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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON REGULATORY POLICIES AND PRACTICES

MEETING

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TUESDAY,

November 16, 2004

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The meeting was convened in Room T-2B1 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:00 a.m., Dr. George Apostolakis, Chairman of the subcommittee, presiding.

MEMBERS PRESENT:

GEORGE E. APOSTOLAKIS
Acting Chairman

MARIO V. BONACA Member

RICHARD SACRS Member

GRAHAM B. WALLIS Member

F. PETER FORD Member

THOMAS S. KRESS Member

GRAHAM M. LEITCH Member

VICTOR H. RANSOM Member

WILLIAM J. SHACK Member

JOHN D. SIEBER Member

ACRS STAFF PRESENT:

Michael R. Snodderly
Designated Federal Official
Allen Hiser
RES/MEB
Rob Tregoning,
RES/DET/MEB
Lee Abramson
RES
Arthur Salomon
RES/DRAA/PRAB
David Lew
RES/ PRAB

Also Present:

John Butler
NEI
Stanley Levinson
AREVA
Ray Schneider
WEC

A-G-E-N-D-A

Discussion of LOCA Frequencies 5
R. Tregoning
L. Abramson
General Discussion, including material to be present to the Full Committee 149

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

8:02 a.m.

CHAIRMAN APOSTOLAKIS: This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Regulatory Policies and Practices. I am George Apostolakis, acting Chairman of the Subcommittee.

Members in attendance Tom Kress, Bill Shack, Graham Wallis and Rich Denning.

The purpose of this meeting is to review the staff's last proposed NUREGs document documenting the expert opinion elicitation of large break loss of coolant accident 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.

Mike 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 November 2, 2004.

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

It was requested the 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.

As I just said, the purpose of the meeting is to review the staff's draft proposed NUREG report but to mention the expert opinion elicitation of large break loss of coolant accident frequencies. This draft NUREG report is to provide the technical basis for determining an appropriate break size.

The Committee at its December, 2004 meeting is scheduled to review and comment upon this draft proposed report. The Subcommittee is prepared to make a recommendation to the full Committee on whether or not the draft proposed NUREG report should be issued for public comment.

The recommendation will also consider how the draft proposed NUREG report will be supportive of a proposal with the risk-informed requirements addressing large break LOCAs.

We will now proceed with the meeting, and I call upon Mr. Rob Tregoning of the Office of Nuclear Regulatory Research to begin.

MR. TREGONING: All right. Thank you, Mr. Chairman.

We'd start out with the mea culpa. If you'll look at your handouts, I think they say we have a number of slides in there that are hidden that we've just provided for your information, but I think said if you give a slide number, it's slide X of 37, we'll there's only 35 slides. So there's not an error in your packet or an error in the slides. So, I apologize for any confusion. But we've corrected that error as we've made things in the elicitation in the presentation itself, so this reflects the most accurate information that we can present.

As the Chairman mentioned, we are here to discuss the expert elicitation that was conducted and develop passive system LOCA frequencies using the risk-informed revision of 10 CFR 10.56. I'm Rob Tregoning and copresenter is Lee Abramson, formerly of the Office of Research.

CHAIRMAN APOSTOLAKIS: Currently where?

MR. ABRAMSON: I guess I'm still currently -- I'm a consultant.

CHAIRMAN APOSTOLAKIS: Are you retired or something?

MR. TREGONING: He's part-time.

CHAIRMAN APOSTOLAKIS: He's paid, right.

MR. ABRAMSON: Not much, but I'm paid.

MR. TREGONING: There's objective for the presentation today. One, we're providing in the presentation just a very high level outline of the LOCA elicitation that's chronicled in the draft NUREG and used as part of the technical basis supporting the proposed 5046 rule revision.

The outline is going to be relative high level, because most of this information has been presented to the Subcommittee and main Committee in prior sessions. So most of the detailed information that we'll talk about in this presentation is going to be a discussion of the research that we conducted, since really the last in depth previous ACRS discussion, which was in reality March, but then we were also at the main Committee in July. However, this additional research is very important to understand because it documents additional sensitivity analysis that we've conducted and also discusses the internal and external review.

Obviously, while the outline of the rest of the elicitation is high level, we're certainly willing and expecting to deal with questions at a very refined level as necessary. And I can always pull up

old slides if we need to as well.

Just wanted to briefly review for everyone, get everyone up to speed. We've been in front of the ACRS numerous times to talk about the elicitation. Most recently was in July of '04 when we presented it in front of the main Committee on the initial results and some of the initial sensitivity analysis, and the use of the results in making a selection for the transition break size. Prior to that in March and April we were in front of this Subcommittee and the main Committee to talk about the results. And prior to that dating back all the way back to March of 2001 was, I think, the first time we came in front of the ACRS, which essentially laid out some of the technical issues and the reasons why we thought at the time we needed to pursue expert elicitation to develop these frequencies.

So since the July meeting we've had quite a number of milestones that, again, we're going to be talking about here in great detail.

In the July time frame when we came in front of ACRS, we had just completed the very first preliminary draft of the NUREG and we had supplied that NUREG to all the external review -- or I'm sorry. To all the panelists that were on the expert panel for

the elicitation.

In July we had a video teleconference with those panelists and we got feedback with them, which we incorporated and used to make revisions. So we had completed this initial review by the elicitation panelists. That was completed around August 30th.

In the beginning of August we also initiated an external review. And the external review, as we're talk of later, focused on the analysis of the elicitation responses. And that was initiated in early August. We completed, for all intents and purposes, at the end of September.

And then the latest milestone is a week or so ago, November 5th, we submitted the latest version of the draft NUREG for ACRS review. And this is the vision that we'd like at main Committee to get a recommendation whether the ACRS believes that this is suitable for going out for the public comment period, as well as supporting the 5046 rule revision.

So I wanted to start with an executive summary of the process at large. Again, we utilized the formal elicitation process to estimate generic BWR and PWR passive system LOCA frequencies primarily associated with material degradation.

As part of this effort we developed

quantitative estimates for piping and nonpiping base cases and we spent a lot of time in the past discussing what those base cases were and how they were used. And we used those for anchoring, for quantitative anchoring of the elicitation responses.

The panelists when they provided this information, they provided quantitative estimates. But as important or even importantly they supported their quantitative estimates by qualitative rationale for the various underlying technical issues that we developed as a group. And they've provided us these estimates in individual elicitations.

In terms of the results or agreement among the panelists, we have generally good agreement about the important qualitative LOCA contributing factors. However, the difficulty that all the panelists face was actually trying to express quantitatively the impact of these various qualitative issues. And when you look at the quantitative estimates, that's where you can see relative large individual uncertainly. And then also panel variability. So, again, good agreement qualitatively what the issues are. Much more difficult to quantify those estimates. And that's one of the reasons we selected on an elicitation in the beginning as an approach we were going to use to

tackle this problem.

The quantitative results that we're using we've determined by aggregating the individual panelist's estimates. The method we've used, and we're going to go into this in great detail subsequently, we've essentially a geometric mean to aggregate the individual results. We believe this approach is consistent with the elicitation philosophy. And one of the things you'll see is that the results are pretty comparable to the NUREG/CR 57.50 estimates.

NUREG/CR 5750 was completed in 1998. This was the last look or the most recent look at that the agency had given to LOCA initiating event frequencies. It was done in a much different manner. So the fact that they're comparable is somewhat serendipitous, but it still provides an interesting an relevant benchmark.

We are going to talk about in terms of sensitivity analyses, there were a number of alternative aggregation schemes that we employed. And one of the things we'll discuss is that the way you aggregate the results definitely can effect the bottom line estimates that you come out with.

CHAIRMAN APOSTOLAKIS: Let me ask you

something about --

MR. TREGONING: Sure.

CHAIRMAN APOSTOLAKIS: The whole point of this is of course to support the revision of 5046, the board break LOCA frequencies. And 5750 was published in the late '90s or something?

MR. TREGONING: In '98.

CHAIRMAN APOSTOLAKIS: Yes. Now you come up with a report five years later that doesn't quite agree with 5750 and you have some arguments why the 5750 results are not applicable to the rule. Now, I'm sure 5750 also critique earlier studies, the reactor safety study estimates were pretty high and so on.

I'm wondering whether five years from now we're going to have another study that would criticize your study, and how would that affect the current effort to risk-informing the large break LOCA frequencies? How much can we rely on all this in this processing, in other words?

MR. TREGONING: Yes. Well, when you look at these estimates, whenever you develop these estimates you start with the same knowledge that you have. And I think all of us hope that the state of knowledge that we have is going to continue to evolve in this area. And I think what we've tried to do with

this is you try to build on and make better the estimates that have gone on in the best.

I would hope that five to ten years from now somebody will look at this work and look at it very critically and say "We can do better." And if they can do better, we can do better at that time, then there's benefit for reevaluating this at this time, then I would say by all means it's a worthy endeavor at that point.

CHAIRMAN APOSTOLAKIS: But the current state of knowledge, though, which I agree with you, you know this is really what we're trying to do with expert opinion elicitations, the current state of knowledge includes what I just said.

MR. TREGONING: Yes, of course.

CHAIRMAN APOSTOLAKIS: That every four or five years we seem to change the frequencies. So that creates an uncertainty that is above whatever we're doing here.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: And that's not a criticism for you, by the way. This is the way it is. And that uncertainty is not really quantifiable.

So it bring us now to the structure of this interpretation of defense-in-depth. So whatever

your experts come up with or the aggregation schemes, it appears that we will have to be conservative and put some extra margin, which I think the stuff is already done. I mean, they go to 14 inches for PWR.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: So how much would this uncertainty, how much margin should we put there? Do you have any comments on that? Or how confident are you that these numbers -- because you claim in there -- well, not you personally, but the report states that the experts expect that their estimates will be more or less stable for the next 15 years or so?

MR. TREGONING: Yes. Yes.

CHAIRMAN APOSTOLAKIS: How can they say that, I mean when we have a record where every five or six years we change the frequencies?

MR. TREGONING: Well, point of clarification. This is the third major evaluation, to my knowledge, that we've really had as an industry to evaluate LOCA frequencies. The first time was back in the reactor safety WASH-1400 estimates time frame. And we just had almost very little operating experience at that time. So we really were relying on information that we had from other industries. And there was a

conscious effort by the people that did the WASH-1400 estimates to ensure that these were conservative estimates.

So that was a decision, and again at the time frame based on the state of knowledge, that was I would argue a very good decision.

5750 compared to WASH-1400 was a radical departure from the methodology to determine LOCA frequencies. And, again, the goal of 5750 was also to be conservative and also look at evaluating the operating experience that we accumulated up to that time, which was certainly much more considerable.

Well, this was completed in '97/'98. It's six years later and this in my mind is the first real in depth multi-disciplinary look that we've had at LOCA frequencies to build on the 5750, you know.

So in five years unless something dramatic happens, I don't know that the agency is going to want to bite this off again.

CHAIRMAN APOSTOLAKIS: I understand that. But it's not only your studies. I was reading -- well, first of all I'm not an expert in this, not in the elicitation in the materials part. So I was reading another paper that recently was published by Fleming and Lydell.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: And Lydell is one of your experts.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: And I think both of them, certainly Fleming, both of them were involved in an early EPRI study on frequencies. And now they say, again, a few years later we emphatically urge people not to use the EPRI results. So what is that telling me about this field? How do these things change every years and should I take your numbers and add 20 to make sure I'm covered?

Lee?

MR. ABRAMSON: You raise a very good point, George. What we've tried to do, at least the way I look at with the study and with the experts, is to try to come up with the best estimate of course in including the uncertainties as to what the frequencies of LOCAs are going to be under all various circumstances.

And you raise, obviously, a crucial question as far as the application is concerned. How is this going to be used in a regulatory arena. And I think it's really important to try to separate this.

In the report itself, and we talk about

conservatisms and so on and so forth, but I think as far as this NUREG is concerned I think what it should be focused on is what is the best we could come up? What's the best expression of this expert judgment, including their uncertainties?

What you're raising is another issue. Considering the regulatory arena and the fact that these things change over time, how should it be applied? I think that's an issue that really goes beyond what the NUREG does and what we've tried to do in the NUREG.

CHAIRMAN APOSTOLAKIS: And I agree with that.

MR. ABRAMSON: So it is a very, very important issue. But I would say that we really do not address that, certainly --

CHAIRMAN APOSTOLAKIS: I know you don't. You're not addressing it.

MR. ABRAMSON: And also I think it's important, too, that when we talk about conservatism you can talk about conservative estimates in terms of the technical responses of the experts. It's another issue as to whether you want to do additional margins on conservatives from a regulatory point of view. And I think that should be, if you want to do this and

that's certainly appropriate, you should separate that type of conservatism, added in conservatism from the built-in estimates of the report.

CHAIRMAN APOSTOLAKIS: No. This comment is not intended to criticize what you guys have done. And you did, you know, what you were supposed to do. But this Committee, of course, is interested in the ultimate use of all this information in regulatory decision making. That's why I'm raising these issues.

Now, another fault is that why didn't you ask the experts to consider these issues? Because they're certainly the experts. And make a judgment about how things can change?

In fact, they make a statement that is exactly opposite of what I am doing here. They say that these estimates wouldn't be -- unless the opposite of the report, not the experts -- that these estimates will be fairly stable in the next 15 or whatever years, which is a pretty bold statement in my view given the history of the thing.

MR. ABRAMSON: Yes. Well, I would respond, they're experts in their subject matter. They're not necessarily experts in the regulatory arena how these estimates should be used in the regulatory arena.

CHAIRMAN APOSTOLAKIS: Graham?

MR. WALLIS: Yes. I'd like to make a comment about uncertainty, and I think that that's really what we're talking about, George.

Well, first of all, we have to recognize that an expert elicitation is a necessary evil in the sense that we really need it, but you can't really make data out of nothing. And I'm not implying that that was done here. But I think that the big issue that we're really addressing here is the uncertainty, and it's very typical of expert elicitation that the experts think that they know more than they really know, that they're more definite. And I think that the area that we have to be particularly critical of the report in terms of looking at the report carefully is I think there is a great tendency to narrow those uncertainties. And the uncertainties are truly large. And we have to make sure that the NUREG report really attempts to reflect those uncertainties and doesn't draw them in.

I think that there's a tendency in the report to underestimate what the real uncertainty is. And we take a set of experts that in a large sense get prejudiced by talking to each other. And that helps to narrow.

I mean, if we look at the range of uncertainty across those experts, that doesn't really represent the true range of uncertainty. It's great than amongst those experts, because they talk to each other. They narrow -- tend to narrow.

CHAIRMAN APOSTOLAKIS: They read 5750, right?

MR. WALLIS: They read 5750.

CHAIRMAN APOSTOLAKIS: They all read.

MR. WALLIS: But, see, what's what you're really talking about. See, I don't know what the 5750 uncertainty bands were whether they really encompassed these, but I think that's what we really have to be careful of is that we do not allow the uncertainty bands to be narrowed artificially. And I'm afraid that there's a tendency for that to happen.

CHAIRMAN APOSTOLAKIS: But there is also another point. I do appreciate Lee's comment that we should really review the report and all that, but there is a bigger issue here. Because we had the presentation by the staff, the regulatory staff, a few weeks ago. And they told us that they added extra margins, as I'm sure you're fully aware.

But if you read the Commission's side, they have a for example that goes on for several

paragraphs. And they sort of hint that once you use the mean frequency of the expert distribution as the transition break size. Now if that is the case, and if the Commission insists on that, in my view it puts a tremendous burden on you guys. Then we have to make sure that your uncertainties are not underestimated. Because it's one thing, you know, to develop a distribution. And, look, we all have been involved in these exercises. We know that there are many ways of doing things and so on. But then if the decision making says I'll take your results and I'll add X, then the details of the processing of the expert opinion is maybe not as important anymore. But if the decision making says I'll take your distribution and use your mean value, whoa, it's a whole different ball game now.

So that it's risk-informing versus risk-basing in the regulation. If you risk-base them, there's tremendous burden on the PRA to be perfect. IF you risk-inform, then you remove some of that burden because you're also using other conservative philosophies and so on to make to decisions. So I don't know how to do that.

If Rich is right or the uncertainties are underestimated, that certainly would effect the mean.

And the choice of the size of the transition break.

I just wanted to get your thoughts on this. I mean, again, I am fully aware of the of the fact that this not an issue that can get a definitive answer by anyway.

But, you see, I mean when I read the SRM, I think whoa, it says that we should go with the mean of the exports.

MR. TREGONING: Well, philosophically I'm in full agreement with everything that you just said. So, believe me, I agree with the fact that if you use the elicitation results it does put more burden on those results.

CHAIRMAN APOSTOLAKIS: Absolutely.

MR. TREGONING: And we've tried to do as good a job as we can do, certainly. And I would argue that we haven't underestimated uncertainty in this report, at least based on the results that we get.

Now, you could argue well did the experts themselves, you know, because you didn't have of a pool or because, you know, they collaborated in some sense. But, you know, I'd argue that the pool of experts in this area is relatively small and they all relatively all have a very similar experience just because the background information. It's communal

knowledge. It's shared.

MR. SHACK: Well, there's also the question of whether the uncertainties are this way or this way.

MR. TREGONING: Well, that's true.

CHAIRMAN APOSTOLAKIS: Up or down for the record.

MR. TREGONING: That's right. That's right.

CHAIRMAN APOSTOLAKIS: There's no video of the meeting.

