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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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WEDNESDAY,

~~JUNE~~ July 11, 2001

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ROCKVILLE, MARYLAND

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., George Apostolakis, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

GEORGE APOSTOLAKIS, Chairman

MARIO V. BONACA, Vice Chairman

F. PETER FORD, Member

THOMAS S. KRESS, Member

GRAHAM M. LEITCH, Member

DANA A. POWERS, Member

STEPHEN ROSEN, Member

WILLIAM. J. SHACK, Member

COMMITTEE MEMBERS PRESENT (Continued):

JOHN D. SIEBER, Member

ROBERT E. UHRIG, Member

GRAHAM B. WALLIS, Member

C O N T E N T S

PROCEEDINGS

(8:32 a.m.)

CHAIRMAN APOSTOLAKIS: -- and he's being replaced by Sher Bahadur, who is joining us as Associate Director for Technical Support.

Also, Mr. Steve Rosen now is a member of the Committee officially. Welcome, Steve.

DR. SHACK: We don't have to quit being nice to him?

CHAIRMAN APOSTOLAKIS: Not anymore. You can be ask nasty as you want.

And finally, you have this pink thing, "Items of Interest." There are four speeches by the Commissioners, three by the Chairmen, and one by Commissioner Merrifield. The second one, the evolution of safety goals and their connection to safety culture, is of particular interest, I think, to the members or should be, but the others are very interesting, too.

And so members have any issues they would like to raise?

(No response.)

CHAIRMAN APOSTOLAKIS: Okay. We can begin then with the first subject: proposed risk-informed revisions to 10 CFR 50.46 and proposed revisions to the framework for risk-informing the technical requirements of 10 CFR, Part 50.

Dr. Shack, would you lead us through this?

DR. SHACK: We discussed this as a subcommittee meeting on Monday. So this is deja vu all over again.

CHAIRMAN APOSTOLAKIS: Today being Wednesday.

DR. SHACK: And I think Mary will be leading us through this. Why don't you just go ahead and start?

MS. DROUIN: My name is Mary Drouin with Office of Research. Also at the table with me is Alan Kuritzky.

We did meet on Wednesday --

CHAIRMAN APOSTOLAKIS: No, Monday.

MS. DROUIN: Monday. Sorry.

We were trying to go through the presentation, and we did a little reordering of it based on how the discussion went so that we would go through it a little bit more smoothly.

Again, just quickly the points that we want to try to get to today; again, the purpose of what we're seeking out of the meeting; a quick background on Option 3 to emphasize some of the points; what our tentative recommendations and schedules are; and then based on that, to get into the detail then for each of our proposed recommendations; and then, again, wrap up with what your current status and schedule is.

Just briefly, again, here we're to report on where we are on 50.46, in particular. I want to remind the committee that the paper that they have currently is still pre-decisional. It's still at the EDO's office, and has not been signed off.

Hopefully, you know, within a week it will get up to the Commission and become public, but right now it still is pre-decisional.

We did have in the paper noting that we have requested a letter from the ACRS, and that hopefully it would follow shortly after the July meeting. But based on that, you know, we would particularly appreciate, you know, comments on our options, any comments on implementation issues, whether we've noted them all, if there's something missing, and then looking at the feasibility, the things that we think are feasible in the short term and things that we think are perhaps feasible, but are longer term efforts, and we will get into each of those.

Again, on the background, this is just a reminder and a refresher that in looking at Option 3, there are two phases to Option 3. The first phase is what we call the feasibility study, and at the end point of the Phase 1 is where we have our recommendations to the Commission.

In going through the Phase 1, we identified, you know, what are the candidate requirements, which are those that are amenable to being risk informed. We prioritized them. The prioritization does look at not only the resources and costs, but the desirability of doing it, which of course feeds in the resources and costs, and you know, how much safety benefit would come out of it.

And then the third part once we have done the prioritization is to take and look at the feasibility and provide recommendations. But I want to emphasize again it's a feasibility study. The Phase 2 is getting into the detailed technical work to support the rulemaking.

It could turn out that when we get into Phase 2 something that we thought was feasible isn't feasible, but we do go into it with a lot of confidence that it is doable.

Based on that, we'll get right into 50.46.

We wanted to quickly give an overview of what we're talking about. We use the term "50.46," and when we talk about 50.46, we always mean in that context Appendix K in GDC 35. All three of these entities go hand in hand.

And when you look at it, we're going to be going through what we call these four topical areas here: the ECC reliability; the acceptance criteria; the evaluation model; and the LOCA size definition.

When you look at the requirements that are stated in the 50.46, Appendix K, and 35, they divide up into these four topical areas or four categories, classes, whatever word you want to use. But, in essence, they establish the reliability and what your acceptance criteria is and what evaluation model you need to be using in analyzing your ECCS, and then looking at the LOCA sizes.

So when we look at those four topical areas and looking at information that we have that we've learned over the years and our knowledge, and looking at the top three, we do feel that it's feasible right now in the short term to do things to the requirements that are associated with the reliability, with the acceptance criteria, and with the evaluation model.

When we look at the fourth one, we haven't completely established the feasibility. We feel we need to do a lot more work in that area, and that's a longer term effort.

I don't know if you want to add some stuff to this, Alan.

MR. KURITZKY: Just mainly for this last one, I think the important thing is that we need to improve our state of knowledge of LOCA frequencies. I mean, right now our current estimates of large break LOCA frequencies, we know or we feel that right now they're not low enough that we could rule out a large break LOCA as a classroom design basis.

As we go and look into more and improve our state of knowledge of LOCA frequencies, we may decide that there are further relaxations that we can afford large break LOCAs. That's just something that will have to be determined as we continue the feasibility study.

DR. POWERS: Let me just understand that a little bit better. You're defining large break LOCA in this case as something like a six inch break size.

MR. KURITZKY: Yeah. In this example, using the PRA definition, which for PWR would be greater than six inches.

DR. POWERS: Do you think that you could rule out some larger size break? Would you feel comfortable that, you know, you can argue that that frequency is low enough at this point?

MR. KURITZKY: Well, the point is that right now what we don't have is data for LOCA frequencies versus pipe size. The current data, the best data we have right now is for the group in the PRA, which is six inches and above.

DR. POWERS: You're not likely to get data on breaks, you know, of larger pipes.

MR. KURITZKY: Well, right, right, right. Well, not data, but you can do analyses, and you're going to have some consideration of service data to help fashion your end -- the frequency you come up with.

But the bottom line is that we recognize that a double ended guillotine break of the large pipe in the RCS is a very low frequency event, but if we're going to eliminate that one from the design basis, we have to set a new point of where we're going to -- the maximum that can be considered.

And right now we don't feel that we have enough information to be able to confidently say what that point should be. So what we will be doing in the next few years as part of this effort will be to try and improve our state of knowledge of LOCA frequencies; maybe to come up with some kind of curve of LOCA frequency versus pipe size; and at that point, we may feel that at a certain point we can either say these LOCAs -- and it doesn't have to be at the six inch point, but at any point -- that LOCAs above this size don't need to be considered at all in the design basis; LOCAs in this interval need to be considered, but not to the full extent they are right now. You know, there could be some graded approach.

But all of that hinges on our improvement of the state of knowledge of LOCA frequencies, which is fairly rudimentary right now.

DR. KRESS: Does that assume that the frequency is only a function of pipe size? It seems like there's some other variables in there.

MR. KURITZKY: Yeah. In fact --

DR. KRESS: You would hide those in the uncertainty distribution some way maybe?

MR. KURITZKY: Well, if you had the metallurgical people here, they could give you a whole bunch of other things, the locations, and I don't know whether it would be -- I don't want to speak in those areas because Dr. Shack can do a lot better job at this than I could, but whether it be like number of wells and, you know, the --

DR. KRESS: In any given plant, there's not a continuum of pipe sizes? There's a set of discrete sizes?

MR. KURITZKY: For the most part, yeah.

DR. KRESS: So you go from six inches to what? The next size up is?

CHAIRMAN APOSTOLAKIS: If you had a guillotine break of the largest pipe, what is the equivalent diameter?

DR. SHACK: Twenty-four to 36.

MS. DROUIN: Yeah, on that order.

MR. ROSEN: I would make a simple point that we are getting data on large break LOCA frequency every day. Every day that we don't have one is another day that we add to the database.

DR. KRESS: It's going to take a long time.

MR. KURITZKY: Right.

CHAIRMAN APOSTOLAKIS: I guess you are proposing that the first three boxes of the previous slide -- maybe you can put it back -- the reliability, acceptance criteria, and evaluation model for the ECCS. We can do something about these in the short term, right? Whereas the size will have to wait for later.

I wonder what the dependence of the first three boxes is on the fourth one.

MR. KURITZKY: It's a good point. Specifically the first box -- the second and third, not as much, but there could be some dependence especially on the third, but the first one very much so could be dependent on the same thing.

CHAIRMAN APOSTOLAKIS: Well, I mean, the third, too. I mean --

MR. KURITZKY: The third to some extent also.

CHAIRMAN APOSTOLAKIS: Realistic assessment of uncertainties, doesn't that depend on what kind of size of break you have?

MR. KURITZKY: Right, and you could adjust that. The current model is looking at all of them. So it's not as important.

CHAIRMAN APOSTOLAKIS: Right.

MR. KURITZKY: If you want to try to scale back what your requirements would be for that model for different sizes, which is something we're also looking for in the long term.

In the graded approach I was referring to, you may have something where certain size LOCAs, it doesn't mean you take them out of the design basis, but you may lessen the degree of conservatism as defined in the modeling for those, in the performance calculations.

But particularly in the first one, too, I mean, the frequency of LOCAs would be very -- you know, improved state of knowledge on LOCA frequency would be very useful to that first one. What we were trying to do in the short term, just to kind of rehash what we discussed on Monday, is we're coming up with two options under there, and I don't want to jump the gun too much, but one of them would be more prescriptive where the NRC would set what boundary conditions you would have to consider for your ECCS performance calculations, and we would do that based on our current knowledge of large break LOCAs.

We would also have an option where a licensee could do plan specific calculations to further fine tune what needs to be considered in those calculations.

One of the areas where they could help them fine tune --

MS. DROUIN: I think we're getting way ahead of our presentation here.

CHAIRMAN APOSTOLAKIS: I understand that.

MS. DROUIN: We've got slides on all of this.

CHAIRMAN APOSTOLAKIS: But the point is that ideally the size of the LOCA should be critical to the evaluation of all boxes, right?

MR. KURITZKY: Un-huh.

CHAIRMAN APOSTOLAKIS: The reliability of response and so on. The reason why perhaps -- I'm trying to understand now -- the reason why the first three boxes are easier to handle in the short term is because they are deterministic. You are really looking at the availability of on-site power and this and that, and you say, "Gee, I can do something about it, even though I don't know the exact size of what a large LOCA is."

MS. DROUIN: That is correct.

CHAIRMAN APOSTOLAKIS: Right? But if I were to do a detailed reliability calculation in the sense of PRA, I would have to know, but the criteria are deterministic due to tradition. So you say, "No, no, no. I can do something about it because what I can do is independent of the size."

MR. KURITZKY: Right, though we try to assume --

CHAIRMAN APOSTOLAKIS: Okay. So that's a correct understanding then.

MR. KURITZKY: We still have to assume a LOCA frequency of the --

CHAIRMAN APOSTOLAKIS: Sure, but whether it's six inches equivalent in diameter --

MR. KURITZKY: Right, exactly.

CHAIRMAN APOSTOLAKIS: -- or ten, it doesn't affect the first three boxes.

MR. KURITZKY: Exactly.

CHAIRMAN APOSTOLAKIS: Does it? I mean, because if it does, we need to know.

DR. SHACK: The first box, the proposal is going to certainly depend on their ability to discriminate from break sizes, but that's getting ahead.

MR. KURITZKY: I understand. There's something we can do now.

CHAIRMAN APOSTOLAKIS: Which is independent of size.

MR. KURITZKY: Right, and then more could be done with --

CHAIRMAN APOSTOLAKIS: Okay.

DR. SHACK: Size matters.

MS. DROUIN: We're going to come back and revisit this as we get into it, go into the presentation. But just quickly, you know, when we just talk about that, the first three boxes are in the short term, the fourth box in the longer term.

What do we mean by that? And you heard on Monday that in the short term we have this A and this B. Well, the A is looking at making changes to the current 50.46, and that's looking at changes that would be made to the acceptance criteria in the evaluation model, and this is stuff that is not so much risk informed as clean-up, and we're going to get into each one of these in more detail.

The only thing I want to point out is that we have two things that we're doing in the short term, and that's changes to the current 50.46 and also developing here a voluntary risk informed alternative.

DR. POWERS: This I don't understand, why you're making these changes.

MS. DROUIN: Excuse me?

DR. POWERS: Why are you making these changes?

MS. DROUIN: If you will hold off, we're going to get into what the changes are and why we're --

DR. POWERS: Why don't you tell me now?

MS. DROUIN: Can I -- can we skip to that slide, then?

I guess I don't understand the why.

DR. POWERS: So you're making changes to the tech. requirements of the current 50.46 related to the acceptance criteria in the evaluation model. I just wanted to know why.

MS. DROUIN: I guess we have --

DR. POWERS: If you don't like them --

MS. DROUIN: We have better knowledge. We're trying to address the conservatisms that we think are unnecessary.

MR. KURITZKY: I think that is the previous slide is where you want to go.

MS. DROUIN: Make it more up to date.

MR. KURITZKY: Option 3 is directing us to look at regulations that we can make changes to technical requirements, to reduce unnecessary burden, to enhance safety.

DR. POWERS: So you've got some measures of burden?

MR. KURITZKY: Yes, we do.

MS. DROUIN: Yes.

MR. KURITZKY: That have been supplied by industry.

DR. POWERS: And you're going to tell me about those?

MS. DROUIN: Yes. I'm sorry. I wasn't -- I just didn't understand what your why -- where you were coming from on that question.

DR. POWERS: Yeah, I just want to understand why you're making these changes.

DR. SHACK: It says for improved safety and to reduce unnecessary burden kind of a question.

MS. DROUIN: Yes.

DR. SHACK: But A is primarily an unnecessary burden question.

MS. DROUIN: Absolutely.

DR. SHACK: And B, we could argue --

CHAIRMAN APOSTOLAKIS: And improved knowledge of, and improved knowledge.

MS. DROUIN: I also want to point out that in both the short-term and the long-term considerations that we're trying to do, is that we are following the guidelines of our framework.

And on the last one, in following the guidelines of the framework, again, to reinforce that this is not risk based. It is risk informed. So if we go back to the framework, we had those six principles of defense in depth, and those always have to be met.

Just schedule-wise, in looking at the proposals that we have under consideration, the short-term one we're looking at in terms of develop the proposed rule that the 12 months after the issuance of the SRM or two months after the completion of the technical work.

Then when you look at the technical work, if you're looking at changes to the current 50.46, we're going to continue. We aren't going to stop right now. We're going to continue doing the technical work. We feel we can have the first one done by July of 2002, and then in terms of the risk informed alternative option, that's in April of 2002.

DR. POWERS: Proposing in this is work to be done in July 2002? Is that why you're proposing changing Baker-Just to Cathcart-Pawel?

MS. DROUIN: Yes.

DR. POWERS: Can you explain to me the rationale behind that change? In particular, what I'd like to understand exactly is Cathcart-Pawel kinetics are based on plant sheds exposed to steam, and how do you apply flat surface data to curved surface fuel clad?

And the issue of spallation at high

oxidation extents.

MS. DROUIN: Okay. We're going to ask Norm, who's the expert.

MR. LAUBEN: I don't want to represent myself as the expert in this. I think Ralph Meyers, our fuels expert, but the idea of changing -- the idea of the change here is to get a more realistic heat generation rate. When it comes to cladding embrittlement and that sort of thing, that's being handled in the criteria part, not the change to Cathcart-Pawel.

Well, there are two things. First of all, as far as embrittlement is concerned, the current rule addresses zircaloy and ZIRLO fuels, and the idea of the change there is to look at embrittlement for all zirconium based claddings.

But the purpose of this is the known fact that the parabolic rate laws that govern all of these things are probably pretty conservative, well, is known to be quite conservative by using Baker-Just at the higher temperatures.

So to use another rate equation for heat generation is certainly justified by the data, and the data is taken from cylindrical cladding. At least it is for Cathcart-Pawel, and Baker-Just is the one that's based on dropping pieces of metal in water and things like that.

So I think that --

DR. POWERS: What I'm really asking is how do you handle the deviation from parabolic kinetics due to spallation of the oxide.

MR. LAUBEN: Okay. That's going to be -- there is no deviation. All of the rate equations now are parabolic, Cathcart-Pawel, Baker-Just and all of them.

DR. POWERS: -- all of them parabolic.

MR. LAUBEN: Right.

DR. POWERS: The question is how the metal behaves.

MR. LAUBEN: Right, and there is ongoing work that's being done by industry and our group of people who's at Argonne and so forth, who are looking at other information on oxidation, including the long-term pre-oxidation prior to the accident and so forth.

Is that kind of what you were asking about? There's a lot of pre-oxidation that might be affected during the accident and so forth?

DR. POWERS: There's a lot of deformation in the material --

MR. LAUBEN: Okay.

DR. POWERS: -- that takes place prior to accident.

MR. LAUBEN: Yes, right.

DR. POWERS: And so what you're going to look at is kind of optimal material that Cathcart and Pawel used, and then you're going to say that applies to something that is beaten, folded, and manipulated before it's oxidizing.

You also bring up the pre-oxidation. That clearly occurs, too, and I'm wondering how do you justify this. The one thing we knew when we used Baker-Just is we were definitely bounding things.

MR. LAUBEN: Well, it doesn't take into account any of these other factors either that you're talking -- excuse me?

DR. POWERS: We knew that it was giving us an upper bound on things.

MR. LAUBEN: Okay. Well, you know, beyond the fact that all indications are that it's very upper bounding when it comes to heat generation rate, this is the only reason we were going to change this.

DR. POWERS: Now are you going with something that is more realistic, or is it a lower bound?

MR. LAUBEN: Oh, no.

DR. POWERS: Cathcart and Pawel takes this nice, pristine material, hasn't been zapped by a single neutron. It hasn't been deformed, hasn't been cold worked, and oxidizes it.

Okay. Is that going to give you correct kind of reaction kinetics, remembering, recalling that there is nothing about zirconium oxidation that predisposes it to be exactly parabolic. It's just what we do to fit the equations, and sooner or later, it's going to fracture.

MR. LAUBEN: Yes, but the idea is that in the criteria itself, which are embrittlement criteria, the idea is that you -- well, let me put it this way then. Maybe this will help. In terms of embrittlement, for instance, Baker-Just would still be used. You can always carry along two equations in your code, one to calculate oxygen update, equivalent oxygen or ECR as they call it, equivalent clad reacted, which is related to the embrittlement criteria. You can carry along Baker-Just for that and still use Cathcart-Pawel to calculate the heat generation rate due to the oxide -- due to a parabolic oxide thickness equation.

So that's my feeling at this point, but

that goes into a lot of detail that we haven't really studied a lot, but I've discussed it with our fuels people, and this is what we think that we would do at this point.

So Baker-Just would still be there to calculate embrittlement. Cathcart-Pawel would be there to calculate oxidation rate or -- excuse me -- heat generation rate due to an oxide thickness diffusion.

DR. SHACK: I thought you had a rather good discussion in the framework document of this problem of you're trying to introduce some conservatism somewhere that are covering uncertainties and non-conservatisms in other elements, and that you have to be careful when you're reducing known conservatisms that you're not leaving yourself vulnerable to the things that you were non-conservative about and uncertain about elsewhere.

MR. LAUBEN: But that's also a somewhat dangerous thing to do because that says that you can use one conservative phenomenology to offset another non-conservative phenomenology, and that's sort of subtracting apples and oranges, and I'm not sure that that's a very effective way to do it.

DR. SHACK: Well, it seems to me dangerous to just go around reducing conservatisms on one element without looking --

MR. LAUBEN: Absolutely.

DR. SHACK: -- at what those conservatisms were covering elsewhere.

MR. LAUBEN: Absolutely.

DR. SHACK: But I thought this is, again, a feasibility argument.

MR. LAUBEN: Yes.

DR. SHACK: What we were talking about here is your current judgment is this can be done, but you will be doing the detailed technical work to support that.

MR. LAUBEN: Yes, and also I think that that are things in 50.46 that we are not changing that say that errors and changes need to be assessed and reported, and so forth. So I think that that provides a basis to assure yourself that acknowledged non-conservatisms are not now going to become dominant in a way that you would not like.

And, therefore, I think the regulatory guide could take care of a lot of this to make sure that you don't allow that to happen.

DR. SHACK: Now, when you say you're going to do this proposed rulemaking, that would have the accompanying reg. guide as part of that when the package is ready?

MS. DROUIN: Yes, that goes with it.

