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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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529th MEETING

+ + + + +

THURSDAY,

FEBRUARY 9, 2006

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ROCKVILLE, MARYLAND

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The committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Graham B. Wallis, Chairman, presiding.

COMMITTEE MEMBERS:

GRAHAM B. WALLIS, Chairman

WILLIAM J. SHACK, Vice Chairman

GEORGE E. APOSTOLAKIS, Member

MARIO V. BONACA, Member

RICHARD S. DENNING, Member

COMMITTEE MEMBERS: (cont'd)

THOMAS S. KRESS, Member

OTTO L. MAYNARD, Member

DANA A. POWERS, Member

VICTOR H. RANSOM, Member

JOHN D. SIEBER, Member-at-Large

ACRS STAFF PRESENT:

SAM DURAISWAMY, ACRS Staff

JENNY M. GALLO, ACRS/ACNW Staff

JOHN G. LAMB, ACRS Staff

JOHN T. LARKINS, Executive Director, ACRS/ACNW, Designated Federal Official

CAYETANO SANTOS, JR., ACRS Staff

ASHOK C. THADANI, Deputy Executive Director, ACRS/ACNW

ERIC A. THORNSBURY, ACRS Staff

NRC STAFF PRESENT:

GREG CRANSTON, NRR

JOHN FORESTER, SNL

HOSSEIN HAMZEHEF, NRR

STEVEN JONES, NRR/DSS/STSB

ALAN KOLACZKOWSKI, SAIS

MATTHEW MITCHELL, NRR

NRC STAFF PRESENT: (cont'd)

ERASMIA LOIS, USNRC

LAMBROS LOIS, NRR/DSS/SBWB

ROBERT PETTIS, NRR

PAUL PRESCOTT, NRR/DIPM/IPSB

DALE THATCHER, NRR/DE/EQUA

JIMI YEROKUN, REES/DRAA

I-N-D-E-X

AGENDA ITEM	PAGE
Opening Remarks by the ACRS Chairman	4
Evaluation of Human Reliability Analysis (HRA) Methods Against Good Practices	8
Proposed Revisions to SRP Section 14.2.1, "Generic Guidelines for Extended Power Uprate Testing Programs"	89
FERRET Reactor Vessel Fluence Methodology	138

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

(8:31 a.m.)

CHAIRMAN WALLIS: Good morning. The meeting will now come to order.

This is the first day of the 529th meeting of the Advisory Committee on Reactor Safeguards.

During today's meeting the committee will consider the following: evaluation of human reliability analysis methods against good practices, proposed revisions to SRP Section 14.2.1, "Generic Guidelines for Extended Power Uprate Testing Programs," the FERRET reactor vessel fluence methodology, the draft ACRS report on the NRC safety research program, and the preparation of ACRS reports.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Dr. John T. Larkins is the designated federal official for the initial portion of the meeting.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's session.

A transcript of a portion of the meeting is being kept, and it is requested that the speakers use of the microphones, identify themselves, and speak

with sufficient clarity and volume so that they can be readily heard.

I have a few items of current interest. You'll note in the handout on current interests that several Commissioners made remarks that are described in the contents. And at the end of the table of contents you will note that there is a regulatory information conference. Our esteemed colleague, Dr. Kress, is on the program. And anybody else who wishes to go, please let the staff know.

I'm very pleased to welcome Dr. Otto Maynard, or Mr. Otto Maynard, to the ACRS. He is now an official member. Congratulations, and welcome.

(Applause.)

I also have to announce that this is the last ACRS meeting for our colleague Vic Ransom. On behalf of the committee, I'd like to thank him for his contributions and wish him good luck in his future endeavors, and good skiing out west.

(Applause.)

Now, to proceed with the meeting, I will invite Professor Apostolakis to get us started on the first item.

MEMBER APOSTOLAKIS: Thank you.

MEMBER POWERS: Professor Apostolakis, I

need to note that I have association with two of the three speakers. And, consequently, members should recognize the possibility of bias and prejudice in any comments that I might make.

MEMBER KRESS: Bias and prejudice in which direction?

MEMBER POWERS: I will not telegraph that.

(Laughter.)

MEMBER APOSTOLAKIS: But you will not keep quiet.

(Laughter.)

MEMBER POWERS: It has proved to be a genetic impossibility.

MEMBER APOSTOLAKIS: Okay.

MEMBER KRESS: You share the same genetics that George has.

MEMBER POWERS: Yes. Yes. We're brothers under the skin.

MEMBER APOSTOLAKIS: I didn't hear that, but I'm sure it was a very kind comment.

(Laughter.)

MEMBER KRESS: Of course it was.

MEMBER APOSTOLAKIS: Okay. The subject is the evaluation of human reliability analysis methods against good practices. The Human Factors and the

Reliability and PRA subcommittees had a joint meeting on December 15th and 16th where we reviewed the human reliability analysis program.

And what we will discuss today was part of it, but I think it's -- it would be interesting to the committee to give you a quick overview of what we did.

We had presentations on ATHEANA and the SPAR-H model, which is used -- was developed by Idaho, I believe, and is being used in the significance determination process. Then, we had some very interesting presentations on data and how to process them in developing numbers for a human reliability analysis. Idaho is developing a database where they develop the so-called timelines during an incident, what happened when, how did the operators respond, and so on.

And then, another interesting presentation was from Halden, where they ran experiments at their simulators. And one interesting result was that they found very -- not very, but in some instances they found significant aleatory uncertainty in the response time of the crews.

I don't remember the details, but let's say they had six crews from Sweden running, you know, the simulator or the same -- the same accident. And

in one case I believe four or five of them responded within five minutes or so. One of them took almost 12 minutes, double. The reason why this is important is because in the HRA models, in general, this aleatory uncertainty is ignored.

Then, I had a meeting with the Chairman, and he told me that there is renewed interest in HRA on his part, and he stressed that he would like to know how various groups -- how long it will take various groups to respond to an emergency and accomplish a task successfully. So time, again, becomes very important.

So this is the latest that we really have to focus on time, which will include the aleatory uncertainty. Time is included in most models right now, but it's included as a performance shaping factor. In other words, the stress level is high, there is a short time, and all that stuff. The probability is six.

There is a change in focus now that I think should take place where time is the actual random variable, the focus of the analysis, and all other things in the performance shaping factors would affect the distribution of the time. Now, that's not the subject for today, but I think it's important to

--

CHAIRMAN WALLIS: Presumably doing the right thing is also important.

MEMBER APOSTOLAKIS: Oh, in some instances it is.

(Laughter.)

Now, coming to this evaluation of HRA models, we reviewed the basic document that describes the good practices, and we issued a letter report in May of 2004 approving it for issuance for public comment.

Now, this new report that we have reviewed several models that are being used in the United States -- they did not include international models at this time -- against those practices. And I think it's commendable that two of the models that have been sponsored by the staff, the staff asked the contractor to review, which is, you know, you've got a more objective evaluation. These are, of course, ATHEANA and SPAR-H.

During the meeting, I think the members were very pleased with what they heard. We pointed out a few errors or statements in the report that should not -- that should be corrected, and the staff assures me that this is happening, although the

version we have is not the corrected version.

So I believe this is an excellent first step towards resolving this issue of model uncertainty in HRA. Although the intent of the report is not to do that, it's the first time that you see in one place an evaluation, fairly critical evaluation, of the various models that are out there and what they can do, what they cannot do, and so on.

And then, as it happens in these cases, you know, starting with the PRA procedures guide of 25 years ago, there are statements there because people feel they shouldn't really criticize too much that, you know, all the models have some usefulness at some point. If I were they, I would delete that comment, but maybe it's asking for too much at this stage. I mean, this is still -- this is still a good first step.

I don't know if the members who were present want to say something. I'm sure Rich does.

MEMBER DENNING: I just had a question, George. And that is, have there been any benchmark experiments with HRA models to see how they compare on a fairly specified --

MEMBER APOSTOLAKIS: Yes. There is an infamous --

MEMBER DENNING: -- problem?

MEMBER APOSTOLAKIS: -- experiment that was run by the ISPRA laboratory of the European communities at the time. It's now almost 25 years old. And they invited groups from the members, from the community, plus an American group, and they gave them an accident sequence in a German reactor. And it was a pretty serious exercise, by the way. A lot of resources were expended there.

And they were -- the teams were free to use any method they wanted. And the results are very interesting. I think you should get a copy of that paper.

Each team used more than one model, and the results were spread over two or three orders of magnitude. Then, the same model being used by different teams also gave results that were different by orders of magnitude. So two or three times I have raised the issue with our colleagues here from the staff that somehow we have to resolve that. That indicates that, you know, depending on the model and on the team, you can get very different results.

And I think, first of all, you have to appreciate that running such exercises is very expensive, and it's a major undertaking. It's not

something that the staff would say, "Yes, we'll do it next month." But I think the report in front of us a good first step towards focusing the attention on model uncertainties and the various assumptions the various models make.

And last time, much to my surprise, Dr. Lois said, "Well, we're thinking about it." You know, in the past, the comment was, "It's too old. Why do you bring it up?" But now it's different. I think the attitude is changing. Eventually, we'll have to do something about it. We can't just say, "Let's forget about it because it's 30 years old." You just can't do that. It's a very bad -- I mean, there is a table there that is really disturbing, seeing the results, you know, all over the place.

Now, I have talked to some of the guys who participated, and they complained that some of the teams used models that they didn't quite understand, and so on. But I don't know, I mean, there is always -- so I think this report is a good first step. It doesn't really pretend to be an exercise, a benchmark exercise, and it's not.

But pulling everything together, and maybe if the language is cleaned up a little bit, but I think in Rev 5 you will see comments like, "Don't do

this; do that." But it's too soon for this.

The PRA procedures guide failed miserably at that time, because they didn't want to offend anybody, and they said, "Oh. If you want to do statistical analysis, here is a bunch of methods." And, of course, only one survived, because only one had any logic behind it. But they didn't want to say, you know, this model is not good, because there are people behind the models.

Yes, Mr. Chairman?

CHAIRMAN WALLIS: Well, I'd like to hear from the staff, but I endorse what Rich asked. I read this report, and you can compare all of these things against good practices, but do they work? I mean, what's the evidence? And if it's expensive to do the test, maybe the test should be done.

MEMBER APOSTOLAKIS: Yes. The evidence that you want, knowing your background, will never materialize. You can't run experiments and compare with -- I mean, no, this is -- these are soft sciences.

I mean, you are trying to structure the judgment of people to do -- to make reasonable assumptions, and, of course, the evidence from the field, although with the database now that Idaho is

building, and the experiments at Halden, I think there will be a much greater dose of realism into these things.

But this is not a science where you say, "Okay. This is a model. Let's go and test it in the laboratory and see what happens." I mean, we have to appreciate the different nature of this.

If it's unreasonable what they produce, then the evidence from the field at some point will say, "Hey, you guys don't know what you're doing." But I have great hopes, after I heard Bruce Hallbert from Idaho presenting what they are trying to do, and I believe the results from Halden should be taken very seriously, because this aleatory uncertainty is pretty important.

And then, of course, the Chairman says, "I want to see something on that," and so that adds some momentum to this.

So with that short introduction, I will turn it over to -- Dr. Lois, is it, or -- okay. Who is next?

MR. YEROKUN: I'm Jimi Yerokun. I'm the Chief of the Human Factors and Human Reliability Analysis Section in the Office of Research.

We appreciate and thank the committee for

the opportunity to come here this morning to present the results of one of our efforts in the HRA area. And I think George -- Dr. Apostolakis has covered all of the introduction I prepared to give this morning.

So with that, we will just let the staff members go ahead with the presentation.

MEMBER APOSTOLAKIS: First of all, you pronounced my name in the Greek way, which is very good.

(Laughter.)

Second, this is the first of a series of meetings where the subcommittees will review the various activities in HRA. We are doing the same thing here that we started with the digital I&C. We had the first overview of the program last December. Now we are focusing on one of the results, and later on, in cooperation with the staff, we will define others.

So, Erasmia, please.

MS. LOIS: Thank you. Erasmia Lois with the Office of Research. John Forester and Alan Kolaczkowski are with Sandia National Laboratory and are helping us out to evaluate the human reliability analysis methods against the good practices that we developed last year.

In terms of outline, I will explain why we do this work, and then I'm going to get into the evaluation of the methods, describe the approach, present some of our own -- some summary results, and then John Forester will walk us through the actual evaluation of the various methods. And then, addressing the ACRS subcommittee meetings and also recommendations from internal review NRC staff.

We have two items here that actually are not covered in the report, and it is staying back and saying what we've seen, what is the overall evaluations we have, and given the limitations of these methods, what are the implications in the regulatory space, how we should use it.

And, of course, we have plans for next steps, so that includes the recommendations from the subcommittee to where we go from here, shall we address the ISPRA results, etcetera.

Why we do this work? Risk information is being used in regulatory space more and more, and the quality of the PRA is an important aspect on how you incorporate the results of the analysis into decisionmaking. The NRC has several activities going on in addressing the issue of PRA quality, and, indeed, has developed an action plan that describes

steps how we can both improve the decisionmaking on the basis of existing quality and also how we can improve the actual PRA quality.

Human reliability is identified as one of the areas that we have to address, and we have several issues on how we can -- we are going to address the HRA quality issues. We will summarize some of these activities.

What we are going to talk today is the development of guidance for performing and review in human reliability. The first step was to develop the good practices the committee is familiar with. We have talked and presented it last year, and now it has been published as NUREG-1792. The second phase was to, okay, given that this is how a human reliability should be performed, we will go back and evaluate the methods that we have with respect to their ability/capability to address these good practices.

And regarding status, we have a draft report that was submitted to the subcommittee and the full committee, and also, as I mentioned, internal staff review. And we would like to go to submit the final revised version for public comment by March of 2006. And, therefore, we would like to have a letter from the committee for going to public comment. And

we plan to submit for publication the final version in September.

MEMBER APOSTOLAKIS: Will we get a chance to see the final --

MS. LOIS: Absolutely. Absolutely. It will -- probably will have the final -- the version that is ready to go to public comment by the end of February, and we'll send it to you. It may not be in the format that --

MEMBER APOSTOLAKIS: No. I'm talking about in September --

MS. LOIS: Oh, yes.

MEMBER APOSTOLAKIS: -- or later.

MS. LOIS: Yes, absolutely.

MEMBER APOSTOLAKIS: The final report.

MS. LOIS: Yes. If the committee wants us, we can come back and --

MEMBER APOSTOLAKIS: Well, it's nice to see the report before it becomes final and is published. But you will not come here requesting a letter.

MS. LOIS: Sure.

MEMBER APOSTOLAKIS: Oh, you will. You don't know.

MS. LOIS: Just depends on -- we'll see.

But we'll certainly submit the revised version before --

MEMBER APOSTOLAKIS: Okay.

MS. LOIS: -- before we publish it.

MEMBER APOSTOLAKIS: Good.

MS. LOIS: Addressing comments, public comments, etcetera.

Approach. How did we evaluate the methods? The first step we did is just going step by step and comparing the methods with each individual good practice. And as Dr. Apostolakis mentioned, we did have an independent external evaluation of ATHEANA, SPAR-H, and also SLIM/FLIM, which are --

CHAIRMAN WALLIS: So, excuse me, what you mean by "evaluation" is you compared with good practices. So you went through the right ritual, essentially. But there isn't an evaluation in terms of comparison with how people really behave?

MS. LOIS: That's in the underlying model of each one of the methods. So, in actuality, going down to our approach, we did this as -- this step as an initial step. And then, we had a meeting where we presented the results and debated our findings.

In actuality, the expert meeting, which was quite impressive I guess in terms of the HRA

expertise that was present in that meeting, and included foreign HRA experts. Recommended that we have to go and look deeper into the underlying technical basis of the model in lieu of our understanding of human performance under accident conditions today, and evaluate that as well.

And also, another area that was recommended to discuss is the use of the method as intended versus how actually it has been used, because some methods are pretty good and provide guidance on how to perform it, and yet people were kind of sloppy in how they would apply the method.

And also, the expert meeting recommended that we have to take the lessons learned from these exercises and prepare for the next steps, where we go from here.

I have discussed about the internal review and --

CHAIRMAN WALLIS: Excuse me. You said evaluate -- these are methods only developed for the nuclear industry? I mean, there are presumably data from other parts of society about how people behave in emergency situations. Are these methods tested against those, or are they only used by experts in the nuclear industry in some way, and they're not

universal methods of some sort?

MEMBER APOSTOLAKIS: Yes. We keep talking about other industries, but the truth of the matter is that we are ahead of everybody else. There is -- the emphasis that the nuclear community has given to human reliability is not something you find in other --

CHAIRMAN WALLIS: I thought --

MEMBER APOSTOLAKIS: -- there is work on human factors and --

CHAIRMAN WALLIS: Airlines are very concerned about how --

MEMBER APOSTOLAKIS: Right. But not -- they are not killing themselves to develop probabilities.

CHAIRMAN WALLIS: But the pilots do kill themselves sometimes.

MEMBER APOSTOLAKIS: The emphasis here is on quantification. There is a community out there of applied psychologists and human factors experts, and so on, which is working on a number of industries. There's no question about it. But these people don't really bother to go to numbers.

What these models are trying to do is -- because you asked the question earlier, you know, how people really behave, to various degrees -- like

ATHEANA I think has really done probably the best job of any of these, where they looked at what the theorists and the applied psychologists said, what experiments they have run, and so on, and then they tried to use the useful results from that in creating, as you remember, the Air Force in context and -- but the last step of developing probabilities is, I would say, a uniquely nuclear fetish.

MEMBER DENNING: Yes. Other places look at human factors engineering --

MEMBER APOSTOLAKIS: Yes.

MEMBER DENNING: -- but they don't try to quantify the probability.

MEMBER SIEBER: It was my understanding that some of the data that went into the earlier models were not necessarily nuclear --

MEMBER APOSTOLAKIS: Absolutely, yes.

MEMBER SIEBER: And so, from that standpoint, there is a wider and more universal application.

MEMBER APOSTOLAKIS: Swaine and Gutman, in their classic handbook, they stated very clearly that, look, what you give -- we give you here comes from our experience with the airlines, nuclear, and, you know, a number of industries.

MEMBER SIEBER: It seems to me that ATHEANA is the only one that looks at the psychological processes individually -- you know, recognition and decisionmaking, execution, that kind of stuff -- and applies numbers to those sciences.

MEMBER APOSTOLAKIS: Well, others -- developers of other models might tell you that, you know, when we have the performance shaping factors we are doing the same thing.

MEMBER SIEBER: Right.

MEMBER APOSTOLAKIS: But the ATHEANA guys are more explicit. But the truth of the matter is if you go to the report, the basic report for ATHEANA, I mean, there are several chapters on the issues that you gentlemen are raising. And then, they move on to try to adjust it to the nuclear reality.

MEMBER SIEBER: Right.

MS. LOIS: Okay. This is the list of the methods we used as some -- it's just domestic matters. We have -- we didn't look at the -- in actuality, there are many more methods here, domestic methods, in what -- these represent the range of methods that we anticipate licensees will come in with applications. In actuality, these few methods are the ones that mainly are used now, so we focused on those.

