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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

APRIL 27, 2000

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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MATERIALS AND METALLURGY AND RELIABILITY

AND PROBABILISTIC ASSESSMENT

U.S. NRC

TWFN 2B3

11545 Rockville Pike

Rockville, MD 20852-2738

Thursday, April 27, 2000

The subcommittees met, pursuant to notice, at 1:00 p.m.

MEMBERS PRESENT:

GEORGE APOSTOLAKIS, ACRS, Chairman

WILLIAM SHACK, ACRS, Chairman

MARIO BONACA, ACRS, Member

JOHN BARTON, ACRS, Member

TOM KRESS, ACRS, Member

ROBERT SEALE, ACRS, Member

JOHN SIEBER, ACRS, Member

GRAHAM WALLIS, ACRS, Member

PROCEEDINGS

[1:00 p.m.]

DR. APOSTOLAKIS: The meeting will now come to order. This is a joint meeting of the ACRS Subcommittees on Materials and Metallurgy and on Reliability and Probablistic Assessment. I am Dr. George Apostolakis, Chairman of the Reliability and PRA Subcommittee. Dr. William Shack is Chairman of the Materials and Metallurgy Subcommittee.

The other ACRS members in attendance are Mario Bonaca, John Barton, Tom Kress, Robert Seale, John Sieber, and Graham Wallis.

The purpose of this meeting is for the subcommittees to review a draft Commission paper concerning options for potential revisions to the pressurized thermal shock rule acceptance criteria. The subcommittees will gather information, analyze the relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberate by the full committee.

Mr. Noel Dudley is the Cognizant ACRS Staff Engineer for this meeting. The rules for participation for today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on April 5, 2000. A transcript of this meeting is being kept, and will be made available, as stated in the Federal Register notes. 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.

At our March 16, 2000 subcommittee meeting, the staff introduced the different regulatory approaches it was considering in developing the draft Commission paper. At that meeting, the staff had not decided which of the approaches it would recommend to the Commission. We will now proceed with the meeting, and I call upon Mr. Mark Cunningham, Chief of the PRA branch of the Office of Nuclear Regulatory Research to begin.

It's a friendly crowd, Mark, here.

MR. CUNNINGHAM: I hope so. With me today is Ed Hackett, who's the Acting Chief of the Materials Engineering Branch and the Office of Research. Ed's branch has the overall responsibility for orchestrating this revision to the technical basis to the PTS rule.

Right off the bat, I'll apologize because I just noted that we're presenting to the Materials and Metallurgy Subcommittee and not a joint subcommittee meeting, so my apologies, and it won't happen again.

By way of overview, we'd like to discuss this afternoon the draft Commission paper which we provided last week, I guess, to the committee. Talk about several things with respect to this paper. One, the purpose of it; talk a little bit about what's included in the paper on the PTS screening criteria as it currently exists in the rule; talk about three sets of information that are more recent than when the rule was established that are relevant to possible changes; what's going on in materials research; what's going on in terms of the Commission guidance with respect to use of PRA; and more information on what's our understanding of severe accident phenomenology. The paper then has four options for modifying the screening, potential screening criteria in the rule, and we're going to lay out what those options are.

Basically I think we'd like to solicit comment from the committee or the two subcommittees on the options that we've laid out in terms of the completeness of the set of options, if you will. Is there an option that we hadn't thought of that might be appropriate, or is there some clarifications we could make on the existing options, and if there's any opinions from the subcommittees on what option they see is most appropriate.

We'd probably need to finish the day with some discussion of what you'd like to hear about from us at the full committee meeting, and I think we mentioned this at the last subcommittee meeting, that we would ask for a letter on this draft paper that you have.

I should note that in parallel with meeting with the committee and seeking your opinions on this, we're continuing to meet with the NRR staff and with the Office of General Counsel's staff to look at these options and assure that they're technically on target and legally on target.

The basic issue that we've got here to discuss is how are we going to modify a rule that establishes an adequate protection rule but has probablistic underpinnings to it, and if we revisiting the probablistic underpinnings, what does that mean to how we might be able to change this rule.

DR. APOSTOLAKIS: I read it also in the -- how do we refer to this? Policy issue. The document.

MR. CUNNINGHAM: Yes, the draft commission paper.

DR. APOSTOLAKIS: The draft commission paper. What exactly is this article of protection thing? What does it do to this issue that it wouldn't do -- that wouldn't be there if it was not an article of protection issue?

MR. CUNNINGHAM: Well, one way to think about it is that they're -- the rules that we have could be set up in at least two bins -- adequate protection bins and cost beneficial safety enhancements. The station black-out rule, for example, was a safety enhancement rule.

If you're considering the cost beneficial safety enhancement rule, the proposed rule, you look at the costs and the benefits to be achieved from the rule. There's an approach that you use to resolve whether or not you implement that rule, and cost benefit is an explicit part of that process.

An adequate protection rule does not have cost benefit associated with it. It's determined that it is necessary to provide adequate protection to the public health and safety.

DR. KRESS: What criteria do you use to make that determination, Mark?

MR. KING: Well, maybe I can help a little. This is Tom King from the research staff. There is no quantitative definition of adequate protection. It's a qualitative judgment.

DR. KRESS: It's a judgment, qualitative --

MR. KING: It's a qualitative judgment, and the issue, in addition to can you consider costs or not consider costs, there were some numerical guidelines established that Mark's going to talk about. Back in 1983 when the original rule was put in place, that are not consistent with -- remember that was before there was a safety goal policy. Before, there were the reg analysis guidelines that we have today that are not consistent with what's on the books today. So, that's an issue that has to be worked out in discussing these options.

It seems to me what we want to talk about today is technically what makes sense in terms of looking at the options and picking the right option. As separate or parallel with that, we'll have to deal with this adequate protection question, but to me the first question is what technically makes sense, given the information we have today as to where we should go with PTS.

DR. SHACK: But you're talking about -- let me beat that once more. I mean, if you go to one of the options that essentially decreases the frequency, is that you now have to do a cost benefit analysis of that --

MR. KING: That's an open --

DR. SHACK: -- or are you arguing that you didn't have adequate protection before and now you need this to have adequate protection?

MR. KING: That's a question I can't answer today.

DR. KRESS: But none of the options decreased the frequency?

DR. SHACK: Now there's one that goes to one times ten to the minus six.

DR. KRESS: Yeah, but -- oh, there is one for that?

DR. SHACK: Yes.

DR. KRESS: I forgot. You;re right. I was thinking the five times two to the minus six was the lowest, but you're right.

MR. KING: Yeah, there are options that go both ways and stay where they are today.

DR. APOSTOLAKIS: But in terms of the goals, I don't know, are we going to talk about it later? Is it a bad time to raise these issues now?

MR. CUNNINGHAM: It might be better when we get to the options.

DR. APOSTOLAKIS: Okay, I'll wait.

MR. CUNNINGHAM: Okay. Just one point for what it's worth.

DR. APOSTOLAKIS: Sure.

MR. CUNNINGHAM: When we talked to the subcommittee the last time, we had an option on here that it's not in the paper and it's not in the presentation, and that was talking about a potential for a reverse backfit analysis, and that was in effect a cost benefit analysis of potentially relaxing the rule. The cost beneficial part of their backfit rule today is do the costs outweigh the benefits, or the benefits outweigh the costs of imposing a rule.

You could think about for some rules, if you're going to relax them, does the benefit in terms of the cost reduction isn't justified, given the risk increase you might get. In the context of this rule, because it's an adequate protection rule, we took out that option because it's been very clearly delineated that adequate protection rules and cost benefit are two different -- are not handled together.

DR. KRESS: Two different regions in a three region process?

MR. CUNNINGHAM: Yes, that's right. Yes, that's right.

DR. APOSTOLAKIS: That's the implication, but I'd like to understand it a little better, but I'll wait until you come to that option, which happens to be B.

MR. CUNNINGHAM: Okay, so the purpose of the paper that we provided to you is basically, just by way of background. As you're well aware, the staff has a fairly large effort underway to revisit and potentially revise the technical basis for the PTS rule. This was started for a couple of reasons. One is our experience in trying the implement the rule and the associated reg guide in the Yankee Rowe case a number of years ago, and also to reflect that in times since the Yankee Rowe decision, there's been a lot of research done on materials properties of reactor vessels that give us a much better understanding, we think, today of how these vessels would respond to a PTS overcooling event.

I'll come back to some of the materials research a little bit later, and we're not going to go into the details of the analysis or the program today. You've heard it at the last subcommittee meetings, and it's kind of -- we're going to focus on one particular issue, and that is basically the probablistic aspect of the screening criteria that underlies the rule, if you will. So, the paper itself is intended to provide a recommendation to revisions to one part of the screening criteria, and it's in the rule and, in effect, to put this before the Commission early in the PTS revisitation program so that we'll have enough time to respond to the Commission decisions and to modify the program if we need to to reflect what the Commission wants to decide on this one factor. So, that's kind of the narrow purpose of this paper and the briefing today.

DR. APOSTOLAKIS: This criterion will be set and without the benefit of the analyses that your staff presented here last night?

MR. CUNNINGHAM: The way we're looking at the criterion is kind of a top down thing. Given the basic policies, what would that tell us about the acceptable value as opposed to from the bottom up.

DR. APOSTOLAKIS: But sometimes it's helpful to also know coming from the bottom what kinds of numbers you're getting.

MR. CUNNINGHAM: Yes, that's right.

DR. APOSTOLAKIS: But you will be doing this without the benefit of that unless there could be some studies here and there.

MR. CUNNINGHAM: We're working first from the general principles down.

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: You know, the exact figure that we come up with in the long run may be reflected by some of the experience in actual applications.

Go back for a few minutes to the rule itself, and it was issued in 1983 as an adequate protection rule. As Tom mentioned a little bit ago, this was before the safety goal. This was before a lot of things that we had done in PRA. It was very early application of PRA in looking at regulations.

DR. APOSTOLAKIS: In what way was it and application of PRA? I don't know the history.

MR. CUNNINGHAM: Okay. I'll come back a little bit to that in a minute.

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: In the rule, there's an acceptance criterion set up in terms of what they call an embrittlement screening criterion, and that's in terms of the material properties of the reactor vessel. If you exceed that -- if a licensee determines that they're going to exceed that limit, that screening criterion, at the end of their life, the reactor vessel, then they have to do some additional analyses, demonstrate that the plant can continue to operate safely.

Reg Guide 1.154 was put into place to provide one way by which you would perform that safety analysis.

DR. KRESS: Mark, I have a couple of questions just for my education on that. One, if a plant actually underwent -- it's got its calculations and it's got its RTPTS value based on the calculation. Now it's going along operating and undergoes a PTS event which supposedly would do something to the cracks -- enlarge them, deepen them. Does it then go back and redo its RTPTS and set a new criteria based on the new crack properties or new crack size? Is it a dynamic thing, or is it set one time and that's it?

MR. MAYFIELD: This is Mike Mayfield from the staff. You set the RTPTS based on embrittlement which doesn't have -- it doesn't derive directly from loading on the vessel. If you actually had a severe overcooling transient, then history suggests that people end up doing some inspection of the vessel just to make sure it hasn't been harmed.

There is, in fact, an appendix in Section 11 of the ASME code that really comes from what happens if you exceed your heat-up, your pressure temperature limits, and

Neil Randall used to refer to this as the Friday afternoon got you kind of rule, and so you exceed the heat-up limits. What do you do? Well, there's some criteria that just says well, is it really a problem and if so, what you do about going on for some additional inspection. If you actually had a PTS event, you wouldn't reset the RTPTS. You might look at having to do some inspection of the vessel before you went back to power, but that would be different than worrying about the material property itself.

DR. KRESS: Well, what I had in mind there is that if you went back and re-did the PTS probablistic calculation, you would now come up with a new frequency or through-wall crack for that vessel, it seems to me like. It would change. Therefore, you would have a new value of RTPTS to be equivalent to the five times ten to the minus six.

MR. HACKETT: Let me see if I can address this a different way. This is Ed Hackett. I think, Dr. Kress, I think your question goes to maybe more the flaw distribution and what might happen to the flaws from such an event.

DR. KRESS: Yeah, it changes.

MR. HACKETT: One of the things we know for sure is that the vast majority of flaws that would be postulated to exist in a vessel weld, for instance, would not even participate in -- if they were hit with a PTS transient, they're not going to react to it, the vast majority of them.

So, I think the answer to your question is yes, there would be some adjustment, but I would wager that in a probablistic sense, it would be a very minor impact.

DR. KRESS: I was --

MR. MAYFIELD: Well, my guess is if you did the Section 11 inspection and you found a crack that had initiated and grown, you'd be doing a lot of analysis.

MR. HACKETT: If you did that inspection and --

DR. KRESS: If you found one like that, you would do so, okay. The other question I had, Mark, is are you going to discuss sometime the actual basis for the five times two to the minus six? They probably had some reason that they chose that as an acceptance criteria.

MR. CUNNINGHAM: Yes. Yeah, let me come -- I'll come back to that in a minute.

DR. KRESS: Are you going to do that later?

MR. CUNNINGHAM: Yeah, I'll do it in a minute.

DR. KRESS: Okay.

