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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

MEETING OF THE SUBCOMMITTEE ON

REGULATORY POLICIES AND PRACTICES

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THURSDAY, APRIL 1, 2004

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ROCKVILLE, MARYLAND

The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. William J. Shack, Chairman, presiding.

COMMITTEE MEMBERS:

WILLIAM J. SHACK, Chairman

MARIO V. BONACA, Member

F. PETER FORD, Member

THOMAS S. KRESS, Member

GRAHAM M. LEITCH, Member

VICTOR H. RANSOM, Member

JOHN D. SIEBER, Member

GRAHAM B. WALLIS, Member

MICHAEL R. SNODDERLY, ACRS Staff

NRC STAFF PRESENT:

LEE ABRAMSON

STEVE BAJOREK

SUZANNE BLACK

BENNETT BRADY

ART BUSNIK

JOHN CLANE

STEPHEN DINSMORE

CAROLYN FAIRBANKS

FRANK GILLESPIE

CATHY HANEY

ALLEN HISER

MICHAEL JOHNSON

GLENN KELLY

MARK KOWAL

RALPH LANDRY

JAMES LAZEVNICK

STU MAGRUDER

EILEEN McKENNA

MARK RUBIN

DAVE SKOEN

ROB TREGONING

PETER WEN

I-N-D-E-X

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

8:31 a.m.

CHAIRMAN SHACK: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguard, Subcommittee on Regulatory Policies and Practices.

I am William Shack, Chairman of the Subcommittee. Members in attendance are Mario Bonaca, Peter Ford, Tom Kress, Graham Leitch, Victor Ransom, Jack Sieber and Graham Wallis.

The purpose of this meeting is to discuss the Staff's proposed approach for responding to the Commission's March 31st, 2003, Staff Requirements Memorandum on Risk- Informing 10 CFR 50.46, and Development of Near Term LOCA Frequencies.

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

Michael Snodderly is the designated Federal Official for this meeting. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on March 23rd, 2004.

A transcript of the meeting is being kept and will be made available, as stated in the Federal Register Notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. We have received no written comments or requests for time to make oral statements from members of the public regarding today's meeting.

I think we'll just start in with the meeting, and I call upon Michael Johnson of the Office of Nuclear Reactor Regulations to begin.

MR. JOHNSON: Thank you. Michael Johnson, Deputy Director, Division of Systems Safety and Analysis. We are happy to be here, of course, to talk about 50.46 and I just wanted to say a few words to put in context where we are on 50.46.

Because I have a sense that at the time that perhaps when we were thinking about scheduling a status update on 50.46, and the response to the Commission's SROM, we anticipated being at a different place.

And as you can appreciate, where we are today is, as we're going to discuss, we, the Staff has done some thinking. We've provided some issues that are open with the Commission with respect to 50.46, and as we proceed and go forward in addressing those issues, we're going to listen to hear back from the Commission in terms of the direction that they give us.

So, this presentation today is really a good opportunity for us to talk about what we've given the Commission in February to address the direction that they gave us in the SRM. Although, I think you'll recognize that where we are in terms of the time line, in going further with this issue, we're in a different place. We're, again, happy to answer whatever questions you have. And we look forward to responding to those questions as best we can.

CHAIRMAN SHACK: Eileen.

MS. MCKENNA: Thank you. Good morning, my name is Eileen McKenna. I'm presently a Section Chief in the Policy and Rulemaking Program in NRR, but up until fairly recently I was the Lead Project Manager on the effort to respond to this SRM on the Large Break LOCA Redefinition.

With me at the table is Glenn Kelly, who is a Senior Reliability and Risk Analyst in the Probabilistic Safety Assessment Branch in NRR, also.

Also in the room we have other members from our working group and we may call upon them as necessary, depending on the nature of any particular questions.

This cover slide, as I said, the names of who we are. Very briefly, in terms of an agenda, we'll give a little bit of a purpose of why we're here. Some background on where we've been and why we're in this kind of situation with the status that we are.

Some discussion of a number of technical issues that relate to the question of Large Break LOCA Redefinition, and finally we'll summarize and give conclusions of where we are.

I think we, we saw, based on kind of Mike's comments, we really saw two main purposes for the briefing. One, is to inform the Committee where we are. We've not had discussion on this topic in quite a while, on some of the option three activities.

And secondly, I think it would be a good opportunity to get feedback, at least on some of the technical areas that we're struggling with.

Obviously, the policy direction may steer us in particular avenues that we will hear back from the Commission. But there is still a lot of technical work that needs to be done and certainly this Committee, I'm sure, has opinions and comments to make in those areas.

Just some very brief background. As you recall, Option 3 was the proposal to Risk-Inform Technical Requirements within Part 50. And there were various candidate rules that were looked at as possible areas to be studied.

50.44 was the vessel gas control, it was the first one that kind of went through this process to be Risk-Informed, and that rule was complete in the fall of last year.

The other candidate that was put forward, based on a number of considerations, was 50.46, and a lot of its different aspects. The sense was that the Large Break LOCA with low frequency has a major impact on plant design and that maybe there was opportunity to make the requirements more commensurate with the frequency of the initiators.

And so there were a number of different proposals of how anyone might approach that with respect to Large Break LOCA and its set of requirements.

There were a couple of papers that went up to the Commission. There was a SECY 01-33, that discussed various recommendations on actions that could be taken. And a follow up paper, SECY 020-57, that have updated the status of things.

And that culminated, then, in an SRM we received on March 31st, 2003, that directed the Staff to undertake a number of activities. I've focused the discussion here on just a couple of them because they're the ones that this paper particularly was responding too.

MEMBER WALLIS: Eileen.

MS. MCKENNA: Yes.

MEMBER WALLIS: This mentions the key role of Risk-Informing?

MS. MCKENNA: Yes.

MEMBER WALLIS: And yet what we've seen is mostly about frequency of pipe breaks. It isn't the same thing as risk?

MS. MCKENNA: Well, I think we saw that the frequency of pipe breaks is information one needs to consider as part of the Risk-Informed decision making.

MEMBER WALLIS: That's right, but there's a lot of other things you change.

MS. MCKENNA: Absolutely. And that's what, I think, our effort has been focusing on, this afternoon's effort we'll talk about the generation of the frequencies but we see it's clearly just one input into a larger process.

Did you want to say something, Glenn?

MR. KELLY: My name is Glenn Kelly, and it's our intention that in developing any rulemaking that would come out of this effort, that we expect to fully Risk-Inform the process that would come out.

This would include taking into account the information that we have about expected frequency, loss of cooling accidents, particularly the larger LOCA cooling accidents.

We'll also take into account any potential changes that might occur to the plant that would be allowed due to removal of these break sizes from the design basis.

We'd look at, potentially, the increase in core damage frequency or a large early release frequency, associated with any changes made to the plant.

We'd also look at the retention of adequate defense and depth in particular things such as adequate redundancy and diversity. We'd be looking at margins aspects and also there's issues about when one takes breaks out of the design basis, ordinarily something that's not in the design basis, you're not required to protect against.

You maybe protecting against it, but you're not required to protect against it. And the question comes, what mitigative capability would be retained for these breaks.

MEMBER WALLIS: Well risk is concerned really with design, beyond design basis, isn't it?

MR. KELLY: Risk is concerned beyond the design basis because of the way we've built the plant. If we had not done such a good job of building the plants, you would have potentially significant risk within the design basis.

However, it turns out the vast majority of risk occurs outside of the design basis.

MEMBER WALLIS: So does that mean what you're going to do is now take something which used to be design basis and put it into risk space?

MR. KELLY: It would, at a minimum, be in risk space. And it might also be in kind of another space, which has yet to be determined.

It might have some additional regulatory controls on it, but exactly how that's going to play out, that's not been determined yet.

MS. MCKENNA: So we're talking about the March 31st, SRM and there were several, as I said, several taskings in that SRM. One was to do this frequency work that you'll hear about this afternoon.

The second was to prepare a proposed rule that, I guess it was to say it allows for a Risk-Informed alternative to the present maximum LOCA Break Size.

And there was a second tasking in there, that I won't talk too much about, but I just want to mention it here. There was also, another effort that had been put forward to risk-inform the ECCS Functional Reliability Requirements.

And this one you may recall hearing more about, because that was part of some of the Staff's original proposals in the earlier SECY's.

This was really dealing with the GDC 35-type of information about the assumptions on single failure, and assuming that loss of outside power has occurred coincident with the LOCA, which has a tendency to drive certain parts of the design and may not be, again, realistic and commensurate with the risk, the frequency of those kinds of events.

And in particular, it really dealt with this coincident LOOP assumption in the analysis. And we'll talk a little bit about that later. There were some other parts, but I'm not going to dwell on those because they're not something that we're covering in the paper that has gone forward.

The other important thing, I think, to

keep in mind is that SRM had a number of specific statements about Commission expectations or desires or direction on certain aspects of this rulemaking that we needed to take into account.

And, I'm not going to go through the whole SRM, but I wanted to just put a few of the more significant, at least in terms of this effort. And just to kind of put, have them in your mind when you hear of some of the issues we've been trying to deal with.

So the first one is, I think, what I repeated on the earlier slide that we should develop a rule allowing this alternative maximum break size. And there was some suggestions in the SRM, that one way this might be done is by revising the definition of LOCA itself.

Either as it appears in 50.46 or as it appears in Appendix A, which is the general design criteria. Obviously, to redefine the definition, then you are, in essence, redefining wherever that definition is used in the respective requirements.

There was also discussion about the Staff establishing a risk cut-off for what this new maximum LOCA break size would be. And there were, again, some examples of possible ways this might be done in terms of a contribution either of the LOCA risk or some percentage of the total risk of the plant. That that might be used as the means of determining the break, the cut-off, if you will, for what break sizes are in the design basis or not.

MEMBER KRESS: When we're speaking of risk, in terms of rules, you're talking about cumulative risk of a lot of plants.

MS. MCKENNA: I'm sorry?

MEMBER KRESS: You're talking about cumulative risk of a lot of plants.

MS. MCKENNA: Cumulative, yes.

MEMBER KRESS: And it's been my opinion that the risk contribution to this cumulative of a given plant, differs from plant-to-plant. So by changing a rule, you're going to affect some plants more than others.

MS. MCKENNA: Yes, I think --

MEMBER WALLIS: And the question is, how do you deal with that type of effect in terms of being sure an individual plant doesn't pose an undue risk as opposed to the whole fleet of plants causing an undue risk.

MS. MCKENNA: Yes, okay, Glenn would like to take that one.

MR. KELLY: We don't have a proposal of exactly how it would be done. I think that the actual physical process of tracking cumulative changes of risk at a plant are challenging, and it's being done now for some of the risk-informed activities that are going on, such as ISI.

I, the expectation is that we will, the cumulative risk would be tracked for individual plants, rather than looking at the cumulative risk for the plants. One could then merely add up all of the individual plant risks, but our expectation is that we will be looking at the cumulative risk at an individual plant and making sure that no individual plant should have its risk become --

MEMBER KRESS: I think that's the way to go. In that respect, you will then be relying on the plant-specific PRAs?

MS. MCKENNA: Yes.

MR. KELLY: If --

MEMBER KRESS: And you'll have to have some specification of scope and quality of PRAs?

MR. KELLY: That's our expectation. And if a plant should choose to take advantage of this voluntary rule, then they would become subject to the, whatever requirements are in the rule that deal with the quality and scope of the PRA.

MEMBER KRESS: So even that would be part of the rule, you expect?

MR. KELLY: It would be part of the rule or it may send you off to some other document that indicates that quality of the rule.

MEMBER KRESS: It looks to me like Reg Guide 1.174, is already a framework for doing this tracking and this risk. Would your expectation be that you would just implement Reg Guide 1.174, for these changes?

MR. KELLY: That would not be my expectation. And in the Memorandum that we sent up to the Commission, we indicated that we thought that the Reg Guide 1.174, provides an excellent framework within which to follow how one does a risk-informed process.

And that the metrics that are used within Reg Guide 1.174, of core damage frequency, total core damage frequency, increases in core damage frequency, increases in LERF are probably the type of metrics that we'd end up proposing is the ones to be used to measure the risk at the plants.

The numbers that are in Reg Guide 1.174, may not turn out to be the appropriate numbers to use in this particular case. One of the important reasons why that may be is that Reg Guide 1.174, was built, the numbers were built around assuming that all of the regulations are met.

You're able to change the licensing basis under Reg Guide 1.174, but you cannot change any regulations under 1.174. Now we're in a situation where you're actually going to physically change the regulations, and therefore it may require a more stringent numerical criteria.

MEMBER KRESS: I'll have to think about that one, because --

CHAIRMAN SHACK: If one risk is acceptable why isn't --

MEMBER KRESS: Yeah.

CHAIRMAN SHACK: -- you know, in one situation, why isn't it acceptable in another?

MR. KELLY: That's a good question. And --

CHAIRMAN SHACK: I mean I can understand, you know, questions of uncertainty and, you know, perhaps the degree of quality that one might expect from a PRA for one kind of application over another.

MR. KELLY: Again, it depends on, well one of the things, what happens here with the changes that potentially could be made under this rulemaking is that they would be, if they were not very carefully boxed in, so to speak.

They could be very, very extensive changes to the plant because the, for many plants the large break LOCA is the design basis event that dominates the thinking behind which the plant is designed.

And if you go, if you physically change that new, you change the maximum design basis LOCA, you're removing some of the mode of force behind what, why we have such a strong containment. Why we have all ECCS capability that we have.

And if you just said, okay, I'll take that out of design basis, and now you're free to do whatever you want to do since these are no longer there, potentially, I'm not saying that we think that is a good idea, but potentially one could, weaken containment, one could significantly reduce ECCS flows, in some cases could get rid of accumulators.

And some of these things may turn out to be acceptable, but we want to look at them and understand them. And it's not clear. EQ would change the requirement and the containment would be different, etcetera.

So we'd have to look at what all the potential risks are.

MEMBER KRESS: Well, it seems to me like perhaps a slightly modified change in what we view as defense and depth. And in addition to the metrics in Reg Guide1.174, all that automatically takes care of itself.

MR. RUBIN: This is Mark Rubin from the Staff. We haven't chosen the metric yet and there's a lot of deliberation that has to go on and your input will be very useful in it.

But our initial thought that perhaps something, something smaller may be appropriate and we have to give it some thought. As Glenn said, the philosophy going into 174 was no fundamental changes to the regulations.

We are still going to keep those solid. Now we are doing fundamental changes to the regulatory framework, and so since we're going in with that philosophical change, some of the underpinnings are changing.

So, we thought, well, maybe a lower metric is appropriate. At the same time, some of the plants have much lower baseline risk to start with.

For example, some of the boilers are down in the low ten to the minus six. And if you go with a CDF limit, delta CDF at ten to the minus five, and making a fundamental change to the plant's limiting design basis accident and you allow this change, you're going to allow some of the BWR 6s and BWR 5s, to change their limiting a DBA and bump their baseline risk up by a factor of eight.

Is that what we want to allow? I'm not sure it is, we need to give it some thought.

MEMBER KRESS: Well, doesn't 1.174 automatically take care of that point?

MR. RUBIN: Well, we talked to the Committee about this, about five or six years ago. 174 doesn't, on the surface, prohibit it, but as we told the Committee and as we talked about it ourselves, we would look with a lot of skepticism at a licensing action that came in with an initiative like that.

The industry, in the discussions with us, said, oh no, we would never propose that. When in fact at this point, no one has come in with a licensing action that did that.

But the difference here, and I want to emphasize this, this is a change to the regulations, and a regulatory change that allowed an increase or a decrement in safety of a factor of eight would be very different.

MEMBER KRESS: But what that amounts to is a potential bunch of changes to the licensing basis. That's all the changes that you're going to need, so it amounts to the same thing, in the long run. And just because it's a change in regulation that doesn't make it different.

MS. MCKENNA: I think somewhat it depends on how the regulation itself is actually structured.

MR. GILLESPIE: Let me --

MS. MCKENNA: Okay, go ahead Frank.

MR. GILLESPIE: Let me, Frank Gillespie from the Staff.

MEMBER BONACA: I mean right now I believe that 1.174 said it has to be a small change, a small increase. In most cases, even if are down to ten to the minus seven --

MEMBER KRESS: Yeah, so it kind of automatically limits it.

MEMBER BONACA: And I guess what they're saying is --

MEMBER KRESS: And what I was also saying is you might want to rethink your defense and depth in the sense that your BWR was a prime example of what I had in mind.

They have low, very low CDF, but sometimes a pretty high conditional containment failure. You might want to have in your defense and depth considerations there saying, no, we're not going to let BWRs change a huge amount on the CDF.

I mean they just invoke a defense and depth contract. But anyway, I still fail to see why 1.174, doesn't do everything you want.

MEMBER BONACA: No, but just to complete my thought.

MEMBER KRESS: I'm sorry.

MEMBER BONACA: The difference I see is that right now, again, you're constrained to a small increase, and, by definition. It doesn't matter how much margin you have there, you can only cash in a very small margin.

If you make a rule change, it automatically allows you to cash in whatever the rule may say, that may be a factor of eight or ten, I see a difference.

MEMBER KRESS: No, the rule ought to say if you make any changes, based on this rule change, you do it according to the guidance in 1.174, and you track the cumulative and let 1.174 keep track of it for you.

MEMBER BONACA: You have to define however, going to define how much small --

MEMBER KRESS: No, it's already defined 1.174. And you just say, for every change you make, you invoke 1.174.

MEMBER BONACA: No, I think this goes beyond. If you really want to achieve some level of excession --

MEMBER KRESS: It goes beyond that in the sense that there may be a lot of changes that weren't envisioned for 1.174 --

CHAIRMAN SHACK: But we don't know exactly how 1.174 handled this issue of how we chop things up to, you know.

MEMBER KRESS: Oh, there was a question about how do you, how do you take the number of changes in time. And there was also a question of how do you accumulate risk and track it.

But both of those were discussed and I thought handled pretty well in the 1.174 document.

MR. GILLESPIE: Yeah, they could. Frank Gillespie from the Staff. Let me bring this back to what I'm going to call plain English for a non-risk guy, and someone who has been hanging around for over 30 years.

The large break LOCA, way back in the

beginning and I think there's people in this room who can actually remember back in the beginning also, was fundamentally kind of a worse-case surrogate for what we didn't know.

And let me suggest that we're talking truly about uncertainty. And before we destroy that surrogate that had served us very well over the years, and I think anyone would know.

Any incident we've had loss of outside power, seems to always have some complicating switch that didn't work, some breaker that didn't trip.

One, you have to be so certain your PRA knows everything you don't know. That before we give up that robustness, and this is what I think what Glenn was trying to say.

Before you give up that robustness, you need to know what are the impacts of what you're giving up. And you're giving up something you don't know.

Would, you know, and I think that's the hesitancy and the reason you see many of the issues, at the core of the issues in the Staff paper are the reason accident management things in the `80s were left as accident management and didn't have more rigor on them, was because we said we have such a robust design, in place with the rules already.

Now, if I take the large break LOCA and take part of that robust design and put it into the severe accident space, was my decision on accident management and not needing to have more regulatory controls on that still valid?

Do I have to go back and revisit every decision of not regulating that severe accident space we made in the `80s again, because I've removed robustness.

And, so it is a question of uncertainty in what we've done in 1.174 is, we've kind of allowed that robust design in compliance with the current rules, to kind of help deal with the uncertainties to allow us to give certain freedoms.

And so, I mean that's the caution you're seeing from the Staff right now, is we don't really understand the impact of what we might do completely.

CHAIRMAN SHACK: But you make this argument about robustness, as Tom pointed out, your design basis didn't provide you with a robust containment in the BWR.

You know, if you'd explicitly had a conditional containment probability, you would have had a more robust design. And this notion that I'm going to get a robust design indirectly, you know, maybe works in some cases, but why not go to what I really want to have and say I have it, rather than allowing, you know, this sort of indirection to, you know, maybe provide it and maybe not.

MR. GILLESPIE: I don't disagree with that because I was responsible for the study that got harden vents put on the small containment back in the `80s. So that came out of a NUREG I sponsored.

The fact that we weren't as smart as we thought we were in the `60s and `70s, when these plants were getting designed and originally licensed is what you just pointed out.

Nonetheless, we're very hesitant to give up even more of what we didn't know. And I think that's what you're seeing in the paper. It raises these kinds of issues. And I think what you're saying the Staff is saying here is we don't have, necessarily all the answers today.

But it's not clear that it's as simple as saying having a better PRA. Then you do have to make the judgement. Do you think the all-inclusiveness of your PRA has hit everything that we've actually seen in operating events?

CHAIRMAN SHACK: Well, that's why I like

conditional probability. Is that I ignore everything I don't know and, you know, it can happen and therefore I take care of it.

MR. GILLESPIE: And I'll suggest the discussion we're having right here today, is what caused the Staff to write the paper they wrote. We actually don't have all the answers. And it was that hesitancy I think you see in going back to the Commission.

CHAIRMAN SHACK: Is this a good place to discuss this kind of semi-risk argument that, you know, as we look for mitigation that really isn't mitigating the kind of risk that we normally consider?

MR. JOHNSON: This is Mike Johnson. Let me suggest that actually these issues, the Reg Guide 1.174, issues are issues that we describe in the paper, and in fact Glenn is going to get to those later on and the issue you raised we're going to get to later on.

CHAIRMAN SHACK: Okay.

MR. JOHNSON: I just wanted to say, you know, we're not, none of us are suggesting, believe that Reg Guide 1.174 is not the right framework to use. We want to make sure that we examine it in light of, as Frank indicated, the far-reaching potential changes that could be permitted by the rule to make sure it's the right, that we need to expand it, if we need to expand, that we do.

But we're going to talk more about all those issues later on.

MS. MCKENNA: That's actually an interesting point to go to the next bullet on the slide because this was kind of the more, sorry.

MEMBER WALLIS: I wanted to pick up on something Frank said about things you don't know.

MS. MCKENNA: Sure.

MEMBER WALLIS: One thing that I haven't seen mentioned in any of the paperwork that I've read on this subject, is the deliberate affect by human being, either deliberate or confused act by human beings initiate something.

If you change the rule to make certain sequences to far more vulnerable to certain sequences, then you're obviously making them more attractive for someone who wants to intentionally initiate that sequence.

And this doesn't seem to be factored into the risk that the frequencies are all dependent on normal operation. It if they happen it's because the happen.

But if you change the rules, so that something because more attractive, to someone who wants to initiate something, then it becomes more likely. You're talking about ten to the minus eight for something.

It seems to be much more likely that some disgruntled, foolish or otherwise motivated person, would do something to initiate something more likely than the ten to minus eight.

It seems a more likely event than a ten to minus eight event.

MR. RUBIN: This is Mark Rubin again from the Staff. The frequency estimates coming from the Office of Research, don't include sabotage events, and you might raise that with them this afternoon.

But the approach being taken for the rule development, which is proposing retaining mitigative capability for the new beyond design basis LOCA redefinition, would not give up a success capability for the new sizes beyond the redefinition.

So it would not be a very attractive location or size for a saboteur or even an insider. Because if we achieve the mitigative capability, if the Commission endorses the preliminary approach the Staff is suggesting, a break for above new definition size would result in thermal hydraulic success.

Now, there might be some --

MEMBER WALLIS: I think that's important, I'm glad you said that. I'd almost wish you'd say it again.

MS. MCKENNA: We probably will later.

MR. RUBIN: Mr. Kelly will be getting into because it is our proposed --

MEMBER WALLIS: Because I'm not quite sure from all that I've read, what it is you're giving up. I mean you're going to give up the large break but you're going to still be able to mitigate it. Now, I'm not quite sure what this means?

MS. MCKENNA: Again, keep in mind there's the design basis space, and there's other space, which a little earlier I think we were saying the Commission has said they're willing to give up on some of the larger breaks in design basis space the way we've classically treated them, with you know, assuming worst-case single failure and loss of outside power and analyzing them with Appendix K and very conservative models.

And all of these kinds of things that we apply to them. I think the thing we're saying is that where you don't think the Commission is willing to give up that, if you had a large, very large break LOCA, that you have no capability and --

MEMBER WALLIS: Well, I think this should be very clear also to the public, if that's the way you're going to do it. It's not as if you're simply saying this thing is so unlikely, we won't even consider it.

MS. MCKENNA: Yeah, and that's not at all --

MEMBER WALLIS: They're going to say this thing is so unlikely that we're not going to give it the full treatment.

MS. MCKENNA: That's correct.

MEMBER WALLIS: But that's got to be very clear.

MR. RUBIN: But we're looking for the Commission to, to confirm that to us.

CHAIRMAN SHACK: So we'll have a conditional probability of success is mitigating a DEGB?

MS. MCKENNA: That's one way of thinking about it, yes. As a matter of fact, as I was saying, this next bullet was kind of an interesting comment in the SRM, because it was kind of a counter-current to the, okay, you redefined the large break LOCA and come up with a new break size, but then there was this statement about the Commission would not support the changes to functional requirements unless they were fully risk-informed.

And it gave as an example, no changes to ECCS coolant flow rates or containment capabilities. And, you know, obviously we've spent a lot of time studying this and, you know, trying to gauge what we thought this meant, because there's a lot of potential.

And depending on how you read that, you could say, well, they really don't want to change much of anything, because they're not going to change flow rates.

Or you could take the more, okay, well change containment capabilities, we still want to have a robust containment because that's a good barrier for protection.

So it did kind of give us some pause in terms of, okay, we're redefining it but we're not, you know, this is Option 3, changing technical requirements, but no changes to the functional requirements unless fully risk-informed.

So that was --

MEMBER LEITCH: That discussion of a minute or two ago seems to presuppose that the Commission is going to erect a narrow approach rather than a broad approach, to use the terminology --

MS. MCKENNA: Yes, yes.

MEMBER LEITCH: And I guess, I don't know, do we have such a signal?

MS. MCKENNA: Well, I think that was the reason we presented the issue back to the Commission. As we said, well we see signals on the one hand suggesting narrow. We see signals suggesting broad.

And Staff, obviously, if we're going to go one way or the other, we need to work harder on certain issues and we wanted to get a sense of which way, which direction do you want us to head, Commission? And that was really the fundamental reason for the paper.

MEMBER LEITCH: The comfort that Dr. Wallis perhaps felt, would only be the case if a narrow approach was taken.

MR. RUBIN: Excuse me, no, I don't believe that to be the case at all. Either the narrow or the broad approach the Staff believes would be predicated on having a thermal hydraulic success for a break above the new redefinition size.

We believe it would be a fundamental

change in our safety philosophy to, at least for the working Staff, the working group to not have success for this, for a break above the new definition size.

MEMBER LEITCH: Then perhaps I didn't quite understand what is meant by a broad scope. I guess maybe we'll get into that a little later on as we proceed here?

MR. RUBIN: I could give you a capsule description now, if it would help you, or we could just wait.

MEMBER LEITCH: Yeah.

MR. RUBIN: Okay. The plant's design basis accidents really provide the definition of the suite of safety systems obviously for the plants.

You look at the Chapter 15, design basis accidents, and to meet the acceptance criteria in the general design criteria and the regulations and the SRPs, what's needed is 2200 peak clad temperature and the clad oxidation limits and the peak clad temperature, excuse me, I already said that.

And the as meet pressure limit. To meet those limits, you look at the equipment that you need. The flow rates, the valve opening times, the diesel generators, the loading times.

All the equipment propagates from the

design basis accidents. You start taking design basis accidents out of the plant, some of the equipment and responses, you don't need anymore.

And they just, like the domino effect. They start falling in and out of the plant design. A broad scope rule, potentially, if you start taking design basis accidents out or redefining them, like dominoes, would go in and out of the plant.

It could be very broad. You could have very large power outbreaks. Big changes to peaking factors. Take diesels out. Take ECCS pumps out. That would be a broad scope change if you were to take a large break LOCA outage, you take many other accidents out of the plant.

Narrow scope will perhaps you could say, well, we're not going to allow any of that to happen. We might change a large break LOCA, but we won't ECCS pumps go out of the plant.

We might allow some peaking changes. We might allow diesel start times to change. We might allow, oh, valve opening times to change.

Well, the perception we get from the industry is they're expecting a lot more for the cost of buying into the program because of the PRA costs. So it's unclear to us exactly what they're willing to do for the price of the admission.

And Glenn will be getting into this, but broad scope would be, as I said, major changes from changing the design basis.

MEMBER LEITCH: And doesn't that take away the mitigative capability beyond redefined, or breaks beyond the redefined LOCA?

MR. RUBIN: Not necessarily. Because it would be a subsidiary requirement put on top of the allow changes. Remember, a design basis accident requirement has all kinds of goodness requirements, like qualifications, qualified models, oxidation, peak clad temperatures, to the GDC requirements.

It's a very strict, qualified analysis methods. And risk analysis, PRA best estimate methods, we would say core coolable geometry, retain the field in the vessel, don't fail the vessel, don't fail containment, keep the core covered.

We want to have thermal hydraulic success with best estimate methods, high confidence, in the best estimate sense, that we have the core covered and cooled and intact, but not qualified Appendix K methods.

And there's a lot of space between best estimate methods and full qualified methods.

MEMBER LEITCH: Okay, that helps, thank you.

MS. MCKENNA: And again that was the area where I was just saying that you need to, I'm sorry.

MR. RUBIN: You're giving up a lot of margin, but as long we have come confidence that you're maintaining the vessel and the containment, public safety is assured.

MEMBER BONACA: The answer to the question in the beginning you implied that there wasn't, but in reality you're going to best estimate. So therefore, you're giving up a significant amount of margin.

MR. RUBIN: Yes, and we think that's appropriate here.

MEMBER WALLIS: So what will happen is instead of worrying about whether thermal hydraulic codes are good enough for design basis accidents, we'll worry about whether they're good enough for evaluating PRA success point criteria.

MS. MCKENNA: Yes. But hopefully we won't worry about them as much.

MEMBER WALLIS: We'll still have to assure that they're good enough.

MR. RUBIN: Yes, sir.

MS. MCKENNA: Yes. Let me move on to just a few more comments about the SRM. This is another one that was in there about the licensees who seek the benefit of the redefinition should use best estimate, and I put in parenthetical, ECCS evaluation codes, because that's where, in the SRM, it appeared under a section that was discussing changes to ECCS evaluation methods, in particular the proposals on Appendix K and other things.

So, again, we've read this one as saying, well this would seem to suggest that in doing, as one of those prices of omission of kinds of things perhaps that Licensees who wanted to take this voluntary alternative would need to use, as we interpret it, 50.46 best estimate codes for presumably, again, there is where you had interpret if whether that meant for the breaks being removed, the breaks that remain, not sure.

So that was another area where the SRM generated questions in our mind about what the Commission really wanted.

MEMBER KRESS: Does that mean that they will still have to use their best estimate code to evaluate a large break LOCA?

MS. MCKENNA: Again, I think --

MR. KELLY: I wasn't clear to us exactly

how the Commission wanted to apply this. And we've indicated, in fact, to the Commission there are some potential roadblocks to using best estimate codes because we don't have a suite of approved best estimate codes for small breaks.

So if they wanted it for small breaks, we'd have to go ahead and get the industry to develop those, submit them and we'd have to approve them before we'd be able to actually apply this, if that was the Commission's desire of how we would proceed.

MR. RUBIN: There's also, in fact, we just had, there's a nuance here. These are approved best estimate code, 50.46 large break LOCA codes.

And there may even be some space between approved best estimate codes and unapproved best estimate codes. Namely, there could be ones with even less margin available that would be acceptable, as Dr. Wallis indicated, that would give us appropriate confidence in coolable core geometry.

MS. MCKENNA: The next one I think is one that you'll all recognize this statement, I think, that was in our SRM and certainly posed a considerable challenge to us that, it was a statement. That once the standards are in place, the PRA should be Level 2 internal and external initiating event, all mode PRA.

It's just been subjected to peer review process and submitted to and endorsed by the NRC.

That was obviously a very high standard that was being placed on the PRAs that might be used for this application. But, of course, as you are aware, the Commission has subsequently provided additional guidance on the area of PRA scope and quality and is part of the action plan that those considerations are being taken into account.

But this was something that was explicit in the SRM on the LOCA redefinition and during the course of our efforts over the last year of something we were looking to see how we were going to try fulfill.

And the last one, is another kind of interesting point. Again, it gives some unique aspects of this rulemaking compared to, perhaps, others.

There was a statement, you know, the direction was to do the frequency review and then on a ten-year cycle to do some re-estimate of the frequencies to see if they've changed significantly with some, I think there was like a five-year look for new mechanisms.

MEMBER KRESS: I have a question about

that.

MS. MCKENNA: Yes.

MEMBER KRESS: The first comment, as opposed to a question is, large break LOCA frequencies are rare events. Ten years is not going to change any estimate you have right now. That's comment Number 1. Comment Number 2, it's been my impression that large break LOCAs generally contribute relatively small amounts to risk.

And, so when one talks about the frequency associated with it, you're going to maybe choose a frequency that still, the break size you choose is contributing a small amount to the risk.

In terms of the LOCA contribution, not in terms of changes to the plant. Is that the correct interpretation of, I think, Number 1, large break LOCAs are relatively insignificant in risk space, and Number 2, even when you choose a new one, the large break LOCA, the new LOCA you choose is probably going to have a relatively insignificant contribution to risk.

MR. KELLY: It's our understanding is, in the PRA world and I think in most places, that because we've actually designed the plants to handle large break LOCAs, all the way up to the double-ended guillotine break, that we're actually very happy to see that they are small risk contributors.

And it's really other events normally that involve multiple failures of equipment that tends to drive the risk numbers. And it certainly would be our intention, as we put forth to the Commission that it's our thinking that we would expect it no matter what happens, that the contribution to risk from LOCAs in general, and in particular the larger LOCAs, those that were removed from the design basis would continue to remain small.

MEMBER KRESS: That, to me, implies that just the thinking of looking at frequency and risk contribution of LOCAs, in terms of redefining the size, is the wrong way to think about it.

MS. MCKENNA: I think we've wrestled with this because we kind of have a foot in both worlds. One of the reasons I think you consider the issue with respect to break size, is so that you know how to deal with it in the design basis deterministic space and you say these breaks are still in my design basis, still handled the traditional way.

I still have my 50.46 analyses showing me, and I know when to stop doing those. But you're right, I mean, you know, you didn't need to use the break size. You could have used some other part of the set of all the requirements to say I'm changing this one and that's going to kind of get me to the same place.

But that was the way it was kind laid out, so that's how we've been trying to respond.

MEMBER KRESS: You'd have to respond in SRM, the way it's put to you, I understand.

MS. MCKENNA: Yes, yes.

MEMBER LEITCH: I would have thought that the third bullet would have said something about the PRA would be updated every ten years, based on operating and experience.

And if that made the CDF or LERF unacceptable, that the changes would have to be reversible. Why is it just based on LOCA frequency?

MS. MCKENNA: Well, we really, you know, I can't speak to how the SRM got written. This was the statement that was kind of there. I think in the paper we were kind of speaking more along the lines than you were.

That if you're looking at affects over time, it may not be the frequency that changes, it may be other things that change and how does that affect, you know, this question of reversibility?

It can be, you know, the concept was introduced but, you know, I think we all hope that we're not going to ever be in a space where we actually have to, quote, reverse something that we're not so, either that the frequencies aren't going to change that dramatically or that we weren't so close to the edge on where we, where the changes were made, that some change in the frequency would take it from okay to not okay.

But, yes, we did, if you saw in the discussion of reversibility, we were asking that question about, well, suppose it's something else that's driving it.

MEMBER LEITCH: Right.

MS. MCKENNA: Does that, does that possibility apply?

MEMBER KRESS: But, along those same lines, you know, I might have expected to see something like, there is a risk level or maybe even a balance between CDF and LERF that's unacceptable to us, including uncertainties.

And therefore, if at some point in time you update and change PRA and plant conditions, show that you've gone outside those boundaries, then you must do something to get back in.

Not necessarily reverse any changes we do to this, but you must do something to your plant to get you back there. And I don't care what it is, as long as it does the job and does it without a lot of uncertainty associated with the way they do it.

That seems to me like that would be a more reasonable thing to do, in terms of risk and --

MR. KELLY: That's what we propose to the Commission is how we would interpret reversibility.

MEMBER KRESS: Yeah, okay, so it doesn't necessarily have to be reversing a particular change. It might be --

MR. KELLY: No, we would --

MEMBER KRESS: -- I believe something else to get you back.

MR. KELLY: And one could do it by physically reversing the change, or one might choose to perhaps change procedures, modify other equipment or the things in a plant such that you achieve the same type of --

MEMBER KRESS: Except, you know, it's always this thing. You don't want to use procedures to, instead of hot wiring the stuff.

But, you know, the other thing that bothers me is I wouldn't have cast this in terms of

LOCA frequencies. I'd cast it in terms of some acceptable, unacceptable risk.

MEMBER WALLIS: I think it has to be cast in terms of PRA and its reliability and believability. All they need is a couple more Davis Besse's and people will say, look, you didn't know enough about these things, you better go back and be more conservative.

And that's a perfectly good reason for changing your philosophy.

MR. KELLY: I wanted to go back to Dr. Kress' comment about in ten years you're not going to expect to see and changes in the frequency.

We certainly hope that we don't change the LOCA and do any real experience associated with large break LOCAs that would cause us to change the frequency.

However, we have seen that there have been, over the years, a number of unexpected degradations in piping, that had not been predicted. And I believe that this ten year period that the Commission chose, was to act as a monitoring device to make sure if these type of new processes evolve and become apparent to us, that we would then take that into account, in our prediction of the frequency of these LOCAs. And if the new numbers turned out to be much higher, then we'd want to go back and potentially reverse some of these changes that we've made.

MEMBER WALLIS: Didn't someone say it would be seven years? Someone presented one of these things, every seven years there's a new materials degradation.

MR. KELLY: Yes. Mike Mayfield has noted that there seems to be a periodicity to the occurrence of new phenomena.

MEMBER FORD: Could I ask a question on the word unacceptable? Could you put a metric on that?

MS. MCKENNA: Well, I think this was part of the challenge to the Staff to determine what would be unacceptable, but it kind of goes back to what's the cut off or what was acceptable in the first.

And then what might lead something to become unacceptable.

MEMBER FORD: From the conversation I've been hearing, would it not be, unacceptable would be a delta CDF which contravenes 1.174. And then how that feeds back into the LOCA frequency? Is that not --

MR. KELLY: There potentially are two ways.

The way we presented the concept to the Commission about how the rule might be written. There are potentially two ways that it could occur.

One way it could occur is if something were to happen, new experience to evolve. We've got a better understanding about some aspects of pipe break frequency.

It would cause us to believe that the frequency was much higher. In that case, we might go back and say that's a reason to reverse it.

The second thing might be that there were some changes that might now actually, the other way of looking at it is there could be changes that occur that could affect the changes in core damage frequency that will predict it based on how they've modified the plant, due to, because, you know, just because we take something out of the design basis, that doesn't change anything in the plant.

Once you've changed the design basis and now you, the utility has the go ahead to propose making changes, and make changes to the plant. You don't have to physically change the plant, but you're going to modify your core damage frequency.

