T-1141

NUCLEAR REGULATORY COMMISSION

In the Matter of: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

270TH GENERAL MEETING

DATE: October 8, 1982 **PAGES:** 268 - 399

AT: Washington, D. C.

TR04

delete B. White

RECEIVED
ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS, U.S.N.R.C.
OCT 9 1982
AM
7,8,9,10,11,12,1,2,3,4,5,6
PM
A

ALDERSON REPORTING

400 Virginia Ave., S.W. Washington, D. C. 20024

Telephone: (202) 554-2345

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
- - -
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
270TH GENERAL MEETING
- - -

Room 1046
1717 H Street, N.W.
Washington, D.C.

Friday, October 8, 1982

The Committee met, pursuant to notice, at 8:30 a.m., PAUL G. SHEWMON, Chairman of the Committee, presiding.

PRESENT:
ACRS MEMBERS:
PAUL G. SHEWMON, Chairman
JEREMIAH J. RAY, Vice Chairman
J. CARSON MARK, Member
MILTON S. PLESSET, Member
CHESTER P. SIESS, Member
ROBERT C. AXTMANN, Member
DADE W. MOELLER, Member
MYER BENDER, Member
WILLIAM KERR, Member
MAX W. CARBON, Member
FORREST J. REMICK, Member
DAVID A. WARD, Member
JESSE C. EBERSOLE, Member
HAROLD W. LEWIS, Member
DAVID OKRENT, Member

M. NORMAN SCHWARTZ,
ACRS Professional Secretary

RAYMOND F. FRALEY,
Designated Federal Employee

ALSO PRESENT:

DEMETRIOS BASDEKAS
GERRY BLAKE
FRANK SCHROEDER
MILTON VAGINS
E. IGNE
MR. CHANG
MR. KLECKER

MR. CLIFFORD
MR. ISRAEL
STEVE HANAUER
A. WHITING

PROCEEDINGS

MR. SHEWMON: Good morning.

This is the second day of the 270th meeting of the Advisory Committee on Reactor Safeguards. Today we will hear reports on and discuss reactor pressure vessel thermal shock and ACRS Subcommittee activities.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act. Elipidio Igne on my right is the Designated Federal Employee for this portion of the meeting.

Portions of the meeting may be closed, it says here, but I really doubt it.

A transcript of portions of the meeting is being kept. It's requested that each person first identify himself or herself and speak up loudly enough so that they can be heard.

We have received one written statement, but no requests for oral presentations. The written statement is the pink cover on the front. It's Basdekas to Ross -- I'm sorry, to Carl Johnson. There are no other statements that have been presented.

The first item on today's schedule is a Subcommittee report on reactor pressure vessel thermal shock. For that I will call on Mr. Bender.

MR. BENDER: Thank you, Mr. Chairman.

This subject has been around for quite a while now. The Regulatory Staff has come to a position on how the regulatory organization should deal with it. I would like to remind you of a few facts and then to try to discuss some approaches to evaluating what the Staff is recommending.

First, there have been a number of occasions, as everyone knows, where pressurization conditions have occurred in combination with working conditions where thermal stress and pressure stress are imposed on pressure vessels. The Rancho Seco case has been talked about a lot. The Turkey Point case has been talked about some. The Ginna case has been talked about some. There are a number of them.

I don't think anyone believes that the events that have occurred have represented cases where the combination of stresses occurred on a vessel that was likely to have flaw growth of significance as a result of the conditions imposed on them. But there are vessels that exist in older nuclear plants that are being exposed to neutrons at fluence conditions that are getting them up in the range of several times 10^{19}. As you increase the fluence level, the likelihood of some reduction in fracture toughness has been demonstrated by experimental work on pressure vessel materials.

We have always recognized that there might come a time when fluence would be high enough to cause a real concern about that matter. Back in the days when the ACRS was reviewing the pressure vessel technology business, a little over ten years ago, the Committee recommended that, in addition to operational controls and manufacturing controls over pressure vessels and provisions being made eventually for the potential for dealing with these vessels, with the anticipation that in dealing with temperatures a little over 700 degrees would result in some relief from the loss of fracture toughness and bring back some ductility that was in the vessels initially.

That subject is still alive and I think it has been brought back into focus again by the questions that have been raised by pressurized thermal shock. Now, as far as I know no one has ever really seriously looked at how to anneal a vessel that has been to high levels of fluence and has some levels of radioactivity associated with it and at the same time is going to be put back in service later on. It's probably practical to do it, but when going into it in the detail that might be necessary in order to establish that it is useable and useful for the plant to carry on this procedure, it would take some time.

So I am sure that it is not going to be esy to suggest annuealing of pressure vessels to people if it isn't necessary to do it. Consequently, most of us would prefer to find a position at which the technology itself provides a reasonable basis for being comfortable with the existing vessels under the right operational controls.

The Staff has been busily trying to develop a position which does not rule out the requirement of annealing, but rather focuses on how far you can afford to go before you begin to look around for alternatives to letting the plants operate in the mode in which they presently operate.

When we formed the working group a little over six months ago, it was with the anticipation that we would get together a group of people who were knowledgeable about this question and that they would in turn review with the Staff the information that was available and review with the industry what they thought the problems were, and in the end be able to develop then for the Committee some kind of a basis for judging how to deal with the issues of pressurized thermal shock.

I think the issues -- and they are outlined in the working group draft report that has been provided to all of you -- really boil down to two things: First, have the vessels really been embrittled enough for us to be concerned about in terms of fracture under the transients that have to be postulated? Secondly, if they have, do we understand the transients well enough to be able to prepare operating procedures that will guard against the combination of conditions that someone might postulate could lead to the vessel fracture?

Both of those issues have to be addressed, and you will hear in the Staff presentation this morning some views on how to deal with this. The Staff's approach has a combination of probabilistic evaluation and deterministic analysis as a basis for its recommendation. I wouldn't call it PRA in the sense that PRA is being used in the common discussion.

In the latter part of the presentation, the Staff is attempting to describe how this work will be related to the proposed NRC safety goals. I personally would not take that discussion too seriously, because I find it difficult to follow. Other people might see it differently. I am not known for my enthusiasm for PRA, and of my expressed skepticism about it no one should be surprised.

Now, with regard to the materials questions there are a few things that need to be kept in mind. First, most of these vessels that are of concern were fabricated at a time when all the information about materials that influence fracture toughness was not available.

In particular, we didn't know enough about copper and nickel and its contribution to know that you had to be very careful about controlling those things. So the welding materials that were used in fabricating some of the vessels included some copper flash coating on the welding rods. As I understood it, that was intended as a way of protecting the welding material while it was in storage, and that introduced some copper into the welds.

Later on we found out copper was pretty important, and so that question of how much copper is in the welds and how much does it contribute to the loss in fracture toughness is still an open item. The Staff has, in cooperation with industry, attempted to develop some correlations.

They have as much data as they can get their hands on right now, and they have developed som curves that were done up at Battelle Northwest that are characterized as the Guthrie curves, that relate fracture toughness in some way to the copper and the nickel composition in the welds. And on the basis of that, they're trying to make a judgment about how these vessels, having been exposed to certain fluence conditions with certain constituents in the weld materials and to some degree in the vessels in the parent metal as well, are behaving in terms of their fracture toughness.

Important to this is the question of whether there are flaws in the welds, whether there are flaws in the vessels, how big they are, which stresses are imposed upon them, and how all of these things add up to something that might cause fracture to occur and progress to the point of causing the vessel to fail.

There are programs in place to investigate this business in Oak Ridge and in other places. It has been going on for a long time. It has been established that you can make flaws initiate growth under the right combinations of pressure and thermal stress conditions.

Most people believe that thermal stresses alone will not cause a flaw to the extent that it will go all the way through the vessel wall. I think you will hear today that the Staff has concluded that if you get the pressure in the primary system down below about 500 psi that there would be no concern for that kind of flaw progression.

At the same time, the view of the Staff seems to be, as I interpret it, that the best regulatory practice would be to operate the vessel and control the loss in fracture toughness through neutron exposure in a way that would make sure that the vessels didn't initiate flaw growth, as opposed to allowing the flaw to progress to a point where it might stop, on the premise that the vessel itself has a difference in toughness through its wall, and even though a flaw might initiate that it would grow to some extent and then stop.

At the same time, there is a recognition that flaws can initiate, grow, and then stop if there is a range of toughness in the vessel and if, as you go toward the outer wall, the vessels are tough enough so that they can resist this combination of stresses at the flaw that causes it to progress. That will be discussed some this morning and I don't want to go further with it.

Now, in order to do anything it's been necessary to determine what kinds of transients are important. So the Staff has spent considerable time trying to develop an understanding of what the transients are, what their probabilities are, and how one might assess such things in determining whether the vessels are vulnerable to cracking.

In my opinion they have done a pretty good job. I think most people think that they have made at least a valiant try. But no one would want to claim, including the Staff, that they have found all the transients that might be of concern.

So they have tried to develop some kind of probabilistic approach to addressing the matter. At the same time, they are not sure that they know -- I take that back. They are sure that they don't know whether flaws exist, how big they are, and how important they are in terms of geometry, location and the like. So they have attempted to deal with that too probabilistically.

When you do all of these things in combination, you clearly wind up with a fairly complicated story, and in Dr. Hanauer -- I think he's here to present it this morning.

MR. HANAUER: Yes.

MR. BENDER: He does a commendable job of presenting it, and we have decided that it would be in the best interests of the Committee to hear it firsthand. So he is going to present it this morning.

I am not going to try to go further with this discussion, other than to say the working group has had a couple of meetings with the Staff. The last one was considerably more satisfying than the first one in terms of enabling us to understand better what the problem is.

I think we would have to say at this stage of the game that the Staff's program for experimentally verifying what they know, for arranging with the licensees to get information supplementally -- what they have for the purpose of just assessing circumstances is still a little vague and probably needs to be strengthened more.

One of the things they have done in the course of presenting the story is to develop some basis for deciding when something should be done. They are offering what they have decided to call screening criteria, which are essentially some temperature conditions that are postulated to be those where one might become concerned about flaw growth to the extent of wanting to prepare for future action.

The numbers that have been developed are 270 degrees Fahrenheit for the longitudinal welds and 300 degrees for the circumferential welds, on the basis that the longitudinal welds have higher stresses. These temperatures, which are related to something called the RT_{NDT}, are presumed to be temperatures at which, if the right combination of stresses and flaws existed, there might be significant flaw growth.

Now, the screening criteria are not the temperatures at which this becomes a concern. They are a temperature level somewhat below the point at which the Staff would postulate growth actually occurring. But they represent a condition where preparatory actions are to be taken.

The Staff has contended that probably about three years before the need to take action one ought to be prepared to do it, or ought to be preparing to do it. So they have set their screening criteria with the anticipation that if the vessels reach RT_{NDT} values that were in the range specified at the values specified in the screening criteria, they would request applicants to start doing something to make sure that the progression in loss of fracture toughness was altered in some degree to reduce the rate of accumulation of toughness loss or, alternatively, to take some action to regain it by annealing or some such other action.

Just exactly what would be done I think we're not clear on right now. That is one of the reasons why I think the Staff needs to do a lot more work in this area.

The working group, which includes a number of

Committee members -- Bob Axtmann, Paul Shewmon, Forrest Remick, me, and -- I think that's all. Is that right? Oh, Dave Ward, excuse me. Dave is an occasional visitor. And a number of consultants who have been identified for you, and I'll not bother to go through the list today.

They at least represent a good spectrum of people who have had an opportunity to look at this thing. I myself feel like we have a combination of people that can offer a pretty good understanding of the whole problem.

The working group has heard the presentation and I think at this stage of the game we have jointly agreed that the screening approach that the Staff has developed is a good one. It is safe enough and it ought to be accepted.

That does not mean that we have complete happiness with the whole situation, but we do know enough to think that the Staff will not be in trouble provided they diligently pursue the problem from here on in, and that the applicants or licensees take enough time to understand what the problem is, to be able to deal with it.

That is about where I would like to stop discussion and invite my colleagues to add anything.

MR. SHEWMON: I guess I have two points. One, you commented you didn't think anybody looked hard enough at annealing a pressure vessel. There is a thick report out that Westinghouse did for EPRI on this. It may not be completely to your liking, but they certainly have looked hard at the possibility of it and what the potential problems and approaches would be.

The other thing that I think has been in the approach is, we would rather not have a crack start moving. That has been what is to be avoided. There have been -- perhaps Steve when he goes over this will talk some about what might be the probabilities, if one did pop in someplace, that it could indeed lead to a core melt, because if one did want to get into a probabilistic thing it is the core melt that people get concerned about, and there are a fair number of things which make it quite unlikely.

A lot of things have to happen between popping a crack with pressurized thermal shock and going the limit. But that is all I have.

MR. BENDER: Did anybody else want to comment? Bob?

MR. AXTMANN: One of the ameliorative strategies one can use in the meantime while this issue gets resolved is to minimize the high neutron fluence to the edge of the -- to the reactor wall. Although the Westinghouse group I believe has a straightforward program to try to start reloading the core in such a way that, A, the outer fuel elements are replaced with stainless steel rods, but the core loading is adjusted in such a way that the total power of at least some of the reactors is not changed, so that the conomic penalty is not there.

I am not sure just how many of the reactors are planning to do this. The Staff report did not seem to talk about this much, although it is mentioned. I think it is worth considering, since it does push off the day when RT_{NDT} approaches the magical number within three minutes or so, that would be worth thinking about and perhaps advising very strongly that all of the reactor vessels at risk get active in this area. Perhaps they are. It was not clear from the presentation.

MR. BENDER: As I understand it -- and Steve will probably straighten us out if I garble it -- in this screening criterion it has been established that that was one of the measures that might be considered. You might want to do some of those things earlier.

MR. AXTMANN: That's what I'm suggesting.

MR. BENDER: I don't know if we've heard

enough to know if that's a good idea or not. There is one plant that has already gone ahead with it.

MR. SHEWMON: I think we've heard that most of them have.

MR. MARK: I think I'll defer my questions until the Staff's presentation.

MR. BENDER: To follow on what Bob has said a little bit, all of the plants I think are looking at changes in the way in which they manage the fuel, so that the spent fuel is moving toward the outer perimeter. That has some inherent advantages in reducing the fluence at the perimeter of the core. So that alone will help, I believe.

In addition, there are some other things that might be done in addition to just adding the stainless steel rods at the peak points around the core. I suspect some people are looking at the value of doing that. You might have to give up some fuel performance or re-examine the Appendix K requirements if that were done, and that might take a little time to do it.

But I think it is fair to say that all of the licensees are thinking about those as alternatives, and many of them may be taking such action without saying they are doing it because they do not want to have the regulatory commitments that go with the actions until they've actually reached some decision about it.

I suspect that if no one else has any comments, that the best thing to do would be to turn the meeting over to the Regulatory Staff.

Jesse?

MR. EBERSOLE: May I ask a question? Mike, I heard you say that as long as you don't exceed 500 pounds probably you don't have a problem.

MR. BENDER: I think that's what the story is.

MR. EBERSOLE: It seems to me that the complex nature of this event sums up to the fact that it is pretty stupid to repressurize a vessel once you've experienced a low temperature, low pressure transient, and some mechanism should be evaluated to put a block in the road to repressurization. This would probably include automatic controls to prevent that. I guess it would automatically have to consider the standard German pratice of blowing the secondary system down.

But I feel that there are clear ways to simply avoid doing this nasty thing to these vessels and we are not properly exploiting the opportunities to do so.

MR. BENDER: Well, as one who has a bias in that direction, you're not likely to get any real argument from me. But what I would prefer to say at this time, or consider, is that prior to being in a position of insisting upon it that we try to find out what the pros and cons are and whether we're inviting troubles in other ways.

MR. SHEWMON: Can we move on?

MR. MARK: I have a feeling, perhaps not well based, there are arbitrary features about the estimate of the motion of the RT_{NDT} curve with fluence. The fluence itself should not really hang up as a major mystery. The question of whether neutrons only above one MEV are counted is of course mysterious.

The question of the fact that they go through the wall and change as you go through and the effect of that are points which seem to me just as important as trying to move the fuel around, because they are wide open. And I would hope to hear from the Staff how we are closing in on getting absolute information of the meaning of one MEV flux distribution, things of that kind.

MR. BENDER: I think we can explore that.

MR. SHEWMON: Steve?

MR. HANAUER: I seem to have a large amount of hardware here, Mr. Chairman.

MR. SHEWMON: That's so you can move around freely.

MR. HANAUER: No, in fact it provides a tether to restrict my movement, and the electrodes have been placed for that purpose.

Mr. Chairman, I am going to give, with some variation, the presentation, somewhat shortened also, that Mr. Bender described to you. Since we appeared before the Subcommittee a week ago yesterday, the status of these recommendations has changed slightly. The recommendations have been adopted by the management of the Office of Nuclear Regulation, which I reported a week ago Thursday had not been accomplished.

The discussion with the Subcommittee of course suggested to us a number of things which required further investigation. So with me today is Mr. Gerry Blake, section chief of Materials and Processes Section in the Engineering Inspection Branch in Region II, who has had cognizance of the three ten-year vessel inspections at Turkey Point, H.P. Robinson, and Oconee, by happy chance all in one region, that have taken place in the last year or so, and for which he can report both methods and results, a subject of some interest to the Subcommittee and which we were not adequately prepared to discuss with the people we had in the room on that day.

Also with us is Mr. C. Y. Ching, the section leader in the the Materials Engineering Branch in NRR, to answer any questions you may have that the regional representative might not have cognizance of.

Mr. Ching and Mr. Blake have to leave at 12:30 to go to another meeting with the Committee to Review Generic Requirements. They have to leave at 12:30. Therefore, depending on whether I'm still alive and talking when it gets to be about 11:30, I will suggest to you, Mr. Chairman, that we should, if we have not already gotten to it, hear their story.

MR. SHEWMON: The program or schedule has you finished by 11:00 and I will do my best to see that that is indeed the case.

MR. HANAUER: Yes, sir.

(Laughter.)

MR. HANAUER: Of course, during my tenure we never run over the allotted time.

(Laughter.)

MR. HANAUER: We discussed this proposed program with the Committee to Review Generic Requirements, the Stello Committee, the night before last. It seemed to last forever. As a matter of fact, the Committee also recommended a number of places for additional consideration. I will enumerate them for you. By no surprise at all, they are some of the same things the Subcommittee zeroed in on.

They discussed with us the completeness of the probabilistic review of the tail of the curve, more about which in a moment, and suggested a couple of other sequences which we should have a look at. They asked us to consider more flux reduction options, including the possibility of a better balancing between, for example, emergency core cooling, limitations on peak power densities, flux reduction, which tends to push up peak power densities but reduces pressurized thermal shock, and economy of operation, which dictates running the plant at full power, and to consider various tradeoffs involved in them.

They also asked us to consider ways to get started sooner on problem plants than the original Staff proposal of getting going at T minus 3 years. We are looking into all of these, but I will not be able to present you with a finished product.

I also feel the obligation to tell the Committee that there are two alternative views within the Staff, and I would like to ask you to be cognizant of them. One you have already mentioned is embodied in the memorandum from Demetrios Basdekas to Carl Johnson and the references therein. The other is an alternative viewpoint by Sandy Israel. And I will recommend to you that you make a few minutes available to hear these alternative viewpoints.

I must also tell you that this morning at 6:00 o'clock we received from our peer review group at Battelle Northwest an updated set of views, which I would hand to Mr. Igne and recommend that he reproduce for the Committee's information in its deliberation. In general, they support the Staff's proposal, but they have some problem with the way in which the Staff proposes to evaluate the change in RT_{NDT} during the plant's life. I would simply want to get the existence of this letter on the record and to invite you to consider it later on in your deliberations.

Now, in the year since the Committee has heard in any extensive way about this problem we and the industry have been doing a great deal of work. A year ago we went to the Committee and the Commission and said, there is no immediate problem, give us a year.

We have now had a year, rather generously dimensioned -- it's about a 14-month year, in fact -- and rather than come out with a simple criterion by which to decide whether a plant can run or a plant must be shut down or annealed, we have discovered that the problem is, as Mr. Bender quite properly put it, very much more complicated and interdisciplinary even than it seemed a year ago, which was plenty complicated enough and interdisciplinary enough, and that the detailed decisions in the Staff's present opinion about what to do about individual plants must be decided plant by plant; that the plant differences are indeed very large and that a simple criterion, if RT_{NDT} is greater than 295 degrees you have to anneal your vessel or shut down your plant, is simply inappropriate; and that there are many other differences in plant transient susceptibility, plant transient response, vessel condition, materials properties, and costs and risk benefits of various alternative decisions -- for example, the subjects discussed by Mr. Ebersole a little earlier -- that make the ultimate outcome for any given plant necessarily, in the Staff's opinion, based on the details of how pressurized thermal shock.

We have come up with a more modest animal, which is a screening criterion to determine which plants, which plant licensees, should spend the rather substantial resources involved in doing a plant-specific analysis of the scope recommended. And I will come back to this plant-specific analysis.

What we have not done, which was originally part of our assignment but which has so far eluded us and which will require further work, is to establish the acceptance criteria or the backfit criteria for deciding plant by plant how much backfitting is required and when. And the state of our knowledge -- I would earnestly request the Committee either to agree with us or provide other guidance -- that the state of our knowledge is such that more work ought to be done and that time is available for more work to establish the better, cost-effective, risk-effective, backfitting guidelines for pressurized thermal shock.

I also would mention, so I don't forget, the extensive industry program, particularly on the part of the Westinghouse owners group, on which a substantial part of what I'm going to say is based, and the very substantial NRC Research and Licensing programs under way and the very important assistance that the NRR has received from the research groups in several aspects of this problem.

Now, what I am going to do -- you have a handout with pages, and I'm not going to show all of them, mindful of the Chairman's admonishment about time. Here's what I'm going to do: I'm going to talk about the general approach. I am going to talk about how we got to the screening criteria, and then I'm going to talk about some probabilistic evaluations that help us to evaluate the screening criteria, and finally talk about some of our recommendations.

(Slide.)

We have chosen not to find one or a small number of design basis pressurized thermal shock events to be evaluated with prescribed evaluation models and compared with conservative acceptance criteria, as we have done for many others and as we did most memorably for emergency core cooling. This change in view and change in approach, which is new and has provided us with many advantages and many difficulties, is based on our experience with a few severe design basis transients, severe, conservative, unrealistic evaluation models, and the acceptance criteria that go with them.

They are so unrealistic, we have found, for example, in our ten years work with Appendix K and emergency core cooling, that they do not provide the basis for estimating the amount of safety or the level of safety actually achieved; and they do not provide the basis for deciding where the problems are, the real probems; and they do not provide the basis for deciding the real risk-cost tradeoffs, and in particular the tradeoffs amongst pressurized thermal shock and other things.

Therefore, we have chosen a more realistic approach, and we solicit the Committee's comments on whether this foray into realism is really something that commands your support.

(Slide.)

Now, the typology of pressurized thermal shock is illustrated in this schematic drawing. Here is the probability of having some event worse than X, and for X I have chosen a temperature representation. I am deliberately vague about this. You will see in the future several ways of representing this temperature.

Now, for pressurized thermal shock low temperatures are bad. You'll notice that in spite of this being pressurized thermal shock I have simplified this presentation and there is no pressure dimension. That is an additional refinement which you will see later also.

Now then, we have experience with overcooling transients, which I have represented in this way. Since the ordinate is the probability or frequency of getting something worse than X, the curves are monotonic to the right, rather than the usual probability curves, which usually peel off because they're defined in the opposite way.

Now, below the limit of our overcooling event experience are other kinds of things that could happen. We know they could happen. They have not happened, therefore they must be reckoned with by some analysis instead of experience.

We have chosen to make this analysis probabilistic and the probability curve is characterized in the schematic way by what I call the tail of the curve, which is simply the frequency-severity curve below the experience limit.

Finally, I indicate the presence of some outliers. We have done a lot of work on this curve, and I'll show you some results after I talk about the screening criterion a little bit.

For a while this work had to be characterized as dealing with the outlier of the week. As we proceeded our way through the various event sequences that could occur one by one, we found things that, oh boy, they were pretty bad, or oh boy, they were pretty probable. And then we had to analyze them in more and more detail.

Now, some of them turned out not to be outliers. Therefore, the tail had to be redrawn to take into account those outliers which did not go away or merge down into the rest of them. And these outliers, one particular class of small break loss of coolant accidents turned out to dominate for the plants for which the study was done.

(Slide.)

Now, the experience in overcooling transients is illustrated by such drawings as this, which is the H.B. Robinson event. The pressure comes down, then up, and has various jigs and jogs. The temperature comes down rather more smoothly typically, but has various kinds of ups and downs.

An even more extreme example is the Rancho Seco transient.

(Slide.)

The pressure has this marvelously sawtoothed appearance, and the temperature again is rather smooth.

MR. MARK: What temperature are we looking at?

MR. HANAUER: This is the temperature measured in the cold leg. That is the closest we can get to the temperature of the water in the downcomer, which is the variable but which is not directly measured. When we infer from actual experience, we use that measurement.

For those transients where there is a fair amount of flow, that is a pretty good thing to do. For those transients where the flow is quite low, there is a leap in inference which is not all that wonderfully well justified.

We are in next year's program going to develop better models and to calculate for these and other events the temperature of the fluid in the downcomer. But that has not yet been done.

MR. MARK: Now, what we know, then, is the temperature of the metal is not this low, unless possibly on some film on the inside?

MR. HANAUER: The temperature of the metal is indeed not this low, for two reasons. One is that this water is somewhere else, and for some transients will mix before it gets to the metal, and the heat transfer to the metal introduces an additional lag. The heat transfer to the metal is modeled in our model, but the difference in temperature between the cold leg and the surface of the metal is not modeled in our present model.

MR. MARK: And it is the metal on the inner surface that we are thinking of that this approximates?

MR. HANAUER: Yes, sir. Well, we have quite an elaborate model and do not have to approximate. These temperatures are not metal temperatures; they are water temperatures. And we model correctly the conduction, or approximately correctly, the conduction between the water and the metal and the time-dependent conduction problem in the metal.

Now, my third example is the Ginna

experience.

(Slide.)

I do it to show you a temperature curve that has sawteeth in it. Now, the reason I emphasize this is, in almost any analytical review of transient sequences the curves are smooth because our transient models and our transient sequences assume things which happen and do not include such bizarre events as actually occur in sequences of people opening valves and deciding to do different things than what we expect.

We model in our transients, and our probabilistic evaluations particularly, the sins of omission and we include the probability that the operator doesn't do something he's supposed to. We evaluate the probability of that and put it in the sequence.

We do not have in our present sequences any allowance or any evaluation for the operator doing some wrong thing not in his instructions, and yet the annals of transients, particularly the one up in Pennsylvania, are full of operators doing wrong things, what one decides later either were or were not justifiable.

Now, in order to cope with these data, we have for some purposes introduced a drastic oversimplification. This is because of the present state of the calculational art, and it is to be improved in the sequel. But for some of our evaluations, and I will tell you which ones, we have used a stylized transient for which the pressure is a constant and the temperature is a simple exponential decay which starts at an initial temperature of 550 and ends up at an asymptotic temperature of T_f.

MR. SIESS: Is that cold leg or metal?

MR. HANAUER: This is assumed to be at the metal.

(Slide.)

But in fact, our models don't even distinguish this sort of thing.

MR. MARK: And it's the metal throughout?

MR. HANAUER: No, sir, it's the water, if we've modeled the metal correctly.

MR. MARK: It's assumed to be the metal, you said?

MR. HANAUER: It's assumed to be the water at the surface of the metal. Water temperature is what I'm talking about. Now, this I will call the stylized transient, and you will see it again.

(Slide.)

The first time you will see it again is this diagram, in which we have held our noses and stylized the eight significant overcooling events which have occurred. You will also see a better rendition of these a few slides away.

Here we have characterized each of these eight by a final temperature T_f. We saw the one in Ginna. We have drawn it with some notion that we have a slow metal conduction, and we have tried to average out some of these sawtooth operations.

Here is the cumulative frequency distribution of all of the significant overcooling transients which have occurred in the United States. On Tuesday the Germans gave us some information about three German ones which have not yet been put into this. The worst one was at 225 degrees, the least one was at 350. There were lots more above 350, about which we pay essentially no attention because they don't crack vessels within the range we're talking about.

And you see, we have this curve which we have drawn.

Now, this is a way -- not a very good way; I will later show you a better way -- to look at the experience.

(Slide.)

The next thing we did was the fracture mechanics calculations, which I have foreshadowed in the previous discussion. We took the vessel wall, and we characterized the material in the vessel wall as a function of location and time. There are two reasons why the material properties are different through the wall and as a function of time. One is that they are irradiated to different degrees.

Let me now treat Dr. Mark's previous comment. The traditional measurement of fast neutron fluence counts all the neutrons above 1 MEV and no neutrons below. We know that is a drastic oversimplification. But neutrons down to 100 kilovolts contribute and, in fact, the numbers I will give you are weighted according to displacements per atom for the entire neutron spectrum and are weighted properly for the neutron spectrum change as the neutron beam is attenuated through the vessel wall.

And Mr. Ross, who is with me, one of my many colleagues of the many different aspects of this, can discuss this with you in more detail than I expect the committee really wants. But this is, as you point out, Dr. Mark, a subject about which a great deal is known. And this is modeled, we think, in quite a sophisticated way. So we have a model for the properties of the material as degraded by radiation through the wall.

The other thing that happens through the wall is that the temperature changes; therefore, we have to have a model which takes into account at any given moment the temperature and irradiation history of the material at any given point through the wall. And this calculation is made.

So what we have first is the heat conduction calculation. We assume that the vessel initially is isothermal at 550 and is struck by, is impinged upon by the water whose temperature variation is whatever we are looking at, either the real McCoy or the stylized version, depending upon which transient we are talking about.

We then solve the heat conduction problem as a function of time and location and determine the temperature of the metal as a function of location and time. We then solve, using materials properties information and the gradation of irradiation through the vessel, the properties of the material as a function of time and location.

We then do a fracture mechanics calculation, assuming either some specified initial flaw, or in most of our calculations we let the code search for any flaw in the range zero to 1-plus inches. And the amount of the plus varies a little depending on who did the calculation which month.

But typically, the limit is somewhere between 1 and 1.5 inches. For those calculations, if any flaw in that size range is critical, then the flaw is assumed to initiate. Then the change in stress intensity K1 as a function of the flaw opening is included.

The properties of the material in terms of crack initiation, K1C, and crack arrest, K1A, taken from the code values which are at the bottom of the materials properties are included. And the calculation follows using time steps whether the crack progresses or not, depending on the value of K1 at that time given the temperature stress and the pressure stress is or is not on the correct side of K1C for initiation and K1A for arrest.

There is also in this model a ductile tearing limit of 200 k.s.i. square-root inch, so that even if the material is worn, any K1 higher than that value is assumed to tear the vessel the rest of the way through. The results of such a calculation are given in this example.

(Slide.)