MR. ABRAMSON: Another point to note is that in the report itself we do not recommend an answer.

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: We have what we call a baseline result and we give arguments why this is the baseline. But what we also do is you'll hear, of course, extensive sensitivity analysis considering excursions from this. And we do mention in the report which of our results you should use or which combination, it depends on the particular application that you're going to use it for.

So we try to separate out, if you like, the technical problem of how to extract the

information with the expert elicitation as far as getting the best estimate you can from the application of it. And this is certainly intended as an input to the application process.

CHAIRMAN APOSTOLAKIS: Absolutely.

MR. WALLIS: Will you explain this bullet that says geometric mean aggregation, results are consist with elicitation philosophy.

MR. TREGONING: Yes, we'll get more. And Dr. Apostolakis, if we want to get into this later -- again, this wasn't going to be the focus of this presentation, but I could provide a little bit more insight how the NRR folks, how we started with this --

CHAIRMAN APOSTOLAKIS: Please do.

MR. TREGONING: -- and how we ended up with what we did. I've got a couple of slides that I'll show, and maybe I'll get them after the break.

CHAIRMAN APOSTOLAKIS: We can use all the insight we can get

MR. KRESS: Before we get off of this particular issue, I want to submit maybe a different view of the subject.

I agree with what was said in general about expert elicitation. In this particular instance where it's being used strictly for 5046 only, now I'm

restricting my comment to that because this could be used for other things. But for use in 5046, I don't think it gives a damn.

CHAIRMAN APOSTOLAKIS: You don't think what? I'm sorry.

MR. KRESS: What choice you use for the expert elicitation and what the uncertainty is. I think you could pull this -- out of the air and wouldn't have mattered. Because what you're asking is, we're asking to control the risk to these plants somewhat. And what the subject matter is given a redefined transition break size, what does it do to the risk of the plant. That has nothing to do with this expert elicitation. That's just a choice, a way to pick this number out of the air. And it doesn't matter what the uncertainties are. The real question is what effect does that have on this. Well, that's of course something you're not going to be able to talk about it a priori because it requires too much information for the PRA to deal with as a start.

But if you have a process dealing with 50.46 which is going to track this risk change in individual plants for individual changes and put a lid on it. Now that's the only thing that gives me confidence.

I could care less what process you use to choose these pipe sizes. Pull it out of the air, use the mean, use the 95 percentile; I don't care. It doesn't matter --

MR. SHACK: -- you should get for the LOCA--

MR. KRESS: Oh, yes, there's another side to this. The choice also effects one set of sequences, the LOCA sequences and their contributions to risk. It will matter a little there. But the point is --

MR. TREGONING: It will matter a lot.

MR. KRESS: Oh, I know. The LOCAs don't--

MR. SHACK: Well, that's because you assigned a certain frequency.

MR. TREGONING: Exactly.

MR. FORD: Could I get in here? Yes, I'm just trying to get moving forward. I've got a request as you go forward that where appropriate you could mention the question -- specificity of the material degradation. What's going to refer to is the unexpected event, Davis-Bessie for instance. It should impact on all the other things.

This report is primarily looking at the mean and the uncertainties on generic BWR and PWR and

there's generic at all about materials degradation. It's plant specific.

So where appropriate, if you could just address that.

MR. TREGONING: And I'll go ahead and address it now if that's okay, if there's a question.

MR. KRESS: But before we leave I hate to leave this comment unresponded to. This choice of a pipe size is going to have very little effect on the actual LOCA frequencies and the actual LOCA contribution. It's just not going to effect it very much. But that's the comment I wanted to say about the response.

MR. TREGONING: In reality, but it could--

MR. KRESS: I'm in a reality space.

MR. TREGONING: But you predict the effect of it, it could have a dramatic effect.

MR. SHACK: You know, how you treat breaks above the transition break size, if you --

MR. KRESS: That's a problem. What he's talking about, that's right. And I think the --

DR. WALLIS: You can't make a categorical saying without knowing what the plants will do. If it's against the rules, there may be big changes in the plant which change a whole lot of things.

MR. KRESS: That's why I said you can't determine the risk ahead of time.

DR. WALLIS: And we haven't talked anything about that.

MR. KRESS: Yes, we have.

DR. WALLIS: Very, very little.

MR. KRESS: Oh, no. There's --

DR. WALLIS: Not positively.

MR. KRESS: But there's a list of things that can be done and there's a process to control risk.

DR. WALLIS: Ah, that's their problem. That's their problem.

MR. KRESS: Ah, yes, that's the important thing.

CHAIRMAN APOSTOLAKIS: Well, maybe we can move on to page five of this.

MR. FORD: Before we move on, could you said, you said you'd better this.

MR. TREGONING: Although, again, and I'll talk about this a little more in depth in later as well, but although the goal was to develop generic frequencies, we spent a lot of time talking about broad plant specific differences. So differences in broad ESSC, for instance, difference in mitigation

techniques that are applied among the plant.

MR. FORD: Right.

MR. TREGONING: And how those differences could impact the generic values.

MR. FORD: Right.

MR. TREGONING: So part of the uncertainty bound was to reflect differences that could exist broadly within plants.

Now we specifically told the experts not to consider the effect of, at least on degradation issues, of a single plant that might have a number of for whatever reason outlying characteristics. However, if there is such a plant that they know about, by all means make us aware of that during the elicitation so we can take appropriate steps to make sure that we bring them back in with the fold.

MR. FORD: So that's specific knowledge from the experts?

MR. TREGONING: Yes.

MR. FORD: Is part of your 95 percentile?

MR. TREGONING: Yes.

MR. FORD: Yes.

MR. TREGONING: And when we got into --

MR. FORD: Is that qualitative or quantitative?

MR. TREGONING: Both.

MR. FORD: Okay.

MR. TREGONING: There's a whole set of questions on safety culture with respect to passive system LOCA failures.

MR. FORD: Yes.

MR. TREGONING: The effect of variability in safety culture among individual plants was specifically factors in that case --

MR. FORD: Right. Yes, I saw that.

MR. TREGONING: -- to the bounds with respect to these average -- you know, the sort of average result.

And one of the things that came out of this, and I brought this up a couple of weeks ago, is that the safety culture was really deficient, many of the experts said this could dramatically effect your LOCA frequencies not surprisingly, a factor of a 100 or maybe even more.

CHAIRMAN APOSTOLAKIS: Yes, but it seems to me, though, that the report says that if the safety culture was included, but at some point later it just dismisses it. That the experts felt confident that the safety culture would be good and there is no impact.

MR. TREGONING: Yes, it does. I think dismiss might be too harsh a criticism. We separated it from the very beginning and then we said we want to consider safety culture separately because it's a separate issue. And what the experts said is that sort of the generic or the average safety culture we expect to stay relatively constant.

CHAIRMAN APOSTOLAKIS: But you see --

MR. TREGONING: However it's the bounds, and we do say in the report that the bounds or the differences at individual plants could be, you know, if proper procedures, protocol, inspections, implementation; if all those things aren't followed, then there is an impact that the experts could quantify.

CHAIRMAN APOSTOLAKIS: Right. So the experts acknowledge that there may be an impact?

MR. TREGONING: Of course.

CHAIRMAN APOSTOLAKIS: But they did not include it in their estimates?

MR. TREGONING: They included in it estimates of the effect of safety culture on the bounding estimates, not the mid-value estimates. Because we were looking for general trends. Look out there on the future and see what's going to be the

effect of steam generator replacement. What are general trend effects? You know, what's going to be the effect of deregulation? What's going to be the effect the fact that the plants are -- you know, that we're getting more experience? What's going to be the effect of, you know, the aging workforce? All of these related issues and how they're going to effect the industry at large. So that's what we were really trying to get at with the safety culture questions, at least in terms of the average responses.

But then for the bounds, tells us about the effects that individual plants and some differences from the average, say, industry safety culture, how that could effect LOCA frequency.

MR. DENNING: But your bounds don't effect your mean?

CHAIRMAN APOSTOLAKIS: Yes, they should.

MR. TREGONING: The bounds don't effect the mid-value. They'll certainly effect the mean.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: Yes.

MR. DENNING: The mid-value? Yes.

MR. TREGONING: That's right. They don't effect the mid-value --

MR. DENNING: And it is the mean we were

looking at, right? Is your geometric--

MR. TREGONING: But one of the things we don't do is we don't modify or multiply the results, the degradation based results by any sort of safety culture modifier. There's no combination in that sense.

MR. DENNING: I mean, I think it's a big mistake. I mean, I don't know exactly how you do it but if you look at the mean of the plants that are out there and suppose there's a plant that's 100 times worse than any other, the mean impact is tremendous. You know, like you saw in NUREG 1150, you often distributions where the means were greater than the 95 percentile.

MR. TREGONING: Yes.

MR. DENNING: And I think you're constraining this in a way that doesn't reflect the reality of the population of plants that are out there today or will be in the future recognizing that there are always going to be bad plants.

MR. TREGONING: Right. But again, we want generic estimates. We don't estimates that are skewed by one particular plant.

MR. DENNING: That's the population of what we face, though. That's what effects the

average. If you have a plant that's 100 times worse than the others, it dominates the risk.

MR. TREGONING: At that plant, not any other plant.

MR. DENNING: No. The risk. No, I meant the whole risk.

MR. TREGONING: Well, okay, it might dominate.

MR. DENNING: The public risk. And by decreasing regulatory requirements that don't -- you know allow him to stay out there and dominate the public risk, you know. So I think that there is a real concern here that we have to worry about those outlier plants and how they effect.

And I recognize you --

MR. TREGONING: Yes.

MR. DENNING: It's not easy to address.

MR. ABRAMSON: Your point well is taken, but again I'll come back to the point I made before. I think this is not the -- the exercise that we went through with the NUREG was not intended to account, I guess, for the full effect or the full range of plants that are out there. As Rob said, we say over and over again this is a generic estimate. And that was the instructions as to the panelist.

The issue you raised as to how you might have, say, outlier plants and how you would effect this, I would submit that this is something that's somewhat beyond what the purpose of this exercise was and needs to be taken into account when you do the regulatory application.

MR. KRESS: That's exactly right. And the way you do that is fix the PRA so it reflect safety culture issues.

CHAIRMAN APOSTOLAKIS: The issue of safety culture confuses me a little bit. There is an extensive discussion in Appendix H of the report on safety culture where they say some of the things you just told us, that some experts consider Davis-Bessie and this and that. But then in summary it says the two principle conclusions from the safety culture elicitation questions are, first, safety culture effects on future LOCA frequencies are expected to be minimal. And second, the ability and regulatory safety culture are high correlated.

Then in bold face "Because of these findings no modification or adjustment was applied to determine if one containing LOCA frequencies presented subsequent."

So after all this discussion, the group

decided that safety culture would not effect the quantitative evaluation later. It's in bold face.

MR. TREGONING: Yes, again, the average safety culture.

CHAIRMAN APOSTOLAKIS: It doesn't say that. It says no quantitative. And the panelists expressed the need for continued vigilance. Well, yes, sure.

MR. SHACK: But I think what they're saying, George, is they don't expect changes in safety culture to change the frequency, not that safety culture can't --

CHAIRMAN APOSTOLAKIS: But the point is that the numbers at the end do not improve any possible--

MR. SHACK: Because they think the safety culture is going -- will remain constant.

CHAIRMAN APOSTOLAKIS: Yes, but that's a very important to know that it's one of the limitations or you know the scoping of the study that, yes, we look at safety culture but then we assume that safety culture will remain constant, even though we know that if changes dramatically, it will have a impact --

MR. TREGONING: Again, we didn't assume

that safety culture was going to -- that was based on the expert responses --

CHAIRMAN APOSTOLAKIS: Yes. Yes. The experts expect that the safety culture will be constant and good.

MR. DENNING: And they're not experts on safety culture.

CHAIRMAN APOSTOLAKIS: And they're not experts on safety culture.

MR. TREGONING: But they are -- well, they are experts on how safety culture can effect LOCAs.

CHAIRMAN APOSTOLAKIS: Absolutely.

MR. TREGONING: And that's only what we asked them about. We are only looking at a very small piece of that safety culture.

CHAIRMAN APOSTOLAKIS: But I would say, though, that maybe -- you know, in your executive summary of the abstract you should be a little cautious to refer to issues like that. Because I find this, what I just read, to be a little bit inconsistent with -- like in the executive summary, page A-1. The effects of safety culture of LOCA frequencies were also evaluated, period. Now that tells me that the numbers that they're going to give me include the effect of safety culture. But on

Appendix H it says well, we evaluated but we really decided that it's going to remain good and we didn't include it in the numbers.

So I think this statement in the executive summary, which a lot of the decision makers are going to be read, should be qualified.

MR. ABRAMSON: But if they said on the average the effect of safety culture is a multiplier of one, in effect we have a value. If we multiple the answer by one, and then they don't change.

CHAIRMAN APOSTOLAKIS: Well, that's not what they said. Some of them actually used a factor of less than one, right?

MR. ABRAMSON: Yes, but --

CHAIRMAN APOSTOLAKIS: And others said -- I just don't think that this sentence on page A-1 is consistent with what you have in Appendix H.

When I went to Appendix H I thought I was going to see more along the lines --

MR. TREGONING: I mean, and if you go on the paragraph before H, you'll see a lot about the treatise and the effect of individual plants. And we do talk about that definitely in H. That last sentence is, and why is bolded? Just because -- because of the generic consideration by the experts

that, again, the average safety culture is going to remain relatively constant.

CHAIRMAN APOSTOLAKIS: Well, that's --

MR. TREGONING: We didn't do any modification.

CHAIRMAN APOSTOLAKIS: I understand that. But that's a pretty strong assumption on their part.

DR. WALLIS: What about the statement on page A-3 that the effects of safety culture are cyclical? And that's very different from them remaining constant. Where did that come there and how did that get changed? And if it's cyclical, I'd like to know how big are the variations.

MR. TREGONING: Well, many experts describe that, you know, safety culture like many things can be a bit of a pendulum. That, you know, something like Davis-Bessie happens and then you have higher safety culture.

DR. WALLIS: Right. And then you get sloppy?

MR. TREGONING: Yes. And the magnitude of the cyclic range is, you know, reflected in the bounds as well. So all that we've said --

CHAIRMAN APOSTOLAKIS: Is it in there somewhere?

MR. TREGONING: Yes, it's certainly part of the bounds as well, as well any plant specific differences.

CHAIRMAN APOSTOLAKIS: Anyway my comment--

MR. TREGONING: Because we asked them to consider with respect to safety culture well how bad could it be, how good could it be. But, again, the mean trend was essentially a flat line. But, again, we realize that things are not truly constant, they're going to be oscillating about -- at least the experts feel they're going to be oscillating about that line as we move forward.

CHAIRMAN APOSTOLAKIS: Anyway, what I'm suggesting is that perhaps you should revisit the executive summary and make sure --

MR. TREGONING: I made a note.

CHAIRMAN APOSTOLAKIS: -- the appropriate caution is exercised --

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: -- when you make statements like that. And make it consistent.

Appendix H is very illuminating. I mean, it just says what you guys did and what their conclusions were. I may disagree with it, but that's what the experts did. But I believe the executive

summary should reflect those findings because you know that because of Davis-Bessie everybody is interested in that.

Anyway, let's go on, unless there is another comment.

MR. BONACA: I have one more question. It has to do with the essential objectives and scope.

MR. TREGONING: Yes.

MR. BONACA: C-1. In that there are a number of discussions about what is not included.

MR. TREGONING: Yes.

MR. BONACA: And after systems -- and then so on and so forth, there are similar things that are not included.

Now later on there are discussions, for example, the seismicity and the role of some kind of consideration. I guess also seismicity consideration have not been included yet?

MR. TREGONING: That's correct.

MR. BONACA: Okay. Although there is a discussion, there's no point. And, you know, as a known expert, I'm left with the question mark in my mind as I'm reading it of how am I going to include for consideration for what is a known intruder. I mean, I thought that these experts would help me with

this, but I haven't been helped what is here.

I hear all this, you know, statements that says only this included and only, you know, initiators could tell events and not potential in certain events, and so on, but it's not included. And so I'm left a little bit helpless in understanding how I go from the elicitation curves to the transition break, and that's really the bottom line. Because I heard some statement that says because of the -- included, we jump -- and I don't know how to make this fit.

MR. TREGONING: Right. One of the reasons for the objective in scope statement was because as clearly as we could lay out what was included and what wasn't included. Because again, there's total risk associated with LOCAs. We weren't able to assemble an expert panel, not a single expert panel that would have been expert in all the various LOCA risks that, again, make up the bottom line risk associated with LOCAs. And we tried to be very clear about what we did consider and what wasn't considered especially if we thought it was conceivably important, and the area of seismic breaks was one that we wanted to make sure that we identified.

Now NRR, at least as they have taken this information and said, okay, how am I going to use this

to take a break size, they have had to have an understanding of what was included and what wasn't included.

MR. BONACA: Well, why didn't they say that? Because I'm wondering how do they get to what you provide --

MR. TREGONING: Well, I told George I'm going to bring a couple of slides down after the break. And I'll try to provide some more information on philosophically how the elicitation results were used as a baseline and how they ended up with -- how it has come to the final break size, or at least the proposed break sizes that --

MR. BONACA: Because my understanding is that would equal the break. The break is how do you go to the transition break size. And then they did a central issue. You know why not one break size, not another one? I think that the statement that's been made that because -- breaking is included were conservative -- well, I mean I got to understand the dynamics of that, because I don't understand it right now.