As so then there are other things that might occur that would cause us, you know, maybe we thought that core damage frequency change is only going to be ten to the minus six, and some big thing occurs and it's five to the minus, ten to the minus five.

Well, maybe we don't like that and we think that they should go back and change that. Exactly what the numbers are and how that would be, still has to be determined, but it would probably be frequence of LOCAs and some kind of change in risk.

MEMBER FORD: I'm going to follow up on the comment that Dr. Wallis made about the ten years, and your reply. If the concept is that ten years is going to be the buffer to cover what we don't know about, for instance, materials degradation.

I think it's optimistic in the extreme. I've got a horrible feeling that well before ten years is out, we're going to have another equivalent to Davis Besse of one sort or the other. The history is telling us that.

And so if that's the reason for the ten years, it's very optimistic in my view.

MS. MCKENNA: Again, I think that this was perhaps in a statement that in ten years there should be kind of this comprehensive kind of re-estimation. And there was a second sentence about a five-year look.

And I think Rob is probably going to get into this a little more later, in fact, maybe now.

MR. TREGONING: Rob Tregoning, Staff. I hesitate getting on the mic now, since I'm going to be on the mic most of the afternoon. But just to make this clear, I think what Eileen and Glenn said is both right.

We're setting up procedures to make this more of a continuous evaluation. It's not like we're going to bury our head in the sand and then every ten years pull it up and see if aging has affected us to a greater extent than we already have.

Like Eileen said, to ensure that we do a comprehensive frequency re-evaluation every ten years. But if we see things, and the other thing, it will give us a chance to build up some, there's two things that could affect LOCA frequencies.

One, unanticipated aging or aging that we didn't characterize properly. The other thing that could happen is there could be allowable plant changes, and changes in plant operation that might develop a different experience base and we might realize that our experience base of data has changed in some sense.

So we need to look at both of those aspects, and that's why I think the ten years is sufficient for that. But, we certainly have to maintain vigilance and on a continual level to evaluate degradation and try to understand its implications when it surfaces in the plants.

So that's something that certainly plants are to continue to be as vigilant as we can be to make sure that we're assessing these challenges as they arise.

MEMBER BONACA: But one thing that happened was that Davis Besse was not considered, and in the elicitation process, was excluded. And the question I have is how many other similar events are going to be excluded?

I mean should you have a repeat of the degradation, say, due to bolting, for example, in the head that results in some leakage, etcetera.

At some point we'll have to face the fact that in fact some of the electrical and system degradations and resulting in leakage is coming from other kind of sources.

I mean you just, I'm trying to wrestle with that issue because, you know, I may come and have some event five years from now, while you have something similar and has nothing to do with the present destinations process.

And say, well, you know, we can't account for that, so we do not include it. And then I'm going to worry about what Dr. Wallis was pointing out before, some deliberate events that might cause leakage.

And yet, I'm not going to include them. So I'm wrestling with those events that we are excluding right now from the database. And I recognize that there's no way to include that now. But still I'm left with --

MEMBER FORD: But surely you can include it, Mario. For instance Davis Besse, fortunately it didn't give rise to a large break LOCA or medium break LOCA.

But you could reasonably say that within the next six months it might have done it. So surely if that occurred, surely that should fit in here now.

Maybe that's a question for this afternoon.

MS. MCKENNA: I think that one might be.

MR. TREGONING: And again, at the risk of getting too much into this afternoon, we did consider Davis Besse-type events and based on the current state of knowledge, what people thought were potential failure modes and mechanisms that could occur in the future.

And it doesn't mean knowledge is perfect, where we're standing now trying to project. And in the elicitation we asked people to project all they way out to the end of license extension.

Well, that is going to be a difficult process. But what we ask people to do, based on what we know now, based on our operating experience, based on not only degradation that we've seen in the plants, but information that people have based on laboratory experiments where we've tried to project degradation into the future.

What's your sense for how these things are going to evolve and the challenges that we could face in the future. And I think the only point of this last bullet is to say, we certainly recognize that the current knowledge isn't perfect.

If we were able to actually predict what was going to happen in 30 years, we'd probably all be in a different line of work. But given that, let's make sure we have a mechanism in place, that we can continually re-evaluate these things.

And reassess ourselves or recalibrate our thinking every so often, to take into account, again, things that maybe we didn't consider because we just didn't think it credible at that time.

MEMBER FORD: Let me ask you a program management question. Which takes into account this topic. The research has got proactive materials assessment program on the books, at least, which will answer some of those questions you just brought up.

In this overall risk-informing 1046, is the timing such that outputs from that proactive materials management program will be input, inputted to this or would this particular program on 50.46, will be finished off the books for the next three months or whatever it might be?

MS. MCKENNA: I was, certainly we're not going to be done in the next three months, I think that's a fair statement. We're trying to keep cognizant of all the activities that the Office of Research is doing.

Certainly the frequency work that Rob is doing is a very important input to what we're doing, and he'll be discussing, you know, the state, where he's at in terms of having that be complete.

If there are other things that bear on this, you know, obviously that's something we will need to consider. But I think as you saw from our paper, we're a long way from having, having this project complete.

There's a lot of issues and work, not just in materials areas, that we think we need to develop. So we're trying to understand what all those things that are doing and feed them in and if there's additional information we need, to identify that, so we can go out and get it.

But, so that's, I think that's kind of my response on that.

MEMBER FORD: Okay, so this is going to go on for quite some time, so inputs from this proactive management could be put into it?

MR. KELLY: Well, I think that, I think you have two different aspects here. I think as we're developing the rule, as we're doing the part that we're going to take into account all the information the research gives up that, you know, our Division of Engineering provides to us, so we can take into account all of this and try to craft a good a rule as we can at the surest of public health and safety.

And then beyond that, as time gives us additional history and we learn additional things, we will build feedback mechanisms into it that would, as we say, if we see something that seems to require reversibility, then we want to provide that ability so that, you know, if we get knowledge it tells us, you know, we should have done things differently, then we can go back and change things.

MEMBER LEITCH: Have you thought about this reversibility as being a, on a fleet basis or an individual plant basis? For example, let's just say that Davis Besse causes us to change our perception about LOCA frequency.

And some other plant says, well, yeah, I've got a good boron-controlled program, inspection program. In fact, I've just replaced my head. Why should I be penalized for something that happened at another plant that certainly could never happen at my plant?

How do you, how do you plan to --

MR. KELLY: My expectation --

MEMBER LEITCH: -- why should I be reversed, so to speak, because of something that happened up the road.

MR. KELLY: I think it would depend on what was the mechanism that's involved. If it's a mechanism that is, would involve all the plants or maybe involve all the PWRs or all the BWRs, then it may be appropriate, on a generic basis, to require them to deal with this on a reversibility basis.

If it's one where we see that there's something that maybe, there's certain aspects of a design that causes it to be more vulnerable. Or maybe some plants have more mitigating capabilities than other plants, and therefore they wouldn't have to, they wouldn't have such a change in core damage frequency or risk associate with whatever this new mechanism is.

Then they might be okay. They wouldn't have to do anything. So I think it would depend on whatever the actual mechanism was.

MEMBER LEITCH: I see that as a potentially contentious issue down the road.

MS. MCKENNA: Well, I think that's one of the reasons we, I think that whatever this is going to be, it needs to be thought out and developed and written down so that we can have the contention now as to what the process and the requirements are, rather than if it comes up in the future.

Because clearly, as he was indicating, Glenn was indicating is that, you know, if you recall this has been a voluntary alternative and we don't know yet what changes to an actual plant might actually result from this.

And so then when you get new information about, well, this potential LOCA initiator has increased substantially. You know, did anything in the plant that was changed, you know, how much is that really going to affect those set of sequences and the change that might occur there, as opposed to another plant that might have made other changes.

And secondly, whether that particular change in mechanism or experience applies to those plants. So, it's not a, I don't think it's a simple question. I think this kind of drives to a need for some process, if you will, in the rule.

Whether that's a strict reversibility. Whether it's this, some kind of cumulative impact type of, you know, as I said, you know that there some level you can't go beyond whatever is driving you there. And that if you reach that, you have to take some action.

Those are the kinds of questions that we are, are thinking about.

MEMBER LEITCH: Can this reassessment be done by the Licensee? In other words, are we implying here that a ten year update of the PRA is implicit in this process?

MS. MCKENNA: Well, I think statement really was a statement for the Staff to look at the frequencies. Depending on the results that might lead us to do something with the Licensees, in terms of what PRA updating requirements will be necessary.

I don't think we've determined that. I think that's going to come out of some of the PRA quality type of initiatives.

MEMBER KRESS: Least, lest we leave you with the impression that we all think there's PRA problems with things we've left out in Davis Besse and degradation mechanisms. That's the whole completions issue that's been around since WASH 1400.

And some of us think you deal with it as best you can, in PRA space. And with respect to sabotage, I think you can do some things in design basis space to deal with sabotage, but not very much.

And that's why I think we tend to do a separate PRA sabotage and deal with it as best we can outside of the design basis, but while still making the design robust.

So I'm not as concerned as some others about that particular type of event. But given that preamble, I have a question about mitigation capability here.

And if one had in its interpretation a defense in depth, that certain safety functions, one of which would be ECCS, are so desirable to have and maybe uncertain in PRA space, that you want to assure that there's redundancy and diversity in those.

Now what I would expect one then to expand on it and say given this large break LOCA definition I've come down upon, I only need one of these. But I've got two of them.

Wouldn't that almost automatically take care of your mitigation capability for the larger break LOCAs? Or could, I think?

MR. KELLY: There's, depending on any additional changes that were made to the plant, if I, let's take a hypothetical situation.

I could take, say, all breaks above six inches and say that those breaks are now --

MEMBER KRESS: In the some other space.

MR. KELLY: -- in the new space. And it turns out that I may be able to get away with, therefore, for all of those other breaks, with only one train, you know. Because I don't need to perhaps, I don't have to consider single failure anymore.

Perhaps the, I have put enough reliability with my one train for that. I probably still need two trains to handle my breaks below six inches because I still don't have single failure and a loss of off-site power, coincident loss of off-site power.

But let's just say that perhaps I could significantly extend the outage times on some of the other equipment. I think under those circumstances on the, on the lesser train, that perhaps one train you might be able to, as I say, extend outage times and things like that and that would potentially be acceptable.

Where you could run into problems is now if I've changed, if I've taken these breaks out of my design basis, I may be able to therefore make significant power uprates that, and so that I still retain myself within the design basis, that I still have adequate peak cladding temperature, but what's going to turn out is that now for the larger breaks, I may no longer have adequate ECCS capability to prevent these breaks from going to core damage.

So that's something that we have to look at, is how you would handle that type of situation.

MEMBER KRESS: That would be the one that would worry me.

MR. KELLY: And there may other things that we haven't even thought about that are other areas of things that they could change. But these are the type of things that we have to consider when we're looking at, because there, you know, I haven't, I haven't reduced my ECCS capability, but what I've done is I've increased my requirements on the ECCS capability to have an okay result.

MEMBER KRESS: And I think that's a real legitimate worry. I made a, I can only call it a back of the envelope estimate that for some plants, if went down to a six inch size for the break, that could mean as much as 40 percent power uprate, and still stay within the ECCS requirements.

And that's a significant change. So that's the one that would worry me more than anything.

MR. KELLY: And what that does is it also, the other thing it does, if you had that power uprate it would probably make things happen a lot faster because --

MEMBER BONACA: But you'll still be probably, be limited, I mean, at some point, you have some limits of how far you can go up?

MR. KELLY: Ordinarily it would be, that would be the expectation.

MS. MCKENNA: And some other break would become limiting in 2200 and there you are.

MEMBER KRESS: Yeah, most of the plants could stand that anyway because the steam generator --

MS. MCKENNA: Well, yes, obviously there's other aspects that have to be brought in. Okay, I think we're finished with just the background on what we were facing with the SRM.

So then the question is what are we going to do about that? As I mentioned, we have a working group that had been originally developed to work on some of the GDC 35 functional reliability kind of tasks.

When we got this SRM that had this, you know, kind of put more emphasis on, they get the large break redefinition and, you know, a very short time frame is what the Commission desired.

We kind of refocused the efforts of the working group on this task. And I think the first thing we did was go through the SRM and try to understand what it was telling us.

Some of the implications of it. If it really says redefine the break this way, and carry it through, what would that really mean? And what kind of information would we need to support that?

And what kind of criteria might we need to help us make decisions as went along. So that was a fairly intensive effort to really try to delve in.

And as I mentioned, we did come to some cases where we couldn't agree really as to what really was the Commission's intent? You know, we could read it, some people read it one way, some people read it the other way.

And some of those areas, the ones you see in the paper, we kind of went back finally and said, well, you know, Commission, we really would like to get some further guidance on these issues where, you know, we really aren't sure we're fully understanding what the Commission had in mind.

I think the other thing we tried to do was to, since this is supposed to be a voluntary alternative, you know, maximum break size, we could try to get some sense from the industry of what kind of changes are process of a rule might they be thinking of?

And, again, I think there was some mixed, people has different ideas. There were some that were looking for, maybe broader changes, more extensive changes than others. And, you know, there were, obviously power uprates is a considerable area of interest because it has certainly a lot of value for a Licensee who could take advantage of that.

And in some cases, they may, large break may be the limiting factor with respect to those. But certainly there were other things that might give more flexibility and some of those kinds of things, Glenn was alluding to, that, you know, could also provide benefits.

So we did have some discussion about, and also this kind of, you know, well what kind of, you know, is this a, is this a, do we all get a process where the Staff is reviewing every single change?

Or is there some way to set it up that you can get reviewed once and then, you know, kind of have an envelope within which a Licensee could make changes provided that certain criteria were met.

Because there's obviously certain advantages one way or the other, in terms of how much review time or, you know, that might be involved.

So that led us into the last bullet on this page, which we gave some thought to, well, what kind of rule might we write? I mean there was this, okay, we could go redefine the definition somewhere, but then there was all this, well, we need to be thinking about, well, what are these criteria that might need to be satisfied.

You know, where are we going to put

requirements on PRA and how are we going to address these different things? And do we want to stuff them into, you know, 50.46? Or does it make more sense to, kind of like we did in 50.69, to say well let's kind of make a new place and put down here's a new way of doing this that here's what no longer applies and here's what now does apply, and put it down in that kind of fashion.

And then also define whether, okay, here's the specific changes you can make. The kind of changes that you could not make or the criteria that would be used to judge whether or not the changes in a particular case would be acceptable.

Part of that, obviously, would be this question of what the new break size was going to be. So, and I think Glenn mentioned this earlier, that we were trying to do this in a very integrated kind of manner, you know, to be risk-informed, to bring in all the considerations about defense and depth and cumulative risk and all these things so we're not just relying on, you know, the low frequency of the initiator.

And, as I said here, adequately monitored and controlled over the lifetime. So, as things change, are the processes there to make sure that we're getting the outcomes that we're looking for.

In the course of doing that, we kind of came to some of these issues that you see in the paper. Like mitigation is a key one. How are we going to do this? Do we take it out of the design basis space and put it somewhere else, what is that going to be that new safety envelope, if you will, that says you can go this far and no further in this area, because we still want mitigation.

And so that was as an example of some of these issues and Glenn will be getting into those in a little more detail.

And we also, as I think I alluded to earlier, in some cases we said well maybe there's some additional technical work and research that we might want to do to look at some of these things.

And there were some activities initiated to look at the, either thermal/hydraulic affects, if you will, if you're making power uprates or other potential, you know, accumulator changes, things like, that as well as risk assessments of how might that translate for some representative plants.

Kind of give us an idea, you know, of where we might want to go with these things. Obviously, we'll have to consider plant specific aspects, but, you know, if we were going to define, for example, particular changes that one might could or could not do, we would have some sense of what those effects are.

And so that work is, I'm going to say power uprates is a particular area. So that work is ongoing, but is not yet complete. Going back to the question over there, is that we do need to bring the pieces together at the right point and time, when there work is done and when we're ready to move forward on this.

MEMBER FORD: Are we going to see anything on this issue, like hydraulic --

MS. MCKENNA: At some point n the future. The work is not yet complete, so there are no results to present as yet. But at a later date, we will be sharing that information when it's available. As I said, we had this tasking to prepare the proposed rule and we were wrestling internally with, well, okay, what kind of rule could we really do that would be responsive and that would maintain the principles that we were talking about.

And we finally reached the point of saying, well, we're not sure we can really deliver on the scheduled rule that is responsive because of a number of the issues that I've already mentioned.

And we had a briefing with some of the Commission Assistants and we discussed the issues that Glenn will be sharing with you in a moment.

And then we, and we got kind of this sense of the Commission, well bring these policy issues to us, we would like to give you that guidance. So we did turn around then, in relatively short term from that time, and put forward the paper, the SECY 04037 that tried to weigh out for the Commission what we saw as the policy areas.

Where we wanted the direction. And also indicating that one of the reasons we needed that sense of which way should we go, was to help us solve the technical issues in the appropriate manner.

So that's kind of what led to the paper that we sent forward. At this point, I think maybe Glenn and I will switch chairs, perhaps.

MEMBER FORD: Could I ask a question?

MS. MCKENNA: Certainly.

MEMBER FORD: It seems that we're hearing a lot about the impact on the regulatory issues, but not an awful lot in terms of data, assumptions, etcetera, on the technical issues.

Has anyone done a kind of back of the

paper envelope calculation as to how much time it's going to take to overcome these technical issues or resolve these technical issues?

Especially with the uncertainty associated with them. Has anyone done that? To see whether you can meet the goal in a reasonable time period?

MS. MCKENNA: I think we've, when identifying the activities that we've laid out a little bit later in the slides that were discussed in the paper that we saw preliminary assessments of, you know, how complex, you know, do we think this could be done in months versus years?

We haven't finished that work. We haven't laid it all down a page and said, you know, this one's going to take three months, this one's going to take two months. This one takes three months, but it can't be done until the first one is done.

And therefore, you know, the total time line is X. But that's the kind of work we are doing.

MR. RUBIN: It's also, Mark Rubin, again. It's also predicated on the guidance from the Commission on which approach they want, broad versus narrow. It's a lot easier on narrow, it's much harder on broad.

And I think we said we were able to go up, our rulemaking plan or action plan within six months?

MR. KELLY: Six months.

MR. RUBIN: Of being told which approach to take.

MR. KELLY: I just wanted to make one comment to something that may help, make things a little bit clearer about the narrow and broad rule.

I think as we envision the narrow rule, not only was it narrow but our expectations were that it would fairly prescriptive in a sense that might well say, you know, these are the following things that you're able to change and you can only change these things.

And that's part of making it narrow or making it possible to do something of an easier basis. Our expectation for the broad rule would that it be more of a process-oriented rule.

Whereby one would build into the process the checks and balances that are necessary to assure that you get an appropriate result. This would, in turn, require a lot more effort on the staff then potentially the industry, to assure that you're getting good results.

But it also would give much, much more flexibility than the narrow approach. So I have a, in looking at these issues that came up as we tried to respond to the SRM, we came up with a number of tricky, technical and regulatory issues that I'm going to talk to you about.

The first is, in essence, what are really the right criteria that one should choose for determining what's the appropriate new maximum break size.

As you know, as Rob is going to be talking about later this afternoon, there's a elicitation process that's going to used to develop these new numbers.

And these numbers will be the latest and best estimates that we have for what are the LOCA break frequencies. We need to understand, given that they're the best that they are, how much confidence should we have in those numbers, even though they're the best?

Is the best good enough in this particular case to allow us to modify the regulations? Or given that there's going to be a significant uncertainty, how much is the uncertainty and then how do we take into account that uncertainty when we set some level at which we want to say, okay, you know, breaks above this can be excluded and breaks below this should be included?

I think that that's going to be a very challenging and interesting evaluation to determine that.

MEMBER SIEBER: How far along are you in the process of determining the large break LOCA frequency?

MR. KELLY: Well, Rob will be talking about that. I think, I believe he'll be telling you that the initial numbers have been put together. That there, there's some additional work to be done, but a lot of the documentation of the work has already been performed.

MEMBER SIEBER: So if I wanted to ask detailed questions, I should wait?

MR. KELLY: Yes, please.

MEMBER KRESS: Are you giving any consideration to allowing individual plants to choose their own break size?

MR. KELLY: We've, that's a potential. They could come out of it, but we have indicated that we think that that's not the way to go, because that would be a regulatory nightmare with everybody having different break sets.

MEMBER KRESS: There would be some

problematic, but it could be done.

MR. KELLY: Yes, it could be done. It would be a lot more work for us and for the inspection work that would be done. But it has, it potentially could be done.

My expectation --

MEMBER KRESS: But for right now, you don't think that's a way to go?

MR. KELLY: That's correct. Our thinking is, right now is that we would probably break it out and maybe you'd take PWRs and you could take maybe old and new BWRs. Or maybe you'd take certain LOOP, PWRs, there are a lot of different ways that it could be done.

And you may even have, I guess you could have a different break size, say for like in BWRs with recirculation, movement and everything else in the plant.

MR. RUBIN: This is very preliminary still. We'll be looking at the work from research. But your point, you know, is absolutely correct.

You could make it absolutely risk-informed and generate it, back it out of the PRA, it could be done that way. But every time the period changes the break size definition changes.

So, it gets pretty confusing. So, maybe just in the name of simplicity, develop based on the frequency.

MEMBER KRESS: Well, you might avoid the problem if every time the PRA changes your break size changes by specifying a desired level of confidence in your selection in the break size.

But I don't know how you arrive at what's the right level of confidence, because that has to be based on what it does to the risk and once again you're back into that mismatched space --

MS. MCKENNA: Well, I think that's what we talked about earlier, where there's kind of a part of the process where you're selecting the break size and using it in a particular space. But there's also the part of the process where are, absolutely have things like plant changes and what is the impact of risk of those plant changes, given that the change in break size, if you will, has enabled those kind of changes to occur.

Because before you would have said, you can't make this one because you have to be able to mitigate that double-ended break in this particular way.

Now that requirement is no longer on the table. Here's a change that might considered, but you need to consider it with some other criteria in mind, and make sure that you don't lead to something that you weren't expecting.

MEMBER FORD: I must admit I don't understand why you're so against this plant-specific decision making. For instance, surely analyses have already been done on the difference in the probability of failure for, say a 316 nuclear-grade pipe versus a 304 pipe.

So if a plant has elected to this, they know, reasonably well, that the probability of pipe failure will be much lower if it went through this mitigation action.

So why are they being discriminated against, if you like, in this decision making process?

MR. KELLY: Well, in some cases, I mean that may, it could be one possibility of the way we do it. But there's also a trade off on resources that it would take to do this work, and for us to review it.

Also, it's not, and you'll have to talk to Rob about the extent to which we have developed frequencies within different typing materials and that, you know, if it goes down to the actual stainless steel, you know, which alloy they're using.

I don't think that they're probably down at that level. But --

MEMBER FORD: This afternoon's discussion is going to be really good.

MR. KELLY: Yes, yes. And I exhort you to just think those all up for Rob.

MEMBER KRESS: Well, let's talk about this Staff resource issue just a minute. If I specify some size break that's the new definition, and that implies okay, I've got this new break. I'm going to do this, this and this, changing my plant.

You've got to review all those, right?

MR. KELLY: That's correct.

MEMBER KRESS: Now, how is that more resource intensive or less resource intensive than saying, he's going to come in and say, this is the size break I want to use and based on that size break I'm going to do this, this and this. Isn't that the same review?

MR. RUBIN: No, part of it is, Dr. Kress, personally I don't believe it's a research issue, I think it's a regulatory consistency issue.

I think having different design basis LOCAs for 103 plants really introduces some lack of consistency, some lack of public perception that would make our job more complex than it needs to be.

I think the way we'll be approaching this we'll be very risk-informed. We'll allow plant unique features to propagate appropriately in a risk-informed sense through the plants, and we don't really penalize them by using the direct LOCA frequency curves developed by the Office of Research.

And I don't think we lose much for making the primary determination of the break size frequency base to start the process, and then let it propagate through being risk-informed through the rest of the process.

We could use your approach, but I don't think we lose much by starting with the frequency-sized curves.

MEMBER KRESS: Well, I think that argument is better than the resource argument.

MR. KELLY: The second technical area that I wanted to talk about is this, I think it's important for us to understand, what are the real practical effects of taking design basis events, formerly design basis events, out of the design basis and how does that propagate through all of the criteria method we have.

And as I mentioned before, there are

potentials for changing things like equipment qualifications, site containment, your radiation doses to workers, many different things that could come out of this and many of them are not clear at this point and we have to think about them very carefully.

When we go to change what's in a plant, you know, deciding what can be changed and how could it be limited. One of the things that we've wrestled with on the process-type rule or the broad rule is how does one actually go about limiting these things without, I mean, we try to write fairly streamlined rules. At least that's the theory.

And one could see that this could become a very convoluted rule in order to try to box in all of the results. And that's, and that comes from, the first part, if you don't understand the first part, which is the effect of taking it out, then how do you box it in to make sure you're not getting the unintended consequences or unacceptable consequences.

So I think we're very interested in these two really kind of go together. And this third part is really to talk about whether we're going to have a narrow scope or broad scope rule.

As was mentioned before, the narrow scope rule, the more specific rule is something that we believe could be more easily developed. The broad scope rule would require, I think, much more thought and careful preparation on our part.

The fourth issue is about the mitigation capability. This is really an area that's totally new. It's something that it would, where it is, where it stands in regulatory space, where it stands in legal space.

What it means to be something that's not in the design basis, but we still have some kind of regulatory requirements on it.

What the appropriate regulatory requirements are? Where do you place it? Is it something that goes, and somebody said FSAR. Somebody something, you know, is it in their license? How do you actually go about doing this?

Those are some interesting issues. Just determining what is the appropriate level of mitigation, and then once you determine that, how do you go about demonstrating that mitigation.

Once we're going, are we going beyond 2200 degrees F? Are we allowing some core degradation by retaining it within the vessel? How are we going to assure that?

There's a lot of uncertainty. The further out you go in a core damage event, the more uncertainty that arises. So how much certainty do we require?

All interesting questions to be answered. The fifth one comes back to, I think one of Dr. Kress' points, is how do we assure adequate defense and depth?

I think Reg Guide 1.174, has done a very good job of listing some examples of ways that one goes about assuring adequate defense and depth. But again, that was based on retaining all of the regulations in place.

One of the things that we've indicated to the Commission that we want to do, is to look and see are there additions that we would propose, beyond what's in Reg Guide 1.174, that may be necessary to help assure that adequate defense and depth is retained.

Or do the, does the guidance that's in Reg Guide 1.174, does that need additional clarification to make it more easy to apply it in a particular set of the utilities understands it more clearly what it means and the regulators are in a better situation to say, yes, that's in or that's out.

MEMBER WALLIS: It appears to me this is

the biggest problem you have. And the reason we have large break LOCA in design basis is for defense index, because we don't know?

We say, let's consider everything, all the pipes. It's a very logical explanation. And 1.174 has a lot of waffle about how to apply defense and depth, which is arguable, because there's no measure of it.

So why not, this is I think the place where you're going to have difficulty arguing one way or the other. But the reason we have large break LOCA now, in the design basis, it is for defense and depth reasons. So what has changed about those reasons?

MR. KELLY: Well, that's a good question. And I think what has changed is that we do have a much larger body of experience about piping in nuclear power plants that have been subject to aging and to the various mechanisms that are different from, say, gas line pipes or fossil fuel plants.

So that's one of the big changes. And we do have, and we also have many thousand reactor years of experience that has said that we do have additional confidence, that yes, that these large breaks are expected to be low frequency events.

We have not seen --

MEMBER WALLIS: So if there were some quantitative way of tying defense and depth to uncertainty, I think you can argue that way. But since defense and depth is always called a philosophy, you have difficulty in making a conclusive argument.

MR. KELLY: No, well, but I'm not using this as a basis for saying I'm going to get rid of defense and depth.

MEMBER WALLIS: But how much is adequate, you see?

MR. KELLY: Right, and defense, and that's one of the things, as I say, one of the things here that because it is so effusive, it's so difficult to define exactly what we mean.

And we've received a lot of comments from industry about that and sometimes they feel like every time they propose something that we don't know what to say about it, except if we don't like it, we say, well, it's defense and depth.

And they mentioned that to us a few times. And, but defense and depth is an extremely important aspect of the design. And I think that we will spend some time looking at this and seeing if we can do an even better job.

I mean we had some of the very best minds in NRC working on what went into Reg Guide 1.174. I'm not sure that we can do any better than that.

If we can, we're going to try to do it, but it's going to be a very difficult problem. None of these are easy problems.

CHAIRMAN SHACK: Weren't you almost going to make a quantitative statement of that when you handled four? I mean somehow you're going to have to put a degree of confidence that you're going to mitigate this thing.

And to me that degree of confidence is almost your defense and depth statement.

MR. KELLY: That's part of it. That's part of it, I mean that's part of it in the sense of assuring that that capability exists.

In the sense of you're going to have the conditional core damage probability part of it. I think that the, it's not going to be a strict core damage frequency aspect, because my own personal opinion is we're going to have exceedingly large uncertainties associated with very large break frequencies.

And when you combine those all together, it will be very difficult to come up with a --

CHAIRMAN SHACK: But if you use a

conditional thing, then you get around that uncertainty problem.

MR. KELLY: That's correct. That's correct. And that maybe the type of thing that we end up with, but and you know, we talked about resource problems.

One of them is that we only have so many risk analysts, and if you notice, there's quite a few issues here. And to try to handle all of these, it's going to be a real challenge for us, because we have so many risk-informed initiatives that are going on right now.

CHAIRMAN SHACK: This a risk philosopher. I mean before you can do the analysis you have to decide what it is you're analyzing.

MEMBER SIEBER: It seems to me, though, that what we're talking about is whittling away at defense and depth. For example, let's say that you said that it's improbable that you would have a full double in the guillotine break and therefore the break size we should analyze is seven inches or ten inches or what have you.

And there's an implication for containment. Containment conditions will not be as severe with that kind of a blow down, as they would with a double-ended break, and therefore you don't have to qualify all your instruments and cables to the same degree.

And you could allow the leak rate from containment to be relaxed because the driving force and the radiation dose would not approach part 100 as they would.

You could say, well, my break is only going to be seven inches, the zone of influence for debris generation is going to be pretty small, and I don't have to do anything with my sump screens.

And to me, once you whittle away at everything like that, then you no longer have the capability of dealing in severe accidents, because of a lot of scenarios. And to me that's very troublesome.

MR. KELLY: Right, and that's why that mitigation aspect is so important. And why, because we understand, as we talk about in the paper, that it is the robustness of the original design basis, that is what allows us to do as well as we do in severe accidents.

It's not because we designed for severe accidents. It's because we designed so robustly for severe accidents. So I think we have to be very careful as we relax things to look at that.

I mean you are correct, Dr. Sieber, that there will be some relaxation of the overall defense and depth aspects here. We have done things, over time, which sometimes we don't think about as defense and depth, but you know, ATWS was a defense and depth thing that we added.

Station Blackout Rule was a defense and depth thing that we've added. So we have done things that have added defense and depth, and here I think we're looking at some other areas that we can reasonably relax and still, but we still are retaining adequate defense and depth.

But it's something we have to be very careful about.

MEMBER SIEBER: Well, one of things that's troubling is the big accidents that have occurred. Chernobyl, TMI, etcetera, I guess there's six or seven of them over the last 40 years.

They've all had a human error element to it. And when we do PRAs we don't seem to be able to agree on how to treat human error consistently in every instance.

And once you have human errors with a failure path, you're into severe accident space where every inch of concrete and every gallon of water you can pump becomes very important.

And so I just feel uncomfortable taking that away.

MEMBER KRESS: But once you start whittling away with defense and depth, it naturally begs the question of what's an acceptable defense and depth? That's going to be a problem.

And in order to say what's an acceptable defense and depth, we have to get away from the vagueness of the definition that's in 1.174.

It's not really quantifiable in 1.174. You have to, there are lots of things that are defense and depth. Like quality assurance and operating procedures and training.

You've got to forget those things, because we're talking specifically now about, in my mind, design defense and depth. Which can be more quantifiable.

And I would suggest you need things like, there's a set of key safety functions, shut down, ECCS, long-term cooling, containment, maintaining containment.

Those are key. I would say you have to have specifications on the redundancy, diversity, capability and reliability, of those key things.

Now that's just the way we do a number of those. And so you just got to have that, regardless what risk space you're in. And then I think the concept of a balance between CDF and LERF and limits on CDF and LERF are actually defense and depth type of things a long with maybe even the balance among the contribution to the sequences, like they have in the framework document.

But for this type of major rule change, I think you've got to have more criteria than just CDF and LERF. I think, at the minimum, you have to include leg containment failure, as a limit, limiting criteria also.

MEMBER BONACA: Also you have to be specific, for example, certainly you're upsetting the balance between the prevention and mitigation which you had before.

MEMBER KRESS: So you have to say why do we want to have an acceptable balance?

MEMBER BONACA: The question is should you have, I mean the balance was taking care of certain conditions that really were not within the design basis was the extra argument you have.

And now you have two guys are left. The other issue that seems to be central is the human factor. You know, and common cause, potentially.

MR. KELLY: Well, we're, I mean we're doing the best we can when we perform these PRAs to take into account the, you know, the state-of-the-art and human reliability analysis and common cause failure.

But again, and I understand, Dr. Kress. I think that to the extent that we can it's good to have balance amongst these things.

The only problem that I have about going down that road, is that the reason why we have defense and depth, in the first place, is to deal with things that we don't about, that we're uncertain about.

And I think we can fool ourselves into thinking if we, if we, you know, if I just only look at it numerically, and what I can do in my PRA, that I've handled things. I think --

MEMBER KRESS: Yeah, that's why I said you also need the, two define some key safety functions and say, I don't care what the PRA says, you've got to have these taken care of. And to me that will help deal with that issue.

It might not be the complete solution, but it will help, it will help deal with that. What you don't know, I'm just going to say you've got to be able to do this, and in a redundant and diverse way with a robust capability and reliability. I don't know what those numbers are.

MR. KELLY: Right. And that's something that we probably can, type of thing that we probably could prescribe for the current light water reactors.

MEMBER KRESS: Yeah.

MR. KELLY: I think one of our ratios, of course, comes up and, you know, we're supposed to be, what is it we call it?

MEMBER KRESS: Technology --

MR. KELLY: Technology neutral, which is sometimes I feel like that means, you know, technology --

MR. RUBIN: Glenn, let me give the Committee an example of common cause failure in a foreign reactor. Why we consider the mitigative capability beyond the design basis is so important to maintain, it goes into a human factors issue.

They were doing a maintenance evolution, reassembling a major valve, some of the members may be aware of this.

They miss-set a torque wrench and they over-torqued the studs in a major recirculation circuit valve at twice the value, and they were reassembling the valve.

Luckily, one of the studs broke as they were reassembling it. These were contract workers. If the stud had not broken, the valve would have come apart as they were pressurizing the system and of course the valve body would have come apart and they would have had a major, double-ended guillotine break in the primary system.

A non-fracture mechanics break, double-ended guillotine break. If they redefined their large break LOCA, they would have had one.

And it was a common cause failure, contract workers, the torque wrench. And it wouldn't have been caught by any PRA, it wouldn't have been caught by any HRA analysis.

And defense and depth, the approach we're taking, would have, will maintain a mitigative capability success for an event such as that. That was, you know, just not modeled in our risk study.

So, this is the type of thing that we want to maintain defense and depth for.

MEMBER WALLIS: But that sort of event is far more likely than these ten to the minus eight things, and fractured mechanics --

MR. RUBIN: Yes, sir, we believe so.

MEMBER WALLIS: -- and it's far more

likely that someone will pick the wrong setting on a torque wrench.

MR. RUBIN: And far more difficult to quantify.

MEMBER WALLIS: So that's what you're going to defend against.

CHAIRMAN SHACK: I'd like to suggest we take a break at this point. Otherwise, we're just going to keep going on here until noon. So, break for 15 minutes, back at 10:35.

(Whereupon, the foregoing matter went off the record at 10:20 a.m., and went back on the record at 10:40 a.m.)

CHAIRMAN SHACK: We can come back into session.

MR. KELLY: The next issue that I'd like to talk about, Issue Number 6, is what limitations should be placed on the cumulative increases in plant risk under the rule, and how should it be controlled?

Two aspects, they're very important and both of them are very difficult. Do you use ten to the minus four core damage frequency as you number, you can say, okay, well you can only go up to ten to the minus four, including internal and external events?

What about plants that don't have external events? Shut down? Now there are some studies who would seem to imply that shut downs usually equal to about what your internal event numbers are.

Forty to 50 percent of the plants have prior external event core damage frequency estimate than they do for internal events. Sometimes based on having fairly conservative external events analyses because they didn't need to do the more expensive detailed analyses.

So what, you know, what's the right number? That's going to be a hard number to choose. And what do you do, you say if the plant is above that they can't be involved in this. We're going to look at it more carefully.

What does that really mean they'll look at it more carefully, etcetera. How do you actually track cumulative risk? An even more difficult problem.

This is one of the things that showed up in things like the ISI when we've looked at tracking risk. Every time I make a PRA update or I'm improving the PRA based on maybe there's an area I'm modeling that I've decided that maybe my HRA wasn't as good as I thought it was or I looked at something else and maybe I really wasn't doing as good a job as I could of.

Or I just want to spend some more money and improve my PRA in that area. That's going to change my risk profile, probably, somewhat. And it's going to increase or decrease my risk.

MEMBER KRESS: But could you, could you have a, like we do thermal hydraulic codes, could you have a PRA that says this is your current PRA, and all your risk changes due to changes, have to be referred back to that one. Although they can approve the PRA and use it for other things, but in order to track cumulative, you need to track it from one baseline.

MR. KELLY: Well, that's an interesting question. But really what we want to track are the cumulative increases due to changes in the plant.

Rather than cumulative increases associated, or decreases, associated with changes in the PRA. Improvement in knowledge associated with how well the PRA really models the plant in reality.

Assuming that every time we theoretically improve the PRA, we're getting closer to reality.

MR. RUBIN: And the point, I apologize for interrupting. The point Glenn is making is both those changes often get done at the same time, so you can't separate out what's due to what.

MR. KELLY: And so now it can become potentially very costly and very confusing to a utility that has to keep a base PRA where every time they make an improvement in the quality of PRA, they're changing that, but they're not changing changes that they made to the plant.

So they've got to go change this model, and then they've go to do another model over here, which is their up-to-date everything model.

MEMBER KRESS: Yeah, that would be a problem.

CHAIRMAN SHACK: Well, it doesn't seem very risk-informed either. I mean, if, you know, one you think is the realistic picture of risk and the other is regulatory risk, I mean, you know, we just, we went from design basis to regulatory risk to real risk. I'm not sure that I gain a whole lot from that.

MEMBER SIEBER: On the other hand, when you're summing the deltas, you can use any model as long as you run the model before the change and after the change.

Get the delta and add it to all the other deltas that you've got using other models.

