Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Reactor Safeguards 494th Meeting	
Docket Number:	(not applicable)	
Location:	Rockville, Maryland	
Date:	Friday, July 12, 2002	
Work Order No.:	NRC-459	Pages 405-491

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)
494TH MEETING

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FRIDAY, JULY 12, 2002

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS PRESENT:

GEORGE E. APOSTOLAKIS	Chairman
MARIO V. BONACA	Vice Chairman
THOMAS S. KRESS	Member-at-Large
F. PETER FORD	Member
GRAHAM M. LEITCH	Member
DANA A. POWERS	Member
VICTOR H. RANSOM	Member
STEPHEN L. ROSEN	Member
WILLIAM J. SHACK	Member
JOHN D. SIEBER	Member
GRAHAM B. WALLIS	Member

ALSO PRESENT:

NILESH CHOKSKI, RES

MARK KIRK, RES

SHAH MALIK, RES

MIKE MAYFIELD, RES

THERESA VALENTINE, NRR

RICHARD BASS, Oak Ridge National Laboratory

TERRY DICKSON, Oak Ridge National Laboratory

CLAUD PUGH, Oak Ridge National Laboratory

PAUL WILLIAMS, Oak Ridge National Laboratory

I N D E X

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

(8:30 a.m._

CHAIRMAN APOSTOLAKIS: The meeting will come to order. This is the 494th meeting of the Atomic Reactor Safeguards. During today's meeting, the Committee will consider the following: Application of the Probabilistic Fracture Mechanics Methodologies to Reactor Vessel Integrity Assessment; Proposed ACRS Reports; Future ACRS Activities; Reconciliation of ACRS Comments and Recommendations; Format and Content of the 2003 ACRS Report on the NRC Safety Research Program; and Proposed Papers for the Quadripartite Meeting.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. Mr. Sam Duraiswamy is the Designated Federal Official for the initial portion of the meeting.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's session. A transcript of portions of the meeting is being kept and it is requested that the speakers use one of the microphones, identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

As requested by Westinghouse, video teleconferencing arrangements have been made for Westinghouse to observe the meeting session of the application of the probabilistic fracture mechanics methodologies to reactor vessel integrity assessment. There is no one from Westinghouse -- oh, there is one. Okay. I'm sorry.

Do any of the Members wish to say anything?

(No response.)

Okay, so we can proceed with the application of probabilistic fracture mechanics and Dr. Ford will chair this part of the session.

MEMBER FORD: Thank you, Mr. Chairman. Probabilistic fracture mechanics, as you know, is central to some of the current problems that we are tackling primarily right now at PTS. Several others have asked for further information on this and calibration and validation of the current models and this is what we're going to hear about. This new letter is asked for. This is purely informational.

Mike, would you like to make a comment?

MR. MAYFIELD: Just very briefly. We've had the opportunity to brief the Committee a number of times on the higher tier aspects of the PTS reevaluation and we haven't had your questions and I've enjoyed interacting with Dr. Powers a number of times on the robustness of our vessel program and we appreciate the opportunity to come down and share with you some of the details and some of the historical basis for why we're pretty confident in the fracture calculations.

Mark Kirk is going to lead off the presentation and we'll go from there.

MR. KIRK: I'd like to invite my colleagues, Richard Bass and Claud Pugh and Shah Malik to come up because by the time I get to the fifth slide, I'm going to run out of steam. So need their help up here.

Well, Mike has given you the intros, so we know what we're talking about.

MEMBER POWERS: But see, when a vessel runs out of steam, it depressurizes and becomes safe. Is that the case here?

MR. KIRK: We'll discuss that in nauseating detail later.

(Slide change.)

As you know by the groans when you saw my lovely face up here this morning, we briefed many,

many times over the past several years on probabilistic fracture mechanics techniques that are being used to assess the technical basis for updating the PTS rule.

Last time, you all requested that we provide additional background concerning both the appropriateness of using LEFM in such assessments and show you that LEFM is valid and applies to nuclear RPV assessment, in particular.

(Slide change.)

MR. KIRK: This just shows you an overall schematic of the PTS reevaluation process. You've seen this before. Start off -- oops. Shouldn't touch the screen. Never touch the screen.

We start off in the gray box on the left of your screen with our initial work. We first go back and forth between PRA and thermal hydraulics quite a bit trying to do the binning and see what sequences are significant. Finally we get -- or after that initial iteration we get out some transients, so we then pass on --

CHAIRMAN APOSTOLAKIS: We can't see almost half of the screen. Can you gentlemen move a little bit to the right and left. You don't have to move away, just move a little bit. I appreciate that.

Thank you.

Thank you very much. I appreciate it.

MR. KIRK: All this schematic is attempting to show is that in the gray box, we do some initial iterations between PRA, HT and PFM to assess the combination of sequences and thermal hydraulic runs that we then take to characterize a particular plant. Once those are established, we go through a final run where we again go PRA to TH to PFM and finally come out with yearly frequency of through wall cracking.

MEMBER KRESS: Mark, do you have a group of expert panels to develop distributions for the things in the gray box for the inputs for the code?

MR. KIRK: The inputs for these, I mean, they come from a number of sources. In some cases, it's expert judgment. In some cases it's data. In some cases, it's well established models from the literature. And I think it's fair to say we've got a bit of both or a bit of all three in all three boxes.

(Slide change.)

MR. KIRK: Of course, this is just to orient us in terms of why we're here talking about PFM. Of course, the focus today is on PFM specifically and when we look at PFM we previously talked to you again in a great degree of detail about the uncertainty framework and now the diagram in the upper right hand side of the screen breaks out PFM into some of its component parts which again could be broken out yet further.

(Slide change.)

MR. KIRK: We've talked about the uncertainties in detail before, so I'm not going to go through that again because the focus of today's discussion is the deterministic calculations that lie at the heart of the FAVOR looping structure. I've shown you here again, a fairly high level schematic of what's going on in FAVOR. At the outer loop, we simulate vessels somewhere on the order of tens of thousands of vessels. Inside that is flaws and transients and time, the point in showing this being when you get to the very bottom of these Monte Carlo loops that in total, help us to simulate all the uncertainties. Down buried at the bottom there is a deterministic calculation and what we hope to show you by the end of the day is that that deterministic calculation is indeed an appropriate tool to use to assess RPV failure.

So with that --

CHAIRMAN APOSTOLAKIS: CPI is what?

MR. KIRK: Conditional Probability of Initiation and Conditional Probability of Failure.

CHAIRMAN APOSTOLAKIS: Conditional Probability of Initiation, given what?

MR. KIRK: Conditioned that the transient has occurred.

CHAIRMAN APOSTOLAKIS: Okay.

MR. KIRK: There's yet again, to show it more generally, I suppose there would be yet again another outer loop or a post-processing box where what comes out of the FAVOR code itself are the conditional probabilities. Those are then combined later with the initiating event frequencies to get the yearly vessel failure frequencies.

MEMBER KRESS: And the failure frequency is defined as a through wall crack?

MR. KIRK: Right now it's defined as a through wall crack. That's right. But we calculate, the point being which gets back to our discussions of Wednesday, FAVOR calculates both initiation and failure. So we have the ability, of course, to look at both. But yes, right now, failure is defined as complete through wall crack.

CHAIRMAN APOSTOLAKIS: What does it mean is appropriate to predicting RPV failure?

MR. KIRK: It does it right. That fracture mechanics predicts --

CHAIRMAN APOSTOLAKIS: You don't have any uncertainties there?

MR. KIRK: No. That's not to say that there aren't uncertainties, but within the range of uncertainties that are characteristic of the material of the data of however you want to look at it, we can predict the failure, let's say, of a reactor pressure vessel with a buried crack, just as well as we can predict the failure of a much more well-defined structure like a test specimen in a laboratory.

MEMBER POWERS: And because of that superior capability, we adequately researched the heavy section steel?

MR. KIRK: Wait for the last slide.

(Laughter.)

So we'll now go on to the presentations that will be made by our colleagues at Oak Ridge. Claud Pugh will do the first set of slides and then Richard Bass will do the second set of slides and then we'll wrap up. And I'll move out of the way.

MR. PUGH: I'm one who likes to stand on my feet.

CHAIRMAN APOSTOLAKIS: Can you please

lower the mike a bit? The other one. Thank you very much.

MEMBER POWERS: Highly sensitive today.

CHAIRMAN APOSTOLAKIS: Enjoy this.

(Laughter.)

MEMBER KRESS: Dr. Pugh, how come you don't have an accent?

MR. PUGH: Well, it's funny, everyone else does in the room except you and me.

(Laughter.)

It's good to see you again, Tom.

For the record, my name is Claud Pugh. I recently retired last year from the Oak Ridge National Laboratory after some 33 plus years there. In my tenure there, included a number of years where I served as manager of the Heavy Section Steel Technology Program which is, of course, the primary pressure vessel technology program for the NRC and for the AEC prior to the NRC's creation and then in the last dozen years or so I served in a larger management capacity for NRC programs at Oak Ridge. So that's by way of kind of giving you an introduction of who I am and where I'm coming from.

I'd like to first made an observation that I think is very obvious and clear to all of us, that as we look at any technology, but in particular, today at the deterministic fracture mechanics technology applicable to the pressurized thermal shock issue, this is not a circumstance where the technology has been looked at in isolation, in particular, and only in particular to the PTS circumstances, but rather, it is a technology that is built upon all that has gone before it and then looked at in terms of either adapting, confirming or adding to as appropriate to the PTS scenario.

