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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON CLASS 9 (SEVERE) ACCIDENTS

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

Wednesday, January 29, 1986

The subcommittee meeting convened at 1:00 p.m., Dr. William Kerr presiding.

ACRS MEMBERS PRESENT:

DR. WILLIAM KERR
DR. CHESTER P. SEISS
DR. CARSON MARK
MR. DAVID A. WARD

CONSULTANTS PRESENT:

MR. MYER BENDER
MR. PETER R. DAVIS
DR. IVAN CATTON
DR. SAM C. SAUNDERS

DEAN HOUSTON, ACRS Staff Member

PUBLIC NOTICE BY THE

UNITED STATES NUCLEAR REGULATORY COMMISSIONERS'

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

WEDNESDAY, JANUARY 29, 1986

The contents of this stenographic transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards (ACRS), as reported herein, is an uncorrected record of the discussions recorded at the meeting held on the above date.

No member of the ACRS Staff and no participant at this meeting accepts any responsibility for errors or inaccuracies of statement or data contained in this transcript.

P R O C E E D I N G S

DR. KERR: (presiding) The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Class 9 Accidents.

I am the subcommittee chairman. My name is William Kerr.

Other ACRS members here today are Carson Mark, Dave Ward -- he says he will be along soon -- and Chet Siess.

We have as consultants Mr. Bender, Mr. Catton, Mr. Davis, and Mr. Saunders.

The purpose of the meeting is to begin discussion of NUREG-1150, entitled "Nuclear Power Plant Risks in Regulatory Applications."

Dean Houston is the ACRS staff member present for the meeting today.

The rules for participation in the meeting were announced as part of the notice of the meeting published in the Federal Register on December 24th of 1985.

A transcript of the meeting is being kept and will be made available as stated in the notice.

I would ask that each speaker identify himself or herself and use a microphone.

We have received no written comments from members

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of the public, nor have we been asked to make time for oral comments from members of the public.

We will proceed with the meeting, but I might say that in contrast to what one might gather from the published agenda, I would hope to end the meeting by 4:30.

Are there any comments before we call upon Mr. Ernst or questions?

(No response.)

DR. KERR: Mr. Ernst, the floor is yours.

MR. ERNST: Thank you.

(Slide.)

We have a three-part agenda today.

First, I will give just a few opening remarks on the purpose, the contents, the present schedule and status, things of that nature with regard to the preparation of NUREG-1150.

We met with the ACRS -- I believe it was sometime in middle to late last summer and discussed in general what we .ere about -- the need to rebaseline the risk estimates on six reference plants, the interface with IDCOR and things of that nature.

So I won't repeat that kind of information, but I will sort of give a status report.

You were passed out a draft outline of NUREG-1150. We will discuss that not in a lot of detail,

but certainly we are here to answer questions.

Joe Murphy then has a presentation on methodology and uncertainties in this risk rebaselining work and then an important aspect of this, he will also be discussing the current approach to assessing reasonable ranges of uncertainties with regard to the risk information.

Mark Cunningham then will discuss some of the major issues that we are addressing in this uncertainty analysis and give a few words on how we are going about this process. I won't call it a process for resolving the issues. What I will call it is a process for coming to a regulatory position on a reasonable range for these major issues.

So that is basically what we are about today.

(Slide.)

First, just a few quick words on the purpose of 1150, somewhat repetitive from last summer, but that is okay to have one slide repetitive, I guess.

The basic purpose is to provide a current understanding of the likelihood of core melt and risk from severe accidents at six nuclear power plants. We won't call them representative of the entire industry, but we will characterize them -- as for these six plants of six different containment types, we will characterize the risk.

We have in these six plants six different

containment designs. The plants are being updated as to TMI fixes. We are using current source term information, not only the science, the chemistry, and so forth of source terms but also updated information on containment performance.

So that is one purpose -- is to document what we know to the best of our knowledge with regard to these six plants.

The second purpose is to sit back and try to assess the use of such information for various types of regulatory application.

In going through these plant analyses, you get a number of insights, "what if" kinds of things, what if the plant might have been built differently, because you are really looking for the important outliers and you are drawing on experience from other plants, from our ASAP modeling, generic modeling base, and things of that nature.

DR. MARK: Excuse me. You made a reference to information concerning containment performance.

What kind of information exists on that subject?

MR. ERNST: This would basically be the results of containment loads and containment performance working groups and that kind of information, whatever exists in the way of information today. We are not generating new information for 1150.

DR. MARK: I can understand that you can talk about pressure levels at which the containment might or might not remain intact, but containment performance is a great deal more than that, and I wasn't aware you had an awful lot of information on it.

MR. ERNST: Whatever is available will be used. There are -- well, I hesitate even to talk about it at this stage of the game.

There is a meeting that is going to be held at CB&I, for example, on their insights on the work they have done for IDCOR on containment performance at high temperature levels, Mark I's at high temperature levels, and so forth, where they have come up with some judgments on where the containment might fail.

We are going to look at information like that. Whatever is available at the present time we will look at, and the information will wind up with some error bands on the judgments we use in trying to calculate risks based on this information.

DR. MARK: Thank you.

MR. ERNST: In assessing the usefulness, one thing we are looking for is to mesh very closely with NRR's ongoing work in trying to develop a methodology and process by which the risk important features of other plants -- what are these features and what might be done to rectify

features if they happen to be risk outliers?

To be helpful in this process, we are asking the analysts to draw upon the information in their detailed analysis of the six reference plants and document in 1150 those important insights you get from this that might be useful to extrapolation to other plants.

Clearly, NRR has the responsibility for developing the methodology, drawing upon all information, and we think 1150 is one aspect of that available information.

Other areas of usefulness, PRA has for some time now served the agency in providing information to help plant-specific and generic regulatory decisions. We will take a look at the existing information we have compared to the old information and see whether or not we have different kinds of insights that might be drawn at this time compared to what was in, for example, NUREG-1050, which was published a year and a half ago.

MR. WARD: Mal, I really always have to -- I always blanch when you say something like PRA has been useful for providing plant-specific information for decisions.

It certainly provides design-specific information, but I still have a lot of trouble with whether the PRA is capable of telling us anything at all about the

plant as opposed to the design.

MR. ERNST: By the plant, you mean the people and the procedures?

MR. WARD: Yes, even the equipment and the systems in the plant. They are all in the performance, but also the people and the procedures and that sort of thing. PRA just has a big blank there.

There doesn't seem to be much interest in developing ways for PRA to deal usefully within those areas.

MR. ERNST: I don't think I would agree completely if I understand what you are saying.

It is true PRA can come up with some judgments on the importance of one design versus another design, with certain assumptions on how the equipment is going to work and things like that.

You can't assume equipment is going to work perfectly, and even comparing one design to another, you have certain assumptions with regard to equipment operation and the behavior of people.

We are trying -- I am sure not as well as we would like to succeed, but we are trying to get at the sensitivity of maintenance procedures. We spent a fair amount of time, for example, on the human reliability aspects of Peach Bottom with respect to the ATWS scenario.

We spent a lot of time on venting of Mark I containments.

MR. WARD: But even there, the way you are approaching human reliability, it is sort of a design standpoint. You aren't talking about reactor operators at Peach Bottom. You don't know anything about them. There is no information on them going in.

You are talking about a designer's SRO or something.

MR. ERNST: To some extent, that is true. To some extent, where we rely perhaps on some simulator data, and so forth, that might be generated, like at La Salle, there has been a fair amount of simulator work done by the operators. So that, you could say, was somewhat plant specific.

But I will agree with you, particularly when you get in the management area and things like that, there is no way to factor that kind of thing into the PRA.

DR. KERR: Now that Mr. Ward has made his sales pitch on human performance -- I am not denigrating it. I agree with you, but I don't believe Mr. Ernst is going to be able to include it in his report.

MR. WARD: I don't know that it is necessary to ignore it.

(Laughter.)

MR. ERNST: The best we can do -- and maybe Joe might want to make a comment on this -- the best we can do is try to characterize how that might vary in the assumptions made.

It clearly has some uncertainty bands around it. That I agree.

DR. MARK: You have mentioned taking data from six plants, I believe.

Do these to any extent overlap with the plants being studied for A-45, the generic issue of decay heat removal?

MR. ERNST: I am not sure.

DR. MARK: I am wondering why they don't exactly overlap.

MR. ERNST: Zoltan is back there. I am not sure what A-45 plants there are.

MR. ROSTOCZY: I don't know which are the plants for that. I cannot state.

MR. WARD: Just looking at the list, Carson, I don't think there are any that are common.

DR. MARK: My question is: why the hell aren't they the same?

DR. SAUNDERS: It would have been cheaper to do it that way.

DR. KERR: Please continue.

We will make note of the question.

MR. ERNST: Okay. That is probably enough on that slide.

DR. KERR: From that slide, I gather that only internal initiators or internal events are being treated as limited?

MR. ERNST: That is correct. There may be insights on external events. There will be a subsection on external events, but not a plant specific analysis.

DR. KERR: This is based on a lamp post criterion? The place to look for the lost quarter is where the light is and not where the quarter is?

MR. ERNST: I hope it is more than that.

DR. KERR: We have numerous -- well, numerous is two or three -- PRAs that tell us that the principal risk is from seismic events. That may be incorrect. So we do a risk rebaselining, which presumably is going to give us a fairly good idea of what the risk is from these six reference plants, and we say very little about what appears to be a strong risk contributor.

MR. ERNST: What will be described in 1150 will be some of the insights gained from other PRAs and a description of the process that the agency is going through to try and address external events in NRR's process for implementing severe accident policy.

There is a Commission paper being put together now to try to treat that.

The basic rationale for not having it is that the proper way to approach the problem and the proper way to weight risk insights that you get from external versus internal is an area of controversy that seems like it is probably more important for the agency to look for the vulnerabilities, which is part of the implementation plan process, than it is to try and come up with a risk statement at this time on the six reference plants.

The issue has not been forgotten. It is just that it seemed like, considering the timing problem of trying to interface with NRR's need in working with IDCOR, plus the state of knowledge of the credibility of the risk judgments, made it suspect whether or not we should -- and we rightly or wrongly made the decision not to include it, but not to forget it either from the standpoint of the agency's implementation of severe accident policy.

DR. KERR: It just seems to me that implicit in this is a decision that the external events are not very important.

MR. ERNST: That is not my characterization, nor is it the characterization --

DR. KERR: But you tell me that 1150 is going to be used to make decisions on what to do about severe

accidents and what to do about plant safety and what to do about other plants.

MR. ERNST: It will provide some information for this process, and excluded from 1150 is bottom line risk determination for external events.

There is other information available to NRR for making that kind of judgment. As I say, there is a staff paper being put together to try and outline an agency posture on just how to do that without forgetting that 1150 does not cover that aspect.

It will say that it is worthwhile spending the money to do the risk analysis as opposed to just money for vulnerabilities. Then 1150 could be updated in that area at some later time.

DR. KERR: 1150 is said to be a risk rebaselining, not a vulnerability rebaselining?

MR. ERNST: That is right.

DR. KERR: So in the context of 1150, risk is deemed to be important, but in the context of something else, vulnerabilities, what is important?

Help me here.

MR. MURPHY: I think there's two points that lead us to that decision.

One is that as we look at the existing PRAs that have included seismic events, the problems that have caused

seismic to be high in them, have tended largely, although not exclusively, to be plant specific, so that it has very little generic applicability.

We have got things like ceilings of control rooms falling down and buildings banging together, things of this sort. Rather than say component fragility dominating, it tends to be some strange portion of the structural design or something like that.

This makes its use, as we intend 1150 to give information to 10-R, to be a little less meaningful. Because it is plant specific rather than generic, it is very difficult to extrapolate.

The other thing is that the uncertainties in the seismic parameter are very large, and seismic analyses, of course, are very expensive. We are investigating right now our ability to come up with methods that can do a reasonable seismic analysis, and if we can find those, then these will be part of the discussions Mal mentioned, where we look at whether we want an assessment of vulnerability that is sufficient or whether we want to go on to an assessment of risk.

Before we can make that we have to have the methods. Methods such as SSMRP are extremely expensive, and considering the uncertainty and the basic hazard curve on that, it makes you wonder whether it is cost effective to

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utilize it for this purpose.

DR. KERR: One more comment and I'll let this go. But, if I remember the review of the GESSAR standard plan, about 50 percent of the risk from the seismic hazard was attributed to relay chatter. That turned out to be a significant contributor.

Now, that's plant-specific in that plants have different relays. But it seems to me relay chatter is a generic issue if, indeed, it occurs in large seismic events.

MR. MURPHY: I would agree.

DR. KERR: And it could turn out to be a big contributor there. It's calculated to be. It bothers me a bit to think that we are rebaselining risk, which I think is important to do, yet we may be leaving out a very significant contributor. I don't know whether it's a significant contributor or not, but at least some calculations seem to indicate that it is.

Somebody had his or her hand up back here. No? Okay. Continue.

(Slide.)

MR. ERNST: This is a chart put together just to give you a feel for the various pieces of this review and how they get put together. The elements in green, if you're familiar, they are associated with the bins that we have. The green is the SARRP at Sandia. The red ones are the ASEP

bins at Sandia, and some work being done at INEL. The yellow represents other funding. The process is basically to develop a containment tree based on the results of 2104 runs and other information to come up with some source term bidding, to try and pick the dominant, also using ASEP information, pick the dominant accident sequences, and then get source term code package code runs made on those, which falls into here.

This also goes into uncertainty analyses. This is somewhat misstated. It's more than just source term uncertainty analyses, we also include the front end in that.

Some other projects. We have a fair amount of work going on at INEL on the venting of Mach I containments, the reliability and usefulness of that kind of process.

We have a large amount of work going on at Brookhaven on the HRA aspects of Peach Bottom. And we have, in addition to that, some additional kinds of uncertainty analyses in what we call the PRUEP program at Sandia.

This feeds into consequence analyses, risk re-baselining, selection of risk production options, and value impact analysis. Actually, at this point, it splits into two points, one of them going into the documentation from Sandia of all of the integrated SARP activities. But that information is fed into the NUREG 1150 preparation.

DR. KERR: Excuse me. What is HRA?

MR. ERNST: Human Reliability Analysis. So that's generally the activities that are ongoing.

DR. KERR: Did Mr. Ward hear that?

MR. WARD: Sure. But, like I say, it's a generic human.

MR. ERNST: That's correct.

DR. KERR: Did you have a question, Pete?

MR. DAVIS: Yes. I don't see any provision in that flow diagram for incorporation of industry-sponsored PRA's or the IDCOR analysis. Is that something that you're going to neglect as part of this exercise, or does that get folded in along the way someplace?

MR. ERNST: That gets folded in basically in your uncertainty analyses. Mark will have more to say about this. We're picking perhaps a dozen or so major contributors to the uncertainties. And in coming to an agency position on the range of parameters to be considered for those issues, we do consider the full range of available information, which, as appropriate, would include the IDCOR assumptions as well as other assumptions, yes.

MR. BENDER: I hope that I can be heard. I'm looking at the chart you have up here. Three elements of it seem most important. First, the risk baseline. Secondly, the selection of risk reduction options. Thirdly, the value impact analysis.

Is it possible to illustrate how those three things are done by some example? I'll pick one if you want me to. Let's say that we --

MR. ERNST: Venting is a good one to pick, for example. Venting of Mach I containments, or filter vents, for example.

MR. BENDER: What things might I have to think about in terms of risk baselining in order to decide whether venting is a risk reduction option I should consider?

MR. ERNST: In that particular case, in the Mach I vending analysis that we're doing, if venting is useful, then you have to consider can you vent, will you vent at the proper time? What are the downstream effects of venting, for example, at Peach Bottom?

One thing I believe we learned -- I'm thinking from memory now -- is that if you vented high pressure, the downstream duct work will disappear because it's not made to take that kind of temperature pressure steam flow. And if you had to do this locally, which, in some sequences, you might have to do locally, the operator wouldn't survive.

Now, there's a plant-specific operator consideration. Therefore, that leg becomes a zero from the standpoint of the likelihood of success.

MR. BENDER: Let me back up for a moment.

The Peach Bottom people suggest that early

venting is one strategy. I think some people have accepted that as a good idea. The circumstances under which you might use it and the impact that it might have on risk depends to some degree on the kind of accident you are presuming.

MR. ERNST: That's correct.

MR. BENDER: But, that particular venting, if done early enough, doesn't introduce the kind of hazards you just described. As a matter of fact, it would be done when the system, when the environment in the containment is still relatively cool and the rate of venting would be relatively slow.

Somehow, I don't get much of a feeling for how to combine the event and the circumstance.

MR. ERNST: It's not clear to me that that's true. I think, in some of the accident sequences, you could have some high temperature, high pressure kinds of situations with a fairly high steaming rate.

MR. BENDER: Some of them could and some of them couldn't. So, in trying to come to some judgment about it, it seems to me you're going to have to address a spectrum of events.

MR. ERNST: Exactly what we're doing. Now, there are cases where you might bypass the pool, for example, in which case the venting exercise would not accomplish the end

that you're trying to achieve. And if you have a residual risk from situations where venting really did not help you, then one might say, well, does that look like it's going to happen often enough, and is the risk high enough to warrent putting in an additional manmade system, which you might call a filtered vent?

MR. BENDER: If I look at the question called risk baseline, what am I doing? What is that exercise?

MR. ERNST: Risk rebaselining is to take that plant as is and try and characterize as best we know how the kinds of sequences that would occur and the kinds of core melt threats and offsite consequences that would be represented by those sequences.

MR. BENDER: Is there a probable value associated with each sequence of events?

MR. ERNST: There will be.

MR. BENDER: Is there a judgment basis for deciding when it's acceptable and when it isn't?

MR. ERNST: What we're doing is providing information. Acceptibility becomes NRR's game. What we provide is the information, the uncertainties, our judgments, the analysts' judgments regarding critical assumptions, factors, and things of that sort, in as scrutable a manner as we can provide it.

DR. KERR: Mal, in connection with the use by

NRR, will NRR be able to identify what is meant by core melt frequency as one might use it to compare it with safety goals?

I ask this question because it at least has seemed to me that what we mean by core melt is shifting some from the WASH-1400 days. At the last ACRS meeting, I asked Mr. Denton what he meant by core melt. He said he meant that the core had completely melted and had gone through the vessel.

I'm not proposing that. I'm simply saying it seems to me there is a bit of a spectrum in what is being meant by core melt by the various people we're talking about.

And in order that it be useful, say, in terms of the safety goals, it seems to me one needs to be able to identify in these descriptions something that one can match against what's being talked about in a document like Safety Goals.

Is that being thought about?

MR. ERNST: The core melt we're coming up with is to the extent we can predict the PRA's, the kind of core melt you're talking about, where you have a high potential for going through the vessel.

DR. KERR: I'm not talking about a high potential. Well, high potential still has a probability

inherent in it, which is unspecified.