MR. KELLY: Well, you can do that, but

here's what happens. I make a change in my PRA that's based on just changing the modeling in my PRA.

That will affect the actual delta that I had done previously.

MEMBER SIEBER: That's right.

MR. KELLY: Because maybe that delta that I had done previously was small, now maybe if I do it now it will become very large.

MEMBER KRESS: But if that's more closer to reality, then all right.

MR. KELLY: But how do you know now that that old delta has now become a large delta. Because if you're only tracking an increase associated with my current change --

MEMBER KRESS: I think you're not tracking actual deltas, you ought to be tracking close to the speed limit on CDF and LERF.

And, you know, if the PRA you use is a better one than the old one, you've got a better, a better measure of how close you are to your speed limit, regardless. That's where you need to put the --

MR. KELLY: When you say that, Dr. Kress, are you talking about just saying, you know, for example my speed limit is ten to the minus four, for core damage frequency and therefore, so I could be a ten to the minus six and if I go up to the ten to a minus four, that's okay?

MEMBER KRESS: Yeah, but I would put a confidence level on mine. I wouldn't use the mean, because, unless, I might use the mean if I put a 95 percent confidence in the mean, or something.

Which allows you to do things to the PRA to improve it and get a better result. Yeah, I think, I know you guys have been hamstrung and I'm using national limits on a given plant.

But I think that's the only realistic reasonable way to do it. Because you're not going to see this PRA at this uncertainty level, this confidence level.

And, I'm going to watch what you do and if you improve our PRA and you tell me, wow, it wasn't really there. That's all right, if I think that's a real improvement in the PRA, but my confidence level has got to be in there in order to take care of things I don't know very well.

I really think that's the only rational way to, I mean it's the rationalist approach, but it's a --

CHAIRMAN SHACK: But I think there's a

strong expectation that one is not going to unduly increase risk. I mean to go from ten to the minus six, to ten to the minus four, to me is unacceptable, even if ten to the minus four is acceptable.

MEMBER KRESS: I think the fact that you've got CDF and LERF and maybe even a late containment failure in there, it will help put constraints on that.

CHAIRMAN SHACK: Well, it may well be that you've ran into other, but you know, I know that ten to minus six to ten to the minus four is unacceptable. What is acceptable is less clear.

MS. MCKENNA: Right. Because we did talk in the paper about you wouldn't necessarily only have cumulative, you may also have individual, you know, again, that's not something we've worked out to the last level of detail.

But I think, you know, that was one of the reasons why you kind of want both, is to make sure you're dealing with different parts of the problem.

Now the thing about whether the model change means that the individual risk that you, the change in risk you had before is now bigger than what you expected. That's another complication, you know, that has to be dealt with.

MR. KELLY: And one of the other things is now, you know, it's a volunteer rule, so I could come under the rule. So now am I, under my cumulative risk, am I only counting those changes that I make under this rule?

What about changes, non-risk-informed that I make, where do they go? How do I count them? I mean they'll be in the PRA but should they not count?

If I do something there that takes me above, you know, if I'm putting, and it's a, there's a lot of interesting questions associated with this.

MEMBER SIEBER: Well, you have the question of offsetting risks, too.

MS. MCKENNA: That's right.

MEMBER SIEBER: And you say I'm going to take away some margin here, and when you make that statement you say, then I think I'm in trouble because I don't have margin.

So I add another feature over here and if I still don't get it, I will work on improving my PRA model, until I do get it.

MR. KELLY: Right.

MEMBER SIEBER: And I don't like that kind of operation.

MR. RUBIN: Yeah, that was addressed in the original 1.174 package called bundling changes. And to get credit, to drive something up and to get credit for another change that drove risk down, it had to be an associated change.

MEMBER SIEBER: It has to be related.

MR. RUBIN: It had to be related, right.

MEMBER SIEBER: Right, yeah, we've been through that several times.

MR. KELLY: Right, well these are related to changing the design basis.

MR. RUBIN: No, it has to be more related than that.

MEMBER WALLIS: Whatever happened to the argument that risk would go down? Industry people sat here about two years ago and said we're going to make the case for removing large breaks from design basis.

We're going to show you that the risk would go down, because now we're going to emphasize better treatment of small breaks. The plant is going to be safer because more likely things are less likely to lead to damage.

And that I thought was a good argument. If you could show us, then yes, do away with this emphasis on large break LOCA and optimize the cooling ECCS for small breaks that the plant will be safer then I might go along with it.

But now everyone seems to be talking about what's an allowable increase in risk, and that doesn't go down too well.

MS. MCKENNA: Yeah, I think we need, we're looking at it as a backstop. I think we certainly hope that just what you said will happen, and it's certainly the kinds of changes --

MEMBER WALLIS: But no one has said it yet?

MEMBER SIEBER: Well you can make the argument if you delay the start times that the diesels maybe more reliable because you aren't stressing them as much.

On the other hand, the testing and maintenance programs are design to make them reliable when they are stressed. So, I'm not sure that any reduction in the risk you would measure in real, under these circumstances.

MEMBER WALLIS: But what is this promise from industry, just something we should not have taken seriously? I mean they said they were going to show us, and I haven't heard it in the discussion at all.

MR. KELLY: Well, we haven't talked about it because, industry has mentioned it a number of times and it may that an area such as diesel generator reliability, that we may get increased improvement there on the reliability.

And it may be that in the focusing of, because, you know, certainly it's our expectation that small break LOCAs are going to be much more frequent than large break LOCAs, that potentially could get some improvement in core damage frequency reduction or get more core damage frequency reduction is you were to focus more on small breaks. How much reduction or any real reduction would you get there? WE haven't gotten any calculations that have come in from the industry to demonstrate that. That may exist, to what extent --

MEMBER WALLIS: I thought that was a very good argument because that's very difficult to argue why the Agency should be working to increase risk.

MR. RUBIN: I can give you --

MEMBER WALLIS: I you had a very good argument that you're actually reducing risk, I think that would go down very well. You wouldn't have all this problem with it.

CHAIRMAN SHACK: But then you get a power uprate and I guarantee it's not going to do --

MEMBER WALLIS: Well, then you go back to same risk as before.

MEMBER BONACA: Just a question I had was, I'm sure you haven't got that far, but assuming, for example, you had a power uprates, you know, and a question would come, you know, about severe accident management guidelines and so on and so forth. What would be expectations there?

MR. KELLY: With respect to what Dr. Bonaca?

MEMBER BONACA: Well, I mean most likely in some cases, you may make changes that may affect, in fact, the actions that are now in those guidelines.

MR. KELLY: Well, whatever changes are made to the plant, as they would affect severe accident management guidelines, any of the other areas of the plant in turn that they would be expected to be upgraded to take into account those changes.

I mean that's something that would have to be considered. The next issue is Issue Number 7. It talks about the appropriate quality and scope of the PRA. And I've already mentioned a little bit about the scope, some issues about external events, shut down risk and things like that.

Again, I think the Commission has indicated that the more flexibility that is in an application the more rigor that they'd expect would be in the PRA.

This whole issue is being addressed separately as part of our response to the Commission's SRM on PRAs. And so I think that they would probably the appropriate people to talk about that, but we will be deferring to them for the, or taking into account what they're saying and incorporate it into where we are.

MR. RUBIN: Well, yeah, this program will leverage the work being done by the PRA quality initiative and the requirements there will be piggybacking as that work gets fleshed out.

MR. KELLY: Because the difference will be that they'll be coming out with something that deals generically without industry's, we'd like to see industry applying this.

But in our case we'll be talking about a specific rule it will be addressing this application. So we'll take into account their thinking and put it into the rule and apply it appropriately.

CHAIRMAN SHACK: It does seem it will add to the complexity of your rule, though, because you're going to have to some way to go through this trade off of quality versus flexibility --

MR. KELLY: That's correct.

CHAIRMAN SHACK: -- I mean you can say that qualitatively, but presumably you have to have some way of making the rule --

MS. MCKENNA: And also the narrow versus broad. You know the broader you go the more complete your PRA may need to be to deal with potential --

CHAIRMAN SHACK: Yeah, I narrow one is much easier problem to deal with.

MS. MCKENNA: Yes, yes.

MR. KELLY: Just the problem, though, with the narrow one is that it --

CHAIRMAN SHACK: It's narrow.

MR. KELLY: -- it's narrow, right. And is it, would it be worth industry's time and effort, would anybody go ahead and, you know, want to actually use a narrow rule?

The eighth issue is what do we do about future reactors?

MEMBER WALLIS: Why do you worry about all this business about industry's time and effort? You job is to ensure public safety.

MS. MCKENNA: Well, we're trying to, this is supposedly a volunteer alternative rule, and if we don't think that there is any use made of the rule, then to what extent should we, why should we write such a rule? So that's the basis --

MEMBER WALLIS: But if it were very broad interpretation, then it would seem that the price for entry into this new world ought to be a real improvement in PRA? And then no big deal.

MS. MCKENNA: Yes.

MEMBER WALLIS: You don't have to worry about this time and effort and quibble about, well you're going to make a little effort here and you get a little there. Just make it absolutely clear.

You don't have this high quality PRA to enter this new world.

MR. KELLY: That was the, that appeared to be the Commission message in the initial SRM. Industry had indicated that it felt that that was too high a bar, in a number of meetings.

And so we've gone back and told the Commission what we heard and told them our thoughts about it and we'll see what we hear back from them.

The last area is future reactors. We've proposed to the Commission in our memorandum to them, that this be postponed. It's difficult enough when you know what the reactor designs are to try to figure out some of these issues.

A lot of this really has to do with, also because we're talking about changing the design basis. For some of these future reactor designs, it's not clear what's going to a design basis accident for them.

And my expectations is that, what we'll may well end up doing is, having a risk-informed set of criteria for the future reactors. And so this will kind of be moot for them.

What will be interesting is how this is applied for anyone who comes in with something like an advanced reactor like BWR, CSIR 80 plus, AP 600 or something like that, and see how it's applied in that area.

MEMBER WALLIS: You're going to have a problem with the AP 1000 and BWR, because that whole design is based on creating a large break LOCA.

MR. KELLY: And they're very, very good at that.

CHAIRMAN SHACK: It's a start.

MEMBER WALLIS: So you're going to have to do away, if you did away with it, they would have a problem justifying their design.

MR. KELLY: Well, we might require them not to have those breaks because --

MEMBER WALLIS: Don't they have a 80S4, I mean it doesn't make any sense.

MEMBER SIEBER: Well, you know, if you were to build a future reactor that looked like today's reactors and this rule was in place, today we contemplate a change to the rule in the sense that all the mitigating systems are already there.

All the design parameters were set when large break LOCA was part of the design basis. And so, you know, we don't have all that much to worry about.

But if you were building a new reactor, just like the old ones, you would skip a lot of that stuff, because it's no longer in the design basis.

And so whatever margin you think you have for a new reactor, it's not going to be there.

MR. KELLY: All right, the question that's going to come up for the future reactors, I would thing, is if you're not having the same kind of design basis where you're contemplating these large breaks or other, you know, were limiting events that were really way out there, in a sense.

And if you're trying to base it on your knowledge, how are you going assure severe accident capabilities. And in some of them severe accident capabilities may not be really necessary, because inherently the designs don't allow for that.

So, we'll see how that happens. The last thing I did want to talk about here is somebody brought up about sabotage. And it's correct that we are not, you know, and the paper talked about sabotage and issues like that.

The Commission has indicated, however, that we are to give considerations to that process. I'm sure there will be things in the rule that will ask us or inquire that considerations of sabotage be given.

And it is correct that one has to be careful. I don't, I think that the major considerations are, I mean today when we protect the reactor, we're taking into account our various areas of the plant that have to be protected.

It might be that fewer areas of the plant would have to be protected, but, in that there are fewer areas, perhaps an individual area might be more important.

But that's still an area that has to be protected. I think that that's more of a problem for some of the passive designs and the future designs that show that they, because they don't have the active systems, inherently as long as you're not disturbing the passive systems, they work well.

But if you can get in there and screw up the active system, or the passive system by, you know, putting some noncondensible or something like that. Or changing the pressure balance in the system, then it's much more subject to, more easy because you don't, like right now what we have at our reactors is very, it's much more easy because they've got all these different ways of putting water into the reactor.

You don't have those same kind, potentially you don't have those same kind of capabilities so it's more of than issue for them.

But that's all I had specifically on those technical issues, unless anybody has additional questions on it?

MEMBER LEITCH: Yeah, I had a question about the coincidence loss of off-site power, I don't see that discussed any place.

MS. MCKENNA: Okay, let me skip ahead to another slide, then, since you asked the question. We had the direction in the SRM about preparing a proposed rule on removing incidents of LOCA/LOOP.

I've kind of been parallel with some of the development of the Staff papers and the SRM and the BWR Owners Group had an initiative to look at some specific plant changes that were of interest to them, that are in large part derived from this coincident LOOP and the resulting impacts, for instance, on diesel start times, that kind of thing.

And they generated a set of six or seven plant changes that as an owner's group they wanted to pursue. And, as we said in the paper, they've been busily at work over the last year or so, developing the topical.

We're expecting submittal pretty soon. You know, we've kind of had some various conversations back and forth and they were in kind of final stages. Where they were coming with the topical to look at those changes as generically as possible and try to bound the various plants and that the Staff could then review the topical.

And then the individual plant could then come in with an exemption say, I would like to implement these four changes. This is how the topical applies to me, and get that undertaken.

And what we've proposed in the paper was to kind of engage on that topical and some of the issues and the specific changes of interest, rather than try to be doing multiple rulemaking that have some of these issues underlying them at the same time.

And it would be a way for us to make progress. To respond to specific proposals and to get some learning on this. So that's what we propose is to do that review. And one of the changes is, would be a change in the diesel start time from the ten second to some, 60, I don't remember the exact number, but some more reasonable time that is, you know, better for the diesel performance, would still respond to a large spectrum of the events in the same way.

But there would be some small space of breaks where, you know, if you happen to have the break and that event, you wouldn't get the same overall result, but in some of the meetings and discussions we've had, in terms of this mitigation capability point that we've talked about, is that they are, have been proposing to show on a true best estimate kind of basis that you would, it would still meet the 2200, in essence, for those large breaks with, you know, if you happen to get the coincident LOOP and the same time.

So if it's, that's kind of our proposal now on the LOOP/LOCA.

MEMBER LEITCH: Would that require the kind of plant specific PRA that we described earlier?

MS. MCKENNA: I'm going to turn that over to maybe Mark of Glenn.

MR. KELLY: There are two aspects of plant specific analysis. One of them is we would want them to go ahead and perform an evaluation of their conditional probability of loss of off-site power, given a LOCA for their plant.

It's very, very site specific. And we've developed some methodology for that and we would like to see them. And we have people here who could talk about that, if you needed that.

But we have a method for determining the appropriate site specific conditional probability of loss of off-site power.

The second area is, we would be interested in understanding what the conditional, with the change in core damage frequency and risk would be associated with these potential changes.

Now the BWR Owners Group had indicated to us that their hope was that they could perform a generic evaluation of the changes and do some kind of bounding analysis to demonstration that that was adequate.

That the change in core damage frequency and risk would be small enough that we'd find it okay.

We've discouraged that concept, because this is such a plant specific issue and while we told them it's potentially possible that they might be able to do it, we don't believe that realistically that it's possible.

That they are most likely going to have to use plant specific analyses.

MEMBER LEITCH: Yeah, the off-site power arrangements are so different and plant specific. And the reliability of those systems is widely variable.

MR. KELLY: The plant's individual capabilities to respond to a loss of off-site power are very different also.

MR. RUBIN: The induced loss of grid is going to depend on, is going to be site specific.

MR. KELLY: Sure.

MR. RUBIN: So we don't need plant specific PRA calculations, we believe at this time.

MEMBER LEITCH: So then the question here is, is, as you indicated before, I guess, whether the benefits, which are much smaller with this, but whether those benefits would be worth the price of admission.

MR. RUBIN: They very much believe so because they're fast diesel start time is a very onerous issue for most plants.

MEMBER LEITCH: Yeah, that's tough on the diesels.

MS. MCKENNA: And it's in our narrow definition, too, of something more specific and confined that's a little easier to deal with. But, yeah, they wouldn't be pursuing this if they didn't think it was beneficial to do.

MR. KELLY: The 60 second start is still a fast start. And it's --

(Several people talking at once.)

MR. KELLY: Right, and what is going to buy them is, I don't, my own personal belief based on talking to diesel experts is that it's not going to significantly increase the reliability of the diesels.

It may not have any real impact on that. But what it will do is if they're running a test and it runs right now, instead of ten seconds, if it's running 11 seconds, they're, they have to play with the system and get it and rerun it and rerun it, rerun it and show that's it's ten.

And there's no real different between ten and 11 seconds, but the fact is that our regulatory basis is ten. So if they move it to 60 seconds, they have that margin and they're much better off, and we'll have no real apparent change in risk.

MR. RUBIN: There's some other changes that may ultimately result in some net safety increase, too. There's a suite, there's a mix and match set of changes.

One is to realign one of the LPSI trains directly for suppression pool cooling instead of going directly into one of the LOOPs. So for small break LOCA, perhaps some of the transients may give you a benefit.

We haven't seen any analysis yet, but that's one of the things they are proposing to do as part of it.

MEMBER LEITCH: Are you aware of any similar activity in the BWR world?

MR. KELLY: We're not aware of any at this time.

MR. RUBIN: Well, the PWRs, I think the WOG owners' group for some time has been talking about redefinition of large break LOCA. I haven't seen any details from them, but they've been working on it for some time.

MR. KELLY: But it's not a, specific to LOOP/LOCA. They are talking about generically coming in on large break LOCA, but not specific to LOOP/LOCA.

The other thing that comes along with the LOOP/LOCA thing is that, I think there were like seven issues, seven areas that they talked about making modifications.

At some plants you can't necessarily make all seven at all plants. Some of them may, you know, like two and five don't go together in certain plants and cause you problems, and that's something that we'll all still have to look at on a plant specific basis.

How well, you know, these modifications really work.

MEMBER LEITCH: Sometimes, you know, one has to settle for small success, I guess. And it seems to me this is perhaps a way to start the project.

MR. KELLY: It's a step in the process and it's going there. And if it works great. And one nice thing that most of the PWRs have significant margin in peak clad temperature.

MEMBER SIEBER: Most of them do? They typically, PWRs typically run closer --

MR. KELLY: The boil, the boiling test.

MEMBER SIEBER: And so I would think that PWR owners would be interested in getting margins.

You know every year you file changes that you find to your Appendix K model.

And which sometimes forces them to improve the model to offset whatever errors they found. And I've always had the impression that most of them don't have as much margin as they would like to have for fuel management purposes and particularly flow design in the fuel assemblies themselves.

MR. KELLY: Well, that's, and that comes back to part of it. The boilers have significant margin, most of them have a significant margin, and therefore they can take advantage of that margin and our understanding is their proposal is going to come in with using their basic best estimate analyses showing that they are still way off, even with these seven changes.

So they're basically eating up some of that margin and not really going into severe accident space and now talking about exceeding 2200 degrees for peak clad temperature.

MEMBER SIEBER: Well that margin is one of the features that allows them to do the pretty good size power uprates.

MR. KELLY: Now going to the, so I think that there are issues there about, for the pressurized water reactor, about how well they are going to be able to do that and take advantage of that.

It's more of a problem for them and there the importance is going to be about how mitigation is defined for this, beyond design basis area. How much they're going to be able to take advantage of it.

MS. MCKENNA: Yeah, I just to back up one slide here.

MR. KELLY: I think we've basically talked about the issues that are outlined here, and the papers we gone through the various discussions. So I'm just going to quickly run through them.

CHAIRMAN SHACK: Are these all ongoing are you waiting for feedback now from the Commission or is that just going to change the weighting of the effort that you give these various.

MR. KELLY: Some of these efforts, I mean, and a lot of this is really, we have a small working group that's working on this. And it's ongoing in the sense that we continue down this path.

But a lot of it is going to, I think the effort will really be geared up once the Commission indicates to us whether we're talking about a narrow or a broad scope.

A narrow scope would be easier to deal

with but, as I said, we're not sure how useful it will be to the industry. Broad scope is going to require a very significant effort and may require redirection of resources in order to be able to handle it in any kind of near term.

MEMBER WALLIS: These are details, it seems to me, that I always have trouble with these changes in rules and regulations. If you get into this world of looking at all these details, but the real question to me is why are we doing it and what are the consequences?

If we do this, well, it's going to enable industry to produce more power with less expense, is that the purpose? And what are the consequences in terms with public safety? Are we allowing them to increase risk by one percent, zero percent, minus one percent? I mean what's the trade off here, and if you make this decision, you go ahead with this sort of a rule, what effect does it have on the industry, the public and so on?

That never seems to come into any of these decisions. You get into the bureaucratic details of how should we write this rule to assure defense and depth or something.

But I can't put it into a perspective, and if we do that, what are we really achieving in terms of things, so I can grasp, the measures of what, our effect on the nuclear industry and the public?

MR. RUBIN: I think right now that's a little bit of an unknown. I think it will, sure, I realize that's a little unsatisfactory, but I think that's the truth.

I think we're going to try to establish the framework that will determine the answer to that question. The framework will at worst what the answer to that question be.

And at worst it will be retained, a robust mitigative capability with a very small increase in risk. Hopefully there will be a safety improvement. But that will be dependent on what changes the industry will make given the increased flexibility from the rule.

But the underpinnings of the framework will probably allow small increases in risk resulting from this.

MEMBER BONACA: I believe the underpinning is, I mean the whole effort is to reduce or eliminate unnecessary burden. To me unnecessary burden is defined in Reg Guide 1.174, as normal changes. And small increases in risk and so we should certainly, hopefully, focus on those.

And the only issue that remains then is the uncertainty. You know, all what is the level of confidence that we have that, in fact, we do have just small increases in risk.

And, again, depends on what I do with this margin, we could find out. And that's, the next question I have is so you'll have to evaluate for each one of the possible changes that they may propose, what will the result of this could be.

MR. KELLY: Well, I think that there's an additional aspect to this that should be recognized. And that is generally when we talk about risk-informing and the reasons why you risk-inform is so that one can concentrate on those things which are most important.

Then when we go back to 50.69, the idea is of having that Risk 2 category, was that there are things that right now aren't covered in, you know, safety significant, but they really need additional treatment and we should be paying more attention to it.

So part of what we're saying here is, you know, the industry has a finite set of resources and where, what are they putting these resources to?

Where are they putting all of their effort in? Industry complains that they're spending a lot of money dealing with very, very low frequency events that are well, you know, covered by the design and that they shouldn't have to be spending all this. That there are other places that can be put.

MEMBER WALLIS: Would they put it elsewhere if they saved it?

MR. KELLY: It's not clear. That's part of, they may or may not. Part of it may go to their bottom line profit or they may decide that they want to enhance the work that's going for, you know, taking care of small breaks or looking at other improved performance and equipment, buy better equipment.

MR. RUBIN: We have no knowledge where --

MR. KELLY: WE have no control or knowledge --

MEMBER BONACA: Well, what is industry giving for this? They seem to be wanting to get something. Well, what are they going to give? Are they going to give better safety with small breaks? Or something, are they going to give something?

MR. KELLY: Well, the understanding is that, I mean if I can, I'm speaking as if I were an industry person. I would say that we believe that the large breaks are really not possible.

That the small are --

MR. RUBIN: Not likely. Not likely.

MR. KELLY: I'm giving you the industry version, not our version. The industry would say that it is really not possible and you're not going to have them.

That the small breaks are improbable but possible and we'd like to put our emphasis there. The risk numbers show very low numbers for the large breaks.

We'd like to basically not put much effort into that. We want to put out effort into the areas that we think are really more risk significant. And that's the proposal that's been put to us.

MEMBER WALLIS: Are they really proposing to put more effort into something?

MR. KELLY: The wording that we've received, the discussion has been that that's where this other work will be. And to what extent they're going expend additional resources in that area, or redirect resources, I don't know.

MEMBER WALLIS: Let's get back to the point I made earlier. They told us two years ago they were going to show that the plants were going to be safer because they were going to put more effort into things that are likely to happen.

I haven't heard a word about that since. There's nothing that's on the positive side about how if we do this, the industry is going to do something to use their resources better so there's going to be improvement in some way in safety.

MR. KELLY: Well, it may be when the BWR Owners Group comes in and does their generic analysis, they will, they may show numbers that show an improvement in core damage frequency associated with what they expect will be an improvement and a reliability to diesels or better mitigative capability for small break LOCAs.

CHAIRMAN SHACK: I mean you can't discount the social benefit of having greater productivity too, I mean that's a real benefit.

MR. KELLY: Right, you get more electricity, and if you can take a plant that's basically been paid for, and you can generate more electricity with it, that's a real benefit to people.

MEMBER SIEBER: It's a benefit to the shareholders. The price of electricity is a market price.

MR. KELLY: It's to the customers, too,

that have that available, and you know, you additional spinning reverses.

MEMBER SIEBER: Well, whatever they do it's probably not our concern, how they manage their company. On the other hand I think that if you're looking for a risk benefit, I think you'll spend a fair amount of time hunting for it.

MR. KELLY: Well, I think one of the, you know, one of the aspects that one, of course, looks at in the entire risk -informed space is, you know, people talk about in two-edged sword. And, you know, are we only removing things from consideration and is there anything worth looking the other way.

And I think that, it's, I think one of the Commissioners was pointing out that they felt that this was our big opportunity to see, you know, perhaps push back and ask for additional programs.

MR. RUBIN: Let me give the Committee a historical perspective on the risk-informed licensing actions. There's always talk about there will be risk decreases. Generally, we see small, small risk increases on the initiatives.

Every once in a while, we see a decrease. Occasionally a decrease on ISI because they pick better locations. Occasionally a decrease on a bundled initiative, even a diesel generator AOT change could be a decrease because they do a seismic enhancement on the diesel generator building as part of an extended AOT.

It's rare to see a decrease as part of a risk-informed initiative, but it happens occasionally. So there's a perspective for you.

MEMBER WALLIS: Well less uncertainty on the PRA would be something you could buy with this. And I think that would really help everybody. Really it would help industry, I don't know whey they're so reluctant to do it.

It costs them something but they can buy a lot with it, too. And it helps the public to understand the real risk if a PRA is more complete and more believable, then we're on a much better, sort of basis for making decisions.

So that at least would be something that we could buy with this from industry. Insist on PRAs being uprated where they're not, and some of them probably now are quite adequate.

MR. KELLY: We have additional, on the next slide we have additional ongoing research work on thermal hydraulics and risk assessment.

MEMBER WALLIS: But, it's work on, what's the objective?

MR. KELLY: These are issues associated with what are the potential changes and risk associated with, you know, if I were to make some of these plant changes that industry has indicating to us that they are anticipating being able to do, what would be the actual effect on core damage frequency and risk.

MEMBER WALLIS: I think the key thing here is what George calls a model uncertainty. And if you have all these PRAs and because of the uncertainties and the predictions of the thermal hydraulics, you don't really know if you're going to g into this branch or that branch and what the probabilities are and so on.

So we're getting thermal hydraulics tied into the PRA, that's what you're talking about here?

MR. KELLY: Well, I mean thermal hydraulics is always tied in and the success criteria is based on your thermal hydraulics. What we're doing here is we're looking at taking into account, as I said, the changes that are being proposed.

I don't think we're actually working at improving the individual thermal hydraulic models that we have now and our codes, not for this particular reason. We're taking into account what the best codes that we have to --

MEMBER WALLIS: I think you might get into the regulation space, and you might, are you going to require these codes? Are you going to require that 95 percent of the time they meet some success criteria or what?

MR. KELLY: Well, you're talking about in the mitigation space?

MEMBER WALLIS: Yeah.

MR. KELLY: That's one of the issues that we have there. If we are, research is looking at what are the capabilities of dealing with these severe accident spaces and, you know, what can we say about that, about how good we feel about the codes for handling, once you go beyond 2200 degrees F.

MEMBER WALLIS: The idea of all these conservatism in the traditional approach was that because we're uncertain, we'll just make these conservative assumptions and that will give us a lot of assurance that no matter what the errors in the code of these sorts of level, we still are not going to cross some boundary. That was the old approach, as I understand it.

MR. KELLY: Well, you still have the old

approach, as you call it, for your design basis LOCAs, up to whatever the new maximum design basis is.

And we're proposing that there should be some additional mitigative capability, although not necessarily with the same assurance that we had for the --

MEMBER WALLIS: Because the rational thing to do would be to rewrite the whole regulation so you could apply it to small breaks as well.

If you understand how to balance off all these mitigative capabilities and so on, why not do the whole thing?

MR. KELLY: One could do that, but they want me to do this in less than ten years.

MEMBER SIEBER: Well, actually the way Appendix K is written, it says that you should analyze the worst break up to --

MS. MCKENNA: Up to, yeah.

MEMBER SIEBER: -- the certain rise and then you put some restrictions on, for example, the DKE model and some correlations and so forth that you're stuck using some old techniques that the agency has found satisfactory, some where back in his history, and I guess those deserve another look, since we're doing all this other work.

And maybe the codes could be approved.

MR. KELLY: Well, we have the best estimate codes that are, have been approved for evaluation, model codes.

MEMBER SIEBER: But you're still stuck with a correlation --

MS. MCKENNA: Yeah, I mean, going back to the papers that generated the SRM, there were some proposals to examine changes in Appendix K and --

MEMBER SIEBER: Well, they wanted to do it piecemeal.

MS. MCKENNA: Of course, the Commission said no, they didn't like the piecemeal approach that was laid out. They said they got the best estimate, let's use those.

MEMBER SIEBER: Well, if you change to EKE models you get some margin, even though it's not clear to me that you get very much. But there's some margin, if already pretty close, you know, any margin helps.

MEMBER RANSOM: It seems like this comment about best estimate really needs some clarification because, you know, like Professor Wallis said, Appendix K was brought into play originally to account, hopefully overwhelm, you know, some of the uncertainties that were involved in this kind of analysis.

If the PRA itself is dependent upon the uncertainties that are involved in analyzing the consequences of any given event that may occur.

And it almost seems like an ill-posed problem where you want small changes in risk evaluated with something with large uncertainty. And I see that as a limitation of what you can do here.

Whether it's Reg Guide 1.174, or trying to define, you know, a maximum size LOCA.

MR. KELLY: If you go back to my very first technical issue, you'll see that it talks about what are the appropriate criteria needed for confidence in the elicitation results. Because we understand that on top of whatever uncertainties is that that we have in the numbers that we're going to be inputted for the expected frequency of LOCA, were usual uncertainties and issues associated with the PRAs that are used to determine for are the effects on risk.

So I mean it's not like they cancel one another out. You know, they're additive. And so we have the uncertainties associated with the elicitation process.

And then we the PRA uncertainties. And

they all have to be considered and that's why I talk about we have to determine, you know, what is the appropriate confidence that we need in all of this, in order to be changing our design basis.

CHAIRMAN SHACK: Just from a simple minded point of view, if I've got an event that's almost never likely to happen, do I really need 95/95 confidence so that I can deal with it?

You know, could I live with 90/50? And you know how much margin would that buy me alone? You know I still have a very high likelihood that I'm going to deal with the event, but since it's not very likely to happen at all in the first place, it may well be good enough.

MEMBER KRESS: The trouble with that trying to decide on confidence levels, generally it's related to, if the think actually happens, what loss is the NRC, the utility and the world going to be subjected to?

And those losses are monetary, life, a whole lots of things. And it's a policy issue because, I mean you can't just say, it's acceptable to us. It's what is acceptable to society.

And so, you know, that was part of the problem with trying to face up to what safety goals are. So when you're trying to say, I need this number at this confidence level, that's almost always what you're faced with.

You're going to establish that confidence level, based on what I can stand if that actually happens. And I don't know of anyone to technically arrive at that number, other than to try to tie it to some societal acceptance and how you get those, I don't know. So really you have a problem with that.

MEMBER SIEBER: Well, that varies from day-to-day, too.

MEMBER KRESS: And that depends on your definition of who society is, too.

MEMBER SIEBER: Yeah, and how close you are to where ever it's going on.

MEMBER KRESS: Well, that's part of society.

MEMBER RANSOM: I know I've tried to think of an analogy and it's almost like designing an elevator and you've got safety breaks on the elevator in case the cable breaks.

Which is probably very unlikely and it seldom happens and I'm sure there's enough statistical data to really examine it. But then designing the breaks so it only holds 90 percent of the load that the elevator could hold. Who `s going to get on that kind of elevator.

CHAIRMAN SHACK: But I mean, you're already ruling out the CDF contribution, because the frequency is so small. I mean you're really asking for an additional, you know, on a purely risk-informed basis, you know, you take the industry, it's just not going to happen.

And you know the associated CDF and LERF with this are small.

MR. KELLY: Well, that would be risk based. If I say I'm only going on the frequency, then it would be risk based.

CHAIRMAN SHACK: Well I mean not just frequency, but if you do the analysis for the CDF and LERF, they're small. And you're asking for more and, you know, it seems to me that that sort of avoids your problem, that you know, well this is almost a conditional sort of thing.

If a large break happens, you know, I want a conditional probability I can deal with it and --

MR. KELLY: As part of your defense and depth capability.

CHAIRMAN SHACK: It's part of my defense and depth, and you know, it really isn't, I've already decided that my risk is small enough to be acceptable with my safety goals. I'm really arguing over how much defense and depth I need.

MEMBER RANSOM: Well, there was some argument, I think in the materials I read preparatory this, that if the maximum size pipes are designed to the ASME code as the vessel, why aren't you considering vessel rupture? And how do you rule that out?

MEMBER WALLIS: Well for reasons that I don't understand, these things get safer the bigger they are.

(Laughter.)

MS. MCKENNA: Save that for later.

MEMBER LEITCH: Well I was trying to think about this like right now we're saying vessel failure in incredible, but yet there is in the present ECCS systems some mitigative strategy in the event, some mitigative capability in the event of vessel failure.

Here we're kind of moving down a little bit and we're saying, well if it breaks, incredible, but there's still some mitigative strategy beyond the redefined break.

MEMBER SIEBER: Well, I guess it's like if the head breaks, why you probably could mitigate that.

But if the bottom breaks, you're in trouble.

MR. KELLY: What we said for vessel failure is that the Commission made a decision that it was, in and of itself, it was considered to be an incredible event.

And when you get incredible events, the way we intended to deal with them and we make sure that they remain incredible by dealing with --

MR. RUBIN: Programmatic, there are programmatic things in place for the vessel that are not in place for the pipes. I think we need to defer that to the Engineering which are the experts.

MEMBER WALLIS: If they're incredible why do have all this work in the pressurized thermal shock?

(Many people talking at once.)

MEMBER WALLIS: But it obviously means they were credible, otherwise we wouldn't do that research.

MR. KELLY: Continuing with the, back to Staff technical activities. We talked already about the LOOP LOCA and where we've asked the Commission to go ahead and work on the topical and then finish that work before we go ahead.

Do you want to do a summary?

MS. MCKENNA:

MR. KELLY: I think we've shown that the application of redefinition has to be very carefully dealt with. A lot of very important, very difficult technical issues. We don't want to reduce margins too much.

We don't want to, you know, there's, potentially there may be improvement in overall risk has yet to be demonstrated that it would exactly work out.

We don't want to do anything that would reduce risk to the point that we would not be happy with it, doing that, and doing that in manner that makes sure that the rule precludes that type of thing, is going to be a tricky business.

There are a lot of expectations about this rule, from the Commission, the Staff, industry. There's parts of industry, these need to reconciled some way to make it to be a functional rule.

And to, then something that's technically justifiable is going to be an interesting challenge also. So we've sent our paper up the Commission and asked them for their policy decision.

We're continuing with your efforts in the meantime, but they're really going to go forward once we've gotten direction from the Commission.

We're interested certainly in what the Committee thinks about this as you've been talking here. We're not expecting a letter, but we are happy that, you know, you've given us your feedback about what's gone on here.

We think that these are going to be very challenging, I've said it a number of times, very challenging technical issues

And, of course, if you have any thoughts about how to answer them or deal with them, we'd be happy to have you.

MS. MCKENNA: I think that's it.

MR. KELLY: I think that ends our presentation. Any questions?

CHAIRMAN SHACK: We should refer all questions to Rob.

(Laughter.)

MS. MCKENNA: Yeah, we'd like that. But I mean frequencies is fair game.

MEMBER WALLIS: Well, I don't have a question but I would like to thank you for what I found to be very frank and serious-minded and helpful presentation.

MEMBER SIEBER: I agree, very well done.

CHAIRMAN SHACK: Well, we're actually

finishing ahead of schedule if nobody has any more questions.

MEMBER KRESS: Maybe we can tack that onto our lunch hour.

MEMBER SIEBER: Yeah, you can, you have too.

CHAIRMAN SHACK: Well, if that's the case, I suggest that we adjourn, recess for lunch, and come back at 1:00.

MR. SNODDERLY: Yeah, and just to let you know, I passed to all of you all of the slides that Rob and Lee are going to present this afternoon, and so those are available.

(Whereupon, the foregoing matter went off the record at 11:40 a.m., and went back on the record at 1:02 a.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

1:02 p.m.

CHAIRMAN SHACK: Time to come back into session. I guess Rob Tregoning is going to tell us about the results from the expert elicitation and development of passive system LOCA frequencies.

MR. TREGONING: Thank you, Chairman.

As the title says, we are going to be talking about, myself and Lee Abramson from the Office of Research, are going to be talking about how we developed these passive system LOCA frequencies for risk-informed revision. The option 3 risk-informed revision of 10 CFR 50.46.

Now the talks were out of sequence a little bit in the sense that this morning we heard some of the broad policy or I guess policy and technical, although we focus more on technical issues here concerned with possible rule revision. Here we're going to focus down very carefully and talk about one specific input to the regulations which will come about at some point.

DR. KRESS: And you're supposed to answer the questions that they didn't answer this morning.

MR. TREGONING: Yes. Yes. They did a good job of deferring until this afternoon any questions that were posed this morning.

DR. RANSOM: Have them all written down.

MR. TREGONING: And all I can say is to the best of my ability I will try to answer anything that I have knowledge of.

DR. WALLIS: What does passive system mean in this context?

MR. TREGONING: Passive system in this context means, and you'll see a flow chart here later, we clearly separated piping, structures versus things that are active.

DR. WALLIS: Okay.

MR. TREGONING: Active components like pumps and even seals. Pumps, valves, seals; things that we have -- active implies that they actually do something, they just don't sit there. But also we tried to exclude things that are covered by the maintenance rule because there are other regulatory measures that are put in place to try to maintain the--

DR. WALLIS: So the valve that had the over bolts would be an active system.

MR. TREGONING: Well, the valve itself would be active, but the valve body is passive.

DR. WALLIS: Is passive?

MR. TREGONING: That is correct.

DR. WALLIS: But the bolts move when they're taut, so they actually are passive.

MR. TREGONING: They move when they're taut, but then they sit there in service.

DR. WALLIS: So they're passive? The bolts are passive.

MR. TREGONING: The bolts are definitely passive.

