ORIGINAL ACRST-3112

OFFICIAL TRANSCRIPT OF PROCEEDINGS

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Title: MEETING: MATERIALS AND METALLURGY

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Docket No.:

Work Order No.: ASB-300-1186

LOCATION: Rockville, MD

DATE: Thursday, March 16, 2000

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MARCH 16, 2000

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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MEETING: MATERIALS AND METALLURGY

Room 2B-3
Two White Flint North
11545 Rockville Pike
Rockville, Maryland
Thursday, March 16, 2000

The subcommittees met, pursuant to notice, at 8:35 a.m.

MEMBERS PRESENT:

WILLIAM J. SHACK, Chairman,
Materials and Metallurgy Subcommittee
GEORGE APOSTOLAKIS, Chairman,
Reliability and Probabilistic
Risk Assessment Subcommittee
THOMAS S. KRESS, ACRS Member
MARIO V. BONACA, ACRS Member
DANA A. POWERS, Chairman, ACRS

PARTICIPANTS:

SAM DURAISWAMY, ACRS Staff
NOEL F. DUDLEY, ACRS Staff
EDWIN HACKETT, NRS
SHAH MALIK, NRS
DEBORAH A. JACKSON, NRS
LEE ABRAMSON, NRS
MARK CUNNINGHAM, NRS
NATHAN SIU, NRS
MARK KIRK, NRS
DOUG KALINOUSKY, NRS
ROY WOODS, NRS
WILLIAM GALYEAN, Idaho National Engineering and Environmental Laboratory
ROBERT HARDIES, Baltimore Gas & Electric
TERRY DIXON, Oak Ridge National Laboratory

CONTENTS

P R O C E E D I N G S

[8:35 a.m.]

DR. SHACK: The meeting will now come to order. This is a joint meeting of the ACRS Subcommittee on Materials and Metallurgy and on Reliability and Probabilistic Risk Assessment.

I am Dr. William Shack, Chairman of the Materials and Metallurgy Subcommittee. Dr. George Apostolakis is Chairman of the Reliability and Probabilistic Risk Assessment Subcommittee.

The other ACRS members in attendance are Mario Bonaca, Thomas Kress, and Dana Powers.

The purpose of this meeting is for the subcommittees to review the status of activities related to the staff's pressurized thermal shock screening criterion reevaluation project. The subcommittees will gather information, analyze relevant issues and facts, formulate proposed positions and actions, as appropriate, for deliberation by the full committee.

Mr. Noel Dudley is the Cognizant ACRS Staff Engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on February 25, 2000.

A transcript of this meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

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

I don't think I have any comments here to start with and we will now proceed with the meeting and I will call upon Mr. Ed Hackett, acting Chief of the Materials Engineering Branch, Office of Nuclear Regulatory Research, to begin.

MR. HACKETT: Thank you, Dr. Shack. I'm pleased to be able to be back here to go over some progress. I think it was about a year ago that it was Mike Mayfield, Farouk Eltawila, and Mark Cunningham briefed the committee on the project. This information is already stated.

Where we started off, and I guess this is even more than a year ago now, was with at least the hope that recent technical developments indicated the potential for increasing the accuracy in these analyses, and these are just some of the categories; improved estimates for flaw density and distribution, embrittlement correlations, and statistical bases for fracture toughness for the first time.

We initiated the project about April last year.

It's fully participatory with the industry. The industry is represented here in the form of the MRP and NEI and EPRI. We have briefed the committee, as I mentioned. I think the first one was last February, but also last summer, and then, of course, today. We are also planning on a briefing, I believe it's in the fall will be the next one.

The project is organized in three key technical areas. I think the subcommittee, the Thermal Hydraulics Subcommittee already heard some of the results of progress in thermal hydraulics yesterday. Today we will be focusing on probabilistic fracture mechanics, after this introduction, and then in the afternoon, the information probabilistic risk assessment.

Just to put a few bullets down on the overall approach. One of the key points is that this overall approach is for a best estimate analysis for these individual technical inputs, with uncertainty addressed explicitly at each point in the evaluation, and this is a departure from what we've done historically, as you know. A lot of what's been done in the vessel area has been done in a bounding sense, particularly with regard to the fracture toughness evaluation and the fracture toughness curves.

The idea then also is to update the technical inputs, as I mentioned, in probabilistic fracture mechanics, thermal hydraulics and PRA, and redo the IPTS studies with this new information.

The IPTS studies, you might recall, were conducted on three plants. It was Calvert Cliffs, Oconee and H.B. Robinson. I'll come to a glitch in our progress in a little bit regarding H.B. Robinson, but the idea was to redo those. Those were done in the 1980s. I don't remember the exact completion dates, but largely in the 1980s. So the idea was to redo those, which was the basis for the original rule.

In parallel, an important part of this that we felt had to go on in parallel was a reassessment of the risk acceptance criteria, and that's what you'll hear about this afternoon. Of course, that was set, at that time. The basis for that is SECY 82-465, from 1982. That was set at the level of 5E-minus-6. Of course, the NRC has changed its outlook on that area significantly since that time.

DR. APOSTOLAKIS: Would you explain the first bullet? I don't understand what the best estimate analysis with uncertainty addressed at each point means.

MR. HACKETT: Yes. This is the way, I guess you would argue, it should have been done all along. A good example where it's not done right now is the fracture toughness analysis. When the analysis for PTS is done, either to set the screening criteria or to evaluate an individual plant against the criteria, they're using lower bound curves from ASME, with no uncertainty. It's not a best estimate case.

DR. APOSTOLAKIS: I guess you are using best estimate and uncertainty in the same sentence, and that's what confuses me.

MR. HACKETT: Okay.

DR. APOSTOLAKIS: Best estimate usually does not go with uncertainty analysis, does it?

MR. HACKETT: In this case, the entire PTS analysis is designed to be a best estimate analysis. But in the past, criticism, I think valid criticism, we've gotten from the industry is that we've taken -- it's a nested chain of correlations and so on that get you to the screening criteria or are assessed against the screening criteria, and in each one of those, we typically, in the past, have made bounding assumptions.

Now we're trying real hard to make best estimate assumptions and --

DR. APOSTOLAKIS: So you mean to use the best models.

MR. HACKETT: To use the best estimate model, right.

DR. APOSTOLAKIS: Then you put an uncertainty on it.

MR. HACKETT: Right, and then build uncertainty in. I just flagged this up because that is very different from what we've done in the past.

DR. APOSTOLAKIS: Okay.

DR. KRESS: Well, your re-look at the risk acceptance criteria you think incorporate the uncertainty in some way then.

MR. HACKETT: Yes, absolutely, and I don't know who is going to address that this afternoon.

MR. MALIK: Mark Cunningham.

MR. HACKETT: Mark will address that. Okay. I think that's at 1:00. So that will be the case then. I just thought I'd summarize status real quick. We have made some significant progress, kind of in fits and starts, I think. There's been a lot of meetings between us and the industry and there's been a lot of progress, there's also been a lot of discussion, a lot of arguments, but I think it's moving.

Sometimes it's one step forward, two steps back, but it is moving forward.

In particular, in probabilistic fracture mechanics area, we have an expert elicitation which is hopefully going to give us a generic flaw distribution that's really based on cutting up old vessel welds and looking at those carefully and also statistically.

We're hoping to have that largely in hand by about May of this year. That's underway right now.

We do have revised embrittlement correlations, thanks to the work of Ernie Eason at Modeling and Computing Services, and also Bob Odette at the University of California-Santa Barbara.

They have a basis now, a database that supports these correlations. It's about five times larger than the one that went into Reg Guide 1.99 Rev. 2, which is what is used right now.

We are looking at statistical bases for fracture toughness. The Oak Ridge Laboratory, Mark Kirk, I think Professor Natishan and others in the room here have been involved in doing that for the first time on a statistical basis.

Then another important feature is plant-specific flux maps are being developed for the plants that we will be evaluating. I didn't mention it earlier, but Palisades is obviously very interested in participating in this project and has been very cooperative, and so we are also looking at evaluating the Palisades plant.

The wrinkle that I mentioned earlier, you can see the dates here when these are supposed to be completed, the Beaver Valley plant is the furthest out because about a month or two ago, the Beaver Valley plant wasn't part of this evaluation. We were originally going to have Robinson. Robinson had some concerns about participating in the project and basically opted out of the project.

We are very lucky, through the work of the MRP particularly, that the Beaver Valley plant volunteered to become part of the project.

Without that, we would have been without a Westinghouse plant, which I think would have been a very weak point for this whole project.

DR. POWERS: Can I come back to your expert elicitation for the flaw distribution? When you described that, you said that you had lots of information from cut-up welds.

MR. HACKETT: Right.

DR. POWERS: How about the free sheet?

MR. HACKETT: Excuse me?

DR. POWERS: How about the free sheet? The unwelded portion.

MR. HACKETT: The unwelded portion, yes. It does also include that. It has not been focused on that, but typically in the cut-ups that are done, we will take at least several inches to a foot on the sides of the welds. So there is information in the ultrasound exams.

DR. POWERS: That only means something if I know what it is relative to the heat-affected zone.

MR. HACKETT: Right. Typically, we mention welds, but a lot of these defects are focused on the heat-affected zone, and also not just the heat-affected zone adjacent to the structural weld itself, but the heat-affected zone that results on the weld metal or in the base metal from the cladding application.

So those are all captured. The plate actually is captured obviously to a lesser degree than the HAZ or the weld, but obviously the rationale for that is you expect a greater defect rate in the weld or the heat-affected zone. But we are capturing plate information, too.

DR. POWERS: Is the distribution strictly size or is it orientation, location?

MR. HACKETT: It's everything. I guess maybe one of the biggest drivers, of course, is the density, how many of them are there, but then, of course, a differentiation is being made now for the first time on whether they're volumetric or planer. When they're volumetric, like, say, for instance, it's spherical, turns out when you run the fracture mechanics analyses that they don't matter, they really don't count.

Also what we're finding is that an awful lot of the defects are small, two millimeters, three millimeters. When you run those through the probabilistic fracture mechanics code, what you find is they don't participate in any kind of failure projection, either. It's only when they're larger. Basically, they've got to be larger, at least four millimeters, and planer, to really contribute to the failure frequency.

DR. POWERS: What happens when I have a cluster of defects such that they act as -- how close do they have to be to act as a single large defect?

MR. HACKETT: Good question. ASME has what they call proximity rules to address that, both for surface breaking and subsurface, and those rules are incorporated into this assessment.

DR. POWERS: Is that a hidden conservatism that you're putting in here?

MR. HACKETT: It would be, because in a lot of cases, as you can imagine, that's --

DR. POWERS: I think you'd really want to flag that. I don't know that you've got any alternative on what to do, but I think you want to make it clear where your conservatisms are and not say that I've universally expunged conservatisms in here. I don't think you can.

MR. HACKETT: Right. That's a good point. You can't. You can't ever do that one completely. We do the best that we can with that, but that will always be there.

Thermal hydraulics, some of you may have heard about yesterday, because I know there's overlap between the committees, but by about the April timeframe this year, we're looking at having a determination of the key transients to be analyzed. I think these will follow probably closely what was done before for 82-465, but we have been examining more than that.

There is also this time, hopefully, going to be some verification from testing at the Oregon State University, the APEX facility, which my understanding is that that will model very closely the behavior of the Palisades plant.

With PRA, and this will be this afternoon, Mark Cunningham's presentation, the idea is to take the criteria that was used previously and then look at consistency with the more recent NRC risk-informed guidance, particularly these areas here, the reg guide, core damage frequency and in LERF, also.

The way it's being done right now is Mark has drafted a Commission paper that presents options for policy decisions in this regard that will be presented for your consideration and also for the Commission's consideration, and Mark will be discussing that this afternoon.

DR. POWERS: It seems to me that there is a potential difficulty in acquiring some feedback from the fracture mechanics folks and the people doing these thermal hydraulics and PRA. Unfortunately, I didn't attend the thermal hydraulics meeting yesterday, so I don't know what they said, but I do know that there is a tendency in the PRA community to analyze accidents that are as if the operators went away and took a break.

MR. HACKETT: Right.

DR. POWERS: Something like that. There's no human involvement. And they're attempting to be bounding when they do that. But that leads to some peculiarities in the accident analysis that you get accidents that don't look like TMI.

MR. HACKETT: Right.

DR. POWERS: Okay. And it's not clear that the accidents that are bounding or somehow poles that are useful in PRA for risk analysis will, in fact, be suitable for looking at the fracture mechanics problem.

It seems to me that you guys would be very concerned about accidents in which operators inadvertently turned on water or something like that.

MR. HACKETT: Right. That historically hasn't been addressed previously. My understanding is it will be addressed this time around much more explicitly. That's a concern.

The linkage particularly between the three areas is critical in this project, as you noted. One of the things that's critical, for instance, just as an aside or it could be far more than aside, depending on how it pans out, is our assumption of the effects of any kind of thermal plume or thermal streaming in the thermal hydraulics sense.

We're assuming -- the assumption going in is that there is good mixing there, that we're not going to have to worry about more than realistically a 1D or 2D problem. If it's a 3D problem, for instance, our fracture mechanics code doesn't address that right now, so that kind of thing could be a showstopper.

We have evidence that seems to indicate that's not the case, but that's something we need to look at closely.

DR. KRESS: On that slide, before you take it off. If you would give me a little more detail about that last bullet. Risk-informed guidance, what is that?

MR. HACKETT: Well, basically, and Mark can probably talk about this much more articulately than I can, but the Regulatory Guide 1.174, as you know, has become kind of a motherhood document for the NRC on how to evaluate risk or evaluate issues like this on a risk-informed basis.

DR. KRESS: So the first sub-bullet really means this kind of stuff that's in Reg Guide 1.174.

MR. HACKETT: Right. Basically, first off, the consideration of risk, but also proper consideration of defense-in-depth and the other elements that go into the reg guide. A lot of those criteria, of course, are set nominally at the 1E-minus-6 level when you're looking at core damage frequency. The PTS criteria is set at

5E-minus-6.

Nathan may want to make a few remarks.

DR. KRESS: You're going to try to make those two consistent some way.

MR. SIU: This is Nathan Siu, Office of Research, PRA Branch. Again, Mark will talk about this more in the afternoon, but I think the point is that he's raising options that might be broader than just Reg Guide 1.174. There's a whole variety of guidance concerning how to use risk in decision-making.

So we're opening the question what's the appropriate guidance here and I think the first bullet just simply says we want to be consistent with past guidance to a reasonable extent.

DR. KRESS: Thank you.

MR. HACKETT: The major issues I thought I'd summarize here. Like I said, actually, things have been going fairly well. But we did become aware about a month or so ago that the H.B. Robinson plant was not going to be able to participate in the project. As I said, that was a major wrinkle, since that would have eliminated a Westinghouse plant.

Roy, did you have a comment?

MR. WOODS: Yes. I'm Roy Woods, Office of Research, PRA Branch. You've got the right bottom line.

H.B. Robinson will not be the plant that we're using for the Westinghouse example. However, they didn't exactly decline to participate. They were participating and they were giving us information about thermal hydraulic characteristics and we had been talking to them and they ran into a problem with the status of updating their PRA model.

They were a few months away from putting out a revised PRA model and they were afraid it would cause them problems if they released the old model to us and it was going to end up in our having to develop a model ourselves, which would be quite inefficient.

So we went to see if we could find someone else that would be able to participate in a more timely fashion with their PRA model and we made the change. But they were willing to participate to a fairly high degree and it just wasn't quite enough to do what we needed to do.

DR. KRESS: That might even be an advantage to have a newer plant rather than the same three.

MR. WOODS: I agree. Right.

MR. HACKETT: Another particularly interesting aspect of this shift is, of course, Robinson is projected right now by the NRC or themselves to have relatively no problem on pressurized thermal shock, even for their license renewal term, whereas Beaver Valley is projected to be right about at the criteria at the end of their current license.

So there's a higher level of interest there on the part of the plant.

The other thing is Dr. Powers mentioned the plate defect distribution. Beaver Valley is a plate-limited plant. So that will put another interesting spin on it from the materials perspective.

I guess in summary, of course, we're here to do these presentations as an informational briefing for the committee. We are obviously very interested in any feedback, particularly if you think we may be heading off in the wrong direction somewhere. But probably it would be good to have some kind of feedback in writing from the committee on a periodic basis and maybe after this would be an appropriate time after these several days of briefings.

With that, I guess I'll sit down and have Dr. Malik come up and start to go through the probabilistic fracture mechanics, unless there are any other questions.

Thank you.

MR. MALIK: I am Shah Malik. This will be the presentation on progress made in probabilistic fracture mechanics as it relates to PTS reevaluation project. I will be helped by Mark Kirk and Doug Kalinousky in several of the subject matters that I present here.

We will be going through the status of the PFM, probabilistic fracture mechanics, activities, and also we'll look where it fits into the PTS reevaluation project as a whole, and then we'll go step by step in progress made in major PFM technical areas and some concluding remarks after that.

In the PFM area, we have this being a fully participatory type of project. We are having open public meetings involving staff, contractors, industry representatives, as well as public, and we had several meetings here in '99 and at least one in 2000 year, as well as we are going to have some more recent.

In these meetings, we decide about what are the order of issues and what could be a near-term and long-term action plan that we need to work on, and depending upon that, we are assigning some tasks.

In addition, we coordinate with PRA as well as the thermal hydraulics group, so that we can have proper interface from their output or input together.

DR. SHACK: What does this fully participatory mean?

MR. MALIK: It means from the very beginning, the industry and public are very much involved in the process. We laid out all the items that we are doing, what our thinkings are, and they come up and provide their feedback; well, this is not the way it should be done, it should be done this way.

So we kind of have a mutual understanding of each other's viewpoint, rather than doing it in the end when everything is done, and then it's not easy to interface and bring new ideas into the picture.

DR. APOSTOLAKIS: But this confuses me a little bit. What are the issues that have to be discussed with the public? Is this a technical issue?

MR. MALIK: Yes, they are technical issues.

DR. APOSTOLAKIS: Like what?

MR. MALIK: Technical issues, how do we implement fracture toughness, how do we implement multiple flaws, how do we implement embrittlement correlation, what are the different -- because those things are still continuing to be developed, and at what stage we put it in, because you always find some more time to do some more work and bring that in.

DR. APOSTOLAKIS: So you settled on one model or one approach for each one of these.

MR. MALIK: We are trying to settle on those, yes.

DR. APOSTOLAKIS: And there are no disagreements, no dissenting views?

MR. MALIK: There will always be some dissenting views, because you can always find a better mousetrap. So we keep on working on that.

DR. APOSTOLAKIS: But the question is why didn't you do it like NUREG-1150, handling the severe accidents? I mean, if there is a number of approaches, then you try to accommodate all of them and you simply assign weights to them by eliciting expert judgment.

MR. MALIK: Approaches are still like ideas that are being thought out and made. So they are not mature technologies. Those are ideas that --

DR. APOSTOLAKIS: That's where you need this kind of approach.

MR. HACKETT: Maybe I'll try. This is Ed Hackett, again. That's a real good point. The major place that's being done right now -- well, actually, it is -- that sort of integration is being done all throughout the project, but the one where it's most striking is this issue with the flaw distribution. That's a very -- has been historically a fairly contentious aspect of this evaluation.

Also, it happens to be a very large driver to what comes out in failure frequencies.

So what we decided to do, I guess it was about eight or nine months ago now, was exactly to take that type of suggestion and we're doing an expert elicitation process there. So not only do we have the data, but then we're talking, we're eliciting the expert opinion of various experts throughout the country, also internationally, to get opinions on flaw distribution, fabrication techniques, welding, metallurgy, distributions like that.

So that type of thing is going on continuously in the project. That's just the case where it's most explicitly being done.

DR. KRESS: Along the same line of George's question, I would be interested in whether such public meetings with all the stakeholder participation have been useful or not. Have you changed your mind about anything you were going to do as a result of these meetings?

MR. MALIK: Well, it brings some fresh ideas to look into and to improve our technical basis. So it has helped us in ways to have all the things ready before we are ready to present all those things. Yes, it has helped us.

DR. KRESS: You think it's been worthwhile.

MR. MALIK: Yes, it has been worthwhile.

DR. APOSTOLAKIS: Six of them, all six of them have been worthwhile.

MR. MALIK: Well, there are times we had some heated discussions in those, as well, yes.

MR. HACKETT: I guess I could make another comment there. This is Ed Hackett, again. Also, the industry has also brought a significant amount of resources to bear on this project which are well over and above what the NRC was able to do. So I think that's been a significant help in the project. Bob wanted to make a few remarks.

MR. HARDIES: This is Bob Hardies, from Baltimore Gas & Electric Company, and I'm chairman of a reactor vessel integrity group with the MRP, an EPRI group, an we're participating in this task.

Our participation includes sort of coordinating the efforts of all the utilities who are providing input to this effort. So a significant portion of those six public meetings is coordinating our contributions of our PRA, our thermal hydraulics models and the data on the materials in the plants.

In addition to that, we have technical input and technical opinions, and you asked for an example of an area of disagreement and one was warm pre-stressing. The way the models were performed in the past, when you had an unisolable leak, they're still treated as if that leak is isolated, and we make the argument that if it's unisolable, then it should be treated as if it's isolable.

The way we work that out is that the modeling gets done with warm pre-stressing not credited, but we do, the industry does sensitivity studies using that model to figure out what the effect would be if it was incorporated. In that way, our needs are accommodated and NRC needs are accommodated.

DR. KRESS: On the last bullet there, FAVOR, is that a new and improved version of OCA-P?

MR. MALIK: Yes. It includes OCA-P plus VISA, which was an NRC code, also. So it combines the best effect of both.

DR. KRESS: Are we going to sometime see the details?

MR. MALIK: Yes. In this presentation, we're going to have details of that, as well.

DR. KRESS: Is there plans to have it be given a peer review of some sort?

MR. MALIK: In these meetings, we are doing some comparative analyses and comparison.

DR. KRESS: So sort of.

MR. MALIK: So it's an ongoing process and if the committee wants to hear more details, we can work on that one, too.

You mentioned that the PFM, probabilistic fracture mechanics, code developed by Oak Ridge has a release of October to the industry for their review and application and see what things they need to work on.

This is sort of an overall flowchart. As you can see, it starts out from the right and it flows toward the left side. Here we have differences in terms of uncertainty. All the red boxes here show where there are uncertainties in the model. For example, when starting with the probabilistic fracture mechanics, we are performing a stress analysis. So we have like a thermal mechanical properties uncertainties, clad differential, thermal coefficient of expansion enthalpy.

The Young's model is another quantity that goes into developing thermal mechanical, thermal stress as well as pressure stress.

In turn, depending on what are the thermal hydraulic transients that are being brought in, so there will be some uncertainty in them. And then you calculate the stresses with another set of uncertainties going in and along with that, we also include effect on weld, residual stress in the weld parts of the region. So there is uncertainty on that, as well.

So we feed all of those --

DR. POWERS: Let me ask. Before you feed all that, let me ask a question. Especially under thermal mechanical property uncertainties, you have a lot of thermal mechanical property values that show up in there. How do you treat the correlation among uncertainties; that is, if your density is high, your Young modulus is going to be high. So there has to be a correlation in the uncertainties there someplace.

MR. MALIK: Yes. In the first set of analyses, we will have something like what is called mean or best estimate type of values and then there will be a set of values selected for them to perform a set of calculations. This will be like an overall loop here and in that we'll have a set of best estimate values selected for that range of values, and those will go into --

DR. POWERS: I find that an interesting uncertainty analysis. I'm not sure how it works. You pick a mean value for everything, there's no correlation -- I mean, there's 100 percent correlation. The means correlate with the means then. Is that factually correct?

If I have a mean value of the density, do I have a mean value of thermal conductivity?

DR. APOSTOLAKIS: In other words, when you select, after you do the mean value calculation, a set of values, are these values correlated? If the alpha tends to be high, would the other parameters also be high or are they sampled independently?

MR. MALIK: They will be sampled independently, I would think so.

DR. APOSTOLAKIS: So the correlation is ignored.

MR. MALIK: At the moment, yes.

MR. HACKETT: That's how you're doing this.

DR. POWERS: Well, I don't think that's an advisable way to do things.

DR. APOSTOLAKIS: That is what?

DR. POWERS: I don't think that's the right way to do things. I think you have to take into account correlations exclusively.

DR. APOSTOLAKIS: If they are important, yes.

DR. POWERS: And what are the chances that the material properties aren't going to exhibit an enormous amount of correlation?

Similarly, what are the chances that the uncertainty and the weld residual stress is then correlated with the properties?

MR. KIRK: Mark Kirk, Office of Research. I have a question. Do you mean correlated for physical reasons or just they happen to trend with each other? Is there a causal relation for the correlation?

DR. POWERS: Yes. I would assume that there is some underlying causal relation. I mean, I don't know what they are.

MR. KIRK: I'm on the materials side, so I can't speak directly to anything thermal hydraulic, but the intent in this process is if there are causal physical relationships between the variables, if there are uncertainties in any of the relationships that are shown by the connection points, that that's all fully captured.

The degree to which it's captured really depends upon our process, depends upon how well we elicit the -- and I shouldn't say that, because that has a specific meaning.

It depends on how well the technical area experts in the areas of materials and thermal hydraulics express their best understanding of the physical bases for these relationships that you're talking about.

To the extent that that knowledge is captured, this model will capture it.

DR. POWERS: Did you ask them specifically about correlations?

MR. KIRK: Certainly.

DR. APOSTOLAKIS: I don't think -- is the PRA group going to see this? The PRA group will see the end, right? The conditional probability of RPD failure.

MR. KIRK: This is the format, and we'll get into this type of input a little bit more in -- a little bit later when we talk about materials, but this is really going to be the form of the input, the way that the understanding of the physical relationships between all the input parameters and the input models, this is the type of information that the technical area experts at least in materials and I assume in thermal hydraulics are feeding to the PRA group. So it is going to get captured.

DR. SHACK: Well, some of these sources of uncertainty, when I look at the flaw distribution uncertainty, any uncertainty I have in Young's modulus is going to be somewhere after the --

MR. KIRK: It will be swamped.

DR. SHACK: -- the 14th decimal place.

MR. KIRK: Yes.

DR. SHACK: And certainly the thermal mechanical properties that I can think about, the yield stress will probably have the widest distribution, the toughness is -- things like density and thermal conductivity.

DR. KRESS: And you're only looking for correlations of the uncertainties, not the correlations between the properties. That will automatically get taken care of. The correlations of the uncertainties. If you have a high uncertainty in one, do you have a high uncertainty in the other?

DR. POWERS: Well, I think you also want to look for correlations in the values, but that tends to be a lot easier thing to do.

DR. KRESS: Normally you factor that into it automatically. But I don't know how you go about getting correlations between the uncertainties, unless you have just a lot of data that tells you.

DR. POWERS: That's the only way you can, is to find out that they're correlated or have a physical model for how they're correlated.

DR. APOSTOLAKIS: He is right, though. The flaw distribution.

DR. POWERS: Then just leave out all this stuff. Just put nominal values in and just leave all this stuff out. If you're going to make that judgment, what I do is nonsense on the first part of it, don't make a big deal about it.

DR. KRESS: Unless it's easy to do and doesn't cost much time.

DR. POWERS: Well, the thing that Joe is worried about is what you know intuitively sometimes turns out to be wrong. I know not in Shack's case, ever, but in my case, what I know intuitively often turns out to be flat wrong when I do these integrated analyses like this. That's why you like to do these integrated analyses.

DR. SHACK: Coming back to uncertainties, one of the things I do notice is that we're always dealing strictly with fabrication flaws and there's never any allowance for growth. Have we done enough analyses to convince ourselves that there is no significant growth of these flaws?

MR. MALIK: For the PWR environment, I don't think there is any growth going on.

DR. SHACK: So all that work you did all those years on cyclical flaw growth of BWRs wasn't necessary.

MR. MALIK: Well, in this particular case, flaws are the most significant contributor for PTS type of analyses, yes.

We combine this with crack flaw size to come up with crack driving force in terms of the stress as far as crack length and crack depth. Then we combine it, compare it with fracture toughness, again. We'll have material resistance uncertainty, such as fracture toughness, as well as fluence, which go into defining the fracture toughness at a given point in the reactor vessel's life.

Once we compare it with crack value and fracture toughness, we also take into account how many flaws are present. To perform this analysis, there are a number of flaws that are present in the vessel. With that, we find the conditional probability of failure for a particular thermal hydraulic transient and when we combine this with initiating event frequency to come up with an overall probability of reactor vessel failure per reactor year, that is vessel failure frequency.

And to perform this analysis, we are selecting several plants, as you can see, four plants, Oconee-1, Calvert Cliffs, Oconee is a B&W plant, Calvert Cliffs and Palisades are CE plants, and Beaver Valley, three-loop, Westinghouse plant.

In addition, we are also redoing generic SECY 82-465 analyses which were done in the early '80s and they were a part of the PTS screening criteria, as well as PTS rule development.

So we will be redoing those along with these plant-specific analyses to come up with information related to reengineering of that PTS screening criteria. There will be some sort of curve coming out, early vessel failure frequency as a function of RT, NDT or some other factor into the vessel, and together with that we can decide whether the screening criteria needs to be adjusted accordingly.

DR. APOSTOLAKIS: What is this criteria again?

MR. MALIK: Screening criteria presently for axial weld and plate material, RT and limiting RTNDT should not be more than 270 degrees three years before the plant is -- actually, they have to estimate and say three years beforehand, when they are going to reach their 270 degrees for axial welds and plate or for circumference welds of 300 degrees. So they have to know three years in advance of that.

DR. KRESS: You can see that corresponds to a given vessel failure frequency and so you can start with vessel failure frequency as your acceptance criteria and work down to the screening.

My question is now that you've got this nice band around the uncertainty band, how will you factor uncertainty into this criteria? I presume we're going to hear that this afternoon.

MR. MALIK: Yes, there will be a whole set of information provided.

DR. KRESS: I just wanted to alert them that that's going to be a question we'd like to address.

DR. APOSTOLAKIS: Is there a distinction between the square and the triangular? Do they mean different things?

MR. MALIK: It's like a choice. It's K for material resistance is greater than the crack value, yes or no, and here is a selection, how many times you select.

DR. APOSTOLAKIS: But the triangle there, what does it mean? The triangle with the circle in the middle.

MR. MALIK: Yes.

DR. APOSTOLAKIS: Yes. Is that different from the square to the left? Does it mean anything different?

MR. MALIK: The only thing here is you're making a selection to say yes or no for answer coming out, where here you're selecting, picking up a value.

There are six different technical areas. The first one or the most important one which we're working on is fabrication flaw distribution in RT beltline materials. That includes welds and plates and forgings.

The next item is regress statistical representation of fracture toughness, crack initiation, as well as crack arrest, K-1-c and K-1-a. Along with that will be improved irradiation embrittlement correlation to predict the shift in RTNDT and improve the stress distribution for material chemistry like nickel and copper, as well as initial RTNDT. RTNDT-0. So item four feeds into item three and item three in turn feeds into item two. That's how it's built up.

And coupled with that is a detailed map of beltline neutron fluence for the four plants and application of all those into the PFM computer code, as it's being revised to accommodate all these developments.

DR. SHACK: Shah, just on that, three and four, is one of the products that's going to come out of here a revision of Reg Guide 1.99?

MR. MALIK: Yes. Item three, it will be discussed in a few minutes, yes.

DR. SHACK: Okay. So there will be no inconsistency between --

MR. MALIK: No. We want to work in parallel, yes. I am providing a brief overview on fabrication flaw distribution. Debbie Jackson will be presenting a good presentation on our work for this, but I'm going to just point the discussion on that.

DR. APOSTOLAKIS: How do you know it's going to be good?

MR. MALIK: Pardon? The objective is to determine generalized flaw sizes, density, that is number of flaws per unit volume, the location of those flaws in welds, plates and forging in the RPV beltline region, and we are using non-destructive examination, as well as destructive examination techniques, and coupling it with expert judgment process, a form of expert judgment process, and the RES contacted staff, Deborah Jackson, and Pacific Northwest National Laboratory is performing the destructive and non-destructive one, as well as helping with the expert judgment process.

We have already performed destructive and non-destructive examination of weld in one reactor vessel, called pressure vessel research user facility. It was located at Oak Ridge. And we are continuing to inspect several of the vessels. There is parallel work going on between industry and NRC, so they do similar kind of NDE work and we do our NDE work and compare the results.

DR. KRESS: Whether you're looking for is the number of laws per unit volume.

MR. MALIK: Yes.

DR. KRESS: How do you get that by inspecting the vessel, just looking at that?

MR. MALIK: For example, if we have a piece of weld we have cut out, we have done examination to find what are the flaw indications. Once we have located those flaw indications, we section the small pieces out from the weld and cut it where they actually exist and what their size is. So there is verification using destructive examination as well.

DR. KRESS: How deep do you go in order to determine this volume?

MR. MALIK: The depth will depend on how deep the flaw indication is showing. There were flaws as big as 17 millimeters found. So they were destructively examined and found that they were in some kind of repair weld, as well as some kind of complex multiple flaws clustered together.

MR. HACKETT: This is Ed Hackett. I think I'll make comment there, too. I think Dr. Kress may be referring to how much of the volume is actually being examined and to what level of detail.

DR. KRESS: Yes.

MR. HACKETT: The answer is the entire wall, of course. If it's an eight-inch-thick wall, we're looking at all eight inches. Not necessarily with the same level of resolution on the entire way. The expectation, of course, is that you'd probably find most of your laws, like the previous conversation with Dr. Powers, in the heat-affected zone or near the cladding interface, and we're focusing very detailed examinations there.

DR. KRESS: So when you come up with the value for this number of flaws, is it distributed?

MR. HACKETT: It is distributed. Right. Exactly.

DR. KRESS: Thank you.

MR. MALIK: There will be some non-destructive examination of plate, as well, which is always from the weld region. So we will have, in the middle of the plate, there will be some flaw distribution coming out from there, as well.

And the data is being collected for that during the month of March and April for the plate material. We expect that generalized flaw distribution using the expert judgment process to be completed in the May to June timeframe.

The next cycle is by Mark on fracture toughness and he will also be going over embrittlement correlation.

MR. KIRK: Okay. Thank you. My name is Mark Kirk, from the NRC Office of Research, Materials and Engineering Branch, and I'm going to be -- today I'm going to be going through with you two separate technical topics.

The first one is the uncertainty analysis for fracture toughness and the second one is an update on our progress on developing some new embrittlement trend curves. So first, the first topic is fracture toughness.

The objective of this activity is to revise the toughness distribution curves based on expanded data and physical knowledge of the physics that underlies cleavage fracture that's been gained since the models were developed that we're currently using, and they were largely developed over 25 years ago.

Those distributions that we're using today are just simply based on data really from the early 1970s. There are about 170 crack initiation data points, about 50 crack arrest data points, and that's the basis of the curves that we use today, both in the ASME code, but more importantly, for this discussion, in FAVOR.

In SECY 82-465 and the IPTS studies, ad hoc statistical distributions were developed from these data in the ASME lower bound curves, and I'll be showing you some graphs of that in just a minute.

The RES staff involved in this activity include myself, Shah Malik and Nathan Siu from PRA, and the contractors involved in this activity include the Oak Ridge National Laboratory and the University of Maryland, and, again, I'll be filling you in on everybody's roles in just a moment.

That gives you sort of an overall flowchart of who is doing what and when in the fracture toughness evaluation. Where we started out was assembling all available LEFM valid K-1-c and K-1-a data. That was a task performed by us, by our contractors at the Oak Ridge National Laboratory.

They collected the data and significantly expanded our existing database. They performed a purely statistical assessment to get us some interim curves based on the best empirical data that's available today to use in some testing runs of FAVOR that are going on now and also we can look at those data to illustrate some likely overall changes in the current FAVOR model relative to the model that was used in IPTS and SECY 82-465.

I will fill you in on some of the details of that, but that activity is basically concluded at this time. Where we moved on to from there is to establish sources of uncertainty in a way that's fully consistent with existing PRA methodologies. Here we're involving contractors for the University of Maryland, and, again, I'll go into more details on this in a minute.

We're doing a root cause analysis of uncertainties, so we don't just have to look at the end data distribution, say, in fracture toughness or in RTNDT. We can pick apart the uncertainties so that the uncertainties are appropriately ascribed to different situations and not just treated in bulk, as they've been done in the past.

Underlying this root cause analysis is we're looking back at the physical basis, the physical causes for these uncertainties, so that we can properly distinguish between aliatory and epistemic uncertainty causes, and, as I mentioned, we're doing with this and we're working with

Nathan to ensure that the methodologies that we're using is consistent with the current PRA framework and we're also working with Nathan and his contractors to make sure that we're -- that the materials experts are describing their state of knowledge to the PRA folks in a way that basically everybody can understand.

So first, I'd like to just spend a few slides reviewing our data collection effort and then I'll go on to update you on where we are in the uncertainty analysis.

In data collection, Oak Ridge searched and collected additional data. Basically, we had a 50 percent increase in the crack initiation data and an over 100 percent increase in the crack arrest data relative to the statistical basis that was used in SECY 82-465 and IPTS studies.

They developed some Weibull distributions for us to use in the FAVOR code, just strictly based on the data fit. There is also a large K-1-c and K-1-a database that was developed in Japan in the late '80s and early '90s. It's been an ongoing activity here at the NRC, even predating the PTS reevaluation, to obtain that data. We hadn't succeeded on that. We still haven't succeeded on that, but now the Japanese workers who put together this database have released the data to the Pressure Vessel Research Council and we're in the process of hopefully crossing the T's and dotting the I's to get access to that data.

So that's an ongoing activity in data collection.

This just sort of shows you on one slide the culmination of the Oak Ridge effort. I'd like to focus your attention first on the left-hand side, for you. This is a plot of the initiation fracture toughness K-1-c versus the normalized temperature. So that's the temperature of the test relative to the reference no ductility temperatures defined in ASME.

As I said, this represents about a 50 percent increase in the statistical evidence that we had relative to our previous work, and the thing that I'd like to point out, and I'll point it out again over here, the black curves are the statistically derived uncertainty bounds that Oak Ridge fit to this particular data set.

The red curves are what was being used in the FAVOR model up until about six months ago. Similarly, over here, this is a plot of the crack arrest fracture toughness relative to temperature normalized to the no ductility reference temperature. Again, you see the red curves that were being used in FAVOR versus the black curves. It's the current best statistical representation of the data.

A message I'd like you to come away from this slide with is that the old FAVOR scatter bands were just too narrow to represent what was really going on.

We have performed some scoping studies using FAVOR to see what effect going from the red distributions to the black distributions has on the predicted probability of vessel failure. Perhaps not surprisingly, whether the new predictions are higher or lower than the old predictions is highly dependent upon the transient. We've done some runs where we get many more flaws initiating and going through the wall and then we have other transients where we have many less.

So the ballot is sort of still out as to the end effect on this and just points out that we can't allow ourselves to be moved around too much emotionally by changes in where we believe we were versus where we are now. We need to look at this in an integrated fashion.

On the next slide, slide number 12 in your packet, which I will skip, is just a mathematical representation of some of the curves that were on the previous slide for your reference, and that's all been detailed in reports that are now in NRC publication.

So just to orient you, I've gone through the data collection and the statistical assessment and I'm now going to move on to the root cause analysis.

There were questions raised earlier in the morning about a fully participatory process and whether that's had any practical benefits or not, and Bob Hardies certainly addressed some areas where the EPRI and the MRP has brought together input from the utilities.

I'd like to highlight here what I see as being a very key benefit in terms of the industry bringing in expert technical knowledge that wasn't available to the NRC and wouldn't be available unless we were in a fully participatory process.

This is the work on the uncertainty analysis of the K-1-c and K-1-a curves being conducted at the University of Maryland. It's being conducted at the University of Maryland by contractors that are working from separate funding sources. Professors Modarres and Mosleh have been working with Nathan Siu in the PRA area for some time. Through EPRI and the MRP, they brought in the expertise of Professor Marjorie Natishan, who is sitting in the back of the room, to help us out from the physical basis in identifying the root causes of uncertainties on the materials side.

These two researchers are collaborating in this effort, but basically the handoff here is in Professor Natishan's work and I will detail some of that, because that's basically where we are.

She's been identifying the reasons for the underlying uncertainties in the bulk data that you saw there and describing that in a systematic way that's then taken by the PRA folks and expressed mathematically to get us to our end result, which is a recommended program structure for FAVOR that treats the uncertainties in a way that's consistent with the underlying physical process.

Now, Shah had used one of the root cause diagrams and, again, this is -- the use of this type of diagramming format has come about as a direct consequence of EPRI's funding of Professor Natishan and I think it's brought us to a very good place in terms of being able to look at existing methodologies and express them a systematic fashion.

It was perhaps not a good idea to use this diagramming process without explaining it first, so I'm a little bit late on this, but we'll try it here.

The idea is that the diagram expresses both parameter uncertainties in the input parameters and really what can go into any of these yellow boxes is a distribution of values. For example, and I will show you some real examples in a minute, say this could be RTNDT and there would be some distribution of RTNDT values which you could look at.

Well, that arises due to uncertainties and a lot of different things, a lot of process things and a lot of parameter things. Back here you might have some of the chemical composition elements. So distributions of chemical composition, say, of copper and nickel could flow through the physical model and give rise to a distribution of RTNDT values, which then you'd ascribe some uncertainty to. So that's the basic idea.

So in the diagram format, parameters with distributions go in the boxes and at the nodes, those represent different relationships between the parameters. You can have equations that are correlations which, in fact, have their own uncertainties associated with them based on the data that they were drawn from and based on the underlying physical basis of the correlation.

You can have nodes that are choices, you pick one or the other, and you can have nodes that are comparisons, min's, max's, things like that.

Just some things to say about the process, and, like I said, this has been very helpful in both focusing our attention on what the models are that we really are using today, and also in involving a lot of different experts from different technical areas and getting all their input into one framework.

One very nice thing is it displays a complex process in a very logical format and it's the only thing I've personally seen that allows you to look at the big picture, while still also capturing the details.