MR. ERNST: We're trying to consider recovery actions as best we possibly can, even in the core damage sequences.

DR. KERR: No. What I'm trying to find out -- I'm probably not asking the question well. Let's suppose that the safety goal finally says 10 to the minus 4 per year is the frequency for core melt. Bad core melt or something else. Will one then be able to go to the rebaselined plan and say: Here is the thing I should compare with the safety goal?

MR. ERNST: The core melt we have there will be what you should compare.

DR. KERR: It won't be ambiguous like the potential for core melt. When you say "core melt" in 1150, it's the same thing, or at least you could compare it with the core melt that might be described?

MR. ERNST: As realistically as we think we can do it today.

DR. SIESS: Realistically what? Molten core in the vessel? Or, molten core on the floor?

MR. ERNST: This would be core on the floor.

DR. KERR: Thank you.

MR. ERNST: To some extent, I mentioned "what if" kinds of studies to try to be helpful to NRR. The analysts

are being instructed not only to look at the results of the ASEP analysis but also other things that they know about in their experience that, if it were different in the plant they're analyzing, to highlight that difference for the usefulness to NRR.

We feel like it's very important for the analyst who is deeply involved in that process to try and identify the thought process, the what if process.

That's, to some extent, where these additional plant studies come about.

As far as NUREG 1150 preparation is concerned, this is an NRC document. However, we do have some contractual assistance in preparation for that document, the vast majority of that, probably 95 percent or so being in the preparation of the technical appendices.

The document itself will be an NRC-written document. The entire document will be NRC-reviewed and approved.

(Slide.)

A few words about QA and QC. This again has boxes similar to the one you just saw and tries to give you a feel about the QA/QC kind of process that's going on hand in hand with NUREG 1150 preparation.

I forget the exact number but I think it's on the order of about 25 percent of the total funding is really

being spent in the QA/QC area.

For several sources on the front end, we have what we call the ASEP senior consulting group. And there's a QA team also that's looking for consistency of approach through all the six plant analyses. So that QA and QC, two different groups, comes into play.

In the red squares, the containment event tree development was being looked at by several people, expert in the containment event tree or containment performance loads and performance areas. We have QA/QC activities ongoing by several people heavily involved in source terms, and this takes place in the source term binning activities and the source term uncertainty analyses.

There's also a QA effort here which should be listed. We have Brookhaven QA-ing the source term code package runs.

DR. KERR: Mal, when you talk about a QA/QC activity, are you talking about QA/QC for this process, or the effect that QA and QC have on risk?

MR. ERNST: No, the QA/QC for the work being done on NUREG 1150. It's our internal process to assure a good product.

MR. WARD: Remember, Bill, you always used to say that the NRC didn't have its own QA program; now they have.

DR. KERR: This is being developed at Brookhaven,

I just thought I heard. And it's only for this project.

MR. ERNST: There are a few blocks here. This one should be a colored block. There are a few blocks here that do not have a formal QA/QC process involved at the present time. That basically isn't here.

However, we have substantial reviews by ourselves and the staff at NRC and, of course, each lab has its own internal QC/QA process for the work.

When NUREG 1150 comes out, it's coming out in draft form. There will be additional QA/QC so to speak at that time.

For the preparation of this NUREG itself, we clearly have our own involvement in the technical aspects of the review. We'll be writing the NUREG itself with NRR looking over our shoulder and, in some cases, perhaps even helping out with the writing.

We do have several consultants to look at this work and knock us on the head if it looks like the work needs some improvement.

After publication, we'll go to a public comment process on the document and I intend to run a rather large workshop to critically review the draft before we finalize it.

DR. MARK: Roughly, how many FTEs are devoted to that formal QA/QC effort?

MR. ERNST: Roughly 25 percent of our funding is supporting that QA/QC effort.

DR. MARK: And the funding?

MR. ERNST: We're right about 6 million.

DR. MARK: We're talking about 1.5 million.

MR. GILLESPIE: That's 6 million for you guys. We're putting more into it also. We're probably in total close to about $14 million, and about 25 percent of that is on quality control.

DR. MARK: Hot dog!

MR. GILLESPIE: We're up in the millions of dollars in the control of quality.

MR. WARD: If that's just called technical review instead of QA, does that make you feel better?

DR. MARK: If it were 10 percent, it would make me feel even better.

MR. ERNST: Some of this is a mixture, because it doesn't just take place at the end. The QC teams, for example, on a particular plant -- Joe can speak to this better than I, because I recall the QC teams meet several times just on the front end, and in QA, the senior consulting group meets several times on the plant, but early on, they're trying to help provide guidances to make it more

efficient, but it is independent input, and it does run sort of parallel with the whole process.

So it is a mixture between what you've conventionally called end of the line QA and actually production line QC, making the product good as you go along.

(Slide.)

A couple of words on schedules. This is the latest schedule. I can't get it all on there, but you've got copies in your handouts. Schedules has been a real bugaboo. We established a schedule last June, which was admittedly simply impossible to reach, because it assumed that everything went according to Hoyle, and nobody found a problem. There were no starting problems, no problems with source term code package runs. The senior consulting group agreed with everything done in the plant analysis. Those things just don't happen.

I am very interested in a quality product. These schedules have been slipping to make room for the quality. I have made some decisions which said don't redo something on the basis of a certain QC or QA finding was not that risk important. It might have been proved. The complete validity of the whole thing and be perfect, not even perfect, since this is PRA, and I don't know what perfect means in PRA. It might have had something that everybody

agreed was not right. But why spend another $20,000 doing a source term code package run, if the run itself is not going to make much risk difference.

So there's been some of those kind of decisions, but where there's clearly a comment that comes back from the process that says improve the product, and that improvement is risk important, it gets done, and that causes a slip.

DR. SIESS: What do you mean "improve"?

MR. ERNST: People agree that, yes, that comment on the modeling for Peach Bottom, for example, is, indeed, true, that the ASEP team, putting that modeling together have made an area, because the plant is really configured this way or has this procedure, or the thermal hydraulic analysis says it really goes this way instead of that way. That kind of improvement.

DR. SIESS: That is correcting errors.

Suppose the comment comes back that you did it right, but you made the wrong assumptions. Why didn't you take this into account, or why didn't you take that?

Does that mean go back and do it over? Do you agree, or what?

MR. ERNST: Joe can speak. He's on the senior consulting group for this effort. I think it's both of these. It's not just protecting errors. It's finding out other things, other assumptions that should be considered.

DR. SIESS: The final point is that when everybody agrees something is okay.

MR. ERNST: Yes. Or you reach a decision. I don't think that the process of human nature says everybody is going to agree on everything, but you should at least flag the issue and make a decision.

This schedule is probably not even good right now. I am quite sure there'll be a date on there that's changed. Even if I had it up-to-date when I caught the Metro down here, it's the best we know it right now. If I have anything to predict at all, it is that the schedule will slip some more. We will be finding some more problems. I'm still interested in quality work. If I had to bet money, I would bet that NUREG 1150 will not come out in the middle of August as advertised.

Right now it's iffy. We can still make it. But based on past experience, I just can't imagine everything isn't going to happen beautifully from this point.

DR. KERR: So it might be the end of August.

MR. ERNST: It might even be into September or something like that.

DR. CATTON: On the diagram that you had on the previous page, there's a big arrow, which comes from BMI 2104 runs.

MR. ERNST: That was input, as well as the

original estimate a year ago, ASEP information on dominant sequences. That and other information was used to pick the dominant sequence that should be looked at in what we call binning to determine the sequences that should be run, using the source term code package.

DR. CATTON: Where does the arrow from 0956 enter the matrix?

MR. ERNST: As a matter of interest, I don't believe that there's a single sequence in 0956 that is being used in 1150, because of minor changes in how the sequence goes, based on the rebase lining work and other information. So I don't think there's a one to one correspondence between any sequence we have now and what's in 0956. My understanding is that the revised 0956 will have the source term code package information.

DR. CATTON: But it won't be ready for you to do anything in time for August, will it?

MR. ERNST: Yes, they're using our stuff in 0956. They're going to use the NUREG 1150 information. And I think we're through now with all the source term code package runs That information will be in 0956.

DR. KERR: I'm confused now, because I thought 0956 was supposed to be the basis for 1150. Now I seem to be hearing that 1150 is going to be the basis for 0956.

Is that the current status of things?

MR. ERNST: They probably should have let somebody from 0956 speak to that.

MR. GILLESPIE: Mal, let me. 0956 describes a modeling package, a code package. The original 0956 was the original BMI 2104 suite of codes, of which 19 runs were done. Some time has gone by. We've gotten criticism on that. Ther was too much manual interface between different models in the BMI 2104 suite. That was basically streamlined and changed to what is today a source term code package, which the revised 0956 is going to describe. It's going to describe a different code package than was in the original 0956. It will describe the source term code package you used today. That source term code package is there and running.

The fact that we haven't got it written up yet in 0956 does not preclude 1150 from using that code package. So they're off using it now in the new 0956, where we're doing some things now to translate or make a step from the original code package of a year ago to the current code package to show the relationship of the changes and what differences they made. And the new 0956 will, in fact, describe the code package which is being used in 1150. So it's a set of documents. One feeds into the other. But 1150 took the code package and went off and made their runs.

They're not dependent on 0956 doing exercising the code iteratively for them.

Now for our examples in 0956, you remember chapters 3 and 4 had some codes and examples. Those curves and examples will now be drawn from the approximately 30 runs of the source term code package that are being exercised iteratively to support 1150.

So the code package is not dependent on 1150. We're going to use some of the work done for 1150 as part of our illustration in 0956.

DR. KERR: So if one reads in the current version of 0956 that the Staff now recommends that this be used for all the calculations, that was true when it was released, but I can disregard that at this point?

MR. GILLESPIE: No, because we're probably going to show the difference between BMI 2104's original package and the source term code package as it exists now. It is a matter of efficiencies, feedback, loops in thermal hydraulics to make more consistencies. There were some internal inconsistencies in conservation.

Help me out. I'm past my second sentence.

MR. MEYER: Ralph Meyer, from Research.

We talked about these before, but just to remind you quickly. There were several cases where in different parts of the original BMI 2104 suite of codes, we were using

different models to calculate the same thing. It was a core concrete interaction model called INTER that was in the MARCH code. Then there was CORCON Block 1, used external to the MARCH code. The same set of calculations. There was an in-vessel fission product release model called FP LOSS in the MARCH code.

Externally, we were using CORSON in the same set of calculations at different times. We were calculating the same phenomenon.

DR. SAUNDERS: So how did you make them consistent? Take the two outputs and average the two?

MR. MEYER: What we've done for the BMI 2104 suite of codes, what we attempted to do was to tolerate the model that we thought was not the best choice in areas that we believed were not very critical.

In the final code package, we have eliminated the models that are not the best choice and have only a single model for doing the calculations.

DR. CATTON: Actually, I was trying to ask a much simpler question. I understand what BMI 2104 is and what NUREG 0956 is, but there have been a lot of criticisms of some of the phenomonology and the impact that certain things may have on certainty. And I'm not talking about hypercube sensitivity. That's not what I'm talking about.

Some of these criticisms are fairly well written

out in the ACRS letter.

Before you draw an arrow into this matrix, don't you have to address those uncertainties?

MR. MEYER: We certainly do address those uncertainties. And let me say that the criticisms have been raised in a number of areas which we call issues. We're quite forward about this. We list issues where people have taken different opinions.

You may believe that your opinion is correct, and therefore, that we should confirm to your opinion on a certain subject.

We don't necessarily believe that just because we receive a call that it is correct and our position is wrong.

We are investigating all of those issues. We'll discuss the status of the resolution, our attempt to resolve those issues in the final report. We are not going on oblivious to that, but neither are we coming to a halt, because there are differences of opinion in several important areas.

I'd like to make one comment on a point that you raised, Dr. Kerr, and that was, you said in our draft version that we recommended the use of this code package, and now we have a new code package. I think the implication was, should you disregard our previous statement.

DR. KERR: It was a question.

MR. MEYER: The fact of the matter was, in the draft report when we made that statement, we had already introduced the notion of the code package. At that time, we knew what changes we planned to make, in order to resolve the egregious errors, the things that could be fixed, that we all agreed needed to be fixed, and the recommendation was made with reference to the code package that was to be assembled from the BMI 2104 suite of codes.

DR. KERR: I misread the 0956 then.

MR. GILLESPIE: The question is, will the final reflect those words? The final will probably have those words in it, but we'll change it back to the words. The new code package will be there rather than the year old BMI 2104 suite.

DR. KERR: My misunderstanding.

Yes, Mr. Bender.

MR. BENDER: If I could go back to Dr. Catton's question for just a minute. I think I would certainly agree with you, Ralph, that just because something is criticized does not mean you have to respond in a direct way and change what you're doing because of the criticism, but it does seem to me that at the front end, you have to say something about the sensitivity of the result of the criticism, and that ought to be out there before you go through these expensive

exercises. And I would think you'd plan to do that, but you didn't say.

MR. MEYER: What is being done in 1150 that was not done in 0956 is an explicit accounting for the related uncertainties in the analysis. Now we reported on an uncertainty study which maybe it was a sensitivity study called Quest, where three sequences had been looked at to try and get some idea of the degree of uncertainty resulting from these various controversial areas.

That was very selective and only addressed three sequences. It was very preliminary. We have done two things because of the importance of that.

We agree with you on the importance of that. One is for the risk rebaselining work that's being done. There is built right into the program an uncertainty analysis. So for every case that they do, they will have an estimate of uncertainties.

In addition to that, we have started a new uncertainty study at Brookhaven, which will try and evaluate the uncertainties a little more rigorously than we were able to do now in the risk rebaselining work.

So really, we're doing everything that we think is possible on this uncertainty analysis.

MR. BENDER: I didn't intend to express the opinion that you didn't, but the sequence for that doesn't come out very well. How much of the evaluation of criticisms is going to be out there before you go through this extensive numerical analysis, which you're going to do?

It seems to me you ought to make that evaluation early, put it before whomever it is that's judging and get some response from them.

MR. MEYER: I've seen the comments we have received from the general public. They're quite voluminous. I haven't read quite all of them yet. But, Jocelyn, you can correct me if I'm wrong. I think we've flushed out most of the important technical criticisms early on in our peer reviews.

We're getting some additional information. We certainly are making changes in our 0956 report in response to these comments.

But, in terms of the basic technical areas, I think we've pretty much had them identified from our peer review process.

So I don't think it's premature to go through the calculational exercise. It's true, if you were to look at a graph of knowledge versus time, we're not at some plateau up at the top where we know most of what there is to know.

And so what we do will soon be outdated.

MR. BENDER: Doing some analysis, I would not quibble with. But doing some analysis is not the same as systematically going through the criticisms and saying:

If this assumption is incorrect, the result of the analysis might be sensitive to it in terms of its end evaluation to some degree because...

MR. MEYER: I think we are doing exactly what you said. There was a meeting at Sandia just about three weeks ago where we assembled the panel of experts that we've been using for the source term work and some other people, where, systematically, through the containment event trees and through the source term work, we listed exactly the issues of controversy you're talking about; indicated what we thought the range of variation of the key parameters would be, polled the experts to see what weight they would put on the various values in the range in order to try and perform the uncertainty evaluation that's going on.

So I really think we are doing exactly what you suggest.

MR. ERNST: We are trying to mesh in here pretty closely that process as described in the uncertainty letter that I sent to Jack Hickman. I think there's a copy passed out on that and you'll hear more words from Mark Cunningham on this process, too, or from Joe. Well, you're going to talk about the issues though. So I think you'll hear more

about that.

I think we are offering it in an integrated fashion.

DR. KERR: Please continue.

MR. ERNST: Let me do something a little bit out of sequence here.

(Slide.)

Let me say a couple of words about the future schedule. I still say August '86 down there for the futures, but I have also said that if I had to bet a paycheck on it right now, I wouldn't bet a paycheck on August '86. It's still possible but not likely, no matter what kind of degree I put in it.

The technical report should be finished and depend on the report or the plant in the May-September kind of timeframe. We would look for public comment in the September-November kind of timeframe. We have a workshop currently planned for early fall. A final NUREG 1150 considering all public comment likely in the spring of '87.

There are currently plans to do B&W and CE. B&W should be of extreme interest because of the less forgiving nature of thermal hydrolics in the plant.

Built into the process is a continuing look at results of new research and the containment of the core concrete area and, as warranted, an update of 1150 if it

looks like substantial changes are occurring that would make big differences in the conclusions we draw.

Then one would consider updating 1150. At the present time, I had only planned on analyzing these two plants and providing a supplement that gives the reactor information about those two plants.

I did have some slides on the content of 1150. We've gone into some of that already in sort of peripheral conversations here. What I'd like to do is go on with Joe's talk on the methodology, and I'd like for him to start with the source term, because I think we're going to lose some people from DAE.

So it's sort of in the middle, but we've been talking about source terms so maybe we could sort of wrap that subject up. Then Joe could come back to the beginning and talk about the accident sequences, and then Mark can talk -- Joe will also be talking about uncertainty. And then Mark will have some words to say about the specific issues we've identified and this range of the issue and degrees of belief that Ralph's been talking about.

Then, time permitting, if I could come back for more on the contents of 1150. But I think you can read the detailed outline, so maybe that part will just be questions and answers, on 1150.

Does that suit?

DR. KERR: Mr. Davis.

MR. DAVIS: Can I slip in a quick question before you go on?

I was wondering what the consequence would be of missing this schedule. Is there some pivotal regulatory decision that depends on 1150 output, or some other policy statement, like the severe accident policy statement or the safety goals?

MR. ERNST: No, it's not safety goals. Basically, severe accident policy, the original schedule was set to try to make as good an interface as possible between the ongoing work between NRR and IDCOR to develop a methodology of extrapolation to other plants.

We have been able to provide some information, clearly not as timely as I would like, and clearly not as much as Zoltan would like, but we've been trying to do the best we can. That was the driving thing.

The second driving thing, and maybe Zoltan has a word to say about that, is there are some times listed in the severe accident policy for coming to the closure and getting on with the process. So, clearly, that drives, too.

There's a strong inclination to try to take the extent of knowledge that we have and see whether or not some regulatory changes need to be made. The position right now

is we need to see what draft 1150 says before we can really start doing anything useful in the regulatory process. So that's clearly a driving force.

I don't know whether Zoltan has anything to add to that or not.

MR. ROSTOCZY: Basically, it's the same thing, that it would depend on it in two senses. One of that is the result, the information that we can use without having a printed report. Again, when it becomes available, we intend to use it, so that's one dependence.

The second dependence is the public comment aspect of it; 1150 is supposed to put out for public comment the methodology of these studies that are being done and the actual calculations for the six reference plants are supposedly to be put before the public to see, study, comment on, whatever.

For that purpose, we need the completed report whenever it is available for public comment and when those comments come back is when we have one of our inputs for our work.

MR. DAVIS: Thank you.