DR. KRESS: This whole discussion seems to me to scream out for a real good uncertainty analysis to be accompanying the Appendix K type things. I just thought I throw in --

MS. DROUIN: I agree.

DR. KRESS: -- my favorite subject here.

MS. DROUIN: I agree.

DR. POWERS: I guess the question that comes to my mind, Tom, is why would you call out a kinetic expression at all if your objective is to get wider utility of cladding without exemptions. Why not just say, "Look. I want you to calculate in a reasonable fashion the amount of heat generation of the chemical reaction, and I want you to estimate the loss of ductility"?

DR. KRESS: Oh, definitely. In fact, I thought actually that was the direction they were going in in rewriting the rule. Yeah, I think you're absolutely right. That's the way to express it.

DR. POWERS: They write out Cathcart-Pawel. You've got a problem. First of all, there's not a specific --

DR. KRESS: Yeah, yeah. I think that's --

DR. POWERS: So the generality is now suddenly suspect. Plus they're hanging onto the concept of old parabolic reactions, a material that inherently doesn't want to be parabolic and geometries that don't inherently want to be stable.

DR. KRESS: Yeah. I think it's the old problem of what do you put in a rule and what do you put in a reg. guide. I certainly wouldn't put it in the rule, but in the reg. guide I would allow a lot more flexibility, but you know, the tendency is to say this is one way that's acceptable to us, and I don't know if they want to make that judgment or not.

See, that's a judgment that --

DR. POWERS: It seems to me that following your tact, that they have to scrutinize the reg. guide very closely because if they're enamored with parabolic kinetics, which are nice and simple, on a material that itself is not nice and simple --

DR. KRESS: And you know, they're trying to loosen it up to allow other types of clad, and I don't think we know what those other -- those other types of clad may very well depart more from parabolic. So I would have written it more generally also.

DR. POWERS: I mean, we're looking at what they're fixing to do, and they seem to have a strategy that inherently has a difficulty in it in that it's simply replacing one set of determinisms with another set of determinisms and saying, "Ah, but these are risk informed."

They're no more risk informed now than they were, or no less, than the original ones were.

DR. KRESS: Yeah. I think that's almost a good description of the nature of everything they're doing under A, and that's why we're more interested in what they're going to do under B, I think.

MS. DROUIN: I agree with your statement, and one of the points that we did make on Monday is that when you look at the A and the changes that we're considering to the current 50.46, that we wouldn't call this risk informed. We were looking at this more as just a clean-up in terms of we've got newer information, and that's why we were in terms of this one modifying the current --

DR. WALLIS: This isn't a clean-up. This is a change in philosophy. I mean it's moved away from prescriptive to performance based. That is a change in approach, and I think we supported that idea, I mean, when you were here a couple of days ago.

MS. DROUIN: Yes. That is correct.DR. WALLIS: And that's the thrust of the previous discussion we just had, is that don't be prescriptive. Don't replace one prescription with another, but replace it with a really well thought out performance based requirement.

Now, we talked a bit about whether or not these requirements were worded in a suitable way, I remember, and we have to be very careful about how we define the requirements that the cladding must satisfy.

I don't see this as a clean-up. I see it actually as a change in approach.

MS. DROUIN: For this part of it, that would be true.

MR. KURITZKY: Yes.

MS. DROUIN: But this is the first part of A. There were two parts to A, and I just --

DR. WALLIS: I think we were supportive generally of this approach on Monday.

MS. DROUIN: Yes, you did. Yes, you did.

MR. KURITZKY: Okay. Actually so let me go right here. Under A, as Mary said, there are two sets of changes we're looking at on A and B, for lack of a better discrimination, but the A is the changes to the current regulations and whether you consider it to be clean-up or more performance based; really the purpose of this first part to make it more performance based is to make it more flexible, allow it to apply to more than just the zircaloy and ZIRLO cladding.

So we have A, which is the changes to the current requirements, and B is the more risk informed piece. So first to discuss the changes to the current requirements, there are two pieces to that really also. The first is this one that Dr. Wallis was just mentioning about the performance based requirement for the acceptance criteria, and the second one is going to be changed to the Appendix K required features, which Dr. Powers was bringing up some questions on.

So first we'll touch base with the acceptance criteria. What we're considering doing is replacing the current five prescriptive acceptance criteria which are in Paragraph B of 50.46 over the performance based requirement, and just as a quick rehash, those current five acceptance criteria are the peak cladding temperature limit of 2,200 degrees F. and the 70 percent total oxidation, and those two are really surrogates for embrittlement, for an embrittlement criterion.

And then there's a limit on hydrogen

generation. The fourth one is to maintain core geometry which deals with the ballooning and swallowing of the fuel to make sure that you have adequate core fold area (phonetic).

And the last one is the need to maintain long-term cooling, and what we were proposing here, we would have a performance based requirement to demonstrate adequate post quench cladding ductility and adequate core flow area to insure that the core remains amenable to cooling, and what that would do is that would really cover us for the first two, like one, two, and four of the original five criteria, the two embrittlement criteria, and the one to make sure that the core geometry remains amenable to cooling.

But in the second part of our performance based requirements after the duration of the accident, we maintain calculated core temperature at an acceptably low value, and we move to K heat, and that's essentially just copying the fifth one, the long-term cooling one from the current criteria.

DR. WALLIS: On Monday we discussed these, and I think we encouraged you to think again about how you define these because adequate cladding ductility is a pretty vague expression.

MR. KURITZKY: Right.

DR. WALLIS: And then you can always cool things, but then the question is what are the consequences of the way you cool. So you have to be very careful about how you define these performance based requirements, and I think we felt that that particular one you've written there needed some work.

MR. KURITZKY: Right, and in fact, I have noted that there were comments on what we talked about as far as remaining amenable to cooling and also, you know, maintaining the temperature and acceptably evaluate. We did note those comments.

MR. LEITCH: Would you then leave unchanged the hydrogen generation specification?

MR. KURITZKY: No, actually as was mentioned, the hydrogen one was actually being dropped from the performance based requirement because it's not found to be controlling, and the hydrogen generation is covered by the 50.44, the hydrogen rule.

MR. LEITCH: Okay.

MR. KURITZKY: The combustible gas rule. So it would have the hydrogen piece left in the ECCS rule.

MR. LEITCH: So this would be silent then on --

MR. KURITZKY: On the hydrogen generation, yes.

DR. POWERS: If your objective is to have a coolable geometry, why do you bring up cladding ductility and flow areas? Why not just say be able to cool this core?

MR. KURITZKY: Well, again, this is not my expertise, and if Ralph Meyer, had he been here probably could fill you in better, but I think you try to start at the highest level possible. Obviously, what you just mentioned would be the highest level, and so there could be some argument of why we would want to maintain it there, but I guess you want to also go down to a level you feel you can have some metric or some way of measuring your performance, and so it's at that level, it's my understanding, that that's where we're comfortable and where we can measure something.

DR. WALLIS: Well, I think we said you could cool a rubble bed, too. I mean you've got to be more specific than just you can cool it.

MR. KURITZKY: Right, right, and demonstrating adequate post quench ductility kind of maintained -- is telling you that you're going to keep the basic structure of the core. So you're not --

DR. WALLIS: Why don't you say something

about release of fission products? I guess we said this on Monday. That seems to be what you're trying to prevent.

MR. KURITZKY: Right, right, and that kind of goes, I think, along with what Dr. Powers said, but I think the same reason was it was hard to specify a limit if we looked at that high level, and this was the next level down where it felt comfortable, and it could become established criteria or measurable criteria.

DR. POWERS: Well, you way it's hard to establish a limit on fission product release. Actually it's easy to establish a limit. You can pick one.

What you're saying is it's hard to pick one that easy to justify.

MR. KURITZKY: Exactly.

DR. POWERS: Now, doesn't risk have a role in trying to make that justification?

MR. KURITZKY: Risk may have a role. We actually had considered some risk informed changes here. We didn't feel that at this time that we were in a position to make recommendations along those lines. So we were sticking with just the so-called clean-up aspects of these requirements.

I mean as part of the long-term work when we're looking at a greater approach, there may be some other changes we would consider.

DR. WALLIS: I'm not sure risk helps you at all because you talk about core damage frequency. A different number of kinds of core damage doesn't tell you how much is tolerable, and LERF just says your large release doesn't tell you. It asks about the frequency. It doesn't talk about how much. So I'm not sure those measures help you. You need some other kind of measure.

DR. KRESS: Yeah, you need an FC acceptance curve.

MR. KURITZKY: So anyway, in the short term --

DR. POWERS: He never gives up. He never gives up.

MR. KURITZKY: In the short-term, just to recap, for this part of the changes to the current requirements, we're just looking at making -- replacing the existing criteria with the performance based criteria, which gives you a little bit more flexibility.

The main point is right there in that last bullet. This is really just to allow use of cladding materials besides zircaloy and ZIRLO, without requiring the licensees to submit an exemption request, which is what currently would be the case or is the case.

DR. POWERS: I guess, I mean, that's the one I just want to ground on. Every time I see the words "Cathcart-Pawel" and then I see this line, why? Why call out the kinetic expression?

MR. KURITZKY: Well, are you discussing the changes to Appendix K now, the Cathcart-Pawel replacement? Because that's going to be the next piece we're going to talk about.

This line here applies to only these changes which apply to the acceptance criteria, not to the evaluation models, is where we're going to discuss Cathcart-Pawel.

DR. POWERS: Okay, but I can't separate those two.

MR. KURITZKY: Well, I think, and again, it's not my area, but as Norm had mentioned, really in the modeling my understanding is that you can actually separate those two or you can use Cathcart-Pawel just for the heat --

DR. POWERS: It applies to a particular material. It happens to be a zircaloy. It doesn't apply to ZIRLO. It doesn't apply to M-5. Why call it out? Why not say you've got to evaluate this stuff, whatever kinetic expression you use?

MR. KURITZKY: Unfortunately I can't respond to that because that's no my area.

DR. POWERS: Well, I mean, it seems remarkable that you've got a strategy here in which each is an isolated fiefdom and there's no contact between the two.

MR. CUNNINGHAM: If I might, this is Mark Cunningham from the staff.

I think it's important to go back to a point, I believe, that Dr. Shack made a little while ago, is that the context of what we're talking about here in these changes is that this is the results of a feasibility study, and there's more work to be done to develop the technical basis for a rule change and to develop the language in the rule or the rule changes.

And we've got a year's worth of work in front of us in order to do that. In the paper, I think we tried to be very explicit that possible changes to the current 50.46 could involve a number of things, including the changes from Baker-Just to Cathcart-Pawel. It doesn't mean it's done, but we think it's something that makes -- based on a feasibility study, it makes sense to do.

DR. POWERS: And I think what I'm telling you is that's not the right way to go at this.

MR. CUNNINGHAM: Okay, and that's fair.

DR. POWERS: The way to go is to say, "Look. We want to include chemical reaction, heat, into these calculations. When you do it, recognize the following things," and don't call out parabolic kinetics. Any kinetics you want to that you can justify, but recognize that the surface is not going to be flat. It's not even going to have constant radius curvature anymore, and address these questions.

There's the technical feasibility to do this. The French have done some very nice things looking at the stability of the oxide coating as a function of the oxide thickness and deformations of the surface. So you're not asking people to invent new technologies here.

MR. CUNNINGHAM: Okay. That's fair. thank you.

DR. WALLIS: When you get down the road a bit, you've got to think about how you actually administer. Probably the NRR folks are going to decide when they've got an application and somebody comes in with some new way of trying to show that the core remains cooled; without using the old criteria, how someone is going to decide whether this is an acceptable application.

You've got to think it through all the way to how is the person who's actually got to make the decision going to be helped by the new rule. If it's not specific enough, it's going to be difficult. Everything is going to be debatable to the nth degree.

MS. DROUIN: If I'm understanding -- maybe I'm still a little bit confused by Dr. Powers' statement -- but is your objection that we're taking specific criteria in Appendix K and replacing it with other specific criteria, or that we should take what's specific and replace it with more performance?

So if I look at the example here, the Baker-Just, don't take the Baker-Just and replace it with something else prescriptive --

DR. POWERS: Replace it with what you're trying to achieve, yeah.

MS. DROUIN: But take the Baker-Just, for example, and replace it more with a performance criteria.

DR. POWERS: Well, tell the people what you --

MS. DROUIN: Not prescribe another -- another --

DR. POWERS: What you want to achieve is you want to take into account, for instance, in connection with your Cathcart-Pawel, as I understand now that's strictly a heat generation. You want them to calculate for chemical reaction heat generation. Okay. That's a reasonable thing to do. There's a lot of heat here.

Okay. As soon as you say use this this kinetic expression, you're taking a whole set of physics and you're saying this is not important. You're saying that it's parabolic at the beginning. It's parabolic at the end. Nothing ever happens. Nothing ever spoils, no new information, no radiation effects on this oxidation process that are important to the calculation of heat.

That may or may not be true for every kind of clad that you get in. So you're far better off to say -- to give them something that says what your objective is and let them figure out how they're going to do it, and put in your reg. guide all of the things that you're going to take into account.

DR. WALLIS: Even so, you still haven't got to a performance criterion. I mean, the real performance criterion has to do with the integrity of the clad and the nonrelease of fission products and all of that, which is affected --

DR. SHACK: But this is an evaluation part here.

DR. WALLIS: -- which is affected by temperature and the temperature is affected by anything that raises or lowers the temperature, including chemical reactions and so on. So the chemical reactions are a means to an end. They're not a performance based criteria.

It's just that in order to properly achieve that performance, you have to consider all of the things that affect it.

MS. DROUIN: Absolutely.

DR. WALLIS: So you think about briefly what are the performance goals and how you define them clearly, and you've got to consider all of the physics and chemistry and everything else that goes into evaluating that performance. That's what you need to do.

MS. DROUIN: I guess, you know, just to reiterate what Mark said, you know, we're just in the feasibility, and I will admit that in terms of the evaluation model and prescriptive requirements in

Appendix K associated with it, we had not looked at that in terms of taking those, as you can see, very prescriptive requirements and replacing them like we did on the acceptance criteria with more performance based. That's not something we had thought about.

I think, you know, it is a good comment and something we should pursue and look at in more detail. It's just a very good comment. We just hadn't gone down that road.

So why don't we get over to --

MR. KURITZKY: I guess since you flipped on the second side, this is just basically the changes that we had looked at as far as Appendix K. We discussed them on Monday. We would certainly take into account the comments today about looking at something more, specifying a performance goal or working along a performance goal as opposed to just replacing one prescriptive criterion with another one, and that can be looked at under our Phase 2 work.

And again, just to reiterate, the last bullet there which goes to the issue that was actually discussed a little bit already this morning, but that when we go about --

DR. WALLIS: Now, those performance items -- excuse me -- those performance goals, if you really wanted to be risk informed, should be risk informed in some way.

MR. KURITZKY: Right.

DR. WALLIS: Use the risk information to determine the goals, and then you say you've got to use whatever physics, chemistry, and so on is justifiable to achieve those goals. Isn't that where the risk informed comes in?

MR. KURITZKY: Yes.

DR. WALLIS: That doesn't come in at the level of Cathcart-Pawel.

MR. KURITZKY: These were not supposed to be risk informed changes, but you're right. To do that risk --

DR. WALLIS: Yeah, but how do you know what your performance goals are until you actually look at the consequences of the accident and do some evaluation of risk? Isn't that where the risk informed comes in?

Otherwise you're just picking something.

MR. CUNNINGHAM: Yeah, this is Mark Cunningham.

I think we agree, Dr. Wallis, that in effect what we're saying is we can make these changes and have confidence from a risk assessment standpoint that we're not changing the risk to the public by making these changes.

So it comes in as a metric at that level as opposed to saying whether or not it's Cathcart-Pawel versus Baker-Just.

DR. WALLIS: But I think you don't know. You see, you're asking for some requirement on cladding ductility. Unless you make the link between cladding ductility and public safety, you don't know what sort of cladding ductility to require.

MR. CUNNINGHAM: But I think we have some confidence at the end where you could make these changes and know that it's not going to change your risk assessment results.

DR. WALLIS: Well, that's a kind of litany though. You've got to make the analysis to show that's the case.

MR. CUNNINGHAM: Yeah, yeah. But that's the underlying presumption here, and I think we can justify that.

DR. WALLIS: I think we're trying to be helpful. We seem to be slowing down your enthusiasm, but we don't want to do that.

MS. DROUIN: No, no.

DR. WALLIS: I don't want to do that.

MS. DROUIN: It's not that. You've just raised a good point we hadn't thought about, and you caught us off guard.

DR. WALLIS: I'm a little bit disturbed because on Monday you seemed to be so full of enthusiasm, and we seem to have slowed you down. I don't want to do that.

MS. DROUIN: I don't think that's occurred.

DR. WALLIS: Okay, good.

MR. KURITZKY: You've just given us more things to look at under our Phase 2.

MS. DROUIN: Shall we go to B?

MR. LEITCH: Just before you move to B, in your discussion of A, I see a lot of discussion about acceptance criteria and evaluation model, but I thought that A was also going to address ECCS reliability.

MS. DROUIN: No, that's B.

MR. LEITCH: That's B?

MS. DROUIN: That's B. So why don't we go ahead and get to B?

MR. LEITCH: Okay.

MS. DROUIN: A was the changes to the current 50.46, and that was just strictly changes to the acceptance criteria, and the evaluation model.

MR. LEITCH: Okay.

MS. DROUIN: Changes to the ECCS reliability, which Alan will get into, that is developing risk informed. Now we're really starting to bring in risk information.

MR. KURITZKY: In fact, it's so different than the last one that we actually call it 50.466 so that there's no confusion with 46.

(Laughter.)

MR. KURITZKY: The B changes, the risk informed changes that we're looking at apply to the ECCS reliability requirements. If you remember the figure that Mary put up in the beginning, we subdivide the requirement into four categories, and the one that we're looking specifically at risk informed are those that deal with the reliability of the ECCS system, and we wanted to make those reliability requirements commensurate with the frequency of challenge to the system.

And we have come up with two options to accommodate --

DR. POWERS: Why do you choose frequency of the challenge to the system and not the consequences of system not being mitigated?

MR. KURITZKY: Well, I guess you would probably get -- you see, actually the two options that we have, what we're going to come up with there is going to be based on a metric -- in our case, we're currently using core damage frequency, but you are looking at the consequence of what happens with system failure.

It's just that in looking at -- I think it was like an equation that we had things multiplied by each other, and so you want to make A times B equal something. You know, you're going to want to make them consistent.

If A is much lower, you can let B be higher, you know, and vice versa. So that's what we mean when we say commensurate with the frequency of challenge. If you multiply them together, you get the consequence, but in looking at what we're going to allow, where we can allow flexibility, you know, if you have two variables, if one goes higher, if you fix the one point, then you can let A get higher and B gets lower, et cetera.

DR. POWERS: That's too abstract for my brain this morning.

MR. KURITZKY: All right. Well, let me go through --

DR. POWERS: It seems to me that an accident that results in lots of consequences, even if it has a fuller frequency than one that results in zip consequences is what am I going to focus on. So I would look at consequences and not on frequencies.

MR. KURITZKY: Let me see if I understand your question. Instead of focusing on, let's say, core damage frequencies are metric, you're saying we should actually look at release.

DR. POWERS: Sure.

MR. KURITZKY: That's something that could be considered. In fact, we had to make a determination at least according to how we feel we're going to approach it, whether we're going to try to tie it to core damage frequency; if it would just be on the frequency of the accident or LERF, which would be on the frequency of obviously release from, you know, containment, or you could go all the way as you suggest, Dr. Powers, and look at essentially a Level 3 and have numbers.

But I think the reason -- and Mary or Mark will correct me if I'm wrong -- but I think the reason why we kind of were thinking of sticking with core damage frequency as I mentioned was just because that was one that had the best understanding and had the best, you know, peer raised (phonetic) in existence, the level on peer raised, and there are a lot more, and there's a little more accomplished, and the results from the Level 1 that we have and some of the Level 2 and Level 3 peer raised. So it was kind of a --

DR. POWERS: You've done a lot in looking at some of these consequences. So you have information that could use at least in your design of the regulation studies.

MR. KURITZKY: Yes, there is information there, and it's something that we can consider whether or not we should make the metric CDF LERF or actual release.

MS. DROUIN: You would not do this in absence without considering your consequences. The primary measure that we are thinking about is CDF, but that wouldn't mean that we would not give any consideration. That violates the basic premise of our framework where we said you will be balanced between your prevention and mitigation. So --

DR. KRESS: The problem with the framework partially, Mary, is that the column on conditional containment failure probability has .1 in it, as best I remember.

MS. DROUIN: At the lower level. There's levels to the guidelines.

DR. KRESS: Yeah, but we're writing a set of voluntary rules for existing plants, and not all of those plants have .1 conditional containment failure probability.

MS. DROUIN: That's correct.

