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UNITED STATES NUCLEAR REGULATORY COMMISSION'S
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

DATE: Thursday, October 24, 1991

The contents of this transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards, (date) Thursday, October 24, 1991, as reported herein, are a record of the discussions recorded at the meeting held on the above date.

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Meeting of the Severe Accidents Subcommittee

Conference Room P-1110
Nuclear Regulatory Commission
7920 Norfolk Avenue
Bethesda, Maryland

Thursday, October 24, 1991

The above-entitled proceedings commenced at 8:30 o'clock a.m., pursuant to notice, W. Kerr, subcommittee chairman, presiding.

PARTICIPANTS:

W. KERR, ACRS Subcommittee Chairman
I. CATTON, ACRS member
T. KRESS, ACRS member
C. SIESS, ACRS member
D. WARD, ACRS member
P. DAVIS, ACRS consultant
M. CORRADINI, ACRS consultant
D. HOUSTON, Cognizant ACRS staff member
C. BECKJORD, NRC/RES
B. SHERON, NRC/RES
F. ELTAWILA, NRC/RES
C. TINKLER, NRC/RES
R. FOULDS, NRC/RES
R. LEE, NRC/RES
A. RUBIN, NRC/RES
R. WRIGHT, NRC/RES
L. SHOFKIN, NRC/RES
D. POWERS, Sandia National Laboratories
J. REMPE, Idaho National Engineering Laboratory

PROCEEDINGS

[8:30 a.m.]

MR. KERR: The meeting will come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, the Subcommittee on Severe Accidents.

My name is Kerr. I'm subcommittee chairman. The ACRS members in attendance at present are Mr. Siess, Mr. Ward, Mr. Kress, Mr. Catton.

We also have as a consultant so far Mr. Corradini.

The purpose of the meeting is to discuss various elements of the NRC Staff's severe accident research plan.

Dean Houston is the Cognizant ACRS Staff Member for the meeting.

Rules for participation in today's meeting have been announced as part of the notice of the meeting published in the Federal Register of Tuesday, October 8th of this year.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice. I would ask that each speaker identify himself or herself and use the microphones so that we and the recorder can hear what is said.

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

Before we proceed with the meeting, I will ask those of the committee or our consultants if you have any comments to make or any questions to raise at this point?

[No response.]

MR. KERR: I see none. I will comment briefly that in April of 1990, the ACRS conducted a review of the severe accident research program and reported to the Commission on the program in a report dated April 24th of 1990. Having reread the letter recently, I think it was a pretty good letter, a useful review of the existing and planned program.

Some parts of the letter apparently were misunderstood by the Staff, and this had to do with our comment on the application of an approach to the planning of experimental research.

We commented on an approach which was presented to us as part of the set of questions that had been developed by the Technical Program Group, I think it is, the TPG at any rate, that was part of the severe accident scaling methodology program, and let me read what we said in connection with the core melt progression program on which we commented:

"We commend especially the lessons learned from that program" -- "that" referring to the severe accident scaling methodology program -- and the evaluation questions for proposed severe accident experimental programs as were discussed with our subcommittee on March 20 and 21, 1990."

And this was part of that presentation, and specifically something labeled "Evaluation Questions for Proposed Severe Accident Programs". I went back and looked this up. And in case it has somehow been lost, I have a copy which can be made available to anyone who is interested.

The questions had to do with how one might go about evaluating a proposed research program and its applicability to the overall NRC program.

With that, I will turn the proceedings over to Mr. Brian Sharon, who will introduce and guide our further presentations.

Mr. Sharon.

[Slide.]

MR. SHARON: My name is Brian Sharon. I am Director of the Division of Systems Research in the Office of Nuclear Regulatory Research. And what I'd like to talk to you a little bit about at first before we start our regular presentations is an overview of where we are with the program.

[Slide.]

MR. SHARON: As Dr. Kerr said, we were down here in April of 1990 and gave a presentation at that time on where we were, and we had been down previously, I think, in

1989 and described our revised severe accident research program.

The objective, if you remember, of our revised severe accident research program was to emphasize resolution of issues, at least one part of it was, to emphasize issues that continue to contribute to large uncertainties in early containment failure, since the events which failed the containment early were the ones which appear to be driving risk in the risk analyses.

I would point out that this approach -- and we are still following this approach, which is basically the pursuit of trying to resolve the issues associated with containment loads -- appears consistent with the ACRS philosophy on containments, as was documented in your letter of May 17th to Chairman Kerr.

Other areas that we were emphasizing were scaling of severe accident experiments and trying to get a better handle on our code development program.

[Slide.]

MR. SHARON: I wanted to put this up just to show you how all of our research programs sort of fit together and tie together into a program which is ultimately designed to predict containment performance and then offsite consequences of severe accident.

The research that we do right now, the

experiments, are all designed to validate the various codes that you see here, either the MELCOR code, which is basically our workhorse which will be designed -- you know, should be able to take us from the initiation of the accident all the way through, and then there are the more detailed mechanistic codes which look at the phenomena in more detail, and you can see the various codes here. I won't go into these right now, because you'll hear more about them during the next day and a half.

But again, as you follow this through, you see that ultimately we're trying to get a handle on containment loads and the containment performance of the plant. So in general, that's really where the thrust of our program is.

[Slide.]

MR. SHARON: Now if you recall, the two early containment failure issues that were identified in the revised severe accident research program are the Mark I liner issue and direct containment heating.

With regard to the Mark I liner issue -- which you'll hear more of as the day goes on; Dr. Eltawila has a presentation -- basically Professor Theofanous had completed his report last year. We had a large peer review meeting at Harper's Ferry, West Virginia in July of 1990. The results of that identified four areas which were believed to require further confirmatory work in order to substantiate the assumptions that were made in Professor Theofanous' report.

We initiated this work. You'll hear about it. It's in progress now. When we complete this work and we decide whether Professor Theofanous' report is still consistent with the result of this work or whether any modification needs to be made, but in any event the final report will undergo a final peer review, and this is going to be a formal peer review. It will be a FACA, Federal Advisory Committee Act, in essence just like we did on NUREG-1150.

The DCH testing at Sandia and Argonne, if you remember, that was stopped, principally because of questions about the scaling rationale that was used for th- tests.

We have restarted that now. They have both come up with scaling methodologies and rationales to support the tests that they propose to run. Sandia has already run the first DCH test, I think about a month ago, and Argonne, I think, has either run it or is planning to run it this month.

MR. ELTAWILA: Planning to run it.

MR. SHARON: Planning to run it. And that's the counterpart test.

We've also put together -- we're starting to put together a draft resolution plan for the DCH issue. This plan basically is going to integrate all of the information we have with regard to likelihood of initiation of an accident, looking at questions like whether a high-pressure scenario and natural circulation within the vessel or else just flow rates of high-temperature gases exiting an open PORV, for example, would be sufficient to fail a surge line and thereby depressurize the system.

We will look at how much information we have and whether we can quantify the distribution of material that eventually arrives in the lower head of the vessel and is available to be ejected following vessel failure, and then look at the likelihood that given a vessel failure at high pressure, that one would generate loads that would exceed the containment design pressure.

This plan has been drafted. Right now, there's a first draft kicking around just within the Division now. We'd like to get that firmed up, and we will most likely be sending it down to the committee for their review, and I presume we would like to have a meeting on it in the future, hopefully by December or maybe January of next year.

[Slide.]

MR. SHARON: As Dr. Kerr said, there was a Technical Program Group that was charged with developing a severe accident scaling methodology called SASM. This report was finally completed as a draft, I think about two weeks ago. There are still some pieces missing, as we understand. What we are doing, though, is we have taken the report, the information that is available from all the authors, and we are putting it together as a NUREG draft for comment, which will be issues hopefully in November, I believe.

The report will be provided to our contractors for their use in developing scaling rationales on any proposed severe accident experiments. Again, I have to emphasize that the purpose of the report was to develop a general methodology or a guidance and that our contractors are held ultimately responsible for providing the scaling analysis that justifies the tests that the propose to run.

With regard to codes, we have put in place a program to peer review all of our major thermal hydraulic and severe accident codes, and you will hear more about that later. As you know, we had the MELCOR code review -- when was it?

MR. ELTAWILA: It is completed now.

MR. SHARON: Okay, yes. That's completed, the MELCOR peer review.

We have the RELAP code, which is being reviewed as part of the CSAU analysis for the small break, and I think the next one up for severe accidents is the SCDAP code.

The purpose of these peer reviews is, we want to get a good, independent evaluation from experts on what areas of modeling are considered good enough, what areas need further work, the adequacy of the documentation, and how well does the code meet its objectives.

As I think I pointed out in a previous presentation, this is a very expensive process. I think as a rule of thumb something, about 25 percent of the costs of a code are tied up in either peer reviews or the documentation process. However, we think the results are well worth the expenditure.

[Slide.]

MR. SHARON: The remaining areas of severe accident research, our primary emphasis again is understanding the containment loads.

There are two major phenomenological areas that really govern loads, when you think about it. One is the amount, temperature, composition, and ejection energy of the melt. Obviously if the melt is ejected at high energy, it can be dispersed in the lower cavity and the lower compartments of the containment, and it will interact with the containment atmosphere, and you have hydrogen generation and so forth, and you have loads that are associated with that phenomena.

If you have a depressurized system, so that when the vessel fails, the melt essentially falls into the lower cavity, you now have basically interaction with the concrete.

Our DCH, as I mentioned before, is being handled -- we have a separate, focused program, I think, to try and come to resolution on the DCH issue. Dr. Siess, I think, will be glad to hear that we've declared victory on core concrete interaction research, and we are not performing any more research. We believe that we've done enough, so that has been stopped. There is no more going on.

The focus right now is on debris coolability. This is not really the interaction of the melt with the concrete, so much as it is as whether an overlying pool of water on the melt can cool the debris, or whether you form a crust and it insulates the water from the debris, and therefore you don't get the cooling, and you continue the core concrete interaction.

This is important for a number of reasons, principally on the advanced light water reactors. This is an important issue. You'll hear more about this. They have -- EPRI has put in their requirements document a criteria for a lower cavity area of .02 meters square per megawatt of decay heat. And the question is whether that will -- and their argument is that by providing that, you will have a sufficient area of coolable debris.

There are tests being run at MACE. There's a question of whether they're prototypical of the advanced designs, and again, you'll hear more about this. But this is an important issue now.

MR. CATTON: Brian, what about hydrogen?

MR. SHARON: Yes?

MR. CATTON: What about hydrogen?

MR. SHARON: That's coming up.

MR. CATTON: Because you list two major phenomenological areas governing loads. Shouldn't there be three?

MR. SHARON: Not -- well, we're not talking about it from the standpoint of --

MR. ELTAWILA: Brian, the area of DCH has embedded in it, is the generation of a large quantity of hydrogen at high temperature, and that is part of this.

MR. CATTON: Well, the hydrogen problem is broader than that.

MR. ELTAWILA: We were going to give you a presentation on it tomorrow, and if you have any question after that or --

MR. CATTON: Well, I'm just wondering why don't you say three. The Germans say three.

MR. ELTAWILA: The Germans -- I hope we don't confuse the German design with the American design. The German design has its own problems, because they want to have a mixing system in the containment. So the issue of hydrogen distribution in the German is very important.

For the American designs, we have a spray system. We have a fail system. And we rely on them on mixing in the containment. And that's why the Germans consider hydrogen to be a very important issue, and we consider hydrogen to be important, but not as important as it is for the German design.

MR. CATTON: Do you have something that deals with this issue that I could read? And I hear the word "mixing", but I'd like to see something more substantive.

MR. ELTAWILA: Do we have --

MR. SHARON: Why don't you let see if there's something that we can provide you on this?

MR. CATTON: All right.

MR. SHARON: And I agree. One could put hydrogen here somewhere, I think, okay. I was sort of under the impression that it was included in the DCH. I think you're saying that there may be --

MR. CATTON: Well, there are cases where there may by no DCH if the hydrogen is still a problem.

MR. SHARON: Yes, okay, I agree. We could probably add that in here.

The last bullet here, right now the focus of our remaining efforts in the severe accident research is on the in-vessel core melt phenomena. Right now, as you know, this is kind of a tough area, the whole in-vessel core melt.

The early phase, as you know, we've done a lot of testing on it and the like. The latter phases of it, when you start forming crucibles and having melt relocate to the lower head, there's not a lot of data available at all, and it's certainly not a process that's easily amenable to experiments

We had some experiments that were defined, the MP series tests and the Ex Reactor tests. Before we went forward -- these were expensive tests -- and before we went forward, I had asked to have them peer reviewed. That process is underway right now. We're getting back some comments from our consultants, and you'll hear more about that later.

MR. KRESS: Brian, before you leave that slide, I know how you know when something like core concrete interaction is completed. That's when you quit.

How do you know when you've done enough research? Do you have a criteria to say that this is enough; we can quit now?

MR. SHARON: I could tell you what I always used to tell people, and that is the criteria is going to run out of money.

MR. KRESS: That is one criteria.

MR. SHARON: That wasn't the case here. I think what it is, is when one looks at the database that we had, we had run experiments, I think, on the spectrum of concretes and so forth and melt compositions. The models seemed to be doing a fairly good job. I know we ran into a standards problem, and I think most people were doing fairly good in terms of predicting the interactions and the like.

I think, you know, the decision was is that, you know, we're not going to gain a lot more knowledge by continuing to spend a lot more money here, and our money would be much better spent by focusing on other areas.

MR. KRESS: It's not a quantitative --

MR. SHARON: No, there's no quantitative criteria.

MR. KRESS: Were the uncertainties due to that or small enough that you can concentrate on something else or something?

MR. SHARON: Yes. It's more of a -- it's a qualitative judgment, I think, and it's collective, okay. I mean, we confer with our consultants, with our contractors, and if everybody seems to have, you know, a general agreement that, yeah, we've probably done enough and we can move onto something else, then that's a pretty good indication.

MR. SIESS: Brian, is the last bullet on that slide still under the heading indicated by the first bullet? Does the whole slide deal with containment loads, or is that the conclusion?

MR. SHARON: This is still under that, because --

MR. SIESS: Then why -- I don't understand. If there's no ex-vessel phenomena, we haven't got any containment problems. So why is the major effort on in-vessel core melt phenomena when we're concerned with containment loads?

If it doesn't get out of the vessel, there is one source, I think, hydrogen. But you didn't have that on there.

MR. SHARON: No. You need to know, as I put on a previous slide, okay -- you need to know, for example, the DCH loads are dependent upon the amount of material that comes that out, how much melt comes out. Is it 20 percent of the core, or is it 100 percent of the core?

MR. SIESS: But that's separate.

MR. SHARON: What is the temperature of the superheat, okay?

As Professor Theofanous showed in his report, that melt superheat is a very important function in terms of --

MR. SIESS: You've already eliminated those now.

MR. SHARON: Huh?

MR. SIESS: We're talking about the last bullet which says "the remaining effort." The DCH and CCI, you said, were taken care of.

MR. ELTAWILA: Dr. Siess, sir, I think the point is that what we are doing here, that we are emphasizing on resolution of the containment load based on -- I will try -- it's slightly -- some conservatisms are applied in the assumption that we are using in-containment load analysis.

And we are very close to the margin of failure in some of these instances. And the more we do research on in-vessel core melt progression, we will be able to reduce some of these uncertainties that will help us -- that can help either in accident management strategy, so we can take advantage of the margin that you have, instead of assuming that the containment is going to fail due to lighter containment heating because you are using conservative assumptions in this case.

So the approach is use as conservative as practical, some conservative assumptions in analyzing containment integrity, continue with the research on in-vessel core melt progression, try to give some information that can help review some of these uncertainties, so we can utilize the margin eventually for advanced light water reactors for the current generation plant in terms of accident management and so on.

MR. SIESS: I wish I could see a clearer relation between reducing uncertainties on the core melt progression and conservatisms in containment loads and containment design.

It sounds awfully tenuous to me, and by the time you get enough information on core melt progression, I suspect we're going to have most of those containments built.

MR. ELTAWILA: Most of these containments are already built, and we have the severe accident policy statement. We want to know how these containments, what is the capability of these containments to cope with the different severe accidents that can happen in this type of operating plant.

MR. SIESS: Do you really mean whatever severe accidents can happen?

MR. ELTAWILA: Severe accidents can happen?

MR. SIESS: Yes.

MR. ELTAWILA: They happen.

MR. SIESS: You mean, can happen, not likely to happen, or --

MR. SHARON: I don't understand. That's just semantics. I mean, you know, they can happen. TMI happened.

MR. SIESS: You don't understand the difference between -- it can happen. We can sit around here and come up with a pretty good accident that no containment will take. It can happen, but it may be 10 to the -9 probability.

Is that what we're trying to -- I guess we're off regulation and into safety, but --

MR. ELTAWILA: Probability of severe accidents are not in the 10 to -9. If you look at most of the PRAs for PWRs, you have a severe -- the frequency of severe accident can be from 10 to the -4, 10 to the -5; it depends on --

MR. SIESS: That's only the ones you've thought of?

MR. ELTAWILA: Pardon?

MR. SIESS: That's only the ones you've thought of.

MR. ELTAWILA: That's correct.

MR. SIESS: I think I could think of some that you couldn't prove can't happen.

MR. KERR: No, I think the question that he's asking, if I understand it, is does the probability of the severe accident enter into its importance in terms of your research program.

MR. SHARON: Yes, it does.

MR. ELTAWILA: Yes.

MR. SHARON: I mean, I'll be quite honest. We could stop doing all in-containment -- I mean in-vessel research tomorrow, okay. The problem is that there would be very, very large residual uncertainties, okay, that would be unanswered.

MR. SIESS: Would the uncertainty be large enough that the Commission would have to shut down existing plants in order to provide reasonable assurance that there's no undue risk to the health and safety of the public?

MR. SHARON: No.

MR. KERR: Let me follow up, I think, the question that Mr. Kress was raising.

It appears to me that there are sort of two ways that one can decide -- well, let me pick two out of a whole spectrum -- when one is finished, one is to wait until the experiment has been done and to look at what develops and say that's enough.

The other is to do some planning and say: Here's what we need in order to provide the assurance that we think is desirable and necessary.

My impression is that the approach being taken is somewhat closer to the first than it is to the second. And my question is: Do you have at least some small group of people looking at what is it that we think we'd need, that's realistic to talk about, that will reduce the uncertainty to an acceptable level?

MR. SHARON: Well, number one, there's no -- I don't have any small group of people that are looking at that. We look at it in a more global sense in terms of taking into consideration the results of PRAs like 1150 and the like.

The difficulty we have is that, for example, I could go to NUREG-1150 and I could say, well, all the plants meet the safety goals, so I shouldn't do any more research. That's one approach. They're all safe enough by definition.

The problem is that a lot of the work that was done on 1150 was based on expert elicitation, as you're well aware. In my mind, expert elicitation means that we don't have any data, so we had to go ask people what their opinion was about this.

And as much as we tried to quantify that opinion in terms of probability distributions and the like, okay -- I'll be quite honest -- you know, there are people that are just not comfortable with that, and they would rather see, you know, a much better experimental database to support the judgments that were being made.

And, you know, I can't - like I said, I could take one side of the coin and say 1150 says the plants are safe enough and I won't do any more research, okay, or I can say, yes, 1150 gave us a very good insight on where the important areas are and where the uncertainties are and where we ought to go after more data. And that's how we're using it

MR. KRESS: You could almost use that as a definition of confirmatory research. That's what confirmatory research is.

MR. SHARON: Exactly. And this what really this is. We're trying to confirm that the assumptions that were made, the expert elicitation results of 1150 --

MR. KERR: No, I have no quarrel with this. I'm trying to find out what sort of set of criteria one will use to say: With this, we'll consider it confirmed.

MR. SHARON: I'm sorry. With what?

MR. KERR: I'm trying to understand what set of criteria one is going to use, so that one will say at some point in the program, we now have enough data and enough analysis, so it is confirmed at a level which we consider appropriate.

And my impression is --

MR. SHARON: Like I just, you know, answered no to Tom, no, I don't have any quantitative criteria.

MR. KERR: Okay.

MR. SHARON: What we are doing, okay, and the criteria I am using, if you want to call it that, is the peer review process. What we have asked the peer reviewers is: If I run these experiments on in-vessel, okay, that we've proposed, am I going to get information that is going to provide substantial reductions in the uncertainty that currently exists in our in-vessel melt progression?

Now if they all come back and they say: No, these experiments are worthless; they're not going to get you a lot, no, I'm not going to run the experiments.

I've also asked them the question of: Can you tell me what experiments I ought to run that could do tha

Now if nobody comes and say: No, there are no experiments that you can run short of, say, a full-scale experiment for, you know, $500 million or something that's going to answer that, then we may have to say it's not worth it from a cost/benefit standpoint to try and do a lot more research in this area.

MR. KERR: Brian, the language that you're using is: Will the experiment provide a substantial set of data or substantial decrease in uncertainty.

MR. SHARON: Right.

MR. KERR: And that isn't the question that I'm asking. At least I don't think it is.

My question is: Whether it's substantial or not, is there some level which you can define either quantitatively or qualitatively at which you would be satisfied with things, and I'm using "you" collectively?

MR. SHARON: I don't know. It hasn't been defined.

MR. KERR: Well, don't you think it would be helpful to at least try to define it? I mean, I raise it in the context, for example, of this question under Table 2 that I referred to, and it says: "Is the experiment the most cost-effective means to develop the desired data in the context of the stated needs and objectives for experimentation?"

To me that says that one has thought about the data and analysis that one would have to have in hand in order to be satisfied that we've gone far enough.

I don't know how to do that. I'm not sure it's possible. But it seems to me that it would be worthwhile to expend some effort on it.

MR. SHARON: No, and I'm trying to explain why we couldn't. Let me give you an example, okay?

I asked the same question after NUREG-1150 came out, and it showed that direct containment heating was a very low-probability event, okay. And I said: Why am I spending over a million dollars a year doing DCH experiments when this PRA, state-of-the-art PRA, tells me it's down in low probability, that I really shouldn't worry about, okay?

And the answer was, is that people did not feel comfortable with the 1150 answer, okay. In other words, it was the best judgment at the time, but they felt that one really needs to provide the confirmatory data that supports those judgments.

MR. SIESS: Excuse me. Have you found out what it would take to make them feel comfortable, so that you will know when you have completed your research?

MR. SHARON: I've tried.

MR. SIESS: Or are you just going to take it --

MR. SHARON: But I apologize. You know, the people that --

MR. SIESS: That's the whole question.

MR. ELTAWILA: I think we have tried there --

MR. SIESS: What do we need to know?

MR. ELTAWILA: The NUREG/CR-5423, the Mark I liner issue, is a perfect example of that, that we go through the peer review process, and then we narrow down the issue to very small number of issues that need additional information that will satisfy the majority of the researchers in this area. And then we focus our research in these, and then we go back again and see if the -- what we researched, additional research that we have done alleviates some of the concerns that they had in this area.

MR. SIESS: And what's your criterion for when you will stop?

MR. ELTAWILA: We'll stop --

MR. SIESS: When the majority of your peer reviewers say they are satisfied, or when a two-thirds majority or --

MR. ELTAWILA: Well, I think you're going to have to put a qualitative criteria like that, but I think you should look at the argument that each peer reviewer is making. And if a peer reviewer, his argument just does not wash or does not agree with the rest of the reviewers, then you have to discard that peer reviewer per se. His opinion is out of the realm of everybody else.

So I cannot tell you if it is two-thirds or one-third, but I think you have to look at the argument that each peer reviewer is putting forth to support his claim that this resolution is not adequate.

MR. SIESS: This, then, is a qualitative form of elicitation of expert opinion, and it gives a group experts to decide whether you're finished?

MR. SHARON: Yes, we asked for expert opinion on it.

MR. SIESS: And you don't -- well, when you tried to use the experts to get numbers to go somewhere in there, you could get a probability distribution, and I'm trying to visualize how you decide when you're through.

MR. KERR: You see, one of the difficulties -- and believe me, I don't think this problem is easy -- insofar as this group is capable of doing so, I believe we're trying to be constructive.

One of the difficulties of not having made some estimate beforehand, it seems to me, is that you don't know how to design the experiment in order to get the data you need, unless there is some thought given ahead of time to say: Here are the things we'll need in order to be satisfied that the problem is solved.

I don't see how you know how to design an experiment to get those data. Now you never know exactly what you're going to get from an experiment, I agree, but at least if you know what you want, you have a better way of planning something that you think will give it to you.

MR. SHARON: If I go out on a limb first -- for example, I could say I want confirmation that DCH will not produce excessive containment loads to some probability level, okay?

Now supposing after I do all my analyses, I find out that running SURTSEY and running Argonne, okay, and the like, that I can't get there, okay. There's going to be a residual uncertainty or something that is not going to give me the level of confidence that I was looking for.

Now I've gone out on a limb and I've said that, okay, say in 1991, okay. And I find out in 1992 or '93 that I cannot achieve that unless I go to, instead of tenth-scale experiment, I have to go to a full-scale, okay, and that's going to cost $300 million, okay. You know --

MR. CATTON: What's wrong with that?

MR. SHARON: Huh?

MR. CATTON: What's wrong with that?

MR. SHARON: It may not be worth it, okay.

MR. CATTON: That's right. That's it exactly.

MR. SHARON: I have said -- I mean, why should I go out on a limb first, okay, and say this is what I must have in order to be -- because that's a judgmental thing?

MR. SIESS: Why was it worth it in the first place, but not worth it now? Why was it worth it in 1991 but not in 1993?

MR. SHARON: One has to look and decide whether or not, okay, that running an experiment, okay, and the cost of running that experiment, okay, is going to reduce -- provide you with a substantial reduction, okay.

For example, I can say that, you know, suppose I want to get down to a probability of 90 percent confidence that DCH will not fail the containment when I do a calculation, okay.

I run SURTSEY. I run Argonne. And I can only get to 70 percent confidence, okay. To get that additional 20 percent, I decide I have to run a full-scale experiment that's going to cost $300 million. Is $300 million worth getting that 20 percent?

MR. SIESS: You stated a go/no-go. You may never get that 70 percent. An experiment is not going to do it. You're dealing with nature.

MR. CORRADINI: Can I ask a different question, Brian?

I think I know what they're asking, but I don't think it's coming through exactly.

Let's take DCH as the example. I had two examples actually. You used one before, and Tom asked the question about it, about core concrete interactions, but let's just take DCH.

I guess the question that Bill is asking, and maybe he'll correct me if I'm misinterpreting, is that let's say, while I know that the Argonne experiments and the Sandia experiments are continuing, and they're looking at the effective scale, is there a technical question that you're hoping to -- not a probability question or a confidence question, but a technical question -- that is, is the expectation at the two scales that you're going to get very little material -- let's just take this as an example -- that you're going to get very little material transport up into the upper region in both experiments, and that's going to be shown at both scales under the appropriate conditions?

If that's the objective of the experiment, and then the experiment is run and you get that, then that tells you something about how -- at least if you've planned it properly, that tells you something that gives you confidence on how you planned the experiment and the answer you're expecting.

MR. SHARON: Right.

MR. CORRADINI: If you don't get that, then it tells you -- and you're way off on the other side -- then it tells you something else, okay, that either you want to do another experiment to understand why the physics you thought was really in control is not in control, all right, or that you just cannot make this case, and you've got to back up and perhaps be more conservative in terms of the regulation.

Am I making sense of what you're saying, Bill?

MR. KERR: Yes.

MR. CORRADINI: It's literally saying -- I put it a different way; that is, if I'm going to do an experiment or calculation, I should always have ahead of time a hypothesis of what the outcome is going to be.

MR. ELTAWILA: Yes, we agree with that.

MR. CORRADINI: Okay, all right.

MR. SHARON: That's the whole reason that we stopped all that experimentation for two years.

MR. CORRADINI: Okay. Well, I think that's all Bill's question was.

MR. SHARON: Yes, so we could get the scaling analysis to find out that when I ran these two experiments at the different scales, whether I was actually going to get results, okay, that could be related to a large reactor, whether I would see the phenomena that was right and the like.

MR. CORRADINI: I purposely picked DCH, because I think that was the whole point of the holdup for so long. Maybe when you get to that later today, it would be useful to use that as the example of showing what you expected the result to be or the technical question that went into the preparing of the experiments, and I know there are some partial results.

I think that would be a good way of explaining the example -- or by example what Bill is asking.

Now can I change the subject and go back to the core concrete reaction?

Tom asked before, you've declared victory on that, okay. I was curious -- and this is back to Bill's question -- maybe it would be a useful exercise for somebody in the Staff to ask the question now, if you've declared victory, why is everybody happy with the victory?

What are the attributes about the knowledge I have about core concrete interactions that make me satisfied that I've done enough? Is there something there on a qualitative generic way that I learn from what's been done in core concrete reactions that I can apply to other of the severe accident issues?

MR. SHARON: Mike, you know, if I thought I would get something useful out of that, I might do it, okay.

Let me give you another example that's not in severe accidents, okay -- thermal hydraulics, okay.

We revised Appendix K, all right. There were a lot of people on the Staff, okay, that didn't want to mess with Appendix K. They liked it the way it was, okay.

There were also a lot of people, and there still are a lot of people, okay, that felt that the thermal hydraulic codes, okay, should have -- still have large assessment and development programs going on with them now.

That's fine, except it's very expensive, and I apologize, but, you know, my job is to determine where best to spend the taxpayers' dollar and where we're going to get the biggest bang for our bucks.

And at some point, one has to bring the whole cost issue into the decisions, okay. I was not seeing great strides being made in thermal hydraulic performance, in the calculation ability, okay. Yeah, maybe I get the reflood temperature within 25 degrees closer or something or the time of peak clad temperature within 50 seconds better or something. But from a risk standpoint, did that matter?