So what Richard Bass and I want to do in the forthcoming slides is to talk you through the big picture of what has gone before and what is today in terms of the deterministic aspect of the fracture mechanics technology that is applicable RPBs under PTS divisions.

MEMBER WALLIS: In this context, is this a standard technology that's throughout all industries, or did you have to develop special things for this purpose?

MR. PUGH: Basically, the answer is the latter. There are very definitely specific aspects that are peculiar and specific to reactor pressure vessels. In fact, about four comments from now you'll see one of those come forward.

CHAIRMAN APOSTOLAKIS: But probabilistic fracture mechanics is used widely, isn't it?

MR. PUGH: As Mark said earlier, probabilistic fracture mechanics entails the performance of a multitude, a large multitude of deterministic fracture mechanics analyses. So what we're focusing on this morning, as I understand it, is the question of the applicability and the validation of the applicability of linear elastic fracture mechanics to the deterministic aspect of the PFM analyses for PTS conditions.

MR. KIRK: Let me jump in.

MR. PUGH: Yes.

MR. KIRK: The probabilistic fracture mechanics is widely used in a number of industries and the underlying linear elastic fracture mechanics was not developed specifically for nuclear applications. But there are unique aspects for nuclear pressure vessels, that over time we've had to address. But the root technology is not unique to this application and the probabilistic techniques are widely used in a number of industries.

(Slide change.)

MR. PUGH: Yes. Well put. So this is just saying that we're going to look at -- trying to give you a picture of the systematic and evolutionary nature of the development over the last two plus decades or three decades, really.

As the nuclear power enterprise developed in the 1960s, it was widely recognized by a lot of people that indeed the circumstances -- well, first of all, that the fracture mechanics technology was rather young in itself and most of the work in developing it and validating it were for situations such as aerospace applications and particularly like rocket motor casings which were high strength steel with low ductility and very thin sections. Here, we had a circumstance developing of very thick sections of relatively low strength in terms of yield strength material, but very ductile materials. So the questions were how applicability is that fracture mechanics technology that was already being developed to the circumstances that had not yet been validated as to being applicable --

MEMBER WALLIS: When you said linear elastic, aren't you beyond this linear elastic range? You have to show you're not or something.

MR. PUGH: Kind of hold the thought as we work through some of this and hopefully the picture will come as to the interface and the transition between regions.

MEMBER WALLIS: Okay, thank you.

MR. PUGH: So let's say this was widely recognized, but perhaps to give the motivation and impetus to actually creating a program to investigate it, there was a certain body called the ACRS which you may be familiar with, wrote a letter on November 25, 1965 to the then AEC and they cast the question more or less in this sense sort of the suggestion, the recommendation saying industry in the U.S. AEC should give detailed attention to RPV integrity assessment methods to support the then existing position that RPV failure is incredible.

MEMBER KRESS: Was that letter signed by Bill Manley?

MR. PUGH: I don't think, so Tom. Actually, I was thinking last night who did sign it and -- no, this is pre-Tom. 1965. Whoever was the Chairman then. I don't think it was Bill Manley.

MEMBER KRESS: He was Chairman in the 1980s.

MR. PUGH: This was in 1965 during the AEC time. And they suggested including assessment of stress analysis, development of inspection methods, improving means for evaluating factors that could affect propagating flaws during the RPV service life.

So this gave the AEC then the basis for rallying all stakeholders, all interests together to the table to develop a plan where the AEC being the entity that had the funding to underwrite a plan once it was agreed upon and deemed a viable plan.

So indeed, they sent forward with stakeholders, from vendors, from universities, from just essentially every person involved, every entity involved, ASME and tremendous voluntary efforts came to the table under the auspices of the Pressure Vessel Research Committee. From this came, after a long intense year of planning, a detailed plan, multi-yeared plan for pursuing the fundamental questions of the applicability of fracture mechanic to thick wall reactor pressure vessels.

And the AEC then looked to Oak Ridge to do the centralized management of this effort. I say it that way because Oak Ridge certainly played an important technical role, but also there was a lot of subcontract participants, Battelle, BMW, Westinghouse and in those was a very strong participant and contributor.

But the very first step in executing the plan was the development of a state-of-the-art technology report and the report is cited here, NSIC-21, technologies for steel pressure vessels in water cooled nuclear reactors and you see it was quite a tome and truly it was a state-of-the-art document that served a good purpose for many years going forward. That plan included looking at the fundamental fracture properties of reactor pressure vessel steels. It looked at an incremental step-wise fashion of once that you something about the characteristics of the fracture, what are the models, what are the properties to gain to quantify the models, what are the analysis methods to use, how do you validate them and they had three stages of structural or pressure vessel experiments, basically, laboratory science, intermediate vessels and full scale. That was the plan.

First priority, I said, was establishing basic fracture techniques. Large scale testing was testing to it. And the models and properties were integral to the overall plan.

MEMBER KRESS: Are you going to explain what the thing is in the picture?

MR. PUGH: That is a pressure vessel from a 1100 megawatt plant, 1100 megawatt unit built at Combustion Engineering. We bought and shipped this to

Oak Ridge as part of the work that we did there and one of the conditions on buying it, actually DOE bought it. One of the conditions of buying it, we were not supposed to be given the identity to it. but when you look at the outlet nozzles and having flats for supports you have a pretty strong indication of who may have made it.

MEMBER KRESS: That's the vessel used by PNL to determine the flaw size and distribution?

MR. PUGH: PNL did a detailed mapping of the flaw distribution inside this vessel.

MEMBER KRESS: One of our questions is if you've got one vessel and you look at the flaw size and distribution, how representative is that of the fleet of vessels that are out there?

MR. PUGH: There have been inspections of segments of other vessels. Of course, I'm sure as you know, Salem vessel being one, all of which creates a database. There is non-nuclear vessel data. All nuclear vessels were inspected prior to going into service, so there is some data from pre-service inspections, all of which gives a database which we, I'm sure, would always like to think we'd like to have a larger one. But it's the best it's ever been.

MEMBER RANSOM: How small is the

probability of failure to be accepted as incredible?

(Laughter.)

MR. PUGH: Ah. May I pass on that question?

Quantitatively? Of course, 10^{-6} has been used in recent studies, so I guess if I were to give an answer I would relate it to that.

MEMBER RANSOM: Less than 10^{-6} --

MR. PUGH: Would be considered in the range of incredible, yes. You remember, I'm speaking here historically, in the context of 1960s when PRA and dose type analyses were not quantitatively being looked at. It was more qualitative.

Could you demonstrate a degree of difference between the actual circumstance and failure of the operation and failure that would give you a feeling that the margin is well sufficient as to not to lead to failure?

By the way, Tom, that picture was on there just for --

MEMBER KRESS: Just to make it look good.

MR. PUGH: Just to dress it up a bit. That's what a real vessel looks like and that's a fellow we work with.

In the beginning, then executing the plan, emphasis was placed on understanding the fracture characteristics of the material and properties that went with that. The AEC, ORNL procured over 500,000 tons of reference test material, typically 12-inch thick plates of A508 and A533 steel primarily.

A large number of exploratory and property experiments were done in those early days to (a) to start out with the question does this kind of material like the high strength, low ductility material show a transition from brittle to the transition to the ductile regime.

Those are some of the first experiments done, not just on small laboratory specimens, but on tensile specimens up to 12 inches thick being prototypical of pressure vessels. And so that question turned out to be yes. They went forward, deciding well, what kind of specimen do you measure properties with? A whole host of specimens were looked at and in the end it was settled on the compact tension specimen which then led to the acceptance of the trial ASTM standard E-399 for fracture testing, so this program and its exploratory work and its round robin led to that being settled upon as the fracture mechanic specimen of use.

(Slide change.)

MR. PUGH: Looking at the properties, there were a number of variables though that could influence them, in particular, of course, we all know temperature, the toughness versus temperature are kind of dependent. But also rate has a very pronounced influence. After a lot of studies and dynamic effects, it was concluded that the crack arrest toughness represented a very reasonable lower bound to the dynamic fracture toughness data.

MEMBER WALLIS: You said load rate had a big influence?

MR. PUGH: Yes. The higher the rate, the lower the --

MEMBER WALLIS: Within what sort of range of speeds, which what sort of time frame are you talking about for an influence?

MR. PUGH: As I just said, kind of the limiting case that was considered to be the arrest toughness.

MEMBER WALLIS: But you're talking about fractions of a second, presumably. You're not talking about long periods -- you're talking about short blows?

MR. PUGH: Something like split Hopkins bar has been used, for example, giving strain rates of

10^{-5}. I mean 10^5 per second. Very fast rates.

MEMBER WALLIS: Very fast rates.

MR. PUGH: Yes. But what one looks for is asymptotic behavior of the rate dependence which seems to be there approaching that of the crack arrest values.

So tremendous progress was made during those years in generating data. Westinghouse played a very important role in that testing specimens up to 12 T. I should have emphasized that within this, it was adopted that plain strained fracture mechanics was to be the reference as in the vessels, if you had a flaw, it was going to have a lot of constraint. So they looked for a specimen that would exhibit plain strained conditions on the crack front.

As temperature goes up to maintain the plain strained conditions, you have to have larger and larger specimens, so many of these data, many are called out here but to get up into this region here of like 200 MP_A meters toughness required 12 inch thick, that meant like 24 inch square specimen. Huge specimens. And Westinghouse, for example, tested a lot of those for us.

All that data was -- is the basis for the K_{IR} curve that is still in the ASME code today.

