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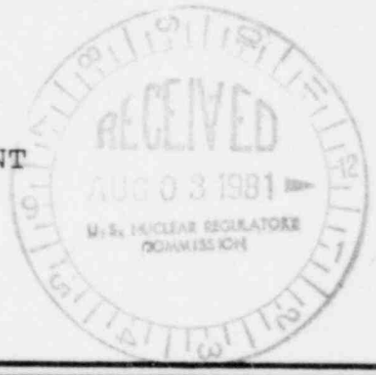
In the Matter of:

ACRS - SUBCOMMITTEE MEETING ON

RELIABILITY AND PROBABILISTIC ASSESSMENT



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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING ON

RELIABILITY AND PROBABILISTIC ASSESSMENT

Belmont Room
Best Western Airport Park Hotel
600 Avenue of the Champions
Inglewood, California 90301

Wednesday, July 29, 1981

The meeting of the subcommittee was convened, pursuant to notice, at 8:30 a.m.

PRESENT: DR. DAVID OKRENT, Chairman
W. KERR
C. SIESS
M. GRIESMEYER
J.C. MARK
M. PLESSET
H. LEWIS

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PROCEEDINGS

(8:30 a.m.)

CHAIRMAN OKRENT: Good morning.

According to the agenda, there is to be an opening statement by the Chairman, but the only thing I'm going to offer now is that I'm going to ask both the speakers who are on the agenda and other participants to try to offer proposals for what they think can and should be done in this area in various ways as they see it. And they should feel free to label these as strawmen, things raised for purposes of discussion, anything that leaves them free to put ideas forward as to what they think might be useful approaches.

I'm going to try to get ahead of the agenda, since I'm sure we won't stay that way. And if Dr. Joksimovich is here, we'll begin.

MR. JOKSIMOVICH: Good morning. It is my privilege and pleasure to be here this morning, in my new capacity as manager of San Diego office for NUS Corporation, which is specializing in reliability and risk assessment.

CHAIRMAN OKRENT: I thought we had a prohibition against us --

MR. JOKSIMOVICH: Fleming challenged me to that last night, so I thought I should respond to the challenge. Like Fleming, I'd like to emphasize that my background in risk assessment has been at General Atomic, where, among other things, I

directed a risk assessment study named AIPA. Before that, in the field of risk assessment, I made some pioneering contributions in Great Britain where I worked together with people like Rech Farmer (ph) and his associates. My boss at NUS, who is Sol Levine, was going to be here this morning. Unfortunately he is prevented from attending by virtue of orders from his doctor. So I asked him whether he had any suggestions for my presentation, and he said, "Say whatever you want, but don't think that's a license for the future." So I intend to make the best I can today, because it may be my last opportunity to speak freely on the subject.

As a believer in historical perspectives, I like to start out by quoting some of the recommendations that I made to the Lewis Committee on the use of PRA in licensing over three and a half years ago. Among other things, my recommendations before the Lewis Committee in December '77 were that PRA should be employed in licensing because it provides balance of framework for assessing existing and proposed regulations.

I felt very strongly that it is a powerful decision-making tool via value/impact type of considerations. I was critical of the siting dose guidelines in -- 100, and I suggested they be replaced with some new approach which would recognize the risk of public injury.

I've also advocated the replacement of single failure criterion with reliability criteria for systems. Now, since

then, I've listened to a number of people who suggested that in addition to having reliability criteria for systems we may have to have reliability criteria for functions, and I go along with that.

I've also advocated that the level of acceptable risk be predefined, namely that safety goals be specified. I've also at the time, I said I was at General Atomic, and I was frustrated with the way ACGR was attempted to be licensed in the first two applications. So I felt that PRA should be used as a principal tool for evaluating advanced reactors like ACGR or LMPR. I also felt that the kind of methodology that we have developed in the ARP study for the selection of safety R&D can be used for water reactors or any other reactor system.

I advocate training programs, and the programs that I advocated were both for the operators to be trained on beyond design basis accident sequences. I also recommended training programs for the future PRA practitioners, recognizing shortage of qualified ones.

And I also advocated something like the procedures guide. With regard to the procedures guide, I have advocated that in the sense that Jack Hickman was talking about yesterday, namely that innovation under no circumstances should be stifled, that it shouldn't be overly prescriptive, and that it shouldn't be a check list for somebody to just use it as a way to assess PRA study in another plant.

And some of my analogies, maybe not very good ones, were that it's like, I don't know, the United Nations having representatives from hundred plus countries speaking hundred plus languages, and trying to communicate. To do so, they have to use five languages. And I feel much the same in this field, that there are a lot of individuals who have made individual contributions, and they're very specific to what these individuals perceived should be done. And I think we're reaching now the point that we have to streamline that into a number of approaches that can be deemed acceptable, like in the case of the common cause failures, would be recommending always the beta factor approach as an effective one, not necessarily the only approach but one that should be one of the approaches to be employed. And there are others viable, but we shouldn't have more than three or four.

Beyond the Lewis presentation, I delivered the presentation less than two months from TMI occurrence to the AIF workshop on safety and licensing. And in that paper, I have added some recommendations beyond those in the Lewis report presentation. I, for instance, suggested that PRA be used as a tool for selecting design basis accidents which would be then specified in Chapter 15 of SARs, replace the existing content of that section.

I advocated that they be used for technical specification requirements. And my bottom line was that we should be

using PRA for generating plant-specific PRA profiles, which then would contain the feedback mechanism from our operating experience when the plants start operating.

And in addition, since at that point in time defense of that type of concept was severely criticized by some critics, I felt that PRA provides a focus for that approach as opposed to undermines the way some people were suggesting last week at the NRC workshop at Harper's Ferry.

Now, where do we stand in 1981? From my narrow viewpoint, I believe that significant progress has been made in a number of areas. One of them is safety goals. I think that many viable proposals have been made, like ACRS's, like AIF's, like Sol Levine's, Ed's, and to be modest, like my own.

Last week, I felt that at Harper's Ferry we may not have moved forward; it perhaps was a move in the wrong direction. But nonetheless I'm still satisfied with the overall progress that's being made. I believe that a paper that was presented last week -- and George is here -- has plenty of room for significant improvement and needs to be revised before we can go any further. We had three workshops on this subject here, two of them sponsored by the NRC, and one of them sponsored by EPRI.

I suggest that somebody like the NRC sponsor a workshop inviting main proponents to argue their proposed approaches, and I believe that many confusing issues that people say are in my proposal, or Dave Okrent's, could be hopefully clarified that

way.

The second item, the procedures guide, I'm delighted that such an activity is in progress. I think it's a great thing just to initiate that. And I give credit to the NRC, and I understand also DOE and EPRI are now sponsoring, I give them credit for that. With regard to the quality, I reserve my judgments when I see the draft. And I'm one of the peer reviewers.

Plant-specific PRA profiles. I'm absolutely delighted with the progress that has been made in this respect, including IREP programs which have some deficiencies, but nonetheless I think the move is in the right direction. I am in particular delighted to see that utilities have initiated PRA studies, some of them voluntarily and some of them involuntarily. I'm particularly impressed with statements made by many utilties, including TVA and Commonwealth Edison, that they plan to PRA all their plants on a voluntary basis. So we talk about significant progress in this area.

Well, the areas where I don't perceive significant progress are listed on this viewgraph. With regard to the assessment of existing and proposed regulations, I'm not aware that very many regulations have been phased out. I believe that there is plenty of room for doing that.

I'm not impressed with value/impact type of considerations that I've seen for the new regulations. I was disturbed to hear that NRC might propose some design changes, irrespective

of any impact type of consideration, which reminds me of going back into the mode of operation of X years ago.

On the siting guidelines, I was very disappointed to see NUREG 0625. In my view it's a major retro-step.

With regard to replacement of single failure criterion, I haven't heard a viable proposal. There are some, I believe, we have developed at GA, but otherwise I haven't heard of any others.

And the same is true for selection of design basis accidents.

Now, with regard to selection of safety R&D, I think some progress has been made, but I still haven't seen a comprehensive rationale for why the public funds are used for funding all the safety R&D programs.

MR. KERR: I'm sorry. I'm not quite sure how PRA decides whether public or private funds should be used to sponsor research. Maybe you didn't mean to imply that.

MR. JOKSIMOVICH: I didn't. I think that's a very good issue. I'm talking about primarily NRC safety research budget. I think there is no other kind of rationale in that budget that I perceive one can come up with. Now, when it comes to private -- I think private budgets, I'm not aware how some organizations have handled that, using or not using PRA.

CHAIRMAN OKRENT: Since we're asking questions, and since you alluded to the existence of some GA proposal for

replacement of the single failure criterion, can you tell me where I can readily find that proposal?

MR. JOKSIMOVICH: I'll come to that. And Bill Houghton (ph) from General Atomic, who has pioneered this, is in the audience. And he may wish to make a statement on the subject.

CHAIRMAN OKRENT: Fine.

MR. JOKSIMOVICH: I'll just give him the introduction. And I'll use this viewgraph for that. This is a kind of approach that was developed at GA for how one can apply PRA to a conceptual design. So we're talking about the new plant, not existing one. The steps in the approach are that once we have some kind of conceptual design, something to work with, then we should perform both safety and investment risk studies.

The results of these studies should be compared with some pre-specified quantitative goals, both for public safety and investor risk considerations. From that, we move into the area where we go through a safety and investment optimization study for the concept.

The outcome of that is that we select the system design options that we believe represent the optimized plant, from both public safety and investor risk consideration. And the same time, we have to modify the PRA study to reflect these changes. And from there, we define, what are the licensing design basis accidents, so that they can be presented to the NRC.

And also, we define safety and reliability criteria for the plant which have to be observed by the designers.

Now, with regard to design basis events you may recall that I presented this viewgraph last year when I appeared before this committee on the subject of my proposal of -- safety goals. And when I talk about design basis events, I'm talking about events which are in the design basis region.

Now, this region can also be called prevention region, if you wish. And I've also allowed for the mitigation region, or design capability region, and hence both prevention and mitigation aspects are represented in the proposal that I -- that we developed for selection of design basis events.

Now, when it comes to safety and reliability criteria, that's a more complicated process. So I have a table here. Unfortunately, I don't have the viewgraph. And I'll just make a few statements. And then if you wish, Bill Houghton can expand on that. The way we have approached that is that we would select a particular system of safety significance, like core auxiliary cooling system. And then we would define responses of the plant, typically in terms of failure to start, failure to operate, each loop, and restoration of the function.

And then we would go through the event conditions for which we want this system to function, and we would list those. The next step would be to pick the system parameter. And typically, for the failure to start type of response, we're

talking about the failure probability. And then we would assign a reliability value to that.

When it comes to failure to operate, we would assign system parameters like failure rate, or common cause factor. And we would assign numerical values for that. When it comes to restoration of the function, we would talk about mean repair time for accessible components, and we will specify a number of hours.

And finally, we would talk about the probability of the failed components, whether they're accessible or not, and we would assign a probability that they're accessible.

I know it's difficult to follow that, so I think Houghton can expand on that. And if you want me to, I'll just finish what I have to say, and then Houghton can contribute.

I'd like to make a few observations on what I heard yesterday. I was very impressed with Consumers Power's approach on Big Rock Point. That's something that I was visualizing for many years, and some, I guess, of my dreams are coming true.

The second issue is with regard to perceived lack of emphasis on the uncertainty assessments in PRA studies. I'd like to make a statement that in the AIPA study we placed great emphasis on that. And we considered always that to be an extremely important part of the study. We have asssessed uncertainties, not only for probabilities but also for consequence. And I'm not aware that any other team has made an attempt to

assess uncertainties for the consequences.

If these uncertainties, by some people are deemed to be large, in my opinion they simply reflect the state of our knowledge. And blaming the tool for that is some kind of escapism.

With regard to the statement that PRA might be an art rather than a science, I would say that it's a combination of the two, and a very delicate combination. If it wasn't, I wouldn't be in it. That's one of the things that attracted me to PRA, that artistic element. There are a lot of artists in the part of the world where I came from, but not very many good engineers.

MR. KERR: Let me inquire as to whether you take the attitude of older artists or modern artists. It's my impression that older artists meant their art to be understood generally, and modern artists are insulted if their art is understood.

MR. JOKSIMOVICH: Well, I'm personally very conservative. I prefer in music Beethoven and Brahms to hard rock or whatever.

MR. KERR: Okay. If that's characteristic of your approach, I understand.

CHAIRMAN OKRENT: You're one of the stars in "Clockwork Orange", are you?

MR. JOKSIMOVICH: So with regard to a statement, I think that Frank made yesterday, providing I understood what he

said, the individual who can fix cars and TV sets in my view is not going to be necessarily a good PRA analyst. And probably the opposite, because he needs to have imagination. And I know that some of the people who can do that well don't have the kind of imagination.

And with regard to the strawman proposals for peer reviews that Dave was advocating, some people have claimed that I'm brave. So I guess to demonstrate that I will make a suggestion, and the suggestion would be that the peer reviewers should be certified PRA practitioners, in a similar manner like utilities people appoint people to the review boards these days, on the basis of their extensive track record in the field of PRA. I think that peer review is an extremely important process, and that people who have been in the field for many years and have credentials and credibility, I believe that those people should be doing those peer reviews.

I think that ANS, IEEE effort is moving that way. They have appointed a number of people to do the peer reviews. I guess I'll be modest and mention some names, like Norm Rasmussen, John Garrick, I was also there. So I believe that the process should be to certify some people with experience, and those people at least for the time being could provide this type of an elite, which would provide quality.

MR. KERR: Speaking of artists, have you seen the movie -- what is it, "The Over-the-Hill Gang", or something?

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MR. JOKSIMOVICH: No. I missed that.

CHAIRMAN OKRENT: Before we leave the subject of peer review, there is some kind of a trend in society toward making engineers legally responsible for the work they do. Would you have these certified practitioners legally responsible?

MR. JOKSIMOVICH: Well, I would, in the same manner like brain surgeons are accountable for what they're doing. However, I would not write the laws in such a manner that we can have malpractice suits every week. So I think the laws should be written in such a manner so that the laws don't abuse it.

CHAIRMAN OKRENT: My question was only half facetious. It seems to me that if we're going to do the kinds of things that you have been advocating, and use PRA very strongly in the decision-making process, somebody has to be willing to stand up and say, "I'll put my professional reputation behind this work."

MR. JOKSIMOVICH: I agree with that. I think we have to do that. And I think in particular it's important in the hearing. For instance, we're going to face Indian Point hearing, I understand. And that's going to be the hearing where PRA is going -- trial. I think it's very important that the witnesses there have credentials in the field, and to just -- you know, not go through a type of rigorous process of selecting the witnesses I think would be disservice to the field of PRA.

CHAIRMAN OKRENT: Well, if I can pursue this a bit,

it was suggested yesterday that even the most qualified peer reviewers, if they only spend a few days reviewing or even a few weeks, will have had a limited ability to review. If there's an obvious error that falls in their area of experience, they may catch it, but -- so what's your definition of peer review?

MR. JOKSIMOVICH: Well, I can tell you right now, as a consultant, I have a client who wants me to review a major study. And they're asking for cost estimates. And the one that I haven't provided yet but I will this week will be for about a month of my time, plus a month of the time of my collaborators. And I believe that with the kind of expertise that we've gained over the number of years that we're selective enough. And despite the fact that, I think Jack was saying yesterday that we end up with PRA studies, like FSAR, with all these multiple volumes, and it's true that the material is voluminous, but I believe that the experience of PRA practicioners can focus on the issues very quickly.

Like I understand from my former associate, Karl Fleming, that in Indian Point study, that for one of the units, the dominant contributor is wind, and for the other one it's fire. Well, it doesn't take much imagination -- so these are the new -- WASH-1400 didn't suggest anything of that description, so here's a new study, and it suggests new dominant contributors to risk.

And I'm going to focus on that. I'm going to focus and

see what are the validity of the assumptions which went into conclusion that wind is the dominant contributor, and also the fire. So it's not that, I think, tough to focus on the major issues.

Now, in the process of doing that, of course, I wouldn't be able to go into every detail of the fault tree and event tree and that kind of stuff, but we do have experience with that. I mentioned this, I think, in some of the meetings before, that the AIPA study that we did was duplicated by the Germans. And in the process of that duplication they spent something like 10 man years. And during those 10 man years, they replicated our event trees and fault trees. And I don't think that what they've done was worth it, because I think they simply were learning how to do things, and the end product is, they had about three or four objections to the way we've done it.

So I wouldn't go into those type of details in the reviews, but I would focus on the major issues, like dominant contributors.

Have I answered your question?

CHAIRMAN OKRENT: Well, you've given me an answer. Do you have other parts to your presentation?

MR. JOKSIMOVICH: No, I'm done. Thank you.

MR. KERR: Earlier on, you indicated some disappointment about the NUREG that has to do with siting. Could you give a little additional detail on what you had hoped, or what you

might have hoped could result?

MR. JOKSIMOVICH: Yes. I was hoping that after all these years -- I think when the first guideline came out, my memory holds, it was maybe something like '62, so here we are 20 years later, nearly. We're coming up with a document which basically is very qualitative in nature and suggests that we go back to remote siting type of considerations, and that we don't give credit to the plant design. And I don't think there's any evidence that I know of which suggests that this remote siting is so great. And in particular, it doesn't reduce significantly the number of latent cancers.

Like, I was involved in California with a study that we did for California utilities where we looked at the Sun Desert site, ill-fated Sun Desert project. And that was, I think, the best site there was in the country, to the best of my recollection, or if not the best, one of the best. There was nobody living around. But because of San Diego and Phoenix and Los Angeles in the background, I think we ended up with a significant number of latest cancers.

So from the standpoint of reduction of latent cancers, I don't think that remote siting is very helpful. Now, from the standpoint of reducing the number of early fatalities, yes, it would be. But in my opinion, there wouldn't be any early fatalities. I think that the source terms in WASH-1400 have been overstated, and I think that the source terms are much

lower in reality. So I think even if we do have the kind of accidents that have been presented in WASH-1400, there will be few if any early fatalities.

MR. KERR: Thank you.

CHAIRMAN OKRENT: Yes. There's a hand. Please identify yourself.

MR. HICKMAN: Jack Hickman, Sandia. I'd like to make two comments, one on the peer review process. I think Wayne is right that the selection of peer reviews is very important. And in the procedures guide effort, that was given a great deal of consideration. I don't know about the certification, but I do know that there needs to be, first of all, a recognition, to be a good peer reviewer, that first of all you need to be a peer. And so those have to be selected.

With respect to the peer review process, however, I think there's two things one has to treat. One has to do, as you have suggested, look at those accident sequences which have been identified as dominant, and those features, and try to convince yourself that these are in two valid sequences. And perhaps you can do that in a couple of months.

Now, what I don't believe you can do is look at those other things which, through oversight or whatever, might have been left out, or down in -- because of some assumption that may or may not be valid. And I think to dig in and do a review and that level -- and that's what one must do before -- when he

gets done, he says, "I now agree with the answer." I think that's a much larger effort.

MR. JOKSIMOVICH: I agree with Jack, but implicitly, I assumed that NRC would be reviewing these studies, and will be doing the kind of job they're doing now. I'm talking about an extra peer review that is necessary.

So, for instance, with Zion, Indian Point study, I still believe that Jack Hickman would be asked to review that for the NRC. So he won't be without a job.

At Brookhaven, they're also involved in the review of Limerick.

MR. KERR: Well, now, it begins to sound as if a peer review, the party's interested in, and assumes that the NRC will pick up the pieces.

MR. JOKSIMOVICH: That's right.

CHAIRMAN OKRENT: I guess --

MR. JOKSIMOVICH: And they have to come up with their own procedure, how to be selective in looking at the pieces that they're concerned about. I know Frank may have thought about that, what's the way to do that. And I think he advocated some procedures which have been written for the IREP, which I didn't quite understand yesterday. But obviously NRC is moving in the direction of designing something which is kind of a guideline how they're going to do their own reviews.

Am I right, Frank?

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MR. ROWSOME: I don't have a plan for the review of the licensee submittals. I do have a plan that is built into the IREP procedures that we have used to facilitate peer review, and it's based upon a mode of documentation called for in the report that enables the decoupling of the analysis of whether the model of the plant really represents the way the plant is built, that is, all of the assumptions in the PRA are also portrayed in conventional engineering language, so that someone who is not familiar with PRA methodology but who is familiar with the plant, is capable of establishing tne validity of the assumptions and to discuss the limitations of the modeling approximations, without necessarily being an expert in PRA.

So that the manpower allocated to consistency checks between the plant description in the report and the quantitative reliability models and event trees can be decoupled, and the expertise used most efficiently.

MR. JOKSIMOVICH: Then emphasis is here on education. I'm not suggesting that. I think the emphasis has to be on how selectively to review a voluminous document like Zion, Indian Point study. I believe that NRC should develop some guidelines how to do that.

CHAIRMAN OKRENT: Yes. I guess I myself find the definition and amplification of peer review that you gave not fully satisfying to me as a, let's say, member of the public. I can understand very well what it was you were saying you

were going to do, and it meets a certain kind of a need. And that's why I made the comment I did when you defined that particular approach.

I must say I have the feeling that there is a need for a much deeper kind of peer review. In fact, there is a need for a much deeper kind of PRA than we have had, if in fact PRA is to meet your aspirations, and deserve meeting them. It may meet them, and not deserve it. Now, let me give an example from just one small issue.

As you well know, recently cold repressurization of PWRs has become a subject of interest. There have been some quick studies done by staff of the NRC, by industry and so forth, arriving at some preliminary judgments on the likelihood of something occurring, and decisions that was okay for a year or so, or whatever -- could go on. I guess my own feeling is, this is a not unimportant question. And it warrants a rather detailed PRA, looking at all the factors that go into this, including where you might be wrong on material in the weld, and how this could affect things, and what kind of flaws might be there in different vessels, because there is not a generic situation, really, plus the -- over the whole range, and in fact in the end displaying what you don't know, the uncertainties, as well as what you think you do know.

And in the absence of that kind of a detailed original study, in fact it's hard to know what a peer review means, unless

the peer does the study. So you don't -- at least I haven't seen the original starting analysis in the degree of sophistication that I think is needed if you're really going to treat this as a serious business. And both the doer of the analysis of the certifier are, let's say, willing to be subject to legal accountability and so forth.

But that's only one issue --

MR. JOKSIMOVICH: But it's an idea issue for PRA application.

CHAIRMAN OKRENT: In fact it is. It's one of those that is subject to less uncertainty, in my opinion. You at least start with a basis of information which is substantially greater than for many of the things we have to address, like the one Alan Cornell mentioned yesterday. It's less complicated than that, and also you have more knowledge. But there still will be uncertainties.

If we don't somehow go into the thing in this kind of detail, both in the original analysis and, I guess, a number of times in the peer review, somewhat similar to what you said was done by the Germans for the AIPA study, maybe by people who are more skeptical than they were, or whatever it is --

MR. JOKSIMOVICH: Or more elective.

CHAIRMAN OKRENT: I don't know about more selective. Until we've done this, I think, a few times, I don't know if we'll know -- we know how good we are or where the questions are,

and so forth.

Now, I don't believe that if you've done this kind of detailed work, which I think would be a lot of effort, until you've done it a few times, it's hard to predict the outcome. But it could be that you have now a fairly substantial basis for what I'll call engineering judgment in the PRA sense, and you have really a better handle on, not only what are the uncertainties, but which are the important ones. And in fact it would mean that you've tackled not only what you know how to analyze, but what you don't know how, now, I think.

MR. JOKSIMOVICH: Well, I agree, Dave. I suggested that the German approach is not something I could recommend be replicated for every PRA study. But I think doing it several times by total independent group of people, like Americans and Europeans, or something, I think definitely is productive. But the Germans have also replicated the RSS as a part of doing their Deutscharidikastudie (ph). And they've done the same thing with regard to AIPA study. So I think a lot of that has already been done.

I think we can draw a lot from those experiences --

CHAIRMAN OKRENT: Right, but none of the studies that I have seen published so far looked at the cold overpressurization event, and none of them considered that there might be a factor of 2 error in predicting something, et cetera.

MR. JOKSIMOVICH: That's very true. I haven't seen

that, either.

MR. KERR: I guess I also have some concern about an attitud on the part of any of us that says, "We will sort of leave the final responsibility for review to the NRC." It seems co me that that attitude may have made some sense in a day when it was assumed that some of the NRC rules were not physically plausible, and so you have to go through the process, but they have very little to do with safety.

To me, one of the advantages of PRA over the previous approach is that I think it can permit one to get closer to a treatment of physical reality. It therefore begins to have some implications for determining whether the plant is safe or not. And if it can do that, then it seems to me it is something the owner and operator wants very much to have done accurately, and not leave to the NRC.

MR. JOKSIMOVICH: Well, I strongly advocate heavy utility participation in the PRA study. And there have been a number of examples already where -- well, as a matter of fact, I can go beyond that, and I can say that no utility that I have talked to would let just consultants do the PRA study. They want to be involved. It's the degree of their involvement which varies these days.

So I think that the utilities have recognized that, and it's just a matter of having rather limited resources right now. But I see that their involvement in PRA studies is going

to grow. And they are obviously ultimately the ones who are going to be the beneficiary of the approach.

And so while utilities will be looking over the shoulders of the -- I think the consultants, by the way, have to continue doing this for a number of years, because that's where the most talent right now is. And I think the utilities will be gradually coming on board and taking over that, and I think several years from now I think they will be probably capable of doing their own PRA studies.

By the way, I don't think there is a TVA representative in the room, but -- yes. Sorry. You may wish to expand on that, but it's my understanding that TVA's policy is that Brown's Ferry study would be done by and large by the consultants, with heavy TVA participation. And beyond that, their policy is, they're going to be on their own. Is that correct?

MR. MONAHAN: (From audience.) John Monahan, TVA. That's correct. We hired Pickard, Lowe & Garrick to help us with the Brown's Ferry PRA. And we intend to, or we hope to continue to do PRA on --

CHAIRMAN OKRENT: Yes.

MR. HOUGHTON: I'm Bill Houghton of General Atomic. I would like to give a brief discussion after lunch during the general discussion. Perhaps that would be a better time for me to incorporate comments wherein my name was used previously.

CHAIRMAN OKRENT: Sure.

Questions for Dr. Joksimovich? Well, if not, we'll stay a little bit ahead of the agenda. We're in good shape.

Thank you.

MR. JOKSIMOVICH: Thank you.

CHAIRMAN OKRENT: The next speaker is Mr. Bitter from General Electric. I see we have another speaker.

MR. HILL: My name is Richard Hill from General Electric. Dave Bitter was unable to be here this morning, a case of strep throat. You wouldn't have been able to hear him if he had appeared.

MR. KERR: I have to gather from what I've heard this morning that the life of PRA analysts must be rather stressful.

MR. HILL: I guess from time to time we all undergo some stress.