MR. TREGONING: Yes, it's certainly an area for debate.

MR. SHACK: Take your chances and go

ahead.

MR. TREGONING: Okay. Motivation. Again, we've probably covered this already, but there's really two motivations certainly as we've all talked about. The primary motivation was developed part, not all but part of the technical basis for developing I call them alternative design basis break sizes, but we refined and we call them transition design basis break size now for use in the ECCS role.

Another secondary but very important motivation was to develop updated LOCA frequency distribution best estimate values that we could use in the plant PRA model as well as provide insights that could be used for risk assessment in terms of where pipes are expected to break, what sort of systems do we think are likely to fail. These are things that could certainly affect the plant risk. And we're hoping that these insights can be used to improve modeling that's used in PRA now to measure and account for the risks associated with LOCAs.

So, Dr. Bonaca mentioned the elicitation objective and scope with the section we have in the NUREG I've tried to restate them here as concisely as possible. Again, the primary objective is to develop generic BWR and PWRs piping and nonpiping passive

system LOCA frequency distributions as both the function of break size and operating time. Again, we mainly focused on LOCAs which initiate a portion of reactor coolant systems. The LOCAs were primarily related to passive component aging tempered by mitigation measures that plants typically employ.

We examined small, medium and large break LOCAs as are historically done in evaluating the plant PRAs, but we also further subdivided the large break LOCA category to consider four different LOCA sizes that are historically just called large break LOCAs. But we wanted to look at pipes breaking over a variety, all the way from 6 inches which is the typical large break LOCA threshold up to a double and guillotine type break of the largest type in the plant. So we go from 6 inches up to roughly 40 inches or so.

And we wanted to -- it because we wanted to see how these frequencies would be effected as we go up in break size.

Time frames we considered. We developed estimates at three discreet points in time. Twenty-five years and the 25 years represents the sort of average operator -- or average reactor life and that essentially corresponds to the current day fleet

average. A 40 year estimate which coincides with the end of the original license in the plants. And then estimates at 60 years which represent the end of life extension period.

MR. SIEBER: First license.

MR. TREGONING: The end of the original license is 40 years. That's right. Sixty years is the first license.

MR. SIEBER: First, right.

MR. FORD: Well, I was particularly interested in this particular one. And I looked through the report trying to find the degradation time algorithm that you should have used. I presume you used in order to go through that time sequence.

Were there specific degradation algorithm used, because I couldn't find them.

MR. TREGONING: By specific degradation algorithms do you mean modeling, for instance, IGSEC and --

MR. FORD: Correct. All that stuff.

MR. TREGONING: Of course.

MR. FORD: Now, was that the original stuff that was extreme uncertainty and the algorithms.

MR. TREGONING: Yes. If you look --each of which -- we had 12 panel members, each of which had

different strategies for dealing with the degradation algorithms that they employed.

MR. FORD: Right.

MR. TREGONING: Some of them had their own models. Some of them felt that hot models based on transit data and their operating experience, plus information they've seen from other models.

We developed the base estimates and we used the Praise Run and also the Rolls Royce Run, yes, we had obviously specific algorithms in there to model subcritical cracking due to --

MR. FORD: Okay.

CHAIRMAN APOSTOLAKIS: But you are not reporting those?

MR. TREGONING: What's that?

CHAIRMAN APOSTOLAKIS: You are not including those in the report?

MR. TREGONING: Oh, yes, they're in there. There's a whole section to talk about the development of the base cases and there's an appendix that talks about how the base case analyses were done using a Praise code. So, yes, those are definitely documented in the report.

CHAIRMAN APOSTOLAKIS: But at each elicitation panel member, maybe you can come to this

later on, and there is areas of magnitude scatter. And just between one expert and the other expert. And you had a process by which you dealt with that scatter?

MR. TREGONING: Yes. I'm sorry.

MR. FORD: This happens -- I'm sorry. What happens if one of the outlier experts in terms of this prediction is correct and the others are incorrect and this is a technical --

MR. BONACA: The Galileo example, right?

MR. FORD: Yes, exactly. Does that come into the thought process --

MR. ABRAMSON: There's correctness and correctness is not one of our objectives here in this sense.

MR. FORD: Oh.

MR. TREGONING: Maybe I should have answered.

MR. ABRAMSON: The truth, we don't know what the truth is. And the whole -- as I see it --

MR. SHACK: You can't handle the truth.

MR. ABRAMSON: Well, only know the truth.

The purpose of the exercise was to do, you know, the best expert elicitation that we could under the circumstances, the constraints. And to have the results reflect the results of that expert

elicitation.

We make no claim in the report I think that this is the truth or this is close to the answer.

CHAIRMAN APOSTOLAKIS: So you just go on until --

MR. ABRAMSON: You see, so you have to ask the question is what's the -- of what use or what value is an expert elicitation process. And that's another issue. You know, there's whole history of -- but we accept this as -- we've started from the fact that expert elicitations are used, and so on and so forth, they feel it will be of value and we're trying to do the best we can under the circumstances.

CHAIRMAN APOSTOLAKIS: Let me, on this point, did you have workshop or some sort of meeting where each expert presented his or her arguments and trying to convince the other guys? And did you try to reach consensus at the meeting rather than taking the individual person and taking geometric means and doing sensitivity studies? Why -- I didn't see that work consensus anywhere. And as you know in the seismic study that you're citing, that was a central theme that the reason -- I mean the main argument was that many times the disagreements are due to the fact that the experts have different states of knowledge. And by

having these workshops where they exchange information and they argue about these, you are bringing everybody up to the same level and there is no scientific proof of that, but there is a conjuncture that if you do that, then consensus might not be out of reach.

MR. TREGONING: Right. You're also potentially producing uncertainty with that process.

CHAIRMAN APOSTOLAKIS: Well, you are.

MR. TREGONING: We tried to do -- unfortunately, you can't have it both ways.

We tried to do I'll say a modified process compared to what was done at the seismic study. We did the elicitations individually because we didn't want to suppress uncertainty.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: We wanted the individual estimates. However, there was a strong component of group feedback that occurred at various meetings.

The very first meeting we had was an issue development meeting where as a group we brainstormed about the issues that we thought that were important as a group.

CHAIRMAN APOSTOLAKIS: And you said earlier that there was a lot of agreement among experts on the qualitative aspects.

MR. TREGONING: Yes, there were.

CHAIRMAN APOSTOLAKIS: Is it possible that it's a result of that meeting, in fact they each understood it --

MR. TREGONING: No, no. Because again -- no, no, no. The brainstorming meeting just said hey what are the different failure scenarios that could occur in piping. So this was essentially a shopping list of things that could happen.

CHAIRMAN APOSTOLAKIS: By the way --

MR. TREGONING: But the agreement was when they each had to go individually and say from this shopping list I think this is important, that's important, that's important. That's where the qualitative agreement was --

CHAIRMAN APOSTOLAKIS: Just as note here, if you are right on a study a few years ago on the seismic issue also and they tried to get the best of the whole world. So they get teams. So within a team there is an exchange of information and trying to reach consensus, but they let the team do it separate.

MR. TREGONING: That's right.

CHAIRMAN APOSTOLAKIS: So you wouldn't have the biases.

MR. TREGONING: That's right. That's

right. There's a lot of different approaches.

And, again, the way he did feedback to deal with outliers is we developed these base case estimates which we had just a very small percentage of a team develop these estimates. Well, they came back and presented all their estimates. And if you look at the base case results, there's a lot of variability. Four people were supposed to be analyzing a very small subset of conditions. So this was a simplified problem. When you look at the result from that, a lot of variability.

So we had an entire meeting where we discussed in depth what each of those four models, how they were constructed, what the assumptions were, what the approach was. And we had a lot of discussion among the experts as to what are the reasons for those uncertainties.

And then what happened is that this was all part of the elicitation. The experts then went back and they said okay of all these various four approaches, here's the one that I believe is closest to reality based on my experience.

So we asked them during their elicitations to weigh in on which approach they thought was more accurate.

And we did all the individual elicitation and we got the results, we had a final wrap up meeting. And what we did at the wrap up meeting is we presented not only the results, and again it was everything was anonymous, you didn't know who was an outlier. But more importantly if people had qualitative responses or rationale that brought up points that no one else had considered, we discussed those specific qualitative points. And we gave the experts the opportunity.

Now knowing this -- knowing this would this cause you to go back and revisit your estimate? We gave them another chance to revisit it based on that.

So we did apply a feedback loop into the process to make sure that at least qualitatively if someone was thinking outside the box and came up with a scenario or a reason for either high or low failure frequencies, we didn't get into the quantification aspects, but at least qualitatively we presented that argument and discussed it in a group. And we didn't try to reach consensus at that point, but we said if this reason or rationale has been enough to move you individually that you think your estimates are too low or too high, we're going to give you the opportunity

to go back and modify them accordingly.

And there was also when we had, again, the individual elicitations, there was a lot of feedback between the facilitation panel and the experts themselves to try to get at hey what's the basis for these estimates. What are you basing this on? What are your reasons for this, you know. What if I told you something different, would that change your estimates in any way?

So we didn't want to give them too much of a hint of what other people were saying, but we tried to again provide a very rigorous look at what they were basing these estimates on and make sure it was consistent.

MR. FORD: So the information exchange between individual panel member was via the facilitation people? It was not me, Tom Devick or -- I mean face-to-face?

MR. TREGONING: No, no, no. No, no, no. There were both. Again, we had three meetings that had the entire group.

MR. FORD: Okay. Only three?

MR. TREGONING: Three meetings. We had the kick off meeting and we had the meeting that evaluated

the base case estimates. And then we had the wrap up meeting. So we had a three meetings as a group.

MR. SIEBER: We also had a videoconference.

MR. TREGONING: Well, yes, we had a video teleconference, but that was for reviewing the NUREGs. And this was sort of after the fact. So, yes, we had that fourth meeting.

But, no, those three group meetings were where we vetted a lot of the -- again, I'll say a lot of the more interesting individual opinions that may not have been shared by -- or may not have been known or thought about by the majority of the group.

But again during the feedback sessions as well we tried to feedback some of this information as well. So there were two slightly different mechanisms.

MR. BONACA: Let me make another comment here regarding the bottom bullet. Assume no significant changes were occurring, plans had already been filed.

MR. TREGONING: Yes.

MR. BONACA: Now here as a Committee we're sitting in front of power-up rates and you recognize in the text that the power-up rates may in fact be

significant changes from a frequency standpoint, and so here again you know we are left with this question mark in our mind. I mean, one from one end we are going to have a power-up rate. In fact, possibly even higher power-up rate because of the change in 50.46. And yet the transition break that is being -- all the information, is really not reflecting this possibility. You know, it doesn't. And, again, that troubles me.

MR. TREGONING: Yes.

MR. BONACA: That troubles me. I mean here we're causing a change to the regulation that may cause a power-up rate even higher than today would be possible and yet we have no consideration in the design of the ECCS system of this change we're going to provide.

MR. TREGONING: Right. Let me address this one. This statement's in there as a cautionary statement to the regulatory community as much as anything.

The big assumption that was made -- look, there was no assumption made that we're going to stop inspecting the plants, okay. If we would have told the experts that we're going to stop doing any inspections of the pipe, their frequencies could be

dramatically different. The knowledge base is based on not only our modeling considerations but also the operating experience database. If you do anything which undermines your operating experience, certainly these frequencies could be effected. You know, if you do power up-rates --

MR. BONACA: I understand where you're going.

MR. TREGONING: Yes.

MR. BONACA: Put yourself in the shoes of the reviewer or somebody who has to buy this.

MR. TREGONING: Right.

MR. BONACA: And realize that the frequency there is one that says, you know, I don't think the are done sufficiently to put a warning. Because, I mean, I could have somebody that statistically go to those curves and wants to choose a mean value, you know, elicitation, and that's not so farfetched.

CHAIRMAN APOSTOLAKIS: Well, that's what yes I am saying.

MR. BONACA: Exactly. And so in all these provisos here are only limited to one summary page and those are the rates. And if there had been like a rationale position -- and I'm not faulting you. I

know there was a time constraint, etcetera. And already on this issue you may have a condition of such an impact that nobody would make that guess and jump to mean value without -- you know --

MR. TREGONING: Look, it's a classic problem. You give somebody a curve, they immediately start using it, you know. So I think your question goes much deeper. How do you ensure that we use these results, and I think that's really what you're getting at.

MR. BONACA: Well, and the first issue of the second comment was I would have liked to see some little ladder there or some help as to bridge --

MR. TREGONING: I'm sorry, a little what?

MR. BONACA: I call it a ladder. Anyway, a little bridge to go from the raw data to the judgment we have to do or use.

CHAIRMAN APOSTOLAKIS: Yes. I think this is a comment along the same lines as several comments we've made this morning.

MR. BONACA: This is it.

CHAIRMAN APOSTOLAKIS: No.

MR. BONACA: No, this is feeding on what the problem is.

CHAIRMAN APOSTOLAKIS: So the thing is Lee

Abramson's scenario, he said that you know there are two separate issues. One is the NUREG report and the expert opinion elicitation and then the other issue which the report does not address is the decision making by the NRC staff. But we all know that this report, this project was done to support that other decision making. And maybe the overall tone, especially of the executive summary, should be changed to have in mind the decision maker that will have to make a decision regarding the transition break size.

MR. BONACA: That's right.

CHAIRMAN APOSTOLAKIS: And offer as much help as you can.

I realize that we cannot go back and redo the elicitation. But just don't look at this as a separate piece of work that will be tracked by people who are expert and expert opinion elicitation. But give it that flavor, you know, if I am now the decision making that has to pick the transition size, how would this help me. And elicitation and so --

MR. TREGONING: The only danger there is when you talk about this a lot, and Dr. Kress raised this issue quite distinctly. It's an integrated process. It's not just a matter of saying what's your transition break size. It's a matter of understanding

how the whole rule is shaped, you know. What are you going to do beyond the break size? If there's no mitigation beyond the transition break size, maybe my transition break size is totally different than if I do have mitigation beyond it.

So that's why the executive summary, again--

CHAIRMAN APOSTOLAKIS: But, look, wait. I'm not saying that you should naturally address the issue of the size.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: I mean with regard to size or the transition size. What you should -- be fully aware of the fact that you are providing input with a guy who will do that.

MR. TREGONING: That's right. That's right.

CHAIRMAN APOSTOLAKIS: Okay? Because as I said earlier, there is one point of view that says take the expert curve, take the mean value and that determines the size.

Now, if you come in here and you give the executive summary arguments that will make me even support that point of view or say no, I need to do something else, then I would greatly appreciate it.

MR. BONACA: And I want to stress this issue here, because here you're talking about operational changes which are never proven, and yet this rule is intended to support operational changes. They will happen as a result of the rule. So that's why there is such a linkage there.

MR. TREGONING: But, yes, we talk about operational changes that would effect LOCAs. That's just a subset of all the possible operational changes.

MR. BONACA: I understand that.

MR. TREGONING: You take out an accumulator, you know, that's not going to effect a LOCA initiating event frequency more than likely, unless it sets up some weird vibration in the plant.

MR. BONACA: No. I was talking about operational changes which may include significant power-up rates.

MR. TREGONING: Right. Right.

MR. KRESS: The thing that worries me is not the choice of the transition break sizes, as I expressed before, this curve can stand by itself. Here's a new frequency versus break size, resulting curve. It's going to be used for other, I guarantee it.

One of them could be, for example, risk-

informed inspection of packing. And there are other examples.

It's there that the uncertainty distribution and assessment of the uncertainty bothers me because it ought to be part of the decision making.

And so you know, I'm not so concerned about the decision maker will deal with this in the transition break size, I think they've covered that pretty well. The decision makers know how to deal with it. It's the other uses that this might be put to that it seems to me like it needs some sort of -- I don't know, word of caution.

MR. SHACK: You know, guys, we'd better get going because page 26 is about where things really get interesting and we got a long way to go.

MR. SNODDERLY: We were going to take a break around 9:15 or 9:20. So in the next ten minutes can you get us to slide 17?

MR. TREGONING: There's no question.

MR. SNODDERLY: You're talking about the results and you're talking about how we got the results. And as you said, we've had a lot of briefings on this. Just in ten minutes just get us to--

MR. TREGONING: If there's no questions I

can go through the whole presentation in ten minutes.

MR. SHACK: There will be some questions.

MR. SNODDERLY: But I think as a goal we ought to try to get there.

MR. TREGONING: I fully support you.

CHAIRMAN APOSTOLAKIS: Yes, we don't need this slide, for example. Next.

MR. TREGONING: Okay. We've seen that slide a million times. This is a factor that we use--

CHAIRMAN APOSTOLAKIS: What happened to 8?

MR. TREGONING: Well, you said -- oh, 8 is a hidden slide, so you don't need that one either.

CHAIRMAN APOSTOLAKIS: Oh, okay. You decide.

MR. TREGONING: Yes. It's your packet. We can talk about it if you like.

CHAIRMAN APOSTOLAKIS: No, that's fine.

MR. TREGONING: It's just more definition of how --

CHAIRMAN APOSTOLAKIS: Go on.