You'll find those data reported in Welling Research Council Bulletin 175. You may have heard the terminology, $1 million curve? Now a $1 million curve today may not be that much, but in 1965 that was a big effort.

MEMBER KRESS: Is that the curve that you have on the thing or did it go through the mean?

MR. PUGH: This was intended to be a lower bound.

MEMBER KRESS: It was a lower bound.

MR. PUGH: Yes.

MEMBER KRESS: Okay.

MR. PUGH: The ASME curve, it is lower bound, not only of the fracture toughness, but of the arrest toughness, initiation and arrest toughness.

MEMBER FORD: Claud, you mentioned earlier on this condition of probability for crack initiation as being one criterion. Surely, the K value for crack initiation from a pre-existing flaw will be plain stressed conditions or could be. Therefore, your methodology would be very conservative? Is my rationale right?

MR. PUGH: It's an excellent question and I'm about two slides hence. I'm going to show you an example of the answer to that.

Your conclusion is correct, generally, but on that one variable, namely the constraint variable, but I'm going to have a very good example popping up here in about two slides that will show that.

MEMBER KRESS: This may be a question to Mark, but you're no longer using this lower bound? You've actually gone to the best estimate?

MR. KIRK: MR. KIRK: That's correct. I mean the data, well, the curve that's shown here is the lower bound K_{1R} curve. There's also a lower bound initiation curve. Those are the ASME design curves that are used. They're still used by the NRC and the nuclear licensees in calculating heat up and cool down limits. And in fact, those lower bound curves are the curves that were used in the early 1980s to establish a current PTS rule. That was a long background. The answer to your question is yes. Now we take that whole distribution of data and use it.

MEMBER KRESS: Yes, you get a substantial distribution.

MR. KIRK: So we use it -- what we do is we sample from that distribution and we draw values out so we capture the uncertainty that you see depicted on that plot, yeah.

MR. PUGH: Thank you for asking that

question, Tom, because I am commingling the evolution of the total technology on fracture prevention with what we know about fracture mechanics, so we do try to make the distinction as we go along with the two.

MEMBER WALLIS: These variations are because the steels are different or because the flaws are different?

MR. KIRK: The variations are just simply inherent to the material. I could show you what on the previous slide that's the result of about 12 different materials that makes the plates in welds and forgings. I could take one material and if you'll forgive the phrase, test the hell out of it, and I see exactly the same variation.

MEMBER WALLIS: This is because the --

MR. KIRK: Because the material is inhomogeneous at a micro scale.

MEMBER WALLIS: It had a different history or had --

MR. KIRK: No, no, no. It's just the local inhomogeneity of the material along the crack front. You could test -- if I took a plate of material the size of this conference table and cut it up into big, small specimens, you pick --

MEMBER WALLIS: You'd get different

answers for these specimens?

MR. KIRK: Tested all under precisely controlled conditions and you would see that variability. And so that's -- that's a classic aleatory variability and we capture that appropriately in FAVOR.

MEMBER POWERS: Mark, if I doubled the number of tests on a single material or multiple materials, either way, would I see large numbers of points below the solid curve that you've drawn in there?

MR. KIRK: No.

MEMBER POWERS: Why can you say that so confidently?

MR. KIRK: Because since that curve has about 175 points --

MEMBER POWERS: About 350.

MR. KIRK: And the years since, I go up to my desk, since I'm a data geek, I collect these things. We have a database now well in excess of 4,000 points and no point has ever -- except on the lower shelf where it wasn't ever meant to be a bound, but in the transition region, I'm sorry, I'm pointing at the screen again. In the transition region, where Claud is pointing, which is where the action is for

RPV failure, no curve has ever transgressed the line and that's --

MEMBER POWERS: I see a point that transgresses the line right there.

MR. KIRK: No. Go up a ways. There you go. As we discussed on -- as Mike discussed on Wednesday, when we were talking about the risk goal, one of the
-- if we had a completely risk-based rule, you might be able to reach the conclusion that you could operate the reactor vessel safely on the lower shelf.

MEMBER WALLIS: What's the lower shelf mean?

MR. KIRK: That's the lowest fracture toughness you can get.

MEMBER WALLIS: That's what you mean by lower shelf?

MR. KIRK: That's what I mean by lower shelf, yes.

MR. MAYFIELD: This is Mike Mayfield. When you plot Charpy energy versus temperature, there is a transition from a lower plateau region to an upper plateau region in energy and that lower region is typically characterized as the lower shelf. It comes below the nil ductility transition temperature for the material. So when we talk about lower shelf, upper shelf and transition, we typically are referring to regions on the Charpy energy versus temperature curve.

MR. KIRK: In any event, there are other -- the point I was trying to get to is there are certainly good engineering reasons why even if your risk numbers told you you could, you wouldn't allow a structure with as high a failure consequence as our nuclear RPV to operate down in this region.

But in answer to Dr. Powers' question, the substantial testing that's occurred in the ensuing years has generated a database well in excess of, I think, 4,000 to 5,000 data points. None of them has ever crossed an RT_{NDT} indexed K_{1C} or K_{1R} curve which is simply a testament to the conservatism that is built in to the current ASME rules.

MEMBER POWERS: Well, the one point that does should be banned and burned and otherwise castigated.

MR. KIRK: When I say none has ever crossed, remember I put the caveat on in the transition region.

MEMBER POWERS: Well, it looks like it's pretty close to the transition region.

MEMBER SHACK: He's up there with a triangle rippling data.

MR. KIRK: That's on the curve. That sets the curve.

MR. PUGH: And recall, this is the $1 million curve which is not necessarily the K_{1R} curve, that is in the code.

MEMBER POWERS: You tell me that there is a point that is exactly on the curve, that's what's said and I will never ever, no matter what I do find a point that falls below that curve. You are a man of faith.

(Laughter.)

CHAIRMAN APOSTOLAKIS: He said he hasn't seen -- right, Mark?

MR. KIRK: Yes.

CHAIRMAN APOSTOLAKIS: Are you also saying you will never see it?

MR. KIRK: I would not expect to see it.

MEMBER WALLIS: It would be incredible.

MR. KIRK: It would be incredible -- okay, the --

CHAIRMAN APOSTOLAKIS: It's really, really unlikely.

MR. KIRK: It's really, really unlikely.

I mean nonparametrically if you've got a database of 5,000 database, no, what are the odds? But equally, we understand making strictly a data argument because it was a database question, but we understand why the curve is there. We understand why we can collapse multiple curves together, using an indexed temperature approach and we also understand the conservatism that's inherent to the RT_{NDT} index temperature which is if you go to establish RT_{NDT} based on nil ductility temperature tests and Charpy tests, the only way you can run the procedure forces you to overestimate the parameter. And that was done intentionally in the early days to make sure we were working with a bounding curve.

MEMBER WALLIS: What does NDT mean?

MR. KIRK: The nil ductility transition is defined in ASTME 208 as the temperature in which you go from a break to a no break condition in a nil ductility test which is a 5/8ths thick by about 6 inches long, 2 inches wide specimen with a brittle weld bead put on top.

MEMBER WALLIS: Well, of course, that's a fairly complicated thing.

MR. KIRK: Oh yes, it is. But it's an order of merit --

MEMBER WALLIS: I thought it meant normal daytime temperature or something like that.

MR. KIRK: No.

MEMBER ROSEN: You see, the logical inconsistency with your remarks in response to Dr. Powers is that the day before you had that test which gave you that point right on the line, you would have said that there's no chance of having any test like the one you were about to get the next day.

CHAIRMAN APOSTOLAKIS: He said that point defined the curve, that's different.

MEMBER ROSEN: He's also said that nothing can be to the right of that. And I'm just pointing out, that's what you would have said on the day before --

MR. KIRK: That's true. So perhaps I'll revise my comments for the record that subsequent testing of thousands and thousands of specimens has revealed nothing below that curve.

MEMBER KRESS: And if it did, why would you care?

CHAIRMAN APOSTOLAKIS: And frankly, I've heard enough about this curve.

(Laughter.)

MEMBER POWERS: Well, George, I'm sorry,

I just have to know.

CHAIRMAN APOSTOLAKIS: You want more?

MEMBER POWERS: When I didn't have that point I would have used the little black square to fix the curve.

MR. MAYFIELD: Mark, let me. There's a bit of perspective to not lose here. The curve and the K_{1R} curve that's discussed in the fourth bullet on that slide is the lower bound -- was intended to be the lower bound curve to crack initiation, dynamic crack initiation and crack arrest toughness. It was intended as the lower bound to all of those conditions.

The data that we are using comes, in the analysis, comes from two aspects. One is essentially static -- it's very slow loading, crack initiation. That's the K_{1C} curve that Mark has talked about before and those data, because these materials are loading rate sensitive, those data tend to be well above that curve.

The lower data points that you're seeing, tend to come from either dynamic initiation or crack arrest tests which -- that's a very rapidly moving crack. Those tend to be the lower -- tend to be the lowest of the data. So there's a mixture of data that went into defining the K_{1R} curve.

When you go back and segregate the data between the two types you're really interested in, initiation and arrest, there are separate curves for those data types and the uncertainty associated with those gets rolled in. I think we need to be a little careful in drawing too many conclusions about whether the curve does or doesn't bound all the data because we're really interested in the application in segregating the data.

MR. KIRK: One perhaps final point is I think it's good to get back to Dr. Kress' comment earlier that now we're using a probability distribution through all this data which gets us away from, as is obvious here, the very difficult question of establishing an absolute lower bound. We take into account the inherent variability that's there and that's included in the calculation.