MEMBER POWERS: I noticed that on your list there are several reports from EPRI.

MS. LOIS: Yes.

MEMBER POWERS: Is there one that EPRI particularly endorses now?

MS. LOIS: In what sense endorses?

MEMBER POWERS: If a licensee comes to EPRI and says, "I need to do human reliability analysis. What should I use?"

MS. LOIS: We had -- Jeff Julius gave us a presentation in this workshop. I don't think they are recommending any particular method.

MEMBER POWERS: They would just hand them all three reports and say --

MS. LOIS: That's right.

MEMBER POWERS: -- "Pick one."

MS. LOIS: But indeed they have what they call "calculator," which is --

MEMBER POWERS: Yes.

MS. LOIS: -- computerized method, and they include all of the methods they --

MEMBER POWERS: Yes. So they are ecumenical in their recommendation.

MEMBER APOSTOLAKIS: The calculator that Erasmia mentioned is actually a good step, again,

towards the formulation of some ultimate model, in the sense that it has specific steps that a user has to follow and do, and so on.

But it's very interesting what you say, Dana, because they have four models that are given as a choice to the user. But now, after a few years, it's emerging that, according to what they told us, one model nobody uses, for example. So there is a natural vetting, I think, of what is happening, because, again, they couldn't come out and say, "This is that model," because the other guys would get upset. But it's -- it's becoming, you know --

MS. LOIS: Yes. So apparently, in fact, this is an improvement.

MEMBER POWERS: How do I get into this field where they're so conscious of people's sensibilities and feelings?

MEMBER APOSTOLAKIS: We are sensitive people.

MEMBER POWERS: The field that I'm in there's no such deferential behavior.

MEMBER APOSTOLAKIS: We are very sensitive people.

MEMBER POWERS: I want to get into this field.

MS. LOIS: Well, in terms of -- shall I continue? In terms of --

MEMBER APOSTOLAKIS: Well, I actually -- I'm sorry. I -- when I first got into this field, I had the same questions. And I went to a colleague of mine who was working with something completely different, neutron transport field, and he told me that in the early days of reactor physics there were indeed several models being proposed by people for the same thing. But now, this has been, you know, 30, 40 years old, and it's --

CHAIRMAN WALLIS: Neutrons don't have psychology. It's different.

MEMBER APOSTOLAKIS: They are not sensitive. Yes. But it's interesting that even there they were different models, because you have to make approximations.

Okay. Back to reality.

MS. LOIS: Okay. We named them HRA methods. In actuality, most of the tools we have right now is just quantification approach, and it's a little bit misleading to call a method such as, I don't know, ASER an HRA method.

So I think I'd like to put my thinking here is one of the reasons for the variability that

we've seen in the HRA results and the lack of consistency, etcetera, is driven by the fact that people are under the impression that when they use a tool such as THERP, for example, they do HRA.

The good practices and the methods -- the guidance that SHARP and SHARP1 and EPRI had developed early on has always the intention that -- had had the intention that in order to do HRA you have to follow a process in a consistent manner. And it appears that that lack of consistently and correctly performing the HRA process is -- was one of the biggest contributions in the uncertainty of HRA results.

MEMBER APOSTOLAKIS: It seems to me that what you said is particularly true for SLIM/MAUD. SLIM/MAUD is not an HRA model. It's borrowed from decision analysis to quantify judgment. It has nothing to do with human reliability.

But because it was first applied to human reliability, everyone says SLIM/MAUD, or FLIM, whatever. But they have nothing to do with -- and even for the quantification of judgment, there are serious questions about what they do.

MS. LOIS: But I also want to point out that the practices where the systems analyst or the PRA analyst identifies the actions needed to be done,

dah, dah, dah, dah, and then they would turn around and say, "Give me the number."

So there was a disconnect between the persons that were doing the quantification and the persons that were doing the HRA steps. And as a result, although good analysts could do correctly the HRA steps, very frequently were not done correctly. And we've seen that in the IPE review.

The IPE review, it was like an eye-opening process to see how sloppy, if you will, people were doing HRA while they were doing very good analysis in these other areas.

MEMBER APOSTOLAKIS: Are you going to come anytime soon before this committee to show us an actual quantification by ATHEANA?

MS. LOIS: As you wish.

MEMBER APOSTOLAKIS: Oh, I wish. I wish.

MS. LOIS: Okay. We'll put it in the schedule. We'll try to schedule it as soon as possible.

MEMBER APOSTOLAKIS: Great.

MS. LOIS: Okay?

MEMBER APOSTOLAKIS: As soon as possible.

MS. LOIS: I note here that ATHEANA and THERP do provide some guidance for these other steps.

And, as I said, EPRI early on had developed guidance, and also the calculator is improving the processes for performing HRA, because they have this computerized.

If we look now, given what the methods are, what we learned -- they have, of course, strengths and weaknesses. In a way, the methods reflect the -- how the evolution of the thinking of how you can model human performance under abnormal or accident conditions, and earlier methods are more simplistic than later methods.

But we have to understand that as we're studying it, and we have seen events, etcetera, and there were advances in the cognitive psychology and also in social sciences, it's fair to acknowledge that the later methods may better reflect human performance than the earlier ones.

So, then, what we see is that different methods have different capabilities for also, if you look now, how do you derive the number for translating that number into human error probabilities?

Also, we have many methods, because we have different -- we have different needs. Detailed analysis versus coping analysis, etcetera, which is kind of -- I think applies to every area here. And as a result, some methods are easier to apply than

others.

What strengths? Well, some methods provide a good technical basis for the underlying model, step-by-step guidance on how to use a tool, and it's a traceable analysis. Once you do it, people understand how you came up with a number and then can be usable.

Weaknesses? We saw some methods where you have weak -- a weak technical basis, and, therefore, I'm noting here that we should make questionable -- we should question the use of these methods for which they have been identified as providing very weak technical basis for --

MEMBER APOSTOLAKIS: I think you should be more explicit in your statements. You know, it's --

MS. LOIS: As a matter of fact, we are. And as Alan will come -- will discuss at the end, we are ready to --

MEMBER APOSTOLAKIS: There is one sentence there that really bothers me. That all models can be useful, depending on the circumstances.

MS. LOIS: We should have taken that sentence away before --

MEMBER APOSTOLAKIS: Yes, you should have.

MS. LOIS: -- before we sent the -- sorry

about that.

Also, methods --

MEMBER POWERS: Professor Apostolakis, I'm anxious to understand how a model couldn't -- one of these models could not be useful in some circumstance.

MEMBER APOSTOLAKIS: It's completely arbitrary. I mean, if you are doing arbitrary things, I don't see how you can say this can be useful. It's useful in the sense that it created some income for the developers. But that --

MEMBER POWERS: So we have an absolute. The sentence seems to be true.

MEMBER APOSTOLAKIS: An absolute what?

MEMBER POWERS: We have an absolute. The sentence does seem to be true. Maybe not useful, but true. I mean, there are lots of arbitrary things in the regulatory process, so it seems to me that models can be useful.

MEMBER APOSTOLAKIS: No, some of these things -- it has been already said that some of them are not even HRA models.

MS. LOIS: We saw one case in the IPEs which the method was totally misapplied, for example, and it was obvious that this -- this method could result in not even the right ranking of the human

actions, given the PRA. It was a convoluted PRA profile -- risk profile.

MEMBER POWERS: Can you find me any model that it is not susceptible to misuse?

MS. LOIS: No.

MEMBER APOSTOLAKIS: No. But I can pick two or three of these models and show you clearly that they are making assumptions that are -- or they are actually making mistakes. I mean, you can't just define as many PSFs as you want and start adding them up. I mean, there are certain rules about these things.

MEMBER POWERS: If I look in the world of high science and thermal hydraulics, would I find models that have mistakes in these?

MEMBER APOSTOLAKIS: Yes. And, of course, the thermal hydraulicists have the benefit of experiments that we don't have.

MEMBER POWERS: I think there are some thermal hydraulicists that might contend the statement benefit of experiments.

MR. KOLACZKOWSKI: This is Alan Kolaczkowski. Yes, I was just going to make a comment that I think the analogy here is that using thermal hydraulics, for example, I mean, we don't solve every

problem doing a finite element analysis. We have -- in TH, we have many different models for solving different kinds of problems.

Some are more approximations than others. We recognize that some are making assumptions that maybe don't always apply to a certain situation. Nevertheless, we use the answer anyway because we think -- we say it's good enough or it's conservative, or whatever.

And I think the analogy is the same thing here. In HRA, we have different methods. They have different strengths, they have different weaknesses. What you want to do is not so misapply them that you really are trying to ask the method to do something that it can't do.

MEMBER POWERS: What I really worry about is, to draw the analogy that hydraulics is probably more finite than it deserves, is that it seems to me we have -- a lot of people went out and they said, "How do I quantify human reliability?" And they set up their models, and they did things, and learned a lot. And now we have a bunch of models that kind of solve half the problem.

The problem that we encounter now is that we're asking more detailed and refined questions than

initially we were smart enough to ask. But because of all of these models, we've learned how to do it. And now we have a whole lot of folks trying to solve a much more difficult problem, and lots of people can solve the first half of it. And they're all solving the same first half of it.

And it seems to me that we need to drive toward something that solves the problem, not comprehensively but to the level of comprehension that we can now ask the questions. And I think this is a field where we have not learned to ask all of the questions. The thermal hydraulicists may know all of the questions to ask. Here I think you're still learning what questions to ask.

And we need some driving force to focus everybody's attention on a model or maybe a couple of models, things like that. And I think that's why everybody is so excited about your good practices document is it's a first step in that process.

MEMBER APOSTOLAKIS: I'll tell you what --

MR. KOLACZKOWSKI: I agree with your comments.

MEMBER APOSTOLAKIS: -- if somebody outside the nuclear business came in here and wanted to find out what we're doing, and we gave him or her

one of the models that were reviewed, probably that person was -- has created an opinion that the nukes don't know what the hell they are doing. And that bothers me, because some of these models are so obviously arbitrary and, in fact, wrong in some of the things they do that they should be eliminated from the face of the earth.

If we gave them ATHEANA, I wouldn't feel so bad.

MEMBER POWERS: Does this sound like RETRAN, or something like that?

(Laughter.)

MEMBER SIEBER: Yes. Let me ask a question sort of along the lines that Dr. Apostolakis is addressing. When I read the draft NUREG, I got the feeling that a lot of these methods really depended on the skill of the analyst. And to me that means that the methods lack the kind of rigor and certainty that it would take, so that two analysts would get the same answer.

And I consider that a pretty strong weakness. Is my impression correct or not?

MR. KOLACZKOWSKI: This is Alan Kolaczkowski. Yes, I mean, I think that's true. During the subcommittee meeting, one of the things

that was brought up was the fact that HRA is probably struggling right now with the fact that it's trying to solve or really deal with two issues, which are unfortunately at different extremes of a spectrum.

One, we would like to have little flexibility, so that analysts will always be -- will always apply a method the same way over and over and over again and make it reproducible, make it traceable, etcetera. On the other hand, humans don't fit in nice equations. You know, Q equals M.CP delta T, or whatever.

And, therefore, you want flexibility to deal with different contexts, different situations, because it's hard to create a method that can treat every situation that you could imagine. And so you also want the flexibility for an analyst to recognize that a certain influencing factor has now come into play. And even though the method doesn't address it, I want to be able to address it anyways.

And so on the other end of the spectrum, you want to add a lot of flexibility, which creates or can create, if not done carefully, analyst-to-analyst variability. And so we're struggling with those two extremes, and it's difficult, because humans don't fit an equation.

MEMBER SIEBER: Will this come to some kind of resolution someday?

MEMBER APOSTOLAKIS: Not yet, but it's an important --

MEMBER SIEBER: I mean, is this where you're driving?

MS. LOIS: Yes.

MR. KOLACZKOWSKI: Yes. That's where we're trying to go to.

MEMBER POWERS: Well, I mean, you're driving -- what is unclear to me is how good you need to be. Do you -- I mean, and I -- that may be okay, that you don't know that, because it may not be -- you may not be sophisticated enough to know the answer to that. And all you can strive for is, "I want to be better than I am now." And I'm certainly -- I mean, there are lots of analogs where we can find that situation.

But do you know how good you need to be? Or is it -- are you just striving, "I need to be better than I am now"?

MR. KOLACZKOWSKI: My opinion is that we don't know, other than I think we know enough that it varies on the application and how much precision and accuracy is required in the answer. And, again, I

think that's true in any other field -- thermal hydraulics or anything else.

MEMBER POWERS: Absolutely. I mean --

MR. KOLACZKOWSKI: How precise do I have to know what the yield capability of this material is? Do I have to know it within 10 percent? 100 percent? It depends on how I'm going to apply it.

And I think we're in the same boat here, so I think it does depend on the application. And so to give a -- you know, just to give you an answer, I don't think we know. I think we're still struggling with how well -- how well it would --

CHAIRMAN WALLIS: I think it would help if you talk about the problem as well as the methods. I mean, the problem is, what number do you put in a PRA?

MEMBER SIEBER: That's exactly what it is.

CHAIRMAN WALLIS: And so you want to say, "That's the problem. The user wants to know." You give me a number. How much confidence do you have in it? How accurate is it? How variable is it? All those kind of questions are being asked by the user. Are you providing the answers? And I'm not sure that you are.

MR. KOLACZKOWSKI: I think we've only -- I think we're starting.

MEMBER APOSTOLAKIS: This is a start.

MR. KOLACZKOWSKI: I think we're starting.

VICE CHAIRMAN SHACK: But suppose you do importance measures for their actions. I mean, does that give you some insight into how good you have to be?

MS. LOIS: Sure.

MR. FORESTER: That will tell you something about how detailed of analysis that you need to do. If you do some sort of a screening analysis and see what turns out to look important, then that gives you guidance on whether you need to do a really detailed analysis or whether you don't have to do much of an analysis at all.

MEMBER DENNING: I'd like to make a comment. Although I realize we're distracting you from your presentation, I think we're in a very important discussion at the moment. And I think that it's an area of great concern to me, that we are using today PRAs very quantitatively in this risk-based -- risk-informed, rather -- process, 1174 kind of process.

And, you know, you asked, "How well do we have to know?" Well, we are using this within a process in which people give very little consideration

to the uncertainty that's associated with the CDFs, the LERFs, that they calculate. So right now we're asking a lot of HRA as far as how accurate we expect it to be.

The concern I have is not that -- I mean, there is some concern about people using methods in the same way to come up with reproducible results. My concern is substantially different. I mean, I have some concern there.

My concern is that we are going to come to agreement on what's the best method, and we're going to narrow down the perception of uncertainty, whereas the reality is -- the uncertainty -- the true uncertainty is going to remain very large.

So I think that you have to be very careful to make sure that we look not just at what's the best number for the probability with the various methods, but what's the uncertainty and force -- and force our regulatory process to consider those uncertainties when we're doing our risk-informed judgments.

MEMBER APOSTOLAKIS: One last comment regarding, how good do they have to be? Maybe we can look at the history. There are two major classes of human errors. One is pre-accident; the other is post-

accident. Look at the last 25 years. The pre-accident probabilities, like routine maintenance and all of that, there were some objections to Swaine and Gutman in the beginning, but universally now around the world everybody uses them. No objection.

For the post-accident, you have all these models. People object to the -- maybe there is a message there that what Swaine and Gutman did is good enough. Nobody is objecting to that.

What are you going to do? They give you distributions, they analyze, they look at various processes, and so on. Nobody has come up with a different model. You know, you may argue about a number here and there, but it's okay.

So we have settled there, "This is good enough for these purposes." When it comes to post-accident, people are objecting. They are developing their own models. So that's an indication that this is not good enough.

MEMBER POWERS: Well, George, I mean, I think from the regulatory process we still struggle a little bit. And I think for instance in our power uprates we have a question of the BWR oscillation, and especially in the BWR 4s. It's not so bad in the PWR 6s, but in the BWR 4s we have a relatively narrow

window for action.

And so, consequently, there is a human error associated that -- that typical licensees calculate with THERP. And they come in, and then we ask them, "Do you train your operators for this accident?" And they said, "Oh, yes, we train them. Every year they go through this." Has anybody ever failed to perform this action? No. Has anybody ever failed to perform that action within the allowed time? No.

How quickly do they perform it? They perform it in 30 seconds. They have five minutes. And we still ascribe a 1 in 100 error to it. And we all scratch our heads and say, "Well, you know, what do I do with this number?" Because it doubles when we do the power uprate.

And is that reasonable or not? Well, and it has some impact. It's a tenth in the core damage frequency, and so we usually walk away from it. But the fact is that this committee itself spends about an hour per BWR 4 uprate going over this thing.

And I assume the licensee spends a commensurate amount of time worrying about it, because we don't know what to do with this fact that the operators on simulator training never fail to perform

their required action, and yet there's an error ascribed to them, about a 1 in 100 error.

MEMBER APOSTOLAKIS: Well, that's inconsistent, yes. But still, it falls in the category of post-accident models, what you've just described. Yes, there is a problem there, and hopefully the databases that are being developed now will shed some light on this, especially the timelines from Idaho. I have great hopes there.

At least from what I heard at the subcommittee meeting. Because then you will have clear evidence of the kinds of things you are talking about, and then you will say, "Well, gee, is ATHEANA giving me" -- well, actually, ATHEANA will not give you separate results, because it's an expert judgment based method. So they will look at this database first before they express their judgment.

But anyway, I think we are distracting you too much.

MS. LOIS: I think I'm done here. And I will let John go to the next --

CHAIRMAN WALLIS: So just to summarize, you're never going to show us a figure which has theory versus experiment?

MEMBER APOSTOLAKIS: No.

MS. LOIS: But we --

CHAIRMAN WALLIS: This committee loves to see a figure which has some points on it.

MEMBER POWERS: The chairman of this committee likes to see --

MEMBER APOSTOLAKIS: I think we --

MEMBER POWERS: The rest of us are less enthusiastic about --

MEMBER APOSTOLAKIS: The chairman's prerogative is a little limited sometimes.

CHAIRMAN WALLIS: Okay.

(Laughter.)

MS. LOIS: So John will go to --

MR. FORESTER: I'm just going to comment on that. John Forester. I think you can test aspects of the models. Whether you can actually completely test the model is a different --

MEMBER APOSTOLAKIS: So how do you plan to proceed here now? Are you going to show us an example of an evaluation?

MR. FORESTER: Well, what I planned on doing was stepping through each of the 10 methods and trying to give you --

MS. LOIS: If there's too much we can --

MEMBER APOSTOLAKIS: Each of the 10? Are

you kidding?

MS. LOIS: No.

MEMBER APOSTOLAKIS: Pick the worst of the best.

MR. FORESTER: Okay.

MEMBER APOSTOLAKIS: And don't tell us which one is which.

(Laughter.)

MR. FORESTER: Well, the first one up is THERP. We probably should at least mention that. It was the first HRA method. It's also probably the one that has been used more than any other HRA technique, so there's a fairly strong database of its use. As you probably know, THERP was based on the HRA work that was done for Wash 1400, and it intended to be a full-scope HRA method.