DR. SIEBER: You are basically treating all these things the way the code, the way the ASME code treats them that makes that differentiation?

MR. TREGONING: Right. Right. In terms of the definition of what's active or passive system, we tend to follow not code.

DR. SIEBER: Right.

DR. RANSOM: But I'll say sort of historical PRA definitions in figuring out we were going to consider and what we weren't going to consider.

DR. SIEBER: Good.

MR. TREGONING: I just wanted to outline the presentation here and give us a sense for where we're going. I want to delve into at the first slide, just the presentation history that we've had for this topic area in front of you folks, the ACRS. And talk about the program milestones that we have had since the last time we were here.

So just to give us a little flavor of where we have been to set the stage of what we're going to be talking about this afternoon.

I will remind you about the objectives and scope for this effort. And I'll also delve into the approach. I will say that we've covered most of these areas pretty extensively in past presentations. So if you notice, this talk it really focuses on the results and the analyses and that's what we thought was proper given the background that we've had here and also this is the stuff that's new, this is the stuff that you haven't seen.

So if it seems we're skimping on approach and things like that, I mean I certainly have backup slides, we'll certainly deal with questions as they come up. But we really given the limited time that we have, we wanted to focus on the results and the analysis.

And in terms of the results, sort of partition them into four or five different areas that we're talking about here. The first one will be general rationale and insights. These are sort of qualitative inputs that we got from the various experts, what things are important, what things potentially aren't important. What things did I really consider when I based my estimate.

We could spend all day on this alone. So what we've tried to do here is just give you a flavor, some of the things that we heard. And a flavor of some of the things that we heard more often from people.

Again, we didn't ask the experts to develop a consensus at all. So I tried to be very careful when I show this rationale that, you know, I don't want to couch it as being a group consensus in anyway, shape or form. This is just a smattering of things that we heard.

We'll then present the actual estimates that we got. And then after we go through sort of the total frequency estimates, we'll start to parse them a little bit and look at piping and nonpiping contributions. We'll also look at system contributions. And then we'll start to look at the next aspect, which was variability among the panel members and uncertainty in their responses.

The last bullet here we'll really only touch on if we have time. But we asked them separately about how safety culture could effect LOCAs. And we were very clear about how we define safety culture realizing that that can be potentially a very broad open ended discussion. So I can provide you some of the insights that we got from them related to the safety culture effects on LOCAs.

And then finally, we'll go into the remaining work that we have on the effort and summarize it.

So that's really where we're headed at this point in the presentation.

So, as I mentioned, we've been in front of the various ACRS committees a number of times. The most recently, and I've sort of listed them in inverse chronological order, the most recently was in November which we were in front of the Subcommittee, although I think a number of the main Committee members were here as well. And we went into pretty good detail on the expert elicitation approach and also the development o the base case frequencies. And we had David Harris here who was one of our base case developers to go into his approach and his technique for coming up with his base case frequency development estimates.

We were here in July. We briefed the main

Committee again on the status of the effort and also the approach at that time.

And then about a year prior in May '02 we had a Subcommittee briefing of, again, various subcommittees where we presented the results of what we're calling this pilot elicitation. When we kicked the effort off, we had an informal staff LOCA frequency evaluation effort that was much more accelerated over about 3 weeks to a month's time where we actually internally came up with estimates. But more importantly than coming up with estimates, we can up with issues and a possible framework that we could use and apply to this full elicitation.

So in that meeting actually presented the results of this pilot elicitation as well as the plans for this formal elicitation.

So we've really been in front of you probably this will be about the third or fourth time depending on how you're counting talking about the elicitation in some way, shape or form. And then even back in '01 there were several presentations as part of the larger effort to risk-informed 10 CFR 50.46 where we outlined the technical basis; why we thought we had to move forward with this elicitation to do break frequencies.

So what have we done since? And even though we were in November I've backed the time line up to September. So what have we done just before the last ACRS presentation to now?

Well we completed all the individual elicitations and, as I indicated, we had 12 experts on the panel or 12 panelists, we'll call them. And we elicited each of those panelists individually and we finished the last one of those on October 24th, so essentially the end of October.

The elicitations weren't by any way shape or form the final input that we got from the experts. Generally what happened in these elicitation is we got some initial input. We would go through the input that we got and point out potential inconsistencies or areas where their numbers may not be matching up with some of the qualitative insights that they were giving us. So I think for every expert involved after the elicitation they had to go back and refine their analyses.

And another thing we did in the elicitation is there were areas where they may have been unclear what we were specifically asking. So we cleared up those areas as well.

So everyone after the elicitation had more work to do. And for the period from October, end of October until about mid-January we were getting sort of the first set of revised responses back from all the expert. Once we had all of those, we conducted an initial analysis of the results. That was done about at the end of January.

And the week of February 10th, or I guess the week of the 9th over three days we had a feedback meeting with the panel themselves where we not only -- we presented them back with their raw data as it had been analyzed, not only by but also presented the information on context of what the rest of the experts had not only said qualitatively, but also estimated quantitatively. So we got the whole group back together, we fed them back the information that they gave us and we fed them back the quantitative estimates that we gave as well.

That was an interesting meeting in the sense that some of the experts realized well, you know, I didn't realize that me saying this had these implications on down the line. And they also didn't realize some of what the other experts had considered in their formulation of estimates.

So after this meeting we gave the experts another chance. Okay, based on what you've heard, based on your final outcome and how we analyzed and utilized your results, if you'd like you can come back and do yet another revision. And a small handful of them choose to do that. And we got our last set of updated responses on the 17th of March. And we completed our preliminary analysis the 19th of March. So you can see these are relatively fresh.

So when we talk about results, I just want to caveat it. You may see more into the results today than I've even had a chance to consider or really try to understand. And Lee and I realize that there is still some additional work.

And it's a fascinating exercise because you get so much information. It's a bit like trying to drink from a fire hydrant in that you have to be careful in what you try to sample and you have to be careful what you try to couch as being real versus just being some sort of artifact from the way we did in the analysis.

DR. WALLIS: Did your interaction with the experts reduce the scatter or the deviation or the variation in the predictions?

MR. TREGONING: And I presume what you mean by that is when we were here in November we presented results for these base case estimates and there was very wide variability there.

DR. WALLIS: Yes.

MR. TREGONING: And that variability was a function of different analytical techniques as well as some other factors. That information was presented to all the experts. And they, obviously, had to rectify those differences in their own testimony to us. And what you'll see is it's certainly reduced over that. That was a wide bit of variability uncertainty, although you're going to see when we get to the results that there still remains a good bit of variability and uncertainty. And that's what we expected going in. We didn't think we'd be able to reduce that just because when you're trying to estimate the frequency of something that's rare, it's always a difficult process.

DR. WALLIS: I think you said that you met with them and you gave them more information and they revised their predictions. Did they come more into line with the other members or did they get more diverse, or did it have no effect?

MR. TREGONING: And I want to be clear, you mean post-feedback meeting in February?

DR. WALLIS: Yes. Did you sort of pull them into line and say look, you guys, I can't tolerate 10 to the tenth variation. I'm sure you didn't do that?

MR. TREGONING: We didn't do that. We did not have edicts about what uncertainty we would accept or not accept. But I think what they did is -- and this is a natural thing. When people made their estimates they had certain factors that they were considering. They heard qualitative arguments that made some of them reconsider their estimates.

I think what we found when looking at the analysis is that the median responses of the group of experts, if I took the median of all their responses, the differences between the pre-February and the post-February 12th estimates was practically nil. And what changed was the variability about the mean. So we did see a decrease in the uncertainty pre versus post.

So some of the people that were more outliers recognized that there were some things that they hadn't considered that they wanted to go back and factor into their responses.

DR. FORD: Rob, I've got a similar question. At the last meeting I asked who was on this committee, who were the materials experts either in terms of analyzing or working with cracking phenomena or degradation phenomena, and you said two. Karen

Gott and Dave Harris. But there were ten other people on that panel who presumably didn't have that experience of either analysis or operational physics or physics of the degradation mechanism. So in light the question that Graham Wallis asked, when you came back and reanalyzed the initial inputs, did everyone tend to veer towards the two experts or what happened?

MR. TREGONING: Right.

DR. FORD: Presumably they've got the highest value input.

MR. TREGONING: We didn't -- there are a lot of things that go into LOCAs, certainly material understanding is one of them. I don't know what I said in November. Probably have to go back and look at the transcripts. But certainly I would argue that most if not all of the experts had some knowledge of materials and degradation mechanisms and/or modeling those and their effects on LOCA frequencies. So we had a number of people, for instance, that have probabalistic fracture background. We have a number of people we maybe didn't have a probabalistic fracture background, but they had a good background in service history and what degradation mechanisms they've seen in service.

So while I think Karen was I think without a doubt the most knowledgeable in material aspects per se, I would argue that all -- most. Maybe not all, but most of them panel had a sense of the importance of materials and had some experience with looking at changes in materials and how changes in materials and operating experience can effect LOCA frequencies.

Okay. So the objectives and scope. And, again, I'm covering old ground here but it's always nice to start off so that we're all clear as a group what we intended to do with this effort and what we didn't do. Because when you say LOCA frequencies, it's a very broad term. There are a lot of things which could lead to a LOCA. But one of the things that we had to do was try to minimize the scope of this exercise so that we could have one expert committee that had a shot in coming up with something that was reasonable.

And when we started these, we were really focusing on the new reg CR-5750 LOCA frequencies which were primarily concerned with estimating passive system LOCA failure frequencies by considering the effects of aging. So really what this effort was intended to do was to provide a fresh more rigorous look at those types of frequencies, realizing that LOCAs can come from other sources. But we've certainly done a lot of work as an agency trying to estimate the frequencies of these other sources as well. And the initial intent was we're going to provide a fresh look at this piece and combine with other work that has been historically but also that doesn't have ongoing at the same time.

So the primary objective really was to develop these generic BWR and PWR piping and nonpiping passive system LOCA frequency distributions as a function of break size, so that's the size of the LOCA and operating time.

And the sort of four subbullets of that. We were primarily concerned with LOCAs that initiate in the inisolable portion, so essentially primary side LOCAs.

We were focusing on LOCAs related to passive component aging, tempered by mitigation measures. Both programmatic and actual that are in place or that will likely be in place in the future.

Even though the focus on the 50.46 effort is really focused on large break LOCAs we thought in the interest of examining total plant risk, that it was really incumbent upon us to look at the LOCA sizes. Because if you're doing relative risk ranking, you just can't take an understanding of the large break LOCAs without also a similar knowledge of the small break LOCA to see how the risk changes and it's effected by potential plant changes.

So we were very clear that we couldn't just focus on large break LOCAs for this. We had to look at the whole spectrum. And the thing that we did with large breaks that's different from what we've done in the past, is we further subdivided the large break LOCAs into different categories depending on flow size, or either flow rate or break size so that we would be able to determine frequencies of these increasingly larger break sizes. And that's something that we haven't done, that no study has tried to do in the past to really partition those large break LOCAs in this way.

In terms of time frames, we looked at three different discreet time periods. We said we want to develop frequency distributions which are applicable and now. And what's now? Well, we said we've roughly got about 25 years of average operating experience, so we want to define a set of estimates that are applicable now.

We also looked at developing a set of estimates which would be applicable at the end of the original license. So about an average fleet life of

40 years or about 15 years from now.

And finally, we wanted to take these all the way out to the end of lice extension. So, again, an average plant life of about 60 years or 35 years from today.

So although we were looking at the effect of time, we were focusing on three discreet time periods and changes that could occur over those time periods and how the frequencies would be effected.

DR. FORD: Okay. Before you get off the subbullets, just to make sure I understand, you talk about in the second bullet mitigation measures. You're talking about regulatory mitigation measures not, for instance, changes in water chemistry?

MR. TREGONING: Both. Both.

DR. FORD: Both?

MR. TREGONING: Yes. Certainly, to use IGSCC for example, there is a number of mitigation techniques that are applied including pipe replacement, including water chemistry, including pipe sleeves.

DR. FORD: Yes.

MR. TREGONING: Including stress improvement. So there are four or five different mitigation techniques there--

DR. FORD: So those are covered?

MR. TREGONING: Oh, yes. Yes.

DR. FORD: Okay.

MR. TREGONING: That was a primary consideration. Because you can't just look at unabated aging. If you look at unabated aging without the effects of mitigation, you'll get a very skewed picture as to what the challenges are going to be.

DR. FORD: So contrary to what we heard this morning, those mitigation actions are plant specific? Some use whatever mitigation action, others don't.

MR. TREGONING: That's right.

DR. FORD: So contrary to what we heard this morning, you do have the ability to quantify the changes in delta CDF or ultimately delta CDF with frequencies -- for various plant specific operating conditions?

MR. TREGONING: Let me clear. While we considered the effect of mitigation, they were considered as an effect of the industry as a whole. So we didn't necessarily go in for IGSCC and say okay, here's a mechanism. Okay. Let me presume that I've got a plant that's operating with a certain water chemistry, has certain pipe materials and is applied this other mitigation mechanisms, i.e, they do inspections so often with this reliability, they have also some stress improvement. What does that mean to that plant's LOCA frequencies? We didn't go down to that fine a level of detail.

DR. FORD: Okay.

MR. TREGONING: It was more of a sense of this is what the industry has done as a whole. They've applied these various mitigation measures as a whole which vary from plant-to-plant. What do we think the impact of these specific measures are on these generic frequencies? So it was a little bit more global in that sense than actually an attempt at a rigorous look at a specific set of conditions for anyone plant.

DR. FORD: Okay.

MR. TREGONING: And again, some of this is very consistent with what has been done with LOCA historically if you look at 5750 and other examples, the estimates have tended to be generic even though everyone certainly realizes that there are plant specific things or plant specific aspects of this which can make those frequencies go up or down. So there's nothing to say that the frequencies that we have in any way are limiting either positively or negatively. And we didn't attempt to do that. We just tried to get essentially a generic average across the fleet.

CHAIRMAN SHACK: I mean you have hot legs off -- 605, you have hot legs off 590 and it makes a big difference, but you're going to average that out.

MR. TREGONING: And when you look at PWSCC or CRDM cracking, that's obviously an important issue and something that the experts or the panelists had to rectify in their mind.

DR. SIEBER: So your data represents a mean and not an average, right?

MR. TREGONING: I'm going to have to ask Lee. We argue all the time about what the data really represents. I'll have to let you field that one.

DR. SIEBER: But from a regulatory standpoint if you're considering public safety, all you need is one LOCA and the one that you get is probably the one that is not at the mean is the worse one out there. And so how do you take that into account.

MR. TREGONING: Right. We asked just not for their best estimate guesses, but we also asked for the uncertainty about that best estimate.

DR. SIEBER: Yes, but do you focus on the outlier worst case plant, say, I don't even want this plant to have an event like this?

MR. TREGONING: Certainly when -- and I don't want to even pretend to answer. We have 12 different panelists that provided opinions.

DR. SIEBER: Okay.

MR. TREGONING: Certainly that was a consideration that we talked about.

DR. SIEBER: Okay.

MR. TREGONING: And that you would talk about. So when you're dealing with uncertainty, it's not just the uncertainty that you had regarding an event happening, but also the uncertainty that there could be another plant out there that might for whatever reason would have a confluence of factors that would lead to much higher LOCAs for some reason.

So that's what the uncertainty was intended to do, although again we were pretty clear in that we said we want to come up with average or generic values.

The other thing we asked for is, oh by the way, if there are specific combinations of factors which you do think are risk sensitive, we want to know about it because we need to do something pretty quickly about that particular plant.

DR. SIEBER: I would think so.

CHAIRMAN SHACK: Let me ask about that one again. I mean, when I saw the 95/5 in median, I assumed that those in fact were -- you thought most plants would be the median, the worse cases would be the 95th, the best cases would be the 5th. But you're saying that your 95/5 are estimates of uncertainty on the median?

MR. TREGONING: Yes. Primarily or about we'll says the means, yes.

MR. ABRAMSON: The kind of instructions or understanding, the instructions we gave to the experts were that there is some under these very specific conditions we're going to be asking them in, there is some true LOCA frequency out there. Think of that conceptually. And they're being asked to estimate that.

And the median, we say well that's your -- you have 50/50 chance that the true value is higher or lower. That was their so called mid value estimate. And the 9th percentile, you've only a 5 percent chance of exceeding it.

However, this kind of begs the question because what you're asking in effect is what it is that they think they're estimating. And I would say it would depend on the particular panel member or expert as to what combination of things they're doing. On the one hand they're asked to do kind of an industry-wide average, although it's separate from BWRs and PWRs. Do an industry-wide average. And at the same time they need to reflect or they need to have their answer somehow reflect the variability in the plant specific conditions. And what kind of mixture there is, we don't -- because we don't know.

MR. TREGONING: But we were pretty clear. It said if there is one plant or let's say a few plants that you think maybe outside of this average, that's not appropriate. But let us know what these conditions are so we can do something about it.

But if there are, let's say, a handful of plants that because of the way they're arranged they have a higher percentage of the risk than other plants because of the materials that they're using, because of the way the plant's designed; that if there's a handful or more of plants that will end up driving the risk, that that's appropriate to consider.

But, again, the amount with which each expert really did that, that's a tougher thing to try to address.

MR. ABRAMSON: We have some insight into

that from their rationale. Because, you know, we kept emphasizing we want their reasons and their rationale for their various choices they made.

MR. TREGONING: For instance with BWRs we got some estimates from one particular panelist that said, you know, for the core brace system, the core brace stainless, here's what I think the estimates are. If they are ferritic, here's what I think the estimates are. And the estimates were quite different.

And so my instructions back to that panelist were okay, this is very good but what we really need to do is get a sense for what you believe is most applicable. So when he went back and thought about his estimates, he said okay I've got a certain percentage out there that are stainless steel. A certain percentage that are ferritic. But I know the industry is moving toward replacement with ferritic and I expect them to get there. So I think that these ferritic numbers are more applicable; more applicable now and they will certainly be as we go forward into the future.

So that was the kind of decision making process that each of the experts had to utilize. Some people made those decisions and did sort of an average; well, I'd have this risk under this set of conditions and this risk under this set of conditions. I don't know how to weight them, so I'm essentially going to average them. But that was a very individual decision, certainly.

MR. SNODDERLY: Rob, I had a question on frequencies associated with normal operating loads and expected transients.

MR. TREGONING: We haven't gotten to that bullet yet.

MR. SNODDERLY: Sorry.

MR. TREGONING: That's okay.

MR. SNODDERLY: What if the Commission comes back and says -- because I think one of the questions that the staff was asking this morning was PRA scope, should it include external events and power shutdown. So if the Commission comes back and says we think it should include external events, could this study be used to account for that or what would you have to do develop frequencies, say, for external events?

MR. TREGONING: Well, again, we had a focus for this exercise which were commiserate with, again, how this type of information has been used in PRAs in the past and also we expected it to be used in

PRAs in the future, which made us have the first limitation of just focusing on normal operating loads and expected transients.

We certainly realized that the rarer transients, let's say seismic event or a very large water hammer event, that is a very plant specific question. And we certainly didn't believe that there would be any rational way that we could develop generic frequencies for challenges associated with those types of events.

So, what we had proposed to do there is we did ask the experts, and this gets at Bill's question. I'm not going to talk about this today, but we did ask the experts, you know, given the large load what's your conditional failure probability given that you've got degraded either primary side piping or nonpiping, to try to address that. I'm not going to talk about that today, but that is one area that while we had the experts together we went ahead and asked them that related question.

MR. SNODDERLY: So you don't care what the load is, but you could say you did ask given a large load what's the frequency?

MR. TREGONING: No. We defined the load. We prescribed the load.

MR. SNODDERLY: On, you defined the load?

MR. TREGONING: We didn't prescribe the frequency of such a load occurring.

MR. SNODDERLY: So this study could be useful in the sense that if someone then came to you and said, okay, given these seismic frequencies that create these loads, then you can say here's the likelihood that --

MR. TREGONING: Yes, I think there's some information -- I hesitate a little bit because we haven't analyzed any of those responses yet. So how useable or applicable they are, I'd like to withhold judgment.

The one thing I will say with rare event transients, there have a lot of work that this agency has done over its history to try to address that specific question. And there as no way within the time frame and scope of this elicitation that we were going to be prepared to majorally overturn that amount of work. So I think what at least our plans are now is within the context of 50.46 possible rule revision, is we have got to go back, and we're certainly planning to do this, and dust off some of that work and see if it's still applicable or see if there's areas of it that need to be refined to make it consistent with the intent of 50.46 revision. And some of what we need to do with follow on gets into the questions that we dealt with this morning in terms of what we get back, feedback we get back from the Commission in terms of guidance. How narrow or broad this rule is going to be? What things we need to consider or not. What PRA scope and quality are.

So all these things are really interrelated. And at this point Research is, I think, like NRR is taking a pretty cautious side because a cautious approach to where we need to go because we want to have a little bit more direction and guidance instead of just rushing off to get to some place.

So let me move to the final bullet. There was an implicit if not explicit assumption that for the future that the plant operating profiles will not significantly change. Now what does that mean?

Well, we have a certain service history that underlines in this whole effort. And what we were trying to do in the effort was take the service history that we have, not only events with respect to NOLOCA's that we'd essentially, especially for the big LOCAs, but also looking at the precursor service history, which is really what we focused on. Let's look at the precursor events and figure out how we can expand that information to evaluate LOCAs of various sizes and LOCAs into the future.

So we were very clear that if there were plant operating changes that undermined the integrity of this service experience, it would obviously invalidate whatever estimates we were making. So that's a pretty important consideration. And that's I think something that, we talked about the ten year reevaluation this morning. I think that's one reason why this is so important. If we do things, as we are, we're moving forward with power operates and things like that; as we make changes we need to see how the plant responds to those changes. And what we may find is that there are some things that we do that may result in increased precursor likelihood of certain types of failures and locations that we've never seen in the past. And that's why it's incumbent upon us to continually reassess these challenges in light of potential changes that could be made.

And I think that's why even though the ten year reevaluation is challenging from a regulatory and a Research perspective, from a technical perspective, it's absolutely necessary. And it's a prerequisite in my opinion for moving forward rationally with anything that we're going to do here.

Okay. And, again, I've covered most of this is pretty excruciating detail in past presentations. So all I've done today is I've sort of encapsulated the approach that we used in the elicitation. And I'm really going to focus on these last two bullets here which are bolded, which is the analyses of the results --

CHAIRMAN SHACK: I just want to come back to this quantify base case frequency, because I think this is historically true but in practice you didn't really do this.

MR. TREGONING: Okay.

CHAIRMAN SHACK: What did you get the base case frequency from? Is it really a service experience analysis?

MR. TREGONING: Well, we're going to talk about the base case frequency.

CHAIRMAN SHACK: You're going to talk about that? Okay.

MR. TREGONING: Yes. I just want to make it clear that I'm in focus on that. We did it, we had four analysts, we had two that used classical PFM techniques, we had two that used service history alone. But they all had information to service history data on which to calibrate or base their result.

CHAIRMAN SHACK: But thought you were here in November you said that only one of the PFM analyses were really thought to be valid, for example?

MR. TREGONING: If you asked my opinion, I think one of the analyses was more rigorous than the other, yes. I would say that. But what we did is all the analyses were presented to the expert. I didn't try to expert -- I'm sorry. I didn't try to bias their opinion in one way or the other. But one of the things we asked in the elicitation we asked them comment directly on the base case evaluation efforts, which ones they thought were good, which ones they didn't think was good, which ones hit the mark, which ones different. So that was a very important -- in fact, that's how we opened up each elicitation was we asked them for a synopsis or an evaluation of the work we did to develop base case frequencies. Was it helpful? Was it not helpful?

Not all the experts used the base case work that we developed as an anchor point. Some used other studies that they were aware of either out in the industry or other local in-house efforts. So it wasn't unanimous that everyone used these base case frequencies that we developed. But I'd say most of the 12, at least 10 or 11 did.

And one of the things we asked is we said okay if you had to pick one of the fours, the one that you think is the best, which one would you pick and which one you like to use. And everyone indicated a response for that.

What we tried to do was just present the information to the panelists without bias, as much as that's possible, and let them decide what they think is appropriate, what they don't think is appropriate.

I will say that the panelists tended to confirm my expectations. So, for whatever that's worth.

Okay. So let me briefly step through the approach again. And, again, we can go into this, the various aspects of the approach in as much or as little detail as you'd like. I'm just sort of sketching what we did here, realizing that we've got a limited amount of time and wanting to focus on the results.

But we started about two years ago. The pilot elicitation, this was the internal staff effort that I talked about. And we used that to develop technical issues, come with a structure for the elicitation and test out some sample questions just with NRC staff.

We also developed some frequency estimates as a result of that exercise, which we used at the time to evaluate the feasibility of elimination of LOCA/LOOP requirements that we talked about this morning. And what you'll see today is I presented the results a few years ago back in front of the ACRS. And you'll actually see some comparisons later between the news results and those earlier results.

The next thing we did is we selected the panelists or the expert panel and the facilitation team. The facilitation team, there was about six of us technical experts and then we had Lee who was sort of our elicitation and statistical expert to help guide the process.

Then as a group we developed the technical issues that we were going to try to address. We constructed an approach for estimating LOCA frequencies and we identified significant issues that we wanted to address and ask about in the elicitation.

Then the next thing that we did which we've talked about a little bit, is we developed a set of base case frequencies. And the idea behind that is we wanted to structure the elicitation so we were asking the panelists to give us relative frequencies, not absolute. But relative differences between, let's say, a base case or a set of quantified frequencies and some other set of conditions. So one of the things we did as a group is we developed a framework for those frequencies; what piping systems are we going to evaluate, what degradation measures, what mitigation measures do we want to employ. So as a group we developed these sets of conditions and then we went and carried out the analysis using a subset of four the panelists. And, again, two of them used primarily PFM and two of them used strictly operating experience type of analysis.

The next step was to formulate the questions themselves, which we fed back to the panel before we asked them the questions and they actually participated in actually formulating the questions which was important so that we wanted to make sure they knew what they were answering. And then we conducted the individual elicitation.

And all of this effort was finished at about the end of October. And since that work was completed, we entered the next phase which is the analysis of the results, which we've completed most of this. There's some additional work to do.

And we're entering the final phase now,

which is the summary and the documentation of the results. And as I indicated earlier, it's these last two bullets that I really want to focus on.

I need to put this up because we're going to be dealing with results. I think it's important for us to view these results within context.

I talked about the fact that we looked at historical LOCA sizes and we also looked at large break LOCA sizes that we further partitioned. So this is the way that we did the partitioning. And I've shown this to you before, we had six different LOCA categories. And when you see all the results, a lot of the results are plotted as a function of LOCA category 1, 2, 3, 4, 5, 6. These categories are cumulative in the sense that category 1 considers any break than 100 gpm. Category 2 is any break greater than 1500 gpm flow rate. And category 6 is any break greater than 500,000 gpm flow rate.

The first three LOCA categories, 1, 2 and 3, are similar or analogous to historical definitions we've had of small break, medium and large break LOCAs. The only difference is that historically small break LOCAs are defined not as a threshold of greater than 100 gpm leakers, but in a range between 100 and 1500 while medium breaks are in a range between 1500 and 5,000. And then large breaks encompass anything greater than 5,000. That's historically what we've done. So the only difference here is we're dealing with the threshold values instead of the ranges. And this was something that we did at the request of the experts because they thought they could provide estimates using this framework and structure and definition easier than they could in thinking about ranges of flow rates.

DR. RANSOM: In terms of relating flow rate to break size, did you just assume that the flow rate or volumetric flow rate is proportional to the cross sectional area or is it more complicated?

MR. TREGONING: It's a little bit more complicated. And I might ask someone from ALARA. What we did is we developed correlations and there are different correlations for steam and liquid and PWRs and BWRs based on simple correlations, closed form solutions but they were not simplistic as simply saying flow rates equal to break size.

And, Steve, do you want to --

MR. BAJOREK: This is Steve Bajorek from Research.

What we did is first we wanted to try to establish a framework for this because as we went back to some of the earlier work, some people had been using codes to predict this, others had been hand calculations. So we used Moody break flow for the larger sized breaks assuming that for a break of that size you'd be hypothesizing a double ended type of break or rapid depressurization of the system. And then as we got to down smaller break sizes, the break size would start to challenge the thickness of the pipe. So we thought we would be looking at something closer to an orifice, so we used the modified Zaloudek for the smaller break size areas.

DR. RANSOM: And what? These are all the initial pressure?

MR. BAJOREK: No. We would assume that they would vent down to a pressure that was corresponding to -- well, initial pressure for the BWRs. For the PWRs it would rapidly go down to a saturation pressure corresponding to hot leg temperature. And that's typically where you would see it get to in the first few seconds of a --

MR. TREGONING: I put the correlations we actually used. And they were a function of the normalized pipe, as Steve has mentioned as well as also the transport fluid.

DR. WALLIS: But the experts weren't asked about flow rate?

MR. TREGONING: No.

DR. WALLIS: They were asked about size.

MR. TREGONING: The experts were asked about size.

DR. WALLIS: And what sort of break shape--

DR. RANSOM: They were asked about size, not flow rate?

MR. TREGONING: Even though we defined the LOCA categories in terms of flow rate, we gave them correlations which I just showed you that relate them to size. And I will say all the experts when they developed their frequencies, they had break sizes in mind and then used that correlation at the end to partition their frequencies into a specific LOCA category.

DR. WALLIS: What did the breaks look like? What shapes did they have?

MR. TREGONING: Again, that was up to each expert. And each expert had to make the assumption or make the determination for a specific type of degradation mechanism and location. Not only degradation mechanism, but location what those breaks might look like.

DR. WALLIS: Well, did they have -- breaks, axial --

MR. TREGONING: They very well could, although again --

DR. WALLIS: So they could have a 19 inch break in a 42 inch pipe?

MR. TREGONING: Oh, yes. Yes. One of the things they had to consider was not only complete failure of a pipe but also partial failures within a pipe. And that was a challenging aspect of the elicitation. And that's very much state of the art trying to understand what the extent of damage is going to be given that you got a rapidly propagating failure event. It's not something that's easily calculable at this point. But people do have -- there is a lot of experience out there, I'll say benchtop, laboratory experience as well as operating experience to know what sort of failures, you know, what sort of degradation mechanisms can lead to certain failures.

For instance, with FAC, we've got experience that FAC can lead to very large sudden failures where some of the more stress -- I'll say thermal fatigue or something like that or areas where you have maybe an isolated crack, you tended to more likely get a much smaller confined failure when you see it.

So there is some of that experiential evidence that people relied on when they were determining again the potential severity of a break for a certain degradation mechanism. And when you get to things like, you know, potentially it's common cause, bolting failures and things like that, then you have to consider potentially that because it's common cause, that you have the entire casing that's split apart. So that was definitely a prime consideration that they all had to have.

DR. WALLIS: Well, did they have things like valve bodies where the some bolts fail and it breaks open on one side and squirts out?

MR. TREGONING: That was something that they had to consider, so yes. When we looked at bolt failures we said, you know, obviously you have redundancy with bolt patterns and things like that. And I'll be honest, this is a very difficult thing to try to access. You know, so you have to make an assessment well how many bolts do I need to fail before I'm going to get leak of any size? I mean, what sort of pattern? If they form -- if they failed in let's say a star pattern versus all in one location, you could have a totally different break size that would result from a certain number of bolts.

So its, you know, I don't want to give the impression that this was easy but that was certainly what we asked them to do and to consider in their analyses.

CHAIRMAN SHACK: I mean, did people actually go out and do an analyses for the flange defemination when four bolts fail on a manway cover?

MR. TREGONING: No. Not that I saw. I didn't see an analysis like that. But what people did, I mean people -- analyses like that have been done and people relied on those type of analyses and their recollection of what the results were from those types of analyses when making their estimate.

I don't want to downplay what these guys had to do. This was like challenging. I had a number of them come up to me and say this is probably the hardest thing I've ever had to do over my engineering career. And I was actually happy to hear them say that, because if they didn't say that it was an indication to me that they hadn't properly considered all the interactions and all the variables that come into play with leading to a break of a certain size. So the ones that told me that, I actually had increased confidence in their results because I knew that they properly weighed the things that they needed to weigh.

And this is the kind of thing at the end of the day, I mean it's almost like -- you know, a lot of us do this as engineers, but we have sort of gut check engineering. And there was a lot of this that they had to apply in their analyses. You know, does this seem right to me? Does it not seem right based on what I know?

And that's why when we made up this panel, you know the panel selection was obviously, if not the most important thing, certainly a key step. We really looked for people that: (a) had a lot of experience in the nuclear industry and I think all of our panelists had a minimum of 25 years. But not only that, but had not necessarily focused knowledge in a certain area like materials, although that was certainly important, but we were looking for people that were really broad based that knew at a minimum sort of a little bit about a lot of things. So we were looking for generalists.

DR. WALLIS: Do you have any idea about how a valve actually fails when you overtighten the bolts and how a manway actually fails or is it all theoretical?

MR. TREGONING: Again, there has been work done, not just in nuclear but in other industries that have looked at those types of things.

DR. WALLIS: Yes, I was worried about it. You said they were all experience in the nuclear. Well, nothing ever happens in the nuclear, so there's no basis.

MR. TREGONING: But we postulate things happening quite a bit. And we quite often as an industry, and there's certainly lots of precedence for this going outside and looking at related industries and related events in our industries to see how they may be applicable here.

DR. RANSOM: What sort of stresses were they told to consider? And I'm thinking like earthquake, water, hammer, over temperature, over pressure? Were all of those considered or --

MR. TREGONING: Well, again, I'll go back to the bullet that's up there now, the primary focus. We were primarily concerned with normal operating loads and expected transients. What are expected transients? We defined them as transients that one would expect over the 60 year life of a plant. So certainly smaller water handlers are something that you would expect over the life of the plant. Certainly heat up and cool down, those types of things. And all we tried to do was isolate those loadings which are truly rare; seismic and, again, maybe the large water handler from consideration. But any other transients were -- not only were they fair game, but certainly they were stressed that they need to be considered.

I don't need to stress this but I will, it's the transients that will lead to the failure usually. You will have condition that will develop usually and then you'll have a transient which will exacerbate that pre-existing condition and lead to a problem. Usually, not always, but usually.

DR. RANSOM: You're saying things like fatigue or something else, stress corrosion cracking.

MR. TREGONING: Stress corrosion cracking that has been evolving over some time period and then you have a minor pressure transient where the crack is close enough that it gets it to run and fail; those types of things.

CHAIRMAN SHACK: We're going to have to move along if we're going to get to the results here somewhere along the way.

MR. TREGONING: If you limit your questions, I can promise that we'll be there quickly.

This is the structure that we used. And, again, I've presented this before. I just want to touch on it here so that we're all thinking about the same thing when we evaluate these results.

So we have LOCA contributions which come from a variety of sources. And I've just focused on, I'll say, primary system LOCAs here. Not any LOCA that you could get. But we split the primary up into passive and active system LOCAs. But the focus of the elicitation was the passive system LOCAs.

We further partitioned that into piping and nonpiping contributions. And then we defined piping system and nonpiping components which could lead to a primary system LOCA. And then we had for each of these systems and components, we had what we called variable classes. So these were areas where the variables within these variable classes would determine our LOCA likelihood. So it's like geometry, what your pipe geometry was, how big it was, what the layout was, what the loading history of the system was, what mitigation and maintenance is applicable, what materials are, what materials make up that system and what aging mechanisms are appropriate for those materials.

So what we did for all these systems was brainstormed all the possible variables that we would expect in these classes and then for a given either given or component, we matched the geometry, loading histories, materials, aging mechanisms and mitigation systems together to try to at least come up with a finite set of variables that the experts had to consider. We did the same thing with nonpiping, although we did it for pumps, steam generators and then the vessel itself pressurizers and valves.

Again, the base case work, again, this has been presented before.

For piping we specified specific conditions. And what do I mean by that? Well, we specified a piping system, size, material, loading, degradation mechanism and mitigation procedures. We specified all those as a group. We had five different piping systems that we looked at, 2 BWR and 3 PWR. And then we had four people estimate the frequencies of those defined conditions as a function of operating time. And two of those people, as I mentioned, use primarily operating experience and two used PFM.

For nonpiping we didn't use the same approach, just because the types of failures that you could get were so variable. With piping, piping designs are all similar, they all follow ASME code. The components are all piping. With nonpiping you're dealing with bolts and you're dealing with -- you know, you're dealing vessel, you're dealing with control rod drive mechanisms. You're dealing with a wider disparity of components and to come up with a base case for each of those components just wasn't trackable, given not only the time but the resources available.

And the other thing with nonpiping is we just don't have the richness or wealth of precursor information that we do with piping. We've got a lot of information on piping precursors. Not nearly as many on non piping.

So what we did for nonpiping is we actually developed a precursor database. We had two of the panelists go back to 1990 through LERF searches to identify precursor events and precursor events are components leaks. And then also partial leak or cracking events. And we supplemented those, this precursor database, with some targeted PFM studies that were done by another panelists to look at CRDM ejection failures and BWR vessel challenges. And by BWR vessel challenges, they would look at normal operating loading and LTOP.

Now, I've excluded PWR from here, although we did consider PWRs. We were very clear to the panelists to not consider the effects of the PTS on vessel failures. Well, why is that? Well, we've got this whole effort as a research community that we've had ongoing over the last four or five years now to evaluate those frequencies. And those frequencies are driven by the LOCA frequencies. So we didn't want to get into a circular argument sort of estimate where we were trying to -- we were going to be changing the LOCA frequencies which would change the PTS challenges. So we didn't want to base PTS failures on some study that could be fluid.

So what we had them do is we said consider vessel failure for the Ps, but don't consider PTS, consider everything else. So consider head degradation, consider failure due to -- I don't know, fatigue even though people have said for years that fatigue is not realistic for the vessel. But consider anything that's none PTS as being a fair game for the vessel.

CHAIRMAN SHACK: Now for the BWR Pete came up with his ten to the minus 35th again?

DR. WILLIAMS:

MR. TREGONING: No. That was not the base case number for the BWRs.

DR. WALLIS: What's the relationship

between these base cases and then the later cases? You've got a very small number of panelists doing the base case?

MR. TREGONING: Yes.

DR. WALLIS: But don't they influenced then what everybody else does later on?

MR. TREGONING: The quantitative estimates potentially impact what everyone else does later on. That's why we had the base case -- the way we set this up, is the four people that developed these estimates, they came back. We had a meeting in June. We defined what they were going to be analyzing. Then they came back in June and presented the results of their analysis to the entire panel, which the panel had a lot of comments about it, some good some bad. And as a result of that June meeting, the base case developers went back and did some more sensitivity analysis, they did some additional analysis. But the idea was while they influenced the results, we wanted to present exactly what was done to each panelist, and that way with them having a clear understanding of what was done if they wanted to adjust frequencies in any way they could go and do that essentially.

So while they do form the framework, each panelist was free to adjust these numbers however they saw fit. And, of course, they all did. And, again, some of them did not use those estimates at all. So we really gave each panelist the freedom to approach this in the way that made them most comfortable.