You can look at these diagrams at any level and you don't have to hide anything if you don't want to. It's been very useful in going through this process with experts from within the NRC and also experts in the industry in building consensus, because it really provides a common language for discussion.

You will have people come in and say, well, copper is very important as a cause of embrittlement. Yes, indeed, it is very important, but where copper comes in is somewhere way down here and if you try to treat copper way up here, you're going to get stuck with gross empiricisms that are a cause of a lot of the over-conservatisms that are endemic to our current process.

So yes, everybody agrees that copper is very important and you'll have people pounding the table and saying that and you'll agree with them, but you're not going to treat it properly and you're not going to capture it properly in the mathematical model. It goes to the PRA people and then eventually gets reflected in FAVOR, unless you understand that copper is somewhere way past that wall and not up there.

So this has really allowed people to put this together and understand it as a group.

It also streamlines the critique, because you can lay it down in front of someone and have them see how it goes and I will warn you in advance, you may find some errors in the diagrams that I'm about to show you, because they are works in progress, and it seems like every time we put them up, somebody finds something that's perhaps not quite right. Hopefully we're converging on a solution.

One thing I do want to point out, and I think I have pointed it out already, is this treats both uncertainties in the input parameters, say copper and nickel, measurements of temperature, as well as uncertainties in the models which are represented by the nodes.

So I have included more diagrams than this in the packet and I would be happy to discuss them in detail, if people would like, but what I would like to do is just sort of show you one very high level diagram and then go into one -- in a little bit of detail to focus on some of the things that the process does. If you want to get into the details, that's fine. That wasn't my initial intent.

So at the highest level, we're looking for a distribution, and I shouldn't have used the word uncertainty here. You assess the uncertainty as a result of the distribution the model predicts, but we get a distribution of K-1-c values. That's related to the K-1-c data that was used in FAVOR and it's also related to the RTNDT in the irradiated condition, because you do your K-1-c test and then you plot it not versus temperature, but versus temperature normalized to RTNDT, irradiated, in this case, end of license.

RTNDT irradiated, based on our current modeling methodology, is a direct function of the unirradiated RTNDT. So the RTNDT measured before operation begins and the shift in the Charpy 30-foot-pound energy, which we take to be equal to the shift in RTNDT. So right there, even at this high level, you see an assumption. We can talk about whether it's a good assumption, bad, whether it's a big error or a small error relative to other things that are in the model, but that's going to get captured here because what you put in is data and physical understanding.

I have included all of -- more diagrams here and they then go on further. What I'd like to do is just show you the T30 shift diagram, because that will enable me to make a few points that I'd like to about really more the process than what's in particular on the diagram.

I will step through it just very briefly. The way we get the shift in 30-foot-pound transition temperature in this -- and this is a -- this is basically a diagram of what's in either staff position or 10 CFR 50.61/Reg Guide 1.99 Rev. 2.

First, you have to decide if you do or do not have credible surveillance. If you don't have credible surveillance or you have surveillance and it's -- or you don't have surveillance, you use the embrittlement trend curves. If you do have credible surveillance, you construct a best estimate of the T30 shift based on testing of your surveillance capsules. You then adjust that value for any potential differences in the chemistry between that little lump of material that you tested and your whole, say, beltline weld, if that's what is limiting.

You also adjust that best estimate of T30 based on your surveillance samples due to -- for any differences in irradiation temperature that may have occurred, for example, if your limiting material was irradiated in another vessel. Then that goes on and flows down, as I said. Cooper is obviously a key embrittling element, but you see it doesn't occur early on in the diagram and, in fact, this diagram then goes to another one where we get our T30 values.

In terms of the points that I would like to make, and these are reflected on slide 20, so I will just say them here and then we an skip slide 20.

A lot of times, when people look at these diagrams, and I've already pointed out that this isn't the end, this continues on, sometimes people get despondent because they say this is possibly complex, we could never -- we could never reach the milestones that it laid out if we go through this.

One thing I want to point out is that you can enter your parameter data at any point on this diagram. You don't have to go all the way to the far right to enter your data and, in fact, in most cases, we don't. We might come in here and say, okay, we have measured values of the 30-foot-pound transition temperature. We could go all the way back to the raw Charpy data and refit it and do all that, or we could enter here, or we might decide that we've already done that and we could enter with Charpy shift values.

That's going to be a decision that has to be made by the technical experts involved in the process in terms of what our quality of knowledge is at any particular level.

But I just wanted to point out that just because we're trying to get this basically all the way down to measurement error and material inhomogeneity, in most case, we won't be entering the diagrams at that point with parameter data.

I've pointed this out before. This appropriately incorporates all the uncertainty sources, both uncertainties in the parameters, any possible correlation between the parameters, and also uncertainties in -- and it's not maybe well reflected on this diagram, but any uncertainties in the relationships between the parameters. Each of these equations, it's not just simply the equation that the materials folks are going to pass to PRA, but it's our best understanding of is this an exact model, is this a correlation, are there other potential correlations, and that then will be treated in an appropriate way by the folks in PRA.

It's probably obvious, from what I've said right now, but the diagrams are much more than schematic. They, in fact, represent mathematical models and will be used as the basis for simulation studies to understand what the uncertainties are.

And there are a few things that this process does that our old way of doing things, which Ed pointed out was lower bounding, can't do and doesn't do, is that we find that when you diagram the process in this way, you find that uncertainties split at certain levels. For instance, every time you encounter a choice node, if, for any particular situation, the uncertainty in a 30-foot-pound transition shift is either going to be the uncertainty that's down here from using the trend curve or the uncertainty that's up here from using surveillance, it can't ever possibly be both, because you have to pick one or the other.

Whereas if you just came in and did a statistical assessment or a statistical analysis, I should say, of delta T30 values at the end of this, you'd be wrapping all those together and you wouldn't be appropriately treating it.

So by taking the process apart, we can make sure that uncertainties that are appropriately burdened onto appropriate situations and we also have the ability to eliminate double-counting of uncertainties, which is a very real potential, for instance, at this particular node, you feed an embrittlement trend curve without -- right now, you feed embrittlement trend curves without use of copper, nickel, end of license fluents and product form, whereas the equation at node four was, in fact, derived from some of those same data.

That needs to be treated appropriately and will be in this process.

So I'm going to -- I have included in your packet diagrams for RTNDT and irradiated in K-1-c. If it's acceptable to everyone, I'm just going to skip over those, because like I said, I sort of viewed my role here as trying to describe the process we were taking a bit more than going into the details.

What I would like to do now is to shift gears and move on to the irradiation embrittlement correlations. I have borrowed a diagram from the last presentation to indicate that everything that I'm about to talk about is ultimately going to impact this box on the uncertainty diagrams and everything to the right of it.

So the objective in this activity is to develop or perhaps I should say revise, refine, improve a model to predict the shift in -- and I want to be specific -- this is a shift in the 30-foot-pound Charpy transition temperature which we take to be equal to the shift in RTNDT in current regulations, due to irradiation embrittlement.

Why are we doing this now? Well, we've got a heck of a lot more data than we did the last time this was revised, which is over a decade ago. In that larger data set, we've got a much better coverage of the primary variables, the primary embrittlement variables of copper and nickel and so on. We've got much longer time exposures and consequently we've got exposures to higher fluences.

The only data that is being directly considered in this trend curve development is data from commercial reactor surveillance. We're using data from test reactors and the physical understanding from test reactors and theories to help guide our models and that's where the physical understanding comes in, but those data are not being directly used in the correlations.

We're using rigorous statistical methods to try to parse out the effects, which is a continuing challenge, and, as I said, we're trying to bring in -- this is not going to be a purely empirical model. It's a highly non-linear model. The variables are -- a lot of the variables are highly cross-correlated and in order to have any sensibility to this, we need to bring in a fairly sophisticated understanding of the underlying physical process of irradiation embrittlement, so we know forms to try to fit the data.

This activity provides guidance to -- actually, the activity started and stands as a separate milestone on all of our charts as Reg Guide 1.99 Rev. 3, for which we're on the hook to provide the technical basis for in December of this year and then Reg Guide 1.99 Rev. 3 will go out for public comment sometime in June or July of '01.

But we also needed to sort of crank up the activity to provide input to the PTS reevaluation project, and what we're trying to do is to get to Shah and his group a new embrittlement trend curve and a new assessment of the uncertainties which will be rolled back into the model that the University of Maryland is developing for us sometime in the April timeframe.

The RES staff that is working on this is myself, Carolyn Fairbanks, Shah Malik, and the NRC contractors that are involved include the Oak Ridge National Lab, Modeling and Computing Services out in Boulder, Colorado, and Professor Bob Odette of the University of California at Santa Barbara.

Just to give you a brief perspective on what's changed data-wise. This just shows you the size of the empirical data set that we're working for in terms of number of Charpy shift values. So each of these values represents at least two Charpy transition curves, one irradiated and one at some level of fluence.

When we developed Reg Guide 1.99 Rev. 2, sometimes known as the Randall-Guthrie-Odette correlation and Rev. 2 hit the books in '88, we had a bit shy of 200 shift values. In the mid '90s, the NRC let contracts with both Modeling and Computing Services, Ernie Eason and Joyce Wright, and also with Professor Bob Odette at UCSB, to do an updated assessment of the embrittlement trend curves. When they published a NUREG for us in 1998, we were up just a bit over 600 data points.

That model was subsequently critiqued, sort of in an informal sense within ASME and E-900 community. That led to some of the NSSS vendors coming to us with about 200 additional data points which have now been included in our assessment. So we're up just a little bit shy of 800 shift values.

I'm going to put an equation here and not explain it, which is the only safe thing for me to do. But I do want to highlight how the model has changed, other than just getting longer. The 1988 Reg Guide 1.99 Rev. 2 model is a multiplicative model for Charpy shift. We have all the chemistry factors in one term that's called the chemistry factor and then we have fluence in a completely separate term.

That reflected pretty much just a pure empirical fit to the data. In the new equation, and this is just -- I just put this up as an example. It's just one of the candidates that are currently being considered and we're hoping to finalize on the best end model sometime in the next two months, but what we see, some features to highlight, like I said, we've got physically motivated -- we've got physically motivated reasons for the forms and the functions that we've selected. We've got separate terms for the stable matrix defects in the A term and the copper rich precipitates in the B term, which is in good agreement with the underlying damage -- the underlying reasons for damage.

And it's not particularly apparent here, but there are copper saturation limits being included, reflecting the fact that beyond a certain point, copper is not soluble in the matrix and will not cause damage.

Some terms that are currently under consideration include terms accounting for phosphorous, and I should note that there was a phosphorous term in Reg Guide 1.99 Rev. 1 that was subsequently removed. We're looking at long time effects and irradiation temperature effects, largely as a result of the fact that we can now see this in the data, and I suppose it is open to some expert debate as to whether we can see it or not.

Some of the models tell us that we should expect to see it and as we collect more surveillance data, at long times, we're beginning to see some effects.

Also, a big change, not so much in the equations, but in the underlying philosophy, is it's quite likely --

DR. SHACK: These long time effects, this is what, growth of the copper precipitates to some point where they're no longer as effective?

MR. KIRK: Yes, or thermal -- a combined thermal irradiation effect, any number of things. I'll get into this just briefly. We spent a lot of time trying to develop what I'm going to call a gating criteria. There is no absolute truth here, of course. We've got some Heinz variety of empirical knowledge and physical knowledge and we've tried to come up with some criteria to help focus ourselves on, okay, what gets in and what has to wait for Rev. 4.

I'll get to that in just a minute. But one thing I want to point out that's very much more procedural and philosophical than the equation is there is definitely a feeling among the staff that we want to move to the use of surveillance data as a check of the correlation rather than as an index to the correlation.

The diagram that I showed you before for delta T30, if you remember, it had the choice branch, where you decided if you had credible surveillance data or not, where credibility was judged as to whether you had more than two points or not.

So right now, if you've got more than two points and they're reasonably close to the mean, you change the whole embrittlement trend curve by moving it up or down to those two data points. From discussions among the staff and indeed discussions that have gone on within the ASTM irradiation embrittlement community, there's, I would say, definitely a consensus developing that that's not really a very appropriate engineering procedure and what we should do is move towards use of surveillance data as a check, which is to say we still encourage the licensees to do surveillance.

It provides more data. It keeps us from going wrong. But we're not going to change unless the surveillance data is just way off the mean curve, perhaps more than three sigma out, and that still needs to be determined.

It doesn't seem appropriate to change the whole view of embrittlement of that particular material based on two data points, when you've got 800 sitting back here saying no, no, no, it's going some other way.

So like I said, that's a procedural and philosophical change --

DR. POWERS: Before you take that equation off.

MR. KIRK: Yes.

DR. POWERS: It is remarkable for its level of parameterization with 600 data points. It looks to me, however, that you don't have a saturation effect built into this equation for copper. You have a saturation effect built in for fluents crossed with copper.

MR. KIRK: Yes. This is the danger of putting up a particular equation. I honestly can't tell you if this is the one with the copper saturation or not. But the one that's being considered in the end is -- if I had to give you my best guess right now, nine chances out of ten, you're going to have the copper saturation term.

If you don't see it here, my error in putting up the equation.

DR. POWERS: I just look at it.

MR. KIRK: Yes.

DR. POWERS: There's no cap on the effect of copper.

MR. KIRK: Yes.

DR. POWERS: There's a cap on the cross with copper and the fluents term.

MR. KIRK: Yes.

DR. POWERS: And it seems that you get a cross also with nickel in a peculiar fashion, and it's remarkable in light of the phase diagram.

MR. KIRK: Like I said, the problem of putting up an equation for illustration purposes when you're not prepared to talk about it.

But this was just one of many and is not going to be the final one, because it's being revised as we speak. So hopefully depending --

DR. POWERS: there must be an enormous amount of structure to your data set.

MR. KIRK: Yes. We could go on forever.

DR. POWERS: I mean, usual metallurgical data like this has enough scatter that straight lines and things like that seem like appropriate.

MR. KIRK: That's, in fact, one -- those are some of the things that, of course, we've struggled with and that's one thing -- well, the process that we're going through right now is we're writing the tech basis document to support whatever equation one of us might show you at a future date boxed in yellow.

And this is part of the process that I'm showing here. Another part of the process that I think we need to -- well, let me back up.

It's very easy to put up a graph of some effect based on data and standing in a room and convince people that you know what's going on. I find it much more difficult to convince people that you know what's going on if you force the investigators involved, and I include myself in this, to put down the graph and basically write the paper.

So convince me, let's write the technical basis, and that's what we're -- that's the rigor that we're trying to put ourselves through in terms of getting any particular effect into this model. Let's convince ourselves that we have an appropriate combined physical and statistical basis for these effects, and this is our sort of provisional strategy for focusing our attention on this, is that we sort of divided a physical basis into a well accepted physical basis, perhaps a plausible one, and one that's just not established, that we don't know what's going on.

And then we can look at our statistical evidence and say we either have strong evidence for an effect, say, a correlation coefficient in excess of 95 percent, or a weak statistical effect, perhaps a correlation or a confidence in excess of 70 percent, but still with a coefficient you can calibrate.

And what we did is we just sort of boxed this up and said, okay, well, certainly if we had a well accepted physical basis and a strong statistical basis, that effect would be included in the model.

And if you had weak and not established, you'd never consider putting it up. Obviously, there is a huge gray zone in between. In our initial thoughts on this, we've placed perhaps a bit more stock in the statistical evidence than in the physical evidence and we felt that if we had something that was a very strong and demonstrable statistical effect within the power reactor database, even if we couldn't establish a physical basis for it, we felt that that was something that, from a regulatory perspective, probably would be included in the model, accepting that it would be going under a lot of scrutiny.

Conversely, if you had something with a weak statistical basis and perhaps only a plausible physical basis, that would be a little bit more dicey in terms of how it gets in.

Obviously, there are no -- it's hard to draw a line on this, but this is sort of a process we're trying to put ourselves through. And ultimately what we'll be doing is publishing a reg guide for public comment, publishing a tech basis where each and every term is gone through in this way.

The staff authors and the contractor authors will basically have to come to the table and say here is where we think it is and then it will open for -- the whole process will be open for public critique.

So what at least the goal of this committee is to get the debates squarely on a technical level and not on any other level. It's the only way to proceed.

The status right now is that we're finalizing the model or trying to. We've frozen the database. That's sort of a necessary procedural step, because there's always one more data point showing up and at some point, you've just got to draw the line.

We've at least proposed a gating criteria for term admission and right now, in order to try and get an embrittlement model to Shah and to Terry and for them to use, what we're doing is we're writing mini basis documents so that we can try to get an embrittlement correlation to them in the April timeframe that is hopefully no different and, if anything, not much different than that which is supported by the final tech basis document, which is due in December.

And I think I just said all this. We're trying to get this to Shah and his workers by April-May and then once we've got the sort of the mean curve established, then all of this knowledge feeds into the K-1-c and K-1-a uncertainty framework and analysis that's being done by the University of Maryland.

The deadlines for Reg Guide 1.99 Rev. 3, tech basis document in December of 2000, draft for public comment available middle of next year, and also just point out other activities in the public domain is that ASTM E-10 has ongoing technical interest in this area and they will be evaluating the model for potential use in the E-900 standard.

With this, that's my last slide. So if there are any questions now, I'd like to entertain them.

DR. POWERS: I guess I'd like know -- I'd like to understand better about the rigor with which you are approaching this problem. If we could look at your slide 29.

MR. KIRK: I'm sorry?

DR. POWERS: Is this one of your slides, 29?

MR. KIRK: No, sir.

DR. POWERS: Okay. Then I can't ask the question.

MR. KIRK: You'll have to get the next guy. Any questions on slides lower than 26? Less than or equal to. As my parting shot, I wanted to point out the next speaker will be Doug Kalinousky, also from Office of Research, Materials Engineering Branch. He is going to be talking about statistical analysis of chemistry and RTNDT data and, again, just to express it in the overall uncertainty analysis framework, where this goes into the diagrams, RTNDT unirradiated is up here, feeding into K-1-c uncertainty, whereas the copper and nickel values are way back here in the embrittlement correlation.

I'd just point out one other thing. This is a diagram of the current embrittlement correlation process in Reg Guide 1.99 Rev. 2. We put this up so we have something to talk about. Ultimately what goes into this analysis is going to be different than this because we're going to have a new process and a new correlation and new data.

DR. POWERS: Let me ask you. You've mentioned several times the word rigor in your statistical analysis. It's been my experience that rigor is a relative thing. Can you give me an idea, some understanding about the strictness of your rigor?

MR. KIRK: The short answer is no. I think that would have to be something that you would judge when you see the product.

The problem -- and I'm not a statistician, so I'm probably not going to provide you with an acceptable answer. As I understand it, the problem with non-linear analysis such as these is there is no one single right answer. If this was Y equals MX plus B, we could talk about rigor.

It isn't and therein lies the problem. So you've got a lot of engineering judgment going into what you then apply fairly routine statistical tests, like student T and analysis of variance type things, too.

So once -- really the points for discussion, at least I think, and this might be a better question when we can present you some more of those results and maybe that's something you'd like to ask for next time, I think the points of discussion are perhaps not going to be so much in terms of the statistical tests that are applied, because the statistical tests that are applied are, in fact, first year statistics.

It's going to be on the engineering judgment that we use to say, okay, this is an appropriate subset of the data to try to apply a student T test to. That's what we keep arguing about at least. So I think that's where it comes in and so that's going to be an argument of engineering judgment that's motivated by people's understanding of embrittlement damage mechanisms, in that case.

MR. HACKETT: This is Ed Hackett. Let me try a slightly different take on Dr. Powers' question. One of the areas where we have introduced, I believe, a high level of rigor to this process is in screening and selection of the data that went into the database, and there I can cite the benefit of working jointly and cooperatively with the industry on this.

For instance, some of the temperatures for the irradiations previously involved melt wires and other forms of selection. It was very rigorously scrubbed by the industry and the ASTM folks this time around to just use downcomer temperature.

So there was a lot of -- that's just one example, but there was a lot of rigor that went into selection and screening of the data that are in this database, and then I agree with what Mark said subsequent to that, but there's a fair bit more rigor in that process now than there was in the previous version of the reg guide.

DR. SHACK: If there are no more questions, it's probably time for a break. Come back in 15 minutes, 10:25.

[Recess.]

DR. SHACK: I'd like to come back into session, since I suspect we're going to be running hard-pressed on our schedule today.

MR. KALINOUSKY: I'm Doug Kalinousky. I'm with the Office of Research, Materials Engineering Branch. Our objective in this portion is to determine the chemistry variability and RTNDT-0, initial RTNDT variability distributions.

We used the NSE database for copper and nickel and initial RTNDT values. We are trying to determine heat-specific distributions, to determine the means of the distributions and the variability, the standard deviation of these.

We also are attempting to get the local variability in a small area of the weld or plate.

We did this within a little sub-region that's used in the FAVOR code and we are debating still whether the through-thickness as the crack grows or not, because as the crack grows, it might run into different coils that were used in manufacturing the weld. So we're still debating whether that be applied to the code or not.

I did this with myself, Tanny Santos, who is off in Canada skiing right now, and Lee Abramson was our statistician that we used as a consultant heavily.

DR. SHACK: What is a heat-specific distribution of copper? Is that really the same thing as local variability?

MR. KALINOUSKY: That would be the whole heat that we have data for.

DR. SHACK: A heat.

MR. KALINOUSKY: A heat number. We would try to find the mean from all the different data we have and the distribution about that mean. The local variability instead would be as if in the code, we have broken the welds down and like two to three inch sections and we say the variability in that section.

If we already assigned a mean to a point and we go to a different point, what would be the variability between those two points.

So we went through -- we used a couple of reports in this thing, one from the CE owners group and one from the B&W owner group, and we used all heats we could find with five or more data points, so we'd have a fair representation of the standard deviation of those mean values. We found 24 heats for copper and 39 for nickel.

We determined a mean value based on the five or more data points for each heat-specific means. Then we used that to also find the standard deviations.

We went ahead and plotted these out, you'll see the next two plots would be the -- next two slides, that is, would be a plot of the standard deviation and the mean. And for the copper, we'll go into this next point as we show you this slide.

DR. POWERS: Let me understand this last line. The previous speaker mentioned statistical rigor. You have uncertainty in the mean values and you have uncertainties in the standard deviation values. So you are going to use a linear regression technique that presumes precision in the independent variable. Why?

MR. KALINOUSKY: Because we have very little data to go by and there really is -- we did a -- this is what you were referring to, obviously. That was the plot where we had the large scatter. But we noticed that there is definitely a trend as the mean value increases, the standard deviation is increasing.

DR. POWERS: That's not an excuse for using linearly squared statistical techniques. They presume that there is no variability in the values for the independent variable. Why wouldn't use something like a min/max procedure?

MR. KALINOUSKY: We did what's called a K-ring squared test to test the --

DR. POWERS: You can test it till the cows come home. The fact is that you have assumed precision on the horizontal axis here.

MR. KALINOUSKY: Yes.

DR. POWERS: And there's not. And there's another technique for fitting the line, a min/max technique that takes into account that there is uncertainty both in the independent and the dependent variable. Why not?

MR. KALINOUSKY: Because we --

DR. KRESS: Does it matter, unless you're going to --

DR. POWERS: It's going to change the slope of the line substantially.

DR. KRESS: Yes, but that matters only if you're going to extrapolate outside this data, you think?

DR. POWERS: Even if you're going to interpolate it, it changes the slope of the line, it's significant. What happens? When you have a non-standard deviation, it tells you that you're plotting the wrong variable. It should be something like the square or the square root or something like that.

Okay. But you don't care. All you care about is the linear variable anyway and you'll live with a varying standard deviation, I assume. But now that slope of that line becomes very critical to you and if you've got uncertainty on both axes, you've got to use a statistical technique that's appropriate for that, especially if you're going to advertise it as statistically regressed.

MR. KIRK: Mark Kirk, RES. That's certainly -- that's a good comment and that's something that we can take away and have Lee Abramson, our statistician look at. But just to clarify, you're concerned about what, measurement error in the mean value?

DR. POWERS: You surely have some variability or you wouldn't be plotting standard deviations here.

MR. KIRK: Right.

DR. POWERS: I assume that you took -- you had five determinations. You found the mean of those five determinations and that's what I'm looking at down here.

MR. KIRK: That's correct.

DR. POWERS: And then you calculate the standard deviation by squaring the differences and dividing by four or something like that and then doing the square root, and that gave you the standard deviation.

MR. KIRK: Right.

DR. POWERS: Okay. So there is some variability in that one.

MR. KIRK: Well, one thing, if I could just interject momentarily, that I do want to point out is that what Doug is presenting is largely a data collection effort to provide input or what I would call seed information to the uncertainty analysis that's being conducted for us by the University of Maryland contractors and is going to go through the PRA process.

So ultimately, for example, if you had a parameter box on the uncertainty diagram that was labeled copper, you'd have two boxes coming out of that that's labeled -- or you'd have one box at least coming out of that labeled measurement uncertainty.

So it might not be the -- the goal here is just to inform you as to sort of the status of our data collection effort and present some overall trends. But the analysis methodology is ultimately going to be captured in the overall uncertainty analysis and therein every time we've got a measured variable like temperature, copper, whatever, there is the explicit question asked of do you need to account for measurement uncertainty or not.

So I think that's a good point to bring up, but that's also something that's going to be considered.

MR. KALINOUSKY: Also, as we did these, not all the -- some of these -- a lot of these points are based on ten values of measurements or more. And basically what we did was I continually filtered out more and more. So I removed the points like less than eight values and I'll remove some of the points with less than ten and I'll remove some more.

And the trend is still there and the slope of the line really didn't change that much for the more certain mean values. So that's one we did do to try to validate it, but we can also -- I'll ask Lee Abramson about what you're saying and see if we're going to do another rigorous way of doing that and see if we come up with the same idea or not.

One thing also I wanted to point out about this graph is that there's no difference between the CE heats and the B&W heats in this one. They're basically all intermingled. Some of these are CE, some are B&W welds. There really is no trend as in one is high standard deviation and one is low or anything like that, which, in the nickel term, which is the next slide, there is a difference.

Here we didn't attempt to put a line through this once we plotted it out, because it's obviously grouped, two separate areas. In this area here, we have -- the majority of these are B&W heats. These are all CE welds. Up here are high nickel addition welds, which are also B&W welds. So we are still looking at this, how we should approach it and how we should use the data the best we can for the heats we're using, because also the same problem we have with the copper is the heats we're using is in the PTS plant analysis aren't all represented here.

Some of them only had one reading, so we can't give it a mean or standard deviation, or might have had two, so we didn't have very certain about the standard deviation. So we can't just use the data that we have right now and say that's the heat mean, the heat standard deviation. We have to find some way of we have one plant that has one reading of a mean, so the mean would be about .6 something, and we don't know where to plot it on there, because we have to find some way of making that determination, and we're still looking at that.

So based on those plots, and by using the other K-ring squared test I talked about, we've determined that the copper could be either normally or lognormally distributed. The readings for those means we have could be either way. We'll be doing a -- in the final FAVOR code, we'll do a sensitivity study and compare the two.

DR. POWERS: How did you determine that the copper could be either normally or lognormally distributed?

MR. KALINOUSKY: We used -- we tested both of them and both of them were acceptable to our limits that we were measuring. There might be other ones that would fit. We didn't test every --

DR. POWERS: What does it mean you could have a normal distribution with a -- with something other than a constant standard deviation?

MR. KALINOUSKY: Say that again, please.

DR. POWERS: Your standard deviation isn't constant.

MR. KALINOUSKY: For a given mean, it is. At a given point, it's constant.

DR. POWERS: For a given mean, it's constant because --

MR. KALINOUSKY: This relation here would be based on that line. Basically, it's the equation of that line is what all this is. So you have a given mean, multiply it by this constant value, gives you the standard deviation at that point.

DR. POWERS: That's not a constant standard deviation. Well, go on.

MR. KALINOUSKY: For nickel, we couldn't do this and we are still looking at it for the same reasons.

DR. SHACK: What you're saying is that for a heat with that mean, then you're getting a distribution of copper in that heat. Is that what you're trying to say? Is that what this is trying to do?

MR. KALINOUSKY: Right. That's right.

DR. POWERS: Whatever that means. I mean, it seems to me that you have prima facie evidence that standard deviation is not constant with copper. How can it possibly be a lognormal distribution? It could well be lognormal distribution on the square, just looking at it, as a guess, the square of the copper concentration of some transform of it, but it's not obvious that -- to me, at least.

MR. KALINOUSKY: The next step we did was go for a weld local variability, which is what I said before would be the variability in a small area. We used a CE report we were able to get, had data for eight weldment blocks and we had five measurements at a quarter-T depth. So we used those five measurements from those eight blocks and we just calculated simple standard deviation for both nickel and copper and both of them came approximately about .01.

This was also independently done by -- it was Matthew Vaughan and -- who was the other one? Yes. Steve Byrne. It's Steve Byrne and Matthew Vaughan that did those also and they also came up with the number approximately .01, as well.

We can't classify what type of distribution it is right now. We still have to look at that and analyze it and

-- but the reflexes say it's normal, but we'll have to verify it through some statistical method.

Through wall variabilities still needs to be determined. We have some data we can use, if they determine that we should use that in the FAVOR code.

DR. SHACK: Again, I'm confused on this one.

MR. KALINOUSKY: Okay.

DR. SHACK: Are my eight blocks from a single weld?

MR. KALINOUSKY: Different weldments, weld blocks. So they made a weld -- a weld heat of -- one type of weld heat, then another type, eight individual ones.

DR. SHACK: But the same weld wire.

MR. KALINOUSKY: No, different heats, different weld wire heats. Does the backup slide show? This one.

MR. MALIK: Right here.

MR. KALINOUSKY: Okay. Anyway, it was a weld block, with the weld heat, and simply they just analyzed it for the content for the nickel and copper. Each individual made by different weld wire heats.

DR. SHACK: So I've got eight different welds.

MR. KALINOUSKY: Right.

DR. SHACK: And I take a T-over-fourth block from each one.

MR. KALINOUSKY: Right. They measured the T depth.

DR. SHACK: What does that have to do with the local variability?

MR. KALINOUSKY: Those points would be across the welds, so they would only be half an inch apart, quarter inch apart. So we've got the variability as you go across the weld at a certain depth. These are all -- so basically you're saying how does point --

DR. SHACK: I see. You're spacing them over the T-over-four.

MR. KALINOUSKY: Yes.

MR. KIRK: In the FAVOR -- it's perhaps important to point out that in the FAVOR code, there are sort of two different versions of local. One is you start off in your sample, you generate a sample from a region, which could be, say, the beltline weld or the plate or whatever.

So say you take a sample from the beltline weld. That beltline weld is then cut up into iso-fluents regions, regions over which we treat the fluents to be constant based on the fluents maps, which Shah is going to show you.

So now you have a region depending upon fluents variability that may be something perhaps big enough to hold in your hand. And the question was raised in some of the public meetings that we had on this that, okay, now, in your analysis of that vessel, you go through and say on run one, you seed a flaw into the circ weld, sub-region B. Then on loop 386, you wind up with a flaw in that same sub-region.

So then the question arises, in this analysis of this vessel, you had some Monte Carlo simulation of what the copper, what the nickel, what the composition of that region of material was, and the question came up, on run 386, should I now go and resample from the whole distribution, which is sort of what Doug was showing you earlier, or is there some smaller tighter standard deviation that you should be sampling from.

So what we were trying to do was to look at what data is available where you've got reasonably -- measurements of material composition reasonably closely spaced to try to make an assessment as to whether that resampling should be done from a smaller standard deviation or not. That's sort of the goal here.

MR. KALINOUSKY: These are what they looked like. We were taking these values here across the weld. That's how come I got the local variability there.

If you had to do a through thickness, this is what basically we would be using as a data set if we need to do that.

So we moved on to plate chemistry and here we have even less data. For every heat, we only had one or two points, so we -- then we couldn't get a standard deviation or any way to really analyze those.

So basically what we suggest doing here is just to take the best estimate we have and let's do not sample about it. Just say that's the best estimate and then we'll go the plate local chemistry variability and sample that.

For the plate local, here we're able to get three groups of data, once again, not much, with six points per each group. This was -- these came from surveillance specimens from St. Lucie and -- I can't remember the other one offhand right now.

Anyhow, we analyzed these and we found standard deviation to be, for the plates to be about .002 for the copper and .005 for nickel. So it's what we expected, very, very small, since plates are very homogeneous.

And we'll sample the previous mean using this standard deviation to give us a final value to put into the FAVOR code.

Let's move on to the initial RTNDT values. Once again, the amount of data we have is always the hard part here, getting enough data to use. So we pulled data out of RPVDATA. We grouped them by the heats and we used every heat we could find that had three or more measurements, so we have some idea of a standard deviation.

And that gives a total of 19 heats and a total of 65 data points. What we did here is we did a transformation of the data. We took the -- for each set of data, say, we had five values for that heat, we'd take a mean of it for the heat mean here, and we subtract the measured value to give us a delta value. So basically just transforming the data to a plus or minus around the average.

What we did do then was we graphed all those out with a histogram and came up with here -- with the blue would be the data we used and the red would be a fit about that data, and it came out to be a normal -- with a standard deviation of 16.6. So what we propose to do in the FAVOR code would be to generate a random number and let's say we get a .7 or so, go across here till we hit that, come down here and say, oh, it's plus .8. So we had that to our best estimate mean to give us some variability about that mean.

And we did the same approach with the plate, as well. Here, once again, we had a little bit more actually, more data for it. We had 128 total data points out of 37 heats. We did the same approach, transforming it with a delta value, and we went ahead and plotted that out and we ended up with this type of fitting, where it shows -- comes out pretty normal, the values, in both cases.

Any other questions? If not, we'll move on to Dr. Shah.

MR. MALIK: The next item in the presentation is developing detailed fluents maps for application of the plant-specific analysis and we have developed end-of-life fluents maps for two plants, and we are using available cycle-to-cycle fuel loading histories.

And also along with that, another objective is to determine what is the uncertainty of the fluents. We are starting out to perform in the FAVOR analysis an initial estimate of one sigma in fluents to be roughly like 15 percent of the mean, which is much better than earlier from the laboratory, 20 to 30 percent were used. So it's a real improvement from that point on.

And the methodology for fluents calculation are in the draft guide on dosimetry, 10.53. It was released in 1999 and another NUREG CR-6115 and this work is being monitored at RES by Bill Jones, as well as work is being performed at Brookhaven National Lab.

Two plant specific neutron fluents maps have been completed. One was Palisades. There was Robinson, but they opted out, so we weren't able to use that one, and the plants next to be analyzed are Oconee, which we will be finishing up in March, Calvert Cliffs in July, and Beaver Valley we are expecting sometime later on that.

We are using defined actual circumferential as well as radial grids to calculate fluents values. For example, Palisades, we have 205 axial, 97 times eight, there's one-eighth symmetry on the circumference.

Similarly, like between 20, around 12 to 20 radial grid points have been used in those two.

Also, we found that the fluents decay in Reg Guide 1.99, which is like minus .24X, is a bit conservative and we will show you a graph on that.

Here is a detailed plot, the circumferential horizontal direction, as you go around the circumference from zero degree to 360 degree and you have peaks here in the beltline area. Mid-core area, there is a peak, and as you go around circumference, this happens to be the area core flats are located. Core flats are the region where the reactor core is very close to the reactor vessel. So these four areas are the core flats and the reactor vessel are very close to those, so that's where you see those peaks.

And this fluents curve is right at the mid-core as you go along the axial length. At the top of the core is substantial drop-down.

Similarly, I have a plot like that for axial variation. Again, in the mid-core level, here is the axial variation, end of the core, top of the core, and here is a mid-core area, where it's the peak values. And you go around the -- this is the core flat area, and other angular locations, these are the values.

And because of this variation in fluents, we have to subdivide the region and perform the analysis in the

FAVOR code.

Here is the exponential decay I was telling you about. These are actual radial distributions of flow through thickness, peak volume in the inner radius, and as you go to the outer radius, it drops down, where minus .24X is used in Reg Guide 1.99 Rev. 2 and this is a straight line on the log graph.

Actual -- most of the initiation and all the PTS significant transients with the crack, quarter T on this side, there is very little difference. And even in this area of the graph, actually the crack was just initiated and the crack arrest takes place in this deeper part of the crack depth.

DR. KRESS: But cracks closer to zero or two inches, where the curves are pretty close together, are the ones you worry about.

MR. MALIK: Yes.

DR. KRESS: Okay.

MR. MALIK: Okay. The next item is the development of the FAVOR code. FAVOR is number of fracture analysis of vessels in Oak Ridge and it implements refined PFM technology and up-to-date materials data and we are trying to make it consistent with the current PRA, as well as thermal hydraulic input data.

In research, it's myself, Nathan Siu and Lee

Abramson from PRA site and the contractor -- the main contractor is Terry Dixon, who is present here. And University of Maryland and PRA areas are Professors Modarres and Mosleh, as well as input from Professor Natishan in the fracture toughness area.

The code is being used to answer to the kind of question, one, at the given -- at what point in the life of a plant will the acceptance criteria, risk acceptance criteria will be exceeded; for example, at present it's five by 10E-6, because your failure per reactor year. So if you are plotting effective from power year versus risk, in terms of failure, then you want to find out at what point this acceptance criteria is exceeded and plus what would happen if you have improved methodology or mitigative action we are taking, what will the effect of that and how much more plant life can we improve with all of that.

As you can see, it involves a number of different items. It starts out with a detailed fluents map, flaw characterization, plates, as well as weldments, embrittlement correlations to define shift in RTNDT, thermal hydraulics transients, and PRA such as event frequency, what are the credible sequence for PTS significance, the reactor vessel integrity database to define material chemistry, as well as industry database.

Also, along with that, and the extended fracture toughness initiation and arrest, and the defined fracture analysis methodology. They are all combined together to come up with a method to use for PTS analysis.

In addition, we are doing some additional development work here and trying to bring that, such as effect of 3D code, plume and things like that. We are also looking into that, as well.

Based on all of these integrated together to come up on a plant specific or on a generic basis analysis to be performed and then they feed in to finally revising the screening criteria.

It combines -- the FAVOR code combines the two NRC funded codes. OCA-P was historically developed at Oak Ridge, as well as VISA-II. So those two into a single combined code with all the best feature from the two combined together.

It also incorporates the lessons learned from the Yankee Rowe in early 1980s, as well as from IPTS analysis in mid '80s. Now, the code is in the third generation, so we have just in '99 released a version of the code. This plan is to continue development of technology derived from NRC analyzing history, available research and data.

This is, again, a list of the same thing, what are the features of flaw characterization in plates and welds, the map, embrittlement correlation, reactor vessel database, fracture toughness, and here we are not using surface breaking, as well as embedded flaw. Both types of flaws are being looked into. This is the first time we are analyzing. So we are taking one big step instead of assuming all flaws to be surface breaking. We are using surface breaking, as well as embedded flaws.

And as well as we're including the through wall residual stresses in the reactor vessel welds.

As I said earlier, an interim version of the code has been released in October and the next version is planned to be available by May, and we are implementing, with a lot of industry, a discussion on ways to improve it, make it user-friendly and efficient. One of our end goal is to have common understanding, what are the methods that are going into the analysis.

Here is a little bit -- a few slides to show what kind of independent verification we are doing. For example, here we went from FAVOR, which is an asymmetric code. We perform analysis using ABAQUS code and tried to compare what were the total gradient through thickness for PTS transients, FAVOR results shown in the red, as well as -- sorry -- in the rectangular black color, and ABAQUS results are shown as well.

And similarly, this is the resulting hoop stress from a thermal gradient shown here.

DR. KRESS: The hoop stress is varying because your pressure is varying. So that's just a plot of how good it predicts pressure.

MR. MALIK: This is a plot of temperature and this is the hoop stress through the thickness.

DR. SHACK: There is a thermal stress contribution to that.

MR. MALIK: Yes.

DR. SHACK: But what was this temperature -- I mean, what was the -- you just changed the temperature on the surface? What problem are we really looking at?

MR. MALIK: It's for exponential decay temperature.

MR. DIXON: Terry Dixon, from Oak Ridge National Laboratory. Actually, there's been many verification and validation problems done. This particular one is for a stylized exponential cool-down rate, but I could just as easily put together slides for discontinuous functions such as repressurizations.

It's a finite element based code. So it will handle any thermal hydraulic boundary conditions that you want to impose on the inner surface of the vessel. This particular one is for an exponential decay, thermal, and I believe it was a constant pressure, but this also includes the through wall weld residual stress, as well as the clad based differential thermal expansion.

DR. SHACK: But this is all -- this is a truly axisymmetric problem.

MR. DIXON: Yes.

DR. POWERS: So you're not looking at what the limits are on how much variation you could have asmuthally and still get it.

MR. DIXON: No. This is a finite element analysis.

MR. MALIK: This is the verification of the stress, again, for this one, I think it was for the region around the crack front and as you can see, both comparing them with FAVOR, using ABAQUS solution as well as FAVOR, now here is the depth point and here is the point along the circumference of the crack, and the K solution are pretty much matching.

The reason they are matching here is because the equation that went into the FAVOR code originated from the finite element itself, this should match up very closely.

And here is the comparison of -- this is for the surface breaking flaws. Now, this is for the embedded flaws. Here we are showing three definition solutions here and both open and close symbol are showing here for our calculation in FAVOR. These three are for three different distances away from the inner surface, but this one is very close, this is a little bit away from the inner surface, this is farther away from the inner surface. So there three different solutions.

Now, this shows a detailed fluents map and fluents in the mid-core area through the circumference, very significantly, and to match that, we need to divide the beltline area into a number of segments, called sub-regions. Here is the axial weld, the plate area, then the circumferential welds, and the lower axial weld and the lower plate area.

So we are dividing into a number of sub-regions to as closely as possible see the distribution through the vessel beltline area, both axially as well as circumferentially.

This is a sample calculation. It shows that application of this methodology in the FAVOR code can improve the or extend the life of an operating plant. It was done for Calvert Cliffs, NUREG report CR-4022. We have taken the same PRA, as well as the same thermal hydraulic results, but only the fracture mechanics part has been varied. There are four different codes showing over here.

