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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

COMBINED SUBCOMMITTEES ON
SAFETY PHILOSOPHY, TECHNOLOGY AND CRITERIA
AND
RELIABILITY AND PROBABILISTIC ASSESSMENT

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UNITED STATES NUCLEAR REGULATORY COMMISSIONERS'

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

WEDNESDAY, DECEMBER 4, 1985

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

COMBINED SUBCOMMITTEE ON SAFETY PHILOSOPHY,

TECHNOLOGY AND CRITERIA

AND RELIABILITY AND PROBABILISTIC ASSESSMENT

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

Wednesday, December 4, 1985

The combined subcommittees met at 9:30 a.m., Dr. David Okrent presiding.

ACRS MEMBERS PRESENT:

DAVID OKRENT, ACRS Member

JESSE EBERSOLE, ACRS Member

FORREST J. REMICK, ACRS Member

CHESTER P. SIESS, ACRS Member

CARLYLE MICHELSON, ACRS Member

HAROLD EHTERINGTON, ACRS Member

MR. DAVIS, Consultant

PROCEEDINGS

DR. OKRENT: Good morning, gentlemen. The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards Combined Subcommittee on the Safety Philosophy, Technology and Criteria and Reliability and Probability Assessment, David Okrent, the Subcommittee Chairman.

Other ACRS members present at the time are Mr. Ebersole, Mr. Remick, Mr. Siess, Mr. Michelson, Mr. Etherington and we have a consultant, Mr. Davis.

The purpose of this meeting is to meet with the EDO and discuss outstanding issues relating to the NRC position on a revised safety goal policy and to meet with the NRR Staff to discuss the results of the Millstone probabilistic safety study.

The discussion on safety goal study will be held in the morning; the discussions on Millstone 1 will be held in the afternoon, Dr. Savio is the cognizant ACRS Staff member for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on November 25, 1985.

A transcript of the meeting is being kept and it

will be made available as stated in the Federal Register notice.

It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

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

We have a proposed agenda for the meeting. I should note for the benefit of the Subcommittee members that didn't make the last Subcommittee meeting, at the time Mr. Stello asked that we advise in writing a little bit of definition for areas that we thought maybe they should focus on in today's meeting, although the meeting is by no means confined to the topics listed here.

In any event, an effort was made to put something on paper. Much of it evolved from questions raised at the last meeting but we welcome additional points by the Subcommittee.

Are there any comments on the proposed procedure for the morning?

(No response.)

DR. OKRENT: By the way, I should note that at least the last version of the full Committee agenda that I read showed that there would be discussions by the full Committee I think on Friday on matters related to safety

goal policy to give the full Committee at least a beginning on trying to see whether it had opinions, Committee opinions, that is, on some or many of these topics that it might be able, in fact, to formulate and it would be a separate decision as to whether or not we transmitted anything at this meeting.

But in part this arose from a comment that one of the Commissioners made that it seemed that the full Committee hadn't reached concensus positions on a variety of the topics involved; in fact, we did write a letter, my memory tells me, in July but I may be off a month or so, on portions of the question of safety goal policy but there were a range of things that we didn't address and the Staff has come up with some new proposals since.

In any event, why don't we begin? Who's the first spokesman for the Staff?

Mr. Stello?

MR. STELLO: Some administrative things: we have some people who have to catch airplanes so we're going to go through responding to some of the issues out of the order in which they are listed. I don't think that should be a problem.

DR OKRENT: No, there is no meaning to the order.

MR. STELLO: One other matter: there is a

document related to this matter that we have sent to the Commission that identifies some areas that we think the Commission needs to focus on. I have had copies of that made and will pass it out to the Committee with the understanding that that is a predecisional document and one that we are not going to put into the public record, and I assume that that is an acceptable way to proceed, to give copies to the Committee with the understanding that that is not yet a public document. I assume that will be acceptable.

DR. OKRENT: As far as I know that's acceptable by the NRC legal rules. Will we be able to have that during the morning?

MR. STELLO: It is being copied as we speak.

DR. OKRENT: Good. Thank you.

MR. STELLO: It was signed out, I believe, yesterday or the day before.

I think the document, for anyone in the audience who's interested in the questions and issues that are in that document, are no mystery; they will be very much the same questions and issues that we're talking about here.

To begin, I would like Mal to go through some of the issues that have been raised.

But let me ask the Committee: would it be useful to take five minut s to present the concept that we are

talking about so that the issues that you have provided us in your memo, Dave, will have more of a context? Do you think everyone understands --

DR. OKRENT: I think it would be helpful to have five minutes.

(Slide.)

MR. STELLO: Let me put a slide up just to describe the concept and let me make sure it is clear that it is a concept for safety goals -- a safety goal, let me call it the safety goal. If everybody can read it, what I mean by the safety goal is the matrix that is on that slide.

To give you a thumbnail sketch of what that says, any facility that has a PRA analysis done and you are trying to decide did it meet -- quote -- the safety goal, one would look at that matrix and see if he is satisfied wherever he fell in that matrix and, if he did, one would then conclude he has met the safety goal.

Starting with some key points. One of the issues we're going to talk about later in depth is for future plants, new plants, where ought you be in terms of this matrix, and that's the top line of the chart which says for new plants we would expect you to meet the less than 10 to the minus-five for a large-scale core melt frequence and we would expect that you would meet the health effects and hence you would enter the right-hand column of the table,

which would indicate that no further fixes would be required. It's a way to describe for the future when enough is enough.

A copy of this is in the handout.

The bottom of the chart tries to come to grips with the other end of the question of when you need to say you have to fix this facility and cost is not a consideration.

It's equivalent to the no undue risk standard when you violate it. That says if reactors come in with a large core melt frequency greater than 10 to the minus-3 and they don't meet either of the health effects -- now remember the health effects are both latent cancers and early fatalities in tenth of percent numbers. If you fail to meet either one of them, then you are required to modify the facility. And cost, you will notice in the right-hand column, it says "no limit." And it means cost is not a consideration, you must fix it.

Continuing then with the bottom line, if you do meet the health effects then clearly there ought to be lesser of a regulatory concern, but still the need to fix is there and it is large. And that's where we introduce the concept of the averted costs, on-site costs.

And we say, in deciding how much money ought to be spent on that facility, if you're in that range, you take

100 percent of the averted on-site costs and the costs associated with $1000 per person-rem and those two and that's the total amount of money that you would spend in getting that facility fixed.

The remaining two elements of that matrix, for which most reactors that have PRA's completed fall, describe -- depending on whether you meet the health effects or not -- how much money ought to be spent for that purpose, and that purpose being to cause the facility essentially to reduce the core melt frequency to a lower value than they have.

There is no fundamental laws that govern how to select the fraction of the averted on-site costs. The attempt was to try to take a snapshot of how much of it ought to be included and under what considerations.

The first principle that I used was to say Look, if you did meet the health effects then clearly there is a bigger safety concern and, under those conditions, you will notice it says 100 percent of the averted on-site costs ought to be included in the equation as well as $1000 per person-rem.

On the other hand, if you are meeting the health effects then it ought to be lesser. And if the core frequency is more favorable, even lesser.

It was trying to take into account the idea of

the frequency of the core melt and whether or not you are meeting health effects. It was trying to wrap up all the concepts into one.

Now this is a different approach than we have had thus far because there is no single number that says you have to meet the single number, you meet the matrix of wherever you are. And we would drop the quantitative numbers as separate identifable goals that would be embraced and embodied in the matrix in trying to bring all of it together.

Now I have described a lot of things very quickly here. The questions that we will be responding to this morning are pretty much I think it's fair to say derived from the discussion previously. So I thought it useful to start with what this table says and if there are any questions as to what it means maybe it would be good to talk about those and then when we get into the --

DR. OKRENT: Not a question about what it means but the Staff has frequently said that PRA's should be used primarily for their insights and you should stay away from the bottom line use of PRA's, and this is clearly a bottom line use. Should I draw any inferences?

MR. STELLO: No, you shouldn't draw any because that same issue is the one that existed with the safety goals we've been talking about for the last four years: how

to use the safety goal in terms of taking that understanding from PRA results is in fact what the discussion has been for the last four years. This is another way to look at how to do that.

MR. EBERSOLE: May I ask a question?

Vic, it seems to me the whole hole in the system is the quality of the critical values in the first column. We, for the last two days, heard about the potential effects of non-consideration of common mode failure -- like plus 10 percent, always positive, to make them worse -- about the non-inclusion of a host of influences in most of the PRA's.

So I think the void in the whole piece of logic there is the quality of the numbers in the first column and the absence of a fourth column that says how you rationalize the whole quality of those numbers.

MR. STELLO: That's one of the major topics and probably the first one we'll talk about. That's listed in the questions, so if you don't mind we'll put that off.

MR. EBERSOLE: Sure.

MR. MICHELSON: Just a clarification. I asked this at the last meeting and just I want to make sure I understood it correctly:

As I understand it if I, for instance, have a core melt frequency greater than 10 to the minus-3, I'm supposed to start fixing it without regard to cost. But

it's my understanding that I only need to fix it enough to move into the 10 to the minus-4, 10 to the minus-3 range and then I'm allowed to re-evaluate whether I want to spend more money or just leave it there or what, and I use the new formula for the next step higher.

MR. STELLO: That's correct.

MR. MICHELSON: So I step my way through the chart until I get to where I want to be, wherever that might be, is that correct?

MR. STELLO: That's correct. And the numbers, as you move up on that chart, starting at about 10 to the minus-3, are on the order of $100 million, which would be derived from the right-hand column for further fixes.

MR. MICHELSON: But I don't fix the problem because I end up with a number like 10 to the minus-2 to begin with, I only do enough fixing to move the core melt frequency on up into a more favorable range?

MR. STELLO: And then the matrix tells you how much more fixing is necessary.

MR. MICHELSON: And how much money I can now spend.

MR. STELLO: That's correct.

MR. MICHELSON: I think that's an important clarification and I wanted to make sure I understood it.

MR. STELLO: That is the intent.

MR. DAVIS: Are these per-plant or per-site?

MR. STELLO: Per-plant.

MR. DAVIS: So multi-plant sites don't do anything different?

MR. STELLO: No. It is the intent that each plant will meet this matrix, depending on where it is. And by definition that's necessary because you do have plants at sites for which there are two or three plants and I'm sure that the PRA results for those three plants -- when there is one built in 1970 and 1978 and one in 1985, PRA results will in fact be significantly different.

MR. DAVIS: But you could have a multiple site that in combination didn't meet the goal but individually met the goal, that would be an acceptable --

MR. STELLO: The intent is to make it per plant.

MR. EBERSOLE: On the multi-plant question, if a plant degrades through core melt or even a containment failure after that, there's got to be some sort of basic premise that the contiguous units go to the same state or do not. What position do you take and on what basis?

MR. STELLO: If they are all identical, then by definition they would go to the same state. If they were different, then they would go to whatever the difference indicates --

MR. EBERSOLE: There has been no difference in

the design of the plants to make them any more protective of the failed plant nearby from the beginning. We've all based our plant shielding and other --

MR. STELLO: Maybe I misunderstood.

MR. EBERSOLE: I'm saying if a plant goes to a containment fail state and a core melt state, what is the rationale on the state of the contiguous units?

MR. STELLO: I'm not sure I understand the question.

MR. EBERSOLE: Well what do you know -- since the operators presumably can't stick around and run them down themselves, where do they go?

MR. STELLO: Let me see if I understand the question.

DR. OKRENT: One reactor has had an accident and the second and third are not currently designed to cope with a very bad release in the first one, so therefore you might --

MR. STELLO: Well the only experience we have with that would be TMI and if you recall --

MR. EBERSOLE: That was a very good experience.

MR. STELLO: -- TMI was that the other unit had to shut down and stay shut down and was shut down for seven years.

MR. EBERSOLE: No, but it was not damaged.

MR. STELLO: No, TMI, there are two units at the site, one had the accident and the other did not.

MR. EBERSOLE: But it was operationally shut down and stayed that way.

MR. STELLO: Yes. If there were operating --

MR. EBERSOLE: No, I'm talking of the case where the human beings had to leave.

DR. OKRENT: Let me make it clear again: we've got two reactors both running at full power. The first one has a PWR 2 release. In the old picture, WASH 1400, okay, the second one is not designed for a PWR 2 release if so the wind is blowing the right way the people there may empty the plant, may be okay; if it's going the wrong way, they may get a pretty high dose and find it untenable to stay, this is Jesse's point.

MR. STELLO: What does that have to do with safety goals? Are you connecting it to safety goals or is it just a question outside the context of safety goals?

MR. EBERSOLE: I'm saying does this become a multi-unit core melt and containment failure in view of the fact that people cannot stay there any more?

MR. STELLO: Well if the people couldn't stay there any more then they would have to make whatever arrangements they could make.

MR. EBERSOLE: That's not in the current

provisions of designer operations.

MR. STELLO: Well the current provisions of the design require access to the control room.

MR. EBERSOLE: Not under these conditions.

MR. STELLO: -- for a TID 14844 release --

MR. EBERSOLE: -- which is completely cancelled by the nominal containment leakage rate.

MR. STELLO: If you had a containment failure sequence beyond that, you would be into the casualties and the Class IX accidents and those are all being developed for the facilities now and I couldn't speak to how each one --

MR. EBERSOLE: I think you just have to concede you have a multi-unit consequence rather than a single-unit consequence.

MR. STELLO: I wouldn't concede that until I had the specifics. It may be, but it may not.

MR. EBERSOLE: Well in the absence of anything to the contrary, you have to examine it and fix it.

MR. STELLO: Yes. I don't -- it isn't clear to me that that is in fact obvious.

MR. MICHELSON: Is it safe to assume that that is part of your determination of averted on-site costs for a given event?

MR. STELLO: The averted on-site costs are per plant, and if the damage at that plant was going to create

a problem for the other plant then I guess you would have to do it on a case-by-case basis to understand it.

MR. MICHELSON: But you would have to do a complete study of the site, I assume, to determine how many billion dollars would be potentially lost from the site. That's what's averted then with the fix.

MR. STELLO: Yes. And if it's a two-unit site then the 100 percent averted on-site costs would double.

MR. EBERSOLE: That's what I was after.

MR. STELLO: In that case then you would be multiplying -- or you wouldn'y multiply but 100 percent of the averted on-site costs would include then that consideration, if that was the question.

MR. EBERSOLE: By and large I think most plants would survive because the people might manage to stay in there, under considerable duress, but I'm not sure.

MR. STELLO: I think for even most core melt sequences with late containment failures that would be the case. If you had a very, very low probability early containment failure, without looking at it, I don't know that you know the answer.

MR. MICHELSON: Because you have to look into the reason why you got into that degraded a condition to begin with. In a shared control room, like Sequoyah, a one-unit meltdown is probably a two-unit loss.

DR. OKRENT: Well I think we had better go on.

DR. REMICK: One more question, if I may:

You mentioned $100 million. Where did that come from?

MR. STELLO: It was just -- it's not a precise number. What I did, you take 10 to the minus-3 and on the order of about two or three times 10 to the six person-rem release. You wind up with on the order of $2 million per year for 30 years, which is $60 million. If you take the 100 percent averted on-site costs and you use on the order of -- I don't know what the number is -- and you multiply that by 10 to the minus-3 and you come up with another million dollars per year. Times 30 years, that's another $30 or $40 million. So it would be $100 million. That's assuming that you have met the on-site -- the health effects.

It's just a crude benchmark to give you a rough idea of where these numbers -- and for the purpose of generating that rough number is to show that when you're down to the order of 10 to the minus-3 that the averted on-site costs are equal to or controlling in terms of the total amount of dollars that would be available.

But again the averted on-site cost is an issue by itself and we're going to talk about that separately.

Now if I can, if there are no more questions --

Mal, why don't you take and identify in the rest of the questions which one's you're going to do, would you, please?

DR. OKRENT: By the way I'm not sure if, in this list of questions -- I just don't recall whether we specifically called out the question of should 10 to the minus-4 per year core melt, large scale core melt frequency be taken as a goal that one strives for without so much regard to what I'll call other considerations as shown in the viewgraph we were just looking at.

There have been Staff members who have urged that 10 to the minus-4 be taken as a first level goal. The ACRS wrote a letter in July saying they thought for existing plants that that is a goal that should be worked for.

So in your comments I would like to hear the thoughts of EDO and so forth on that specific point, which is not called out sort of specifically via the chart, okay?

MR. ERNST: All right.

MR. STELLO: I understood that kind of thinking to be in question six. We'll deal with it there.

DR. OKRENT: Question six is for new plants and I would say that 10 to the minus-4 was -- may be talked about as the place you should strive toward in terms of the ACRS letter and some of the Staff: Murilee, Ernst, Minogue, series of memos.

(Slide.)

MR. ERNST: I have sort of a skip-around kind of presentation. I was asked yesterday to provide some somewhat formal viewgraphs in response to four of the questions. I will identify the questions and characterize the questions before I give an answer.

This is not a rehearsed program at all, in fact, I think we had about a half an hour yesterday to sort of get an idea of who was going to do what. Time was pressing and I prepared some charts which I have. If Vic has any differences or questions about the charts, I'll probably hear from him as well as from the Committee. It's an unrehearsed kind of presentation.

I found some of the remarks so far this morning interesting -- not different but interesting and it causes me to make I think an introductory remark which bears on uncertainties. I think in the original safety goal formulation there were general words that talked about uncertainties and the desire not to have a very prescriptive safety goal was implemented by letter of the law or something that if you're 9 times 10 to the minus-5 on core melt, for example, you're perfectly okay; if you're 1.1 times 10 to the minus-4 on core melt you have to do something.

I think in the safety goal evaluation report we thought long and hard about how to address this particular

question and came up with a proposal, some kind of a matrix, I think, that has in general the same kind of effect of what Vic was talking about, namely, to indicate that there should be a range of concern and the more so-called out of spec that you are with the safety goal the more you have regulatory concern and the further down -- or the further removed you are from the safety goal in a safe direction the less regulatory concern there should be.

And at some point on either end of the spectrum -- at one end of the spectrum you should say enough is enough; at the other end of the spectrum you should say at some point cost becomes really not a question and you should fix.

I think that is in principle the same thing that Vic is describing in his matrix and clearly I think has the full support of people that certainly I have been dealing with on the safety goal question.

There are debates, perhaps, as to what these numbers are and what should be included in the algorithm and how much and so forth but I think that is more of a detail debate rather than a conceptual debate.

There are also questions about what about things that you don't know about. There are inadequacies in PRA's, there are things that are not treated in PRA's such as sabotage. There are very large uncertainties in PRA's,

for example, in judgments on seismic risk. These are serious questions that should not be dismissed lightly.

I think the answer to that is that you can't use safety goals as a regulatory decision process for all safety issues. Safety goals need to have some kind of perspective, some kind of rationale for implementation. But if you have an issue that involves plant security against sabotage, that is not really quantifiable with any degree of accuracy, again you should use judgment. And even in the areas where you can quantify it, you have to consider the uncertainties and it will require some degree of professional judgment as to the weight that you give uncertainties.

So I think this is just sort of a preamble to my prepared talk.

MR. EBERSOLE: I wanted to ask: you can't use them as a surrogate for good practice, I don't think. I find the absence of a threshold where you know perhaps some minor feature which would augment safety; it doesn't cost much but there is no rigorous rule to put it in and that has been the bane of the industry for many years.

DR. OKRENT: Jesse, can we hold that question, not lose it, because it is a non-trivial question but why don't we let him go on?

MR. STELLO: I'm going to answer that myself.

MR. EBERSOLE: Okay.

MR. STELLO: Well those were just a few sort of off the cuff opening remarks basically for the overall tone or whatever.

The first question I was asked to respond to was question two, which reads:

"The current safety goal policy does not contain any guidance as to treatment of uncertainty. What guidelines would the Staff give to a utility proposing that they not implement certain backfits; what kind of criteria should be appropriate; how can the safety goal policy be used in an ISAP review?"

I'm ready to talk about some of these; I'm not ready to talk about all. When we get to specific regulatory applications, for example, in ISAP I think we're getting into a level of detail that we have really not proceeded with regard to safety goal implementation.

What Vic showed was a basic concept that would be considered in implementation, which I think conceptually makes sense. When you get down to detailed criteria or sets of criteria or how would you apply in a specific ISAP case, I think I would need NRR up here to talk about things, or if Vic has some comments.

But I think that --

MR. STELLO: That question directly is going to

be addressed this afternoon. They're going to answer the question of what does this all mean.

DR. OKRENT: Okay. But ISAP is only an example and I don't want to see the question avoided this morning on the basis that we will discuss it this afternoon, because each time you arrive at some decision as to how to proceed on a generic issue you are implicitly or explicitly -- I don't care whether it is ATWS or station blackout or whatever, there are some bottom-line numbers, there are uncertainties in these bottom-line numbers, there are omissions in whatever went into the calculation. There is no current way of knowing the level of assurance, if any, that the Staff thinks is relevant.

On the other hand, if you look at the current standards proposed by EPA -- not proposed, adopted by EPA for geologic disposal of high-level waste -- and in fact they provide a standard and they provide a statement of the confidence with which that standard should be met, you know, one chance in ten, as it were, therefore in dealing with uncertainties.

So we really want to hear this discussed this morning. We'll come back to it on ISAP but let's not put it off.

MR. STELLO: We won't because I think some of the other examples you raised, specific examples that are also

in the questions will be talked about.

MR. ERNST: There will be some, I will call it, fumbling around I think in implementation to get a better feel. As you recall, in ATWS -- and I'm reflecting back at least a year or two ago -- there were some conservatisms in the Staff's value impact analyses.

I think from my standpoint anyway the crucial point is that the conservatisms were identified in the algorithm and people could make their own judgments about whether or not they felt that there was sufficient conservatism or, to put it another way, a sufficient degree of confidence that it was a proper decision.

DR. OKRENT: If I could offer a comment: when you say there are conservatisms, it provides no measure of what your criterion is. You could say if there is a 50-60 chance of meeting it that that's good enough for you or that you want something that you call, I don't know, reasonable assurance, what you might define as roughly a one sigma over the mean or something. You know, acknowledging that things are not easily quantified because the distributions are poor. But right now no one has no idea what philosophy the Staff thinks should be used as a general guide, nor what one does when you have differing sets of opinions.

MR. ERNST: Let me get into that a little bit: The Staff is not homogeneous. When we get into some of

these --

DR. OKRENT: Neither is the ACRS.

MR. ERNST: Oh, is that right?

(Laughter.)

MR. ERNST: When I get into that I clearly can't speak for the NRC. I might speak for several people in the room and might not be speaking for several others.

MR. ETHERINGTON: I think you were talking about uncertainties in the causes of core meltdown. Are you considering also the uncertainty in the behavior of a melted core?

MR. ERNST: Oh certainly.

MR. ETHERINGTON: With respect to containment and whether it will behave the way the code says it will?

MR. ERNST: There's no uncertainty in that answer.

(Slide.)

MR. ERNST: Let me try and respond at least in part to some of these questions, if not completely to the Committee's satisfaction.

In the first place, I think I would disagree with the first sentence that there is no guidance as to treatment of uncertainty. There may not be very explicit by rote kind of guidance but I think there is certainly policy-level guidance.

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As I mentioned in the introductory remarks, there is also a caution that you must consider in uncertainties in the development of an approach by the Safety Goal Evaluation Steering Group:

There was a table presented which certainly gave a little bit more guidance as to what was meant, and particularly in cases where the uncertainty was larger than one normally runs into in PRA's, larger than normal data distributions on pump failure and things like that. And then clearly the table that Vic put up is another way of approaching uncertainties; it could be argued that way. Instead of just taking 10 to the minus-4 and saying if you're below that you're okay, if you're above it you're not okay. There is a range of concerns.

One of the ranges of concerns, I think, that motivates such a range is the kinds of uncertainties involved. It's an inherent consideration of uncertainties, I would argue.

DR. OKRENT: I would argue there is no guidance on that table concerning uncertainties and I haven't heard any philosophic approach yet.

MR. ERNST: Not explicit. But if you really wanted -- if you really believed the numbers and believed that 10 to the minus-4 was the goal and you come up with a number that was 9 times 10 to the minus-5, you could argue

that you need do nothing. It's debatable.

From my perspective, I think any table that has ranges like that inherently considers that you're in an uncertain area. It doesn't necessarily have to --

DR. OKRENT: It's a different issue you're raising, though. I think we will both acknowledge that you don't know the numbers perfectly and therefore one shouldn't make a huge distinction between 9.9 times 10 to the minus-5 and 1.1 times 10 to the minus-4, you don't get disagreement there, both of those are uncertain is the issue, quite uncertain.

And now how do you deal with the matter in view of the fact that they are both quite uncertain?

MR. ERNST: I will stipulate we have no real formal algorithm for treating uncertainties. I will not stipulate that we have not expressed concern for uncertainties and pretty much along the line of saying in many cases it's going to be ad hoc -- you might want to treat, ETS, for example, differently than some other safety issue because of your knowledge about phenomenological uncertainties, uncertainties in how heavy metals react and things of that sort. We're not ignoring the subject is what I'm saying.

The second point is about completeness. Again as I think I mentioned in the opening remarks completeness is

a problem. I don't think that safety goals are going to be very useful in areas where you haven't analyzed the issue. For example, in sabotage.

So there are areas. And if a PRA does not identify a particular problem then you won't be able to do anything about that problem. That's really no different than any other aspect of regulation, you can do something with what you know about.

So I would envision the safety goals applied to things that you have analyzed and know something about. I would submit that the PRA is not necessarily a good representation of the overall risk but for what you've analyzed you can get some good perspectives of the drivers and decide whether or not you need to have some additional regulatory concern.

DR. OKRENT: Well does the definition of the safety goals say this is a safety goal of the risks we are able to analyze or is this a safety goal for the risks?

MR. ERNST: When you talk about in a regulatory manner, the thing that the regulator has to be concerned about is do I have to do something? Well clearly he is going to be looking at things that need to be fixed. Well if your analysis hasn't identified a problem then you are not going to have that problem of regulatory concern so you won't be applying a safety goal.

I don't think the intent here is to apply a PRA to a plant and if that plant turns out in the analysis that you write that plant off forevermore as a potential beast that needs to be monitored -- if indeed at some later time you find an issue for that plant and you analyze that issue and that was absent in the other PRA and then you find out that that seems to be a risky thing, you would re-open the issue. But it would be very difficult to re-open the issue without the knowledge of what the problem was is my only point.

Vic may want to comment on this but I don't think the safety goal -- the purpose of a safety goal is to say I'm going to stop looking at the plant in a regulatory sense, you may stop doing something about the plant but you're not going to stop looking, you're not going to stop gathering other information from research or operating experience or other insights as you go along. And you may have to reapply those new analyses to that plant at some time.

MR. EBERSOLE: Have you sat in any of these grindly detailed sabotage meetings, which are always classified?

MR. ERNST: No, I have not.

MR. EBERSOLE: Well I think some of you fellows need to sit in on these to get a perspective on what the

picture is. I don't see that you have told us anything about what to do about -- things can be done, not perfect by any means --

MR. ERNST: I thought I was trying to answer the broader question, not whether or not you should look at sabotage probabilistically, that's a different question. But if you have absent in a PRA the consideration of sabotage, then I think you don't say I'm not going to regulate sabotage because it's not in the PRA, you say I'm going to look at it with a different pair of eyes and then you may argue whether or not you should quantify sabotage. That's a different question.

MR. EBERSOLE: Well you know there's no organized approach to minimizing sabotage. It's a zero.

MR. STELLO: Can I make one comment and see if I can help put things in perspective?

There is inherent in the concept of a safety goal which relies on information from PRA's to raise the obvious question of how do you view, evaluate and finally make a judgment on what that PRA really says and what are those numbers.

What Mal is really getting into is how you analyze, synthesize and finally come down with a judgment on worthwhileness of the PRA.

There is an effort and there is a fairly

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substantial document -- I guess documents now, Mal; two NUREG's, Mal -- that have been developed to detail how we're going to go about trying to review a PRA which obviously will include the consideration of the uncertainties in the PRA analysis right from the initiators up through and including core melt and the effect of the core melt on containment and also containment failure.

That whole question of how to handle and how to review the PRA is an additional effort that is going on. I think it is important that that effort go on even if we didn't have a safety goal, because we have the new methodology today to use to help us make safety judgments -- but that's the nub of the question.

Now if you've done that and you have a judgment on the PRA, well, where is the template to help you, guide you, the policy to guide you with the results that you get? That's the safety goal matrix. The question of how do you do the review is separate from, after having done the review, how does it fit in?

But I think you need to know both. You need to know how you're going to do the reviews and then after they're finished, what you do with the results. The whole question of uncertainties derives from how you are really going to do the reviews and display those uncertainties in making a judgment.

Is that a fair characterization?

MR. ERNST: I agree.

MR. STELLO: Has the ACRS gotten copies of the NUREGs?

MR. ERNST: ?300 I guess is one which is methodology, and then there is 1050 which came out last year. Yes, they have copies of that.

DR. OKRENT: 2300, is that the one that is in seven volumes or something?

MR. ERNST: That's the methodology guidelines. I think it is two volumes.

DR. OKRENT: I'm thinking of something else.

MR. ERNST: My only point is you've got some

things you have analyzed and you've got some things you haven't analyzed. The things you haven't analyzed clearly are uncertainties. There are missing things in the PRA.

You clearly would not apply those to safety goals because you haven't got a number. You may debate whether or not you should have a number for that, but that isn't the question I'm trying to debate right now.

DR. OKRENT: Well, let me disagree with that statement in the following way:

My recollection is that in the first form that proposed safety goals were put out, -- and I may be wrong; it's a recollection now, and time flies -- the NRC said these safety goals apply to the risk as estimated for all initiators except sabotage; we'll have to treat sabotage in another way, and we will assure that sabotage is not a significant contributor to risk; something like that.

No seismic was part-- I'm sorry.

Up to the time they had the last meeting with the ACRS, seismic was also there. And I can remember commenting at the time of the meeting with the Commissioners that they would be laughed out of California if they proposed safety goals where they said seismic was not part of what was included. And in fact they included seismic.

MR. ERNST: They also included routine releases and design basis accidents.

DR. OKRENT: The one specific way that was stated would have to be handled some other way was sabotage, but all other initiators I think were intended to be included in the original -- at least this is my recollection. I find my recollections are not very accurate these days. Partly I find if one reads five different documents, sometimes you remember a piece of one and a piece of another, and the pieces don't always fit together.

Let's take those questions as two separate parts.

With regard to sabotage, I guess it is not clear to me, and it's not clear to the Committee, I think, in view of its letter that it wrote some time ago, whether the Commission has in fact looked deliberately to see whether sabotage provisions are commensurate with what it said it would do in that safety goal policy. And the Committee has suggested at least it should reevaluate the threat policy. And various people have suggested things that one might look at, at least for new plants.

It is not a separate issue, and nobody pretends it is, and although I have recently seen somebody's effort to quantify the sabotage risk in a constrained problem for a non-nuclear reactor but a nuclear plant, in fact I'm skeptical that one can do anything very quantitative about estimating the risk.

But nevertheless there was sort of a policy

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implication that the Commission would in fact go back and look at sabotage to make sure that it was not a major contributor that was the tail that swung the dog, or something.

MR. ERNST: Time passes for all of us. I can't remember explicitly whether they said they'd go back or not. I remember discussing, and I thought it was in the context of monitoring for sabotage, and feel that their current process provides sufficient assurance.

DR. OKRENT: Well, let's go back and find the words. I don't remember them exactly.

The other part, it was to include all initiators and if some are not in the study, it seems to me you can't just say Well, I'm not going to deal with these and I will take the numbers, lacking these initiators, and treat that as the study.

You have to I suppose use your best judgment estimates, whatever, as to what might be the range of contribution from the things that are missing, and say Okay, I will allocate a portion of the safety goal for that, and now what I have calculated has to meet the risk. And then still we haven't talked about the uncertainty question here, and the fact that you've had one proposal from Cave and Kastenberg along those lines, risk allocation kinds of things.

But I don't see any basis on which -- where you know you are incomplete, for you to say Well, I'll act as if those are zero contributors.

MR. ERNST: Again my memory is not the best, but I think I can remember -- I don't know how long we've been having fun in this area -- four or five years ago,--

DR. OKRENT: I suppose so, at least.

MR. ERNST: -- I think I came before this Committee with a thought on allocation of some sort, and I got a few bruises from the Committee, as I recall.

DR. OKRENT: Well, you know, people propose allocations now and then.

But again, I think you cannot ignore that you're incomplete. You may be able to argue that you have a margin in some of the things you have included which is of a certain order, and that this margin is, in your best judgment, enough to cover the incompleteness. All right. Then at least you have not ignored the incompleteness. Okay? And you just say why you think this is the case, and people can look at it and agree, or try to shoot it down.

MR. ERNST: I would think in practice, rather than trying to argue about how much one would allocate, 30 percent, 60 percent, whatever, I think in practice what this evolved to is if you start threatening a 10 to the minus 4

with the internal events only, and don't have an external events kind of analysis, you might pay a little more attention to that, realizing that there are some leftovers that have not been quantified that might be important.

So again, no formula like 30 percent or 40 percent or 50 percent, but a clear caution.

MR. STELLO: Maybe it helps to take an example to illustrate this point. Let's take seismic.

We've had the issue of seismic under review and a lot of work going on in trying to estimate seismic hazard at various sites. Lawrence Livermore has nine or ten sites that they're looking at.

DR. OKRENT: They did a seismic hazards curve.

MR. STELLO: Yes, at nine or ten sites.

The industry is trying to get in and develop methodology for looking at that better.

DR. OKRENT: Differently. It's very hard to know what's better.

Go ahead.

MR. STELLO: I said they are also looking at it. I didn't say better or differently.

Now what we all know is that in the area of treatment of uncertainties for seismic, for example, it is going to be very, very difficult. As I remember the spectrum, the frequency spectrum, it was very, very broad,

and probably on the order of an order of magnitude or so between-- Let's not get into the median argument.

Now it is the Commission's intent in what they have said to include all other initiating mechanisms including seismic, but that particular program is the one that you're going to look to give you this kind of insight in trying to determine what it is that you get out of the results of the PRA, and how to include them.

There are a lot of theories that don't have the seismic initiator included, so you are going to have to include it and make that judgment to the best of our ability. And until the science and the methodology develop further, you're going to be making some judgments.

But clearly you are going to be able to make the judgments with a great deal more refinement having had that knowledge and having that information in front of you than, clearly, without it.

But let's all agree that judgments can clearly still be needed in terms of deciding how to use that information. But its intent, except for sabotage, is to include all initiators to the best of our ability to do that.

We have gotten programs in all these areas to try to help make that judgment, and I suspect some of them will be fairly substantially complete in the next year or so,

and others won't be, and you'll continue to have to look at it and make the judgment.

And whether you call it a specific allocation of how much of the risk from that area you're going to include, the net result is that you will apportion a certain amount of that risk to the extent it is controlling for that external event, whatever that external event is that we are trying to come to grips with, or whatever that initiator is that we don't have a full understanding of. So that will be included inherently in the judgment.

But I don't know of any way to articulate that in a quantitative sense except to announce and enunciate the policy for it.

Clearly the seismic initiator is going to vary all over the map across the country, so I don't know of any way you could say, for example, specifically for seismic I will allow a certain percentage of the risk to be contributed or to be a result of a seismic-initiated event.

DR. OKRENT: Excuse me, Vic. I don't think you have heard a recommendation from the Committee to make an allocation for seismic for plants where no seismic look has been taken. In fact, I think that would be the wrong way to go. Experience tells us that seismic has proved to be very plant-specific, and that there are ideosyncrasies in design of a limited number of plants that make them maybe an order

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of magnitude more vulnerable than the bulk of the p nts out there.

And really it is those ideosyncrasies that one wants to pick up, whether it is a failure in welding, the motor control centers, or whatever.

MR. STELLO: But are we in agreement with the rest of it?

DR. OKRENT: To ignore seismic, I think you have an incomplete story.

MR. STELLO: That was precisely what I said. We are not going to ignore seismic. We are going to review the results of this program that the Committee I know has interacted with, and then use the results of that to help make the judgment of how much of a problem, in terms of risk, you are going to get from this facility based on the state of completeness of that study.

That's the best we can do. If that isn't good enough, then we can't include it at all. And that certainly isn't a good idea.

DR. OKRENT: Well, I guess from the answer from the Vugraph I really can't tell at the moment what you are going to do about completeness and what you are going to do about uncertainty.

Let me postulate two circumstances:

One is suddenly the people in this room are

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replaced by people working for Stone and Webster and Bechtel and Westinghouse, and they are the ones who are looking and making judgments about how well you fit the criteria, and how to apply judgment to these uncertainties.

And on the other hand, these people are replaced by people from the Union of Concerned Scientists. Again they are applying judgment; there is no policy whatsoever to tell them how to apply judgment.

I think we would get such a wide divergence of judgments it becomes uncomfortable. And if it is possible to have some kind of policy -- I don't say it is easy, but it would be useful to try.

MR. STELLO: Let me take my example and bring in new ones.

When you go ahead and try to make your judgment on seismic, what you do, you're trying to get the best quantities that you can find on the subject available to give you these judgments, so that you are getting, hopefully, the best scientific advice possible. That's what we seek. We seek, to the best of our ability, to find truth.

Now engineering is not a precise science to where we are going to get the kind of precision that we hope for, especially in areas where we are just beginning to scratch the surface. But it is that kind of information tha we are

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hoping to get to be able to make the judgment. It's good, hard science and engineering, to the best of our ability. We can't do any more than that.

That's what we're striving for; that's what we're trying for; that's what we will use, to the extent we have it available. If we don't have anything available, you're going to have to make the judgment just based on the information available. I don't know what else there is.

MR. ERNST: I think it's about the same thing I was going to say. It's not clear what this chart says, but let me get into this a little bit.

NUREG-1150, some of you might not know, is the Staff document that is going to document the Staff position on the results of the six reference plant severe accident risk analyses, and come up with judgments on how such information might be used in the regulatory process, insights gained from the analyses.

What we're in the process of doing right now is struggling with how to treat uncertainties in this document, and by this I don't mean-- What a typical PRA does is put some flags or distributions on data and say that's the uncertainty range, but you don't get much comfort out of that as to what is really driving the uncertainties or whatever.

What we are finding, which is of not great

surprise, is that most of the uncertainties are driven by assumptions regarding phenomenology and core melt progression, containment loads, threats, containment performance, fission product transport; things of that nature.