I showed this, and this -- Graham had mentioned this before, the wide disparity in the base case estimates. I think I presented this in November and we talked about this slide quite a bit. I hesitated putting it up, because I didn't know if we could get past it in the allotted time, but I thought I needed to do that again just to refresh everyone's memory about what we put up in November and use this as saying this was the basis for some of this work.

So what you see here at the two BWR base cases and the 3 PWR base cases plotted side-by-side. And these are the estimates at 25 years. So what the analysts predicted were the LOCA frequency estimates for the base cases right now.

And you see failure frequencies as a function of these LOCA categories that we define. So LOCA category 1 is the small LOCA, LOCA category 6 is the large LOCA.

The way we defined the categories, the numbers all decrease because category 1 also include category 6. So when we look at all the plots that I'm going to show today, they all have to be going down --

DR. WALLIS: Except number 5 is peculiar. Well, it's just the bottom end has disappeared. It's sort of -- everything's come together again.

MR. TREGONING: Oh, well --

DR. WALLIS: Very peculiar.

MR. TREGONING: It's not that it came together. So you're looking at BWR 2 base case. It was that expert did not give us an estimate for --

DR. WALLIS: It was just off scale. You didn't show it.

MR. TREGONING: No, no, no. I've shown you. The scale is down to ten to the minus 18, so you know I didn't have to go too much further off scale. So, no, these are all the results as actually developed.

DR. FORD: Oh, I see. So the two points for each of the cases are the two panelists?

MR. TREGONING: Yes. The number of points that you see here are the number of panelists that we got an estimate from of the four. Of the four, we didn't get an estimate for everything.

For instance, one of the analysts did not feel that he had sufficient expertise in BWRs, so he didn't give us any BWR estimates. He only gave us PWR estimates. That's why you see a fourth data point for the Ps and why there are only like three for the Bs.

And even for the Bs, not all of them gave us estimates for every LOCA size.

DR. FORD: Now just to calibrate myself on the BWR cases you've got to deformation mechanisms. The one is IGSCC presumably at 304 and the other one is transgranular cracking and assisted corrosion presumably in carbon steel piping.

MR. TREGONING: That's correct.

DR. FORD: Looking at category 1, there's a lot of data in industry for failures around the world for those two failure modes. Do those frequencies -- observed frequencies correspond to those frequencies that give an --

MR. TREGONING: Well, again, the service history estimates certainly base their estimates on that information, on the available information of precursor events that --

CHAIRMAN SHACK: There's no failure data, Peter. There's plenty of cracking and leaking data, but there's no failure --

MR. TREGONING: There's precursor data. Precursor.

DR. FORD: Well, cracking data.

CHAIRMAN SHACK: Yes. Well --

DR. FORD: But looking at category 1 --

CHAIRMAN SHACK: But a crack -- a crack has -- well, no, no. A 100 gallon leak --

DR. FORD: No.

MR. TREGONING: Yes.

MR. TREGONING: Category 1 is a 100 gallon leak. A gallon -- what we did -- what we did is we defined a LOCA category zero, which is not on here. But on LOCA category zero is essentially a leak.

DR. FORD: A drip?

MR. TREGONING: Yes. And we didn't define it that precisely. It was essentially through --

DR. FORD: But what my question is really driving at is there must be some kind of qualification of those opinions.

MR. TREGONING: Again, those opinions were qualified by the amount of precursor data that's out there. So that category zero information.

DR. FORD: Well, let me have a follow up question. Taking the BWR 1 situation IGSCC. That might well have been the situation, say, 20 years ago when we were operating at .5 -- per centimeter. Now it's literally even for a drip, it's essentially zero.

MR. TREGONING: Well, essentially zero,

but it's much lower than it was.

DR. FORD: Way down at the bottom. It's way down at the bottom. So if you're looking at current fleet, how does that fact meld into your prediction?

MR. TREGONING: And that's one of the reasons that certainly service history -- when you look at service history data you have to have a very keen eye to evaluate it because knowing the changes that have been made and how it potentially effects things, you can have dramatically different estimates. And IGSCC is a great example of that because we had so much data that we generated on IGSCC in service in the mid-'70s up to sort of mid '80s.

So what we did specifically for IGSCC, we developed estimates pre and post mitigation. And we defined mitigation time as roughly being 1983 or so.

DR. FORD: Well, okay.

MR. TREGONING: So the way we defined the base case we said, all right, was normal water chemistry. Okay. Normal three or four stainless, but it's got a weld overlay. So we had on particular mitigation mechanism that we applied. And that's something that we felt could model with PFM as well as capture through the service history database. So while we didn't -- you know, there's no plant out there that has that specific set of conditions, those were the conditions that we defined to analyze our base case results.

DR. SIEBER: What's the reason for the 15 orders of magnitude variability in category 4 for BWRs?

MR. TREGONING: I can address that one, and this is why it's important to feed this back to the experts. The PFM analysis that was done was, even though the conditions defined we're evaluating both thermal fatigue and flow accelerated corrosion failures, that particular PFM algorithm did not have an appropriate -- so they really only estimated thermal fatigue. And as you might imagine, the thermal likelihood of failure for the feed water is pretty low.

And these differences -- this is why we had this meeting with the expert to point out exactly these differences. And these differences have been the things that I think in the past is what we've always aught ourselves on. Because we've had these PFM estimates and we've had service history estimates. We've never really tried to rectify them in some way. It's been even, I'll say, quasi-rigorous.

Here we at least said okay, go do your estimates, provide the basis for these estimates to the expert and then let them decide what's more appropriate when they make their assessment.

So there's clearly some big differences, and those big differences are due to a variety of things, not the least bit of which is limitations of the specific analytical technique.

DR. SIEBER: That's probably the biggest difference I've ever seen in any analysis.

MR. TREGONING: Yes. For LOCA frequencies, no. I've seen -- this did not surprise me.

I see Bill shaking his head. It's disconcerting but it's not something that's unusual, unfortunately. It's one of the reasons that PFM has got a bad rap over the years for this stuff because you come back --

CHAIRMAN SHACK: Well, but I mean they're really not comparable. If one guys looking only at thermal fatigue and the other guy's looking at FAT --

DR. SIEBER: Well, on the other hand, it's on the same chart and you ask yourself the question what do you make out of this when you have such a huge variation.

MR. TREGONING: Right. I know, again, I'm doing a little bit of a disservice to the information to plot it all on one chart.

DR. SIEBER: Yes. It'd been better had you not even told us.

MR. TREGONING: No. In the interest of disclosure, you know, I have to tell you. And this was information that was provided to the experts. And, again, I'm not --

DR. SIEBER: But this is the first round and then they got an opportunity to sit there and ponder?

MR. TREGONING: No. Let me be clear. This is the base information. So this was information before the experts went off and gave us any judgment as to what these frequencies were.

DR. SIEBER: Oh.

MR. TREGONING: This was just I'll call it underlying technical information that we provided to each expert.

DR. SIEBER: Okay.

MR. TREGONING: And believe me, the people that developed the base cases, they realized themselves, obviously, that there were limitations in their approach.

DR. SIEBER: Right.

MR. TREGONING: So, you know, the guy who came back with ten to the minus 18, he didn't say well that's the frequency of the B water line failures because he realized that he had limitations in his analysis to cause that frequency to increase. And those were something that he had to consider in his elicitation.

DR. FORD: So for the BWR case, one panel member said you've got to be kidding, you're never going to get 25,000 gallons per minute from that fact?

MR. TREGONING: That's right. That's right.

DR. FORD: Presuming that fact situation.

MR. TREGONING: Yes.

DR. FORD: Whereas the other guy, said, yes there's a --

MR. TREGONING: Right.

DR. FORD: It was as uncomplicated as that? Is that right?

MR. TREGONING: It was as simplistic as that.

DR. FORD: Simplistic.

MR. TREGONING: Where what we did with the base case people is we met as a group and we had weekly phone calls to make sure we were analyzing as close as we can the same thing, you know the same set of conditions. But then we turned them off and said don't consult with each other. Do your analyses and come back and tell us what you get.

DR. FORD: Okay.

MR. TREGONING: So then they came back and told us what they got. And, of course, you look at this and you say well, you know, that's a pretty big disparity.

DR. FORD: Yes.

MR. TREGONING: And the next thing we did as a group is we looked at this and we said well let's identify some possible reasons for this disparity that we can give to the experts or the panelists so that, again, when they make their assessment they have these things clear in their mind. And, again, that's the way we structured it.

DR. SIEBER: I would be delighted if we would move on.

DR. RANSOM: Well, this thermal fatigue thing -- are those frequency units supposed to be different on those two graphs? Yes, you got like cal per year and then you got R per year.

MR. TREGONING: I'm sorry. They should be calendar year. That's just a typo.

DR. RANSOM: It means calendar year?

MR. TREGONING: Yes, per calendar year. No, I apologize for that.

DR. RANSOM: So the minus one should be outside, I guess, the bracket, right?

MR. TREGONING: Well, yes.

DR. RANSOM: And what is the R year.

MR. TREGONING: That's reactor, but they should be calendar. So that's a typo, so I apologize for that.

DR. WALLIS: Well this thermal fatigue for instance, he got such a small number. He must assume something about a very mild thermal condition and it's probably quite likely that that it's a probability of ten to the minus six or something that you could get very severe thermal conditions, but he doesn't know that.

MR. TREGONING: Well, no what -- and I know you want to move on.

DR. WALLIS: It all depends on what goes in. If you move garbage in you're going to get garbage out.

MR. TREGONING: Right. At the danger of belaboring this point, this is all the same person.

DR. WALLIS: Yes, but has to have some inputs to his analyses.

MR. TREGONING: Of course. What he said here is that there's a relatively high likelihood that he gets a small LOCA at a thermal leak. And transient is really what's driving how quickly that crack goes through a wall. What drives here are the characteristics of that crack as it goes through a wall. Because one of the things we made very clear, hey, if this thing goes through a wall and we get leaks that are greater than 1 gpm, we have to assume that it's detected at that point. Because we have a lot of regulatory basis for ensuring that that happens.

So what this guy is saying, not that the likelihood of a thermal fatigue is small --

DR. WALLIS: Could you use the microphone?

MR. TREGONING: Oh, I'm sorry. Sorry.

DR. WALLIS: The thermal figure has happened and there have been pipes that have failed in reactors due to thermal fatigue.

MR. TREGONING: Yes. But, again, let me be clear. What he's saying is not that thermal fatigue likelihood is unbelievably small, but that the likelihood of getting a very large LOCA from thermal fatigue is very small. And that's a different thing. That's a totally different statement.

And the thermal conditions for the most part I would argue are going to be more important in determining this. And it's the characteristics of the cracking and the failure which are going to determine that.

CHAIRMAN SHACK: In the simple-minded term the crack goes through a wall at about a two to one ratio. So that by the time you go through a wall an inch and a half, you've got a three inch long crack. Well, in a 22 inch diameter line, a three inch crack doesn't mean much except that you've got water on the floor.

MR. TREGONING: And that's our leak before break philosophy, which is again --

DR. SIEBER: This is why the big pipes are better than the small ones?

MR. TREGONING: That's right. You asked this morning, and that's definitely one reason. Definitely. And it's a strong reason. Even though they're designed to the same nominal margin, that's one reason why they tend to be more robust.

Okay. Moving right along. This is the flow chart which we used, not only to analyze the responses but also to develop the responses that we got from the experts. And they're very similar for piping and nonpiping, a similar structure. But at the starting point for each of these and these sort of pink shaded regions and it doesn't show up very well here, are the panel input areas. And then at the lower right are the results that we got.

So we asked from each panel member to select a base case for each -- either piping system or nonpiping components. Then we asked for their adjustment ratios. How would you adjust for that system these base case frequencies as a function of time and LOCA size.

Based on these two inputs we developed a set of system related frequencies for either a piping system or subcomponent frequencies for a nonpiping component. The sum overall either the piping systems or the subcomponents, and there's another adjustment to adjust for the percent contribution that the expert thought that they were providing us with. We didn't ask them to evaluate every single thing or every single piping system or issue. We said focus on the ones that you think are most risk significant, or I'll say LOCA significant. Let me clear. Not risk, but LOCA significant.

And what we defined significancy, we said essentially consider those systems which in your opinion give us 80 percent of the LOCA contribution.

So this percent --

DR. FORD: Or more.

MR. TREGONING: Or more. Some considered all the systems. So for those people there would be no percent contribution adjustment. Some didn't even make it up to 80 percent. Some were at 70 percent based on their opinion. So we adjusted --

DR. WALLIS: So if they did nothing, you divide by zero, is that right?

MR. TREGONING: That was not an opinion. That was not opinion.

DR. WALLIS: If they did very little, though.

MR. TREGONING: Yes, but this was a minor adjustment and it doesn't affect the result. It was usually again 81 over, you know, eight or 1.125. So in the LOCA frequency game, it's almost imperceptible.

But then once we make that adjustment then we get for each panel member either piping or nonpiping frequencies. And Lee's going to go into the analysis framework a little bit in more detail now if there are no more specific -- we're going to come back to this, because he's going to show you how the responses factors into each of these blocks.

MR. ABRAMSON: Okay. We start with the flow chart idea, the participants used the base case conditions and the frequencies and then they provide the ratios. And the ratios, as we mentioned before, we asked them always three numbers. Their mid values and then what we called their upper bound and their lower bound. The mid values was like the medians and the upper bound was like a 95 percent confidence bound, if you like at 95 percent and the lower bound was 5 percent.

So we asked this for everything that they gave us. All the numbers they gave us.

And we also, as Rob indicated, focused on the important contributing factors. They didn't have to consider everything because there was just so much to consider, but just what were the big contributors.

What we did is we took each panel member's results and we took those and then we, as I'm going to show you in the next couple of slides, we propagated all those numbers through to their final estimate for each panel member. So we got individual estimates for each panel member and then you'll show how we compared them and so on and so forth.

And we also did, as we'll touch on later, so called kind of consensus approach or something like that. But our main results and essentially everything you're going to see today is all based on the individual panel results.

A big advantage of this is that they're so consistent. If you try to do any kind of a consensus approach, they you always have the problem of how do you know that the answer for this part, this component, is consistent with that part. Because like you have, you know, it's a big Chinese menu. You have one from group A and one from group B and one from group C and so on.

And we took a lot of -- I mean, certainly I'm sure the panel members did and also we in our elicitation took a great deal of effort and time to try to have their results be as consistent as possible. So from this perspective, you can say that we've gotten -- well, in the case of -- I think we had 8 panel members. We had enough information to get, what was it? Eight PWR estimates and 9 for BWR estimates.

MR. TREGONING: Reverse.

MR. ABRAMSON: Reverse? Okay. Eight for the Bs and 9 for the Ps. All right.

Now what did we get? What we were interested in our bottom line is we want to get some kind of distribution. We're assuming there's some kind of distribution to express all of the uncertainties. And we did this, and we did this separately from the Bs and the Ps in the piping and nonpiping, as we indicated and you're going to see the details of those results.

As far as the distribution is concerned, we got four parameters for each of the distribution; the mean, the median, the 95th percentile and the 5th percentile. I estimate that this 95th percentile is not the same thing as what we got from the experts, the 95th bound because all of these were propagated through. But you think of that the final answers we got, the LOCA frequency, there is an uncertainty distribution and what we're trying to do for each expert again is to estimate what the parameters, these four parameters of that distribution are.

And in addition, we're going to calculate the confidence intervals for these parameters, and we'll go into the detail of that later.

Now, it's very important as we've of course emphasized and you're well aware, that our estimates reflect what we call both uncertainty and variability. By uncertainty we're trying to be pretty consist here. Uncertainty we mean the uncertainty that comes out of the individual panel member responses. And that is driven by the fact that we got not only their mid values, but their upper bounds and the lower bounds. And so these upper bounds and the lower bounds are their expression of their uncertainty about the numbers they're giving us.

And we propagate that through, that's uncertainly. And that propagates through into the 95th percentile, the 5th percentile of those final distributions.

Then we have variability. And variability it just has to do with the fact that we had 12 panel members so each one is giving us a different answer. So that's panel variability. So that's the distinction. Uncertainly is based on the individual uncertainty and variability is the difference between different panel members' responses.

Now as far as the, say, the mathematical details of the propagation, we made the usual assumption about lognormal distribution. This seemed very appropriate because everything here in effect is on a log scale. We're always asking for ratios in their responses. And every indication is that this is, you know, this as good as an assumption as any.

So to start with we assumed that for each question that we asked that we asked the experts that their mid value, their upper bound and their lower bound were points on a lognormal distribution for that particular expert. And then we propagated that through.

Now there's one problem with that, because it is a lognormal. See, the upper bound is supposed to be the 95 percentile and the lower bound is supposed to be a 5th percentile. If this is a lognormal distribution, they'll be symmetric in a ratio sense about the mid value. Sometimes they were, they gave us those answers. And it was a natural thing for them to do. But sometimes they weren't.

If that's the case, what we did we is we assumed in effect what we called a split distribution. And in fact the distribution was two parts. There was an upper part and a lower part. And so the upper part was a lognormal, but just -- and determined by the mid value on the upper bound. And you can do that because we just have to parameter for the lognormal. And similarly the lower part was another lognormal determined by the lower bound and the same mid value.

So what we did is we propagated these

things through separately. That's how we did it.

Okay. So this set the stage as far as what we were assuming --

CHAIRMAN SHACK: Yes, I went through your numbers. They were surprising lognormal to me when I-- you know, I just did the quick check. Either they were cheating or they think lognormally.

MR. ABRAMSON: Well, each one knew what they were giving us, so they said -- you know, they gave us a number, say, a ratio of say five to one. I said all right what's your uncertainty in this? Well, I think it's a factor of ten. On the high side. What is on the low side? Oh, maybe a factor of ten there, too.

MR. TREGONING: Yes, it was the latter. They tended to think lognormally.

MR. ABRAMSON: They tended to think lognormally. So in a sense this was partially forced but not everybody -- you know, they weren't going locked stepped this way. Some people did give us asymmetric numbers and we had to deal with that as well.

MR. TREGONING: So, there were two or three estimates which were very asymmetric that a signal lognormal distribution would not have been applicable.

MR. ABRAMSON: Yes. And, of course, I think needless to say what I think what everybody's interested in is on the upper end not on the lower end. The lower end is going to drive the 5th percentile, that's the upper end that's going to drive the 95th percentile.

Another way of putting this is that even though we assumed a lognormal distribution for each, what really matters is the upper part of this distribution as far as what we're concerned about here. We're not interested in how low the lowest can be. We're interested much more in how high they can be.

All right. Then going back again to the flow chart, if you recall that, you have in the box there is an adjustment ratio. So you see the first two boxes there's a multiplication. All right. So each one of those, the base numbers are assumed lognormal and the adjust ratios are assumed lognormal. So what we need to do is to multiple these two numbers. We're multiplying two lognormals.

Well, the product of lognormals is always a lognormal distribution, so it's very easy to do that and calculate the parameters.

We also assumed that those two were independent; that is the base case frequencies and the ratios were independent, that's statistically independent. And this seemed like a very plausible assumption because they come from completely different, you know, sources. The base cases were, as I said, developed by these and maybe adjusted by the base case panelists. But then each panel member decided what his ratio would be to that base case. So this seemed very plausible that it would be independent, and we assumed that.

And then what we did is we just calculated, you have a product of two lognormals, you calculate the mean and the percentiles for that product given the initial assumptions.

Then the next step, as you see, we have to sum things. So we have a sum of lognormals. Now, a sum of lognormals is not a lognormal distribution in general. It never would be unless they happened to be perfectly correlated. How do we handle that?

Well, we have a sum of these log normals. Well, we're interested, of course, ultimately in the mean. Well, the mean of the sum is the sum of the means regardless of what -- correlated or not. So it was very easy to get the mean of that sum, because we had the individuals means.

Then as far as the variance is concerned, we had to consider the fact that these are correlated. In fact, what you have there is the very system frequencies. It's the system frequencies their distribution that we're adding. Now, they would tend to be correlated and they would tend to be correlated, maybe even highly correlated but they are positively correlated. Because if somebody, some expert said well this is high, we would also tend to think that the others are high as well. So it was plausible to assume they were positively correlated.

If that's the case what we can do is we can say that the results are bounded in two ways. First of all, you have the independent case which is zero correlation. And then you have -- you consider the perfect correlation case where the correlation is as high as it can possibly be.

And where we used that was in calculating the variance. Because we're doing the writing of random variables. And all you need for the sum is you need the mean and the variance. Because ultimately we're going to assume that the final results is lognormal. That's what we did. So we need its mean and variance.

We have the means already because we just add up the means of the individual components. And then if you assume that they're perfectly correlated-- this perfect correlation case which gives you an upper bound on the variance. It's very easy to show that. It's going to give you an upper bound on the variance, so that's a conservative situation. And you can then calculate what the variance of the sum would be with the perfect correlation. And that's exactly what we did.

We're also do, as Rob's going to note later, as a sensitive case we can look at a lower bound, which is an independent case. And we've done, I think, some partial calculations and it turns out that it doesn't really make much difference.

MR. TREGONING: No, we've done this calculation. You can bound the uncertainty by either assuming full correlation or independence.

MR. ABRAMSON: Independence, right.

MR. TREGONING: And the variance doesn't matter very much.

MR. ABRAMSON: The variance doesn't matter very much.

MR. TREGONING: That's a truism about all the results.

MR. ABRAMSON: Yes.

MR. TREGONING: I haven't seen one case where that assumption affected the variance.

MR. ABRAMSON: Yes. One partial rationale for that is we're adding up a number of things, but generally in this there's going to be one or maybe two dominant cases. If there's one dominant contributor, then it doesn't matter what the others are. If there two, well then maybe it does matter a little bit. So this is why I think one reason why you have the difference -- why these -- the actual variance is bounded pretty closely on top and bottom, where the two bounds are fairly close.

All right. So this is the methodology we used. We just added up all the various system frequency distribution. We got their means and then we got the variances. And now we have -- a variance. And then we assumed that the results was a lognormal distribution. And then from that you can back calculate what the median is and what the 95th and 5th percentiles are.

I should also say we do this separately for this upper bound and the lower bound for the split distribution. So we carried that thing through all the way.

And that's essentially the structure that we used. If anybody has any questions about this.

So the final results, and that's what you're going to be seeing now that Rob will go into the details, is based on this analyses. And it's all based on the assumption that what you finally have is from a lognormal distribution. And what you see, again, is we're summarizing this by in various cases the means, the medians, the 95th percentile and the 5th percentile.

MR. TREGONING: Okay. Thanks, Lee.

DR. SIEBER: Thank you.

MR. TREGONING: Now next I've got, and I'll let you decide as a Committee where you'd like to go. I've got a number of slides that present or provide sort of general rationale and insights. These are qualitative opinions that we got from the panel. Again, this isn't exhaustive. It's just some things I wanted to highlight.

I wanted to use these first to set the stage for the results so you can understand the basis of the results better. But if you would like to go right to the quantitative estimates and come back to these qualitative rationales as need be, we can follow that approach as well.

Is there any sense for -- would you like to hear this first? That was my original intent, or would you like to jump right the numbers and start looking at the numbers and then trying to understand them maybe through some of this rationale and insights later?

CHAIRMAN SHACK: Well, without the rationale and insights, the numbers are just numbers.

MR. TREGONING: Okay.

CHAIRMAN SHACK: Let's data, hopefully.

DR. SIEBER: Before you'd launch into this, I have a question --

CHAIRMAN SHACK: It's data with a small "d" at least.

DR. SIEBER: -- that relates to this. For example, if I look at operating history, to me a significant event was the crack in the RCS piping at Summer. Now, I presume, you know, that pipe cracked and leaked on the floor, but I presume a fracture mechanics analysis would have shown that that crack would have arrested before it became a large break. Is that correct? If that's correct, then that is really not a precursor to a full blown LOCA. Are these fair statements for me to make? Because right now I worry about the existence of that event and how it impacts what it is you're telling us here. And the numbers that I see are pretty small, the frequency numbers. On the other hand here's an example of a crack that leaked and arrested, and my question is how far away were we at that point in time from having a major LOCA?

MR. TREGONING: And that's why when you look at service history and you apply it and look at precursors, and I'll use precursors globally to mean cracks or leaks.

DR. SIEBER: Right.

MR. TREGONING: It's a very difficult assertion. Because cracks like the cracks that were found in Summer don't tend to be LOCA challenges just because they axially oriented instead of circumferentially oriented.

DR. SIEBER: Right. And they're arrested.

MR. TREGONING: Yes. Well, it would have arrested in the base material if it --

DR. SIEBER: Right.

MR. TREGONING: At some point

DR. SIEBER: You would have found some --

MR. TREGONING: Yes, that's the expectation.

DR. SIEBER: Okay.

MR. TREGONING: And the experts, I'd say to a person, certainly understood that distinction. And when they looked at service history they were concerned with, again, estimating the challenges of those types of degradation and flaws which can lead to LOCAs, which again tend to be the circumferentially oriented cracks or mechanisms where you have a more, I'll say, global erosion of the material, something like FAC or something like we had Davis-Besse

DR. SIEBER: See, I bring this issue up because a member of the public who has superficial knowledge of what is going on but knows about that event would point to what you're saying and say you're wrong. And so I think at least for the sake of the record we ought to say that what you're doing is not inconsistent with what's been observed.

MR. TREGONING: Right. And I would argue all of these things are precursors.

DR. SIEBER: Right.

MR. TREGONING: But the challenge associated with the precursors varies dramatically. And what happened at Summer has ramifications that the panel, I think, expressed pretty clearly. But not related to that particular event, but related to their concerns that that may event may uncover more widespread degradation where we could have circumferentially oriented cracks, it could be leading the larger LOCA challenges for the fleet in general.

DR. SIEBER: Well see now, I share that kind of concern. But to me there doesn't seem to be a lot of actual real data out there that would allow you to draw that conclusion with any certainty.

MR. TREGONING: It's coming. We're starting to see it, I fear. We've started to see over the last year or so -- we certainly have that indication and at the risk of -- Dr. Shack would be much more eloquent than me at speaking about this. But similar degradation as we saw at Summer we have indication of that happening in base material on a pretty wide spread nature within the CRDM mechanisms. and we have --

DR. SIEBER: Yes. But I don't worry so much about that because that has a -- a hole size.

MR. TREGONING: Okay. But --

DR. SIEBER: And it's in a pretty good position as far as taking care of the core.

MR. TREGONING: Right. But what the experts -- what you need to do then is you need to say, okay, that's a specific location.

Are there features of that degradation

which lend themselves to --

DR. SIEBER: How about the surge line?

MR. TREGONING: Exactly. And we've started to see some surge line indications potentially. So -- and certainly pressurizers in other places. So PWSCC was something that was considered very -- there was a lot of concern. And I would say across the board for Ps, and we're going to get to that, but that was the mechanism that the panelists were far away most concerned about for Ps.

And they I think at the expert -- I mean what I was told, they really looked at where we are in the history of Ps and their opinion is being somewhat commiserate with where we were back in the late '70s with IGSCC and boiler.

DR. SIEBER: Right.

MR. TREGONING: So they take it that seriously.

DR. SIEBER: So I can rest assured that as you attempt to risk-inform 50.46 that these factors are well known to you and are taken into account, including this recent history?

MR. TREGONING: Again, we're continually updating our knowledge. I don't want to use well known because there's a lot about PWSCC that we're still trying to understand.

DR. SIEBER: Well, the only things that are well known are the things that you know well.

MR. TREGONING: Right.

DR. SIEBER: If you don't know it, you know, you don't.

MR. TREGONING: And that's why as Dr. Ford said, you know, ten years is not always sufficient. And that's why we need to be continually updating.

You don't do this effort and say all right we're done, that's stop, we don't need to worry about this stuff anymore. That's not the intent. And I don't think that was the expectation of any of the panelists.

You continue to try to increase your understanding as you go and you evaluate things as they come up and look at their severity and potential generic implications just as we have all along. In fact, you hopefully try to do it better and more intelligently. That's why, you know, proactive degradation programs are becoming more the vogue because of the potential ability to do this more intelligently with more foresight than we have in the past where we've just said we're going to wait until something happens and then address it. The idea is now that we're evolving to, let's be smarter about where we think things are going to happen so that we can try to nip it before we really see it.

All of these things are inner related and philosophically they all have to be considered when you're developing these estimates. And I will say the panelists were, again, very serious in their task in terms of making those types of assessments.

DR. SIEBER: Well, I do have a concern about what the agency is doing with 50.46. And the questions I just asked reached to one of the roots of that concern. So I guess I will wait and see where we go as to whether my concern goes away or gets worse. But I appreciate your explanation.

MR. TREGONING: Sure.

Okay. Again --

DR. RANSOM: Has this elicitation process been used in other industries? Did you model what you have done after --

MR. ABRAMSON: I would say it's probably most developed in the nuclear industry. It's been used a lot in quite a number of cases, 11.50 used part of it and so on. And we also used it for PTS that was reported on a few years ago, and so on. They also had a big elicitation, we had a panel of 17 people there.

Although it's been developed in a sense independently of the nuclear industry by decision analysts and applied psychologists and so on, some of the techniques and so on, it's been around a long time maybe even before there was a nuclear industry here. Remember the Delphi method? There was a Delphi which is a predecessor to this. So there were things that were done maybe 40 years or so ago which led into this.

In other words, how do you take a group of people and get expert opinion with them as a substitute for data and so on. Data theory modeling and so on.

So in this sense, it's evolved. I'd say it's in most used in the nuclear industry.

MR. TREGONING: But, yes, there have been a number of pretty well known instances in the nuclear industry. Seismic curve determination, flawed evaluation -- flawed distribution evaluation for PTS. Doe, through the Yucca Mountain, they're using quite a bit of elicitation to address material and other issues. So it's a fairly well established tool, not only outside the industry but certainly within the industry.

MR. ABRAMSON: And I think the reason for that I think is, you know, it's fairly clear as to why. Because you would only want to use this when you have issues of really of great concern and also for which there is very little data and available information, and which there's no -- essentially no -- so in the nuclear industry, certainly, you're trying to estimate these very low risks, very low frequencies it's very important for regulatory purposes for things like this, for earthquakes, for PTS and so on to try to get some kind of answer. And also the NRC and the industry things has enough resources to be able to carry this through. Because, as you know, it takes quite a bit of time and effort to do this.

DR. WALLIS: They're not really measuring a frequency? They're giving you a state of knowledge, is what they're giving you?

MR. ABRAMSON: Well, of course. That's right.

DR. WALLIS: And of course as more experience develops, the state of knowledge will evolve. You shouldn't think that they're actually predicting something.

MR. ABRAMSON: No. I mean, as a statistician I like to kind of think of it as an estimate. It's an estimate.

DR. WALLIS: But there's no test which could tell you whether or not something like ten to the minus 8 estimate is correct. There's no way you could test that, it seems to me. You're going to test a huge number of large pipes.

MR. ABRAMSON: That's correct. And that's exactly why --

MR. TREGONING: Well, in this case that's exactly right.

MR. ABRAMSON: -- you do this. However, we never asked the experts what do you think the frequency of this LOCA is and in which case we never asked directly what this number is, because I think that would be a meaningless thing they would have no basis for it. That's why we took a great deal of effort, we and the panel of course, to break this down into these small pieces and to start with the base cases about which we do have some information. We both have data, and we have models and so on. And then to extrapolate from there in small pieces, so to speak, where you say what is the effect of, say, changing the degradation mechanism or what's the effect of this different material. So you try to break this down into this relatively small parts for which the experts have -- this is what they're expert in. They're expert in the physical phenomenon. And so you try to relate this, in this particular case the physical phenomenon.

And then, of course, we bring this all together. And that's why when you multiple and so on and so forth, and you extrapolate and of course starting from the low frequencies from the base cases, this is why you get these low numbers.

So you have to look both at the process and, of course, at the components of the numbers that we finally generate to see to what extent this is credible or not and to what frequency you're going to give to it.

DR. WALLIS: Well, I think what my colleague may have been getting at, though, is this all sounds very good but is there any measure of whether or not it really does it work? There's a sort of history of expert elicitation where they've been way off.

MR. ABRAMSON: Well, what we do have, and I use this in the training, we train them on so called almanac type questions; that is things that we know the answers to and they don't.

DR. WALLIS: Hey, you told us about that.

MR. ABRAMSON: That's right. And the idea there is that we demonstrate there, and I think this is useful that N heads are better than one. That even though each person might feel rather uncomfortable and has very wide uncertainty on a particular question, still the group opinion there is some wisdom and there's some knowledge, there is some information there and it kind of encompasses in a way the answer. So you do this kind of by analogy in that way. And this -- where I think people have done this -- you know, this is how you in a sense the validation of the process. Because ultimately, of course, you're only doing this for things that you have no data on and you never expect and never hope to have any data on, at least for these frequencies we're talking about.

MR. TREGONING: Okay. I guess I'll caution given where we are in the presentation. We're going to introduce now in my opinion entering in the interesting areas of the talk. So I'm concerned about length of time, but I'm certainly prepared to stay here as long as ACRS would like me to. But I'll say we've got a lot to get through.

CHAIRMAN SHACK: Just keep going.

MR. TREGONING: We want to answer all your questions, but we want to make sure that we give you some -- what we've covered so far is really just prior ground. Now we're really starting to enter the new ground from here on out because we're going to talk about results from here on out.

So I've got a number of slides, six or seven, which talk about, again, sort of general rational and insights. As I mentioned, this isn't exhaustive. This is just sort of a smattering of some of the information that we got that I've decided to share. I can't share all of it just in the interest of time.

And this first slide talks about sort of generic rationale and insights about LOCA frequencies. The first sort of insight, and I think this was shared by most if not all of the panels, service history precursor events which we just talked about, and by precursor we mean cracks and leaks, they are a good barometer of LOCA susceptibility.

DR. WALLIS: Right.

MR. TREGONING: Now you have to keep in mind these certain caveats that not all precursors result in the same LOCA challenge. But the fact that you have a preponderance of precursors in one system or one location due to a certain degradation is valuable information as far as the panelists were concerned. We tried to assess the potential challenges that you may have due to a given degradation mechanism or a specific, I'll say, system or location.

Just about all the panel members used service history in one way shape or form for anchoring their responses which, again, I think is a rational, reasonable way to go.

DR. SIEBER: It's probably the only choice one has.

MR. TREGONING: My opinion would be that I would agree with that, that that is clearly to answer difficult questions the easiest thing to do is to try to base it on the body of knowledge that we do have.

DR. SIEBER: Right.

MR. TREGONING: Service history of data -- and these are some of the reason why service history data was preferable. If they're degradation mechanisms and they show up, they're in the service history database. So you can postulate that a mechanism is important. If you never see it, then maybe it is or it isn't important. So I think that was something that people focused on; the fact that if a degradation mechanism is important, it will show up at some point in time. And you certainly have later blooming degradation mechanisms that may not show up until much further o in the service history. And one might argue that PWSCC could be one of those. And that's not to say that that's not a consideration. But just that service history is good at finding what our challenges have been in the past.

Again, with service history, the loading that the plant has seen and the mitigation implicitly considered. If your mitigation works, you don't see evidence of precursor leaks and cracks. IGSCC is a good one when you look at pre and post-mitigation data.

PFM approaches, many people said -- and we large disagreements between the PFM people in the group and the non-PFM people in the group. So I put this up at the risk of offending somebody, which I'm sure I will. But I think there was a general sense that PFM's great for identifying trends for well defined mechanisms. But coming with absolute numbers for LOCA frequencies is just a very difficult thing to do. And a number of people use service history and then PFM insights to determine what the effects of continuing operation time would be and the relative likelihood of a failure as a function of break size. So they found those particular attributes of PFM to be helpful in making projections from that baseline service history data.

DR. SIEBER: Do you have to group the whole service history record into small classes of events and then somehow sum those in relation to the probability of their occurrence?

MR. TREGONING: That's right. That's exactly right.

DR. FORD: On using service history as a barometer of the analysis, the failure history you see is really the beginning of a distribution which is moving forward with time. So what you're seeing is the first events of what could quite possibly be a big -- maybe a big subset. And generally those are, I was going to say bathtub effects, but that's not exactly what I meant. Things such as cold work. Much of the cold work effects on surface cold work, a piece of grinding, which are not covered I suspect in the PFM approaches. So how do you take into account the pragmatic aspects and the first of the distribution tree that you see coming up towards you are things like cold work effects, a piece of grinding at the surface, which are not taken into account into the analysis, as far as I know?

MR. TREGONING: Right. And, you know, I'd say Summer is a good example of that. A very atypical weld, weldable repairs that were -- some documented, some not. And so you had a set of conditions that led to the evidence of this cracking maybe sooner than they would have been expected otherwise.

DR. FORD: So are those physical phenomenon taken into account in your thinking?

MR. TREGONING: Again, I don't want to speak for each expert. With 12 different experts, I can honestly say that we had 12 different approaches to tackling this. But it's something that we certainly discussed and talked about in the meetings. And it's something that I will say that several of them explicitly mentioned that, yes, this was a consideration.

DR. FORD: Because that can alter your frequency by an order of magnitude.

MR. TREGONING: Of course. But we have some historical precedents as well. And, again, I'll go back to IGSCC and things like that where, you know, some of the earliest failures were again more atypical in nature. But then when you started to look, you really found out that you had a big problem. So I think, again, there is some historical evidence to fall back on that the members did try to make that ascertain. And that's one reason why PWSCC, which if you look at the service history, you would say this is not an important event. Okay. Really not -- we've only got a relatively small number of piping PWSCC events in our databases. It's like one or two at this point. Got a large number of CRDMs, but not piping.

So if you say that, you'd look at that and you'd say this isn't important. Well, the experts didn't say that at all. They said, no, this is important and here's why it's important.

Okay. These next two are sort of motherhood statements. I think they're probably obvious, but it's good when you look at calibrating the panelists that they came up with these assertions. There's certainly greater uncertainly in making estimates, and they all said this is the LOCA -- not only the LOCA size increases, but as it increases the relevancy of the precursor events becomes less. So the precursors may have more relevance to smaller LOCAs than they do for larger LOCAs.

And then this last bullet in is, I guess, to quote Lee, you know, prediction is always difficult especially of the future. Well, I think this is an obvious statement. But as we go out in time there's certainly more uncertainty association with assessing

LOCA frequencies. Again, especially out at the end of the license extension period. So these last two bullets are maybe obvious but I think, nonetheless, they're important to state.

DR. RANSOM: One thing on the license extension, I thought the philosophy was that the plants were held to their initial licensing base and through aging management programs that there would be no increase in likelihood of accidents. This seems to be in contradiction to --

MR. TREGONING: No, no, no. There's no trend in here. I just said estimating is more uncertain into the future. Because you're trying to project further out. If I jumped to our results, we asked them to estimate over three different time periods. There wasn't huge differences --

DR. RANSOM: What are those time periods? You mean in the future?

MR. TREGONING: The 25, which is current day and 40 years and 60.