The effect of full power year or the RTNDT values versus what is the probability of failure per reactor year. The first -- the top curve is for surface flaw distribution. That was the flaw distribution available before we have our own distribution, we are just working on that.

So there were just surface breaking flaws and Reg Guide 1.99 Rev. 2, the correlation. The top curve. Here is the acceptance criteria for the risk, five times 10E-6, and it shows up like 32 effective power year.

Whereas if we take the surface flaw distribution and improve the correlation, it's one -- the embedded correlation has been used, not the one that you're going to be using.

DR. SHACK: A revised.

MR. MALIK: A revised, yes. And you see a significant improvement, as you can see. At this point, it's almost like it doubled the life in the plant.

The next step, what happens if you just use the PVD -- a distribution in the process. It has only embedded flaws. There were no surface breaking flaws in it. But using Reg Guide 1.99 Rev. 2, earlier correlation, you see this curve here. A significant improvement compared with the top one.

Now, what happens if you combine the two together? Here is the last one in which you have used embedded flaw distribution from PDF and the revised correlation gives at least an order of magnitude on the curve.

DR. SHACK: Now, these are presumably done with the old K-1-c and K-1-a distributions.

MR. MALIK: Yes. There will be some more benefit derived from that as well, yes.

DR. SHACK: I thought they got worse when they did the statistical analysis.

MR. MALIK: You cannot say that it has to -- there are three different transients considered here and in some cases, it goes up. So this is all three transients considered together.

In summary, work in the PFM area is coming along very vigorously, actively, and some of the major technical activities are in the correlation of fracture toughness. We'll be completing in the April to May timeframe. The plant release the FAVOR code with those in May to June timeframe.

We are implementing those technical enhancements as they become available. We don't want to wait around for them.

And we come to new coordination and interaction with PRA and thermal hydraulics sub-group to bring their ideas into especially the uncertainty analysis part of the program.

And there are some delays, as you can see, one of the plants moving out and replacing it with another plant means we have to go do fluents calculations and some of it is materials related, as well as frequency and all those things, systems, it needs to be done again.

At least for the PFM part, we see about two month lag on that.

This is my part of the presentation, if there are any questions.

DR. SHACK: I don't see any further questions, so we can move on to the flaw distribution, I guess.

MR. MALIK: All right.

DR. SHACK: From rocket science to expert opinion.

MR. MALIK: Debbie Jackson will be the one who will tell us about that.

MS. JACKSON: I am going to give you updated information on what's going on with the development of the flaw distribution. That's part of this PFM work. This is just a quick list of some of the topics that I'm going to discuss today.

I'm going to go over the background, which I think Shah has touched on today; the approach that we're using; a little bit of information about the reactor vessel fabricators; the material that we're using for developing the flaw distributions; the expert elicitation process and some concluding remarks.

DR. POWERS: When you say flaw distribution, are you speaking strictly of density and size or do you include orientation and location?

MS. JACKSON: Density, size, location, orientation. I'm going to get all that information.

DR. APOSTOLAKIS: Is it really expert opinion elicitation process rather than expert elicitation process?

MS. JACKSON: Expert, yeah, expert judgment process. We kind of -- I was using that interchangeably, but actually, yeah, it's expert judgment and elicitation is one section of it.

These are the objectives of the presentation. I'll discuss the need for the generalized flaw distribution, talk about the process, and then discuss the status.

This is the background, which was discussed a little earlier today, as to why we're doing all this work we're doing with the PTS and the flaw distribution is an important input to the fracture mechanics calculation, so that's why we're going through this effort.

And we believe that the fabrication process presents a number of variables that we need to review for the flaw distribution; specifically, the fabrication process and the different welding processes that are used.

We're going to go over a little bit about the expert judgment, why we're doing it. It's needed to review, interpret and supplement available information on the reactor vessel fabrication process. A lot of the people who are involved in the actual fabrication processes for reactor vessels are getting up in age and we don't have a lot of the information here. So that's why we decided put together this expert panel, so we could get people who are actually involved in the fabrication process.

This is a list of some of the reference documents that I've used. The NRC has done some expert judgment processes in the past for other subjects and these were some of the documents that I used just for reference in terms of determining how you go through the expert judgment process. In addition, Lee Abramson, who is going to do a part of this presentation, he has been involved with the majority of these elicitations or expert judgment processes.

This is a list of the domestic reactor vessel fabricators, Combustion Engineers fabricated a majority of the vessels. Babcock & Wilcox, Chicago Bridge & Iron, Rotterdam, and New York Ship Building, and this data was obtained from the reactor vessel integrity database, the RVID, which NRR is responsible for putting together.

Those numbers were just the operating reactors, the ones that are presently on line.

This slide shows the material that we're using. Midland was done some time ago and PVRUF, Shoreham, River Bend and Hope Creek, which were being examined by Pacific Northwest National Lab, they are all done using an upgraded SAFT UT system. So this is the current pieces that we're using.

The PVRUF, Shah mentioned this briefly, this was completed. One issue came up in one of the meetings that we had with industry sometime late last year. They asked a question, they said a lot of the -- the majority of the material that we have was weld material, so what are you going to do with the base metal, because there's so much more base metal, and the numbers that are presently being used for the base metal were just kind of developed through discussions with some of the experts.

So what we have decided to do, we have started actually inspecting some of the base metal so that we can get a valid distribution for that.

DR. SHACK: Now, EPRI is also doing some evaluation of the flaws in these weldments, right?

MS. JACKSON: Right. The Shoreham material specifically is what we're working on with EPRI. PNL has done some exams of the Shoreham vessel material and then we've sent the material to EPRI so that they can use the methods that are currently used in the plant, because the SAFT UT method isn't presently used in the plants. So that's what EPRI is doing.

DR. SHACK: So their goal is not to characterize the flaw distribution, then. It's to benchmark the current techniques through the SAFT.

MS. JACKSON: And also to verify some of the data that we have, just kind of like a backup of the information we have.

I just have a very old photograph that I have that I found going through some paperwork that I had. This shows one of the vessels being fabricated at Combustion Engineering.

This is one of the methods where they -- you can see the weldment here. There are two methods that they used to make the rings. One of them, they actually did the forgings, and another one, they used three plates and they weld them together to form a shell.

As you can see by the by this picture, it's very old. This was taken in the early '60s.

The data that PNL is gathering from the PVRUF, this is how it was determined that they were going to categorize the flaws just for ease of classification and determining what we would use, because there is a different flaw distribution -- well, a different number of flaws in the welds versus the base metal.

And a lot of the flaws so far from the PVRUF were found in the fusion lines or they were found in repairs, weld repairs. The largest flaw in the PVRUF was found in a weld repair and that was 17 millimeters.

This graph shows the comparison between the

Marshall distribution, which was the existing flaw distribution that was used for many years, and this is the PVRUF data that we have. There are approximately 2,500 indications that were found in PVRUF.

DR. SHACK: These are combined flaws, right? You're not discriminating here between this is the planer flaw, this is --

MS. JACKSON: Right. These are just all the --

DR. SHACK: All the indications.

MS. JACKSON: Yes. These are just all the flaws. All of the different flaws. And we have some data from the Shoreham vessel. They've just finished doing the UT exams of the Shoreham vessel and this compares the Shoreham to the PVRUF. They found a lot more flaws in the Shoreham vessel than they did the PVRUF. Both of those vessels were fabricated by Combustion Engineering, but they were fabricated in different timeframes.

There were no surface breaking flaws located in either of the vessels so far to date, and they just started doing the UT exam on River Bend.

Now, I'm going to go through some of the steps that were involved with the expert judgment process to determine the generalized flaw distribution. First of all, the staff and the contractors, we discussed some different issues that we felt needed to be addressed and information that we wanted out of this expert panel.

We determined the level of complexity and what we had decided, we had wanted information specifically on the weldments, the base metal. We broke the base metal up into two groups, the forgings and the plate material, and the cladding. We identified an expert panel. We developed the issues and we sent them to the panel for their review, to see if they had any comments, if there were anything that we were overlooking.

We had a panel meeting. This was our first -- I'll go over this more in detail a little later. And we had elicitation training. Elicitation training is important because during the individual elicitation sessions, you want to eliminate as much bias as you can from each individual expert. So we spent a day and a half going through elicitation training with each of the experts.

DR. SHACK: You were looking at Prodigal for a while, which is another expert judgment approach to the characterizing flaws in weldments.

MS. JACKSON: Yes. Prodigal is actually a simulation. They don't have -- we did put the PVRUF data into a Prodigal simulation code and it came out, the results were pretty similar to what we actually got from the data from PVRUF. But the -- two of the people who are actually on the expert panel for the Prodigal are on this expert panel that we have for the flaw distribution.

And so far, we've elicited one of the experts so far who was on Prodigal and we -- he had some interesting comments, so we just need to talk with him a little more to verify some of the issues that he stated during his elicitation session.

DR. SHACK: What is the expert judgment supposed to -- I mean, are they supposed to come up with a hypothesized distribution? Prodigal sort of constructs a distribution based on judgment. Are these guys supposed to -- a beauty contest or what, five flaw distributions?

MS. JACKSON: What we've done, initially, we gave them a list of issues to try to get them thinking along the lines. We presented them the PVRUF data that PNL did and we made a presentation on the Prodigal work that was done to date.

What we want them to do is from their own expert -- well, from their experience, each expert has individual experience. Some of them were actually involved in the fabrication process. Some of them did the NDE inspections of the individual vessels. One particular expert provided some of the welding material to the vessel fabricators.

So we want their own individual opinion from their area of expertise on what we've done so far to date, if they feel that's the correct path to go through to get the generalized flaw distribution, and also if they think a generalized flaw distribution can be developed, one flaw distribution.

DR. APOSTOLAKIS: What is flaw distribution, again?

MS. JACKSON: Excuse me?

DR. APOSTOLAKIS: A flaw distribution, what is it?

MS. JACKSON: It's the --

DR. APOSTOLAKIS: Probability distribution of what?

MS. JACKSON: It's the measurement of the number of flaws per cubic meter in the vessel material.

DR. APOSTOLAKIS: Independent of length or just flaws?

MS. JACKSON: Just flaws, but the flaws have been broken down into the different sizes. Some of them in the inner 25 millimeters of the vessel and then the outer vessel, those flaws that are in the weldment.

DR. APOSTOLAKIS: Now, the experts are going to give you the whole distribution? I think that's what --

MS. JACKSON: No, they're not going to give us a distribution. That's -- they're going to -- well, Lee will go into a little bit more detail about that, because he's going to go through as to how we go through the statistical process to actually develop the flaw distribution.

DR. APOSTOLAKIS: Okay.

MS. JACKSON: Through these experts.

MR. HACKETT: Let me make a quick comment on that, too, again, because Debbie touched on it. This is Ed Hackett. One of the things, the key things that we're looking for from the expert elicitation process is, is there a generalized flaw distribution or is that some kind of fantasy construct. Just speaking as a metallurgist myself, I could say that there would be good reason to expert a standard or generalized distribution for CE vessels that were fabricated with submerged arc welding over some time period.

Whether or not you can extrapolate that kind of thing to cover all vessels that were manufactured in the United States over the last 20 years and is there a generalized flaw distribution, I think we know there are some exceptions to that already, just based on the fact that we know B&W used electroslag as a process.

It's not a multi-pass process. It's very, very different from the other populations.

So there is a big question just in terms of is there a generalized flaw distribution or do we have to get more specific about it.

MS. JACKSON: Thanks. Yes, because of the varying processes that they used for the different vessels, it may

-- we hope that we can get one distribution.

DR. APOSTOLAKIS: Who is your technical facilitator in the group?

MS. JACKSON: The technical, Lee Abramson. The TFI?

DR. APOSTOLAKIS: Yes.

MS. JACKSON: Lee Abramson is heading it, but it's going to be a group of us who are going to be doing -- actually analyzing the results. There will be three to four of us who will be doing that.

DR. APOSTOLAKIS: What kind of expertise will be represented there?

MS. JACKSON: What type of expertise do this --

DR. APOSTOLAKIS: I know Lee's expertise.

MS. JACKSON: Lee's -- we're going to have metallurgists, NDE experts, fracture mechanics, and the -- Lee, being the statistics expert.

DR. APOSTOLAKIS: The three NUREGs that you cited earlier, are they using this concept of TFI? I know NUREG-1150 did not.

MS. JACKSON: They didn't actually use the TFI, because they have -- what I was looking for was what the process they used in terms of getting their experts and how they analyzed the data. There is another document that was put out by ASME that -- this is more of a formal process using the technical facilitator integrator and developing the panel. It's a document that ASME has put out. I can't think of the exact number right now.

DR. APOSTOLAKIS: A standard? Are you referring to the PRA standard?

MS. JACKSON: No. I don't -- I'll have to get back with you, but I used that document to get the format for going through this process and discussions with Lee.

DR. POWERS: I had thought you did use the technical facilitator. They didn't use the terminology.

DR. APOSTOLAKIS: They didn't really use the TFI. The TFI -- I think NUREG-1150 tried to be more neutral. The TFI, according to the original definition, to, in fact, put things together if the experts disagree, according to his judgment.

MS. JACKSON: Different documents --

DR. APOSTOLAKIS: Is this what you intend? 1150 didn't do that. 1150 elicited rates and processed them.

DR. POWERS: They made a decision on how they were going to run things, but in those cases where they had difficulties, and there were a couple that did have difficulties, the equivalent of TFI --

DR. APOSTOLAKIS: It comes close.

DR. POWERS: -- made a judgment and they went with it.

MS. JACKSON: Right. Some documents use different terminology, but it's basically the point of the process where you aggregate all the results from the experts.

DR. APOSTOLAKIS: That's a technical integrator.

MR. ABRAMSON: This is Lee Abramson. Perhaps I could clarify that. Here, the TFI we're just referring to is the team of people. I guess the NRC and maybe some of our contractors who are going to pull everything together and come up with the -- I guess, in effect, the input which can be used for this generalized flaw distribution, based on the expert panel elicitation, on the rationales and so on.

DR. APOSTOLAKIS: I understand that. Well, there is a NUREG on the probabilistic seismic hazard analysis which defines this thing and makes a distinction between a technical integrator and a technical facilitator. So that's why I'm pressing the point, because there is a difference.

MS. JACKSON: What was the number that you said, again, please?

DR. APOSTOLAKIS: It's in NUREG report on probabilistic seismic hazard.

MR. ABRAMSON: That's the Shack report, right?

MS. JACKSON: Okay.

MR. ABRAMSON: The Shack report.

DR. APOSTOLAKIS: Yes. And there is a distinction between a TFI and a TI. And from what you are saying now, you are really going to be technical integrators, more like 1150, with maybe some --

MR. ABRAMSON: That's probably correct. We may be a little lose in the language here.

DR. APOSTOLAKIS: If you put the word facilitator there, it means something specific.

MS. JACKSON: Okay. We'll remember that, because that's something we've been using. Okay. The expert panel that we put together, there are a total of 17 people on the expert panel. We have people from the U.S. Navy, from academia, EPRI, independent consultants, and retirees from different organizations.

DR. APOSTOLAKIS: How many you have total?

MS. JACKSON: Seventeen.

DR. APOSTOLAKIS: Seventeen.

MS. JACKSON: This is areas of expertise of the various experts. The construction code failure analysis, fracture mechanics, metallurgy, NDE, reactor vessel fabrication, reliability of flawed welding structures, and actually welding.

We also have people who are involved with the steel fabrication process for the vessels.

This is the schedule. These next two slides, I'm going to go over the schedule. The items that have checks on them are items that have been completed to date. These two group -- these three items actually happened when we had the Atlanta meeting. We had the first meeting of all of the experts and Lee performed the elicitation training and we discussed issues and we also have the elicitation team identified.

We're going through the elicitation of the experts right now. We've already completed the elicitation of four. We're doing one elicitation tomorrow, one of the experts. This process, where we're going to take all of the elicitation data from the experts and integrate, that's going to happen late this month and sometime in April.

We're going to have another meeting of the expert panel, so that all of their responses and their rationales can be reviewed. That will be done the first part of May. The final responses and rationales will be put together in the end of May and then we're going to have a workshop at the end of June where we're going to present all of the information from this expert judgment process, and that will be the 27th and 28th of June here at the NRC.

The next two slides are going to have a list of the issues that were presented to the experts to develop conversation and so that they could get a general idea as to what type of information we wanted from them.

From the PVRUF data, we haven't found any surface breaking flaws, so we wanted to find particularly if anyone knew of any existence of any surface breaking flaws and we also have the two experts, one from -- who has information from the UK Navy and the US Navy. So we have people outside of the nuclear industry also.

This particular issue with Hatch, there is a flaw that was fond in a nozzle region in the Hatch vessel and after that was found, they had changed the inspection methods for vessels at CE. They increased the inspection process, so that resulted in additional weld repairs and from the PVRUF data, we found out that a lot of the flaws were found in the weld repairs.

And this particular event happened in the early '70s and in the mid '70s, they said that maybe they were a little bit too reactive and they were doing too many weld repairs, so they back and changed the inspection process, not to what it was before the Hatch incident, but it was so that they wouldn't have to do so many weld repairs, because the weld repairs were just increasing at an alarming rate.

This is just a brief summary of what went on during the first expert panel meeting. The definition that we came up with for flaw was an unintentional discontinuity that had the potential to compromise vessel integrity. That's what the definition of the flaw that's going to be used through this process when we're eliciting the individual experts.

DR. POWERS: Can I ask a couple questions? You chose distributions which consist of density versus --

MS. JACKSON: The through wall extant.

DR. POWERS: And extant, right. Do you have anything that you can show us on how you're handling orientation?

MS. JACKSON: I don't have a backup slide with that information, but I can give that to you. That is one of the other presentations, the location and orientation of the various flaws.

DR. POWERS: The other question is, in the densities, is there any likelihood that the flaws are not uniformly distributed within the local volume, but are, in fact, clustered? And if you do, how do you handle that?

MS. JACKSON: Some of the flaws were clustered. They used the ASME proximity rules to separate them, because when we initially went through the NDE exam, some of them did appear to be clustered.

DR. POWERS: They can separate them for the measurement purposes, but now how do they transmit into the rest of the process to say what's the probability that you have a cluster of flaws in this particular piece of metal?

MR. HACKETT: I think I'll comment on that, also. Ed Hackett. Dr. Powers raised this question earlier in the day and it's a good question. The answer does basically relate back to the ASME proximity rules, which are going to take a series of flaws that are grouped together, as you say, in some kind of cluster and then look at the dimensions and the orientation and decide if those should be counted as a single bounding flaw, which is then what you would feed into the fracture mechanics.

So the short answer to it is that the ASME proximity rules would be applied to any clusters and then are there clusters, I think the answer is absolutely yes. You certainly see a very large cluster of discontinuities, as Debbie put it, at the clad-base metal interface with the heat affected zone for the cladding, basically, which is an expectation you would have from the metallurgy in this situation.

So that's the short answer. The good news is that, as Debbie pointed out, we're not seeing surface breaking flaws and these discontinuities that we do see that are clustered are generally inconsequential when it comes to the single dominant flaw fracture mechanics type driving force.

The ones that the clad-base metal interface, I believe, in PVRUF, for instance, were largely of the two millimeter type extent. A lot of them were also volumetric. So a lot of those are just not participating in the -- in contributing to the failure frequency of the vessel and the probabilistic assessment.

DR. SHACK: But is that saying, in that size distribution we're looking at, then some of those are actually clustered, that they've decided to build together based on the ASME rules?

MR. HACKETT: I'd have to go back and check that, Bill. I'm not entirely sure. It should be. The answer to that, if that's the case, they are clustered and they're close enough, like you have this grouping of flaws that are nominally two millimeters, but they're only a half a millimeter apart, well, then, I think the ASME rules would say, no, you better add those all up and count them and make the -- they're close enough to the surface, you're also going to have to count that as a surface breaking flaw.

So those things should be addressed as part of the flaw distribution.

MS. JACKSON: Right.

MR. DIXON: I've got a couple of comments to try to address your question. The question with regard to orientation, flaws that reside in circumferential welds are considered to be circumferential flaws. Flaws that reside on axial welds and plate are assumed to be axial flaws.

So in the axis of the principal stress, to answer your question, there is no sampling.

DR. POWERS: Okay. That's really the question.

MR. DIXON: There is no sampling with regard to orientation.

DR. POWERS: Whatever the axis of the stress is.

MR. DIXON: Right. However, with regard to the second question, Ed addressed the fact that putting together the flaw size distributions, proximity rules are used, but in the sampling, there is no proximity. The way the flaw distributions are, it's something like this. The first 15 percent are postulated to reside in maybe the first one-eighth of the wall thickness. The next 25 percent are between one-eighth and three-eighths.

So the wall thickness are partitioned. So when you are in the loop, if you want to call it a loop, of placing flaws, you're going to first decide is it a category one, two, in other words, in with partition does it exist.

Then the other assumption is that it has equal probability of being at any location in that partition. Does that address your question?

DR. POWERS: Maybe. Maybe I have to see exactly -- go through the mechanics exactly. Let me see if I've got it.

MR. DIXON: Okay.

DR. POWERS: You end up with a flaw distribution. That has some big flaws in it.

MR. DIXON: Yes.

DR. POWERS: Okay. There is as fair probability that the big flaws are in -- were, in fact, stemmed from identifying a cluster of flaws that you added all together.

MR. DIXON: Yes.

DR. POWERS: You may not have ever seen a flaw that big, but just saw a cluster of them that was effectively that big. So now when you apply the distribution in your analysis, you sample, as statistics would dictate, from the whole distribution.

Sometimes you're putting in a big flaw which corresponds to that part of the distribution that came from both big flaws and from clusters that were effectively big flaws.

So you don't actually say there's -- okay, there's flaw, flaw, flaw, cluster of flaws, then flaw, flaw, flaw, cluster of flaws.

MR. DIXON: No.

DR. POWERS: I think I understand what you're doing.

MR. DIXON: Every flaw is treated independently.

DR. POWERS: Okay. It would, incidentally, be useful for the benefit of mankind and possible future people that want to go in and further improve in your work if you did, in the documentation, keep track of clusters and their distributions. Maybe not be part of your work, but the next guy that comes along might be interested in what you found there.

MR. ABRAMSON: I would like to describe how we're going through the elicitation sessions. First, we're doing this individually with each experts, each of the 17 experts. And we have a team there and normative expert, I'm serving as that, and then we have various subject matter experts available, and also the recorder, and Debbie has generally been doing that.

Then we present a list of characteristics to each expert, and I'll have a detailed list of that in a moment, and then we ask the experts to identify and discuss the pair-wise interaction between the characteristics, and let me explain what I mean by that.

We generally -- we start off the session by just giving each expert a copy of this interaction matrix. Now, here are the -- we have identified 14 what we call characteristics, the product form, forgings, plate, cladding, weldment, weld processes, form mechanisms, and so on.

And these are just the headings. We have a very detailed discussion of each one of these. Like for the form mechanisms, there are any number of them, for example and so on.

We say, all right, each flaw can be characterized by each of these characteristics. Each flaw can be characterized like in 14 ways or 14 dimensional flaw and it has a particular product form and has a weld process that it was formed by and it has -- the flaw has a particular mechanism, et cetera, et cetera, et cetera. So each flaw is unique in this point of view.

Now, what we ask them to do is we know that these aren't necessarily -- that -- what we're going to be asking them, in effect, is the likelihood that each one of these will lead to a flaw of a particular size and we know that there can be interactions between these.

For example, the welder skill could be very important as to whether or not you have a flaw and that could interact with the flaw mechanism, for example and so on. The experts are going to tell us all this.

So we ask -- we go through this one by one, basically each one of these characteristics and we ask them to discuss any possible interactions with all of the others.

And, of course, we're recording all of this.

DR. APOSTOLAKIS: But, Lee, just to know that the welder skill is important gives you half the picture. Don't you have to know how skilled the actual welders were? I mean, you're talking about the significance of each one of these. How do you know that?

MR. ABRAMSON: Yes. Well, this is what we ask the experts, whether they consider welder skill. I mean, all the welders are qualified and so on. And so we talk about the effect of the particular skill of a welder and whether that might make a difference or not.

DR. APOSTOLAKIS: But, I mean, let's say that they tell you yes it makes a difference. Now what do you do? Wouldn't you have to decide --

MR. ABRAMSON: We're going to ask them -- I'm going to tell -- I'm going to come to that in just a moment as to how we're going to use this.

DR. APOSTOLAKIS: Okay.

MR. ABRAMSON: In effect, we're doing this -- there are no numbers. Eventually, we're going to have to elicit some numbers in order to be able to get a distribution, but here, this is all qualitative and what it does is assess the stage, as I see it, it gives the experts a chance to discuss how they view each one of these characteristics and, in particular, they're going to focus generally on their own areas of expertise.

And I ask them to talk about interactions. Again, I think very useful material as far as the rationales for everything like that. It kind of sets the stage. We don't ask for any numbers at this point.

So this discussion goes on for maybe a half an hour or longer, going through this matrix. And I think it serves that useful purpose, also to get the experts oriented into the mode of thinking that as to how each of these characteristics might possibly affect the likelihood of a flaw.

DR. APOSTOLAKIS: And that's a scale from one to 14?

MR. ABRAMSON: I'll come to that in just a moment.

DR. APOSTOLAKIS: So what are the columns?

MR. ABRAMSON: Pardon me? On, the columns. When I say interactions, you have 14 characteristics and here are 14 columns.

DR. APOSTOLAKIS: You just put X's.

MR. ABRAMSON: You just put X's, that's right. They put X's there. So that this -- as I said, this gives the experts an opportunity to give us a benefit of their experience, how they see these particular characteristics, and to ring in how they see it affecting, in a qualitative way, the various likelihood of a flaw.

All right. And then we get to, I guess literally it will be the bottom line that we're going to need in order to get the distribution, although we consider -- this is an essential part of the process of getting these rationales out in the open and we're going to report these back, as Debbie indicated, to the experts and, of course, in the final report.

So after we've gone through this discussion, we go through the characteristics one at a time. For each one, we ask the experts to identify that alternative with the largest likelihood of leading to a flaw. Now, for each of the characteristics, we have a number of alternatives, and Debbie is going to talk about those.

For example, the weld processes, there's automatic and unautomatic, we have a number of them, versus manual. So these are the alternatives for the characteristics. So we have these sub-categories. We have a number of these for each one of them and we say, all right, which is the most important, in your opinion, that's going to be number one.

And then what we do is we don't ask them for any absolute numbers. We ask them for only relative numbers. And we say compare each alternative with the highest ranked alternative, how much less likely is it to create a flaw. We get a factor, a factor of two, a factor of three, whatever, ten percent less, 15 percent less and so on.

And we ask them for that number and, also, in addition, we ask them for three numbers. First of all, I ask them for high, mid and low value. The mid value is one that's where they say their best guess, if you like, a 50/50 chance. And we went over all of this in detail when we did the expert elicitation, what a mid value and a high value are.

A high value is supposed to be a subjective 90 percentile -- excuse me -- 95 percent. So we say a high value is such that you're almost sure that it's not going to be higher than this. You've got about a five percent chance roughly. And a low value, you're pretty sure it's not going to be lower, it will be less than five percent.

So you've got the high value, which is 90 percent, mid value is about the median, all subjective, of course, low value is five percent, so the difference between the high value and the low value is like a 90 percent confidence level. So we ask all of this.

DR. APOSTOLAKIS: I don't understand that bullet, frankly.

MR. ABRAMSON: Pardon me?

DR. APOSTOLAKIS: I understand the first two. So you're comparing each alternative with the highest ranked alternative.

MR. ABRAMSON: That's right.

DR. APOSTOLAKIS: What is the relative change in likelihood? I don't understand that. What do you mean by that?

MR. ABRAMSON: Okay.

DR. APOSTOLAKIS: Let's take the example on slide 25, the processes, you have automatic --

MR. ABRAMSON: Okay.

DR. APOSTOLAKIS: -- and then manual.

MR. ABRAMSON: Right.

DR. APOSTOLAKIS: So now somebody says the highest ranked alternative is manual.

MR. ABRAMSON: Manual, right.

DR. APOSTOLAKIS: So now I compare the three automatic alternatives to the manual.

MR. ABRAMSON: Exactly.

DR. APOSTOLAKIS: As you say, somebody says SMAW is a factor of two less likely and so on. We've done all that.

MR. ABRAMSON: Right.

DR. APOSTOLAKIS: What is the relative change in likelihood of a flaw and how that plays into this?

MR. ABRAMSON: Okay. Let me say how we're going to use this. You have no question about we're making the relative -- you get the relative values. The question -- I think what you're asking is, and that's, of course, essential for this process, is how is all this going to be used in order to get what we call a generalized flaw distribution.

DR. APOSTOLAKIS: Because that's where we're headed.

MR. ABRAMSON: That's where we're headed. Okay. Let me tell you how this is going to be done.

DR. APOSTOLAKIS: It's not clear. I really don't understand what you mean by assess relative change in likelihood.

MR. ABRAMSON: All right. We're going to start with the PVRUF distribution, because that's based on data. That's the only thing we have, and we've got some hard -- we've got some numbers out of that and Debbie has gone over that and you've heard presentations on that.

Now, the PVRUF flaws all have their characteristics. It was a CE vessel, some of them are automatic, some are manual, some are repaired and so on and so forth.

Therefore, for every kind of flaw, for every kind of flaw there, we can characterize it -- flaw size, and we have the distribution, for every flaw size, we can characterize the PVRUF data according to this 14 characteristics in the matrix.

And we have -- we know what the flaw distribution is. We know what the likelihood, what the probability of a getting a flaw of a particular size is. That's the data -- that's what the data gave us.

Now, we have another pressure vessel, with other characteristics. Let's, for example, say one of the PVRUF flaws was a manual weld. All right. Another pressure vessel had an automatic weld. Now, the experts are telling us that, say, an automatic weld is half as likely to have a flaw of a particular size. So what we do then is we're going to take that distribution and we're going to divide by two.

DR. APOSTOLAKIS: So you are, in essence, adopting the original distribution to the new vessel with the new characteristics using input from the experts.

MR. ABRAMSON: Precisely, that's right. We have this benchmarked distribution, it's a PVRUF, and then we have all the relative comparisons.

DR. APOSTOLAKIS: So the experts never give you absolute results.

MR. ABRAMSON: No.

DR. APOSTOLAKIS: It's always relative to the original distribution.

MR. ABRAMSON: That's right. Frankly, I think that this is -- it's fortunate that we have the PVRUF data, because it's much harder to give absolute numbers than it is to give relative numbers, especially when they have no basis for it. They have no basis.

We're fortunate -- I mean, obviously, that's what we did in the project to get this PVRUF data and we intend to use this as an anchor in order to be able to get the generalized distribution, with, of course, the uncertainties and so on and so forth.

So that's the program and that's how we intend to use this information.

DR. APOSTOLAKIS: Again, I don't understand the inspector skill or the welder skill. How does that enter? I understand the materials, the procedure, the weld processes we just discussed, because they're more or less objective. But when you come to welder skill, what does that mean?

MR. ABRAMSON: Well, the experts have told us, of course, that the particular skill of the welder can matter. The problem is, of course, I think many of these welds are -- well, I don't know if any records exist as to which welders did which welds and what their skill level was and so on and so forth.

Of course, we assume they're all qualified welders. Recognizing that there could be some variability, one way this could enter into it is to say, well, we may want to try to put some kind of a fudge factor or an uncertainty factor based -- let me back up a minute.

Let's say that the experts tell us that for a particular kind of weld characteristics, welder skill is important. Maybe it isn't, maybe it is, but let's say it does. The particular kind of weld, the manual welds, it's a very complex weld for repairs, for example, repairs. It's a repaired weld and welder skill is important, but we don't know what the welder skill is.

So what this tells us then is since we don't know, that maybe what we should do is we should add some factor for increasing the uncertainty in the effect, because they're telling us that welder skill is important. We don't know what a welder skill is, so this, in effect, would add to the uncertainty on to the flaw distribution.

So that would be how we could use it, and, again, we're going to be guided, of course, to a great extent by what the experts are telling us and our own judgment of how to incorporate this.

DR. APOSTOLAKIS: The last question has to do with your 14 by 14 matrix. So you've explained now what the third bullet meant, but you had the original distribution as the reference point.

Now, if you had these correlations, how do you handle adjusting the values?

MR. ABRAMSON: Again, we're going to do what the experts tell us and we're asking them for a particular product form, for example, what are the answers. What we're doing is where it does matter with these interactions, we elicit different values for these relative changes.

DR. APOSTOLAKIS: So is it possible then that you say, well, look, welders kill is important and it's strongly correlated with inspector skill?

MR. ABRAMSON: Yes.

DR. APOSTOLAKIS: So we're not going to count inspector skill because we have already done the other one.

MR. ABRAMSON: That's right.

DR. APOSTOLAKIS: These are the kind of judgments.

MR. ABRAMSON: Exactly.

DR. APOSTOLAKIS: That I would have to make.

MR. ABRAMSON: That's right. Exactly. Now, we recognize that some of these, like welder skill and inspector skill, you're really not going to be able to get any numbers for, but, again, what we're trying to do is to identify all -- as Debbie said, all of the issues which could be important and listen to what the experts are telling us and to try to incorporate as much as possible.

DR. APOSTOLAKIS: The 14 by 14 matrix then protects you against double-counting. That's really what it does.

MR. ABRAMSON: Yes, that's right, I mean, assuming that things are -- inspector skill and welder skill, that's right, we're not doing it together, of course.

DR. APOSTOLAKIS: That's a clever idea.

MR. ABRAMSON: Yes.

DR. POWERS: I guess I didn't understand how you handle the correlation.

MR. ABRAMSON: What we do is where there is a significant correlation, we'll elicit different values from the experts for each of those. For example, they tell us that the difference between weldments and plate, so we'll do, all right, first for weldments, what are your values for this, then for plate, what are your values for this, and so on.

So when we do this initial discussion with the experts on the interactions with the 14 by 14 matrix, we make a note of what's important and, of course, we don't forget to come back to it and ask the experts say, yeah, this is really important, we'll come back and we'll just re-elicit it.

In effect, we're getting it conditional on what they say are the important values.

DR. POWERS: I mean, I understand that you might do plates and welds differently.

MR. ABRAMSON: Right.

DR. POWERS: But suppose you come back and you say, gee, inspection procedure and inspector skill are highly correlated. You use the worst possible procedure with the worst possible inspector. They combine.

MR. ABRAMSON: Right.

DR. POWERS: Whereas by the time you get down the best possible inspector, it's pretty much independent of procedure. He does a good job no matter what procedure is there.

MR. ABRAMSON: Right.

DR. POWERS: How do you recognize this?

MR. ABRAMSON: Well, we ask them about it. They tell us this. We'll say, all right, what would it be for this particular kind of -- assume, say, you've got a good inspector and we're dealing with -- what was the characteristic you were dealing with, with the procedure, say, so say you've got a good inspector and you have a procedure.

By the way, I should emphasize one thing which we tell the experts right away going in. What we are interested in is the flaw distribution as -- a pressure vessel, as installed and ready to operate. This is after it's gone through all the pre-service inspection. So this isn't the flaw distribution that may have existed and then was caught by inspectors and so on and so forth. So then the question with inspector skill has to do with, well, are there some things which might have escaped the inspector because there weren't the skills.

DR. POWERS: You're going to clip this distribution somehow?

MR. ABRAMSON: You mean truncate it?

DR. POWERS: Yes, because you're going to say certain kinds of flaws get caught.

MR. ABRAMSON: Yes, absolutely. Absolutely.

DR. POWERS: And you're going to get some assessment of the inspector's skill and that's going to cause you -- for poor inspectors, you will clip less than you will for good inspectors, and some procedures are better than others.

What I'm asking is how do you decide when you've got correlation between them? That is, you have a bad inspector and a bad procedure. Does that -- how does that change where you clip this distribution, truncate the distribution?

MR. ABRAMSON: Well, let's say, all right, well, you see, we would have to -- in order to be able to actually apply this information about the quality of the inspections, we would have to know for a particular pressure vessel whether the inspector was good or bad.

DR. POWERS: We don't know that.

MR. ABRAMSON: We don't know, so we've got a random sample of inspectors. So I think a way we would handle that, and I mentioned it previously, is to increase the variability and increase the uncertainty on what the distribution is, because we don't know whether the inspector was good, bad or indifferent. However, we do know that depending upon his skill, you might have a different distribution.

Well, the way to handle that would be you'd have to have an uncertainty bound range of some sort on the distribution.

DR. POWERS: I can see how you'd handle the individual. Now what I'm asking is you've got both, you've got to account for both the inspector and the procedure that was adopted.

MR. ABRAMSON: I think we would know the procedure from the records.

DR. POWERS: Go back to the records.

MR. ABRAMSON: Go back to the records when you try to do that.

DR. POWERS: And if it turned out, lo and behold, that you used the worst possible procedure you could, the worst one you've ever heard of, you've already corrected the distribution for the fact that you know that the inspectors are of a random sample, some of them were bad and some of them were good, whatnot. Now, what do you do with the procedure? Is it just completely independent of the inspector or do you add another fudge factor on top of it or do you say no, bad inspectors, I've already added enough fudge factor, I'll add no more, but for the good one, I haven't added enough, so I have to add some.

MR. ABRAMSON: I think it will have to be a matter of our judgment based on what the experts are telling us how to interpret this. That's the best I can tell you. Each one, in effect, each distribution is going to be custom made.

DR. KRESS: You would have to ask the experts, if I had a high-high or a high-medium or a high-low, you would have six different things, you would have to ask them what factor goes in to those. I don't see any other way you do it. Wouldn't you have to -- you would have to have them define the correlation for you.

DR. POWERS: You're going to have to know. It could well be that good inspectors are doing a fantastic job and it doesn't matter what procedure you use.

DR. KRESS: Absolutely.

DR. POWERS: And then bad inspectors do a bad job, but it's a little bit better with a good procedure, but not a lost worse with a really bad procedure. You've got to know that information, somebody has got to tell you that.

DR. KRESS: And then they have to extrapolate this to suppose you have a three-way correlation. You've got a three-dimensional matrix you have to deal with.

DR. APOSTOLAKIS: The uncertainty is in the result. That's probably overkill.

DR. POWERS: I don't know that it's overkill, George. The problem is if you just go through and do it randomly, you are going to put a tail on this distribution, that when you're talking about things at six-times-ten-to-the-minus-fifth amounts to a bunch. But because it's correlated, you shouldn't have that tail.

It's the classic problem of dealing with the tails of distributions, correlations count out there.

DR. APOSTOLAKIS: Sure.

DR. POWERS: They don't affect the means very much at all, but they sure affect those tails

MR. ABRAMSON: Recognizing that, is that -- that's why we emphasize these interactions when we're going to try to -- not to double count or triple count or whatever we're going to do, we recognize that.

DR. POWERS: Good.

MR. ABRAMSON: If there are no more questions, I Debbie has a few final remarks to make.

DR. POWERS: It gets up to about 16,000 different ways that you have to handle things.

DR. KRESS: Yes, I think so. That's asking a little too much of the experts.

DR. POWERS: We've got really good experts. They all come from Oak Ridge. They're great experts. We don't want any of the Argonne guys coming to the expert elicitation.

MS. JACKSON: These are from the discussion, you've gone through these. One point I want to make in terms of the inspection procedure, the inspection procedure is a final inspection procedure after the vessel is fully assembled, because the welding procedures themselves have individual inspection procedures for different points.

So the inspection procedure that's listed in the list of characteristics is the final inspection procedure.

So I'll just go to the --

DR. POWERS: This is after the cladding?

MS. JACKSON: Yes, after the cladding. After it's ready to be --

DR. POWERS: Then we can throw that one away.

MS. JACKSON: So these are just some concluding remarks that we've put together so far. The expert elicitation process is complex, as well as the expert judgment process, and we want to identify some significant issues in the development of flaw distribution. We want to address the combination of the relative effects of the characteristics in the PVRUF distribution and that the flaw distribution may vary by vessel fabricator.

Are there any other questions?

DR. SHACK: We'll know the answer by June.

MS. JACKSON: Yes.

DR. APOSTOLAKIS: Are we writing a letter this time?

DR. POWERS: Can they ask 16,000 questions by

June?

MS. JACKSON: I'd like to get the title of that NUREG that you mentioned, that you mentioned before, the title of that NUREG.

DR. APOSTOLAKIS: Abramson knows. The Shack report, she would like to have it.

MS. JACKSON: Are you familiar with that?

MR. ABRAMSON: Yes, I've got it.

DR. SHACK: What we'd like to propose is to come back into session at quarter to one, since we're likely to be a little pressed for time this afternoon.

[Whereupon, at 12:02 p.m., the meeting was recessed, to reconvene at 2:45 p.m., this same day.]

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

[12:45 p.m.]

DR. SHACK: I'd like to come back into session and I guess we're going to have Mark Cunningham who is going to give us the big picture.

MR. CUNNINGHAM: My nickel?

DR. SHACK: Your nickel.

MR. CUNNINGHAM: Good afternoon. My name is Mark Cunningham. I'm in the PRA Branch in the Office of Nuclear Regulatory Research.

I'm here this afternoon to give you kind of an overview of where we're at and where we may be going in terms of re-looking at the acceptance criterion that's established for the PTS rule.

Basically, just as an overview, we have a deadline in May of this year to provide a Commission paper describing what changes or recommending potential changes the acceptance criteria that are used in the PTS rule or a recommendation maybe to leave it the way it is or whatever.

We wanted to take on this issue early on, because if the policy decision took us in a certain direction, we wanted to know that early enough in the process so that we could adjust the rest of the program to accommodate it.

So basically what we'll have is that what I'm going to do today is walk you through a number of items that will be in that Commission paper or kind of the structure of the Commission paper, talk about the acceptance criterion itself as it currently is, talk about two issues of things that have arisen since 1983 or whatever when the rule was established in terms of guidance on use of PRA, and then information on severe accident phenomenology, and then talk about, at least introduce some potential revisions or ways that we could change the acceptance criterion, talk a little bit then about how we plan to finish up the paper over the next couple of months, including coming back to the committee perhaps in late April or May or something like that.