There are some uncertainties in the front end such as hydraulics, but the bigger drivers are in the back end.

MR. MICHELSON: I have a comment in that regard.

I find in looking at the PRAs it is very difficult to read about what are the basic assumptions made that went into the PRA, and what effect they could conceivably have. In other words, I find a lack of warnings to the reader that this PRA is limited, and here are the kinds of things it does not consider, some of which could be quite important.

Common cause may be inadequately treated or not treated at all, for instance, and yet they don't warn the reader that these kinds of things are necessarily absent or if they do warn the reader, they do in some obscure part of the document where it is a little difficult to find.

MR. EBERSOLE: I certainly agree. That's a common characteristic. They never exhibit and make clear the shortfalls in the analyses.

MR. MICHELSON: And those are the things we're

talking about I think here today, are these incompletenesses or uncertainties. Now these introduce uncertainties.

MR. STELLO: I commend for your reading the two NUREGs that were talked about. They specifically identify the need to call out that kind of information and identify it.

MR. ERNST: It makes the same caution you do, and I'm extremely glad that you brought up the comment. What we are trying to do in 1150 is to do exactly that, in some kind of useful way to identify to the reader what the drivers are, how important they are, what the ranges c those drivers -- reasonable ranges might be, so the decision-maker can then make his own -- his or her own judgments as to where I want to be, and which horse do I want to ride in this risk assessment.

Do I want to believe that venting of Mark-ls, for example, is going to solve all the problems that spread through the suppression pool and I won't have any risk, or should I believe there is enough hydrogen generated perhaps so that when you vent you are going to get an explosion in the secondary containment that causes substantial problems?

Those kinds of uncertainties in the ranges will be address is NUREG-1150. We are in the process of identifying the 10 or 15 major drivers for each of the

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accidents, dominant accident sequences, at the present time.

We are going to go through a process of identifying reasonable ranges for these and a judgment on why that reasonable range has been chosen, based on existing scientific and engineering information. So there will be documentation of why these -- what the ranges might be, and why these ranges were chosen, and why the Staff has chosen a certain value for their base line risk estimate.

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DR. OKRENT: I'm not sure yet this gets at all of what I'm concerned about, for instance, sabotage as an example.

MR. ERNST: I can't address sabotage, sir.

MR. MICHELSON: Well then common cause, hopefully you can address that. Common cause is clearly always a driver, but in some cases it could be much more significant than in other cases. I would like the writer to tell me how sensitive this particular plant might be to certain common cause problems and what they are.

MR. ERNST: That's one of the issues that we have that we will address at 11:50. There is no good way of addressing common cause failures. We are trying in our research program to do it better. There is no clearly good way of doing that outside of basically going into the data.

MR. MICHELSON: We heard about some of this at our recent Subcommittee meeting. And even at that meeting, after further inquiry into the materials they had written, it became clear that they had not done a very good job of identifying all of the possibilities for common cause, some of which could be quite significant. That didn't mean they weren't aware of it but they simply didn't write about them, they didn't make the reader aware of them.

In other words, you have to be the Devil's Advocate, I think, in doing some of this writing to be sure

the reader understands where you weren't too smart and didn't really know how to do your job very well. Writers tend not to advertise that too extensively.

MR. ERNST: Well we're going to try to do a successful job, and what I'm going to be doing in that area is investigating the influence of various assumptions on common cause failure, on the likelihood or consequences of that particular accident sequence as one of the parameters that could be of interest. Some sequences are not of as much interest as other sequences.

But we're trying to do this.

I guess after going through this we would have a statistical sampling of the 10 or 15 important drivers and their ranges with degrees of belief as to the end points of the ranges and then come up with a display -- what I call a limited uncertainty analysis with a display based on the sampling of the important drivers and their likely importance to the analyses.

DR. OKRENT: You see there is still some concern that you may not have all of the important risk drivers.

MR. ERNST: That's right, I cannot guarantee perfection but I certainly can guarantee a better job than has been done in the past.

DR. OKRENT: I agree with that.

MR. MICHELSON: It's in the right direction at

least.

MR. ERNST: Let me give....

Clearly then this is the analytical part. When you get down here, I've got to go back to statements I made and Vic has made and a lot of other people that you really have to then in the decision process figure out how much weight I'm going to give to those uncertainties.

The important point here is to display the uncertainties, display the drivers. Then you can make a better decision about how much conservatism you may want to choose.

DR. OKRENT: Excuse me, how do you display the uncertainties of something you haven't included in your analysis?

MR. ERNST: I pass on that, I can't.

DR. OKRENT: I'm not talking sabotage, I'm talking about other aspects.

MR. ERNST: I can't.

MR. STELLO: Well I wonder if there is a different answer though, Mal. If there was something you knew was not included in your analysis and you knew it and you could identify it then you can go out and try to evaluate it.

I think the problem is is there's some-hing that I don't know about that I didn't include in the analysis.

DR. OKRENT: I'm sorry, there are many things that have been mentioned that are not in the analyses that you do not and the purveyors of the PASS do not go back and include.

MR. STELLO: Give me an example, if you will.

DR. OKRENT: All too often, for example, seismically-induced interactions between non-Class I and Class I systems. A lot of the kinds of concerns --

MR. STELLO: That is included, seismic interactions, to the best of our ability.

MR. MICHELSON: Unfortunately your ability is not so good in some cases. A specific example which came up at our meeting was fire protection and the inadvertent actuations due to a seismic event. It was not analyzed or even considered as a part of a potential system interaction. It ought to at least have been discussed and indicated why you didn't think it was a problem. You know, if you can't do it qualitatively -- quantitatively, at least do a qualitative discussion of the inadvertent actuation due to seismic events but don't ignore it completely and leave it out of the study.

MR. STELLO: I agree with you. But as long as there is something that we can identify or know about then to the best of our ability -- but even if we go back it doesn't mean when we do seismic interactions that they are

perfect by definition.

DR. OKRENT: But they have not been done well on hardly any plants. Maybe one fairly well and I don't know how completely because I don't even know if they picked up the point that Carl just identified.

MR. EBERSOLE: Well the current state of environmental qualifications is extremely ambiguous.

DR. OKRENT: And that's another one. It's hard for us to tell really whether it has been done meaningfully in a PRA.

And you keep referring to uncertainties in seismic and I must say the more I look into human error, the more I look into how different PRA's are treated, the more I look into what happens in the events that you sent investigating teams for, the more I feel that it falls in the same ballpark of uncertainty with regard to its contribution to risk.

MR. STELLO: That is clear and it is also clear that there are a lot of things that are also uncertainties that can be done to arrest sequences that are started down toward core melt that we also cannot identify.

DR. OKRENT: I'm sorry but recent PRA's I looked at make every attempt to include that factor in the PRA and I'll cite --

MR. STELLO: But it is not perfect, far from it.

DR. OKRENT: It is not perfect, I don't think anybody is prepared to say that it's always conservative, that it's always optomistic or so forth. I'm just saying if you look it is in there. And just look at the Millstone PRA for an example.

MR. STELLO: It is not clear to me actually at this state what we are really debating. It strikes me we're debating usefulness of a certain safety goal structure. There are imperfections in PRA's.

I would submit that if you had a safety goal policy out there that was in reasonable use that you'd be focusing more and more on some of these trying to improve your quantification in areas where you need improvement, that it would be a little bit of incentive to maybe answer some of the questions quantitatively that come up. I don't think safety goals are going to answer every question you have, but if we have to wait for perfect information for safety goals we may as well bury the animal right now I think.

DR. OKRENT: Let me try then to put it in at least one kind of perspective: in the end the Committee is going to be asked to provide comments on what you send to the Commission or maybe even before. And one of the things it's going to have to keep in mind is the existence of uncertainties and incompleteness and how this is being

addressed as the Staff describes it. And is this adequate for the purpose or should there be something different or more or at least additional consideration given to it or so forth. Okay? So there is a reason for this discussion.

You know -- I must say I would be only too happy if you would come in with something and I would say Eureka, we have found it, the perfect way to deal with uncertainties.

MR. STELLO: Don't wait for that because it's never going to happen.

MR. ERNST: That's right, and that's my point. Do we proceed as best we can, improve, or do we wait for the perfect one?

DR. OKRENT: Okay. But anyway there is a reason, it is a non-trivial --

MR. STELLO: But while you're on the subject, let me suggest again that there are clearly a lot of issues that are important issues and I think -- what I hope to be able to see is to have the Committees focus on some of these issues and at least give advice on those, the concept itself that we're talking about and then the issues that are raised as a result of the concept.

I think it is important to have the Committee be able to do that before we get to a final piece of paper that is the policy and I'm sure there is going to be an

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opportunity for the Committee to deal with that, too. But let's start picking some of these troubling areas, recognizing we have to make a judgment of how to proceed in light of imperfect information because it is imperfect. We have to decide. Perhaps the answer is let's not have a safety goal. But if we are going to have one we are going to have some problem areas, let's deal with them to the best of our ability to deal with them and come out with the best product that we can come out with that identifies all these concerns.

And I think this kind of discussion is very, very helpful to make us all understand where these problems lie. We'll do the best we can but remember we're not, any of us, are going to be happy that we have in fact put any issue -- quote -- to bed -- quote -- completely. The science is still very young. We've got a lot to learn about it. All of us understand that.

But I think it is far better than -- quote -- deterministic -- quote -- thinking in trying to make regulatory decisions. I feel far more comfortable in having a body of knowledge that I get out of a PRA to help me make my regulatory judgments than making them without it.

All of those uncertainties, all of these questions exist with or without a PRA. The fact that you had the PRA didn't make them happen, they're there, they're

part of nature. They have to be dealt with to the best of our collective abilities to do so.

So the advice we can get from the Committee in dealing with some of these difficult issues, I think, is extremely helpful. And I urge that we don't look for resolving this issue today but, rather, coming to grips with some of these difficult questions.

MR. ERNST: Let me try and close out uncertainties, I think it is extremely important. And sometimes a picture may be worthwhile and sometimes may get me in trouble, I don't know.

(Slide.)

MR. ERNST: Let's take a simplistic kind of analysis that says you calculate risk based on a certain set of phenomenological assumptions and this gives you this kind of distribution and this kind of best estimate -- I will call it best estimate -- mean, okay?

Assuming that these assumptions that are inherent in generating this distribution, I get a mean and I call it a best estimate mean because it embodies a whole bunch of phenomenological kinds of assumptions in generating the distribution.

Now let's assume from this that I make a couple of sensitivity studies. One of them might be that I have been overly conservative on the amount of water that it

takes to keep a core melt from happening and some analyses that seem to be credible say less water will suffice.

I run that through my analyses and it turns out with that assumption I get a different distribution with a different mean, but that I have another parameter that says Hey, venting may not be all it's cracked up to be, there may be some problems that weren't really recognized in this best estimate and I will take some credible ranges on the other side, keeping all other variables the same and can generate another distribution with a mean. One could advertise that this might be a reasonable range of means within which the true mean exists. I have sort of coined a phrase on this thing.

A decisionmaker could then decide where do they want to fall? Do they really want to believe that it takes two pumps and then it's not going to work or do they really believe that one pump is enough and that venting is going to work the way the best estimate is advertised.

This is simplistic, it only deals with two different variables and two different assumptions for those variables. This is the process we are trying at 1150 with more variables, degrees of belief put on these distributions and then a statistical sampling to see what your reasonable range of uncertainties might be.

I think enough said on that.

DR. OKRENT: I think we had better go on to other topics.

MR. STELLO: He has another slide.

DR. OKRENT: All right.

(Slide.)

MR. ERNST: This is what I call a perspective chart and I guess it is based on experience. The assumption being that experience has not shown a core melt accident with a vessel melt-through. It takes a reactor years of total commercial experience in the free world and gives 50 percent confidence based on no bad experiences to date.

What the estimate would be for core melt frequency at 50 percent would be no greater than 1.9 times 10 to the minus-4, 90 percent confidence would be this one (indicating).

DR. OKRENT: Can I ask a question on that? Do you have any feel for in the free world what fraction of the reactor years, for example, are in France and Germany compared to the rest of the free world?

MR. ERNST: We have a reference. I don't have it on the tip of my tongue.

MR. STELLO: Why don't you send that directly down, the IAEA report? It gives the actual numbers.

DR. OKRENT: And maybe Japan; France, Germany and Japan, do they include the bulk of that?

MR. TAYLOR: I don't remember the exact numbers but I'll try to get them to you.

DR. OKRENT: It would be a little of interest. I'm not sure what the U.S. would do in evaluating the perspective on risk for U.S. reactors if there were a core melt abroad.

In other words, certainly U.S. industry did not take the Bhopal incident and say this is representative of U.S. industry.

MR. ERNST: I really don't want to make more out of these numbers than what they might imply. I think one interesting thing -- rather than worrying about whether that's the right number or whatever, this is far removed from general results in PRA's in the past, what those PRA's indicate. I think that what that shows me is maybe the PRA results aren't that far from what reality might be, that's what it indicates to me.

DR. OKRENT: Again, you know, what is not included in there also is the, I don't know, fraction of a core melt suffered in what they call a Hall reactor, it's not a light water reactor but it was a power reactor.

MR. STELLO: Well you recognize that the number itself, if you were to say that you did have a pressure vessel melt-through, one of them, it won't change the number very much statistically; it doesn't change it very much.

DR. OKRENT: Again I'm saying if one is going to shed perspective, one may want to ask some questions. I have given you a few different kind of questions....

MR. STELLO: Exactly.

DR. OKRENT: You know, again, I'll repeat: the U.S. industry doesn't accept I think as representative of U.S. the Bhopal incident.

On the other hand, I lived in Cleveland when an LNG facility did rupture, it wasn't India, it was in Cleveland, and over 100 people burned to death. And I don't know if the rest of the world accepted it as representative of their LNG facilities.

MR. STELLO: You know, the problem is the only people that are doing these kinds of analyses are us in the nuclear industry and the chemical industry and LNG, very little of that has been done and they have very little understanding. I think we have far more understanding of the relative risk because of the efforts that we have made. I think the problem is you don't have a like data base to compare.

DR. OKRENT: Well they're moving ahead in the chemical industry.

MR. STELLO: Well they may be now but based on what I have seen I have yet to see a calculation of what the likelihood or the probability of the Bhopal incident was; I

have not been able to find one. After you made that comment I went back and looked and there isn't any that exists, so you don't know whether the probability was 10 to the minus-2, 10 to the minus-3 or 10 to the minus-4.

DR. OKRENT: I don't know if Bhopal was analyzed. There are now evaluations of chemical installations.

DR. SIESS: I don't know where we are on the agenda --

DR. OKRENT: We are on 2.

DR. SIESS: But I have one question that relates to the matrix and I have one question that relates to seismic.

DR. OKRENT: Well why don't you ask your question on the matrix?

DR. SIESS: Can you put the matrix chart back up?

(Slide.)

DR. OKRENT: And then ask the seismic, okay?

DR. SIESS: If we look at the top line of that it says that if I've got a plant that does not meet one of the health effect guidelines but it has less than 10 to the minus-5 core melt probability I essentially don't have to do anything because that fix at $1000 a man-rem doesn't amount to very much.

Now it seems to me that's elevating the core melt

guideline not up to equal to the health effects guideline but actually higher than the health effects guideline.

MR. STELLO: Well it turns out that for less than 10 to the minus-5 it's academic. In all cases --

DR. SIESS: I'm not talking about what it is, I'm talking about what it says.

MR. STELLO: I understand that.

DR. SIESS: Okay

MR. STELLO: I'm making two points: first, it's academic because any plant with a frequency of less than 10 to the minus-5 you will meet -- based on everything that we have seen -- you will meet the health effects.

The whole purpose though of putting it on there was to preserve the trend or the perception we are trying to create of the impetus for driving fixes in terms of cost.

The intent, when we got up there, to drop the averted on-site costs -- and that's the reason for keeping that line there -- was that at that point the regulatory interests in reactors, facilities that are less than 10 to the minus-5 is diminished significantly in that any further consideration or six ought to be left for the judgment of the utility in terms of him deciding for himself that at that point averted on-site cost, because it has the aura about it of being -- quote -- economic regulation -- quote -- at that point we say okay we're dropping out of the

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picture.

DR. SIESS: Except if they don't meet whatever health effects --

MR. STELLO: Yes, and then we say it's strictly based on safety and you will decide any further fixes because you were already so far removed from safety concerns.

DR. SIESS: But some of the arguments were that $1000 a man-rem was not a good representation of societal effects, health effects type things, and yet that's the only thing that will drive you in that case.

MR. ERNST: Let me throw in a word here. I think if you're on this line, which I think is where you are --

DR. SIESS: That's where I am.

MR. ERNST: I think the $1000 a person-rem is not an inconsequential sum. I think, depending on the site, you're going to be in the range of, life time, 10 to 100 million dollars -- I think.

DR. OKRENT: Suppose it were a reactor vessel rupture that was 9 x 10-to-the-minus-6 and there was no uncertainty in it; you know; so that it led to a major release. In fact I think you would want to look at fixing for external effects. I think that's not unreasonable.

DR. SIESS: You think there's enough money?

MR. STELLO: I think we need to emphasize the point that based on everything we have seen so far, if you have a number less than 10-to-the-minus-5 you will meet the health effects. It's academic. There won't be any don't-meet either for a case where the core melt frequency is less than 10-to-the-minus-5. It is strictly academic. It was put up there to illustrate a point--

DR. SIESS: Okay, I--

MR. STELLO: I'm telling you why I put it there. I put it there because I wanted to show at that point that the averted on-site costs were no longer the consideration and it was strictly person-rem, which is health and safety.

DR. SIESS: But when I read this memo from Dirks to the Commission that was handed out today dated December 3, on page 4 of it there is a long discussion of why a thousand dollars a person-rem is not a good value for all cases. It says,

"A thousand dollars a person-rem can also fail to cover actual costs borne by the general public,

and should precautionary emergency response actions be instituted for those more benign and well-contained core damage accidents."

Now, what about one where, you know-- So is a thousand dollars a man-rem good enough -- or person-rem?

I mean, you know, the argument for averted on-site costs is more than one. But in this thing I was reading, part of the argument came from the fact that a thousand dollars a person-rem wasn't really representative strictly in terms of off-site costs. And yet that would be the only thing driving it right now in that case.

MR. STELLO: That's correct. And I don't know what more to say except what was my reason--

MR. SIESS: I'm just looking at the appearance of this thing.

MR. STELLO: It's trying to describe where does this agency say it's finished, that we've decided enough is enough. That is what that top line is trying to do. It also has the point at which you have the full transition, to say the whole question -- and it's a significant issue, a significant debate within the ACRS of when do you stop considering averted on-site costs. That's where. I was trying to describe that.

DR. SIESS: I think I'd like that matrix better if the health effects came first.

MR. STELLO: Well, I don't care which order. Put them in any order you want. The intent is that--

DR. SIESS: This tends to make the core melt probability be the dominant determining factor, and then you look at health effects.

MR. STELLO: In terms of driving a need for a fix, it you look at the table it is clear that what drives it is the health effects. That's where you pick up the averted on-site costs.

You'll notice that if you are at a frequency greater than 10-to-the-minus-5 you will include -- if you don't meet the health and safety standard, which is the health effects -- 100 percent of the averted on-site costs in every case, except when you get down to 10-to-the-minus-3 then cost is not even a consideration.

The message you should get from that table is, if you don't meet the health effects the impetus for fix is very, very large.

DR. SIESS: But before you had this, if you didn't meet the health effects, what was the-- Was it always a thousand dollars a man-rem?

MR. STELLO: Yes. This is adding in averted on-site costs which were not done before.

DR. SIESS: We were never in the position of saying you will meet the health effects? It was always a

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cost-benefit?

MR. STELLO: That's right.

DR. SIESS: Okay; I hear your answer.

My question on seismic, Dave, is in reference to that same document that was handed out, and it is on page 9, Open Issues on which Commission Guidance is Requested, which is III, and it's Item 1(d). It says,

"Does the Commission support the use of this integrated matrix to reflect only those severe accidents resulting from initiating events internal to the plant?"

And then the next sentence says,

"This approach is being considered because an external event such as a large earthquake of sufficient magnitude to cause a large consequence accident to occur at the plant is, of itself, large enough to distort the accident mortality risk to to those individuals residing in the vicinity of the plant."

Can somebody explain what that means?

MR. STELLO: Yes. This is a memorandum which is supposed to be for internal use. I'm beginning to wonder whether we ought just to distribute it since it seems to be going on the public record.

But to answer your question: The issue is, if

you had a very large earthquake and it did damage the plant you would clearly have associated with that earthquake enormous other effects other than at the nuclear plant, with the cost being very, very high. And how should we deal with that issue? Or should we?

DR. SIESS: In other words, if health effects from radioactivity should be 1000th of those from other causes, then you might say the seismic effects on the plant would be a fraction of the seismic effects outside the plant?

MR. STELLO: Yes; because by definition you can only die once: you can die from the earthquake and its direct effect or from the accident, and you won't be able to make a distinction. And that's a big problem with the earthquakes of the magnitude we're beginning to talk about.

DR. SIESS: That's the first time I have heard anybody say that.

DR. OKRENT: I think that this is a troublesome and flawed philosophy or proposal or concept in several ways.

In the first place it is by no means clear that the earthquake that might cause a reactor accident will in fact kill anyone off-site. And we don't even have any estimates on the number. It would be very clearly site-dependent because, in the first place, some plants are

many m.les from structures of the type that might be damaged by an earthquake. If you have small towns with houses like mine built of wood, and so forth, within the first ten miles you're not going to have -- you may have damage, economic damage. But it is not clear: it depends on where the epicenter is.

MR. STELLO: By definition.

DR. OKRENT: So there is no automatic off-site casualty effect. You design against, for example, tornadoes. Tornadoes have, in fact, killed ten, twenty, thirty, maybe more people with reasonable frequency in this country. And I would suppose that a tornado that comes close enough to damage a nuclear plant is likely to be part of a swarm that is, in fact, killing some other people. Yet we're not ignoring it.

But just from a philosophic point of view I think society doesn't want to see -- let's move away from reactors; because there is a flood and, in fact, forty people are drowned in the flood, that some big plant handling hydrogen cyanide failed and released the hydrogen cyanide and killed another hundred. I think they would feel the plant handling the hydrogen cyanide should be protected of its own.

MR. STELLO: I think what you're doing is answering the question. I have no trouble with your answer.

It's a question: it wasn't policy; it was asking for the Commission to give us guidance should we get into that area. All of what you said is true. And the inverse of what you said is equally true, you could have an epicenter at a place where there is a very high population, kill enormous numbers of people and damage the plant. Both cases are possible.

Should the staff look at it, consider it? A question to the Commission.

I think your answer, if you were a commissioner, would be no. That's okay. Whatever answer we get is fine.

DR. SIESS: Dave, as an aside, would your house survive the flood?

DR. OKRENT: My house would survive almost any flood I'm aware of. In the first place, even if the earthquake failed the dam upstream, we're on the right side of a hill.

(Laughter.)

MR. EBERSOLE: Dave, let me ask this question. What is the current sociological attempt to defend life loss in the event of earthquake in the context of standards of construction, say for San Francisco and New York and wherever? What are the risks that are currently taken and reasonable for earthquake risk in these standards, and would it not be appropriate to use a percentage of that risk?

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I'm saying that an engineering defense against earthquakes to prevent loss of life existing at this time--

DR. OKRENT: It's so variable on the University of California campus, for example, it's now an issue.

MR. EBERSOLE: I'm saying take a percentage of it, whatever it is.

DR. OKRENT: And it depends on which building you're in whether you're in excellent shape or poor shape.

MR. STELLO: Jesse, that's another answer. Take some fraction of it.

Please, we're not trying to get that answered

MR. EBERSOLE: Well, everybody recognizes they get killed with earthquakes in San Francisco.

MR. STELLO: Look, Mal has got an airplane to catch. We're using enormous amounts of his time. We're not going to get very many issues--

DR. OKRENT: Let's go on.

MR. STELLO: Could we go at least to the next and finish Mal, so he can catch his airplane?

DR. OKRENT: Yes, please do.

MR. ERNST: The next question was Question 3, and there was a series of questions regarding containment performance design objectives.

I guess my answer there is yes, it would be logical to have a statement in the revised safety goal

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policy stating that such work is being done.

As far as time scale, the present time scale for development of a draft to go to the Commission is early '87. There's certainly no problem in that. I think we had a meeting on this about a month ago on performance objectives.

Should some preliminary statement be included stating the Commission's expectations? I guess I'd have no problem. I don't know what the Commission's expectations are, but if they wish to put expectations in there I guess that would be their decision.

MR. STELLO: I've got a fundamental problem trying to deal with expectations before you do the work. I think you have to do the work, and then you find out what you get, and then you do that.

MR. ERNST: I would certainly be against it. But it would be up to the Commission whether or not they want to put in expectations.

MR. STELLO: I don't think you can predict beforehand -- describe what the results of the research will be.

But the short answer to this question is yes, our intent is to deal with it in the safety goal, we are going to put it in.

On to the next question.

DR. REMICK: I would like to comment at this point. Two things. Earlier, Vic, you correctly pointed out that with a core melt frequency of 10-to-the-minus-5 you have basically wiped out the public risk, because any reactor that meets 10-to-the-minus-5 core melt is going to meet the public risk objectives.

Also, once you put in a containment performance design objective coupled with a core melt frequency, once again you've got the need for a public risk design objective. You've constrained the thing and tied it down to two design objectives that are not really public risk. I'd just like to point that out.

The whole concept of the safety goals being public risk oriented, with a core melt frequency of 10-to-the-minus-5, with a containment performance design objective, you've wiped out any kind of internal consistency between core melt frequency and those design objectives.

DR. SIESS: You can still have the health effects as qualitative, and then they're met quantitatively by meeting the core melt containment performance.

DR. REMICK: I guess I don't know what you mean by they could be qualitative if you are still going to keep then one part in a thousand: that's not qualitative to me.

DR. SIESS: I mean the original--

DR. REMICK: Oh, yes. But that is still true.

DR. SIESS: Then you could control the one in a thousand by the proper selection of core melt.

DR. REMICK: That's basically what's being proposed. But you take that out of the -- I think any meaning to the public of what is that risk.

DR. OKRENT: Is it unequivocal that at 10-to-the-minus-5 per year of a PWR-1 or 2 would not exceed the public risk?

DR. REMICK: Basically what you've done, with 10-to-the-minus-5 you only have to go a factor of 5 for that to get out of the primary system, get out of the containment, and the person-rems would be less than 10,000.

DR. OKRENT: What's this factor of 5?

MR. STELLO: Well, with 10-to-the-minus-5, if you look at an individual risk of 2 x 10-to-the-minus-6 you're only a factor of 5 to compound your probabilities with any severe core melt of 10-to-the-minus-5; it has to get out of the primary system, it has to get out of the containment system, and has to be less than 10,000 person-rem of exposure to people within--

DR. OKRENT: 5 x 10-to-the-minus-7, I thought.

DR. REMICK: No, that's individual risk.

MR. ERNST: We ran a number of analyses which indicate that for an SSD-1 release at 10-to-the-minus-5 you're right at the prompt death--

DR. OKRENT: At the edge.

MR. ERNST: Right at the edge at about 10-to-the-minus-5. So I would assume that if you got prompt containment failure for every core melt then you'd be right at the edge. You would not be exceeding it, you would be right at the edge.

DR. SIESS: What's the containment failure probability that's inclusive in that calculation?

DR. OKRENT: 1.

MR. ERNEST: An earliest containment failure probability of 1.

MR. STELLO: The studies that he's referring to assumed the probability for containment failure to be 1.

DR. OKRENT: SSD-1.

MR. STELLO: And then you still meet the health effects. That's why I said it's academic.

DR. SIESS: That's a nice kind of number.

MR. STELLO: It's hard to argue that it's worse than that.

DR. SIESS: It's much better than zero.

DR. OKRENT: The reason why I asked, my recollection was that there was of the order of a factor of 10 in WASH-1400 for the people living closest. But you're saying there's a factor of, I guess, 1, basically; right?

MR. ERNST: What you really is the fact that this

has been integrated around the one mile, and the plume only covers about one-sixteenth of that. So if you look at risk you divide by 16.

MR. STELLO: If anybody wants the answer to that question it's in the Steering Committee report. It's on page 430. It gives you the parametric study I was referring to that shows it's academic.

MR. ERNST: Question 9 deals with the true societal quantitative safety goal. We did look at this question in the Steering Group report--

DR. OKRENT: Excuse me; before we go on. That's a interesting result.

MR. STELLO: What is?

DR. OKRENT: That if you have a SSD-1 at 10-to-the-minus-5 you don't violate the safety goals. Yet I'm not sure that the Commission would be happy with 10-to-the-minus-5 frequency for an SSD-1, even though it seems to meet the safety goals.

MR. STELLO: Now, wait a minute.

The whole purpose was to do a calculation to show what frequency of a full scale core melt there had to be and still meet the health effects goal. It doesn't at all mean that that is, in fact -- that the probability is 1 of containment failure. The probability is what it is. It turns out that based on the analysis we've done thus far,

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that isn't the probability.

DR. OKRENT: That's not what I said.

We were just told that with an SSD-1 at 10-to-the-minus-5 per year you meet the safety goals. An SSD-1 implies very considerable economic off-site effects, nevertheless.

MR. ERNST: You meet the cancer fatality goal by a couple of orders of magnitude.

DR. OKRENT: But an SSD-1 implies a large release of radioactivity.

MR. STELLO: That's true.

DR. OKRENT: And I'm just saying that it's not clear to me that even though one meets the safety goals, with that frequency of, I won't call it "total release" but very large fractions.

I think it warrants some thought: let me put it that way.

MR. STELLO: And maybe some research.

DR. REMICK: That's roughly once every 10 ice ages, to put it in perspective.

MR. ERNST: Shall I proceed with Question 9?

DR. OKRENT: Well, it would be if there were one reactor. But not if there are...

All right. Go ahead.

MR. ERNST: As I mentioned, Question 9 deals with

societal quantitative safety goals. I assume those words mean an aggregate rather than-- The Commission has a societal goal in the cancer. But this is an aggregate goal that the Committee has--

DR. OKRENT: The Commission has something it calls a societal goal. There are some people who wonder if that's the right title.

MR. ERNST: Right.

I think the feeling is it might warrant a brief discussion in the safety goal policy. I didn't have a lot of time yesterday. I did briefly go through Rev. 1 to 0880 and I couldn't see any description of the logic behind dropping this societal goal or the logic in dropping the individual cancer risk goal. I thought there was some there, but I couldn't find it in a casual scan through Rev. 1. It may be in there.

If it's not in there, it does seem to me like it's worth a few words to indicate what happened between 0880 and 0880 Rev. 1.

As far as the question of why it was dropped, again, I guess I can't speak for the Commission as to why the individual commissioners decided to drop this particular goal. I suspect it was because of the technical judgments that such a goal really wouldn't be controlling in any event unless you greatly exceeded the core melt or the

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Commission's latent cancer goal. So I can only speculate as to what the reasons might have been.

The feeling is -- and the Steering Group did look at this in a fair amount of detail: there clearly were differences of opinion. The feeling is that current siting policy which restricts the number of people around a site, the establishment of the currently formulated safety goals which do have consideration of core melt frequency and public risk, the thousand dollars a person-rem lever on keeping the risk down and in areas where -- and also the fact that a thousand dollars a person-rem, particularly for the large releases, which are the ones we should really be concerned about, seemed to overestimate the economic impacts off-site. And the fact that if you had any usual kind of impact off-site, that would be picked up in your NEPA analysis. It did seem like that was a suitable structure to solve this particular problem.

The conclusion the steering group arrived at was that no change was felt to be needed.

Looking at just a couple of examples of what the impacts are,--

(Slide.)

-- this is a histogram that assumes an SSD-1 release at a frequency of 10 to the minus 5, which would be, as I mentioned a little while ago, right at the prompt-death safety goal. And with this-- This is a site-dependent histogram of the mean average number of early fatalities that you would get, and I have a similar histogram for cancer fatalities, again at 1 times 10 to the minus 5 frequency for an SSD-1.

(Slide.)

Both of these clearly indicate that you are a long way, a decade away or so, for the worst site from the number of one estimated fatality per reactor year of operation, which is a number that has been considered from time to time-- In the range of one to two, I believe, it has been considered from time to time by the Committee and by AIF and others.

So this just seemed to indicate that what we have in place in terms of a prompt goal and backed up by core-melt-frequency considerations would provide better protection actually than the establishment of a one or two

estimated deaths per reactor year. There did not seem to be any health and safety reasons why such a goal would be useful.

DR. SIESS: Is your abscissa per reactor year?

MR. ERNST: Yes.

DR. SIESS: That's something times 10 to the minus 3 per reactor year?

MR. ERNST: Yes.

DR. SIESS: For one reactor at that site?

MR. ERNST: Yes.

This is a histogram. There are 16 sites, for example, that would be right at 10 to the minus 2 estimated deaths per reactor year.

DR. OKRENT: Given an SSD of 10 to the minus 5?

MR. ERNST: Yes. And the SSD at 10 to the minus 5 is roughly the same as being right at -- assuming that you had that prompt containment failure, it would be right at the prompt death safety goal.

DR. OKRENT: I think it is interesting, but I'm not sure you have addressed directly, although you have addressed indirectly, the question posed in 9, namely that in the current wording, there is something -- there are qualitative things about individual societal risks.

And then there is some kind of design objective, I guess, which purports to be a societal risk design

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objective and which at least many feel is not a societal risk design objective.

MR. ERNST: This is my statement. I think it might not hurt to have some language in there that solves this problem that does not assert that this is a societal goal but that the structure of the goals is sufficient to provide societal protection. It might not hurt.

DR. OKRENT: Okay.

Now if you are going to go that route, that means you are going to have some kind of a latent cancer thing which is really an individual kind of risk goal. In other words right now they integrate out to 50 miles and call it the societal risk but it is really the average individual risk over 50 miles.

And now the task force suggested that you integrate out to 10 miles, but it would still be an average individual risk over 10 miles. Denton suggested integrate over one mile. The ACRS suggested integrate over one mile but call it an individual risk of latent cancer.

I'm not clear which way the EDO thinks it is heading.

MR. ERNST: One needs to be a little careful. You would not want to aggregate out to 10 miles, for example. That would be wrong the other way.

In other words, the closer you come in, the less

the aggregate is going to be.

We are recommending setting the averaging distance out to 10 miles, but then you figure an average individual risk and then you figure the diffusion of the fission products, and so forth, the dispersion of fission products. And if you wanted to have an aggregate goal you can still the total impacts out to the 50 miles, or whatever.

DR. OKRENT: Well, I'm leaving aside for the moment whether the Commission has something that is really a societal goal. At the moment I don't know what aggregating out to 10 miles is. It doesn't tell the person living within the first mile what his risk is unless you are satisfied and you can show that it's the same over that 10 miles when you allow for weather, et cetera.

MR. ERNST: Your risk is going to be higher closer in by factors of 5, say.

DR. OKRENT: Well, then, what is the meaning? It is not a societal risk if you integrate over 10 miles and it doesn't give the--

MR. ERNST: Averaging or aggregating?

DR. OKRENT: You're averaging. In what you propose you're averaging.

If you average over one mile you can say Well, we don't know quite where people will be, and so forth, so

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this represents a fair representation of risk to the people living closest to the plant, the average individual risk of latent cancer.

I myself think that if one did that and then used the words you said that when we do this, in fact we have a very low -- if we meet these two, our studies show that we have a very low risk to society, you know, numbers well below one, without giving a number.

MR. ERNST: The fact of the matter is that averaging out to 50 miles you are still going to be very low compared to-- When you aggregate, you are still going to be fairly low. There's a difference between averaging and aggregating.

DR. OKRENT: I'm talking about aggregating now out to 50 or 250.

I think the Commission had a problem -- maybe I'm wrong -- in putting down a specific number as a societal goal, like one or two, or some number like this, as the ACRS and the AIF did. That may be a public-relations problem. But they could say that if we meet our proposed acute and chronic, if you will, risk to the individuals living within one mile, we will have an aggregate over society which is very small, well below one.

Then they will say, "And therefore, we are meeting our qualitative societal goal." They might get

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around having to give a numerical societal goal.

Do you understand what I'm getting at?

MR. ERNST: That's why I think putting a statement like that in there might be useful.

DR. OKRENT: But if you go out to 10 you don't have an average individual risk. If you call it a societal risk, it's not a societal risk.

MR. ERNST: I would submit that how far you average is irrelevant to the technical analysis. You can average out to 50, 10 or one, and you find you're still going to have very low aggregate numbers.

DR. OKRENT: I agree. But nevertheless I think there ought to be a measure of the -- that the risk to the individual who is most exposed, say within the first mile, of cancer is below some goal.

MR. ERNST: I think we have drifted into Question 10.

DR. OKRENT: Well, they are related. It may even be repetitive.

MR. ERNST: On Question 10 we did look at 50 versus 10 versus one mile in some depth.

(Slide.)

It goes without saying that the average cancer fatality risk does increase with decreasing averaging distance. The factors that will decrease are site-dependent

to a large degree. You may find some numbers that will fall outside this range but generally you get about a five to ten increase averaging out to 10 instead of 50, and another factor of five to seven if you come in to one compared to ten miles.

If you look at all these cases though, and we took all existing PRAs, Indian Point and everything, and calculated the actual risks using these various averaging distances, and concluded that for reasonably expected operating conditions, -- and that is reasonable with respect to the safety goals -- in all cases the cancer risk was at least a factor of ten below the design objective for any averaging distance.

Clearly it is much, much below the safety goal if you average out to 50 miles. It is somewhat ess below if you average out to 10 miles. But it is still at least a decade below if you average out to one mile.

Yes?

DR. SIESS: When you look at those different distances, are there appropriate corresponding changes in the assumptions about evacuation or sheltering? If you look at one mile instead of 50--

MR. ERNST: For this case we assumed standard evacuation. I think it was the summary evacuation that we assumed in all these cases.

DR. SIESS: So if it takes 40 minutes to fail the containment there would still be somebody within one mile? That's not much of an evacuation plan.

MR. ERNST: Well, the summary evacuation is certainly not the most optimistic evacuation scenario, and for those assumptions we still got these results. If you have a better evacuation, then there would be less injuries.

DR. SIESS: It seems to me that I can almost-- I can have a high probability of having nobody within one mile, if I had some warning.