MEMBER KRESS: I think Dana's question bears on that because you have to set a distribution to sample from and how you set that distribution depends on what form you assume it takes. Do you sample vertically? Do you fix the temperature and sample vertically?

MR. KIRK: Yes, we do and the form that

the distribution takes can be established by data. It can also be established by physics and it's indeed a happy circumstance that the physical expectation, the distributional form that you expect physically is well substantiated by the data.

MEMBER KRESS: Is it log normal?

MR. KIRK: It's Weibull.

MEMBER KRESS: Weibull.

MR. KIRK: Yes.

MEMBER POWERS: So that's why we can't have points below the curve.

MR. KIRK: That's right.

MEMBER POWERS: I took from Mike's comment that the way to get points below the curve was to increase the rate of loading.

MR. MAYFIELD: Dana, this curve was drawn by a guy with a French curve and he didn't know how to use it. This is not a statistical figure. Okay? And he tried to pin his curve to the lowest data point he had at that point.

When you look at the curve that's in the ASME code, this K_{1R} curve, you'll actually find a cusp in it and so that the gentleman that didn't know how to use this French curve to create a smooth curve, that has propagated its way up until the last four or five years when the ASME finally made a change. So I don't want you to leave this discussion with the impression that there's great high science or mathematics behind that particular representation. The work that Mark and company have done subsequently, to move away from this sort of historic plot is, in fact, much better science and I think we're drawing far too much significance to this particular plot.

CHAIRMAN APOSTOLAKIS: So it's Weibull vertically?

MR. KIRK: Yes, that's correct.

CHAIRMAN APOSTOLAKIS: And what is the probability? Do you remember roughly of the point falling below the curve?

MR. KIRK: I'm sorry, I don't understand. I know this sounds stupid, but I don't understand the question.

CHAIRMAN APOSTOLAKIS: Isn't there a probability that if I pick a temperature, there will be a point below the curve now, as soon as you have a distribution?

MR. KIRK: Yes, but the Weibull, it's a three parameter Weibull. It has an absolute cutoff, yes.

CHAIRMAN APOSTOLAKIS: And the absolute

cutoff coincides with this curve?

MR. KIRK: No. As Mike said, we have a Weibull distribution that's been statistically fit to not only these data, but also the data that have been developed since then. That curve has an absolute lower bound since it's a three parameter Weibull. It's agreement or disagreement with this particular curve, which as Mike said was hand-drawn, would be a complete circumstance.

This is a historical design curve.

MEMBER FORD: If I could suggest that we've used up half our time.

CHAIRMAN APOSTOLAKIS: That's what I think.

MEMBER WALLIS: But still, this NDT, is it ductile to the right or the left of this point, zero point? There's a nil ductility transition?

MR. KIRK: It's more ductile to the right.

MEMBER WALLIS: I would expect it to be more ductile to the -- the region of interest, there is ductility.

MR. KIRK: Yes.

MR. PUGH: Perhaps I can shed some additional light on that type of question as we look at the next two or three slides.

Remembering the earlier technology was directed at fracture prevention, not fracture prediction in the applied sense of the ASME code, so that's one of the reasons of this type approach to it earlier.

We are going to look at real quickly, hopefully three, large scale sets of experiments for purposes of validating the applicability of the fracture mechanics technology which is based on uniaxial specimens. So obviously, the application is multiaxial conditions of pressure loading being multiaxial at the very outset, plus any other factors that come to bear.

MEMBER WALLIS: This is nonirradiated steel?

MR. PUGH: All of these experiments run -- RPB steel, but nonirradiated, yes sir. Very typical of steel though.

I'll speak very briefly about intermediate vessel tests. If you would, Mark, please?

These were experiments conducted on one meter diameter by 6-inch thick walled specimens with axial flaws, the very deep ITB7. One happened to be portrayed here in this schematic. Most of them had like a quarter to a half thickness in the flaw. Ten such vessels were procured. Like I say, they're 8508 Class 2 steel, RPB steel. You recall I said that early plan included a full scale testing phase. It was concluded in the early to mid-1970s. That was just too cost prohibitive to pursue, so the added importance was taken on by this set of ITB experiments to demonstrate the transferability of the fracture mechanics developed in the laboratory and even in these large plain strain fracture uniaxial specimens would transfer to a constraint situation prototypical of the vessel.

MEMBER KRESS: This flaw you show on here is on the outside of the vessel.

MR. PUGH: That is correct.

MEMBER KRESS: Can you explain the rationale behind that?

MR. PUGH: The constraint and loading conditions are essentially the same as if it were on the inside. You have the pressure loading, which is still the 2 to 1 pressure loading. You have in the case later, we'll look at it, thermal stresses, the case of the crack front is loaded in the same way as if it were internal and it was much easier to work, of course, experimentally with the external flaw.

MR. BASS: This is Richard Bass from Oak

Ridge National Laboratory. I just want to reiterate a great deal of analytical effort went into evaluating these vessels with the flaws on the exterior surface, via-a-vis the IPV laws on the intersurface to assure that we had stress fields, fracture toughness fields and gradients in these vessels that were also correspondent to RPVs.

MEMBER KRESS: This didn't have to be a cylinder, did it?

MR. BASS: Pardon me?

MEMBER KRESS: The only reason you made it a real cylinder is so you could pressurize the inside of it?

MR. BASS: Yes.

MEMBER KRESS: It could have been a flat plate for all you cared, if you could provide the loading.

MR. BASS: I don't know that we would have wanted to use a flat plate in this case.

MR. PUGH: Certainly in subsequent experiments where we looked at thermal shock, definitely would not have wanted a flat plate because of certain inertia and bending effects that develop in the arc of the cylinder.

MEMBER SHACK: It probably would be hard

to stress state the flat plate too.

MR. PUGH: You're probably thinking about a set of experiments that was done in Japan once upon a time, thermal shock.

MEMBER SHACK: You did do some sort of trick with the heat treatment to embrittle this material.

MR. PUGH: Not on these. These are prototypical. This is a detail. There are two sets. One of them was normalizing temper and the other was tempered to impact the fracture properties slightly.

MEMBER SHACK: This is a relatively bicuspus material. This is like the --

MR. PUGH: Yes, this is like the real stuff.

MEMBER SHACK: The real stuff.

MR. PUGH: Absolutely. Now there was one experiment run on a lower per shelf weld that was absolutely tailored to study the fracture properties of the lower shelf weld in one of these vessels. That was ITV-8(a), but no, the basic principle was to use the real pressure vessel conditions to validate the fracture behavior and the ability to predict that fracture behavior.

(Slide change.)

MR. PUGH: So if we look at the next slide, we'll see that here is a schematic of this Charpy curve, Dr. Wallis, that Mike was describing earlier, the lower shelf, transition region, upper shelf.

Operating conditions of reactor pressure vessel is up here on the upper shelf. Should you have the thermal shock circumstance, you have the injection of the coolant, then you're progressing back down in this region. So this is the region of real interest to the fracture behavior to the PTS issue is in the lower to mid transition range. So we're going to focus right here on this slide on these, what looks to be three experiments, I mean four experiments. Actually, this 8(a) had two initiation and arrests, so there will be five points plotted over here of failure pressure versus predicted failure pressure. Here's the one-one line. You see these four line up very nicely.

Peter, I don't remember if it was you a while ago or Dr. Rosen, asked the question about what if you did not have this constraint to yield the plain strained conditions. This test here is ITV-9. This is a nozzle experiment. There was a flaw in the inner corner of the nozzle and this -- I don't know if you can see it or not, but this is what a vessel with a nozzle looked like.

With a flaw in that inner corner of the nozzle, you absolutely do not have the constraint conditions. You lose much or most of your constraint, so in answer to the question do you elevate or decrease the fracture, apparent fracture toughness in that circumstance? You increase it. This is why this point is well above here in terms of the actual failure pressure, well above what LEFM would have predicted.

MEMBER POWERS: Let me ask a question.

MR. PUGH: Yes sir.

MEMBER POWERS: Converting of units, I'm never very confident about these things with pressure, but 100 mega Pascals corresponds to, I think, 14,000 psi. Is that correct?

MR. PUGH: 6.895 over whatever is the conversion.

MEMBER POWERS: So roughly 14,000 psi.

MR. PUGH: Yes.

MEMBER POWERS: Which of our pressure vessels in the United States, reactors, 14,000 psi?

MR. PUGH: Tthe design pressure for this vessel is 9 point something mega Pascals or is it ksi?

MR. BASS: 9.75 ksi.

MR. PUGH: 9.75 ksi. It is the design pressure for the ITV that's equivalent to a full RPV under 2250 psi.

MEMBER POWERS: So there's some scaling that's been done here to decide that what you think the critical flaw, stress field that the critical flaw will be to mimic what it is in the much bigger pressure vessel?

MR. PUGH: Yes sir.

MEMBER POWERS: It's an element of faith involved in going from here to the actual pressure vessel.

MR. PUGH: But with detailed stress analysis, I think one can feel that it is well simulated.

MEMBER SHACK: Which is why they scaled this test from a Charpy compact test specimen to this vessel.

MR. PUGH: Yes.

MEMBER SHACK: You verified the scaling to that extent.

MR. PUGH: Yes. You recall the original plan had in it full scale testing that would have validated perhaps the nth increment of scaling, but it was just too expensive to take on.

MR. MAYFIELD: Dana, I think the other point that's worth making is there's been a lot of work done in showing that this fracture toughness parameter and that the stress near the crack tip is what controls. You just need to do something that looks like a cylinder to get the stress state to be similar?