MR. CUNNINGHAM: Anyway, associated with this embrittlement screening criteria in the RTPTS is of a value of the through-wall crack frequency of five times ten to the minus six. It's linked in two places in the rule and the Reg Guide. One is the value that was chosen for RTPTS as an acceptable value of 270 degrees for some types of welds, is linked to the five times ten to the minus six, and I'll come back to that in a minute.

Also in Reg Guide 1.154, if a licensee is doing the safety analysis and does the PRA analysis that gives them an estimate of the frequency of a through-wall crack, the Reg Guide basically says if that value is a five times ten to the minus six or lower, it's acceptable.

DR. APOSTOLAKIS: Now, this is on the basis of point estimates?

MR. CUNNINGHAM: Let me jump ahead to one slide.

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: This slide is reproduced from -- I got it from SECY 82-465, which is a paper, obviously in 1982, which provided a lot of the technical information that was going to support the final version of the rule. What we've got is basically a curve -- this curve here is a summation of all the other curves that provides information on the frequency of different surface temperatures coming from a risk calculation. So, you could say they've taken information on the challenges -- the initiators that could cause PTS -- small locas, transients and that sort of thing, combine that with probablistic fracture mechanics information to provide estimates on the likelihood of having a through-wall crack if RP, RT and DT were at different values, if you will.

DR. APOSTOLAKIS: So the vertical axis is the frequency of through-wall cracks?

MR. CUNNINGHAM: Yes, yes, yes. That's correct, per reactor year, as a function of RT and DT.

DR. SHACK: So, okay, you'd get enough initiators in a year that if your RT and DT was 350, you would then get ten to the minus something or other?

MR. CUNNINGHAM: Something like that, that's right. The five times ten to the minus six comes in in one way here, one particular way. If you go to 210 degrees, you get -- I'm sorry. Associated with the frequency of five times ten to the minus six is an RT and DT of 210 degrees. There was an estimate made at the time about the uncertainty in this curve, if you will, at that location. It was an estimate that sigma, the standard deviation would be about 30 degrees.

So, the decision to make the RTPTS 270 degrees was saying well, we have -- at five time ten to the minus six, we have a value of about 210. We want to be confident that it's not going to really hit that, so we're going to move over to sigma and set the value to be 270 degrees. So, that's what shows up on the rule.

The five times ten to the minus six, to get back to Dr. Kress' question as I understand it, was not set from some global standard, if you will, to say that's an acceptable value based on high principles, if you will.

There were discussions about the safety goal underway when this was being established, and there were discussions about what was an acceptable frequency for core damage from any particular type of initiator, any type of accident, and that was generally talked about. It was about ten to the minus five per year. The five times ten to the minus six is what was more of an analysis of what was the frequency. It was more of a bottom up type of calculation. We think we can accomplish five times ten to the minus six. We think that's a reasonable frequency, and if we get there, it's probably okay.

DR. KRESS: That's where the five comes from.

MR. CUNNINGHAM: That's where the five comes from.

DR. KRESS: I was interested in that five. Basically, then, you're saying that if one had an acceptance criteria, say, on CDF, and if that value were ten to the minus four per year, and if one looked at the set of sequences that contributed to that -- they're being in PRA, so there might be something like ten to a dozen sets of them.

MR. CUNNINGHAM: Yeah.

DR. KRESS: If you say you had a principle that you don't want to be overly influenced by any one of those, if that were the principle, then you may divide the total by a factor of ten or order of ten. You end up with ten to the minus five, roughly.

MR. CUNNINGHAM: Right.

DR. KRESS: Is that the rationale you end up getting a number like that?

All of the discussion -- in 1982, that discussion was held with the advisory committee, among other places, but there was a general sense, although the safety goals weren't established, that ten to the minus five for an individual set of sequences was probably about the right value. We'll get to in a little bit, you'll how a little bit later in time that showed up in the black-out rule and the atlas rules and that sort of thing.

DR. APOSTOLAKIS: But I -- I'm sorry, go ahead.

MR. CUNNINGHAM: The difference between the five times ten to the minus six and the ten to the minus five, there was discussion of whether or not --

DR. KRESS: There's two really important thoughts in that. One of them is -- number one, you have to have an acceptance criteria.

MR. CUNNINGHAM: Yes.

DR. KRESS: All right, now we're talking here about ten to the minus four, but in my mind, that's not synonymous with adequate protection. That's something else.

The other important thought in there is should one allocate among the sequences an acceptance criteria and what rationale should one use in terms -- and how should one factor into that allocation the uncertainties in each sequence. Each sets of sequences have different uncertainties associated with them, and how does that enter into it? There's some real deep thoughts that go into that.

MR. CUNNINGHAM: Yes, definitely, and that type of discussion was held as kind of a backdrop to this. Again, the five time ten to the minus six per se didn't come from that type of --

DR. KRESS: It probably come out of a judgmental analysis.

MR. CUNNINGHAM: Out of the analysis that that was what you would get at this --

DR. KRESS: But you could.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: I'd like build on what Tom just said. First of all, I really -- if this is not an article protection rule, you can't use any of the goals we have.

DR. KRESS: That was one of my problems.

DR. APOSTOLAKIS: What's funny is you have to use lower -- higher numbers.

DR. KRESS: Yeah, minus three probably.

DR. APOSTOLAKIS: And the second question is I'd like to understand how this fits into the level three PRA.

I don't remember the sequence now, but is it really appropriate to worry only about core damage frequency? I mean, when you have this kind of failure, is the containment going to do anything?

DR. KRESS: Well, this is another issue with PTS. You should probably give it more thought because it's one of those things that could lead to early containment failure. So, yeah, this issue of is it a CDF or is it a CDF and a LERF at the same time really is important.

DR. APOSTOLAKIS: Or is it the same all the way to the QHO's? I don't know.

DR. KRESS: Yeah, so I think that's very important, George.

DR. APOSTOLAKIS: But not just the core damage.

DR. KRESS: How you deal with the acceptance criteria ought to depend on whether it's a LERF issue or CDF issue or both.

DR. APOSTOLAKIS: That's a question then, Mark. Why didn't you have in this write-up something about the PRA sequences and where this fits into the picture. I think that would have been very informative in placing everything into perspective because you are going later on and discussing what should I do about the LERF or what should I do about the CDF. Then it hit me. I said well, gee, but I really don't know. These arguments would have been much clearer if you had had some discussion earlier on how this phenomenon fits into the level two PRA or if necessary, level three, which I think would be very easy to do, you know.

DR. KRESS: Okay. I think you can almost bypass level three and talk about LERF.

DR. APOSTOLAKIS: Yeah.

DR. KRESS: Yeah.

DR. APOSTOLAKIS: All I want is a convincing argument. I'm not arguing, but I'm convinced, though, you have to look at level two.

DR. KRESS: Yeah. I think you have to look at the LERF part of level two.

DR. APOSTOLAKIS: Yeah, and some of the --

DR. KRESS: Not necessarily the vision products.

DR. APOSTOLAKIS: Yeah, yeah, that's right.

DR. KRESS: Yeah.

DR. APOSTOLAKIS: Because there are arguments in the options you are developing that really would become much clearer that way.

DR. KRESS: Okay.

DR. APOSTOLAKIS: The other thing, I think this issue of article protection is going to create headaches here because we have no numbers for article protection.

DR. KRESS: I think it's going to be a real headache.

MR. CUNNINGHAM: That's again, this is why this is kind of an interesting situation from a policy standpoint that you've got an adequate protection rule then you've got built into this indirectly and in some cases more directly unacceptable frequency, again, set 17 years ago. That's, again, part of the reason for getting this to the Commission is how do we deal with that.

DR. APOSTOLAKIS: Now, the other thing I -- I have difficulty with, you gave us an explanation why they weren't up to 270 degrees.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: Now, it seems to me that the uncertainties on the frequency, so it's vertical, and I don't know, by moving to sigma -- what sigma is that, on the RT and DT? How did they come up with that? It's a horizontal sigma.

DR. KRESS: Horizontal, yes.

DR. APOSTOLAKIS: Now, I don't know what kind of uncertainty it represents because there is also large uncertainty vertically.

DR. KRESS: That is almost strictly data, that horizontal one.

DR. APOSTOLAKIS: Okay, so the uncertainty, then, in calculating the core damage frequency is not there at all.

DR. KRESS: It's not in it at all, and that's one of the things they want to improve I understand, in the new process.

MR. CUNNINGHAM: Going back to what is my understanding of what happened in the early 80's on this, on that point. My understanding is it's a sigma in the material properties.

DR. APOSTOLAKIS: Oh, okay.

MR. CUNNINGHAM: Because it was believed at the time, with good reason, I think, that the dominant sources of uncertainty in this calculation were the uncertainties and the understanding of the flaws. Are they embedded flaws or surface flaws? What's the size of the flaws? What's the density of the flaws and that sort. It was believed that that was the dominant uncertainty, but again, that was 17 years ago.

So, the calculation of the frequency, the challenges, if you will, was a fairly, you know, not what you would do today, if you will, in terms of an analysis. It was an analysis originally based on I guess some work done by Westinghouse on some challenges to the vessel -- you know, perceived challenges of the vessel, and it was adopted for more broad use by the staff. It was a quite different type of analysis than what you would do today.

The sigma is 30 degrees. It's hard to relate it in the context of thinking about alliatory and epistemic and all of those things.

MR. WALLIS: This RTNDT, is the temperature different, or am I misunderstanding something?

MR. CUNNINGHAM: It's a measure of the material properties of the reactor vessel.

MR. WALLIS: Oh, it's a weird thing, so I can't --

MR. HACKETT: It's an index mark.

MR. WALLIS: I can't relate it to --

MR. CUNNINGHAM: Not being a materials person, I have a hard time thinking of this as a temperature, but people are apparently very comfortable with it.

MR. WALLIS: Yeah, so I probably won't understand it.

MR. HACKETT: It's an index, is what it is, and as mark mentioned, it's basically a material property that you're relating to, you know, the degree of embrittlement. Then what you're obviously trying to get to in this analysis is fracture toughness, and this is a way of indexing that's been used within ASME for many years now, indexing to the fracture toughness curves. That's really what it boils down to.

DR. KRESS: It actually can be looked at as a temperature because it's where you transition into ductility, and it's temperature related. It's related to a lot of things about the materials, but it's a material property.

MR. WALLIS: So, it's not a temperature difference.

DR. KRESS: No, it's a temperature.

MR. CUNNINGHAM: It's not. Just stick on -- go back to one point of Dr. Kress's a little bit ago. The difference between five times ten to the minus six and ten to the minus five, there was a discussion at that time of should we go with something like ten to the minus five, or should we work with what we think we can reasonable achieve, and given our state of knowledge at the time, of five times ten to the minus six. I believe it was Dr. Oakret on this committee argued, given the nature of this accident, let's keep it a little lower because in a sense, it was discussing the uncertainty or lack of knowledge.

DR. KRESS: There's some good rationale to that. Number one, here's a set of sequences that compared to some of the other sequences that contribute to CDF, have relatively large uncertainties.

MR. CUNNINGHAM: Yes.

DR. KRESS: At least perceive to have. Not only that, it's a set of sequences that probably could be viewed as have an impact on both CDF and LERF simultaneously.

Given those two things, they tell you well, maybe we ought to -- in view of that kind of thing, a defense in depth argument would say knock that down a little bit. Instead of having, dividing the total CDF by ten, let's knock this one down a little bit more because it deserves a little more attention. So, there's some rationale -- I mean, some good arguments. It's just how to put that in terms of specificity and quantifying it. That's the problem.

DR. APOSTOLAKIS: That's why I asked for the entries, to appreciate that.

DR. KRESS: Yeah.

DR. APOSTOLAKIS: The context.

DR. KRESS: Yes.

MR. CUNNINGHAM: Basically one way to think about all the work that's going on to revisit the technical basis for the rule is to go back and think about that it's going to reformulate that line, if you will, the colored line. We're re-looking at the frequency of the initiators. We're looking at what we would expect to be the accident response or the systems response to it. We're re-thinking the thermal hydraulics given what we know today. We're re-thinking the materials, given all the stuff that Ed will talk about in a few minutes. So, it's going to be a reformulation of that line.

DR. KRESS: And you're going to put vertical and horizontal uncertainty?

MR. CUNNINGHAM: And one of the key pieces of it is how you really assess the uncertainty in that line, if you will, as well. I believe when Nathan Su was here the last time we talked about trying to build a more formal uncertainty analysis into this whole process, and both to be able to understand what the uncertainties are and what's contributing to those uncertainties. All of that's going back to revisit that line. The issue for the paper is --

DR. KRESS: At the same time, you're going to revisit the five times ten to the minus six as a separate --

MR. CUNNINGHAM: As a separate -- as another piece of this whole process. What do you say about the right acceptance criterion, given all these changes?

Just to stay on that for a minute and go back to some -- we've touched on these already, but some of the key underlying assumptions of the 1983 rule in the context of the screening criteria was that through-wall crack frequency of five times ten to the minus six was acceptable.

There's also that a through-wall crack was equivalent to a large opening in the vessel. It was equivalent to core melt. So, there was no distinction made between starting -- once that crack may get through the vessel, you assume that you're going to melt the core.

DR. APOSTOLAKIS: Are we going to do that different now?

MR. CUNNINGHAM: I don't think we'll deal with that part of it differently today. I don't think we have the technology to really say much more about that today than we did 17 years ago.

