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UNITED STATES
NUCLEAR REGULATORY COMMISSION

In the matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS,

SUBCOMMITTEE MEETING ON ANTICIPATED TRANSIENTS

WITHOUT SCRAM (ATWS)

Place: Washington, D. C.

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INTERNATIONAL VERBATIM REPORTERS, INC.
499 SOUTH CAPITOL STREET, S. W. SUITE 107
WASHINGTON, D. C. 20002
202 484-3550

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UNITED STATES

NUCLEAR REGULATORY COMMISSION

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

Wednesday, March 26, 1980

The Advisory Committee on Reactor Safeguards, Subcommittee Meeting on Anticipated Transients Without Scram (ATWS), met, pursuant to notice, at 8:30 a.m., Mr. Kerr, Chairman of the Subcommittee, presiding.

PRESENT:

Dr. Mark

Mr. Ray

Mr. Ditto

Mr. Epler

Dr. Lipinski

Dr. Saunders

Mr. Thadani

Mr. Mattson

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P R O C E E D I N G S

CHAIRMAN KERR: The meeting will come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, the Subcommittee on Anticipated Transients Without Scram.

My name is William Kerr. Our subcommittee members present include Mr. Mark and Mr. Ray. Consultants are Mr. Ditto, Epler, Lipinski, and Saunders.

The meeting is a continuing discussion of the ATWS problem with representatives of the Nuclear Regulatory Commission Staff and the nuclear industry. The meeting is being conducted in accordance with provisions of the Federal Advisory Committee Act and the Government and the Sunshine Act.

Mr. Paul Boehnert is a designated Federal employee. Rules for participation have been announced as far as the meeting notice published in the Federal Register of March 11th, 1980.

A transcript of the meeting is being kept and will be made available and stated in the register notice. We have received no written comments or requests for trying to make oral statements from -- mainly to the public.

We do have, if you have an agenda, you will note scheduled time for statements or comments from members or representatives of various components of the industry as well as the scheduled presentation by the NRC Staff.

I don't know how to guess about the schedule. I will probably have to leave by 5:15. My guess is that the meeting will last that long, but we will see how things develop.

I will plan to try to break for lunch if it seems reasonable at some time around 12:30 or so. And, other than that, I guess we will play things by ear.

I am informed that Mr. Hanauer who was scheduled to make part of the presentation by the NRC Staff is ill today and other staff members will fill in for him.

We will proceed with the meeting and I call upon Mr. Mattson who I believe is serving as spokesperson today. Mr. Mattson.

MR. MATTSON: Thank you, Professor Kerr. It's been over a year since I had the pleasure of sitting on this Subcommittee on this subject. I can see the faces haven't changed a bit.

Considerable progress has been made, I think, on ATWS deliberations in the last year, not as fast as we had hoped a year ago. For obvious reasons people were occupied with some other things the first few months of the last 12.

I've had a couple of opportunities in the last three or four months to hear of the approaches that were being suggested to bring this long-standing issue to some conclusion. And, I know Harold Ditto's committed to reaching some early decisions on the subject and not letting it continue to trail along.

In order to expedite reaching those decisions, some modifications of some previously proposed alternate solutions have been made. Even though you've got a copy of Volume 4 of NUREG 0460, Shook is here to walk you through it and talk about it in some detail.

We hope there hasn't been a whole lot of confusion of the previous alternative approach by the slight modification of those approaches. They are the product of what you'll recall we talked about a year ago, early verification. That early verification showed us some new information, some new ways of thinking about some of these approaches which have required their slight modification to continue to be able to say that they do the things we thought they did when they were originally constructed.

I'll try to answer any questions you might have as we go along in the course of the morning on how ATWS, the ATWS solution might fit into other things going on in the Staff or to answer questions on how Mr. Denton or Dr. Hanauer viewed the need for timely decisions on ATWS.

But the rest of the technical presentation, I'm going to turn over to Mr. Thadani since he and Steve were the prime architects of Volume 4 of NUREG-0460. We have a number of other technical staff here to respond to your technical questions.

With that brief introduction, I propose to turn it over to Mr. Thadani, unless you have questions.

CHAIRMAN KERR: Mr. Mark has a question, I believe.

DR. MARK: I'm afraid this is a bit vague as a question.

ATWS, viewed as a thing in itself, Roger, pays most of it's attention to the operation of the Scram System. If you are prepared to deal with anticipated transients without Scram, this is another theorem that you must then surely be able to deal with all transients, that this would constitute a -- I mean, regulations for ATWS would thereby put you in a perfectly solid situation so you'd never have to talk about them again, all possible transients.

I believe there are some items in the action plan which also discuss transients. And, are these then supposed to be, if not amalgamated, amalgamo, or in the course of becoming amalgamated.

So, that the analyses required perhaps by some group or some transient could be skipped because you already know how to do it even if there's no Scram.

MR. MATTSON: I think there is a whole in that theorem, but there are efficiencies that will accrue of the sort that you suggest. Let me try to describe both of those things.

First, the whole is, you can melt the core down when you had Scram when the transient is the initiating event. And you can melt the core down when you have a transient and a failure to Scram.

So, simply fixing the failure to Scram, doesn't necessarily fix all core melt sequences.

CHAIRMAN KERR: I think the question was that if you could deal with ATWS which is anticipated transient and failure to Scram and dealing with the problem, that is. Am I correct?

DR. MARK: I believe, something like that.

MR. MATTSON: I think the answer is no.

CHAIRMAN KERR: So, there are some transients that are worse with Scram than if you didn't have Scram?

MR. MATTSON: No, there are some transients that could get worse even though the reactor Scram, than transients for which the reactor didn't Scram, but there was a backup to Scram.

CHAIRMAN KERR: But then you would not have dealt with that transient --

MR. MATTSON: TMI-2 being a classic example. It scrammed and it had severe core damage. Fixing ATWS on TMI-2 would not necessarily have prevented the significant core damage that resulted from that feed water transient.

DR. MARK: Well, in any event, there will be some tying together of what we're talking of today and some of the things which might seem to appear under separate headings.

MR. MATTSON: There are things which you do for ATWS which improve your capability to handle other transients. For example, the number of the changes made in auxilliary feed water systems in the course of the last year because of Three Mile Island, and AUX feed water reliability studies done last summer, are the same kinds of things that would have been done to AUX feed water systems under the proposed ATWS fixes of the Staff for the last ten years.

CHAIRMAN KERR: Are there other questions to inquire of the Subcommittee members at this point?

DR. LIPINSKI: Well, perhaps I can express

-- not so much a question, but perhaps a perplexity with which I am faced as I try to understand the documentation associated with this rather complicated problem.

I feel naive at this point to say that I am still not quite certain what the ATWS problem is, but I must confess to this naivety. And, I express it best if I say it seems to me that there are atleast two ways of raising the question of describing the problem and from the documentation I'm not sure which is the appropriate way and I would appreciate some assistance as the presentations develop.

It occurs to me that one way of expressing it is to say that a failure to scram in the case of those transients in which one needs a Scram system to handle the problem can occur, is it possible that one does get a failure to stand, thus invoking Murphy's law or some other appropriate theorem, one assumes that the failure will occur and that we therefore have an obligation to demonstrate that the plant must be capable of dealing with the situation.

That is an impression I get as I read part of the documentation. It strikes me that another alternative is to say that the probability of ATWS can be demonstrated with existing information to be acceptably low and therefore we have an obligation to take steps that produce a situation in which it is perceived to be acceptably low.

I'm not sure these are mutually exclusive and I'm pretty certain they aren't all inclusive. But, -- I guess the difficulty I have is in shifting from the probabilistic to the deterministic.

It was my impression that the original demonstration of the problem was one which appealed to the probabilistic, rightly so, I think. And I think the probabilistic approach with the goals that were stated does drive one or bring one to the conclusion that existing information can't demonstrate compliance with those goals.

But then, as I read what is being proposed, I find myself back into a deterministic situation in which I could almost arrive at the conclusion and we have concluded that failure to Scram will occur and that plants, therefore, must be capable of dealing with it on a deterministic basis almost with a probability of one.

I'm perhaps not expressing my perplexity appropriately, but -- And maybe it's because of this perplexity that I have difficulty in expressing it.

I hope you get some flavor of what I'm talking about.

CHAIRMAN KERR: Other questions or comments?

Mr. Thadani, I guess the floor is yours.

MR. LIPINSKI: I wanted to add one comment to your statement. The second part, given that the ATWS will occur, is probability that one, the systems that function in sequel don't necessarily have to perform to the same reliability of the ATWS initiative.

That's another consideration as to how effective the backing system is.

CHAIRMAN KERR: I don't disagree with your viewpoint. I'm saying that I have some difficulty in determining from the reports I read, what the viewpoint is of the documentation.

MR. LIPINSKI: Right, I agree with you in terms of the way you stated the problem. But, given the fact that you end up with this viewpoint that ATWS will interfere with probability one, what then is the probability that you can handle the sequence? That is another question that remains to be addressed in terms of the total solution.

MR. THADANI: My name is Andrew Thadani. I'm on the NRC Staff. I'll make an attempt to summarize the major portion, the contents of NUREG-0460, Volume 4, hopefully the last one.

It might be worthwhile just to spend a couple of minutes before we get on to Volume 4 to talk about how we got to Volume 4.

You remember in December of 1978 we issued NUREG-0460-- Volume 3.

NUREG-0460, Volume 3 proposed or it seemed to be a fairly way involving this highly controversial issue.

In Volume 3 we proposed three different alternative plant modifications, three different categories of pla ts.

The first category of plant consisted of the O-11 operating units, whose designs, in our opinion, are sufficiently different than those which we've looked at generically. And thus we felt the conclusions that we had arrived at as a result of our review of the generic studies might not be completely applicable to those early 11 designs.

Therefore, in Volume 3 we recommended that those 11 plants be modified so as to reduce the likelihood of an accident and in a few minutes I'll get into some details of the types of hardware modifications that were considered then.

And, it was further recommended in Volume 3 that plant unit analysis for these early 11 units be performed and any additional hardware modification be considered in the context of overall safety of these units, as well as volume impact considerations.

Another class of plants defined in Volume 3 with those operating units and units which had received their construction permit license prior to January 1, 1978. When these plants -- It was proposed in NUREG-0460, Volume 3, that:

(A) Modifications be made in Scram Systems to reduce likelihood of ATWS event.

(B) To provide some mitigation capability.

The industry was to demonstrate by generic analyses, the adequacy of the hardware defined in Volume 3 as alternative 3 modifications.

Yet, another alternative defined in Volume 4 was so-called alternative 4 for a class of plants which had received their construction permit on or after January 1, 1978.

In that case, the staff had required modifications in designs to assure confidence that the consequences of ATWS events would be mitigated.

Notably accent from alternative falls in NUREG-0460, Volume 3, was any requirement to modify Scram System to reduce the likelihood of an accident.

The major emphasis at that time was to assess the capability of operating plants which were going to be operational in the near future and to determine if, indeed, just som electrical modifications in the case

PWR's and electrical as well as some hiking modifications in BWR's would be adequate.

We issued a set of questions and guidelines to the industry to a letter from Roger Mattson on February 15, 1979.

The industry was to respond to this set of questions to demonstrate what they believed at the time to be indeed the case that plants modified in accordance with alternative three would be able to withstand the consequences of these ATWS events.

Now, I said, these ATWS events for a very particular reason, because not only had we talked about different possible criteria for alternative three as well as alternative four class by way of acceptance limits, but we also specified a significant difference in terms of PWR's that alternative three plants, it's either that volume of moderated temperature -- which would be, experience no more than 5 percent of the time, 5 percent of the life time of the plant.

Alternative 3 also did not require any additional single failure considerations in mitigating systems. It suggested that all systems be presumed functional unless the consequences of ATWS events in that given system.

With that background of what was in Volume 3 and subsequent requests that we set out, I think we can get into the bases to a certain extent and some discussion of what's in Volume 4.

I would try to give you highlight of what is in Volume 4 and some of the reasons. We have a number of Staff members here today.

If you have a need or desire to get into any specific technical area in great detail, I think we would be able to do that today.

As a result of our request for February 15, 1979, industry submitted a number of reports. At the last upcoming meeting, I summarized the information that had been submitted and I provided you with an initial reaction to the submitted information.

These submittals came in groups, if you will, so the information was provided late last year, whereas a fair amount was provided early this year.

The major considerations that still are open as a result of our view of these documents are summarized on this slide. I want to make it pretty clear that these submittals addressed alternative three as defined in NUREG-0460, Volume 3.

Some of these undisolved considerations would also be applicable to alternative four type of design modification.

The first one, under PWR is code verification. Well, it is our belief that the peak pressure calculations performed by the vendors using what we call systems codes, area reasonably good.

We describe in response to an earlier ACRS questions, the basis for our judgment of the peak pressures transmitted by these codes were believed to be reasonably accurate.

When we recognize that there is insufficient infirmatory experimental verification of these pressure calculations, and thus over the long term, we would like to have some experimental verification using some of the facilities such as locked, semi-skill, separate effects, other available experimental facilities.

The big concern in terms of codes, capability, is, I believe, and we believe in general, that these codes are not capable of handling significant void fraction in the primary system.

As you know, an ATWS event is also in the early portion of an increase of temperature of the reacting system, followed by pressure purge and opening of the releaving devices on pressurizing, a significant amount of coolant is lost in containment, in some cases as much as one half of the capacity of the coolant in the reactor room system.

This, as the pressure turns around, when power reduces, voids are formed and you end up with fairly significant void fraction in the primary system.

And this concern is no different than what you have heard in the last several months, that large voids in the primary system, some concerns about various velocities and bubbles, what would be the density in the core which really is what effects the temperature and reduces the power.

I think that's not of major concern. I think the major concern is, under those conditions, are how well can we remove energy from the primary system to the steam generators.

We think that those types of calculations should be done in acceptable small LOCA codes as is done over the last summer.

DR. MARK: But, you're talking of a time frame which might begin to start when, like an hour after ten zero?

MR. THADANI: No. I'm talking about roughly on the order of 10 minutes after time here or even sooner than that.

DR. MARK: The voids might form?

MR. THADANI: The voids form roughly, if I remember correctly, it depends on the design, but on the order of two minutes to 4 or 5 minutes.

DR. MARK: Right. Now, the difference between this and the thing like Three Mile Island where there were voids formed and so forth, consists of what, that the power is impossible to get down below 20? Because, if the power's the same, then it's -- It's what?

MR. THADANI: I think you hit it. I think - I agree with you, that if the power were the same, we would say whatever conclusions we reached this summer. And, we said it was okay in terms of small break LOCA's to go ahead and continue to operate on certain bases, those bases would be applicable to ATWS. You hit the key point.

The difference now is delta in terms of the power that's being generated. How much of an influence that has in terms of being able to go through natural circulation, the reactful will be decapitated fairly early and depending on what requirements are based on tripping of reactical pumps, it's conceivable that in the event of an accident, the reactor coolant pumps may be tripped early, before even getting to the situation where they would start to cavidate.

CHAIRMAN KERR: This discussion is somewhat illustrative of my confusion. We're talking about ATWS now in the way that we talk about small break LOCA's, which is a design basis accident.

Is it implicit in Volume 4 that ATWS is to be a design basis accident?

MR. THADANI: I hope not.

CHAIRMAN KERR: -- when we're through with the implications? You see the -- I mean, we're talking about it the way one talks about a design basis accident.

Confirmation codes which are suitably conservative and details which are deterministic and I don't know how to distinguish between this and a design basis accident anymore.

MR. THADANI: If I may --

CHAIRMAN KERR: I don't mean that you have to settle the question here, I'm just trying to point out the confusion that I feel in trying to distinguish between this and a design basis accident.

MR. THADANI: Yes, I understand the point and during the course of this morning's discussion, I hope atleast some aspect would become more clear to you, or they would be less clear to me, one or the other.

You said a word that concerns me. You said -- We're talking about doing calculations using suitably conservative approach. I don't know for sure what suitably conservative means.

Our attempt all along has been to try to assess the -- cores of the postulated -- event. In that regard we have not added on uncertainties in calculational tools. As for the small LOCA codes are concerned, they have intransit conservatisms in them, things like 1.2 times the ANS in case it occur.

We would recommend that for ATWS the calculations be done realistically, that some verification of the codes be provided at a later date and should that verification be insufficient, then we would go back and require some relative assessment of --

The second part of your comment was deterministic vs. probabilistic and I think you're quite right when you started, as I'm sure you remember, in April of '78 when we published Volumes 1 and 2 of NUREG-0460, the proposed numerical safety objective which would reduce from 10 to minus 7 as specified in March 12th, '70 through a minus 6 for reactor year in NUREG-0460 Volumes 1 and 2.

The rationale, of course, for that changed from 10 to the minus 7 to 10 to the minus 6, came almost entirely from the reactor safety study, consequent discussions of the Commission of the Lewis Report on Safety study and the uncertainty in the overall -- discussions that ensued indicated to us --

And I might add, that we had been told by our management, even prior to that, that they weren't fully convinced that the prose that was proposed in Volumes 1 and 2 would be easily implemented.

We didn't totally throw away the numerical considerations that had gone into some of the requirements, but rather we went more towards what I would call some kind of engineering judgment and that was the main reason for proposing alternative three to see if it could be verified by analyses.

But that's the numerical analyses that had been performed up to that time, were not thrown away. They were maintained and they were used as supplementing piece of information to be used in determining our requirements.

If you look at Volume 4, there is indeed a discussion of equipment reliability and there are some numbers and are hopes of the kinds of overall unreliability of the total mitigin system that we hope to achieve under alternative 4-A.

While I think you're right that we're not talking explicity about the numerical safety endorse, but we're still talking about giving an ATWS. What are relative improvements if you go 2-A, 3-A, 4-A, as defined in Volume 4 and these are slight modifications of alternatives 2, 3, and 4, as they were defined in Volume 3.

Going on to the unresolved considerations in these industry summerals, the peak pressures that were calculated by some -- P. W. R. Landers, are extremely high. I indicated to you last time that pressures well in excess of 4,000 pounds were calculated by combustion engineering and that the actual heat pressure was substantially above 4,000 pounds and substantially above 5,000 pounds.

If credit had not been taken for -- and subsequent discharge of the primary coolant through the orin seal.

We've also, as a result of the information that we have seen recently, been concerned about the capability of instruments, especially in the type of instrument would be extremely useful for the operator to determine the course of events and to take corrective actions.

B&W analysis, as I indicated to you at the last meeting, are in our opinion inadequate because of the assumptions that were made in these analyses. We have called these functions optimistic, overly optimistic in our volume 4 report. I just don't have any basis to agree with B&W on the type of assumptions that they have made in analyzing ATWS events.

They're -- On the basis of what we call optimistic assumptions, they're calculating peak pressures in the range of I believe 35 to 38.

I would guess that if they were to modify and found calculations consistent with our set of guidelines, the peak calculated pressures would go up by a few hundred pounds. I'm not sure exactly how many hundreds of pounds.

CHAIRMAN KERR: Well, when you use words like realistic or optimistic or pessimistic, is this in relationship to what? What would one would expect to find in a normally operated plant some percent of the time or --

MR. THADANI: That is correct. I can give you some examples. For example, auxilliary feed where actuation in the B&W plant -- The timing is very significant. The tech specs, indicate, I believe, that AUX feed system be available in 40 seconds or it might vary for some time maybe longer than 40 seconds in some cases.

But the ATWS analyses -- early ATWS analyses of many years ago assumed AUX feed would be available at 40 seconds which was consistent with atleast some of the tech specs.

Subsequent analyses assume AUX feed would be available at 25 seconds. Recent submittals seems that AUX feed would be available in 15 seconds.

No justification is available that we've seen. From the little experience that we have, we find 15 seconds certainly to be optimistic.

The requirements are 40 seconds on some plants that I said and it is conceivable that on some plants

AUX feed may indeed come on as early as 15 seconds but to imply that that is indeed the case for all plants seems to us to be unreasonable without some further, what I would call substantial justification of that assumption.

Other examples, you touched upon fraction of life time. The value of the moderated temperature co-efficient that they have used, we're in somewhat disagree with them on that. If they were to use the value that we have suggested, that would result in pressure increase probably, I would guess, about 200 pounds, based on sensitivity studies that we've seen in the past.

And, it's factors like these that need us to be concerned that we may be at this argumentative stage for quite some time to come and not arrive at what might be a reasonable solution to the ATWS problem in the near future, and that is the major reason that we have embarked on what Roger described earlier as the two study process.

And, I would touch upon later on and indicate to your hopes and plans, probably would intend to go by requiring modifications at what stages and in what weeks.

I'd like to quickly go through some of the other items. The next one should not be insulated PORV's, it should be isolated PORV's.

There are a number of operating POVR's, are indeed operating but there are operative release valves -- isolators, but they experience leakage through these valves.

And, they have operated with those valves isolated for extended periods of time, in some cases months and in one or two cases in years.

Again, as an example in terms of alternative three, we had indicated to the industry that to assume our systems were functional which meant that all the valves would be available to limit the pre-calculated pressure and this would include the R-operated leak valves.

Now, in a number of Westinghouse designs, for example, the PORV's represent roughly 25 percent of the leading capacity which is significant and does have a significant impact on the calculated peak pressures.

We have not received any information as to how those plants would be addressed in response to the set of questions that we transmitted to the industry on February 15, 1979.

Tape 2/1

DR. MARK: First you count on them in the calculation but second in the field. Some of them aren't operable. And, what affect does it have on a pressure calculation to assumed an operable PORV with the old set points and now to have to follow the orders to change the set points?

MR. THADANI: You are addressing the B& W designs for the PORV set point was changed from 2250 to 2450.

DR. MARK: Right.

MR. THADANI: And the early high pressure set point. Essentially there should be no impact. As far as the calculative speed pressure is concerned, if you open the relief valve at a set point of 2350 versus 2450, because the pressure rise in the B&W design plant is so rapid that you're talking about milliseconds before you changed the pressure from 2350 to 2450.

The major considerations that are discussed in the report in substantial detail in the Appendics as far as boiling water reactors are concerned are again summarized on this slide. The first one -- the new one that I addressed at the last meeting is the so called oscillations that the reactor seems to go through 2 or 300 seconds following the initiation of an actless event. And the oscillations seem to continue for several hundred seconds.

Tape 2/2

I indicated then that the altitude of these oscillations were substantial on the order of at least by key calculations 110 to 120 per cent increase in flux with period of 4 ot 8 seconds.

The difficulty with these oscillations is two fold, as I see it. One is that the timing -- when these oscillations occur there's also about the same time when the climbing temperatures gone up towards the range of phase transformation. And the -- of course the fuel temperature has gone up to a very significant spark that you get and the increase in neutron flux that you get from BWR transients.

The fuel temperatures they may approach values which could result in some --. This increase in fuel temperature --

CHAIRMAN KERR: Do you remember off hand, how long after the initial beginning or after the beginning of the transient this sort of thing is expected to occur?

MR. THADANI: On the bases of these calculations it happens roughly 3 1/2 minutes following the on set of the event.

CHAIRMAN KERR: This assumes pump trip has occured in the interim?

MR. THADANI: That's correct. This assumes pump trip has occured in the interim. The the AD6 DPM

Tape 2/3

SOCS with 13 per cent sodium penabard solution at roughly 2 1/2 minutes. That is at 2 1/2 minutes you start to get some boran in the coolant water.

DR. MARK: Am I right in calling these oscillations, up to now at least, have only been observed in GE in calculations? You often tend to question the results of GE calculations if they show the temperature drops to an acceptable level. These oscillations are disturbing. Are they real?

They come in the ready code, I presume which you also are inclined to regard as unreliable. Do they also show up in the oden code? Or do we have any pure physical bases for knowing they would be there?

MR. THADANI: Okay. I think -- I think many questions. Let me give it a try. First, we do have concern with ready code. We've asked GE repeatedly to perform these analyses using oden code which we reviewed and evaluated and find it more satisfactory as a calculational tool than ready. We have also indicated in our new short term calculations for our ready are probably good.

The oscillations were seen using ready code. We've asked GE to extend their analysis from first 60 seconds to first several minutes using oden. We have not been successful so far in getting that kind of analysis.

Our discretions with GE indicated to us that the

Tape 2/4

oscillations are probably real. That there has been some experience at some plants. If I remember correctly Dr. Lipinski, I think, indicated the last supplement reading that he knew of a reactor which had indeed gone through some substantial oscillations. And I believe,Steve Hanar has mentioned to me and I just don't have the facts, but he did indicate to me that he had also seen some oscillations in some other BWR designs. And this particular plant, I think he was talking about, is not in this country. I think it was at Brage. And perhaps Dr. Lipinski can -- can tell us about his experience.

DR. LIPINSKI: Well,the borax 1 and 2 experiments explored the fundamental behavior of boiling water reactors. And in those particular reactors it was natural circulation and the power levels were deliberately increased untill the chugging phenomenon did set in.

In the case of the experimental boiling water reactor, that was a conservative design at 20 mega watts thermal. But based on our oscillation experiments and stability measurements, we concluded we could run that facility to 100 megawatts thermal without encountering instability and we did.