At this point, we're not looking for a letter or anything, but we may at the -- in the May timeframe.

You probably heard a great deal about this the last couple of days, but the rule was established in 1983 as an adequate protection rule, on contrast to some of the other rules that we'll talk about later, like the station blackout rule that were cost-beneficial safety enhancements. So it was developed under different provisions of the backfit rule.

The rule itself established an embrittlement screening criterion that licensees had to evaluate their plants against to determine whether or not they had adequate safety margins in their vessel.

The acceptance criterion is in the form of a frequency of a through wall crack. Basically, if you could demonstrate that the frequency of that through wall crack was less than five-times-ten-to-the-minus-six per year, then you could continue to operate that plant.

If you went above that, then you had to demonstrate that, through additional analyses or changes to the vessel design or changes to how you're operating the plant, to reduce the frequency down to acceptable level.

There's a couple of key underlying assumptions in that five-times-ten-to-the-minus-six. Basically, you may have heard about this today, but it's a five-times-ten-to-the-minus-six of basically having a certain no ductility temperature or whatever you call it, the RTNDT or RTPTS.

From a risk standpoint, there's a couple of key aspects to it. One is that if you talk about a through wall crack, we made the presumption that the through wall cracks equivalent to a large opening in the vessel and it's equivalent to core damage, that you're not going to have a capability once you start one of these through wall cracks in a PTS accident to mitigate it in erms of preventing core damage.

When the rule was established, there was an argument made that the containment performance was not particularly an issue in these accidents.

DR. KRESS: Is that assumption going to be revisited there?

MR. CUNNINGHAM: Yes. I'll come back to that, but that's one of the things that we need to think about. The argument at the time was that the types of accidents that get you into a PTS are accidents where there is a great deal of water around, that you're over-pressurizing or over-cooling the vessel. So you've got a lot of water in the core, in the vessel.

You also have availability and presumably operability of containment sprays. So the effects of that was even if you opened up the vessel and weren't able to cool the core, that you're not threatening the containment itself, and depending on where we go in some of the discussions of how we might re-look at the rule, what the acceptance criterion that may or may not be an issue, but we'll come back to that or I'll come back to that.

There's at least four key pieces of Commission guidance that have been established since the rule was established in the early '80s. You're well familiar with these. We've got the safety goal policy statement. We established two other rules that are similar in some respects, the station blackout rule and ATWS rule dealing with accidents that were identified in PRAs as being very important to risk or core damage frequency at least.

The backfit rule became a little more codified and well established and we -- in these timeframes and the regulatory analysis guidelines that went with the backfit rule that introduced risk information into the backfit rule process in a particular way was also established.

Then just in the last couple of years, we've come up with Reg Guide 1.174. So I'm going to talk about each of these in a little more detail. As you know, the safety goal policy statement defined qualitative and quantitative goals for acceptable risk. That was in the 1986 statement.

Later on, in 1990, the Commission approved having a ten-to-the-minus-four subsidiary core damage frequency goal. That has an impact on defining what's an acceptable overall core damage frequency and then that starts to impact decisions on what could be an acceptable frequency of particular initiators, and as we'll get to in a little bit, it kind of reflects our thinking in the station blackout rules and the ATWS rules in terms of what was an acceptable frequency of having core damage accidents from those initiators.

Again, it was intended for generic decisions using industry average information, I think. So in one respect, it's very relevant to the PTS rule in the sense that this is a rule that -- it's a generic rule and that sort of thing.

So let's come back to some of the options that deal with do we have the potential for using -- how do we use the safety goal information in re-thinking the acceptance criterion.

In the late '80s, we had two new rules established, as I said, with the station blackout and the ATWS rules were established as cost-beneficial safety enhancements. So the staff had to argue why the benefit of achieving these rules and what core damage frequency or risk reduction we achieved was wroth the cost of implementation.

In both cases, there was a goal established of ten-to-the-minus-five per reactor year. So in the sense, this starts to lay out and says that we want to have -- even if we have an overall core damage frequency goal of ten-to-the-minus-four, we don't want to have any particular initiator or group of accidents contributing more than about ten percent.

DR. KRESS: That's a real significant item.

MR. CUNNINGHAM: Yes, and it comes back and when we come back to some of the options, it kind of precludes, I think, some options that we might have in terms of how you would re-established or re-think the acceptable criterion for the PTS rule.

DR. APOSTOLAKIS: How are these groups of accidents defined?

MR. CUNNINGHAM: Not very precisely, unfortunately.

DR. APOSTOLAKIS: I mean, the LOCAs, how do you treat the LOCAs? As a group or small LOCA and the medium LOCA?

MR. CUNNINGHAM: In this case, most of the station blackout issue was a transient-initiated. So it could be -- it was basically any transient that would get you into a situation of loss of off-site power and on-site power.

DR. APOSTOLAKIS: So it's specific for this.

MR. CUNNINGHAM: Yes, very specific for this, with --

DR. BONACA: Would the LOCA in design basis, you consider core damage?

MR. CUNNINGHAM: I'm sorry.

DR. BONACA: You can see the core damage also from a LOCA that meets design basis, which is a limited amount of fuel oxidation.

MR. CUNNINGHAM: In the context of these, those would not be station blackouts that would have to meet the goal of ten-to-the-minus-five.

DR. APOSTOLAKIS: But it is apportionment of risk to certain categories of accidents.

MR. CUNNINGHAM: Okay.

DR. APOSTOLAKIS: I'm trying to understand what you meant by core damage.

MR. CUNNINGHAM: Really core melt, if you will.

DR. APOSTOLAKIS: Core melt. Okay.

MR. CUNNINGHAM: Core melting.

DR. APOSTOLAKIS: If you add them all together, you get where you want to be. All right.

MR. CUNNINGHAM: Yes. And just to be clear, there is no Commission guidance that really says we're going to allocate ten percent, there is not that -- we had talked at one time ten or 15 years ago about the idea of reliability allocation or risk allocation, but it wasn't formally established for this. It was more general guidelines.

In fact, these rules were established a little before the Commission formally approved the ten-to-the-minus-four as an overall goal for acceptable frequency, but it was always in people's minds of having roughly those numbers, if you will.

The rules themselves, these two rules, were justified basically on an off-site risk analysis. So at this time and using -- when they were justified, you didn't -- there was no specific guidance on containment performance. So it was basically you've got this initiators and the final decision metric, if you will, was averted off-site population dose. So it was, to some degree, irrelevant what specific containment performance -- how containment performed in these accidents.

It could have been good or bad or whatever. It was kind of -- the analysis was indifferent to that.

Then came up with the backfit rule and the regulatory analysis guidelines. It has two parts to it, one of which -- the first part is an initial screening on potential reductions in CDF and conditional probability of early containment failure. So at this point, we introduced containment performance as a particular issue into the backfit rule process.

One of the things we'll talk about a little bit later is the idea of using the same type of information in a reverse sort of way to justify potential increases. This is focusing on what is the potential benefit of a proposed change in terms of a reduction in core damage frequency and a reduction in -- and an analysis and evaluation of containment performance.

So if a proposed change did not gain you much in terms of core damage frequency, then very often they were just excluded and said you can't pursue the backfit with those. If they passed that test and said, yeah, it might have this substantial benefit, then you went on to look at the off-site risk averted associated with the accident, but this is the place where the backfit rule and the safety goals started to come together in terms of using the safety goals to define that initial screening.

Last, but not least, of course, is Reg Guide 1.174. It goes off and it has a little bit different flavor to it. One is that it introduces a set of general principals, as you know. We discussed them for many, many times here. But the five principals that we talk about in Reg Guide 1.174 are not explicitly laid out in some of this other earlier guidance, like the backfit rule.

So when we come back to it, it has some advantages in terms of how we would use -- might use some of this guidance to look at the PTS rule. It introduces probabilistic guidelines in terms of CDF goals and delta CDF and LERF, so, again, it's a little different than what was in the reg guide analysis guidelines.

It was conditional probabilities of containment performance. Again, I think we're basically consistent in terms of the numerics of it to show how changes in risk, in this case, going up, might be consistent with the backfit rule, which is intended to look at changes in the risk going down.

As you may recall, when we talked about 1.174, one of the goals was that we would allow increases in core damage frequency, fairly small increases in core damage frequency. One of the goals was that we don't, on the one hand, allow core damage frequency to go up to a magnitude where if we applied the backfit rule, we'd take them back to where they were to begin with. So we wanted to avoid that situation.

So that's some of the more recent guidance type information. The other part of it is more recent work that's been going on in accident phenomenology. As I said, the rule itself was -- at the time of the rule, the staff opinion or judgment was that there was not a strong correlation between having a PTS event and containment performance, that you were likely to keep the containment in place.

Needless to say, in the last 15 years, there's been a lot of work going on in trying to better understand severe accident phenomena, not the least of which is described, if you will, in 1150, and then a lot of work that's been done since 1150 in trying to understand the impacts of direct containment heating. There's probably a lot of other things.

So part of what we're going to have to address is depending on how we go on establishing the -- re-thinking the acceptable criterion, we may have to bring -- re-think the issue of containment performance. The question is, is there anything that we have not learned in the last 15 years that would run counter to what we decided 15 years ago, that the containment performance was not much of an issue.

We've got -- the issues I've got at the bottom of the slide here, we're going to think about what about the dynamic loadings on the core and in the internals and the vessel and the piping. Can you --

DR. KRESS: Is this the rocket ship?

MR. CUNNINGHAM: The rocket is part of it, but it's also a question of tilting and that sort of thing, just general motions of the vessel that -- one possibility is that that can pull penetrations, that you move the piping enough that you pull a penetration out.

DR. KRESS: Fail containment.

MR. CUNNINGHAM: Fail containment and then you've got to decide is that a large -- could you have a large release under those circumstances. Combined with some of these other things.

DR. KRESS: Is it implicit in there the thinking that at the bottom of the vessel, that you have no way to get a lot of ECCS through the core? So that what you have is a passageway for natural convection for air and you may have air combustion to the team, which changes your hydrogen thinking and your energy thinking and what goes into containment. Is that part of this?

MR. CUNNINGHAM: I hadn't thought about that, but yes, that belongs.

DR. KRESS: It's part of the thinking.

MR. CUNNINGHAM: Yes, that's right. That's a good point. So the dynamics aspects at the time of the PTS event. You're going to have some pressure loadings at that point from the steam escaping and that sort of thing, but again, it's a little different in the sense that you're -- the reason you're breaking this vessel is because you've got a lot of water inside.

So that's a little different scenario.

DR. KRESS: When we use a large break LOCA, we have this low-down calculation to get the loads, steam going in. If you just suddenly break off the bottom --

MR. CUNNINGHAM: Yes.

DR. KRESS: -- of the primary vessel, I don't know how you would redo the choke flow equation. You're going to get a definition loading.

MR. CUNNINGHAM: That's right.

DR. KRESS: Versus timing.

MR. CUNNINGHAM: That's right and it would be different, too, if you were to take the bottom head off or having one of the axial welds go.

DR. KRESS: Yes.

MR. CUNNINGHAM: And open up that way. That's right. So related to that is the -- are the loadings such that you might tend to disperse the core. One possibility is -- especially with a core that's kind of old, you might be breaking it apart and things like that and what impacts does that have. You're doing this before you would melt, before you would expose it to air or anything like that.

So you have those sorts of things, and then you come back to the question of what's the availability of your containment sprays and things. This is not a scenario where --

DR. KRESS: In the risk basis, you generally have to assume some frequency or probability that they will be failed.

MR. CUNNINGHAM: Yes, that's correct, but it's different in character than, say, a station blackout, where conditional probability of containment ESF failure is essentially one. Here you've probably got them operational and that is going to impact the phenomenology somehow.

DR. KRESS: The failure probability.

MR. CUNNINGHAM: Yes. That's right, if it's one percent or something like that. You've got to bring all these things together in some sort of way to sort out what is -- how close -- what's our real estimation of the containment performance and is it really any different than what we thought about 15 years ago.

So we're trying to bring those two sets of new information together into several potential revisions, if you will.

One potential re-thinking of the acceptance criteria is to focus more on the core damage frequency, and that, in a sense, what we're talking about is bringing the PTS rule into line with the blackout and the ATWS rules. I'll come back to that in a minute.

Others are more focused, bring in the concept of containment performance, as well. So they're a little more modern in terms of our thinking about how you understand accidents. One I have kind of alluded to earlier is you might develop some sort of a reverse backfit process.

The second is you basically work from the Reg Guide 1.174 guidelines, which are really oriented towards changes, burden reduction changes, if you will, associated with license amendments. Now, in effect, you're going to apply that same set of principals and guidelines to a rule change. So it has that difference in flavor, but it has the same general concepts underlying it.

I am going to talk about all of those potential revisions a little bit. And one idea is that you could apply the goals for the ATWS rule and the station blackout rule.

So one possibility is that you deal with and say that the acceptable frequency in PTS is ten-to-the-minus-five. So it's a little bit of a relaxation of where we are today.

You would justify, if you will, and looked at the rule in terms of off-site consequence risk instead of containment performance, because that was the basis for justifying the rules to the SBO and ATWS rules to begin with.

So in one hand, it does establish some consistency among these three rules. It would allow some increase, but it doesn't introduce any particular -- no explicit consideration of containment performance into it, and so, in a sense, it's a little dated relative to our policies of today.

So another option is to develop a reverse backfit process, if you will. What we mean is basically you take the reg analysis guidelines, which are used to justify potential reductions in core damage frequency, and turn it around and say, well, how can I develop some sort of mirror to that which would allow me to justify increases in core damage frequency.

DR. KRESS: Is it one over 2000?

MR. CUNNINGHAM: Something like one over 2000 or some such thing. So you would have to do some sort of cost-benefit analysis to say how much can we agree to allow this to increase. There are several issues associated with that, problems with that. One is that this is an adequate protection rule.

So you're exploring very --

DR. KRESS: It's apples and oranges.

MR. CUNNINGHAM: That's right, and how you would turn that into fruit salad or whatever is a little unclear at this point as to what you would do in those areas.

So clearly there is a policy implication and there's a lot of work that has to be done to sort that all out.

Another approach then is to basically take the principals from 1.174, which, again, were designed for license amendment, changes, and apply it to a rule change. It has the advantage that it ensures consistency, what we think is the right -- is the most current, anyway, and the best way of thinking about using -- making risk-informed decisions.

DR. KRESS: How do you go from a backfit -- 1.174 was supposed to be tied to specific individual plants. You now go to a rule which is supposed to cover all the population. Do you divide those things by a hundred, those CDFs and LERF?

MR. CUNNINGHAM: That's a good question. I think what will happen is that the rule -- the application of the rule is going to be a plant-specific basis. There are only going to be a few plants --

DR. KRESS: You may just treat it with --

MR. CUNNINGHAM: Yes. And that's the way --

DR. KRESS: You're right, it would be plant-specific.

MR. CUNNINGHAM: You set up the rule in some sort of generic way, but it has to be applied on a plant-specific basis. In reality, that's the way it's happening today with the present rule, is that each plant has to evaluate their vulnerability to the PTS and you'd have to have the same thing here.

This has implications. If you're starting now with a goal of five-times-ten-to-the-minus-six, the Reg Guide 1.174 process would basically say you're probably not going to let it get any bigger, much bigger than five-times-ten-to-the-minus-six, but you bring in the LERG consideration and if LERF is -- if containment performance is not an issue, then you can end up with something like five-times-ten-to-the-minus-six.

If containment performance is an issue, then you could -- you may have to ratchet the five-times-ten-to-the-minus-six down a little bit to deal -- to make it more in line with our LERF criterion in 1.174.

So in this one, one of the disadvantages of going this way is that it introduces more explicitly the consideration of LERF and that means we've got to nail down some of these phenomenological issues a little bit better than where we were, than where we are today.

So that kind of gives you an idea of where we are on this paper right now. What we're doing is developing a Commission paper. We'll be trying to have a draft the end of this month that's basically going to look a lot like what you've just seen here, with -- we want to go through and say what was the basis for the original acceptance criterion, what have we learned since then in terms of the Commission guidance on PRA, and on accident phenomenology, look at some potions for potential revisions, including this issue of containment performance, and the one thing that would -- the paper would have is it would have a recommendation on where -- how to go on this.

What we would like to do is get the paper to you sometime in the next month probably, with the idea -- let me back up. We owe it to the Commission in early May. We think some of these issues would be worthwhile talking to the committee about. So maybe in late April or early May, we would get the draft paper to you, or I guess it would have to be late -- sometime mid to late April.

DR. KRESS: Sounds like a joint PRA and Severe Accident subcommittee meeting.

MR. CUNNINGHAM: So that would be the idea.

DR. BONACA: Would you run something like this through the generic issue program?

MR. CUNNINGHAM: I'm sorry?

DR. BONACA: Would you run something like this through the generic issue program? This is a situation where you have -- I mean --

MR. CUNNINGHAM: If the issue of PTS came up today as a new issue and not be -- have a rule already and that sort of thing, then you would -- one way to deal with it would be to put it through the generic issue process and say what's the value of pursuing a rule or some other regulatory mechanism to deal with this.

DR. BONACA: You have a burden reduction issue here, to some degree.

MR. CUNNINGHAM: It's a burden reduction issue, yes, that's right. So the generic issue process is, strictly speaking, not applicable here because we've got an existing rule and we're talking about modifying it, because we have a different set of processes for changing rules like that.

The flavor of this one is a little different because the rule itself started out as being probabilistic, basically. So we have to re-think some of those aspects of it, as well.

DR. APOSTOLAKIS: So you're proposing to have another subcommittee meeting to discuss this or bring it back before the committee?

DR. SHACK: We would have to have a full committee meeting to write a letter.

DR. APOSTOLAKIS: Sure.

MR. CUNNINGHAM: Yes.

DR. KRESS: It's the sort of thing you might be able to put it before the full committee. That is all we're talking about.

MR. CUNNINGHAM: This is basically all we're talking about and the key element --

DR. KRESS: We didn't have all the other parts of the PTS in there, we're just talking about this right here.

MR. CUNNINGHAM: Yes. I think -- and we wouldn't -- in the March-April paper, we wouldn't be proposing to resolve the issues on the phenomenology. We just kind of acknowledge them and say they have to be worked. The principal difference between what we've seen here and the paper would be some sort of recommendation on what's the right fit of PRA guidance, if you will, for this and you may have gotten some sense of where I'm coming from anyway on this.

So it may be that a full committee meeting is all that's needed.

DR. KRESS: That's a meaty issue, allocation of risk among sequences.

DR. APOSTOLAKIS: The problem with a full committee meeting is if we don't like it.

DR. KRESS: It might be better to --

DR. APOSTOLAKIS: It might be better to have --

DR. KRESS: -- subcommittee and a full committee.

DR. BONACA: I think so, too.

DR. APOSTOLAKIS: Yes, because --

DR. BONACA: One of the potential revisions you mentioned is driven by consistency with -- among the three principal risk-informed rules. This particular case, you really have lost a vessel. You still have an ability of cooling it through, I guess, injecting into the vessel and draining and then -- or through the spray system.

MR. CUNNINGHAM: Yes.

DR. BONACA: How different is this kind of scenario from what you had for the station blackout and ATWS rules? In those cases, we have some fraction of scenarios where you end up with a failed vessel, but others you don't and you're able to cool long term. I just don't see this as a -- I mean, if this is driven by consistency, I would say I don't care about consistency there.

I have a situation here where I have to rely on containment. So it seems to me that that would be driving some. I guess this is all preliminary, so you don't have any thoughts.

MR. CUNNINGHAM: The value of the consistency is if somebody is looking out -- if somebody is looking in from the outside to try to understand, well, what are you really talking about in terms of trying to have acceptable core damage frequency from your major rules, there is an advantage to having them all kind of line up.

There are disadvantages. The nature of this rule is different and I think part of the reason that the present acceptance criterion is more restrictive than that for the ATWS rule and the station blackout rule is the recognition of the different character of this accident. Again, right off the bat, you've compromised one of your barriers, but you also seem to have -- at least relative to a blackout rule, you have perhaps more confidence in the containment performance than you would have had.

So it is a different beast. So I guess I would be surprised if we go the route of saying, well, just for the purpose of consistency, we're going to set up the rule to be like the blackout and ATWS rules.

DR. BONACA: One other question I had was it seems the main consequence of applying these new insights to -- it's really license renewal, allows a vessel to probably be operable for a much longer period of time. By much, I mean some longer period of time, but the question then becomes are there other effects that are not really within just the rule that now come together to -- I haven't thought about this enough, but I'm saying that as you age these plants and you allow the vessel to continue to be operable for a long period of time, doesn't it open up other issues, other questions regarding --

MR. CUNNINGHAM: I'm not sure offhand whether that comes up or not. I haven't thought much about that aspect of it.

DR. BONACA: I haven't either, but I just --

DR. KRESS: Another thought on your consistency question. You talk about, say, the one-times-ten-to-the-minus-five versus the five-times-ten-to-the-minus-six. Both of those, I presume it is some sort of representation of a mean value.

MR. CUNNINGHAM: Yes.

DR. KRESS: The ATWS rule -- the ATWS sequence has certain sequence-specific uncertainty associated with it. That's a lot different in the uncertainty associated with -- and that ought to fit into the system somewhere.

MR. CUNNINGHAM: That's right.

DR. KRESS: And that either means you lower the mean value you're dealing with or you put some sort of confidence level on it that's different than just the mean.

MR. CUNNINGHAM: Yes.

DR. KRESS: So somehow I wanted to get across that that thinking needs to be into this acceptance criterion.

The sequence-specific uncertainties are different and should be accounted for when you go to this acceptance criterion some way.

DR. BONACA: Especially, and I completely agree with you, Tom, especially in the case where you have burden reduction. And so that becomes a very important issue to understand what this ten-to-the-minus-five means.

*Mr. Foley. And the five-times-ten-to-the-minus-six, maybe this has been gone through in the last couple of days somehow, but there is a -- one of the things the paper needs to do is explain the -- what's the -- it's five-times-ten-to-the-minus-six of what and that's a through wall crack frequency, but it's also tied to a particular RTPTS or RTNDT and that value was set based on some conservative assessments of what was really going to happen and that sort of thing, and all of that needs to be laid out a little more carefully in the paper and, in a sense, re-thought of how we would do -- how we would address the uncertainties in the acceptance criterion as we go forward.

So it's another piece that belongs in this paper.

DR. BONACA: And also just one last comment. We talked about rigor this morning.

MR. CUNNINGHAM: I'm sorry?

DR. BONACA: WE talked about rigor in the calculations. I think that because of what's happening here, I mean, rigor is not any more a desirable thing and is an expectation. We understand how this is derived and there is rigor.

MR. CUNNINGHAM: Okay. If there's nothing else on that.

DR. SHACK: Comments from the committee? Perhaps we can then start with Nathan's presentation.

MR. CUNNINGHAM: Yes. We can move into a discussion of how we're going to do some of the PRA calculations that assess the performance of the plants.

DR. KRESS: I did want to say I think it's crucial that you look very carefully at this question and whether changes to containment failure probability impacts it.

MR. CUNNINGHAM: Yes. Okay. I'm going to stay here. We've got three other folks who are going to join me and do most of the work. Nathan Siu and Roy Woods from PRA staff in the Office of Research and then Bill Galyean, who is a contractor to us from Idaho National Engineering and Environmental Laboratory.

MR. WOODS: As mark said, I'm Roy Woods. I'm from Mark's branch, he's my branch chief, the Probabilistic Risk Analysis Branch in our Office of Research.

With me at the table is Nathan Siu, on the far side there, who is senior technical advisor in the PRA and human reliability analysis parts of this PTS effort. Nathan is also one of the driving forces behind the uncertainty analysis for the entire PTS effort, including the thermal hydraulics and the probabilistic fracture mechanics and the PRA and HRA.

DR. POWERS: I can't help but say it's better to have him back working on the fire risk assessment.

MR. WOODS: I'm pointed out he has several hats and I've mentioned three or four of them right there.

DR. POWERS: He's got an important hat on most of the time.

MR. WOODS: And I think Ali Mosleh, Professor Mosleh, from University of Maryland, Materials and Nuclear Engineering Department is here, back there somewhere. He is heavily involved in the uncertainty analysis, also.

Also with me here is Bill Galyean from Idaho National Engineering and Environmental Laboratory. He is Research's contractor for the PRA and the PRA now includes HRA. He doesn't have those contractors, but they're working very closely together, as I will get to in a minute here.

Anyway, that's the work that his doing for us.

The objective of the PRA part of this, of the whole project actually, is to support development of a technical basis for revised pressurized thermal shock rule. In doing that, we want to ensure that the overall process is coherent and risk-informed and that there is a good integration of the different aspects.

As I pointed out, I'm the leader of the PRA team which now includes HRA. That, of course, identifies the sequences and various errors that you would be worried about and failures that you would be worried about.

That determines the sequences that we need to do, the thermal hydraulics analyses for which I think David Bessette talked about. He's the leader of that team. And then the output of the thermal hydraulics analyses tells you the input conditions for the probabilistic fracture mechanics, which Shah Malik is the head of that team. So those are basically the three teams.

Throughout all of these efforts, we are doing a unified effort to take into account the uncertainties and we are dividing them into aliatory and epistemic uncertainties, which George wants, and it's a very good idea. That's what we are trying to do here.

All of this is in support of the development of a screening criteria which will probably be very much like the type of screening criteria we have now at least, which is based on the reference temperature for the nil ductility transition, which is an embrittlement parameter, really.

In developing this, we will be looking at trying to relate whatever criteria we have to risk figures of merit; that is, through wall crack frequency or one of the others that Mark referred to a few minutes ago.

Right now we are aiming it mostly toward through wall crack frequency, which we are hoping to be able to equate to a core damage and if that comes out acceptably, then what might go after that wouldn't make any difference in the conclusion, then we can stop there. That's where we are kind of hoping we will at moment.

Also, as I mentioned, we are definitely doing treatment of uncertainty, which will be related to the qualitative issues; in other words, where you have a great uncertainty is where you might want to maintain your defense-in-depth to attempt to compensate for the uncertainty that you have.

The way we're approaching this while thing is to update the early 1980 PRA studies that we did. Those were for Oconee, Calvert Cliffs, and H.B. Robinson. What we are doing in updating these studies is reflecting changes to the operation of the plant and changes to the hardware of the plant. For example, emergency operating procedures have changed a great deal since the early '80s.

They are now symptom oriented instead of event oriented. An example of the changes to the plants themselves, we are currently working on Oconee and they've made significant changes to their integrated control system.

So we have to take those changes into account.

Those are just examples. We're looking at the whole plant.

We also are reflecting changes to the PRA state-of-the-art and the example I would use there is HRA, human reliability analysis. We're using basically the ATHENA team in this effort and the ATHENA team is meeting with the PRA people. They are indistinguishable now, in my mind. We sit down and we meet together and we talk about what sequences are going to be modeled and what's going to be in the sequences, both hardware and people oriented things in those sequences.

DR. POWERS: What is it that you are looking for to get from ATHENA that you wouldn't get from something like THERP?

MR. WOODS: One of the things is errors of commission, plainly. What might the operator -- when might the operator be misled and think that he should do one thing, when actually that's not what he should do in the particular situation. He thinks he's in one place, but he's actually in another place and he takes the right action for where he thinks he is, but it's the wrong action for it, that type of thing.

That can be very important. It can be a significant contributor to the risk and that's not in there now and we're trying to put that in there.

MR. SIU: The other thing I think they can say is that we're going to have a more causally beast description of why the error occurs, whether it's an omission or a commission error, and it's going to reflect what's happening during the sequence.

That's something that you can include in the THERP analysis, but it's not tied in quite as explicitly, I would say, as in what we're going to be doing.

MR. WOODS: And on the other side of the coin, also, they're better able to look at recovery actions.

DR. POWERS: Both those things that you mentioned there, the causality and the recovery, aren't those going to get terribly plant-specific?

MR. WOODS: Yes. As are some of the other issues, some of the hardware issues. We're finding -- in fact, I'll get to that in a minute, where we talk about wrapping their arms around the total population of plants from basically four analyses.

That's a difficult issue because all of these things are -- I mean, it's not unexpected, but it's turning out the more we look at it, the more we realize how plant-specific they are. That is a problem.

In fact, when I get to that, if you guys have any good ideas on how to handle that, that's one place we'd really appreciate input.

DR. POWERS: It raises the issue of how representative are the plants that are being run through this thing. How big of a sample set does it take. Have you wrestled with that issue?

MR. WOODS: That's exactly the next point at the bottom of this slide, address other plants. Let me get to that now.

What we need to do is make sure that within the scope of the analyses we do, we somehow include all plants that have a significant PTS risk at the end of their license, and we need to do this in a defendable manner. We want to -- I guess what I'm trying to say is we end up with four analyses and we might find that some plant that's not among those four has a higher safety injection pressure or safety injection flow capability or something.

So we need to somehow take that into account. Now, this is assuming that that high capability exists in the plant where there will be a significant embrittlement at the end of the license. If there isn't, then for this purpose, it's not of concern.

If you find such a plant, then we would have to somehow also, in all fairness, take a look and see if there is some other feature of that plant that might tend to counter that. Maybe they have better whatever capability somewhere else and take all that into account, but we have to somehow do that without doing a full-blown PRA, because we don't have the budget or the time to do a PRA for each and every plant.

We're struggling with that. If there are any constructive ideas, we'd welcome them.

DR. POWERS: They're mostly desperation ideas. I can see how you can screen out based on embrittlement, there are data that you could go to. You might even be able to screen that out in the hardware because you can certainly look at the FSAR.

But if indeed errors of commission are important, screening based on procedures is a very tough thing to do, because you have to read the procedures.

MR. WOODS: Right.

DR. POWERS: You have to get them, and that's an enormous task.

MR. WOODS: That's exactly what we're in the process of doing at Oconee right now. We were down there -- these three, Mark wasn't with us, but we were there yesterday and the day before talking in some detail, well, great detail actually, with everybody we wanted to talk to at Oconee. They were cooperating quite well with us.

But the more we got into it, the more we realized, hey, they have certain procedures, they approach these problems in a certain way, and you can't assume that someone else will. It's different and we're struggling with how to handle that. You've hit on a very significant problem we're facing.

MR. SIU: If I could add, Roy. I think there are two parts of this screening which Mark pointed out in the previous presentation. One is this initial screening criteria, which is based on embrittlement, and what we need to do is to be able to pick the embrittlement screening criteria that gives us confidence that if the plant passes that, there's just no problem, period.

Once you get past that point, then there will be a plant-specific analysis that will demonstrate that the particular risk criteria are satisfied. So at that point, I imagine that's where your procedure issues are going to come in and that's not something that we're going to perform.

Our main task is to set the embrittlement criterion appropriately and to set the right level for the second step.

MR. GALYEAN: Also, if I could add. We are engaged in an effort right now to try and categorize plant to plant differences that we feel are relevant to the PTS issue, things like turbine bypass capacity, high pressure injection capacity.

DR. POWERS: Things you can read about the plant.

MR. GALYEAN: Right. And our expectation is that -- and, in fact, it is in the program plan that towards the end, we are going to do sensitivity studies on the PRA models to quantify what impact these plant to plant differences could have on at least the frequency of these PTS sequences.

DR. POWERS: Do you want to give me the risk achievement worth of the operator? Nobody wants to do that for me.

MR. WOODS: That leads right into this. We've already covered a good deal of this slide, but basically we're trying to calculate through all correct frequencies from four plants, including uncertainties, that we're doing PTS and PRA models for Oconee and Beaver Valley. The NRC is -- or Bill Galyean, at INEL, with our sponsorship, are developing those models.

Two other plants already include PTS sequences in their PRA models and that's Calvert Cliffs and Palisades, and we are planning on obtaining those models. Bill is putting them in the SAPPHIRE code, so we can manipulate it and change it and massage it and do sensitivity studies and that sort of thing and use all four of those.

And what we'll end up -- then there's a significant time at the end of this last point here, a significant time after we develop those things and use them, we realize we'll have four different models with four different sets of assumptions and we're going to have to somehow come to grips with how to put it all together in a coherent way.

But what we'll end up with is four or more points which each point from that graph behind me represents one plant and what you do is you evaluate that -- you evaluate that plant for its through wall crack frequency assuming that the material condition is at an RTNDT which is evaluated in a certain way as required by the PTS rule at the end of that plant's license.

And by definition, that RTNDT is RTPTS, that's just what we mean by that. And once we come up with an acceptable through wall crack frequency, based on safety goals or whatever, as Mark discussed, then that determines the through wall crack frequency star on the vertical axis and you could read across to some representation of those points that you have and determine what the correlated RTPTS is.

That would then be your screening limit. The problem is, as you pointed out, Dr. Powers, that you've got four points at most and you need to somehow come to grips with how to handle the other plants.

That's the point of this slide, really. I'm pressing the end here at this part.

Open questions, in addition to the ones that we've talked about, at the moment, we're not treating internal fires, floods, external events in these analyses. We realize that the resulting failures, for example, for internal fire that causes cables to burn and causes hot shorts and causes various equipment to fail, which might confuse the operator, this could involve the whole process, that could cause PTS events to initiate or it could make ones that have initiated for some other reason worse or both.

DR. POWERS: Maybe we should stop all this and just get into that fire problem right away.

MR. WOODS: Put Nathan's fire hat back on and keep it on. I understand.

DR. POWERS: Take the resources from this, devote them all to the risk assessment and validated models, sounds good to me.

MR. WOODS: So you have several hats. You probably understand why it's necessary to have several hats.

Anyway, we've already mentioned the problem with coming to grips with the relationship between through wall crack frequency. Well, maybe we haven't, but the problem is when you go beyond through wall crack frequency and you're trying to say that's not equal to core damage frequency, what you're looking at is something that's very, very uncertain and we're not sure that we can predict how big the hole is and whether or not the core would actually be damaged with enough certainty to actually take credit for it, and that's the problem with going beyond through wall crack frequency and just assuming that's equal to CDF.

Also, if you go from CDF to LERF, it's a similar uncertainty. So as I said, really all we're doing at the moment is we have a task in place to identify the various issues that would be involved if we had to or wanted to, for whatever reason, go beyond through wall crack frequency and we're sort of keeping track of those, but we aren't spending a lot of our resources on that at the moment.

DR. KRESS: On your previous slide, would you put it back up?

MR. WOODS: Certainly.

DR. KRESS: I had a question. You implied that the three points were three different plants.

MR. WOODS: That's correct, yes.

DR. KRESS: But the PTS, RTPTS is extrapolated out to the end of current life.

MR. WOODS: The RTPTS for each plant would be the RTPTS for that plant at its end of license, either extended license or license now, if it hasn't applied for an extension, or whatever problem you --

DR. KRESS: My point is there is a time involved in there and you have to extrapolate something about the fluences and so forth.

MR. WOODS: Yes.

DR. KRESS: Why can't you just continue that extrapolation and have more than one point per plant and define what this curve looks like for each plant? And isn't it like having more data points to fit this curve?

MR. WOODS: No, it's not.

DR. KRESS: It's not.

MR. SIU: Again, don't take the graph too seriously. This is just an example. One of the things we're showing, for example, is a monotype relationship between RTPTS and through wall crack frequency, and that may not exist just because of the system differences or the procedure definitions.

DR. KRESS: It would be monotonic for a plant.

MR. SIU: For a plant, that's right, and you could plot --

DR. KRESS: That's why I was suggesting it.

MR. SIU: That's right. You could do that, certainly. I think Mark Kirk had a comment.

MR. KIRK: The only thing I wanted to point out is that RTPTS is, by definition, fluence, it is at end of license fluence.

DR. KRESS: But maybe you could plot it versus effect of full power year or something.

MR. WOODS: I was going to turn this over to Bill Galyean now to give you some more details on the PRA and with the incorporated HRA model that we're developing.

DR. POWERS: Having convinced us that the problem is impossible.

MR. WOODS: That was not my intent.

MR. GALYEAN: I'm going to just -- I have these three slides that I'm going to talk about just to give you a feel for the general philosophy of the PRA analysis.

Afterwards, I will turn it over to Nathan and he will get into more details on the uncertainty and the integration aspects of the process.

As has been mentioned before, our intention and our approach is to build on the original PTS PRA analyses. We have the benefit of their results that we can allow us to more cleverly develop the PRA models and develop the accident sequence, the PTS accident sequences and evaluate the importance of the various initiating events.

Again, as has been mentioned, we intend to update these models in the analyses based on the current plant designs, operating procedures, operating practices, and also update on our current understanding of reliability for the various systems, components, and also, in particular, the initiating event frequencies.

So basically it's just an update both on the state-of-the-art of PRA and -- and when I say PRA, I also mean HRA. And also to update them based on the current designs and operations of the plants we're looking at.

DR. POWERS: I'm just curious. In setting up and deciding how you're going to update the PRAs and what not, you had some basis for deciding you were going to do these things, but you were going to leave out fire.

MR. GALYEAN: As was pointed out, external events is still an open issue, and so the decision to leave out fire has not yet been made. It's still being talked about. We're still trying to understand what the implications are. There was one event that occurred at Oconee, in fact, that did result in some over-cooling. When I say one event, I mean a fire in a switch gear.

And so we are certainly aware of that and aware of the potential, but as far as how significant a contributor external events are in comparison to all the other initiating events, that's still something we're wrestling with and still trying to decide what the -- whether it's worthwhile to pursue that.

Again, that decision has not yet been made.

DR. POWERS: Are we ever going to get the IPEEE insights document, report?

MR. CUNNINGHAM: Yes. The insights report is, on the present schedule, I believe we're supposed to have a draft this summer. That will not happen because we've had -- we want to develop the insights report after we got the reviews done and the reviews won't be done this summer, for a variety of reasons, some of which are resource limitations, some of which are related to fire issues that we're dealing with with a number of utilities.

So I believe that realistically it will be early next year -- late this year or early next year.

DR. KRESS: Couldn't you ask yourself whether any fire events will activate the ECCS and sort of estimate the effect on the frequency, initiating frequency?

MR. GALYEAN: Well, we do, in fact, the -- an obvious area where we can improve on the original is the initiating event frequency. We do have quite a bit of operating experience data that we have collected and analyzed through another program sponsored by the NRC and in there we do have a frequency of inadvertent SI actuation, for example. So theoretically, any contribution --

DR. KRESS: Due to fire.

MR. GALYEAN: Due to fire would be in there.

MR. SIU: I think it's fair to say that the tools and techniques that we have now can be applied with the same degree of certainty that we have with other core damage scenarios associated with fire.

The problem is in the data-gathering, because the concern actually with Oconee, this was non-safety switch gear that was affected and you're talking about affecting control systems on the balance of plant side. We don't trace those cables.

MR. GALYEAN: This slide is intended to be more illustrative of kind of the approach we're taking. It lists the initiating events that we're looking at. It compares the frequency from the original Oconee IPTS analysis and the -- to the frequency that we anticipate using in the current analysis.

The initiating event frequencies come from NUREG CR-5750 initiating event frequency report, came out recently. Also, in the last column, we just have some comments or observations that we've concluded based on our look at these various initiating events.

An obvious point of comparison are the top -- is the top event, the reactor trip, turbine trip event, where, in the original analysis, they assumed six events per year and the current industry performance is less than one a year.

Some of the others are not so different. But we are also looking at a number of initiating events that were not included in the original IPTS analysis. Also note that we are looking at both at power events and events that occur at essentially hot zero power, which because of the thermal hydraulics response of the plant, could be more severe than at power events.

The other obvious area for improvement over the original analysis is in the HRA portion, which we've already touched on. In the original analysis, it took a very conservative and a very crude type of approach toward quantifying human errors and we -- the state-of-the-art, I think, the current state-of-the-art will allow us to significantly improve over that application of -- that was done in the original.

In particular, and, again, as mentioned, we will be utilizing the ATHENA folks in the development of the human reliability analysis and they will be looking at, again, kind of a broader range of human interactions in the response to a PTS type of transient.

That pretty much concludes my prepared comments. If there are no questions on the PRA portion of this analysis, I will turn it over to Nathan and he can talk about the uncertainty and integration issues.

MR. SIU: Thanks. The issue of uncertainty has come up a number of times in discussion here, so we just wanted to talk briefly about what we're planning to do, what we are doing, and I guess I will start off by saying that a lot of this is discussed in the white paper, which I believe was distributed to the committee, and I know it's a lot to read there. But if you have any comments on it, by all means, we'd appreciate them.

I think one of the main points to raise is this framework diagram. It's kind of hard to see on the screen there, but, again, it's in the paper and it's in the handout. Basically, that shows how we go from the PRA event sequence analysis, which identifies sequences at a certain level of detail, such as you have an initiating event and subsequent successes and failures of your safety systems.

Obviously each PRA sequence can represent a bundle of thermal hydraulic sequences, actual realizations, because of, for example, different timings of events within the definition of the PRA sequence, CF sub-scenarios that have to be analyzed.

One of our problems, of course, is deciding which sub-scenarios to analyze to represent the PRA sequence.

Once we have identified those sequences and they have associated frequencies, then you pass them on to the probabilistic fracture mechanics analysis, which is basically all the material embedded in the FAVOR code. In fact, the FAVOR code takes a lot of this information and does the integration. So we're talking on a conceptual level rather than the level of what actually is going to be done.

And if you're interested in the mechanics, we can talk a bit about that a little bit later.

What I did want to point out here is that the PRA analysis does identify sequence frequencies. There will be sub-scenario frequencies associated with the thermal hydraulics analysis and each of these frequencies, of course, are uncertain. There will be uncertainty quantified.

How we do that in PRA space is it's the standard procedure, it's well known, and we can talk about that, if you wish, but I was going to touch briefly on what we're doing in thermal hydraulics and TFM, because that's something I think that's certainly a little bit unusual for the kinds of analysis that we usually perform.

I did want to point out also that in the PFM analysis, you see this little -- these two distributions overlapping. That's supposed to be a representation of stress and strength. So basically what we're saying is that some fraction of times that the vessel is hit with a particular thermal hydraulic sub-scenario, some pressure or temperature characteristic curves, it will fail.

But it's some fraction of time, it's not necessarily one, it's not necessarily a zero.