MR. MARK: Is it only in the throat?

MR. HILL: Excuse me? I didn't hear you.

Again, my name is Richard Hill, and I work for General Electric. And I want to discuss some of our views on PRA, and safety goals, as they apply in the licensing area. An overview or an outline of what I have to discuss today, use of PRA analysis, evaluations in our BWR/6 design, and the use of PRA in design improvements we've made, not only are looking at in the BWR/6 design but also in previous product lines, the PRA applications in the licensing process, as well as

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improvements we feel could be made in that application.

An evaluation of our PWR/6 Mark III design, the approach we've used -- and I should say that this evaluation is ongoing right now, it's not complete, but it's similar to the WASH-1400 study, that type of methodology. The analysis of the Mark III is looking at, as it shows there, both core damage as well as the total risk.

And we're going to use that analysis to look for design improvements, potential improvements, and we'll do a reanalysis, if you will, for the delta, or difference in the core damage probability, as well as total risk. I believe some of the results of this were presented previously to the ACRS in May. And we have this program scheduled for completion early in 1982.

Some of the same type of things have been looked at, as far as design improvements are concerned, for the total product lines, methodology for identification of improvements, as stated here, is basically to take a look at the reliability of the systems, and the design, the way it is, identify potential improvements, make those improvements basically in the system models, and reanalyze to find out what resultant risk reductions there are.

The planned standard BWR/6 improvements are listed here, and that includes the improved ADS logic, and that is the logic to take care of steam line breaks outside of containment

type of thing that was identified by a bulletin and orders task force, increased RCIC reliability, alternate decay heat removal, which we're looking at as being a containment over-pressure relief type system, and ATWS Brown's Ferry modifications.

To give you a feel for the results, looking at those, these are examples, preliminary results. Across the bottom of the events that we're looking at, from loss of feedwater, stuck-open relief valve, loss of off-site power, various break sizes, ATWS, and loss of heat removal. And you can see the before modification and after modification levels of risk. And you can see especially the large risk reduction in the ATWS area.

Application of PRA in the licensing process, we see both benefits and potential problems. In the way of benefits, as it shows there, it's really -- can be a useful tool in decision-making. And we've listed two areas of decision-making underneath that, and -- providing a basis for rule change decisions as well as assessing backfitting of requirements. And we feel that this ability to lend more definitive nature in decision-making processes is the real benefit of the PRA.

Potential problems, as listed here, is that if it becomes a requirement, it could be a real resource limitation in trying to do all of the PRA analyses. Right now there is a lack of consistent PRA scope. Some only go to core melt, some others go out to total risk, and calculate the consequence models. And there seems to be a lack of methodology, consistency, and

right now the IEEE and ANS groups are working to come up with a standard in that area.

There's a lack of quantitative safety goals. And right now OPE is working in their workshops -- they just finished last week a workshop discussing safety goals, and I'll touch on that a little bit later -- as well as if -- with these other potential problems, that all could kind of cascade into potential licensing delays, which in itself is a problem.

Basically, our conclusion is that the PRA is really a good tool for decision-making, and helping to do that. It's not necessarily a good thing if it's put as a requirement.

CHAIRMAN OKRENT: Could you put that back a minute, please?

MR. HILL: Certainly.

MR. MARK: Why do you suppose that PRA will lead to an extension of licensing delays?

MR. HILL: In the process of trying to uniquely evaluate each one of the PRAs, if it's a requirement for each utility, the lack of resources, as well as some of these other things that I've mentioned up above here, could lead to delays. We're not saying it's a probability of one that we're going to have delays, but just in trying to review a PRA as a requirement for a license -- could very easily lead to delays.

MR. MARK: That might, I suppose, be true, in some short range. But I would have, myself, hoped that once you'd

done a PRA on the BWR/6, that the utility doesn't have to do the same thing over again, nor does the staff have to look at the same thing again.

MR. HILL: I would agree that we would encourage generic PRAs, if you will, or product line type of PRAs, however, not every plant within a particular product line, like BWR/6, is identical. When I mention requirement there, I'm looking at a plant-unique requirement, where each licensee would be required to have his own PRA.

Again, I'm not saying it's bad to have -- we would support any utility that would want to have their own PRA, and that's probably a good thing for them to have. And I missed some of the earlier discussion here, but I'm sure you've touched on that. But in the realm of a requirement for license, where a review has to be completed and approved, then I think that we're in an area where manpower and other decision-making tools that are not available right now consistently would cause -- could cause delays.

MR. MARK: I wonder if Mr. Thadani could comment on the thoughts he would have.

Supposing you have a plant with a BWR/6. And supposing you have reviewed a PRA for things specific to that -- to the NSSS, I guess that would be. Each plant is different, but they're not different in all respects. Would it be imaginable that NRR would use some of these things by reference?

MR. THADANI: Dr. Mark, obviously, at least I would hope, myself, that there would be no need to repeat some of the analyses, if indeed the analyses are applicable, to, let's say a similar design located somewhere else. I suspect that the reality would be somewhat different, because of a number of factors.

Certainly the external environmental effects would be different. The power supplies, the role of the architect-engineer would play, in my opinion, a very signicant role in terms of risk assessment to the plant. NSSS supply -- in terms of -- let me go through some examples. I think that might be a little easier to get into.

Let's say that Westinghouse designs auxiliary feedwater systems. Let's say that they have typically three trend system, and let's say there is a standard plant with certain power rating, associated aux feed trends, flows, et cetera. Presumably success-failure criteria are specified. And I would expect similar criteria would apply to a plant located elsewhere.

An area where I'm not so sure one can apply the same PRA, if I may use reliability analysis of the auxiliary feedwater system, would be how many diesel generators are available, in the event of a loss of off-site power event? Is there a backup power supply to diesel generators? What DC power supplies you have? It's just not clear to me at the outset that General Electric performing some risk analysis for a BWR/6 product line

would be applicable to all, or even a large number of BWR/6 plants which may be built.

CHAIRMAN OKRENT: If I could ask one or two questions that arise from this viewgraph, under potential problems, you didn't list uncertainties. Do you think there's a problem there?

MR. HILL: Well, I believe the treatment of uncertainties is with the lack of consistent methodology. Right now there may not be a consistent treatment of uncertainties. I do believe that until we decide -- "we", industry and NRC, decide on how uncertainties ought to be treated, and what's acceptable methodology, acceptable assumptions, et cetera, I believe that fits into that category.

Those are the things that need to be, at least in some sort of consistent manner, addressed, before we -- probably is a good thing to do -- you know, if the NRC is involved in making a requirement out of this, that would have to be addressed.

CHAIRMAN OKRENT: Do you think that when the IEEE, ANS committee has their methodology, we'll then have the uncertainties in hand, or what?

MR. HILL: I doubt that we'll have the uncertainties in hand. I would hope that we would have the methodology by which we could look at as a -- some sort of base line, and make judgments from. In the area of probabilistic risk assessment, I'm not sure that we'll ever totally have the uncertainties in hand. Maybe you have a different concept of what "in hand" means

than I do, but there are always going to be uncertainties in your various assumptions that you make.

CHAIRMAN OKRENT: I suspect I may have a different concept.

Dr. Kerr, do you have a question on this point?

MR. KERR: Well, I interpreted your question to mean, how one dealt with the uncertainties, and not with the methodology used to treat the uncertainties. And I agree, the methodology is a problem, but it seems to me a greater problem may be the fact that the uncertainties exist. And I think almost any methodology is going to demonstrate that they do.

And they exist in precisely the area in which they're crucial, the high consequence area. And it would seem to me that this is difficult.

MR. HILL: I agree.

CHAIRMAN OKRENT: You didn't tell us in your presentation what you would consider to be adequate peer review. No reason why you should, but as you can tell, I'm interested in the question. What do you feel would constitute adequate peer review within General Electric, and then after you've done your generic PRA?

MR. HILL: An adequate peer review of our, for instance, BWR/6 assessment --

CHAIRMAN OKRENT: Well, of a product that you would put out, that's a PRA.

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MR. HILL: I would consider adequate peer review being the design process that General Electric goes through in its own design review methods, including the review and internal approval of our own risk assessments.

CHAIRMAN OKRENT: Well, in the past, the design approach that has been used from time to time has been not trouble-free. I mean, we've had a lot of interest, for example, in dynamic forces in pressure suppression systems, and steam relief systems, for example. So I'm trying to see better how General Electric thinks the process of peer review should be done, and to what standard it should strive, and what responsibilities should be accepted by those who advance a PRA as the basis for decision-making. Because you're saying in fact that you would do that.

MR. HILL: PRA is not the only tool, obviously, for decision-making --

CHAIRMAN OKRENT: But it's being proposed as an important tool.

MR. HILL: Yes. And I do believe that the standard design practices that General Electric uses would be those practices by which we would arrive at a particular design, and then, if you will, maybe, in your language, the peer review would be -- the PRA risk assessment would be a complement to the design, and looking for various improvements in that design, especially if that reliability work is done in conjunction with the development of the design.

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CHAIRMAN OKRENT: I'll leave it as an open question.

MR. HILL: Maybe I'm not truly understanding the question, but I believe there's a lot of philosophy in the question that it's not easy to nail down in a single answer.

CHAIRMAN OKRENT: Well, let me just make one comment. I have a little concern with the proposal, which I well understand why it's made, that we should not have regulations that require PRAs. We should use PRA as a decision-making tool. And the concern goes like this. I can envisage a situation where subconsciously or consciously PRA is advanced selectively for decision-making. People advance it where it supports what they want to achieve. They think, "Here's a weak area. If I do PRA --" weak from my point of view. In other words, the regulation that shouldn't exist, or somebody may say, "It's a weak safety area", either way.

And they'll use PRA only to advance their point of view, but not be willing to do it comprehensively, because comprehensively it may turn up some things that go against my current point of view, or whatever I'm advocating.

Do you have any concern that way?

MR. HILL: The problem you've stated is definitely a potential problem within industry or the NRC.

CHAIRMAN OKRENT: Yes, indeed.

MR. HILL: In trying to use analysis to show your own point of view. The root to that problem I guess lies in human

nature, of trying to get the designers and the regulators to look at the problem beyond their own interests, and look at it very open-mindedly for both the benefits of a particular change as well as the negatives of that change.

And, yes, there could be a problem there. I think that problem, though, will be checked and balanced as we deal with various other organizations within industry and the NRC in any particular design change.

CHAIRMAN OKRENT: Maybe.

MR. HILL: Continuing, improvements in the application of PRA that we see, that, as we've discussed, need to have a standardized methodology in a uniform application. And this would include a consistent -- or a safety goal that would provide a consistent measuring stick for the PRA results that are -- that you've calculated.

The basic elements that we see that have surfaced in all the safety goal proposals to date are those three right there, the individual health, societal health effects, as well as core damage probability. And as we look at these major elements, we see that they're actually very similar --

MR. KERR: Excuse me. Before you go to the next slide, you referred to the need for standard methodology for uniform application. I'm trying to get an understanding of what you mean by standard. For example, do you think pressure vessel design is done by using a standard methodology in the sense in which

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you made it here?

MR. HILL: No. That's the sense in which I mean -- standard methodology here is a standard or an acceptable set of assumptions, modeling assumptions, as far as systems are concerned. I guess I would go back to all the various uncertainties, if you will, those type of things --

MR. KERR: I'm trying to draw an analogy. In your view, is pressure vessel design, as it is done by various people who sell pressure vessels and design them, done using a standard methodology?

MR. HILL: ASME Code, yes.

MR. KERR: So that you would like to see something like an ASME Code if it could be assembled?

MR. HILL: Something of that nature. I believe that's the area in which the IEEE, ANS organization --

MR. KERR: Well, I'm puzzled by this, because I keep hearing of an IEEE, ANS effort. My impression before this meeting was that IEEE was going to work on safety goals, and that ANS was going to work on methodology, and the two activities seemed to be mingled. But that's probably not a question for us to settle at this point.

MR. SIESS: I got the impression from your response to Dr. Kerr that you think that the standardization may turn out to be something like Division 3 of ASME Code. That's a third party type of agreement, a voluntary standard type thing.

What's the possibility that it'll turn out to be something like Appendix K, which is also a sort of standardized assumption, standardized methodology, which, if followed, is an acceptable procedure, but quite a bit different than the ASME Code?

MR. HILL: Well, the difference between those two, outside of obviously the technical differences are, one is a law, or one is a rule, and the other one is voluntary, as you mentioned.

MR. SIESS: That's a big difference.

MR. HILL: We would hope that it would be a voluntary type of standard, more as the ASME, rather than having to take something of that and put it into a rule-making type of process. I don't see any particular need for that. When you make anything into a rule, then you've obviously locked the technology into the law. And in this area specifically where technology is changing quite a bit, it would really, I think, be a disservice to the practitioners of PRA to have the methodology locked that tightly.

MR. SIESS: Sounds very much like the triumph of hope over reason.

MR. HILL: This next chart is just here to point out the various proposals for safety goals. And the point that I'm headed towards is really the safety goal elements that have been proposed are actually very consistent. And as you can see, there are various categories; core melt and mitigation systems, as well as societal, individual health effects, latent early, cost-benefit type of proposals. And the little circles there

indicate which proposal has which qualities. And you can study that maybe a little more thoroughly. But -- that'll fit horizontally.

What I'm intending to show here is how those various proposals group in each category. The top category there is core melt, in other words the core damage type of goal. And as you see, that that groups around, and this is plus or minus a decade, basically, it groups around the 10^{-4} number.

The individual groups here right in this area -- and if you come over to the societal risk, you're at the one, death -- this is at the 10^{-5}. And these happen to be the particular numbers that the AIF proposed in their goal, and also happen to be the numbers that were used at the OPE workshop last week.

The point here is that really, although there are several safety goals being proposed, all the proposals are relatively uniform in these three. And as the last chart showed, those are the three that seem to be consistent through most of the proposals, as a general consensus.

CHAIRMAN OKRENT: Can I make a comment here?

MR. HILL: Yes.

CHAIRMAN OKRENT: I think the comparison that you just made, which I've seen made before, suggests a similarity which I don't feel exists. I'll speak for what is in NUREG 0739. And I guess speaking for myself, I look at it as a package. And to have, for example, the goals on individual and societal risk

39 ? without an alara would be only half a package, in my opinion. And so to say, for example, that the goals on individual and societal risk appear to be in the same ball park, among all of them, and therefore these are similar, and these words have been used here, to me is in fact not a real representation of the situation.

I think a proposal that does not have an alara has a different philosophy in it, even though it seems to be very similar where the two are the same.

Similarly, the AIF proposal, which has an alara, does not have any requirement on mitigation. Again, I think that's a basic philosophic difference in the proposal, in NUREG 0730. It proposes that both prevention and mitigation have goals. So let me suggest that when people say that these approaches are similar, in the future, that they make the point that there are fundamental differences. I have seen comparisons on slides and in books that say that these are very similar in their ways of doing it. I find, myself, important differences, very important differences.

MR. HILL: You make a good point, Dr. Okrent. And I do acknowledge the fact that there are fundamental differences in the philosophy in which the various safety goals were developed. However, if you look at them from a quantitative nature or quantitative view, and those three major areas, they do become similar quantitatively. but you make the point very well that

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they're different fundamental philosophies.

CHAIRMAN OKRENT: Dr. Griesmeyer would like to comment, if he can reach the microphone.

MR. GRIESMEYER: Again, when you make comparisons about the numerical values in the three categories that seem to be common, you have to remember that until you specify how you're going to calculate the numbers and in what context you're going to interpret them, and how you're going to treat uncertainties, the numbers are not similar at all. Because without a context, the numbers don't mean anything.

MR. HILL: In general, however, if -- and this is one of the points made earlier. Once you standardize, if I may use that word, the context of the way in which you calculate the numbers, the numbers that are specified in the goals are generally the same measuring stick for that number.

And you're right, if you calculate the number differently for a different goal, you could have a different measuring stick.

CHAIRMAN OKRENT: Let me elaborate on it, because I'm very glad Dr. Griesmeyer made the point. We've been talking about uncertainties and so forth, and for example, how one brings uncertainties into a calculation of the mean value. It can have a big effect on what answer you get. And whether or not you exclude some accident sources from the calculation can have a very big effect. In other words, if you do it without

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including natural events and sabotage, for example, you might have a rather different conclusion than if they were included.

So this question of how you treat uncertainties, and the context in which you're doing the calculation, again may represent a difference. We tried to make, I think, the point that uncertainties were to be treated fully, and all accident scenarios were to be considered in the suggestion that we proposed for discussion. It's not clear to me, and some of the others, just what the intent is.

MR. KERR: I would suggest that one put the emphasis on "similar", which I think is the word you used, rather than "identical".

MR. HILL: Yes.

In summary, I guess the points that I would like to have you take out of the presentation today, from our point of view, is that we do believe PRA can be a useful tool in design evaluation of nuclear power plants. And it can also be useful in analysis of various safety improvements that you'd want to make. We don't believe that PRA should be a regulation or required for every plant.

We do believe that PRA can be used very effectively in decision-making. And the last bullet there is really, methodology development of the safety goal formulation and application in the licensing process should proceed in parallel.

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All of these things should be developed on the same time frame.

That completes my formal presentation. Any further questions?

MR. KERR: We have raised this question earlier, but let me pursue it a little more. Your slide indicates that the results of PRA should be used in the decision-making process. If I put myself in the position of people responsible for regulating and licensing nuclear power plants, this means to me that they must take the results of somebody's analysis and use that to judge whether a plant is licensable and whether it should continue to be operated.

Now, they may not require that one do a PRA to get those results, but if they require that the results be available in order that a decision be made, what is the difference between that and requiring that a PRA be done?

MR. HILL: Dr. Kerr, I might have missed just the first part of your question, because, in shuffling my paper here -- I apologize. The first part -- somehow you arrived at the fact that the results were required.

MR. KERR: Well, you said that they should be used to make decisions. Presumably this means decisions by, for example, a regulator.

MR. HILL: I see.

MR. KERR: I guess. I mean, I don't want to misquote you. I assume that --

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MR. HILL: No, I think that's a good point of clarification. I was coming from the standpoint that vendors or architect-engineers, utilities could use the results in decision-making tools as how to best optimize the design for safety, in that area. I wasn't using them in the context that you would regulate the plant by the results of the PRA.

MR. KERR: But having reached this conclusion, it seems to me it has to be demonstrated to the organization responsible for regulation, as a valid conclusion. And if PRA is useful to the person making the initial decision, it should be useful to others who have to make decision, or would you anticipate that the NRC, for example, would use an entirely different decision-making process?

MR. HILL: I wouldn't anticipate necessarily they'd use a different decision-making process, however, I do believe that from a vendor's point of view, as we look, for instance, at the BWR/6, we could do a standard generic type of PRA that would fulfill our purposes in the way of results for optimizing the design, which may not fill, as Mr. Thadani said, his purpose --

MR. KERR: No, I -- I thought you were referring to people who were going in for licensing and were saying that they shouldn't be required to do PRAs.

MR. HILL: True.

MR. KERR: And yet they should use the results of PRAs to make decisions. And my question is, how is one to

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demonstrate that a decision is valid, unless he can demonstrate that the results of the PRA have some general validity?

MR. HILL: I think the difference there is, I changed the subject of the sentence. In one case, those that have done the PRA for the purpose of design work or evaluation of a design, which does not necessarily mean each utility or each licensee, where those going in for regulation would be each licensee. Maybe I'm missing your point, Dr. Kerr. I don't --

MR. KERR: Well, I think even at present, although in a sense NRC requires that each utility prepare safety analysis report, much of the contents is identical with other contents. So in that sense, although there is a requirement that all this paper be assembled, it isn't, in practice, a requirement that each person do a safety analysis de novo, but rather than in many cases much of what has been developed previously is used.

Now, it would seem to me that if a PRA is needed in those situations in which they can be identical, one can use the results of previous work.

MR. HILL: I agree. And in doing so, it is possible to look generically at a group of plants. And that information, as you say, if it's useful to the designer, it could be useful to the regulator also.

CHAIRMAN OKRENT: Dr. Joksimovich.

MR. JOKSIMOVICH: I'd like to endorse what Dr. Okrent said about lack of similarity in the various approaches proposed.

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And that led me to my proposal this morning that we get together and debate the thing so that we can tell the rest of the world that there might be some similarities but there are some gross dissimilarities, and then we put those things in perspective.

CHAIRMAN OKRENT: Yes. And I think certainly your proposal is one that is quite different from the others, in that you have a limit line kind rather than numbers.

MR. JOKSIMOVICH: I think that's a very fundamental difference.

CHAIRMAN OKRENT: I agree.

Mr. Temmy.

MR. TEMMY: I'm Mark Temmy from General Electric Advanced Reactor Systems. And there are two points of the discussion just past that I wanted to respond to briefly. The first is the apparent confusion over which technical society is doing what in this business that Dr. Kerr expressed. And I feel that perhaps I've contributed to his confusion myself. So let me attempt to clear things up, hopefully not make them worse.

The joint ANS, IEEE effort which has been mentioned a number of times yesterday and today to produce a document which describes some sort of standard methodology for carrying out PRA is an ad hoc activity which is sponsored in part by NRC. And in fact, NRC has given grants to both organizations to carry this out. DOE is also a financial contributor. And

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the whole activity is outside of the standards organizations of both societies.

It has a fairly well defined purpose, with a specific schedule that's unusual with respect to the schedules under which standards-writing groups operate. So it's not an organization, it's not an activity that's within the framework of the standards-writing groups in either ANS or IEEE.

In ANS, there is at least on the organization charts of the standards groups a group that has the charter to write a standard that describes PRA methodology. That is an inactive group, but it exists on the books. It has a scope.

In IEEE, there is a working group in operation which I chair which is focused on writing a standard which expresses safety goals. And there's been discussion of coordination between these two groups.

There also happens to be in ANS a standards working group to wrote quantitative safety goals for breeder reactors.

Does that help or hinder, Dr. Kerr?

MR. KERR: Well, I have more information now on which to misunderstand it than I did before.

MR. TEMMY: Well, then I'll get on to the other point I wanted to respond to, and that's the question, the point that came up about the similarity of numerical goals. And I wanted to express a personal reaction that I have to some of this. I have seen and heard statements to the effect that although they

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are arrived at by vastly different forms of logic and rationale, the conclusions, namely the numbers stated by various groups and individuals, are remarkably similar.

Personally, I take little or no comfort in that kind of statement, and in fact quite the contrary. I find it a very disturbing statement. It leads me to wonder to what the common cause failure is there. And one of the important reasons that I feel that way is exactly what Mike Griesmeyer stated. So I don't think we should feel content and satisfied because if we plot all the numbers proposed on the same graph they all fall within some narrow range. It's been suggested to me, and I certainly haven't confirmed it, that that may just be a reflection that everybody's qucting WASH-1400, for example.

That's all I wanted to say on that point.

MR. KERR: Mark, it may go back to the old story that they used to tell about, what is an engineer, in the days when there were slide rules. An engineer is a guy who takes a slide rule and says, "Two times two is 3.98 -- oh, hell, call it four." These goals may not have much to do with the logic that was used to arrive at them.

CHAIRMAN OKRENT: Well, thank you.

We're at the point on the agenda where it calls for a break. We'll reconvene at 10:15.

(Brief recess.)

CHAIRMAN OKRENT: The meeting will come to order.

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If Dr. O'Donnell is here, we'll begin.

MR. O'DONNELL: Good morning, Dr. Okrent, members of the subcommittee. My name is Ed O'Donnell. I'm a Division Vice-president with Ebasco Services, Inc. And I also serve as the Chairman of the Atomic Industrial Forum Subcommittee on PRA. The subcommittee is composed of representatives of utilities, A-Es, all four reactor vendors, and many of the consultants that you've heard during the session.

I'm here in that capacity today as Chairman of the AIF Subcommittee to give you our views, our thinking on the use of PRA in quantitative safety goals in the regulatory process. We believe that these are very important issues and are pleased that the ACRS Subcommittee has seen fit to undertake this inquiry.

Much of the discussion regarding PRA to date has focused on numbers, quantitative safety goals and methodology. These issues are also important. But it appears to us that very little thought or discussion has been given to how the NRC would actually use these tools in formulating its regulatory decisions.

We do endorse the use of PRA as a tool by utilities, and I was very impressed, as others were, with the steps taken by Consumers Power in applying PRA as an internal management tool to Big Rock Point. And we endorse and encourage utilities as well as A-Es and NSSS vendors to use these tools in determining need for modifying designs.

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However, the question of how the NRC should use these tools is quite different and requires much more focusing and discussion. The Atomic Industrial Forum's views on these issues is spelled out basically in two documents. On June 2nd of last year, we sent a letter to Harold Denton that detailed our views in general on how PRA should be used in licensing, and gave some overall thoughts on establishing of quantitative safety goals.

In May of this year, we issued a policy statement that dealt with the establishment and use of quantitative safety goals in the regulatory process, and detailed, in some degree, our thoughts on how the decision-making process should be formulated. And in my presentation this morning I will summarize and hopefully elaborate on these views in these two documents.

I'd like to start off with a general discussion of the considerations that should be taken into account in using PRA in the regulatory process. And these basically come out of our June 2nd, 1980 letter. Number one, we believe that PRA should be used to support and not supplant deterministic requirements. That is, we do not envision a regulatory regime in which the current deterministic approach would be totally withdrawn and supplanted by, let's say a set of numbers or statements of acceptable risk, or quantitative safety goals.

The general design criteria we believe should remain in place and we should adopt PRA as a basis for justifying the

need for change in those requirements. And that cuts both ways. That is, in terms of justifying additional requirements, or if someone proposes to reduce the existing requirements, that PRA should be part of the underlying justification for any change.

In doing PRAs, we feel that it must be done as realistically as possible. And the degree of uncertainty and conservatism in the results should be very explicitly stated. We feel this is necessary to avoid any bias towards looking at sequences that appear to be major risk contributors in that they are high consequence events but may be very low probability events.

We feel it is essential for introducing PRA in this process that we have quantitative safety goals to be used as the basis for evaluating whether or not a decision should be made if PRA is being introduced as part of that decision-making process. And lastly, we feel that it's necessary that both the NRC and the industry are using a common set of rules in doing these assessments.

MR. KERR: Excuse me, Ed. I'm not quite sure that I understand the distinction between bullet two and the next to the last one in which PRAs shouldn't be used as a licensing condition, but it is apparently to be used for PRA-based decision-making. What --

MR. O'DONNELL: Yes. I'm sorry. I must have skipped over bullet two. Bullet two is basically that we do not feel at this time that PRA should be introduced in individual

licensing cases. That is, that an applicant for a CP or an OL should be required, in addition to meeting the deterministic requirements in place that he somehow come forth with a PRA that demonstrates that his plant is safe.

We feel that the introduction of PRAs should be done on a gradual basis, and as generically as possible. That is, the NRC should use PRA, at least initially, in determining whether these existing requirements are in fact adequate with respect to some safety goal.