DR. KRESS: In fact, some of them have .5, and I'm not sure how -- if one of those plants wants to voluntarily use this new rule, do they have to improve their containment down to .1 or are they just excluded from being able to make use of this rule?

MS. DROUIN: No, that wouldn't be the case.

DR. KRESS: Well, how --

MS. DROUIN: Because that's where you could back up to your CDF.

DR. KRESS: So you are saying focus on maybe a LERF, and they could back down on their CDF.

MS. DROUIN: Yes.

DR. KRESS: But then your guidelines and your framework document are not hard and fixed. You're saying that they could -- as long as --

MS. DROUIN: The primary guideline of the quantitative guidelines and the framework starts with a LERF.

DR. KRESS: See, what I'm getting at is what George brought up in the subcommittee meeting, that why not just say in words that the LERF will not exceed a certain level, and you must also have a CDF that doesn't exceed a certain level. Why not word it like that rather than put those numbers in the framework boxes that you have?

MS. DROUIN: I guess I don't see the difference. To me the framework, in essence, does say that because it starts out with the LERF and then the next consideration that we have there is the CDF.

DR. KRESS: But I don't think it gives much guidance for those plants that have a conditional containment failure probability that's .5 or .8. I mean, what do those people do? I don't think it gives any guidance there and doesn't seem to say, "All right. If you've got" --

MS. DROUIN: But let's remember the framework is not for the licensees. The framework is for us in formulating the regulation and how we want to do it. This is an internal document.

The framework is not something that licensees have to meet. That is, those are guidelines for us.

DR. KRESS: But you're going to use it to write the rule, and they'll have to meet the rule now.

MS. DROUIN: Yes.

DR. KRESS: So there's a nexus between them.

MR. CUNNINGHAM: This is Mark Cunningham.

If you look at Item 2 here, ECCS functional reliability requirement, we talk about it in terms of a CDF, but in effect, it's a CDF and a LERF that would have to come into play here, and a plant that has a high conditional containment failure probability, given core damage, is not going to get as much benefit out -- wouldn't get as much benefit out of Item 2 than a plant that would have a very low conditional containment failure probability because they'd be constrained more by LERF than by CDF.

MS. DROUIN: Right.

MR. CUNNINGHAM: And that's going to have to come into play as we develop the details associated with one and two there.

MS. DROUIN: Correct.

MR. KURITZKY: In fact, this original option had LERF in there instead of CDF, which we changed later, and so Mark's point that LERF is some aspect to be considered also.

DR. WALLIS: We talked on Monday about what you mean by reliability. It seems to me there are two areas here. One is that you've got an ECCS system. The requirements like the 2,200 degrees are really based on the idea that you've designed the system so that there's no core damage frequency if it works. And then you can talk about reliability.

Do you have one train or two trains and all of that? That's in the PRA space, but there are almost two worlds, and I don't quite know how to bring risk information into the first world, about how to define the criteria for whether it works or not, the 2,200, the technical criteria for it's supposed to work if all of these things happen.

You, I think, claim that reliability covered both the question of whether there's one train or another, and also there's the technical questions. If it works as designed, then there will be no core damage. Is that also in the area of reliability or is that something else?

MR. KURITZKY: Like I said on Monday, when I think of something as being reliable, it only has to, you know, turn on or run, but it also has to meet the safety --

DR. WALLIS: Yeah, but if you think deeper than the 2,200, well, what's the reliability or the rationale that we went through in our thermal hydraulic analysis to pick 2,200? You know, that's also reliability in a sense.

MR. KURITZKY: I guess the 2,200 was more a safety margin type of thing, but again, the point being when we talk about being reliable, those performance calculations have to show that what one pump or two pumps or X pumps, you're going to be able to keep your --

DR. WALLIS: That's PRS/PRA space and you aren't allowed to do that.

MR. KURITZKY: Well, I was trying to show you that it's below 2,200. Then the PRA space says, okay, given that that calculation showed I needed one or two or three pumps, now how reliable -- or I need some set of pumps -- how reliable do I have to make that set of pumps be?

DR. WALLIS: Yeah, but then there's model uncertainty that we talked about, and we don't know how to put that into the PRA space.

MR. KURITZKY: Right, and it kind of almost has to be addressed in the hydraulic space. But I guess, again, I don't want to dive into a whole discourse on uncertainty. I'm not expert on that.

Everybody here knows a lot more about it than I do, but it does have to model uncertainty. You know, data uncertainty, I think we have a fairly good way of handling. Model uncertainties are obviously a trickier beast to deal with, but it's obviously something that has to be considered.

I mean, that's true, and that's going to be part of our brain scratching for that second phase.

DR. WALLIS: That's what we talked about under A, I think.

MS. DROUIN: Right. I mean, when you get into your model uncertainty, that's where you're getting into your acceptance criteria and your evaluation, and that would be taking care of that part. You have designed that ECCS system, and that's where you're also getting into, as Alan said, the thermal hydraulics.

Whether or not you're going to be providing enough coolant and the uncertainties of that would be taken care of under your acceptance criteria and the performance you have to meet.

DR. WALLIS: Well, then you have to bring some sort of risk criteria into the performance criteria, and we discussed how difficult that might be.

MS. DROUIN: Yeah. I don't mean to allude that it's going to be easy.

MR. KURITZKY: Yeah, I think Dr. Wallis' point is well taken. That is, that's just one of the areas that we have to -- that interconnection or connection, whatever you want to call it, you know, that passover, that's something that has to really be thought out to make that seamless connection.

DR. KRESS: Does that bullet under two imply that you may have different CDF thresholds for different plants?

MR. KURITZKY: No, actually that is supposed to -- that the NRC would prescribe a CDF threshold, and then --

DR. KRESS: For all plants?

MR. KURITZKY: -- has to be required to meet that.

They could use plant specific information on LOCA frequencies or ECS reliability to --

DR. KRESS: To arrive at it.

MR. KURITZKY: -- to arrive at it.

DR. KRESS: But the threshold itself will be --

MR. KURITZKY: Right. It is one across the board, and essentially it's going to be -- you know, it's only specified in a reg. guide or whatever for the licensees to use in Option 2. Under Option 1 the NRC is going to give prescriptive requirements. We're still going to be using that same -- whatever we arrive at, that same threshold will be used in our thinking to come up with what we're going to prescribe.

And again, as Mary said in the beginning, you know, right now we've done Phase I work, the feasibility study. There's a lot more technical work to be done under Phase 2, which would be to support the rulemaking.

Specifically for the risk informed, the voluntary risk informed alternative, there's a number of things up there. We've been discussing a lot of them in the list. I don't know if the list is necessarily growing, but we have some more ideas under these bullets that we have to consider that we're going to put emphasis on.

But coming up with acceptable methods and assumptions for performing these CDF or LERF analyses or system reliability analyses and also coming up with the appropriate thresholds, making -- you know, giving due consideration of uncertainties and both coming up with the thresholds and how the calculations are done to meet those thresholds, something Mary's staff has put a lot of work into.

Also we have to identify features or conditions at the plant that would tend to decrease the conditional probability of losing off-site power, given a LOCA, and determine acceptable methods and assumptions for estimating on a plant specific basis what that probability would be.

And I think that third bullet really goes to the first option on the previous slide, which is the prescriptive one, where we would say, yes, you don't need to consider loss of off-site power for this set of LOCAs if your plant has this feature or that feature, for instance, the ability of the plant to communicate with their transmission system operator, which would tend to decrease the probability of loss of off-site power after a LOCA.

So there would be a certain feature that maybe a plant would have to meet in order to be able to qualify, to not consider the fountain pen as loop (phonetic).

In the second case, the fourth bullet really applies to the second option from the previous slide where the plant would be doing plant specific calculations, and licensee would be doing plant specific calculations, and so therefore, they would need to consider in their analysis of the reliability of the ECCS -- they'd have to consider the probability of losing off-site power after the LOCA. And so they'd have to do some kind of plant specific calculation in that regard.

That's what's going to keep us busy through April 2002.

MS. DROUIN: July.

MR. KURITZKY: July. No, this is the April one. This is the April one.

DR. WALLIS: I'm still sort of thinking. Suppose that you in the prescriptive world were to say or were to consider the possibility of raising its reliable temperature to 2,300 from 2,200. Could you calculate the change in CDF resulting from that?

MR. KURITZKY: If there would be a change in that peak clad temperature?

DR. WALLIS: Right. Is there a mechanism? Does PRA enable you to bring in that sort of information and calculate a change in CDF?

Undoubtedly there is a change in CEDF if you do that.

MS. DROUIN: Yes, it does. That's for your success criteria.

DR. WALLIS: It does enable you to do that?

MS. DROUIN: Yes, because that goes to your definition of your core damage.

DR. WALLIS: So you assume that if you get over 2,300 you get core damage? That's the definition of core damage?

MS. DROUIN: Well, I mean, when you go to the PRAs, you know, the definition of core damage will change. I mean, for example, somebody might define it as top of the active fuel uncovered, the bottom of the active fuel. Some people define it as the peak cladding temperature, but my point is that that gives directly to the definition, and as you change the definition, then you change your success criteria.

DR. WALLIS: So you can change the CDF by changing your definition of core damage?

MS. DROUIN: Absolutely.

DR. WALLIS: Just by words?

MR. KURITZKY: Well, not --

MS. DROUIN: It's not by words. I mean, then you have to go and you do your calculations to see which systems. If, for example, I look at a boiler, and if I define my core damage as top of active fuel versus a little bit of uncover versus GP above the bottom of the active fuel versus my peak cladding temperature --

DR. WALLIS: How do you make those choices?

MS. DROUIN: You do a lot of calculations to see, you know, where you're going to start getting your releases.

DR. WALLIS: Oh, so you get back to releases, yes.

MS. DROUIN: You always go back to releases.

DR. WALLIS: Thank you. Good, good.

MS. DROUIN: But you will get different systems available.

MR. KURITZKY: Or the number of trains required.

MR. LEITCH: The third bullet there concerning loss of off-site power following a LOCA, you used as an example there the ability of the plant to communicate with the system operator. I'm just having trouble figuring out how --

MR. KURITZKY: That would help?

MR. LEITCH: Well, first of all, who wouldn't have the ability to do that? And how would it help?

In other words, what I'm picturing is some kind of a seismic event that causes a LOCA and brings down the transmission line simultaneously. So how does talking to the system operator help that?

Maybe I'm not following exactly what your thought is there.

MR. KURITZKY: Well, I think you bring up a couple of good points. The issue of the seismic event that would bring down the grid at the same time that it, you know, tripped the large break LOCA is definitely in that case talking and isn't really doing you any good. I mean that's got its own problems, and that's one piece that goes into the calculation.

That would be a case where the conditional loss of power is one for that event. I mean, there's nothing you can do about that.

However, in other types of situations -- and I'm not the electrical engineering expert, and I don't see the gentleman that we've been working with on that -- but the idea being that if you have -- in a lot of cases you have the grid tends to -- the area grid will have bolted sags and may have a degrade of voltage there, which would increase your chance of causing a separation to grid when you have the plant trip and the ECCS pump start.

Communication with the transmission system operator would allow you in certain situations, I think, to allow the operator to readjust the flow of current in the grid to may be beef up, you know, voltage to the plant so that you're not as susceptible to having that going to the trip point, going to the under voltage relay set points trying to separate you from the grid.

So you're getting the area grid managed where the transmission system operator in communication with you, you know, in sync with you to help you ride out, you know, that electrical transient, can help reduce your transient having a separation from the grid.

And my understanding is that far from all plants actually, you know, have procedures in place to routinely, you know, contact the transmission system operator under such conditions. Some plants do, but some plants don't.

MR. LEITCH: I would just think they all would. I'm surprised at that.

MR. KURITZKY: I guess, they all have the capability obviously. I guess it's more of whether it's proceduralized, I guess, is maybe the issue.

MR. LEITCH: Yeah.

MR. ROSEN: There are INPO requirements that have been in place for several years to have good communications with the grid operators, and I think I would be very surprised of any plant has not already complied.

MR. KURITZKY: Then that would help for all plants to take advantage of if that's the case.

DR. SHACK: But, again, just coming back to that, I mean, you would work out a sort of statistical analysis of the likelihood of loss of off-site power, and then you'd modify that in some way by features. Is that the kind of thing that's envisioned?

MR. KURITZKY: Yes, that' the type of thing we're looking at. We'll build out some of the work that was done for generic Issue 171 and try to identify, and some of the work that has been done since then and try to see if we can identify features that can have some measurable impact on that conditional loss of off-site power probability.

MR. LEITCH: Coming into that, again, is this issue that we always have a lot of trouble with, and that is assessing how well the operator would perform under those circumstances. In other words, he's just has a LOCA. Although he has procedures to contact the system operator, you know, is he going to do the right thing? Is he going to --

MR. KURITZKY: And that's the error of probability, right.

DR. BONACA: And I had the same thought, but my understanding will be that from more credible break sizes you would still preserve LOOP and single failure requirements.

MR. KURITZKY: Yes.

DR. BONACA: You would relax those for break sizes which right now seem to be so unlikely, like the double ended guillotine break. You don't have the basis to eliminate that from the rule right now, but you have the basis for utilizing that inside till it lacks something like the LOOP or -- okay. So that's the context of that.

MS. DROUIN: What you've heard to date is the proposed considerations for the short term, and now looking at what we're considering for long term, our long-term effort.

MR. KURITZKY: Okay. That goes to the fourth box. That was in the figure Mary put up before, the definition of large break LOCA sizes. Additional changes to 50.46 may also have some merit, including possibly change in the definition to inspection of breaks and locations.

The extent of the potential changes to 50.46 is dependent on our state of knowledge of LOCA frequencies as we discussed before, and here's an example of how we might use that information.

If we could feel confidence, considering uncertainties, that we have a collecting infrequency of occurrence below let's say ten to the minus four a year for a set of LOCAs, then we may believe that some regulatory relief is appropriate for those, that set of LOCAs, which is pretty much what we're doing right now with the changes we discussed under Item B.

If, for instance, we could also or in addition we could feel confident that the LOCAs were at frequencies below ten to the minus five, we may feel that we could go even further and possibly remove these from the design base, but as long as there was maintained some plant mitigated capability to address those accidents.

And further, if we could feel confident that we could -- that there was a set of LOCAs whose collective frequencies were below ten to the minus six per year, we may say that's so low we can take them out of the design basis completely and they don't have to be addressed at all.

So that's kind of how as we improve our state of knowledge of LOCA frequencies we could, you know, further change what would be the regulatory requirements associated with those set of LOCAs.

DR. POWERS: You'll never get somebody to come in and say probability of LOCA is definitely absolutely less than ten to the minus fifth per year. I just know how people are. They'll say, "Well, I'm not too certain about this, but it's the mean value maybe is less than ten to the minus fifth," or some confidence level or something.

What kind of confidence level are you looking for?

MR. KURITZKY: Okay. That will have to be decided as we go through. This is long-term work, and so that's something that will have to be considered, but you're right. You consider uncertainty analysis. You have to consider, you know, would it be something that we want the 95th percentile to be below that level? Would it be the mean? You know, do we feel the mean would be sufficient?

If you used the mean, you would probably want to have a more -- maybe a little more strict on what the professional guy would be. If you used the 95th percentile, then you may be a little more or less concerned exactly what that threshold is.

But all of that has to be figured out as part of that as long term.

DR. POWERS: Yeah, I guess I'm wondering how you do that. I mean, you said if you use the mean you'd be a little more restricted. You're doing something when you say that, doing some mental integration there. Can you tell me what that mental integration is?

MR. KURITZKY: Well, again, I guess you're tying it to release or, you know, core damage frequency or LERF or actual fission product release, but you're going to have some metric that you're going to run up against, and then you're going to back out, you know, frequency that you're going to --

MS. DROUIN: We're in the very early stages here on this. I mean, we know we're going to have to deal with the uncertainties. That's a given. How we're going to incorporate that into this, it's a blank sheet of paper almost at this point. We just know it's an issue we've got to address, and when you look at the time frame that we talked about where this is up to three years, this is one of the factors that comes in there because it's not a trivial issue.

So there's a lot of work that's going to have to be done here, particularly when it comes to the uncertainties.

DR. WALLIS: Well, thinking about long term, there's a certain CDF allowed by the present rule due to LOCAs and LOCAs contribute a certain amount to CDF, right? We know that. We know that LOCAs contribute.

It may be plant specific, but you can evaluate the contribution of LOCAs to CDF now, and presumably that's tolerable because we license reactors.

Now, we talked on Monday about optimizing the ECCS system. Why don't you in the long term think about allowing the licensee to simply optimize the ECCS system in any way, response to break sizes, reliability of this? I mean whatever it is as long as some CDF value criterion is met. Then it's up to the licensee. I mean, you don't specify all of this stuff. You simply say, "Evaluate all of your LOCAs, and the total CDF contribution is not to be more than a certain amount."

MR. KURITZKY: I mean that's --

DR. WALLIS: Isn't that really risk informing the whole thing in the long term?

DR. BONACA: Well, I must comment that

people can be very creative in telling you that single failures can happen, and yet you know, TMI had many failures happening, you know. They happen, I mean, and I agree that as a general principle I think -- but you've got to be very careful.

DR. WALLIS: This is long term.

DR. BONACA: Yeah.

MR. KURITZKY: The risk-informed in that respect --

DR. WALLIS: Well, that's a little bit of a --

MR. KURITZKY: We don't want to put all of our eggs in the CDF basket.

DR. WALLIS: You can always quibble about it. Whenever you want to do something really logical say, "Oh, don't do that because it's risk based," you know.

DR. BONACA: With this, you know, I had a question regarding it a little bit. Are we going to have at some point in the future a menu driven LOCA requirement such that this plant will have this criterion and this plant will have relaxed large break LOCA based on -- you know, simply that won't relax it, but simply that it assumes still double ended guillotine break, but only one train above a certain size break, and others will come in and in the future use -- I'm trying to understand what's going to be. I mean, you know, these plants are so different from each other already. I mean one of the issues have always been lack of standardization.

Are we going to have a future where we're going to even less standardization?

MR. KURITZKY: Well, I think if you just even look at the changes under B, that first option with the NRC would specify. Everybody would either pick that or something else.

But the second option under B, even there plants would -- you know, that's the change we're considering if it should go to implementation. You would have plants that could -- if they wanted to go through the analysis, they could demonstrate that they could adjust their --

DR. BONACA: Okay.

MR. KURITZKY: -- that different plants would, in fact, have different requirements for LOCA. I mean, you're right. It flies against standardization, but it allows for more use of risk information.

DR. BONACA: Okay. Plant specific relaxation, yeah, right.

MR. KURITZKY: Okay. Again, as part of this long-term work, one of the main things that we're really trying to strive at is to improve our state of knowledge of LOCA frequencies. In that vein, the staff is continuing to meet with representatives of the nuclear industry in public meetings to try and address a set of technical issues.

For example, the initial fall distributions, degradation mechanisms, uncertainty analysis, et cetera, that would go into those calculations of LOCA frequencies. As has been --

DR. POWERS: That seems to address the regulatory guide aspects of this. Those meetings don't seem to address, discussing earlier, your ten to the minus four, ten to the minus fifth business.

MR. KURITZKY: Yes. I was just going to point out that the next set of means that we have set up with the industry are to address this particular topic, but in order to actually carry through these changes to their implementation, there are many issues as were discussed here at these meetings today as well as other issues that would all have to be considered.

We currently have plans to meet with industry because this is one hurdle that has to be dealt with to move forward, but it is certainly not the only hurdle that has to be jumped over.

DR. POWERS: This seems to address the question of whether LOCAs occur at all.

MR. KURITZKY: Well, or whatever frequency they occur at.

DR. POWERS: Maybe it addresses what happens when you see that they reflect knowledgeables (phonetic) and things like that.

MR. KURITZKY: In other words, all of those issues need to be -- that all falls into those metallurgical concerns.

And just to sum up, if something -- if large break LOCA redefinition, the size redefinition is found to be feasible, the staff would recommend, you know, additional changes potentially involving additional rulemaking to change the wording in Part 50 to allow the licensees to select an alternate pipe size as their pipe size for the design basis subject to some level of NRC approval. So that's a potential carrot out there if this can be done.

MS. DROUIN: So far what we've covered is what our plans are in both the short term, what we're considering in both the short term and the long term for 50.46. In terms of just the global look at Option 3 activities and some things that are on the plate, we had talked about, you know, the ECCS reliability. That gives directly to GDC 35 that talks about the single failure criteria.

Right now what is proposed in the short term is just fixing that as it pertains to the ECCS, but as you look at Appendix A, there are a lot of other GDCs that also address the single failure criterion.

So one of the things that we are considering that do think that it is a generic change to the single failure criterion to take it beyond just as it applies to ECCS.

Another Option 3 activity also that we had started was looking at the special treatment, at the technical requirements of the special treatment. Now, when we mentioned this, we mentioned this in terms of Option 3. Option 2 is moving forward, and that is looking at the scope of your components, your structure systems and components that are under the special treatment.