This one is for stylized transients. So we see here TF, TF, beta, and constant pressure characterizing the transients. And these lines for different combinations of parameters are for crack initiation only. But most of the calculations I will show you are vessel failure calculations, and they are crack initiation, which does not arrest in accordance with the model I have described.

MR. SHEWMON: Is there a crack shape or length assumed?

MR. HANAUER: Yes, sir, there is. And my next slide will talk about that at some length. Sorry about that. We will talk in some depth about crack distribution.

(Laughter.)

MR. MARK: Yet the value is 200, what, KS?

MR. HANAUER: These values are pressure in p.s.i., temperature difference in degrees, I will talk about in a moment. And these TFs are temperaturs of the asymptote of the water temperature for the stylized transient.

MR. MARK: You mentioned a value of 200?

MR. HANAUER: 200 k.s.i. square inch. That is for deep cracks which would be calculated to arrest, given the K1A curve over which K1 is, in fact, high enough to produce ductile tearing.

MR. MARK: So if you have a stress larger than that, you assume tearing proceeds?

MR. HANAUER: Yes, sir.

MR. MARK: How does that compare with actually known things?

MR. HANAUER: Well, the experimental basis of that is somewhat obscured. I will ask either Mr. Klecker or Mr. Randall to deal with that.

MR. MARK: Does most material have 400 for that number?

MR. KLECKER: Typical values would be approximately 200 plus or minus, say, about 25, depending on the specific facility. There are some that could be lower. What we assume is when you get deep into the metal there is a remaining ligament that is still warmer than the surface area and hence that metal is still tough; that is, on its upper shelf. For a very deep crack, and if you repressurize it late in the transient, there is still some conservatism in the assumption that Dr. Hanauer mentioned, but not a great deal.

MR. MARK: That was the point of my question.

How much conservatism? And you say not very much, it was 200 plus or minus 25. That is really not a lot.

MR. HANAUER: It was intended to be realistic. For some of the low upper shelf materials -- well, the lower upper shelf materials are not irradiated out there very much, are they?

Okay. Other questions?

(No response.)

MR. HANAUER: Now, then, the abcissa is a temperature which is relative to the reference temperature of the vessel. We have plotted here T final, the asymptotic temperature of the stylized transient minus RT_{NDT}. The first time we did these calculations with any accuracy, we had these curves coalesced. We did them more carefully, and they do not quite coalesce, so the T final minus RT_{NDT} is not quite a correlation of the whole business, but the differences for final temperatures 50 degrees apart are only 10-20 degrees. So almost the final temperature minus the reference temperature is a good way to think about this.

Notice that for fast transients, .15 minutes to the minus 1, the pressure effect is really quite small. It is only about 40 degrees from very low pressures all the way up to 2500. So the constant pressure assumption is really not as awful as it might seem when you look at the sawteeth I showed you earlier.

For lower transients, in fact the pressure effect is substantially more important and it is worth something like 100 degrees. That is to say, the high-pressure fast transient would get down to about RT_{NDT} and would get the vessel in trouble in this model. A slow transient at low pressure could go of the order of 100 degrees below the reference temperature and not get the vessel in trouble.

Now, if you put crack arrest in these curves below about 500 p.s.i., the curves turn sharply to the left. This is the reason for Mr. Bender's suggestion that below about 500 pounds per square inch, things are perhaps somewhat less critical. Now, there is --

MR. SHEWMON: Steve, one of the things that came up at the subcommittee meeting was the amount of conservatism in the heat transfer coefficients. And that, as I vaguely recall, came into what you could assume for beta and should not assume. Will that come up again later?

MR. HANAUER: No, sir. Now is the time for that. The curves that you draw here depend on what you assume for heat transfer between the water and the metal. What you assume for heat transfer in the layer of ostinitic material on the inside of the ferritic material.

Now, slow heat transfer is better. It produces smaller thermal stresses, just as lower betas produce; that is to say, smaller temperature change rates produce smaller thermal stresses and, therefore, less crack likelihood.

These calculations have been done with various numbers on heat transfer. There was some possible confusion in how these numbers are reported. For many of the earlier calculations the effect of the stainless-steel and the effectof the boundary layer were lumped into a single heat-transfer coefficient for which typical values were 300 or 330. These values, in fact, imply a film coefficient of about 1000, which is appropriate for flow situations but not for stagnant situations. The Westinghouse Owners Group have used an explicit correlation of heat transfer with flow and an explicit inclusion of the heat transfer properties of the stainless-steel, which is a better representation.

The probablistic calculations, which I will show you later, use a single heat-transfer coefficient from the water to the metal, but include explicitly the effect of the stainless-steel on the heat transfer and, therefore, the thermal stresses into the ferritic material. I would guess that you do not want any numbers -- which I will call on my colleaguges if you do want them, and now is the time.

MR. SHEWMON: No. It was just all of our consultants there said the numbers that were being used sounded quite conservative. This was Mr. Catton and Mr. Theofanous.

MR. HANAUER: They are because they are typical of stagnant rather than pool, and the heat-transfer coefficient has been looked at a little bit in a couple of sensitivity studies but not a lot.

MR. SHEWMON: Fine.

MR. HANAUER: There are other conservatisms in this thing that should be considered. One of them is that the cold water is assumed to uniformly distributed around the inside of the vessel and that therefore all of the welds are assumed to be effected by this cold water, which may or may not be true depending on which transient you are talking about.

Secondly, this is one of the calculations which searched for the critical crack size, and if there was a critical crack anywhere in the range, it was used. Therefore, there is here an implicit assumption that there is a crack in the weld you are talking about in the vessel.

Now, most vessels do not have 1-inch cracks, and therefore this assumption is a substantial conservatism.

MR. MARK: Is 1 inch the smallest crack that would represent something on the graph?

MR. HANAUER: No, sir. In fact, for many of the calculations a much smaller crack will initiate. For the curves I will show you for the experience, the typical crack sizes were in the 1-inch size, however, the critical size. Smaller cracks would take a more severe transient to crack, in general. Is that right?

MR. RANDALL: Down to a quarter of an inch.

MR. HANAUER: I thought so.

MR. MARK: It seems to me there is an important point embedded in here. I mean there is a crack that you see and there is a crack which will initiate. And one needs to know ultimately the difference between those.

MR. HANAUER: Yes, sir. And I will come bakck to that point.

Now, there is in this kind of calculation an important difference. The only present difference between us and the Owners Groups in modeling the Westinghouse Owners Group modeled crack initiation and extension in the way shown on the right-hand side of this diagram. They assumed a crack with an A-over-B, is it, aspect ratio of 3-to-1 was 6-to-1. But that is not A anymore because this is a half-A. And the Staff calculations assumed an infinitely long initial flaw with the depth whatever it is in the calculation.

Now, we think that the Westinghouse initial flaw is probably more realistic than the Staff's initial flaw, and this difference is about 20 degrees. More serious. The Westinghouse model that was previously used predicted or used the crack extending in this same aspect ratio as I show here at A, whereas we have a lot of evidence that shows the crack indeed gets longer because it tends to run in the more brittle material near the surface. And we have from the HSST program considerable experimental evidence.

MR. SHEWMON: That is in the absence of stainless-steel, and you do not know whether the stainless-steel -- another theory is the stainless-steel would tend to arrest the crack. Is that not correct?

MR. HANAUER: We have neither theory nor experiments that we give much credence to with stainless-steel.

MR. SHEWMON: But what you were talking about is for unclad vessels?

MR. HANAUER: Yes, sir.

MR. SHEWMON: Thank you.

MR. HANAUER: Yes, sir. Experiments are now under way in the HSST program to try to get us some technology on this subject.

Now, then, if you take the -- and Westinghouse has done this at our request, the Westinghouse Owners Group -- if you take an initial flaw, Westinghouse shape, but if when it propagates it propagates to an infinitely long flaw, which is, of course, a conservative abstraction, there just are not any infinities in finite-size vessels, then that result is something like 100 degrees less conservative than our inifinite -- I am sorry, I said it wrong -- then that one is about 80 degrees more conservative than the original Westinghouse. Let me say that again because I garbled it.

If I compare a Westinghouse elliptical calculation as they used to do it, with a Westinghouse elliptical to infinite curve as they did at our request, the difference is about 80 degrees. The difference between our infinite infinite and Westinghouse's finite infinite is about 100 degrees. No, I said that one wrong, too.

The difference between our infinite infinite and Westinghouse's finite finite is about 100 degrees.

Now, this 100-degrees difference, about 20 of it we think Westinghouse is right that the initial flaw is more likely to be little, and we think about 80 of it we are right and this 80-degree difference produces a drastic change in the probability curves, as will be seen in the sequel.

(Slide.)

Now, the next thing to discuss is operations consideration; that is to say, what the operator does and when he does it and why he does it. Clearly, the operator, as Jess has pointed out, is a central player in this event. He can, in fact, be the cause of the initiating event. He can take the needed action or he can delay it or he can omit it. Those are modeled in our probabilistic and Westinghouse Owners Group probabilistic model.

He can also take creative action to really do something good and mitigate the sequence. We do not model those. He can also take some bizarre action to aggravate the sequence. Those are not modeled. Therefore, our calculations and the Westinghouse Owners Group calculations and everbody else's calculations show, in general, smooth behavior rather tahn the zig-zags characteristic of real life.

Now, what the operator needs, of course, is decent procedures, decent instruments, and decent understanding. So we have in the seven plants that have been our principal review LOCAs for the last year, done audits of the procedures and training of these operators as regards pressurized thermal shock.

(Slide.)

The most important aspect of this is the necessity to take an integrated view. You really do not want the pressurized thermal shock crew to come through and sensitize everybody to pressurized thermal shock and melt the core while keeping the vessel warm. In fact, there is a "devil and the deep blue sea" here.

This could be illustrated by drawing a facecloth. Here is pressure, here is temperature (illustrating). The curves I showed you, the deterministic curves for pressurized thermal shock, look like this. They are pressure slopes are very small until we get to low pressures. The initial values are low, below zero. So the initial location of this curve is over here somewhere. You see, I do not have complete freedom here. I am tethered.

MR. FRALEY: That is all right. We will give you a little more room.

MR. HANAUER: As the vessel is embrittled -- you would not call it "complete freedom" -- as the vessel embrittles, this curve enlarges to the right, and over here somewhere is the curve imposed by saturation of subcooling. So here you have an undercooled and over here to the left of this curve you have an overcooled vessel.

Now, if the vessel is not terribly embrittled, there is plenty of room between those curves. As the vessel becomes more embrittled, the left-hand curve marches to the right, and it becomes more and more difficult and requires more and more operator attention and expertise to stay within the confines of the allowable area in the face plot.

Now, what we have found in our review was that the degree of pressurized thermal shock consideration in these procedures varies widely. We asked for a whole lot of changes at H.B. Robinson. By the time we got to Maine Yankee, either because Maine Yankee was better to begin with or because they had taken the lessons of the eralier plants to heart, we made no recommendations for early changes.

Now; then, what we have going is our integrated I.C.1 program following the TMI Action Plan. This includes symptom-oriented procedure guidelines, which include a very wide variety of considerations, including cooling the core and not overcooling the vessel, and a whole lot of other things.

The Westinghouse Owners Group has gone to their draft guidelines with pressurized thermal shock in mind and has found eleven places where they need improved consideration of pressurized thermal shock, improved consideration to stay in the middle of that diagram.

(Slide.)

You have, I hope, the audit reports of about half of these plants which have so far been published. The rest are under way.

Now, the next thing we did was to recalculate the transients which had already occurred, the eight overcooling transients. Oak Ridge did this for us. And to calculate using this fracturemechanics model but using the real pressure and temperature curves as actually measured rather than the stylized representations. This is shown in the solid line in this vuegraph.

(Slide.)

I have reproduced the dotted line, which is the Tfs. Now, the same steps here are not the same steps here because they have a somewhat different order. This is not the same transient, in any case. But the curve of the critical RT_{NDT} -- now I have to wave my arms a whole lot -- this is the temperature curve. For the dotted line, it is a TF temperature scale. For the solid line, it is an RT_{NDT} scale.

The Oak Ridge calculations were done using an RT_{NDT} search. The result is for that transient in that vessel, here is the highest -- the lowest value of RT_{NDT} that would have resulted in that vessel failure.

Yes, sir.

MR. BENDER: Steve, everytime you make that statement, I have to ask the related question. What flaw goes with that conclusion?

MR. HANAUER: The flaws are different. They did the flaw search. The flaws are typically about an inch.

MR. BENDER: But they are oriented in the worst place at the worst time?

MR. HANAUER: Yes, sir. This is a deterministic calculation. One assumes the flaw is there. If any flaw can get you into trouble, it is assumed to be there.

MR. BENDER: That is not a probabilistic calculation? It is deterministic?

MR. HANAUER: Yes, sir. This is a deterministic calculation with the same conservatisms and nonconservatisms I described earlier.

MR. BENDER: But it is the worst set of
probabilities that could be imagined?

MR. HANAUER: The probability of this curve is
the 1.

MR. BENDER: So that is worst.

MR. HANAUER: Yes. There are none higher than
that.

(Laughter.)

MR. SHEWMON: And it is the worst kind of flaw
that could exist?

MR. HANAUER: Yes, sir. This is an infinite
flaw model.

MR. OKRENT: While we are talking about flaws,
do we know whether for some of the older vessels there
are any for which there was not 100 percent volumetric
examination originally or later, not only of the welds
now but of the volume?

MR. HANAUER: It is known, but I do not know
if it is known in this room.

Does anyone know in this room?

(No response.)

MR. SHEWMON: Let me ask a different
question. Starting at some earlier time, said time we
are not certain about, there was 100 percent volumetric
inspections before the vessel went into service. So the

question is whether there are any that beyond or come before that window. Is that right? Is that the question?

MR. OKRENT: Well, I know that in the earlier days they did not do 100 percent volumetric on the base metal. In fact, they may or may not have picked these up a part of some kind of initial in-service. My guess is there probably is one or more of the older vessels that did not have 100 percent volumetric on the base metal. I am just wondering if in their review they had examined this and if it is a consideration at all. I am just looking for information.

MR. HANAUER: No, sir, we did not, because except in a very small number of vessels, the weld dominates.

MR. OKRENT: In other words, one is rather confident that the shift in NDT will be primarily in the weld, if it going to be important?

MR. HANAUER: Yes, sir, because it is copper that does it, and that is in the weld. There may be some early exceptions to this. That is one of the many reasons we have to do this plant-specfic, and we have not yet done this. There are people in the Staff who know this, but they are not the pressurized thermal shock team.

MR. BENDER: Steve, I want to pursue this point for just a minute. The fact that one presumes that the weld dominates also recognizes the fact that the welds were irradiated. Ultrasonics may not have been a prevalent mode of inspection at that time. I am not really sure. But I do not think I am wrong in saying that every weld has been fully radiographed.

MR. HANAUER: Yes, sir.

MR. BENDER: So in terms of inspections of welds, we have at least some inspection records that are good?

MR. HANAUER: And it is not an assumption that the welds were radiographed. This is done as part of each vessel, as part of the Appendix G calculation that has to be made for every vessel.

MR. BENDER: But that is because of the assumptions we are making about copper in the welds.

MR. HANAUER: Well, those, I would suggest that those are not just assumptions, those are based on measurements.

MR. BENDER: They are based on interpretations of information. "Measurements" would imply that you have taken samples of the welds and actually taken compositions.

MR. HANAUER: In some vessels this has been done, not during operation but beforehand, prolongations and so on.

MR. BENDER: To the extent that they are representative of the welds, you are right.

MR. HANAUER: Yes, sir.

MR. BENDER: But if it turned out that the welds did not dominate, the question of what we know about the vessel materials would be important or not important?

MR. HANAUER: We know, in general, more about the plates than the welds. And we know --

MR. BENDER: What might that do to that set of curves up there is what I am trying to develop?

MR. HANAUER: Nothing. The plates material is known, and the plate material was studied long before the weld material was studied. And its RT_{NDT} is for most plants substantially lower. It does not get there. It has been calculated and studied.

MR. BENDER: So we are comfortable in ignoring plates as being vulnerable?

MR. HANAUER: Well, we do not ignore them. We have the value for the plates, and they do not dominate except in one plant, one of the Indian Point plants. The plate does not have copper welds, and the plate dominates. This is known, and it has been looked at in every plant.

MR. BENDER: Okay.

MR. EBERSOLE: None of these vessels have carbon steel; they all have cladding?

MR. HANAUER: Yes, sir.

MR. EBERSOLE: Do you consider the presence of the cladding a threat to your argument here or significant or not significant?

MR. HANAUER: The presence of the cladding has conflicting results and we do not know where it comes out. First of all, if the cladding is seamless and if no under-clad cracks were introduced in the cladding process, then the cladding is a substantial reason why maybe we do not have any cracks.

Secondly, the cladding, if it remains ductile throughout life, provides a substantial restraint on the crack extension. On the other hand, because of its differential expansion, the cladding adds to the thermal stress, and the HSST program has a series of experiments to straighten all this out.

Now, the selection of the screening criterion was done from these curves or from their predecessors. The better curve to do it from is the black curve because it has the real transients with all the zigs and zags in it, and it includes -- and it depicts the critical value of the reference temperature.

The problem, of course, is to decide where on this frequency curve one should be in order to pick the screening criteria. This was a matter of judgment, and it is a little hard to say now where this judgment came from.

The initial selection of 270 came about in the following way: We had earlier versions of these curves which had less elegant evaluations and some errors in them. We picked as an initial point 10^{-2}, which is comfortably below the anticipated operating occurrence range. And as a trial value we observed that the dotted curve went through 10^{-2} at 260 in this whole curve, and the solid curve went through at 280 at this point.

So we picked 270, which is not supported today by the details of these curves. In fact, 270 now corresponds to some freqency somewhat below 10^{-2}, but not drastically so. We then did these other things that I will describe which show that, in our opinion, 270 is a pretty good judgment.

And therefore, we did not go back and blindly pick at 10^{-2} some larger value which would be justified by the solid curve and by the fact that RT_{NDT} does not really limit the transient. If we did only 10^{-2} in this curve, we would have picked 320 degrees, which is very nearly the value proposed by the Westinghouse Owners Group.

As you will see in the sequel, 270 gives us a comfortable feeling about conservatism and we have largely on a judgmental basis kept 270.

MR. BENDER: Steve, before you take that slide off, I want to go back and try a little more on some probabilistic aspects. Obviously, that set of curves has two kinds of information. One is probabilistic and the other is deterministic.

The deterministic part of it might be converted into some kind of probabilistic position if a few things were kept in mind. One, in the welds, where we have done some inspection, we might be able to argue that we know a lot more about the existence of flaws than the fact that only the worst flaw can be assumed to exist there. Now, whether that should enter into the argument or not, I do not know, but it is at least as valid a point to make as to argue that we are selecting low probability transients.

MR. HANAUER: In fact, that is just the point in the probabilistic calculations which I will show you, to do that in an orderly and technological way and try to evaluate that factor and a few others. But that is one of the most important ones.

MR. BENDER: The plates were also inspected, as I recall, by some kind of nondestructive method but they were not too discriminate. Some kinds of flaws could have been identified but probably not very many. All plates had some scanning of them.

MR. HANAUER: At various areas, the plates were inspected over a period of years with increasingly sophisticated inspection techniques. These were provided in the mid-'60s, about '66 as I recall, '65 or '66. But in the future the plates should be inspected in a more complete way. This was done and had been done for some vessels previously. That is why I could not answer Dr. Okrent in a more specific way because I do not know which ones were done which way.

MR. BENDER: Okay. You are coming to that. I just wanted to emphasize the point.

MR. WARD: Mike, I guess your argument there depends upon an assumption that a weld radiograph is going to show up a flaw and a crack of interest in this sort of thing. I am not sure that is the case.

MR. BENDER: It may not show every crack, but it certainly gives you a reason for saying that there is some probability that you have identified the cracks that exist. Now, the fact that they are not the best nondestructive testing examinations does not mean that you have to accept the premise that the worst flaw exists, does not exist necessarily, and probably does not exist.

MR. EBERSOLE: I have the impression that an X-ray would not show a vertical crack of fine dimensions.

MR. HANAUER: That is my impression also.

MR. BENDER: It will indicate certain kinds of cracks.

MR. HANAUER: Mr. Chan of the NRC Staff.

MR. CHAN: I do not know how many of the earlier vessels received the UT examination when they are new. But most of the vessels, I would say, built after '70, they had received not only the radiography but also had UT inspection when they new.

MR. HANAUER: Thank you.

MR. EBERSOLE: But just an X-ray will not show vertical cracks.

MR. CHAN: You can use the angle X-ray.

MR. EBERSOLE: Was that required?

MR. CHAN: I am not sure whether it was.

MR. HANAUER: All right. Having selected 270 as the screening criterion for longitudinal cracks, we redid th calculations for circumferential cracks. The difference is in the constraint imposed on crack opening by different geometry and in the different pressures stresses, the difference between hoop and actual stresses. These combined both to make the situation less severe for circumferential cracks.

On the other hand, the consequences of the circumferential crack can, at least in principle, be a great deal more spectacular in the limit if one has a complete separation all the way around the vessel there is a lot of energy inside and so the top half becomes a jet, potential missile.

There are two calculations, one of which shows that it almost surely will be restrained by the bolts and pipes and so on that are hooked to the vessel. The other one comes out right in the middle of the uncertainty band. But there is a lot of energy absorption capability in all of the tinware hooked to the top half of the vessel.

There is also the escape of fluids and the decrease in pressure, and this would be, of course, a much more serious event even than a large axial crack over the entire 80-inch height of a single course or a single weld.

Therefore, we tried to stay a little conservative, and we picked 300 degrees for the circumferential crack criterion.

MR. EBERSOLE: Does it include carrying all the rods out with it?

MR. HANAUER: It would carry the rods out with it, and the core, too. The core barrel is suspended up there. If it really took off, it would be a very interesting event. It is also not at all clear whether it would wreck containment, whether this is just an other species of core on the floor or the core species spread around. We do not have any technical analysis of this situation, but it has really got more severe potential than the more mundane failure modes.

(Slide.)

Now we will look at how to evaluate specific misspelled vessels to determine the actual state of the vessel. They made the recommendation that the RT_{NDT} of some given vessel for comparison with the screening criteria be done in a conservative way at the two-signal level, that the initial value be evaluated in its best-estimate and standard deviation, that the shift as a functoin of fluence be evaluated as to its best estimate and standard deviation and that the result be expressed at the two-signal level, as I have shown.

Now, the other thing to worry about is how to calculate this change. We have Reg Guide 1.99, which is pretty schematic as far as copper and nickel content is concerned.

(Slide.)

We have a more recent analysis by Guthrie, who includes a continuum of copper and nickel values. Guthrie chose to do his correlation in a single straight line and change in RT_{NDT} with a change in fluence with some nonlinear scales, which I will not try to describe.

(Slide.)

However, Guthrie's upper values are based on nickel. The data in the upper fluence is based on lower nickel material, and Guthrie's curve for higher nickel material give very large changes in RT_{NDT}, larger than any data which have been meaured.

We have chosen to cap the Guthrie curves with Reg Guide 1.99 curves in the high-fluence high-copper, high-nickel range. It has been suggested to me on the bus coming down this morning that this is an interim position, that we are getting better data, and we will in a year or so have a better correlation.

This came in for a lot of discussion at the subcommittee meeting, and we have several assignments; for example, evaluating two different populations, one below to make the Guthrie curve, one above to make the Reg Guide 1.99 curve and to consider whether this changes the results.

This is not something that we have been able to do in a week. This is something to do in the course of the next year, very carefully, and it is on our plate to do.

Now, this schematic diagram, I have also shown the real McCoy for one particular value of nickel, 1 percent, and for three representative values of copper. Here, because you cannot read it, is the change in RT . Here is 100, 200, 300, 400, 500 degrees. Here is the Guthrie curve. And for high copper it goes off into the stratosphere. Here are the Reg Guide 1.99 curves. And there is the break point for highest copper down around 3×10^{18}.

There is a whole family of such curves that has to be considered. This is a subject which has been discussed at the most excruciating length with subcommittees of this committee over many years and will be discussed at whatever length you desire either now or with a subcommittee.

The results for the first eight plants in the list is given here.

(Slide.)

MR. HANAUER: Here I have shown the initial RT_{NDT}, the change as of December 31st of last year, which is a convient fiducial for making these calculations back when we made them, and the standard deviations. A large number of these plants, but by no means all, a large number of these welds are a member of the large population for which the two standard deviations is 59 degrees. Jack Strohschneider checked this with his probabilitic calculation, and this number is consistent with the kind of variations that he is using. This will become important. Therefore, here is the value of RT_{NDT} for these vessels, and here is a projection of when these vessels will get the screening criterion.

Please do not assume that this has all the 1990 vessels in it. They were made when we had Table P-1 in the back of your draft, in accordance with their RT_{NDT}. Since the rates are different, there are a bunch of plants on the second page in the 1990s which do not show here.

The first plant, however, only gets to the screening criterion in late 1987-1988 timeframe, thus validating our statement a year ago that no immediate plant changes are required.

MR. OKRENT: Validating, I would say, is a pretty strong word.

MR. HAKAUER: Well, whatever we have learned between then and now gives us the same answer.

MR. OKRENT: I'll accept that, where your judgment came out in a similar way might also be appropriate.

MR. HANAUER: Yes.

Now, then, the next thing I want to do is to talk about the probabilistic approach. Being mindful of the time, Mr. Chairman, I am going to go a little faster.

Strohschneider and his colleagues in research have done the following kind of probabilistic calculation which is described at some length in one of the appendices of the draft that you have. They took the deterministic model that I have described, and for the parameters which are important, they replaced the deterministic and in many cases very conservative numbers, crack depth, for example, with a probabilistic approach. For copper, they replaced the number by a distribution whose width was characterized by some data sets that were available and whose center, whose mean was one of the parameters of the calculation.

The crack depth, the deterministic idea that there was a crack there was replaced by a crack probability distribution which is discussed in the appendix which you have, which is in fact one of the key numbers in this calculation.

The result is a set of curves or response surface for which the curve cuts are given here. I'll just show one of them.

Here we have the mean value of the RT_{NDT}.

(Slide.)

MR. HANAUER: Here is the conditional failure probability.

Now, then, what we see is a series of calculations for stylized transients. We are backed to stylized land again. The scheme is here is the stylized transient characterized by T_f, beta, and 1000 pounds per square inch. You can't have everything vary on one two-dimensional viewgraph. Here is the probability of failure for a single longitudinal weld, given these transients, and of course, temperature is a very important parameter. You get about a factor of 10 for something like a 15 degree change in temperature for the steepest high speed curve, and rather less than that for the lower speed ones. That is to say, if the temperature of the transient goes down to RT_{NDT}, the probability of the failure of the vessel is low, but if it goes to RT_{NDT} minus, say, 50 degrees, the probability of vessel failure is quite high. So there really is rather a cliff there.

I have to point out to you that the temperature involved here is the difference between T_f and a mean value of RT_{NDT}. The various probability distributions used in this curve produce a distribution in RT_{NDT}. You can think of this as an ensemble of vessels with different RT_{NDTs} characterized by this distribution, or you can think of this almost accurately as the variation of RT_{NDT} in various parts of these welds because of the materials properties variations in the vessel. It would be a little smaller in that case, and this is not very well delineated by our series of measurements.

Anyway, the value plotted here is the mean of this distribution. This includes vessels that are less brittle and vessels that are more brittle than this number would imply. We will have to come back to this. It is very important in interpreting the results.

All right. I have given you in your handout, but I will not labor here over the other cuts.

MR. BENDER: Steve, the process is so

important, maybe we should stop for a moment and ask one or two questions.

Do you know anything about the distributions?

MR. HANAUER: You mean the one he used? They are given in his report.

MR. BENDER: But are they representative of real cases or are they just a bunch of computations?

MR. HANAUER: They were intended to be as realistic as possible, and they were the results of the detailed studies that were available. Various parts of them are better understood, and we have more data for some parts than for others.

We know a lot about copper, and we don't know very much about crack size distribution. Therefore, the crack size distribution has a lot of speculation in it, and the copper is based primarily on measurements.

We have Dr. Vagins here who worked on that.

Do you want to characterize it any further?

MR. VAGINS: No, sir.

MR. HANAUER: A well-trained member of the gang.

MR. BENDER: That may be overstating the thing some.

MR. HANAUER: Yes, and Strohschneider and his colleagues, of which Dr. Vagins is one, have given us the ones they actually used for review.

MR. SHEWMON: Steve, even though it is going to come out of your time, since we have got you interrupted, let me make two comments. One, these flaws whose distribution you have are still infinitely long?

MR. HANAUER: Yes, sir.

MR. SHEWMON: So if you really wanted to get it, you would bugger the distribution some to account for that.

The other thing is some people drink a lot of coffee for breakfast and would like to stretch their legs.

MR. HANAUER: I think that is a superb idea, Mr. Chairman.

MR. SHEWMON: Let's take ten minutes.

(A brief reces was taken.)

MR. SHEWMON: Could I get you to move toward your chairs, please?

We have a question here.

MR. LEWIS: So far the conversation has been at a very high intellectual level, so let me lower it a little bit.

I am unable to reproduce the arithmetic on your Chart 14 which calculates the RT_{NDT} for the plants.

MR. HANAUER: Well, there were probably mistakes in it, although we worked very hard on it.

MR. LEWIS: Since the arithmetic I can't reproduce is for H. B. Robinson, I wonder if it is too much to ask --

MR. HANAUER: Mr. Randall, please, sir.

You haven't told us what doesn't compute.

MR. LEWIS: The top row doesn't add up to the last number in the row.

MR. SHEWMON: Take 56 from 295 and then add 34.

MR. LEWIS: That is certainly the right thing to do.

MR. SHEWMON: It ends up 78, 278?

MR. LEWIS: 273.

MR. SHEWMON: The Robinson people I suspect are responsible for that.

MR. LEWIS: It is not a big deal.

MR. RANDALL: Let me check another file and get back to you.

MR. LEWIS: I just wondered whether it was a real miscalculation or a miscopy.

MR. RANDALL: I think it's a miscopy. Let me look.

MR. HANAUER: Ready?

The next thing I want to do is talk about some probabilistic calculations.

In order to look at this thing probabilistically, you have to look at events probabilistically and look at vessel failure probabilistically. I described to you a way that has been developed to look at vessel failure probabilistically, and now I will tell you a little bit about looking at events probabilistically.

When we were here in June, we had none of this available to us, and when we met with industry about a week after we met with you, the Westinghouse Owners Group said what's the matter with you guys, we have done probabilistic evaluation of this. Why did you ignore us? And sure enough, on the 20 something of May, the Westinghouse Owners Group mailed to us and presented to us a thing which I now recognize to be in fact the nucleus of a probabilistic analysis. Since June we have been working very closely with the Westinghouse Owners Group, and we have been reviewing this, and so the next few minutes story are primarily a Westinghouse story, although the Staff also is involved in the story.

What they did was to look at a very large number of possible pressurized thermal shock event initiators and pressurized thermal shock event sequences. What they first did was to screen them as to whether they could fail the vessel or not and present those results as three probabilities. What they have since done is more conventional. They have several hundred boxes, event tree branches, if you prefer, each one of which is an event sequence that can lead to pressurized thermal shock.