Well, there's a lot of people that, you know -- I'll be quite honest with you -- I think thermal hydraulics is a very good academic exercise for them, and they'd like to work on it until they could get that peak clad temperature down to four decimal places.

But from my point of view, okay, I had to put the money where I think the risk was, okay, and where I could get the greatest benefit in reducing the perceived risk of nuclear power.

So that's why we said, okay, thermal hydraulic codes, okay, we've done enough, okay; we've revised the ECCS rule and everything. Everybody seems to be satisfied with it. There was no criteria that said we reached some level, okay.

As a matter of fact, what we did, if you remember, is we put a 95 percent probability or confidence level in terms of the statistical approach. You know, if we had a lot of data, I could do that with severe accidents. If there was a severe accident rule that was akin to Appendix K, we could do the same thing -- best estimate, 95 percent level, okay. And they provide the database that supports what their calculated 95 percent level is.

But the database isn't there.

MR. KERR: I'm going to suggest, in order to keep somewhere nearly on schedule, that we at least defer further discussion of this for now and that you go on with your presentation.

MR. SHARON: Sure. Just to conclude, if there was a way to do it that I think would work, I would be more than willing to do it, but I don't feel comfortable. I think each area has to be looked at as a unique area on a case-by-case basis.

[Slide.]

MR. SHARON: This is a cartoon. Don't take it too seriously, please.

All I was trying to show here was, at least in my opinion and other people on the Staff, to give you a general idea of the relative amount experimental data that exists for the relative phenomena and what we perceive as the relative uncertainty in the processes that govern those phenomena.

And all I'm trying to show here is that in the early phases of a severe accident when the core is starting to uncover, you get boiloff and the like. We've got a lot of data. The codes do a pretty good job. Not a lot of question there, okay.

On the early phase, okay, where the core starts to melt, the cladding starts to run down and so forth and you oxidize, we've run a lot of different tests, okay. There's still some uncertainty, but it certainly is not as much as in the latter phases, okay.

Once you start getting into the later phases of core melt, there is not a lot of data, okay. On the molten pool circulation crucible failure, the only thing we have are the MP tests, and they're not going to get anything on the circulation problem at all.

The FARO facility will get us some information on the ceramic pool relocation to the lower head. We've got some work looking at the TMI results on lower head failure. But the bottom line is that once you get on late phase, there is not a lot of data that's available.

[Slide.]

MR. SHARON: And so when one is trying to come up with the amount of material that is ultimately going to get into the containment and produce the loads, there are large uncertainties.

As I said, the late phase -- and these are all some of the phenomena associated with it -- it still has a lot of uncertainties in it.

There's a lot of areas that we do know, okay, but the real question -- and this may get to what Dr. Kerr was talking about -- it's difficult to construct and run relevant experiments. I'm going under the assumption that this is information that we would like to get, that we would like to reduce the uncertainty.

We had previously defined two experiments, the Ex Reactor experiments for the BWR and the MP experiments, which would be applicable maybe to both, if one gets crucible formation in a BWR.

MR. CATTON: What is MP?

MR. SHARON: Melt progression. What it is, is it's a small -- you'll hear more about it, but it's basically a mockup of a metallic crust with an oxide debris bed above it, fuel rod stubs. You put it in the ACRR reactor at Sandia, and they zonk it and watch it interact with the crust.

I had a lot of questions on both these experiments, principally with the MP experiments, on whether or not they were going to get us information that could be applied and would shed light on the actual phenomena we expect in real reactors. And before we went into these experiments, I sent out letters to peer reviewers and asked them to take a hard look at these experiments and tell me whether they thought these were the right experiments to run, whether they would get us information that would help us, or whether when we were all done we were still going to have big question marks.

I also, before we went forward, I had asked Bob Wright and John Kelly from Sandia, Bob Wright on the Staff, they put together a draft of an in-vessel melt progression research program. My biggest question was, is that when I go back and I look at all of these phenomena that are associated with in-vessel, okay, obviously I want to get to a point where I feel confident I can calculate how much material gets out off the vessel and its temperature and composition and the like.

What I did not see was an integral test program that had experimental tests defined that would get me to this point. I had some experiments here, for example, but I didn't have that linkage that told me, okay, what's the next step, how do I get to the relocation.

The MP experiments have horizontal crust. I asked the question: Well, gee, if the crust fails on the incline, I said, does that make any difference? Some people think it does. So the question is: Should we run tests with an incline crust?

Do I have an integrated experimental program that ultimately will get me the answer, or am I going to do a bunch of experiments and find out that I'm still missing a key piece that's going to have a residual, large uncertainty?

[Slide.]

MR. SHARON: Now for current plants, certain accident management strategies have some large uncertainties associated with them, and we have some experiments that are focused on trying to reduce these uncertainties.

The two that we're very much concerned about is quenching of a damaged or molten core, the question of what happens if I add water, if I restore electric power or I get my HPI pumps back on, and I start pumping cold water back into the system where I've got a molten core that's either in the vessel, or it's gone through the vessel, and it's down at the bottom of the cavity, how does that water interact with the molten core?

We're not trying to predict whether or not I cool this stuff or not, but we're looking at the symptoms, for one. What is an operator going to see? Will there be misleading symptoms that an operator takes the wrong action?

And then debris bed coolability, and I spoke about why we're doing that.

Another thing we're doing now is, we're looking --

MR. CATTON: Brian, when you say "debris bed", do you mean both molten and particulate?

MR. SHARON: Yes.

MR. CATTON: All right.

MR. SHARON: The other thing we're doing now is, we're looking for the advanced passive reactors, AP600 and the SBWR. We're trying to look at the applicability of the current severe accident database to these plants to see whether or not, if we use our codes to predict severe accidents in these designs, whether we think there are some unique features in those plants that would make our current database not applicable and whether additional experiments need to be run.

MR. KERR: Brian, in connection with future plants, if I understand the current status of things, they will be required to conduct a PRA.

MR. SHARON: Yes.

MR. KERR: But I haven't seen anywhere a description of how the PRA will be used in the decision-making process. That is, how important, for example, are uncertainties? What uncertainties are acceptable?

I don't see -- and this is not criticism of your program at all -- I don't see how you can plan a very meaningful research program until somebody else has answered those questions. It must be difficult.

MR. SHARON: Well, there are certain severe accident sequences that I think one can look at and say that we can analyze this design, and we don't really have to identify exactly how we got there, okay.

MR. KERR: But unless you know what is going to finally be used to make a decision, I don't see how you know what needs to be analyzed and what doesn't need to be analyzed, unless NRR or somebody has decided what they're going to do with the PRA, and I use the PRA as a surrogate for severe accidents.

How can you plan a meaningful severe accident research program?

MR. SHARON: Well, with what we've done right now, I mean --

MR. KERR: That's what bothers me.

MR. SHARON: Current plants are not required to design to a severe accident.

MR. KERR: I am completely aware of this.

MR. SHARON: Right.

MR. KERR: And the new plants may not be either, unless some decisions are made.

MR. SHARON: Right.

MR. KERR: And not by you. You're not the guy to make these decisions.

MR. SHARON: NRR will have to.

MR. KERR: But are you putting pressure on NRR to say: Look, guys, I can't plan a meaningful research program until you tell me what you're going to do about severe accidents?

MR. SHARON: Yes. We have certainly tried to work with them in the sense of asking them to help us define the research program. But right now, we are not planning -- at this time, we are not planning any major experiments, for example, okay, that need to be done for those designs.

Right now, our efforts are strictly analytical.

MR. KERR: But, I mean, how do you know what to analyze even, if you don't know what sort of --

MR. SHARON: We know that if a severe accident occurs in AP600, for example, it will most likely generate a lot of non-condensable gas in the containment.

MR. KERR: But who and what process -- well, I shouldn't be asking you this, because this is not your job, to decide these things. It's just that I can't imagine being in the position that you're in, where you're trying to do research that will help a regulatory process which nobody yet has defined.

MR. ELTAWILA: There was -- Brian, one of the design objectives, for example, of the ALWR, they have certain design objectives like, for example, if there is core on the floor, they provide a floor flow --

MR KERR: These are criteria that have been defined by the industry. I haven't seen any NRC approval or disapproval of these.

MR. ELTAWILA: No. That's what our research program is going to be able to, when we finish development, for example, of --

MR. KERR: Has the NRC accepted these as --

MR. ELTAWILA: No, they are not accepted. For example, the debris coolability, as you recall from SECY-90-016, we're saying that it's a reasonable criteria, but it needs to be confirmed. And the industry is working on confirming it.

MR. KERR: Why confirm it if you're not going to use it?

MR. SHARON: Well, the Staff is in the process of coming up with a revision to Part 50, okay, in which the source term information that was previously in Part 100 is going to be codified in Part 50, and there will be severe accident requirements in Part 50 that need to be considered in the design process.

That is being done in the Office of Research, but it is not very far along yet.

MR. KERR: But do you think you know enough about what it's going to finally be, so that you can plan research program?

MR. SHARON: Well, like I said, the only research program we are doing right now with regard to severe accidents on these advanced designs is analytical.

MR. KERR: But an analytical program is important and costs money.

MR. SHARON: Yes. We don't know.

MR. KERR: And unless you know where to aim it, the direction in which to aim it --

MR. SHARON: If I could, you'll hear about this, but --

MR. BECKJORD: Isn't it a fact that the severe accident issues for the advanced reactors, we've concluded they are not new issues in addition to those on the operating reactors?

MR. SHARON: Yes. We have not seen any new --

MR. BECKJORD: There are no new issues.

MR. SHARON: -- any new phenomena or issues.

MR. KERR: Well, but that doesn't make me feel very comfortable, because the Staff hasn't dealt with severe accident issues on existing plants. I mean, there's nowhere in the regulations, other than hydrogen --

MR. SHARON: No. It was dealt with by the Commission. It's called an IPE.

MR. KERR: But the IPEs have not -- I mean, nobody has yet established an acceptance criterion or set of criteria that I know of for the results of IPEs.

MR. SHARON: The Staff said the closure of the severe accident issue for operating plants will take place when each plant has conducted an IPE and implemented the fixes that it feels are necessary, put in place an accident management program, and that's it. The Commission has agreed that that is closure on severe accidents for operating plants. Nothing more needs to be done.

MR. CATTON: So why research? Where does research enter in?

MR. BECKJORD: It seems to me that it's been stated, and I think it is recorded in the documents on the IPE, that its purpose is to look for vulnerabilities, and in particular outliers: for example, core damage frequencies that would exceed substantially something of the order of 10 to the -4.

And then when those are identified, it's expected that something will be done about them and that fixes will be pursued under the backfit rule. And that, I think, is the criteria for the completion of the IPE.

If it appears that a fix is cost-effective under the backfit rule, then it would be pursued. If it is not, it will be dropped. I think that's the criteria.

I would just like to comment on this and go back to your question, because I think this is a very important one.

The severe accident plan, the severe accident research plan, is for closure, and it's defined in terms of issues. And Brian has started to set forth these issues here. And they are the ones that arise in the evaluation of severe accidents in particular, as he said, those that -- the accidents that can or may lead to containment failure, especially early containment failure, because those have much more importance in risk base.

In some of these, as Brian has said, the uncertainties are large. Hydrogen generation was mentioned. Another one is the mode of pressure vessel failure in a high-pressure core meltdown sequence.

The objective of the research program is to provide the information that will resolve these issues.

Now if the issue can be resolved convincingly, fine. There are cases, and Brian has touched on this, where there are large uncertainties. It seems to me that the approach in the case of large uncertainties is to attempt to define an experiment which can be performed at a cost which we can afford and to do that experiment.

That's what we're doing in the case of the direct containment heating now, and it seems to me that there has been a lot of progress in that regard in the last year, both in the scaling methodology, which you're going to hear about, and in terms of the experimental setup, and those experiments are underway again.

And it's my understanding -- do you agree, that we're going to wind that up this year in '92?

MR. SHARON: I have scheduled a draft resolution report to be issued around December of 1992.

MR. BECKJORD: Now in a case where you may not be reduce uncertainties to a desired amount, then finally I think you look for bounding values in these severe accident phenomena, and you use those in evaluating containment loads and plant risks.

And it seems to me that when the safety goals are convincingly met and when the cost, if you can meet it with a definitive experiment, fine; if you're still left with uncertainties, you use bounding values, and you do the research, and when the cost of the research exceeds the value of the experimental results in terms of evaluating these uncertainties, I think that's the time when you stop. It is not when we run out of money; I can assure you that. It's when the value is less than the cost of the research.

MR. SHARON: Okay. I'll try and hurry along here to get back on schedule.

As I said, we have efforts underway to look at the applicability of the current severe accident database, and code modifications are underway, as I was saying.

Principally, we need to model -- we need to have a containment model that can accommodate a water film flowing on the outside and also to be able to treat condensation on the inner surface of a steel shell, and we know, as I said, without defining what the sequence is or the probability, we know that severe accidents will generate most likely a lot of non-condensable gas, so we need to have models in our containment codes that can deal with the non-condensable gas buildup that will most likely take place on the condensing surface, so that we can get the heat transfer rates through the containment to see how well the passive containment can remove the heat load on the containment in a severe accident.

We're making these modifications right now with both the CONTAIN and the COMMIX codes.

[Slide.]

MR. SHARON: The other areas we're looking at, we have research going on at Brookhaven on high-temperature, high-speed hydrogen combustion. This is being performed cooperatively with the Japanese, with NUPEC people. That program is just starting to get underway probably in this coming year.

We have work going on to better characterize fuel coolant interactions. If you remember, the Steam Explosion Review Group recommended that we conduct experiments at intermediate scales, around 100 kilograms of melt, to characterize the fuel coolant interaction mixing and the explosion yield.

And the fission product work that supports source term revision is essentially finished. This is not "eventually"; it should be "essentially" finished. Our remaining work right now is confirmatory, and it is mostly in support of the PHEBUS fission product program, which is being carried out at the PHEBUS reactor in France.

[Slide.]

MR. SHARON: My last slide is -- I just want to walk through budget real quick. These are the major activity areas that we're pursuing. This is the funding that we've projected for this fiscal year, as well as the next two fiscal years.

You can see that the core melt progression work is increasing slightly, as is the core coolant interaction work.

Fission product behavior, this shows it going up, but it's fairly constant, and this is the level of support that we agreed with the French with regard to the PHEBUS program.

Debris coolability, which I also said was an issue, we would be spending a fair amount next year, and then it will start to come down.

As you see, direct containment heating, we very much hope to pretty much close that out within the next fiscal year.

The hydrogen work, as I said, that's just starting up right now with the Japanese, and it peaks next fiscal year, and then it would start to come down, I think.

And the code work is holding fairly much constant in here.

You'll note that the one place where we are going to take a big jump is in the advanced light water reactor work. Right now, we only have about $1.1 million, and we're projecting increasing that to about $5.5 million in the next two fiscal years.

MR. KERR: Is it reasonable for me to assume that the part of that that isn't labeled ALWR is aimed at existing reactors?

MR. SHARON: It's applicable to both existing reactors and ALWR.

MR. KERR: Okay.

MR. SHARON: Obviously much of this information will be applicable to the ALWR either directly or in an indirect manner.

Debris coolability, for example, we're not proposing to run any tests, any wet core tests, that produce debris beds that are totally typical of an ALWR with regard to, say, depth of the debris and the composition. But obviously the information that we get from the wet core tests that we're doing now, we can certainly relate to the ALWR perhaps in our model development.

MR. KERR: Okay. Let me put the question a different way. What fraction of that th t's not labeled ALWR is aimed at existing reactors?

MR. SHARON: Oh. All of this, then.

MR. KERR: Okay.

MR. SHARON: It's all existing reactors. What I'm saying, though, is that the information that comes out of this, there's a lot of indirect information that can be -- information that can be indirectly applied to the ALWR.

MR. KERR: No, I understand. But you would find it maybe not necessary, but desirable, to do that research, even if one weren't talking about advanced reactors?

MR. SHARON: Yes.

MR. KERR: Thank you.

MR. WARD: Brian, a few minutes ago, I think Erik said that you've concluded that the severe accident issues and the severe accident research needs for the passive plants or the ALWRs really aren't a different set of issues or needs than what you have for existing plants.

So I guess I'm trying to figure out what the $5.5 million is. What separate program is going to be carried out there?

MR. SHARON: Okay. This is the amount that was put in the budget, okay. When we project in the outyears, okay, many times we're not really sure what experiments we're going to need. NRR could send us over a letter tomorrow and say: We need a certain experiment in order to understand better the ALWR.

Right now, this is --

MR. WARD: Okay. So this is sort of a contingency allowance in case some new issue does arise?

MR. SHARON: Yes. Well, let me give you an example. We most likely may want to purchase some experiment time at the Westinghouse one-eighth scale containment test facility that's currently being built.

That facility is -- I think essentially it's constructed now, and I think they're just putting in the instrumentation and the like.

My guess is they would start their testing in '92, maybe in the latter part of this year. It's quite possible that once they submit their test matrix to the NRC, and we can see what testing they propose, there may be additional tests we would like to have run for our own purposes.

At that point, we would define what additional tests we would like and enter into contract negotiations with Westinghouse to perform additional containment tests.

I can't tell you what they are right now. I can't tell you when exactly we would propose to run them. But we have a pretty good feeling that we would like to have some of our own tests somewhere in the future.

MR. CATTON: But when you do that, shouldn't they pay for it? If this is --

MR. KERR: I would say that I'm going to rule that question out of order. It's an important question, but I don't think we have time for it in this forum.

MR. BECKJORD: I think those are containment tests.

MR. SHARON: Yes.

MR. BECKJORD: It's not new severe accident phenomena.

MR. SHARON: No.

MR. BECKJORD: It has to do with temperature distributions and containment structure and that type of thing; isn't that right?

MR. SHARON: Yes.

MR. WARD: So that really isn't in this program. Or it doesn't sound like a severe accident thing to me.

MR. SIESS: Did I hear that containment is not severe accident?

MR. SHARON: No. We may -- like I said, we may wish to ask them to run some tests. For example, I don't know where NRR is going to come out on severe accidents yet and what they will require of Westinghouse. However, we may --

MR. WARD: Okay. This is a contingency allowance.

MR. SHARON: Yes.

MR. WARD: That's all.

MR. SHARON: It's a contingency allowance.

MR. WARD: That's a good enough answer. Okay.

MR. ELTAWILA: I would like to make a comment, Professor Kerr, please.

A few minutes ago, you asked why do severe accident research on ALWR if you are not going to use it, and I'd just like to refer you to the May 17, 1991, letter of ACRS.

MR. KERR: If I asked that question, let me reword it.

What I meant to say was how do you design it if you aren't certain how it's going to be used, and I did that in the context of, at least, from my point of view, I don't think the Commission has annunciated a clear-cut policy of how it's going to deal with the severe accident issue for advanced plants.

If you know where one is, I would very much like to see it.

MR. ELTAWILA: That's a policy issue, then.

MR. KERR: Further questions of Mr. Sheron?

MR. WARD: I think Farouk was about to say something interesting.

MR. ELTAWILA: The ACRS proposed a criteria to accommodate severe accident containment design.

If the staff has to support that criteria, if the NRC decided to accept the recommendation of the ACRS, and we have to support this criteria and evaluate licensee submittal, we have to have our own analytical tool to be able to do this work, and that's why we are going into that direction right now.

MR. KERR: I apologize to Mr. Ward and to Farouk. If there is evidence that the ACRS is being listened to, I don't want to suppress it.

MR. ELTAWILA: We always listen to you.

MR. WARD: Okay. I thought you were going to say something like that.

That certainly is an approach. That's one way to focus, to identify severe accident research needs and to focus the purpose for the program, but other than you just saying that, I don't see any evidence of interest in taking an approach along that line.

I mean I'm waiting. Over the next day-and-a-half I may get all excited about something, but I don't see any indications of it yet.

MR. SHERON: Just one other point for your information, Professor Kerr:

We had a review earlier this year with the Nuclear Safety Research Review Committee on Severe Accidents, and essentially the same point came up in that review that you and several others have made here about closure.

As a consequence of that, it was clear that we should undertake to revise the severe accident research program plan, and that is underway now, and I expect that we will have a draft on that in by the end of this year, by the end of December, and it's my expectation that that is going to set forth very clearly the closure points on these issues.

I expect it will take somewhat longer than that to have that plant approved, but we do plan to have a draft by the end of this year.

MR. KERR: Thank you.

MR. SHERON: Just to answer Dave's question, we are reviewing the letter that you sent us. We think there's a lot of good ideas in there, a lot of good suggestions. We have had a number of meetings.

MR. SIESS: What letter?

MR. WARD: The containment criteria letter.

MR. SHERON: The May 17th containment letter.

MR. SIESS: We didn't send that to you, did we? You said the letter we sent you.

MR. SHERON: The letter to Chairman Carr.

MR. SIESS: Okay.

MR. SHERON: I'm saying that the staff has been discussing and reviewing it and interacting on it very actively. So, I just want to let you know that we think that there's a lot of good suggestions in there and a lot of good points, and it's a tough issue, as you know, and it's not something that we can deal with very lightly.

MR. SIESS: Is NRR looking at it, too?

MR. SHERON: Yes. When I said we're interacting, we've had a number of meetings with the NRR staff on it with regard to what areas that we think we can agree with, maybe some areas that we think are good ideas.

We have some of our own suggestions on different ways to treat it and the like. But please understand that we are pursuing it, and it's not being ignored or anything.

MR. WARD: Okay.

MR. SHERON: Are there any other questions?

MR. WARD: Well, Bill, one comment maybe to you or to -- Erik just said a new severe accident program plan is coming out in two months. Why are we having this meeting today?

MR. KERR: Because we didn't know a new severe accident research plan was coming out in two months.

MR. WARD: Okay.

Is that going to be substantially different from what we're hearing today?

MR. SHERON: No. The information you're hearing here, namely that we have pretty much wrapped up the Mark I work, we have a plan in place to wrap up the DCH work, core concrete work is finished, and where our new emphasis is going to be placed.

That's what you're hearing now. You're sort of getting a preview of what's going to be in this revised plan.

MR. WARD: Okay.

MR. KERR: Further questions of Mr. Sheron?

[No response.]

MR. KERR: Thank you, Mr. Sheron.

Mr. Eltawila, it is now 9:15, according to my agenda.

[Slide.]

MR. ELTAWILA: Good morning. My name is Farouk Eltawila. I am the Chief of the Accident Evaluation Branch.

I'd like to update you on the Mark I issue, but before we start, I'd like to indicate that we have a lot of our contractors sitting in the back here. So, please, if there is any question asked and you can help, please just stand up and provide an answer.

[Slide.]

MR. ELTAWILA: The Mark 1 failure issue, I am just going to go through the issue and the process that we went through before, and I can go ahead and update of what happened since the workshop that we had at Harpers Ferry.

The issue has been with us for a long time. I think it started in 1985, when the Containment Load Working Group identified this issue as an important issue, after some work was done for IAEA, and it's the issue, if you look at the Mark I configuration, that if you have a core meltdown and vessel breach, the corium can, when it hits the drywell floor, it can spread through a door in the drywell and attack the stainless steel liner shell, which is the containment pressure boundary.

MR. KERR: Excuse me, Mr. Eltawila.

Is there anyone here on the Subcommittee that doesn't understand what the melt liner issue is?

[No response.]

MR. KERR: Why don't you assume that we understand the issue and go on?

MR. ELTAWILA: Okay. You want me just to go to the update of NUREG 1150 since the Harpers Ferry workshop?

MR. KERR: Yes, I think so.

MR. ELTAWILA: Okay.

[Slide.]

MR. ELTAWILA: I think all of you have heard Professor Theofanous' approach that was documented in NUREG/CR-5423, and some of the important observations of that NUREG/CR was that -- on page eight, tab two, of the handouts that you have -- that the melt superheats are low and short-lived due to interactions of the corium as it leaves the vessel, interacts with the atmosphere, going through some of its superheat.

While it's spread on the drywell floor, it will lose some of the superheat, too. So, the conservative assumptions that were used in previous analysis is unrealistic.

The heat transfer problem to the liner itself, it's two dimension, and most of the analysis done in the past was one-dimensional; they assume that the liner is vertical.

We know that the liner is at a 45-degree angle and that there will be a reduction in heat transfer to the liner.

The amount of melt that is coming out of the vessel, although there is uncertainty, it can be bounded by some of the approach that was presented in the analysis.

Some credit -- we did not take credit, for example, that if you have water on the floor, that the interaction between the water and melt will quench the melt and will reduce the superheat significantly.

One thing that -- although it's a judgement call right now -- is that liner failure by itself can be localized, and once you breach that, if you breach it, it can reseal itself, and fission product will not be transported to the atmosphere.

MR. KERR: Excuse me. What is meant by the "liner failure can be localized"? Does that mean it will be naturally?

MR. ELTAWILA: No, no. If it happened, we know that this corium, that once it loses its heat very fast and form a solid crust, and you can seal that -- if a failure occurs, it can be resealed. There is a finite probability, very small, though, that the liner can fail.

MR. KERR: No. I didn't make my question clear.

When it says "can be," does that indicate that the operators have to do something or that it is likely that, in the course of the event, it will be localized?

MR. ELTAWILA: In the course of the event, it is going to be localized.

MR. KERR: Okay.

MR. ELTAWILA: The operator is not going to control how the corium is going to spread of anything like that.

[Slide.]

MR. ELTAWILA: The review of the NUREG/CR-5423, we had issued a report in draft form.

So, a list of the reviewers, you will have in on viewgraph 10, and you can see that they are encompassing a lot of people that have done work in this area, including some of the people that were involved in the NUREG-1150 study.

We asked the national laboratories directly involved in the severe accident research program to review the reports and make comments on it. We distributed the report to NRR, AEOD, and other research divisions, and we received comment from them.

We received comment from all those people, attached them to the NUREG, and we held the Harpers Ferry workshop in July of 1990.

After all the discussion, a day-and-a-half at Harpers Ferry, there was a strong support that the methodology provided in NUREG/CR-5423 is appropriate framework to try to resolve the issue of the Mark I.

However, they identified four residual issues that additional work needed to be done in order to probably justify the quantification that -- provided by the author.

The four issues are the liner failure criteria issue, the duration of melt superheat, the melt spreading to the liner, and initial melt release quantity and composition.

The Harpers Ferry group, peer reviewers, recommended that a smaller group be formed and address each of these issues separately. We already did that in December 1990. We had two smaller groups meeting to review the issue of liner failure criteria and time and duration of melt.

The second meeting took place in January 1991 to address the other two issues.

The follow-up work as a result of the smaller group recommendation is going on right now at Sandia and their subcontractor, ANATECH, at Argonne National Lab using the melt spread code at Oak Ridge and their subcontractor, RPI, and we plan to update the result of the -- of NUREG/CR-5423 in the fall of 1992. As indicated, we will subject it to a peer review.

[Slide.]

MR. ELTAWILA: I would like to go over each issue in a little bit more detail.

The first issue is the liner criteria. The member of the panels, as you can see here, are people that have done work in this area, and we're confident that they understand the problem.

The concern that was expressed at Harpers Ferry and was reiterated here by this group is that the author of NUREG/CR-5423 used a very high melting temperature or used the melting temperature as the failure criteria for failing the containment, and the concern was expressed that you can have creep failure or you can lose the strength of the containment even before you reach the 1,500 degrees, maybe as low as 900 degrees centigrade.

[Slide.]

MR. ELTAWILA: When we had the workshop, the author provided some finite element analysis which showed that losing the strength of the liner itself is not a major concern, because you might lose strength in the area adjacent to the high concentration of heat.

However, the liner itself will be supported by the concrete shield wall behind it, and the other part of the liner itself will be at colder surface and will be providing some support to that.

The peer reviewers agreed with his recommendation here, but they still identified the need for a detailed finite element analysis to assess the issue of creep rupture, that you heat the drywell uniformly and - the elevated pressure during that accident scenario it can cause a creep failure of the containment.

We contracted with Sandia to do this finite element analysis, who in turn are doing the analysis at ANATECH, and they are using the high temperature creep data that's developed at INEL recently.

We expect this evaluation to be available in the next couple of months, two or three months.

MR. WARD: What is going to be different here? I guess, what's going to be different there? What is the finite-element analysis going to show? That failure won't be just local or may not be just local? Is that what the issue is?

MR. ELTAWILA: No. The issue is whether the failure will occur or not, and if it occurs, where it's going to be occurring, if it's going to be local or if it's going to be a global failure of the containment resulting from high pressure and softening of the material of the shell itself.

The authored analysis indicates that the shell will not fail, and we want to confirm that conclusion, if that is the case or not, even though with the new identified failure mode, the creep failure of the containment -- for example, you have at the junction between the concrete and the liner itself, you have a complete junction, and the structure people say that there would be a concentration of stresses in this area that can lead to a failure. This issue was not identified by the author of the NUREG, and they want to look at it in detail.

MR. WARD: Okay. So there is a possibility there could be a more general failure, and this will help confirm the original assumption that there is not.

MR. ELTAWILA: That's correct.

MR. CATTON: That impacts on the hardened vent, because if the liner fails, the vent doesn't do much good.

MR. WARD: Okay.

MR. CATTON: At least that's my view.

MR. WARD: I mean, if the liner fails in a large failure.

MR. CATTON: Right, because that's a direct path to outside.

MR. WARD: Yes.

MR. CATTON: Whereas if it doesn't fail, then it goes through the downcomer, and the pool can scavenge out all the nasty stuff.

MR. KERR: Does that take cares of your question, Mr. Ward?

MR. WARD: Yes, it does. Thank you.

[Slide.]

MR. ELTAWILA: The second working group, one of the concerns that was identified during the Harpers Ferry is that the authors used their own core concrete interaction model and did not use the more recent state of the art CORCON-Mod 3 that's sponsored by NRC at Sandia, and they recommended that we benchmark the analysis that is recommended in the NUREG against the CORCON-Mod 3.