But had we proceed beyond 100 megawatts, all the indications were that the chugging phenomenon would have set in. Again that was an natural circulation

Tape 2/5

reactor. It was not forced circulation. But there is a limit in terms of the amount of reactivity that can be inserted into the core of a boiling water reactor before the chugging phenomenon sets in.

And when General Electric first raised that issue of pump trip, that was my first question. That when the pumps did coast down, what was the assurance that the reactor would operate in a stable emulsion.

MR. THADANI: We have also as -- as you well know, over the past 2 or 3 years been trying to get more information on silitory behavior.

The next item is one that we have discussed at great length with you in the past. Just want to indicate to you that under a tentative speed of 3A as defined in the 0460.

In some cases the local cool temperatures and the still exceed what we've talking about in excess of 200 degrees Fahrenheit. But I don't really beleive, at this stage, that in terms of the temperature, that it would go much beyond 200 degrees, local temperature.

However, the concern is in terms of loads that are imposed by the actuation of the safety relief valves. We believe that the loads imposed during an apuse would be substantially higher than those experiencing during transcience.

Tape 2/6

And even though the evaluations are generally performed in a conservative manner, we're not yet convinced that the design bases consideration would indeed incorporate the kinds of conditions we're talking about doing anakalis.

DR. MARK: I was curious about this at that point. These oscillations are mechanics of the suppression pool have received tremendous attention in connection with the double ended pipe break. In fact, they're suppose to sit quietly through that.

Are you saying that anakalis load is going to a larger demand than that?

MR. THADANI: Yeah, maybe I wasn't very clear. What I was testing there was you know if the down comer is submerged, the suppression pool is a column of water and it's a column of air. And as the valves open, you compress air and shoot through the down comers. And you create initially large loads. Shoot the air clearings.

DR. MARK: Well, that's the same as in the large look.

MR. THADANI: Yes, but the difference here is from our understanding of data and the analytical methods that we've looked at, we find that 3 things that seem to have impact on loads.

One is the suppression pool pressure. Another is the pressure pool temperature, and a third one is

Tape 2/7

the threstle pressure.

During an acquicavy you do have substantially more severe conditions than you would during a transcient. You would indeed open up all the valves. Your primary system pressure is fairly high.

When I said primary system I'm talking about the vessel pressure of BWR is fairly high. And it's just not obvious to us why that situation is covered by a transcient situation.

We have specified in Volume 4 how we might be able to get a resolution on this issue. And I might point out this issue would still be applicable under alternative 4A. It's not to say that if we go to alternative 4A, this concern would go away. No, I think it's just a matter of providing enough information to justify that GE believes that apres loads are covered by the transcient impulse loads.

And we've listed the kinds of information we would need to -- before we could agree with General Electric that indeed the loads are no never mind.

The big difference between what I would call alternative 3A and 4A is the next item. System realiability. As I indicated earlier under alternative 3 and 3A, I'll discuss later on, we assume all the systems were viable regardless of the reliability or unreliability of those

Tape 2/8

systems. Whereas under alternative 4A we pay some attention to what I would call an implicit safety objective. Dr. Kerr's concern, I think, is very real but if you look very closely and carefully at alternative 4, I think you tend to get a feeling that there is some numerical safety objective consideration here.

If it hadn't been, the requirements would not have been writen the way they were. For example we say, given an innocuous event, would like to make sure 1 out of 100 -- no more than 1 out of 100 innocuous events would receive certain pre-specified conditions.

In order to do that we impose some requirements on systems which are relied on to litigate consequence of innocuous events. These requirements are specified both in Volume 3, as well as in Volume 4. Volume 3 has more details in the non-reliable area requirements. Whereas Volume 4 amplifies what we meant by the reliable criteria that was specified in Volume 3.

Our hope is to be able to show, given an innocuous event, the combined unreliability of the systems that are relied on is on the order of 10 to the minus 2.

And we also recognize the problem of calculations, assumptions to arrive at an unreliable estimate. And it was for that reason that we also proposed that another mechanism will achieve the same objective would be to

Tape 2/9

design these systems to met the so called IEEE 279 criteria. From our experience -

DR. LIPINSKI: Let me ask a question on that point. IEEE 279 doesn't led you to any degree of reliability other than giving you prescriptions for good practice. And the way the document is proposed saying that IEEE 279 is acceptable, I don't see where the 279 offers any degree of assurance to even get to the 10 to minus 2 number that you just quoted.

MR. THADANI: Well, the reason -- rationale there is based on experience. We've looked at systems which are so called safety systems or -- and the data that we have particularly I'm referring to the data in WASH 1400.

If you look at that information carefully, you realize that a system that's designed to these criteria and standards does seem to have an unreliability in the range of 10 to the minus 2 to 10 to minus 4.

And that was an implicit consideration that these standard guides move you in a direction of good practices. And that the data base, which was certainly reviewed in WASH 1400, seemed to concur with our judgement that those systems would have a reasonably high reliability.

We could get into some specific systems and numbers at some time but for you information in response to a question that was raised, I believe by one of the

RRRC members. We addressed just that question in a different way. That if you apply the reliability approach to the systems that are out there today, all kinds of conclusions would you walk away with regarding unreliability of those systems.

And in Volume 3 in one of the appendics, I forget whice appendix, but at least in one of the appendics, we have the responses to questions from NCRS as well as from Blarcy. And we provided some numbers of unreliable systems there. And if our -- it's my judgement that those are pretty consistent with our hopes of achieving 10 to the minus 2.

DR. LIPINSKI: There is one further problem with 279 in that it covers only electrical systems. It makes no stipulation about mechanical components.

In the case of a PORV it would include the celluloid actuator and it stops there. As to what quality of bell exists after celluloid is not covered by IEEE 279.

So even though you may have single failure requirements and multiplicity redundancy, you still have no assurance when it comes to the mechanical components.

The systems we're talking about here, for back-up, are primarily mechinical in terms of valves and pumps. Pipes, tanks, and their not at all covered by that standard.

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MR. THADANI: Yes, you 're right they're not. Again if you look at the data base you find generaliy mechanical failures are less likely than electrical failures. Quite frankly I think the point -- the PORVs.

The PORV reliability was assessed in one of the vendor publications from many years ago. In which they concluded that the unreliability of the PORV to often on demand was somewhat greater than 10 to the minus 2. And was roughly 10 to the minus 2.

That was one of the major reasons why in Volume's 1 and 2 of NUREG 0460 we specified that as a single failure. Now, on those designs there are 2 or more relief valves. And we were looking for an overall of about 10 to the minus 2, as I indicated. And if you look at sub-systems like PORV's safety valves, oxzy systems and so on.

At the time we were looking for a number like 10 to the minus 3. And if you assume that one out of 2 PORV's fails to open on demand, you would get the kind of unreliability that we're talking about. 9 of 30.

As long as the system responce was not influenced by the acquicent event itself.

DR. LIPINSKI: Well, let me conclude by saying I could design a system that would meed IEEE 279 that may never function and still provide evidence that I'd met

Tape 2/12

279 in all it's respects yet have totally unreliable mechanical pieces.

Implied in what you're saying is some kind of faith that mechanical components that have been used in the class will be used in conjunction with 279. But 279 in itself does not get it in there.

CHAIRMAN KERR: What about 279 along with a single failure criteria and for mechanical components?

DR. LIPINKSI: Even the single failure doesn't get you there. We saw a chart --

CHAIRMAN KERR: Well, what is the there that you want to get us to?

DR. LIPINKSI: Well, even the 10 to the minus 2 -- we had a chart presented to us on diesel starts and I forgot what particular plant it was. It was at the top of that list. And the probability was almost 1 that the diesels would not start on demand.

And the single failure exists in that particular case cause there at least 2 diesels.

But the reliability per diesel is so long --

CHAIRMAN KERR: But it seems to me Walt, if neither diesel starts, that's a double failure so I see how one could satisfy the single failure criteria and have neither diesel start.

DR. LIPINSKI: But, again his overall reliability --

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The point I'm trying to make is the single failure criteria does not give you a reliability base. Because I can give you the probability of failure to start as about 5 9's.

MR. THADANI: I think, if I may just make one comment. On di[illegible]sels you need a whole abundant of close appropriate power sources and so on before you can get the diesels g[illegible].

There maybe a common faults. For example, common power faults which if it fails could possibly result in loss of diesels. Based on some of the reliability studies that have been made so far, and I believe there is guide on the kinds of testing that's required of diesels.

You would probably not achieve 10 to the minus 3 for diesel. Or 1 out of 2 diesels. At least that's what my understanding of the data is.

On the other hand, your new diesels only for an event like loss of outside power. Loss of outside power is an initiating event. It's a low probability event followed by a very low probability failure per schram.

We made numerical assessment as you will -- we found that that number always -- somewhere between 10 to the minus 5 and 10 to the minus 6. That is if you believe these methods, you would say the likelihood of the loss of outside power followed by failure to schram, would have frequency in the range of 10 to minus 5 and 10

Tape 2/14

to minus 6. But certainly no more than 10 to the minus 5. And for that reason and for loss of outside power then, our requirements on litigating systems are more relaxed.

And when I say our requirements, now I'm indicating to our diesel specifications. And the requirements for diesel reliability is lower than that for other litigating systems which are relied on for essentially all events. But if they challenge people to be -- pretty high in the relative sense.

DR. LIPINSKI: I will still repeat my first statement. Single failure criteria does not in itself imply a final reliability.

MR. THADANI: It does not necessarily but I think it's a good indication. I am not challenging your statement that you can't design a system which would not work on demand even though it might satisfy Sigmund Fergler.

One can and one might end up with mechanical components of such poor reliability that you maybe right. But the general industry package and the kinds of requirements we normally face and my understanding of the experience has been that continual failure requirements does give you unreliable relief and ranges from 10 to minus 2 -- to 10 to minus 4.

Now, I'm just going on the bases of the data that I've seen. Dr. Mattson?

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DR. MATTSON: Maybe we've been fighting with one another on this issue too much in the last year, last 2 years, 10 years. I think right here Walt saying he'd like a numerical criteria or some other more exacting reliability statement for some of these components or systems.

DR. LIPINSKI: Let me add a comment. You have given the option. You either say 279 or --

DR. MATTSON: Yeah. And Chuck is saying that the or is probably the way people will proceed and he's trying to defend against your argument that's it's equivalent. Maybe we ought to think for the future of not only in Atlas but in some other places about softer reliability criteria.

For example, we think it's implicit in everything we say that we want reliable equipment. Maybe we ought to start saying we want reliable equipment. And some of the things that go into making up reliable equipment are single failure criteria, other deterministic statements or one sort or another, and analyses of a system reliability. To demonstrate some level of reliability but not hold or hardened fast numerical reliability number like we are in the either or statement that we've associated with system reliability for Atlas.

We take some of the terministic things. We

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want a reliable system. We encourage people to do analyses and to think through what the unreliability might be so that as designers in designing the system and as licensening representatives and representing into the regulatory agency they have some grasp or concept of the numerical reliability of the system.

Must be some middle ground where it isn't either or, it's both.

DR. LIPINSKI: That's my point. To me 279 does not imply that you achieve your numerical numbers and I would not say or, I would say and.

DR. MATTSON: Well, why don't you help us write how you would specify to a designer that he do that kind of thing. How would you go about articulating that?

CHAIRMAN KERR: Let's leave that for a conference or something. I think the points well taken. Proceed Mr. Thadani.

MR. THADANI: Okay. I think I have summarized for you our reactions to these middles. And as I get into the rest of the presentation I'll highlight other areas.

As I said earlier towards -- later on if you desire to get into more detail discussion in specific area, we'll be glad to do that.

Based on these documents that we looked at and our prior understanding of acquivace of -- by having

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reviewed in this pre-analyses as well as our own independent hard calculations. I think we have gained reasonable understanding about the cemex to the point that we think we can probably specify the types of modifications which would deal improvements in Atlas protection.

Recognizing that there are some voids in our knowledge. Voids which particularly relate to our clients specific considerations. The capability of equipment.

I indicate our concern with influence of high pressure on primary system compliments or Electric Power Research Institute it's planning to conduct tests of valves. When I say valves I'm talking safety and relief valves on pressurizing with some consideration of that is associated with these valves. The class was suppose to cover steam to replace water. And there suppose to -- at least at this stage be able to handle pressures above 2500 pounds and they maybe able to handle pressures as high as 29 to 3,000 pounds.

It's our attention to take advantage of the results of those tests. And we're in the process of discussing with them how much more they can do to satisfy some of the accuracy considerations.

If we do end up with alternative 4A on a large number of plants, if not all. And the limit we're talking about is 3200 pounds. And I think that the capability might exist to be able to verify the functionability.

So I think there would be some information available to us over the next year. I think the first set of tests should be completed by July 1, 1981. Any additional tests, in terms of Atlas, I have at this stage no further information as to what it will take to do these tests and what kind of data we're trying to find.

But the point I'm trying to make, is that although there are some voids in our understanding, we do have a reasonably good idea of how an active event might proceed.

On the bases of these calculations, we have identified some design modifications in groups. Different groups providing different level of testing.

The first group I described earlier. 11 plants. We are still requiring that they modify there pram system so as to reduce likelihood of an actless event. The exact reduction of actless events, we do not know how to count. And I doubt very seriously if we have a good enough group to be able to do that industry wide.

We also required that analyses be provided with these early operating plants so we could determine what other modifications, if any, that we acquired off these old designs.

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The second grouping consists of what I would call the rest of the plants. We would require that all these plants implement alternative 3A with a small variation of what was alternative 3 in Volume 3. In the next 1 to 2 years. And I'll describe in a few minutes what we mean by that. What kinds of hardware modications we're talking about.

We reached a conclusion that more checking, in our opinion would be achieved, if we went for alternative 4A. We'd like for all present to be able to make a showing that they achieved that kind of level of safety. But we recognize some of the limitations that are very real on operating plants and plants that will become operational over the next 1 or 2 years. --Sisemic structures that maybe replaced -- resume capabilities that might be already be overloaded.

Our concerns would be that making drastic changes by erecting structues on -- one may be introducing some problems. And the actual reduction and risk from Atlas would not be as high as we had anticipated.

We would therefore recommend that those plants which have those kinds of limitations pursue other ways to improve the level of sen: you have to achieve over and above alternatives BA.

Now, on the example --

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CHAIRMAN KERR: Am I correct in concluding from these comments that it is your belief that your goal will be achieved by atlernate 4 and alternate 3A does not achieve but it moves one in a direction toward the achievement?

MR. THADANI: That is correct. And early implementation of Alternative 3A would permit, I believe, a proper review, consideration by various parties, consider information of possibly to rule making proceeding of the information that's available. And if indeed it is appropriate or necessary to go beyond alternative 3A and how much further should one go.

DR. MATTSON: Can I say that in a little bit different language? I think what he's saying is 3A's clearly needed for safety. Depending upon one's judgement as to what's totally required for safety, that is how safe is safe enough.

4A might be necessary. That's the judgement that's somewhat subjective. He feels more comfortable we feel more comfortable saying that the way to make that decision is through rule making and the subjective process that that involves and the --

CHAIRMAN KERR: And so you not any longer talking about -- you're not any longer talking about the original had was goal, you're now simply talking about safety?

DR. MATTSON: Yeah.

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CHAIRMAN KERR: Question? Mr. Ray.

MR. RAY: I'm a little confused. I thought that you were legislating both 3A and 4A on the part of all the plants. Do these last remarks mean that having legislated 3A and having implemented 3A, a plant can still then have the opportunity to demonstrate that that's adequate? Without having to go forward and add 4A?

MR. THADANI: Okay. OUr recommendation is that the -- if you would people. First one would be early implementation of what we call alternative 3A which would provide inproved safety. The degree of improvement would certainly vary from plant design to plant design.

Having read that decision, we would hope that the requirements to put the request to mean such as orders and that the kind of front that is described in Volume 4 so that these modifications could be implemented in a reasonably short time period which I described as 1 to 2 years.

If that is done, I think that permits people more time to deliberate somewhat on the various proposals that have been made. By ourselves, in Volume 4, by the industry in various pieces of paper that they have submitted and in Volume 4 we have also indicated that for some plants there may be a need to just be satisfied with something between alternative 3A and 4 A. And what we call

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alternative 3 1/2. We're running out of numbers.

That would be our recommendation to the Commission. And if indeed the Commission does agree that it's appropriate to go through rule making on alternative 4A requirements as well as optimization, then the comment period, I would hope, would be the time when industry would submit their view as well as the optimization studies to point out what they can actually accomplish without having to go to alternative 4A.

MR. RAY: Are you saying then that having implemented 3A, whether or not a plant must go further and I'd specifically 4A, will be determined on a plant's specific bases based on the ability of a plant to demonstrate the lack of necessity to completely implement 4A?

MR. THADANI: No. I think and it may end up that way. I don't know.

MR. RAY: There's a door open for it to end up that way.

MR. THADANI: Yes. Yes.

DR. MATTSON: I think there's a 2 step, maybe even a 3 possible outcome thing here. You go with 3A now. You say you want to conduct rule making on 4A. What you must mean there is whether 4A is necessary and if it is necessary, whether a person must comment exact conformance with whatever criteria evolved from the rule

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making or best effort conformance, with the criteria. There are several options for beyond 3A.

And the rule making is the vehicle for deciding which of those options are to remain open.

MR. THADANI: That's exactly what I was thinking.

MR. RAY: I'm afraid I don't understand what rule making means. Does this mean you'll have a rule for a specific plant that's different from a equivalent rule for another plant for that stage of the evolution of the fixes?

MR. THADANI: If the rule is hardware oriented, specifies hardware, then I can see the difficulties you are having.

CHAIRMAN KERR: Asgrim, let me try to help. Now, Jerry, the current ECCS requirement resulted from rule making. 50.46 is a set of criteria. Appendix K tells how one may met those criteria. That which is not plants specific is a result of rule making.

One would anticipate, I guess, a similiar set of criteria.

MR. RAY: Well, then do I --

DR. MATTSON: Well, let me try a better example maybe, that's more flexible than 50.46. It might be too rigid for what people might have in mind for Atlas. I'd say 50.44. In so far as it treats recombiners would

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be a more approximate past example of what's in mind here. Where it came out saying a certain capability for hydrogen control was required. New designs and another method of coping with hydrogen was allowable for old designs.

50.46 didn't allow that flexibility.

CHAIRMAN KERR: I was simply trying to judge what might happen on the bases of what I see in Volume 4, on the bases of Volume 4. I would have anticipated that it might be close to the 50.46 but that's just a matter of judgement, I guess.

MR. RAY: Let me give you now my return interpretation of what I just heard. I doubted that the rule making will establish criteria rather than hardware or equipment. And that different plants may met this criteria by adding different components of equipment or systems and be acceptable to the Commission and staff.

MR. THADANI: I would hope that the rule making and the proponent rule would be of a nature that would specify the necessary hardware to meet safety objectives. It may turn out that in some cases the plant owners may opt not to implement that hardware; that they may find an alternative way which will provide significant improvement because of the specific plant layout considerations. That kind of information, we would hope, would be provided during the comment period to the rule.

Having received those comments, I would hope that the final rule that is issued would still be more or less essentially hardware oriented. It would not require continued

** analyses of ATWS events as is required for local. It would specify hardware. That is our hope, and that is our objective.

Now, I do see a possible problem, as you I think correctly point out; that unless the optimization studies are provided well in advance before the rule becomes effective, you may end up with a rule which is more criteria oriented, rather than hardware oriented.

Okay. I keep promising I'll talk about some of the hardware differences.

CHAIRMAN KERR: Mr. Thadani, let me, if I may prevent your talking about it for another couple of minutes. On page 63 of the blue back report -- it probably is the same page number on the earlier version -- under "conclusions" there are some estimates of risk reduction, V and VI. Here are the estimates: that alternative 3A would decrease average risk by a factor of 20 for BWR's, two or more for CE, and two or 80 for Westinghouse, depending on the particular plant; that further, going from 3A to 4A would decrease ATWS risk of about an additional factor of 10 for BWR, 25 for B&W, CE, and there would be no change in risk reduction for Westinghouse plants going from 3A to 4A. Is that still -- the Staff position has not changed --

MR. THADANI: No, that's still our belief, based on -- as I indicated earlier -- the recognition of uncertainties, and the best calculation of probability. But in terms of relative improvements, we still believe that.

CHAIRMAN KERR: Okay. In terms, then, of, say, risk reduction for PWR's, aside from the Westinghouse plants where the Scram mitigating systems are not now diverse, risk reduction is expected to be around the factor of 2 or 3, going from now to 3A, I gather.

MR. THADANI: That would be -- if you do not have diverse mitigating systems, the risk reduction may be only a factor of 2; but if you do have diverse mitigating systems, then the reduction in risk would be considerable. And we indicated a factor of 80. Today's plants, where they do not have diverse accuation of auxilliary feed water, diverse means to trip the turbine.

CHAIRMAN KERR: I think I understand that. I'm saying, ignoring that --

MR. THADANI: Okay.

CHAIRMAN KERR: -- assuming we're talking only about plants that have that diverse capability, I seem to see that risk reduction factor of PWR's is expected to be about the factor of 2, going from existing to 3A. Is that a correct interpretation of what --

MR. THADANI: That is correct. Yes.

CHAIRMAN KERR: Okay. Now, did the Staff make any estimate of the uncertainty which it would attribute to factor 2? I raise the question because a factor of 2, when one is talking about low risk, is a pretty small factor, and I just wondered --

MR. THADANI: No. We've had a lot of difficulty, as you well know, in trying to come up with uncertainties in these calculations. We recognize -- and I hope we said two or more -- yes, we did -- we recognize that the unreliability of Scram system may be reduced by a factor much greater than 2 by making the kinds of modifications that we've been talking about. But how much more beyond a factor of 2 we don't know, and we just at this stage don't --

CHAIRMAN KERR: No. But you see, I'm also worried --

we're talking about factors -- it doesn't take very much uncertainty on a factor of 2 to make the improvement less than 1. And, you know, there is some possibility that if you make changes in a region in which the improvement is expected to be 2, it doesn't make a very big uncertainty. Now, I'm not trying to play games with numbers here. I really have some concern about making changes. And if I can assure myself that the factor is likely to be 10 or 100 with an uncertainty of maybe a factor of 2, I will feel so much uncertainty. But if I'm trying on a factor of 2 and there's an uncertainty that might take me into the .5 or 5, then I begin to have concern.

MR. THADANI: Okay. I think I understand the question you're asking. The problem is not -- at least, my point of view is not as much of a concern, simply because the factor of 2 reduction is based on simple considerations, if you will. We do believe that making these changes in the Scram system -- when I say Scram system I am at this point just talking about the electrical portion -- that the unreliability of the electrical portion of the Scram system has been substantially reduced, not by a factor of 2. But that is just a part of the total system.

CHAIRMAN KERR: Well then, our concern is not with the risk of Scram electrical systems. It's with the risk of something or other--core melts, I guess -- that, at least, was the original goal. Now the goal may be just to make -- but I assume this factor of 2 refers to some original goal of core melts or something. The factor of 2 risk reduction in a situation in which risk reduction -- the risk is already comparatively low -- I don't know what comparatively low means, but the factor of 2 is -- well, maybe significant or not.

MR. THADANI: At least we don't think that a factor of 2 is significant reduction. Actual reduction may be somewhat better than factor of 2. We don't know. And I don't think that uncertainty in that factor of 2 should be a problem, because the reason you end up with a factor of 2 is not due to the changes in the electrical system, but it's because of the limitations of the hydraulic and the mechanical portion of the Scram system.

And indeed, you're right when you talk about a factor of 2, you're talking about a factor of 2 reduction in what I would call potential for core melt. I'm not sure it is necessarily a core melt. We are talking about for these designs very high pressures, and we're getting areas which I don't believe are completely understood; a phenomenon that people have indicated to us that raise more concerns rather than resolving the problems. And I guess I would prefer to characterize it as a factor of 2 or more reduction and the potential for core melt, rather than --

CHAIRMAN KERR: You use the term "risk." Risk, to me, doesn't mean certainty. And indeed, that's the language that's used in the report.

MR. THADANI: Yes. Well, their risk -- I would say a risk term is probably not too bad. It's reasonable. But the key point here that was being addressed was, the front end portion of the risk component -- that is, the probability of an accident, and not the consequences. But I'm saying that's implied.

DR. MARK: Is it -- it's my impression that in those numbers, which are hard to come by, I know, for the improvements realized by 3A, those are mostly in the probability term of risk; whereas the 4 includes rather more in the consequence term.

MR. THADANI: That is correct. And there are some other factors from consequences, in particular for boiling water reactor designs, and to a certain extent for Westinghouse designs also.

CHAIRMAN KERR: Mr. Thadani, I gather this is a transition point. I'm going to declare a ten-minute break.

(Whereupon, the meeting was continued at 10:25.)

CHAIRMAN KERR: Mr. Thadani, would you please proceed?

MR. THADANI: Dr. Kerr, I thought I would take a minute or two to hopefully indicate to you why in my thinking your concern is probably not as serious as it might appear.

(Whereupon, he proceeded to the blackboard.)

MR. THADANI: I, during the break, put up a simplistic approach to what the Scram system might consist of. There is unreliability of, what I call, a total Scram system. I'll say it's composed of two parts. If one or the other fails, I have a failure of the Scram situation. And I split it up into two parts. One part I call the active protection system or the electrical portion of the total Scram system. And the other part is the hydraulic and the mechanical portion of the Scram system.