MR. MURPHY: Mr. Chairman, with your forebearance, I'll sit instead of stand. I either have one ligament in my knee that's a lot longer than it was a couple of hours ago or I've got one that's in two pieces; I'm not

sure which. I hit a patch of snow on my way in here, and there's no way I can stand for an hour.

(Slide.)

I'll try to describe the methodology we're going through, both in terms of what we're doing and how we're doing the uncertainty analysis, and then proceed. But I'll take it in a somewhat disjoint fashion to meet the needs of our people by starting with what we're doing on the containment of entries and the source terms, and proceeding then into a discussion of uncertainty as it applies to those features.

And then I'll come back and discuss the so-called front end, the risk rebaselining of the accident sequence frequency and go from there to what we're doing in the consequence modeling. So it's not necessarily the logical flow of information.

May I have the next one now?

(Slide.)

On the containmant analysis, we're developing the detailed containment event tree for the reference plant. We're actually in this reduction program.

I'll show you an example of the type of questions that are on these trees. These are very large trees. They have undergone peer review by a team put together by Mike Cordini of the University of Wisconsin.

The containment analysis considers the accident progression, the fission product release, migration and removal mechanisms and phenomenology uncertainties and process-induced failures.

Implicit in developing the tree is the consideration of the IDCOR and SASA issues. As we get into the uncertainty analysis, part of this...

(Slide.)

...this is a very abbreviated example of what a containment event tree looks like. They're what we call virtual trees. They're done in a generic way that can apply to any plant of a given type.

They begin with a number of questions on the tree that specify the conditions under which you are entering the tree. So, to a large extent, the first 20-30 questions on the tree have to do with the finding of boundary conditions in the analysis.

For this reason, the trees are very large. You have more than two possible choices and you have well over 30 or 40 questions. It doesn't take long to realize that, too, that 30 or 40 is a rather large number.

The trees are not as complex as that sounds, so many of them are definitions of boundary conditions. The questions that are asked on the tree are questions like: the state of the plant. Is the vessel depressurized? Are

the drywall sprays operated? Is the containment acutuated? That is, dry wells.

Some of these are questions on the performance of the operators. Some of these are defined by the basic input conditions of the accident sequence hereon. It's worth noting that, in some cases, these conditions are not accident sequence specific even, but rather cut set specific. There are different ways you can get into the various accident sequences.

All that has to be factored into your analysis of the event tree. Basic questions of the event and timing of the plant conditions and the various phenomena involved. What is the containment failure pressure? How do you handle it? What kind of delta P do you get due to hydrogen burning, and that sort of thing, were all represented in this containment event tree.

The next one now.

(Slide.)

We've used a variety of sources of information for this. Certainly, a lot of the work that's coming out of the source term code package and the various codes that we've used were also obtaining information from the containment loads working group and the containment performance working group. The various analyses that have been done under SASA. Assorted steam explosion review

groups. Generic issues studies.

Essenti-lly, everything that's been done that can provide us information and understanding, how to work our way through the containment event trees and what the possible steps through those trees are is being factored in, as I'll describe when I get to the uncertainty analysis.

The weights we put on various portions of the tree and how we progress through are handled as we consider the uncertainty in each of these basic elements, if you will.

MR. BENDER: Joe, are we still operating on the basis that we're analyzing the plants as they are, or are you analyzing the plants as they might be?

MR. MURPHY: We are analyzing the plant as it is.

MR. BENDER: What help is that?

MR. MURPHY: There's one minor exception to that.

MR. BENDER: What help is that going to be?

MR. MURPHY: I think you have to know where you're starting from. One of our first steps as we do the accident sequence rebaselining, and I'll get to that as I talk to that, is to send a team of people to a plant for a week to find out what the as built drawings are, what the actual procedures are, how does the plant implement the procedures?

I don't think you can really say you know what

the current status of the plant is until you've done that.

MR. BENDER: I think that's what the operators might ought to be doing. It seems to me, expressing, obviously, just an opinion, that what the NRC ought to be doing is trying to find out how the answers might be influenced by certain pieces of technological treatment that could be included in the sequence. To look to see whether a plant is vented or not is one thing. It might be better in my mind to say what could be the effect of the thing if it were done?

MR. ERNST: We'll have both.

MR. BENDER: That's what I'm trying to find out. What you've told me so far is you're going to see what the plant is as it exists and do the second part, which is to look at the sensitivity of it, the changes in design, that seems to me to require some kind of criterion for judgment. I don't know what the criteria are.

Do you?

MR. MURPHY: I'm not sure I understand your question. There's essentially two steps in the analysis. One is to find out what the risk of the plant is today. That's as best we can within the resources we have. That's the process I've been describing so far.

Then there's an evaluation of risk reduction options. What options could you take that would reduce the

risk?

Now, some of these come...some of these can be handled as sensitivity studies off the basic model. Others require changes in the model to implement. What would you do if you installed the new feature X -- perhaps it's a bunkered RHR system, or something of that sort?

You could also take the models and preturb the models and rerun the case and see what value you'd gain from various risk reduction options. That is also being done.

DR. SAUNDERS: Let me understand this. If you have a large fault tree or event tree that's specific to a plant, you know the boundary conditions or the range of variation of every input. You calculate then the associated risk through your probabilistic analysis.

Now you come into an analytic question which says: What can I do for, say, a given amount of money which will reduce the risk the most?

If your model is correct, that's an analytic problem, what are you doing simulating things? I don't understand that.

MR. MURPHY: What you have to do is rely on the judgment of your analyst as well as the history of the sort of things going on around the country, things that the committee has suggested in the past, and go back and perturb your models and see what would the risk be if I changed the design of the plant to incorporate some option for risk reduction.

That way you evaluate what you get if you implemented that option.

DR. SAUNDERS: Yes. My question is: why isn't it an adequate procedure to try to say, given this amount of money, what can I do to the plant to reduce the risk the most?

MR. ERNST: That is basically what we are doing. We are using several things as criteria, one being the $1000 a person rem set forth as a safety goal.

The display hopefully will show whether or not certain risk changes -- how they match up with the $1000 a person rem, for example.

MR. MURPHY: I will have a slide in a little while that discusses the way we are doing the risk reduction and risk rebaselining. That may help a little bit.

DR. SAUNDERS: All right.

MR. DAVIS: Joe, what do you do about changes that have not been made at the plant but have been

mandated by the NRC and are scheduled for implementation at the next refueling outage, for example, like the Mod 3A ATWS situation?

MR. MURPHY: With the exception of the ATWS fixes, we are modeling the plant as it is the time we visit it. With the ATWS fixes, we are including what they are permitted to do.

MR. DAVIS: Are you going to acknowledge in your report that other changes are coming? You know, you may conclude that the plant needs to have some fix put on it, but with changes that are being mandated that conclusion may not be valid.

Do you see what I am saying?

MR. MURPHY: I see what you are saying.

MR. DAVIS: You may find out that the B sequence dominates risk.

MR. MURPHY: I can't promise that we are doing it on each and every thing. There have been so many things that are being backfitted.

But for the major items that we are aware of from our team going to the plant and talking to the plant as to what they plan to do in the near future, many of those will be treated as sensitivity studies.

But the base case is essentially based on the plant as is, with the exception that the ATWS fixes are

considered to be installed and operated.

MR. ERNST: As is? If you have something that is going to be installed at the next shutdown, you have the drawings where you have the procedures that are going to be implemented, and they have been instructed to consider that as an accomplished fact.

If they don't have the drawings, don't have the procedures, we don't want to guess how the thing is going to be implemented.

MR. MURPHY: We have essentially the final design and the procedures to operate the system, but if we hear there is going to be a redesign of System X two years from now and all we can hear is general statements as to what the purpose of that design is and we don't have PMIDs and electrical drawings and operating procedures, we assume it doesn't exist.

DR. KERR: Well, now under the current operating procedures, if it is a backfit, then the calculations of risk reduction have already had to be made, haven't they?

MR. DAVIS: Not in all cases.

DR. KERR: Well, if it is a backfit mandated today, that would be the case, wouldn't it?

MR. MURPHY: A backfit mandated today. But there's newer TMI fixes in the mill.

DR. KERR: I have been told it is impossible to

calculate risk reduction due to TMI fixes.

DR. SAUNDERS: By whom?

DR. KERR: On several occasions by members of the NRC staff.

MR. DAVIS: I think this may be a bigger problem than you may be aware of right now.

For example, B&W is putting an electric driven aux feed pump now into their Davis-Besse plant. I don't know how you would consider that if you were choosing their plant at this point in time. But it would make a big difference, I suspect, on the core melt problem.

DR. CATTON: They are also planning a high power vent of rather large size.

MR. DAVIS: Plants always have these things they are thinking about doing, and at least when I have talked to them there is always -- I know you have got to draw the line.

MR. MURPHY: At some point in time you have to fix the design and progress with your analysis or you never get off of go.

MR. DAVIS: It is a problem, and I don't know the answer.

DR. KERR: This is a living document, Pete.

DR. SAUNDERS: What he says is reasonable.

(Slide.)

MR. MURPHY: In terms of what we are doing with the source term, I think we have covered most of the material already. The methodology is essentially the same as that used in BMI-2104 and the N-0956, with some improvements.

I think Ralph has already covered the points that I have here.

The Surry analysis will rely heavily on what is presently N-0956. There were only a few source term code package runs to support the analysis of the Surry plant.

The other plants will rely primarily on new runs, using the source term code package.

Basically, our tool for the source term methodology is the source term code package.

MR. DAVIS: Joe, on that Surry analysis, what do you assume about whether or not the RHR pipes are underwater for the feed sequence?

MR. MURPHY: We are treating it probabilistically, based on a judgment call, if you will, a Bayesian analysis as they get to the uncertainty.

Basically the drawings were looked at by a team of five experts, and looking at the situation, a subjective estimate was made as to whether it would be underwater or above water. That weight is factored into the uncertainty analysis.

As I recall -- I don't recall the number, but I think they were leaning towards about a 75 percent chance that it would be underwater and about a 25 percent that it would be above water.

MR. DAVIS: That is an important number, I suspect.

MR. MURPHY: I expect so, too.

(Slide.)

I think again Mal pretty well covered the binning process.

Basically, the way we get from the front-end analysis through to the exercise of the codes is by many accident sequences into -- for want of a better word -- bins or groups that have the same characteristics in terms of how we would expect the accident phenomenology to be.

We start off by identifying the accident, the dominant accident sequences associated with the rebaselining effort to the estimate of the core melt frequency. We take the containment of entries that these sequences feed and reduce those to the key containment failure modes and then proceed on to say which of these situations have we had source term code package for 0956 runs that describe this process. Are you aware that we need more analysis?

This is a judgment call because every sequence is slightly different. The binning process is an amalgamation

that causes you some uncertainty, and we rely on the judgment of a team of experts from Sandia, Battelle, Brookhaven, Oak Ridge, as well from NRC.

Coming out of this, we get release characteristics for each bin. This means whether the primary system is pressurized or not, whether the sprays are operating or not, whether you would expect the core to melt before the time of containment failure or after -- these type of characteristics -- and then the source term code package is run.

Those runs are deemed to be representative of all the sequences which get into that bin. Typically, we wind up with something about 15 bins per plant.

DR. KERR: Joe, what is meant by a dominant sequence? Is that based on risk?

MR. MURPHY: It is based primarily on core melt probability, but recognizing that the sequences that dominate core melt don't necessarily dominate risk, we look at everything that is greater than a 1 percent contributor to the core melt probability. And therefore, I think it does -- that discrimination is low enough that I think we can pick up the risk as well.

(Slide.)

Proceeding onward, let me talk a little bit about sensitivity and uncertainty analyses.

Our objective here is to provide a good engineering perspective on the modeling assumptions that are driving the analysis and to provide a reasonable envelope in which the actual value would likely be found. We do not feel it necessary to express this in terms of a formal statistical bound.

In other words, what we are trying to express is an engineering judgment, calculated using statistical techniques in many ways, but recognizing the impreciseness of our input data. We are not trying to assign these results as prec.se 5/95 percent bounds or anything of the sort. We don't believe they are defendable as such.

Rather, we are saying that we believe coming out of this process these are reasonable engineering bounds that express a range in which we think we will find where the outcome would lie.

If you have more confidence in the statistical methods than I do, you could probably interpret these in a statistical sense because they are calculated formally.

I would caution against that, however. I don't think our input knowledge is based on a degree of belief type of assessment. I am far from convinced that if I asked an engineer for his 90 percent degree of belief that I would get a different answer than when I asked him for his 95 percent degree of belief.

What I can get from him is what he thinks is a reasonable engineering upper bound, and he doesn't know whether it is 90 or 95, and I don't either.

So we are treating it in this general engineering approximation sense rather than in a formal statistical sense.

DR. KERR: I think it is entirely reasonable. I don't see how you can do anything else.

But what I would hope is that where you deem things to be fairly important, that you give a little bit of discussion as to how this process might have developed.

It seems to me there will be some engineering uncertainties on wheezier grounds than others. It is helpful to people that are trying to judge that have some feeling for the degree of uneasiness that might accompany a particular decision.

MR. MURPHY: We are not at a point where I can give you a specific example yet. I have a backup slide that gives sort of a theoretical example of the hypercube. If you want to take the time, I can show you how it works, but we are not at the point where I can give you a specific example yet in terms of our understanding of the uncertainty analysis.

(Slide.)

What we are using to handle the uncertainties is

the limited Latin hypercube method. This is a constrained or stratified Monte Carlo sampling scheme.

Now, feeding into it is not just, say, the source term code package.

DR. KERR: Excuse me. Is this connected somehow with the earlier statements about good engineering judgment?

MR. MURPHY: Yes. This is the way we are doing it.

MR. ERNST: You use your engineering judgment to get at your ranges and degrees of belief. That is what Joe meant by then you take this and put it into a statistical sampling process.

MR. MURPHY: The problem you get is that you have a very, very complex problem.

Now, I can give you one thing, and I could get a team of experts together in the room and say let us look at the bottom line number and everybody give me an estimate as to what the uncertainty would be. We could do that. I don't think it would be very meaningful.

But instead what we are doing is looking at the major parts of this containment. In particular, now we are talking about the containment of the source term analyses, and we are trying to develop 10 to 15 issues which we think drive the uncertainty in our ability to predict what happens for each one of those 10 to 15 issues, going through

a process where we try to estimate what the probable possible range on this phenomenon is.

One of them might be direct heating. That is an issue in itself. Mark will talk about the specific issues later.

But we will take each of these issues and say what is the range -- not only what does the source term code package predict for this, but what is a reasonable range, considering the work that has been done, on request, the IDCOR results, everything else we know.

Bring people together in a room and try to estimate potential values for these phenomena as they could occur.

DR. CATTON: Could I maybe have you give me an example?

The CORCON/VANESA get tied together. One thing they found, that the temperatures that CORCON predicted were too high. This leads to a particular kind of a tack, and it leads to some conclusions about the goodness or badness of VANESA.

The KFK tests show that the temperatures are lower. The TV camera shows that you produce big bubbles.

Now, those are very different. What do you do with that?

You get two experts in the room, one of whom has

never seen the KFK test and one who built the code. Do you vote? What do you do?

MR. MURPHY: In a case like that, rather than trying to say this is my degree of belief; namely, I believe one and this would be the other, given conflicting information, neither of which I can discount, I would weigh each one of them as 50/50, and I would go into my Latin hypercube saying either one of them could occur; which is the probability?

DR. CATTON: There is another example. That is this in-vessel recirculation. You fail some of the small pipes on the hotleg. You fail the surge line, or you fail the steam generator tubes.

What do you do now? Divide it into four and various combinations?

DR. SAUNDERS: Sure. That is what he said.

DR. CATTON: Is that what you would do?

DR. SAUNDERS: I don't know.

MR. MURPHY: For each of these issues -- and we are going to select about 15 of them. I don't have the list with me now. Mark does -- we are taking these and we are looking at the possible range of outcomes that comes out of this expert panel that we put together to look at them.

The expert panel has a variety of different degrees of belief. Some of them, if you would, are very

conservative in their approach. Some of them are very optimistic in their approach.

DR. CATTON: Are any of them deterministic or physically based?

MR. MURPHY: To the extent possible, they are based on what calculations have been done. But in a number of these areas, we are at a point where we may have models without good experimentation to validate them. We are in areas where there are different models, and the IDCOR results differ widely from some of the calculations we have.

DR. SAUNDERS: The point is, I can understand how you can ask an engineer for an envelope of possible values. Most of them will agree on limits. Whenever you start sampling within that range, the idea that you would take a uniform distribution or any other distribution is what's arbitrary and which governs, as you will admit, the distribution of outcomes.

MR. MURPHY: I fully agree it's arbitrary. Not everything is flat. There are some cases where we could say as we look at the technical community, a large body of the technical community believes that the thing is somewhere int the middle, but there are some people on both extremes that say this could be a bad problem, or this is no problem at all, and they have done some calculations to quantify this. That will give a lower rate to their opinion, but we won't reject it.

DR. KERR: Then you're going to weight the opinions of experts. Expert 1 will get more weight maybe than Expert 5.

MR. MURPHY: Effectively, what we did is, Ralph started it. For the first plant, we had a meeting in Albuquerque a couple of weeks ago, where we had essentially the teams comprised of our review groups of the source term and containment. Each member -- the Sandia personnel involved in the study gave their idea as to what the

potential outcomes would be. Their base case, if you would, would be based on the source term code package run, but then, recognizing some phenomenon that may not be in the source term code package that somebody thinks is important, they look at various options as to else a phenomenon could behave rather than the way our present model calculates it. And the experts are being asked to give their opinion as to what weighting factors would be appropriate to use in the sensitivity analysis to represent the possibility that these other things could exist.

These weights are roughly akin to a degree of belief, but I suspect the large majority of the people who are involved in establishing them would object to calling it a degree of belief, because their spread is wider than their own individual degree of belief on it, but rather what they think is appropriate to use as a spread.

Given that this happened, we asked each of the chairmen of these groups to take the results of these individual votes, sit down with their members and give us a consensus opinion of what weights should be used in doing the analysis.

So there's a distribution. In some cases, the distribution is flat. In other cases, I might point out that the nature of the process here is a discrete probability value. We're not inputting curves, we're

inputting specific discrete values and assigning weights to those values.

DR. SAUNDERS: But they're all unimodal. You don't have engineering opinion on one hand, shifted to this hand of the bound, another one which shifts the other end.

MR. MURPHY: Only to the extent that since we're using discrete values, we're saying, if you calculated using method X, you get a point here, and if you calculated it using method Y, you get a point here. We don't allow for anything in between.

So to that extent, you can call it dimodal, but it's because we're using discrete values and assigning weight to them. If the team of experts felt that there was potential for ranges in the middle, then discrete values would be used. The technique we're using to handle 10 to 15 issues in 3 to 5 specific values for each issue with weights assigned to it and then progressing through this with the constrained Monte Carlo technique of the limited Latin hpercube, there will be 20 to 30 runs made samples through the three. The nature of the sampling process is that it's constrained to sample from various parts of the distribution. So it's more effective in terms of the number of runs that you made than the standard Monte Carlo.