MEMBER POWERS: I don't have any trouble of scaling this up. I think you've left out an element in the presentation of discussing that scaling.

MR. MAYFIELD: Fair enough.

MEMBER POWERS: It's not so important for the presentation, but in making the case for the PTS when you use this information to say this elastic fracture mechanics is a valued technology for doing your PTS analysis and verified, you're going to have to put that step in.

MR. PUGH: The next slide will have something relevant to that step coming up.

MR. BASS: Richard Bass at RNL. I have the analysis that you're talking about on my desk at Oak Ridge and I'll be glad to provide that.

MR. PUGH: It's more important that they

provide it in their ETS.

MR. MAYFIELD: If the Committee is interested, we can certainly provide the information, but I think the point is well taken to make sure, as we document what we're doing, that we lay that basis.

MEMBER POWERS: This is a crucial part. And it's important not to leave out what is a crucial step and in thermal hydraulics land, they spend an enormous amount of time discussing that scaling and I think you'd be remiss to gloss it over. I mean it's very familiar to you, but it's not very familiar to some of your critics.

MR. MAYFIELD: And there's a very good story to that.

MEMBER POWERS: I bet there is.

MEMBER FORD: Claud, you mentioned that those five data points come from the -- towards the lower shelf area.

MR. PUGH: This is the four which is the --

MEMBER FORD: The five, rather.

MR. PUGH: Right here. And they are down.

MEMBER FORD: If you had done the tests on the specimens on the upper shelf region, you would presumably start to deviate from that one to one line towards the upper part of it.

Do you and -- does that, in fact, happen?

MR. PUGH: Yes. The short answer is yes.

MEMBER FORD: And so therefore the use of the fracture mechanics is not necessarily fickle to low fluence stations. Now I recognize --

MR. PUGH: No, that's the wrong conclusion, I think.

MEMBER FORD: Less irradiated pressure vessels. Am I going the wrong way?

MR. MAYFIELD: No, you're going the correct way. And in fact, we've had some situations. Claud mentioned this low upper shelf energy weld that we tested. That was one of the motivations for us developing the elastic plastic fracture effort and applying, in fact, Jim Rice's J analysis and subsequently the development of the JR curve, so there's a lot of that work that goes into doing an analysis for a fully ductile condition.

MEMBER FORD: The reason I asked the question --

MR. PUGH: I understand better your question now, where you're coming from.

MEMBER FORD: You show a good correlation for lower shelf, i.e., embrittled conditions. When you go to fully ductile conditions, i.e., beginning of life, then this must fail to a certain extent, unless you use --

MR. PUGH: In the sense of being accurate in your prediction of failure, you will be ever more increasingly conservative.

MEMBER FORD: Correct.

MEMBER RANSOM: I understand the vessels are fabricated using axial welds from rolled plate?

MR. PUGH: Most of them are.

MEMBER RANSOM: How do you know that the data you're using is characteristic of what's in the weld region?

MR. PUGH: Well, because this is a weld, that's a weld and some of the other experiments are involved with welds.

MEMBER RANSOM: The previous curve you gave though is for homogeneous material.

MR. PUGH: Data have been collected on weld material as well.

MR. MAYFIELD: In ferretic material, the welds, the forgings, the plates, they all show the same characteristics in transition, same characteristic temperature.

The things that change will be the index temperature, where that curve is on the temperature axis. But all the other characteristics of the curve remain the same.

MR. PUGH: But I'm glad you asked the question, Dr. Ransom, because some of these are absolutely weld material, even in these ITVs.

I realize our time is getting away from us.

CHAIRMAN APOSTOLAKIS: Yes, we really have to finish at 10.

MR. PUGH: If one looks, then at an application of the ASME approach for these ITVs and looks at a ratio of the load factor, that is, say the failure pressure versus design pressure, ASME design pressure, these are roughly the -- they're not roughly, these are the ratios. There are roughly three for all cases, except this one and this is at nozzle -- excuse me, this is that very deep flaw was 80 some percent way through the wall to begin with.

If you apply to a full-scale reactor vessel, then the same technology, these are designed failure

-- these are fracture failure curves for a full reactor pressure vessel under like 2250 psi. Based on the stress design limits, here's the operating condition. Here's the design level. Here is the fracture mechanics for the quarter T analysis allowed by the ASME code. Quarter T would be like 2 point something depth. Compare it to this curve and you'll see like a factor of three safety factor that exists in applying the ASME code versus failure curves. So you're talking about that lower bound curve, etcetera. That's not the end of the story in terms of being conservative when applying ASME code.

MEMBER WALLIS: What's the parameter on these curves, is it inches?

MR. PUGH: Those are flaw depth.

MEMBER WALLIS: Flaw depth, okay.

MR. PUGH: So even with flaws much deeper than ASME code quarter T, still we see tremendous margin before one gets to a predictive failure condition.

After this set of experiments is done, the question -- not done, but during their performance, a question came up about thermal shock with the most severe condition thought to be a large break LOCA where one would have a loss of pressure, but a very extreme thermal shock. So the set of experiments using a vessel something like this where -- not something -- this is a vessel, 6 inches thick, 1 meter diameter, dunked in liquid nitrogen from a temperature of about 280 degree C to give the thermal shock.

This type of experiment gave the through the wall variation as one would anticipate in reactor pressure vessel for stresses. This is through the wall versus the --

MEMBER WALLIS: Did you get film balling at the liquid nitrogen?

MR. PUGH: Interesting you should ask that. A tremendous effort went into developing a coating that would --

MEMBER WALLIS: Would nucleate.

MR. PUGH: Would nucleate, yes. It was a rubber cement process that Dick Sheverton actually got the genesis idea from some people in France and worked on it, a very high priority topic to perform these experiments, yes sir.

As a flaw exists in a particular place in the wall during the thermal shock, though as the thermal shock passes, the stresses and stress intensity on that flaw will peak and start down and it may not reach the critical value of K_{1C} until later, namely when it's cooled, and this can give rise to something called warm pre-stressing, which will inhibit the initiation of the flaw in certain sense, especially for deep cracks in thermal shock conditions.

So these experiments were to examine behavior under thermal shock, one prestressing, whether or not arrest would occur in a rising K field or above the ASME limit.

As a qualitative example which was actually done quantitatively, TSE-5, it was predicted that three crack initiation arrest events would occur in this one thermal shock. It was predicted that since the initial flaw was a tenth of the way through, that it would go to halfway through if (1) prestressing occurred on the third event. If it did not, it would go to .7. You can see from the cross section going from the inner radius to the outer radius of this cross section, indeed, three jumps occurred and propagated to .8.

MEMBER FORD: Would you mind just going back to the previous graph. This is a question that came up when we -- I forgot which one it was, one of the reviews. What is the data to support that fluence distribution or is it calculated?

It seems to me to be very important in terms of whether the crack arrests or not.

MR. PUGH: Certainly, the toughness

through the wall depends upon the fluence through the wall and I'd rather defer the question to Mike.

MR. MAYFIELD: It's a calculated fluence. There have been some experimental activities looking to benchmark the calculations. There's something of a raging debate that probably that Dr. Bonaca is in a better position to describe than I am. There's some raging debate among the purists about whether the attenuation function that we have incorporated in this which is a power law attention, as to whether that really is technically sound or not. The experimental data that we -- what limited experimental data there are, looking at through wall attenuation suggests that it's reasonable. It may not be precisely correct, but it's within the uncertainty of the experimental data. It seems to work fairly well.

MEMBER FORD: I think we asked the question, but I can't remember the answer as to what the effect of that uncertainty of that fluence distribution would be on your resulting calculations. Is it a huge swinger?

MR. KIRK: Since most of the flaws that play a significant role in PTS are very close to the inner surface, because that's where the thermal shock is the greatest. The differences between -- as Mike said, there's a debate as to how fast the exponential fall off is, but since most of the flaws that get you occur within say 10 percent of the thickness between the inner wall and 10 percent of the way in, the differences between those two attenuation functions is really pretty small.

MR. MAYFIELD: I think the other thing I would say is that the uncertainty in the calculated fluence at the vessel, the inner surface, far dominates the uncertainty of the through wall, the uncertainty associated with through wall attenuation.

MEMBER FORD: And there's more data to back up that early -- presumably more fluence data --

MR. MAYFIELD: Data to support the discrete ordinants of code approaches than calculating the inner surface.

MR. KIRK: Where you may wish to re-raise this question, where it, in fact, is very significant, is when we get to talking about heat up and cool down limits because they're controlled by notional flaws that are a quarter T thick, both on the inner diameter and outer diameter. So you've got to attenuate to all the way to three quarter T in a heat up or cool down calculation and there, the differences between the functions that are advocated by one group of experts versus another is indeed very significant, but for PTS, since the flaws that are important are on the inside and like Mike said, the greater uncertainty is on the inner wall fluence, it's not that huge a factor.

MEMBER SHACK: But wouldn't it have a more significant effect on your conditional probability of failure. I can understand your argument for the conditional probability --

MR. KIRK: That's correct. Yes. Yes, that's correct.

(Slide change.)

MR. PUGH: Just one last slide, I believe, that I'd like to show you briefly and that is if you look at the -- in this presentation at least on this slide, four of the TSE experiments, if you look at the calculated initiation values and the calculated crack arrest values, some of this -- the experiments were multiple initiation and arrest; initiation and arrest. You can see that the data fall in a good trend with that from small specimen data. These are approximately upper and lower bound for small specimen data and you see the initiation values follow the trend very well and so do the arrest values.