Here is a big liberty taken. I said, okay, if I were to split these in two halves, if you will, the contribution to overall unreliability of Scram system is distributed equally between these two systems. And all I'm doing is working on that system, when I talk about modifications in the Scram system, or the actual protection system, then I don't think you can see this -- I might write this up here. Here is my initial value of unreliability, which is one-half X. Having made this modification, some of the industry calculations indicate that reduction in unreliability is much -- well, at least a factor of 10. And they seem to indicate that that factor is even lower than one-tenth.

Thus, this is now the new unreliability of the protection system, and that becomes .05 X instead of .5 X, if you will; and that is again the conservative views of one-tenth improvement. This 10 becomes .05 X. But this one-half X remains unchanged. Even if I have some uncertainty ban on this number, it does not influence the second count. I hope I helped.

CHAIRMAN KERR: Well, I think I understood that, but it's a good illustration, Mr. Thadani. I still find myself with a risk reduction of only, in effect, I guess -- I have risk reduction of only a factor of 2 for a risk that I think is low. And I also am not certain that I know what may have been done to the plant in the process of making changes which are different than those changes that one makes on a blackboard. They involve going in and installing hardware in a plant that is already operating. I'm not certain that I know what may have been done to the plant in the process of these changes that may increase the risk somewhat. So, only a factor reduction of risk by a factor of 2 for a risk that is already low, is the uncertainty of -- the possibility of increasing risk is there. I have to try to balance these, one against the other.

MR. MATTSON: Bill, I think I understand your concern, but haven't we addressed that concern? Didn't we have it on pumps on, pumps off? I think we have it on ours.

CHAIRMAN KERR: Well, I've seen it addressed on risk reduction, but I have not seen anything in ATWS that gives me an estimate of the possible deleterious effects that may occur in the process of attempting to implement off 3A. We have to go in --

MR. MATTSON: Well, not specific to 3A, but there has been a concern, at least for the last three or four years on ATWS's fixes. And people have addressed potential decrements to safety. We've discussed them with this subcommittee.

CHAIRMAN KERR: I express my question in terms of the uncertainty and risk reduction, and I don't see it addressed at that point.

MR. MATTSON: How would we address the point other than --

CHAIRMAN KERR: I don't know. That's -- I wish I could tell you how to do it, except to say that I think you need to be aware of it. And if the risk reduction is only a factor of 2, and if you aren't too sure about the factor of 2, it becomes more of a concern to me than if I say due to risk reduction with a factor of 100, then the uncertainty was between 70 and 120.

MR. MATTSON: You're talking about what maybe our general ethic ought to be for safety improvements. If we can only get a factor of 2 out of some big change, and if the big change disrupts a lot of things that are there for 20 years worth of history, then we ought to be careful doing something that only gets a factor of 2.

CHAIRMAN KERR: And if the factor of 2 still has some -- if the factor of 2 still has some uncertainty.

MR. THADANI: Yes. We tried to indicate, and that's why I went back and made the point that we said the factor of 2 or more improvement. And we think if we have erred, we have erred on the side of conservative.

CHAIRMAN KERR: So you're saying it really ought to be a factor of maybe 10, with an uncertainty band that goes from 2 to something else. 2 is the lower level.

MR. THADANI: We believe so, because of the general concensus, I think, that the overall unreliability is probably dominated by electrical portion.

Okay. Now on to the specific alternatives and how they defer from volume III. The alternative 2 in volume III and 2A in volume IV are essentially identical. There are two changes. I would characterize one change as significant. The other one was implicit in volume III, although it was not explicitely called out. In general alternative 2 considers modifying Scram systems to make improvements in the electrical portion of the Scram system. And these are called BUSS for B&W, Back-Up Scram System; SPS, the Supplementary Protection System for combustion; ARI, Alternate Rod Injection system for BWR's. AMSAC, as you remember, stands for ATWS Mitigating System Actuation Circuitry; that is, things like turbine trip and aux feed actuation.

There is, if you will notice, a difference between 2 and 2A for B&W and CE; that is, it requires analyses. Now, Volume III required analyses also. It just did not specify what kinds of analyses and when these analyses were to be provided. So I don't consider that as a difference between Volume III and Volume IV. It's just that we've specified now the time period when the analyses should be provided.

But there is one major difference I do want to point out, and that is in the case of Westinghouse. We have gone beyond what we say in Volume III, and we're requiring what we call modifications in Scram system to improve the electrical portion of the Scram system. This is a particularly difficult recommendation to make for us. We considered factors such as Three Mile Island, considered unknowns that accidents may proceed in a path which may be different than what we perceive today. We do know there have been some problems with some of the breaker operations in the Westinghouse design, the protection system. When I say problems, I'm talking about random failures; that there are two breakers in series. Both of them have to fail to result in failure to Scram.

One possibility we were considering was possibility of having two highly reliable breakers, but one manufactured by A, while the second breaker is manufactured by B, to introduce some kind of diversity without losing reliability. That's an example. I'm not suggesting that that's what we're asking them to do; only we would ask them to look at their protection system, just as B&W and CE, as well as GE has done to determine the kinds of changes which would reduce the liklihood of failure of a protection system to function on demand.

There were considerations like Three Mile Island, like the realization that there are some ATWS events which would result in consequences far beyond those considered in these evaluations. That is because we have been looking at ATWS in reasonably realistic manner. Most of the parameters are to take nominal values with the exception of the moderator temperature coefficient. The codes are supposed to be realistic.

So, there is certainly a potential that the actual consequences for certain periods of times, if an

ATWS were to occur in a Westinghouse designed plant, maybe more severe than those considered --

CHAIRMAN KERR: Mr. Thadani, again, I don't want to make a big part, and I certainly don't disagree with you, but on page 13 of the report there is a list of five items which are said by the report to be exceptions to nominal values of system parameters. I assume, therefore, these are meant to be conservatisms.

MR. THADANI: These are meant to be conservatisms. Certainly for -- I would call alternate four plants. The reason I say alternate four plants is because there are words like assumption of failures in mitigating systems, which we're not addressing under alternate 2.

CHAIRMAN KERR: Okay. So -- okay. We're not just talking about alternate 2, not the spectrum of alternates.

MR. THADANI: Not right now, no. No. When we get to alternate 4 your comment would be perfectly applicable.

CHAIRMAN KERR: Okay.

MR. THADANI: But it was a very difficult recommendation, and we would particularly like to have your advice on appropriateness of requiring this modification on Westinghouse designed plants, especially considering that these plants, at least based on our understanding today, can withstand most of the ATSW events.

I did not go into some of the other factors on BWR simply because I thought we had covered these in the past. SD is Scram Discharge volume modifications, and there the requirement is, there are level sensors which indicate the amount of water in this volume. If there is a lot of water there, and you need to Scram the reactor, you may experience some difficulties. We have suggested that there be some diverse level sensing devices. And next item, RPT is of course the recirculation pump trip. No words are needed for that.

LOGIC is the potential changes in set points such that a large number of transients do not end up being isolation type transients in BWR's, since isolation type transients are the most serious transients in BWR's. We're hoping that these logic changes would reduce the number of times you isolate the reactor, and hence challenge the safety relief valves; as well as the chnage would permit being able to continue to use the condensor by means of running back on feed water pumps to reduce sub-cooling, and at the same time have some high pressure inventory source available.

These were characterized in Volume III as alternate 2, and as I say, the difference really is that now we've given date to when we would like to get the analyses for these 11 old plants.

Going on to alternatives 3 and 3A, you'll notice that they're basically the same as alternative 2. The distinction between alternate 2 and 3 was that analyses were required for PWR's to demonstrate adequacy of these hardware modifications. There are two new requirements that have resulted from the information that we've seen on PWR's. And this may turn out to be not the case for all PWR's. Two new requirements are, containment isolation.

Most of the radiological assessments assume that the containments were isolated. And what we want to make sure is indeed the containment is isolated fairly early during an ATWS event; serves two purposes. One, it cuts down on our arguments on faction of fuel failure, as well as some of the assumptions that go into performing radiological dose calculations.

It also would provide additional benefits in that it may limit consequences to well below part 100. It's helpful from two points of view. Not only would it make it easier for us to agree that we have satisfied our criterion, but in reality it may limit the actual off-site releases to much lower levels than part 100 guideline values. It may be that on the large number of plants this diverse containment isolation already may be in place. It's our understanding that a number of plants have, for example, isolation on high radiation signals, and there may be other signals which would isolate containment. That's a plant unique feature, and it may turn out that a large number of plants already have this feature and do not have to make any modifications.

DR. MARK: Is that not also being addressed in the action plants?

MR. THADANI: It's being addressed in the action plants. In response to a question from Dr. Kerr, I was going to try to indicate that there is a real need for us to take what we think is required for ATWS and go to the action plant and make sure that we get multiple benefits from one change. And I think there are areas, and I can actually give you some examples later on.

DR. MARK: I have a question which is called to mind by something you just said, talking of the radiological consequences. I guess I don't understand why, in connection with an ATWS study, it is necessary to take those studies any further than release from containment; because the rest of it is now the -- I forget the name of the code that one uses to carry the radioactivity from here to there. But it's absolutely --

MR. THADANI: Are you talking about plural or saracode?

DR. MARK: Crack.

MR. THADANI: Oh, crack. Okay, I'm sorry.

DR. MARK: It's absolutely common to all kinds of releases. And why your people bothered to have to say it's only 18.9 rem to the thyroid under conditions that nobody that I can think of could possibly believe the calculation is made. Why not just limit these studies at so many curious which is acceptable, more which is not. If it's plant specific, then that's, of course, marvelous, because I put my plants so far away that I don't need to observe any of these things, and get radiological consequences which are acceptable.

MR. THADANI: I think that the approach we're proposing would also get us away from a lot of calculations; because with the fairly simple releases, release fractions, the amount of coolant that gets out in the containment, the leakage from primary to secondary in the pressurized water reactor, you have to worry about simply because that -- the steam leaving the steam generators is going to the environment directly, because the safety valves are open. The pressure is up, and the steam generator safety valves blow directly to the environment. So you do have to consider that aspect.

DR. MARK: I'd be content to leave it, however. It's released to the atmosphere. I'd like to know how many curies. And then I don't believe what you say about it after that anyway, or what Crack says about it, rather.

MR. THADANI: But that's essentially what you're doing when you're talking about part 100. If you take a specific high over Q or a plant, that's all you're doing, is you're saying X number of curies getting out, which would be consistent with the guideline value for part 100. I think that's what you're doing. What you're accomplishing, I believe, is that -- okay, that's X number of curies get out, but by early containment isolation, in all liklihood what gets out is going to be much smaller fraction than that X.

DR. MARK: Fine. I'm just referring to the fact that somewhere in this 460.3 is a list of dose numbers which somebody evidently had to work out in order to meet the requirements. And I would have been happier had you just settled so many curies, and that's all right.

MR. THADANI: I really think in the final process that's what we did. We were just looking at various pathways. There may be a large amount of activity in the containment, but our concern was how much of it was getting out. And so we had to look at the pathway. And I think that's where the biggest problem comes in in calculations, the kinds of assumptions you use, and what's leaking through various seals.

DR. MARK: No, I follow that, and I concur you have to do that. It was the taking it from here, now, to the boundary of the plant in some case or other, which is affected and has to be done before you can talk about rems to the curies and rems to the bone.

MR. THADANI: Yes.

DR. MARK: And I'm wondering why that step is even thought of in connection with an ATWS discussion.

MR. THADANI: I guess the only way I can answer that is to say because of the dispersion factor considerations, they may be different for different plants. But -- because part 100, number of curies may vary --

DR. MARK: Yes, but I think you're not expecting to allow the ATWS fixes, whatever they ultimately are, to be plant specific -- site specific, rather.

MR. THADANI: That's right. Again, I think that what containment isolation does is it says with a fairly conservative site dispersion factor, I can still have reasonable confidence that I'm not going to exceed part 100 values. And in reality, you probably are not going to exceed a small part of part 100 values.

Okay, the second change is the capability of the instrumentation to withstand ATWS conditions. This is, if you notice it's particularly true on pressurized water reactors where the calculated pressures are far in excess of the normal limits that are applied for over pressure events. Normal limit applied is 110 percent of the design, which works out to about 2750 PSI as the limit that is not to be exceeded for anticipated transients. As you know, for ATWS, we're talking about 3200 and in some cases possibly above 3200 pounds peak pressure. We have some information from combustion engineering, which seems to indicate that we may lose a significant number of instruments because of these high pressures. And the requirement here is to make sure that the instruments that are relied on by the operator to shut the plant down would indeed be functional following an ATWS event. This was implicit in Volume III, where you looked at the requirements for mitigating systems, that the systems be able to withstand ATSW conditions. But because of what we saw in combustion report, we thought it was very important to highlight it, because it could mean some changes or additional hardware modifications. And it was for that reason we specifically identified this item.

In terms of -- that's generally true, if you notice, of all PWR designs. And as far as boiling water reactor design is concerned, we did not bring up the question of instrumentation because the peak pressure that is experienced during an ATWS event is not that much above the pressures that are calculated for anticipated transients.

We feel reasonably confident that ATWS does not impose conditions which are significantly more severe than the conditions these instruments are expected to see during transients as they are analyzed in safety analysis reports with conservative assumptions.

But containment isolation, of course, provides the same benefits that I described for pressurized water reactors. And for that reason we have included ability to isolate containment fairly early following the onset of an ATWS event.

CHAIRMAN KERR: In some situations it seems to me I've seen the statement, early isolation following an ATWS event; and in another context, early isolation following fuel damage. Are these used synonymously?

MR. THADANI: The concept -- actually a better way to say it is to say early containment isolation following fuel failures. If you don't have fuel failures, it's still probably a good idea to be able to isolate containment. But the problem becomes serious after you have fuel failures.

CHAIRMAN KERR: Is the implication then that one will isolate on a signal which measures radiation somewhere?

MR. THADANI: That's the thought that we had, because we thought that that kind of circuitry might already exist in a number of plants.

CHAIRMAN KERR: NOw, why is this significantly different from the isolation requirement that you'd have for a locus?

MR. THADANI: I can't think of a very good reason why it should be any different? Now, I do know that sometime back containment isolation was based only on two types of signals. There may be others, as I said, high radiation is one -- two types of signals. One was the safety injection actuation signal. On some plants, that signal alone would isolate containment, as well as initiating the high pressure safety injection pumps. And another signal that was used to isolate containment was high containment pressure, which takes some time before you get there in most cases.

In the past, I think we always looked at large locus. I think our attention was focused on large locus and not small locus. In large locus you got the high containment pressure very rapidly. And so you got early containment isolation. Three Mile Island has taught us that for small locus, you may not get high containment pressure.

CHAIRMAN KERR: Well, perhaps my question then should have been, given the experience of today, is this containment isolation requirement different from the one that is likely to be associated with a loca?

MR. THADANI: I would guess -- it would be my judgment they would be about the same, because the objective is identical.

CHAIRMAN KERR: It would make some sense if this becomes a requirement to try to integrate it with whatever requirements may be associated with loca.

MR. THADANI: Absolutely correct. And as I indicated earlier, it's -- we would like to go back and compare these requirements with whatever is in the action plants to see what we can do.

Tape 4

Okay, next one I'm going to compare the alternative 4 and 4A requirements and how they differ. And the key point here, you'll notice alternative 4 did not have any requirement for BUSS, or SPS -- that is modifications in the Scram system -- because alternative 4 said mitigation is the way to go, and highly reliable systems which mitigate the consequences of ATWS events were preferable because we did not know how much of an improvement these modifications and electrical portions would offer.

The specified hardware was based on the analyses that we had seen. We, of course, recognized then and now that there may be alternative ways of getting there without having to necessarily impose hardware changes as specified here. The advantage of, of course, specifying hardware is that you get away from the kind of rule that we have today, BCCS rule, analysis after each reload, constant discussions and sometimes disagreements over what may turn out to be not very important safety considerations, but they are legal considerations.

Our hope was, and still is, to have a rule which would be hardware oriented. And in order to do that, you have to be able to specify the kind of hardward you think would provide the level of safety that's desired. We thought the modifications that we identified under alternate 4 would indeed provide the necessary protection.

These differ from alternate 4 to 4A, I would address in terms of containment isolation, instrumentation. The other major point, of course, is the requirement that we should also try to do whatever we can to prevent accidents. This is the result of, I believe, what we learned from Three Mile Island. And as I said earlier, this is one area where we would certainly like to get advice from the Committee as to its appropriateness, considering that alternate 4 itself was supposed to have provided sufficient mitigation capabilities.

The key point besides containment isolation and instrumentation is what's OPT, which stands for Optimization.

CHAIRMAN KERR: May I comment; that the Three Mile Island experience is important. I am not sure why one appeals to the Three Mile Island, however, as a motivation for trying to prevent accidents; because it seems to me that the NRC staff has for a long time tried to prevent accidents. Nor do I understand why in a number of places in this report I find the statement that the experience of Three Mile Island demonstrates that plants do not always behave as they're calculated to behave. I had thought that this was engrained in people who had operating experience. But things do not always behave as they were calculated to behave. And if this is a new lesson based on Three Mile Island, then it's high time, I guess. But I am surprised that it took Three Mile Island to teach people that. Or that one draws any new lesson from it.

You know, I almost get the impression that one is saying, since plants don't behave the way they're calculated to behave, we might as well quit calculating and build plants so that they'll always behave the way we expect them to behave. It also turns out that plants don't behave the way they're built to behave, I think.

MR. THADANI: I think you're right, and I guess there's a difference of perception. If you have an accident like at ATWS, you recognize the potential severity of consequences. And you look at Three Mile Island. It started out with a transient, and ended up with a small loca. The procedures in terms of throttling HPI, and when he throttled HPI, and his lack of recognition of the role of pressurizer level --

CHAIRMAN KERR: You surely aren't trying to convince me that there were surprises at Three Mile Island.

MR. THADANI: I'm trying to point out --

CHAIRMAN KERR: I'm convinced.

MR. THADANI: I'm sure you are, as well as I suspect most people in this room. All I'm trying to point out is that --

CHAIRMAN KERR: What I'm not convinced of is that this is the first time that one ever encountered a surprise in terms of the way plants behave.

MR. MATTSON: Why don't you stipulate the answer?

MR. THADANI: Yes. Okay. I was going to go on to optimization. What we mean by that, we would still like to stay with alternate 4A with as many plants as are practicable. We would recognize that in some cases it may not be practical or advisable to make the kinds of modifications that are described under alternate 4A. We have had some discussions with the industry, both some PWR industry, as well as the BWR industry. We have explored thoughts, say, okay, I can only get 86 GPM of SLCS, but can I change the poison concentraion, or the boron concentration so that I'm more effective, and I turn it on earlier? Can I rely on off-site power to pump some of the pumps? Can I modify my lattice design, or my burnable poison concentrations to end up with more favorable temperature coefficients? It may be that there are other things that could be done more easily on plants which are limited by considerations of layout. Yes, sir?

DR. MARK: In the boran used in the -- whatever that set of initials is -- SLCS -- what's the enrichment of boran 10 in that?

MR. THADANI: It's 13 percent by weight solution. I used to know PPM. I've forgotten.

DR. MARK: The enrichment of the boran 10 isotope in the boran?

CHAIRMAN KERR: Is it natural boran, or is it abridged?

MR. THADANI: Oh, I think it's natural boran.

DR. MARK: Why on earth, then, does one not raise the question of going to highly enriched boran 10? 43 gallons would then mean twice as much as now.

MR. THADANI: That's exactly what I'm talking about, highly enriched boran, boran 10. I think you're right, and to the best of my knowledge GE has been exploring that possibility with the manufacture of boran 10 in this country. They may be better able to tell you some of the difficulties of getting sufficient quantities of boran 10 in time periods.

DR. MARK: Boran 10 is one of the easiest isotopes to get separated. It was done very quickly, then the pack was thrown away because it made all the boran 10 anybody wanted.

MR. THADANI: Yes. . think that -- well, GE may be able to tell you more tha. I can as to its availability. But that is one aspect that they are considering, and we recommended things like, as you said, boran 10, or gatalanium. This is a kind of concept,optimization; let's not stick with the sort of thinking we have employed in the past of just specifying A, B, C, D, E type of hardware, and saying that, uh-oh, plants X, Y, Z cannot implement A, B, C, D, E. Therefore, we can't do very much. I think -- and I would hope -- that there are ways that we could provide protection beyond alternate 3A without resorting to any serious ripping out of structures, and so on.

CHAIRMAN KERR: Well, one could translate optimization as clever design. Maybe?

MR. THADANI: It's clever design, yes, I think, where you can find minimize impact, economic penalty, and see how much benefit you can get as far as ATWS is concerned, without, obviously, affecting other considerations of safety.

DR. MARK: In the report I was fascinated to find in connection with just this, that Staff proposed to reward the industry. Were you going to give them golden fleece, or what?

DR. THADANI: I'll fess up to it, that that word was not in the initial draft of the report. Might not have been a very good choice of words, I think.

But as long as we get the thought across, that plants that are operating today, maybe there are other things we can do to get protection, not just for ATWS. And maybe these things would help for other events and accidents. Okay.

CHAIRMAN KERR: Was there some reason for choosing 99 percent temperature coefficient? Or is 99 just better than 95?

MR. THADANI: Initially we picked 99 percent temperature coefficient because we realized that the plants were, at the time we thought, overly sensitive to the temperature coefficient initial value. And we also had some judgmental concern that we were using nominal values for all parameters; that there were some probability factors associated which would cause the plant to be in non-nominal condition. And at that time we thought picking 99 percent MTC value was reasonable in that it would probably cover some unknowns in values of some of the nominal -- other nominal -- parameters; as well as there is an uncertainty in these calculations. The uncertainty, I think, Dr. Richings has indicated to you in the past, is on the order of 1 to 2 PCM. If you include the uncertainty you're saying MTC could be on the order of 90 percent or so. And if you tacked on the uncertainty, you get something like 99 percent. And then subsequently we perform what we called a simplified statistical study wherein we decided to pick a small number of parameters. We picked about seven parameters, and we set up a factorial experiment. We met with renders and made some judgments on the distributions associated with these parameters, and did a monte carlo calculation. And we found that by having gone bo a 95-99 percentile kind of value on MTC, it turned out that there were sufficient influence from other parameters such that roughly, if I remember correctly, 25 to 30 percent of the time the actual pressure would be higher in this probability density function -- would be higher than that would be calculated with the prescription that we had provided to the industry.

So, that, I think, more or less convinced us that 99 percent on MTC did not mean the plant would exceed that calculated pressure only one percent of the time. It turns out it would exceed that calculated pressure quite a bit more -- it's quite a bit more likely that it'll exceed that pressure than .01.

It was a change from past practices, wherein the temperature coefficient that's used in transient analysis is zero, which is, I believe, the tech specs requirement before you go up in par, temperature coefficient has to be zero. And we felt it was unreasonable to use a zero for ATWS conditions, but the value we picked then was purely based on judgment and some understanding.

CHAIRMAN KERR: I would guess that there would be a lot more uncertainty in your specifications at 99 percent then there would be in the 90 percent, for example.

MR. THADANI: I would think that would be true.

CHAIRMAN KERR: You indicated, for example, it might well be a factor of 10 uncertainty on the 99 percent.

MR. THADANI: I think you're right. I think it would be more reasonable to have higher uncertainty in a smaller area.

Okay. I have -- actually, the next slide, I'll put it up, but I think I have already discussed it. It's a very subjective view of looking at these alternatives and seeing the kinds of improvements, benefits, if you will, that these alternatives yield. I have another slide which I'll put up in a minute, that does get into what you referenced earlier, Dr. Kerr; relative improvements from various alternatives and numerical terms.

I do have a slide recognizing, of course, that are qualified that these calculations may have large uncertainties is always there. Here, alternative 3 1/2 is really what I've been talking about in terms of optimization. It may turn out that we can't get all the way to 4A. We can get substantially beyond 3A, or reasonably beyond 3A. The key words, I think, are maximum practical ATWS mitigation. And I have repeated myself many times today. That means, don't rip out structures that are there today. Think of other ways to provide improvement. If you can't get there all the way, let's see how far we can get.

The rest of the stuff is -- I've addressed in one form or another. I'd just like to make the point that alternate 4 does provide high reliability in the sense that you do consider single failures, and you have more stringent requirements on the designs of mitigating systems.

We would be concerned about some systems that we're relying on, and I can give you an example. In BWR design, under alternate 3A, you have to rely on high pressure coolant injection system, HPCI. The reliability of HPCI, based on our experience so far, has been less than satisfactory. Of course, we recognize this is a low probability event, but on the other hand, if HPCI is not available, we're concerned that you're moving into a situation that would most likely result in core melt. And for consequences like that, we would like to find ways to improve reliability of those critical systems. And maybe there are some ways we have not -- we, NRC -- have not thought of that the industry may know of, or may have some recommendations on.

We can take care of the large boran system possibly by going to boran 10 and having and maintaining a smaller system.

