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METAL COMPONENTS WORKING GROUP

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NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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METAL COMPONENTS WORKING GROUP

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Room 1167
1717 H Street, N.W.
Washington, D.C.
September 30, 1982

The Working Group met, pursuant to notice, at 8:30 a.m., MIKE BENDER (Chairman of the Working Group) presiding.

PRESENT:

ACRS MEMBERS:

M. BENDER (Chairman)
P. SHEWMON
D. WARD
F. REMICK

CONSULTANTS:

I. CATTON
Z. ZUDANS
MR. ABBOTT
MR. BINFORD
MR. KOUTS
MR. THEOFANOUS

CONSULTANTS (Continued):

MR. WECHSLER

MR. IRWIN

ALSO PRESENT:

BILL BOCK, ACRS Fellow

DESIGNATED FEDERAL EMPLOYEE:

AL IGNE

* * *

P R O C E E D I N G S

(8:30 a.m.)

MR. BENDER: This meeting will now come to order. This is an open meeting of the Metal Components Working Group of the Advisory Committee on Reactor Safeguards. I am Mike Bender, the subcommittee working group chairman. The other ACRS members present here on my left are Dr. Axtmann, Dr. Shewmon, Mr. Ward, Mr. Remick. In addition, we have a number of consultants for the working group. Going around the table, Mr. Catton, Mr. Abbott who is not here, Mr. Binford, Mr. Kouts, Mr. Theofanous, Mr. Wechsler, Mr. Zudans, Mr. Irwin. In addition, there is Bill Bock, our ACRS fellow who has been working actively with the group, and on my right is Al Igne, who is the designated representative for the NRC.

The purpose of this meeting is to hear and discuss with the NRC staff their pressurized thermal shock position. The views of others will also be heard. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the government and the Sunshine Act. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on September 13, 1982.

A transcript of the meeting is being kept, and it is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard. We have not received either written or requests for oral statements during this meeting, so no time has been set aside. However, if there is anyone who wishes to make comments, if you will let Mr. Igne know what it is you would like to make a comment about and we can work out some arrangements and provide some time, we will certainly do so.

I wanted to add a few thoughts about our situation at the moment, partially to get the working group up to speed and partially to help the staff in its organization and presentation this morning.

We have received what I would presume to be near to a position from the regulatory staff on a screening procedure for deciding when thermal shock is a problem that needs special regulatory attention. I don't know whether that means regulatory action. And hopefully, all of the consultants and members of the subcommittee have had a chance to take a look at what has been proposed and we will be able to make intelligent comments on the presentation or what's in the written material as it is presented.

Steve Hanauer has offered to try to provide a coherent discussion of it, and we have set aside a couple of hours for his presentation. And hopefully, he will tolerate some questions, but if we work on it too hard we may not hear the whole story, so I would like to encourage you to address your questions to him in the line of trying to be sure you understand what he is saying rather than trying to digress into areas that may need to be taken up later on today.

I have sent out for the working group's consideration what amounts to a draft discussion of the issue as I understood it, with corrections for various members of the working group and particularly consultants who have suggested things that should be included or alterations in the way words should be said.

I don't offer that discussion as either a highly professional technical document nor a literary masterpiece. It was only intended to try to get together as well as I could in the a short time the information that had been presented, and offer some kind of interpretation.

I would again urge people that have strong feelings about improving it to provide alternative discussion material that could be included in the document or added as an appendix. Any way which makes any sense.

A lot of the work was done by Bill Bock, and I appreciate his diligent effort to respond to a lot of recommendations from a lot of different people. The working group has not yet reached the position on what probably are the important questions we asked, and I think I would like to run down through those that I can think of today and see if they represent a reasonable list of things which ultimately ought to provide some kind of -- we ultimately ought to provide some kind of position on.

The first is whether the staff's screening criteria, as it is presented, is acceptable. The criteria for judging those screening criteria probably are: Do they provide adequate time to do something about the issue, if it is important to do something about it. Whether they are exactly the right screening criteria clearly is an important part of that issue. And so it should be kept in mind when you're listening to the staff presentation.

The second point that I believe we ought to make sure we have a position on is the question of what the licensees and applicants should be doing about pressurized thermal shock. And the things that come to mind are the question of whether the training program is adequate, whether the non-destructive examination program is a useful one and can be effective; whether we know about the materials' properties and materials in question to be able to make a judgment about them; whether we understand the neutron damage question well enough to be able to relate it to the materials' properties; do we understand the transients that are of concern, and have we identified those that represent the important issues, and do we accept the operational strategy which goes with the safety judgments.

By operational strategy, I mean the capabilities of the operators in terms of being able to diagnose the accident. Their ability to respond in a timely way; if they have diagnosed the accident properly, and whether we accept the circumstances which require them to think not only about when they need to be pressurized, but also, when they need to keep the system pressurized. And the fact that that is a somewhat contradictory kind of operating requirement is something we need to give some thought to.

In addition, we need to think more, I believe, about the matter of whether there is enough time involved in this program which the NRC staff is presenting to the Commissioners to get the results which are needed to have a position on the safety of these vessels. We may not have heard enough about what's going on to make that judgment, but if it is necessary to do further research and development work we ought to have some understanding about whether they can accomplish what they claim they are going to accomplish within the specified time.

And lastly, I suggest we think a little bit about whether the systems that are licensed and operating have enough diagnostic instrumentation to be able to judge the seriousness of a problem from the indications which have been available to the operators. And if there is not enough diagnostic instrumentation, then it may be appropriate to suggest what needs to be done to provide for additional instrumentation.

The last point I would like to make has to do really with whether the story which is being presented is really understandable by anybody except the technicians that developed the story. And one of the things that was suggested was to try to use Pellini's fracture analysis approach as a more understandable way of presenting the story to people who were not steeped in technological issues.

I think some comments on that approach, and Combustion Engineering people offered it as their way of telling the story. Whether it's a good one or not I don't know. Dr. Zudans has some commentary on it. You might want to think some about whether that is an approach that is more understandable than one we are likely to hear today. I don't put it out as necessarily the best way to present the story.

That is essentially all of the thoughts I have on the subject matter. I would like now to ask the working group members and the ACRS members, including Dr. Remick who has just joined our sterling cast, and Mr. Ward who has showed up occasionally --

(Laughter.)

-- whether they have any additional thoughts on this subject. Why don't we just start around the table?

MR. AXTMANN: I will wait until the end of the day.

MR. SHEWMON: I have nothing now.

MR. REMICK: Not now, thank you.

MR. KOUTS: I think we might ask what should -- you say what should the licensees and applicants do in light of the problem and what we know about it and what we don't know about it. And I wonder if we shouldn't add: what should the NRC do.

MR. BENDER: Well, that is certainly an important point, indeed. Thank you.

If there is nothing else, I think the order in which we have planned this agenda was to turn the meeting over to the NRC staff for what amounts to about three hours. So, Dr. Hanauer has the stage.

MR. HANAUER: Thank you, Mr. Chairman. A year ago, we told the ACRS and the Commission that there was no immediate need for changes or shutdowns of plants, and asked for a year in which to address this problem. And for the past year, we and the industry have been involved in an intensive, multi-disciplinary study of the pressurized thermal shock problem, whose initial culmination is the report, a draft of which you received a month or so ago.

The report which you received was the draft which we sent to our colleagues in the NRC, and both our colleagues and their comments and questions on our own continuing study have produced a large number of important but non-essential changes in this report, for which a new edition is due on my boss's desk in a day or two.

So that what you have represents a lot of thought and has been discussed with a lot of people but is not the NRC staff's management's position on pressurized thermal shock. We agreed with your subcommittee and the staff to have a dialogue at this stage in order to discuss at some length and with some freedom the various technical issues involved, of which there are a large number, so that what you see is Steve Hanauer and a bunch of his colleagues' proposal to Mr. Denton, to Mr. Stello, for discussion by peers, by the public, by the ACRS. And eventually, will result in some recommendation to the Commission.

The exact context of this is still being discussed by lawyers and so on. Whether it will result in rulemaking or some other piece of legal paper is not yet decided and will be decided, I suspect, by a group of people. No single one of them is in this room at the present time.

So that what I would like to present you with is a technical discussion of this difficult and disorderly problem. Our first analysis which you heard a year or so ago was an attempt at generic review of the pressurized thermal shock problem, based on what we knew a little over a year ago.

And what we did then was to pick the eight plants which seemed to be, at that time, based upon what we knew, the lead plants for each of the three manufacturers of nuclear steam supply designs, and we asked them in a period of some few months to analyze the pressurized thermal shock problem, the transients, the vessel properties, the integration of these two into some kind of analysis and to make us recommendations regarding where they viewed the safety of the plant and the justification for the continued operation.

These plants have, in fact, -- the owners of these plants have, in fact, done this, and we have eight reports. In every case, the plant owners at least started, and most of them finished, with a traditional safety analysis approach to this problem. They selected a few relatively severe pressurized thermal shock sequences. They calculated the response of the plants to these sequences in some detail, using the kinds of evaluation models that we traditionally use around here, and concluded that the plants were okay today but that in a few years some of these plants might not be okay.

Not surprisingly, some of the technical material which we received in the course of this disagreed with each other, partly because some of the reporting was generic and some of the reporting was plant-specific. And this was perhaps our first serious insight as a result of this process, which is that generic analysis will get you just so far and in order to decide the risk or the situation or the necessary remedies for any given plant, a plant-specific analysis is required.

And whereas a year ago we were looking for a fairly simple prescription that would establish whether a plant had to shut down or anneal its vessel, or whether continued operation was allowed, one of the results of this past year's study is that no such simple prescription has, in fact, emerged. And that what we are proposing instead is a simple prescription to be used for screening those plants for which a plant-specific analysis is required and necessary in order to provide the justification for continued operation, or in order to guide both the owner and NRC in deciding what remedial measures are necessary.

So, the slide from generic analyses to plant-specific analysis is the first lesson of the last year's work. The second lesson of the last year's work is that these design basis evaluation model, highly conservative, over simplified sequences analyzed in this conservative, over-simplified way did not, in fact, address the real problem in the way in which we cite.

I am not imputing bad faith or anything like that; that is how we have traditionally always done our work, but in fact, the results in my opinion, just as many such results, tended to obscure rather than illuminate the problem.

The grandfather of such analysis is the emergency core cooling requirements, about which a great deal has been said. But I think it is now clear to me, and I think to many other people as well, that the highly schematic, highly simplified, highly conservative emergency core cooling approach which we approved a dozen years ago is not today the way to optimize the use of resources, or even to get the best safety. I will give you one example from emergency core cooling which most of the people in this room are familiar with.

It is required to calculate the behavior of the plant to a loss of coolant accident with the assumption that the off-site power is not available. And so, we saw some designs in response to these rules in which the off-site power, even if it was on, was incapable because the startup transformer was too small for powering all of the trains of the emergency core cooling system.

It's a little bit like sailboard handicapping rules. We had plants built to match the acceptance criteria, and where we had over-simplified them, the plants became over simplified and did not adequately make use of cost-effective ways to improve safety.

It is my believe and the belief of my colleagues who have helped me write this report that this is also true of pressurized thermal shock. And we have, therefore, not used the traditional design basis approach.

Now, the staff is not monolithic on this, and so we have found this very difficult. This is one of our first forays into the use of probablistic schemes, and application of the safety goal in trying to decide what level of safety should be provided, and there are gaps in the reasoning.

There are places where the scientific basis of what we are doing is less than adequate, but it has always been that way. And I will try and tell you a connected story in which we will expose to you the places where we used science and the places where we used judgment and conjecture.

The outline of what I'm going to tell you today is shown here.

(Slide.)

I have already talked some about the general approach which we used, and I will talk about it some more. This general approach involves the evaluation of the over-cooling transients which have already occurred, and the deriving of initial screening criteria from this actual experience.

We then used probabilistic techniques to find out how conservative the screening criterion is and deduced from it our recommendations, and from the gaps that we found in the reasoning recommendations for future work and for the regulatory approach for pressurized thermal shock.

Now, this approach will be seen to be both deterministic and probabilistic. The deterministic approach, as usual, has the difficulty that it is very hard to know how conservative it is, and that it is very hard to put realism into it, although we have made some serious attempt to do so.

The probabilistic approach is beset by the usual difficulties of probabilistic approaches in the present state of the art. Completeness, realism, adequacy of the input data, the stuff that has been debated around this table many times. And so, we have put these two things together with the result that you have seen and which I will discuss.

(Slide.)

The overall topology of the problem I have tried to indicate in this viewgraph. Here is the probability of something worse than the abcissa occurring. And I have used the temperature as a measure of the severity of over cooling. Now, this is an over-simplification. There are lots of over-simplifications in this business.

In fact, the temperature rate, the pressure and the characteristics of the material are all central variables which have to be considered, and so this considers a part of the problem; namely, the transients. And since lower temperatures are more severe, the curve is monotonic in this way. This is an integral cumulative probability or frequency distribution at some temperature. The probability or frequency of getting anything worse than this temperature, a lower temperature, is given by the intersection of these curves, and there is some unspecified probability or frequency scale.

Now, I first plot here the over-cooling transients that have actually been experienced, and this is some kind of a distribution. And we have used this distribution, as I will show you, to derive in a substantially unscientific way, a screening criteria to use. We also realized, however, that experience stops at something substantially over 200 degrees, but that much more severe transients are possible. And at least one such has been reported in Europe about which we know not enough and about which we hope to find out next week in the discussions with the German RSK.

We, therefore, show schematically a probabilistic approach in the usual way, where we consider initiating events, safety functions, probabilities of success or failure of the safety functions and consequences, and we draw then a probability consequence curve in the usual way, except that severity goes to the left, so it slants the way in which you don't expect from cumulative distributions.

And this I call for shorthand the PRA. Up at this end, the PRA should satisfactorily agree with the experience. Down at this end, we have no experience so in the tail of the curve we used the probabilistic results because that is all we have.

In deriving these curves we find from time to time sequences which are apparently outliers. If they turn out not to be outliers, if they turn out to be real, then the curve has to be distorted to include the effect of the sequences.

For a while, we had the sequence of the week which determined the aspect of the discussion of the week with one or another owner's group and which constituted perhaps, if you were excitable, the crisis of the week in the regulatory staff. But we have surmounted this, and we now have a curve which has some substance to it, although by no means is this adequately fully delineated.

You see how far this is from the concept of the design basis accident and the evaluation model, simplified conservative analysis. The analyses here are all intended to be realistic, and the level of safety or degree of conservatism is intended to be provided by how far down you go on this tail, and what you choose to be the probability of what you don't provide for.

Now, this is imperfectly provided in the present scheme, and so we have, in fact, provided at least one substantial quantified conservatism in how we reckon up the actual state of a given reactor vessel for comparison with our screening criteria.

Furthermore, in the present state of the art we really don't believe this curve very well, and the people who have done the calculations have told us that there is plus or minus at least two orders of magnitude uncertainty in the frequencies or probabilities associated with the vertical location of the curve on this diagram.

Such being the case, we have not done what we would do if we had a better curve. If we had a better curve and a well-defined safety goal, we would simply plot the safety goal in probability space. Where it crossed the curve would give us the answer in severity space, and that would be the regulatory result.

Of course, we don't have that.

MR. BENDER: Steve, this seems like the right time to ask this question on the matter of the uncertainty in that curve. It probably varies all along the curve. As you know more about what is up at the top part of it than you do down at the bottom, when you describe the matter of two orders of magnitude uncertainty, how do you perceive it? Is it that uncertainty at the tail end or is it in the middle? Or is it all along the curve?

MR. HANAUER: Well, there are two major components of the uncertainty. One is in the transients. The other is in the response of the vessel to these transients. The people who calculate the response of the vessel, say, two orders of magnitude plus or minus uncertainty in that calculation, and they have told us -- I will ask Jack Stroisnider, who makes these speeches to me about not misusing his calculations, is it relatively uniform, as far as we know?

MR. STROSNIDER: Yes, the vessel response, I think, is uniform along the whole line, the whole curve.

MR. BENDER: We will leave it that way for a while.

MR. HANAUER: Well, let me complete the answer, as far as transients are concerned. The uncertainty arises as much from not knowing what is going to happen as from the uncertainty in how you calculate it. Neither of these is two orders of magnitude. If you draw your trees correctly, then what might happen is represented by the various branches of the trees, and then the uncertainty is calculating the probability that the operator does not do something or the pump does not work or whatever it is.

In general, the operator response is very difficult to predict, and the machinery response is somewhat easier, and in general these uncertainties are somewhat less than two orders of magnitude.

MR. BENDER: Well, I am trying not to prolong this digression, but if I listen to what you are saying, I guess I would read two things into it. First of all, the people that are analyzing the vessel have assigned an uncertainty to the results that represents two orders of magnitude. I do not know which way the uncertainty is biased or if it is biased at all. Maybe you have drawn a median curve or an average curve or some such thing.

In addition to that, I would have to add that there is the uncertainty associated with the transients that must be multiplied, added, or subtracted from the uncertainty in the other part of the analysis.

MR. HANAUER: Never subtract it, Mr. Chairman. Never subtract it.

MR. BENDER: Statistical people say there is a plus or minus to everything, and that sometimes the uncertainties offset each other, and I do not know in a statistical sense whether this analysis comes out that way or not.

MR. HANAUER: Neither do we.

MR. BENDER: I guess we have to make those kinds of judgments about them.

MR. HANAUER: That is right. What I am going to show you is in general point estimates of probabilities, and I wave my arms vigorously in talking about uncertainties, but in fact the science behind these estimates is very modest indeed.

MR. BENDER: Okay. We have probably digressed enough.

MR. HANAUER: No, it is an important point. Now, where do you put design basis accidents along here? A design basis accident is a specified sequence of events. You get a double-ended break in the cold leg at a time when the off-site power is unavailable, and there is -- in addition the worst single failure occurs in the emergency core cooling system. You calculate in accordance with an evaluation model that requires you to throw all the water injected during blowdown on the floor. You can recite this stuff as well as I can. That is a design basis accident.

Such a sequence contains traditionally a large number of improbable factors. The off-site power isn't available. The water all spills on the floor, and so on. And therefore, it ought to be found rather far down in the tail of this curve. However, in fact it may be far off the diagram because of its cascading of event, of disadvantageous events that goes into it, but in fact you can't find it on this curve, because for this curve the severity which I have represented by temperature has been calculated realistically or as realistically as we know how, and I will have a slide later, and there is some considerable reckoning in the report about the conservative and non-conservative and realistic aspects of these calculations.

So, if you want to try to put a design basis sequence on here, you will find that it is very severe. That is to say, it has a very low temperature, and if the probability is reckoned realistically, it is way off the page, but since the low temperature is also done unrealistically, you can't plot it on here at all, and so there is a dysjunction between the traditional design basis approach and the approach that we are using here, which is to the best of our present knowledge essential and difficult to manage.

MR. ZUDANS: But you might remark, this is not a PTS problem anyway.

MR. HANAUER: What isn't?

MR. ZUDANS: A design basis accident.

MR. HANAUER: Well, one approach to the PTS problem would be to make a design basis PTS accident, and this was in fact what the atoners did when we asked them for their analysis of PTS. There wasn't anything wrong with it. That is how we were all thinking a year ago. It just didn't seem to solve the problem.

(Slide.)

MR. HANAUER: Now, let me talk a minute about experience. We have had eight overcooling transients, and I have represented two of them here in two vu-graphs. Here is what happened at H.B. Robinson, when the relief valve blew off during preoperational testing, and you see the pressure went through a considerable gyration, and the temperature behaved rather smoothly. The two lines are for two of the three loops at Robinson. The one that was associated directly with the break in the secondary system came down to some lower temperature which was then restored, so you see, there is a temperature transient which looks sort of amenable, and there is a pressure transient which goes all over the map.

This is even more noticeable if I display the Rancho Seco transient of infamous memory.

(Slide.)

MR. HANAUER: Here is, again, a rather modest and amenable looking temperature transient associated with a pressure transient that nearly defies description.

MR. REMICK: What temperature are you referring to?

MR. HANAUER: These temperatures are the temperatures which were measured in the cold leg, and that is one of the problems in this thing. We don't have any thermocouples in the downcomer. We don't have any thermocouples on the vessels in these plants.

MR. REMICK: Is this fluid temperature?

MR. HANAUER: Well, it is almost fluid temperature. In these plants, there is either a bypass line with some resistance thermometers in it, or the resistance thermometers are stuck into the cold leg in wells or in clamps of some kind, and so they are intended to measure fluid temperature. As long as the main cooling pumps are on, they measure fluid temperature rather well. Water is well mixed in the cold leg and the bypass samples this in an adequate way, and the detectors stuck into the line sees a well-mixed sample.

When you turn the main pumps off and you get either natural circulation or stagnation, then the measurement is in fact not very good. You get stratification in the cold leg. You get peculiar temperature changes along the cold leg as well as up and down in the cold leg and it becomes a matter of chance and substantial uncertainty what you are measuring.

What we have done in this analysis is to take what these temperature measuring devices measured, since we don't have a model for correcting them, and we have assumed that these measured temperatures are the temperatures of the water in the downcomer right at the vessel wall, which is in fact a rather poor assumption for some of these.

Okay. Finally, I show you the temperature transient in the Ginna steam generator tube rupture less than a year ago.

(Slide.)

MR. HANAUER: And you will see that in this case the operators did a thing which at least looks bizarre on a temperature trace. They depressurized the system and produced a substantial temperature excursion measured in the cold leg, and you also see here a stylized representation, this dotted line, of this temperature transient, and that is the next thing.

MR. BENDER: Excuse me, Steve. Before you go on, because these numbers and curves may get discussed more later it is important to know whether we have any feeling for the relationship between those temperatures and what the real temperature of the vessel was.

MR. HANAUER: Very little, except that we know there are substantial differences. I don't have any analysis. We don't have a very good model, as a matter of fact, although we have now the Criari experiments supported by the Electric Power Research Institute which are being correlated and which we have used in the analysis of one of our transients, which I will talk about later on, but we do not have analyses of these eight overcooling transients in that respect, namely, some model that predicts what the temperature of the fluid was right down along the vessel wall as related to the temperature in the cold leg.

MR. WECHSLER: Can you say the vessel wall was no lower in temperature than these values?

MR. HANAUER: No, sir, you can't say that. We can't say either of those possible statements, because of the unknown degree of stratification in the cold leg during these measurements.

Furthermore, it could, at least in principle, be investigated with some calculation which would have whatever validity it had after you looked at it for a while, but this has not been done. Mr. Throm is our expert on this subject. Do you know of any calculations that have attempted to look at this?

MR. THROM: No, not on these specific events.

MR. BENDER: Monroe's question is extremely important to think about, at least. If we haven't done any analysis that relates back to the vessel wall yet, then somehow or another I have to believe that what has been going on in the last year is too generic.

MR. HANAUER: I thought I told you that already, that it was too generic.

MR. BENDER: I am not complaining about that observation, but somewhere along the way it seems to me during this period of time those that own vessels should have been doing some computations of some sort, and I am a little surprised that we don't have access to them. Is it that they haven't really done any calculation, or they haven't provided the results? Or either one?

MR. HANAUER: You will talk to the owners' groups this afternoon. I suggest you ask them. As far as I know, we have everything they do.

MR. BENDER: I will in fact ask that question again. Go ahead.

MR. HANAUER: Neal, you wanted to say something? Mr. Randall?

MR. RANDALL: I thought there was some confusion in Professor Wechsler's remarks. I think he was referring to how you get from the water temperature in the downcomer to the temperature at the crack tip in the metal. You were referring to how you get from the water temperature measured in the cold leg to the water in the downcomer?

MR. HANAUER: Yes, sir, that is what I was talking about. I don't know what -- Dr. Wechsler, was that what you were talking about?

MR. ZUDANS: No, I don't think so. The question was very simple and straight. Is this the lowest possible temperature in the downcomer or not? And if you can't make that statement --

MR. BENDER: I think you said vessel. I think vessel is the right question to ask.

MR. WECHSLER: Ultimately, that is what we have to know.

MR. HANAUER: The answer is no, this is not the lowest possible temperature, because of the unknown degree of stratification at the point of measurement.

MR. BENDER: It is certain that the vessel temperature cannot be lower than the temperature at the vessel wall.

MR. HANAUER: The temperature of the fluid at the vessel wall?

MR. BENDER: Yes.

MR. HANAUER: Quite so.

MR. CATTON: But you don't know whether this is the temperature of the fluid or not. The wall could be heating up with the RTD, so that could be higher.

MR. HANAUER: This is in the pipe.

MR. CATTON: It is still in the well, and the well is connected.

MR. HANAUER: It is a very large pipe. Based on experience, not calculation, and not analysis, I would say that effect is fairly small.

MR. CATTON: It sticks pretty far into the pipe.

MR. HANAUER: Yes, sir.

MR. BINFORD: Steve, what you are saying is, you are measuring a temperature in a pipe which bears an unknown relationship to temperatures elsewhere in the system?

MR. HANAUER: Unknown is too strong, but I cannot certify to you that this measured temperature is the lowest temperature of water even in the pipe.

MR. BINFORD: But you really don't know the relationship between that temperature and the temperature anywhere else. You may have some qualitative feeling for it, but you don't have a quantitative relationship, because you don't know the conditions, and that is no fault of yours. The conditions are very variable.

MR. HANAUER: Well, the cold leg pipe should be the coldest place in the system for the most important transients, because the coldest water in the system is being injected into the cold leg pipe.

MR. BINFORD: Well, I would agree with that.

MR. THEOFALOUS: I think that the impression that is being generated here is that we know very little about those things, and if we had this temperature we could almost say nothing about the temperature in the downcomer, and I really don't agree with that.

MR. HANAUER: I don't, either.

MR. THEOFALOUS: Well, if you agree with me, then why are you saying that?

MR. HANAUER: Well, let me try and say it better, then. There is an uncertainty here, and I cannot in response to somebody's question certify that this measured of temperature as a function of time is as cold as the water in the downcomer right at the vessel wall can be. There is an uncertainty. This uncertainty is caused by the difficulty in measuring in this large pipe at very low flows, and I would not want to represent, and tried very hard, maybe too hard, not to represent that this temperature was the temperature of the water in the downcomer right at the vessel wall. It is related to it, and as you point out, we know a lot more than nothing about this relationship, but we have not made the calculation.

MR. THEOFALOUS: I guess my point was referring to this aspect of it, that I was concerned that people might get the impression that we cannot make those calculations. I think in some of your earlier statements you referred to the difficulty of making such calculations, and I guess I don't agree with that, and I don't agree that a year later we still don't have those calculations. I really see no reason for that.

MR. KOUTS: Was high pressure injection being done here?

MR. HANAUER: On Ginna, yes, it was.

MR. KOUTS: So there was that source.

MR. AXTMANN: Does that negative spike correlate with some action that was taken?

MR. HANAUER: Yes, the operators depressurized the primary system, reduced the pressure in the primary system.

MR. CATTON: They were also playing games with the safety injection. It was on and off.

MR. BENDER: Steve, with great reluctance, I would like to ask whether in a probabilistic sense we know something about the temperature of the vessel wall.

MR. HANAUER: No, sir, not in the sense we are talking about. We know a lot about the temperature in the vessel wall. We have taken this temperature to be the temperature of the water in contact with the vessel wall, and we have heat transfer both at the wall and in the metal calculations. What we don't know deterministically or probabilistically, we have not evaluated in any quantitative way the difference between this temperature and the temperature of the water at the vessel wall.

MR. THEOFALOUS: Let me rephrase my question following this one. Would you agree that we can find what the temperature of the wall would be if one was given, let's say, a month's time?

MR. HANAUER: We could find this temperature with some assumptions about what is going on in this pipe, yes, and these assumptions would not be completely arbitrary, because they have some measurements. Yes, we could make some calculations.

Mr. Throm?

MR. THROM: Given an event, there is a lot of information that you would like to have concerning the plant conditions that we really haven't tried to get together, nor really are available in a plant. The data we are seeing from Criari indicates that the real problem is in the very low flow situation, stagnant loop flows or loop flows that are even less than the anticipated natural circulation flows, and we don't have data that really verifies what those conditions are. Given the assumption that it was stagnant, we are coming up with models that would allow you to predict what the downcomer response would be, but then you are also assuming either a no loop flow or some assumption of what the loop flow is, and I think it is kind of sensitive in that range.

MR. THEOFALOUS: Again I think that you are trying to say that because we are not absolutely certain, that is a good enough reason for not trying to do the job here, and I really don't agree with that. I think we know much more than what you are implying, and I think a good job can be done in determining those temperatures, and I think that should be done as soon as possible.

MR. BENDER: We have probably belabored this point enough to be sure that there is some more discussion to be had about it, but let's go on.

MR. HANAUER: Now, this temperature or some temperature related to it, in the spirit of the last ten minutes, is the driving function for a calculation of the temperature distribution in the vessel wall. Because of the very large thermal inertia, we have represented these rather unwieldy curves with exponential temperature decays of which an example is shown here in the dotted line.

We are changing our code so we can put these traces in directly, but we don't have that capability.

(Slide.)

MR. HANAUER: Here is the stylized temperature pressure transient which we have used for some fraction of our work, and I will try to be clear about where we have used real transients and where we have used stylized transients. The stylized transients begins at a temperature two zero and ends asymptotically at a temperature TF with an exponential behavior, and the pressure is assumed to be a constant. This, of course, is a gross oversimplification for some transient in which the pressure does this.

(Slide.)

MR. HANAUER: As shown on the top of this curve. And so an improved model which, as I say, is under development, is very badly needed in this area. On the other hand, the pressure dependence is not enormous, rather surprisingly, as we will come to in a moment.

(Slide.)

MR. HANAUER: Now, the result of this calculation is this frequency, this cumulative frequency distribution. Here is the temperature, and now this temperature is the temperature TF, which is used to give a stylized representation of the temperature in the inlet pipe as measured, and which has in it then the uncertainties which we have discussed, and here are the eight incidents which are calculated and discusssed in the report which you have.

The most severe one we paid any attention to had a final temperature of 350 degrees. Above that, one has very little problem with pressurized thermal shock. And the lowest one had a final temperature evaluated of 225 degrees, and what we did was, we took the 350 reactor years for pressurized water reactors and divided the numerator by the denominator, and there we have the frequency.

Now, statistically, this is not very well defined with eight events in this way, but here is their frequency distribution.

There is one other thing that needs to be said about this curve. These eight incidents contained no incidents at a Combustion plant, and the three worst ones were at B&W plants, which, however, have been the subject of substantial backfitting programs to deal with the causes of these three transients. So, here is an additional uncertainty. If you try and separate these into three B&W events, and four or five depending on which reckoning you use, Westinghouse plant events, then these statistics get really awful, and we have not chosen to do this, but in fact it needs to be done, and this is one of the pieces of unfinished business, is to investigate in a more serious way whether there are any essential differences which would affect the pressurized thermal shock risk in the three kinds of plants that we are dealing with.

MR. BENDER: Steve, those eight events as you have cited have resulted in some backfits, two kinds of backfits, procedural changes and some changes in the physical plant.

MR. HANAUER: Well, one of them is Three Mile, for which a very large list has been imposed.

MR. BENDER: The point I am trying to make is this. That curve there, or whatever you want to call it, is clearly not a good statistical representation of anything. It is just a computed probability of an event that has occurred. But when you take into consideration the corrective actions, a new probability curve has to be drawn. If you are only going to work on the basis of historical evidence, then all the events are random, and there is no way of correcting a random kind of occurrence, but in view of the fact that there are corrective measures that have been taken, would the staff want to argue that this is probably a worst case representation, or less than worst case? Are we better off today or not?

MR. HANAUER: Well, I will give you my opinion, and I will invite my colleagues on the staff to flesh out the staff opinion. This is surely not worst case. Much worse transients are possible, and have not occurred, but there is no reason why they couldn't, except their lower probability prima facie based upon what is happening.

There is no way in my opinion that this could be a worst case. Since we have had corrective measures, it is my opinion that this curve is somewhat worse than plants today. How much worse, well, if we do our PRA very well, and if it has the kind of discrimination which would show up such differences, then the PRA curve would lie below this one, and if that were the only reason for the difference, you would say that that shows the benefit of what we have done in backfitting, or you could wait ten years and draw -- or 350 more reactor years and see if the curve looked any different.

Well, we don't have time for that. The current answer, since we can't do it with experience, has to be in the probabilistic evaluation which should be done on the plants as they are now with the backfits in, and those plants that do plant specific evaluations need to do that, and it will show for a variety of reasons that present day plants are better than this, and that will be one of the reasons.

Now, let me invite my colleagues to say either something different or any other remarks that should be added.

MR. ZUDANS: Since most or let's say the key argument in PRA is this experience, is eight data points, does it represent any kind of a credible basis for any statistical analysis at all?

MR. HANAUER: One can do statistical things with eight points. We haven't done it. And Mr. Bender's question is one of the reasons it doesn't represent today's plants, and therefore doesn't justify very much messing around in our opinion.

MR. BENDER: Well, there is a little bit of a contradiction in the discussion. It doesn't represent today's plants. I fully agree with that. And because it doesn't, trying to present a frequency relationship of the sort you have there distorts the problem probably.

MR. HANAUER: Yes.

MR. BENDER: And distorts it in what may make the public safety question seem worse than it really is.

MR. HANAUER: If you really believe that all our backfits have made things better, which seems probable if you look at the backfits, then, yes, this gives a picture of the public risk which is worse than the facts.

MR. BENDER: What is missing here is, and it troubles me, and it troubles you, and probably the whole staff, are the events that haven't occurred.

MR. HANAUER: Of course. That is what the other half of this discussion is, the probabilistic discussion.

MR. BENDER: And whether the events that haven't occurred can be presented probabilistically may be the crucial issue.

MR. HANAUER: Indeed. We are in violent

agreement.

(General laughter.)

MR. BENDER: Okay. Go ahead. I had not expected to reach any agreement with you, Steve.

MR. HANAUER: I now pause extremely briefly to present some results from a deterministic fracture mechanics analysis. This has gone far beyond Pellini's diagram because we now know how to calculate these things in at least in elastic fracture mechanics in a rather deterministic way.

(Slide.)

And we now have a great deal of experimental and theoretical evidence that in the range of applicability that this stuff does indeed predict the failure modes and the failure effects of vessels made out of the kinds of materials of which we are talking.

What we do not know, and what is the amount of conservatism in this analysis for warm materials which are very ductile and for which linear elastic fracture mechanics is an approximation.

Now, as an old instrumentation and control engineer, I am far out of my depth; I have studied this subject, but I am no expert in it. And so I may not even answer the first question, but call on my expert colleagues.

This is the result of a whole series of calculations using a code similar to OCA about which the fracture mechanicians in the room know far more than I. What we have done is to calculate --

MR. BENDER: Would you spell "OCA"?

MR. HANAUER: O-C-A.

MR. BENDER: Thank you.

MR. HANAUER: A whole series of stylized transients, they had constant pressures and exponential temperature decays. Later on I will show you some results for real transients.

These are deterministic transients. They assume that there is a flaw wherever there needs to be a flaw. They include the time-dependent heat transfer of the water, whose temperature is given by the exponential decay into the metal and the time-dependent heat conduction within the metal.

They include the metal properties as a function of depth through the wall, as a function of neutron irradiation which varies through the wall, and also as a function of the local temperature which varies with both time and position.

They include thermal and pressure-related stresses, and they include the effect of crack arrests, but not, in these calculations, the effect of warm prestressing.

Now, the abcissa is the relative cooldown temperature; namely, the TF from the stylized transient minus RT NDT, the reference temperature which characterizes the material.

MR. SHEWMON: They also assumed a flaw size. Is this a quarter of the wall size or what?

MR. HANAUER: No specific flaw size was assumed. The flaw was assumed to be however big it needed to be. I told you I would need help.

MR. KLECKER: Ray Klecker.

From the standpoint we assumed that a flaw greater than, say, from the clad up to about 1 inch, actually a little larger. And we looked at all crack sizes within that range.

MR. SHEWMON: Go ahead.

MR. BENDER: That answer is not too clear to a lot of us. If you are doing a computation, you have to do it with explicit flaw size in mind. If I were to pick a point on that curve, on any of those curves, could I identify a flaw size that was related?

MR. KLECKER: We can go back to the original calculations, yes, and pick out what that flaw size was.

MR. HANAUER: It does a search through flaw sizes and picks the one that starts. If any one starts, it assumes the worst flaw, not some specific depth, but it looks and sees if there is any flaw that we can --

MR. SHEWMON: If it assumed the range, then there would be a probability, and I do not see any probability up there.

MR. HANAUER: There is no probability here.

MR. SHEWMON: The dividing line between crack initiation or no crack initiation, there was a flaw size or else there was some -- well, let us go on.

VOICE: Very simply, a parametric study was done which assumes the presence of a flaw, and the final solution was a flaw as a basis of a range of flaw sizes.

If the study showed crack initiation for any size flaw from very small, let us say, quarter-inch up to an inch, that initiated it. We assumed the presence of a flaw. It was not probabilistic. The size was indeterminate and determined by a parametric study.

MR. BENDER: Go ahead. We will come back to it someday, maybe not today, but someday.

MR. HANAUER: Now, the original curves which we saw from an early version of this study had, in fact, coalesced into a single curve here and a single curve there. And more detailed studies have resulted in these dependencies.

Notice that the whole length here is 150 degrees or so, so that these differences are really quite small. We have here the pressure and here the cooldown temperature relative to the reference temperature of the material at its inner surface. We did not assume a constant temperature, and we did not assume a constant reference temperature through the thickness of the material. But we pinned it, in order to plot it, to the reference temperature at the inner surface.

MR. BENDER: Well, Steve, there is a shape of the temperature distribution.

MR. HANAUER: Yes, sir. I will say it again. The temperature, TF, is this asymptotic temperature of the water at large times. The RT is the reference transition temperature at the inner surface of the ferritic material.

MR. SHEWMON: Did you use a bounding or a most probable heat transfer coefficient to get from the T-final to the steel temperature? That has been a question of argument before.

MR. HANAUER: We have used several. For this study the heat transfer coefficient was, I think, 300.

MR. KLECKER: I think on this one it was 1,000. The later ones, we used 300 or 330.

MR. HANAUER: We have used various numbers, and this can be discussed.

MR. SHEWMON: And the 1,000 is bounding, and

300 is more likely, or what?

MR. KLECKER: I would say 300 or somewhere thereabouts is more realistic.

MR. SHEWMON: Okay. Thank you.

MR. HANAUER: The Westinghouse calculations, which give similar results, is actually a heat transfer correlation and gives similar results to our 300 curves.

All right. Now, first of all, the two families of curves are for two different values of beta. The inverse time constant for the assumed water transient. Here is a large value of beta where the water temperature comes down quite quickly, and the heat transfer is almost entirely dominated by the conduction into the material.

Here is a much smaller value of beta, where the water temperature comes down much more slowly, and both then the water temperature variation and the conduction into the material contribute. And as you can see, a somewhat either higher pressure or lower temperature can be tolerated if it happens more slowly, not surprisingly.

Here are the different final temperatures. If TF minus RT NDT were really a correlation parameter, these two curves would coalesce. In fact, for a 15-degree change in TF, there is approximately down here a 15-degree change in the severity, so that they do not quite coalesce and therefore, TF minus RT NDT is not quite a good correlation parameter.

MR. BENDER: Steve, if I were concerned about the pressure condition and wanted to try to make some judgment about where I would like the pressure to be, clearly I would like to have it as low as practical, but there are some operational questions associated.

MR. HANAUER: There is also a question of cooling the core.

MR. BENDER: Yes. And you were trying to make a judgment as to where a suitable pressure might be, where might I draw a line?

MR. HANAUER: Well, the first thing to notice about pressure is its surprisingly small contribution if you take the more severe curves, the pressure slope from zero all the way up to 2500 is only worth about 40 degrees. So that the stresses in this model are primarily thermal, and the pressure stress is significant but not really a large part of it.

For the slower one, where conditions are less severe, the pressure has much larger importance and is worth something like 100 degrees. For this purpose, you would like the pressure to be as low as possible, for the most severe transients, which turn out to be the small-break loss-of-coolant accidents.

In the intermediate size where flow stagnates, the pressure calculated is about 1000 pounds. We think the pressure will hang up at about 1000 p.s.i. So you are down here, and there is not much hay to be made in trying to get the pressure much lower.

MR. BENDER: I do not know if you are going to proceed from here to the question of crack arrest, but if you are not, then I may as well lay the question out here.

The Staff, I think, probably is following a good regulatory strategy in arguing you should protect against crack initiation. But in the sense of what puts the public in jeopardy, there is a question of whether a crack which initiates will arrest.

MR. HANAUER: But crack arrest is, in fact, in this model, and a similar curve can be drawn with crack arrest. What you get is that down below about 500 pounds per square inch, these curves slant to the left quite strongly. I do not think I brought one with crack arrest. I am sorry about that. It is in the report, however, and it is also in the P&L report, very clearly indeed.

Crack arrest seems to make a significant difference only at pressures below about 50 p.s.i. Now, the models which we have used later on in our probabilistic study include the effect of crack arrest, and in one important transient, include the effect of warm prestressing also in order to get more realistic.

MR. ZUDANS: Steve, do you have a similar set of curves for a coefficient of 300?

MR. HANAUER: Such data exist. I do not have such a curve with me.

MR. ZUDANS: Have you seen them?

MR. HANAUER: It becomes less severe because the heat transfer is less. It is really more important for this one than for this one. And my recollection is they do not coalesce very much better. Somebody please correct me.

MR. RANDALL: In Appendix D, page 18, there is a table giving that effect of a difference of H-300 versus H-1000.

MR. ZUDANS: Which page?

MR. RANDALL: D.18, the biggest number in the table is 29 degrees, and both of them are around 10. So it would not affect that.

MR. HANAUER: It is not negligible, but it is not very large either, and I cannot tell you whether they coalesce any better or not. One would have to go look through a whole bunch of calculations to find out.

MR. REMICK: Steve, there is something I do not understand. You talk about flows stagnating, yet when the curves that you had showed an exponential measured decrease in temperature that was fairly rapid, how could you have stagnation and temperatures changing that rapidly?

MR. HANAUER: Because in the first place, we have not had a transient where the flow completely stagnated, as far as I know. In the second place, the Criari data show that there is mixing in the cold leg. And this is a physical fact. The flow does not stagnate all the way. The third reason is that we are injecting the cold water right into this space.

MR. REMICK: I understand. But if you are injecting it in, you cannot inject it in if there is stagnation. Something has to be moving.

MR. HANAUER: Yes. The flow stagnation is not total. The motion is not zero. If it were, the cold water would not get into the reactor, and we would not have any problem.

MR. THEOFALOUS: What do you mean by "stagnation" now? Do you mean the loop flow, what you normally call "loop flow," the flow going through the loop, or convection currents?

MR. HANAUER: How I use it is that for a

certain class of transients which turns out to be very important, the net flow in the loops is essentially zero, but the local flow and mixing, as was pointed out a moment ago, is not zero. You are injecting cold water, and it has to go somewhere. And we have these measurements that show that there is a certain amount of local flow and mixing. Now, when I mean "stagnation," I mean that the natural circulation through the loops is stopped.

MR. THEOFALOUS: And I thought you said the Criari data show that there is good mixing in the cold leg under those conditions?

MR. HANAUER: There is a substantial amount of mixing. I do not know whether you want to call it good or not.

MR. THEOFALOUS: Because my interpretation of that is that the data shows there is very good stratification.

MR. HANAUER: There is stratification, yes. Levy has made a model to show the stratification and what mixing takes place, and it seems to correlate a substantial amount of the Criari data. And they are now doing some more tests to see if it works.

MR. KOUTS: I thought Forrest was referring to the temperature spike in the Ginna transient, and that, I thought, was a result of temperature change which caused local flashing.

MR. HANAUER: Yes, sir.

MR. REMICK: No, I was referring to the beta. If you had a beta .12, as you did in one curve, that meant every 8 minutes roughly the temperature was changing by a factor of E, which is several hundred degrees. To me, that hardly seems like stagnation. That is what bothered me.

MR. HANAUER: No, that is not quite right. The temperature difference to the final temperature is changing by a factor of E. The exponential is related only to the difference between TL and TI. So that, yes, in the first 8 minutes you get about a factor of E change in the temperature difference, and after that --

MR. REMICK: Well, that could be several hundred degrees in 8 minutes.

MR. HANAUER: Our TFs are in the range of 200 to 350 degrees. So, yes, there is 200 to 300 degrees between TO and TF; that is quite right. And in this range the temperature changes quite quickly on the 8-minute schedule.

MR. BENDER: Steve, when you are making this computation in the face of the other essentially stagnant core circulation, the cooling of the wall is dominated by the ECCS flow. Is that what you are saying?

MR. HANAUER: For certain transients, that is true, yes.

MR. BENDER: Now, some people have asked questions about whether the temperature of that coolant of the ECCS coolant could affect the wall temperature under those conditions.