So there was guidance in there for identifying the human failure events, for how to model them, and how to quantify them in a PRA. On the other hand, it was the first method, and there are some gaps in terms of the HRA process information.

For example, there's not guidance in there for how to incorporate, how to model the human failure events into the PRA. So that's not covered in detail.

Also, an aspect of THERP is there was a

strong emphasis on decomposing the operator actions into subtasks. So there's much more of a focus on doing a task analysis, and sort of a microanalysis of what each of the steps that the -- that the crew might have to take. And there's less of an emphasis on diagnosis in the THERP methodology, which over time is becoming recognized as much more of an important driver in terms of concern, as George was talking how, you know, the diagnosis part of it is much more complex.

There is guidance in THERP for how to quantify pre- and post-initiator human failure events. And so, again, there hasn't been a lot of other work outside of ASEP, which is a follow-on to THERP, to address pre-initiators. And that has become sort of an industry standard, I think.

With respect to diagnosis in the THERP methodology, they quantify the probability of error in diagnosis. They use a time reliability correlation. And it's a fairly simple, generic curve, basically a single curve that -- you know, the basic notion obviously is that with more time there's less chance of failure.

MEMBER APOSTOLAKIS: I thought -- I remember a figure where he had upper and lower bounds.

MR. FORESTER: He does have an upper and lower bound, and those -- that's -- they basically start with sort of a basic human error probability, given the time available. And then, they'll adjust for a --

MEMBER APOSTOLAKIS: But the fundamental issue here, and I hope in the report you emphasize it more, just saying that they are doing it with the TRCs is not good enough. The fundamental question is: where did the TRCs come from?

MR. FORESTER: Exactly. And they came from expert judgment.

MEMBER APOSTOLAKIS: It was really Alan's judgment, wasn't it? Not this Alan.

MR. FORESTER: It was. And he is very straightforward about, in the paper, acknowledging that, you know, this curve is, you know --

MEMBER APOSTOLAKIS: And at the same time, when we say this, I mean, let's be fair. I mean, when Swaine did that, that was 30 years ago.

MR. FORESTER: Absolutely.

MEMBER APOSTOLAKIS: When there was nothing in the literature, really. So, I mean, he deserves all of the praise we can give the man.

MR. FORESTER: Absolutely. And all the

issues are there for the most part.

MEMBER APOSTOLAKIS: And all the issues were there. I mean, it's really a tremendous report.

MR. FORESTER: Oh, absolutely. Can take nothing --

MEMBER APOSTOLAKIS: But this is really the issue here. Where are these curves coming from? Yes, Vic.

MEMBER RANSOM: Do these methods account for human failure due to physical incapacity? Like heart attacks, stroke --

MEMBER APOSTOLAKIS: No.

MEMBER RANSOM: -- onset of a headache.

MEMBER APOSTOLAKIS: No.

MEMBER RANSOM: You name it. They are not -- you're not talking about that kind of failure, right?

MEMBER APOSTOLAKIS: Well, unless there is an accident that threatens the control room. Yes, then, they worry. The PRA guys worry about it, not the HRA people. But under normal conditions you can't say all of a sudden --

MEMBER RANSOM: So the major uncertainty are just cognitive mistakes that the normal human would make?

MEMBER APOSTOLAKIS: Yes.

MR. FORESTER: Yes, unless there's something to be done, say, outside the control room, there's physical activity, they have to climb up to get to a valve or something like that, those sort of -- those kind of conditions are taken into account. But not -- not the physical --

MEMBER RANSOM: These others are what, considered too rare or too small a probability to be incorporated?

MEMBER DENNING: Like in SL-1 where, if you recall, it is at least -- whether it's true or not -- the belief that the perpetrator had had an argument with his wife, if I remember it correctly. Whether that's true or not, I mean, there may be some evidence that strange things happen.

MEMBER RANSOM: There's clearly some mistakes that were made there that they don't really know what the reasons were, but --

MEMBER APOSTOLAKIS: Okay.

MR. FORESTER: Well, I just wanted to note that one of the -- I think one of the limitations of THERP, though, is that even though there's a very nice discussion, a whole chapter, in THERP that does address all of the -- a range of all the influences

that could affect human performance, when you actually go to the quantification part of it, only a few of those are actually included in the models. There's no guidance for how to use all of that information.

And then, as George mentioned, the data that underlie -- I mean, the empirical basis for the human error probabilities that are included in the model is essentially, although they did -- you know, they looked at the resources available at the time, and then based on their own expert judgment they, you know, extracted that information. So --

MS. LOIS: Which one you were going to --

MR. FORESTER: I thought maybe we'd jump to one of the EPRI methods. The CBDT method would probably be -- unless, you know, someone has any preferences, we can go to the CBDT method. It was part of an EPRI-developed methodology. The original document included the HCR/ORE time reliability correlation approach, and then the CBDT method was essentially developed to cover the cases where time was less of a factor.

If there's more time available, if you use the TRC as you get -- you know, more and more time gets available, there is really no discrimination about what it might do. So the idea was that for very

low error probabilities and where the curve seemed to not be very functional, then they used the CBDT --

CHAIRMAN WALLIS: So you're describing all of these methods. I mean, did somebody grade them or anything? I mean --

MEMBER APOSTOLAKIS: Did somebody what?

CHAIRMAN WALLIS: Grade them, or is there some kind of judgment about which one is better?

MR. FORESTER: Well, we do make some judgments about some we think really probably shouldn't be used. There's enough doubt about them. And there's some that we would recommend be used, yes, but we don't put a -- try to grade them in any way.

MEMBER BONACA: It's interesting. During the subcommittee meeting, I mean, Dr. Rahn said that the way the methods were chosen into the calculator was purely on the basis of which ones were being used by the industry. There was no judgment on quality.

MEMBER APOSTOLAKIS: But as I said earlier, though, practice now is showing which ones are being used more than others.

MEMBER DENNING: But recognize there is a danger there, because some of these are very difficult to use. And so there's a tendency to use the ones that are easier to use.

MEMBER APOSTOLAKIS: Oh, yes. Exactly. ATHEANA is not the easiest method in the world.

MEMBER DENNING: No, it's not.

MEMBER POWERS: George, how can you say that?

MR. FORESTER: I guess I'd just make one comment about the CBDT. One of the reasons we bring it up is that it -- it does attempt to -- it uses a causal model, where this is an effort to identify a range of failure mechanisms, and the kinds of factors that could lead to those failure mechanisms. So there is, again, more of an effort to understand why crews might make diagnosis failures.

And I think that's sort of a -- at that time, that was sort of a first step to go beyond the very basic --

MEMBER APOSTOLAKIS: What is the focus? Maybe it's too late now for this report. But given the developments of last December, you know, as I briefed you earlier, shouldn't you also state somewhere very explicitly what the focus of this method is? This method produces a probability that something will be accomplished within a certain period of time. This method produces a probability for an event. This is very different things.

MR. FORESTER: It is. And one of the distinctions -- here we talk about the non-response probability. That followed from the HCR/ORE method, which was really trying to say the probability of non-response within a certain time period.

MEMBER APOSTOLAKIS: Within a certain time, yes.

MR. FORESTER: And this followed that, although it was --

MEMBER APOSTOLAKIS: So this is a function of time? The normal response probability is a function of time here?

MR. FORESTER: No. In fact, this is for the case where there's no time limitation.

MEMBER APOSTOLAKIS: There's no time. That's a very important point, I believe. Now, I don't know if you guys have time to do this for you issue it, but some --

MR. FORESTER: Sure. We can take another look at it. There's some of that in there, but we can certainly --

MEMBER APOSTOLAKIS: A little bullet anyway, you know.

MR. FORESTER: Sure.

MEMBER APOSTOLAKIS: The focus here is the

time, which --

MR. FORESTER: Yes.

MEMBER APOSTOLAKIS: -- aleatory, whatever. Here the focus is an event, the operator's failure to do something. And time is a PSF, because that I think will lay an additional foundation to what we plan to do in the future.

MR. FORESTER: Okay. I would not that CBDT has become the user stand-alone method. In fact, it's one of the methods that's included in the EPRI HRA calculator.

In terms of that data that's used by CBDT, this is sort of a continuing saga in a way that the data that's used -- if you follow these decision trees in CBDT, you have probabilities at the end. And the data for that was actually based on THERP. So the authors looked at the -- you know, similar kinds of cases from the THERP data and extrapolated it to -- for use in those models.

Okay. What was the -- SPAR-H?

MEMBER DENNING: Yes.

MR. KOLACZKOWSKI: Yes, we're trying to figure out -- maybe just show you -- rather than going through all 10, maybe show you three or so, give you a feeling for the summaries -- the kinds of summaries

we're making about it.

CHAIRMAN WALLIS: When I look at the numbers that come out of these things, sometimes I see things like one E^{-3}. Sometimes I see numbers like $7.31\ E^{-1}$. Now, I just don't quite understand what -- how to take those sorts of numbers. One seems to be a guess, and one seems to be extraordinarily accurate.

MR. FORESTER: It's almost an artifact of the method. I mean, surely they're not that accurate, but the way you add and multiply and add things up, those kind of values come out with some of the methods. It's just being --

CHAIRMAN WALLIS: But should I do? I'm trying to evaluate a PRA, and I see numbers like that. When I see $7.3\ E^{-1}$, I say, "Well, how did you ever get it so accurate? And how could someone possibly have such a high probability of error?"

MEMBER APOSTOLAKIS: Depends on the context.

CHAIRMAN WALLIS: Well --

MEMBER APOSTOLAKIS: It depends on the context.

MEMBER POWERS: The human errors at TMI had a probability of one.

CHAIRMAN WALLIS: After the fact.

(Laughter.)

MEMBER APOSTOLAKIS: But, actually, when you see in PRA numbers like that, at least one good thing you're doing is you are eliminating model uncertainty.

(Laughter.)

.6, I mean, what do you want me to do? .9? Sure.

(Laughter.)

But one other thing -- we keep saying "number." I mean, good methods don't produce a single number. They produce distributions, not a number.

CHAIRMAN WALLIS: Well, we're looking at the SBWR PRA, the 1,800 pages, or whatever it is. It has tables in there of these numbers.

MEMBER APOSTOLAKIS: Yes.

CHAIRMAN WALLIS: They look like the numbers I've just described.

MEMBER APOSTOLAKIS: Well, and I think you have seen numbers for core damage frequency like 3.82 10^{-7}. I mean, we have to decide that, come on. We know that these are the results of computer programs. We know what they mean. What do you want the analyst to do, say, what, this is ridiculous, I'll make it four? Okay. Then, we start doing that, if you want

them to, but it doesn't make sense, really. We know that the accuracy is not that high.

But, yes, much to my surprise, in fact, I looked at some cases of risk-informed applications. And there were human error probabilities -- .5. And you say, well, okay, fine.

MS. LOIS: I just want to add here, even .5 sometimes may not be a pessimistic number. For example, if it is a very heroic action, which is open the containment once it has been contaminated, and people will have to put their lives on the line, probability of that number should be one.

MEMBER APOSTOLAKIS: Yes. But from the regulatory perspective, I can't imagine anyone in NRR making a decision that would really be based on the factor of .5, and it is not .8.

CHAIRMAN WALLIS: These numbers aren't --

MEMBER APOSTOLAKIS: They will never do that, so --

(Laughter.)

CHAIRMAN WALLIS: These numbers I'm citing aren't for those kind of heroic actions at all.

MEMBER APOSTOLAKIS: Yes. Yes. No, we'll come to the SBWR. We know that.

SPAR-H, we will have a special review of

that, aren't we?

CHAIRMAN WALLIS: What I'm trying to get at is, are you helping me with the problems I have in trying to understand --

MEMBER APOSTOLAKIS: No.

CHAIRMAN WALLIS: -- these numbers.

MEMBER APOSTOLAKIS: No, you are way ahead.

CHAIRMAN WALLIS: You're not helping me.

MEMBER APOSTOLAKIS: Well, they are helping you in the sense that they're helping you get there. But they are not answering today. I believe this report is really unique, because I don't know of any other report that brought together all these models with some attempt at criticism, without hurting anybody's feelings. What kind of science is this? Anyway --

MR. KOLACZKOWSKI: If you don't understand the differences among the methods, and at least some of their relative strengths and weaknesses, then you can't even begin to grade them. And what we've done is the first step. At least let's understand what the differences are, where they're particularly strong, where they're particularly weak, etcetera, etcetera, and then maybe we can begin to grade them, do some

benchmarking, etcetera. And those are plans for work yet to come.

But, first, we have to keep them coming -- make sure we understood what the differences among the methods were. That's what this step is. It's only the first step in your process, Dr. Wallis.

MEMBER SIEBER: I think the tables on page 231 and following do a pretty good job of simply laying out what these methods do, what the strengths are and the weaknesses.

MEMBER APOSTOLAKIS: In fact, I'm surprised you are not showing any of those tables. They were --

MEMBER SIEBER: Yes, they were -- that was well done, I thought.

MR. KOLACZKOWSKI: We've added some tables as well. In fact, it's the latter part of our presentation this morning.

MEMBER APOSTOLAKIS: Anyway, coming back to decisionmaking, this model is being used by the agency, the significant determination process. And I believe the review these guys did is not sufficient. This committee should look at this model much more seriously and exhaustively.

I'm not criticizing you. I mean, it's

part of the bigger picture that you had to deal with. But, I mean, if you want to talk about real life, this is real life, and --

MEMBER DENNING: I am curious --

MEMBER APOSTOLAKIS: -- some things are really bothering me, what I saw there.

MEMBER DENNING: Based upon your review of this, do you think it is adequate for the significance determination process? I mean, I -- in my own looking at -- I mean, I think there's a real need for SPAR-H as part of the SPAR process. But when you get down to a specific event that you're going to analyze, is it your impression that SPAR-H is adequate, then, for a significance determination? Or would you feel that a more powerful method should be employed for that?

MR. FORESTER: Well, I guess honestly I'm not sure how much level of detail is required for the significance determination process. I would say that I think SPAR-H is developed for a higher level of analysis, for the ASEP-type analyses, and it does a lot towards that.

I think there are some limitations in terms of the PSFs that are involved, and so forth. But, so I guess it's hard for me to answer that question, because I don't know how good an answer they

need for --

MEMBER APOSTOLAKIS: But I can't --

MR. FORESTER: You will miss some -- given the limitation of PSFs that are covered, it's possible you will miss important factors that could influence performance.

MEMBER APOSTOLAKIS: But it's important to point out to the committee -- you were at the subcommittee meeting -- there was disagreement among the staff as to whether SPAR-H or ATHEANA should be used. And I tried very hard to make them say, "Yes, we'll start with SPAR-H, which is approximate, and then we will use ATHEANA for more detailed events," and they refused to do that.

One member of the staff felt very strongly that one should use ATHEANA everywhere. So it is an indication of a state of the art, I believe. But I think SPAR-H is something -- is a project we really have to review, because it is being used in regulatory actions. We will do that.

MR. FORESTER: Okay. Is that enough on that?

MEMBER APOSTOLAKIS: Go on. Don't ask. We interrupt on our own.

MEMBER DENNING: Do you want to do ATHEANA

next?

MEMBER APOSTOLAKIS: Do you want to send a message to the committee? Which slides would we use? I think we have looked at -- yes. Alan, do you want to take over now, or what?

MR. KOLACZKOWSKI: Yes. I think --

MEMBER APOSTOLAKIS: Okay.

MR. KOLACZKOWSKI: -- first of all, we want to give you a feeling is that -- we went through the 10, we went through in as an objective a fashion as we could -- as Dr. Apostolakis pointed out, those that the staff were involved in the creation of -- ATHEANA, etcetera -- we had an outside reviewer provide review to us, and then we took that as input.

And we've gone through each one in the same way. What we've tried to do is take comments we received from the subcommittee as well as internal comments that we received from within the staff, and we've added a lot more conclusions and comparisons of the methods than even the version that you currently have in front of you.

And so I'm going to show you a few slides, which will be sort of a preamble of what the public version is going to look like, and that tries to draw some more comparisons and has more of the kinds of

tables, Dr. Sieber, that you talked about in the document.

Let me just address those, and so I'm going to start with slide number 20 in your package.

MS. LOIS: Not 19?

MEMBER APOSTOLAKIS: 20. He said 20. Don't go back.

MS. LOIS: 20.

MEMBER APOSTOLAKIS: He said 20.

MR. KOLACZKOWSKI: For example, if you look at the underlying quantification approach that the methods use, you really -- it really comes down to really two, but we'll say three different ways that methods quantify, and try to take the qualitative information that they gather and change it into a probability.

The methods you see listed in the first bullet, they use this concept of a basic or initial human error probability, a generic number, and then you adjust it through a series of tables or multiplicative factors, etcetera, etcetera, to account for different influencing factors -- those that are positive, those that are negative.

CHAIRMAN WALLIS: Isn't all this plant-specific? I mean, there's different training in each

plant. There's a different culture, and so on, and -- how can you have a basic HEP that isn't plant-specific?

MR. FORESTER: Well, I think that is that you account for plant-specific factors through the use of the PSFs. So it is -- you know, it's a stretch for my mind to assume there is some sort of basic human error probability. But that's the approach, and then the idea is you adjust for plant-specific factors with the PSFs.

MR. KOLACZKOWSKI: Yes. I mean, that's the attempt, but, you're right, it starts off with a basic premise. We'll start with a basic number. Typically, that number is a THERP number or a THERP-like number, around .03. Saying for the kinds of activities that the nuclear industry is involved with, a three percent error rate is a starting -- a good starting point.

Now, let's adjust that depending on whether the influencing factors are very positive, like I have lots of time, procedures are clear, I'm training on it a lot, I tend to lower that probability. If, on the other hand, time is very short, procedure is very ambiguous, I hardly ever train on this scenario, etcetera, then you up that

probability. Maybe it becomes .1 or something.

So the attempt is to try to start with a basic number. You can argue whether that even has any premise or not. And then, you just it based on the plant-specific factors.

The other approach that's used primarily more on the SLIM/FLIM type of an approach, or ATHEANA, is to basically look at all the contacts and all of the influencing factors. And rather than starting with a number and adjusting it, you take all of these factors and you basically compare that with situations that you know of in your own experience, and, therefore, try to draw a parallel between the action you're trying to quantify and experiences that have similar context to this particular action. And, therefore, on the basis of that try to assess a probability of failure for that situation.

So, and that's more of the expert judgment types of approaches that ATHEANA uses, that SLIM/FLIM uses, etcetera.

There is an empirical approach that HCR/ORE uses in which they actually try to measure the time it takes for actions. But then, to still turn it into a probability, there's a formula that's applied that, again, you could argue whether that has an

adequate premise or not. Okay?

Now, do we know whether one basic method is better than another? In other words, do we know that the first bullet would produce a more validated, accurate number than the second bullet approach, or vice versa? I think yet we don't know.

But at least now we understand and we have the understanding that there are two basic quantification frameworks out there. And part of what we need to do is -- in future work is to see, can we validate both? Or, in fact, prove that one is not very relevant at all and shouldn't be used? I think that's where we need to go.