So we had the panel that consisted of Dr. Bradley from Sandia and Dr. Dana Powers. Both of them are partly involved in the CORCON-Mod 3. In addition, we have Bruce Spencer and Bob Hendley from FAI and ANL.

This analysis has been completed already, and it was available to the peer reviewer when we had the workshop in December. The Sandia analysis indicated that the time of duration of the Superheat that's used in the NUREG report is much lower than what is estimated. So he used a longer time duration for Superheat. The actual number is about one-third of what -- CORCON-Mod 3 value was about one-third.

You know that one of the important parameters for the thermal loading on the containment is the duration of the Superheat, the degree of the Superheat, and the high of the melt. So there is a conservatism here in that, and that's based on the state of the art CORCON-Mod 3.

MR. CATTON: Has CORCON-Mod 3 been validated for shallow pools?

MR. ELTAWILA: No, it has not been validated. Dana, do you want --

MR. POWERS: My name is Dana Powers. I am from Sandia National Laboratories. The CORCON-Mod 3 has certainly been applied against tests that are as shallow as the melts that are being used here, which, in Dr. Theofanous' analysis, he runs some very shallow melts, but up to about foot deep, and CORCON has been used to compare very well against tests with melts as shallow as 14 centimeters.

MR. CATTON: Okay. Thank you.

MR. KERR: Farouk, I didn't understand your statement earlier and the conclusion. I thought you said earlier that the CORCON showed that the Superheat was greater than that which would be predicted by Dr. Theofanous' analysis. I must have misunderstood you.

MR. ELTAWILA: The Superheat duration --

MR. KERR: Yes.

MR. ELTAWILA: -- that the liner would be loaded -- when the corium settled against the liner, there is a duration of Superheat that once that duration expired, the liner will no longer be loaded thermally.

MR. KERR: Yes.

MR. ELTAWILA: So the CORCON-Mod 3 analysis indicated that this duration is shorter than --

MR. KERR: Okay.

MR. ELTAWILA: CORCON-Mod 3 than used in the NUREG analysis.

MR. KERR: Thank you.

MR. CORRADINI: Superheat is defined her as T of the pool minus T Liquidus?

MR. ELTAWILA: Minus T Liquidus, yes.

MR. CORRADINI: Why Liquidus?

MR. ELTAWILA: That was along the discussion -- because you have to have the melt -- the corium in a melting stage in order for it to attack the liner, you know.

MR. CORRADINI: So any solid formation at all would --

MR. ELTAWILA: Any solid formation would -- yes. And I think this discussion took place during the workshop, and they finally agreed that the Liquidus temperature is the correct temperature to use.

[Slide.]

MR. ELTAWILA: The conclusion and recommendation of that working group is that the time duration of the Superheat in the analysis of the author is conservative, and again, the driving force for the heat transfer liner is the Liquidus temperature, as I just mentioned to Dr. Corradini.

MR. KRESS: Farouk?

MR. ELTAWILA: Yes.

MR. KRESS: I'd like to turn to Dr. Corradini's question on the Superheat. It seems to me like you have two choices: to use the Liquidus or the solidus, or somewhere in between. We are wanting to confirm that you really meant the Liquidus was used in the definition as opposed to the solidus.

MR. ELTAWILA: That's correct. The Liquidus is the one used in the definition here. Theo, is that correct? Yes. He nodded his head that that is correct, yes.

[Slide.]

MR. ELTAWILA: The Working Group 3 was formed to address the issue of corium spreading underwater, and that is a concern that was expressed by Professor Catton here, and the author has done experiments at his facility that indicated that Froude scaling is the correct scaling, but the concern that Professor Catton expressed is that there is a potential that you have underwater lava flow behavior that can focus the corium to the -- towards the liner, and you will have -- almost revert to a dry condition.

This is one of the most difficult issues that the group was not able, really, to come to grips with, but let's see what is the conclusion here.

[Slide.]

MR. ELTAWILA: The best way to handle Professor Catton's concern is to deal with the issue using the MELTSPREAD code and see if we -- this is a code that's developed by Argonne National Laboratory for calculating a spread on a concrete floor.

MR. KERR: Excuse me. I want to wait until Professor Catton can listen to this.

Okay. Proceed, please.

MR. ELTAWILA: We'd like to see what this analysis is going to show.

MR. CATTON: What is the MELTSPREAD code?

MR. ELTAWILA: The MELTSPREAD code is a one-dimensional code based on the energy balance between the corium, and you have the concrete, and that you have to specify a certain angle for the corium to spread through, and it gives you different spreading behavior, based on if you have water.

It just calculates the energy balance and tell you if you form a crust or don't form a crust, how far it's going to reach.

MR. CATTON: Crust formation is an energy balance at the coolant/melt interface, not over the whole thing.

MR. ELTAWILA: Theo, do you want to come forward, so you can hear this question, and respond to them, please?

MR. THEOFANOUS: This is Theo Theofanous.

It's not enough for me to speak about 5423; I have to speak about MELTSPREAD, also?

MR. CATTON: I didn't ask you to.

MR. THEOFANOUS: MELTSPREAD is a code that is developed at Argonne National Laboratory by Bruce Spencer and Mitch Farmer, and it has not been out as a full document yet. My understanding is this was under the sponsorship of EPRI.

It is my understanding that the draft document is in the hands of EPRI, and I think it is going to come out in something like a month or two.

My understanding, from what I hear, from presentations, is that this code can account for crust formation. Also, they claim that it can account for slide formation. In fact, slide formation is their mechanism for getting it to stop as it is spreading.

They claim, also, it accounts for free surface effects; in other words, it's moving with the momentum, is driven by gravity. Excuse me. The momentum is accounted for and the melt is moving by gravity, and also, it is symmetric inside the pedestal, and when it gets out of the door, then it splits into two parts, which like one-dimensional floods, are moving out around the drywell.

It has energy balances in the upper interface and the lower interface, and also has chemical reactions going on in there.

MR. CATTON: Does it actually treat the heat transfer to the interface and from the interface separately?

MR. THEOFANOUS: My understanding is yes, but that's not firsthand information, because I haven't seen the document.

MR. CATTON: Okay.

MR. KERR: Thank you.

MR. CATTON: Thank you.

MR. ELTAWILA: There was a couple of issues that are --

MR. CATTON: There is one more question, and that's how well do they know the thermal-physical properties of the melt?

MR. ELTAWILA: We have a program at Argonne that NRC is sponsoring to get the physical properties of corium melt for different -- taking that corium and concrete out of the MACE and the ACE program and analyzing them and trying to get the physical properties of that melt composition.

So, they have some idea about the melt composition that can be in a situation like the Mark I issue.

So, there is a material properties program at Argonne, sponsored by NRC, to provide all the relevant information for the core/concrete interactions.

MR. CATTON: And then some experiments to confirm all this.

MR. ELTAWILA: There are experiments to confirm that, yes.

There were a couple of concerns that were expressed in the peer reviewer meeting, that small group meeting.

MR. CATTON: NRC is [illegible]ved in these experiments, aren't they? I mean aren't you guys involved?

MR. ELTAWILA: The MACE program, ACE/MACE?

MR. CATTON: Yes.

MR. ELTAWILA: Yes, we are a partner in that program.

MR. CATTON: Is there any way to get sort of progress reports or get an idea of what's going on?

MR. ELTAWILA: Absolutely. I don't see why not. Yes.

MR. CATTON: I don't either. Could you take care of that for me?

MR. ELTAWILA: Sure. Yes. We'll find out.

Two concerns were expressed during the Working Group 3 meeting.

The first one was that they thought that there might be some high elevated like concrete steps inside the drywell for mounting of equipment and so on that could lead into a channeling of the floor into a certain direction.

So, we agreed that we're going to look at this arrangement for a couple of plants and try to run experiments similar to the one that's run at the University of California-Santa Barbara to identify if mounted obstacles or floor-mounted obstacles can lead into a different melt spreading than what we accounted for in the analysis.

The second issue that was raised during the peer review meeting, too, is that when the corium hit the floor, it can basically fly through the door and hit the liner at an elevation higher than the water level.

So you can get a hotter spot in that area that can lead into a local failure above the floor, and it was agreed that this issue can be addressed analytically, and we are going to pursue that issue.

Again, the issue of lava analogy is that we're going to deal with it with MELTSPREAD right now and try to come to grips with what is the issue and what's entailed if we need experiments or not on that issue.

[Slide.]

MR. ELTAWILA: Working Group 4 is formed to enable the -- us to determine if the initial and boundary condition that was chosen by the author represents a reasonably conservative envelope, although that was not defined by the peer reviewer at Harpers Ferry, but we decided at NRC that we should go ahead and assess to see if he reexamined the amount, the superheat of the melt and the composition of the melt to see that we are using a reasonably conservative amount.

[Slide.]

MR. ELTAWILA: The recommendation of the working group is that the Scenario I and II identified by the author represent an upper pound of what is expected to happen, so Scenario I was definitely the most limiting scenario because you get a massive release of the corium on the floor and it's spread out very rapidly into the drywell, so they agreed that this is the most limiting scenario.

There was some discussion that some analysis, for example, for PWR indicate that by radiation from the molten fuel, you can start melting some of the upper internal, so you will have a scenario that you will have a lot of oxidic melt like in Scenario I, but on top of that, you will get a lot of metallic melt which can increase the amount of melt or increase the depth, increase the degree of superheat in the melt.

So, we decided to undertake an analysis at Oak Ridge and RPI to see if that scenario; that is Item 5 for PWR, is applicable for boiling water reactors here. This program is ongoing right now, and we hope to get the results from it very soon.

[Slide.]

MR. ELTAWILA: So, in conclusions, the NUREG/CR 5423 methodology is physically based on the composition on a cause and effect basis, it's based on either test data or analytical analysis so each phenomenon or each part of the parameter that can affect the issue has been dealt with, based on either test data or computer run or hand calculation, conservative calculation.

We have included multiple scenarios and that, in our opinion, encompasses most of the scenarios that can be included in severe accidents. As the methodology was originally proposed, it's subjected to peer review. The peer reviewers make their recommendation, we go back and refine the analysis and see if our conclusion is going to be changed or not, and that's the process we're going to be undertaking in the next year.

After that, we plan to prepare a final report which will be subjected to a peer review, as Brian indicated, and definitely we'll come and brief the ACRS Subcommittee and the full ACRS and after that, if everybody is happy, we'll go and recommend it to the Commission about resolution of that issue.

[Slide.]

MR. ELTAWILA: If you will recall, in all our presentations, we ask that we provide applicability to ELWR and ALWR, and we hope that we will not have a Mark I built that it's with the liner close to the pedestal, just in case of the severe accident. I think that's one of the criteria in the EPRI requirement document; that they will not have any steel boundary close to the center of the vessel where the corium might relocate.

So, although the issue of debris coolability and core concrete interaction is applicable, in general, to ELWR and ALWR, there is no configuration similar to Mark I in these designs.

MR. KERR: Excuse me, in what sense can this give information that is useful on adding water to a degraded core? That's one of the parts of this bullet. It says information generated, and one of the things -- it says adding water to a degraded core.

MR. ELTAWILA: Are you talking about this viewgraph?

MR. KERR: Yes, the first line of the first bullet, over to the extreme right, adding water to the degraded core.

MR. ELTAWILA: Adding water to the degraded core, the --

MR. KERR: Does that mean the core on the floor?

MR. ELTAWILA: The core on the floor, yes. That's the vessel that you have --

MR. KERR: If that's what's meant --

MR. ELTAWILA: We're going to have a full presentation on adding water to a degraded core, quenchability, fuel interaction, all in one presentation, yes, tomorrow, the first thing tomorrow morning.

MR. KERR: I recognize that. I just wondering how this was applicable. You're referring to the degraded core that's already out of the vessel; is that right?

MR. ELTAWILA: That's -- I apologize for that bad terminology.

MR. KERR: I just wanted to understand it. Thank you. Mr. Catton?

MR. CATTON: In the earlier issues, is FCI included in the core recoolability part of it?

MR. ELTAWILA: In which issues?

MR. CATTON: Well, you're talking about the liner and water.

MR. ELTAWILA: No, we did not include the effect of SAF or SEI on -- or the effect of it on quenching the debris before it reaches the liner. It's a conservative assumption that we did not use.

When you have corium fall into a water pool, don't forget that we are assuming in this analysis that we will have water, one way or another either by NRC requiring them to add water or some of the scenarios that you can fill the lower head with a lot of water still in the lower head.

MR. CATTON: So your focus is narrow and only on the liner?

MR. ELTAWILA: That's correct.

MR. CATTON: If there were to be an FCI that might pop the lid off it, that's a separate issue?

MR. ELTAWILA: That's completely --

MR. CATTON: Okay.

MR. ELTAWILA: If there are no other questions --

MR. KERR: Any further questions?

[No response.]

MR. KERR: This is a scheduled break, if I remember correctly, and we will recess until 10:35 by the clock on the wall.

[Brief recess.]

MR. KERR: We are ready to proceed. And my agenda says "Completion of Core Concrete Interaction," Tinkler and Foulds. And that sure ain't Tinkler.

[Slide.]

MR. POWERS: My name is Dana Powers. And I'm here to talk about the melt-concrete interaction. And I believe I'm here talking simply because the agenda overburdened the staff in preparing all the materials, and I'll do my best to supplement their presentations on core debris-concrete interactions.

[Slide.]

MR. POWERS: Core debris-concrete interactions of course were one of the essential features of severe accidents identified in WASH-1400. I put up here a photograph of one of the very early tests of melt-concrete interaction simply because it illustrates nicely the safety issues.

You see in this experiment the intense aerosol generation which of course in the case of a reactor accident would be radioactive aerosol, intense production of flammable gases burned in the reactor, in the experiment, in the laboratory atmosphere, but could accumulate in a reactor accident. And of course, you're getting erosion of the concrete down below.

The studies of core debris-concrete interactions were in fact prompted by this generation of combustible gas which was not recognized in WASH-1400.

[Slide.]

MR. POWERS: There has been a substantial body of research conducted in the interim, both here in the United States and in Europe. And the thesis of the presentation that I am providing here is that through this research, we have now gained a sufficient understanding of core debris-concrete interactions, especially in the case where there is no water present, that we can meet most of the foreseen regulatory activities that we have, and, in fact, an extensive confirmatory database supports a set of predictive models.

[Slide.]

MR. POWERS: I have provided in the output a substantial body of what I would call background information. It has been some time since we've reviewed this program before the ACRS, but I look around and I see that many of the people that are here are acutely familiar with this core debris-concrete interactions, and I will ask how much of this background material I should go through, or should I move promptly forward into the more substantive issues of the database?

MR. KERR: I would suggest that we go to the substantive issues, and if people find themselves completely lost, they can ask questions.

MR. POWERS: We can come back. Yes.

MR. KERR: Okay.

MR. POWERS: There are several viewgraphs that simply define core debris as a two-phase mixture.

[Slide.]

MR. POWERS: Let me point out on the definition of core debris that one of the constraints one has in looking at the ex-vessel phase of the accident is, we don't have a thorough understanding of the in-vessel phase of the accident. Consequently, we do our experiments and analyses with a broad set of initial conditions. That is, we don't know exactly what kinds of core debris we're going to get out. We do know it's a two-phase mixture. How much of this mixture will consist of unoxidized zirconium spans quite a range, depending both on the reactor type and the reactor accident, and also because we are fairly uncertain about the in-vessel phase of the accident and how much oxidation will take place.

Similarly, when this core debris comes out into the reactor containment, it is an area of uncertainty. There are two major cases that we use. And I think Dr. Eltawila, in his presentation, spoke of Scenario 1 and Scenario 2.

Initial temperatures are quite uncertain. They can range, depending on the experts you speak to, from a very hot core debris, fully molten core debris, down to material that is barely molten at all, a two-phase mixture.

That is a constraint we have on the ex-vessel interactions. It is one that forces us to look at a broad range of initial conditions in defining our database.

[Slide.]

MR. POWERS: And again, I believe that safety issues are familiar to everyone.

The biggest issue provoked by melt-concrete interactions is the subject of gas generation, both the generation of combustible gas augmenting the threat of a deflagration failure of containment, non-condensable gas, which pressurizes containments over the long term. And this non-condensable gas figures prominently in thinking about advanced light water reactors, where they use natural circulation and condensation heat transfer in order to maintain containment integrity.

There is also the issue of basemat penetration that figured very prominently at the times of WASH-1400.

Now, in this country, basemat penetration is largely discounted as a major safety issue. It is certainly recognized, but not considered a prompt threat to the public.

More of interest are the collateral damage issues. Dr. Eltawila in his previous presentation certainly discussed one of those, the liner-melt interaction issue. The other issues of collateral damage, and certainly in the case of BWRs, is the failure of the pedestal that supports the reactor vessel and the possibility that you would rupture the drywell when that vessel failed.

Always, there is this issue of aerosol generation and radionuclide release into the containment and if the containment fails into the environment.

[Slide.]

MR. POWERS: We have produced in the area of core debris/concrete interactions without water present a very extensive database.

The database is of a confirmatory nature. That is, it is not a database where we have collected a huge amount of data and, based on that data, formulated a model.

Rather, the database has been generated having formulated a model or a hypothesis, then conducting experiments to see if we validate those model hypotheses and then, based on, typically, a failure to confirm, refining that hypothesis.

In generating this database, we have looked at a wide range of materials, types of concrete, test conditions, and we've been forced to do this again, because there is uncertainty in the initial conditions specified as a result of the in-vessel accident progression and also because melt/concrete interactions go on for a long time, and the type of interaction you get changes over that course of time.

You go from an interaction dominated by a very high-temperature melt that includes a lot of uranium dioxide to one that's dominated by iron and relatively low-temperature melt.

[Slide.]

MR. POWERS: I include in your handout a list of what I think are the major tests that have been done.

The history of melt-concrete interactions is there was a period of a lot of scoping tests in which we just exploring how high-temperature melts behave when they interact with concrete.

It is an exciting event when you first see it, because there is a production of a tremendous amount of combustible gas. You are working with high-temperature materials, an area that is a challenge in any aspect of science.

From those scoping tests, there have evolved test technologies, the capability to do experiments, to instrument those experiments, and now we have sets of tests that I think are fully-instrumented tests that truly illustrate the phenomena that occur, and I have listed down what I think are the major tests here, beginning with hot solid tests in which we looked how hot but still solid steel and UO2 would continue to attack the concrete, the SURC tests in which we looked at sustained molten UO2 ZRO2 and zirconium melts interacting with both limestone and siliceous concretes, and I note on several of these tests that are one-dimensional, and that's an element of test philosophy that I thought I ought to carefully explain.

One can look at a prototypic situation. Here I have somewhat schematically drawn a vessel with core debris in a reactor cavity, and one can design a test in which one says let's shrink that prototypic situation down to some smaller tractable experimental scale, and certainly, that's been done in a lot of tests, and those tests have been labeled as two-dimensional.

The difficulty one promptly gets into there is that when you shrink this geometrically down, sidewalls become the dominant influence of the melt interaction with the concrete, and you're tending to look at radial attack on the concrete more so than downward attack, whereas in the real situation, downward attack on the concrete predominates.

The other approach, and the one that's been adopted also in a large number of tests, is to say, rather than shrinking this down and trying to look at a miniature reactor cavity, let us take a region out of this and formulate a one-dimensional-type interaction in which we look exclusively at the downward, which is the dominant interaction in the real accident situation.

MR. CATTON: What's your scaling parameter?

MR. POWERS: The scaling parameter for the one-dimensional, the thing you're trying to reproduce there, Ivan? Is that what you're asking?

MR. CATTON: Yes.

MR. POWERS: We try to reproduce the power we're generating in the melt to prototypic levels, and lately, we have had more concern about the depth of the melt over the concrete as an important scaling parameter.

MR. KRESS: Dana?

MR. POWERS: Yes.

MR. KRESS: Before you leave that, an important consideration in these interactions is the heat transfer that one gets.

MR. POWERS: Yes.

MR. KRESS: Now, just because you make a wall an adiabatic wall does not mean it doesn't affect the heat transfer.

MR. POWERS: Yes, it does.

MR. KRESS: Is that part of your scaling consideration, also?

MR. POWERS: Yes. We have to -- the way we have approached that is we a little bit overdrive the amount of heat we put into the melt to compensate for the conduction out the walls.

What Dr. Kress is bringing up is that, in creating a one-dimensional piece of concrete that's being attacked by the melt, I do have to constrain it with some walls, and I usually use a refractory out here.

In fact, the technology that we have developed is to develop magnesium oxide as the refractory. It is quite insulating in and of itself. It's a high-porosity material. Nevertheless, a certain amount of heat gets conducted into it.

About a third of the heat we put into the melt actually ends up in these walls. So, when we are trying to achieve something like .3 watts per gram, we put in .4 to compensate for that conduction into the walls.

MR. KRESS: I also had another consideration in mind, and that was that the nature of the natural convection currents one gets depends on that wall.

MR. POWERS: Yes.

One of our very initial concerns in looking at melt-concrete interactions, one that arose in the scaling program itself or the scoping program itself, was saying, gee, if you're talking about something that's meters wide, would you get a natural circulation current that, in a small-scale test, you could never, never reproduce, and would that be a major problem?

The discovery that was made very quickly in the scoping program was that this gas generation is taking place all over the material.

It's creating violent stirring that occurs over a length scale, really, probably dictated by Taylor instabilities, that's about like this, and that the key to doing good melt/concrete interactions is, in fact, to do something so that you have several of these -- what are called Taylor wavelengths -- across your diameter and then, after that, to -- it just goes on ad infinitum, and that a gross circulation pattern is not developed.

Nevertheless, people still look for that gross natural circulation pattern that might be affecting the results.

It is an area that continues to be worried about that, but there is never anything shown up very definitive that we have massive differences in the nature of melt attack between things on the diameter ratio of 20 centimeters and a meter.

MR. KRESS: I would have thought that both Taylor scales and Rayleigh cells would be of importance, and it's the Rayleigh cells that I'm worried about. You automatically reproduce the Taylor size.

MR. POWERS: Cells, that's right.

MR. KRESS: But it's the Rayleigh cells I was worried about.

MR. POWERS: What I can tell you, Dr. Kress, is that, again, we continue to look at this, but the code seems to be able to reproduce well at 20 centimeters, 40 centimeters, and more recently, nearly a meter without recognizing a Taylor cell, and we continue to look at it.

There was some initial evidence and some tests done with two-foot-diameter hemispherical crucibles that we had a large wave occurring. If that occurs, it is not something that grows with time.

That is, if you have some higher heat transfer locally, that doesn't result in the melt all going into that direction, that higher heat transfer tends to heal itself out.

The defense I can offer is that we just don't see anything that suggests we need to do that. It's not something that's neglected, by any means.

MR. CATTON: You could also use as a defense Dhir's data.

MR. POWERS: Yes. I understand.

MR. KERR: Commercial break.

[Slide.]

MR. POWERS: Let me continue through the lists of tests.

SURC 1 and 2 tests looked at UO2-ZRO2 interactions.

SURC 3 and 4 looked at a very important phenomena, how un-oxidized zirconium metal would affect the interactions.

It had been presumed, certainly in WASH-1400 and for several years thereafter, that all the zirconium in the core would be completely oxidized as part of the natural core melt progression.

The change in our technology and our improvements in our ability to predict how cores melt down within vessel has resulted in us understanding it.

In some cases, a rather small fraction of the zirconium will be oxidized in vessel, and a lot of it will be present to participate in the ex-vessel melt interactions, and having zirconium metal present is an extraordinarily important thing, because zirconium metal is an extremely electro-positive species, and its reactions with oxidizing materials, such as steam and carbon dioxide from the concrete, produces a lot of chemical heat to augment the decay heat.

The BETA tests were conducted in West Germany. They consist of metallic mills, and they use the two-dimensional design.

MR. KERR: Excuse me, just out of curiosity, what does the SURC acronym represent?

MR. POWERS: It originally stood for Sustained Uranium Concrete Interactions. As you can see, some of the tests involved uranium and some did not. I think it's best just to take it as a label.

MR. KERR: That was just a matter of curiosity. What about ACE?

MR. POWERS: Advanced Containment Experiments. The ACE program involves things beyond melt concrete interactions. It involves a lot of containment phenomena.

MR. CATTON: Is that the Argonne work?

MR. POWERS: It is, indeed, and the Argonne ACE tests are looking at UO2 ZRO2 melts. They're primarily directed towards Source Term issues.

[Slide.]

MR. POWERS: I did want to go through one of the tests just to show you the level of test technology and give you some idea that these are relatively sophisticated experiments. The example I chose was the SURC-4 test where the objective was to show how strongly zirconium, unoxidized zirconium would affect melt interactions with concrete.

The procedure adopted in the SURC test was to establish a steady-state interaction of stainless steel with concrete. I think this illustrates a very important point. In the past, when melt/concrete interactions were first investigated, just putting hot steel or concrete was a major event, a very exciting event.

Now, we've gotten to the point that establishing a steady-state interactions is an every day sort of thing. Nobody thinks very much about it. Once we'd established a steady-state stainless steel interaction, we added zirconium -- not zirconium dioxide; I'm sorry about that -- we added metallic zirconium to the melt and the transient observed.

What you see is that the test procedure was set up so that we got the differential effect of zirconium metal very clearly. This just shows you the experimental apparatus, again, a one-dimensional slug of concrete being exposed to melt. The melt was heated by induction heating, all contained so that we could quantify the gas generation and aerosol production.

The instrumentation that's applied in these tests is, indeed, extensive. I show you a layout and also provide you a list of the instrumentation. The instrumentation philosophy is to instrument heavily and to provide replicant measurements of most of the critical phenomena, for instance, gas generation is measured in at least three ways, gas composition in two ways, aerosols are characterized in at least three different ways. So, we have a high degree of confidence in the data that are being produced for the codes.

[Slide.]

MR. POWERS: Let me show you a plot of the melt temperature in the SURC-4 test. The time is the experimental time. You will see that we heat up the melt, it begins to attack the concrete. We establish what we call our steady-state melt/concrete interactions. Twenty kilograms of zirconium metal was then added to the melt.

The melt was interacting with the basaltic concrete.

Had you made a prediction of this with the codes that existed at the time, you would have calculated that adding the zirconium would have caused the melt temperature to drop. Instead, we saw a tremendous increase in the melt temperature and then a slow decay in that, eventually getting back to a steady-state steel interaction with concrete.

MR. CATTON: How do you separate the coupling of the electric field? When you add the zirc, you're going to change the character.

MR. POWERS: Minuscually, hardly at all.

MR. CATTON: Hardly at all?

MR. POWERS: Hardly at all.

MR. CATTON: It doesn't change it?

MR. POWERS: Undetectable change. The only things that are going to change about the melt is its resistivity and the magnetic flux. Your temperature where the magnetic fluxes don't contribute to the forces at all at this point, so all you're changing is --

MR. CATTON: I'm not talking about forces; it's the heating, the eddy currents.

MR. POWERS: All of your heat is being applied, really in a fairly narrow zone around the outside and then being stirred into the melt by the gas generation. Dropping in 20 kilograms of zirconium hardly changed that at all.

MR. DAVIS: What was the original mass?

MR. POWERS: About 200 kilograms and adding 20 kilograms in was about a 10 percent change.

MR. DAVIS: Thank you.

MR. POWERS: We chose the 20 kilograms -- quite frankly, it was not chosen on a -- it was chosen on the basis of fear. Zirconium metal is a very energetic material when you put it into the concrete, and you can see the kind of temperature excursion we got here, quite different than what the codes would predict at the time.

[Slide.]

MR. POWERS: I should mention that melt temperature is probably the most sensitive arbitrator of whether a code is doing well or not. Erosion of concrete is probably the least sensitive arbitrator, and so I'll be showing you a lot of melt/temperature plots. I'll show you a lot of concrete erosion plots, but pay particular attention to comparisons between the codes and melt temperature, because temperature is a very important parameter to us from fission product release, and it is extremely difficult to predict temperatures accurately with the codes.

MR. WARD: When you say it's a sensitive arbitrator, you mean because it's easiest to measure or because you're most interested in it or what?

MR. POWERS: It's the most difficult to get a good agreement between the code and the experimental measurements. The measurements are not, by any means, easy to make, but it's a technology that I think we've successfully developed, so we do it fairly routinely now. W will get code predictions that match erosions very well, and are completely off on the melt temperatures.

MR. CATTON: I understand.

MR. POWERS: That poses a major problem to us when we try to calculate aerosol release.

MR. CATTON: The only reason that would be true would be if you can't calculate the heat transfer to the interface?

MR. POWERS: Exactly so.

MR. CATTON: If I can't calculate the heat transfer to the interface, I'm not sure what good this is. That's what I need.

MR. POWERS: What I'm indicating to you is, look very closely how well we do on the melt temperatures.

MR. CATTON: You need both.

MR. POWERS: Admire what we do on the concrete erosion, but understand that they can fool you pretty well.

MR. CATTON: The good temperature predictions could just mean that you've got something wrong with the heat balance in the code. It doesn't tell me a thing.

MR. POWERS: You have to look at all of them, I agree.

MR. CATTON: I have to have a heat balance. I need to know the heat transfer coefficient and if I know both of those, I should be able to predict the pool. The pool is least important.

MR. CORRADINI: I guess what I was going to ask was a different question, but if you think of it just from a problem standpoint, I've got power up/power down, and sensible heat storage. So, the erosion is an indication of power down.

The temperature is an indication of sensible heat stored. Where are calculations compared to experiments of power up?

MR. POWERS: Say that again?

MR. CORRADINI: Energy up, power up; in other words, if I look at it strictly as energy balance, I have power down, power up and sensible heat storage.

MR. POWERS: Exactly.

MR. CORRADINI: So you're telling us to look at temperature as an indication of sensible heat, erosion is an indication of power down; do you have --

MR. POWERS: I will show you nothing on that in the plots that I have, Mike, on power up, simply because we tried to maintain an adiabatic boundary. We do measure the power that we are losing upwards, but I didn't bring any slides to show it.