CHAIRMAN KERR: Mr. Thadani, when you refer to a situation which there is a consequence of core melt, I agree that that's a serious consideration. Have you ever attempted, or do you plant to attempt, to put this in the context in which one says here are all the things that we know of that contribute to core melt, and this is some fraction of that total contribution. It seems to me that's fairly important in determining where one commits one's resources, because we have other things that are unfixed that are much larger contributors; then that maybe makes us take one attitude toward this. If this turns out to be a large contributor, this means that it requires a good bit or priority, and it may not be possible to do this.

MR. THADANI: I think you're absolutely right. One should do that. Roger wants to address this.

MR. MATTSON: We did that in draft -- in Volume I of this thing, where we said -- we were talking about 10 to the minus 6 goal for ATWS. We had a rationale. I'm not sure it was in Volume I, or it was in some slides that we used --

MR. THADANI: No, no. That's exactly --

MR. MATTSON: We said, assume there were 10 dominant contributors to core melt, and that the reactor safety study was right with its central value estimate of 5 times 10 to minus fifth, or 10 to minus fifth. But the reactor safety study only identified for five dominant contributors, so that leaves you some room for uncertainty, the difference between 5 and 10 dominant contributors. And on that kind of logic then you say you don't want any single contributor to be greater than 10 to the minus sixth. And it was in the context of other contributors for core melt risk that the 10 to the minus sixth value was arrived at for an ATWS goal, as opposed to the previous 10 to minus seventh ATWS goal.

CHAIRMAN KERR: Using this chain of logic, which is okay, not to talk about ATWS generally, but to talk about a specific attributor to ATWS, unreliability of HPCI. Granted, this is part of ATWS, but I don't know how important it looms in the total picture. That's all I'm saying. And I think, as you try to decide among the things that you do, if you can readily.

MR. MATTSON: You could do it either at the lower level, or you could mix levels. You could talk about how improvements to HPCI might cut into other contributors to core melt probability.

MR. THADANI: Yes, I think that's exactly what I was going to get to. As Roger correctly points out, at least we did go back to the reactor safety study in Volumes I and II, proposed rationale for chaining from 10 to the minus 7 safety goal to 10 to the minus 6, based on reactor safety study as well as our understanding of the number of plants that we're going to be operating in the year 2000, as against what WASH 1270 had considered.

CHAIRMAN KERR: I think you also manage to watch the people who had been associated with WASH 1400; that your evaluation of the contribution of ATWS to core melt was a valid one in light of the additional information, didn't you?

MR. THADANI: That's correct. That is correct. We -- I think that the research organization and ourselves are -- have a joint understanding of ATWS contribution. I was going to go into -- since you brought up the contribution of HPCI, it may be quite high as far as ATWS risk is concerned. That's one example. There are other areas one could talk about. But I think that's very important also in other areas. If you look at WASH 1400, major contribution to risk in WASH 1400 was from transients and ATWS. And if you make improvements in HPCI reliability, RCIC reliability, these are the two high pressure make-up systems in BWR 4 designs and a number of earlier designs. I believe that you have not only reduced ATWS's risk, but you've also made improvements in terms of risk from transients followed by Scram action. But something else goes wrong. And I think you have made improvements in other areas. While I can't think of the numbers at this time, but it was approximately 50-50 transient contribution and ATWS contribution.

Loca were a small factor on boiling water reactors. I made some points here I thought were useful points: alternate 4A would mitigate most of the ATWS events; it relies on highly reliable systems; the reliance on operator is somewhat reduced because -- an example, peak pressure under alternate 4A would be lower than that would have been the case for alternate 3. The conditions of the equipment would have been less satisfactory under alternate 3A as compared to 4A. The potential for steam generated tube damage would have been higher under alternate 3A as against alternate 4A, simply because the delta P is significantly higher under alternate 3A, delta P meaning the pressure inside the steam generator tubes, versus

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pressure on the shell side of the steam generator.

Instrument capability would be expected to be better at lower peak pressures. And this, in turn, would give the operator better information. Timing also is, I believe, less critical under alternate 4A. And this becomes more important, in my opinion, on boiling water reactors than on pressurized water reactors. When you start to cool the containment -- that is, higher type pumps would be in line in LPCI mode of operation following -- if an ATWS were to occur. In some plants there are limitations. They cannot switch LPCI pumps to pool cooling mode without some time period, or waiting for five minutes or so.

There are cases where you can override those delays by keys and modifying switches. But what this does, is it permits the operator much longer time period before he may initiate pool cooling, and the total containment temperatures that would be reached would be lower than 200 degrees local temperature that we've been talking about. And that in turn would make it more likely for pumps like HPCI, which would take suction from suppression pool, not to be affected by MPSH considerations. The question of qualification of HPSI pumps always has been a concern. In many ways these pumps trip, and would like to try to get reasonable assurance that the pumps would be available and pumping the water into the vessel.

These are, I think, some factors which I believe would help the operator to do his job a little bit better, give him a little more time to see what's happening. There's another item under boiling water reactors which says, eliminate oscillations. We sort of talked about oscillations, our concerns with collable geometry and their plant response because of these wide variations. What happens to control systems and how they react, and these factors, I think, would influence the operator's reaction, in some cases, probably in a non-satisfactory direction.

It may be that these oscillations may be eliminated even under alternative 3 1/2 or 3A by use of boran 10 and early actuation of poison; in which case, that thought would just be taken out of 4A; that is, you have eliminated that concern for all alternatives under serious consideration.

CHAIRMAN KERR: I gather that in analyses that you plan to require, that operator action cannot be taken until the transient is ten minutes into history, or something of this kind.

MR. THADANI: Yes. Our recommendation is that, while the operator might, when confronted with failure to Scram, might go and try to manually Scram the reactor pretty quickly, I would guess he'll probably try to do that pretty quickly. But that a credit for a correct operator reaction not be taken for ten minute. And even then, only if he has sufficient information available to him, which tells him what actions he's to take. There's no ambiguity of the information displayed to him. If they are ambiguous displays, then he has to take somewhat longer time to analyze the event and take corrective action.

We have asked the industry to develop ATWS procedures, and that would be a mechanism to determine what sort of information does he have available to him, how does that information differ from other accidents, and are his actions consistent with what he might be required to do for ATWS.

CHAIRMAN KERR: Do you anticipate then that after the procedures are developed, you may be willing to consider what he might do before ten minute? Or -- is that still an open question?

MR. THADANI: When one says ten minutes, if it means nine minutes, and we recognize the limit is, I guess, as somebody may characterize as not a hard limit. I'll give you an example. If my containment temperature in the BWR is 200 degrees farenheit, I might think that there may be some margin there in reality, but I don't have experimental data to support going beyond 200 degrees. And then, if it means going to operator action in nine minutes, for example, it would be very difficult for me, I would think, for anyone to sit back and say, if he waits till ten minutes, the peak pool temperature is going to be 205 degrees; to go on and require some hardware changes for that.

CHAIRMAN KERR: That's a good point. I had two other possible considerations in mind. I get the impression that there is going to be some effort toward better operator training and removal of ambiguity. The first question is, is that so that after the ten minutes are up the operator will know what to do? Or would he be in principle or practice expected to be doing some things before the ten minutes are up, but you're just not going to give credit for them.

MR. THADANI: In practice he may well be doing --

CHAIRMAN KERR: Will the training perhaps train on what he's supposed to do after he waits ten minute? Or will the training say there are some things that you can do maybe immediately?

MR. THADANI: The training would say that there are things you can do earlier. My personal hope would be that we institute procedures which are based on realistic analysis and not conservative analysis, and try to help the operator, give him some guidance on, here is the kind of trend the event will take subject to the actions you take at five minutes, ten minutes, 15 minutes, 20 minutes. Have him understand what's happening and what might happen in the few minutes, depending on what actions he takes. I would hope that the procedures would include operator action earlier than ten minutes. It's -- whether credit is given for that action, at this stage we've been talking about ten minutes.

CHAIRMAN KERR: Because it seems to me there is another part of this. If one assumes that the ten minute rule is an effort to take into account the fact that the operator either may not know what to do, or may do the wrong thing, or some combination thereof, there is some logic in considering prohibiting the operator from doing anything for the first ten minutes, under the assumption that there's a high probability that what he does will be wrong. I haven't looked at this, but it seems to me the same logic that leads you to the ten minute rule could also lead you possibly to that.

MR. THADANI: Yes, we have considered that factor. But on the other hand, it would seem to me that -- and I'll focus my attention on the ATWS -- if he has an ATWS event, I surely would hope, and I would encourage him, to go try to manually Scram the reactor as early as possible. I would hope, and I would encourage the person to trip the turbine if it didn't trip, in the case of a Westinghouse designed plant; because my analysis assumes that the turbine would trip because I have introduced the circuitry and so on, does not mean, it seems to me, that the operator should be prevented from making sure that action took place that was supposed to have taken place, but it didn't.

CHAIRMAN KERR: I don't quarrel with your conclusions. It seems to me this same logic could be used to arrive at something which says I'm going to give credit for operator action, because the chances are fairly good that anything they could do will be an improvement, or at least there's some probability that this occurs. So, I really -- I'm not in a position to settle the ten minute rule. I'm simply saying that it has implications in the direction which -- I mean, if it's founded on the assumption that operators make mistakes, it has implications that need fairly careful exploration in terms of trying to prevent those mistakes.

On the other hand, if a careful look at the situation convinces you that the chances are that a well-trained operator will ameliorate the situation, it seems to me that at least a realistic analysis would take that into account.

MR. THADANI: THere would be, I think, as your point verifies -- it's such a subjective consideration. Not all operators are alike. Some react much more quickly than others. Some may be able to assimmilate information, digest it, and react to it much faster than others. The ten minute rule that we've used in ATWS -- and it's been used in a few other areas in licensing -- is probably somewhat optimistic as compared to what has been done in the number of other areas in licensing. There is a plant that is a ray guide -- I forget, 660, I think -- no, it can't be 660. There's a standard 660, or some such number -- I forget -- which is supposed to look at human factors, operator machine interface, information displayed to him, what is expected of him and when. And they were supposed to have come up with the kinds of times one ought to use in relying on operator action.

As far as I know, that draft is still a draft. It's still being looked at. Not much progress has been made. I hope that, plus the research that -- NRC research office is doing in terms of human factors, operator reaction, would be utilized to develop or modify whatever positions have been taken up till now. I just can't tell you any more. I don't know.

MR. EPLER: Mr. Chairman, there is one point in this discussion that seems to be lacking. I discussed this question with an individual who gives examinations to the operators who are getting licensed, and we discussed these rare events. He was a little surprised that anybody would train an operator to take action in a low probability event, like 10 to minus 4 event; that there were so many events that were highly probable that would keep him occupied, that if you wanted it that way it would be occupying our time with these rare events. So this point seems to be missing in this discussion.

CHAIRMAN KERR: I didn't raise the rationale. My impression is that the report recommends such training. And I was trying to follow the implications of that.

MR. EPLER: Well, I think I'll still ask the question. Is such training justified?

DR. DITTO: I think the history of the ten minute rule goes back to the NC standard, I believe you cited, 660. And it related to actions in which the operator had to make a considered judgment, is where it first started out. And I don't think it meant this automatic response to an unambiguous signal, although it has been carried into the regulations, I think, in this way. But I don't think that's how it started out.

MR. THADANI: Well, the draft that I've seen talked about 30 minutes and not ten minutes. So I think that's been going through changes. Go ahead, Roger?

MR. MATTSON: I think Ep's got a good point. Let's say that we take all the operators in the country and we sensitize them to all the 10 to the minus fifth, and 10 to the minus sixth events we can think of that might melt the core; and we follow Dr. Kerr's suggestion that we try to make this realistic dependent support operator action for these low probability events as we can, which might provide stimulus to do this first thing that I suggested. I guess that means that they'll be so busy giving any offset condition, worrying about the ten to the minus fifth and ten to the minus sixth event that might get them; that they're going to fail to recognize the once per year or ten to the minus one event that's actually going on to such an extreme extent that they'll turn it into another ten to the minus sixth event that we hadn't thought of to train him beforehand like T and like 2. That seems to be counter to safety to me.

CHAIRMAN KERR: On the other hand, they might turn it into a ten to the minus fifth event that they would recognize.

MR. MATTSON: Yes.

(Whereupon, a short recess was taken at 11:40 a.m.)

(Whereupon, the meeting continued at 11:50 a.m.)

MR. THADANI: So far I've discussed the type of hardware modifications that we foresee under each of the alternatives, and the bases that went into developing those requirements.

I have actually gone through almost all of the considerations that are described here on this slide, to go to this two-step phased implementation approach that's recommended in Volume 4. The improvement in reliability, better capability to withstand ATWS much more easily be able to verify that that mitigation capability exists. We would get away from some of the big problem areas that we have seen from the analyses done under alternative 3 set of guidelines.

And I think the more important factor is that the value-impact consideration, in the sense that the schedule would permit sufficiently long time to minimize and hopefully eliminate any additional down time for making changes to, to satisfy ATWS requirements.

It was this consideration which led us to recommend alternate 3A now, alternate 4A later, because we believe that alternate 3A can be implemented over a period of about two years without impacting delays or downtime in operation and permitting sufficient time so that the Com - ission, with help from industry and other interested sources can decide if alternate 4A is indeed the right way to go.

I'm not going to repeat myself. I think I've already addressed most of these items.

CHAIRMAN KERR: Mr. Thadani, is the Staff at this point, or does it expect to be at some later point, prepared to make a recommendation as to what it would do?

I gather that what you are likely to do at this point is to recommend, or what you are doing is to recommend, implementation of 3A perhaps by order, but at any rate fairly soon, and to postpone 4A to effectively a decision-making process in which the Commission will be involved.

Is it the Staff's strong recommendation that 4A be implemented? Or is that somewhat an open question at this point? -- as far as the Staff is concerned. Or have you reached a decision?

MR. THADANI: Well, Staff's recommendation certainly would be that for new plants -- when I said "new plants," plants which would be operational maybe two, three, four years from now I might characterize as new plants -- but they all implement alternate 4A modifications and certainly satisfy the criteria that we use in alternate, under alternate 4A.

But for the rest of the plants which are pretty far along in construction stage and plants which are operating today, we would recommend that protection be achieved to a level which is as close to 4A as is practicable. And that is the so-called optimization study I was describing earlier.

So our intention would be to recommend to the Commission a proposal which would say, "Make certain minimum modifications early," and this could be done through orders. And additional modifications could be based on two factors: the plant stage of operation or construction; value impact would be the other consideration.

How much further do we want to go to reduce ATWS risk? And at what cost?

Our recommendation based on our understanding of value impact would be alternate 4A or essentially all new plants and optimization study, to make sure that we can get additional protection without significant downtime, because downtime is what really adds to large impacts and makes, if you will, the value impact ratios look unattractive.

So that, that seems to me would be a very central consideration here.

CHAIRMAN KERR: Thank you.

MR. THADANI: Here's -- I have a slide here on proposed plants and our hopes of what we wish to accomplish. As I said earlier, alternate 2A plants are required to make only electrical changes. We would hope that these changes could be implemented by July of '81. Most electrical changes by July of '81, piping changes and boiling-water reactors we hope would be implemented by July of '82. We would require design information, of which we have essentially little to be provided to us approximately December of '80, as well as the optimization study.

The reason for December '80 for optimization study would be so that that information can be taken into consideration before any rule becomes effective. And I, I, I believe that if we don't get optimization study at that time, it would be very difficult to end up with a rule which is explicitly hardware oriented.

And as you notice, the rest of the plants where we have been talking about alternate 4A, we propose that the information requirements, the degree of information requirements, be established on the basis of rule-making. We have indicated the kind of information that we think would be necessary for us to completely close off the ATWS issue.

But it may be that during the rule-making considerations there may be changes. I don't know in which direction.

But our recommendation would be to implement these changes by January of '84, which we believe would be sufficient time period for industry to make the changes; and where there are limitations of plant layout, we would certainly know, hopefully by December of '80, what kinds of alternate methods one might want to apply.

The next is the major reason we are here today.

We certainly would like to have your advice on this two-step approach that I've described and which is described in Volume 4. We'd like to have your views on appropriateness of alternative 3A as a short-term improvement and the kind of approach we are planning to take for going beyond alternative 3A requirements.

We are, it's all hoped that we'll get your letter in April, and that in May we will prepare a Commission paper, which would include proposed orders, proposed rule, and a discussion of value impact for various alternatives -- and hope that our discussions with the Commission would end in June or July, to the extent that some kinds of orders could be dispatched to require alternative 3A type of modifications on a schedule that the Commission would then make a recommendation on.

If we follow this projected plan, we would hope to have an effective ATWS rule, I would say, early '81 optimistically; it may turn out to be some time beyond early '81, depending on the kinds of comments that are received and the types of optimization studies that are provided.

But I thought it was useful to, to at least give you general feelings that we have of how to proceed from now on.

I have Paul Boehnert -- called me, I guess it was two or three days ago and raised some questions, and asked me if I would address them.

I told Paul then, after listening to his questions, I would certainly address them -- as to the extent, you can decide for yourself.

The first question was -- by the way, I also recommended to Paul that it might be useful to get industry reaction on these questions.

It, you had asked, What is the probability of control-wide insertion as a function of time following an anticipated transient?

And I guess I put down the answer: Honestly, I don't know; I don't think anyone knows. And I don't even know to go about getting it -- or attempting to get it, that is.

The second part of the same question, at least I'm hopeful that I can give you a little more information on -- that said, "Okay, what are the consequences if you delay SCRAM action by some discrete time periods?"

I think the numbers I had from Paul were 210 and 20, but I took the liberty of trying to put different time periods just to show, highlight, certain characteristics.

If you'll notice, I, I put what I thought were important parameters: design; time, and time is in minutes;

reactor power, which is after all what we're trying to reduce by inserting rods; and depending on when that action is taken, what the concern might be, if any.

Now, if the reactor is SCRAMmed at about 2 minutes, the reactor power is still 50 percent at that time; and in most PWR designs you have already gone past the major concern area, which is the peak pressure. Peak pressure ranges from roughly 40, 45 seconds for some B&W designs, on up to about 90 seconds to a hundred seconds for a Westinghouse and CE designs.

That is, at that time you have gone through this plateau of high, the highest calculated pressure. But that's not to say that SCRAMming at that time is not helpful. It's very helpful, because now you get to a situation where you can at least proceed, to a certain extent, in a normal shut-down mode. You have reduced, if you can SCRAM at 2 minutes you would have reduced the amount of coolant dis-charged to the containment. Consequently, you would have reduced the amount of voids or the void fraction in the primary system, which is certainly very helpful for the behavior of the plant.

So I would say that if you can SCRAM the reactor at 2 minutes or after, that certainly helps you very much in terms of long-term shutdown. That makes the job of the operator, I think, a lot easier; but still does not get you away from these peak calculated pressures, with one exception:

Now, that would be that transients can take place at different conditions, high power levels, and so on; and on situations, I think, that the peak pressure may take place beyond 2, 3, 4 minutes. And for those kinds of events, SCRAMming at 2 to 3 minutes would certainly help.

DR. MARK: Thadani, those power percents column, which is there --

MR. THADANI: Yes.

DR. MARK: Is that the power to which the reactor has dropped by 2 minutes or 3? Or is that the power at which it's running at time zero?

MR. THADANI: No. I'm sorry. That is the power to which the reactor has dropped at that time. It started at 100 percent at time zero.

DR. MARK: In all cases?

MR. THADANI: Yes.

DR. MARK: So you've got it down to 5 percent in 20 minutes in the BWRs, which is just the decay heat --

MR. THADANI: That's correct.

DR. MARK: -- alone. What brought it down to, to that?

MR. THADANI: Okay. Because of the poison in this case -- I might point out that these numbers are best on mitigation systems being functional. That assumes that you have induced recirculation pump trip early, which reduces the power, as you can see; in a minute you're down to about 25 percent power. That's accomplished by recirc pump trip.

Further reduction in power is accomplished by injection of poison.

DR. MARK: Okay. So these times then, for BWRs, are not the times at which the SLCS comes on.

MR. THADANI: No. No.

DR. MARK: It's assumed they'll come on at 2½ minutes.

MR. THADANI: That is correct.

And you'll see at 3 minutes and beyond, the power is starting to go down, because you're seeing the effect of the poison on it.

In, in general, I think by having a SCRAM action 2, 3, 4 minutes later does an awful lot for you on boiling-water reactors, in the sense that you can now maintain inventory with redundant systems. And if one were to fail, like HIPSI were to fail, you can still maintain a level using RCIC, which has got lower capacity but if SCRAM action has taken place, you should be able to maintain vessel inventory, even at the lower capacity high-pressure system.

As you go further and further in time before you get SCRAM, those benefits start to disappear, because you can start to uncover or lose a lot of inventory if you go too far before you get SCRAM action.

This, this I hope is just a, just to -- gives you a, a, a feel for the times involved and the kinds of benefits one can derive from SCRAM action at different times.

DR. MARK: Is there a power level below which you don't need SLCS at all?

MR. THADANI: If --

DR. MARK: Twenty-five percent? That's the power at time zero.

MR. THADANI: Oh, I'm sorry. You're saying if the transient starts at 25-percent power?

DR. MARK: Right.

MR. THADANI: In a BWR it's not going to make a significant difference. It'll make some difference, but not significant difference, because if you, if you have, if you're operating at 25-percent power, and if you have the pumps running, which I -- depending on what mode of operation you are at, I would expect you probably don't have the pumps running. But I've forgotten their natural circulation line as to exactly when the pumps are not running and so on. But what happens is, if you have a turbine-trip type of event, you bottle up your primary system, you again are going to collapse the voids; your gain pressure is going to go up, as well as reactivity is going to go up.

You have to be able to find a way to reproduce voids; that is, you have to still be able to trip the circulation pump to bring the power back down. But when you do that, your power will, I believe will not come down much below 25 percent, so you're back to the same mode, I think.

DR. MARK: Okay, so what you're answering for me is that the same need of autoinjection or autoactivation of the liquid control exists at power levels all the way down to --

MR. THADANI: It would. The only difference now would be the timing may -- you may have more time. The, also the containment, if you have isolation event at 25-percent power, I'm not sure that you would open up all the relief valves. You may.

So that the -- my understanding that the energy that is dumped into containment, the integral amount may be somewhat lower, because you don't have initial period of higher power operation. That's really the key difference. After, after pump trip you're basically to the same set of conditions, so the only doubt that you have is in the first few seconds.

CHAIRMAN KERR: Now what about the oscillatory behavior of the power on a long-term basis?

MR. THADANI: Okay. It --

CHAIRMAN KERR: Is it likely to be improved?

MR. THADANI: Yes, if you believe the calculations, and I think most of us seem to think those oscillations may be real -- they start roughly at 3½ minutes or so. So if you can get SCRAM action in about 3 minutes, I would think that you would eliminate oscillations, within 3 minutes.

MR. EPLER: Mr. Chairman, could we revert back to the previous discussion of the transient initiated at lower powers?

We didn't take into account the bypass to the condenser. Wouldn't that be quite a stable operation at 50-percent power and less?

MR. THADANI: No. What, what I, what I was talking about, Mr. Epler, was oscillation events. Oscillation events, you don't have bypass.

MR. EPLER: Oh, oh, mainstream isolation valve -- yes, of course.

MR. THADANI: I have a summary if this slide and -- which answers the questions you asked, you know. It does, I think it does eliminate oscillations, as an example.

I think, I think I've gone through these considerations.

I'll go on to the next question that was asked. And this one, I must admit some of your questions are very difficult to answer.

This one, if I interpret your question correctly, said, "You've made some recommendations on the type of hardware changes that should be made for ATWS. " Now, what these modifications in any way have other Class 9 accidents are -- look differently, I suppose. Are there some features that are going to be required under Class 9 accidents which may help ATWS?

I believe there are.

A number of items -- I can go through a list or just give you a rough idea. Of course, relief and safety valve testing I described earlier and discussed it.

We have also gone out and talked about, in various reports, the need to be able to address multiple failures; and multiple failures could be something like extended loss of feedwater.

CHAIRMAN KERR: Let me explain a little bit.

Paul and I discussed this question.

What I had in mind might be typified by the following kind of argument. Let's suppose that one finally concluded that one needed a core catcher for the design such that one could be certain that the core catcher would contain the core and that containment itself would maintain its integrity with a probability high enough to be acceptable for whatever purpose.

One might at that point say -- I don't advocate this position, but it's a possible one -- "I don't really care from a safety point of view whether I get core melt or not, because I can handle it." And if I'm concerned about core melt now, I'm not concerned because of danger to the public, but because it's going to cost a lot of money to clean it up; or it's going to injure the plant; or whatever.

And it was in that context that, to some extent, I was asking; that is, are there other anticipated requirements which says that one must be able to handle either partial or full core melt? Which in some senses might permit a tradeoff on whatever mitigation one is planning for ATWS.

Or, or have, have you looked at that possibility? Is that true?

MR. THADANI: No, I, I'm not absolutely clear in my own mind, based on what we've been doing recently, whether and if and how far we would be going with this concept of core catchers, filtered vented containment.

I cannot really address that aspect of it. I just don't know. But I do want to make a comment that there are other areas where I think there are some benefits from what is being required in action plants, as well as what we're talking about in ATWS.