MEMBER POWERS: I mean, that's really an interesting point of view, Alan. Explain to me -- as I understand, what you're saying is we've got these premises, three of them, that you identify. And you want to explore the premises rather than the product. I mean, why do you think that's a useful way to go? I mean, the THERP approach is very intuitive.

I think if you came to human reliability analysis out of thermal hydraulics, for instance, and somebody gave you the tour, that's probably where you'd start. I think I'm on sound ground there. That's where all of the early models started.

As you become more sophisticated, maybe you move to these other techniques. Is it really productive to explore that?

MR. KOLACZKOWSKI: Well, you're right in that I -- I guess you're right in that the standpoint that if you could -- if you could validate the number at the end through some experimental process, then maybe trying to also validate the premise is not as important. But as we've already pointed out, we're never going to have a theory versus experiment kind of curve.

We can test parts of this thing, and parts of that -- parts of that is going to be, is the premise even correct? Is the idea that influencing factors can be treated independently as these methods all used, is that an appropriate way? Or do you have to account for the interactions we attempt to do --

MEMBER POWERS: I can see doing that. I mean, that's a very, very common thing to do is to assume the multiplying matrix is first diagonal, and then, all right, let's look at the off-diagonal terms, which is what you want to do. I mean, that seems like a productive thing.

It also seems to be productive to go through and say, "Okay. Given this premise, what can

this model not do?" Because I'm sure there are things that a given premise makes it quite impossible to do some things.

MR. KOLACZKOWSKI: Yes.

MEMBER POWERS: And to do other things poorly.

MR. KOLACZKOWSKI: Yes.

MEMBER POWERS: That seems useful. But to go back and try to say which of these premises is the correct one, which one is the strongest one, which one is -- makes the subset of undoable smallest. By some measure, that seems all productive thing. But "go back and ask about the premises" does not seem -- I mean, it's like going back and saying, "Okay. Which one of these axioms are true?" I mean, you'll never get anywhere.

You can only say, "Which one of the axiom sets do I like the best?" That's useful. But you've satisfied only yourself, because George would like a different axiom set, probably guaranteed. Even if you consult with him first, find out what his preferences are, by the time you make your presentation he will choose a different axiom set.

(Laughter.)

MR. KOLACZKOWSKI: You apply suspicion.

MEMBER POWERS: Based on a lot of experience.

Now, it seems to me that --

CHAIRMAN WALLIS: You're conducting a human reliability study here with George?

MEMBER POWERS: His reliability in this regard is --

MEMBER APOSTOLAKIS: We are approaching 10:00.

MEMBER POWERS: Yes. I mean --

MEMBER APOSTOLAKIS: Is there anything more useful to say on this subject?

MEMBER POWERS: It seems to me that I would couch things in terms of looking at the space explored and off-diagonal terms, and things like that, because I think you're on firm engineering ground when you do that. I mean, taking your softer science and trying to put a quantitative veneer on it, that's a safer round rather than looking at the axioms.

MR. KOLACZKOWSKI: Understood.

MEMBER APOSTOLAKIS: And I think from now on, really, the focus should be on the time. Not that this point is not relevant, but it really is important. With EPU it was raised earlier, you know, the impact is on time.

MEMBER POWERS: I think that's not where I would focus, George.

MEMBER APOSTOLAKIS: I would.

MEMBER POWERS: I think there's been a strong focus on time. I think if I were going to wrestle with this, I would wrestle in two areas. One, I would say, how do I use the fact that people train and have some reliability on simulators? And how do I factor that in, recognizing accidents are not simulators? And then, I would worry about transferability.

MEMBER APOSTOLAKIS: No, this is the next step, but you have to start out by saying, "What is it that I'm trying to produce?" And then, I would look at the evidence and see how to use it, and so on. But the fundamental question is: what am I trying to produce? And up until now, most of the models say, "Okay. The operator needs to do this thing here. What's the probability?" And I don't think that's very helpful to us. We have to focus on a different variable.

And then, all these questions of course become very important. I mean, how do you relate it to real experience, simulator experience, and so on?

Alan, you are really very slow. Come on.

MR. FORESTER: For me, the time is, you know, the -- what measure you use is not whether it's just error, or whether it's the time to respond within a certain period, as long as you're addressing all of the factors that could influence those sorts of things, not just error and not just delay, but all of those thins.

MEMBER APOSTOLAKIS: John, I think if you put something in a performance shaping factor group, you are downgrading it, because you are saying here is a -- you are saying, you know, it affects your judgment. But if you focus on it, it is different. That's the difference.

Okay, Alan.

MR. KOLACZKOWSKI: For the sake of time, let me just cover, if I would -- if I may, Slide 22 --

MEMBER APOSTOLAKIS: Okay.

MR. KOLACZKOWSKI: -- and Slide 29 and 30.

MEMBER APOSTOLAKIS: Wonderful.

MR. KOLACZKOWSKI: Okay? 22 only because it's a subject that I think this committee is always interested in. If you compare the methods in terms of their address -- how they address uncertainty, you also find some interesting observations. First of all, many of the methods provide you with uncertainty

bounds. They say, "When you come up with a number, slap an error factor of 10 on it and assume a log normal," or whatever.

Okay. Those types of approaches are not context-specific. They're not scenario-specific. I mean, the number -- you put the same error bounds, whether it's in a station blackout scenario or an ATWS scenario, or whatever. Okay? And they came to cover aleatory and epistemic, but, in fact, it's just a claim. You can't separate the two. You can't say, "Oh, this part is aleatory. This part is" -- it's simply a statement that goes unproven. Okay.

And so you have methods that apply uncertainty that way. You have others that provide some limited sort of qualitative guidance, but really no quantitative guidance as to how to put an uncertainty bound on the value.

And then, there are methods, more the SLIM/FLIM type and ATHEANA, which are more expert judgment. And, interestingly enough, SLIM and FLIM pretty much concentrate more on trying to come up with addressing the epistemic uncertainty. And to come to the very initial comments that Dr. Apostolakis made about how important aleatory uncertainty is, it's funny that ATHEANA's uncertainty really focuses on the

aleatory aspects.

The uncertainty range that it attempts to put on the HEP is largely due to aleatory influences, because it basically asks the question: how could this scenario be different? How could it be slightly different? And those are all aleatory aspects. What if this alarm doesn't come in now but comes in later? Etcetera. And so it addresses more the aleatory. But --

MEMBER APOSTOLAKIS: Well --

MR. KOLACZKOWSKI: But it also doesn't address the epistemic.

MEMBER APOSTOLAKIS: -- I read again this wonderful paper of which you are a co-author. Expert elicitation -- nobody says "expert opinion elicitation" -- approach for performing ATHEANA.

MR. KOLACZKOWSKI: Yes.

MEMBER APOSTOLAKIS: And while it says that -- what you just said is partly true, you must admit, because it claims -- it has a nice equation and claims the epistemic uncertainties in P(HFE/S) arise primarily from the P(UA). So it's both. But I would really love to see an application of this and see how people came up --

MR. KOLACZKOWSKI: We will show you.

MEMBER APOSTOLAKIS: Yes. But it's interesting that the aleatory is there, too.

MR. KOLACZKOWSKI: It's largely --

MEMBER APOSTOLAKIS: It's very nice.

MR. KOLACZKOWSKI: The way we actually do it, it's largely aleatory and little, if at all, epistemic. It's treated in reality.

MR. FORESTER: But it can be considered. Depends on what the -- how they do --

MEMBER APOSTOLAKIS: Well, we'll see. We'll see when you present it if it's --

MR. KOLACZKOWSKI: I just want to point out there's different approaches out there for treating --

MEMBER APOSTOLAKIS: You have to realize, Alan, when you write something some people read it.

MR. KOLACZKOWSKI: I know. And it will come back to haunt me, right?

Okay. Without going through all of the other slides, which talk about some other characteristics and compare them, and then in slides like 28, 27, etcetera, we start talking about, well, what does this mean in terms of which methods and when I should use this method versus that?

And I think the committee is already

probably fully aware that we can't give you a hard-and-fast rule, "Use ASEP when it's this, and use THERP when it's that." It's hard to do that in the abstract. If you have a particular application in front of you, etcetera, you can begin to get -- and you really understand what the application is and what kind of decision you're trying to make, you can perhaps better come up with what would be the most appropriate one or two methods to use.

But in the abstract, it's hard to say always that ASEP should be used for only this and ATHEANA should be used only for this, etcetera. That's hard to do in the abstract.

I want to leave, really, the committee with this thought. And it's one of the things that we now have in our report that is not in the version that you have in front of you -- is that -- Slide 29. We feel that the HRA community at large has got to get out of this idea of you select a method first, and then you make the decision or issue fit the method.

You know, well, I know THERP, so I'm going to always use THERP. And I don't care what the decision is; I'm going to make it fit THERP. And THERP only handles six PSFs, but that's what I'm going to do. Or I only use ASEP, or I only use ATHEANA, or

whatever. We think that's the wrong approach, and, in fact, we think the HRA process should be the other way around.

We think you have to determine, what is the decision I'm trying to make? And in order to make that decision, what do I require from the HRA to support that decision? How precise does it have to be? Does it have to cover a full range of contacts, or can the typical average contacts is all I'm really worried about, because I'm just trying to get the average number, not necessarily the range on how bad it could be, and also how good it could be? Etcetera, and so forth.

So we think we have to figure out, what's the decision you're trying to make? What does that mean that the HRA has to provide? And then, you select the appropriate method accordingly, and justify why that method was selected.

CHAIRMAN WALLIS: Well, in thermal hydraulics, when we have a problem which is difficult, and different people have different methods, we're not sure which to use, we usually use them all and compare them. And here you're saying, "Select one and use it."

MR. KOLACZKOWSKI: No. Notice I said,

"Select the appropriate method or methods." And, in fact, one of the things that we have in our report as a suggestion is that it would make for a more robust answer if, in fact, you took your application and did two different methods and see, do you get roughly the same answer, not only in terms of the number --

CHAIRMAN WALLIS: I would think this would be routine in a field where you're uncertain. You would always do that.

MR. KOLACZKOWSKI: Well, but we have tended to not do it in the HRA field. And we're saying in this report it's time to start doing that.

MEMBER APOSTOLAKIS: Using more than one method?

MR. KOLACZKOWSKI: Yes.

MEMBER APOSTOLAKIS: And if one -- and one of those would be ATHEANA?

(Laughter.)

I mean, you are completely unrealistic. It's --

MR. KOLACZKOWSKI: Exactly. It's expensive, and that's why --

MEMBER POWERS: I think you're -- I mean, there is certain logic to what you say, but it's fairly impractical, isn't it? If human reliability

analysis or regulatory applications is going to be done by non-specialists.

MR. KOLACZKOWSKI: I guess, Dr. Powers, I would argue differently. I would say, yes, if you're going to do -- if you're doing a full-blown PRA on the EPR, a new reactor design, yes, it's very expensive and it would be very difficult to do the entire PRA all using ASEP, and then again all using CBDT or something. That would be very difficult.

But if I have an application, and I'm down to just one or two things that are really important, the decision I'm trying to make, I'm doing a power uprate problem, there's two errors I'm really worried about, to ask a licensee or an analyst to apply two different but apparently appropriate methods, and see whether or not you get the same drivers, roughly the same answer, the same ranking, etcetera, to see how robust your answer is, I don't think that's asking too much.

MEMBER APOSTOLAKIS: Six months ago I was reviewing the PRA for the shuttle, and it came down to two or three critical human errors. And I recommended to NASA that they do that. Yes. They were stunned. "Are you asking us to use different models?" You know, of course I didn't do anything.

(Laughter.)

MR. KOLACZKOWSKI: I don't know.

MEMBER APOSTOLAKIS: No, but that's -- before we finish, that creates another issue. How do you decide that you have those two or three? You really need some screening approach first, which is demonstrably conservative. And then, use ATHEANA or something else more sophisticated on these three, four, five, whatever, human errors. And I don't see anybody trying to do that.

MR. KOLACZKOWSKI: And the problem -- in my opinion, the problem is that, because we don't know that a method is demonstrably conservative.

MEMBER APOSTOLAKIS: Well, have we tried?

MR. KOLACZKOWSKI: If you don't investigate a certain performance shaping factor, because it's not in the method to be accounted for, and so you don't look for crew dynamic concerns, because it's not a PSF that's handled by the method, and for whatever reason you don't look for it, if you don't look for it and you don't mind a negative influence, how do you know the answer you had was conservative? How do you know?

Whereas thermal hydraulics, etcetera, again, would set equations, you can for a lot of

applications say, "I know if I use this code I'm going to get a conservative answer, and then I'll decide whether I still have to go and do finite element analysis." HRA is a soft science, and we don't even know if the method is --

MEMBER APOSTOLAKIS: But I'm very pleased, though, that you have reached a level of maturity where now you can --

MR. KOLACZKOWSKI: Thank you.

MEMBER APOSTOLAKIS: -- be compared to thermal hydraulics, a well-established science.

MEMBER POWERS: Alan, let me ask you this question. We will, in fact, have a couple applications coming before us. The staff will review them. They will have -- have human reliability analysis built into them. Is it appropriate for us to go -- and typically what the staff will do in their review, not every case, but typically they'll go through -- and the guy used THERP. They all use THERP. And they'll check, and, yes, he looked up and -- he used the right table. He got the -- he multiplied by the right factor, and what not.

Should we be saying, "Oh, no, no, no, no, no. This is not good enough. Go use one of these other methods and show to us that enough performance

shaping factors were taken into account"?

MR. KOLACZKOWSKI: I do think that if -- that as a minimum, at least some sensitivity analyses ought to be done, so that you get a feeling for what -- if I was to change a number by a factor of three, five, ten, whatever it is --

MEMBER POWERS: Well, they'll do that.

MR. KOLACZKOWSKI: -- would I come to a different decision?

MEMBER POWERS: They will do that. They will go through and they will say, "Okay. I used THERP and, whereas this table told me to multiply by 1.2, I multiplied by 2. And it didn't make any difference at all." But if he doesn't take into -- I mean, what you just told me is that he didn't take into account one of the performance shaping factors I have no idea whether two is conservative or not.

MR. KOLACZKOWSKI: That is correct.

MEMBER APOSTOLAKIS: That's right. That's right. That's where we are.

MEMBER POWERS: Okay. Now I'll turn to you, Erasmia. Have you made available to the NRR staff that will do this, review all of these techniques so they can pick them and use them?

MS. LOIS: So, then, this is the intent.

We are going to have this public comment period, and by September of this year is going to be published.

MEMBER APOSTOLAKIS: Has NRR participated in any of this?

MS. LOIS: Yes. NRR has been our collaborator, guidance.

MEMBER POWERS: What I'm asking is I'm over there looking at -- at this COL, and there's the human reliability analysis, and they're reviewing it. And all of the stuff is laid out. I don't need to really research this plan. Can I pop up onto the computer my CBDT, I think it is, methodology and run it through and see if I get the same answer as they got with THERP?

MR. KOLACZKOWSKI: They'll use THERP. I'm very confident of that.

MS. LOIS: I think the calculator allows you to do that, if you --

MEMBER POWERS: But what I'm asking is: can people in the next building over do it today? I mean, do they --

MR. KOLACZKOWSKI: I can't speak for the staff, but I think the answer is no. But I think -- I'm not that sure that that's what they have to be able to do. I think they have to understand, again,

at least what the major strengths and weaknesses and what is within the scope of the methods, so that if they see, for instance, that an application comes in and they didn't -- for whatever reason an influencing factor was not, I don't know, the ergonomics of the situation, and yet they know that there are some ergonomic issues, and it hasn't been addressed, you would hope at least the staff could ask the question: why do you think this method was appropriate, given it doesn't seem to handle ergonomics, and yet we know that this is an ergonomic problem, because you have to climb up a ladder to reach the --

MEMBER POWERS: Yes, I'm --

MR. KOLACZKOWSKI: -- or whatever it is.

MEMBER POWERS: -- what you're saying is know enough about human reliability analysis to be able to critique the THERP method as lacking the proper axioms. Okay?

I'm asking, why shouldn't we go another step? Why should we provide another tool more readily -- I mean --

MEMBER APOSTOLAKIS: It's too soon, Dana, for that. I know where you're going to --

MEMBER POWERS: And that's an answer I will accept is it's too soon --

MEMBER APOSTOLAKIS: It's too soon.

MEMBER POWERS: -- to do that, but it --

MEMBER APOSTOLAKIS: But it's nice to have a goal.

MEMBER POWERS: A goal. I mean, that's an acceptable answer, too.

MEMBER APOSTOLAKIS: Okay. Is there anything else you gentlemen or lady want to add? You are requesting a letter?

MS. LOIS: Thank you very much.

MEMBER APOSTOLAKIS: Okay. Good.

Any other comments or questions from members? No questions?

CHAIRMAN WALLIS: Back to you, Mr. Chairman.

CHAIRMAN WALLIS: Thank you very much. We will thank the presenters. Thank you for your presentations. Thank you for your patience with our questions.

We will take a break until 10:15.

(Whereupon, the proceedings in the foregoing matter went off the record at 10:03 a.m. and went back on the record at 10:17 a.m.)

CHAIRMAN WALLIS: Please come back into

session.

I invite my colleague Rich Denning to lead us through the next item.

MEMBER DENNING: Thank you. Good morning. We will hear from the staff regarding a revision to Standard Review Plan 14.2.1, Generic Guidelines for Extended Power Uprate Testing Programs.

The committee will hear presentations by, and hold discussions with, representatives of the Office of Nuclear Reactor Regulation.

The staff's objectives for the revision were very limited and are largely editorial. I have asked them, however, to focus on the Section III.C, Justification for Eliminating EPU Power Ascension Tests," where the ACRS has had some historical issues.

I think that we are now ready to hear from the staff, so I will turn it over to Mr. Dale Thatcher of NRR.

MR. THATCHER: Good morning. I'm Dale Thatcher. I'm the Chief of the Quality and Vendor Branch A. I emphasize the A because there is -- we got split into two groups, and there's also a Quality and Vendor Branch B. Hossein Hamzehef is the Branch Chief of that group, and I think he intended to join us, so I think he will probably join us later.

I'll also point out that we have representatives from both of our branches, people that have worked on power uprates, worked on the standard review plan revision. Mr. Paul Prescott, he's in my branch, Branch A, and Mr. Robert Pettis, who is in Branch B.

The two branches were formed out of the old Quality and Maintenance Section, which had lead responsibility for power uprate, this particular SRP section. In addition, we have some representatives from some of the technical review branches that have been involved in the power uprates. We have representatives from the -- formerly Reactor Systems Branch. We've got a -- I think that group has probably been split into about three different branches.

Also, Mr. Steve Jones from what used to be the Plant Systems Branch, which is now the Balance of Plant Branch. He was intending to be here and said he would be. So hopefully he can join us later also.

We understood that the committee wanted to take a look at this revision. As we said, or as Mr. Denning said, we considered the changes minor in this particular revision, because we had been to the committee before and had talked about the draft that

we were going to put out. And we went out for public comment. There were basically no public comments.