Where we now stand, the staff has now reviewed this and has made some changes in the Westinghouse Owners Group number. Here is a representation of these event sequence frequency distributions.

(Slide.)

MR. HANAUER: Here is the same frequency scale of something happening worse than X, and here again is X, a temperature, and in all cases here limited by our calculational abilities. We have stylized the transients into T_f, beta and pressure, and by plotting only T_f here, we have simplified still further in the effort to obtain a two-dimensaional result.

Here is the depiction in these terms of the original Westinghouse PRA. Here, the solid curve is a Staff PRA based on about three months work between May and the time we plotted it a few weeks ago, and which we have revised in some cases, always with discussion but not always with concurrence with the Westinghouse Owners

Group technologists. We have revised both probabilities and severeities of some of the event sequences.

And here is where we had the event sequence of the week.

Now, the event sequences that turned out to be most significant, as will be seen shortly, are the small break loss of coolant accidents that involve stagnation in the loops, that is to say, where the natural circulation flow is interrupted and where the mixing is thereby substantially degraded, and you come a lot closer to getting just the emergency core cooling water poured into the vessel.

There are a number of stylized aspects to this analysis which may or may not be conservatisms, depending on just exactly how you look at them. But what we have done is for each event sequence, we have characterized the probability and the severity, and by no surprise at all, the severity is characterized as a T vinal of bet and of pressure. We have used these, then, to go into the probabilistic fracture mechanic results.

Thus, what you see next is a convolution of these event sequence frequency distributions and the probabilistic fracture mechanics.

Now, there are a number of approximations and rationalizations that go with this.

(Slide.)

MR. HANAUER: The results for the Westinghouse Owners Group probabilistic event sequence is shown here. Here we have the frequency per reactor year of vessel failure. The model, I remind you, has in it crack initiation and crack arrest, but except for one case on another one which I will come to, no warm prestress.

Here we have the characteristic of the vessel plotted as this mean RT_{NDT} for the distribution, so that this is some kind of a best estimate evaluation, so that vessels with RT_{NDTs} up to about 250 degrees in this model --

(Slide.)

MR. HANAUER: Here we have the same thing. We are using the Staff's model which crosses 10^{-6} down to about 210 degrees, 205 degrees. As the vessel becomes more and more brittle, the vessel marches from left to right across here, and as the vessel becomes more and more brittle, then more and more event sequences can contribute to the probability of the vessel failure, and therefore one gets these rising curves of vessel failure frequency as a function of the single characterization of the vessel condition.

Now, one of the principal results of

Strohschneider and his coworkers is that all those probability curves and surfaces do indeed correlate within some accuracy limitation as a function of the RT_{NDT} mean of the distribution of the vessel characteristics.

Now, what you are actually seeing here is the interaction of the tail on the RT_{NDT} vessel distribution and the tail on the probability curves and the tail on the event sequences. So, as you come onto the page, what you have is some low probability vessel conditions, some low probability sequences combining to give a low probability of vessel failure.

As the vessel embrittles, it is more likely to be in its brittle state; then events can give us trouble.

Here you see plotted for the low end of the curve the dominant small break LOCA, the steam generator tube rupture which doesn't contribute very much in this model, and the steam line breaks which are in fact dominated by the fairly small breaks.

Yes, sir.

MR. BENDER: Steve, two points. First, my recollection from the previous discussion was the probabilistic group had assigned an uncertainty factor to these numbers, and I think it would be well to identify that.

MR. HANAUER: Yes, I was going to get to that.

MR. BENDER: I didn't want you to forget it.

And secondly --

MR. HANAUER: Would I forget that?

MR. BENDER: You interjected a comment about warm prestress. I think it would be useful to know what family of events it doesn't apply to, and is it that whole family up there?

MR. HANAUER: Yes, sir. I will say those two things you asked me to.

In the first place, the authors of the probabilistic fracture mechanic study assigned an uncertainty band of plus or minus two orders of magnitude failure probability. Since that is directly involved here, these curves have an uncertainty of plus or minus two orders of magnitude.

There is also a substantial uncertainty in the event frequency curves, but since they add independently, this is not very large. Therefore, thees curves should be used, if the author's view is correct, as having plus or minus two orders of magnitude uncertainty.

MR. OKRENT: This is two orders of magnitude to some confidence level?

What do they mean when they say two orders of magnitude?

MR. HANAUER: Dr. Vagins.

MR. VAGINS: Bill Vagins, Material Engineer Research. That is an attempt to establish a level of confidence.

MR. OKRENT: But is it 90, 95 or 99 or what is it?

MR. VAGINS: I would say it is about 95 percent confidence.

MR. OKRENT: That's all, thank you.

MR. HANAUER: The second point is that for the small break LOCA, warm prestress was included in the vessel failure calculations. We are just learning how to do this, and we did it for the dominant action for which warm prestressing does take place for many of the accidents in that class.

MR. MARK: Does that account for the fact that the steam line breaks get to be bigger than the small break LOCA at 300 plus degrees, that you didn't do the warm prestress?

MR. HANAUER: The small break LOCA has a probability which limits it. This 3×10^{-4} is the probability of the small break LOCA in the range where the probability of vessel failure is 1. So iot is limited in that range by the probability of that class of events.

Steam line breaks is kind of a misnomer because opening steam line PORVs and safety valves are also steam line breaks and have a much higher probability, and therefore the curve goes on up.

MR. EBERSOLE: Does this include the probability of the operator doing the worst thing that he might do?

MR. HANAUER: No, sir. I thought I characterized that earlier. I will do it again.

This includes sins of omission of the operator, delay or total failure to do what he is supposed to. It does not include bizarre actions.

MR. EBERSOLE: Do you have a number for the probability of a bizarre action?

MR. HANAUER: No, sir.

MR. SHEWMON: Please go on.

MR. HANAUER: Now, these things in our opinion are much too indistinct, and the error bands are much too large to pick some probability, go across here, look at the intersection with this curve, and pick the screening criterion that way. However, we have done almost that as shown on the next viewgraph.

(Slide.)

MR. HANAUER: This reproduces the curves of the previous viewgraph, and superimposed on them, as I promised, is a distribution of vessel characteristics, an ensemble of vessels, if you like, or a probability distribution function, if you prefer, for a vessel which just gets to the screening criterion on the conservative basis I described earlier.

Therefore, I have drawn a Gaussian -- my colleagues and I have drawn a Gaussian with the two sigma width of 60 degrees, which is an approximation to 59, and with the two sigma point pinned at 270. The mean of this distribution is at 210, and it is the mean of this distribution which is what was plotted here in plotting the results of all this probabilistic consideration. Therefore, if all of what I have been talking about is a representation of real life or to the extent that it is, to the extent that we ignore the conservatisms and non-conservatisms which we have been discussing for the last hour and a half, a vessel just at the screening criterion will have about 2×10^{-6} frequency per reactor year of failure, with the large uncertainty that I have already described.

Now, this is why we did not move it on up to the 300 range where the experience curve, if 10^{-2}

tells us anything, told us it might be an appropriate place to put it.

This is now the key to the comparison with the safety goal and to the program which we propose. We do not think that further detailed investigation of the shapes and so on of these curves is justified in the short term. That is to say, we do not think that a lot more work would get us a substantially better screening criterion. We propose that additional work is needed, both generic and plant specific, but that we know enough to choose a screening criterion based substantially on judgment with the scientific, technological underpinning I have described, and with the lack of it I have described, and that we would like to go on from there.

(Slide.)

MR. HANAUER: The next thing we did was to compare this with the safety goal. The numbers in your draft are incorrect. The correct numbers are depicted in this viewgraph. The formalism goes like this. What we have calculated to the extent with the uncertainties is the vessel crack frequency. Now, not all vessel cracks melt the core. Some fraction X of these vessel cracks melt the core. We do not have a technical analysis of this number X except that it is certainly no larger than 1, and for longitudinal cracks, may be substantially less than 1.

Similarly, not all vessel crack core melts produce significant releases, some fraction Y of these. Again, there is no technical evaluation. There is a little bit in the reactor safety study, but none of the probabilistic risk analysis since do anything significant about vessel failure. Therefore we now in many ways more about Y than we do about X.

For large dry containments, for the other kinds of core melts, Y is quite small, except for the circumferential crack which lets the top half of the vessel go. We would think Y would be small here, but that is a conjecture, not the result of an analysis.

MR. SHEWMON: When you did these vessel crack exercises, I take it much of this is based on F, then, your calculations of F?

MR. HANAUER: F is the result of the previous calculations

MR. SHEWMON: Okay. This is just your goal.

Go ahead.

MR. HANAUER: There are two subsets of public risk. The core melt is XF, and I have arbitrarily assigned a tenth of the Commission's draft safety goal to pressurized thermal shock. This has a large degree of arbitrariness in it, but I have done it, and that says that if the product XF is less than 10^{-5}, we are consistent with the draft Commission safety goal.

Today we think --

MR. OKRENT: Excuse me. Before you come back --

MR. HANAUER: I am going to come back because the risk is harder.

(Slide.)

MR. HANAUER: Now, then, a vessel which just is at 270, I suggest it has an F of 10^{-6}. It goes up approximately a factor of 10 for ech 20 degrees so that in returning to this slide, so that if F is something like 10^{-6}, then a vessel 20 degrees above the screening criterion, if you believe all these numbers, gets -- and if X is a large number near 1 -- gets to the safety goal. And each 20 degrees more, if that curve is correct, gets you another factor of 10 core melt probability.

So that it looks pretty good with the safety goal, but a fairly large change makes a small change in the result. Similarly, if you look at risk and if FXY is the frequency of a large release, and if we are looking at the risk to people fairly close in, as the Commission has directed us to do, and if we conventionally divide the azimuth into 16 regions so that any one person has a 1/16th chance of being in one of these regions, then the safety goals implies that FXY over 16 should be less than five times 10^{-8}.

That is with an average site with a bunch of other provisos in it. Now if F is two times 10^{-6} and if I divide that by 20 instead of 16 because of my fear of trying to do algebra in my head, then this lefthand side is 10^{-7} times X times Y, and it is pretty easy to speculate that X and Y only needs to be one-half and that it ought to be pretty easy to show that this, too, is within the safety goal.

Similarly, it goes up a factor of ten per 20 degrees, if that slope is correct, and so you can get into trouble on the risk with a not very large increase in RTNDT above the screening criteria. So we think the screening criterion is pretty good and that we should not have plants above it without a lot of plant-specific evaluation which might then tell us that the value of F is different.

That is all I want to say about the safety goal. It is your turn.

MR. OKRENT: I just was wondering whether there is more to the examination of this factor 16 than just dividing 360 degrees up into 16 pieces of pie.

MR. HANAUER: I am talking secondhand now. I have only dabbled somewhat in the safety goal. Of course, there is more to it than that, although that is one of the factors. If you want to do acute death risk, that is pretty much all there is.

If you want to look at cancer risk, there are a lot more numbers and they come out in this range.

(Slide.)

MR. HANAUER: Now, then, I have already talked at some length about the uncertainties. I will not go through it again except to remark that arriving at the screening criterion has been primarily a Westinghouse plant analysis. If you will look at the experience you will find the worst three have been B&W plants, the next five have been Westinghouse plants, and there are no severe overcooling transients for Combustion plants.

However, if you disaggregate the experience to these three kinds of plants, you find a very small number of reactor years for the Combustion and the B&Ws

and you are in trouble as to whether any of this is statistically significant. We have, therefore, aggregated all three kinds of plants and propose for the moment to use the same screening criterion for all three.

The Combustion owners' groups have averred that their plants are different and that they are better protected, but they have not yet shown me or the Staff an adequately story about whether this is in fact true. It may be true, but we do not have the kind of data we need.

For the B&W plants we have, first of all, that the worst three events have occurred in B&W plants. However, we also have that substantial rectification programs have been provided for the causes of all three of these events, so that presumably operating B&W plants today are less prone to these events than they were when they happened. We have not given any benefit for that.

We have a suspicion, based on essentially no technical evidence, that B&W plants are sufficiently different that 270 is a very gross approximation for B&W plants. One of the things the CRGR told us to do was to go back and get together a program for understanding B&W plants better. I think I have discussed in some detail these other uncertainties.

In answer to the question that inevitably comes up, there are a substantial number of conservatisms and some non-conservatisms in all of this analysis. Many of us think that the result is that 270 has some conservatism in it. The Staff is not monolithic on this. Some people have a better feeling about the conservatisms than others.

We do not have and see no way of getting a quantitative evaluation of the conservatism except to the extent that you accept the probabilistic view of things. The probabilistic view of things tells you that things which crack the vessel at about 10^{-2} in the deterministic view are at about 10^{-6} in the probabilistic view. If that is valid, that is a measure of the conservatism of what we are doing and I myself derive a great deal of comfort from that, although I cannot defend the precision of that kind of a number.

MR. OKRENT: I have a question that relates to your discussion of the safety goal. I realize that you say that you are correcting now what is in the draft report on pages --

MR. HANAUER: 8-something.

MR. OKRENT: Nevertheless, I am a little curious about something. In making the case in the draft, the factor of 16 was not used. Nevertheless, the

Staff was able to conclude that they were meeting the safety goal and this required values of Y, which is the likelihood of a large release given a PTS which led to core melt, values smaller than one in 100.

Somehow the Staff seemed to be able to accept that and now, if I have done my arithmetic correctly, at 10^{-5} you would still need values smaller than one in ten for this to hold. But I am trying to understand how it is one could present the material in the draft in view of the absence of analyses which are stated in the draft and which you had stated orally.

I just do not understand how that could have appeared, and was there some real consideration of it. It is almost as if there was a position and whatever came out would fit, if I can be harsh.

MR. HANAUER: Well, I wrote the passage in question and I will tell you what was in my mind. The problem is not that Y has to be less than one over 100, but the product XY has to be less than one in 100.

MR. OKRENT: But that is not the way it is written. It is written XF equal to or less than 10^{-5} per reactor year, which rests on Y. In your mind you may have had a product, but it was not written that way.

MR. HANAUER: The basis was two thoughts. One is that our best evaluation is that the circumferential crack, the high consequence circumferential crack, was very improbably because of the way we had chosen the screening criterion and because of the high likelihood that the vessel would not jump, and that for large, dry containments -- which is all I considered because I do not have a containment matrix for an ice condensor plant and the water containments are not relevant here -- for large, dry containments many PRAs show Y to be a low value for the mixture of the various kinds of things that can melt cores.

I projected this onto pressurized thermal shock after some non-quantitative consideration of what kinds of things happen if you put a split in the vessel. There is nothing more than that.

MR. OKRENT: I guess if it turns out that you can get these very small values of Y, it would be very nice, and I do recall that for Indian Point and for Zion and, I believe, for Midland, in fact, the reactor vessel cavity was strengthened to cope with this kind of split. In other words, it was not something that was there originally, so I have to assume that at least as of now there is some question as to whether you generate problems with that particular structure in some of the PWRs you are thinking about.

MR. KERR: I would like to understand this discussion and I do not understand the question you are raising. Do you understand?

MR. HANAUER: Yes, sir. Yes, sir.

MR. KERR: What is the question? Could you tell me what the question is, Dave?

(Pause.)

MR. OKRENT: I just find the draft, as it is worded, to give me little confidence for seeing how one gets from the stated value of XF to the --

MR. KERR: I understood that, but I did not understand the question you were raising. That is a statement -- that you do not have much confidence in those numbers. What is the question?

MR. OKRENT: I am trying to understand the basis by which the Staff thought it was plausible to arrive at the conclusion by this chain of logic that one would be meeting the safety goal -- in fact I have the same question even with this new set of numbers, which includes this factor of 16, because, as I have just mentioned, implicit in this -- if you take XF at 10^{-5} -- is that the containment has to keep one from having a substantial release more than nine times in ten, giving a PTS failure and a core melt.

MR. MARK: Safety goal is not the probability of release. It is the probability of a death..

MR. HANAUER: Yes, sir.

MR. OKRENT: That has been factored in with a factor of 16 now. I think you needed some factor. I agree. I do not know whether it is 16 or not, so I am not questioning that there should be some such number.

MR. MARK: That is in the 16, and Y is just a major release, Category 1.

MR. OKRENT: One or 2, 2.5 -- something like that. Okay, I will let it go.

MR. BENDER: I wanted to try to understand a couple of numbers -- 10^{-6} and 10^{-2} -- in a slightly different way.

If I were to postulate that if I went to the Grand Canyon the chances are about one in a million that I will fall over the cliff and kill myself, is that about equivalent to what the 10^{-6} number is you are talking about here?

MR. HANAUER: I have not any idea. I do know that when you got on the airplane to come to Washington your statistical chance of arriving at your destination alive is 999,999 out of one million, and your chance of not arriving here alive is about one in a million per flight.

MR. LEWIS: The chance of arriving at the wrong place is much higher.

MR. BENDER: There is a probability in this case that the pressurized thermal shock will propagate in a way that it would kill someone is about one in a million. Is that what you are telling us?

MR. HANAUER: That is not what I am telling you.

MR. BENDER: What did you tell us? I will come to the 10^{-2} in a minute, but tell us what you told us.

MR. HANAUER: Given the whole spectrum of overcooling events, the probability for each one that the vessel would break, the probability that that would melt the core, release a bunch of stuff, and hurt someone, and adding them up over all the modes in which this could happen, the result in this calculation is about 10^{-6} per reactor year that pressurized thermal shock will hurt someone.

MR. BENDER: In three years it is about three times that?

MR. HANAUER: Yes, sir.

MR. BENDER: Now for a reactor, for one reactor I think we are talking about -- ten reactors, or thirty.

MR. HANAUER: Yes, sir.

MR. BENDER: Let us talk about the 10^{-2} number. That is a separate number, as I understand it. There is a chance of one in 100 that the event will occur. That is independent of the 10^{-6}.

MR. HANAUER: The 10^{-2} came from a study of the eight events that have happened and if life goes on the way life was when those eight events happened, we would predict something worse than the screening criterion about every 100 reactor years.

MR. BENDER: Now if I happen to live in Germany, using your airplane as an example, the chances are probably about one in 100 that I will fly from where I live to Washington, is it fair to say that in the context of the likelihood of these events coming about that it really is 10^{-8}?

MR. HANAUER: No, sir. Those are two independent calculations. The 10^{-6} has something like that 10^{-2} already in it. You cannot multiply them.

MR. BENDER: That already assumes an event is occurring?

MR. HANAUER: No, sir. That gives the probability the event will occur.

MR. OKRENT: I think, Mike, what he is saying is, crudely, of the order of one in 100 per reactor year you get an overcooling event, and, given one of these, another 10^{-4} to get to the serious release. Okay?

MR. HANAUER: Actually it is vessel failure. I misspoke myself.

MR. SHEWMON: 10^{-4} of what he calls vessel failure or vessel cracking, depending on which slide you looked at.

MR. OKRENT: So 10^{-6} multiplies the fair probabilities, one of which is the chance of getting an event.

MR. BENDER: And having accepted that, how am I dealing with all these uncertainties that have to do with the complications? Where are they hidden in this 10^{-4}?

MR. HANAUER: The uncertainties are in many places. There is the measurement of uncertainty of materials properties in the probabilistic calculation. That is included in a distribution of vessel properties for each value of mean. There is the uncertainty in how cracks grow. That is done conservatively in our model, even in the probabilistic one.

There are the uncertainties whether one has all of the events which could occur. We are sure that is not conservative because we have not included the operator doing something dumb and unforeseen. Some of them are explicit, like the materials property uncertainty, and some of them would just have to be -- they are kind of outside the numbers. We do not know how to put them in the numbers.

MR. SHEWMON: Some of them have to do with whether there are cracks there that size or not, and that is what we will get to next, when we can get finished with Dr. Hanauer.

MR. BENDER: I have stopped asking questions.

MR. EBERSOLE: Could you comment? Within this 10^{-4} bandwith, just what do you expect of the operator? Is this a super operator, an average operator? Do you expect him to do something within eight seconds or minutes?

MR. HANAUER: This is an operator who is average as regards sins of omission. There are probabilities in there for him forgetting to do things he should do. What is better than average with regard to doing bad, unforeseen things for which zero is the probability in this model --

MR. KERR: Am I correct that there is no credit given for him being a very good operator and doing something ameliorative?

MR. HANAUER: Well, it is in there with the probability which is characteristic of average operators. It comes out of Swain's handbook.

MR. KERR: Thank you.

MR. HANAUER: Mr. Clifford, have I represented you correctly?

THE REPORTER: I can't hear you.

MR. HANAUER: He said, talk to Mr. Israel.

(Laughter.)

MR. ISRAEL: Israel of the Staff. You have to look at the individual events. The events that we looked at, the one that is driving, is the small break LOCA. For that event, everything works. The plant depressurizes, you have stagnation in the pumping HPCI, all the HPCI pumps work and that is the result. There is no operator action, detrimental or beneficial.

I do not know that anybody has looked at the potential beneficial actions. They would have to probably occur within twenty minutes or so -- whatever they were going to be. That was the critical time frame.

MR. KERR: It assumes that you trip the pumps, the coolant pumps.

MR. CLIFFORD: That is true, but the size range, the pumps would probably go anyway.

Now let me just carry on. The next one was the steam line break and there we gave credit for the operator terminating auxiliary feedwater to the faulted steam generator. If he does this in a relatively short period of time, you can limit the cooldown for most of the events.

The event that probably was driving was the event at zero power. My recollection was the temperature came down to 200 degree anyway, regardless of whether the operator terminated auxiliary feedwater and for the steam line break events we postulated that they went back in pressure.

The steam line break you get a depressurization in the primary system. The HPCI comes on. That keeps pumping away. You stay at low pressure until you start filling up the pressurizer, and then at some point, if you have a low pressure HPCI system, the pumps would stop pushing in the water. However, the residual water in the primary system would ultimately depressurize you.

MR. SHEWMON: Doctor Carbon,, you had a question.

MR. CARBON: Steve, do you have any feeling for what the probability is for the operator doing some bizarre action and are you going to look at what opportunities are available to him to do so and try and get a good feel?

MR. HANAUER: Not in the context of

pressurized thermal shock. We have programs in human factors that address that in a very general way. I do not plan to wait for them in completing pressurized thermal shock. It is simply an incompleteness, as in all probabilistic evaluations.

Now that is included in the evaluation of experience which was one of the important reasons not to walk away from that.

MR. SHEWMON: You see, the inverse of that is, Max, they have done audits on the procedures or the training of the operators in these plants to recognize overcooling events and to try to trace the line between overpressurization and overcooling or something of that sort. So in a sense they are looking at the operators and one would hope that would also cut down the chances of bizarre events on the particular plants which are critical here.

Are we ready to go on?

MR. BENDER: Just because we have become a little uncertain about it, there is some Staff disagreement. Do we know what the disagreements are?

MR. HANAUER: You have the two Staff members with differing views here.

MR. SHEWMON: You have Bastikos' comments and these are editorial comments on the reports and things which he feels the report does not reflect his viewpoint.

MR. BENDER: Was that one of the --

MR. HANAUER: Yes, sir, and the very last item in Mr. Bastikos' memorandum is a general disagreement with the conclusions of our report.

MR. BENDER: What is the other one?

MR. HANAUER: The other is Mr. Israel, and I will invite him to speak his piece to the Committee.

MR. SHEWMON: He has not committed any of these to paper yet, so I hope they are not too diffuse.

MR. ISRAEL: No. Mostly my comments deal with the draft report and since that time the draft report has been re-edited and most of my comments were taken care of and Dr. Hanauer in this Committee has brought up points I had made.

The only one I wish to make at this time is the fact that, well, I guess people are fixating on the probability curve of the small break LOCA, which Joe Sneider, who is involved in this, was very vehement about that sort of thing in an absolute sense. I will just reiterate his concerns.

I would also like to point out that what is driving that curve is the small break LOCA. The small break LOCAs that we looked at are small break LOCAs where the hole opens up, the plant depressurized down to whatever the pressures and temperatures as well. That was basically obtained, this type of insight was obtained from the Westinghouse owners' group.

The events that we had not potentially looked at are the events where we have stagnation in the loop, pump in cold water, and essentially have the same type of fluid conditions. Subsequently, at some later time, you repressurize. Those type of events could potentially change the frequencies that you are seeing at the 210-degree figure -- figure 21 or figure 22.

So it is that uncertainty that I just wanted you to realize for completeness.

MR. MARK: Perhaps Israel or perhaps Steve, I learned from figure 22 that steam line breaks are rather more probable than 10^{-1} and that I find surprising.

MR. HANAUER: Steam line breaks, I do not see how you learn it from figure 22, but they are --

MR. MARK: You find out that with a two times 10^{-2} factor at 250 degrees are divided, 1.5 times.

MR. HANAUER: But that will not do. The conclusion is correct, but that calculation is not correct. It is the result of a very large number. It is the Monte Carlo and it is the interaction of details on these distributions for which you have quoted only the centraus.

In fact, steam line breaks occur not only from rending metal but from the opening of bypass power-operated relief and safety valves because the slow ones are more severe than the large ones and, therefore, the frequency of the small ones is indeed quite high.

MR. MARK: Whereas the frequency of the small break LOCA is, ballpark, three times 10^{-2}?

MR. HANAUER: Three times 10^{-4}.

MR. MARK: There is a 10^{-2} for getting down to the temperature.

MR. HANAUER: I am sorry. That is not correct for the small break LOCA. You are trying to make something too simple out of these curves. The frequency of the small break LOCA in this size range, the restricted range in which stagnation occurs in the loop flow, has been evaluated to be about three times 10^{-4}.

Again, the components are rending metal, leading to breaks of this size, plus combinations of safety valves sticking open, multiple power-operated relief valves opening, and multiple coolant pump seal failures. But in every case the results from these curves is a combination of this three times 10^{-4} times a probability of breaking -- cracking the vessel, if you prefer -- given any one of these possible sequences.

MR. MARK: And the possibility of undercooling.

MR. HANAUER: The probability of the undercooling I just told you. If you have a loss of coolant accident in this break size, it will, with probability one, lead to this overcooling.

MR. SHEWMON: Okay for now? Good. I do not see any other hands. I am almost going blind, but go ahead.

MR. AXTMANN: I am somewhat bemused by the thing that is missing today over and above -- excuse me, missing from the Staff position paper, and the discussion that we had at the Subcommittee meeting, where the time for action was defined as about three years from now.

MR. HANAUER: The remaining vugraphs have not yet been presented with that information.

MR. SHEWMON: You are not through yet?

MR. HANAUER: No, sir.

MR. SHEWMON: Let's let him finish, then.

MR. AXTMANN: That is fine.

MR. SHEWMON: Please get through.

MR. HANAUER: Very quickly.

(Slide.)

MR. HANAUER: The proposed program. We

conclude no need for immediate modifications. Plant-specific analysis is needed for those plants which have embrittled vessels. The screening criterion I have discussed.

The Committee -- the Stello Committee -- has asked us to reconsider who should do plant-specific analysis both on vessel material properties and on detailed evaluations of the kind I have been talking about, when this should come, and, in particular for flux reduction programs, how this can be looked at somewhat earlier.

We have looked at flux reduction programs and we consider four cases. The first one is do nothing. The second one, implement the so-called low leakage course that has been talked about by the owners' group, and give about a factor of two reduction.

There is a much more drastic low leakage core used in Germany where the outer row of fuel elements is replaced by a row of dummy fuel elements with stainless steel pins. This produces a very large reduction in the flux, but involves a potential degrading, and we are now at the Committee -- the Stello Committee's request looking at what might happen if we considered various ways of trading off relief from derating for improved pressurized thermal shock.

MR. KERR: Steve, is there any way of estimating about how much rating?

MR. HANAUER: We hear numbers between five and 20 percent. It is extremely plant-specific.

MR. KERR: Thank you.

MR. HANAUER: The biggest hole in this whole thing is, first of all, to delineate well what is needed in the plant-specific evaluation, to do, as Mr. Axtmann asked, to consider when it should be done, by how many owners, and to arrange a scheme to get some of these long-range things like flux reduction and improved instumentation and controls -- Mr. Ebersole's question two hours ago -- and to get that done much earlier than when the plant actually gets within three years of reaching the screening criterion, which was the Staff's proposal in the draft.

So this is really kind of up for further consideration and any guidance the Committee might choose to give us would be gratefully accepted. The thing which is left out here and which we have not done is to decide what is an acceptable plant-specific evaluation and what is an acceptable plant-specific plant after the evaluation has been done.

That turned out to be a very difficult problem -- no simple characterization. For example, a value of RTNDT above which thou shalt not pass seems to fit the proper requirements for spending millions of dollars of the public's money on backfitting or shutting down, and yet such criteria need to be developed.

The point of the earlier conclusion is that there is some time to improve our plant-specific understanding and do that in a better way.

(Slide.)

MR. HANAUER: Finally, I will flash in front of you the long-term program, including research and such which requires no detailed discussion.

That is the end of my presentation, Mr. Chairman.

MR. SHEWMON: All right. Does that take care of your question?

MR. AXTMANN: I think the timetable is reasonable for trying to resolve the problem in three years, but I think tieing it to calculated figures for RTNDT and estimated changes in RTNDT in three years is kind of artificial.

MR. HANAUER: Those numbers have already been calculated and are subject only to further refinement. Those numbers are presented for every PWR vessel in Appendix P to our draft report.

MR. AXTMANN: Yes, with a certain

uncertainty.

MR. HANAUER: That uncertainty will be improved to some extent. We are talking about the leaders of the parade now, but that would only lead to refinements of those numbers.

MR. SHEWMON: Run, quickly.

MR. HANAUER: Thank you, Mr. Chairman.

We have two other presentations this morning which I hope will be somewhat brief because I am sure you will have questions to extend to them. One of these is from the Staff, and that will be Mr. Blake, who will talk about what kind of inspections are done and I guess the probability that there are flaws there and whether we can find them if there were, or what is going on in an area and indeed what the chances of having these flaws are in our findings. Let's leave it that way.

MR. BLAKE: I am here in response to some questions which came up at the Subcommittee meeting.

As I understand, the questions that were asked were on the vessels that have been inspected for the conditions of the cladding surface and the effect on examination and the capability of ultrasonic inspection procedures to detect the underclad cracks.

(Slide.)

MR. BLAKE: I am here because three vessels have been inspected in accordance with Reg Guide 1.150 in Region II, of which I witnessed two of them -- the Robinson inspection and the Turkey Point-3 inspection. We also had Oconee-1 inspected in accordance with the best effort to the first edition of Reg Guide 1.150.

Three vessels were inspected by three different inspection agencies using different approaches. It came back to the first question. These are some test blocks, pictures of test blocks used in UT studies which show an as-welded and a hand-ground condition which, without knowing the background of the blocks, I would guess they were a hand-welded product, and it is only --

MR. SHEWMON: Sir, you are standing right in front of the screen for several of us. There is a wooden pointer around to help you.