The last point is that the argument was made in at the time was that if you have one of these through-wall cracks, you would not substantially affect the containment. So, coming back to your issue of level two and level three, there was a fairly strong distinction made that you may melt the core but that you're not going to fail the containment.

DR. APOSTOLAKIS: So, I can then still assume that the condition or containment failure probability is point one?

MR. CUNNINGHAM: The people in 1983, as I read the Commission paper, basically said it was much less than .1.

DR. APOSTOLAKIS: And what did we say today?

MR. CUNNINGHAM: We'll come back to that. That's a good question, and that's one of the questions we have to face.

DR. APOSTOLAKIS: They said it was substantially less than one? On what basis?

MR. CUNNINGHAM: The arguments that were made in the paper were more qualitative as to why it would not be substantially impacted.

DR. APOSTOLAKIS: So then they focused on core damage?

MR. CUNNINGHAM: Then they focused on core damage. So, again, this is 1983. There was -- we had never talked about LERF's. Conditional probabilities of early containment failure, we didn't talk in those terms back then, but that's a key factor in where we go from here. That's a key change in policy or practice of the agency that we have to address.

I'll turn it over to Ed here for the next two or three slides to talk about some of the basic materials information that led us to start the re-visitation of the rule.

DR. APOSTOLAKIS: Is the industry requesting that you change this criteria?

MR. CUNNINGHAM: The industry is very interested in working with us on this.

DR. APOSTOLAKIS: Who started this?

MR. CUNNINGHAM: The Office of Research started it by re-examining the materials. That's what Ed will go into.

MR. HACKETT: To follow up on that, the industry has been a full participant in what we've been doing. As a matter of fact, we're meeting with them on an ongoing series of meetings that I guess has been going on for about a year-and-a-half now next week, and the industry is doing a substantial portion of work on this project also.

MR. CUNNINGHAM: And particularly in the PRA. They're helping us, at least for two of the plants, giving us a lot of information where they have done PRA's for their own plants. We've got four plants that are having things studied here, and two of them were working from what industry has provided us, or starting from industry has provided us in terms of their estimates of the frequency of these challenges. The other two, we're getting a lot of information on frequency of initiators and that sort of thing.

MR. HACKETT: These next few slides are pretty much in the way of background and also probably review for the committee because, as Mark mentioned, this has been presented before. I think March 15, I believe, is when we last went over this, but kind of in the order that they're shown here on the slides, the most important driver historically and still is the case from the materials perspective has been the issue of flaw size, density, and location, particularly in the reactor vessel welds, which are usually the limiting considerations from a materials perspective.

What had been done before, when Mark mentioned 82-465, the distribution that was used at that time was the one attributed to Marshall in the United Kingdom. That was based, at least in part, on as much data as they had at the time, but it was not a whole lot. If you were to read the Marshall report, there was a lot of extrapolation that had to go into what they did. Since then, we've had the benefit of a fair bit of research that's been performed out of our branch in the Office of Research where we've actually done detailed ultrasound examinations of welds from vessels that never saw service, and then to confirm what was there, done destructive examination of those welds. That's what's described on this slide.

By and large what we've seen from that is a larger density near the clad base metal interface of small, I guess what I'd call indications, not necessarily flaws, and a lot less of flaws that participate in a PTS that would be affected, as Dr. Kress's question earlier, would be affected by PTS transient.

So, from this we drew some hope. I think what you'd see the theme through the materials research here is that these are reasons to say that we thought we were conservative previously, that maybe there's, you know, a good technical basis now for backing off some of the conservatism on the materials side.

The last point there is there is at least the hope in this that we develop a generalized statistical distribution of flaw sizes that would apply to USLWR's.

Maybe that will result, maybe not. What we hope to do is next time we come before the committee, we'll have the results of this study which is being done through expert elicitation and by I think September time frame, I think, is when we're scheduled to come back. We ought to know the results of that study. We know pieces of it now, and I think as a minimum, we can get down to flaw distributions that are specific to vendors or NSSS, you know, fabricators. So, we can at least go there, and that's a huge step over where we were in 1982.

To move on to the next slide, another piece has been irradiation embrittlement correlations. I think most of the committee is probably familiar with Regulatory Guide 199, Revision 2, on which all of our embrittlement predictions, correlations are based. We have since that time had some ongoing research to augment and enhance those embrittlement trend correlations. That work is now largely completed, and just to give you an example, the database is expanded by about a factor of four over where we were previously, and I think it's also a lot more rigorously defined than it used to be.

The net result is I think we have a better feel for the rigor and statistical distributions of -- and the uncertainties involved with the embrittlement correlations. Again, that's an improvement. By and large what this new embrittlement trend prediction does is taken on is own, would tend to improve the situation for PWR's on balance, at least the type of equation that we're looking at right now.

Not necessarily true for all plants but on balance, there would be an improvement in the trend with the embrittlement correlations.

Statistical distributions for material fracture toughness, historically when we've run the probablistic codes for fraction mechanics, the material fracture toughness values that have been used have been lower bound values from the ASME code. As Mark mentioned, a big part of us going through this project is to try and not do that, and everywhere we go making worse case assumptions and lower bounding things. We're trying to put specific uncertainty distributions on the individual pieces here so for the first time, these material fracture toughness curves will be address in a statistical fashion.

DR. APOSTOLAKIS: Now, what kind of uncertainties are presented there? Why are you uncertain?

MR. HACKETT: This is basically -- the uncertainty is several sources at least come to mind immediately. There's just the uncertainty that goes with testing of that sort in what's called the transition region for foritic materials like reactor pressure vessel steels, that there is an inherent uncertainty that goes with the material variability in that region, which can be significant, and for these materials, typically is.

There's also an uncertainty that goes with the test data and how you got there. For instance -- well, in this case, if you're just looking at the ASME curves, they were based on tests of fracture toughness specimens per ASTM standards, most of them per at the time ASTME 399 and since that, you know, augmented by test to other standards. So, you have the test uncertainty that goes in there, too. So, you're at least addressing those two aspects of the uncertainty. The overwhelming one would be just the variation you'd see in material toughness in the transition region.

DR. APOSTOLAKIS: So you have actual data and you estimate variances and so on?

MR. HACKETT: That's correct.

DR. APOSTOLAKIS: Or is it the judgment of people?

MR. HACKETT: It's actual data, actually an awful lot of data. Another thing I could add there is -- I forget the number of data that were involved in the original bases for the ASME curves, but there's probably about a tenfold expansion in the amount of data that's available there now Again, then you have to start -- you get into refinements here because not all of that data were generated in accordance with this original ASTM standard. There are other standards that apply now, but I believe it's tenfold or more expansion in the database for those types of materials over the last 20 years. So, the answer is yes, we are working from data.

DR. APOSTOLAKIS: So again, I have a particular pressure vessel. This is a unique vessel. That's what I own, and there is this uncertainty. What does this uncertainty mean, that if I order another one from the same manufacturer, the same specifications and so on, that fracture toughness would be different because of some random variations, or is it that I just don't know? I expect it to have the same value, but I don't know that value?

MR. HACKETT: What you said is one aspect of it. The other probably more important aspect is when you look at these welds in specific where a lot of influence -- you're seeing a lot of influence from the chemical composition variability within the welds themselves, mostly related to copper. So, what you're seeing, even within a specific manufacturer is the type of variability you might see through wall in that person's or in that manufacturer's weld just due to variation of copper in the weld wire that went into it. So, those are the kinds of things you're picking up.

DR. APOSTOLAKIS: Just for the alliatory.

MR. HACKETT: I would agree.

DR. APOSTOLAKIS: There's an element of randomness there.

DR. SHACK: Well, there's both.

DR. APOSTOLAKIS: Yeah, there's both, as usual.

DR. SHACK: In this particular curve, I think it's probably alliatory.

DR. KRESS: It's 95 percent alliatory.

DR. SHACK: The other curves are mostly -- the other uncertainties are mostly epistemic. This particular one --

MR. HACKETT: If we were to get back to the embrittlement correlation of curves, for instance, I consider that largely epistemic. I mean, if we had all the resources in the world and we could chase these things down, we would get better and better. To some degree for material fracture toughness, you're stuck with the inherent nature of material variability and transition.

DR. APOSTOLAKIS: Is there any particular reason why you're avoiding those words? I had to ask it to figure out what's going on.

MR. HACKETT: No, other than the fact that --

DR. APOSTOLAKIS: The statistical of this division is typically materials people use these.

MR. HACKETT: Yes.

DR. APOSTOLAKIS: But we have gone a little bit beyond.

MR. HACKETT: See, Mark prepared these viewgraphs, so I can blame Mark. I can say the short answer for me is probably --

DR. APOSTOLAKIS: It would be nice to explain those things somewhere.

MR. HACKETT: Yes.

DR. APOSTOLAKIS: Because, you know, the words carry some meaning there.

DR. SHACK: Nathan has a chart.

MR. HACKETT: Yes.

DR. SHACK: He has all the uncertainties labeled -- epistemic, alliatory.

DR. KRESS: That was a nice chart, by the way.

DR. APOSTOLAKIS: I can't carry that chart with me. I read this document. I must comment on this document.

MR. CUNNINGHAM: There were a lot of things, as I said, about the revisitation on the technical basis that we did not get into in this paper, and we purposely stayed away from some words like alliatory and epistemic because, at least as I perceived the paper, they weren't necessarily going to help us discuss the issues at hand in this paper.

DR. APOSTOLAKIS: When you get into the uncertainties, though, it's important.

MR. HACKETT: Yes.

DR. KRESS: Nathan's document was very helpful. That was a good document.

DR. APOSTOLAKIS: Why isn't it reflected here?

DR. KRESS: I don't know. Well, you could append it, I guess.

DR. APOSTOLAKIS: Statistical distributions means nothing. One of these days, we should have non-statistical distribution.

[Laughter.]

Anyway, we're making a big deal out of it. I'm just wondering why these words don't appear here anywhere.

DR. SHACK: It's the code for saying they have data.

DR. APOSTOLAKIS: Well, I don't know. All three of these -- I mean, statistical distributions for both of these. They are both alliatory. That's what you're saying?

DR. SHACK: Yes.

MR. HACKETT: Largely alliatory, the last two.

DR. APOSTOLAKIS: Okay.

MR. HACKETT: I think the short -- Nathan's had to put up with a lot of us crude materials types and school us. I can at least speak for myself.

DR. APOSTOLAKIS: What's the purpose of developing that long document that Dr. Su developed if it doesn't influence the real ones? This is the real one, not that one. That says an opinion.

MR. CUNNINGHAM: When we come back to the point of coming to you and talking to you about reformulation of the curve that I showed a little bit ago, that's where the uncertainty analysis more comes into play.

DR. APOSTOLAKIS: Sure.

MR. CUNNINGHAM: And that's why we didn't talk about it here.

DR. APOSTOLAKIS: Okay.

MR. HACKETT: I think we've really, just by virtue of the discussion here, we covered the last piece, too, so we'll probably move on to the next slide.

Another piece that we've been working for a long time, probably longer than we thought we would be, is revision to calculational procedures for the fluence values per what's now draft Regulatory Guide 1053, which will hopefully come before the Committee for approval for final Reg Guide before the end of the year. At any rate, the importance here is that we are doing calculations on the fluence, basically are being updated per the three IPTS plants that were done in the 80's, which were Robinson, Oconee, and Calvert Cliffs. Robinson is now not part of the project in that level of detail, but we've picked up Beaver Valley. So, we are looking at basically the details down to the cycle by cycle fuel loading and distributions or plant specific fluence maps in this project, so that's another level of refinement that was not done for the original basis for the rule.

The last piece from a materials perspective is just general improvements in the fracture mechanics methodologies. Dr. Shah Malik, I think at the March meeting presented a lot of the development and details that went into what's called the favor code, which was itself an expansion of the previous code, called VISA, which originally was developed in-house by Jack Strosnider in the development of 82-465 and then later refined it at PNNL.

So, it's been a very evolutionary treatment and then just some of the refinements or enhancements -- I guess I don't need to read through them all -- are listed there that we're picking up now that were not there previously, and that's also a major improvement and cause for optimism in where we thought this would end up. Obviously what the hope would be, that this criteria ends up at a higher temperature value than where it is now, but you know, as Mark says, the whole thing remains to be integrated and seeing where that ends up.

I think that pretty much summarizes just a quick overview of where we've been with the materials aspects. I guess if there are any questions on any parts of that, this would probably be a good time. If not, we'll move on.

MR. CUNNINGHAM: Moving on then, the last subcommittee meeting, we talked about four areas of guidance that the Commission had established since the 1983 rule was established. Basically the safety goal policy statement, station black-out and atlas rules, backfit rule, and particularly the establishment of the reg analysis guidelines and the tests in there for cost beneficial safety enhancements, and Reg Guide 1.174. The paper itself summarizes all of these things. I'm just going to kind of quickly go through some of the key points related to these policy documents.

With respect to the safety goals, I think the big issue in that whole area that related to this is that the Commission settled in on a subsidiary core damage frequency goal of ten to the minus four. Again, back in the early 1980's, that was a thought, but it was not, you know, in 1990 basically, they said yes, ten to the minus four is an okay subsidiary objective. They did not establish any other subsidiary objectives for pieces of the core damage from individual initiators. They didn't parse it out any finer than just ten to the minus four.