MR. CORRADINI: They're in the reports, though?

MR. POWERS: Absolutely. This just shows you concrete erosion as a function of time. Concrete erosion is indicated by the ablation front. I also show what's called the wet/dry front; that is, concrete has water in it and that water is evaporating from the concrete. A front can be defined for that evaporation as taking place.

I've also indicated that here, and you will notice that that wet front and the ablation front, are moving forward rather similarly, at similar velocities, once we're into the vigorous melt concrete.

MR. KRESS: Do you have a conductivity meter in the concrete that measures that wet/dry front? How do you establish where that is?

MR. POWERS: This particular plot is based on a temperature isotherm, the 400K isotherm. We did, in fact, in the early scoping work, Tom, put in electrical conductivity devices to see where the wet/dry front would show up, and we're rather surprised when starting seeing temperatures on 150 degrees rather than 100 degrees, until we remembered that the concrete is a low porosity material and you develop a substantial pressure in there so that the dehydration front is typically around 150 degrees.

I just chose the 400 degree K isotherm and that's what we measure now. In fact, if you plot the temperature responses of concrete, you will find that the temperature starts going up, hits a plateau, holds at a plateau a little bit and then goes on up.

During that plateau is when you're evaporating water.

MR. CATTON: Dana, how good is your heat balance?

MR. POWERS: How good is taking the sum, adding it? It is a measurement that we take very seriously. We try to do it in three different ways. Probably our best that we do is getting within plus or minus ten percent. Plus or minus 20 percent is the worst we'll allow for a competent test.

MR. CATTON: Do you have anything that describes the process by which you do this?

MR. POWERS: Both the SURC reports have an extensive description of that heat balance process.

MR. CATTON: For this kind of test, plus or minus 20 percent is pretty damn good.

MR. POWERS: Yes, it's -- 20 percent, if we get over 20 percent, we start discounting the test heavily. If we get to ten percent, we congratulate ourselves pretty well, as a matter of fact.

MR. CATTON: Compensating error.

MR. POWERS: It could well be.

[Slide.]

MR. POWERS: I just wanted to show you here the hydrogen gas coming as fraction of the gas coming off during the experiments. It is a basaltic concrete, so that it is primarily water and hydrogen coming out of the experiment. The drop here at the point of the zirconium addition was because we open a valve that allows some leakage out of the system.

You see that once the zirconium is added, not only does temperature go up, but the rate of gas production and the amount of hydrogen that you're producing goes up fairly dramatically.

[Slide.]

MR. POWERS: Sustained uranium dioxide experiments are now being done fairly routinely. This shows you the apparatus that's used in the SURC tests. The heating to the UO2 is actually done with tungsten plates that are heated by induction.

The ACE program uses a somewhat different experimental philosophy. This shows their apparatus. They put their UO2 charger here and heat it with jewel heating between the two tungsten electrodes.

[Slide.]

MR. POWERS: As I have said, there is now an extensive database looking at both metallic melts and oxide melts, solidified materials, and we have looked a the effects of zirconium and we've certainly looked a various concretes. The database, though it is extensive, certainly does not span all the possible combinations that one will imagine will arise in the course of accident analysis.

We are, in fact, developing a computational tool that are to be used generically that plants have sufficient diversity and people come up with sufficiently diverse accidents. I think we've certainly heard today, volunteers coming up with even more diverse accidents that experimentally, I doubt that we could look at all those possible combinations.

We are, in fact, dependent upon the codes to interpolate and extrapolate these data. If I were to try to characterize weaknesses in our database, the things that stand out in my mind is that we have certainly looked a horizontal downward attack more extensively than we have horizontal attack; that is, the radial erosion of concrete is an area we've investigated less thoroughly than the downward.

For a general reactor accident, the erosion is less important. It becomes more important when you think of things like pedestal failure in boiling water reactors to understand that radial attack better. I think you'll see, when I show you some comparisons between the codes and radial attack, that we don't do too badly.

Nevertheless, it's not been very thoroughly studied. We have looked at essentially metallic melts and essentially oxidic melts. Sometimes those oxidic melts will have a little bit of metal in them.

We have never looked at a test that has both metallic melts and oxidic melts where the two phases have about equal volume. We depend there heavily on our codes to tell us how metal and oxide melts simultaneously interacting with the concrete, themselves interact, and it's a fairly important interaction and one that I do believe we understand fairly well.

We have never done tests that, once started, go on for long periods of time; that is, something on the order of 10 hours.

MR. KRESS: Dana, we heard earlier that victory has been declared. In view of that, are we to interpret these areas of weaknesses as not being very important?

MR. POWERS: Certainly my view is that I think, for instance, on the radial attack, it's a relatively unimportant in most reactor accidents. You can undoubtedly define a particular accident and a particular plant where it might be very important, and I think that the victory that's been declared doesn't address that very specific issue.

MR. ELTAWILA: We will continue to get data from the BETA experiments which will help us validate our model in this area, so although we don't have a program at NRC, there are other programs that produce these data that we will be using to update the models and validate them.

MR. POWERS: I think you'll see that we don't do badly. So, the scenario weakness of the database -- but I don't think -- it doesn't keep me awake at nights.

MR. KERR: You will recall that at one point in the Vietnam War, Senator Aiken, I think it was, suggested that we declare victory and pull out of Vietnam.

MR. CORRADINI: Am I to take that there's an analogy here?

MR. POWERS: I hope not. I very strongly believe now, and I think that we have met the enemy and they are our's, in this case, that we went from an era where melt/concrete interactions are an extremely daunting, very complicated subject, that we continued to find new phenomena as the descriptions of the melt that we would get from the in-vessel became more complicated. We can now explain most of that phenomenon.

So, I don't think we are backing out. I thought I would put out these weaknesses, not to dilute the theory that we understand enough, but to point out that, yes, you can probably find areas that I have not explored thoroughly in the experimental database, but to my mind, these are not very serious.

MR. CORRADINI: I asked this of Brian before, and we -- I got kind of an answer of money, but I want to make sure I understand this on a technical basis. If tomorrow we were to pick an experiment and we said, okay, before the experiment is to be run or before the data is to be released, we do the calculations, what's an acceptable uncertainty -- what would you guess to be the range of uncertainty we would get and the number of people trying to see what the results of the experiments are, and what's acceptable?

MR. POWERS: I, myself, have what I will call goals that I have in mind for these codes. For the erosion of concrete, the amount of gas generation, hydrogen production, I think we should be within about 30 percent in our code predictions. For aerosol generation and what not, I like to be within a factor of five on the release of specific radionuclides and prefer around a factor of two.

Okay. That kind of uncertainty means that we have reduced the uncertainty due to melt concrete interactions down to where it's well below the uncertainty in how well you can specify an accident for me. Certainly, for a fission product release we well below the uncertainty you have in calculating the consequences of that release.

MR. KERR: Excuse me. Would you give those numbers again?

MR. POWERS: I wrote it down. My personal goals, and it's nothing agency-endorsed by these goals, but to the extent that I advise them when I think I understand things well enough, about 35 percent on things like concrete erosion, hydrogen gas generation and the heat transfer aspects of the problem, and about a factor of five on the release of particular radionuclides, preferably around a factor of two.

I'll show you a little later in the talk why I have that constraint on the fission product release particularly.

MR. KERR: These, you think, are achievable, in most cases of importance, with existing codes?

MR. POWERS: I think so.

MR. KERR: Thank you.

MR. CATTON: I would agree.

MR. POWERS: We're, in fact, engaged in a little exercise right now, doing blind calculations on tests.

[Slide.]

MR. POWERS: I mentioned that computer codes are an essential part of the melt concrete technology. I think this group is very familiar with the codes that are available. CORCON is the code that's being developed by the NRC. WECHSL is a very similar code developed in Germany. The industry uses a code called DECOMP.

MR. CATTON: Before you take that away. I was in Germany for some reason or another, and observed some of the BETA tests.

MR. POWERS: Yes.

MR. CATTON: And they didn't look like anything I'd seen before. It looked like oatmeal. Now, will the CORCON go all the way into the oatmeal stage?

MR. POWERS: Oatmeal?

MR. CATTON: You know, my mother used to make oatmeal, and it would get very thick.

MR. POWERS: Your mother made strange oatmeal if it looked like melt concrete interactions. Did she not like you?

MR. CATTON: Big bubbles would form.

MR. POWERS: Yes, yes. The codes don't --

MR. CATTON: It looked like Yellowstone mud pits.

MR. POWERS: I think that's -- having that mud pit look, especially once the melt has dropped down to relatively low temperatures, where the viscosity, particularly of the siliceous melts that are used in the BETA tests are very high. That's not uncommon to see this kind of mud plot, every once and a while a bubble coming up, throwing materials up and very hot.

MR. CATTON: And no aerosols.

MR. POWERS: Well, certainly the BETA tests report aerosols that agree rather well with what we would calculate. They sometimes are not very visible aerosols.

MR. CATTON: But the CORCON has been validated well out into that regime?

MR. POWERS: CORCON does very well out in that regime, as a matter of fact. Since you brought it up, I'll just --

MR. CATTON: And there was a second problem.

[Slide.]

MR. POWERS: This is the aerosol generation observed in the BETA 3.3 test. It's the aerosol generation, as a function of time. The data collected in the experiment are shown as the bars. VANESA, which is now the supertine of CORCON that calculates aerosol generation is shown by the solid line. The early part is when their apparatus was open, they had not sealed it up yet, so we miss -- we are high there, relative to their data, simply because their data is not collecting everything.

Then, after that, the code is predicting the aerosol source rate. We are probably a little high for most of the measurements, as we should be, because the measurements are not corrected for deposition in the lines.

So, in fact, our ability to calculate that aerosol generation, which is virtually invisible --

MR. KRESS: Before you take it away, your ordinate, or is it abscissa, I forget, is grounds per mole of gas?

MR. POWERS: Yes.

MR. KERR: That begs the question as to how well did you predict the moles of gas predicted? You know, that's a relative number.

MR. POWERS: My recollection is that I used the CORCON calculation of those moles of gas, because the experimental data in the BETA test is polluted by the fact that you're getting a lot of radiant heating on concrete sidewalls, and that gas that comes from those sidewalls doesn't pass through the melt, doesn't contribute to the aerosol generation.

I don't have a plot, nor do I remember how well I predicted the gas generation in that test. We -- you have to make that correction, to see how well you predict in the BETA test. It's a problem with two-dimensional tests.

MR. CORRADINI: If I can point out one thing. I mean, you said you were going to get to it with fission products, of why two to five was the number that, if you were to be given the choice you would choose that. The 33 percent, just for gas as an example though, all of the experiments, BETA, ACE and the Sandia experiments, unless I'm wrong about this, either over or under predict -- over or under measure the actual gas generated in the MCCI for various reasons. Because I've read the reports on that, and I'm pretty clear on that. If I remember reading the SURC 4 Report closely enough, what you would get from erosion and what you measure are off by a factor of two. In the BETA test it's off by a factor of two.

MR. POWERS: Not in the SURC 4 Test.

MR. CORRADINI: Yes.

MR. POWERS: Not in the SURC 4 Test. Definitely not.

MR. CORRADINI: And in the ACE Test it's low by about a factor of two. Unless I'm missing -- because I sat there with the person who had written parts of the report, and [illegible] to make sure my hand calculations were write, from different parts of the report. The difference was of that order.

MR. POWERS: Definitely not in the SURC 4. That one is extremely accurate, unbelievably accurate, as a matter of fact. Now, I believe that what you say is true for the ACE test, but I believe they have a back side leakage problem.

MR. CORRADINI: Right. But, I -- what you said about BETA I thought was an identical problem with SURC, unless I'm missing something, in that you have a -- even though you have a one-dimensional, in terms of heat loss, you still can generate gasses from above, from radiant heating.

MR. POWERS: You get a small, like a one percent release from just the water absorbed in the MGO. That's usually gone by the time you go molten.

MR. CORRADINI: Oh, because that --

MR. POWERS: It's very hot up there.

MR. CORRADINI: All right. Then maybe I should talk to you separately. Because the way I read the report and how you print out the results of gas release, particularly the water, there is a difference. I got the impression it was from the water release of the MGO from the radiant heating.

MR. POWERS: That's a relatively small amount. Now, they do take that into account because they do collect that.

MR. KERR: Incidentally, this reminds me of something not related to this, but, in an earlier slide that Mr. Farouk had, he said CORCON Mod three was not yet available to the public, or not yet available. What did that mean?

MR. POWERS: That means that we have not -- before we can release a code to the general public, two things have to occur. There's an extensive set of document that has to be produced, it's not just a user's manual, but it's also a models and correlations document, I guess you would call it. In the case of CORCON, we will also have a validation document, and I will talk about that validation document at the end of my presentation.

Then it has to go through a peer review that the NRC dictates, and that can vary from a fairly elaborate review, which they might oppose, for instance on the MELCOR program. I think they have described that to you. For a code such as CORCON, it might be somewhat less formal, consisting of people experienced that would use the code.

MR. KERR: But the local people understand it well enough to use without documentation and the validation is sufficient that you assume it's the most accurate code that exists.

MR. POWERS: We use it routinely for our work.

MR. KERR: All right.

[Slide.]

MR. POWERS: I think people understand, and we've suddenly discussed that all the codes essentially are doing this problem of a melt interacting with concrete, where you have heat being generated by decay, predominantly in an oxide layer and heat being generated by chemical reaction predominantly in a metal lawyer. I depicted these as two distinct layers. In fact, they can be vigorously stirred and intermixed. And simply, you have heat losses. Heat losses going radially, heat losses going upwards, either into the atmosphere or overlying water, heat losses going into the concrete. Because you have heat generation in the two-phase, the oxide phase and the metal phase different, that is, decay heat is slow heat generation over a long period of time; chemical heat is a very in tense heat generation that goes on until you deplete the reactive materials, you also have to worry about heat transfer within the interim region.

MR. KRESS: Dana, the chemical heat includes zirconium reacting with the silicon coming from the concrete.

MR. POWERS: Wait. You're messing up my punchline here.

[Slide.]

MR. POWERS: I just wanted to illustrate the modeling that's done in CORCON to get that mixing. Interlayer heat transfer is done by the entrainment of high-density material into a low-density phase, as a result of gas bubbles rising through the melt.

MR. CATTON: Is that George Greene's stuff?

MR. POWERS: It builds directly on George Greene's correlations and models.

MR. CATTON: What about radionuclides in the metallic phase?

MR. POWERS: They are certainly taken into account. It's about 20 percent of the radionuclides are in the metallic phase as far as heat generation goes. That's a very inconsequential heat source, when you have this tremendous chemical heat source going on.

[Slide.]

MR. POWERS: And this just shows you how much mass is entrained as a function of time for melts with the density ratios shown here. For instance, eight grams per cubic centimeter is the dense phase; seven grams per cubic centimeter were the low-density phase. You see, you get a curve that looks like this. About 5 percent of the high-density material gets entrained in the low-density material, and the entrainment takes place very quickly.

Similarly, as the densities get closer, one should get down to 7.25 grams per cubic centimeter. For the high-density phase, you see you get to 100 percent entrainment.

MR. KRESS: I'm not sure what I'm looking at, Dana. Is that the model --

MR. POWERS: It's the model of calculations, yes. And I put it up just to illustrate for you that there are periods of time during melt-concrete interactions where in fact you don't have two distinct layers, you have complete entrainment. And then that goes away as you start incorporating lower-density material into the oxide base, and you drive the density differences up.

[Slide.]

MR. POWERS: There have been a variety of improvements that have led to what is called CORCON Mod 3. I've listed down several of them here.

I wanted to illustrate for you how we've used the experimental data to improve our models in two areas, the areas marked with the stars, that is, the heat transfer models to the concrete and the inclusion in this condensed phase chemistry that I silenced Tom about because I didn't want to lose my punchline.

And there are some materials in your handouts that will explain some of the other model improvements.

[Slide.]

MR. POWERS: I wanted to turn back to the SURC-4 test that I had shown you and remind you of the melt temperature as a function of time. This is the experimental data.

Had we predicted that melt temperature with the older models of heat transfer to the concrete, the models based on gas films, we would have gotten the curve shown with this dotted line here, that upon addition of the zirconium, in fact, the melt temperature would drop, simply because you had to melt the zirconium, and that the amount of gas coming in and the amount of chemical heat was not able to reverse this drop in melt temperature.

[Slide.]

MR. POWERS: A lot of effort was expended to improve the models of heat transfer to the concrete. It would become evident from a lot of tests. This test certainly demonstrated it. But more important, the BETA tests clearly demonstrated that the gas film model of heat transfer to the concrete was simply not adequate.

And in your handout, I certainly described that we have now gone to one that's called an intermittent contact model. It shows you schematically some of the features of that model. You have a melt interacting with concrete, generating gas bubbles. You have options. Either an interfacial crust forms or not. If there's no crust formation, then heat transfer is limited by convection from the melt as these bubbles form, and heat transfer across a thin film slag.

If a crust forms, it depends on whether that crust is stable or not stable. If it's unstable, you get the bubbles breaking up the crust intermittently. If a crust is stable, then heat transfer is limited by conduction across that crust.

[Slide.]

MR. POWERS: When we apply that kind of a model to predicting tests, this is one example, the so-called SWISS-I test. This shows you the ablation of concrete as a function of time, predicted with this CORCON with an improved heat transfer model. Extremely good agreement.

MR. KRESS: Dana, could you go back to that previous slide just a moment?

[Slide.]

MR. KRESS: These are model options built into the CORDON-Mod3.

MR. POWERS: Yes, it's not really options, in the sense that the user selects them.

MR. KRESS: That was going to be my question.

MR. POWERS: We have a crust solidification criterion built into the code. The option the user has is to say do you want the intermittent contact model or does he want to go back to the old gas film model.

MR. KRESS: There's no built-in criterion within the code that selects those.

MR. POWERS: The code calculates whether the crust is formed and then a strength criteria is built into it.

[Slide.]

MR. POWERS: I have included in your handouts comparisons with a lot of other data. In view of our previous discussion, I ought to show you, with the BETA tests we get both axial and radial erosion data, and these plots show you how we do. This happens to be the V0.2 test. The axial erosion is indicated by the dark squares, the radial erosion by the open squares. In this case, we obviously are missing some of the initial transient attack. Typically, we do a little better than that. I can't tell you what the difference is.

[Slide.]

MR. POWERS: Here is the BETA 1.7 test, again showing you the predictions of axial erosion and radial erosion. And though we have not studied radial erosion as extensively as axial, we really don't do too badly.

MR. CORRADINI: How did you do in comparison to 1.8 and 1.9? Those were highest-powered tests --

MR. POWERS: Yes, those were very high-powered tests. I bet you know something more about it than I do, because I don't remember seeing calculations.

MR. CORRADINI: I've never seen any calculation that's been able to predict those and consistently predict others.

MR. POWERS: There's a problem at least with one of the tests in that the melt starts wormholing around.

MR. CORRADINI: That was a different one.

The reason I'm asking this is I'm kind of going back to your overall thing of 33 percent.

MR. POWERS: Yes.

MR. CORRADINI: Because all this is well within 33 percent. And my question goes like this: why did you pick 33 percent for the, I'll call it for want of a better word, heat transfer thermal hydraulics aspects fo the MCCI?

MR. POWERS: I will tell you quite frankly, the correlations that we use for these heat transfer models, when I look at the coefficients that are in there, they know them to about 25 or 30 percent. That's why I picked 35 percent.

MR. CORRADINI: Another way of looking at it is, if you look at another population of available dry data and you blindly try to predict any of them, you are lucky if you can get it better than about a third.

MR. POWERS: Yes. Well, I feel good about that.

[Slide.]

MR. POWERS: When we take this improved heat transfer model and apply it to the SURC-4 test to predict melt temperature as a function of time, we don't match. It matches on the others, but when we had the zirconium present, it was clearly not matching. There had to be something else that was driving things.

What we believe is that the zirconium metal has a more pervasive influence than simply making the melt more electro-positive and allowing for more vigorous chemical interaction.

[Slide.]

MR. POWERS: That, in fact, the zirconium metal not only reacts with gases coming from the concrete, it reacts with the molten concrete itself, and that you get a reaction of zirconium dissolved in the steel with silica coming from the concrete to form zirconium dioxide, and reducing to a form of silica. That silica can subsequently react.

This does not change the overall amount of heat generation, but it certainly changes the rate at which you're generating this chemical heat very dramatically.

[Slide.]

MR. POWERS: When you make this change in the code and include that condensed phase chemistry, this is your prediction of the melt temperature, indicated by the blue line, an extremely good match for the peak.

There are some interesting features in here that may have to do with more of the description of the test than the code itself, but essentially, you match very well.

MR. KRESS: Dana, in the case of that test, it was clear where that energy ought to be put.

MR. POWERS: Yes.

MR. KRESS: If you had a two-layer mixture early in the accident, the oxide phase might be on the bottom.

It's more likely to be on the bottom.

MR. POWERS: That is what a lot of people would estimate based on both densities. There are some subtleties on that, but go ahead with your point.

MR. KRESS: Then this reaction would depend on the ability to mix the two at the interface.

MR. POWERS: That's right. And they would very dramatically mix in that case because if the oxide is on the bottom, then it's density is very close to that of the metal, and you would get a lot of inter-mixing. Once again, we don't really have a problem of where to put that heat.

[Slide.]

MR. POWERS: This is just the calculation of the ablation as a function of time, and again, you see, with the improved chemistry, we match very well.

[Slide.]

MR. POWERS: Well, the $64 question is, we have developed models largely based on metallic melts interacting with concrete. Certainly the beta tests and many of the early tests in the SURC program were metallic melts interacting with concrete.

The critical question is, can you now take these melts with the heat transfer correlations that you've developed and apply them for a strictly oxide melt interacting with concrete, making no other changes except what you have to do for -- accounting for material properties.

[Slide.]

MR. POWERS: These are the results of melt temperature for the SURC-1 test of uranium dioxide interacting with concrete. These are the measurements in the CORCON prediction. In general, the CORCON prediction is very satisfying. We realize that this is one of the first predictions of an oxide melt interacting with concrete.

MR. CATTON: It would be helpful if you would put both erosion and temperature on the same diagram because then --

MR. POWERS: It would be helpful, but they get awfully confused.

MR. CATTON: It would enable you to make some sort of judgment about how well you're doing your heat balance.

[Slide.]

MR. POWERS: Well, here's the erosion results, and if I were to characterize it, I was extremely happy with this. Maybe we miss a little bit on the early -- we're not generating heat quite as fast as we ought to. Maybe our viscosity is a little high in our thermal physical properties, but in general, we predict the erosion quite well -- maybe just a little offset on that.

[Slide.]

MR. POWERS: We are now in the process of predicting other people's melt concrete test, especially with respect to fission product release. This shows you the results of fission product release in the ACE test of uranium dioxide interactions with concrete, and I plotted release fractions for a variety of materials listed across here, and I show with the dark bars the predictions from the VANESA model of CORCON-Mod 3 versus the experimental measurements, and I have not been careful about indicating what the experimental error is on the measurement.

This is an extremely satisfying result we have obtained here.

MR. CORRADINI: Is this part of the benchmark, because I a not familiar with these calculations.

MR. POWERS: These are calculations we've done.

MR. CORRADINI: They are part of the benchmark exercises done for L6.

MR. POWERS: Now, whether they have actually been submitted to anyone, I don't know. I asked specifically for them, and this is what was given to me from the calculation.

MR. DAVIS: Dana, why aren't there any VANESA calculations for molybdenum and ruthenium?

MR. POWERS: I'll touch upon that. Can I walk through the rest of them, and then I'll get to it. It's a very clear calculation. There are calculations. They're right down in here.

Uranium and telerium were extensively released in the L6 test, and when we got large amounts of release, the code was extremely accurate in its predictions for both of those. As we get into the more trace release materials, we are certainly within about a factor of five on these.

For the molybdenum and ruthenium, we missed them completely and we were completely low, and for reasons that we very well understand. When the VANESA model was created, we did not believe that you would in any experiment involving a metal phase get so oxidizing that you could create molybdates that would vaporize.

The code simply does not have them in it, does not recognize molybdates, and molybdates are very volatile. So the code never gets any molybdates and it never predicts any volatility here.

We did not know that any ruthenates existed. The code does recognize ruthenium oxides, but we never get so oxidizing that those ruthenium oxides can form. We have just recently discovered there are ruthenates, potassium ruthenates and sodium ruthenates. The code does not have them, and so we did not predict them.

My suspension is that as soon as we add those additional species in here, we will be able to match these tests very well.

MR. KRESS: Dana, when you say VANESA, are we to conclude that's the VANESA as it exists in Mod 3 of CORCON?

MR. POWERS: Yes. And that's what it was done with. I simply put VANESA up because VANESA is my baby. But VANESA is now a sub-routine of CORCON-Mod 3, and the calculation was done as a full-blown calculation of A6. That is, it was CORCON's prediction of melt temperature and gas generation and the aerosol generation, and I should have written that down to be clear.

[Slide.]

MR. POWERS: I wanted to comment on accuracy of fission product release. Fission product release is nearly exclusively due to vaporization of materials out of the melt.

In other words, you have some melt material here designated A being driven into a gas phase, A in the gas phase, and the thermodynamics of that vaporization, the partial pressure of A, is a function of the concentration of A, an activity coefficient of A, and this thermodynamic factor involving the change in the free energies.

This activity coefficient here is usually given as some Function -- that should be a capital F -- of the composition in the temperature.

A typical uncertainty that you'll have in these thermodynamic properties of either A in the liquid phase or

A in the gas phase is like plus or minus 5,000 calories per mole. That's typical for these refractory species that we're looking at.

That kind of an uncertainty, at 188 degrees K, would amount to a factor of four uncertainty in the partial pressure, and that partial pressure uncertainty translates directly into an uncertainty in the release.

It's this reason the inherent uncertainty and the properties, the thermodynamic properties I have available for these species that I can't expect fission product release codes to be more accurate than about a factor of two to four.

To resolve that uncertainty, to reduce that five kilocalories down to some other level would be a formidable research undertaking. You're literally talking about billions of dollars worth of work, because people work very aggressively to try to get thermodynamic properties very accurately, and we are really right at the limits of where technology is.

[Slide.]

MR. POWERS: Let me just conclude by saying that I think we have predictive models, that we can treat all the essential phenomena. I believe the heat transfer mod, I think, is particularly well developed.

There are some refinements still possible in the chemical modelling to get us somewhat better predictions, particular late time melt, properties like the melting temperatures.

There is a relatively large database to compare our codes against. We have compared against a few of them. I think Dr. Corradini has certainly pointed out beta 1.8 and 1.9 which we have not compared against apparently that still need to be done.

[Slide.]

MR. POWERS: We have set a schedule or a list of what we think are the critical tests to do the comparisons against. These are the ones we are focusing on now. Perhaps we should add 1.8, 1.9 of the beta test to this if they have been difficult in the past.

MR. CORRADINI: I am kind of curious, not to pick those out of any -- pick one from ACE, one from the new set of BETA. Are there any others? Why this list versus something else?

MR. POWERS: The shortness of the list is dictated by the manpower and the funding we have available.

MR. CORRADINI: Right. I understand.

MR. POWERS: These were tests that we thought were relatively well instrumented and well characterized so that we had a range of things to compare against, and we were happiest with them.

There certainly is a certain amount of capriciousness in the selection, though I think it spans quite an interesting range of conditions.

MR. CORRADINI: The reason I'm asking that is that the WECHSL users group in Europe has picked a list which is different than this, and the ACE group is in the process of picking a list which is different than this. It seems to me that that's good, that you've got --

MR. POWERS: Well, I think some consolidation of those --

MR. CORRADINI: Yes.

MR. POWERS: -- so that we assure that at least there's overlap, a considerable amount of overlap would be a worthwhile thing to do. Thank you, Mike. I'll act on that.

MR. CORRADINI: And then I have one last question. That is, you mentioned DECOMP, but I'm kind of curious. Wouldn't one get about the same sort of agreement on erosion and temperature from DECOMP as I would see in these, or has that been done? Maybe I'm asking the wrong person, but it seems to me --

MR. POWERS: You really are. I guess I really hesitate to comment about DECOMP because it is a proprietary code and because I do not use it. I really hesitate to comment.

MR. CORRADINI: The only reason I'm asking this is that if we pick your numbers as a straw man for acceptability, then any sort of predicted tool ought to be within that predictability.

MR. POWERS: Well, for me to be happy with using that -- I can't use DECOMP, so I really hesitate to comment on it.

MR. KERR: Are there further questions to Dr. Powers?

[No response.]

MR. KERR: Thank you very much.

Our next agenda item: Completion of the source term research, chemical form of iodine. Mr. Lee.

[Slide.]

MR. LEE: The purpose of my talk is to give you a summary of this NUREG report that we just published in July. That is the Oak Ridge Study on iodine chemical forms in LWR severe accidents. The NUREG report number is CR-5752, and we published that in July.

MR. KERR: I would note that Dr. Kress participated in the preparation of this report and I have suggested that if he feels it is desirable, that he may make clarifying comments on occasion, but other than that, he will be an interested listener on what you do to his report.

MR. LEE: I just want to make a note that this was published in July and it was published in the Federal

Register. It went out for public -- it's a draft for public comment. So, I'm going to summarize that.

I want to mention that this study is part of an overall effort of research in revising the Source Term, and it is -- the next viewgraph --

[Slide.]

MR. LEE: This is just an outline of the presentation so let me move to the next one.

[Slide.]

MR. LEE: We are basically talking about the design basis accident Source Term, the TID-14844. As you know, we are in the process of revising it and that part includes three items, the timing part, the form and the magnitudes of the fission products that will be released into the containment. This is different from the Source Term that we're talking about, the one getting into the environment.