The action plan does require changes to PORVs, does require improvements in the auxiliary feedwater system reliability, does require automating aux feed system on plants where it's manual today. All of those factors are consistent and would, in fact, help ATWS or vice versa.

Where they automate aux feed, we would hope that they would take into account ATWS considerations, avoid having two sets of actuation circuitries --

CHAIRMAN KERR: Who is the "they"? Responsible for this --

MR. THADANI: "They" would be, would be the industry. It's our hope to point out to the industry that automating aux feed helps you satisfy our TMI-related concerns, as well as ATWS concerns. Here are the requirements for ATWS. Find a way so you just have to do it once and not twice.

The other one that we have talked about, as you know, Dr. Kerr, that I did work for Denny Ross last summer; and we were interested in looking at natural circulation and feedwater-related transients. One area we were concerned about was extended loss of feedwater, and we have communicated to a certain extent what might one do if one ends up with an extended loss of feedwater?

On some plants you can still shut down by going to feed and bleed method. On other plants you are limited by either the relief valve capacity is not high enough or the shutoff head on the HIPSI pumps is pretty low, in which case it would be helpful to have additional relieving capability, so that you can reduce the pressure sufficiently to come in with your high-pressure safety-injection pumps to keep the core covered.

I think in a way it's factors like that which may be of help as far as ATWS is concerned. But I looked at a recent response to a unit of concerned scientists, list of concerns on Indian Point and Zion. And I picked off these four that I thought had ATWS relationship. Others may, but at this time I don't know how far we're going to go.

Well, that's all I had planned to say, unless you have questions.

Again, as I said earlier, if you would like to discuss any specific area that might be helpful to us, if you would let us know, I know there are two or three people who would like to head back, especially in the area of computer models and value impact considerations.

CHAIRMAN KERR: Questions.

(No audible response.)

CHAIRMAN KERR: I have a feeling that people want to consider possible questions over lunch. And I would therefore deflect --

Yes, sir?

SPEAKER FROM AUDIENCE: Are you entertaining questions from the audience here?

CHAIRMAN KERR: No, I'm not. I would assume that most of the audience is in a position to communicate with the Staff directly.

I would propose, therefore, that we break for lunch and reconvene at 20 minutes after 1:00, at which point, if there are questions, we will pursue them; and if not, we will go on with additional presentations.

(Thereupon, at 12:20 p.m., the luncheon recess was taken.)

Tape 6
rcp fols rcp

AFTERNOON SESSION

(1:22 p.m.)

CHAIRMAN KERR: Let me ask: who among the consultants and the committee members expect to have to leave before, say, 4:30.

MR. DITTO: 5:15 is my --

(Various responses.)

CHAIRMAN KERR: If time permits, I would like for the Committee and the consultants, the Subcommittee and consultants, a bit of time in discussion. We'll simply have to see how it develops.

After your noontime ruminations, do you have any questions for Mr. Thadani that -- I don't want him to feel neglected.

(Pause.)

I, I hear no questions. Excuse me.

DR. MARK: I don't know that Thadani wrote Volume 4 --

CHAIRMAN KERR: Is your next statement going to be a comment, a criticism, or a compliment?

DR. MARK: Well, I was wondering from where the idea came that on the very last page of it the cost in radiological exposure of some of the revisions, as estimated by somebody, should come out to be 320,000, which I think means 329 rem -- this was only exposure that was being figured -- is then also said to be, confirms our idea that that's negligible.

That seems like a most unpolitic kind of approach, whether it's in man-rem or dollars.

CHAIRMAN KERR: That's a statement, not a question.

DR. MARK: I guess it's a statement.

(Laughter.)

CHAIRMAN KERR: Did you understand the statement, Mr. Thadani?

(Pause.)

Mr. Ray.

MR. RAY: I have a statement that has a question in it.

You've alluded several times, though, to the concerns that possibly an alleged improvement would result in an impairment, rather than an improvement. And I'd like to know, if thinking of this, and being a little bit concerned with all the changes, particularly in electrical control systems that are involved with these fixes, like adding an aery and so on, if there is anyone, either in the agency or elsewhere, who sits down after this whole aggregation is conceived and assesses the aggregation of systems as a whole to make sure that one of the new fixes or one of the new insertions hasn't set the stage for serious interaction that would really impair safety system performance in the all -- somewhere down the road:

Does the Staff do this? Or will it do it? And if not, whom do they expect to do it?

CHAIRMAN KERR: Do you understand the question, Mr. Thadani? Do you, do you agree to field the question?

MR. THADANI: My response is going to be, I believe both ends, both sides would have to address it, consider it -- implications in terms of impact on other accidents, as well as interactions.

We do not have any final designs available to us for us to be able to comment beyond that.

MR. RAY: Well, when designs are submitted, will the agency, will the Staff undertake such an appraisal?

It seems to me some agency should, because they're not simplifying things by rude additions; we're really complicating the things.

CHAIRMAN KERR: Well, as a, as a legal authority, I can tell you that it is a responsibility of the licensee to investigate any changes that he makes in his plan, to be certain that it does not represent, for example, an unreviewed safety question or does not make the plant unsafe.

Does that make you feel any better?

MR. RAY: No.

CHAIRMAN KERR: I tried.

Other questions or -- Mr. Epler.

MR. EPLER: One which is -- I'll try to contrive to make it a question:

We have many, many problems and issues raised in -- it's rather complex, detailed. But out of all of this there is one all-important question that stands head and shoulders above all the others.

This problem is a BWR problem, because the BWR is seriously surely in trouble without a SCRAM, whereas a PWR might be in trouble.

And the mitigation for BWR has to be, for sure, reset complicated or the game is up.

Now, I ask how are we going to disentangle the recirc pump trip out of all of this other jungle of -- so we can make sure.

What we do to ensure that we don't get spurious liquid poison injection doesn't also impair the likelihood of getting recirc pump trip. We have to have -- I don't see any discussion of that; and I think that that's one issue that will stand out from all others.

The question is, what do you propose to do about it?

CHAIRMAN KERR: Do you understand the question?

(Pause.)

MR. THADANI: I believe I do. If I may make a comment on the first part, though, you said this is a BWR problem. I, I guess I believe it's a pretty darn serious problem for some pressurized water reactor designs also.

MR. EPLER: A little less serious.

MR. THADANI: I'll be honest: I don't know. On some PWR designs.

CHAIRMAN KERR: Suppose that you accept Mr. Epler's hypotheses and go on from there.

I think his question is, how do you disentangle the pump trip from the other parts of the logic system that may be required.

Is that --

MR. EPLER: Right.

MR. THADANI: We have looked at only a small number of designs, the recirculation pump trip circuitry. We have set up a number of check points, areas to watch out for, look for. I believe we sent that information to the ACIS some time ago. I don't have it with me. But the objective there was to make very sure that there was separation from protection system, that the hardware that was used was different than that used in the protection system, that the reliability of this hardware was acceptably high.

I think I also mentioned considerations of testing and so on that would be performed at different times than that for protection system. Environmental qualifications, as you well know, the actual protection system is qualified rather strict standards, so at least it is our judgment at this stage that those environmental factors probably will cause the entire system to fail.

As to the possibility of poison actuation circuitry, resulting in inadvertent actuation of poison and having an impact on recirculation pump trip, it's not clear to me what impact that could have on recirculation pump trip. We did address the potential for inadvertent actuation of poison; we did discuss with you the circuitry that was to be used in a rather perceptual sense to actuate the standby liquid control system, as well as the timer that is supposed to be associated with that system's actuation.

The part that I'm not clear in my mind about is how, even if there were an inadvertent actuation of poison system, how that would have a deleterious effect on the recirculation pump trip.

MR. EPLER: Well, that isn't quite my concern. My concern is that if you put in the same schematic and in the same system both the recirc trip and the liquid poison, and if you go to great lengths to ensure that the liquid poison is sufficiently impeded, that it won't go off -- so you may also inadvertently impede the recirc pump, which is kind of hard to do.

MR. THADANI: No, the logic is quite different on the two systems. In one you make sure the pumps trip.

In the other case you have coincident logic to actuate the system, which I think has a reliability consideration. But it seems to me that the two are quite different in the sense that if they were to have a common design, I would be concerned like you, because I think I agree with you. The importance of recirculation pump trip just cannot be minimized.

DR. LIPINSKI: I'd like to add to that comment because on January 25th, Hank Fairfelund (phonetic spelling), of General Electric Company, had pointed out that the plan at that time was to use minicomputers to determine what brads were in motion and whether SCRAM had been accomplished.

And based on the number of rods involved, this can be a fairly complex system. And the reliability of such a system has to be established.

MR. THADANI: I, I hope that was a comment. I don't have any response to it, other than to wait for GE to propose the system.

DR. LIPINSKI: Right. But in terms of Mr. Epler's concern, it's my concern as well when we're just given a basic description of how the actuation of the pump trip is to be obtained, based on rod position.

SPEAKER: May, may I make a clarification to that point?

RPT does not go through that logic. That is only the boron. RPT is initiated with, with each call for SCRAM, as is ARI. It is only that the boron, then, would, would go through that logic.

CHAIRMAN KERR: Thank you, sir.

Mr. Saunders, I don't want you to feel left out.

DR. SAUNDERS: I'm not left out, sir. Thank you.

CHAIRMAN KERR: There being no further questions or comments, we go to the next part of our agenda, which has a presentation by the Atomic Industrial Forum, represented by Mr. Sorensen, I believe.

(Pause.)

MR. SORENSEN: Dr. Kerr and members of the Subcommittee, as was indicated, my name is Gerry Sorensen; and I'm chairman of the AIF Subcommittee on ATWS.

The discussion of anticipated transients without SCRAM is often quoted as having been, having consumed 11 years of NRC and industry review. This is a somewhat simplistic characterization of a lengthy series of evaluations, discussions, and analyses of the potential failure to SCRAM of a power reactor and the design bases for its evaluation. These have involved the reactor vendors, the utilities, EPRI, and others in the industry, as well as the AEC and NRC.

By inference, the reference to 11 years creates a perspective of NRC requests and industry opposition of almost incredible proportions. A careful review of the development of this topic reveals that the continual evolution of proposed criteria has also contributed repeatedly to the frustration of attempts to resolve the issue. Volume 4 of NUREG-0460 continues this history.

The slide indicates we have used this previously in AIF presentations to indicate the progression of criteria with respect to ATWS and its evolution from its inception.

The first slide takes us up through the point of the status reports in 1975 to '78. And this was then followed in '78 with the issuance of NUREG-0460, and up through the issuance of Volume 4 this past month. And as noted here, there has been a continual changing of the ground rules that we were investigating.

As noted, in the changes between Volume 3 and Volume 4, we have gone from prevention and/or mitigation to prevention and mitigation for all plants. A series of generic and plant-specific analyses that will now be required; and as you saw on the schedule that was put forth today, it is desirous that those analyses be completed by the end of this year.

(Pause.)

The nuclear industry is interested and anxious to resolve ATWS. The utilities, through the N-triple-S vendors, have repeatedly responded to NRC staff requests to evaluate

ATWS transients with changing assumptions and criteria. The utilities have supported and closely followed the vendors' efforts. Our opposition to the proposed Staff resolutions has been based upon our disagreement with conclusions drawn by the Staff, the assumptions and criteria used, the absence of a technical basis for their positions, and particularly in the case of Volume 4 the process itself.

The NRC Staff stated in NUREG-0460, Volume 3: quote, "Simply stated, it is our judgment that the individual and societal risk from ATWS have been, and are today, acceptably small."

And we in the nuclear industry agree with this judgment, and we have consistently maintained this position. Even though ATWS is a very low-risk issue, we have agreed to do more than we felt was necessary in order to close this issue.

The technical bases for consideration of anticipated transients without SCRAM have not been considered within the priority of all other risk contributors. The various review groups and inquiries since the incident at Three Mile Island, have concluded, as did the WASH-1400 study, that the low-probability events do not dominate risk to the public health and safety.

Both the President's Commission on TMI and the NRC recommended the use of risk-assessment techniques for identification and resolution of potential safety issues.

However, rather than resolving ATWS on a probabilistic risk-assessment basis and by an orderly verification process, the nuclear industry was suddenly confronted with NUREG-0460, Volume 4, which intends to implement the requirements for elaborate plant modifications and extensive additional ATWS-related analyses without an appropriate technical justification.

Significant design changes are being recommended for the reactor trip systems and the reactor coolant-pressure boundary without sufficient consideration of the resultant effects upon the overall system's safety. Plant modifications, such as additional safety-valve capacity for PWRs, should not only consider the reduction in risk for an ATWS event, but they must also consider the potential increase in risk from small LOCAs.

The NRC Staff has not adequately assessed this increase in risk. Rather, portions of the Staff have arbitrarily considered only their higher probabilities for ATWS events, which tend to mask any effects of the probabilities of small LOCAs.

If a more realistic probability for an ATWS event were assumed or considered, it would demonstrate little or no net increase in safety and may even show a net decrease in overall plant safety.

Other examples of technical deficiencies we have found in NUREG-0460, Volume 4, are the recommendation that Westinghouse-designed plants install a backup SCRAM system is not justified by the NRC Staff. The rationale that additional protection is always justified by improvement in defense-in-depth and that design changes are needed because the actual incident sequences do not behave as foreseen in the safety-analysis scenarios is totally inadequate.

The NRC staff has attempted to address ATWS as an isolated issue. However, the industry, and indeed the licensee, cannot focus only on one issue, but must consider ATWS in the context of the total integrated plant and other safety or licensing issues.

The industry will support resolution of ATWS, or any other single issue, when the bounds of the associated risk and impact are defined on a sound technical basis. NUREG-0460, Volume 4, recommends more plant modifications and additional analyses, which again do not define the limits to resolution of ATWS. The issues are too numerous and the plants too sophisticated to continue to add "band-aid fixes" without considering the overall systems effects.

This is the same concern that was just expressed by Mr. Ray. It's the same thing that bothers us in this context.

The value-impact assessment is incorrect and incomplete to the point, in our opinion, of being somewhat deceptive. We note that the items listed as values in NUREG-0460, Volume 4, only one of those issues is related to safety. Further, if the value-impact comparison were restricted to safety-related values such as man-rem exposure and increased probabilities of other accidents, it is our jusdgment that the impacts would far exceed the values. In addition to the dollar estimates of costs, which the industry can neither agree with nor understand their derivation, the man-rem assessments are based upon estimates done for the Turkey Point Steam Generator repair efforts which are not directly applicable or relevant to the recommended ATWS plant modifications.

Also, with respect to impacts, I have provided the Subcommittee with a letter to the NRC from my utility, Washington Public Power Supply System, which identifies a gross misinterpretation of cost information previously provided to the Staff.

New containment isolation requirements are recommended even though these isolation requirements appear to be unnecessary or ineffective. The current ATWS event dose calculations show ATWS doses that are well within the the 10 CFR 100 limits. For PWRs, for example, the major portion of that calculated dose comes from the secondary side of the steam generators which are utilized for post-ATWS core-heat removal.

We would be pleased to provide the Subcommittee with a more complete list of technical deficiencies after we have had more time to study Volume 4.

In our opinion, the NRC Staff did not bring together the expertise and technical competence necessary to develop the technical justification for asserting that implementation of the recommended plant modifications will be accomplished with a minimum of disruption and downtime.

Implementation of Alternative 4A hardware modifications on the operating plants, particularly the recommended additional safety-valve capacity for operating PWRs, cannot be accomplished with any extended shutdowns well beyond normal refueling outages. The potential addition of nozzles to accommodate more safety valves, addition of more safety valves, replacement of safety valves discharge piping, additional safety valve support intallation, installation of increased capacity quench tank, the installation of larger associated piping, and the post-installation testing that would be required are only a few of the items that were not fully considered by the Staff. And these items cannot be accomplished during a normal sequence of refueling outages.

Additionally, considering that the proposed rulemaking process must first finalize the Alternative 4A modification requirements, there is not sufficient time to then design, procure, and install the plant modifications within the schedule proposed by the NRC Staff in Volume 4.

As far as the procedural aspects, the industry has been totally frustrated in the resolution of ATWS by the various positions that the NRC has assumed and the techniques employed by the Staff.

The most recent example of these techniques has been the premature rejection of the early verification program. Many of the ATWS submittals made by the N-triple-S vendors on behalf of the majority of their owners were less than one month old before the NRC issued Volume 4 of NUREG-0460. Indeed, the NRC's rejection of the early verification program was made even before some vendor submittals were reviewed.

On such a complex issue as ATWS, it appears to the industry that no substantial review was performed by the Staff of the early verification program submittals and that the program was doomed from inception by the Staff's preconceived attitude on ATWS.

The NRC has attempted, on the ATWS issue, to take a design engineer role in the resolution of the problem, rather than to assume the position of regulator. This procedural approach has taken the responsibility of implementing safety criteria from the licensees and placed both the establishment of safety criteria and the resulting system design criteria for ATWS fixes with the Staff. The basis of the ATWS perspective fix -- prescriptive fixes are at best obscure and, in our opinion, totally without substance from a risk-assessment or value-impact point of view.

The industry questions the manner in which the safety evaluations for these modifications will be made within the legal restraints of existing NRC regulations. The NRC Staff has inferred that they do not intend to rely on supporting analyses of the ATWS modifications as a basis for their installations and, in fact, will insist on installation of the hardware fixes prior to complete review of analyses, using the Lessons Learned mode of implementation.

In that instance, the industry recognized the need for implementation of most of the NUREG-0578 modifications for either design, operational, or political reasons.

However, we don't agree that continued use of this prescriptive regulatory approach is in the best long-term interest of nuclear plant safety.

The AIF has recommended that the NRC establish an overall plan to define and prioritize all outstanding safety issues that are presently before the industry and the NRC.

The industry is continually being plagued with an isolated approach to the resolution of nuclear safety or licensing issues. And as does the Staff, the industry also has finite manpower resources. If the Staff is committed to maximizing the usefulness of these resources in resolving outstanding safety issues, then an integrated approach to resource management must be utilized.

As a case in point, both the Report of the President's Commission on TMI and of the NRC Special Inquiry Group have been very specific in this area. And integrated and controlled approach to the resolution of safety issues is essential. The changing requirements of the ATWS issue, the current furor over NUREG-0660, and the proliferation of NRC bulletins exemplify the industry's concerns in this area.

As a precursor to the resolution of the ATWS issue, the AIF recommends that appropriate and realistic acceptance criteria for nuclear safety be developed. The NRC Staff should also ensure that all of the necessary peer review is accomplished within the Staff for all criteria specified.

Rather than defining prescriptive fixes for the resolution of ATWS, we feel strongly that the Staff should establish safety criteria. The industry would then be able to move forward with these defined safety criteria, using sound engineering principles to determine the need for system modifications. The resolution of the ATWS issue requires a complete understanding of the problem, a definition of all acceptable criteria, and an integrated approach to the selection of the fixes that will result in a potential for safety improvement.

We share the Staff's concern that the ATWS issue needs to be brought to a close. As we have indicated, however, we do not agree that the ATWS issue is well-founded in risk terms.

We recognize that the Staff considers ATWS to be an unresolved safety issue and that they have made commitments to resolve these issues on established schedules. We note, however, that these schedules were established independent of the Staff's present efforts to prioritize the lessons learned from Three Mile Island.

Effective utilization of resources, both those of the industry and the NRC Staff, requires that ATWS be placed in proper perspective, based on the contribution to risk.

One of the first lessons learned from Three Mile Island was that the NRC and the industry had concentrated too much on low-probability events. We must not forget this important lesson when considering ATWS.

Volume 4 makes reference to the need to achieve the required level of safety. We need to have the required level of safety defined.

As stated in NUREG-0460, Volume 4, the industry still believes, one, that the probability of an ATWS event that might jeopardize public safety is so low that no plant modifications or other protective measures are needed; and two, if ATWS modifications are to be improved as a precautionary measure, they need only be preventive measures.

We find insufficient technical justification for the resolution proposed by the Staff in going forth. We are prepared to work with the Staff to further respond to their concerns and to address ATWS based on its relative priority.

In conclusion, urge this Subcommittee to recommend that the Staff reevaluate the approach put forth in Volume 4, in accordance with the technical and procedural concerns which we have discussed above.

(Pause.)

Concludes my comments, Dr. Kerr.

CHAIRMAN KERR: Thank you, Mr. Sorensen.

Are there questions? or comments? on the part of members of the Subcommittee or consultants.

While you are thinking, let me ask a few, Mr. Sorensen.

You refer on page 2 to various groups' concluding that low-probability events do not dominate risk to the public health and safety, and specifically you refer to the WASH-1400 study.

One might conclude, on the basis of the study as originally published that ATWS was not a significant contributor. Now, you have heard, if you were here this morning or earlier, the Staff's statement that they re-evaluated the ATWS contribution; and on the basis of the reevaluation, have concluded that it is a significant contributor and they further advocated that the probabilistic analysis staff should be fairly familiar with WASH-1400, agrees with their evaluation.

Do you agree with their reevaluation?

MR. SORENSEN: I guess I had seen that in Volume 4, that they indicated a reevaluation and indicated that ATWS now was of more concern than they had previously thought.

I guess I have not seen what that evaluation consists of, sufficient to be able to make a judgment one way or the other.

CHAIRMAN KERR: Well, I would urge that you try to have a look at it, because if the industry is placing some significance on at least the approach taken in WASH-1400 and if indeed a reevaluation indicates some change in conclusions, you would, I believe, want to look at this.

MR. SORENSEN: It is definitely of interest to us, that's correct.

CHAIRMAN KERR: On page 3 you refer to the possibility that one might assume a more realistic probability for an event.

What in your view is a realistic probability?

MR. SORENSEN: I guess to date I have not seen anything that would alter my opinion from the probability for an ATWS event as put forth in the EPRI reports a few years ago. Those reports were based on the best available information to us at that time. And to my knowledge, there has been no, no real change to that at this point.

I believe the, that the number placed by the EPRI reports put an ATWS risk down in the, in the realm of 10^{-6} or below.

CHAIRMAN KERR: And that's 10^{-6} per year for what?

MR. SORENSEN: It's been so long since I looked at that report, I may have to see if there's somebody here who has a feeling for that.

Hank, have you looked at those data?

CHAIRMAN KERR: I'm just trying to get an idea --

SPEAKER: It's a probability of an ATWS, per year. A-T-W-S. The consequences aren't in there.

CHAIRMAN KERR: Oh, the probability of, of the ATWS is 10^{-6} per year, and you consider that realistic? What sort of uncertainty do you attribute to that number?

DR. SAUNDERS: Since your, since your analysis is based on a Baysian analysis, it already has all sorts of uncertainties cranked into it.

CHAIRMAN KERR: I'm trying to get an idea of how the industry thinks. I --

DR. SAUNDERS: Oh, excuse me.

CHAIRMAN KERR: Because they've, they've given us very careful consideration, and they're being realistic; and I'm trying to get some idea of what uncertainties does one attribute to that 10^{-6}?

MR. SORENSEN: Yes, without going back to look at the report again, Dr. Kerr; I'm not sure what the uncertainty was that was attributed to that.

CHAIRMAN KERR: Okay, I'd be interested in finding out.

Now, at what point would you become concerned, if you are concerned with the EPRI number. How big would it have to be before you would become concerned?

MR. SORENSEN: I guess put in the context of other issues that we are designing for that, that we have concerns about -- I don't know whether the number might be 10^{-2}, 10^{-3} -- if we were to place all of those things that we look at as being realistic events in a priority basis, if ATWS were right in with the things such as, you know, a loss of off-site power, certainly we would be concerned.

I guess I'm not sure at this point exactly where one might draw the line.

CHAIRMAN KERR: Well, you, you have drawn the line, apparently on the basis of 10^{-6}; and I'm trying to get some idea of how far away concern this is and what it would take to move it into concern if you discovered that your numbers were perhaps worth revising on the basis of different information.

MR. SORENSEN: I guess we have --

CHAIRMAN KERR: And you, you had told me if it were about the same as the probability of loss of outside power, which is, what, maybe 10^{-1} per year?

MR. SORENSEN: That's roughly about where we'd put it, yes.

CHAIRMAN KERR: Okay. You'd be concerned --

MR. SORENSEN: We certainly would be concerned at that one.

CHAIRMAN KERR: Suppose it were 10^{-2}.

MR. SORENSEN: 10^{-2}, I think I probably also would be concerned.

CHAIRMAN KERR: What about 10^{-3}?

MR. SORENSEN: I guess that's about the point where I might start questioning.

CHAIRMAN KERR: Well, would you want to give it some attention if it were 10^{-3} or --

MR. SORENSEN: I'm sure we would want to give it some attention; and again, I think it would depend a lot on, you know, on how that stacked up against other issues that we felt were, were of a first-type priority. It was an ATWS, you know, if it's 10^{-3}, I think that's in the realm where we would begin to be concerned about the number.

CHAIRMAN KERR: Now, suppose that you concluded it was, say, as small as 10^{-4}.

Do you think you can demonstrate to the satisfaction of an unbiased audience that existing systems satisfy that?

(Pause.)

MR. SORENSEN: I guess --

CHAIRMAN KERR: I mean, suppose you decided that it should be that small --

MR. SORENSEN: Yes.

(Pause.)

Well, to me, that, you know, that is what the EPRI reports did. And those presented a, you know, a basis for an argument of, of what the number is.