Back in I guess it was October timeframe, something like that, we had written a waiver request to the CRGR for their -- you know, to dispense with their review. And at that time, I guess the committee looked at the draft version and said that they wanted to hear some more on it. So that's basically why we're here, to address the committee's questions.

So I'll turn it over to Paul and Bob, and we'll move forward.

MR. PRESCOTT: Good morning, gentlemen. My name is Paul Prescott. I'm joined by Bob Pettis. I'm in QV A, and Bob is in QV B. What we plan to do is -- today is try to provide more detail to the ACRS about SRP 14.2; specifically, the recent changes that were made since you last saw it, give you a little oversight into how the staff evaluates SRP 14.2.1; and go into a brief overview and technical discussion of Section III.C, which has been the area of focus before with ACRS on specifically the justification for elimination of power ascension tests.

Like was said earlier, most of the changes that were done to SRP 14.2.1 since the last time you saw it were editorial in nature. As you may be aware,

we have actively sought input from the staff on recommendations to try and improve the interface between our group, which has general oversight of the testing program for EPUs, and the specific -- and the respective technical branches that would input into 14.2.1.

The most significant change had to do with III.C, subsection c, which is facility conformance to limitations associated with computer modeling and analytical methods. Specific areas that were enhanced, because that was already in that section, were deeper discussions on the setpoint and parameter changes and modifications to ensure that they don't invalidate the analytical methods.

And if the analytical methods are inadequate, the secondary review branch would make a recommendation on what kind of testing to propose. And as a -- I'll give a real life example I guess I'll call it. VY is a recent example that -- of a test that was proposed for a specific analytical method that the technical staff felt was not adequately addressed by the applicant.

On this next slide, the purpose of this next slide is to try and refamiliarize you with the four basic sections that involve SRP 14.2.1. And that

is the initial test program review, and essentially that takes a look at all -- at the expectation that there's staff review of tests greater than or equal to 80 percent.

And also, take a look at the lower power ascension tests that were done for initial testing to ensure none of those tests were invalidated. And guidance is given on -- in Section III.C on how to take a look at those differences and make a judgment call on what to do with that.

The next thing is plant modifications. We expect -- we, and also the technical staff, take a look at the effects of plant modifications on normal plant operations, and also on abnormal operating occurrences, or AOOs.

The next thing we look at is power ascension test elimination justification, and that is not just only from transients, but we also request that the applicant respond to other issues that may have occurred due to EPUs. A good example of that would be the increased bus duct air flow that caused elimination of the bus duct -- bus ducting and resulted in an LER from Clinton.

And we take a look to see how they implement that into their testing program. In that

case it would be: how do they improve their vibration testing? Or how did they take a look at that to incorporate that into their vibration testing?

And, finally, we take a look at their proposed EPU testing program for --

CHAIRMAN WALLIS: On the test elimination, you have a bullet here which says, "Justification for eliminating tests coming from the licensee." Are you clear on your justification for requiring the test?

MR. PRESCOTT: For requiring elimination of --

CHAIRMAN WALLIS: What's your justification for requiring the test in the first place?

MR. PRESCOTT: We take a look at whether their analytical methods, if that's what they're proposing, if you can -- that's actually on the next couple of slides.

CHAIRMAN WALLIS: You're going to address that.

MR. PRESCOTT: We're going to address that.

CHAIRMAN WALLIS: Okay.

MR. PRESCOTT: And this is just that staff guidance acknowledges that licensees may propose

justification for not performing certain testing, and that the supplemental guidance is in III.C for staff evaluation of that justification.

And the next slide hopefully will address what some of your questions are.

Some of the factors that are considered by the staff are -- as stated on this slide are operating experience, thermal hydraulic phenomena or system interactions, computer modeling, plant operations and use of procedures, as well as about three or four other areas.

The ones listed here -- bulleted here are the ones most frequently addressed by an applicant as a way to propose not just -- justification for not performing testing.

The operating experience that they propose, we asked them to address operating experience at their plant and facilities with similar plant design. Thermal hydraulic phenomenon and system interactions, that would relate to -- and we have other people here to answer specific questions on this, if you have those, but that would take a look at -- we would expect, or it's in the other SRPs that are done by the technical branches that they take a look at thermal hydraulic phenomenon system interactions.

And next would be computer modeling. As you're well aware, that's been an area of focus where we expect that the technical branches will take a look at the computer codes that are being used by the applicant to justify not doing any potential transient testing.

CHAIRMAN WALLIS: Is this the slide where you respond to my question about what's the justification for requiring the tests?

MR. PRESCOTT: Well, the justification for not requiring the test?

CHAIRMAN WALLIS: Or for requiring. Why do you require them in the first place and then ask them to explain why they're not doing them? I mean, you must have some basis for requiring these tests.

MR. PRESCOTT: And, again, that goes back to the first -- one of the earlier slides where we discuss -- we take a look at their initial testing program, and 14.2.1 was based on Reg. Guide 168, which required testing. And the bases for that testing was -- was a number of things.

One of those things being to test the plant for that had been designed -- had been designed might have been a first of a kind plant, so, therefore, testing of certain systems may have been

performed to demonstrate that those systems would perform as designed.

Next, you would have that -- the construction in the plant was adequately performed. Some of that testing was performed to show that the plant had been constructed as has been laid out by their construction program.

Finally, another big piece of that was the operating experience or the operators. We would take a look at -- the NRC wanted to take a look at the adequacy of the operator training, because back in those days the familiarity of operators with the plant would not have been on the same level that it is today.

So you have these -- these really three call them big ticket items of where -- of why the plant testing was required. Now you step forward 30 years, and now the staff evaluates -- takes a look at what was proposed in Reg. Guide 168 and looks at additional -- looks at additional items that we think are relevant, or that we have determined to be relevant, to determine whether or not they need to do tests.

MEMBER DENNING: So you start out, though, with -- you look at the test program that they did

during your initial power ascension and then looked at, well, what are the reasons you did those tests. So the presumption to start off with is that -- that you would have to do all of those tests if the changes affected all systems.

And so you're looking for -- or you're allowing areas where some system is not -- is clearly not affected, that a test related to it would not be required. So, I mean, it looks to me like the presumption to start off with -- and I think you may have an argument with this -- is that basically you'd have to do all of the tests that were initially required, except for those that aren't necessary because they just don't affect the systems or there's some -- there was some part of the rationale as to why you initially had to do that test that's no longer valid.

For example, like the analytical methods. You might require the test initially, because you want to make sure that your analytical methods are adequate, and you could say -- you could make an argument that, well, gee, I'm still within the same range of transient, so I don't really -- that's no longer a good reason for a particular test, right? I mean --

MR. PRESCOTT: That's correct.

MEMBER DENNING: Right. Now, if you look at the large, integral transient tests, which are the ones that are of greatest concern to us, are they the ones where -- where the plant doesn't really want to do them for various reasons, and there are reasons that are legitimate.

One of them is a risk reason, although that risk is extremely small. I mean, you don't want to put the plant through that, because there is some risk. But we kind of all agree that the plant is going to go through some kind of integral -- integral transience at some time or other.

So those are the ones that are really kind of the focus here of our concern is that -- is that you'll have not just those four areas of consideration. You have seven areas of consideration that could perform the basis for saying, "We don't have to do a particular test." Okay?

MR. PRESCOTT: That's correct. And just to go back on what you were saying about the risk implications, there hasn't been a licensee yet that has used that as their bases for proposing not to do a test. It has always been one of the other options that has been one -- multiples of the other options

that have been chosen to not perform testing.

The risk right now -- I believe there is one licensee that is coming in currently with a risk proposal of why they don't need to do the testing. But for the other evaluations that have been done to date, risk has not been proposed as the main reason as to why they are not going to do tests.

MEMBER DENNING: But there is clearly a benefit in these integral tests, in that you test not only components that are changed, but in a single test you also check changes that might have been made in control systems, and there is also the benefit in these integral tests of just testing the unexpected.

And, clearly, you'd like -- if there is some problem, if there is some component that's going to take more and break something, you want to know it. I mean, better to do it during the power ascension than it is to have it happen two years later unexpectedly at 2:00 a.m. Right?

MR. PRESCOTT: Right.

MEMBER DENNING: So there's a benefit. So, and that benefit is awfully difficult to quantify, but it's recognized that it's there. Okay. So somehow they -- you need an excuse to not do it that must have a positive element to it that's -- I mean,

the positive element to do it is you don't want to -- even though there's a small risk in doing this transient test, you don't want to incur any risk that you don't have to.

So if we perceive no benefit from doing one of those transient tests, then that's a good argument to say well -- "Well, we don't want to incur any risk if it's of really no value." Okay?

The problem that I see in your considerations is that you're considering all the right things, but I don't think you're doing it in a structured manner, and you're not doing it in a way in which you have criteria that are very clearly defined.

I think it's all kind of on the side of the applicant, in that there are all sorts of considerations as to why they might make an argument that, while I don't want to do such-and-such a test, I don't see any clearly-defined criteria that say -- say -- you know, provide limits to that.

MR. PRESCOTT: Right. And there's a reason for that, and let me -- let me try and explain that, and hopefully you'll get my point. We initially put into the procedure, into the SRP, specific criteria that the licensee would have to meet. And one of those specific areas was in the reduction in

margin to safety, trying to follow the guidance that's given in 9900 for how we -- when we get an NOED, the staff gets an NOED, how we would take and look at some reduction in margin.

When we did that, other staff questioned whether that was really a good idea. You have to remember that this procedure is written for -- I don't know how many plants. There's like 80 plants. We're trying to deal with 80 plants here that are all -- some can be grouped into, you know, lightwater reactors of a specific design, but they have all done little things and tweaked this and redesigned that, and so they're all configured in a different way.

So to come up with specific structured criteria that they would have to meet was -- was -- we considered an insurmountable task, because of the uniqueness of the plants that are, you know, in the industry today.

So coming up with specific criteria, like saying if you go below 10 percent margin -- your margin of safety in a specific area, again, you kind of have to step back and look at, okay, it was -- they went below -- we'll just make up an example where they went below, in feedwater, their 10 percent margin of -- 10 percent margin in their suction pressure to the

feedwater pump.

Some people might say, "Well, the safety significance of that doesn't warrant a test, or it may not be significant enough to warrant a test, or that even with a reduction to a little bit less than 10 percent we still have a comfort level that that system will perform."

So you end up in this -- in this argument of, okay, you've crossed the criteria, but for this particular plant is it -- is it relevant, or is it not relevant? So we couldn't come up with specific --

MEMBER DENNING: Well, I think those criteria are a little more specific than -- would you bounce -- can you go -- bounce out of your presentation there. I'd like you to put up something that I've done.

MR. PRESCOTT: Is it at the end or the beginning or --

MEMBER DENNING: Well, you'll have to get out of there.

MR. PRESCOTT: Okay.

MEMBER DENNING: Go to the desktop. Let's see. Just escape. Okay. There we are.

Okay. Now, I'm not implying that this is what should be in there. What I want to get a feeling

-- I don't mean to imply that this is what ought to be in there, but I wanted to get a feeling for if you made a more structured approach, why it is that we can't have criteria along these lines.

So basically, as I looked at this, you start off and identify each of the tests from the initial startup program, state the objectives of that test, determine which systems, operations, and procedures are changed by the upgrade, assess whether the test is affected by the changes, and, if it's not, then the tests can be omitted. Determine whether other tests will be performed that will be -- that will assure that each modified component will perform as intended. If not, an integral transient test is required.

Assess whether there are multiple modified components such as the system is effectively new. If so, transient testing is required. Assess whether analytic modeling capability encompasses the change of range in parameters. If not, transient testing is required.

Assess whether physical phenomena are -- you get the idea here. So --

MR. PRESCOTT: Yes.

MEMBER DENNING: So to kind of go down

here, to start off with, what were the purposes of those tests? And then, to have more specific criteria here as to, when do we need a transient test? Or when don't we? Because, again, as I read the considerations, I don't see any criteria. I see considerations, but I don't see where you say, "Okay. That's not a good enough" -- you know, there really is a reason to perform a transient test, and you haven't provided good enough excuse.

So that's the question. Is it -- is that too prescriptive for some reason?

MR. PRESCOTT: Again, I think it may be too prescriptive for what the intent of this procedure is. And this a programmatic overview of the power uprate testing program that the licensee proposes to do.

Where I would see the benefit of some of these bullets -- proposals going -- and, again, I'm just seeing this for the first time, but --

MEMBER DENNING: Yes.

MR. PRESCOTT: -- I think, having read it real quickly, some of these I believe that we do address. The ones that we don't address I believe would be addressed by the specific technical area of review.

MEMBER DENNING: But see, what I don't see is -- you address the issues, but I don't see the criteria that you use in saying it's good enough or it's not good enough.

MR. PRESCOTT: Well, let's do an example. Like MSIV closure testing, I wouldn't expect this procedure to give the specific criteria of where the valve should close within a certain timeframe. Okay? The .2 milliseconds. Okay? That criteria would be spelled out by the technical branch that has responsibility for reviewing the main steam system.

Another example would be code analysis. Whether or not the code is adequate, that decision is made by the technical group. It is not -- it's not made by me. We just -- we are in the business of ensuring that what we -- we try to take a holistic look.

The specific criteria that would be looked at about whether -- if this bypass valve, there's only seven open, that criteria would be spelled out, or it would be better handled by somebody who has the specific knowledge in that area, not by this procedure.

Does Mr. --

MR. HAMZEHEF: Let me add something. This

is Hossein Hamzehef, the Quality and Vendor Branch Chief. I think your thoughts are very good. But if you look at the procedure that we have in place, number one, there are some general design criteria requirements that the licensees have to perform the test. And these are the requirements.

And now, in III.C, the intention is to show under those seven criteria how a licensee can justify not to perform those tests. And your bullets are already included in those seven criteria, but that is the licensee's responsibility -- to look at these and come back and tell us why they don't believe that that test has to be done, and then their justifications could be because the changes they made to the structures, systems, and components do not change any of the operating condition for some other situations.

And when it comes to us for review, then those criteria would cover all these things that you have specified, but not in a specific term, because that's the licensee's responsibility to tell us why they are not supposed to perform those tests.

And then, I am almost positive -- I think Bob and Paul can correct me -- that in the past submittals I am almost positive that they have used

some of your bullets to justify why they did not have to perform the test. Then, it came back to us, and we went to our specific disciplines asked them to review the justification, and if it made sense we approved it. If it did not, then we will go back and require those transient testing.

MEMBER DENNING: It just isn't clear to me when you would ever deny a request not to do one of these large transient tests, and I'd like to get a better understanding. I mean, I see coming up a system that's going to have significant modifications, and they're going to come in and ask not that -- to do the test.

And I think because of the fuzziness of the way the considerations are done that you could very well accept those, because I don't think you have clearly kind of established, well, this is the line at which we think you really do have to do transient tests, because perhaps of this area of -- they do uncover things that we haven't thought about.

So if you had a system that was -- had a lot of modified components, would you say you've got to do a system -- full system test that would -- full transient test?

MR. PETTIS: That would be a function of

the secondary technical branches doing the review. The SRP is basically the higher level document, outlines the guidance to the staff. There is probably eight or ten secondary technical branches that are engaged in performing the technical review -- plant systems, reactor systems, PRA groups -- and we have had those same questions in the past.

To the extent of the modifications, it's up to the secondary tech branch to say that those modifications are extensive enough to warrant either transient testing or other types of testing. And in the past the EPUs, even the ones that were proposed up to 120 percent, in general did not require or necessitate the need for large system modifications. Most of the modifications were balance of plant, they were handled through the typical tech spec surveillances, quality assurance of programs, bench testing of components.

And the secondary technical branches, like I say Plant Systems Branch, who evaluates the BOP systems, would draw a safety conclusion that the argument that was proposed was satisfactory.

MEMBER DENNING: Well, again, I see the considerations that you have been the right considerations. I just don't see the criteria along

the lines of, what's an adequate level of argument? You know, that's what I'm missing.

MR. PETTIS: Yes. That wasn't done by mistake. The development of the SRP never focused on being so prescriptive to have thresholds that would trigger when a certain test would be performed. It was basically designed as part of the overall review standard for the EPU, which has come before ACRS on many occasions, which was the RS-001.

MEMBER DENNING: Well, again, the types of things that are up on the board here now, they are not very specific. I mean, I don't see that -- and, again, I do recognize that this requires judgment. But, again, I just don't see a lot of guidance for the lower level reviewers coming out of this as to -- to what are the benefits of large transient tests, and then what's an adequate argument? What's the adequacy of the argument?

MR. PETTIS: In most of the ones that the staff has reviewed to date, as you're aware, there has not been a need on the tech branch side to require the licensee to perform the traditional large transient test, which was the MSIV closure and the generator load reject.

MEMBER DENNING: Right.

MR. PETTIS: Those were analyzed in Chapter 15 of the FSAR. They are -- like you mentioned earlier, those are events that most likely the plant would see at some time down the road. But the feeling on the part of the staff, they would have to have much more basis for justification to deny that request, other than condensate feedwater, which is --

MEMBER DENNING: Tell me, though, why is the burden here on -- it seems to me that the burden is in the wrong place. That they have to come up with a strong justification not to do the test, and we -- and I'm not sure that we're seeing the burden turned around.

MR. PETTIS: Well, it's not that unusual. The burden, in a lot of technical cases, is on the licensee. The burden is not on the staff to be --

MEMBER DENNING: Well, that's what I think. I mean, I still --

MR. PETTIS: -- prescriptive. The licensee proposes an alternative to doing something similar to a reg. guide. It wouldn't be that much difference in that the reg. guide would embody the staff technical guidance. And if used by the licensee, fine. But the licensee can propose an alternative approach to that same --

MEMBER DENNING: Would you say for every EPU, regardless of what has changed and what has not changed, there is some potential benefit that could come out of that large transient test? There is potential, because there is unknowns out there that -- that we may just not have understood for a variety of reasons.

I'm not arguing that we didn't have them for every -- it's just that there is -- we have to recognize that there is some potential benefit. Now, I see negative sides, and I don't like to take that plan and have another transient on it and impact it. You know, so I see some arguments that say, "Well, you'd better have some justification for doing these tests." But I'm still having a hard time seeing where you would draw the line and say you've got to do the test.

MR. PETTIS: Well, I think just my own -- my own perception is that somehow these transient tests have taken on a life of their own over the last five years of doing this. They were originally spec'd in the GE topical report, the ELTR1, the ELTR2 documents that the staff approved back in the '94/'95 timeframe.

I think when the EPU was first in its

infancy there was a reliance on GE's part, let's just do all of these types of tests, because it seems like it's prudent engineering to do that. And the ELTR1 document does have limitations on when you do the MSIV and when you do the generator load reject. And they had to do with the thresholds of power, 10 percent to 15 percent above any previously recorded testing that the plant had experienced.

Over the years, there were arguments made with respect to newer plants coming online, operating experience that showed a correlation between transients that took place at the plants. In the case of Vermont Yankee, for instance, they had made an argument that they had extensive recorded documentation with pressure transients all the way up to 100 percent power. And those tests, when correlated with the model, demonstrated that they would be satisfactory at the 120 percent power level.