MR. BLAKE: This is the as-welded and hand-ground presentation. In my experience of looking at the Robinson vessel, the Turkey Point-3 vessel, as well as St. Lucie in pre-service inspection, they are all presented as machine-welded. The surfaces are considerably smoother than what is shown here.

MR. SHEWMON: The usual technique looks for flaws beneath the surface and beneath the cladding.

MR. BLAKE: That is true.

MR. SHEWMON: What we are most interested in is the degree you can find surface flaws with the approved techniques that are being used.

MR. BLAKE: By "surface" do you mean under the cladding or in the cladding that were propagating into the surface?

MR. SHEWMON: Yes, but within the first half inch or an inch of the surface.

MR. BLAKE: Okay. That is the next part. I just put this up here to show you that when you start talking about degree of roughness of cladding, grinding is not always necessary because some of the as-welded products can be smoother than some of the ground products and you just have to go on a plant-specific basis.

The next page shows the experience in Region 2, the methods in tabular form of how the inspections were done and by who and when. The Reg. Guide came out in June of 1981. The first plant inspected in Region 2 was in June 1981 just about the time the Reg. Guide hit the street. It was inspected by Southwest Research using contact methods, 45 degree sheer wave, single element, fill view pad. The next was done in July of '81 of Oconee 1, the same frequency, but emergent technique using 70 degree sheer wave, single element, and scanned to a depth of approximately 2 inches, the area of interest from the surface of the clad.

In March and April of '82, Westinghouse did the Robinson plant using the 60 degree refractive longitudinal with the same technique.

The next one on the list is Turkey Point 4, which is scheduled, the last word I heard from PF&L is November 3 of this year.

To give you an idea of what we are talking about here, we are talking about the Westinghouse approach being dual transducers, the web guide in between to keep interference down, generating at 60 degree wave that is picked up by the other transducer. The technique they use for determining this, by the way, is an array in pairs where you are looking in the same area from two different directions at the same time.

When something is found by -- that triggers the signal that you are above a certain DAC level or the area of interest, then the transducers are put on a pitch sketch mode, and the indication, whatever it is, is viewed from four different directions to make a determination of how real the signal is.

(Slide.)

MR. BLAKE: The Southwest Research method that I witnessed at Turkey Point is a contact 45 degree sheer with the bouncer signal off the outside wall, by getting out the response for the majority of the metal path, then they look at the signal that is generated from this area to the cladding with particular interest being made at the clad. The cladding interface is very distinctive, and what they are looking for here is anything that is coming up in this area.

I will get into the results of these in a second.

There was a presentation on B&W. They are using a single element with the immersion in the standoff and looking at a 70 degree angle to determine what is in the first two inches, including the cladding.

(Slide.)

MR. BLAKE: The results of these three inspections were not in your handout, but the reports, Oconee report and the Robinson report were both submitted. For Robinson there were 36 indications detected, of which 21 at the time that they -- when they started putting these in the signal pulse acromode they were able to screen out 21 as being extraneous noises, not real signals, leaving them with 15 indications, of which 13 were circumferential, two of them are actual, all of them appeared in the cladding, were determined to be in the cladding. And based on fabrication records, this cladding material had a history of having slag inclusions between weld passes. They found that during the baseline inspection and the fabrication inspection at CE.

So they have indications in the circumferential welds, no indications at the interface or in the vessel material.

They did have one problem in their inspection. It is that they inspected what they thought was longitudinal welds, and after the fact, they found out that because of some discrepancies in the fabrication documentation on the vessel, they were actually inspecting an area of base material and they missed the welds. They had a -- as I understand it, they planned to go back at a later time and to complete the inspection. They just ran out of time.

The Westinghouse equipment ran into problems and they just weren't able to do it this outage.

(Slide.)

MR. BLAKE: The Turkey Point recorded no recordable indications.

MR. SHEWMON: No recordable or no reportable.

MR. BLAKE: Recordable, which in itself leads to no reportable. No recordable, but that may be because of the direction that they are looking in because they are examining from the base metal towards the cladding. They are using the cladding as the end point of their examination, and they are really looking at an area of base material under the cladding for cracking in that area, and they may not be seeing anything in the cladding. That is my speculation.

We have Southwest Research here making another presentation later. Maybe they can go into that in more detal.

The Oconee, where they were looking at from one direction with a 70 degree sheer, they reported 16 indications, and all of them were determined to be slag inclusions or manufacturing type inclusions in the clad material.

MR. SHEWMON: In the clad?

MR. BLAKE: In the clad material itself, developed from the welding process.

MR. EBERSOLE: Don't you have to look in two directions for these scans?

MR. BLAKE: In fact, they are looked at from four directions.

MR. EBERSOLE: Four directions.

MR. BLAKE: I just schematically put it up.

MR. EBERSOLE: You looked at one you said, at Oconee.

MR. BLAKE: No, I looked at -- I observed the Turkey Point and Southwest Research inspections. I had another inspector that looked at Oconee, and we had, NRR had a consultant. We borrowed the consultant.

MR. SHEWMON: Please get on to your recommendations. They are good ones.

MR. BLAKE: Getting on to my recommendations, Jack and I came up with this. Based on what we have, we think we ought to implement this Reg. Guide. The Reg. Guide came out and was intended to be self-implementing. In effect, before the dust settled, the generic group instituted -- we have a Reg. Guide out that says things shall be done. That is why people started scrambling to do them, and then other people started saying, wait a minute, a Reg. Guide is a recommendation, and then there's no generic letter, there is no bulletin requiring it, and it is still that way.

The Owners Group got together. They formed an ad hoc committee. They did an awful lot of work reviewing the Reg. Guide and coming up -- recognizing, they finally started telling people that you are not inspecting under the clad, and that's the area that's important. All the inspections up to this time have been looking at the outside wall of the vessel. Now we are saying the inside wall is important. So that needs to be implemented, including the recommendations from the Owners Group. We need to require or demonstrate the capability of their procedures. We have looked at three different methods, all three of them by their -- by the people that generated them were touted to be the best in the industry. They are finding what we are looking for. We want that to be demonstrated.

Then we need some kind of research to determine the confidence levels of the probability of detection of the underclad cracking. We don't have any quantitative numbers that I'm aware of that say what the chances of using any particular inspection technique would find them.

MR. SHEWMON: Would you talk a little bit about hose these recommendations, and particularly the Reg. Guide, would help on protecting stainless steel piping, because there we have pretty good evidence that staff approved procedures or didn't find them at Nine Mile Point until they started to leak, and then people went back and said, well, yes there were indications, but they weren't reportable or something or other.

MR. BLAKE: That particular subject was the subject of another reg guide. I think Mr. Chang can discuss that at better length. That is a little bit different problem. We are talking about the manual scanning using the manual process. Recording of the indications in most cases is not done. The indications are evaluated on the spot by the operator who in a lot of cases this is -- I'm not trying to cast any disparaging remarks on the industry. There are a lot of people out there that are certified to calibrate an instrument who may or may not have ever seen a crack appear on the scope.

But you can obtain a level 2 qualification certification on ultrasonics and be able to do nothing to calibrate the instrument. That is at the extreme end. There are an awful lot of good ones out there that they can tell you they can distinguish between slag and crack, and very convincingly so.

MR. SHEWMON: Are there any questions for Mr. Blake?

MR. KERR: What is meant by implementing Reg Guide 1.150?

MR. BLAKE: Make it a requirement.

MR. KERR: Then it's no longer a reg guide. You're suggesting it become a regulation?

MR. BLAKE: I am suggesting that with a generic letter or a bulletin or whatever it takes to do it, let's make it a requirement that when vessel inspections are done they are done using the techniques and methods that are described in the reg guide.

MR. KERR: So you're suggesting that it become a requirement rather than a reg guide.

MR. BLAKE: Yes, sir.

MR. SHEWMON: Thank you very much.

Mr. Whiting.

Would you start by identifying yourself and say a little bit about what you do for a living?

MR. WHITING: My name is Alan Whiting. I work for Southwest Research Institute in San Antonio, Texas. One of the major things that I've been involved with over the last twenty years is the reactor inspection business. The performance of pre-service, in-service examinations constitute a major activity in the division that I represent at the Institute.

I was asked to come and speak today concerning I guess the sequel to the presentation that was just made from the perspective of a vendor, and kind of what's happening out there today, and the performance of examinations of primarily dealing with the reactor pressure vessel, and more specifically, the concern area being the inside surface of the reactor pressure vessel and to some depth within the wall.

There has already been a presentation or a picture given to you of the type of clad material that exists on these reactor pressure vessels. This exists on all the PWR systems on the ID surface. It ranges between a quarter and a little over three-eights of an inch in thickness. It is put on in many different ways. On the vintages of the plants that are in the states today, manual overlay clad is the most common. Machine process starts to be applied to some of the vessels that were on this list that has been presented today.

So I agree wholeheartedly that it's a plant-by-plant question because you find all conditions existent in the field today from as-clad surface to ground with what is termed valleys remaining, the position between the well beads still showing, to a condition where it is ground smooth. The ground smooth regions of the reactor pressure vessel have historically been in the nozzle blend radius area on the ID surface of the vessel where the intersection between the nozzle and the vessel come together. This was an early defined area of maximum stress, and they anticipated that there might need be more concern spent in reviewing that area as the examinations were performed through the life of the plant, so they prepared those regions by surface grinding.

We have worked many years in this, since the mid-60s and through the evolution of Section 11 and through the ongoing evolutions of the code requirements both from the code and the NRC.

Just a little background on the subject that was presented here a minute ago -- we typically used a 45 degree sheer wave V-path examination in the core belt region of the reactor pressure vessel to assess the integrity between the cladding, immediately beneath the cladding on the structure.

The reason that was done historically is when the first requirements came out, the requirement to find reflectors on the order of one-half an inch deep was considered the level of information desired. Since that time there's obviously been some additional concern, particularly in these units that are being addressed today, that there might need to be defined smaller reflectors in that zone.

We have a lot of history of work that has been done to qualify this V-path examination technique below the cladding on the former size reflector that we just identified on the order of half an inch in depth. We felt comfortable through the years in being able to define a quarter of an inch below the cladding on a very highly reproducible and reliable base.

The detection of that type of indication -- I want to delineate the difference between detection and sizing because it's very important. The detection is the thing that has been asked for. A hundred percent reliability of detection is what we'd like to strive for. We feel we can accomplish that if it is set on this kind of a basis.

Now, the sizing has always been the next question, particularly since the advent of fracture mechanics. To do that you can use other technology. There is other technology that's in use today that does get to that circumstance. I would just mention a little bit of what transpired and has transpired and is transpiring just today. When I say "just today," it's been within the last year in regard to this question of under-clad and the requirement to find the smaller target in as reliable a manner in the detection mode as the larger target had been concerned before.

One of the things that is key to the ability of any ultrasonic technique to do the job of finding the indication of concern needs to get away from the subjective amplitude. We find that amplitude historically has been a basis for determining whether something is fair or not. We have found over the years that an equally important criterion to apply is the location where the reflection is coming from. So information about the time base or the time of flight of the ultrasonic beam is an important consideration to have as part of your analysis with these two things. And I think this can go to some degree to explain what happened at Nine Mile Point. I was asked to touch on that maybe a little later.

The thing we have looked for in this concern is, as was evidenced here when the presentation was made about how Southwest applies a 45 V-path examination bouncing off the outside wall and coming back up -- that's a long metal path distance. There is beam spread and the possibility of redirected energy as it penetrates the cladding the first time and goes back, and then the possibility of being somewhere where you are off some amount over here when it returns to the clad surface.

That is all well and good, and we can certainly go through the laws of physics and show that is the case, but it doesn't become a real problem as long as the symmetry of the vessel is basically as it is in the core belt region: parallel wall and reasonably uniform throughout the circumference.

The reason it doesn't become a problem is you have the guaranteed monitoring mode of the cladding interface available on the instrument so that you can see exactly the zone of interest -- the zone of interest here being the region of the -- the area of the base material immediately below the cladding preceding the signals you get from the cladding-base metal interface. So that feature has given us a great deal of ability to detect reflectors that occur at that point.

MR. SHEWMON: Does this allow you to go on both sides? You certainly then can see any reflections that come back before you get to the interface. Can you also -- do you also study what is in the cladding, in that V-mode?

MR. WHITING: It's possible to do that. We typically have gated the region because of the area of interest. What is in the cladding has not been of interest to us. It's not been a requirement to determine that from the code perspective or from the NRC heretofore. We were interested in knowing what was in the base material immediately below the cladding. We can in fact see that region, because what you do there is you move it further out.

The difficulty with this approach for

assessing what is in the cladding with the full V is that you may have a masking of some of the information that's in the cladding because you have the presence of the cladding-base metal interface. So on that basis and since there was some interest in changing the target size of this reflector, there was a need to generate a supplemental technology to go and be able to perform in that area immediately below the cladding, which might consider in the cladding as well.

This came about actually at the first stage of time in France. It utilized and implemented the technology that was developed in Germany called the BAM probe. What it amounts to in principle is a high angle reflected longitudinal probe on the order of 270 degrees that is mounted in one unit typically. You saw a concept of it here presented as a 60 degree, I believe. It was the longitudinal, the longitudinal mode rather than the sheer mode of energy.

One of the reasons for the longitudinal mode being utilized is it gives better penetration ability through the stainless steel material, the cladding being stainless. This was used first primarily for the nozzle regions for the vessels in France because they had cracking problems in their nozzles. We then applied that technology. We had some of those probes. We adapted some in our labs. We have some test results where we looked at the principle that was applied. We actually built some search units that do a little bit different kind of a focusing that we wanted to implement, and we compared them with the standard BAM delivery.

We took those probes and used them in Korea in an examination within the nozzles of the reactor vessel there. We also have utilized those in looking at indications where we had excessive clad noise.

In our normal 45 mode examination V-path we found regions where we had a little higher noise from the metallurgical interface between the cladding and the base material. We then supplemented our examination and looked in those areas.

MR. SHEWMON: Soon could we get you to go on to what you think should be done to increase one's confidence that you will find flaws by MVE techniques in the near-cladding region or in the stainless steel?

MR. WHITING: Okay. Since the question came up on Turkey Point 3, which is a vessel that's to be examined here in November -- Turkey 4, I'm sorry; 3 is the one we did last year. Turkey 4 is the one to be examined.

The intent in the core belt region or the region of interest, of concern is to apply the approach we've done to the Section 11 examination and at that time apply the 45 V-path exam that we've always done through the course of the material, but then supplement that examination with a high angle reflected longitudinal exam in the weld region and a half T of base material on either side of the weld region, both looking from the multiple directions that were addressed here -- that was the intent today -- to guarantee that we will in fact see reflectors that might be of interest beneath the cladding.

MR. SHEWMON: Is it quite possible with the standards which the Section 11 allows you to use and the level of what you can report that there would be half or one-inch cracks there, and they're easily visible but not recordable, or they are visible but not recordable?

I have the question I've been had on occasion by what has to be reported and what is feasible to be reported.

MR. WHITING: Recorded versus reported. I don't feel because of some other information that I gave you that deals with sizing that there would be any question about whether or not there would be an indication in there on that order of magnitude, because there would not be one that we would not know about.

MR. BENDER: Some people have said that it is necessary to find flaws of an order of a quarter of an inch deep or something like that. Is that still within the realm of capability of these detection devices?

MR. WHITING: Yes, sir. We feel very confident that we will see and flag an area of interest to do further analysis work in with the scanning mode sensitivity we're talking about on the order of a quarter of an inch. We feel like once that's been done that the possibility to determine, depending upon the condition of that surface of the cladding in that vessel, we can have a reasonably high confidence, 95 percent or better, of being able to size it down to a tenth of an inch.

MR. BENDER: Now, because I want to get this surface condition requirement clear, I would like to have you say what -- you've inspected with ground surfaces, you've inspected surfaces that are machine welded and those that are manual overlay.

Will this conclusion apply to manual overlay welds without any grounding?

MR. WHITING: The ability to size down to a quarter of an inch may be limited in that event. I would say we might be able to go to an eighth. Our experience has shown we might be able to do an eighth.

MR. BENDER: But if you satisfy that criterion, you're comfortable with manual overlay, and you'll be able to do a good inspection job?

MR. WHITING: Yes, sir. The reason I say that is we've done tests in the laboratory that substantiate that. We've also run into many occasions where you run into both vessel clad with automatic cladding as well as manual cladding.

MR. BENDER: Thank you.

MR. SHEWMON: Yes.

MR. OKRENT: A related question. Let me speculate that if people looked with the sensitivity on occasion, they will find a flaw -- a quarter inch, half inch, three-quarters of an inch. What will be the meaning on whether something should be done, a finding in other words?

MR. SHEWMON: You mean if it was there would the NRC let them start up without fixing it or what?

MR. OKRENT: Well, should they or should they not, and on what basis would such a decision maybe occur? It will shift these probabilities somewhat. But, you know, just having the flaw obviously doesn't give you a failure.

MR. BENDER: You're not addressing that to the speaker.

MR. OKRENT: No. And it's unlikely that something will be found.

MR. BENDER: As a matter of fact, I'm sure if we find them we'll wonder if we're seeing hash or flaws.

MR. SHEWMON: Would you like to leave that as a homework assignment for the staff, or would you like to respond now?

MR. OKRENT: It's probably a homework assignment. I don't know for whom.

MR. SHEWMON: I am sure Steve is wide awake and will keep that in mind.

Are there other questions for Mr. Whiting?

Would you shift a little bit to stainless steel? You must do some of that in your business also. The stainless steel piping is harder. You're talking about thinner sections. Apparently you are talking about hand-held things instead of machine and recorded or machine-driven and recorded. The cracks are tighter, branched and harder to find, and we don't always find them before they leave. And they they go back and say yes, there were indications there, or once they know there are cracks in that region then they can see things which they say yes, they're cracks, and that tends to shake one's faith in the NDT business or profession or something.

MR. WHITING: Okay. We certainly do look at lots of stainless steel. We do the balance of plant in many plants. That represents a large number typically of ostonetic weldments that are in the plant. I think again I could summarize this whole subject by the fact that the code when it evolved address carbon steel predominantly in the piping mode. It addressed low cycle fatigue as a mechanism for failure, which gives you a different type of target to write your procedures and to develop your scan plans and to teach people how to find.

When you deal with stainless steel with the cracking mechanism of stress corrosion, cracking being the object necessary to find, it takes a different kind of an approach, and the procedure becomes the key to success, the adequacy of the procedure.

When I said a while ago that one of the major benefits that you have as you use the ultrasound as an examination process is where the target is occurring. We take a great deal of advantage of the fact that we with a high degree of probability have an idea of where stress corrosion cracking will occur relative to the proximity of a weld geometry.

While the procedures do talk about sensitivity levels and those kinds of things, when we find an indication occurring on the screen in the area of interest -- we call it this window -- that we feel is a high probability of where this problem might occur, it doesn't matter what gain level it occurs at. If we get a signal that shows up there, we will investigate it. That is not an industrywide practice, and I think that may have something to do with some of the circumstances that develop as others apply the examination.

MR. SHEWMON: Do you know offhand whether Reg Guide 1.150 or whatever it was speaks to that, or is it only on vessels?

MR. WHITING: It deals with the reactor pressure vessel.

MR. SHEWMON: Is there a comparable reg guide extant or in the works which deals with stainless steel piping?

MR. CHANG: It's not in the reg guide, but there is a cold case put out by the committee which intends to cover piping, so not just the CRGR. We haven't officially adopted that one yet, but I guess the staff is in the process of evaluating that cold case.

MR. SHEWMON: Are there questions for the staff or Mr. Whiting in this area?

MR. OKRENT: I have a question again on flaws. In the probabilistic analysis what would be the difference between assuming a flaw of one-inch existed and the probabilistic distribution that was assumed to be the one used for purposes for calculation?

MR. HANAUER: The probabilistic analysis used frequency distribution of flaws and a probability distribution. The probability distribution that we used for flaws was on the order of 10^{-4}, so the difference would be for the events that involve boiling, about a factor of 10^4, but that's a very crude answer.

MR. OKRENT: But I was told that half-inch flaws, maybe even quarter-inch flaws contribute also. So I'm trying to understand the difference between assuming a flaw is there of whatever size you need and the probabilistic distribution. Is it a factor of 10?

MR. HANAUER: That is one of the principal components of that 10^{-4} difference between deterministic and probabilistic.

MR. OKRENT: All right. That's what I wanted to know. Thank you.

MR. WHITING: There are a couple of slides that might be of interest to you. They show the statistical results of some actual cracks we've interrogated with this multiple-beam satellite pulse high refractory angle here.

MR. SHEWMON: I would like to see them, but I think in view of the time and the particular place we are at I would rather not. But I thank you very much for coming in. This has been helpful.

Does that conclude what you want to develop here, Mike?

MR. BENDER: I think we've had as good a story as we are likely to be able to absorb this morning. The committee has available to it a draft of the committee position which I invite people to look at. I personally think the staff is a lot better off than they were when they started this thing a lot of months ago, but there are still some things to be done.

In spite of the fact that I have little attachment for the PRA and safety goal part of the analysis, the position which the staff is taking seems to me to be a pretty reasonable one and has a lot of conservatism in it. And I think we ought to seriously consider accepting it and recommending to the Commissioners that they accept it as a way to deal with this matter over the period of time that we have.

MR. SHEWMON: I at this point am about to call a five-minute break, as much as I hate to given the lateness relative to the schedule, so that we can clear the room of those who really don't want to stay on and hear some exciting reports about some other subcommittee activities and things of that sort, and then we'll try to get through the subcommittee reports for whatever the agenda says is three-quarters of an hour before we break for lunch.

(Whereupon, at 12:05 p.m., the committee was recessed into executive session.)

* * *

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS/270th General Meeting

Date of Proceeding: October 8, 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.

Jane N. Beach

Official Reporter (Typed)

Jane N. Beach

Official Reporter (Signature)

Attachment to 12/2/81
Submittal

Duke Power Company
Oconee Nuclear Station
Unit 1

Summary Report of the 10-Year Inservice Inspection
Reactor Vessel Welds

Introduction

This report summarizes the 10-year inservice inspection (ISI) of the reactor vessel welds at Duke Power Company's Oconee Unit #1 Nuclear Station. The inspection was performed during July and August of 1981. The reactor vessel weld inspection is only a portion of the total 10-year ISI that is being conducted. The full report will be provided following completion. Additional details of the examination results are maintained in the Duke corporate offices.

Background

The 10-year ISI of Oconee 1 was in the planning stage for many months prior to the start of the outage. In early 1981, significant efforts were started to support the inspection of the Oconee 1 vessel. Regulatory concerns relative to reactor vessel pressurized thermal shock were present as well as a draft Regulatory Guide addressing the ultrasonic testing of reactor vessel welds.

With regard to reactor vessel pressurized thermal shock, Duke decided to conduct a vessel examination that would reliably indicate the structural integrity of the beltline region welds. Further, being aware of the draft regulatory guide and its schedule for issuance, Duke determined that the requirements of the guide should be addressed and implemented where practical and technically justifiable. To this end, after several meetings with B&W, the Oconee NSSS vendor and reactor vessel examiner, Duke met with the NRC on March 24, 1981 to discuss the proposed inservice inspection of the Oconee 1 reactor vessel. The results of the meeting were used in the preparation of the final inspection plan which is described in the next section.

Examination Plan

The Oconee Unit 1 reactor vessel examination was performed in accordance with the requirements of the 1977 Edition of the ASME Boiler and Pressure Vessel Code, Section V, Article 4 with Addenda through the Summer of 1978. The recommendations of Regulatory Guide 1.150 "Ultrasonic Testing of Reactor Vessel Welds during Preservice and Inservice Examinations" were also satisfied to the extent possible, considering hardware, schedule, and engineering concerns.

The weld volume examined meets or exceeds the minimum requirements of the 1974 Edition of Section XI of the ASME Boiler and Pressure Vessel Code with Addenda through the Summer of 1975. The reactor vessel welds were prioritized in order to ensure that the minimum Code required examination would be performed and that the maximum lead time would be available in the event a flaw was detected which required a fracture mechanics analysis. A total of two outlet, four inlet, and two core flood nozzle to vessel welds and nozzle inside radius sections were examined 100% of the weld length. All six of the longitudinal welds were examined 100% of the weld length, and five of the seven circumferential welds were examined 100% of the weld length. The two exceptions were the lower head to dutchman weld, which is located in the lower head, and the upper nozzle belt to lower nozzle belt, which is located in the center of the nozzle belt. Only 5% of these weld lengths were examined.

These examinations were performed using the Automated Reactor Inspection System (ARIS) tool (See Figure 1). An additional circumferential weld located in the reactor vessel closure head was also examined; however, conventional manual contact examination techniques were used on this weld and 43% of the length was examined.

Special emphasis was directed to flaw detection at the I.D. surface. The ARIS inspection tool utilizes immersion ultrasonic examination techniques, whereby many of the variables which usually limit or preclude an effective examination of the near surface (I.D.) can be eliminated. The techniques used for this examination provide qualified sensitivity to reliably detect flaw sizes consistent with those identified in the acceptance standards of IWB-3500 of ASME Section XI. The area examined with the near surface technique on each side of the weld was approximately equal to 1.8T when scanning perpendicular to the weld and .75T when scanning parallel to the weld (see Figure 2). This is substantially more than required by Code and, in the beltline region, amounts to approximately 60% of the total surface area.

Figures 3 and 4 identify the reactor vessel and closure head welds examined in accordance with Regulatory Guide 1.150. Each weld location number identified in these figures corresponds to a figure and weld identification number as identified in Table 1, Weld Examination Summary Evaluation reports, included in Appendix A, which are referenced by a specific figure number for each weld.

Examination Results

A total of 133 indications were recorded, all of which were acceptable to the Section XI evaluation criteria. Of the 133 indications recorded, 114 were laminar reflectors.[1] The remaining indications were comprised of 16 seventy degree and 3 sixty degree reflectors. The 114 laminar indications were less than 16% of the allowable limit of Table IWB-3510.2 of Section XI.

---

[1] An indication is considered to be laminar if it is oriented on a plane within 10 degrees of being parallel to the component surface.

The 16 seventy degree indications are manufacturing-induced slag inclusions, all of which were located in the clad material applied following removal of the mid shell to lower shell circumferential weld backing ring. Since the clad is not considered as part of the pressure retaining boundary of the component, no Section XI evaluation is required for these indications. A precautionary evaluation was performed, however, at the time of examination as it was not known for sure that the indications were located in the clad since they occurred at a depth slightly greater than the nominal clad thickness. These indications ranged from 8.46% to 94.3% of the Section XI acceptance criteria. It was later determined that the clad was thicker in the areas where the backing rings had been removed and that the indications were located in the clad as mentioned previously.

The 3 sixty degree indications were subsurface reflectors and could be correlated to baseline reflectors in the same general area. These indications are planar flaws[2] which do not exceed the Section XI acceptance criteria. A detailed evaluation of the 3 sixty degree indications referencing size and location is shown in Figures 5 and 6.

The 70° flaw sizing techniques used were applied to a calibration notch which is 0.20 inches in the through wall direction, starting at the clad-base metal interface and penetrating into the base material. The notch is perpendicular to the clad surface of the calibration block. The results are that at 50% DAC, the recorded size of the simulated flaw is 0.25 inches. This represents a recorded dimension 25% greater than actual flaw size. At 20% DAC, the recorded size of the simulated flaw is 0.60 inches, which represents a recorded size 300% greater than actual flaw size. The data suggests that indications sized to the examination technique are conservative measurements and actual flaw size would be less than the recorded flaw size.

A complete correlation was not made between the observed indications and the baseline data due to the many differences in test variables between the type of examination performed for the baseline and that performed during this examination. The major variables include the manual contact examination technique versus automatic immersion technique; baseline examination requirements versus current examination requirements; and calibration blocks used for baseline versus calibration blocks used for this examination.

Summary

All of the indications recorded during the examination were evaluated to be manufacturing-induced and are less than the maximum allowable flaw size specified by the acceptance standards of IWB-3500 of Section XI. Based on the examination performed, there is no evidence of any service-induced flaw in the Oconee Unit 1 vessel. Specifically, the examination has provided a high degree of confidence in the beltline region in that there are no surface flaws in the pressure retaining material that exceed 0.15 inches.

---

[2] An indication is considered planar if it is oriented in a single plane, other than parallel to the surface of the component.

DRAFT

Recd 10/1/82

H. B. ROBINSON ISI NEAR SURFACE EXAMINATION SUMMARY

During the period 3, interval 1 inservice inspection of the H. B. Robinson reactor vessel, conventional ASME XI ultrasonic inspections of the upper-to-intermediate shell circumferential weld seam, the intermediate-to-lower shell circumferential weld seam, and the lower shell longitudinal weld seams were supplemented with an examination technique developed to improve detectability of near surface reflectors. Certain areas of the intermediate shell course, subsequently determined to be entirely base material, were also scanned for near surface flaw detection. The transmitting and receiving elements are separated by an acoustic wave barrier to minimize the effect of signals from the water/steel interface. The technique described herein has been employed previously to identify underclad cold cracking in reactor vessel nozzles.

Near surface examinations were conducted from the vessel inside diameter (ID) surface using dual element, transmit-receive, 2.25 MHz, focused immersion search units inclined to generate longitudinal waves at a refracted angle of 60°. Primary test sensitivity was established on a 0.125 inch diameter hole located 0.75 inches deep from the clad surface of a representative calibration block. Scanning was conducted on 0.25 inch increments in two directions parallel to the welds and in two directions perpendicular to the welds. Scan limits were set to include a minimum of 1 Thickness (T) of adjacent base material on both sides of the longitudinal weld seams and 1/2 T of adjacent base material on both sides of the circumferential weld seams. Indications equal to or exceeding 50% of the primary reference response were recorded. Sizing information was collected at 50% Distance Amplitude Correction (DAC) and 20% limits.

In order to verify the performance of procedures and equipment designated for these examinations, each calibrated transducer/inspection channel was demonstrated capable of detecting fatigue cracks in a clad test speciment. After completion of the calibration sequence described in ISI-153, Revision 1, "Inservice Inspection of Reactor Vessels", and Appendix 1, Revision 0, each transducer was scanned over a test specimen made up of three SA-533, Grade B plates, each containing a surface crack initiated via mechanical fatigue prior to overlay with 0.2 inches of stainless steel cladding. The specimen, therefore, represents a clad component containing three cracks which initiate at the clad/base metal interface and propagate to depths of nominally 0.12 inches, 0.24 inches, and 0.36 inches in base material.

All four transducer/inspection channels were demonstrated capable of detecting the cracks in both scanning directions perpendicular to the crack lengths. Maximum indication amplitudes from the 0.12 inch deep crack were in the 60% to 70% of reference range at a 4 Microsecond (μsec) metal path. Those from the 0.24 inch deep crack were in the 100% of reference to 100% of reference + 2dB range at a 6 μsec metal path, and those from the 0.36 inch deep crack were in the 100% reference +1dB to 100% reference + 3dB range at a 10 μsec metal path.

Examinations of the H. B. Robinson reactor vessel identified a total of thirty-six indications for investigation to determine their cause. Of that number, thirty-four were detected with search units scanning axially with

DRAFT

respect to the vessel (circumferential reflector orientation) and two were detected by circumferential scanning (axial indication orientation).