MR. TREGONING: So this flow chart just shows you how we broke up or considered the various technical issues or structured the technical issues for dealing with the elicitation. And we split them in passive and active system LOCAs. And the passive

system LOCAs were further subdivided by piping, nonpiping contributions. And then the rest of these small blocks get into the individual variables that we identified as a group as being important to the LOCA frequency contribution for both piping and nonpiping issues.

The elicitation questions, as we move to-- and again, I'm jumping ahead again. We go from slide 9 to slide 13. The three slides in your packet talk about the base case analysis and --

MR. SHACK: Well, I'd like to come back to the base case.

MR. TREGONING: Now or --

MR. SHACK: Well, it's as good as time as any.

MR. TREGONING: Okay.

MR. SHACK: One of my concerns with the base case, or at least what I want to understand, when I look through the base case I find dependencies on diameter that are 1.5 for the people building essentially the Belczey and Schulz kind of stuff like roughly a factor of five for the people doing the probabilistic fracture mechanics. Dick Chapman does probabilistic fracture mechanics, but somehow he ends up with the Belczey-Schulz thing rather than the

Harris one.

Your final estimation uses an intermediate dependency on D, which is like a 3.4.

MR. TREGONING: Yes.

MR. SHACK: You took geometric means of the frequencies, is that the average of the dependency on D went from 1.5 and 5 to 3.4?

CHAIRMAN APOSTOLAKIS: Just to understand that. Is that 3.4 divided by the diameter gives you the conditional probability --

MR. TREGONING: Conditional probability given --

CHAIRMAN APOSTOLAKIS: Okay.

MR. SHACK: No, no. That's the D probability. I mean, when I just do the plot versus D in your final draft, I get 3.4

MR. TREGONING: No. But a ratio of 3.4 from one size to the next.

MR. SHACK: Yes, to the next.

MR. TREGONING: That's what you mean.

The base case results were developed, again these were idealized results that were developed for a specific set of conditions using a couple of different methods. So they ended up with different ratios for those specific base case conditions.

Now for the individual elicitations themselves, one of the things we said, we said for each person pick a set of base case values. You can use one of these four or if you choose, you could come up with something of their own. Some people that amalgamated of the four different methods, they came with an amalgamated method that used information from both of them. Some people didn't use the base case estimates at all.

So what happened is each individual gave us essentially a ratio between each of those sizes, and sometimes they were constant and sometimes they weren't. And what happens when we amalgamated by taking the geometric mean of all the experts for any given LOCA category, that's how we end up with that final bottom line number. So if the ration was whatever it is, 3.4, that's based on an aggregated or an amalgamated response from all the experts at that point.

MR. SHACK: Okay. So it really is the geometric mean of the product and you treated all experts equally?

MR. TREGONING: Yes, equally. That's right. That's correct.

MR. SHACK: Even if they were wrong?

MR. TREGONING: That's correct. Even if they were wrong. But again, we have no real -- no real way. If we knew who was correct and who wasn't correct, we wouldn't have done this process. We would have taken our model and predicted LOCA frequencies and we would have been done.

MR. SHACK: There was one base case that would have been interesting. Why you didn't let Wilkowski do a base case where he took the conditional probabilities from fracture mechanics, because that's the part of fracture mechanics I believe, versus the initiation models which Peter would argue and Praise, you know, highly suspect.

MR. TREGONING: Yes.

MR. SHACK: And then add in the empirical occurrence of cracks from Galyean and Lydell and you have a base case where -- you know, because Galyean and Lydell have lots of data for their initiation, then they take Belczey-Schulz which comes out of the air and, you know, it's connection to reality is never quite clear and it's probably quite conservative. And so you have a realistic one and a nonrealistic one. And then in your Praise codes you have a nonrealistic one probably with a realistic estimate of the relative probability. Why not combine the two?

MR. TREGONING: We could have. For the base case analysis, again, we were trying to be simplistic in the sense that we wanted to give people a sense or --

MR. SHACK: The numbers really come out of the base case.

MR. TREGONING: No, no, they don't. The numbers come out of the elicitation. The base cases are just a starting point.

You see, what you just described, Bill, we had experts on the committee that did exactly that. And if you would have been expert, because I see you working through your mind, that would have been the approach that you would have decided to take. And we fostered that approach in the elicitation.

I had several experts that did exactly what you just described.

MR. SHACK: The numbers didn't come out of the base cases. The absolute numbers had to come out of the base cases. Everything else is a relative waiting.

MR. TREGONING: Yes. But, again, (a) there was no single set of base case numbers that were applied. Again, we had multiple different base cases. We had four different base cases for piping. We had

all these different precursor events for nonpiping. For any given question, let's say they were evaluating IGEC and the feedwater in the reactor coolant, in the main resert piping to depict one set of base cases. If they were evaluating FAC in the feedwater line, they could have picked another set of base cases. They could have picked results from, you know, one expert that calculated IGC for this system. They could have picked results that another expert calculated for thermal fatigue in another system. So these weren't constant things.

Again, the base cases weren't the starting point. The elicitation were just a starting point. Given that that's the base case, how should they be modified to account for reality. And that's what those relative ratios were actually decided -- were actually designed to do.

Okay. They're predicting a frequency of ten to the minus six using this model with these limitations, these assumptions and this approach. So when you modify that, you have to make an assessment for how accurate you think they approach assumption and model is. That's part of what goes into the ratios.

But again, we set the elicitation up

because we didn't want people to provide us just raw frequencies estimates, because there's a lot of work in elicitation that shows you can provide relative estimates ratios where the ratios combine different conditions. And that's what we tried to get them to think about. Different conditions. You know, what are the conditions that would lead to higher or lower frequency estimates than the base case estimates? Well, it could be different water chemistry. It could be a different model that you use to account for the conditional failure probability of having a LOCA of this size. And so there were a lot of different conditions that get rolled into those ratios.

So you're right in the sense that they don't come directly from the base cases. They start from there. Start from there, but again they're really based on individual expert opinions and their bottom line estimates.

MR. SIEBER: And it's the geometric mean then. And you actually did --

MR. TREGONING: For the baseline results. When you get the numbers, it's --

MR. ABRAMSON: Well, that's the issue of how you aggregate all this.

MR. TREGONING: Yes. Yes.

MR. ABRAMSON: It's very different thing, and we'll talk about that later.

MR. TREGONING: Okay. So I'm already behind, again.

Quickly we've talked about this. We asked questions in the elicitation on the following topical areas. We asked each expert to evaluate the base case evaluation that the subcommittee did, the subcommittee of four people, the four experts did. We asked them to provide us information on regulatory and utility safety culture. Again, only pertaining to LOCA initiating events. And then we asked a series of questions designed to quantify LOCA frequencies of piping components and then LOCA frequencies of nonpiping components.

Again, we asked for two things: quantitative responses and qualitative rationale. Again, all the questions in the elicitation were relative to these chosen set of base case conditions. Again, but these weren't necessarily constant conditions. They could have been highly variable. They could have choose among the four estimates that we gave them or they could have developed their own.

Each question we asked them for mid value and then low and high value estimates. And we had two

different elicitation structures that they could follow depending on their expertise of the top down or bottom up approach that go into a lot more detail in the NUREG about that.

But as important as the quantitative responses are the qualitative rationale. We asked for rationale to support all the quantitative assessments that were made and in the elicitation we really examined inconsistencies between the quantitative answers and the rationale. And we brought those to the panelists' attention.

I can tell you in all the individual elicitations we found some inconsistencies that required the experts to go back and modify their estimates in order to be in line with their stated qualitative responses.

The next slide. This is a very, again, high level look on how we analyzed the responses. We calculated individual estimates for each panelist. And by individual estimates, we got total BWR and PWR LOCA estimates. Total means that we combined the piping and the nonpiping contribution. We decided to this because this approach was the most self-consistent and it allowed us to get estimates associated with each of the various experts.

Then we've got all these individual estimates and we have to aggregate them at some point in some way.

CHAIRMAN APOSTOLAKIS: But you assumed that the low and upper bound of the expert gave, you will repeat the 95th percentile --

MR. TREGONING: Yes.

MR. ABRAMSON: Well, except for the overconfidence adjustment.

MR. TREGONING: Yes, we did do an overconfidence adjustment that's separate. But we treat all their responses --

CHAIRMAN APOSTOLAKIS: No, but in the final result the adjustment is included, the overconfidence adjustment is included?

MR. ABRAMSON: Yes.

MR. TREGONING: In the baseline result it's not, but we talk about the effect of overconfidence on the baseline results in the NUREG report. The baseline results --

CHAIRMAN APOSTOLAKIS: Again, if I am the decision maker --

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: -- which curve do I use and does it include the --

MR. TREGONING: The decision maker curves have included the effect of overconfidence, yes. Yes.

MR. ABRAMSON: The error factor adjustments.

MR. TREGONING: The error factor adjustments.

MR. FORD: So that means that when you look at the LOCA frequency --

MR. TREGONING: Yes.

MR. FORD: -- they would use the worse case scenario because that would take into account the bad guys, the Davis-Bessie people, the people who don't use work chemistry or --

MR. TREGONING: I want to understand your question. By "they"?

MR. FORD: The decision makers.

MR. TREGONING: Okay.

MR. FORD: If you look at this box in the executive summary, because George is saying that's what they're going to look at, do you use another fact at the same time or are they using --

MR. TREGONING: Well, you know, do you want to take this or --

MR. ABRAMSON: I don't know what NRR is going to be doing, so I can't tell you what they're

going to suggest to do that.

MR. DENNING: I think there's an important interpretation question here. You're assuming the 95th percentile represents a range from among plants as to what it would be in a bad plant.

MR. TREGONING: No.

MR. DENNING: That's not true at all in the -- it's his assessment of -- I think, of a generic plant as to what that range of uncertainty is. He doesn't know what the true model is.

MR. TREGONING: Right. But there are -- not only uncertainty, but again but also accounting for broad plant specific differences. Not the single rogue outlier plant, but broad differences.

MR. FORD: Because when I asked the question before, what about the bad guys --

MR. TREGONING: Yes.

MR. FORD: -- you said well it is factored in -- at least your quota is factored in. That's not true?

MR. TREGONING: No. Again, we explicitly -- and maybe I wasn't clear earlier. Explicitly said we don't want the balance to reflect a single plant. We want you to reflect broad, you know, uncertainties--

MR. SHACK: It says two plants.

MR. TREGONING: What's that?

MR. SHACK: It says two plants.

MR. TREGONING: Yes, two plants. You know, we want to consider broad plant -- we asked them to consider broad plant specific differences but not again the individual plant or just a small handful of hands so that there are two factors of uncertainty that go into the percentile estimates. One it's the uncertainty that each expert has for how accurate is my mid value response. That's one component of uncertainty that is incorporated in there. The other factors is okay, now what additional uncertainty do I have because, again, for IGSCC different plants have different water chemistries, different plants are doing different mitigation strategies, different plants may have different inspection strategies. So both of those components of uncertainty are incorporated or we asked the experts to incorporate those in their evaluation of the bounds.

DR. WALLIS: Can you tell us how important they are. How relatively important are they? The differences between plants a small part of their variation or do they count for most of it?

MR. TREGONING: That would depend on the

expert. I mean, I would say that that's highly -- that -- I don't know that I could make a general --

DR. WALLIS: So I can understand there are differences between plants. But when experts have very different opinions about things, you presume they're all wrong. And you know, I'm not quite sure what I should do with that. But if the differences are due to plant variations, which they all understand, then that's much clearer to me.

MR. DENNING: Which one is Galileo?

DR. WALLIS: Yes. Which one, yes. So how important are these various between plants compare with the fact that the experts don't know what they're doing?

MR. TREGONING: I would say a lot of the uncertainty, again, to try to make as general a statement as possible. A lot of that uncertainty is due to the fact that it's difficult to quantify these estimates.

CHAIRMAN APOSTOLAKIS: Do you use different theories?

DR. WALLIS: Different theories. Again, the plant specific differences can play a role. But the bigger role is the uncertainty that they have.

DR. WALLIS: About what theories, what

methods that they use --

MR. TREGONING: No. But for each expert, a lot of their bounds were, you know, if I make -- for instance if I run -- and I'll use analogy that some of the people can appreciate and some can't. If I run a probabilistic fracture mechanics analysis to try to take into account of subcritical cracking how quickly these things might evolve, those models are unbelievably sensitive to the input assumptions that you have as well as your modeling methodology. So you could end up with very wide error bands, even for a relatively simplified set of conditions.

MR. BONACA: That figure that a little bit to that figure that we skipped on page 11 where we have the piping base case --

MR. TREGONING: Yes. Yes.

MR. BONACA: And a huge spread. And then there is a bunch of -- you know, you described a process to go forward. And then we have to page 18 which you haven't covered yet, but at some point I would like to understand how do you get this huge band of uncertainty. I know there is a lot of -- but how do we get from that to this? I've got to understand, not so much the time but the uncertainties.

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: I think there is a lot of interest in your actual results and maybe we can stop this discussion of how things were done and if necessary while we look at the results we can come back to how.

MR. TREGONING: That's fine.

MR. BONACA: That's a good suggestion.

CHAIRMAN APOSTOLAKIS: By the way, in order not to have the wrong impression about Galileo, this is a good example of somebody being right. There is a counter examine. Because a lot of people always think that there is a Galileo somewhere. In the seismic arena, there was one expert. The results for years. He was awfully conservative. And the NRC being a federal agency assigned an equal weight to all the experts. EPRI came with the results that were on the other side, complete paralyses for ten years.

NUREG 11.50 produces two sets of results. EPRI, Livermore. And we all knew what Livermore meant. Now I think the community, the expert community in that field believes that what that expert did was not reasonable. It was awfully --

MR. TREGONING: Of the mind.

CHAIRMAN APOSTOLAKIS: -- of the mind. And in fact, I believe even he himself finally

gradually agreed that maybe he was way too conservative.

So the issue of assigning equal weights and going with the most conservative guy because we are regulators, we're supposed to be conservative, you know there are counter examples to that. You know, and in the seismic case it was really an excellent example. I mean for about ten years people didn't know what to do.

And I remember when the new production reactor was considered by DOE, it made a hell of a difference in the cost, whether you went with Livermore curves or with the EPRI curves. And there was one guy, okay.

And on that happy note we will reconvene at 9:44 a.m.

MR. TREGONING: We're going to renew some of that past experience.

(Whereupon, at 9:28 a.m. a recess until 9:44 a.m.)

CHAIRMAN APOSTOLAKIS: Rob, would you continue?

MR. TREGONING: Yes. We're at slide 15, but I'm going to -- if there's no question, the next two slides I've presented in the past. They document

some of the more generic or general qualitative insights that support the elicitation. I'll just move past this because I think we're going to -- we've got a lot more ground to cover and I think there's going to be a lot more questions.

So if we could move to slide 17. You've seen this in the past as well, but I think it's good just to show this again for those who haven't.

DR. WALLIS: Well, I was going to ask you about aging. You said that aging may have an effect. Isn't aging something which was really understood?

MR. TREGONING: Well, I talk about aging may have the greatest effect on intermediate type sizes. The whole elicitation dealt with aging. So by aging I mean all the various generic issues that could have --

DR. WALLIS: Well, aging isn't something different. Aging is the same thing.

MR. TREGONING: Yes. It's exactly the same. I just used that generically to describe the fact that all --

DR. WALLIS: Okay. It's not something else. It's nothing else?

MR. TREGONING: No. It's not something new.

DR. WALLIS: Okay.

MR. TREGONING: It's not something new.

What I show here quickly are the mean and the 95th percentile estimates. These are aggregated estimates now, and by baseline results we mean they've been aggregated using the geometric mean of the individual panel's estimates. So there's no accounting for, at least in these estimates, differences of opinion among the experts, okay.

DR. WALLIS: But you're going to show us the means later?

MR. TREGONING: Yes, later. The next slide -- well, I'm sorry could you repeat the question?

MR. BONACA: The question was, you know, we are presented on page 11 with the packing base case so many results.

MR. TREGONING: Yes. Yes.

MR. BONACA: A huge spread. And then we move onto this results. And I show the ones at the next page and they're much more converged. Could you tell me how we managed to do that?

MR. TREGONING: Well, again, the base -- I will say the results on slide 11, those are old results. They have converged somewhat. They're not

quite as bad as they look. However, we discussed in the base case evaluation meeting some of the reasons for those differences. So some of the difference are really due to limitations on the current modeling procedures that were employed. All the experts recognized that, and they all agreed that the differences that you got in the base case evaluations was probably exaggerated and here's why it was exaggerated.

DR. WALLIS: Well, I think the reason it's come together is because you forced it to be log normal does something about the 95th percentile. And it's the tail, it brings the tail in.

MR. SHACK: No. I mean, for example on his BWR2 case, the guy with the ten to the minus 16th considered only thermal fatigue. The guy with the higher number considered FAC.

MR. TREGONING: FAC, right.

MR. SHACK: And so there's no reason that the numbers should be even in the same galaxy.

DR. WALLIS: The reason this looks so broad because you've made it log normal.

MR. DENNING: I don't think it's just that. I think it's also the geometric means of the --

MR. TREGONING: We're going to get into

that. We're going to get into that. This is done by taking, again, the geometric mean of all the individual panelists' response. This particular curve, like I said, there's no measure of variability among the panel given by this result at all.