DR. APOSTOLAKIS: But again, the problem here, Mark, is what we discussed earlier. If the rule is not a good protection rule, you cannot use any of this because this is a goal statement.

MR. CUNNINGHAM: At this point, this is what's changed. Yeah, when we get back, we have to sort out what's the relevance of all of this --

DR. APOSTOLAKIS: That's right.

MR. CUNNINGHAM: -- to the PTF rule as it is. That's what we're trying to get at. Station black-out and atlas rules were established as cost beneficial safety enhancements. Basically in both cases, there was a -- you know, there were --

DR. APOSTOLAKIS: How many -- the question you asked Tom. There are no rules how to decide whether a rule is adequate protection?

DR. KRESS: That was my point of asking the question.

DR. APOSTOLAKIS: That's interesting, very interesting.

DR. KRESS: That was the whole point of asking the question.

DR. APOSTOLAKIS: It's a brother question, of course.

MR. CUNNINGHAM: I'm not going to say there aren't any rules. It's just I'm not the right person.

DR. APOSTOLAKIS: I know you're cautious.

DR. KRESS: You know, George, we once wrote a letter that called for such criteria.

DR. APOSTOLAKIS: Yeah.

DR. KRESS: It was a good letter, but it had a lot of added comments on it.

MR. KING: No, there are no rules, you're right. Probably the most recent discussion of this is the recent paper that came out from NRR that talked about using risk information and non-risk informed license submittals --

DR. APOSTOLAKIS: Yes.

MR. KING: -- where they put in all of the qualitative things that you really don't think about when you're thinking about adequate protection. That's probably about the closest you're going to get to some guidance that deals with that issue.

DR. APOSTOLAKIS: Maybe this, you know, there is some cause to quantify what we mean adequate protection, you know? Up, down, if you will. Maybe issues like that will create some pressure bottom up, that there are real decisions that have to be made, and we are making them without guidance.

MR. KING: Yeah. In the NRR document --

DR. APOSTOLAKIS: Because we're now trying to satisfy the Center for Strategic and International Studies, there are real issues why we need guidance.

MR. KING: Uh-huh, and the NRR document is, in effect, in my view, a bottoms up type document.

DR. APOSTOLAKIS: I'd like to see that, by the way. Is that something we can see?

MR. KING: Yeah, in fact, I think this committee reviewed it.

DR. APOSTOLAKIS: When?

MR. KING: A few months ago.

DR. APOSTOLAKIS: The context was?

MR. DUDLEY: This was a SECY paper. What we're talking about is a SECY paper that went forward to the Commission that tried to provide guidance on when you use risk information --

DR. APOSTOLAKIS: Oh, yeah, yeah.

MR. DUDLEY: -- in reviewing the license application in a deterministic arena.

DR. APOSTOLAKIS: I wasn't looking at it from that aspect. But yeah, I know.

MR. CUNNINGHAM: Okay. Again, in the context of the black-out and ATWS rules, they were cost beneficial safety enhancements with different types of rules, but they both had either fairly explicit or more implicit goals of getting the frequency of core damage from these initiators to be about ten to the minus five per year.

The backfit rule in the reg analysis guidelines, what the reg analysis guidelines introduce or document is a way of using the safety goals to screen out potential cost beneficial safety enhancements. I think I mentioned we had at one time an option which would somehow use that process and invert it to look at potential burden reduction rules, but we've taken that out because of the nature of this rule is an adequate protection rule.

Reg Guide 1.174 brings two things, at least two things to the table. One is it introduces a set of principles on how you would judge the acceptability of license amendment changes, which may be more broadly applicable in a rule revision like this. It also introduces, in this one context, the context of LERF, which we'll come back to. It has important implications as to how we might change the screening criteria.

Maybe this starts to get at the issue that you alluded to earlier, George, about not explaining very well of the level 2, 3 context of the PTS. In the early 1980's, there were qualitative arguments made that there was not a substantial challenge to the containment. Since then, we've had a lot of work in severe accident phenomenology and that sort of thing. The bottom part of that slide are basically a set of the issues that I think we would have to deal with as level 2 issues, anyway, in the context of PTS. There's the dynamic nature of it. If this was to -- an event were to occur and you have one of these big, through-wall cracks or you've got the dynamic loads, what's going to happen to the vessel? Is it going to move and that sort of thing?

The impact on the internals, what's it going to do to the fuel itself? What's it going to do to surrounding structures. You have the potential for pulling penetrations, that sort of thing. You're going to have a pressure loading if this were to occur. How severe is that relative to other types of loading that are part of the design basis? Again, some people have said maybe you could just end up pulling some of the fuel or dispersing the fuel. That has implications on source term, on the coolablity of the fuel and that sort of thing. Another feature is what's the availability of the containment ESF's in this context?

Going back again into the early 1980's. I think there was a lot of credit taken for the fact that the situation you're in here. You're breaking the vessel apart, but you're breaking in a situation where you have a lot of water. The argument was made, with some legitimacy, that all that water has got to be a good thing, that you're not melting this fuel. You've got water there. You've presumably -- you haven't done things to compromise your containment sprays, all of which could impact how this accident proceeds. Maybe we need to do a better job in the paper of explaining all of that.

MR. WALLIS: You said breaking the vessel apart?

MR. CUNNINGHAM: Creating a through --

MR. WALLIS: That's very different, though. You can have a through-wall crack which just leaks very slowly.

MR. CUNNINGHAM: What we're --

MR. WALLIS: Not breaking this apart.

MR. CUNNINGHAM: The assumption in the analysis is once you get a through-wall crack --

DR. APOSTOLAKIS: It unzips all the way around?

MR. CUNNINGHAM: It's going to be a large opening in the vessel, a very large opening. The weld is going to -- I'm sorry?

DR. APOSTOLAKIS: Conservatism built upon conservatism. Because five ten to the minus six was in the name of the person that I --

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: Now you have another conservatism.

MR. CUNNINGHAM: There is some conservatism in that. How much it is --

MR. WALLIS: It is going to break the vessel apart because a local crack isn't going to break the vessel apart. I see it goes all the way around. It's a pretty massive vessel.

MR. MAYFIELD: This is Mike Mayfield from the staff. When we looked at this before and the context was the axial cracks -- cracks running in the axial welds, both through experiments done at Oakridge on scale model vessels, and these are vessels that are about six inches thick and roughly a meter in diameter. So, they're not small things, but they're not on the same scale.

through analysis conducted at Pacific Northwest National Laboratories, but we -- the conclusion we reached is that if you get an axial crack that extends through the vessel wall, it will propagate. Under these kind of pressure loadings, it will propagate the full belt line of the vessel. That's 12 feet.

The crack opening will be measured in feet. So these are not small, tight cracks that will just lead.

DR. WALLIS: But it still has to open, doesn't it?

MR. MAYFIELD: Yes, sir, it does.

DR. KRESS: But those are pretty spectacular.

MR. MAYFIELD: Those are spectacular, and the flexibility of this vessel, even though it is a massive component, the diameter to the wall thickness is such that it will open. Even the scale model vessels tested at Oak Ridge, which were much stiffer, did open, and they opened rather remarkably. Tests that we have conducted on piping, where the R/T diameter to thickness ratios were more representative of vessels, and some of the experiments, the pipe actually flattened. It opened so much that it looked more like a plate than it did a pipe.

These are very high energy kind of events, and they are spectacular when they go on, so, Dr. Apostolakis, it's not just conservatism on conservatism. The reason that we built in that assumption was based on engineering analysis coupled with experimental data.

DR. APOSTOLAKIS: Good; thank you.

DR. WALLIS: So your remark about the lots of water was that the water was in the containment. It wasn't --

MR. HACKETT: Well, there are two factors with respect to water. One is you're pressurizing this vessel because of water, so there's water in the vessel as it --

DR. WALLIS: It doesn't stay in very long under the scenario as it's --

MR. HACKETT: It doesn't stay in very long, but there is water around the fuel when the vessel fails. The fuel is not melted. The fuel is cool at that point.

DR. WALLIS: Yes.

MR. HACKETT: There are a lot of other questions on what happens after that, but my second point was that there is water in the containment also; you're right, in the sense that you're going to have pools of water, liquid water, standing there, and you also have the potential for operation of the containment sprays. You're not in a blackout situation, for example, where you cannot get cooling, heat removal or decontamination of the containment atmosphere, so that has some potential merit in these accidents.

DR. APOSTOLAKIS: Okay; maybe this is a good time to take a break. You're starting with the options now, right?

MR. CUNNINGHAM: Yes, that is correct.

DR. APOSTOLAKIS: Yes; so we'll be back at 2:25.

[Recess.]

DR. APOSTOLAKIS: We are back in session.

MR. CUNNINGHAM: I propose that we would turn now to some potential options for revising the probabilistic aspect of the screening criteria. In the paper that you have, there are four options identified; again, from what we'd be interested in in talking to the subcommittees are are there other options that ought to be put into the paper? Are we clear on the options that we have? That sort of thing. So I'd keep it as broadly or wide open as you like.

The four options in the paper, the first is just make no change.

DR. KRESS: That's pretty clear.

MR. CUNNINGHAM: Yes; that one seemed pretty clear, okay?

The second option is work to make the PTS rule, in general, consistent with the blackout rule and the ATWS rule in the context of an acceptable CDF, if you will.

DR. KRESS: When you say consistent, that bothers me, because I think you mean make it the same.

MR. CUNNINGHAM: Okay; yes.

DR. KRESS: And consistent implies to me that you're going to have some other thinking going into it.

MR. CUNNINGHAM: No; it's really much more narrow than that.

DR. KRESS: Yes.

MR. CUNNINGHAM: Use the same numerical value for the acceptable CDF, if you will.

DR. APOSTOLAKIS: But again, the issue of whether that's legitimate is there, because these, as you state very clearly, were cost-beneficial --

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: -- safety rules.

DR. KRESS: I might want to propose an option E.

MR. CUNNINGHAM: That's what I'd like to hear, another --

DR. APOSTOLAKIS: Do you want to wait until he goes through A, B, C, D?

DR. KRESS: Oh, yes, yes.

DR. APOSTOLAKIS: Okay; and we'll add the comments to your option.

MR. CUNNINGHAM: The third option is apply the

1.174, reg guide 1.174 principles and acceptance guidelines to help us define how much we could change the acceptable frequency of a PTS event, and basically, you'd work from that to say how much could we afford to change this, or should we change it?

DR. WALLIS: You mentioned defense-in-depth in this context in your paper, in item C.

MR. CUNNINGHAM: Yes.

DR. WALLIS: I'm not quite sure how defense-in-depth applies to a split vessel.

MR. CUNNINGHAM: This is one of the interesting challenges to a PTS type of an event, that you could deal with it several ways. One would be balancing the challenge rate versus the conditional probability of vessel failure, if you will, looking at the materials. Another way to think about it would be vessel versus containment, that sort of thing, but yes, this accident type introduces some unique challenges to the issue -- or unique aspects of the defense-in-depth concept. Basically, then, our option D is similar to option C, except that we, in a sense, sidestep the issue of LERF by just saying, de facto, that a three-wall crack is a large early release, and you work from there.

So I was going to go into --

DR. APOSTOLAKIS: So again, instead of the equivalent, you should say the same.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: The two frequencies are the same.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: Because equivalent confused me a little bit.

MR. CUNNINGHAM: Yes; we will.

DR. WALLIS: You need to say frequencies are the same, too.

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: What I was going to go through in the next four slides is kind of summarize what's in the paper in terms of the --

DR. APOSTOLAKIS: Yes; you convinced us, though, with your area presentation that this is no option.

MR. CUNNINGHAM: Okay; moving right along.

DR. APOSTOLAKIS: And why did you have that series of view graphs telling us how much we had advanced, right?

DR. KRESS: That doesn't have anything to do with the acceptance criteria. Advance is how you calculate it.

DR. APOSTOLAKIS: They were talking about acceptance criteria, and for eight or nine view graphs, he was telling us how great we are now, and these guys didn't get much of a --

DR. KRESS: No, that was how to go about calculating --

DR. APOSTOLAKIS: That was the acceptance criteria.

MR. CUNNINGHAM: That's option A is that, yes, in effect, you don't -- you just cite --

DR. APOSTOLAKIS: If you like, he could go back and talk about it. I think you made the case.

MR. CUNNINGHAM: There's been -- there is at least one advocate for that option, but that's not sitting at the table here.

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: Okay; so, we'll move on.

DR. APOSTOLAKIS: So instead of consistent now, what is the word? The same?

MR. CUNNINGHAM: The same, yes; utilize a core damage frequency which is the same as that -- well --

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: You have to be careful, because the ATWS rule didn't establish a goal of 10^{-5}, from what I can tell.

DR. KRESS: I was trying to milk you on this consistent rule bit. What I would have thought might be the sort of principle would be that we look at station blackout set of sequences and the ATWS sequences; they look at how much the contribute to the CDF, and you look at whether or not they also impact containment, and you look at the uncertainties in how well you can determine them. And then, you factor those things into the acceptance criteria some way. I don't know how yet, but some way, so that you could -- you might actually come up with a different value for the PTS set of sequences that would be consistent with these but factor in these other things. That's why I asked you whether --

MR. CUNNINGHAM: Okay.

DR. KRESS: -- you meant consistent, or did you mean the same.

MR. CUNNINGHAM: Yes; okay.