That is, if one complies with the GDC, does that deliver a level of safety that is adequate for protecting the public health and safety? The onus should not be on an individual applicant to demonstrate de novo that his plant in fact is safe. We feel that's an undue burden. As a very practical matter, neither the NRC staff nor the industry has the resources currently to do this. Number two, we do not have in place the last two items on this list; that is, the decision criteria that one would use in making a decision in a specific case, nor do we have agreement on what the methodology would be if one were to use PRA.

And I think bullet two just basically reflects what we're doing, that the NRC is in fact continuing to issue licenses, the ACRS is continuing to support those license issuances, without having to do a PRA as part of a license application.

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MR. KERR: What is the PRA-based decision-making then that is referred to in the next to last bullet?

MR. O'DONNELL: Well, I think it's primarily generic, that is, if the NRC proposes to change a rule, we are in bullet three saying, "Well, PRAs should be part of that justification." And in making that decision on whether there is a need for a rule change that we should have some decision criteria. So I'm divorcing --

MR. KERR: So you're suggesting that PRAs should be made to make generic decisions, but not individual decisions?

MR. O'DONNELL: Well, I'll get into that in more detail, but I think the primary application is in a generic sense.

CHAIRMAN OKRENT: I wonder whether you're sort of suggesting it's okay for the NRC to devote a lot of effort using PRA to evaluate its requirements, past and proposed future ones, but you're not willing to propose that licensees use resources to evaluate their existing plants and plants that are under construction, using PRA to see if there are things that it would be prudent to fix.

MR. O'DONNELL: No, on the contrary, I am endorsing the use of PRA by utilities. And in fact, much of the work that's going on at present that I will refer to later is in fact work that is being supported either by individual utilities or by groups such as NSAC or EPRI.

What I'm suggesting is that we do not at this time

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introduce PRA directly into a decision on a specific application regarding whether ot not a license should be issued. That's quite apart from the question of whether we need to do better. And I think what I'm saying is very consistent with what's going on right now and what is being -- the course that is being pursued both by the NRC staff and being supported by the ACRS. That is, licenses are being issued without the need to do a detailed PRA as part of the licensing application.

CHAIRMAN OKRENT: But the proposal is that it be used generically. I keep hearing these terms, and not on a plant-specific basis, when the NRC is doing decision-making. And it's actual plants that are running --

MR. O'DONNELL: Yes.

CHAIRMAN OKRENT: -- and actual plants that pose safety questions, and as we well know, that have differences from generic studies. So I'm still trying in my own mind to understand whether in the package you're proposing there is a sufficient emphasis on using PRA to look at specific plants.

MR. O'DONNELL: Yes. Well, I hope I will get into this in my presentation, but when I say generic studies, obviously one cannot do a -- one must have a specific design to do a PRA. I guess what I'm saying is that the NRC should try and draw generic conclusions from a sample of plant-specific PRAs, if that's possible. And I think it is possible. I'll get into this a little bit later.

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With respect to the specific applications of PRA in the regulatory process, we see a number of areas that could most benefit from this. In doing value/impact analyses for introducing new requirements, it appears to us that PRA is a very important component of that and could hopefully give us a more global aspect on what the value is of any new requirement.

Many of the value/impact assessments that have been done in the past have focused very narrowly on what the value or impact on the NRC staff is of some new requirement. And a broader view of this would certainly tend to look at the value in terms of risk reduction and the impact in terms of cost of some new requirement.

In prioritizing and resolving existing and future generic unresolved safety issues, we feel that PRA can be used to great effect and in fact has been in reducing what was once the list of 133 open items down to 14, in our view a very effective use of ranking and prioritizing these issues.

In the generic rule-makings, we feel that PRA and safety goals are a very essential part of resolving these issues and making decisions, again on a generic basis.

Establishing priorities for safety research, again, by looking at the things that are on the table, and from a PRA standpoint, one can determine where the research dollar could best be spent.

Determining need for backfitting. Now, this gets

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into a plant-specific application. And in particular on those plants that are included in the systematic evaluation program, we feel that the use of PRA can lead to better decision-making than if one merely goes through the standard review plan and lines up all the deficiencies of those plants against what's in the standard review plan.

In determining need for plant shutdown orders, when new safety issues arise, again PRA we believe can serve a very useful function here. And the Commission's order to shut down five plants in early 1979 because of a discovered problem in a seismic analysis comes to mind here. And one would feel that if that issue had been looked at from a PRA standpoint, the outcome of that decision might have been quite different.

In establishing technical specification requirements on LCOs and testing, again, PRA can help to improve those existing requirements and make them more rational from the point of view of minimizing unavailability of the systems that are covered in the tech specs.

And lastly, in evaluating operating experience, this again appears to be an area where PRA could have useful function in sorting out and prioritizing the various events that are covered by LERs.

MR. KERR: Let me ask a question. In determining the need for backfitting, is it anticipated that that will be done on an individual plant basis?

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MR. O'DONNELL: Yes. When I speak of backfitting here, I'm talking about on a plant-specific basis, as opposed to a rule change --

MR. KERR: So you think it could be used there on a plant-specific basis but it couldn't be used in licensing on a plant-specific basis?

MR. O'DONNELL: Well, I guess I distinguish -- when I speak of licensing, I speak of the basic decision on whether or not a license should be issued.

MR. KERR: Yes, I understand that.

MR. O'DONNELL: Okay. And there I do not believe that it is appropriate at this time to use that as a basis for that decision.

MR. KERR: I understood that that's what you thought. I'm trying to understand how one can use it for a backfitting decision on an individual plant basis but it's inappropriate to use it for licensing on an individual plant basis. I don't disagree with you, I just don't --

MR. O'DONNELL: Well, I think one gets to the basic regulatory philosophy under which the NRC operates. If one applies for a license, the NRC has a list of regulations that one must meet as a basis for that license issuance. Now, if we are to say, "Well, we don't have confidence in those regulations but we feel you must submit a PRA in order for us to determine whether or not we should issue a license", it seems to

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me we are undermining any confidence that we have in the existing system. That does not seem to be justified on the basis of operating experience or the facts.

It would seem prudent that we can continue to issue licenses based on the existing regulations. What we're talking about in terms of backfitting or rule-changing is fine-tuning those. And those are not the basic decisions of whether a license should be issued or not, but whether or not, having issued a license, one needs to change the conditions under which the license was issued.

MR. KERR: That distinction is too fine for me, but maybe I just haven't thought about it enough.

MR. O'DONNELL: Well, it would seem to me if we were to adopt a position that we must have a PRA before we can issue a license, I would think that we would be in a very uncertain regime and certainly would not be in a position to issue any licenses within the next couple of years, because we do not have in place either the manpower resources, number two, the methodology that we would agree would be acceptable for doing this, and number three, the safety goals upon which the decisions would be made.

MR. KERR: Okay. I understood your earlier comment to be one which said that one simply couldn't do it on an individual plant basis. What you're now saying is that if one had the resources and technique in place, maybe one could, but one

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couldn't do it starting tomorrow.

MR. O'DONNELL: I think that's right.

MR. KERR: Now, let me also ask, in determining the need for plant shutdown orders, you used as an example the plants that were shut down. Would, in your view, the application of PRA there have had to be done on an individual plant basis, or could it have been done on a generic basis?

MR. O'DONNELL: Well, I think there, in keeping with our philosophy, which I'll explain later, if it was determined that those plants were somehow in noncompliance with the conditions of their license, that is, they had set forth some methodology for calculating the seismic stresses, on which the NRC said, "Yes, we agree, that's acceptable", it was later determined that in fact that had not been the case, that is, those things were not calculated in that manner, it would appear to me at that point the burden of proof would be on the applicants. That is, they were found to be in noncompliance with the conditions of their license. It would be necessary for them to come forth with some sort of a PRA to justify why those plants should continue operating for some period of time.

MR. KERR: Okay. So in that case the justification would be on an individual plant basis --

MR. O'DONNELL: Yes, and I think it would be a matter of the burden of proof resting on -- again, this would be a case where those applicants were in effect requesting an exemption

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from what they had said they did --

MR. KERR: Okay.

MR. O'DONNELL: -- in designing those plants.

MR. KERR: In this particular case, how long would you estimate it might have taken an applicant to do the required PRA, to demonstrate that he should continue to operate?

MR. O'DONNELL: Well, I think in that particular issue if one looked at it rationally, in view of the issue --

MR. KERR: That's asking too much.

MR. O'DONNELL: I don't think it would affect them very long.

MR. KERR: You mean two days, two weeks, two months?

MR. O'DONNELL: I don't know, but one, I think, could have looked at generic studies --

MR. KERR: I'm just trying to get an idea of what would have been different about the way NRC proceeded, had the --

MR. O'DONNELL: Well, hopefully the plants would not have been shut down in 48 hours.

MR. KERR: Well, if one requires a PRA before making a decision, and if one has to spend two months getting a PRA together, what does one do in the meantime?

MR. O'DONNELL: Well, I think that the issue would be somewhat simplified there by the fact that the concern under review was contingent on an earthquake occurring, a sizable earthquake, in the first place. So I would not think it would

have been necessary to go in and do a detailed fault tree analysis of every system in the plant in order to justify, let's say buying some time to do a more detailed analysis.

MR. KERR: But you would not really have done a risk analysis, you would have done a probability of an earthquake.

MR. O'DONNELL: Well, yes. I guess when I use the term PRA, it is not limited to event/fault tree analysis.

MR. KERR: No, but --

MR. O'DONNELL: That is, if one is looking at the risk of allowing those plants to continue operating for a month, that's basically a probabilistic concern --

MR. KERR: Yes, but it's not just the probability of the earthquake, it's the consequences of that earthquake on the structure and the subsequent --

MR. O'DONNELL: That's true, but it was conditional on that occurrence.

MR. KERR: Yes.

MR. O'DONNELL: And if one could prove the conditional case, then one would not have to go through the detail case up front.

CHAIRMAN OKRENT: Dr. Siess.

MR. SIESS: With regard to the use of PRA in the licensing process, do you believe that the licensing requirements, the regulations at some time in the future could or should be recast in PRA terms?

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MR. O'DONNELL: No. I think our basic assumption is that we would not ever reach that sort of regime where we would have solely, let's say numbers like 10^{-6}, as design goals, that it would appear to be necessary in any case to have some specific design rules that designers could follow.

MR. SIESS: The NRC --

MR. O'DONNELL: Certainly not in the near term.

MR. SIESS: For example --

MR. KERR: You don't think it could be done.

MR. O'DONNELL: Well, I don't --

MR. KERR: Whether it should be done, you didn't answer.

MR. O'DONNELL: I don't think it's practical. And I'm not sure it's wise.

MR. KERR: Would you anticipate, for example, retaining a single failure criterion?

MR. O'DONNELL: I would anticipate that the single failure criterion would be re-evaluated, and modifications made in it if necessary and justifiable on a PRA basis. You know, one may say, "Well, station blackout is something that one ought to consider, even though it does not comply strictly with single failure criteria."

CHAIRMAN OKRENT: Why is it not fair to say that it's the licensee that's responsible for the safety of the plant, and if there's a question concerning the single failure criterion and its adequacy, that it's up to the licensee to show that where his

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plant has been designed using the single failure criterion it is still adequate from some risk point of view, and in fact that this be not something that the NRC has to do but the NRC could merely say that questions had been raised with regard to the adequacy of this generally, and we're going to give licensees 18 months to show why, if they have used this criterion, it's still okay?

MR. O'DONNELL: Well, I would think that would be appropriate for the NRC to do, if they had done a PRA that justified that order or directive. But to go to the licensee and say, "Well, we want you to find problems and fix them" is just an abdication of responsibility.

CHAIRMAN OKRENT: I'm sorry, but I thought that the licensees said that it's their responsibility for running the plant. They don't want the NRC coming in and mixing in if they're in the middle of a transient. They have the responsibility. I mean, do they only have part of the responsibility for assuring the public health and safety? Or just what is it? Only when it's convenient --

MR. O'DONNELL: Well, they certainly have part of it, yes. But as far as the basic decision on, number one, whether a license should be issued, it seems to me the NRC has to make a finding on that. And then number two, on whether, having issued a license, it is necessary to make a change, certainly a part of that responsibility rests with the applicant,

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and there should be in place procedures to make sure that he is constantly re-evaluating his plant. But to totally put the burden on the applicant for -- it's always a question of whether or not that plant is safe, I think, is too great a burden to place on an individual applicant. You're asking the applicant to take on very profound issues that it seems to me the NRC should address up front in whether or not they're going to issue a license.

CHAIRMAN OKRENT: I guess what I'm getting at is, while in fact I like essentially all of the things you've shown on the board as useful PRA applications, I, from my vantage point, perceive a sort of one-sided use of PRA. And I see a reluctance to put what I would consider an equal share of the responsibility on the industry to use it. And I was just giving you one possible way in which I could envisage using PRA and giving the industry a responsibility, which in fact maybe they should volunteer, instead of waiting until --

MR. O'DONNELL: Well, maybe I should go through the rest of my presentation and hopefully allay those concerns.

MR. KERR: Let me ask -- I think this won't require too much. In what way is PRA useful for value/impact analyses? I would have thought it would be almost useless there.

MR. O'DONNELL: Well, I guess I would determine value/impact analysis maybe ought to be replaced with something very more specific in terms of cost-benefit analysis. And that is --

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MR. KERR: Well, I still ask the same question. I think you can estimate the cost of a given fix pretty accurately, and you don't use PRA to do it.

MR. O'DONNELL: Well, the PRA would be used in estimating the benefit.

MR. KERR: But the benefit is very difficult to estimate using PRA because you have to decide the value of a human life or the value of an accident. PRA doesn't tell you that.

MR. O'DONNELL: Well, if one adopts the alara concept as put forth by the ACRS, you certainly could.

MR. KERR: Even the AEC, when it adopted the $1,000 per man-REM, said it had no particular basis for using that number. It was just the biggest number that anybody had suggested.

MR. O'DONNELL: But you're quarreling with the number, not with the concept.

MR. KERR: I'm talking about a cost-benefit analysis, which requires that one assign a cost and a benefit. And I don't think PRA really gives you much of a leg up on that process.

MR. O'DONNELL: Well, the AIF, in our proposed safety goals, have very specifically endorsed the concept of using PRA to quantify that benefit in terms of man-REM risk reduction. And if one -- if you feel you cannot do that, then I would question how you would use PRA in resolving some of these issues of reductions in residual risk --

MR. KERR: I was -- I was questioning how you would use it to do a value/impact analysis, because I don't -- perhaps it's one of the bits of information that's needed, but to me not a very important part.

MR. O'DONNELL: Well, I think one would go through a PRA, if he were looking at a design change, such as a filtered, vented containment, and try and estimate the actual risk reduction in terms of man-REM, total societal societal risk. And that would be a measure of the benefit which would be balanced against the cost.

I don't think that's at all -- it's my understanding that the ACRS's proposal on safety goals envisions exactly that approach.

MR. KERR: Well, I would emphasize again that the ACRS proposal was billed as a sort of trial balloon. It is not a final decision, and I would be surprised if it solved all the problems.

MR. O'DONNELL: I would, too, but --

MR. KERR: And I think this is one that is one of the more difficult ones.

MR. O'DONNELL: I agree, Dr. Kerr. But it certainly appears to me to be a part of the things that one would consider in trying to determine whether you needed to incorporate something such as a filtered, vented containment, or something else that may be more benefical at lower cost.

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With respect to implementation of the PRA, again, we feel that there are two major ingredients that are missing. One is the ground rules on how one does it, and we believe that there is a need to establish a commonly understood methodology on how one would go about doing a PRA between the industry and the NRC. And that has to cover the things on this slide. That is, the level of detail one would go to in doing an event tree/fault tree analysis, establishing the component failure data base, how one treats common cause and failures and systems interaction, the consequence modeling, both in terms of the core containment interactions and the off-site health effects modeling, how one would actually do this sort of value/impact or cost-benefit methodology; that is, what elements are appropriate to be considered in the cost, and what elements are appropriate to be considered in the benefit; how one treats human factors. And very importantly, how one goes about quantifying both conservatism and uncertainty in the results.

MR. SIESS: I wonder if we couldn't agree somewhere simply to get rid of those words "value/impact".

MR. O'DONNELL: I couldn't agree more.

MR. SIESS: They're not worth -- they were coined by the NRC to avoid saying "cost", and we don't mean value/impact. You don't mean value/impact there. And it's just confusing. You mean cost-benefit --

MR. O'DONNELL: I mean cost-benefit.

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MR. SIESS: -- where benefit is risk reduction and cost is dollars, and --

MR. O'DONNELL: Exactly. I would wholly support that idea.

MR. SIESS: It's a euphemism we might as well forget about.

MR. O'DONNELL: With respect to the safety goals, I don't want to dwell in large measure on the numbers or the concepts. This is essentially what we have proposed in our May '81 policy statement, and we discussed with this subcommittee last year. We've made some adjustments in it since then, but we essentially see the goals as being split into primary and secondary goals. That is, the primary goals are those that relate directly to public health and safety.

And we feel it's important to establish a limit, both on individual and population risk. And these limits should be met before one goes to the secondary goals, and most importantly, applying the concept that we've discussed here of cost-benefit ratio.

Lastly, we have introduced an additional criterion on large scale fuel melt probability, which we feel is secondary in nature in that it does not relate in itself directly to impact on public health and safety, but it is important from the point of view of preventing accidents and can serve, as we'll later discuss, as a screening criterion to make sure that you have

not -- you can do a limited PRA analysis on a plant, and demonstrate that you've met this goal, without going through the whole detailed analysis.

MR. KERR: Are you using safety as synonymous with risk? Because when you say --

MR. O'DONNELL: Yes.

MR. KERR: -- the large scale fuel melt does not impact on public health and safety, I would disagree, unless by safety you mean risk.

MR. O'DONNELL: Well, I said, does not directly, without other considerations.

MR. KERR: Well, it seems to me it very definitely impacts directly on public health and safety. As I understand --

MR. O'DONNELL: Well, without consideration of --

MR. KERR: For example, psychological trauma has something to do with public safety. And large scale core melts are likely to produce that.

MR. O'DONNELL: Yes. I guess our consideration here is direct health effects in terms of --

MR. KERR: Well, a psychological disability is a fairly direct health effect.

MR. O'DONNELL: Well, I don't want to debate the issue.

MR. KERR: There is no debate about that. It's one of the more serious issues --

MR. O'DONNELL: Well, I guess in terms of health

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effects, I would consider that somewhat secondary to effects such as cancer or fatality.

MR. KERR: I just -- you ought to talk to the people who treat mental health problems. I think you might change your mind.

MR. O'DONNELL: Well, I think we've established a limit on that which may hopefully address your concern --

MR. KERR: I just --

MR. O'DONNELL: But again, we did not feel it was a primary effect.

CHAIRMAN OKRENT: Along that line, why do you use only radiation effects and not economic losses in your cost-benefit ratio?

MR. O'DONNELL: Well, I'm not sure we don't use economic losses. I would think in calculating costs one would take that into consideration, in terms of damage to the plant.

CHAIRMAN OKRENT: Damage to the plant or damage of an economic nature, cleanup, whatever.

MR. O'DONNELL: I think that would be rolled into the cost.

CHAIRMAN OKRENT: I see. It wasn't clear that that was your intent.

MR. O'DONNELL: But to get to some of your comments earlier, Dr. Okrent, I think I would agree with your statement that one needs to consider in any safety goal proposal the

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total package. And that's what we've tried to do. And in fact the cost-benefit criteria is an important element of this package. And if one did not have that, the numbers on top might be different.

We think it's philosophically very important from the point of view of allocation of societal resources to have this kind of criteria in there, and that the primary goals not be set so restrictively that they would penalize nuclear power in relation to some other alternative energy source.

And in fact, these kinds of goals we feel would be proper, or something similar to them, for any energy source. With respect to your comment about the conditional probability on containment failure, we have not introduced that factor. But we feel that establishing the upper limits on primary goals in conjunction with the secondary goal, and large scale fuel melt, in effect do just that. That is, you -- one cannot just meet the large scale fuel melt goal, or at least you have to have some assurance that you've met the top level goals before you pay only attention to the large scale fuel melt goal.

It seems to us that introducing additional factors such as conditional probability of containment failure, given a core melt, in effect establish -- or take away from the primary goals of individual and population risk, and may in fact conflict with any cost-benefit goal you set. You may have to do things that are not cost-beneficial in order to meet the

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conditional goal on containment failure.

Now, turning to the question of how one would apply this whole regime of PRA and safety goals in the licensing process, again, it is, I think, a combination of generic applications and plant-specific applications. And I guess what I'd like to discuss is how we might get into that and how we might start applying these things. And I think the first or basic application of PRA and quantitative safety goals should be to re-examine where we are now. That is, we have an existing set of regulations that are set forth in Part 50 and Part 100 and Part 20 that specify deterministic requirements for design and operation of plants and for siting of plants.

The basic question, it seems to me, is -- well, are these -- is the existing regulatory structure providing a level of safety such that we would -- are already meeting the primary goals, in that if plants that are licensed under those regulations -- do they also meet those primary goals? And it would seem to me that's a very profound question, and one that should be addressed if possible on a generic basis.

And it would seem, based on existing studies, that it should not be necessary to go through each and every of the existing operating plants and those under construction and do a complete plant PRA before one could make such a finding. It would seem that a sampling of plants for which PRAs have been done should serve as a basis for comparison in reaching

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some generic conclusions with respect to compliance with those primary goals.

And we've listed on this slide those plant-specific studies that have been performed or are currently under way and should be completed sometime next year, which may provide a basis for the NRC making some sort of generic finding or determination that, yes, the existing regulations do in fact deliver the level of safety that would meet those primary goals.

And as you can see, we're talking about a dozen or more plants for which PRAs are under way or have been completed, which cover a spectrum, in terms of their operating license state, going all the way back to Yankee Rowe in 1960 up to plants such as Limerick that aren't on line yet and will be in operation in a few years.

We have a wide variety in terms of ratings, the sizes of those plants. We've covered all four reactor types, each of the major types of containments that are currently in use, and we've reflected this balance of plant designs that have been arrived at from a wide variety of architect-engineers.

We heard yesterday a lot of discussion about individual differences between plants in terms of dominant sequences and which sequence was an important contributor for which plant. And those are certainly important considerations at that level. But if we're talking about the basic finding of whether or not one meets the individual population risk --

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MR. KERR: Excuse me. They are important considerations at that level; at what level?

MR. O'DONNELL: Level of detail of whether an auxiliary feedwater system probability of failure is 10^{-4} or 10^{-5}. And there are undoubtedly differences among these plants on the system level. But if the objective is to reach a determination on whether these plants or existing plants in general meet goals such as 10^{-5} to the individual or one fatality per year per 1,000 megawatts, it would seem to me that if we could review these studies, and if in fact we could determine that regardless of the specifics of their design they do in fact generally fall well within that envelope, that the NRC could make some sort of generic finding on that issue.

CHAIRMAN OKRENT: Why is that the vital issue, that the NRC make some kind of generic finding?

MR. O'DONNELL: Well, I think it's vital from the point of view of what one does next. If one can conclude that the primary goals are met, then the issue becomes one of cost-benefit.

CHAIRMAN OKRENT: Well, suppose in fact the levels that they chose against which to make the finding were 10^{-7} per year risk of early death to the most exposed individual, and then they said, "Okay, we're going to look at all of these and see if it's been met." They might make a finding that was counter to the one you anticipate with the AIF proposal.

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MR. O'DONNELL: I agree. That's why it's important that the number set in the first place be a number that is supportable and reasonable.

CHAIRMAN OKRENT: Well, again, at the moment, it's not so clear to me that a finding is the vital thing. I thought we were trying to use PRA to improve reactor safety, if in fact it needs or if in fact it can be done this way. So I'm just wondering whether the objective you state is the number one objective.

Let me ask a different question. When one does a PRA on a plant and says, "Yes, it's met the goal", which plant is it? Suppose you look at Oconee. Is it Oconee as it was when it was first built, is it Oconee in 1977, is it Oconee in March 1979 and May '79, in February 1980? Which Oconee is it that you evaluate? Is it Oconee as it would have been 10 years from now if people hadn't looked hard at cold overpressurization?

MR. O'DONNELL: Well, I presume these studies -- it's Oconee as it exists now.

CHAIRMAN OKRENT: But what I'm getting at is, you're saying people will have done risk studies on specific plants. But in the process, some of these plants have been fixed up. Oconee is not the same today as it was a couple of years ago. But there are some other plants that will have not had the benefit perhaps of whatever fixings have been done on Oconee. How will you make this decision that, "Yes, these regulations

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have in fact given us the level", when in fact if you looked at the plant as built, you might conceivably not be so willing to make that same judgment, even using the AIF risk level?

MR. O'DONNELL: Well, I guess what we propose is that, well, you want to -- if you can demonstrate you've met these primary goals, then the issue again becomes one of cost-benefit balance.

CHAIRMAN OKRENT: Again, let me make my point. You were saying, "Well, NRC should go through these and make a finding, 'Yes, the plants that we've looked at here, which were built according to our deterministic regulations, meet these safety goals.'" I'm not so sure that these plants, all of them as built and not changed, I mean, from that time, but taking them as they were built, if you did a PRA, knowing what you know now, would in fact meet the AIF safety goal. It's not 100 percent clear to me.

MR. O'DONNELL: It's not 100 percent clear to me, either.

CHAIRMAN OKRENT: Well, then, did the AEC and NRC criteria in fact correspond to the safety goals? Those plants were licensed, you know, they met whatever --

MR. O'DONNELL: Well, I think that's the question. I mean, if -- but if one could look at these studies and could make the -- arrive at that conclusion, then we would know where we are with respect to the existing regulations and then could apply the cost-benefit as the primary basis for decision-making.

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If we cannot make that determination, then we're in a regime where we don't know whether or not the existing regulations deliver the level of safety that meet the primary goals. And then one must look at other things.

So I think it's a very pertinent consideration, is, where do we stand now? I mean, we could say, "Well, okay, each and every plant, go do the PRA and then come back and tell us whether you've met the goals." But I think there's a shortcut, and I think this kind of approach, wherein one would look at a broad spectrum of sampling of plants, and if in fact the results are such that one can make these findings, then we can have some assurance that we're already in that regime wherein cost-benefit is the major consideration.

CHAIRMAN OKRENT: Let me, if I can, ask the question along a slightly different line. You've mentioned, I think, that this secondary goal of large scale fuel melt at 10^{-4}/reactor year could be a trigger point. So if I understand correctly, the suggestion is that if you do some kind of a limited PRA and you end up with this as being a little smaller than 10^{-4}, you don't have to do any further. That would be the suggestion, is that right?