MR. BONACA: The point I wanted to make is that before, you know, during the exchange I believe Shack mentioned that still, I mean, those are the base case. To the degree to which you have it in a report, okay, with the proviso, that someone has gone more with certain effects than others do, you know you're puzzled when you begin to move to this -- so --

MR. TREGONING: Again, base cases are just a starting point. And each expert had to believe -- they had to make a selection as to what base case they thought was more appropriate for them to experiment.

MR. BONACA: And that is more the concern that I have that, you know, these were individual evaluations.

MR. TREGONING: Yes.

MR. BONACA: Then the group got together, they began to -- there is a normalization process on the part of the team that works together that tends to probably look like almost what I would call a herd effect. I mean, people converging.

MR. TREGONING: Yes.

MR. BONACA: Is there something of that in here?

MR. TREGONING: No, no, no. Not at all. I mean, this curve was developed, this was our -- these were estimates that were developed after the elicitation using all of the individual estimates. There was no feedback where the experts sat down and tried to rectify or minimize the differences that they had in their individual estimates.

MR. BONACA: Okay.

MR. TREGONING: We explicitly did not want that because we wanted to make sure we got as much variability and uncertainty as we thought were, you know, applicable for these type of estimates. So, no, we specifically did not attempt to get any sort of consensus estimates from the group as whole, you know, sitting in a room and saying I think as a group we're going to take a vote and we think LOCA for 2 inch breaks, it should be ten to the minus four.

MR. BONACA: I didn't mean it that way. I didn't mean it that way.

MR. TREGONING: But there was nothing, you know, even conceptually like that at all. It was these were just developed, again, based on the

processing techniques for aggregating the individual results.

Now if you look at the next slide, you get more of a sense for some of the differences among the experts because we've included not just the geometric mean of the individual panelist estimates by these curves, but we also have 95 percent confidence bounds about each of those points. So you get a sense for how wide the variability was among the various experts. And as you can see here, if you look at it, for instance the PWR case is a great example.

If you look for the very small breaks, there's pretty tight confidence bounds associated with those results, relatively. Because, again, this is closer to our operating experience. There's not an expectation that that operating experience needs to be significantly modified.

But then when you get all the way down to the bottom when you're looking at the biggest LOCAs, there's a lot of variability there. And, you know, if you look at the 95 percent confidence bounds, there's about two orders of magnitude plus or minus the means.

So, you know, I would argue these results do not suppress the uncertainty that the experts have given us, or the variability that was apparent among

the different estimates that we got from a plant.

CHAIRMAN APOSTOLAKIS: But there is a question here now. The decision maker who will pick the transition size --

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: -- we look at slide 17 or slide 18?

MR. TREGONING: Slide 18.

CHAIRMAN APOSTOLAKIS: Eighteen?

MR. TREGONING: Yes. Right.

CHAIRMAN APOSTOLAKIS: And that's all you're giving that person?

MR. TREGONING: Well, no.

MR. KRESS: We're giving them the whole report.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: Well, yes.

MR. TREGONING: You know, the intent is not to just hand these things over to NRR like giving them the car keys and saying, you know, have at it.

MR. KRESS: Or a 16 year old?

MR. TREGONING: Or to a 12 year old.

DR. WALLIS: Tell NRR that.

MR. TREGONING: No, and it's not -- and it would be unfair. I mean, it would be unfair to do

that. We've tried to provide them not only the curves, but again we've tried to give them a full understanding of the elicitation process, it's limitations, how you use the results. Not all of that is in the report, of course. But, no, when we provided these results to NRR, we've had working groups between Research and NRR that have lasted for the last two years. And they're well informed of the process. They are -- we've had lots of discussions on how these results should be used, how you could use them, what are pros and cons of using these baseline results versus some of the sensitivity analysis results that I'm going to show later.

DR. WALLIS: Are these done as times of frequency for calendar year per plant?

MR. TREGONING: Yes.

DR. WALLIS: Per plant?

MR. TREGONING: Yes, this is essentially per plant.

CHAIRMAN APOSTOLAKIS: But wouldn't it be, though, useful here to actually try to get a consensus curve on the experts? Again, if I go to the SRL and he tells me use the frequency distribution from the experts, and the mean value of that, I would have to develop it from this information, won't I?

MR. TREGONING: Yes. If you were going to follow that approach.

CHAIRMAN APOSTOLAKIS: So you've given them what they want?

MR. TREGONING: If we would have developed consensus estimates, which would have been one approach we could have used --

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: -- but we would have been necessarily suppressing the uncertainty value.

CHAIRMAN APOSTOLAKIS: No. Because the experts would be fully aware of these uncertainties and then they might say, okay, given the uncertainty of the 95 percentile you bate it and so on, and say okay this is our best guess. Because now the Commission wants a distribution of the frequency of LOCA.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: And you're not giving it to them.

MR. ABRAMSON: If you want a consensus, I would submit that the closet thing to a consensus is essentially it's indistinguishable from it, I think, in this report would be the --

MR. TREGONING: Geometric.

MR. ABRAMSON: -- geometric mean of the results. Because it's in the center of the expert, of their opinion. If we're going to use -- I mean, what is a consensus? A consensus is something that the group can, if not agree on, at least live with. And I would submit that the only thing that they could love with, it's got to be somewhere in the center of the distribution. It cannot be --

CHAIRMAN APOSTOLAKIS: Well, we don't know that. We don't know that.

MR. ABRAMSON: Well, I know --

MR. TREGONING: We do know that in the sense that we -- when we had the BPC feedback meeting, we presented the results of these baseline results and some different ways of aggregating, especially specifically using the arithmetic mean instead of geometric mean, there was a hue and cry from the experts when the arithmetic mean was --

CHAIRMAN APOSTOLAKIS: Yes, I know.

MR. DENNING: Now wait a second, because again I want to go back to the arithmetic mean. I agree that you get a consensus with the geometric mean when you're all done.

MR. TREGONING: Right. Right.

MR. DENNING: But I think that by not

using the arithmetic mean I think that you underplay the model that's associated with the more conservative people, whether they're conservative or they're real, we don't know. But I think that you have really driven down the very large uncertainty that will exist in being able to model these things by taking the geometric means of, for example, the 95th percentiles as well as the medians.

MR. TREGONING: Right. But again --

MR. DENNING: I think you would see a much larger dispersion that's more representative of real dispersion of knowledge that exists if you're taking the arithmetic mean.

CHAIRMAN APOSTOLAKIS: I think all of this discussion assumes that you have to do some sort of a mathematical method to process this information. What I mean by consensus is the people sitting in the room debating these things and coming up with some distribution that everybody is not happy, but maybe not too unhappy with the results. It's the result of a deliberative process, not necessarily an arithmetic mean or geometric mean and whatever.

MR. TREGONING: Right. And we did get some feedback when we presented the various estimates. I mean, you get a sense from the panel. We didn't want

to develop these curves by committee because, again, the experts that we have are expects in the subject matter. They're not experts on what this distribution should look like. This distribution was developed based on their raw input, so --

CHAIRMAN APOSTOLAKIS: But if I look at slide 17 and 18 I get very different impression regarding what the current space of knowledge is regarding --

MR. TREGONING: Well, of course, because we don't present anything about variability in this slide. So, of course, as well you should. And that's why we explicitly calculated these confidence bounds to express the difference of opinion among the experts. We're not trying to suppress that here. It's just a different way of looking at it than you get if you create like a mixture distribution, which we're going to look at later, which I think is what you're --

DR. WALLIS: Well, why does the 95 percentile so important? Why not 99th or some other percentile?

MR. ABRAMSON: Well, the 95th percentile is an expression of the individual expert's uncertainty about their results.

DR. WALLIS: Well, we've got 100 reactors. Maybe I need to worry about one in a 100 rather than five in a 100.

MR. ABRAMSON: Well, the experts were asked to talk about the so-called generic the bulk of the reactors and so on. And all their responses were focused on that. So we asked for their mid values and their uncertainty bounds on the mid values on their medians. So I think the best interpretation of the 95th and the 5th percentile is the individual expert's uncertainty about their responses. That's what it is. And then when we talk about diversity, we're talking about the difference between experts.

MR. TREGONING: But just to follow up, we could have processed these results and term it any percentile we wanted.

MR. ABRAMSON: Yes, right.

MR. TREGONING: The problem is because some of these distributions were so greatly skewed, the further out in the percentiles you try to calculate, the more the assumption of the distributional shape becomes important.

So the 95th is relatively robust in terms of that consideration again.

MR. ABRAMSON: And also --

MR. TREGONING: There are pragmatic reasons for limiting it to the 95th as well as, you know, theoretical reasons as well.

MR. ABRAMSON: Well, and I wouldn't say theoretical, but the traditional reasons. I mean, traditionally the 95th percentile has been used in an upper bounds and used -- in particular, the NRC has used it. So that's why we picked that.

DR. WALLIS: You're establishing a tradition. It hasn't been very much. But it has been used.

MR. ABRAMSON: It was used in 95.95 and NUREG 11.50, I believe used the 9th percentile estimates and so on.

MR. TREGONING: So, no, we're not trying to establish policy with this, certainly.

Okay. Let's get into some of the more interesting discussion that I think people would like to see. And there's been a number of sensitivity analyses. Given the time that we're at, I don't want to go into great detail into all of these. There's a lot more detail in the NUREG report.

I think what I'll do is --

DR. WALLIS: It doesn't make much difference, though, does it really?

MR. TREGONING: Oh, it can make a huge difference. And you're going we're going to see this.

DR. WALLIS: Really?

MR. TREGONING: I'm going to focus on two areas. We did sensitivity analyses in five areas, and I've listed them here. We looked at the effect that the distribution shape has on the mean. We looked at the effect of overconfidence adjustment. We looked at the effect of the correlation structure that we applied to the panel's responses. We looked at different methods of aggregating expert opinion. And we look at panel diversity measurements.

Of these five, I'm going to try to tackle two, maybe three. Let's look at the overconfidence adjustment, the aggregating expert opinion and the panel diversity measurements. I think those are probably the most interesting. Certainly if there's questions on the other areas, we can cover those as well.

We're going to go to slide 23. And this is the overconfidence adjustment. We've talked a little bit about this already. It's well know that experts are generally overconfident about their uncertainty. So another way of stating that is people tend to underestimate their true uncertainty.

Now, this has been demonstrated, as Lee likes to quote from the research, many times in studies evaluating elicitation results using almanac type questions; the questions where you ask people where you actually have an answer that you can use to evaluate their response and how accurate their response is.

The rule of thumb here, and again it's no -- there's no hard and fast rule, but the general rule of thumb is that the true coverage interval is approximately half the nominal coverage interval. So the implication is we asked in the elicitation for a given response to give us the 90 percent coverage interval. So essentially an interval at which your response is not likely to -- you know, there's a ten percent chance that the true response could be different from your coverage interval.

Well, the implication from this various research is that the true coverage interval that we get is somewhere around 50 percent. Okay. And I think if you look at the research, this can vary quite dramatically from 30 percent up to 70 percent depending on the specific questions in the elicitation and things like that.

So certainly because of this known fact we

wanted to evaluate the effects of doing an overconfidence adjustment on the results. So how did we do that?

Well, we never altered or adjusted any of the mid value responses that we got from the panelists. So whatever they said their best guess was for a particular response, we never altered that at all. What we did do is we evaluated adjusting their bounds for the individual responses. And we looked at two different ways, again much more detail in the NUREG.

We looked at an ad hoc method where we actually individually adjusted the coverage intervals of their individual estimates, and then we looked at a more quantitative estimate where we adjusted the error factors associated with their bottom line responses.

So we did all the processing. And one way we did the processing, got their final estimates, come up with error factors, we adjusted those error facts. And the other way we went back to every particular response and adjusted them individually.

CHAIRMAN APOSTOLAKIS: So when you adjusted the other factors, are you going to show?

MR. TREGONING: I'm going to show you.

Yes. And I'm really going to -- the only one I'm going to talk about now is the error factor adjustment. If I go back, the other adjustment where we did broad adjustment, we have to make a lot of assumption about how overconfident they were. And that's not a -- again, other than this rule of thumb, it's not an easy thing to quantify.

So with the error factor adjustment we let the rest of the experts do our correction for us, in a sense. So how did we do that? Well, with the error factor adjustment, you can look at the philosophy on page 24. We compared all the individual estimates with the group estimates for uncertainties or the group estimates for error factors. And based on the difference between any individual and these group estimates, that would determine how much adjustment they got.

MR. ABRAMSON: And the group estimate for the geometric means.

MR. TREGONING: With the geometric -- so we took all the error factors for all the different LOCA categories for all the experts and calculated the geometric mean of all those different error factors. That's what documented in this table here. So this is the geometric mean of all the individual panelist's

error factors.

Now what we did for panelists that error factors below the geometric mean, we adjusted them up to the geometric mean of the panel. So we didn't change their median responses, we changed their error factor. For all those that had error factors above the geometric mean, we didn't do anything. We just left their responses as is. So we only adjusted those experts that had uncertainty that was less than the group average.

MR. DENNING: And why didn't you adjust them all? They all under estimated their --

MR. TREGONING: We tried that, and it was clear when we tried that that some people did not under estimate their uncertainty.

MR. DENNING: Oh. Well, how would you know that?

MR. TREGONING: When you do broad adjustments of the results, some of the results had such large error factors associated with them, that when you do a broad adjustment the results aren't supported by the operating database anymore.

MR. DENNING: So --

MR. TREGONING: And you would end up with mean values -- the distributions would get incredibly

skewed.

MR. DENNING: Yes. But you know what it's telling you is that the uncertainty here is extremely large.

MR. TREGONING: Of course.

MR. DENNING: Is what it's telling you.

MR. TREGONING: Yes, of course.

MR. DENNING: Yet but now you're artificially narrowing it.

MR. TREGONING: No, no, no, no. We're not artificially narrowing anything.

CHAIRMAN APOSTOLAKIS: Why don't you got slide 23.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: Wouldn't that be--

MR. TREGONING: Go back?

CHAIRMAN APOSTOLAKIS: Yes, go back and go to the section subbullet in the middle there. The true coverage level is about 50 percent. Take each expert's estimates --

MR. TREGONING: We did that. We did that.

CHAIRMAN APOSTOLAKIS: And then instead of, you know, assuming it's 95th or assume it's 75th--

MR. TREGONING: We did that.

CHAIRMAN APOSTOLAKIS: And then proceed. I mean --

MR. TREGONING: We did that.

CHAIRMAN APOSTOLAKIS: And what did you get from that?

MR. TREGONING: Nonsensical estimates.

CHAIRMAN APOSTOLAKIS: Why? Nonsensical according to whom?

MR. TREGONING: According to not only the shape of the distribution, again, they just were unsupported results. We document those results in the report.

CHAIRMAN APOSTOLAKIS: I don't understand what it means unsupported.

MR. TREGONING: Well, a couple of things happened. One, when you do these individual adjustments, the mean gets dramatically adjusted. Much more so than the percentiles. So you had many estimates where you could end up in the extreme with mean frequencies predicting for maybe a double break LOCA of, I don't know, ten per year or something like that.

CHAIRMAN APOSTOLAKIS: No.

MR. TREGONING: Well, okay. Well, that's what I meant by nonsensical.

DR. WALLIS: So maybe it's not nonsensical and it's telling you something.

MR. TREGONING: Yes, it did tell us something. It told us that that overconfidence adjustment was not appropriate.

DR. WALLIS: Because you didn't like the answer, either.

MR. TREGONING: Because it didn't make sense.

DR. WALLIS: If you think that they really can tell the difference between the 95 percentile and the 75th percentile in their judgment, I think you're wrong.

MR. TREGONING: No, I'm not making that claim. I'm not making that claim.

DR. WALLIS: Well, you just said if we interpreted it to the 75th percentile that you got--

CHAIRMAN APOSTOLAKIS: Yes, this is a very old result.

MR. TREGONING: Yes, there's nothing new, there's nothing new here.

CHAIRMAN APOSTOLAKIS: What you're trying to do is really confirm --

MR. TREGONING: There's nothing new.

CHAIRMAN APOSTOLAKIS: When people give

you their upper and lower bounds, they're really biasing it towards the 25th and 75th. This is a pretty good result, in fact, you know sort of ground. But I don't understand why if you apply it, you get nonsensical results.

MR. TREGONING: Because the distributions that we got from the experts, they're skewed. They're highly skewed in some cases.

MR. ABRAMSON: I think it is because the initial results are very -- their orders of magnitudes it not only different between the upper and lower bounds that the experts give us for the individual responses, and we multiply this and we add them up, we combined them. And that's the way it works out.

You have a great deal of uncertainty and then if you like -- the process, I wouldn't say magnifies it, it reflects it. It reflects it. The result reflects the uncertainties.

MR. TREGONING: The thing you have to be careful about with elicitations is even though this rule of thumb is, again, it's old hat, there's no -- you know, there's no agreed upon way as to how to correct for these estimates. And there's tremendous variability depending on the elicitation structure and the specific questions as to what that true coverage

interval is.

CHAIRMAN APOSTOLAKIS: And the question is--

MR. TREGONING: And if you look at the literature, and you know this better than me, you see ranges of amounts of under estimate of uncertainty that can vary from 30 to 70 percent. The implication on the results between 30 and 70 difference, a difference in under confidence estimation is huge. Tremendous.

CHAIRMAN APOSTOLAKIS: But all this discussion about huge and tremendous and so on, I wonder if that's reflective on this slide on page 17 and 18? I mean, because these distributions and -- you know, again, the decision makers are not expert of these things.

MR. TREGONING: That's right.