MR. ERNST: As I say, we used the summary and I don't think the evacuation summary is that effective for an SSD-1 which dominates this calculation.

DR. SIESS: I really don't understand the term "summary" there. It must be something I haven't read. Can you give me a very short explanation of it? What do you mean by summary evacuation? Is that a description of the evacuation?

MR. ERNST: It's a description of the evacuation. You evacuate certain segments at different warning times and speeds. I forget the numbers.

MR. MARGULIES: Basically it is something like one-, three- and five-hour delays, when people evacuate regularly at about a ten-mile-per-hour speed.

DR. SIESS: It is one scenario out of several?

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MR. MARGULIES: 30 percent of the people evacuate with an hour delay, and 30 more I guess, or 40 more, with a three-hour delay.

But for the release you're talking about, you are not going to have much effect on these numbers with your summary evacuation.

MR. STELLO: I think the easy answer to the question is if you did what you described, go preferentially to evacuation plans close in faster, it would reduce these numbers significantly.

MR. ERNST: That's right.

MR. DAVIS: Were these calculations done with the CRAC-2 code?

MR. ERNST: Yes.

MR. DAVIS: Typically that code assumes that you relocate people when their projected 30-year dose is 25 rem. And in all the calculations I've seen, that dominates the cancer fatalities so that the evacuation model has no sensitivity.

MR. ERNST: I believe our analyses did not consider the relocation. I think we were just looking at the early phase. Is that correct? We didn't want to cloud it up with the re-entry business.

DR. OKRENT: The slide is fine, but I don't know how you draw the conclusion that 10 miles is appropriate.

MR. ERNST: Again the same as the societal aggregate kind of a thing. If nothing is really broke from a technical or safety standpoint, then there is not much incentive to fix it. In this case there didn't seem to be anything really broken because the other aspects, the prompt death and reasonable adherence to a core melt frequency, would provide the safety protection.

As far as the rationale then, it become more of a "Is there a better was to express this that might communicate better to people, to the public?" And the judgment of the steering group, as I best recollect, is that if you go down to one mile, you can say clearly that is not a population, and there is some reluctance to change the terminology if the Commission wants to have what they can call a societal goal.

If you go out to 50 you could advertise that you are watering it down with an awful lot of people who don't get much exposure. Ten seems like a reasonable compromise, and ten seemed to fall in the same general area as what we're talking about for emergency planning, so it might have some public understanding from that standpoint.

DR. OKRENT: I guess I need to understand what do you mean or what do you think the Commission means by the term "societal goal" now.

MR. ERNST: What the Commission meant, without

putting-- I'm gathering what they meant is take a large enough suitable sample of people that they can call it a population that they are protecting rather than an individual.

Now it is true the way they described it you can convert that into the average risk to the individual within that particular population, but I think that is probably true in many kinds of societal risk descriptors, too.

You could convert that to your own individual risk. You do it in automobiles and everything else to figure out what your--

DR. OKRENT: Well, let me go back to Bhopal, and let's suppose-- I don't know how many people lived around the plant, but I will invent a number.

DR. SIESS: They lived right next door.

DR. OKRENT: But within the city.

Let me assume that there were a million people living in the city, just so I can do the arithmetic easily, and that 10,000 people were killed. And again I'm inventing a number. I don't really know what the real number is.

One could say well, the risk, the societal risk was 10,000 over a million, which I gather is one in a hundred, and suppose that had been the only number given out. It appears out that-- given this event, I mean, so it's a conditional probability.

Under the circumstances, the risk to the people living close in was obviously larger than one in a hundred.

The Commission called something a societal goal that I think wasn't a societal goal in the earlier version, and to perpetuate calling it a societal goal but just to reduce it from 50 to 10 seems to me leaves them still in -- you know, with a misnomer.

MR. STELLO: This seems to be an excellent area where the ACRS can consider this issue and perhaps frame some suggestions and guidance which may be very helpful to the Commission in reaching a decision. We have done the best we know how, and if the Committee feels they can develop a better approach to this, by all means.

MR. ERNST: My personal feeling is this gets wrapped up in what we were talking about a few minutes ago, and that is I think you should construct a proper paragraph or two that puts this in perspective.

What we are recommending is we don't need another safety goal to add to it, but we certainly need a better descriptor.

DR. SIESS: I've been sitting up here thinking how I would explain this to my wife.

MR. MICHELSON: How would you?

DR. SIESS: I don't hear the kind of words that I think would put her mind at rest. Living 32 miles from

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Clinton, her mind was uneasy. She has a great deal of confidence in me.

DR. OKRENT: And you're here.

DR. SIESS: We're talking about the public perception.

I frequently try things on my wife. I also think about how I would explain it to the Rotary Club, which is another aspect of the public. And I would hope that there's enough discussion on this thing to make it something that you could tell a layman just what it means. And certainly this business doesn't help me.

MR. ERNST: I guess I might submit that the words the Commission has probably explains to the average person pretty well. I think it is the scientists that have disaggregated this into individual risk and invented this term that is not in the safety goal. It may be the more confusing part.

DR. REMICK: I agree. There are a couple of points I would like to make.

Ten miles to me makes sense. The original 50 miles was selected because it was better than what the Staff practice was of going out to 250 miles averaging in general, and that was ridiculous.

Fifty miles is consistent with Appendix I of Part 50 there on that 50 miles, and that's how it was selected,

somewhat arbitrarily but consistent with Appendix I and better than the practice of integrating out even farther.

So to me the ten miles I think is a reasonable compromise, because you are diluting when you go out as far as 50 miles.

And the argument over whether it's societal, it is not a societal goal inasmuch as you're picking the number of deaths, but it is a societal goal like we talk about a societal goal of farm accidents and things like that, and how you approach it, so it is not that far-fetched from being a form of a societal goal.

I agree very much with what you are saying, Mal, and I'm not sure that there is a need-- I think the goals as they are preclude the need for something that is classified as so many deaths per plant.

DR. OKRENT: Well, I will repeat a statement the ACRS made with regard to NUREG-0881, I guess it was, that when you call something a societal goal this way, and you have a numerator and a denominator, in fact you in no way ask a plant to do more if it is surrounded by a thousand times more people.

And there seems somehow to be something basically wrong in a general philosophy that has that effect.

MR. ERNST: Does that mean if you had a thousand times more people you should still have the same number of

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airline flights?

You would probably have more airline flights and you would probably kill more people in airlines, but the ratio of deaths by airlines to people would probably remain about the same.

DR. OKRENT: You know, I don't think you can justify what you do here or justify an improperly worded philosophy by looking -- by drawing on some other example like that.

DR. SIESS: I'm confused again now. If I've got a thousand more people I've got a thousand more natural cancers.

DR. OKRENT: Exactly.

DR. SIESS: Therefore, I can have a thousand more accidental deaths.

DR. OKRENT: Exactly.

DR. SIESS: Is that good?

DR. OKRENT: Well, for accidental, they look within the first mile, early deaths, and they compare it to the accidental deaths for the people living within the first mile, and I think it is a reasonable way to measure the relative risk to those people.

But here you are not getting a measure of the relative risk to the people living within the first mile of--

DR. SIESS: A societal goal is an absolute rather than relative?

MR. STELLO: We've spent a lot of time talking about this subject, and most of this issue is with respect to the latent cancers. But as I recall, in all cases, early fatality controls.

MR. ERNST: Yes.

MR. STELLO: So whether you meet the health effect or what it ought to be is not governed by this conversation, it's governed by early fatalities.

It is important to find a way to say this as clearly as we can, and since Mal has got to leave here in four minutes, I would suggest that if the ACRS, in debating this and kicking it around, can find some way to articulate something to help, then by all means.

We have spent an awful lot of time arguing and debating this subject among ourselves, and you've got to make some judgments. There are two kinds of harm from different reactors, the early fatalities and latent cancers. It's a judgment of how to express them.

Forrest gave some of the early reasoning, and now we've talked about why we want to pull them back from 50 to 10. And both of them were subject to the same kind of problems. If the ACRS can find something for us to do, I would suggest that we'd be glad to have it.

MR. EBERSOLE: Should the airplane that carries a thousand people be more reliable than the one that carries a hundred?

MR. ERNST: I would think that would be a desirable goal.

MR. EBERSOLE: Ten times as much?

MR. STELLO: Not if you have ten times as many.

MR. ERNST: I think in practicality you wind up with--

MR. EBERSOLE: I understand it's the same, that there is no differential.

MR. ERNST: There is one thought that came to me during the latter part of this discussion though.

Suppose you took one aggregate estimated death per reactor year. I think we're missing too much when we talk about Bhopal, for example. And reactors might be having too much risk in consequences.

If you had an accident that had this SSD-1 and SSD-1 does drive the estimate of one per reactor-year, if you said your SSD-1 is something like 10 to the minus-5 in calculating risk. If you actually had that accident then, conversely, you would be saying you would have 10,000 estimated deaths, assuming you were right at that one per reactor-year.

The question really is there is that acceptable? I mean, that would be well within the one per reactor-year. And I think you would still have the basic Bhopal question in retrospect. If you had that accident, was that an acceptable risk for those people, looking back and Monday-morning quarterbacking. It's the same question as Bhopal but putting in a one or whatever it is aggregate societal risk wouldn't solve that question.

DR. REMICK: I would like to make one more point on this:

If you have more people around the site, don't forget you're going to have more dollars in the bank to make changes and that was a question I had and, I'm sorry, I forgot to ask it earlier:

If you go to ten miles, what are you going to do about aggregating person-rem to determine money in the bank?

MR. STELLO: Mal is going to have to run to make his plane.

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DR. OKRENT: Well Mal has to run for the airport. Why don't we take a 10-minute break? Thank you, Mal.

(Recess.)

DR. OKRENT: We are going to finish this part of the Subcommittee meeting at the scheduled time because we have another one scheduled to begin in the afternoon. So we had better move along.

MR. STELLO: I was going to go back through the items as they exist.

Mal went into some of this stuff -- when is 1150 coming out, what's the schedule for 1150?

MR. DAVIS: July from what we've been told, I think.

MR. STELLO: And that's going to get into a lot of the severe accident issues and they'll come out and get spelled out in NUREG 1150. There are a lot of other related issues. Backfit was one of them. How will things change if you had a safety goal policy, will you be changing the way you do things today?

One minute in terms of backfitting. Backfit Rule 5109 that is now on the books requires us to go ahead and do an analysis and essentially display all of these things. There are clearly some differences, however.

The averted on-site costs are not included in

terms of determining an impact. The 5109 has a substantial addition of protection standard. It isn't in any way quantitative and there is no guidance. There is no algorithm available in the backfit rule today.

If you had the policy statement, in my view, that would allow someone to start using it as a beacon to guide you -- not yet as an algorithm, but in terms of being able to guide you in making some of these backfit decisions. It would lay out how the Commission intends to proceed.

The idea of adding the averted on-site costs, I have already made that point I think that this would clearly suggest that the impetus for causing further changes would be very, very significant at higher core melt frequences, obviously, or for the case where you didn't meet the health effects, however you calculate it. So it would have an effect depending on how the Commission finally formulates it.

I don't think there would be any real inconsistency between the present articulation of 5109 and the present concept of the safety goal except in one particular way that I do believe is very, very important.

When you look at the matrix that we talked about as having a bank of dollars, then I think it says to the regulator as well as to the industry Let's be wiser and think more carefully about where and how to spend those

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dollars to make sure that we indeed are going to get the real significant improvement in safety for the dollar.

Right now it is case-by-case and one at a time and we'll continue to do that. I think having some sort of overall aiming point and keeping that in mind I think would go a long way in making sure that the kinds of retrofit we're talking about really will have significant contributions to safety. That kind of thinking is not there now. I do think that kind of thinking is needed, however. So I think this approach will be very helpful.

Let me just keep -- You have the handouts, I don't know that we need to get into the severe accident policy statement in any detail. Again I don't see any inconsistencies between where we're going and what the severe accident policy as it exists today suggests we ought to go.

I think the real outcome of the severe accident statement is going to be the kind of things Mal already picked up. That's going to be included.

Also the six reference plants in NUREG 1150. I think until that is done we really aren't going to have a real measure of how these two go together. At least based on what I've seen so far I don't see a problem, but you have to wait I think until you're done.

I think the two policies eventually will fit

pretty much hand-in-glove when we're finished but I always reserve -- you know, you're predicting the future by saying that.

Severe accident issues I think are very much what I had in mind when I said make sure we get the real changes we need for safety because I really identified those major contributors; calling them out and identifying what they are, what their likelihood of frequency is and how they contribute to risk. And I think they'll go a long way to giving us the kind of insights to make sure we're working on the right problems.

Let me move to question four. I think maybe the first thought that comes to my mind in terms of apportionment or allocation is not so much trying to decide how to apportion or allocate but getting into the results of the PRA and identifying and understanding what in fact are the real contributors.

Dave's classical example of -- he talked about last time that if you had a single scenario that was responsible for 99 percent of the risk of the plant and all the others were small and insignificant we would clearly say that any changes they ought to be looking at in the facility ought to be looking at that 99 percent without suggesting that that number has to be reduced to not more than 10 percent or 1 percent or 5 percent or some other specific

number.

But if you look at history, if you look at the way we have been doing things as a general approach we have tried to assure ourselves that the contributors of the half-dozen to a dozen dominant scenarios are reasonably distributed. And when we did ATWS, as I recall we did ATWS as suggesting that it ought not to be more than a 10 percent contributor.

And so we dealt with previous issues in terms of looking at what contribution that particular issue -- and ATWS was a good example in terms of dealing with an issue that was roughly about a 10 percent contributor.

I think it would be a big mistake to try to say we know enough about this methodology and application of PRA's to start slicing it up right now; I don't think we're there. I don't think we could, for example, say with confidence that the auxiliary feedwater system, its contribution or system level reliability ought to be no more than a 10 percent contributor or 20 percent contributor or whatever the number is. Or that the ECCS systems be any number because there are so many things that we still don't know well enough to go in and start trying to cut that up.

I know others have tried to do this and I don't think it is -- I don't think we are mature enough yet.

MR. EBERSOLE: I don't know you would get started

unless you start that way.

MR. STELLO: Start what way?

MR. EBERSOLE: You start with the pieces of the plant.

MR. STELLO: Yes, but I don't know how to set each of those pieces --

MR. EBERSOLE: Well you set them and say subject to practical findings as we evolve, what we can do.

MR. STELLO: I'm not sure I follow.

MR. EBERSOLE: I'm saying you have to set some kind of a target date for the reliability of a given piece of the plant, I mean a target reliability, however arbitrary it may be, and then see if you can attain that.

MR. STELLO: We've done some of that. Auxiliary feedwater systems is an example --

MR. EBERSOLE: -- a case in point.

MR. STELLO: -- yeah, a case in point where you've done something. But I'm talking about breaking it in terms of risk contributors.

MR. EBERSOLE: I'm talking about design divisions and --

MR. STELLO: Yeah, there's a big difference in terms of system reliability versus not more than a 10 percent contributor to overall risk from the plant due to failure.

MR. EBERSOLE: I'm saying that gives you a point to start in the design process.

DR. SIESS: These plants are already designed.

MR. EBERSOLE: Well I guess I'm thinking in the context of starting with a new one.

DR. SIESS: This talks about scenarios, Vic. And you got off on the systems. I mean if you had 10 scenarios, each contributing 2 percent to the risk and all of them involve the same system, what would you do?

MR. STELLO: The same thing. Then it meant that that system were -- and you can extract that number and say that system were probably responsible for 9ʋ percent of the risk because of its unreliability, that it would suggest that that's the system I wanted to work on.

Let's take station blackout as an example --

DR. SIESS: This is worded in terms of scenarios and I was saying systems contributing to different scenarios.

MR. STELLO: Like station blackout, the loss of all AC power, that scenario is roughly about a 10 percent contributor now.

MR. EBERSOLE: It's more than that.

MR. TAYLOR: It depends on where the level is of the PRA. In GESSAR it's one of the dominant contributors. Even though it is dominant, it is still down at a low

level. So it depends on -- you have to reference it to the overall but in general I'd say 10 percent of the overall contributor.

MR. STELLO: Let's take another scenario: overpressure transients. They contribute -- and I'm not even going to try to guess at the fractions -- large LOCA's, small LOCA's. There are certain fractions of the risk that is attributable to those kinds of scenarios.

If you were able to find a particular thread that ran through all of these and said that there was a reason that all of these were the contributors is because a particular system or electrical power supply, then that's the kind of insight you really want to get out of the PRA because then it directs your attention to saying that's where I ought to be putting my effort and that's where I'm going to get the -- you'll be able to show that if you are able to increase the reliability of that particular system, you get a fairly substantial change then in reducing risk.

DR. SIESS: So you wouldn't stop looking just when you found the scenarios for level?

MR. STELLO: Oh no.

What I was trying to do is to answer the question are we ready to apportion or allocate risk in some light. My personal view at the moment is I don't think so. I think you can set targets to answer Jesse's question in terms of

system level reliability goals; in fact, some countries are doing that.

MR. EBERSOLE: I thought that's a good start. You know, the service water system ought to have some level of reliability as a design goal.

MR. STELLO: Well we do it with diesel generators, we've done it with DC power supplies, we've done it with --

MR. EBERSOLE: Yes, and this has ascetoic characteristics and then you finally come to an end point and say I must invoke diversity.

MR. STELLO: I really don't know how to cut up the risk part. I don't know if we're ready to do that.

MR. EBERSOLE: Well if you can't cut up the pieces, I don't think we can cut up the whole or identify the whole. I think --

MR. STELLO: I really think you can make engineering judgments about reaching certain levels of reliability.

MR. EBERSOLE: Some things are easier to do than others. Low pressure water is easier to furnish than high pressure water.

MR. STELLO: Yes. And you get a lot more experience too.

MR. EBERSOLE: And you can pump it with engines

and not AC power if you need to. And mainly pumping the water is the essence of safety in this business.

MR. STELLO: There's two of these: there's one, in NUREG 1050 Matthew has given me -- to answer your question, Chet -- in NUREG 1050 how those significant contributors break down in the system level. What we did is we took all the PRA's and then out of the PRA's took the relative importance of each of the systems, so you could see how they are. And in fact if you look at this one it shows that the contribution of the sump -- the relevant importance of the sump at the plant vesus auxiliary feedwater, you can see that this is far less important relative to the auxiliary feedwater system. And it says this is the one that I ought to be paying attention to.

MR. EBERSOLE: You're talking about PWR's.

MR. STELLO: Yes. There's another one for BWR's, a similar one.

We did the same thing -- I'm trying to answer Chet's question and we did in fact break them apart under the system level.

I think one of the issues that I want to come back to at least briefly is in terms of some of the external events. I see us with our current safety goals trying to describe contributions to risk. When we get into the seismic area, this is going to be a very, very difficult

area and we're going to have some tough issues to deal with in terms of making that judgment.

And the principle reason for that is if you're trying to reduce things to the median for the 50 to 85 percent confidence level so that you get the numbers that people generate -- those are very, very large, I think they span an order of magnitude on them -- this could be a very, very difficult area and if you pick a mean out of some of these analyses done on seismic, you could very easily talk about a full order of magnitude and you have to be very, very careful then in making the judgment is that really the controlling -- you know, the significant contributor to risk. You might wind up spending a lot of time, effort and money worrying about modifying a plant with seismic and it may very well turn out that that's money that's wasted and you really don't gain a great deal.

Site distance is another example of a very difficult area to deal with.

DR. OKRENT: How are you going to judge, though, about is money wasted or in fact money underspent? What you have a situation with uncertainty and you're loading the dice without some knowledge, say, I am going to assume this is probably money wasted.

MR. EBERSOLE: Dave, I don't see how anything solid other than taking a view that it's societal defenses

against earthquake and using a percentage of that, that is the only thing you can sink your teeth into and the rest of it, I don't see how you get any absolute figures. I think it's going to have to be relativistic to come to some grip.

MR. STELLO: Matthew has all of these wonderful charts to illustrate this point. If you look at the distributions for the internal, you can see they are much narrower than the distributions you get out of, say, the external events.

DR. SIESS: Are you sure of that? Mal urged to put up three distributions: an optimistic, a pessimistic and a best estimate. You've only got one of those drawn on there. And for the seismic, they've got the equivalent of all three of those.

MR. STELLO: That's why you get this, this is precisely why you have it.

DR. SIESS: It's just the way you treat it, they're not that much different.

MR. STELLO: But for the internal events, when you do all of that you still have a fairly, relatively speaking, narrow --

DR. SIESS: I don't think so. I think if you put the three of them up there, the sensitivity-type stuff you did -- when you're doing seismic hazard and you get a panel of experts zoning the United States, you don't say one panel

is pessimistic and one is optimistic and one is best estimate, you lump them all together and call that an uncertainty, not a sensitivity study. This is something Elliott Cornell has reported on on a recent paper and I think it is worth thinking about. I don't think the uncertainties are that much greater than in the seismic when you really look at the things that you don't know about the internal that you're making a guess at.

MR. STELLO: This chart is initiators. This doesn't go -- this doesn't talk about the phenomenological problems associated with what happens after the core melt. This is just up to core melt. And the uncertainties in terms of whether a pump will or won't start, a valve will or won't work, relatively speaking, those are indeed much narrower and that's all this is dealing with.

DR. SIESS: Okay.

DR. OKRENT: But what is not in your uncertainty picture for internal initiators are the uncertainties about aging, about adequacy of qualification, about some of these valves not functioning under the accident conditions. A variety of kinds of things. They are left out of the uncertainty picture.

We earlier talked about the human contribution and whether in fact -- you know, people are all saying I'm going to use swaying or I am going to use a new EPRI method,

they will agree, they will all get the same answers, they will say there's a small uncertainty in human error. And I don't believe it for a minute.

So we are questioning a little bit whether this lore, these what I will care fairy tales to exaggerate that, the uncertainties are really very much smaller for -- quote -- internal events -- quote -- than seismic is really a well-founded thing that one should be telling children as if it is gospel.

MR. STELLO: If you don't agree that the uncertainties are much greater in terms of estimating frequencies due to earthquakes than internal events, then I can just simply disagree with you. But let me go on.

DR. SIESS: One quick statement: I look at the difference between the results in the PRA that's done by the Staff and one that's done by LS&C and they are usually two orders of magnitude.

MR. STELLO: No, not two.

DR. SIESS: Do you want to bet?

MR. STELLO: Up to one I would be inclined to --

DR. SIESS: No, one is a minimum. Look at GESSAR, it was too easy.

But you know, that to me is some indication of the measure of uncertainty. All the knowledge and intelligence doesn't rest in the Staff and I think the Staff

is usually on the conservative side and the licensee may be on the other side. But still there are differences of opinion about some of these things.

MR. STELLO: I'm not going to debate that, Chet. I'm trying to talk about a difference between two things, external events and internal events. The reasons that you have the spread on the data of why a valve didn't work, why a motor didn't work include things like poor maintenance, operators didn't do what they were supposed to do. It included the kinds of interactions that you didn't inspect, all of that's already part of the data base. It's not complete. You don't have an enormous data base but all of those contributors that caused failures for all of these things are already part of the data base from which we're extracting the information necessary to do the PRA.

It's not perfect, I'm not going to argue that it's perfect. But it is far different from collecting data which already has all of those effects in it to a degree versus collecting these panels of experts and trying to make the judgment of what the seismic hazard is. That's just a world of difference.

MR. EBERSOLE: Let me make a comment:

All of these things you're talking about about reservations about the internal events is primarily due to the retained and allowed enormous interdependencies among

a host of systems which are necessary to simply keep the core under water. And the roots going to that solution is to find a system someplace that's got about three elements in it.

MR. STELLO: That clearly would suggest that you would be able to get a larger increase in safety if you did that.

MR. EBERSOLE: Simple, rugged, non-power, whatever.

MR. STELLO: I agree with you. I don't debate that at all. But I'm trying to deal with a different kind of a question in terms of how to handle two kinds of initiators: the external and the internal and how you're going to --

MR. EBERSOLE: If the internal initiators could be -- if the test case due to failure could be breached by the presence of such a system you would be in high cotton.

MR. STELLO: That's true.

MR. EBERSOLE: And I think they can be designed.

MR. STELLO: I don't disagree with that either. I think they can.

MR. EBERSOLE: And much more comfortably than patching on and on into the ones we've already got.

MR. STELLO: I think you're preaching my line.

I don't know where to go with that. If the

could find a way in which to articulate -- provide some guidance, insight of whether there is a good way to apportion risk it would be very helpful. If somebody's got some ideas, I'd certainly like to hear them.

DR. OKRENT: The only point which I think I tried to make last time is if your goal is 10 to the minus-4, for example, for mean core melt frequency and if your estimated contribution from each scenario is on the order of 10 percent -- and I'm not by any means trying to impose something rigid there -- and if you don't have some single system which is contributing to five of these scenarios, so you have a joker there, to some extent that helps reduce your sensitivity to uncertainty. In other words, something that at best estimate is only a 10 figure goal has to miss by quite a bit in order to swing you into far beyond where you want to be. So there is that kind of merit. It's a kind of management of uncertainty indirectly, okay?

MR. STELLO: Yeah. I think -- Let me see if I capture what you're saying. I want to make sure I got it right.

You're suggesting that you take a typical plant at 10 to the minus-4, look at the dominant sequences and you made a lot of other assumptions. If there is one which is a 50 percent contributor and all the others are smaller then I would want to be focusing on that 50 percent and I agree

with that. But there are a lot of other exceptions you didn't pick up that I think you would want to add as well.

If the 50 percent contributor is a scenario that leads to a release that is not a containment failure and all of the others are contributors that are in fact going to lead to containment failure, maybe even early containment failure, then I might want to wait, dealing much more with one of those others that lead -- so trying to describe all of those things in terms of what you're going to do I think are very difficult but at least the concept is you clearly want to take the PRA apart and make sure you're working the right problem in terms of getting the largest benefit that you could get to public health and safety.

DR. OKRENT: Well I don't disagree with you that you look harder at those that have the potential for larger consequences, and in fact that has to be folded into the consideration, I agree.

MR. STELLO: I said if we can find a way to -- maybe we ought to look at whether we can frame some language even in the policy statement that says when you have a plant that isn't meeting the matrix we talked about or some other safety goal that when you're trying to determine where it is that you ought to be looking and getting a benefit that the kinds of considerations you just talked about are the kinds of things that the Staff ought to be looking at in

determining where to get fixes. I think that is probably worth saying and I think we had an attempt at that in the implementation plan that the steering group came up with. Maybe we ought to look back at some of those words, at some of that policy in there.

Question number five requires a bit of gazing into a crystal ball. The question is:

"What would be the anticipated period of implementation before additional review and possible revision to provide safety goal policy?"

In order to answer the question I would have to assume when is it likely we could get agreement on what the safety goal ought to be, and I'm beginning to wonder whether that's going to take --

DR. SIESS: Agreement among whom?

MR. STELLO: The Commission. The Commission will in fact make that decision, it's their decision to make. But all of us are, if you will, in the critical path since they can't decide something until we offer them something to decide. And we seem to be having considerable difficulty in arriving at what it ought to be that they ought to agree to.

DR. SIESS: Don't give them two choices.

MR. STELLO: Given that we move forward in the reasonable near future I would look to having a reasonably

extended period of time of trying to learn how to use this philosophy and policy and there are a lot of questions that will be coming up that we'll be faced with, including the severe accident policy statement and how it finally evolves: ISAP, which you already brought up, source terms initiatives, the usual backfitting issues. And I think you're looking for a fairly extended period of five or so years of accumulating that kind of understanding before you really can gain enough experience to really feel comfortable that you know how.

But I kind of again feel that it ought to be a little bit of a living document; we ought not to frame it so that it is so rigid that our hands are tied and we can't do anything. I think that would be unwise and that's one of the reasons it's a policy statement is that clearly allows a lot more flexibility than trying to codify it as a regulation, which I think we're very, very far from.

In that same five years or so we ought to have the benefit then of where we are with the containment performance objective. And depending on how that comes out then you might want to add an element to the policy statement on how to deal with that.

Of course, a new Commission five years from now may have different views. They may want to visit the issue sooner, I don't know.

But in terms of developing the regulatory approach, I really advocate a really go slow approach and not trying to decide that all of a sudden we are ready to make some dramatic and precipitous change, but it is one of evolution rather than one of revolution in terms of trying to figure out how to move forward with this kind of policy.

I think it will take considerable time for us all to gain the kind of experience so that we feel comfortable that we have in fact developed the right kind of guidance and policy.

I want to move to topic six. I made the mental note that there is something I failed to say when I put the matrix up and I want to make sure, before I turn to the feature plants, that I make this point which is a very important point in my view:

When you talk about any particular plant in terms of having a number whatever the number is that results from the PRA, that number by itself is a living number. If the management of that plant deteriorates and its maintenance gets bad, its training gets bad, then clearly the representation of that number will no longer be valid and you need to be mindful of monitoring the performance of that plant with time to make sure that that plant is not deteriorating and that those numbers that were reflective of the considerations of the plant and the way the plant was

operating remain valid. So that just simply saying we did it, we're finished and you walk away from it, I want to make sure that the point is made emphatically that the answer is no. Not only the management and the training and how they run the plant and maintain it is important, but also new information.

If all of a sudden we find out that there is some system interaction or some other problem that we didn't understand that is going to create a question of making invalid the PRA that was done, then that question, too, suggests the need to revisit.

So that this is a living kind of a thing, it just doesn't come and go when you're finished with it and then you wash your hands. It is something that will be with us in terms of the benchmark to always go back and test, are those numbers correct and are they valid for a lot of reasons: both from the results of experience and research as well as the actual operation of the plant itself.

Now for the future plants I've already said that I think we want to be striving for the future plants to be at that 10 to the minus-5, the top of the matrix, no more fixes going in. The plants that are coming down the pike in the future, that that's where we ought to be moving and that that ought to be the goal. And I would suggest that that's what the Commission would state in its policy statement,

that that is a view of what the goal ought to be for the new plants; they ought to be coming in at about that level.

But all of what I've said about how to treat where you are, even though you're at the end of the table you're still going to have to look and take apart this PRA, all that goes with it, having done that and then conclude that that's the number that I suggest, that that's the point at which you say you're finished.

DR. OKRENT: May I ask a question?

If I think about the results estimated for GESSAR II by General Electric and then by the Staff and its reviewers, GESSAR, if I remember correctly, fell below that 10 to the minus-5 and the Staff and its reviewers fell above. What would occur then as you view, you know, the regulatory process?

MR. STELLO: As regulators we always get the last vote. It will be our result, not theirs. Until we're finished, we identify what the differences are, we debate them and then we obviously get the last vote. We pick it.

It has to be that way, it can't be any other way. It has to be our judgment -- and Chet is probably correct, ours will be biased on the conservative side. And that I think is proper.

DR. OKRENT: You think that would be the regulatory philosophy?

MR. STELLO: Yes, by definition. It's the number that we agree with, not just the number that somebody comes in with on a piece of paper. We're not going to buy that for sure. It's going to be our view that that is the correct representation, not someone's calculation that is thrown in.

I hope -- as I said, I think a very, very important part of all this is we start finding a way to -- I hate to use the word "standardize" but I can't think of another one -- standardize the way PRA's are done without being too rigorous to allow development of the process and the display of the information and the uncertainties, the phenomenological shortcomings and all that so it is clearly visible and identifiable. We're developing documents to do that but that, too, will take some time to evolve. And I see that as part of the same about five year span to get where we're comfortable with it.

MR. MICHELSON: How do you treat the problem for future plants? At the time you do your PRA's and so forth you don't have the same level of knowledge of the details of the plant that you have for an existing plant, you don't have a three-dimensional full-scale model in front of you to check out your potential for systems interaction and all the other good things. So this creates an additional uncertainty, perhaps, in the bottom line you're coming up

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with until such time as the plant is actually built.

Is there some allowance somehow made in your thinking on -- are you going to be more conservative and in what way?

MR. STELLO: Well first we've got to decide what we mean by "new." New plant not yet --

MR. MICHELSON: A future plant.

MR. STELLO: I'm talking about the CESSAR's and GESSAR's, the ones that are now coming down the pike.

MR. MICHELSON: But they're not built yet.

DR. OKRENT: They're not fully designed.

MR. MICHELSON: And yet we're pulling off PRA's and getting numbers.

MR. STELLO: We are but there are two things about it: you're right that there is going to be that nagging question since we don't have experience with this how do we really feel comfortable --

MR. MICHELSON: Or you can't even see it.

MR. STELLO: Right, but at the same time there are a lot of features that are built into the plant that got rid of a lot of uncertainties, like for fires.

MR. MICHELSON: But they're taking credit for those.

MR. STELLO: Yes, so there are a lot of those that have gone away and now you're coming up with what I

will call a new collection of problems that you don't have a lot of experience in and are you comfortable with. And you have to take that into account. I suspect that --

MR. MICHELSON: The question is how do you take it into account?

MR. STELLO: I suspect that's part of the reason why the Staff's number for GESSAR is different than the Licensee's for trying to make --

MR. MICHELSON: I didn't sense that's why they came up differently. Perhaps you're right, though. I didn't sense that is the case.

MR. STELLO: You have to recognize that you're dealing with something for which you don't have that experience base.

MR. MICHELSON: And some allowance has to be made for that on the conservative side.

MR. STELLO: I agree with that, you have to do it.

MR. MICHELSON: They're taking credit for design and improvements but there are questions about the arrangements which haven't even been detailed yet. So PRA's done at a PSAR stage, for instance, have to be treated differently than those done at an FSAR stage.

MR. STELLO: Well the one we're talking about is the FSAR one.

the FSAR one.

MR. MICHELSON: Which one?

MR. STELLO: The 10 to the minus 5.

MR. MICHELSON: Yes. But in the case of future plants, GESSAR 2, for instance, is not an FSAR level plant.

MR. STELLO: No.

DR. OKRENT: But it's the final design.

MR. MICHELSON: But your understanding of the plant in terms of doing a good PRA is just simply quite different.

MR. STELLO: Agreed. And you have to deal with it to the best of your ability.

MR. MICHELSON: But there is no guidance indicated anywhere to my knowledge as to how to deal with it.

MR. STELLO: The guidance for how to deal with the PRA are the documents that I keep referring back to.

MR. MICHELSON: Is that going to be in those documents, how to deal with a PRA on an unbuilt plant?

MR. STELLO: It is going to be how to display the information and identify where the problems are, to the best of our ability to do that.

MR. MICHELSON: It should be articulated clearly.

MR. STELLO: I would think you would have to

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have a minimum of a paragraph identifying that for the plants that are--

MR. MICHELSON: I should think you would need a whole chapter on that because it may involve a large fraction of the risk factor. We don't know.

MR. STELLO: I don't know either, and without doing it I don't know how you would do it yet.

DR. REMICK: With the proposed change, with the Staff coming out with a core melt frequency of 10 to the minus 5 for new plants, basically what you're saying is that how safe is safe enough is a core melt frequency of 10 to the minus 5. That's basically what it's coming out.

It seemed to me that either the public risk design objectives should be changed so that the goal is one chance in 10,000 of other risks, or that those should be abolished, especially when you have a containment performance guideline because I think any kind of -- you lose any kind of consistency between the two, and it seems to me that you have either got to get rid of those public risks or modify them in light of the change to 10 to the minus 5. And not to do so I don't think would be straightforward.

MR. STELLO: Let me say straightforward what I think, so there is no question about it.

I think that we can describe a level of safety

of what the current operating plants are out there, and how they are safe enough, and that is what that matrix is intended to do. All of those plants out there in my judgment would then be safe enough if they meet it.

But at the same time I say even though they are safe enough and the technology does exist that the new plants can be made better, ought the Commission take the view that they ought to make them better -- can make them better? I think the answer is Yes. I think we ought to make them better when we know how to make them better.

That doesn't say, though, the current plants aren't safe enough, nor does it say that 10 to the minus 5 is -- quote -- what we mean by safe enough. What I said is that matrix is safe enough, and that's all.

A plant at 10 to the minus 3 that met everything else and spent all the money we said they had to spend and they were done, that's safe enough. That says how much you are willing to do, and that describes it.

What I mean by safe enough is that matrix, and I say for new plants-- I'm suggesting -- I don't know whether the Commission will agree -- that for new plants that the policy statement will say for new plants I want you to be here, that that is the Commission's expression for new plants up here.

DR. REMICK: I understand, and I agree with what

you're saying, that that is what you're doing. But I think it should be made obvious that what you're doing is philosophically saying what we say are regulations, we're going to make things -- how safe are we going to make them? We mean a core melt frequency of somewhere between 10 to the minus 3 and 10 to the minus 5. That's what we mean by definition of how safe is safe enough.

It is no longer saying that we believe that you're safe-- How safe is safe enough is when you are no more -- your risk is no greater than one chance in a thousand for your accidental death risk or your cancer risk. The whole philosophy is being changed, and I think the safety goal needs to be changed now with the new thrust.

How safe is safe enough is now a core melt frequency somewhere between 10 to the minus 3 and 10 to the minus 5, depending on how many dollars.

MR. STELLO: I know that this is an area on which you and I do not agree, but you left out one very, very important column, and that is dollars.

I think we start by regulating these plants at the time we issue a license under NEPA, a judgment as to our belief that these plants would reliably produce electricity over their lifetime. And if we knew that they were going to be unreliable and couldn't produce that, I don't think we could make the NEPA finding that on balance, we ought to go

forward.

So inherent in what we do at the beginning of the process, before we actually pass on the judgment of issuing the license is we have made the judgment that this plant is going to produce a product of benefit to society with fairly high confidence that it will do that, and the cost and the risk that the public will be faced for that benefit are outweighed by the electricity that plant will produce.

So inherent in that is what I call averted onsite costs, and costs then become part of it. It's the core melt frequency; it's the health effects in the cost. Then it's saying the Commission, in making the judgment that spending this additional money with a plant that has this frequency and these health effects, and when they spend that money, then that's enough. Then we're finished.

It's all three. It is not just two; it's all three, and it includes averted onsite costs.

Now I recognize that's a contentious area and we're going to get there in a minute, but I'm convinced--

DR. REMICK: What I was trying to say is that the safety goal itself now, if you go with 10 to the minus 5, and as Chet pointed out, you are not only taking it as a secondary goal, not a primary, so the safety goal now is a core melt frequency basically. That is what it is hanging its hat on. And that is a change in policy statement. Therefore, I think that the safety goal itself has to bring that out.