Now, if those reports should come up to, say, 10^{-4}, for example, yes, I think it could be demonstrated to an unbiased audience. I guess I'm not sure in this industry where one finds an unbiased audience.

CHAIRMAN KERR: No, I wasn't proposing that you do it within this industry. I just assumed a hypothetical unbiased audience with whom you could deal. You can demonstrate --

Do you think you could demonstrate 10^{-5}? EPRI came up with 10^{-6}. Do you -- are you willing to accept the

EPRI argument that one could demonstrate nonreliability or probability of ATWS that low?

MR. SORENSEN: Yes, I would be willing to accept that.

CHAIRMAN KERR: Well, that's one way of calibrating my (laughter) understanding of your understanding of the EPRI report.

Now let's see:

Do you think a realistic number for ATWS is about 10^{-6}? And you're going to get me some indication of what, what you --

MR. SORENSEN: I will try to refine what the, what the uncertainty was on that number, yes.

CHAIRMAN KERR: And you would be concerned about ATWS if somebody could convince you that it was as small as 10^{-3}.

MR. SORENSEN: Believe so, yes.

CHAIRMAN KERR: I, I, I'm not holding you to that number; you might change your mind on the basis of new information. I'm just trying to get an idea what you're thinking today.

Now, you have said on a number of occasions that the industry doesn't -- I think you were telling me, the industry doesn't think anything needs to be done about ATWS, but you'd be willing to do some things, provided you could be convinced that they didn't contribute to unsafe situations.

Does that conclusion apply to BWR pump trip, since there apparently are BWRs now operating that don't have pump trip installed?

Do you think if you were operating the BWR, I guess you're not --

MR. SORENSEN: But we do have a BWR under construction.

CHAIRMAN KERR: Do you think that the BWRs ought to have pump trip?

MR. SORENSEN: We have committed to put a pump trip on our BWR. And --

CHAIRMAN KERR: That's in answer to a question I didn't ask.

(Laughter.)

MR. SORENSEN: Yes, now, I guess I am not in a position to speak for all the BWR owners, and I might refer question to Mr. Fefelun; but --

CHAIRMAN KERR: Unfortunately or fortunately, you came here to speak for AIF; so let me ask you not to speak for the owners, but to speak for AIF.

As you speak for AIF, do you think it's worthwhile to put pump trips on BWR?

MR. SORENSEN: My understanding of the pump trip is that that is worthwhile.

CHAIRMAN KERR: Okay.

MR. SORENSEN: And as I indicated, we have made that commitment.

CHAIRMAN KERR: So you, you, you are willing to do a little bit about ATWS.

MR. SORENSEN: Yes.

(Pause.)

CHAIRMAN KERR: Now, let's see: on page --

By the way, I said I was going to stop and give somebody else a chance.

(Laughter.)

Ep.

MR. EPLER: Well, I wouldn't want you to stop on my account.

(Laughter.)

I do have a question.

CHAIRMAN KERR: Please.

MR. EPLER: I think we have here a fundamental issue that needs to put on the table.

A while ago I expressed some concern that the liquid poison injection is an abomination to industry and, therefore, we will have difficulty in ensuring that it will be actuated as freely as we might like.

Now, we do actuate the SCRAM very freely. The only penalty for a SCRAM actuation is an economic penalty. Not very serious. We don't worry about it much. But you raised a new issue:

Any sensing device that you're afraid to use is not a very good safety device.

Now, you raised the issue that the safety valves may be in that category, because you say if we had safety relief capacity, which I would believe would be the order of a factor of 2 or less. And this is what exacerbates the small LOCA situation.

Then the small LOCA problem must be a lot worse than I thought it was .

MR. SORENSEN: I guess our concern with the additional valves is that, you know, the more valves --

MR. EPLER: You believe that we were within a factor of 2 or less of being in serious difficulty with our safety valves causing small LOCA.

If this is true, then I think the safety valves themselves should be an issue that we should look at.

MR. SORENSEN: I guess I'm not sure of where to, where to place those.

MR. EPLER: I hate to believe that the addition of a few more relief valves would put us in that kind of trouble. I hate, I would not want to believe that we are not in shape at this moment.

MR. SORENSEN: I think our concern is only one of the addition of the valves, does provide greater opportunity for there to be a valve that's going to stick open, that's going to give the problem --

MR. EPLER: Well, after the first approximation, if you double the number of valves, you double the --

I would think you'd have to have maybe a hundred valves before you'd be in such trouble.

MR. SORENSEN: That may be a possibility, and my comment on that was that that is a, a potential increase in risk; and it's one that we think needs to be looked at carefully, before we go out and make that modification.

MR. EPLER: Well, I would hope we could look at it rather quickly and say it's negative.

CHAIRMAN KERR: But you see, that wouldn't convince him because he thinks the ATWS risk is negligible. And what he's doing is comparing, in his view, two negligible risks. I'm not trying to --

DR. MARK: Some are more negligible than, than others.

End T6
rcp

CHAIRMAN KERR: I am not trying to disagree with his viewpoints -- it seems to me, given his hypothesis, his concern may be legit. He's saying the average risk is very low. The risk with small LOCA is also very low.

If I add another valve, I may be doing something to risk a small LOCA which is comparable to, in my view, the negligible risk due to ATWS. That's not your view, because you don't think they have a touristic negligence.

MR. EPLER: No, I think that you are indeed putting these in perspective. What you just said was that the atmospheristic is indeed variable --

CHAIRMAN KERR: No, I am saying that I think that's your view. I don't want that to be attributed to me.

MR. EPLER: No, I'm saying that this is what this statement adds up to.

CHAIRMAN KERR: Gerry?

MR. RAY: May I get back to a point that -- one of your answers to Professor Kerr?

In your presentation, you indicated that you would prefer that the fixus for ATWS be concentrated on prevention rather than medication?

MR. SORENSEN: Yes, sir.

MR. RAY: If you were satisfied or it could be demonstrated to you that the probability of an ATWS were as high 10^{-3}, would you still feel the same way?

MR. SORENSEN: No, as I indicated to Dr. Kerr, I believe at that point my thinking changes and I would need to take a good look at -- maybe we need to mitigate if the probability is that high.

MR. SAUNDERS: But, suppose that you calculate and show that the probability was 10^{-4}, since this is based upon finite amounts of data and also constructed by mathematicians who have not the wisdom of God, and they may be subject to error, don't you believe that a reasonable and prudent person, even if it were 10^{-4}, would look at the problem at that level?

MR. SORENSEN: I think you have to look at the problem, and you would have to do some type of decision analysis to determine, you know, are the costs and the benefits --

MR. SAUNDERS: You have to look at the problem.

MR. SORENSEN: Yes.

MR. SAUNDERS: So far, we've neglected the consequence. The consequence is of such extreme value, like a core melt, factor that into the risk then maybe we're down to 10^{-5}, what a prudent person would look at. Well, that's with the Epsilon where the Staff is. There's not very much difference between your position and theirs, really, I would say, when you get right down to the nub.

CHAIRMAN KERR: You know, I remember the story in the Old Testament of the Angel who came to Abraham or to Lot. He used exactly this same argument in asking Lot, "How many righteous men you'd have to find in Sodom and Gomorrah before he destroyed the city by fire," it's a technique that still works.

[Laughter.]

MR. SAUNDERS: That's right.

MR. STETSON: My name is Fred Stetson, and I am the Licensing and Safety Projects Manager for AIF.

With regard to this risk question, I think there are two parts that have not been sufficiently distinguished here. One part is the probability of the ATWS event itself, and the other is the consequences that are related to it. My understanding of the Staff's position is that the Staff has a essentially equated that ATWS with core melt, and I think the way to go at the risk contribution for core melt is to, first, find out whether or not the reactor safety study number 5 times 10^{-5} is about right, and if it is to then look at fractions of that, that come from the different contributions.

CHAIRMAN KERR: The Staff is quite capable of defending it's own viewpoint, but I don't think you're interpreting their viewpoint correctly.

MR. STETSON: I said essentially.

CHAIRMAN KERR: I don't think they are essentially equating the ATWS with core melt. They're saying that in their view, having done a reanalysis using WASH-1400 techniques, ATWS now becomes a significant contributor to core melt. Isn't that --

MR. THADANI: That is correct. We have looked at the contributions in the spesc study with our estimates on ATWS and looked at the contributions of ATWS to the overall core melt probability --

CHAIRMAN KERR: I gather that analysis is not in the hands of AIF? It strikes me, on the basis of what you have said, it would be very useful if they had it, or could get access to it. You might convince them that they, indeed, should have another look, because they're still using the WASH-1400 numbers.

MR. THADANI: I might point out that indeed it's available publicly.

CHAIRMAN KERR: You're going to tell me I can find it in the Public Document Room?

MR. THADANI: Ten, I think, in Volume II of NUREG-0460.

CHAIRMAN KERR: Appendix 10?

MR. THADANI: I believe it's appendix 10, yes.

CHAIRMAN KERR: I'm not being facetious now, it is there in a form in which it can be examined and interpreted not just be answered.

MR. THADANI: I believe it can be examined there. There are a bunch of entries as well. They are provided in appendices 14, 15, 16, and 17 of Volume II of NUREG-0460.

CHAIRMAN KERR: Okay.

I would urge that AIF become familiar with this if you're not already.

MR. STETSON: Let me make one point of clarification. We also recognize the problems in the WASH-1400 numbers, and the basis for many of our judgments is rather the EPRI work on ATWS.

CHAIRMAN KERR: I'm simply suggesting that a later treatment of this exists, and apparently it's one with which the PAS staff agrees, and if this is relevant to your discussion, since you are apparently are willing to give some credence to that approach, I think you ought to look at it.

Other questions or comments? Mr. Saunders?

MR. SAUNDERS: I should say that whenever I become disenchanted with lack of scientific perspective and objectivity of the Staff, I gain a different perspective when I hear the presentations by the antagonists.

CHAIRMAN KERR: I hope that's sufficiently ambiguous and it won't be clarified.

[Laughter.]

CHAIRMAN KERR: Other questions or comments?

I have one other-- my first comment, I think the first paragraph on page two, and I'm being charitable when I say this is taking a statement of the NRC Staff somewhat out of context, Mr. Sorensen, and I would refrain from that sort of thing if I were you.

MR. SORENSEN: They go on to discuss the --

CHAIRMAN KERR: They go on quite a lot, I would say.

Now, on page eight of your presentation, you feel strongly that the NRC Staff should establish safety criteria. I guess I feel strongly that they should, and even they feel strongly that they should. But given their safety goal, risk goal, for ATWS -- let's suppose it is 10^{-6} or 10^{-5} for an ATWS with unacceptable consequences. Suppose they establish that goal, could you meet it? Would you be happy with the establishment of that goal?

MR. SORENSEN: I guess their goals would be -- yes, that's basically where our reports had put it. I think if they would establish a goal and we could go on from there to say, "Okay, we'll resolve ATWS on that basis," and forget about ATWS from that point, I think we would be happy.

CHAIRMAN KERR: But, if they said that they would be happy if you demonstrate ATWS contribution to core melt risk, but not greater than 10^{-6} per year per reactor, that would be a perfectly, straightforward, quantitative goal, and I don't believe you could do it.

MR. SORENSEN: We may not be able to, that's true.

CHAIRMAN KERR: But you see, here's the position where the Staff finds itself. They consider this -- whatever the number is -- I'm not sure I'm quoting the right one, but it's in that region. They consider this the appropriate goal, and they have wrestled manfully, and I hesitate to use the masculine here, but at least they have wrestled, and they don't know how you could meet that goal. I don't know how you could meet it either, frankly.

MR. SORENSEN: We wrestle with that, too.

CHAIRMAN KERR: What are you going to do? You disagree with the number, but given the Staff has arrived at that number in all good conscience, what are they going to do?

MR. SORENSEN: I guess the same thing we have done previously, we'll meet with the Staff about how we can or cannot meet the goal; why we may think the goal is either good or bad. In the end, they are the regulator.

CHAIRMAN KERR: Yes, but you see, on page eight you say that you feel strongly that they should establish safety criteria, and the rest of the report the implication is that they say the criteria ought to be quantitative to risk goal. Now, I don't whether that's what you mean or not. I think you're going to have difficulty with a quantitative risk goal that's in the region of 10^{-5} per reactor year, at least for ATWS. I don't know how you meet it. I wish I did.

Yes, sir.

MR. STETSON: I'd like to address that briefly.

I think the goal has to be more than a number. The goal has to be a conceptual framework in a technical basis for what you're doing, and our problems with the Volume IV in earlier years has been in the lack of technical bases for the proposals that are being made, and the goal has to be more than a number.

CHAIRMAN KERR: Well, a goal should be backed, perhaps, by a technical basis, but I guess I don't know what you mean by, "One needs a number and a technical basis." A technical basis could mean the way in which one achieves the number, and the Staff has sort of done that. They have said, "If you install this equipment, as far .s we're concerned you will have achieved the goal," they don't quite say a number. You may disagree with that approach, but in a way it's a technical basis. So I guess I don't know what you mean by technical basis.

MR. STETSON: I would categorize that as a lack of technical basis.

CHAIRMAN KERR: Okay, give me a "for example" of what you would consider to be a technical basis?

MR. STETSON; I would want to know what the Staff means when they say that the risk has to be made acceptable or acceptably low. I would want to know what the analyses have been performed on the safety valves and the electrical fixus that are being proposed.

CHAIRMAN KERR: Well, let me suggest that in your presentation you use the same language by implication, because you say the probability of an ATWS event that might jeopardize public safety is so low that no modifications are needed. Now, what do you mean by that?

MR. STETSON: What I mean is that our information that has been generated by the industry, EPRI, and the utilities indicates that the ATWS contribution to public risk is a very small fraction of the total risk to the public.

CHAIRMAN KERR: Is that the kind of goal that you'd want the Staff to give you?

It's such a small fraction, and it's so low, those things are not very quantitative as far as I'm concerned.

MR. STETSON: There has to be some --

CHAIRMAN KERR: And I think that statement is being nonquantitative. I think at the present the Staff is being somewhat nonquantitative, but I think as I have watched the thing develop, they are now being somewhat nonquantitative. They tried to be quantitative, and they didn't know how to be.

MR. STETSON: That's right.

CHAIRMAN KERR: And you see, I find you using the same sort of language, and it's sort of inevitable when you get down to risks that are the sorts of risks that we're setting as goals. It's a dilemma that we all face, at least I think it is.

I believe in, and I strive for heavens, but I just don't think we're there yet. We've got to do the best we can. The trouble is, I don't know what that is.

MR. STETSON: I agree with that.

DR. LIPINSKI: Let me offer a comment in response to your first question as to how you verify. Well, it's 11 years which we now got 70 reactors. If we went for another 10 years, we'd have another 700 reactor years, plus the ones that are coming online that would be integrated over the next 10 years, so, Thadani, how many thousand reactor years did we require to confirm the 10^{-6}?

MR. THADANI: It was on the order of a million reactor years.

CHAIRMAN KERR: It depends on competence level, that's about 90 percent --

DR. LIPINSKI: I'm being facetious --

CHAIRMAN KERR: Anybody -- Mr. Ray?

MR. RAY: Am I correct in my understanding that you feel that by these fixus reaggregating we'll have reduced the probability of an ATWS by a factor of 100 or thereabouts?

MR. THADANI: Not that we would have reduced the probability of an ATWS by a factor of 100, rather we would have reduced the potential for severe consequences by a factor of 100.

MR. RAY: Well, in a way that's a goal. And I wonder what the response of AIF would be to that? You see --

CHAIRMAN KERR: Where are you reading from, Gerry?

MR. RAY: I'm not reading any specific place, but just a general impression I have as a result of the presentations I've heard.

CHAIRMAN KERR: Quite frankly, I'd call it factor two, but --

MR. RAY: I have progressively built the impression though from presentations --

CHAIRMAN KERR: You're talking about --

MR. RAY: Well, not just the fixus, the total package.

Let's stay with it for a moment. If this program would reduce the potential of severe accidents by a factor of 100, in a way that's an expression of a goal. Do you collectively feel that you would have another program that could be as effective as this to do the same thing?

CHAIRMAN KERR: Let me see if I can clarify. What is being suggested here is that if one is reducing the contribution of ATWS by a factor of 100, not that one is reducing the risk of all severe accidents by a factor of 100. They're quite different things.

MR. RAY: No, I'm talking in addition to that --

CHAIRMAN KERR: All right.

MR. RAY: Could you by some other prescription do this same thing, a prescription you prefer. Have you thought it through to that extent?

MR. SORENSEN: I guess the things that the industry has been willing to support in this area have related more to the electronics fixus that would prevent the event in the first place. That would provide some diversity and redundancy in the electronics.

MR. RAY: Okay, but prevention is going to be just as effective when we see the potential for a serious accident due to an ATWS.

MR. SORENSEN: Yes, that's right.

And as to the level of change that may affect, whether we're talking 100 or not, I think that's -- it's in those things that one picks up the major part of the reduction in risk, you know, that Staff talks about.

MR. RAY: Well, the point I'm making is regardless of the debate as to whether a probability is 10^{-3}, 10^{-4}, or 10^{-6} in an ATWS event, particularly with the present situation, there's been an expression of a goal of a sort which the Staff is attempting to accomplish by this program. What is your response to the possibilities of another program that you would prepare, you would endorse, that would do the same thing? Do you have an alternative?

MR. SORENSEN: Right offhand, I do not, but I think that's something we would be willing to undertake.

MR. RAY: Well, it seems to me that if you're going to respond to the Staff effectively, it may very well be along those lines.

MR. SORENSEN: That's a good point.

CHAIRMAN KERR: Other questions or comments?

Mr. Sorensen, suppose if you were faced with a situation which, 1) had only to deal withAltern. 3-a, would you feel any better about the situation, or would you feel that you were in something more nearly a situation with which you could -- and I am not using "you," I am referring to AIF -- with which you could live comfortably?

MR. SORENSEN: If Alternative 3-a was the thing that was proposed as being the resolution of ATWS, is that what you're saying?

CHAIRMAN KERR: Yes, sir.

MR. SORENSEN: I believe so, yes.

CHAIRMAN KERR: Any more questions or comments?

Thank you very much.

MR. SORENSEN: Thank you.

CHAIRMAN KERR: Mr. Enus representing, speaking for, the B&W --

MR. ENUS: Thank you, Dr. Kerr, members of the subcommittee.

My name again is Ted Enus, and I am Chairman of the B&W Owners Group Subcommittee for ATWS.

During the past couple of weeks, we've devoted a considerable amount of time to reviewing NUREG-0460, Volume IV, looking at its merits and what we consider some of its problems, and we do have some serious reservations with regard to Volume IV. We've been work ng in the last couple of days with the Atomic Industrial Board, and have addressed to them what we consider were some of our concerns, and would like to briefly make a statement to you on behalf of the Owners' Group as to how we perceive ATWS as a whole, what briefly our concerns are with Volume IV; what we would possibly propose as a resolution; and how we feel with regard to frustration of the issue and how we would like to get rid of it.

The Staff issued NUREG-0460, Volume III, in December of 1978, contained four alternatives for the resolution of ATWS. In February of '79, the NRC Staff requested, in a 58-page letter from Dr. Mattson, the industry provide the NRC staff detailed, quantitative system and stress analysis relating to the ATWS event in order to initiate the verification program.

The B&W Owners ATWS Subcommittee met with the NRC Staff and defined the scope of this effort and schedule consistent with Staff's need for the information. The TMI-2 incident in March of 1979 placed a significant strain on both the industry and the NRC's manpower. By necessity, much of the effort that we had devoted to ATWS and to the early verification program had to be diverted causing delays in the submittals that we had planned for ATWS. However, we did as best as we possibly could during that period of time continued efforts on ATWS within the framework in the environment that existed.

In July, and again in August, 1979, we met with the Staff to discuss the impact of TMI on ATWS, and as we perceived, agreed upon the scope and the schedule of appropriate responses to the Staff and with our concern in regard to TMI. Most of these issues we now have addressed to Staff in our submittals.

We further stated to the Staff in July, our support of the early verification approach and our position that we felt it was a reasonable means to achieving a resolution to ATWS, and again verified to them that we were interested in resolving ATWS and would support the early verification program.

Subsequent to those meetings in July and August, our submittal dates again slipped beyond the original commitment date that we had made to NRC. This, again, was due to the finite limitations on our manpower because of the number of higher priority items that were being imposed upon us at that particular time.

Again, we did manage to continue work on the effort, not to the same degree that we had desired and committed, but we did continue to work, and we did submit the results of the analysis as we had committed.

Subsequent to the issuance of Volume III, the B&W Owners Group has -- and B&W -- has expended over $1 million in analyses and manpower to support the early verification program. We continue to believe the results of these efforts support our position that the probability of an ATWS event is acceptably small. Furthermore, we have demonstrated that if an ATWS event should occur, we believe the RCS would remain intact and functional such as it could achieve a site shutdown.

It was, with considerable dismay, on our part, we discussed with the Staff in January the rejection of the early verification program and the rejection of the work we had done during the period of 1979 in support of that program. The Staff indicated to us at that time that the ATWS issue was no longer a negotiable issue, and that the verification program was being terminated, and that the dialogue on the issue was being terminated as well.

It was disheartening to us again, at that time, because the decision that was made was made before the largest of the B&W submittals on the ATWS analysis had been made to the Staff or had been reviewed, so we felt it somewhat unfair to withdraw from the early verification program, having not seen those submittals.

We read 0460, Volume III, and the early verification program was approached to resolving the ATWS issue to the B&W owners and the Owners' Group could accept and as a result we initiated the analytical programs requested by the NRC Staff.

Volume IV, in our opinion, is without basis and is inappropriate to resolve the ATWS issue. The NRC Staff's vascillation on the ATWS issue over the past 10 years has resulted in the expenditure of over $5 million by B&W and the B&W owners in analyses and manpower which we now perceive will not be applicable to the resolution of the issue.

Finally, we cannot support Volume IV as we believe it was generated independent of technically justifiable criteria. We firmly believe that if called upon, our systems, as they exist today, will successfully shut down the reactor. However, we do recognize from a probabilistic approach that a potential reduction in the probability of an ATWS event can be allayed by the addition of an electronic redundant diverse electronic trip system.

Therefore, to resolve this issue, we would most probably be agreeable to the installation of the redundant and diverse trip mechanism to resolve the issue once and for all.

We sincerely believe this is an adequate and appropriate resolution to this issue, firmly supported by our existing analysis.

In a conclusion to this statement, we would like to state that it is not the purpose of our presentation to deliberately and purposely attack Staff. We do not want to do that. We are interested in resolving the ATWS issue. We believe that there are many other issues that the industry and the Staff and the ACRS has to work on that are more important than the ATWS issue. We believe it extremely important for us to take an integrated approach to these issues to define all of the different issues, to look at the relative risk contribution of all of these issues, and let's work at them from the standpoint of which provides the highest risk to the public, and down that line. We do not believe at this time that ATWS ranks very high on that list as contribution to the overall risk, however, it does rank very high on the expenditure of our manpower and our efforts, and we are most interested in resolving the ATWS issue in a realistic and justifiable way in order to allow us to devote manpower to what we consider to be more appropriate safety issues.

Thank you. That concludes my remarks.

CHAIRMAN KERR: Thank you, Mr. Enus.

Any questions or comments?

DR. LIPINSKI: Yes.

On your closing remarks, you make reference to devoting your time to other issues. Could you enumerate as what you regard as being more important?

MR. ENUS: Well, it is very difficult within the industry right now for us to prioritize what are the more important issues.

Now, I'll give you an example. Recently we received I-Bulletin 7901-B, "The Environmental Qualification Bulletin," which does not make the 0660 Task Action Plan, and is basically a sleeper within the industry, yet the schedule that was provided on that bulletin of a 45- and a 90-day response in order to complete the schedule makes it a high-priority item.

Now, the question arises within the Staff, "Do we devote the 200-some-odd manyears that it is going to require to respond to this bulletin in the next 90 days, or do we apply that manpower to something else," and it's difficult for us to prioritize which is the more important of the two. What is the overall risk that the Environmental Qualification Bulletin versus something else? This is the thing I'm asking that I think we need to do, that we need to integrate all of these things. Look at them collectively. Work on the most important ones first. It's difficult to take things from a schedule or standpoint and determine which is the more important.

CHAIRMAN KERR: Mr. Mark?

DR. MARK: You referred to a system which was diverse and basically electrical. I forgot the words you used exactly.

MR. ENUS: Redundant and diverse.

DR. MARK: There have been in the 0460 volumes references to a BUSS and then AMSAC. What is the relationship between what you said you regarded as reasonable and possibly acceptable and those two terms?

MR. ENUS: Okay.

Particularly what I discussed was redundant and diverse electronic trip system would be very similar to the BUSS. I choose that terminology in that since the BUSS was developed several years ago, there are now -- also with regard to some of the modifications that have been made from NUREG 0578, there are now ways of which you can incorporate the same thing into the plant without installing a package or a black box called a BUSS, so you could install effectively the same thing without putting this copywrited, labeled thing on it.

DR. MARK: You do not include AMSAC, whatever that is supposed to mean in the list of things which you consider straightforward to accept.