CHAIRMAN APOSTOLAKIS: They're not aware of the values bases and so on.

MR. TREGONING: That's right.

CHAIRMAN APOSTOLAKIS: So when they look at these results, in fact make a decision, the question is how much information do they have? Are they fully aware of these uncertainties?

MR. TREGONING: Yes, these particular

results that I'm showing on 18 don't include any overconfidence correction.

CHAIRMAN APOSTOLAKIS: Yes.

MR. TREGONING: But, again, this error factor correction that we employed, we do correct the results based on that. And those were supplied to decision maker --

CHAIRMAN APOSTOLAKIS: Well, let me understand this then, because I'm a little confused. Which slide do you think the decision maker will rely on? You say this does not result, which slide is your final result that somebody at NRR would have to really use it as his basis for making a decision? Is it 18?

MR. TREGONING: You know, I don't explicitly have that slide here because we were walking -- we wanted to walk you through the different changes that could occur.

CHAIRMAN APOSTOLAKIS: But in the report then, is there a figure somewhere that if I were the NRR probably making the decision, would be my baseline?

MR. TREGONING: It would be the baseline results. Our recommendation would be the baseline results that are corrected for overconfidence using

the error factor adjustments.

CHAIRMAN APOSTOLAKIS: Okay. So it's slide 18 here, but then there is another slide where there is a correction?

MR. TREGONING: Yes. Yes. There are other slides in the report that are correct.

CHAIRMAN APOSTOLAKIS: Looks very similar. But which one is it? Can we identify in a figure?

MR. TREGONING: Yes. Go to section H.

CHAIRMAN APOSTOLAKIS: Section H?

MR. TREGONING: Yes. Section H. And look in the section -- if you've got the report --

CHAIRMAN APOSTOLAKIS: Yes, we have the report. It would be nice -- oh, correction results.

MR. TREGONING: The figures H-21 and H-022.

CHAIRMAN APOSTOLAKIS: Now these are the main figures --

MR. TREGONING: These would be the main figures, yes.

CHAIRMAN APOSTOLAKIS: Okay. Thank you.

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: They're on page H-29. This is a very important --

MR. TREGONING: Yes.

CHAIRMAN APOSTOLAKIS: But they don't look very different. There's a slight upward motion.

MR. TREGONING: Yes. Well, if you look at the error factor adjustment --

CHAIRMAN APOSTOLAKIS: It doesn't look very different.

MR. TREGONING: -- scheme, it ends up, and I show it here --

CHAIRMAN APOSTOLAKIS: Maybe it's a factor two, but it doesn't show it in a large scale.

MR. TREGONING: There's about a factor of two.

If you look at the results here, the BWR there's about a factor of two difference with the baseline. For the PWR there's a factor of three or less difference with the baseline estimates.

MR. DENNING: But it does bother me that even in error factor correction, we only make that correction for people that are below the median of the things. We don't make it for everybody.

MR. TREGONING: Well, what would you --

MR. DENNING: You made the correction, if I understood it properly --

MR. TREGONING: Right.

MR. DENNING: -- anybody that was below

the median used --

MR. TREGONING: Mean not the median, but it's close. It's approximate.

MR. DENNING: But anybody that was above, they did not get a correction because if you've made that correction for them, then the results were dramatically impacted and --

MR. TREGONING: No, no, no. How would we have corrected them? Down?

MR. DENNING: No, no, no. You would have increased the error factor according to the difference--

MR. ABRAMSON: No, what you say is correct. I mean, we could have made an adjustment for those above the geometric mean also.

MR. DENNING: Yes.

MR. ABRAMSON: We just didn't do that. But that could be another sensitivity study.

DR. WALLIS: You should just present those results, though.

MR. TREGONING: Pardon me?

DR. WALLIS: You should present those results.

MR. TREGONING: They are presented. They're presented in the NUREG.

MR. DENNING: Oh, those are with an error factor for everything?

MR. TREGONING: There are, if you look in the adjustment scheme, we called blanket and target adjustments where we adjusted the coverage interval. The blanket adjustment is exactly that, it adjusts everyone to the same degree. It's not an error factor adjustment. We adjusted the individual responses to reflect from a 90 percent to a 50 percent.

MR. DENNING: But not using error factor.

MR. TREGONING: Not using -- no. We went into the individual responses themselves and adjusted all of them the same way. So those results are described in there.

DR. WALLIS: Of which curve summarizes those results?

MR. TREGONING: There's some -- I don't -- I don't have a curve like this, but there's some plots in there that show --

DR. WALLIS: Well, I thought you said when you went from 90 percent to 50 percent coverage, you got absurd results. I don't see any absurd results in this report.

MR. TREGONING: I don't present every single results for every LOCA category. We presented

selected results. I think I showed the case for LOCA category three.

DR. WALLIS: So why didn't you present them for LOCA category? It's just another way of presenting the results?

MR. TREGONING: We could have. We tried -- there's so much -- I think earlier you said this was an incredibly dense report to get through. And we tried to present things that were reported. And we tried to summarize every --

DR. WALLIS: But you're giving us summary curve to a decision maker which you've selected from a bunch of things you could have given this decision maker.

MR. TREGONING: That's correct.

DR. WALLIS: Why didn't you give him the one where he corrected everything from 90 to 70?

MR. ABRAMSON: Just let me try to respond to that. We're doing sensitivity studies, sensitivity analysis. And sensitivity analyses you examine excursions from your assumptions to see how they effect the answers, but you don't examine all possible excursions. We try to use a kind of a rule of reason and the same ones that seem plausible in some sort, or at least bound throughout possibility.

DR. WALLIS: You threw out the ones you didn't like.

MR. ABRAMSON: Pardon me?

DR. WALLIS: You threw out the answers you didn't like.

MR. ABRAMSON: No, that we didn't like that we felt were not supportable, that were not supportable, would not be accepted by anybody really. And therefore, we didn't see --

DR. WALLIS: And you left it to me to make the choice.

MR. ABRAMSON: Well, we could do that. We could say here is a complete range of stuff --

DR. WALLIS: But it in an appendix --

MR. ABRAMSON: We could have just presented the raw data for all of us. Right. But we tried to exercise some judgment here and to guide the reader and say, look, the range of sensitivity studies we've done, we feel in effect encompasses a plausible range that you would want to consider when you're making a decision. It's a very wide range by itself. SO we made that choice. And you're suggesting we could have made it broad --

DR. WALLIS: Well, what we're trying to do is to see whether you have artificially narrowed the

uncertainty range in a way which might be misleading to someone interpreting the results. That's what I think what we're trying to determine.

MR. TREGONING: But here you're trying to determine if we have corrected -- if we have artificially corrected the uncertainty range that was provided by the expert to a sufficient enough degree to account for some of these known rule of thumbs. But, again, these are rules of thumbs, there's no procedure. There's no standardized procedure that holds for the analysis of this stuff.

CHAIRMAN APOSTOLAKIS: Well, you have to-- I mean, that's what I meant earlier by giving the report that later -- or supporting the decision maker.

The SRM is very clear. It was not addressed to you but it was addressed to the big question of risk-informed in 50.46. And it gives a suggestion there that we may want to use the mean value of the expert opinion based this division. And you do work with all sorts of sensitivity studies and so on, but you don't provide that distribution to the Commission.

So maybe when you revise this as the result of -- after the public comment period, you have

to have that distribution which is your best shot at it. By telling me go to page H-29 and look at all these uncertainties, if I am a Commissioner, you're not helping me. Do you expect me to go to that distribution?

I think that, you know, in the future you should really seriously consider developing that distribution with all the caveats, you know, and all that because that's what the SRN says. You don't want to --

MR. ABRAMSON: Well, again, what you seem to be in mind intruding into is a whole area -- well, not intruding, but bringing it up, a regulatory application. Now, this report is just trying to report on the results of the expert elicitation. It's a separate job. And I guess that's what you're asking for is how you would use the results of this report for making regulatory decision --

CHAIRMAN APOSTOLAKIS: Well, that's not what I'm saying here. I realize that there is a separation of powers there.

MR. ABRAMSON: Yes, right.

CHAIRMAN APOSTOLAKIS: But you still have to provide the information that these other guys do.

MR. ABRAMSON: Of course.

CHAIRMAN APOSTOLAKIS: And if I go and read the SRM, it says the distribution from the expert opinion elicitation. And I look at this thick report and I can't find that distribution. You're expecting me to do it. And that's not -- I really -- I think you should consider seriously, because this is not the final job, right?

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: I mean, you go now to public comment and so on.

MR. TREGONING: Right.

CHAIRMAN APOSTOLAKIS: Maybe, you know, your stuff because I don't know whether you can go back to the experts, should try to take the figures on page H-29 and using some judgment say now if you really want a distribution, this is it. This is our best --

MR. SHACK: It gives them a table?

CHAIRMAN APOSTOLAKIS: Huh?

MR. SHACK: It gives them a table.

MR. ABRAMSON: Yes, but you see --

CHAIRMAN APOSTOLAKIS: Well, give them a table? What table is that?

MR. ABRAMSON: Well, George, I think what's happened. What you're saying is you would like

some information to assist the decision makers, in this case the Commission, in ultimately making the right decision. But I think the staff should only go so far as to do the decision maker's job for them. I think it would be a mistake in this whole regulatory process is to present the result and say, look, this is it, you got to use the mean value, and that's it.

CHAIRMAN APOSTOLAKIS: No, no, no.

MR. ABRAMSON: I think you have to present the results in such a way that the decision makers can exercise some judgment and also use the individual criteria they all have about how they want to focus this in, how much uncertainty, how much do they want to build into it, how much conservatism they want to build into the regulatory process.

CHAIRMAN APOSTOLAKIS: I can understand that.

MR. ABRAMSON: I think it's the proper job of the staff to present this in as clear a way as possible as to what the range of possibilities is and what the value, the arguments pro and con are.

CHAIRMAN APOSTOLAKIS: Is there a table of raw data of what the experts actually said?

MR. TREGONING: There will be in the final report.

CHAIRMAN APOSTOLAKIS: Because then we get a student from MIT will look at it and come up with completely different results from yours.

MR. TREGONING: Yes. No. We will have the raw data. But just to--

CHAIRMAN APOSTOLAKIS: I don't understand why you're resisting. I mean, why is so hard to give your best distribution when all sorts of --

MR. ABRAMSON: I don't know what a best distribution is. I don't know what this means. It's a vague term and we tried to avoid using that here.

CHAIRMAN APOSTOLAKIS: Page 24.

MR. DENNING: Let's look at page H-24.

MR. TREGONING: Just one thing of philosophy behind the NUREG. We tried to strike a balance. We've heard two different things. We heard Dr. Wallis say you need to present everything. And then we heard Dr. Apostolakis say you really need to boil it down to one curve that the regulators --

CHAIRMAN APOSTOLAKIS: And these are not inconsistent. I'm not saying that --

MR. TREGONING: Okay.

MR. DENNING: Let's look at page H--

MR. TREGONING: What we tried to do --

MR. DENNING: Let's look at page H-24.

CHAIRMAN APOSTOLAKIS: H-24.

MR. DENNING: Figure 817.

CHAIRMAN APOSTOLAKIS: Okay.

MR. DENNING: Okay. Now if I'm interpreting this properly, then this is looking, at least for these category three LOCAs, the variety of different ways that you've treated the data and the kinds of results you can get. And when you look at that, what it tells you -- what it tells me, and I'm overstating it for effect, is that it makes a heck of a lot of difference as to how you treat this elicitation as to how the regulator interprets it.

CHAIRMAN APOSTOLAKIS: Right.

MR. DENNING: And I'm not sure that there's a clear path forward where they can say, well, this one curve would be the one that would really help the regulator because reality is there's an awful lot of uncertainty here. And I fear that if you just show the curve that we've been seeing here like on page 18, it gives you a feeling of much more rigor and definitiveness than exists.

DR. WALLIS: You want to be careful, because now you may again publish the raw data. A lot of Ph.D students around the country can use those and come up with other results. If they come up with

results that differ substantially from yours, it puts the whole thing in question. So you've got to have a really defensible thesis.

MR. TREGONING: And again, I guess from an academic viewpoint, I would value other people looking at these results and looking at different --

DR. WALLIS: But you see what you mean?

MR. TREGONING: No, I see what you mean, and that's why the report is such that we tried to present the sensitivity analysis that effect the results most dramatically. I feel very comfortable in stating that if a Ph.D. student from MIT would look at this data --

CHAIRMAN APOSTOLAKIS: Let's leave MIT out.

MR. TREGONING: Ph.D. from St. Louis University would look at this data, that I don't think they're going to determine estimates that fall outside of the various bounds that we've described that are possible.

DR. WALLIS: So it's very robust. Your answer is going to be very robust?

MR. TREGONING: No. We've presented a wide range of possible -- of ways of looking at the data and possible bottom line estimates that you could

come up with.

DR. WALLIS: I think it's very important--

MR. TREGONING: Depending on how you decide to view this data.

MR. ABRAMSON: Depending on how you decide to apply it, obviously.

MR. TREGONING: We'll talk about the most fun thing. Everything's been a prelude to this so far when we talk about differences and variability uncertainty and what happens when you look at different ways of aggregating the --

CHAIRMAN APOSTOLAKIS: No, no. Let's come back to this, because it's really important.

MR. TREGONING: Come back to what?

CHAIRMAN APOSTOLAKIS: To what we were just discussing.

MR. TREGONING: Okay.

CHAIRMAN APOSTOLAKIS: I mean, the reason why it's called executive summary is because presumably the executives read that. And all you're presenting there is the pressure break -- the BWR and PWR baseline results. I don't see figures -- the figures from page H-29. I don't see the figure from page H-24.

Surely you're not expecting the decision

maker to go back to Appendix H --

MR. ABRAMSON: Section H.

CHAIRMAN APOSTOLAKIS: Section H to interpret what you give them in the executive summary. The executive summary should be the bottom line, should it not?

MR. ABRAMSON: I would suggest that this input is not for the decision maker, but for NRR. In other words, for the regulatory arm of the NRC to use. Because this is intended as input to the proposed rule and so on to the proposed regulation.

CHAIRMAN APOSTOLAKIS: Yes. Yes.

MR. ABRAMSON: And I think it really -- you're talking about, I believe, what should be I think a document which says take this and here is our regulatory philosophy and so. And this is what we use from this document as the basis for our proposed rule.

DR. WALLIS: Then you should with H-21 or something like in the executive summary, too. You should explain that there are at least two ways to look at this. This is the base case. This is our best estimate of how it should be looked at. And it's figure H-21 or whatever it is.

MR. ABRAMSON: What you're suggesting is that the executive summary should reflect the large

uncertainty and diversity of opinion.

CHAIRMAN APOSTOLAKIS: Yes, of course. Yes. Yes.

MR. ABRAMSON: Rather than just putting out something --

CHAIRMAN APOSTOLAKIS: Right.

MR. ABRAMSON: -- and saying this is our-- this is what we call our baseline results.

MR. TREGONING: We certainly say that in the executive summary, but I think what I'm hearing is they'd really like to see the curves themselves as well.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: And that's -- you know --

CHAIRMAN APOSTOLAKIS: Well, I mean, Dr. Denning just said that the figure on H-24 gave a very different impression of what is going on. The uncertainties are skewed and so on. And I don't see that in the executive summary.

Anyway, let's go on now.

Remember now, we have to finish by 11:00.

MR. TREGONING: Right. Can we stop the clock?

So let me try to go to slide 26, if I can. This is about aggregating expert opinion. And we

talked a little bit about this. The baseline method uses the geometric mean of the individual panelist estimates to give group estimates for all the total LOCA frequency parameters, and by parameters the 5th, 50th, 95th percentiles and then the mean estimates.

This methodology was based on the not only assumed, but also the structure of the individual elicitation responses do support a log normal structure. So the fact that the individual estimates were distributed essentially log normally. This baseline method assumes the estimates aren't significantly influenced by outliers. And the results that you could use other measures of central group opinion, either using the median or the trend geometric mean, if you look at the NUREG, you end up with those selections to get very similar estimates as you do with just the geometric mean themselves.

However, an alternative method is instead of the baseline method, is to use or use an arithmetic mean of all the individual panelist distribution and create essentially what's called a mixture distribution.

And it's a different philosophical principle and a different viewpoint for aggregating the expert opinion. You're essentially making the

assumption that the individual results are all obtained from equally credible models that are randomly picked from the population of expert. If you make that assumption, then the mixture distribution falls naturally.

And a point to be made here, though, is some of the key regulatory parameters may be dominated by the outlier. And one of the things that you see is certainly the difference between the 5th and 95th percentiles that I showed earlier and the 5th and 95th for this mixture distribution. The mixture distribution percentiles are much wider.

DR. WALLIS: Would it be true to say that this would be sort of explain to the public in saying that each of the experts could be equally right?

MR. TREGONING: Each of the --

CHAIRMAN APOSTOLAKIS: In both cases, the experts get equal weights.

MR. TREGONING: There's an assumption here or there's a nuisance here in that how you assign -- how you consider them being equally right various on your philosophy. I mean, if you assume that they're equally right in log space and you weight the opinions so that one opinion doesn't dominate, that might lead to the baseline --

DR. WALLIS: Then you're downgrading that one, yes.

MR. TREGONING: If you assume that they all have equally credible models, models not opinions, but models then that might lead you to this other methods.