MR. O'DONNELL: I think if -- that's probably correct. If -- if one has a containment that meets existing requirements and one meets existing siting rules. I think based on past studies, the 10^{-4} number would be much more restrictive than the

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other numbers that we saw in terms of individual or population risk. If one could combine that with this sort of generic finding, it would alleviate the need for each and every plant to go through the full PRA, that is, including the consequence analysis. But we would still envision each and every plant going through the IREP type analysis as confirmatory to confirm that, "Yes, indeed, they did meet the large scale fuel melt goal", or if they didn't, then expanding that study into a broader PRA study to make the finding directly.

CHAIRMAN OKRENT: Well, I guess I don't get enough information from the 10^{-4} number, because I can readily postulate accident scenarios, as you can, like vessel failure, which you would feel uncomfortable at if it were 8 times 10^{-5}.

MR. O'DONNELL: You're talking about --

CHAIRMAN OKRENT: Because in fact the containment is not going to function in the same way as it does for what you now consider your average mix of core melts. So implicit in this 10^{-4}, I have to assume, is an assumption of some kind of containment effectiveness.

MR. O'DONNELL: The assumption is, you have a containment, and that it in fact meets current requirements.

CHAIRMAN OKRENT: I don't know what the term "current requirements" means to you. To the Atomic Energy Commission it was that it has some kind of leak tightness and something that met the part/100 kind of things. It used to mean that to the

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NRC until the last year or two. So you have to tell me what you mean by, "It meets requirements."

MR. O'DONNELL: Well, I mean the general design criteria spell out requirements for the containment in terms of being able to tolerate double-ended pipe breaks and such --

CHAIRMAN OKRENT: Well, I find that insufficient, because again, your criteria would allow someone to come in and say, "Well, it so happens the biggest contributor to core melt in my plant is reactor vessel, but it's smaller than 10^{-4} per year. It's 8 times 10^{-5}. And when I add up everything else, it comes out 9 times 10^{-5}. So we don't have to do any more."

MR. O'DONNELL: Well, I would think perhaps this sampling again would -- I mean, if there are such cases of vessel failure that are totally out of line with what's been reported in other studies, I would think that would be a flag in itself. I don't know how --

CHAIRMAN OKRENT: I'm just using a scenario that we can quickly envisage as a PWR/1 or 2. But there are other ways of getting to the PWR/1 or 2, or BWR/1 or 2. I mean -- well, let me leave it at the point that it seems to me you have to rethink whether this is an adequate basis for a no-go decision on doing more.

Mr. Thadani.

MR. THADANI: Mr. O'Donnell, just for clarification purposes, I'm looking at your primary and secondary goals. Let's

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say for the sake of argument that there are certain uncertainties in estimating core melt probability or large scale fuel melt and that there are even greater uncertainties in estimating risk to individuals. Let's say that there are sequences, or at least there is one sequence which has a likelihood of occurrence of on the order of 10^{-4}, or say 5 times 10^{-4}, which might well result in large scale fuel melt, but that calculation showed that risk to individuals, or that your primary goals will not be violated, recognizing there are large uncertainties to these calculations.

What would you propose we do in that case?

MR. O'DONNELL: You're talking about uncertainties now and --

MR. THADANI: No, no, no. I'm suggesting that we all recognize there are uncertainties to these calculations, and that we recognize the uncertainties in estimating risk to individuals might be greater than the uncertainties associated with estimating core melt frequency. Personal viewpoint, in any case.

MR. O'DONNELL: Yes.

MR. THADANI: Let's take a sequence which has an estimated frequency of occurrence of 5 times 10^{-4}/reactor year but which is calculated not to violate your primary goals. What would you propose we do?

MR. O'DONNELL: Well, leaving aside the question of uncertainty, which I'm going to talk about later, presumably

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then if the plant met the primary goals and if one were looking to reduce that risk, one would look at it from a cost-benefit point of view, that if one could reduce that risk in accordance with the cost-benefit criteria, you would do that.

MR. THADANI: Do you think that from our past experience, of a few years, at least, that that's practical?

MR. O'DONNELL: From our past experience, no. Whether it ought to be practical, I guess, is what we're recommending.

CHAIRMAN OKRENT: Dr. Smith.

MR. SMITH: Paul Smith. If I could have the previous viewgraph, I just have a small comment to make. In thinking, of course, as I do a lot about the earthquake problem, and if it is a dominant contributor to risk, the implications that the two top goals could be -- on, say, the seismic design requirements, could be significantly different, comparing, say, a multi-unit site versus a single site, the top one for multi-unit site might lead you to believe that the design requirements for earthquake are more stringent than the second one. Because the common mode nature of the earthquake threat extends to simultaneous exciting entire plants for a multi-unit site, and may even extend, in the eastern United States, because of the special circumstances with earthquakes, to more than one site.

But the implications, say, for -- and of course it may be that the same thing goes for flood and perhaps wind and other external events that have a large-scale common mode aspect or

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common cause. Just a comment.

MR. O'DONNELL: If I can just move on, maybe I can --

CHAIRMAN OKRENT: I'll let you go ahead.

MR. O'DONNELL: Maybe I can explain a little better with these next two slides how one would see the process working. Again, getting back to -- again, the concept of looking at where we are, that is, the existing regulations, and trying to make generic determinations, again, these are generic studies of -- trying to draw generic conclusions based on plant-specific studies. And if the NRC can in fact make the findings with respect to individual and population goals that, "Yes, we do in fact -- are already delivering -- or if one complies with our regulations, one delivers that level of safety", then it seems to me the burden of proof rests with the staff as to why we need to change those rules. And that burden of proof should be carried out with respect to the cost-benefit goal.

If in fact we cannot make those findings, or we make negative findings, then there is clearly a need to make rule changes without respect to the cost-benefit criterion.

Now, turning to the plant-specific applications, we do not -- we endorse the concept of each plant doing at least a limited reliability study of the scope that is currently being conducted under IREP as pretty much a confirmatory exercise, and also to set the level, the risk profile for that plant, and determine need for further changes. And in that view, we

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think a large scale fuel melt goal can be useful in making that sc eening decision. That is, if one goes through the IREP study, which is aimed at calculating the probability of core melt, and you can in fact assure yourself that you meet that goal, well, then, we're in a realm where the cost-benefit -- we're in a realm where we have in fact verified -- and again, absent unusual siting or design conditions, that is, again, if we have a containment that meets current requirements, I think if you meet the large scale fuel melt goal, there is assurance, if it can be coupled with the generic findings, that in fact that plant is well within the primary goal, which is really the main consideration.

We are then in a realm where we're looking at plant changes from two viewpoints. That is, backfitting to bring about reductions in that residual risk. And it would seem to me that if the NRC is proposing a backfit, they would then bear the burden of proof, using cost-benefit criteria as to why that plant, that specific plant needs to be changed.

Alternatively, if the applicant is proposing something that would result in an exemption from his applicable regulations or wanted to get relief, then he would have the burden of proof for that change.

Now, if, having g e through the IREP study, one cannot demonstrate that you meet the large scale fuel melt goal, that does not again reflect that there is undue risk from that plant.

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And in that case, we would envision the plant going through a more detailed, comprehensive PRA study, including consequence analysis, to make these findings directly on individual, population risk.

If the finding is negative, again we would conclude that changes need to be made without consideration of cost-benefit balancing. If those findings are affirmative, we are then back in this loop here. And again, getting back to the -- and there was an intertie between the plant-specific and the generic. For example --

MR. KERR: Excuse me. When you refer to IREP type studies, do you include ignoring external initiators, seismic, fire, and so on?

MR. O'DONNELL: We're talking about basically the IREP studies as currently scoped, that is, looking at internal initiators and determining whether or not -- determining the probability of core melt, without looking at consequences.

Again, getting back to the generic findings, if in fact the NRC went through this process generically and looked at a rule change from a cost-benefit viewpoint and generically was able to justify that, and it applied to specific plants, well, then, one would then be seeking an exemption from that, and the burden of proof would shift to the applicant.

The question of uncertainty comes in here, too, because I've been speaking as though these things could be determined

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very specifically without much uncertainty. Obviously, there's great uncertainty in any of these analyses and in any of the results. We're comparing ourselves to specific numbers in the decision criteria.

And one must temper the use of PRA and the safety goals themselves, according to the degree of uncertainty. If we can go through a PRA analysis and make a determination that a particular value, whether it's individual risk or population risk or cost-benefit number, is far removed from the safety goal, by orders of magnitude that are much larger than the uncertainty, then it would seem that one could rely on both the PRA and the safety goal very confidently.

If the results are near the safety goals, then obviously you must have something besides the PRA and the safety goals to decide your decision-making. And that's just one input.

But I think in this area here of burden of proof, I think the question of uncertainty can come into play. That is, if the NRC bears the burden of proof, perhaps the uncertainty that they have to demonstrate is shifted towards the high end, or the lower end, I should say, and the other case where the applicant bears the burden of proof, he has to deal with the uncertainty on the other end. We certainly have not worked out in detail how uncertainty should be treated. But I think it could enter into some of these decisions, depending on where the burden of proof lay.

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I'd like to turn to the question of the role of the NRC staff in reviewing PRA studies. And here again, we do not envision a regime where a PRA or an IREP as performed for a specific plant would be a part of the FSAR or the PSAR, and would be reviewed in detail by the NRC staff. It's supplemental. The basic requirements are set forth, as we said, in the regulations. And one should not supplant those with new requirements, or overlay those with new detailed requirements as part of the licensing application.

The NRC staff should very definitely review the applicant's methodology, the data base that he is using. The results should be submitted to the staff so the staff can review them for comparison with results of similar studies. And of course the detailed calculations should always be available for audit by the NRC staff.

And I think this is analogous to the process that is followed in similar detailed engineering calculations that have safety-related consequences, such as seismic. The NRC staff does in fact set down and affirm that the applicant is using prescribed models and data. They look at the results, they compare them with other plants, and they do in fact come in and do detailed calculations.

I do not subscribe to the idea that PRA calculations are somehow more important or more mystifying than other detailed safety-related calculations. And again, it's consistent

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with our basic philosophy that these techniques are used to support and not supplant existing deterministic requirements.

Just so summarize, I guess, the current AIF position on these issues, again, we feel that licensing of plants should continue, that is license issuance, under existing regulations. The NRC should attempt to perform or draw generic conclusions on existing PRAs to determine the need for changing those rules, both generically -- well, generically, primarily.

Individual licensees should be required to perform IREP studies, or in cases of special concerns on siting or design, more comprehensive PRAs, to confirm those generic findings. But again, we stress this should not be made part of the licensing application, or really is not a licensing action.

We cannot stress enough the idea that we need quantitative safety goals to use in evaluating all these PRAs that are currently being done and will be done. It does not make sense to go through these exercises and to use them in the regulatory arena without some rules on what is acceptable or what is not acceptable.

And again, if these basic findings on compliance with primary goals can be reached, we feel we're in a realm where the NRC staff bears the burden of proof for rule changes. That is, why we must get better, on a plant-specific basis, why backfits are required. By the same token, the licensee has burden of proof if he wants to deviate from those requirements, in terms

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of exemptions, or if it's determined that he's not in compliance with those regulations that are applicable to his plant.

That basically concludes my presentation. I'll be glad to answer questions.

CHAIRMAN OKRENT: All right. We'll take one or two questions.

Yes.

MR. GRIFFIN: Charles Griffin, AI.

On your goals, primary goals, the one death per 1,000 megawatts per year, I was wondering how you -- what type of methodology you'd use in calculating this. Now, for the past couple of years, I've been trying to utilize some methodology in conjunction with some risk assessments on plutonium production plants. And I used Dr. Joksimovich's line as a guide. I've tried to use the AIF criteria. And when you start to apply it, you have all types of assumptions that you can make. You can either say, all right, you've got the vector, the sector and the worst -- highest population direction. You assume the slowest wind velocity, say, all right, I'll just settle with that. Or you can use wind rows and you can say, all right, we'll take it over all with the total circumference and use the probability of a given wind row and the velocity in that section, add these up. You can go on ad infinitum to different methods.

Have you thought of a standard method of applying this criterion?

MR. O'DONNELL: Well, in concept, the one fatality -- the total population risk is essentially the integrated risk under a consequence probability curve. That is, you integrate all the contributors to risk, you look at their consequences in terms of population risk, and multiply that by their expected frequency. And you come up with an expected value or an average value of impact in terms of population risk.

Now, how you go about calculating that curve is something that is -- we agree, there is a need for agreement on, in terms of the methodology. We are not -- AIF is not proposing that, any detailed methodology for doing that. We're proposing the concept that it's the integrated risk under the curve. How one goes about calculating that I think is a subject that is being addressed by the NRC, IEEE, ANS procedures guide. And in fact people are doing that. I mean, there are methods for calculating that that are being used in current studies.

MR. GRIFFIN: We've been using specific methods. I was wondering if someone actually reviewed our method whether it would be acceptable or not. That's --

MR. O'DONNELL: Well, again, we feel that there is a need for some agreement on methodology. What that is, we're not at this time in a position to say.

MR. GRIFFIN: Thank you.

CHAIRMAN OKRENT: Well, I'm going to suggest that we go on to the next speaker, since we're nominally 30 minutes

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behind the agenda. I'm sure we'll come back to some of these points in general discussion.

Dr. Zebroski is next. I think you should assume we'll permit the order of 45 minutes to presentation and questions, if that's reasonable.

MR. ZEBROSKI: Okay. I have a written text, which I don't propose to go through in detail, since I think it would be more useful to make observations on some of the topics already discussed, if that's --

CHAIRMAN OKRENT: Yes. As you wish.

MR. ZEBROSKI: Okay. With respect to the Questions 4 through 7, I think the observation that might be made is to recognize the wide variety of uses that have been made for a PRA type discipline, recognizing that we have a very difficult time defining what we mean by that. And I would speak here from the viewpoint of a user and a patron of the arts of this kind of discipline as distinct from a purveyor or a practitioner.

We are using PRA for a number of purposes which have not been alluded to in this meeting but which I think are relevant to the use in licensing. For example, the use of dominant sequences as a test for what information an operator should have to manage a severe accident has been a very useful exercise and one which can verge rather quickly to a reasonable set of parameters, contra the 500 to 1,500, which people originally thought might be necessary.

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Similarly, the use of probabilistic discipline to test, first to develop and then to test ATOGs, the abnormal transient guidelines, has been very useful, again focusing on dominant sequences. One exercise going on now is to attempt the use of a PRA kind of discipline as a judgment basis for environmental qualification question. I think the industry is totally unable, both physically and institutionally, to test to all conceivable environments, all conceivable equipment. And a common sense approach suggests that some equipment is more important than others for severe environments, and that again the probabilistic discipline gives you a way of sorting out the more important from the less important.

Another area where probabilistic discipline is being used is to evaluate the significance of what we are calling significant events, planned events which have some kind of troublesome implications, even if they happen to be benign. Some of them are not even events but simply observations taken on inspection.

And this implies the need of a sensitivity analysis. And finally, there's the impact of the PRA process on the kind of data base development that we do, long pull. We would all hope, for example, that NPRDS would be able to give updated duty cycle, frequency of challenge information as well as raw annual failure rates, from an unknown total population size, which is where we are now. That improvement clearly is needed if we are to go

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systematically to a PRA.

So coming now more specifically to the general uses of PRA, I think the classical use, of course, is to establish an estimate of a risk curve of severity versus probability, and some things that flow from that risk curve. Second use has been to provide a systematic framework for design review and for checking of procedures that go with design. And a subcategory of that design review is that it identifies outliers in risk contribution of two kinds; either where a plant might be an outlier in terms of the whole plant population, or whether a particular system within a plant is an outlier in contributing to sequences with high vulnerability or high risk.

Those classical uses I think are now giving way to several other uses which I believe in the long pull may turn out to be much more important and much more beneficial to safety. And the practical use, which, again, I think it was remarked by several people at Harper's Ferry that the discussion paper provided at Harper's Ferry greatly understated this use by both the industry and by NRC staff, is that the systematic use in a spotlight sense, not a global PRA but a local PRA on a particular system or subsystem, for evaluating specific alternatives and design approach, Design I versus II versus III, or details of that design, and also for evaluating alternates with respect to procedures, Procedure I versus II versus III.

The widely published example of this of course has

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been in the feedwater systems, but I think you'll find that both in the NRC staff and in the industry this kind of comparison now is being used very widely, very intensively, and I think very effectively.

The blend, however, here -- the way we tend to be blinded to this use, I think -- because wherever possible you go deterministic instead of statistical. Statistics is the cover you hide under when you have ignorance. If you have deterministic data, you generally use that. So this is a blend of classical use of engineering tradeoff, supplemented by the use of a statistical or probabilistic approach, where the deterministic data has uncertainty. So that the PRA inherently is a treatment which lends itself to uncertainty.

Fourth use for PRA is to provide a systematic basis for ranking the relative worth of changes or improvements and eventually, hopefully, in regulations. And I think that was also discussed in all three of the safety goal meetings in recent months. And I think it has been formulated in two fashions, in respect to regulation. One is what some of us have been calling the Bernaro (ph) principle, and the other one is the fashion which was formulated on page 35 of the Harper's Ferry discussion paper. But basically, my formulation of this is that the intensity and timeliness of response to a perceived safety problem should be proportional to the time integral of risk. If you go any other way than that, you're not adding to safety,

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you may be decreasing it. And specifically perhaps a broad issue here is that the most severe criticism of the whole industry, the supplier, the reactor operator, and particularly the NRC, in the Kemeny and Rogovin reports, has been the inability to distinguish more important from less important.

And the only thing going right now that we have to change that -- if you say, "What is changed on that subject since TMI?" the answer is, "Almost nothing, except the starting to use the relative risk assessment as a way of prioritizing things." So a 10^{-8} issue, just because it happens to hit the front page of the newspaper, does not dominate the attention and resources of the staff and the industry for months, as has happened many times.

And I think that is clearly so negative to public safety and public interest that if we do nothing else with the PRA process than to get this relative assessment going in a more intelligent fashion, we will have failed monumentally.

The fifth use that I have listed here is to provide a systematic framework for rigorously integrating cumulative learning from operating experience. One of the people that I respect in this business remarked that the industry sometimes seems to have one year of experience repeated over and over again. And I think that's a reflection of the fact that the process of cumulative learning from experience has not worked terribly well. And I unfortunately have to second that, because

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in the significant events program now which we've been operating for just under two years, we are now being unfortunately unable to find any new precurors. Most of the things we study now have one, two, five, 10, or would you believe 80 precursors. So the plants aren't being inventive enough in developing new initiators. What we're seeing is the same kind of problem recurring many, many times. That's troublesome in the sense that it tells you that the learning from experience process isn't working well enough.

On the other hand, it's very encouraging in that it gives you a clue on the question of, are our event trees complete, are our initiator perceptions complete? I think as the learning from experience process is rigorously applied, you can do some theorums on what the likelihood of the undetected initiator or the undetected chain or the undetected system interaction is, and that likelihood clearly will go down with time.

CHAIRMAN OKRENT: Excuse me. Where would you put the flooding incident at Indian Point in that scale of, everything that's happening this year has happened last year?

MR. ZEBROSKI: I'll have to qualify that, that the precurors are at the initiator level; at the instrument failure level or at the -- the consequences can vary, depending on how far you let a thing run. But water on the floor has many, many precurors for many, many reasons.

CHAIRMAN OKRENT: But that's then a little bit of a

highly qualified conclusion, because we're --

MR. ZEBROSKI: In the sense of prevention, it is not.

CHAIRMAN OKRENT: I mean, obviously, you could say, "Well, leakage from an air cooler can lead to leakage and accumulation." But --

MR. ZEBROSKI: No, the leakage wasn't the fault, it seems to me. The leakage was the lack of timely detection and response. And the symptoms of that -- again, you have many prior examples of that. So had you taken care of the prior examples thoroughly, you would have reduced the probability of this sequela.

CHAIRMAN OKRENT: Okay.

MR. ZEBROSKI: So the use of PRA in licensing implies to me -- this is an incomplete statement, but it implies setting up a template or set of criteria of which the risk curve severity versus probability is one important element of the template, or variety of these templates, since you have different shapes of the curve. You have to specify what a PRA consists of -- and I'll talk about what is the possible verification process, briefly -- and then you compare the specified or the proposed analysis, PRA_I, versus the template. And you have to decide on some acceptability criteria. And finally you have to decide on a rate and intensity of response for a perceived departure from the template.

And as has been mentioned by many people, none of these

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elements are in place. I will focus on one part of the PRA uncertainty, which Dr. Okrent alluded to, but which has not been really covered in most of the other discussions. And that is, a PRA, if it is to be useful for risk estimate for public policy, implies that you believe you have good risk predictors or behavior predictors. But components change due to replacement or maintenance or deterioration or backfits. Duty cycles change, sometimes because of regulations and sometimes because of changes in procedure or in duty cycle requirements. Procedures change for the same set of reasons. Experience of the people involved changes as data increases and as interactions are perceived which were omitted in the design analysis. I think that's another interesting point that many of the major events that we now look at are not really very well covered in the FSAR. I think that's a little bit troublesome, and it suggests an updating process which I'll mention later may be desirable.

So we have the situation where if a PRA is to perform its function in terms of public policy and risk estimate, it must be dynamic, but if the performance historically has been static -- and again, you have to say whether it's Oconee, 1973 or 1978 or 1982, and in some respects it's a different plant, different operating management on each of those dates. So how do we bridge the gap between a static and a dynamic -- static PRA, and a real world that's dynamic?

Well, the classical method has been what I'd call an

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ignorance blanket. You throw in wide margins of error, you take multiple worst values of parameter sets where you do a deterministic analysis, and you define a nondeterministic worst case. In other words, what's the worst that can happen? And I think we ought to have an annual contest for worst case invention. Because in this business the more fright the more fame, it seems to me. And I guess we have many people competing for that.

There are two extreme worst cases I haven't heard for a long time. And that is a nuclear accident which itself triggers a massive earthquake, with sequelae, and another one I haven't heard for a long time is a nuclear accident which triggers a hypothetical nuke reaction of nitrogen 16 in the atmosphere and blows up the whole world. But I think if somebody could get any physical plausibility on these, we'd hear this kind of worst case being brought up.

But let's get back from the classical method, which is the ignorance blanket, to what I'd call modern method, which is still developing, which is, wherever possible, use the best available physical model of at least the dominant sequences of the severe accidents. And the using of worst case, either parameter sets or "fails-doesn't fail" assumptions, is clearly a cop-out. It doesn't help the operator, doesn't help the design. So at least the best estimate, physically best modeling, is certainly the right way towards improved safety.

Secondly, you have to provide an environment in which

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the cumulative learning process is rigorous and systematic. And I think that's starting to come into being. I'm not sure that it's being fed into the PRA process yet.

And finally, to use a deterministic approach wherever possible versus the statistical guesses. I've given example of that.

Now, in passing I'd like to make an observation on the roles of codes and standards, because this was brought up by one of the earlier questions. And I think clearly the validity of a PRA -- if I am to be a responsible legislator, regulator, public policy person, when I accept something based on a PRA, I'm assuming that it in fact predicts, has some predictability of behavior. And there's one element of the system which attains that predictability on a dynamic basis, and we alluded to that before, and that's the use of an experiential code and standard.

The consensus code or voluntary code, as it was referred to, has the interesting aspect that it tends to integrate the lifetime experience of this kind. So when you say, "I have a 10^{-6} pressure vessel probability", it happens to be almost at the experiential level. We almost have 10^{-6} vessel years of vessels generally similar to nuclear code design; actually, many of them inferior, which strengthens the conclusion. I think the interesting thing to me was at the SMRT (h) conferences, the people who spent a lifetime in refining these codes and studying the reliability of pressure vessels, I think the

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consensus is that the technology is now, when thoroughly applied, really going to give you something on the order of 10^{-7}, as a technically realizable result of applying code discipline. And when they say, "But if we have one tomorrow it will be because of a failure of implementation discipline" -- so this comes to the next point, that a PRA, unless it rests upon a body of codes and standards and a whole assumption of the implementation and enforcement discipline, is just paper work.

So I think I might say that my attempt to formulate a safety goal, I have told everybody who has put it in tables since then to withdraw it, because I no longer believe that a safety goal in terms of a simple few numbers is feasible. Because when you go to the operational definition of what I mean by the safety goal, I end up saying it's really the whole culture of the implementation, if I'm going to make any measure of whether I'm -- or not.

Now, I also should mention that there are other kinds of codes and standards, of which Appendix K is not an especially good example, and that's a code or standard which is not based on experience but which just says, "Wouldn't it be nice if we did this? Or wouldn't it be profitable if this was required by regulation?" Such codes I think had a grand efflorescence in the early days of the breeder program. Some of them are good and useful, some of them are not. They breed a low degree of respect when they are arbitrary and not tied to experience. So

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I think we really need a blend. Where there is no experience, clearly you must proceed on a good judgment basis and make a code and standard. But I think in the application, particularly in the enforcement and punishment of a tentative code not based on experience, we ought to treat it rather differently than an ASME code, where you do have the experience base on it.

Okay. What do we mean when we say, "to PRA a plant"? We've used that as a verb several times, yesterday and today. And I guess sort of the basic discipline of modeling and fault trees and statistics and cut sets and combinations is -- there are a number of textbooks on it and there are a lot of practitioners of it. And it turns out not to be very hard to teach this discipline.

But the treatment of uncertainty is still the weak spot. And I'll refer to both Harold Lewis' APS study and the discussions by Bernaro and Matt Taylor and to Harper's Ferry last week. There are the methodology uncertainties, there are the completeness of event trees uncertainties, the accuracy of the event trees physically, the applicability of the statistics. I think if you're a little bit skeptical, you find all of our statistical data bases are inhomogeneous, because the component and the systems that they're based on are no longer being manufactured or are being maintained differently or operated differently. So you know there's a systematic bias, for good or worse, but they're different.

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The use of specific data from -- plant-specific data is still a very weak discipline, because we don't have the massive statistical data bases in a rigorous fashion. We do have to worry about undetected initiators and change in system interactions. We do have to consider -- incidentally, the single failure criterion in a way is easy, because most systems, you can easily postulate have a large cloud of malfunctions in them at all times. I'd be very surprised at any plant that you went into and examined didn't have 200 or 300 things which you would say were not in an ideal state, some degree of inadequate function. And the plants actually treat random multiple failures very nicely. That's sort of part of the engineering discipline historically, to kind of assume Murphy's law. And so we really have overkilled the single failure criterion in one respect.

We haven't gotten a statistical way of treating that resilience of a system to multiple failures in a way that can be used for regulatory purpose.

And finally, I guess one worry I have on PRA, again being not a practitioner but trying to be a user and a comprehender of it is, as the old word, scrutability, the errors in recording, errors in calculation, can be due to ambiguities in the symbolism. And I think the need to make the event trees and fault trees scrutable by making them as deterministically definable as possible is very important.