All indications were investigated after completion of scanning per the following:

(1) Return array to position of indication per examination results, verify water path and plate perpendicularity, and monitor detection transducer/inspection channel to verify presence of indication.
(2) Monitor transducer/inspection channel in the opposite scanning direction while scanning the area of interest.
(3) Rotate array plate 180° and verify the reflector is detectable with the complementary transducer/inspection channel.
(4) Return the array plate to its original detection position and monitor the detection transducer/inspection channel while scanning toward and away from the reflector. Determine whether the indication travels.
(5) Return to the peak amplitude location and monitor the search unit transmitting element in the pulse-echo mode to establish the position of the indication relative to the water stell interface reflection. Repeat same for the search unit receiving element. This operation will establish whether the indication is due, in fact, to surface reflections.

Using this sequence twenty-one of the thirty-six indications were interpreted to be the result of innocuous conditions such as surface conditioning or extraneous noise.

The fifteen remaining indications could not be placed in those categories because they appeared as discrete indications separate from the water/steel interface pulse when individual elements were operated in the pulse-echo mode and demonstrated some travel on the CRT.

They were, therefore, considered real reflectors and mapping commenced per procedure requirements. Thirteen of those indications were oriented circumferentially with respect to the vessel and two were oriented axially. None of the indications were detectable in two complementary scanning directions, i.e., clockwise and counter-clockwise or axial toward vessel flange and axial toward vessel bottom.

Mapping of the thirteen circumferentially oriented indications showed they were generally predicted between 0.28 and 0.34 inches from the vessel ID surface and demonstrated very little travel on the CRT. When determining indication lengths, however, it was noted that the signals appeared intermittantly over the entire scan limit and, in fact, in two cases were traced at varying amplitude over a 360° scan of the vessel suggesting some surface phenomenon or a reflector associated with the cladding process. As part of this investigation the surface condition of the cladding at the beam entry points for two of the indications were observed closely via remote television camera. In one case, the entry point appeared at a valley between beads (longitudinal weld 17/indication #14), in a second case it appeared at 1/4 bead width (longitudinal weld 17/indication #17).

This visual examination indicated the clad surfaces were generally rough, even in areas which had, apparently, been prepared for preservice ultrasonic examination.

The aid in interpretation of these results Combustion Engineering (CE), Chattanooga was contacted to discuss the fabrication history of the vessel especially with regard to the cladding process. The vessel shell courses were clad per CE procedure CE-WA-6866-273-1. The procedure calls for a three wire process; i.e., two electrodes in series with a cold wire addition. Only one layer was applied to vessel shells using a travel speed of 8ipm. Resulting clad thicknesses were on the order of 0.375 inches to 0.625 inches. In addition, CE reported that slag between adjacent beads was a problem with this procedure since the edges of the thick beads were uneven and subsequent passes sometimes resulted in entrapment at the overlap.

When examination results were evaluated with this information relative to the cladding history of the H. B. Robinson vessel shell courses, it was concluded that their depths with respect to the vessel ID surface, their orientations, and their semi-continuous nature around the vessel were consistent with results expected in instances where slag entrapment between beads was present. Subsequent to these examinations, additional laboratory studies on test samples with very irregular clad surfaces indicate that deep valleys between adjacent clad beads can defeat the function of the near surface transducer wave barrier and result in geometric indications having characteristics identical to those noted during the Robinson examinations. This phenomenon appears as a result of reflection from the valley between adjacent beads. The velocity difference between water and steel results in the indication appearing as buired in the material. This finding is still under investigation.

In either case, i.e., slag entrapment between adjacent clad beads or geometric indications due to rough clad surfaces, the circumferentially oriented indications noted during the H. B. Robinson vessel examination are acceptable per the requirements of the 1974 Edition of Section XI of the ASME Boiler and Pressure Vessel Code with Addenda through Summer 1975.

The two remaining indications, #23 and #24 were detected with transducer/inspection channel #3 during circumferential scanning of the upper-to-intermediate shell circumferential weld (#2) on two successive scan increments. During the mapping operation it was established they represented one single reflector oriented axially with respect to the vessel. The indication was only detectable in the counter-clockwise scanning direction at 130.89° vessel axis (145.89° tool axis), six and one-quarter inches below the centerline of the upper-to-intermediate shell weld or 151.25 inches from the top of the vessel flange. The indications exhibit a combined length of 0.59 inches and a combined through-wall dimension of 0.25 inches when sized to 50% of reference. The peak amplitude is at a depth of 0.56 inches from the vessel ID surface. Indication through-wall dimension at 20% of reference is 0.11 inches when beam spread off a 0.125 inch diameter side drilled hole at 0.5 inches deep is considered.

In light of information relative to the rotation of the intermediate shell long seam welds in the Robinson vessel, this reflector is located 0.89° or 1.2 inches off the centerline of the intermediate shell longitudinal weld seam at 130° vessel axis (#16).

DRAFT

Because the vessel clad thickness in this particular region is unknown the indication has been assessed as a planar surface defect. This assumption does not appear likely as laboratory studies indicate that sub-clad base metal defect conditions such as fatigue cracks, underclad cold cracks, and reheat cracks are generally detectable in two scan directions, 180° apart.

The thickness of the vessel wall in the area of interest is 9.5 inches. Using dimensions provided previously for 50% of reference sizing, the reflector aspect ratio (^{a}/1) is 0.42. The allowable dimension of a surface indication (^{a}/t) is 3.48% of the vessel wall thickness or 0.33 inches as compared to the ultrasonically determined depth of 0.25 inches. When 20% of reference sizing is considered the reflector aspect ratio is 0.16. The allowable dimension of a surface indication for this case is 2.48% of the vessel wall thickness or 0.23 inches as compared to the ultrasonically determined depth of 0.11 inches. Thus this reflector represents an acceptable condition per the requirements of the 1974 Edition of Section XI of the ASME Boiler and Pressure Vessel Code with Addenda through Summer 1975.

CONCLUSIONS

Indications detected during near surface examinations conducted during the period 3, interval 1 inservice inspection of the H. B. Robinson reactor vessel have been evaluated in terms of the 1974 Edition of Section XI of the ASME Boiler and Pressure Vessel Code with Addenda through Summer 1975 and found to be acceptable.

PRESSURIZED THERMAL SHOCK

BASIC ISSUES:

1. HAVE SOME NUCLEAR REACTOR VESSELS BECOME EMBRITTLED BY NEUTRON IRRADIATION TO THE EXTENT THAT SPECIAL PROVISIONS MUST BE MADE TO AVOID FRACTURE UNDER SOME TRANSIENTS?

2. DO WE UNDERSTAND THE TRANSIENTS WELL ENOUGH TO ESTABLISH OPERATING PROCEDURES TO AVOID FRACTURE-INDUCING SHOCK EVEN THOUGH THE FRACTURE-TOUGHNESS PROPERTIES ARE LESS THAN PRUDENT SAFETY PRACTICE WOULD PREFER?

Hanauer
T2

PRESSURIZED THERMAL SHOCK

MATERIALS QUESTIONS:

1. WHAT INFORMATION IS NEEDED TO ESTABLISH FRACTURE TOUGHNESS? MATERIALS COMPOSITION? WELD FILLER METAL EFFECTS? CLADDING STRESS LEVEL? INITIAL FRACTURE TOUGHNESS?

2. HOW EFFECTIVE ARE NON-DESTRUCTIVE EXAMINATION TECHNIQUES? WHAT CRITERIA SHOULD BE USED TO ESTABLISH DETECTABILITY?

PRESSURIZED THERMAL SHOCK

THERMAL TRANSIENT QUESTIONS:

1. WHICH TRANSIENTS ARE OF CONCERN? SMALL LOCAS? SECONDARY SYSTEM BLOWDOWN? FEEDWATER MALFUNCTIONS? FAST ECCS WATER INJECTION? UNCONTROLLED TURBINE BYPASS?

2. CAN THE HEAT TRANSFER AND TRANSPORT PHENOMENA BE COMPUTED RELIABLY?

PRESSURIZED THERMAL SHOCK

HUMAN FACTORS QUESTIONS:

1. TO WHAT DEGREE SHOULD THE OPERATOR BECOME A PART OF THE PTS PROBLEM? DIAGNOSTIC CAPABILITY? RESPONSE TIME?

2. WILL TRAINING OFFSET PREVIOUS CONCERNS?

3. COULD ALTERATION IN CURRENT OPERATING PROCEDURES LESSEN HUMAN FACTOR DEPENDENCE? E.G., REQUIRE PROMPT DEPRESSURIZATION OR EXPLICIT CONTROL ACTIONS UNDER ALL PTS CIRCUMSTANCES?

PRESSURIZED THERMAL SHOCK

ANALYTICAL METHDOLOGY:

1. SHOULD LINEAR ELASTIC FRACTURE MECHANICS BE THE ONLY BASIS FOR DETERMINING FRACTURE INITIATION AND ARREST? WHAT ABOUT 3-D ELASTIC-PLASTIC ANALYSIS?

2. WHAT THERMAL ANALYSIS TECHNIQUES SHOULD BE USED?

3. HOW SHOULD PROBABILISTIC ANALYSIS TECHNIQUES BE USED? TO ESTABLISH FLAW SIZE AND LOCATION? TO DETERMINE FRACTURE TOUGHNESS? TO ESTABLISH THERMAL SHOCK PROBABILITY?

PRESSURIZED THERMAL SHOCK

REGULATORY ACTIONS:

1. SCREENING PROCESSES
2. INFORMATION NEEDED FOR REGULATORY ACTION
3. TIMING OF ACTIONS
4. FLUENCE CONTROL

PRESSURIZED THERMAL SHOCK

EXPERIMENTAL INVESTIGATIONS:

1. NONDESTRUCTIVE EXAMINATION CAPABILITY
2. CHEMICAL COMPOSITION OF SUSPECT MATERIALS
3. CLADDING BEHAVIORAL CONTRIBUTIONS

Battelle
Pacific Northwest Laboratories
P.O. Box 999
Richland, Washington U.S.A. 99352
Telephone (509) 375-2424
Telex 15-2874

October 7, 1982

Dr. Roy H. W. Woods
Generic Issues Branch
Division of Safety Technology
Office of Nuclear Reactor Regulation
Nuclear Regulatory Commission
Phillips Building, Mail Stop 268
Washington, D.C. 20555

Dear Dr. Woods:

The following brief conclusions and recommendations by the PNL team on PTS are based on the draft NRC staff report on PTS dated September 13, 1982. We expect to revise our draft Supplement 1 to NUREG/CR-2837 to substantiate these findings.

1) The 270°F generic screening criterion for longitudinal welds is acceptable. This conclusion is largely based on the following factors:

   a. The plant specific assigned RT_{NDT} will be selected as described in Section 5 of the NRC staff report. This conservatism provides approximately 60°F to the mean RT_{NDT} used in constructing the staff's PRA results. It should be understood that the material properties conservatisms include mostly known uncertainties that reflect true variability in actual properties of vessels. Less than one-forth of the total conservatism can be attributed to measurement procedures unique to pressure vessel embrittlement that do not reflect variability in actual vessels. This added conservatism is likely more than compensated by unquantified uncertainties associated with added uncertainties of (1) key plant welds having extreme characteristics (high Cu, high Ni and high fluence), (2) extrapolation of surveillance characteristics to the vessel wall and (3) the correlation of charpy V-notch values to fracture toughness values.

   b. Using the more conservative methods described under 1.a., the probability of crack extension without arrest would have a frequency probability per reactor year of approximately 10^{-6} using the NRC staff PRA results, Figure 8-3.

   c. Currently the NRC staff PRA and operating history data analysis does not separately address each reactor type (W, B&W, CE). Therefore, the magnitude of conservatism inherent in the screening criterion is not consistent among plant types. The requirement for plant specific analysis to be started within three years of reaching the screening criteria should compensate for any specific unconservatism.

2) The predicted uncertainty of the PRA results reported as plus or minus two orders of magnitude could result in a frequency of failure of 10^{-4}. This range is apparently consistent with the safety goal(1) for core melt and significant release events. However, the vessel integrity prediction of less than 1 x 10^{-6} could be seriously compromised by PTS events. The plant specific PTS evaluations should be required to demonstrate a predicted vessel failure frequency probability of no greater than 10^{-6}(2), methods for satisfying the NRC safety goals, or an effective increase in the plant RT_C of 50°F by corrective actions before any adjustment is made to the plant specific limiting RT_{NDT}. The 50°F is approximately equivalent to two orders of magnitude on the NRC staff PRA curve, Figure 8-3.

Factors which support this conservative approach include:

Uncertainty and probability appear throughout the evaluation of pressurized thermal shock. These topics have been handled through a combination of statistical methods and conservative judgment. Overall, uncertainty has been handled about as well as available techniques, knowledge, and data permit. Even so, there are still enough imponderables so that identified conservatisms should be relaxed only with due caution. Some reasons for this caution are given below.

Operating History

Useful interpretation of the accumulated operating experience of PWRs is hampered by the facts that relatively few PTS events have occurred, and these events are not well characterized. To some extent one can avoid these difficulties by considering "distribution of exceedances"(3); that is, events that are more severe than any that have occurred to date. If we assume that the history of 350 operating years is relevant to the present 47 plants, then there is a probability of 0.118 that one of the plants will have a severe PTS event in its next operating year. Further, the basic data suggests that there is approximately a 2% chance that 1 of the 8 sensitive plants will experience a severe PTS event in its next operating year.

PRA

The techniques used in PRA provide the most sophisticated and reliable method available for assessing risk in the face of uncertainty. Unfortunately, experience suggests that failures of a complex system are frequently due to a combination of circumstances that were not, or would not have been, discovered using PRA. Also, such failures are often of the "common mode" or dependent type of failures where the occurrence of a single unfound event engenders the occurrence of several "unlikely" events which culminate in system failure. One such example is the Rancho Seco PTS event; another is the Brown's Ferry fire.

Uncertainty on RT_{NDT}

The use of a "2σ" uncertainty term for RT_{NDT} probably does not provide as high a level of confidence as was intended by the staff. An interval of the "mean ±2σ" covers 95% of a population if (1) the population has a normal distribution and (2) the mean and standard deviation are known exactly, not estimated from data. Neither of these conditions are satisfied in the present case.

VISA Analysis

The primary shortfall of the VISA code, and indeed, our present state of knowledge, is the lack of a definitive stochastic structure for the system simulated by VISA. The present structure is the default that arises from assuming that all errors or uncertainties are independent. The effect of this assumption is to make unfavorable combinations appear infrequently in the simulation. However, if an unfavorable value of some variable tends to result more frequently when some other variable is at an unfavorable value, then the estimated probabilities may be much too low.

Material Properties

Uncertainties should be applied uniformly to all forms of metal and irradiation conditions. Hence, the Reg. Guide 1.99 upper bound should not be used to replace the statistical trend curves for the high Cu, high Ni and high fluence welds. Also, an appropriate standard deviation for the initial RT_{NDT} of plate and forging metals should be used as for welds.

References

1. NUREG-0880 (for comment), Safety Goals for Nuclear Power Plants: A Discussion Paper, February 1982.
2. Report on the Integrity of Reactor Vessels for Light-Water Power Reactors, The Advisory Committee on Reactor Safeguards, January 1974.
3. Letter, Donald L. Stevens, Jr., to Dr. Roy H. M. Woods, dated June 22, 1982.

Yours truly,

L T Pedersen

L. T. Pedersen, Manager
Special Projects

LTP:mkw

cc: S. H. Bush, PNL
S. H. Hanauer, NRC
F. B. Litton, NRC

PRESSURIZED THERMAL SHOCK

PRESENTATION TO THE

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

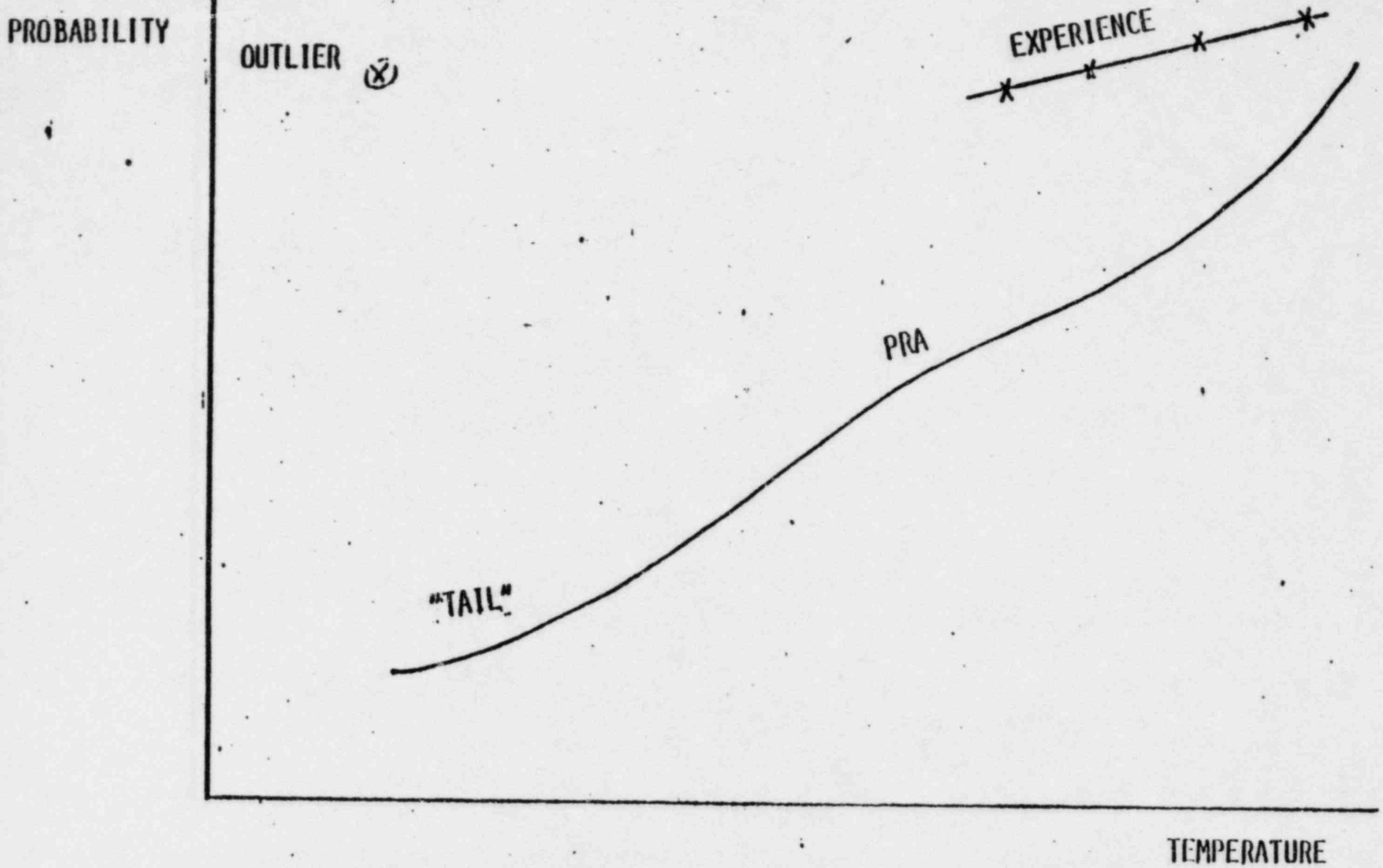
OCTOBER 8, 1982

STEPHEN H. HANAUER

T2

OUTLINE

- o GENERAL APPROACH
- o EVALUATION OF EXPERIENCE
- o SCREENING CRITERION
- o APPLICATION TO PLANTS
- o PROBABILISTIC EVALUATION
- o CONCLUSIONS AND RECOMMENDATIONS


PROBABILITY
OUTLIER
EXPERIENCE
PRA
"TAIL"
TEMPERATURE

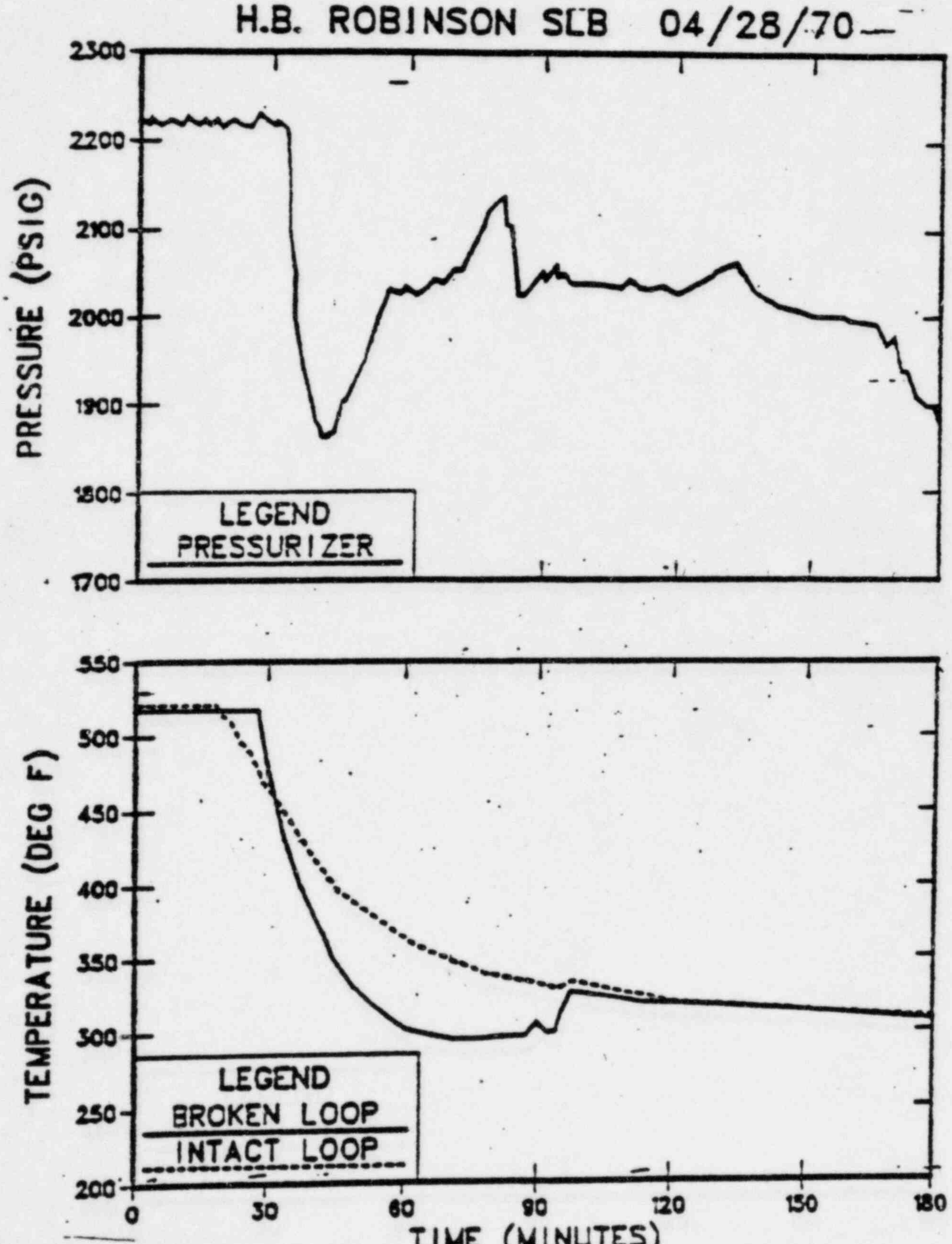

H.B. ROBINSON SLB 04/28/70
PRESSURE (PSIG)
2300
2200
2100
2000
1900
1800
1700
LEGEND
PRESSURIZER
TEMPERATURE (DEG F)
550
500
450
400
350
300
250
200
LEGEND
BROKEN LOOP
INTACT LOOP
0
30
60
90
120
150
180
TIME (MINUTES)

FIGURE 2-1

(4)

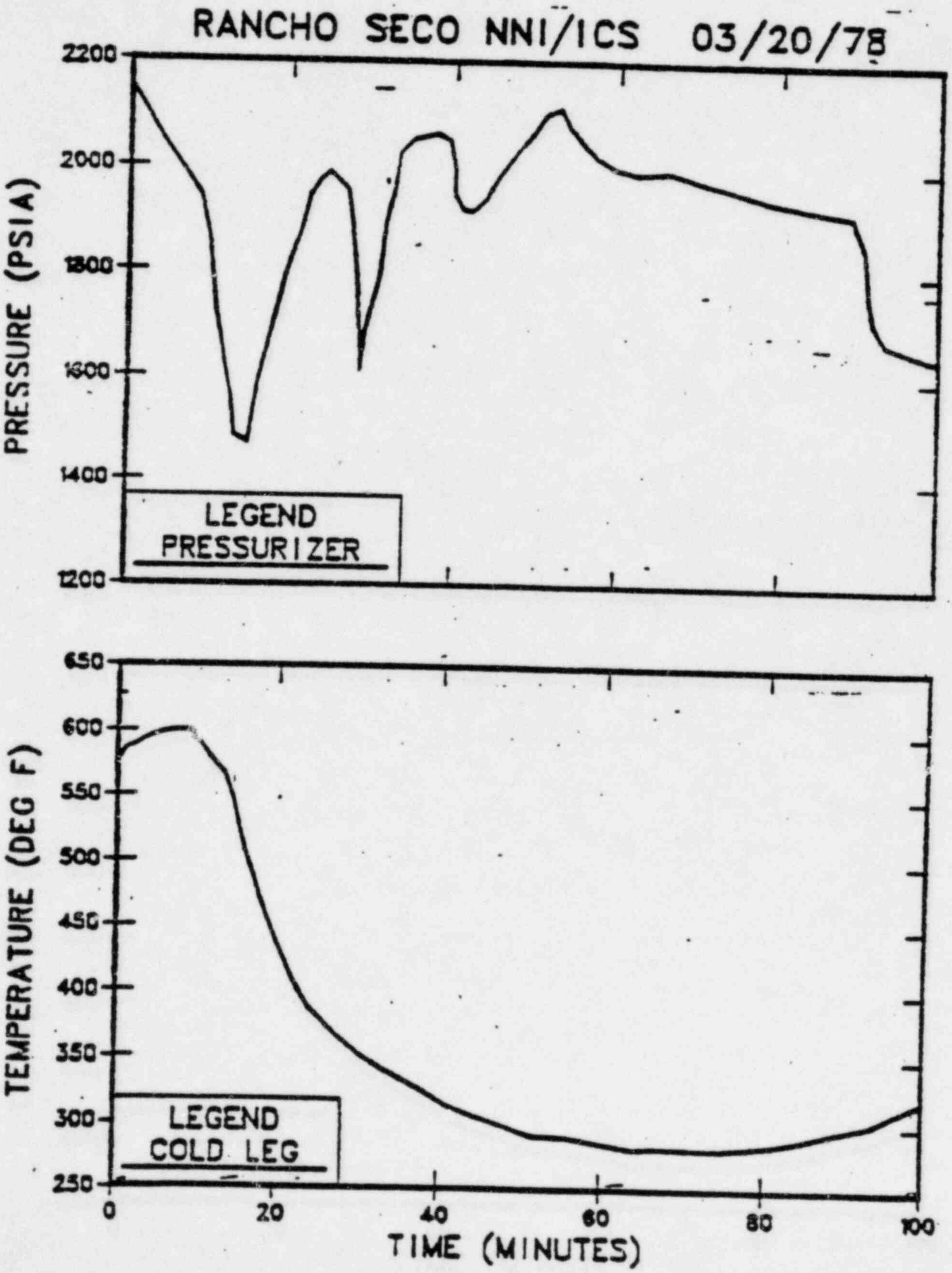

RANCHO SECO NNI/ICS 03/20/78
PRESSURE (PSIA)
2200
2000
1800
1600
1400
1200
LEGEND
PRESSURIZER
TEMPERATURE (DEG F)
650
600
550
500
450
400
350
300
250
LEGEND
COLD LEG
0
20
40
60
80
100
TIME (MINUTES)