DR. KRESS: I'm still not sure; I think you mean the same.

MR. CUNNINGHAM: The option B was the numerical values would be the same, in this 10^{-5} range. Maybe there is another option, which is you again, factoring in all of the other things, you develop 17 years or 15 years after the fact, develop a consistent set of principles, if you will, to make those three rules kind of align properly, considering the uncertainties and that sort of thing but --

DR. KRESS: That was going to be my option E.

MR. CUNNINGHAM: Okay; well --

DR. APOSTOLAKIS: Yes, why -- what is the contribution, what percentage of the core damage frequency in existing plants is due to PTS?

MR. CUNNINGHAM: Typically, it's very small.

DR. APOSTOLAKIS: Right.

MR. CUNNINGHAM: Because most plants are not anywhere near this $5x10^{-6}$ frequency. There are a few plants that, because of the vagaries of the design of the vessel or something like that that could approach it at the end of life.

DR. KRESS: If it got up to that $5x10^{-6}$, it's a code factor of 20 of the total.

DR. APOSTOLAKIS: Twenty percent?

DR. KRESS: I'm sorry; it depends on the thing. If you had one of these reactors that's 10^{-3}, and you got up to $5x10^{-6}$, why, it's, you know, a factor of two orders of magnitude. If it's 10^{-1} or 10^{-5} plants, then, it's a factor of two, you know, two. It depends on the plant.

MR. CUNNINGHAM: Yes.

DR. KRESS: I don't know if you can do this on a generic basis. You have to look at the plant.

MR. CUNNINGHAM: Yes; it's hard to do it on a generic basis.

DR. SEALE: On the other hand, the station blackout is the major -- station blackout is the major contributor for a lot of plants.

MR. CUNNINGHAM: It is a very important contributor.

DR. SEALE: So you're really bringing this up into the forefront.

MR. CUNNINGHAM: And we're talking about what would be acceptable, not what it is; it's acceptable at the end of life and that sort of thing.

DR. KRESS: Yes.

MR. CUNNINGHAM: Which is a little different than what it is; than what it is; that is correct. So it's a little different, but in a sense, that's where you're going that you would, in a sense, tolerate a larger contribution from PTS under this option than you would today.

DR. KRESS: There's a real issue here on how you allocate among sequences, and I think it's something that deserves a lot of debate and thought.

DR. APOSTOLAKIS: Okay; if you remove -- the sense I get from this document is that you believe that what we're doing now is conservative. So if we remove that conservatism -- no? I thought that was the idea.

DR. SHACK: Well, in the analysis, it's conservative. I mean, the option on the acceptance criteria is, in fact, to lower it.

DR. APOSTOLAKIS: One of the options.

DR. SHACK: One of the options, the recommended options.

DR. APOSTOLAKIS: The recommended option says if. They're not saying they're going to do it for sure. May have to be used; may have.

MR. CUNNINGHAM: May have to.

DR. SHACK: I'm inserting may into --

DR. APOSTOLAKIS: But the question -- that's a good point, in fact. Are you doing it because you feel that better science can be applied to this, or are you doing it because it's -- you will remove unnecessary burden?

MR. CUNNINGHAM: Let's back up. The context of this option or in the context of --

DR. APOSTOLAKIS: The whole thing, not just this option.

MR. CUNNINGHAM: The whole thing?

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: If they started the materials, these folks here started the materials research long before the issue of unnecessary burden ever made it -- this was started because we think we could -- thought we could get a more realistic understanding of the real risks associated with PTS accidents, and I think that's where we're going today. We think it also could allow some licensees to avoid being shut down because of the vessel questions than it might otherwise.

DR. APOSTOLAKIS: When?

MR. CUNNINGHAM: At the end of their life, at the end of life.

DR. APOSTOLAKIS: So they might get a license extension?

MR. CUNNINGHAM: It could impact some plants on the -- it could impact the ability of some plants to get a life extension, yes.

DR. APOSTOLAKIS: Okay; so that's really --

MR. HACKETT: Again, maybe just to lay out, set the stage a little bit, right now, I think we visited this maybe with the committee before, or NRR has. There's only one plant right now that's predicted to reach the current PTS screening criteria before end of license, and that's Palisades. Every other plant is at or after the current expiration of license. When you look at the license renewal period, depending on -- and again, you have to get into extrapolating some of this; what would you estimate in terms of plants that might experience, you know, PTS difficulty during that period, probably five to 10 or somewhere in that range.

So, like Mark said, it's -- other than Palisades, it's not exactly a here and now problem, but where it becomes a problem that we hear a lot from the licensees, of course, is that they're trying to argue now, in front of their boards, to get approval for license renewal, and these boards don't want to hear that there's going to be a vessel problem or some kind of show stopper like this. So that's where it's playing into the here and now for us.

MR. CUNNINGHAM: And they're also not interested in hearing that the uncertainty in what the Commission is going to do with respect to their vessel is still fairly large, if you will.

DR. SEALE: Maybe setting them up for 80-year lifetimes.

DR. APOSTOLAKIS: Now, coming to this option, again, I have a problem, and since you are making very clear on page 5 that -- you say that PTS is an adequate protection regulation, and SBO and ATWS regulations are safety enhancement regulations. So I don't know what it means to make this consistent with the station blackout. I mean, if you so clearly state that they are two different things, it's going to be a problem, I think.

DR. BONACA: Yes; I was going to ask the question: what was the logic for having them under these two different criteria, I mean? You would have to go back into that issue to understand, in fact, you know, what consistency means or equal to, it means in the context. Also, I wouldn't see any difference between B and A, with the exception that under A, you would maintain it as it is today, and under B, you would just arbitrarily, for the purpose of consistency, just increase the number.

DR. KRESS: My option E was going to do away with the references to things like 1.174 and station blackouts and ATWS, because those are mixing apples and oranges --

MR. CUNNINGHAM: Yes.

DR. KRESS: -- to some extent.

MR. CUNNINGHAM: Yes.

DR. KRESS: And say derive a new criterion based on fundamental principles, starting from some quantification of adequate protection, which is the ringer in there, but that was going to be my option E, and incorporate in there some factors, something to do with defense-in-depth and uncertainties in the termination and some principles that you have to develop yet which don't exist on how you can deal with allocation among sequences: do they all have to be the same, or can there be some variation? What principles would you use to decide?

MR. CUNNINGHAM: Yes.

DR. KRESS: And I think those are things that need to be thought out in here. This is a good place to do that.

MR. CUNNINGHAM: Yes.

DR. KRESS: Because those are all issues, and they all have to do with the acceptance criteria.

The other part of it and, you know, the program to do the calculations things, that's great. I love that. I mean, no problem at all. It's just acceptance criteria.

MR. CUNNINGHAM: Yes.

DR. KRESS: It needs some more thought.

MR. CUNNINGHAM: You're right, and maybe a better way to characterize this is you could set up some sort of a reliability allocation process.

DR. KRESS: I don't know what it is, but some thought needs to be given.

DR. APOSTOLAKIS: I think it's best not to have an allocation process per se.

DR. KRESS: But some principles to guide, yes.

DR. APOSTOLAKIS: Some principles and guidelines.

DR. KRESS: Yes.

DR. APOSTOLAKIS: As to what is good or achievable and reasonable.

DR. KRESS: Yes.

DR. APOSTOLAKIS: And I'll give you an example: when DOE was designing the new production reactor about 12 or 13 years ago, I was involved in that. And the same issue came up. They wanted to use PRA and design and so on, and balanced design -- a balanced design was considered a design where no sequence dominated, okay?

DR. KRESS: Yes.

DR. APOSTOLAKIS: Okay? So, everyone says great, let's do it, until somebody did the calculations for the seismic risk, and the seismic risk was way out there, dominating everything else. So now, the director was in deep trouble, because, you know, he had to do something about it, and I remember that he gave an estimate that if he had to bring down the seismic risk to a level where it would be comparable to the other contributions, it would cost to the project an extra $700 million which he did not have, and he said, you know, might as well forget it. And then, of course, they decided to live with it, that seismic risk was going to dominate, because overall, the risk was acceptable.

So, you have to have some flexibility, because you never know what you're going to get.

DR. KRESS: And it may have some cost-benefit --

DR. APOSTOLAKIS: Yes.

DR. KRESS: -- considerations in there.

DR. APOSTOLAKIS: Exactly; cost-benefit.

DR. KRESS: I think you need principles.

DR. APOSTOLAKIS: Exactly, or guidelines.

DR. KRESS: Guidelines or something.

DR. APOSTOLAKIS: Yes; rather than saying, you know, you know, Mark, that in the eighties --

DR. KRESS: You certainly don't want to say the idle ought to be about the same.

DR. APOSTOLAKIS: Yes.

DR. KRESS: I mean, that's not the right --

DR. APOSTOLAKIS: Or you might say that would be desirable but.

DR. KRESS: But, yes.

DR. APOSTOLAKIS: Yes.

DR. KRESS: Or it may be desirable that they not vary by more than a factor of 10 or something like that.

DR. APOSTOLAKIS: Oh, yes, exactly.

DR. KRESS: Yes.

DR. APOSTOLAKIS: Or at least ask people to look for --

DR. KRESS: Yes.

DR. APOSTOLAKIS: -- the reasons why there are discrepancies and maybe give an argument why we should live with them or --

DR. KRESS: Yes.

DR. APOSTOLAKIS: -- do something about them, which is the same thing we do now if you are between 10^{-4} and, say, 10^{-3} core damage. I mean, people can convince you that they have to stay there; otherwise, you have to come down.

DR. SEALE: I have another problem with this approach, and that is that I don't think the function or the validity of defense-in-depth is anything like, for PTS, is anything like as helpful as it is for station blackout in

ATWS. Remember when we were doing Shoreham, you could bring in floating turbines and a few things like that, and you could drive the station blackout risk down into the mud. You're going to have fun and games for years before you're going to drive the risk from -- assuming a PTS event -- down into the mud.

Let's say the countermeasures are --

DR. KRESS: There's not much you can do.

DR. SEALE: That's right; the countermeasures are in a whole different class of event. So I don't think you want to put those on the same piece of paper.

MR. CUNNINGHAM: I guess there's an argument, question, I guess, could you substantially affect the frequency of challenge, frequency of a pressurized overcooling of the vessel, and there's some arguments that you might be able to do that through --

DR. KRESS: But work on the frequency.

MR. CUNNINGHAM: Work on the frequency.

DR. SEALE: That's not quite the same.

MR. CUNNINGHAM: No, I agree, but given that you're in a PTS --

DR. SEALE: Yes.

MR. CUNNINGHAM: -- and you've cracked this vessel, it's not a trivial thing to recover from that.

DR. SEALE: Yes.

MR. CUNNINGHAM: Okay.

DR. SEALE: I don't want to be the little boy whose finger goes in that dike.

DR. APOSTOLAKIS: Okay.

MR. CUNNINGHAM: Option three is -- and perhaps I'd already gotten, to some degree, where Dr. Kress was, calling this the reg guide 1.174 principles is maybe not the best way to characterize this; that there's a set of principles that the staff has developed on how to deal with potential changes to the license that talk about how we're going to maintain defense-in-depth and those things. And the question is are those principles still -- are those the appropriate set of principles to apply to this -- to a rule change such as this? And those principles happened to be written in reg guide 1.174.

Some of them, I don't think, people would have much problem with. I think the tough one is the probabilistic aspect of it, where again, you're starting to mix together safety goals, adequate protection and that sort of thing.

DR. APOSTOLAKIS: And one more thing. I agree that, you know, we are missing adequate protection and safety goals, but also, can you talk about delta CDF, referring to a screening criterion? That would be a very novel use of 1.174. 1.174 refers to the actual estimated --

what, the calculated CDF. Now, you're saying I'm taking a screening criterion, and I'm estimating the delta CDF, and if that's less than 10^{-5}, it's acceptable. That doesn't make sense.

MR. CUNNINGHAM: Again, there's a principle. If you back up to the principle, which is you might be able to apply the principle, which is that any change either to an actual risk or to an acceptance criterion would be small.

DR. APOSTOLAKIS: Then, you'll have a point, yes.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: In other words, but then, you have this extra work, which is not negligible figuring out if I change the screening criterion, what's really going to happen for individual plants? Because ultimately, you have to look at real CDFs.

MR. CUNNINGHAM: Yes; that's right, too; eventually, each plant has to make some assessment against that.

DR. APOSTOLAKIS: Or maybe you can tie it with what you said earlier, that you don't really expect that many plants to come close to the screening criteria; maybe a few of them by the end of life, and maybe for those, there will be some guidance as to what they have to do. But you can calculate that as a change in risk.

DR. KRESS: I suspect the difference between

1×10^{-6} and 5×10^{-6} is substantial. I don't think it's linear but, you know, it may mean five different -- five years longer you could operate.

DR. SHACK: Well, if I look at their graph, I would say that it would lower the screening temperature by about 30 degrees.

DR. KRESS: It's not exactly --

DR. SHACK: Well, that would be fairly exciting, I would suspect.

DR. KRESS: That's quite a few number years of operation; you're right.

MR. CUNNINGHAM: I think what we're trying to convey here is should we try to work with the principles that were established in 1.174, the five principles, and use those and apply those to the reconsideration of the screening criteria.