MR. ENUS: To the most part, AMSAC is being installed as part of the 0578 requirements. The upgrade in the ACS feedwater system, the anticipatory reactor trip -- so, to a high degree that system is there.

CHAIRMAN KERR: Other questions or comments?

Thank you, Mr. Enus.

MR. ENUS: Thank you.

CHAIRMAN KERR: Mr. Ganglof is going to speak rather than Mr. --

MR. GANGLOF: My name is Will Ganglof. I work for Westinghouse in Pittsburgh.

I'd like to open my remarks with the rather flat statement that we in the industry are interested and very concerned about the safety of our plants.

That being said, I then would like to go on with my comments in regard to Volume IV of NUREG-0460.

I guess the best expression of our reaction is that we're disappointed and troubled by what we see happening in Volume IV of NUREG-0460.

We're disappointed with what we consider to be unnecessary new requirements. We've performed hundreds of calculations. I don't have any millions of dollars of figures, but it's in the millions.

For all varieties for the Staff and for this Committee showing that ATWS and Westinghouse pressurized water reactor does not lead to core melt. We've shown that less than half the rods scramming is adequate to eliminate any concern for an ATWS. We've shown that there is no ATWS problem in Westinghouse PWR if one uses a reasonable moderator temperature coefficient characteristic of the bulk of core life.

We've also done extensive probabilistic calculations to demonstrate that the probability of ATWS is already acceptably small, and we continue to disagree with contention that there is any significant risk to public health and safety from ATWS in Westinghouse PWR.

We are disappointed with the superficiality of the technical basis behind the new requirements or at least the superficiality of the documentation of that basis, if there is, in fact, more substance to it, we haven't seen it. We did receive the copy of the draft of the document that only very recently, and our technical people haven't had opportunity for more than a cursory review.

Out of the questions or the deficiencies or whatever that are listed in there for Westinghouse submittals, appear on the surface to be trivial. Yet the conclusion is reached that generic verification of adequacy could not be achieved. We don't understand why.

We are disappointed with the lack of logic that connects the various bits and pieces of requirements in the various alternates. We're disappointed with lack of dialogue with the Staff in recent months, coming to the conclusions they came to.

There are questions it often helps to ask for the answers rather than assume there are none or you wouldn't believe them if you heard them.

We're disappointed with the totally inadequate value impact treatment which assumes the answer and understates the impact. Moreover, we're troubled bv the process which seems to be at work.

What we seem to have here is a situation where the longer we talk about it the more real and the more urgent the problem becomes, regardless of anything that happened, regardless of what the results of analyses are, we can always do another analysis and find a different answer. Make a more conservative assumption, find a worse answer.

The longer it goes, the more real it is. We thought that one of the TMI lessons was that we are focused on the little things that happen every day, or every year. Rather than expend large amounts of computer on very interesting but very remote events.

Now, orders are convenient for the Regulatory Staff since they circumvent the need for a lot of interaction with other people. And I'll have to admit that it is embarrassing for a generic issue to be outstanding and unresolved after all these years.

But repetition of a conclusion doesn't create truth, and we still see no compelling argument that plant changes for ATWS will significantly reduce the risk to the public from nuclear power plants.

It is embarrassing that the industry still unanimously disagrees with Staff's prejudgment after 11 years of discussion. But we feel it's inappropriate to resolve that matter by pasting together all the hardware changes anyone thought of in 11 years, issuing orders to implement them.

Westinghouse suggests that the Staff be asked to justify each and every requirement on a techrical basis not merely by repeating the assertion that it is needed for safety.

We further suggest that a rulemaking hearing is proper means for expanding the design basis criteria for nuclear power plants in the absence of any demonstrated clear and present hazard.

The hearings should be scheduled prior to implementing Volume IV of the NUREG-0460.

Question?

Thank you.

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CHAIRMAN KERR: I call on Mr. Sherwood.

How long do you think your presentation will take?

MR. BUCHHOLZ: My own presentation will take about five or ten minutes. After that, I will introduce Mr. Pfefferlen and Mr. Holland, which would take -- presentation-wise, would take about, I suspect another 20 minutes.

CHAIRMAN KERR: Okay. Please proceed.

MR. BUCHHOLZ: My name is Robert Buchholz. I represent today General Electric Company.

We're speaking here to the subcommittee because of our serious concern regarding the Staff recommendations in Volume IV of NUREG-0460.

Our discussion today is divided into three phases, as shown on this chart.

First, I'd like to express some specific concerns about NUREG-0460, and then Mr. Pfefferlen will review the BWR capability to mitigate ATWS, very briefly, and subsequently respond to some of the stated Staff concerns that serve as their bases for the recommendations relative to the BWR in Volume IV.

Before I begin, though, I'd like to clarify the is and is nots of our presentation of our posture here today. We share the frustration of Mr. Thadani regarding the speed of resolution of the ATWS issue. It's particularly true of the need to resolve it in light of the additional or competing needs for manpower and application of resources to respond to the TMI Action Plan.

The presentation today is focused on how to resolve ATWS, and its purpose is not to delay ATWS resolutions. We believe that for the BWR that the Staff proposal will actually delay the resolution of the ATWS issue.

Specifically, we believe that the information that we've submitted as late as December of 1979 supply the information or the basis that the Staff needs to make a judgment regarding verification.

I'd like to just briefly review on the next chart the situation we thought we were in at the time of the issuance of Volume III.

Volume III, to us, recognized the impact of the mitigation requirement on operating plants and plants under construction, and it did that by defining alternatives that we're all familiar with and I needn't go into, and from our viewpoint, we were grateful for the acknowledgement of the SCRAMS, the credit given the SCRAMS system improvements, namely ARI and the SCRAMS discharge volume.

Now, as a result of the Volume III review, we understood the Staff position to be as shown on the chart, and this was the position that was discussed in January 1979 ACRS meeting.

Subsequently, the RCUBSEQ (?) concurred with the Volume III approach, and the history shows that the industry considered even Volume III to be not overly palatable, let's say.

Now, in response to that, GE and BWR owners performed assessments in accordance with the requirements of a request of the February 15th letter from Dr. Mattson. However, now, with the issuance of Volume IV, we find ourselves in a different position. A position in which we are once again being asked, "Go forth to alternate IV," and on the basis of that, we feel that the requirements are significantly in excess of what Volume III recommended.

I must say that I noted during the day today that to be a slight difference in the words that were spoken relatively to implementation for the intent of the Staff to implement Volume IV and the words that are in the document, and I'm sure the ensuing days will clarify that.

My chart reflects, I think, the intent that's shown in the documents. At least that was my intent to do in the chart.

Now, we note that there's a contrast in the document itself, in that Volume IV allows for the possibility that the information supplied the Staff is already sufficient. I think I've quoted fairly here since I started off with saying, "Concerns nonwithstanding."

Mr. Pfefferlen will address the concerns, at least our initial reaction to those concerns, after I talk.

CHAIRMAN KERR: At least those that are not withstanding?

MR. BUCHHOLZ: Yes. The current set of concerns, I would say.

Even though there's a significant increase in the requirement of Volume IV, we note that there's been no interaction at least with BWR part of industry during the development of the NRC position. In fact, I think it's accurate to say that we have not had any working meetings to submittal of our documents, and we view that with concern.

General Electric considers that we have provided already the necessary information to demonstrate verification, and we would like to look forward with meeting with the Staff to resolve those concerns.

In terms of observations, just at the time that we felt we were nearing resolution of the ATWS problem, we feel that there are arbitrary requirements being placed on us very suddenly.

CHAIRMAN KERR: Would it have been better if they had been placed gradual?

MR. BUCHHOLZ: One can only speculate, I suppose.

I think they might have been better if they had been placed after exchange of information and exchange of views, at least we would have been sure in our minds what each of us were saying.

We note that these new requirements in Volume IV are not consistent with any previous Staff position, and, frankly, we, at General Electric, feel that we're about to embark on another 11 years of haggle about this issue. We believe that there's a possibility for that because of the openendedness of the Staff proposal where the Staff is requiring a significant amount of more analysis -- we think that's the case anyway.

We see that these analyses are being requested or will lead to responses of a plant unique -- on a plant-unique basis, the optimization studies.

We note -- and I'll just say this because I've covered this point before, that, you know, we don't feel that the currently submitted documents have been thoroughly reviewed. I believe that's been indicated in the January subcommittee meeting and in Volume IV itself.

We feel that the TMI lessons have not been learned. Once again, we find ourselves in a situation where we're having prescriptive requirements, prescriptive specifications of hardware, and we find ourselves performing analyses on what the response is again. We urge that there be some priority setting done as was asked by an earlier speaker.

The BWR performance capability, we don't believe is being recognized, in part because of just not having reviewed the documents. We feel that there must be given that recognition of what the characteristics of response of the BWR are.

Finally, as you'll see from a chart that Mr. Pfefferlen has, we believe that the proposal is a costly one and yields little improvement in safety if you go all the way to 4-a. That last implement.

So, what we're asking the subcommittee to do is to not endorse the Volume IV as it stands, and we believe that our current submittal is really sufficient to demonstrate the mitigation capability of the BWR's.

I'm through with my portion. Mr. Pfefferlen will cover the second two parts of this discussion, unless there's some questions specific to what I've said.

CHAIRMAN KERR: Mr. Mark, do you have a question?

DR. MARK: Some point was made of the fact that the extent of the oscillations that might follow some of the steps in connection with an ATWS --

MR. BUCHHOLZ: Yes.

DR. MARK: -- looks as if they might be troublesome, and I believe that analysis and inspection of the looks of those were amongst the things called for in Volume IV, they had not appeared previously, that I recall.

Would you find that an unreasonable point to raise?

MR. BUCHHOLZ: No, I would expect the Staff to ask these questions about that, and that, frankly, is what I'm asking for in a sense, to exchange with the Staff.

Mr. Pfefferlen has some discussion of those oscillations that you'll see in a minute, and we can kind of get into it there.

DR. MARK: So, it's not an unreasonable thing to have in mind that needs further discussion, but you would like it done on the basis of discussion?

MR. BUCHHOLZ: That's correct.

Hank.

MR. RAY: One more question?

MR. BUCHHOLZ: Sorry.

MR. RAY: One of your associates discussed the aspects of TMI lessons not learned. Did I miss that point?

MR. BUCHHOLZ: No, that was -- I covered that point by saying, Mr. Ray, that we find ourselves -- the President's Commission and the Rogovin Report did not really emphasize, at least in my judgment, the need for a lot of additional equipment in our plants. It urged us, I think, to understand the plant behavior better and to provide the operator more intelligence information regarding the behavior of the plant. What I'm saying is if that direction is not being followed by continuation of the ATWS situation in openended manner, because I believe that we're going to be expended a lot of our analytical effort and resources both within General Electric and within the utilities in order to perform these optimization studies. And I'm just asking that we appropriately prioritize them. I am hoping to avoid a discussion of probabilities during my talk here, but all I'm saying is there needs to be that discussion so that we're working within, and we're assured that we're working wi-hin, the right framework.

CHAIRMAN KERR: Other questions?

Thank you, Mr. Buchholz.

MR. PFEFFERLEN: Gentlemen, my name is Hank Pfefferlen. I am manager of BWR Licensing Programs for General Electric, and I would like to present to you some of our thoughts, as Mr. Buchholz has pointed out.

On the BWR ability to accommodate ATWS, some comments on the stated Staff concerns and draw some conclusions from these.

So, let me begin by putting up this chart, and let me make a point here that we have been spending quite a bit of time talking about the boron injection mitigation capability, and I think perhaps loss in that is the fact that we believe there's a significant improvement to be derived from the ARI-RTT combination. I think it's our opinion that the Staff has not given proper credit for it, and Mr. Thadani spoke to that point earlier where he pointed out that there is, indeed, mechanical and electrical components and that they had assessed each as a equal reliability function. And by putting in ARI, we eliminate half of it, so we're left with the other half, and we, therefore, conclude that there's a factor of two improvement.

We, on the other hand, believe that the mechanical portion of the SCRAMS system is significantly more reliable than the electrical portion, largely due to the number of rods, and we made this point in 1978 when we spoke to this committee a number of times. And we believe that the insulation of the ARI and RTT leads us to a factor of like 100 improvement rather than a factor of 2, and that there's a lot more to be gained from this.

So, given this, should an ATWS occur, we believe very strongly that alternate 3, based on the assessment that we've provided to the Staff in December, provides a high degree of confidence that the BWR will mitigate such an occurrence without exceeding the specified limits.

Now, let me just remind you of what we talked about in January, based on our assessment, we have concluded that the peak vessel pressures for each of the product lines that we show over here is on the order of the high 1200 to 1300 psi range. I'd like to point out that it is well below the emergency limit, as defined in the SME code, and, in fact, very close to the upset limit, also as defined in the SME code.

Therefore, from the point of view of integrity and operability, we do not see ATWS as a real challenge to those complements.

The point has been discussed quite a bit with regards to the BWR, is what happens to the suppression pool? We isolate the reactor vessel, we put steam into the suppression pool, and that is why we have to initiate our boron system, and indeed with alternate 3, we have concluded that the bulk temperature for each of the product lines will be as indicated.

We feel there have -- test evidence that lead us to conclude that, indeed, 210 degrees is reasonable limit, and even there we do not expect any sudden departures from an acceptable approach. That's where our test data leads us. So we see no cliffs in this, we just see that the pool is condensing steam as the temperature goes higher and higher, this condensation capability is reduced. We do not see an instability in a condensation sense developing with the use of pointures, and I have to make that point. So, we believe that our reports for alternate 3 demonstrates that we can stay within what we believe to be an acceptable suppression pool condition as indicated in our report.

Given these temperatures in the suppression pool, we have evaluated our containment pressure and find that these are the values, and again, in each case, they're well below the design value that exists for the containment.

Thereby, we believe that we've met all the requirements that have been established. We have looked at the radiological consequences.

DR. MARK: Could I ask the suppression pool temperatures there? They are with quenchers -- that's to resolve the bumping?

MR. PFEFFERLEN: Yes.

DR. MARK: Are they then also with 86 gallons per minute, coming on at 2-1/2 minutes boron injection?

MR. PFEFFERLEN: That's correct.

DR. MARK: It acquires that in order to meet those numbers?

MR. PFEFFERLEN: That is correct.

We have also done a radiological assessment, and as I indicated, we have found -- evaluated on the basis of ATWS that we are really not outside other events that are currently evaluated in our licensing submittals.

In other words, it's within other design basis accidents. So, we've concluded from that that there's really no pressure temperature or radiological limits exceeded, and, therefore, in our minds serves as a -- you know, here is a solution, here is a solution to our ATWS problem if we have to go with mitigation. We believe that very strongly. And I guess that's all that needed to be said. We talked about this quite a bit at the last meeting, and so I want to remind you of this point. We believe that we're there.

Now, so far -- that is the capability of the BWR. I'd like to shift now and talk about the St~ff's concern with our assessment, and try to, in the context of what I've just shown you, try to address each one of these.

These are the concerns that were specified in the Volume IV, and at the head of the list is the limit cycle. I would like to defer that to my next slide, so that we can get through this and I believe there's more discussion on the first one.

The second item, equipment qualification, since there is nothing adequately addressed, we believe, based on my previous discussion, that the ATWS environment is not very severe. It certainly is not as severe as a LOCA environment, and, therefore, equipment within the containment, the majority of equipement required for ATWS is outside of the containment, but the majority of equipment inside will not see any new design requirements, we believe, based on our assessment, that we do not have a problem and that this will require some discussion with the Staff to convince them of that matter.

CHAIRMAN KERR: Does that include evaluation of the equipment in the reactor vessel, if there is any, that is subjected to the pressure peak?

MR. PFEFFERLEN: Well, I think the answer to that is yes, in that, we design to upset in an emergency limit, that was my point. The initial pressure peak is very close to an upset condition, which is --

CHAIRMAN KERR: But that has to do with the vessel. You would not necessarily design your instrumentation for that. Perhaps you do.

MR. PFEFFERLEN: Instrumentation -- I believe the instrumentation is also well able to handle that situation, yes.

CHAIRMAN KERR: And the Staff is aware of that or you didn't tell them?

MR. PFEFFERLEN: Well, I think that Mr. Thadani indicated that he felt that this was indeed the case. We may not have communicated the specifics of this.

CHAIRMAN KERR: All right.

MR. PFEFFERLEN: Seven indicated also that there was insufficient information on the reactor coolant pressure boundary component integrity and operability, and our response again is that the pressures are low, and that based on our assessment, and again a significant amount of information has been presented to the Staff and again perhaps some more will have to be in order to resolve the issue, but again we see no problems coming from the pressures that are part of ATWS.

A third concern was ATWS containment loads, not shown to be bounded by design load. Now, when we are discharging into suppression pool, there are certain dynamic loads that are generated, as someone pointed out today, that occur under any kind of discharge in the suppression pool, and we have evaluated ATWS conditions relative to our normal design conditions. And we find that really there's a very small sensitivity to the changes in suppression pool temperature associated with ATWS, and also impact of the slightly higher pressure that we may be discharged at, so that our conclusion, and this conclusion is based on test data that we have discussed with the Staff and also on methodology that has been submitted to the Staff, and used in approval of the non-ATWS conditions; we believe that we have demonstrated that ATWS containment loads are indeed bounded by the design loads, and, therefore, there's nothing new in the ATWS sense with regard to dynamic loads and the need for dyanmic analysis.

Questions were raised on the radiological evaluation of the containment if not isolated early. Our response is that our assessment was done based on the sensing radiation in the Mark III case, and in isolating one level reached the predetermined point, and this is our design, and we believe that we do have -- do meet this early isolation requirement.

I think Mr. Thadani indicated that, perhaps, this is the case. We believe it is the case. And again we do not see a problem in meeting that requirement.

The follow point was that there was insufficient design information provided on ATWS systems. Now, this case, we have provided conceptual information which we believe was adequate to make an evaluation of the adequacy of the approach. We did not, as indicated, provide the details of the design, because we believe that this is a very complex problem. I think it's been indicated several times around this table today that we have to look at ATWS in the broadest not totally from an ATWS point of view. We cannot design a system without worrying about what impact that system may have or a change in that design may have on all the other goings-on that could happen in the life of a plant, so we have approach the design process very carefully, and we did not have the resources at the time to provide this and then have the design changed, so we have taken what we believe to be a logical step in providing a conceptual design and once we get direction that this is indeed the way we go, I think there has to be a very logical and a very disciplined engineering approach to designing the total system to meet the overall ATWS requirements.

And I believe that our conceptual design has indicated there is a path to solution, and that is really what we've tried to get across.

I guess, based on what I've said here, and if you will bear with me a moment on the oscillations, we do not believe that these concerns justify going to Volume IV, namely the alternate 4 type of solution for all plants.

We see nothing in here that says we have not or cannot demonstrate that alternate 3 is acceptable.

I will now turn to discussion of the oscillations. The first item on our list. I can make the point that these oscillations are calculated to occur only in some turbine trip cases, that they are associated with low-power, low-flow operations, powers are less than 25 percent of the rated. They were conservatively evaluated assuming steam blanketing, and this steam blanketing occurred in roughly 10 percent of the core, so that the next bullet, the temperature oscillations are representative of only that 10 percent of the core.

And here we see, roughly, 130 degree peak-to-peak oscillation at a temperature of about 1150 degrees. Based on that, we further found that these oscillations are eliminated automatically as the bore-in begins to take effect, so that they grow and are terminated without the need for any operator intervention or action.

Based on the size of the temperature oscillations, we have not calculated any fuel failure, and again this is for the 10 percent of the core we do not believe there will be a significant amount of fuel failure. The rest of the core, because of low power, is really not experiencing very severe oscillations at all, so our original conclusion was that really in the context of ATWS, the low probability of ATWS, there is really not a problem with these oscillations, however, we had discussed this with the Staff or we have heard that the Staff is concerned about this, and we have looked at possibilities for elimination of these oscillations. And, indeed, several come in mind.

CHAIRMAN KERR: First, do you think there will --

MR. PFEFFERLEN: Beg your pardon.

CHAIRMAN KERR: I know you've calculated, are you convinced that they would occur if one ran a plant this way?

MR. PFEFFERLEN: I am not in a position to answer that. I can only respond that we have calculated them to occur, and I believe that our codes are --

CHAIRMAN KERR: See, you can eliminate them if all you're doing is calculating them, but changing the code --

MR. PFEFFERLEN: We're not proposing to change the code. We're proposing to make changes to the mode of plant operation.

CHAIRMAN KERR: But you see if the code is not accurate and the oscillations aren't really occurring in a physical sense, then the changes you make might produce oscillation, in a case where oscillation isn't really occurring. It seems to me you need to know something about your codes.

MR. PFEFFERLEN: We believe the code accurately models the interaction between the control and on the system in general. We believe that it is telling us how the plant will respond. Now, in my perception I take that as the vis-a-vis if it's calculated. I believe that this is a real --

CHAIRMAN KERR: You think if it's ratified this way it would oscillate?

MR. PFEFFERLEN: I believe perhaps we can get some comment from the people --

CHAIRMAN KERR: I just want to know what you believe. I am not trying to ask you --

MR. PFEFFERLEN: I believe that that's probably what we would see. We would see a sort of a situation.

Eliminating the oscillations, one way to eliminate them is to approach the SMIV's. We have done the SMIV closure case and did not observe these oscillations, and the reason for that is that the oscillations are caused by the pressure regulator from the control under a very low-flow condition, and so we have gotten ourselves into a state where the balance of plant that the pressure controller is causing oscillations. So, one way to eliminate the problem is to isolate the pressure controller from the core.

DR. LIPINSKI: Let me interrupt you at that point. We're talking about two different kinds of oscillation at this point. One is the regulator-induced oscillation which will expect, going directly into the void. The other oscillation you can anticipate is when you close those SMIV's and turn off your research pumps as to whether you have inherent instability in the core.

MR. PFEFFERLEN: I have done that calculation and the --

DR. LIPINSKI: Those oscillations are not present?

MR. PFEFFERLEN: No.

The oscillations -- I have a chart. The next one shows it is a turbine trip and it is induced by the depressure regulator.

DR. LIPINSKI: Now, in any of your starter programs do you start the reactor without the research pumps in operation, go to some power level --

MR. PFEFFERLEN: Yes, we do.

DR. LIPINSKI: -- for instability? It comes up there in terms of oscillation?

MR. PFEFFERLEN: We start under those conditions, but we do not have an instability problem.

Now, let me point out that one of the reasons we see this in an ATWS condition is because we are level drops lower than where our level would be under these startup conditions, therefore, our core flow is lower, and we're in a different reactor state which is more susceptible to these conditions. That's why we do not see them during a startup.

At any rate, closeup of SMIV is a way to get out of it. Now, there are programs afoot to minimize the SMIV closures because that does put a more severe duty on the pool, and so we would like to retain the condenser as a heat sink and have feedwater or other sources of water available to cool, of course. And this is not necessarily -- we have to have a balance now. We have the oscillations. One way to eliminate them may be not as desirable as accepting the oscillations.

But I throw that as a potential source.

CHAIRMAN KERR: But at least you think it's a soluble problem?

MR. PFEFFERLEN: Yes.

That's the point I want to make today that we believe it is a soluble problem.

MR. BUCHHOLZ: If I may, Hank.

MR. PFEFFERLEN: Certainly .

MR. BUCHHOLZ: It's not clear to us that it is a prob.em. Mr. Holland will speak in a few minutes to that po.nt. We don't believe that it is a problem, but if someone conceives that it is a problem and disagrees with us, we believe that there is a way to avoid this perceived problem.

CHAIRMAN KERR: I'm not going to try to improve on that.

[Laughter.]

MR. PFEFFERLEN: The other choice we had was to investigate the use of a greater boron injection ring, a greater rate of introducing boron-10, whether jn48

that be through enriched boron, increased concentration, or increased pumping capacity, whatever mechanism to get added boron into the core early, plus reducing the lay time that we been talking about. A two-minute delay to the start-up of the SLC pumps. We investigated the fact of reducing that delay on the impact of the oscillations. We find that that eliminates them also, and I have a chart here that shows that.

May I very quickly point out that we did look at other poisons, the potential for using a lacquer material. Unfortunately, there we were concerned with an inadvertent injection and the -- not really knowing what the effect would be in the long term, so we -- based on the success of these other -- getting away from the oscillations by these two mechanisms, we did not pursue that much further.

CHAIRMAN KERR: Are you sure you know what the long-term effects of boron would be?

MR. PFEFFERLEN: We believe we do. We believe we have a better handle on those than we do on some of the other potential sources.

Let me put this chart up and as Bob indicated, we will have Kay Holling get up and discuss the oscillations in a little more general term. But I wanted to get the point across that when we look at the enriched boron or the increased boron injection rate in the reduced time, this is the comparison, the before and after. This is for a turbine trip case. This is what was reported in our December submittal, and it reduces some very significant -- excuse me -- very significant flux oscillations.

I think it's important to note that this is neutron flux and that really buried in here is the heat flux, so that when we talked about earlier the temperature oscillation, that is more a function of the heat flux, and that is much more in line with the average power than the neutron flux. However, this is normal boron with the two-minute timer.

If we go to twice the boron-10 capacity, and initiated a 30-second, you can see that the oscillations are gone.