MR. HANAUER: Indeed, it could. And warming that water is one of the things that ought to be done in plants with brittle vessels.

MR. BENDER: But the computation now is based on what water temperature?

MR. HANAUER: The computation which we now have, which is a Westinghouse Owners Group computation, used 60-degree water, allowed for --

MR. BENDER: Is that centigrade of Fahrenheit?

MR. HANAUER: Fahrenheit. Allowed for mixing in the cold-leg pipe in accordance with a model derived from the Criari tests, allowed for heat transfer from the cold-leg pipe wall, in accordance with a model and then put this water into the downcomer, I do not think with any further mixing.

I see nods in the Westinghouse bleachers.

MR. MEYER: I am Daniel Meyer of the Westinghouse Owners Group.

There was some further mixing.

MR. HANAUER: So that was the model that was used in the current calculations.

MR. BENDER: Go ahead, Steve.

MR. HANAUER: Now, this is the deterministic calculation, and the people at Oak Ridge fixed up their deterministic calculation -- or already had it fixed, I do not know which -- and used a calculation of this type, not on the stereotyped transients but on seven, on five of the seven transients which actually occurred -- five or six. Six, I think. One of them we just did not have the data on the time-dependence, and so we could not do it.

(Slide.)

Here is the results. Now, this is a deterministic fracture mechanics calculation of the kind I described, with a heat transfer coefficient of -- what did they use for this one? I cannot remember.

MR. KLECKER: That one was 330, as I recall.

MR. HANAUER: And what they did was they calculated for each transient a value of RT NDT at the interval for which no crack would be initiated even if there was a flaw. That is a net result, is this solid curve. And I have also plotted here as a dotted curve the T_f evaluation of these same events off the previous vuegraph.

But now you see this is not the stereotyped anymore. For this calculation it is not necessary to represent the transient with a constant pressure and an exponential temperature decay.

We used these zigs and zags in these temperature and pressure plots that were taken from the actual events. And as you see, it is somewhat less severe because it is somewhat stereotyped, and it was possible then to use a more realistic depiction of the actual transient as it occurs.

And here they are plotted in the same cumulative frequency way with the same strictures that have to be placed on it for backfits since then. This represents the plants as they were at the time the transients happened rather than the plants as they are now.

And you will observe the, crudely speaking, about a 50-degree difference between these. That is to say, if you believe the critical RT NDT curve, then the TF representation was about 50 degrees conservative. Of course, this is a better way of representing this kind of phenomenon.

MR. THEOFALOUS: Does this mean that if the calculation of RT NDT was a good one, a correct one, and if the temperature in the particular transient was 50 degrees lower, we would have a crack initiation?

MR. HANAUER: No, it does not mean that. Here are two calculations of the same set of transients. One is stereotyped TF-style; the other uses the actual transients.

MR. SHEWMON: TF?

MR. HANAUER: TF constant pressure, the same business. TF constant beta, same pressure. That says that for these transients RT NDT could be about 50 degrees higher than the TF we were using, without cracking the vessel.

MR. KOUTS: That is assuming that the water temperature according to the top curve is the vessel wall temperature. According to the bottom curve --

MR. HANAUER: No, sir. This top curve represents the water temperature by TF and beta. This curve represents the water temperature by what was actually measured in the cold leg. This curve has a constant pressure. This curve uses the pressure as was measured.

MR. KOUTS: Where did they get the measurements?

MR. HANAUER: There are measurements of pressures in primary systems. The temperatures were in the cold leg; the pressures are on the pressurizer. But the pressure, remember these are transients characterized in minutes so that there should not be any problem.

MR. KOUTS: I thought we had no temperature measurements in the cold leg.

MR. HANAUER: But not the downcomer.

MR. KOUTS: So this assumes the measure of the temperature in the cold leg is the measure of temperature in the downcomer? That is what I said the first time.

MR. HANAUER: I am sorry, I did not understand you to say that. Yes, this has that same problem in it. We do not have a way out at the present time. We do not have a calculation.

MR. THEOFALOUS: So this difference then is just the effect of the pressure?

MR. HANAUER: It is the effect of the pressure. It has three effects in it: one, the effect of the pressure; second, the stereotyping of the temperature variation; and third, the fact that the final temperature is not just a temperature that will break the vessel.

(Slide.)

Here you see depending on situations -- here is RT NDT you see out here -- and the transient can be colder than RT NDT by an amount that depends on the temperature, the beta, and the pressure.

MR. THEOFALOUS: Probably that is the main effect. That is about 50 degrees.

MR. HANAUER: Well, 50 degrees is here. So there are lots of ways to get 50 degrees.

MR. THEOFALOUS: If you look at the difference between TF -- okay. Yes.

MR. HANAUER: It is different for each transient. There were high-pressure transients and low-pressure transients. The high-pressure transients are up here; the lower-pressure transients are down here.

MR. THEOFALOUS: Is there any way you can give a feel of how important the pressure variation is? I would not guess that it is too important.

MR. HANAUER: I do not have anything hard. It seems to be worth, for this low one, as much as 15 degrees plus; for the fast ones, rather less.

MR. ZUDANS: Steve, could you go back to the previous slide?

(Slide.)

MR. HANAUER: Yes, sir.

MR. ZUDANS: And look at one of the shelves, the dashed shelf, and the correspondingly solid-line shelf.

MR. HANAUER: Yes. This one and this one, for example.

MR. ZUDANS: That is supposed to represent the same event; right?

MR. HANAUER: Yes, sir.

MR. ZUDANS: And therefore, the probability is the same. And the dashed curve shows the fluid temperature at that point, your stylized?

MR. HANAUER: Yes.

MR. ZUDANS: And the solid curve represents which RT NDT would initiate the crack; is that right?

MR. HANAUER: Yes. In accordance with this model, of course.

MR. ZUDANS: And the fluid temperature, of course, and the solid curve varied all over the slope?

MR. HANAUER: Yes. We used the measurement in the cold leg to represent the fluid temperature.

MR. ZUDANS: This actually then shows what you said how much higher the RT NDT would have to be than the fluid temperature?

MR. HANAUER: Exactly. Okay.

Now, from this collection of information, we derived a screening criteria.

(Slide.)

Now, I have to tell you plainly that there is not as much science as one would like in the derivation of the screening criterion. I will also tell you exactly how we did it, which will tell you how little science there is.

What we did is, we had a much earlier version of these curves. Because our initial curves had some mistakes in them which the industry owners groups pointed out to us, and they were right in some of them, and so we made some major changes in our curves from the ones we had in June to the ones we have now.

On the original set of curves -- completely arbitrary, because we did not have the Strosnider results at that time in the form in which we have them now -- I took 10^{-2}, a completely arbitrary value based on the idea that anticipated operating occurrences have a frequency bound of about 1 in 40 years. So 10^{-2} is comfortably below that.

But without any really scientific basis, I took 10^{-2}, and at that time this curve crossed the 260 and this curve crossed at 280. The 50-degree difference had not shown at that time, and so I held my nose and picked 270.

Now, that is the amount of science there is in the 270-degree screening criterion. But since that time we have developed some better idea about how conservative it is.

If you really believed these curves and wanted to use them in some detail, and if you really any scientific basis for 10^{-2}, you would pick a number in the low 300's for the screening criterion based on this curve. It is my opinion that that is too high, based upon some probabilistic discussion that I will give you a little later.

There is really more justification to 270 than that, but the original basis is indeed rather thin.

MR. BINFORD: Steve, let me ask one question here. It appears to me that the dotted curve is merely the solid curve, as you have said and presented in a stylized fashion. Once you have the solid curve, which is the actual conditions, what is the use of using the dotted curve at all?

MR. HANAUER: The dotted curve is a grosser approximation, which I don't use much any more.

MR. BINFORD: Well, it just appears to me that all this does is to demonstrate that your simplification is reasonable.

MR. HANAUER: Now, the reason to show the dotted curves in the Tf is that we had to do our probabilistic work using Tf, We don't have the codes today to do RT critical for the probabilistic work. We will one of these days, but today we don't, and that is one important estimate. I have carried the Tf along.

So the screening criterion -- and I will get to a stopping place, Mr. Chairman, in about, I predict, ten minutes or so.

MR. BENDER: Fine.

MR. HANAUER: Well, I will one way or another get to a stopping place pretty soon.

MR. BENDER: Why don't you announce when you would like to break.

MR. HANAUER: Yes, sir. You will see my coattails disappear through the door.

(Laughter.)

So all the work I have been talking about so far is for longitudinal cracks, and so we picked 270 degrees for longitudinal cracks in the manner which I have described. We then ask, what about circumferential cracks. This turns out to be very important, for three reasons:

First of all, it's different and in some vessels the circumferential cracks, the circumferential welds will dominate because they contain higher copper material;

Secondly, some vessels don't have longitudinal welds, and for those vessels the circumferential welds will surely dominate;

And thirdly, the consequences for a severe vessel break all the way around the circumferential weld are substantially more serious.

MR. BOCK: Steve, what about plate material?

MR. HANAUER: I beg your pardon?

MR. BOCK: What about the reactor vessel plate material? Don't the vendors believe in some cases it is more limiting than the welds?

MR. HANAUER: There are some cases on which the plate material is more limiting. In that case you don't have any good reason for picking one crack orientation over another until we learn something about rolling directions and so on, about which I think very little is known today about flaws. And I suppose for those vessels one should pick longitudinal flaws, for lack of any information.

There are only a small number of such vessels. In general, the copper-coated welding electrodes in the vessels of interest created a material which is substantially more susceptible to radiation embrittlement, and so the welds almost always dominate in all of the high-brittle vessels. In general, even in vessels where the plate dominates the situation is reasonably in hand.

Now, there are a few vessels about which we know very little, and here we are simply going to have to get some more information if it can be hand. The circumferential crack therefore has to be treated. It is restraint-contrained in an entirely different way, as the crack begins to open in the fracture mechanics calculation. And of course the pressure stress is half as much for the circumferential flaw as it is for the longitudinal flaw, because of the way longitudinal and hoop stresses are related in a pressure vessel.

We put these into a series of calculations which are reported in Appendix D, and the result is we have selected 300 degrees Fahrenheit as approximately equivalent to 270 degrees for longitudinal flaws.

MR. SHEWMON: When I see "longitudinal crack" up there, should I think of a longitudinal weld, that this crack then -- so the crack is always running in the weld material?

MR. HANAUER: Yes, sir. That is the picture we have.

MR. SHEWMON: That is not the picture I was shown yesterday by one of you guys. But let's go ahead.

MR. HANAUER: That is our current picture, and our current model is based on long longitudinal cracks or long circumferential cracks.

MR. SHEWMON: Running always in weld material?

MR. HANAUER: Yes, sir.

Okay. Now, a final question in deterministic space is, okay, suppose that is the criterion, how do we evaluate the vessel in Skunky Hollow Unit No. 3 and determine its properties to be compared with the screening criterion.

We convened a peer group, an expert group, to do this.

(Slide.)

And their recommendation, which we have adopted, is the following: The RT NDT of any given vessel at any given time is of course -- starts at some initial value and then increases in accordance with the radiation. I will come back to this and talk about the -- no, I'd better talk about them now.

Neither of these things is known perfectly, of course. There are substantial measurement uncertainties, and also there are uncertainties regarding the material which is being measured. In many cases the material which is being measured is not the actual weld or a prolongation of it, but a qualification piece which was made on a different day, with nominally the same materials.

And so there are at least these two sources of uncertainty. Now, for some vessels and for some weld types, less material is available and so one is forced to consider a population of vessels and welds to infer the properties of weld X in vessel Y from a much larger population, which may or may not be made of the same material.

MR. SHEWMON: I used to think that a best estimate was something like a median or mean value. have yet to see anybody who works for the NRC give me a median or mean value. So is that what I will term an NRC best estimate, or is that sort of a best estimate in the sense of mean or median?

MR. HANAUER: As one NRC employee, I will tell you that what is intended there is the mean or the median.

MR. SHEWMON: Okay.

MR. HANAUER: And you will find in Appendix P of the report values for this which may convince you that at long last somebody in the NRC is trying to do that for best estimates. It comes hard. Our whole tradition is different.

Now then, the object is to use the best estimate of the initial measurements, which are available for almost all vessels, and then to use

Guthrie's correlation for different families, different populations of weld materials, to estimate the change as a function of irradiation.

Now, this is not a simple matter. We have already had a discussion this morning with one of the owners groups who would like to quarrel with some of the numbers in Appendix P. We seem to be forever quarreling with the numbers in Appendix P.

There is, first of all, the calculation of neutron leakage flux, a subject understood by at most seven people in the world, I think.

(Laughter.)

And calculated with great difficulty. The codes are not very easy to use and the assumptions that go into the codes can be argued about almost interminably.

Having calculated the flux at the inner surface, it is necessary to calculate the attenuation of the neutron flux through the wall and the change in neutron energy spectrum through the wall, because the energy of the neutrons determines their effect on the properties of the material.

This matter can also be discussed at greater length. I will say today, I will say now, it could be reopened at much greater length if you want. We now use a model somewhat different from the one in Reg Guide 1.99 which includes, we think, the effect of the spectrum hardening through the wall, and we have Mr. Lois and others who are prepared to discuss those questions with you.

Then, for conservatism -- and this is one of the places where we put it in explicitly -- for conservatism we add twice the standard deviation of this value. Since there are two components, we consider separately the standard deviations involved in these two components. And since they arise from different physical phenomenon, we add them up at statistically uncorrelated standard deviations.

So the result is the initial, the change, plus two standard deviations.

MR. BENDER: Steve, in putting in the two sigma allowance you are trying, I suppose, to bound the data?

MR. HANAUER: Yes, sir.

MR. BENDER: How much of the data is bounded by that?

MR. HANAUER: I'm not an expert on this. I have seen the curves and it is, two sigma gets the right percentage of it, which is 95. And there are in Appendix D scatter diagrams that show these bounding curves and how they do it, and there is -- has Guthrie's report come out? Where is Les?

MR. RANDALL: This is George Guthrie.

MR. HANAUER: How do you do, sir. I have never met you. Has your report been published?

MR. GUTHRIE: Not yet, no. No, sir.

MR. BENDER: Is it necessary to cover 95 percent of the data?

MR. HANAUER: Well, how bounding would you like to be? Would you like to deal with a best estimate, with one sigma, with two sigma, with something else? That selection has a lot of arbitrariness to it.

MR. BENDER: Well, if every point in a set of curves had equal weight, I guess I would probably accept the argument pretty well. I'm not sure that the points should be given equal weight, because there is a lot of variation in how the determinations are made.

Can you comment on that?

MR. GUTHRIE: Well, they were given equal weight. But we also had in here, there are two factors to the uncertainty. One part of the uncertainty is due to the fact that we don't know. When we are fitting a curve, we have a Sharpey shift given to us for each point. We have the chemistry given to us for each point and we have the fluids given to us for each point.

That assumes that there was an error in the fluence. There are several errors in each of the data points that have to be considered. There is an error in the reported chemistry, there is an error in the reported fluence, and there is an error in the reported Sharpey shift.

I took into account the errors in the Sharpey shifts, I took into account the errors in the fluence, and I minimized the sums of the squares of the errors between, the discrepancies between the measured Sharpey shift value and the calculated Sharpey shift value, plus the sums of the squares of the errors between the reported fluence and the fluence as it was adjusted by the fitting code.

In other words, the fluence for each one of these points was an adjustible parameter, and within that sort of a method all of the points were weighted deeply.

Other people have made studies where they have studied various populations separately and they find that the exponential power, the exponent on the fluence, is different for welds and for plate material. In particular, Combustion Engineering has an opinion that the exponent on the fluence for weld material is a lower value than for the plate material.

MR. BENDER: Well, that's enough for now.

MR. HANAUER: Here is the Guthrie correlation, or one of the many depictions of the Guthrie correlation.

(Slide.)

This is hard to see, so the abscissa is the fluence 10^{18}, 10^{19}, 10^{20}; the ordinate is the RT NDT plus 2 sigma, which had to be uncoupled. And as you can see, this is for three different percentages of copper and these are the correlations we are using.

Now then, these correlations, as you can see from this simplified curve --

(Slide.)

Here is a curve which is intended to show schematically how these things go together. Here is Guthrie's mean curve here for copper and nickel, and here's Guthrie's mean curve plus two sigma. And I've added in the sigma in the RTO also, which would move these curves up or down depending on what RTO was. However, the Guthrie correlation gets very large at very large levels of fluence.

And we believe that the Reg Guide 1.99 limit is a more realistic limit at very large fluence levels. However, since the Reg Guide 1.99 already is a limiting curve, we added twice the value of the initial standard deviation and didn't put in another term with the standard deviation in it. It is already in it.

So that the way in which we predict RT NDT for this vessel is we decide, we estimate the fluence at some particular time and then we go to this curve, which -- we go to this curve, plus the RT0, whatever it was. And if the fluence is higher than this amount, we use the Reg Guide 1.99 limit.

And this then is a defined procedure for giving a conservative estimate of the state of any particular vessel. The results are shown on the next vugraph. You had better use your handout, because my vugraph machine did me dirt and this is essentially illegible.

(Slide.)

I'm sorry about that. I don't have my handout up here. I will try and work from this.

Here are the first seven plants in Appendix P, Table P-1 in your report. Here is the initial RT. Notice that we have numbers like minus 56, so I hope, Dr. Shewmon, you can accept that we really tried to do some best estimate here.

Here is the delta obtained in the way that I have described, and here is the standard deviation for a large fraction of the population. The two standard deviations is 60 degrees, but some others are no better.

And here is the result as of the last day in December in 1981, that being the date for which these calculations were made. And these are more or less in order, and you will see that in the right-hand column is quite a crude estimate of when these plants will exceed the screening criterion.

The first one is Robinson Unit 2 -- or Unit 3, which will exceed the criterion in February 1987, four and a half years from now. So that even for our lead plant, our lucky lead plant, there is a substantial amount of time to do something.

MR. BENDER: Steve, is that prior to actions to change the fuel?

MR. HANAUER: These estimates are now somewhat -- Neil, do you want to comment?

MR. RANDALL: For Robinson we took into account the reduced fuel loading because we had the numbers. All of the others, we did not take into account reduced flux.

MR. HANAUER: This is obviously a somewhat moving target. Fuel loadings change, calculational methods are improved. And so I predict by the end of this year there will be a different set of numbers. I know last year there was a different set of numbers.

MR. BENDER: When you look up there you become aware of the two sigma value, which doesn't look like a very big number by itself.

MR. HANAUER: Plus or minus 60 is not small.

MR. BENDER: It is an important number, but if it were the only one --

MR. HANAUER: I've seen whole days spent on 10-degree differences.

MR. BENDER: I'm sure that is the case, and as a matter of fact you are sort of leading to the question I was trying to ask. Because there are a lot of other places where those incremental values are being put together, you are led to wonder how many numbers like 10 to 30 degrees are being cranked into that value.

MR. HANAUER: Into this value? I hope none in this value. Now, you might ask how much of that stuff is in the screening criterion. The answer, there is a fair amount in the screening criterion. The OCA code, for example, assumes that there is a flaw every place there needs to be a flaw; what is that worth? And the probabilistic discussion after the break gives some insight into that.

MR. CATTON: What is ten degrees worth in years, just to get a feel for those numbers?

MR. HANAUER: One to three.

MR. CATTON: One to three years. So 30 degrees up there would be three to nine years?

MR. HANAUER: Yes. It's a lot of years.

MR. CATTON: So it is important.

MR. HANAUER: Yes.

MR. BOCK: Of the RT NDT numbers given up there, can you break those down as to which are Guthrie-limited and which are 1.99 limited?

MR. HANAUER: It can be done. I can't do it.

MR. SHEWMON: Is he approaching the end? Are we going to take a break?

MR. HANAUER: Yes. This is my last pre-break vugraph.

MR. SHEWMON: Well, before or after the break, I would like to hear a discussion of whether the operators are sent home the days we think we're going to get a transient, whether we are doing anything to work with them. That discussion has been completely devoid in that area.

MR. HANAUER: Yes. That is after the greak.

MR. BENDER: Are there any other questions?

MR. REMICK: On Robinson, Steve, the difference in the delta RT between the circumferential and the axial, is that due to a difference of materials in the weld material or is fluence factored into that difference?

MR. HANAUER: Both factors are there, but the largest one is the difference in the copper content of the weld.

MR. REMICK: Thank you.

MR. BENDER: This sounds like the right time.

MR. SHEWMON: One last point, and we will see what we're talking about after the break. Not that those sorts of things have ever inhibited this Committee particularly.

But I have the feeling that these numbers are probably about what they should be, or at least much of the gross conservatism has been squeezed out of them. The other thing comes back to how these relate to the particular trip points that you have said and what is likely to happen after these criteria or trip points or whatever you call them do happen, and that maybe we will discuss for the next couple of years. But maybe we will discuss it today.

MR. HANAUER: We will discuss it at some length after the break.

MR. SHEWMON: Fine. Okay.

MR. ZUDANS: The explanation of difference in delta RDT was given as being different chemistry in the welds. So why are the initials the same?

MR. SHEWMON: Because we don't know any better.

MR. BENDER: Could we break now and let Dr. Zudans get that question after the break?

(Recess.)

MR. BENDER: If we could reconvene, there may be a couple of open questions yet. Bob?

MR. AXTMANN: If I heard you correctly before the break, you were saying that neutron spectrum hardens while it goes through the wall. Is that right?

MR. HANAUER: Yes, sir.

MR. AXTMANN: Could you explain that a little bit?

MR. HANAUER: Not very well. I will call on the experts very soon. The neutron damage comes from the interaction of the fast neutrons with the atoms in the metal.

MR. BENDER: Could we have some quiet in the back of the room, please?

MR. HANAUER: The nominal measurement of neutron flux to include all neutrons above one MEV and no neutrons below MEV is a gross approximation. In fact, there is a spectrum of damage per interaction which depends in a continuous way on the neutron energy. There are various models to represent this. The model which we presently favor uses displacement per atom of the metal as a function of neutron energy. When you do this, you will find that, and you have to know the interaction probability as a function of neutron energy, which of course changes.

When you do that, you find that the neutrons of lower energy have a higher probability of interacting, and therefore as this beam of neutrons goes through the material, the lower energy neutrons are preferentially removed, and so the beam is attenuating as it goes through the material, but the beam at the -- deep into the material has proportionately a larger fraction of higher energy neutrons which have a higher damage potential per neutron, and there is a model for this based upon metallurgical and neutron physics measurements.

MR. AXTMANN: Thank you.

MR. HANAUER: Now, if you want anything more than that, I have to call on my experts.

MR. BENDER: Let's presume he doesn't. Any other questions?

MR. HANAUER: I left one open, which is the operator thing, which I will get to.

MR. WECHSLER: Steve, you mentioned just before the break the fact that the Guthrie regression analysis leads to very high delta RT NDT's at the higher fluences, and that because of that, you choose to use the Reg. Guide 1.99 curve to govern at the high fluence rate, at the high fluence portion of the curve. I wonder if you could amplify that a little for me. I am a little puzzled, because if the Guthrie analysis leads to high values, one has to feel that that is because the surveillance results lead to high values at the high fluence end, and thus there must be some rationale that allows you to prefer to go to the Reg. Guide rather than the Guthrie fit at the high fluence end.

MR. HANAUER: Well, a little is all I can discuss it, and then I will refer to my experts. The Guthrie correlation, and we have Mr. Guthrie to discuss it, chose to use a very simple form for the correlation which was justified in many different ways. When you look at how it relates to the data at very high fluence, we have decided that in fact it is substantially above the data in the very high fluence in spite of its overall least squares characterization, and in our examination of the data at high fluence, we believe that the Reg. Guide 1.99 satisfactorily bounded the data.

Now, that is an extremely general answer, and I will call on Mr. Randall and Mr. Guthrie to collectively or together to answer you in more detail.

MR. RANDALL: Well, when we applied the Guthrie mean plus two, some of the squares, sigma nought plus sigma delta, and compared that number with the Reg. Guide upper limit plus the two sigma nought, the former was higher, but we know that we don't have any surveillance data which fell above the upper limit line of the Reg. Guide 1.99, which was an upper limit line, so we simply said we won't make them use a number higher than any we have observed, and that is why we fell back to Reg. Guide 1.99 in that fluence region.

MR. ZUDANS: But that raises another question. You did that yet in the rest of the portion those data points were included. Then the correlation would have been different if you dropped those points, the ones -- the section of the curve you based on Reg. Guide 1.99 covers a certain range of points, data points that were put in the correlation that affected the previous set, and that means in one cases you included them and in one case you didn't, and that is rather arbitrary.

Is that what you did? In other words, there was a set of data points where the correlation would be right. The Guthrie correlation exceeds that Reg. Guide, and at that point you say, okay, because the Reg. Guide is known to be a bound, you use the Reg. Guide yet you leave the correlation the way it was. And actually, you should have excluded those points in the correlation to see what the correlation does then.

MR. BENDER: So you are saying break it into two populations.

MR. ZUDANS: That is right. Did you do that or not?

MR. RANDALL: No, we did not.

MR. ZUDANS: In that case it is quite arbitrary.

MR. RANDALL: Well, if I understand you, we did not go back and redo the Guthrie correlation.

MR. ZUDANS: But, see, the Guthrie correlation is affected by those points beyond certain fluence.

MR. RANDALL: Certainly.

MR. HANAUER: Perhaps Mr. Guthrie could comment on this point.

MR. WARD: Were there any data above that fluence in the Guthrie correlation?

MR. GUTHRIE: As I understand it, whether --

MR. RANDALL: This comes about because of the addition of the two sigma nought plus the sigma delta squared. There is a scatter point that will show you what the data were.

MR. WECHSLER: That is Page 17 in Appendix E.

MR. RANDALL: Yes, E-17 is right. Now, that shows how well the Guthrie formula fit the data base that we had. Now, when we add that plus two sigma line is really two sigma on that calculation, which is two times 24 degrees or 48. It does lie above the points of the high fluence, and maybe that explains why there is this difference.

MR. WECHSLER: That lies above the points at low fluence as well.

MR. RANDALL: Yes, it does.

MR. WECHSLER: So that would hardly be a reason. The fact remains that the residual calculated minus observed for data points, let us say, above 10^{19} shows roughly the same scatter as the points below 10^{19}, as the points far below 10^{19}. So I have to say I really don't understand your answer.

MR. SHEWMON: Would you restate your original question?

MR. WECHSLER: Yes. My question relates to the statement that Steve Hanauer made that the Guthrie fit, the Guthrie equation shows very high predicted delta T or delta RT's NDT and for that reason instead of using the Guthrie fit for the higher fluences, they chose to use the Reg. Guide 1.99, and so I asked, what is the rationale for having done that. I understood Neal Randall to say that the reason was, as you can see in this figure, E-17 in Appendix E, you can see that the plus two sigma line is above all the residual values in that plot, not just those that pertain to fluences above 10^{19}, and so I still remain uncertain as to the rationale for using the Reg. Guide above $10^{19\text{th}}$.

MR. RANDALL: All I can do is repeat my original statement that being pressed to not be overconservative, we used Reg. Guide 1.99 upper limit in that region because no data points from surveillance fell above that Reg. Guide upper limit curve.

MR. BINFORD: If you superimposed on that diagram of E-17 the Reg. Guide 1.99, I wonder what it would look like.

MR. RANDALL: I have not done that.

MR. GUTHRIE: I was not intimately involved in drawing this broken part of the curve there, where the 1.99 went in. But if you look at this figure that we have been referring to for the last couple of minutes, if you became sympathetic to the owners and wanted to give them as much as you could and still maintain safety, you could draw a slightly tilted line in the upper righthand part of the figure and still bound the data that is plotted on this graph, and this does give -- well, it doesn't penalize the owners as much as they would be penalized if you used the plus two sigma lines.

It is possible to draw another straight line which intersects the plus two sigma line somewhere around 1 x $10^{19\text{th}}$, and goes down to the right, and therefore is lower in the higher fluence ratings.

MR. WECHSLER: I see, so you are essentially saying that above 10^{19th} in Figure H-17, there are more points lying above the zero line than lie below it. In other words, your formula overpredicts based on the actual data for fluences above 10^{19th}.

MR. GUTHRIE: In H-17, what I am saying is that in that pattern under the plus two sigma line in the upper righthand corner there, if you wanted to give the owners everything you could without sacrificing safety, you could draw that plus two sigma line with a slight downward slope up there in the upper righthand corner -- you don't have to keep it flat -- and still cover all the data that is available.

MR. BENDER: Let me ask, if I can, that we leave the detail of this discussion to the private conversations. I want to get Dr. Hanauer through this story today, and we may be able to come back to this or get to questions later on.

MR. ZUDANS: There still remains this question that I asked, namely, why the population wasn't split up into two pieces at that point.

MR. BENDER: You might even ask whether it should be in several populations, or whether you should have a straight line. There are a lot of things you could ask. Or how good is the fit.

Bill, did you have something different?

MR. BOCK: Well, I wanted to elaborate a little bit on this subject. I think I know what the problem is. But we could defer that. I can do it in about two minutes, or we could defer it.

MR. BENDER: Well, go ahead.

MR. BOCK: The problem, I believe, is not so much the fluence as the nickel, and if you flip over two pages to where you see percent nickel as the abcissa, virtually all of the test specimens used have a nickel composition of less than .75 percent, so we have good data in that range, but we are now trying to apply it to vessels with much higher nickel content, for example, Robinson is 1.2, or Calhoun with 1.0.

So, we are beyond the range of experimental data, and we are trying to extrapolate out there.

MR. BENDER: Well, that is one viewpoint. Steve, I think we had better go ahead.

MR. HANAUER: What I propose to do now is to talk about the probabilistic analyses we have done, to bring in the question of what the operators do and what we have done about what the operators do, and then to talk about where we go from here.

The probabilistic analysis in June was viewed by us as a long-term research program. However, as a response to our June position paper, in this room, the industry pointed out a number of things which in their opinion we have done incorrectly, and brought to us in May, actually, a Westinghouse owners' group probabilistic study of pressurized thermal shock that provided a great deal of insight, and which has, I certainly hope, been provided to you.

There is a very large amount of documentation on this whole subject. You will find it summarized in Appendices A and B of the report. If the working group discovers it doesn't have some important pieces of paper, they may lay the oversight on us, and we will of course make all of it available. I don't believe any of it is proprietary, but if it is, we know how to do that too.

Now, we have a research program going on which in a couple of years will presumably walk in the same footsteps as the work I will now describe, perhaps with additional precision and completeness, perhaps not, but the work I will now describe is the work of the Westinghouse owners' group reported to us in a May letter and in several meetings between June and now.

What they did was to consider about 20 initiating events that could lead to overcooling transients and pressurized thermal shock. They then drew in the usual way event trees and safety functions, and determined which event sequences of the possible ones were in fact significant in terms of probability and could in fact result in pressurized thermal shock sequences. Those that they consider to be significant, after some of our discussions, they then characterized in terms of TF, theta, and constant pressure, and frequency or probability as a result of their evaluation of the trees.

They then used Strosnider's results, which have been discussed with the subcommittee on several occasions, and which I will recapitulate very briefly, to determine probabilistically an evaluation in detail of the curve beyond but including the range in which the experience was involved.

Now, here is the place I suggest to consider the role of the operator in the actual experiences, the role of the equipment functionability, and the actions of the operator determined which sequence actually took place in these eight events amongst the dozens or hundreds of possibilities of sequences, and so what we have in these eight events, evaluate them how you want, what they have evaluated is the eight sequences which actually occurred, and any inference you draw from them assumes that the operator actions as well as the equipment actions of those eight sequences are somehow typical of how the operators and the machinery works in a more general way.

Now, we know that at best such an extrapolation is only approximate, and that the right way to do it is to consider all of the possible sequences or all of the significant sequences, and to include in them in some way at the branch points the operator does or does not do this or that important function which significantly affects the output of the pressurized thermal shock sequence.

Now, the methodology, the science behind this is not very well developed. Swain and his co-workers at Sandia have over the past number of years published a number of handbooks and methods to estimate the probability of whether the operating crew will or will not do some necessary thing. We have at this time no methods for estimating whether the operators will do better than that and will in fact mitigate the situation beyond their stereotyped procedures.

Similarly, we have at the present time no models for predicting whether the operators will do something bizarre and make the situation worse outside the parameters of their operating procedures. About 150 miles north of here is one data point in which the operators did in fact do some bizarre things, and the consequences were severe indeed. However, we don't have at this time any really scientific way of making such predictions, although we do have the beginnings of a scientific way of predicting whether they will or will not do some defined correct thing.

Now, this has been handled in two ways which are really quite diverse. In the Westinghouse probabilistic study, the Westinghouse owners' group probabilistic study, one of the parameters is the time delay of the operators in doing certain important, correct things, and one of the time delays is infinity, they don't do it at all, and so operator action has been included in this way in the probabilistic study.

Now, it is also clear from the pressure curves which I showed earlier and from the course of some of the actual events that the operators can really make things a lot worse or a lot better, and so we have in progress and more than half completed a program in which we have audited the procedures related to pressurized thermal shock in the eight plants for which we got pressurized thermal shock evaluations from the licensees last year.

Then, in addition to auditing the procedures, audit team has discussed with representative members of the operating crew how well they understand pressurized thermal shock, and has assessed in an extremely crude way the likelihood of whether these operators would do the right thing in a pressurized thermal shock sequence. This evaluation was not quantitative, but was rather an overall evaluation. The results have been varied. Some crews did rather well and some crews did rather poorly.

These audit results -- I guess there are now three or four, Jim, that have been issued. Do you remember?

MR. CLIFFORD: All of them have been completed. The reports are in and should be distributed very shortly.

MR. HANAUER: There are a few reports that have already been distributed, aren't there?

MR. SHEWMON: The Robinson report was distributed several months ago. We have not seen anything since.

MR. HANAUER: Jim, what is the present status, please? Mr. Clifford.

MR. CLIFFORD: Jim Clifford of the staff. All of the reports have been received and submitted to licensing and should be distributed shortly.

MR. BENDER: We have seen more than one, but I don't think we have seen them all.

MR. HANAUER: You have certainly not seen them all. At least one has been deferred.

MR. SHEWMON: Well, I haven't, and the consultants who are most interested in that haven't.

MR. BENDER: Well, I agree with that, that they haven't been probably generally distributed.

MR. HANAUER: The results have been, there is one other piece of information, and that is, there is an ongoing very large and well known to the Committee improvement program in emergency operating procedures involving a large program of realistic analyses of a large number of event sequences. The correlating of these realistic analyses into new technical procedure guidelines, including some symptom-based guidelines for maintenance or restoration of the critical safety functions, this is under Three Mile Island Action Plan Item 1-C-1. It has been going on for at least a couple of years, and very likely will be going on for at least a couple of more years.

As part of this program and as part of the pressurized thermal shock analysis, we have a Westinghouse report dated a couple of months ago in which a team of Westinghouse owners' group evaluated the presently developmental Westinghouse owners' group emergency operating procedure guidelines, and that is probably not the formal title, and concluded that in fact they don't treat pressurized thermal shock particularly well, and a substantial number of changes were appropriate.

This report I certainly hope has been sent to the Committee.

MR. BENDER: Steve, your observation leads me to a direction which I hoped you would be able to discuss some. The Robinson report was not very comforting.

MR. HANAUER: No, sir.

MR. BENDER: It was an audit done very early in this program, however. Are we in a position now to say that we know what kind of training program the operators need?

MR. HANAUER: Well, I will give you my opinion, which is that the procedure guidelines, at least the Westinghouse procedure guidelines, which are the relevant ones for Robinson, are being changed in quite a drastic way for Three Mile Island type reasons, and that a few months ago the developmental revised procedures were shown to need some more work regarding pressurized thermal shock. That tells me, no, we don't know all we should about pressurized thermal shock in emergency operating procedures, and in fact the audit team developed a very useful, I think, set of guidelines about the things that should be done now in auditing pressurized thermal shock procedures, and that these guidelines can be found in Appendix -- Which one is it, Jim? Do you remember? Of this report?

MR. CLIFFORD: Appendix C.

MR. HANAUER: Appendix C of this report contains a discussion of what can be done now. The right way to fix this for the long term is to get these guidelines right for pressurized thermal shock and everything else. You walk, you see, between overcooling the vessel on one side and undercooling the core on the other side, and that is one of the reasons why I don't want a highly conservative pressurized thermal shock design basis accident which impels the operator to undercool the core and make us another kind of an accident.

Your point is very well taken, in other words, that we don't know some things about how to write these procedures, and the training, of course, goes with the procedures. We in our offices, the kinds of people we have in a room, in our quiet offices, must solve the pressurized thermal shock problem before and not give contradictory instructions to the people on the night shift who have to make these decisions and operate the plants.

Therefore, the audit teams have very properly been rather modest in what they have recommended as short-term changes.

MR. BENDER: Well, I don't think we can explore this further today, but as a long-time proponent of depressurization for a number of reasons, recognizing that it can't hurt anything in the thermal shock business, I was rather hopeful that at least that aspect of things would have been developed better and sooner, and I am not clear that we understand even what needs to be done.

MR. HANAUER: It has not, and practically every line or every page in the Westinghouse procedure review points out that for this, that, and the other sequences the operators are told to repressurize to 2,000 psi.

MR. BENDER: Well, that is hardly reassuring, but let's go on.

MR. HANAUER: All right. Now, at the present time, we do not have the calculational facility. We have all of the theory, but we simply haven't done the code work to represent these various transients in terms of critical RT NDT and thereby to take into account the reality of the transients. We have been in the present state of development constrained to use TF, beta, and constant pressure to characterize all of these different sequences.

(Slide.)

MR. HANAUER: When you do it you get these results. If you will direct your attention first to the dotted curve, this is the Westinghouse PRA expressed in terms of TF. Now, in fact, the Westinghouse PRA used TF, beta, and pressure, and so I am oversimplifying an oversimplification in order to get it onto a one-dimensional vu-graph, but this is the curve which is obtained. You will notice that it has the same shape as my first vu-graph that showed tails and so on, and that shouldn't surprise anybody. It goes from very high frequency above about 300 degrees to rather low frequency, 10^{-4} or lower below 200 degrees.

Now, the staff has not accepted every detail of the May-June Westinghouse PRA. The staff has found a number of sequences we believe were incorrectly characterized, some overconservative, some overoptimistic, and the staff has not agreed with some of the Westinghouse frequencies and probabilities, and so the staff has, and this is explained at some length in the report and in Appendix G to the report, the staff has devised a PRA which differs to some degree from the Westinghouse PRA, and which is shown as the solid curve here.

Now, neither of these has had the kind of man-years of work or the kind of peer review that this subject deserves, and both of these results must be characterized as preliminary in some ways. My own belief is that a great deal more of this work is justified. We have a research program in this area, and I think the industry ought to work on this, too.

Moreover, we have only the Westinghouse generic analysis, in which they have tried to bound or typify in various parts of analyses the Westinghouse plants, so that it is generic to Westinghouse plants and individual plants may differ substantially. This question has not been evaluated, and the applicability of this to Combustion and B&W plants is not now known. We do not have comparable analyses for these other kinds of plants.

MR. SHEWMON: You've plotted the frequency of what up there?

MR. HANAUER: This is the frequency of getting a transient more severe than TF equal to this value. We are back to TF, beta and pressure, and what I did, or what we did was to take the dozens of different sequences --

MR. SHEWMON: TF is fluid temperature?

MR. HANAUER: Yes, sir. Each sequence we characterized in terms of TF, beta, pressure and frequency, and this is a result plotted in TF space.

MR. BENDER: Steve, a separate aspect of the same question has to do with the matter of how much operator reaction is in those curves.

MR. HANAUER: There's a lot.

MR. BENDER: Westinghouse, as I recall it, set up three time periods -- maybe there were more -- for operator response and assigned some frequency of correct action to each time period.

MR. HANAUER: Yes, sir.

MR. BENDER: Is there any judgment of whether using those kinds of what amounts to arbitrary evaluations of operator response has an effect on the depiction up there? And are they important to the argument?

MR. HANAUER: I can speak best to the staff analysis. The dominant events in the staff analysis do not depend very much on operator action. We evaluated the Westinghouse numbers. They seemed reasonable to us, but in the end in ours, -- I haven't undone the Westinghouse one enough to answer. But in ours, the operator action is not terribly important because the dominant event is the stagnant, small break loss of coolant accident and the operator can't do much about it.

In some of the others, the operator reactions are important. I don't have an analysis that answers your question in any quantitative way.

MR. ZUDANS: Just quickly to make sure that we are on the same wavelength. You said these curves represent the beta and p is constant. Did Westinghouse consider constant pressure when they did their PRA?

MR. HANAUER: Westinghouse did exactly what I described. They also characterized sequences for constant pressure, TF and beta because the only probabilistic vessel response calculation in town requires that as input.

Now, this can and should be improved, but has not been at the present time.

MR. ZUDANS: I have a problem with this concept of constant pressure.

MR. HANAUER: Of course. It is a gross over-simplification.

MR. BENDER: Well, it depends upon what pressure you use.

MR. THEOFALOUS: It seems to me if you could take this curve one step farther, a small step farther, to give it in terms of critical RT NDT, it would be extremely useful. And of course, that could help you focus on exactly the kinds of sequences that are important to look at. And one can dig in more detail into those sequences.

MR. HANAUER: It could, indeed. That is a piece of business that has to be addressed.

MR. THEOFALOUS: You could use that chart to get from this kind of a plot to this plot down here.

MR. BENDER: But you have to become very plant specific I think in order to do that.

MR. VAGINS: You have it in the following curves.

MR. THEOFALOUS: Maybe you have it already.

MR. VAGINS: Within a range of some transients.

MR. HANAUER: Yes. At the moment we have this stereotyping of the transient as an intermediate step. You are quite right, we should develop a better method.

(Slide.)

I now show you three of the probabilistic vessel failures. You have had this described to you before. In two minutes, what Strosnider and his colleagues have done is taken essentially the same deterministic fracture mechanics model, as we discussed earlier, but instead of assigning conservative or estimated values for the parameters, they took frequency distributions of the parameters which were not well known or which were subject to variation and calculated a large number of possible events in which these various parameters were picked in the Monte Carlo probabilistic way from these frequency distributions.

The result was then they did this 10^6 or more times for each case in a code that does this for you, and the result is that some fraction of these 10^6 resulted in vessel failure and some fraction did not. And from this was deduced in a simple way an estimate of the probability of vessel failure, given this transient.

Now, the problem at the moment in doing what you suggested a moment ago is that this code today is set up with TF, beta and pressure. Next year we will have something better. And I have three cross-plots of this response surface, which are in the report and which you have no doubt seen before.

Here it is with temperature as the abcissa.

Always the probability of failure is the ordinate, and here, beta is the parameter. As you can see, these curves are very steep in temperature. That is to say, a factor of 10 change in probability is associated with a very small number of degrees, like 10 or 20, change in temperature. So the temperature is, indeed, a dominant variable.

You also see that for large values of beta there is very little difference, but as beta gets small it makes a substantial difference and decreases substantially the probability of vessel failure calculated in this way.

Another cut of the same response surface is shown here --

(Slide.)

-- in which the abcissa is pressure, and here is an answer to your question now, in probability space. Here is the probability of failure as a function of pressure with a constant beta of .15, and now the temperature, T final, is the parameter. And you see that the slopes of these curves are not very large; that a factor of 10 in failure probability is associated with 1000 psi or more. This is not exactly negligible but it is not the very steep behavior that was shown in temperature space.

(Slide.)

A third section or cross-plot of the same surface is given here, in which we plot the beta dependence, and not surprisingly for all large betas, the dependence is very small. But as the betas become smaller, the accidents become much less severe. So as we knew already, but it is comforting to see it, as a result for slow transients the thermal stresses are small and the probability of vessel failure is small.

MR. THEOFALOUS: The question I was asking before could be answered also in terms of these graphs here. Do you have a way, -- if you knew that you were interested in a distribution of this plot, do you have a way of backing out which sequences are contributing to that?

MR. HANAUER: Yes, but a fairly crude one, which we can show very quickly in the next slide. Let me show you an example.

(Slide.)

We have, in fact, made the calculation, exemplified by this curve. This one has -- this one is for the Westinghouse probabilistic analysis, and the Strosnider, et al vessel failure analysis, coupled in the way that I have described. And you see here the contributions of various kinds of accidents. So yes, it can be done. It is some work.

Now, yes sir?

MR. SHEWMON: Before you go with your development I want to go back and finish your question. I wanted to see the previous slide, but you can answer the question here.

MR. HANAUER: Do you want the next previous one?

MR. SHEWMON: I wanted beta versus conditional failure. We were talking earlier about heat transfer coefficients of 300 or 1000. Is that with 300 or 1000?

(Slide.)

MR. HANAUER: Three hundred.

MR. SHEWMON: And that, what, tends to flatten it off then, when one gets the very high betas?

MR. HANAUER: Well, it is a combination of the 300 and the conduction in the metal.