DR. APOSTOLAKIS: As long as you make other issues very explicit, you know, that you are -- the adequate protection versus goals.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: And the fact that you are dealing with the screening criterion.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: Now, there is one other comment on the document itself, on page 5. There is something about the tone of the second from the bottom full paragraph that I think I find objectionable, and maybe you could change that.

MR. CUNNINGHAM: The numbering on my copy is different than probably the --

DR. APOSTOLAKIS: It's the full paragraph under C, just above D.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: Do you see where D starts? It begins this option would be most consistent.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: Okay; if you go down the paragraph a little bit, where it says that is if it were determined the containment performance was relatively poor, given the PTS-initiated core melt accident, then, the acceptable CDF may have to be reduced to ensure that the LERF guidelines would be met. This could prevent -- could potentially lead to a smaller value of the acceptable CDF and potentially result in different screening criteria.

The way I read this is that the author of this really didn't want this to happen and was apologetic, and I don't think that's the way we should write regulatory documents. If that's the case, that's what we're going to do. There's something about the tone of these sentences that I find --

MR. CUNNINGHAM: The tone -- okay.

DR. APOSTOLAKIS: -- unacceptable.

You know, if it happens that you have to lower the screening criteria, well, what can we do? This is adequate protection.

DR. KRESS: The way I would have read that is you may have to -- you've got a criterion on LERF and one on CDF, and instead of just assuming CDF and LERF equivalent, you may have to do a lot of analysis to actually calculate LERF on a plant-specific type basis --

MR. CUNNINGHAM: Yes.

DR. KRESS: -- and meet both criteria.

MR. CUNNINGHAM: Yes.

DR. KRESS: The one that controls would set it.

MR. CUNNINGHAM: That's right.

DR. KRESS: Which is reasonable to me. It's going to take a lot of work to do this LERF calculation for a PTS event like that. It's going to have a lot of uncertainty in that problem but --

DR. APOSTOLAKIS: Yes; my point is that each of the options, give the pros, and then, you end with the negative, and this is the end of this; in other words, you consider this a negative of this option, and that's what bothers me.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: The possibility of lowering the screening criteria.

DR. KRESS: If it happens, it happens.

DR. APOSTOLAKIS: If it happens, it happens.

DR. KRESS: Yes.

DR. APOSTOLAKIS: Okay; good; let's move along. Any other questions on C?

DR. BONACA: By the way, the same issue actually was previously considered a con rather than a pro.

DR. APOSTOLAKIS: In the attachment?

DR. BONACA: The attachment at page 2-8, where the description of these four options is discussed there.

DR. APOSTOLAKIS: That's fine.

MR. CUNNINGHAM: It's okay.

DR. BONACA: Just a note.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: What is your deadline for sending this to the Commissioners?

MR. CUNNINGHAM: May.

DR. APOSTOLAKIS: May?

MR. CUNNINGHAM: May. I had that same reaction. I kind of do this doing it.

[Laughter.]

DR. APOSTOLAKIS: May what? After our meeting here?

MR. CUNNINGHAM: On the books today, the due date to the Commission for this is May 15. We have been talking about whether or not that's realistic to do.

DR. APOSTOLAKIS: I think it's unrealistic as the screening criteria we discussed.

[Laughter.]

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: You're not going to make it.

MR. CUNNINGHAM: Well --

DR. APOSTOLAKIS: Okay; option D?

MR. CUNNINGHAM: As we kind of talked about before, this is similar to option C, except that in a sense, it says that rather than us up front trying to analyze the LERF of the containment performance issues in a PTS accident, we'll just say that -- for whatever reason, we're going to say no, we're going to assume that it's -- a through-wall crack is equivalent to core melt is equivalent to large early release. And that's the option. And you adjust the acceptance criteria down, if you will, to deal with that. That's a distinct difference between what it is today, this option and what the rule says or the basis of the rule back in 1983.

DR. KRESS: Is there a possible variation in that from the --

MR. CUNNINGHAM: Yes.

DR. KRESS: It is for different containment types, they have this assumption, whereas for others, you might not?

MR. CUNNINGHAM: Yes; there's variations of that possible. To some degree, it almost could be option C, the burden is on the staff to show what the containment performance is; on option D, you could put it on the licensee, in a sense.

DR. WALLIS: You're going to consider defense-in-depth, but you're going to throw away the containment? It doesn't seem consistent.

DR. KRESS: That's consistent with defense-in-depth.

DR. WALLIS: Throwing away the containment? That's for defense-in-depth, isn't it?

DR. KRESS: Yes, but you're throwing away in your acceptance criteria. That's a defense-in-depth --

DR. SHACK: But you're saying the challenge is so large, you don't want it to --

DR. KRESS: And the uncertainties are so big that you might as well assume it's gone. That's a defense-in-depth concept.

MR. SIEBER: We are presupposing that the licensee cannot or we'll never give them an opportunity to show that its containment is robust and --

MR. CUNNINGHAM: A variation on this is to build that in, to afford the licensee the opportunity to do that.

MR. SIEBER: I think sometime along in the future, the technology of analysis of what happens to containment under these conditions may advance to the point where there is a reasonable argument that can be made. It would be a shame to have the rule just arbitrarily preclude it.

MR. CUNNINGHAM: Yes; and that's why the paper, as it stands today, goes more toward option C, which builds in that flexibility, than D, which doesn't.

MR. SIEBER: It doesn't.

MR. CUNNINGHAM: But again, it's fair to say that there's no resolution among the staff of whether C or D or A or B is the right way to go on this at this point.

DR. BONACA: Just a question before the four options, you know, there seems to be a significant difference in the amount of work you have left to do to support any one of the options, is there? And I'd like to have a sense of what it would be.

DR. KRESS: You're right.

DR. BONACA: I mean, C seems to be much more open-ended, too.

MR. CUNNINGHAM: I guess I hadn't thought much about what the difference in work is.

DR. BONACA: It seems like -- I look at A; A seems to be just --

MR. CUNNINGHAM: Yes, yes; A is --

DR. BONACA: Whatever you know deterministically just --

MR. CUNNINGHAM: Yes.

DR. BONACA: -- plug it in, and that's it.

MR. CUNNINGHAM: That's right.

DR. BONACA: B is pretty much the same thing, I mean, because all you're doing --

MR. CUNNINGHAM: Yes.

DR. BONACA: -- you're changing that number here.

MR. CUNNINGHAM: Yes; that's right. A and B probably fit much better with the current reg guide and that sort of thing. C and D would, yes, go beyond that, because they impose some additional considerations; you're right; that's right.

DR. KRESS: I would have a number of questions that I would want answered before I decided on an acceptance criterion, and they would go along this line: one, why does a quantitative measure for CDF and LERF that's equivalent to adequate protection or consistent with adequate protection; number two, if I had such a measure, how -- what principles or guidance would I use to allocate the -- that value among a set of sequences that I have? What principles can I use for there?

How in that determination of both the overall acceptance criteria and the allocation among sequences, how would I implement thinking that would be called defense-in-depth or uncertainties in the determination of each particular set of sequences and the overall uncertainty in the final number? Those are the questions I think you need to ask, and they're fundamental. They're almost policy. Some of them are policy. It would be nice to have a policy statement on this, but I don't know --

DR. SHACK: Just how about a purely technical one? I mean, could you do the analysis of the containment performance C?

DR. KRESS: Not at the moment; I don't think you can. That's what Jack was saying: it's never been done. It's not to say that somebody couldn't sit down and develop the models and try to do it but, you know, I'd just say I've never seen it done. Some people have made estimates of the forces you get depending on the size but --

DR. SHACK: You can certainly make estimates.

DR. KRESS: Yes but --

DR. SHACK: The question is whether you would have one that could get --

DR. KRESS: Yes, that you could defend and get consensus on.

DR. BONACA: That's why I was asking the question about can you do it? I mean, the effort is very, very different and, you know, can it be done, and what would it involve to do that?

DR. SHACK: And since the staff was recommending option C, just what did you have in mind?

MR. CUNNINGHAM: Again, that's a draft paper, and the staff does not have a recommendation today. Again, I think -- you can go back several slides; we talked about some of the key issues that you have to address in the accident analysis, and I think you would have to go through some sort of an event reanalysis or something like that to deal with that. When you talk about the question of containment performance, we're doing it in the context of large early release. In my mind, anyway, you've got the question of is -- can you argue that containment performance is good enough so that you don't have a large early release? And that's a different question than saying what is the probability of containment failure given this, and what's the magnitude of the release.

So to some degree, the question is a little easier.

DR. KRESS: I don't think you want to deal with the release at all. Just ask that question you're asking.

MR. CUNNINGHAM: You're asking; and if you can argue, if you want to play with the numbers a little bit, the distinction between $5x10^{-6}$ and $1x10^{-6}$, the conditional probability of large release would have to be 20 -- the break point, if you will, is 20 percent of 0.2. If you can't argue that it's better than 0.2, then LERF is going to control. If it's better -- if it's anything less than 0.2, then the CDF would control, okay? And then, that's the level of question you're talking about.

DR. BONACA: Well, right now, you have a lot of conservatism that you're talking about, particularly the flood distribution and so on and so forth. That gives comfort that although you're not calculating a LERF with the current rule, okay, you're really covering for it, because probably, you're more in the 10^{-6} range for CDF than in the 10^{-5}, $5x10^{-6}$, it seems to me. But now, how could you consider A, B or even E or certainly A and B, which is reducing conservatism, okay, in the deterministic portion when you don't know if you are conservative on your LERF criteria?

What I'm trying to say is that you have $5x10^{-6}$ for CDF.

MR. CUNNINGHAM: Yes.

DR. BONACA: Okay? You have to have some confidence that you have some margin to allow for a LERF of $1x10^{-6}$, but you are removing some of the conservatism out there, okay, without verifying that you have, in fact, a marginal LERF. I'm talking about independence of the CDF that you are going to use as a criterion and the LERF; they are not equal right now insofar as numerically.

DR. WALLIS: It seems to me you need some technical analysis of containment failure instead of just juggling probabilities.

MR. CUNNINGHAM: Yes, that's right, and that's what you'd have to pursue as part of the -- again, part of the reason of raising these issues early is to say is that what we need to do as part of the program, or if the Commission or somebody makes a policy decision, we're going to go someplace else, and we may or may not have to do that technical work.

DR. WALLIS: I think the public believes the containment is there to contain any accident, so the belief.

MR. CUNNINGHAM: But again, the PTS --

DR. WALLIS: It provides some help with any accident.

MR. CUNNINGHAM: Yes, but a PTS event is not a design basis event for the containment; never was, and so, it's a beyond design basis event for the containment, in the sense that the containment is not specifically analyzed for a PTS event. Again, that's -- that's where we are today. We recognize the importance of LERF from a policy standpoint. We have to think about the arguments of whether or not, in a PTS event, what's the implications to LERF from a PTS event?

MR. SIEBER: It would seem to me, though, that there are so many variabilities in the containment analysis. Under a vessel fracture, you know, you have pipe width, and you have all kinds of stresses on penetrations and so forth --

MR. CUNNINGHAM: Yes.

MR. SIEBER: -- that it would not be reasonable for the staff to try to have a generic calculation that would show what the relationship between CDF and LERF was. I would leave that to the licensee to use reasonable methods backed by good scientific and core test data that would show that.

MR. CUNNINGHAM: In a sense, you could do that under option C, you know.

MR. SIEBER: Yes, and, well, you'd need the data to support that.

MR. CUNNINGHAM: Okay.

DR. WALLIS: If you get into a public meeting which is really public, the public is going to ask you: is the containment going to fail or not? One of the first things that I think they want to know when you describe this horrendous event.

MR. SIEBER: On the other hand, if you set the criteria as in option C, you can answer that with some surety, because you're basically saying that I'm going to establish, under rule, the probability that it's very unlikely that the containment will fail, and I wouldn't have a problem answering that kind of question that way. You know, you can't say any phenomenon in the world isn't going to occur with certainty.

DR. KRESS: It would be nice, though, to have some analyses that said, well, for large dry containment, maybe not; for an ice condenser, it looks like yes, more likely to fail; it would be nice to have some -- yes, I think you could do some analysis that wouldn't be too costly --

MR. CUNNINGHAM: Yes.

DR. KRESS: -- that would just give you some guidance on how to think about the LERF issue, and I think that might ought to be part of this somewhere.

MR. SIEBER: Every containment has some kind of analysis as part of the original licensing basis: how big is it? How strong is it? What's the pressure increase? How many heat absorbers are there? How much does it stress penetration?

DR. KRESS: Yes, but we're dealing with a different set of forces here.

MR. SIEBER: Yes; it's the penetrations that are often unique.

DR. KRESS: Yes, that's basically the unique part, yes.

MR. CUNNINGHAM: That's right; the dynamics of when the vessel, if the vessel were to open up and what that does to the penetrations --

DR. KRESS: I don't know if that's been looked at as part of some of the seismic analysis or not; maybe you can draw on those some way.

MR. CUNNINGHAM: That may be. That may be.

DR. KRESS: Look; you know, just some level of analysis --

MR. CUNNINGHAM: Yes.

DR. KRESS: -- to give you guidance.

MR. CUNNINGHAM: In a sense, that's what we've been trying to do offline, if you will, is do some of that analysis and set it up, at least set up the problem a little more precisely than it is here.