But if you look at the actual requirement, for example, your QA requirements, are those the correct requirements when you start bringing in risk information?

So changing the actual technical

requirements is an Option 3, but in both of these in terms of looking generically at the single failure criterion, looking at the special treatment, both of those efforts for right now have been deferred because we want to take our resources and focus them in getting 50.44 and 50.46 through the system first.

So for the next couple of years, that's where our focus is going to be.

DR. POWERS: If I change the reliability of the ECCS systems and risk assessment, is the CDF linearally related to that change?

MS. DROUIN: It is certainly related. Is it linear related?

MR. KURITZKY: Yeah, I would be hard pressed -- that one I'm trying every --

MS. DROUIN: I would be hard pressed to say that.

MR. KURITZKY: -- every core damage sequence in the PRA has ECCS failure in it, and there could be things at risk that may be not --

DR. POWERS: I don't think so. They would just treat those -- all those other things would be constant contribution maybe, nd so all you'd do is just increase the contribution due to LOCAs.

DR. SHACK: But you have a reliability

that depends on the frequency, right? Doesn't that make it non-linear? They're proposing that the reliability essentially be a function of frequency.

DR. POWERS: Maybe that's what they are proposing. I was asking a simpler question. What's the situation now?

MS. DROUIN: I would not say that it's a linear relationship personally.

MR. KURITZKY: Because, first of all, the reliability of the ECS now is different. I mean it's different for a small break LOCA versus a large break LOCA. You have different success criteria. So you already have some disconnect there that, you know, wouldn't tend to lend itself to being linear.

So, yeah, I would have to tend to -- without having actually looked at it, you know, I would guess that it probably wouldn't be unless you somehow couch your definition to account for the differences in success criteria for different cases.

DR. SHACK: Just coming back to a slightly different question, you know, we always make the assessment of the risk impact of these things based on the existing PRA, and if we change this rule, somebody is going to make changes, and that presumably could impact the PRA in ways that you don't -- but when you make this rule, somebody is going to have to make a -- you know, if he wants to change his tech. spec., you will then come back and be able to assess all of the risk implications of changing that tech. spec.

So you really have a second chance to look at all of the risk implications that you're introducing by changing the rules.

MR. KURITZKY: Yeah, I think that's the way to do it because you can't a priori identify every single possible change a plant could make and come up with what would be the impact to risk because it's different for different plants. So I think you need to have some -- the rule change can be made, but then when it goes -- a plant can choose to take advantage of that or a licensee can choose to take advantage of that, but when they want to actually make a change in their plant, then because of that you need to have some, you know, reg. --

DR. BONACA: Oh, so you would require something like that?

MR. KURITZKY: I don't know. We haven't thought that all of the way through, but I mean --

DR. SHACK: But he doesn't have to come back in in 1.174 space. He comes back in now in, you know, I'm following the regulations.

MS. DROUIN: That's right. In 1.174, he doesn't have to meet 1.174. He chooses the alternative rule.

DR. SHACK: But you presumably now -- it comes back again to this threshold of when you can come back and ask him for risk information.

MS. DROUIN: That would be dependent on how we write this and how the reg. guide -- I mean it might not necessarily be --

DR. BONACA: Well, sine it is a plant specific change --

MS. DROUIN: -- that way.

DR. BONACA: -- and since there is plant specific options, wouldn't you want to have an analysis like that?

MS. DROUIN: I'm sorry. I couldn't hear you.

DR. BONACA: It seems -- I mean, this I raised earlier, that plant specific changes and they're based on some risk informed consideration. Wouldn't you want to know that? The point that --

MR. KURITZKY: The total risk impact at that plant.

DR. BONACA: Yes.

DR. SHACK: I would. The question is

whether the regulations --

DR. BONACA: That's right.

MR. KURITZKY: And I think it goes back to it depends on how much -- when we look at it ourselves and start to assess what we think the changes would be and what would be the risk impact, it depends if we feel confident that all other possible changes for the different plants all fall within some band, and that band is acceptable, maybe we're not as concerned. If we can't get that comfortable feeling, you know, then I guess the reg. guide or whatever would have to be worded in such a way that you'd have some kind of way of assessing that.

MR. ROSEN: I think any competent utility PRA practitioner group which was intending to take advantage of the provisions of new rule as a very first step would run a "what if" calculation through their PRA based on the changes that they would make in their modeling and see what the impact is and see in which sequences it's important and make a judgment based on that, very first level judgment.

Do they want to proceed with it? And that would reveal a lot of things to the utility, and I'm sure they'd share them with the staff.

MR. KURITZKY: But the question is if they would be required to share with us. I think that's what Dr. Shack and Dr. Bonaca are getting at.

DR. BONACA: Well, yeah. The reason I was asking is you had on this slide a number of bullets regarding, for example, single failure criteria, and I agree that it may be over burdening in some cases.

It has served well the industry in other cases. I mean, anybody who lives close to a plant knows there are a lot of valving problems at time. Systems are left out by accidents. I mean, there are errors taking place out there.

The fact that you had single failure capabilities oftentimes save the day because you have --

CHAIRMAN APOSTOLAKIS: Well, they are not eliminating them.

DR. BONACA: I understand. I'm only saying that we are asking these questions for that very reason, that you may try to put together a rule in place -- let me finish --

CHAIRMAN APOSTOLAKIS: Yeah, sure.

MS. DROUIN: Okay.

DR. BONACA: -- and then -- and then -- okay.

MS. DROUIN: I'm sorry. I thought you

were --

DR. BONACA: I just was trying to complete the statement. I think the message got there.

MS. DROUIN: Thank you.

Just to wrap up again, to go over our schedule, again, the paper is pre-decisional. We do hope that it will get up to the Commission within the next week. We are requesting a letter from the ACRS.

In looking at our short-term changes that we have proposed, development of the proposed rule in the short term, 12 months from the issuance of the SRM or two months after the technical work is complete, whichever is later. A very important point there.

In terms of performing the technical work, we will not wait for the SRM. We're going to continue proceeding forward. In terms of modifying the current 50.46, we feel we can have that done by July 2002.

In terms of looking at the ECCS reliability requirements, which is the alternative to 50.46, to have that complete by April of 2002.

Looking at the feasibility to complete the feasibility of redefining the large break LOCA, we feel that could take up to three years. A lot of issues involved in that one.

MR. LEITCH: It seems to me that a lot of the benefits that industry could see from this program may be in the longer term options, and I guess I wonder if in the short term options, particularly in the part that you've called A, are there any -- do you think there would be sufficient benefit that there would be any takers? In other words, is there a danger that we are spending a lot of money here to throw a party and nobody will come?

MS. DROUIN: I can't speak for industry, but we do feel there is some benefit there. Otherwise we would not be pursuing it.

MR. KURITZKY: Yeah, I think to keep the reduction at the K level --

MR. LEITCH: I'm talking about for the current generation of plants. I mean --

MR. KURITZKY: Yes.

MS. DROUIN: Yes.

MR. KURITZKY: Under A where we have -- the part on acceptance criteria is not meant to provide unnecessary burden reduction. I mean it may, but that's not what is intended. It's intended to allow other types of cladding materials to be used without having to --

DR. SHACK: You're also not intending to give them a choice.

MR. KURITZKY: Right.

MS. DROUIN: No, no, no, no, that's not true. They will be allowed to stay with what they have.

MR. KURITZKY: But the decay heat one particularly, our understanding is that there could be a fair amount of benefit for some plants under that second part of A.

DR. SHACK: Under the second?

MR. KURITZKY: Yeah, the reduction of the decay heat level.

DR. SHACK: That certainly seemed to get some enthusiasm on Monday.

MR. KURITZKY: Yeah, particular BWR. We heard from the BWR owners group.

MS. DROUIN: But the answer is, yes, we do think there's benefit to be had. And that concludes our presentation unless there are anymore questions.

DR. SHACK: We have a representative of the Westinghouse Owners Group if he'd like to make some comments.

MR. WARD: Come up here or --

DR. KRESS: It would be better if you'd come up.

MR. WARD: Good morning. I'm Lewis Ward.

I'm the Chairman of the Westinghouse Owners Group, large break LOCA redefinition project. I work with Southern Nuclear Operating Company with the Vogtle Plant.

I missed the Monday meeting, but I did get several reports from the occurrence from here from the people who were here Monday.

We did meet yesterday and last night with NEI and some WOG (phonetic) representatives, and we believe that the priority that we just saw is reversed from what we would really like to see.

The large break LOCA redefinition for us has by far the most benefit down the road with plant changes and core changes and analysis changes that we can get some benefit out of fairly immediately and some of them much longer term.

So that's what we want to focus on within the Westinghouse Owners Group. I know that the BWR owners are fishing for the loss of off-site power option with this, and I don't want to take away from that at all, but within the Westinghouse Owners Group, the only thing that I remember are interested in and have put a significant amount of money on the table to support is the LOCA redefinition.

The other two owners groups, B&W and CE, are very interested in that, and we've had presentations here with them also.

So I think it's safe to say where our money is right now if you want to judge from that is on large break LOCA redefinition.

There is a meeting in progress right now down at NEI to lay out the risk informed task force, you know, what they want to present as a strategy, and I'm not there obviously. So I may be speaking a little bit out of turn. They could come back with some different conclusions, but I kind of doubt it.

Presuming this recommendation goes forward and gets Commission approval, our tendency right now is to petition for rulemaking for the redefinition, and we have started that effort already. We have started a draft of a rule with statements of considerations, and our time frame would be to submit that possibly in about six months.

The one technical issue that was presented this morning that we do want to focus some more on is the pipe break frequency. We get some indications that there may need to be more rigorous work done to support a frequency curve, depending on how you want to use that curve, and I think that is pure science.

A pipe break curve ought to be good. If

it's a sound curve, it ought to be good for however you're going to use it, and so we want to meet with the staff, and we're trying to set up a meeting with the staff hopefully early next month to go over some of those issues and see if we can get them at least on the table so that we can get our technical people, Warren Bamford and the other experts in the reactor mechanics group in Westinghouse working on that.

They are already addressing the issues that we know of, and you know, we plan to put together a paper probably in the form of a WCAP (phonetic) that will be part of the technical support behind the petition.

Some of the other issues that came up this morning, just very briefly --

DR. POWERS: Well, could we pursue just a little bit this LOCA redefinition?

MR. WARD: Certainly.

DR. POWERS: I take it what you're coming in -- they had a slide there, ten to the minus fourth, ten to the minus fifth, ten to the minus sixth. You're saying that the frequency of these large break LOCAs is small enough that we don't need to include them in the design basis.

MR. WARD: Yes. Yes, sir, and I think we

need to come to a very, very good consensus, understanding of where the break-off of that curve is.

One of the questions earlier, our pipe sizes are incremental. At least in the Westinghouse fleet, they go from six to eight to ten, up to 14 in two inch increments, and then the next largest size is the RCS. So, you know, it's not a continuous spectrum of pipes in there.

The calculated frequency of the large LOOP breaking double ended guillotine is on the order of ten to the minus ninth or ten to the minus 11th. So if you integrate that curve down, which is not a continuous curve, and you throw in a 14, the total combined frequency of those is more, ten to the minus eight.

DR. POWERS: How do you know these frequencies?

MR. WARD: How do I know that?

DR. POWERS: I mean, I'd like to see this database that gets these frequencies.

MR. WARD: And I'm not a mechanics expert at all. So I --

DR. POWERS: That means there can't be any data at all, right? I mean, if it's ten to the minus 11th, that's a little longer than the age of the universe --

MR. WARD: Right.

DR. POWERS: -- by a factor of ten years or by a factor of ten.

MR. WARD: Right.

DR. POWERS: So what makes you think that this is -- I mean, this must be the products of calculations.

MR. WARD: Yes, that's right.

DR. POWERS: I mean, is this like F equals MA? I mean, these calculations, there's no question about them? They're known absolutely?

MR. WARD: There are questions in there, and I think that's what needs to be agreed to, that all of the known uncertainties and conservatisms --

DR. POWERS: Has there never been in the history of the universe a large pipe break?

MR. WARD: Yes, there have been large pipe breaks.

DR. POWERS: Oh, well, so the frequency is ipso facto wrong.

MR. WARD: There has never been a large pipe break on the type of material in a nuclear reactor of the grade and type and pedigrees that are in nuclear reactors.

DR. WALLIS: There have been through wall cracks.

MR. WARD: Yes. But, for example, one of the things we're wrestling with is not only just the pipe size, but possibly a flow criteria instead of a pipe size criteria. For example, at full pressure, the six inch break is equivalent to about 30 to 40,000 GPM flow. That's a huge crack that would produce that amount of flow, and whether it's a severance of a small pipe or a smaller split in a weld of a larger pipe, you know, it would encompass all of that.

But you know, it may turn out that this curve that we're talking about is a flow based curve instead of a pipe size break, but that to me is one of the key technical pieces that we really need to get nailed down, and we're working with the staff, and we would like to have a meeting fairly soon to at least get on the table all of the issues that we need to address.

And a fair part of our money is allotted to addressing a technical issue in coming up with a common standard.

I guess the next piece is how do you use that. You know, say we did get a rule change, and what we're going to propose in the guide is that we have or in the statements of considerations is basically to put some bounds on how we would like to use the rule and how we would propose that it not be used.

For example, we don't want to degrade containment requirements in any form. We do not want to allow ourselves to even work on throwing out imported ECCS equipment, like the charging safety injection or RHR pumps or accumulators, but there may be some relaxation in the requirements on some of those, in particular accumulators.

Some of the other ECCS equipment, particular room coolers that are there just peripherally for the long-term operation of the ECCS equipment, we may see some relaxation in those, and that's where we get some benefit.

But what we would like to do in this, what we're going to draft is to try to put some bounds on maintaining some mitigation capability, but try to put a bound on what is off limits for future changes.

So I heard your question earlier about are we going to have to come in and ask for a license change on every change or do we have the liberty to run off and do anything we want to, and I don't think you're going to let us do that, for number one.

But I'd like to have some boundaries on what we can do and what we cannot do under 50.59.

DR. POWERS: It seems like a lot of things that you've mentioned specifically, you've said requirements on accumulators, requirements on room coolers, a couple of other things like that, that those can be addressed in risk based without getting into arguments over the pipe break frequency.

MR. WARD: They can be, but they cannot be done across the board.

DR. POWERS: Well, I'd like to just see one of them dealt with. I mean, I'm listening to a performance on risk informing 50.46, and I haven't seen a risk number yet.

Has anybody tried to do these things, to look at how you can adjust success criteria and stuff like that around and see what impact it has on CDF or LERF or some more reasonable FC curve?

You can react, but it seems like that would be a good context to look at these thing because I'll bet you can.

MR. WARD: We probably can. It requires each example that you come up with to go do a detailed analysis to justify that change and then justify a change to your design basis, you know, one piece of equipment at a time, and that does not at all lead to any uniformity within a unit, for example.

We just cut out a cooler, a room cooler, at Vogtle temporarily for a cycle. We based it on deterministic calculations as well as PRA, but in order to keep our design basis consistent between two trains and both units, we're going to go replace a cooler. It's minor, $100,000, but if I had this rule change, that cooler is really inconsequential and doesn't need to be there, and I could go remove all four of them from my design basis and be done with it.

DR. POWERS: Suppose you said you did the risk analysis and said, "Okay. The cooler means nothing." And I could show that based on risk. Couldn't you do it under 1.174?

MR. WARD: I could justify it. I would probably -- I may need to do a 50.92 license change to get it permanently removed from the design basis anyway.

CHAIRMAN APOSTOLAKIS: Bill, when do you think we're going to roughly stop? We're 20 minutes --

DR. SHACK: We're just about finished.

MR. WARD: Yes.

CHAIRMAN APOSTOLAKIS: There's a lot to do this afternoon.

MR. ROSEN: Could I ask one quick question, George?

I'm surprised, Lou, not to hear you express any interest in the replacement of the 1971 ANS decay heat curve with a more updated version. That seems like a simple thing.

MR. WARD: There is interest in that. I believe NEI is planning to -- and I can't speak for NEI again -- but I believe they're planning to petition for a rule just on that part, to go from 71 to 94 fairly soon, a couple of months.

MR. ROSEN: So your silence on that matter doesn't imply that you're not interested in it. You're just letting that be handled by NEI.

MR. WARD: Yes. Now, the LOOP part of it, I've got some serious questions about that, but I'm not going to sit up here and try to undermine interests for other people, you know, with the issues going on in the utility today on the grids and newspaper articles every day on power blackouts in California. The PR end of that bothers me as much as anything, but that will have to be addressed, and I guess the staff has got a plan on how to deal with that.

CHAIRMAN APOSTOLAKIS: Now, you said that you think that the staff's approach places priorities the wrong way.

MR. WARD: Yes, sir.

CHAIRMAN APOSTOLAKIS: But they didn't really say, as far as I recall, that they think Plan A is more important than B. They just created A and B on the basis of what can be achieved real quick and what can wait a little later.

So it's not a matter of they are thinking that A is more important than B. It's just that they feel that in 12 months they can do certain things, clean up the regulation and so on, and so you disagree with that?

MR. WARD: I don't disagree with that, and I think if the staff wants to work on that, that's fine. I think we would like to not wait for three years or however long the long-term program is to get to the last element.

CHAIRMAN APOSTOLAKIS: They believe it will take that long because there will be issues to be address, is a different issue now. Do you believe that the issues are not such significance or they will not be so time consuming so it can be done in a year and a half?

I mean, that's a different kind of disagreement. It has nothing to do with priorities now.

MR. WARD: Right.

CHAIRMAN APOSTOLAKIS: It's a matter of assessing what it will take to do it.

MR. WARD: I believe the issues on the redefinition should be able to be addressed in the next year if we work on it.

CHAIRMAN APOSTOLAKIS: Mark?

MR. CUNNINGHAM: Just to clarify one point, the staff is not suggesting in the paper that it will take three years to do that. It could take up to three years.

CHAIRMAN APOSTOLAKIS: Up to three years. I was aware of that.

MR. CUNNINGHAM: And the discussions that Mr. Ward alluded to earlier about between us and the staff will have a strong element in defining whether it's one year or two years or three years. So we're not a priori saying that it's going to be three years.

CHAIRMAN APOSTOLAKIS: But it's not really a matter of priorities though. It's a matter of, you know, what can we do immediately and what can we do a little later.

MR. CUNNINGHAM: And the way we think of it is that these are three parallel activities.

CHAIRMAN APOSTOLAKIS: Yeah, that paragraph.

MR. CUNNINGHAM: The work on the large break LOCA redefinition is not being slowed down, if you will, because of a lack of resources, a reallocation of resources to the other parts. I think it's intended to go on, and you couldn't tell it from the presentation, but in the paper, I think it makes clear that the staff is committing substantial resources to the third part of this.

CHAIRMAN APOSTOLAKIS: Very good.

Bill, where are we?

DR. SHACK: We're through at this point unless there are any --

CHAIRMAN APOSTOLAKIS: Thank you.

DR. SHACK: -- further comments or questions.

CHAIRMAN APOSTOLAKIS: Well, I'd like to thank the presenters, and we are 20 minutes behind. I want to remind the committee that we have a large number of letters we have to complete this month. So I'm very sensitive to the timing here.

So we'll break until 10:40, and we'll come back. We're recessed.

(Whereupon, the foregoing matter went off the record at 10:24 a.m. and went back on the record at 10:40 a.m.)

CHAIRMAN APOSTOLAKIS: Our next topic is policy issues related to safeguards, insurance and emergency preparedness regulations at decommissioning nuclear power plants, storing fuel in spent fuel pools. Dr. Kress is the cognizant member.

DR. KRESS: Thank you, Mr. Chairman.

Just to remind you, the staff was considering some generic rulemaking related to decommissioning plants, particularly with respect to the possible requirements for insurance and emergency preparedness and other things, and in order to guide their thinking on this rulemaking, they developed a technical study which we reviewed and wrote a letter on, had some opinions on.

They used this technical study then to identify some policy issues associated with possible rulemaking and rule changes, and it's these policy issues that we're going to hear about today, and the options they've identified to deal with policy issues and what their preferred options are.

We do expect to have a letter on this

because I suspect the Commission will want to know what our opinion is on the policy issues themselves.

So with that, I guess I'll turn it over to Bill Huffman of NRR.

MR. HUFFMAN: Thank you very much.

I am Bill Huffman. I'm a project manager at NRR, and I helped push this paper through, and I'm going to present a series of slides basically that overviews the paper and hopefully give you an opportunity to ask and probe as we go through the presentation.

The very last issue I'm going to turn over to a colleague of mine on the EP area.

Dr. Kress summarized this, and so I don't know if we need to go over it again, but the three areas -- there are certainly a lot of areas that we want to improve in the decommissioning regulations, but the three that have significant burden to the industry that looks amenable to reduction and also directly tied to the zirconium fire issue are insurance, security, and emergency planning.