MR. SHEWMON: Yes, but if you get -- I mean, they are in series, so the conduction of the metal isn't a disposal parameter.

MR. HANAUER: That's correct.

MR. SHEWMON: And as you get the heat transfer in the liquid slower, that ought to --

MR. HANAUER: There's a curve in your report where the heat transfer coefficient has a parameter which I didn't reproduce. It would be -- or it's in the appendix, isn't it? Jack, maybe you can refer to it. We have the surface cut in heat transfer space.

MR. SHEWMON: I would have taken a yes for an answer.

MR. HANAUER: The answer is yes.

MR. STROSNIDER: It's Figure H-30.

MR. HANAUER: Figure H-30 has just that dependency in it.

MR. ZUDANS: Was cladding included in these calculations for both heat transfer and mechanical, or neither, or some other combination?

MR. HANAUER: Heat transfer, yes. Mechanical, no. The heat transfer of the cladding was included in deciding to use 300. That includes the effect of the cladding; it was lumped.

Now, in these calculations the stress due to the differential expansion of the cladding is not included. We have done some more recent calculations, but it is not included in this.

MR. ZUDANS: So if the 300 represents both the film coefficient and the cladding, what was the film coefficient in that combination?

MR. KLECKER: If one assumes water to metal heat transfer coefficient of 300, the effect of heat transfer coefficient is approximately 200. If you assume roughly 600 to 1000, then the effect of heat transfer is more like 300.

MR. ZUDANS: So this is not the effect of coefficient of heat transfer and metal? The 300 was assumed for the film coefficient and the metal contribution was added to it?

MR. KLECKER: We have done that in our deterministic calculations.

MR. STROSNIDER: Let me answer that. The effective heat transfer coefficient was 300 which would correlate to a film transfer coefficient of 600 to 1000.

MR. ZUDANS: So you never run any calculations of film coefficient of 300?

MR. STROSNIDER: No.

MR. ZUDANS: Plus the conductivity in the metal. So really, all of this conversation has been misleading. That is not the film coefficient; it is the 600 to 1000, and that is quite a different story.

MR. SHEWMON: Well, let me ask, since another factor of 2 in whatever they have called their transfer coefficient, and all of a sudden it starts having an order of magnitude change in the probability of the accident. So I would be interested in -- it is your Figure H-30, go look at it -- I would be interested i hearing the heat transfer people comment on what they think of a film transfer coefficient of 600 to 1000 by a factor of 2 or 4.

MR. THEOFALOUS: Two or even three.

MR. BENDER: But how do you convert that into the overall heat transfer coefficient? What I'm talking about is if I cut the film coefficient in half, what would be the overall heat transfer coefficient equivalent to 300, when you are using a value of 600 to 1000?

MR. ZUDANS: That's a good point.

MR. THEOFALOUS: I have done that in the little thing I gave you, and that is one I remember. I was coming up with a best estimate, something like 200 for overall, 200 to 300 overall.

MR. BENDER: Well, it seems to me one of these days you've got to look at that point. It is not a one-to-one relationship, but it does have a big effect.

MR. SHEWMON: Steve, if you look at your curve, it has between one and two orders of magnitude difference in going from 300 to 150. So there is a profound effect on the probability, if I might use that word.

MR. ZUDANS: And it's not sure that this is the film coefficient. Maybe this is combined.

MR. SHEWMON: But at least he is on a steep part of the curve.

MR. BENDER: If I heard Jack Strosnider right, he was talking about the overall coefficient.

MR. ZUDANS: That's right. And that is 600 to 1000.

MR. BENDER: In Theo's number, the 300 might be 200, and that might be an important difference.

MR. HANAUER: It might, indeed.

MR. BENDER: Carry on, Steve.

(Slide.)

MR. HANAUER: The results. I have two, one for the Westinghouse and one for the staff. The results are plotted as frequency per reactor year of cracking the vessel without arrest. Now, arrest is in the model. If the crack arrested, we didn't count it as a failure, and we included in our arrest model an upper shelf tough failure limit of 200 ksi square root of inch, so we didn't allow arrest with a K1 applied higher than 200 ksi square root of inch.

So we don't show any arrest for cracks that extend more than about halfway through the metal, if I recall correctly. But crack arrest is now in here, and when it says "crack extension, no arrest," it really means crack extension with arrest calculated not to occur.

Now then, here is the frequency per reactor year, and it is plotted in terms of the RT NDT at the surface of the vessel. So a vessel starts off at the beginning of life off the lefthand side of this diagram, and as the vessel ages its RT NDT creeps to the right in accordance with the fluids and the properties of the material. And then, as the temperature gets in the interesting range, the probability of it failing per reactor year or frequency per reactor year goes up in accordance with this curve.

As you can see, --

MR. THEOFALOUS: Are you going to change?

MR. HANAUER: Soon.

MR. THEOFALOUS: Before you change, I was curious on the small break. Some small break LOCAs will result in loss of natural circulation, while some other ones will not. And the difference in the behavior is very different, drastically different. And I was wondering how do you assign relative probabilities to those small breaks that don't have natural circulation versus the ones that do? And which ones are you plotting here?

MR. HANAUER: I will tell you how we did it. This is a plot of the Westinghouse analysis in which the small break loss of coolant accident has a negligible contribution because in their analysis, which included the effects of warm pre-stressing, as ours did not, they calculated that the small break loss of coolant accidents would be arrested by warm pre-stress phenomena.

As you will see in the next slide, in ours the small break LOCA is dominant. What we did was we divided the spectrum of break sizes and selected for a separate set of boxes the break size in which -- the break size range in which stagnation would occur. We calculated its frequency and its consequences separately from the others, and they turn to to dominate.

MR. BENDER: Before you get off that, --

MR. HANAUER: I have another one for the staff PRA.

MR. BENDER: I just want to ask what pressure is associated with this computation?

MR. HANAUER: Each sequence was evaluated in this approximate way and characterized by a constant pressure. They were different for different sequences.

MR. BENDER: But they are mostly elevated pressures now?

MR. HANAUER: Well, the one that dominates the small break LOCA that stagnates is -- we used a pressure of 1000 psi, which is where we calculated the pressure would hang up.

MR. ZUDANS: In determining this arrest or not arrest curve, essentially the critical RT NDT at the surface was the crack propagation through the thickness followed, and the stress factor reduces, and the RT NDT reduction taken into account?

MR. HANAUER: Yes, sir, that was done in some detail for each calculation. The properties of the material as a function of the irradiation, which is different through the wall, and the temperature as the temperature changes, the change in K1 as the crack enlarges, and the change in the critical in the crack initiation K and in the crack arrest K as a function of temperature were all taken into account.

MR. BENDER: But the crack is always in the worst place?

MR. HANAUER: This is all for longitudinal cracks.

MR. BENDER: Yes, I know.

MR. HANAUER: And there was a crack size frequency distribution applied here. It wasn't assumed that a crack of the correct size was always there for this calculation.

MR. ZUDANS: The reason I asked the question is when you discussed initiation of a crack you stated the pressure effects are not greatly significant. Now as you propagate in the wall the temperature stresses disappear.

MR. HANAUER: Not in the timescale of these calculations they don't.

MR. ZUDANS: Well, they go for minutes. The heat transfer and temperature distribution get stresses. In fact, thermal stresses, in fact, on the outside are compressive.

MR. HANAUER: Yes.

MR. ZUDANS: So there is some point where there is no thermal stress contribution whatsoever, and the only driving force is pressure.

MR. HANAUER: We have one experiment where thermal stresses alone drove a crack 95 percent through the wall.

MR. SHEWMON: You have what?

MR. HANAUER: There is one of the HHST experiments where the vessel was cooled with liquid nitrogen on the inside, and thermal stresses drove a crack about 95 percent through the wall. Which is what was predicted by this model.

MR. ZUDANS: But that was --

MR. HANAUER: That was an extreme case. That wasn't a pressurized thermal shock case. That was a very severe thermal transient.

MR. ZUDANS: But here, -- well, --.

MR. SHEWMON: That was a long time where they kept on doing something to it?

MR. HANAUER: Cooling it. No pressure.

MR. KLECKER: I was just going to comment on the fact that you do, indeed, have compressive stresses at external, but the fact that you have a stress distribution puts a moment on the vessel, so even though they were compressive without any crack being present, the fracture mechanics takes into account the generation of this moment.

MR. ZUDANS: In other words, if you compute it correctly and remove the material on the crack that is already propagated, the stress factor is not --

MR. BENDER: Well, we want to be very careful about using the small experimental models as a basis for evaluating.

MR. HANAUER: This one was six feet long and a couple of feet in diameter, and six inches thick, as I recall.

MR. BENDER: Well, I'm not going to try to debate the experimental circumstances so much as to just recognize that we don't understand the structural models as well as we ought to. And while there are people that calculate things, might be able to calculate the results relating them, to the specific vessel that we are talking about needs to be understood a lot better than I understand it.

Go ahead, Steve.

(Slide.)

MR. HANAUER: The most significant curve is the one -- to us at the present time -- is the one based on the NRC analysis which differs from the Westinghouse one in several important respects. But by far the most important is the dominant feature of the dominant contribution of the small break loss of coolant accident in the range where it stagnates.

In the interesting range -- this is, in fact, the dominant contributor, and it goes from about 250 in the Westinghouse curve to about 205 in the staff's curves.

As I said before, there are a substantial number of youthful features of these curves, and the methodology in some respects and the input data in some respects are not matured to the point where one really ought to believe these curves in the sense of deriving regulatory requirements directly from them, and we do not do so.

That is to say, we have not laid an ordinate here as some value of acceptable risk and read it over to the curve and read out surface RT NDT that tells whether these plants can run or not. We don't believe that the methodology -- and goodness knows, enough differences and difficulties in this methodology have been discussed by you and us today to make my point I think entirely adequately.

What this tells us is, first of all, the slopes. The slopes are, in fact, a factor of 10 for about 20 to 30 degrees change in RT NDT. And that tells us that, in fact, within the limitations of this model, the RT NDT of the vessel is, as we expected, a central parameter in judging the acceptability of operation of one of these embrittled vessels.

Now, at first site, this implies that a vessel at 270 degrees is in big trouble, and that it implies a failure probability of greater than 10^{-4} per reactor year, and vessel failure makes my corns itch and gets a lot of people itchy in a lot of places. And 10^{-4}, if I really believed it, would be kind of an alarm signal.

This isn't true because of the conservatism in selecting the value of the vessel to RT NDT to compare with 270.

(Slide.)

Here I have plotted in a very crude way a frequency distribution of vessels that satisfy the 270 degree criterion. Whether this is actually the case is not known, but I have used the picture provided by the earlier discussion of the scatter of the data. This is a curve for which two sigma was 60 degrees, which is kind of a typical value for the plants which we were talking about, and I have located the two-sigma point at 270 degrees. So this is the spectrum of the actual properties of vessels which just hit the screening criterion if vessel properties are normally distributed and if two sigma is 60 degrees, so there is a kind of an assumption leap here.

MR. SHEWMON: Would you explain if the small break LOCA is the worst one you have come up with, stagnation, I would be interested in hearing you go through what is the scenario which gets us into trouble, or what is assumable, since you differ by several orders of magnitude from Westinghouse. There must be some difference. The perception might be interesting.

MR. HANAUER: Yes. The accident occurs in the following way. There is a break in the primary system in which the leakage rate exceeds at high pressure the injection rate of the high pressure injection system. And in which the energy carrying out the break exceeds in the early part of the accident the decay heat which has to be removed.

The result is that the inventory of primary fluid decreases because the outflow is greater than the income. And the inventory decreases but the heat generation means that the pressure doesn't go down. The pressure is calculated for the case I've seen to hang up at about 1000 psi. As the pressure decreases below the shutoff head of the high pressure injection, the high pressure injection comes on, the inventory continues to decrease but there is finally a secular equilibrium established. The pressure goes down to about 1000 psi, the input from the HPI can make up the loss at some lower pressure. The leakage rate goes down with pressure and the pump injection rate goes up with pressure, and an equilibrium is established at about 1000 psi.

But during this period, the level is too low to support natural circulation. And so, the water going in the cold leg -- there isn't any loop flow to mix up. There isn't any significant flow from the steam generator through the cold leg into the reactor vessel. The only flow which is available is whatever churning goes on from the heat input from the walls of the inlet pipe, plus whatever mixing takes place. And here we have reference to the criari tests.

MR. SHEWMON: So in that case you are cooling one part of the vessel badly and the other part isn't getting cooled?

MR. HANAUER: Well, that depends upon what goes on in the downcomer. If you have no activity in the downcomer at all, you have a stripe of cold water going down the downcomer which, depending on the vessel and depending upon which downcomer you look at, either does or doesn't have a weld close enough to get cold.

Now, that is one of the things that has to be looked at plant specific. We've assumed that there is a weld there. That's the scenario.

MR. THEOFALOUS: On this problem, I think that you -- I think your temperature in the downcomer is overly conservative. You are using probably -- you have the cold leg as part of the downcomer, like you say, in your report and I think this is just way, way, overly conservative. And I have done myself some calculations on that, and if you would like I could give you numbers. I gave it to Ed yesterday.

MR. HANAUER: Well, if you gave them to Ed, you gave them to me. And yes, this is subject to plenty more calculational refinements.

MR. KOUTS: I think if you look at it carefully you will probably find a number of other very large conservatisms there. The heat transfer coefficient under these circumstances is likely to be much lower than you assume for other kinds of transients that you've analyzed.

The probability distribution for the cracks, which really have to take into account the probability that a crack exists in a location, in a weld, which is in the thermal stripe region.

MR. BENDER: Herb, could you use the mike? The reporter is having trouble hearing you.

MR. HANAUER: Yes, I think all of these are things which have to be taken into consideration and have not yet been.

MR. KOUTS: In light of the steepness of the curve with respect to heat transfer coefficient that was just pointed out earlier, these will probably have a profound effect on the thing which is having the largest influence on the shape of your total curve.

MR. HANAUER: Yes. Now, they do not have an enormous influence, because the next one comes in 20 degrees higher. And there are a number of contributors within 20 to 50 degrees.

MR. KOUTS: Well, I am looking at the probability.

MR. HANAUER: Yes. These curves are steep. If this curve were to disappear, the probability would change by about a decade in this region. So, yes, as I said, these things are not mature; they are still under development.

MR. BENDER: Steve, to just get back to the pressure question one more time. If the number was 1000 p.s.i., would that be because a break is limiting the rate at which the pressure can decay?

MR. HANAUER: Yes, sir.

MR. BENDER: And that presupposes that an operator does not do anything to change the pressure from that level?

MR. HANAUER: That is correct.

MR. BENDER: Thank you.

MR. HANAUER: It is this kind of consideration that led us to stay at 270 in spite of the fact that our best notion of operating experience tells us that if 10^{-2} were a real number, we should go up above 300 degrees somewhere.

(Slide.)

But it was the kinds of considerations in this curve that told us for the present to stay at 270 rather than switch.

Now, the amount of science in this conclusion is not very large. Ther are large uncertainties in the probabilistic evaluation. There are uncertainties we have discussed in the inference from experience.

You can get more safety, more conservatism, by decreasing the screening criterion. You can get a lower level of safety, less conservatism, by increasing the screening criterion from 270. If you believe the slopes of these curves, then a 30-degree change in the screening criterion gives you about a factor of 10 in the probability of wrecking plants due to pressurized thermal shock.

MR. ZUDANS: One question. When the heat transfer analysis was done with a stagnant hot leg and a certain amount of fluid coming in, was still the assumption made that the entire surface of the reactor vessel and the downcomer gets washed with that cold fluid?

MR. HANAUER: Yes, sir.

MR. ZUDANS: That is not likely to happen, is it?

MR. HANAUER: No, it does not fit. We used the available calculations of thermal hydraulics, and we used the available reactor failure probability calculations for which that incompatible assumption was made.

MR. ZUDANS: So the thermal stresses then computed were based on asymmetric configurations?

MR. HANAUER: Yes.

MR. ZUDANS: And that is not right either.

MR. HANAUER: That is correct. That is the present state of our calculational ability.

MR. THEOFALOUS: Well, you found a lot of reasons why you assumed the small break, and I want to look at the second one. Is there anything in doubt concerning the steam line breaks calculation?

MR. HANAUER: Of course.

MR. THEOFALOUS: But anything of substance?

MR. HANAUER: Of course. The small steam line breaks turn out to be more serious than the big ones. First of all, they have a much higher probability; and secondly --

MR. THEOFALOUS: But those are reflected in here. I am saying, is there anything which puts the line into doubt -- this line, which already reflects the different sizes and so on -- is there something in the methodology is what I am asking? A heat transfer coefficient or what have you?

MR. SHEWMON: That is why we are paying you this exorbitant fee to come here and help us with that.

(Laughter.)

MR. HANAUER: The answer is that many of the same models were used; in particular, the reactor vessel failure model was identical. Of course, now you have either forced circulation or natural circulation, so the heat transfer coefficient and the mixing in the downcomer is more likely to be realistic.

It turns out that the worst steam line break is a small break which occurs at hot standby, rather than in operation, because you do not have the power generation to heat things up. But that has a lower probability. That is all in this curve, and all of the numbers are subject to discussion. What is the probability of a large small steam line, a small steam line break, a bypass valve opening, which is a steam line break; all of that stuff.

MR. THEOFALOUS: So the interesting question is how does one proceed from here? If we wanted, for example, to take advantage of what Professor Shewmon has said, and we wanted to look at the probability of

10^{-5}, and I want to know what steam line break and what particular scenario gave you that point, can I get that point from somebody?

MR. HANAUER: Yes. It is discussed briefly in the report. And we have some more information.

MR. THEOFALOUS: I do not think you have this kind of information in the report. I looked at the report.

MR. HANAUER: Look in Appendix G.

MR. BENDER: Steve, separately from Theo's comments about the point, is the matter of how much we understand the heat transfer, heat transport behavior, of the steam generator as a whole, which seems to me to have a big influence on steam line break behavior?

MR. HANAUER: For large steam line breaks, there is a lot of uncertainty because the steam generator is far outside its normal operating conditions. For small steam line breaks, remember small steam line breaks are not necessarily rending metal, they are bypass valves open or relief valves open or something like that.

The steam generator is operating near its normal mode, and that is not a large uncertainty.

MR. BENDER: Well, maybe not, but I would like to know more about it.

MR. HANAUER: There is a limiting return here. You would not want to pay too much or delineate too elegantly the heat generator elements of this curve, if you are imprisoned in the reactor failure probability calculations with the many uncertainties that are involved there.

MR. BENDER: Well, I agree with that. And maybe we would be working too hard under the tail. But a lot depends upon how much feedwater is in the steam generator, particularly under shutdown conditions and what its temperature is and what is being done to control it.

MR. HANAUER: You can be more elegant in this area by picking more scenarios, chopping up your probabilities, and considering many different branch points in your event tree and analyzing each one. This, to the extent that the models are available, this is only the use of resources to do this.

MR. BENDER: Paul, did you have another question?

MR. SHEWMON: No.

MR. BENDER: How near are you, Steve, to being at the end of your harassment?

(Laughter.)

MR. HANAUER: I would say with a typical

question density somewhere between 30 and 50 minutes.

MR. BENDER: Well, carry on. We will try to hold down the question density.

MR. HANAUER: I do not learn anything if you do not ask questions.

MR. BENDER: I know. But we are the absorbers, not you.

(Slide.)

MR. HANAUER: The next thing to do is to compare these numbers with the safety goal. This turns out to be extremely difficult to do, not suprisingly. The safety goal is in terms of coremelt and in terms of public risk. And so we have to get from the frequency or probability of vessel cracks, which is what we have been working on, to the probability that cores melt and the probability that the public is at risk.

I have expressed these as two unknown quantities X and Y, those being what you do with unknown quantities. The discussion in the report that you have has misled almost everybody, and I have therefore completely rewritten in the next edition of the report, which you will receive in the not-too-distant future.

X and Y are not known. They are both less than 1. X is the probability that the core melts if the vessel cracks. Not every vessel crack melts the core.

It depends upon the size of the crack compared to the size of the replenishing capability and on the location of the crack, whether the core drains or whether one can, in fact, continue to fill the vessel to some reasonable level.

And it depends also on the heat transfer characteristics of a core which is partly covered with water and which is told to some extent by the various loss-of-coolant accident experiments, but about which there is a great deal more to be known.

Now, what I have done is to write down how you compare this kind of calculation with the safety goal. You take the safety goal guideline of 10^{-4}. You do not want to spend it all on pressurized thermal shock. And so I took a tenth of it, an arbitrary fraction. And I said, we are okay with the safety goal if X times F is less than or equal to 10^{-5}.

Well, the numbers you get for F are in the range 10^{-6}, 10^{-5}, if you believe these calculations for a vessel, at 270.

(Slide.)

Now, for most of the vessels, the F is a very small number for vessels below 200, which is a lot of them. F is a very small number, and there is no safety goal problem at all.

But let us take a 270-degree vessel, and I have suggested to you that this line characterizes the population of 270-degree vessels, and maybe half of them -- well, they are in the range, very small to 10^{-5}, 10^{-6}; and some of them are much higher in an unknown proportion.

I have not done any calculation, but only drawn this curve and waved my arms.

(Slide.)

If F is in the 10^{-6} to 10^{-5} range, everything is just beautiful. For some fraction of 270-degree vessels, F will be in the 10^{-3} range. And for those fraction of the vessels, we probably do not meet the coremelt criteria.

Now, this fraction of vessels is fairly small. There is a nice intellectual problem here which I do not know how to address. Here is a vessel which is calculated to be 270 by our formula. The probability of its actual value is either on this curve or on some other curve that we do not know. I do not know how to put that into safety goal language.

But surely the fact that most such vessels are down in the 10^{-5} range has to be taken into account in looking at the safety goal. This is a question of application of the safety goal that deserves a lot of attention and has not had it. I do not know how to go from there.

Similarly, with public risk, it is simply that XF x Y, the probability that a lot of stuff gets out if you have one of these coremelts must be less than some number like 5×10^{-8}. Now, that number has a whole bunch of assumptions in it about meteorology and population and stuff that is some kind of an average number. Again, we do not know why for longitudinal cracks that produce orderly coremelts, Y is probably a pretty small number. At Indian Point Y is less than 10^{-2} for such things.

If you get the crack that goes all the way around and you get a jet-propelled top half of the reactor going up, we have a calculation that says there is just about enough stuff to restrain it, it maybe will and maybe will not come apart and fly up. And whether it has enough energy if it flies up to significantly damage the containment so a lot of stuff gets out is not clear because there is not a lot of stuff when this happens; the core melts forthwith but not instantly.

And we do not have analyses like this. This sequence has not been analyzed, but I can turn the problem around. I can say, if F is 10^{-5}, then coremelt is okay and risk will be okay if Y is less than

5×10^{-2}. And those sound like reasonable numbers, but I have no science behind them.

So the comparison with the safety goal is not very satisfactory. It seems like we are not wildly out of line, but we do not have enough science and enough calculations today to make a quantitative check. Why? Because we do not have PRAs in which vessel failures of this kind have been calculated in any significant detail.

MR. BENDER: And if you had, you could not believe them.

MR. HANAUER: They would have whatever degree of belief was appropriate. We are having some trouble believing the vessel crack calculations. They need more work. I do not know whether you believe vessel failure coremelt calculations or not. I have not seen any.

(Slide.)

Now, then in section 8.7 or thereabouts, we have spent a good bit of time on the uncertainties. By the way, section 8.7 has a few statements that are not exactly right, and they have been rewritten, too. But, in fact, there are -- I do not need to dwell on any today -- a very substantial number of uncertainties in all of this.

(Slide.)

Now, the conclusions we draw, let me now emphasize that these are the Staff's conclusions. And they are being submitted to Mr. Denton within a week, to Mr. Stello and to Mr. Dircks probably within a few weeks, to the Commission probably in November. So that what you hear is what Steve Hanauer and his colleagues think and not what the management of the NRC Staff thinks, and certainly not what the Commission thinks. And what the ACRS thinks would be a very important part about this decision process.

What we now think is that 270 is about right for the present, that the situation is that if 270 is about right, that there is no need for immediate action on any plants, but that within the next small number of years these methods must be refined; and that we will need plant-specific analyses on the top few plants in that list which are getting embrittled into the high 200s; and that what needs to be developed -- and this is the biggest hole in everything I have told you today -- what needs to be developed is given a plant-specific analysis, which is described in some detail in Chapter 9 of our report and which I have a viewgraph on, we then have to decide what is acceptable when we get such a plant analysis.

And that, gentlemen, is the largest hole in our present work. We have not, since we have gone away from an RT NDT or a single-probability safety goal style, we have yet to develop the critical question of what is accepted as these plants continue to embrittle.

Now, it is my opinion that the plants in the high brittle range, in the high 200s, should initiate steps to slow down their embrittlement and steps to decrease the risk from the highest risk contributors which, if it continues to be the small-break LOCA, is perhaps most easily done by warming up the emergency core cooling water. It is rather easy to get 50 degrees out of that with essentially no problems as far as we know.

And finally, since we did not pick some design-basis accidents and some evaluation models, it is not clear that some of our older regulations are compatible with what I have told you today and what we propose to do. This is the question that the legal beagles are now working on.

Now, what I wanted to do is to talk some more about the plant-specific analysis. And I wish I knew how to talk some more about the acceptance criteria. But I really do not.

(Slide.)

The plant-specific analysis, we believe, should include these factors. And you will observe, among other things, the operating procedures and training program improvements which need to be done in the fairly short term in some plants, but which need to have a major improvement in connection with the I.C.1 TMI-procedure-based improvement.

What we think we should from these plants is a much better look at transients and at the vessel. You do a better look at transients by doing plant-specific and, in a way, that encompasses some of these questions that have been discussed today, a study of the overcooling transients which dominate the risk in that particular plant, using the experience of that plant and similar plants as well as the generic experience of all plants, which is all we have done, using the plant configuration and the plant sizes and the plant behavior of that particular plant.

We propose this to be done not in 100 plants, but in a small number of plants that have brittle vessels. Similarly, we propose that in such plants, as good a look as can be obtained of the properties of that vessel and of the present state of that particular vessel, should be obtained.

Now, you have scheduled a discussion of nondestructive testing. So I will only remark that the code-required in-service inspection is not well directed toward pressurized thermal shock cracks, but that methods are under development and have been developed that can do a lot better job of detecting pressurized thermal shock-type cracks, and that in these vessels there is an obvious place for application of these.

Furthermore, what one has to do then is to figure out what to do with the results. When we do these probability curves, in general, we have a crack probability distribution. If you do a good in-service inspection and you find this and that or nothing, then this ought to change for that vessel the crack probability distribution. That is an area where we have not done anything except wave our arms, but clearly it is something that we should do.

And then we need to consider flux reduction plant modifications. Your favorite, automatic depressurization, is here, Mr. Chairman. And operating. And finally, for the most brittle vessels, in situ annealing and the basis for continued operation must be considered.

We have for annealing an EPRI report. There is a thin report on the feasibility of annealing, and I just got a wheelbarrow-size report with 2800 pages on the metallurgical data that underly the annealing process. And so a fair amount is known about what you can do with annealing, and I will suggest that very little is known on whether annealing is actually a practical possibility. But this report is available to you as well as to me.

(Slide.)

In the longer term, we think that some generic things ought to be done, too -- the longer-term, meaning not this summer and this fall -- but we think that this procedure program that we have discussed at some length and the training that goes with it are really very important and procedures are not now well cognizant of pressurized thermal shock and need to be in a connected and integrated way so that we do not foul up the plants in some other way in trying to cope with pressurized thermal shock.

We think that the generic analyses that I presented here have a very large number of holes in it. The number of holes is somewhat larger than when I brought it in this morning at 9:00 o'clock, and that these topics need to be investigated and we need to have a better generic idea of what is going on, particularly for the Bs and the Cs, because our present analyses is essentially a Westinghouse analysis.

We need to improve the in-service inspection. We need to decrease the leakage neutron flux, and we need to do these plant-specific analyses.

Now, that is our proposed program. And to the extent that a single discussion of this length can cover it, I have done my best to try to connect it. It should be clear that this is a problem that embraces many different disciplines.

And one of the most difficult things in coping with it over the past year has been to make sure all of the disciplines were involved and to avoid going off and working very hard on one particular piece to the detriment of other parts.

I give you one example: the heat transfer coefficient is obviously an important point both as regards how we do our thermal hydraulic analysis and as regards how we do our vessel deterministic and probabilistic failure analysis.

At some point, the heat transfer coefficient is known well enough so that the other uncertainties will dominate, and we ought not to do for pressurized thermal shock a lot more work than that in the heat transfer coefficient. If you let yourself think too much along these lines, you induce paralysis. And so we have tried to walk the curve between this.

This is the end of my prepared discussion. I will be glad to answer any other questions, and call on my colleagues for things I do not know.

MR. BENDER: Well, let me say I will entertain a couple of questions and then we will break for lunch and decide whether we want more when we come back.

Does anyone want to pose a question?

(Pause.)

Steve, an observation and not a question. It seems to me that you have come a long way in developing the screening concept, assuming you get the minor disagreements straightened out. It does seem to me that the second step, which requires plants to respond to the screening criteria when a plant gets to the point where it requires some action, is probably not the best way to deal with it.

MR. HANAUER: Well, we have proposed that plants for which the screening criterion is predicted in 3 years be treated so that there is plenty of time to decide what to do about the plants.

MR. BENDER: I see. Okay.

Bill.

MR. BOCK: It seems that the important thing you are really looking for our opinion on right now is do we agree with the screening criteria, or, if not, what else should we do? And whether or not we agree with the screening criteria depends to a considerable extent on what happens when you reach it.

If it simply becomes a level at which point the plant should exhibit some increased concern over the PTS problem, we might have one opinion. But if it were something that when you reached it, you would trigger off a chain of events which would eventually require you to anneal the vessel or do some other drastic modification, our opinion about that might be different.

I appreciate the problems in establishing a position. But can you give us anything better to go on as to what you do when you reach the screening limit?

MR. HANAUER: Let me describe our present status, as the start of an answer, and to say that I think my own opinion is where we need your advice, you, of course, give whatever advice you think appropriate.

I would suggest that we need your advice in the following areas: First of all, is our conclusion that no immediate action is needed acceptable? Do you believe it is correct? You did 6 months ago. You wrote the Commission in that vein. And have we learned anything different, or is this still the correct conclusion in the committee's opinion? That is kind of a threshold thing, that we really do solicit advice on.

Secondly, is the screening scheme appropriate, and is the screening value about right? And, of course, Mr. Bock is correct. The question that follows immediately is: is our proposal, to the extent that it is delineated, for what plants should do when they trigger the screening criterion appropriate?

Now, where we really need some advice and where we have not made a proposal is what should be the criteria for operation or shutdown of a reactor whose vessel is substantially embrittled? And to pick a sample, I will simply say that a vessel which reaches, according to our formula, 270 degrees must make a showing that operation of this plant is acceptable.

Now, there are two general categories of things. One is a general sharpening of the pencil to show that, in fact, if you really look hard at this vessel, it is not a 270 degrees the way you think it is at some lower value. That is one category of responses.

Another category of responses is that because of the way this plant is configured, even though this vessel is at 270 degrees in this conservative way, that the risk of pressurized thermal shock is acceptably low. That is also a sharpening of the pencil, but in a somewhat different area.

The third thing to do is to improve the safety of the plant by procedural and hardware changes that make the risk acceptable.

Now, what I have skirted all around is what is the acceptability of a plant with a substantially embrittled vessel? About this we really are only at the beginning of our consideration.

You are quite right, Mr. Bock, we have not adequately done this, because we were working along toward an acceptability criterion that turned into a screening criterion about 2 or 3 months ago.

Any advice the subcomittee wishes to give us would be very welcome. I will tell you some early ideas I have on the subject. If, as I expect, the Commission proceeds with some ideas about safety goals and value impact justifications for backfits, we will be, when these things come due in a small number of years, working in probability space. And we will be trying to develop perhaps the first probabilistic acceptance criteria, or perhaps we will use some fairlv generic based upon a few plant-specifics to develop deterministic acceptance criteria.

I hope and expect that we will not go back to a set of design basis accidents, highly conservative evaluation models and highly fanciful acceptance criteria, the way we have now in emergency core cooling. That is not an answer. I don't a good answer.

MR. SHEWMON: Let me make on comment on this. I feel rather comfortable with your 270 and 300 numbers, not because I feel I know what is going to happen there or exactly what you should do, but I feel that by the time the vessels get up there it is fully appropriate that people be looking very seriously at it. And I'm sure the utilities will look at it maybe before they even get there and decide that they would like to slow down. And I think I feel relatively comfortable with it.

MR. BENDER: I think there's a point to be made here that goes like this. Somehow or another people think that the NRC has to establish some regulatory action process associated with the point where the screening criteria require some action. The owners own the plants and they have a responsibility, and my inclination is to say that the screening criteria probably show -- if you accept the evidence so far -- that there is time to do something.

But the initiative is clearly in the owners' camp, and every owner that might find some vulnerability to this thing ought to be thinking about his strategy. Why should the NRC be inventing a strategy for him?

Can we break for lunch on that note? And we will reconvene at 1:25.

(Whereupon, at 12:25 p.m., the meeting was recessed for lunch, to reconvene at 1:25 p.m. the same day.)

AFTERNOON SESSION

(1:30 p.m.)

MR. BENDER: Let's reconvene. I think before proceeding with the presentation on PNL, I would like to find out if there is anything else on Dr. Hanauer's presentation, and if so, we will cover them now. Does anyone have any additional questions to pose to Steve?

(No response.)

If not, let me --

MR. ZUDANS: Mr. Chairman, I would like to raise a question. I'm sure there's no answer for it. Has anyone during this year in the process attempted to precisely identify where concerns exist in this process? You know, you use certain linear fracture mechanics analysis and they're associated with some form of surface. Temperatures were used, a fuel coefficient was used, a whole slew of things.

MR. HANAUER: There have been various lists. There is one in Section 8.7 of the report. I have no big, long list and we have no prioritized list.

MR. SHEWMON: There has been a sensitivity study.

MR. ZUDANS: We, of course, mentioned here this is a concern and that is a concern, but a comprehensive list of everyplace where the whole group thinks that the issues result in a conservative factor. That would be interesting to see.

MR. HANAUER: It would, indeed. There are, of course, some non-conservative factors which have to be included.

MR. BENDER: Let us leave that as food for thought. I would like to suggest that the working group think during the next hour or so about how it views the proposed screening criteria. Because one of the things I think we will have to respond to is whether we think this approach is a good one.

And secondly, I suggest we ought to think about what other kind of recommendations we might make to the Regulatory Commission, reminding ourselves of the original recommendation or the original request which Chairman Palladino made to us, which was to provide any input we could to the staff in time so they could be included in their proposed regulatory effort which would be presented to the Commission. So I will leave you with that opportunity to think.

MR. AXTMANN: I do have a question for Dr. Hanauer. How far has the staff gotten into the flux management program?

MR. HANAUER: We studied it quite a bit. We have had a contractor do some work for us on it. There is a short summary of this in the report and a longer summary of this in one of the appendices, and there are some technical reports if you would like to see them.

MR. AXTMANN: Have the economic aspects been explored very much?

MR. HANAUER: Well, we have done very little on that except to write down what people have told us. We don't feel that is really within our purview.

MR. BENDER: I think the next item on the agenda was a presentation by PNL on their review of what is known about the pressurized thermal shock issue. Mr. Peterson will be the initial speaker, and I will leave it up to you to introduce the rest of your gang.

MR. PEDERSON: Steve is a very tough act to follow, so I brought along quite a bit of help.

(Slide.)

I am Les Peterson and I will start out with a summary of NUREG-2837, which is the PNL's technical review of the PTS issues, which has now been distributed and only initially submitted in June, and it was issued by the NRC in July. I will also talk briefly about PTS screening criteria, and then Shaw Bian will talk about events in the thermal and the thermal hydraulic issues. Ed Simonen will talk about material properties. Fred Simonen will talk about fracture mechanics issues, and he also will speak some about the actual NDE or in-service inspection and what gains are possible through that. And then on the end there, Tom Taylor will talk about the NDE methodology and pplications to in-service inspection.

(Slide.)

We were asked by the NRC to review the PTS and particularly in the near term to come up with a regulatory position for them. And the source of our information was the 50, 100 and 150-day responses from the licensees, the owners' group submittals and supporting ongoing research at the various laboratories and EPRI and also, consultants that we had onboard that reviewed the material and made comments and suggestions. And I wrote the consultants on this copy and the areas that they were particularly involved with.

(Slide.)

MR. BENDER: Mr. Peterson, it would help a little bit if when you're talking if you'd stand back from the slide.

MR. PEDERSON: In our NUREG-2837, there were three main categories of recommendations that we came up with. Initially, we found from our judgment that there was an immediate problem with any of the present plants, but from the reviews that were made by the NRC people and also some of PNL people at the plants reviewing procedures and training and so forth, it seemed quite obvious that there were upgrades in procedures and training and some control room instrumentation that was needed on a near to longer-term basis.

Our report gets into more details there on our recommendations for the near term and for the intermediate term and the longer-term periods as far as which procedures and which instruments and so forth should be reviewed on a site-specific basis for upgrading.

Secondly, from the analyses that were submitted on the 150-day, they were quite different, and some of them had some deficiencies as far as being accurate or totally acceptable or covering the total problem. And so, we developed some criteria to be used for figuring effective full power years remaining before further corrective actions would be required.

And I think these might be particularly useful in the site-specific analysis that will be required when various reactors are triggered by the screening criteria.

And lastly, we have recommended improved NDE techniques for in-service inspection, and Tom Taylor, of course, will talk about that further.

(Slide.)

Now, the screening criterion, of course, as Steve mentioned this morning, was for axial welds, 270 degrees F, and for the circumferential welds, 300 degrees F, and we searched quite diligently for a basis for that. Initially it started out looking at 10^{-2} for frequency because there was -- looking at a number of them, why, when you use that you get a probability of crack extension of about 10^{-6}.

That has changed some, and now the basis of that 270 is a little larger than 10^{-2}. But the crack extension at 10^{-6}, as Steve mentioned, is very difficult. The probabilities are not something that you would want to rely on for regulatory policy, but it is probably the best we have, and the safety goals that are in the comment stage, of course, can be related to about a 10^{-5} or $^{-6}$.

Also, the report on the integrity of reactor vessels for lightwater reactors that the ACRS issued in January of 1974 also used a 10^{-5} and 10^{-6} value, so that seems like a pretty good base. But we have to learn how to really apply it. And I have got to admit that we are some distance from that yet.

(Slide.)

This is one of the earlier curves that Steve showed earlier, and it just shows how at 270 you have a frequency of six or seven to the 10^{-2} or $^{-3}$.

(Slide.)

Getting into this particular slide, which I think may have changed a little but I think it is basically the same slide, and I apologize for the reproduction that didn't come out very good. But as we were told not to do -- and I recognize why we shouldn't, because of not having a real good or not having a good handle on these curves, and I'm sure they're going to shift a lot and they will shift even more when people look at it on a site-specific basis.

But looking at the 270, you come out to about two times 10^{-4}, which even with a couple of orders of magnitude of unknowns or uncertainty as far as where that is, you still are at least a little hesitant to see it up that high in probability. That is, of course, the NRC staff's curves.

(Slide.)

On Westinghouse curves, it looks better, but it is still -- you still have the probability in the 10^{-5} and 10^{-6} range.

(Slide.)

There was one other curve that was in an earlier rendition that now hasn't been used, but I think it says the same thing. This is operating history and the same type of probabilistic analysis, and using that, we would get about 10^{-4} or pretty close to what the NRC staff's calculations are on a PRA basis.

(Slide.)

So what we asked ourselves is -- excuse me, I'm missing one slide. I guess you will have to look on your handout. The thing we asked was how was probability of a crack of 10^{-6} satisfied, considering those type of data points that we get. And in the NRC staff evaluation of September 13, on page 8.6, it says, and I will read it, "PTS event sequences leading to reactor vessel failure have overall frequency F per reactor year. Figures 8.2 and 8.3 which were the two curves provide an estimate of F. A plant evaluated as described in Sections 5 or 9 and Appendix E to be at the 270 degrees screening criteria is likely to have a true RT NDT of 150 to 270. Now, that is based upon the two sigma.

Then, for a mean of 210 -- in other words, a mean reference temperature of those reactors would then be 210, and of course, the frequency would be about 10 per reactor year of the NRC curve, and much smaller on the Westinghouse or owners group curve.

(Slide.)

Looking at those again, we are using the mean value of 210, and you see you would be down here in the -5 to -10 frequency for a crack extension on the staff PRA curve, by using the Westinghouse curve. Of course, you are off of the scale. In other words, quite a bit lower than 10^{-6}, or here again, using the curve from the operating history, you again are in the 10^{-6}, 10 range.

So I guess I look at this and I think for a screening criterion, the PNL people felt that the 270 was a good value considering the two sigma conservatism that was talked about. And primarily because of the probability and all of a failure being that low, which is in the range you would hope to have it.

Also, you need some criterion for the licensees to come back and show that their procedures, training design fixes and so forth have improved the safety factor to really gain on their reference temperature. So I suggest that that probably is the base that they need to work to.

Shaw Bian will now talk about the events in thermal hydraulics.

MR. ZUDANS: Could I ask a question? I still have some question relative to the frequency of operating history. As I understand, there were three cases in B&W and four to five cases in Westinghouse.

What would those statistics look like if you did not mix the cases? It makes a lot of sense not to mix them because they are completely different reactors. Would the B&W case be much more serious in terms of the statistics?

MR. PEDERSON: It would certain vary the statistics, but of course, there just isn't enough information to really do that and have enough left for a generic basis.

MR. ZUDANS: In either case, eight is not enough for a generic basis and three is worse, and five is just as bad. But you see, if you mix apples and oranges, your overall picture might look better than it really is. Say for a class of reactors such as B&W, what would that curve look like for the B&Ws?

MR. PEDERSON: I agree it certainly would be different, and I am sure on a plant-specific basis, this point will be treated by the licensees. In B&W's case I am sure they will point out that there have been a lot of retrofitting where they have made design fractions to the instrumentation system which was quite often to blame. And that things are improving.

MR. ZUDANS: I see. That may have invalidated the entire portion of the B&W effort.

MR. PEDERSON: It very well could be. There is certainly a lot, as Steve mentions, that he wouldn't want to use any of that probability for regulatory, and a lot of this is the very reason he wouldn't want to.

On the other hand, it may be the best we have at this time.

MR. ZUDANS: It's really not right to say the best we have. I think it is right to say we have nothing at this time in terms of basis for statistics, but it does not stop us from using the statistics to get the sensitivities, and that is all right.

MR. PEDERSON: Well, we need to certainly improve on those statistics that we have.

MR. BENDER: We don't want to do it by having more PTS events. We need to work on the methodology. Can we proceed with the rest of the presentation?

MR. BIAN: My name is Shaw Bian from PNL. I am more involved in thermal hydraulics than the events, but I'm going to cover the area that is mainly done by another person on the initiating events in the area of PRA, as we are talking about just in the last few seconds in the original NRC report.

They derive the probability of, say, a steam line break or a small steam line break based on some extrapolated boundaries from the primary side breaks. What was done was that some data on the primary side part piping breaks were taken and then were extrapolated based upon how many times longer of the steam line pipe compared to the primary piping could come up with a new probability because the length of the pipe is longer and so forth.

And you may have a lot of uncertainty on that because of certain events and welds, and also, the flow rate of steam is much higher than the primary coolant. So based upon the straight linear extrapolation of the probabilities, there is quite a bit of uncertainty involved in that area.

So we said we think that some more work based upon the probability of the break locations, instead of how long the length of the pipe should be, and with that kind of approach the result probably is more acceptable.

And the second item on the initiating events is the operation reaction time. In the draft of the staff report there was a discussion about the time allowed for the operator to do certain actions, and we found that there was a lack of discussion on the minimum time required for the operator to take certain actions.

For example, from the ANSI standard, N660, we found that general criteria for the operator action is that a minimum of six minutes should be allowed for the operator to start any action and then for each distinctive operation of the control component, one more minute should be allowed. And in one of the submittals by B&W on the small steam line break, they assumed the operator to start to shut the aux feedwater in five minutes, and so this apparently indicates some kind of discrepancy in that area.

Next I want to get into the thermal hydraulic area. Right now, on the probabilistic risk analysis the dominant scenario is the small break LOCA with no flow. And we found the approach of using long-term probability for a two-inch to six-inch break. I think we can refine that by doing some more detailed analysis to bracket the exact size that the natural circulation will stop. And then, if we do that, it will depend upon a lot of parameters. So I think the scenario will be the exact size that the small break LOCA would cause the stagnant flow situation is really very much site-dependent or plant-dependent.