FIGURE 2-7

R.E. GINNA SGTR + PORV '01/25/82

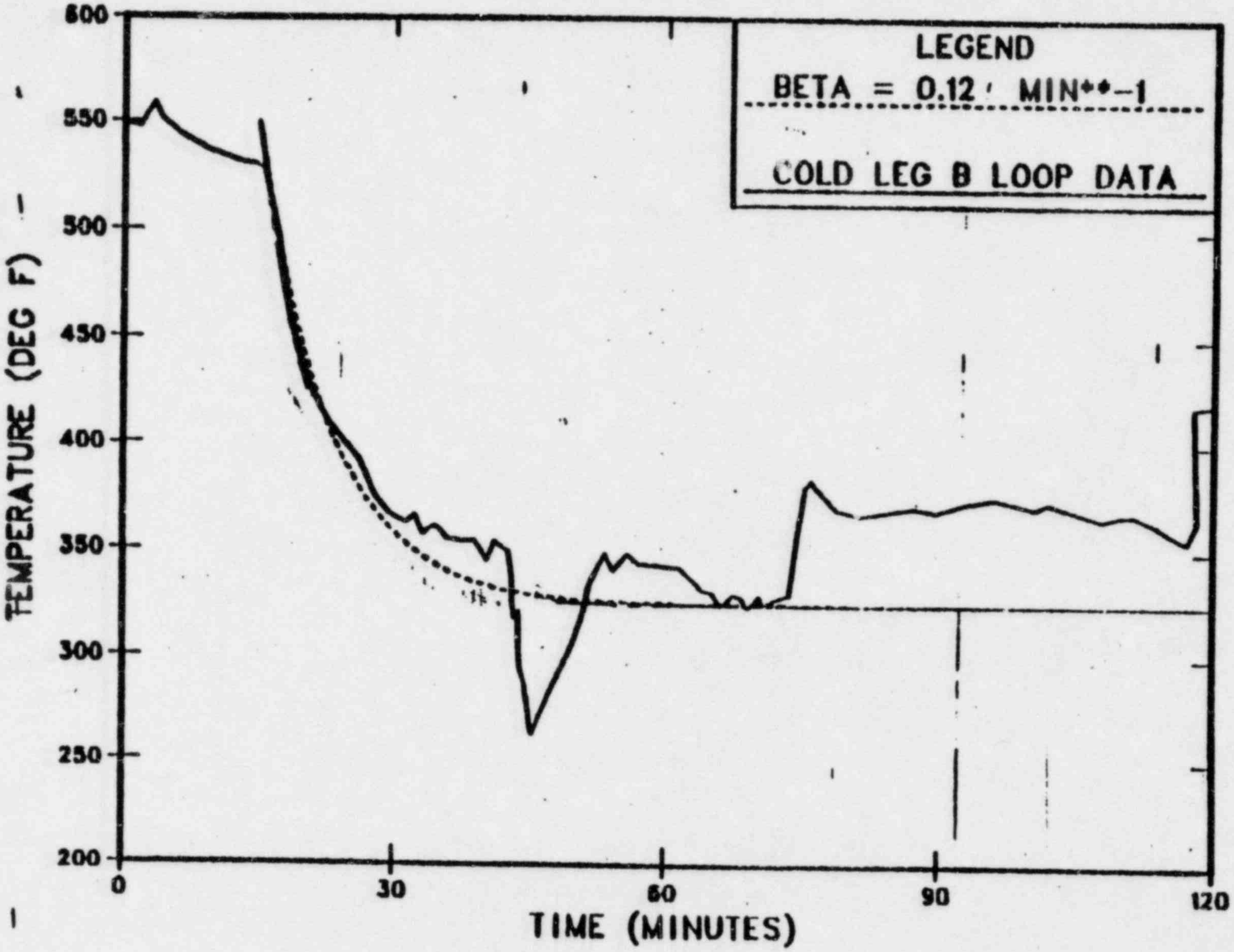
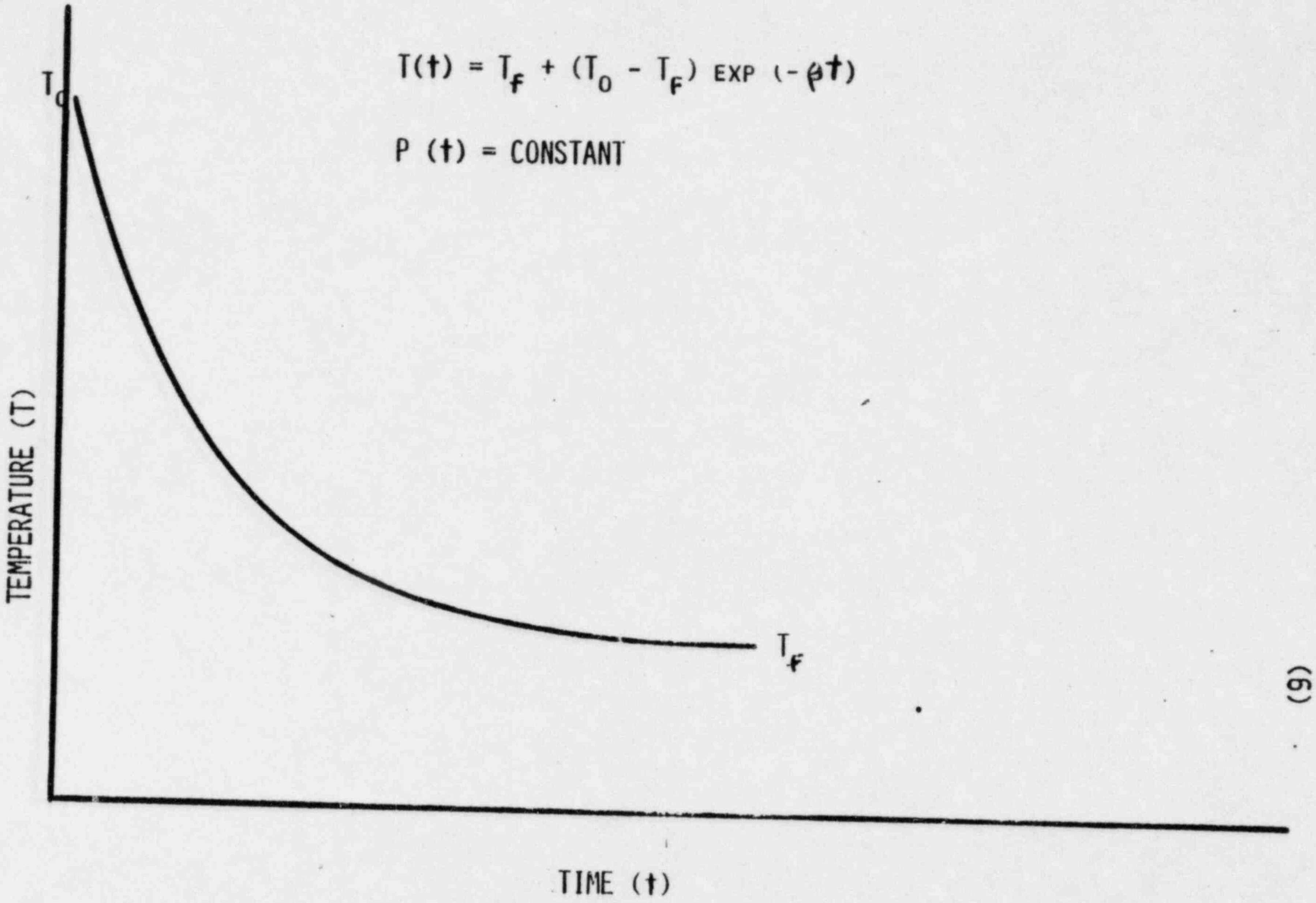
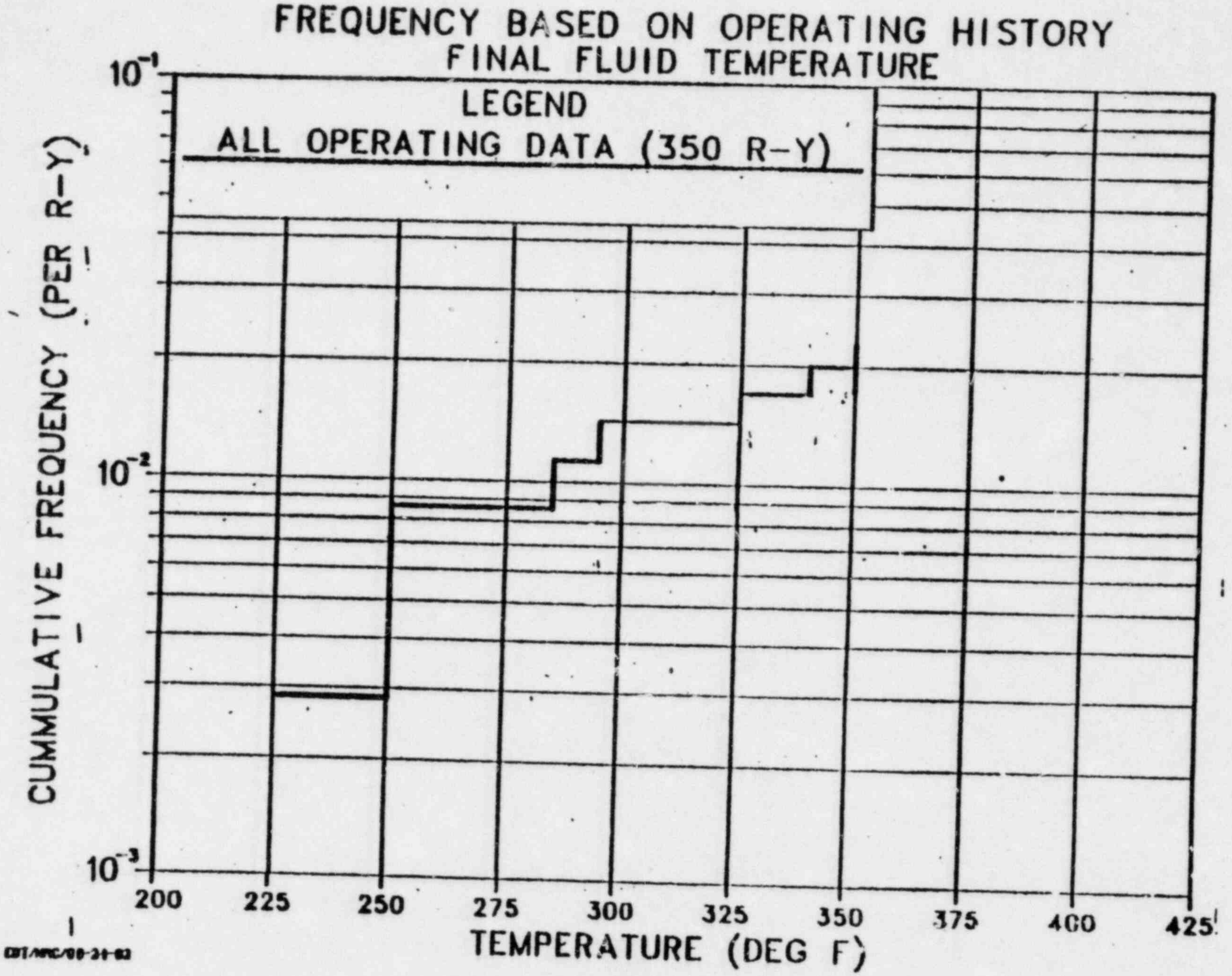


FIGURE 2-12


$T(t) = T_f + (T_0 - T_F)\ \mathrm{EXP}\ (-\beta t)$
$P\ (t) = \mathrm{CONSTANT}$
T_0
T_f
TEMPERATURE (T)
TIME (t)



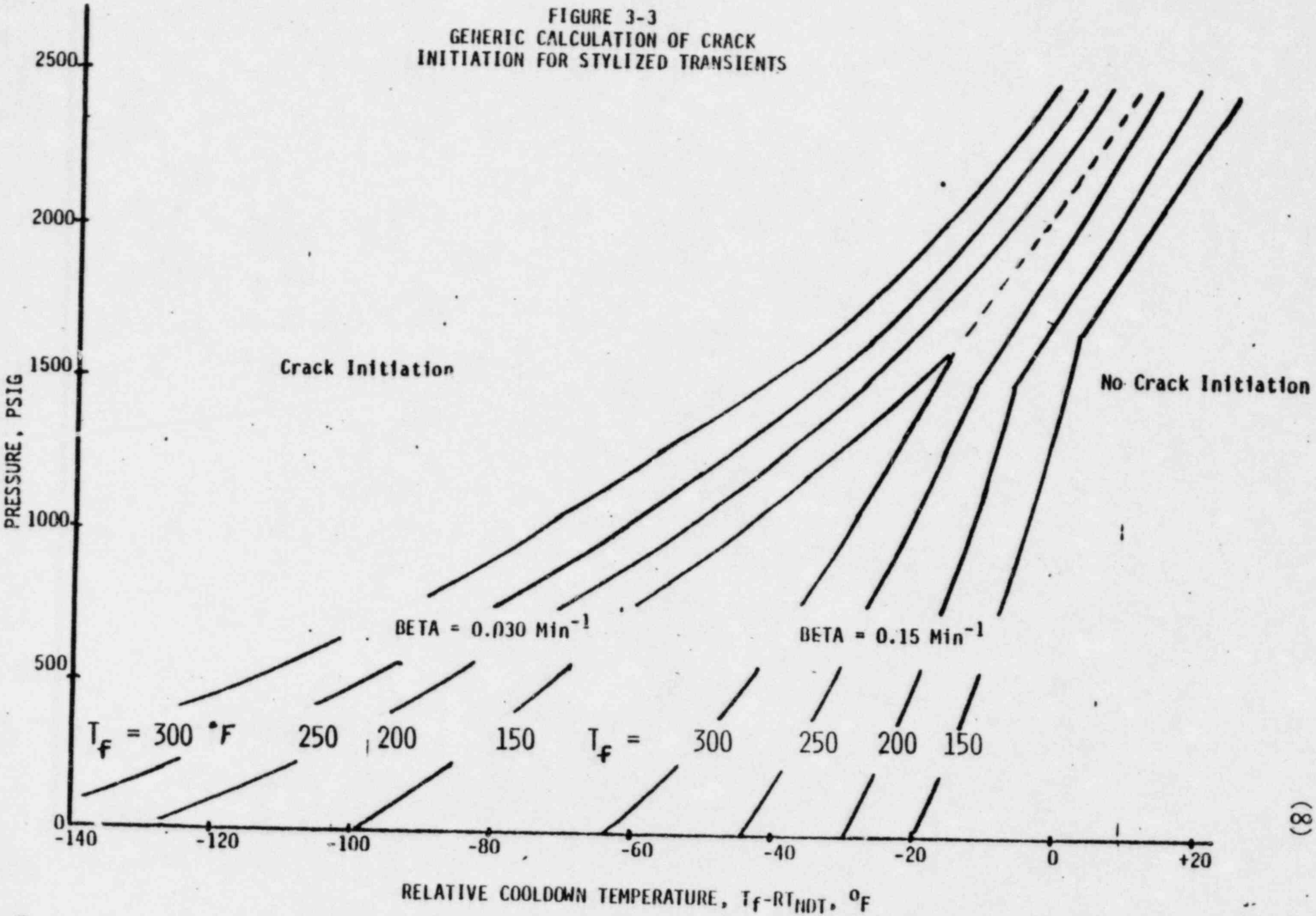
FREQUENCY BASED ON OPERATING HISTORY
FINAL FLUID TEMPERATURE

EBT/NRC/08-24-83

FIGURE 2-14

(7)

FIGURE 3-3
GENERIC CALCULATION OF CRACK
INITIATION FOR STYLIZED TRANSIENTS

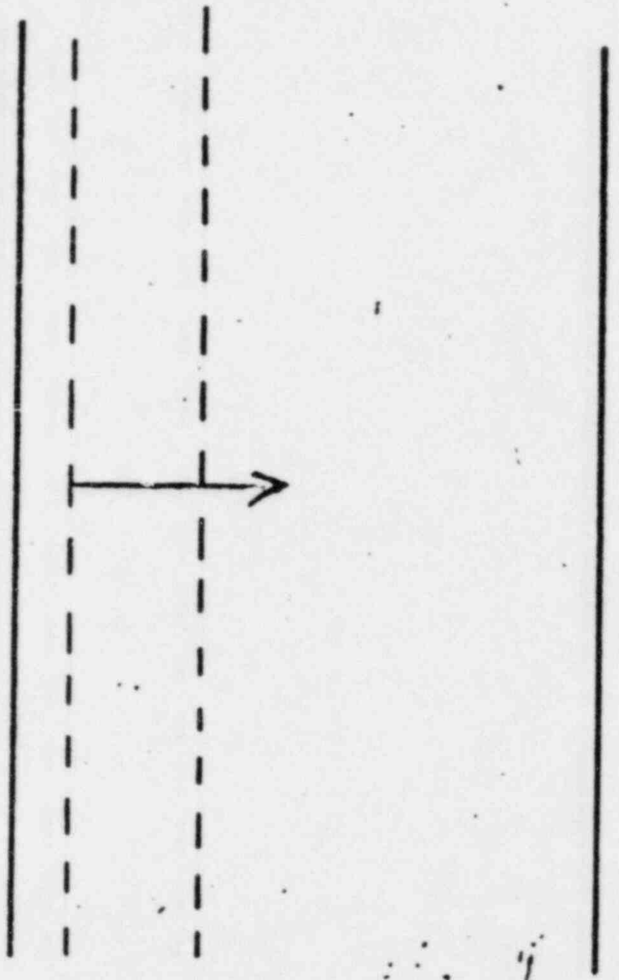


(8)

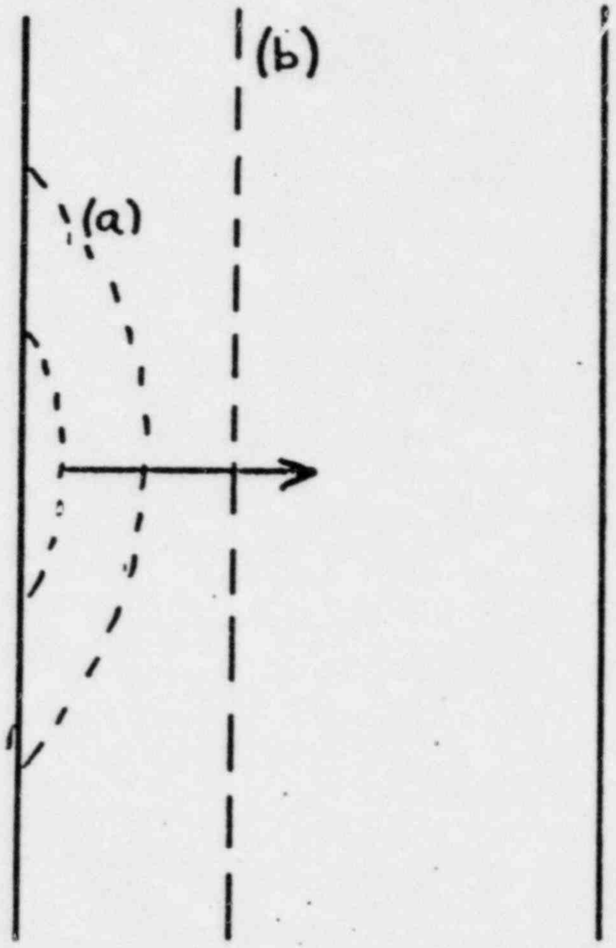
T3

ASSUMPTIONS ON FLAW AND CRACK GEOMETRY

STAFF



WOG



8B

OPERATIONS CONSIDERATIONS

- OPERATOR ACTIONS AFFECT EVENT SEQUENCE
  - INITIATING EVENT
  - TAKE NEEDED ACTION
  - OMIT OR DELAY NEEDED ACTION
  - CREATIVE ACTION TO MITIGATE SEQUENCE
  - BIZARRE ACTION TO AGGRAVATE SEQUENCE
- OPERATORS NEEDS
  - KNOWLEDGE AND UNDERSTANDING OF PLANT
  - PROCEDURES
  - INFORMATION FROM INSTRUMENTS

AUDIT OF PROCEDURES AND TRAINING

- o SYMPTOM ORIENTED PROCEDURES PROGRAM
  - HANDLE CONFLICTING REQUIREMENTS (SUCH AS UNDER vs. OVERCOOLING)
  - RESOLVE BEFORE OPERATOR IS IN MIDST OF COMPLICATED EVENT
  - INTEGRATED TMI - I.C.1 PROGRAM
- o WOG PTS REVIEW OF NEW GUIDELINES
  11 PROPOSED MODIFICATIONS
- o AUDIT AT 7 PLANTS
  REVIEW CRITERIA
  (1) DO NOT VIOLATE NDT LIMITS
  (2) DO NOT VIOLATE SATURATION LIMITS
  (3) PROVIDE GUIDANCE TO RECOVER FROM PTS CONDITIONS
  (4) PROVIDE SUPPORTING TECHNICAL BASES
  (5) CONSIDER PTS IN OPERATION OF HPI AND CHARGING SYSTEMS
  (6) CONSIDER PTS IN FEEDWATER AND AFW OPERATIONS
  (7) INCLUDE INSTRUCTION ON NTD VESSEL LIMITS
  (8) EMPHASIZE TRAINING ON TRANSIENTS AND ACCIDENTS REQUIRING OPERATOR ACTIONS TO MITIGATE PTS
  (9) INCLUDE SIMULATOR TRAINING FOR PTS

FREQUENCY BASED ON OPERATING HISTORY

CRITICAL RTNDT VALUE (OCA RESULTS) AND FINAL FLUID TEMPERATURE

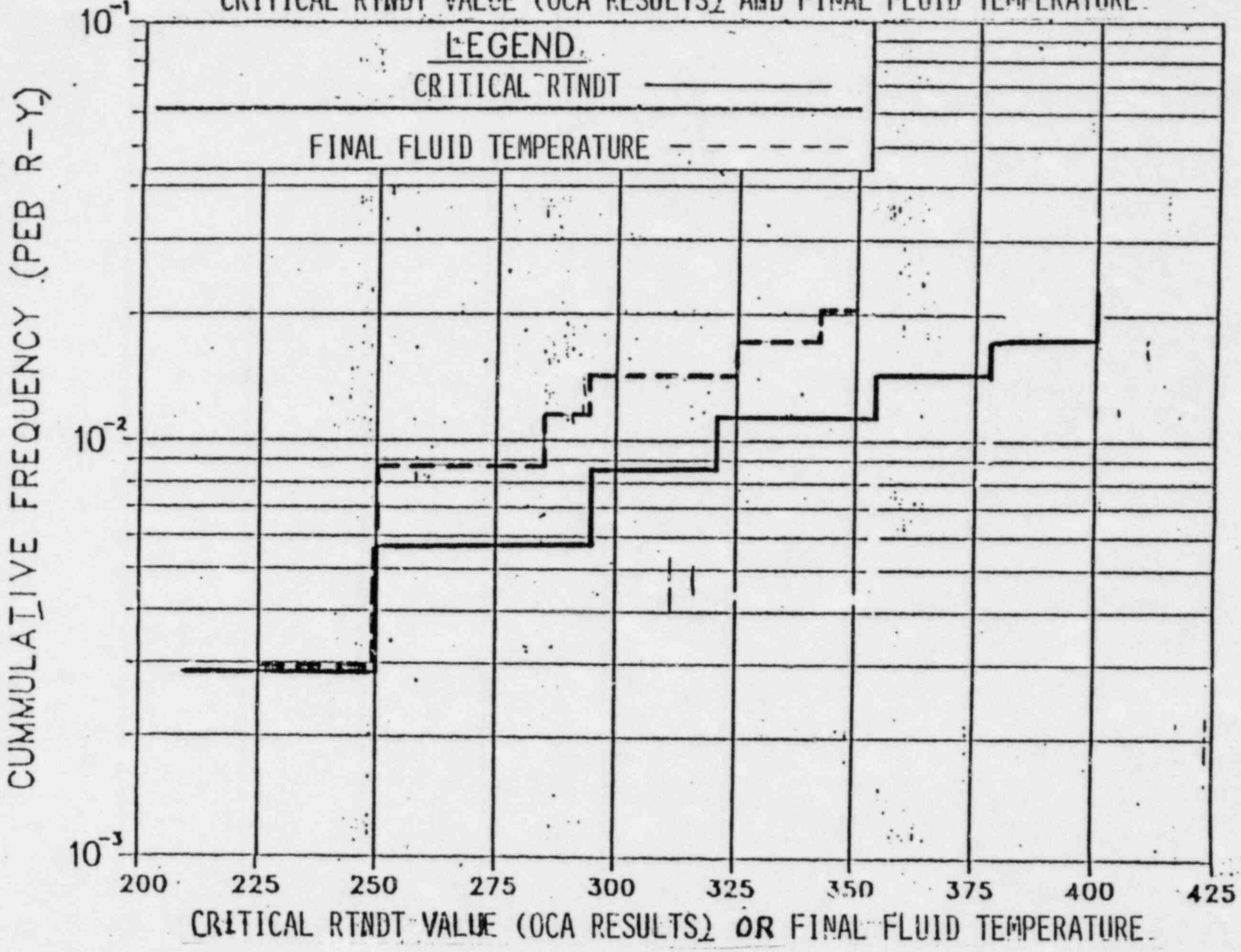


FIGURE 4-1

SCREENING CRITERION

o LONGITUDINAL CRACK	270°F
o CIRCUMFERENTIAL CRACK	300°F

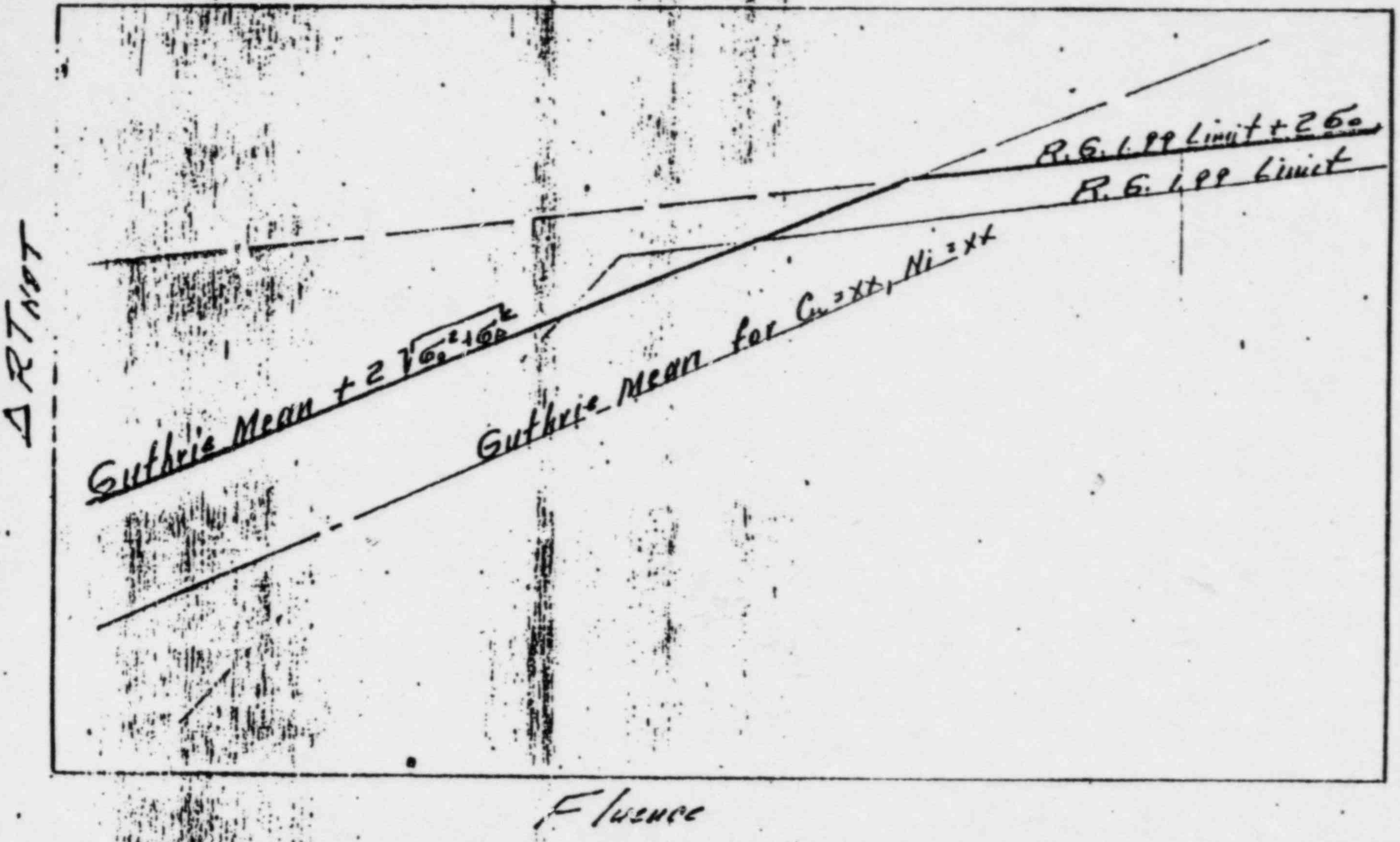
EVALUATING A SPECIFIC VESSELL

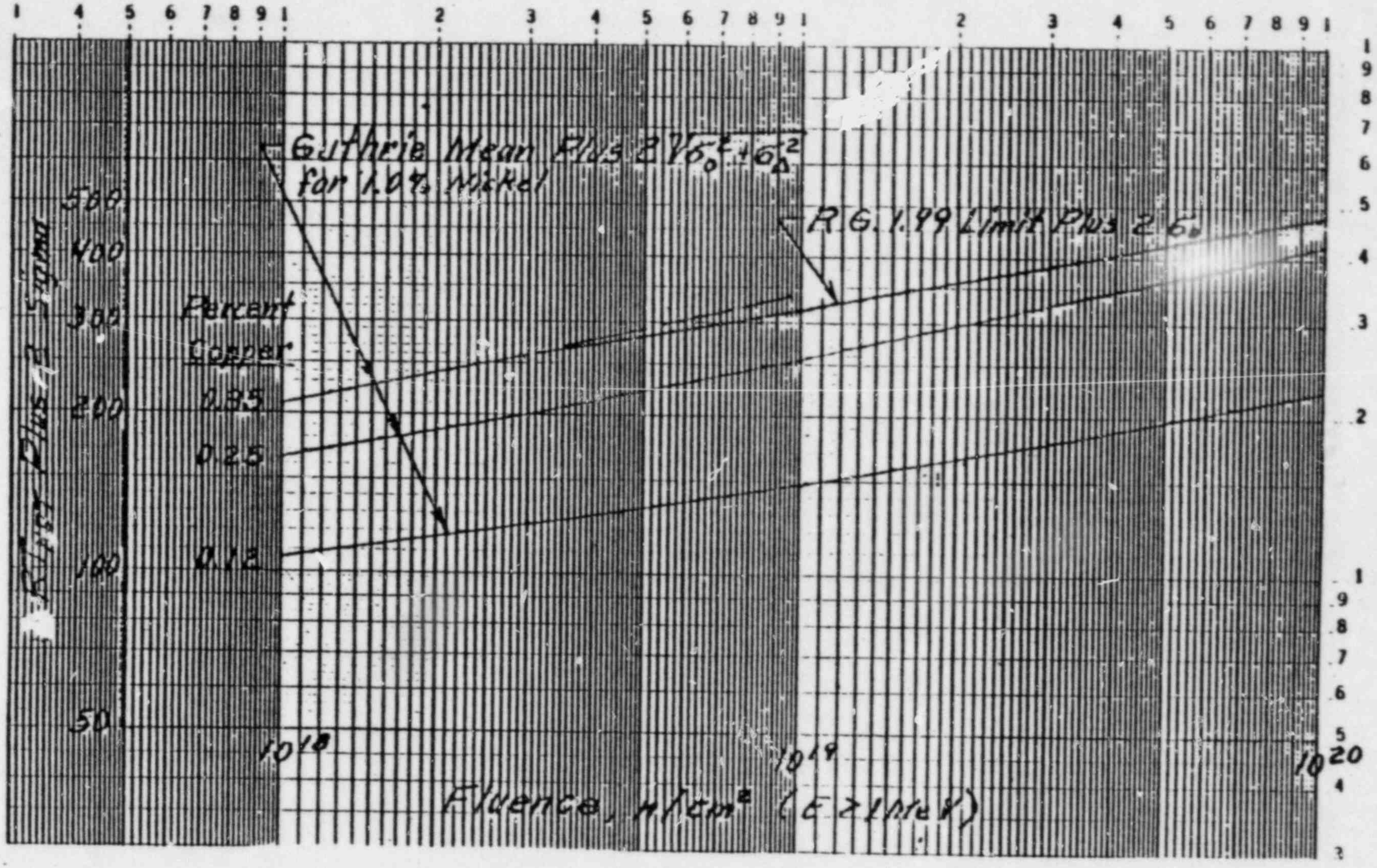
$RT_{NDT} = RT_0$ (BEST ESTIMATE)

$+ \Delta RT$ (BEST ESTIMATE - GUTHRIE)

$+ 2\sqrt{\sigma_0^2 + \sigma_\Delta^2}$

LIMITED BY RG 1.99 + $2\sigma_0$


R.G. 1.99 Limit + 2σ_o
R.G. 1.99 Limit
Guthrie Mean + 2 √(σ_o² + σ_D²)
Guthrie Mean for Cu = X%, Ni = X%
ΔRT_NDT
Fluence



EXAMPLE OF NRC PRESCRIPTION FOR RT_{NDT} (FOR ASSUMED RT_{NDT} (o) OF 0^oF)