It is not longer-- Basically the safety goal before said to the public, your risk is going to be no greater than one chance in a thousand of your other risks. I don't think that is any longer the case. When you take this to 10 to the minus 5, the predominant thing you are telling the public is how safe is safe enough wtih a core melt frequency of 10 to the minus 5.

Actually you're saying somewhere between 10 to the minus 3, 10 to the minus 5, depending on how many bucks are involved.

MR. STELLO: I recognize you can take that table, that matrix, and make that argument. But it must be made clear at the outset that is not what the intent is. The intent is to cover the full spectrum of plants and describe for that full spectrum when is the regulator satisfied.

He is satisfied depending on which core melt frequency you have, where you are with health effects, and then how much money you've spent. It's all three. And you're right, somebody can pick out a number and say Hey, what it means now is this one number. And if that happens it would be very, very unfortunate, and I think it would detract significantly from what is intended because it is all three of those elements: frequency, health effects and dollars.

DR. REMICK: That was also true of the original, was it not?

MR. STELLO: No.

DR. REMICK: Sure. Core melt frequency was in there, health effects was in there, and dollars were in there; not the same dollars but there were dollars in there.

MR. STELLO: But what were the dollars? The way it was formulated originally is if you met 10 to the minus 4 then you didn't even do the health effects.

DR. REMICK: Oh, no, not originally.

MR. STELLO: If you didn't meet 10 to the minus 4 then you did the health effects. If you met the health effects then you were finished.

DR. REMICK: No, you're talking about Staff implementation. If you're talking about revision and Staff implementation then yes, but the safety goal that the

Commission published did not have that in.

MR. STELLO: I'm talking about how it was intended to be used, and this is a different formulation of how to do all that.

DR. OKRENT: Forrest, two things come to mind from this discussion.

My recollection, and correct me if I'm wrong, is that in the safety goal, 0880 Rev. 1, that the cost effectiveness was applied if you didn't meet the health effects. In other words there was no rigid restriction that you had to meet these, but if you didn't you had to look at possible improvements and if they were cost effective, put them in. And there was no looking for improvements beyond--

DR. REMICK: In an earlier draft, there was ALARA, that you applied those dollars even if--

DR. OKRENT: Yes, but it was dropped.

DR. REMICK: It was taken out.

DR. OKRENT: Right.

Now with regard to the current proposal for future plants of possibly 10 to the minus 5 per year core melt frequency guideline, it does not seem to me that that undoes the stated safety goal concerning individual risk or, if they had a properly defined latent effect individual risk, it would undo that for a couple of reasons.

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In the first place, it doesn't seem to me that in view of the large uncertainties that exist in assessment of risk, even if you know what the release is, in assessment of core melt, even though you have calculated a mean, in an assessment of containment behavior and so forth, that one necessarily needs or even wants some kind of consistent set of numbers that multiply together to give the end result.

In fact I guess I would argue that when we're thinking about defense in depth and keeping in mind the uncertainties, one might indeed look for a core melt goal, a containment performance goal which in fact, if they were really met, would make it too easy, as it were, to meet the health effects goal when you did the calculation for weather, and so forth.

In fact, originally the proposal was laid out that way.

DR. REMICK: You mean 0739?

DR. OKRENT: 0739. In other words, there was a kind of goal for core melt, and there was a kind of a goal for containment. And we knew very well that if you factored weather, and even without evacuation into it, you would beat the stated public risk goals.

But it was a kind of an approach to defense in depth in view of the uncertainties that we felt then and I

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feel now will exist in these things.

I am just saying that if in fact 10 to the minus 5 core melt frequency automatically puts you on the borderline or better than the acute risk goal as stated, I don't find that requiring some restatement of the safety policy.

DR. REMICK: I do, because I think it's a different philosophy, and I'm not differing-- There are a number of ways you could approach safety goals. One is the nuts and bolts that you set a core melt frequency and a containment performance, and that's your safety goal. And that is basically the direction in which it seems to be going.

Then I think you are kidding the public if you tell them that our real philosophy is -- in answering how safe is safe enough is risk, and say it is one part in a thousand. There's an inconsistency there and it seems to me that you ought to throw out the public risk portion.

DR. OKRENT: How are we kidding them?

DR. REMICK: That's an engineering approach, and I'm an engineer and I can understand that, and that is much easier to implement than one that says one part in a thousand. But if you are going to tell the public that our philosophy is one part in a thousand, and then we do inconsistent things along the way, then I think we are

being basically inconsistent. I won't say dishonest, but we're being inconsistent.

DR. OKRENT: I don't think we are kidding the public. I will have to disagree with you, because if in fact we were accepting core melt frequency of one in a hundred per year and the containment performance that it held only five percent of the time and then we said -- and we were looking for health effects of 5 times 10 to the minus 7 early, I would say we were kidding the public, because our engineering requirements were not enough likely to achieve this unless we had Hanford as the typical site.

But if in fact we said Well, if we set engineering requirements which if they are met make it easy to meet this health effects thing, in fact give you some margin for the existing uncertainties in these predictions, I don't see that as kidding the public. In fact, I would say it is reassuring the public that in fact the engineering requirements make it very likely that these plants will achieve and beat the stated goal.

Well, anyway....

But I don't see it kidding the public.

DR. REMICK: Not kidding, but not being straightforward with what is the policy, what is the Commission's policy on how safe is safe enough. And I think

they need to state that as clearly as they possibly can. And I'm becoming concerned that that no longer is very clear. It is certainly not clear to me.

While I can see that it is going on core melt, that's okay if that's what the Commission decides. But I don't think we should hang our hat that it is really risk-based.

MR. STELLO: Since I drafted up that matrix I would like to make it clear it was not my intent to call in core melt. My intent was to make sure that we put the spotlight on where it rightfully belonged, averted onsite costs, which we're going to talk about, put that in and say what ought to be the consideration when you don't meet the health effects they ought to meet the most.

We carried 100 percent of the averted onsite costs except when you were down below 10 to the minus 3, whenever you didn't meet the health effects cost to say that was the NRC saying We are very concerned about the safety of the plant and want modifications made because you are not meeting our health effects.

At the same time we wanted to build in -- I did, and there was a general agreement about the approach -- another element that said If you start to get higher core melt frequencies, that's the regulatory concern. We are concerned over higher core melt frequencies, and we want to

build in an incentive to cause that to be reduced. That's a safety issue.

I am hard-pressed to say that I am not concerned if the frequency of core melt is very high, even if you met both health effects. I think something ought to be done to try to deal with that.

Now the question becomes I've met health effects so at least I can say that the health and safety issue is lesser, but at the same time your frequency is higher than I would like it to be. Now comes the question, the tough question: How much effort, money and dollars are you willing to put in to correct that situation? And that is having the Commission then say with this amount of money being spent for a plant with that frequency and that status of health effects, then you're safe enough.

So it answers the third question: how much more am I going to make you spend? And that's in contrast to the old way of just -- even when it wasn't a thousand dollars per person-rem ALARA, that went on forever. So you could be spending a lot of money not really getting any benefit, public benefit, out of it, not really changing risk very much. And this puts a cap. It says this number of dollars; that's it. You spend them and you're finished.

It has a way of identifying when you're finished for every plant in every category.

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DR. REMICK: I agree that's the intent of the matrix. It does that. You're getting into the point I guess on whether you need to -- you can do that with just the public risk costs, the costs associated with man-rems versus putting in associated offsite -- averted offsite costs.

MR. STELLO: Right. And I think there was a general--

DR. REMICK: My point is your matrix could make the point you're making without having averted offsite costs in there.

MR. STELLO: We're going to come to that. Let's face it then, but I don't think you can make it without having all three elements because there is a very, I think, consistent feeling in the Staff that we also ought to be interested in regulating core melt frequency as well.

DR. OKRENT: Maybe we ought to move into that.

MR. MICHELSON: Let me ask a question on something that is related. I need a clarification.

To what extent is this technique going to be applicable to the kind of cases that have come up from time to time wherein, for instance, it has been determined that maybe QA wasn't adequate at the site, or maybe the materials records were missing at the site?

Are you going to use this kind of a technique to

decide how much money to spend to upgrade the materials records, the welding qualifications, or maybe even rebuild part of the plant because of the lack of adequate documentation?

MR. STELLO: The current Commission rule on backfitting, 5109, specifically says compliance with our regulations is not a backfit, so cost considerations are not part of compliance.

MR. MICHELSON: So you wouldn't intend to use this at all when deciding that you need to upgrade the records on a given plant?

MR. STELLO: No, sir. It's a matter of compliance.

MR. MICHELSON: So how safe is safe enough is not going to be answered in that respect?

MR. STELLO: I hope we haven't said that in doing all of this there is somehow an excuse for not meeting the Commission's regulations. You will meet the Commission's regulations in addition to.

MR. MICHELSON: You mean if you haven't met the regulations, this doesn't even pertain?

MR. STELLO: That's right, and that is covered specifically.

MR. MICHELSON: Yet, having not met the regulations, the decision-maker now has to decide what to do

because in some cases it's impossible now to meet the regulations. You've already welded the pipe, except maybe you can cut it out and reweld it.

MR. STELLO: Well, then you have to decide--

MR. MICHELSON: But you won't use this technique at all?

MR. STELLO: No, sir.

MR. MICHELSON: Thank you.

MR. STELLO: It is specifically precluded by the Commission's regulations.

MR. MICHELSON: I just wanted clarification.

DR. OKRENT: Even if the improvement that would be achieved in meeting the regulations is both costly and not seemingly very effective in reducing risk?

MR. STELLO: That's right. 5109 says specifically you will not do a cost-benefit analysis when you are considering compliance with the Commission's regulations. That's what it says. I don't think there is any ambiguity about that point; none.

DR. OKRENT: I'm not sure it is the wisest way to spend money.

MR. STELLO: But that's the intent of it.

MR. EBERSOLE: Can you argue about whether the regulation at issue really provides safety?

MR. STELLO: Sure. And you can ask for an

exception pursuant to the regulation.

But in terms of doing a cost-benefit to decide whether you ought to comply, the rule says for compliance it isn't backfit. We're not backfitting someone when we tell him to comply with the regulations. He can ask for an exemption and I'm sure if they ask for an exemption they will use every argument available to them to justify why not to do it. And that's permissible, and in fact it encouraged. It ought to be that way.

Moving right along, the time scale, Topic Number Seven.

Do we have any notion of time? That is, I understood the thrust of the question to be if you are at a higher core melt frequency than a lower core melt frequency, you would have some sense of urgency about getting the fix done faster for one for a higher core melt frequency. And that is clearly what would be the case.

But have we embodied in the safety goal policy and do we advocate putting in the safety goal policy the notion of how to deal with the timing?

DR. OKRENT: The Bernero criteria.

MR. STELLO: No, we don't intend to do that. We believe that that ought to be reserved for the prerogative of the decision-makers like Harold Denton, who has to make those decisions on what to do with the plant. If he has to

make the decision that the plant ought to be shut down, he has to know all that is to be known about it and make the decision based on the facts available.

We did not intend nor propose that we would add at this time in this version any time scale for taking the action.

DR. OKRENT: I wonder if the Commission at least shouldn't have some kind of qualitative statement. I recognize that it is not easy to defend any particular time scale, although to make some risk-based arguments, you know, which are plausible but....

Again the question is put there because we talked about it, and it is not the intent to urge it or not urge it, but it is something that one should not cover by accident. Okay? In other words, one should consciously not cover it if he doesn't cover it. This is what I'm saying.

MR. STELLO: I think that you could probably go with some qualitative statement, a paragraph or so that says it is clearly the expressed intent of the Commission, if you want to say that, for results of PRA from which you don't meet the health effects and the core melt frequency is higher in contrast to those at the other end of the spectrum, that any attempt at getting fixes would be done with more of a sense of urgency.

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But that's about all that could be said. I think in terms of trying to be quantitative about that kind of an issue, it would be a big mistake. I think in terms of just what seems to be fairly evident -- obvious that one could say, I don't know that that adds very much or would be very helpful to the decision-makers that have to make the decisions, so to speak in the trenches.

I will again leave the Committee to find some way to articulate a phrase or suggest language if necessary. I would certainly appreciate having the benefit of their guidance.

Let me go to the last issue, averted onsite costs, and let me start by saying, Forrest, I agree that all of what I said could be accomplished without raising the issue. I feel strongly that the issue ought to be raised, and I recognize that there are others who feel equally strongly it ought not to be raised. The issue of economic regulation and the rest of it has come with us and created considerable controversy.

The first issue, because I have someone who has to leave, was a legal question. Can we do it legally? And we brought someone down to deal with is it legal.

MR. DORIAN: It is rare for you to meet I know a lawyer who doesn't say on the one hand, and on the other hand.

(Laughter.)

You may today perhaps meet one, this lawyer.

The question was whether averted onsite costs can be included in the safety goal's cost-benefit guidelines. With the way that Vic Stello has described his matrix today, as a legal matter the legal staff, the senior legal staff of both OGC and OELD can say that the answer to the question is Yes, averted onsite costs may be included.

Now let me explain by analogy why, and I'll be very brief.

DR. REMICK: Can I ask interrupt to make sure? Under the Atomic Energy Act?

MR. DORIAN: Under the Atomic Energy Act.

DR. REMICK: Okay. Not only NEPA?

MR. DORIAN: Not only NEPA. Under NEPA we obviously can. It is easy under the Atomic Energy Act in the ways that Vic Stello described to you today. That's the caveat.

Let me give you the analogy, and make it somewhat humorous.

George Bernard Shaw was at a party at Lady Wembley's house and Lady Wembley went on for a long time about her purity, her ethics and her morality. And after she was finished he went up to her and said, "Lady Wembley, would you, for a million pounds?"

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And she said, "Why, Mr. Shaw, what can I say?"

"Lady Wembley, would you tor five pounds?"

"Mr. Shaw, who do you think I am?"

He said, "Lady Wembley, we've established that. We're only arguing about the price."

Okay. I think my point is that when cost-benefit is discussed, especially onsite costs, if it is a small number of dollars, my point is exactly the inverse of the analogy I just made. If the cost is very small, no one argues about it; if the cost is huge, people argue about it and say why does the Commission want to get involved in economic regulation.

I think that the real question is to what extent does the Commission want to look at not only health and safety effects but, involved in those, the cost benefit assessment in terms of dollars of preventing an accident.

DR. REMICK: One point I would like to make. I understand what you're saying, and it's very interesting. But I ran some calculations earlier. We were talking something about 100 to 400 million dollars. I guess-- Is that small potatoes?.

DR. OKRENT: This is for the industry or per plant?

DR. REMICK: That's per plant. If you put in averted off-site costs, depending on whether you use 1 million or 10 million dollars as the averted off-site cost.

DR. OKRENT: Excuse me; that has to be multiplied by some probability.

DR. REMICK: I have the 10-to-the-minus-3 probability in there.

Over forty years you come out with the yearly probability and you multiply it by a 40-year plant life. So we're talking about a figure in our equation of somewhere between -- well, up as high as 200 million. Maybe that's small potatoes; I'm not sure.

MR. DORIAN: If it were a billion dollars, if it were big potatoes, my point was that at some point you say

enough is enough. And the only question you're arguing about is when do you say enough, on the one hand, and how small is small, on the other hand. And as a legal matter we saying that you can look at averted on-site costs when you're doing your cost-benefit assessment for the guidelines under the safety goals.

In the end -- let me add this: you have to make your final decision based on your complete cost-benefit health assessment. The end decision is a public health and safety assessment: that's why you're requiring some sort of a change, or you decided not to make any change.

MR. STELLO: Let me add some perspective. I don't whether you're aware or not, but Oyster Creek just went into a outage. I came out, I guess, this year some time early. And they spent 170 million dollars during that outage. I suspect a significant fraction of that was backfitting, probably on the order of 100 million dollars. They finished. So when is enough enough?

100 million dollars sounds like a big number, but I think you need to decide what is the total number of dollars that you're justified in spending for a facility for anything.

Now, if 100 million is too big let's cut the number down, if all we're going to do is argue about the number. But the real issue is are we regulating in the

MR. STELLO: I don't like the way you said. I like the way I said it.

(Laughter.)

DR. REMICK: I say that's the other side of looking at it.

MR. STELLO: And I understand.

DR. REMICK: It's a prime example of Big Brother.

MR. STELLO: I think the public has a right to expect its govermment is going to operate and regulate and do its business in their interest. And I think this agency's business includes issuing regulations requiring changes, because that's what we do, that are in the public interest. And if a facility, and the averted on-site costs of an accident can have that kind of a public burden, then I think we ought also to be regulating in their interest.

DR. REMICK: Anything, we can argue, is in the public interest, but a lot of things we do aren't, really.

MR. STELLO: When I think of a lot of the things we have done in the past, I would be inclined to agree.

DR. SIESS: What are you including in the public interest? Ratepayers? Shareholders?

MR. STELLO: Taxpayers.

DR. SIESS: You know, public service commissions seem to make a clear distinction: they think that the shareholders should bear the burden, that they are not the

public.

MR. STELLO: I understand that.

DR. SIESS: I think what you said about the government acting in the public interest is great, but you would have to search pretty carefully to find very many examples of it, even in Washington.

MR. STELLO: Well, I contrast the approach that I've outlined with the way we have been issue requirements for changes and retrofits and backfit, and we have done an awful lof of that. And I contend -- and I don't have the data to back up the number I'm giving you, but my guess is that the total number of retrofits we have on plants are -- and I would just punch out a number -- between 50 and 100 million dollars already. And what have we got for it?

DR. OKRENT: I would like to look at a different aspect of that table, if I might, the region between 10-to-the-minus-3 and 10-to-the-minus-4.

As you know, the task force seemed to feel that one should work toward a core melt frequency of 10-to-the-minus-4 for existing plants. And in fact Minogue also indicated that in his memo. Denton, I think-- Maybe many people were talking about different core melts: I don't think so. Denton seemed to suggest somewhat more stringent a goal for existing plants. And the ACRS did recommend in, I guess it was July, that in fact one work toward

10-to-the-minus-4 core melt frequency, mean core melt frequency, and that cost-benefit should not play a major role in striving toward that goal.

It seems to me that if one were to adopt the philosophy, for example, of the ACRS letter, and one still tried to live within a framework of the sort you have generated, you might still keep this framework but have greater incentives, financial incentives, if you want to put it that way, because whatever you choose has to be somewhat arbitrary, I think, in the region from 10-to-the-minus-3 or 10-to-the-minus-4, where right now it says somehow 10 sliding to 1 percent -- and I'm not quite sure whether that's a function of how close you are to 10-to-the-minus-4, but I assume you've got some kind of graph with a straight line.

DR. SIESS: On a long scale.

DR. OKRENT: I don't know on what scale, unfortunately.

But if one were to take the point of view that at a little below 10-to-the-minus-3, that's really not -- especially with the uncertainty therein: we've been talking about incompleteness, omission, and so forth: that one should try to get close to the 10-to-the-minus-4. One might say equally well, Well, not take 20 to 2, or 50 to 5.

How did you decide 10 to 1? I'm trying to

understand.

MR. STELLO: I just happened to want to divide things by a factor of 10. The 10 was intended to mean 10-to-the-minus-3. I wanted to be ten times worse if you didn't meet the health effects, because I think that's the bottom line, safety. The health effects you're meeting, and I wanted it to be ten times worse.

DR. OKRENT: But it could be 50 to 5--

MR. STELLO: I don't care what numbers.

DR. OKRENT: --and that would exert--

MR. STELLO: I'm telling you why I picked the ones I picked.

DR. OKRENT: Okay.

I would still have a factor of 10, but I would be-- Would it be 50 to 5?

MR. STELLO: No; because if you didn't meet the health effects you'd have a factor of 2 difference. And I think it's far more important--

DR. SIESS: If you don't meet the health effects it's 100 percent?

MR. STELLO: No. I'm sorry.

DR. SIESS: It's 100 percent if you don't meet the health effects.

MR. STELLO: Slow down.

The way it is now it's 100 percent if you don't

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meet the health effects. And then it's 10 percent if you do. If Dave changes the 10 to 50, then there's only going to be a factor of 2 difference between where you meet the health effects-- And I didn't like that.

DR. SIESS: What's 100 percent worth?

MR. STELLO: Forrest just gave you the number. At 10-to-the-minus-3, it's between 100 and 400 million.

DR. SIESS: And at 10-to-the-minus-4 it would be...?

MR. STELLO: 10 to 40.

DR. SIESS: Which is either a dedicated RHR or--

MR. STELLO: No, no, no, no; that's not all of them. You've got to add the person-rem. That's another 40 million, roughly.

DR. SIESS: 10 percent buys you a lot.

MR. STELLO: Yes.

What I didn't like about what Dave said, if he changes the 10 to the 50, then instead of having a factor of 10, more impetus, when you don't meet the health effects you have a factor of 2.

I think health effects are very, very important. That is, in my view, our business. And I wanted to make a big difference between whether you met the health effects or you didn't.

DR. OKRENT: Well, we have no less an authority

than Raustrom who recently corrected on an estimate of the risks. I thought I had read somewhere they were 40 on the one hand to 200 on the other hand, the uncertainty. And I think he said 40 to 400.

So what does it mean to meet the -- you say "meet the health effects," when there's this big uncertainty in whether you've met them. And that uncertainty is bigger than the difference between the factor of 2 or factor of 10 you've talked about.

MR. STELLO: I'm trying to make you understand why I picked the number. I picked them because I wanted to put more emphasis on driving a fix when you didn't meet either of the health effects than for the case where you met both of them. I think that's an important distinction.

MR. STELLO: But there's another concern, and that is that maybe, given a core melt, we don't know the health effects that well and so let's -- in view of the uncertainties and how well we know the likelihood of core melt anyway, let's put some additional emphasis on decreasing the core melt frequency when it appears to be pretty high. To meet 10-to-the-minus-3 or 9 x 10-to-the-minus-4 is pretty high. And at that point it's hard for me to--

DR. SIESS: He just said to put more emphasis on the one with least uncertainty. There has got to be more

uncertainty in the health effects than there is in the core melt since it occurs two steps further down the line.

DR. OKRENT: Look, if the answer were 1 in 10, the uncertainty might be very small, it could only be 10 in one direction; right? That doesn't mean you don't put any emphasis on correcting it.

Well, I'm just saying that philosophically one could find a logic for feeling uncomfortable with too large a core melt frequency from the safety point of view. I'm not talking now about protecting the shareholders.

Anyway, it seems to me certainly within the Committee, if we accept the matrix format we want to look at the proposed numbers, and in light of our emphasis on trying to approach the 10-to-the-minus-4, that box might -- for existing plants, that is -- might warrant a little more review.

MR. STELLO: I agree. If you want to reveiw that. There is no magic. I explained my rationale.

But to at least comment on what you said: this calculation when you meet the health effects-- Since you're not meeting them I hope that you would have more concern over safety when you don't meet the health effects than for a case where you did.

DR. OKRENT: Again, right now I don't know whether I should have more concern about a calculation tells

me the core melt frequency is 9.9 x 10-to-the-minus-5, but I omit the health effects. I'm not talking about Big Rock Point. And another calculation that says the core melt frequency is 5 x 10-to-the-minus-5, I'm not blowing up any pressure vessels, but somehow I don't estimate as effective -- I'm not claiming as effective a containment performance because I'm not quite that sure about the integrity of the containment at high pressure and temperature, and so forth. And so maybe they missed the health effects a little bit.

It isn't clear to me--

MR. STELLO: I think you're cheating with the numbers a little bit, though.

Let's take two cases, both are 10-to-the-minus-4. One case I don't meet the health effects and the other one I do. The uncertainties in the 10-to-the-minus-4 are the same.

DR. OKRENT: That's a simpler case.

MR. STELLO: Oh, yes, indeed.

Now, where I don't meet the health effects I want to be more concerned. And I think you ought to be.

DR. OKRENT: I said that's a simpler case.

MR. STELLO: But that's what we're talking about.

DR. OKRENT: No, we're not.

DR. SIESS: They're both 10-to-the-minus-4 and you say one meets the health effects and the other doesn't.

the calculation of health effects is very uncertain.

MR. STELLO: Yes, for both cases.

DR. SIESS: And so it might not meet it.

At 10-to-the-minus-5 the chances that both would meet it are a lot better than are at 10-to-the-minus-4.

You've got to realize you're arguing from different premises. You're talking about your core melt probability as a societal cost type of thing, and Dave is talking about core melt as a challenge to the remaining part of the system.

DR. OKRENT: Right.

MR. STELLO: You've mischaracterized me. I am talking about core melt as a challenge to health effects, public health and safety.

Now I take the example, two cases: (a) 10-to-the-minus-4, I do the calculation, consequences, I meet health effects; Case (b) 10-to-the-minus-4, I do the calculation, I don't meet health effects. The uncertainty bands, there's no reason to believe they're any different. So if the uncertainty in the case where I didn't calculate meeting health effects caused me to meet them -- that is, to exceed them because of uncertainties -- then it's even more so for the case where I already calculated you did meet them. And I say as a regulator I am far more concerned when the result of my calculations said I did meet the health

effects, and I would think the ACRS would have the same view. And I would encourage you to have the same view.

DR. SIESS: In that example you're correct. But in your previous discussion of the core melt probability you were talking about societal costs. And that is not entering into Dave's argument.

MR. STELLO: Yes, that's why I told him I think he was cheating a little bit with his numbers.

DR. OKRENT: No.

DR. SIESS: There's two separate things: you know, one is to get a set of numbers and the other is to justify them. And we're mixing them both up here. Maybe we ought to find out what numbers people like.

DR. OKRENT: If the Staff knew so much about containment -- and I mean all of the containment, not one containment, how they behave in their failure modes, and so forth and so on -- that they were now able to, with great confidence, write a containment performance criterion and convince everyone of this knowledge, you know, maybe I would feel differently. But at the moment I'm somewhat skeptical that all containments, for example, all dry containments will behave as well as is calculated for the two or three that have been calculated. I know the two or three that have been calculated will behave that well, but I'm skeptical that all will. It's just my nature, you know.

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DR. SIESS: Dave, what level of confidence would you like to have for containment? Would you like the same level of confidence that you have now in the DBA Part 100 type thing, whatever that might be?

DR. OKRENT: I don't think we'll get that good.

DR. SIESS: Have you got any feel for how much confidence you've got that the containment won't leak more than .2 of 1 percent?

DR. OKRENT: Well, I'm assuming the containment is not likely to leak more than 1 or 2 percent; okay? The .2 percent I'm not confident in.

DR. SIESS: If it leaks 1 or 2 percent we're darn near home free, because you can't overpressurize it.

MR. STELLO: I need to close with two thoughts.

Comment No. 1, the promissory note. We delivered. You have that table that breaks down the experience data from the IAEA.

The second point, Dave: Let me conclude again by urging that-- We talked about a lot of issues. --that we can take each of these issues and the ACRS can deal with them and provide some advice that would be available to us as we try to write this document in the sense of urgency by the Commission, to try to have something in front of them. To the extent you can take positions we talked about, make a decision, say what you think we ought to go with it, let us

have it, we'll say what our view is, and the Commission will have our view. If we agree with you, both us and your views supporting the same concept, I think that will make their decision easier. But not having an idea of where you stand on a lot of these issues has got to make their job a lot tougher, too, and I hope you understand makes our job--

DR. SIESS: We're supposed to decide where we stand?

MR. STELLO: No, no, no; I say try to decide what issues you can come up with some commonality of view on, and give that advice. Not all issues; which ones you can. I recognize that you can't--

DR. OKRENT: In fact, the agenda was modified so that there would be a couple of hours for the Committee to try to decide on some of these issues. You and I both know they're difficult. But maybe there'll be some success.

Anyway, there is going to be some full committee time, and not just a subcommittee report.

MR. STELLO: Well, I know that the ACRS always rises to the difficult issues, and those are the ones they can deal with most of the time.

MR. STELLO: Okay; with that we'll recess the meeting until 2:45.

(Whereupon, at 1:50 p.m., the subcommittee was recessed, to reconvene at 2:45 p.m., the same day.)

AFTERNOON SESSION

(2:45 p.m.)

DR. OKRENT: The meeting will reconvene.

If I understand correctly from Dr. Savio, we should go to Agenda Item Four, discussion of the Millstone 1 PRA. So we will do that.

And Chris Grimes is the lead-off man.

MR. GRIMES: Yes, sir.

Good afternoon, gentlemen. My name is Chris Grimes. I am the director of the Integrated Safety Assessment Project Directorate, PWR, Division B. Our office is responsible for conducting the integrated safety assessment program for Millstone 1 and Haddam Neck.

And as part of the licensee's work under this program they have prepared a plant-specific probabilistic analysis to provide probabilistic insights and measures of safety importance for specific issues, much like that that was done under the systemmatic evaluation program. And the reason that we are here today is to provide this Subcommittee with a description of what has been done thus far in its review.

We will start off with the representatives of Northeast Utilities describing what they have done and what they have found. And we will after that go into a Staff presentation to describe the results of the Staff and

contractor review of their probabilistic analysis, the significant findings, and then where we are going to take those significant findings.

As requested by the Subcommittee, we will also describe a comparison of the results found in this study with the previous Millstone 1 IREP and a comparability with the Millstone 3 probabilistic safety study to provide you with some information on the different ways that the studies have been performed.

DR. OKRENT: Could I understand better than I now do what the function of the probabilistic risk analysis is supposed to be?

MR. GRIMES: In the contractor's discussion of their review of the probabilistic study they will get into how these results will be folded in with other issues and how they will be applied to other issues in the overall context of the integrated assessment.

DR. OKRENT: I read that part of the review. But let me see again if I understand.

Is every ISAP plan supposed to do some kind of a PRA?

MR. GRIMES: That's correct.

MR. SIESS: Both of them.

DR. OKRENT: Both of them.

(Laughter.)

DR. OKRENT: Well, thank you.

And how does one decide the -- I guess what's called the level of the PRA that should be done for an ISAP plan?

MR. GRIMES: In the context of the pilot program the Commission directed that we start with volunteers. So we have taken the probabilistic safety study as it was presented to us.

And one of the things that we hope to develop from this is a recommendation in future work; you know, what level PRA should be done. To a certain extent as we go through the findings here we will talk about what other things we have learned in SEP that relate to information; for example, related to external events. It may be that as a result of this work we identify a need for further probabilistic analysis.

I know the licensee is already looking at additional work and improvements in the study they have got now, in addition to creating plant-specific probabilistic studies for their other facilities.

DR. OKRENT: Okay.

So in this particular case, though -- You said you were willing to -- This was a voluntary program, at least with regard to the PRA part, and it was the licensee's choice.

MR. GRIMES: We will accept whatever the licensee wants to submit.

DR. OKRENT: Okay.

MR. GRIMES: And then, you know, judge the adequacy of what we have got to work with.

DR. OKRENT: So I can ask him why he did it a certain way, and nevertheless he wouldn't be deficient in your eyes if he didn't do one thing or another because it is a voluntary thing.

A different kind of a question: How did you judge what would constitute an adequate review of what he submitted? I saw it was done a certain way and so forth. I'm just trying again to get a better feel for this part of the process.

MR. GRIMES: I will make two observations:

One is that as a matter of expediency and schedule, Millstone Unit 1 was first both for the utility and for us because IREP had been done, and so we had a basis, a starting point that we would not otherwise have if we started from scratch on some plants. So the review process is in that sense different than the review that we would have done on any other plant-specific probabilistic analysis.

And then in another sense there is a Staff procedures guide for reviewing PRAs. We hope to be able to

do a comparison with what we do on Haddam Neck with that procedures guide and then try and develop or refine or improve or focus a Staff procedure review for PRAs in any subsequent ISAP efforts.

DR. OKRENT: Okay.

But the Staff procedures guide was not the basis for this review?

MR. GRIMES: No, it was not. There was not sufficient time, nor resources, available to do a full PRA review as we have normally done.

DR. OKRENT: And again, so I can understand, you say there were not sufficient resources available. In fact I have heard that in connection with another review lately.

How is it decided what is the needed resource? Does somebody just say, 'Well, here is $30,000; do what you can.' Or, you know...

MR. GRIMES: In this case, because we had done the Millstone 1 IREP study, we took as many resources as we felt we needed to do an adequate review of this study. I can't say now what we might find after we have collected all this information together; that we might not recommend some further study of specific aspects of the probabilistic safety study for Millstone 1. We might find that what we have done is sufficient. We might conclude that we need to do more.

DR. OKRENT: Okay.

MR. GRIMES: One area that we plan to do more in is the licensee has indicated that they are nearing completion of an internal fire study. And we certainly would like to see that and how it affects, you know, dominant contributors to risk.

They also excluded internal flooding, as I recall. And in that sense we are relying on the work that we have done on SEP to give us a feel for how important that--

DR. OKRENT: You are taking away all my questions.

MR. GRIMES: Thank you, sir.

(Laughter.)

MR. GRIMES: I am pleased to know that I have anticipated properly.

DR. OKRENT: Are there any other questions for Mr. Grimes at this point?

(No response.)

DR. OKRENT: All right. Let's go on.

MR. GRIMES: I would like to turn over the meeting to Northeast Utilities to provide a description of what they have done.

DR. OKRENT: Carson?

DR. MARK: Perhaps I have a question.

One of the things was the comparison you guys or somebody contracted with IRA.

MR. GRIMES: SAI?

DR. MARK: SAI.

DR. OKRENT: I think there is a difference.

(Laughter.)

DR. MARK: And I figured out there were about 4200 lines in that report. And one of e lines said, after pointing out that something was 3.6 and some other estimate was 3.4 -- 4.4, the difference was therefore .9. You know, great information. And if you paid $4000 for that report, you are paying a dollar for that line. But I'll bet you were paying $45,000 for the report and you paid ten dollars for that illuminating wonderful line.

What did that crazy thing cost, because it had nothing in it.

MR. GRIMES: Since SAI will follow the utility's presentation, maybe I will let them describe that. But the cost of the report itself was a small fraction of the effort that they put into going through the plant, delving into the analysis, walking through the event sequence.

DR. MARK: Look, they were making -- because of limitations of time and of funds, they weren't doing anything except reading one report, reading the other report and pointing -- bringing to our attention the differences in

those two.

MR. GRIMES: I would like to point out that the reason that they did that is because of their close familiarity with the work that was done in IREP. And the reason that we selected SAI as the contractor is because of their knowledge.

DR. MARK: I'm not complaining aboiut SAI at all. I'm just wondering about the process in the organization of asking for a report which, as I read it, told me nothing that you guys couldn't have learned in an afternoon by reading the reports. And I'll bet you it cost 45,000. And if you are short of funds you might want to think about it.

Let's drop the subject.

MR. GRIMES: Yes, sir.

MR. ETHERINGTON: Delighted.

MR. KACICH: My name is Richard Kacich. I'm with Northeast Utilities.

Copies of the handout that we're going to use today should be passed around the room and available to everyone now.

(Slide.)

I'm just going to do one slide which is already shown up here which identifies the contingent of six people from Northeast Utilities that is here today. And we're

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here for this subcommittee meeting, and, as I'm sure you're aware, for the full committee meeting tomorrow for the portion of the agenda that's devoted to the provisional operating license to a full-term operating license conversion for Millstone 1.

Briefly the individuals are:

Mike Bain, who is in our licensing group and was very heavily involved in the SEP review from our standpoint for Millstone.

John Bickel was the supervisor of our PRA section. And John is going to be giving the majority of the presentation today since it focuses on the PRA specifically.

Mitch Lederman, who also works in our licensig group, is heavily involved in our ISAP activities.

Wayne Romberg, who is the Millstone Station superintendent.

And John Stetz, who is the Millstone 1 Unit superintendent.

Briefly, as I believe you already have copies of it, but the Millstone 1 probabilistic safety study was performed during the '84-'85 time frame and was submitted in July of this year. And it was done for a variety of reasons that John will get into; but among them is the fact that we're using it as an integral part of our ISAP program that

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we're in the midst o implementing for this unit.

Without any further ado, I will introduce John.

DR. BICKEL: Thank you, Rick.

Good afternoon. I am John Bickel, supervisor of probabilistic risk assessment, Northeast Utilities.

My first point I would like to make about the probabilistic safety study was our motivations in performing the study.

(Slide.)

The majority of this -- and I think you have heard this in other presentations our company has made -- is we are embarking on the development and maintenance of living probabilistic risk assessment models for all of our units. This was an internal corporate decision. Although it has some benefits and uses in our interactions with the NRC Regulatory Staff, the decision to do this was an internal one which we are doing independent of any actual formal requirements.

The fact that ISAP requires having a PRA to pursue it was just an additional piece of -- you know, reason why we did it.

(Slide.)

We are pursuing the development and maintenance of these living PRA models for the three items I have shown in this slide, the first being is we wish to use PRA in

doing safety evaluations on plant design changes of significant safety importance.

Additionally, the PRA is being utilized in doing safety evaluations of technical specification changes. We find it is a very efficient way in evaluating when trade-offs are being made in one area versus another. And this is something we, as an operating utility, have to deal with on a very frequent basis as part of our day-to-day business.

The third reason, which is I think the majority of the interest in why we are here today, is the use of the PRA in the integrated safety assessment program. I will be trying to touch briefly on, you know, areas where we use the living PRA model on Millstone Unit 1 in all three of these areas.

(Slide.)

The desired end products, therefore, when we initiated the Millstone Unit 1 PRA were for the four bullets I have listed in this slide. The first thing is we want to identify and have a tool which is capable of studying and identifying significant safety issues on the plant, be they generic or be they plant-specific things that are unique to our own plants.

We additionally want to understand the engineering issues and implications of some of the safety

issues which are out there and which may exist in terms of plant-unique features on Millstone Unit 1 in terms of engineering insights. If I have a system that has a predicted unavailability, what is the most dominant piece that contributes to that unavailability.

The other thing is we want to have the PRA model become a living tool for use in future safety evaluations. It is probably no surprise that utilities face hundreds and hundreds of individual changes being made to our plants on a year-to-year basis, all of which require a review of their safety importance and significance.

Additionally we want the PRA model that we established in the study which was docketed in July and which I see laying there on the table to form the framework for future modeling which we will add on in the external events area.

One of the immediate questions, you know, that comes to mind and which I will only touch on briefly is why did we not just immediately pick up the Millstone 1 IREP study.

(Slide.)

We were in fact rather active participants in the preparation of that study several years ago. And it would seem to be a very simple issue to just take what we had and, you know, try and clean it up. There were a number of

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reasons which we feel compelled us to go out and rework a lot of it, and I would like to briefly go through those.