There are three contractors involved with that, the timing, INEL is doing their part. The quantity and also the form coming out from these severe accidents, Brookhaven is doing that work, and specifically from Oak Ridge, we focus on the iodine chemical form as it come out from the RCS, and as it enter the containment, how does the iodine behavior changes with time.

MR. KERR: It seems to me it's a slight

misrepresentation to say that TID-14 844 is used in licensing. It could be, but in practice, because of Reg Guides 1.3 and 1.4, it actually is not used in siting evaluations, I think. Those Reg Guides just specify Source Term that is somewhat different than 14844.

MR. KRESS: Yes, somewhat different, but they were based on TID 14844.

MR. KERR: Based on it in the sense that they're different.

MR. KRESS: The only difference is in the quantity that one uses.

MR. KERR: Yes, but that's pretty significant.

MR. KRESS: Yes.

MR. LEE: Now, you know that the TID 14844 was used in the 10 CFR Part 100 for the siting criteria and you know that the other use for this TID 14844 -- the environment that we have to look at some of the plant systems in plants. Those are, for example, the pulse test sampling systems and the electrical equipment qualification. Then if you look into plant mitigation features like the filter design and all those, we use that Source Term to evaluate how effective they are.

[Slide.]

MR. LEE: The Reg Guide 1.3 and 1.4, as you note h here, has specified the iodine chemical form. Fifty percent of that goes into the containment and of that, only 25 percent is available for release, available for leakage from the containment. The iodine chemical form is noted that 91 percent of that is I-2.

The five percent is in particulate and then you have like 40 percent in organic iodine. Assume that once it's released, it stays constant. There's no change in any of this throughout your evaluation when you use that.

[Slide.]

MR. LEE: Now, why are we considering a change? Now, you know that the TID 14844, the experimental database was based on some data that we did back in the late 50's and it based on some UO2 pellets. Now, from the TMI accident, we found that the iodine, the amount of iodine released in the containment is on the order of magnitudes less than what was predicted at that time, and that was mainly attributed to the iodine chemical form.

Now, we have undertaken a lot of research since the TMI and a lot of those are done at Oak Ridge as well as the ACE program that you know about, and we found that predominantly, the form of the iodine, instead of I-2, it is CSI, cesium iodide.

Then we also know that cesium iodide can react with other products, can give you a modest amount of other HI instead of CSI. But at the same time, we also found from the Oak Ridge study that the magnitude of that is that the airborne iodine in the containment is not to sensitive as to whether it's CSI or HI.

So, whether you get CSI or HI, its behavior is pretty much the same. Also, once you get it in containment -- we did a lot of study on how iodine behaves in water. Oak Ridge works in that area, and also ACE, and we know that based on temperature, the radiation level and the pH, iodine, I-2 can come back out from the water pool.

Then last year, the Commission asked us to speed up our research and try to complete on the iodine chemical related to TID 14844, so we redirected our programs at Oak Ridge last year. At that time, we were wrapping up the iodine chemical research work; that is, the documenting of all the results. We stopped that and we asked Oak Ridge to perform this evaluation.

[Slide.]

MR. LEE: The Oak Ridge analysis. I would like to walk through with you briefly on that. The Oak Ridge only considered the chemical speciation of the iodine as it comes off of the RCS. And then after it comes out from the RCS, we look at how the iodine reinvolve from the water pool, and the formation of organic iodine. The timing of the release or the magnitude and amount of release are not considered on the Oak Ridge study. That was done by Brookhaven.

MR. KERR: Would you go through this again? Oak Ridge took Brookhaven calculations, and the Brookhaven calculations were for what?

MR. LEE: The Brookhaven calculation took the studies, the accident sequence and --

MR. KERR: Which accident sequence?

MR. LEE: Both of those coming from the NUREG 1150.

MR. KERR: Okay. They took the 1150 sequences and --

MR. LEE: And they analyzed for all those nine groups of cesium, iodine and all those noble gas, and they will calculate, initially, how much is released during the initial phase. If the vessel was breached how much came out, MCCI and all of those. That is done under the Brookhaven study.

MR. KERR: So they, in effect, calculated the amount of iodine that would be in containment?

MR. LEE: The amount and the type that came out.

MR. KERR: Okay.

MR. LEE: The focus of Oak Ridge is only on iodine.

MR. KERR: Yes. And they took the BNL results without comment?

MR. KRESS: Yes, for both the releases and the thermal-hydraulics.

MR. LEE: Yes.

MR. KERR: Thank you.

MR. LEE: Oak Ridge and Brookhaven use the same database to -- for the study. So, they are consistent.

[Slide.]

MR. LEE: The seven sequence that were used in Oak Ridge was shown here. The first, Surry is a three-loop plan, Westinghouse, the subatmospheric atmosphere in the containment. This is a high-pressure sequence. The next one, A/B is a low-pressure sequence.

Then in the Grand Gulf, as you know, is a Mark III containment, is a BWR 6, it's a high pressure, low pressure. High pressure, low pressure, this is a BRW 4, with a Mark I containment, and for PWR with an ice condenser, we only use one. So, these are the seven sequence we use. And from these studies we obtain this information so we can start on the calculation.

MR. KERR: How were those sequences chosen?

MR. LEE: They were to maximize, from the risk point of view, that they release the most material, in terms of iodine.

MR. KERR: Thank you.

MR. LEE: So, actually, we examined more than this seven sequence, and we picked those seven. Dr. Kress went and another staff went there twice to ensure everything is --the one we chose gi.e us the maximum.

MR. KRESS: In general, they represent a low pressure and a high pressure sequence for each plant type.

[Slide.]

MR. LEE: Now, after the iodine enter -- this is the part -- the results of the analysis, the one that enters into the containment.

The system that we focus on the Cesium, Iodine, water and hydrogen system. This is the governing equation that shows the reactions. We also note that the Cesium Hydroxide here can react with other surface structures surfaces in RCS system, like the stainless steel. It can also react with other borates, other things in the system.

We also note that the Cesium Iodide can revaporize from the RCS system after it is deposited after some certain time, because of decay heating.

Taking into account what we get from here is that you will get a -- instead of Cesium Iodide, you will get an HI-type of a product. Okay, this is what we are looking for here. But, as I mentioned to you earlier, it really doesn't matter whether you have CSI or HI, when you get out in containment they behave the same way.

So, this is the result that we have concluded from this study -- that 95 percent of this is in CSI and then there is only about five percent that's in this form. Okay. And this is in big contrast with what TID-14844 prescription is. It's almost a flip-flop between the two distribution. Okay.

MR. WARD: Where do organic compounds of iodine come into this?

MR. LEE: We didn't see too much for the part that enters into the containment.

MR. KRESS: Because we didn't have -- these chemical forms were produced by a full kinetic calculation, chemical kinetics calculation. In making such a kinetics calculation, you have to define the whole set of chemical reactions, and a set of reactants. We did not put into those set any organic pulling materials. So, obviously, we wouldn't get any organics out.

MR. WARD: Okay. Well, why didn't you? I mean, is that --

MR. KRESS: It's because we didn't know how to quantify that. The feeling was that they would not be stable in the temperatures we're talking about in the RCS. Mostly, the organics come and are produced in the containment anyway. And we did deal with organics in the containment.

MR. WARD: Oh, okay. As produced in --

MR. KRESS: As produced later.

MR. WARD: Later, okay.

MR. KRESS: -- when the stuff gets in the containment.

MR. LEE: With respect to the containment, most of them would be produced in the containment, with the I2 reacting with the cable and paint services. Besides, the NUREG 1150 has any organic calculation.

MR. KRESS: Yes, it does.

[Slide.]

MR. LEE: After the iodine gets into the water pool, the phenomena that we consider are radiolytic conversion I minus to I-2 that is based on radiation dose.

We have new experimental data for that, both for PWR and BWR. The PWR has a much larger dose rate, by 4-5 megarads per hour and then for BWR it's much less. It's less than 1 megarads per hour.

Those information were obtained from experiment.

Then the next one you have to deal with is the gas liquid partition of the iodine. That is the I-2, how much is in the gas phase versus the one in the liquid phase and that is based on the volume to the liquid, the pool ratio, the volume ratio.

As you see, the larger containments, for the PWR there is less liquid there so you have -- that affects the difference between how much gas will come out in I-2.

Then once I-2 comes out we consider how it reacts with the paint and cables also and you can estimate how much organic iodides you will get.

Now we did two cases as to the control of the pH. If you maintain pH --

[Slide.]

MR. LEE: -- this is the diagram that shows the re-evolution of the iodine, I-2, from the water pool. If you look at this, the Surry, this is for the pH control is greater than 7, this is around 2 percent, 1 percent and these are much less than 1 percent.

You have very little come out.

The reason this is bigger because if you look at the Surry, the amount of liquid versus the containment volume, the containment volume is very large so that's why you have more gas in the part that is not in the liquid.

If you don't control the pH this means you let it go down at whatever value it gets to. This one came out to be around 97 percent or so. This is around 60-something percent, 20, 10, 6 and 2 -- something along like this.

It's the same way because of the PWR containment, the way that amount of water it has in there is much less compared to the BWR.

Then the difference between these two are based on the partition of the iodine at the interface. The temperature is larger so there are more, if you compare with this sequence for the same plant.

MR. CATTON: For the BWRs, is that what has gotten beyond the pool, beyond the suppression pool?

MR. LEE: Yes.

MR. KRESS: What Richard was saying is this is an equilibrium partitioning between the liquid and the gas phase and for a BWR your containment volume is small, compared to the water volume.

MR. CATTON: The drywall volume is small.

MR. KRESS: Yes. What that doesn't consider then is if you had a continuous leakage you may continue to evolve the stuff from the water and you may eventually release much more out of the water phase.

This was an equilibrium partitioning number.

MR. LEE: Although I don't show the organic iodine that results from the --

MR. CATTON: That's the equilibrium of the space over the wetwell with the water in the wetwell?

MR. LEE: Yes.

MR. CATTON: All right.

MR. LEE: This is only for the I-2. If you look at the organic iodine, if for the uncontrolled case it is about less than 1 percent in terms of coming out.

This is for the I-2. If you look at the organic iodine part, it is around 1 percent maximum.

If you control the pH, then it is about one to two orders of magnitudes lower.

MR. DAVIS: Excuse me. Most of the iodine in the suppression pool would be cesium iodide?

MR. LEE: No. Once it gets in there --

MR. KRESS: Once it goes into the pool, it becomes I minus. Then it undergoes a series of chemical reactions that may convert it back to organics or I-2 that makes it airborne again, so if it is in the water it may be in several different chemical forms. If it's in the air it's mostly organics or I-2.

[Slide.]

MR. LEE: Essentially we should note that currently all the major, the plant mitigation systems are optimized with respect to iodine is based on I-2 form, if you look at your filters and all these designs.

We understand much more about iodine since TID-14844 so we like to incorporate that into our daily business.

[Slide.]

MR. LEE: As you know, we are proceeding with updating the technical basis for the source term, the so-called revising the old source term.

I believe a document must be sent to you already because next Monday will be a full ACRS committee to review this matter and it will include in that document the timing of the release, the composition and magnitudes, as I mentioned, from Brookhaven.

This is INEL. This is from Oak Ridge. Also it will include, some credit will be given for the natural removal process once the iodine gets into -- or this radioactive material gets into the containment.

There is a briefing to the ACRS in November and after that it will be sent to the Commission where it will be out for public comment.

MR. KERR: You said credit will be given for the removal process?

MR. LEE: Yes, like the --

MR. KERR: What will be the environment in which the removal process is expected to take place?

MR. LEE: We are not considering about spray or anything. You just consider whatever the condition inside the containment --

MR. KERR: The condition as defined by what?

MR. LEE: By these accident sequences.

MR. KERR: Okay, so one will take the sequence and from that determine the environment within the containment.

MR. LEE: I think it's mostly that the thermal hydraulics condition that you encounter within the containment for all the sequences that you are looking at.

MR. KERR: I see. I think most containments' removal systems are designed to operate in an environment which is produced by a design basis accident.

Isn't that the case, currently?

MR. LEE: That's correct, yes.

MR. KERR: And that environment could be significantly different so one has to take equipment designed to operate within a DBA environment and estimate how it will behave in a sequence environment.

That could be an interesting challenge.

MR. LEE: I think you will hear the presentation to this committee next month.

MR. HOUSTON: Just a point of clarification.

I think the updated TID report is scheduled for a subcommittee review on January 7 and 8.

I don't believe there is anything on the November issue.

MR. LEE: That was the last schedule that I knew.

[Slide.]

MR. LEE: Now some regulatory implications or applications or whatever you want to say.

Now the pH control for the containment spray and the pH control in the BWR plant is in place for current plant, and I understand it will also be the same for the

ELWR and also for the ALWR, for the BWR type.

The BWR has a huge volume of water in the suppression pools. Now there is no such thing as pH control in the current plan system, and there is no plan to have those in the advanced one or the evolution one either.

So these are the questions that you need to think about in view of what the iodine chemical forms that we have are.

MR. KERR: Will the answers to those questions require additional research, or does the information now exist to settle those issues?

MR. LEE: I'm not sure you need to do more research. Maybe you have to do some more calculations on it.

MR. KERR: Okay. So this is not --

MR. LEE: You need to do some --

MR. KERR: This is not aimed at saying we need additional research to get these, but rather this is --

MR. LEE: I think you need to do some evaluation on it.

MR. KERR: Thank you.

MR. LEE: On those.

[Slide.]

MR. LEE: At this time, I'm going to delve into a little different subject, not on the Oak Ridge, but I am telling you that there is some additional information that will be available in the future that we can further asses the models that we have now and also other models that we use for the RCS system.

Now there is one activity that is going on right ow this year, is to take all the technology we have from Oak Ridge and put that in the CONTAIN code in Sandia, like the iodine. All the information we know about those will be put into CONTAIN.

MR. KERR: Do you think a Sandia code will gladly accept data from Oak Ridge? I'm not sure that they're compatible.

MR. WARD: As long as they're all metricated, right?

MR. LEE: We are already in the process of transferring those models to the CONTAIN code already.

MR. KRESS: Oak Ridge and Sandia are very compatible.

[Slide.]

MR. LEE: Now this is a project in France that we got involved in a few years ago. It started a few years ago. We entered into an agreement with CEA to participate in the PHEBUS-FP program, and this program is run by CEA and also by the Commission of the European Communities.

[Slide.]

MR. LEE: We participate in two phases of that program. The first phase was just an experiment -- defining the experiment, and that was from the period from '89 to '91 that were involved.

Now the experiment will be done in the second phase, which will start next year. The first experiment is scheduled in October of next year.

[Slide.]

MR. LEE: What is PHEBUS-FP? It stands for fission products. It is an integral experiment, and the focus is on fission products chemistry, and they will study how the fission transport is in the reactor cooling system, and then once it gets into a containment system, we would like to study the iodine chemistry. We also expect that it will get some core melt progression type information, because you have to melt the core in order to generate the fission source.

[Slide.]

MR. LEE: It uses a 21-rod fuel like this. It's about one meter long, I mean in terms of length, and there's a control rod in the middle. So basically it is about this small, the bundle is.

[Slide.]

MR. LEE: The six tests -- there are six tests in the test matrix. The first one is actually a scoping test, and it uses fresh fuel, all fresh fuel for that, and the rest will be -- there are five tests that are going to use the fuel, all irradiated, and they will radiate it for two weeks or more to generate those short-term fission products before they initiate the experiment.

These are the experiments, the type of severe accident sequences they will try to simulate or study. And the first two are cold leg break, and then these others are V-sequence, TMLB-prime, two of them, and then the bypass.

As far as I can tell you, the test matrix is pretty much set for the first two, but the rest may change as we learn more information from this facility.

[Slide.]

MR. LEE: This is a schematic of the facility. There is a driver coil right here. It's about 40 megawatts nuclear power, and this is the test bundle. It's a loop here and will melt the fuel. The fission products go into here, and you study how it behaves inside at this point here.

[Slide.]

MR. LEE: This is the simulation of the RCS system. You need to have a steam generator, or you have a pressurizer, and this represents the containment, and all this thing is located in a large vessel called a caisson, and you have all sorts of remote manipulation to retrieve samples.

[Slide.]

MR. LEE: A picture of this facility. This is before the facility was modified. That's the old one.

[Slide.]

MR. LEE: This is current construction going on. This is the part where the containment will be, the vessel will be situated.

And that's all I need -- I would like to tell you something about this program at this time, and I'm sure in the future we'll brief you more in detail on this program.

MR. KERR: Thank you.

Thank you very much.

Any questions?

[No response.]

MR. KERR: Defining a new source term; is that anticipated to be completed before the results from the PHEBuS experiments are available?

MR. LEE: Yes.

MR. KERR: What are you going to do with the results from PHEBuS?

MR. LEE: As you know, the PHEBuS is an integral experiment, and the data from there, we would like to look at it and validate our computer codes like VICTORIA or CONTAIN.

One good thing from PHEBuS is that the radiation source coming out of PHEBuS, for example, in the containment, will be the real products that come into the pool, and you can evaluate how the internal radiation dose is generating, changing the evolution the iodine.

MR. KERR: So you don't anticipate anything that will have an effect on your source term definition?

MR. LEE: In PHEBuS, one thing we are considering doing in PHEBuS now is to include boric acid in the tests. The boric acid, the effects of boric acid is that instead of having a cesium iodide or cesium hydroxide later deposit onto the RCS system, you can end up with other products, like cesium borate and acid. It is more stable than the cesium iodide.

So once it binds onto RCS surface, the late prioritization of the cesium, that is a question that one can address. We can learn something from that.

MR. KERR: I'm not so much interested in the details of the experiment as I am in how you are going to use it in defining the source term. And apparently the answer is it won't be used in defining the source term.

MR. ELTAWILA: You are correct, Professor Kerr. I think that the source term definition is almost complete. We are confident that we have sufficient information right now to modify the source term.

The PHEBuS project is, as an integral experiment, as Richard indicated, is going to be a long-term confirmatory program, and it always would be nice to have data to validate the model and say that we did the right thing, you know, that it's like we did with the ECCS and so on.

But we do not at this time envision our contractor telling us that there would be any new information coming, or surprises coming out of the PHEBuS project.

MR. WARD: So how much are you putting into PHEBuS?

MR. ELTAWILA: Actually, the net payment, if you know the history that the French were not part of the Cooperative Severe Accident Research Program at NRC, so we let them join in and we joined over there, and the net cost is just a small, few dollars going across the border. But it's $100,000.00 a year for the next five years.

MR. KERR: One further question.

My impression is the source term work you're describing is mostly applicable to existing reactors and will have maybe peripheral relationship to new reactors?

MR. LEE: I don't think so.

MR. ELTAWILA: We've been having discussion with EPRI and the ALWR user group, and in general, most of the work is quite applicable. The only difference is the timing of the release. Because of the low-power decay in the advanced light water reactor, it takes a long time for the fuel to heat up and start failing. So the information will be the same; however, the timing of the release out of the fuel, that might be the only issue that needs to be addressed. But that's a separate issue from the source term issue.

MR. KERR: For example, is it likely that a new defined source term will lead to request for significant modification of emergency planning?

MR. ELTAWILA: I don't think so.

MR. BURSON: I'm Bradley Burson. I'm working on the source term project with Len Sofer. And I think the answer to your question is that the revised source term is more directed toward the advanced reactors than the present generation of plants.

The application as with regard to present generation plants at this time is more considered to be in a sense optional. So the focus is primarily on the advanced plants.

MR. KERR: And it is unlikely that the changes will be significant enough or that one will consider or is likely to get requests to consider changes in emergency planning?

MR. BURSON: I think that is highly unlikely. The basis for the development of the source term is largely predicated on the NUREG-1150 data. And of course, that's been pretty well on the market for several years.

MR. KERR: Thank you.

MR. BURSON: I hope that answers your question.

MR. KERR: Are there further questions?

[No response.]

MR. KERR: We will recess, then, until -- the agenda says 1:30 -- I'm going to change that to 1:15, if I'm within my legal boundaries.

[Whereupon, at 12:15 p.m., the hearing was recessed for lunch, to reconvene the same day, Thursday, October 14, 1991, at 1:15 p.m.]

AFTERNOON SESSION

[1:15 p.m.]

MR. KERR: We now have a discussion of the core melt progression activity, the discussion to be led or begun by Mr. Rubin.

[Slide.]

MR. RUBIN: Good afternoon. My name is Alan Rubin. I'm in the Accident Evaluation Branch of the Division of Systems Research and I'm here today to give you a brief overview on the status and the plans for core melt progression research and also to present a summary of plans that we have for developing a comprehensive core melt progression research program plan.

This presentation that I'm giving will serve as an introductory remarks to the more technical, detailed discussion that Bob Wright is gong to present following my presentation.

[Slide.]

MR. RUBIN: For the purposes of background, core melt progression is one of many topics that was included in the NUREG 1365, which is the revised Severe Accident Research Program Plan that was issued in 1988. In-vessel core melt progression is only one of the issues which was identified in that document as Long Term Issue L-2. The objective of research on core melt progression is to do confirmatory research to improve our understanding of melt progression which will lead to a reduced uncertainty associated with risk estimates from severe accidents.

Our plan is to update the Severe Accident Research Program Plan to reflect progress that has been made since the plan was issued in 1988. You've heard that mentioned this morning. As far as the core melt progression part of that plan, the focus for research today has primarily been on the early phase melt progression.

There have been many experiments. You saw Brian's slide this morning showing you the uncertainties and the number of experiments in terms of early phase and late phase core melt progression. There have been many more experiments in the early phases of melt progression and generally many fewer experiments in the later phases.

The general feeling is that today there's a feeling that the early phase melt progression behavior can be pretty well predicted. But there are much more uncertainties and larger questions to be answered in terms of the later phase melt progression.

The plan, when updated, will identify and prioritize the research that is needed, that we see as needed, for core melt research.

MR. KERR: Under the Objective, we're improving understanding and reducing uncertainty; is the feeling also that in the process, risk will be reduced somewhat?

MR. RUBIN: This is a long term confirmatory research program in melt progression. So that the risk estimates that are our understanding today, will be confirmed by the research that we do to understand better, the melt progression issues.

MR. KERR: I don't think that's an answer to my question. Maybe there isn't an answer to my question.

MR. WRIGHT: Bob Wright, Accident Evaluation Branch. Actually, I think that there is a very good chance that not only the uncertainty in risk, but the level of risk will come down if, as indicated by Three Mile Island, we can establish a strong technical basis for reducing our estimates of the melt mass and the melt metallic content.

So I think that there is a fair amount of potential gain in that area.

MR. KERR: What you're telling me is that calculated risk will go down?

MR. WRIGHT: I'm saying --

MR. KERR: I'm asking if the actual risk will go down.

MR. WRIGHT: I'm saying actual, and that would be our assessment of the actual risk, based on a technical basis of reducing essentially the melt mass and the metal content from what is normally -- has been assumed in our risk assessments.

MR. KERR: But you can't reduce the actual risk unless you make physical changes to the plant; can you?

MR. WRIGHT: Okay, I'm sorry. I misinterpreted -- I mis-responded to the question. Our assessment of the risks, I think, can very well go down. You're quite right that that's different from actual risk but our current assessments may very well be high because we are tending to assume large melt masses.

MR. KERR: I recognize that. I was asking just a question as to whether this research is expected to reduce risk.

MR. WRIGHT: Oh, you mean change the plants?

MR. KERR: Whatever.

MR. RUBIN: It will lead to a better understanding of the phenomena which will lead to hopefully some improved margin in our risk estimates. Whether they understand that they're conservative or not, --

MR. KERR: I'm not suggesting that research has to reduce risk. I'm simply ask if, in your view, the research will reduce risk?

MR. RUBIN: By reducing risk, if you mean, will there be some modifications to the plant made on the basis of this research --

MR. KERR: Well, modification to the plant, modification to the operation of the plant, modification of NRC so that -- I mean, whatever. Is there expectation that the research will reduce risk?

MR. RUBIN: I do not think that's the primary objective of the core melt progression research.

MR. SHERON: Dr. Kerr, no, we're not going into this with any expectation that we're going to reduce actual risk of the plant. We're going into this with an expectation that we will be able to reduce what we believe are still large uncertainties in our understanding of the risk and our perception of risk.

I agree with what Dr. Siess said; the risk in the plant is what it is. It's only our perception of what it is that we're attempting to quantify better.

MR. SIESS: Is the risk in the plant as we now perceive it too great?

MR. SHERON: No.

MR. SIESS: If our perception of risk at this point in time is not of any concern, why do we want to change it?

MR. SHERON: We're not proposing to change it.

MR. SIESS: You said it might reduce our perception of risk. I think if you reduce the uncertainties, you probably reduce the mean risk that you calculate. Whether you'll do anything to the median, I don't know.

MR. SHERON: I said it would reduce our perception of the uncertainty in risk. It would not reduce our perception of the risk.

I agree with what Bob said, that somewhere down the road maybe it might, if we learn that the amount of fission products that are released are lower than what we've previously assumed.

MR. KERR: It would seem to me that it is more likely that risk would be reduced if the research program were planned with the idea that maybe there are some things that could be looked at which would result in risk reduction.

I don't know that this is necessary or not, but I think unless one is looking for these sorts of things, it will be only serendipity that will produce them.

MR. SHERON: We are. It's called accident management.

MR. KERR: Well, if this will contribute to better accident management, it seems to me that it can reduce risk.

MR. SIESS: But it won't reduce your risk of core damage, because we're talking about core melt progression.

MR. KERR: When I say risk, I'm talking about risk to the public, although you're right, I should be more specific.

Please continue.

[Slide.]

MR. RUBIN: There are two basic areas of research in our current plans, and these are with experiments and associated analysis and modeling.

In terms of melt progression experiments, there currently are three experiments which are planned. And I'll just briefly mention them and tell what the objectives of these experiments are, and you'll hear a lot more detail from Bob Wright.

The first experiment is an ex-reactor experiment which is planned to be performed at Sandia National Laboratories sometime in early 1992, which is to look at the relocation of material in BWR cores during a severe accident. And the experiment is designed to see if core blockage occurs in dry core BWR severe accidents.

MR. CATTON: At some point, would it be possible to find out how what you measure is going to come to bear on the models that are in the codes you use?

MR. RUBIN: How what we measure in the experiments will bear on the models? That's the intent of doing the experiments in part, is to verify and validate the models and improve the models where need be.

MR. CATTON: Well, you've got about five codes out there that near as I can tell pretty much predict the same thing, roughly speaking, or at least close. And they all have different models. Not totally different, but somewhat different.

How are these tests going to come to bear on that?

MR. WRIGHT: I'll address --

MR. CATTON: If you're going to address it later, I'll wait.

MR. WRIGHT: I'll address it. And I may not cover it as much as you would like, so hit me later.

MR. RUBIN: The second experiment is experiment on melt progression.

MR. KERR: Excuse me.

MR. RUBIN: Yes.

MR. KERR: We are past the first bullet now, which says "designed to see if core blockage occurs"?

MR. RUBIN: Right.

MR. KERR: Now, is that an experiment or a series of experiments?

MR. RUBIN: That's planned a series of experiments.

MR. KERR: Okay. And as a result of those, one will be able to see unequivocally that core blockage does or does not occur?

MR. RUBIN: Well, the question is whether core blockage does occur or does not occur, and that's what this experiment is intended to lead to, an answer to that question.

MR. KERR: Is there some reason to believe that it will answer that question?

MR. RUBIN: Yes, there is, and you'll hear more about that --

MR. KERR: Okay.

MR. RUBIN: -- from the presentation --

MR. ELTAWILA: Maybe it will be worth it to say at this time that this program is going to undergo a peer review and we will try to get consensus from the scientific community around here if this program is going to address most of the phenomena and going to reduce the uncertainty further.

So this is the plan right now as we envision it and we are undertaking the peer review for that plan right now.

MR. KERR: Okay. Thank you.

MR. WARD: Why is core blockage important in a dry core? When you say dry core, you mean it's being precooled or --

MR. RUBIN: It's more likely that you may not have a blockage in a dry core as compared to a severe accident where you have water in the core or a sufficient amount of steam in the core to create a blockage.

MR. WRIGHT: I'll be going into the technical background of all these things in detail later.

MR. RUBIN: The second experiment planned is in the annular core research reactor at Sandia, which is to look at melt and crust behavior where you have a blockage in the core. And this experiment will address the interaction of the metallic crust with the melt pool and with the crust relocation during a severe accident.

MR. KERR: To say that something is addressed doesn't really give me much information. But I guess I'll wait until Mr. Wright's presentation.

MR. RUBIN: We can get into the details, but I think you probably would be better off to get a better picture of the details, when we go into, you know, picture of the experiment and what it actually will undertake.

MR. KERR: Okay.

MR. RUBIN: This is to give you a brief overview first, and we will get into the details.

MR. KERR: It's just that I'm not sure what I'm getting an overview of, when the word "addressed" --

MR. RUBIN: Hopefully, an impression of the intent of what these experiments or the objective of these experiments, where they will try to address questions to answer uncertainties which we feel now are key that need to be answered in the melt progression research area.

MR. KERR: Okay.

MR. RUBIN: The third test is a full-length high test called FLHT-6 at the NRU reactor i Canada. This test is scheduled after quite an extensive delay because of leakage of heavy water in the reactor, and subsequent examinations to be sure that the reactor is back in good operating shape.

This test is planned for April of '92 and it's a BWR severe fuel damage test which will provide data on the full length fuel rod behavior for BWR geometries following boildown of the coolant.

The geometry will model the BWR control blades and channel boxes and will provide experimental data on the tendency for melting and blockage in BWR cores.

We realize that these experiments that are planned and those that have already been completed, they don't necessarily address all the melt progression issues, particularly with regard to late phase melt progression.

For example, one of the issues that has been discussed and identified relates to molten pool convection which determines heat transfer from the pool to the crust and subsequent location and timing of failure of that crust. And this phenomenon we'll be looking at and may be addressed in terms of analysis, or perhaps our follow-on experimentation.