PLANT STATUS AS OF DECEMBER 31, 1981

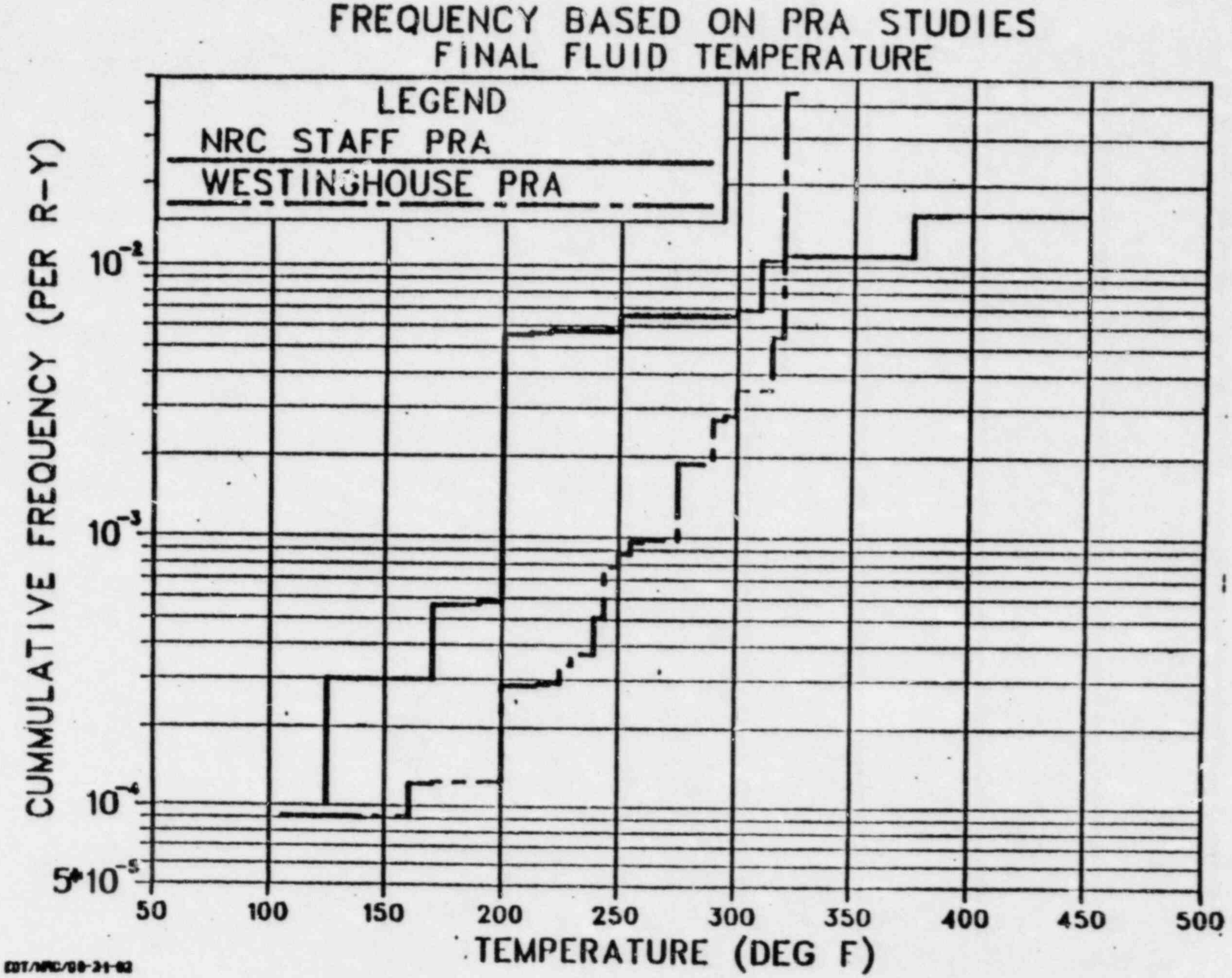
PLANT		RT_0, °F	ΔRT, °F	$2\sqrt{\sigma_0^2+\sigma_\Delta^2}$	RT_{NDT}	DATE SCREENING CRITERION EXCEEDED
ROBINSON 2	CIRCUM	-56	295	34	281	~~February, 1987~~ Sept., 1988
W/CE	AXIAL	-56	151	59	154	
FORT CALHOUN	CIRCUM.	-56	264	34	242	April, 1990
CE/CE	AXIAL	-56	248	34	226	
TURKEY POINT 4	CIRCUM.	0	200	59	259	July, 1988
W/B&W	NO AXIAL					
TURKEY POINT 3	CIRCUM.	0	200	57	257	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, 198[illegible]
	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	

PLANT	DATE WHEN EXCEED RT_{NDT} CRITERION
ROBINSON 1	1988
TURKEY POINT 3	1989
TURKEY POINT 4	1989
CALVERT CLIFFS 1	1989
FORT CALHOUN	1990
RANCHO SECO	1993
MAINE YANKEE	1995
THREE MILE ISLAND 1	1995
OCONEE 2	1996
ZION 1	2000

ALL OTHER PLANTS ARE LATER THAN 2000

SIGNIFICANT PTS EVENT SEQUENCES

- o SECONDARY (STEAM SIDE) DEPRESSURIZATION
- o MAIN STEAM LINE BREAK
- o SMALL STEAM LINE BREAK (OR STUCK OPEN STEAM GENERATOR SAFETY/RELIEF VALVE)
- o SMALL BREAK LOSS-OF-COOLANT ACCIDENT
- o STEAM GENERATOR TUBE RUPTURE


FREQUENCY BASED ON PRA STUDIES
FINAL FLUID TEMPERATURE

EDT/NRC/08-21-82

FIGURE 8-1

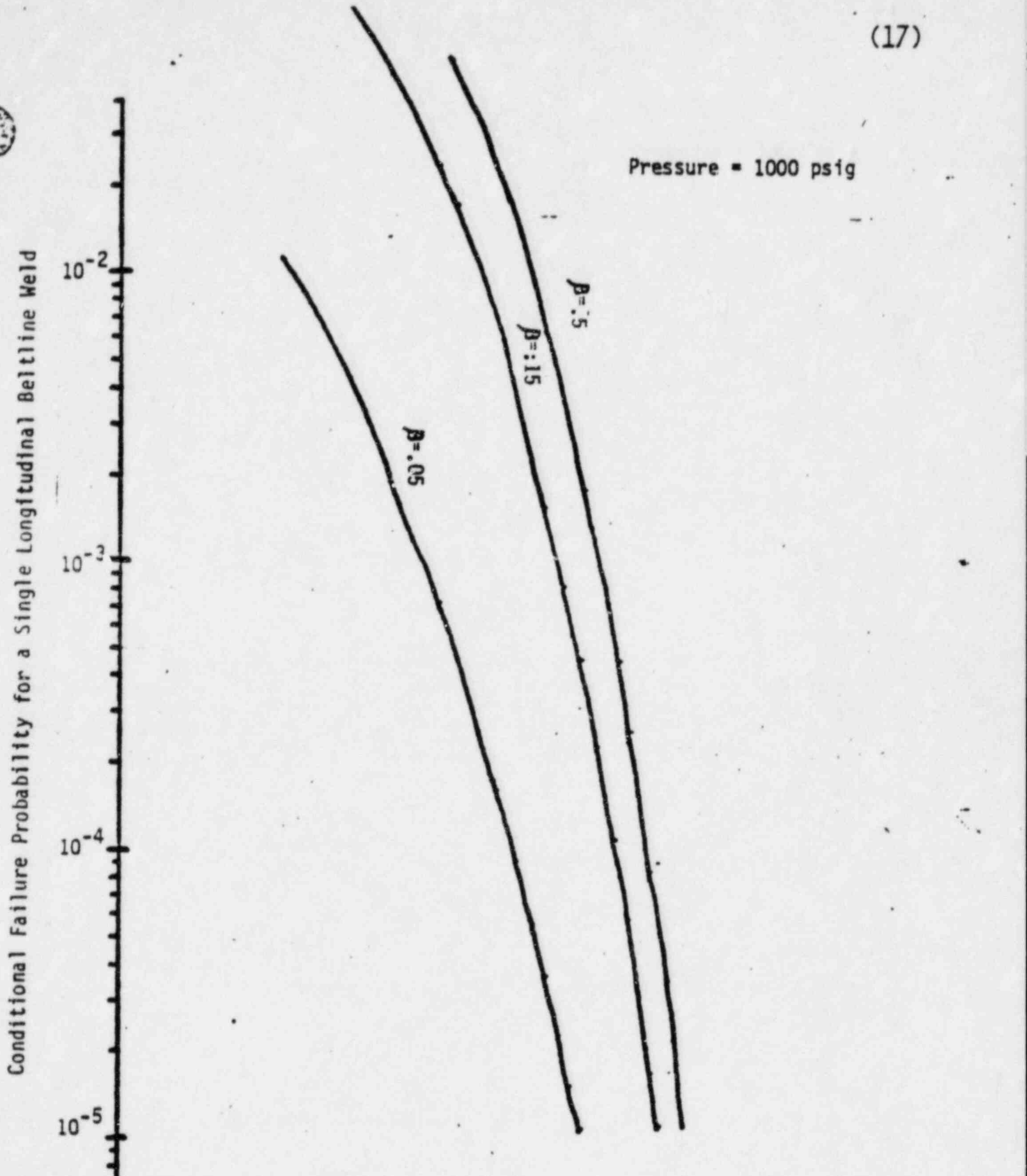


FIGURE 7-1

CFP AS FUNCTION OF T_F-MEAN VALUE OF RT_{NDT}

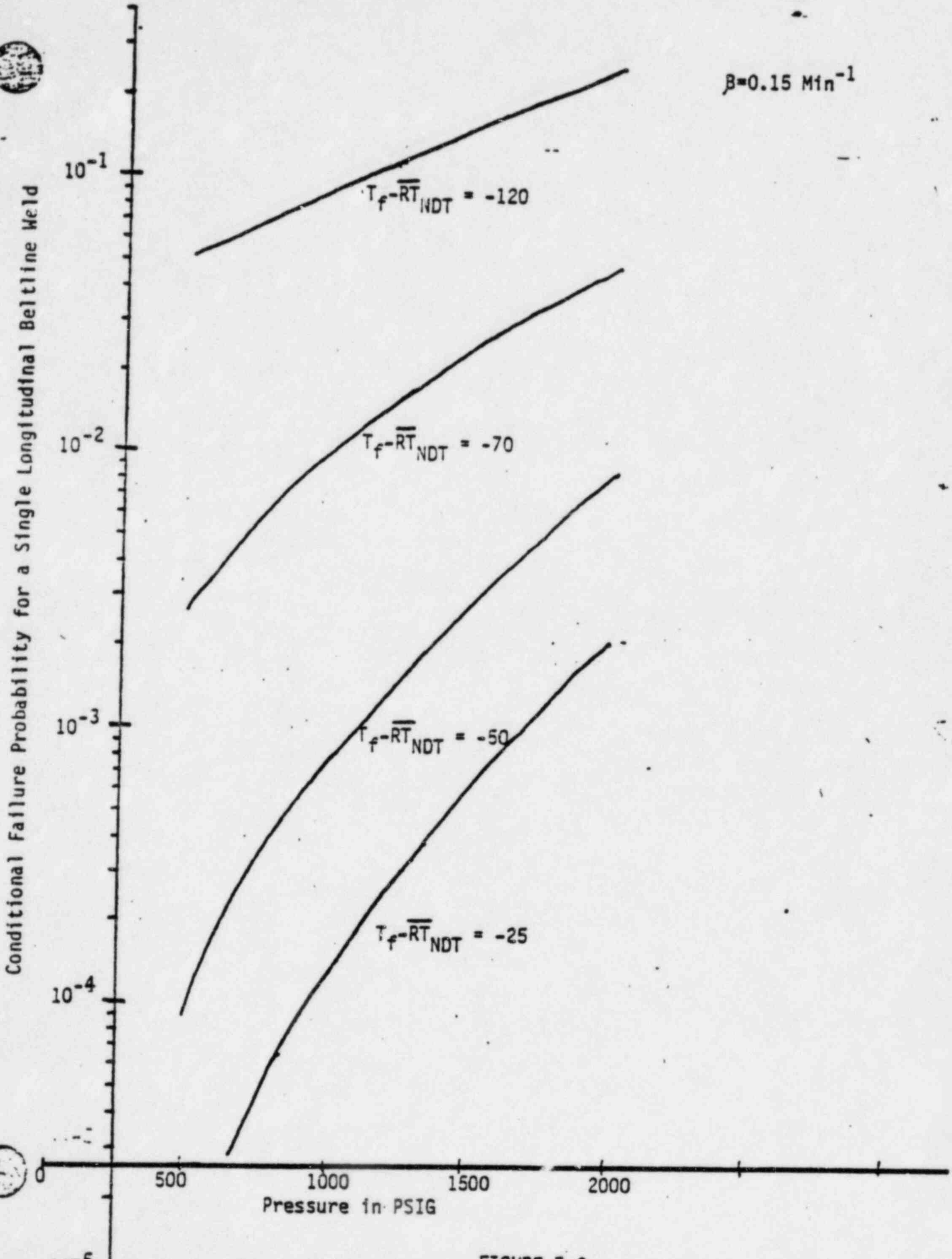


FIGURE 7-2
CFP AS FUNCTION OF PRESSURE

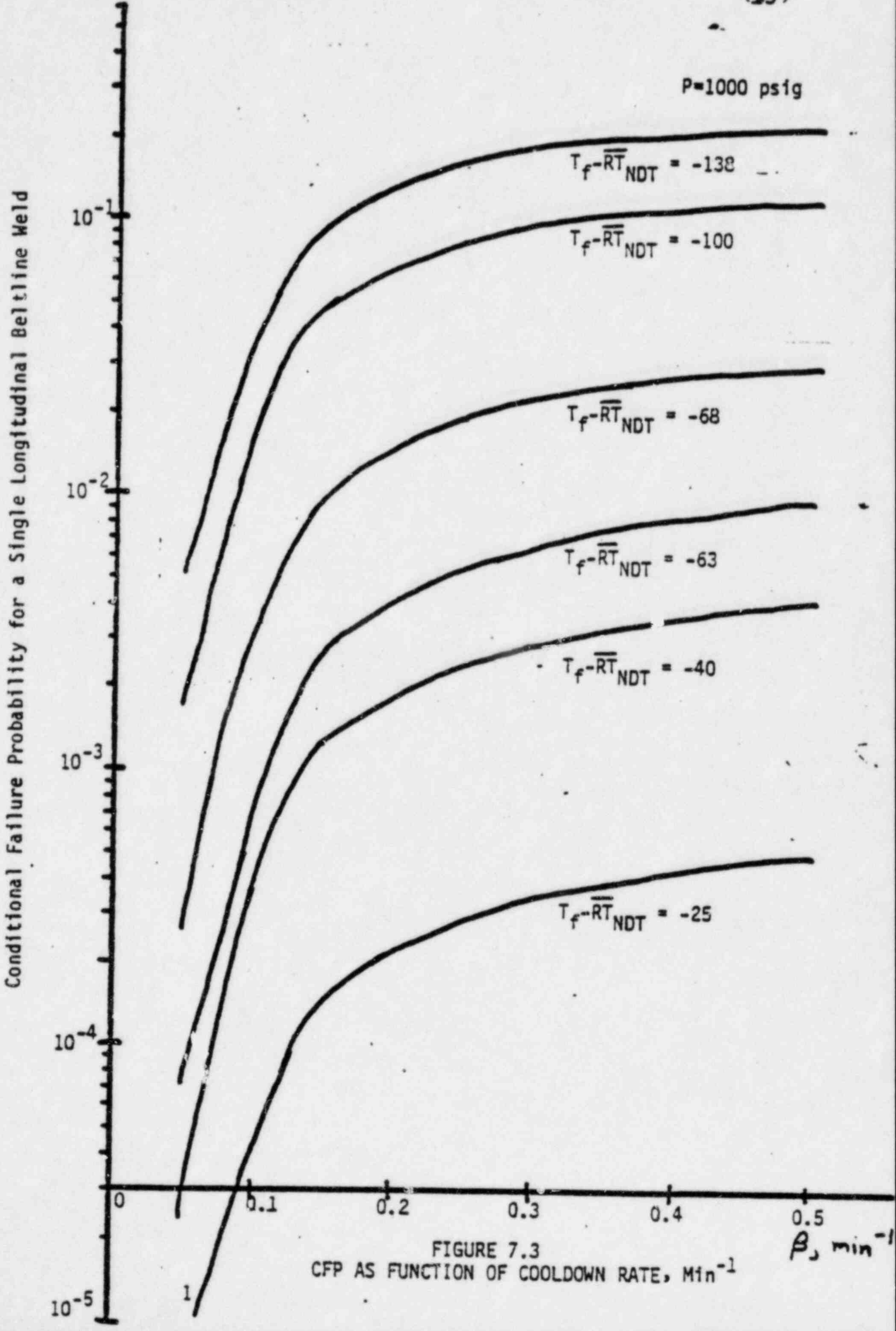


FIGURE 7.3
CFP AS FUNCTION OF COOLDOWN RATE, Min^{-1}

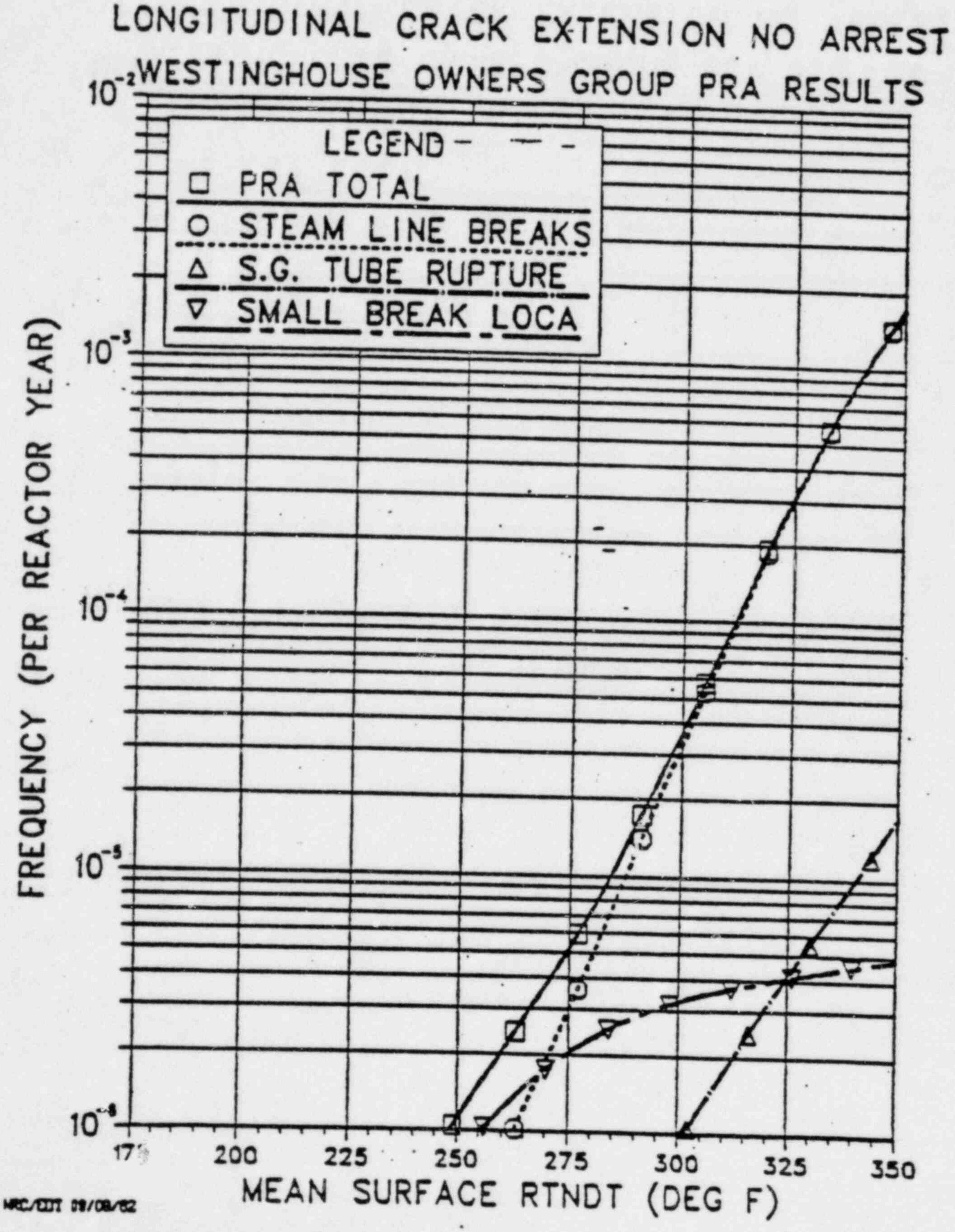

LONGITUDINAL CRACK EXTENSION NO ARREST
WESTINGHOUSE OWNERS GROUP PRA RESULTS
LEGEND
□ PRA TOTAL
○ STEAM LINE BREAKS
△ S.G. TUBE RUPTURE
▽ SMALL BREAK LOCA
FREQUENCY (PER REACTOR YEAR)
10⁻²
10⁻³
10⁻⁴
10⁻⁵
10⁻⁶
17
200
225
250
275
300
325
350
MEAN SURFACE RTNDT (DEG F)
NRC/EDT 09/08/82

FIGURE 8-2

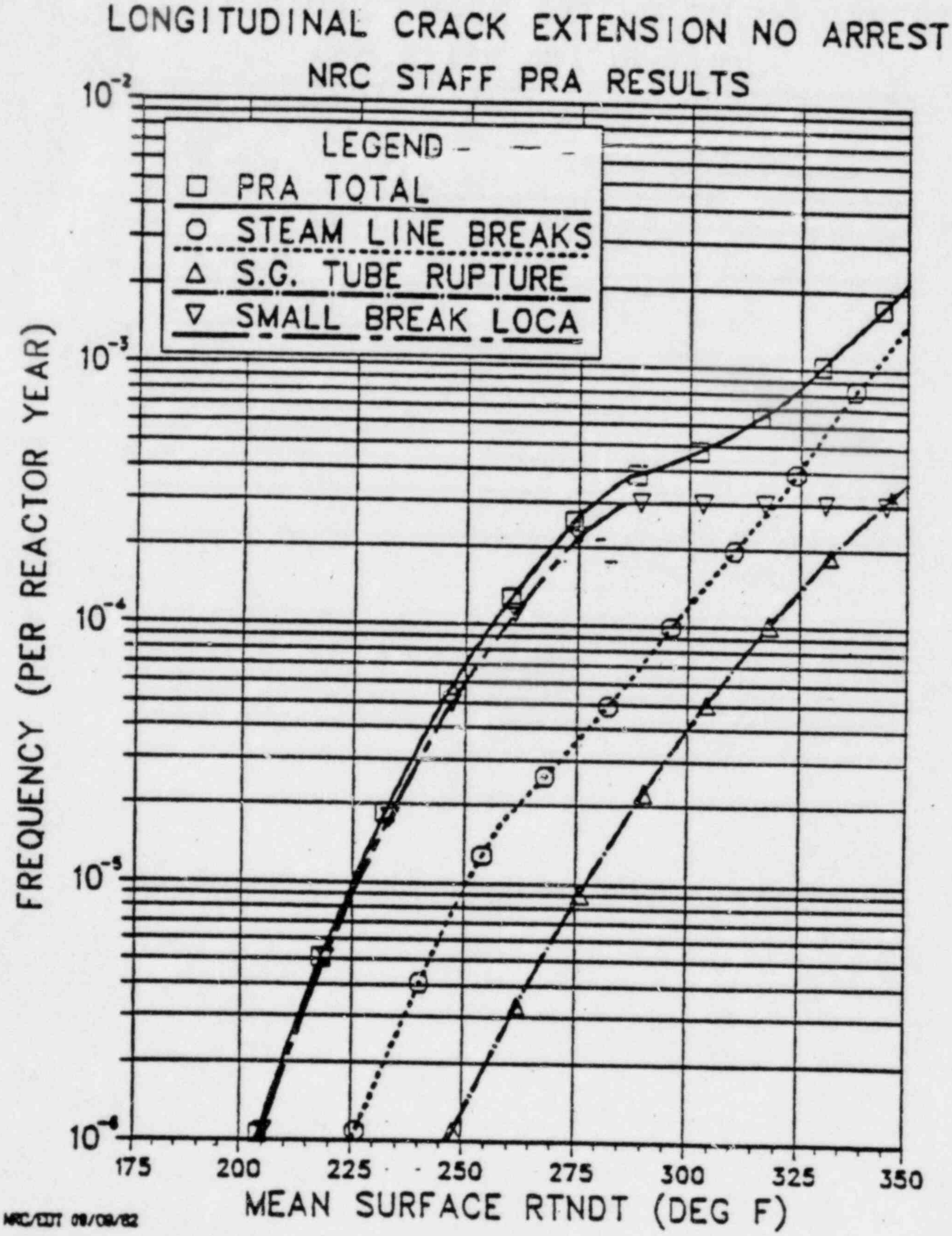
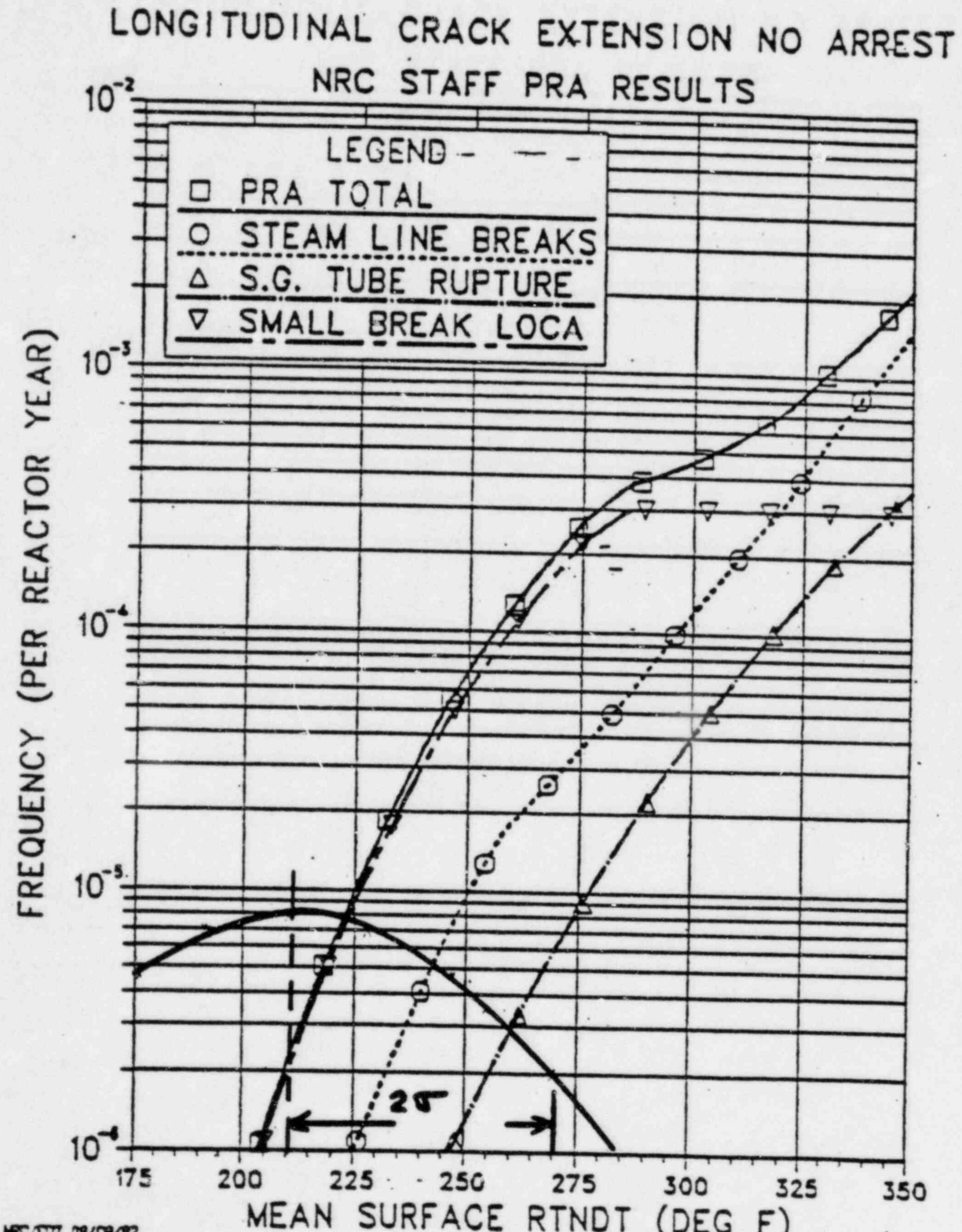

LONGITUDINAL CRACK EXTENSION NO ARREST
NRC STAFF PRA RESULTS
LEGEND
□ PRA TOTAL
○ STEAM LINE BREAKS
△ S.G. TUBE RUPTURE
▽ SMALL BREAK LOCA
FREQUENCY (PER REACTOR YEAR)
10^{-2}
10^{-3}
10^{-4}
10^{-5}
10^{-6}
175
200
225
250
275
300
325
350
MEAN SURFACE RTNDT (DEG F)
NRC/EDT 08/09/82

FIGURE 8-3



NRC/EDT 08/08/82

FIGURE 8-3

SAFETY GOAL

F	VESSEL CRACK
X	________CORE MELT IF VESSEL CRACKS
Y	SIGNIFICANT RELEASE IF CORE MELTS

CORE MELT $XF \leq 10^{-5}$

RISK $\frac{XFY}{16} \leq 5 \times 10^{-8}$

UNCERTAINTIES

- OPERATING EXPERIENCE
- OPERATION ACTIONS
- FLAWS AND CRACKS
- STRESSES
- MATERIAL PROPERTIES
- FRACTURE MECHANICS
- PROBABILISTIC CALCULATIONS

SHORT TERM

1. NO NEED FOR IMMEDIATE MODIFICATIONS

2. NEED PLANT-SPECIFIC ANALYSIS OF SELECTED PLANTS

3. SCREENING CRITERION

4. PLANT-SPECIFIC ANALYSES

   - WHO
   - WHEN
   - SCOPE
   - ACCEPTANCE CRITERIA

5. REGULATION CHANGES MAY BE NEEDED

6. FLUX REDUCTION PROGRAMS CONSIDERED

FLUX REDUCTION PROGRAMS

	FLUX REDUCTION FACTOR	DERATE IF REQUIRED	RELIEF FROM DERATING
1	1		
2	~ 0.5		
3	~ 0.1	X	
4	~ 0.03	X	

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 FOR ALL EVENTS INCLUDING PTS (SUMPTOM ORIENTED PROCEDURES)

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

5. RESEARCH PROGRAM
   - MATERIAL PROPERTIES AND FRACTURE MECHANICS
   - ANNEALING
   - SYSTEMS

1. CONDITION OF CLADDING SURFACE AND EFFECT ON EXAMINATION

2. CAPABILITY OF ULTRASONIC INSPECTION PROCEDURE TO DETECT UNDER CLAD CRACKS.

TEST BLOCKS FOR BACKGROUND NOISE

"AS WELDED" CONDITION

HAND GROUND CONDITION

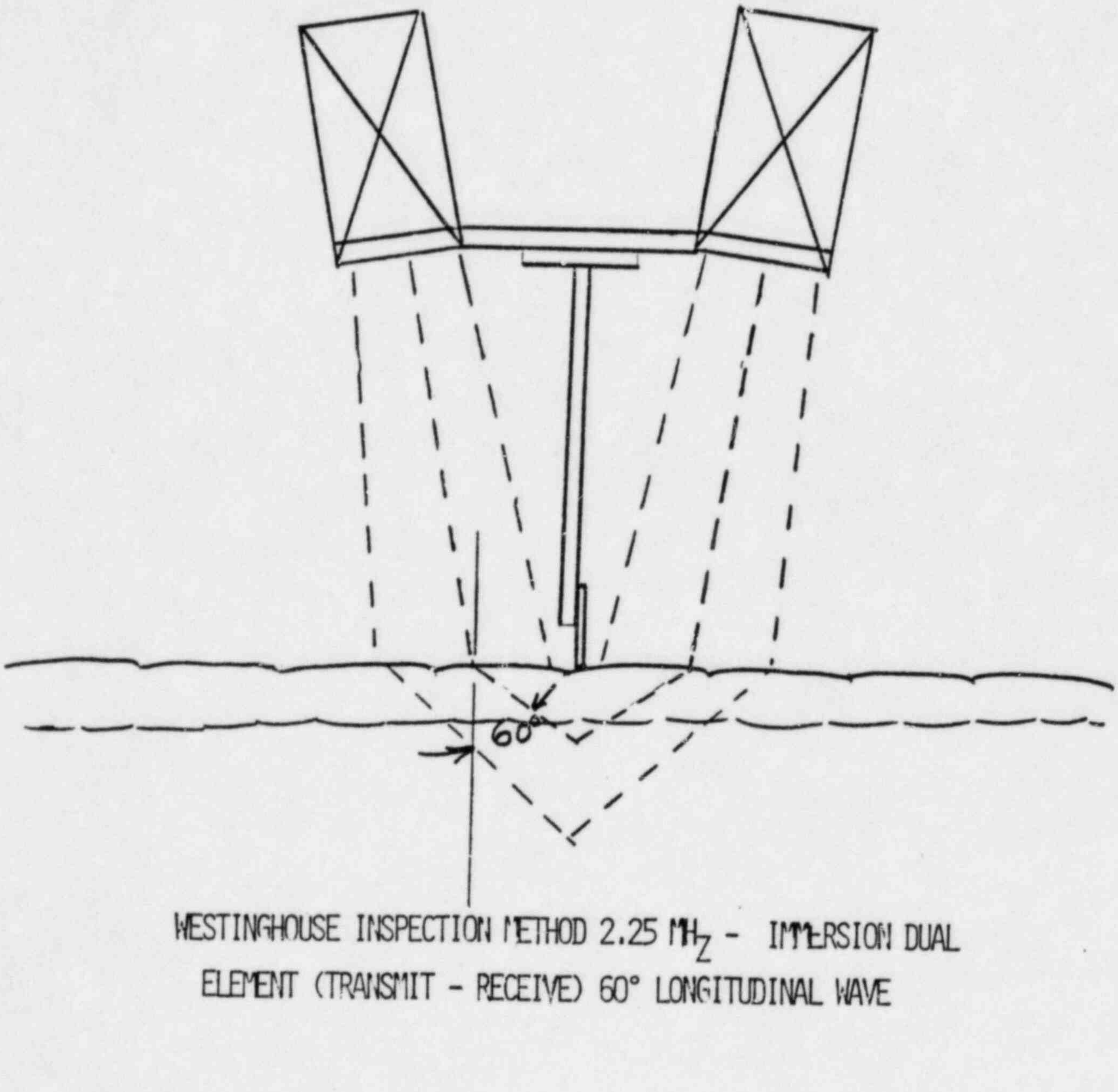
82GC68-2.