Okay. Let's go on to the question of verification,

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which has had a lot of discussion. How do you verify something like a PRA? And I think of only three conceivable methods. Hopefully, that's not all the possibilities, but it's pretty broad. One is a process spec. The IEEE and ANS committee, which EPRI is also participating with, both with people and funding, is trying to make a process spec, trying to say, "Here is an encyclopedia or a -- not exactly a cookbook, but a list of ways of doing the different steps, hopefully many of them acceptable, and hopefully some of them more useful than others, depending on the particular problem being treated." So the process is one way.

The danger here is that you substitute ceremony for sense. You know, if you go to the extreme of saying, you shall hold your pencil at a certain angle while you do this calculation, it clearly is not productive to the quality of the result. So there's a tendency sometimes to go deterministic, the other extreme of saying, well, take a look at the system and see how it functions. That doesn't help very much, either.

So I begin to wonder ultimately whether a process spec beyond the idea of a textbook for practitioners is really going to be feasible.

Another way to verify is a product spec. I say that I will look at the final output and I will audit it or study it or evaluate it in some fashion. Now, for the things which are based on experiential codes, like ASME related systems, the

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product spec works because you have a whole body of experience behind it and you have many, many testable features which give you assurance that the criteria are being met. For many parts of the system, the product spec is unfeasible. Now, we don't really have a complete pedigree of much of the equipment in the plant. We don't have a pedigree versus time. There's the classic complaint that we don't even have as-builts in many cases. So the product spec at the plant level is difficult, and the product spec at the paper level is extremely difficult.

And I really question the feasibility of it, other than on a sample check basis.

A third way to verify is brute force. Replicate the whole darned thing. And we have now some paralellism between industry-based PRAs and IREP and NREP and RSSMAP and several other attempts. Now, the replication on an industry-wide basis is too many man-years. We don't see a way to get there from here.

But now there are some simplifications which can be suggested here. I'm just throwing these out as wild ideas that have only the virtue that they seem to be in current use, without much recognition, and that is to recognize that the main use of the PRA discipline is to make relative risk assessments, and that the proof of the risk envelope was a nice academic exercise at MIT of tolerably little value on the long pull nationally. If you use the relative risk assessment, then you

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can do a specific limited rerun on the particular systems and chains involved, and a full, either product or replication audits approach, becomes feasible

So if you simply narrow the globalism of the PRA process, then you have the technique for verification at hand. It may not employ quite as many PRA artists as the other way, but I think it's a very valuable way to go.

I think this can be used then in evaluating design alternates, in evaluating alternate procedures, evaluating the need for backfits on particular systems, evaluating the particular backfit alternates that are used, if you decide to implement them.

If I may, Dave, take about three minutes to talk about Oconee study, I was asked by Duke Power also to represent them also at this meeting. And the previous comments do not represent Duke Power. These hopefully will. We're doing a massive PRA on the Oconee 3 plant. And Duke Power was picked by us because of two circumstances. First of all, they showed great willingness, if not eagerness, to turn over all the rocks. One of the worries about PRA is, "My God, I might find something I don't like." So I think the fact that the management was eager to find any problems and eager to search for and implement fixes if needed made it an ideal environment from the standpoint of trying to do this kind of a study.

The team was organized on the basis of two workshops

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in which we invited everybody who had done or participated in a primary contributor way in all of the PRAs that we knew about. And we asked the advice of many of the people in these room, who were at some of these workshops. So we had asked the advice of the people of how best to organize such a PRA with the thought not of establishing the risk envelope as the primary goal, but with the thought of establishing a useful decision tool. After all, PRAs are a branch of the decision theory discipline. And Dave's theorem is the rock of ages for PRAs. And maybe you should use it for decisions. So our experience had already suggested that the use of the PRA as a framework for rigorous decision-making was very important.

We distinguished decision-making at several levels. There is decision-making based on local experience, which works for frequent problems. There's decision-making based upon, say, system or reactor type experience, which works for some design problems. And there's decision-makings on low probability, high consequence problems, for which the experience base of any given individual is deadly dangerous. Because he will inevitably underestimate. He'll say, "Well, hell, we've run this plant for 20 years. It never happened, so why should I worry about it?" So experience is a very bad guide for dominant sequences and severe events. And therefore, hip-shooting or experience-based decision-making on that aspect of safety clearly is unproductive, and clearly a PRA tool is the way to go. And we're hoping to

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try to design then, or do a pilot procedure which will lead to a decision tool for in-plant use.

These workshops of the people who had the most experience in this business were almost unanimous in advising that the key ingredient of a good PRA is extremely solid event modeling. The experience of most of the PRAs, including WASH-1400, was that when you think you're about done, people start to look at the event trees and say, "My God, the thing doesn't really work that way physically. You've mismodeled it." And when we looked at some of the IREPs, we were asked to do a little peer review, when you got people who actually operated or designed the plant, that was the thing -- chain after chain fell apart on being insufficiently realistic physically.

And so it's very dangerous to draw conclusions from unrealistic event chains. So we constructed a team of 19 people, of whom 10 are engineers who have been in either the design or operation of the plants. And many of them are also SRO-trained, although none of them are actually operators. We had five people representing two, and now three, of the lead contractors who have worked on prior PRAs. And we had three NSAC staff people who have some background in this field, sometimes by osmosis and sometimes by courses.

And this team now is supplemented by our internal peer review committee, which includes the usual Norm Rasmussen, Sol Levine, Ian Wallace, Stu Asalin, Tony Buehl (ph), and -- we,

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the people of WASH-1400, help advise on where the weak spots are that need more attention.

So the basic assumption was that the PRA is to be the tool for systematic design review on a continuing basis, and is to be a tool for continuing decision-making in the plant, and most importantly, it's to be the framework, the Christmas tree, if you will, on which to hang a rigorous cumulative learning process. Every time we look at a significant event in any plant, where there's a legitimate analysis done -- again, not hip-shooting, presumed cause, but where people really dig into it and get the observations and the theory to agree -- then that is a further detailing of a particular event chain or set of event chains. And yet that -- since the mass of that information is so tremendous, unless you have some systematic way to accumulate it, it becomes lost. A year later it's forgotten.

So we hope that this discipline of the continuing building of the event chains in detail will prosper. And that discipline is really just starting this year. I know it's far from matured. It has the feedback on the other side that it gives you a tool for evaluating the relative significance. We have now -- jointly, between NSAC and INPO, we have put out something like 80 recommendations for significant events that we've studied, or 80 sets of recommendations. And we have the same problem as the NRC. When the utility comes to us and says, "Well, I have this whole laundry list of things to get done.

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Which one do I first? And can I delay this one one year or three years? Or should I do it tomorrow?" And the resources decisions force people to make that decision all the time. And we need to give them some guides. Again, if we don't give a structured guide, you'll get back to experience and hip-shooting, which have some obvious deficiencies.

And the hip-shooting is on the NRC side, too, as we all know.

So let me finish with what I think is a reasonable path to the future in this field, which I'm surprised, the degree of convergence that we've had in the last couple of workshops. People say it in different ways, but it seems to me the actions come out fairly consistent. I think that we need to do a modern, deeply detailed -- which is to say, of the order of 20 man years -- deterministically detailed PRAs with intensive technical review for lead plants of each design family. We're a long way from having this situation in place.

Then I think we can do the plant-specific PRAs. Building on that base, it's much easier to do the second one, because much of the design is in common. And you look for the differences, and hopefully it will be considerably less effort to do it more generically across the board for all plants.

However, for reasons alluded to before, it's a reasonable guess that that process is of something like three to five-year time frame before that happy state occurs.

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Then the PRA should not be something that you bind with gold leather binding and put it on the shelf. It has to be an in-house, continuing working tool. And this is now happening. Many utilities are assigning from their safety review teams, which are one of the byproducts of the action plan, they're assigning anywhere from three to five people of that safety review team to be the keepers of the PRA, which is to say that they will use it as a tool for evaluating their own experience and use it as a tool for accumulating and recording increasingly detailed understanding of the event chains.

Finally, the deterministic analysis of plant experience is now a discipline which is working well. In fact, the main concern that I have personally is the rapidity of the feedback to the implementation, and configuration control decisions. I think one of the tremendous lessons from the aerospace program for NASA, they tried many systems of this kind, both PRA and the operating experience to go with it. And the systems, the rockets that flopped were the ones where the feedback part of the process, that is, the final implementation, configuration control were weak. When those were strengthened, then the rockets flew a lot better.

I think right now, I can almost say that the analysis part is almost a lead pipe cinch, because we're now seeing so many repeaters. So you begin to get to know them by the numbers.

I think one of the things that eventually should arise

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out of this is to reach the objective of a documented and defensible judgments on the priorities assigned to various postulated improvements in equipment or procedure. In other words, really, I think both the industry and the regulation process still have a lot of hip-shooting in them. I think the documented defensible judgment is an objective we should all seek for.

And finally, my wild hope that we will allocate resources in proportion to the time integral of risk, which is -- it's a sort of thermodynamic theorem. If you part from that, you are decreasing safety, clearly. Because only if you assume resources and time are infinite can you get any other conclusion. So if you assume resources and time are finite, then you must allocate resources, both in the plant and in regulation, in proportion to the time integral of risk, particular contribution. And the PRA is a tool for doing this.

Now, finally, a thing which comes out of this, which I guess I skipped over, which is of immediate use, which is not in the three to five-year time frame, the use of the localized tradeoff process, sort of just an extension of classical engineering tradeoff, you can call as a spotlight PRA of a local part of the system. And that's available in all plants right now, and it's in common use, both in the NRC and in the industry.

I think we just don't give it -- that blend of deterministic and probabilistic, we don't give it that identity,

because we're focusing too much perhaps on the global PRA.

I happen to be rather doubtful that the verification process will come along fast enough that a utility doing the template, or doing their PRA and comparing it with some sort of risk goal template, will get very much credit for it. I think that will only work for the extreme cases. If I have a bad outlier, it will certainly say I go fix it. If it says I have a factor of 10 or 50 margin of safety over the template, there will be a kind of a grudging likelihood, "Well, even if I discount your study by a factor of 10, you're not too bad off, and I'll let you keep running."

But that's not really much different than, and perhaps in some respects worse, then the already existing body of procedures. So if I look at the template model, I think we're a long, long way from it. If I look at the local relative evaluation model, we're ready to do it right now. And perhaps that's the thing that needs to be recognized more in the documents relating to safety goal and use of PRA.

Thank you.

CHAIRMAN OKRENT: I think what I'm going to propose, instead of opening discussion on this now, since we're close to the time I had suggested for the break, is that we break for lunch, and we'll take questions that relate to Dr. Zebroski's presentation first, and then get into the general discussion.

So we'll break for lunch, and let's be back by 10

after 1:00 or sooner.

(Whereupon, at 12:10 p.m., the hearing was recessed for lunch until 1:10 p.m.)

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AFTERNOON SESSION

DR. OKRENT: The meeting will come back into being. When we recessed for lunch we had just heard from Dr. Zebroski. We might begin this general discussion period with questions or comments that relate to Dr. Zebroski's presentation.

Let me start this by asking him about one point I recall from his talk. There was this use of the term "relative risk studies", if I remember correctly, and there was sort of an emphasis on our current ability to use focus studies on segments of the problem in the near term -- and presumably these were relative risk studies in some way. While I agree there are certain kinds of studie where much of the work that you do and much of the information of interest is relative in nature -- and I will go back to the auxilliary feedwater study that was done shortly after Three Mile Island as, in my mind, an example of this. It is not so clear to me that for many of the specific issues where one might think of using probabilistic techniques that in fact it is a relative risk study that is involved.

Certainly if you are trying to balance between improving containment heat removal or trying to improve auxilliary feedwater reliability in a PWR, I find it hard to envisage how I do this in arriving at a judgment of which of these two let's say is a better way to go without bringing in what I would call absolute risk numbers.

Would you care to comment?

DR. ZEBROSKI: Well, I agree that there will be cases where it will be difficult to cook up a judgment which entirely obviates some absolute measure, but I guess I would make the much gentler point that there are a large number of cases in which the relative assessment is straightforward, is relatively unarguable since so much of the -- if you look at it as a ratio between two risk calculations, a large part of the ratio terms are identical and therefore cancel and you focus on the differences between System A and B. In the ultimate, this is not much different than the classical discipline of design tradeoff except where the measure here of worth is the probability of an unfortunate consequence rather than dollars.

So I think I have to agree with you that with a little imagination you can cook up cases in which you cannot avoid some absolute judgments -- and I discussed this a little bit at lunchtime today. As a sufficient condition for covering all conceivable cases, I'll have to grant that sometimes you will involve an absolute judgment; but as a necessary condition for making the best use of real information that you already have, I believe the relative assessment is a necessary condition, even if perhaps not sufficient in all cases.

DR. OKRENT: Well, I guess we have a different perspective on the ratio of situations where relative is a sufficient or even in fact the way to go. I mean Prof. Kerr here on my right will ask concerning the auxilliary feedwater study how did you know

anything needed to be done, even if you did see ways you could improve it. It seems to me you end up having to look at that on an absolute basis.

DR. ZEBROSKI: Yes. And another current example is filtered vented containment, core catchers, dedicated heat removal systems. These are proposed as improvements and, in a microscopic sense, I can look at alternative designs of each of these and make some probabilistic kind of valuations of alternate design. But whether I need it at all then gets you into an absolute question. I agree with that point. But nevertheless, at the very least, you should do the relative thing where it is applicable because it is far less arguable in many instances than the absolute one.

DR. OKRENT: I won't pursue the matter any more except to note, as I have in the past, I think people overuse this term "relative" as the way to go and not absolute. I will just leave it at that.

DR. ZEBROSKI: No. I think a blend of the two is fairly clearly called for and certainly in the societal risks you can't avoid the issue of absolute at some point. But let me point out that the relative risk is one of the sequelae of an absolute risk approach which you can describe as bootstrapping. I say that my average experience of a given discipline has by acquiescence been one definition of acceptability and therefore I look at a new case of whether it does better or worse than that average historical base. That might not be very scientific, but I think a great many

decisions in real life are made on that basis and there has been some scholarly treatment of this approach for environmental and nuclear risks, the bootstrapping approach.

DR. KERR: Isn't one of the reasons that one sometimes finds it easier to deal with relative risk the fact that in many cases one is dealing with systems in which the amount of uncertainty is relatively small and the probabilities of failure are frequently large and hence not quite so uncertain? So it seems to me what leads people to this is not so much that they are doing comparative risk or comparative reliability or whatever it is but that they are dealing with systems in which the uncertainties are small. If you go to the absolute where you are talking about the whole plant you inescapably get involved in those situations in which the uncertainties are large.

DR. ZEBROSKI: Even there, if you use it as a decision tool -- and again, not proving societal risk is adequate or inadequate, but as a decision tool for either plant fixes or for regulation -- clearly if a given malfunction that you perceive is a participant in a dominant sequence with very unfortunate consequences you would be inclined on a rational level to give that greater priority in remedial measures than the failure of a component which has, with great stretch of the imagination, no direct participation in a dominant sequence. For instance, we have a great many LER's which are either set point drift or calibration problems with radiation monitoring instruments in and

outside of the containment. In many cases, these are highly redundant, that is, there are many, many measurement points, and the failure of any one instrument for its whole lifetime in the time integral of risk scale would rate extremely low. You give that attention, nevertheless, because if it is a symptom of what might become a common mode or a generic problem you want to learn from it. But if it is just a one-off thing you say why are we devoting roughly a third of our paperwork in the LER process to something which is so low on the risk scale.

DR. KERR: Would you be willing to use PRA to estimate the probability that we can eliminate some of those things from the population of LER's?

DR. ZEBROSKI: Yes. We are proposing precisely that process, that we give things which are not involved in severe accident sequences a lower reporting requirement -- in fact, move them into an NPRDS kind of a system environment -- without losing some ability to retrieve the event circumstances. NPRDS does not give you event circumstances, LER does. Sometimes the event circumstances should be retrievable, but in most cases that kind of information could well go into a failure statistics kind of data base without the -- somebody estimated -- actually I think somebody connected with the ACRS -- that it is costing $3,000 a copy to issue LER's. So you wonder if you are making a few thousand of these a year which are of relatively low value, whether that wouldn't be better directed at a more detailed treatment of

the things which are clearly on or potential contributors to dominant sequences. That is kind of a qualitative use but, again, a relative one which I think could have some merit.

Similarly, in the NPRDS I think one of the things that is being discussed -- as you know, the NPRDS structure will change next year -- one of the things that is being discussed is that if you are going to go for duty cycle or frequency of challenge data and you want to do this across the board for 5,000 components you are talking dozens of manyears per plant and it just won't happen. The resources don't exist to make that happen. On the other hand, if you say I have some sort of a selection process that I really want this kind of duty cycle and challenge data on a limited number of key systems or components, that becomes feasible. Here again, you can use a PRA kind of logic tool to identify at least with some validity the things that warrant this kind of treatment. That is not in being yet, but we are discussing it in a number of the committees and have a couple of studies going on trying to help that identification fall into place.

DR. OKRENT: Other questions for Dr. Zebroski?

(No response)

DR. OKRENT: Let's see. We are in what's called the general discussion period. Mr. Houghton indicated earlier that he had some comments to make at this time. Why don't we begin with him.

MR. HOUGHTON: In these two days we have heard some

good presentations, and in this case I refer particularly to those that talked about using PRA in the design process as well as safety. The team at General Atomic has done this in the past and continues to do this. We do this on a trial basis because, as has been abundantly clear here, you can't do this on a final basis. We set tentative goals for safety and some of these are the limit line.

I find in working with this that the limit line approach and the integral approach are not necessarily so different and in fact our utilization of limit lines based on families only has about a 50 percent difference between the limit line approach and the integral approach in your judge frequencies.

I also suggest it may be possible to interpret your criterion and the subcommittee's criterion -- criteria -- in a limit line context, even though they are basically integral statements.

We also within the company utilize an integral statement which is a limitation on frequency of core heatup, analog of core melt, which is in fact an integral statement. We impose on these additional engineering management criteria, in particular, margins. We are not willing, even on a trial basis, to come within 10 percent of the limit lines or limit integrals and we put margins on these. Of course these are particularly suitable when you are in a conceptual design phase or preliminary design phase.

Uncertainties are indeed a problem. The more nearly

subjective uncertainties really get dealt with on a management-judgment basis. But the analytic uncertainties, the statistical uncertainties, we handle on a first order basis by using mean values, which is also what you suggest in your criteria.

DR. OKRENT: Excuse me. I don't want you to think we were suggesting using first order mean values.

MR. HOUGHTON: I was just referring to the headings and I think the text would say "mean" as opposed to "median".

DR. OKRENT: Okay.

MR. HOUGHTON: Just a simple statement. When we do the PRA analyses on the plant we do include common mode in the various ways. Just as a side comment, we have upon occasion found a system which was believed to be designed to the single failure criterion which in fact resulted in common mode failures giving the potential for core heatup that was much too high. And we had that design changed. It was done on the basis of safety, but when you look at the numbers, one also does it on the basis of investment risk because we didn't want one plant out of fifty or so to have a major accident, even if the doses were in fact negligible.

The review processes we have used in the past have been peer review and have consisted of on-going review by EG&G people as well as subsequent reviews by KFA and so forth. At the current time, our review processes are much more limited and in fact involve engineering and management much more than they used to.

From the PRA analyses we currently develop interim reliability criteria, of course particularly oriented to safety systems, and we put margins on some of these that are less critical in terms of dominant accidents. This is the principal way that we go beyond the single failure criterion. These reliability criteria do involve explicitly the common mode problem. Even though it would be approximate in the safety area, it can be dominant.

The application of all of this is interacting with the designers and there is indeed a lot to be learned here. There is no single person who can be trained to do everything in PRA. The designers teach us about in fact how their plant does work or can work or can be modified and the people doing the fluid flow analyses help us to make sure that the phenomenologies are done as best possible. In fact, we end up contributing -- the probabilistic people -- end up contributing to them an insight to the failure data base that in fact is not always emphasized in engineering disciplines directly.

Even after this, it ends up being a matter of judgment as to whether you start to change your design. You have to consider the development problems. We have to consider the licensing considerations because these things take time to implement, in this sense. For example, we don't even know, of course, what the NRC rules will be for probabilistic aspects of licensing and we have to anticipate and make judgments there as to which way to go. We of course need utility acceptance. As you can probably

tell, some utilities are very strong in this and some perhaps don't use it very much. And then of course costs are a matter as well.

This is no doubt the same kind of judgment process used today by people like Consumers Power. I grant you that it is not quite the same process that you deal with in trying to make recommendations to NRC as to how this can be more formalized in the licensing process.

So we have hopes that this kind of work with PRA will yield better safety and better acceptance in the regulatory and public forums. As we learn better and as we learn to reason with PRA, we hope that the reasoning becomes ascendant and that we can get away from the potential problem of having hopes triumph over reason.

Thank you very much.

DR. KERR: I'm sorry. Which one did you want to triumph, hope or reason?

MR. HOUGHTON: Each in its turn.

DR. OKRENT: Do we have some other comments or trial positions or anything of this sort that members of the audience would like to advance?

MR. BRAID: My name is Bob Braid and I am from Oak Ridge National Laboratory. I am a political scientist, which may give you an idea of what my perspective may be on this. I sympathize with the situation the subcommittee is confronted with and I think representative of the NRC in trying to figure out just how they

synthesize what they have heard here in the last day and a half. I have had some of the same problems. I think we made some progress along that line, particularly today, in trying to determine what the NRC should do with PRA in its licensing process. But I am not convinced that we have gone nearly as far as we can go.

The reason for that, I think, is probably because we have only heard from half the side in PRA. I think there is another side to be heard, and that is basically that which represents the public's perceptions of just what the risks are in nuclear licensing procedures. That's not to say that they may be anti-nuclear, it may be to say simply that much of the public just doesn't know what the situation is.

I think that probably the job of the NRC is to try to figure out just what sort of mechanism they can use to be able to take all the information of the type we have seen presented to us in these studies for the last day and a half which, if I may use two extremes, is out here on one hand and the public's perceptions, which may well be over here on another hand, and try to bring those together, to bridge the gap at least to some extent to where the NRC can fulfill its job of not only protecting the public but also serving as somewhat of a spokesman for the public at the right time, being and advocate for the public.

I think this is particularly important when it comes to PRA. The public's level of knowledge, I suspect, is quite low as far as the art of PRA is concerned; my own is also. We've had

it pointed out to us that it is a subjective art that has a fairly wide error band, that it calls for some rather sophisticated techniques from engineering and math disciplines for the public to be able to absorb. I would argue that the process that we are using at the present time, which is to have the experts go through their PRA, send it to a review panel composed of other experts, send it then also to the NRC, which has its own experts, and then have the NRC finally put its stamp of approval on it and then announce from on high in Washington that this is indeed what the risks are of a particular plant will not probably fly too well with the public because the public simply does not know enough about the situation.

The public is ignorant of these practices, it is somewhat distrustful of them. It is not sure of the NRC. It regards the NRC oftentimes as an institution which is going to impose decisions on them. I think this is wrong quite often; nonetheless, this is the way much of the public perceives it. So I would argue that the process might be the following, that I would recommend -- at least it maybe should be tested.

When a utility decides it is going to possibly site a nuclear power plant, go ahead and do the PRA at the outset. The utility is supposed to be doing alternative site analysis. When it identifies four or five sites at which it is going to locate a nuclear power plant, do PRA for each of those. It shouldn't be that much more difficult. You've got the same plant but you've

got different communities. At the same time, permit the communities themselves to be incorporated in the process to where they can go through this process in part with the experts who are doing the PRA and in part with their own people that they choose to pick from both the community and also from maybe outside the community to pose the questions in terms that they can understand. Let the community help decide this question and certainly be better informed about the ways that the risks are going to be measured, the types of risks that may occur, the magnitude of the risk, the error bands, and then let the dialogue come about at the community level with the NRC present and then, when the NRC does its EIS, have the findings of this process brought out in the EIS. I would suggest that if PRA really becomes institutionalized that the EIS is going to have to discuss the findings of that PRA anyway.

And if it does that, then it has to incorporate public perceptions because public perceptions are important. I would argue that risks are in part in the eyes of the beholder. They may be wrong. It may be that much of the public's fears about nuclear are indeed ill-advised, but that is irrelevant when we are talking about the impacts of the decision to site a plant at a particular location. If a percentage of the population feels that it is going to be endangered by nuclear and chooses to dismiss the findings of the experts, the arguments of the experts, but still has the plant imposed upon them, then there are certain emotional burdens -- I think Prof. Kerr alluded to them earlier --

which that segment of the population has to experience for a long-term basis. Consequently, that is an impact from the decision to license the plant that the NRC simply has to take into consideration in its licensing decision and later on may well have to take into consideration from a mitigative standpoint.

Thank you.

DR. OKRENT: Dr. Siess?

DR. SIESS: Since the last speaker brought up the point about people who weren't here, I'd like to suggest that since we are talking about the use of PRA in the licensing process and the licensing process as it is practiced has a very, very extensive legal content, I think at some point we ought to try to hear from the lawyers. I suspect the Executive Legal Director, if they have looked at this, might have some ideas as to how this fits or what constraints might be placed on the use of PRA in the licensing process as we know it. I am making a distinction now between licensing process and safety analysis.

DR. KERR: I don't disagree with your comments, most of them, about public perception and the need for taking this into account. I do wonder a bit about the recipe that you suggested for amelioration of some current problems. I wonder, for example, how one would identify who the community is. If I took some examples of plants in which there has been a considerable amount of opposition, take Seabrook, for example, I don't think much of the opposition has come from the community. It has come from a

much broader representation of the Northeast and, indeed, as far south as New York City. I am not quite certain how one would get that particular community involved with the process, nor do I see that such involvement is likely to reduce the opposition. I can think of other examples.

Indeed, it seems to me that opposition from the very local population to nuclear power plants tends to be more the exception rather than the rule. I think the opposition frequently is a much broader community than would be feasible to reach in the way in which I thought you were suggesting.

MR. BRAID: I would agree that the question of who constitutes the community is a problem. I would have in mind basically the local communities which would be impacted most directly by the siting of the nuclear plant. I would be inclined to deal with it on a site-specific basis. And I would leave much of the identification of those communities or those interest groups up to the communities themselves to determine. The NRC role would simply be to make sure that this was done as fairly and openly and with as least arbitrariness as possible.

Now as far as the national interest groups is concerned, I think this would probably have to be tackled in more of a generic sense. It may well be that NRC will have to establish some other procedure to get at the role that national interest groups would have in the whole question of nuclear versus coal versus some other type of energy source. And I think it is probably ill-advised

to debate the question of nuclear versus other alternative sources of energy each time you license a nuclear plant. I would have that brought out in the comparative risk assessment, though, that I would think ought to be a part of this, within the context of the local communities as to whether it wants to have a nuclear plant sited there, a coal plant, or go conservation, whatever the case may be.