The disadvantage of that is obviously, if I'm making only 20 runs through, if I assign something a weight

of 1 percent, that's the constraint put into the limited Latin hypercube.

There are some disadvantages associated with it, compared to an ordinary Monte Carlo, but we feel they're well outweighed by the advantages and the fact we can make many more runs and cover much more ground.

DR. SAUNDERS: But of course, information always behave like the square root of the number of trials or sample size. So the more you run, the more information there is. Presumably, the best thing would be to run all possible values, but that's too expensive. So what you do, I'm trying to understand. You take the range of values and sample from those independently -- not independently. You select what you call a Latin square in several dimensions. Then sample from that a fixed number of times.

MR. MURPHY: Let me show you a fairly simple example. If I had seven issues with three potential values for each issues --

(Slide.)

-- I'd wind up with different weights assigned to the different levels on each issue. On X 1, for instance, 40 percent of the samples, I've weighted at level 2, 40 percent. So if I run 20 samples, I'll get 40 percent of 20 or 8 samples that involve level 1 of X 1 mixed up with some of these other samples here, but I'm constrained to take

40 percent of my samples from that value.

So if I get to a point where I have a 10 percent weight, at least two of my runs would involve that 10 percent weight, but it could be involved with any of the other parameters. It's a way of assuring a sample space, in terms of each of the individual parameters. So it gives me a more uniform sampling technique, but obviously, I can miss things by the contrained nature of it.

MR. ERNST: There might be a couple of points that need to be made, and someone correct me if I'm statistically incorrect in what I'm saying. When you finished the limited Latin hypercube, let's just, for argument's sake or example's sake, say that there's 20 runs, and then we're going to, in most cases, go to a 595 on the LLH runs and say that represents a reasonable range of mean values. That isn't the 595 percent of the number, but that's a reasonable range of mean values.

DR. SAUNDERS: The expected output has this number, and then expected output will fluctuate with it.

MR. ERNST: It isn't a full range of uncertainty.

DR. SAUNDERS: I understand that.

MR. ERNST: It's very important to understand that, and then Joe mentioned this 1 percent item that would more than likely not be sampled, if that is an important

item to risk, it will still be identified in the writeup, in my view. And say it did not get picked up in the reasonable range of means, but there is a possibility of -- and then whatever the risk importance importance and the origin of that uncertainty might be.

DR. SAUNDERS: But the point is that when you say mean, what you're doing is, you're averaging across the set of opinions.

MR. ERNST: Sure.

DR. SAUNDERS: And it's just an expected value. We get a measure of what the expected value will fluctuate.

MR. ERNST: There'll be a range of expected values.

DR. SAUNDERS: Yes, I understand that, but again, you say it does not give us any idea of the total range of outcomes.

MR. ERNST: We cannot advertise that as a 5.95 percent on the range; that's correct.

MR. MURPHY: What we're going to do, the way we're treating it is, we're going to take the 5.95 percent limited Latin hypercube, which means that in the case that I had 20, I'd eliminate the high and the low.

DR. SAUNDERS: I understand that.

MR. MURPHY: I just at that point treat that band I have as a reasonable engineering estimate as to where I

think the value may lie, with no statistical support, and so on.

DR. SAUNDERS: These are not confidence intervals, in the sense that the statistician uses the word. All he's doing is chopping the range. He's chopping the upper and lower ends of his distribution of expected values.

MR. ERNST: Absolutely correct. We don't want to advertise this at something that it isn't.

DR. CATTON: But it's really averaging across a set of opinions.

DR. SAUNDERS: Of opinions.

DR. CATTON: Times expert forcefulness.

MR. MURPHY: It's taking the expert opinions.

DR. CATTON: In a group of experts, one can dominate just by sheer personality, whether right or wrong.

MR. MURPHY: I hope we have enough people involved that that's not happening.

DR. CATTON: It does happen.

MR. MURPHY: We're taking these values and putting weight to them. One thing I forgot to mention is that the code does have a correlation between issues. So the fact that you've driven one item high, may, by its very nature, drop something else low.

DR. CATTON: I mean, one of your experts may be

a very strong-willed individual and everybody capitulates to the wrong --

MR. MURPHY: I think that's a problem anytime you get a subjective judgment.

DR. CATTON: As long as your chairman is demanding a consensus, it's a problem. If your chairman is willing to use his own judgment, based on what he hears, you can eliminate some of that.

DR. SIESS: He has to be smart too.

MR. MURPHY: There's another part to this process that I ought to describe to you. What I've described now is what's going on in the laboratories. What's going on is part of the SARRP. In terms of the issue papers, we're developing, that have been alluded to a number of times, and that Mark will be talking about, if I ever get off, there we are taking our own look at the issues and we're developing an NRC position on these issues as to what NRC feels the proper range and the proper best value for each of these issues is.

DR. CATTON: Are these the white papers we've been hearing about?

MR. ERNST: Position papers.

MR. MURPHY: These may or may not agree with what has come out of our activities with the lab that I've just described. If they do, fine and dandy. If they do not,

what's in 1150 will be the opinion that the Staff developed out of the issue papers and we'll go back and rerun the analysis through the Latin hypercube, using the Staff opinion rather than the laboratory opinion.

The NUREG CRs that come out addressing the severe risk reduction program will have the laboratory's version of what they believe should be in there, but NUREG 1150 will have Staff opinion on it, which may or may not agree with the laboratory.

MR. ERNST: Let me describe the process, which might help a little bit. The process that happened in the meeting at Sandia about three weeks ago.

In any one set of issues, there may be five or six people on this panel, but there's also an audience of about 20 to 25 people, which would include other contractors, NRC people and everything else. And they're free to chime in with their own opinions as information to that panel. Then as Joe said, we're still free to make up our own minds. If we want an additional range to be described in 1150, we will ask the contractor to do so.

MR. MURPHY: I should mention that we do have one check on the process, if you will, at least the process of the limitations that are imposed by the limited Latin hypercube. We're going to be looking at two sequences, TC and TB on Peach Bottom. There we'll forget the word

"limited," in terms of the limited Latin hypercube. The Latin hypercube analysis will be done, but at this point, there will be no constraint on the number of issues to be studied or the possible values for them, or whatever.

A thorough analysis will be made of those two sequences, using this technique without the constraints which go with the word "limited," and we'll get some idea as to what we have gained or lost by the limited portion of the analysis.

DR. SAUNDERS: You have 15 variables, 5 possible states. That's 5 of the 15 different calculations you're going to make? That's what you told me a few minutes ago. 15 variables, 3 to 5 different states.

MR. MURPHY: What I'm saying is, that we've constrained the number of variables to 15, as we start off, by saying that these are the key elements. There may be 50. In this type analysis, we'll do 50.

DR. SAUNDERS: So you'll have 5 of the 50 as calculation. That's getting up there, you know.

MR. MURPHY: Within obvious constraints. At some point, you have to cry "tilt." All I can say is, from what the program's costing, they're doing a lot of calculations. I don't remember the number of issues you're considering.

MR. CUNNINGHAM: I don't think it's been settled yet, but it's much larger than the 10 to 15 we've been

talking about.

DR. KERR: You're convinced this will give you better results, that if you just ask the experts to vote on what they finally believe the risk to be?

MR. MURPHY: It would be far more traceable, with far better defense.

DR. KERR: It would certainly be less complicated. This other looks more sophisticated. I'm just wondering if the results would be different, but I suppose they would be.

DR. SIESS: When you say "more traceable," you mean more credible?

MR. ERNST: No, I said it would be more traceable, with a better explanation of what happened. Let each individual make up his mind whether it's credible or not, I guess.

MR. MURPHY: A lot of us have different opinions on this, but my own opinion is that if we had a team of experts, and we were sure each of them went through the logical thought process that's required by using asking the 15 questions, if we were sure they were going to go through that logical process, their answer probably would be roughly equivalent to what we're getting out of it, although we couldn't trace what was driving the importance of it.

DR. KERR: This is what bothers me about the

process, Joe, because of I think an uneasy feeling about asking people for their opinion. We're taking this opinion and we're massaging it with a process that looks very sophisticated, and then we're saying that massaging has made the process more logical.

Now maybe it has, but I'm reminded of the apocryphal story about the early days of computers, when CBS, let's say, had set up a computer that was supposed to analyze election returns. At the last minute, they discovered the program wouldn't work, so what they did was take people and analyze them, and then they ran a short program that permitted the computer to print what you put into it. They printed out the computer returns, and they looked much better, because they'd been printed out by a computer.

I know this is not the same process, but for some reason --

MR. ERNST: I'm very sympathetic for what you just said. I think Joe and Mark will both agree I've been campaigning long and hard to try and make sure that the statistical sampling process doesn't overpower the engineering judgment. The words in 1150 have to describe what we're doing. What we're doing is not basically a statistical process. It's basically a way of coming up with what we believe are reasonable engineering bounds on the estimated mean values.

DR. KERR: You've given it a lot of thought. I'm willing to wait and see what happens.

DR. SAUNDERS: But I don't understand why you don't take the 5 to the 50 and do a sampling to whatever degree you can afford, instead of going through this Latin hypercube, or fractional, incomplete balanced block designs, or whatever you want to call it.

I can make up some new designs, you see, and give different answers perhaps than yours. Why don't you just sample from the big scheme?

MR. MURPHY: The thought that went into it was it was a way of covering the sample space with a relatively few number of runs.

DR. SAUNDERS: So you get sufficient information at less cost? Is that what you're telling me?

MR. MURPHY: We thought it was a technique that would give us the kind of information we needed to identify this general type range in a manner that would maximize our use of resources.

MR. ERNST: We're actually doing it both ways. We pick the issues we think are the dominant risk contributors, in which case, the others should be very small contributors.

We're not that interested in the exact number of the range because we're just advertising it as a reasonable

range. So if it's a little bit off, the check is to see whether we're way off.

If we're way off, then we have to go back and rethink the whole process again.

DR. SAUNDERS: All right, I understand what you're doing. Thank's.

MR. MURPHY: Could I have the next one, Mal?

(Slide.)

I think I've said this all already. Let's go one more and see if you have questions.

(Slide.)

My summary is a little out of place because it assumes you've already heard the front end analysis part of what I'm about to proceed to, but I'll give you the summary anyhow.

We're doing the rebaselining and the actual frequencies using compressed PRA methods, which we believe are highly cost-effective. Really an intriguing tool that has worked out very well.

We have the evaluation of the accident methodology again with the best tools that we have available to us at the time. That's basically the source term code package.

We're doing the offsite consequence analysis using an improved code max, which I'm going to talk about

later.

And I've described the uncertainty analysis to you, which we feel will give us reasonable engineering perspectives on the possible range of outcomes with the purpose of using it to focus attention on the important engineering and phenomenological assumptions, on which we have a lack of knowledge.

And as should be completely obvious from our last 20 minutes or so of discussion, our uncertainty estimates are not rigorous.

Now I'll go back to the beginning of my talk.

(Slide.)

Again, many of the comments here were answered by questions that Mal answered as he was giving his general overview. In the interest of saving time, I will try to gloss over areas that we covered before.

We are doing the risk profiles and perspectives on the six reference plants in terms of the risk reduction potentials that are being looked at.

On these technical items Dr. Catton asked about earlier, the fixes were identified in 0900 earlier. Several years ago, the vented photo containment. Add-on decay heat removal. Add-on containment heat removal features.

We also asked the analyst to look for plant specific modifications that would be suggested by the

accident sequence rebaselining itself, and looking at the analysis of the plant as it exists today, is there anything that strikes the analyst as sensible to do to try to reduce risks?

I may be biased but I think the risk analyst, as he has done his analysis, has a fairly good idea of what's driving the risk to his plant and what type of modifications might be cost-beneficial.

This is more than just getting importance measures, say, coming out of fault tree evaluation, or whatever. Rather, he understands the system pretty well from having done it, so he has an intuitive feel for where he can gain advantages.

So he looks at it both ways in terms of what's driving the risk and he gets that out of a standard importance measure type of thing.

He also knows the plant pretty well after he's been done with it. He's been living with this thing for a few months. He may well be able to suggest a way of reducing risk that would be cost-beneficial. I mean, a specific example rather than review cut set X. He has a design modification in mind that can accomplish the reduction of those cut sets. But perhaps cut sets in several sequences at the same time, just because of his integrated knowledge of the plant.

We've asked him to look for that and use their engineering judgment in that type of process.

DR. KERR: It seems to me at this point it would be more likely to work if the people who were doing the analysis also were good plant designers.

It is rare to find that combination in the same person.

MR. MURPHY: I agree.

DR. KERR: Do you have people who are that kind of people?

MR. MURPHY: We have some pretty good people but I don't think too many of them have design experience, in the sense you're talking about. Most of them in this part of the analysis tend to be PRA types.

DR. KERR: That's sort of what I thought.

MR. MURPHY: There are a couple of exceptions to that but, in the main, that's the case.

Again, we talked about the display of the important uncertainties and the insights in the use of risk important information.

(Slide.)

This I think everybody knows. I put it up and described the sequence in which I was going to talk about things, instead of talking about them in almost exactly the opposite sequence. We've talked about the two in the

middle. I'm now back on the first one and I'll come back to the last one.

MR. ERNST: That sort of comports with how we've been doing this project anyway.

(Slide.)

MR. MURPHY: In terms of the activities that we have in process, I think we covered everything on this. Let's proceed with the next one.

(Slide.)

Getting to teh approach we're using for the first four of the rebaselining activities -- Surry, Peach Bottom, Sequoia and Grand Gulf. And I will address later the process that we use in these other plants.

We start off in the preparation phase where we essentially learned everything we could about the plants, based on records, where the information was already on hand for the analyst. If there was a previous PRA on hand, they reviewed the PRA. If the accident sequence evaluation program had done generic modeling of the plant systems to understand what was done there. To study the FSAR and whatever other information was on file with the NRC.

Having a knowledge of the plant from the documents, the team then went to the plant for roughly a week for a process, as I say, to obtain as built drawings to assume the as built condition of the plant, comporting with

their understanding of the plant. To get the procedures, the current procedures, and then to progress from those on to interviews with the operators to understand how the operator implemented the procedures.

At this point, they went back home and did more or less standard event tree and full tree modeling. I'll talk a little bit more about the scopes of those in the next slide, I think.

From there, we went to base case quantification and then, through uncertainty and sensitivity analyses.

DR. KERR: Why is Zion not on the list?

MR. MURPHY: Zion we're handling somewhat differently. Zion, the analysis, and the same thing is true of Lasalle. In Zion, the analysis is going to be based mainly on an updating of the Zion PRA rather than starting again drawing trees in this manner.

Basically, what they're going to do, the basic document they'll be working on is on the front end, is the Sandia review of the Zion PRA. And then update that information and go on forward with that.

Similarly, on Lasalle, Lasalle will be based on the models that exist already in the Omni program, which are to a significant greater level of depth in some areas than what we're doing in this rebaselining activity.

We will use those where they exist and we will

use the simplified method where they don't exist.

They are a somewhat different process. At the end of it, before they document it, they go back to the plant and go over the conclusions with them to make sure that the plant performs in the way they have assessed, and revise the document.

We have detailed technical QA at each phase. To give you an idea of the scope of the QA, we have a QA team at Sandia that I believe is four people long and they're spending 50 percent of their time QA-ing these four analyses. So the people doing the analysis have an expert looking at what they're doing, not only from the standpoint of finding errors, but making sure that they're using the system assumptions and boundary conditions plant to plant so we come up with something and we're not comparing apples and oranges.

MR. DAVIS: Joe, all four of these plants have previous PRA's?

MR. MURPHY: Yes.

MR. DAVIS: You're using that as the baseline to start into this?

MR. MURPHY: That's one of the places we're starting, but you get something like Surry and WASH-1400 that was published 10 years ago -- actually, the design was frozen about 12 years ago -- that plant doesn't look really

too much like it did back in the days of WASH-1400 in some key features.

MR. DAVIS: And it may not 10 years from now.

MR. MURPHY: Well, we're going to be closer timewise, at least. You know, I'll agree, it's a moving target.

(Slide.)

To give you an idea without going through all the gory details of what we're doing in each of these methods, let me just show what we're doing to give you a comparison of what we're doing versus WASH-1400. The earlier RSSMEP stuff, the IREP stuff we did a couple of years ago. And now our RMIEP program, which is now in progress.

This gives you a good idea of what the scope of te analysis is.

MR. DAVIS: I'm surprised your human factors detail is not any better than WASH-1400.

MR. MURPHY: There should be an asterisk on that one, Pete. The basic human error probabilities associated with leaving components in the wrong position are basically based on the same type of analysis done in WASH-1400, although the procedures for doing it are significantly better now. They're documented and they've been tried; where, in WASH-1400, they were not documented in areas where human factors appeared to be very important.

The main one is TC on Peach Bottom, the Mach I. There we're doing a detailed state of the art human factors analysis on that, as good as anything I'm aware of having been done in any PRA.

MR. DAVIS: I got that impression. That's why I was surprised to see that.

MR. MURPHY: On that one case where it appears to be really the dominant thing in the accident sequence, we're spending a lot of money and we're getting what appears to be a very good analysis of the impact.

We have a limited search of the accuracy available to us, the data. We're doing limited data analysis on key components the analysts believe are important, plant-specific data. In some cases, there's a lot. In some cases, there's not too much. It varies plant to plant.

I guess, rather than go through each one of these in detail, the next viewgraphs, I believe, highlight the ones where what we're doing is somewhat less than IREP. I can address those directly. If you want to discuss any of the other ones, we'll see what we can do.

MR. WARD: What plants are being done under the RMIEP program?

MR. MURPHY: RMIEP is one plant, Lasalle.

(Slide.)

Let me just talk about actuation and control. In terms of the pressurized water reactors, the actuation and control circuitry is treated as a black box. What I mean by that is they are taking the actuation circuitry for a given system and they're developing a fault tree model for the actuation part of this once. And then they're assuming they can treat that as a black box with that reliability value forever, for every other system of the plant.

On the boilers, where the nature of the control system is such that many relays and coils feed different systems, it wasn't possible to do that, so there is detailed modeling of the actuation circuitry on the BWR's but not on the PWR's.

In general, we're modeling the TC power interface directly, explicitly. For AC mode of control power, we generally that's transformed from the motive power source so we don't model the control power except we assume that it's taken off the motive power.

The local control circuits are generally considered to be part of the component. I'm talking about the local control circuit at the component itself. The lead switch on a Limitorque valve is considered part of the valve and included in the valve failure rate rather than modeled explicitly.

Of course, in developing the valve failure rate

model, you have to have that in mind, too. Apples and oranges don't mix.

(Slide.)

In terms of the electrical systems, we generally modeled them at teh 4160 level provided that there's firm indication that we have independence at the lower buses.