The first of those was the ease in using the models that were developed in the IREP study. The models that were developed in IREP first of all are generally based on the SETS computer code which we do not have at Northeast Utilities and which would be a burden for us to try and install and use.

Additionally, there were a number of significant changes between the time period of 1980 and 1985 which expand for the time that the IREP study was started.

The major change in the procedural aspects of running the plant occurred with the adoption of the generic BWR Owners' Group work on the emergency procedures. They call them emergency procedure guidelines. These were implemented on Millstone Unit 1 and have been for a number of years, and in our opinion have a very dramatic impact on recovery actions and mitigation of accidents beyond the design basis.

The fourth item was that in our perception there were modeling deficiencies in certain areas of the IREP. I would not say that they were -- you know, it was all-encompassing and was all across the board. There were a few areas that we felt needed to be cleaned up.

In some areas there was the scope of the

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initiators considered and the frequency that were assumed for them. We felt that it was important, if this was to be a living model, reflecting the best we knew about Millstone 1, the frequency of the most likely initiators should be very reflective of the actual operating statistics of the plant.

The success criteria we felt that was used in IREP was limited because of the fact there was not really what one could call a budgeted time set aside to develop actual best estimate calculations of all phases that were critically important to understanding the risk of the plant. We felt in a reworking we wanted to firm up what was the basis of the success criteria, both in terms of its best estimate, but also in terms of the uncertainties that were bound in the success criteria.

We wanted to use actual plant reliability experience in the report or in our analysis because we think that if there is a squeaky wheel we ought to give it attention. Using the PRA with the actual plant statistics will force us to focus on actually important problems.

Additionally we felt it was necessary to have it be a credible state of the art PRA. There had to be a major upgrading in the area of common cause failure analysis, particularly in the dealing of common cause failure among like components. The IREP process treated common cause

failure mainly by looking for merging of fault trees and looking for commonalities that way. In some areas that was effective; in other areas it missed things because of the complexity of the models.

An additional area we felt was important to address was the coupling versus uncoupling of human errors. As an example, if an operator makes an error in deciding he has to restore water level or, you know, there is a need to take manual control of the water level in the reactor vessel, if he makes that error once you don't need to ask is he going to make it again when he considers a different system. That is a symptomatic -- He has made the error; he is going to continue to make it.

The IREP models had a tendency, because of the way they were developed, to include the chance that the operator is going to question recovering levels several times with different systems. We viewed the issue as being one of a coupled error.

(Slide.)

The scope of our model as a starting point --

DR. MARK: Excuse me.

DR. BICKEL: Yes, Dr. Mark.

DR. MARK: You are making the assumption there that the operator learns nothing. You say if he does it once he will do it again and he'll do it again and he'll do

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it again, I believe.

DR. BICKEL: Yes. What I'm referring to is --

DR. MARK: I don't regard that as, you know, necessarily realistic.

DR. BICKEL: Yes. What I was referring to was that if one had an event tree that said, 'I've got a condition where water level is decreasing in the vessel and I have an event tree that shows maybe three different notes addressing the availability of systems that can put more water in,' they essentially ask the question does he restore it with feedwater, does he restore it with core spray or does he restore it with, you know, LPCI water.

In fact what we believe is is does the guy make a, you know, make an early diagnosis of the fact, 'Hey, I've got to put water in the vessel,' in the first place. And that's one of the areas that we felt was necessary to model. That's one of the major changes that has been done in the event tree modeling of the study. That's what I was referring to.

DR. MARK: Well, I maybe understand what you're referring to there. And I'm a little bit uneasy about the apparent assumption that this human error factor is, in the first place, known at all and, in the second place, propagated correctly. That is, a human being is not a piece of statistics. He is a real thing. And if he forgot to

put the water in the first time, that doesn't mean he will forget it the second because he might be all the more eager to put it in the second. And if he did it wrong the first time he might have become accustomed to doing it right the second time.

And to use this number as a measure of actuality troubles me a great deal.

DR. BICKEL: Yes. I understand the issue and I guess the significance of the uncertainties that are involved in that. And within the scope of what we were trying to do we have made a stab at it. But I would acknowledge that this is a difficult problem, predicting statistically how people are going to behave in a given situation.

DR. DAVIS: John?

DR. BICKEL: Yes.

DR. DAVIS: Do you have a feel for how important that kind of assumption is in a PRA like this?

DR. BICKEL: Our understanding of the thing was that the key impact was that it shifted a number of the sequences from IREP that were originally non-dominant to the ones that stood out very prominently, particularly in situations where the operator was called on to take action fairly promptly.

In other words, as an example, a loss of

feedwater event where the isolation condenser failed, the operator has got to do something quick. The old modeling said -- you know, he revisited the issue of should I do something to control water level several times. What we are saying is that he has made that decision once; he has got a limited period of time to do it. He is either going to make -- If he makes a decision he is going to do it, then it becomes an issue that we have addressed a cognitive decision versus procedural error, you know, in turning on the pump correctly.

DR. DAVIS: So that could be a significant impact--

DR. BICKEL: Yes.

DR. DAVIS: -- how you model this kind of thing.

DR. BICKEL: That's correct.

DR. MARK: And is it not, as Peter is thinking, I believe, almost guaranteed conservative; that is, things are not that bad?

DR. BICKEL: I would say we have made the best stab we have at what I call being a best estimate value. Obviously there is a great paucity of data. There are techniques which people, you know, bat around as being state of the art, things like time reliability correlations and all that kind of stuff. But it is the best shot that we know how to take at this point.

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We would be looking in the future when we have our operating simulators at collecting this type of data; in other words, force some errors through the system and watch how the operators respond. Will they catch these types of things in time. Maybe with a number of years of that type of experience and collecting that, we would be in a better position to do that. We aren't right now.

DR. MARK: Are you and Northeast Utilities foreseeing the possibility of actually acquiring some data on this subject?

DR. BICKEL: Yes.

DR. MARK: Because at the moment I regard it as a thing on which there is no data but some assumptions. And it would be marvelous to think that one began to acquire actual data.

DR. BICKEL: As a brief sideline, maybe leaving the subject, we are engaging right now with EPRI on such a project in terms of a pilot thing. We are going to be starting it up some time next year, people and time permitting.

But I guess, you know, this slide essentially summarizes the scope of what we carried out as the first step of our living PRA model. And again I point out this is the first step; this is a level one PRA. It is the basic framework from which we will add all future additions to

cover other issues onto. It uses large event trees, large fault tree modeling. It uses an extensive amount of best estimate safety analysis performed using very sophisticated computer codes.

In other words, we have attempted to try and benchmark and show exactly what we are saying when we say that this is a best estimate number. We have considered the usual scope of initiators similar to what was considered in the IREP. We extended it to cover a few more which we felt we wanted to know a little bit more about.

We have attempted to make a maximum use of plant experience in the computation of initiating event frequencies -- components, system reliability -- in its system maintenance outages.

So what I am trying to say is we have gone through everything to try and make this a fairly accurate reflection of the type of reliability experience we have seen to date.

(Slide.)

As examples, this is typical of some of the data which was collected which we thought was important to get into the study. We have computed breaker reliability on Unit 1 based on actual experience and we do see trends that the numbers we experience in our plant are different from what people call -- quote -- "generic" data.

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Now the data for the 4160 volt breakers is essentially from a population of over 34,000 individual demands which were logged and recorded by cycle counters on the breakers. We have got similar data coming from the 480 volt breakers.

(Slide.)

Additionally, we have data which we have collected on our diesel and gas turbine units. And again the ability to actually look at actual failures and the actual total number of demands placed on those units both in periodic tests and in actual system demands on them allow us to understand that, you know, the number is a little bit different from what people call generic data, or WASH-1400 data.

(Slide.)

In terms of --

DR. OKRENT: Excuse me. Before you get into this--

DR. BICKEL: Yes.

DR. OKRENT: I have looked at a couple of PRAs in the last week or so. And one of them -- I can't remember which --

DR. BICKEL: It wouldn't be Millstone 1, I suppose?

DR. OKRENT: -- one of them, if I'm not wrong,

had a statement in it that the contribution of human error to the overall coremelt frequency was relatively small. Is that something I would find in yours if not in the others? Do you recall?

DR. BICKEL: Are we talking about Millstone 1?

DR. OKRENT: Millstone 1.

DR. BICKEL: I would sa, the contribution we found of human error was not very small at all.

DR. OKRENT: Okay. Then thank youi.

DR. BICKEL: Ge ierally we find that the older plants have a tendency to be more reliant on a lot of manual actions. That's typical of, you know, the vintage and period they were licensed in.

I show here --

DR. OKRENT: Excuse me. If I can follow along, I remember more about why this was the case according to this PRA.

It was the use of symptom-oriented procedures in which they then concluded that the operator pretty much was running using skill, but he didn't have to use cognitive processes hardly at all. And therefore the human error rates were smaller.

It must have been the other one. Okay.

MR. EBERSOLE: John, may I ask you a question? You said you used plant operating data.

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DR. BICKEL: That's correct.

MR. EBERSOLE: To get statistics on performance of valves and breakers, et cetera.

DR. BICKEL: That's correct.

MR. EBERSOLE: What did you do to compensate for the fact that your operating data was generally obtained under mild nonchallenging circumstances, totally different from circumstances in which real life is being played out, such as a valve intercepting a full mass flow case or a pipe failure or a breaker that is interrupting a full short-circuit?

DR. BICKEL: Well, the area of -- The question about the valves I'd have to look and see what was done.

In the case of the breakers, the main function we were looking at in terms of PRA was its ability to start, start large pieces of equipment and protectively open in some areas where we needed to trip things, like to strip buses following a loss of normal power.

MR. EBERSOLE: Where it was piloted by a signal, not by an overcurrent.

DR. BICKEL: That is correct.

MR. EBERSOLE: Does that old plant have the old HPCI RCIC combination? It does, doesn't it?

DR. BICKEL: No, it does not.

MR. EBERSOLE: Well, what does it have for

emergency feedwater?

DR. BICKEL: Pardon?

MR. EBERSOLE: What does it have for emergency feedwater?

DR. BICKEL: Millstone Unit 1 utilizes essentially a category one safety grade feedwater system. It is called a feedwater coolant injection system.

MR. EBERSOLE: Is it turbine-driven?

DR. BICKEL: No -- Excuse me. It's -- The main feed system just stays on line. It's electric-driven.

MR. EBERSOLE: And it's ramped down according to need?

DR. BICKEL: That's correct. Basically it's a normally running feedwater system that has an ECCS mode of operation.

MR. EBERSOLE: Does it have auxiliary power from a diesel source?

DR. BICKEL: Yes, it has a power source coming from either the offsite switchyard or an emergency gas turbine, which is the -- There are two, you know, category one power sources. One is the diesel and the other is a gas turbine.

MR. EBERSOLE: So it's got a diesel and a gas turbine?

DR. BICKEL: That's correct.

MR. EBERSOLE: I guess turbine takes a while to get going, doesn't it?

DR. BICKEL: That is correct, about 48 seconds.

MR. EBERSOLE: Oh, is that all?

DR. BICKEL: Yes.

MR. EBERSOLE: Gee, we should have them all over.

(Laughter.)

DR. BICKEL: There are others that have other opinions.

I would like to maybe get into some quick comparisons of the results.

DR. DAVIS: Excuse me, John. Before you do that, I just can't let that diesel generator failure slide go by.

DR. BICKEL: I knew I had better speed it up.

Yes?

DR. DAVIS: I don't want to get into a debate about that, but the curve for Millstone 1 is significantly better than data that has been compiled recently from other sources. And I just want to point out that there still is a difference of opinion about what diesel generator reliability really is.

And from that curve can you tell me what you used as your median value -- I'm sorry, mean value?

DR. BICKEL: I was going to say we used mean

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values.

The mean values that were used...

DR. DAVIS: We can take care of it later.

DR. BICKEL: The failure to start was used about six times ten to the minus three per demand. That is consistent with the experience collected at the time, which was three failures in 652 actual bona fide demands.

DR. DAVIS: Thank you.

DR. BICKEL: Yes.

I have shown up here for some attempted comparison -- I would point out there are a lot of differences in why one would not expect the numbers to be similar.

I have shown up here the median value out of the Peach Bottom, the IREP result and the Millstone 1 PSS result. Qualitatively the differences in the numbers are due to the different scopes involved in the various studies; in other words, namely the types of initiators which were considered for the issue of initiator completeness.

There were different success criteria used among the three studies, and there were different plant reliability assumptions about the equipment and systems. Additionally there were significantly different recovery assumptions. That's why you get kind of a spread among what would be thought to be, you know, roughly comparable systems

of maybe similar age.

(Slide.)

The breakdown of the --

DR. OKRENT: Excuse me.

DR. BICKEL: Yes.

DR. OKRENT: Before you leave that viewgraph, I just can't recall.

(Slide.)

DR. OKRENT: Did the IREP really do a probability distribution and predict the median, or did they do point estimates?

DR. BICKEL: They utilized a point estimate which essentially was, I believe, a propagation of median values. I really can't comment on how it was --

DR. OKRENT: All right.

DR. BICKEL: Maybe some of the other people back there could.

Paul?

MR. AMICO: Paul Amico. I was principal investigator on the Millstone IREP study.

We just used the, Dr. Okrent, we used the median values of basically the WASH-1400 data base as revised for IREP. And that just used a point estimate median value and didn't do any statistical propagation whatsoever. We just used those, and they are multiplied together and you get --

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And that's what we do.

DR. OKRENT: But that doesn't necessarily give you a median result.

DR. BICKEL: That's correct.

DR. OKRENT: Okay.

DR. BICKEL: We all are aware of that. That's correct.

The breakdown on this slide shows the various contributing pieces that go into the overall predicted core melt frequency by the various initiators. I have shown here the loss of normal power contributes about 30 percent, of which about one-third of that, or ten percent, involves station blackout events.

DR. REMICK: We need the next slide.

DR. BICKEL: Excuse me. I'm out of sequence here, folks.

(Slide.)

DR. BICKEL: At the bottom of the page you see three initiators, namely the loss of service water, loss of RBCCW and turbine building secondary closed cooling water, which we treated uniquely as initiating events versus like, say, lumping them into other categories as was done, I think, in the IREP. That gives you some shifting of results.

These were -- I guess the word is more broken

down a little bit finer than was done in IREP. The main reason there again is driven by our need to have a fairly fine separated-out breakdown so that when we do a -- say, if we were looking at a design change on the reactor building closed cooling water system, the PRA model can show in very close detail exactly what the implications of that are and the chances of damaging the core.

MR. EBERSOLE: Before you leave that, why is it that the number was larger when you had the main condenser?

DR. BICKEL: What is shown on that is the following:

With the main condenser you are looking at an initiating event in which the secondary part of the plant is working but on which you subsequently lose the feedwater system post-trip. Without the main condenser that is classically an isolation event. The MSIV slams closed while you are operating.

MR. EBERSOLE: The way it looks to me from here is if you have the main condenser it is a disadvantage; the contribution is larger.

DR. BICKEL: Yes. That would not be the exact inference we would make from that. It is related to the -- I believe it is really more related to the frequency, the fact that we expect more transients to occur, challenges to the plant to occur in which the condenser is on line. The

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frequency of those events is much higher.

MR. EBERSOLE: So it is a summation of events when it stays on line.

DR. BICKEL: Yes.

MR. EBERSOLE: Okay. I can understand that.

DR. REMICK: A question before you go on to the next slide.

On the previous slide -- You don't have to flash back -- you had the mean value of coremelt for the probabilistic study. Did you happen to figure it as a median rather than a mean?

DR. BICKEL: We did not at this point for a number of reasons.

I guess the key reason why we did not pursue all the exotic uncertainty analyses, the key reason is that the dominant outliers that came from this study were reflective of what we felt to be bona fide issues that we thought needed some major changing. In other words, they stuck out because of the issues of adequacy of systems and designs.

They were not random failure type issues. They were things that kind of -- They were uncovered in the course of doing the PRA. Doing an uncertainty analysis at this point had been a -- we were taking a lot of time. We were already convinced we had to do something about the implications of the study. We would be doing this at a

later point when we get like some of these things resolved and more modeling added to the overall picture.

This is the first building block. It is the base of a big model that would be slowly developed over the years.

(Slide.)

DR. OKRENT: So what you call mean is a point estimate?

DR. BICKEL: It is a true mean because the initiating events, the unavailability data and all that, does have both means and variances. We have propagated them as mean values using, you know, Monte Carlo codes. At a later date we will go back and do the overall stuff to ç t a median value.

DR. OKRENT: All right.

DR. BICKEL: In terms of LOCA type initiating events we added one additional category to what was covered in the IREP study. We added to that a case of a very, very small small LOCA type event.

This one was assessed to have a much higher frequency but could be mitigated using normal plant systems. In other words, it is a verv, very small leak. You could keep the main condenser on line. You would not be getting into a scenario with a leak that small at full power that would cause the MSIVs to go closed.

MR. EBERSOLE: Would that include leaks in impulse lines?

DR. BICKEL: Excuse me?

MR. EBERSOLE: Would that include leaks in impulse or static lines that drive instrumentation systems in which the response equipment is disabled when the leak occurs?

DR. BICKEL: Are you talking about like in the level sensing?

MR. EBERSOLE: Yes, the one-inch lines that drive the level sensors.

DR. BICKEL: Yes. We evaluated breaks in that area I think in the study.

My recollection was if you have an event this small it was our perception -- and based on discussions with the operators -- that the other symptoms would be driving the operator to manually initiate safeguards. And that is in effect what is modeled for that type of a scenario.

MR. EBERSOLE: On a level sensing device it could go either way. It can tell you when it is high water or low water.

DR. BICKEL: That's correct. It would be obvious if the level indicator pegged high or low that he was getting something fishy.

MR. EBERSOLE: Does he have redundancy after such

a failure to drive his response operations?

DR. BICKEL: Yes. He has got eight indicators in the control --

MR. EBERSOLE: I didn't mean indicators. I mean how many tubes has he got?

DR. BICKEL: Wayne, are you going to answer that question?

MR. ROMBERG: This is Wayne Romberg, station superintendent.

There is multiple indication in a BWR.

MR. EBERSOLE: I don't mean an indication.

MR. ROMBERG: In the narrow range you've got --

MR. EBERSOLE: I'm talking about the primary hydraulic system --

MR. ROMBERG: Primary sensors, that's what I'm talking about.

MR. EBERSOLE: Oh.

MR. ROMBERG: You've got reference legs on both side, both hot and cold. And a failure of a single line would leave you with at least two or three separate and redundant, all the way back to the vessel, or all the way back to the reference sensors.

MR. EBERSOLE: Great. Okay. No common manifolding.

MR. ROMBERG: No risk to certain instruments

that are redundant in the control room. Those are indicators, though. But there is redundancy in the site of the drywell on the actual reference legs themselves, so that a single reference leg failure would not -- you would still have redundancy back to the control room.

And in fact we train for that. That's part of the scenario.

MR. EBERSOLE: Great. Thank you.

(Slide.)

DR. BICKEL: We additionally added on some interfacing system type LOCA events and issues to the study, again to widen the scope of what we looked at and again to leave us in the position where we could use the model to address a broader range of issues.

Again these are beyond the scope of the considerations of IREP. They included items such as an isolation condenser tube rupture in which you have a total failure to isolate. We looked at LOCAs in the reactor water cleanup system and interfacing system; LOCAs involving the LPCI and core spray systems.

MR. EBERSOLE: Let me ask a question about the feedwater system again.

DR. BICKEL: Yes, sir.

MR. EBERSOLE: Is it totally electric?

DR. BICKEL: Yes.

MR. EBERSOLE: So a total AC power failure and you dry out?

DR. BICKEL. No. We additionally have, in addition to the quickie system, we have what could be amounted to a passive decay heat removal system that's an isolation condenser.

MR. EBERSOLE: Oh, that's right. It's a return condensing system.

DR. BICKEL: That's correct.

MR. EBERSOLE: Oh, yes.

DR. BICKEL: You open one valve with a battery in there and you are in Fat City.

MR. MICHELSON: Excuse me. The reactor water cleanup movement that you are referring to, apparently it has a contribution of 100dth of one percent core melt, is that right?

DR. BICKEL: That is correct.

MR. MICHELSON: And the model was apparently for an unisolated reactor water cleanup.

How big a break were you postulating for this?

DR. BICKEL: The model looked at the different ranges of piping. And it was calculated using like a fault tree model. It turns out that the most dominant type of break in the reactor water cleanup system was due to a failure of the pressure regulating value in the cleanup

system. In other words, it would fail wide open. That gives you essentially a flow in there that is going to pressurize all the low pressure piping downstream of that pressure regulator.

Now the most likely thing that would occur --

MR. MICHELSON: Well, wait a minute. A LOCA can be -- I assume you are postulating a break in that six-inch line, for instance, weren't you?

DR. BICKEL: This would be after you have failed this valve open. The pressure -- The piping is high pressure piping all the way up into this --

MR. MICHELSON: Well, let me ask the question differently:

Did you postulate a double-ended rupture of the largest line in the reactor water cleanup system as the model for your LOCA?

DR. BICKEL: As the model for the LOCA/

MR. MICHELSON: Yes.

DR. BICKEL: No, we did not.

DR. DAVIS: Don't you have flow limiters on those lines?

DR. BICKEL: Yes.

MR. MICHELSON: But then what size do you use, then? The size of the throat of the flow limiter or what?

DR. BICKEL: The flow limiter -- My understanding

is when you fail the thing full open I think you get something like -- if you attempted to, you would not get significantly more water leaving and going into the cleanup system piping as you would under normal full flow operation.

MR. MICHELSON: And what's normal?

DR. BICKEL: I think it is about 1100.

MR. MICHELSON: So you are talking about a 1200 gallon a minute leak?

DR. BICKEL: That's correct.

MR. MICHELSON: At full temperature and pressure.

DR. BICKEL: That's correct.

MR. MICHELSON: So it is partly the steam and partly the liquid, and goes unisolated according to the slide I was looking at.

DR. BICKEL: That is correct. That's what the scenario is.

MR. MICHELSON: Even though that continues indefinitely there is a very small chance of coremelt occurring, 1/100dth of one percent is the contribution?

DR. BICKEL: That's correct.

DR. DAVIS: Less than that.

MR. MICHELSON: Yes, less than that. That's right. I find that a little difficult to believe, and I

would like to look at the study that shows that you can stand those kind of releases indefinitely into the reactor building without any adverse effect on any equipment, including the flooding effects or the water that goes down -- the hot water that goes down as well as the steam that goes up.

DR. BICKEL: One point that I think should be kept in mind is the fact that there are redundant valves capable of isolating --

MR. MICHELSON: This is unisolated, though. That means the valves are closed.

DR. BICKEL: That means the valves have failed to close.

MR. MICHELSON: So the valves are immaterial, aren't they?

DR. BICKEL: No.

What I am saying is you had an event, the pipe failed, water is rapidly flowing through it. The protective equipment then successively fails to close two redundant valves, one inside the drywell, one outside the drywell. So you are talking about a chain of three or four different failures.

In addition to that, you know -- Well, enough said. I think it is discussed somewhat in the study.

MR. MICHELSON: What you are saying is the

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probability of this event even occurring is so small as to give it an infinitisimal contribution to core melt.

DR. BICKEL: That's correct.

MR. MICHELSON: That doesn't mean that the vent went unisolated like the slide says.

DR. BICKEL: No. I said if you had this type of a LOCA it was not isolated; it would lead to a coremelt.

MR. MICHELSON: Oh, yes, it sure would.

DR. BICKEL: No argument there.

DR. REMICK: In the case of Millstone, what is your reactor water cleanup system, a full pressurization system, or does it have a letdown valve?

DR. BICKEL. It has a letdown. It is like a PWR.

DR. REMICK: And if that regulating valve fails, I guess would this be the type of event that would be included in the ones that Mr. Michelson was just talking about?

DR. BICKEL: It would, and it would require a number of additional failures, the key one being the failure of the spring-loaded mechanical safety, which is a full capacity release that dumps right back to the torus. So you would additionally have to fail the mechanical spring-loaded safety which is located right downstream of that pressure regulating valve.

DR. REMICK: I see.

MR. MICHELSON: Were these valves tested.... This is Unit 1. Were these valves tested in any way to confirm their ability? I guess you have routinely closed them under full flow conditions.

DR. BICKEL: Yes. The isolation valves do get moved around under various phases of operation; you know, isolating and un-isolating: they are moved around.

MR. MICHELSON: But the worst case that they would be functional for would be full flow?

DR. BICKEL: Which is comparable to its normal operation; yes.

MR. MICHELSON: Yes; but you haven't quite told me what the flow restriction is. If it is comparable to full flow, then that's right.

DR. BICKEL: It is.

MR. MICHELSON: It is. Thank you.

DR. REMICK: Excuse me; one more question.

Did you always have that safety valve on the low pressure side? --the regulating valve?

DR. BICKEL: My understanding is yes.

DR. REMICK: That was always there?

MR. ROMBERG: Yes; that valve was in the original design.

DR. REMICK: And it returned to the torus?

MR. ROMBERG: That's right; its return line is full size, it is designed for full flow.

DR. REMICK: Thank you.

DR. BICKEL: I think it is like an 18-inch or something, it is huge.

MR. ROMBERG: I don't remember the exact size. It looks a lot bigger than it has got to be. It looks oversized.

DR. BICKEL: The next slide shows the breakdown of the core melt frequency results in terms of key safety

issues.

(Slide.)

The most predominant one that we did determine in the course of the study was related to the long term decay heat removal systems.

MR. EBERSOLE: What stops that process? What is the critical aspect within that category?

DR. BICKEL: The critical aspect in this is related to the sizing of the RHR heat exchangers. And if you make an assumption of one train of RHR running in a mode where you were using the RHR in an alternate shutdown cooling mode or a bleed-and-feed cooling where you are blowing steam from the vessel down into the torus, you are not pressurizing the containment, you heat up the water pool in about a period of four hours, at which time you begin to lose the NPSH of the LPCI pumps.

MR. EBERSOLE: Well, what about the isolation condenser function?

DR. BICKEL: We are looking at this mode, given that the isolation condenser has already failed.

MR. EBERSOLE: Oh, I see.

DR. BICKEL: In other words, you get into a chain where this is an important feature, where you lose the--

MR. EBERSOLE: I understand. If you lose outside connections you are out in the woods pretty far.

How reliable is it in block?

DR. BICKEL: It is about 1 or 2 percent unavailable per demand based on calculations, and it has never failed in actual operation.

MR. EBERSOLE: 1 or 2 percent.

DR. BICKEL: That is based on calculations, yes.

MR. DAVIS: Is that driven by the valve opening failure probability, or do you recall?

DR. BICKEL: It is; it is driven by MOV-IC-3, which is the single valve that's closed. It is a condensate return valve. When that thing is stroked open, the water begins to flow, and it turns it on. If that fails and the operator is incapable of manually opening it as a backup, that's where you get that type of--

MR. EBERSOLE: Isn't that worth parallel valving?

DR. BICKEL: That is a type of change we are looking at. We have initiated, as a result of this finding of such a high dependency in these area, we have initiated, and have formally committed to the Staff to perform a long term cooling study to essentially go through and look at all the issues that are raised.

The business about the HRH heat exchangers is but one failure in a chain of events. There are many others that lead to the need of that bleed-and-feed cooling capability. We intend to look at what possible fixes could

be made in each of the systems and prioritize them in terms of payback and costs, and essentially come up with a menu of the best fixes to make this thing way the heck down.

MR. EBERSOLE: It seems such a valuable system that not to have another valve is kind of a contradiction.

DR. BICKEL: I agree with you. It will be looked at.

DR. REMICK: That valve is DC operated; is that correct?

DR. BICKEL: That's correct.

DR. REMICK: Is it also AC operated?

DR. BICKEL: It is just DC operated.

Another thing I might point out is, under normal operation you can walk up and open it manually. It is a few minutes walk from the control room. So the backing up of that, should it fail to open automatically, is very credible.

The next issue, which was also sort of a bigger issue, is, of course, the station AC blackout issue. One of the unique things that we found on Millstone-1 is that the blackout issue has a significant contribution not fully based on just emergency generators, we found a very high contribution to the service water coupling with the cooling of the diesel.

What is involved in here is the fact that we

have four service water pumps. Following a loss of normal power only two of them automatically get started, one being powered by the diesel, one being powered by the gas turbine.

Should the gas turbine fail to start, that means you have one service water pump -- excuse me; one service water pump providing cooling to the diesel.

There are other loads on that service water cooling that have to be isolated. That requires closing a normally open valve which is untestable during the normal operation of the plant.

Now, because the plant is now experiencing, because of -- you know -- good operating history, longer and longer fuel cycles, the period between which you can test it and verify that it is operable is beginning to stretch over the old days when, you know, we might have been tripping more frequently and had the ability to do a check on it. That obviously points to the fact that, you know, we ought to again be looking at that: could we put in some redundancy; could we be doing something here to, you know, add the ability to test that and verify that that valve, if you need it to cut off non-vital cooling loads, can actually be closed. That's again something that we are going to be -- that we are looking at as a potential improvement.

MR. EBERSOLE: What provides the cooling water to the isolation condensers?

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DR. BICKEL: The isolation condenser is cooled by the firewater system.

MR. EBERSOLE: Okay.

DR. BICKEL: And one of the other areas we are looking at there is the fact that there are three pumps that provide water. Now, the design of it right now is that the two electric pumps are shed following a loss of power, because they are really more related to fire protection. You are left with a dedicated diesel-driven pump.

DR. REMICK: Isn't water supplied by other than the fire protection normally, though? Aren't there two or three sources of water?

DR. BICKEL: The original design of the plant used a line from a condensate storage tank. However, that was valved out because the condensate storage, of course, has got some residual amount of activity in it, and the isolation condenser is essentially -- it is a shell and two heat exchangers that vents to the outside world. So you have another source. It has been permanently pretty much valved out because of environmental considerations.

DR. REMICK: I see. So that's a change?

DR. BICKEL: That's a change.

DR. REMICK: Didn't you used to have some kind of a-- What do you call it when you have something you put in a pipe line if you have a pipe line blocked off and then you

put a... A spool piece. Didn't you used to have a spool piece you could add to get firewater in, or something, at one time?

MR. ROMBERG: Right now that is valved down. But the piping is all attached. It is a matter of an operator going up to the control room and opening a couple of valves. And we can use that.

Also, as a third method you can use mineralized water from the mineral water header: that's how we normally can fill it.

So there are a number of options available. It's easier than just putting a spool piece in, it's a matter of just opening a valve.

DR. REMICK: I see.

I thought originally you had three methods, of which the firewater was the third alternative. But now that is--

DR. BICKEL: It is the primary alternative.

DR. REMICK: Do you have a radiation monitor on the exhaust from that?

DR. BICKEL: That's correct.

MR. EBERSOLE: Do you put boiling water in the secondary?

MR. ROMBERG: Yes.

MR. EBERSOLE: At what pressure?

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MR. ROMBERG: A little above atmospheric. It is an open tank with an oversized vent, 18 inches, something like that. It is a little above atmospheric.

MR. EBERSOLE: And you are monitoring on the discharge?

MR. ROMBERG: That's correct.

(Slide.)

DR. BICKEL: I would like to discuss the actions taken by NU to date and actions we are currently pursuing based on the implications of this study.

The first one was the review by our staff and our licensing people related to, I guess what I would call the regulatory issues raised by long term decay heat removal.

Essentially what we were looking at was is there something really unique here that we've found? We've managed to determine at this point that the issues raised about long term decay heat removal, particularly in terms of the RHR system, are unique to Millstone-1. And it has to do with the vintage of the plant design. And, additionally, the original design of the plant utilized safety relief valves that discharged directly to the drywell air, and they were subsequently, for personnel safety and other reasons, pumped into the torus.

So what in essence we have is something that is pretty much plant-unique.

(Slide.)

Recognizing that if we are going to pursue some changes or improvements in that are we are going to have to live with that situation as we perceive it for a number of months until we can get to the next point where we can make a big modification, we then began to look at what are the limiting items which affect when you are in this 2-out-of-2 RHR mode for success. We determined that the surveillance procedures for determining the adequate lube oil cooling in the LPCI pumps which are used as your RHR pumps needed to be improved such that we could assure that there was, in fact, good lube oil cooling going on.

So we have essentially modified the procedures in that. The impact of this was we eliminated what was predicted to be a dominant sore spot in that alternate shutdown cooling or bleed-and-feed cooling mode. The net result of that change, because it was so big, was that it reduces the -- there is about a 20 percent reduction in what would be predicted as a core melt frequency.

(Slide.)

Additionally, in the process of the overall work on the long-term decay heat removal we did observe an error that was in the emergency operating procedures, and that was essentially fixed up. It was determined that there was an area in the procedures where the operator could be walked

into cavitating the pumps. That has been addressed by procedure changes.

(Slide.)

In the course of doing the study we identified a design error in the low pressure coolant injection loop selection logic in which the timers to the injection valve were crossed. That has been corrected in this current outage.

(Slide.)

SW-9, which is that valve on the service water system which I mentioned, is essential to be closed to cut off non-vital cooling loads, so the water goes to the diesel. It had an automatic bus transfer breaker. We have modified that so that it is now permanently aligned to the diesel bus, it cannot be connected to the -- it cannot be automatically connected to the gas turbine.

(Slide.)

We additionally found an issue related to if you lost reactor building closed cooling water the drywell air temperature could go up to high temperatures in some regions and lead you to a situation where you locked out your cooling. We determined that this could be addressed by a procedure change, and this has also been implemented.

(Slide.)

The condensate return line on the isolation

condenser has had a new motor operator put on it with a little big slower speed on it, and it provides better flow control by eliminating a limit switch dead band problem that we found. This, again, has been corrected in the most recent outage.

(Slide.)

We have recently initiated a project to resolve the long-term decay heat removal issue via a combination of analysis procedure changes. And in some cases we envision there are going to be some major hardware modifications. They are going to be done based on, you know, achieving our corporate safety goal, and such.

(Slide.)

Additionally, we have initiated several formal technical specification changes which we find are useful in reducing the frequency of reactor scrams due to the performance of high risk surveillance activities. What I mean by a high risk surveillance activity is a procedural test that has a very high chance of causing a reactor scram, particularly vessel isolation events. We have identified a couple of those. We have done safety evaluations. They are being reviewed and evaluated right now.

That concludes my talk.

DR. OKRENT: Any questions for Dr. Bickel?

(No response.)

DR. OKRENT: I guess I don't have any.

MR. GRIMES: If it would be convenient, I would suggest if you would like to take a short five-or-ten-minute break so we could switch the projectors, now would be an appropriate time.

DR. OKRENT: So ordered.

(Recess.)

MR. GRIMES: Mr. Okrent, Bahman Atefi will present the Integrated Safety Assessment Program for Millstone 1.

DR. OKRENT: Before we begin, I was just trying to figure out some procedural things. For example, when you get a report from SAI reviewing something like the Millstone probabilistic safety study, do we get a copy as soon as you get it, or do we get a copy as soon as you send it to Northeast Utilities or if not, when do we get a copy and how do you decide when?

MR. GRIMES: The distribution for contractor reports goes something like this:

Normally the Staff-- The contractor provides the Staff with a draft report to review before it is released probably, in which case the ACRS would not get a copy. The cognizant technical staff who is responsible for the contract would.

They would review it and identify any corrections, changes or augmentations which should be made. And then those comments are incorporated into a final technical evaluation report that the ACRS would receive on normal distribution.

In this case, because of the licensee's application of the probabilistic safety study, we sent them a copy of the draft report as well, to identify the errors

in fact, or any observations that they had relative to what they intended with the probabilistic safety study, since we were reviewing just a summary and not the whole raft of analyses that were performed.

They subsequently sent us a formal response, and you should have received both of those on normal distribution.

DR. OKRENT: We should have received both of which?

MR. GRIMES: The draft report as we forwarded it to the licensee, and the licensee's response.

DR. OKRENT: Okay. We may have received it. I didn't get it, but I can't swear to what came to the ACRS. I only got hold of these very recently.

MR. GRIMES: Dr. Okrent, we will go back and assemble all of the various pieces of documentation. I have been told that we provided at least one report to Mr. Savio, but we are not certain whether or not you have gotten everything that we have traded with Northeast Utilities, or everything that has been generated. We will try and assemble all of that and see that it is provided to the ACRS.

When the final report is published we would expect there to be an appendix and the integrated assessment for ISAP. You would at least receive it then, but we would

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hope you would get the SAI final report as it is submitted to us in the normal distribution. I will try and make sure that that happens.

DR. OKRENT: Well, I hope we got a copy of whatever it was that was sent to Northeast Utilities at the same time you sent it to them. Why don't you check to see whether we were on that distribution list? Okay?

MR. GRIMES: We will do that.

DR. OKRENT: Because if we weren't, we should be. Okay.

MR. ATEFI: My name is Bahman Atefi.

(Slide.)

I am going to talk about the review of the risk-based evaluation of ISAP issues for Millstone Unit 1.

(Slide.)

What I am planning to over is to go over the overall process and where does the plant probabilistic safety analysis fit in the whole risk-based evaluation of ISAP program, and then go over the results of the review of Millstone Unit 1 probabilistic safety study, then look into some insights that we gain into areas of plant vulnerability.

And finally, I have results of the review of the Millstone Unit 1 risk-based topics which I will go over after the utility has had a chance to go over their

analysis.

(Slide.)

This Vugraph shows the whole process where ISAP provides a licensing mechanism for resolving safety issues and where the plant-specific probabilistic safety analysis fi s into the whole picture.

Throughout my talk I am going to go back and forth calling PSA and PRA, and the latest word is probabilistic safety analysis, PSA, but I am sure I am going to make mistakes, and I have to apologize for that, for using PSS, PRA and PSA interchangeably.

Basically the plant-specific probabilistic analysis is used in two areas in ISAP. One, it is used to evaluate those topics which one can analyze using probabilistic analysis, and topics which are included in SEP Phase II, outstanding lessons learned from SEP II, TMI plant items, unresolved engineering safety issues, and issues that were brought up by the utility.

Also a plant-specific PRA and its results are used for looking into areas of plant vulnerability which one would find by looking at significant operating events, and also dominant core melt contributors.

And here what I want to emphasize is that this is one area where this program is different than previous programs such as SEP in the sense that in the sense that

part of the mandate of the program is to look into any area of safety significance beyond the issues which are already identified as a result of NRC requirements and find out areas of safety significance and try to resolve them.

