month.

DR. POWERS: You know, that's not -- I mean, that's the kind of forum where you need to socialize these ideas and what-not and put it before the technical community. If they find fault with it, of course, you learn something.

But if you're of sound position, then they learn something. So I mean, there's no loss here.

MR. KING: We'll take a commitment to do that, figure out the right forum and the right vehicle.

DR. POWERS: Yes, you know, because otherwise, you get this very unfair accusation because people just haven't seen that sort of stuff.

MR. KING: Right.

DR. POWERS: They haven't thought about it as much as you have.

CHAIRMAN SHACK: Yes. I mean, in July you just sort of had a general statement that, you know, you have to consider the compensation non-conservatisms, but now you have actually something fairly specific, and you know, I think it makes it a much more substantive case than saying, there may be non-conservatisms that won't be bounded.

DR. POWERS: And it gives the model builders some grist to think about, too. They may find that, well, you're talking about RELAP having some less than desirable features perhaps in the code. So maybe people developing models can think about it. We'll see.

MR. BONACA: The other thing which is significant here is that so many of the comparisons were based on calculations performed by licensees.

DR. POWERS: Sure.

MR. BONACA: And so those are facts,

really, and not speculation on the part of the staff, really. It's coming out of presentation and some meters provided by the licensees.

CHAIRMAN SHACK: If there are no additional comments we can adjourn the Subcommittee meeting.

(Whereupon, this ACRS/ACNW Joint Subcommittee meeting was concluded at 12:09 p.m.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: ACRS Joint Subcommittee Meeting

Docket Number: (Not Applicable)

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Pippa Antonio

Pippa Antonio
Official Reporter
Neal R. Gross & Co., Inc.