And there's a long history in trying to fix these, and it's failed several times, and we're going to try again.

We're starting from a technical study that was recently issued, NUREG 1738. It was a technical study. There were findings in the study, but in reality it just reports the risk, but it doesn't make any specific recommendations.

And as the project manager dealing with that study and trying to develop rulemaking, we felt that we needed some further clarification and direction from the Commission because some of these areas are new and unique in rulemaking, especially an application of risk informing.

And so we have the technical study. The risk is very low. It meets the quantitative health objectives and safety goals. If you agree that the safety goals are applicable to decommissioning plants with fuel stored in a spent fuel pool, then it develops a methodology that you can apply, use reg. guide 1.174.

And it also concludes that taking a thermal hydraulic approach to eliminating or precluding the possibility of a zirc fire is difficult to do, and therefore, we do have to look and see if maybe the risk argument is the best approach.

MR. LEITCH: Is the operative way there generic? If you went plant by plant, could you define a time?

MR. HUFFMAN: The study says generic, but if there's someone who wants to take a gander at that, I don't believe a plant specific thermal hydraulic analysis would be any easier because the problem is the configuration after these dominant events, like a seismic event or some other event that might rupture a spent fuel pool or drain it rapidly. You just don't know what it's going to be in.

So even on a plant specific basis, in the past when we approved exemptions in these areas, we assumed a normal design basis configuration for the fuel. We assumed that air flow was unobstructed. In some analyses, I think we assumed building ventilation existed.

It's not likely in a seismic event that those conditions are going to exist. So don't know that you could even do it on a plant specific basis.

DR. FORD: Excuse me. I'm new to this group. So I don't know anything at all about background. Can you just tell me very briefly in one sentence or paragraph what physical instances go into the first bullet, what physical processes? What happens there, a leak in the pool or what?

MR. HUFFMAN: Well, we have this report that looks at various accident scenarios, looks at slow drain-downs, evaporations.

DR. FORD: Okay.

MR. HUFFMAN: Those take hundreds of hours. There really has to be a breakdown in the safety culture of the plant to have those kind of things occur without mitigation.

What you're looking at is a very, very low frequency event, such as a -- in fact, the dominant event that the report concludes would cause this rapid drain-down of the pool would be a very large, low frequency, seismic event.

DR. FORD: So it would be seismic events primarily?

MR. HUFFMAN: Yeah. The second dominant event, although there's some industry disagreement with that, might be a cask drop, a 100 ton cask drop moving spent fuel.

DR. FORD: Okay.

MR. HUFFMAN: It might punch a hole.

DR. FORD: Okay. Thank you.

DR. WALLIS: And your assumption is that if this happens, a fire happens.

MR. HUFFMAN: That's the assumption.

DR. WALLIS: -- is one

MR. HUFFMAN: There is no conditional --

yeah, your answer is correct. It's one. There's reason to believe that there can be mitigated actions taken, that it probably wouldn't be one, but the assumption was that if the pool is drained, the fire occurs.

MR. ROSEN: Now, when you say spent fuel pool accident risk is low, do you have a number on that?

MR. HUFFMAN: We have a bounding number, again, pretty much dominated by the seismic frequencies of the plant locations, but it's on the order of two to three times ten to the minus sixth, is the upper limit. It's less than that.

DR. POWERS: There have been a number of instances within the zirconium production industry of zirconium fires, spontaneous fires. Have you looked at those at all?

MR. HUFFMAN: The technical group did look at that. George, do you want to answer that?

MR. HUBBARD: George Hubbard from the Plant Systems Branch.

The individual who looked at the thermal hydraulics in NRR and also in research, they did look at the information available regarding fuel fines for the zirconium fines, and I believe, you know, that is addressed in there.

I know we did take a look at it in the 1738.

MR. HUFFMAN: Okay. I'm going to proceed on. One clarification -- oh here.

MR. GILLESPIE: Frank Gillespie from NRR.

In the report there were really two conditions to get to the zirconium fire which were listed. One was a drain-down of the pool, but then you had to violate what you already said, and that's our previous assumption of the design basis configuration.

So, one, the pool has to be drained down, and, two, the fuel has to somehow be reconfigured to allow the heat build-up that you need to start the zirconium fire.

MR. HUFFMAN: It's not necessarily reconfiguration. For instance, if the roof of the spent fuel pool fell and capped the top of the spent fuel pool and prevented air flow circulation.

MR. GILLESPIE: Going out in the longer term, the report assumed an adiabatic situation.

Tim.

PARTICIPANT: Well, we did both.

MR. GILLESPIE: Okay. Because a simple

drain-down is not going to lead to one for one to a fire. That's all I'm saying.

PARTICIPANT: Right. We looked at a situation where there was 80 there to heat up, and we also looked at a situation where there was full air cooling, and even with full air cooling for short decay times, you can have a fire situation. It depends on the amount of decay heat that was still present.

So there are situations. Even with the normal channels available for air flow, if it's soon enough after the last off-load, you could get to a fire temperature.

DR. WALLIS: So I think you're wrong, Frank.

MR. GILLESPIE: Well, when I think of five to ten years after fuel load --

DR. WALLIS: Oh, yes. Maybe that's the case.

MR. GILLESPIE: And that's what I mean. We're in decommissioning space, and the question implied that you could have a fire going out to infinity, and that's really the problem we're trying to deal with, is this finding that we cannot preclude the fire ever.

DR. WALLIS: But no one is smart enough to tell where the time is, where the scenario just described stops.

MR. GILLESPIE: Yeah, right. But it becomes more complicated as it goes out.

CHAIRMAN APOSTOLAKIS: Would you go back to Slide 2? Would you explain a little bit the third bullet?

MR. HUFFMAN: All right. In the report -- and then I might let Bob Palla (phonetic) speak a little bit more detail on this, but in the report they applied the principles of the methodologies in reg. guide 1.174 to EP. They looked at what would happen if you removed or significantly reduced off-site EP shortly after a reactor permanently shut down. They chose 60 days as the threshold where you don't have to worry about decay gap release of iodine.

So they did a study with EP in effect, without EP, early evacuation, late evacuation, and they showed that there is a small increase in risk in terms of early fatalities, but it meets the criteria of reg. guide 1.174.

DR. WALLIS: Does normally 174 refer to fatalities or does it refer to LERF?

MR. HUFFMAN: It doesn't, but they

quantified it by the -- I may be digging myself in a hole. They turned it into a probabilistic quantification.

CHAIRMAN APOSTOLAKIS: You converted the delta CDF.

DR. KRESS: They have a fraction of LERF that's permissible, and so they say, "Well, we'll have a fraction of change in early deaths," which is basically equivalent application of it.

MR. HUFFMAN: Okay. The first policy issue is a pretty simple one, but we had to ask it because we're relying in the report, the technical study, saying that it complies with the safety goals, but the safety goal policy statement doesn't address a decommissioning reactor. It talks only about operating reactors and core damage.

So simply we wanted to clarify with the Commission that, you know, we can use the safety goal policy statements and the quantifications for this application.

DR. WALLIS: These are the safety goals that one strives for, but never hopes to achieve or are these the safe goals one actually enforces?

MR. HUFFMAN: I'm not sure I understand what you mean.

DR. WALLIS: Well, I keep being told that safety goals are things that we should strive for, but not go beyond, and if we don't quite get there, it's because we're trying to get there eventually. There's no kind of assumption that you ought to get there.

And so I'm just asking the question again that I always ask. Are they things you try to get? Are these the safety goals you actually use to say you've got to get there or are they something which is out there as an ideal?

MR. HUFFMAN: Well, again, the safety goals are qualitative statements on cancers and on early fatalities. At a decommissioning plant especially, if you get there, you've got some serious problem, but --

DR. WALLIS: So you're saying you must not step over these goals.

MR. HUFFMAN: Well, you would hope that the only accident we're talking about here is a zirc fire. You would hope that you would never even come close to challenging it, but for making risk informed decision making, again, going back to reg. guide 1.174, which is an implementation of this long train of logic, you know, ultimately its roots lie in the safety goals.

So we just wanted to get a blessing from the Commission that it was okay to use those safety goals, but we visualize that there will be other regulatory areas just besides these where we may want to apply reg. guide 1.174 in making our decisions.

DR. WALLIS: Well, what you're really meaning is it should be used on 174.

DR. KRESS: Well, it's hard to use 1.174 since it deals with CDF and LERF.

MR. HUFFMAN: Yes.

DR. WALLIS: Just explain how you --

CHAIRMAN APOSTOLAKIS: Well, we just derive delta risk though.

DR. KRESS: Yeah. So they use the concepts in 1.174. Plus 1.174 calls for other things like defense in depth and margins and compliance with other rules and things of that nature, and I think you tried to apply that thinking also.

MR. HUFFMAN: Yes.

CHAIRMAN APOSTOLAKIS: You would have a problem with the monitoring, particularly with emergency planning, how you monitor.

So your recommendation is one?

MR. HUFFMAN: Yes, do it.

DR. KRESS: Before you leave that slide,

your rationale for saying, yeah, go out and use the safety goals is because the consequences from a zirconium fire could be just as bad as --

MR. HUFFMAN: the consequences are similar to a core damage accident with a large release or can be worse if you're talking about multiple cores.

DR. KRESS: Okay.

MR. ROSEN: It seems to me you're asking the wrong question about this. Is there any evidence that the Commission didn't intend safety goals and the safety goal policy statement to apply to the whole life cycle?

MR. HUFFMAN: Yes. It says specifically, and I thought we stated so in the paper, that it doesn't apply to anything other than -- we've quoted things out with the safety goal policy statement. It's clear it doesn't apply to fuel cycle areas. Just operating reactors.

MR. ROSEN: it applies to operating reactors only.

MR. HUFFMAN: Yes.

MR. ROSEN: And it says that very clearly.

MR. HUFFMAN: Yes.

MR. ROSEN: It doesn't apply to decommissioned reactors.

MR. HUFFMAN: Yeah, it doesn't mention decommissioned reactors, and in reality most people would probably say a decommissioned reactor, if it weren't for the zirc fire issue, is more of a material site rather than a reactor.

DR. WALLIS: So these goals could be applied to a pool which is connected to an operating reactor, but as soon as you shut down the reactor and decommission it, it becomes something else?

MR. HUFFMAN: The goals are applied to the entity of the reactor operating. It's my interpretation. I didn't look at it before I got involved in this paper. I'm not an expert here, but it's my interpretation based on reading it that it was pretty clearly applying the core damage of an operating reactor.

I don't think -- again, this is speculation, but no one visualized that fuel would be kept in these reactors for eternity. They'd be moved off site to Yucca Mountain by now.

DR. KRESS: And the safety goals also explicitly excludes issues of safeguards.

MR. HUFFMAN: Yes, it does. It does explicitly exclude safeguards considerations. That's a good point.

Speaking of safeguards considerations, when SRM from the Commission asked us to try to resolve this zirc fire issue a couple of years ago, you know, using a risk informed approach, one of the things that probably wasn't thought out real thoroughly at the time was safeguards, and it really turns out that it's difficult, if not -- it's just not within the state of the art right now to characterize the probability of an adversarial attack on a site, the probability of radiological sabotage.

There are limited, I guess, methods used once you assume the attack takes place that might be able to characterize the probability of success, probability of interdiction, but the initiating event we just don't have a handle on, and therefore, a lot of --

DR. KRESS: Is it so much that the methods don't exist or the database?

MR. HUFFMAN: Does someone from NMSS want to take a shot at that?

MS. WARREN: There are many databases available to us, including ones that we have available to us here in the Commission that involve the intelligence community. However, it's the methodology that at this time does not exist.

You know, we have looked into that area very thoroughly, and the state of the art is that we don't use this type of method in determining probability for these type of events.

DR. WALLIS: Well, the problem is that it's not based on science. It's based on human behavior, and I can think of historical events and changes in society which would make the likelihood of sabotage either minute or much larger than today.

MS. WARREN: Well, we certainly have all of this type of activity under continuous evaluation.

I'm Roberta Warren. I'm the team leader for our threat assessment team, and I think we've spoken before to your group about, you know, the program that we have in place for threat assessment.

MR. HUFFMAN: So what motivated us to ask this policy question is, well, we can try again. It's going to be a deep hole, throwing a lot of resources and money and effort into, and there's no guarantee to success. That's option one.

We can just give the Commission an update, a kind of a paper as to where the state of the art is now without actually committing to try to do it, or we can continue our current practice and with an open mind as, you know, the PRA methodology is developed.

If something comes up, we'll revisit this, but our recommendation is Option 3.

DR. KRESS: Well, what then is your judgment about the associated risk?

MR. HUFFMAN: Well, I was trying to articulate that in the paper, and our judgment is the risk is kept low. Hopefully it's not greater than the risk of these other dominant events, but the risk is kept low by a rigorous, deterministic program for safeguards.

DR. KRESS: Is that an assumption or do you have some way of validating that?

MR. HUFFMAN: Well, we do have things like OSRES, but I don't want to get any further than that. I, again, would like somebody from our safeguards group or NMSS if they want to. You know, OSRES tests the capability of plants and able to defend against DBTs, other inspections, assessments.

MR. TRACEY: Bill, Glenn Tracey.

OSRES are at operating reactors, however, not decommissioning sites.

MR. HUFFMAN: Right.

MR. TRACEY: And we don't conduct OSRES at those sites, and essentially you have safeguards in place at decommissioning facilities. Of course, the next policy question is what level of protection and what level of safeguards should be maintained, and Bill will get to that in a second.

MR. HUFFMAN: Well, I think the gist to your question was, well, if there's so much uncertainty, you know, how do we know that the frequency of these events and the risk isn't greater than the others, and I don't think I can say.

DR. KRESS: Well, I guess my question is if you want to try to decide on what level of safeguards is necessary and even what level of emergency preparedness is necessary, it would be predicated on what level of risk you're trying to protect against.

And my question was what do we know about that risk and do we know enough to make judgments on those things.

MR. HUFFMAN: You know, that exists in all PRAs. I guess most PRAs, the expert judgment would be that the risk of sabotage is in the grass compared to the other dominant events.

MR. GILLESPIE: Bill, let me help you out here.

MR. HUFFMAN: Yeah.

MR. GILLESPIE: Twice a year Bobby's staff actually prepares a briefing, and Mike Webber I saw here earlier, prepares a briefing for the Commission which addresses the design basis threat and what they feel the credible threat is. And that goes in every six months, the deterministic view of what it is is revisited by the Commission.

So her staff does do that, and I don't want to get into any more detail because all of a sudden I'll creep into something that's classified.

DR. KRESS: I have the same problem. You know, we have a set of design basis accidents for operating reactors, and then when I ask myself, well, what does that -- being able to meet those design basis accidents by the design and operation, what does that mean in terms of the risk status of the plant?

And nobody can ever answer that unless they go and say, "Well, we'll do a PRA then."

And then you do the PRA, and it tells you the risk status, and it tells you something about the correlation between those design basis events and the risk status, but here all we have is the design basis event. We don't have a PRA.

So I don't know how to make that correlation. How do I know that the design basis threat and being able to meet it results in an acceptable safeguards risk status with respect to decommissioning plants?

I don't know how to make that jump, and I was wondering how you guys are making it. That's the nature of my question.

MR. ROSEN: And the other side of that is how do we know that the resources we're committing both at the staff and at the utilities is in any way commensurate with the risk.

DR. KRESS: Yeah. I don't know.

MR. ROSEN: It may be way too much.

DR. KRESS: It may be too much. It may very well be too much.

MR. ROSEN: Not too little.

CHAIRMAN APOSTOLAKIS: At the same time though the problem here is not of the same nature as risk due to accidents. I'm not even sure that the policy issue is phrased the appropriate way. Using probabilistic risk assessments for quantifying the likelihood of sabotage, I mean, here you have somebody who's intentionally trying to damage, and PRA doesn't deal with that. PRA deals with accidental failures.

There are other ways of attacking this. So I'm not even sure that the whole question is meaningful, although I do agree with you guys that some sense of what are the numbers we're talking about is needed.

Also, I'm bothered by the loose use of language like assessing likelihood in a qualitative manner. I really don't know what that means, although I understand what you're trying to say, but I mean the language is not the right one.

So, I mean, you will never attempt to develop PRA methods to estimate the likelihood of sabotage because it's a different thing, a different question. You know, you are playing games now.

DR. KRESS: That's like an initiating event frequency. You don't use the PRA.

CHAIRMAN APOSTOLAKIS: It's not initiating. I mean here the guy is some smart fellow who's trying to do damage.

DR. KRESS: That's an initiating event.

CHAIRMAN APOSTOLAKIS: Yeah, but it's not like pipe break.

DR. KRESS: Well, it's not stochastic.

DR. POWERS: It could be exactly a pipe break.

MR. HUFFMAN: I guess what we were trying to get at was that there may be vulnerabilities that could cause -- you know, responding to Mr. Ford's question, you know, how do you rapidly drain down the pool? That's one of the ways to do it. And that's an incompleteness in the study.

DR. WALLIS: Or how you change the geometry that you were talking about.

MR. HUFFMAN: That would do it.

CHAIRMAN APOSTOLAKIS: I think you still have the problem of how likely is it that somebody would try to do it.

MR. HUFFMAN: That's the answer we can't --

CHAIRMAN APOSTOLAKIS: Maybe we can separate that.

MR. HUFFMAN: That's a question we can't answer.

DR. WALLIS: Well, obviously if you took the fuel away, which is the sensible thing to do, the problem would disappear.

(Laughter.)

MR. HUFFMAN: That's one way to solve the problem, is to put it in the dry cask storage.

MR. ROSEN: I think you're formulating the problem in a manner that makes it too hard to solve. You're talking about trying to use PRA to assess the likelihood of an initiating event, and I don't think that's something we could touch.

But, on the other hand, you could assess the likelihood of the mitigation of an event with the systems that remain in operation, successful mitigation of a radiological sabotage event with the systems that remain in operation in decommissioning plants, and PRA would be a tool that could potentially be used for --

DR. KRESS: As a function of the level of the threat.

CHAIRMAN APOSTOLAKIS: But you would not be addressing the question of quantification of the likelihood of sabotage.

MR. ROSEN: Of successful sabotage because you wouldn't have a number for the initiating event. You'd only have a number for the mitigation potential.

CHAIRMAN APOSTOLAKIS: But is Option 3 using this kind of input from PRA right now?

MR. HUFFMAN: No. Option 3 is not. Option 3 is the status quo.

CHAIRMAN APOSTOLAKIS: Why nothing? I mean, it makes sense to me.

MR. ROSEN: Well, it makes perfect sense to me. I mean if we were willing to talk about conditional containment probability, conditional core damage probability given an initiating event yesterday at some of the subcommittee meetings and we argue about whether it should be one or some number less than one, it seems to me you could apply exactly the same logic here.

Forget about the initiating event. We know so little about it. It's not under our control anyway, and how robust is the existing decommissioning plant and its systems and facilities and its people and its management against an initiating event?

CHAIRMAN APOSTOLAKIS: And then perhaps a third observation there, that it would take substantial resources to develop these methods, would not be applicable, right?

That's 34 PRA, isn't it?

MR. ROSEN: I think it is.

CHAIRMAN APOSTOLAKIS: In a much simpler system.

MR. ROSEN: Yeah, and applied to a very simple system compared to what we now apply PRAs to, which are much more complex than this.

DR. KRESS: Well, it's more comparable to the Level II, where you don't have the phenomenological things to go into your model.

MR. ROSEN: Well, you have a spent fuel

pool, and you have fuel in it. Now, that's what you're trying to protect, and you have cooling to it, and with air conditioning, and so you look at all of those kinds of systems and see how robust.

DR. BONACA: What's the complexity involved in developing a PRA methodology? I don't understand the methodology word. The methodology is there. It's more like --

CHAIRMAN APOSTOLAKIS: I think it's more like two. Evaluate.

DR. BONACA: Yeah.

CHAIRMAN APOSTOLAKIS: The methodology you have now to identify its uses in the sabotage question, which is, I think, what Steve and Tom just told us. There are places where you can use it. So two would seem to be a reasonable option.

MR. ROSEN: It won't give you the whole answer, George, but it will give you some of the answer, and that's better than nothing.

CHAIRMAN APOSTOLAKIS: Not after a complete answer ever.

MR. GILLESPIE: George, the staff has actually been grappling with this for several years, and we're kind of doing what you're saying. Let me just go to operating reactors and your OSRE program, and one of the major questions we had when we ran exercise is if you run four scenarios and you fail one out of four, does that mean you fail security? If you fail two out of four?

So conceptually in the OSRE program already, in fact, in the significancy determination process for evaluating drills is the concept of we're not striving for perfection; we're striving for reasonable protection, and there is a consideration that if you fail one out of three scenarios, did you fail it because of a programmatic failure that could be repetitive.