DR. WALLIS: If you don't do analysis, what are you left with? Just guessing or --

MR. CUNNINGHAM: You have to go more conservative.

DR. WALLIS: -- judgment or --

MR. CUNNINGHAM: You have to go more conservative and say go with option D rather than option C and just by fiat say it's --

DR. KRESS: The same as the CDF.

MR. CUNNINGHAM: Yes; that's right.

MR. SIEBER: If you can't do the proper analysis, that's where you are is in option D.

MR. CUNNINGHAM: Yes; you default to option D is what it amounts to, yes.

DR. SHACK: You sort of have to decide how that value coincides with an adequate protection argument.

DR. KRESS: Yes; that's one --

MR. CUNNINGHAM: We're back in that --

DR. SHACK: You're back in that ball game.

MR. CUNNINGHAM: Back in that, yes, that's right.

DR. SHACK: I mean, you can pick a CDF based on option D, but what CDF you pick --

DR. APOSTOLAKIS: Yes.

DR. SHACK: You still have that problem.

MR. KING: I'm not convinced that's a real problem, because I'm not convinced there's anything that's strictly an adequate protection rule, and you never bring in the additional step of can I add some safety enhancements that are cost-beneficial beyond that.

DR. APOSTOLAKIS: Yes, but using the safety goal numbers routinely in those rules I don't think is wise.

MR. KING: No but the --

DR. KRESS: It's the redefinition of adequate protection.

DR. APOSTOLAKIS: Yes; essentially, you are redefining adequate protection, and you're making it more stringent.

MR. KING: No, I think what you need to do is you say maybe the old rule was declared an adequate protection rule, but that doesn't mean this new rule has to be declared an adequate protection rule.

DR. APOSTOLAKIS: Then you have to do this cost-beneficially.

MR. KING: But that, to me, is not unreasonable. Why wouldn't we do that on any rule?

DR. APOSTOLAKIS: That might be a way out of this.

MR. KING: Then, the safety goal is really here's what we'd like to see.

DR. APOSTOLAKIS: You just call it something else.

MR. KING: In terms of a level of safety, and that's what we shoot for in this rule, and we have to do the cost-benefit, and if it doesn't work out, it doesn't work, but I think in any rule, we're obligated to do that. So I don't really think there is anything that's strictly adequate protection, and I don't think we ought to get hung up on that question.

DR. WALLIS: I like what you say. I think adequate protection doesn't exist. It's all cost-benefit, really; with adequate protection, there's some bound, because you don't know enough. It's really all cost-benefit. There's no benefit, no cost at all.

DR. APOSTOLAKIS: Yes, but there are certain benefits under the certain conditions in the country; you're right. I mean, if there's war tomorrow, we might change there the definition of adequate protection, but right now, the way things have been the last 50 years, for example, there is a certain level of --

DR. WALLIS: It's a convenient idea, because you don't want to get into the details where it gets very fuzzy.

DR. APOSTOLAKIS: Anyway; I think Tom's idea, Tom King's idea has some merit.

DR. KRESS: I think so, too.

DR. APOSTOLAKIS: You know, on the other hand, you have the issue there of arbitrarily renaming things, but well, anyway, we can't resolve that today. Do you want to move on to 19, or you have already covered that?

MR. CUNNINGHAM: Nineteen and 20, in a sense, say where we're going from here.

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: And if you want to do those and then come back, it doesn't much matter to me but --

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: I did, at some point, want to come back to Dr. Kress' option E, but if you want, I can go ahead with 19 and 20 and just get through them and then --

DR. APOSTOLAKIS: Well, we're going to have a round of discussions as to what should go into the letter and what your presentation should be at the committee meeting.

MR. CUNNINGHAM: Okay; well, let me go ahead, then.

DR. APOSTOLAKIS: So, with this --

MR. CUNNINGHAM: This is basically where we're going to be over the next few months. I thought about it in the context of what it would be happening and what would be the subjects of discussion at the September, I guess August or September subcommittee meetings, the next set of subcommittee meetings on PTS.

Basically, in terms of this paper right now, we've got a May deadline. We need to talk to the full committee; we need to continue the discussions with the rest of the staff and with the legal staff. So that's going to proceed.

In terms of the PTS program in general, a number of things that are going to be going on. Ed alluded to it earlier that the development of the generalized statistical distributions, using that term, on flaw sizes and things ought to become available before the next meeting, so we kind of expect that that would be a subject for the next meeting.

DR. APOSTOLAKIS: The next meeting, you're referring to the full committee meeting?

MR. CUNNINGHAM: No, the August-September subcommittee meeting if you will; I'm sorry; not the May full committee meeting.

Maybe we'll just go to slide 20 and say that this is, in a sense, what might be an agenda for the next subcommittee meeting. Where are we on this particular issue? What have we learned about the flaw distributions based on the expert elicitations that are underway now? Other things that are going on; the materials area; what are we doing in the uncertainty analysis, and how does that reflect back into these other things and then maybe some initial risk analyses for a plant or something like that.

DR. APOSTOLAKIS: So the Commission is planning to decide on what the screening criterion soon? Or they may choose not to do it?

MR. CUNNINGHAM: They may choose not to do it. From our standpoint, we thought it was important to the whole program to get these issues identified and discussed early on in the program, because we don't want to wait until a year from now to raise these kind of fairly fundamental issues in front of -- before the Commission and give us no time to react to them, depending on what the Commission decides. So we would put these before the Commission; again, right now, it says May, and then, the Commission may decide -- well, the Commission will decide what it decides. In times past, in some circumstances, they've said, well, we're going to sit and wait and see --

DR. APOSTOLAKIS: Maybe you should give them an option for that, like A now says make no change to the CDF value underlying the screening criterion, and this is permanent, right? Maybe you say make no changes now until the staff has resolved a few issues.

MR. CUNNINGHAM: That's possible.

DR. APOSTOLAKIS: I think that's the most reasonable --

DR. SHACK: One question; when I read through, like, C and D, these options where you could potentially lower the number, why is there no sort of discussion here of a cost-benefit analysis? Wouldn't you have to do that?

MR. CUNNINGHAM: If you use Tom King's approach, yes, you would do that. Again, the rules, as they're set up today, is an adequate protection rule and cost-benefit are two different things. So we don't do it that way, but it may be --

DR. SHACK: I mean, isn't the presumption that you've met adequate protection --

MR. CUNNINGHAM: Yes.

DR. SHACK: -- the $5x10^{-6}$ and then, lowering it to

$1x10^{-6}$ would then be judged on a cost-beneficial basis?

MR. CUNNINGHAM: That's why I say I think Tom's idea has merit in the sense of how to tackle that.

MR. KING: And I think the $5x10^{-6}$, being a 17-year-old number, I wouldn't hold that up as some measure of adequate protection. We don't have a measure of adequate protection in terms of a numerical measure. I would -- maybe I'm following Dr. Kress' argument.

DR. SHACK: That number is not adequate protection?

DR. KRESS: I think that's a risky argument, Tom, because when you first put that number out, you said this criterion is the -- meets adequate protection. Now, you're saying it doesn't.

MR. KING: Well, we were giving you the historical basis for the rule, and the word adequate protection is in the old rule.

MR. MAYFIELD: This is Mike Mayfield from the staff. When we modified the PTS rule in Nineteen-Ninety-something to incorporate the latest embrittlement trend curves, the argument against having to do a backfit analysis was that it was, in fact, an adequate protection rule, and what you were doing by imposing the new embrittlement correlations, and there were some plants that had their RTPTS value go down; others where it went up, so it was a mixed result, but the argument was it's an adequate protection rule, and this is redefining what you mean by adequate protection so --

DR. SHACK: Yes, but I think that's an easy argument. If the criterion stays the same in your analysis of how close you get to them, I mean, that's a purely technical question. When you're changing the criterion, I think that's a very different kind of argument.

MR. MAYFIELD: Well, one of the other notions that some of us have had about the various options is there is a danger in taking a rule that was put in place to guard against failure of the reactor pressure vessel, and you're turning that into a fair bit of dialogue on containment integrity, and we've had some difficulties with the level of uncertainty in doing pressure vessel analyses; the uncertainties and vagaries in doing the kind of containment integrity analysis just to describe the accident phenomenology for this kind of accident is orders of magnitude more difficult. The uncertainties we've talked -- I think Dr. Kress mentioned large, dry containments. You're now off into are you on a shield tank plant? Is it a nozzle-supported plant? Is it one of these -- I guess it's a C design that uses long columns; has to do with the amount of movement you can get out of the vessel, which if you have this kind of long, axial rupture, and Dr. Wallis had talked about or mentioned the failure of the circumferential wells, that's a different -- takes you into a whole different scenario, but just staying with the long, axial welds; the first thing that's going to happen is you're going to shove the vessel up against the side of the shield structure, whether it's a shield tank, concrete wall.

And now, how far can you drag the piping? Well, that has to do with how much movement you can get inside that shield. For shield tank plants, this is a completely different scenario, so that the vagaries here would be major to try and sort through this and do a credible analysis that I think this committee would accept as a credible analysis for containment failure given this scenario. I think that's a major challenge.

So some of us have had some concern about going down this path, because you're starting to focus on other -- things other than vessel integrity. In fact, that's how some of the interest in getting away from essentially mandating that analysis and looking for other ways to stay focused on the pressure vessel.

DR. BONACA: I understand the complexity. I still am puzzled by the fact that options C and D, it seems to me, imply that the evaluation of the PTS rule may identify LERF scenarios, okay, that would cause the reduction in CDF criterion, right? So, not enough is known, but that could be possible. Option A and B, without consideration of these possibilities, propose to reduce margin outright, and, in fact, in option B, they're proposing to actually increase the CDF criterion to $1x10^{-5}$. I don't see how we can, in the same breath, consider them on the same basis. If you really think that there is the possibility that you're reevaluating the rule, you may find that you're forced to really lower the CDF criterion in some cases; how can you, then, without evaluating LERF, go to option A or B? I just don't understand it. I just don't see it anymore.

MR. KING: You're suggesting maybe those aren't real options; we shouldn't even talk about them.

DR. BONACA: That's right; that's exactly right. I'm not sure that -- do you have that option anymore if, in fact, and I believe that you are right that there are possibly scenarios in the evaluation of the PTS rule where they may have LERF forcing CDF changes, and if that is the case, you should take them out as option A and B, because they're not options. I think you have to think about it.

MR. KING: I understand your point. I think we were trying to cover the waterfront, not just eliminate something because we don't think it's real at this point.

DR. BONACA: Yes.

MR. KING: But as we get feedback, that may end up being the case.

DR. BONACA: Particularly option B; I mean, B, it's simply for the purpose of consistency and just relaxing the criterion, but you don't know what LERF is; really, you don't.

MR. KING: No, that's right.

DR. BONACA: So I think you should eliminate B. I mean, A may be a possibility. A simply states that what you've done to date is right, okay? And all you know is that you've been very conservative on your flow distribution mostly, and you can give up some of the margin there. Now, even in that case, I'm not sure you have a solid ground for reduction of margin without looking at LERF.

MR. KING: No, I agree. I mean, my own personal view is, you know, the Commission has put out safety goals that express their expectations on safety. From those, we've developed subsidiary CDF and LERF objectives, and that ought to be the starting point, and you work backwards. Maybe I'm supporting Dr. Kress' option E. And you work backwards, and you say okay, how much of that do I want to allocate to PTS? And that drives your option and your decision, and I think there has to be some cost-benefit in there. I wouldn't get hung up on whether it's adequate protection or not. You're trying to meet the expectations the Commission has put forward starting with the safety goal policy.

DR. BONACA: Again, it may be time consuming and complex, but it may be the only thing that you can do if you want to revisit the rule.

MR. CUNNINGHAM: Yes.

DR. BONACA: Leave it where it is.

MR. CUNNINGHAM: But I think this issue of if you do have to calculate LERF, can you do it? Do you have the tools and the information and the data to do it is a real question. Look at the direct containment heating issue. It took us 7 years and millions of dollars to develop the tools, the analytical tools and the data, to do away with that issue, and it was plant specific in the sense of cavity design and connecting compartments to the cavity.

DR. KRESS: On this business of calculating LERF, when you do 1.174, and you specify, actually, because of the way the safety goals are written that it's the mean value of LERF, I think that implies that there was some known uncertainty in the calculation of the LERF that went into 1.174 and that the mean, this mean value was acceptable. Now, when one comes to a new LERF that has -- that wasn't even part of 1.174, the PTS, where you have a huge uncertainty in that LERF, and you say maybe the mean value is no longer the right one to talk about; if we talk about the 95 percentile of that because it's got such a huge uncertainty in it, then, you may have a LERF that's such a value that you just automatically say that CDF and LERF is equivalent.

MR. KING: You mean option D.

DR. KRESS: I think in an argument like that would support it, because the 1.174 mean value was based on some sort of thing in mind of what the overall uncertainty in LERF was.

MR. KING: But leave the door open for a licensee to come in and make a plant specific case if they want to.

DR. KRESS: Or open if they want it, if they've got the analysis tools that they can back up.

DR. APOSTOLAKIS: So we are right in the middle of discussing options and giving advice to the staff.

DR. KRESS: Yes; I think that's what we're in.

DR. APOSTOLAKIS: And maybe we should go --

DR. KRESS: Let's get Tom's version of the options.

DR. APOSTOLAKIS: Well, that's certainly one option.