That is, it is much more complete than any previous program in that sense, and will allow NRC to look into any area which looks as if it has any safety significance.

The results of the probabilistic-based analysis of topics and areas of plant vulnerability are then mixed with the results of the deterministic evaluation and the operating experience so that as an integral project NRC can made licensing prioritization and any plant modification, procedural or hardware, as a result of both the deterministic and probabilistic results.

(Slide.)

As Chris Grimes mentioned at the beginning, the review of the utility's probabilistic safety study was done not in the traditional sense of reviewing the PRAs but by reviewing and comparing it to the earlier IREP study which was done on this plant. And the reason for that was partially the limitation of time and resources, and partially it was one of the most efficient ways of doing this, especially since some of the team members who were involved in the review of the utility's probabilistic

analysis were principal investigators in the earlier IREP study, so they were fairly familiar with the plant systems and familiar with the core melt contributors.

It was obviously realized that many things had changed during this period, both in terms of procedures and hardware, so it was expected that there would be differences between the two studies.

Now it is also--

DR. OKRENT: Excuse me.

If Northeast Utilities had not of their own initiative, for example, examined success criteria but used the IREP success criteria, then everything would have looked unchanged.

MR. ATEFI: Not only success-- If nothing had changed, either procedures or success criteria,--

DR. OKRENT: No, no. Really no procedures were changed at the plant. I'm saying by your review process in effect you relied very heavily on how much Northeast Utilities did anything that was different from IREP, without you yourself knowing that IREP was a good basis.

MR. GRIMES: Dr. Okrent, I would like to respond to that question, because Bahman would provide you with a biased answer.

(Laughter.)

Obviously the people who originally did the IREP

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study are going to be very defensive about the quality of the success criteria that they applied in their analysis, and I think simply by having the people who developed success criteria for that study interact with the utility people who decided what they thought were appropriate success criteria, we have been able to focus on those areas where there might be some uncertainty or doubt.

So I am reasonably confident that the success criteria are--

DR. OKRENT: No, you are responding to a different question.

I have seen some other -- what I'll call preliminary plant-specific PSAs, not from the Northeast, where very heavy dependence was made on an IREP or whatever were the right set of initials, RSMAP, or so forth. And if one compared the utility document with the IREP or RSMAP, there would be rather little in the nature of original review. Let me put it that way.

And what I'm saying is here it seems to me the review was very dependent on the degree to which the utility did an independent job of the IREP.

MR. ATEFI: Right.

DR. OKRENT: And if you made the decision to do your review this way, without knowing how independent the utility study was, it could well have faulty decisions.

MR. ATEFI: Right.

Now I think John referred to-- They basically did-- They used a different process and they pretty much started fresh, even though the IREP existed. So your point is valid but I think in this case they did not really rely as much on the results of IREP for doing anything.

Basically the process, even the methodology that was used by Northeast in terms of large event trees and fault trees as opposed to small event trees and large fault trees was substantially different. The event trees, the operating action event trees are substantially different.

DR. OKRENT: I'm aware of this.

MR. ATEFI: So in effect your point is valid but in this case, the study was pretty much started fresh, and is substantially different in its procedures and assumptions. So that we feel the comparison, being almost independent, provides that kind of insight one needs to know if something was done right or wrong.

If they had heavily depended on IREP I guess this point would have been completely valid.

I don't know if the utility wants to make any comments on that.

DR. OKRENT: Proceed. Go ahead.

MR. ATEFI: As I mentioned, this was done by comparing with IREP. I want to make one point very clear,

that the reason it was compared to IREP was essentially for the efficiency of doing the work as opposed to assuming that the IREP study was a standard against which a utility's work has to be reviewed.

So we did not assume that IREP was the standard, correct study to compare this to. It was just another study which we were very familiar with. We compared it. We tried to identify differences. And the major point was we tried to explain the differences to our satisfaction.

If we could understand the reason for changes, whether it was procedural, whether it was hardware changes or assumption changes, and we understood those and could explain them, then we have done that with you. If we could not, then that was an item which was raised as what's wrong here?

The major areas that were covered included initiating events, event tree analysis, component and system reliability analysis, human reliability analysis, and dominant accid^nt sequences.

Also, to mention the level of effort type of thing so one gets an idea, a standard detailed review of a PRA with even running the sequences, it takes about two to three man-years level of effort. We spent essentially two to three man-months because of the familiarity with the plant and the earlier IREP study, so there was a tremendous

savings going through this process.

Now once we got to the area of dominant accident sequences and results, what we tried to do was we tried, for every dominant accident sequence which was identified by the utility study, to find the corresponding dominant core melt accident sequence in the IREP study if we could and if there wasn't, understand why it wasn't identified earlier, or vice versa, and also for the identical sequences, understand the difference, both in terms of probability of the sequence and the progression of the accident, and try to explain that again.

(Slide.)

To give you an overview of the kind of comparison and results we got, this Vugraph shows the contribution to the core melt probability in terms of percentage from various accident initiators and some functions such as decay heat removal, station blackout, and ATWS. And again, following the statement I made about each dominant accident sequence, we tried to understand why the contribution of various initiators and functions are different in the two, -- and they are in many areas -- and tried to explain those.

(Slide.)

To give you a couple of examples of those explanations, as an example the loss of normal power in the

IREP study contributed to 85 percent of the core melt probability. The contribution in the ISAP, the utility's study, was about 30 percent of the core melt. And we looked into that, and there are many competing factors.

But the major reasons for this reduction was that the diesel generator, gas turbine and switch gear breaker failure rates were lower in the utility's study, based on the plant-specific data, and on the basis of updating that they had done. There were some modifications to LNP logic to get rid of some single relay faults, and there were modifications to IC makeup.

So in effect we tried to either satisfactorily explain the differences in terms of major contributors or raise an issue of why it has happened.

(Slide.)

Another area which was mentioned by John -- and I will spend a little more time going over it -- is the loss of the decay heat removal system where in the IREP study, the contribution of the loss of the decay heat removal system to the core melt was about 15 percent or so whereas in the ISAP study it was about 65 percent.

The reason was that the failure probability of the alternate shutdown cooling system had changed dramatically, and this is one topic that I will spend a few more minutes later on on it. And it is an area that the

utility is quite aware of it and they are spending a substantial amount of effort and time in resolving it.

(Slide.)

Now responding to the earlier comment about the report and the review, one of the problems with reviewing a document is that unless you find major holes in the document, you cannot see the amount of effort that goes into making sure that what was done was right. And so this is one of the problems with reviewing, because if the original document is done well, and we feel that the utility did a very good job in terms of developing their probabilistic safety study, the comments are -- some are important and many of them might be minor, so it would not look as massive as doing the job itself.

Our general comments as far as the utility's PRA was concerned was that we thought that, based on our review of it and comparing it to earlier results and explanations of differences between IREP and ISAP, that the utility's probabilistic safety analysis we believe provides a reasonable plant risk perspective.

The main reasons are that they used the latest techniques that could be used for doing a PRA analysis such as operator action event trees and plant-specific data, and the latest techniques for human reliability analysis. And also we were able to satisfactorily explain the

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differences between ISAP and IREP in terms of whatever changes have taken place with respect to contribution to core melt probability.

(Slide.)

Now as I mentioned earlier, one of the interesting parts of the whole ISAP process is looking into areas -- inside areas of plant vulnerability or major contributors to core melt. And our preliminary look at the probabilistic safety study by the utility identified three areas for a further look.

The first area is the subject of the long-term decay heat removal value which is included in sequences that contribute to about 65 percent of the core melt frequency, and that is the alternate shutdown cooling problem which I will talk about a few minutes later on.

There are a couple of observations, and these are areas that we are looking at more carefully without major conclusions yet. And these are that the cognative operator error that John talked about earlier to restore pressure vessel level is included in sequences that contribute to about 20 percent of the core melt frequency.

The third observation was that 60 percent of the dominant accident sequences contain failure of isolation condensor or makeup or the safety relief valves being stuck open. And these two items are areas that we are looking

into more carefully, seeing what the implications are and what can be done about it.

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(Slide.)

Now the alternate shutdown cooling system reliability, as I mentioned, is an area which the utility is quite aware of and is spending substantial amounts of effort in trying to resolve.

And what we did was that we looked at the system and did some conceptual studies about what kind of reduction in the failure probability of the system conceptually is possible if one does any modifications into the system.

This picture shows the alternate shutdown cooling system which is used when the main condensor, isolation condensor and shutdown cooling system is not available. So it is essentially the last stage of heat removal system when other systems are not available for whatever reasons.

It consists of the primary system taking water from Taurus, using low pressure cooling injection pumps and going through containment heat removal, heat exchanger, and going back to the core. The heat on the secondary side is removed by four emergency service water pumps.

During a LOCA, because of the break, there is a path from the core to the Taurus, so the water is circulated to the core and the heat is removed from the secondary side by the emergency service water pumps.

In case of transients, at least two safety relief valves must be opened to provide a path for this kind of

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recirculation.

Now in the IREP study, when the IREP study was done, the success criteria for this system consisted of one low pressure coolant injection pump in one of the loops operating with one emergency service water taking heat from the secondary side.

Based on more recent calculations done by the utility, they felt that a more restrictive criteria is more appropriate. And to the best of their knowledge the success criteria right now consists of one out of two pumps in both loops recirculating the primary water and all four emergency service water pumps functioning to remove heat from the secondary side, which is a considerably more restrictive success criteria.

(Slide.)

As a result of this change in the success criteria, the unavailability of the system in its present configuration is about .15, it is .148 because of this and this is the major reason that it contributes so dramatically to about 65 percent of the sequences that contribute to core meltdown.

And what we did was that -- again this is an area which is being looked at carefully -- we adjusted some conceptual calculations just at the system level. We said well what if we make the primary side, the low pressure

coolant injection/containment cooling loops redundant, however that is done, by sizing up the pump or heat exchanger, it doesn't matter. That is a decision that has to be made by the utility. But conceptually let's assume the primary system is made redundant. What kind of reduction in unavailability of the system can one get? One can get a factor of about 1.7 or so by itself.

Then let's assume that the secondary side by itself, without doing anything to the primary system, is made redundant by making the emergency service water loop redundant. You get a comparable thing, about 2.2 percent, a factor of 2.2.

Now if you make both the primary system and secondary system redundant, again by sizing up the pumps or heat exchanger or what have you, you can essentially reduce the unavailability of this system by a factor of about 40, which essentially eliminates this large contribution of the system to the core melt probability.

Again this was done just on a conceptual level, without any specific recommendation on how this should be done, which is a decision to be made by the utilities.

(Slide.)

Now we also went over the ISAP topics which were done by the utility using PRA techniques and reviewed those, and I have the results of that in the back of the package.

But I will go over that if necessary after the utility has gone over their presentation.

So I will basically conclude by saying a few things about our experience in being involved in the first ISAP plant:

We feel that the ISAP process provides a unique opportunity for systematically resolving all safety issues. We feel that the plant-specific probabilistic safety analysis is required to get this kind of insight that one needs both in resolving the ISAP -- some of the ISAP topics and also for looking into the areas of plant vulnerability. The process allows for looking into areas of plant vulnerability, which is a unique feature of this program and makes this program much more complete.

Also the risk-based topics, those topics that can be analyzed using PRA techniques, are resolved using a plant-specific PRA.

And the final conclusion is that all safety issues can be prioritized using the results of both the deterministic and probabilistic in a more comprehensive, collective manner so that this process would allow NRC to focus on the most important issues to be resolved first and then do the resolution of issues in a more systematic manner.

That is basically all I have to say.

DR. OKRENT: Any questions?

(No response.)

DR. OKRENT: Okay. Thank you.

MR. ATEFI: Thank you.

MR. GRIMES: Dr. Okrent, the next item on the agenda is the Staff/utility comparison of insights from the Millstone 1/Millstone 3 PSS. Mr. Glen Kelly of my staff is going to have a brief discussion on our observations relative to those differences. And the licensee can make whatever observation he would like.

DR. OKRENT: Okay.

MR. KELLY: My name is Glen Kelly. I am the Integrated Assessment Project Director.

(Slide.)

Mr. Atefi has already discussed a considerable amount about the differences between IREP and ISAP and the conclusions between the two risk assessments, so I won't dwell significantly on that.

It has been asked a number of times of me whether the PRA done for Millstone 3 and the PRA done for Millstone 1 are comparable, or what we can learn by looking at the two PRA's.

I would expect that there is really very little direct comparison that can be made between the two risk assessments in the sense of trying to gain information that

would run across between the two plants.

The only real common things about the two plants are the common site for the plants and also the fact that they share a common switchyard. Neither one of the PRA's were done from the point of view of assuming that failure of one would somehow affect the failure of the other.

I would also point out that it is known that Unit 1 is a BWR and Unit 3 is a four-loop Westinghouse pressurized water reactor. Unit 1 has operated about 15 years and Unit 3 has just received its low-power license, so we have considerable operating experience for Unit 1 whereas almost none for Unit 3.

The Millstone 1 PRA only covered internally-initiated events and we are still awaiting a fire analysis, though I understand that perhaps in the future we may -- Northeast may plan on providing some additional external-event analyses for Millstone 1.

We have received a detailed summary for Millstone 1 PSS rather than the complete PRA, but I will say we did receive a very detailed summary, as you can see from the four volumes that we've got.

The Millstone 3 PRA covered both internal and external events and it had a detailed containment and consequence analysis which was not really provided for Millstone 1 and the Staff received a complete package for

Millstone 3. We had looked at whether there was any way to effectively compare Millstone 1 to other PRA's, either recent or some of the older ones, and we would expect that it's very difficult to compare numbers between PRA's because of the different ways that they are performed and the different scopes of the PRA's.

So from our point of view I think what we can say is that we have to look at each PRA at least at this point on an individual basis.

That's all I really have to say about a comparison between the Millstone 1 PRA and other PRA's.

MR. GRIMES: Dr. Okrent, I would like to add that as a result of the SEP reviews for external events like seismic, winds, external flooding and other issues like that, we are reasonably comfortable that when an external event PRA is gone that we are not going to find -- I am trying to pick a word that is something like "significant" but qualified probably -- a contributor to the risk from those things because we have essentially brought the plant up to some equivalent to the current criteria for those kinds of events.

It is conceivable that a detailed external PRA might identify some weaknesses, but we don't think that they are going to substantially increase the risk to the plant so that we are compelled to do anything in the immediate

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future.

DR. OKRENT: Okay.

Any questions?

(No response.)

DR. OKRENT: Any comments Northeast wants to make on this?

DR. BICKEL: No.

DR. OKRENT: Okay.

The Staff is up again.

MR. GRIMES: We would like to note that we have got a list in Mr. Atefi's handout that identifies a comparison of their calculations on individual issues versus the utilities' risk perspectives for those issues. And if you would like, we can go through that individually. But I think it would be easier if you just tell us which ones you would like to discuss and we can go into them individually.

DR. OKRENT: I think I will leave it up to the Subcommittee to see if they have any specific areas in which they wish to raise questions, and that includes Mr. Davis.

MR. GRIMES: While they are looking through their list, I would like to remind the Subcommittee that we are here today primarily to provide a status report and about all we can tell you about the individual issues at this point in time is to explain any differences we might see

in numbers. But we had planned on coming back to the ACRS when we finish ISAP to talk about these issues in greater detail and also hopefully tell you about how they are going to be resolved or if they need to be resolved. And so there will be another opportunity to discuss these issues.

As you know, tomorrow we are scheduled for the full Committee meeting to talk about the licensing version. We had hoped to provide enough information to the Subcommittee today so that we could have some sense of whether or not you would agree that it is appropriate at this time to go forward with that kind of licensing activity, knowing that ISAP is going on.

DR. SIESS: Do you see any relation between the licensing conversion and ISAP?

MR. GRIMES: No, sir. I have five more license conversions, at least the NRC Staff has five more license conversions going on. We don't see ISAP as a prerequisite to concluding on the license conversions. As a matter of fact, the Commission has already decided that the SEP review by itself would be sufficient.

DR. SIESS: And the licensing conversion is really no basis for coercion, is it?

MR. GRIMES: No.

DR. SIESS: As long as they apply, you can't shut them down. You either give them a license or let them go,

right?

MR. GRIMES: That's correct. None of the utilities have found it particularly troublesome that they still have provisional operating licenses.

DR. SIESS: I think I recall one conclusion at Ginna, which is the other one; we didn't really think that the conversion would make any difference on the progress toward the resolution of the SEP issues. I didn't see any connection. I just wondered.

DR. REMICK: I would hate to see the NRC miss an opportunity to further delay conversion of licenses.

(Laughter.)

DR. OKRENT: Mr. Mark, did you have a question?

DR. MARK: It is not really a question, it was a -- well, maybe it is. In reading Enclosure A or something to some handout I was a little bit unprepared to understand one of the things which I think you fellows laid on about hydrogen control. Here we have got a MARK I containment which is mandated to be inert, by which I believe one means that the oxygen concentration is kept below 4 percent.

MR. GRIMES: That's correct.

DR. MARK: And then you hold them up -- or it seems as if you meant to hold them up because they didn't have a redundant hydrogen abundance instrument.

Now if the oxygen is below 4 percent, you don't

give a hoot what the hydrogen is. But because it isn't redundant, you say this is a problem to you.

Why is it a problem to you? Why should it be a problem for them? Why should it even come to mind?

MR. GRIMES: I would just like to observe that I believe that is one of the issues that we are hopefully going to address in ISAP and based on my previous responsibilities I can tell you that depending on what assumptions you want to make about metal-water reaction and radiolysis that inerting buys you time. And if you want to assume --

DR. MARK: Inerting buys you inertion.

MR. GRIMES: Radiolysis will still create holes of hydrogen that are going to accumulate in --

DR. MARK: I will take all the hydrogen you have got.

DR. SIESS: It will also produce oxygen.

DR. MARK: If I only have 4 percent oxygen then I don't care how much hydrogen you have got.

DR. SIESS: Radiolysis produces both.

DR. MARK: I have a 4 percent tool -- I mean a tool for measuring oxygen content, I don't need one to measure hydrogen and I certainly don't need it to be redundant.

MR. GRIMES: We may eventually agree. I observed

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that an action plan reduces --

DR. MARK: I understand that some plan might require redundancy, but I wonder what measure of good sense requires it. Are you going by some written words or are you going by some thought process whereby you decide if I have ensured the oxygen content then I don't need to redundify the hydrogen content measurement?

MR. GRIMES: That is correct, because in a number of cases we have approved the use of either hydrogen or oxygen. But the Staff as a matter of policy, or historic precedent, I don't know which, tends to require redundant instrumentation for safety systems. And I can assure you that I can get a reasonably half-competent chemist in here to show you how long it takes to get to a combustible mixture of hydrogen if you don't do something about it.

DR. MARK: I am in favor of keeping this in sight and in mind, and I was just struck by the statement in here somewhere that that is one of the open issues that you still have going.

DR. SIESS: Is there still an issue of recombiners for boilers?

DR. BICKEL: Dr. Mark, John Bickel on behalf of Northeast Utilities.

A number of years ago we submitted on behalf of ourselves and the BWR Owners' Group analyses of the

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post-accident hydrogen generation, which formed the new basis of our hydrogen control at Millstone Unit 1.

The key thing that we analyzed is the fact that in a post-accident environment a range typical of DBA and in some cases quite a bit beyond it, the generation of a lot of hydrogen in the containment has the effect of suppressing radiolytic decomposition. Essentially you reach a self-limiting process which is analogous to what you use in a PWR. You are going to be maintaining some fractional atmospheres of hydrogen overpressure in all the water that is being circulated and it suppresses it.

This is the basis of the current Unit 1 hydrogen control. That's why we do not rely on purge repressurization after a long period of time; in other words, we're saying that we hit an equilibrium point where it says there indefinitely. That is the basis of what the plant is properly designed for.

DR. SIESS: You mean you're relying on natural processes rather than engineered safety features?

(Laughter.)

DR. BICKEL: They have lower failure rates.

(Laughter.)

DR. SIESS: Chris, am I right that there has been some discussion of recombiners for MARK I, MARK II's, for accident conditions?

MR. GRIMES: The only thing I can recall, Dr. Siess, is that one of the TMI accident plan requirements was that every plant in the country gets a connector for a recombiner. You don't have to have a recombiner but you have got to have a connection. And I believe the BWR Owners' Group has been fighting that issue for some time.

DR. SIESS: I remember the fight. I couldn't remember the requirement.

DR. OKRENT: Any other questions?

MR. DAVIS: I have just a couple, Mr. Chairman. The first one may not be too significant but I just can't let it go by, and this is back to the issue of median versus means:

In the October 21 letter that we were supplied from NUSCO to the Commission there is a statement made that Northeast Utilities -- to the effect that Northeast Utilities finds the SAI approach taken in converting the IREP frequencies of core melt from median to mean values to be arbitrary, incorrect and without statistical basis.

The approach that was used was to take essentially a factor of two, as I recall.

The letter goes on to say that Northeast Utilities does not know of any method of converting these two values and that only a qualitative comparison is technically feasible.

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But we were also supplied with a July 10 letter from Northeast Utilities in which in fact they made the same conversion.

(Laughter.)

DR. SIESS: What was the date on the first one you read?

MR. DAVIS: October 21.

In the July memo, Northeast says that typically mean to medians are on the order of two to three times and therefore the IREP values are essentially equivalent to what the PSS says.

It seems to me that these are obvious inconsistencies here. I'm wondering if you hired a new statistician or what.

(Laughter.)

DR. BICKEL: One was before vacations, the other one was afterward.

John Bickel on behalf of Northeast Utilities.

The comment we were raising was that when we published a mean value in July -- last summer the only basis of any comparison were some median values from both WASH 1400 and the IREP. And the immediate question anybody getting it says is well are they comparable. And we say Well gee folks no, because one is a median value and you get that out of doing like a Monte Carlo calculation and ours is

a mean value, which was based on propagating sums and products of other mean values.

What we were attempting to state there -- and this is where I think the confusion is -- is that the numbers are probably -- if we had done a conversion to a median might be in the range.

I think what we were commenting on in the earlier draft of the contractors' review of our report was the fact that they had taken all of the sequences from the IREP study and, because there wasn't an availability analysis and they were trying to do the same thing that we also couldn't do, they essentially applied factors of two or three to all of the individual median point numbers and then tried to add them and sum them up.

You might say that it is reasonable to do it with one number and say that converting a median to a mean is roughly two or three. If you take 20 or 30 dominant IREP sequences, apply two's and three's as multipliers to all of those and then add them up, I think our comment was more of one of degree: how much you are doing it versus is it a good thing to do.

I think in general there is no disagreement between us and the contractors, that if you had the best of all worlds you would do the full-blown Monte Carlo calculation and get the actual means and you would have a

real good basis. Regrettably neither of us did that. And I acknowledge the fact that there is an appearance of a white man speaks with forked tongue.

(Laughter.)

MR. DAVIS: Well my own perception is that means should be the way to do it, if you can achieve a mean.

DR. BICKEL: Yes.

MR. DAVIS: Because I have seen cases where use of medians has distorted results significantly and in fact led to erroneous conclusions. But maybe we should drop that.

DR. MARK: Is it harder to ascertain a mean than a median?

DR. BICKEL: Just the opposite.

DR. MARK: Do you need more data to do one than the other or what?

DR. BICKEL: We did it in terms of mean. It is our belief that it is mathematically simpler to do. And in our simple minds, maybe simpler to understand.

MR. DAVIS: I would agree with that.

DR. MARK: I have another question which is really just, Mr. Chairman, not terribly important. I have a document here which I really read quite a bit of. It refers to some responses by the Applicant in a letter with a date I can't quite turn up here, but I think it was maybe January

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1984. This is not a terribly old document but it also refers to some things which transpired in July of 1985.

Anyway on several occasions I find that the Staff is reviewing -- or you know, assessing or doing something or other with this stuff which came in by now almost two years ago, more than a year and a half ago.

Does that mean that there is some man somewhere in Bethesda who has been working night and day for a whole year and a half on some particular item which the Applicants submitted and he hasn't finished his work yet? Or does it mean that nobody is doing nothing about it and it just hasn't been gotten around to?

MR. GRIMES: I believe the latter characterization is probably closer.

(Laughter.)

MR. GRIMES: We have a number of issues where particularly, like in SEP where we came up with a laundry list of things to do that was separate from the TMI action plan, a laundry list of things to do which was separate from the outstanding or pending licensing requirements list of things to do. And the Licensee sends us the results of his evaluation and says now we have identified three other things that we need to look at. And so we try and split them up and give them to the reviewers to review.

And they get lost somewhere and I get a list of

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here is something that is now two years old, somebody hasn't finished his work. So I have to make a few phone calls and eventually we get around to tying off all those little loose ends.

The phrase that we typically use in a letter is it is under Staff review, means we found it, we are going to fix it and we will let you know.

(Laughter.)

DR. MARK: Thank you.

DR. OKRENT: Mr. Davis.

MR. DAVIS: I found a couple of things that were of considerable interest to me -- and I think maybe this isn't the time to discuss them, but they need some attention I believe.

One of them was a dispute between the Applicant and the NRC about the advisability and value of cross-connecting the safety buses. That issue keeps coming up. And some of us have definite opinions about it and I don't know whether this is the time to discuss it any further or just leave it as a dispute that needs to be resolved.

The other one that I found interesting was a disagreement over whether to change the ADS actuation logic. That keeps coming up. And some of us had felt that a logic change would be risk beneficial, but the Applicant

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has disputed that.

And then the final thing that I wanted to ask: Are there any insights from either of these studies that would give us a better feel for the benefits of containment venting, either prior to or after core melt? Maybe there isn't because of the lack of a significant containment event matrix.

That's all I have, Mr. Chairman.

DR. OKRENT: Well, this question of cross-connection is on other reactors as well, I believe, and not necessarily in the ISAP program. I don't know whether-- Is the change in logic you referred to a generic one, or is this something specific to Millstone?

MR. DAVIS: No, it is a generic consideration, and that is whether to eliminate the logic that requires high drywell pressure and temperature as a prerequisite for ADS activation logic. It is too bad Mr. Ebersole isn't here to talk about this, this cross-connection of the safety buses.

DR. SIESS: That cross-connection of the safety buses goes back as long as I can remember, and I'm sure Dave can remember farther back than that. But it was always in a non-quantitative sense.

What does quantitative PRA tell us about the cross-connection?

DR. BICKEL: We credited the cross-connection of buses under certain scenarios, and that is consistent with our -- we have actual procedures on how that would be done, and is done on some of our other plants.

The impact, my recollection was, is if you do not credit it in some areas the core melt frequency will increase by about a factor of about 20 percent. What it buys you is an uncertain system for which you need multiple

trains to mitigate certain events. If you are out of the ballgame because you don't have power from one of your power sources, and you have something like a big gas turbine that can supply all the trains, if you cross-connect you can use it.

DR. SIESS: Okay. But the argument has always been that if you have a cross-connection you have a risk to have a failure in both systems because of a common fault.

DR. BICKEL: That is correct. What we are looking at is the use of cross-connections, and there are two areas where it is looked at. In one area you would be looking at following the accident, having a procedure on how one would go in, rack out breakers, install jumper cables or utilize breakers that would not be normally closed, to effect a power transfer from one area to another. That, of course, would not be vulnerable to the type of electrical faulting that I think, you know, maybe Dr. Davis was talking about.

DR. SIESS: But it involves questions of time and human error.

DR. BICKEL: That's correct. We would be looking at this mainly in the area of long-term decay heat removal. It has a benefit.

In the area of spurious problems like, say, a fault on a bus, transferring the fault to a redundant train,

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yes, that is looked at in the PRA. As a matter of fact, that item was evaluated probabilistically as an ISAP topic which I believe there is documentation on.

We looked at the key areas where they have automatic transfer devices. I believe the ones looked at were in the LPCI area, some of the injection valves, you know, have auto bus transfer capability. And that area doesn't dominate mainly because of the fact that the LPCI is fully backed up by the core spray system, which does not have such breakers. But it is a benefit in the area of a LPCI.

There also are some auto bus transfers in some of the vital AC. Again, you can look at the thing and say yes, you can rip it out, and you are going to leave yourself surprisingly worse off than you were before. You know, that is what our analysis at the present indicated.

DR. SIESS: So your analysis suggests that the risk of a common fault disabling both systems is counterbalanced by the benefit you get from having the connection?

DR. BICKEL: That is correct.

DR. OKRENT: I don't think--

DR. SIESS: I just wonder how sensitive that is to what assumptions you make and what numbers you use.

DR. OKRENT: I don't think we will try to study

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the problem here tonight. But I will ask Dr. Savio if he can arrange to start us looking at both of the two questions, the connection and the ADS logic, by maybe having one of our fellows review the matter and write sort of a status report. He can contact the Staff.

The cross-connect question arises in a variety of systems. It's not only the discussion we heard here, and the answer may not be unique. And I suspect the approaches vary from country to country. And it may depend also on the redundancy that's available, and so forth and so on.

But, anyway, we ought to begin a look at it; okay?

DR. SAVIO: Sure.

DR. OKRENT: Let's see: on containment I don't think we are ready to talk about. I'm still waiting to see the definitive white paper on Mark I containment behavior for serious accidents. I don't know when we will get that.

MR. GRIMES: We would not intend to address this unless some kind of recommendation came forward from the staff in time for us to address it. And it doesn't look like we will have time.

DR. OKRENT: Well, we will have to bother another part of the Staff.

DR. SIESS: You had better wait until you find it, where they are.

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(Laughter.)

DR. OKRENT: In any event, are there any other questions pertaining to today's subject, or comments?

(No response.)

DR. OKRENT: Well, if not, I will thank all the participants, and I will thank them also for letting us finish ahead of Dr. Savio's agenda. That doesn't happen to often. This is a rare event.

(Whereupon, at 5:05 p.m., the Subcommittee was adjourned.)

CERTIFICATE OF OFFICIAL REPORTER

This is to certify that the attached proceedings before the UNITED STATES NUCLEAR REGULATORY COMMISSION in the matter of:

NAME OF PROCEEDING: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

COMBINED SUBCOMMITTEES ON
SAFETY PHILOSOPHY, TECHNOLOGY AND CRITERIA
AND RELIABILITY AND PROBABILISTIC
ASSESSMENT

DOCKET NO.:

PLACE: WASHINGTON, D. C.

DATE: WEDNESDAY, DECEMBER 4, 1985

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission.

(sigt) W R Bloom

(TYPED)

WILLIAM R. BLOOM

Official Reporter
ACE-FEDERAL REPORTERS, INC.
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(sigt) Madelon P. Bloom

(TYPED)

MADELON P. BLOOM

Official Reporter

ACE-FEDERAL REPORTERS, INC.

Reporter's Affiliation

DISCUSSION WITH ACRS SUBCOMMITTEE

ON

SAFETY GOAL POLICY

VICTOR STELLO, JR., DEDROGR

DECEMBER 4, 1985

TOPIC I

RELATIONSHIP TO BACKFIT RULE/SEVERE ACCIDENT POLICY

I. BACKFIT RULE (10 CFR 50.109) - COMPATIBLE

- o MATRIX SETS FORTH BENEFIT-COST ALGORITHIM FOR STAFF DECISION USE
- o MATERIAL AND RELEVANT FACTORS COMPATIBLE WITH EXISTING GUIDELINES (EXCEPT AOSC.)
- o SUBSTANTIAL INCREASE IN OVERALL PROTECTION
- o ALARA APPROACH WITH COST INCENTIVE TO REDUCE CORE MELT FREQUENCY

TOPIC I (CONTINUED)

RELATIONSHIP TO BACKFIT RULE/SEVERE ACCIDENT POLICY

II. SEVERE ACCIDENT POLICY/OPERATIONAL EXPERIENCES

- o COMPATIBLE WITH POLICY
- o MATRIX ACCOMMODATES FINDINGS SHOULD SEVERE ACCIDENT VULNERABILITIES/OUTLIERS BE REVEALED
- o ESTABLISHES GOALS FOR DESIGN OF FUTURE/NEW STANDARDIZED PLANTS
- o COMPATIBLE WITH INCREASING USE OF RISK MANAGEMENT TECHNIQUES
- o FLEXIBLE TO PROMOTE SAFETY IMPROVEMENT WHERE OPERATIONAL EVENTS REVEAL DRIFT TO HIGHER THAN EXPECTED SEVERE ACCIDENT FREQUENCIES (E.G., NEED FOR IMPROVED AFWS DESIGN)

TOPIC 4

APPORTIONMENT/ALLOCATION OF RISK CONTRIBUTIONS

NOTE MATRIX ESTABLISHES LEVEL OF SAFETY WHERE NO FURTHER SAFETY IMPROVEMENTS NEEDED REGARDLESS OF SPECIFIC CONTRIBUTION OF INDIVIDUAL (OR CLASSES OF) ACCIDENT SCENARIOS

SOME GENERAL RULES OF THUMB - STRIVE TOWARD BALANCED DESIGN

A. INTERNAL EVENTS

- o BALANCED CONTRIBUTIONS OF RISK IMPORTANT SCENARIOS HAVING A SMALL FRACTIONAL CONTRIBUTION TO OVERALL CORE MELT FREQUENCY
- o BALANCED-CONTRIBUTIONS OF SCENARIOS DOMINATING OVERALL CORE MELT FREQUENCY
- o IMPROVE AGAINST SINGLE-SCENARIO/OUTLIER DOMINANCE

TOPIC 4 (CONTINUED)

APPORTIONMENT/ALLOCATION OF RISK CONTRIBUTIONS

- IN THE ABSENCE OF PLANT SPECIFIC PRAs, 10 PERCENT FRACTIONAL CONTRIBUTIONS OF SCENARIOS IS USEFUL AS ROUGH APPROXIMATION TO GUIDE GENERIC ISSUE RESOLUTIONS
- RECOGNIZE CONFIDENCE LEVEL FOR REQUIRED SAFETY DECISIONS MAY DIMINISH AS CORE MELT FREQUENCY APPROACHES 10^{-5}/RY

B. EXTERNAL EVENTS

- SEPARATE TREATMENT MAY BE NEEDED (E.G., EARTHQUAKES)
- LARGE UNCERTAINTIES: APPLICABILITY OF MORTALITY STATISTICS

TOPIC 5

REVISIT OF SAFETY GOAL POLICY FOR POSSIBLE UPDATING

- o TIME FRAME OF ABOUT 5 TO 7 YEARS ENVISIONED AS BEING COMPATIBLE WITH SEVERE ACCIDENT POLICY, ISAP, SOURCE TERM INITIATIVES

- o TIME FRAME OF ABOUT 2-3 YEARS ENVISIONED IF DECISION ON INCORPORATION OF CONTAINMENT PERFORMANCE OBJECTIVE IS MADE

- o OTHERWISE URGENCY OF REVISIT LIKELY DRIVEN BY DECISION NEEDS ON FUTURE DESIGNS NOT NOW FORESEEN

TOPIC 6

SPECIFIC GUIDELINES: FUTURE PLANTS

STAFF'S CURRENT THINKING:

- o GUIDANCE PLANNED IN REVISED SAFETY GOAL POLICY
- o MATRIX PROVIDES GOALS OF CORE MELT FREQUENCY OF $\leq 10^{-5}$/RY AND MEETING THE MORTALITY RISK OBJECTIVES
- o GOALS FOR THE EXTERNAL EVENTS (E.G., EARTHQUAKES) MAY REQUIRE ADDED GUIDANCE (SITE-SPECIFIC)

TOPIC 7

NRC STAFF - ACTION TIME SCALES

- o ACTION TIME SCALES ARE NOT ADDRESSED IN CURRENT SAFETY GOAL POLICY

- o. STEERING GROUP REPORT CONSIDERED SUCH ACTION LEVELS DURING THE TRIAL EVALUATIONS BUT DECIDED THAT SAFETY GOALS SHOULD REMAIN AS ONE OF THE FACTORS TO INDICATE NEED FOR URGENCY IN REGULATORY ACTIONS/BACKFITTING/SHUTDOWNS

- o NO SPECIFIC ADVANTAGES CURRENTLY FORESEEN FOR INCORPORATING SUCH ACTION CRITERIA INTO THE SAFETY GOAL POLICY

- o DECISIONS WOULD INCLUDE CONSIDERATION OF HIGH CORE MELT FREQUENCY, HEALTH RISK IMPLICATIONS, EXISTING REGULATIONS, TIME SCALE FOR POSSIBLE FIXES, ADVERSE IMPACTS OF SHUTDOWN, AND OTHER PLANT SPECIFIC MATTERS THAT MAY GO BEYOND SAFETY GOAL POLICY

INTEGRATED SAFETY GOAL DECISION MATRIX

CORE MELT, HEALTH EFFECTS AND COST-BENEFIT)*

LARGE-SCALE CORE MELT FREQUENCY (PER RY)	HEALTH EFFECTS @0.1%/RY (EARLY/LATENT)	COST BENEFIT ($1,000/P-R + AVERTED ONSITE COST)
$<10^{-5}$	MEET BOTH DON'T MEET ONE	NO FIX FIX ($1,000/P-R)
10^{-4} - 10^{-5}	MEET BOTH DON'T MEET ONE	FIX ($1,000/P-R + 1 → 0% AOSC) FIX ($1,000/P-R +100% AOSC)
10^{-3} - 10^{-4}	MEET BOTH DON'T MEET ONE	FIX ($1,000/P-R + 10 → 1% AOSC) FIX ($1,000/P-R +100% AOSC)
$>10^{-3}$	MEET BOTH DON'T MEET ONE	FIX ($1,000/P-R +100% AOSC) FIX (COST NO LIMIT)

*ALL VALUES ARE TAKEN AS MEAN VALUES

UNITED STATES
NUCLEAR REGULATORY COMMISSION
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
WASHINGTON, D. C. 20555

November 21, 1985

MEMORANDUM FOR: D. Okrent, Subcommittee Chairman
J. Ebersole
H. Lewis
J. Mark
C. Michelson
F. Remick
C. Siess
D. Ward
C. Wylie
P. Davis

FROM: R. Savio, Senior Staff Engineer R. Savio

SUBJECT: INFORMATION FOR DECEMBER 4, 1985 MEETING OF THE SUBCOMMITTEE ON SAFETY PHILOSOPHY, TECHNOLOGY AND CRITERIA AND RELIABILITY AND PROBABILISTIC ASSESSMENT

Discussion with the EDO on the Safety Goal Policy is scheduled for the morning of December 4. The enclosed list of discussion topics will be on the agenda for this morning discussions. This replaces the list sent with my November 21, 1985 status report. If you would like to make any additions or comments I can be reached on 202-634-3268 (office) or 301-757-6276 (home).