Again, in general, they're looking at bus faults, transformer faults as the main source of faults.

We haven't done...if something is sitting on a 480 bus, we haven't done the detailed fault tree modeling that takes you from the 4160 down to the 480 for every circuit breaker that exists on that train.

Experience has shown that the kind of things that get you in trouble in a PRA tend to be faults at the higher level, in the 4160 range, or the 125 volt DC range. Those are the ones that are explicitly modeled.

In terms of subtle interactions, the kinds of things you find from a very detailed investigation of control circuitry or the very fine details of the plant.

DR. SAUNDERS: I don't know what an interaction is.

MR. MURPHY: A dependent failure. I can give you an example of the kind of things we found in the past. But what we had the team do as their first step is, basically, the QA team, as a matter of fact, was to go through all the

PRA's they were familiar with and we have a team that essentially encompasses almost the entire PRA community, who have done these PRA's. And to find these subtle interactions that were very easy to miss.

DR. SAUNDERS: What's a subtle dependent failure?

MR. MURPHY: Let me give you an example.

DR. SAUNDERS: All right.

MR. MURPHY: When we did our PRA on Millstone, we found it was possible to get into a situation looking at load shedding relays, that there was redundancy to assure that the load was shed so that the relays would open -- two relays would open, either one of which would shed the load from all diesels.

If any one of these things stuck open, you were getting a signal that said when you got your diesels back on, you started the diesels up, there would be a signal to all the engineering safety features, both buses from each relay that said don't load.

So a single failure of a relay contact would disable electric power to all trains of safety equipment.

This was found -- it was found actually by fault tree analysis that was being done by Northeast Utility personnel at the time. It was the first LER that I ever saw that started off at 4:00 p.m. while doing fault tree analysis that we found.

DR. SAUNDERS: Why is that subtle, though?

MR. MURPHY: Because it requires you to go in your modeling all the way down to the load shedding relays, which takes you through train after train of relays and coils as you go down through.

DR. KERR: "Subtle" means nobody has seen it before?

MR. MURPHY: Yes, that is what it means.

DR. SAUNDERS: That is fine.

MR. MURPHY: It is that you have to get to such a great level of detail to get it that it goes into your modeling activity. Since we knew we weren't going into that level of activity, we asked people what they had observed in the past, and we had a 20-or-30-page list of essentially all the strange things that PRAs have found -- I won't say all, but most of the strange things that PRAs have found.

We have taken those and then given explicit instructions to the teams to go deeper into this area and look for these type of interactions. But it is more an inductive process than a deductive process.

Ordinarily, you go through a fault tree to do it, and there is a deductive logic that allows you to find it. This time it is an inductive logic based on having seen it in the past.

In general, we modeled instrument air as single failure rather than going into the details of the fault trees and the air compressors and all the various ways you can lose air. Taking generic data where possible, we have looked at the history of instrument air losses and essentially treated the whole system as a black box.

MR. BENDER: Joe, there is a big environmental qualification program. What influence does it have on the

PRA program?

MR. MURPHY: At the present time I think the right answer is not enough.

MR. BENDER: In what way?

MR. MURPHY: If I was talking in the Army program rather than what we are doing now, I could talk in much more detail about what we had last week.

But in fact we are not looking at what we call external events in this program. We are not looking in this rebaselining effort at things like fires and floods.

Flooding, in particular, is an interesting one because you may be spraying hot water rather than just having the water come out. In that kind of information, information coming out of the EQ program, we give you some idea of how the failure rate varied as you increased temperatures and pressures. That would be very helpful.

Unfortunately, most of the equipment qualification programs are not designed with PRA in mind. They are designed with other objectives.

The fact that I have taken a pump and put it in an autoclave and run it for 30 days under a given set of conditions and I tested it when it came out and it worked is enough to meet the equipment qualification requirements that are under the regulations, but it doesn't tell me anything on how the failure rate varies now that I have exposed it

to this harsh environment.

So the ability to take that information and feed it back in the PRA requires that I have a lot of data. Right now I don't think I have enough to do it. It is something we are looking into very seriously in some of our research programs, but it is very difficult because of that problem.

MR. BENDER: It is a two-way street. If you had the information, then, it might change the failure rate that you are using as data for the analysis. If you are not taking any credit for it, though, in one way or the other it is hard to understand why there is any significance attached to doing it.

MR. MURPHY: What we are doing in our later PRAs is to try to get a handle on the potential importance, to identify where environmental stresses can affect the component.

This is done by putting, say, location identifiers on the component as we are in the trees and then identifying what locations will be affected by a given stress, be it high pressure, temperature, high humidity, or whatever.

Then we can see if there is one area where we have a number of components all subjected to a high temperature/pressure environment, say, and to assess whether

these are of critical importance.

If they appear in the dominant cut sets that require accident sequences, it suggests that this is an area that needs further work. If it turns out upping the failure rate by a factor of 10 doesn't make any difference, then maybe it is not.

MR. BENDER: I think that is a good way of doing it, but you didn't say you were doing that. You were just saying you could do it.

MR. MURPHY: We are at the process now on our La Salle PRA where we are putting these identifiers on -- the location identifiers on the trees. We are putting the environmental hazards on the trees, but that is something coming out of that we can look at. I can't look at it until it is done.

(Slide.)

Now, we get to the consequence model, as I shift around up here.

As part of the MELCOR Code development system, we have developed a new consequence model called MACCS. It has essentially seven new features that are different from CRAC 2, which is the tool we have been using in the past.

We have the ability now to treat time dependent releases. CRAC is pretty close to the back, for those of you who are looking for the viewgraph. CRAC was a puff

release model. This is a multiple puff release. So you can model the time dependent failure of the release rather than assuming it all goes in one puff.

This has some substantial influences. For example, if you are looking at dose to people who are evacuating and you spread the time demand of the release over two or three hours, it may be instead of getting the whole thing, which they do if you had expressed it at a puff at time zero, since they now assume they are going to be some distance before the road before the last piece comes out, there is a reduction in the influence of it.

There is improved atmospheric dispersion, including crosswind distribution, better particle size distribution within the plume, and up-to-date plume rise models.

They are adding additional exposure pathways -- the food pathways, deposition on the skin, improved dosimetry. This is basically work coming out of Harvard.

In terms of emergency response, there is ability to consider graded response. There is ability to consider the road network evacuation, where in the past it was a straight line evacuation, at least in the CRAC 2 modeling, delocation of sheltered individuals and relocation outside the EPZ.

In CRAC 2, once you were sheltered on site, you

never left.

In terms of the long-term effects, it now includes long-term ground shine and direct deposition on crops. I don't know what the OR there means.

And we have the improved health effects models with hazard functions used for early effect, additional injuries and latent effects.

DR. KERR: What does improved dosimetry mean?

MR. MURPHY: I don't have the guy that works on that with me now, and I can't answer you, Bill.

DR. MARK: Does it include the option of sheltering?

MR. MURPHY: Yes. It allows you to handle sheltering much better than in the past and allows you -- if somebody is sheltered and it is a prudent thing not to be sheltered, it allows them to move later.

The decision in the earlier CRAC 2 model was once you were sheltered, you were sheltered forever.

MR. DAVIS: Joe, what do you anticipate the composite effect of all these changes will be versus the CRAC 2 results? Will the risks go down or up given the same source term?

MR. MURPHY: In what I have seen to date, it varies a little, but it looks like it is going up.

MR. DAVIS: That surprises me because one of the

problems I had with CRAC is that unless you tell it otherwise it assumes people are relocated and their lifetime commitment would be 25 rem. About 90 percent of the latent cancer fatalities were based on that avoidable dose.

Now, I would have assumed something different would be done in a revised code like that.

MR. MURPHY: I believe there is something different. If I understand the improved point model, it really has the effect of increasing things.

Al and Mark may know more about this than I do.

MR. ERNST: There is a fair amount of increase in the health effects model, but we have got the wrong guy here to answer the specifics.

MR. BENDER: Is there a report which tells in more technical detail what has really been done?

DR. KERR: On MACCS you mean or what?

MR. BENDER: On MACCS.

MR. ERNST: I am not sure there is documentation yet.

MR. MURPHY: There is some form of documentation. I don't think the final documentation is out yet, but we are at the point where we are putting it out for trial use for the national labs. So there has to be some sort of definition as to what it is.

MR. BENDER: I just wondered if there was some

discussion as to what was being improved and why.

MR. MURPHY: I am sure we could give you a relatively short description of what that is. We are talking three, four or five pages. I feel reasonably sure that exists somewhere already.

If you are talking about a couple of hundred --

DR. KERR: Why don't you send a copy to Dean?

MR. BENDER: I am not looking for a couple of hundred pages report; I am looking for a few page report that has some substance to it.

MR. MURPHY: The model has been presented to the CS&I Committee and to assorted technical meetings. I am sure there are papers that describe it in detail that I am not familiar with.

MR. BENDER: If I could just know what is being done about improved atmospheric dispersion, which I think is the most important technical matter to be dealt with. That by itself would be very useful.

DR. CATTON: For example, the crosswinds distribution -- how do you use it? What do you gain?

DR. KERR: We are going to get all that later.

MR. MURPHY: An obvious problem with a lot of these things, you could do a lot better modeling if you had better data.

DR. CATTON: It should be a balanced approach, I

would think.

MR. MURPHY: I hope we have achieved that.

(Slide.)

Now, I am back to where I started, the sensitivity analysis. So I think this means that we have covered all my viewgraphs in a rather disjointed fashion.

I do want to emphasize -- oh, I am sorry. As we are going through the front end, we are doing something slightly different, and I hope that is where we are now.

(Slide.)

In terms of the accident frequency rebaselining for the base case, we are doing essentially what is traditional in PRA. Data is being treated as having a lognormal distribution and that uncertainty associated with that distribution is being propagated through the event trees and the fault trees the same way it was on the IREP.

Then we are doing sensitivity analysis on the key assumptions that entered into that calculation. We are asking the analyst where his assumptions on success criteria, in some cases the effect of using generic common cause factors as we do the analyses, various assumptions they may make as to whether component cooling is needed for a given component, whatever. It varies from plant to plant.

But typically, they have identified 14 or 15

issues that the analysts feel are important enough to merit sensitivity studies, and most of these have to do with either modeling or boundary condition assumptions.

They are going back and doing the sensitivity study on that, repropagating the data through the tree for each of these issues, and then effectively associating a weight or a degree of belief and which way they believe the sensitivity should go.

Again, it will be combined in one of two ways. I have one way illustrated on the next viewgraph. There is another way we are still considering using.

(Slide.)

What I have here is the way we had initially planned to do it and still do, for that matter. This shows the results of the base case. This is the standard upper and lower bound with a mean associated with our estimate of our base case.

Then we run various sensitivity issues, each one of which now has a new mean and a new upper and lower bound, and we plan to present the results in a format like this which says that for these sensitivity cases this is the range of the means.

These are the extremes that we have observed, and this is the 5 and 95 percent bounds that we originally associated with our base case. So we have this box and

whisker type presentation, the range of the means associated with this and the upper and lower case.

We have a code that we are considering using, called TMAC, which is essentially the limited Latin hypercube. That would allow us to take each of these and put a distribution on them, put a weighting function or a weighting factor on to what extent the analyst thought there was a chance that the sensitivity analysis, the sensitivity issue could be the way it was represented -- in other words, two pumps needed instead of one -- and then it would be possible to fold that through and generate something similar in concept in terms of what came out at the end.

DR. SAUNDERS: Box A is the result of your limited sampling scheme from each of the cases or from the base case?

MR. MURPHY: I have done the base case, and I have done all my other sensitivity issues. I have propagated through using sets in rather a thorough way.

DR. SAUNDERS: I understand that, but what data gives you the box?

MR. MURPHY: The box comes from the range of the means of these sensitivities. It indicates a spread of the means of the various sensitivity studies.

DR. SAUNDERS: \ll right. So you have how many of these means all together that go into calculating the

box?

MR. MURPHY: Typically, on the order of 10 to 15.

DR. SAUNDERS: All right. Then you sample from these at different levels, is that right? How is it you have done it?

MR. MURPHY: We have done each one of these calculations separately. All we have done is to say that the range of the means here in the three cases I have got on here -- the highest one is here, which defines this one; the lowest is here, which defines the lower part of the box.

DR. SAUNDERS: I see. No calculation was made?

MR. MURPHY: No, it just describes the bounds on each of these sensitivity studies. What we are considering but haven't gotten to yet is going one step further and attaching weights or degrees of belief associated with each one of these and folding that in. In that case it would be weighted, and you wouldn't get the direct correlation that shows in this picture.

DR. SAUNDERS: Just one other thing, one other question: you say it is standard to assume that everything has a lognormal distribution, and therefore if these things are affected as product, as most of them are, then you can use the convolution and get the direct distribution of consequence of several variables, right?

Suppose -- of course, when you do that, you have the folding together and cancellation of errors that always results -- suppose you take a fat-tailed distribution and

did the same analysis.

Do you have any idea what it would look like?

MR. MURPHY: One of the sensitivities we are exploring right now is to change the distribution, change from a lognormal assumption to something else. A beta or gamma essentially would do the same thing.

DR. SAUNDERS: That is my point. A fat-tailed distribution is going to be such that the sample mean has the same dispersion as each individual mean. You don't get the concentration that you get with the normal. That is the worst case probabilistically.

I wondered if anybody -- if you had ever thought about doing that or tried that.

MR. MURPHY: I haven't thought of it. I can't say nobody on our staff has.

DR. SAUNDERS: Well, you know what the Koshi distribution is?

MR. MURPHY: Yes.

DR. SAUNDERS: A sample mean has the same distribution as each observation, the same scatter that will prevent this cancellation through this, and of course it will be not a more difficult analysis but it might scare me if we did it.

But you ought to think about that.

MR. MURPHY: We will see if we can input

something like that into our code. To the extent we can do it, I think it is something we definitely need to consider.

MR. BENDER: ust the fact that you are doing a sensitivity analysis has value. Any way you do it will be useful.

But the problem really is to decide what part of the analysis needs to be considered. In some cases it won't make a damn bit of difference whether you use the lognormal. In other cases it may have great significance.

But a selection of things to do in order to do the sensitivity analysis is kind of critical. What data should be examined? You can't examine them all because there is too big a chunk of information.

Have you given thought to what data is going to be considered?

MR. MURPHY: As I said, most of what the sensitivity analysis is based on now is based primarily on the modeling assumptions, critical modeling assumptions, and they come out from the analyst's knowledge that he got to System X -- I will give you an example that I had happen to me at least three separate times.

You take an assumption and you look at the numbers that are in the FSAR, and they say there's 4000 gpm should be flowing to the heat exchanger. You assume some fault that is causing a diversion of flow out of the system,

and you do your calculation. You find out there is 3500 gpm going through that heat exchanger, and the fault tree being a dichotomous technique, you have got to say that is either success or failure.

Your first reaction as an analyst is it doesn't meet the success criteria that is defined in the FSAR, but I know if it isn't enough flow it is awful bloody close to it. Therefore, temperatures will increase very, very slowly.

We still have to finally answer the question: do I model it as one pump or two pumps?

So you make an assumption, and if you are a very conservative guy you say two pumps and if you are a very realistic guy you probably say one pump makes sense because there is got to be conservatism in the FSAR assumption.

Whichever way you go, you are not sure, and that is the kind of thing we are treating in the uncertainty.

I have seen them go both ways.

MR. BENDER: That is a very good example. If you did enough of those kinds, I think you could improve the knowledge of how much you can do. That is to some degree an issue.

MR. MURPHY: You run into a lot of these where you just don't have the time and resources to go back and do the whole thermal hydraulic analysis of the plant to find

out what a true success criterion should be for the situation.

MR. BENDER: If I could use an example that is closer to my heart, the probability of a power failure, which maybe 10 to the minus 2 per year or 10 to the minus 3, I think examining the sensitivity of that kind of thing with a probability distribution might tell you a lot, and it is those things which have a fairly high probability of happening that we need the most depth down in the low probability range.

It probably doesn't make much difference what the distribution is, but if it is a high probability range it does.

MR. MURPHY: I tend to agree with you.

DR. KERR: Further questions?

(No response.)

MR. MURPHY: That completes it, I guess.

Mark will go on.

DR. KERR: Before Mark gets started, I am going to suggest a 10-minute break.

(Recess.)

DR. KERR: Before Mr. Cunningham gets started, have you come to some sort of agreement on the appropriate frequency for the accident initiators?

It seems to me I remember earlier there was some

uncertainty about the initiator frequency you were going to use.

MR. MURPHY: What we are doing for the initiator frequency, we had a study done by Idaho about two years ago, I think. They published a report on the frequency of accident initiators by plant on an average. That is the basic resource.

Then the team also looks at the plant history, where there is a lot of history. On something like La Salle, there is effectively no operating history to judge on the generic values you used. On something like Surry --

DR. KERR: Have you arrived at what you consider to be a satisfactory value now for those initiators?

MR. MURPHY: The answer there is "yes."

DR. KERR: Thank you.

(Slide.)

MR. CUNNINGHAM: I am here to talk this afternoon a little bit about the work that is going on within the staff to develop staff positions on the risk importance of particular severe accident issues.

DR. KERR: Does risk importance mean risk now and not core melt?

(Slide.)

MR. CUNNINGHAM: In general, most of the issues we are talking about are phenomenological issues. So we are

talking risk. In some cases we will also discuss their impact on core melt frequency. Generally, it is risk.

We are doing this for two reasons.

The first one is that for NUREG-1150 that we discussed here and for the IDCOR process that the staff is going through to resolve and close out the IDCOR interchange, we need to develop staff positions on specific issues that could be important for the risk of a plant.

The issues we are dealing with are related to issues that have come out of the technical interchange meetings that the staff has had with IDCOR. They have come out of the specific instances perhaps of work that has resulted in the ASEP and SARRP analyses to date, and they are issues that have been identified in BMI-2104 or the draft of NUREG-0956.

A second reason for doing this is that, as Joe described before, SARRP is performing an uncertainty analysis for itself and then eventually for NUREG-1150, and we wanted to have staff interaction on the ranges that were being used by SARRP and in a sense concurrence on them. If we didn't have concurrence on them, then we would develop our own ranges and have the SARRP people do the calculation again with what we accept as the position on these issues.

(Slide.)

There are a series of steps that are probably

pretty straightforward. The important part is that the work that is being done is being done jointly with the NRR staff and the Research staff and our contractors.

For each issue that has been identified, there is a staff person in Research and a staff person at NRR respon~ible for seeing this process through.

So again the process itself is pretty straightforward. We have identified a set of issues and identified people who will be involved from the staff to develop these position papers.

In addition, once these teams get going, they will identify their own supporting contractors, et cetera.

The first step they are going through is a compilation and interpretation of the existing data. This gets into something Joe was talking about before, in addition to data sources from RES and NRR considering the IDCOR reports, Special Technical Report 85.2, I guess it is, which is IDCOR's analysis of the NRC technical issues and other sorts of data as well.