Going along with the experiments and closely coupled to them obviously are the analysis and modeling for core melt progression.

We undertake in our core melt research program to analyze the results of all the experiments that are available. And the facilities where those experiments have been held are listed on the slide along, obviously, also, with the TMI-2 core examination.

These experiments are used to validate the codes and are used to also improve the phenomenological modeling in severe accident codes for BWRs and for PWRs.

And Mr. Catton, you mentioned five codes in terms of core melt progression. You'll hear later on more about the codes. We're focusing primarily for core melt progression on SCDAP/RELAP5 and MELCOR. It's the NRC-sponsored codes for core melt progression.

MR. CATTON: I just mentioned a whole bunch because they all predict essentially the same thing.

MR. RUBIN: But these are the two that we're primarily focusing on right now, that the NRC is sponsoring.

You'll hear a lot more about the code presentations tomorrow afternoon on each of these codes plus the containment codes as well.

[Slide.]

MR. RUBIN: Let me continue by telling you about the plans we have to develop a comprehensive research plan for melt progressi

Currently, we realize that a comprehensive program that shows how all the phenomena for late-phase melt progression will be addressed. It has not really been developed. And the objective that we have and that Brian Sheron mentioned earlier this morning was to develop a comprehensive core melt progression research program that would really identify the research needs, the research that is needed to improve our understanding of core melt progression, and, in particular, with regard to key questions of what is the amount and the composition and the temperature of melt in the lower head at the time of lower head failure.

MR. SIESS: Is it assumed that the lower head always fails?

MR. RUBIN: No, it is not assumed. We have another program addressing lower head failure. But that's separate. It's part of our severe accident research plan, but not directly part of the melt progression issue itself. You will hear later on this afternoon a presentation on the lower head failure.

MR. SIESS: In other words, the melt progression carries it all the way through to lower head failure.

MR. RUBIN: Yes. And there is analysis of potential lower head vessel failure modes.

MR. SIESS: Does the melt progression research then look at various stages of progression at which it could be stopped and how, or does it just assume we're not going to do anything and let it go through until it melts the head?

MR. ELTAWILA: Yes. It looks at, for example, the potential that the operator flood, at the late stage of the accident, flood the core and see what is the potential for stopping the accident, and so on.

MR. RUBIN: At the risk of raising som questions that were asked repeatedly to Brian Sheron this morning, let me just try and identify that in setting these priorities, it's important that we consider or the technical community considers that the uncertainty is acceptably low or that further reasonable expenditures will not substantially reduce the uncertainty. That's the qualitative criteria that we'll use to determine whether we will or will not fund additional research in this area.

MR. CATTON: There are some rather interesting arguments in one of the appendices to the scaling study that was done. And I believe that it was the appendix written by Sol Levy. And in it, that's where he looked at these different codes. And he comes to some conclusions about the whole melt progression process. And I thought they were rather interesting.

Is there anything you're going to do that will change that view?

MR. RUBIN: The view that all the codes are --

MR. CATTON: No, no. Maybe I made a bad assumption that you might have read it. But it was how the melt process occurred, and roughly what the results would be.

MR. RUBIN: I think roughly there is a pretty good understanding of the melt process and what occurs.

MR. CATTON: I'm wondering what's being done that would change it.

MR. RUBIN: Some of the understandings, for example, where a failure of the blockage would occur, the timing of a blockage would occur in the lower or the side portion of the crucible. That I don't think is very well understood. That's part of the objective, to reduce the uncertainty in that area.

MR. ELTAWILA: Let me try and maybe jump into that.

I saw what Sol Levy had done in this area. And his work is actually based on looking at what all the codes are predicting this phenomena. And I will be honest with you. Most of these codes right now have been tuned to the TMI accident. So they know how to predict this accident.

If we have a different situation from the TMI or the melt progression did not proceed the same way, we really don't know if the prediction of the code right now will still be valid. That's why we have an experimental program, to try to say that whatever we see in this code is really, this is the hypothesis that the code developer put forth in saying that's how I perceive the accident to progress.

We don't have enough database to go over the late phase melt progression, and that's what we want to be sure that this is not a unique situation that happened at TMI, and it is representative of most of the accidents that might happen in nuclear power plants.

MR. CATTON: So what I should look for are things that would do that in this program?

MR. ELTAWILA: That's correct. And we will try, and that's what the whole idea that Alan was trying to put the big picture first and before we get into the technical details.

MR. CATTON: I understand. One of the codes he looked at was also the one that we, many of us had treated rather poorly. That was the source term code package codes.

MR. ELTAWILA: That's correct.

MR. CATTON: They weren't a whole lot different.

MR. ELTAWILA: Absolutely.

MR. CATTON: That was pre-TMI.

MR. ELTAWILA: But the problem in addition to that, that most of the experimental data that has be ! developed in the past few years have not incorporated in any of the models right now. We can get into that. It's an unfortunate situation, but most of these models have not been updated or upgraded to represent the experimental observation that we've been seeing.

MR. CATTON: I understand.

MR. WRIGHT: I might comment on say, the source term code package and TMI. I have not seen Sol Levy's writeup.

But the assumption, and it's just out of the hat in March, in the source term code package, is that you have this so-called corium, which is a false idea, which has a quite high metal content. And the TMI results have been quite different. The metal content in the melt and lower plenum was less than 1 percent. So I guess that was not addressed. But I don't think that that's a generally valid conclusion, that those results are in agreement.

MR. CATTON: You ought to look at the appendix.

MR. WRIGHT: I haven't seen it. But I know that in that case it can't be, because the input is the high metal content, and what was seen was almost zero, and that's important.

MR. RUBIN: I would just like to identify what some of the five key --

MR. KERR: Is it required reading for the staff that they read drafts of the severe accident scaling methodology, those people who are planning experiments?

MR. SHERON: The report is not even complete yet. As you know, Dr. Zuber is retired. He left with the report in a slightly unfinished state. We've been trying to get the contractors to complete their sections. The report hopefully will be printed in November.

When we do have a report, yes, we expect, as I said in my slides, the report will be issued in draft for comment. I will expect my staff to read it, I expect our contractors to read it.

MR. KERR: Okay. Thank you.

MR. RUBIN: Getting back to the core melt progression program plan.

There are five key elements, key parts of this plan.

The fist one is to describe the current understanding of in-vessel core melt phenomena, where do we stand today, and to identify ongoing research programs that address these phenomena.

Also, the plan will examine the applicability of existing experiments and data that describe these phenomena.

So that's sort of where we are.

Now, where we're going is the report is intended to plan and to identify that new research would be needed to address the uncertainties and improve our understanding of melt progression phenomena to a quote "acceptable level."

MR. KERR: Now, how does one judge when an acceptable level has been reached?

MR. RUBIN: I knew that question was coming.

MR. KERR: I only asked it because I knew you would have been disappointed if I didn't ask it.

MR. RUBIN: Absolutely. You wouldn't be doing your job if you didn't ask that question.

Acceptable level, in my view, is a term that's used, unfortunately, throughout the agency, as a catchall for everything. We're trying to get a general feeling of consensus through a peer review process of this melt progression plan that the technical community feels that, as I tried to say earlier, the uncertainties are either acceptably low, they won't impact the overall risk, or the costs to do the experiment that might be needed to reduce the uncertainties is prohibitively high.

MR. KERR: Have you ever tried to get a committee to make a decision?

MR. RUBIN: Yes, I have. And let me just say that in terms of the peer review process that we have on this, we are not attempting to achieve a consensus.

MR. KERR: My experience would lead me to b that if you want to get a committee to make a decision have to present them with something which already rep a lot of work on the part of some one or two individuals.

If you are going to decide on what is an acceptable level, I don't think you'll ever get a result that means much by just asking a committee to tell me what is an acceptable level. Somebody's got to put in some effort in trying to arrive at a way in which one decides what an acceptable level is, and then try to make some recommendations.

Unless somebody does some of that kind of that kind of work, I don't think you're going to get very much out of a committee or a peer review group or whatever. Do you?

MR. RUBIN: I would agree. You certainly need to present a plan or a document for review. Bob Wright and John Kelly have prepared a draft program plan.

MR. KERR: I have seen a draft one which came along with a caveat that said this is so preliminary that it's probably not worth reviewing. I thought it was worth reviewing, personally. I didn't agree with everything, but it seems to me it's worth reviewing, which leads me to ask, is their going to be another version soon?

MR. RUBIN: Yes there is. I'll get into that on the next slide.

MR. KERR: Okay.

MR. RUBIN: But I just wanted to say that in terms of the plan, itself, we don't expect to develop detailed models for all the phenomena in melt progression. I mean, it may be that they're bounding calculations that can be done that show that the uncertainties just are not that large in certain areas. So, we don't expect to have detailed models of every phenomena in the melt progression included in this plan.

MR. DAVIS: Let me ask, if I may, related question?

MR. RUBIN: All right.

MR. DAVIS: In NUREG 1150, and other PRAs do attempt, at least to show uncertainties in the risk estimates. I am wondering, if you were to successfully complete all of this research, how much would you expect those uncertainties to be reduced?

MR. RUBIN: At this time, I don't think I can give you a quantitative --

MR. DAVIS: Well then, let me ask you a different question. Are the uncertainties that you're talking about here important to the overall risk uncertainty that's calculated in NUREG-1150?

MR. ELTAWILA: Mr. Davis, let me try. Some of the information that was provided to the panel of NUREG-1150 was based on subjective judgment and did not have any scientific data to back them up. That's good for the purpose that was intended for, to identify, on a global picture, if the risk is too high or not.

Now, what we're trying to do is to try to provide some information to support this subjective judgment by the panel. They did not have all the information in front of them, so we cannot tell you how much uncertainty you were going to be reducing, but at least we will say that their judgment is based on observation that we can see in some of the experiments, in some of our analysis, and they were not out of whack with what's going on. That's the gist of the research in this area.

MR. DAVIS: I understand that. But, all I'm concerned about is I suspect that uncertainties in the frequency of external events, particularly seismic, is probably driving most of the uncertainty in our risk estimates, possibly even some of the consequence analysis uncertainties are also dominating those uncertainties.

MR. ELTAWILA: That's correct.

MR. DAVIS: And when you say you're going to do this to reduce uncertainties, I -- what uncertainties are you talking about? The overall uncertainty risk or the uncertainties associated with these issues or both?

MR. SHERON: As we s[illegible] hopefully this all focuses down to the containment [illegible] way.

Let me give you an exa[illegible] would be sitting here a year or two or three years [illegible] way, and you may be hearing a presentation on the [illegible] of the debris coolability in an ALWR, and you may hear a lot of wonderful words from the industry about how they believe the debris is coolable, and then you're going to turn around and ask the staff whether we agree with that. The first thing you're going to say is well, you don't have any data to support that and the like.

One of the things we have to find out is how much stuff is going to come out of that vessel and what is it going to look like? Is it going to be metallic first, is it going to be oxitic first? I'm just saying -- I'm anticipating that these are the questions we're going to get down the road.

MR. DAVIS: But, if the advance plants meet their core damage frequency estimates, this issue will not even be raised, because the core damage frequency goal is already considerably lower than what we're getting out of existing PRAs.

MR. SHERON. Well, I can't speak right now for the NRR people. I don't know where they're coming -- EPRI has put in their requirements document certain things about severe accidents.

MR. DAVIS: That's right.

MR. SHERON: I presume they're doing that for a reason. If your recommendation is that the staff should ignore them, then please make it. But, they have put something down in front of us, we're taking a look at it. We are not ruling out severe accidents in these plants, okay. There is no database that exists for these plants in terms of the new systems they've put in and some of the new components.

That's another question I expect we would always be asked. They'll walk in and say we have a core melt frequency of 10 to the minus 6th or 10 to the minus 5th, and then someone is going to say, well, where is your database to support it? Beats us. There hasn't been a plant built yet for like an ALWR, like a passive plant. We have to consider severe accidents. We just can't ignore them at this stage.

In order to consider them, there are certain things we have to know. I know we're going to get the questions later on. If we don't get the data now, then it's going to be too late when the questions arise later on.

MR. CATTON: Let me maybe rephrase a little bit of Pete's question. There's this .02 square meters per megawatt thermal that's been put forward by EPRI. And that's based on, I think, the whole core being in the bottom of the reactor cavity. If .02 megawatts -- .02 square meters per megawatt thermal is a sufficient area, then what you do with this core melt progression doesn't really matter. So, the real question is in the coolability of that layer of stuff. It's not a matter of whether it's half of the core or the whole core, they say it's for the whole core. Right now we can't answer it. I understand.

MR. ELTAWILA: For the ALWR that's not completely true. They are not putting the whole core on the floor. There are -- I can say that there is an assumption about 75 percent of the core on the floor.

MR. CATTON: Okay, 75 percent.

MR. ELTAWILA: We don't know even if 25 percent will be coolable or 50 percent would be coolable. We have not had any demonstration of any test to show coolability.

MR. CATTON: I understand.

MR. ELTAWILA: Yes.

MR. CATTON: But the reason for the .02 is to handle 75 percent of the core. Can we answer that question? Is this core melt progression going to help us answer that question?

MR. SHERON: The industry right now has not proposed any program that we are aware of that will answer that question.

MR. CATTON: I actually believe you should be finding the answer to that question. I'm not trying to say you shouldn't. I'm just wondering what role this core melt progression plays in answering that question.

MR. SHERON: Well, the first thing this will tell us is how much of the core we think will most likely come out of the vessel when the vessel fails. We don't know if it's a hundred percent, we don't know -- we're pretty sure it's not a hundred percent. We only have one data point, it's called TMI. We're trying to understand how much material, how much of that molten ceramic material is going to relocate. Where are we most likely to get a crust failure? Is it going to be high up on the crust or lower down?

Once we know how much material is in that lower head, then we're going to know a lot better how much is available to go down in the lower cavity and whether or not that criteria if they have is going to be any good or not.

MR. CATTON: Okay.

MR. KERR: Maybe I completely misunderstood Mr. Davis. I thought he was addressing himself to risk calculations, and he was saying or was asking will this research, or have you looked to see if this research, no matter how it develops, will have a significant change on the risk that is calculated given what he considers to be the fact that much of the risk in current PRAs is driven by seismic uncertainties, the uncertainty at risk.

Now, I think if one extends this a bit, one might ask, has there been a systematic look at those uncertainties, be they core melt progression or whatever, that drive the uncertainty and risk. I think the answer probably is yes and you have concluded that the thing that drives most of the uncertainty or drives a major fraction of it is core melt projection.

MR. SHERON: That's correct. We have gone through 1150 and looked at it. Now, you will not find --

MR. KERR: Somewhere that will be available to us as a publication, won't it, where we can look and see how you've identified this?

MR. SHERON: We will try to provide that. I think that is a fair request. I don't think it is written down anywhere.

We have looked at 1150, okay. One of the problems is that if you just take 1150 you realize that the estimates of the uncertainty are just subjective judgments that were made because there was no data.

MR. DAVIS: They are incomplete. They don't include external events and they don't include consequence uncertainties.

MR. SHERON: Right. Now we have made a judgment, okay, based on the fact that this is a major part of the whole severe accident area, that there is a substantial amount of uncertainty in this latter phase of the core melt which we don't -- I could probably tell you right now -- I don't think was handled in a way that most people would have liked to see it handled in 1150.

It was based on subjective judgment with no data, what people think happens.

MR. KERR: What was based on subjective judgment?

MR. SHERON: I think the uncertainties in most of the core melt progression --

MR. KERR: No, but it is not unreasonable it seems to me to expect, and I would hope that you'd done this, that before you plan your research program you will have had somebody rather systematically look at the various estimates of risk that exist and try to identify those areas from which the greatest uncertainty comes, be it due to expert judgment or whatever.

MR. SHERON: It's a matter of one's perception of uncertainty though, Bill.

MR. KERR: Beg pardon?

MR. SHERON: I could argue right now that reactor vessel failure doesn't show up in risk studies as a major contributor unless the likelihood of the vessel failing goes up, like if we start worrying about embrittlement and stuff and all of a sudden it becomes a major contributor, okay?

It is a matter of perception of what is driving -- you know, somebody has to look at it and say, you know, does this thing have --

MR. KERR: Look. I am simply asking if you have had some people go over the kinds of things that contribute to uncertainty. I think the answer is probably yes -- and done something other than sitting down and making a gut judgment so that you could collect a few things and say here's how we decided that core melt progression was a major contributor to uncertainty and anybody else or a number of other people given the same set of data would be likely to reach the same conclusion.

MR. ELTAWILA: I have difficulty. You are looking at the uncertainty, for example, in external events so large that it is overwhelming all other uncertainties. That, you know, is your initiating event and if you try to compare the uncertainty in the core melt progression to that uncertainty, maybe you will decide that you don't need any research on core melt progression --

MR. KERR: I'm not --

MR. ELTAWILA: Let me finish, please. But if we try to separate what is -- if an external event does not lead into core melt progression, nobody will care -- but once a severe accident occurs, you need to analyze this accident regardless of the initiating event.

There are internal initiating events and external initiating events and they have different frequency so we are looking at the most probable one. Without trying to combine the uncertainty from the initiating event of a small probability event like the frequency of earthquake or something like that, compared to what will happen to the vessel later.

MR. KERR: I am not necessarily endorsing Mr. Davis's view that external events are the major contributor. I don't know whether they are or not.

I am saying that I just used as an example. I would assume at some point in this process somebody would look carefully to see what are the major contributors to uncertainty that we know about.

MR. ELTAWILA: Yes, we know about them. We know the core melt -- the mass, the temperature and the composition. We can see them enough, you know. Let's go back to the point --

MR. KERR: If this analysis has been made carefully, somebody has written something.

MR. SHERON: let us take a commitment to try and put that down, that logic for you, okay? Like I said, I think that is a fair request. We have looked at the contribution of the core melt uncertainties to our perception of risk uncertainty.

Like I said, we have not put that down in any formal document.

MR. KERR: I would say not do it except I think it would be useful to you if you did it and so if you can --

MR. SHERON: Like I said, we have already done it. It was an informal look and I don't see any problem with writing it down and sending it down here to you.

MR. KERR: Thank you.

[Slide.]

MR. KERR: Please continue.

MR. RUBIN: Let me conclude with the status of this core melt progression research plan.

A preliminary draft was prepared in late May of 1991. An information copy was provided to the ACRS. The reason it was sent, Dr. Kerr, with some caveats is that we just didn't want to request a review of that document from ACRS with formal comments right now because we were undergoing a review by technical consultants and once the results of their comments are incorporated into the revised melt progression plan, we then plan to issue a revised draft to the plan for comment by a peer review committee, a wider group than has looked at it now, the ACRS and the Nuclear Safety Research Review Committee.

The results of that plan will be incorporated into an update of the severe accident research plan.

MR. KERR: Any further questions of Mr. Rubin?

[No response.]

MR. KERR: Thank you, sir.

MR. TINKLER: I am Charlie Tinker from the Staff, and I would just like to comment a little bit about the debris coolability.

MR. KERR: Please stick close to that mike, so that we can hear you.

MR. TINKLER: I'd like to comment briefly about the remarks on the general subject of debris coolability. It was noted that the design criteria for debris coolability that has been offered by industry is a criteria that is irrespective of debris depths.

There is some relevance to core melt progression in terms of the amount of core that relocates to the floor. While the specific power density does not change whether you melt 50 percent and relocate it or 100 percent of the core, the demonstration of coolability of 50 percent of the core, should it form a 20 centimeter deep debris bed does not assuredly demonstrate coolability if 100 percent of the core relocates and forms a 40 centimeter deep debris bed. There are implications of that.

MR. CATTON: I wouldn't disagree with that.

MR. TINKLER: Well, there seemed to be some notion that whether you melted 50 percent of the core or 100 percent of the core, it didn't matter for debris coolability.

MR. CATTON: I thought it was 100 percent. But it's 75 percent. So as long as the amount that's going to come out is less than 75 percent, core melt progression wouldn't change things under those circumstances, if the 0.02 is a good number. If it's not, then the amount comes back into the picture.

MR. ELTAWILA: For example, the debris coolability depends on the amount of zirc that's going to be in the melt. Now if we assume like EPRI -- I forget the exact number they have -- only 30 percent of zirconium and the core melt progression is still going to come out as 70 percent zirconium, let' say. So it has implications here.

MR. CATTON: There are more serious problems about the EPRI modeling of that, I think.

MR. KERR: Mr. Wright.

[Slide.]

MR. WRIGHT: My name is Robert W. Wright. I'm a member of the Accident Evaluation Branch. I would like to talk about core melt progression, and as requested in your letter to us, give a summary of the current status of understanding and then comment on research needs and future plans.

[Slide.]

MR. WRIGHT: Melt progression, as I think most people know, describes the state of the reactor core from core uncovery up to reactor vessel meltthrough, including hydrogen generation. And in approved knowledge -- and I'd better get this one kind of straight -- of the state of the core at vessel failure will provide initial conditions for confirmatory assessments of the safety margins and the core melt threat to containment integrity. This is confirmatory research, and improved knowledge will give us the ability to assess the margins more carefully and improve our confidence in this assessment.

The output or the characteristics of melt progression are listed here.

The melt mass, which is a primary consideration in many aspects of the accident, particularly in all of your threats to containment integrity.

The melt composition, including the metal content, which Dr. Powers was emphasizing this morning, the importance this has on melt concrete interaction. It has major impact on DCH and on other things.

The melt temperature. In particular, we're concerned about the superheat, the margin above, we'll say, the liquidus, the liquidus/solidus.

The rate of melt release and then the time of release in a given sequence.

So this is the output that we're interested in, and this does have a major impact on the consequences and the results of severe accidents.

In addition, melt progression provides the conditions for in-vessel fission product release and attenuation and transport. It also provides the core conditions for use in assessing accident management strategies and their effectiveness.

In this presentation, I will only discuss the characteristics of the melt release from the reactor core, not continuing on through to vessel failure.

Can you see these bottom lines or not?

MR. KERR: Yes.

MR. WRIGHT: Okay. I need to know that further on.

I will not be addressing lower plenum melt cooling interactions or melt vessel interactions, but only what happens up to the release from the core.

MR. KERR: Mr. Wright, is it expected that this work will permit you to represent a worst case scenario, a representative scenario, most if not all scenarios, or none of the above?

MR. WRIGHT: I think that the results of the research are to give you a best estimate and a range of uncertainty from which the user can put conservative upper limits, and for many uses, that's all you need, it may turn out.

But it's to try to understand -- get an understanding of the processes and the ranges of these conditions for various -- the melt mass and so forth -- for various accidents.

MR. KERR: Okay. Now in this context, what do you mean by "the range of uncertainty", because I can convince myself that what would happen in a particular melt scenario might depend a lot on whether the core was at the beginning of cycle or at the end of cycle or the management strategy, a lot of other things.

When you talk about uncertainties, do you mean that kind of uncertainty?

MR. WRIGHT: No. I mean the uncertainty for a defined set of initial conditions.

MR. KERR: Okay.

MR. WRIGHT: And then you need to put in the other. If you're going to --

MR. KERR: Now is this --

MR. WRIGHT: -- include a range of conditions, then you have to spread them.

MR. KERR: Is this a defined set of initial conditions? See, I'm trying to understand how this eventually going to be used, and if the work is to be used primarily to verify the code, then the code will tell you what happens if you follow a preset set of initial conditions and a decided-upon core structure and so on.

But eventually somebody has to make a decision. Do we look at the worst case, the most representative case? I mean, how --

MR. WRIGHT: Let me jump ahead and try to answer the first part of your question.

MR. KERR: All right.

MR. WRIGHT: About how things get used, because Dr. Catton was asking that earlier also.

It seems to me that what we're doing here is developing a technical understanding of the governing processes. And then we are developing phenomenological models, simplified models, not big systems codes, but models of the governing behavior.

Then we can use that to assess the modelings that are in the current codes. Is it adequate? And "adequate" means not having all the details, but does it give the right range?

MR. KERR: No, but it seems to me --

MR. WRIGHT: Maybe you need parametric inputs that's sufficient for the codes.

MR. KERR: We will all agree, I think, that the codes will never be perfect.

MR. WRIGHT: They can't be.

MR. KERR: Okay. Now that then says that we need to design the codes for the way in which they're going to be used, insofar as we can. And if they're not perfect, we want their major contribution to be in those areas where we want to get information.

This, it seems to me, assumes that one knows how these codes are going to be used in licensing or accident management or whatever.

Now what I want to try to figure out is are you trying to design a code that will tell you something about the worst case or the best case or the average case, or have you decided?

MR. WRIGHT: In melt progression research we are not designing the codes. That is a separate question and maybe I'll buck that off to Farouk.

MR. ELTAWILA: The code that we envision that would be used for calculating the progression for any scenario identified by the PRA as a dominant scenario so whether it is unmitigating scenario or a mitigated scenario, so we'll try to provide data in the different area that if it is, let's say, a station blackout scenario, unmitigated, we want the code to be able to do that as much as it can do, a scenario that the operator can reflood the core and recover the accident.

It is a general purpose code for the melt progression. Does that answer your question?

MR. KERR: Well, it answers my question but it isn't the answer I expected to get because I think it is impossible to do what, if I understand what you are telling me to do, and to do it equally well for all cases. It seems to me therefore that you need to concentrate on those cases that you think are going to be most useful to you for something.

MR. ELTAWILA: If we want to do the worst accident scenario, we can just say that we don't need a code for that.

MR. KERR: No, I'm not suggesting the worst. I am saying you need to decide what it is you will want the code to do and where you need it to be most accurate.

MR. WRIGHT: I think I can respond in terms of melt progression and that is most risk significant accidents are the ones that we are addressing primarily. We would like to have the flexibility to be able to cover the range of conditions that are applicable and others that are risk significant but not the worst.

Now -- another point is that the difference between our technical basis for understanding and what is in the code is the codes can be simplified but you can't, you don't know where you are in your codes unless you have a technical basis for understanding and that is the purpose of the research.

I don't know whether I got there or not.

MR. KERR: Well, see, I don't know how much you need to understand in order to have a technical basis for the codes. Do you know how much you need to understand?

MR. WRIGHT: When I see assumptions on the melt mass from 20 percent to 100 percent, I think that we need to understand that better probably.

MR. KERR: But isn't it possible that you could have scenarios in which you got both of those? I mean you have got 20 in one case and 75 percent in another?

MR. WRIGHT: Yes. If you have the mechanistic understanding then you vary with scenario and you should be able to reduce your uncertainty range and in many -- there is a question of margin and there are questions of thresholds where factors of CA-2 are more in melt mass could be quite significant.

MR. KERR: It's clear that we can have core melt scenarios in which you get zero release and I would guess that one could postulate at least one in which you got almost 100 percent release so this whole spectrum -- I just say I don't see how a research program is going to tell you how much release will occur unless you specify a sequence.

MR. WRIGHT: Sure.

MR. ELTAWILA: You specify the sequence but you don't build a sequence dependent code. We are going to specify the sequence and we'll go through the physics in the code itself to see the outcome. If we have different sequence we hope that we build a robust code that can analyze other sequences and that is the hope, the target that we are shooting for.

We are not going to try to develop a code to analyze station blackout only because there are other scenarios that can happen in the plant and we are trying to be able to assess the consequences of these other scenarios.

MR. KERR: Okay, then what do you mean when you say you want a code that will tell you how much of the core material is released?

MR. WRIGHT: Under these conditions -- excuse me.

MR. KERR: I guess unless you are going to do something which depends on the amount of -- see, I am trying to relate this to something you are going to do either in the regulatory process or in the accident management process.

MR. ELTAWILA: Yes. Let's look at regulatory. Let's look at this perspective.

Let's say that for the purpose of assessing containment integrity and the PRA identified there are certain sequences that will be unmitigated, we want to look at these sequences and for that particular sequence we want to determine what is the material composition and temperature and so on that will enter the containment, what is the primary system pressure at the time of vessel failure if it fails, so we can justify the margin that existed in our analysis.

As I indicated earlier, we are assessing containment integrity separately but we'll try to specify what is the margin that exists so we can deal with it, that one use of the code, the determination of the composition and the thermal hydraulic conditions that will enter in the containment so we can calculate containment performance.

Now for other purposes, the code can be used for certain other scenarios that you have the potential for operating, interdicting into the course of the accident and try to add water, to depressurize the system, pour water on the core, flood it from the bottom -- all these are options to the operator in the control room.

We want to see what are the consequences of these actions that the operator will take in terms of the amount of hydrogen produced which can enter in the containment and effect containment integrity, the amount of the steam produced. We are trying to get a handle about what the consequences of operator action.

That is the second purpose of using the code, so not all the uses of the code is in determining the mass composition as it enters the containment. We are not assuming that every severe accident scenario is going to lead into lower head vessel failure.

MR. KERR: Please continue.

[Slide.]

MR. WRIGHT: As I think has been mentioned, the melt progression research is part of long-term confirmatory research. It's not short-term issue closure. Nevertheless, we think that it is important and appropriate to focus on the most important uncertainties, and we have tended to use the word "issues" here, with maybe two meanings, that require resolution, in order to provide a reasonable mechanistic understanding of the melt progression progresses.

So when we're talking about issues here, we're not talking about the short-term issues; we're talking about major uncertainties.

And in particular, unprioritized laundry lists of technical uncertainties are not useful in guiding research. You have to go through on what is significant and set up priorities and focus on the major issues in terms of risk significance. And we have endeavored strongly to do that. So we're focusing our research effort on two key issues for resolution in that sense, and then additional secondary uncertainties that we're monitoring but we're not currently doing research on.

And these two key issues stated in general terms are the conditions for the occurrence of a blocked core, such as happened at TMI-2. The negative is really the more operational end of this. Or the conditions in which you do not get blocked core, in particular, under BWR dry core conditions that we're going to come back to do you get melt drainage, metallic melt drainage from the core without forming core blockage.