DR. RANSOM: Right. That's assuming the same component has been in use all that time or --

MR. TREGONING: Not always. I mean for some things like steam generators, and when we talked about steam generator tube ruptures, a number of the panelists took into account the idea and the belief that many, if not all of the licensees are going to move toward replacement. So, in fact, one of the things that they did is they said well I think a lot of these are going to replace by 40 years, which would cause a decrease possibly in steam generator tube ruptures, but these new ones will probably -- there'll be an increased frequency out at 60 years because they will start to age. And even though we'll be using improved materials and hopefully better management of secondary and primary site chemistry, I think we've proven historically with steam generator tubes is they've been fairly frequency -- you know the failure frequency has been relatively high even though we've made various improvements.

DR. SIEBER: Well, I think it's also fair to say that when you think about license extension, we are not assuming that the risk stays constant throughout 60 years of plant life.

MR. TREGONING: Right.

DR. SIEBER: And, in fact, you don't know how the risk changes necessarily because PRAs do not contain aging mechanisms unless they're explicitly put in there.

MR. TREGONING: Right.

DR. SIEBER: And you rely on the ongoing set of rules and the inspection programs to assure that the plant remains acceptably safe for operation for the next day or the next week or the next month.

There may be a plant out there that has enough degradation in enough areas that they will decide not to run the full 60 years and say it's just economic to do the kinds of replacements that we have to do with the short remaining lifetime.

MR. TREGONING: But just again, let me restate this point and make sure it's obvious. We had panelists that said the frequencies would go up somewhat. We had panelists that said they would go down somewhat. We had panelists that said they would stay the same. But across the board for the most part they said my uncertainty about this trend increases with time.

DR. SIEBER: Yes.

MR. TREGONING: That's all I'm trying to make here. It's not necessarily the trend's preordained to go one way or the other. But what they all felt was preordained was that their uncertainty in that trend was certainly going to go up. And I think to me that's just common sense at this point. And if they didn't say that, I would sort of raise my eyebrows at each of the panelists. Because, again, you're not considering all the possible things that could happen over that time period.

DR. WALLIS: It could actually go down.

MR. TREGONING: Of course.

DR. WALLIS: All we need is a few LOCAs and then your uncertainty goes down.

DR. SIEBER: Or you need a lot of confidence in piping.

MR. TREGONING: Yes. We're not anticipating and we're certainly not planning to have that -- have that sort of -- those sort of events happen.

DR. RANSOM: Well, typically, too, there's a learning curve involved in any system. And so you might expect that these things might actually decrease with time. But then as they age and another mechanism might come in and cause them to increase.

MR. TREGONING: That's right. That's right. And, again, those were factors that people had to weigh when they were looking at predicting effects in the future.

Okay. All I've done now with these next couple of slides is I've got one slide that gives a few insights on BWR plants, one slide that gives some insights on PWRs plants, one that gives some insights on piping in general and one that gives some insights on nonpiping. And then I've got a slide that goes at future trends qualitatively to get to Dr. Ransom's points here.

So for BWRs these were, again, I'm just sort of listing some of the degradation mechanisms that the people though were most important. And there were really four or five.

Thermal fatigue certainly for Bs was -- and these are in no particular order of severity. These are just some things that came up time and time again.

Thermal fatigue was important especially for the Bs. You have a larger temperature fluctuations due to operating performance than you do with the Ps.

IGSCC even with all the mitigation that we've done, that was still a paramount concern with the panel. And many said that even though we've done things like weld overlay and things like that, and while we've shown that it reduces the cracking rate and improves the margin on the piping, it does some things like potentially increase the residual stresses of the cracks that are there that may actually have a deleterious effect.

So IGSCC even thought they all agreed that we had done a lot of good mitigation work, this was still a concern and still a challenge for them in terms of mitigation.

Mechanical fatigue or vibration fatigue at the small diameter lines was sort of unanimous here. Here's an area that where we do have probably more actual data to base failure frequencies on. And not only small diameter lines, but the socket-welded lines were certainly a concern. We certainly had a number of historical problems associated with those lines.

Flow accelerated corrosion. This was not something that was shared unanimously by the panel. Certainly the industry has many good inspection programs in place today and a lot of the panelists said that these programs were sufficient to really reduce or eliminate the FAC concerns. But we had some conflicting opinions that said, hey, we're doing so good at controlling SCC through hydrogenated water chemistry but you have to be careful because as you reduce the oxygen that can potentially accelerate FAC. And, oh, by the way it could accelerate it in areas that you hadn't expected to see it before. So not necessarily in just flow transition regions or regions where you get turbulence.

DR. FORD: That's not strictly true currently. Hydrogen water chemistry may be, if it's not well controlled, but it's certainly will improve the FAC resistance.

MR. TREGONING: Yes. And this is specifically hydrogen water.

DR. FORD: I'm just putting a plug in there.

MR. TREGONING: I know. And I put a lot of this stuff up at the risk of being shouted down. I realize that. But -- and again, I don't want to imply that any of these points that have any sort of panelist consensus. There's no consensus among the panel.

All I'm doing is highlighting some of the more interesting things that came up.

The other things with Bs that people talked about is -- and we talked about the consideration of transients. Well, with Bs especially water hammer and things like that you do have compared to the Ps an increased number of operating transients. And most of the panelists certainly considered this fact in their analysis.

This was an interesting point that many

people raised, and I guess I hadn't expected it, but there was this notion that, you know, the BWR community because of what they had to go through with IGSCC, they've got a lot more experience identifying and mitigating for degradation than does the PWR community. And for that reason they said, you know, if new mechanisms were to emerge, we would expect the BWR community sort of in general to be further up the learning curve so to speak in both identifying, inspecting and finding those challenges. And of course, several of them also reflected the opinion that, however, PWSCC may also get the PWRs up on that same learning curve as well. So this was an interesting point that several people expressed.

And we talked about this point, the fact that even though service history is important, you really have to carefully evaluate it because -- and IGSCC is a great example. You have mitigation and post-mitigation data. And really if you look at any degradation mechanism, you have to look at the service history in context with whatever mitigation or operating procedures were in place at the time that that data was developed. So I think that's what makes even just evaluating the service data particularly challenging for this.

For PWRS. Again, I wanted to list some of the important degradation mechanisms. Certainly we've talked about PWSCC and mainly concerns there in the Inconel welds and then the alloy 600 base materials like we have in CRDMs.

Certainly there's a realization that there's a strong material temperature dependence that's been exhibited or certainly on a laboratory scale with PWSCC, although there was certainly a realization from many of the panelists that we've seen evidence and service of what appears to be PWSCC in regimes that we wouldn't have expected it initially, i.e., lower temperature. Some of the lower head cracking like at South Texas was obviously a bit of a surprise.

DR. SIEBER: But that's a different mechanism.

MR. TREGONING: I don't know that -- I'm not going to comment on that, but if somebody else would like to.

DR. SIEBER: All right.

MR. TREGONING: It's not my understanding that it necessarily was.

DR. SIEBER: Well, it had elements that it was a unique situation.

MR. TREGONING: There's certainly realization of this strong dependence, but also a concern that just because we've demonstrated and it doesn't necessarily mean that those are the only considerations, so that lower temperature couldn't evolve PWSCC problems, and that you couldn't see things in lower temperature heads or cold legs versus hot legs. You still could. They could be delayed, but you could see them at some point in time.

DR. SIEBER: On the other hand, the hottest place in the plant is pressurizer heater sleeves. You know, the hottest surface temperature. And, of course, there's failures in the Inconel 600 sleeves. On the other hand, it is not at a rate that's anymore alarming than CRDM welds that are probably operating 30 to 40 degrees lower temperature.

MR. TREGONING: Right. So, I think just the understanding that while temperature's important, it's not paramount.

DR. SIEBER: Right.

MR. TREGONING: There are things like stress history, stress state, fabrication that comes into play --

DR. SIEBER: Right.

MR. TREGONING: -- when determining if

you're going to have potential cracking events.

DR. SIEBER: Yes, it's just one factor.

MR. TREGONING: It's just one factor.

Again, thermal fatigue is important for PWSCCs and mechanical fatigue as well. So those were some -- and again, we talked about a number of issues here and certainly boric acid corrosion and things like that were degradation mechanisms that we talked about and assessed. But the ones I've listed are the ones that came out time and time again that the panelists said these are our biggest challenges for the most part.

And, again, I said earlier as far as PWR plants, PWSCC would really -- I think that was a paramount concern for the panel just because the fact that many of them felt that we were on the precipice, maybe, of seeing many more PWSCC events. And that's why many of them said hey, near term frequency increases due to PWSCC are potentially likely because as we learn more about this and we learn really the extent of the PWSCC in the plant, you know as we learn more that might cause those frequencies to increase somewhat.

Now, most of the panelists expected that we would have mitigation techniques that would be developed and employed and that would be successful. However, several of them said it may take ten to 15 years before --

DR. SIEBER: To get there.

MR. TREGONING: To really get there in a comprehensive fashion. And, again, I think a lot of them looked back on the experience we had with IGSCC for a model there.

DR. FORD: Would you not say that 15 years, saying you're going to wait for 15 years essentially before -- well, I was about to say irresponsible, but I don't mean it quite that way.

You know, if you look at --

MR. TREGONING: Read what I say. I said they would be successful resolved within the next --

DR. FORD: Yes, but if you could have a whole lot of really bad things happening within that 15 years.

MR. TREGONING: Of course.

DR. FORD: And therefore you not be using your analysis to address not when the median of that particular problem is going to occur and give rise to such-and-such of LOCA, but the first one use extreme value statistics that come up with when are you going to have the first bad LOCA event? And that gives you time frame, and you'd better darn well come up with a resolution to this problem.

MR. TREGONING: Yes. I guess I would say, you know, use sort of extreme value statistics. I mean, there has to be a framework to do that.

MR. ABRAMSON: You'll need some statistics to have extreme value statistics.

DR. FORD: Well, I know. But you pointed out all we need is one really bad event to occur and we're in deep trouble. And should we not be using the expert panel to come up with some sort of judgment for when the first one.

MR. TREGONING: Well, that's what they did here.

DR. FORD: Okay. You've been talking about the mean --

MR. ABRAMSON: No, if you look at the frequency, the frequency, the expected return time is a reciprocal of that, you know. I mean, that's the frequency. That's the number of years to wait.

I think the numbers are still going to be very, very small.

DR. FORD: Which is --

MR. ABRAMSON: I don't believe you don't expect this to happen within the lifetime of the plant at all. But then we might consider if you have a 100 -- well, you've raised another issue. You have 100 plants --

DR. FORD: I'm sure at Davis-Besse, you're awfully close to getting to 100 gallons per minute --

MR. ABRAMSON: All right. Well, to put this into perspective, this is all of course per calendar year.

CHAIRMAN SHACK: And I don't think that this particular one is probably the hot leg cracking. The reactor head problem is going to be resolved, I think, considerably sooner than 15 years.

DR. SIEBER: As fast as they can make heads.

MR. TREGONING: The key phrase is within the next 15 years. It's not that it's going to take 15 years.

DR. FORD: I guess I'm just responding to the ten years that we saw this morning and now the 15 years we've seen now. I think we've been far too complacent about the time frame in which you got to resolve these problems.

MR. TREGONING: And, again, I don't mean that -- I don't mean this in any implication that we as an industry are going to be cavalier about addressing these issues. This was just the expected opinion that when the panelists looked at their frequencies as a function of time, they expected resolution of this issue to not so that the frequencies would decrease again out by about 15 years. And that doesn't mean that we don't -- that's due to this particular issue.

Of course, in the interim we've got to maintain due vigilance to assess and address these things aggressively.

DR. FORD: If you're talking about, you know, one event is going to be very bad, a 100 gallons per minute, aren't you talking about a 99 percent confidence limit on these evaluations? I'm not a statistician.

MR. ABRAMSON: I don't know. You'll have to look at the numbers and see what that implies for the 100 plants or as many has --

DR. FORD: Right. All we need is one.

MR. ABRAMSON: That's right. And you could certainly make that calculation. That's right. Yes.

CHAIRMAN SHACK: Okay. I think this is probably a time for a break. We're about due. And this is kind of natural place to do it.

So let's take a 15 minute break.

(Whereupon, at 3:09 p.m. a recess until 3:27 p.m.)

CHAIRMAN SHACK: Back into session.

MR. TREGONING: Okay. Thank you, Mr. Chairman.

The next thing that we want to look at with general rationale and insights just relate to piping contributions to LOCA alone. And, again, these are just a smattering of opinions and sort of interesting insights that we've got within the whole laundry list of rationale and insights.

I think many people, when you had to access the piping contributions for a LOCA category, each LOCA category was associated with a certain flow rate. And certain pipe failures could lead to flows of that size or not. Pipe that were too small, obviously, couldn't lead to a 500,000 gpm flow.

So one of the things you had to do is for a given degradation mechanism make the assessment well is complete failure of the smallest pipe more likely or is a partial failure of the larger pipe more likely for a given degradation mechanism. And I can say in general what most of the panelists came back with when they did their assessments is they thought, you know, for a given LOCA category what's more likely is a complete failure of a small pipe versus a partial failure of a larger pipe due to some degradation mechanism.

So when you see the results, and you see the important systems for a given LOCA category, they tend to be systems that are the systems with the smallest pipes that could lead to that LOCA. Tend to be, not in all cases but tend to be. And then there's another set of I'll say variables associated with those systems or component which make them potentially sensitive to LOCAs, i.e., the environment could be -- the loading could be such that they see large thermal transients. There could be areas where they see water hammer. It could be areas where, let's say, the environment's relatively stagnate so they were worried about effects of, I'll say, environmental cracking. It could be areas that had alloy 82, 182 welds that would be associated with PWSCC. So there'd be other reasons that would make them be important or not. But in general if you look at a LOCA category, it'll be some system where the smallest pipe is sort of the smallest pipe that could lead to that LOCA tends to be the one that's most important.

I think one of the things that we saw was that aging, most people predicted that aging would have the biggest effect on the intermediate size piping.

What do I mean by intermediate size? Well, piping that's about 6 to 14 inches.

Why is that? The smallest piping, say less than 6 inches but potentially or specifically 4 inches or less, we've got a lot of data and a lot of history applicable to those pipes more than we do so the large pipes. Those are where we've actually had failures, those are where we've seen a lot of precursors. Those are the pipes that we've tended to have mechanical fatigue problems. Those include things like the steam generator tubes. Of course --

DR. SIEBER: Vents and drains.

MR. TREGONING: Vents and drains, things like that.

Most of the panelists felt like the surface history failure rates that we had were most applicable for those. And because of that, that service history failure makes those pipes less susceptible to aging because we already have a body of failure knowledge and they didn't necessarily expect those to be effected by aging any worse than they had been in the past.

The largest piping, those are more robust of two reasons. One we have higher quality inspections in general. And more importantly, we have primarily increased leak before break margins. So for any given mechanism the likelihood of having a leak before a break is higher for the largest piping than it is for a smaller piping.

So, I think if you look at sort of deviations and if you look at panel variability, what you see in the results is for the smaller piping and the smaller LOCAs there's much less panel variability. And you go to the biggest LOCAs, and the biggest pipes there's actually much less panel variability and even sometimes more on the less uncertainty. But when you go to the intermediate pipes, that's when you have a lot more panel variability where the panelists expected much more effects of aging and where the uncertainty even goes up because of all these potential variables which are more important for those types of pipes.

I think information like this is important, even as qualitative information. When we set and define an alternative break size, you know, even this sort of qualitative insight is valuable because it gives you a threshold to say okay, you know, maybe 6 to 14 inches, maybe I don't want to go much below 14 inches just because of this fact alone. I mean, I don't want to say that that's where we're we'll go with this, but it's part of the knowledge and part of the information that you want to use when you make these decisions, other than just the raw numbers.

DR. LEITCH: It surprises me that it's not mentioned there about the number of welds in those various piping systems. I would think the smaller piping had a lot more welds than intermediate, kind of an intermediate number and the large would have a fairly small number of welds.

MR. TREGONING: Again, I didn't put everything up here. But when people made their assessment they looked at what we called them were risk relevant locations. Welds are certainly an important consideration, as are things like elbows and Ts and things like that which, in the elbows may be cast -- for a lot of people.

So certainly systems with higher or more sensitive locations tend to elevate their failure frequency.

What a lot of people, though, really focused on is that the number of welds is important. But for any given system you may only have a small handful of welds sort of weakest link philosophy that are really driving the risk. So that minimizes some of the contributions from systems that have a lot of welds and some that don't.

But some panelists, they did do essentially a weld census approach that they tied to estimate for each system what a per weld failure frequency would be. And then they essentially just multiplied by the number of welds for that system, and that's how they came up with their total numbers. So they just made the assumption that, hey, I'm going to assume all my welds are with this risk significance even though I know that's not true.

DR. LEITCH: Yes. Some of them did take that approach.

MR. TREGONING: Some of them did. In fact, quite a few did. And, again, I didn't put up everything.

DR. LEITCH: Understand that.

MR. TREGONING: But that was certainly a consideration.

DR. WALLIS: Now, there's less focus on large break LOCA, presumably there'll be less focus on inspecting large pipes?

MR. TREGONING: There could be. And

that's something that when we design programs we have to make sure that we keep that in mind. I think there were a number of opinions expressed by various panelists that, you know, just because the largest piping is more robust, it doesn't mean that you don't do inspections on it. You know, you continue to validate and verify that -- and why not? Because it potentially has high consequences of failure.

So we didn't really get into the question of risk reward type of analysis. And I think many of the panelists had the implicit assumption and even cautioned us to the fact that, yes, but you still need to keep inspecting large pipes. It's not that you don't inspect large pipes.

And this really led into the next topic of risk-informed ISI. And we talked a lot about that. Of course, that's an area that the industry as a whole is moving into more uniformly and more comprehensibly. I think the general expectation from the panelists were is this is generally good thing. That inspecting the risk-informed areas, that philosophy is very sound. It's a very rational thing to do. However, you have to be concerned and you have to be concerned certainly about the consequences. And I'd just point a couple of concerns.

You know, our inspection locations that we picked, they're largely based on experience. And we have to be particularly vigilant if we see precursors in other areas that we assess those, address them and if necessary, modify our inspection programs accordingly very aggressively to account for that.

So, the concerns that people had were not so much about risk-informed ISI, but the fact that we would be continually maintaining due diligence and updating these things, making them sort of like -- we talk about living PRAs, but almost like living risk-informed ISIs where you're continually assessing your challenges as they may occur and you're updating those as necessary. And that's why with risk-informed ISI there is a certain percentage still of low risk significant yet high consequence systems that have to be inspected.

DR. SIEBER: Well, section 11 is based partly on the consequence situation and your ability to protect against certain kinds of defects. And section 11 does not call anyplace where you would inspect a straight length of piping. It always looks at welds, elbows, fixtures and fittings.

MR. TREGONING: Right. Right.

DR. SIEBER: And so that's where the

likelihood of failure is. And size and consequence are related.

MR. TREGONING: Right. And I think the notion here is just that if for some unforeseen reason straight piping started to become an issue for whatever reason, that --

DR. SIEBER: You'd change the code.

MR. TREGONING: -- we would not only change the code, but change the way that we were dealing with addressing risk relevant issues. So that was sort of a caveat on I'll say the general optimism or general advantageous features of risk-informed ISI.

Okay. Nonpiping. This was much more interesting in a way, just because of the variety and the disparity of the possible failure modes are so huge. I think across the board, and again this is one of those things that if they didn't say this, I would have questioned their appropriateness for the panel, but they indicated that the estimation of these nonpiping contributions was clearly more challenging. You have widely varying operating requirements among the nonpiping components. The design margins vary dramatically from things as small as steam generator tubes up to steam generator shells. You know, you have potentially a wide array of design margins. The materials and the inspectability also vary dramatically.

The failure modes and scales are also very disparate. Again, you have steam generator, small penetration failures. You have to worry about common cause bolting failures, which we've talked about. And even things like component casing failures.

Another issue that all the panelists recognized is that we tend not to have the same wealth of precursor information for the nonpiping components that we do for piping. There have been a lot of efforts that have gone into addressing and accumulating piping degradation precursor events. There's been much less focus, I would say in general, on nonpiping precursors. And part of that's related to the inspection compliance as well. A lot of the nonpiping stuff just doesn't -- it's not subject to the same inspection quality or quantity.

DR. SIEBER: Did this study include gasket material --

MR. TREGONING: No.

DR. SIEBER: -- like the flex --

MR. TREGONING: No, we didn't consider gaskets at all. Gaskets or seals. Mainly again metallic, passive system component failure.

DR. SIEBER: And the leaks usually aren't very big.

MR. TREGONING: Usually not. I mean, we would classify those as sort of active system LOCAs, even though a seal is not maybe active. But it's something that you do maintenance and testing on to try to see if you're having degradation. So it's certainly an area that you have to be concerned about with generating LOCAs, but it was outside the scope of this.

Again, you have larger components which I've mentioned. The larger components can have a bigger design margin compared to piping, but that's sort of degraded by the fact that the inspection quality and quantity could be decreased compared to piping.

The smaller components, again --

DR. WALLIS: What about manufacturer? I mean, we always seem to assume that big pipes are made to the same standards as small pipes and welds on big things are just as good as welds on small things. It probably is okay, but --

MR. TREGONING: Sometimes you get --

DR. WALLIS: -- making something so big that you can't make it very well.

MR. TREGONING: I mean there are -- and certainly this is one of the things with the vessels. I mean, thick section materials tend not -- and I'm talking base materials, you tend not to have as good-- the properties tend not to be as good as you would with a small section component. Because you can do a lot more hot work for the most part. But in some areas, like welds, you might actually get a benefit with bigger components. Because even in small pipe, you only got one or two weld, if that one weld is bad you're in trouble for that small pipe. But a big pipe, you happen to have one bad weld, then that's okay. You've still got plenty of other margin with respect to --

DR. WALLIS: But you can cover them all up with a surface weld. So you have to inspect inside.

MR. TREGONING: Yes, you have to do subsurface inspection, no doubt. No doubt. And that's another challenge with the larger pipes. Is the larger the inspection regime, the quality and the resolution that you're inspection technique can have will go down because you're trying to penetrate more material.

With the small components, and by those I mean the CRDM nozzles and the steam generator tubes, people thought that these are where improved inspection and mitigation program in the future can really have a benefit and really act to reduce the future failure frequency. So the expectation was these programs will likely benefit the smaller component failures more than they would the larger component failures with respect to nonpiping.

I've got a slide up on future trends, and again these are sort of qualitative insights that we got from the panel. As with many of these things, there's a number of compensating factors in the future. And I don't list them all, but just a few. So they're both advantageous and detrimental factors.

Some of the advantageous factors. Well, as we go into the future we'll certainly have more operating experience, and that operating experience will hopefully transmit into knowledge. So by more operating experience we'll also have more knowledge.

There's the hope and the expectation based on the past that we'll also continue to improve inspection and mitigation procedures in the future. And so an expectation that as plants continue to age, that people would continue to do material replacement and repair to address degradation mechanisms that they find in service.

So these three factors many people thought were advantageous to minimizing the effect of the LOCAs in the future; to either keep them constant or actually decrease the LOCA frequencies in the future.

What are some detrimental factors or challenges? Well, you always worry about the unknown. So what happens when we get aging in either new or unexpected locations and new degradation mechanisms pop up? That's certainly a challenge.

We heard today that every seven years we have a new degradation mechanism. So, you know, if that's true and that continues on into the future, then every time we have a new challenge it's incumbent upon us to try to meet that challenge while minimizing these LOCA frequencies.

Possible detrimental factors or changes in the operating profile. If temperature range, if pressure transients increase, if pressure increase; if there are things which change the operating environment, those could potentially be detrimental toward the future. And it's something that we have to be, again, particularly vigilant about.

And finally, this was an interesting one I thought, but that a couple of people expressed. They said one of the concerns they have with the nuclear industry and that many other industries as they're designing new components and building new components, they get design benefits because of research that's gone into the new designs and fabrication that they can turn around and apply to their current fleet potentially. And there was just a concern that, you know, we haven't had this continual refreshment of ideas and technology in the nuclear community. And that's not to say that we won't in the future. But just a concern that that's not helping us in the material realm or the degradation realm as much as it could.

So given these, I'll say competing factors, what most of the panel said in large was that these things tend to be compensating in a way. And that if I looked at all the 12 responses, most of the 12 expected the future LOCA frequencies to be pretty similar to the current frequencies. And why is that? Well, future problems they didn't expect to be significantly different from what we have seen historically. They expect degradation will continue to surface through precursor events and that we as an industry will take appropriate measure to mitigate the effect of those precursor events in a timely manner.

And, again, what they supported their prognoses of the future on was, again, not just past response to degradation, but what we're doing currently to combat some of the degradation issues that we're dealing with now.

DR. FORD: That it rather a sad reflection of our industry. I don't think it's correct for the PWRs, the boilers.

MR. TREGONING: That future problems --

DR. FORD: I think that things will improve. But that's just my personal opinion.

MR. TREGONING: And, again, I don't want to say that everyone had this opinion.

DR. LEITCH: I didn't quite hear you.

DR. FORD: I said that things will improve as far as the boiling water reactors. I think there are improvements being made. But that's purely my personal opinion.

MR. TREGONING: Well, again, and people recognize the improvements that have been made historically in boilers and they focus that into their estimates of the current day frequencies.

DR. FORD: Right.

MR. TREGONING: I think that the expectation was based on today they didn't expect them to necessarily improve that dramatically in the future. It doesn't mean it won't happen. That was just what sort of the typical opinion was.

DR. LEITCH: The changes in the operating profile, that could be positive or negative.

MR. TREGONING: It could be.

DR. LEITCH: It's listed there as a negative, I assume because they're thinking in terms of power upgrades. But many plants have many fewer cycles than was the case 20 years ago. It's a make it or break it.

MR. TREGONING: Right. And that's a good instance of compensating factors; the fact that we have outage times or the length between outage times in some ways is a very good thing because we reduce the heat up and cool down transients that the plants see. That's a good thing.

Now the bad part of that is you can't inspect as frequently. So there were a lot of these things that the issues had compensating factors that in many peoples' mind tended to neutralize them in some sense.

This qualitative information is really born out in the numbers as well. So when I start showing you results in here, they're only results for the current day estimates. I'm not going to show you, except for maybe one incidence, any future predictions. And I'm really basing that on these qualitative reasons that the panel just gave. And again, it's born out in their numbers. So, what we are going to see from this point forward is current day or 25 year estimates.

So the first thing I'm going to show - and we're going to start globally and then was as we have time, and people are interested, I'm going to deconvolve these frequencies into pieces. So I'm going to start out with the total frequencies for BWR and PWR plants and then again, I'll say partition them in many ways to try to get more insight into the panelists' responses.

What I've shown here is just simply the frequency plotted as a function of the LOCA category. So again, each higher number LOCA category as higher number break size or flow rate. And these are cumulative categories so that category 1 encompasses category 6. So these curves will naturally go down. What this shows - there's four curves on here. The BWRs in red, the PWRs in blue. The solid lines are the mean results and the dashed lines are the 95th percentile estimates. These are based on the median of the panelists' responses for each of these. Okay. So there's nothing in here that shows panel variability at this point. It's on the median panelist's response.

The BWR, you see more gradual decreases with LOCA category and you see the slope there is a little bit flatter than it is with PWR. Well why is that? It's primarily driven by the IGSCC concerns. And IGSCC was the one case, as we'll see later, that sort of violated the axiom that smallest pipes tend to have the biggest challenges. With IGSCC, the re-circ system showed up the biggest 28 inch re-circ showed up in all LOCA categories as being relatively important. So that's why the BWR decrease is much more gradual. Because you have contributions from these big piping systems due to IGSCC that are happening in all the LOCA categories.

Generally the BWR's -- they're actually a little bit lower for category 1 LOCAs and they're pretty consistent, though, for categories 2, 3 and 4. They're higher for category 5 and then they decrease greatly for category 6. Now why is that? Well, again, they're higher for category 5 primarily due to the remaining concerns with IGSCC. They decrease rapidly for category 6 because of the PWR pressures and piping sizes. You don't really expect any category 6 LOCAs out of piping. So really what's driving the PWR category 6 failure are failures of the big things like the vessel. In fact, primarily the vessel because even pumps and valves you're not going to get that flow rate size out of a pump and valve even splitting apart at that point. So that's why you see a big decrease with the Bs between five and six.

The Ps are highest for category 1 and highest for category 6. Well, why are they high for 1? Well, that's really driven by steam generator tube rupture, as you might expect. And for the higher LOCA categories, the nonpiping contributions tend to be important just because there are so many possible contributions that you get from Ps. You could have vessel failures, pressurizer failures or steam generator failures. And that's help buttress the category 6 results somewhat or increase them somewhat.

Again, there's a lot of similarities in the frequencies between those middle LOCA categories.

And the other thing that's similar is sort of the ratio between the means and the 95th percentile. And that doesn't even vary dramatically as a function of LOCA size. You get fairly consistent ratios there, which was a little bit surprising to me.

I would have expected, if anything, that we would have seen the uncertainty possibly increase with LOCA size. But we don't, at least in the median responses we don't really see that.

DR. RANSOM: They seem pretty high, though. Because if you look at the category 1 LOCA for PWR, which is about ten to the minus 2, that would mean that we would have one LOCA per year. We should have had 30 or 40 LOCAs. Have we really had that many?

MR. TREGONING: Again, look at the 9th percentile verses the mean. The mean response is pretty consistent with sort of -- if we went back and looked at the number of steam generator tube failures greater than 100 gpm that we've had since '87, and there's been four of them since '87 in about 1200 reactor operating years, that works out to be about 3.5 times ten to the minus 3. And it ended up being pretty close to where that mean estimate ended up being.

So there actually is a pretty high evidence of steam generator tube type failures.

DR. RANSOM: Have there ever been any other tube failures that have led to a LOCA by your definition?

MR. TREGONING: By other tube failures, piping failures.

DR. RANSOM: Piping failures, right.

MR. TREGONING: Not that -- there have been some small pipe failures that we've had some fairly large leaks. But I'd say a relatively small handful.

DR. RANSOM: Bit enough to be a category 1?

MR. TREGONING: I want to -- not to my knowledge, but we've had several that have been approaching that potentially. And I don't want to say several. We've had a handful that have approached that.

I'm going to divulge this in a minute. If you look at what's driving this for PWRs it's purely steam generator tube failures. So, you know, what tends to happen is the biggest contributor, and in this case it ends up being steam generator, that dominates. And that's clearly the case here with PWRs.

DR. RANSOM: Is there any significance to the fact that the 95 percentile looks like it's about four times the mean?

MR. TREGONING: I guess -- let me make sure I sure --

DR. RANSOM: That's a general characteristic of lognormal distributions. I wonder if the experts knew that.

MR. TREGONING: No. I mean, lognormal distribution can have different error factors associated with it.

DR. RANSOM: The 95 percentile seems about four times the mean.

MR. TREGONING: It doesn't have to be. It can be any error factor.

CHAIRMAN SHACK: You just use a standard error factor.

MR. TREGONING: What many people assumed in earlier LOCA estimates was that -- and this is the way they were done in 57.50 and also I think 1400, but they calculate the mean responses. And then what they did is they said all right, for all of these I'm going to assume that they're lognormally distributed and I'm going to apply an error factor that's relatively small, an error factors that's 3 on the small LOCAs, but then I'm going to use a relatively large error factor of ten on the larger LOCA.

So, no, you can certainly have more spread in your lognormal distributions. So the fact that this ended up being -- and I don't even know what the average is. I haven't calculated. But I'll believe you when you say it's four. That's not a function of the fact that we assumed lognormal distribution.

DR. LEITCH: Did I understand you to say in the BWR, the reason that drops off so fast in the category 6 LOCA is that you really can't have a piping failure that leads to a category 6 LOCA? It's got to be a nonpiping failure?

MR. TREGONING: I'm sorry. Could you repeat that. There were two conversations playing out.

DR. LEITCH: I'm sorry.

MR. TREGONING: I'm sorry. Go ahead.

DR. LEITCH: Did you finish your thought? I didn't mean to -- did I understand you to say that in the BWR you cannot have a category 6 LOCA due to a piping failure, that it must be something nonpiping?

MR. TREGONING: Primarily, yes.

DR. LEITCH: So a double ended guillotine break of the resert plate does not give you a category 6 LOCA?

DR. WALLIS: It would be a 5.

MR. TREGONING: We didn't couch things in terms of whether the break as single or double. We couched things in terms of effective whole sizes or effective diameters.

So you could get a double ended -- and I'm not a thermal -- you know, Dr. Wallis would be better able to answer this. I don't have to hazard a guess whether a double ended guillotine break you could break flows that large. But it would take a double ended -- you couldn't for a single hole of that equivalent break size, you couldn't get it in BWR piping.

What you would get for a double ended break is unknown to me.

DR. SIEBER: Double ended breaks are --

MR. TREGONING: If you make that assumption, then no you can't get up to a category 6.

DR. SIEBER: Doubled ended breaks are idealistic and hardly never occur anyplace.

DR. LEITCH: But what I'm saying is that's what 50.46 requires at the moment.

MR. TREGONING: Requires you to do.

DR. SIEBER: Yes.

MR. TREGONING: If you look at our correlations you get up to about -- a little bit over that in the BWRs with the resert pipe. So, you know, double that and not with that simple correlation that we used, you don't get up --

DR. SIEBER: And they're not --

MR. TREGONING: Okay. Now all I'm showing here is just the mean results for BWR and PWR plant. And all I've done is partition into the piping and the nonpiping contributions. And these piping contributions are blue. And the nonpiping contributions are red. You see Bs on the left and Ps on the right.

BWRs you get the nonpiping and the piping are pretty similar for categories 1 and 2. Again, and what's driving the nonpiping response is up for those categories and Bs are stuff -- certainly a concern. And, in fact, that was really it. Was probably the biggest concern for 1 and 2s that really elevated them.

But then when you get to the larger LOCAs and you move away from potential stub tube failures, it's the piping that tends to dominate for the Bs. And then the piping runs out at category 5 and you're only left with these -- essentially vessel failures that you could get up to that category in the Bs.

With PWRs you've got a bit of a -- you've got a different trend. You have the nonpiping which clearly dominates for categories 1 and 2. Category 1 are driven by steam generator tube failures where category 2 is governed by primarily CRDM failure concerns, but then you also have pressurizer heater sleeves that also factored in to both 1 and a little LOCA category 2. So these smaller LOCA sizes were really -- the nonpiping for the Ps were far and away the most important consideration.

Beyond category 2 you have roughly equivalent contributions as a function of LOCA size. And several experts just expressed a sort of rule of thumb opinion that, hey, I would expect for large LOCAs to get about the same contribution for large LOCAs for piping as I would for non. And then when the results came out that way, a number of people felt like their gut was satisfied in some sense.

DR. KRESS: Why is the PWR acting in categories 1 and 2, smaller than the BWR in the same category?

MR. TREGONING: Again, primarily the BWRs, and again it's -- they're pretty close for category 1. And, again, these things are plotted on a --

DR. KRESS: But they should be the same.

MR. TREGONING: Yes, BWRs a little bit higher because, again, I'd say remaining IGSCC concerns. A little bit. Category 1 they're very close and it's on a log scale. I mean PWRs about 1E4 for piping and you're looking at about two to three E4 for BWRs. Within this game, that's essentially almost identical.

CHAIRMAN SHACK: There might even be more small diameter piping in a BWR, too, which would --

DR. KRESS: That I wondered about. There might be.

DR. SIEBER: That's hard to say.

MR. TREGONING: Yes. We separated the instrument and drain line piping from the rest of the system. So we treated those as separate systems. And what you saw for both the Bs and Ps, that's what dominated the category 1s and then the category 2s is that small instrument in drain line where you not only got failures due to degradation mechanisms, but you get failures due to things like human error. And much more likely to get human error type failures. Driving into it with a fork lift, you know.

DR. LEITCH: The Bs you have all that -- in certain withdrawal piping, too.

MR. TREGONING: Yes.

DR. LEITCH: I mean, there's a couple hundred of those.

MR. TREGONING: Yes. Any of the inserts for the most part were treated as nonpiping. Any of the big vessel penetrations or input. We lumped with nonpiping.

DR. LEITCH: Nonpiping. Yes.

MR. TREGONING: That's why the CRDMs for the Ps end up being so big. That included CRDMs, but ICIs and things like that.

So we tended to follow the ASME definition between what was pipe and what was not a pipe, although some people said, hey, you know it receives pressure, it looks like a pipe, it was fabricated like a pipe, it's a pipe. But, you know, we didn't--

DR. KRESS: If it looks like a duck and walks like a duck.

DR. WALLIS: So if I gave you $2 and if there were a LOCA in the category 6 in the pipe in the PWR, you'd give a billion?

MR. TREGONING: I want to make sure I understand the question before I make my wager.

DR. KRESS: He's betting there will be and it's your bet that there won't be.

DR. WALLIS: Yes, and I've given you a good break. Two against a billion. That's pretty good.

MR. TREGONING: Repeat the question again.

DR. WALLIS: Well, look at that blue dot

over there at 6, right.

MR. TREGONING: The Ps, right.

DR. WALLIS: I give you two, and your chance of losing is so low that you could afford to bet a billion against it, couldn't you?

MR. TREGONING: That's where consequences comes into play. I would look at the consequence of losing a billion dollars versus the reward --

DR. WALLIS: Well, $2 is sure.

MR. TREGONING: Yes, but $2 doesn't buy me much economic -- at the risk of trivalizing it, I think --

DR. KRESS: How about if he bets -- he gives you 2 billion against --

DR. SIEBER: Four dollars.

DR. KRESS: What's the product --

MR. TREGONING: I'm not an economist, so at some point --

CHAIRMAN SHACK: Now you're talking real money.

DR. KRESS: The trouble is you have to wait too long before you get your winnings.

MR. TREGONING: Well, we didn't define the time --

DR. KRESS: Because this is 25 years.

MR. TREGONING: This is sort of current.

DR. KRESS: Oh, this is the current. This happens per year. So you only have to wait a year.

MR. TREGONING: I only have to wait a year. Only have to wait a year.

DR. WALLIS: Two bucks a year, that's all right.

MR. TREGONING: Yes, I don't think I would -- I don't think I would take that bet for several reasons.

DR. SIEBER: Declare bankruptcy.

MR. TREGONING: The next several slides I've got, I'll leave this. In the next eight slides we delve into system contributions for these LOCAs both piping and nonpiping. We can skip those and come back if there's time. We can -- I'd planned to do maybe one or two of these and then say the rest of them are in the packet. We could do that. Or we could try to plow through them all.

CHAIRMAN SHACK: Let's go ahead to the uncertainty and panel variability and come back.

MR. TREGONING: Okay. That's fine.

And there's 2 of these slides. One for Bs and one for Ps. I can -- we'll go through one and then we can go quickly through the other.

One of the things I will say, we haven't done statistical confidence bound assessment yet. And so what did we did here? Well, we just expressed variability in terms of what is called an interquartile range. And what the interquartile range is, it's the difference between the third and first quartile of the responses that we got.