The reason for the more specific analysis on that is with that, we probably can refine the probability that certain breaks will occur, say, for example, a two-inch or a three-inch. Apparently, if we look at -- I talked to one of the persons at PNL and the comment we have is that a two-inch break to six-inch break probably has an order of magnitude difference on the probability of occurrence. So that will definitely affect your curve of the failure probability, and the curve that Mr. Peterson just showed, the dominating curve, is the small break LOCA.

MR. BENDER: Are the natural circulation characteristics grossly different for different systems? That is, would the B&W plants be less likely to lose natural circulation than the Westinghouse plants, for example?

MR. BIAN: Without detailed analysis, I would say probably if you have the much higher -- I guess depending upon the different elevations of the two components, if the heat source is much higher you have a much higher driving source of natural circulation. I haven't done the parametric studies on different systems. I really cannot say.

But the recommendation is that we should look into more on the exact situation; namely, the break size. And different vendors have -- I mean even within one vendor they have different arrangements of the loop. The elevation difference of the heat sink and heat source. So it is really kind of plant-dependent.

MR. THEOFALOUS: Why do you think the elevation difference has anything to do with it? How do you lose natural circulation?

MR. BIAN: Okay. As Steve mentioned about the loss of inventory to a certain extent --

MR. THEOFALOUS: So what does that have to do with the difference in elevation?

MR. BIAN: Okay. That is the point. The point is when did the void form in the higher points.

MR. THEOFALOUS: You give me a break and I will find the time in the injection level where you will lose natural circulation.

MR. BIAN: But I think it is plant dependent.

MR. THEOFALOUS: It's plant dependent to the extent that maybe different plants have different injection rates, and therefore, the optimal size to reach that plateau would depend. Because really, you balance between what you get in and what you get out. But it doesn't have to do with elevations.

MR. BIAN: That is a different aspect. That will have an effect on natural circulation, but not in this case.

MR. THEOFALOUS: Do you have any feel for what kind of a spread in break sizes would be in the mid-range where it just about balances so the thing could stay up?

MR. BIAN: All we have done is a little research, and one-half inch is about the size. PNL doesn't know, I don't know, but we would like to know that. We do more work by ourselves or the owners group.

MR. ZUDANS: It is interesting that Steve's presentation and the present discussion indicates that there is a limiting transient for PDS, and that would be defined as a design basis transient the size of a small break LOCA that leads to natural circulation interaction. This is it.

All they have to do is analyze it in great detail and they would have all the information. Nothing worse can happen.

MR. THEOFALOUS: Not true, because the small break was analyzed in such a conservative way that that might not be limiting after you do it right. So something else might pop up.

MR. CATTON: It would probably fall right on top of the next line, which was below, which I think was the steam line break, if they take out some of the conservatism.

MR. ZUDANS: That means that the statement that this is a limiting transient is not necessarily correct.

MR. CATTON: It's not, that's right.

MR. BENDER: It's very easy to make that particular one go away.

MR. CATTON: You can pull it down until it falls on top of the next one.

MR. THEOFALOUS: Then you can pull the next one down. And I think really, we have to pull all of these down.

MR. CATTON: Theo, I think the lack of natural circulation probably has more conservatism in it.

MR. BENDER: Can we move on?

MR. BIAN: Okay. The next one is the local mixing in the downcomer, and we did some analysis based on the Levy model. And while I will not go into the detail, I will just show you.

(Slide.)

I will show you the assumption that the model got from. This model is basically assuming that you have instantaneous mixing of the cold water with the hot water at the no-flow situation, and we believe with this model we can come up with a result which is really close to the NRC result.

(Slide.)

And so we believe that is basicaly the approach they used. We got a final temperature of 136 degrees and they got 125, and the beta is .13 and theirs is .12.

So that leads to the next question: How good the approach is as far as realism is concerned. Of course, there are certain arguments either for or against that. One argument for that is that it is conservative because the way that we do the analysis we didn't allow for the hot water inside the downcomer, I mean below the weld's location, to mix with the cold water up above. And also, it didn't allow for the thermal shield energy to be released.

But on the other hand, if you look at the Criari data and also some analysis we did at PNL with our code, we found there's a thermal stratification in the cold leg, that the equation didn't allow for that, the Levy model.

By the way, we add the wall heating on that, too. So there is a no on that. Maybe the weld location has an even hotter temperature than the Levy model approach takes, if we allow for the cold water to settle down into the lower part of the cold leg and just flow towards the cold barrier, instead of the vessel side.

We don't know. There is certainly uncertainty on that, and I think continued 3-D analysis will probably help the situation to figure out exactly the phenomenon that is involved in it.

(Slide.)

The last one I would like to go through quickly is heating the ECC water, and I think it is fairly clear that it would be very effective if we have a stagnant flow situation, but it would be not effective, but it is not critical, if the natural circulation is maintained, because you have a continuous source of heating coming from the cold leg.

But again, the effects on the core cooling and also, for example, the active containment pressure, the effect on those things really still is not analyzed well enough or not analyzed at all.

MR. BENDER: Excuse me. Before you take that off, what do you envision as the problems of heating the ECCS? Are there any limitations that would have to be put on what could be done?

MR. BIAN: Now, if we are worried about using the ECC water for the no-flow situation, then we would think that the core cooling probably is not mainly caused by the temperature of the ECC water and it's really mainly caused by the suppression of the water level from the upper head, because of the steam forming and so forth, and it is not the temperature of the ECC water coming in.

MR. BENDER: If I wanted to set the ECC water at 150 degrees instead of at the nominal 60 that was used in the previous analysis, would that make the problem go away altogether?

MR. BIAN: At least it would bring the small break LOCA curve down, so that it would reduce the probability of failure to maybe 10^{6} or whatever the new number should be, instead of that dominating effect that we see right now.

MR. BENDER: And the only problem you see in raising the temperature would be that -- would be what? Would there be any problem except spending money?

MR. BIAN: There is a problem that we haven't analyzed. For example, the effect on maintaining active pressure of the containment. You have extra heat source there and that may cause some problems. We don't know. That has to be analyzed, really.

And two more comments that are not in the slide, that just came up this morning. One is that the argument on the cold leg temperature measurement against the downcomer temperature measurement, and we know in certain plants that the RDT locations are upstream of the injection location, and in that case the data will be in serious doubt of usage for the analysis. That's all I want to mention on that.

And the second is the fuel coefficient. We found that using a constant value probably is not as realistic, depending upon what value it is. If you used a 300 Btu per hour, it probably is not as realistic as using a variable value based upon natural convection for at least the stagnant flow situation, because in the latter part of the transient that's where you have a concern about low temperature.

Usually, the delta T is smaller, so that you have much lower H than the constant 300 value.

And that is the end of my presentation.

MR. SHEWMON: Thank you.

(Slide.)

MR. SIMONEN: I am Ed Simonen and my responsibillity on the PNL assessment of PTS is in the area of material properties. There are three areas which I would like to comment on this afternoon. One has to do with variability and the material property determination and the justification of using conservatisms. The second area deals with our support of the use of statistical-based trend curves and some comments on how we feel they ought to be used in the PTS evaluation. Lastly, we recommend some testing that could be done to enhance the development and application of the statistical trend curves.

(Slide.)

With regard to the variability in material properties and the conservatism that is assumed, we examined the issue with respect to what is accepted ASME code practice of using lower bound fracture values versus what is used in the PTS evaluation. What is used in the PTS, in addition to using the lower bound K-1R curve, are two contributions to variability. One is from measurement error and the other is from a generalized data base that is used in the development of the statistical trend curves.

With regard to the measurement error, there is an added contribution of conservatism that comes in due to the fact that property is determined three times rather than just one time, and it is -- like in the lower bound toughness value, there is measurement error that is incorporated in that lower bound.

But in PTS we have an indirect determination of the material property. We have to make measurements on the initial property that has its measurement error, and then the shift that also has a measurement error. So there seems to be a piling on of opportunity of putting in measurement errors that have nothing to do with variability of material property, of real variability in the pressure vessel material.

We estimate that this measurement error added conservatism is something less than 30 degrees, and this is based on, if one looks in the literature and finds how well one can determine Sharpey values and the RT NDT, it seems like the best one can do is to come within 10 degrees Fahrenheit.

The best experiments, where the material property is controlled as best one can, the error never goes down below 10 degrees. And if you add on the 10 degrees from the initial property and the 10 degree contribution from the shift and do that square root of the sum of the squares, it comes out to be, I think, 28 degrees.

So there is an added conservatism in that with regard to the generalized data base because the trend curve is based on a wide range of different types of material, different compositions, different radiation environment spectra, flux. There is a lot of uncertainty that comes into this data base.

My point would be that the actual vessel wall has those same kinds of uncertainties with regard to what was the temperature that the wall was radiated at, the flux, the spectra, errors in the assumed composition. So these -- this added conservatism really seems to be justified because the vessel wall itself is subject to that same variability.

So the end result is that one looks at the total conservatism between mean value and the upper and lower bound approach; there is something over 100 degrees Fahrenheit that is involved between those two estimates, and of that 100 degrees something less than 30 degrees seems to be not really justified on real material property variation.

This seems to be a minor contribution in the total analysis because these uncertainties are known with some degree of accuracy. There is this large substantial conservatism, but we do understand the uncertainties. We believe that the RT NDT is an appropriate criterion to focus on for identifying plants that are susceptible to PTS conditions.

The next comment having to do with conservatisms has to do with the NRC report, in which sigma values are used on the initial property welds, but not used for plates. It would be our recommendation to use -- to identify the appropriate population and uncertainty distribution for each type of material, and always included in the analysis in the welds there is a population density that is accepted by NRC and it is used for the plates.

I'm sure a similar distribution could be found with much less uncertainty, but feel that that uncertainty should be included the same way as it is for the welds, just for consistency in the analysis, to make the analysis less complicated.

MR. SHEWMON: Do the properties of weights enter into this consideration at all? Is that where the Guthrie data comes from or what?

MR. ED SIMONEN: It could. There are some plants in which plates could be limited. I wonder, with the flux distribution, that if plants start redistributing their core so that they redistribute the flux to the welds, that eventually the plate will be limiting.

I think what we are advocating here is a consistency in the treatment of material properties, to not make exceptions for this and that, so it is clearest what the policy is.

MR. IRWIN: This measurement, by which you are talking about a measurement error, is it a measurement of fracture toughness to yield strength or whatever?

MR. ED SIMONEN: I would say that I am particularly thinking of the EPRI round robin experiment, in which they took a plate and chopped up specimens from the same portion of the plate, sent it around to different laboratories and had a whole bunch of tests done on the same material, and then looked at these uncertainties laboratory to laboratory, instrument to instrument.

MR. IRWIN: I believe that was pre-CRAC Sharpy testing.

MR. ED SIMONEN: Right.

MR. IRWIN: That's not a very good comparison from the point of view of the type of measurements actually used in the NRC analysis for pressurized thermal shock calculations.

MR. ED SIMONEN: You think the values should be even less than what I'm saying?

MR. IRWIN: No. I think that you can't look at a whole bunch of small specimen data. You will always see a very large scatter simply because you pick the tiny specimens. As you get a bigger and bigger crack front, you will see less and less scatter, because the crack front will represent all possible variations to a greater degree.

What do you do about that?

MR. ED SIMONEN: Well, I guess you would have -- the measurement error in the Sharpy's is greater than it is for actual flux fracture toughness, which is relevant to the PTS issue, so that there is probably maybe a greater conservatism in here that really doesn't represent performance of the vessel wall, that the Sharpy's give more scatter than you would get with appropriate kind of fracture toughness of vessel.

MR. IRWIN: Whether or not you can represent the scatter in terms of degrees Fahrenheit is also a question. It depends upon the slope of the curve when you plot that measurement result against temperature. If it is perfectly flat, then your uncertainty in temperature might be 200 degrees.

MR. ED SIMONEN: Right. That comment had to deal with all of these uncertainties and the whole analysis of when Sharpy uncertainties are introduced.

Another comment with regard to this consistency on how to treat the uncertainty in initial property on the new plants, where the welds are characterized better than they have been in the past: It's a question of how will they be treated, with the same type of sigma uncertainty or will there be -- or will you take the new plant welds that are well characterized and say they are like the plates because they are well characterized.

This seems to be an opportunity for misunderstanding as to how NRC will determine these material properties for different examples. It would be good to have a clear, consistent way of treating these in all cases.

(Slide.)

A second area I would like to discuss had to do with our recommended use of the statistical trend curves. We do believe there are great benefits in the use of the statistical trend curve. It allows the owners to take credit for the low nickel that we would normally be able to assign.

We believe that the statistical trend curve should be used to describe all data and a statistical model ought to describe the whole data base satisfactorily, and that the reg guide upper cutoff should not be used, as it is presently proposed, that a statistical model should be identified that includes, that does not make it necessary to put the upper cutoff in the reg guide. And I think there are ways of doing it that George Guthrie is looking at. That I think in the end is what should be done.

Another problem with using the upper cutoff of the reg guide is, what happens when new data comes in that exceeds present reg guide line? If a new data point comes in 20 points above the present line, does that mean everything gets shifted up and then there is a dramatic change in the valuation of all of the plants?

With the statistical curves, an outlier that occurs like that simply gets incorporated in the whole statistical data base and there is no dramatic change in the evaluation of individual plants.

Also, I might point out limits to the statistical curves with regard to our present PTS evaluation, and pointing out that it is least conservative at the extreme values, namely high copper, nickel, and fluence, where copper is greater than .3 percent, nickel greater than .5 percent, and fluence greater than 5 times 10^{18}.

Of the 139 points in the data base with the statistical analysis, 7 satisfied those requirements, which represents 6 percent of the data base. Within our PNL or in the PTS evaluation, you look at the NRC list of plants and the first five plants on the list are in this category of 6 percent of the data base.

You must recognize that the uncertainty that is given from the statistical uncertainty is really much greater for these plants, because it is at the extreme edge of the population of the data. So that is a non-conservatism, if you would want to call it that.

Also, I might note that the reg guide is used for three of these top five plants as the upper cutoff. That is a serious issue, that many of the plants are affected by that issue by the upper cutoff of the reg guide.

(Slide.)

The last area I wopuld like to discuss has to do with identifying testing needs that would enhance the development and application of the trend curves with regard to a characterization of these radiated pressure seals other than Sharpy testing. In the 1960's, there was a rather ambitious effort to look at properties of vessel materials, namely at Oak Ridge to try to identify submicroscopic -- these mechanisms that are responsible for embrittlement.

Since that time there have been many new advanced characterization techniques that have been developed and that ought to be promoted to use these techniques to identify reasons why, for example, this nickel influences embrittlement the way it does. Does low nickel have a unique effect, perhaps, on saturation? Is there a limit to the high nickel effect and how it is supplied in the development of the trend curves?

MR. SHEWMON: Now, nickel tends to enhance the effective radiation? Is that the conclusion?

MR. ED SIMONEN: Right. It is a product of copper times nickel in the trend curve.

MR. SHEWMON: Are you close enough to this to know whether that same effect is found on the other side of the Atlantic? I have heard that there is some question over there, where they use a lot of higher nickel alloys?

MR. ED SIMONEN: I don't know. Perhaps George could feel free to comment.

MR. GUTHRIE: I think it is still pretty much up in the air.

MR. SHEWMON: Do you mean whether their data is different from ours or whether there is an effect of nickel at all?

MR. GUTHRIE: Combustion Engineering thinks that there is an effect of nickel.

MR. SHEWMON: And it's a deleterious effect?

MR. GUTHRIE: Westinghouse I think thinks there is an effect of nickel. People down in Santa Barbara working for EPRI think there's an effect of nickel. Russ Hawthorne thinks there is an effect of nickel, and almost everybody I talk to thinks that there is an interaction.

And they way they look at it mostly at this point, people in the United States, is that when nickel gets in there, if you're familiar with the copper-nickel phase diagram, nickel can join a copper cluster and the thing still maintains a copper-like character, or so you would guess from looking at the copper-nickel phase diagram.

Nickel is transition metal and it is magnetic, and as you add copper to it it is still a transition alloy and it is still magnetic. And you get up to 60 percent copper and it goes over to a copper-like material and it is paramagnetic, and finally in high copper it is diamagnetic. It is not a transition metal any more.

If the same thing happens in small clusters, nickel in small amounts joining a copper cluster would make the clusters more numerous or make them bigger, and still be copper-like in their character. If you try to get too much nickel in there with just a little bit of copper, it wouldn't work.

So what people think at this point is that nickel is detrimental, especially in a high-copper alloy, because the nickel can join the copper clusters.

MR. SHEWMON: In small amounts of nickel?

MR. GUTHRIE: In small amounts of nickel. But when you get an awful lot of nickel, so that the nickel can't join the copper clusters without driving them to a transition metal or a nickel-type character, then the additional nickel isn't going to do too much. And people think that nickel by itself, with no copper in there, really isn't all that bad a thing.

So that's the way it's looked at in the United States at the present time, or at least that is what I believe. And I have tried to put together models more recently that have this in them.

But I noticed in going over the printout sheets that I have with me that it doesn't seem to do a whole lot of good on some of the outliers. And I am beginning to suspect that in the high copper-nickel data, that it is just scattered and we just don't have very much of it.

And I agree with Ed, I would sure like to see somebody get some submicroscopic information on what the mechanisms are. And it is very hard to get any mechanistic information on these things. We would feel a lot more comfortable if we had purely mechanistic models that we really believed in, where the parameters had a physical basis for each parameter and where we could then go in and get a least squares adjusted value for each one of these real parameters where the parameters meant something.

But we're stuck with trying to put together mathematical models that fit the macroscopic data as we see it and which account for the macroscopic properties as we are aware of them.

MR. BENDER: Why don't we move on.

MR. ED SIMONEN: I guess I would comment that it took five to ten years to identify copper as being deleterious, and it took another five to ten years to identify nickel. That is kind of through empirical searches for effects, and that is what, 20 years worth of effort. And maybe some key understanding could direct these trend curves to get them on line a lot faster.

The last area I would like to comment is on trend curve application. We've talked about in situ characterization of vessels in our report. We've mentioned chemistry measurement on the outside of the vessel. There is also possibilities that in micro-hardness tests on the vessel, some types of information that you can get that reflect mechanical properties on a small scale of actually doing tests in situ on an irradiated vessel.

The last area of concern has to do with this: If vessels have to be annealed, there has to be a way of using the trend curve information to establish re-irradiation embrittlement guidelines, that it is not good enough just to use the two isolated surveillance specimens to establish the re-irradiation embrittlement, but one should use the trend curve data base.

That is the best knowledge that there is on the effect of radiation on the properties, and one ought to develop the right kinds of tests that allow one to know how the trend curve information will be used in the re-irradiation embrittlement of an annealed vessel.

MR. BENDER: Are there any questions?

(No response.)

MR. BENDER: Let's move on them.

ED SIMONEN: Next, my brother, Fred Simonen, will talk about fracture mechanics and probabilistic fracture.

(Slide.)

MR. FRED SIMONEN: I guess Ed is Mr. Materials today and I am Mr. Mechanics. I would just like to say a few words on my critique of the fracture mechanics issues and statistical rather than probabilistic fracture mechanics. I guess there are really two essential questions.

(Slide.)

MR. FRED SIMONEN: One is, are the conservatisms in these anlyses appropriate, and the other is, what is the significance of these probabilistic fracture mechanics analyses and how do they relate to the more conventional deterministic analyses?

(Slide.)

MR. FRED SIMONEN: I have tried to sort through some of these questions, and I guess I view it in the way engineers do. Stress analysis, structural integrity analysis. They tend to introduce conservatisms in perhaps three different ways. One is placing bounding values on the input parameters. In this PTS example, talking about putting -- assuming flaw sizes and toughness, we are taking minimum toughness to overbound toughness curves and lower bound and upper bound shift curves and that sort of thing, and the important thing is, these types of conservatisms can be quantified because you have got a data base to work with, and this is addressed in the NRC work through the probabilistic fracture mechanics.

There is another class of conservatisms I see as just analytical or modeling assumptions, and in this PTS work. For example, these things are like flaw shape. Is it a short flaw, long flaw? Those things aren't quantified. Clad effects. They are either -- it could be accluded and warm prestress. The conservatisms are not readily quantified, and these are not addressed by the probabilistic fracture mechanics.

And as engineers usually perform these, they usually attempt to take these assumptions, or you may make a few non-conservative assumptions, but you always want to make sure you kind of balance it against something you are sure is an overriding conservatism. If you leave something out, make sure you put some other conservatism that is much greater than what you leave out.

I guess a final safety factor or conservative simple safety factor which is, if you look at design and pressures according to the ASME code, we do have safety factors from two to three, and in some cases lower, and essentially what these allow for is factors which you really can't quite include in your model, kind of an unknown, unknowns.

(Slide.)

MR. FRED SIMONEN: Okay, what I have done on this slide is to try to list what I feel some of the unquantified conservatisms are in the NRC analysis. Essentially, these are conservatisms that aren't really reflected in the probabilistic fracture mechanics. They are not quantified, so there may be an additional lower failure probability in my view due to these factors.

I have listed the various items on this column. I looked at the various models. I had what the NRC staff fracture mechanics has done. I have listed what the ASME code would give you as guidance. And in our report we went through quite an effort to try to see just how would the ASME code dictate a PTS evaluation. The code really kind of skirts around the issue, so you kind of have to imply what the code would tell you, and I think the reason why you look at the code, this was a set of rules that was written long before this PTS concern arose. So it was, I guess, a set of rules written by the industry when they were looking to justify rules to the public, and showing that these are in fact conservative rules rather than trying to justify the use of a particular vessel, and then I have listed some of the industry's responses.

The first item, the safety factor, none of the PTS evaluations have put any kind of engineering type or code type safety factors. The code is very vague on this issue. I would read in the code a factor maybe of about the square root of two, about 1.4, 1.4 for the faulted type loads we are talking here. There are two other factors, clad thermal expansion and flaw length. The code does not require you to prove clad thermal expansion. You can just forget the clad is there.

The NRC staff has included this, and this, the values I have seen in the NRC report, I have read something like 17 percent increase in stress intensity factor. The NRC evaluation includes very long flaws, essentially infinitely long flaw where the ASME code and in the Appendix G Section 2 would say a six to one aspect ratio flaw, and the NRC codes would say something like, that would give about a 20 percent enhancement in K.

And essentially what I did can compare the code with the NRC approach. The NRC does not include a safety factor. They do have a couple of other minor conservatisms included that are not dictated by any provisions in the code. It is quite interesting. You take the 17 percent, take the 1.17 and multiply it by 1.2, and you get a factor of 1.404, which is very nearly close to the code, so I guess the conclusion is that the way NRC is doing their analyses essentially would give us very much the same result, if you follow the code.

MR. ZUDANS: The 17 percent is not a conservatism. It is a reality. The 20 percent, yes.

MR. FRED SIMONEN: Okay. Well, I guess there is other conservatism. I look at just what is your flaw description. You are assuming the flaw extends through the clad.

MR. ZUDANS: Yes, that exercise, although it works out nice, it is not reality. Seventeen percent is a real thing. You have the clad, and it is going to expand thermally, and it is going to change the stresses.

MR. FRED SIMONEN: That is given that you have a flaw of two that extends from the base metal through the clad. I agree.

MR. BENDER: Let's move along. We are quite a bit behind schedule.

MR. FRED SIMONEN: There are a number of other conservatisms I would mention here. There is the vessel life. The flaw description has been based upon flaw size description essentially. There are flaws that were volumetric flaws in the weld. I guess all of the analysis, to take a worst case assumption, that this flaw is located at the vessel ID, and it is oriented normal to maximum stresses, and extends through the clad. The NRC did not include warm prestress. I believe that there are many of these transients where warm prestress will be a factor, and this is not quantified in the probabilistic work. I guess all of the analyses did consider crack arrest. The code does allow crack arrest, although I think it was put in there in the context of a large break LOCA, and applying it to PTS, we would say that you might want to apply it in a somewhat more conservative manner than was written into the code.

And I guess the other factor is this question of suppression of crack growth by tough clad. None of the analyses have included that, but there is work at Oak Ridge that will be looking at just that factor, and it may show that this is a rather conservative assumption as far as preventing crack growth lengthwise.

(Slide.)

MR. FRED SIMONEN: I guess our conclusion is that essentially the NRC staff analyses are essentially consistent with the conservatisms of the ASME code. The probabilistic fracture mechanics does quantify many of the conservatisms in deterministic analysis, but there are many conservatisms that simply are not quantified in this, and the failure probability does not reflect these effects. I guess in looking at the fracture mechanics we also feel that perhaps the greatest uncertainty in all of these analyses is the size and nature of the flaw. The flaw size distributions are, I think, based upon some very limited data.

MR. ZUDANS: How much conservatism do you think there is in the fact that all of the stress calculations are based upon a two-dimensional model rather than a three-dimensional, and on the fact that it is assumed that a whole vessel is uniformly cooled down rather than just a limited range in a downcomer? Do you have a feel for that?

MR. FRED SIMONEN: I think on initiation perhaps not too much. If you note, most of the crack arrest analyses simply, as they are done in the simple 1-D models, simply do not show a big effect of crack arrest in PTS type transients, and I would think that a three-dimensional analysis that would reflect the fact that the embrittlement on the worst, there is a gradation embrittlement down the length of the region. As the crack grows long, it will tend to run into tougher material. Some of the cooling from the downcomer may be very severe on one part of the weld, but may be much less on another part.

Perhaps if you were to go in and look at some of these three-dimensional effects, the fracture mechanics analysis may predict arrest in many more situations than they are now. But within the simplistic analyses, you don't seem to predict arrest for very many of these transients when there is pressure, particularly.

MR. BENDER: Is that it, Mr. Simonen? Are you about finished?

MR. FRED SIMONEN: I have got some other quick things, just kind of leading into the discussion on inspection.

(Slide.)

MR. FRED SIMONEN: We did some simple calculations to try to estimate what will be the benefit of inspection on vessel reliability. The method, what we did was use some of PNL flaw detection estimates.

These were used to modify NRC staff estimates of failure probability, and when the results essentially predict a failure in decreased probability and corresponding allowable increase in RT NDT.

(Slide.)

MR. FRED SIMONEN: Just to illustrate, essentially you have seen curves like this this morning. These are a replot of some of Jack Strosnider's data. All this shows is that as you increase RT NDT by some amount, the probabilistic analyses show some corresponding increase in failure probability. Factor ten and failure probability correspond to about a 20 degree difference in RT NDT.

(Slide.)

MR. FRED SIMONEN: What we have done, then, is taken -- okay, there is a vu-graph in your package that kind of illustrates some of the details of the calculation. I will skip that in the interest of time here, but what we have done is taken flaw detection estimates that Tom Taylor will be talking about in just a minute. Essentially what they show here is, using an approved inspection technique, this is something above and beyond the type of requirements that are now used as a typical ASME code type inspection. The estimate showed that the detection probability is quite dependent upon clad and surface finish. As you go to smoother, on the upper end, the best detection capabilities for smooth strip clad, about 95 percent flaw detection for flaws greater than a quarter of an inch, and on the worst end for manual unground clad it is down to a level of about 50 percent for flaws even as big as one-half to one inch.

Okay, going through the calculations, this is what we predicted on what is the benefit of in-service inspection. The one column is simply the predicted factor of improvement in reliability, and these range from like 16 to 32 on the best clad conditions and down to something, oh, three to five on the very rough clad, and a corresponding increase in RT NDT allowable increase.

As I say, these are just rough estimates at this point, and they are intended only to say, is there some real benefit in doing a good in-service inspection. I would not use these at this time for any kind of licensing decision. You would have to do much more detailed evaluation. I guess what the trend shows is that at most you can get maybe a 30-degree benefit on allowable increase in RT NDT for the best conditions. The worst condition is, it may be down as low as ten degrees.

(Slide.)

MR. FRED SIMONEN: So I guess our conclusion is that improved in-service inspection can justify an increase in allowable NDT, and that under the best conditions, it can be increased up to about 30 degrees Fahrenheit, or even under the worst conditions you can justify increases up to maybe only about ten degrees Fahrenheit.

Tom Taylor is next, and will cover just what is behind some of these estimates of flaw detection capability.

MR. TAYLOR: My name is Tom Taylor, from PNL, and as Fred has just said, I will discuss the nondestructive testing techniques that are currently used or are currently available for use in pressurized thermal shock.

(Slide.)

MR. TAYLOR: This vu-graph summarizes the non-destructive testing techniques that are currently proposed or used in the field. This particular technique of dual transducer L-wave techniques was developed by the Germans about ten years ago specifically for interrogating the base metal underneath clad of the reactor vessel. A focus transducer technique has been recently developed by the French, likewise for interrogating other clad crack techniques. These are fabrication defects that this particular technique has been developed for.

In the United States, some people have proposed using a single transducer, either an L-wave or a sheer wave at a relatively high angle and using a pulse echo or an emersion technique, and another particular vendor has proposed using a full V technique where sound is bounced off the OD surface of the vessel. This is the ID clad surface, and then trying to detect cracks on the ID surface.

MR. AXTMANN: What is it bouncing off of?

MR. TAYLOR: It is bouncing off the OD surface. The sound is introduced on the ID clad surface of the vessel. It penetrates through to the OD surface and is bounced off at a 45-degree angle, and it is supposed to detect flaws in the clad surface here. To date, our evaluations have shown that this technique has not been evaluated. This technique has, and the subsequent slides are based upon an evaluation of this particular technique that was developed by the Germans several years ago.

(Slide.)

MR. TAYLOR: The dual probe for the crack detection involves sending a sound beam in with one element and receiving it with another. Both elements are canted at an angle and cross just underneath the clad surface. This helps near surface resolution under clad resolution, and this shows you a schematic diagram of the sound beam directivity pattern of the dual unit transducer.

MR. SHEWMON: One of the elements of this is that one is a sender and the other is a receiver?

MR. TAYLOR: That is correct, and they are canted at an angle, so that the sound beam would be focused underneath the surface. Any further questions?

(No response.)

(Slide.)

MR. TAYLOR: As Fred has shown earlier, since this is a technique and we know it works, how well does it work? Well, on an internal round robin test at Batelle and with some flaw signal amplitude measurements on various blocks made available to us through EPRI and our own that have been fabricated, these are our estimates of probability of detection for a nice strip ground perpendicular and with the sound beam going both perpendicular and parallel to the direction of the clad.

We estimate that there is a 95 percent probability of detecting cracks that are a quarter of an inch in through wall depth and greater.

MR. SHEWMON: Are these tight cracks you are looking at?

MR. TAYLOR: These are thermal fatigue type cracks. The data is based upon thermal fatigue type cracks and some hydrogen cracking.

MR. SHEWMON: Are they put under any compression when you are looking at them? Or are they by their origin under compression?

MR. TAYLOR: The only compression would be any compression resulting from cladding. The way the blocks were fabricated was to induce a thermal fatigue crack and then clad over the thermal fatigue crack. As you can see, as the surface roughness, which is what this illustrates, increases, our probability of detection decreases considerably.

MR. BENDER: The vessels that we are concerned about now, do they correspond to those at the bottom of the list, or at the middle of the list, or at the top of the list?

MR. TAYLOR: That is a very good question, and in large part unknown. What we do know today is that all vessels have to undergo a surface examination so undoubtedly between here and up there have been some kind of surface condition done to them, and Mr. Bender, exactly how much is not known at this point, and it would be incumbent upon the utility to determine that.

MR. SHEWMON: Do you mean Robinson won't tell you? You haven't gone to the trouble of asking Westinghouse? Or what?

MR. TAYLOR: I mean that often times the utility itself doesn't know what the condition of its vessel is.

MR. SHEWMON: They don't know how it was fabrictaed? Nobody knows any more?

MR. TAYLOR: They know how it was fabricated. They do not know or have not taken pictures or documented what the inside condition is like.

MR. SHEWMON: Do you know how the Robinson vessel was fabricated or how any of the vessels in question were fabricated?

MR. TAYLOR: Do I personally know how they were fabricated? They were fabricated to Section 3 rules.

MR. SHEWMON: When you learn which one of those is, you might come back and tell us some day.

MR. TAYLOR: If the information were made available to me, I could tell you.

MR. BENDER: I think the staff needs to get a better story on this, and I would just suggest that the story is not very well known right now. Go ahead.

MR. TAYLOR: I have also done some studies to show the relative sensitivity of various calibration techniques

(Slide.)

MR. TAYLOR: In this particular slide, the signal response of a notch or a thermal fatigue crack is plotted as its depth through wall and the response of a three millimeter FBH is plotted here. The response of a Section 11 2 percent notch, which is the current American standard calibration notch, is plotted here. The response of thermal fatigue cracks are plotted here, and the response of through clad notches are plotted here.

As you can see, the current Section 11 2 percent notch is used in amplitude, is much above or is considerably above the thermal fatigue cracks. If this were used as a calibration reflector, one could have difficulty in detecting thermal fatigue cracks unless one adds gain or goes at some percentage level below the distance amplitude curve achieved by the use of this.

MR. SHEWMO : Do you know what the Germans were looking for when they developed this technique and what standards they used?

MR. TAYLOR: Yes, the Germans used not a three millimeter but a two millimeter flat bottom hole, and they were looking specifically for under clad cracks as a result of fabrication defects.

MR. SHEWMON: So you are saying there is a substantial difference between the ASME notch which the regulatory people in this country accept and what the Germans use as a standard, and they feel reflects what they choose to inspect to? Is that right?

MR. TAYLOR: That is correct, yes. I might also add that currently there are no specific code requirements for detection of under clad cracks. It was only intimated in the current vessel Reg. Guide that the cracks near the inner surface are important. Exactly what size one is supposed to detect is not --

MR. SHEWMON: That is progress, you have got to admit.

MR. TAYLOR: Slight progress.

MR. BOCK: Can you tell us anything about ultrasonic imaging technology which is in use in other industries, and whether it has been investigated for reactor vessel inspection?

MR. TAYLOR: It has not been extensively investigated for reactor pressure vessels. EPRI currently has a program in which they are going to use some ultrasonic holography for examining the vessel, but to date I don't know of any particular reports.

MR. BENDER: Can we move on?

(Slide.)

MR. TAYLOR: In summary, I would say that the future work is to be in optimizing the detection techniques and developing a standard criteria for calibration reflector and for flaw recording levels that currently does not exist. And finally, one needs to develop criteria for a verification block, and this would be a block with under clad cracks so one could prove one's procedure upon it.

MR. BENDER: Let me just put things in context. Some time ago when the staff came in, probably six or eight months ago, something like that, there was some optimism that the techniques available in Europe could be readily translated into application in this country. What is the judgment today?

MR. TAYLOR: The judgment is, the techniques are adaptable to our industry.

MR. BENDER: What have we done in the last six months to get in a position to use them?

MR. TAYLOR: I can't see that there has been any specific requirements done other than these studies that the NRC has through its research to develop the technique that is currently part of the program.

MR. BENDER: In order to use them, do we have to establish that the condition of the vessels that they would be used on is such that they would be -- that those inspection techniques would be meaningful?

MR. TAYLOR: Yes.

MR. BENDER: So you would have to investigate each vessel that is under consideration to determine what its surface condition is and its surface conditions, I presume, would have to be of a certain quality to make the technique usable?

MR. TAYLOR: That is correct.

MR. BENDER: It might be well if that were written down in some way so it were understandable by somebody.

MR. TAYLOR: It is addressed in the report.

MR. SHEWMON: The report that what?

MR. TAYLOR: It is addressed in Appendix L of the PNL report. There is also a report coming out through the research branch that details more of the under clad crack study that is currently going on at PNL.

MR. BENDER: Other questions?

(No response.)

MR. BENDER: Is that it, Mr. Taylor?

MR. TAYLOR: That is it.

MR. BENDER: Thank you. Why don't we take a ten-minute break? When we come back, we will listen to the Westinghouse presentation.

(Whereupon, a brief recess was taken.)

MR. BENDER: Can we reconvene, please? Would the people in the back of the room sit down, and we will proceed with the Westinghouse owners group presentation.

MR. SPEYER: My name is Daniel Speyer. I'm chairman of the Analysis Subcommittee for the Westinghouse owners group.

I would like to point out that this is my last day as chairman of the Analysis Subcommittee for the Westinghouse owners group. Beginning tomorrow Frank Scheuer takes over the reins of the chairman of the Analysis Subcommittee.

MR. BENDER: Are you being fired?

(Laughter.)

MR. SPEYER: No.

(Slide.)

We have had a chance to go through the draft report from the NRC, and I would like to talk about that report. I would like to tie in as part of that discussion the work that we have been doing -- that is, the Westinghouse owners group -- and then hopefully I can bring out some more perspective on some of the questions that have been raised by the subcommittee. As part of that, please feel free to ask the same kind of questions again of me. And, finally, I'm going to show a slide which the bottom line gives our conclusions about the draft, the screening criteria that have been presented by the NRC.

The owners group is in substantial agreement with the work that has been done by the NRC. In particular, we agree with the idea of the use of RT NDT as a screening parameter. In order to come up with screening criteria based on RT NDT the staff used operating experience to select the screening criteria and then used the probabilistic approach to support or give additional support for the screening criteria.

We are in agreement with all of that, although we did things in the opposite direction. We used the probabilistic approach first, and then in fact the staff pointed out that our probabilistic approach didn't seem to match the operating experience. We took a look at that and in fact reached a different conclusion. We reached the conclusion that the operating experience in fact just about fell in line with the probalistic stuff we did.

We do disagree on some technical details. A, I just mentioned, the consistency of operating experience with the PRA. There is some dialogue about the frequencies that we had in our probabilistic study. NRC came up with different numbers, as you heard -- some higher, some lower -- that I think will be the subject of some ongoing dialogue.

With regard to the calculational techniques in fracture analysis, there are two areas that we have some difference. One is the effect of clad. We have basically assumed a neutral posture for the clad in the fracture mechanics calculations. In fact, it is our expectation that this is probably in that benefit of the cladding effect, fitting the clad, keeping it closed, as opposed to residual stresses.

The other area is the use of finite flaw for arrest. Historically, the work that's been done by the Westinghouse owners group initially considered finite flaw for initiation and kept a self-similar geometry, and in fact, finite flaw for arrest, in fact, we used something that we called the two flaw criteria. I'm not going to go into the detail of that. But subsequently, rather recently, in fact, after more discussion with the NRC we did switch to continuous flaw for arrest; however, made with the finite flaw for initiation.

We believe that is a significant conservatism, and we think the truth lies somewhere in between those two; that is, between the position we had, which was self-similar flaws, 6 to 1 aspect ratio throughout, versus the other end of the assumptions one could make which is a continuous flaw for after-crack initiation.

We believe that higher values for the screening criteria can be justified.

Let me state these last two bullets I'm going to amplify a little more on at the end of my talk when I give a final conclusion slide, but we do believe higher values for screening criteria could be justified. And as far as the programs for plant-specific evaluations, we think 18 months prior to exceeding this criterion, criteria would be appropriate. And we also point out that those specific analyses should permit event sequence comparisons.

I will describe more later on what I mean by that statement.

MR. BENDER: Excuse me. It's probably better to ask this question, since you have some differences between yourselves and the staff concerning the flaw geometry.

Could you say why you think you are more likely to be right than the staff is?

MR. SPEYER: Ted, did you hear the question? I think I will leave that to the fracture mechanics types. May I suggest we hold that and get back to it, because I'm not technically competent.

MR. BENDER: I'm willing to wait.

MR. SPEYER: Okay. We will get back to that.

(Slide.)

Very briefly, I'm not going to read from the next two slides except to say they are in the handout which I believe has been brought in. And the slides, the next two slides, go through a progression of reports that we provided to the NRC. I will note a few things on them.

A, the first one, WCAP-10019, was the kind of design basis accident evaluation that has been traditionally done that in fact was quite a bit in the past now in terms of where we are today on what has been done for PTS.

The May 28th is a significant report. That was the one where we did in fact use the probabilistic approach to come up with probabilities for various event sequences. There was a fluence report. That was the last bullet on that slide.

(Slide.)

Subsequently, we did a step-by-step review of the owners group emergency response guidelines, the ERGs, relative to the impact on PTS, and you heard about that a little earlier from Steve Hanauer.

I would p'int out the owners group perspective on that is we found out some important things by that review. We found out areas where we think changes are beneficial. We are going ahead and doing that. We found areas where clarification is useful, notes could be added. We are doing that. However, the bottom line is on the whole we feel the ERGs that we had were in fact quite good and did adequately address PTS, and we are improving them. That is ongoing.

MR. SHEWMON: What's the ERGs?

MR. SPEYER: The emergency response guidelines. They are the generic procedures that the owners group was comparing. Those were then brought to the plants. The plants should take those and prepare their site specific emergency procedures.

MR. SHEWMON: You mean that we heard the Westinghouse procedures misquoted this morning when the statement was made they were fair densities and then take the pressure back up to 2000 psi, or that was indeed the best way to cope with the PTS in those conditions?

MR. SPEYER: I guess what I'm saying, A, is that was a bit of an overstatement; B, in fact there are cases where one would like to keep the pressure up. For instance, if you had a transient where you might trip the reactor coolant pumps -- the steam generator tube rupture, for instance, tripping the reactor coolant pumps puts you into a stagnation condition which raises more potential PTS questions than if one were able to keep the reactor coolant pumps running. Keeping the reactor coolant pumps running would be generally synonymous, at least if you forget loss of offsite power, which is a low probability event, and keeping the reactor coolant pressure system up would enable you to keep the pumps running.

MR. SHEWMON: But does the NDT go up to 2000 psi to keep these pumps running in the plant?

MR. SPEYER: Current criteria are on the order of 1250 to 1500 psi. It depends upon the instrumentation used. I would say 1500 psi would be a typical number that we could use for this discussion.

MR. SHEWMON: Why does he have to keep it up to 1500 psi?

MR. SPEYER: Because of Appendix K questions for the small break LOCA. If the reactor coolant pumps are not tripped at pressures lower than that, and that is, let's say, 1250 plus, this brings you up to 1500. If they are not tripped prior to going to 1250 for very specific break sizes and locations, you get inventory loss that will be fairly significant. And if you lose the pumps at the worst time into their transient, it is a fact you will in fact violate Appendix K; that is, 2200 degrees PCT.

To be sure you don't lose the pumps at that worst time in the transient, you trip the pumps purposely before you get to that point, before you have gotten significant loss of inventory. The reason for the excess loss of inventory with the pumps running is you tend to keep an elevated two-phase mixture, so you have more mass going out as opposed to steam relief, and yet you have less energy removal out of the break.

MR. BENDER: Just to pursue Dr. Shewmon's question a little bit further, if we set aside Appendix K and just say what fuel failures are we subject to by this kind of action, would the depressurization further result in massive fuel failure, or are we just concerned about violating the legal limit?

MR. SPEYER: I think it's partly the legal limit B. I think it is a little more than that, and I would not feel too comfortable with small break LOCAs without having the reactor coolant pump criteria in there. I believe it is useful on low pressures, not based on detailed calculation.

MR. CATTON: Before you get away from emergency response guidelines, we continually hear about emergency training of operators. Are you doing anything to try to get better methods for the operators to interpret what is going on?

MR. SPEYER: You're my straight man, but it's a little early.

(Slide.)

These are programs, and we'll jump back to this later on, but I would just mention now the slide I'm showing now is current Westinhouse owners group PTS-related programs. Two of them are in fact generic training packages, one in the area of PTS and one in the area of steam turbine tube ruptures, steam generator tube rupture.

MR. CATTON: I was thinking of a little bit more than training. In the LOFT program where they monitor pump current, that showed that that could tell them pretty quickly what was going on. There are probably other ways like energy balances on the steam generator or whatever.

Is there any research going on at all looking into that?

MR. SPEYER: Yes. That is here under the reactor coolant pump trip criteria development; also somewhat related to systematic evaluation effects of stagnant loop transients.

Under the reactor coolant pump trip criteria development we are looking at the plants and inspecting the plants and comparing it, doing additional analysis to see can we improve our reactor coolant pump trip criteria so we don't have to trip the pumps. Either we could allow lower pressure, or are there some other symptoms we should be looking at.

MR. CATTON: Are you sure the process interpretation in the control rooms is good enough? Maybe we need to do a little bit better. Is that a part of this?

MR. SPEYER: I don't know. I don't believe it is. I believe it is based principally on what we have available right now.

MR. CATTON: I think there is a real opportunity to improve the operator's awareness of what is going on behind the wall. I don't see owners groups doing it, and I don't see NRC doing it. It is kind of like you're going to live with what you've got, and you're just going to be telling the operators some more.

MR. SPEYER: Well, we are as far as the review of procedures. As part of that we are developing these pressure-temperature curves which will tell him when he is getting into a potential challenge, but that is not additional instrumentation. That is an additional diagnostic tool.

MR. CATTON: Why aren't things, like I think the time constant of the RT NDTs must be 200 or 300 seconds.

MR. SPEYER: Of the RT NDTs? I believe they're on the order of a couple of seconds, half a second or two seconds.