Given that, there is an interpretation phase where we try to make comparisons between the data that is available here and then use that to develop the types of ranges and subjective weights that Joe described a little while ago in his uncertainty analysis.

(Slide.)

In support of the development of these ranges of weights, we have time to permit the identification and performance of some supplemental calculations. The amount of work that has to be done will vary a good bit from issue to issue. In some cases there will be essentially no new work. In others we are almost starting from scratch.

This will all be folded together, the short term work and the existing data, to develop the position paper. This whole process then is going to be overseen by senior staff and then the management will focus on that.

(Slide.)

In general, we have tried to define the issues in terms of our major headings or intermediate products in the SARRP analysis; that is, in terms of accident sequence frequencies, source terms, or the various parts of the containment, either loads, structural response, or the failure mode probabilities, so that you don't end up in a situation where your issue result, for example, is a variation in the mass of thorium expected to exit the vessel.

That, in itself, is useful information, but doesn't translate into something -- in itself, it doesn't provide you much risk information. Only when that is translated into a containment load source term or something like that can you really bring it into the risk picture.

DR. KERR: Do you have a good definition of what you mean by containment failure?

MR. CUNNINGHAM: Jim Costello just left, unfortunately. He is the one who is dealing with this.

DR. KERR: Does Jim Costello have a good definition of what is meant by containment failure?

MR. CUNNINGHAM: He has a good definition of it. I can't tell you what it is.

DR. KERR: As long as somebody has one.

DR. MARK: And everybody uses it.

DR. KERR: Eventually, other people ought to know about it, but for now --

(Laughter.)

(Slide.)

MR. CUNNINGHAM: I just want to go through the issues that the staff is considering, and in places where they correspond to IDCOR issues I have identified the corresponding IDCOR issue.

An issue that we brought up a little while ago is the issue of core concrete interactions. In this issue we consider both the thermal hydraulics and the CORCON aspect of it, if you will, and the VANESA aspect, the radionuclide releases. We have amalgamated that into one issue.

Direct heating is one. We have included in here something else that Ivan brought up, which is the potential

for reactor coolant system failure, which would preclude the direct heat expulsion that would result from direct heating. That is tied up in this issue.

In-vessel conditions and dispersal X vessel, hydrogen, both the generation aspect and the burning aspect, the chemical form of iodine within the reactor coolant system and containment.

MR. BENDER: Excuse me. Before you take that off, are you going to try to develop a probabilistic discussion of those issues, or is this merely a technical discussion?

MR. CUNNINGHAM: Is it technical or is it probabilistic? Is that it?

MR. BENDER: I am asking whether it is one or the other. Maybe I should have asked is it going to be both? Up to now it has never been probabilistic.

MR. CUNNINGHAM: The intent is to try to make it probabilistic. There is an engineering aspect of it, if you will, but by the weights that Joe talked about and the degrees of belief, the intent is to try to make it probabilistic as well.

MR. BENDER: The way I heard it discussed before was divide the statistics up in accord with the distribution of the number of technical experts that had opinions.

Is that the way you are going to do it?

MR. CUNNINGHAM: It is the same process, yes. It is a judgmental, probabilistic analysis.

MR. BENDER: It is not all that satisfying, I might say.

DR. CATTON: That is an excellent example, the example I chose for a reason, because it is not probabilistic. It is deterministic, and somebody has developed a model that hasn't included all the things that ought to be in it. You can't treat it probabilistically. It is just not right. You should fix it.

Maybe that is a foolish belief.

DR. KERR: Mr. Meyer has already told you that just because some expert disagrees with the NRC they are not necessarily going to change their mind.

DR. CATTON: What about more than one?

(Laughter.)

DR. KERR: We have only identified one so far.

MR. MURPHY: If I can interject, it is not quite as bad as you are making it sound.

What happens basically is -- let's say we have a model of known efficiencies and our base case is based on that model. People will try to make bounding calculations, hand calculations, based on their understanding of the deficiency and whatever else that has been done. In large part what has been done that they are relying on are the

questions to get some idea of how large the release could be in this phase if they had misrepresented, if the model is inaccurate.

And if essentially the team of experts agrees that this deficiency is in the code and that this bounding analysis is a reasonable analysis, then I would expect a high weight on the bounding analysis and a low weight on the code.

DR. CATTON: Somehow, that doesn't sound very satisfying. Why don't you fix the code?

MR. MURPHY: Presumably, if we come to the conclusion that there's major deficiencies in the code, the code's going to get fixed. But, in the interim, we're going to handle it that way.

The code fix may take another two years.

DR. KERR: Let the record stipulate that Mr. Catton is unhappy with the VANESA code.

DR. CATTON: I just chose VANESA as an example. I wouldn't want Dave Powers mad at me. I could pick others.

MR. CUNNINGHAM: As Joe was saying, the process that was gone through a couple of weeks ago in Albuquerque, the discretized set of parameter values that were chosen, for example, for something like VANESA, one of four typically was the VANESA calculation from the source term code package calculation, or whatever; and there were others based on quest analyses and things like this.

I don't recall exactly how they weighted them. But, in many cases, I think they weighted other calculations almost equal in value in their opinions to a VANESA calculation, given that we recognized the uncertainties in VANESA.

DR. KERR: I think what Mr. Catton is saying, I'm

reluctant to speak for him myself, he's so articulate, but the fact that you weight against something which is utterly wrong along with other things that are right doesn't strike him as being a very logical approach.

Perhaps you disagree with him that VANESA is utterly wrong. I'm not trying to take sides on this. I don't know enough to do so. But I think I get his point, and I think you do, too.

MR. CUNNINGHAM: Clearly, if something we believe is utterly wrong --

MR. BENDER: Whether it's right or wrong or not will be important only if there is some difference in the way in which the NRC handles its regulatory requirements.

I guess I would have to use that as the test: What regulatory actions am I likely to consider concerning this technical issue and how might they be influenced by a change in the judgment?

That may not be totally probabilistic, but probabilities will enter into it.

MR. ERNST: That's why I think it's very important to have this so-called range of expected means, or whatever you want to call the range. Then, if you understand what drives that range, depending on the regulatory application you choose to make -- you may decide to be at the high end, or you may decide it doesn't make a

lot of difference where you are -- but you can make some judgments of where you want to be for a regulatory problem.

MR. BENDER: I like your approach, Mal, but I'm inclined to say that the technical discussion will have to include something that says here are the regulatory decisions that will be made in connection with this matter.

Without that, the technical discussions aren't much help.

MR. ERNST: I think you're getting into Chapter 4, that says: Given all this information, how might it apply in certain kinds of regulatory situations?

MR. BENDER: There's a tendency, when you put the discussion in Chapter 3 and the application in Chapter 4, to fail to correlate.

DR. KERR: Back to the process. I hate to say something that's so obvious, but I don't need to very much. It certainly must have occurred to you as you've done this that you can influence the outcome of this process very much by the experts you choose and by the instructions you give them.

That is a very important part of the total process. I certainly can't tell you how to do it. But it will have a strong influence on what you finally did.

MR. ERNST: That's exactly right.

MR. CUNNINGHAM: Going on with the other issues

that we're considering in the staff...

(Slide.)

...considering the issue of reactor coolant system natural circulation. This is one case where the issue, in a sense, one of the statements I made earlier that it's a direct input in a sense to the SARRP analysis. The natural circulation issue is in fact an input to three other issues -- the direct heating issue, that part of it which deals with the potential for failure of the reactor coolant system; this issue, the revaporization of radionuclides; and hydrogen generation.

Going on, we have again revaporization of radionuclides in the reactor coolant system before the vessel breach and after.

DR. CATTON: Mark, under the first one, do you include the possibility of steam generator tube rupture, and everything else? When you say "localized", you mean anywhere in the RCS?

MR. CUNNINGHAM: Yes. Again, the containment response issue, which deals with, again, we have some sort of failure criteria which I've described as gross failure, leakage before that failure, requirements of pressure and temperature that cause that failure and the size of the resulting hole in the containment.

DR. CATTON: On this last one, do you include

location of the leak as well? It seems to me, if you're putting in an enhanced plume rise model here, you ought to be looking at elevation that the release takes place from; or else there's no sense because of the augmentation of atmospheric dispursion.

MR. CUNNINGHAM: Implicit in this is the penetration is low in the building or the spring line.

(Slide.)

Finishing out the issues are the performance of the secondary containment buildings and the PWR's, the influence of containment venting in boilers in terms of the human reliability aspect and the engineering feasibility, the likelihood of having an alpha failure mode steam explosion, and something that I'll call common cause failures, which is actually one front end or accident frequency-oriented issue here.

This is probably going to act as a surrogate for several types of common cause failures. For the moment, we haven't concluded exactly what would be in that issue.

DR. CATTON: Didn't the steam explosion review group resolve IDCOR No. 7?

MR. CUNNINGHAM: For all intents and purposes, the position paper we see here will reflect the surge work, unless something dramatic happens.

MR. ERNST: That is the intent. The position

paper on steam explosions, there is a position. It will be characterized for completeness in NUREG 1150.

The other issues all have some working to do.

MR. DAVIS: Mark, let me ask you about one I didn't see on the list. That's pump seal failures. Is that one you're going to consider?

MR. CUNNINGHAM: At the moment, I believe that's being considered as an issue, not as an issue in this sense, but gleaned from the sensitivity studies as part of the ASEP work.

MR. DAVIS: Let me ask you why. I'm worried about it. Two of the most recent PRA's -- Seabrook and Millstone III -- assume that you do get pump seal LOCA. But there's a lot of dispute over when it occurs and what the flow rate is.

Now, the NRC, in their A-45 decay heat removal program, assumes that it never occurs. The assumption is being made that it's going to be fixed. I'm curious as to how you're going to handle this problem if you have both ends of the spectrum being addressed in the NRC and the industry.

MR. CUNNINGHAM: Again, that specific issue is being addressed, as far as I know, in ASEP, so that the accident frequencies predicted for the various plants and the core melt frequency for the various plants will reflect

a sensitivity if you accept pump seals as always occurring or never occurring. You'll see them on the sensitivity. In a sense, if need be, that kind of work could be elevated to become an input into a position by staff on that issue.

MR. MURPHY: Pete, I don't have a direct answer for what I know you want. In the ASEP study, I believe they're making an assumption that if component cooling water to the seal is lost for X-minutes, you get seal failure of size Y. I can't tell you what the X and Y are.

MR. DAVIS: It looks like NRC is going to have two conflicting positions, perhaps, between this program and A-45 on whether it happens or not, and what perhaps the consequences would be.

The other one that I'm curious about is the V sequence; the IDCOR analysis for Zion indicates that it won't occur. Yet, in the Zion PRA done by PLG, it does occur and is a dominant risk contributors for internal events.

Now, you're going to have to resolve that somehow, I would guess. And I don't know which analysis is correct.

MR. MURPHY: That one will be based primarily on the Sandia review of the PLGd report, where I believe, and I'm going back a couple of years in memory now, I believe they found it was more probable than PLG did.

MR. DAVIS: But IDCOR says it won't happen.

MR. MURPHY: I understand that. It will be based on the best estimate.

MR. DAVIS: I don't think PLG did any stress analysis for that accident. They did the same thing that was done in WASH-1400. They assumed it would occur if the check valves failed. But IDCOR took it one step further, actually looked at the pressurization of the RHR system and determined that only the pump seal would fail and you'd get a totally different accident that's relatively beneign.

MR. MURPHY: What's happening here is part of these sensitivity issues they are considering event V, the event V consideration that I mentioned earlier is whether it's wet or dry. Not so much on the PWR's as on the boilers. There's a real question as to whether the stress on the pipe is such that it would cause a failure.

The question exists no the PWR's but on the boilers, the delta P at 1,000 psi less. But, on the boilers, it is definitely phased into it. I think it is on the PWR's but I'm not positive.

MR. CUNNINGHAM: This is a somewhat different set of issues than the SARRP program is analyzing. I think, for Surry and SARRP they are considering the issue of, one, is it in fact a large enough rupture of the system to lead to core melt? Two, is it above water or below water?

I think both issues may be handled as inputs to their uncertainty analysis.

MR. MURPHY: I think that's right.

(Slide.)

MR. CUNNINGHAM: Just to show these same issues again, these slides do a couple of things. One, it just simply indicates what organizations are responsible for developing these position papers. And it's just a mapping of issues to the six reference plants.

What it breaks down to is that there are a number of the issues that are specific to boilers. Others are specific to PWR's. There are some that go across the board.

DR. CATTON: What do those initials stand for -- DSRO?

MR. CUNNINGHAM: That's Dr. Spiess' division at NRR. The other ones are DAE, Division of Accident Evaluation in Research; DET, the Division of Engineering Technology in Research; ourselves, DRAO.

DR. CATTON: So this is all in NRR then?

MR. ERNST: Most of it is in Research.

MR. CUNNINGHAM: One division within NRR is responsible for their side of these positions. The separation on the other side goes among the three divisions.

(Slide.)

The product in the sense that we generate here is a series of position papers that will end up as an appendix to NUREG 1150. The format of the issue papers is very similar to what IDCOR has put together in technical report 85.2. It's pretty straightforward. Definition of the issue, relevant plants and sequences. What we think the range of the issue is. The importance of issues. Weights. Comparisons and conclusions on the risk importance of this issue. Long-term work that needs to be done.

Let me make it clear that going from step three to step four is not obvious. These sets of ranges for the individual issue papers have to be put together and integrated together to come up with the risk to the plant, and uncertainty of risk to the plant. And, in fact, the output of that risk is the measure of the importance of individual issues.

(Slide.)

Just to give you an idea of the schedule, for all intents and purposes, the positions papers are going to be developed and finalized over the next four months. This is the timeframe that 1150 will actually be written, so they're just in time to get into 1150.

And that's all I have to say.

DR. KERR: Are there questions?

MR. DAVIS: What's the RVW oversight?

1 DAV/bc

MR. CUNNINGHAM: I'm sorry. It should be review and oversight. Data compilation and interpretation. Position paper development.

MR. DAVIS: Thank you.

DR. KERR: Other questions or comments?

(No response.)

DR. KERR: Thank you very much, gentlemen.

I would like your written comments at some reasonable time sent to Mr. Houston. In the meantime, if you have any specific thoughts you want to make, it seems to me that the approach is a reasonable one. I look forward to hearing more about it. It's a tough problem. I would encourage that, insofar as you can, that you use not only Latin hypercube sampling but English to find out where the difficulties still lie.

(Laughter.)

DR. KERR: The meeting is adjourned.

(Whereupon, at 4:10 p.m., the meeting was adjourned.)

CERTIFICATE OF OFFICIAL REPORTER

This is to certify that the attached proceedings before the UNITED STATES NUCLEAR REGULATORY COMMISSION in the matter of:

NAME OF PROCEEDING: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS SUBCOMMITTEE ON CLASS 9 (SEVERE) ACCIDENTS

DOCKET NO.:

PLACE: WASHINGTON, D. C.

DATE: WEDNESDAY, JANUARY 29, 1986

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission.

(sigt) David L. Hoffman

(TYPED)
DAVID L. HOFFMAN

Official Reporter
ACE-FEDERAL REPORTERS, INC.
Reporter's Affiliation

NUREG-1150 STATUS

J. A. MURPHY

JANUARY 29, 1986

NUREG-1150

- o RISK PERSPECTIVES AND PROFILES ON SIX REFERENCE PLANTS:
  - — SURRY
  - — PEACH BOTTOM
  - — SEQUOYAH
  - — GRAND GULF
  - — ZION
  - — LASALLE

- o RISK REDUCTION POTENTIAL
  - — GENERIC NUREG-0900 FIXES (E.G., FILTERED VENTED CONTAINMENT, ADD-ON DECAY HEAT REMOVAL, ADDITIONAL CONTAINMENT HEAT REMOVAL, ETC.)
  - — PLANT-SPECIFIC MODIFICATIONS SUGGESTED BY ACCIDENT SEQUENCE REBASELINING AND CONTAINMENT TREE INSIGHTS

- o DISPLAY AND CONSIDERATION OF IMPORTANT UNCERTAINTIES

- o INSIGHTS ON USE OF RISK INFORMATION FOR PLANT-SPECIFIC AND GENERIC REGULATORY APPLICATIONS

RISK EQUATION

$$RISK_K = \sum_I \sum_J FREQ_I \cdot CRMP_{I,J} \cdot CONS_K(FP_{B_{IJ}})$$

SYMBOL	DEFINITION	SOURCE
$FREQ_I$	FREQUENCY OF CORE MELT ACCIDENT SEQUENCE I	ASEP, IN COORDINATION WITH SARRP
$CRMP_{I,J}$	PROBABILITY OF CONTAINMENT RELEASE MODE J, GIVEN ACCIDENT SEQUENCE I	SARRP CONTAINMENT EVENT ANALYSIS
$FP_{B_{IJ}}$	FISSION PRODUCT SOURCE TERM FOR BIN TO WHICH SEQUENCE I WITH RELEASE MODE J IS ASSIGNED	BCL STCP CALCULATIONS, BINNING MEETINGS, AND SARRP ANALYSES
$CONS_K(FP_{B_{IJ}})$	MAGNITUDE OF CONSEQUENCE K, GIVEN SOURCE TERM $FP_{B_{IJ}}$	MACCS AND CRAC2 CALCULATIONS

ACTIVITIES IN PROGRESS

- UPDATING AND REBASELINING SIX REFERENCE PLANT PRAS
- CONTINUING CONTAINMENT EVENT TREE ANALYSIS AND RISK REDUCTION ANALYSES FOR REFERENCE PLANTS
- SUBSTANTIALLY SUPPLEMENTING THE SEQUENCES INVESTIGATED IN BMI-2104 TO INCLUDE ADDITIONAL POTENTIALLY RISK-DOMINANT SEQUENCES
- PREPARING RISK REFERENCE DOCUMENT SUMMARIZING THE RISK ASSOCIATED WITH THE SIX REFERENCE PLANTS AND INCLUDING A CHARACTERIZATION OF THE ASSOCIATED UNCERTAINTIES

GENERAL APPROACH

(Surry, Peach Bottom, Sequoyah, Grand Gulf)

1. Preparation Phase
2. Plant Visit
3. Event and Fault Tree Modeling
4. Base Case Quantification
5. Uncertainty and Sensitivity Analyses
6. Confirmatory Plant Visit
7. Revision and Documentation

* Technical QC Review at Each Phase
* Senior Consultant Group Review of Approach and Results
* Continuous NRC-DRAO Interactions

NUREG-1150 LEVEL OF DETAIL COMPARED TO OTHER STUDIES

ISSUE	WASH-1400	RSSMAP	IREP	RMIEP
Accuracy of Info.	Same	More	Same	Same
Data	More	More	More	Less
Initiating Events	More	More	Same	Same
Human Factors	Same	More	Same	Less
Recovery	More	More	Same	Less
Actuation/ Control	Less	More	Less	Less
Electrical Systems	Same	More	Less	Less
Other Support Systems	More	More	Less	Less
Common Cause	More	More	More	Less