So the symbols are essentially the median of the panelists' response for the -- and then those bands are the difference between the 25th and the 75th -- or the lower one is the 25th and the upper one is the 75th percentile. So that bar contains 50 percent of the panelists' responses.

One of the things we still need to do and that we plan on doing shortly is calculate rigorous statistical confidence bounds on this data.

MR. ABRAMSON: We expect the 95 percent confidence will be a little bit wider than that.

MR. TREGONING: Yes. So one thing you see is -- and while the ratios between the 95 percentile and the means didn't change much with LOCA size, what you do see is the uncertainty obviously increases with LOCA size. And, again, this was certainly an expectation that we had going in, that this would indeed happen.

And the other thing which we saw which we certainly expected is that there is more variability amongst the panelists in their 95th percentiles than there is in either of their median estimates. And I've shown median up here because the means effected more radically by the relationship between 95th and the median percentile. So that's the only reason I've shown medians here to make the point that there's much less variability in the median for 50th percentile responses than there are in the 95 percentiles.

MR. ABRAMSON: In fact, it's the mean is the percentile, somewhere between the 50th and the 95th.

MR. TREGONING: Not necessarily.

MR. ABRAMSON: No, but I think for these cases it probably is, isn't it?

MR. TREGONING: Yes.

MR. ABRAMSON: For these cases?

MR. TREGONING: For these cases.

MR. ABRAMSON: For these cases that's right. These were skewed for a lognormal, which is what these all are, it will be. It will be bigger than the median.

MR. TREGONING: Right. Right. But it's not necessarily less than the 95th for a lognormal.

MR. ABRAMSON: That's true. Right.

MR. TREGONING: They are in all these cases.

MR. ABRAMSON: Correct. That's right. If it's really skewed, then it would be bigger.

MR. TREGONING: And if they were that skewed, then we've got to go back and look at our analysis techniques.

DR. WALLIS: These are cumulative things, aren't they, you told us?

MR. TREGONING: The categories themselves are cumulative.

DR. WALLIS: So how can one up as you go to the right, even if it's a --

MR. TREGONING: That's artifact -- that's an optics artifact.

DR. WALLIS: Because it looks as if it goes up, but it doesn't.

MR. TREGONING: Right.

DR. WALLIS: It's actually level. It's actually level.

MR. TREGONING: It's essentially the same. It's essentially the same.

DR. WALLIS: Okay.

MR. TREGONING: Yes. I spent some time

looking at that, because I obviously had the same question myself.

DR. WALLIS: Maybe if I stood up, it would look clearer.

MR. TREGONING: You can squint, it still looks -- it looks like it's higher.

DR. LEITCH: Did you say last time, and I think you did -- I'm just trying to refresh my memory accurately, that the panel was specifically told not to consider acts of terrorism, sabotage, disgruntled employees, those types of things?

MR. TREGONING: Yes. Yes. We asked them not to consider that.

MR. ABRAMSON: That's not normal operating conditions.

MR. TREGONING: With any of the rare event stuff, you get into the quandary of estimating the frequency of the event itself.

DR. WALLIS: It's hard to exclude with ten to the minus 9.

MR. TREGONING: Well and that's -- that this is why when -- we look at this information as just -- and again, somebody from NRR will obviously need to jump in. But we don't look at this as the total picture. We look at this as one piece of information. And I think based on this information, that really determines the other things you need to consider as well and the amount of rigor you put into evaluating these other things.

I mean, some of the these numbers you're getting to where the large seismic earthquake risk could start to become important again.

So we certainly realize consequential LOCAs need to be evaluated in some way, including the LOCAs that you could get through a terrorist challenge, but that's something that as we as an agency, and we've got this combined working group with Research and NRR, one of the things we'll be doing with these numbers first is looking at them and trying to make a rational assessment of what other challenges we really need to assess to provide a good basis for going forward.

So there's certainly an expectation that when we consider the rule or consider what ever regulation, while it may be based on LOCA frequencies such as these, there will be consideration of other challenges as well.

DR. KRESS: Are these frequencies per plant or is it frequencies for 100 plants?

MR. TREGONING: These are per plant per

calendar year.

DR. KRESS: Per plant?

MR. TREGONING: Per plant for calendar year.

Okay. So that's Bs. This is Ps. The thing that you notice about the Ps is the IQR ranges are generally larger than the BWR ranges. Well why is that? Well, we had a lot of disagreement about the importance of things such as PWSCC as we move forward. Some people thought it was going to really elevate frequency, some people thought it was no never mind. So, I think that was one of the things that was driving the PWR increased variability. And you see that really in the categories 3 to 5, which are again that LOCA that are fractures in pipes of the 6 to 14 inch range that people thought were most susceptible to aging. So I think that, along with the fact of the difference of opinions and PWSCC is one of the things driving the variability, the increased variability in the Ps plants.

I guess one point I'd like to make is these large ranges, they're really not surprising. We're trying to estimate things that are rare. We would expect that the ranges would be large. If they weren't large, we would probably have to question the underpinning for our entire approach and whether we had somehow biased the group opinion in some way.

So I personally believe that the large variability is a good thing, and it's an indication of, I think, the quality of the estimate. Now, that doesn't mean that there's not challenges that we're going to have an agency to try to come up with decisions based on information that has a lot of variability in it.

And, again, I talked about this. Some of the reasons for this variability are not only the nonpiping contributions, but also the PWSCC concerns.

This is always interesting I think, and we could spend a lot of time on these types of plots. But what this shows you is the BWR total frequencies and uncertainties, and these are just plots for each expert. And the experts have been identified by a letter. These were all the ones that answered -- that provided frequencies for us for the BWRs that answered all the various BWR questions.

And what this plot shows is really it just gives you a sense of the range and where the experts fell relative to each other.

So the bar represents the fifth median and the 95th percentile estimate provided for each expert.

And these are the show of frequencies.

And the dash line just connects the medians to give you some idea of what the trends -- what the consistency in the trends were.

So the things you see here, I mean again the uncertainty intervals that the experts gave us were wide. Again, this is what we would have expected. To get anywhere -- and this is LOCA category 5, two to four orders of magnitude variability. And it's actually the uncertainly was greater than the panel variability in the medians here.

So the experts were much closer in their predictions of what their best guess estimates were than they were in the uncertainty about that best guess.

One of the things I don't show, but if I showed all the LOCA categories, these uncertainty intervals for each expert increases as LOCA size increases. Not dramatically, but it does increase. It goes anywhere from one to three orders for the smallest LOCAs up to what you see here, two to four orders for category 5.

DR. WALLIS: You probably can't estimate. It would be interesting to see how it goes with expertise. If the person who knows more, gives a wider range than the person who is sort of naive and only considers a few things to give them a narrow range.

MR. TREGONING: You tend to see the opposite. People that thought --

DR. WALLIS: Know more are more certain?

MR. TREGONING: Yes. That tended to be the expectation.

DR. SIEBER: But that's a fact, that's a fact of normal life. People who know the least are --

MR. TREGONING: You know, it's interesting, you know a good comparison is you look at D there which had the smallest uncertainty compared to E which is the largest. I mean they're one right next to each other. And D's total uncertainty range is encompassed within E's upper bound uncertainty range. You know, I will say that D had a lot of experience with BWR plants, and that was one of the things -- probably more --probably more experienced than E did.

DR. SIEBER: IS there any pattern to the order of those are they just random?

DR. KRESS: They're random.

MR. TREGONING: You mean in terms of --

DR. SIEBER: Who is A, who is B --

MR. TREGONING: Well, I'm not going to tell you who they are. They were assigned randomly.

DR. SIEBER: Oh, okay. So this is like the order in which they came through the door?

MR. TREGONING: And all I've done is plotted them alphabetically. That's all I've done. So these were assigned randomly and they're just plotted up there alphabetically. I didn't make any attempt at ordering or anything. I didn't think that was necessary or appropriate.

MR. ABRAMSON: I guess it's A through H, I think you had to say you assigned them randomly for the 8 people who gave you BWR answers.

MR. TREGONING: No. We assigned them randomly in general. It just so happened that I, J, K and L didn't give us Bs.

If you look at the Ps, for instance, see there's no D, there's no F because they only gave us B. Some people gave us one estimate or the other and some people gave us both.

So we assigned them randomly. It just so happened that A through H were the ones that gave us the B estimates where I, J, K and L did not give us B estimates.

DR. KRESS: Those of us who believe in

randomness, would say that's a highly unlikely outcome.

MR. TREGONING: Possibly.

DR. WALLIS: Yes, that's true.

MR. TREGONING: You know, I didn't assign the letters. I know who did. And maybe it was not as random as you would think.

DR. SIEBER: Well, it's certainly not important to us since we don't have a clue.

MR. TREGONING: But I will say when those numbers were assigned, we didn't necessarily -- there were some people that hadn't provided us their BWR estimates at that time. So, again, I -- I still think that's serendipitous, but whatever.

DR. SIEBER: Okay.

MR. TREGONING: The other thing that we found which was interesting, and again we've just started looking at these trends to try to determine more about it, but one of the things we saw with Bs is that the relative ranking of the panelists was pretty consistent as a function of LOCA size. So what's that mean?

Well, that means, you know, let me pick out C for instance. C was about the highest for this LOCA category 5. If I go back and look at all the

LOCA categories, C was about the highest for every LOCA category.

If I look at E or G or H, they're about the lowest for this LOCA category. They were also about the lowest for the other categories. So it's interesting that you see the variability for smaller LOCA in the ranking almost tracking for Bs as a function of LOCA size. So that's an indication of the importance of whatever base frequencies you started out with.

MR. ABRAMSON: It says the experts had to be self consistent.

MR. TREGONING: Yes. Look at the same plot for Ps. And here I'm showing a smaller LOCA category just to spice it up because this is one of the LOCA categories that people expected to have, again, bigger detrimental effects, bigger effects due to aging. Again, I think we see it here, again, larger uncertainty in the PWR estimates. Two reasons for that which we've talked about. One it was really the unknown extent that we've got currently at PWSCC. And the second one, we've just got a lot more potential nonpiping LOCA contributors in the PWR plants that the people were just more uncertain about in their estimates.

So again with category 1 you're about at one to three orders, about the same order of magnitude uncertainty as you were with the Bs. Again, that's driven by the small piping, so that's to be expected.

When you get up to the large LOCAs, LOCA categories 3 and 6, you can see as much as five orders of magnitude uncertainty for some of the experts, which is really obviously quite a lot.

The other thing we noticed with the Ps is relative ranking of the panelists was not as consistent with LOCA size as it was for the Bs. So if somebody was high for category 1, they weren't necessarily -- they didn't necessarily remain relatively one of the higher ones for category 6.

The other thing we see -- and, again, you see more variability in the median responses. And this we saw in the earlier plot as well for those higher LOCA categories where we had much bigger spread in the median number.

I want to go ahead and show this. I've got a couple of slides just comparing these results with some prior studies. And what you see here, this is just targeted selection.

I think PWR MB LOCA comparison and a BWR LB LOCA comparison. And I'm showing three sets of numbers of here. The black ones are the WASH-1400 which were originally primarily based on oil and gas transmission pipeline failure frequency data. The red curve or the red dots at the NUREG CR-5750 estimates which we have been using since about '96/'97 time frame. And then the green one here are our current estimates for the same effected break size.

Now when I get to this one and when I show the large break LOCAs, I show all of our different -- I show LOCA categories 3, 4 and 5. Why is that? Because the break size associated with a large LOCA in 5750 of about 6 inches falls between category 3 and category 4. It's actually a little bit closer to category 4. That's why I show both of those estimates on there.

Generally what you see is that our estimates were obviously lower than WASH-1400. I think that's no surprise. They were generally pretty comparable, actually, with 5750. And I'll show the breakdown here later.

The biggest difference occurred with the PWR MB LOCAs, which is what I'm showing there. Here we got about an order of magnitude difference higher than they were estimating within 5750. And, again, that's really largely driven by PWSCC concerns as well as the number of LOCA sensitive areas within the PWR plants.

DR. KRESS: Refresh my memory. How did NUREG 5750 get their numbers? Is that a probability of fracture mechanics?

MR. TREGONING: What they did is they looked at service history data for leaks. And they found all the leaks that they had in class 1 piping for the most part. And they said, okay, this is my frequency of leaks. Now I'm going to have a conditional probability of failure to get a certain LOCA size. And that was based on some mechanistic work. And that was done by Helmet Schulz and Belizi in Germany where they looked at data and they also tried to look at experiments to come up with conditional as a function of pipe size failure probability curves. But that curve was only -- you know, if you talked to Helmet Schulz, it was only developed for fatigue type of failures. And they applied in 5750 for anything.

So they just said I've got leaks and apply this conditional failure probability and that's going to give me my LOCA frequencies.

So very different estimate. Very different technique. And the reason that we're coming up as similar in some ways is comforting, but it's almost irrelevant because we used totally different basis to establish that estimates.

DR. LEITCH: Did I understand you to say that prior to this solicitation process, the NRC did an internal assessment?

MR. TREGONING: Yes.

DR. LEITCH: And I was just wondering how those numbers compare?

MR. TREGONING: That's on the next slide.

DR. LEITCH: Okay.

MR. TREGONING: And this table. And all I've done, I didn't want to plot all these, but I just had a table comparing the means. And the table's a little bit convoluted, so let me walk you through it.

But the upper four at plant type Bs versus Ps. And then the next column is historical LOCA size, either small, medium or large. There are two large breaks, and why is that? Because I've compared them against our LOCA categories either 1, 2, 3 or 4. So because the large break historical falls in the break size with between our LOCA categories 3 and 4, I make both those comparisons.

Do the next column gives the comparison of the current elicitation ratio with respect to the 5750 estimates. So that's here. So this is current divided by 5750.

This last column is current divided by pilot. It has a pilot elicitation result.

The only difference to keep in mind is 5750 is making current day estimates. When we did the pilot study, we only wanted to look at one time. So we went all the way out to the end of license extension. So these results that I do with the ratio are actually the current elicitation results that we got out of 60 days so I could make a direct comparison.

So if I focus on first the column that shows a comparison with NUREG CR-5750, generally I'm within a factor of three from those estimates for all the categories except that PWR MB LOCA, which I'm a factor of -- again, about 7 or 8 higher than the current estimates. So all the rest of them are actually within a factor of three, which again for these estimates is actually pretty close. And that certainly wasn't the intent, but it's just how it ended up.

DR. WALLIS: There is a four -- am I misreading it?

MR. TREGONING: Well, you have to be careful because, again, the LB break size really falls in between this.

DR. WALLIS: Oh, okay.

MR. TREGONING: And it's actually our 4 inch size start at about 7 inches where these start at about 3 inch break. So the 6 inches is actually closer to a LOCA category 4, which is about -- within a factor of two, which is actually pretty close. So, there is that factor.

But if you look here, the biggest -- you know, generally we got higher estimates for the MB LOCAs. And, again, this is consistent with the fact that the aging effects were thought to be more detrimental to the intermediate size pipe. It tended to be a little bit lower on the other break sizes, either small breaks or large breaks, but not dramatically so.

If I make comparisons with the pilot elicitation, they're a bit more despairing, but still not -- I wouldn't consider them dramatically so. But we did see a difference between the Bs and Ps.

The BWR, the current elicitation results are always lower and they're up to almost ten times lower.

The PWR results are currently in the current study are higher and they can be almost up to ten times higher. So you have wider disparity with the earlier elicitation results that we had.

DR. FORD: So you're saying that if you do it for 60 years, the LOCA frequency is going to be lower than currently?

MR. TREGONING: No. No.

DR. FORD: It's the other way around?

MR. TREGONING: No. These are just comparisons with the other study. And all I'm saying is compared to the other study our current BWR estimates are up to ten times lower than this other study. Where with the PWRs they're about ten times higher. That doesn't say -- actually the trends that we got are relatively constant, relatively.

You see some small -- I'd say relatively small increases out to 60 years of maybe factors of three or something like that. But it's not -- again, it's not dramatic.

DR. FORD: You don't expect them to go up?

MR. TREGONING: For the most part it's the 60 year estimates in the current elicitation, they're slightly higher than the current day estimates. But, again, a factor of 3 or less usually.

You saw the uncertainty we have in the current day estimates. There's even more uncertainty in the 60 year estimates. But the median responses are only about a factor of three or less higher.

And the 40 year estimates, depending on LOCA size, some of them go down, some of them go up. There's no consistent. But they're all, again, relatively minor trends and usually within a factor of two or so, which given the uncertainty -- you know, we don't consider many of these trends to be statistically significant until you get up to about a factor of five or so.

DR. WALLIS: So is that just saying -- if it is -- that was considering it's a linear -- it's purely linear the time? Like 25 -- 60 over 25 is roughly --

MR. TREGONING: These are comparative rations. These don't say anything about the absolute numbers. And these are comparing against two different studies. All that you can say here is that the pilot elicitation, right, we got much higher frequencies at 60 years for the pilot elicitation than we did in the current elicitation. And conversely at 60 years in the current elicitation, we got higher estimates at 60 years than we did in the pilot study.

DR. WALLIS: They're both at 60 years?

MR. TREGONING: They're both at 60 years.

At 60 years.

MR. ABRAMSON: I think we'd also have to say that the pilot, since it was a pilot and I think we feel that this study has much more credibility than that. That was just a pilot study.

MR. TREGONING: Well, the other thing, the pilot was two years ago and I think PWSCC has sort of exploded on the scene since then, too.

MR. ABRAMSON: It was done in a much quicker time frame and so with all NRC people, too, I should mention.

MR. TREGONING: I think these trends are actually relatively consistent with a lot of the qualitative rationale that we heard from people.

MR. ABRAMSON: Yes.

MR. TREGONING: Even though again the techniques that I'll say the pilot, the current elicitation and 5750 used to estimate frequencies were very different. We did it -- the structure that we used in the pilot elicitation ended up being quite different than the structure we used in the formal elicitation because we had a different panel. And that's the question Tom asked me before; hey you did a different panel, what would you expect the differences with that different panel?

Well, you see here. But I would argue that that earlier panel even though they did a very good job, we weren't able to apply the same rigor just because it was an accelerated exercise.

MR. ABRAMSON: And also it was all NRC staff.

MR. TREGONING: And it was all NRC staff.

So one heard arguments, that well you --

MR. ABRAMSON: They're bias.

MR. TREGONING: --numbers from that --I don't know that that's true, but I've heard arguments.

DR. KRESS: I know it's not your job, but you have any notion of these new values and distributions can be used to establish a new definition of a large break LOCA size?

MR. TREGONING: You're asking my opinion or --

DR. KRESS: It's not your -- that's somebody's else job.

MR. TREGONING: No, it's not -- I'm going to explain how we're going to go about doing it or the process and then I'll give you my own opinion, and I'm sure I'll be shouted by others in the room. But we've got a working group formed between Research and NRR that the charter of that working group is to address those very issues. And that's why our working group compatriots have been very vocal about understanding the basis and the scope for this elicitation exercise. Because we've got to take that as a group and turn around and make decisions potentially for limiting -- for altering the design basis.

MR. SNODDERLY: Well, not just the group. All of us. I mean, that's what it comes down to.

MR. TREGONING: Of course. But first it's going to be the working group. And one of the things I would argue is that this is just a piece of the information. We've got some work going on now that's looking at thermal hydraulic response, responses from plants. And what we're going to do, I think this information is going to be very useful to: (a) to sort of focus the efforts that we need to from here on out. And what do I mean by that?

Well, we've broken these numbers down into system related LOCA frequencies. And I can show some of that. But we predicted systems that we expect to be specifically challenging for LOCAs.

One of the things we're going to do is before we're going to postulate breaks in those systems of various sizes that are commiserate with the frequencies that we expect here. And we're going to develop or we're going to determine what the thermal hydraulic response is.

I think those frequencies along with the thermal response predictions or plant operating characteristics that we predict to responses for those breaks as well as the risk sensitivity to breaks in various systems and locations, those three pieces of information will be what we will be basing any postulated rule or design basis changes on. And I really think you have to consider all those things equally. You just can't take the LOCA frequencies and say, haha, based on this frequency this is going to be my break size. I think it would be --

DR. KRESS: That's too simplistic.

MR. TREGONING: -- too simplistic.

DR. KRESS: If you were to do that, why would they choose? Ten to the minus six?

MR. TREGONING: I don't know. We talked about that today. That's a challenge.

DR. KRESS: That's traditionally what they call a break between design basis and other plates.

MR. TREGONING: Right. One of the challenges in this was discussed quite a bit this morning. So at the risk of retreading on the ground, one of the challenges with this is how do you use frequency information that has quite a bit of uncertainty in it and develop regulation from that. So that's the challenge that we --

DR. KRESS: Yes, I was wondering how you'd use this. Say the 95 percentile numbers or whatever.

MR. TREGONING: Yes. I think our intent is -- and, again, it's too early to determine and we're just -- we're hashing this out. I mean, I think we want to use --

DR. KRESS: First you got to get the numbers.

MR. TREGONING: First we got to finalize the numbers. But I think we'd be looking at high failure probability, high competence type of numbers to base part -- to base this regulation on.

MR. ABRAMSON: I would just add I think we would have to consider both the uncertainty and the variability. That is each -- the uncertainty for any one expert plus the variability in the panel. We'd have to consider that. But how, of course, is the big question. Well, of course, we would have to do anything, it goes without saying, in a somewhat conservative way because we're doing this in a regulatory framework. But that's all we can say this morning.

DR. KRESS: How did they put that together in NUREG 1150? Didn't they have the same problem there?

MR. TREGONING: I don't know if anyone else -- I don't feel --

MR. ABRAMSON: My general impression there is they got one distribution at the end. And, you know, they had the means and the 95 percentile.

DR. KRESS: Yes, but how did they put it together it, though?

MR. ABRAMSON: You mean how was that -- I don't know. I can't answer that.

MR. TREGONING: Yes, I can't address that. I don't know if anyone --

MR. ABRAMSON: I was not involved in 1150, so I couldn't answer that.

MR. TREGONING: That's something that we're as we look at going on and how to use these numbers, obviously looking for precedents within the agency is going to be important. But even though understanding that history is important, we're also as we talked about this morning, we're breaking a little bit of new ground. So we can't rely totally on precedent either. So we just have to be -- and I think there's a general understanding of the challenges ahead. Not on the solution to the challenges, but at least the challenges that exist. And I think as an agency the sense that I get, and this is my opinion of course -- is that you know these things are going to be weighed very carefully and really discussed very thoroughly before we really move forward with anything that we propose.

So, I don't know that I've answered your question, but --

DR. KRESS: Well, I didn't really expect an answer. You gave me more than I expected, frankly.

MR. TREGONING: Okay. We're not going to touch on safety culture. We'll spend the rest of the day on that.

4:30 we're scheduled to be -- I've got three slides. Let me try to finish up.

MR. SNODDERLY: Rob, if we could try to quickly summarize safety culture? It was essentially, the impression I got from the paper was that in our past experience if a plant with a poor safety culture is discovered, it's usually quickly rectified within a year to two years. And that pattern would tell you that the current framework is capable of account for that or correcting for it.

MR. TREGONING: Well, not entirely. Now you've dragged me into it. So to address the question I've got to go to this slide.

Safety culture is, again, a bit of a nebulous thing. It means different things to different people somewhat. But when we had an initial panel discussion there was an overwhelming sense that, yes, safety culture can effect LOCAs. All the panelists agreed to that fact. And then we said okay given that it can have an effect, how do we deal with that? And what we decided in the discussions is that there was a decision made that safety culture is not a function really of piping system or piping component. It's not a function of those things. So because it's independent of these other variables, we separated that from the rest of the discussion. We asked a separate question that just said, okay, what do you think is the future safety culture effect, what could it be on LOCAs? We asked for their best guess and their uncertainties.

And, you know, that's why we decided to separate it. It made it clean in the sense that when we looked at the piping failures, we were really just considering just aging and mitigation and things like that. We weren't considering safety culture on that.

And then we said, okay, now just consider safety culture in general. What do you think those effects will be. Well, these were some of the results that we got.

And we had further subdivided it into utility and regulatory safety culture. So the first result we got back was that most, if not all of the panelists, felt that utility and regulatory safety cultures were highly correlated. Well, what's that mean? Well, that means that something that one body does it going to effect the other and they're almost going to move in locked step. So this notion that there's not a separate regulatory utility safety culture, that there's really just this overriding safety culture that's at play here, most of the panelists that we talked about expected either improvements or no change in the future due to safety cultures effects. And we talked quite a bit about Davis-Besse in this area of it and how peoples' expectation of the Davis-Besse event is and would continue to shape evolving safety culture within the whole industry and how we would treat passive system degradation.

So, however, even though that people that in general that safety culture would continue to improve, there was a recognition that a lot of the uncertainties developed by or due to plant-to-plant variability. And that while generally you could be moving toward a better safety culture, your concern is always the one or two plants that for whatever reason has a deficient safety culture.

So if you look at the results we got from people when they calculated their 95th percentile, many experts did say, hey, my 95th is driven by sort of the -- the rogue plant that for whatever reason is more safety deficient than the other. But because of this we noted and we tabulated all of these results, but we didn't apply any sort of safety culture adjustment to the frequencies that we developed.

And, again, most of what people recognized is -- you know, and we didn't want to get into addressing safety culture in the sense that providing ways to improve it, that wasn't the focus of this panel.

DR. BONACA: You know, I totally agree with your approach in that you're looking for a technical result. And I think the other one, safety culture, it's really intangible with respect to what the expertise of the individuals are and what you're looking for.

However, when you translate these results into regulatory requirements, then I am puzzled by how we can include them, if there is any way. I mean, that's two different things. One is a deviation of certain insights coming from probabilistic fracture mechanics and past experience, and that's one thing. And then the other one is to establish a regulation that is based on this and ignoring other -- I could content that maybe -- I mean I am concern that at times, you know, we like to create boxes and to put our problems inside there. So we had Davis-Besse, we're all puzzled and troubled by that. So we create a box called safety culture and put it inside there. It's like saying it won't happen again because we're going to recognize that and fix it.

You know, I could contend that it is a broad organizational failure. A truly cognitive failure where they kept thinking that leakage was coming from the flanges. They all convinced themselves. And they weren't the only one to be convinced. There were other oversight functions.

And I'm just wondering if one of these days we're going to have another organizational failure, you know, about some other issue. I don't know what it is. Some bolting thing that we will not recognize and we will call it safety culture. The fact is it's going to come back and bite us.

So I'm not saying I have a solution. I just am troubled about not considering events such as this as possibilities and sabotage, of course, we discussed this morning.

DR. WALLIS: You're concerned with the human aspect, really?

DR. BONACA: Yes. Absolutely.

MR. TREGONING: Yes, we did consider the human aspect in the sense of mitigation that we considered the human errors as potentially being a contributor. So, for instance, bolt over-torquing, that was something that we specifically talked about and we asked people to consider.

Many people talked about usually mitigation is a good thing, but you have things like, you know, people leave wrenches in steam generators and things like that. And a lot of the panelists did have anecdotal if not actual data on those types of events.

So I know a number of them did really consider those effects when they said, you know, I expect due to aging potentially that the frequency will be X. But, I know these other factors, human and otherwise, are going to continue to keep it elevated in the future.

DR. BONACA: And I guess, you know, the way I view it a little bit, one way is to talk about the design basis risk. I mean, if we are moving, going to smaller breaks and the issue we discussed this morning about, you know, how much now you have left beyond design basis, you know, maybe there is one way and the way you can control the risk beyond design basis, for example, by pretend inspecting that whatever changes you make, you'll still have a small increase in risk in total. Then you're taking care of possibly of these other events because you are considering performance of systems. You know, the -- of the systems, most likely. They're going to take care of beyond design basis event that way, through small increase in risk.

But anyway, we can talk about that later on when we talk about, you know, members perspective.

MR. TREGONING: Do we want to forge ahead, Mr. Chairman?

CHAIRMAN SHACK: Yes, I think so.

MR. TREGONING: Okay. As I mentioned, we've had a first look at the results and we've done a good bit of analysis. But one of the things that we're continuing to do to examine the robustness of these results is we have a number of sensitive studies that we already have conducted, but that we are also ongoing. And I just wanted to list a couple of things that we've got ongoing, and this is, again, we're doing these things to increase our confidence in the final estimates that we will be using for these LOCA frequencies.

One we're doing sensitivity studies where we're evaluating individual uncertainties. And did you want to do this slide?

MR. ABRAMSON: Yes, I can.

MR. TREGONING: I'm going to defer to Lee. He said for me to do this, but I'm going to go ahead and defer to you on this slide.

MR. ABRAMSON: Okay. Yes. The first one is the over confidence adjust. It's been well established or we accepted wisdom in the elicitation community that X for anybody -- not just X, but anybody tends to be over confident. And when they give you, say, a 95 percent and 5 percent bound, that includes 90 percent of their -- it's suppose to be 90 percent confidence. In fact, based on almanac type questions for which you know the answer, they're off by about a factor of two. So that 90 percent is more like 50 percent. And this has been established over and over again. And in some of the training exercises I did with them, they can kind of see this. So people tend to be over confident.

And so what we're planning to do is to do what we call a targeted adjustment or at least to try that as a sensitivity study.

Some of the experts had rather narrow on confidence bands, uncertainty bands. Others had very broad ones. So we're going to see what happens if we take the ones that are very narrow, and I think there were just there in particular who were extremely narrow, relative to the others, and adjust them a little bit. And say instead of these being really 90 percent coverage, maybe it'll be something like 80 percent coverage or 75 percent coverage and see what effect this would have on the answers.

This will be a sensitivity study.

Another thing which I already mentioned is when we're adding up the lognormal distributions, we had to make assumption to generate the variants. We assumed that they were perfectly correlated, which gave you an upper bound. A lower bound you can get by assuming they're independent. And so we'll look at that as a lower bound.

Then under panel variability, what we've done so far you've seen a lot of that, is we took the panel numbers and we replaced them with the medians. Essentially, this is very much in the spirit of a box plot, that's in fact what we did.

But instead of the medians, one thing to do is to take the geometric means and we'll try that. And another one is a so called trim geometric means. This is like Olympic type scoring where you throw out the high and the low values, so they won't be effected by it.

So this will be another way of taking the information or the results we get from the panel and seeing what's a reasonable way to replace them with a single number instead of just the median.

MR. TREGONING: Yes, and these are different ways to get the central value of these various parameters of the distribution.

MR. ABRAMSON: That's right. Yes. Because that's what we did to get a lot of the --

MR. TREGONING: And we've actually done this. And what you find out is the medians, there tend not to be much difference for the smaller LOCA sizes. But for the bigger LOCA sizes you can haver about an order of magnitude difference just in the way you estimate the central values.

Medians for this exercise always gave us higher estimates. Geometric means always gave us the lowest estimate. And the trimmed geometric means, as one might expect, was somewhere in the middle.

DR. KRESS: I would recommend you go see what they did at 1150, because at least you'd have an NRC precedent on how to deal with that issue. You may not want to use it. But it would at least be worth looking into.

MR. ABRAMSON: That's right. But we'll certainly -- we'll certainly take a look at that.

And then as was already mentioned, we've used so far the interquartile ranges as our measure variability. What we are going to do, and probably make that our main measure of variability, will be the 95 percent statistical confidence bounds.

Then we're going to calculate what we call group estimates for BWRs and PWRs. What we'll do is we'll take the piping numbers where you haven't seen those because you didn't have a chance to do the separate piping and nonpiping.

MR. TREGONING: They saw the means.

MR. ABRAMSON: Pardon me?

MR. TREGONING: I showed the means.

MR. ABRAMSON: What we'll do is we'll take those and we'll get an answer for the piping -- well, let me just back up a minute.

What we did for the total estimates with BWRS and PWRs we based this on individual -- each expert was propagated individually. What we planned to do here is to do what we call a group estimate to get a group number for the piping, which will be based say on the medians and a group number for the nonpiping and add those up. So this will be a kind of a group consensus. This is, again, backing away from just propagating individual estimates.

And then along those lines, we'll get what we call a panel estimates. We'll assume there's a 13th panel member. And the 13th panel member, how will we get his results? Well, we're going to take the responses for each of the panel members. We have literally, I don't know, hundreds probably -- hundreds of responses altogether and we'll just take the medians of those responses. And we'll say all right, we have our 13th panel member who is the median of each one of all the panel members who answered those questions and we'll just propagate that through and see what it looks like.

It'll be an interesting exercise to see

how this compares to our results that we've gotten so far.

So these are some of the sensitivity studies that we're planning to do. And probably as we go along with these, we'll probably maybe thing of some others. If we have time, we'll certainly do those.

MR. TREGONING: I think we're planning on -- the final estimates are clearly going to be these individually derived estimates just because of --

MR. ABRAMSON: Yes, I think so. I would say so. Yes. We're leaning very strongly --

MR. TREGONING: We're not planning to deviate from that philosophy.

MR. ABRAMSON: Yes, that's right. And these are sensitivity studies to see, obviously, that the question is well suppose you had done this differently, how would the results have changed.

DR. WALLIS: Well, that's very interesting. But, really, what you do here should depend on what you're going to do with the answers you get. Because, I mean, how you look at your sensitivity to uncertainties may depend upon how you're going to use that in some regulation later on.

MR. TREGONING: Well, again, we need to

find the statistical confidence intervals, those confidence intervals could be based on --

MR. ABRAMSON: I'm not sure --

DR. WALLIS: Yes, but suppose the regulation comes back that you have to be 99 percent confident about --

MR. ABRAMSON: Oh, yes, that's true.

MR. TREGONING: Right. That's right. We just said 95th percentile. But once we develop those bounds, we can obviously determine any ---

DR. WALLIS: You can massage it?

MR. TREGONING: Yes. Any percentile of confidence we want to apply can be determined.

MR. ABRAMSON: I would characterize what we're going to do, what we're going to come out with we're going to try to be as, let's say, as honest and thorough as we can be in summarizing the results of this whole big elicitation exercise. And so in other words, we want to give -- I mean, this working group and of course many other people in the NRC are going to take these numbers and use them as part of a much larger project. At least we want to feel as confident as we can that what we're giving them to start with a reasonable, let's say, unbiased expression of what we got out of the panel of this whole exercise.

MR. TREGONING: And as Lee mentioned, we've done several of these sensitivity studies already, so some of these are completed. Some of them are ongoing. Some of them are yet to be initiated.

So what do we have remaining --

DR. WALLIS: This is going to be a NUREG, this whole thing?

MR. TREGONING: That's the plan. That's the plan.

So what do we have remaining to do with respect to the elicitation only, not with respect to the whole risk-informed rulemaking exercise?

Well, the first thing we have to do is we have to complete the analysis, which we're close. I think we're estimating another two to three weeks before we're done with our initial analysis.

We have to finish our sensitivity studies. We have to develop statistical confidence intervals and determine our final frequency recommendations that we'll use as the basis for moving forward with regulation.

Another important component that we have to do is we've gotten feedback from our panelists throughout the entire process, which I will say has been generally good. I think at every stage I think we have good buy-in from the panelists themselves, which is important. If we didn't have buy-in from the panelists, we'd certainly question the integrity of the results. And we've provided these initial results to them, but we also have to present the results, the final results in the sensitivity studies back to the panel.

And we'll be doing that -- in fact, they'll get some information within the next couple of days that will show what the final initial set of results are. But the main thing we're going to use for feedback is once we're finished with the sensitive studies, we'll complete our draft NUREG on the process and we'll actually submit that to the panel members for some initial feedback, as well as internal comments before we move too far down the process of getting that NUREG published.

Once we have the NUREG available for public consumption, we're going to solicit feedback from, obviously, all interested parties and that would ACRS, stakeholders and the public at large. So we really are expecting to get quite a bit of feedback on this NUREG when it's out.

And then the final part of the process is some independent peer review. We're planning very shortly to initiate a peer review process of these estimates.

The peer review that we're planning is not going to focus on the input that we got from the experts. We don't want to re-derive that. But we do want to review the process to questions that we used in the analysis to make sure that they are -- that they were suitable and not biased in anyway and that the analysis and the process that we followed have been rigorous.

DR. KRESS: Suppose the peer reviewers say, hey, you shouldn't have ought done that? You aren't going to go back and redo it, are you?

MR. TREGONING: It's a little premature to say. If they had significant issues with something that we did in the process, then potentially we would have to.

DR. KRESS: I think you're putting the cart before the horse. I would have done that peer review on the process and the questions first and then worried about -- but after the fact, I don't know --

MR. TREGONING: We couldn't do it first because a lot of the structure involved in --

DR. KRESS: Yes, I understand that.

MR. TREGONING: We talked about doing it

in parallel with. And there would have been some possible benefits to that. I agree with that. But that's -- that's just not how we choose to do it.

DR. KRESS: Yes, it's a little hard for me to believe you'll go back and redo the expert elicitation based on a peer review.

MR. TREGONING: There is precedent for that.

DR. KRESS: I guess you could, yes.

DR. WALLIS: All your experts are somehow tied up with the nuclear business it seems to me? It would be good to have a peer review that brought in some outsiders who were honest and experts in something else that was related to this but who could not be cited as being all part of the nuclear club.

MR. TREGONING: Right. And I think our idea was we really wanted people that were experts in elicitation and minding --

DR. WALLIS: Right. Right. Right.

MR. TREGONING: What community they come from, we hadn't necessarily considered.

MR. ABRAMSON: But there are certainly some who are not, let's say, identified with the nuclear industry, although they may very well have done some work at one time or another, but not identified with the nuclear industry.

MR. TREGONING: Right. But you're right, that's an important consideration. We haven't gotten far enough down this peer review process to really know what structure it's going to take, other than a couple of these principles that we want to try to follow. But this is something that we hope to initiate in the spring. And I think when we come back to talk to you about the NUREG, we'll have some more information at that time about the peer review process. And certainly if you'd like to weigh in before that, we'd certainly welcome it.

Summary. So just quickly, I'll just be pretty quick here. We've covered most of this.

We used formal elicitation to estimate generic BWR and PWR LOCA frequencies as a function of both flow rate and operating time considering both piping and nonpiping contributions.

We developed quantitative estimates --

DR. WALLIS: I think it's not really a function of flow rate. Really it's a function of hole size.

MR. TREGONING: Okay. Break size, which we correlated back to flow rate.

DR. WALLIS: Well, isn't that really what the experts considered?

MR. TREGONING: Yes.

DR. WALLIS: They didn't sort of do their thermal hydraulic calculations.

MR. TREGONING: Right, we correlated it back to flow rate.

DR. WALLIS: An gallons per minute of steam is a very strange measure, as I've said before.

MR. TREGONING: Yes.

DR. KRESS: Well, you have to have a special bucket --

MR. TREGONING: You'll get no argument from me on that. Although, I will say the way it evolved, we defined as a group the LOCA categories based on flow rate. That's how they were defined. And then we did the correlation for the pipe size later. So we did the initial definition based on flow rate. And we got some input from Westinghouse on that because they said, you know, when you parse the large break, you know, look at this partitioning because that will determine the different mitigated capabilities that need to be brought to bear.

So we actually did initially partition with respect to flow rate. Then once we got the correlations, all the estimates were based on those break sizes. There's no argument there.