Of course, we're uncertain about a lot of the parameters that go in here, so there's a layer of uncertainty that's not explicitly represented in this diagram. That's what the note at the bottom of the diagram indicates.

Regarding the probabilistic fracture mechanics parameter and our treatment of uncertainty, the white paper talked about what are the sources of uncertainty in the key model parameters, the ones that we have been told are the ones that seem to drive the results, and based on some guiding principals as to how we're doing this modeling, those uncertainties were characterized as being either aliatory or epistemic.

DR. KRESS: I gather that wasn't as straightforward as you might think.

MR. SIU: It's not -- neither -- well, I don't know if it's straightforward. It is something that -- there are modeling decisions being made as you go through this. You have to decide what's your model of the world. Professor Apostolakis' papers talk about this.

Once you fix on that model, then you can derive what is -- how you would categorize each of these, but I would say at this point, the paper is still being digested by lots of folks and I'm sure we're going to get some ideas as to maybe whether the categorization that's in the paper is correct or not.

I think it's a pretty good stab at it, I'd like to think that.

The aliatory uncertainties in this -- again, I'm talking about the probabilistic fracture mechanics part, so that's that third box in that diagram. I'm not talking about the whole spectrum. But certainly you have uncertainties there arising because of the uncertainties in the thermal hydraulics scenario. So the frequency with which you get hit with a particular scenario trace or at least a scenario trace that represents a bin of thermal hydraulics sub-scenarios.

And then there is this issue of conditional failure of the vessel given a thermal hydraulic scenario, and that's the point I was trying to raise through that stress-strength diagram.

We are -- so that's -- we're addressing aliatory uncertainties through those two mechanisms, through the scenario frequencies and through the stress-strength model.

The epistemic uncertainties, we're just using standard estimation techniques. You heard some discussion this morning about such things as the copper, nickel content at, let's say, a particular position in the reactor vessel. The point about the correlation of parameters is obviously an important one, and I don't know that we've looked into it as carefully as we should yet.

But once we have characterized the uncertainties and the propagation of these uncertainties through the model, it is done in the FAVOR code, it's a standard Monte Carlo propagation approach and I don't know that we need to talk about that very much.

Again, FAVOR is the tool being used to assemble all these results.

I'd say that we're a little further behind in our treatment of thermal hydraulic uncertainties. The white paper, as you have seen, is focused primarily on the issue of the probabilistic fracture mechanics issues. But certainly we have the same objective. We need to characterize and quantify the uncertainties, in this case, in the thermal hydraulics analyses.

Right now, we expect that whatever we do, that characterization will be compatible with the current version of FAVOR, which means basically we're talking about deterministic pressure and temperature traces over time, also the heat transfer coefficient of the downcomer, and that the uncertainties in the thermal hydraulics scenarios will be represented through uncertainties in the frequencies of those scenarios, but we won't have bands of scenarios to propagate through the code, because of just computational limitations. We don't think we can do that.

The University of Maryland has the lead with this work. Professor Ali Mosleh is sitting back there. He and

Professor Modarres are our PIs, and we've initiated planning on how to actually do this work. This is, as many of you know, not an easy task to look at the thermal hydraulic uncertainties.

We have a cooperative research program with the University of Maryland, and so they will address this issue under that task.

The first part of that task will be to look specifically at PTS issues and later on we expect that they will broaden out and look at non-PTS applications and maybe broaden the approach to go beyond just the assessment of uncertainties in the thermal hydraulics scenario frequencies.

We do believe right now that the approach will involve a considerable amount of screening because of, again, the computational resources that are required. We have to get down pretty quickly to scenarios where it appears that a detailed analysis is needed.

We hope to use both thermal hydraulic models and probabilistic fracture mechanics models in that screening process.

That's all I have to say about uncertainty analysis. Again, we have some backup slides, we'd be willing to chat with you about that, if you have any questions.

DR. POWERS: The thing on that last slide that's most striking is this rapid screening and if you're going to use Monte Carlo methods, why do you care about screening things out?

MR. SIU: Well, as you know, you can do Monte Carlo in the crudest fashion. You would end up simulating things that you really don't care about. So you could use screening in the sense of important sampling, where you focus your Monte Carlo analysis on those parts where it really makes a difference.

What we're talking about is trying to eliminate scenarios where there just doesn't look like there's going to be any PTS challenge whatsoever. That's obviously the first screen. Then you can, from a PRA standpoint, say, well, this is possible, but it just is highly improbable and because of the systems failures that you require and you throw those out, as well, and the hope is, and obviously we don't know that this hope will be realized until we do it, is that we really can narrow down to a smaller number of scenarios that are reasonably tractable.

We might have to develop some sort of simplified thermal hydraulic representation to address uncertainties, propagation of uncertainties, but, again, that's open to question right now.

DR. POWERS: It's just that the screening is going to be based on intuition and judgment.

MR. SIU: Yes, that's fair, and I will also say that I think we're way better than where we were back then in the '80s.

DR. SHACK: Could you explain a little bit more about the notion that the thermal hydraulic, the uncertainties is all in the frequencies and not in the time traces?

MR. SIU: Roy, could you go back this diagram?

MR. WOODS: Sure.

MR. SIU: As a philosophical matter, I suppose you could say that if you define the scenarios finely enough, let's say that you know exactly when everything occurs and if you're comfortable that you have a very robust model for the system behavior, that most of the uncertainties would be in just the specification of -- I don't know what parameters, I'm certainly not an expert here, maybe Farouk in the back might be able to help me out here.

But if you -- there are some parameters that, let's say, your empirical coefficients in the heat transfer correlation, we all know, you know those within plus or minus 20 percent, at best.

Okay. But if you've nailed everything else down and all you have to know is that particular coefficient, you could say, well, there could be some uncertainty there, yes, and then I could have a bundle of scenarios rather than a single one.

What we're saying right now is hopefully we will carefully define the scenarios such that we can get down to that point where if we really are talking plus or minus 20 percent, it's not really a big issue compared to some of the other things that we've got in the other parts of the model.

And one of the concerns is that we don't do an overkill here if we have huge uncertainties in other parts of the analysis.

But it's clearly an approximation, but doing it this way. We've had some discussions with the thermal hydraulics modelers and have some sense of feeling at this point that a lot of the uncertainties have to do with the input to their models and if that's the case, then I think we know how to handle that.

MR. WOODS: As we pointed out, I guess this is just summarizing. The development of the Oconee PTS PRA model is going very well. The integrated PRA team, HRA team, is developing a plant model and has visited the plant.

It's 2:00. They're still visiting the plant, aren't they?

MR. GALYEAN: That's right.

MR. WOODS: We were there Tuesday and Wednesday of this week, the three of us, plus three HRA people and --

well, anyway, you get the idea. There was a part of the meeting regarding integrated control systems. The guy that we needed to talk to was only available today. So they stayed and the three of us had another important engagement. So we left and left them there to do it.

DR. POWERS: And then you dropped by here, right?

MR. WOODS: I'm sorry?

DR. POWERS: Then you dropped by here.

MR. WOODS: Yes, right, then we dropped by here. I think this is an accurate statement here, screening level results are expected shortly. It depends. You don't want to say shortly as this afternoon, but like toward the end of this month, middle of next month, we expect to have some idea where the through wall crack frequency -- no, no. I'm sorry -- where the frequencies of some of these significant sequences are. We will not have run it through the thermal hydraulic analysis and we will not have run it through the PFM calculations.

But we will begin to have PRA results, PRA/HRA results at that point.

And that's the one we're working on now. We have made some initial contacts with Beaver Valley. I think we pointed out they are the ones that are going to step in for the Westinghouse three-loop plant. We wanted a three-loop plant because H.B. Robinson is a three-loop plant. We had the previous analyses back in the mid '80s for H.B. Robinson, so if you choose a similar plant, you know some of that's applicable. The thermal hydraulics models are applicable more so than they would be for a four-loop plant or something.

So anyway, that's been initiated. We're getting some requests to them. I guess I have to back up and say for the Oconee people, that the cooperation has just been excellent. If they had it or could imagine where it might be or could dredge it out or call somebody in, then we had it just as quickly as they could provide it.

So that really is going very well.

And the last item on this slide, uncertainty analysis, I guess we just talked about that. There's not much else to add. That's the presentation. Do you have questions?

There can be several reasons for no questions. Some of them are not complimentary and some of them are.

DR. POWERS: I know this committee pretty well. They're all being complimentary right now. That was a very nice presentation.

DR. KRESS: If we had criticisms that were severe, we wouldn't be reluctant to say them.

MR. WOODS: I've seen that over the years maybe.

DR. KRESS: Actually, I think this looks pretty good.

DR. POWERS: You can get back to some good fire analysis.

DR. SHACK: I guess there is sort of one comment. I look at that embedded analysis and it sort of looks like it makes the whole problem go away. If I live with embedded flaws, everything else goes away. Is this overkill? Have you seen anything that indicates that you're unconservative somewhere else?

So that if they produce the new flaw analysis, you could declare victory. Dana wants it done completely, but he wants it done quickly so you can get back to the fire analysis.

DR. POWERS: But you have to do fire analysis to do it completely.

MR. CUNNINGHAM: There could be a couple of places where we're under-estimating, if you will, the frequencies. One is the human element of it, the human performance element of it. We're adding some different wrinkles to that that we haven't done before. So that could change our perspective on the frequencies to some of these challenges.

The other part comes back to the acceptance criterion that I talked about, is that I wouldn't imagine that it gets any less conservative, if you will, or higher value of an acceptance criterion today.

Under some scenarios that I don't think are probable, but under some scenarios, that could become tighter. So it offsets, to some degree, some of the benefits we get in the materials area.

I don't think that's a likely scenario, but I think we need to nail that down. So there are at least a couple of places where it could come into play, where other features of the analysis could come into play to counteract some of the benefits we're getting out of the materials research.

MR. SIU: There are some other places, like treatment of support systems, where, like Bill pointed out, some new initiators that were not in the old studies and might raise the numbers. Again, the hope is that it doesn't raise them tremendously, but you don't know until you do it.

MR. DIXON: Also, Terry Dixon, from Oak Ridge. I assume you're referring to the plants that Shah put up this morning. Those analyses were done in 1998 based on the PVRUF data and it's my understanding that the Shoreham data is coming in with higher flaw densities than PVRUF. So that's one thing that could be negative relative to the analysis results that Shah put up this morning.

Also, the statistical distribution of the K-1-c database, it was discussed this morning that the effect is transient dependent, so who knows. So those are two possibilities that could go counter to what you saw this morning.

DR. SHACK: Just to finish up, does anybody have any -- go around the table, if anybody wants to add any comments.

DR. POWERS: Well, the probabilistic is going to be looked at and we'll see what we get and the plan seems to be fine. My biggest concern is that when people start telling screening, I think of babies and bath water and things like that, because the intuition just doesn't work.

We wouldn't go to PRA if our intuition was so good on these things. But these are cautious people that have a lot of expertise in doing this and so I have a great deal of confidence in them.

We talked this morning about rigor in the statistical analysis and whatnot and quite frankly, I really didn't understand the rigor there. I think what they really mean is they're doing a pretty careful job and to an engineering detail and they don't really mean they're going to go through a rigorous statistical analysis on this stuff that would leave us all confused and befuddled. They're doing things that are pretty obvious, is what I think actually, and it looks very promising.

This is one of the really nifty research programs, because it brings together three disciplines and a focused attack that probably is a lot of fun to work on, actually, because you probably learn a heck of a lot in the project meetings.

So I guess I'm pretty positive on this, except for the fact that it deters a really good fire safety analyst, so he's not available to work on one of the really important problems.

DR. BONACA: I can only say that I am favorably impressed by the effort, by the comprehensiveness of all the elements coming together. This is a very good example of a lot of deterministic and probabilistic analysis coming together.

The area where I have still questions, in my mind, is regarding criteria that will be used to modify the rule. That's really a much more, I guess, sensitive issue, because of all the things we discussed before.

I'm sure that I recognize that you recognize that it is a sensitive issue and I'll be very alert to how it's being modified, because there is a lot of information coming together here, but, again, this is a quite unique scenario we're talking about, more different than most.

So that's where I have more questions.

DR. SHACK: George?

DR. APOSTOLAKIS: The presentation this afternoon was fairly high level. I think the implementation is really where difficulties will be. So I guess I'll form an opinion then.

DR. POWERS: One aspect of it that was not pursued other than just to bring it up that will be really interesting to see what they do is the hot standby analyses and how you approach those problems. That will be new and different.

DR. KRESS: Yes.

DR. SHACK: I just basically thought the presentations were very good. It seemed to me a very comprehensive and interesting program. We're looking forward to sort of seeing how it all plays out.

DR. KRESS: I frankly was very impressed. I think this is can serve as a model program on how to risk-inform regulation. I think it's very good. I'm quite glad to see this very nice uncertainty incorporation in the process. I think, as a follow-on to that, I think we need to really think about how are we going to use those uncertainties in the decision-making process, and I didn't really see that come through.

Now, I think it has to do with the acceptance criteria and I think acceptance criteria, to me, is a matter of policy and it's something that really could impact this whole thing as much as anything, because moving it just a little bit one way or the other can make a big difference.

The other thing is I wasn't -- I had some minor concerns about the expert elicitation process, but that may just be my bit. I don't like expert elicitation. But I recognize that there are some places where that's the only way you can get the uncertainty and so you have to use it.

But I agree with Dana that you have to watch out for correlations and there may be better ways to correlate the mean versus -- or the variance versus the mean and what they have, but those are minor issues.

I really think you have a good thing going here and I urge you to continue with it. It's a good way to -- you've wrapped up all the data, you've got all the models wrapped up, you've done an uncertainty analysis. I think it's a complete package and that's really what I like about it.

People can come back ten years from now and look at your report and say, whoa, they'll know exactly what you did and it will all be retrievable. It's good stuff, I think. You guys can be proud of it.

DR. BONACA: Just one thing. In addition to that, I would really -- I really enjoyed the documentation you provided. I think the paper on uncertainty analysis was very clear, helpful.

MR. SIU: Thank you.

DR. SHACK: Tom, are you ready to make a decision on whether we need a subcommittee meeting on the Commission paper topic?

DR. KRESS: I think we ought to have a subcommittee meeting and then bring it to the full.

DR. SHACK: Rather than just a full committee meeting.

DR. KRESS: Yes. And just on this part that Mark talked about.

DR. APOSTOLAKIS: Half a day?

DR. KRESS: Half a day would be plenty, I think.

MR. DUDLEY: And we would be looking at a full committee meeting in May.

DR. KRESS: I don't know what the timing was. I think we'll have to --

DR. SHACK: Because of the way they plan to do it, it almost has to be.

MR. CUNNINGHAM: We owe a Commission paper in May.

DR. KRESS: That would be a good time to do it.

MR. CUNNINGHAM: It would be good. I'm not expecting that the Commission will have to make an immediate decision on where we go on this, so I don't know that it -- if the letter happens in June versus May, that it will make that much difference, quite frankly.

DR. SHACK: It sort of has to be in that timeframe.

MR. CUNNINGHAM: Yes.

DR. POWERS: Do we have it in our future activities list?

MR. DUDLEY: No, we don't. So this meeting was to define what future meetings we would have.

DR. BONACA: That would mean a subcommittee meeting next month.

MR. DUDLEY: That's correct.

MR. HACKETT: Just a point of clarification. This is Ed Hackett. The full committee then, Noel, in June, would address the entire project or are we looking at just addressing the acceptance criterion?

MR. DUDLEY: Well, there would be one full committee meeting in May to discuss the risk criteria and then the expert elicitation would be heard either in June or July, based on your progress.

MR. HACKETT: Okay. Thanks.

DR. POWERS: Yes. We don't want to schedule an expert elicitation process until they're ready. I don't want it cascading, June, and then next it's July, and then it's September and October. Give yourselves some padding on your schedule.

I assume experts are a little bit like herding cats and you'll not go wrong.

MR. HACKETT: It's been tough. I got to say, Debbie probably deserves some kind of award for what she's been able to do so far, Debbie and Lee.

MR. CUNNINGHAM: Thank you very much.

DR. SHACK: We are adjourned.

[Whereupon, at 2:16 p.m., the meeting was concluded.]

REPORTER'S CERTIFICATE

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

NAME OF PROCEEDING: MEETING: MATERIALS AND METALLURGY

CASE NUMBER:

PLACE OF PROCEEDING: Rockville, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Mark Mahoney
Official Reporter
Ann Riley & Associates, Ltd.

INTRODUCTORY STATEMENT BY THE CHAIRMAN OF THE MATERIALS AND METALLURGY SUBCOMMITTEE

11545 ROCKVILLE PIKE, ROOM T-2B3
ROCKVILLE, MARYLAND
MARCH 16, 2000

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

The other ACRS Members in attendance are: Mario Bonaca, Thomas Kress, and Dana Powers.

The purpose of this meeting is for the Subcommittees to review the status of activities related to the staff's Pressurized Thermal Shock (PTS) Screening Criterion Reevaluation Project. The Subcommittees will gather information, analyze relevant issues and facts, and formulate proposed positions and actions, as appropriate, for deliberation by the full Committee.

Mr. Noel Dudley is the Cognizant ACRS Staff Engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the *Federal Register* on February 25, 2000.

A transcript of this meeting is being kept, and will be made available as stated in the Federal Register Notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

We have received no written comments or requests for time to make oral statements from members of the Public.

[Chairman's Comments]

We will now proceed with the meeting and I call upon Mr. Edwin Hackett, Acting Chief of the Materials Engineering Branch in the Office of Nuclear Regulatory Research, to begin.

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
JOINT MEETING OF THE MATERIALS AND METALLURGY AND RELIABILITY AND PROBABILISTIC RISK ASSESSMENT SUBCOMMITTEES
PTS SCREENING CRITERION REEVALUATION

MARCH 16, 2000

ROCKVILLE, MARYLAND

- PROPOSED AGENDA -

	TOPIC	PRESENTER	TIME
I.	Opening Remarks	W. Shack, ACRS	8:30-8:35 a.m.
II.	Pressurized Thermal Shock Analysis Methodology Overview	E. Hackett, RES	8:35-9:00 a.m.
III.	Probabilistic Fracture Mechanics Analysis A. Methods B. Input C. Issues D. Computer Code (FAVOR)	S. Malik, RES	9:00-10:00 a.m.
	- BREAK -		10:00-10:15 a.m.
IV.	Probabilistic Fracture Mechanics Analysis (Continued)	S. Malik, RES	10:15-11:00 a.m.
V.	Status of Flaw Distribution Expert Elicitation Process	D. Jackson and L. Abramson, RES	11:00-12:00 noon
	- LUNCH -		12:00-1:00 p.m.
VI.	Introduction	G. Apostolakis ACRS	1:00-1:05 p.m.
VII.	PTS Risk Acceptance Criterion	M. Cunningham, RES	1:05-2:15 p.m.
	- BREAK -		2:15-2:30 p.m.
VIII.	Probabilistic Risk Assessment A. Identified Scenarios B. PRA Analysis Efforts C. Uncertainty Analysis	H. Woods, RES N. Siu, RES W. Galyean, INEEL	2:30-3:30 p.m.

IX.	Discussion	W. Shack, ACRS	3:30-4:30 p.m.
X.	Adjournment	W. Shack, ACRS	4:30 p.m.

NOTE:

Presentation time should not exceed 50 percent of the total time allotted for specific item. The remaining 50 percent of the time is reserved for discussion.

Number of copies of the presentation materials to be provided to the ACRS - 25.

OVERVIEW OF PRESSURIZED THERMAL SHOCK TECHNICAL BASIS RE-EVALUATION PROJECT

E. M. Hackett, Acting Branch Chief
Materials Engineering Branch
Office of Nuclear Regulatory Research

Presentation to the
Advisory Committee on Reactor Safeguards
Materials and Metallurgy Subcommittee

March 16, 2000

PTS RE-EVALUATION PROJECT
BACKGROUND

- Recent technical developments indicated the potential for increasing the accuracy (reducing conservatisms) for PTS analyses:
  - Improved estimates for weld flaw density and distribution
  - Improved embrittlement correlations
  - statistical bases for fracture toughness
- The project was initiated in April, 1999 as a fully-participatory effort with the industry (MRP, EPRI, NEI)
- ACRS briefings - 2/99, 7/99, 3/00
- Project organized in three key technical areas:
  - Probabilistic Fracture Mechanics (PFM)
  - Thermal-Hydraulics (T-H)
  - Probabilistic Risk Assessment (PRA)

PTS RE-EVALUATION PROJECT APPROACH

- Overall - Best estimate analyses for individual technical inputs with uncertainty explicitly addressed at each point
- Update technical inputs in PFM, T-H and PRA and Re-do IPTS studies with new information
- In parallel, re-assess the PTS Risk Acceptance Criteria

PTS RE-EVALUATION PROJECT
CURRENT STATUS

○ Significant progress in key technical areas:

○ PFM
- Expert elicitation for generic flaw distribution (5/00)
- Revised embrittlement correlations (4/00)
- Statistical bases for fracture toughness (4/00)
- Plant-specific flux maps being developed:
  - * Palisades (1/00)
  - * Oconee (3/00)
  - * Calvert Cliffs (7/00)
  - * Beaver Valley 1 (9/00)

○ T-H
- Determination of key transients to be analyzed (4/00)
- Verification from testing at Oregon State University APEX facility

○ PRA
- Consistency w/NRC risk-informed guidance
- RG 1.174, CDF, LERF
- Options presented for Policy Decision (Commission paper - 5/00)

PTS RE-EVALUATION PROJECT ISSUES

- H.B. Robinson declines participation (2/00)
- Beaver Valley Unit 1 agrees to participation (3/00)
  - Additional PRA and T-H work
  - 2 month schedule delay?

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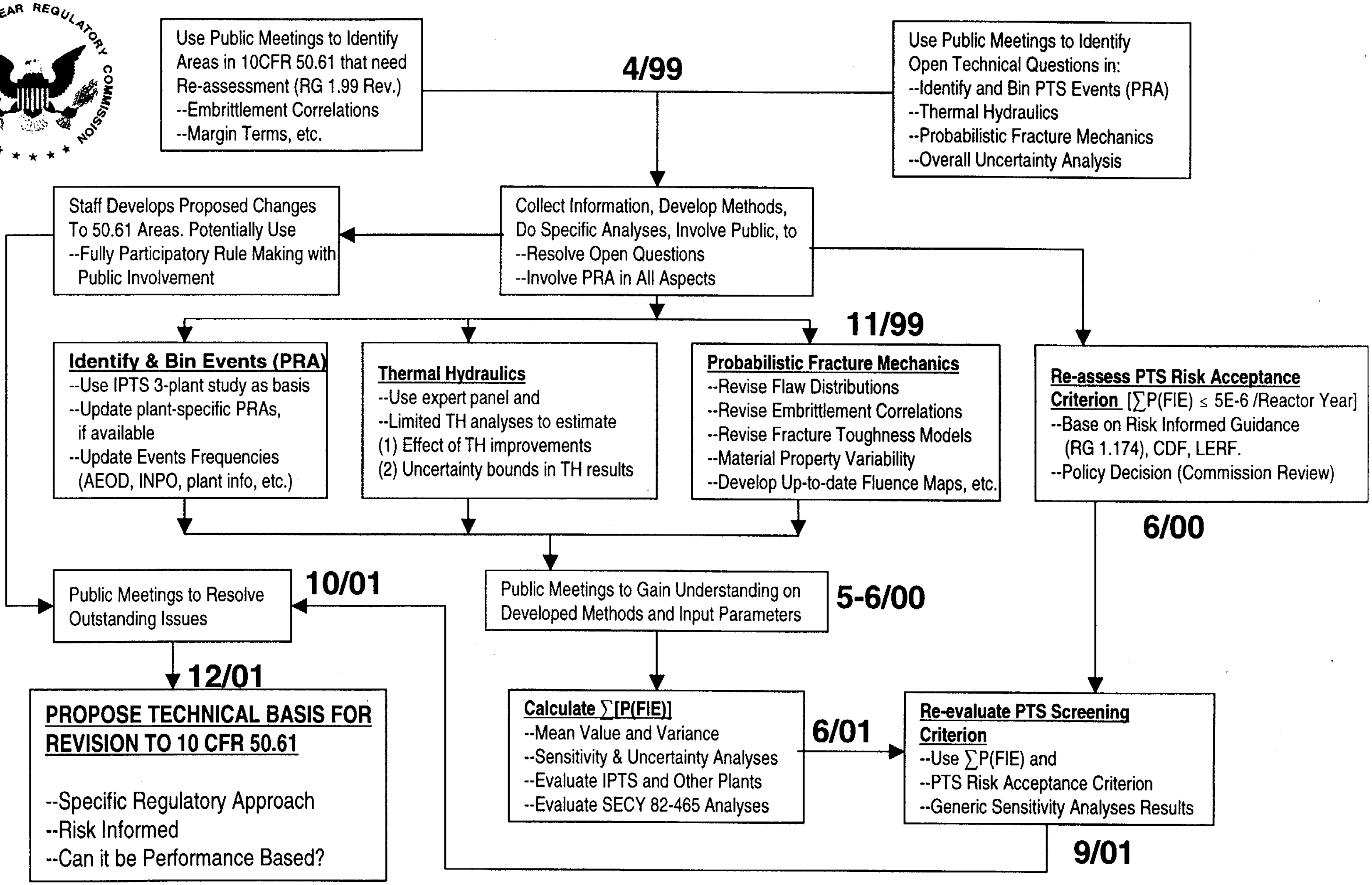

Use Public Meetings to Identify Areas in 10CFR 50.61 that need Re-assessment (RG 1.99 Rev.)
- --Embrittlement Correlations
- --Margin Terms, etc.

4/99

Use Public Meetings to Identify Open Technical Questions in:
- --Identify and Bin PTS Events (PRA)
- --Thermal Hydraulics
- --Probabilistic Fracture Mechanics
- --Overall Uncertainty Analysis

Collect Information, Develop Methods, Do Specific Analyses, Involve Public, to
- --Resolve Open Questions
- --Involve PRA in All Aspects

Staff Develops Proposed Changes To 50.61 Areas. Potentially Use
- --Fully Participatory Rule Making with Public Involvement

11/99

Identify & Bin Events (PRA)
- --Use IPTS 3-plant study as basis
- --Update plant-specific PRAs, if available
- --Update Events Frequencies (AEOD, INPO, plant info, etc.)

Thermal Hydraulics
- --Use expert panel and
- --Limited TH analyses to estimate
  - (1) Effect of TH improvements
  - (2) Uncertainty bounds in TH results

Probabilistic Fracture Mechanics
- --Revise Flaw Distributions
- --Revise Embrittlement Correlations
- --Revise Fracture Toughness Models
- --Material Property Variability
- --Develop Up-to-date Fluence Maps, etc.

Re-assess PTS Risk Acceptance Criterion [$\sum$P(FIE) ≤ 5E-6 /Reactor Year]
- --Base on Risk Informed Guidance (RG 1.174), CDF, LERF.
- --Policy Decision (Commission Review)

6/00

Public Meetings to Gain Understanding on Developed Methods and Input Parameters

5-6/00

Calculate $\sum$[P(FIE)]
- --Mean Value and Variance
- --Sensitivity & Uncertainty Analyses
- --Evaluate IPTS and Other Plants
- --Evaluate SECY 82-465 Analyses

6/01

Re-evaluate PTS Screening Criterion
- --Use $\sum$P(FIE) and
- --PTS Risk Acceptance Criterion
- --Generic Sensitivity Analyses Results

9/01

Public Meetings to Resolve Outstanding Issues

10/01

12/01

PROPOSE TECHNICAL BASIS FOR REVISION TO 10 CFR 50.61

- --Specific Regulatory Approach
- --Risk Informed
- --Can it be Performance Based?

Development of Technical Basis to Revise PTS Rule 50.61

PTS Re-evaluation Project

Progress in Probabilistic Fracture Mechanics

Shah Malik, Mark Kirk, Doug Kalinousky, Tanny Santos
Materials Engineering Branch
Office of Nuclear Regulatory Research,
U.S. NRC

Presented to: Advisory Committee on Reactor Safeguards,
Materials and Metallurgy Subcommittee
March 16, 2000

PTS Re-Evaluation Project
Probabilistic Fracture Mechanics
Presentation Outline

- Overall status of PFM activities
- Where it fits into PTS re-evaluation
- Progress in major PFM technical areas
- Concluding remarks

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PTS Re-Evaluation Project
Probabilistic Fracture Mechanics
Overall Status

- Held 6 public meetings (staff, contractors, industry representatives, and public)
  - 1999: May, July, Sept., Nov., Dec.
  - 2000: Feb.
- Near and long term action plans developed
  - Tasks assigned to groups and individuals
- Coordinated with PRA and TH activities
- Interim version of PFM code, FAVOR, released in Oct. 1999 for comments and use

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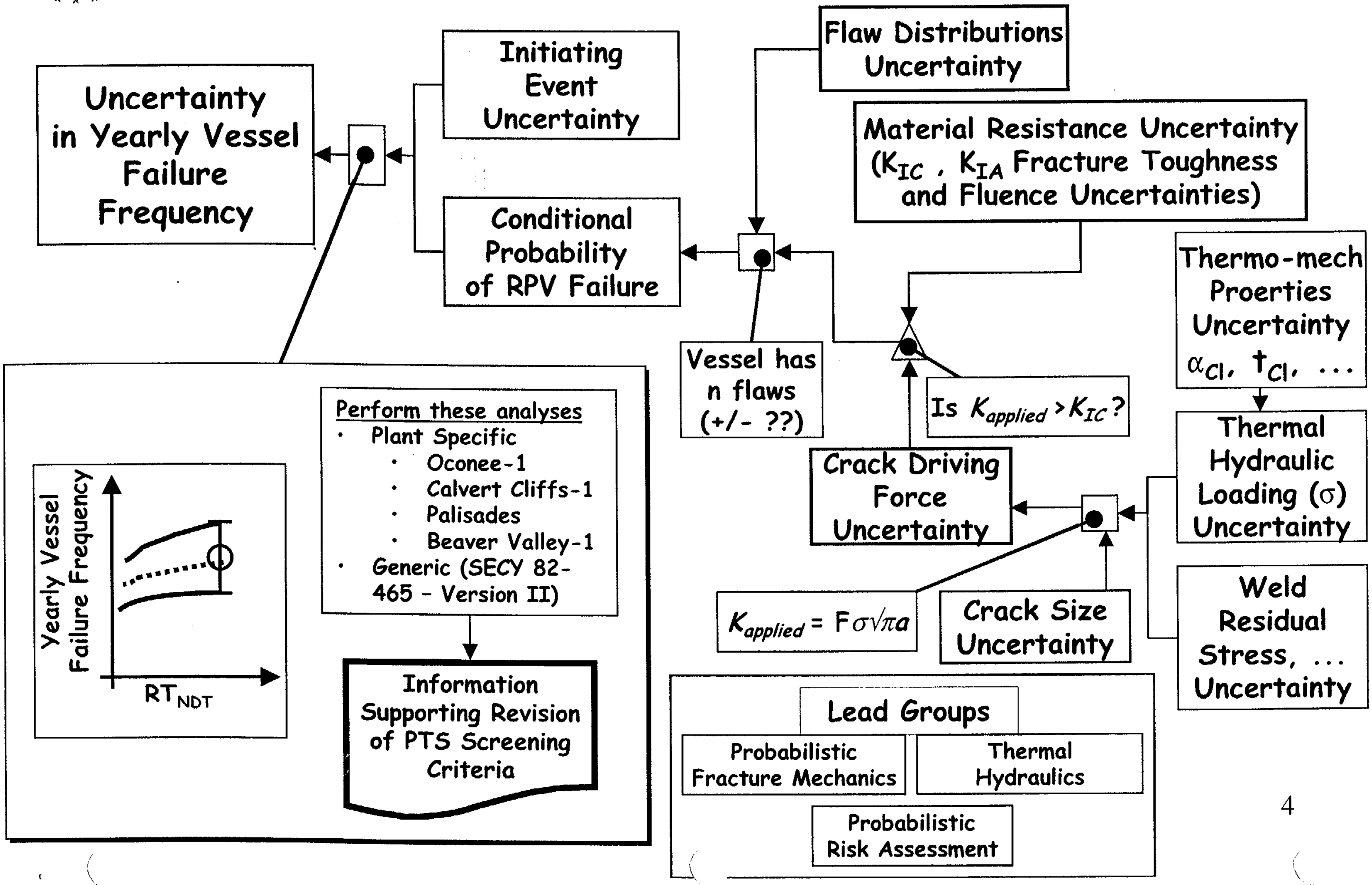
Where this Fits into PTS Re-Evaluation



Uncertainty in Yearly Vessel Failure Frequency

Initiating Event Uncertainty

Conditional Probability of RPV Failure

Flaw Distributions Uncertainty

Material Resistance Uncertainty (K_{IC} , K_{IA} Fracture Toughness and Fluence Uncertainties)

Thermo-mech Proerties Uncertainty α_{Cl}, t_{Cl}, ...

Vessel has n flaws (+/- ??)

Is $K_{applied} > K_{IC}$?

Crack Driving Force Uncertainty

Thermal Hydraulic Loading (σ) Uncertainty

$K_{applied} = F\sigma\sqrt{\pi a}$

Crack Size Uncertainty

Weld Residual Stress, ... Uncertainty

Perform these analyses

- Plant Specific
  - Oconee-1
  - Calvert Cliffs-1
  - Palisades
  - Beaver Valley-1
- Generic (SECY 82-465 - Version II)

Information Supporting Revision of PTS Screening Criteria

Lead Groups

Probabilistic Fracture Mechanics

Thermal Hydraulics

Probabilistic Risk Assessment

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PTS Re-Evaluation Project
Probabilistic Fracture Mechanics
Major Technical Areas

1. Fabrication Flaw Distributions in RPV beltline
2. Rigorous statistical representation of fracture toughness, K1c and K1a, data
3. Improved irradiation embrittlement correlations to predict shift in RT_{NDT} (ΔRT_{NDT})
4. Improved statistical distributions for material chemistry and initial (unirradiated) RT_{NDTo}
5. Beltine neutron fluence maps
6. PFM computer code, FAVOR, revision

PTS Re-Evaluation Project
Probabilistic Fracture Mechanics

Fabrication Flaw Distributions

- **Objective:** Determine generalized flaw sizes, density (# of flaws/unit volume), and location distributions of fabrication flaws in welds, plates and forgings in RPV beltline region, using:
  - NDE/DE techniques and expert judgement process
- **RES Staff:** Deborah Jackson
- **NRC Contractor:** PNNL

Probabilistic Fracture Mechanics Fabrication Flaw Distributions --Contd.

- NDE/DE of welds in one RPV (PVRUF) completed, and continuing for others
  - NRC, Industry/EPRI working in parallel
  - PVRUF, Shoreham, River Bend-2, Hope Creek-2 vessels
- Expert judgement process is continuing
- NDE of limited plate material has begun
  - Data to be collected in March/April ‘00
- Generalized distributions to be developed by May/June ‘00

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Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

- **Objective**: Revise toughness distribution curves based on expanded LEFM-valid (ASTM E 399) available data and rigorous statistical methods
  - Current distributions were based on:
    - limited 1970's/80's data
      - 171 data points for K_{IC} (LEFM-valid)
      - 50 data points for K_{IA} (LEFM-valid)
    - In SECY-82-465 and IPTS studies, Ad-hoc distributions developed from lower-bound ASME toughness curves

- **RES Staff**: Mark Kirk, Shah Malik, Nathan Siu
- **Contractors**: ORNL, Univ. of Maryland

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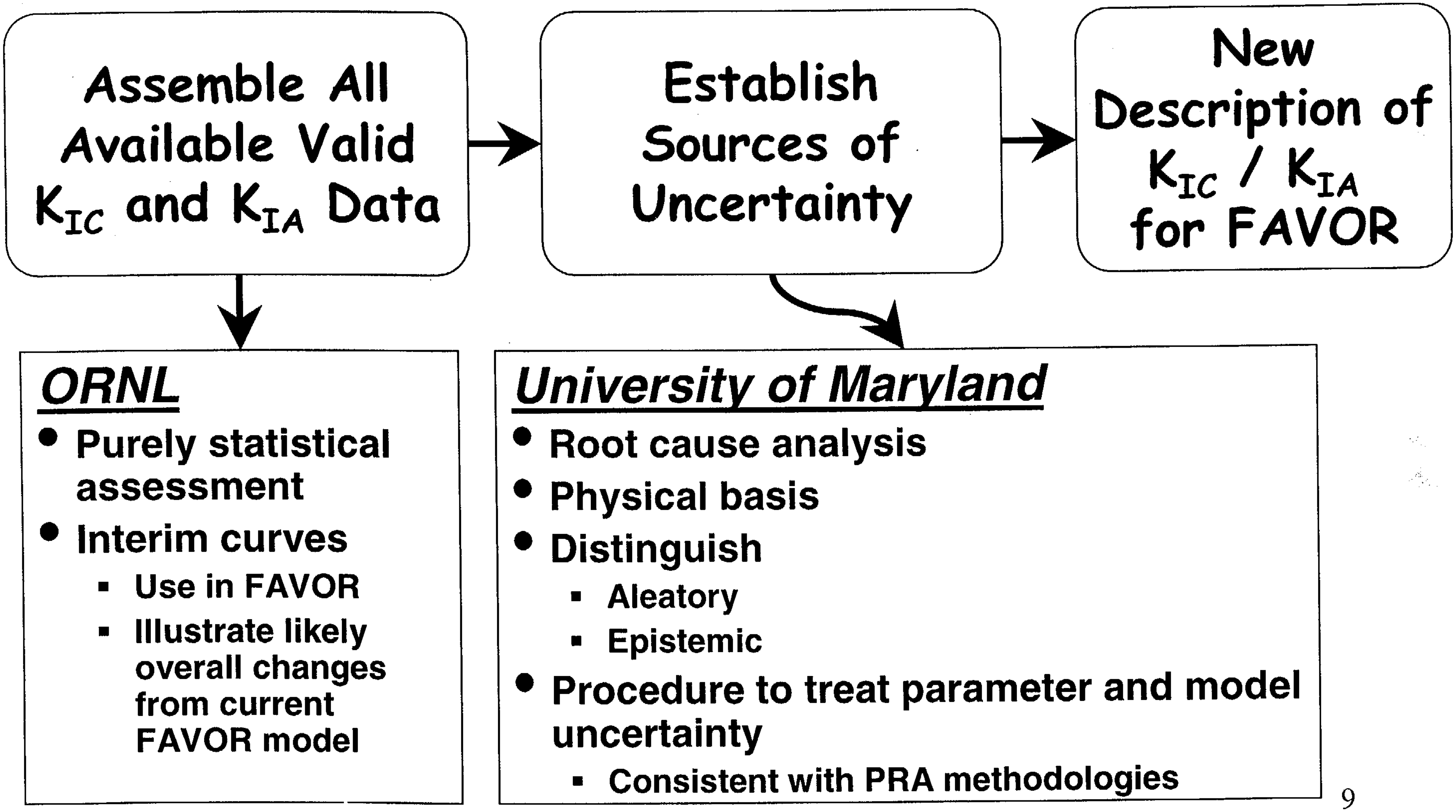
Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves



Assemble All Available Valid K_{IC} and K_{IA} Data → Establish Sources of Uncertainty → New Description of K_{IC} / K_{IA} for FAVOR

ORNL

- Purely statistical assessment
- Interim curves
  - Use in FAVOR
  - Illustrate likely overall changes from current FAVOR model

University of Maryland

- Root cause analysis
- Physical basis
- Distinguish
  - Aleatory
  - Epistemic
- Procedure to treat parameter and model uncertainty
  - Consistent with PRA methodologies

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Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves, Cont.