DR. KRESS: I still have a little problem with that.

DR. APOSTOLAKIS: Yes, I do.

DR. KRESS: With adequate protection versus safety goals.

DR. APOSTOLAKIS: Yes; well, would you like to go around the table and maybe make comments, or do you want to take a break first, 5 or 10 minutes? We'll be back at 3:30.

[Recess.]

DR. APOSTOLAKIS: So, who wants to start? Bob or Jack?

MR. SIEBER: I can start.

DR. APOSTOLAKIS: Jack, please?

MR. SIEBER: First, I would congratulate the staff for a good presentation. I thought it was logical; easy for me to understand and also to congratulate the initiative to try and use the advances in metallurgy to come up with better ways of doing things.

As far as probabilities are concerned and what the goals should be, I have a tendency to prefer Dr. Kress' approach, but I think that's complex and requires a policy adoption by the Commission. Lacking that kind of approach, I would prefer option three, the third of the four that were presented.

DR. KRESS: C?

MR. SIEBER: C, yes; three, C, because I think it provides enough flexibility for the future; places the burden on licensees and the review options on the staff and probably, in the long run, would last longer from the standpoint of do I need to change the rule to accommodate some new and different situation.

From a technical basis, I don't think that option C is as good as Dr. Kress' approach, which is applicable to more situations than just this one and would make a greater degree of consistency in a number of rules that are going to come up in the risk-informing of Part 50. But otherwise, that would be my opinion as to where we are right now.

DR. APOSTOLAKIS: Okay; thank you.

Mario?

DR. BONACA: I also think that the option C or modified as Dr. Kress is suggesting would be the way to go. The main point I made before and I repeat is I don't think -- I think we should be clear whether or not we have more than one or two options alone. I think we should try to understand if, in fact, the only options we have are either C or D here and then, again, a modified C as suggested by Dr. Kress would be appropriate.

Just because I think it's important in that before I came here, I really thought that we had four options, and some of the simple ways to get there are attractive, A or B. But then, because we don't understand LERF associated with those changes for A and B, I don't think you can perform a tradeoff of margin against an unknown LERF effect, and that's why I'm saying that I think it would be important that the Commissioners understand whether or not there are more than two options. I think it's important also for the committee to understand it. And that's pretty much my comments.

DR. APOSTOLAKIS: Bill?

DR. SHACK: Well, I guess I'm really not thrilled about any option that requires me to evaluate LERF, although Mark seems to indicate that maybe it's not as -- you know, if I had posed the question properly, it's not as horrendous as it seems to me, and I guess, I mean, I'm always willing to live with an option that says okay, you know if you can present one to me, you know, it's sort of a fictitious option, but that's okay, you know.

On the other hand, I'm not sure, you know, if I look at even option A as one where I'm essentially assuming a CDF equivalent to LERF, then, I still have to decide why, you know, why do I pick five for, you know, as in D, why do I pick one? And, you know, the arguments as to why I pick one over the other are not clear to me, and it seems to me that somehow that has to be made -- that case has to be made a little bit better. But just as a practical point of view, I guess I, you know, I kind of prefer options where I assume that for all practical purposes, CDF is equivalent to LERF, and yes, I'm willing to leave them in doubt.

DR. KRESS: I would have to say my feeling is almost exactly like Bill Shack just said it, plus some thinking along the lines of how to get those numbers. I would not call this a 1.174 option. I would say -- I would rephrase it to say using the principles, be sure that, you know, and come up with a better justification, some sort of justification for the actual numbers.

DR. BONACA: Just to understand it, so, you're talking about option D?

DR. KRESS: Yes; yes, the equivalent.

DR. BONACA: Okay.

DR. KRESS: You know, I feel sort of like Bill does. I leave open the possibility, but I think right now, it's a fictitious opening.

DR. WALLIS: Well, I'm not sure that option A is unacceptable until you know more. Things seem to be a bit iffy, and if you had to justify C or D before a critical Commission, you might have difficulty making a really good case. I wonder if you can't do nothing for awhile until you've got -- then, the question is what is it you could learn that would help you better?

DR. SEALE: Well, I don't know that you're -- I don't know that you really have to make a decision right now. I understand that this is something that sort of sits out on its own, and there's not a lot that you're doing that helps you make this decision with any more information now than you would later, except possibly this whole question of risk-informing the regulatory process will be a little further along, and so, you might have a few more insights just by experience if you waited until everything else catches up to this point.

But excepting that, I would say I think Tom's approach is the one that sounds at least cosmetically the best right now. I have a great deal of trouble, as I indicated earlier, with writing ATWS and station blackout on the same sheet of paper as PTS, because they're just not the same kind of problem.

DR. APOSTOLAKIS: Thank you; anything else?

I expressed my views earlier. I, in fact, agree with what Graham said. I don't think we can -- we know enough right now to make a recommendation to the Commission as to which option is best. It's probably a good idea to offer an option for them to -- out of this and keep thinking about these issues of adequate protection; how do you use the principles and so on.

DR. SEALE: Yes.

DR. APOSTOLAKIS: And then come back later and make a recommendation.

DR. SEALE: Yes.

DR. APOSTOLAKIS: That's my view at this time.

Yes; Mark, do you want to say something?

MR. CUNNINGHAM: I was just going to say one option, if you will, is that we just use the paper that's going up here in the near future to tell them what's going on --

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: -- and not provide a recommendation.

DR. APOSTOLAKIS: Yes; and some of the things you're thinking about --

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: -- the issues that have been raised.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: I think that would be informative to them.

DR. KRESS: Yes; I think I would support that.

DR. WALLIS: Wouldn't you be more comfortable with that really?

MR. CUNNINGHAM: It's a mixed --

DR. WALLIS: If you picked one of these other options, you might find you had stepped in something.

MR. CUNNINGHAM: Yes; the down side of putting it off, if you will, is that there is so much going on in risk-informed regulation right now that all of these things we talked about apply to them as well as PTS, and at some point, we'd have to make this decision.

DR. KRESS: I think you need a policy statement on how to risk-inform the regulations.

[Laughter.]

DR. KRESS: Yes; just ignore me.

DR. SEALE: He gets out of hand that way occasionally. You have to pat him on the head.

MR. SIEBER: On the other hand, I think some issue has to be first.

DR. KRESS: Yes.

MR. SIEBER: And this one is fairly clear-cut from the standpoint of the phenomenon that is occurring, and there is benefit even with that absent risk-informing it. On the other hand, since somebody has to be first, why not this one?

MR. CUNNINGHAM: This rule has advantages over others. This is a cleaner rule than many of them.

DR. KRESS: Clean one to look at; that's for sure.

MR. CUNNINGHAM: Yes.

DR. KRESS: I like that thought.

DR. SEALE: That is a good point, yes.

MR. HACKETT: I guess the comment I would add is a decision sooner rather than later obviously helps us from a resource and planning perspective, because we have this project planned to go out through 2001 now, and were we to select one of these options versus another somewhere sooner rather than later, we may look at very different allocation of resources possibly. At any rate, that would be a consideration.

MR. CUNNINGHAM: And how quickly we complete this program has implications to decisions licensees have to make about license renewal and things like that too so --

DR. APOSTOLAKIS: Yes; I don't think we are suggesting that you stop it.

DR. KRESS: No.

MR. CUNNINGHAM: No I --

DR. KRESS: But particularly the other part of it.

DR. APOSTOLAKIS: The other part should go ahead.

DR. KRESS: Drive ahead with it.

DR. APOSTOLAKIS: It's just that -- you know.

DR. KRESS: And I don't think that developing the principles for acceptance criteria would be really resource intensive. Put one good guy on it, I don't know, Tom Keyes.

MR. CUNNINGHAM: He seems to find other things to do most of the time.

DR. KRESS: Tom doesn't work anymore, but I don't think that's very resource intensive.

MR. CUNNINGHAM: The issue if we want to get into -- you can conceive of a very resource intensive program to investigate LERF.

DR. KRESS: Oh, yes, if you had to go that route, but, you know, I'm sort of assuming you're not going that route.

MR. CUNNINGHAM: On personal opinion, I don't think we have to go that way, but, so, I've been wrong about these things before.

MR. SIEBER: On the other hand, if you went with the option C, you certainly would have to know how to evaluate the licensee's effort to determine what LERF is, which I don't think is easy either.

DR. KRESS: That's right.

MR. SIEBER: There's a lot of things that go on in containment integrity under this condition.

DR. KRESS: Yes.

MR. CUNNINGHAM: Either C or D both have implications for some sort of a modification of the reg guide or something --

MR. SIEBER: That's right.

MR. CUNNINGHAM: -- to lay out what we would find at least as one acceptable way of doing it.

MR. KING: If we laid out a framework for risk-informing Part 50, our option 3 framework which we presented to the committee. If you -- it would seem reasonable for whatever rule we're risk-informing, we may want to have a similar approach, which I think to me, that framework applies to Dr. Kress' approach more than any of the other options we present. So maybe a realistic option will be turn this paper into one that says this is the way we're proceeding; we've sent the Commission this option three framework; when you say when you apply that to the PTS rule, it leads to an approach similar to what Dr. Kress came up with, and we just tell them this is the way we're heading, and if they have objections, they can speak.

If we don't hear from them, we'll assume they don't have any big heartburn with it. I mean, that's an intriguing thought instead of making this something where you've got to pick and choose from four options.

DR. KRESS: I think that would be your best bet at this point.

DR. APOSTOLAKIS: Okay; other thoughts? The staff?

[No response.]

MR. CUNNINGHAM: Thank you for your good advice.

DR. APOSTOLAKIS: Members of the public?

DR. WALLIS: There will be a letter written on this?

DR. APOSTOLAKIS: We will write a letter? Do you want a letter still?

MR. CUNNINGHAM: We had requested a letter. I think we need to talk -- the staff needs to talk about whether or not, on our options for this paper, if you will.

DR. APOSTOLAKIS: So, you don't know whether you're requesting a letter. That's what you're saying.

MR. CUNNINGHAM: I'm much less sure of it now than I was at the beginning of the presentation.

MR. KING: Why don't we tell you at the full committee? We're scheduled --

DR. APOSTOLAKIS: Yes, but then, we have to write it on the spot.

DR. SHACK: We'll work on a draft, and then, we'll decide on what to do with it.

MR. KING: How much time at the full committee do we have?

MR. CUNNINGHAM: It's an hour and a half, I believe.

DR. APOSTOLAKIS: An hour and a half? Okay; and you will go basically over the same presentation?

MR. CUNNINGHAM: If that's what you'd like.

DR. APOSTOLAKIS: Except --

MR. KING: It may be a little different based upon the discussion.

DR. APOSTOLAKIS: Can you add some of the background in terms of the event trees, fault trees on how this thing fits into the big picture of PRA?

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: You know, the comment I made earlier? It's just a matter of pulling information from a

PRA.

DR. SEALE: And if you really do want a decision now, if you decide, you may want to tell us more about what all the goodies are that you can harvest if you get the decision. You know, I mean, like the point that today, you can put a skin on the wall and say here's our trophy from risk-based regulation, all of that stuff.

MR. HACKETT: In that regard, I'd make one other comment. A not inconsequential consideration in this entire project is the industry interest in it. If the industry -- Ron Gamble is here representing the industry -- and Ron was one of the people who identified early on that we needed to take this on earlier rather than later; one of the issues would be, I think, if the industry sees a significant uncertainty on the part of the NRC or a delay, perhaps, in decisions on this that their interest may wane accordingly I guess is the way I might look at it, and that's something that's just there.

DR. APOSTOLAKIS: Well, I think you should address the issues of the benefits of the result from something like this.

DR. SEALE: Yes; definitely.

MR. CUNNINGHAM: Yes.

DR. APOSTOLAKIS: That would go a long way.

What is the current situation? How does this whole PTS issue fit into the risk assessments that have been done? And then, what would the benefit be? I think that would go a long way toward setting the stage.

DR. KRESS: We're not part of the risk assessment.

DR. APOSTOLAKIS: Oh, so we are using risk information to do something that's not part of the risk assessment?

DR. KRESS: It's usually screened out.

DR. APOSTOLAKIS: In all the --

DR. KRESS: It's usually screened out.

DR. APOSTOLAKIS: In all the PRAs, it's screened out?

MR. CUNNINGHAM: Not all PRAs, but there are a few PTS specific risk analyses around.

DR. SHACK: But again, if your embrittlement temperature is low, it's going to screen out.

MR. CUNNINGHAM: That's right.

DR. SHACK: I mean, it's going to be zip.

MR. CUNNINGHAM: There are a large number of the plants where it should be low because you're not anywhere close to the embrittlement criteria.

DR. APOSTOLAKIS: Yes.

MR. CUNNINGHAM: But maybe we could provide a better story on that, too, to the committee.

DR. APOSTOLAKIS: Yes; that's what I want to understand better.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: Because I've never paid that much attention to it.

Anything else?

[No response.]

DR. APOSTOLAKIS: Well, we'd like to thank you very much. It was a very good discussion, presentation. Thanks, everyone, and the meeting is adjourned.

[Whereupon, at 3:48 p.m., the meeting was concluded.]

REPORTER'S CERTIFICATE

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

NAME OF PROCEEDING: MATERIALS AND METALLURGY AND RELIABILITY AND PROBABILISTIC ASSESSMENT

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