But I would be arbitrary in the sense that I wouldn't think the NRC should be obligated to have to debate the nuclear issue every time it wants to -- with national interest groups -- every time it wants to site a nuclear plant. I think it could handle that in more a generic setting.

DR. KERR: Thank you.

DR. OKRENT: Dr. Cornell.

DR. CORNELL: Cornell, of M.I.T. would like to make a few comments about the general discussions of the last couple of days with respect to the role of PRA or probabilistic analysis, let's call it -- PRA seems to be a bit redundant. Some of this comes from experience in civil structural building area, where we have rather successfully introduced probabilistic bases for some of our licensing or code -- in this case, building code -- work.

It is clear that there are a variety of places where -- at least four distinct levels I can come up with in which PRA kinds of studies fit and I think in each of those levels questions that

we have been really concerned with today, such as the importance of uncertainty treatment, the importance of cost benefit analysis, the importance of comparative versus absolute probabilities enter in very different ways, I think.

The first of these levels might be those that we have heard people talk about where it is used in industry, by designers, individual designers -- by GE, for example, or whatever -- to help them understand how to come up with safer designs, safer drawings, let's say, of plants that are supposed to work better. And also perhaps in this class are those situations where the utilities are supposed to learn more about how their plants work and they are supposed to get better operator benefits and so on by doing these studies.

In these kinds of studies I don't think we need to decide whether uncertainty analysis needs to be included or not. I don't think we need to do any regulating of how these PRA studies are done. They are going to be done in innovative ways by different people for different purposes. And this probably applies also to the research application.

Another level is this question of setting safety goals or public policy levels of decisions, that is, where one is in a sense designing codes, designing regulations, maybe even designing these stylized methods which we were discussing as a next level. Here clearly uncertainties -- the necessity to display both the probability estimates and the uncertainties are important in the

process, at arriving at decisions as to what form regulations are going to be in. It does not necessarily imply that the regulations must include formal uncertainty analysis -- to me it does not. It is clear to me in those safety decisions or safety goal setting levels that cost benefit analysis is a requisite part. In that part of the study which is not absolute probability but cost benefit, one does not need, again, explicit uncertainty analysis. One needs only mean probability. That may require some uncertainty propagation, needless to say, in certain situations.

And in that case certainly one is trying to deal with absolute probabilities as opposed to comparative probabilities.

A third level would be perhaps these stylized studies that we are talking about which could fit well into a regulatory process. They become part of, in a sense, the rules by which the game is played just as allowable stresses are today in other studies, they become a set of regulations. One doesn't necessarily look for them to be internally terribly consistent, internally to be complete and a variety of other things. I think for those kinds of stylized methods, again, it would be quite possible to set these up without explicit uncertainty analysis, with only best estimates of probability placed into an analysis procedure.

A large number of prior decisions have to be made to fix those stylized methods, obviously. One of them is that one should use event and fault trees. That is not an obvious decision a priori, but it seems implicitly to have been made. Others

might well be exactly which elements should be included and which elements should not be included. For example, as Dr. Rowsome was suggesting, it may be useful at this point to exclude -- and I would agree with this -- exclude the seismic considerations initially. Nonetheless, many of the benefits which have been attributed to PRA, that is, the qualitative benefits, the direct review of plant characteristics, but even something more, maybe ranking of relative sequences and so on, can come out of these studies without the complexities and disadvantages of trying to do explicit uncertainty analysis at the same time.

I think the uncertainty analysis will make the licensing process more difficult. I would very much encourage those stylized procedures, however, to be the type of dynamic framework that was proposed and that such things as seismic be excluded initially and added later, as we gain rather a facility with doing those studies easily and economically.

I would like to clarify my comments yesterday. I do think that -- I do not think that the SS/MRP level of detail, for example, in seismic studies is economically feasible for routine PRA studies. It is obviously crucial. There is nothing else going on to help us learn to develop tools that can use standard structural calculations as the basis for developing routine PRA's. We need to have that step of the more detailed analysis at that point and SS/MRP will hopefully develop those kinds of capabilities.

I think that maybe a final level would be that of what I might call comparative decisions, at least decisions which could be set up in a comparative basis. These would be such things as retrofit decisions. Again, like the preceeding level, the stylized method, these are basically safety checking as opposed to design or decisionmaking. That is, they are checking whether the design proposed meets regulations. And although in retrofit studies it may be desirable for the designers to consider cost benefit analysis to decide what is the proposed way of meeting the regulations, what is the most economic way to meet the regulations, in the process of checking whether that proposed design is satisfactory it becomes basically a question of using regulatory tools. I would suggest that those might well be in this category of stylized tools as well, again perhaps suggesting that, first, explicit uncertainty analysis would not be necessary -- that has yet to be demonstrated -- but that -- and furthermore, that perhaps some of the probabilistic statements that are made might in fact be relative probability statements in the sense that one is now suggesting that in this particular part of the system that one is attempting to retrofit it comes out with a relative probability, a probability which is lower than some standard, which is not quite the same as saying it meets an absolute standard.

End of comment.

DR. OKRENT: Dr. Cornell's comments raise one or two points to my level of interest, where I will pose some questions or comments. I think during the meeting there has been a lot of emphasis on not using conservative calculations but best estimate calculations in doing PRA's. Sometimes people say best estimate mean. And a moment ago, Dr. Cornell mentioned perhaps we don't have a need for explicit uncertainty analysis for certain applications.

I guess I, myself, have a little bit of concern that the term "best estimate" may be receiving a lot of emphasis without the necessary qualification. The best estimate may be wrong. It may be very wrong, in fact.

DR. KERR: Are you insisting that the best estimate be a good estimate?

DR. OKRENT: Well, you have alluded to one of my problems, of course. The best estimate is not necessarily a good estimate. That's right. And the larger the uncertainty band, the better chance there is that the best estimate is not a good estimate, presumably. So if we don't make a very serious effort to not only carry along but display the uncertainties that go along with the estimate, I guess I have a concern about the trend. I think we may end up in a position where we are not necessarily on a sound basis for making the decision using these best estimates or point estimates or so forth.

There is another part of it, too. It seems to me that

if the uncertainties are large, even if you have somehow factored them in to a mean calculation -- and how one does that is not crystal clear -- the knowledge that the uncertainties are very large or might be very large somehow has to be factored into the decisionmaking. For example, if we get back to the safety goal question, when Griesmeyer and I were trying to work on proposed numbers and a proposed approach, for a long time we favored using 90 or 95 percent confidence limits for acceptable risk, let's say, to an individual rather than the mean.

I think in part is is because we were making comparisons with the things for which there was statistical information. I mean we do know what most of the other risks are that the individuals are facing statistically. We have pretty good information on the accident risk, et cetera. And here we were comparing something that was much more uncertain and trying to say well, if it is a factor of -- whatever it is -- x smaller, it is smaller, when the uncertainty band in fact might overlap considerably the risks that we were saying was smaller. So our thinking was maybe if we said 90 or 95 percent confidence that would remove that concern.

I guess we were doubtful we knew how to calculate a 90 or 95 percent confidence number unless it were a very big value indeed for some of these things and so we retreated, I think you might say, to expected value or mean value. But I'm not sure that it necessarily meets the needs.

I think the same kind of question about large uncertainties really enters into what are called these narrow technical areas or focused things. I am not, for one, satisfied that it is not equally important to look at the uncertainties and have them reflect into the decision that you are making if it is just a question of should these auxilliary feedwater systems be seismically qualified or whatever it is that is the current narrow issue.

Well, let's see if I have raised any sparks of dissent. I see a hand.

MR. SMITH: Paul Smith. I don't think I have any sparks. Just a couple of points. I think -- I don't want to put words in Alan's mouth -- I think my interpretation of what he was saying is that you can work in analysis at a deterministic level, for example, as we have done, you can work in analysis at the probabilistic level and come up with just probability of release or risk number and that's it, and then you could also, in addition to that, come up with the risk with some uncertainty. These are perhaps three classes of analyses and we could come up with more.

Each has its own validity. Each presumably in at least the scenario I have laid out adds more information to the problem. But you really don't have a well-defined stopping point because, for example, we have heard the word "uncertainty" and "uncertainty analysis" used, but that is not a well-defined term. You could go through with a certain risk analysis and do a very, very vigorous one and then a very vigorous uncertainty analysis. And

ultimately the stamp of approval that that uncertainty analysis was an adequate one is an expert judgment. It is either a judgment by a peer review group, the people who do the analysis or whatever, that yes, we have examined the problem, we have looked at the different aspects, and we believe that the issue of uncertainty has been addressed as vigorously as we would like to for the purposes of whatever the exercise was.

But ultimately that's a judgment. That is a judgment. It is not a well-defined term. You can get into a lot of controversy -- and we did at one point in the SS/MRP on this uncertainty analysis. It really was helpful to me to understand that it is not a well-defined term. There are no universally agreed-upon methods and procedures accepted by everyone of an uncertainty analysis.

I guess the second point I'd like to make is that we've heard throughout the two days allusions from time to time on topics such as dominant sequences and in one I thought thoughtful presentation yesterday I heard a problem alluded to that I think is of interest. But I think we need to see more emphasis in PRA on essentially expanding this issue of uncertainty, on decisions in the face of contributors to risk, for example, dominant sequences, as I interpret what I have heard. But there also is another class of decisions which may be the same or may be different on decisions in the face of contributors to uncertainty in risk, all the different classes of decisions that we have seen identified.

And I just haven't seen that issue addressed, and maybe we need to. It would help clarify this whole uncertainty issue to try and identify which decisions are best made based on which information, whether it is contributors to risk, say a mean value of dominant sequences, that whole thing, or decisions based on contributors to uncertainty in risk. It is very hard to separate these two, but you will find yourself doing it quite a bit.

But I would like to see in at least the write-ups that are going on, the continuing discussions that are going on a more vigorous exercise of that and I think it might help clarify certain aspects of the issue.

Thank you.

DR. OKRENT: I think that is a good way of putting it. I like that. I see a hand. Is that Dr. Joksimovic next, and then up front.

DR. JOKSIMOVIC: I would like to endorse what you said, Dave, with regard to knowledge of uncertainty and probability distribution being equally important. Sometimes it is more important than just knowing what the mean values are. I recall that I was in a position of a decisionmaker on a number of occasions and I refused to make a decision on the basis of just best estimate of mean values. I insisted on knowing what the probability distributions were. Typically that happens when you have a number of alternatives and you have to make a decision which one to choose. It could be very misleading to pick an alternative simply on the

basis of what the mean value is suggesting. The probability distributions are much more telling.

MR. ABRAMSON: Lee Abramson, NRC. I very fully support what Dr. Joksimovic just said and I would like to add something else about the use of the term "best estimate" or "mean value". If you have any kind of a large uncertainty and you use the mean value or the best estimate you know for sure that it is not going to be the right value, as Dr. Okrent mentioned. And I think by using the term you are kind of implying a validity to it or a meaning to it which really doesn't exist. You are trying to dress it up into something. There is a danger that it will be misinterpreted and it will be given more credence than it really deserves.

What it really amounts to is your best judgment and engineering guess or something like that. But don't put it in the trappings of pseudostatistical trappings of best estimate, mean value or something. What is it the mean of? You don't even know that. If you have to insist on using it, at least don't give it any more validity than it has, which often I think isn't very much.

DR. OKRENT: Dr. Kerr?

DR. KERR: I think another advantage of insisting on knowing the probability distribution before you make a decision is that it permits you to put the decision off several days.

DR. OKRENT: Houghton.

MR. HOUGHTON: Bill Houghton. Certainly it is true that if one has a table of values one knows a lot more about the frequency. But in those cases where you need to deal with some kind of a single number as a characterization there is another small technical point that may be of interest to you. It may be that there is a more fundamental requirement than a frequency, for example, that you don't want to have more than 1 in 300 reactors have a large scale fuel melt.

Then if you do have some idea of what the probability distribution is on the frequency, by theoretical means you can come up with formulas that will give you a number, a single number, which is plugged into some assumed simple distribution like an exponential and will in fact give you the likelihood of the one plant in 300 taking into account all the numbers in the distribution that you have.

Indeed, that is a mathematical artifice,but it does provide some bridge between some of these ideas.

DR. OKRENT: Mr. Hickman.

MR. HICKMAN: Along the lines of Dr. Kerr's comments, I think there is one other advantage on insisting that we have the distribution before we make the decision, and that is we won't have to make many decisions because we just simply do not have those distributions.

DR. OKRENT: Yes?

MR. ERNST: I am Malcolm Ernst, NRC. I think it is quite

clear to everyone that we still must make decisions, in spite of the uncertainties; although we want to understand the uncertainties the best we can. I think we went through the same process that you mentioned, Dr. Okrent, in coming up with a draft or tentative draft implementation process for safety goal and how does one usefully use a safety goal in making decisions.

One perspective that at least I went through in my thought process was that we have extant regulations governing reactors at the present time that have generally served us pretty well. So in overlaying or using as an adjunct PRA in the licensing process, it seemed to us at least initially that it might be useful to use generally best-estimate values as an augmentation process to see if there are any areas which perhaps should be beefed up, but to use a more conservative analysis if there are occasions where existing regulations are deemed to be not necessary, either in the backfit basis or maybe in the foreseeable future starting to elimination some regulations.

So that is a possible way of treating uncertainty in regulation. In this vein, I think I might disagree somewhat or to some extent with Ed O'Donnell's discussion this morning, where I seemed to get the feeling that the use of PRA in the licensing process was perhaps more biased towards decreasing the amount of regulation. My perception is that PRA is a useful tool for improving safety as well as perhaps drawing back from some of the regulations. I think I would prefer a better balance that way, to

use PRA in its beneficial sense to try and understand the weaker elements, if possible.

DR. OKRENT: I see a hand at the back.

MR. O'DONNELL: Ed O'Donnell. I guess I must apologize if that was the impression I left with the audience that we were proposing to use PRA in safety goals to back off from existing regulations. That is certainly not the intent of the AIF proposal. What I tried to show was that we have to start from where we are now and make decisions on need for change and that is in both directions; that is, need for increased safety and if there is a need to remove some requirements that may be particularly onerous. I think the safety goals can be used in a non-discriminatory sense such that it provides a measure of discipline both on the regulator and the industry and that will enable decisions to be made on a neutral basis or a rational basis.

That is why some of my comments related to burden of proof. I think very clearly put the burden on the party proposing to change. In the case of the industry, if one is proposing to eliminate requirements or reduce requirements, the burden clearly rests with the applicant or the industry who is proposing that. In that sense, I think uncertainty may play a role, as I indicated, without having arrived at any specific conclusions on that. Certainly the burden of proof resting with the industry may require demonstration that uncertainty is taken into account and the change is justified.

On the other hand, on the NRC proposing a new requirement, again, if we have established that the primary goals have been met, I think it is appropriate to place the burden of proof on the regulator if there is in fact a proposed new requirement. Again, here we are looking at cost-benefit and, again, the uncertainty may play a role in that direction where the presumption is that the plants are already safe or at least with respect to these primary goals, and the uncertainty may have to be taken into account in the other direction.

But I view this as an even-handed process and not something that is going to be used to justify change.

Underlying all of this I think is the basic tenat that we are not going to depart from concepts such as defense-in-depth which have in fact served well -- and TMI I think illustrates that quite dramatically.

DR. OKRENT: Other comments? We have a few more minutes on the agenda. Dr. Kerr?

DR. KERR: Mr. O'Donnell, I think I understood what you said but let me ask what I hope will be a clarifying question. Assuming that the goals have been met, you then would suggest that if the licensing body asked for changes that they would be responsible for the burden of proof. Now the burden of proof would involve an evaluation based on some ALARA recipe, is it?

MR. O'DONNELL: Yes.

DR. KERR: But suppose they simply said we want an

additional improvement in risk? They couldn't do that, apparently, because you have already met the safety goal. But if one said we will use ALARA, then they could say come up with so many dollars worth of improvement or something and I am not sure that I know what burden of proof means in the ALARA context. I am trying to think in terms of Appendix I where, if I understand it -- and I am never sure I do -- the applicant satisfies the numerical criteria and then he is required to look at available technology.

In that sense, is the burden of proof on the applicant or on the NRC, from your view?

MR. O'DONNELL: Well, I think the issue is one where the NRC would not say to the industry give us any and all changes that meet the cost-benefit criterion. We would have to look at specific proposals, that is --

DR. KERR: Let me, if I can, using an analogy with Appendix I. As I understand it, the licensee is required to look at least at all available technology. Is that the way you understand it?

MR. O'DONNELL: That's not what is done in practice and I am not sure that is practical. I mean --

DR. KERR: Tell me what is done in practice.

MR. O'DONNELL: Well, what's done in practice is the NRC has set down a set of deterministic design requirements for right away systems, some of which have been tested against the thousand dollar per manrem, some of which haven't. It is not in fact a

true application of cost-benefit methodology, but in fact certain things such as off-gas filters on BWR's have been tested against the thousand dollar to manrem and have been found to be acceptable and have been incorporated essentially into the deterministic requirements.

DR. KERR: What happens if the price goes up by a factor of 2?

MR. O'DONNELL: Well, I imagine one would be able to argue the point.

DR. KERR: What you are telling me is that Appendix I is not interpreted it literally, as I would have interpreted it, but there is a reg guide somewhere that says these are the things that meet the cost benefit criteria of Appendix I, sort of. Is that the sort of thing that you were suggesting when you said the burden of proof is --

MR. O'DONNELL: Well, yes. I would -- the burden of proof, if someone comes forward with a new requirement, let's say filtered vented containment, the person proposing that, you know, if it happened to be the NRC, as a new requirement that is aimed at reducing that residual risk --

DR. KERR: But suppose the requirement simply said some sort of extension of Appendix I, you must use any available technology.

MR. O'DONNELL: I think that would be an impractical use of the concept.

DR. KERR: I think I understand what you mean.

MR. O'DONNELL: And I think you have to apply -- someone has to come forth with a specific proposal, whether it is the NRC or the applicant wants to take something out, and then justify it.

DR. KERR: So if the NRC would say we want you to install filtered vented containment because we are convinced it will -- and they would have to prove this before the licensee would have to do it, in your view?

MR. O'DONNELL: Yes. Right.

DR. KERR: So in a sense, they would have to do the design, do the costing, guarantee that the cost was a valid cost.

MR. O'DONNELL: Yes. They would have to defend the proposal. As they do now. If they come forth with a new requirement now there is ostensibly a requirement that they do some sort of -- that they have some defensible basis. This would be a matter of incorporating --

DR. KERR: Until I got your interpretation of Appendix I I would have assumed that it would be up to the licensee to see what available technology existed and to demonstrate that he did or did not have to use it. I think I could write a requirement like that that would require the licensee to do the evaluating. But in your view, that wouldn't be fair.

MR. O'DONNELL: Well, I think it would be open-ended unless I think someone -- if you wrote a requirement --

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DR. KERR: I think it would be open-ended, too. It would depend on what available technology existed and what it cost.

MR. O'DONNELL: Well, if one had a rule that merely said that the applicant must do all things that are -- make all changes that are --

DR. KERR: Make use of available technology, say, that will decrease risk and won't cost more than whatever number you want to use -- you suggested what, $100 per manrem.

MR. O'DONNELL: Yes.

DR. KERR: In your view, that would be impractical.

MR. O'DONNELL: Well, I think it would constantly open the issue as to what is in fact available in terms of technology and among what is available, what is cost beneficial.

DR. KERR: Yes. I think it would, too.

MR. O'DONNELL: I think it would be a very unstable regulatory process under those conditions.

DR. KERR: But it would encourage the innovation which somebody suggested this morning, maybe.

MR. O'DONNELL: Yes, it would. And I don't -- I see no reason why, under a regime whereby the regulator or anyone else who feels that he has a piece of available technology that meets the requirements, could propose that in terms of a new requirement.

DR. KERR: Thank you.

DR. OKRENT: I think this past discussion has been quite interesting in that it points out a somewhat different concept of

ALARA, as you have described it, in which I assume you mean to apply both for existing plants and for future plants, as contrasted to the ALARA recommendation in NUREG 0739, which was intended to apply only to new plants, plants to be designed. The proposal there was not that having met the goal, a new plant would be sort of home free unless the NRC came in with some cost-effective change over and above its existing regulations, the intent in fact was to allow the designers to be innovative, to look at their plans, to see in fact what was still cost-effective with regard to reducing risk, and to give the designer the responsibility for including these in his design. And if he did a good job, in fact, no one would have an additional cost-effective improvement. If he did a poor job, I suppose, the NRC or some third party could point a finger.

So in fact there is a somewhat different philosophy there and I think our feeling was what is sauce for the goose is sauce for the gander, if I remember it correctly. In any event, it is fair to ask the NRC to use ALARA, but it is also fair to ask the industry, and certainly on new plants.

It is not completely clear to me that it is impractical to do that for new plants. I think if you or I were given the job of guiding the design of a new plant we could probably structure an approach using an ALARA to improve the design after we had met the goals. In the first place, we have already heard that the utilities want you to use an ALARA with regard to what was called

investment risk protection or some such word. They already have that in mind. Of course, some of the things that one might choose might meet both of these goals, but not necessarily.

Anyway, it is worth thinking about the fact that there is a seeming similarity that is not quite similar in the two approaches.

MR. O'DONNELL: Yes. I guess I'm just beginning to get a full understanding of what NUREG 0739 meant. But I would agree that -- my comments this morning were mainly directed at the regulatory and licensing uses of PRA. I tried to make clear at the outset that I thought PRA had much broader uses in terms of internal management tools by utilities, by designers, by vendors, in improving design. So in one sense, most of my discussion this morning was on the narrow questions of regulatory decision-making.

The ALARA concept I think does play a very important role, as you said, aside from whether the NRC is making you do it or not, if it were in fact in place in stimulating innovation, certainly in a competitive atmosphere whereby that innovation was directed towards reducing the probability of accidents in the first place or prevention, that if GE came up with a new design, as we heard yesterday, that vented containment that in fact was not only cost-beneficial from the point of view of ALARA but was helping to protect the utilities' investment, I think that is stimulus in itself to help the industry make sure that they are constantly looking at these things and introducing them.

The question of whether the NRC has to require you to do it or to prove that you have done it as part of the licensing process is a different matter.

DR. OKRENT: Yes. I think the Congress in some of the laws that passed setting up the EPA and giving them a job to do on occasion has said use best available technology, on other occasions has said, in effect, use best available technology which is cost effective in reducing risk. I suspect the Congress might do well to include words like that the next time it appropriates money for the NRC and then they would give the industry the guidelines and maybe -- I mean for future plans -- that would then perhaps at least give the same kind of guidance that they are giving to other regulatory agencies.

MR. O'DONNELL: Yes. I think the industry would very much like to see -- to clarify the issues in some legislative changes in the Atomic Energy Act that would very clearly give the NRC both the authority and the responsibility to look at cost-benefit balancing.

DR. OKRENT: Well, I think we had best go to the next agenda item, which I will explain to the others present is sort of an in-house matter. The subcommittee members have been asked to look at a couple of draft documents to see whether they have in mind some way in which the ACRS may wish to use them in elaborating on what's in NUREG 0739. I wonder, have any of the subcommittee members had a chance to look at these and offer any

comments now?

DR. SIESS: We have read them, Dave. Which one do you want to talk about first?

DR. OKRENT: Take your choice.

DR. SIESS: Let's take the discussion of some comments. Do you want this on the record?

DR. OKRENT: I don't care. We don't need this on the record for now because it is sort of an in-house thing. We will give you fifteen minutes' rest.

(Whereupon, at 2:25 p.m., there was an off-the-record discussion among the ACRS members of Draft Elaborations on NUREG 0739.)

DR. OKRENT: The meeting will reconvene. Dennis Rathbun, from the NRC Staff, will open up the next topic, I believe.

MR. RATHBUN: I am Dennis Rathbun. I am Acting Director of the Office of Policy Evaluation. I am going to give you a few remarks on our paper, which we put a substantial amount of effort into and worked up cooperatively with various members of the NRC Staff and outside consultants as well. Also, I am going to give you some of my impressions, anyway, based upon the second workshop. Following me, George Sege, who is the major project guy in our office on the safety goal, will give you some additional thoughts that he has had on the same subject.

We prepared a draft paper presenti..g elements of a

reactor safety goal for discussion at our second workshop. As our discussion paper notes, we presented three broad options for consideration in this workshop: continuation of the present NRC safety policy, adoption of qualitative goals, and adoption of quantitative goals based upon qualitative goals.

Our own belief is that adoption of quantitative goals based upon qualitative goals of the type contained in our discussion paper is potentially the most useful for guiding regulatory actions relevant to nuclear reactor power plant safety, particularly in light of the recent trends toward an increased use of probabilistic risk assessment in regulatory analyses.

However, in stating that, I hasten to add that we must find a defensible basis for quantitative safety goals and a practical mode for use of such goals or statements in regulatory decisionmaking. I am particularly concerned about this particular aspect. While we believe that the quantitative safety objectives would be most useful, realistically we recognize the possibility that it may be very difficult to develop satisfactory quantitative goals. In that event, an alternative would be to formulate qualitative safety objectives and continue the present practice, perhaps augmented with the Commission's statement which further interprets the Atomic Energy Act's standard of adequate protection of public health and safety.

Let me just go over quickly what our goals were that we offered for the workshop consideration and then some reaction

that we got from the workshop. Our discussion paper proposed three such goals, one relating to individual risk, which we believe is important; one to societal risk, also important; and lastly, one to reactor design.

The first goal is stated as follows: that the estimated mean probability of fatality from an accident at a nuclear power plant should be less than some number between 5 and 10 in a million per year to individual members of the public living or working in the vicinity of the power plant throughout their lives. We believe that achievement of the first quantitative goal would insure that the radiological risk to individual members of the public in the vicinity of the nuclear power plants would be small relative to risks from other hazardous activities and technologies.

The second goal is stated as follows: that the statistically estimated mean fatalities per thousand electrical megawatts nuclear power plant capacity should be less than two per year of plant operation. This goal relates to the aggregated risk to society and is needed to take into account the size of populations, demographic factors, at some risk and efforts that should be expended to mitigate the consequences of major accidents.

The third goal relates to reactor design. That goal is that the estimated mean probability of a nuclear power plant accident which results in a large-scale core melt should normally be less than 1 in 10,000 per year of reactor operation. The public risk associated with serious core damage is of vital concern

since it can lead to major release of radioactivity. We believe that this goal specifically focuses -- that a goal of this type which is specifically focused on prevention of accidents would be very useful.

With respect to the implementation of the proposed goals, proposed quantitative goals, we suggest that the goals should not substitute for NRC's regulations, that is, the regulations in 10 CFR Parts 50 and 100. Individual licensing decisions should still be based primarily on compliance with the Commission's regulations. What we believe should be required in implementing the quantitative safety goals is to develop and disclose the relevant probabilistic risk estimates, along with the underlying assumptions and uncertainties for consideration as one factor among others in regulatory decisions involving major safety issues when those decisions relate to rulemaking or decisions not covered by the regulations per se, such as questions of retrofit of existing plants, exemptions from the rules, enforcement actions, and the like.