All the work, of course, that these two

sets of experiments represent, it was like 15 years of work. We really can't do it much justice here in 30 minutes or less, but hopefully I've given you a little snapshot to show you that the validity and the applicability for operating conditions in the case of ITVs and an accident condition in case of a large break LOCA, the results of these experiments suggest LEFM is applicable to fracture prevention and in toughness prediction.

MEMBER WALLIS: It suggests to me that something is different about the French work from the other work. When you say follow the trend, that's a very gross statement. If you actually look at the data from one lab, the trend is not obvious. The gross way, you're within your bounds.

MR. MAYFIELD: You're talking about the French data?

MEMBER WALLIS: All the others, any other data. I don't want to prolong this, but you said follow the trend. To get a trend, you really need to look at one lapsed data or something other than saying it's within some statistical --

MR. BASS: This is Richard Bass from ORNL. I'll make a comment on the French data. That's been a topic of discussion for at least 20 years and it's our feeling that there's doubt about the actual temperatures that were made, the data that was gathered in those experiments.

MEMBER WALLIS: So we should throw it out?

MR. BASS: The French, that's their data and that's what they provide. It's not our place to throw it out. We can --

MEMBER WALLIS: You put uncertainty bounds on the temperature?

MR. BASS: Yes.

MR. MAYFIELD: Richard, as Dr. Apostolakis said, we just need to make sure we hit 10 o'clock.

CHAIRMAN APOSTOLAKIS: No, 10:15.

MR. MAYFIELD: 10:15.

(Slide change.)

MR. BASS: In keeping within the rules, my name is Richard Bass, Oak Ridge National Laboratory. And I am the current manager of the Heavy Section Steel Technology Program and I want to spend the remaining time of our presentation focusing on the third set of large scale experiments that were carried out in the 1980s at ORNL. These are the so-called pressurized thermal shock experiments. And again, these experiments were performed to confirm and develop a fracture analysis methodology.

The particular feature that we introduced in these experiments is subjecting these flawed vessels to a coordinated thermal shock and internal pressure loading. You see in the plot on the right hand side here, the loading factors of not only the temperature that we investigated in the original thermal shock experiments, but now we introduce a coordinated pressure transient that apply to the inner surface of these vessels.

The objective here is to again to coordinate these loading factors so as to produce a desired evolution of crack driving forces on the flaws of interest that we've installed in the vessel. In this particular case, we see an example of a transient where we have increasing K, crack driving force on the shallow flaw which then reaches the maximum, the flaw, the tirade of change of K crack driving force on a shallow flaw which then reaches the maximum. The flaw -- the tirade of change of K then becomes negative. We say that this flaw then is subjected to simple warm prestressing and warm prestressing was a particular effect that we wanted to investigate in both the first and second pressurized thermal shock experiments.

Also, we wanted to look at the -- get a better understanding of the nature of cleavage crack arrests at temperatures near or above onset what we discussed previously here as the Charpy, the onset of the ductile upper shelf.

The PTSE-2 experiment addressed low upper shelf energy steel. We're not going to have time to talk about that this morning. We're going to focus on these first two elements that were studied in the PTSE-1 experiment.

In both of these experiments, we had long surface cracks that were inserted into the ITV vessels, pictures which you've already seen. Shallow flaws subjected to this coordinated loading conditions to achieve specific objectives and we'll look at what those objectives were shortly and of course, one of the elements that we're very interested in here this morning is the features of LEFM that were used to design these experiments and also to analyze them. And specifically, we did a good deal of small specimen fracture toughness testing that we used to construct our fracture test as models. That's the essence of the LEFM approach. And then we used the -- applied our methodology to design the loading conditions which are properties and so forth to achieve these objectives and our particular tools in doing that, in this -- these two experiments performed in the 1980s, the so-called OCA code and OCA was a precursor to the FAVOR code. FAVOR and OCA still used basically the same methodology for doing deterministic structural and fracture mechanics calculations.

So we'll look briefly at some results from OCA in analysis of these experiments and also touch on application of the current FAVOR code to, in essence, demonstrate that we still get the same answers. Next slide.

(Slide change.)

MR. BASS: Okay, this is the vessel. We've seen pictures of this before. Again, we've got a wall thickness of about 6 inches, 148 millimeters and the vessel is long enough to give us this constraint condition that -- plain strain/constraint condition that we're looking for in our test specimen that we can then transfer or use to evaluate our methodology that we can then transfer to the RPV.

Again, you see here, we have a flaw in the outer surface. In the PTSE-1 experiment, the first experiment that we're going to focus on, the flaw was 12.2 millimeters in depth here on the outer surface, one meter in length.

The photograph on the right hand side, you see this test vessel is being lowered in what we call a shroud or an outer test vessel. That outer vessel serves two purposes. First of all, to heat the specimen up to the test temperature of approximately 290 degree Celsius operating temperature for an RPV; and then since this was then connected into a large thermal hydraulics test loop, the outer vessel served as a shroud and we had about a one-half inch gap between the outer shroud and the inner surface here that would then allow us to thermally shock the outer surface of the vessel and the flaw with a fluid temperature that varied anywhere from say 15 degrees Celsius down to minus 29 Celsius.

We started out in the first transient of this experiment using water because we thought we could get away with avoiding the hassles that go along with using a refrigerant, but the second and third transience necessitated we use a mixture of methanol and water so that we could lower the coolant temperature and achieve a more severe thermal shock.

That's the essence of the experimental set up that we used in this program.

MEMBER ROSEN: How do you make the flaw and how do you know that it's representative of the real flaws in service?

MR. BASS: Well, a lot of work over the

years has gone into that and this was a well-developed methodology. We used an embrittling weld technique that was then hydrogen charged, a little bath around this thing and put a potential across it of sulfuric acid that spontaneously generates a very sharp flaw. Of course, there was a lot of research that went into this, trials and so forth. By the time we got around the mid-1980s of doing these experiments, this was a very well-developed methodology.

I think we're ready to go to the next slide.

(Slide change.)

MR. BASS: Okay, this --

MR. MAYFIELD: Richard, could I say one thing?

MR. BASS: Sure.

MR. MAYFIELD: The second part of your question was about how are these representative of what flaws are in vessels in service. By and large, we don't think these flaws are in vessels. This was a test condition designed to give us -- to support this particular analysis. We don't really think there are 10 percent wall, deep cracks that run for extensive lengths along the vessel surface.

MEMBER ROSEN: Yes, I know that, but I was just asking really about the morphology of the crack.

MR. BASS: I took your question to mean about the sharpness of the crack and of course, in this technique you get a very sharp crack. That certainly would be -- a representative say a sharp defect in a weld or between clad weld interface, that kind of thing.

This is a diagram here that illustrates the applied K factor, the crack driving force as well as the crack initiation toughness and the crack arrest toughness for this material. This is really a schematic. This is not something from an actual test. The purpose of this particular slide is to describe the planned transient in this experiment and then compare that with what actually happened.

The original plan called for doing this experiment in a single transient, but failing the objective of achieving all of these in a single transient, then we would do a backup second and third transient, but we want to give this a first shot of let's see if we can do it all in one effort. That actually did not work out. We actually ended up doing an experiment in three transients. But we'll get to that shortly.

The objective in this PTSE-1 experiment

was to load the crack, this shallow crack with the thermal shock up to Point A here where the crack becomes critical. We would then experience, observe a cleavage initiation of the flaw. It would then arrest at the crack arrest toughness curve at a deeper point in the wall. We would continue loading this flaw. It would then go into a mode of warm prestressing here, prestressing where the K dot is negative. And the crack will not initiate when K dot is negative in cleavage.

The crack becomes critical here and it crosses again the K_{1C} curve at this deeper point. It becomes critical at this point, but it does not initiate because of warm prestressing. We apply pressure loading here to alleviate the one prestressing, reload and at some point, F here, we want to achieve a cleavage reinitiation and then drive the crack very deeply into the -- say up to 40 percent of the way through the wall of the vessel, 30 percent or so to -- and get a cleavage arrest at a temperature that corresponds to the Charpy upper shelf of this material.

Those were the objectives in the transient and if we go to the next slide we'll see what actually happened.

(Slide change.)

MR. BASS: This particular diagram on the left plots again the applied K_1 loading of the flaw. The K_{1C} fracture toughness and the K_{1A} toughness for this material as a function of crack tip temperature. We do this -- this is a very popular curve or type of curve to use in depicting these analyses because we can draw the K_{1C} and the K_{1A} curves, fixed, on the plot for the crack tip. And then look at the evolution of the transient loading applied to the flaw and compare it with these fracture toughness curves. Remember, these are our -- we haven't talked about this before, but these are our predictions from small specimen testing. We went out and did a lot of testing of 1T specimens. We did size corrections of the 1T specimens and we developed a lower bound fracture toughness curve to that data. That's the K_{1C} curve you see here.

We did similar testing of small specimens for K_{1A} crack arrest and we developed a median curve that you see here to that crack arrest data which was also size adjusted. These are very small specimens. We wanted to adjust them to a plain strain constraint condition we used in methodology developed by George Erwin long ago to do that.

And then let's talk about what happened in the actual test in these three transients. The first transient we see here is A. This is the original flaw of 12 millimeter depth. OAW is .08. The time is moving to the left here. The crack tip is cooling from the thermal shock and we see that the crack first becomes critical at this point where the applied curve crosses the K_{1C} curve, fracture toughness curve.