MR. CATTON: Well, then, you've already got all the instrumentation you need to do a pretty good heat balance in your system.

MR. SPEYER: I'm pretty sure the time constant is on the order of two seconds.

MR. CATTON: Well, then, you've already got all the instrumentation you need to do a pretty good heat balance in your system.

MR. SPEYER: I'm pretty sure the time constant is on the order of two seconds.

MR. SHEWMON: I think it is very important that you be able to drop pressure. And you say on one hand you're going to provide your operators with a curve. On the other hand you're going to say if we're going to make him trip his pumps when he hits 1500 psi, so he is going to bleed it on down.

MR. CATTON: I think he has to know whether he's overcooled or undercooled.

MR. BENDER: Well, let's leave it that we wish the operating procedures and the instrumentation matched up a little better and move on.

MR. SPEYER: But we are, to a very serious --

MR. CATTON: I was afraid I might have to sneak out.

MR. BENDER: Well, I'm going to allow you a few minutes to make your pitch, and you may get some sympathetic ears, but go ahead.

MR. SPEYER: The other point is the procedures do in fact provide for termination of safety injection. I believe the pressure is 700 psi -- correct me if I'm wrong -- in the procedures. And that is for RCS temperatures less than 350 degrees. There are other indications that are required in order to do that termination. I think it seems very level, and pressurizer level. But those are specifically in there, they were in there, and we've looked at them again as part of our PTS review to ensure that he doesn't keep the pressure up when he shouldn't. And so if the temperature is less than 350 degrees, that is a potential PTS scenario, and therefore, he does in fact have specific guidance or direction to terminate the SI when the pressure is approximately 700 psi.

(Slide.)

This is the rest of the outline of the reports that the owners group has provided. I mentioned the review of the emergency response guidelines. Those are the procedures, the generic procedures.

Beyond that we have the July 15th, September 2nd, those two reports. I'm not going to mention those in this slide. They are on the next slide in more detail.

(Slide.)

In the May 28th report from the Westinghouse owners group we demonstrated the likelihood of severe cooldown transients was on the order of 10^{-3}. That was, A, for crack initiation; and B, that was simply the probabilistic view of event sequences without any question about probability of flaw extension. There was no combination with what you've heard the NRC subsequently did in that area.

The conclusions of this particular study were that RT NDTs in the range of 310 for longitudinal and 335 for circumferential flaws were acceptable. That, in part, is why we're saying the current screening criteria are in fact too conservative.

Subsequently, the July 15th owners group report combined the 5-28 report with the staff's probabilistic fracture mechanics calculations. We came out with a number of less than 10^{-6} for probability. That is small, and in fact small compared to the proposed safety goals for core melt. That was discussed earlier.

I would point out you saw plot of probabilities versus RT NDT, and one was the owners group, Westinghouse owners group, and the other was the staff's; and there was a large discrepancy for small break LOCA. I didn't address that at the time because I wanted to be sure, but the owners group submittal with the low probabilities for small break LOCAs or for small break LOCAs less than two inches. That is, the cases that lead to breakage of natural circulation, that lead to stagnation and therefore lead to questions about mixing and have the potential as far as PTS is concerned were not on that. Those were handled by the deterministic calculations. Those are not on that figure.

So that is the reason why you see -- one of the reasons why you see such a divergence in the probabilities between the NRC's plot and the Westinghouse owners group plot. So we used the combination of the probabilistic approach both for event sequences and fracture mechanics to demonstrate plant safety for all Westinghouse vessels.

We have done this. I have here the number 270 degrees Farenheit and 325 for circumferential flaws. This is in part because all of the analysis we did supports higher numbers, quite a bit higher numbers, certainly 30 degrees higher at least for this small break LOCA which is an event that has been termed an outlier; that is, it has a reasonably high probability of getting to very low temperatures because of mixing questions in part, because of stagnation.

For that particular transient we did detailed deterministic calculations. We took the worst size -- that is, the minimum size -- that leads to the breakage of the natural circulation, and we did detailed deterministic calculations. We did that. The numbers you see here are based on finite flaw for initiation. However, subsequent after initiation they're based upon infinite flaw.

That is in conformance with what the NRC requested, and in fact, based on that, using DPA, using warm prestressing, we obtained a number of approximately 270 degrees for longitudinal and 325 for circumferential. We believe that is a conservative analysis, in particular because the question about the finite flaw for arrest versus continuous flaw.

That fits in any event fairly well with the NRC's proposed screening criteria. So I think that lends credence to their screening criteria. And if you haven't gotten the drift yet, in fact, when I get done my bottom line is going to be we are supporting the NRC screening criteria. We think it is appropriate, although we do believe it is a little bit too conservative.

Ted, there was a question raised. Would you like it addressed now?

MR. BENDER: Sure. My question has to do with explaining why the Westinghouse owners group disagrees with the staff's concept concerning flaw size and growth characteristics.

MR. SPEYER: In particular, I guess --

MR. BENDER: The question is continuous versus discontinuous flaw characterization.

MR. MEYER: Ted Meyer, Westinghouse. I will handle it in two different ways. One is the flaw initiation and the other is flaw arrest. One is cleaner than the other.

For flaw initiation we believe a finite flaw is appropriate, and the NRC is in apparent implicit agreement with that fact, based primarily on data that has been taken from pre-service inspections of vessels during the fabrication process, for flaws that have been found have been found to be either typified or bounded by a 1 to 6 semi-elliptic shaped flaw. In fact, those typically were not surface flaws. They were probably embedded flaws, which adds another degree of conservatism that we haven't taken credit for in this initiation. When it comes to arrest it is not nearly as clean as even that.

We do not obviously have a hard empirical basis for using a specified shape finite flaw for arrest. If we had, we probably wouldn't be having this discussion right now. We would have shown our data or whatever and been on with using a fine flaw for arrest.

What we do have is more qualitative than quantitative assessments that says the flaw shape should not change from the finite flaw to a continous flaw upon a first initiation during a thermal shock.

Now, there is apparently test data that shows both kinds of things that it goes through continous flaw, and then it also maintains some finite flaw shape. Some of the tests showing that it goes through continuous flaw do not have what we consider as the benefit of cladding to help constrain the growth of that flaw. So we don't have a hard empirical basis, and that is why for the sake of getting on with the resolution of this subject on an interim basis we did not press the issue, because in fact we don't have a hard basis for it; but in fact we are doing more work, again analytical rather than empirical at this time, to further justify our assumptions, be it a 1 to 6 or 1 to 7 aspect ratio or whatever.

In the future apparently Oak Ridge is going to be doing more work on empirical basis for looking at crack growth with the clad and with the assumption of a starting flaw that is finite.

MR. BENDER: With regard to the growth of a flaw, does it make a difference if the flaw penetrates the cladding?

MR. SHEWMON: Do you mean before it starts to move under the stress?

MR. BENDER: Before or during.

MR. MEYER: Well, it does make a difference if the flaw is through the clad or beneath the clad. If it is beneath the clad, it is obviously more constrained than if it is through the clad. If it is through the clad you lose any benefit, or you may lose any benefit that you may assume or calculate for constraint due to clad.

MR. BENDER: Well, I want to be a little bit more explicit. If the flaw penetrates the clad do you still argue that there is a limit on the rate of growth, or will it still be an elliptical flaw of relatively short length?

MR. MEYER: I don't have a well-founded answer to the question. But, first, I want to clarify what I think the question is: Is it more likely to become or approach a continuous flaw if the flaw is a through clad flaw?

MR. BENDER: Yes, that is the question. I'm just asking the question because I know there's one plant that says it is, and I would like to know what Westinghouse's view is.

MR. MEYER: I think the only thing that we can say right now is that the probability of going towards a continuous flaw is greater for a through flaw or a through clad flaw than for a flaw that is beneath the clad. I don't know if I could say anything more detailed right here now.

MR. BENDER: Okay. That's enough.

MR. SHEWMON: I would like to bring up one point. If I wanted to learn about what kinds of flaws are likely to be found or exist in vessels before they went into service or ten years into service, where might I look?

Let me talk a little bit more on that. It seems to me that the good news about this is I don't think the flaws are there. The bad news is I'm not sure we could detect with the ASME-approved techniques if they were. And so a lot of this is going to come back down to somebody coming in, or part of it is in saying there are no flaws there of the sort which all fracture mechanics assume to give them a job and get on with what they like to do best.

Okay. Any thoughts for one minute would be nice.

MR. MEYER: As far as a reference document for summarizing flaws, I personally don't know what the document is. I'm sure there is information that is available in some written form. In fact, some of the code requirements were based on some of that kind of data. I don't know what it is here. We certainly can find out what that is if you want it and provide such information.

What was the balance of your question?

MR. SHEWMON: That's enough. Thank you.

MR. ZUDANS: Your analysis of fracture mechanics was based on through the clad flaws?

MR. MEYER: Can you repeat the question? I couldn't hear you.

MR. BENDER: Would you speak into your mike?

MR. ZUDANS: The analysis that Westinghouse owners group did on fracture mechanics, did you assume the initial crack to be through the clad?

MR. MEYER: Westinghouse assumed the flaw was underneath the clad, starting at the base of the clad material. And the clad is intact except we don't take benefit for the fact that the clad is intact. We say there is no negative and no positive effect of the clad. We use the zero effect. Because there are positive and negative effects there that aren't well-quantified at this time, we used a zero effect.

MR. ZUDANS: And in the structure model you simply did not have the clad in?

MR. MEYER: In a structural model we used a zero effect of clad.

MR. BENDER: Can we go on, Mr. Speyer? I want to get done in 10, 15 minutes at the most.

MR. SPEYER: The final point I would like to make about the small break LOCA analysis that we did, in the deterministic calculation we did in fact use the better estimate of mixing in that calculation. That was based on the Criari test data.

There has been some discussion about the Criari test or in general about the measurement of temperature in the cold leg versus the temperature in the downcomer for conditions of pump running or natural circulation. And I think the general view is that there is pretty good mixing. The condition of stagnation, Criari did some tests into that condition. Mixing is pretty good. In fact, there has been a model developed called a teacup model. It is basically flow into a finite volume and flow out and mixing within that volume. If one defines the volume appropriately, then the data from the Criari test can be predicted pretty well. That in fact is what we used.

It is our expectation that in fact of our sources of hot water -- this was discussed in fact at an NRC meeting or an NRP meeting -- there are the sources of hot water that are present, in particular the lower plenum, that were not modeled on the Criari test. So it would be our expectation that there are in fact additional sources of hot water that would make the situation even better.

In any event, our analysis assumed a better mixing calculation using the Criari test. It also included heat input from the appropriate heat slabs that were available in the cold leg and in the vessel region.

(Slide.)

Going on, I would like to touch on the current owners group PTS-related program. As I said, we are developing emergency response guideline modifications. That was based upon the review that we already completed. We are implementing critical safety functions for reactor vessel integrity. That is basically a graph or graphs of pressure versus temperature. As to the reactor pressure, let's hold the pressure fixed for a moment as we drop the temperature in the system. We would approach a curve, and in fact at some point we would cross a curve that would give the operator indication that he is getting into a potential PTS regime.

As one went further along this line that I am talking about into a PTS regime, another curve would be crossed. That would tell the operator you are into a PTS situation, go to -- or you are into a potential PTS sitation, go to a function restoration guideline, which in essence says simplistically drop everything else; you ought to worry about PTS to get yourself back into an acceptable regime. And that is in fact what would be done as far as a hierarchy. That is the second highest of the function restoration guidelines. The only one that is higher would be inadequate core cooling. In fact, if the operator got into a situation of high core exothermocouples, that would in fact take precedence over PTS, but that would be the only one.

So we are developing that, and that is designed to give the operator quantified information that he can use as part of the procedures. Those are going to be developed for various plants based on various full power years. We are doing a generic training package for operator training on PTS. This is to be able to give consistent information and up-to-date information the best we can to all of the plants for their use in training.

We're doing the same thing, too, in the steam generator tube rupture because this is a potential PTS transient that has to be looked at. It also happens to be a very complicated event, and that is why it is appropriate to develop a true generic training package for that event as well again for consistency.

This is not to mean, by the way, that people like CPOL and others who are in fact upgrading training and have done so and have developed packages -- those are very good packages. This is to provide consistency and provide assistance for all the members of the owners group, which is in fact all of the operating Westinghouse reactors.

As was mentioned before by Ted Meyer, we are doing testing and analytical work in the area of warm prestressing, specifically with respect to clad effects and flaw shapes.

We are working on reactor coolant pump trip criteria, to be able to prevent tripping the reactor coolant pumps under conditions where it is not needed and, in fact, where tripping the pump would be detrimental either because it tends to give more potential to PTS concerns, or, in fact, it removes some of the operator's capability of very effectively terminating transients.

Finally, we are doing a systematic evaluation of the effects of stagnant loop transients. To summarize before I put up my final slide or final two slides, the screening criteria of the NRC, even if it is too conservative in the view of the owners group, is in fact reasonable or, to put it another way, is not unreasonable. It enables all of the parties to effectively utiliize our resources. It is technically a sound method. And it ensures safety.

(Slide.)

Therefore, we believe we should go forward with the NRC proposed screening criteria, up to 70 degrees for longitudinal and 300 degrees RT NDT for circumferential flaws. Again, we believe the values of approximately 30 degrees greater would be more appropriate, but we think we should go ahead with what the NRC has proposed. Plant-specific programs be developed 18 months in advance of the screening criteria. NRC has proposed 3 years. We believe 18 months would be a more appropriate number.

We also point out that there needs to be work done on what, in fact, are the requirements to be met in a plant-specific analysis, not what is the analysis that the plant does and how does it do it, but what are the criteria one has to meet? Presumably, that is going to be a probabilistic space. We do not think you could pick a single scenario -- for instance, a small-break LOCA -- and call that the PTS DBA. We think it will be in probabilistic space, but we do think we have to have those criteria developed before we go doing the analysis. And that I think will be a little while in the future. We also think 18 months is sufficient here.

MR. CATTON: What is the basis for the 18 months rather than 3 years? You just think that is plenty of time to do the job?

MR. SPEYER: I think it is plenty of time, yes. B, I think there is some problem if you make it too long.

C, in fact, the plants are actively looking at this for other reasons; that would be not necessarily just the safety, but they have the economic questions that, in fact, are more limiting, if you will. If you have a crack that initiates and arrests and you did not have a safety problem, you maybe would have to write off the vessel.

So we believe 18 months would represent a balanced amount of time in order for the plant, 18 months prior to exceeding the screening criteria, the plant would submit to the NRC the program they are going to use to resolve it.

MR. SHEWMON: You are taking as self-evident that the operators or owners at the plant are likely to look down the road 3 to 5 years and wonder about core modification, is that it?

MR. SPEYER: I take it, in fact, a little differently than that. I believe the study that EPRI did, looking at all the utilities, found that something like 85 percent of the utilities have, in fact, initiated low leakage patterns. It may be for a multiple of reasons, but almost everybody has done it.

As far as the plant-specific programs, we believe you should utilize comparative plant sequence analysis. What I mean by that is we, in fact, have done a generic probabilistic study that considered event sequences. We believe that, A, represents an average Westinghouse plant, if you will. B, it, in fact, is representative of all the Westinghouse plants.

And we feel the appropriate way to go is for plants to utilize that study, if they wish, and then tailor it by showing or, first of all, showing that, in fact, it is appropriate for them, making such changes as they would feel better tailored to them.

For instance, a plant with low head safety injection pumps would probably take that report and modify the calculational numbers to represent the fact that they do not have high head safety-grade charging pumps.

They would also, I think, appropriately take into account operator action -- excuse me -- better operator training, upgraded assistance, upgraded procedures, and then utilize that generic report, tailor it somewhat to the situation, and use that as pposed to a full-blown probabilistic study done by each plant. They can do that, but I do not think that is necessary or appropriate here.

Finally, the way one would utilize such a plant-specific analysis, what would one do? Prior to passing a screening criteria analysis would be done either, A, to show, in fact, that a higher screening value was, in fact, appropriate for that plant either because of plant-unique systems, control systems, what have you, or lack of control systems.

B, the plant could show that there are some events that fall outside a probability acceptance goal. Both events would then be calculated on a deterministic basis to show whether or not they are acceptable. In fact, exactly the way we did our calculations for the small-break LOCA.

And finally, the third level would be if, in fact, it is shown that there are unacceptable results or, in fact, the probabilities are higher than desirable, there would be remedial actions. The remedial actions could extend from, you heard, reheating the fuel and storage tank to other areas.

I might point out on that heating storage tank, it is not clear right now that there is a major problem with doing it, but I do not know what is involved in plant-specific cases to do it, number one.

Number two, it does have potential impact, at least on containment integrity calculations, and that may raise the pressures above some of the design pressures that are shown in FSARs. I do not know; that would have to be looked at on a plant-specific basis.

Finally, we think there are a number of conservatisms present currently in the way the calculations are being done. I think there is probably general agreement about that, although there is different agreement as to quantification of them and should we remove them now or not.

We do believe it is important that for the future, given that we are going to use the screening criteria approach as we start to have more results coming in, that definitively would give a basis for or more definitively give a basis for removal of conservatisms.

We think it is appropriate to factor that in and, in fact, raise or appropriately adjust the screening criteria to recognize the removal of what were presently less quantified conservatisms than will be more quantified in the near future.

That is what I have to present for the Westinghouse Owners Group. Are there questions?

(No response.)

MR. BENDER: If not, I would like to thank the Westinghouse Owners Group for giving us this presentation. I think it helps us to understand the several views that exist. And we will take those into account when we try to develop some recommendations to put before the full committee.

Could we stop the Owners Group presentations for a few minutes so that I can try to solicit comments from the consultants and those members of the committee that might need to leave by 4:00 o'clock, so that at least we have the benefit of a collective set of thoughts at least while everybody is here. Let me start with Ivan Catton.

Ivan, you have some thoughts?

MR. CATTON: I have several things. Earlier this morning we discussed the A transient. I really think the Staff should analyze this so we can get -- we are using best-estimate techniques -- so we could get a better feel for what kind of conservatisms are involved in the results that they have been showing us.

I think Steve made the comment about the operator having to walk between overcooled and undercooled and the desire not to be ultraconservative. I think this emphasizes the need for establishing what symptoms tell us quickest whether we have overcooling or undercooling.

And I think it really deserves more attention, and that to try to throw the whole thing into more and better training for the operator might be a mistake.

MR. BENDER: You are saying a study of the diagnostic capabilities is in order?

MR. CATTON: That is right. The only thing that has been done along those lines is the brief study that was done on LOFT on how well pump current could tell them what is going on.

I think they can do a lot of things, particularly if, as was just mentioned, the time constant of the RTDs is 2 seconds. You could surely do a heat balance on the system and know what is happening and be able to tell the operator what is happening. I think just more training is not going to do it. I think it is too much to expect.

As far as the four items that Steve mentioned, I think no immediate action is wise, because I think there is still a great deal of conservatism in the Staff position on the thermal hydraulic side as to the spec. There is still a great deal in fracture mechanics, I think that 270 degrees is fine, and with 3 or 4 years before the first plant runs up against the wall, it seems to me there is plenty of time for adjustment if it is not right.

I would like to see some magic number where the plant has to shut down and somebody has not done something and have that number fixed so that when the time comes there are no surprises.

MR. BENDER: Monroe?

MR. WECHSLER: I find myself interested in the question of the calculation of the delta RT NDTs. Following the discussion we had earlier, I find it difficult to understand the wisdom of the kind of two-tier approach that is being used, in which for the higher fluences, I understand that this depends upon copper concentration, but generally for the higher fluences, the Reg Guide 1.99 is used or the Reg Guide 1.99 criteria are used, whereas for the lower fluences the Guthrie fit is used.

I think consideration should be given to using the Guthrie fit throughout the entire range of fluence values. That is, for the entire population of points obtained from the surveillance samples.

MR. BENDER: Do you have any view about this two sigma criteria?

MR. WECHSLER: Well, I think the basis for that is fairly well explained in the NRC draft document. And one might argue with the precise values. Certainly, there is nothing sacred about the values to plus or minus 1 degree F., and certainly there is some room for argument there. But I think in general terms the document justifies the basis for introducing that two sigma approach, and I am generally satisfied with it.

MR. BENDER: Herb.

MR. KOUTS: Well, I certainly agree with what Ivan Catton has said just now and what Theo is going to say on the subject of analyzing these transients. And I think some of this analysis is really under way now. It is just not at the stage where it can be reported. I believe there is Los Alamos work proceeding along these lines.

But as this continues, of course, there are some insights that will develop. And one thing that bothers me very much, especially listening to what I have heard today, is how much space is there between what operators are supposed to do if they are subjected to conditions of pressurized thermal shock that they have to worry about, and LOCA. And, you know, you may be put in a position where you are damned if you do and you are damned if you do not.

With respect to such things as repressurizing the plant, is there space enough in between the two sets of requirements under these two circumstances, so that trying to get out of trouble in one place you do not get into trouble in another. And I think this needs exploring.

Beyond this, I notice that during the day we have not heard anything about elastic plastic fracture analysis and its implications for this problem. It seems to me that this is a fruitful area for research programs to be initiated. I think that NRC really ought to get more heavily involved in the elastic plastic methods.

And, in particular, I would like to follow up on some of these comments that Professor Irwin made in the note that he wrote to you on the question of whether or not you get crack arrest in material above the upper shelf if it is initiated in the brittle region. I think this is a very important thing to try to establish: under what conditions this takes place and under what conditions it does not.

MR. BENDER: Theo.

MR. THEOFALOUS: First, I would like to say that I am very pleased with the progress that the Staff has made in the recent months. And also, I like very much the forceful way in which they presented their approach. I think it is a sound approach, and they are on the right track.

I agree for the time being with the screening criterion also. And I like that approach because it is specific because it puts things down so that other people can look at them. And therefore, hopefully, in this way one can keep refining things so eventually you can come to where the truth is.

And in that spirit, I would like to suggest that this aspect of refinement not be left completely, but try to be formulated in such a way by the Staff so that it becomes easy and possible by the different interested parties here to keep working on a similar framework instead of everybody working in his corner.

And what would help a lot in this direction is if a base case or a set of base cases are documented in a way that they are totally scrutable so you go out and take a look at it and say, I do not agree with that, you had better do it this way; and then if everybody agrees, then you can work on that step. And if everyone was to do that, a lot of the questions we had today, for example, from me and Ivan and so on, they would not be present.

So that I do not disappoint Mr. Kouts, yes, I think the analysis of those specific transients, the fact that the only experiments we have should be done, although there are difficulties with such allowances that are to be recognized. I think one can do quite a bit more than saying, I have difficulties, therefore I cannot analyze. And I think we can learn quite a bit from such analysis.

I think that this is -- also I want to make a note agreeing with Ivan again that we should come up with procedures for the operators that basically are diagnostic signatures, so that they can recognize the situation and know what to do.

MR. BENDER: Zenon.

MR. ZUDANS: At this stage, everything has been said. But I have to add some caveats. I personally feel that one should be able, by continuous collapsing and removal of conservatisms, to define what is called PTS design basis. But that is my feeling, although I heard Westinghouse Owners Group saying that that was not a problem. I have no qualms about these criteria the NRC presented. I think they are adequately conservative.

I would like to repeat again that I would like to see some of the conservatisms more specifically identified. Some are identified now by the Westinghouse Owners Group. Others were stated by Dr. Hanauer. And, for example, what is the effect of three-dimensional stage of temperature and pressure stress distribution as one compared to one used in these analyses? How important is that aspect? It is not quite clear. It could be significant conservatism, or it could also be none.

The other thing is I do not understand the philosophy of excluding cladding from the vessel. It is physically there, and not to have it in all models, heat transfer and structural model, it just does not strike me as being right.

MR. SHEWMON: Was it ignored in the heat transfer?

MR. CATTON: No. They deal with an effective heat transfer coefficient. That has in it. They put it into the heat transfer.

MR. ZUDANS: Did they consider the heat capacity, or just conductivity?

MR. CATTON: I think it is considered.

MR. ZUDANS: Well, if I am smart enough and considerate enough, I can sit and reason everything out and come up with a clean core. And that is not the case, to show that this is indeed true by analysis.

MR. CATTON: That would be easy enough to do.

MR. SPEYER: It is excluded implicitly in the Westinghouse Owners Group with its associates for the heat conduction aspects.

MR. ZUDANS: All right. I remain unconvinced about statistical basis on operating experience on these events, having three B&W and four or five Westinghouse, and putting them in the same basket and coming up with statistics. One of the two that hurt and the other two got advantages of the first. So I do not know if we should treat Westinghouse separately from B&W. You might come out differently. That's all.

MR. BENDER: George.

MR. IRWIN: I believe the NRC has made a very good effort insofar in response to this problem. And I also appreciate that the probabilistic work at Westinghouse has been quite useful.

Now, with regard to conservatisms, I like conservatisms myself. I am glad to see them. I do not particularly want them to go away. But I would like to know how much they are. And I made a little list of these that are just in the fracture mechanics area.

For example, the variation of the mean K.1.C with crack trunk length, that will be a substantial one because the more rapid the chill the more we are propagating tiny cracks, and the tinier the crack the greater the mean K.

A second one was that the crack shape after initiation is definitely not going to remain self-similar, but it is definitely not going to be infinite. And after all, the shell course is only about 80 inches, and if you crack goes halfway through an 8-inch wall, you have got 4 inches. That is a length of 20-to-1. And the calculation of 20-to-1 in a crack halfway through the real reactor vessel is quite significant in its effect on the K value.

The third one was warm prestress. Now, that is going to take time. People will have to agree on how to do the experiments. And it may not come soon, but definitely there are benefits of warm prestress which can be ascertained in a straightforward manner -- and should be.

And number four was the fully plastic conditions for deep cracks. That has been mentioned before. I am not sure that we will get a great deal on help on that score. But there is an uncertainty there that should be removed and can only be removed by going in and studying the deep cracks on an elastic plastic basis.

Number five overlaps the fluid. And I do not know anything about the fluid dynamics. But it does seem odd that you can have cold water coming down like a plume and at the same time chilling 360 degrees of circumference. And I believe that discrepancy was mentioned this morning by Steve Hanauer.

Now, that made quite a difference in the fracture mechanics calculation. Whether I cool three wall thicknesses or five wall thicknesses, all the calculation assumes you cool 360 degrees.

Well, those are my comments.

MR. BENDER: Bill, I will let you have the next word.

MR. BOCK: I will keep it short, since you have most of my comments already.

Traditionally, what we were faced with in reactor problems, we have always relied on a defense-in-depth approach. And I see us getting away from that, in that right now we are oriented toward placing all of our eggs in one basket; namely, that we believe that the reactor vessel is in a ductile, low RT NDT condition.

And we are not approaching at this point any of the other ways around it, which would include, for example, trying to reduce transient frequencies or trying to get better inspection techniques.

The way the screening criteria is currently set up, it looks like we are going to defer those until our first line of defense, meaning the ductility of the vessel finally gives up. And I would like to see us pursuing the others perhaps a little more vigorously and perhaps independently of what the current RT NDT of any particular vessel is.

MR. BENDER: Forrest.

MR. REMICK: Thank you. Just an observation.

The operating reactor data base is based on several hundred reactor-years of domestic experience. I would be curious to know what the foreign experience would show. Would that enhance our understanding? Would it improve the data base? Could it be used? Or would it just further complicate our lack of understanding?

Also, I wonder very much about the naval reactor operating experience. I do not know if the owners groups know about that. Do they have access to the information? I do not know if the NRC has that access.

And along that line, has anybody thought about Shippingport, which I now understand is decommissioned. Is there anything about that vessel? I know some designers who were involved in Shippingport, and we are somewhat glad to see that Shippingport might be decommissioned. Is there any information available, any thought about how that might be used?

And if the answer to my questions about do we know what the naval reactor experience is is "No," then maybe the subcommittee or the committee itself might take up the naval reactor people offer for exchange of information and see if this is where they might share some of their thinking?

MR. BENDER: Paul or Bob?

MR. AXTMANN: I am kind of curious about the seven reactors defined as "at most risk," perhaps the three worst, whether the owners groups or the individual plants have looked seriously at the possibility of changing the fuel loads not 3 years from now but starting today and what the penalties for that might be?

I noticed on one of the slides from WOG that they have a report, and I am anxious to see that, but I wonder if any of the involved people have any comments?

MR. BENDER: Is there a hand back here?

MR. MORRIS: I could comment on that in a minute when I get up there.

MR. BENDER: Would you identify yourself?

MR. MORRIS: I am Ken Morris of the CE Owners Group. But I can address that when I get up.

MR. BENDER: Fine. We will hold it until then.

I am in the enviable position of being able to say, having heard all of the comments, I agree with most of them, and I do not think there is much that I want to add to this conversation beyond saying that what I would like to do is collect the set of comments here and put them into some more organized form and presumably pass them on to the committee as the collective views of this working group.

Were there any problems that anybody sees in making that kind of presentation? We heard different viewpoints, but I did not hear much dispute about what was presented. And, hopefully, the committee will, in turn, digest them and pass them on in a somewhat similar way to the Regulatory Commission.

With that, I think I would like to go back to the CE Owners Group, and we will follow from there.

MR. MORRIS: My name is Ken Morris and I'm with the CE owners group of the Omaha Public Power District. Dr. Hanauer has often wondered who I am with, so I was going to try to clear that up to start with.

I would like to respond to a question regarding fuel loading patterns. I cannot speak for the owners group; I can speak for Fort Calhoun Station. We are scheduled to go into our refueling outage beginning January 3, 1983, at which time we will go into a low leakage fuel loading, which we currently estimate will make a rather substantial reduction in our fluence; probably a factor of two.

I don't have too many of the other CE owners group members here. I am not aware of what their actions are or timing for any changes in their fuel loading patterns.

MR. AXTMANN: Do you have a number for what happens with the power level?

MR. MORRIS: There is no penalty regarding the power level. There will be some reductions in our operating margins. We do feel we have adequate margins to maintain our currently licensed power.

MR. AXTMANN: So the economic penalty is essentially little?

MR. MORRIS: That is true. I might say that we will explore methods of making further reductions in that fluence, also, in the future.

I would like to say that we do appreciate the opportunity to comment on the staff's evaluation of the reactor pressure vessel thermal shock. The received the report middle of last week and we have performed a prelimimary review of that report. We recognize there have been some, we consider, very substantial improvements in the program, in the staff's program since we had seen last spring.

We are pleased that the evaluation confirms the staff's and the industry's findings that there is no immediate need for plant modifications to protect against pressurized thermal shock, other than those improvements in procedures and training which are already underway or completed.

The NRC proposes to use the screening criteria involving RT NDT as a method to identify those licensees which must perform additional kinds of specific evaluations. The CE owners group believes that proposed approach is rational and conservative, and will serve to identify those plants with potential pressurized thermal shock concerns. Properly applied, we believe this could be a very effective method.

The CE owners group has revised the methods of calculating the RT NDT, and agrees with the staff. We believe this method will provide a reasonably accurate and conservative value for RT NDT. We agree that plant-specific evaluations are needed to determine what, if any, plant modifications to equipment, systems and procedures may be required.

As stated in the report, more detailed guidance for these evaluations must be provided. Acceptance criteria must also be developed. We urge the staff and their consultants to work closely with the industry on the development of these guidance and acceptance criteria.

We believe caution must be used to assure that the guidance and acceptance criteria do not become overly prescriptive and thereby eliminate other activities which could assist in resolving pressurized thermal shock concerns.

The CE owners group believes that the timely development of guidance and acceptance criteria, coupled with reasonable schedules for the completion of any additional evaluations, plant-specific or otherwise, will permit an orderly and timely resolution of the pressurized thermal shock concerns.

I want you to know that we stand ready to work with the staff and your consultants on the development of the acceptance criteria and the guidelines for resolution PTS. And I do want to make a few comments regarding ongoing activities of the CE owners group.

You have heard quite a few comments regarding procedures and operator training that CE owners group is developing. We began sometime ago a training program for PTS. We also have been working for quite a while on emergency procedure guidelines which do incorporate PTS concerns. PTS work has been a major part of our training and our procedural work. We are proceeding with that work as rapidly as we can to assure that it does reflect concerns for LOCA and PTS, and we agree it is something that needs to be clearly defined to the operating people.

That is about all I have to say. Everything else has been said today, so there wasn't any point in my repeating it.

MR. SHEWMON: Given the comment about what kind of room you feel your operators have for maneuvering or getting confused between PTS and LOCA concerns, have you gotten into that enough to feel that that is not a problem with your present procedures, or assumed procedures?

MR. MORRIS: I can't give you an operating parameters that would quantify that margin. We are insuring that the proper people, LOCA, PTS, are involved in the procedure guidelines so that we are not improving one at the same time doing harm to some other set of procedures. There has to be a balance there.

The way we believe it has to be done is letting the experts in the areas work hard on those procedures. After that is done, have those procedures and procedure guidelines thoroughly reviewed by the operating people.

MR. SHEWMON: You haven't heard any screams from that quarter yet?

MR. MORRIS: No, we haven't. We are paying a lot of attention to the procedures and training.

MR. BENDER: You will recall that the staff position suggests an 18-month period. Excuse me, three years. My timeframe is fouled up. A three-year period of preparation prior to seeing the bell ring, so to speak. And the Westinghouse group has suggested 18 months. What is your view?

MR. MORRIS: I guess our view is we are going to proceed. Let me take my owners group hat off now and put on my OPD hat, if you will permit me, because I can't speak for the other utilities on this one. But Omaha Public Power District, our vessel is one of those on the top of the Hit Parade. You can bet we will proceed as timely as possible, consistent with the staff's effort on this to get the proper corrective actions identified and implemented just as soon as we can.

I think someone else mentioned commercial interest in this, and we are very sensitive to the commercial inteerst, also. I just can't hardly see us identifying an action that should be taken to resolve or to reduce the PTS concerns and not taking those actions once we are in a position to implement them. It just doesn't make any sense for us.

MR. BENDER: Other questions for the CE owners group?

(No response.)

Thank you very much. I guess now we will have the B&W group with the last opportunity.

MR. SHORT: I'm Barry Short from B&W, and I am here to represent the B&W owners group at the request of Mr. Lee Pacino. Lee regrets that he couldn't be here today but he had some unexpected business come up at the last minute. So he asked me to just cover a few points here for you.

I'm going to keep my comments brief, also.

(Slide.)

What I want to do is cover three areas. Give you an idea of what the B&W owners group is doing with the thermal shock program, give you a few comments. I have one slide on the first one and four slides on some general comments on the staff's draft report, and then I have a concluding slide that will give you an indication of where the B&W owners group is going to go from here.

(Slide.)

I guess a most of you know, the B&W has been involved in this issue for quite a while, and what we have done is we have issued some generic reports back in 1980. They are basically discussed in the staff's draft report. There is a B&W 1628 and B&W 1648. They are generic reports back in 1980, and they concluded that if you do a generic analysis, first of all, it's unrealistic and you can see some imminent problems in the future.

At that time or shortly thereafter, we started some plant-specific analyses and in fact, we had planned just prior to the staff issuing their August 21st letter to do the plant-specific analyses. The August 21st letter kind of target Oconee-1 and GPU, TMI-1, so we were already doing those plant-specific analyses and basically concluded that those two plants are acceptable for the thermal shock issue.

Now, in the process of going from generic to plant specific we learned a lot. In fact, in that process -- well, I guess we've always known the small break LOCA was a limiting event, but if you can solve that problem you may end up with a different limiting event, so you can't just pick small break LOCA and say that's what your problem is.

Between the Duke Power Company and the GPU analysis, we learned a lot about mixing, and that report, the TMI-1 report. We have used a COMEX code mixing analysis which shows quite a bit of margin for the small break LOCA analysis, or small break LOCA transient.

Those plant-specific reports were submitted back in early 82 for Duke Power and July of 82 for GPU, and basically, the two owners are waiting for the staff's review of that in detail. And as a result of that, the other plants have told us to just put everything else on hold. So in this regard, nothing else is being done by the B&W owners group as far as plant-specific analysis.

Let me just comment. I have four overheads here to comment on the staff's draft report.

(Slide.)

In general, it is a very comprehensive report. I think they have done an admirable job of putting everything in one place. I do have some specific comments.

Like I said, first on the approach, we agree that the screening criterion is an acceptable way of targeting to determine when or if plant-specific analyses should be performed. However, you've got to watch out for a couple of things, and I just want to get a couple of points across here.

We do believe that the values are conservative, and that is based on what we know today. One of the key things that we've got to keep in mind is that we should, in my opinion and several of the other owners, we shouldn't establish a limit for RT NDT today and not be flexible. So flexibility is a key point here because we know that even in the past year things have changed. We have seen it go from where we have only had, say, two or three years left and now we've got maybe four or five years. B&W analyses show that you've got somewhere beyond 30 years.

So I think technology is going to advance. We have seen examples of that in the mixing area. We know that fracture toughness is being worked on. Elastic plastic techniques. And we also know that experience enters into this.

The staff is using a rather limited experience base, but as time goes on, that experience base is going to change. And if we really believe we have told the operators to watch out for the certain type of events, hopefully in the future, we're not going to see any operator errors. So flexiblity is a key that I think we ought to be considering.

Somewhere -- I guess it's in Chapter 10 or maybe the recommendations -- it says the RT NDTs must be calculated, or should be calculated, in accordance with the way it is shown in Appendix E, or whatever it is. I'm not really sure. But again, in the lines of flexibility, if there are other ways of calculating RT NDT and the B&W owners group is working on some of this, we shouldn't close our minds to that.

So screening criterion is an okay way to go, but let's be flexible about it. In a couple of years we may have a new criterion. Maybe it is 300 for longitudinal, maybe 350 for circumferential. We don't know.

Item 2 suggests that the staff maybe just clarify what their sequence of events are, and this I think is on page 1.4. Or maybe it's 4.1. Screening criterion is acceptable. You move from that into a plant-specific evaluation. Depending upon what you find in that plant-specific evaluation will determine what you do next. Plant modifications may or may not be required. There should be a three-step process here, as opposed to a two-step process.

Third, we talk about -- I guess it's talked about [illegible] the report, -- a technical basis for if you have to do a plant-specific analysis, you have to have a technical basis for the remainder of the plant's life, and I think basically, this issue of thermal shock is a reactor vessel integrity issue, much like the issues that Appendix G of 10 CFR 50 already talk about. And I think the goal there is to make sure we stay ahead of it.

If a plant-specific evaluation can show that this plant has no problem for 20 years and the plant is at five years now, that may be acceptable. That plant may not want to make modifications yet. I think that is the philosophy behind Appendix G.

I know the staff has referenced Appendix G several times in their report. I think it is a pretty smart philosophy. It is kind of tied in with keeping flexible and making sure that you watch out as technology advances that you are ready to make the changes as appropriate.

(Slide.)

As far as acceptance criteria goes, I had a problem when I was reading the report add I wasn't quite sure what failure was. It is talked about several times as being no crack initiation. Very extensively it went into crack arrest There were safety goals that talked about radiation releases, core melts or whatever. I did not get a clear impression -- and this was relayed to me by a couple of other owners, also -- of what the criterion was when the RT NDT of 270 was calculated. The word "failure" is used. I think maybe that ought to be clarified

I think it was no initiation of crack, but as I got further along I am not quite sure if it wasn't arrest. It doesn't really matter, I guess, as long as we know what it is.

And secondly here, the point that Steve made this morning, one of the key things, and one of the reasons why the B&W owners have put the plant-specific evaluations on hold, is that we don't have an agreed-upon acceptance criterion. We shouldn't waste time doing plant-specific analyses without knowing what is going to be acceptable. And like I say, that is one of the main reasons why the rest of the B&W plants are on hold right now.

MR. BENDER: There is something illogical about that statement. It generally follows that people do some analyses before they try to set acceptance criteria in order to get some understanding of uncertainties or how to allow for them, what options are available.

I think essentially what you are suggesting is that the NRC staff go back to its old generic basis. I find that very discomforting, and if the position which the B&W owners group is taking is that the staff has to have arbitrary criteria before it will do any work, I think we ought to make the criteria very conservative in order to make sure the work starts early. What's wrong with that logic?

MR. SHORT: Let me explain what I meant by that. The B&W owners group has already submitted two plant-specific analyses. We have a criterion in there, and that is the criterion that we have used. And based upon those analyses extrapolating, if you will, to the other B&W plants, we don't see that there's going to be any problem even with the other plants.

What we would like to see is we would like to see some kind of evaluation done of the acceptance criteria that we have chosen prior to the staff --

MR. THEOFALOUS: What have you chosen?

MR. SHORT: We chose in those two reports within one quarter of the thickness of the vessel wall crack arrest. And for a regulatory purpose, that may be acceptable.

I guess what I'm saying is we want to get some feedback on those reports. It's not that you sit by and don't do anything. We have done something, and for that reason, the other reports are on hold. I don't know if that answers your question.

MR. BENDER: In addition to submitting the reports, was there a specific request for a critique of items within the report? Or did you just send the report in and say, what do you think of this?

MR. SHORT: I don't know if you've seen the reports, but the reports cover everything. We've related to pressurized thermal shock. Everything we know today.

MR. BENDER: Are they more comprehensive than those which the staff prepared?

MR. SHORT: Do you mean the draft report?

MR. BENDER: The draft report, yes.

MR. SHORT: I commended the staff for being in such agreement with what we have. Yes. They have covered every area. I see nothing left out. We did not go quite as extensively into the probabilistic or safety goal area.

MR. BENDER: Do you think that is unimportant?

MR. SHORT: I think in the future -- I think if you can show that you've got, with nominal calculations, 30 or 40 years on a plant today, granted there are a lot of uncertainties, but it certainly tells you that there is a margin there.

In one of my later slides I'm going to show you what the B&W owners are planning to do now. It's not like we're sticking our heads in the sand.

MR. BENDER: Well, why don't you go ahead with the remainder?

(Slide.)

MR. SHORT: I just want to continue some comments here, looking at the applicability of the methodology and the results. As we have been hearing all day, the results are based largely on a lot of input from the Westinghouse owners group, and there are some basic differences in the plants. I know this was talked about earlier, and I have a couple of examples here. The vent valves and the once-through steam generator.

And I guess what the B&W owners group would comment on is the significance of those differences ought to be at least evaluated before the report is considered a final report. And I think they've got that. The staff has as a recommendation in Section 10, so I would agree with that.

(Slide.)

My last of the general comments on the report is on timing. I know we talked a little bit about this today as well. I think it might need some clarification, and I think maybe some other people are thinking along the same line. If we go ahead and begin plant-specific evaluations three years prior to exceeding some criteria, that's fine, we have no problem with that.

Somehow, though, that report must be accepted by the staff at some point in time because someone has to make a judgment on what those plant-specific evaluations are; if they are good or bad. And that is a time factor that is missing. And I would say maybe a year ahead of time. I don't know.

Based upon our experience, we have had the staff reviewing the Oconee and GPU report or the TMI report, and maybe a year is good enough. I don't know. I think it is something that is missing as far as timing goes.

MR. BENDER: Well, have you thought about what you might have to do in the course of that year in order to avoid being shut down? Do you expect the staff to think that out for you, or are yu going to do it yourself?

MR. SHORT: If you do a report three years ahead of time and the report comes out and says hey, I'm good for 30 years and you submit it, --

MR. SHEWMON: Maybe no news is good news. You don't understand what I'm saying. I mean, the staff obviously is worrying about PTS. If they aren't on your back maybe it is because they figure somebody else has more of a problem than you do.

MR. SHORT: That could be. We have suggested just adding a column, which I saw was added earlier today, just talking about calendar years and not effective full power years. And I saw that that column was added in Steve's presentation this morning. Not exactly as it is labeled here, but the column was there.

(Slide.)

My last slide talks about where do we go now. And as I mentioned before, the remainder of the B&W plants are on hold. We're trying to just find out whether it's acceptable or unacceptable.

The B&W owners are continuing with their reactor vessel materials program. This is a program geared to find out as much as we can about the reactor vessel. It has several phases to it. The phases talk about the actual chemistries, the RT NDTs, the actual fracture toughness, enhanced ISI techniques and also, in the dosimetry area which is also an important area.

And then I just want to conclude with a couple of statements here that Table P-1 at the end of the report, it was suggested to me that -- I think the statement was made here that the staff get the values of RT NDT from the licensees. I believe that is one of the recommendations of Section 10, Number 4. Moving back to what I suggested before is that screening criteria is fine, but as we learn more and more about it, maybe we should be a little flexible with it, and a few years from now that criterion may change up or down as we know more.

Again, the B&W owners, we have basic differences in the reactors, and I think they were acknowledged in the report, in Section 10. I guess there is something that is being looked at in that area. And the last comment, I guess, is that again I think the staff's report is good and very comprehensive. I think the approach is good. I have suggested a couple of areas to be clarified here, and I think maybe a little bit more time -- we have only had about a week to look at this thing -- a little bit more time might be appropriate to get some more detailed comments. That is all I want to say, and I will answer any questions.