In this way, each proposed safety decision could be related to quantitative safety goals with due regard to the nature, range, and potential consequences of the uncertainties that are present.

Establishment of a defensible base for the use of quantitative safety goals leads to the controversy of just how to specify in numerical terms the concept of how safe is safe

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enough. Decision analysts and other professionals who have studied this problem have highlighted various logical and methodological difficulties. We had some discussion at the workshop on this and we are still only a couple of days after the workshop and we have yet to decide exactly how we should proceed in a major overhaul of our paper.

We noted there that some risk of death for an activity with the scope and value of nuclear power is unavoidable but clearly a large number of deaths, including statistically estimated deaths, would not be acceptable, at least not without some compelling reason. As the literature in this area suggests, there is simply no sharp line beyond which acceptable becomes too many.

Another aspect of formulating quantitative goals which we found -- is another aspect which we found particularly troublesome. Agreement that zero risk is not practical and necessary as a goal leaves the reasonable presumption that a small risk is a proper goal. This raises the question of small compared to what. The choice of what the risk is to be compared to is a key in the formulation of quantitative safety goals.

Now for the comments on the workshop, let me just go through those quickly. The second safety goal workshop was held last week at Harper's Ferry. We had some 30 technical experts representing a broad range of disciplines who attended, in addition to George Sege and myself and around half a dozen members of our NRC Safety Goal Steering Committee. Commissioner Bradford came on

Friday to the morning session as an observer. The centerpiece for the discussion was the paper which I just presented to you some highlights of, which was drafted by our office. We had three panel discussions leaders, Herb Counts, Paul Slovic, and Lester Lave, and technical input was supplied by NRR and the RES Staff. There was an evening discussion by Matt Taylor from Bob Bernaro's staff in which he presented the results of some risk assessment studies that they have there, six or seven estimates of core melt probabilities and other related material.

We felt, on balance, that the workshop was at least useful to us in acting as a sounding board -- and a strong one, at that -- for both the subject of quantitative and qualitative safety goals which were embodied in our paper. Based on the reaction from the workshop, the safety goal proposals and the discussion paper will have to undergo a substantial overhaul in the course of drafting a policy paper for the Commission's consideration in the near future.

There were three panels at the workshop, one on quantitative elements, one on qualitative goals or elements, and a third on economic, ethical and sociopolitical factors, inasmuch as we did have a number of sociologists and political science, public administration type people at the workshop. However, in contrast to the earlier workshop, a major part of this workshop was dedicated to plenary sessions which contained panel reports and general discussion of implementation in other issues.

On the subject of quantitative elements, Herb Counts noted that the paper which we proffered for the workshop's consideration was a great extrapolation beyond the Palo Alto workshop, but he found that most of the features considered desirable from the first workshop were incorporated in the discussion paper.

In this subject area of quantitative elements we were urged to have greater clarity of the expression of the goals and avoidance of jargon. For example, avoidance of estimated mean probability of fatality. This particular subpanel saw a need for an expansion of the statements of the qualitative goals and saw some merit to a suggestion from one of the participants that we should identify as distant or aspirational goals, that is to say, goals which are not necessarily achievable by today's technology.

I guess I must say at this point there does seem to be something of I guess a semantic problem when one talks about goals to other than technical people. I think that the common reaction is that a goal should have some aspirational feature to it, something this is where we are going, this is what we want to do in the future; whereas, my impression in talking to the technical people is that that isn't really what they had in mind. It is more a matter of establishing something along the lines of an informal engineering standard or Commission guidance to the Staff on how to treat these PRA's which are coming in. We will come back to that in a little bit, I think.

On the qualitative elements, with respect to the

qualitative elements, this particular subpanel suggested that we might have different goals as a function of their application or intended end use. For instance, the NRC might articulate an aspirational goal for the long run for the public and the Congress. For instance, a commitment to avoid major accidents. At the same time, specifying something in the nature of quantitative goals or informal standards or whatever which might become the Commission's guidance to the Staff to be used in their own way.

However, it was stressed that these different goals, of course, must still integrate and mesh together. This particular subpanel, not unlikely, saw an inextricable linkage between the quantitative safety goal and the means to verify such a goal, that is, probabilistic risk assessment.

With respect to the hazards of nuclear vis a vis risks from other electric generating technologies, they called attention to the fact that one must be careful to insure that you only make relevant comparisons. That is to say, that you should not use statistics which embody old safety standards or safety standards from another era which would cast something of a shadow over the point that you would like to make by virtue of having made such a comparison.

This particular subpanel believed that the probabilistic risk assessment, despite its limitations and uncertainties, was in any case a good way of looking at risk, certainly in relative terms.

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The last subpanel, on economic, ethical and sociopolitical factors, saw a need to insure that the goal statement would be logically sound and internally consistent. They saw a need to develop more fully a social value model as a part of the paper. We are still struggling with that one. This subpanel criticized what they saw as the strong value-ladened statements in the paper like those on pages 6 and 7 which we have some statements, for instance, some radiological risk might be unacceptable regardless of benefit. I think they also took us to task on our lead-in statement to that, that we thought that these things were by and large self-evident. And indeed they may not be. We will have to look at that again for sure.

They found a number of implicit or hidden assumptions such as the notion that costs don't count or that some probabilities may be small enough in which one may ignore equity considerations. And they saw a need for greater or a more explicit treatment of the topic of risk aversion. Overall, on the subject of quantitative safety goals, the participants I think seem to approve of quantitative safety goals in principle or in theory, but kept coming back to the notion that we may not be ready for them for want of a sound rationale. At least the rationale was not in our paper. And some among them stressed the shortcomings of PRA as a yardstick for demonstrating the extent to which these goals are in fact met.

There was also some suspicion that the numerical goals

in the paper weren't goals at all but a reflection of what was technically achievable today. Dr. Okrent, I must say, stressed the need for an additional goal statement in the area of mitigation to complement the statement in prevention. We are thinking about that. It has only been two days. We haven't decided exactly what we are going to do by way of a fix in the next draft. We are going to meet next week in the NRC Steering Group on this subject, George and I are going to meet with our consultants tomorrow and get some more input on this before we go back to craft the next paper.

I think you should hear now from George and then we can go to questions.

DR. OKRENT: Okay. Why don't we hear from George and then we will ask questions to both of you at the same time.

DR. SEGE: Thank you, Mr. Chairman. As Dennis mentioned, I am going to go quickly through an overview of the issues and add some observations from the workshop, concentrating particularly on the results of the final wrap-up session.

First, I am going to present a very brief overview of some salient issues and how they fared in the reference safety-goal statement before you. The overview will suggest some questions for your consideration.

The discussion paper reflects a proposed settlement of a number of issues pretty much in line with prevailing views that emerged from the first workshop and other discussions. Also

discussion did not reach the issue or did not proceed to a conclusive result. I'll skim the surface of ten substantive issues in this category.

1) What should be the qualitative goals? The goals stated in Section VI of the discussion paper should be read in conjunction with the principles guiding development of a safety goal in Section IV. The qualitative goals themselves are stated in terms of limiting risk to any one person and in keeping aggregated social risk adequately low in relation to other risks. Such qualitative goals tend to have something very general and commonplace in their articulation. Perhaps necessarily so, perhaps properly so. We would, however, welcome improvements that you may suggest in the draft qualitative goals to help make them as sound and useful as possible.

2) What should be the extent of quantification? Our draft's approach is simple, limiting itself to three elements. Is more needed? Can more be managed in a practical scheme of implementation? Should some of the greater elaboration of the ACRS proposal be adopted?

3) What quantitative elements should be specified? At what values? The reference goal specifies an individual exposure limit, a social impact per unit of energy production, and a core-melt probability. Should other parameters be specified in addition to or instead of these, for example, from the ACRS proposal? What are subcommittee members' views about the numerical

values for the quantitative goals proposed?

4) What qualitative elements should be specified? The reference goal statement includes rudimentary specifications for individual and social protection and discusses some qualitative principles that have guided development of the goals.

5) What should be the implementation process? How should the goals be used? How should uncertainties be dealt with? How should any burden of proof be allocated? The proposal in the discussion paper would implement the safety goals with a very light hand. The goals would influence rulemaking and other standard setting with respect to major safety issues. They would also influence decisions on issues not covered by regulations, backfitting of existing plants, exemptions from rules, enforcement actions. But the key is analysis and disclosure. A disclosure of the bases and uncertainties, as well as results of the analysis and consideration of this information as one factor among others in the major safety decisions involved. The manner and extent of the use of that information would be determined by the nature of the issue. Uncertainties would be respected; prescription and proof would generally not be involved.

6) Should the scope of the safety goals be extended to aspects other than accidents and to facilities other than nuclear power plants? The discussion paper suggests deferment of any such extension.

7) How should equities be taken into account, particu-

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larly equities in the distribution of risks and benefits and genetic risks? The discussion paper acknowledges that it is not possible to devise a system of regulations whereby the distribution of risks and benefits is always equitable for each individual; however, if the risks are small enough, there should be correspondingly reduced concern byindividuals regarding the balance of risks and benefits. Genetic risk is not specified as such. Rather, the reference goal structure depends for validity on the thesis that regulatory action with respect to reactor accidents would not be sensitive to inclusion or exclusion of a genetic risk specification.

8) Should there be safety goals beyond the specified level of minimum adequacy? The discussion paper endorses a concept of "as low as reasonably achievable", without specifying a numerical formula for cost-based cutoff in cost-risk trade-offs. In a broader sense, the issue of safety improvement beyond minimum adequacy is left somewhat open by the non-prescriptive nature of the implementation process.

9) Should there be a special emphasis on high consequence accidents even if low probability of occurrence? What form should that emphasis take? The discussion paper expresses interest in considering incorporation of some special approach. The first workshop produced criticism of the concept of an exponent on the consequence factor -- at 1.2 or any other value -- but arrived at no widely supported specific alternative. We

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would welcome suggestions. The paper notes remote siting and measures to mitigate accident consequences as specific actions to reduce catastrophic potential.

10) What should be the role of economic considerations? The discussion paper provides proposed guidance on recognition of economic factors in ALARA -- the "as low as reasonably achievable" concept -- in decisions concerning possible backfitting of existing plants to new requirements and in timing of corrective action when required in an operating plant and the severity of the problem is not such as to demand immediate action.

I hope the subcommittee members will give us the benefit of their views on a number of these issues. An at least equally important desired result of this review from our standpoint is identification and guidance on any additional issues that should be considered further at this time.

I shall now turn to the second safety goal workshop, particularly its final wrap-up session. The results of that session offer perspectives on the issues that may be of interest to the subcommittee.

The wrap-up session was devoted to participants' statements of what they viewed as the most significant judgments concerning the OPE discussion paper and changes needed in its reference safety goal statement. I shall present some highlights of the discussion, noting particularly views that appeared to have wide support. Complete consensus on the issues involved was,

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however, not sought or found.

To summarize, a widely shared general theme appears to emerge from the various statements of views by participants. The highest safety goals should be qualitative goals devised as aspirations to be aimed at rather than as requirements geared to what is currently achievable.

Quantitative goals should be stated as standards against which performance could somehow be measured.

Safety goals should include distinct elements expressed in language and specificity appropriate to the various users and uses, articulation of public concerns and values, guidance and decision rules for the Staff.

Introduction of safety goals should be a gradual and circumspect phase-in rather than an abrupt displacement of current practices. Notably, quantitative goals should not be allowed to displace the defense-in-depth concept.

The safety goals should address avoidance and mitigation of catastrophic accidents, that is, high consequence, low probability accidents.

I will now turn to highlight in a little more detail on a few of the specific points from the wrap-up session. First, qualitative goals as aspirations. A number of participants advocated making a clear distinction between broad and symbolic goals that set the aim and thrust of safety regulation and narrower operational standards that codify specific decision rules.

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The general goals should be qualitative and need not be and in the view of some should not be constrained by what can currently be demonstrated or even by what is believed to be currently attainable. They should be aiming points, perhaps distant, but seriously pursued. Specific safety policies and standards should be related to these goals and adjusted from time to time on the basis of periodic review of performance in relation to such primary goals.

The qualitative goals as stated in the discussion paper were viewed as needing consderable expansion.

No. 2, quantitative goals as standards. For reactor licensing by NRC, goals are needed that are stated as standards in specific, technical -- including quantitative -- terms. These should be consistent, be coherent with the primary qualitative goals, admit verification of compliance at least in retrospect, and reflect balance of a trio of components: accident probabilities, accident consequences, and social values.

The quantitative goals should be stated in a manner that is integral with the implementation process. That is, the relationship between goals and processes to achieve them should be stated. They should be related to the qualitative goals by a clear rationale. The regulatory decisions to which the quantitative standards are to be applied should be identified and decision rules should be spelled out. These should include procedures for use of probabilistic risk assessment.

Reservations were expressed about quantitative safety goals in several respects. In view of problems and uncertainties in PRA, there may be too much room for error and abuse in implementation. Quantitative standards tend to drive out qualitative ones. Quantitative safety goals may weaken the defense-in-depth concept. And we may be far away from having proposed the right quantitative goals. One participant noted that use of PRA does not require probabilistic goals.

No. 3, Gradual introduction and dynamic evolution. Several participants stressed the need for cautious, gradual, tentative introduction of safety goals. After initial experience with the goals applied on a trial basis, performance against the goals and utility of the goals should be evaluated. Adjustments in the goals, perhaps especially in the quantitative goals, and in the method of implementation should be made as warranted.

The cautious attitudes about the introduction of safety goals stem from several considerations: uncertainties in technical knowledge underlying PRA, problems in the quality of PRA calculations, social and equity issues not taken into account or only addressed in a tentative or controversial manner, uncertainty as to precise effect of particular goal elements on regulatory actions, possibility of counterproductive effect through excessive displacement of established safety practices, and a general need to learn from new operational data and from experience with the safety goals. There should be a conscious effort to keep what is valuable

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in current practices. Safety should be enhanced over time. PRA practices should be improved over time.

No. 4, Problems of comparison. Some participants cautioned against simplistic comparison of technologies in seeking a suitable goal for nuclear power plants. The risks are qualitatively different. Uncertainties are involved in estimating the risks of both nuclear and alternative technologies. The risks of old coal-fired electric generating plants should be not assumed to be acceptable today, nor should today's risks be assumed to be acceptable in the future.

A suggestion was made that the relevant comparison is with existing nuclear plants, whether the safety goals and standards should be set as equal to, higher than, or lower than achieved by existing nuclear plants.

No. 5, Equity and social values as matters of policy. The view was expressed that the safety goals should articulate public views and concerns rather than take an apparently patronizing posture of contributing to better understanding by the public. Policy principles should be stated as agency decisions -- we come down here for these reasons -- rather than asserted as "self-evident" without stated basis.

Where social or equity issues on which opinion is sharply divided are avoided or addressed incompletely there should be acknowledgement of the incompleteness and provision to revisit these issues. The issues of equity of distribution of risks and

benefits and a special consideration of high consequence accidents were mentioned as examples.

Thank you, Mr. Chairman.

DR. OKRENT: Subcommittee questions, comment, answers?

DR. SIESS: George asked for answers.

DR. OKRENT: Yes, I know. But I am giving you a wider flexibility than he did.

DR. SIESS: George, you mentioned who was at that workshop. Were there any "members of the public" there?

DR. SEGE: Yes.

DR. SIESS: I said it that way because everybody is public.

DR. SEGE: The workshop participation was limited to 25 invited discussants and the several NRC representatives mentioned by Dennis. But it was open to the public for attendance as observers and we had about 40 to 50 members of the public present at peak, which included a contingent of 20 very nice young people who came as a group for a few hours.

DR. SIESS: Did they participate?

DR. SEGE: No. The members of the public came as observers. There was opportunity for them to mix with participants during breaks and meals and so on.

DR. SIESS: The invitees were only the technical people or social scientists?

DR. SEGE: Both. There were people with technical

backgrounds and people with backgrounds in a number of other disciplines, social disciplines, philosophy and political science, sociology, social psychology, ethics.

DR. SIESS: No members of Congress?

DR. SEGE: No members of Congress.

DR. OKRENT: No housewives as such. They may have been housewives but --

DR. SEGE: No, not as such.

MR. RATHBUN: Let me just add we were going to have four public meetings at one time, scheduled in mid-May through mid-June. But as a result of the House report, Congressional action, we cancelled those. While there were members of the public present there, they did not participate and offer their views.

DR. SEGE: I think one other point that should be added, Dr. Siess, is that in addition to striving for a balance of different relevant disciplines we also strove for some diversity in attitudes towards nuclear power and, in addition to representatives of the nuclear industry, we had people who are identified with criticism of nuclear power as well as a large number of people who had no clear or clearly-known attitudes in favor of or against nuclear power.

DR. OKRENT: Can I ask a point of information about the discussion paper. On page 35, where you identify a proposal for timing of corrective action and you say that if a goal is exceeded by a certain factor then a certain action would be appropriate,

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was it your intent that each goal would be examined this way or only one of the three quantitative goals or what?

MR. RATHBUN: It wasn't clearly stated. I will defer to Mal on that, if you have anything to say.

MR. ERNST: Malcolm Ernst. It is not clear what the intent was. I believe it would be fair to say that the intent would be to apply to any of the goals, not just perhaps the engineering goal. As long as I am up here, I might like to say a word about that table, I think about the same kind of a word I said at the workshop -- I'm not sure whether you were there when I did say it. I was towards the end. It is very difficult to have a table of this sort where you have rather explicit and what appear to be sharp cut-off points, like factors of 3, 10, and 100, et cetera, whereby you take some kinds of actions or don't take some action or do it in certain kinds of timeframe.

One could draw from that that you would take some action if you had a 3.1 factor but wouldn't if it was a 2.9 factor, and we know very precisely what the risk is and things of that sort. I think we need a fair amount of work on the paragraphs describing that table to try and get away from that feeling of preciseness. In the first place, there is a fair amount of uncertainty as to whether one meets a given risk value. In the second place, even if there were not this degree of uncertainty as to whether or not you had met a goal, I submit there is a substantial degree of uncertainty of what the goal should be anyway, probably much

greater than this factor of 3 or so as to really what should be the level of acceptable risk, so to speak.

So the table is offered as guidance to try and help the decisionmaker make an appropriate decision. Another thought that comes to mind is that if you do exceed one of the levels as indicated in the table you are probably exceeding it because of the existence of a dominant sequence, an accident sequence. You are probably not going to be exceeding it because of the summation of the 20 or 30 sequences, but there will probably be a dominant sequence that really hadn't been anticipated.

If that is the case, I submit that you should fix that sequence and, once you fix it, you will probably be substantially below whatever the criteria is for action anyway.

So those are the kinds of thoughts that I think need to be considered when looking at a table of this sort.

DR. OKRENT: I'll recognize you in a while. I want to see what comments of substance there may be from the subcommittee.

DR. MARK: I'm not sure if there is any agreement or if this is really a correct statement, but the question was raised whether you should have quantitative goals, I understand, by at least some of your discussants. There has been a change, of course, in the NRC's approach to accidents. Having lived for so long with the class of accidents that needn't be considered providing their probability was as small as was believed is no longer the case; consequently, there is no stopping point anymore at what has to be

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considered, and the quantitative goal certainly has some important role to live in that new situation.

MR. RATHBUN: I quite agree. Quite frankly, I think that is why we continue to be committed to come up with some sort of numerical guidance in that area. The problem that we ran into was if you say something like 10^{-5} individual risk or 10^{-4} core melt probability or whatever and then your next statement is we think that is good enough, the social scientists will come in and say why. Just because that's the best you can do? Is it sort of a supply-side thing, that's what technology delivers?

So I think the paper was weak, in my belief, with respect to rationale. I think that was a fairly common reaction across the board, irrespective of persuasion or the like. So I think that's where we really have to do our work, frankly.

DR. SIESS: On this subject of qualitative goals, George mentioned in his thing he just read us that you have to look at the qualitative goals in conjunction with the principles guiding the development of the safety goals. As he has it set up, they simply represent an intermediate sort of a summary statement between the principles and the quantitative goals. Now as such, I can't get particularly excited about them. If I read them alone, they don't mean any more to me than reasonable assurance that there is no undue risk to the health and safety of the public. I have lived with that for 13 years and I guess I could continue to live with that or any other type of statement.

I think the explanation of the goals qualitatively followed by quantitative goals is an essential sequence. The intermediate step of a one sentence qualitative statement, I can take it or leave it. I don't think it helps the public any and it may help the social scientists -- if you want to keep them happy, along with the engineers, all right.

DR. MARK: Couldn't they get the word "motherhood" in somehow?

DR. SIESS: I deliberately avoided it. It is no longer popular. I refuse to get into that issue.

DR. SEGE: That is a comment to which we are, of course, very sensitive ourselves. We are quite dissatisfied with the platitude that we have as qualitative goals. There are alternative platitudes that are possible and have been proposed. As a matter of fact, some longer statements containing just as little information were proposed at the workshop. However, the suggestion that was made at the workshop that the qualitative goals should be stated as aiming points, something that would influence the general thrust of regulations, that they should be stated in terms that motivate rather than specify, that suggestion seemed to have drawn support at the workshop. Perhaps that would offer a direction for resolution of the question of qualitative goals.

DR. SIESS: I can't speak for all engineers but, as an engineer, I don't get very motivated by qualitative goals. I can get pretty motivated by quantitative. I get out my calculator and

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start working. Let me address your second question about --

DR. OKRENT: Excuse me. Can I make one comment here before we go to another subject. I guess I would like to make the case that the proposal in NUREG 0739 tries to address the question of aspiration and it tries to address it in a way that the engineers can understand if they want to. That is in the ALARA on new design. It says, on new design, after you have met the goal limits you try to do better in a cost-effective way and this is something that the industry is supposed to do and it is not something that the NRC staff is supposed to come in and make a case for. It would be the policy that this is the approach.

I will take the position that this is in fact a practical way of expressing this aspiration without imposing goals that are not meetable, that they are so small that you always fail the test consciously, if you want to put it that way.

DR. SIESS: Your second question, what should be the extent of quantification, and you mention that you have limited to three elements -- I think NUREG 0739 has five or six, depending on whether you count the targets of limits -- and you say our draft approach is simple. I think that the complications in applying quantitative safety goals and the PRA that has to go along with it are so great in themselves that whether you have three or four or five or six numerical goals with which you compare things really doesn't contribute much to simplification or complication, whatever way you want to look at it. I think it

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is important that the goals be rationally related to the risks or the perceived risks or the kinds of risks and maintain some concept of defense-in-depth. I don't think it would be a lot simpler if there were one goal instead of six if you have to do a PRA to show whether you have satisfied it.

And the ALARA is on top of that and that is another complication which I think can be justified on other grounds.

DR. OKRENT: Dr. Kerr has been waiting.

DR. KERR: I don't know to which question I am responding; maybe the one that wasn't asked.

DR. SIESS: That's unlikely, Bill. I think George covered everything.

DR. KERR: It appeared to me when we started this process that there had to be some way of determining what was acceptable, because to me that is one of the facets of a safety goal, that it must be acceptable to some consensus -- not to everybody. I don't see anything in this policy paper that indicates how acceptability is going to be determined.

It seems to me there are two extremes. One is to take a vote or something and tabulate the results. The other is to get together a group of experts and come up with something and tell people, say this is an acceptable set of goals because we experts tell you it is acceptable and so it must be.

I don't think either one of those is appropriate. What I had expected is somewhere beyond the ACRS, this is such a small

task that we don't undertake this sort of thing, there would be a methodology developed to say if we have gone through this sequence of operations, talking to people, Congress, even the NRC itself, we now have what we think is an acceptable set of criteria.

I don't know what those final numbers will be because what we have done within the ACRS is to set up some that maybe to us might be acceptable. At least when I participated in this in a small way I didn't think I was writing something that would be acceptable to the public because I didn't know what would be acceptable to the public, nor do I pretend to know how to establish that acceptability. And this may indeed be one of the more difficult parts of the task. I am and have been skeptical that this task can be done. I think it has some advantages which make me willing to expend some effort to do it. I think one of the difficulties -- and this has been pointed out, the basis for this I think has been pointed out by almost everybody who has been involved in this -- certainly Starr's early article highlights it -- is that it appears to be that people are more willing to accept risks if they can see benefits.

I think that one of the big difficulties that nuclear power has is that very few people see any benefits. And the reason they don't see any benefits is because they don't really believe you need nuclear power to have electricity. That makes it extremely difficult to sell anything above zero risk, I think.

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There have been surveys, certainly, that seem to bear this out. To me, that is the sticking point of this operation, how you convince or come to a conclusion that anything other than zero risk is acceptable when the benefits are not really identifiable. And I am afraid they aren't. I don't know whose fault that is, but as I talk to people who aren't in the technical community, the impression I get is that they don't really believe you need nuclear power to have electricity. After all, we have had electricity for years without it. Why should the future be any different.

This acceptability question, to me, I think is an important one and I am not critical that this discussion paper doesn't have a solution for it, but it seems to me that is a fairly important part of the future activity if one is going to finally arrive at a goal that has some general acceptability.

MR. RATHBUN: Let me just say I agree 100 percent. Maybe we are using different words to describe much the same thing, but when I was saying that I felt we needed more rationale to develop that sense of acceptability I had hoped, frankly, that the workshop would help us more in that respect than I think it actually did. The only thing I can say is we will go back and work this paper again and try and build in some additional perspective or rationale. I don't know. Some have said that the public meetings might help in that respect. But they in themselves turned out to be fairly controversial, others arguing that after all, reactor safety is something that is a very technical area and

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you really need to have a substantial degree of expertise to know and understand and the like.

DR. KERR: I guess I don't think necessarily that people will have to understand a goal in order to find it acceptable. There certainly are other areas even in the federal establishment in which numerical goals exist and they don't cause any great amount of controversy and I don't think people understand those any better than they do in the nuclear area. I think people have to have some feeling that the matter has received careful consideration by responsible people, that it has been widely discussed, that it has some credibility, that there is some way of checking it. But I don't think necessarily they will want to understand it as long as they feel that people that they trust understand it.

DR. OKRENT: Let me recognize -- there were I think three hands -- Joksimovic was the first up.

DR. JOKSIMOVIC: In the area of clarifications, I would like to believe that those multipliers for corrective action are applied to core melt frequency and not to all the goals proposed. That's wrong?

MR. ERNST: I won't say it's wrong, but it isn't necessarily true.

DR. OKRENT: Let me note that there is a very big difference, in my mind, between applying it to core melt probabilities only and to the other goals, because in the one case you have not dealt with the risks from the core melt and you would

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equate the PWR 7-ish kind of core melt with the PWR 2 kindish core melt and, in my own mind, that is an incomplete situation not tenable by itself.