There may be an optimization here initiating it at 40 seconds or 50 seconds will give us a -- will remove this effectively and we have not investigated that that thoroughly.

But here is a solution to the problem of the oscillation. And that is the message that we wanted to get before you today.

Within the constraints, within the definition of alternate 3, you do have solutions, I believe, to all of the NRC concerns.

It's a matter now of working these off.

I think that at this point in time, I would like to --

DR. LIPINSKI: Excuse me.

MR. PFEFFERLEN: Yes, sir.

DR. LIPINSKI: The upper one, I assume, has the bell oscillation that you are attributing the power space to?

MR. PFEFFERLEN: Yes.

DR. LIPINSKI: On your other slide, you list them as SMIV closure as a way for fix?

MR. PFEFFERLEN: Yes.

DR. LIPINSKI: You have that analysis showing in what its effect is?

MR. PFEFFERLEN: I do not have it with me, but it is one of the December submittals, and I do not have a chart on that, but it is one of the NR's transients.

CHAIRMAN KERR: Are there other questions?

Mr. Ray?

MR. RAY: Who is going -- or will someone tell us what your present concepts are on the long-term effects of boron?

MR. PFEFFERLEN: We had not planned to address that today.

My recollection of discussions that I've had on the subject is that based -- as far as we can determine, there will be no long-term effect, deleterious effects, on materials from an inadvertent injection because we do clean up.

MR. RAY: Do you have any comments to make on the magnitude of the cleanup operation after injection?

MR. PFEFFERLEN: We have gotten that information from our customers and understand it is significant. I think it depends largely on the particular plant condition when it happens, whether or not there is tankage available to remove or whether we have to go through a fill-in flush and things of this nature, but there is a very significant potential. But, again, I would defer to some of our customers to address that in more detail.

MR. RAY: Is there any intent on your part to control this injection, to minimize in some fashion the possibilities of inadvertent injections?

MR. PFEFFERLEN: Well, that is the purpose of the two-minute timer to permit --

MR. RAY: To permit intervention by the operator, is this --

MR. PFEFFERLEN: To permit the operator to take action to get the rods to go in, and if the rods inject, then that would give us the inhibit.

MR. RAY: You mean the timer would stop?

MR. PFERFERLEN: The timer would stop, yes.

MR. RAY: Not inject?

MR. PFEFFERLEN: That's correct.

Now, I will make the point once again, and this in no way interferes with the RPT or the ARI function, it's only the SLC. That would be the only thing we would inhibit.

MR. RAY: Do you have any intent in your control from the viewpoint of the operator pertaining to intervention to prevent the injection other than inserting the rods?

MR. PFEFFERLEN: Right now -- again, we have not come up with a detailed design. Right now our intent is to have the timer check the rods after it has run down and to make a judgment there.

CHAIRMAN KERR: Other questions?

DR. LIPINSKI: One more question.

Does your December submittal present the steam flow through the valve as well as the pressure swings on the vessel that accompany those oscillations?

MR. PFEFFERLEN: I believe it does, doesn't it, Gene?

MR. WEISS: Yes.

MR. PFEFFERLEN: Yes, it does.

MR. RAY: Okay, I want to dig into that, thank you.

CHAIRMAN KERR: Other questions?

Mr. Pfefferlen, let me make sure that I understand or that you and I have the same understanding of a comparison between, say, alternate 4-a and alternate 3. As I read a chart which appears on page 11 of Volume IV, 3 requires ARI, so does 4-a; 3 requires RPT and so does 4-a; logic is in both. I assume it's the same thing. One says, "Autos 86 gpm," that's 3; and 4-a says, "Auto high cap," so that's different. Both say, "SD-11;" both say, "analysis," although there's some indication that analysis is different. The difference seems to be then the auto high cap poison, the optimization, and I don't know what that means exactly. Perhaps you don't either.

MR. PFEFFERLEN: That's part of our problem.

CHAIRMAN KERR: And then 4-a says, "containment isolation," and I take it that that is likely to occur independently, I mean if it's necessary and apparently it is, anyway, so your concern, assuming you felt you could live with alternate 3, which I gather was a possibility is over the auto high cap poison, the optimization and the analysis.

MR. PFEFFERLEN: That is primarily the auto high capacity boron, the -- an operating plant has an installed system, and we are talking about --

CHAIRMAN KERR: I understand.

MR. PFEFFERLEN: OKay.

CHAIRMAN KERR: That is the concern? I just wanted to make certain that I understood what your concern is.

Thank you.

MR. PFEFFERLEN: Now, at this point in time, I'd like to ask Kay Holling to come up and to address a little bit more detailed the oscillations. When he is finished, I will come back and conclude our presentation.

I'm not quite finished yet, but I think this may be a good time to turn it over to Kay.

CHAIRMAN KERR: How many minutes of oscillation will we see?

MR. PFEFFERLEN: How many do you want to see?

MR. HOLLAND. My short discussion should focus primarily on some of the detailed points associated with the oscillation. Let me talk to it in the context of what the volume 4 contention statements reflect and what our response to those might be.

First, there is some discussion of severe power and flow oscillation. I would like to try to put that into perspective as a beginning point. We do see large neutron flux variations as was shown in the previous slide. And the heat flux on the other hand is relatively small. It is of the order of the 20 percent peak to peak of upgraded heat flux. The events occur only for one type of event, the turbine trip type event, and as Dr. Lipinski indicated it is a result of a control interaction and not the inherent instability of the reactor at that point. It is also applicable only to one particular product line. So it is very limited in the potential, or the prediction of its occurrence.

I would like to point out that the flow oscillations really are not severe either. They are less than 10 percent peak to peak upgraded flow, given this oscillatory condition for those few cases where it occurs.

The report also suggests that oscillations have been observed in operating reactors. In the literal sense, that is true. I would like to put it in perspective, however, that we have observed no such oscillations in an operating

GE BWR. I have personally participated in some testing in an overseas plant in which we set up conditions intentionally trying to establish an oscillatory condition and did some measurements leading to transfer function determination for the plant and so on.

In that situation where we did have an oscillation it was a local oscillation within the core around one fuel bundle which we had removed the inlet orifice and had arranged the control rods in such a fashion to get a power pattern that would yield oscillatory conditions locally. And, of course, that oscillation was also observed in other parts and portions of the core as a result of the neutronic inter-tie.

Also stated was a concern with our predictive code capability. And I guess I have lived with these for so many years that I--and seeing the response of the reactors confirmed in general by their predictions, that I am a little more generous than others might be. REDY, for example, has a very good history of conservative predictions compared to performance.

MR. KERR. What is a conservative prediction?

MR. HOLLAND. It over predicts the response, is that I mean by that. It is designed in terms of the way we input to the code and--

MR. KERR. It always over predicts, no matter what the situation is.

MR. HOLLAND. I will not go that far, no. There are certain variables, for example, which depending on the input assumptions, will not over predict.

MR. KERR. I fail to see why people feel good about a conservative prediction, except in the context of licensing.

MR. HOLLAND. Yes.

MR. KERR. --because it is very important. It seems to me you would want an accurate prediction.

MR. HOLLAND. And that is the main reason we have gone to the odyn code which is a best estimate, a more-- I referred to odyn here, in a sense that redy very nearly approximates or is equivalent to the odyn results; which odyn is a 1_D--push this up a little.

MR. KERR. Why do you suppose the staff expresses their concern about predictive capability then when you are so convinced that they do a good job.

MR. HOLLAND. Well, the main reason here is that-- and I think they refer to the Peachbottom Test as being one example. What happens is the initiating event on a pressurizing transient causes a pressure wave to come back into the reactor vessel which causes the collapse of the voids and a neutron flux spike. Now the nature of the model is that the steam line is represented differently in the two models, and the timing of that pressure wave is different.

The reason the net result is the same in the sense of--or nearly the same in the sense of heat flux and pressure response over the course of the event--is that even though the pressure wave may come in differently, the modeling of the reactor core and the total model of the power flow loop is such that the integrated energy effect is essentially the same. And so we see very comparable net response for the reactor, even though the initial neutron flux--

MR. KERR. I guess I don't know what a net response is. It seems to me if you wanted to predict a pressure and behaviour with time, it would be sort of important that you could be able to do that, independent of what the net response of the reactor is.

MR. HOLLAND. That is true. The pressure response in the reactor is essentially the same with both models. Let me cite some approximate numbers from the report. When we did the analysis both with odyn and redy, in the odyn case the neutron flux showed a peak of the order of over 600 percent; whereas redy predicted-- I guess that is just backwards. Redy predicted 600 percent and odyn predicted 700 percent.

And in that context they have suggested that it is a non-conservative. However, if you look at the corresponding heat flux peak they are within 1 percent, 133 to 134, as I remember the numbers. And similarly that peak reactor pressure which is a few seconds beyond that in time occurs within a very few psi of each other. The key parameters which impact the performance of the reactor are very similar in behaviour for the two codes. I don't know if that is given the proper perspective there, but I want to make a point that I don't think in the context of an oscillatory behaviour that we will see, when we get the odyn code working in that mode, we will see any significant difference in the results using odyn than we do with redy. That is my own personal opinion.

Another point was the impact on control systems. We have already suggested that the controller on, pressure controller, is a contributor to the response that we have observed in the previous case. In actual fact, for the events we are concerned with, that is the only controller that is in service. And then only on those events which are not isolation events. The feedwater controller is out of the picture. So is the flow controller because we have tripped the pumps. So the only control interaction would be with the pressure controller. The only time it interacts is when the power level is at approximately the capacity of the bypass system. And that is the reason we saw the oscillatory condition in that case.

I have one more slide here. The concern about fuel integrity has been raised, during the oscillation. The really expected case is that very few rods will enter transition boiling, during this condition. In the initial spike we get into conditions which would be under transition boiling conditions.

MR. KERR. Am I remembering the wrong thing? I thought the staff's concern had to do with pellet clad interreaction and not so much with departure from nuclear boiling.

MR. HOLLAND. I think they have both concerns if I understand. I will address the PCI in a moment.

MR. KERR. Oh, I am sorry.

MR. HOLLAND. That is a separate point.

MR. KERR. From where I sit I am missing that.

MR. HOLLAND. We expect that there will be very few rods will enter into the transition boiling, for this event. And the reason is that after the initial transient where you may go into transition boiling, by the time you get to the point where oscillations were to occur the conditions are most likely to be such that you have no further transition boiling conditions. If they do appear it would be only in a very few of the hot rods, hot bundles cases. And even given the condition of transition boiling we do not equate that to failure. We do not believe that failures in the cladding would occur.

What we presented in our report makes a somewhat more conservative assumption, in which we assume no rewetting after that initial transient and that the average temperature as previously presented was about 1150 with oscillations of 130 degrees, about that average. In that situation, approximately 10 percent of the rods might be in transition boiling and, again, only a very small fraction of those might be expected to fail. All of our discussion of radiological consequences have been done on the assumption of 100 percent failure. We are very far away from that assumption of 100 percent failure.

MR. RAY. Are these temperatures calculated by your code; or did you have in-core thermocouples in that test reactor delineator on the other side wherein you induced localized oscillations that might have given you a measure temperature.

MR. HOLLAND. These are calculated temperatures. These are the clad temperatures as calculated by our codes.

MR. RAY. In the case of the plastics, do you make any measurements of the plastics?

MR. HOLLAND. Not of temperature, no. The few fatigue event failures that might possibly occur, according to our fuel people, they declare that there would be absolutely no prospect of that failure being such as to cause any flow blockage. We believe that that is a realistic criterion for this application.

With regard to safe shutdown equipment, and the impact of the Atlas transient oscillations on that, I think that has already been addressed. I won't spend too much time on that, but we are within the service level C or the emergency limits and the equipment is expected to act very close to service level B. And we expect no consequence to that.

The parameters of concern, in fact, there are pressure, flow and temperature, and they are very well behaved. It is the neutron flux is the one that is oscillating in through rather large oscillations.

The last point is relative to the pellet clad interaction, and I think the report suggests something the order of 10 percent potential pellet clad interaction. Again, I am reflecting the rather strong feelings of some of the fuel designers. The linear heat generation rate which is one of our primary measures of the pellet clad interaction phenomena--we would be oscillating with an amplitude of about one kilowatt per foot, about a four kilowatt per foot average.

The criteria for pellet clad interaction concern is at about the 9 kilowatt per foot level where we would be concerned with the potential for pellet clad interaction to be of consequence.

Under the oscillatory conditions the fuel designers say we are far from any potential for pellet clad interaction consequence for the oscillations that we have represented. That is a summary of several of the points. I am sure there are others, but I kind of wanted to put in perspective what some of the concerns were relative to the oscillatory situation.

MR. RAY. The assumption you made that led to this potential limited fuel failures was no rewet. How extreme a conservatism is that?

MR. HOLLAND. That there is no rewet?

MR. RAY. Yes.

MR. HOLLAND. As I understand it, the rewetting is likely to occur at around 800 degrees condition. We go to temperatures that may be, during the first spike--that may be in the range 14 to 1600 degrees. During the next mini seconds before the oscillation sets in it is our perception that that temperature will drop down to the range and near or below the 800 degree threshhold and we believe that in the real world rewetting is most likely to occur, although we haven't claimed it.

MR. KERR. Question?

MR. LIPINSKI. On your linear heat generation, oscillations, is that 1 kilowatt per foot plus or minus? Is that peak to peak?

MR. HOLLAND. That is peak to peak.

MR. KERR. Other questions? Mr. Pfefferlen.

MR. PFEFFERLEN. Thank you. I would like to take just a few more minutes to finish my presentation. I would like to turn to the subject of value impact and perhaps in this case start out with value and address--there was a lot of discussion about the probabilities and things of this nature.

I would like to share with you our perception of what we have got, and what I have done is I have listed the NRC values as we see them for the approximate frequency of unacceptable Atlas consequences.

This begins with a $2^{t}10$ to the -4 Atlas probability, an improvement of about a factor of two going to ARI, roughly a factor of 10 additional going to alternate 3 and another factor of 10 in going to alternate 4. That is how the staff arrives at their 10 to the -6. Now as I have indicated earlier, we believe there is a significantly larger benefit to be derived from ARI. Namely, starting with their value we believe we can get down to very close to the 10 to the -6 just be the scram system improvements and that the next steps will drive us down below that value.

Now in the context of a statement in NUREG 0460 that said that the--based on the staff's assessment--that the current Atlas risk is approximately twice the total risk for non-Atlas events; we believe that reductions of that order of magnitude have, indeed, driven Atlas down to a point where we--relative to the other risks that have not been affected--believe that we have accomplished the goal. So, as I say, these are the extremes over which we have been discussin the needs to go beyond alternate 2 or beyond alternate 3. I am not using this to make any argument, although I think--other than to say that we believe that alternate 2 has done the job and if mitigation is required we question going beyond what we have indicated here as alternate 33 (a) fix.

That is kind of the value of it. I have the same basic charts but I have added to that some impact figures. And, again, various fixes but, this time, what does it cost to go each step of the way?

The alternate 2, the ARI and RPT, we see as being in the range of 1 to $2 million. These are now total costs now direct plus indirect. Take the next step we see a 12 to $15 million impact. And going to alternate 4 because of the larger pumps, because of the safety grade and liability requirements that were inferred and I understand Mr. Zane has indicated some possibility of derating from that. But our concern is that that starts looking like very large dollars and in some cases, depending on the plant, there is a potential for a very significant impact. So that we see the potential impacts going up quite rapidly as we move away or move beyond this alternate 3.

And conclude from that that the alternate 4, 4 (a) really is not cost effective.

MR. MARK. Are those costs for presently operating plants, or for plants which have yet to go in operation.

MR. PFEFFERLEN. These are presently operating plants.

MR. MARK. They would be smaller.

MR. PFEFFERLEN. They would be smaller. Yes, certainly these would be--you know the larger end of this one would certainly come down.

MR. MARK. Even alternate 3 (a) would be smaller?

MR. PFEFFERLEN. Yes. Of course, as you get down here you are using more and more existing equipment and, of course, the delta will be smaller.But the familiar impacts are out there with the operating plants.

I would like to now summarize the message we have tried to convey to you today. First of all, as Bob Buckholz presented, we don't believe volume 4, the approach, really provides closure to the Atlas problem. It provides another step and we believe that closure is available with what we have analyzed to day that we do have a mechanism with alternate 3 to satisfy the staff criteria. In that context, satisfying the criteria, we believe that the limit cycles can be eliminated and that the other NRC concerns can be resolved. So we see no basis for going to alternate 4s. We don't believe it can be justified in the case of the BWR. But the solutions that we believe we have presented to the staff and are willing to discuss with them in some more detail.

That is our summary, and from that we conclude--and would like to recommend 0460, volume 4, should not be endorsed as it now stand but rather that if something needs to be done in a near term that if something that should be done there is an RPT and an ARI. That provides, in our mind, a very significant reduction in the risk associated with Atlas. I think we can all agree that it is a reasonable step to take and if the implimentation is going to be by order, that this makes a reasonable near term solution.

And if, indeed, going beyond that is necessary, that we shoulc--the recognized or documented assessment of that was, for alternate 3, should be recognized in coming up with the final solution. And that, further, that that solution should not be implemented until all the concerns across the industry that we have resolved the total picture of Atlas to avoid getting into a situation of making a fix now which will have to be modified some later date and move off in some other direction. Because of the concerns that I had indicated on the detail design and the efforts associated with making changed to existing plants.

So we believe this is--if we have to move it towards foreign mitigation, this is a reasonable approach to that end, and one that should be taken under consideration.

That concludes my presentation. Any questions, I would be glad to answer it.

tape 10

-IV-92

CHAIRMAN KERR: Let me ask in connection with the third bullet, is it documented BWR mitigation capability should be recognized, by whom?

MR. PFEFFERLEN: By the Staff in doing their assessment of what comes next. Our submittal --

CHAIRMAN KERR: When you say document, you mean the Staff should recognize what you have already submitted?

MR. PFEFFERLEN: Our concern being we submitted it and they have responded with Volume 4, which seems to --

CHAIRMAN KERR: You consider Volume 4 non-recognition, and you'd like recognition?

MR. PFEFFERLEN: We believe we've demonstrated -- you know, we've provided the necessary early verification, and with our discussion here the essential resolution of conccerns, therefore --

CHAIRMAN KERR: Are there parts of the presentation today that Staff has not seen previously?

MR. PFEFFERLEN: I believe they are aware of the information that we've discussed with you today.

CHAIRMAN KERR: But they don't have it fully documented --

MR. PFEFFERLEN: They do not have it fully documented.

CHAIRMAN KERR: Thank you.

MR. RAY: Have you had this dialogue with the Staff since you submitted with documentation they have and the issuance of Volume 4?

MR. PFEFFERLEN: We have talked to the Staff, yes.

We have not talked -- in my mind, we have not had an opportunity to discuss with them prior to their decision on your closing Volume 4, but we have talked to them on some technical details.

MR. RAY: Are you implying that the decision was made before you talked?

MR. PFEFFERLEN: That's my observation, yes.

DR. MATTSON: That's not facts of the situation.

CHAIRMAN KERR: I don't think that we need to establish that. That's irrelevant.

DR. MATTSON: I haven't been at many meetings, but I was at one of them.

CHAIRMAN KERR: Mr. Mattson, did you say that you had a question?

DR. MATTSON: Yes, could we go back to -- to the difference between present, normal and two? It's either three or four slides, either one. It's on both slides.

How does one get factor of 100 increase between present and alternate 2?

MR. PFEFFERLEN: We believe that the mechanical system is at least that more reliable than the electrical system, thereby putting an ARI, in essence, eliminates electrical system and we start seeing the number closer to the mechanical.

CHAIRMAN KERR: Square with -- technique. The overboard we talked about a year and a half or two years ago --

MR. PFEFFERLEN: That's obvious. That's in our submittal.

CHAIRMAN KERR: What the Lewis committee said.

MR. PFEFFERLEN: We're not talking about the 10^{-6}. We're talking about a factor of 100. I've chosen, to start with, your numbers at the starting point. We're just looking at deltas to that.

MR. THADANI: You didn't start with our number on total SCRAM system. You started with that number as being impicable to electrical portion, not the idolic mechanical portion. In the idolic mechanical portion you applied these firm -- techniques to try to show that the unreliabilities on the order of 10^{-9} in your report BWR SCRAM system reliability effect and what Roger was saying is that that technique if it were reviewed extensively by us, reported in NUREG-0460 in II, and I believe --

MR. PFEFFERLEN: That's right, but the point I am trying to make is that is what we're talking about here is the absolute numbers may be -- 10^{-9}. I'm talking about relative, one relative to the other, that we can eliminate part of it if we see the improvement. The improvement is a little bit different. I am giving you this as our profession. I believe the factor 2 is overly conservative, and we believe that there's indeed a significant improvement to be derived from the ARI.

CHAIRMAN KERR: Earlier I heard some criticism of the Staff for not having a technical basis for concluision. Are you saying that you get this character of 100 just on the basis of a feeling?

MR. PFEFFERLEN: No. We submitted a -- sense of liability assessment --

CHAIRMAN KERR: What was it, the square with bounding technique?

MR. PFEFFERLEN: The bottom line numbers were. But it also evaluated --

CHAIRMAN KERR: Now let's not play games. The bottom line numbers are pretty important here. The numbers are very small.

MR. PFEFFERLEN: But they're important if one is making comparisons between small numbers, in comparison, say, to rely on the numbers, if the comparisons are to mean anything.

CHAIRMAN KERR: What of the reliability assessment of the failure modes, and the factor assessment which a lot of us look at. Where are the failure modes in the electrical system versus the mechanical system?

MR. PFEFFERLEN: And as a result of that, we were able to establish a relative reliability.

CHAIRMAN KERR: I think the Staff agrees the mechanical system is probably more reliable than the electrical system. Now, where do you go from there?

MR. PFEFFERLEN: To get this improvement that we're asking about.

Well, that is why we're not arguing today that we stop at alternate 2. We have agreed --

CHAIRMAN KERR: You're saying --

MR. PFEFFERLEN: I'm saying that we're getting a lot more than we're getting credit for.

CHAIRMAN KERR: Do you or do you not believe the number that shows there opposite alternate 2?

MR. PFEFFERLEN: I believe, based on our assessment, that is a number that is indicative --

CHAIRMAN KERR: Oh, come on now. Do you think that's a good number or not?

MR. PFEFFERLEN: Yes, I think it s a good number.

CHAIRMAN KERR: Okay.

Do you have some technical basis for it?

MR. PFEFFERLEN: I am not a reliability expert.

CHAIRMAN KERR: I didn't ask if you were an expert. You must have some technical training. You may not have done the calculations yourself -- but do you believe it's a reasonable calculation, have you seen it, or are you skeptical of it?

MR. PFEFFERLEN: I am not skeptical, no.

CHAIRMAN KERR: Okay.

MR. RAY: Did you submit support for that figure?

MR. PFEFFERLEN: Yes, it has been submited.

CHAIRMAN KERR: And it was the square root value technique?

MR. PFEFFERLEN: I cannot even say that for sure.

CHAIRMAN KERR: I guess we haven't answered your question.

MR. THADANI: We know the answer.

CHAIRMAN KERR: Okay.

Are there other questions?

MR. DITTO: I'd like to ask Mr. Pfefferlen what he thinks a reasonable figure for just putting ARI into the -- portion. What do you think that would be? If it's not 100, you say --

DR. MATTSON: I've been sitting over here trying to make a number myself, and I can't come up with a factor of 100.

MR. DITTO: I believe that's true.

DR. MATTSON: It might make a very strong argument for factor of 10, but I would have to make the same argument. I'm not a reliability expert either. I'll pass it to mine.

MR. SAUNDERS: I don't think this requires a reliability expert, Mr. Chairman, to determine this. It depends on what your substance are. You can make factors of 100 with a pencil, but I don't think you can any other way.

DR. MATTSON: In alternate 3, what we call ARI what?

MR. THADANI: Yes, we did recognize that ARI did offer an improvement in the reliability of the total plant system, but if we look at the electrical portion we thought that including -- hours significantly reduced what -- electrical portion -- common cause.

On the other hand, as you well know, we've had a number of discussions in the past as to the methodology that one might apply -- mechanical portions.

Industry has -- followed the method that was used in the reactor safety study -- we disagree with that method, and somebody can invite him to -- an alternative method which may not be any better, but it might well be thought -- that's exactly what we're after. But, again, I think from a licensing point of view, the type of model that we use seems to be a more reasonable model applied to a -- like this. We try to look at it -- we will find some failures and some -- we didn't find some places with more than one -- but we identified very -- that we did not have a good enough -- and we still don't. I fully justify whatever -- obviously there is room for disagreement -- it gives you --

DR. MATTSON: That's right.

CHAIRMAN KERR: Any further comments?

MR. BOCHMORT: Let me thank all of today's participants for your participation and your patience.

This is a tough question and I hope we contribute to it. We do plan to consider this question at the meeting in April, and I don't the day on which it is scheduled.

Is April tenth a Thursday. Tentatively scheduled right now. It starts at 2:30 with a subcommittee report, and 3:30 to 6:15 for a meeting with the Staff.

CHAIRMAN KERR: I declare the meeting -- We will proceed to executive session.

[Whereupon, at 3:45 o'clock p.m., the committee continued to executive session.]