MR. BENDER: Are there any questions?

(No response.)

MR. BENDER: I would like to pose a couple. I think we have not seen from the B&W Owners Group an assessment of operator actions and the diagnostic information that is needed to make sure that the operator actions are the correct ones to avoid pressurized thermal shock. Is there a study of how the operators will respond, what kind of symptoms they have available to them, and some kind of emergency guidelines that are understandable to the operators?

The last time I heard somebody discuss this, it was pretty much the operator's judgment as to which way to move.

MR. SHORT: I am not sure if you are familiar with the B&W ATOG program.

MR. BENDER: I can say I am not familiar with it.

MR. SHORT: It is a program that has been under way ever since TMI, and it is addressing the issues of thermal shock. There have been guidelines issued to all of the B&W utilities. I think the closest thing that could come to what you are looking for is simulator training.

MR. BENDER: Well, that is not very good unless the simulators are capable of simulating the kind of conditions that we have expressed concern for. I am not convinced that the B&W Owners Group has really taken the problem as seriously as they ought to take it, and I think the fact that you are giving the kind of answers you are giving to what seems to be a crucial issue of operator behavior suggests that you ought to go back and give some more thought to the problem.

I personally would like to see a much more positive response than just, we think maybe simulator behavior is the right way to study this thing. I still have [illegible] doubts about whether the simulators are capable of simulating the situation.

MR. SHORT: We don't want to have any actual PTS events. What I am saying is that the owners have recognized this for quite a while, the B&W owners. They do have an active ATOG program under way.

MR. BENDER: Isn't it true that the 30-year presumption you have on life is very much dependent upon operators doing certain things in a certain way?

MR. SHORT: Yes. We do take credit for operator action.

MR. BENDER: And if I were to take issue with your assumptions, I might prove that the 30 years is more like three. If that were the case, what would you do?

MR. SHORT: Well, I don't think that is the case, but if you could prove it --

MR. BENDER: I understand that.

MR. SHORT: If you could prove it, I would like to know how.

MR. BENDER: Well, there is a lot of subjective judgment in this business.

MR. SHORT: There are a lot of uncertainties, and we came up with a list one time that was almost 20 areas of potential uncertainty, and one of the reasons why we are talking here so long about it, I guess, is because if you go worst case in every one of those areas, you get yourself into a problem, and we have done that, and so I am not standing back and saying we haven't done it. We have done that.

MR. SHEWMON: Sir, one of the other reasons we are here is that B&W plants are real hot-shot items, and they have generated enough of these events so that we realize it is a problem. So maybe they are a lot better than they used to be, but we are here because of B&W plants to a fair degree.

Now, you come out looking good by the staff's current criteria, but your reactors don't, or your automatic control systems don't, and maybe you can handle them now, and you've got your operators trained to where they can, but it is not immediately obvious, given the past experience.

MR. BENDER: Well, let's not overreact to the statements we are making. I guess my reaction is to say, you are pretty blase about the whole thing, and it is discomforting to hear it presented that way. Perhaps we have misinterpreted the view, but I think, just speaking for myself, I would be happier to see a more positive kind of attitude rather than one which says, look, we have shown there is 30-year life in this plant, go away and leave us alone.

MR. SHORT: That is not the attitude, and I didn't want to get that across. That is why I showed the last slide, that we were doing things actively to stay ahead of the issue. This is not the only vessel integrity concern, and I think there has been a lot of thought that has gone into it, and I think the B&W owners are taking it seriously.

MR. BENDER: Thank you. Are there questions?

(No response.)

MR. BENDER: Does the working group have any other thoughts, or would the staff like to add anything?

MR. HANAUER: Mr. Chairman, I wouldn't like to add anything, but I would like to get either now or later some guidance on what you want in the full committee meeting in October.

MR. BENDER: Well, that is a good thought. I think the Committee was very well disposed toward the presentation you made this morning, and while it might not be necessary to present all of it, I think it would be helpful if you would provide as condensed a version as you could.

MR. HANAUER: Do you want the Committee to hear either the Pacific Northwest or the Owners Groups?

MR. BENDER: Well, I was going to get to that point separately. I think we need some kind of story about the capabilities of a nondestructive examination presented in a way that is more explicit than we heard today, and one which speaks to what can be done about the existing vessels, and states explicitly what the uncertainties are associated with nondestructive examination.

With the knowledge we have of the vessels today, I think that would probably be enough to enlighten the Committee from the staff's point of view.

MR. ZUDANS: I think at that point it would be important to analyze the state of vessel cladding and see which of these methods potentially could be used. Nobody knew what the state of cladding is.

MR. BENDER: What I am hoping is that the staff would be able to get somebody to tell us what they know and what else they can learn in the period of time between now and X, whatever that is. I think I will leave it to the discretion of the Owners Groups as to whether they would like to make a presentation.

Now, we have some time available for them.

MR. IGNE: We have three hours, and I think Steve wanted two hours, and that left an hour for the others.

MR. MORRIS: I just want to make a comment. We do not have any, I believe need is the right word, to make a presentation at the full Committee meeting. I don't really see where it would serve any purpose for us unless it would help someone else.

MR. BENDER: My suggestion is, the Owners Groups have representatives there to respond to questions, but if they would like to make presentations, we would allocate some time. Does anybody see a need for another kind of presentation? If not, let me suggest first that those consultants that feel like they would like to hear this again, they are cordially invited to attend the full Committee meeting, but what I intend to do is to convey to the Committee the collective recommendations that we have had, and hopefully they will be taken in the spirit of thought in which they were developed, and they will be passed along in an appropriate way.

MR. SHEWMON: Your criteria at this point in time take no account of the kind of plant or the kind of control system or the kind of experience? Is that true?

MR. HANAUER: That is quite correct.

MR. SHEWMON: Well, maybe at the full Committee meeting we could discuss it further, then, whether you think that properly reflects the effective mass or whatever you use or the acceleratability of these things.

MR. HANAUER: It does not. We just haven't seen enough data to do anything different up to the present time.

MR. SHEWMON: Thank you.

MR. BENDER: If there are no other comments, this seems like a good time to adjourn this meeting. I thank everyone for coming and contributing.

(Whereupon, at 4:45 p.m., the meeting was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS/Metal Components Working Group

Date of Proceeding: September 30, 1982

Docket Number: ____________________

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Ray Heer

Official Reporter (Typed)

Ray Heer

Official Reporter (Signature)

PRESSURIZED THERMAL SHOCK

PRESENTATION TO

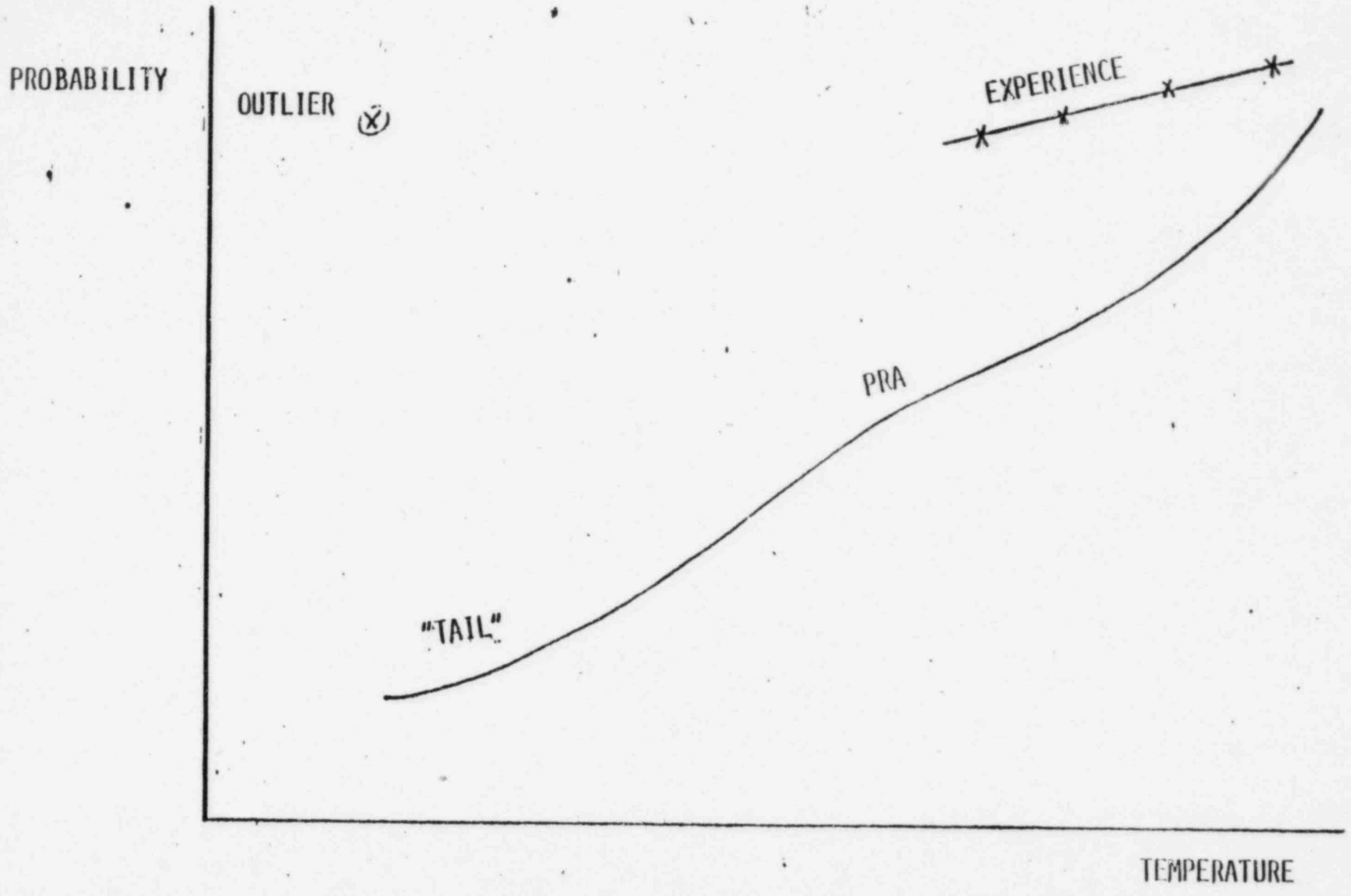
ACRS METAL COMPONENT SUBCOMMITTEE

SEPTEMBER 30, 1982

STEPHEN H. HANAUER

OUTLINE

- o GENERAL APPROACH
- o EVALUATION OF EXPERIENCE
- o SCREENING CRITERION
- o APPLICATION TO PLANTS
- o PROBABILISTIC EVALUATION
- o CONCLUSIONS AND RECOMMENDATIONS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OUTLIER ⊗
EXPERIENCE
PRA
"TAIL"
TEMPERATURE

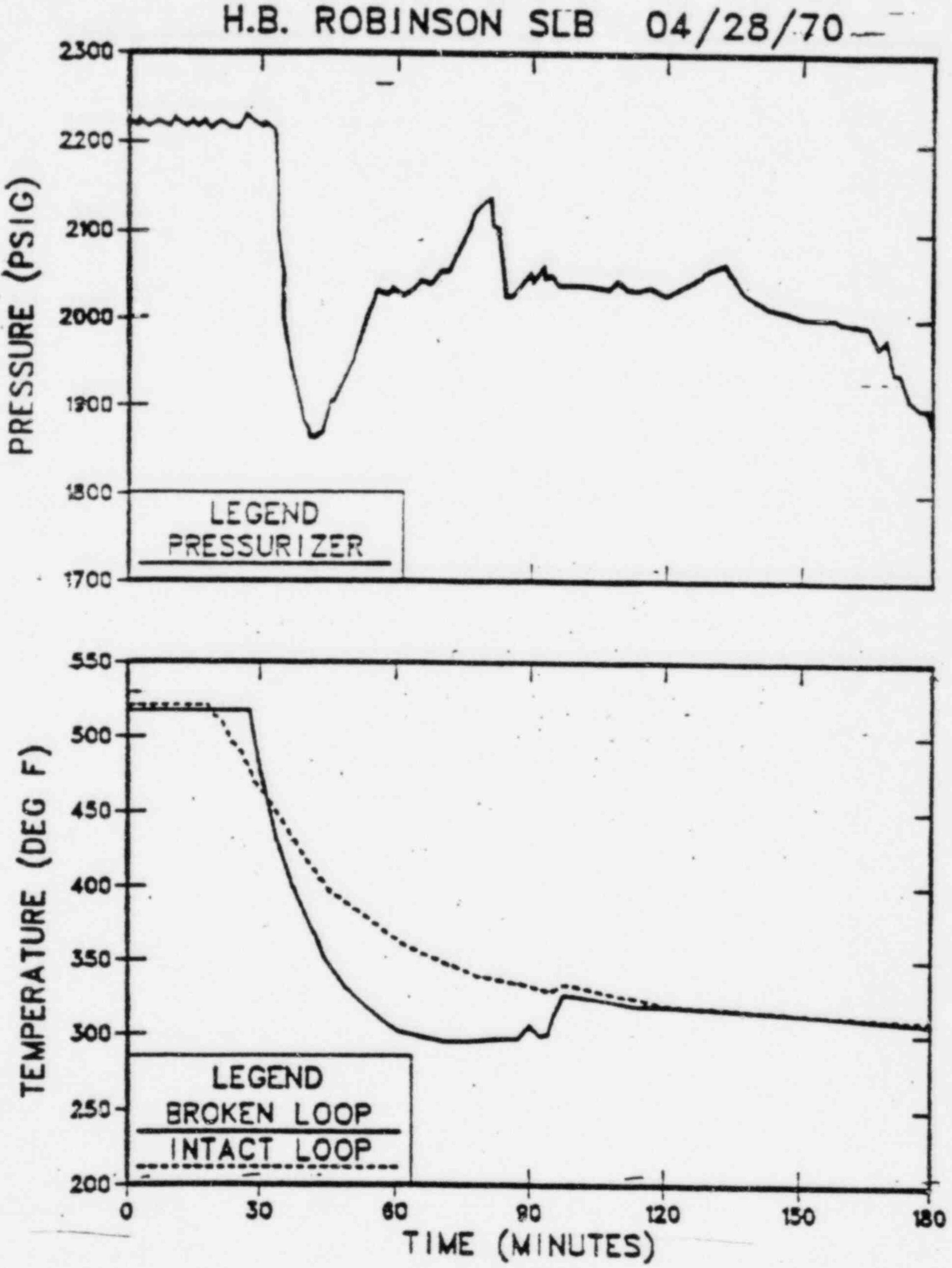
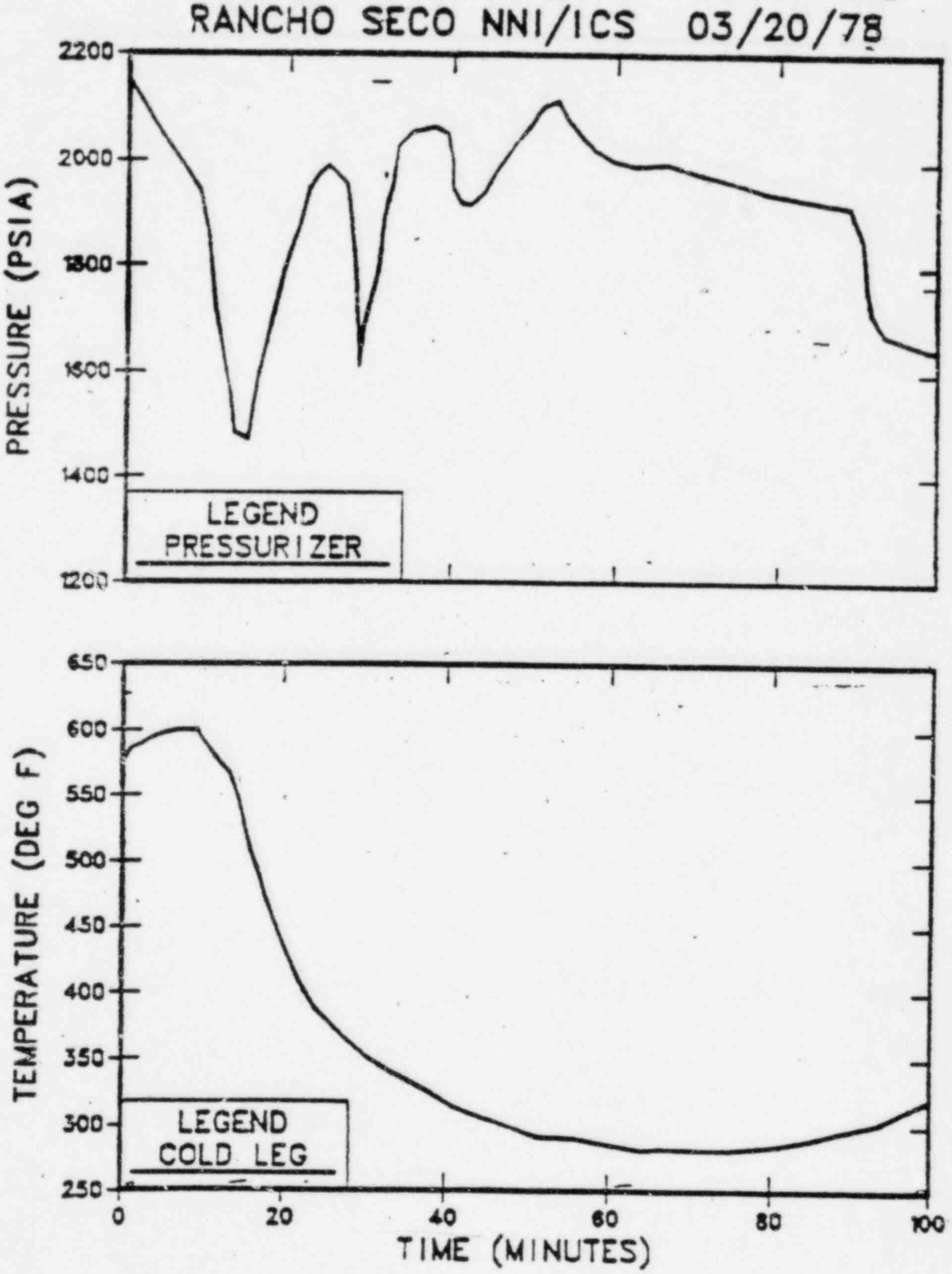

H.B. ROBINSON SLB 04/28/70
PRESSURE (PSIG)
2300
2200
2100
2000
1900
1800
1700
LEGEND
PRESSURIZER
TEMPERATURE (DEG F)
550
500
450
400
350
300
250
200
LEGEND
BROKEN LOOP
INTACT LOOP
0
30
60
90
120
150
180
TIME (MINUTES)

FIGURE 2-1



RANCHO SECO NNI/ICS 03/20/78

FIGURE 2-7

R.E. GINNA SGTR + PORV 01/25/82

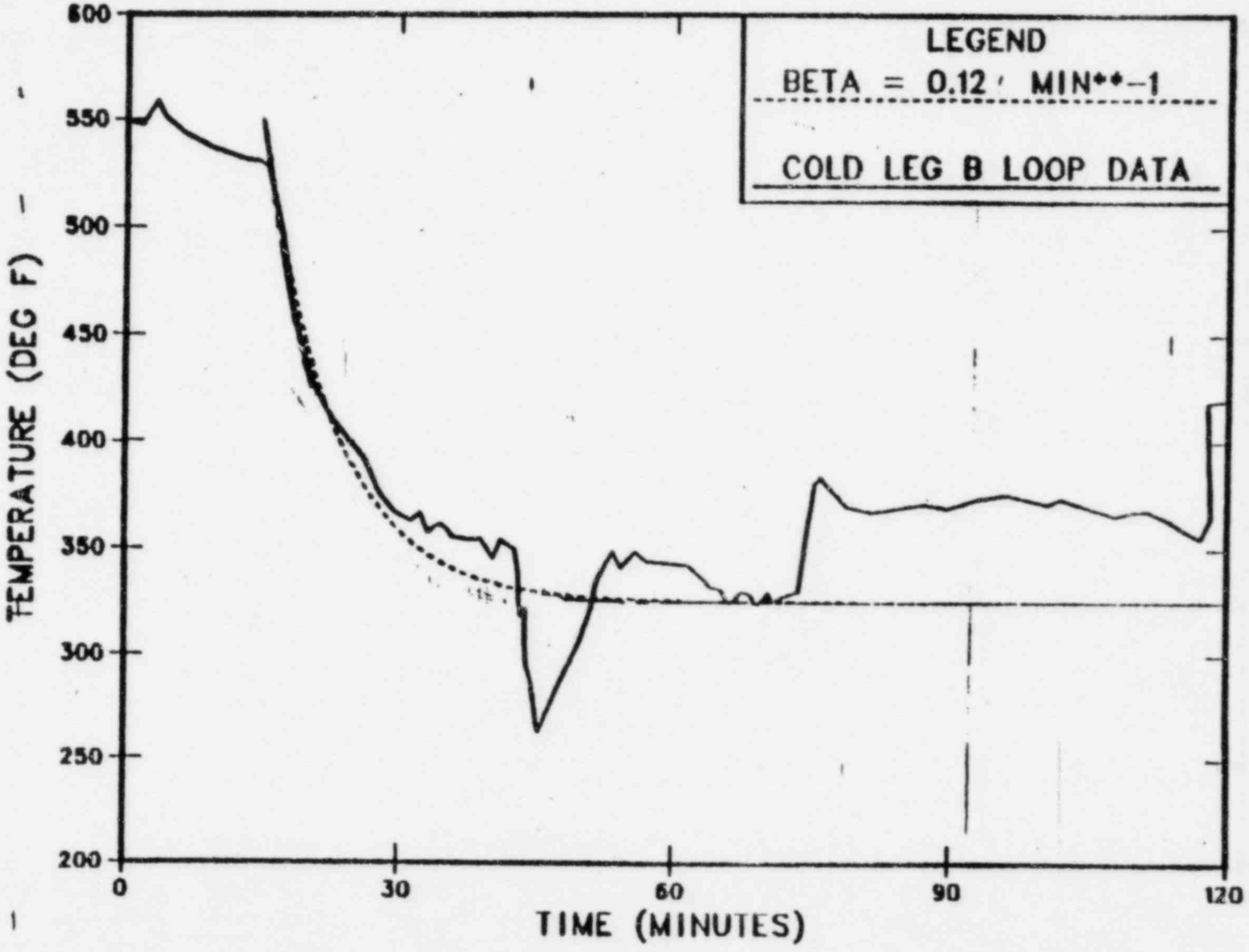
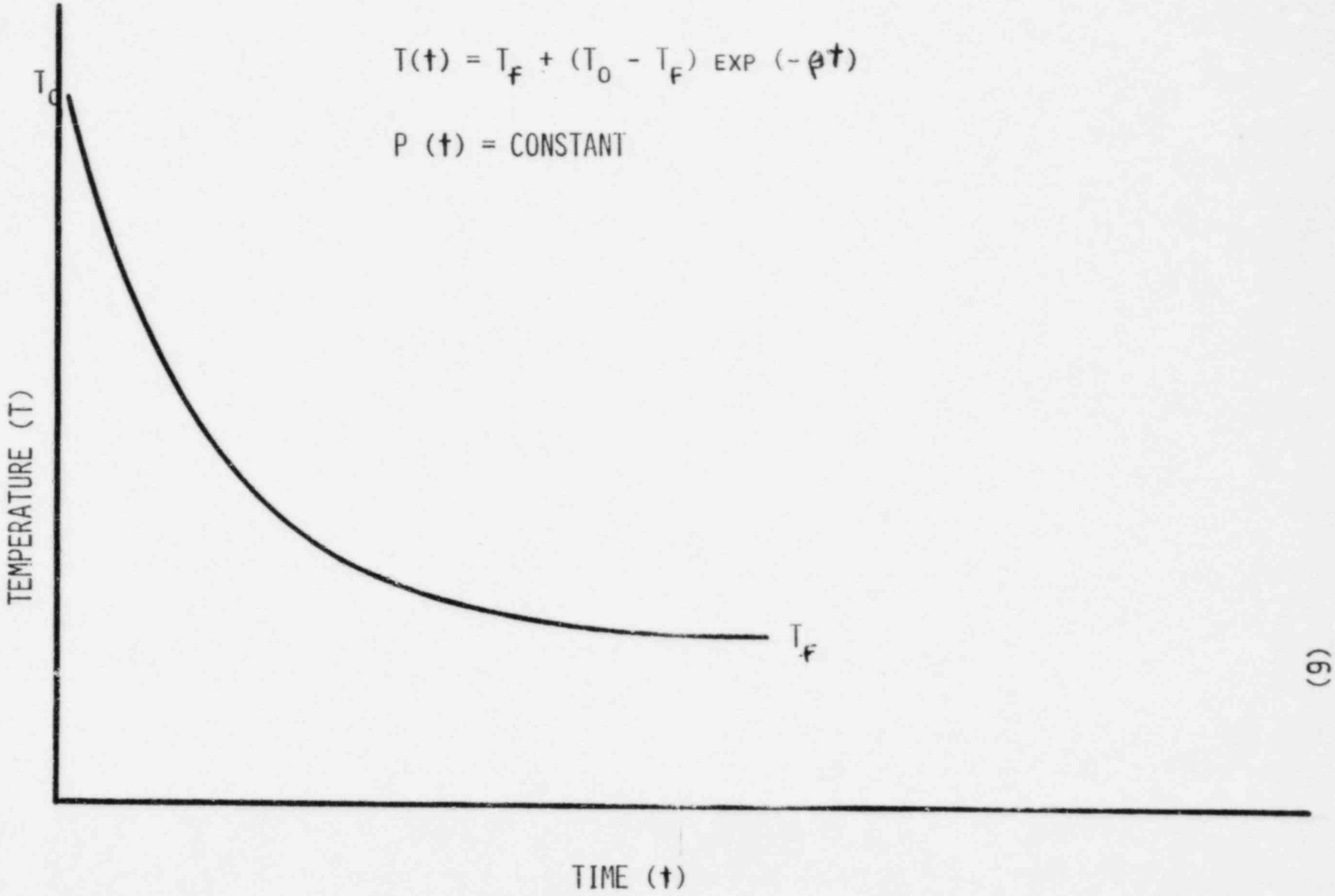


FIGURE 2-12


$T(t) = T_f + (T_0 - T_f)\ \text{EXP}\ (-\beta t)$
$P\ (t) = \text{CONSTANT}$
T_0
T_f
TEMPERATURE (T)
TIME (t)

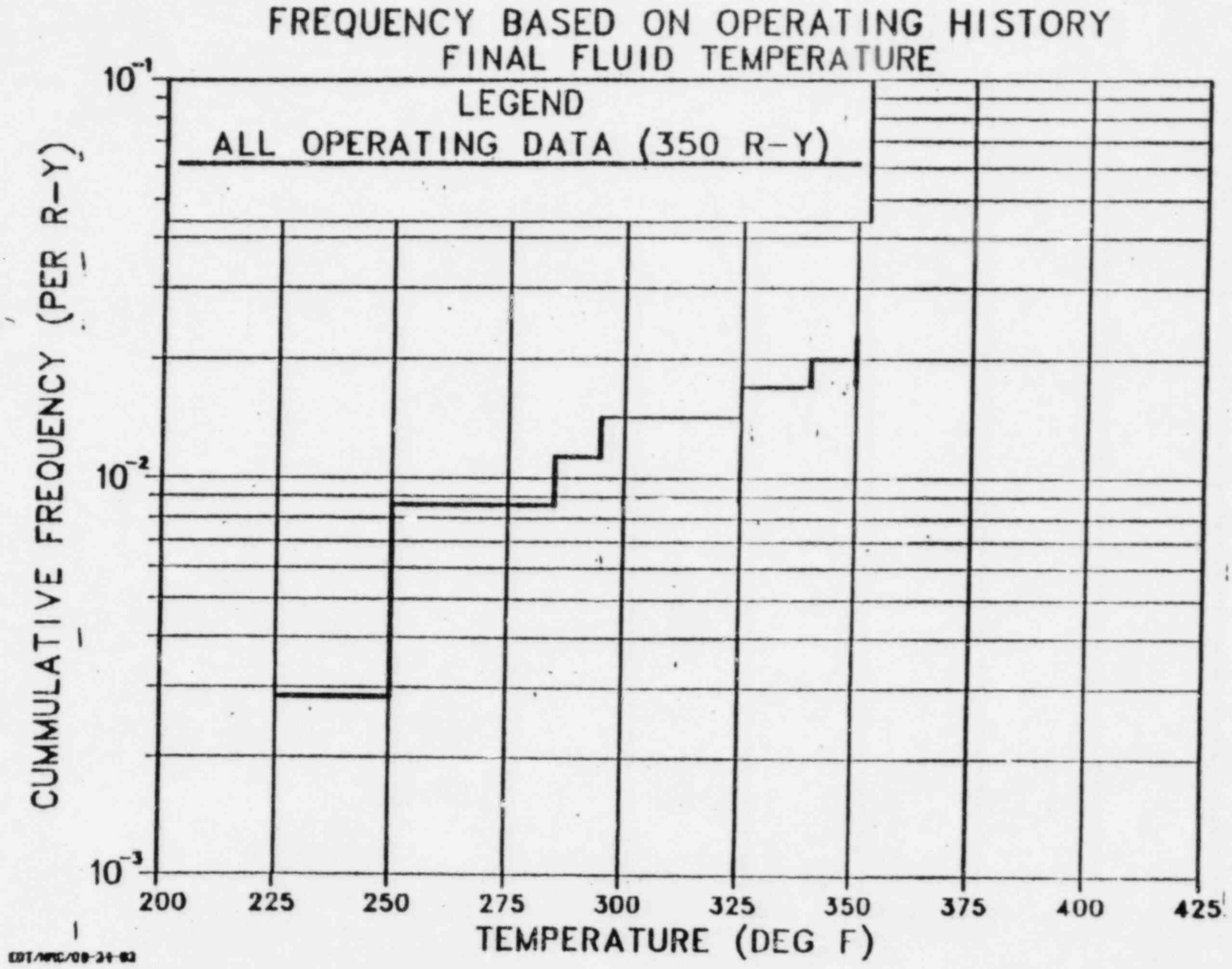
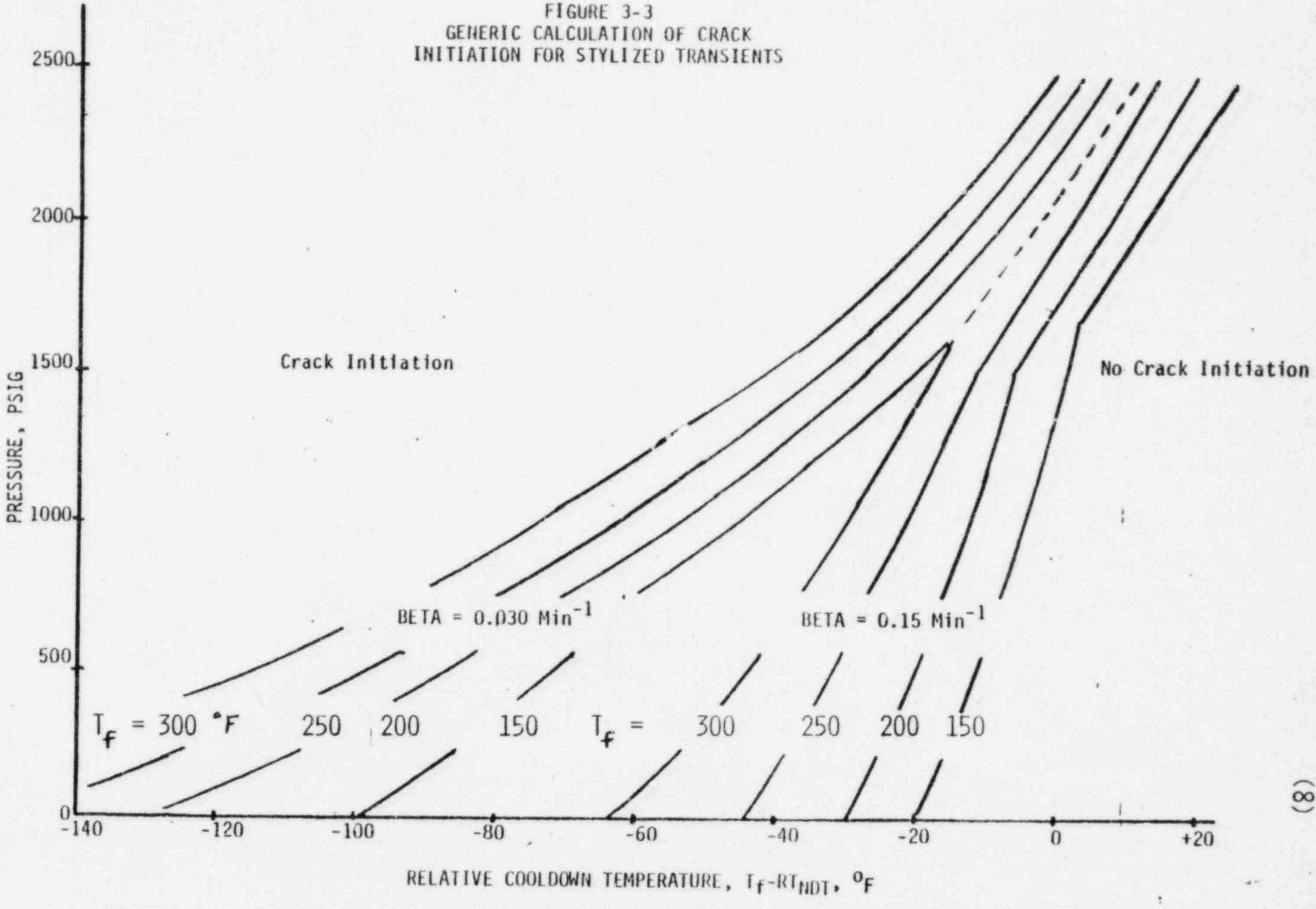
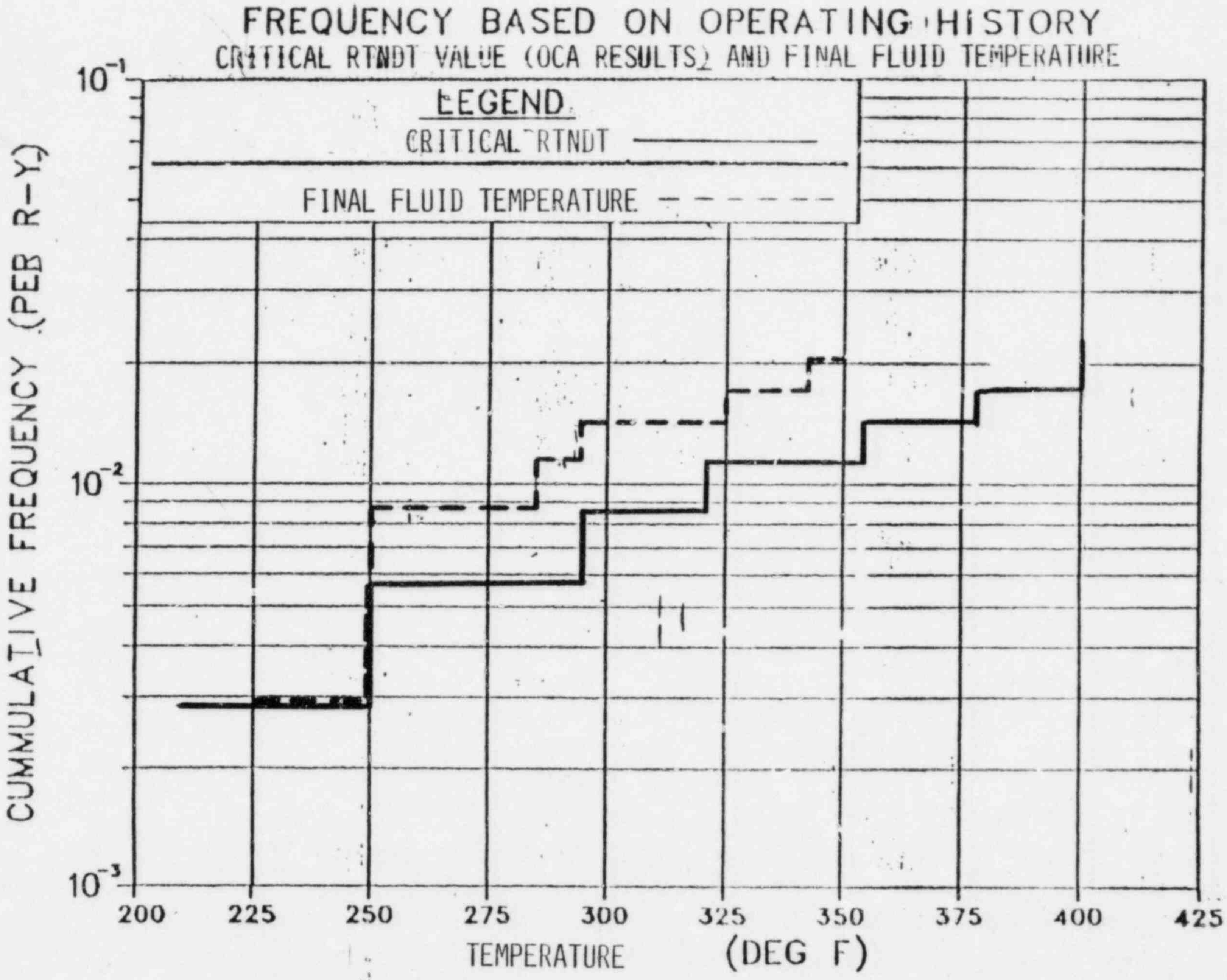

FREQUENCY BASED ON OPERATING HISTORY
FINAL FLUID TEMPERATURE
LEGEND
ALL OPERATING DATA (350 R-Y)
CUMMULATIVE FREQUENCY (PER R-Y)
10^{-1}
10^{-2}
10^{-3}
200
225
250
275
300
325
350
375
400
425
TEMPERATURE (DEG F)
EDT/NRC/08-24-83

FIGURE 2-14

(7)

FIGURE 3-3
GENERIC CALCULATION OF CRACK
INITIATION FOR STYLIZED TRANSIENTS





FREQUENCY BASED ON OPERATING HISTORY

CRITICAL RTNDT VALUE (OCA RESULTS) AND FINAL FLUID TEMPERATURE

FIGURE 4-1

SCREENING CRITERION

o LONGITUDINAL CRACK	270°F
o CIRCUMFERENTIAL CRACK	300°F

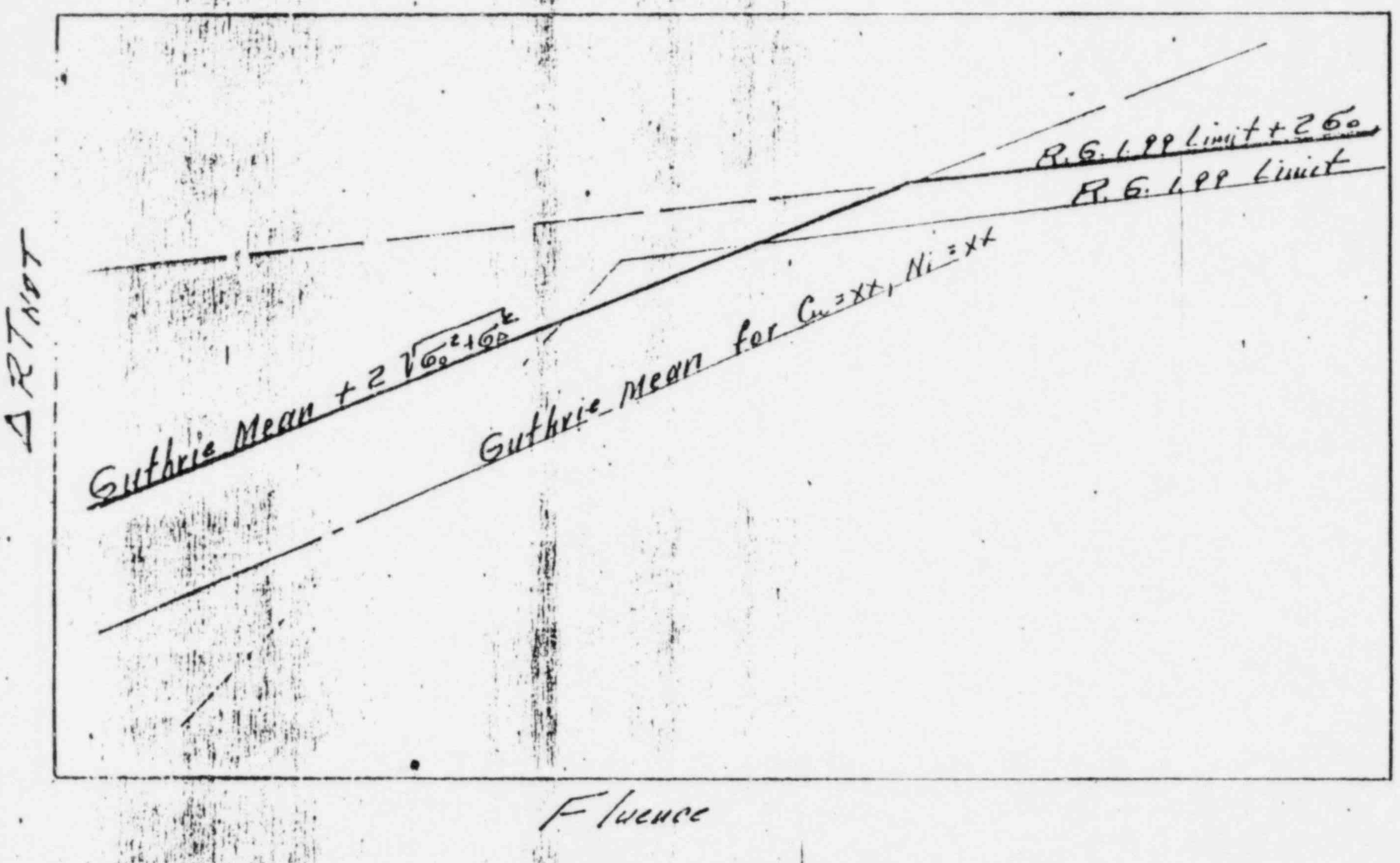
EVALUATING A SPECIFIC VESSELL

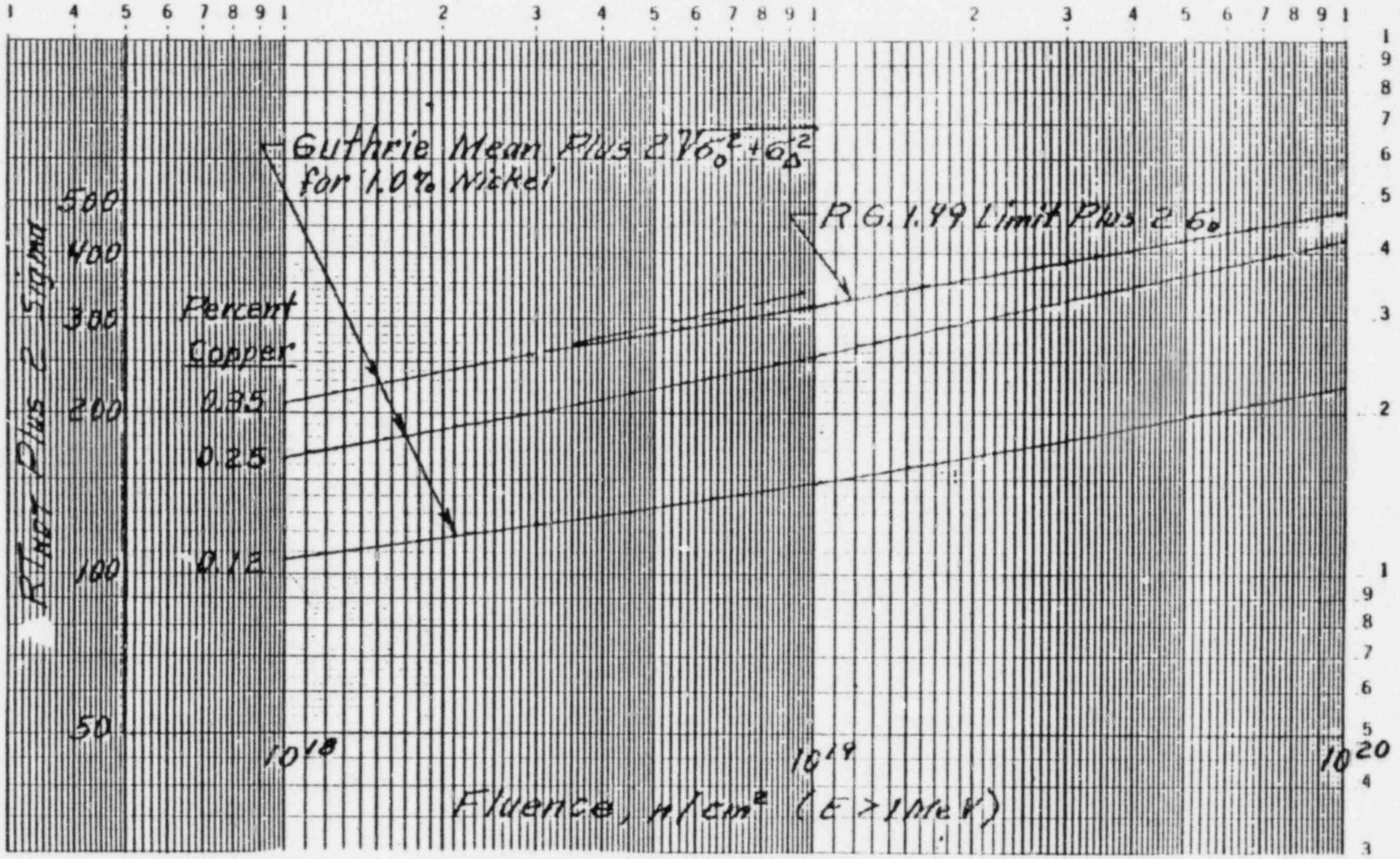
$RT_{NDT} = RT_0$ (BEST ESTIMATE)