It's a suitable yet it's an important philosophical distinction between these two different ways of aggregating the expert opinion.

DR. WALLIS: I want you to be conservative, you say you've got ten experts.

MR. TREGONING: Right.

DR. WALLIS: And two of us say that you should be up here and eight of us say you should be down here.

MR. TREGONING: Right.

DR. WALLIS: To be careful, we'll say maybe those guys are right.

MR. TREGONING: And that's the mixture distribution. Yes.

MR. ABRAMSON: I think again it's kind of a red herring in this context to talk about what's right and what's wrong. The purpose of this is an expert elicitation. And there's an unstated assumption in this that this is a worthwhile activity. And if

it's worthwhile, it means that you need to go to the-- you have to aggregate in such a way as to respect the philosophy of expert elicitation, which means you need to be near the center of the group.

Now, this is just a summary of their opinion. Whether it's a useful summary for regulatory purposes and so on, is another issue. And then if you want to build conservatism and so on and so forth, by all means do so. But it's separate from this, what we're trying to report on here.

CHAIRMAN APOSTOLAKIS: But the objective, I think, of an exercise like this is to show what the community of experts think they know about this issue. Is that correct?

MR. ABRAMSON: Well, that's one way to look at it.

CHAIRMAN APOSTOLAKIS: Well, that's the way in my view. I mean --

MR. ABRAMSON: We had a panel of 12. We had 12 representatives of this community in there.

CHAIRMAN APOSTOLAKIS: Yes. Yes.

MR. ABRAMSON: We did not explicitly ask them to try to judge what the community as a whole, we asked them for their opinions.

CHAIRMAN APOSTOLAKIS: And why not?

MR. ABRAMSON: This was -- well, why not? Well, I can give you my personal answer.

MR. TREGONING: We got to end by 11:00.

MR. ABRAMSON: Yes, we got to end. And I think it's difficult enough for them to come up with their own, with what they think in their own minds rather than to think what the community is thinking, which is another level of abstraction.

MR. BONACA: Could I make a point on this question? I have a question on the sensitivity analysis that I need to place at some point in this.

What I was looking for is, you know, in the elicitation process there are assumptions being made and stated by the experts which is actually things may improve because the safety culture may improve and also that, you know, ISI will continue the same way. There will be litigation, etcetera, etcetera. Well, however, I believe that there is a rule here that we'll have a transition break size and what will happen is beyond transition break size there is going to be relaxation of the environment. There is going to be relaxation of the environment stating that they realized that they may use 50.69 to do less inspections, less modification and so on and so forth for those kind of issues.

Have they looked at all -- I mean, I didn't see anywhere consideration of the potential impact of this rule on the fundamental assumptions of this studies, which were essentially that things were going to be -- you know, and we will have mitigating strategies and so on and so forth.

I just had to ask that question, and I don't know that if there is an answer to that. But I would need to address that.

MR. TREGONING: No, and we specifically asked them not to because if the elicitation were going to focus on that, we didn't know at the time and we still don't know how this rule's going to develop.

MR. BONACA: I understand.

MR. TREGONING: What specific -- and that's why we make it clear very clear in the elicitation that if you do things -- and here's how, at least programmatically I expect that we're going to handle this. If you do things in the regulatory space with respect to this rule, it causes -- you know, it undermines this elicitation, then yes, it could rain these frequencies muted that way. And the example I like to give is with it operates it BWRs.

Okay. When they started power-up rates to BWRs, you started seeing more vibration failures than

you had in the past. Well, this is why at least as we go forward with this rule, we're evaluating the precursor histories quantitatively as we go along. You can see things that are resulting in increases due to the various --

MR. BONACA: I guess we will have an opportunity to raise those questions at the full meeting.

MR. TREGONING: Yes.

MR. BONACA: We have a second meeting on the 50.46. On the other hand, I mean, to me it's a fundamental issue because --

CHAIRMAN APOSTOLAKIS: Yes, of course it is.

MR. BONACA: -- here you would go, we support this based on separate assumptions that may not be in fact consequences or the change of 50.46. And 50.46 might take us to a different environment, in fact the statement has been made it'll be the relaxation that degrades the very assumptions --

CHAIRMAN APOSTOLAKIS: As a matter of fact, I believe in the report it says someplace that the expert assume -- the experts assume that the programs that we have in place now to control aging mechanisms will remain in place.

MR. BONACA: Yes.

CHAIRMAN APOSTOLAKIS: So we need 50.46 revision to relax some of those, right?

MR. BONACA: But I think we have to make a distinction. --

MR. TREGONING: You have to make distinction.

MR. BONACA: Just as whole to really highlight it.

CHAIRMAN APOSTOLAKIS: Absolutely. Absolutely. Again, to write the rules.

MR. TREGONING: And again, it's not that you can't do relaxation. It's that you have to be vigilant an take great care that your relaxation doesn't effect the LOCA frequencies in some way. That's a different question. That doesn't mean that you can't relax. And taking an accumulator out may or may not effect LOCA frequency.

MR. BONACA: But, you see, I understand you're defending what you've done. But, you know, you have created a weapon here, okay. Potentially something here that could be used negatively, okay, or positively. And so we have to understand the implications of it. And I'm saying that, you know, there has to be recognition on our part --

MR. TREGONING: Of course.

MR. BONACA: -- a communication that there can be deleterious effects coming from --

MR. TREGONING: Any set of data, any model has the same caveats that have to be understood. There's no difference, probably more so here. And that's why we try to spell those out quite explicitly. I think we -- we must have done a pretty good job of that because there's no much concern amongst the panel members.

CHAIRMAN APOSTOLAKIS: Could you then when you revise the report emphasize this stuff when you say that the expert opinion -- that all the problems we have now remain in place -- if the rule decides to change, though, this is not part of the expert --

MR. TREGONING: The intent was certainly to emphasize it.

CHAIRMAN APOSTOLAKIS: I remember, though, there is something about the expert --

MR. TREGONING: But even in the executive summary --

CHAIRMAN APOSTOLAKIS: I don't remember anything about --

MR. DENNING: Could we look some more at this arithmetic mean versus geometric mean. I'd like

to get the feeling here of the Committee, because I mean I would think that -- and I'm not going to strongly argue with the arithmetic means better than geometric mean, but just the fact that it such a substantial difference for the results.

CHAIRMAN APOSTOLAKIS: Show the next slide, yes.

MR. DENNING: I think that we ought to be shown. I mean, you know this I think has to be up in the executive summary with the other results, I think. Now whether the other committee members think that or not, I don't know.

MR. TREGONING: Right. Right. Well, again, it's certainly -- the intent is not to suppress it and that's why it's in the report. You can see here with these curves, these just -- there's no plain old variability. These are essentially just the best estimate mean curve, so to speak, aggregated in different ways.

The blue curve are the results that we had seen using the geometric mean aggregation. The red curve represents aggregating using the arithmetic mean. And it shows, again, the way you aggregate can significantly effect the frequency.

DR. WALLIS: Well, it makes a different.

You've changed your local category by one. If you changed it -- for an order of magnitude, local frequency --

MR. TREGONING: Or more. I mean the biggest difference is with the BWR frequencies.

DR. WALLIS: Right.

MR. TREGONING: The differences between category two, three and four LOCAs. I don't show the 95th percentile, but the differences are roughly at the same order of magnitude.

And I've just quantified them here. Okay. So all this table shows is a ratio of the mixture distribution results compared to the geometric mean aggregation for both the mean and the 95 percentile. Of course, the mixture distribution will always lead to higher means. That much we know. So they're always higher. Increases are generally less than a factor of ten with a few notable exceptions. And they really stand out, it's PWR LOCA categories 5 and 6, which are the biggest LOCAs. And then the BWR LOCA categories 3 and 4.

What you find when you go in and look at those results, not surprisingly, is that when the differences are biggest between these two different aggregation schemes, it's a reflection that you have

one -- usually one, sometimes two panelists that are significantly higher than the rest of the group.

Okay. These BWR frequencies are driven essentially by one panelist in terms of the arithmetic mean. Especially the LOCA categories 2, 3, and 4. So that's important to understand that.

The same thing with LOCA categories 5 and 6 for PWR. There's one or two panelists, I think two panelists there, that end up being the prime contributors to the arithmetic mean result in the sense that their distributions of the mean are most reflective of these communal -- of these aggregated distribution of the mean.

MR. FORD: How does the expertise of one of these -- well, of all of these experts -- into this? The reason I bring it up, is if you look at the members of the panel, there's really one -- expert, all the rest are mechanical engineers.

MR. TREGONING: And maybe the experts knew while their specialty is not --

MR. FORD: I recognize that. But for instance, coming back to my specific comments earlier on, the environmental chemistry contents to this analysis are obviously pretty important.

MR. TREGONING: Of course.

MR. FORD: And yet in my opinion there's really one of those panelist understand the nuances of for instance presence of salt that could be -- it will change at times. In fact, this is mentioned in the report. But I'll bet you that only one of those panelists knew about that.

MR. TREGONING: No. Because again the concerns that were raised, especially the concerns that were raised in the report, those were raised to the entire panel and some of the differences that you can get.

I can tell you that those concerns are not the reason for the high estimates in the arithmetic mean.

MR. FORD: Okay. Well, was that the reason why I brought that --

MR. TREGONING: Yes. Well, that I can tell you for a fact. And again, the arithmetic mean relates -- once you decide to use a mixture distribution, you can drive yourself nuts by trying to see well are people outliers, are they not outliers, should I weight people differently. You can really get yourself spun around developing different schemes potentially for either including or ignoring the outlier results.

MR. FORD: Not I seem to remember, somebody, but it was mentioned in the report some panelists recused themselves from some decision because of lack of knowledge.

MR. TREGONING: Yes, of course.

MR. FORD: How does that affect this arithmetic --

MR. TREGONING: If we didn't get estimates, they're not included.

MR. FORD: Yes. But the population is that much stronger --

MR. TREGONING: Well, we had 12 panel members. Nine panelists gave us PWR estimates. Eight gave us BWR estimates. For an elicitation that's a pretty good sample, actually. So multiple gave us both, and at least -- there was one expert that we didn't get any quantitative information from, only on safety culture. On some of the safety culture questions, but not on anything else. And then we had several panelists that either gave us PWR and BWR estimates based on their experience and expertise.

So, no, we did ask them to self censor or recuse themselves in areas that they just didn't have the background and the knowledge.

CHAIRMAN APOSTOLAKIS: But I'd like to

emphasize again that there is nothing mathematically rigorous about aggregating using -- I mean, geometric and arithmetic means. There are outcrop methods. But the thought just occurred to me at the beginning you gentlemen said that the method you followed was pretty much NUREG 11.50 or expert opinion. And NUREG 11.50 didn't use either of these methods. They took the distribution from each expert and then they added one to each X, they went back. They took the arithmetic average of the --

MR. TREGONING: That's the mixture distribution.

CHAIRMAN APOSTOLAKIS: Is that the mixture the same here?

MR. TREGONING: Yes. The same thing as the mixture distribution.

CHAIRMAN APOSTOLAKIS: So when you say arithmetic average, you don't mean that to go to the 95th percentiles and take the arithmetic average --

MR. TREGONING: No, we did. We did --

CHAIRMAN APOSTOLAKIS: Well, that's what's confusing.

MR. TREGONING: We did both. What I'm presenting here are the mixture distributions.

CHAIRMAN APOSTOLAKIS: Okay. So that's

NUREG 11.50.

MR. TREGONING: It's a straight NUREG 11.50.

CHAIRMAN APOSTOLAKIS: So there is -- well, I mean, when you go with the geometric mean, you work with each person --

MR. TREGONING: That's right.

CHAIRMAN APOSTOLAKIS: But the mixture means that distributions themselves?

MR. TREGONING: No. It ends up if we did the -- if we did the arithmetic means of averagings of the percentiles, the 95th arithmetic means being ends up being pretty close to the mixture distribution 95th percentile. The means, of course, are identical.

CHAIRMAN APOSTOLAKIS: Right.

MR. TREGONING: It's only the lower distribution that, of course when you get an arithmetic mean of the 95th percentile, it's rated by the highest 5th. But the mixture distribution is rated by the low method. So the 5th percentile varies dramatically --

CHAIRMAN APOSTOLAKIS: You see this is another example now of what I was saying earlier. I remember the guy who was a -- arguing very forcefully that taking the arithmetic average in the sense we

just discussed was, you know, very reasonable and this is really what we ought to be doing. Now you guys come a few years later and you say here regulatory parameters may be dominated by outliers and you are sort of rejecting that method.

MR. TREGONING: No. I would suggest that the argument of 11.50, whatever it is, did not take proper account of the fact that this is expert elicitation. I think the key element in my judgment is never to forget that this comes out of an expert elicitation. And if you're going to use an expert elicitation, I think it seems to me you are required-- and the philosophy of it is to take the center of the group, not an outlier. Because if you don't, then you're not going to get agreement with the group. And the center is, if you like the median, we didn't use that although we did that for some of our initial calculations we gave. But the geometric mean is a very good approximation numerically --

DR. WALLIS: There is ad hoc, there's no theory of expert elicitation, there's no history of --

MR. TREGONING: No, there's no real theory of it. No. And also should I point the results we get are much broader in range to the one two. Sometimes three orders of magnitude that you would

have in most expert elicitation as far as the uncertainty is concerned. So in this sense there really isn't very much precedent to draw on as far as being able to aggregate this. We do have some information, and we haven't had a chance to get to this yet, about the review we did, the external review. We had a decision analyst, we had a statistician, two people are doing it. And there's some evidence, the decision analyst basically I think agreed to --

DR. WALLIS: How do you bring this in to help the public? And the public would view it. And if you had a lot of experts and terrific disagreement, then the public attitude would be very different from what you've done here, I think. They'd say we don't believe any of those expert people. We'd be very careful.

MR. TREGONING: Well, this is a very difficult area --

DR. WALLIS: They wouldn't look for the mean.

MR. TREGONING: We're talking about this process of expert elicitation, which is relatively new and, you know, it's a very, you know, difficult thing to accept. And I think for the experts themselves I

thought we had to do, you know, in a way a selling job or at least to explain to them and try to get buy-in from the experts into the whole expert elicitation process itself. And a lot of this, that we had training questions and so on and so forth. And at the end I think we did get reasonable buy-in and agreement that the whole -- and we did ask them very explicitly on feedback meeting how do you feel about this whole process, having gone through it. And I think there was general agreement that, yes, it was a valuable process. And we didn't ask them the same question at the beginning, but I think if we had asked them right at the beginning how do you think this, you going to buy-in, I think we would have gotten very different results.

You have to have gone through it. You know, George, people who have gone through this, it's a process. And you have to see, you know, this will lead to reasonable results, so to speak, does it really reflect the opinion of the group and so on and so forth. And that's really what we're trying to do here. This is a very difficult thing to do.

CHAIRMAN APOSTOLAKIS: There is only 14 minutes left.

MR. TREGONING: I know.

CHAIRMAN APOSTOLAKIS: You have to choose your messages from now on very carefully.

Dick, did you have a question?

MR. DENNING: I have a quick question. What evidence is there that the NUREG represents a consensus among the experts?

MR. TREGONING: Well, again like Lee had said, the philosophy of the elicitation was to come up with central estimates and group opinion.

MR. DENNING: Have they reviewed the NUREG?

MR. TREGONING: Yes. When you say choosing your message, I want to go to the next few slides. Sorry to do that.

Okay. I'm going to jump to slide 31 really quickly. And then I want to talk -- there are two different reviews we've given. There's a review of the elicitation panelist themselves and then there's also a review that were done by external reviewers that had no a priori knowledge of the elicitation structure result until we brought them in. So as I mentioned, there's a preliminary version on slide 31 that was distributed to the panel in July. We did a video teleconference over about a two day period with all the panel members where we sent

through every point, section of the NUREG as well as the results. And we had a number of, I felt, very good revision suggestions that came out of that.

And the NUREG that you have has ben modified significantly compared to what was first distributed in July. So, again, and we had suggestions generally in areas on the backgrounds, the approach, the base case results, the analysis, the qualitative insights and then the quantitative result section. And the point to make is most or just about all except of these 50, maybe one or two, we didn't incorporate for various reasons. But all of these revision suggestions were reflected in the version of the NUREG that you all have.

This updated version that you have has also been circulated a second time to the peer reviewers for comments. And we've gotten only a very little --

MR. SHACK: To the panelists?

MR. TREGONING: I'm sorry, the panelists. To the panelists. And we've only gotten a few additional relatively minor comments. I think they had it about a mon th ago.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: So we've done one level of

review there.

The next slide, we did a level of external review as well. We had two reviewers who selected, again a decision analyst as Lee mentioned, and a statistician. And we really asked them to focus on the analysis of the expert results and the quantitative result section of the NUREG. And we wanted them to emphasize the methods and really examine the methods that we used for aggregated group opinion, because we obviously knew it had such great importance.

So, again, we asked them to focus on these areas because they're most important. We also asked them if they wanted to comment on other NUREG sections, but we wanted to focus the external reviewers on the analysis and the processing of the result.

Just quickly with approach. They had the same preliminary draft NUREG that the panelists had in early July. They reviewed this for about a month. And we had a two day kick off meeting in August after they had read the report, and in many cases already had an initial set of questions that we had to answer for them.

But we had the kick up meeting and we were

all well up to speed as to what was in the report and what was done.