DR. JOKSIMOVIC: Since I may not have recommended this at the workshop, I recommend now that that section be drastically reworked and clarified.

DR. OKRENT: Mr. Ernst?

DR. KERR: What you want to do is tell him how to do it.

MR. ERNST: Yes. We would like any specific suggestions. Let me add a little bit to that table. There is the third quantitative safety goal, which deals with core melt, has the word "normally" in it, which was put there for a very decided purpose, and that is that in addition to the operative nature of the table on page 35 or whatever it was, there was a judgment and, to some extent, it addresses the problem you are talking about since core melt is not necessarily directly proportional to public risk. The word "normally" was put in there to allow some additional judgment to be put on that particular one.

DR. OKRENT: I would say normally is not the right word to describe the situation. We heard Garrick explicitly say and indicate that in fact core melt did not correlate well at all with risk and, if you remember, I think it was either his table or maybe Ed Burns, one of the earlier speakers, things that were number 19, roughly, on core melt probability were number 2 or 3 on risk.

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MR. ERNST: I agree, but I also would advise that perhaps this table is incomplete since the relative likelihood of occurrences was not present in the table. So I don't know what difference there is say between a 4 ranking and a 17 and flipping them around. There may be only a factor of 3 or 5 difference, in which case both sequences would still be perceived to be risky.

DR. OKRENT: Perhaps. But again, I will go back to my comparison between the PWR 2 and the PWR 6 or 7.

MR. ERNST: Yes. However, I guess you are faced with two extremes, and that is just saying we don't like core melt, period, therefore we don't put the word "normally" in there; we'll just say our perception is you should at least have this degree of likelihood of not having a core melt. Or the other extreme of changing your options with respect to core melt frequency depending on what the offsite consequences are going to be. I think our perception at least in the draft paper still is that normally you would prefer to see a fair degree of certitude that you are not going to have a core melt, irrespective of the offsite consequences, in which case maybe the word "normally" is not too inappropriate to say that you can deviate, but you would still like to see the core stay intact to the extent possible.

DR. OKRENT: Again, if you have some test against more than one goal and risk was there as well as core damage, you can say you are covering both parts. If you are doing it only against core melt or core damage, we have the point we were just talking

about.

MR. ERNST: Yes. I think we really would prefer having a mitigation so-called engineering standard also. I think we had some problems in determining really what that should be, considering the uncertainties involved and phenomena and things of this sort. As long as we are on the mitigation area, it did pass my mind that in a forward-looking or here is the kind of a goal we would desire to have -- "aspirational" I guess was the word used -- it struck us a little bit that not only did the ACRS document hit at this aspirational aspect under ALARA, but perhaps it also hit a little bit in the aspirational sense in the mitigation numbers.

DR. OKRENT: Oh, I think in the mitigation numbers it is aspirational for some of the containment approaches. The table that Matt Taylor showed would indicate that we weren't aspirational enough for some social scientists because some people might say they were there already, and then there may be others who would come in and say they are there when they look at all the decontamination or whatever you want to call it that occurs in pools of water and so forth.

MR. ERNST: Right. That was our quandry. We didn't feel too comfortable with what the numbers should be or whatever.

DR. OKRENT: But those were, again, in the NUREG 0739 there were not threshold criteria posed in terms of mitigation criterion.

Let's see. Ed O'Donnell had his hand up.

MR. O'DONNELL: Just a couple of comments on this issue we have been talking about on the large scale fuel melt and the meaning of the word "normally". The way I read the policy statement and the intent of the word "normally" was to indicate that this somehow wasn't as important as the other criteria. And I think I would agree with that, if that is what you mean, that it is somewhat secondary. That ought to come across more clearly than just trying to insert a word "normally".

The other thing deals with the ALARA. I think there is apparently an area of disagreement between what AIF is proposing and what is proposed in NUREG 0739 regarding the use of the ALARA principle. But I think there is essential agreement that it needs to be in there in a quantitative sense. I did not gather -- and this was the point I made at the closing session of the workshop and I thought it was supported by several other people, including Lester Lave -- but if you are going to make this an operational part of your set of goals or quantitative standards I think it is very important that it be explicitly stated in quantitative terms. I just wonder whether or not there is going to be consideration given to that in the revision of the paper.

MR. RATHBUN: Yes, we will consider it. We'll think it through again.

Mal, do you have any reaction from the regulatory perspective on that? Frankly, I guess I've gotten some reaction from some others that it is a good idea, but not all that easy to

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implement or not all that easy to operationalize.

MR. ERNST: Yes. I can certainly agree with that. I think the problem --

DR. KERR: Does "operationalize" mean "do"?

MR. RATHBUN: Do.

MR. ERNST: I think the problem that one has when one picks a number is the social or ethical problem, perhaps, of choosing a value in a non- -- well, I guess it is all right for the courts to choose values, but to choose a standard up front as to what the worth of a human life is is sort of fraught with political and social problems. I guess it strikes me that there is a range of values that are proposed and the extreme -- well, I guess the extremes don't, but I guess the reasonable extremes probably vary by as much as a factor of 10 on dollars per manrem.

And considering the uncertainties in calculating the manrem to start with, I guess one can wonder whether there is sufficient benefit in setting an explicit value that would outweigh the problems of trying to set such an explicit value. I don't think we are trying to establish a cost-benefit process here where you turn the crank and make extremely cost-effective decisions on the margin that are sort of automatic. I think you have a large area of judgment on the benefit side and pragmatically somewhat on the cost side and sometimes you may err on the side of -- in fact, a lot of times you may err on the side of implementing an improvement even though the cost-benefit might not be greater

than 1. It might be a half or maybe even a tenth. So I am not sure how important that quantification of the dollars per manrem is.

DR. KERR: There is an alternative that probably doesn't solve any of the problem, and that is to simply tax the residual risk that exists after one has met those standards and then that provides some incentive for reducing it.

MR. ERNST: As an aside, we I think will be coming to the Commission shortly with a paper on prioritization of generic issues and safety matters in general, some sort of a scheme which is really based on curies released rather than dollars a manrem or things like that.

DR. OKRENT: Total curies?

MR. ERNST: Well, I don't want to debate the merits or demerits of this particular one here. But I am just offering a thought that maybe instead of talking about dollars, which makes believe you have assigned a worth to a life, you still may be able to make useful cost-benefit analyses on some other basis,whether it be manrem or curies or something else, something other than dollars, even though implicitly you are considering dollars somewhere in your standard.

DR. OKRENT: Well, it seems to me in talking about ALARA or cost benefit it is probably important to draw a distinction between existing plants and new plants because one doesn't necessarily take the same approach or have the same guiding philosophy.

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If you are looking at existing plants right now, the backfitting rule I guess requires that the staff may define some changes if necessary to accomplish some significant improvement in safety -- I think that's the word that is there; "significant" not being defined.

I suppose one could argue that something is significant because without it the existing level of risk is too large and with it you get an appreciably significant amount in the direction in which you wish to go. Or you might argue that something is significant because in fact it is very cost-effective in reducing risk, you are making a significant reduction at a certain cost, which is less than 1.

Now if you don't have an ALARA-kind of consideration at all there because you are reluctant to assign a dollar value and if you don't have an ALARA consideration on new plants because you are reluctant to assign a dollar value, I think the case might well be made that you will be achieving less safety by your timidness because in fact what you will have is people will be meeting the regulations but not making cost-effective improvements. So by unwillingness to say look, we think this is a reasonable approach and we will try to put in reasonable numbers you may in fact not be helping the public welfare.

MR. ERNST: Yes. I think I'd support what Dennis said, that we certainly need to think this out more. I don't think that the implication in the paper was that we were trying to be too

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timid or not really accepting the ALARA principle. I think the thrust of the paper was to establish an ALARA principle but perhaps that this principle could be reasonably operative without assigning of a specific dollar per manrem.

DR. MARK: I'm not sure I fully agree with what you seemed to say was desirable. If you put dollars on manrem, there are people who think they know the correlation between a manrem and a death, but those are the ones who probably haven't worked in the field. The BIER 3 report of experts will allow you factors of 5 or 10 on a low rem exposure and physical consequences. So your fear that you are putting a price tag on a human life could be relieved somewhat, since you don't know what that factor is.

DR. SIESS: If you take dollars per manrem out of this will you take it out of Appendix I?

MR. RATHBUN: I have a big enough problem as it is. Please don't expand anymore.

DR. SIESS: I mean you have already done it once; now you seem to balk at doing it again.

MR. RATHBUN: Let me turn it around and ask a question for the subcommittee and also the participants here. I have seen the sheet that Mal is talking about, wherein NRR is attempting to establish these priorities on issues where they will allocate the resources. I recall in Denton's discussion at one of the staff meetings on this paper something to the effect that we really don't know very well at all costs. Now in the context of

probabilistic risk assessments, which are themselves subject to uncertainty, let me ask if you want to include an ALARA concept what kind of additional complication does that superimpose on the analyses which are already done? Is that just an overview, a single sheet which attaches to voluminous PRA studies wherein they specify system costs? Or does that really propagate through their assessments and their analyses and make them introduce another factor subject to significant or substantial uncertainty plus another layer of complication?

DR. OKRENT: Let me try to offer a comment on this. We heard yesterday from the Big Rock Point group who, of their own volition, have tried to do a kind of an ALARA study in which they first did a PRA and then looked at changes in design or procedures or so forth to reduce the risk, and they looked at both the risk reduction and the cost of the various measures. And in fact they are arguing that what they are recommending is a more cost-effective way to reduce the risk of Big Rock Point than automatic implementation of the TMI 2 requirement plus automatic implementation of many things that might arise from the systematic evaluation program.

Now they didn't do it by adding one sheet to the PRA. But I don't feel myself that it was, to use your word, a complication. I mean, the PRA itself is a complex affair. Having done the PRA in fact -- and if you have done a good job on the PRA you are in a fairly good state to consider what modifications in

fact are likely to be cost effective. Now there will be others who may have other ideas, but it is not only something I guess that is operational or doable, at least in the sense that one utility has tried to do it. It may or may not be bought as done. It is in a sense beginning to be a fact of life.

DR. KERR: It seems to me one advantage of this approach is that it encourages that iniative that a number of our speakers have not wanted to stifle and permits innovative approaches to improving safety. It occurs to me that some of these things could be fairly simple and might involve even a half sheet added to the PRA. We had an example last week in subcommittee meetings in which we were examining two power plants that were coming in for license and there was a significant difference in the estimated dose due to releases.

Now I don't know whether we got the right answer or not, but we asked why and the answer we got from the staff was it is very simple, one of them has a lot more charcoal in their filters than the other. You know, that's pretty straightforward.

DR. GRIESMEYER: It would seem to me that the main point of establishing a safety goal would be to insure that the plants are safe. Probably one of the better ways of assuring that the plants are run properly and are designed properly is to have the people do a PRA. If they have to do a PRA for licensing it seems that -- it is not clear that a PRA for licensing makes much sense if they haven't done a PRA to improve their design. The ALARA

part of the PRA is more important than the licensing aspect of it.

DR. OKRENT: Mr. Ernst.

MR. ERNST: I think I will sit up here permanently. I would like to offer a suggestion here that we fully support the ALARA and in the Big Rock one, for example, clearly support that one. In fact, I think I was more than a small part of the process that in essence recommended that we should see what Big Rock has to offer in the way of alternative ways to reduce risk other than ones that had been deterministically suggested.

I should also point out that an assignment of a dollar per manrem was not a part of this useful ALARA exercise that Big Rock is going through.

DR. OKRENT: Comments on this point or a new point?

MR. THADANI: I am a supporter of conducting PRA's. In fact, like Mal indicated, I also support use of PRA's in terms of determining what modifications may be more safety-oriented, if you will. But I do want to make a point that there are ways you might abuse the risk studies and I don't think enough has been said about that. I do agree that Big Rock Point did come in with some recommendations, requests for delays for implementing some of the TMI 2 related modifications. But there were some suggestions by Consumers Power to delay implementing some of the TMI 2 related fixes, if you will, which I don't see how one can quantify in terms of the risk study that was conducted for Big Rock Point.

An example that comes to mind is instead of having three

people at the on-site emergency center they recommended two people. Presumably if there is a serious event they would take some action. I'm not sure that things like that are amenable to evaluate in a risk sense, while actual hardware modifications I think one ought to be able to relate to in terms of the risk studies. I would like to caution people that there are aspects of risk studies which are perhaps being abused.

DR. KERR: It would also seem to me that the staff would be cautious about recommending things whose benefits can't be quantified, but I have said that before.

MR. THADANI: I certainly agree with you.

DR. OKRENT: In any event, I agree with you that they can be abused. I think we tried to say that in NUREG 0739. I think you can abuse it in the quantitative area as well as in the kinds of things you have just alluded to. But in spite of that, I still think there are many merits to the process and to looking at the ALARA part of it.

MR. TEMME: I'd like to go back to this issue or discussion of the qualitative versus the quantitative goals for a moment, having been one of the discussers of this point in the workshop last week. I think there is more involved there than just semantics and the distinction between motherhood statements and the things that engineers can work with. I am certainly in agreement with the idea that qualitative goals are generally not helpful to technical people in making decisions. They need more.

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But I also think there is some potential in pursuing this idea a little bit to produce perhaps a winning strategy or at least one that is more likely to be a winning strategy.

First of all, I think qualitative versus quantitative is the wrong dichotomy. I think what we should be separating from one another are goals and the processes which include quantitative decision rules by which the goals are achieved or by which we attempt to reach the goals. The goals ought to be aspirations. But there are a few more characteristics that I think they ought to have.

They can be either qualitative or quantitative. The important characteristics I think are that they should be measurable, that is, performance against them should be empirically measurable. Now that is an objective that I am sure we can't achieve to perfection. But at least we can state goals in such a way that the controversy over whether or not they have been met in retrospect is minimized.

DR. KERR: Mark, it seems to me that that almost implies quantitative. If you can measure --

MR. TEMME: Let me give you an example. It may. But what is quantified is at issue here. As an example of what I would consider a goal -- in fact personally I think it is a good goal to set -- that can be measured is that there be no immediate fatalities as the result of nuclear plant accidents. Now I did not say no expected fatalities. That is not a statistical state-

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ment. I didn't mean it to be interpreted as such. But if that were elevated, stated in a policy statement as a goal, it would be very easy to determine in retrospect at any point in time that that goal had not been met. I think there would be very little argument.

DR. KERR: Do you consider that a quantitative or a qualitative goal?

MR. TEMME: I would consider it a quantitative goal.

DR. KERR: So would I.

MR. TEMME: But as I said, I don't think the issue is qualitative versus quantitative; the issue is goals versus the process by which you intend to meet them. Goals are important --

DR. KERR: Are you implying when you say that that you can separate the two? Because to me they are almost inseparable.

MR. TEMME: I think they must be distinguished from one another.

DR. KERR: I don't see how you define a goal unless you also define the way in which you propose to determine whether it is to be met.

MR. TEMME: Well, the example that I just gave is an example of a goal, the meeting of which does not call upon a controversial calculation. Now the minute you add to that goal words like "probability" and so forth, the demonstration that you have met that goal is not done by saying look, we haven't had any deaths or something observable like that that people can

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generally agree with. It is done by doing a calculation that you've got to get people to agree with. A risk calculation, to be specific. That doesn't mean that you avoid all of the arguments. Certainly no matter what goals are stated, there will be arguments by people who say if you keep doing what you are doing now you are not going to meet that goal. I think those arguments are unavoidable and they should be faced. But the other argument is the one that looks back and says have you been meeting your goals.

If the answer to that question requires a PRA, we have some problems. We have been talking about those problems for a couple of days. If the answer to that question requires merely observation and you can get general agreement to a yes or no answer and it doesn't call upon PRA, I think there is a better potential for winning the argument.

Now none of this is to say we should not use PRA. In fact, the other part of this is the process by which you lead yourself the goal -- and that's what we have been talking about here. And I think that process is amenable to the use of what has been termed "stylized" analysis methods, decision rules, and then the relationship between those decision rules involving quantitative risk calculations and so forth and the goal that has been set forth becomes a subject of continued study and research. But it is not a question that comes up when you ask have we been meeting our goal.

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DR. OKRENT: I guess if I thought that the NRC had adopted the goal you just stated and then I were put on a hearing board as a member and asked to review a new reactor, I would not find it meaningful to look back at what had occurred. I would look at this reactor and ask myself is there any capability for producing early deaths. If there is, how can I say there will be a zero probability from this reactor, they will have to make it a more remote site or something.

MR. TEMME: I didn't --

DR. OKRENT: I know how you were going to restrict its usage, but I'm just saying given this is the goal and then translating it to what you do with regard to a future plant, in other words, for a construction operation, it hasn't run yet and so you have to look at it and see does it have the potential for violating the goal.

MR. TEMME: Of course you do. And I was not advocating that goals of the sort that I have tried to describe be a replacement for PRA and a structured decision process relative to the licensing of new plants. The two go together. You don't do one or the other. You have a goal and you institute a process, the intent of which is to meet the goal. And the examination of a new plant, the licensing of a new plant, is a part of the process by which you meet the goal.

It places the arguments in different contexts and I think it is arguable whether or not --

DR. KERR: You've lost me. You mean if the goal is zero -- what did you use, immediate?

MR. TEMME: I said zero immediate deaths, merely to avoid another controversy that comes up if I say something else.

DR. KERR: And you carry out a process to show that you have tried to meet it or that you have met it?

MR. TEMME: You institute a process, the intent of which is to meet it. And in my view --

DR. KERR: I don't understand. I mean English I know is a very poor medium of communication, but I don't know what you mean when you say "the intent of which is to meet it". You mean you don't get to build the plant unless you can demonstrate that you do? Or you get to build the plant if you have demonstrated that that's your intent?

MR. TEMME: A goal of this sort becomes a part of the public policy statement of the NRC. The licensing process which they carry out should be justifiable and connected to their goal. The goal itself is not something that each applicant addresses. What he addresses and responds to are the decision rules, the procedures, regulatory requirements, and so forth. I think PRA has a place in the decision rules, but not in the statement of a goal to the public. As soon as you state a goal to the public that says 10^{-6} something or other, I think you are in trouble because the demonstration that that kind of goal is met is one that invokes a highly controversial analysis. It is a calculation.

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It isn't an observation.

DR. KERR: I think you are saying that you don't think a quantitative safety goal is practical, aren't you, because you want to be able to find one which is acceptable?

MR. TEMME: Oh, I think you can find one which is acceptable.

DR. KERR: I mean to the public.

MR. TEMME: Zero deaths is an acceptable goal to the public, is it not?

DR. KERR: But you can't achieve that.

MR. TEMME: I think you can. We have been achieving it for a number of years. I think one of the values of PRA is to try to get a better understanding of what we have been doing that does achieve it.

DR. OKRENT: Okay. I think that is one more idea into more than Rathbun and Sege can handle already. Mr. Rowsome?

MR. ROWSOME: I just have a couple of observations and a couple of way out ideas I want to suggest to you.

DR. OKRENT: These are ways out of the problem or they are ideas that are way out?

MR. ROWSOME: Ideas that are way out. Let me start with the simple observation. I have done some back-of-the-envelope calculations of the present worth of expected losses associated with core melt accidents, considering on-site effects as well as off-site effects, costs such as loss of capital investment in the

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facility, replacement power and on-site cleanup costs. If you consider those alone for a thousand megawatt modern plant for much of its life still ahead of it you come up with numbers like the following.

If one had a susceptibility to core melt at 10^{-3} per year, the present worth of expected losses is around $100 million. If it is 10^{-4} per year, it drops to $10 million. If it is 10^{-5} per year, it drops to $1 million. I suspect that cost-effective ways of dealing with discovered or suspected vulnerability to core melt could be found to drive you down to about 10^{-4}, so the 10^{-4} criterion suggested by the AIF and in NUREG 0739 is I think very nearly synonymous with a cost benefit or an ALARA criterion even one that is based exclusively on protecting the investment in the facility.

It is less clear that monetarized values of off-site effects will drive you to an extra two decades on containment. Prof. Okrent has made the point in several forums that he thinks it important to have not only a criterion on the frequency of core damage or core melt, but also a criterion on the containment. Considering this fuzzy area in the ALARA or cost-benefit approach, one might say that you could drop the criterion on core melt but keep a criterion on containment or alternatively put a frequency goal on that class of accident sequences that produce release category 1, 2 or 3 level severity, say 10^{-6}, which would be compatible with the 10^{-4} plus two decades on containment criterion.

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Difficult to verify, but no more difficult than the 10^{-2} is as written.

The radical suggestion, the way-out idea, is that with such calculations with present worth of expected losses calculations that include on-site and off-site property damage and include dollar values associated with society's willingness to invest in the avoidance of statistical casualties, one could really do the entire analysis. One could have a criterion that was drafted exclusively in terms of an ALARA criterion or a cost benefit criterion by comparing cost of implementation with the change in present worth of expected losses. Such an approach has a number of disadvantages that you can think up as fast as I can. Among them, unworkability when you have a clean sheet of paper in front of you and you are trying to design a plant from scratch, and a lot of political exposure on the dollar values you put on health effects.

Nevertheless, it has some merits from the point of view of coherent, objective public policy, and has some merits of being very simple and straightforward, a simple, one-dimensional measure that embraces the whole issue.

DR. KERR: I agree. I think that is way out.

DR. OKRENT: Yes, sir?

MR. ABRAMSON: I'd like to make two points. First, just a comment on Dr. Temme's goal of zero immediate deaths. I think that is going to be rather more difficult to measure and achieve

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and I'm not sure we even have seen it yet. For example, in Three Mile Island a lot of people evacuated the site. Now as a direct cause of the evacuation perhaps you might argue there were automobile accidents. As a matter of fact, I think there even may have been some fatalities. Or possible heart attacks caused by stress. So I think even something as apparently transparent as this, it may be rather controversial as to whether in fact you can achieve that.

The second point is I would like to follow up Dr. Kerr's observation that most people see no benefit from nuclear power and, as a result, the risk they are willing to accept is very low, perhaps even zero. There has been a recent Ph.D. thesis by Litai at M.I.T. in which he suggests a new approach which his taking-off point is that risk has many dimensions. In fact, he identifies nine of them. One of them is the idea of benefit. He dichotomizes each one of them, so you have things like is the benefit clear or immediate, are the risks voluntary or involuntary, is it natural or man-made, are the effects immediate or delayed, and so on.

And then by actually looking at insurance records and other statistical records he infers what people's perceived risk is and what people in fact -- how people in fact act and how they value these particular risks. From this, in fact by multiplying things together, he found that people see that, for example, nuclear is some 9,000 times as risky as coal, which by itself might be kind of controversial.

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But I think that there is an important point here, and it is recognized to some extent by the safety goal -- although I don't think the goal is far enough -- namely, that there are many elements and many dimensions to risk.

DR. KERR: Do you think if I get copies of that thesis and distribute it to my friends that they would feel differently about nuclear power?

MR. ABRAMSON: I think that if you -- no, not if you distribute the thesis. But I think that the factors are the factors which people are familiar with, like is this -- are the benefits clear or not, is it manmade or natural. I think that these aspects of it are the aspects that people really do think in. So I think if you are going to worry about public perception -- and I think we have to -- I think it is very important to do it along these dimensions, that is, to speak people's language about this.

You raised the question about benefit, for example.

DR. KERR: Well, most people think they understand electricity and they don't believe that electricity is really generated by nuclear power. They think it is somewhere out there when you turn on a switch. For years that's the way they have been getting electricity and they did it before nuclear power and they don't see any difference.

MR. AMBRAMSON: Well, I wonder if that is true now, you know, with the oil crisis. I think people are much more aware

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that electricity comes from something.

DR. KERR: I think the last thing that people thought the oil crisis had anything to do with was electricity. It had something to do with automobiles, but nobody ever had to go somewhere to get a new bag of electricity during the oil crisis. It still came on when you turned on the switch.

MR. ABRAMSON: Well, they saw it in their monthly bill, though. The price of electricity has been going up and a lot of people have been seeing it that way.

DR. SIESS: Not if you live in central Illinois.

MR. ABRAMSON: No, but if you live in the Northeast or in New York City --

DR. SIESS: I know, but you are talking about people. Now you had better start defining the people you are talking about.

MR. ABRAMSON: I agree. It certainly depends on the part of the country you are in.

DR. SIESS: I know people in Washington, D.C. that can tell whether PepCo is operating by the size of their electric bill, whether the nuclear plants are operating. I think they know where the electricity comes from and they are not against nuclear power.

DR. KERR: A friend of mine has an alternate suggestion, and that is that the people who buy electricity from a utility that has a nuclear plant be given a choice of whether they buy

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nuclear electricity or coal electricity. That way they would clearly indicate which they preferred. But nobody -- that would be a clear way of demonstrating benefits. But we don't have that now.

MR. ABRAMSON:I think, as a matter of fact, that the French are actually charging people less for electricity who live in the vicinity of nuclear plants. I don't know how this program is working out, but they are trying to face the issue that way.

DR. OKRENT: Thank you. Are there other comments? Do the subcommittee members have other answers for Mr. Sege? I don't think you gave him very many.

DR. SIESS: I got through two questions.

DR. OKRENT: I've been giving you time by taking comments from the floor. Can I ask Mr. Sege and Mr. Rathbun, did you have in mind showing your next discussion paper or draft approach or whatever it is you want to call it to the ACRS before going to the Commission with it? Or were you planning to go to the Commission and have the ACRS comment afterwards or what?

MR. RATHBUN: Frankly, we really haven't thought that through. The end product should be a draft policy paper for the Commission to consider. In light of the reaction the last time around, I've toyed with the idea of submitting the paper for comment from the Commission and perhaps from the ACRS as well prior to sending -- transmitting in a quasi-formal form the paper and say this is the OPE product, the policy paper which we believe

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Are there other comments that members of the audience would like to make?

(No response)

DR. OKRENT: Well, by some magic which I don't understand I just now looked at my watch and we seem to be on or about the time that was estimated by Dr. Griesmeyer for when we would complete this meeting. I think what we might do is try to see if there are individual comments or so forth that members may be able to get you. There will be some discussion of this subcommittee meeting briefly -- assuming we are not crowded out by the many cases on the agenda at the August meeting. We would try to encourage members to get you any comments at that time. I think we would be willing to try to offer specific comments via a subcommittee meeting on your next draft, if you are so inclined.

MR. RATHBUN: Yes. Thank you very much.

DR. OKRENT: All right. If I don't see any other business, a comment or so forth, I will thank everyone for their active participation and declare the meeting adjourned.

(Thereupon, at 4:33 p.m., the meeting was adjourned.)

This is to certify that the attached proceedings before the

Nuclear Regulatory Commission

in the matter of: ACRS/Subcommittee on Reliability and Probabilistic Assessment

Date of Proceeding: July 29, 1981

Docket Number:

Place of Proceeding: Inglewood, California

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Horace W. Briggs

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