ACTUATION AND CONTROL

- O ACTUATION AND CONTROL CIRCUITRY TREATED AS A "BLACK BOX"
- O DC POWER INTERFACE IS EXPLICITLY MODELED
- O AC MOV CONTROL POWER GENERALLY ASSUMED TRANSFORMED FROM MOTIVE POWER SOURCE
- O LOCAL CONTROL CIRCUITS GENERALLY CONSIDERED PART OF COMPONENT

- 0 ELECTRICAL SYSTEMS
  - GENERALLY MODELED AT 4160V LEVEL PROVIDED INDEPENDENCE EXISTS AT LOWER VOLTAGES
- 0 SUBTLE INTERACTIONS
  - BASED ON SURVEY OF PLANT FOR INTERACTIONS OBSERVED IN PREVIOUS PRAS OR THE HISTORICAL RECORD
- 0 INSTRUMENT AIR GENERALLY MODELED AS SINGLE FAILURE

CONTAINMENT ANALYSIS

- DETAILED TREES FOR REFERENCE PLANTS BEING PREPARED UNDER SARRP

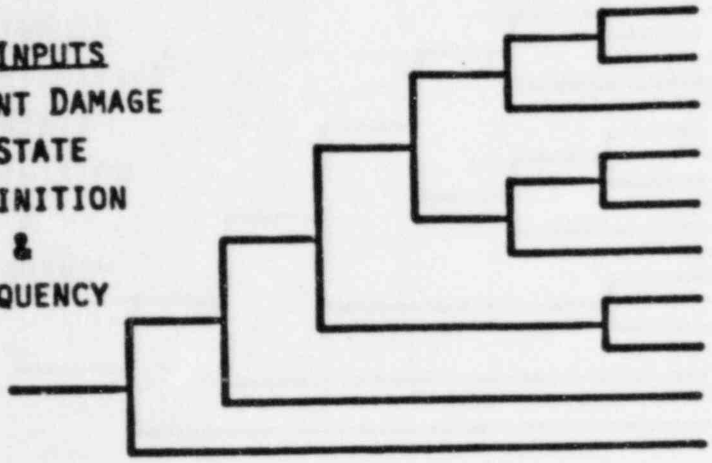
- PEER REVIEW OF PLANT-SPECIFIC TREES COMPLETED BY UNIVERSITY OF WISCONSIN

- PROGRAM CONSIDERS ACCIDENT PROGRESSION, FISSION PRODUCT RELEASE, MIGRATION AND REMOVAL MECHANISMS, AND PHENOMENOLOGY UNCERTAINTIES AND PROCESS-INDUCED FAILURES

- CONSIDERATION OF IDCOR AND APS ISSUES IN UNCERTAINTY ANALYSIS

GENERALIZED CONTAINMENT EVENT TREE
(SCHEMATIC)

INPUTS
PLANT DAMAGE STATE DEFINITION & FREQUENCY



OUTPUTS
1. CORE MELT OR SUCCESSFUL RECOVERY (LINKED TO ASEP)
3. CONTAINMENT RELEASE MODE FREQUENCIES
2. SOURCE TERM BIN FREQUENCIES

QUESTIONS ASKED ABOUT

- OPERATOR ACTIONS, E.G.
  - IS THE VESSEL DEPRESSURIZED?
  - ARE DRYWELL SPRAYS ACTUATED?
  - IS THE CONTAINMENT VENTED?

- EVENTS & TIMING, E.G.
  - CORE DAMAGE BEFORE CONTAINMENT FAILURE?
  - CONTAINMENT FAILURE BEFORE, AT, OR AFTER VESSEL BREACH?

- PLANT CONDITIONS, E.G.
  - WHAT IS THE CONTAINMENT LEAKAGE LEVEL?
  - WHAT IS THE POOL BYPASS FLOW LEVEL?

- PHENOMENA, E.G.
  - CONTAINMENT FAILURE PRESSURE?
  - CONTAINMENT PRESSURE RISE DUE TO H_2 BEFORE VESSEL MELTTHROUGH
  - HYDROGEN BURNS IN REACTOR BUILDING?

SOURCES OF INFORMATION

NRC-SPONSORED STUDIES

- CONTAINMENT LOADS WORKING GROUP (CLWG)
- CONTAINMENT PERFORMANCE WORKING GROUP (CPWG)
- SEVERE ACCIDENT SEQUENCE ANALYSIS (SASA) PROGRAM
- BATTELLE ANALYSES FOR ACCIDENT SOURCE TERM PROJECT OFFICE (BMI-2104)
- REACTOR SAFETY STUDY (RSS) AND SUBSEQUENT RISK ASSESSMENTS
- STEAM EXPLOSION REVIEW GROUP (SERG)
- GENERIC SAFETY ISSUE STUDIES (TAP A-43, A-44, A-45, ATWS)
- OTHER STUDIES BY THE NATIONAL LABORATORIES (SNL, BNL, PNL) REPORTED IN NUREG DOCUMENTS

UTILITY

INDUSTRY-SPONSORED STUDIES

- FULL SCOPE PRAs
- INDUSTRY DEGRADED CORE (IDCOR) PROGRAM
- FINAL SAFETY ANALYSIS REPORTS (FSARs)
- PLANT-SPECIFIC EMERGENCY OPERATING PROCEDURES
- STATION BLACKOUT DATA AND ANALYSIS (EPRI)

SOURCE TERM METHODOLOGY

METHODOLOGY IS ESSENTIALLY THE SAME AS USED IN BMI-2104 WITH SOME IMPROVEMENTS

SOURCE TERM CODE PACKAGE--INTERIM VERSION

MARCH3

- CORSOR INTEGRATED INTO MARCH
  - CONSISTENT TREATMENT OF FP RELEASE FROM FUEL
- CORCON REPLACES INTER IN MARCH
  - CONSISTENT TREATMENT OF CORE-CONCRETE ATTACK

TRAP-MERGE

- COUPLED ANALYSIS PERMITS TREATMENT OF REVAPORIZATION IN-VESSEL

VANESA, NAUA, SPARC, ICEDF

- SAME AS BMI-2104 EXCEPT INTERFACES REQUIRE LESS USER INPUT

SURRY ANALYSIS RELIES HEAVILY ON NUREG-0956

BINNING PROCESS

- 0 DOMINANT SEQUENCES IDENTIFIED BY REBASELINING ANALYSIS
- 0 CONTAINMENT EVENT TREE REDUCED TO KEY FAILURE MODES
- 0 SPECIFIC NEED FOR STCP RUNS DETERMINED BY JUDGEMENT OF ANALYSIS TEAM AND SOURCE TERM EXPERTS (SNL, BCL, BNL, ORNL, NRC)
- 0 RELEASE CHARACTERISTICS GENERATED FOR EACH BIN

NEW FEATURES IN MACCS

1. TREATMENT OF TIME DEPENDENT RELEASES

2. IMPROVED ATMOSPHERIC DISPERSION

   CROSS WIND DISTRIBUTION, PARTICLE SIZE DISTRIBUTION, UP TO DATE PLUME RISE MODEL

3. ADDITIONAL EXPOSURE PATHWAYS

   (FOOD PATHWAYS, DEPOSITION TO SKIN, EARLY RESUSPENSION) (ADULT DOSE CONVERSION FACTORS, SKIN DOSE, POPULATION DOSE)

4. IMPROVED DOSIMETRY

5. EMERGENCY RESPONSE

   (GRADED RESPONSE, ROAD NETWORK EVACUATION, DELOCATION OF SHELTERED INDIVIDUALS, RELOCATION OUTSIDE EPZ)

6. LONG-TERM PROTECTION

   (LONG-TERM GROUNDSHINE, DIRECTOR DEPOSITION ON CROPS)

7. HEALTH EFFECTS

   (REVIEW AND UPDATE WASH-1400, HAZARD FUNCTIONS USED FOR EARLY EFFECTS, ADDITIONAL INJURIES, LATENT EFFECTS--BEIR III, UPDATED GENETIC EFFECTS)

7

NUREG-1150 SENSITIVITY/UNCERTAINTY ANALYSES

OBJECTIVES:

1. PROVIDE A GOOD ENGINEERING PERSPECTIVE ON MODELING ASSUMPTIONS THAT DRIVE THE ANALYSIS (SENSITIVITIES)

2. PROVIDE A "REASONABLE" ENVELOPE IN WHICH THE ACTUAL VALUE WOULD LIKELY BE FOUND AND WHICH NEED NOT BE EXPRESSED IN TERMS OF FORMAL STATISTICAL BOUNDS (UNCERTAINTY)

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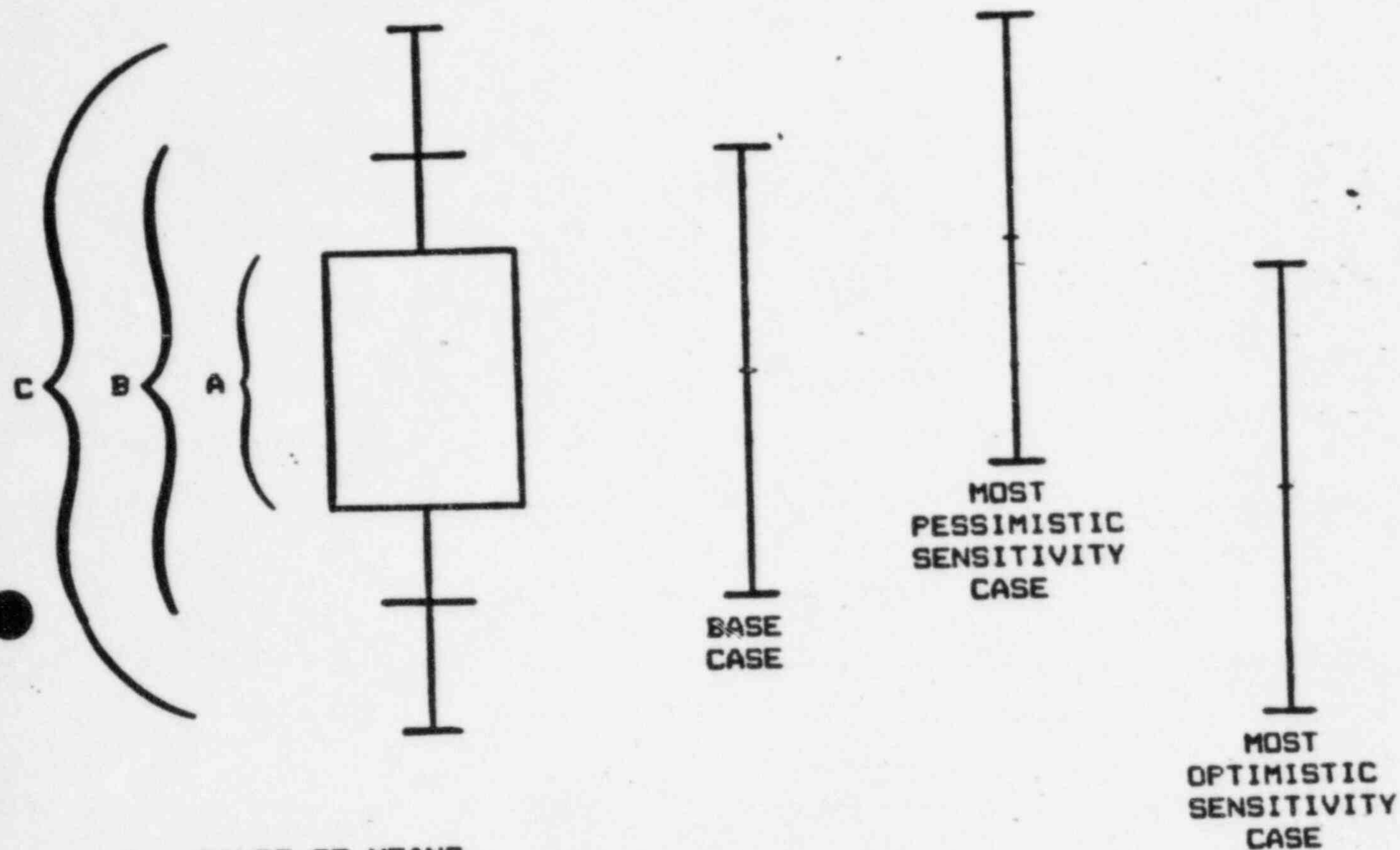
ACCIDENT FREQUENCY REBASELINING

O DATA--USE LOG-NORMAL DISTRIBUTIONS AND PROPAGATE THROUGH FAULT TREES AND EVENT TREES

O MODELING ASSUMPTIONS--PERFORM SENSITIVITY ANALYSES ON KEY ASSUMPTIONS

CORE MELT FREQUENCY DISPLAY



A. RANGE OF MEANS
B. "5/95" -- BASE CASE ONLY
C. "5/95" -- EXTREMES OF SENSITIVITY CASES

UNCERTAINTIES IN MODELING OF SOURCE TERM AND CONTAINMENT PHENOMENOLOGY PERFORMED USING LIMITED LATIN HYPERCUBE METHODS

- O CONSTRAINED (STRATIFIED) MONTE CARLO SAMPLING SCHEME (NUREG/CR-3624)
- O REQUIRES SUBJECTIVE ESTIMATES OF POSSIBLE RANGES ON PARAMETERS
  - BASED ON EXPERT JUDGEMENT
  - UTILIZES 3-5 SPECIFIC VALUES WITH WEIGHTS TO EACH VALUE
  - WEIGHTS ROUGHLY AKIN TO DEGREES OF BELIEF
  - SHOULD BE INTERPRETED AS ENGINEERING JUDGEMENT, NOT PRECISE STATISTICAL PARAMETERS
- O CONSTRAINED TO 10-15 IMPORTANT ISSUES AND 20-30 RUNS PER PLANT BY AVAILABLE RESOURCES
- O CORRELATIONS BETWEEN ISSUES EXPLICITLY CONSIDERED

LIMITED APPLICATION OF LATIN HYPERCUBE SAMPLING

DISADVANTAGES:

1. DISCRETIZATION AND WEIGHTING OF INPUT PARAMETER SPACE REQUIRES ENGINEERING JUDGEMENT.

2. RESULTS ARE CONDITIONAL UPON

   A. DISCRETIZATION AND WEIGHTING OF INPUT PARAMETER SPACE

   B. LIMITED NUMBER OF ISSUES CONSIDERED

3. SEPARATE CALCULATIONS BASED ON RESULTS OF EXISTING STCP RUNS WILL BE REQUIRED TO QUANTIFY SOME ISSUES.

SUMMARY

- REBASELINING USING COMPRESSED PRA METHODS WHICH APPEAR HIGHLY COST EFFECTIVE
- EVALUATION OF ACCIDENT PHENOMENOLOGY WITH BEST TOOLS AVAILABLE
- OFF-SITE CONSEQUENCE ANALYSIS USING IMPROVED CODE
- UNCERTAINTY ANALYSIS DESIGNED TO YIELD REASONABLE ENGINEERING PERSPECTIVES ON POSSIBLE RANGE OF OUTCOMES CAPABLE OF FOCUSING ATTENTION ON IMPORTANT ENGINEERING AND PHENOMENOLOGICAL ASSUMPTIONS OR LACK OF KNOWLEDGE
- UNCERTAINTY ESTIMATES NOT RIGOROUS

STAFF POSITIONS ON ISSUES FOR NUREG-1150

MARK A. CUNNINGHAM

OFFICE OF NUCLEAR REGULATORY RESEARCH
DIVISION OF RISK ANALYSIS AND OPERATIONS

JANUARY 29, 1986

THE PURPOSE OF THE NRC WORK ON POSITION PAPERS IS TWOFOLD:

- TO DEVELOP, FOR NUREG-1150 AND THE IDCOR RESOLUTION PROCESS, STAFF POSITIONS ON THE RISK IMPORTANCE OF A SET OF MAJOR ISSUES, DRAWN FROM:

  - IDCOR-NRC TECHNICAL ISSUES
  - ASEP-SARRP REFERENCE PLANT STUDIES
  - NUREG-0956/BMI-2104

- TO SUPPORT THE SARRP AND NUREG-1150 REFERENCE PLANT UNCERTAINTY/SENSITIVITY ANALYSIS WITH BEST-AVAILABLE INFORMATION

NRR AND RES STAFF AND CONTRACTORS ARE SUPPORTING THE DEVELOPMENT OF POSITION PAPERS THROUGH A SERIES OF STEPS

STEPS

1. ISSUE IDENTIFICATION

2. SELECTION OF ISSUE TEAMS

3. COMPILATION AND INTERPRETATION OF EXISTING DATA

   DATA SOURCES
   - RES
   - NRR
   - IDCOR
   - OTHER

   INTERPRETATION
   - COMPARISONS
   - DEVELOPMENT OF RANGES, WEIGHTS

STEPS (CONTINUED)

4. IDENTIFICATION AND PERFORMANCE OF SHORT-TERM ANALYSES

5. DEVELOPMENT OF POSITION PAPER

6. COORDINATION/REVIEW BY STAFF/MANAGEMENT

   - SENIOR STAFF COORDINATING GROUP
   - MANAGEMENT OVERSIGHT/CONCURRENCE

IN GENERAL, THE NRC ISSUES ARE DEFINED IN TERMS OF MAJOR SARRP INTERMEDIATE PRODUCTS:

- ACCIDENT SEQUENCE FREQUENCIES
- SOURCE TERMS
- CONTAINMENT
  - LOADS (PRESSURE/TEMPERATURE)
  - STRUCTURAL RESPONSE
  - FAILURE MODE PROBABIL'TIES

ISSUES

- CORE-CONCRETE INTERACTIONS
  - THERMAL-HYDRAULICS (IDCOR #10)
  - RADIONUCLIDE RELEASES (IDCOR #9)
- DIRECT HEATING (IDCOR #8)
  - POTENTIAL FOR INDUCED RCS FAILURE
  - IN-VESSEL CONDITIONS
  - EX-VESSEL DISPERSAL
- HYDROGEN
  - GENERATION (IDCOR #5)
  - BURNING (IDCOR #17)
- CHEMICAL FORM OF IODINE
  - WITHIN RCS
  - CONTAINMENT

ISSUES (CONTINUED)

- RCS NATURAL CIRCULATION (IDCOR #2)
  - LOCALIZED OVERTEMPERATURE FAILURE OF RCS
  - REVAPORIZATION OF RADIONUCLIDES
  - HYDROGEN GENERATION
- REVAPORIZATION (IDCOR #11)
  - PRE-VESSEL BREACH
  - POST-VESSEL BREACH
- CONTAINMENT RESPONSE (IDCOR #15)
  - LEAKAGE AREA BEFORE "GROSS" FAILURE
  - PRESSURE/TEMPERATURE LOADS FOR "GROSS" FAILURE
  - SIZE OF RESULTING HOLE

ISSUES (CONTINUED)

- SECONDARY BUILDING PERFORMANCE (IDCOR #16)
- VENTING
  - HUMAN RELIABILITY
  - ENGINEERING FEASIBILITY
- STEAM EXPLOSIONS (IDCOR #7)
  - PROBABILITY OF α FAILURE MODE
- COMMON CAUSE FAILURES

REFERENCE PLANT ISSUES

PLANT	SU	PB	SE	GG	ZI	LA
CORE-CONC. INTERACTIONS (DSRO & DAE)		X		X		X
DIRECT HEATING (DSRO & DAE)	X		X		X	
H2 GEN/LOADS (DSRO & DAE)			X	X		
CHEM. FORM OF I (DAE & DSRO)	X	X	X	X	X	X
RCS NAT. CIRC (DAE & DSRO)	X		X		X	
REVAPORIZATION (DAE & DSRO)	X	X	X	X	X	X
CONT. RESPONSE (DET & DSRO)	X	X	X	X	X	X
SEC. BLDG. PERF. (DAE & DSRO)		X		X		X
VENTING (DRAO & DSRO)		X		X		X
STEAM EXPLOSIONS (DSRO)	X	X	X	X	X	X
COMMON CAUSE FAILURES (DRAO & DSRO)	X	X	X	X	X	X

THE POSITION PAPER FORMAT IS SIMILAR TO THAT USED IN IDCOR TECHNICAL REPORT 85.2

1. DEFINITION OF ISSUE

2. RELEVANT PLANTS AND SEQUENCES

3. ISSUE RANGE

   A. IMPORTANT SUBISSUES
      - DEFINITION
      - RANGES
      - WEIGHTS
      - COMPARISONS (BMI-2104, SARRP, IDCOR, ETC.)