The staff also looks at the codes and looks at the transient codes, and already looks at the margin that's in those codes and has pretty much determined that up to 120 percent power, for the ones we've looked at so far, the codes were fine. They would predict performance.

So here we are today, 2006, we've had the

benefit of maybe 14 EPU applications that have come in, the bulk of which came in under the pre-review standard application. It was just Waterford and VY. And now there's a body of knowledge out there, and there's kind of a groundswell of activity with respect to licensees providing adequate justifications for why this testing is not necessary.

We're only talking about those two tests. There's many other transient tests that take place within the plant, but somehow the focus has been on those two tests particularly. Some of the applications that are in-house right now, like Paul had mentioned, are actually taking advantage of this risk argument in which their applications do contain a little paragraph with respect to the additional risk involved in performing these.

But primarily they have all based their justifications on Section III.C and primarily the operating experience, taking advantage of other plants, both domestic and foreign plants, some of which have been upgraded and some have -- and some have not.

But the technical review branches look at all of the applications, they look at the need for testing, and if they need testing, or additional

testing, or different tests in order to make their safety determination, then that's what comes up from the secondary review branches to us, because we're basically the programmatic gatekeeper of the SRP.

And it's -- you know, I think the test -- and maybe Paul could supplement this -- but the test is not a go/no go test. When we talk about the large transient testing, somehow it seems that some people are of the opinion that if we do these tests we have -- we have totally validated the EPU and validated the entire integrated response of the plant, and that really isn't the case.

MEMBER DENNING: Well, I certainly recognize that.

If you want to, you can now bounce -- can you bounce of that and back into your thing? Or shall we make an effort --

MR. PRESCOTT: I'll try. I'll try. But just to add to what Robert was saying is that you have to also look at the fact that we've -- we've -- since Reg. Guide 168 has been issued, since the earlier plants have been tested, we're now talking 30 years later, we have a great deal of operating experience. We and the industry have a great deal of operating experience that we can take a look at and factor into

the calls that we make on whether testing is adequate.

My personal belief is this, is that the technical review branches are good at what they do. They know feedwater systems. They know the main steam systems. The know whatever system they're looking at. When they take a look at what the proposed modifications were to that system, it's my belief -- and us acting as gatekeepers, making a final call on whether or not their review was adequate, I think we do a good job at taking a look at EPUs.

And then, when you put the operating experience on top of that, I think it gives us some -- some assurance that what we've approved was adequate, and that we have looked at it adequately.

And as you know, this SRP has been out for a long time and has had lots of comments involved with it. And one of the things that we did to try and give ourselves -- we and our group did was to take a look at LERs and operating experience that's out there in the industry to see if it looks like we have really missed some huge gap in the testing requirements that -- the testing we've been asking to do, and it just doesn't show it.

I mean, when I took a look at the LERs, there's maybe four LERs related to EPU, two of them

you are well aware of are the -- have to do with the steam dryer/separator issue, which live transient testing would not show any impact on. One was that -- was the bus duct conductor delaminating that happened at -- well, it doesn't matter where it happened, but the fact that the bus duct conductor delaminated that wouldn't have been shown by large transient testing.

The one that did show up on the radar screen was the HPCI fill line event that occurred, and that was prior to this SRP being implemented. And I think now that this SRP has been implemented, we give more focus to the staff, the technical staff, hopefully, on where to look.

And I think a good example of that was the questioning attitude they had towards the computer modeling that was done for the feedwater system at VY, and questioning it enough to say that, "Hey, we think you need testing for your feedwater system," which it looks like VY is going to do.

So when you take into account the historical perspective, when you take into account the staff's technical ability to do these evaluations, and when you take into account that we're not looking at some homogeneous pool of plants that we have to kind of give leeway to the staff to do these reviews, I

think -- I think we've done -- that the final product here is the product that's going to -- that best suits the needs for what -- what's out there, and for the staff's review.

MR. PETTIS: Let me just add something to that to make you feel a little bit more comfortable. The plants that were done under the new review standard, which the SRP was developed to fill a gap in the staff's knowledge with respect to EPUs, this was developed as part of the review standard. The first plant was Waterford, the second plant was VY, and currently we have several other plants in-house right now.

If you look at the guidance that this document has in it as we speak, obviously it's not prescriptive and it was not intended to be that way. But if you go back and look at the dialogue between the staff and the licensee, all the way through the application stage, through the acceptance stage, through the RAI stage, you will see a story that's told that is extremely detailed and extremely articulate and analytical in where the staff questions the licensee with respect to items like the need to not perform certain transient tests.

The record would demonstrate that the

staff is not in a passive role in which the licensee submits an application, cites operating experience at their plant or some other plant, or brings in some other justification, and then that's the end of the story. It's just the opposite, and especially with the plants that we just completed, which we had the public meeting up in Vermont.

MEMBER DENNING: In fact, I don't argue with that. And I think that the discussion in VY was a very good one from looking at the record. But, again, I don't think you're providing much in the way of guidance on where the boundary is. But please go ahead and continue with the next --

MR. PETTIS: Well, even the lack of guidance produced a very well-documented story between us and the licensee. And that demonstrates I think what Paul was saying, in that the technical branches have their own story to tell, they have their own thresholds, they have many, many, many years of experience, and they are asking the types of questions, based on the extent of modifications and other factors, back to the licensee. And you're probably going to see more of that with the current applications that we have in-house right now, which is Browns Ferry and Hope Creek.

MEMBER BONACA: How are you going to deal with -- there's new arguments coming in regarding risk associated with transient tests. I mean --

MR. PETTIS: Well, the only applications -- there's two applications that included no more than about a paragraph or two of a risk -- I won't say it's a risk argument. I'm not a risk person, so it's all foreign -- all foreign to me. But basically if you look at the seven factors that are in the SRP, one of them happens to be risk.

Although none of the applications are risk-based, several licensees have now taken the opportunity to include in the application a small discussion about risk and about how the performance of the transient testing would impact that risk.

MEMBER DENNING: And there is a statement that risk would not -- can't be the only determining factor --

MR. PETTIS: Yes.

MEMBER DENNING: -- which I certainly support.

MR. PETTIS: Yes. You'll probably see more of that, because the Hope Creek application and the Browns Ferry application actually have a little discussion about that, which would then necessitate,

say, the PRA Branch, which is one of the secondary technical branches, to review that component of the justification.

MEMBER BONACA: Yes. You know, this whole discussion of risk has been true even doing this 30 years ago. Everybody was trying to make some, you know, qualitative judgment on that. And you could argue either way. I mean, there is some value in doing a control test in a controlled fashion, especially for an anticipated transient that is going to happen anyway, and not reject. It's going to happen. And so there is -- you know, one could argue otherwise.

All I'm trying to point out on this, you know, somewhat supporting Dr. Denning's point, that they can introduce all kinds of arguments, and then the evaluation becomes so vague. I mean, there is no criteria, there is no -- almost no basis for making the judgment right now. You know, they are introducing a new issue that we haven't seen yet.

MR. PETTIS: Yes. I think with respect to the new applications that have taken the opportunity to put that risk paragraph in, I think that's more supplemental than primary. Their primary justification for not doing certain things follows a

technical -- well-documented technical argument. I think they are just adding a little gravy in saying, "Oh, by the way, there would be no benefits from a risk standpoint to perform these two tests."

MR. PRESCOTT: The intent of this next slide is -- it may read like why we feel it's okay that licensees give us justification. But that's not the basis we work on. I think the staff should be given credit that -- that we keep an open mind on what we receive in the application, and our adequacy of review.

One of the areas I did want to touch on is that after 30 years of operating experience in the nuclear industry some credit needs to be given to Appendix B and the tech spec surveillance requirements that the NRC imposes.

Appendix B, as you know, criterion 11 requires that the licensee have an adequate testing program. We require in our regulations in 50.59 that they do an adequate evaluation of any design modifications that they do. And also in line with that, that they propose adequate testing for the modifications that they've done. And that's what this slide is really trying to bring out.

And, finally, and it's not -- we're not

trying to look at this last defense-in-depth thing, but obviously tech specs -- tech spec requirements that the important safety systems tested -- as you know, when a plant is coming up in power, the expectation is that they meet their tech specs before they go to the next level.

And as part of that, systems are tested at certain power levels as the plant is brought back online. And this is part of the overall consideration that's given, that is justification that a licensee may propose for not doing additional testing, and that the staff takes a look at to -- for the purpose of their review. So that's the purpose of this slide.

MEMBER RANSOM: Well, one item that seems important is the operation of components beyond their design. For example, the main steam isolation valve they mentioned in that closed -- certainly, its ability to close is a function of the flow rate as well as the pressure that it experienced. In most of these uprates, the pressure remains the same, but the flow rate is 20 percent greater.

So you wonder, will that valve function as it was designed? And it would seem to me that's a justification for added testing.

MR. PRESCOTT: And that is a tech spec

test. The tech spec test --

MEMBER RANSOM: But that's one that was I think eliminated from Vermont Yankee.

MR. PRESCOTT: No, there's no -- no. The tech spec test cannot be eliminated. How they perform the tech spec test is they close one MSIV at a time, and it's done under that flow. Now, they're not all closed at the same time, as would be proposed for the MSIV C test. However, there is a tech spec test that requires testing of those valves.

MEMBER RANSOM: Well, the other aspect are the water hammer effects on the lines are these -- under transient closure valves like that, and you would wonder, is that something that should be examined by the large transient tests? And there's no way to do that except to go through that test.

MR. PRESCOTT: But, again, I think that would go back to the technical branch and their review, and whether or not they think there would be water hammer that would occur at the plant at the EPU rated power. So I don't feel comfortable answering that question.

But if we're talking a water hammer event as you said, that would be an event that would be looked at under one of the Chapter 15 accidents by the

staff. And that would be their call as to whether or not they felt that potentially there could be a water hammer event.

MEMBER RANSOM: I would think that, you know, this -- maybe the spec should include something like that. If there are components that are operated beyond their original design basis, that they would either have to provide separate effects testing or large transient tests or some way of verifying that that component will, indeed, function as it was -- and must function under the uprated conditions.

MR. PRESCOTT: Right. If you take a look, one of -- I've done a lot of research into this. I've taken a look at what has happened overseas with power uprates and what testing other commissions proposed. One of the things I also do is take a historical look as to why -- why did we do testing?

Why did we come up with Reg. Guide 168 in the first place? And one of the interesting things when you start reading about the historical aspect of this is that most of these plants were originally overdesigned in the first place. They weren't designed to the minimum requirements. They were designed to some higher requirement.

So most of the plants, as I said, when you

think that they're on their margin, and that the EPU is bringing them up to some margin, safety margin that's borderline, that's really not the case. The reduction of margin to safety has not been seen to really be pushing the limits in any area.

So -- and, again, that's more of a technical staff review. What I'm trying to give you is some historical perspective here. But in my own experience with testing -- I have quite a bit of experience with testing. But I'm not seeing where we're approaching some margin of safety that's -- that some branch -- some technical branch should say or call out that they need testing to prove a water hammer event or a --

MEMBER RANSOM: Well, certainly, you know, over time and history there has been -- like Appendix K for a lot of the thermal hydraulic aspects of plants, there was margin there undoubtedly, but nobody has ever been able to quantify exactly what that margin is. And so while margin exists, I don't think you can eliminate the argument that margin is being eroded when this happens, when you uprate the plant.

And so the question is: is it significant or not?

MR. PRESCOTT: Right. I did not mean to

imply that there was no reduction in margin to safety in any case -- in all cases. All I'm saying is that the technical staff do review that as part of their review to ensure that that would not be an issue. And if they would feel that some margin of safety was being eroded in some certain area, I would hope that they would call out testing.

What we're saying is gatekeepers of the program, the overall program, we're not -- we haven't seen where some great amount of margin of safety has been reduced.

CHAIRMAN WALLIS: Well, I don't quite know how to decide this. I mean, I read your review plan and it -- it makes some sort of sense. It's full of a lot of regulatory language that needs some interpretation. It's got a lot of generalities, and you have to rely on the reviewer to do the right thing each time.

And then, I look at Rich's very crisp and succinct set of steps to go through, which all seem to make sense, too. And I haven't really had -- you know, haven't had time to go and say, well, why one shouldn't do that, so I don't really know how to come down on this.

I think, generally speaking, I'm in favor

of a rather crisp, succinct set of steps which are clear rather than a lot of vagueness which is up to interpretation by different reviewers and can be sort of weaseled through by some clever, you know, arguments from a licensee.

MR. PRESCOTT: I guess my response to that would be: is that -- as you're well aware, this -- because of the amount of interest in this procedure, this SRP, that it has been in draft form for --

CHAIRMAN WALLIS: But you haven't changed things very much.

MR. PRESCOTT: No.

CHAIRMAN WALLIS: This little editorial stuff is --

MR. PRESCOTT: And I'm getting to that. And I want to get to that. And my point -- my point being is that, obviously, you've had a lot of staff interaction, as you know, even in front of the ACRS. But nobody has proposed some cleaner or crisper way to do it than what was initially started out.

Like I said, it would have been great to put -- if they reduced margin of safety to three percent, we need to --

CHAIRMAN WALLIS: That's not what --

MR. PRESCOTT: If they reduced the

feedwater flow, the suction pressure to less than 50 pounds, then we need to do a test that --

CHAIRMAN WALLIS: That's not the level of detail we're looking for at all.

MR. PRESCOTT: Okay.

MEMBER SIEBER: And I personally don't think that's a warranted way to look at these plants anyway. Most of the components, particularly pumps and valves, come in classes of components as opposed to each one being specifically designed for a given application in the plant. So there's still a lot of margin there, just because of the way that these components are purchased off the shelf.

You know, in PWRs, components are 2,500 pounds or better, and that's where you set your relief valves. And so there's a lot of margin there, and --

MR. PETTIS: Let me add one thing. I almost hate to use the term, but there is a little skill of a craft involved in this review for EPU that the staff employs. There is consistency in the reviews. It's not like they take people off the street and say, "You're going to do the EPU for Browns Ferry Units 1, 2, and 3."

So there has been consistency in reviews. The tech branches are well aware of what is important

to them in making a safety determination, which goes into the SE. Some of these other issues, like water hammer and hangers coming out of the wall, and, you know, HILTIES and that kind of thing, obviously those are all the remnants of some of this activity. We get reminded from time to time this may fall into the reliability side of the house as opposed to the safety side of the house.

So there is certain aspects of that we have to look at. A lot of the modifications made on the EPU are on the secondary side, which tend to fall -- although we don't use the argument that much, they tend to fall on the non-safety-related side. So the staff is looking very critical.

And, again, even though this is not prescriptive, if you follow the dialogue between the staff and the licensee, you will find very prescriptive dialogue back and forth. So that must be coming from the fact that we have, you know, well trained staff, professional staff. They've seen this for many, many, many, many years, and they're making a determination as to what's important for them.

CHAIRMAN WALLIS: Here you're telling us, then, that you don't think any value would be added by having a sort of -- a one-page summary of the steps to

go through a la Rich Denning, rather than many pages of regulatory terminology which somehow the staff interprets. You don't think any value would be added by having something sort of summarized as a set of steps somewhere in this stage, which would be very useful to particularly some new reviewer.

Let's say I go through these steps, and I show myself that all of these things are okay. You don't think that would add any value to this SRP?

MR. PETTIS: No, I didn't say that.

CHAIRMAN WALLIS: Well, it seems to be the trend of the conversation here.

MR. PETTIS: Well, we've lived with the SRP through many evaluations in its current state, and we've produced many SEs that look very technical and very succinct and very articulate. And, of course, we can always look at, you know, recommendations to the SRP and have the staff take a look at it and see if some of these could be incorporated.

But I guess the point I want to make is that don't view a lack of specificity as being an indicator that the end product is not without sufficient detail, because it is. And you'll probably see more of it at some of the plants that we currently have in-house that may not have an adequate

justification for doing some elimination of power ascension tests.

MEMBER DENNING: Let's see your last viewgraph. Does that add a different perspective or --

MR. PRESCOTT: This last slide was to give the perspective that, since we've implemented the EPU, that there has been testing proposed by the staff. One was a proposed license condition, which was the VY, for the condensate/feedwater system. And one is the proposed manual trip from 30 percent power, and it's being proposed -- and this one is being proposed by the licensee, Ginna, for their EPU test program.

CHAIRMAN WALLIS: Why would you go back to a lower power than you tested earlier on?

MR. PRESCOTT: What, for the 30 --

MEMBER DENNING: Why 30 percent?

CHAIRMAN WALLIS: Presumably they've already tripped from a higher power than 30 percent --

MR. PRESCOTT: Again, it's --

CHAIRMAN WALLIS: -- during the history of the plant.

MR. PRESCOTT: Well, you know, again, I don't know if the staff has made -- this one is in-house, this proposal is in-house. So the staff has

not made a final determination. But having viewed what they propose, it's based on the fact that they'll do a 30 percent power trip, and it will give enough of a simulation of what should occur at power for the systems that would be involved in this sort of trip.

MR. JONES: This is Steve Jones from the Balance of Plant Branch. I can address that question briefly. In the case of Ginna, their turbine missile protection relies in part on the overspeed protection system operating in a reliable way to keep the turbine below its design speed.

For that turbine, they have -- they are replacing the rotors and the high-pressure turbine in order to ensure that the -- that given those changes in the internals of the turbine it responds as designed and stays below its design overspeed in the event of a turbine trip. This test is just to confirm that that will, in fact, be true. It's not necessary to go up to 120 percent in order to test that particular design feature.

MEMBER SIEBER: It seems to me that it would be, though, because the extent to which the turbine overspeeds and, thus, the trip setpoint is proportional to the amount of stored energy, and the higher power level you're at the more stored energy

you have in the feed system that can flash back through the turbine and scratch things, and so forth -- but a 30 percent trip rate doesn't -- it will assure you that the mechanism for overspeed operates at the right speed, but it does not tell you much about how the turbine will respond after the trip occurs.

CHAIRMAN WALLIS: Well, maybe to get back to the list that my colleague put up there, maybe what we're telling you is these are the questions you can expect from us next time around. If you don't want to change the SRP, you're still going to have to respond to these kind of questions.

MR. PRESCOTT: Well, we didn't say we didn't want to change the SRP. Obviously, we've had an open mind for the last three years, and we will look at these. We didn't have an opportunity to look at these earlier, and I'm -- so don't get the idea that we're outright saying that they're not --

CHAIRMAN WALLIS: Maybe the best way for us put these thoughts on the record might be to put them in a letter. I'm not saying we're going to do that, but it might be. Then you could respond.

MEMBER DENNING: I don't have any more questions. Again, I haven't been made more

comfortable by the discussion. I understand that you're content and feel the process is working correctly, but I still have concerns about it.

If anybody else on the committee has any questions, now is the time, or you're going to risk having a very long lunch hour.

(Laughter.)

MEMBER SIEBER: That's not a big --

CHAIRMAN WALLIS: Maybe we can fill the time with --

MEMBER DENNING: Any other questions? No? Okay. Thank you very much.

CHAIRMAN WALLIS: Thank you.