$+ \Delta RT$ (BEST ESTIMATE - GUTHRIE)

$+ 2\sqrt{\sigma_0^2 + \sigma_\Delta^2}$

LIMITED BY RG 1.99 + $2\sigma_0$


R. G. 1.99 Limit + 2σ₀
R. G. 1.99 Limit
Guthrie Mean + 2 √(σ₀² + σ_D²)
Guthrie Mean for Cu = xx, Ni = xx
ΔRT_NDT
Fluence



EXAMPLE OF NRC PRESCRIPTION FOR RT_{NDT} (FOR ASSUMED RT_{NDT} (o) OF 0°F)

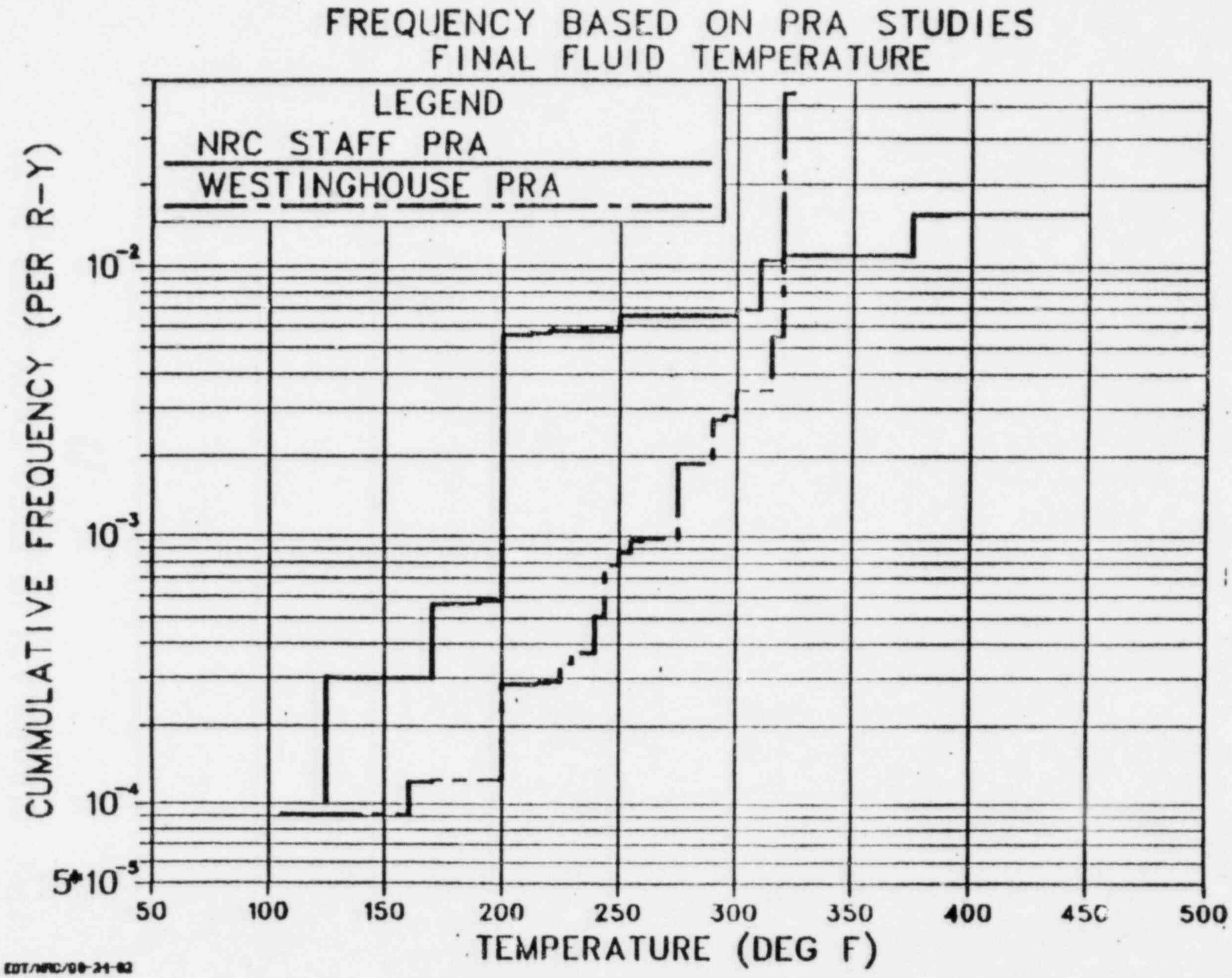
(14)

PLANT STATUS AS OF DECEMBER 31, 1981

PLANT		RT_0, °F	ΔRT, °F	$2\sqrt{\sigma_0^2+\sigma_\Delta^2}$	RT_{NDT}	DATE SCREENING CRITERION EXCEEDED
ROBINSON 2	CIRCUM	-56	295	34	281	February, 1987
W/CE	AXIAL	-56	151	59	154	
FORT CALHOUN	CIRCUM.	-56	264	34	242	April, 1990
CE/CE	AXIAL	-56	248	34	226	
TURKEY POINT 4	CIRCUM.	0	200	59	259	July, 1988
W/B&W	NO AXIAL					
TURKEY POINT 3	CIRCUM.	0	200	59	259	July, 1988
W/B&W	NO AXIAL					
MAINE YANKEE	CIRCUM.	-56	248	34	226	September, 1995
CE/CE	AXIAL	-56	238	34	216	
CALVERT CLIFFS 1	CIRCUM.	-56	135	59	138	October, 1989
	AXIAL	-56	212	59	215	
INDIAN POINT 3	CIRCUM.	74	90	48	212	December 2002
W/CE PLATE GOVERNS	AXIAL	74	90	48	212	
YANKEE ROWE	CIRCUM	30	133	48	211	December 2033
W/B&W PLATE GOVERNS	AXIAL	30	133	48	211	

SIGNIFICANT PTS EVENT SEQUENCES

- SECONDARY (STEAM SIDE) DEPRESSURIZATION
- MAIN STEAM LINE BREAK
- SMALL STEAM LINE BREAK (OR STUCK OPEN STEAM GENERATOR SAFETY/RELIEF VALVE)
- SMALL BREAK LOSS-OF-COOLANT ACCIDENT
- STEAM GENERATOR TUBE RUPTURE



EDT/NRC/09-24-82

FIGURE 8-1

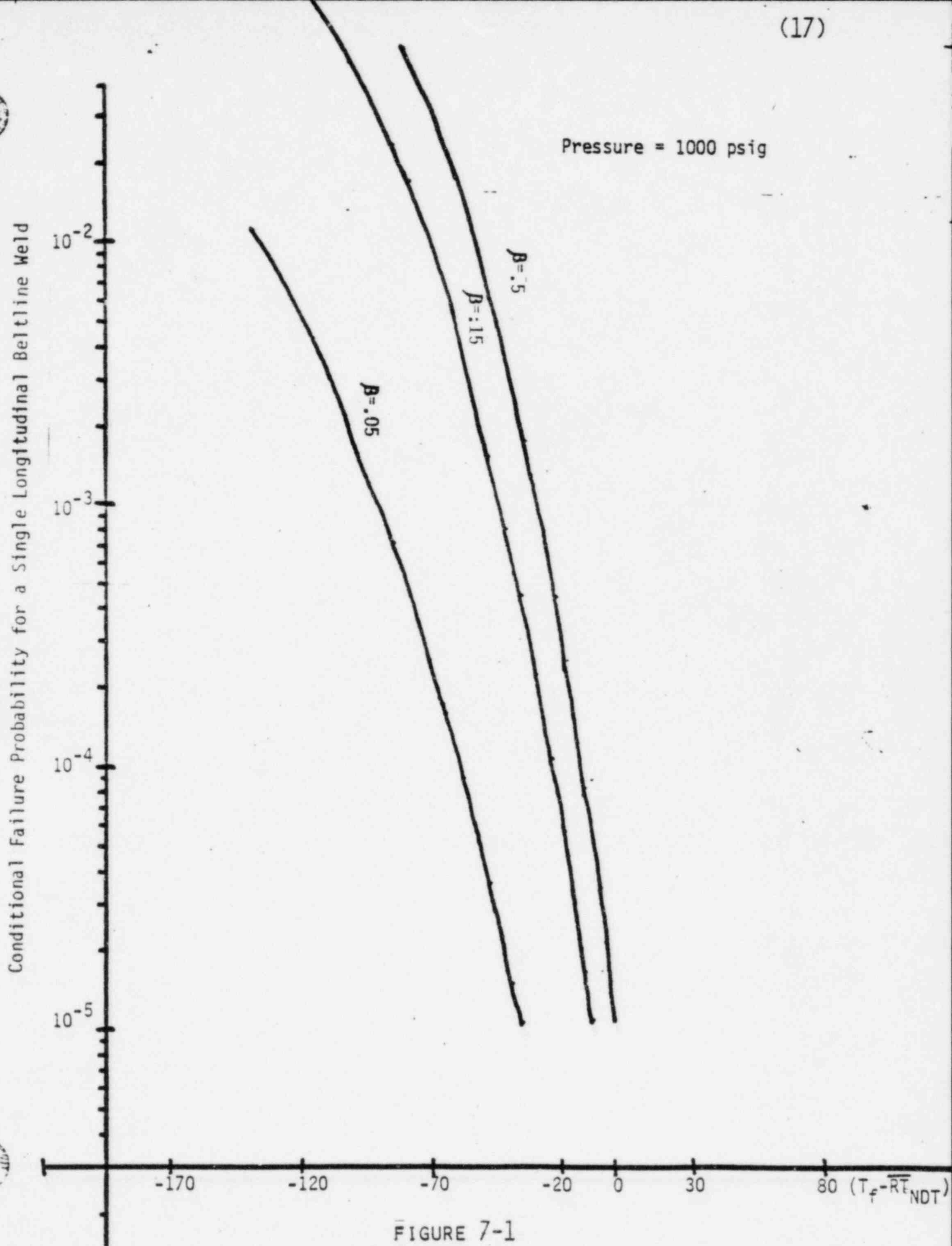


FIGURE 7-1

CFP AS FUNCTION OF T_F-MEAN VALUE OF RT_{NDT}

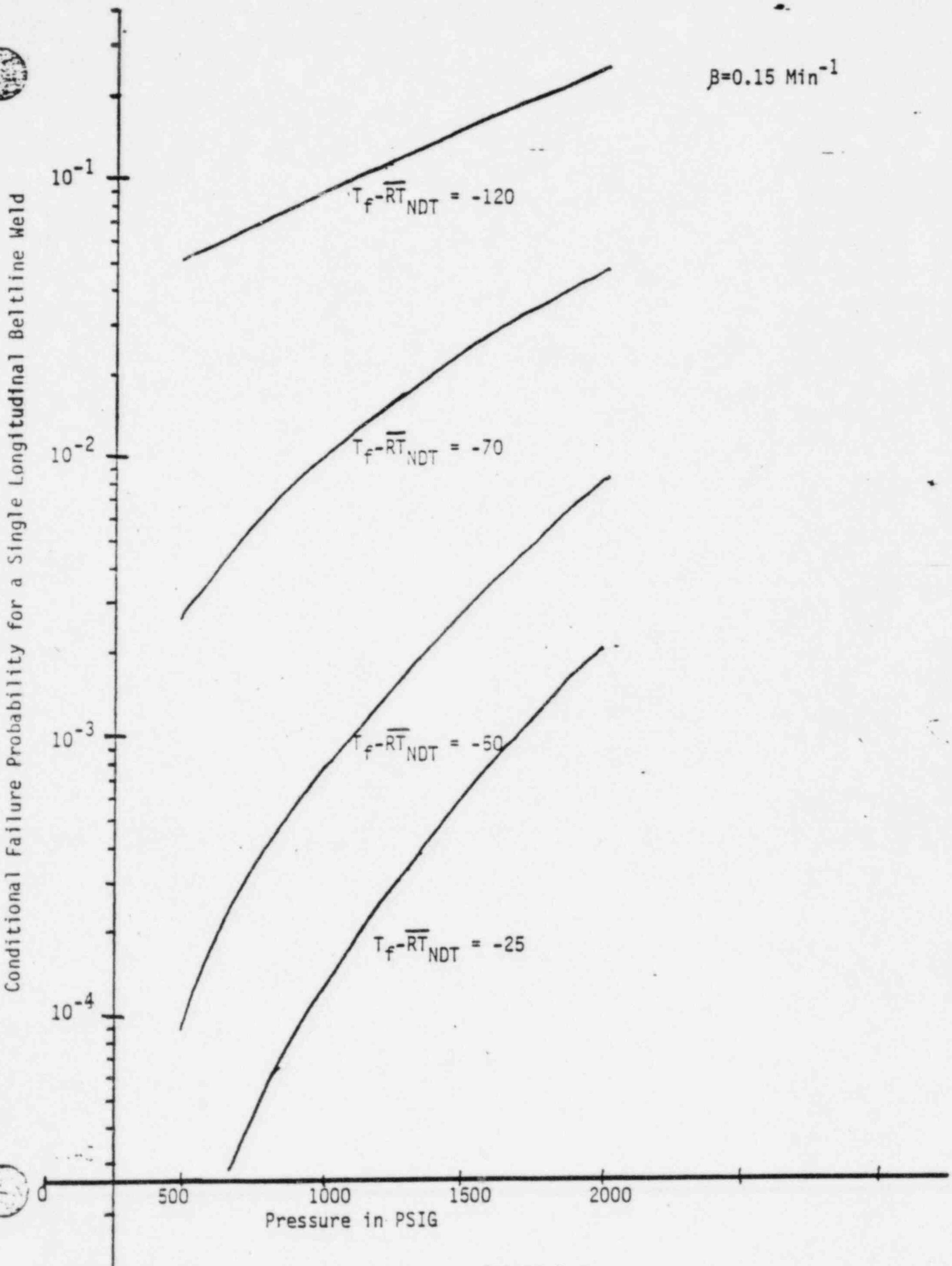


FIGURE 7-2
CFP AS FUNCTION OF PRESSURE

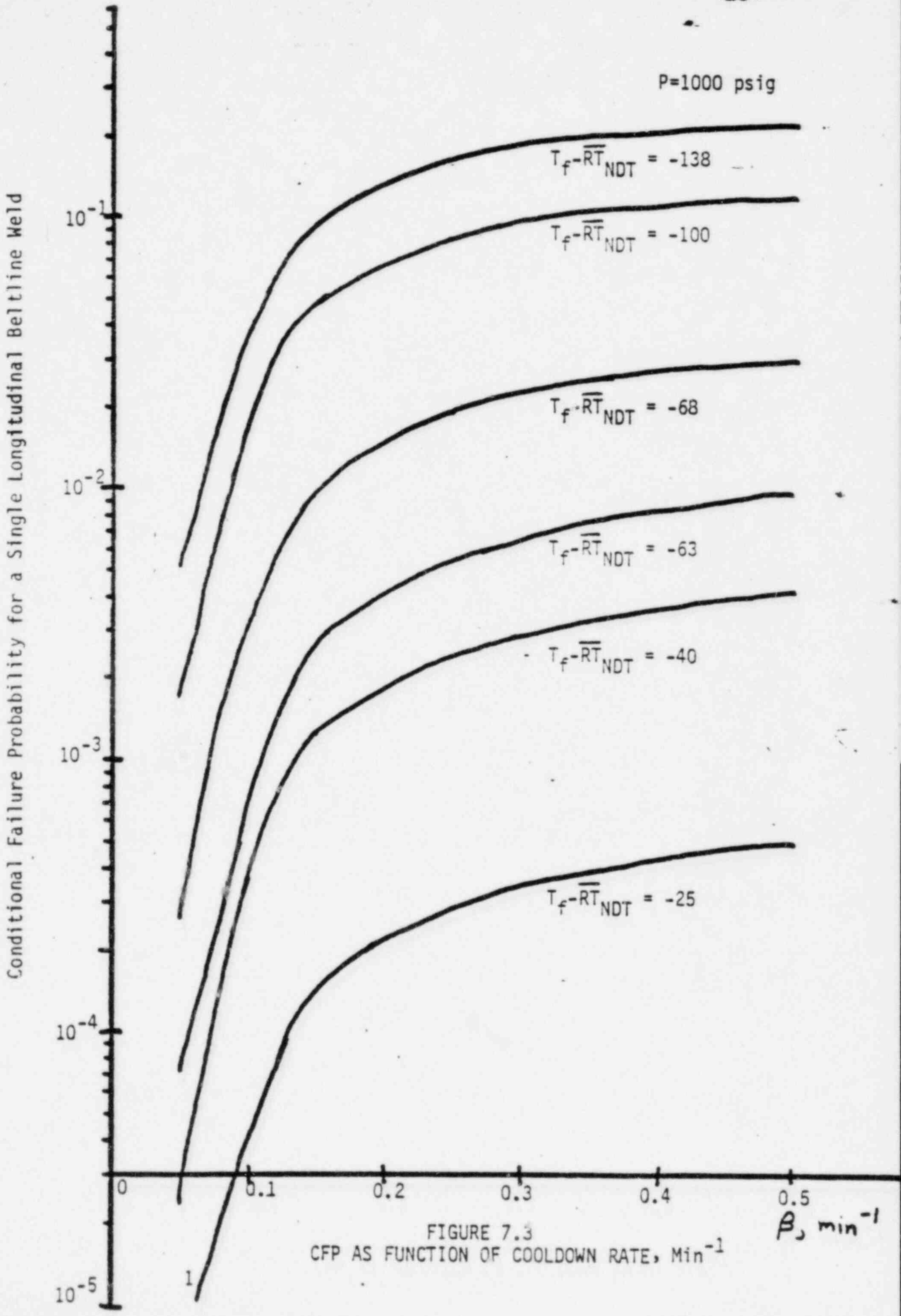
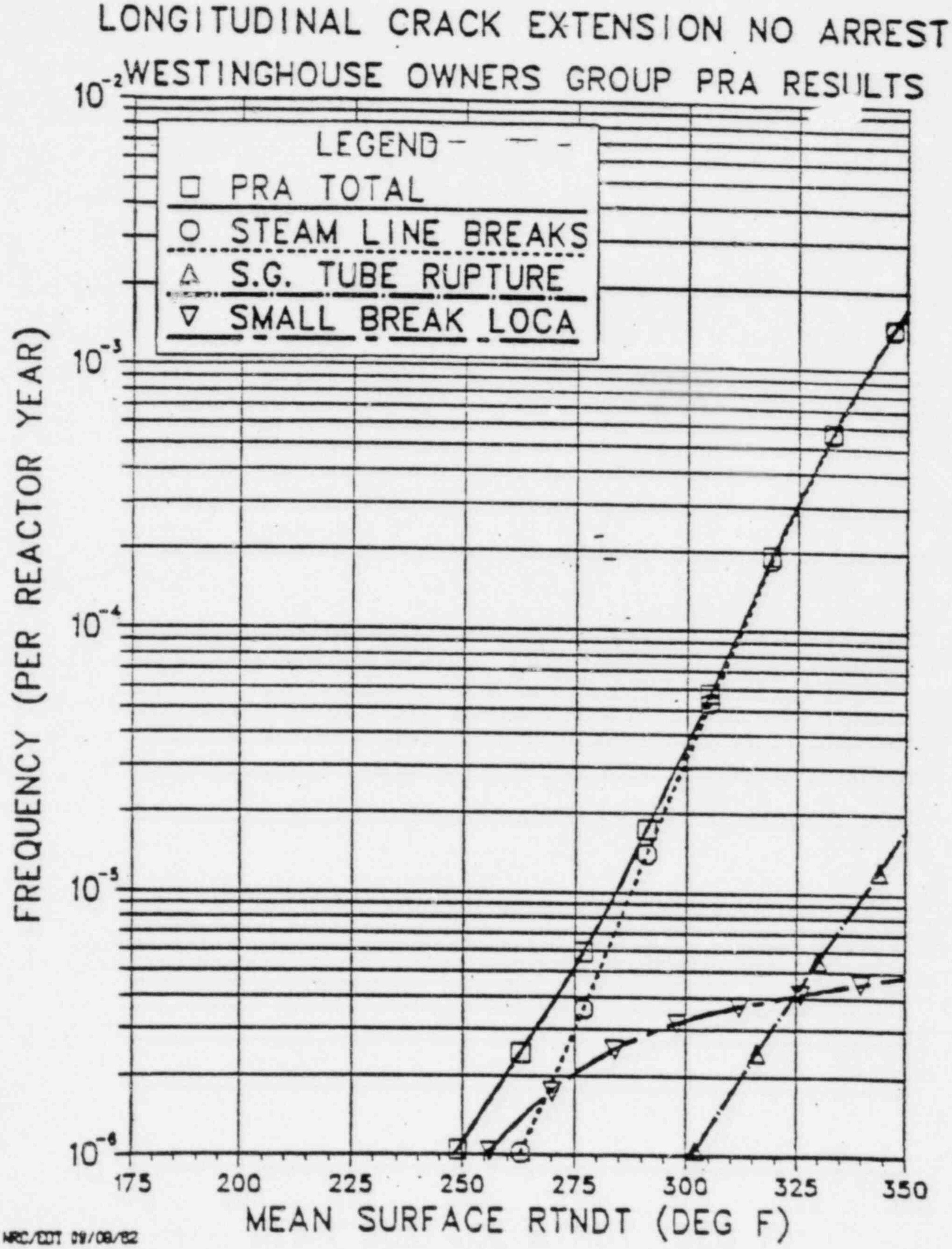


FIGURE 7.3
CFP AS FUNCTION OF COOLDOWN RATE, Min^{-1}



NRC/EDT 09/08/82

FIGURE 8-2

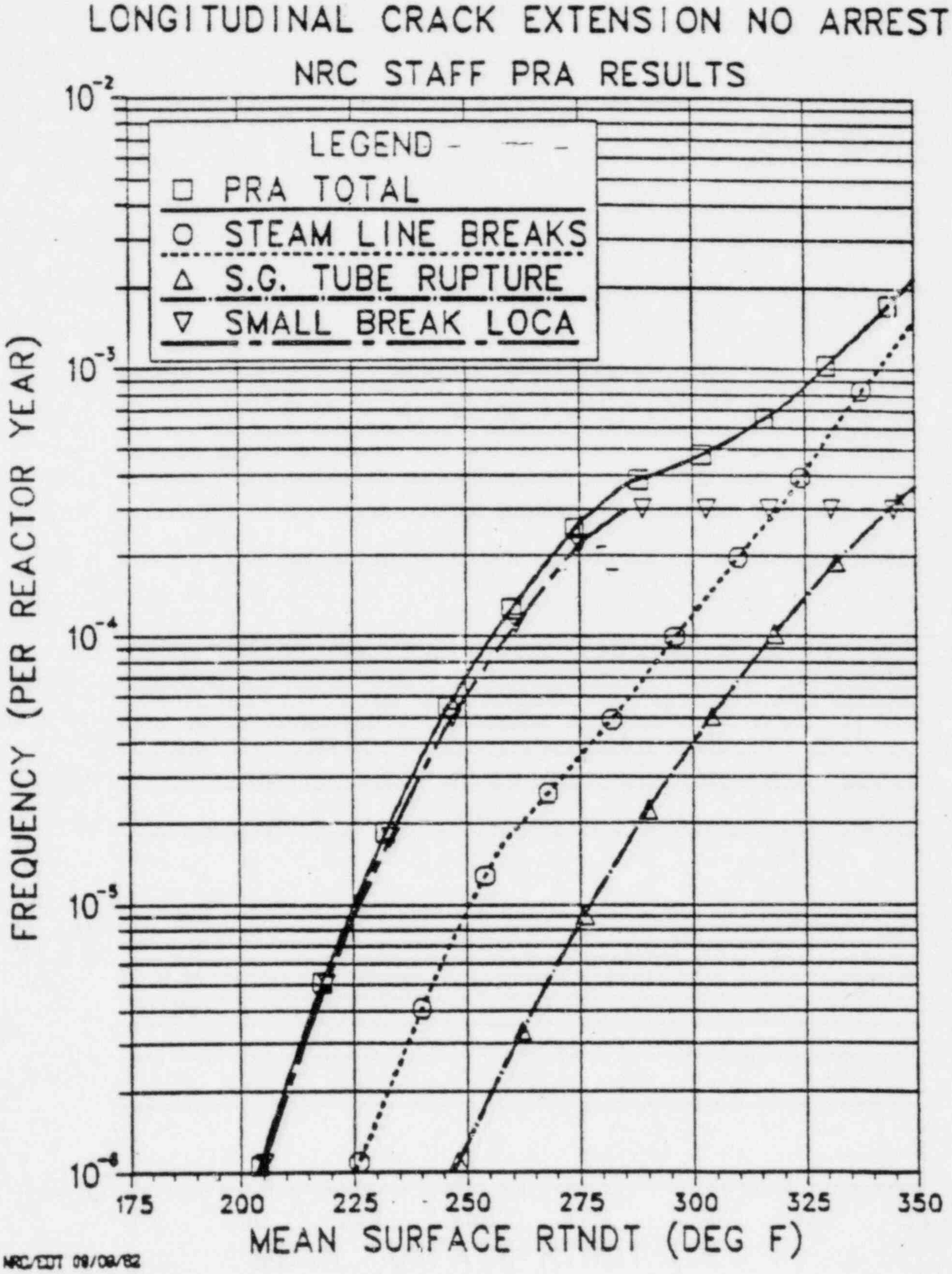


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FIGURE 8-3

LONGITUDINAL CRACK EXTENSION NO ARREST
NRC STAFF PRA RESULTS

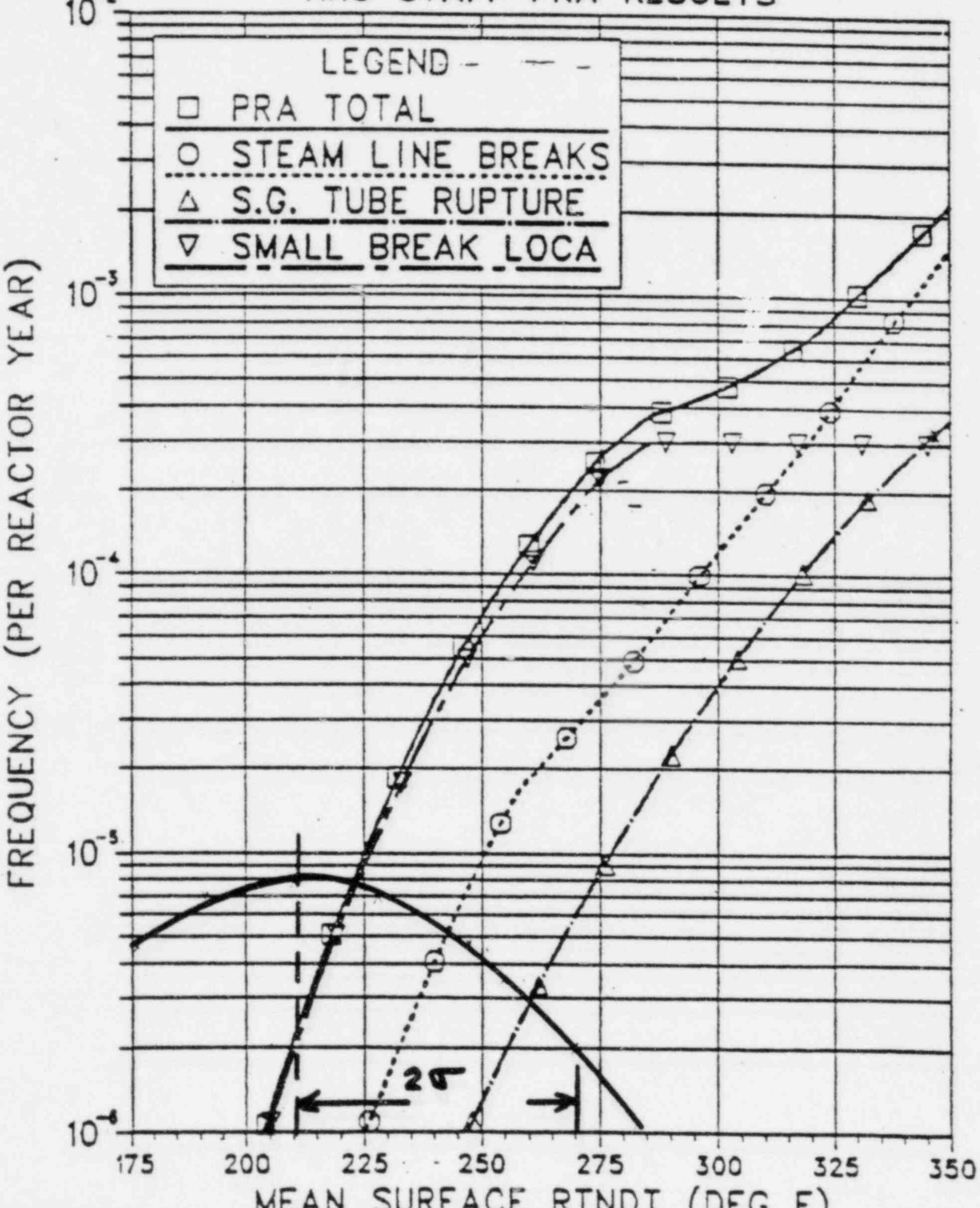

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FIGURE 8-3

SAFETY GOAL

F	VESSEL CRACK
X	CORE MELT IF VESSEL CRACKS
Y	SIGNIFICANT RELEASE IF CORE MELTS

<u>CORE MELT</u> $XF \leq 10^{-5}$

<u>RISK</u> $XFY \leq 5 \times 10^{-8}$

UNCERTAINTIES

- OPERATING EXPERIENCE
- OPERATION ACTIONS
- FLAWS AND CRACKS
- STRESSES
- MATERIAL PROPERTIES
- FRACTURE MECHANICS
- PROBABILISTIC CALCULATIONS

SHORT TERM

1. NO NEED FOR IMMEDIATE ACTION
2. NEED PLANT-SPECIFIC ANALYSIS OF SELECTED PLANTS
3. SCREENING CRITERION
4. ACCEPTANCE CRITERIA FOR FUTURE PLANT-SPECIFIC ANALYSES ARE NEEDED
5. REGULATION CHANGES MAY BE NEEDED

PLANT-SPECIFIC PTS EVALUATION

- EVALUATION OF OVERCOOLING EVENT SEQUENCES
- VESSEL MATERIALS PROPERTIES
- DETERMINISTIC FRACTURE MECHANICS EVALUATIONS
- FLUX REDUCTION PROGRAM
- INSERVICE INSPECTION AND NONDESTRUCTIVE EVALUATION PROGRAM
- PLANT MODIFICATIONS
  - INSTRUMENTATION AND CONTROLS
  - AUTOMATIC DEPRESSURIZATION LOGIC
  - INCREASED EMERGENCY CORE COOLING WATER AND EMERGENCY FEEDWATER TEMPERATURES
- OPERATING PROCEDURES AND TRAINING PROGRAM IMPROVEMENTS
- IN-SITU ANNEALING
- BASIS FOR CONTINUED OPERATION

LONG TERM

1. IMPROVE PROCEDURES AND TRAINING

2. IMPROVE AND EXTEND GENERIC ANALYSIS
    - o INDUSTRY AND NRC
    - o BETTER EVALUATION OF EXPERIENCE
    - o BETTER PROBABILISTIC ANALYSIS EXTEND TO B&W, CE

3. IMPROVE ISI OF HIGH RT_{NDT} VESSELS

4. DECREASE LEAKAGE NEUTRON FLUX

ACRS SUBCOMMITTEE MEETING 9-30-82

PNL AGENDA

• SUMMARY - NUREG/CR-2837 AND NRC PTS SCREENING CRITERION	L. T. PEDERSEN
• EVENTS AND THERMAL HYDRAULIC ISSUES	S. H. BIAN
• MATERIAL PROPERTIES ISSUES	E. P. SIMONEN
• FRACTURE MECHANICS ISSUES	F. A. SIMONEN
• NDE METHODOLOGY AND APPLICATION TO ISI	T. T. TAYLOR

NUREG/CR-2837

PNL Technical Review of Pressurized Thermal Shock Issues

Manuscript Completed: June 1982
Date Published: July 1982

Prepared by
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Prepared for
Division of Safety Technology
Office of Nuclear Reactor Regulation
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555
NRC FIN B2510

ACRS SUBCOMMITTEE MEETING 9-30-82

NUREG/CR-2837 RECOMMENDATIONS

- UPGRADE PROCEDURES, TRAINING AND CONTROL ROOM INSTRUMENTATION ON A SITE SPECIFIC BASIS IN THE NEAR- TO LONG-TERM PERIOD.
- DEVELOP UNIFORM CRITERIA FOR FUTURE ANALYSES USED TO EVALUATE THE EFFECTIVE FULL POWER YEARS (EFPY) REMAINING BEFORE FURTHER CORRECTIVE ACTIONS ARE REQUIRED.
- ADAPT IMPROVED NDE TECHNIQUES DURING FUTURE INSERVICE INSPECTIONS, ISI.

ACRS SUBCOMMITTEE MEETING 9-30-82

NRC SCREENING CRITERION

RT_{NDT} FOR AXIAL WELDS - 270°F

RT_{NDT} FOR CIRCUMFERENTIAL WELD - 300°F

BASIS

- FREQUENCY OF OCCURRENCE FROM OPERATING DATA $\sim 10^{-2}$
- PROBABILITY OF CRACK EXTENSION $\sim 10^{-6}$

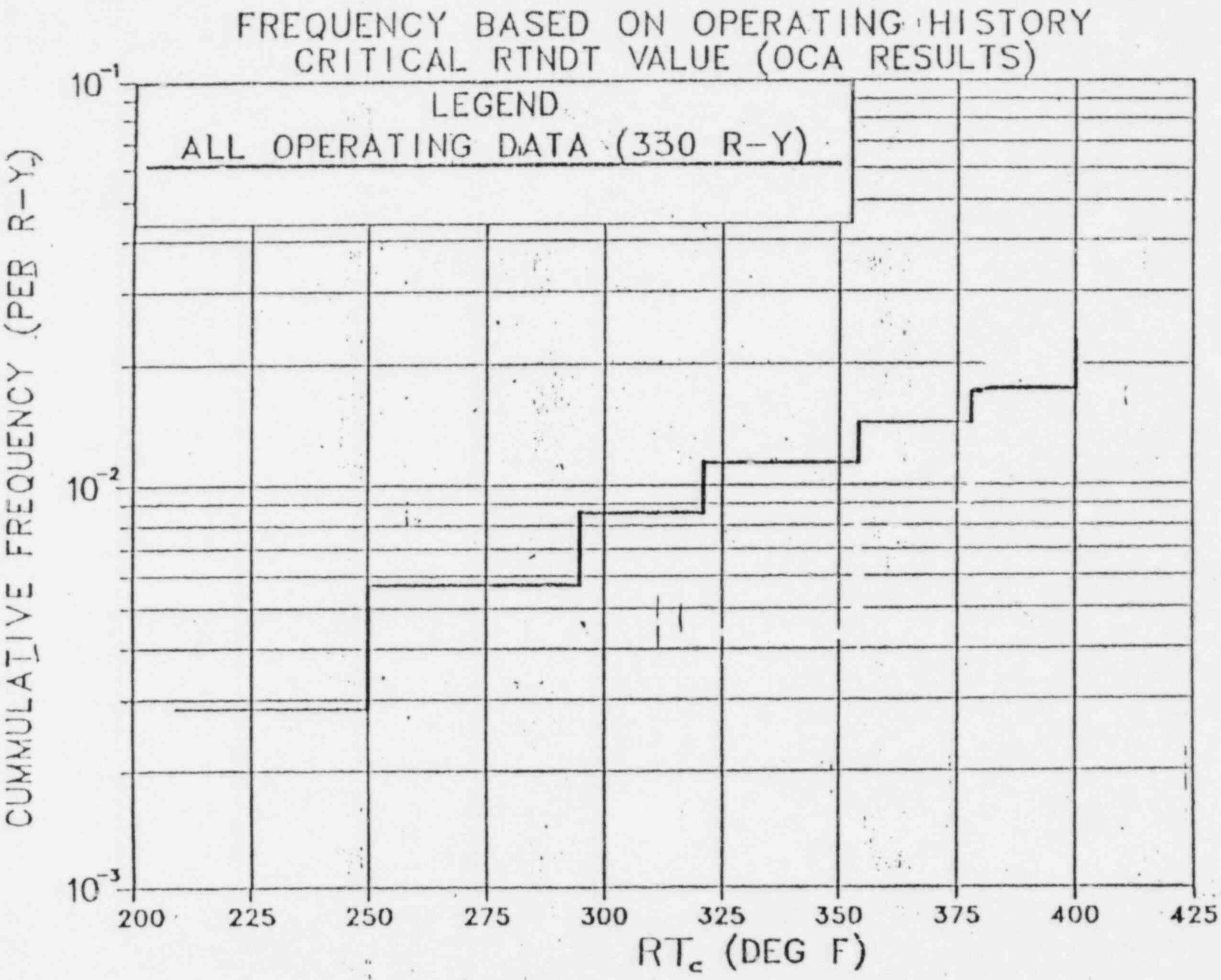
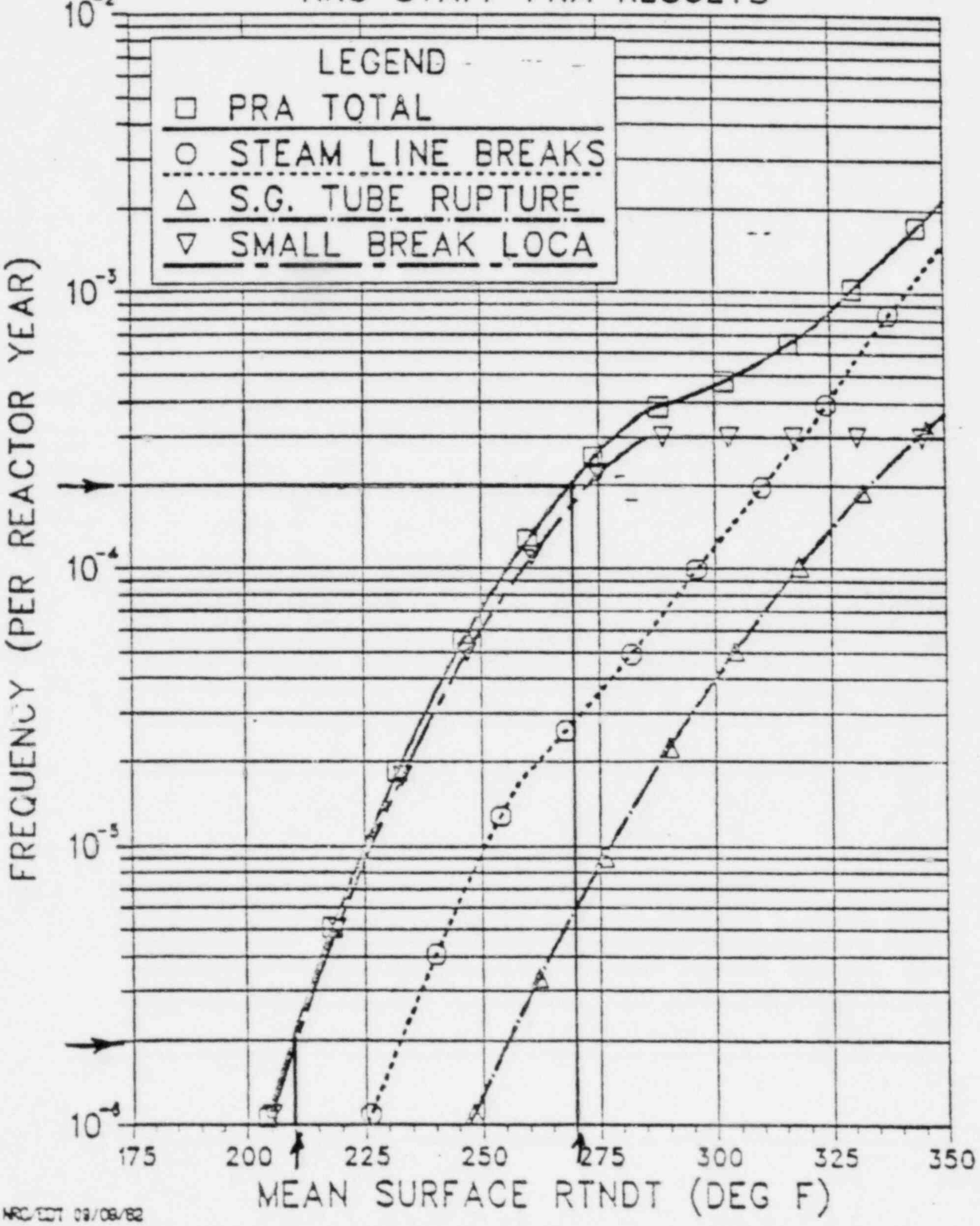


Figure 4-1

DRAFT

DRAFT


LONGITUDINAL CRACK EXTENSION NO ARREST
NRC STAFF PRA RESULTS

NRC/EDT 09/08/82

FIGURE 8-3

DRAFT

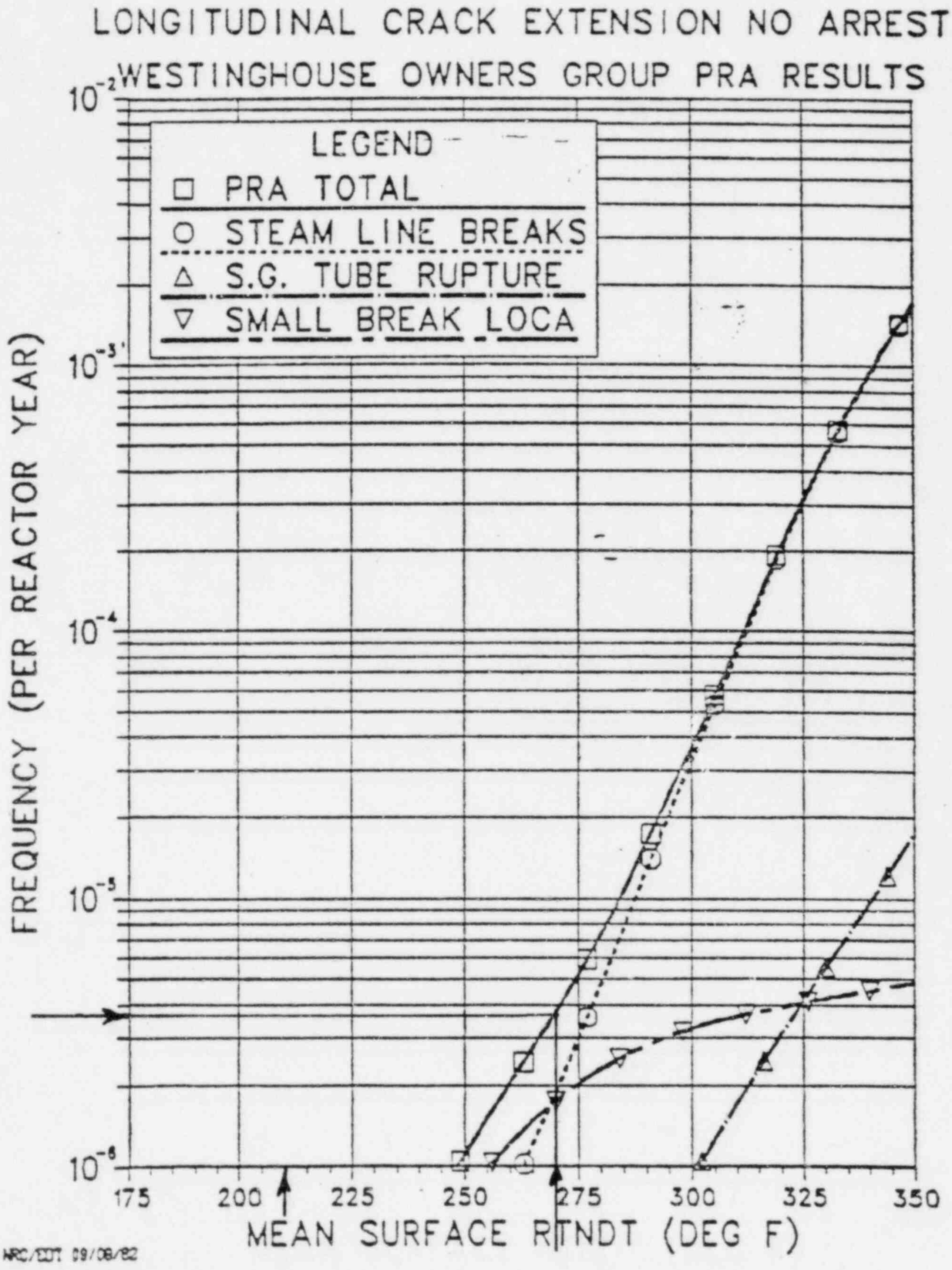


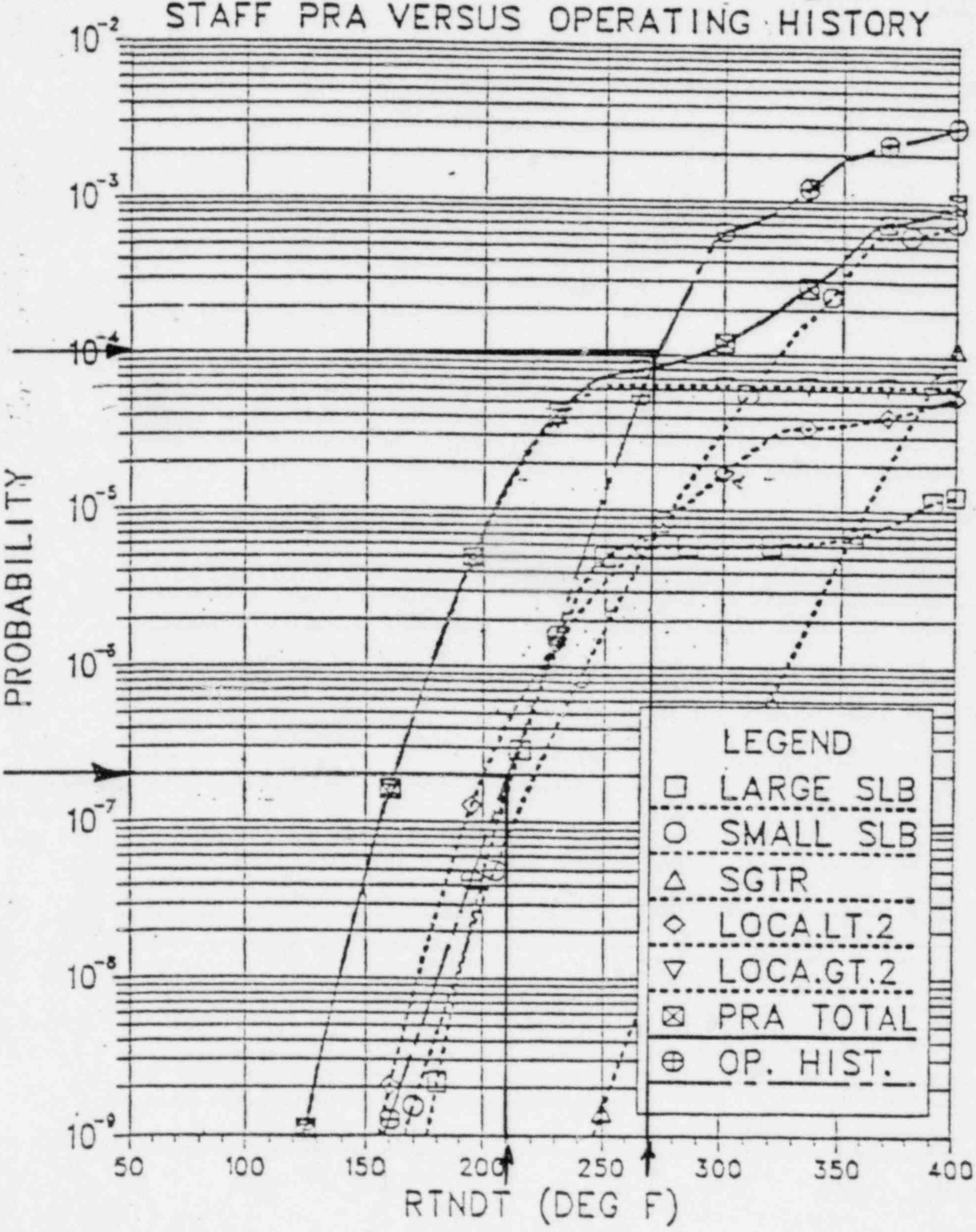
FIGURE 8-2

~~[illegible]~~

FIGURE 2

PROBABILISTIC VESSEL FAILURE

replace with
later version



8/11/82

ACRS SUBCOMMITTEE MEETING 9-30-82

HOW IS PROBABILITY OF CRACK EXTENSION OF $\cong 10^{-6}$ SATISFIED

FROM DRAFT NRC STAFF EVALUATION OF PRESSURIZED THERMAL SHOCK, SEPTEMBER 13, 1982, PAGE 8-6.