   B. METHOD FOR DEVELOPING ISSUE RANGE FROM SUBISSUE RANGES
      - CODE CALCULATIONS

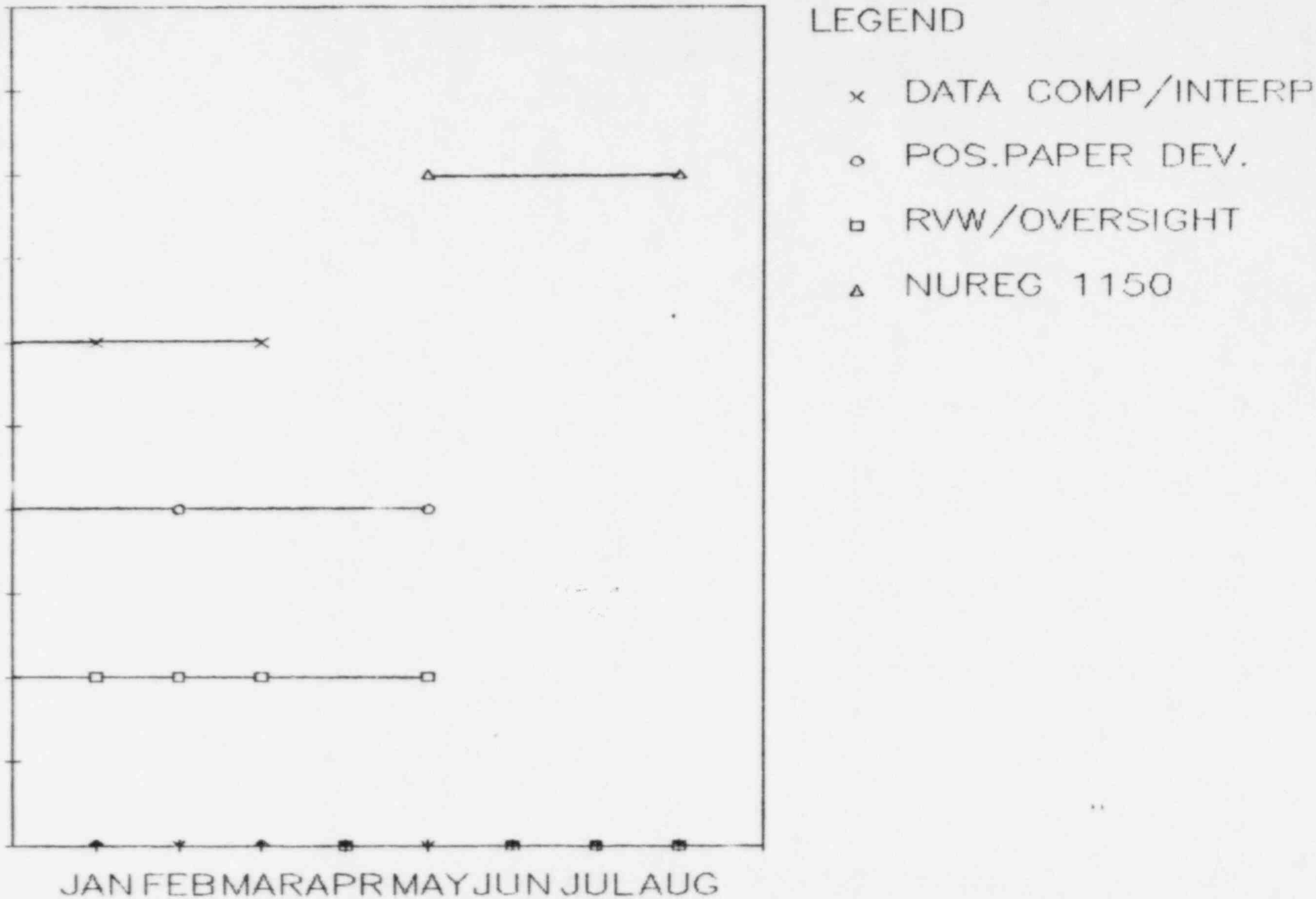
   C. RESULTS FOR ISSUE RANGE WITH COMPARISONS (BMI-2104, SARRP, IDCOR, ETC.)

4. CONCLUSIONS ON RISK IMPORTANCE

5. LONG-TERM WORK

6. REFERENCES

POSITION PAPER SCHEDULE



B12 1-28-86

ACRS SUBCOMMITTEE MEETING

NUREG-1150

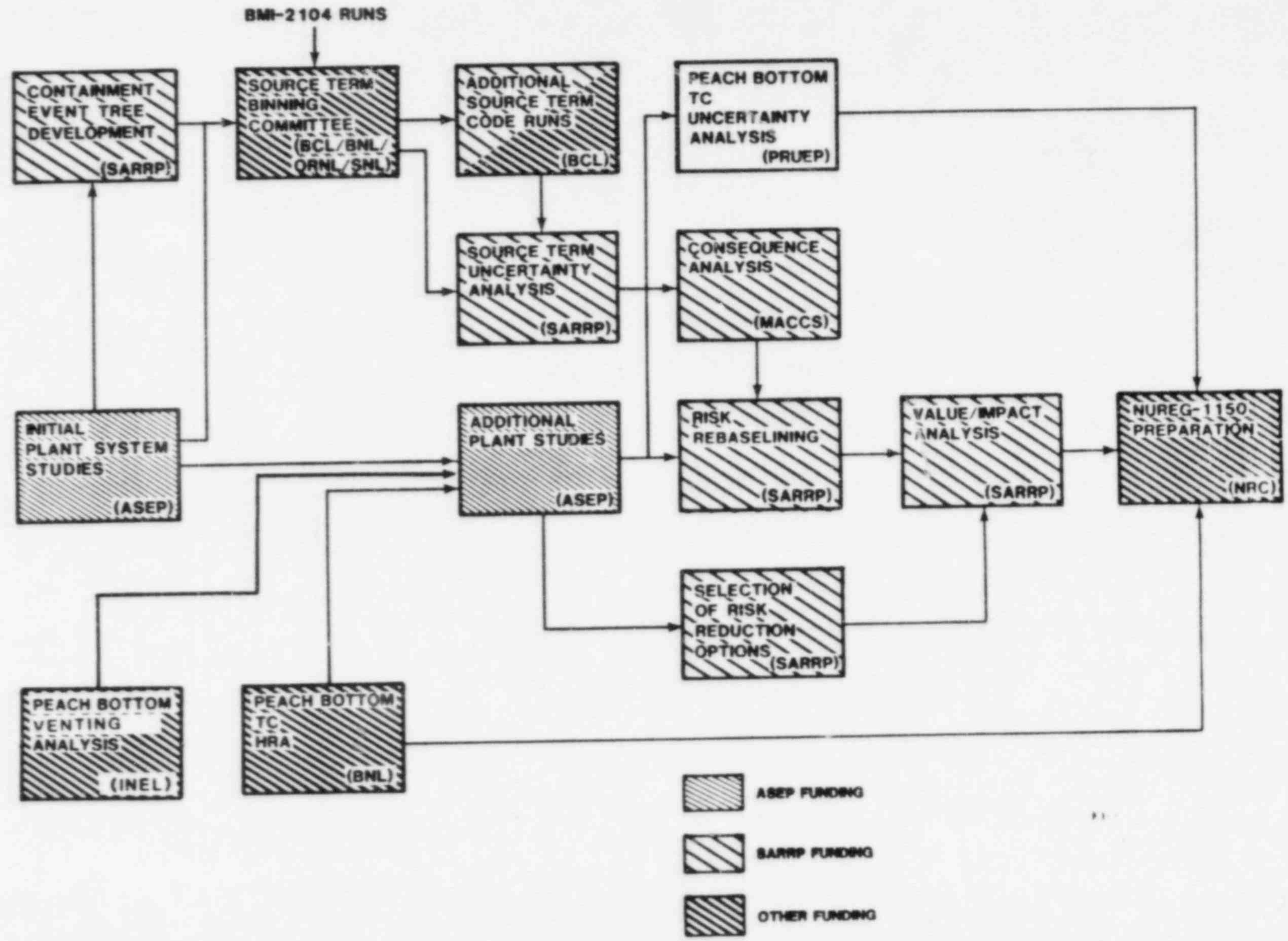
JANUARY 29, 1986

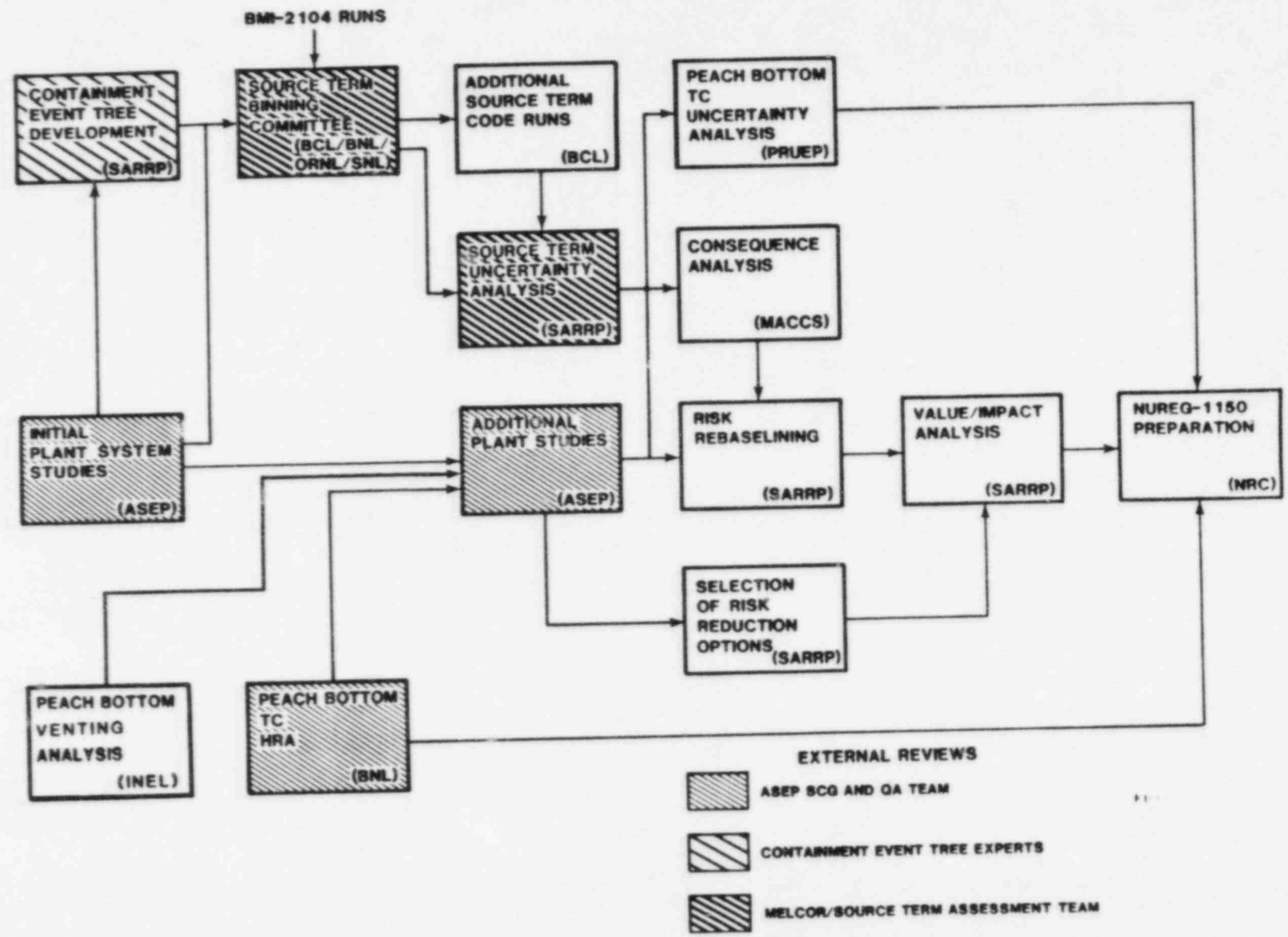
1.	OVERVIEW (PURPOSE, CONTENT, SCHEDULES, ETC.)	M. L. ERNST
2.	METHODOLOGY AND UNCERTAINTIES	J. A. MURPHY
3.	NRC POSITIONS ON MAJOR ISSUES	M. A. CUNNINGHAM

PURPOSE OF NUREG-1150

- o PROVIDE GREATER UNDERSTANDING OF CURRENT RISKS FROM SEVERE ACCIDENTS AT NUCLEAR POWER PLANTS
  - INTERNAL EVENTS AT SIX REFERENCE PLANTS
  - DIFFERENT CONTAINMENT DESIGNS
  - TMI FIXES
  - IMPROVED SOURCE TERM INFORMATION

- o ASSESS USEFULNESS OF SUCH INFORMATION TO:
  - EXTRAPOLATE TO OTHER PLANTS
  - MAKE PLANT-SPECIFIC AND GENERIC REGULATORY DECISIONS
  - PRIORITIZE AGENCY RESOURCES


BMI-2104 RUNS
CONTAINMENT EVENT TREE DEVELOPMENT (SARRP)
SOURCE TERM BINNING COMMITTEE (BCL/BNL/ ORNL/SNL)
ADDITIONAL SOURCE TERM CODE RUNS (BCL)
PEACH BOTTOM TC UNCERTAINTY ANALYSIS (PRUEP)
SOURCE TERM UNCERTAINTY ANALYSIS (SARRP)
CONSEQUENCE ANALYSIS (MACCS)
INITIAL PLANT SYSTEM STUDIES (ASEP)
ADDITIONAL PLANT STUDIES (ASEP)
RISK REBASELINING (SARRP)
VALUE/IMPACT ANALYSIS (SARRP)
NUREG-1150 PREPARATION (NRC)
SELECTION OF RISK REDUCTION OPTIONS (SARRP)
PEACH BOTTOM VENTING ANALYSIS (INEL)
PEACH BOTTOM TC HRA (BNL)
ASEP FUNDING
SARRP FUNDING
OTHER FUNDING



BMI-2104 RUNS
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CONSEQUENCE ANALYSIS (MACCS)
INITIAL PLANT SYSTEM STUDIES (ASEP)
ADDITIONAL PLANT STUDIES (ASEP)
RISK REBASELINING (SARRP)
VALUE/IMPACT ANALYSIS (SARRP)
NUREG-1150 PREPARATION (NRC)
SELECTION OF RISK REDUCTION OPTIONS (SARRP)
PEACH BOTTOM VENTING ANALYSIS (INEL)
PEACH BOTTOM TC HRA (BNL)
EXTERNAL REVIEWS
ASEP SCG AND QA TEAM
CONTAINMENT EVENT TREE EXPERTS
MELCOR/SOURCE TERM ASSESSMENT TEAM

1/28/86
(REVISED)

SCHEDULE FOR NUREG-1150 AND NRR SUPPORT

ACTIVITY	SURRY	PEACH BOTTOM	SEQUOYAH	GRAND GULF	ZION	LASALLE
1. ACCIDENT SEQUENCE INITIAL INPUT	C	C	C	C	C	2/13/86
2. SOURCE TERM BINNING	C	C	C	C	C	2/13/86
3. NUMBER OF SOURCE TERM CODE RUNS	4	6	8	3	4	3-6
4. SOURCE TERM CODE RUNS	C	C	C	C	1/31/86	3/27/86
5. RELEASE CHARACTER-ISTICS	C	C	C	1/30/86	1/31/86	4/21/86
6. CONSEQUENCE CALC.	C	C	2/3/86	2/13/86	2/28/86	5/5/86
7. REFINE ACCIDENT SEQUENCES	C	C	C	C	C	3/12/86
8. CONTAINMENT TREES DRAFT REPORT	C	2/7/86	C	3/3/86	3/31/86	6/4/86
9. BASELINE RISK CALCULATION	C	2/12/86	2/17/86	3/6/86	3/14/86	6/4/86
10. RISK/RISK RED. TABLES	C	2/28/86	3/10/86	4/15/86**	3/28/86**	6/16/86**
11. RISK/RISK RED. DRAFT DETAILED RPT.	4/2/86**	5/5/86**	5/29/86**	5/13/86	4/30/86	7/15/86
12. RISK/RISK RED. FINAL DETAILED RPT.	5/29/86	7/1/86	7/24/86	7/10/86	6/30/86	9/10/86

**INCLUDES FINAL SENSITIVITY ANALYSES AND UNCERTAINTY RANGES

CONTENTS OF NUREG-1150

NUREG-1150 CONTENTS (CONTINUED)

FUTURE SCHEDULE

o PUBLISH DRAFT NUREG-1150	AUGUST 1986
o PUBLISH FINAL TECHNICAL REPORTS	MAY-SEPTEMBER 1986
o PUBLIC COMMENT	SEPTEMBER-NOVEMBER 1986
o WORKSHOP	OCTOBER 1986
o FINAL NUREG-1150	SPRING 1987
o ANALYSIS OF B&W AND CE PLANTS	SUMMER 1987
o ASSESSMENT OF NEW RESEARCH RESULTS	FY 1987
o UPDATE OF NUREG-1150	AS WARRANTED