DR. KRESS: The trouble with break size, a peak is a given size in a BWR will give you a different flow rate than a given break size in a PWR. I like flow rate better, frankly.

MR. TREGONING: Not only break size, but break location and all those things will reflect your flow rate, there's no doubt.

One of the things that I think are nice about these is if you develop them versus pipe break, if you can make an argument that you've got a better correlation for a break in a certain system and location that's applicable there. So, for instance, this might consider that as a small break LOCA when in reality maybe it's a medium break or the other way. Maybe it's not even a small LOCA.

I think you have the opportunity to make those sort of evaluations given these numbers.

I think that's why what we did will have maybe some more use downstream as people continue to estimate and look at the effects that these breaks may have on plant system response.

We developed quantitative estimates for these base cases that we used as anchoring the elicitation responses.

We asked the panelists not only for quantitative estimates, but we wanted to support it by qualitative rationale. And as a group we determined these important contributing factors. And what are they? Well, they're piping and nonpiping systems, degradation mechanisms, things which govern LOCA frequencies. And then we asked the experts to provide the relationship between these factors and the base case. So that's where they earn their money.

In terms of the results, we got relatively good agreement about the important contributing factors within the community of panelists. So there was fairly good consensus on what things were important. There was, obviously, much more disagreement uncertainty and variability in quantifying the frequencies associated with those various issues. And that will certainly be the challenge that people face.

At the end of the day we got results, again, maybe serendipitiously, but they ended up that they were generally comparable to some of the earlier estimates that we got not only in 5750 but they weren't too far from the pilot elicitation estimates that we got.

And that was all I had. If there's any

further questions?

DR. WALLIS: How long does it take before the NUREG is actually finished and out there and can be used?

DR. SIEBER: As long as you want it to be.

MR. TREGONING: I'll tell you that the schedule that we have now, we're looking to finish up our sensitivity analyses by the end of April. And we're looking to have the draft NUREG completed by about end of any time frame. And the only thing at that point that we'll -- and then we'll have a public -- I'll say an internal comment period within just our working group and then also the panelists.

DR. WALLIS: And a peer is going on then--

MR. TREGONING: The peer review will be going on simultaneously. But I think the plan is end of June time frame we'll have something that will be ready for consideration by this panel. That's the hope. Assuming we can get buy-in from the experts. That's the unknown at this point. You know, if the NUREG is so contentious that I have a number of experts that just won't buy off on it, then I've got a decision to make. We've got a decision to make whether we move ahead with it or not. So that's my big unknown at this point.

I don't think that that's going to be an issue, but I don't want to --

DR. SIEBER: But that's an important piece of information --

MR. TREGONING: Yes. Of course.

DR. SIEBER: -- should it come out.

MR. TREGONING: And if it does come out, then I think what we would have to do, we'd try to encapsulate that in the NUREG somehow.

DR. SIEBER: Yes.

MR. ABRAMSON: I think also the theory here, too, I mean you know we certainly would want to take account of -- consider any feedback we're going to get from the peer review. If they have some problems with it, then we may have to --

MR. TREGONING: Right. But I think we want to make the NUREG available, at least a draft NUREG available for consumption before that peer review process is completed. That would be my opinion. Because the peer review is going to take a little bit of time. And we want to get these results documented and out to people like the ACRS, and maybe even in advance of that.

And if the peer review comes back and it's particularly detrimental to the effort, then --

CHAIRMAN SHACK: Well, I had the impression that you were going that you were going to issue a draft new reg for public comment and, presumably then the draft, the final new reg would incorporate both the peer review and any public comment that you got. Is that the process you have in mind?

MR. TREGONING: Essentially, yes.

DR. KRESS: What's the purpose of public comments on something like this? I just don't see it. I mean, I can see the value of a peer review. But, you know, you have public comments on particular rules when you get around to making the rule, which would incorporate this stuff. I don't see -- you know, I don't see the value added of going out for public comment for a NUREG like this.

DR. SIEBER: Well, part of the public are the vendors and the licensees. And that's where the comments will probably come from.

MR. SNODDERLY: And if they don't buy-in to these frequencies, they may not be as willing to participate in the --

MR. TREGONING: But there's also precedent within the NRC. I think it's the package performance study and things like that which have potentially sensitive issues that have followed a similar path. And I think that this is of the same level of importance that I think that's going to be a prerequisite.

DR. SIEBER: Let me ask a real quick question that's, perhaps, frivolous. But if you were to task the category flow rates in terms of mass flow rate instead of volumetric flow rate, would that really distort things? You know, because the mass flow rate is from a thermal hydraulic sense and a mitigating system performance --

MR. TREGONING: Yes, did you want to say something?

CHAIRMAN SHACK: Yes, I mean it is a mass flow rate. You just kind of them put in a funny unit.

DR. SIEBER: Well --

MR. TREGONING: It's calculated as a mass flow rate. It's converted to a volumetric flow rate.

DR. SIEBER: Yes. So you could actually go back the other way and not hurt anything.

MR. TREGONING: Well, again, and one of the things I like -- one of the things I like about our results is the results aren't a function of that correlation. If you don't like the correlation, you can run your own calculations to determine -- and again, I don't have to tell you folks, but it's simplified, it's usually not applicable. I mean, we know that especially for large breaks we have an evolution of flow rate through the break size as a function of pressure and everything else. So these things aren't constant by any stretch of the imagination.

So, you know, it was just something hat we did to give us a link between the definition of a LOCA size and the pipe size.

DR. SIEBER: Right.

MR. TREGONING: And that's something that, you know, that I think in the future if people want to evaluate the acceptability of that, they'll be easily able to do.

The one thing that I found actually troubling, and we talked a little bit about this, when I looked back historically and tried to find the basis for the correlations that we had been using since really, about the time the NUREG 1150, very scant basis at all. And especially for the Ps. At least the BWRs I was able to find some documented results. But they were really based on GE neode studies where for certain plant types they postulated breaks in certain locations and assumed certain mitigating systems were applicable. Ran the best estimate codes of the time and made a prediction as to what the flow rate would be. And then did a number of these things and essentially picked a correlation based on that.

So it's -- you know, I would argue that what was done in the past was no better than this in terms of correlations, and maybe even weaker technically. Especially given the evolution of the codes over the last 20 years.

DR. WALLIS: Well, I think the idea is though if you cover all sizes from smallest up to the double ended break, that sort of the uncertainty in the codes gets washed out except perhaps at the very highest end.

MR. TREGONING: Right. Right. No. I'm only talking about the codes with respect to the correlations to break size.

DR. WALLIS: Yes. I know what you mean. I mean, usually we say that -- you know, no one has actually broken up pipe under these circumstances and measured flow out of it. So it's all based on ideal--

MR. TREGONING: Not in a plant, anyway. Not in an actual plant. Try to do it on scaled experiments and things like that.

DR. WALLIS: But even then they tend to

just take a straight pipe with an orifice at the end at something.

MR. TREGONING: Yes. Yes.

DR. WALLIS: They don't get a real break in a real pipe.

MR. TREGONING: Exactly right. So we realize that we're on shaky ground with whatever we try to develop here. That's why we tried to keep simplistic for this analysis. Stay simplistic.

MR. SNODDERLY: Bill, I just want to take an opportunity to thank Rob and Lee. They've both been outstanding in their support of this Committee on this issue, in keeping me up to speed and getting me the information that I needed to try to bring this meeting together. So I just wanted to thank them.

CHAIRMAN SHACK: If there are no further questions for Rob and Lee, then I think we can thank them. It was a superb presentation I thought. And, actually, you know all things considered, pretty close to on schedule.

DR. SIEBER: It depends on the schedule you're on.

CHAIRMAN SHACK: At this point, you know, although Mr. Kelly this morning said that staff wasn't expecting a letter, I think at least we want to consider a letter, you know, with some of our positions on some of the technical and policy issues and perhaps what we know about the elicitation process if we want to comment on that.

So, at this point I'd just like to go around the table to see if people, you know, have issue they think we ought to be addressing in a letter or, you know, opinions on where 5046 is going, some of the technical and policy issues that we heard about this morning.

MR. SNODDERLY: And one other thing, though, we should consider is we have a 2 hours and 15 minute schedule for April 15th to brief the full Committee. So if we could give Rob and Eileen some idea of what we want them to present. And also if we think it might be a good idea -- well, we'll definitely invite NEI to say something, but we need to figure out what we want to tell the full Committee in that 2 hours and 15 minutes.

DR. SIEBER: Well, I guess I'm not prepared to address the latter point that you made. But as far as would we send a letter at this point, I personally don't have any issues with what's been presented today. You know, I have some concerns that are broader in scope, but until we discuss those and examine those issues thoroughly, you know, I would not want to comment on it.

So, I think what was done to get to this point in the expert elicitation was done very well. And I think the results are reasonable, and I look forward to reading the NUREG and any comments coming out of the peer review.

CHAIRMAN SHACK: The comments on the broader issues, for example, of narrow scope versus broad scope application or, you know, this question about what kind of mitigation should be kept in mind for beyond design basis?

DR. SIEBER: When we look at risk-based relaxation of requirements, I would prefer that the hardware part of the plant stay the same and meet the same criteria as the original Appendix K which had the assumption of the double ended guillotine break. Now, when you change things like diesel start times or allowed outage times and so forth, I think that is within the realm of being reasonable. But if you carve out a class of accidents that you can't mitigate because you decide, well, you know, my high head safety injection pumps really don't pump very good anymore and so you can't really deal with a double ended break, I would prefer the licensee fixed his pumps rather than allow or take up margin that way.

And so I would look for the softer the issues, the reversible issues to be able to give some relaxation on.

The other thing that concerns me is maintaining the independence of barriers. For example, even though you say that the most likely break size is smaller than the double ended break of the large pipe, the containment should still in my opinion be capable of taking the full pressure that you would have with a double ended break, the environmental envelope should be the same, the zone implements for debris generation should be the same. And those are not areas where you would, in my opinion where I would feel comfortable in granting relief or saying, for example, I don't need to modify my screens because I'm not going to blow all that stuff around in containment because I'm going to have little breaks instead of big ones.

And so that's sort of my feeling on where 50.46 ought to go. There ought to be some separation and independence between the characteristics, the design and engineering characteristics of the barriers so that we don't make a decision in mitigation space that degrades defense-in-depth as we go through.

Of course, I'm willing to listen to other people's arguments in that area, but that would be my feeling, if I had to express it right now. And since you asked me, I guess I have to express it.

MR. SNODDERLY: Jack, as far as the latter question that we asked, do you think we just discuss the 04-0037 SECY at the April 15th meeting or should we discuss both that SECY and the LOCA frequency distribution work?

DR. SIEBER: Well, I think the most important product right now that's reaching a culmination is the LOCA frequency distribution. To me, I think that's the item of most interest.

On the other hand, we aren't done yet. You know, the NUREG has to be published, there's additional statistical work that you want to do. And so I'm not sure that that's appropriate.

DR. BONACA: Well, on May 1st, or the May meeting we have a meeting with the Commission. We are on the agenda. I would expect that they would want to have our views on policy issues. Because, I mean, on this issue at the level of the Commissioners we can comment as to the quality of the work and the value that we attribute to that. And I think that would be probably just part of what they expect to hear from us. So I think we should be prepared to discuss the other issues, too. Don't you think so?

CHAIRMAN SHACK: Yes. But I do think we probably also want to have -- I think we do want to have both present at the Committee meeting.

MR. SNODDERLY: Yes, I agree.

DR. BONACA: I agree.

DR. KRESS: But in terms of relative times.

MR. SNODDERLY: That's what I'm trying to get out.

DR. KRESS: You want to put a lot more on the NUREG -- what's the number again?

MR. SNODDERLY: SECY-04-0037.

DR. KRESS: Yes. Because I think there will be a lot more contentions and a lot more problems.

I don't think the Committee will have any particular problems with this.

MR. SNODDERLY: I agree with that.

CHAIRMAN SHACK: I think this afternoon's presentation could be just summarized as to the results, a couple of those key slides that we saw. I think the methodology and the --

DR. KRESS: Yes, and the Committee's

already heard a lot leading up to this, like Rob says. So they're very familiar with what is going on.

MR. TREGONING: Sort of pick ten slides out of here, maybe even that's too many.

DR. KRESS: Yes. Maybe.

DR. SIEBER: Have some backups.

DR. BONACA: I would almost say, you know, half an hour for this and two hours for the other.

DR. KRESS: That would be my guess as reasonable.

DR. BONACA: As the breakdown, I would say. Because that's really where the hard spots are. And that's where we, hopefully, can influence.

MR. TREGONING: I'm sorry. You said a half hour or an hour for this?

DR. BONACA: A half hour.

MR. TREGONING: Okay.

DR. BONACA: It seems very short. But the issues that we discussed that we discussed this morning are going to be contentious and I think there's going to be a lot of questions.

MR. TREGONING: Right. I just wanted to make sure so I can prepare toward it. And I think ten slides will be too much. But, okay.

CHAIRMAN SHACK: Well, that's 2 minutes

per slide.

DR. BONACA: What a sign of success. That's a sign of success, I believe. I mean, in part, I don't think there will be any arguing about. Anyway--

CHAIRMAN SHACK: But no more than ten slides I guess is the answer. You've been known to come in with packages, Rob.

MR. TREGONING: I don't want to get -- it's probably not an unfounded --

DR. SIEBER: It should be greater than one and less than ten.

MR. TREGONING: I will be careful.

DR. KRESS: Very good.

CHAIRMAN SHACK: Peter?

DR. FORD: As far as letter, we just discuss that.

As far as the LOCA frequency, I think it's great work, as we have all come to that conclusion. Obviously, there's debate on some specifics. I'll send a note to you on four specifics.

One is a question of uncertainties, the physical aspects of the uncertainties.

A question of calibration of the predictions against historical evidence.

The third item is that I would recommend that we do some plant specific calculations, for instance, water chemistry for BWRs and temperature, things for the PWRs. The reason why I say it is, is that gives the business driver for the licensees to use this methodology.

The final one is that I still think we should be concentrating on the upper end, the 95 plus aspects because we're a bit concerned about the first incidence is going to kill us, not the mean.

But I'll send a note separate expanding on those ideas. That's it.

DR. LEITCH: I would like to differentiate between this morning's presentation and this afternoon's presentation pretty clearly.

I think this afternoon's presentation is an excellent piece of technical work. You know, I think it's as good as can be. I think it's been accomplished very professionally. We're dealing with great unknowns, great uncertainties here, but I don't know a better way to go about it. I think it's been done very well.

And as, as I've already said, I think the next full presentation to the full committee I think this can be greatly condensed just to show the results and not get into too much of the methodology. I think some of the insights, a couple of those slides on insights might be important and the basic results I think is about all we need.

I think by far the bigger issue, though, comes in the discussion we heard this morning concerning the revision to 50.46. And I would say that I have a great deal of problems with that. First of all the application of this afternoon's conclusions to 50.46. But I also have a problem with this whole concept of narrow versus broad application.

I think on one hand I'm very much concerned that the broad application is too much of a relaxation and the narrow may not give sufficient benefit for the utility to want to invest the time and money in the PRA that would be required. So I basically don't know how that's going to work. And I have a great deal of concerns about it.

Some of the other concerns are the -- I don't have a clear understanding in my mind at the moment as to if we do revise the maximum break size, just what are those systems going to look like that are designed to mitigate between the maximum break size and the DBA or the double ended break of the largest pipe. All the hardware still in place?

DR. SIEBER: Maybe.

DR. LEITCH: But maybe. And how would that hardware be maintained? Would it still be in the tech specs? Would there still be surveillance tests required for that? Still be quality assurance of the environmental qualification? I just don't know the answer to those questions. I don't know what's being proposed. But I think we do need to hear the answer those, because all those things have an impact on the reliability of that equipment. I mean, it's one thing have a core spray pump sitting there, but if you never test it, if you don't check the logic and so forth, how do you know that it's going to work when you need it? By the same token, if you continue testing it, in what way is that different than what we have today?

So I just don't have a clear understanding of what's being proposed. In fact, I guess that's the essence of the discussion here, is we're looking for policy direction as to what is being proposed.

I also have a concern about terrorism and security in this that I've expressed before as it relates to public confidence. I really have a concern about whether this is the time that we should be moving to redefine break sizes. I think some of these potential for terrorist attack, although we don't know what those potentials are and can't quantify them, I think they are probably higher than some of these break number probabilities that we've seen here. Not only external terrorist attack, but the concept of a disgruntled employee either because of labor relations situation or perhaps even an internal terrorist I think can do significant damage.

So I'm just really -- this afternoon; that's fine, that's good, I understand that. But I'm really concerned about what I heard this morning or maybe more importantly, what I didn't hear. I mean, there's the uncertainties that are still on the table, I think are big concerns.

CHAIRMAN SHACK: Well, of course, those are policy issues that do have to be addressed. And we sort of could take a position on just how some of these could go.

DR. LEITCH: Yes. Yes. And I think we could -- you know, if we wanted to frame a letter, we could develop some thoughts about just what are the level of readiness, so to speak, of these systems that mitigate the delta between the new LOCA and the current LOCA.

DR. BONACA: Yes. I share some of the views.

First of all, on the issue of the development of LOCA frequency, I think is a very good effort. But I expressed before the concern I have with the really human factor, as they were, and I think that's really the issue.

To the point where I would say that I see this estimation as very valuable and useable, but my level of uncertainty about those is much higher because of some considerations like human factor, sabotage or terrorists or things like that have not been taken into consideration. And yet when I go to develop a regulatory basis, I have to take into consideration in my mind these are the factors in some way.

And so in a way that pushes me is to what is a narrow rule rather than a broad rule. And now I'm talking about this morning's presentation. You know, in a narrow rule I would see relaxations that are favorable to the licensees, for example in diesel start times and other -- many of those things which are really a pain to the licensees right now, but they're not used to then get power upgrades and so on and so forth, and so therefore to -- so I would view a narrow rule in that sense, and I think that's the way it was presented this morning.

Now, whichever way we go, narrow versus broad rule, I do believe that there had to be mitigated capability for beyond design basis LOCA. They should be retained at some level. I mean, there had to be some assurance that you have no low vessel failure, no containment challenges that will cause more likely large releases, early releases. And I think one way to address it is to focus on the criteria Reg. Guide 1.174.

What I mean by that is that if I could show that I make changes there, whatever changes I make, that will increase risk by a very small amount, that assures me that also the scenarios beyond design basis which are modeled in the PRA are contained to no risk. So that would be one way, it seems to me, that I could verify. So in that sense that would be for an application that still expects a very small increasing risk.

I mean, I don't think that the burden reduction should be interpreted as you're taking in the margin you got and you can do whatever you want with that. It seems to me that burden reduction means you're reducing the burden, but you're still contained to a small increasing risk, because that's really what we license there. That's where we are today with the risk appreciation for these power plants. And I think that's the baseline where we should stay. And that's my view.

On the reversibility issue, I think that the reversibility issue should not be subjected to -- analysis. I think that this is almost like an agreement that the staff develops into a contract, I mean into the regulation whereby in fact if these estimations are used and then there are changes that are agreed to -- I mean, are coming in, that show that the change was not appropriate, there shouldn't now be a burden on the staff to demonstrate robust -- analysis that the reversal can be done. It doesn't make sense to me there should be one.

I think we should establish criteria for what it means that you would reverse. You know, you reverse by what? Some insights and then maybe you can translate it into medians or means or, you know, percentiles.

On the best estimate evaluation methods, I really don't have a judgment. It seems to me more of a concern of the staff with the fact that right now there are small break LOCA models that do not allow you best estimates, and that would be a burden on the licensees. But I -- I think still if you go a risk-informed way, I mean, also depending how much you relay on that, you would want to have best estimate methods even for LOCA, even for small break LOCA. And then may not have to have an Appendix K pedigree. But something that certainly supports best estimate for LOCAs -- for PRAs.

I think application to future plants it's a little bit far in the future it seems to me now. I think we have to resolve this issue for the existing plants. That will give us some insight on where we go. But I don't have an idea about that.

And finally, on defense-in-depth, again, I mean if you contain the risk increases through criteria such as the one on Reg. Guide 1.174 and you say that they're going to be very small, that should resolve some of the concern about defense-in-depth. It doesn't resolve still the concern with the proper balance between prevention and mitigation, maybe. And so -- but some of those criteria, prevention and mitigation, human factors and common cause has to be dealt with. I mean, those are issues that are important. And I think that maybe there have to be some specific consideration on how the defense-in-depth is applied there.

That's pretty much my thoughts.

CHAIRMAN SHACK: Suppose we pick the ten to the minus six frequency cut off.

DR. BONACA: Yes.

CHAIRMAN SHACK: By definition, you know, the risk associated with ten to the minus eight accidents would then be small, would you want a mitigative capability for those ten to the minus eight accidents or you're willing to live with the fact that the risk is small and you don't need a mitigative capability?

DR. BONACA: That's a good question. There is a certain point where you have a cut off point. And I think the one on the main issue is what uncertainty I have on those results.

CHAIRMAN SHACK: Okay.

DR. KRESS: With respect to this afternoon's presentation, I agree with most of the comments. There was not much to complain about. It's a very good presentation. And I don't know how it's needed information --

MR. TREGONING: We could work on something.

DR. KRESS: Yes. I don't know how else you could get this information.

I do think there's an issue about how you glomerate the results to get a final single distribution. And I reiterate my comments that they ought to check what they did in NUREG 1150 there.

And I have some doubts about the value of peer review in here, but it looks like it's one of the things you do.

What I would do with the results of peer review is not go back and redo the elicitation. I would try to figure out how to adjust the results of the elicitation I have based on the peer review comments. But that's just a comment.

With respect to this morning's stuff, that's where I think the meat of the thing is. I believe we have such diverse views and sort of a conundrum as a result of the fact that we have never articulated a good connection between design basis base and risk base. And that articulation needs to be done.

The question is how do you choose design basis base and why. Well, the philosophy is really that you look at all the types of accidents you have and you try to include those types. And you do it in a conservative way with things like the single failure criteria and how you calculate and what you have to meet in terms of acceptance criteria. And you hope then that this renders the plant in a state of acceptable risk, acceptable uncertainty, acceptable balance; all those things that you're really after.

We've never articulated that, and what we're now in the process of doing has said we have a design basis space for the plants that exist. We're talking about changing that. But what we're really after is controlling the risk, controlling the uncertainty, controlling the balance to acceptable levels. There again, we've never articulated what those acceptable levels is. What's a quantitative acceptable level of defense-in-depth?

I recall a letter that ACRS wrote about 7 or 8 years ago saying these things need to be quantified, they need to be articulated better, they need to be applied on a plant specific basis. We got kicked in the teeth and shot down I don't know how many times for that.

I still think those are absolutely necessary things to make this change. They have to be articulated somewhere, and they're not. They are in a sort of an ad hoc manner now in Reg. Guide 1.174.

So if I were doing this, I would grab hold of 1.174 with both arms and I would pull it into this thing and say this is the thing that's going to control how I deal with this issue, because it's going to limit risk increases to acceptable errors, it's going to track them in a accumulative basis, it's got already balance between LERF and CDF. It's got in it the things we need to control this.

So I would certainly never throw 1.174 out. I would grab onto it with both hands.

DR. BONACA: And deal with design basis --

DR. KRESS: That's right. And with respect to the question of maintaining mitigated capability, I think if we're going to go risk-informed, we ought to go risk-informed.

CHAIRMAN SHACK: There's only one kind of risk?

DR. KRESS: That's right. Well, there's balance and defense-in-depth and those things have to be properly accounted for. But let's go risk-informed. And otherwise you're using this kind of stuff to decide on what stays and what doesn't.

With respect to the terror issue, once again I say keep that separate. Let them deal with that some other way.

And with respect to the new plants, I think I would just say okay, we'll relegate that to the new framework for technology neutral and let that take care of it for us, maybe. I don't know.

CHAIRMAN SHACK: Do you have any thoughts on cumulative risk and how much it can accumulate?

DR. KRESS: Yes, let Reg. Guide 1.174 do it for you.

CHAIRMAN SHACK: Okay. That works.

DR. KRESS: I was looking to see if I had any other notes. Ah, that ought to be enough for now.

DR. BONACA: Reversibility is an important issue.

DR. KRESS: Well, I think reversibility shouldn't be an issue at all. I think if you do the Reg. Guide. 1.174 you will have limited risk due to the change cumulative. And if once some new information comes about that says you've gone beyond an acceptable risk, then the back fit rule will be there for you and you can say, okay, put something back in there to fix this. And it'll pass the back fit rule. If it's still an acceptable risk, it won't pass the back fit rule and you can't do it.

CHAIRMAN SHACK: I think it'll be more like the pressure vessel; that you will then have an aging management program that will maintain a LOCA frequency to --

DR. KRESS: You may have that, yes.

CHAIRMAN SHACK: -- to a certain level.

DR. KRESS: Yes. And I think the business of CDF and LERF will take care of mitigation capability for both the containment and the core for you properly. So, you know, that's my opinion right now.

CHAIRMAN SHACK: Vic?

DR. RANSOM: Well, I think the elicitation work, it's certainly a good start and like a piece of the puzzle which is the break sizes -- or the probability of the break is a function of size.

The thing that puzzles me in 50.46 is the benefits are not clear. I'm not seeing what is coming out of this. Maybe I haven't been around long enough to understand completely why eliminate the large break LOCAs from the design basis accident. I guess I'd like to hear a little more what are we gaining by doing that.

It's not clear what the safety implications are if you do this and what it means in terms of defense-in-depth, which is sort of a vague term anyway.

And in terms of risk informing the methods, I believer the uncertainty of consequences predicted by system simulation are still one of the biggest uncertainties in this whole puzzle. And the best estimate methods are quoted without really quantifying what that is. I don't know that anybody has really -- I think the methods exist for establishing that, but we've not established that.

And that's still, I think, a strong piece of the puzzle and there needs to be more effort in that. And not only that, the NRC seems to have backed off in the past 20 or 30 years of being able to provide a measure of what is best estimate. And the words I'm hearing, this is more up to the licensee to prove or provide what that is. And I think we've heard of statistical methods recently that could be used to quantify these terms.

I personally believe that a better approach is to treat to the break size as a statistical variable, like we heard in the S-RELAP5 presentation from Framatome where the probability of this break is simply incorporate into the other sources of uncertainty that exist in predicting the consequences of an event. And to me that seems like a stronger way of doing this, and it's consistent with risk informing completely.

I guess as a last thought, is what happened ALARA. You know, a lot of this defense-in-depth in ALRA were philosophies that you did whatever was reasonably achievable in terms of reducing risk and throwing out a part of the history of this is somewhat disturbing to me because I don't really know what I'm gaining by doing that.

CHAIRMAN SHACK: Graham?

DR. WALLIS: Well, the expert elicitation work I think is very good. It's near completion. I don't think we need to say much more than that.

The big picture is very interesting. I think the staff did a very good job this morning of describing the issues and things that needed to be considered and in some ways indicating how they might be treated. But this agency's going to have to, I think, reexamine some fundamentals of how it regulates and why it regulates. I don't understand how you take something out of a design basis accident and yet you sort of require mitigation as if you were in the design basis. That's a sort of mysterious thing there. That's why I think that they are, the agency has to examine why do we have design basis accidents and what are we trying to achieve and is mitigation and risk the only measure of what we're trying to achieve. If it is, then let's use it.

But, you know, I'm sort of waiting for the agency to decide because I think an awful lot of this is going to be at the policy level.

I think that the agency will have to be more explicit about what defense-in-depth means, as my colleague Dr. Kress says. Give more specific descriptions. Even dare to try to quantify it.

I'm not sure that 1.174 is adequate.

DR. KRESS: No, that's the one part of 1.174 I'd say needs augmenting.

DR. WALLIS: Right. It's too waffly. It doesn't really say what is adequate defense-in-depth and how you decide.

In two specific areas or one specific area, I do have this concern with human actions. I think deliberate or accident human actions could well have far more influence than these ten to the minus eight ten to the minus 9th material events that we've been discussing.

That's the --

CHAIRMAN SHACK: Although to the extent that the service related things include things like human events --

DR. WALLIS: They can do that.

CHAIRMAN SHACK: -- you know, an obviously the odds of having included a human event on the ten to the minus eight scale is about the same as a material event on the ten to the minus eight scale --

DR. WALLIS: But you never know what humans are going to get up to. I would be very --

CHAIRMAN SHACK: No. I's just saying that experience base may not include all the human actions that we have to be concerned about.

DR. WALLIS: And this whole society may change in the ten years between now and the next time when you want to reevaluate all this.

I suspect that an awful lot of this is going to be decided at the policy level. That someone's going to make some policy maybe not having considered all these things that need to be considered, how much do we want to get involved in that?

I think some of the main issues -- the big issues that my colleagues have been talking about here really represent policy. And sometimes the Commission hasn't been receptive to us getting involved in policy.

CHAIRMAN SHACK: That's a question, yes.

DR. WALLIS: Although I think this is one of those things where as representatives of the public, we may want to get involved. Again, I'm waiting to see how that plays out.

DR. KRESS: Interesting comment. You got any? We want to hear your views. You can't get out that easy as far as the Chairman.

CHAIRMAN SHACK: I have to write the letter. So you're going to get my views.

DR. KRESS: We'll get yours. Okay.

DR. SIEBER: That's one of the advantages of being chairman.

DR. BONACA: You know, policy or not, I mean set of issues that use of Reg. Guide 1.174 as a guidance solves a lot of these issues. You think about beyond design basis, within design basis, we change the envelop -- but the PRA fits both. There are distinctions. So therefore, you know, it will address if you have a quality PRA and you have good --

CHAIRMAN SHACK: No. But I'm more sympathetic to this notion of having a mitigated capability for things beyond design basis as my method of quantifying of defense-in-depth. That's what I mean by quantifying defense-in-depth.

DR. KRESS: I think some of that ought to be part of the defense-in-depth, and I agree with you. How you do --

CHAIRMAN SHACK: -- what defense-in-depth means, I think in this case that could well be what I mean by --

DR. KRESS: It could well mean that you, regardless of the risk results, you have some sort of mitigating capability that's redundant and diverse and has certain capabilities. I think that could be --

DR. WALLIS: That's why the large break LOCA is in here, in the design basis.

DR. KRESS: What?

DR. WALLIS: For this very reason is defense-in-depth.

DR. BONACA: -- that it is much more less stringent than 2200 degree --

CHAIRMAN SHACK: Yes, I think there's a great deal of difference between a mitigative capability and a design basis accident.

DR. SIEBER: On the other hand, when you choose a design basis it's unlikely for the accidents that are likely that provides margin and robustness, the same as defense-in-depth. The problem is for the last 45 years we have not sufficiently quantified what kind of margin there is or ever said you got to have this much margin, you know. And so we talk about it and say that we have it, but nobody really knows what they're talking about.

DR. WALLIS: Well, isn't the ECCS there simply for mitigation?

DR. SIEBER: Yes.

DR. WALLIS: And it's there to maintain a coolable geometry and to prevent the accident getting out of hand. That is mitigation, isn't it? So how do you take --

CHAIRMAN SHACK: Yes, but it mitigates to an extreme --

DR. WALLIS: Well, the degree of mitigation maybe. That's where you can start.

DR. KRESS: You have to recognize that we don't mitigate all accidents anyway. We do have core melt accidents in PRA, they're the problem. So we don't mitigate all of them.

DR. WALLIS: We mitigate the LOCA because there was a great folderol about LOCA and hearings and it went on for a couple of years.

DR. KRESS: There was a history behind it, that's right.

DR. BONACA: -- had the best estimate done after they were meeting Appendix K to from, you know, like keeping temperatures so low, especially BWRS, that you have no damage at all, you know. So therefore there's no margin there in the criteria that right now --

DR. KRESS: Even the rules that we have now where we talk about ESSC having some redundancy and some capability don't specify the reliability. So you could have two systems that aren't very reliable at all and have a bad ESSC. I mean, so you know, I think some of those things need to be specified better in terms of defense-in-depth if we're going to change the design basis space.

DR. LEITCH: I think there's a good example and a distinction between a system that's operable and a system that's available. I mean, operable is a tech spec word and in order for this hypothetical core spray pump to be operable it has to meet all those requirements. Now, if you seismic support out someplace on the system whose pedigree is questionable, the system's not operable.

DR. SIEBER: It's not operable.

DR. LEITCH: But is it available? Well, sure it's available. I mean, it'll run. It'll run just fine. But it's not operable and from a tech spec sense you can't count it's operable. And it's all those little kind of things that drive the power plants crazy.

And I think there's room for relaxation of some of that stuff and still have this theoretical pump available without having all those "i's" dotted and "t's" crossed. But then the question is how far one goes in that direction and still has a confidence that it still is available.

DR. KRESS: Well, you know, in spite of all the things we keep saying and keep assuring, I think when we do this change, I personally think we're going to increase risk and let's bite the bullet and say that is the nature of risk informing some of this stuff. We're going to increase risk and reduce burden. I think that's almost a given to me. The question is how much is a risk can go to be increased. And I think 1.174 has already given those limits.

CHAIRMAN SHACK: Yes. I don't think changing our focus, you know, all of the things will not remain equal. You know, we then introduce changes that will increase risk.

DR. KRESS: Right. That's my opinion, yes.

CHAIRMAN SHACK: Does the staff have any last comments they'd like to make?

DR. KRESS: Oh, is the staff here? I might have been a little more -- if I had known it.

MR. KELLY: This is Glenn Kelly from the staff.

If I gave you the impression this morning that it was not our intention to follow the Reg. Guide 1.174 process, then I -- then if I gave you that impression, then I made an error in how I spoke about it.

I think we tried to write in the paper that it's our intention to follow Reg. Guide 1.174. We think generally it does provide exactly the type of process that we want to be using. And it probably uses exactly the type of metrics that we want to be looking at, and possibly as we've said, we've talked a lot internally about the need to consider metrics leg containment failure because we have some concerns about how well that's covered if you're only looking at core damage frequency.

DR. KRESS: I definitely would have that as a metrics --

MR. KELLY: And you brought up some interesting things for us to think about should -- you know, if it's okay to make these changes in ten to the minus five change and core damage frequency for licensing basis change, why isn't that okay for a change in the regulations? And I think part of it comes -- there are so many things that are inner related and changes in the large LOCA design basis makes such a fundamental change in so many areas of the plant. This whole issue is so unbounded at this point that we were not easily prepared to say that a ten to the minus five increase was okay. Because number one, it wasn't clear that I could come in this week for a ten to the minus five increase --

DR. KRESS: And come in next month with the ten to the minus five.

MR. KELLY: But there's really nothing even in Reg. Guide 1.147 that limits you ten to the minus to four. I mean, potentially you could just -- all it says is, you know, you're going to get extra attention. Well, maybe that means that you're not--

DR. KRESS: Well, we asked Gary Holihand that one time. And he said if you got it in a certain range, you would be putting into question adequate protection -- a certain level. And he didn't want to say what that level was.

MR. KELLY: That number's pretty high, though. That number is really a pretty high number. And I think I would have thought he would feel probably uncomfortable for a --

DR. KRESS: He didn't say what that number was, but he said that would be the implication. He would putting into question adequate protection.

MR. KELLY: Right. I mean, we had talked internally about being uncomfortable about taking a plant from ten to the minus six and bringing it up to ten to the minus four or even higher. And that's a big policy decision. And that's one of the things that we intend on bringing forward with a paper, with these numbers on saying here's what we think -- because I think it has to go together as a package. You have to say that, you know, I can't just take this one number and go forward.

DR. KRESS: You know, those ten to the minus sixes generally are BWRS.

MR. KELLY: Right.

DR. KRESS: Your LERF is going to protect you there because they got such a high conditional containment failure.

MR. KELLY: Right.

DR. KRESS: The higher CDFs are generally PWRs, but they've got the LEF protected by their containment.

So I think in principle you're probably pretty well protected from those potentialities. But you may have to look at it.

MR. KELLY: We certainly hope so. And as you say, we have to look at it.

The other area and the last thing is -- see if I can still pull it out here after my brain's sitting here for the whole day.

I think that in looking at the overall package -- give me one second here to hold my thoughts back together.

DR. KRESS: Take your time.

MR. KELLY: Yes. Well, if I think before I sit down --

DR. KRESS: I have that problem, but I'm older than you are.

DR. RANSOM: I have just a couple of comments with regard to removal of the large break LOCAs a design basis accident. It seems like we know more about that accident from research in the past and can predict its course more reliability than even a small break LOCA. There are probably more unresolved issues in small break than there are in large break.

And the second one is the advance light water reactors are turning are large break as a means of mitigating the accident. So I'm not sure what is being gained, again, by eliminating --

DR. KRESS: You're eliminating a lot of burden. They can make a lot of hay out there. You're giving them some flexibility that they think they need.

DR. WALLIS: The advanced reactors create the large break at the right time under the right conditions.

DR. KRESS: Absolutely.

DR. WALLIS: They don't just let it happen.

DR. KRESS: It's not a random event. And in fact --

CHAIRMAN SHACK: We got to a lot of trouble to make sure it doesn't happen at the wrong time.

DR. KRESS: That's right.

DR. SIEBER: You know only -- once --

DR. WALLIS: And also you need it in order to let gravity do the work. You have to depressurize the system, which isn't the case with the other reactors. It's an interesting point, but it's good for this and bad for that.

MR. KELLY: The other point that I wanted to make was the industry had shared with us a white paper that they had prepared on this process that they would propose for risk information 50.46. And in that process they included many of the concepts that we had put forward in our paper to our Commission in discussing the various areas. But the important thing I think there was that industry had indicated that they expected that in essence, where we would go with 50.46 would constitute a precedent for a process of how the industry would like to risk-inform the rest of Part 50. And therefore, not only have we been concerned with the implications that this process would have directly on changing of large break LOCA, but potentially changing all of the other design basis accidents, but changing anything from code acceptance to whatever it is that you might look and it's covered in Part 50.

And so therefore, also when we were talking cumulative risk, are we talking about cumulative risk associated with only the change to Part 50 50.46 or is it all the other changes that might be proposed under a similar process?

And so we've tried to keep this in the back of our minds as we've looked at what we should be doing here. And so that's just I think something else to think about.

DR. KRESS: I think you're wise to think about this thing carefully, because I can see the potential for real criticism from certain groups. I think this would be one issue that they really would latch onto, if it's not done property and with good justification.

CHAIRMAN SHACK: Do the bystanders have any comments they'd like to make?

MR. BUTLER: John Butler, NEI.

It was a very interesting day, and I too enjoyed this afternoon's discussion.

As far as the morning's discussion, we're very interested in the process, obviously, and are looking for ways to in part short circuit some of the issues that we're dealing with here to utilize the option 3 thinking in addressing GSI 191 sump performance. So I imagine that this Committee will be involved in some of those discussions.

And I know that we're going to participate in a Subcommittee meeting in June on sump performance, but I would hope that there's an earlier opportunity to make some progress and we can speak with you, we'll take advantage of that.

CHAIRMAN SHACK: Well, I think with that, it's time to adjourned. Thank you very much.

(Whereupon, at 5:57 p.m. the meeting was adjourned.)