Enclosure: As Stated

cc: M. W. Libarkin
G. R. Quittschreiber

'85 NOV 25 P3:50

NOV. 22, 1985

Possible Discussion Topics

1. The current proposal for a Safety Goal Policy sets forth qualitative health and core performance goals as primary goals and at some later date will include containment performance goals. The revised Safety Goal Policy will become part of the NRC's decision-making process. How will this policy be implemented? What will be the relationship between the Backfit Rule, the Severe Accident Policy, and a revised Safety Goal Policy? How should this relationship be expressed in the revised Safety Goal Policy? It would be useful if the EDO could illustrate, via examples, how a Safety Goal Policy could be applied to some safety issues currently under NRC review.

2. The current Safety Goal Policy does not contain any guidance as to the treatment of uncertainty (or the completeness of the PRA methodology) in the decision process or in measuring compliance with a safety goal. What guidance would the NRC Staff give to a utility if the utility were proposing that compliance with a safety goal be demonstrated in lieu of the implementation of some set of backfits? If a single set of criteria cannot be identified what approaches could be used? How could a Safety Goal Policy be used in an ISAP review?

3. The NRC Staff is developing containment performance design objectives. Should a statement be included in the revised Safety Goal Policy stating that this is being done and the time scale on which it will be accomplished? Should some preliminary statement be included stating the Commission's expections as to the outcome of this work?

4. Proposals have been made to limit the risk contribution of individual accident scenarios to some fraction of the total safety goal, with 10% having been frequently suggested. What are the pros and cons for doing this and what are the views of the Senior NRC Staff on this subject? Would the NRC Staff limit the risk contribution of classes of accident scenarios, e.g, external events?

5. What would be the anticipated period of implementation (before additional review and possible revision) of a revised Safety Goal Policy?

6. Specific guidelines for future plants are not included in the current Safety Goal Policy. What should be done in the way of including such guidance in a revised Safety Goal Policy? At the November 6, 1985 Subcommittee meeting, it was suggested that a core melt frequency of less than 10^{-5}/reactor year and meeting the prompt fatality and latent cancer criteria would be an appropriate goal. What is the NRC Staff's current thinking on this?

7. The time-scale on which NRC Staff action should be taken when safety goals are not met is not addressed in the current Safety Goal Policy. Should the Safety Goal Policy be modified to include guidance of this type? NRC Staff proposals have been discussed in the past. Could these proposals be used in a revised Safety Goal Policy? If criteria were not incorporated in a Safety Goal Policy, how would such matters be decided?

8. There is still some controversy as to the inclusion of averted on-site cost within the cost/benefit guidelines. What are the views of the Senior (including Senior Legal) NRC Staff on this subject? It would be useful if the EDO would discuss examples of what modifications could result for the different "fix" categories displayed in the EDO's proposed decision matrix. For example, what frequency of core melt would justify a dedicated decay heat removal system or extensive modifications to a AFWS?

9. The current Safety Goal Policy does not contain a true societial quantitative safety goal. Should there be one or some sort of NRC discussion of this in the Safety Goal Policy statement? During the discussions prior to the issuance of the 1983 Safety Goal Policy some societal goals were suggested but not adopted by the Commission. What were the reasons for this, and would they still exist? Should the current societal risk goal be changed to an individual risk definition of latent cancer risk by integrating the risk over one mile as suggested by Harold Denton and in the ACRS report, NUREG-0739?

10. There has been some debate as to the distances to be used in measuring compliance with the prompt fatality and latent cancer quantitative goals. The 1983 proposal was for 50 miles, with the Steering Group recommending revision to 10 miles. NRR has suggested 1 mile. What distance does EDO believe appropriate?

QUESTION 2. UNCERTAINTIES

- o THERE IS GUIDANCE IN THE IMPLEMENTATION PLAN--INHERENT IN CHOOSING "ACTION" LEVELS

- o COMPLETENESS WILL ALWAYS BE A PROBLEM

- o NUREG-1150 WILL CONTAIN:

  - IDENTIFICATION OF RISK-IMPORTANT DRIVERS

  - REASONABLE RANGES FOR PHENOMENOLOGICAL DRIVERS, TOGETHER WITH "NEUTRAL BETTING ODDS"

  - STATISTICAL SAMPLING AND DISPLAY OF REASONABLE RANGE OF RISK UNCERTAINTIES

  - DEFINITION OF RISK-IMPORTANT DRIVERS

- o WEIGHT GIVEN TO UNCERTAINTIES DEPENDS ON PROBLEM BEING ADDRESSED--IMPOSSIBLE TO DEVELOP COMPLETE GUIDANCE

PERSPECTIVE

ASSUMING NO CORE MELT ACCIDENTS WITH PRESSURE VESSEL MELT THROUGH

	LIKELIHOOD OF ACCIDENT IS NOT GREATER THAN:
BASED ON COMMERCIAL EXPERIENCE (3650 RY)	
50% CONFIDENCE	1.9×10^{-4} PER RY
90% CONFIDENCE	6.3×10^{-4} PER RY
BASED ON U.S. EXPERIENCE (900 RY)	
50% CONFIDENCE	7.8×10^{-4} PER RY
90% CONFIDENCE	2.6×10^{-3} PER RY

QUESTION 3. CONTAINMENT PERFORMANCE

- YES, A STATEMENT SHOULD BE INCLUDED IN THE SAFETY GOAL POLICY THAT SUCH WORK IS BEING DONE

- THE TIME SCALE IS A DRAFT DESIGN OBJECTIVE BY EARLY FY 87

- DO NOT KNOW WHAT THE COMMISSION'S EXPECTATIONS MIGHT BE

QUESTION 9. "TRUE" SOCIETAL QUANTITATIVE GOAL

- A BRIEF DISCUSSION WOULD BE APPROPRIATE IN THE SAFETY GOAL POLICY STATEMENT

- DON'T KNOW BASIC REASON WHY COMMISSION DID NOT ADOPT SOCIETAL GOALS, BUT PRESENT GOALS REASONABLY IMPLEMENTED WOULD RESULT IN LESS THAN 1 ESTIMATED CANCER FATALITY PER RY

- CURRENT REGULATORY POLICY IS SUFFICIENT TO MAINTAIN LOW SOCIETAL RISK:

    - CURRENT SITING POLICY

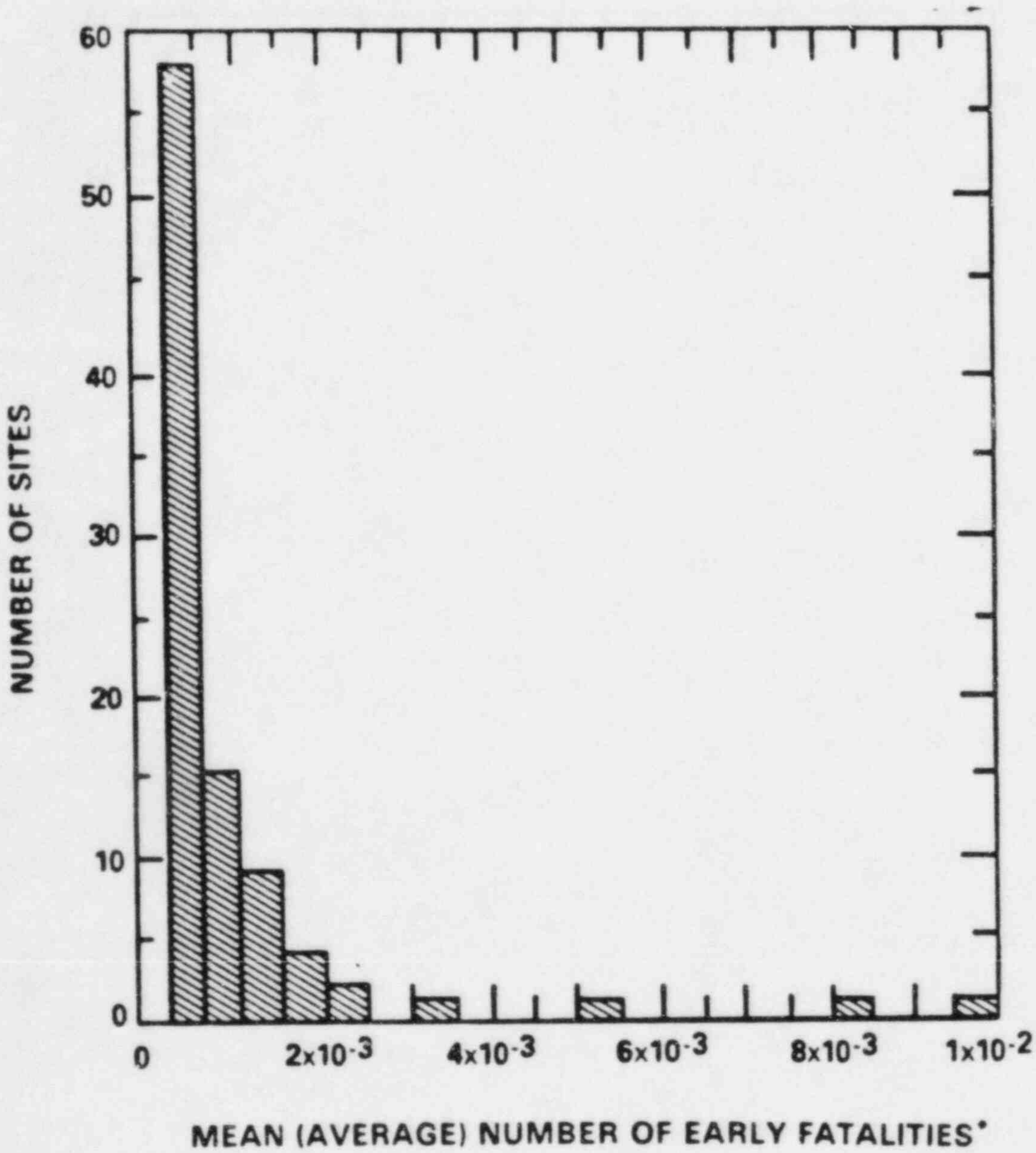
    - CURRENT SAFETY GOALS

    - $1,000 PER PERSON-REM

    - NEPA ASSESSMENTS

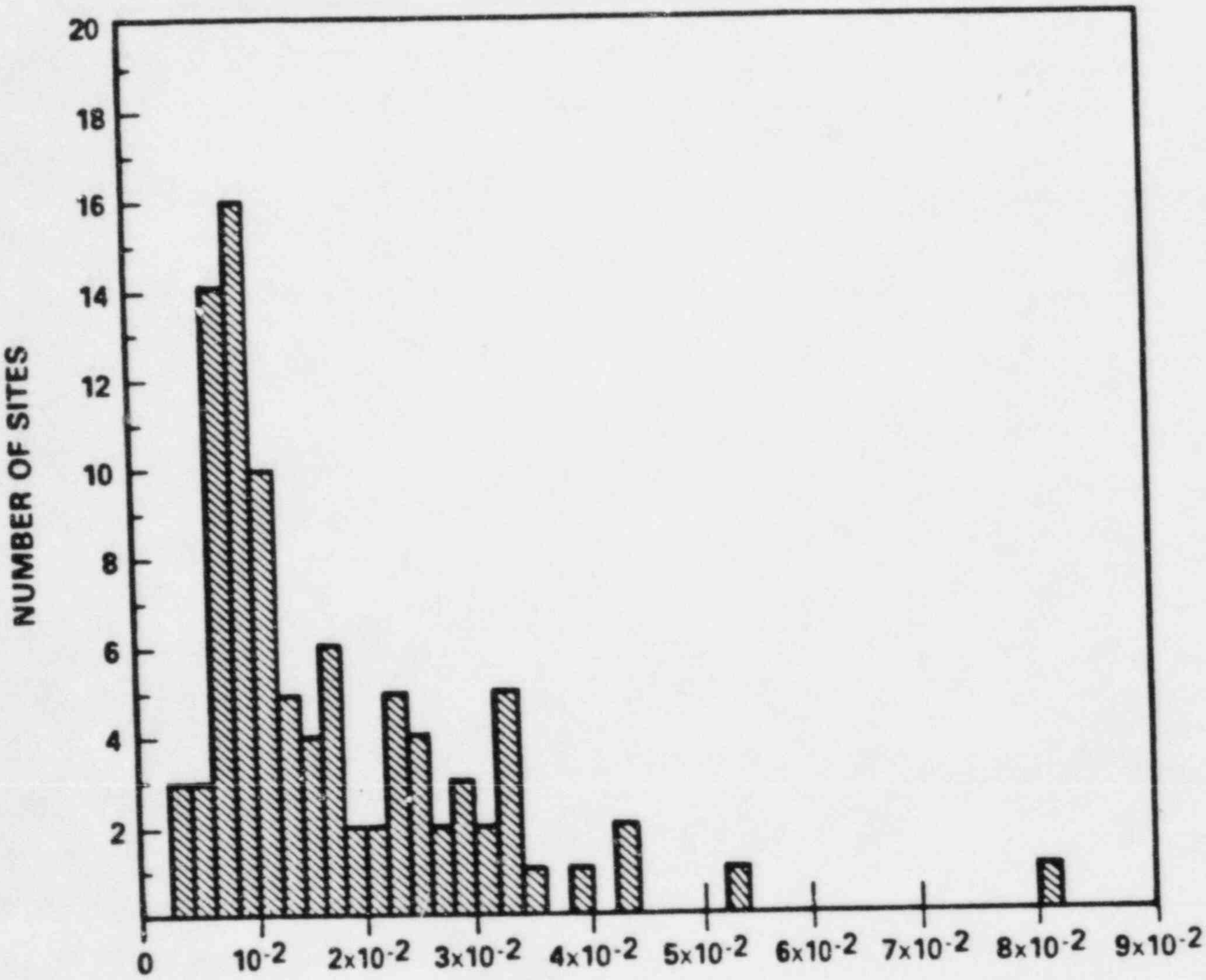
- NO CHANGE IS FELT TO BE NEEDED

HISTOGRAM OF MEAN EARLY FATALITIES FOR 91 SITES



* Assumptions: 1120 MWe Reactor; Summary Evacuation;
SST1 = 1×10^{-5} Ry^{-1}

HISTOGRAM OF MEAN NUMBER OF LATENT CANCER FATALITIES FOR EXISTING SITES



MEAN (AVERAGE) NUMBER OF LATENT CANCER FATALITIES ASSUMING SST1 = 1×10^{-5} Ry^{-1}

QUESTION 10. AVERAGING DISTANCE FOR CANCER FATALITIES

- AVERAGE CANCER FATALITY RISK INCREASES WITH DECREASING DISTANCE

  - FACTOR OF 5-10 BY AVERAGING OUT TO 10 MILES INSTEAD OF 50

  - FACTOR OF 5-7 BY AVERAGING OUT TO 1 MILE INSTEAD OF 10

- FOR REASONABLY EXPECTED OPERATING CONDITIONS, CANCER RISK IS AT LEAST A FACTOR OF 10 BELOW DESIGN OBJECTIVE AT ALL AVERAGING DISTANCES

  - IF CANCER FATALITY QDO IS BARELY MET AT ANY AVERAGING DISTANCE, BOTH CORE MELT FREQUENCY AND PROMPT FATALITY QDO WOULD TYPICALLY BE EXCEEDED BY AT LEAST A FACTOR OF 6

- 10 MILES IS APPROPRIATE

TABLE 8. SUMMARY REACTOR YEARS EXPERIENCE, DECEMBER 31st, 1984

Country	Reactors Connected to the Grid			Shut Down Reactors			Total, Operating and Shut Down			Cumulative Generation TW.h(e) Net to end 1984
	No	Capacity MW(e) Net	Experience Years Mons	No	Capacity MW(e) Net	Experience Years Mons	No	Capacity MW(e) Net	Experience Years Mons	
ARGNTINA	2	935	12 7				2	935	12 7	26.4
BELGIUM	6	3474	56 9				6	3474	56 9	138.7
BRAZIL	1	626	2 9				1	626	2 9	1.7
BULGARIA	4	1832	26 6				4	1632	26 6	72.5
CANADA	16	9521	111 11	2	456	23 8	18	9977	135 7	379.1
CZCHSLVK	3	1194	11 4	1	110	6 8	4	1304	18 0	29.5
FINLAND	4	2310	23 4				4	2310	23 4	82.8
FRANCE	41	32993	240 4	3	149	56 8	44	33142	297 0	770.3
GRMN.DR	5	1694	52 5				5	1694	52 5	92.5 (2)
GRMNY FR	19	16133	150 8	5	595	45 7	24	16728	196 3	507.0
HUNGARY	2	820	2 5				2	820	2 5	5.8
INDIA	5	1020	49 3				5	1020	49 3	37.7
ITALY	3	1273	48 7	1	150	18 3	4	1423	66 10	74.7
JAPAN	31	21751	235 10	1	12	18 3	32	21763	254 1	727.7
KOREA RP	3	1790	11 5				3	1790	11 5	34.3
NETHLNDS	2	508	27 9				2	508	27 9	41.1
PAKISTAN	1	125	13 3				1	125	13 3	3.8
S.AFRICA	1	921	9				1	921	9	3.9
SPAIN	7	4690	49 7				7	4690	49 7	111.1
SWEDEN	10	7355	77 4	1	10	10 3	11	7365	87 7	277.0
SWITZRLD	5	2882	48 10				5	2882	48 10	138.5
TAIWAN	5	4011	20 2				5	4011	20 2	79.9 (2)
UK	37	9564	623 4	2	46	33 9	39	9610	657 1	596.0
USA	85	68867	792 8	6	1514	78 1	91	70381	870 9	2901.0
USSR	46	22997	483 8				46	22997	483 8	680.7 (2)
YUGOSLAV	1	632	3 3				1	632	3 3	10.5
Total	345	219718	3176 8	22	3042	291 2	367	222760	3467 10	7824.1

Summary, as at 31st December 1984:

Total World Operating Experience is 3467 Years, 10 Months for Operating and Shut-Down Reactors.

Total Cumulative Generation is 7824.1 TW.h(e)
or 3051.4 Megatonnes of Coal Equivalent
or 1799.5 Megatonnes of Oil Equivalent

Note: No Operating Experience data available in IAEA PRIS for this country. Generation data obtained instead from UN Statistical Office, New York.

pm-1

NRR STAFF PRESENTATION TO THE ACRS

SUBJECT: MILLSTONE UNIT 1 PROBABILISTIC SAFETY STUDY
- INTRODUCTION

DATE: DECEMBER 4, 1985

PRESENTER: CHRISTOPHER GRIMES

PRESENTER'S TITLE/BRANCH/DIV: DIRECTOR, INTEGRATED SAFETY ASSESSMENT PROJECT DIRECTORATE

PRESENTER'S NRC TEL. NO.: 492-8414

MILLSTONE UNIT 1
PROBABILISTIC SAFETY STUDY (PSS)
INTRODUCTION

- NORTHEAST UTILITIES DEVELOPMENT OF PROBABILISTIC ANALYSIS METHODS

- ISAP APPLICATION OF "PSA" (49 FR 45112)
  - PENDING REQUIREMENTS AND ISSUES
  - PLANT IMPROVEMENTS
  - OPERATING EXPERIENCE
  - PROBABILISTIC INSIGHTS
  - INTEGRATED SCHEDULE

- COMPARABILITY TO OTHER PRA FINDINGS

- STATUS OF ISAP REVIEW

NRR STAFF PRESENTATION TO THE ACRS

SUBJECT: COMPARISON OF MILLSTONE 1 PSS TO MILLSTONE 1 IREP AND MILLSTONE 3 PSS

DATE: DECEMBER 4, 1985

PRESENTER: GLENN KELLY

PRESENTER'S TITLE/BRANCH/DIV: RISK ANALYST, INTEGRATED SAFETY ASSESSMENT PROJECT DIRECTORATE

PRESENTER'S NRC TEL. NO.: 492-9613

COMPARISON OF MILLSTONE 1 PSS TO MILLSTONE 3 PSS

- ONLY SIMILARITIES ARE COMMON SITE AND COMMON SWITCH YARD/RIGHT-OF-WAYS.

- UNIT 1 IS A BWR MARK 1. UNIT 3 IS A 4-LOOP WESTINGHOUSE PWR.

- UNIT 1 HAS OPERATED SINCE ABOUT 1970. UNIT 3 HAS A LOW POWER LICENSE.

- MILLSTONE 1 PSS ONLY COVERED INTERNALLY INITIATED EVENTS (FIRE ANALYSIS NOT YET RECEIVED) AND HAS A VERY CRUDE CONTAINMENT AND CONSEQUENCES ANALYSIS. THE STAFF RECEVIED A DETAILED <u>SUMMARY</u> OF THE MILLSTONE 1 PSS.

  MILLSTONE 3 PSS COVERED INTERNAL AND EXTERNAL EVENTS. MILLSTONE 3 PSS AND ITS REVIEW HAD DETAILED CONTAINMENT AND CONSEQUENCE ANALYSES. THE STAFF RECEVIED A COMPLETE SAFETY STUDY FOR MILLSTONE 3.

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS MEETING

MILLSTONE UNIT NO. 1

DECEMBER 4 - 5, 1985

NORTHEAST UTILITIES REPRESENTATIVES:

MICHAEL P. BAIN	GENERATION FACILITIES LICENSING
DR. JOHN H. BICKEL	SUPERVISOR, PROBABILISTIC RISK ASSESSMENT
RICHARD M. KACICH	SUPERVISOR, OPERATING NUCLEAR PLANT LICENSING
MITCHELL S. LEDERMAN	GENERATION FACILITIES LICENSING
WAYNE D. ROMBERG	MILLSTONE STATION SUPERINTENDENT
JOHN P. STETZ	SUPERINTENDENT, MILLSTONE UNIT 1

NU

MILLSTONE UNIT 1
PROBABILISTIC SAFETY STUDY

NU

DEVELOP AND MAINTAIN LIVING PRA MODELS FOR USE IN:

- PLANT DESIGN CHANGE - SAFETY EVALUATIONS
- TECHNICAL SPECIFICATION - SAFETY EVALUATIONS
- INTEGRATED SAFETY ASSESSMENT PROGRAM

DESIRED END PRODUCTS:

- IDENTIFICATION OF SIGNIFICANT SAFETY ISSUES
- ENGINEERING INSIGHTS ON SAFETY ISSUES
- LIVING MODEL FOR FUTURE SAFETY EVALUATIONS
- FRAMEWORK FOR FUTURE EXTERNAL EVENTS MODELING

MILLSTONE UNIT I IREP NOT USED BECAUSE:

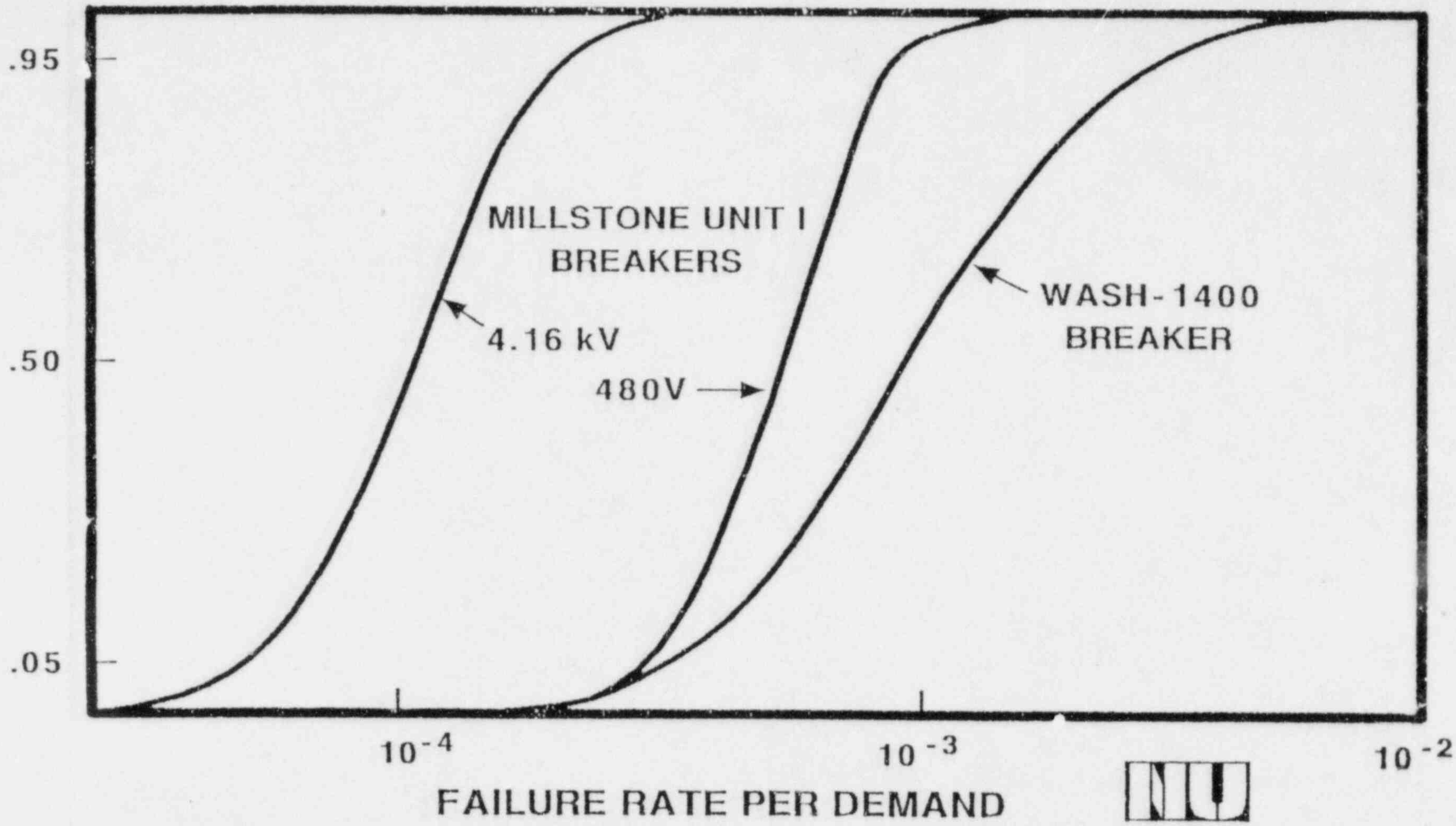
- EASE OF MODEL USE
- DESIGN CHANGES (1980-1985)
- IMPLEMENTATION OF SYMPTOM ORIENTED EMERGENCY PROCEDURES
- MODELING DEFICIENCIES IN IREP
  - INITIATORS
  - SUCCESS CRITERIA
  - "GENERIC" VS. ACTUAL PLANT RELIABILITY
  - NO COMMON CAUSE FAILURE ANALYSIS
  - UNCOUPLED HUMAN ERRORS

NU

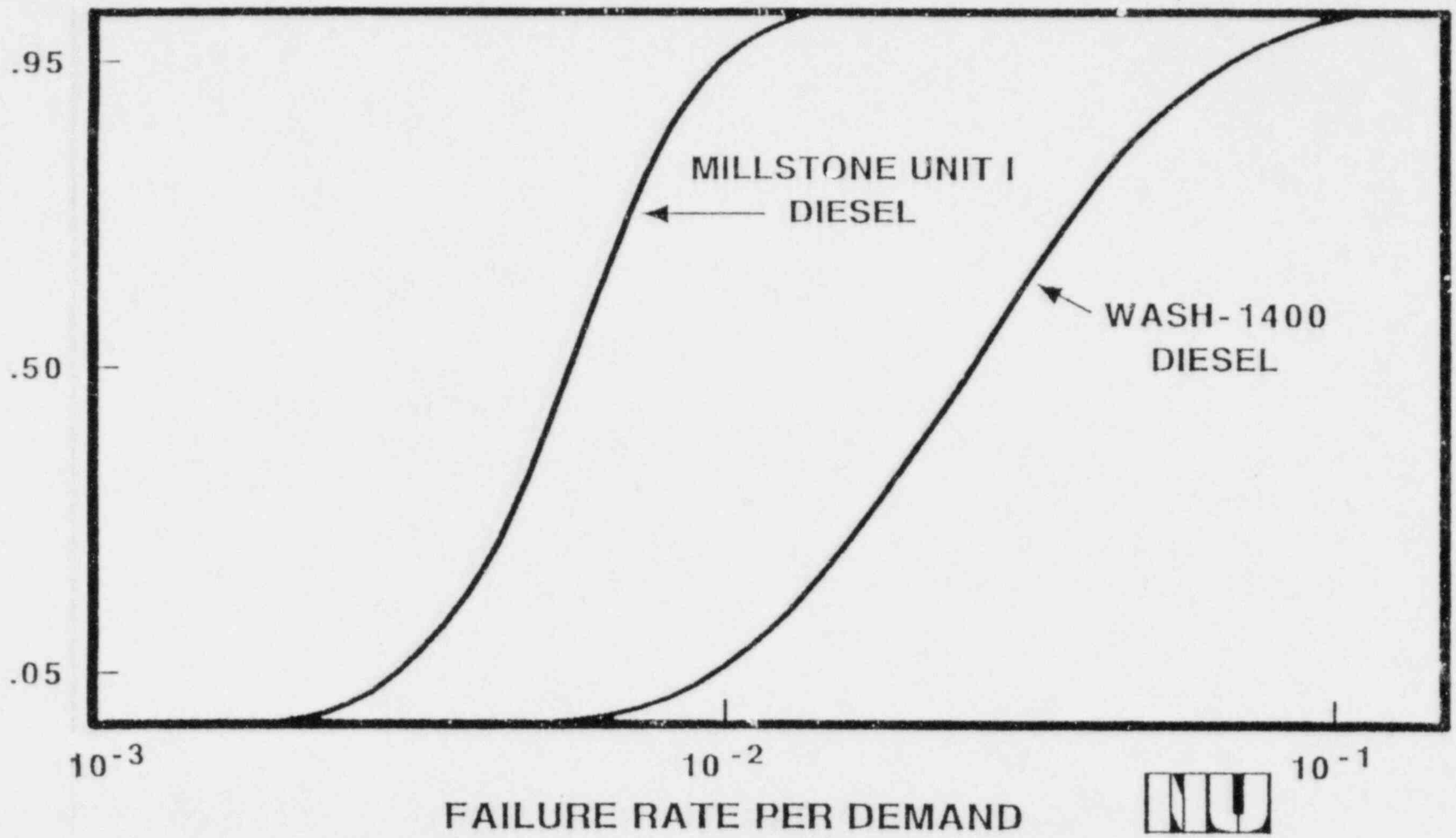
SCOPE:

- LEVEL I PRA
- LARGE EVENT TREE / LARGE FAULT TREE MODELING
- BEST ESTIMATE SAFETY ANALYSIS
- INITIATORS CONSIDERED:
  - ANTICIPATED TRANSIENTS
  - SYSTEM INTERACTIONS / CONTROL SYSTEM FAILURES
  - DESIGN BASIS EVENTS
  - BEYOND DESIGN BASIS EVENTS
- MAXIMUM USE OF PLANT EXPERIENCE
  - EVENT FREQUENCIES
  - COMPONENT RELIABILITY
  - SYSTEM RELIABILITY
  - SYSTEM MAINTENANCE DOWNTIME

COMPARISON OF MILLSTONE UNIT I DATA WITH WASH-1400



COMPARISON OF MILLSTONE UNIT I DATA WITH WASH-1400



CORE MELT FREQUENCY RESULTS:

PEACH BOTTOM (WASH-1400)	MILLSTONE 1 (IREP)	MILLSTONE 1 (PSS)
2.13×10^{-5}/YR. (MEDIAN)	3.3×10^{-4}/YR. (MEDIAN)	8.07×10^{-4}/YR. (MEAN)

CORE MELT CONTRIBUTION BY INITIATORS

• LOSS OF NORMAL POWER	30.13%
• REACTOR TRANSIENTS	
- WITH MAIN CONDENSER	11.28%
- W / O MAIN CONDENSER	2.44%
- REACTOR TRIP	6.19%
• LOSS OF FEEDWATER	12.94%
• LOSS OF SERVICE WATER	4.91%
• LOSS OF R.B.C.C.W.	0.01%
• LOSS OF T.B.S.C.C.W.	0.26%

CORE MELT CONTRIBUTION BY INITIATORS

• SMALL SMALL LOCA ($<0.01\ FT^2$)	5.14%
• SMALL LOCA ($0.01\ FT^2 < AREA < 0.2\ FT^2$)	20.84%
• LARGE LOCA	2.15%
• INADVERTANT S/R VALVE OPERATION	3.71%

CORE MELT BY INITIATORS

• IC TUBE RUPTURE (UNISOLATED)	0.02%
• R.W.C.U. LOCA (UNISOLATED)	<0.01%
• INTERFACING SYSTEM LOCA	
L.P.C.I.	<0.01%
CORE SPRAY	0.014%

CORE MELT FREQUENCY BY SAFETY ISSUES

• LONG TERM DECAY HEAT REMOVAL	64%
• STATION AC BLACKOUT	12.01%
GAS TURBINE / SERVICE WATER	6.66%
GAS TURBINE / DIESEL	5.35%

NU

ACTIONS TAKEN BY NU BASED ON PSS IMPLICATIONS:

- LONG TERM DECAY HEAT REMOVAL EVALUATED IN TERMS OF 10 CFR 50.72, 10 CFR 50.73 REPORTABILITY
- UPGRADED LPCI PUMP SURVEILLANCE (LUBE OIL COOLING) RESULTING IN ~20% REDUCTION IN CORE MELT FREQUENCY
- CONTAINMENT CONTROL EOP UPGRADE INITIATED TO CORRECT ERROR IN PUMP THROTTLING INSTRUCTIONS
- WIRING ERROR IN LPCI LOOP SELECTION LOGIC CORRECTED (1985 FALL OUTAGE)
- SW-9 (NON ESSENTIAL SERVICES WATER CUTOFF) A.B.T. BREAKER PERMANENTLY ALIGNED TO DIESEL BUS
- PROCEDURES IMPLEMENTED TO ADDRESS DRYWELL H.V.A.C. LOCKOUT LOGIC
- IC-3 REPLACED WITH SMALLER VALVE TO PROVIDE BETTER FLOW CONTROL
- PROJECT INITIATED TO RESOLVE LONG TERM DECAY HEAT REMOVAL ISSUE VIA ANALYSIS AND MAJOR HARDWARE MODIFICATIONS
- INITIATION OF SEVERAL FORMAL TECH. SPEC. CHANGES TO REDUCE FREQUENCY OF REACTOR SCRAMS FROM HIGH RISK SURVEILLANCE ACTIVITIES

TOTAL CORE MELT FREQUENCY PER REACTOR YEAR

PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
2.13×10^{-5} (1) (1975)	3.30×10^{-4} (1) (1983)	8.07×10^{-4} (2) (1985)

(1) MEDIAN VALUES

(2) MEAN VALUES

CONTRIBUTORS TO CORE MELT FREQUENCY BY PERCENT CONTRIBUTION

INITIATING EVENT	PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
LOSS OF NORMAL POWER	0.47%	85.45%(1)	30.13%(2)
REACTOR TRANSIENTS (WITH PCS AVAILABLE)	96.10%	2.12%	17.47%
REACTOR TRANSIENTS (W/O PCS)	NOT EXPLICITLY ANALYZED	8.48%	2.44%
LOSS OF FEEDWATER	NOT EXPLICITLY ANALYZED	0.30%	12.94%
SAFETY/RELIEF VALVE TRANSIENTS	N.A.	2.73%	3.71%
SMALL SMALL BREAK LOCA	NOT EXPLICITLY ANALYZED	NOT EXPLICITLY ANALYZED	5.14%
SMALL BREAK LOCA	1.64%	0.91%	20.84%
INTERMEDIATE BREAK LOCA	1.22%		CONSIDERED IN OTHER CATEGORIES
LARGE BREAK LOCA	0.61%		2.15%
LOSS OF SERVICE WATER	N.A.	N.A.	4.91%
LOSS OF TBSCCW	N.A.	N.A.	0.26%
LOSS OF RBCCW	N.A.	N.A.	0.01%
INTERFACING SYSTEMS LOCA	N.A.	N.A.	0.04%

N.A. - NOT ANALYZED IN THE STUDY

(1) 28% DUE TO STATION BLACKOUT

(2) 33% DUE TO STATION BLACKOUT

SYSTEM SUCCESS CRITERIA

SYSTEM	PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
HIGH PRESSURE MAKEUP	1 OF 1 HPCI PUMP	1 OF 2 FEEDWATER TRAINS AND 1 OF 2 CONDENSATE TRANSFER PUMPS	1 OF 2 FEEDWATER TRAINS AND 1 OF 1 EMERGENCY CONDENSER TRANSFER PUMPS(1)
HIGH PRESSURE CORE COOLING	1 OF 1 RCIC PUMP	I.C.(2)	I.C.(2)
LOW PRESSURE CORE SPRAY	2 OF 2 CSIS LOOPS(3) (4 PUMPS)	1 OF 2 LPCS PUMPS	1 OF 2 LPCS PUMPS
LOW PRESSURE COOLANT INJECTION	3 OF 4 LPCI PUMPS	2 OF 4 LPCI PUMPS	1 OF 4 LPCI PUMPS(4)
AUTOMATIC DEPRESSURIZATION (SAFETY/RELIEF VALVES)	3 OF 5 VALVES	2 OF 4 VALVES OR 4 OF 6 VALVES OPENED BY OPERATOR	2 OF 4 VALVES OR 2 OF 6 VALVES OPENED BY OPERATOR(4)
STANDBY LIQUID CONTROL	NOT SPECIFICALLY CONSIDERED	NOT CONSIDERED	1 OF 2 PUMPS IN SLCS
RESIDUAL HEAT REMOVAL HEAT EXCHANGERS (LOCA EVENTS)	1 OF 4 RHR HEAT EXCHANGERS	1 OF 2 LPCI HEAT EXCHANGERS	1 OF 2 LPCI HEAT EXCHANGERS(5)