And those kinds of questions are built into the current significancy determination process so that the risk concepts that you're talking about are there in the security area, but they're not articulated in mathematical terms.

CHAIRMAN APOSTOLAKIS: But I repeat. I think the language in this slide is not the right one, and I think what you just said supports that.

MR. GILLESPIE: Yeah, the potential, the probability of success is, in fact, right now a qualitative consideration in what's being done in security, and in fact, DOE has computer programs that actually do that mathematically for them when they run similar exercises.

CHAIRMAN APOSTOLAKIS: But even in the significance determination process, when you decide it's a green, I mean there is some sort of quantitative evaluation, although you're not saying it's 1.2, ten to the minus four or five or whatever.

MR. GILLESPIE: Oh, yeah.

CHAIRMAN APOSTOLAKIS: And there is some element --

MR. GILLESPIE: For operating reactors it's a little easier.

CHAIRMAN APOSTOLAKIS: It's a qualitative assessment of a likelihood, right?

MR. GILLESPIE: Yeah, that's what it really is, and in the second paper, which unfortunately we haven't gotten to you, which is the safeguards addendum kind of to this paper, it does talk about the consideration in safeguards terms of systems and walls and how much reinforcement bar is in and what's the likelihood of the design basis threat doing something to the systems that are inherently there already and how you might go about looking at that.

CHAIRMAN APOSTOLAKIS: But what's wrong with proposing, recommending Option 2? I mean, it seems to me from the whole discussion that this is really -- and some of the evaluation has already been done because you're already using the methodologies, right?

MR. GILLESPIE: I think what you're doing, you've switched. This was written really, and you got it in a different -- the context of this was in the return frequency of the event, in the likelihood of what armament that event would pertain to. So you have to understand these words and the idea of how they were written, and they were really written in the context of return frequency of a particular threat, and what is the size of that threat.

So it wasn't written in the idea of coming up with the probability of success on a conditional level, which we're kind of doing already.

CHAIRMAN APOSTOLAKIS: But the way it's written now, if I take it literally, it says for assessing sabotage, and that probably is out of the question. But if you say, "Evaluate current state of the RPRA methodologies," or "evaluate the role that current state of the art RPRA methodologies can play in this issue of sabotage," then it's a different story, isn't it? Because then the role is clear, what is conditional on the threat.

MR. GILLESPIE: Yeah, and that is --

CHAIRMAN APOSTOLAKIS: And you're doing some of it anyway.

MR. GILLESPIE: We're kind of doing it qualitatively right now, and DOE does it in a very -- they have handbooks that say if a soldier runs X number of yards carrying a weapon loaded so much, the likelihood of his shot being successful is this percent. They have it down -- yeah, they have handbooks down to that level of detail.

I don't know that we want to get there.

CHAIRMAN APOSTOLAKIS: That's not what I had in mind.

MR. GILLESPIE: No, I didn't have that in mind.

MR. ROSEN: I have the feeling I'm not being understood. What I'm talking about is using PRA in almost a traditional way to talk about how robust are the mitigating systems if someone were to take and break a pipe somehow. We're not talking about whether it's a soldier or with explosives or whatever. Forget about all of that. Just assume the pipe is broken and how robust is this system. Is it two trains, three trains? How big a pipe do you need to break?

CHAIRMAN APOSTOLAKIS: I understand that.

MR. ROSEN: It's like use the PRA methodologies we now use to assess the capability of the decommissioning plant to mitigate potential sabotage, and forget about -- forget completely about the probability of --

DR. KRESS: They actually did that in the technical study.

MR. HUFFMAN: I guess this goes a little bit beyond that though, and I don't want to get into any safeguards information, but just say theoretically that you could put a very large hole in the bottom of a spent fuel pool. There are no mitigating systems for that.

DR. KRESS: It's going to go.

MR. HUFFMAN: There's no mitigating system.

MR. ROSEN: That's one of the events that you say is going to go --

DR. BONACA: But I think that's the issue.

MR. HUFFMAN: Yeah.

DR. BONACA: If I understand it, on the mitigation side there isn't every much.

MR. HUFFMAN: We're not talking about pool cooling here.

DR. BONACA: On the initiation side it's

a very complex issue, the different sequences you have, potential initiator of sabotage that is difficult to estimate, and that's really probably what the complexity is.

MR. HUFFMAN: Absolutely.

DR. BONACA: That's what it is, and there isn't much you can describe as a mitigation.

CHAIRMAN APOSTOLAKIS: But still you would not be developing PRA methods for estimating the likelihood of sabotage. You would be doing something else because this is not accidental anymore.

MR. HUFFMAN: No.

CHAIRMAN APOSTOLAKIS: A different story.

MR. HUFFMAN: Okay. So you agree that number one would be a waste of time.

CHAIRMAN APOSTOLAKIS: Number one would be a waste. Wait a minute.

(Laughter.)

CHAIRMAN APOSTOLAKIS: "Commit resources."

DR. WALLIS: Are you saying this is something like aircraft sabotage?

CHAIRMAN APOSTOLAKIS: I think it's not phrased well.

DR. WALLIS: I mean it doesn't make sense to do a PRA to study what happens to the aircraft if the bomb explodes in it.

MR. HUFFMAN: I would agree. That would be a similar analogy.

CHAIRMAN APOSTOLAKIS: I guess what you're saying, Steve, would apply more to the reactor.

MR. ROSEN: No, I don't agree. I don't agree to that. It seems to me that number two, if you constrain it and phrase it properly, constrain it properly, then you can get some sense of the relative robustness of the barriers to eventual successful radiological sabotage, absent discussion of the initiating event.

Now, I recognize, and I think I agree with Bill Shack, that you can envision an initiating event which no existing mitigating systems can do anything with, but that's true in the reactor, too.

DR. BONACA: Well, I think what we were saying is that whole PRA here would be driven by a number of initiating events which dominate the risk

MR. ROSEN: Right.

DR. BONACA: And the mitigation portion is probably not a very complex thing because there isn't much that you can do, even initiate it to deal with it, but are limited so that you are still missing most of the insights that you expect from the PRA, which are coming from in this particular case from initiators that you can't quantify.

MR. ROSEN: If you don't do the work and you don't put some people who know how to do PRA to work on this thinking about it, you'll never get the insights that would allow you to assess the capability and perhaps enhance the capability of the mitigating systems. You're simply foregoing any of that.

So my feeling is a properly structured Option 2 would be of some value.

DR. KRESS: Looking for vulnerabilities maybe.

CHAIRMAN APOSTOLAKIS: I agree.

DR. KRESS: That you can detect.

CHAIRMAN APOSTOLAKIS: I think Option 2 is rephrased.

MR. ROSEN: Yes.

DR. WALLIS: So design changes could make a difference. I mean you could probably require, if it were desirable, that the fuel should be stacked in a way so that if the pool drains it cannot catch fire.

MR. ROSEN: Well, that's one possibility.

DR. WALLIS: That's one possibility as an extreme measure.

CHAIRMAN APOSTOLAKIS: No, but I think

Steve is right. Unless you look --

DR. WALLIS: There are physical things you can do to make a big difference to the probability of a major accident.

DR. BONACA: I understand from Mr. Gillespie that, hey, look.

CHAIRMAN APOSTOLAKIS: That isn't what the --

DR. BONACA: Yeah, I understand. I understand.

CHAIRMAN APOSTOLAKIS: I think we -- policy issue three.

MR. HUFFMAN: Thank you.

All right. That question goes well beyond decommissioning. We just decided to tackle it because we are trying to address the more specific issue of how do we address what safeguards requirements are appropriate for the spent fuel pool.

And we have three options. The slides are on this. The third opinion is on the next page, but Option 1 would be essentially the requirements that now really exist at a decommissioning plant because it is still a Part 50 licensee, which should be the operating reactor requirements, you know, with the full response force, you know, being able to interdict the DBT.

It seems excessive. Certainly if it's good enough for an operating plant, there can be little argument that it's good enough for a decommissioning spent fuel pool, but that's one of the options.

Option 2 is something that you may or may not be familiar with since it went in parallel with this paper, is staff's proposed rule changes to 73.55 to make it less prescriptive, more performance based, and in addition, put a performance standard associated with it.

This is basically look at where the DBT can do damage and see what you have to do to prevent that damage from occurring, you know, such that you don't exceed radiation doses at controlled area boundaries that exceed five rem. In a spent fuel pool situation, the only situation that's going to get you to that radiation level is a zirc fire.

So if, for instance, licensees were given Option 2, you know, depending on the plant specific configuration in the fuel pool, say it's a seven foot thick pool, high seismic area, a lot of rebar buried in the ground. It's probably not too difficult for them to demonstrate that you can't poke a hole in it to drain it. And they may have to do a very minimal amount to be able to demonstrate that they can meet that criteria.

DR. KRESS: Would the design basis threat be the same one you use --

MR. HUFFMAN: Yes.

DR. KRESS: -- in operating the reactor?

MR. HUFFMAN: Yes, the same design basis threat, a slightly different approach, and you don't have to interdict it. You don't have to stop them at the fence. You don't have to worry about defending something unimportant.

DR. WALLIS: Aren't you coming around to Steve's argument? There are a lot of things you can do with design to change the problem.

MR. HUFFMAN: Absolutely. But, again, we're going to use judgment, engineering, and perhaps PRA can play a role here, but this is a broad based recommendation right now. You know, the details, reg. guides, how we figure that out in regulatory space is not what we intended to do.

DR. WALLIS: Proposing to redesign fuel pools in some way or modify existing fuel pools in some way then?

MR. HUFFMAN: That would not be our

recommendation, but that would be an option. If they wanted to build a cage around the pool, and believe me, this is not what we're recommending, but pursuing your thought, it might make it impossible to put an explosive charge close to the wall, and that's cheaper than maintaining additional guards 24 hours a day, 365 days a year.

DR. WALLIS: This is the option you're --

MR. HUFFMAN: That would be an option to take.

DR. WALLIS: -- recommending.

MR. HUFFMAN: Yes.

DR. WALLIS: Is to make the thing impregnable or whatever you want to call it or robust, robust.

MR. GILLESPIE: Graham, what we're offering is a performance criteria that leaves to the licensee the method.

MR. HUFFMAN: The licensees would make that decision, right.

MR. GILLESPIE: So if they want to trade off design features for manpower costs, that should be their economic decision, not ours. So this is a rule that would set a limit at the site boundary, and the licensee figures out how to do it.

And this also brings into harmony with Part 72.

DR. WALLIS: Are there licensees who think this is feasible?

MR. HUFFMAN: We'll find out within a --

DR. WALLIS: Well, I mean, it doesn't seem to make much sense to suggest an option that if it's not feasible.

MR. HUFFMAN: I agree. We would not want to propose a rule if nobody was going to be able to utilize it to reduce burden, if it made no sense.

DR. KRESS: As time went on in decommissioning, presumably the probability of a zirconium fire gets less, although you know there's some question about that. Would the level of protection then be variable with time rather than a fixed something?

MR. HUFFMAN: Does Glenn Tracey -- would you allow me to speculate on this?

MR. TRACEY: Yes, speculate.

MR. HUFFMAN: Okay. Again, since this is a performance based rule, mitigation time is certainly a consideration in the argument a licensee may make. If you're Humboldt Bay and you've been shut down for 30 years and it would take even in an adiabatic thermos, you know, weeks to get there, you could certainly argue; a licensee, I think, could make a pretty darn valid argument that they could do something ad hoc to mitigate the results of the sabotage.

DR. WALLIS: Enclose it in a block of ice 100 foot thick and it slowly melts away.

MR. HUFFMAN: Or in Humboldt Bay's case, the water will probably leak in anyway.

(Laughter.)

MR. HUFFMAN: So maybe that was a bad choice. Anyway, I think that's what we're getting at.

DR. WALLIS: Well, seriously, is this something that's going to find some takers?

MR. HUFFMAN: I can't answer you right now.

MR. TRACEY: It's very consistent with current Part 72, which NMSS has pools and also has dry casks. So since it's consistent with what the industry is practicing from the materials side, I would think it will have takers.

MR. HUFFMAN: There is the third option, and the third option is to apply what we currently apply at dry cask storage storage facilities.

DR. SHACK: How is it different? My

criteria seems to be the same.

MR. HUFFMAN: Dry cask storage, the facilities aren't required to meet the DBT. So it's significantly different. That's why it's a policy decision.

DR. POWERS: It seems to me that that's not an exactly equivalent option, is it?

MR. HUFFMAN: Well, I don't think that a dry cask is equivalent to a spent fuel pool. So, right, I don't, and I hate to confuse the issue, but there is one wet storage facility out there, G.E. Morris, and the same regulations apply there.

DR. POWERS: So all you're saying is we may have made a mistake.

MR. HUFFMAN: May have, but that's a policy decision that the Commission has to make.

DR. POWERS: Well, it seems to me that in thinking about Option 3, you need to be very clear that dry casks, a little bit of the fuel per cask; spent fuel pools are a whole bunches.

MR. HUFFMAN: I agree that they're apples and oranges.

MR. GILLESPIE: Also, Dana, it's not quite as clear cut as even it was just made to sound. Within Part 72, if you have a dry cask storage under a general license at a reactor, the design basis threat does apply, but if you have a specific license, it doesn't. So there's even an inconsistency there within the same rule.

So there's some clean-up here that needs to take place.

DR. WALLIS: Well, I'm concerned about this really performance. Some design that assures no fuel damage that exceeds off-site dose limits? No idea what it might be?

MR. HUFFMAN: The design -- I hope we're not being thrown off by the word "design," because that includes security plans, and it's not just a physical design. It's the security system.

DR. WALLIS: You're going to trade off. You said you're going to trade off sort of people versus physical things. How do you do that if you can't assess the sort of risk associated with people performance? I don't know how you do the tradeoff in a logical way.

MR. HUFFMAN: Anybody out there want to answer that question?

MR. GILLESPIE: You've got a design basis threat. You could make certain decisions. For example, if putting a hole in the pool drains the pool, but you decide to put -- I don't know -- a spray system of some kind in to put a fog spray over this thing, you could --

DR. WALLIS: For a long time.

MR. GILLESPIE: -- that could become a consideration.

I don't know what the tradeoffs could be. The idea of having a performance criteria was to allow licensees to make that decision and not have the regulator making it for them.

So the system, the design of the system, it's the pool; it's the security system; it's the number of guards; it's how close the LLEA is to come.

If the LLEA can be there in 15 minutes versus 30 minutes, that could make a significant difference, particularly if you have older fuel, which is the point I was making earlier on having to reconfigure the fuel if it's older fuel potentially.

DR. WALLIS: It's amazing. Maybe we should have the same rule for reactors.

MR. HUFFMAN: Well, that's what we're proposing. That rule is for reactors right now proposed, and we're suggesting that --

DR. WALLIS: No fuel damage?

MR. HUFFMAN: No. That's --

MR. GILLESPIE: No core damage.

MR. HUFFMAN: -- no core damage.

MR. TRACEY: No spent fuel sabotage which would exceed five rem, 100 meters. So the 72106 standard.

DR. WALLIS: No damage?

MR. TRACEY: That would exceed.

DR. WALLIS: You're going to get probabilities in there.

MR. HUFFMAN: No damage that exceeds five rem. If you threw acid in the pool, that definitely would damage the fuel, but that's not what we're talking about.

DR. WALLIS: You'd have to talk about probabilities. You can't go with no damage. You're going to have to do a PRA for this new design, whatever it is. You're going to have to do something like a PRA.

MR. HUFFMAN: You would have to have some kind of a justification that demonstrates that it doesn't exceed the performance criteria of five rem at the controlled area boundary.

DR. WALLIS: You can never do nothing. You've got to have a probabilistic measure. You cannot say you'll never have.

MR. GILLESPIE: I think we have to agree. The thought is that it will likely not exceed. We have to have reasonable assurance it won't exceed because the right words may be yet to be selected, but we're looking for reasonable assurance, which gives a sense of we're not dealing in absolutes. We're not looking for the absolute assurance. We agree.

DR. KRESS: I think we need to move on to the next policy issue.

MR. HUFFMAN: Okay. Policy Issue 4 is what do you do about insurance. There's two types of insurance in the facility, and our recommendation would probably be applicable to both, but we're primarily concerned about Price-Anderson, and when do you reduce Price-Anderson insurance coverage from limits and allow these decommissioning facilities out of the secondary retrospective premium?

We looked at it, had a lot of debates, and this is probably the closest you're going to get to a risk based recommendation, number four, because it doesn't directly impact public health and safety, and frequencies are very low, and our recommendation is to allow them out of the insurance limits for operating reactors very shortly after they permanently shut down and off-load.

MR. ROSEN: This is Price-Anderson?

MR. HUFFMAN: Yes, Price-Anderson.

MR. ROSEN: Not 50.54(w)?

MR. HUFFMAN: We would also recommend changes to 50.54(w). We would recommend in the details of this -- again, this is the broad scope for the Commission -- to say, "Okay. We agree that you can use a frequency based argument on this one."

But the details were proposed back to the Commission in '93, SECY 93-127, where we would let them out of the secondary retrospective premium. We would lower the primary limits on Price-Anderson to 100 million, and probably reduce 50.54(w) to around 50 to 25 million.

MR. ROSEN: From its current --

MR. HUFFMAN: From it's current, yeah, exactly.

DR. KRESS: Now, how did you arrive at those particular numbers?

MR. HUFFMAN: We endorsed the same -- there are other things that can cause, you know, exercising of insurance. Even if, you know, somebody thought that there might be an accident and suffered mental anguish, they can sue for insurance.

You could have a tank with radioactive

liquids spill and there would be off-site liability associated there. Ninety-three, one, twenty-seven went into detail what those appropriate limits might be in those situations, and we see nothing in 93-127 that's been superseded, and so we thought those limits were still appropriate.

DR. KRESS: That's where they came from.

MR. HUFFMAN: Yeah.

DR. KRESS: Okay.

MR. ROSEN: Fifty, fifty-four (w) covers property insurance, minimum requirements of property insurance. What you'd be covering is the radiological to clean-up on site, not off site; on site.

MR. HUFFMAN: Right, on site.

MR. ROSEN: Clean-up of a --

MR. HUFFMAN: With a tank spill.

MR. ROSEN: -- tank spill.

MR. HUFFMAN: Right.

MR. ROSEN: Or some other accident that resulted in some radioactive contamination of the fuel handling facility.

MR. HUFFMAN: Yeah, right.

MR. ROSEN: And that would be in the range of 25 to 50 million.

MR. HUFFMAN: Twenty-five to 50 million,

yeah.

MR. ROSEN: That seems consistent.

MR. HUFFMAN: Yeah. That concludes my part. Randy Sullivan from the EP Branch is going to come up here and finish it off.

MR. SULLIVAN: Hi. Thanks. Randy Sullivan from NRR.

I wanted to talk about emergency preparedness, and as it relates to this SECY.

Option Number 5 is to determine policy of what level of emergency preparedness would be appropriate, and the options are fairly simple. It's just a logical step through.

Do little. Do most everything we're doing now, or do something in between. We suggested that the something in between is appropriate. That's Option Number 3.

DR. KRESS: The policy issue stated with the qualifier on it is given the low likelihood of a radiological release large enough to exceed protective action guidelines. That's based on, since it's a likelihood -- is that based just upon the earthquake PRA that they did in a technical study or does that include safeguards?

MR. SULLIVAN: We feel that it envelopes

the safeguards issue. Actually in emergency preparedness space, we feel that that issue is enveloped by the emergency planning that this would propose. So it doesn't matter what the initiator is.

DR. KRESS: You're making a judgment that the safeguards --

MR. SULLIVAN: Yes, we are.

DR. KRESS: -- risk is low enough --

MR. SULLIVAN: Yes, we are.

DR. KRESS: -- that it probably is the same order of magnitude of the earthquake.

MR. SULLIVAN: Well, you know, I'm glad you brought that up actually. It's a little more -- I want to address that in a little more depth. In emergency preparedness, we're not working simply against risk. Our obligation is against defense in depth, you know, as stated in the '86 Commission policy on the safety goals. It reiterates a couple of times that emergency preparedness is an element of defense in depth.

So while we want to risk inform this process, you know, as is appropriate and in accordance with Commission guidance, what we're attempting to do here is maintain the appropriate level of defense in depth.

Now, --

DR. KRESS: So you'd have to answer a question, and that is: what is the appropriate level of defense in depth?

MR. SULLIVAN: Exactly, and we think we can do that in the rulemaking process; that there be a level of emergency preparedness we believe is appropriate, and we have technical arguments for that.

Now, what we would base these decisions on are not only risk, but the four bullets in the middle of the page. There's the physics of the situation. You know, you cannot get a rapidly evolving accident with the spent fuel pool. There is --

DR. KRESS: Even if you rapidly drain the pool.

MR. SULLIVAN: Right.

DR. KRESS: It still takes a long time.