1. PTS EVENT SEQUENCES LEADING TO RPV FAILURE HAVE OVERALL FREQUENCY F PER REACTOR-YEAR. FIGURES 8-2 AND 8-3 PROVIDE A VERY APPROXIMATE ESTIMATE OF F. A PLANT EVALUATED (AS DESCRIBED IN SECTION 5 OR 9 AND APPENDIX E) TO BE AT THE 270°F SCREENING CRITERION IS LIKELY TO HAVE A TRUE RT_{NDT} OF 150-270°F (TWO SIGMA $\cong$60°F). FOR THE MEAN OF 210°F, F $\cong 10^{-6}$ PER REACTOR-YEAR ON THE NRC CURVE (FIGURE 8-3), AND MUCH SMALLER ON THE WOG CURVE (FIGURE 8-2).

EVENTS AND THERMAL-HYDRAULIC ISSUES

- INITIATING EVENTS
  - ESTIMATION OF FREQUENCIES.
  - OPERATOR ACTION TIME - ANSI STANDARD N660
- LOSS OF NATURAL CIRCULATION - CONDITIONS LEADING TO IT NEED CLEAR DEFINITION (SITE-SPECIFIC)
- LOCAL MIXING IN DOWNCOMER

9/30/82

- HEATING ECC WATER - VERY EFFECTIVE UNDER STAGNANT FLOW CONDITION, NOT EFFECTIVE AND NOT CRITICAL IF LOOP CIRCULATION IS MAINTAINED. FURTHER INVESTIGATION NEEDED ON OTHER EFFECTS (E.G., CORE COOLING).

LOCAL MIXING UNDER STAGNANT FLOW CONDITION

USING ENERGY BALANCE WITH WALL HEATING:

$$T = (T_0 - T_F) \text{ EXP } (-\beta t) + T_F$$

$$H_F = H_{HPI} + \frac{\dot{Q}''A}{W_{HPI}}$$

(ENTHALPY AT 50 MINUTES INTO TRANSIENT IS TAKEN AS FINAL ENTHALPY)

$$T_F = F\,(H_F\ ,\ P_{SYS})$$

$$\beta = \frac{W_{HPI}}{M}$$

LOCAL MIXING UNDER STAGNANT FLOW CONDITION

RESULTS:

PNL: $T_F = 136°F$ $\beta=0.13$ (W 4-LOOP)

NRC: $T_F = 125°F$ $\beta=0.12$

DOWNCOMER TEMPERATURE VS. ECC TEMPERATURE

T_{HPI}(°F)	T_F(°F)*
60	136
80	154
100	174
120	194
140	213
160	233
180	253
200	270

---

* FOR W 4-LOOP STAGNANT FLOW

MATERIAL PROPERTIES ISSUES

PNL ASSESSMENT (9/30/82)

*VARIABILITY JUSTIFIES CONSERVATISMS

*STATISTICAL TREND CURVES RECOMMENDED

*TESTING TO ENHANCE TREND CURVES

(DEVELOPMENT AND APPLICATION)

VARIABILITY JUSTIFIES CONSERVATISMS

- *NEED FOR ADDED CONSERVATISM
    - -MEASUREMENT ERROR
        - (<30 F CONTRIBUTION)
    - -GENERALIZED DATA BASE
        - (CONSERVATISM JUSTIFIED)
- *NEED FOR NRC CONSISTENCY
    - -WELDS VS PLATES/FORGINGS
    - -OLD VS NEW PLANT WELDS

STATISTICAL TREND CURVES RECOMMENDED

*BENEFITS OF STATISTICAL CURVES

-DESCRIBE ALL DATA

-INSENSITIVE TO NEW DATA

*LEAST CONSERVATIVE AT EXTREMES

-HIGH Cu.Ni.FLUENCE IS 6% OF DATA

(Cu>.3) (Ni>.5) (F>5e18)

-WORST FIVE PLANTS

(HBR2/FC/TP4/TP3/MY)

TESTING TO ENHANCE TREND CURVES

*TREND CURVE DEVELOPMENT

-SUBMICROSCOPIC CHARACTERIZATION

(EFFECTIVE FOCUS ON PARAMETERS)

(ADD CONFIDENCE IN MODEL)

*TREND CURVE APPLICATION

-IN SITU CHARACTERIZATION OF VESSELS

-REIRRADIATION EMBRITTLEMENT

CRITIQUE OF FRACTURE MECHANICS AND STATISTICAL ISSUES

F. A. SIMONEN

SEPTEMBER 30, 1982

BATTELLE
PACIFIC NORTHWEST LABORATORY

ESSENTIAL QUESTIONS

- ARE CONSERVATISMS IN FRACTURE MECHANICS ANALYSES APPROPRIATE?

- WHAT IS THE SIGNIFICANCE OF PROBABILISTIC FRACTURE MECHANICS CALCULATIONS?

CONSERVATISMS IN FRACTURE MECHANICS ANALYSES

I. BOUNDING VALUES ON INPUT PARAMETERS

- EXAMPLES ARE FLAW SIZE AND TOUGHNESS
- CONSERVATISM CAN BE QUANTIFIED
- ADDRESSED BY PROBABILISTIC FRACTURE MECHANICS

II. ANALYTICAL ASSUMPTIONS

- EXAMPLES ARE FLAW SHAPE, CLAD EFFECTS, WPS
- CONSERVATISM NOT READILY QUANTIFIED
- NOT ADDRESSED BY PROBABILISTIC FRACTURE MECHANICS
- ASSUMPTIONS SELECTED TO GIVE NET DECREASE IN FAILURE PROBABILITY

III. SAFETY FACTORS

- ALLOW FOR FACTORS NOT EVEN IDENTIFIED IN ANALYTIC MODEL

UNQUANTIFIED CONSERVATIVE FACTORS

FACTOR	ANALYTICAL MODEL NRC STAFF	 ASME CODE	 INDUSTRY
SAFETY FACTOR	NONE	1.414	NONE
CLAD THERMAL EXPANSION	YES (K INCREASED 17%)	NO	YES (CE) NO (W, B&W)
FLAW LENGTH	VERY LONG (K INCREASED 20%)	6 x DEPTH	VERY LONG (CE) 6xDEPTH (W, B&W)
FLAW DESCRIPTION	AT VESSEL ID NORMAL TO STRESS EXTENDS THRU CLAD	SAME	SAME
WARM PRESTRESS	NO	NO	YES
CRACK ARREST	YES	LOCA, YES PTS, ?	YES
SUPPRESSION OF CRACK GROWTH BY CLAD	NO	NO	NO

CONCLUSIONS

- NRC Staff Analyses Consistent With ASME Code (i.e., Code Safety Factor of 1.414 Accommodated by Conservatisms Not Required by Code)

- Probabilistic Fracture Mechanics Quantifies Only Some of Conservatism in Fracture Mechanics Model

- Greatest Uncertainty in all Fracture Mechanics Models is Size and Nature of Flaw

BENEFIT OF INSPECTION ON VESSEL RELIABILITY

OBJECTIVE - ESTIMATE INCREASE IN VESSEL RELIABILITY THAT CAN BE ACHIEVED BY USING IMPROVED ISI TECHNIQUES

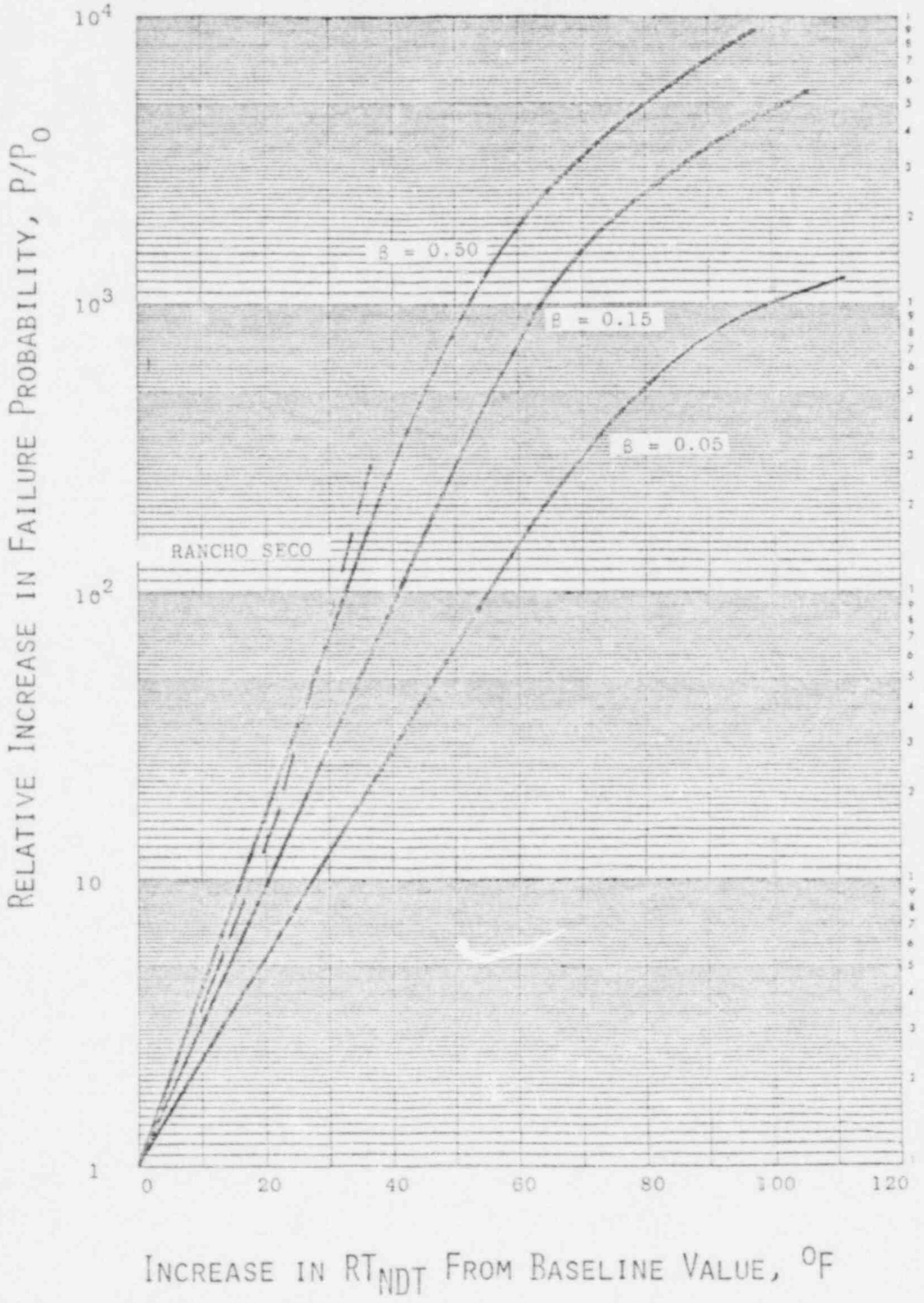
METHOD - PNL FLAW DETECTION ESTIMATES WERE USED TO MODIFY NRC STAFF ESTIMATES OF VESSEL FAILURE PROBABILITY

RESULTS - DECREASE IN FAILURE PROBABILITY

- ALLOWABLE INCREASE IN RT_{NDT}

TRENDS IN VESSEL FAILURE PROBABILITY

(BASED ON NRC STAFF PTS ANALYSES)



EXAMPLE CALCULATION FOR BENEFIT OF INSPECTION

				Failure Probability	
A	P(A)	P_{ND}	P(F/A)	$P(A) \cdot P(F/A)$ (without ISI)	$P(A) \cdot P_{ND} \cdot P(F/A)$ (with ISI)
0.125	8.3×10^{-1}	0	0	0	0
0.25	1.6×10^{-1}	0.10	1.5×10^{-4}	2.4×10^{-5}	2.4×10^{-6}
0.50	4.2×10^{-3}	0.05	1.0×10^{-2}	4.2×10^{-5}	2.1×10^{-6}
1.0	4.1×10^{-4}	0.05	5.4×10^{-2}	2.2×10^{-5}	1.1×10^{-6}
1.5	1.3×10^{-4}	0.05	5.6×10^{-2}	7.3×10^{-6}	3.6×10^{-7}
2.0	4.2×10^{-5}	0.05	4.5×10^{-2}	1.9×10^{-6}	9.5×10^{-8}
2.5	1.3×10^{-5}	0.05	-	∿0	∿0
3.0	5.0×10^{-6}	0.05	-	∿0	∿0
3.5	3.3×10^{-6}	0.05	-	∿0	∿0
				$P_o(F) = 9.7 \times 10^{-5}$	$P(F) = 6.1 \times 10^{-6}$

Notes: (1) Based on data from status report by Jack Strosnider on "Failure Probability of a RPV Subject to Pressurized Thermal Shock," March 5, 1982

(2) For "Rancho Seco Transient Reference Case," mean copper = .34, initial RT_{NDT} = 0.0 and mean fluence = 3.0×10^{19}

(3) Probability of flaw nondetection (P_{ND}) for smooth strip clad

A = Flaw depth

P(A) = Probability of a flaw of depth A in the critical weld

P(F/A) = Probability of failure for the Rancho Seco transient given the presence of a flaw of depth A

$P_{ND}(A)$ = Probability of not detecting a flaw of depth A based on PNL estimates

$P(A) \cdot P(F/A)$ = Probability of failure without ISI given the occurance of the Rancho Seco transient

$P(A) \cdot P(F/A) \cdot P_{ND}$ = Probability of failure with ISI given the occurance of the Rancho Seco transient

ESTIMATED INCREASES IN ALLOWABLE RT_{NDT}

Clad/Finish/Flaw Direction	Probability of Detection (a = Flaw Depth, Inch)	Factor of Improvement (1) In Reliability	Allowable Increase in RT_{NDT}, °F
Strip/Smooth/Perpendicular and Parallel	95%, a > 0.25	16 to 32	24 to 31
Three Wire/Smooth and Unground/Perpendicular Strip/Unground/Perpendicular	85%, a = 0.25 to 0.5 90%, a > 0.5	7.5 to 15	17 to 24
Single Wire/Smooth/Parallel Strip/Unground/Parallel Manual/Ground/Perpendicular and Parallel	80%, a = 0.25 to 0.5 85%, a > 0.5	5.5 to 11	15 to 21
Single Wire/Unground/Perpendicular and Parallel	75%, a = 0.25 to 0.5 80%, a > 0.5	4.3 to 8.5	12 to 19
Manual/Unground/Perpendicular and Parallel	50%, a = 0.5 to 1.0 75%, a > 1.0	2.8 to 5.6	10 to 15

---

(1) Factor of Improvement = Probability of Failure <u>without</u> Inspection/Probability of Failure <u>with</u> Inspection.

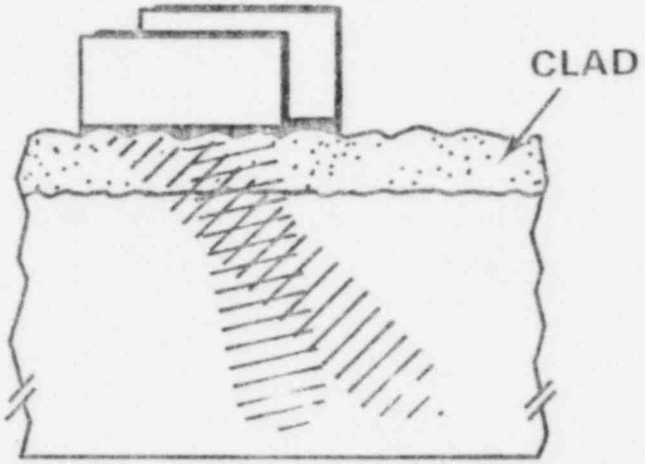
Lower bound assumes flaws are isolated and independent occurrences. Upper bound assumes possible occurrence of multiple flaws in a given weld (i.e., only half of flaws are random occurrences).

CONCLUSIONS REGARDING BENEFITS OF IMPROVED ISI TECHNIQUES

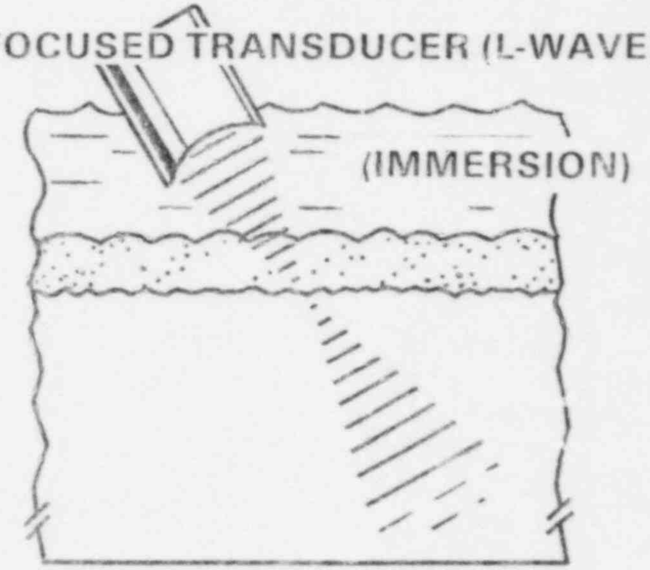
- IMPROVED ISI CAN JUSTIFY AN INCREASE IN ALLOWABLE RT_{NDT}
- UNDER IDEAL CONDITIONS (SMOOTH STRIP CLAD) RT_{NDT} LIMIT CAN BE INCREASED UP TO 30°F
- EVEN UNDER ADVERSE CONDITIONS (UNGROUND - MANUAL) AN INCREASE OF 10 TO 15°F CAN BE JUSTIFIED

'PTS' INSPECTION TECHNIQUES

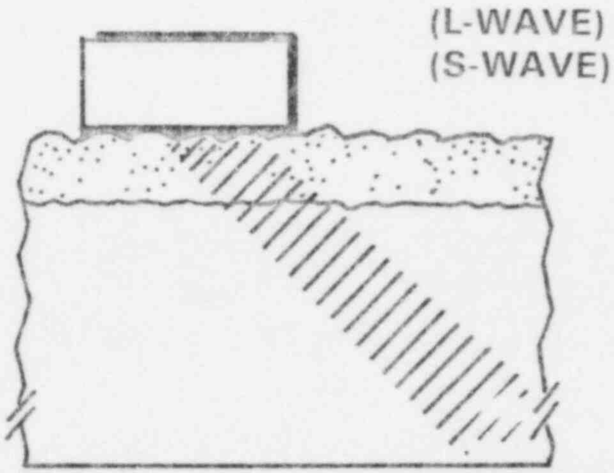
DUAL TRANSDUCER (L-WAVE)



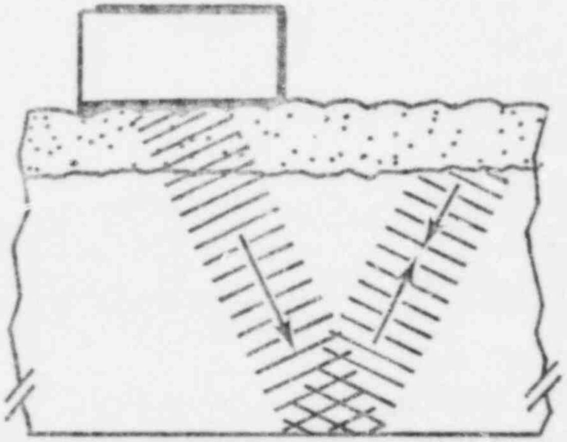
FOCUSED TRANSDUCER (L-WAVE)



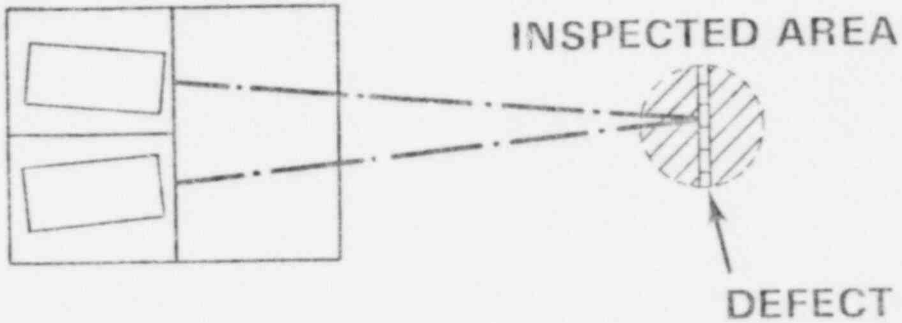
SINGLE TRANSDUCER

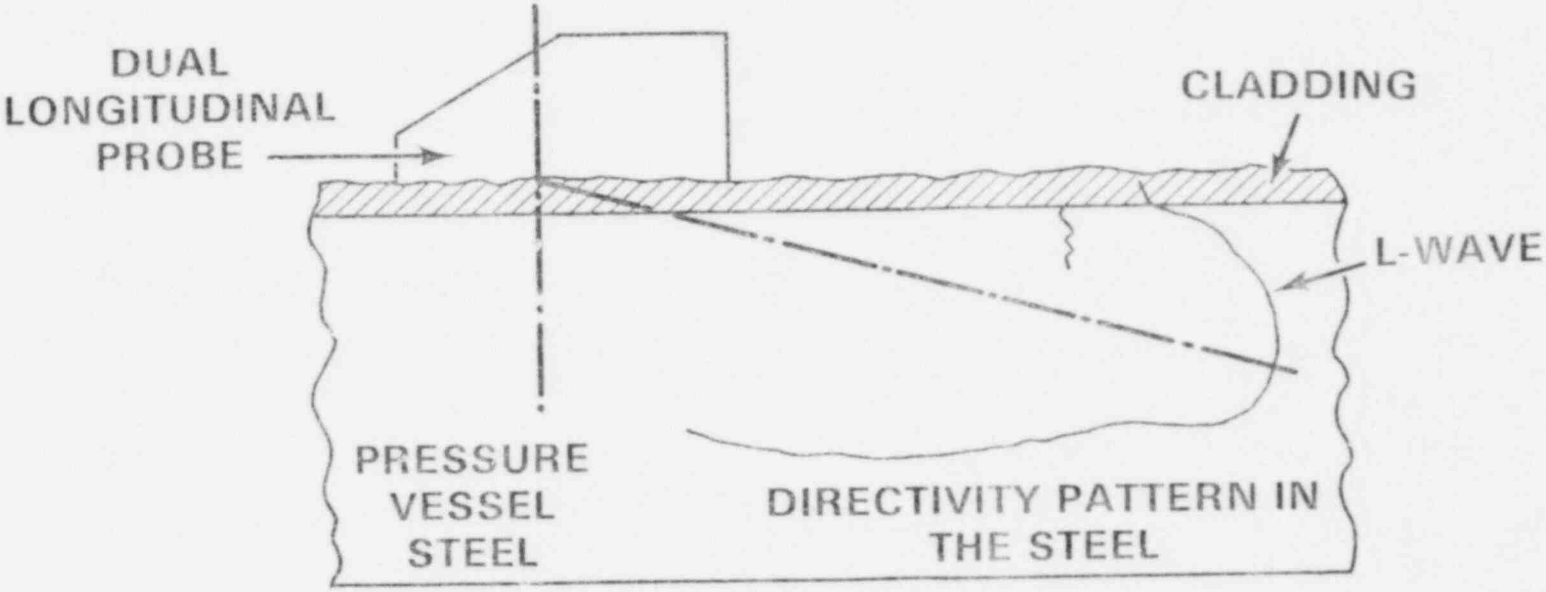


FULL VEE (S-WAVE)



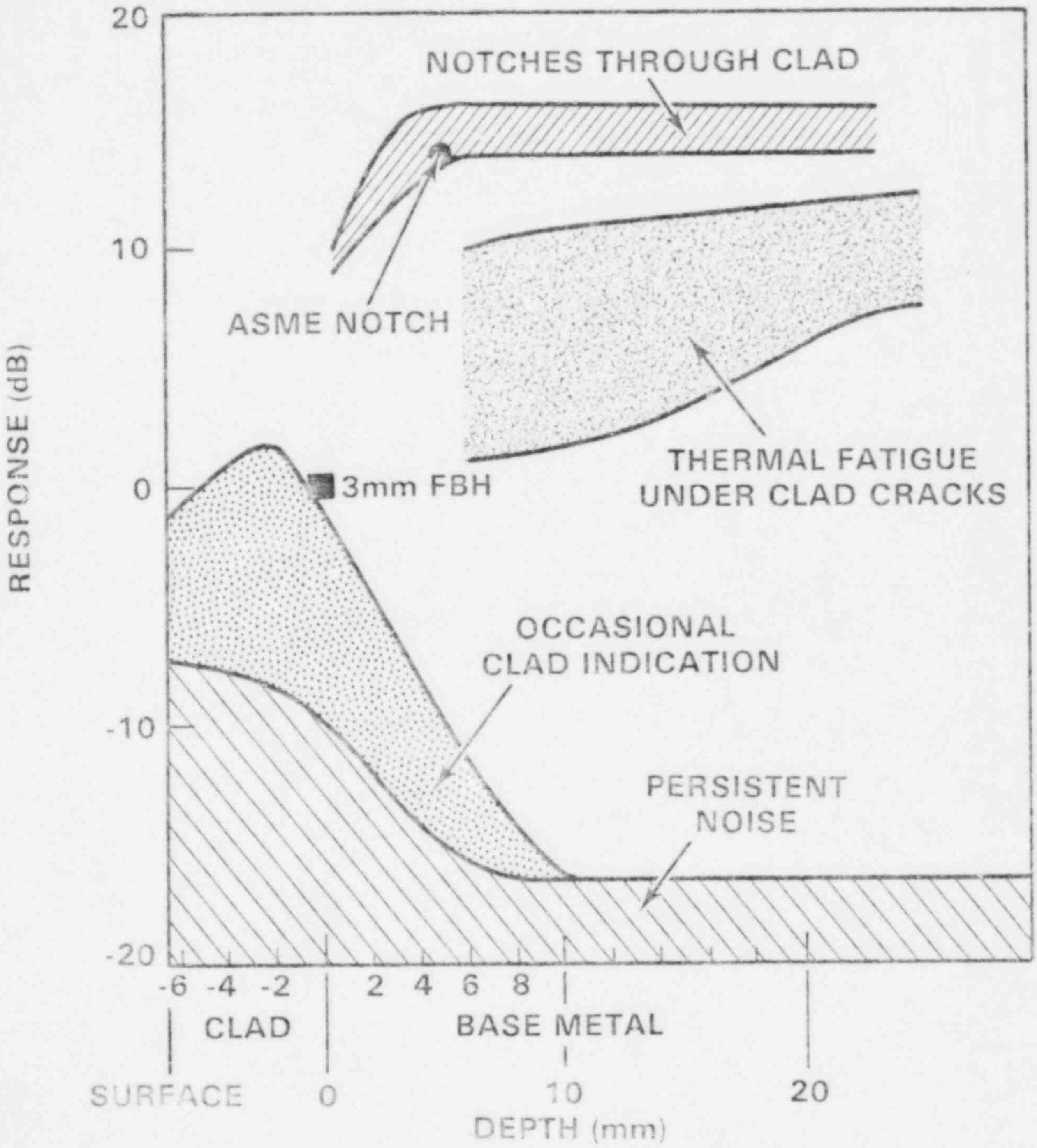
DUAL PROBE FOR UNDERCLAD CRACK DETECTION





UNDERCLAD CRACK DETECTION STUDY

CLAD	FINISH	FLAW DIRECTION WITH RESPECT TO CLAD	SINGLE CRACK PROBABILITY OF DETECTION
STRIP	GROUND	PERPENDICULAR AND PARALLEL	95% 0.25" AND GREATER FLAWS
THREE WIRE STRIP	GROUND UNGROUND	PERPENDICULAR PERPENDICULAR	85%, 0.25"-0.5" FLAW 90%, 0.5" OR GREATER FLAW
SINGLE WIRE STRIP MANUAL	GROUND UNGROUND GROUND	PARALLEL PARALLEL PERPENDICULAR AND PARALLEL	80%, 0.25"-0.5" FLAWS 85%, 0.5" OR GREATER FLAWS
SINGLE WIRE	UNGROUND	PERPENDICULAR AND PARALLEL	75%, 0.25"-0.5" FLAW 80%, 0.5" OR GREATER FLAW
MANUAL	UNGROUND	PERPENDICULAR AND PARALLEL	50%, 0.5"-1.0" FLAW 75%, 1.0" OR GREATER FLAW


20
10
0
-10
-20
RESPONSE (dB)
NOTCHES THROUGH CLAD
ASME NOTCH
THERMAL FATIGUE
UNDER CLAD CRACKS
3mm FBH
OCCASIONAL
CLAD INDICATION
PERSISTENT
NOISE
-6 -4 -2
2 4 6 8
CLAD
BASE METAL
SURFACE
0
10
20
DEPTH (mm)

FUTURE WORK

- OPTIMIZE DEFECTION TECHNIQUES
- DEVELOP STANDARD CRITERIA FOR CALIBRATION REFLECTOR AND FLAW RECORDING LEVELS
- DEVELOP CRITERIA FOR VITRIFICATION BLOCKS

WOG COMMENTS ON NRC POSITION

(NRC STAFF EVALUATION OF PTS)

- IN SUBSTANTIAL AGREEMENT WITH OVERALL APPROACH, METHODOLOGY, TECHNIQUES, CONCLUSIONS.
  - USE OF RT_{NDT} AS "SCREENING" PARAMETER
  - SELECTION OF SCREENING CRITERIA UTILIZING OPERATING EXPERIENCE.
  - USE OF PROBABILISTIC APPROACH TO SUPPORT OPERATING EXPERIENCE.
- DISAGREE ON CERTAIN TECHNICAL DETAILS
  - CONSISTENCY OF OPERATING EXPERIENCE WITH PRA.
  - FREQUENCIES OF SOME EVENT SEQUENCES.
  - CALCULATIONAL TECHNIQUES IN FRACTURE ANALYSES - EFFECT OF CLAD, USE OF FINITE FLAW FOR ARREST.
- BELIEVE THAT HIGHER VALUES FOR "SCREENING" CRITERIA CAN BE JUSTIFIED.
- PROGRAMS FOR PLANT SPECIFIC EVALUATIONS SHOULD
  - BE REQUIRED 18 MONTHS PRIOR TO EXCEEDING THE SCREENING CRITERIA
  - PERMIT EVENT SEQUENCE COMPARISONS

PRESSURIZED THERMAL SHOCK ISSUE
ELEMENTS OF WOG PROGRAMS

- WCAP-10019 (12/81)
  - Responds to NUREG-0737, Item 2.K.2.13
  - Analyses of design bases transients

    Conclusion: No near-term safety concerns
- May 28, 1982 Report
  - Provides assessment of frequency of occurrence of cooldown transients
  - Supports WCAP-10019
  - Establishes temperature limit criteria for potential flaw initiation

    Conclusion: No near-term safety concerns for > 5 EFPY
- June 16, 1982 Report
  - Provides an assessment of benefits and penalties of fuel-management techniques to reduce vessel fluence

PRESSURIZED THERMAL SHOCK ISSUE ELEMENTS OF WOG PROGRAMS (Cont)

- June 22, 1982 Report
  - Step-by-step review of WOG Emergency Response Guidelines relative to impact on PTS
- July 15, 1982 Report
  - WOG assessment of relationship between 5/28/82 report Transient Event Sequence Results and NRC Probabilistic Fracture Mechanics Analysis
  - WOG recommendations for future regulatory activities, and Plant-Specific Programs
- September 2, 1982 Report
  - WOG approach to "outliers"
  - WOG interpretation of plant experience

WOG APPROACH TO PTS

- WOG 5/28 REPORT DEMONSTRATES THAT THE LIKELIHOOD THAT A SEVERE COOLDOWN TRANSIENT WOULD OCCUR WHICH COULD LEAD TO FLAW INITIATION IS OF THE ORDER OF 10^{-3} PER REACTOR YEAR FOR VESSELS WITH RT_{NDT} IN THE RANGE OF 310°F (LONGITUDINAL) AND 335°F (CIRCUMFERENTIAL).

- WOG 7/15 REPORT DEMONSTRATES THAT VESSELS WITH MEAN RT_{NDT} OF $\sim$300°F EXHIBIT FLAW EXTENSION PROBABILITIES $< 10^{-6}$ PER REACTOR YEAR WHEN SUBJECTED TO TRANSIENTS OF 5/28 REPORT (WITH EXCEPTION OF A CLASS OF SBLOCAs IN THE 2" - 6" RANGE).

- COMBINATION OF PROBABILISTIC APPROACH IN 7/15 REPORT AND DETERMINISTIC ANALYSES FOR SMALL NUMBER OF TRANSIENTS THAT LIE OUTSIDE PRA RESULTS DEMONSTRATES PLANT SAFETY FOR ALL W VESSELS FOR RT_{NDT} $\sim$ 270°F (LONGITUDINAL) AND 325°F (CIRCUMFERENTIAL).

CURRENT WOG PTS-RELATED PROGRAMS

- DEVELOPMENT OF EMERGENCY RESPONSE GUIDELINE MODIFICATIONS TO ADDRESS PTS.
- IMPLEMENTATION OF CRITICAL SAFETY FUNCTION FOR R. V. INTEGRITY AND SUBSEQUENT FUNCTION RESTORATION GUIDELINES INTO THE WOG EMERGENCY RESPONSE GUIDELINES.
- DEVELOPMENT OF GENERIC TRAINING PACKAGE FOR OPERATOR TRAINING FOR PTS.
- DEVELOPMENT OF GENERIC TRAINING PACKAGE FOR OPERATOR TRAINING FOR STEAM GENERATOR TUBE RUPTURE.
- TESTING AND ANALYTICAL WORK IN THE AREA OF WARM PRE-STRESSING (FLAW SHAPE AND CLAD EFFECTS).
- REACTOR COOLANT PUMP TRIP CRITERIA DEVELOPMENT.
- SYSTEMATIC EVALUATION OF EFFECTS OF STAGNANT LOOP TRANSIENTS.

WOG SUPPORTS/RECOMMENDS

- GO FORWARD WITH NRC PROPOSED SCREENING CRITERIA OF:

  270°F RTNDT - LONGITUDINAL
  300°F RTNDT - CIRCUMFERENTIAL

  ALTHOUGH WE BELIEVE VALUES APPROXIMATELY 30°F GREATER, WOULD BE MORE APPROPRIATE.

- PLANT-SPECIFIC PROGRAMS BE DEVELOPED 18 MONTHS IN ADVANCE OF EXCEEDING THE SCREENING CRITERIA

- PLANT-SPECIFIC PROGRAMS UTILIZE COMPARATIVE PLANT SEQUENCE ANALYSIS AND DETERMINISTIC FRACTURE MECHANICS EVALUATIONS OF SIGNIFICANT EVENTS.

- FUTURE REMOVAL OF CONSERVATISMS SHOULD BE UTILIZED IN INCREASING THE SCREENING CRITERIA.

PRESSURIZED THERMAL SHOCK ISSUE
WOG POSITION

- NRC should use 7/15/82 "screening values" of RT_{NDT} to prioritize attentions to operating plants
- Plant-Specific Programs can be prepared ~18 months in advance of exceeding "screening limit"
- Programs could include
    - Comparative plant sequence analysis
    - Plant-specific RT_{NDT} calculations
    - Deterministic fracture mechanics evaluation of contributing transients
    - Enhanced training
    - Procedure modifications
    - Evaluation of potential remedial actions

9-30-82

B&W OWNERS GROUP

- THERMAL SHOCK PROGRAM
- GENERAL COMMENTS ON NRC STAFF'S 9-13-82 DRAFT REPORT
- WHERE TO NOW?

9-30-82

B&W THERMAL SHOCK PROGRAM

- GENERIC REPORTS
  - BAW-1628 (DECEMBER 1980)
  - BAW-1648 (NOVEMBER 1980)
- PLANT SPECIFIC
  - DPCo OCONEE-1 SUBMITTAL (JANUARY 1982)
  - GPUN TMI-1 SUBMITTAL (JULY 1982)
  - OTHERS ON HOLD

COMMENTS ON THE STAFF'S DRAFT REPORT

APPROACH (PG. 1-4)

1. SCREENING CRITERION IS AN ACCEPTABLE WAY OF INDICATING WHEN/IF PLANT SPECIFIC EVALUATIONS ARE REQUIRED
    A. VALUES ARE CONSERVATIVE, BASED ON TODAY'S KNOWLEDGE
    B. MUST BE FLEXIBLE TO ACCOUNT FOR TECHNOLOGICAL ADVANCES. (E.G. MIXING, FRACTURE TOUGHNESS, EXPERIENCE)
    C. SHOULD NOT LIMIT CALCULATION OF RT_{NDT} TO METHODS SHOWN (OTHER METHODS SHOULD BE ALLOWED IF SOUND)

2. SUGGEST CLARIFICATION OF SEQUENCE:
    A. SCREENING CRITERIA
    B. PLANT SPECIFIC EVALUATIONS (EFPY)
    C. IF NECESSARY, EVALUATE MODIFICATIONS/IMPROVEMENTS

3. NOT NECESSARY TO PROVIDE THE TECHNICAL BASIS FOR THE REMAINDER OF PLANTS DESIGN LIFE. STAYING ADEQUATELY AHEAD (SIMILAR TO 10CRF50, APPENDIX G) IS ALL THAT'S REQUIRED.

COMMENTS (CONT'D.)

ACCEPTANCE CRITERIA

1. SUGGEST A CLEAR DEFINITION OF "FAILURE" FOR REGULATORY PURPOSES. (CRACK INITIATION, CRACK ARREST, CORE MELT, RADIATION RELEASE)
2. PLANT-SPECIFIC CRITERIA SHOULD BE ESTABLISHED BEFORE ANY EFFORT IS "WASTED" ON EVALUATIONS.

COMMENTS (CONT'D.)

APPLICABILITY OF METHODOLOGY AND RESULTS

1. RESULTS BASED ON INPUT FROM WESTINGHOUSE OWNERS GROUP
2. THERE ARE SOME BASIC DIFFERENCES IN THE B&W PLANT (E.G. VENT VALVES, OTSG)
3. THE SIGNIFICANCE OF THESE DIFFERENCES SHOULD BE DETERMINED

COMMENTS (CONT'D.)

TIMING - NEEDS SOME CLARIFICATION

1. BEGIN PLANT SPECIFIC EVALUATION 3 YEARS PRIOR TO REACHING SCREENING CRITERIA
2. MUST BE SUBMITTED TO STAFF _?_ YEARS PRIOR TO REACHING CRITERIA
3. SUGGEST ADDING A COLUMN TO TABLE P.1:
   "RT_{NDT} AFTER 3 ADDITIONAL CALENDAR YEARS"

WHERE TO NOW?

1. REMAINDER OF B&W PLANTS ON "HOLD" UNTIL OCONEE-1 AND TMI-1 EVALUATIONS ARE DETERMINED TO BE ACCEPTABLE OR UNACCEPTABLE
2. B&W OWNERS CONTINUING WITH R.V. MATERIALS PROGRAM
3. SUGGEST STAFF OBTAIN ALL VALUES IN TABLE P-1 FROM LICENSEES BEFORE FINALIZING REPORT
4. SCREENING CRITERIA SHOULD BE PERIODICALLY RE-EVALUATED BASED ON EXPERIENCE AND/OR EMERGING TECHNOLOGIES
5. B&W BASIC DIFFERENCES SHOULD BE ACKNOWLEDGED BEFORE FINALIZING THE REPORT
6. TIME SHOULD BE ALLOWED FOR DETAILED REVIEW