- ORNL searched and collected additional data (database has now increased considerably)
  - 83 additional LEFM-valid K_{IC} (initiation) data (254 total)
    - Increase of almost 50%
  - 62 additional LEFM-valid K_{IA} (arrest) data (112 total)
    - Increase of over 100%
  - Weibull distributions developed based solely on data
  - Japanese K_{IC} / K_{IA} database was not available, but we may be able to obtain it now

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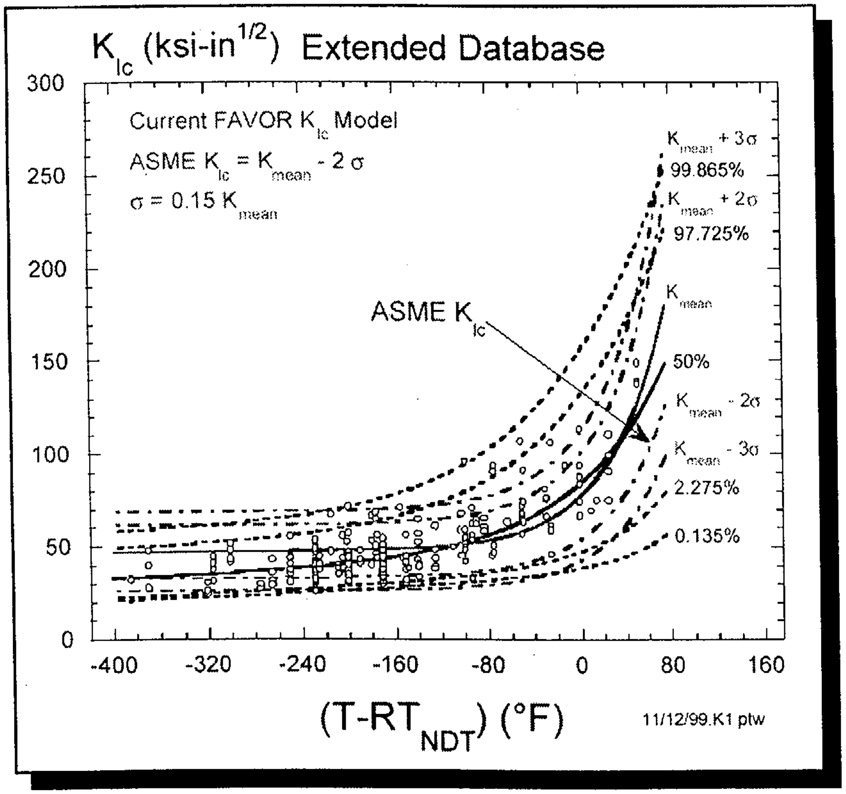
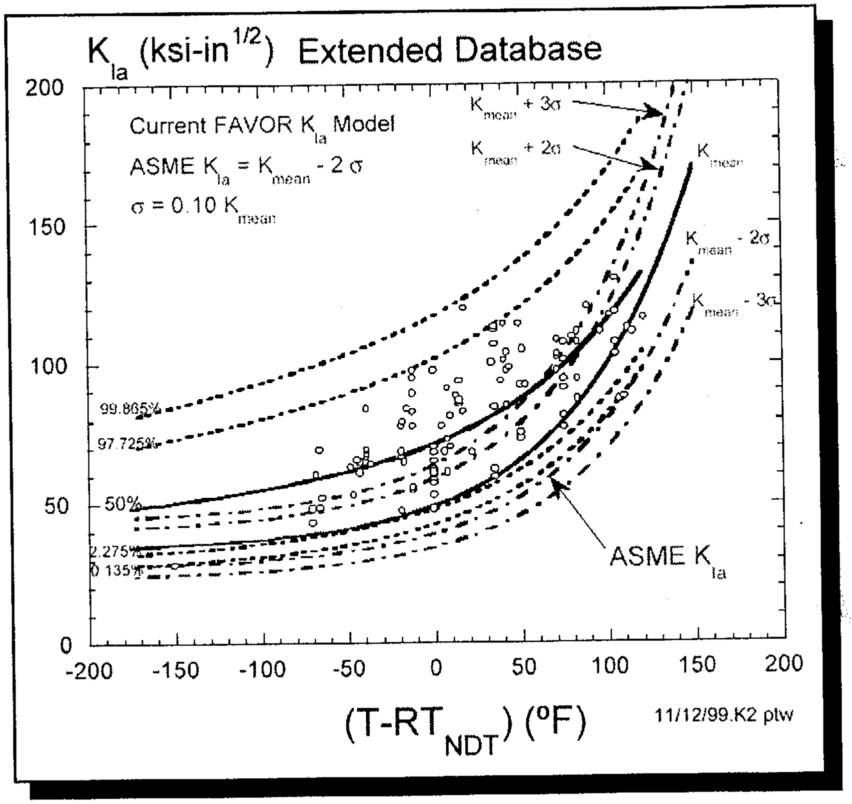
Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves





Old *FAVOR* scatter-bands too narrow

Effect on Probability of Vessel Failure Depends on Transient Considered

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Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

Form of ORNL Equations

$$K_{Ic}(p, \Delta T) = a\Delta T + b\Delta T \cdot \{-\ln(1-p)\}^{1/(c\Delta T)}$$

$$a = 10.8957 + 23.4192 \cdot \exp(0.0023 \cdot \Delta T)$$

$$b = 14.7582 + 42.6312 \cdot \exp(0.0124 \cdot \Delta T)$$

$$c = 2.03025 + 0.4983 \cdot \exp(0.0135 \cdot \Delta T)$$

$p =$ Cumulative probability of failure
$\Delta T =$ Temperature relative to RT_{NDT} (in °F)

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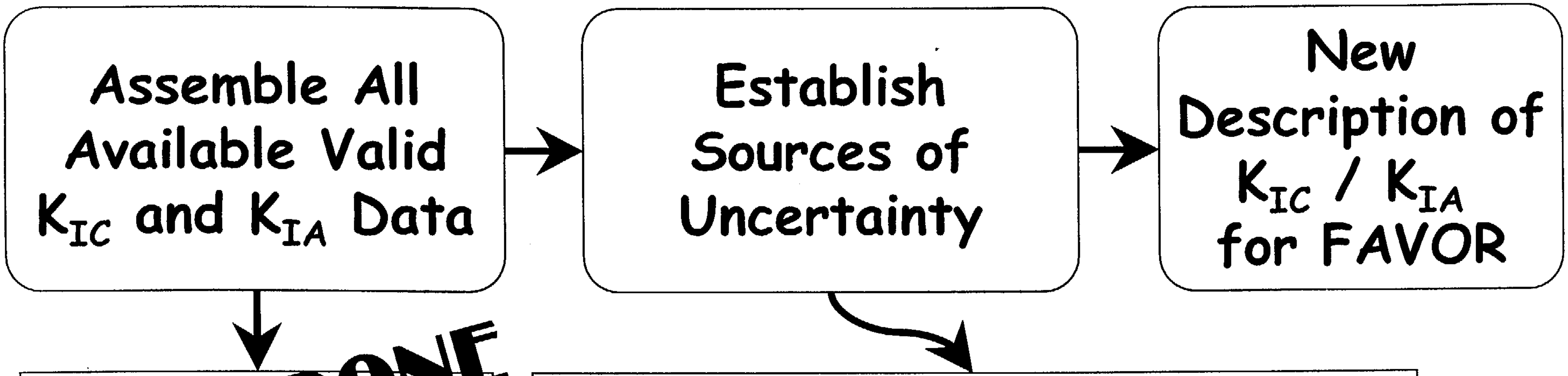
Probabilistic Fracture Mechanics

Fracture Toughness (K_{IC} and K_{IA}) Curves



Assemble All Available Valid K_{IC} and K_{IA} Data →

Establish Sources of Uncertainty →

New Description of K_{IC} / K_{IA} for FAVOR

DONE

ORNL

- Purely statistical assessment
- Interim curves
  - Use in FAVOR
  - Illustrate likely overall changes from current FAVOR model

We Are Here

University of Maryland

- Root cause analysis
- Physical basis
- Distinguish
  - Aleatory
  - Epistemic
- Procedure to treat parameter and model uncertainty
  - Consistent with PRA methodologies

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

University of Maryland

Root Cause Analysis

- ➢ Identify sources of uncertainties
- ➢ Physically based material's model for the toughness curves in ductile-to-brittle transition temperature range
  - ◆ Prof. Natishan (EPRI funding)

PRA Framework

- ➢ Distinguish epistemic (state of knowledge) and aleatory (randomness) parts of uncertainties
- ➢ Mathematical model to quantify uncertainties

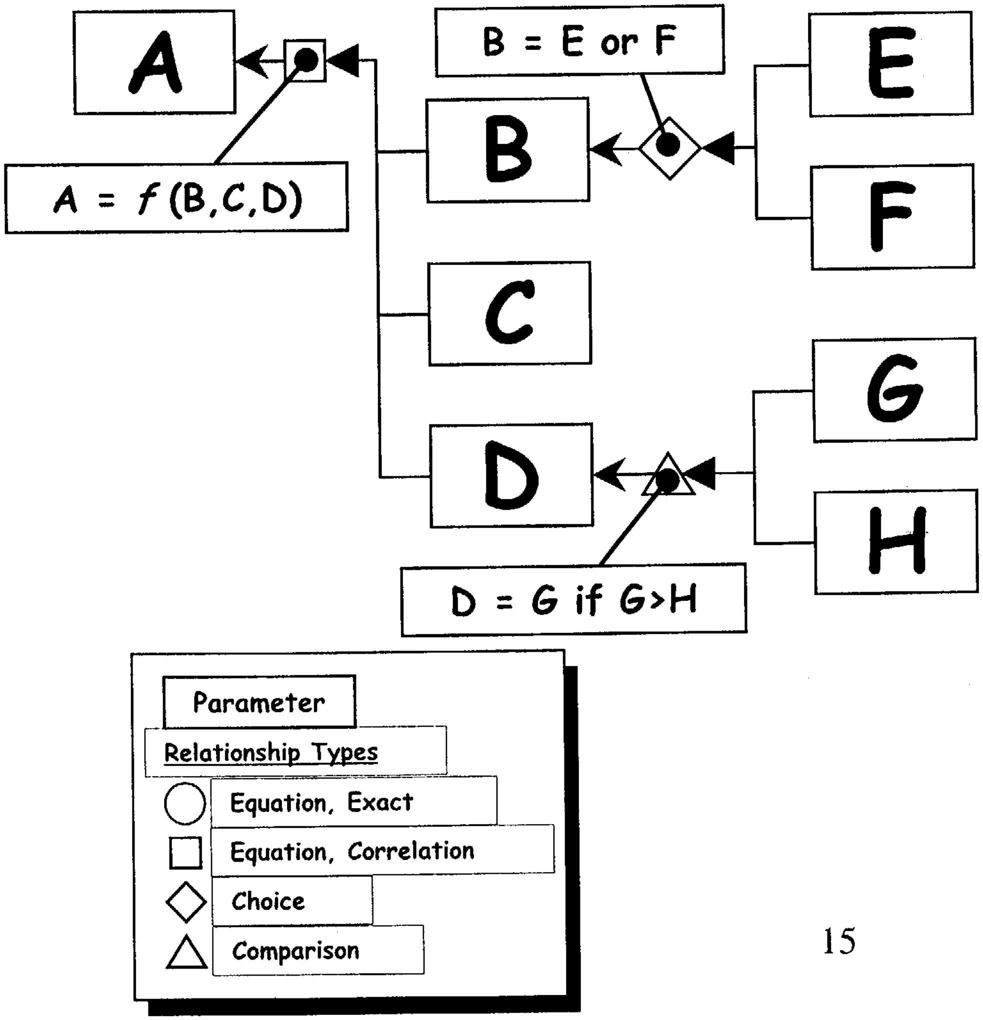
Product:

- ➢ Recommended program structure for FAVOR
  - ◆ Profs. Modarres and Mosleh (NRC funding)

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

Root Cause Diagrams

- Displays complex process in a logical format
- Provides big picture view while preserving details
- Builds consensus
    - Provides a common language for discussion
    - Allows participants to “see” their input
- Visual hierarchy
    - Streamlines critique
    - Enables understanding by non-experts
- Both parameter and model uncertainty treated
- Process ensures use of technically correct models



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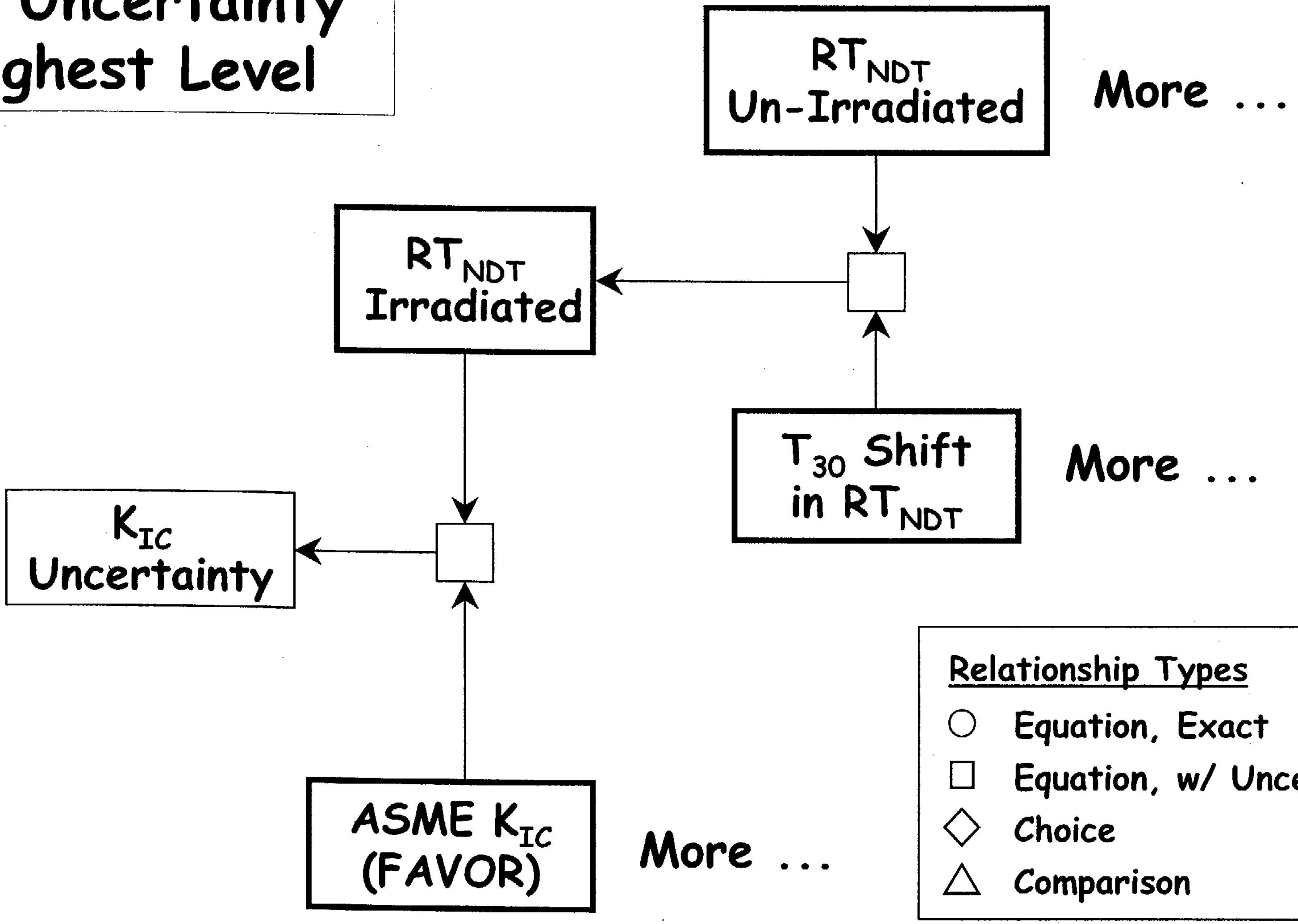
Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

K_{IC} Uncertainty
Highest Level

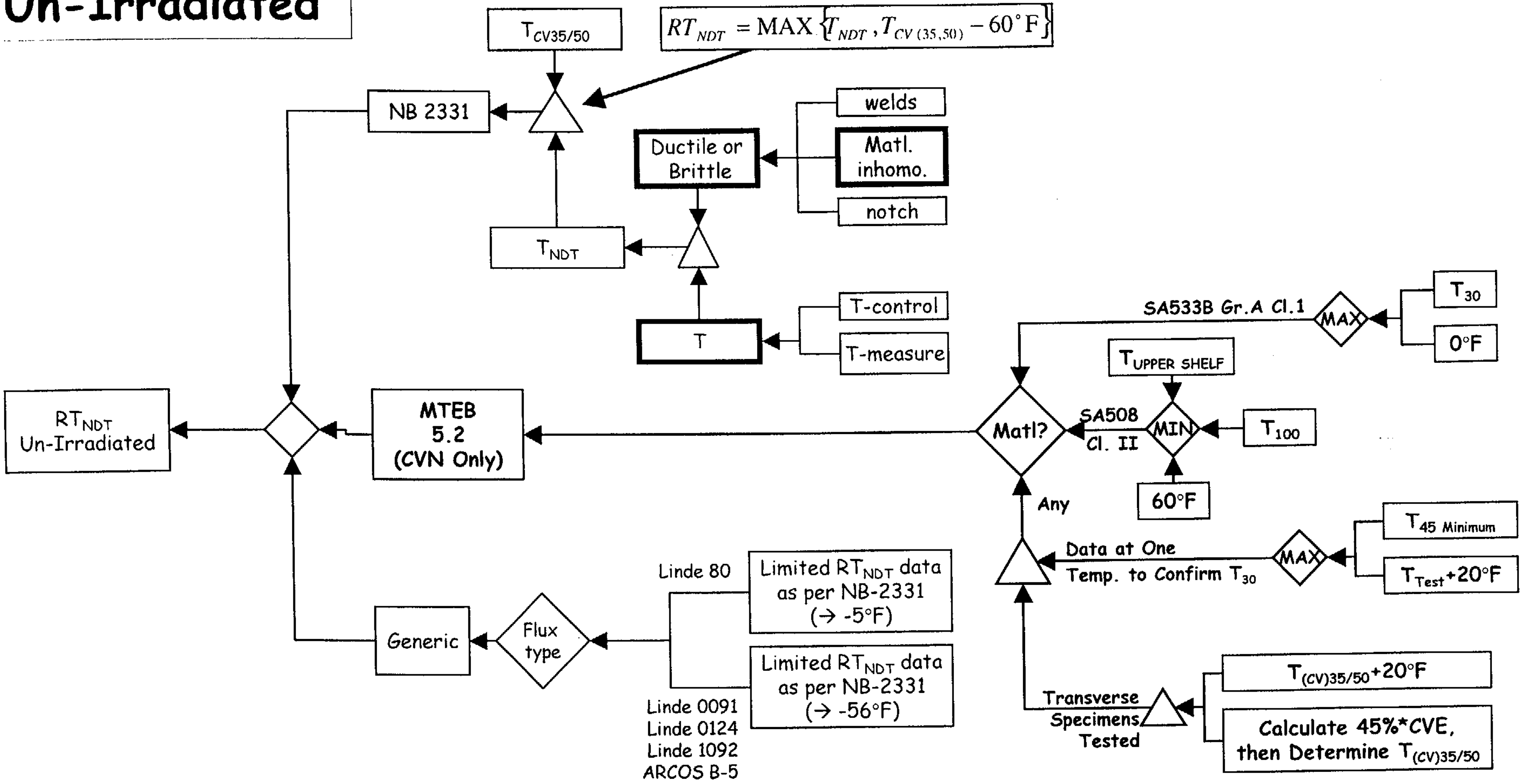


Relationship Types

- ○ Equation, Exact
- □ Equation, w/ Uncertainty
- ◇ Choice
- △ Comparison

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

RT_{NDT} Un-Irradiated



$T_{CV35/50}$

$RT_{NDT} = \text{MAX}\{T_{NDT}, T_{CV(35,50)} - 60^\circ\text{F}\}$

NB 2331

Ductile or Brittle

welds

Matl. inhomo.

notch

T_{NDT}

T

T-control

T-measure

RT_{NDT} Un-Irradiated

MTEB 5.2 (CVN Only)

Matl?

SA533B Gr.A Cl.1

MAX

T_{30}

0°F

$T_{UPPER\ SHELF}$

SA508 Cl. II

MIN

T_{100}

60°F

Any

Data at One Temp. to Confirm T_{30}

MAX

$T_{45\ Minimum}$

T_{Test}+20°F

Transverse Specimens Tested

$T_{(CV)35/50}$+20°F

Calculate 45%*CVE, then Determine $T_{(CV)35/50}$

Generic

Flux type

Linde 80

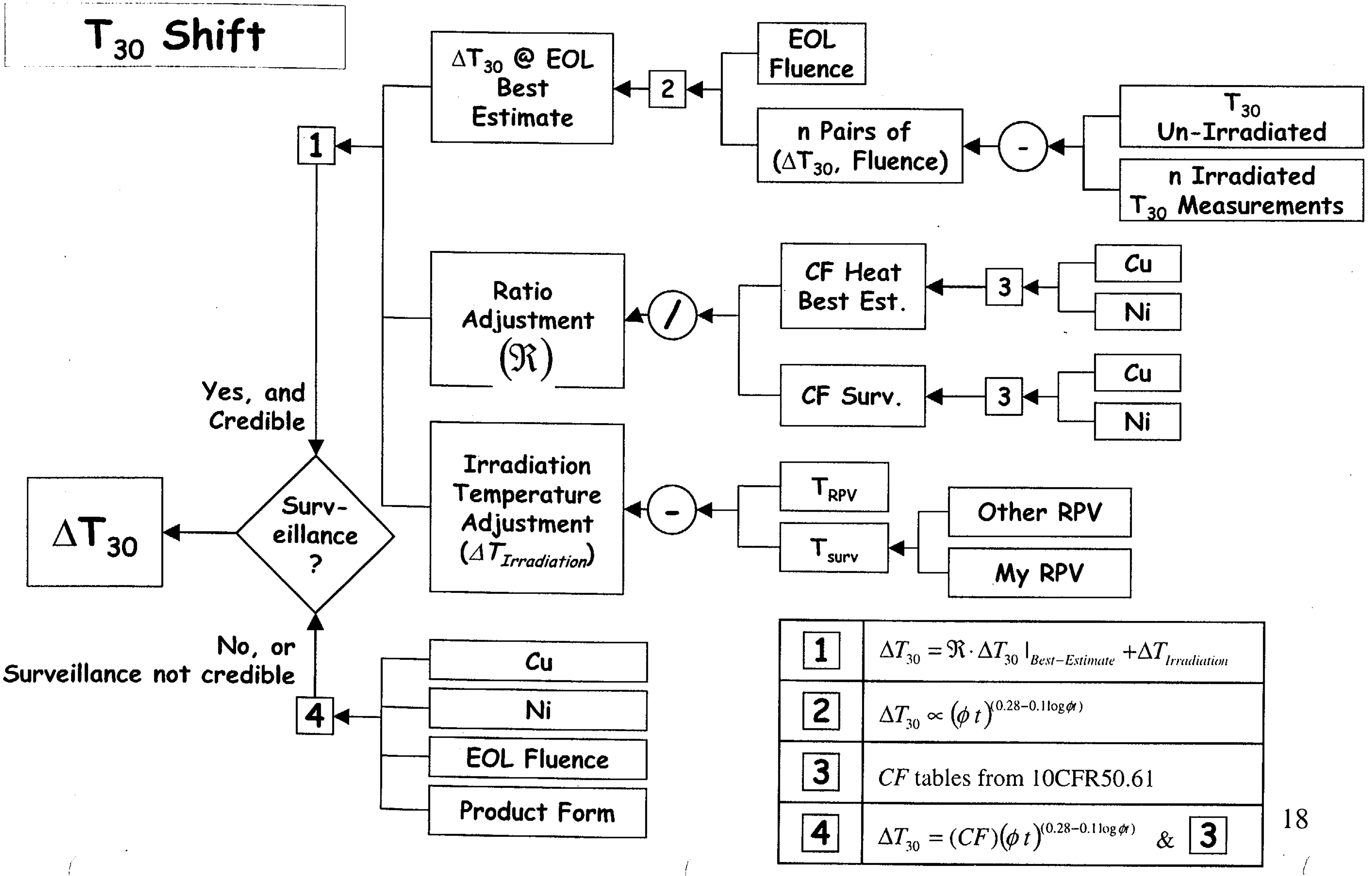
Limited RT_{NDT} data as per NB-2331 (→ -5°F)

Linde 0091
Linde 0124
Linde 1092
ARCOS B-5

Limited RT_{NDT} data as per NB-2331 (→ -56°F)

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

T_{30} Shift



ΔT_{30} @ EOL Best Estimate

EOL Fluence

n Pairs of (ΔT_{30}, Fluence)

T_{30} Un-Irradiated

n Irradiated T_{30} Measurements

Ratio Adjustment ($\Re$)

CF Heat Best Est.

CF Surv.

Cu

Ni

Cu

Ni

Irradiation Temperature Adjustment ($\Delta T_{Irradiation}$)

T_{RPV}

T_{surv}

Other RPV

My RPV

Yes, and Credible

Surv-eillance ?

ΔT_{30}

No, or Surveillance not credible

Cu

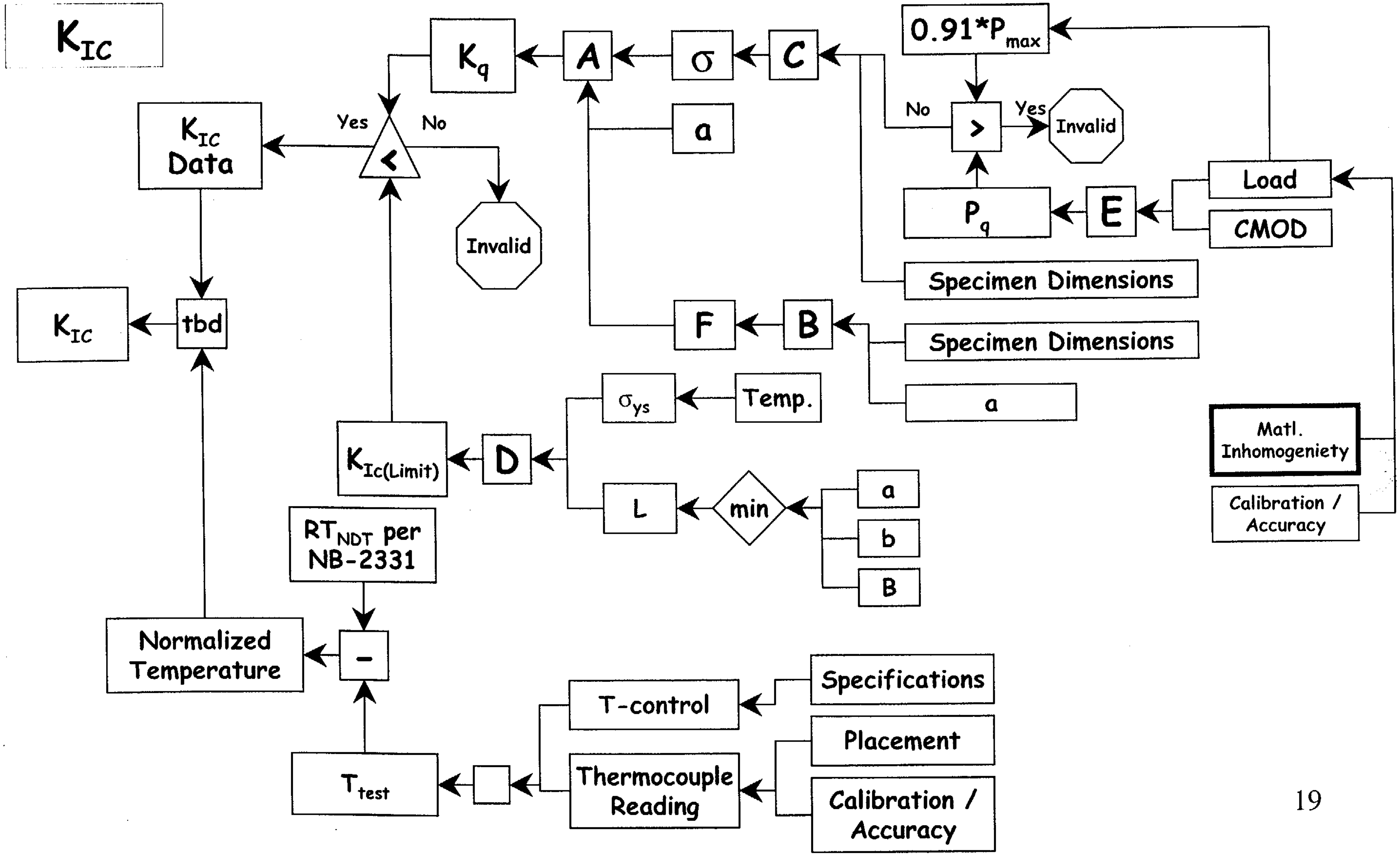
Ni

EOL Fluence

Product Form

1	$\Delta T_{30} = \Re \cdot \Delta T_{30}\vert_{Best-Estimate} + \Delta T_{Irradiation}$
2	$\Delta T_{30} \propto (\phi t)^{(0.28-0.1\log\phi t)}$
3	*CF* tables from 10CFR50.61
4	$\Delta T_{30} = (CF)(\phi t)^{(0.28-0.1\log\phi t)}$ & $\boxed{3}$

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves



K_{IC}

K_q

A

σ

C

$0.91{*}P_{max}$

K_{IC} Data

Yes

No

<

No

>

Yes

Invalid

a

Invalid

Load

P_q

E

CMOD

Specimen Dimensions

K_{IC}

tbd

F

B

Specimen Dimensions

σ_{ys}

Temp.

a

Matl. Inhomogeniety

$K_{Ic(Limit)}$

D

L

min

a

b

B

Calibration / Accuracy

RT_{NDT} per NB-2331

Normalized Temperature

–

Specifications

T-control

Placement

T_{test}

Thermocouple Reading

Calibration / Accuracy

Probabilistic Fracture Mechanics
Fracture Toughness (K_{IC} and K_{IA}) Curves

Root Cause Analysis, Summary

- **Data distributions can be entered at any level of the diagram (not JUST to the far right)**
- **Appropriately incorporates all uncertainty sources**
- **Diagrams more than schematic**
  - Represent a mathematical model
  - Basis for simulation studies
- **Separates uncertainties at “choice” nodes**
  - Eliminates double-counts
  - Impossible in a purely statistical assessment
- **Model uncertainties identified and treated**
  - Eliminates double-counts

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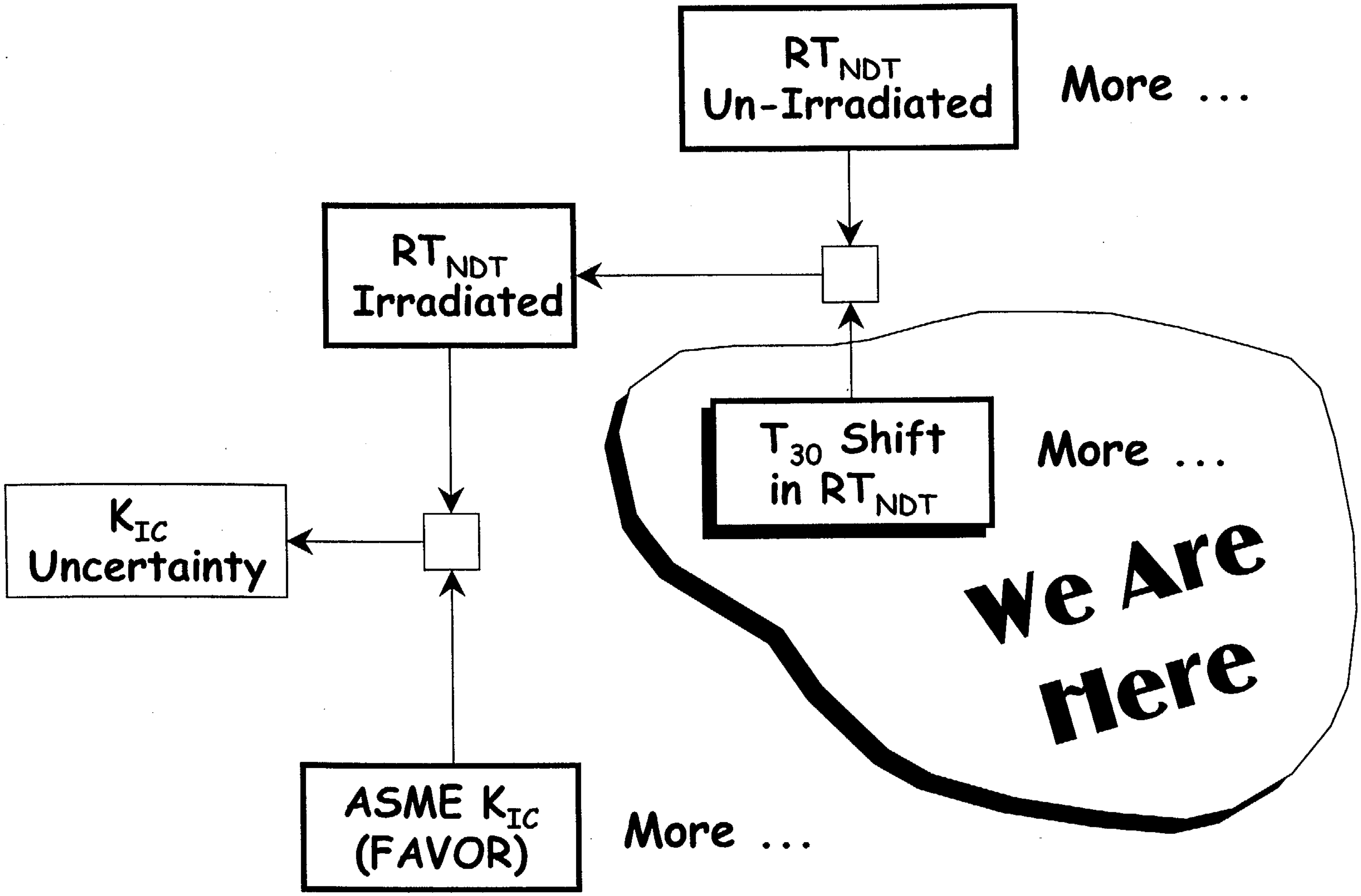
Probabilistic Fracture Mechanics
Irradiation Embrittlement Correlations



RT_{NDT} Un-Irradiated

More ...

RT_{NDT} Irradiated

T_{30} Shift in RT_{NDT}

More ...

We Are Here

K_{IC} Uncertainty

ASME K_{IC} (FAVOR)

More ...

Probabilistic Fracture Mechanics
Irradiation Embrittlement Correlations

- **<u>Objective</u>: Develop a model to predict the shift in RT_{NDT} due to irradiation embrittlement**
  - More data
    - Better coverage of primary variables
    - Longer time exposures
    - Higher fluences
  - Rigorous statistical methods
  - Physical understanding
- **Activity provides guidance to**
  - Reg. Guide 1.99, Rev. 3
  - PTS re-evaluation
- **<u>RES Staff</u>: Mark Kirk, Carolyn Fairbanks, Shah Malik**
- **<u>NRC Contractors</u>: ORNL, Modeling & Computing Services, Univ. of California/Santa Barbara**

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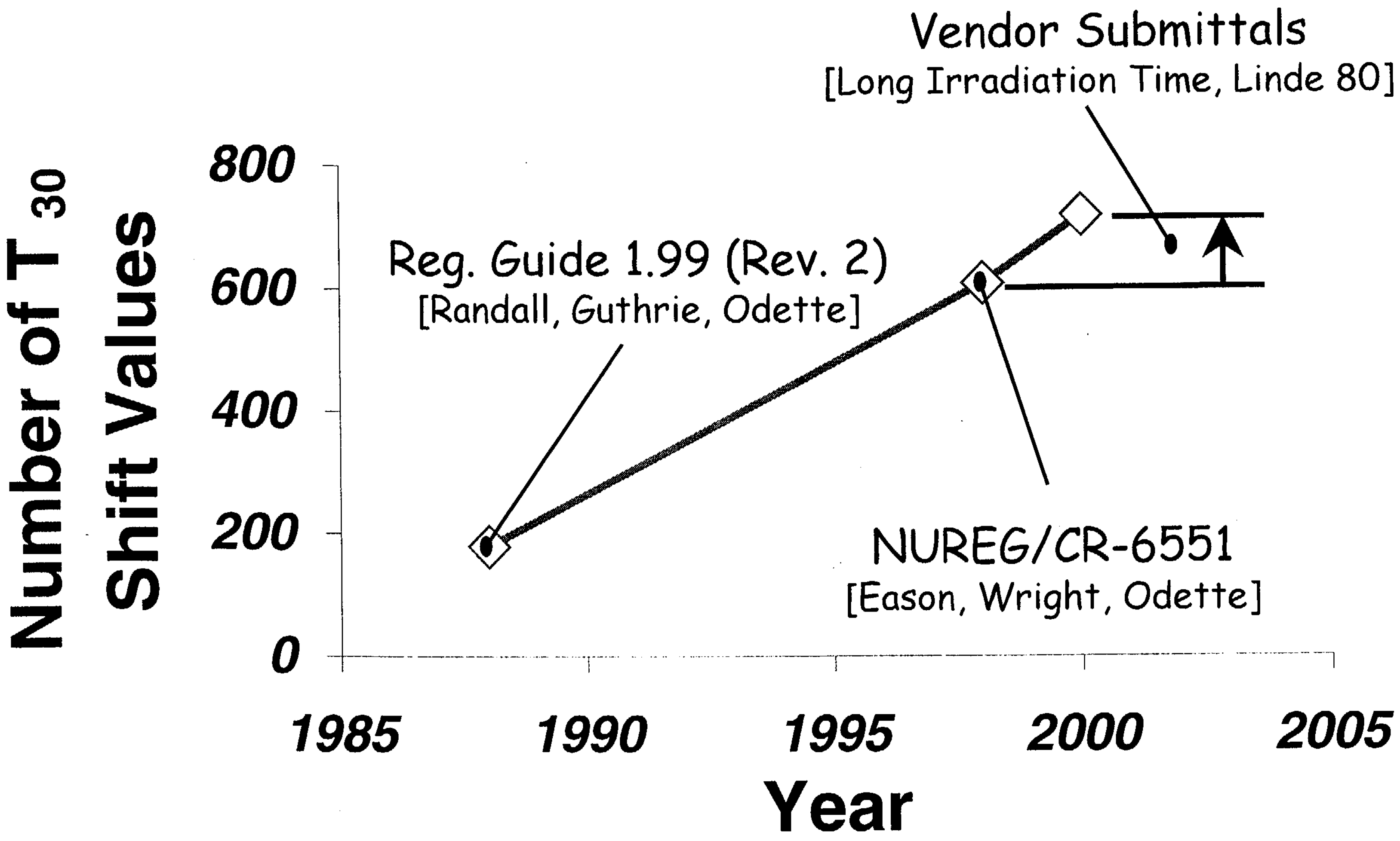
Probabilistic Fracture Mechanics
Irradiation Embrittlement Correlations



Vendor Submittals
[Long Irradiation Time, Linde 80]

Reg. Guide 1.99 (Rev. 2)
[Randall, Guthrie, Odette]

NUREG/CR-6551
[Eason, Wright, Odette]

Probabilistic Fracture Mechanics
Irradiation Embrittlement Correlations

How Have the Models Changed?