MR. SULLIVAN: Well, even with fairly new fuel, it takes a couple of hours. You know, in a reactor, our emergency plans are poised for immediate action. I mean within 31 minutes.

DR. POWERS: It seems to me that when you make these arguments about it taking a long time, you're presuming that the decay heat is going to be responsible for raising the temperature to the point that you get some rapid reaction. It's not evident to me that that's necessarily true as we move to high burn-up fuels where we have zirconium hydrides inclusions distributed in the clad.

MR. SULLIVAN: Well, we're basing our path forward on 1738. So I'm using the technical analysis.

DR. POWERS: Well, I would hope that we include in that looking at the issues of ignition by spontaneous combustion of zirconium hydrides.

MR. SULLIVAN: As part of the emergency preparedness rulemaking process or as part of the technical --

DR. POWERS: In general.

MR. SULLIVAN: Okay. Certainly.

DR. KRESS: I think there's a significant of zirconium hydrides even without high burn-up fuel in the spent fuel pool fuel.

DR. POWERS: I think most of those, Tom, though -- until you get to high burn-up, those hydrides are distributed. It's only when you start getting inclusions where you have real microscopic quantities that you need to worry about them as ignition source it seems to me.

DR. KRESS: Because they concentrate near the surface of the --

DR. POWERS: They concentrate near various points that are a little lower in temperature, and it's incrementally lower, but they concentrate as nodules, big, and so if you get just a casual fracturing, they get exposed to air.

But what I don't know, and we've not had any discussion from the staff on this, is the pyrophoricity (phonetic). Is the pyrophoricity on these things equivalent to uranium hydride, in which case they'll ignite things, or are they more like magnesium hydride, in which case maybe they won't?

MR. SULLIVAN: Well, that's clearly an issue that's going to have to be dealt with, but we're basing it on the accident as described in 1738, and we're assuming that we've got, even at the very shortest time that we're talking about, you know, less than a year, a number of hours before this accident can begin.

That would allow certain -- that would change the focus of emergency preparedness from the way it is at operating plans in some fundamental ways.

There are some other angles. Potassium iodide wouldn't be relevant, you know, because you don't have iodine. So there is a series of our regulations that --

DR. POWERS: It just wouldn't be radioactive.

MR. SULLIVAN: Right. So there's a series of our emergency preparedness regulations that are obvious that would no longer be applicable.

DR. POWERS: That would be a health benefit to the population?

(Laughter.)

DR. WALLIS: This ten hours doesn't make any sense to me, and suppose that an event happens, and then ten hours later you know there's going to be a fire, and there's going to be a large release of radioactivity, a very large release of radioactivity. Ten hours? What can you do in ten hours that's going to make much difference?

You can get people out of there. You may get them to hunker down or something, but --

MR. SULLIVAN: I don't want to confuse you with what we're proposing here. We're proposing that emergency preparedness in Option 3, what we're proposing to the Commission, is that emergency preparedness be maintained as is at the operating plant for some period of time. That period of time is less than a year, but it will be determined in rulemaking. All right? We don't know what the exact time is.

Then there will be a decrease in requirements commensurate with the physics of the situation, these four bullets in the middle here, the physics of the accident, the ease of mitigation, the ease of protective actions, and the low risk.

MR. ROSEN: And paying attention to Dana's point about hydrides.

MR. SULLIVAN: Yeah, well, apparently, yes.

Now, at some time in the future when the fuel is sufficiently decayed such that the physics of the situation dictates under worst case conditions, minus what you mentioned, at least ten hours, but probably no more than 24 hours to take action we would propose making the EP regulations similar to that for a spent fuel storage facility.

DR. WALLIS: So you're assuming that someone is going to prevent the fire occurring in that ten hours?

MR. SULLIVAN: No, and the basis of this is that we've looked at a study of evacuations in the United States, and in fact, there's an -- this is 1980s data, I'm afraid to tell you, but it was the best data that I could find -- there's an evacuation about every two weeks. They are largely successful, and they are largely ad hoc, and yet, indeed, there are cases where 12,000 people are moved in six hours without a nuclear grade emergency plan to do it.

There are cases when 6,000 people are out and back in in 11 hours. Now, we are using that historical evidence to base --

DR. WALLIS: But now you've eliminated the EP after this situation. So what's going to get them out when the fire is waiting to happen for ten hours? What's going to happen to them then? Are they just going to sit there and wait for the fire?

MR. SULLIVAN: No, not at all. The emergency plan would always require off-site notifications, that there be an on-site emergency capability. The issue really is the level of off-site emergency.

So we're expecting notifications to be made, and we're expecting there to be off-site agencies that are capable of acting on those notifications, and there's a good reason to assume that. You know, there's a good, historical record that these evacuations take place. They're done effectively, et cetera.

DR. KRESS: So the technical issue is how long do you have to decay before you reach some ignition temperature that's currently ill defined for ten hours. So that's the technical issue that has to be determined.

MR. SULLIVAN: We came to consensus with FEMA on 24 hours being an absolute maximum.

DR. KRESS: Well, whatever the time is, you still have to decide what the ignition temperature is, and you'll have a pretty good handle on the decay heat..

MR. SULLIVAN: Yeah.

DR. KRESS: I guess you'll let it be adiabatic just to not have to deal with the --

MR. SULLIVAN: We're proposing using the graphs in 1738 and not doing a site specific analysis. So if it's a BWR, it's X number of hours and X number of years and off you go.

DR. KRESS: For a fixed ignition temperature.

MR. SULLIVAN: Yes.

DR. KRESS: That somebody has specified.

MR. SULLIVAN: That's right.

DR. KRESS: But that has to be put on a pretty good technical basis in view of, say, Dana's hydride.

MR. SULLIVAN: This determination is based on 1738. If 1738 needs to be informed by Dr. Powers' question, then so be it.

DR. WALLIS: Well, Bill Huffman said you cannot predict when the zirconium fire is not physically possible, and now you're going to predict how long it takes to occur when it is possible.

MR. SULLIVAN: Yes, 1738 does exactly that.

DR. WALLIS: Well, it just seems to me there's some inconsistency here.

MR. SULLIVAN: It's assuming there --

DR. WALLIS: You can't predict when it's possible, and yet you can predict how long it takes to occur. Well, you've got to first know if it's possible.

MR. SULLIVAN: No, you can't predict when it's not possible.

DR. WALLIS: Well, then you can't predict when it is possible. That's the logics.

MR. SULLIVAN: No, I think 1738 would say that it is possible.

DR. WALLIS: If you can't predict when it's not possible, you can't predict when it is possible. It's sort of if A, then B, and if not A, not B.

MR. SULLIVAN: Well, why don't we let somebody who helped write 1738 address that rather than me?

MR. HUFFMAN: I'm not the one who did the thermal hydraulic analysis, and I'm not an expert in that area, but I guess 1738 on the most conservative approach, the adiabatic approach, it is a fairly simple thermal hydraulic calculation of how long it will take if there's no energy leaving the system to reach the assumed zirconium ignition temperature. Okay? That's not a complicated equation calculation, and that's the number that Randy is talking about.

On the other hand, it's very difficult to prove that you won't reach that zirconium ignition temperature when you don't know what the configuration is and what the cooling and heat removal from the system is. That's the difference.

DR. KRESS: And the problem there is deciding what that ignition temperature ought to be, and they fixed a number, you say, in this other one, and that's the one that I think we're saying may be in some doubt.

MR. SULLIVAN: Well, yeah. When 1738 was presented to the Commission, it was presented as a bounding. In a sense, you could interpret the way it was presented was that it was an adequate bounding analysis, and that if we worked off this, you know, reasonable assurance that public health and safety could be insured. So that's --

DR. KRESS: Do you recall what the ignition temperature was? Was it about 1,500 degrees C. or something like that?

MR. SULLIVAN: I think it was 900.

DR. KRESS: Nine hundred.

MR. SULLIVAN: Is there a thermal hydraulicist?

MR. TINKLER: When we evaluated the criteria that we consider to be appropriate for ignition, we considered a range of different possible thermal hydraulic boundary conditions, and we specified ignition temperatures for a long-term operation as low as 600 C. up to about 900 C., depending on the age of the fuel, whether or not we thought there was a significant ruthenium inventory remaining the fuel.

And as I recall, the lower temperatures of 600 degrees C., we did address the issue of hydride reactions and considered that they would not be limiting at such low ignition temperatures.

Now, the concentration of hydrides and inclusions in the fuel, the extent to which we have data on whether or not that would lower an effective ignition temperature, I don't recall exactly, but the general conclusion was that those temperatures as low as 600 would cover the effects of hydriding.

Now, I wanted to also just say briefly, too, that was when we were considering things like the break-away oxidation and the range of different temperatures and the low temperature.

No matter what ignition temperature was selected, we recommended that the calculation had to be done to show that you could maintain an equilibrium condition, you know, regardless, and whether or not a particular calculation is conservative, that judgment can't be reached unless you are considering the effects of reactions, the chemical energy. It may be slow, but now you're competing against a decay heat level, which is low.

So reaching a judgment as to whether or not oxidation energy is important or unimportant can only be made relative to the magnitude of decay heat and the heat losses. So we recommended that a calculation be done actually addressing heat generation and losses.

DR. WALLIS: Well, I go back to my point. You're going to predict here when it reaches a temperature for at least ten hours and maybe it's 24 hours, and then you can also predict reaching a temperature for 100 hours, but predicting when it can reach an ignition temperature for any number of hours is just a thing which we were told you couldn't do.

So something just doesn't seem consistent.

MR. SULLIVAN: Well, from an emergency preparedness point of view, we think that somewhere we'll come down with our stakeholders' FEMA states and decide what's a reasonable number.

DR. WALLIS: So you're going to negotiate by words rather than by technical analysis.

MR. SULLIVAN: Well, the technical analysis will give us some indication of when this ignition temperature is possible between the ten and 24 hours time frame, and that's in the report.

The infinite time frame is not in the report. The 100 hours is not in the report. I suppose it could be.

DR. WALLIS: You've got equations. Presumably you can run them for any number of hours.

MR. SULLIVAN: Yeah, the equations could be run, but --

DR. WALLIS: We're told you can't do that.

MR. SULLIVAN: Not by the EP guys. The EP guys are simply telling us 24 hours would be more than adequate to take protective measures off site, and so if you'll give me chart that will go up to 24 hours, I can write an emergency preparedness regulation that will assure public health and safety reasonably. That's kind of what we're saying here.

MR. LEITCH: Can I probe a little bit as far as what you mean by emergency preparedness? Full scope, I assume, you know, initially after shutdown, full scope is full scope, including sirens, emergency response organizations, ELF, Technical Support Center, all of that, Media Center, all of that kind of thing.

MR. SULLIVAN: All of that stuff.

MR. LEITCH: And in the last category, the eventually eliminate EP, I guess what I'm picturing is once you get down to that situation where you have ten hours before you reach ignition temperature, what you have at that point is no sirens, no requirements for sirens at any rate, no off site facilities. What you really need at that point is one guy who's still standing who can call the local --

(Laughter.)

MR. LEITCH: -- and his instructions are

if for any reason that water goes down, call the local law enforcement agencies and tell them they've got ten hours to evacuate.

Is that it?

MR. SULLIVAN: Somewhere more than one guy is still standing, I think.

No, I hope i Haven't confused you. We're really talking about off-site EP requirements. The on-site program would look a lot like an ISFS on-site program. There's training; there's drills; there's people; there's pagers; there's call-outs. You know, there's a --

MR. LEITCH: Even an eventually?

MR. SULLIVAN: Yeah, yeah. If you have spent fuel under ISFS regulations, 72.32, there's a whole program. It just doesn't involve off-site folks much. The off-site folks are invited to training. They're invited to participate in drills. They're invited to an annual meeting, but that's about it. There's no sirens. There's no funded EOC. There's no mandatory off-site drill.

But on site there's a fairly, you know, reasonable level of emergency preparedness. It's not inconsequential.

MR. LEITCH: So when we say eliminate EP, you're talking about eliminating off site.

MR. SULLIVAN: It should have said eliminate off-site EP.

MR. LEITCH: Okay. So --

MR. SULLIVAN: And really it's not totally eliminated, but that the requirements are very much reduced.

MR. LEITCH: And on site would be similar to the ISFS requirements.

MR. SULLIVAN: Yes.

MR. LEITCH: Okay.

MR. SULLIVAN: Which doesn't have a TSC and an EOF and all the stuff you're used to at power plants, but it's not inconsequential either. It's a fairly robust program.

DR. KRESS: Okay. At this time I think we'd like to hear from NEI. I think Lynette Hendricks and Bob Henry are here.

MR. SULLIVAN: Thank you very much.

DR. KRESS: Thank you.

PARTICIPANT: Do we know how long? Till 12:30?

DR. KRESS: This will probably last another 20 minutes or so or half an hour. Half an hour.

MS. HENDRICKS: Good morning. It's a pleasure to be here again, and thank you for the opportunity.

This is an overview, upside down overview, of what we'd like to cover today. I'd like to talk -- I guess we're showing our hand here, is that we're going to return a little bit to our premise that we had the last time we were here, that the study is in large part an excellent compilation of outstanding work, but we think in a few critical area it isn't complete, and that the benefits, cost-benefit of the completion is definitely there and definitely worthwhile.

I'm going to begin briefly to talk about some of the impacts that we see if the study is not completed, and I'm going to turn it over to Dr. Henry to go over a summation of what he believes would be necessary to complete the study, and then I'm going to briefly cover our views, industry views, on the options that are presented.

DR. POWERS: And I know you'll include zirconium hydride ignition in your things that need to be completed.

MS. HENDRICKS: That's right.

DR. POWERS: Bob will cover that one,

right?

MS. HENDRICKS: Let's see. Where am I here?

Here's some of the impacts that we view for failure to complete the study, and again, to sort of revisit where we are on that, and Bob will go into detail, but we think best estimate is possible with existing data for percent of ruthenium release, and we question whether the cask drop has the energy necessary to catastrophically fail the pool. That's sort of where we're coming from here.

Failure to address these deficiencies in our view -- and I'll get into the discussion specifically when I talk about the options, but it definitely impacts the options shown and associated value impact and cost benefit of those options.

Also, going forward, these things once on the shelf, this issue, zirconium fires, is going to essentially be with us forever, and without completing the study and getting a sort of best estimate with a quantification of the uncertainties, you end up with a less than number, that the risk of the zirconium fires much less than 3E to the minus six, it's not going to be feasible to deal with a less than number in the context of PRAs, and yet we're already seeing that the number is -- you know, we are taking actions. The example was for safeguards for operating plants based on this zirconium fire.

DR. WALLIS: Excuse me. I'm sorry. I'm lost here. Are you talking about NUREG 1738?

MS. HENDRICKS: Correct.

DR. WALLIS: Well, we were told it is complete, and you're saying it should be completed. What's the truth here?

MS. HENDRICKS: Well, we're suggesting that it be recalled, if you will, and completed subject to peer review.

DR. WALLIS: Oh, you're saying it's a bad study and it should be revised.

MS. HENDRICKS: It's a very good study to a point, and we think --

DR. WALLIS: Yeah, I know. That's what we always say when we want to criticize someone's work.

(Laughter.)

DR. POWERS: I think that in my understanding, and correct me if I'm wrong, is that in doing this study there were several points that they reached a conclusion that said they were happy with that conclusion, but now they're taking things that would be logically follow-ons to that and using that in formulating their strategy here.

In addition to that, we have this question which Dr. Henry addressed to us last time about whether one of the initiating events was, in fact, a feasible initiating event or not.

I mean, I think the staff would admit to you that they cut off work at various points when they thought they had reached their risk assessment conclusions, but we already saw today, as you so deftly pointed out, that trying to draw conclusions about what the follow-on work would have shown to design their other strategy.

So it's not completed. There needs to be a follow-on to get the technical data they need to draw these conclusions. That's my understanding of it.

MS. HENDRICKS: Yeah, and I agree with you completely, Dr. Powers, and to say that definitely I think the staff has stated that this less than three to the minus six number is well within the safety goal. So good enough.

And maybe, well, I'd like to suggest as we look at the options later that maybe it's not good enough, and you have the question of how does it then relate in PRA space to relative risk of core damage.

How accurate are your value impact assessments going to be when you need to look at the delta in risk for cost spent if you are dealing with, again, a less than number, and it's going to be difficult, I think, to accurately quantify benefits.

One final issue is that when studies like this sit on the shelf, they are going to be used for other applications. We had some indications talking to the staff, for example, that the cask drop results were going to be pulled directly into a PRA that's being done from the dry cask standpoint.

We cautioned them about that, and I'm sure they're going to look carefully at it, but I think the point is that pieces of this study will be used outside the context that it's, you know, as a very bounding number less than the safety goal.

With that I'd like to turn it to Dr. Henry.

DR. HENRY: Thank you, Lynette.

I'd just like to touch on a couple of points. As Graham just said, this is not a complete study so the second word up here can't possibly be correct in terms of a draft study, but we do believe it provides a good summary of the much needed information related to this, and it is a good foundation for evaluating the probability of losing cooling to the pool.

I think our biggest difficulties arise when we then get to using this for risk informed type of decisions as is discussed in the issue and options paper. We believe that when you do this, it certainly would be very helpful to people when assessing issues and options to have a best estimate evaluation for the different types of initiators that are considered.

As Lynette said, the last time we had the pleasure of discussing with you, talking with you, we presented a technical database related to whether or not the cask drop could actually be something that would cause catastrophic leakage from the pool, and we surely don't find that to be the case, and we think that that should be put into the study in a very mechanistic way so that people truly understand what this risk is or is not.

The same thing is related to the issues that Dana has been talking about earlier with respect to oxidation, ruthenium release, et cetera. There is a technical basis out there. We suggest that that technical basis be formulated in a way that we could make use of it through the current evaluation.

In particular, if we look at the screen

criteria, which I think are very well founded, the two that got my attention is would a risk informed approach help to effectively communicate a regulatory decision or situation, and of course, here you want to have the best technical foundation that you can have, and that, of course, trips to the success criteria number five or screening criteria -- excuse me -- that says if you have information or analytical models that exist or are of sufficient quality or could be made of sufficient quality to really support that kind of decision.

Certainly in the term of the cask drop, I think that's clearly there. In the term of zirc oxidation and the release of ruthenium, I believe that it's there. That's my opinion, but I think that it should be put in the form where it's a sound technical basis, and it's made use of instead of we just can't use hand waving in this type of assessment. It's too important.

In particular, I won't use all of these in the interest of time because I know you're running a little bit behind time, but a couple of things here that I find of particular importance is once the study is done, it's amazing how many different ways these things get used.

I think it is certainly as it says here, difficult to provide the necessary foundation for risk informed decision making without having something, without having an analysis, which is a best estimate. I mean, what is it you're really talking about?

The best estimate also needs to represent the uncertainties involved, and without using the technical basis in something in a quantitative manner, then you're really not sure what it is you're coping with.

DR. WALLIS: Why don't we have the only CRS?

(Laughter.)

DR. KRESS: Sounds like something we've said before.

DR. WALLIS: We've been saying that for years.

DR. HENRY: Well, I already travel enough.

After a study is completed, the results are likely to be used for other tangentially related studies, and if you don't have a sound description, you know, well informed basis that you've drawn from that people are forced to then live by when they use those studies, you have no control over how those numbers get used.

DR. WALLIS: I guess you're saying all of this with the implication being that these guidelines have not been followed in some way by the NRC?

DR. HENRY: Well, I think particularly with the cask drop. I think that's one that should be very clearly -- the technical basis should be put out there. I think it relates to issues of EP that we were just talking about because that is the issue unless you're talking huge seismic events. We're not talking about EP related items here.

So we want to make sure that we're communicating what's really known in a technical community.

One of the places where I had a small part of how the number got generated, I remember why it got generated. I also saw how it got misused, and I just put this example down, is in the Swedish reactor safety study, which the people that did that had their own bent on what they wanted to accomplish in that study, but it was paid for by the Swedish government.

And they say in the study that they are using WASH-1400 methodology. One of the places where I thought they grossly misrepresented what was done was in the area of steam explosions. WASH-1400 used a number of ten to the minus two because at that level, that issue was no longer risk significant, and we didn't have to go through all of the weeping and gnashing of teeth from then on. So it was a compromise, and it's all the further the number had to be taken.

They turned this number around and said, "Well, it's ten to the minus two. Those are the experts. We're not the experts, but they gave us the number, and we have three places instead of one where it applies. So we'll make it three times ten to the minus two."

And just forced it into something which now was risk significant because they wanted it to be that way, and then when they showed the consequences of all over northern Europe, that was one of the first grains of sand in the oyster that brought down Barseback, and so there was no control of how it got used.

And I think we have to be concerned about that because we didn't really say at the time we're just going to make this number the way it is because from here on it's not risk significant, and below that number maybe people disagree, but the disagreements don't matter.