The second one is ceramic pool meltthrough from a blocked core. The threshold of meltthrough and the failure location, because these determine the melt mass that has built up before you get failure in drainage from the core.

MR. KERR: Do those two issues have any bearing on accident management?

MR. WRIGHT: Yes. Because in the different times at which you reflood a core, you're dealing with different conditions. And in order to asses the consequences of your reflood, you need to know something about the state of the core at the time of reflood.

And if you have a blocked core with a big buildup of a melt pool, as at Three-Mile Island, versus drainage from the core, you're dealing with a quite different situation for reflood.

[Slide.]

MR. WRIGHT: The research needs in melt progression.

I think we've talked about this. We need a technical basis for determining the characteristics of the melt released. And I mentioned this earlier from the side mike. I think we have a major goal or prize, if you will -- that's not quite the right word -- for this research, in that Three-Mile Island it was observed that the melt released from the core was only 20 percent of the melt mass. It wasn't the 75 percent or 100 percent or 60 percent that is currently assumed in many analyses of severe accident behavior.

A major part of what we're doing is to try to get a technical basis to extend the very important experimental point, and it's more general -- I shouldn't say point -- general behavior that we observed at Three-Mile Island, find the range of applicability of these low melt releases masses, and also the low metal content, for our assessment of severe accidents in general.

So I think that there is a payoff in this research to try and make more useful the very important observation we had at Three-Mile Island. It's very important for the assessment of DCAH which is very sensitive to metal content as well as melt mass, for melt-concrete interactions, and so forth.

MR. KERR: In reference to the first sub-bullet, you will, as a result of this research, be able to say 20 percent is not the right number, what we get is something between 20 percent and 70 percent, let's say.

MR. WRIGHT: My reaction is different from yours in asking that question.

I think that I'm not concerned whether it's 20 percent or 25 percent or even 30 percent. I am concerned about the range of accident conditions over which this result is generally applicable.

MR. KERR: Which result?

MR. WRIGHT: The low melt release fraction, also the low melt content.

We have one result, Three-Mile Island. How generally applicable is that? We need to have an understanding of the governing processes in order to really with confidence be able to apply this information that we've obtained from Three-Mile Island.

MR. DAVIS: But Bob, correct me if I'm wrong, but Three-Mile Island was a pretty unique situation. They only had two to three months burnup on the core and they had also some water added to the vessel during the melt --

MR. WRIGHT: I'm going to come back to that, if you'd defer it.

MR. DAVIS: Okay. I'm just curious as to why you think that our assumptions of 75 and 100 percent are incorrect based on TMI.

MR. WRIGHT: I'm saying that we have -- let me come back to that.

MR. DAVIS: Okay.

MR. WRIGHT: Because I am going to address that point. And to do this generalized usage of these results, and I am assuming that we will find that the mechanisms -- I'm getting ahead of myself, because I'm going to come back to show that there is a wide range of validity. We need to have some validated models of what goes on so we can apply the models and not just say it was 20 percent at Three-Mile Island, but by the processes over a range of conditions, then we'd get these melt masses.

We also need to determine the accident conditions, if there are any, under which we do not get the blocked core by the metallic melt which occurred at Three-Mile Island, but get drainage of the metal from the core as formed and later on from ceramic melt from the core as formed and also from the BWR core plate.

And this is particularly important for the BWR dry core accidents where analysis is currently assuming that you do get drainage, that you don't have the blocked core, you don't get the big melt pool in the core or on the core plate, really.

MR. KERR: Do your studies up to date give you some information as to where it is most likely or less likely that you will get what you're looking for?

MR. WRIGHT: Yes. And we do not have experimental information for the BWR --

MR. KERR: No, I'm saying have you, is it possible to do analyses that say here is where we ought to be looking if we want to go to blocked core, here's --

MR. WRIGHT: Yes.

MR. KERR: -- where we want to be looking --

MR. WRIGHT: We've done that.

MR. KERR: Okay.

MR. WRIGHT: And that's what is guiding what the proposed research is, what we're doing, underway with.

In the drainage case, the behavior is quite different from what happened at Three-Mile Island. You form frozen debris layers in the lower plenum water according to the time of melting, and they build up with the lower melting material at the bottom, and then you boil it dry, you boil the lower plenum dry, and then you start to melt this, so that you get a large amount of metal, molten metal, down low, and you get a quite different result from what you see at Three-Mile Island.

And then when you fail the lower vessel head, what comes out is it's lower temperature melt, but it starts out as being mostly metallic and very, very different, and highly consequential result.

So this is the purpose, the importance of this issue. And as I said, the primary concern is the BWR dry core conditions following the ADS blowdown.

MR. KERR: Is it your expectation that you'll find that for some cores, say the BWR core, you will always get one of those and it won't be possible to get either or maybe even all three?

MR. WRIGHT: What happens in a BWR when you depressurize --

MR. KERR: No, I'm not asking you how you reach the conclusions, but --

MR. WRIGHT: The conclusion is, well, in a BWR, if you do not get ADS blowdown, it certainly appears that you get a blocked core, for the same reason you do in a PWR. And conversely, if you depressurize a PWR, it appears as if you go into the dry core scenario.

MR. KERR: Are you going to try to do research to confirm both or those or one of them?

MR. WRIGHT: We are taking the most important case, which is the dry core BWR, to assess the blockage or drainage int that case.

The wet core PWR -- we're getting ahead here, I talk about this a little bit later -- in the wet core PWR there is extensive information for many, many experiments in a number of different facilities that you develop this metallic core blockage, the TMI-2 blocked core. And that has nothing to do with the reflood. That's a consequence of a boildown with water level lowering in the core during the oxidation transient that melts the metal.

[Slide.]

MR. WRIGHT: I mentioned earlier the source term code package in March with its corium that I call "phony corium" of a so-called eutectic mixture of metal and oxide at a temperature that only exists for one particular composition.

What really happens is that we have several different types of material in the core with vastly different melting points and when they melt they relocate and in many cases freeze lower down.

You get a separation of the metallic from the ceramic components and this up at the top -- I don't know whether that may be blocked off -- we have the purely ceramics, UO-2 and ZrO-2 with melting points in the 2800 to 3100 K region.

MR. KERR: Unless you are going to use that to talk about something later, why don't we stipulate that information.

I think it is very useful and very interesting information and why don't you go on from there?

MR. WRIGHT: There are a couple important points.

I think it helps because it is behind a lot of other things.

MR. KERR: All right.

MR. WRIGHT: In the middle here, around 2000-2200K is zirconium, zircalloy, which melts at this intermediate temperature.

Then we have eutectic interactions of control rod materials with the zircalloy that melt in the lower temperatures first.

Now coming back up, in an accident when you get up from decay heat to these levels you start to lose your core geometry. The control materials start to go. In a BWR you may lose your poison.

You have the rapid oxidation transient that takes you up into the zircalloy melting.

Now these metallic materials have gone down in a boil-down accident with water in the core. They freeze out and form this lower metallic crust of blockage.

They have separated the metal from the ceramic and then later on you get up with a debris bed formation and melting the ceramic and you have separated the materials.

They behave differently later on and they give you very different results on vessel failure.

[Slide.]

MR. WRIGHT: I will show this just because it's prettier, but it's harder to read so we'll put on black and white.

This is the end state configuration at Three Mile Island and it illustrates the processes.

Here is the solidified region of the melt pool. After reflooding into the accident, 20 percent of the mass drained out the side of the core and down into the lower plenum head which was filled with water.

Below the pool is a metallic crust. Above it is a ceramic crust and here there is rubble and debris above the metallic crust and the cool and the hot center.

Then of course you have the relocated melt in the bottom plenum, in the lower plenum, which was water filled and water cooled.

Now there's several important points here.

One is that actually at the time of melt-out, the pool was bigger. It had about 50 percent of the core mass. the melt-out in this case was out the side of the core and it only drained half of the melt pool, so the location of failure is important.

The threshold of failure tells you how much melt mass you have built up before you melt out and that is why we are interested in the process of melt-out or melt-through of the ceramic melt pool through the metallic crust and, as we learned, around the ceramic melt pool there is also ceramic crust.

If we melt through early, we don't have much melt mass release. If we melt through late, then we build up a large mass which is suddenly drained.

MR. KERR: Is there some reason for you to believe that if you had exactly the same vessel core and operating conditions and went through this same scenario again you would get exactly the same formation?

MR. WRIGHT: If you will take the word "exactly" out, I would say yes.

This pattern of behavior of the buildup of the oxidation transient and the buildup of a block core with relocated metallic melt has been seen in all the PBF tests and the CORA test and the LOFT-FP-2 tests and the ACRRDF tests, also in a modified form in the NRU tests -- in everything that we have done but all of the data is for wet core conditions, of the boil-down conditions.

That part of it certainly has much, much experimental basis for believing it happens.

Now the other part of the detailed behavior of the melt pool, this is the only data that we have except, well, there is some small scale experiments that we are following on, that are predecessors of what we are doing now and there is analytical modelling that indicates this pattern of behavior in general, not necessarily in detail.

I think we would get a repetition, very close, and for the latter part I can't prove it.

MR. CATTON: That picture looks like it was pretty close to dumping it all straight down, if you look at that.

MR. WRIGHT: That -- somehow I wish Idaho would come through and fix this up. When you question the people -- fix that slide. When you question the people there they say that's misleading, that there was a -- this is where there was a burnable poison element and there was some depression but there was no indication it was even close to melt-through.

That was not due to -- it was due to materials interaction but it was not close to failure.

There is another important point to make here --

MR. SIESS: How do you know it wouldn't happen next time?

MR. WRIGHT: As I said, there is a large volume --

MR. SIESS: How would I know if I were on the outside trying to decide what to do?

MR. WRIGHT: Oh. You mean that it is not going to melt through here at the metal?

MR. SIESS: I assume it is important or you wouldn't be talking about it.

MR. WRIGHT: Well, I was asked the question and as I said, I wish they'd mask it out. I don't think it's that important but it --

MR. SIESS: Is it that simple?

MR. CATTON: Chet, it would have dumped 50 percent of the core.

MR. SIESS: Is the accident management that simple that we just look at it and say that's wrong and we don't have to worry about it?

MR. WRIGHT: When I asked the people that were doing the work, that same question. I mean I jumped on this immediately. They said no, the artist blew it up.

There was no indication they were even close.

As you will notice, there is not a depression in here and this shows you where the melt pool was and that this is some solidified material and it gets kind of iffy but there are no real indications that this was about to melt through.

MR. WARD: Well, Bob, were the experience in the ACRR and EBF and whatever else you mentioned, did they all involve breakout at the side?

MR. WRIGHT: They didn't get this far. They just get to the formation of metallic crust or the blocked core.

MR. WARD: Okay, so there isn't any other evidence.

MR. WRIGHT: I don't think they are quite completely blocked. They are getting right up to it, so this is our only data that got through in here to the melt pool and all that.

MR. ELTAWILA: Most experiments were terminated before a ceramic melt-through at locations so we did not really have any complete meltdowns --

MR. WRIGHT: Essentially the experiments fell apart and couldn't have been pushed into it.

MR. CATTON: Most of the experiments have been fairly small diameter so you wouldn't get the --

MR. WARD: So you really are dependent on this one observation.

MR. WRIGHT: For the melt poor behavior, TMI-II is it and it is of course full-scale.

MR. DAVIS: You wouldn't expect any difference for an end-of-life core where you have a different flux profile and a different --

MR. WRIGHT: Detail differences is our best knowledge, and I am going to beg a question about fragmentation. I would like to not get into this because it is a long story -- in the upper part of the core.

The high burnup fuel probably comes apart better sooner and I'll not go into this unless pushed.

MR. DAVIS: Bob, do you know how close the vessel was to failing? Has that determination been made?

MR. WRIGHT: The answer is very close and anything more I would defer to someone else.

MR. CATTON: The shape of that pool would depend quite a bit also on the peaking, because if the power profile is highly peaked, it would tend to penetrate more in the center and not so much toward the walls.

MR. WRIGHT: Oh, yes, yes. And it's also dependent on the natural circulation.

MR. CATTON: So the interest is to know what the profile looked like here, because if it was very flat --

MR. WRIGHT: Yes, it was pretty flat.

MR. CATTON: It was pretty flat?

MR. WRIGHT: BWR.

MR. CATTON: Then penetrating the wall, that's the reason for it.

MR. WRIGHT: Okay. Well, I've got another point here I want to make.

MR. CATTON: It would be nice to see, just for me the lines of constant power profile put on top of that picture.

MR. KERR: Don't do it, Bob.

MR. WRIGHT: I can't answer --

MR. CATTON: Not now.

MR. KERR: Don't do it. Go ahead.

MR. WRIGHT: Yes, how closely that matches. As you know, the natural circulation of the pool also affects this.

MR. KERR: What you see is what you get, Ivan. I'm sorry.

MR. WRIGHT: But there is a point I want to make, and that is that in Three Mile Island, the core was reflooded at 200 minutes, and the water level got clear up, very, very high. Analysis indicates that at that time you already had something like 20, 25 percent of the core mass molten. You already had this pool; it was just smaller. It got up to 50 percent before it melted through.

Reflooding the core did not stop the progression of melting. You didn't have enough surface area to pull out the heat in that big a volume, and it kept on melting until it melted out.

Now I think, Pete, you were asking me about, this is a reflood case, so why do you apply it to coolant boildown accidents?

Well, the phenomena, when we go back and look at it, look in general the same in a boildown accident witn the water level going down as it did with a steam bubble -- and I can't give you the interface on this -- I've been told it was about a foot below, but I think that's a very soft value.

But the general phenomena of meltout were similar, but with a great big question mark that I've only become aware of, I guess, in about the last three months. And that is, it's clear that when the core was reflooded at 200 minutes, the progression downward of the lower metallic crust as remelting, relocating, and refreezing was stopped, and the water level, and it was penned, so that you had good heat transfer, relative to the good heat transfer down, and under these conditions we know that the melting proceeded sideways out the side of the core to where there was no more core.

And what we don't know is, if we had had the boildown, whether it would have continued to just migrate downward and out, either fail the lower crust or eventually go out the bottom of the core. And then you would drain the whole melt pool.

So that's one of the areas of uncertainty, and that appears to be a major difference between reflood and the unrecovered accident.

So that's the general technical situation that we're going after in the late-phase melt progression work.

MR. CATTON: If you're going to incorporate that into some kind of modeling, you're going to need some pretty detailed understanding of the heat transfer process.

MR. WRIGHT: Yes.

MR. CATTON: I look forward to seeing how you're going to get them.

MR. WRIGHT: Okay. Well, I may not satisfy you, but that's coming up.

[Slide.]

MR. WRIGHT: Here is a brief summary of the current status of understanding. There are many things that we do know about melt progression. And this is broken down, and the correspondence is pretty good.

In the reasonably well-understood things, which are in the early or metallic melt phase -- and they include clad ballooning and the intact geometry oxidation and hydrogen generation here. The UO2 liquefaction or dissolution in molten zircalloy, which happens. There are many eutectic material interactions, particularly with the control rod materials, amongst the various components. Those interactions and governing rates are fairly well understood.

An important point is that the compartmentalized BWR core, which in the past was often assumed to maintain that compartmentalized structure through an accident, doesn't. Even early in the oxidation transient, you get a B4C stainless eutectic that then attacks the zircalloy channel box walls, and the BWR opens up like an open PWR core.

And an important point here is that the process of zircalloy relocation, which goes down to build up that lower blockage, is a very non-coherent, non-coplanar rivulet flow process with the fingers of melt penetrating different distances. We can see that visually in the CORA test. So that calculations of complete blockage by film flow are wrong.

And another point is that all the blockages that have been observed, short of Three Mile Island, are partially open. They're not plugged. So there is continuing steam flow, and there's continuing oxidation and hydrogen generation.

Then in the late ceramic melt phase, we have a more general understanding, not a detailed understanding, and it's based primarily, almost completely on TMI-2 core examination.

And we've talked about the general applicability of the results. The pool growth and meltthrough, the reflooding probably stopped the downward pool and crust relocation in getting the side meltthrough, limited melt mass, and the low metal content.

A third thing that is understood, and I don't know whether this is visible or not, the bottom one, is on reflooding a hot damaged core in the early phase. The reflood steam produces very strong oxidation and hydrogen generation and heating in the unrecovered upper part of the core, producing very high temperatures up to ceramic melting. The LOFT FP-2 test showed that clearly.

[Slide.]

MR. WRIGHT: Now we're going on into the future. It's useful as a roadmap -- not that I'm going to go into all this, but to just keep things straight -- to keep track that there are four different accident conditions that are relevant here.

The first is a wet core coolant boildown such as you had in TMI-2 before reflooding, where the water level is just lowered, and you get a blocked core like TMI-2, and all of our experimental database is for these conditions, not necessarily with a water pool, but with a steam flow and axial temperature gradients that are appropriate to boildown.

The second case of interest is a dry core from depressurization, and in U.S. BWRs, these are the emergency operating procedures.

On the dry core, as I have indicated, there is a question of metallic blockage or drainage, and that's one of the things we're after. And this is to be addressed by new Ex-Reactor experiments on the process of metallic melt, blockage, or drainage for BWR dry core conditions.

The third set of conditions is early phase reflood that promptly quenches the core, and that produces this oxidation by the reflood steam, hydrogen generation, fission product release, and also some fragmentation of the fuel. I'll say nothing more about this.

The fourth case, which for us is most important today because of what it did at Three Mile Island, and our interpretation of Three Mile Island is late phase reflood, which there did not stop the melt pool growth -- and there are conditions we know it won't stop meltthrough growth -- this is a TMI-2 case, and reflooding, an important thing we have discovered is that reflooding stopped the downward migration of the metallic blockage and that we got a meltthrough out the side of the core.

What we don't know is, if we had not reflooding, whether it would have gone out the side or out the bottom.

MR. SIESS: Is this a complete list of the accident conditions?

MR. WRIGHT: I'm sorry?

MR. SIESS: Is this a complete list of the, quote, different accident conditions?

MR. WRIGHT: Oh, I didn't put that much thought into it. I think it's logically complete.

I found that I was referring to these things too much, and it was better to put them on one slide.

MR. SIESS: So there is some uncertainty in this definition of four accident conditions?

MR. WRIGHT: Yes.

MR. SIESS: Have you quantified the uncertainty.

MR. WRIGHT: For example, I think it would be very useful to know the boundaries here in which reflooding will promptly turn the temperatures over and which it won't. But that's really not what I'm talking about today.

MR. SIESS: That's not what you're talking about. You're only talking about these four conditions?

MR. WRIGHT: Well, I'm mentioning that when you view the melt progression, it's useful to keep in mind that these are different conditions.

MR. SIESS: For me to keep in mind, or for you to keep in mind, or for the person managing the accident?

MR. WRIGHT: Well, of course, before he's managed it, he's dealing -- no. I think it's clear that the operator is going to put water in the core whenever he can. And the question is for accident management assessment knowing the consequences of that action.

And I think it's important to know that under Three Mile Island-like conditions, putting the water in doesn't stop the melt progression and does not guarantee that you will not melt through the vessel.

MR. ELTAWILA: It did guarantee that it did not melt through the vessel.

MR. WRIGHT: No, it did not. It did not guarantee it. It was very touch-and-go, and they didn't know it at the time.

MR. KERR: It must have guaranteed it, because it didn't happen.

MR. ELTAWILA: It didn't happen, yes.

MR. WRIGHT: No. At the time of the action, it didn't guarantee it.

MR. KERR: Well, it sure did.

MR. WRIGHT: No, at the time they reflooded it was not known, because it was --

MR. ELTAWILA: It did melt through, so --

MR. WRIGHT: It did not melt through, but that was later in time.

MR. ELTAWILA: That's fine.

MR. WRIGHT: It came awfully close.

MR. KERR: If they had known what you know now, would they have not put water in the --

MR. WRIGHT: Oh, no. They'd have put water in. Of course, they would have, you know, closed that block valve early on, prevented the whole mess.

[Slide.]

MR. WRIGHT: This viewgraph just illustrates these two different pathways of the block core sequence, like Three Mile Island, or the drainage sequence for -- assumed for a dry core BWR, and it goes through here with the block core, the metallic crust, ceramic debris bed, ceramic melt pool, pool meltout and so forth, and you don't get that with the drainage pathway.

[Slide.]

MR. WRIGHT: Now the first of these major issues to be resolved, and the terminology here is the conditions for the occurrence for a blocked pore, and as I have indicated there have been major disagreements on whether in the dry core accident sequence with BWRs, you have the metallic blockage formation or the drainage.

MR. KERR: Do they people who disagree, are they all experts on BWRs, or is it just the people who disagree don't know much about BWRs, or what?

MR. WRIGHT: Well, Steve Hodge is back here, and he is -- Steve is in one camp. And I think that a lot of us -- and that includes me -- are uncertain. And I don't know whether Steve wants to defend himself.

MR. KERR: Well, do you disagree because you're uncertain?

MR. WRIGHT: No, I'm --

MR. KERR: I see something up there that says there may be --

MR. WRIGHT: I'm uncertain whether we may have the answer. That's why we're doing the experiments.

MR. KERR: That says there are major technical disagreements.

MR. WRIGHT: Yes, I agree. I think that's accurate. Would you agree Steve, that that's accurate?

MR. ELTAWILA: Yes. The experience, the BWR experience, is concentrated at Oak Ridge National Laboratory. They have done more work on BWRs, more than any other lab. And that's why --

MR. KERR: Is there a major technical disagreement at Oak Ridge among the people who --

MR. ELTAWILA: No, no, not among the people. I think between Oak Ridge and maybe the people -- the people at Sandia, I think, might believe that you're going to get a blocked core situation exactly like you have in TMI.

MR. SIESS: So they will disagree, but isn't a result of uncertainty -- disagreement is a definition of uncertainty.

MR. WRIGHT: Do you want to say something, Steve, maybe?

MR. KERR: That's what we've been told.

MR. WRIGHT: Chris?

MR. KERR: Steve didn't write this.

MR. WRIGHT: I know. I would now change that maybe to "uncertainty" from "disagreement."

MR. KERR: Okay.

MR. HODGE: I'm Steve Hodge from Oak Ridge National Laboratory.

We've been engaged in studying boiling water reactors since about 1980, when the SASA program was first set up. At that time, Larry Ott was assigned the responsibility for writing code models which ultimately went into a code that some of you may have heard of called BWR-SAR, or boiling water reactor, severe accident response code.

This code predicts that the continuous relocation of material downward in the boiling water reactor, chiefly as a result of the fact that the core is completely dry, including the core plate and the water level is some foot, foot and half beneath the core plate.

Core debris, therefore -- and also based upon the fact that the BWR is much more open with the individual channel boxes and the open spaces for control blades in between the channel boxes -- lead to more proclivity for molten metals, which are the initial things that melt, to run down and continue load upon the core plate, overheat and fail the core plate, and fall into other water in the lower plenum where they are quenched. So that the metals enter the lower plenum first, followed later by the ceramic debris, and all of this material being quenched in the lower plenum of the debris bed -- lower bed of the reactor vessel, I'm sorry.

Obviously this is much different than what happened at Three Mile Island.

Now there are schools of thought that say that what happened at Three Mile Island would also happen in a boiling water reactor. And this is the disagreement, I think, between the two camps that Bob spoke of.

I will say -- Bob asked me, and I want to say something in my own defense -- and that was I really don't think I have a one-man camp. There is at least one person in the tent with me, and that's Larry Ott.

MR. WRIGHT: I might comment that I should have pulled disagreement out. I think it really is an uncertainty, and I think that we need some experimental evidence on this point, regardless of how one places one's bet --

MR. KERR: How many experiments will it take to determine whether this occurs or not? Suppose you run one experiment, and you don't get blockage. Will that do it?

MR. WRIGHT: It would depend on how thorough and completely it drained and if the conditions were right. I think we need a series of experiments.

MR. KERR: Two?

MR. WRIGHT: Well, we currently have planned or are talking about four. We have kind of two pretest experiments in simplified geometry to get it working. Obviously, if you get the answer after one or two and you don't need any more, you don't do any more.

MR. KERR: That's what I'm trying to ask. How many will it take to get the answer? Suppose you get blockage on the first one. Will that prove that blockage --

MR. WRIGHT: I think you have to look at the process and look at the conditions and where you are, and if you're at the edge of the parameters.

MR KERR: But you must, if you're planning -- if you're planning a set of experiments, you must have given some thought to how many you will have to run.

MR. ELTAWILA: You would usually try to run more than one experiment. I would say two. You know, you just try to see that you have not done anything wrong in the first experiment that biases your experiment in the direction one way or the other. So at least you try to see repeatability of the data. So I will assume two experiments will do it.

MR. KERR: Two experiments.

MR. WRIGHT: There is also a range of relevant conditions.

MR. KERR: Do you all agree? If you run two and they don't agree, do you run a third to see --

MR. WRIGHT: I would not propose, if I have the say on this, to run two experiments under identical conditions to look for statistical consistency. I think we're looking -- I don't think we're doing statistical experiments. I think we're looking for governing phenomena. And there are other parameters like melt composition and melt superheat and melt rate that I think are more important than trying to do a reproduceability check.

MR. KERR: I'm just trying to get your ideas. Your idea of statistics is somewhat different from mine. But I'm just trying to get an understanding of how many experiments it will take.

I mean, presumably you're trying to determine when a core blockage will occur. I'm just curious --

MR. WRIGHT: The conditions under which it will occur, which is different from the way you stated it. I can set up the conditions at which it will occur.

MR. KERR: Okay.

MR. ALLISON: May I make a comment? Chris Allison from Idaho National Engineering Laboratory.

MR. WRIGHT: Sorry, Chris.

MR. ALLISON: And I'm on the other side from Steve Hodge. Our speculation is that the BWRs will also block much like TMI. But it comes back to, I think, two basic technical issues.

One is, we know from the early melt phase experiments, both BWR and PWR, the control material melts out very quickly, both the PWR and the BWR. That melts out well before the fuel rods start to relocate. So that's one thing that we do know.

But as Steve pointed out, in the BWR when the control blade is gone, you have a much wider gap. So in my perspective, the technical issue is: Is there sufficient melt relocating when the fuel rods start to relocate, once the control rods have moved, to bridge that gap and form the crust?

The XR experiments are really designed to look at that, and I believe they can be designed in such a way that they can run the first experiment to an extreme to test one hypothesis or the other. And if they show that that test proves that, then perhaps they only need to run one other.

But I think that first test will be very critical, if it's designed in such a way as to emphasize blockage or not blockage.

MR. KERR: So one experiment, in your view, would do it?

MR. ALLISON: If it confirms that hypothesis.

MR. KERR: [Laughing] Wait a minute.

MR. ALLISON: Okay. If, for example, I make --

MR. KERR: How do you tell whether it confirms the hypothesis?

MR. ALLISON: Okay. If I make the hypothesis that the melt will bridge across the gap, okay, if that experiment is designed to, under those conditions which would show that -- or the experiment is designed such that either the material -- well, the temperature gradients are such that it won't bridge across the gap -- in other words, if you intentionally try to set up the experiment that if it's not going to bridge, it won't, and it ends up bridging after all, I think then you've proven the point that even under the most extreme conditions, it will bridge. That's my point.

MR. SIESS: Can you design an experiment to do that?

MR. ALLISON: Well, I think that's what Sandia is trying to do. They are trying to set up the temperature gradients to look at those conditions.

MR. SIESS: No. I'm sorry. I say, could you do it?

MR. ALLISON: Can I personally do it?

MR. SIESS: Yes.

MR. ALLISON: Certainly, with their help.

MR. SIESS: Yes. Now do you think anybody else would agree with you that that was the right way to do it?

MR. ALLISON: I think that that initial design will be a consensus design. I think that Steve Hodge and Sandia and a number of people will have input into that.

MR. SIESS: Do you think they could agree on the crucial experiment?

MR. ALLISON: Well, I think they --

MR. SIESS: As you would describe it.

MR. ALLISON: Yes. I believe that first test will be a consensus test.

MR. SIESS: Has it ever occurred to anybody that the testing may actually increase the uncertainty rather than decrease it?

MR. ELTAWILA: That's what we've been discovering, yes.

MR. ALLISON: There are always --

MR. SIESS: I mean, you've been making --

MR. ELTAWILA: The more we do experiments, the more we find uncertainty.

But let me go back to the Ex-Reactor experiment, and I will be blunt with you that the peer reviewers have not believed that this experiment is going to provide us with all the necessary information, and we're going to take a closer look at this experiment before we go ahead and conduct it. So we are not jumping into that, that this experiment is going to prove or disprove a hypothesis.

We're going to do it. We're going to look --

MR. KERR: Now Mr. Farouk, you know that we wouldn't accuse you of jumping into something without looking first.

[Laughter.]

MR. ELTAWILA: We'll try just to get the peer review and see if we really have the adequate justification for that experiment.

MR. WRIGHT: I might mention there's another approach to this, which is to look into the most likely conditions and test that for blockage, rather than going at what you think is one of the boundaries.

MR. CATTON: Do I understand the bottom line correctly, that the question really gets back to whether or not you bridge the space between canisters, the fuel canisters?

MR. WRIGHT: I think there's more to it than that.

MR. CATTON: What?

MR. KERR: He thinks there's more to it than that. That's what he said.

MR. CATTON: Oh, I was asking him to --

MR. KERR: No. You asked him a question and his answer was: I think there's more to it than that.

MR. WRIGHT: Yes. It's not just the gap in between the channel boxes. You also need to block within the fuel section, because the channel block walls go out, and then there is a subset of the core plate.

But that's more detail probably than we should be getting into.

MR. CATTON: I'm just wondering what kind of calculations you've done to try to point out what the specific problems are.

MR. KELLY: This is John Kelly, Sandia. I can address Professor Catton's comment.