So you want to have a large lunch, and do a large transient test?

(Laughter.)

It would appear that we have gained a lot of time. Does the committee wish to discuss this any more? So you're ready to take a break?

Well, we can't start before we specify, so we will take a break until 12:45.

(Whereupon, at 11:21 a.m., the proceedings in the foregoing matter recessed for lunch.)

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

(12:46 p.m.)

CHAIRMAN WALLIS: Please come into session.

At this point, we're going to ferret out the truth about the fluence methodology. And I turn to Dr. Denning to lead us through it.

MEMBER DENNING: At the staff's request, the Westinghouse Owners Group submitted a topical report regarding the FERRET code for least squares evaluation of reactor dosimetry. The topical report was submitted in July 2004 and later revised in March 2005.

Based on staff comments, the staff issued its final safety evaluation of this topical report in January 2006. The ACRS requested a briefing from the staff on this fluent methodology -- fluence methodology.

This presentation is for information purposes only, and we do not plan to write a letter.

Historically, there have been major discrepancies in the measure to calculated ratios for fast flux for the various foils in the surveillance capsules. With time, the reactor cross-sections, foil cross-sections, and the analytic capabilities have

improved substantially.

FERRET uses a least squares fit method to optimize the results from the foils and the analysis, which substantially reduces the reported variance in the results. They report an adjusted-to-calculated ratio rather than a measured-to-calculated ratio as was done historically.

The comparisons with experiment are very good. In fact, they are so good that one wonders if there is some artificiality that we may be missing in the approach.

FERRET only does part of the problem, which is estimation of the fast flux or dpa at the capsule location. One then has to determine the fast flux or dpa at the critical points in the vessel wall by other analytical methods.

I also asked to have Matt Mitchell give a little discussion for you on how the results of surveillance capsules are used than in the development of pressure temperature curves, because in order to have some feeling as to why these are important we have to know how are they really used in the operation of the plant, and he will do that briefly.

But first, Lambros, why don't you go ahead and start.

DR. LOIS: I'd like to invite my branch chief to --

MEMBER DENNING: I'm sorry. Yes.

MR. CRANSTON: I'm Greg Cranston. I'm the Branch Chief of PWR Systems, and it's my pleasure to introduce Dr. Lambros Lois, who is going to discuss the FERRET code.

Overall, the debate -- there has been a debate on this ongoing for quite some time, for almost a decade. Overall, the debate has been instructive, to help clarify several issues without impacting the licensing process. Lambros was the central figure in the evolution of FERRET, as well as DOT, RAMA, and other radiation transport codes, and reactor dosimetry applications for reactor vessel shrouds and reactor internals.

He's been with the NRC for almost 33 years, and his name has become synonymous with fluence I think.

(Laughter.)

He performed the initial calculations on vessel fluence which led to 10 CFR 50.61 in 1985, and Reg. Guide 1.190 on the calculational methods for pressure vessel dosimetry in 2001.

He has reviewed and approved several

vendor and owner methodologies for calculation of vessel fluence. He is Chairman of the ANS 19.10 Standards Committee on Pressure Vessel Dosimetry which prepared a vessel fluence standard to be issued by ANS. And he is also credited by some for coining the term "low leakage core" for core leading minimizing vessel irradiation.

Doctor?

DR. LOIS: Thank you. Thank you, Greg.

MEMBER APOSTOLAKIS: Yes. But is he a nice fellow?

DR. LOIS: Good afternoon.

CHAIRMAN WALLIS: We're going to hold him to very high standards.

(Laughter.)

DR. LOIS: Thank you. Well, in today's presentation I'm going to discuss the requirements of GDC 30, calculated and measured values of fluence, the "old" FERRET, some questionable applications which created the disagreement, if you wish, or problems we had with some of the vendors that Greg referred to, the FERRET review which came about, oh, after this decade or so of disagreement with the licensee, and licensees and vendors, the new FERRET as it was formulated after a number of questions, and then --

and discussions we had with Westinghouse, and some conclusions out of that.

But before I proceed with that, I would like to point out -- well, ask a question and answer it myself. Namely, FERRET is just another code. In fact, it is not very sophisticated at all. It's a fairly simple code, and why we should have an interest in that, or you should have an interest in that.

And the question -- and the reason is that it has been quite the center of some disagreement for a long time, and the disagreement evolves from the fact that the dosimetry that Appendix H requires, it was not up to standard that the licensees and the vendors claimed to be.

For example, I have in my desk an old -- by the way, one of the requirements is that we -- we receive the capsule report within a year from the time of its removal, and we have those. And I have one in my desk, so the same plant -- the three of them, actually -- they had one capsule which has a calculated-to-measured value of the fluence at the pressure vessel base, which is about 30 percent higher, another one which is about even, one, and another one which is about 30 percent lower.

And those discrepancies are way out of

what one might expect from the uncertainties of what goes into it, namely the --

MEMBER DENNING: But, on average, they are pretty good, huh?

(Laugher.)

DR. LOIS: On average, yes, indeed,and that was one of the problems we had. There are some other capsules which they had discrepancies up to about 40 percent.

Now, when it came to licensing actions, we had a specific plan that claimed to benchmark in one of the dosimeters, which was in the lowest value. And we said, "Well, if we benched that one, we're okay. That's a measurement. It's as good as anything else."

And, of course, that wasn't really the case, and we raised strenuous objections to that, that we had to find a better way to benchmark the dosimetry for the measurement and the calculated values that we go that. The disagreement was escalated through management, and eventually I had to put my foot down so to speak and say, "I do not accept any dosimetry which is benchmarked to any of those dosimeters."

I would rather have a calculated value over code, which is benchmarked and the -- and the ingredients are the cross-sections, the densities,

diameter of the vessel, and the edges of the core, etcetera, because we know those in some accuracy, which is more trustworthy than the value of a dosimeter.

The suspicions of the inaccuracy of the dosimeter were heightened, at least in my mind, when we performed the calculation of a specific set of dosimeters. One was copper titanium, and the other was iron nickel. Either copper nickel or copper and iron, or any one of those pairs of dosimeters.

The copper titanium traditionally is much, much higher. The nickel iron are much, much lower. And, of course, licensees were interested in benchmarking it to our nickel.

And we -- I performed the calculation where I removed the iron dosimeter closer for -- the capsule closer to the vessel, to the core, by a fraction of an inch, or a quarter or an eighth or something like that.

Now, I want to remind you that the copper dosimeter and titanium, they -- they get activated, since their threshold dosimeter -- and gets activated for full access of fluences of energy greater than 5-1/2 to 6 MeV. Now, those fluxes, of course, they decay much lower than the iron fluxes, which are, of

course, another threshold, but gets activated above 1 MeV.

So, therefore, if I move it towards the core by a small amount, the iron is going to get much higher much faster than the copper titanium, which gets fast a little bit, but not a great deal. And, of course, let me remind you that we are very close to the edge of the vessel.

And all of a sudden I find out that if I were to move it a tiny bit towards the core, the values click. And all of a sudden, they come out to be exactly what one calculates to be the case. That sort of implies in my mind the fact that the possibility that the accuracy of the dosimeter location is not all that well known.

I repeated that again with another case, another reactor, and the same thing happened also again. So that sort of solidified my belief that the dosimeters were not accurately known.

Add to that the fact that most of these dosimeters -- well, in the older plants, the dosimeters were placed in the -- they were placed in the capsules after the fact. That's what they plants did -- the older plants were built, because Appendix H did not appear until a few years later after the NRC

sort of initiated its operations.

And probably I thought that this was the came, namely placing those dosimeters in there were not all that accurate an operation, because it was remotely done. And, of course, I remind you that in one class of plants, namely the BWRs, those dosimeters didn't stay placed. They were -- eventually licensees were forced to take them out of there and put them into two plants, namely Davis-Besse and Crystal River, because they couldn't hold them down because of oscillations and problems with thermal hydraulics.

Anyway, all these things I tried to convey those -- those thoughts and fears so to speak to the licensees and the vendors, and they insisted that this was not the case. So, therefore, I was forced to -- to go to the point -- get to the point where I did not accept any values which were pegged to any of the dosimetry that we had there until such time we could clarify that.

And something else that I did at that time, and this -- this -- we're talking about the early to mid '90s -- I looked at the dosimeters from Siemens in Germany and from the French reactors, at least those that I could get hold of. And their values seemed to -- calculated-to-measured ratios were

just about where you expect them to be, at about 5 to 7 to 10 percent in the C over M ratio. And so that indicated -- showed me that we were not on the right track.

CHAIRMAN WALLIS: Could you just explain to me a little bit what the dosimeters are like and where they are put and how they are mounted?

DR. LOIS: Yes.

CHAIRMAN WALLIS: Because I don't know that.

DR. LOIS: Yes. There is a capsule which is -- it's about two by two inches in diameter, roughly, thereabouts.

CHAIRMAN WALLIS: So it's a round thing.

DR. LOIS: No, it's a flat, square capsule.

CHAIRMAN WALLIS: Square capsule.

DR. LOIS: Which is -- it's about two feet long, and it is mounted halfway at the center of the beltline, the half -- at the center of the core.

CHAIRMAN WALLIS: So is it totally surrounded by water, or is it against --

DR. LOIS: Yes.

CHAIRMAN WALLIS: -- the wall?

DR. LOIS: It is against -- it is against

the wall or could be against the thermal shield or -- I mean, against thermal shield or against the skirt inside.

CHAIRMAN WALLIS: Does it make a difference if it's near a discontinuity like that? Or does --

DR. LOIS: Well, it makes a big difference because when -- the closer they are to the core, the larger the lead factor --

CHAIRMAN WALLIS: Yes. Not just closer, but you've also got it -- a change in material and property that --

DR. LOIS: Yes. That is easy. We can calculate that. We can account for it. There's no problem there. And to -- yes, indeed, it does -- in fact, if you follow fluxes, they are greater one -- that we count, you'll see them, they go --

CHAIRMAN WALLIS: Right.

DR. LOIS: -- are wavering between them. But that's not really the issue.

MEMBER DENNING: Explain the lead factor again. You were about to get into it and --

DR. LOIS: No, that's irrelevant. It's the accuracy of what we -- we were doing it at that amount.

And now, the contents of the capsule are the archival material samples, which are inserted in there and irradiated to just about the same -- as close as possible -- to the vessel. In the middle of those capsules, there are foils which consist of certain isotopes of copper, as I mentioned before, titanium, U-238, nickel, iron, and neptunium-237.

Those dosimeters are taken out and -- when the capsule is removed. The capsule -- its residency into the core is governed by --

CHAIRMAN WALLIS: It's an activation of --

DR. LOIS: Yes, the activation dosimeters and the threshold dosimeters. That's where they are, and roughly they are one cycle, one year, 5 and 15 I believe. Mark? Thereabouts.

MR. MITCHELL: That's real close.

DR. LOIS: And some plants, if they have more than three, four, or five, and so they have -- they can -- they have extra ones, they remove them later on when they need to. So that is the -- that thing there.

Now, we can calculate the -- once we know the model operation of the plant, the location of the capsule, the fuel loadings, and the materials in between -- temperatures, pressures, density, and

composition -- we can do a pretty good job in calculating what the activation is. Maybe we can also measure it. So these two things, if we are on the right track, they should coincide. And that's where the problem is that it did not really come out with this.

So calculating uncertainties in that way is very cumbersome and very tricky, and it's very important. Otherwise, we really don't know what the activation is and what we are doing as far as the exposure rates.

It's a long -- lengthy introduction to what I tried to say, but I need to give you the picture of where it is. Okay. Let me then go to the GDC requirements. So it says that we need to make sure that the pressure boundary behaves in a non-brittle manner, and that consideration should be given to uncertainties in determining the material properties.

So, therefore, uncertainties in the calculation and in the measurement of the fluence to the pressure vessel is not a matter of luxury. It is a requirement. We have to know how good they are.

To assure that the reactor pressure boundary behaves in a non-brittle manner, we need to

predict or project the value of the fluence to the end of the operating license. And that is, of course, something which we need to make sure that we understand what the calculation techniques give us.

Now, in addition to the requirements of 10 CFR 50.61, which is for the pressure vessel, we -- fluence is also used for the irradiation-assisted stress corrosion cracking, the weldability of materials, quite frequently, and, of course, the pressure temperature limits.

I think at this point I would like to have Matt tell us how it is applied to pressure temperature limits.

MR. MITCHELL: Okay. Matthew Mitchell, Branch Chief of the Vessels and Internals Integrity Branch in NRR. As Dr. Denning pointed out earlier in his introduction, I was -- I was asked as an impromptu guest speaker this morning to sort of come over and give you I think a perspective on the use of these fluence calculational numbers and why it's important for us to have reliable values that we can use for the evaluation of numerous components, both the reactor vessel and the reactor vessel internals.

As Lambros had suggested on his slide, there are number of aspects related to the reactor

pressure vessel itself for which these fluence calculations are very important. Appendix H related issues, in terms of the ability to interpret the material testing results that we get from the surveillance capsules, that we get from the Charpy tests, the tensile tests, etcetera, and to be able to put those into perspective and understand what they are telling us about the behavior of these reactor vessel materials.

You need reliable fluence values to be able to understand and interpret that data. We need reliable fluence values for the production of pressure temperature limits for normal plant operation as associated with 10 CFR Part 50 Appendix G. We need --

CHAIRMAN WALLIS: Is the energy distribution of the neutrons important?

MR. MITCHELL: Our interpretation of embrittlement data has consistently, through the years, been linked to the part of the neutron spectrum of energies greater than 1 MeV. So the way we have spliced and analyzed the data, we have agglomerated all of the neutrons of a greater than 1 MeV energy level, and used that as sort of a scaler metric for interpreting our material test data.

CHAIRMAN WALLIS: But these materials that

you radiate pick up different levels of neutron energy, don't they?

MR. MITCHELL: Yes, they do.

CHAIRMAN WALLIS: You do get a spectrum out of it that's --

MR. MITCHELL: Yes. They would see, you know, a range of energies all the way from thermal neutrons all the way up through the highest energy levels. And in fact, when you get --

CHAIRMAN WALLIS: It's the high energy ones you care about.

MR. MITCHELL: Those are the ones that we index to. Yes. You would get some contribution of all neutron energies having enough -- having enough energy to displace atoms in the matrix, but we index to E greater than 1 MeV.

VICE CHAIRMAN SHACK: How much do all of the changes in the fuel that people now go through for their things change the spectrum? This all works fine as long as the spectrum stays constant.

DR. LOIS: We account for that. We account for -- as the material -- as the fuel edges and you have twice or thrice per an assembly, which particularly go in the outside perimeter of the core, which eventually are the ones that they leak most of

the neutrons out, which cause irradiation, the plutonium that's in there produces hotter, more energetic neutrons, and more of them as a matter of fact. But we account for that as time goes on, because that's --

VICE CHAIRMAN SHACK: When Matt does his correlation against 1 MeV, he is really assuming that when he takes that data and he applies it, the spectrum is sort of the same. And in the average, I guess it is the same. I mean, it hasn't changed from a BWR 4 to an ESBWR, has it?

DR. LOIS: No. Actually, we don't have a differentiation between the potential spectral changes above 1 MeV. We assume that they -- once above 1 MeV, they all do the same thing.

MR. MITCHELL: I think another way of addressing Dr. Shack's question is to say we base all of our understanding of the material property changes with the radiation off of power reactor surveillance data and the smattering of data that we get from test reactors to give us some insights into the behavior of materials.

And I think the understanding is is that that data, as a population, accurately represents what the vessels are seeing. So in the agglomerate, you

are getting an appropriate characterizing set of data to be able to use and to be able to transfer to understanding vessel material behavior performance.

DR. LOIS: And by the way, if I might add, by the time they propagate to a pressure vessel, most of these they are smoothed out. You know, or the contribution of the plutonium component kind of disappears.

MR. MITCHELL: The slight differences are --

VICE CHAIRMAN SHACK: Thank you.

MR. MITCHELL: Yes. And then, just to pick up again, you know, certainly with PWRs I think the committee is aware that certainly pressurized thermal shock is an issue in relation to 50.61. Again, a very important evaluation that requires us to know the fluence calculations as well as we possibly can.

Then, with respect to reactor pressure vessel internals, Lambros alluded to the fact that issues of irradiation-assisted stress corrosion cracking, potential issues of void swelling, embrittlement of stainless steel internals due to irradiation, and the production of, for example, constituents like helium, which may cause weldability

issues for licensees who wish to repair their internals if they do find cracking.

All of these subjects have threshold values, have insights that need to be related to the fluence that these components have seen in service.

CHAIRMAN WALLIS: But your uncertainty about floors and that kind of thing is far greater than your uncertainty in the fluence calculations.

MR. MITCHELL: Certainly with respect to some of these thresholds, for like IASCC, void swelling, yes, that statement would be correct. We set the thresholds rather conservatively, so the magnitude of uncertainty that Lambros will tell you about in terms of the fluence calculation is only -- would be a minor component when compared to those other related uncertainties.

VICE CHAIRMAN SHACK: When we see these independent calculations for license renewal, are they independent all the way to the fluence calculations?

MR. MITCHELL: Do you mean in terms of our calculations for the vessel integrity calcs?

MEMBER SIEBER: Yes.

MR. MITCHELL: We have to assume that the fluence calculations, as given by the licensees, are, in fact, accurate and useable. So we would not go

back and reproduce --

VICE CHAIRMAN SHACK: You don't go back and reproduce those.

MR. MITCHELL: No. We would take it from that step -- we would take -- in my branch, we would take that as an input, as a given, and then work with the rest of our information that we have from prior submittals that we know about -- to characterize the vessel, and then do our own independent calculation from that point forward.

CHAIRMAN WALLIS: I think we've had examples of power uprates where they've increased the power, they've flattened the flux distribution, and the fluence has gone down because they've used a better method to calculate it.

DR. LOIS: It happens, especially with GE, both ways. Some of them went down. Some of them went up. GE obtained an approved code in 2001, I believe it was, and after that point they sort of used their own handshaking methodology.

We fixed that in 2001 and required all of the BWRs to recalculate those values, especially for the pressure temperature curves.

MR. MITCHELL: Well, I'd like to make just one last point, and this particularly applies to

reactor vessels and the embrittlement of ferritic steels, a topic that we often come to talk to you about. One point to bear in mind when you start thinking about the degree of uncertainty on some of these fluence calculations is that the way these materials behave with increasing fluence and with increasing time, you see essentially an almost saturating effect.

So the embrittlement of these ferritic reactor pressure vessels is not linear with fluence. You get a very almost asymptotic behavior, because the early part of the life of these vessels is dominated by copper precipitation, which gives the greatest magnitude of the embrittlement. Once the copper precipitates out, then you're left with only what we would call stable matrix damage as being the continuing function which continues to strengthen and harden the material and change its fracture toughness.

So when you start talking about calculations of fluence out at 2, 3, 4 E^{19} levels, you may start to think that a 20 percent uncertainty or a 15 percent uncertainty on that number seems like a pretty big value. But from an effectiveness standpoint, in terms of actually embrittling the material, because it's on that asymptotic part of the

behavior, it really doesn't have that large an effect in that range.