We got some informal comments, not only at that meeting, but then also a week or so later from the external reviewers. And we got reports from both of them in mid September. We had a wrap up meeting to discuss these review reports at the end of September. And after that time we've asked them to finalize their external review reports.

I've got one of the two are finalized. The other one's not quite finalized, which is why I've got November of '04 for finishing that effort.

We referenced the review reports in the NUREG and we are going to certainly be making it publicly available after they're finalized.

I wanted to jump to selected conclusions from the external reviewers. Again, our one decision analyst, and he was the only one -- we didn't ask him to do this but he felt compelled to do it anyway, but he thought or thinks that the elicitation process is adequate and sound for determining or for meeting the stated objectives --

CHAIRMAN APOSTOLAKIS: Can you tell us whether the objectives are stated in the report? I'm sure they are somewhere, but I --

MR. TREGONING: The objective and scope sentence section of the NUREG.

CHAIRMAN APOSTOLAKIS: Which section?

MR. TREGONING: Section C.

MR. SHACK: Section C-1.

MR. TREGONING: Objectives and scope.

CHAIRMAN APOSTOLAKIS: All right.

MR. TREGONING: The reviewers concurred with many specific aspects of the analysis procedure. I think they both really liked the relative ratio structure that we developed to examine technical issues. In fact, the decision analyst thought that this was a model way to conduct these types of elicitation. And he also expressed that using this relative ratio structure, there's some evidence to suggest that this may help compensate for what tends to be overconfidence in other elicitation studies. So it's a way for potentially to minimize that, although again that's just an opinion.

They both agreed that the overconfidence correction using the error factor approach was appropriate.

They didn't like any of the other overconfidence adjustment schemes because they just felt like they were too ad hoc and severe.

We had a number of corrections that were suggested by the reviewers that we've incorporated in the NUREG, things from developing improved correlation structure bounds. We had a whole new section on evaluating the effect of distribution shape on the mean. We were using approximate formulas to calculate means. Now we're using exact formulas. It doesn't matter much, but for what it's worth they're exact formulas.

And in the interim we conducted a number of Monte Carlo simulations to look at the effect of not only the correlation structure, but also verify the approximate calculation procedure that we use to develop the final LOCA estimates.

We had way too many estimates to rigorously do all the simulations by Monte Carlo. So we had to take just a subset for checking. And we did that, there's a discussion of that not in your NUREG report, but this is a new section that we've added to reflect the latest. This is the last sensitivity analysis we did.

CHAIRMAN APOSTOLAKIS: Okay. Will we have these additions before the December meeting?

MR. TREGONING: Yes. Yes. Yes. They're almost in now.

Again, we had a lot of discussion and we developed a mixture distribution and aggregation team as a result of this review. And, again, there were numerous suggestions for clarifying exposition.

Continued on the next slide selection conclusion. You know, we've gone to the heart of this today and I think we've also, some of the stalemate that I'm sensing amongst you also exists, I'll say internally with the staff and also with the peer reviewers themselves in that there was no agreement reached on what was the most appropriate aggregation scheme.

One reviewer favored either geometric or the arithmetic mean, or the mixture distribution approach. But they did state some advantages and disadvantages of -- and I've only listed the geometric mean approach here because of the advantages of this approach or disadvantages of the mixture distribution approach and vice versa. So I only showed advantages and disadvantages of the geometric mean aggregation approach, which is our baseline method.

The advantages are that the group estimates are more acceptable to the panel. And, again, we got some feedback from this in the video teleconference when we presented both the baseline

method and the arithmetic mean aggregation schemes. But this technique may be, and probably is, most appropriate for low frequency events where the variability among panelists could span several order of magnitude, and that's certainly what we're dealing with here. And you have another advantage is that the results are not dominated by one or two outliers.

The disadvantages of the baseline approach is that you do end up with less conservative mean and 95th percentile estimates compared with the mixture distribution. And also the 5th and 95th percentile differences are not quite as wide as you get for the mixture distribution. Although when you factor in the 5th and the 95th with appropriate confidence bounds, you actually end with similar. You know, if you use the 5th with the 5 confidence bound and the 95th with the 95 confidence bound you get ranges which are actually pretty similar.

Again, the authors and, you know, we make no bones about it because we've been trying -- the stated objective all along was to come up with a central group opinion. We strongly favor the use of the baseline method to meet that stated objective. And some panelists also were very strongly in favor of that. So strongly that, you know, we had a lot of

input, there was concern that NRC was going to take and bias their results accordingly by not fully weighting them.

And a number of the panelists I said, you know, their opinions were so strong that I had just said that you need to see the report when it comes out. And if you feel that strongly, make sure that you comment during the public comment period.

CHAIRMAN APOSTOLAKIS: Okay. I really would like to go around the table here, so unless somebody has a very important question to ask of the staff, I suggest we start doing that.

Do any members feel that they have questions for the staff or shall we go around the table?

DR. WALLIS: I think the staff did a good job.

CHAIRMAN APOSTOLAKIS: Good. Well, thank you gentlemen. I think it was very lively discussion.

I'd like to go around the table to get the first impression that you guys have about this. Shall we start with Jack or Bill?

MR. SHACK: I think they did a good job. I kind of agree with the notion of using the geometric mean aggregation. It seems to me that since we don't

know the truth, somehow giving everybody's opinion some kind of comparable weight. I mean, in this case with an arithmetic average, you know, you're going to be dominated by the most conservative -- I mean, these things differ by orders of magnitude. It comes down to, you know, just sort of a sampling there.

I, by and large, think their results are weighted to the conservative side, so you know I think--

MR. TREGONING: Even with the geometric?

MR. SHACK: Even with the geometric, because your panelist I think will, by in large, using what I would consider conservative approximation. So your baseline methods are weighted conservatively. So I'm fairly comfortable with the results.

CHAIRMAN APOSTOLAKIS: That's it?

MR. SHACK: Yes.

CHAIRMAN APOSTOLAKIS: Okay.

MR. FORD: Like Bill, I'm very -- I think it was a very comprehensive report. I wish I had had more time to look at it in more detail.

I'm concerned the make up of the panel. It is a multi-op problem. It does involve chemistry, stress and that fully -- was that the make up of the panel.

And the other concern I have is I'm really concerned about a worse case scenario here. And I think it should be therefore much more plant specific to look at situation -- for instance, Davis-Bessie --

CHAIRMAN APOSTOLAKIS: What is your concern about the worst case scenario?

MR. FORD: That, for instance, you could have plants which are not implementing -- BWR plants which are not implementing effectively how to do chemistry, for instance, which would therefore effect cracking frequencies and potentially LOCA frequencies. But that's very plant specific and it's not all BWRs.

PWRs a question of whether they're using appropriately replacement materials, etcetera, etcetera.

CHAIRMAN APOSTOLAKIS: Okay. I understand. Anything else?

MR. FORD: No. That's it. Thank you.

CHAIRMAN APOSTOLAKIS: Rich?

MR. DENNING: I think the expert elicitation was done very well. I am concerned about the application and the treatment of aggregation and I disagree with Lee with regards to what the purpose of this is. I don't think it's real to come up with a consensus of judgment. I think it's to try to

characterize the state of knowledge that we have. And since you really can't get into weighting the different experts, I think that you have to -- I think that you at least have to show the arithmetic average. And I think it's really better the average myself anyway.

And I think that when we look at the potential applications of this, we have to be very careful to show the broad uncertainty that exists here. And I think that the way that the baseline treatment has been done, I think it really dramatically under characterizes what the true uncertainty is.

Now, where reality is, like Bill, I think it probably is down lower. But I think that there are lots of elements of the uncertainty that are minimized in the treatment here. I think the variability across plants is extremely important, difficult to deal with and maybe can't be dealt with directly there, but I think that it has to be recognized clearly what the limitations are.

Just because we've done a good expert elicitation doesn't mean that it's going to have direct applicability to regulation. I think it will. But I think that -- I completely disagree with the

statement here that says the advantage you should geometric mean aggregation when you have low frequency events with these very broad variabilities. Because I think what you're doing is you're again minimizing the real uncertainty that exists in our knowledge base.

CHAIRMAN APOSTOLAKIS: Thank you.

Mario?

MR. BONACA: Well, first of all, I would like to say I think it was a good effort. I have the same concern that has just been expressed here regarding the uncertainty range and the variability among plants. But that's, you know, that may be there.

I would like to address is from the underpinnings of this study, they need to be highlighted more in the executive report. I recognize that, you know, you meet your goal. And I think it's a good product. But in the executive report is it fundamental that there is a clear understanding in the message that there were limitations, active failures, component were not included. Seismic considerations have not been addressed in the report yet. And most of all the feedback mechanism that the development of the rule may create in the sense that if there is a relaxation of -- I mean, the underpinning was that things would continue to be as good or better in the

future as far as inspection, testing and so forth and so on. And that if in fact that changes, there is a significant effect. I mean, of what you have represented in the report when I read it over shows a significant sensitivity to that. So it's a question more of the communication part. I think it's important the executive summary contains that information there.

CHAIRMAN APOSTOLAKIS: Tom?

MR. KRESS: I think as far as the expert elicitation process and the way they went about it, I think they did about as good a job as could be done.

With respect to the choice of arithmetic mean versus geometric mean, as I've said before, for the purpose 50.46 I don't care which one they use. But, in general for possible other uses of this distribution, I would prefer the arithmetic mean because we don't how to provide weights to the given experts. And I think that does a better job of reflecting the consensus.

So, you know, for 50.46 I don't care, but there are other possible uses of this. I would rather see arithmetic.

CHAIRMAN APOSTOLAKIS: Arithmetic is 11.50, right?

MR. KRESS: Yes. I don't see any way the end can get to their ability across plants, frankly. So, you know, I don't fault them for not discussing that very much, because I don't know they'd go about doing it anyway.

I think the really nice thing about this is they do have a quantified uncertainty that I think, like Rich, represents uncertainty in the state of knowledge. And that's a good thing to have. I think you can make decisions with it.

And so I believe the curves they have in the Appendix H, for example, are the decision making curves and ought to be brought forward into the executive summary.

That's it.

CHAIRMAN APOSTOLAKIS: Dick?

MR. RANSOM: Well, I feel the real change benefits have been made unclear as far as I'm concerned, so I feel a little uncomfortable with that. But generally the results that the staff presented, I think were a good result, sound of the elicitation effort with the exception of there does seem to be a need for a regulatory -- the summary include how regulatory should actually use these data. And part of that I think the degree of consensus among the

experts is important, especially if this is the public that's to believe what they thought of all this. And I personally would feel more comfortable, I guess, with the more conservative arithmetic mixing of the data.

CHAIRMAN APOSTOLAKIS: Thank you.

MR. SIEBER: Okay. I'll be very brief. I agree, and I guess it's unanimous that a good job has been done by the staff.

One of the things that impresses me is the amount of work that goes into try to figure out how to statistical present the information when the basic information comes out of one's imagination, so to speak. And so we've very carefully arranged all this information so that it makes sense. But I have an uneasy feeling about the overall basis that's there. And that's okay, I guess.

I was struck by the fact that it seems to me like you're trying to assemble LOCA frequency data for an average plant from a regulatory standpoint is the worst plant that makes a difference, which makes the idea of safety culture -- you know, you're only going to have one big LOCA in this industry. You know, and then the industry goes away. So that makes the safety culture issue very important to me, because

that is going to be the major factor in the initiating event; either faulty inspections or lack of inspections or tolerance of leaks. And I think we all know how these things comes about.

One could say that it really doesn't make a lot of difference in what the transitional break size is or what the LOCA frequency is if all it's used for is to fiddle with things like defense-in-depth and single failure criteria, because the concept there is that the plants are supposed to be able to tolerate and mitigate any kind of a break other than a reactor vessel break. And therefore, the risk doesn't change a whole lot if the plant maintains the capability to do all these functions.

On the other hand, once this is published, who knows what bright young person will dream up to use this data for. And so there may be regulatory concepts that those of us around this table aren't even dreaming about, but somebody will say now that we have this, let's go do this. And the process of doing that may take you into unchartered waters where this expert elicitation may not be totally appropriate. So I would put that caution in there.

I guess in summary I think the job is well done, it's well documented. I think a lot of effort

went into it. I have faith in it, and so those would be my comments.

MR. SNODDERLY: George, do you want to make a couple of comments?

I just wanted to remind you that the full Committee meeting is going to be on Thursday morning, December 2nd, for half day. So right now the way the schedule is set up is from 8:35 to 10:00 a.m. we would be briefed on the expert elicitation. And then from 10:15 to 11:45 we would be briefed by NRR on the proposed rule, which would include the statement of considerations and the regulatory analysis.

Now, the point that I wanted to bring or the feedback I think that I would like to try to get to the staff or the recommendation I want to make to you is that there appears to be disconnect. Because we remember from our previous presentation from NRR, they said the way they -- it appears to me that they're going to use this report is they're going to say, well we looked at the distribution. I'm not sure exactly which one, but they said we looked at -- the mean was about a 5 inch break for a PWR and 8 inches for the 95th percentile. And then we decided to conservatively choose 14 inches. And so that's how they addressed the uncertainty, and that is one

approach. And with that you're not -- an understanding of the underlying uncertainties is not as important. But now if you use the report as the Commission SRM suggests, which is a mean value corresponding to an initiating frequency of ten to the minus fifth, well then now the report becomes -- and the understanding of uncertainties is crucial.

So I think, and what I heard from Lee was, well we're not going to tell them how to use that. And so what I want to suggest that you consider when we provide the staff feedback on how we do this presentation, well first of all one concern I have is -- and I know NRR on Tuesdays have their weekly meeting and they're busy trying to get the proposed rule together here. But it's a little discouraging that there wasn't heavy NRR participation here today. And I'll make sure that we feed this back to them, or you know what we want to hear on Thursday.

I think the best advise that we can give the Commission, I think what the Commission is going to be looking for is okay, I've been given this -- I'll call it a tool, meaning this expert elicitation and it's going to allow decision makers to decide where this transition break size should be. To help. Right. And if we use the criterion, I'm sure -- and

I'm assuming the Commission wants to the criteria they suggested in their SRM, well here I think the Committee should lay out what are some of the pitfalls or what distribution do we think you should use, or what further work needs to be done to give you a distribution. Maybe it's a further consensus study. But those are the things that we need to think about.

MR. BONACA: Lay out specifically today. I mean, the issue how do you bridge from this report to a transition break.

MR. SNODDERLY: Exactly.

MR. BONACA: You're covering the issues of active component have not been addressed, seismicity and the issue of what you do to these components beyond transition break. Are you going to decrease you inspection rate and so on and so forth. If you do so, all the underpinnings of the study are weakened and they're not there. So this is very important.

CHAIRMAN APOSTOLAKIS: It seems to me that what Mike is saying is though the presentations on December 2nd should be coordinated, right?

MR. SNODDERLY: Number one. And number two I think I'd like to emphasize the Office of Research to emphasize more to help us to try to identify those pitfalls or an understanding of what distribution and

the problems you may have using those distributions when you use the Commission's approach as opposed to the staff's. I think the staff's approach is fairly easily because I think we all feel fairly comfortable that a 14 inch break for PWRs adequately balance the uncertainties in the study. I'm not sure how I consider the uncertainties when I use the mean value corresponding to 10.56

CHAIRMAN APOSTOLAKIS: Right. No, coming back to my earlier comment of that distribution, Mike just reminded me. He said the staff looked at the distribution and they said, okay, the 95th percentile is 8 inches, we make it 14. Where is that information that the staff, they based their choice of 8 on? Does it come from your report or did they do something else. You see, we have to have that.

MR. BONACA: We have to understand how they read this report.

CHAIRMAN APOSTOLAKIS: Yes. Now the other thing is --

MR. TREGONING: That's laid out in the statement of considerations.

CHAIRMAN APOSTOLAKIS: Okay.

MR. TREGONING: And we focused today on just the NUREG.

CHAIRMAN APOSTOLAKIS: I understand.

MR. SNODDERLY: We're trying to decide here, I think, what do we really want to hear on.

CHAIRMAN APOSTOLAKIS: But the issue of uncertainties it might raise for everybody else.

It seems tome that we feel more comfortable the choice of 14, but when it comes to the technical matter it's all a matter of how uncertain are you about your results. The underpinnings that Mario mentioned and everybody else, you know, had various questions about the methods of this and that. Why? Because I'm sure that the choice of 14 will be challenged.

How did you decide to go from 8, 95th percentile, to 14? That means that you don't have much confidence in what the report says, not because it's a bad report but because of other reasons that I mentioned earlier, you know, and so on what's that about and all that. So the issue of uncertainties and the major assumptions, because these are really the major uncertainties is critical no matter which way you go. With the Commission choice, of course, it's even more critical. But still, you know, the choice of how much conservative defense-in-depth to impose realize or rests on what kind of uncertainties you

have.

MR. BONACA: And that would say the portions of the 50.46 portion of the elicitation should be really this discussion of the bridge --

CHAIRMAN APOSTOLAKIS: Yes. Yes.

MR. BONACA: -- how we got from this report to the recommendation.

CHAIRMAN APOSTOLAKIS: Very good.

We have another meeting coming up. So thank you very much again, gentlemen.

So we take only 45 minutes then.

MR. SNODDERLY: No, we can always start later. We can start that briefing at --

CHAIRMAN APOSTOLAKIS: We are the off the record.

(Whereupon, at 11:13 a.m. the meeting was adjourned.)

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