At this particular point, due to several factors, the flaw is just going into warm prestressing. You can see the K dot becomes slightly negative as it hits this point and consequently as we move in here with the one prestressed flaw, we do not get an initiation to recover from that warm prestress effect. You require more loading of the flaw which we did not have in this pressure transient. Consequently we have here a highly critical crack which did not initiate. The ratio of the applied K to the K_{1C} here is on the order of 2. So this is really an unambiguous testimony to the effects of warm prestressing.

The applied load achieves the -- it exceeds the fracture test by a factor of 2.

Well, recognizing that we had a warm prestress crack and we didn't have quite enough loading power at this particular point in our planned pressure transient to get an initiation, we went back. We cranked up the -- basically doubled the pressure level in the cylinder and we lowered the coolant temperature to introduce a more severe thermal shock. That's the B transient here and you can see it does not initiate at the K_{1C} curve. We get a little bit supercritical with the crack. We then did get a cleavage initiation. It jumped to an OAW.165, about 24 millimeters in depth. The thermal shock, pressurized thermal shock continues. We're continuing to increase the load. The flaw became critical again at this particular point, just crossing the line, the K_{1C} curve here we then go into warm prestressing phase again without an initiation.

LEFM would have predicted an initiation, of course, at this point, but just across the line there was no initiation.

Again, this was a very strong demonstration of the effects of warm prestressing in this PTS event. You see here we have then an applied K value which well exceeds the K_{1C} fracture toughness, but K dot is negative, you're going to get an initiation of the crack.

The last experiment we simply cranked up, ratcheted up the pressure up to a little more, dropped the coolant temperature a little bit more and we got an initiation here pretty much on the line, the intersection with the K_{1C} curve and the arrest point here at a crack F about 41 millimeters, very close to the small specimen K_{1A} curve. And finally the system runs out of gas at this point here.

So we achieved all of the objectives here. We wanted to investigate the warm prestressing. We had two very strong examples of that here in the A and B transients. We were able to drive the crack here into temperature regime that was on the Charpy upper shelf of material and get a very high crack arrest value.

MEMBER FORD: When you say it ran out of steam, you mean you couldn't apply any more pressure to the system to push up --

MR. BASS: That's correct. Yes, the pressure -- our accumulator just didn't have anything left to -- and we would not have wanted to do that anyway. We had planned to -- I didn't mean to imply that we were trying to get to the line again. We wanted to preserve the fracture surfaces which meant that we did not want to burst the vessel. We were very happy with the arrest where it was.

MEMBER WALLIS: So it's pretty good in view of the uncertainties you have in K_{1A} and K_{1C}.

MR. BASS: That's the story here and this is really -- this is the punch line here. And this whole story about pressurized thermal shock and applications of LEFM and the methodology, you're looking at it right here. And just in a few words we've got the K_{1C} curve that was generated from small specimen data.

MEMBER WALLIS: That's the best estimate, K_{1C} is that what that is?

MR. BASS: No, that's the lower bound.

MEMBER WALLIS: Oh, lower bound.

MR. BASS: It's the lower bound to the size of just this data from small specimens for this particular material and the K_{1A} curve that you see here, this is again --

MEMBER WALLIS: That's why it can cross and not do it because you've got --

MR. BASS: It's a lower band. We've got a long flaw. You've got a lot of opportunities for sampling of defects along that flaw, so you would expect to get -- you hope to get very close to the line and in effect, we see that we did achieve that objective of getting initiations very close to the smallest specimen predicted curve, and likewise, with the K_{1A} curve. This is unretouched curves and data. This is the median toughness curve for the K_{1A} small specimen data. Again, it's adjusted for size effects.

Also, down here you'll see this is the ASME section 11 K_{1C} and K_{1A} curves and you can see that these are very conservative in predictions. As a matter of fact, we did a code analysis on this experiment and the code analysis said that we would fail the vessel in three crack jumps, 40 seconds into the transient. Obviously, that did not occur.

MEMBER POWERS: The code curves, the conservative ones are the product of reflection by a large number of people thinking about a large number of things. Why did they make them so much more conservative than your curves that have been --

MR. BASS: Oh, we're -- remember, we were trying to conduct a particular experiment here.

MEMBER POWERS: I understand what you're doing --

MR. BASS: We're not --

MEMBER POWERS: I'm not asking about what you're doing. I'm asking about what the code people are thinking.

MR. BASS: I wasn't there. I can't tell

you.

MR. MAYFIELD: I'm sorry, I didn't hear the question clearly.

MEMBER POWERS: What I'm asking is, Mike, you've got some data curves where your particular experiment, your curves are -- the code curves are substantially more conservative and I'm wondering what was the thinking of the ASME Committee that moved those curves in this conservative direction.

MR. MAYFIELD: Okay, the approach that the code has taken is to develop generic fracture toughness curves and then to try and index those to a particular material based on the nil ductility temperature and some adjustment from there.

So their intention was to develop a conservative generic curve so that when you picked what was believed to be the limiting material in the vessel and index the curves to account for the embrittlement of that limiting material, you still have a conservative representation.

They were not trying to do best estimates. That's where this lower bound concept came from. There's a lot of concern and I think a number of us on the staff share the concern about the level of conservatism in those ASME curves and the approach taken. But the underlying piece of it was to develop generic curves so that you didn't have to have exactly the right material from each pressure vessel and you could still account for a lot of the variation in material.

MEMBER POWERS: So what you're saying is the shift that's manifest in this figure is capricious. It's not in response to any particular phenomenalological thing that they're worried about.

MR. MAYFIELD: That's correct.

MR. BASS: The plot on the right hand side shows the plot of the --

MEMBER ROSEN: I didn't understand what you said about what the ASME code would have predicted. You said a code predicted it would have failed --

MR. BASS: If you do a code analysis, what it would -- what the code analysis showed was that the crack would propagate in three jumps and would penetrate the wall and fail the vessel. I'm sorry.

MEMBER ROSEN: Okay.

MR. BASS: The plot on the right hand side is the crack arrest plot of the arrest toughness data versus -- now this is normalized temperature relative to RT_{NDT}. And again, we've got the two values here from our PTSE-1 experiment and some other data generated from various sources: Japanese data, the thermal shock experiments that Claud has discussed recently, and again, we've got the French TSE data that's in an area of its own down here. And finally, a wide plate -- the first wide plate experiment that we did here at NVS back in the 1980s.

A couple of things about this plot. First of all, this K_{1A} curve is from the PTSE-1 small specimen data here. And you see, as we look at it, we think that's a pretty good representation of these large scale experiments when you reference them to the RT_{NDT}.

One other thing I did not point out here is that one of the landmark pieces of data that came out of PTSE-1 was that we calculated or measured, I should say, a very high crack arrest value here, roughly 300 MPA root meters which is well above this implied upper bound in the ASME Section 11 curve of 220 MPA root meters. We demonstrated that in a thick section, highly constrained thick section that we could generate a very high arrest toughness value here without any sign of intervention of stable or unstable tearing to muddy the picture here.

And also, you will see down here that this is well above the onset of the Charpy upper shelf here by about -- for the PTSE-1 material by about 30 degrees Calvin.

Next slide.

(Slide change.)

MR. BASS: This particular slide gives an example of a recent analysis that we carried out at ORNL where we went back and used the current FAVOR version of FAVOR code to analyze the first two transients, A and B. We're only showing the transient B here and we find again that we get basically the same solutions that we generated back in the 1980s with OCA and we would expect to do that. We're using basically the same methodologies. And if your input is the same, the output should be the same and sure enough it was.

On the left hand side here, we see K_1, K_{1C}, K_{1A}. Again, you've seen the curves versus the crack tip temperature. This is what we call the classic LEFM model, classic LEFM prediction. When you hit K_1 equal to K_{1C}, you forecast the crack propagation, arrest the K_{1A} curve. This prediction is something like 19.2 millimeters. Actually, in the experiment, the flaw initiated a little bit later in the transient and it jumped a little bit deeper, as a consequence to about 24 millimeters. Again, we would have predicted initiation here. It did not reinitiate at this particular point, so we did not see that in this second transient.

These are the actual calculations over here.

MEMBER WALLIS: Did FAVOR predict reinitiating or not? You don't show that.

MR. BASS: Well, yes, FAVOR would -- FAVOR is -- FAVOR is classic LEFM, so it would -- we would predict a reinitiation at this point.

MEMBER WALLIS: You don't show it.

MR. BASS: Well, we didn't get it in the experiment. This is the analysis of the actual experimental data over here with FAVOR. This is what would have been predicted.

You see that we did not get the second jump.

MEMBER WALLIS: Is that because of your uncertainty in the temperature turnaround or --

MR. BASS: Well, it has to do with the nature of cleavage fracture. Cleavage at a given temperature is a distribution and we think a Weibull distribution in our models now. And, of course, where -- you can't anticipate that you're going to initiate at the earliest possible -- the lowest possible value in a methodology here where you're looking at a distribution of possible toughnesses.

MEMBER WALLIS: There's also the K dot that you've got to take into account.

MR. BASS: K dot here, yes, after this point. The current version of FAVOR and I don't know if I should even mention this, does not currently incorporate warm prestressing, but Mark, do you want to say anything about that?

MR. KIRK: The next version probably will.

MEMBER WALLIS: I would think it should. It seems to be a significant defect.

MR. KIRK: Yes.

MR. BASS: Let's move on to the last of the technical slides here.

(Slide change.)

MEMBER WALLIS: It's wonderful to see some data for once. It's a technical discussion one could follow more or less.