This next table is just abstracted from

that Swedish reactor safety study, and you can see this is the WASH-1400 number, and they merely said, "We're going to use something three times that because we have three different places in the RCS and the containment where this could happen."

The next you've already seen before, but this is just things that we believe are strong parts of the technical basis, things that are well known that should be included in the study, and in the interest of time, I know Lynette has a few other things to say, too. I won't take you through that because we talked about it the last time, but the real issue is we strongly recommend that the technical basis be formulated and used to the best ability that we can use it now.

So in conclusion for this part, we believe that a best estimate evaluation is essential to communicate what we really know about the subject here, the cask drop being the one that we think there's a very clear basis on, and even thought it's a completed study now, I think a peer review of that would still be most helpful, and it certainly would be very helpful in the public arena, which these types of studies get reviewed and used.

Lynette.

MS. HENDRICKS: I'd like to go now to our analysis on the specific options. For the safeguards option, the way we read the -- and I'll always be referring to the option that the staff indicated a preference for, and I'll characterize it and then give some insight on what I think issues are with the option as proposed.

For safeguards, the way I read the option, and I think it's still correct after listening to the discussion earlier, is that you will start out having to protect against the design basis threat analogous to what is existing for reactor operations with the difference that you'll have a specific five rem at the site boundary standard that you're working against.

A number that the staff put in the options paper that I think is very important is that they equate that to a need for five armed guards, and the point I'm going to try to be conveying throughout these options is when you're decommissioning cost is people. Cost is the number of staff that you have to carry forward is a good way to think of it, and for these five armed guards for the first part of the option, it's not really five because you have to go around the clock. It may be 15 to 20, and when you do that, you're talking about an amount of, let's say, a low estimate would be $2 million a year.

So just to kind of put a bound around the costs, that's what we're talking about.

The "or" I think is the option that the staff indicated that you could demonstrate through plant specific analyses if the fire is, in fact, precluded, and so your standard then doesn't become one of defend against the frontal assault by these adversaries in the design basis threat. You've precluded it, and then you're in more of a standard that's a little more equivalent to this, but not quite the same. It's more lost of control, call the local law enforcement. So that's the difference.

The specific indication that they gave on how you could demonstrate that the fire was precluded, we talked earlier about design features, heat up analysis. They also referred to mitigating actions, including response by law enforcement before the fire commences, and this makes a real important point that I wanted to emphasize.

The timing is very different for this event compared to a reactor, and even in the worst case situation, the two or thee hours, the thing with the safeguards option is you regain control of the facility essentially immediately. So your opportunity to mitigate is there.

It's not like the seismic event where you can still claim some mitigation capability, but it can be questioned on the basis that the magnitude of the seismic event may render some of your mitigation uncertain.

And I'd also like to point out that one of the industry commitments that's referenced in the study is we do have a commitment for capability for remote access such that if the pool was drained without undue concern for the radiation hazard, you could have a mechanism to try to reflood. I think it was pointed out this morning it's not so important to reflood as much as it is to spray the core in the pool such that steam cooling will remove the heat as it's generated and prevent you from getting to the critical temperature for the zirconium exothermic reaction.

DR. POWERS: Have we ever done that?

MS. HENDRICKS: Pardon?

DR. POWERS: Have we ever done that experimentally, take a bunch of fuel rods, let them get warm, and then spray some fog or something like that over that and see if that, in fact, keeps -- the steam cooling actually works?

MS. HENDRICKS: No, I guess I'd have to

say I haven't.

DR. WALLIS: But it's predictable.

DR. POWERS: Well, you know, I wonder how predictable it is because the things I'm familiar with, it's steam and steam, but now we have steam and air, and I don't know what happens in steam and air. I don't know what happens with irradiated clad.

DR. WALLIS: Yeah, the cooling isn't so bad. It's the chemistry, I think.

DR. POWERS: Yeah, I'd like to know what kind of chemical reactions actually do occur.

DR. HENRY: I can't remember the exact references now, but there are references out there, Dana, which are steam and air sprayed in their overheated surfaces. Condensation is obviously much more susceptible to small air concentrations than evaporation is. Evaporation just depends upon local contact.

But as Graham says, the chemistry obviously is a little more complicated with steam and air.

DR. POWERS: I just don't know what it would be because, well, irradiated clad, especially high burn-up irradiated clad, and quite frankly, that's the ones you're worried about, is the more recent off-loads, which are going to be more highly burned than the things in the pool right now, and I have no idea what exactly happens in those kinds of circumstances.

DR. HENRY: Obviously if you're going to have to spray anything, you'd like to spray at the lowest temperature possible. Thermomechanically it's much better, but there are data taken with sprays on surfaces in air that effectively remove heat. That's not an issue. The chemistry is, again, more complicated.

DR. WALLIS: You're also getting the droplets into the interior of a bundle. That's not a trivial matter. You're going to spray somewhere.

DR. HENRY: You want to spray somewhere, and the best thing you could have is to begin to at least accumulate some films that drain down on the surfaces.

DR. POWERS: I had another question on your previous viewgraph. You began that with a discussion of the cost, and until we have provided adequate protection to the public health and safety, we really can't consider costs. So you must have some idea in your mind in bringing up costs what adequate protection to the public health and safety is.

Can you kind of equate that?

MS. HENDRICKS: Well, I don't think it's an absolute number, but I think when you do a cost-benefit, you look at the delta. If you're going to spend $2 million, can you demonstrate that there will be some increase in protection of the public as you take it all the way out through, you know, eventual exposure to the public?

DR. POWERS: Okay. So you would come back in saying you really do need this probability of sabotage.

MS. HENDRICKS: I don't know whether it's wise to go there. Probably not, but here I go. Whoever said I was wise?

DR. POWERS: I was definitely leading you to a trap.

MS. HENDRICKS: I think actually that the issue earlier about whether you can apply probability to the probability of the event happening, I've never understood really why not. I mean, you have crime statistics. You have a certain number of these events of any type that have happened, let alone have they happened to facilities that are hardened targets where the probability of these people going for those targets is admittedly low.

So I can't help but think even if you didn't have a perfect value and perfect methods, that you could do an awfully law in that area in addition to the mitigation potential and some of the other things that were talked about.

But in terms of sort of giving an absolute indication of our industry views on the options, I think it's difficult to do so because we haven't had any opportunity to comment on the safeguards analyses paper that's being prepared which will give us, I think, a sense of how feasible it will be to get to the "or preclude" part of the options such that you could potentially reduce your staff significantly, which is where the savings would be; get rid of some number of the armed guards, and that's where the cost savings would be.

And then some of these other things are sort of the comment that any change to the adversary could invalidate the entire program. That's sort of just a constant whenever you talk about safeguards, but I think it's always important to reinforce that. We're always on sort of a tightrope when you come to the safeguards area from that perspective.

And then just to note that the standard does say preclude a zirconium fire, and you know, there's no such thing as zero, but I would like to see when we talk to the EP option of getting a little more acknowledgement of that approach, that we do have -- you know, the word "preclude" is used, and it doesn't seem to be reflected in the EP analysis when you start talking about difference in depth.

The insurance option, I think it was covered pretty clearly. We may question the $100 million, and we did when the SECY was originally put out, that even 25 million is probably excessive. It's more than has ever been spent on clean-ups that would involve a truck spill or some other local contamination.

But it doesn't get to really in the end matter that much from what I've gleaned from talking to folks because your premiums aren't necessarily going to go down that much ironically if your coverage goes down from 100 million to 25 or 50 million.

DR. WALLIS: What is the premium for 100 million?

MS. HENDRICKS: It's difficult to quote a number. I think if you're a single unit facility in decommissioning, it's a couple million dollars. A good part of that -- if you're a multi-unit facility, it's an increment, and it's --

DR. WALLIS: So they're charging you two million for 100 million coverage?

MS. HENDRICKS: No, no. That's what you'd be going down from. You'd get a significant reduction, plus you wouldn't have to have the liability of potentially participating in the secondary pool.

DR. WALLIS: So we don't know how much the premium would be.

MR. ROSEN: The premium is not going to be two million year for primary coverage if that's what you meant. I don't think you did, but --

MS. HENDRICKS: No, I'm saying as an operating plant, when you had --

MR. ROSEN: Yes, as an operating plant, it could be that much, but for a shutdown --

DR. WALLIS: In other words, there's no quotes yet for premium.

MS. HENDRICKS: It would go down significantly.

MR. ROSEN: Well, we're talking for property coverage, two million a year. Primary coverage at an operating plant is much less than that. I think the coverage we're talking about here, your first bullet, this is Price-Anderson primary coverage you're talking about, right? Would be in the order of tens of thousands of dollars per year, not millions. Very inexpensive because it's so unlikely that would be called upon.

MS. HENDRICKS: It's obviously very different than the number I was quoted, but I certainly accept your first-hand experience.

But anyway, the bottom line for insurance is we don't have any issues with the option the staff is proposing. It seems very rational and risk informed.

The EP option, as Randy, I think, deftly explained, you get some reduction in the first year, and the vision is complete elimination of off-site EP at five years. Again, this is a bit like safeguards analyses. It's difficult to quantify the benefits.

EP is very analogous to safeguards cost, as the number of people. So until you get into the specifics, as they had indicated through the regulatory guide of exactly what you would be exempted from in the reduced period, and ultimately it's hard to put a benefit in terms of cost savings on what they're talking about here.

In terms of, you know, EP space, you're obviously not in the position of feeling comfortable going completely on a risk basis, and so you acknowledge up front that you are in defense in depth space, but I think having said that, even though defense in depth is a great concept, I think it's incumbent upon the agency to demonstrate that you really have defense in depth, that you have something quantifiable that you can point to for the cost of maintaining the off-site EP in limited form until the five-year period.

The reason I question whether you can actually quantify defense in depth is it may be nonexistent if ad hoc EP is just as effective shortly after shutdown. That was discussed at, I think, great length in the technical report.

And finally the technical report also acknowledges that it's optimally of limited effectiveness and causes delays if you're talking about an earthquake of .6 G, and it goes on to say if it's in excess of .6 G, evacuation is virtually -- I think they come down and say it's virtually not going to happen. You get no credit for it at all.

When we were here last time, we had a graph that tried to break out the percent of the risk of pool failure by seismic magnitude, and what the graph showed was in excess of 90 percent of the risk is attributed to earthquakes in excess of 1 G, and that's really not hard to come to intuitively because of the robustness of the pool.

The HCLPF based on very conservative analyses that the staff did is .5 G. So it's logical that the biggest contribution of the risk is going to come from enormous earthquakes, and when you have then the acknowledgement that you are going to get basically zero benefit, zero evacuation is going to be possible, again, I bring the question back to defense in depth is a good concept to explore, but can you actually point to it and say, "We have. We can provide defense in depth"?

Otherwise you may be misleading the public to talk about a concept when, in reality, it's not going to really materialize.

Another thing that I guess leads us to that question is the fact that, again, to reiterate, we don't believe the cask drop is a realistic event, which leaves essentially seismic and sabotage as the issues that you're talking about, and sabotage, again, you're using the standard of preclude and potentially -- and again, unlike the seismic event, you have recontrolled the facility in very short order, and unlike the seismic event, your mitigation capability should remain intact.

So I guess to summarize, we would recommend that the risk study be completed and peer reviewed, and that efforts be made to derive a best estimate using existing data on ruthenium release and also cask drops, with an effort, of course, to quantify the uncertainties.

I've mentioned under EP and also insurance that in order to get these benefits, you have to meet the industry commitments as well as the staff assumptions. One of these staff assumptions is that you will fulfill the requirements of the seismic checklist, which because of the -- it wasn't based on a large number of plant fragilities, is acknowledged to be rather conservative. The net effect of that is that our experts indicate that to comply with the checklist you're getting into sort of an engineering evaluation of the seismic considerations around the pool, and we estimate that's going to cost about 50 to $100,000.

And then certainly if you failed the checklist and get into any kind of a situation where you're going to go back and ironically maybe do any engineering to improve the situation when you're going to, you know, a few years down the road tear it apart anyway, but it certainly gets into real money very quickly if you have to go back and do much structural work to comply with the checklist.

And finally, the recommendation that the EP reductions be commensurate with risk and some quantifiable assessment of the defense in depth that you actually get with EP.

DR. WALLIS: If we could quantify defense in depth, that would be a wonderful thing.

MS. HENDRICKS: Well, I think in this case you can quantify it.

CHAIRMAN APOSTOLAKIS: Isn't that what PRA does? It quantifies defense indefinitely. Am I missing something?

DR. POWERS: That's what it misses a great deal.

CHAIRMAN APOSTOLAKIS: Huh?

DR. POWERS: It misses it a great deal. Your PRA does not address the sabotage issue at all.

CHAIRMAN APOSTOLAKIS: No, no, no, no, no.

DR. POWERS: Defense in depth does.

CHAIRMAN APOSTOLAKIS: It says quantifiable defense in depth. If you want to quantify defense in depth, that means you want to do a PRA.

DR. POWERS: No.

CHAIRMAN APOSTOLAKIS: How else would you quantify it?

DR. POWERS: Well, certainly not with PRA because you --

CHAIRMAN APOSTOLAKIS: How else would you quantify?

DR. POWERS: We're going to ask her how to do that here in a second. I think that's what Graham just asked, as a matter of fact.

MS. HENDRICKS: I think it's a very fair question. I think you can do it qualitatively or quantitatively. Qualitatively you can ask --

DR. WALLIS: But you can't qualitatively quantify. Now, wait a minute.

MS. HENDRICKS: Well --

CHAIRMAN APOSTOLAKIS: Yeah, because I mean that's exactly what PRA does. It quantifies the level of defense you have. So if you want quantification, that's what you are calling for.

Now, that doesn't mean you're going to do a small LOCA. I don't mean PRA the way we do it for -- I mean the philosophical approach. That's exactly what it does.

Now, whether it's incomplete, that's a

different story. Whether you don't believe it.

MS. HENDRICKS: The example is if you acknowledge that you're looking at the primary risk from an enormous seismic event and you can't evacuate people, then you can quantify that defense in depth from fancy evacuation procedures aren't going to do you much good.

DR. POWERS: I think what you really said there, to avoid provoking the Committee, you would have said quantify the benefit --

MS. HENDRICKS: Thank you.

DR. POWERS: -- derived from these measures, which I think you've made the point. We ought to look and see if there's some benefit there.

MS. HENDRICKS: Thank you, Dana.

DR. POWERS: Because otherwise defense in depth is a difficult thing to do for quantification, and that's why I was trying to put it in the context of fire. You know, we're certainly going to try to prevent the events from occurring. That's the first step in fire protection. We're going to suppress the effects of a pool drain-down or something like that. That's why you've got fog nozzles and things like that.

And then we're going to protect the

vulnerable components, which in this case is the public, from the effects of fires that we failed to suppress, and what you're saying is you may not be able to do very much there. And you ought to know how much you're going to do before you spend a lot of money on it.

That seems fair.

MS. HENDRICKS: Exactly.

DR. POWERS: It seems fair.

MS. HENDRICKS: Thank you for your mentoring. That's a wonderful skill, to learn how to not provoke the committee. I will learn.

DR. POWERS: We spend a lot of time discussing defense in depth here.

MR. BAGCHI: Mr. Chairman, can I quickly share a perspective on cask drop?

CHAIRMAN APOSTOLAKIS: Can you identify yourself to us?

MR. BAGCHI: Yes, sir. My name is Goutan Bagchi. I'm the Senior Advisor in the Division of Engineering.

I understood from previous discussions that there were some comparisons back 20 years ago based on every (unintelligible) missile studies, which in the report itself concluded that the analytical predictions were way too conservative.

The basis for the conclusions that were presented in the NEI slide today and the previous time was perhaps this, that the (unintelligible) comparisons were too conservative and, therefore, the implication is that the cask drop is now going to lead to some kind of a failure of the pool slab.

There are clearly some kinds of pools where the pool slab is supported by the soil medium where the failure is not very likely, but there are other cases where the slab is way up in the air, as in some or most of the BWR spent fuel pools. A 100 ton cask falling through maybe 30 feet depth is going to produce a significant amount of impact force that is, in my opinion, going to lead to a failure, and I'm going to tell you why.

Because, you know, the 20 year analytical capability, 20 year old analytical capability has now been substantially improved, and our colleagues in NMSS have undertaken a study where drops were made and then comparisons were made by using more recent techniques, and they have plotted various deceleration diagrams based on those studies, actual studies and comparisons with analytical tools.

And based on that I got a deceleration

force and applied that to the full slab, not supported by soil or anything, but the slab by itself, and there is no way that kind of a deceleration force will be withstood by a pool slab, which is, you know, not supported by anything else.

So when you say that the cask drop is way too conservative, please take a look at more recent data, more facts that have been substantiated by even NRC initiated studies.

DR. HENRY: My response to that, I think the last time that we had the opportunity to talk to the committee we did look at at least the most recent data, and we asked if there was anything that we didn't have, we would sure like to know about it because the data that we had -- give me a second here. Let me find the right one -- included the studies that BNFL did here up through 1993, and they were with full size casks dropped from heights at least up to five feet, and those just barely made a dent in the concrete.

We're not here to argue which is right or wrong. What we really would like to do is make sure that the technical basis is clearly spelled out in the report and where the conclusions come from because I didn't even see any of these tests referenced in the report.

Now, if there were calculations behind the report that we didn't know about, we'd be more than happy to hear about them, but all we're really here to plead for is let's have the technical basis clearly defined, especially the experimental basis.

MR. BAGCHI: Those studies, those study reports came from the NUREG CR that was developed, and comparisons were made. Deceleration forces were given. You can take that and apply to the slab.

I don't know that you have had any of your structural experts look at that. I am a structural expert as a professional engineer, and I looked at that. My conclusion has been that pool slabs that are not supported by any soil are not going to withstand that kind of a drop.

DR. HENRY: I'm not here to impugn your capabilities, you know. All I'm saying is if you've done those analyses, I didn't see those referenced in the report and anything I could really point to.

I mean, this is a very important part of the study.

MR. BAGCHI: I'm confused about what you're implying here by this slide.

DR. HENRY: Give me a second then. I mean this is the test that was done.

DR. WALLIS: Which is what you talked about last time.

DR. HENRY: The last time, and this is the damage that was seen.

MR. BAGCHI: That's not my point. These are not one-to-one comparisons to what I was referring to, a slab purely unsupported by anything else.

My point was whether or not those references were included in our report. The answer is, no, we didn't because we had a very specific study done on a pool, Vermont Yankee pool, and the results are available in the NUREGs here.

DR. HENRY: They are?

MR. BAGCHI: Yes. There was a finite element study, and I think your implication was that that study used rather conservative assumptions.

DR. HENRY: But where were those in the report? The conclusions may have been there.

MR. BAGCHI: It was a reference in our report, 1738.

DR. HENRY: It may have been referenced, but I mean, that's all that was there.

MR. BAGCHI: It is based on that finite element analysis in that report.

DR. HENRY: I'll be more than happy to -- if you can send me that, I'd be more than happy to look at it because I'm most interested in it, but the other aspect is did you conclude from that that it's a catastrophic leakage of the pool?

MR. BAGCHI: If there is a drop of 100 ton cask on a pool that is high up in the air and not supported by soil, my personal conclusion is that, yes, it's going to fail catastrophically.

DR. HENRY: And in that analysis, did you include the water and the other fuel that's already there as any kind of shock absorber mechanism, regardless of what its configuration?

MR. BAGCHI: It was -- in that analysis it was assumed to fall freely.

DR. HENRY: But since this is an initiating event, there would be fuel there. There would be water there also because your whole basis was on rates of deceleration. So the more you slow it down in advance of hitting the concrete, the more you spread out the load, right?

MR. BAGCHI: If in the path to fall to the floor there is no intervening fuel, then you cannot assume any resistance from that.

DR. HENRY: Or the water.

DR. WALLIS: It seems we have a technical debate going on with the results somewhere else.

DR. KRESS: This seems like something you guys could debate --

MR. BAGCHI: I just wanted to say that there was a study that updated the analytical methods and compared that against the drops, and deceleration values are clearly relevant.

DR. POWERS: I thought the exchange was helpful, by the way, because it puts context on two different analyses that we've seen.

DR. KRESS: Yeah.

DR. POWERS: And it's a fairly -- I mean it's a non-trivial issue to resolve as whether this is an initiating event or not.

DR. KRESS: Are there any other questions or comments from the committee?

(No response.)

DR. KRESS: Well, thank you very much.

MS. HENDRICKS: Thank you.

DR. KRESS: We appreciate the input, and we'll turn it over back to George.

CHAIRMAN APOSTOLAKIS: Thank you, Tom.

Yeah, I'd like to thank also the presenters today, the staff, and NEI.

How about if we have a 45 minute lunch break?

DR. KRESS: Yeah.

CHAIRMAN APOSTOLAKIS: So 1:30, 1:30.

(Whereupon, at 12:40 p.m., the meeting was recessed for lunch, to reconvene at 1:30 p.m., the same day.)