H. B. ROBINSON - INSPECTION BY WESTINGHOUSE - MARCH-APRIL 1982
- 2.25 MH$_Z$ - IMMERSION
- 60° REFRACTED LONGITUDINAL WAVE
- DUAL ELEMENT (TRANSMIT - RECEIVE)
- FOCUSED 0.75 INCHES DEEP FROM CLAD SURFACE OF CALIBRATION BLOCK

TURKEY POINT 3 - INSPECTION BY SOUTHWEST RESEARCH - JUNE 1981
- 2.25 MH$_Z$ - CONTACT
- 45° SHEAR WAVE
- SINGLE ELEMENT
- FULL V-PATH (FIRST 3/4 OF V-PATH GATED OUT - INSPECTED TO CLADDING SIGNAL)

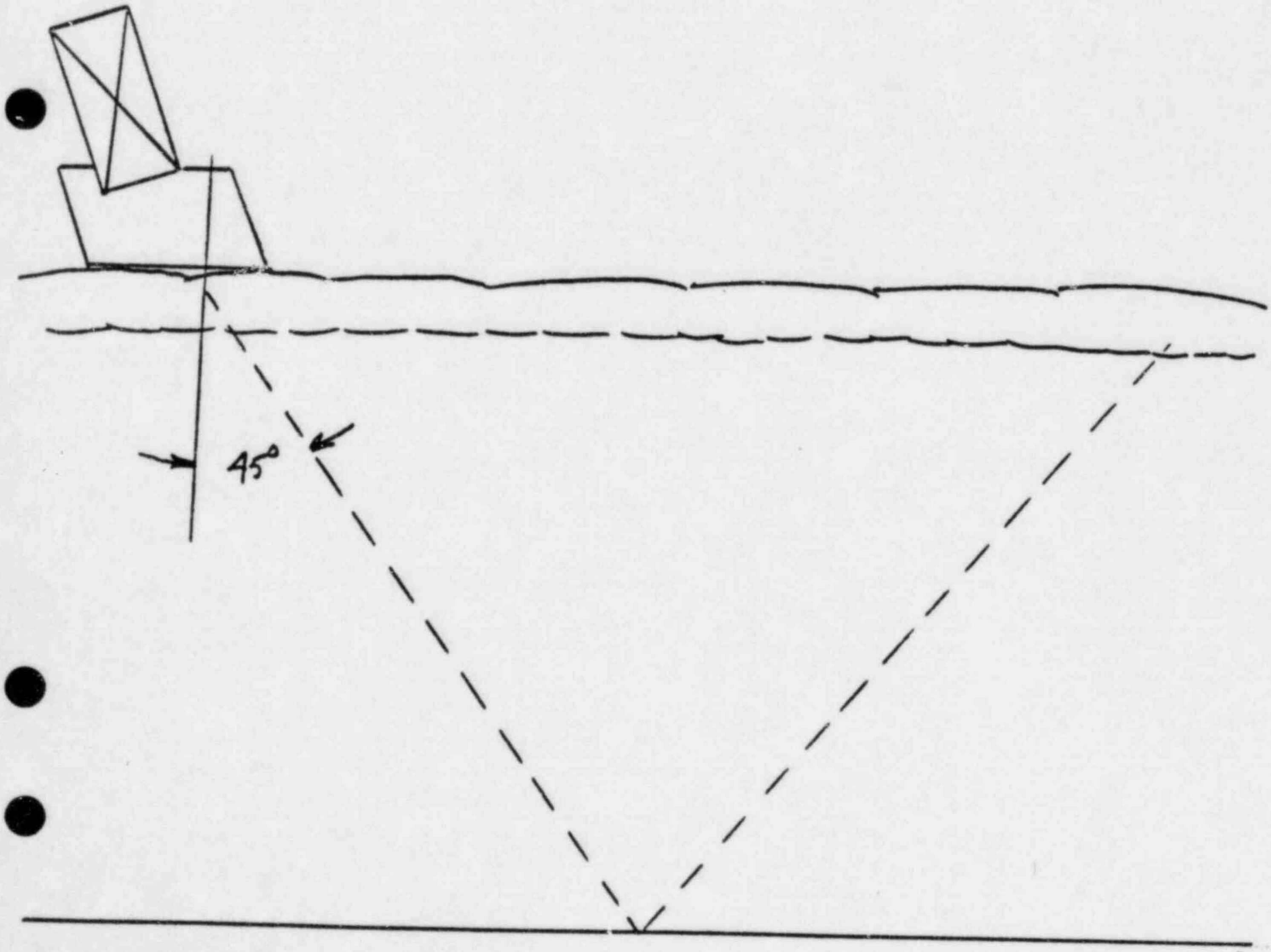
TURKEY POINT 4 - INSPECTION BY SOUTHWEST RESEARCH SCHEDULED FOR NOV. 3, 1982

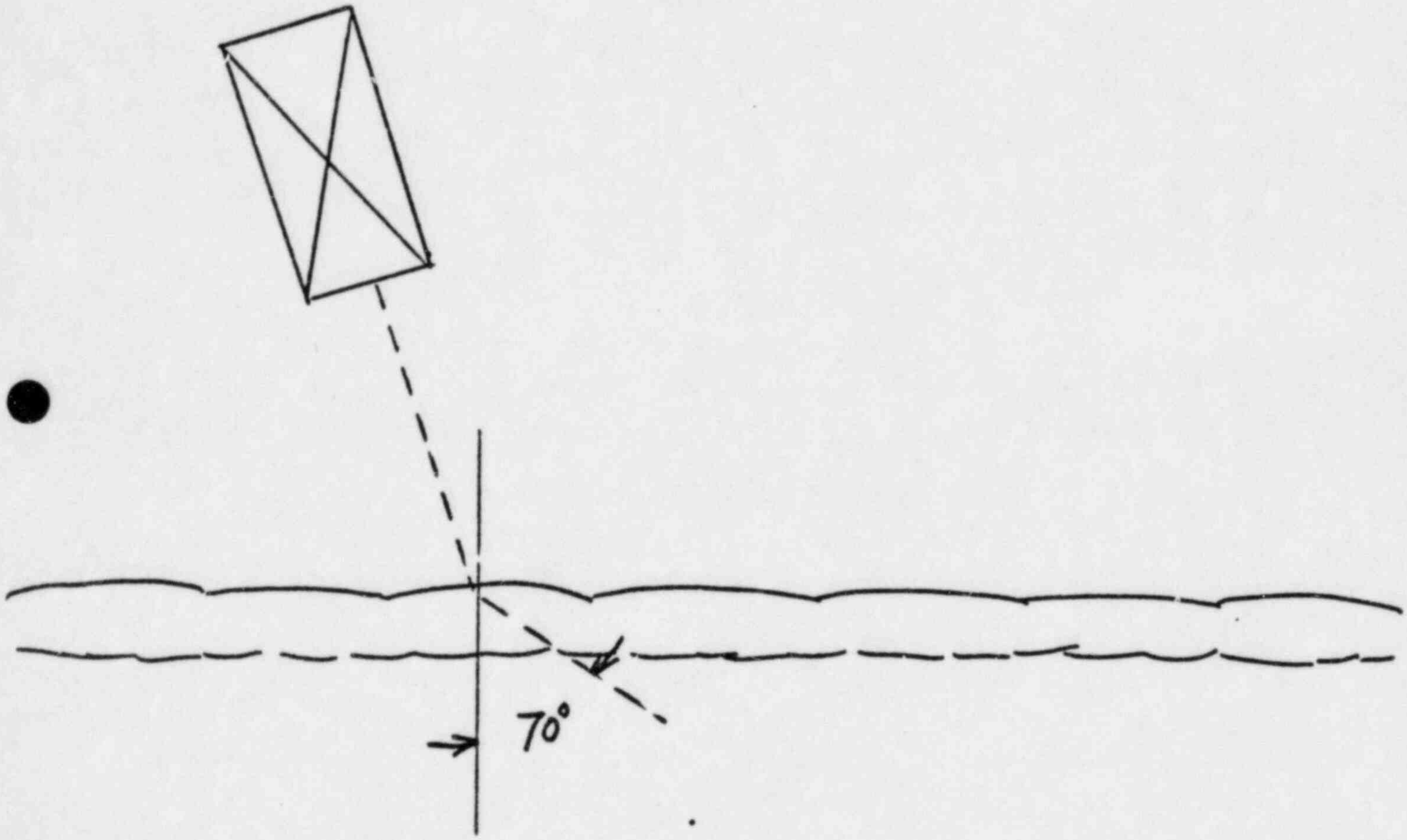
OCONEE 1 - INSPECTION BY BABCOCK AND WILCOX - JULY-AUGUST 1981
- 2.25 MH$_Z$ - IMMERSION
- 70° SHEAR WAVE
- SINGLE ELEMENT
- SCAN TO A DEPTH OF APPROXIMATELY 2-INCHES



WESTINGHOUSE INSPECTION METHOD 2.25 MHz - IMMERSION DUAL ELEMENT (TRANSMIT - RECEIVE) 60° LONGITUDINAL WAVE

SOUTHWEST RESEARCH NEAR SURFACE INSPECTION METHOD
2.25 MHz - CONTACT SINGLE ELEMENT (PULSE-ECHO)
70° SHEAR WAVE





BABCOCK AND WILCOX NEAR SURFACE INSPECTION METHOD
2.25 MH_Z - IMMERSION SINGLE ELEMENT (PULSE ECHO)
70° SHEAR WAVE

RECOMMENDATIONS

- IMPLEMENT REG. GUIDE 1.150
- REQUIRE INSPECTION AGENCIES TO DEMONSTRATE CAPABILITY OF DETECTION OF UNDERCLAD CRACKING
- INSTITUTE RESEARCH PROJECT TO DETERMINE CONFIDENCE LEVELS OF PROBABILITY OF DETECTION OF UNDERCLAD CRACKING

SIZING OF NEAR-SURFACE FATIGUE CRACKS IN CLADDED PRESSURE VESSELS BY THE MULTIPLE BEAM-SATELLITE PULSE TECHNIQUE*

George J. Gruber
Southwest Research Institute
San Antonio, Texas 78284

ABSTRACT

The stainless steel cladding of the inside surface of a reactor pressure vessel makes ultrasonic inspection for detection and sizing of cracks immediately under the cladding significantly harder. One solution to the inspection difficulty has been found in the multiple beam-satellite pulse technique. (While this technique both detects and sizes, only sizing is addressed in this paper.) The technique employs a multiple-beam transducer, which produces both longitudinal and shear waves. Novel waveform-processing and pattern-recognition methods are used in conjunction with this transducer design. The longitudinal-wave component is diffracted mainly by the upper extremity of the crack at or near the clad-base material interface, and its shear-wave components are diffracted mainly by the lower extremity of the crack in the base material. Proof-of-principle sizing results, based on the observance of a pair of satellite pulses from the diffracted beams, were obtained for three sets of planar flaws. They were (1) six side-milled underclad notches ranging in throughwall dimension from 3.1 to 12.9 mm, (2) fatigue cracks implanted in three cladded pressure vessel blocks and ranging in depth from 3.7 to 27.9 mm, and (3) six underclad fatigue cracks in the 2.7 to 8.5 mm depth range.

1. INTRODUCTION

A requirement placed on an ultrasonic technique capable of failure prediction is that reliable information about the type, shape, size, and orientation of a detected flaw be contained in the waveform received from a region of the material containing the flaw. Additionally, waveform-processing and pattern-recognition methods readily capable of extracting unambiguous, interpretable signal parameters must be available. The challenge for ultrasonics is to provide quantitative information needed to distinguish between those small, nonpropagating defects that are benign and those propagating, crack-like defects that are malignant or critical with respect to failure.

Table 1 lists three ultrasonic techniques that do not require special transducers, instrumentation, or training for the characterization of cracks as to orientation, depth, and length. The effectiveness of these and other characterization techniques may be rated on the basis of quantiative criteria such as sizing range, signal-to-interference ratio, etc. and qualitative criteria such as cost, training requirements, etc; a complete list is given in Table 2. Reported herein are sizing results for the multiple beam-satellite pulse technique compared to conventional decibel-drop and amplitude sizing methods. Results were obtained for three sets of cladded test specimens with near-surface cracks and crack-like flaws.

2. SIDE-MILLED UNDERCLAD NOTCHES

Four notches were milled into two edges of a carbon steel block that had an 8-mm thick stainless steel cladding. These notches were designed to simulate underclad fatigue cracks perpendicular to and just beneath the cladding. Notch depth ranged from 3.1 to 12.9 mm.

The 6-dB decibel-drop technique was used to estimate the depths of the underclad notches. The results obtained with shallow (i.e., nearly lateral) longitudinal waves are plotted in Figure 1. Small cracks are oversized and large cracks are undersized. The overestimates can be understood in terms of the finite beam width of the pulse-echo transducer, and the underestimates are due to the insensitivity of the shallow longitudinal waves to the lower extremities of the large cracks. Also, with increasing depth the notches become more directional, and Snell's law of specular reflection dominates the ultrasonic backscattering phenomenon. The average sizing error of the decibel-drop technique is 31 percent (see Table 3). Table 4 shows the measurements on the same specimens using the multiple beam-satellite pulse technique; the average sizing error is 8 percent (1,2). Shear and longitudinal waves appear to be equally effective in producing diffracted waves from the upper and lower extremities or edges of the underclad notches.

3. IMPLANTED FATIGUE CRACKS

The results for the characterization of near-surface fatigue cracks in three cladded test blocks (A, B, and C) are summarized in Table 5. The ultrasonic results obtained by using the multiple beam-satellite pulse technique are compared to the crack dimensions and orientations obtained from the design drawings. With the exception of the orientation of crack B and the throughwall dimension of crack C, the test results along with their probable error range agree with the nominal crack characteristics (3).

---

*This work was supported in part by the Electric Power Research Institute, Westinghouse Electric Corporation, and the General Electric Company

T10

4. UNDERCLAD FATIGUE CRACKS*

Six underclad fatigue cracks were produced in a program designed to evaluate the accuracy and precision of the three characterization techniques listed in Table 1. The amplitude comparison technique underestimates the crack depths by a factor of 3 or 4 (see Figure 2). The decibel-drop technique results obtained with longitudinal waves do not correlate with nominal crack depth (see Figure 3). The shear-wave, full-vee examination results, on the other hand, are related to the nominal crack depths. The corner effect of the clad-base material interface at the upper edges of the underclad fatigue cracks appear to explain the observed correlation.

Significant improvements in the accuracy and precision of the depth estimates can be obtained by the application of the multiple beam-satellite pulse technique (Table 6, Figures 4 and 5).

5. CONCLUSIONS

The results indicate that the multiple beam-satellite pulse technique is applicable to cladded pressure vessels. A successful further development of the multiple beam-satellite pulse technique inspection procedure with a proper remote manipulating system would make it possible to detect, confirm, and subsequently size near-surface fatigue cracks and reliably monitor their growth.

6. REFERENCES

1. Gruber, George J. Detection of cracks in bimetallic structures by the ultrasonic multiple beam-satellite pulse technique. Proceedings of the 13th Symposium on Nondestructive Evaluation, San Antonio, Texas, April 21-23, 1981.

2. Gruber, George J. Defect identification and sizing by the ultrasonic multiple beam-satellite pulse technique. Journal of Nondestructive Evaluation, 1, 263-276, 1980.

3. Mager, Thomas R. Summary of the test results for the characterization of near-surface fatigue cracks in the three Westinghouse test blocks. Westinghouse Report MT-MNA-3142 to the Electric Power Research Institute, November 1981.

---

*The results reported in this section were not obtained with 'blinded' examiners.

TABLE 1. ULTRASONIC CRACK-CHARACTERIZATION TECHNIQUES THAT DO NOT REQUIRE SPECIAL TRANSDUCERS, INSTRUMENTATION, AND TRAINING

TECHNIQUE	MAIN SIGNAL PARAMETER	TYPE OF MEASUREMENT	ESTIMATED DEFECT CHARACTERISTIC
Amplitude Comparison	Amplitude of Reflected Pulse	Absolute Amplitude	Orientation Depth
Decibel-Drop	Decay of Amplitude Envelope with Transverse or Longitudinal Scanning	Relative Amplitude	Depth Length
Multiple Beam-Satellite Pulse	Delay Time Between Associated Pulses	Relative Pulse Arrival Time	Orientation Depth

TABLE 2. CRITERIA FOR RATING ULTRASONIC CHARACTERIZATION TECHNIQUES

QUANTITATIVE/SEMIQUANTITATIVE

- ACCURACY/PRECISION OF SIZING
- SENSITIVITY TO CRACK
  (1) LOCATION
  (2) ORIENTATION
  (3) TIGHTNESS
- SMALLEST SIZEABLE CRACK
- LARGEST SIZEABLE CRACK
- SENSITIVITY TO
  (1) VESSEL CURVATURE
  (2) SURFACE FINISH
- DEPENDENCE ON SIGNAL AMPLITUDE
- SIGNAL TO INTERFERENCE RATIO

QUALITATIVE/SUBJECTIVE

- COMPATIBILITY WITH
  (1) STANDARD ULTRASONIC EQUIPMENT AND TRANSDUCERS
  (2) MOST RELIABLE DETECTION TECHNIQUES
- POTENTIAL FOR AUTOMATING DATA ACQUISITION
- COST OF IMPLEMENTATION
- LEVEL OF SPECIAL TRAINING
- ACCEPTABILITY OF TECHNIQUE BY THE NDE COMMUNITY

TABLE 3. DEPTHS OF UNDERCLAD NOTCHES IN BLOCKS ESTIMATED BY THE DECIBLE-DROP TECHNIQUE

NOTCH	DEPTH PARALLEL TO Z-AXIS IN MM[1]			DEPTH PARALLEL TO Z-AXIS IN MM[2]	
	ACTUAL	MEASURED	ERROR IN %	MEASURED	ERROR IN %
UN1	3.1	4.6	48	5.6	81
UN2	6.4	6.3	3	4.9	23
UN3	9.5	8.1	-15	8.8	-8
UN4	12.9	11.9	-8	9.4	-27
AVERAGE SIZING ERROR IN %			27		35

1. Half-Vee Examination with 60-Degree Longitudinal Waves

2. Half-Vee Examination with 72- and 77-Degree Longitudinal Waves

TABLE 4. DEPTHS OF UNDERCLAD NOTCHES IN BLOCKS ESTIMATED BY USING THE MULTIPLE BEAM-SATELLITE PULSE TECHNIQUE

NOTCH	DEPTH PARALLEL TO Z-AXIS IN MM[1]			DEPTH PARALLEL TO Z-AXIS IN MM[2]	
	ACTUAL	MEASURED	ERROR IN %	MEASURED	ERROR IN %
UN1	3.1	3.3	6	3.6	16
UN2	6.4	6.2	-3	5.5	-14
UN3	9.5	9.2	-3	9.3	-2
UN4	12.9	10.9	-16	13.0	1
AVERAGE SIZING ERROR IN %			7		8

1. Half-Vee Examination with 45-Degree Shear Waves

2. Half-Vee Examination with 55-Degree Longitudinal Waves

LE 5. CHARACTERISTICS OF IMPLANTED FATIGUE CRACKS IN CLADDED PRESSURE VESSEL BLOCKS ESTIMATED BY VARIOUS ULTRASONIC TECHNIQUES

	DEPTH PARALLEL TO Z-AXIS IN MM		[illegible] PARALLEL TO Y-AXIS IN MM		ORIENTATION TO YZ PLANE IN DEGREES	
	NOMINAL	MEASURED	NOMINAL	MEASURED	NOMINAL	MEASURED
A	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
B	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
C	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

SATELLITE-PULSE AND MULTIPLE-BEAM TECHNIQUES

DECIBEL-DROP TECHNIQUE

AMPLITUDE-COMPARISON TECHNIQUE

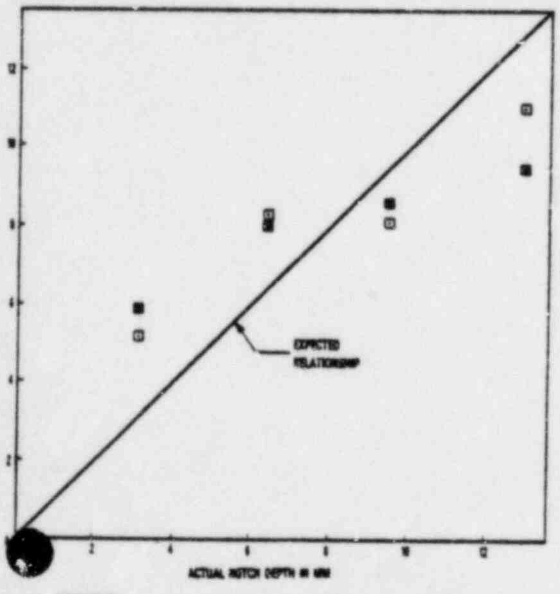
TABLE 6. DEPTHS OF UNDERCLAD FATIGUE CRACKS IN BLOCKS ESTIMATED BY THE MULTIPLE BEAM-SATELLITE PULSE TECHNIQUE

	DEPTH PARALLEL TO Z-AXIS IN MM[1]			DEPTH PARALLEL TO Z-AXIS IN MM[2]	
CRACK	NOMINAL	MEASURED	ERROR IN %	MEASURED	ERROR IN %
UC1	2.7	2.1	22	2.1	[illegible]
UC2	4.1	4.5	[illegible]	4.5	[illegible]
UC3	6.4	7.2	[illegible]	7.3	[illegible]
UC4	6.6	6.7	2	6.7	[illegible]
UC5	8.3	7.4	11	9.4	[illegible]
UC6	8.5	5.8	[illegible]	7.2	[illegible]
RAGE SIZING ERROR IN %			[illegible]		[illegible]

---

[1] HALF-VEE EXAMINATION WITH 45-DEGREE SHEAR WAVES

[2] HALF-VEE EXAMINATION WITH 60-DEGREE LONGITUDINAL WAVES



IGU 1. CORRELATION BETWEEN UNDERCLAD NOTCH S OBTAINED BY THE DECIBEL-DROP TECHNIQUE AND ACTUAL NOTCH DEPTHS

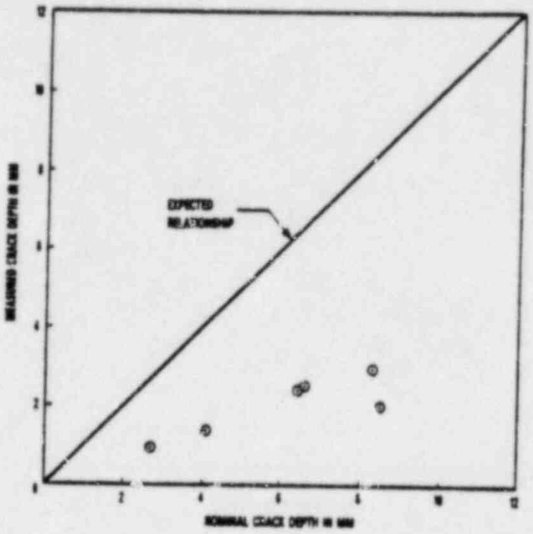


FIGURE 2. CORRELATION BETWEEN UNDERCLAD CRACK FATIGUE DEPTHS OBTAINED BY THE AMPLITUDE-COMPARISON TECHNIQUE WITH 45-DEGREE SHEAR WAVES AND NOMINAL CRACK DEPTHS

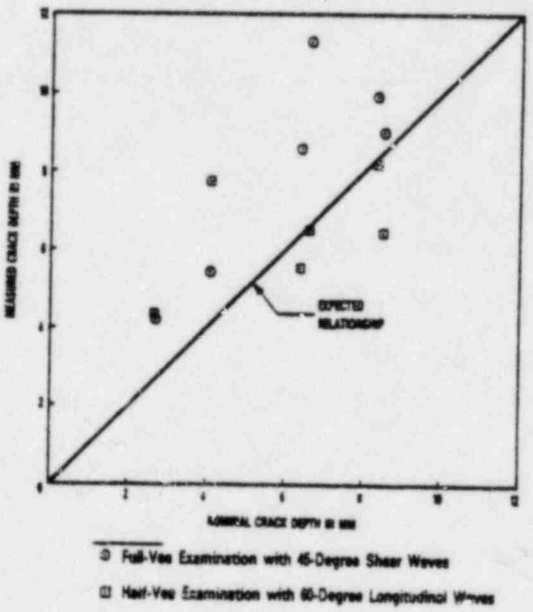


FIGURE 3. CORRELATION BETWEEN UNDERCLAD FATIGUE CRACK DEPTHS OBTAINED BY THE DECIBEL-DROP TECHNIQUE AND NOMINAL CRACK DEPTH

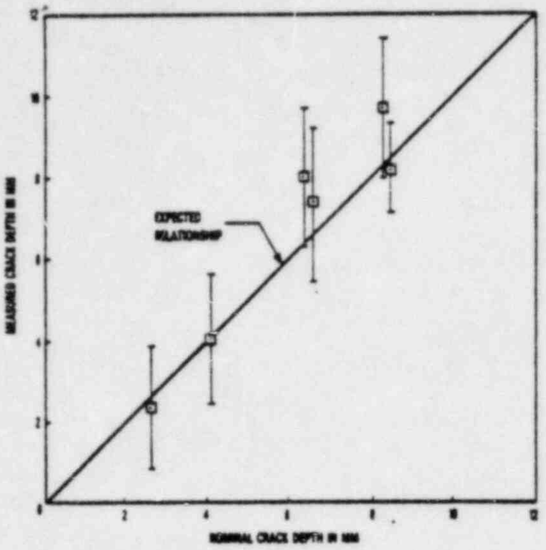


FIGURE 4. CORRELATION BETWEEN UNDERCLAD CRACK DEPTHS OBTAINED BY THE MULTIPLE BEAM-SATELLITE PULSE TECHNIQUE WITH 35-, 45- AND 53-DEGREE SHEAR WAVES AND NOMINAL CRACK DEPTHS

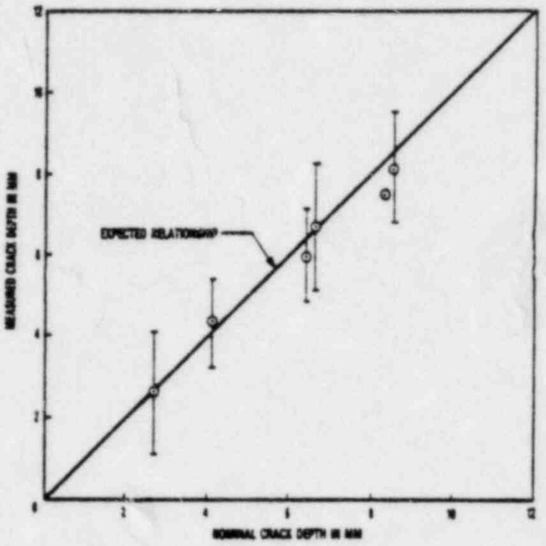


FIGURE 5. CORRELATION BETWEEN UNDERCLAD CRACK DEPTHS OBTAINED BY THE MULTIPLE BEAM-SATELLITE PULSE TECHNIQUE WITH 55-, 72- AND 77-DEGREE LONGITUDINAL WAVES AND NOMINAL CRACK DEPTHS