1988
RG 1.99 Rev. 2

$$\Delta RT_{NDT} = (CF) f^{(0.28-0.1\log f)}$$

1998
NUREG/CR-6551

$$\Delta RT_{NDT} = A\exp\left(\frac{2.207\times10^{4}}{T_c+460}\right)\left(\frac{\mathrm{f}}{10^{19}}\right)^{0.4132} + B(Cu-0.072)^{0.682}(1+2.35Ni)^{1.381}\left\{0.5+0.5\tanh\left[\frac{\log_{10}(\mathrm{f})-18.27}{0.849}\right]\right\}$$

- Physically motivated fits
- Separate terms for
  - Stable matrix defects (A)
  - Cu rich precipitates (B)
- Cu saturation

- Under consideration
  - Phosphorus
  - Long time effects
  - Use of surveillance as a check

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Probabilistic Fracture Mechanics Irradiation Embrittlement Correlations

- **Status**
  - Finalizing the model
    - ➢ Database frozen
    - ➢ Gating criteria for term admission established
    - ➢ Mini-basis documents being written
  - Model for PTS re-evaluation
    - ➢ Available in April / May
    - ➢ Feeds into K_{IC} / K_{IA} uncertainty analysis
  - Regulatory Guide 1.99 Revision 3
    - ➢ 12/00: Technical basis document
    - ➢ 6/01: Draft for public comment
- **ASTM E-10 evaluating the model for E900 standard**

Physical Basis	**Statistical Basis**	
	Strong (>95%)	**Weak (>70%)**
Well Accepted	**Include**	**Include**
Plausible	**Include**	**Maybe**
Not Established	**Probably**	**Exclude**

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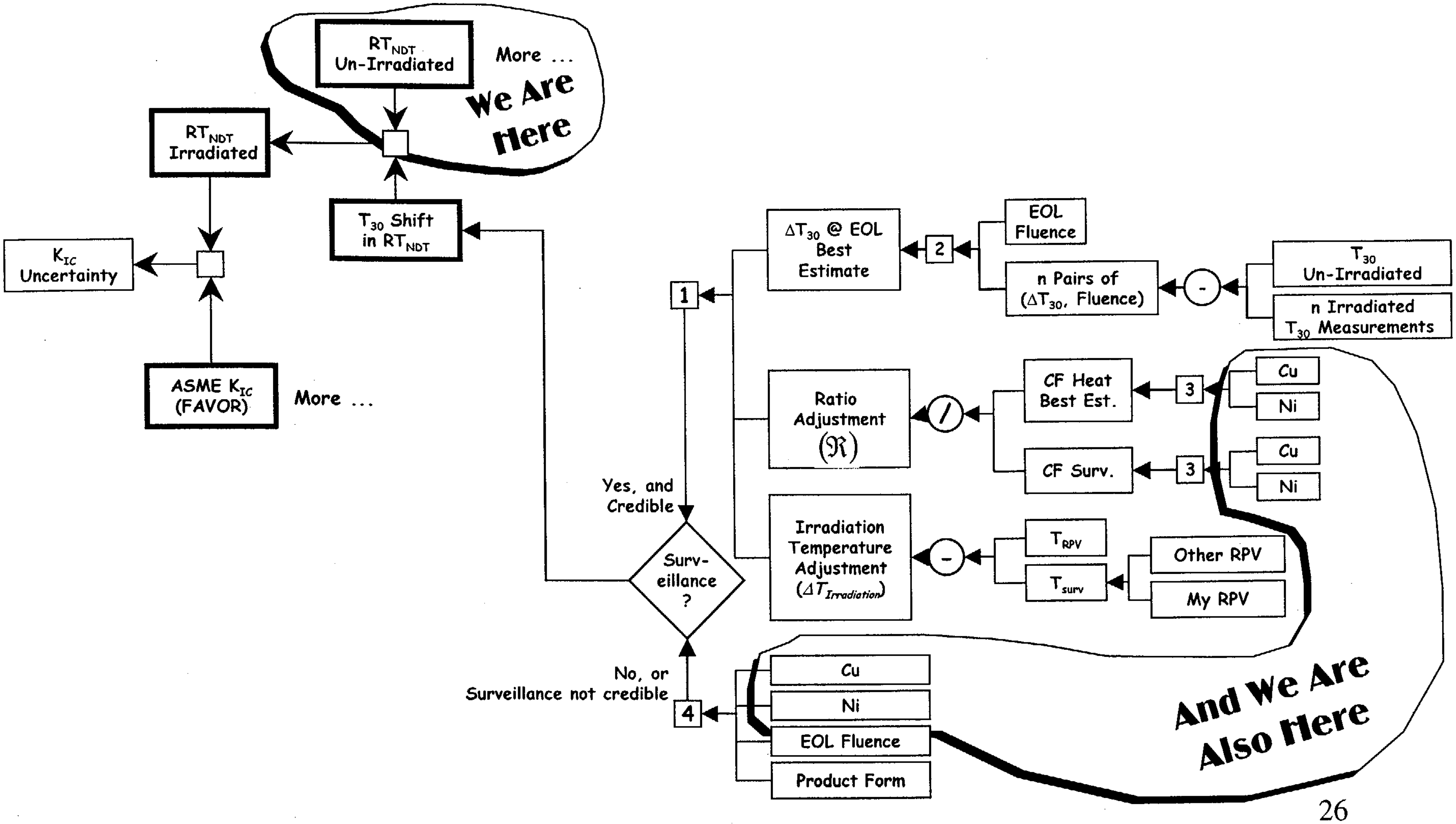
Probabilistic Fracture Mechanics Chemistry , RT_{NDTo} Distributions



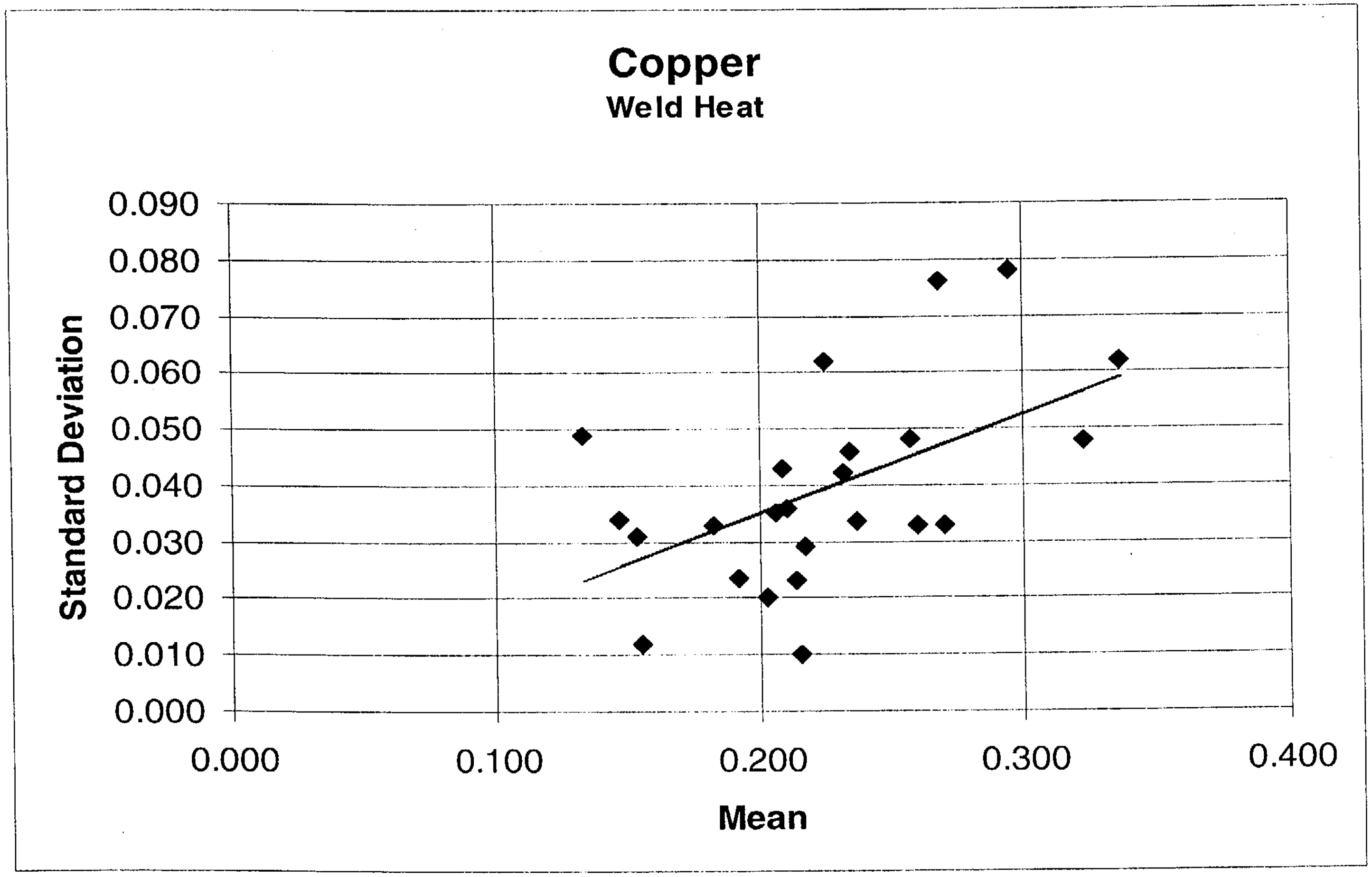
Probabilistic Fracture Mechanics Chemistry , RT_{NDTo} Distributions

- **Objective**: Using NRC and industry data, determine --
  - Heat specific distributions of:
    - Copper, Nickel, Initial RT_{NDT} (RT_{NDTo}), Phosphorous?
  - Heat to heat variability of the mean
  - Local variability
    - Within sub-region
    - Through wall-thickness (coil-by-coil ?)
- **RES Staff**: Doug Kalinousky, Tanny Santos, Lee Abramson

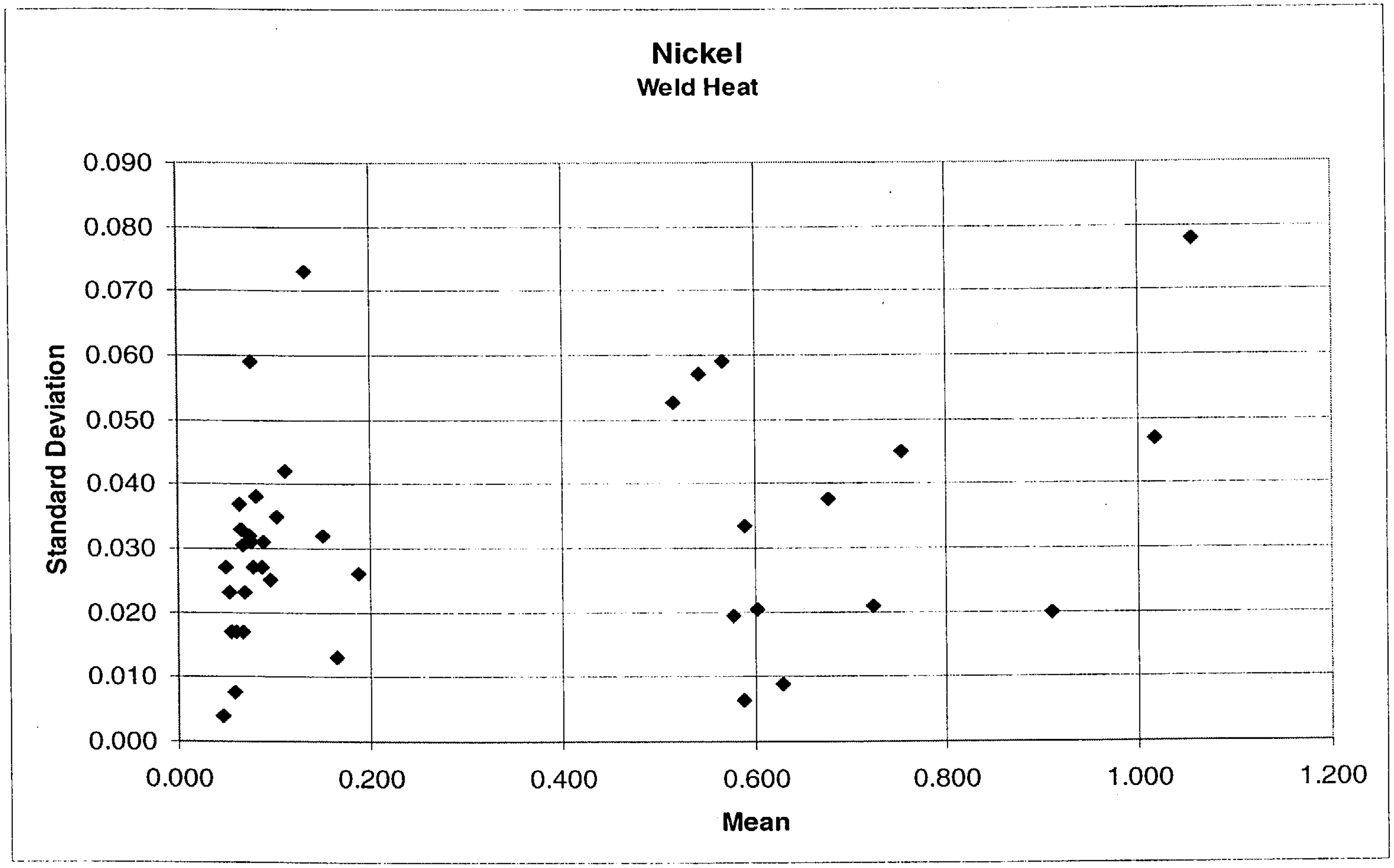
Probabilistic Fracture Mechanics
Weld Chemistry Distributions

- Considered all heats with 5 or more data
  - 24 heats for copper
  - 39 heats for nickel
- Determine heat-specific mean values and standard deviations for Cu and Ni
- Plot mean value vs. standard deviation
- Determine best fit line by linear regression

Weld Chemistry (Cu)



Weld Chemistry (Ni)



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Weld Chemistry

- Copper
  - Normal or Lognormal Distribution
  - Best estimate
    - ➢Mean of means from data
  - $Cu_{STD\ DEV} = Cu_{(Mean)} * 0.175$
- Nickel
  - Distinct populations for low and high Ni
  - Treat nickel-addition welds separately
  - Further analysis ongoing

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Weld Local Variability

- Local variability determined from a separate CE Report with measurements on 8 blocks of materials
  - 5 measurements at t/4 locations in each block
- Copper & Nickel
  - Standard Deviation of 0.01 for Ni and Cu
  - Type of distribution needs to be determined
- Through-wall variability (coil-by-coil ?)
  - Still to be determined if needed

Plate Chemistry

- Limited Amount of Data
  - 1 or 2 points per heat
- Use heat best estimate from RVID2
  - Cannot justify a distribution
  - Do not sample

Plate Local Variability

- Assumed Normal distribution
  - Limited amount of data (3 groups of 6 data/group)
- Standard deviation
  - Copper: 0.002
  - Nickel: 0.005

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Weld RT_{NDTo}

- Data from RPVDATA
- Grouped by heat
  - Used data for all heats with 3 or more measurements
  - 19 heats with 65 data
- Sample Δ_{weld} (deviation from heat mean $\equiv$ $heat_{mean}$ - measured $value_i$)
  - Add Δ_{weld} to $heat_{mean}$ values

Weld RT_{NDT0}

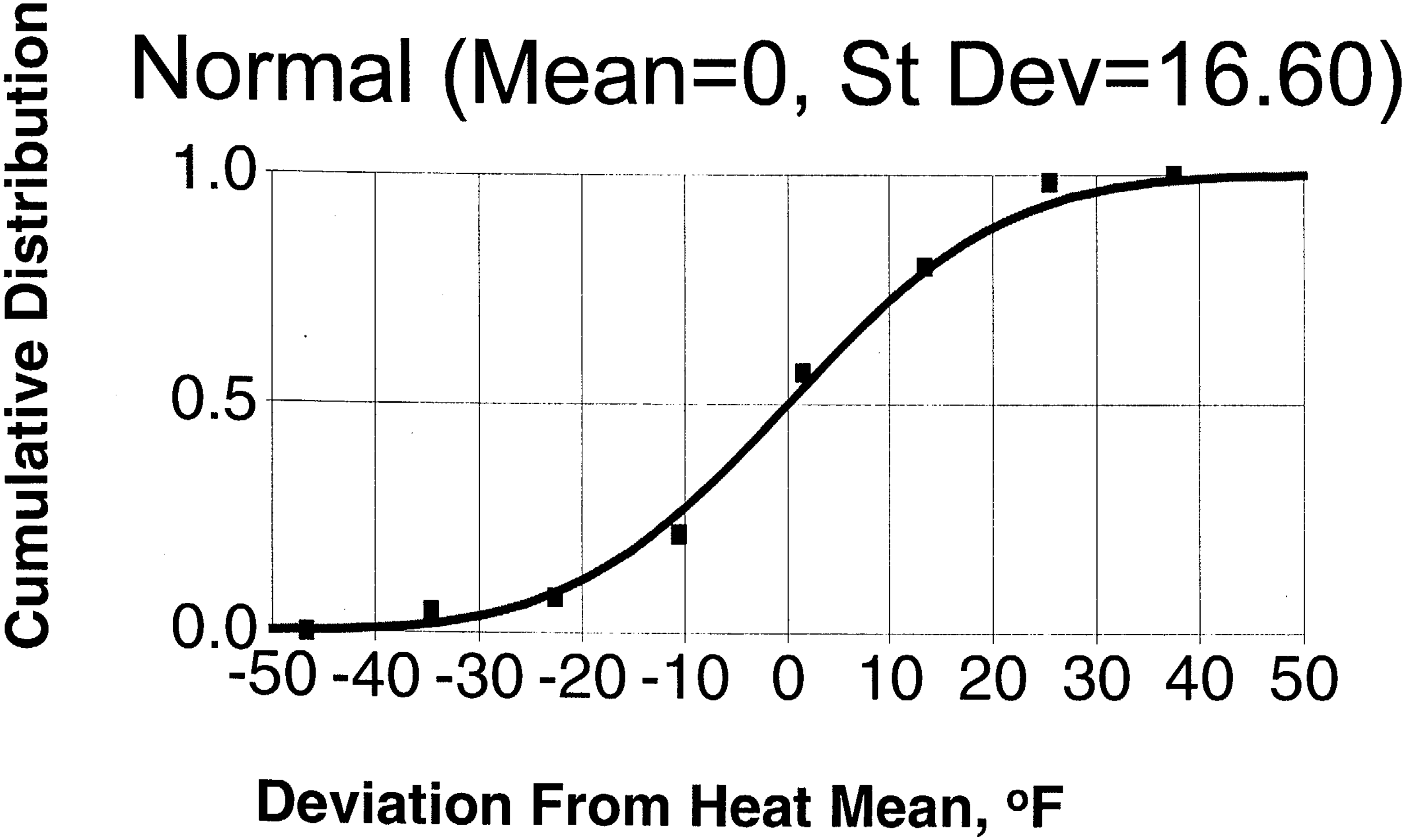
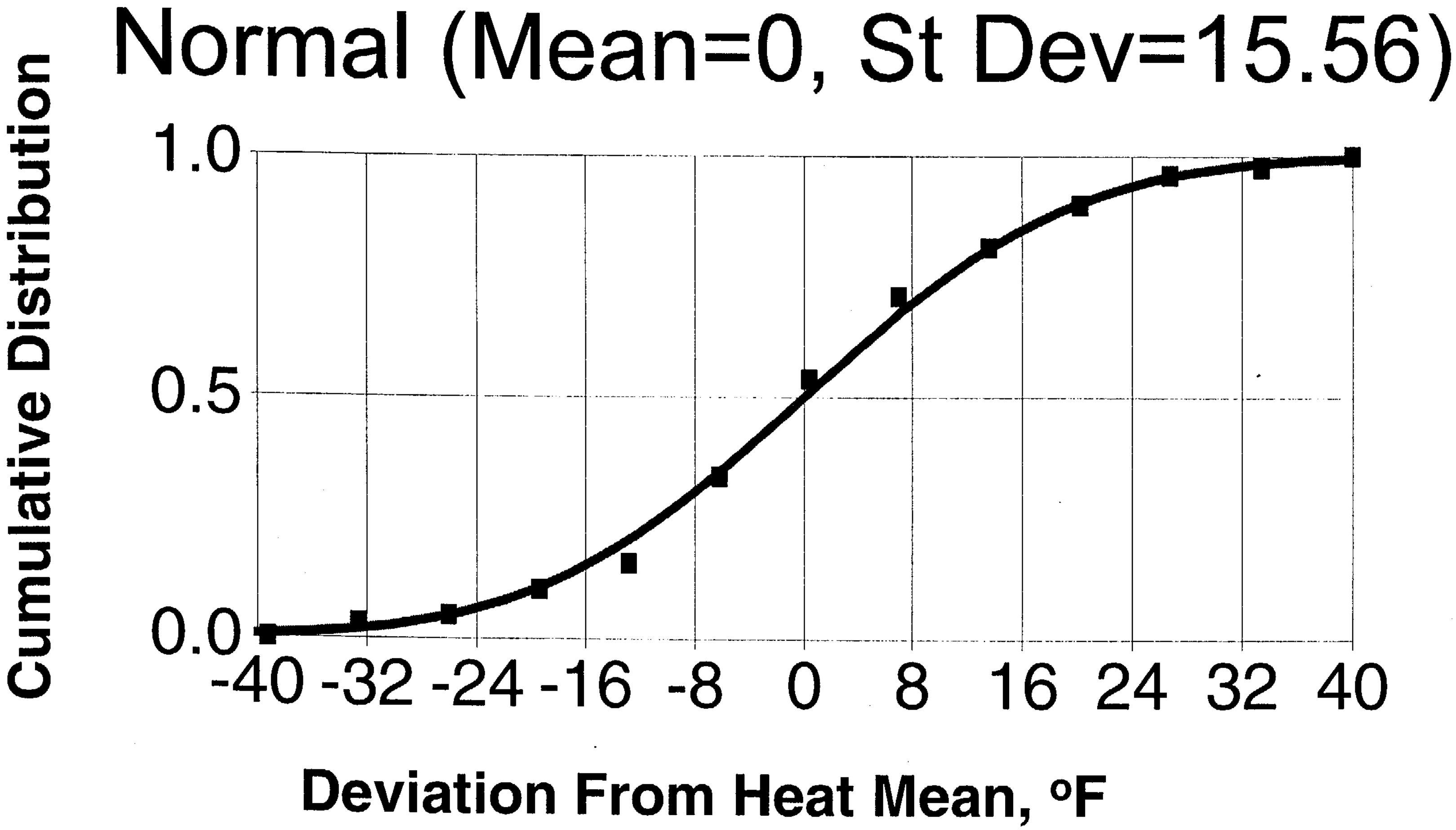

Normal (Mean=0, St Dev=16.60)
Cumulative Distribution
1.0
0.5
0.0
-50
-40
-30
-20
-10
0
10
20
30
40
50
Deviation From Heat Mean, °F

Plate RT_{NDTo}

- **Data from RPVDATA**
- **37 heats (3 or more measurements)**
  - 128 total data points
- **Same approach as Weld RT_{NDTo}**

Plate RT_{NDTo}



Probabilistic Fracture Mechanics Beltline Neutron Fluence Calcs.

- **Objective:**
  - Determine up-to-date EOL fluence maps for the plants using currently available cycle-by-cycle fuel loading histories
  - estimate 1σ uncertainty in fluence calculations
    - ➢Initial estimate of 1σ fluence = 15% of the mean
- Dosimetry Draft Guide-1053 (1999), and draft NUREG/CR-6115 methodology used
- **RES Staff:** Wiliam R. Jones
- **NRC Contractor:** Brookhaven National Lab.

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Beltline Neutron Fluence Calcs.

- Plants analyzed:
  - Palisades and HB Robinson-2 (has now opted out)
- Plants to be analyzed:
  - Oconee-1 (3/00), Calvert Cliffs-1 (7/00), Beaver Valley-1 (schedule to be finalized)
- Very refined axial, circumferential, radial grids used
  - 205 axial, 97 x 8 circum. grid for Palisades
  - 145 axial, 73 x 8 circum. grid for HB Robinson-2
- Fluence decay thru-wall, $e^{-0.24x}$, in RG-1.99 Rev. 2 is a somewhat more conservative than the calcs. (at about t/4 and greater distance from the inner surface of the vessel)

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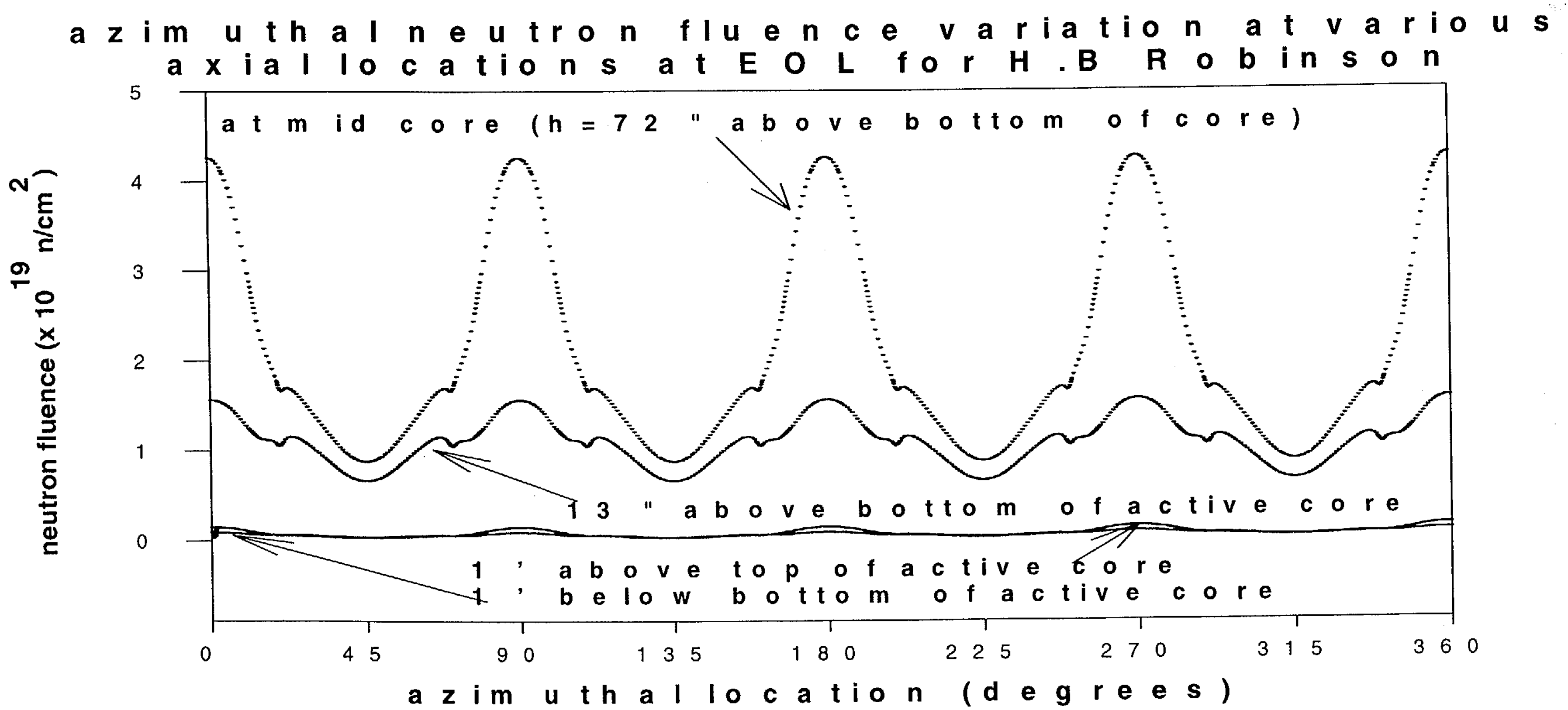
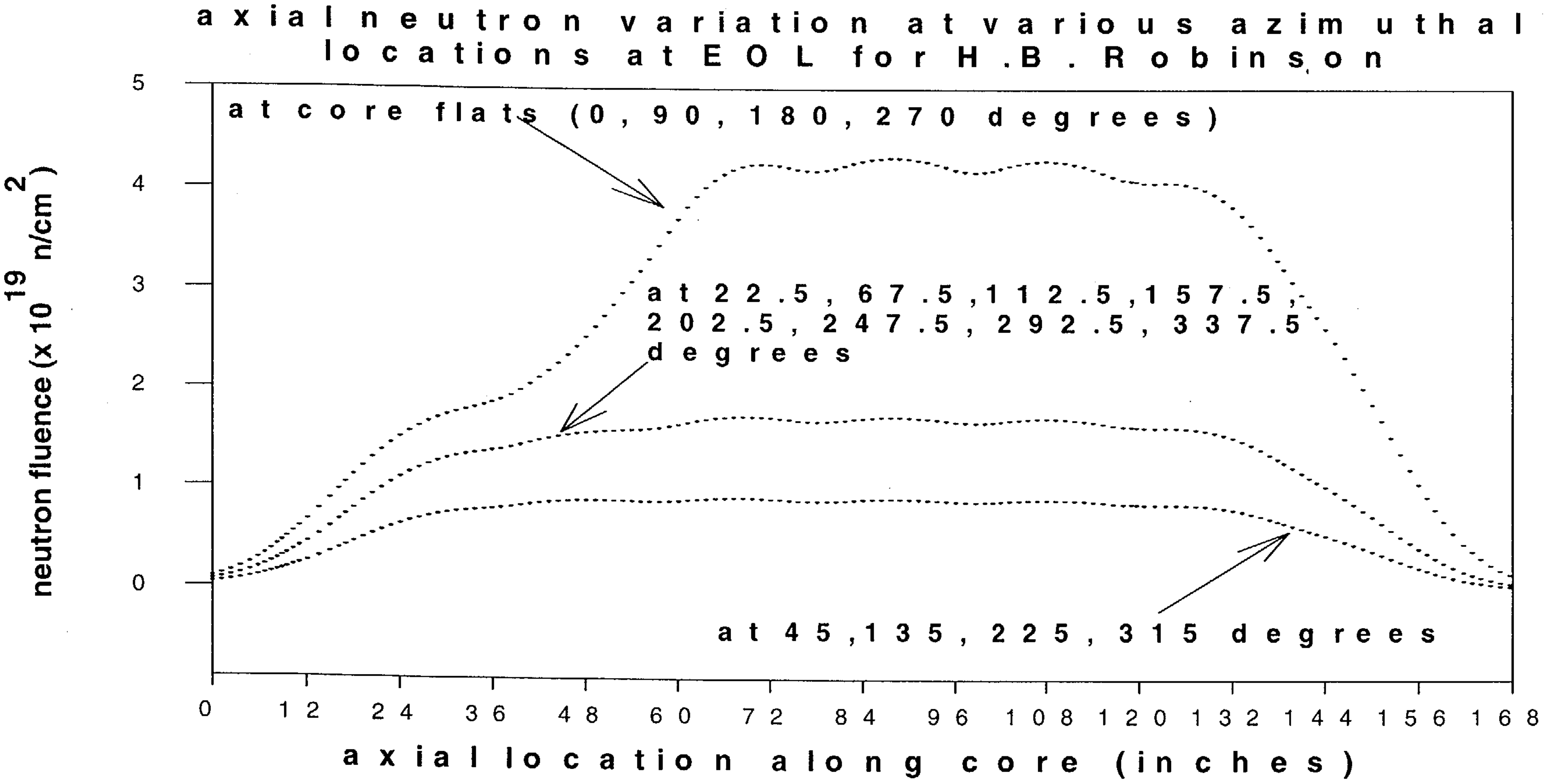
Sample Beltline Neutron Fluence Calcs. Azimuthal Variation



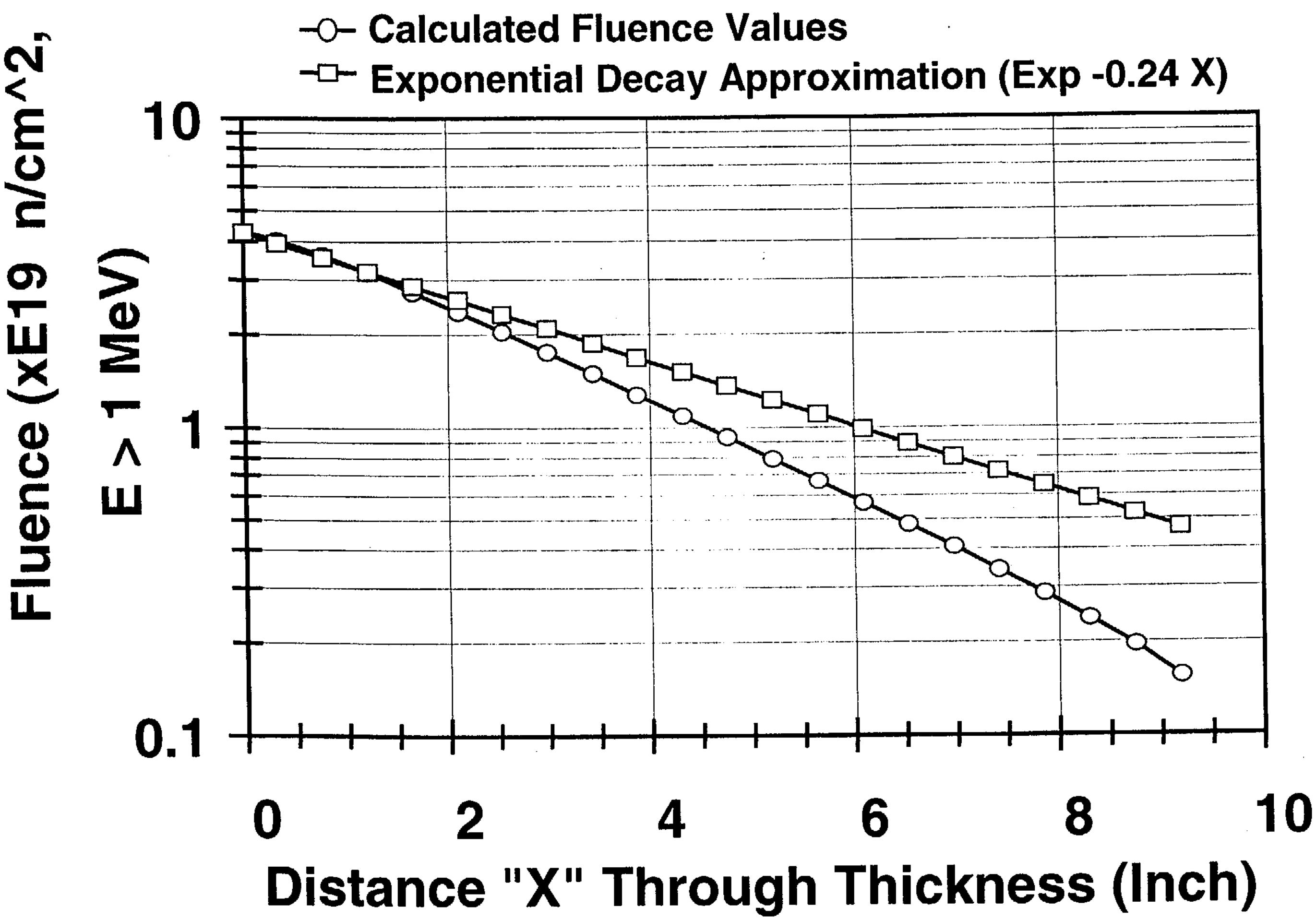
azimuthal neutron fluence variation at various axial locations at EOL for H.B Robinson

Sample Beltline Neutron Fluence Calcs. Axial Variation



Sample Beltline Neutron Fluence Calcs.

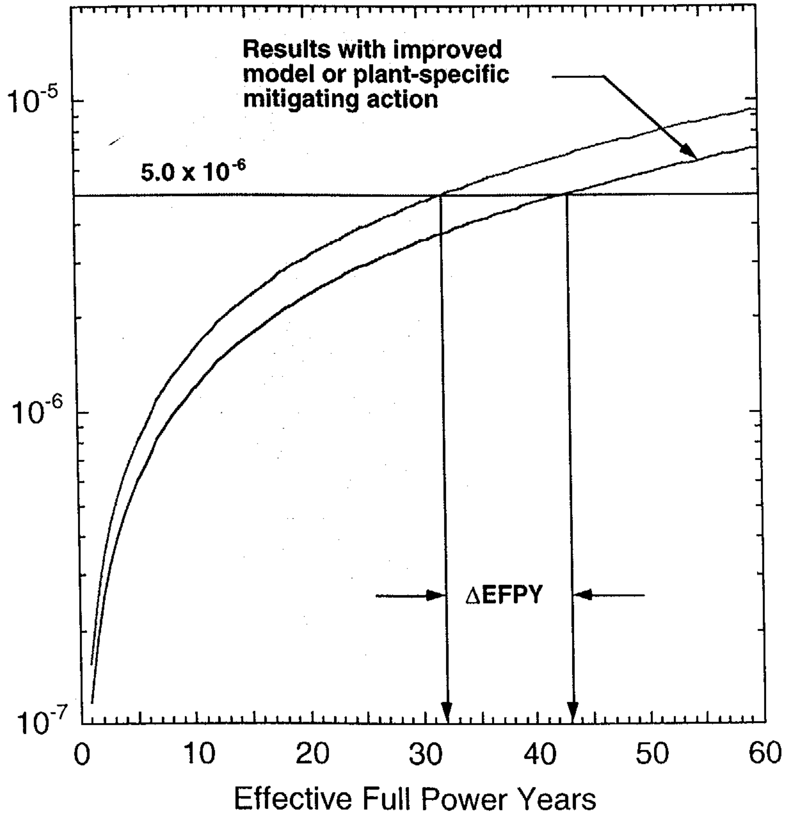
Fluence Decay Through Vessel Wall-Thickness



The Probabilistic Fracture Mechanics (PFM) FAVOR Code is Being Updated

- FAVOR (Fracture Analysis of Vessels - Oak Ridge)
- Implements refined PFM methodology and up-to-date materials data
- Makes it consistent with current PRA and thermal-hydraulics output data and methods
- Participants include
  - RES Staff: Shah Malik, Nathan Siu, Lee Abramson
  - NRC Contractors: ORNL (Terry Dickson),
    Univ. of Maryland
    (PRA: Modarres, Mosleh)

PFM Applications (via FAVOR) will Provide Answers to Two Questions



At what time in operating life does frequency of RPV failure exceed acceptable value (currently 5 x 10e-06)?

How does integration and application of advanced technology affect the calculated result?

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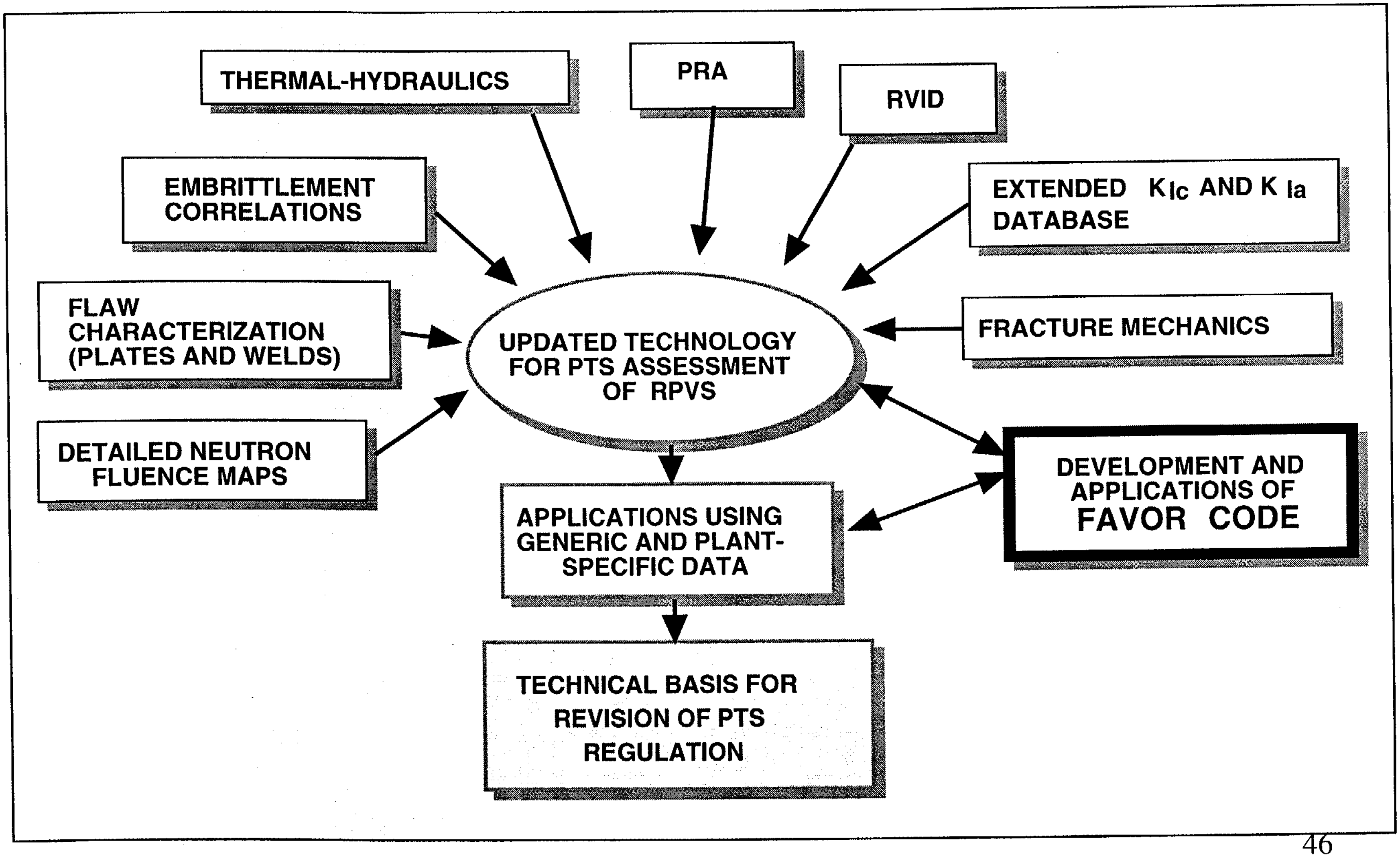
FAVOR Code Developed for Integration and Application of Updated PTS Assessment Technology



THERMAL-HYDRAULICS

PRA

RVID

EMBRITTLEMENT CORRELATIONS

EXTENDED K_{Ic} AND K_{Ia} DATABASE

FLAW CHARACTERIZATION (PLATES AND WELDS)

UPDATED TECHNOLOGY FOR PTS ASSESSMENT OF RPVS

FRACTURE MECHANICS

DETAILED NEUTRON FLUENCE MAPS

DEVELOPMENT AND APPLICATIONS OF **FAVOR CODE**

APPLICATIONS USING GENERIC AND PLANT-SPECIFIC DATA

TECHNICAL BASIS FOR REVISION OF PTS REGULATION

Development of FAVOR Code Initiated After Yankee Rowe Review in Early 1990s

- **Objectives:** - Combine best attributes of NRC funded OCA-P and VISA-II codes into a single, validated, user-friendly, fracture code that complies with regulatory criteria
  - Incorporate lessons learned from IPTS and Yankee Rowe
- **Status:** - Code now in third generation (FAVOR 99.01)
- **Plan:** - Continue development to incorporate advanced technology derived from NRC and industry R&D

Advanced Technology is Integrated into FAVOR to Support Possible Revision of PTS Regulation

- Flaw characterizations from NRC research (plates and welds)
- Detailed fluence maps
- Embrittlement correlations
- RVID
- Fracture toughness models
- Surface-breaking and <u>embedded</u> flaws
- Inclusion of through-wall weld residual stresses

Continued Development of FAVOR is a Cooperative Effort Among NRC Staff/Contractors and Industry

- Interim versions released to industry for application and evaluation
  - Version 9901 (October 1999)
  - Next version planned for May 2000
- Evaluations are vigorously assessed via frequent combined NRC/industry public meetings
- Goal is to gain understanding on PFM methodology

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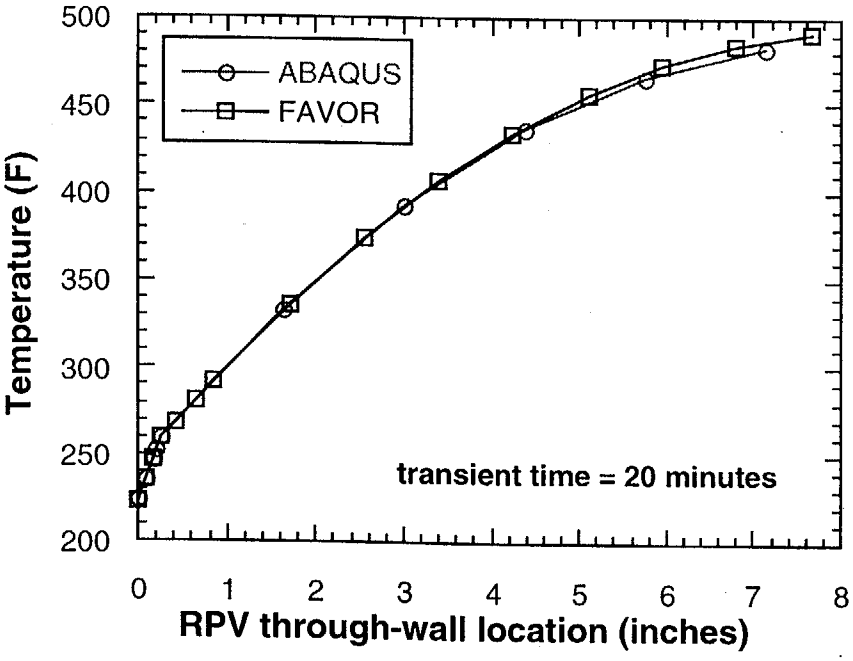
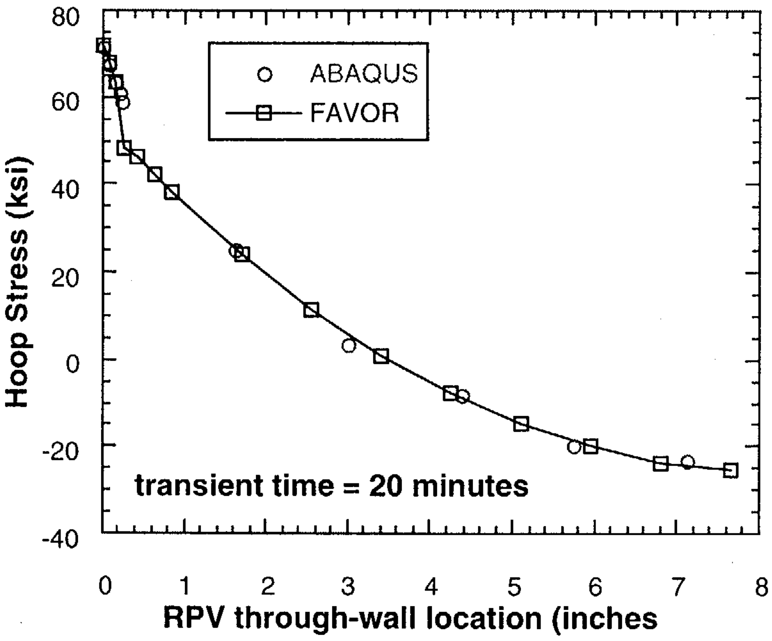
FAVOR Thermal and Stress Analysis Solutions Have Been Validated Against ABAQUS* Solutions