SYSTEM SUCCESS CRITERIA

SYSTEM	PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
CONTAINMENT SPRAYS	1 LPCRS PUMP	1 LPCI PUMP	1 LPCI PUMP
EMERGENCY AC POWER POWER	1 OF 2 DIESEL GENERATORS	1 OF 2 GENERATORS(6)	1 OF 2 GENERATORS(6)
COOLING WATER	1 OF 2 ESW PUMPS AND 1 OF 4 HPSWS PUMPS	RBCCW - 1 OF 2 PUMPS AND 2 OF 3 HEAT EXCHANGERS AND SWS - 1 OF 4 PUMPS FOR ALL EVENTS AND EWS - 1 OF 2 PUMPS IN EITHER LOOP	RBCCW - 1 OF 2 PUMPS AND 2 OF 3 HEAT EXCHANGERS AND SWS - 2 OF 4 PUMPS FOR NON-LOSP EVENTS(5) OR 1 OF 2 PUMPS FOR LOSP EVENTS(7) AND ESW - 2 OF 2 PUMPS IN BOTH LOOPS (4 PUMPS)(5) AND TBSCCW - 1 OF 2 PUMPS AND 2 OF 2 HEAT EXCHANGERS

SYSTEM SUCCESS CRITERIA

SYSTEM	PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
LONG TERM DECAY HEAT REMOVAL (FUNCTION)	NOT SPECIFICALLY CONSIDERED	I.C. (NON-LOCA EVENTS) OR 1 OF 2 SDC TRAINS (NON-LOCA EVENTS) OR 1 OF 2 LPCI TRAINS WITH ASSOCIATED HEAT EXCHANGER COOLED BY 1 OF 2 CORRESPONDING ESW PUMPS	I.C. (NON-LOCA EVENTS) OR MAIN CONDENSER (IF FEEDWATER AVAILABLE) OR 1 OF 2 SDC TRAINS (NON-LOCA EVENTS) OR 2 OF 2 LPCI TRAINS (1 OF 2 PUMPS) WITH ASSOCIATED HEAT EXCHANGERS COOLED BY 2 OF 2 CORRESPONDING ESW PUMPS(5)

(1) THE NORMAL CONDENSATE TRANSFER PUMP WAS DETERMINED TO BE INSUFFICIENT FOR FEEDWATER FLOW REQUIREMENTS

(2) ISOLATION CONDENSER USED IN PLACE OF RCIC FOR NON-LOCA EVENTS

(3) REQUIRED ONLY FOR LPCI FAILURE. OTHERWISE ONLY 1 OF 2 CSIS LOOPS REQUIRED

(4) BASED ON G.E. BEST ESTIMATE ANALYSIS FOR MILLSTONE UNIT NO. 1

(5) BASED ON ORIGINAL EQUIPMENT DESIGN SPECIFICATIONS AND BEST ESTIMATE CALCULATIONS

(6) MILLSTONE'S FEEDWATER (FWCI) SYSTEM REQUIRES SUCCESS OF THE GAS TURBINE GENERATOR FOR SYSTEM SUCCESS FOLLOWING A LOSS OF OFFSITE POWER EVENT (I.E. FWCI IS A ONE TRAIN SYSTEM)

(7) BASED ON ACTUAL CONFIGURATION OF SERVICE WATER SYSTEM FOLLOWING A LOSS OF OFFSITE POWER EVENT

FREQUENCY OF ANTICIPATED TRANSIENTS PER REACTOR-YEAR

EVENT CATEGORY	PEACH BOTTOM (R.S.S.)[1]	MILLSTONE UNIT NO. 1 (IREP)[2]	MILLSTONE UNIT NO. 1 (ISAP)[3]
REACTOR TRANSIENTS (PCS AVAILABLE)	7	6.60	5.07
LOSS OF FEEDWATER	3	0.18[4]	0.096
REACTOR TRANSIENTS (W/O PCS AVAILABLE)	NOT SPECIFICALLY CONSIDERED	2.14	0.435
SAFETY/RELIEF VALVE TRANSIENTS	NOT SPECIFICALLY CONSIDERED	0.20[5]	2.02×10^{-2}[6]
LOSS OF OFFSITE AC POWER	0.20	0.20	0.124
TOTAL LOSS OF SERVICE WATER	N.A.	N.A.	7.83×10^{-03}[7]
TOTAL LOSS OF TBSCCW	N.A.	N.A.	8.05×10^{-4}[7]
TOTAL LOSS OF RBCCW	N.A.	N.A.	4.73×10^{-4}[7]
LOSS OF 120V VITAL AC POWER	N.A.	N.A	1.65×10^{-2}[7]

(1) BASED ON NUCLEAR POWER PLANT EXPERIENCE FOR PWR's AND BWR's

(2) BASED ON GENERIC BWR PLANT EXPERIENCE

(3) BASED ON MILLSTONE PLANT SPECIFIC EXPERIENCE

(4) BASED ON TOTALING THE UPPER BOUND CHI-SQUARE APPROXIMATIONS FOR NO FAILURES IN THE FEEDWATER, SW AND TBSCCW SYSTEMS IN 12 YEARS (I.E. .06/YR FOR EACH SYSTEM)

(5) BASED ON DATA FOR 3-STAGE VALVES (OLD DESIGN)

(6) BASED ON DATA FOR 2-STAGE VALVES (NEW DESIGN)

(7) BASED ON FAULT TREE ANALYSIS OF SYSTEM DEPENDENCIES AND INTERACTIONS

N.A. - NOT APPLICABLE SINCE SUCH EVENTS WERE NOT ANALYZED

Additional Information on NOTE (4)

In IREP it was noted that Millstone Unit No. 1 had never experienced a total loss of feedwater in 12 years of operation. Because of this, IREP approximated the upper bound failure frequency at a 50% confidence level by using a chi-squared sampling test, which yields an estimated frequency of .06/yr for feedwater system failure. IREP also noted that feedwater is lost whenever there is a total loss of TBSCCW system cooling to feedwater or service water cooling to TBSCCW. Consequently, it can be implied that zero feedwater system failures equates to no TBSCCW or service water system failures as well. IREP then derived a total frequency for loss of feedwater by adding the chi-square approximations for no failures of the feedwater, TBCCW and service water systems (e.g. 0.18 failures per year).

FREQUENCY OF LOCA PER REACTOR-YEAR

EVENT CATEGORY	PEACH BOTTOM (R.S.S.)	MILLSTONE UNIT NO. 1 (IREP)	MILLSTONE UNIT NO. 1 (ISAP)
SMALL SMALL BREAK LOCA	NOT SPECIFICALLY CONSIDERED	NOT SPECIFICALLY CONSIDERED	1.0×10^{-2} (1)
SMALL BREAK LOCA	1.0×10^{-3}	2.0×10^{-3} (2)	1.0×10^{-3}
INTERMEDIATE BREAK LOCA	3.0×10^{-4}	2.0×10^{-4} (2)	CONSIDERED IN (3) OTHER CATEGORIES
LARGE BREAK LOCA	1.0×10^{-4}	2.0×10^{-4} (2)	1.0×10^{-4}
INTERFACING SYSTEMS LOCA	NOT EXPLICITLY CONSIDERED	NOT EXPLICITLY CONSIDERED	2.9×10^{-7}

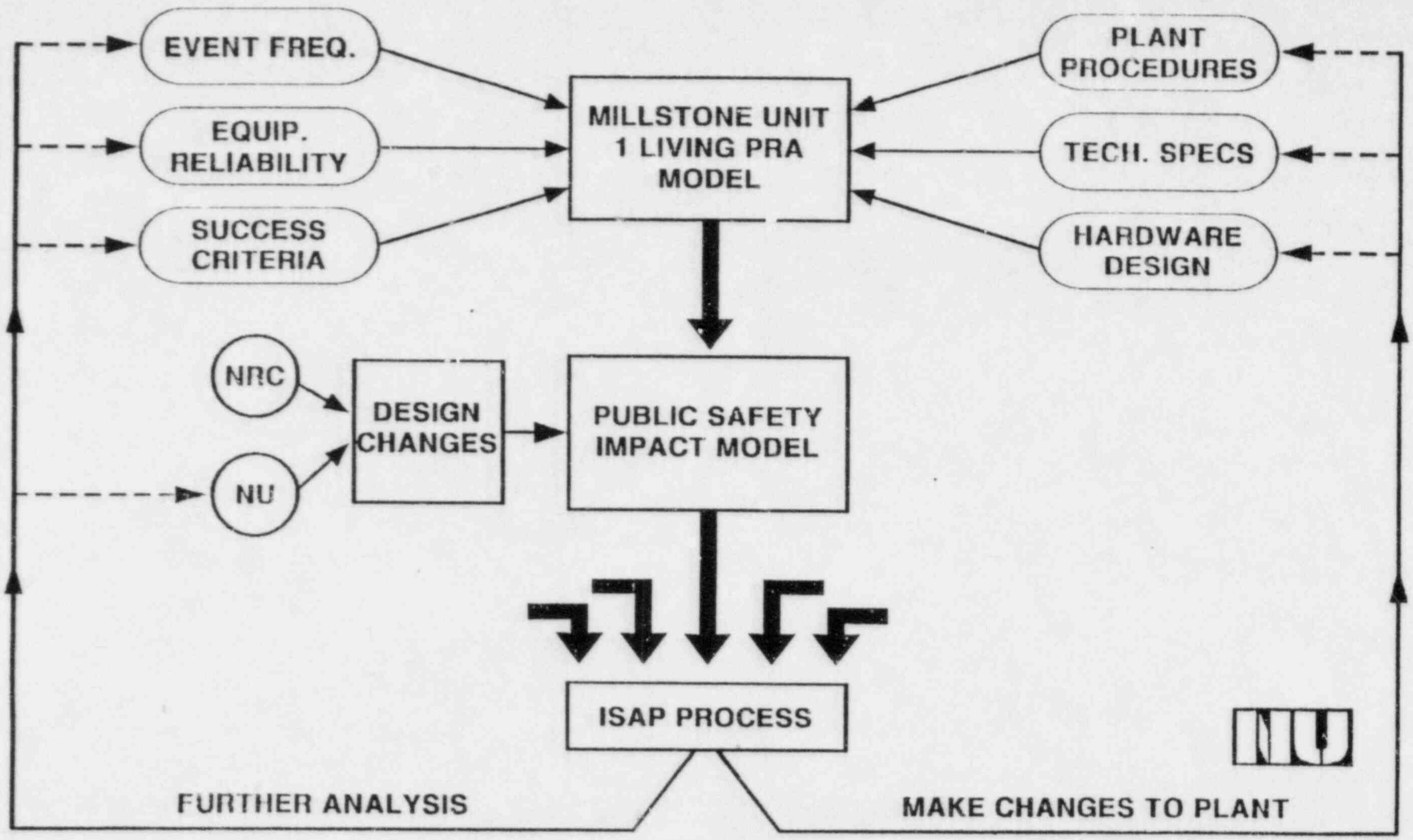
(1) BREAK SIZE 0.01 FT^2

(2) FREQUENCY BASED ON EQUAL CONTRIBUTION FROM STEAMLINE BREAKS AND LIQUID LINE BREAKS

(3) INTERMEDIATE BREAK LOCA DIVIDEND BETWEEN SMALL AND LARGE BREAK LOCA'S

MILLSTONE UNIT 1
ISAP PUBLIC SAFETY IMPACT MODEL
APPLICATIONS

NU



EVENT FREQ.
EQUIP. RELIABILITY
SUCCESS CRITERIA
MILLSTONE UNIT 1 LIVING PRA MODEL
PLANT PROCEDURES
TECH. SPECS
HARDWARE DESIGN
NRC
NU
DESIGN CHANGES
PUBLIC SAFETY IMPACT MODEL
ISAP PROCESS
NU
FURTHER ANALYSIS
MAKE CHANGES TO PLANT

MATRIX QUANTIFICATION PROCESS

$$\Phi_j \times M_{jk} = P_k$$

Φ_j = FREQUENCY OF THE J^{TH} INITIATING EVENT (I.E. LOSS OF FEEDWATER, TURBINE TRIP, ETC.)

M_{jk} = CONDITIONAL PROBABILITY OF ENDING UP IN K^{TH} PLANT DAMAGE STATE, GIVEN J^{TH} INITIATING EVENT.

P_k = FREQUENCY OF K^{TH} PLANT DAMAGE STATE (I.E. EARLY CORE MELT / INTACT CONTAINMENT, LATE CORE MELT / FAILED CONTAINMENT, ETC.)

NU

EVALUATION OF RISK IMPACT

$$\Delta RISK = T \sum_{k} \Delta P_k \; E_k$$

ΔRISK = CHANGE IN MAN-REM DUE TO IMPLEMENTATION OF CHANGE

T = EXPOSURE PERIOD (I.E. REMAINING OPERATING YEARS)

ΔP_k = CHANGE IN THE K^{TH} PLANT DAMAGE STATE FREQUENCY DUE TO IMPLEMENTATION OF CHANGE

E_k = MAN-REM CONSEQUENCES GIVEN K^{TH} PLANT DAMAGE STATE

NU

POTENTIAL IMPACT OF RESOLVING LONG TERM DECAY HEAT REMOVAL ISSUE AT MILLSTONE UNIT 1

Δ CMF = 64.0%

Δ RISK = 38,000 MAN-REM

POTENTIAL IMPACT OF CONTROL ROOM HABITABILITY MODIFICATIONS

ISSUES ASSESSED:

- CHLORINE GAS RELEASE
- MILLSTONE 2 CORE MELT / RADIATION RELEASE
- HVAC IMPACT ON OPERATOR ENVIRONMENTAL STRESS

NU

INSIGHTS FROM PSS

- CHLORINE GAS RELEASE

  Δ CMF = 9.2×10^{-7}/YR.

  Δ RISK = 34.5 MAN-REMS

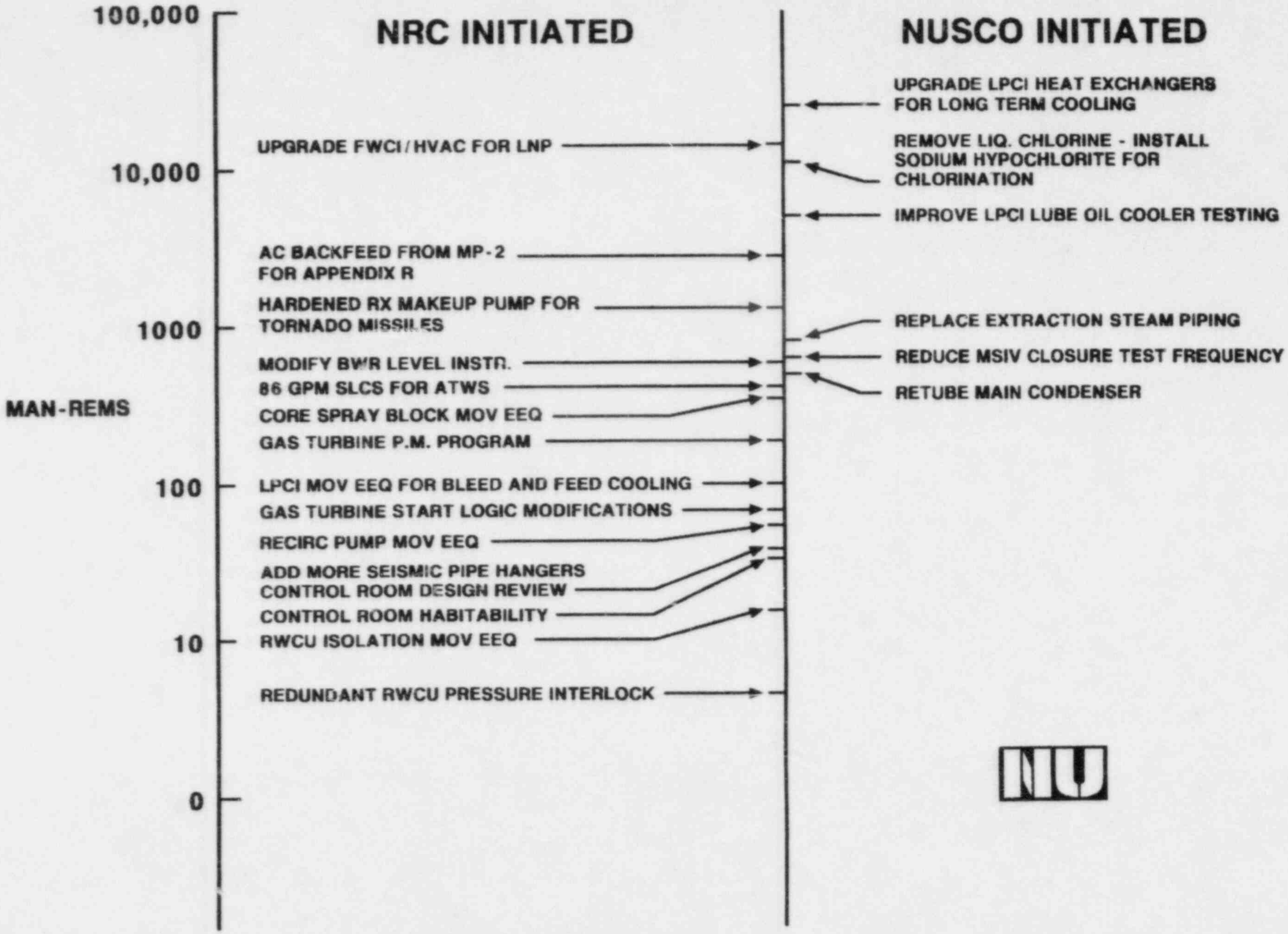
- MILLSTONE 2 CORE MELT

  Δ CMF = 2.5×10^{-7}/YR.

  Δ RISK = 10.0 MAN-REMS

- ENVIRONMENTAL STRESS (ENGINEERING JUDGEMENT)

  Δ RISK = 45 MAN-REMS



NRC INITIATED
NUSCO INITIATED
100,000
10,000
1000
100
10
0
MAN-REMS
UPGRADE FWCI/HVAC FOR LNP
AC BACKFEED FROM MP-2 FOR APPENDIX R
HARDENED RX MAKEUP PUMP FOR TORNADO MISSILES
MODIFY BWR LEVEL INSTR.
86 GPM SLCS FOR ATWS
CORE SPRAY BLOCK MOV EEQ
GAS TURBINE P.M. PROGRAM
LPCI MOV EEQ FOR BLEED AND FEED COOLING
GAS TURBINE START LOGIC MODIFICATIONS
RECIRC PUMP MOV EEQ
ADD MORE SEISMIC PIPE HANGERS
CONTROL ROOM DESIGN REVIEW
CONTROL ROOM HABITABILITY
RWCU ISOLATION MOV EEQ
REDUNDANT RWCU PRESSURE INTERLOCK
UPGRADE LPCI HEAT EXCHANGERS FOR LONG TERM COOLING
REMOVE LIQ. CHLORINE - INSTALL SODIUM HYPOCHLORITE FOR CHLORINATION
IMPROVE LPCI LUBE OIL COOLER TESTING
REPLACE EXTRACTION STEAM PIPING
REDUCE MSIV CLOSURE TEST FREQUENCY
RETUBE MAIN CONDENSER

pm-3
T-14

REVIEW OF PSA-BASED EVALUATION OF INTEGRATED SAFETY ASSESSMENT PROGRAM (ISAP) ISSUES

FOR MILLSTONE UNIT 1

BY

BAHMAN ATEFI

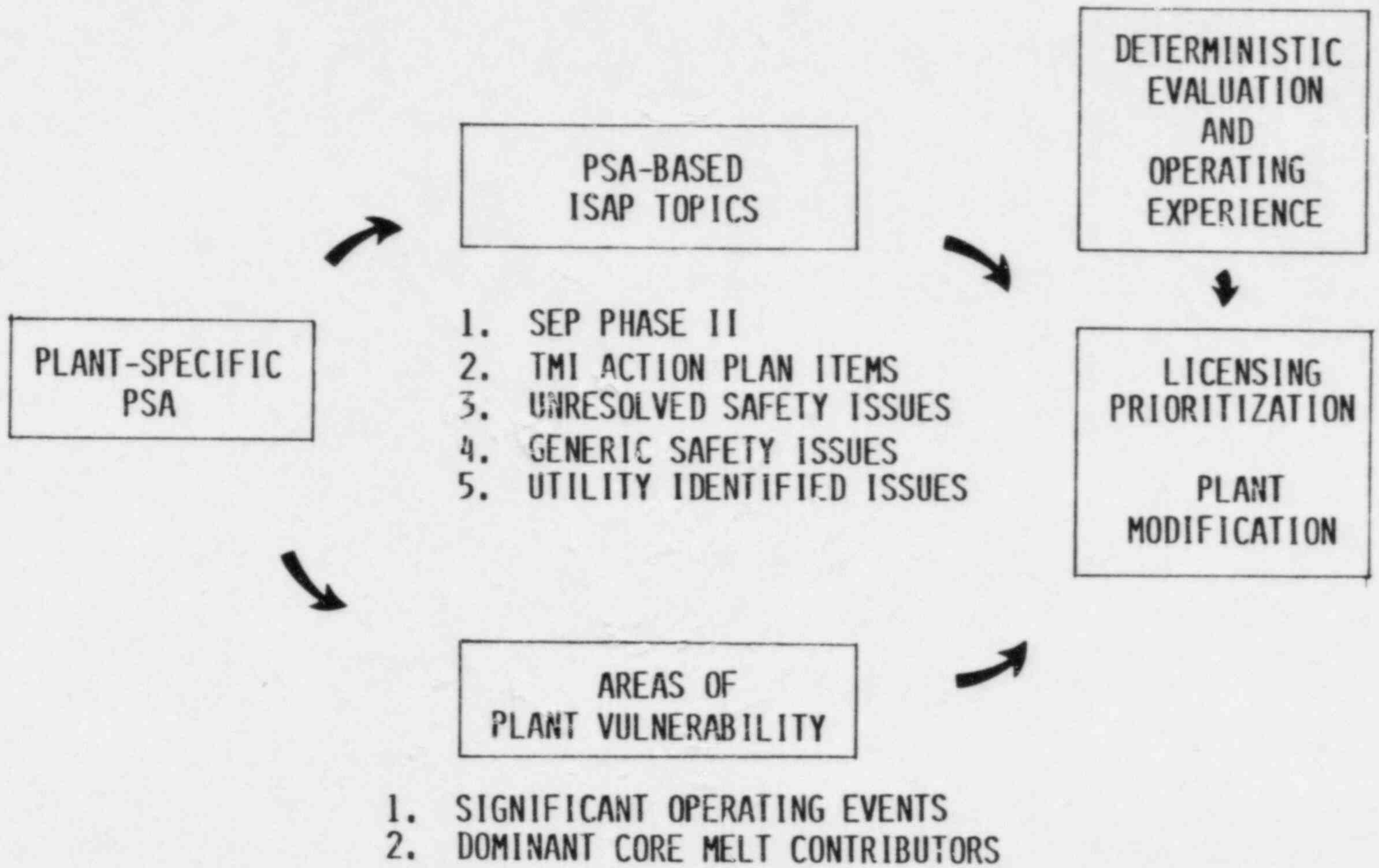
PRESENTED TO THE ACRS

DECEMBER 4, 1985

OUTLINE

- OVERALL PROCESS
- REVIEW OF THE MILLSTONE UNIT 1 PSS
- INSIGHTS INTO AREAS OF PLANT VULNERABILITY
- REVIEW OF THE MILLSTONE UNIT 1 PSA-BASED ISAP TOPICS

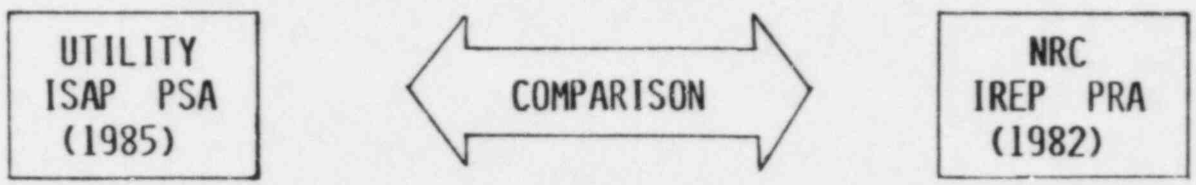
ISAP PROVIDES LICENSING MECHANISM FOR RESOLVING SAFETY ISSUES



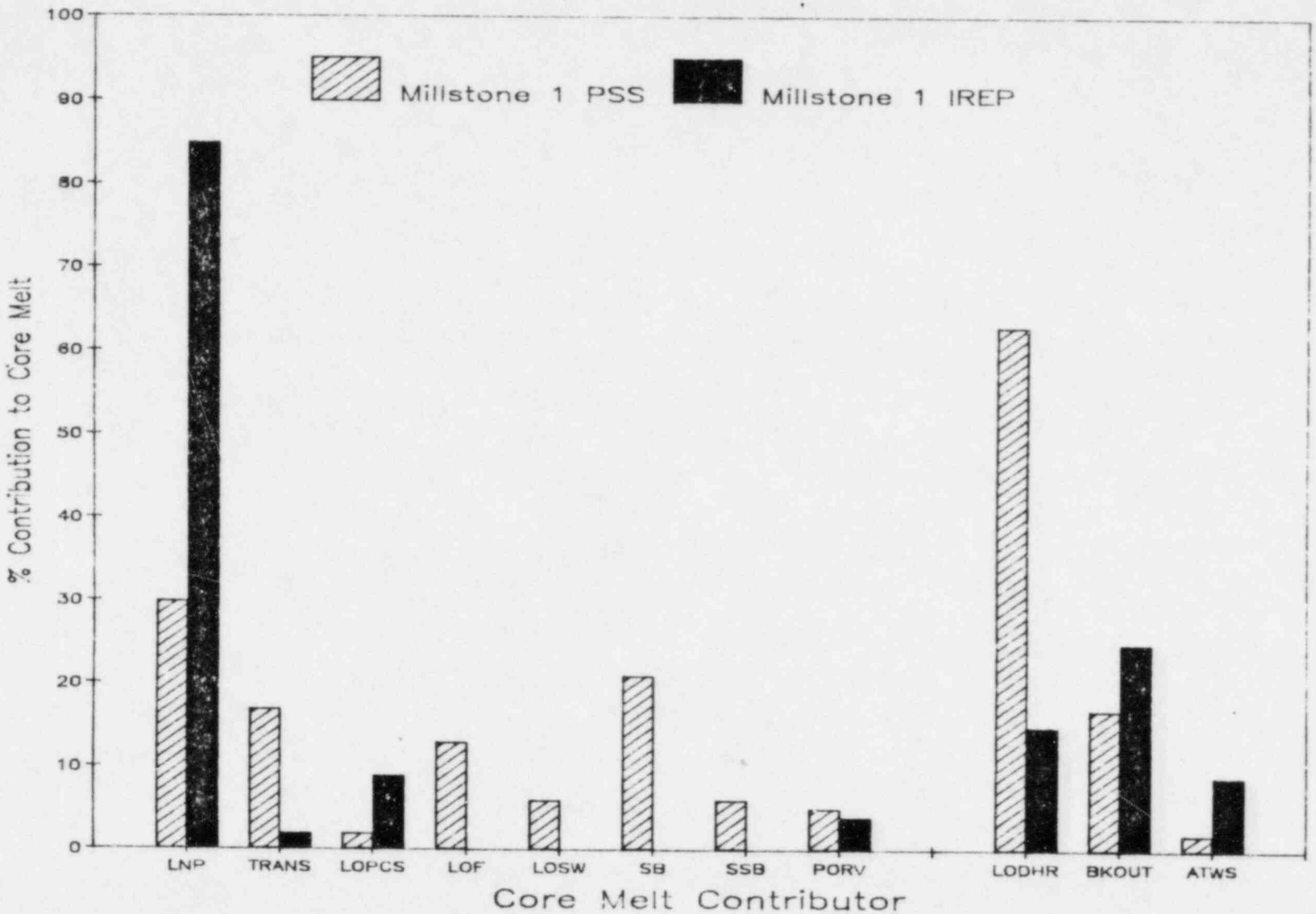
PLANT-SPECIFIC PSA IS REQUIRED TO

PROVIDE PERSPECTIVE ON ALL SAFETY ISSUES

PSA REVIEW PROCESS



- INITIATING EVENTS
- EVENT TREES
- COMPONENT AND SYSTEM RELIABILITY
- HUMAN RELIABILITY
- DOMINANT ACCIDENT SEQUENCES AND RESULTS



Millstone 1 PSS
Millstone 1 IREP
% Contribution to Core Melt
100
90
80
70
60
50
40
30
20
10
0
LNP
TRANS
LOPCS
LOF
LOSW
SB
SSB
PORV
LODHR
BKOUT
ATWS
Core Melt Contributor

ISAP AND IREP PSAs

PROVIDED DIFFERENT RISK PERSPECTIVES

DIFFERENCE:	LOSS OF NORMAL POWER
IREP:	85% CORE MELT
ISAP:	30% CORE MELT

REASONS:

- LOWER DIESEL GENERATOR, GAS TURBINE AND SWITCHGEAR BREAKER FAILURE RATES
- MODIFICATIONS TO LNP LOGIC
- MODIFICATION TO IC MAKEUP

ISAP AND IREP PSAs

PROVIDED DIFFERENT RISK PERSPECTIVES

DIFFERENCE:	LOSS OF DECAY HEAT REMOVAL
IREP:	15% CORE MELT
ISAP:	65% CORE MELT
REASON:	HIGHER FAILURE PROBABILITY OF THE ALTERNATE SHUTDOWN COOLING SYSTEM

UTILITY'S ISAP PSA BELIEVED TO PROVIDE REASONABLE PLANT RISK PERSPECTIVE

- LATEST PSA TECHNIQUES UTILIZED
  - OPERATOR ACTION TREES
  - PLANT-SPECIFIC DATA
  - HUMAN RELIABILITY
- DIFFERENCES BETWEEN ISAP AND IREP PSAs SATISFACTORILY RESOLVED

REVIEW OF THE ISAP PSA PROVIDED INSIGHTS INTO SIGNIFICANT AREAS OF PLANT VULNERABILITY

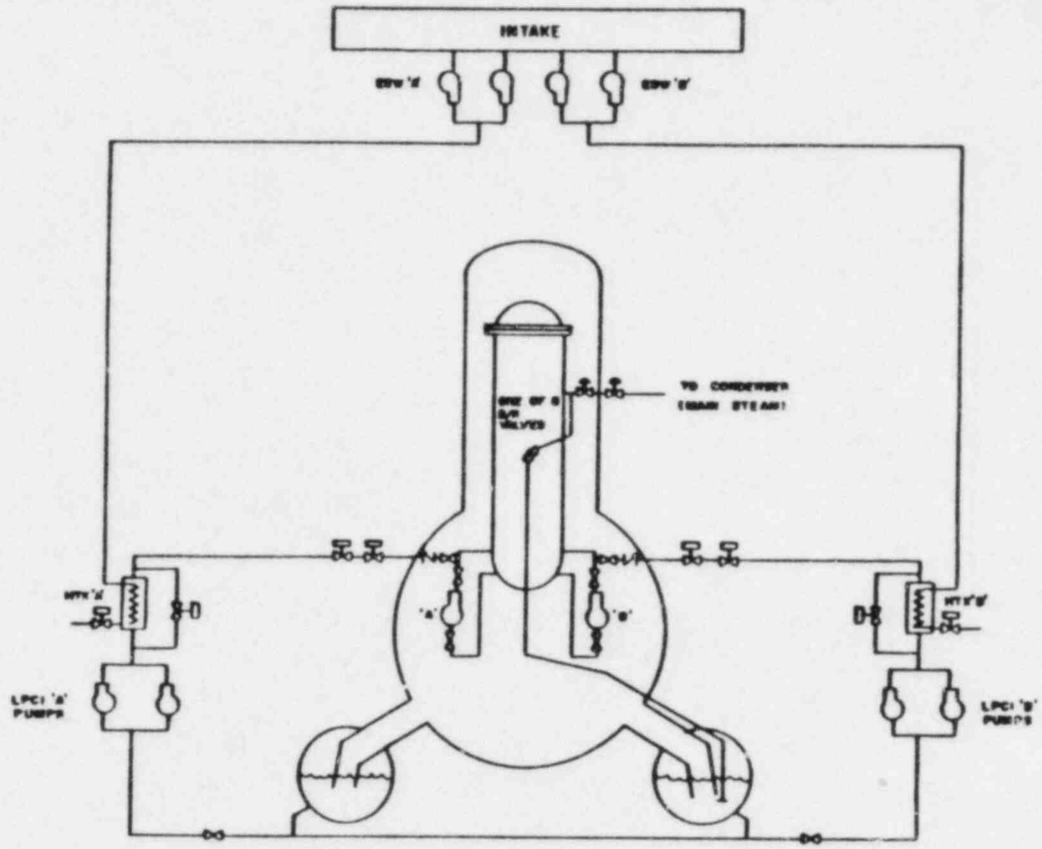
- LONG-TERM DECAY HEAT REMOVAL SYSTEM FAILURE INCLUDED IN SEQUENCES CONTRIBUTING TO ABOUT 65% OF THE CORE-MELT FREQUENCY

- COGNITIVE OPERATOR ERROR TO RESTORE PRESSURE VESSEL LEVEL INCLUDED IN SEQUENCES CONTRIBUTING TO ABOUT 20% OF THE CORE-MELT FREQUENCY

- 60% OF THE DOMINANT ACCIDENT SEQUENCES CONTAINED FAILURE OF ISOLATION CONDENSER OR IC MAKEUP OR S/R VALVE BEING STUCK OPEN

AREAS OF PLANT VULNERABILITY

ALTERNATE SHUTDOWN COOLING SYSTEM RELIABILITY



ALTERNATE SHUTDOWN COOLING SYSTEM

UNAVAILABILITY OF THE ALTERNATE SHUTDOWN COOLING SYSTEM

AS A FUNCTION OF DIFFERENT SYSTEM CONFIGURATIONS

System Configuration	Alternate Shutdown Cooling System Unavailability	Unavailability Reduction Factor
1. Present	0.15	-
2. LPCI/Containment Cooling Loops Redundant	0.09	1.7
3. Emergency Service Water (ESW) Loops Redundant	0.07	2.2
4. Both LPCI and ESW Loops Redundant	0.004	40.0

CONCLUSIONS

ISAP PROVIDES UNIQUE OPPORTUNITY TO

SYSTEMATICALLY RESOLVE ALL SAFETY ISSUES

1. PLANT-SPECIFIC PSA REQUIRED
2. AREAS OF PLANT VULNERABILITY IDENTIFIED
3. PSA-BASED ISAP TOPICS ANALYZED
4. ALL SAFETY ISSUES PRIORITIZED USING THE RESULTS OF THE DETERMINISTIC AND PROBABILISTIC ANALYSES

RESULTS OF THE PSA-BASED ISAP TOPICS

Reduction in Core Melt Frequency (Per Year)

(Total Core Melt Frequency = 8E-4)

Topic	SAIC	Utility	Notes
PSA Identified areas of Vulnerability			
Reliability of the Alternate Shutdown Cooling System	5E-4	N/A	
Risk-Based Isap Topics			
2.31 LPCI Lube Oil Cooler Test Frequency	1E-4	1E-4	1
1.05 Ventilation System Modifications	6E-5	2E-4	2
1.16.1/2 Millstone Unit 1/Unit 2 Backfeed/Modify CRD Pumps	5E-5	5E-5	3
1.02 Tornado Missile Protection	3E-5	4E-5	3
2.08 Extraction Steam Piping Replacement	2E-5	2E-5	

RESULTS OF THE PSA-BASED ISAP TOPICS

REDUCTION IN CORE MELT FREQUENCY (PER YEAR)

(TOTAL CORE MELT FREQUENCY = 8E-4)

TOPIC	SAIC	UTILITY	NOTES
1.17 REPLACEMENT OF MOVs			
1-ICU-2,3	9E-6	2E-7	
1-Cs-5A,B	5E-6	5E-6	
1-RR-2A,B	1E-6	1E-6	
1-LP-7A,B AND 1-LP-70A,B	9E-7	9E-7	
1-IC-2,3,4	3E-8	2E-9	
	1E-5	7E-6	
1.05 BWR VESSEL WATER LEVEL INSTRUMENTATION	1E-5	1E-5	
1.0/1.24 GAS TURBINE GENERATOR START LOGIC MODIFICATION/ EMERGENCY POWER	1E-5	1E-5	
2.30 MSIV CLOSURE TEST FREQUENCY	9E-6	8E-6	
2.07 SODIUM HYPOCHLORITE SYSTEM	7E-6	1E-4	4

RESULTS OF THE PSA-BASED ISAP TOPICS

Reduction in Core Melt Frequency (Per Year)

(Total Core Melt Frequency = 8E-4)

Topic	SAIC	Utility	Notes
2.06 Main Condenser Retube	3E-6	6E-6	
1.18 ATWS-Upgrading of SLCS	1E-6	6E-6	
1.06 Seismic Qualification of Safety Related Piping	5E-7	5E-7	5
[illegible].01 LPCI Remotely Operated Valve 1-LP50A&B	5E-7	5E-7	5
1.12 Control Room Habitability	2E-7	1E-6	
1.04 RCWU System Pressure Interlock	3E-8	3E-8	
2.04 High Steam Flow Setpoint Increase	+1E-6	+7E-8	6
1.21 Fault Transfers	+3E-6	+3E-6	6

NOTES:

1. THE MODIFICATIONS RECOMMENDED BY THIS TOPIC HAVE ALREADY BEEN INCORPORATED.

2. BOUNDING ANALYSIS WAS USED FOR THE EVALUATION OF THIS TOPIC.

3. FREQUENCY CHANGE LISTED IN THIS TABLE IS FOR EACH OF THE TOPICS 1.16.1/2 AND 1.02 INDEPENDENTLY. THESE TWO TOPICS ARE VERY OBVIOUSLY INTERRELATED. IF BOTH ARE DONE (I.E., IF THE BACKFEED CONNECTION IS PROVIDED AND THE CITY WATER HOOKUP IS MADE) THE TOTAL BENEFIT IS 6E-5/YR NOT 9E-5/YR.

4. EQUIVALENT CORE MELT FREQUENCY

5. ENGINEERING JUDGMENT WAS USED BY THE UTILITY TO ASSIGN TO THIS TOPIC A RANKINIG ON THE STATE OF -10 TO +10. THIS RANKING WAS CONVERTED TO AN EQUIVALENT CHANGE IN CORE MELT FREQUENCY.

6. RESOLUTION OF THIS TOPIC RESULTS IN AN INCREASE IN THE CORE MELT FREQUENCY.