So just one other point of perspective to keep in mind for those --

MEMBER POWERS: I guess it take it as a reason that we'd pursue an aggressive heavy section steel research program.

VICE CHAIRMAN SHACK: Well, he says the uncertainties are all in the material. We can calculate the flows to a --

CHAIRMAN WALLIS: What fraction of these atoms actually get these major collisions and get knocked around in the matrix? What fraction of the atoms actually get displaced?

MR. MITCHELL: Well, Lambros could probably answer that question better than I could, but I think --

DR. LOIS: Well, I don't have the exact numbers, but it is -- it is one of the other measures that we have, unofficial measures that we have the --

CHAIRMAN WALLIS: Roughly speaking, how much over the lifetime of the vessel?

MR. MITCHELL: Two-tenths of a dpa?

DR. LOIS: Yes. .2

MR. MITCHELL: .2. About one-fifth

essentially of the atoms.

CHAIRMAN WALLIS: One-fifth of them get knocked around?

MR. MITCHELL: Yes, would get displaced.

CHAIRMAN WALLIS: So it's a significant --

VICE CHAIRMAN SHACK: When you get into the internals, it gets more exciting than that.

CHAIRMAN WALLIS: But it's not like a fusion process where there's sort of hundreds of events per atom, or something like that.

MR. MITCHELL: It depends upon your perspective. Having come from a fusion research background myself, I was used to thinking in terms of 50 and 100 dpa. But we're talking here two-tenths, three-tenths maybe, of a dpa over the lifetime of the plant. But it is -- because of the material and because of the copper that's available in solution, it's a very significant -- you don't have to go high to get the strengthening and the reduction in fracture toughness.

VICE CHAIRMAN SHACK: But a PWR internal will get to 50 or 60.

MR. MITCHELL: Yes. Yes. Could very well, and that's what brings us into the void swelling questions on these stainlesses. That may come to more

of a forefront in the license renewal period for a lot of those reactors.

DR. LOIS: To give you a perspective in numbers, at least with the exposure of E greater than 1 MeV, the fluences for -- the peak fluence for the vessel in the PWRs is in the neighborhood of three to five 10^{19}. The BWR shrouds are in the neighborhood of five 10^{21}. Sometimes it will be higher than that.

And if you go to the -- farther into the core, then you have values in the 22nd, 23rd, in that neighborhood.

CHAIRMAN WALLIS: So they are quite different materials.

DR. LOIS: Entirely different phenomena, too, that they induce out of that. That's no question about it in this case.

CHAIRMAN WALLIS: Okay. Thank you.

DR. LOIS: Thank you.

MR. MITCHELL: Thank you.

DR. LOIS: Okay. Let me, then, pick up from there. The values, of course, that we are interested in for a pressure vessel obviously is not -- it's in one quarter of the thickness, three-quarters of the thickness, and back inside the boundary of the vessel.

Now, there is no direct way of measuring fluence. It's very unfortunate, but that's the way it is. There is, of course, the classical technique of hydrogen knockout, but that is a technique which is not suitable for the environment of a plant.

Why? Because the result of it is from -- produces a very weak gamma ray field, which is a measure of the spectrum of neutrons, and that's -- of course, it's impossible to use in the environment of a plant. And, of course, it is a bulky sort of piece of machinery.

So, therefore, we resort to having dosimeters to measure that.

Now, the activation of a dosimeter, it's fairly straightforward to calculate, provided, of course, that one knows the spectrum at a given location, where this activation takes place. The other quality which is difficult to calculate, or to guess, is the -- how many neutrons leak out.

Now, we do have computer codes, which the -- those are the transport codes, discreet ordinates codes, which they can do that within certain accuracy, by splicing up the work space and, of course, separating out the energy levels.

So, therefore, the objective of our

calculations and measurements is to make sure that the ratio of calculated-to-measured values at the same spectrum, at the same location, are close to one.

Let's go back to the old FERRET. It is a spectral adjustment code. What does that mean? It means that if we were to take the activation equation -- namely, activation is equal to cross-section times the flux -- we know how to calculate, and we know how to measure at the same location.

Here is -- the activation, of course, is subsequent to calculation. We know that. And we have several dosimeters that we can get several values, and, therefore, come up with a value which includes the uncertainties.

On the other side, it's a cross-section, which again is well-known, and with some uncertainty -- with the uncertainties associated with it, and the flux which contains a group-wise value, which each one of them can be calculating.

So we do have a calculated spectrum on one side. Spectrum cannot be measured. Of course, one can utilize a whole series of dosimeters and say, "Well, I can do an analysis and variable -- a variant analysis of a number of parameters and calculate it." Unfortunately, that methodology, that technique, does

not give specific results. It gives areas -- gives a band for the spectrum, but does not give specific values.

So having said that, it means that the only methods available to us to compare and calculate the measured value is to have calculated spectrum and then a measured activity -- activation, and then try to match these two. And that's exactly what FERRET does.

FERRET is not unique in that sense. There is quite a number of other codes which do the same thing. As a matter of fact, there are some other ones which are extremely much more elaborate than what FERRET is.

The reason for which FERRET was chosen is because Westinghouse decided to use that. At the beginning, we were told that FERRET included a covariant matrix which made adjustments to the -- to the code, to the activities in such a way as to incorporate the data, the measurement data which we had from a number of measurements.

At that time, when we discovered that there were uncertainties, and there were difficulties with the measured-to-calculated values, we requested Westinghouse to submit FERRET for review. Well,

Westinghouse did not do that. And they proceed with a number of applications which we thought were questionable.

Using the dosimeter values, as I pointed out in my introduction, Westinghouse wanted to -- to benchmark against iron, which traditionally yielded the lowest value. As I said before, the C over M ratios, calculated-to-measured ratios, they were as high as 30 to 40 percent different.

The C over M values we expected to be -- were in the neighborhood of 5 and maybe 10 percent. And so we decided that there was -- something needed to be corrected. So the staff, again, requested that we have FERRET be submitted for review. And that not having taken place, we refused to use values that were benched to the measured values of specific dosimeters.

Eventually, in 2004, Westinghouse decided to give us a version of FERRET which was submitted for review. We looked at it, and the report sort of ignored completely the data we had up to that point which showed disagreement between the calculated values and the measured values at that point.

So I wrote them a letter saying that the staff was reluctant to initiate reviews of this report, which appears to be technically correct with

least squares method, but it is seriously flawed in its physics. And we requested that they supplement --

VICE CHAIRMAN SHACK: Other than that it was fine, right?

(Laughter.)

CHAIRMAN WALLIS: That looks like something the ACRS might have written.

(Laughter.)

MEMBER POWERS: We might want to note that down there someplace. We're bound to be able to use that phrase.

DR. LOIS: So Westinghouse took several months and came back in 2005 and gave us a supplement to that review. We took a look at that, and it seemed to have now a database which represented about 30 percent of the actual existing dosimeters and the capsules, which were really adequately analyzed.

I tried to see whether or not they knew what was the basis or what was the reason for the discrepancies which appeared previously, and there seemed to be no specific answer. In between, I might remind you that in 1996 we discovered that the scattering of iron, and nickel for that matter, that happened -- the first observation was done at Los Alamos. We picked up on that and we established a

project at Oak Ridge to investigate to what extent -- what were the extent of that discrepancy, and how it would affect us.

We found out that the effect of the discrepancy and the scattering of iron for PWRs in particular would be in the neighborhood of about 10 to 15 percent. Slowly, we pushed for that, and eventually we had NFB6, the transport cross-sections, corrected. And we published at that time a draft of Regulatory Guide 1.190, which required that the transport calculations be performed using cross-sections at the count for the scattering cross-section for iron.

That brought in some sort of more accurate -- somewhat more accurate values, and closer to agreement between calculations and measurements. But still, there were significant disagreements left.

Apparently, the way that we were -- the activations were calculated, they seemed to evolve because there were references and the codes themselves -- the transport codes were also evolving. And it seems that the aggregate of all of those changes in the initial form were leading into the -- into differences of 30 or 4 percent.

CHAIRMAN WALLIS: Now, you said earlier

that there was a problem with the location, knowing the location of the dosimeters.

DR. LOIS: Westinghouse never accepted that.

CHAIRMAN WALLIS: Well, how does -- well, if that is a problem, how does changing the code correct that problem?

DR. LOIS: I did not say that there was a problem. I said that there was a suspected --

CHAIRMAN WALLIS: It seemed to be the problem. Did anyone determine it was or was not the problem?

DR. LOIS: We never resolved that issue.

CHAIRMAN WALLIS: Well, it may still be a problem, then.

DR. LOIS: It may be within the acceptance that we have, because now we can calculate and measure values which seem to be within 5 or 10 percent.

CHAIRMAN WALLIS: Which seems to indicate that it's not as much of a problem as you thought it was?

DR. LOIS: I'm sorry?

CHAIRMAN WALLIS: Because you're now closer to predicting things --

DR. LOIS: Yes.

CHAIRMAN WALLIS: -- in line with the experiment, you the conclude that there was not so much uncertain due to dosimeter --

DR. LOIS: Yes. It may not be the real cause.

CHAIRMAN WALLIS: Okay.

DR. LOIS: This was a suspicion that we had, and we thought that these two calculations were performed to support that argument. But, again, the vendor never subscribed to that.

Okay. So eventually, they submitted a new database. The database seems to be quite extensive with this as far as statistical purposes. And we felt that this was adequate, and approved the issue of FERRET. And we have the promise that -- and we put the limitation on it that the -- its application is acceptable, provided that the conditions that apply for the database are applicable to specific applications they have.

In other words, if there is an -- it's a ratio, a C over M ratio, which is still 20 or 30 percent away. FERRET doesn't look --

CHAIRMAN WALLIS: I'm trying to figure this out. I mean, you have something there which measures neutron flux --

DR. LOIS: Yes.

CHAIRMAN WALLIS: -- activation. And that's your data. The theory you compare it with is, what? Is it the calculated neutron flux history at that location for the entire life of the plant?

DR. LOIS: Yes.

CHAIRMAN WALLIS: So you have to know all about their fueling and their fuel cycle and everything.

DR. LOIS: Absolutely.

CHAIRMAN WALLIS: Everything.

DR. LOIS: Absolutely.

CHAIRMAN WALLIS: All those power levels and --

DR. LOIS: Yes.

CHAIRMAN WALLIS: Everything.

DR. LOIS: We account for that. That's how they do it, by the way.

MEMBER DENNING: Well, let me ask you a question. That is that the -- the foils have different half-lives.

DR. LOIS: Yes.

MEMBER DENNING: As well as having different spectra dependencies, they have different half-lives, which means that the different foils

actually give you evidence of different histories. There's a shorter -- you know, I don't know how much effect that has, but I don't see where it is included in the analysis. Is it?

DR. LOIS: Yes, it is. Oh, absolutely. There is quite a lengthy formula that will give you the total activations you expect at the time you measure it, because you remove it now, and you're going to measure it six months later. So all of these intervals, starting from the beginning of the irradiation, when the plants starts up, and the power level, and the loading type -- because, remember, in the old days -- old days, I mean when we first started operating PWRs and the other plants, the mode of loading was out/in, or three circles so to speak roughly, and we get new fuel in the outside, and then push the outside fuel farther in, and then farther in, and then we remove the one in the center.

Now, in that mode of operation, the outside elements were operating at a much higher power, and, therefore, the leakage was much, much higher. Now, with time we realize -- I mean, realize -- the utilities realized and the vendors realized that neutrons cost money. So they don't want to operate that.

And as soon as they started differentiating the loading of the fuel assemblies, now they started doing -- spreading them around and locating on the outside assemblies which burn at least twice.

Now, the power production in the outer assemblies, which contained, by the way, 80, 90 percent of the fuel that they leak, they only -- something like about .4 or less than that, .4, .45 of the average power assembly. So, therefore, the majority of the power is produced inside and the neutrons stay inside. They don't leak. And, therefore, we get longer half -- longer lifetimes of the core.

CHAIRMAN WALLIS: And the vessel.

DR. LOIS: And the vessel, yes, that's right. Two of --

CHAIRMAN WALLIS: What are the half-lives of these foils? Are they long compared with the time --

DR. LOIS: Yes. Well, we have a whole version of those. Nickel, for example, it's about 70 days. Iron is --

CHAIRMAN WALLIS: 70 days?

DR. LOIS: Yes. Well, it will give us an

indication what happened the last -- in the last cycle.

CHAIRMAN WALLIS: It won't tell you anything about years --

DR. LOIS: It won't tell you anything beyond that. However, iron is 30 years, so, therefore, that one gives you an indication that's pretty good, because that accumulates a pretty good estimate of that. So, and the calculation accounts for each one of the cycles, for the period that the plant was shut down, for the portion of the power level the plant operated, and so forth and so on.

And so these things can be taken into account in detail, and --

CHAIRMAN WALLIS: That's assuming that the right fuel bundle was put in the right place, and they knew where it was, and --

DR. LOIS: They did, yes. All those things have to be in the right place. But eventually we have managed to get the right answer, and we are now in a much better position than we used to be about 10, 15 years ago.

So going back to my original thesis, that is the reason why the discrepancy -- the arguments we used to have for a long, long time with the licensees

and vendors, why that --

CHAIRMAN WALLIS: Now, did you examine first to make sure that the physics was right throughout?

DR. LOIS: The physics? I'm sorry?

CHAIRMAN WALLIS: To make sure that the equations and the treatment and the neutron transport and everything is correct to --

DR. LOIS: Yes. We have Regulatory Guide 1.190, which requires -- which describes what the requirements of the methodologies for the calculation is. And also, it describes the requirements for the measurement of the dosimeters in general.

MEMBER DENNING: But I think that Graham did -- that FERRET doesn't do --

DR. LOIS: This doesn't do that.

CHAIRMAN WALLIS: Just the least squares stuff?

DR. LOIS: It's just the least squares.

CHAIRMAN WALLIS: That's all it does?

MEMBER DENNING: Yes.

CHAIRMAN WALLIS: Oh, okay. So it's rather trivial.

DR. LOIS: Yes, it is.

CHAIRMAN WALLIS: Compared with the --

DR. LOIS: That or those go to what the transport was. And they are extremely elaborate, they take a long time, but there is a way to make them give you a right answer.

That's all I have to say. Thank you very much.

Any other questions?

MEMBER DENNING: Yes. Of course we have questions. One of the things that just amazes me is -- is how accurate these results appear to be, particularly in those most recent comparisons that have been done against experiments. And they're getting adjusted to calculated values that are extremely close to unity, and I don't know whether that's -- whereas the accuracy of the transport calculation itself is probably 10 or 15 percent or something like that. Is there something that I'm missing in -- when we look at an adjusted-to-calculated ratio, another thing I didn't understand is apparently they are putting in not just the spectra but the absolute value of the flux.

DR. LOIS: Yes.

MEMBER DENNING: Is there some in-breeding here where we're making -- we're driving the adjusted value heavily by the analysis, so that that's why the

adjusted-to-calculated come out close to one?

DR. LOIS: No. Let me -- let me repeat that. The only true comparison between measurement and experiment -- I'm sorry, calculation and measurement is if you have the same spectrum at the same location, and that's what FERRET does.

It's, namely, calculate a spectrum at the location of the sample of the dosimeter. And then, calculates the -- and then measures the activation and compares these two. And that is a true measurement of the performance of the transport code, and those -- I mean, the transport calculation has -- the methodology has an accuracy in the neighborhood of four to five percent, because it accounts for the accuracies of the cross-sections, of the diameter of the vessel, the density of the water, of the consistency of the -- the geometry of the core, etcetera, etcetera. And, of course, the inaccuracy of the source, which is considerable.

MEMBER DENNING: And you think that that is -- that you can predict that with a four to five percent accuracy?

DR. LOIS: Yes. It did. And may I remind you how good the transport codes are nowadays, or the neutronic codes are nowadays. We can calculate the

lifetime of the core, which is about 400, 450, 460 days, within a day or two.

CHAIRMAN WALLIS: I want to be sure the record is clear. We're talking about four to five percent. We're not talking about 45.

DR. LOIS: No.

MEMBER DENNING: No.

CHAIRMAN WALLIS: It's four to five.

MEMBER DENNING: It's four to five percent.

DR. LOIS: Now, that comes along because there is the benchmarking is benchmarking to actual problems. We have sponsored two of those. I mean, the staff has sponsored two of those -- the PCA and the PSF -- which are samplings. But they are very accurately measured and accurately known. The sources were known almost to a few percent. And then, we can -- we can reproduce them.

MEMBER DENNING: How do you account for the capsule itself?

DR. LOIS: Yes.

MEMBER DENNING: I mean, in the analysis.

DR. LOIS: Yes.

MEMBER DENNING: How is that taken into account in -- I mean, here you have a merger of an R-

theta, an R-Z, and --

DR. LOIS: Yes.

MEMBER DENNING: -- how do you do the detail of the capsule wall itself and the capsule geometry?

DR. LOIS: Very small steps. Both in the radial and axial direction, very small steps.

MEMBER DENNING: So the R-theta calculation --

DR. LOIS: When you get to the capsule, you get more and more angles, and then you get more radials. So that you -- you have a very fine calculation.

There is a code by the way, RAMA, which I approved about a year ago or so, a year and a half ago, which is a combination between a Monte Carlo and discreet ordinates. And the innovation of RAMA is that it shifts neutrons in the directions of the routes for the -- for those integrations.

So it essentially does that, and you can calculate a true three-dimensional core and the surroundings extremely fast, extremely accurately.

CHAIRMAN WALLIS: But the capsule has water in it?

DR. LOIS: The capsule, yes, has water.

CHAIRMAN WALLIS: It's filled with water.

DR. LOIS: Yes, sir. But we account for it in -- yes, to a great -- minor detail so to speak.

MEMBER DENNING: Okay. Do we have any other questions?

CHAIRMAN WALLIS: It's very nice to hear something which is predictable within three or four percent.

MEMBER APOSTOLAKIS: Unlike some other things we heard this morning.

(Laughter.)

MEMBER DENNING: Notice the importance of measurements here, you may --

(Laughter.)

Well, let me just summarize that I think that -- even if things are a little worse than what they are, even if things are a lot worse, it certainly appears that the methodology has evolved to the point where it's -- it is adequate for the job that we are trying to do here, and that FERRET is acceptable for that kind of analysis.

DR. LOIS: But, again, FERRET is not really the primary code here. FERRET is --

MEMBER DENNING: It's a methodology that's acceptable.

DR. LOIS: And we do have, as I said, Regulatory Guide 1.190, which we issued back in 2001, and, I mean, prescribes the acceptable steps for the construction of methodology.

CHAIRMAN WALLIS: And you've spared us from having to review all of the neutron transport theories.

(Laughter.)

MEMBER DENNING: Yes. Okay? If there are no comments from anybody, then I turn it back.

CHAIRMAN WALLIS: Thank you. Thank you very much.

We have gained a tremendous amount of time. I don't think we need the transcript any more. Thank you very much. So we will come off the record, and we can still work.

(Whereupon, at 1:42 p.m., the proceedings in the foregoing matter went off the record.)