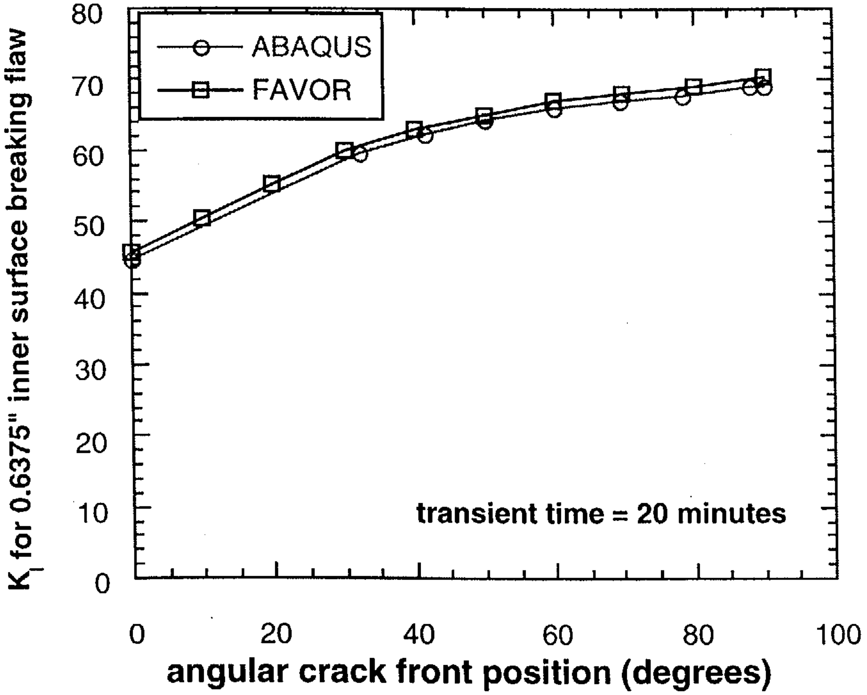




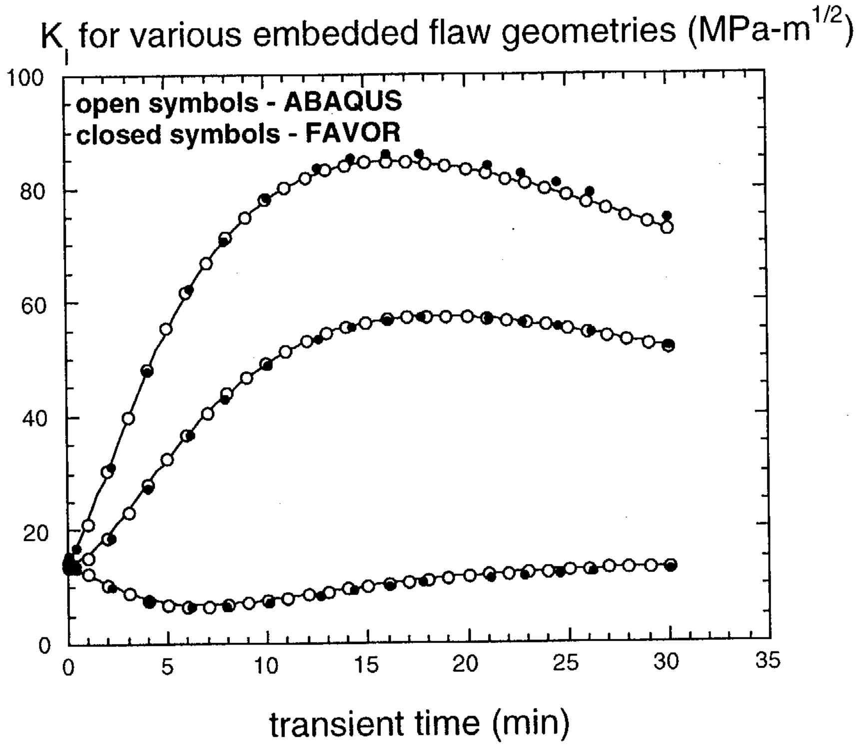
*ABAQUS is a Commercial Multi-dimensional Finite Element Code with Fracture Mechanics Capabilities

FAVOR K_I Solutions were Validated Against ABAQUS Solutions for Inner Surface-Breaking and Embedded Flaws

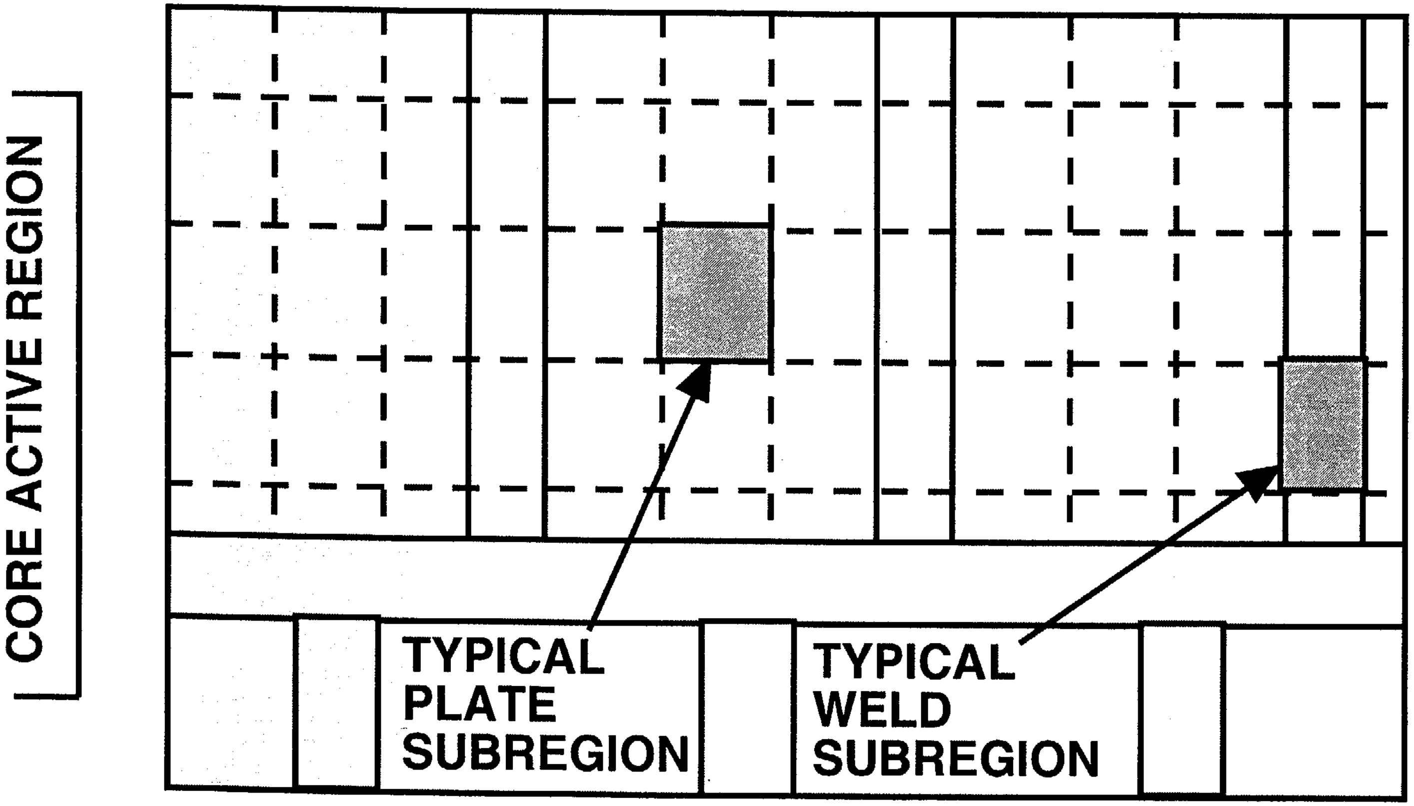
Surface-Breaking Flaws



Embedded Flaws



FAVOR Allows Analysts to Divide Beltline into Discrete Subregions to Accommodate Detailed Fluence Maps



TYPICAL VESSEL BELTLINE LAYOUT

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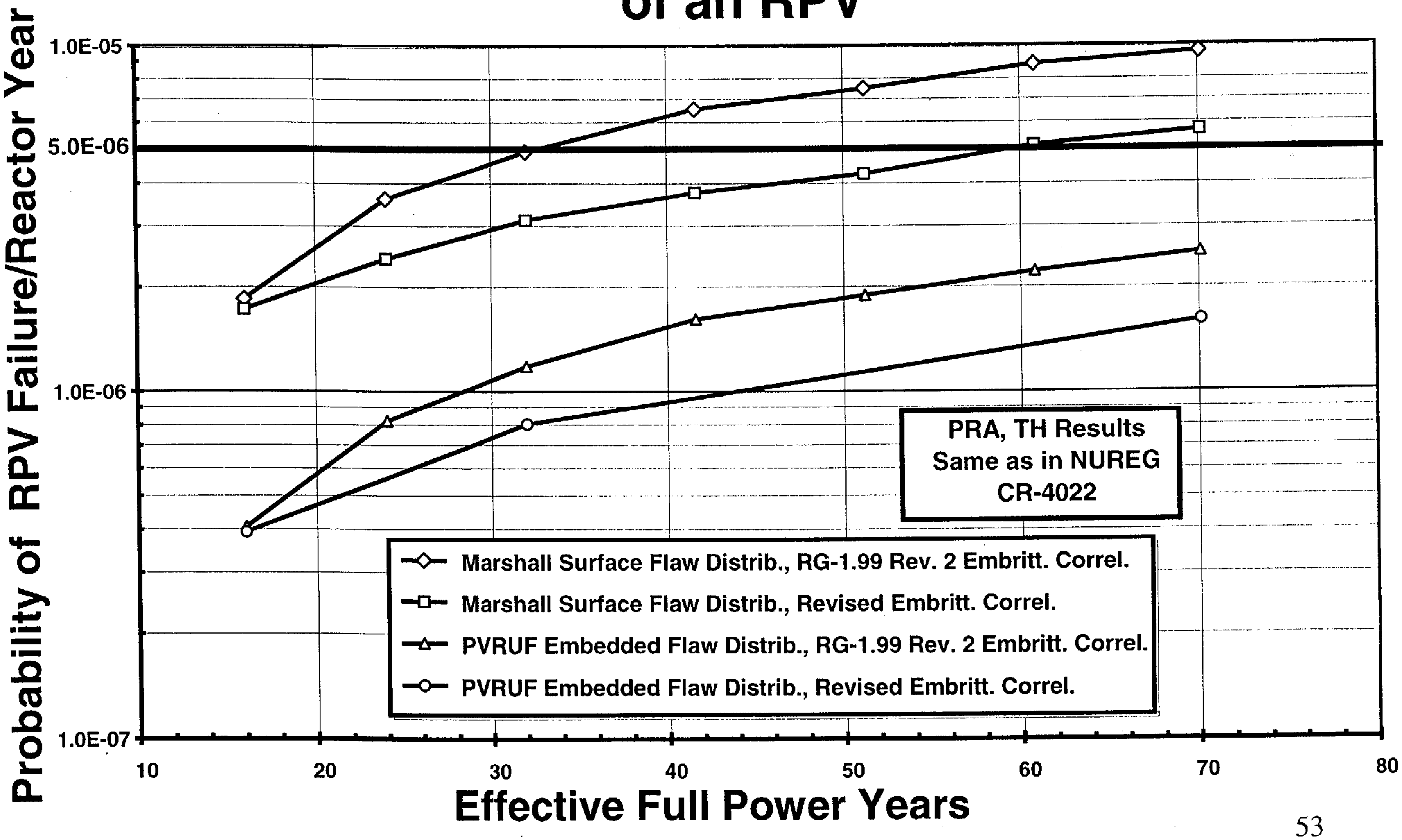
An Application of Advanced Technology Shows Potential for Extending Operating Life of an RPV



PRA, TH Results
Same as in NUREG
CR-4022

Probabilistic Fracture Mechanics Concluding Remarks

- Work in various PFM technical areas continuing vigorously
- Major technical activities expected to be completed during April/May
- FAVOR code planned for release in May
- Technical enhancements to be implemented in FAVOR as they are completed
- Coordination and interaction with PRA/Risk and TH Subgroups continuing
- HB Robinson's opting-out and additional developments have moved the schedule by 2 months or so in PFM area.

DEVELOPING A GENERALIZED FLAW DISTRIBUTION FOR REACTOR PRESSURE VESSELS

ACRS Subcommittee on Metallurgy

March 16, 2000

Presented by:

Deborah A. Jackson, Division of Engineering Technology

Lee Abramson, Division of Risk Analysis Applications

Office of Research

Topics Presented

- Objective of presentation
- Background
- Approach
- Reactor Vessel Fabricators
- Reactor Vessel Material
- Expert Elicitation Process
- Concluding Remarks

Objective of Presentation

- ➤ Discuss the need for a generalized flaw distribution
- ➤ Explain the process that is being used to develop the distribution
- ➤ Discuss the status of the expert elicitation process

Background

- US NRC is re-evaluating the guidance and criteria in the Code of Federal Regulations as it relates to reactor integrity, specifically Pressurized Thermal Shock (PTS)
- Fracture mechanics calculations are used to address the consequences of transients in a commercial nuclear power plant reactor vessel
- Reactor vessel flaw distribution is an important input to fracture mechanics calculations

Background (con't)

- Fabrication process of reactor pressure vessels presents a number of variables that can have a significant effect on the flaw distribution
  - reactor vessel plate fabrication process
  - welding process

Approach Used to Develop Generalized Flaw Distribution

- Expert judgment is needed to review, interpret and supplement available information on reactor vessel fabrication processes and reactor vessel flaw distributions
- Structured expert elicitation process
- Expert panel to resolve specific technical issues for which there is significant scientific uncertainty
- Areas of expertise of experts will allow us to address all aspects of the reactor vessel fabrication process

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Approach Used to Develop Generalized Flaw Distribution (con't)

- Reference documents for the expert elicitation process
  - NUREG 1150-Severe Accident Risks: An Assessment for Five U.S. Nuclear Power Plants - 1990
  - NUREG/CR -5411 Elicitation and Use of Expert Judgment in Performance Assessment for High-Level Radioactive Waste Repositories - 1990
  - NUREG 1563 - Branch Technical Position on the Use of Expert Elicitation in High-Level Radioactive Waste Program - 1996

Domestic Reactor Vessel Fabricators

- Combustion Engineering - 50
- Babcock and Wilcox - 21
- Chicago Bridge and Iron - 19
- Rotterdam - 9
- Societe Creusot - 2
- New York Shipbuilding - 1

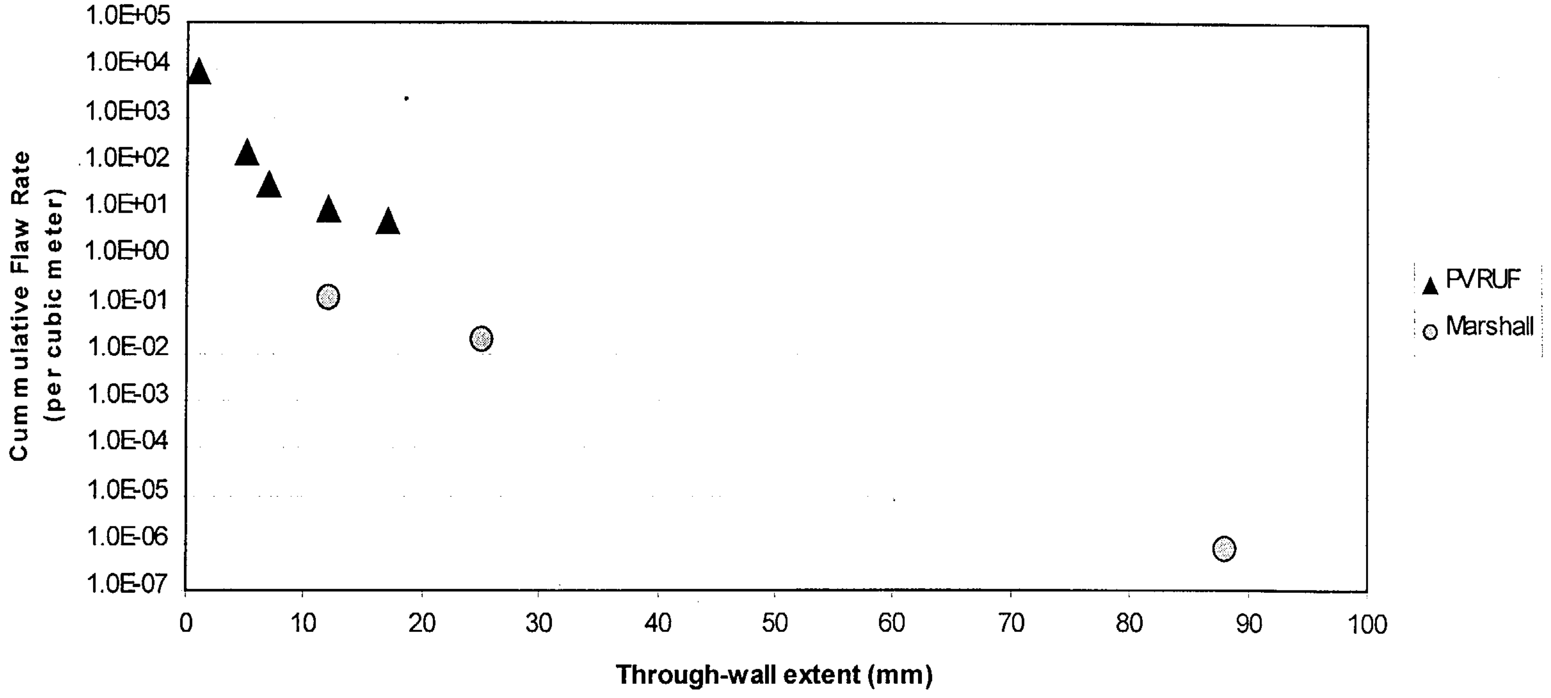
RPV Material Selected for Generalized Flaw Distribution

	Midland	PVRUF	Shoreham	River Bend 2	Hope Creek 2
Manufacturer	B&W	CE	CE	CB&I	CB&I
LWR Type	PWR	PWR	BWR	BWR	BWR
Meters of Weld	4	20	25.45	15	4
Years of Construction	1968-1974	1976-1981	1968-1974	1974-?	1972-
Fabrication Process	Circ Welds	Circ and Axial Welds	Circ and Axial Welds	Circ and Axial Welds	Circ and Axial Welds
Welds Inspected	Circ Welds	Circ Welds	Circ and Axial Welds	Circ and Axial Welds	Circ and Axial Welds
Base Metal Inspected	N	Y	In progress	In progress	In progress
NDE Inspection Completed	Y	Y	Y	In progress	In progress

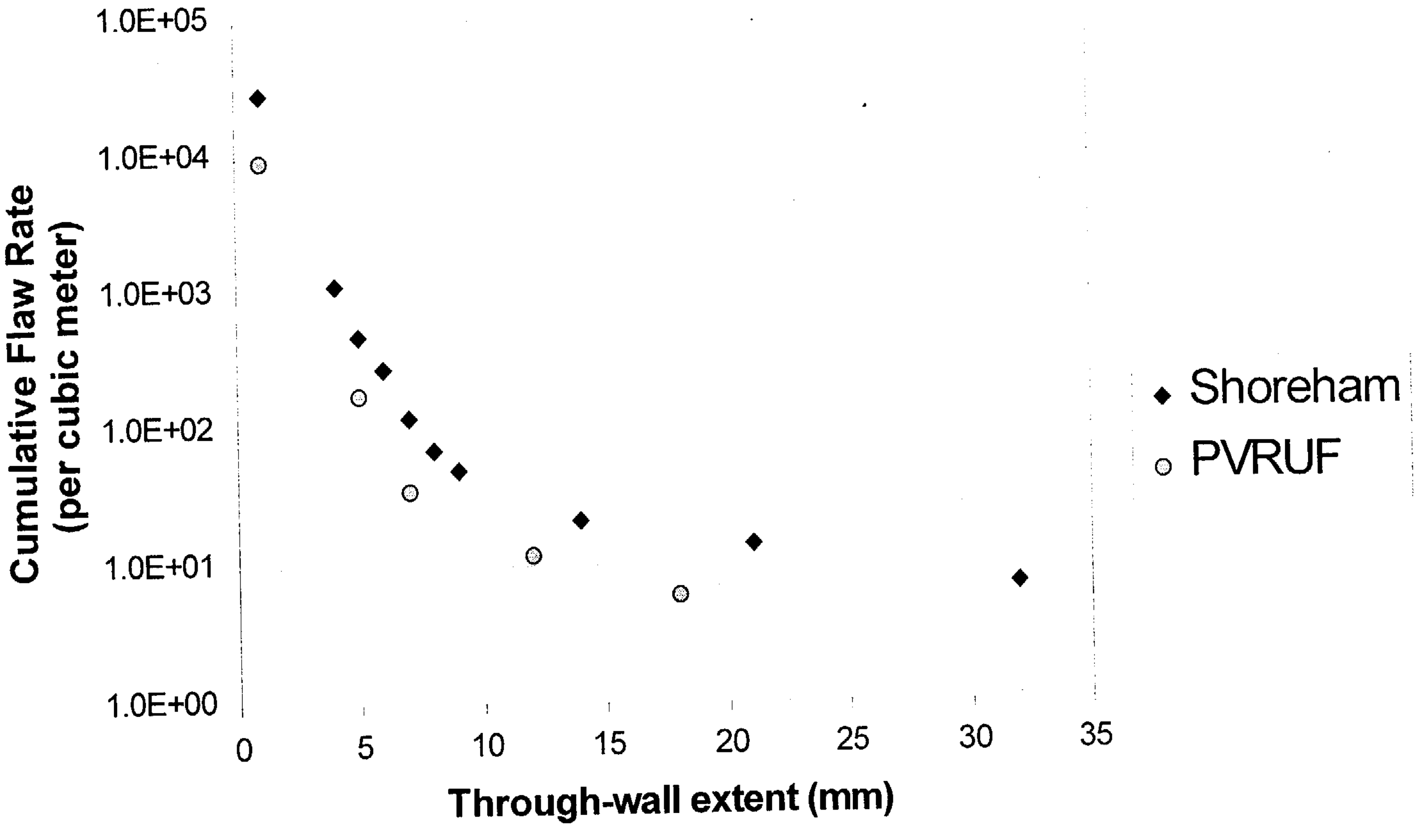
Categorization of Flaws

- Inner Region (25-mm of wall) versus outside inner region
- Volumetric versus planar
- Weld versus clad versus base metal
- Repair weld versus original weld

Comparison of PVRUF and Marshall Flaw Rates



Pacific Northwest National Laboratory Data PVRUF and Shoreham Size Distribution all Flaws



Expert Elicitation Process

- Define the specific issues/scope to be addressed
- Determine level of complexity
- Identify an expert panel
- Send strawman of issues to panel
- Panel meets to agree on scope, and issues
- Elicitation training

Expert Elicitation Process (con't)

- Identify elicitation team
- Elicitation of experts
- Technical facilitator integrator (TFI) processes results
- Expert panel meets to review responses and rationales
- TFI aggregates panel responses and summarizes rationales
- Publish results and rationales

Expert Panel

- US Navy
- Academia
- EPRI
- Independent Consultants
- Community of Retirees
  - Rolls Royce
  - Reactor vessel fabricators
  - Steel manufacturer
  - Supplier of welding materials

Areas of Expertise

- ASME Construction Code
- Failure Analysis
- Fracture Mechanics
- Metallurgy
- NDE
- Reactor Vessel Fabrication
- Reliability of Flawed Welded Structures
- Welding

Schedule

- ✓ Send Issues to Panel - 10/99
- ✓ Identify Expert Panel - 11/99
- ✓ Panel meets to discuss issues - 1/00
- ✓ Elicitation Training - 1/00
- ✓ Identify Elicitation Team - 1/00

Schedule (con't)

- Elicitation of experts - 2/00 to 4/00
- Technical Facilitator Integrator processes elicitation results – 3/00 to 4/00
- Expert panel meets to review responses and rationales - 5/00
- Technical Facilitator Integrator aggregates panel's final responses and rationales - 5/00
- Publish results and rationales - 6/00

Some of the Issues Presented to the Expert Panel

- Clarification of objective
- Definition of a flaw
- What is a generalized flaw distribution?
- What must be done to create a surface breaking flaw?
- Where has industry located surface breaking flaws?
- What are the differences in steel used in domestic nuclear power plants and naval vessels?

Some of the Issues Presented to the Expert Panel (con't)

- Are NDE results of pre-Hatch vessels less reliable than post-Hatch vessels?
- What are the limits of NDE in determining
  - the depth of a long surface flaw?
  - the length for a deep flaw?

First Meeting of Expert Panel

- January 19-21, 2000; Atlanta, Georgia
- Completed elicitation training
- Developed definition of a flaw
- Determined characteristics which were important to the development of a fabrication flaw

Elicitation Sessions

- Each expert meets individually with the elicitation team (normative expert, subject matter expert, and recorder)
- List of characteristics presented to each expert
- Experts identify and discuss pairwise interactions between characteristics
- For each characteristic, experts access relative effects of alternatives on the flaw distribution

Interaction Matrix

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Product Form (Forg, Plate, Clad, Weld)														
2. Weld Processes (Automatic & Manual)														
3. Flaw Mechanisms														
4. Field vs. Shop fabrication														
5. Repairs (Weldment, Basemetal, Cladding)														
6. Weld Procedure														
7. Weld metal materials														
8. Welder skill														
9. Inspection procedure														
10. Inspector skill														
11. Base metal properties (thickness, chem,)														
12. Surface preparation parameters (geometry)														
13. Flaw location														
14. Flaw size														

Relative Assessments of Alternatives

- For each characteristic, identify that alternative with the largest likelihood of leading to a flaw
- Compare each alternative with the highest ranked alternative
- Assess relative change in likelihood of a flaw
  - High value
  - Mid value
  - Low value

List of Characteristics

- Product Form
  - forgings
  - plate
  - cladding
  - weldment
- Weld Processes
  - automatic
    - SMAW
    - SAW
    - ESAW
  - manual

List of Characteristics (con't)

- Flaw Mechanisms
  - weldment
  - base Metal
    - forging material
    - plate material
- Field vs. Shop fabrication
- Repairs
  - weldment
  - base metal
  - cladding

List of Characteristics (con't)

- Weld Procedure
  - plate to ring
  - ring to shell
  - repairs
  - cladding
- Weld Materials
  - plate to ring
  - ring to shell
  - repairs
  - cladding

List of Characteristics (con't)

- Welder Skill
  - plate to ring
  - ring to shell
  - repairs
  - cladding
- Inspection Procedure
  - forgings
  - plate
  - weldment
  - repairs
  - cladding

List of Characteristics (con't)

- Inspector Skill
  - forgings
  - plate
  - weldment
  - repairs
  - cladding
- Base metal properties
- Surface preparation
- Flaw location

Concluding Remarks

- Expert elicitation process is complex
- This process is expected to identify significant issues in the development of a generalized flaw distribution
- Combination of the relative effects of the characteristics, and the PVRUF distribution will yield a flaw distribution for any pressure vessel
- The flaw distribution may vary significantly by pressure vessel

United States
Nuclear Regulatory Commission

Potential Revisions to PTS Acceptance Criterion

Mark A. Cunningham
Division of Risk Analysis and Applications
Office of Nuclear Regulatory Research

Presentation to ACRS
Materials and Metallurgy Subcommittee

March 16, 2000

Overview

- ❏ Provide status of planned Commission paper on PTS acceptance criterion
  - ❏ PTS acceptance criterion
  - ❏ More recent Commission guidance
  - ❏ More recent severe accident information
  - ❏ Potential revisions to acceptance criterion
- ❏ Plans to complete paper
- ❏ No letter requested at this time

PTS Acceptance Criterion

- PTS Rule issued in 1983 as adequate protection rule
- Established an acceptance criterion (embrittlement screening criterion), above which licensees are required to demonstrate pressure vessel safety
- Acceptance criterion is a frequency of a through-wall crack in the pressure vessel
  - Acceptable crack frequency -- less than $5x10^{-6}$ per reactor year

PTS Acceptance Criterion (cont.)

- Key underlying assumptions
  - Through-wall crack equivalent to:
    - large opening in reactor vessel
    - core melt
  - Containment performance not substantially impaired by PTS event

More Recent Commission Guidance

- Safety Goal Policy Statement
- Station blackout and ATWS rules
- Backfit rule
- Regulatory Guide 1.174

More Recent Commission Guidance (cont.)

- Safety Goal Policy Statement
  - Defined qualitative and quantitative goals for acceptable risk
  - Subsequent Commission decisions established a subsidiary core damage frequency goal of 1×10^{-4} per reactor year
  - Intended for generic decisions using industry-average core damage frequency and risk estimates.

More Recent Commission Guidance (cont.)

- Station Blackout and ATWS Rules
  - Developed as cost-beneficial safety enhancements
  - Used probabilistic goals for the acceptable frequency of core-damage accidents
    - Goal was $1x10^{-5}$ per reactor year
  - Justified on averted offsite risk basis

More Recent Commission Guidance (cont.)

- Backfit Rule (and Regulatory Analysis Guidelines)
  - Includes initial screening on potential reductions in CDF and conditional probability of early containment failure
  - Uses screening criteria based on the Safety Goal QHOs and subsidiary CDF goal
  - Uses final decision criteria based on averted public risk

More Recent Commission Guidance (cont.)

- Regulatory Guide 1.174
  - Describes a set of general principles for risk-informed license changes
  - Provides probabilistic guidelines defining acceptable changes in CDF and LERF
    - Consistent with Safety Goals and Regulatory Analysis Guidelines

More Recent Severe Accident Information

- ☐ Major improvements in understanding accident phenomenology since rule established
  - ☐ NUREG-1150
  - ☐ Direct containment heating analyses

- ☐ Impact on containment performance issues in PTS accidents
  - ☐ Dynamic loadings on core and vessel internals
  - ☐ Dynamic loadings on reactor vessel and piping
  - ☐ Containment pressure loadings
  - ☐ Dispersal and coolability of core material
  - ☐ Availability of containment engineered safety features

Potential Revisions

Alternatives focusing on core damage frequency

☐ Apply goals used for Station Blackout and ATWS Rules

Alternatives focusing on core damage frequency and LERF

☐ Develop and apply "reverse" backfit test

☐ Apply RG 1.174 principles and acceptance guidelines

Potential Revisions (cont.)

- Apply goals used for Station Blackout and ATWS Rules
  - Goals:
    - CDF of 1×10^{-5} per reactor year
    - Offsite consequence risk
  - Establishes consistency among three principal risk-informed rules
  - Permits increase in acceptable PTS core damage frequency
  - Includes no explicit consideration of LERF/CCFP

Potential Revisions (cont.)

Develop and apply "reverse" backfit test

- Provides most clear link between the present backfit process and a new "reverse" backfit process
    - Use or adapt Regulatory Analysis Guidelines to determine if burden reduction outweighs risk increase
- Development of reverse backfit analysis process would take significant effort
    - Adequate protection issue
    - Policy decision

Potential Revisions (cont.)

Apply RG 1.174 principles and acceptance guidelines

- Ensures consistency with current Commission policy
- Requires consideration of LERF
  - Physical process uncertainties
  - Potential for containment-specific acceptance criteria

Future Plans

- Develop draft Commission paper (March)
  - Basis for original acceptance criterion
  - More recent Commission guidance
  - More recent accident information
  - Potential revisions
    - Including discussion of containment performance issues
  - Recommendation
- Discuss with ACRS/CRGR (April/May)
- Transmit to Commission (May)

United States
Nuclear Regulatory Commission

PRA for PTS Rule Revision

H. Woods, N. Siu, M. Cunningham

Office of Nuclear Regulatory Research
U.S. Nuclear Regulatory Commission

W. Galyean

Risk and Reliability Assessment Department
Idaho National Engineering and Environmental Laboratory

Presentation to the ACRS Subcommittee on Reactor Safeguards,
Materials and Metallurgy
March 16, 2000

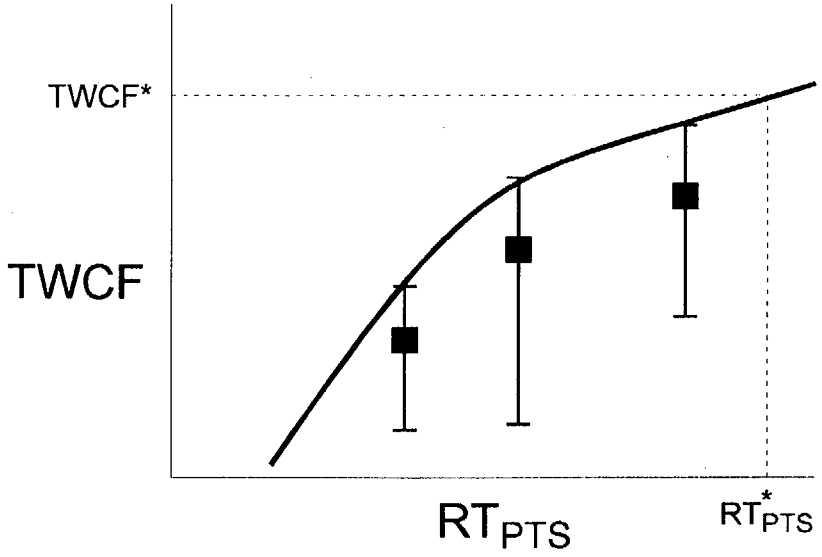
PRA Objective

Support development of technical basis for revised rule

- Ensure overall process is coherent, risk-informed
  - Appropriate integration of T/H, PFM, and PRA
  - Consistent treatment of uncertainties
- Support development of screening criteria
  - Derivation of embrittlement criteria from risk figures of merit
  - Criteria for risk figures of merit
  - Treatment of qualitative issues (e.g., defense in depth)
- Update old PTS/PRA studies
  - Reflect changes to study plants
  - Reflect changes to PRA state of the art, knowledge base
  - Address other plants

Overall PTS/PRA Analysis Approach

- Estimate PTS-induced through-wall crack frequencies (TWCFs) for 4 plants, including uncertainties
  - Develop PTS/PRA models for Oconee and Beaver Valley
  - Review PTS/PRAs for Calvert Cliffs and Palisades
  - Resolve inconsistencies, generalize results to at-risk population
- Develop TWCF vs. RT_{PTS} relationship, e.g.,



Open Questions - PTS/PRA Analysis Approach

- Treatment of internal fires and floods and external events
- Relationship of TWCF and CDF
- Treatment of LERF

PTS PRA Model Development

- **Build on the original IPTS PRA analyses**
- **Improve by:**
  - **Utilizing the results and insights of the original PRA**
  - **More thorough review of potential initiating events and operating experience**
  - **Update to current plant design, procedures, and operational practices; use current reliability estimates (including initiating event frequencies)**
  - **Generally update to current state-of-the-art; improve on state-of-the-art in HRA**

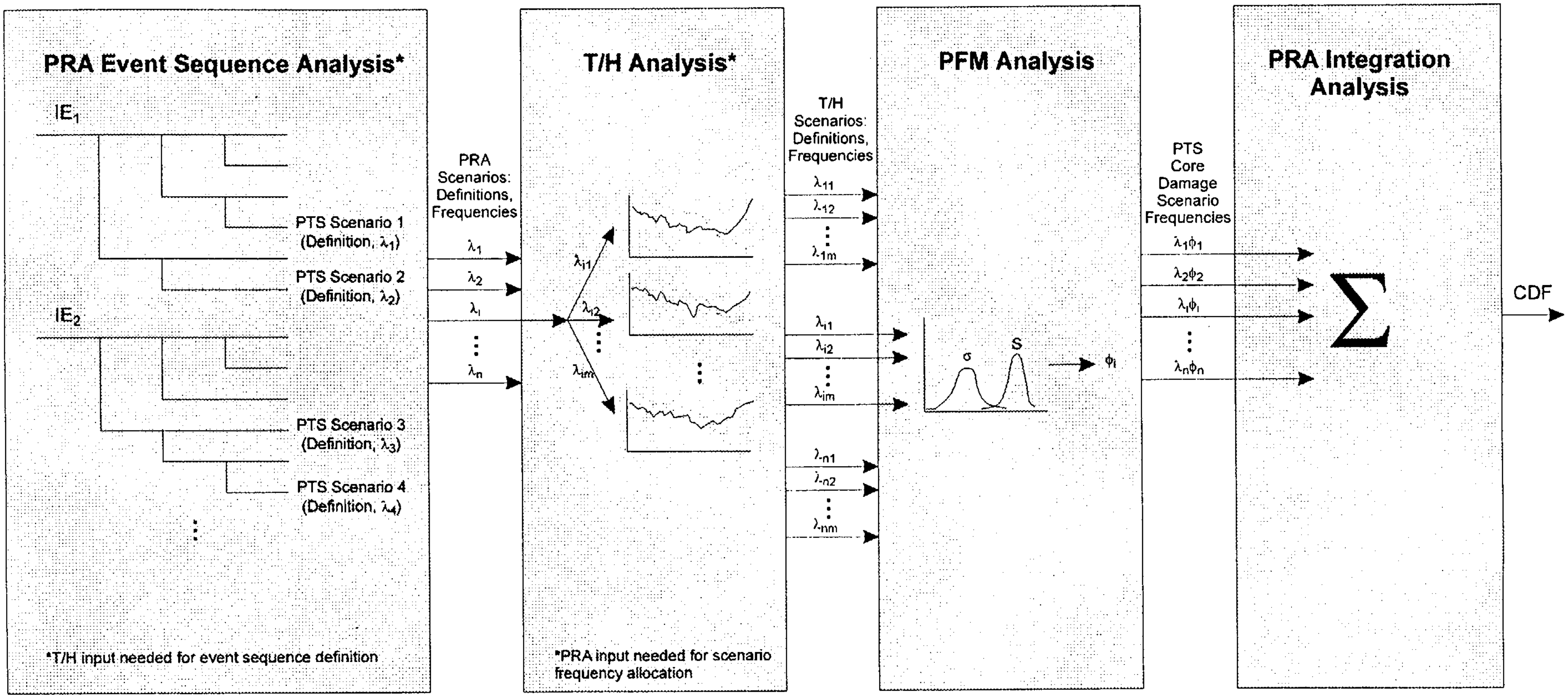
Initiating Events

Initiating Event	Oconee IPTS (events/yr)	This Analysis (events/yr)	Comments
Reactor/Turbine Trip	6	0.9	
Excessive Main Feedwater (MFW) At power Hot standby, zero power (HZP)	0.1	0.1	By itself, might be minor concern. (Likely high probability of recovery.)
Large Steamline Break (at power) Small Steamline Break (at power)	1E-3 1E-2	1.3E-2	Prototypical PTS concern
Large Steamline Break (HZP) Small Steamline Break (HZP)	N/A	TBD	Could be worse than at power case, but small exposure time.
Loss of CCW/SW	N/A	9E-4	Fails HPI and feedwater
Loss of AC/DC Bus	N/A	6E-3/1E-3	Plant-specific
Loss of Instrument Air	N/A	5E-3	Could affect MFW and TBVs
Failure of Integrated Control System	N/A	TBD	Plant-specific
Inadvertent Safety Injection	1E-2	3E-2	HPI
SBLOCA	0.1	4E-3	May be dominant contributor
MBLOCA	1E-2	4E-5	Need high RCS pressure for PTS
SGTR	1E-2	5E-3	Motivates RCS depressurization
Loss of MFW	0.5	7E-2	Depends on operator response

Human Reliability Analysis

- HRA recognized to be an important source of uncertainty in previous studies
- Issue: addressing the role of operator decision making in balancing need to keep the core covered and the need to avoid overcooling the core
- Approach
  - Integrate ATHEANA methodology into PTS/PRA analyses for the NRC analyses
  - Support analysis with development of event database
  - Use results to focus reviews of utility analyses and to develop an integrated picture of TWCF

Uncertainty Analysis Framework



Note: the quantification of epistemic uncertainties in all parameters is not shown explicitly, but is assumed.

PFM Uncertainty Analysis Approach

- Identify sources of uncertainty in all model elements and categorize as being aleatory or epistemic
- Address aleatory uncertainties through T/H subscenario frequencies and conditional probability of vessel failure, given the subscenario
- Address epistemic uncertainties using standard estimation and uncertainty propagation techniques (e.g., Monte Carlo simulation)
- Assemble results using FAVOR

Thermal Hydraulic Uncertainty

- Objective: Characterize and quantify uncertainties in thermal hydraulic calculations used in PTS analysis
- Expectation: Characterization will be compatible with FAVOR requirements
  - T/H scenarios have uncertain frequencies
  - T/H scenarios have associated, deterministic pressure-time and temperature-time traces
- Planning initiated at U. Maryland under cooperative research agreement
- Approach is likely to involve considerable screening, efficient methods to propagate uncertainties
- Follow-on to PTS work may lead to a more generalized approach

Concluding Remarks

- Development of Oconee PTS PRA model is progressing well
  - Integrated PRA/HRA team is developing plant model and has visited the plant
  - Screening level results expected shortly
- Beaver Valley work is being initiated
- NRC team has received excellent cooperation from participating utilities
- Uncertainty analysis framework has been developed and is being implemented in FAVOR
  - T/H uncertainty work being initiated
  - Uncertainties in K_{1c} being analyzed