MR. BASS: This is our view of the world summarized here insofar as the applicability of LEFM, FAVOR, OCA and so forth to the assessment of RPV integrity under pressurized thermal shock. We saw in these tests that we did achieve cleavage fracture in these large scale tests and an important element here is that they were consistent with the small specimen data.

Another really important element that came out of these tests was the warm prestressing evidence. Clearly, warm prestressing is a reality. It's there and very effective in certain types of transients.

And in our punchline here the observed cleavage-crack behavior in these thick-section experiments has been well described by the LEFM methodology and that methodology is embodied in the current version of the FAVOR code and it is historically consistent with all of the other calculations that we made in these large scale experiments.

MEMBER KRESS: If you were to put warm prestressing into FAVOR, you would just simply say if you ran into the negative thing you just stop the crack there?

MR. KIRK: You wouldn't, as Richard said, you wouldn't allow the crack to reinitiate like -- if I can point -- you wouldn't allow the crack to reinitiate on the downward slope whereas now -- well --

MEMBER KRESS: So you have a problem here though with -- even if you had warm prestressing in FAVOR, you would have hit K_{1C} and would have thought you would have initiated there.

MR. KIRK: In the current weld you need to be -- it's a little bit more complicated than that because in the current version of FAVOR, because we're treating the uncertainty in both K_{1C} and K_{1A} as aleatory --

MEMBER KRESS: So it depends on which sample you --

MR. KIRK: It never really initiates. You've got a probability of initiation. You've got a probability of failure, but suffice it to say right now -- where is the pointer? I have got to point and talk into this thing. Right now, in FAVOR, we count the probability of -- we allow a crack to initiate in this area. So we're essentially counting up that probability whereas we shouldn't be. The reason, warm prestressing has been around for a long, long time. In fact, it was around, well recognized and well researched when SECY-82-0465 was published and the original basis for this rule was established.

It wasn't included in the calculations then, not because anybody on the staff didn't believe the physics of the situation, but because at that stage we were using what were called idealized thermal hydraulic transients. So instead of having all of the bumps and squiggles of a real thermal hydraulic transient, they were idealized as exponential fallouts so the concern was that you might believe, based on the idealized transient that warm prestressing had occurred when, in fact, in the real transient it wouldn't have.

We've revisited this recently and in fact, warm prestressing is next on our list of things to add in and what we anticipate it will do is stop a lot of cracks from going all the way through the wall.

MEMBER KRESS: Yes, it's conservative to not --

MR. KIRK: Yes, indeed. It's conservative to not put it in.

MR. MAYFIELD: Let me add one other consideration. As Mark noted, these were idealized transients. And the concern was that the operator might intervene, particularly when you've got primary fluid escaping somehow through a break or whatever. The operator might intervene, isolate the break and then a lot of the system would tend to repressurize which negates the warm prestressing effect.

The one thing that we've done that I think is a significant improvement now is to bring in operator actions explicitly. So now you can go back and within the vagaries of that analysis. You can now go back and include whether you will or won't repressurize the system. Before, with the idealized transients, without really having something you could track, it just didn't seem to be a good idea to include warm prestress.

MR. KIRK: Are there any more questions regarding the technical part of the presentation?

(Slide change.)

MR. KIRK: Okay, then to summarize and of course, the Committee is, as always, free to draw its own conclusions. We believe that the NRC research programs have established both the calculational methodologies and the empirical data needed to enable our assessments of RPV fracture resistance for both routine loading conditions and most importantly, in this context, accident conditions using LEFM.

We have shown that LEFM predictions of both crack initiation and crack arrest and of course the combination of them would leave to vessel failure or not agree well with the results of prototypic large scale RPV experiments and consequently suggest that LEFM is indeed an appropriate methodology for use in assessments of RPV fracture resistance.

(Slide change.)

MR. KIRK: The logical conclusion from that is to turn off the funding spigot. However, in our day to day operations the staff is routinely motivated to take this yet a bit further. We've got -- there are both regulatory and commercial motivations to not stop here.

On the regulatory side, the licensees are now fairly routinely making exemption requests to our current LEFM based methodologies and they're even queuing up to make exemption requests to a modified PTS rule that we haven't even got in place yet. We have no systematic way to deal with those. The easiest example which I think several members of the Committee are familiar with is the licensees have routinely come in with request to use the master curve which is an EPFM based methodology. Right now, we're dealing with that on a case by case basis and right now we don't really have any systematic way in place to assess the appropriateness of those applications --

MEMBER ROSEN: What's their underlying motivation?

MR. KIRK: Their underlying motivation is down in the bottom, is that in a deregulated -- or rather, in a regulated energy environment, they were paid on a dollar per kilowatt hour basis. They were paid based on capacity. So they had a plan sitting there. They were paid based on whether they could generate.

Now they're in competition with everybody else, so they're motivated to do things with their reactors that they never would before. For example, one of the things that we said on Wednesday, 10 CFR 50.61 says that if you're in danger of crossing the line for the PTS rule, you're obligated to install flux reduction which reduces the number of neutrons going through the steel, reduces the embrittlement rate.

Lots and lots of plants have flux reduction. Of course, you pay a penalty for that in production. The Beaver Valley nuclear plant which is, I believe, per current regulations, .5 degree Fahrenheit from the current screening limit, has flux reduction in place, but believes, based on the use of new technologies, namely the master curve, that they can justify removal of that flux production, increase their productivity, increase their profitability and still stay below the regulatory screening limits. Removing flux reduction is simply something that nobody would ever have thought of or even considering putting up NRR in a regulated environment. Also, other motivators are that some plants that aren't as close to the line as .5 degrees Fahrenheit, have to make significant economic decisions like whether to replace the steam generators. In order to make that economically feasible, they have to show to their business people that those, the cost of those generators can be amortized over 20 to 30 years. If you've got a plant that's sitting within 5 degrees Fahrenheit of the current screening limit which some are, and they need to buy a new steam generator, and they've only got 10 years left in their current license, the business people will say unless you can show us that we're not at risk at bumping up against this thing that we understand is called the PTS Rule, we're going to shut you down. So they have significant economic motivation to use available technology which right now we've done and this gets over to our activities. The gentlemen who have been so helpful in making this presentation have been contractors for us for years, developing elastic plastic fracture mechanics methodologies.

We right now have on the shelf, I'd say probably 85 to 90 percent of the research that's needed to have a systematic elastic plastic fracture mechanics evaluation methodology. What we're engaged in right now in the current HSST project is what we've called FAVOR^{EP} development which simply means an elastic plastic version of FAVOR which would enable what when in place will enable the staff to do systematic and rigorous reviews of licensees' requests that right now --

MEMBER POWERS: Can I ask the question? If the economic incentive all lies on the part of the industry, why don't they pay for this next 10 percent that has to be done? You said you had 80 to 90 percent. Why don't they pay for the 10 to 20 percent?

MR. KIRK: Taking it from the industry's perspective which --

MR. MAYFIELD: Let's not speak for the industry.

MR. KIRK: Okay.

MR. MAYFIELD: Okay?

MR. KIRK: Okay.

MR. MAYFIELD: The reason the staff does these things is to provide the staff an independent capability to perform these kinds of analyses. And mixing the industry perspective in with this, to take money from their pocket or to take their code gets to be problematic because we still need an independent capability. This is an area where the staff does have significant expertise through staff capabilities and contractor capabilities.

We routinely go through this and ask ourselves the same question, when is enough enough? We believe that this next piece is justified and supportable and to support staff capabilities.

MEMBER POWERS: Believe it or not, so do I.

MR. MAYFIELD: But that's the why. We think that's something the staff needs to do.

One other point I'd like to make as we close, Dana, is that we've talked a number of times and you and I have had some enjoyable banter about whether we should or shouldn't continue this program. The fact is the budget for pressure vessel related activities has declined significantly over the years. We can agree or agree to disagree on whether it's gone low enough, but the fact that when we were doing the kinds of large scale experiments that have been discussed this morning, we were up what $7 or $8 million for the combined pressure vessel testing and embrittlement activities. That has declined significantly. We can certainly provide the Committee or come back and talk to you about what that's looked like over the last few years, if you're interested. But I wouldn't want to leave the impression that the funding level today is anywhere near where it was in the 1980s when we were doing this kind of work, 1970s and 1980s.

MEMBER FORD: Are there any other last minute questions? Mark and --

MEMBER SIEBER: Yes, I'm curious about one thing you said when you were discussing the economics of flux reduction. Flux reduction is a low leakage loading pattern which basically has a peak to average that's pretty substantial. The hot fuel is in the middle of the core and thrice burned fuel is on the outside edge. And that gives you less fluence to the vessel walls.

When you say there's an economic incentive to abandon that kind of loading pattern, I presume that what you're saying is if you want to do a power uprate, you would try to flatten the flux which would place fresh assemblies more on the outside. That has a fuel cost penalty. It does have a rating advantage.

Is that basically what you were saying?

MR. KIRK: Yes, that's it and to be fair, in discussions I've had with various licensees, different operations seem to take a different view of whether flux reduction is an economic penalty or not. Certainly, some plans do, but to be fair, we should also say that other plans don't.

MEMBER SIEBER: Well, at the current license power at Beaver Valley, it can run 100 percent power with a flux reduction core. And a flux reduction core is cheaper from the standpoint of dollars spent than one that has flux flattening.

I used to be the fuel guy there.

MEMBER FORD: Before handing it back to you, George, I'd like to thank very much everybody for coming and we have been informed and I really appreciate it. Thank you.

CHAIRMAN APOSTOLAKIS: Thank you, Peter. We'll recess until 10:35.

(Off the record.)