We have performed some calculations to look at that question of spreading radially within -- between the fuel assemblies and within the fuel assemblies. And there's a good indication that if you do get initial metallic coming down and freezing, that subsequent material will spread radially and tend to bond the rods together and form that kind of cross-structure.

So we support what Chris Allison was saying, that we think that there is an indication that you will get bonding, basically form a support structure that will hold the rods together like we saw at TMI, and then that will eventually form into the crucible that we saw at TMI.

MR. CATTON: So then maybe it just tracks outlines of power.

MR. KELLY: Again, that's a significant factor, is that there are very low power regions in the BWR where you have the potential to at least initiate crust formation, and then the real key question is: Can it basically, as more material comes, continue to form crust, or does the material melt out due to gamma heating or other types of things?

MR. CATTON: Did you come to this conclusion based on freezing and remelting kinds of calculations?

MR. KELLY: That's right.

MR. CATTON: Okay. Thank you.

MR. KERR: I think this is going to be an interesting experiment if it is ever run.

MR. CATTON: I think a lot of people think so.

MR. WRIGHT: Let me try to finish up here. We talked about the experiment. Now, what it consists of is a full radial scale section of the lower quarter of a BWR core and core plate, with prototypic initial temperature distribution, which is applied by gas preheating.

At the top of this quarter, you dribble -- used to say pour, but that's misleading -- it really is a dribble --

MR. CATTON: Do you mean 90 degrees and full radius?

MR. WRIGHT: No. Of a section.

MR. CATTON: What's a section?

MR. WRIGHT: I'll show you on the next viewgraph.

MR. CATTON: Okay.

MR. WRIGHT: It's a partial section of one channel box, an appropriate gap, a bladed and unbladed gap.

You dribble in the pour. Now, this is what conceptual is coming from the upper three-quarters of the BWR core, and it has to -- you need to have the right Superheat and the right composition of the eutectic material, and that comes from --

MR. CATTON: How much uncertainty in dribble?

MR. WRIGHT: What?

MR. CATTON: How much uncertainty in dribble?

MR. WRIGHT: Well, we have the CORA test. We sit there with a camera and watch the dribbling, and from the size of the blockages and from the size of the transients, we can get some mass flow rate information.

There are uncertainties here. Sure. But we are not mocking up a full twelve-foot length. That was kind of beyond the capability.

But that's conceptually what we're doing, is dribbling into the lower quarter of the core, a mock-up, and, as I've said, we've taken the pours from CORA results and analysis.

I'll come back to --

MR. SIESS: Excuse me. You have a statement there that says --

MR. WRIGHT: -- the porous media modelling. There is additional information that comes --

MR. SIESS: Excuse me.

MR. WRIGHT: Okay.

MR. SIESS: The words are up there, ex-reactor.

MR. WRIGHT: Yes.

MR. SIESS: Could you --

MR. WRIGHT: Out of pile?

MR. SIESS: What?

MR. WRIGHT: Out of pile.

MR. SIESS: But inside the vessel?

MR. WRIGHT: Oh, it's in-vessel. It's an ex-reactor experiment of an in-vessel phenomena.

MR. SIESS: Okay.

MR. WRIGHT: Yes. Somebody didn't like out of pile a couple of years ago, and it got switched over.

MR. SIESS: All right.

MR. WRIGHT: All right. I'll go ahead and finish up here, although the order isn't the best.

One of the things we've been doing is assessing this melt relocation and blockage formation by a porous media modelling rather than the geometric descriptions, and the initial -- the porous media modelling requires less detail and the initial results have been favorable.

It includes -- this is for the metallic melt relocation and the freezing and the question of blocking fully across. It includes capillary effects, and I guess that's about all I should go into now.

MR. CATTON: You're getting down to a scale that you're never going to get done.

MR. WRIGHT: What? Oh. This has been running for over a year. We got good results.

MR. CATTON: You're going to get to a scale where you don't know what you're doing. You know, porous media modelling is straightforward. We know how to do it.

MR. WRIGHT: Sure.

MR. CATTON: What we don't know here is permeability, tortuosity, scale size, contact angle --

MR. WRIGHT: Well, you start --

MR. CATTON: -- and you're never going to know those.

MR. WRIGHT: Now, we have experiments, and again, the CORA are the best, where we have movies of the process. The trouble is those experiments give up, they lose their geometry before they get into this blockage phenomena.

So we have information on what the system is doing beforehand so we can kind of mock it up. I guess that's about as much as I should say in answer.

That is one of the reasons why I think, to have a robust result, you need a few tests more than -- probably more than -- certainly more than one and probably more than two to explore the sensitivity of your blockage results to these kinds of variations, like composition, which affects contact angle and things like that.

MR. CORRADINI: Bob, isn't it fair to say, though, that you're going to have to have a pretty dramatic -- I'm taking another attack at it. If you're going to hypothesize a result, you're going to have to have a pretty clearly unambiguous, dramatic, measurable result to say that this test is a success, or am I misinterpreting it?

I'm listening to what you are saying, and I conclude that unless something is clearly one way or the other, any ambiguous result is going to result in qualitative data that you really can't --

MR. WRIGHT: Well --

MR. CORRADINI: -- you can't dissect and understand from. Am I missing it?

MR. WRIGHT: I think that we're going to get -- we will see -- I think we will see either a drainage or blockage, and if it's -- I think we'll see how this system -- and I haven't talked about the complex geometry, the core plate -- how it behaves. I think we'll know so much more after that in our technical assessment of the problem, and either we may -- I think after the first experiment, we won't be able to say, "This is the way all BWRs behave under all conditions."

MR. KERR: Okay. So the experiment is not just being run to see whether you get blockage or not, but it's just run to see what happens.

MR. WRIGHT: No. I think we want to see, does it block or not, but I really think that the results will be quite meaningful.

MR. CORRADINI: No, but the reason I'm asking the question like this is, I've been listening to what everybody is asking you, and my sense of it is that here, you're coming into the middle of an accident. You're not sure what the initial conditions are. You are setting up some initial conditions based on some calculations.

MR. WRIGHT: Right.

MR. CORRADINI: Let's say you're successful in replicating those initial conditions and then you run the experiment. Unless you get a dramatic one-way-or-the-other result from that experiment, if I were trying to analyze the experiment, I'd be loss as to what to conclude because I have so much unknowns in terms of the initial conditions that I chose to represent in the single experiment or the two experiments or whatever.

MR. WRIGHT: Well, the first point, Mike, is that --

MR. CORRADINI: I'll just finish, and then I won't ask anymore about it.

MR. WRIGHT: Oh. Excuse me.

MR. CORRADINI: But what I'm saying is that if I'm trying to freeze a point in the accident or investigate a point in the accident that you think is crucial, probably just as crucial as the point in the accident is the stuff that happens just before to provide the information.

So if you have -- if you can't be sure of what those things are, you have to have a result in that experiment that is so dramatic in either direction that it's absolutely clear what the behavior is or your ability to diagnose what you just did and come away with a useful conclusion is pretty limited.

MR. WRIGHT: Okay. In the third case, which I think is the real one, the third case is that you have defined your initial conditions for the experiment, and you do the experiment that way, and you know the result of those conditions, and if you can have a reasonable model, then the model has a range of applicability, so that you can apply it over that range. So you don't have to have hit the conditions exactly on the nose. You want them to be close.

MR. CATTON: Do you have a model?

MR. WRIGHT: Yes. This porous media modelling I was just mentioning. We have done calculations on this system already. I don't have them with me and I don't know whether John Kelly does. He probably does not.

MR. KERR: We don't have any hesitation about the ability to calculate something with your model. That's not the point.

MR. WRIGHT: The question -- well, do you want to comment, John, on the model --

MR. CATTON: If you have a model that is anisotropic from permeability, you need to measure the anisotropic permeability in your experiment and that's very difficult.

MR. WRIGHT: No. You start out -- the initial condition is clear. It's geometry. It's defined. And then, as the experiment progresses, the permeability changes as according to the mass relocation and freezing.

MR. CATTON: You bet.

MR. WRIGHT: And that's what the porous media model does, and John will comment now in more detail than I could on it.

MR. KERR: Do you know what you're supposed to comment on John?

MR. KELLY: Well, I think it's on the modelling. We recognize what we have is a hypothesis on what the governing processes are in this melt coming down and potentially migrating radially.

We understand that there are uncertainties related to permeability and porosity and other factors in there, but we believe right now that we could estimate those things well enough to calculate the global response -- that is, the global melting and freezing and crust behavior -- to within acceptable uncertainties.

So the uncertainties in those individual things, we don't believe are going to be dominant at this point, but that's one of the purposes of running an experiment like this, is to validate that hypothesis.

MR. CATTON: I would think that the way you would validate hypotheses like this is you would do a very simple experiment on the table top and show that you can do it. Then you would go to this complicated system.

MR. KELLY: Well, part of the initial planning was to pour water through the system to get the hydrodynamic behavior correct first. So the thing you're talking about in this geometry, because this is very rod geometry dependent, we believe, this behavior of radial migration, so --

MR. CATTON: I understand that, and there are difficult things like, just what are the equations one should use in an anisotropic porous media. What kind of averaging do you do to get them? The number of questions within that field that you are talking about are immense, much less taking and doing this complicated problem without first trying to do something that gives your modelling some basis.

MR. KELLY: Well, let me say that we've used this modelling on other experiments, debris meltdown experiments in the past, and have had very good agreement with the results of the experiments. So we do have a basis, but --

MR. CATTON: I believe you get very good agreement when I consider the number of tunable variables in these kinds of processes, simple stuff like just change the relative permeabilities and the directions. That's all you've got to do. Change the coefficient in the expression you use to get the permeability. There are a lot of things.

If you're having phase change in melting, change surface tension. You're not even sure what it is that's running down the side of the thing to freeze.

I think you've got to do the simple kinds of things and prove your model before you go to th sort of an experiment. If you don't, then you've got lots of room for tuning and the uncertainty doesn't go away.

MR. WRIGHT: Ivan, on the initial conditions, we have the CORA tests which go through starting from the rod geometry and essentially, they fall apart just before we get to the blockage. So we're trying to take and extend them in a very real sense to get the next step.

MR. CATTON: But you see, you come from the top down, a very complex process.

MR. WRIGHT: We've done the metallurgy you see on the melt.

MR. CATTON: Why not come from the bottom up and demonstrate that your model works on something where you know the answers already?

MR. KERR: Gentlemen, let me suggest, I don't think a decision has yet been made as to exactly how to run this or where it will be run. I think you can gather that there are some questions on the part of the subcommittee, and since we have a good bit more to cover, I would suggest that we continue.

MR. KRESS: Let me ask one more question.

MR. WRIGHT: Ivan, you know, there has been a lot of concern about the questions you're raising, and we should talk to you more about where it's gotten.

MR. KRESS: The question, Bob, has to do with the term "anisotropic permeability."

MR. WRIGHT: Yes.

MR. KRESS: Are we talking about movement of melt downward between the rod spaces and radially?

MR. WRIGHT: And upward.

MR. KRESS: That seems to me like a misuse of the term "permeability" in terms of a porous medium. Could you comment on that?

MR. WRIGHT: I don't think so, but I'm not expert. I'm an experimenter rather than an analyst.

You do have anisotropic permeability --

MR. KRESS: It's anisotropic. I --

MR. WRIGHT: And it changes with time. What's wrong with that?

MR. KRESS: It's a misuse of the word "permeability."

MR. CATTON: You know, I don't have the foggiest of what he's talking about, then.

MR. KERR: Please continue.

MR. WRIGHT: Okay.

MR. KERR: Otherwise, we'll get Mr. Catton out of the fog.

MR. WRIGHT: There is additional information on this process of blockage formation from the CORA test, and part of what they are doing is going to be looking at some oxide film effects on wetting, which has not really been examined well yet. Then there's the final NRU test, which is a full-length test on metallic melt relocation in BWR core geometry. I'll mention that a little bit later. But that amounts to the work in this area.

Now, let me come back to a little on what this experiment is like, what I was trying to get at here.

[Slide.]

MR. WRIGHT: First, the geometry in the BWR core that we're talking about -- here's one channel box, here's one section of a control blade. There is a missing blade here. There is none. Then the next control blade is centered over here. Now, what we are modelling is essentially this much of the system, and in radial extent.

Now, what we're doing axially is the following. We're taking -- you'll notice that because of the BWR, the way they're -- the control rods and the boiling, the decay heat curve gives you an axial temperature distribution that looks like this. Different curves are for different points, the blade and in the box, but the important point is you have the high temperature gradient at which freezing might occur in the lower quarter of the core.

So what we're doing is, the experiment is this, and then we dribble in metallic melt at this point, corresponding to the characteristics that we get from the CORA test, metallurgical examination of the melt in the CORA test, so that we have the right material for contact angle. That's the general idea of the experiment.

[Slide.]

MR. WRIGHT: A cross section -- this is the real design of the experiment. Here's a blade around the corner into the unbladed gap, and the corresponding fuel rods and the fuel-rodded section on the other side of the gap, and the dribbling, the pour, into the -- the original pour is into the blade -- into the gap section, and later on a refinement, if we do more, very much, would be some pours directly into the rodded sections for melt coming -- zircalloy melt coming down that way.

This is the first test to see that one is doing them right, which does not -- and it's simpler. It does not have the rodded sections and will not have a model of the core plate region below as this does.

Again, the purpose of these experiments is to develop -- is to determine whether you get blockage or drainage in dry core accidents, particularly BWR, although if you depressurize a PWR, it looks like you're into a similar situation.

[Slide.]

MR. WRIGHT: I mentioned that we're doing the last NRU test. The purpose is to provide full-length data in BWR core geometry on the metallic melt relocation of blockage problem and on hydrogen generation.

There have been a series of NRU tests. These are performed in the Canadian NRU reaction. They are full length, twelve-foot-length test sections, but very narrow four-inch diameter test assemblies.

There will also be from this test important and unique information on fission product release and transport in the BWR core chemistry with the boron control blade. We don't have any data on that at all. There are two high burn-up rods in the test assembly. So I think this is worthwhile.

Okay. We don't need anything more. Oh. This test has been delayed by a breakdown in NRU, and it's to be early next year.

[Slide.]

MR. WRIGHT: This is a cross section of the NRU test. Here is the bladed section of gap. Here is an unbladed section of gap. These experiments, DF4 and the CORA BWR tests, were all done the same way. The box goes around the control blade rather than around the rodded section.

You have set different steam flow rates in the gap section and in the rodded section, and here are the two high burn-up fuel rods. They are 30,000 megawatt days per ton, I think H.B. Robinson.

MR. CORRADINI: Bob?

MR. WRIGHT: Yes?

MR. CORRADINI: What is the logic of the -- this is going to sound like an old question from years ago, but what is the logic of the full-length test given the fact that you're doing -- the plan is perhaps to do a series of non full-length tests, where the key -- I thought the key uncertainty or the key thing to get correct was the gradient in temperature down the length?

You wrote down or you had objectives of the test, but I didn't see that they were that strikingly different --

MR. WRIGHT: Well, I state --

MR. CORRADINI: -- than previous NRU tests.

MR. WRIGHT: Okay. Well, I state it a little bit differently. All of the previous work has been -- CORA is one meter long; ACRR DF-4 test, the first BWR test, was a half-meter long. We know that there are length effects in terms of zircalloy mass, and we've had no data with this. We will get this data from NRU.

Now, my own personal opinion is that --

MR. CORRADINI: This would be number what test out of the NRU tests?

MR. WRIGHT: Well, this is the fifth of five.

MR. CORRADINI: The fifth of five. And you don't feel that the previous four gave you this full-length effect clearly enough?

MR. WRIGHT: Well, we weren't in BWR geometry, which is a horse of a different color. You've got a lot more zircalloy, 50 percent more per fuel rod; you've got the open structure; you've got the boron carbide stainless blade with different eutectic interactions. They react differently, and certainly, there are significant length effects.

MR. CORRADINI: That was clear from --

MR. WRIGHT: Now --

MR. CORRADINI: That was clear from past tests?

MR. WRIGHT: Well --

MR. CORRADINI: What I'm asking --

MR. WRIGHT: Okay.

MR. CORRADINI: I'm not trying to press on the point now, but what I'm asking is, if you've done four tests, is it --

MR. WRIGHT: They were all PWR.

MR. CORRADINI: It's the same material. We're talking now relative masses.

MR. WRIGHT: No. No. The BWR, the -- you've got the control blade with the boron and stainless. You've got the open gap geometry and you've got a lot more zircalloy, and the blockage thing is, --

MR. CORRADINI: I understand.

MR. WRIGHT: -- as you heard on BWRs, that's important.

MR. CORRADINI: I understand. Maybe I'm beating this to death. If you have some knowledge, empirical knowledge from four previous tests, and there must have been calculations that have gone with this, I assume, what do those calculations and the previous empirical knowledge tell you should happen in this fifth test?

MR. WRIGHT: First of all, our modelling is real good in impact geometry, and, you know, oxidation, hydrogen generation, and when we get into relocation, our modelling is lousy.

MR. CORRADINI: Right, but just before, we were talking --

MR. WRIGHT: Now, there's another problem on these, and that is that the radial heat losses are large, and the interpretation has been difficult. That's why you will see me describing them, and not in terms of full length but in terms of length effect.

MR. CORRADINI: Is that a nice way of saying that the tests weren't what you hoped to be?

MR. WRIGHT: Well, all experiments have problems.

MR. CORRADINI: Yes, I understand that. I think I understand --

MR. WRIGHT: I'd rather leave it at that point.

MR. SIESS: Mike, when they cost this money, they are not tests, they are experiments. You should note that.

MR. WRIGHT: I don't know. Farouk, I'd like to just quit at that point on NRU.

MR. ELTAWILA: Yes. We can discuss that separately, the reason for the test and everything like that.

[Slide.]

MR. WRIGHT: Now let's get on finally to late phase melt progression. As we have indicated, in the blocked core accidents, the mass and the other characteristics of the melt that's released from the core are determined largely by the threshold and the location of failure of the pool supporting crust system, and actually it's a dual system.

You have the original metallic blockage, and then around the melt pool, you build up a ceramic crust around it, and as the pool grows, that crust grows. What we're interested in is the failure threshold and location of that complex system, getting some data on it.

As was said, TMI-2 failed out the side; only got half of the pool mass out. But at TMI-2, we had the reflood water below the crust, so that downward relocation of the blockage was stopped.

So with TMI-2, whereas it gave general information on the behavior of blocked core accidents, I think in this detail it's probably not representative of the unrecovered accidents, although our understanding is not good enough to make definite statements.

MR. KERR: Again, how many experiments do you think it will require to get the information you think you need?

MR. WRIGHT: Well, we have done one which was a partial success, and the plans are to do one more, which has been constructed and will be performed early next year.

MR. ELTAWILA: After it's peer reviewed.

MR. WRIGHT: Yes. The peer review will then examine the results and the question of the direction for further work in this and make decisions on what should be done next.

In my view, this is not too surprising. When you have very little data, you get huge technical disagreements. When you get a lot of data, then the disagreements tend to go away, and --

MR. KERR: I don't doubt that --

MR. WRIGHT: -- various people think we should be doing various things.

MR. KERR: Okay. Suppose that the next one is not entirely successful. How many --

MR. WRIGHT: Well, I'd say there'll be no more.

MR. KERR: Okay.

MR. KRESS: Bob, could you explain one word in that to me? What do you mean by the threshold of failure?

MR. WRIGHT: Well, the pool mass at the time you meltthrough; when it happens in a time scale of pool mass, if you will. Do you meltthrough when you have, say, ten percent of the core in the melt pool, or does it go up to 80 percent before it melts? That's the threshold. When it melts through is the threshold. Maybe I'm not using the language right. I don't know.

There are three major determinants or inputs on this failure threshold as it appears to, I'll say, to me. One is this question of whether boil down is lowering the core water level or whether it is fixed by reflooding as it was at TMI-2.

The second is the surface heat flux distribution from the internally heated melt pool because of a natural circulation within the pool. That determines, then, the thermal loads around the boundary of the pool.

The third one is the actual process of meltthrough of this complex structure, which is the pool, the ceramic crust intervening debris bedded until the pool expands at least into the metallic crust. This structure is supported by the fuel rod stubs, and they are very strong and very massive.

It's behavior of that system that we need to be able to model in some sense to determine this threshold. We don't need a high precision, but we need to know the governing phenomena, what's really happening.

Now, the process that seems to be happening is that as the pool expands the crust, it melts from the inside and gets thin, and then it melts through and you get ooze coming out and then refreezing, so that you get a relocation downward, a remelting and a relocation and a refreezing.

We see that in metallic crusts in still is what early phase melt progression and in their formation, but we don't -- we can't describe quantitatively this process in any reasonable way. We don't know what's running the show.

So that is the third point here, and that is the one that we are currently investigating experimentally.

One point here that's significant is that the material interactions between the control rod material and the zircalloy and the zircalloy cladding on the rod stubs and potentially between the ceramic crust and the metallic crust, these material interactions play an unknown effect, but they -- we don't have an idea of what's governing here, but they may well be -- they appear to be significant, and a little bit of data from PIE can be a big help in this regard.

MR. KERR: Bob, my agenda shows your finishing about 3:30, and when I look at the clock and your set of slides --

MR. WRIGHT: I'm close.

MR. KERR: Okay.

MR. WRIGHT: Yes. At 3:30, I think I'll be finished.

MR. KERR: All right.

[Slide.]

MR. WRIGHT: There are several viewgraphs here on why we're going at this problem the way we are, and, as I say, when you don't have much information, various people are going to have various ideas.

But here is the current approach we're using. One part of the problem is the melt pool thermal hydraulics, and the question here is, is the existing database from low Rayleigh numbers and in rather not the best geometry adequate for our need in late phase melt progression?

We're proposing to have -- to do some analysis on this and then have it peer reviewed to see whether more work was needed. We're not proposing at this time to do more work, although if an experiment is needed, it's not that big a deal. It's about a one cubic meter heated water pool kind of experiment to get Rayleigh numbers at ten to the 16th.

What we are working on, part of the problem -- gee, I missed a viewgraph in here that was important in the logic. All right. Let me break in right here and just give that one verbally.

The late phase melt progression is a difficult problem. It is a whole core phenomena, and we do not have the luxury of going out and melting down a whole reactor. We have these component parts of the melt pool thermal hydraulics and then the mechanisms of failure of the supporting structure.

In this situation, we have no choice. We are forced to break the problem down and do it in pieces and look at the pieces, and that's what's being done here.

I should also point out that that's not unique to late phase melt progression. You have the same problem on, say, any experiments on the melt attack on the lower vessel head, including the natural --

MR. KERR: Why don't we stipulate that it's difficult and that it's not the first time, and tell us what you're going to do.

MR. WRIGHT: Okay. But this is not unique, breaking the problem up into pieces, and it's often ignored that small scale pools, you cannot get the convection with internal heating that scales right, so you have to do it in pieces.

All right. We are performing two of the experiments in ACRR, the MP experiments on the process of this meltthrough, the cross relocation and failure dynamics.

The MP-1 experiment was performed in December '89, and MP-2 is scheduled for early 1992. I'll describe these experiments next.

In addition, we have the debris porous media model of late phase melt progression, which is different from the metallic melt relocation of blockage porous media modelling although the physics are similar.

This treats the ceramic phase. It treats a debris bed, a ceramic particulate debris with a growing melt pool. It does the melt relocation if things freeze. They freeze, ceramic crusts automatically form. You can put in the metallic crust, you can put in the structural geometry.

This has been the phenomenological tool that we have used thus far in approaching this problem, and I will show you how well it fits in the MP-1 results in just a minute.

Now, the debris modelling -- this isn't a code. This is -- well, you run it on a computer, but it's not a code development effort. Although the original physics was put together as a part of MELPROG, it's no longer that, and it's used to address the phenomenology. In particular, in its current form, it incorporated the MP experiment boundary conditions, initial conditions and structures and so forth.

So what we're trying to do is get a validated model of the governing phenomenology. Then, with that validated model, you can -- we will be in a position to assess the modelling that's in the codes, the current codes -- in particular, SCDAP/RELAP5 -- and see if it is adequate.

If it is not adequate, than something, some of the phenomenology from the experiments and the debris or other modelling, would be incorporated into the codes. It might be incorporated simply as parametric inputs. It might be incorporated as simplified models. But I think the important thing is that you have a technological basis that goes beyond what you have in your simplified systems codes.

[Slide.]

MR. WRIGHT: All right. This is a description of these two experiments, and we talked about what the purpose is, and it's to get the information for determining the melt mass and other characteristics on meltthrough from a core.

The experiment is a particulate ceramic debris bed inside it, which a growing pool grows from fission heating of the UO2, the UO2 in the debris. The UO2 is dissolved in the UO2 crust. The UO2 in the fuel rod stops.

Fission heating provides the internal heat generation which is necessary to describe these processes. You can't really do them in a furnace because you need the temperature distributions to drive the melting and the freezing and the relocation behavior.

The experiment has extensive characterization of internal temperatures with special high temperature thermocouples, the high purity thermocouples which we've had survive on MP-1. None of them fail, although the melt pool -- we had a substantial melt pool for over 20 minutes. They are very, very good. From those, you back out the temperature distributions.

Most of the information comes from the PIE. You cut it up, you examine it, and then you look at the material compositions.

MPI had a eutectic zirconium UO2 metallic crust. That's -- I've forgotten -- that's about ten mole percent UO2 in the zircalloy -- that number ten is off somewhat -- with a melting point around 2100K. MP-2 has a prototypic TMI-2 metallic crust, including the control rod materials.

MP-1, we had an early shutdown because in the design of the experiment, we reached the safety limit. I'll show you a wall. It turned out the insulation, the Zr-O2 insulation had a lower conductivity than had been tested. Of course, we were running it way beyond limits at which tests had been run. So we had an early shutdown and did not get as far into the accident condition as we would have liked.

As I said, following the performance of MP-2, the Melt Progression Review Group will analyze the problem, the results of MP-2, do reanalysis, and decide on the further continuation -- what we're going to do in late phase melt -- continued melt -- continued work on late phase work progression.

[Slide.]

MR. WRIGHT: Sandia furnished me this colored viewgraph -- we don't make things like this at NRC --

showing the layout of MP-2.

Here is a particular debris bed. There are 32 rod stubs, which is the size of the PBF experiments. Here is the precast metallic crust with the control rod materials and the dissolved UO2 in it, and the thermocouple array is shown in these black dots from which you can block out, and it's done on line what the temperature distribution is during the experiment on making decisions on operation of the test as well as for post-test analysis.

On MP-1, this tantalum shield got up to its pre-agreed limits, so they had to shut down, and the design has been changed so that won't happen again.

MR. KERR: Move to your conclusions as soon as you can, please, Bob.

[Slide.]

MR. WRIGHT: Right. I really am. The results of MP-1 -- here is a cross section with the debris, and here is the ceramic melt pool, the gap from densification. Of course, this is non-prototypic because the debris bed is held up by the side of the pool.

Here are the rod stubs, and the metallic debris bed that was just reaching -- the metallic crust reaching melting at its surface, and over here are shown the calculated by the debris model and the measured densities in this system with the black is the calculated, and then this is the measured material density distribution.

There is also one I won't show out -- two-thirds of the way out to the edge.

[Slide.]

MR. WRIGHT: This is a list of the current uncertainties and where research is being done on them, both in the conditions for the occurrence of the blocked cores, and there is work, much of it not extensive, on remaining uncertainties, and then in the late phase melt progression, the ceramic melt phase. I will not go into that unless somebody asks specifically for details. It's available.

[Slide.]

MR. WRIGHT: We were asked to give you some comments on the applicability of the research to ELWRs and ALWRs. This summarizes that situation.

Essentially, the general phenomenology of melt progression is directly applicable. To the extent that mechanistic models are available, they can be used directly for the advanced reactors. Particularly the core heat-up and hydrogen generation models are directly applicable, all the fixed geometry stuff.

MR. KERR: Bob, what we had hoped to get was how they were going to be used. What you are giving me is how they might be used, and perhaps that's all you can say. If so, that's --

MR. WRIGHT: I'm not that close to those programs. Farouk, would you have any comments?

MR. ELTAWILA: That's again NRR, and we would like to have another session on that.

MR. KERR: I am not trying to ask for the impossible.

MR. ELTAWILA: Okay.

MR. WRIGHT: I am not very knowledgeable.

MR. KERR: Okay.

MR. WRIGHT: I'm not a good one to answer.

There are some code modifications that --

MR. KERR: Let's just assume that these give one an idea of the way they might be used.

MR. WRIGHT: Okay.

MR. KERR: And at this point, nobody knows --

MR. WRIGHT: That's all I know.

MR. KERR: -- how they will be used.

MR. WRIGHT: And I might say that the lower power density does have some effects that are listed here on the lower steaming rates, lower heat-up rates, somewhat lower peak temperatures, and you're going to get lower metallic and ceramic melt masses.

There is some chance -- I don't think this -- there is some chance that you might not even get the TMI-2 blocked core melt pool, meltthrough scenario, although I'd bet a beer that you would, but it needs to be looked at.

That's it. I thank you.

MR. KERR: Thank you, sir. We will now take a 15-minute break and convene again at ten minutes of.

[Recess.]

MR. KERR: We are ready to begin with direct containment heating at future plants. Mr. Tinkler, Mr. Shotkin. Mr. Shotkin.

[Slide.]

MR. SHOTKIN: I will be talking about the accident management strategy of depressurization.

[Slide.]

MR. SHOTKIN: First, what I'd like to do is put this in perspective of the DCH issue.

MR. KERR: Whose perspective is this?

MR. SHOTKIN: This will be my attempt to give the ACRS a perspective of where the depressurization strategy fits into the DCH issue.

MR. KERR: Thank you.

MR. SHOTKIN: There are three main questions related to resolving the DCH issue. The first looks at the likelihood that there could be high pressure melt ejection, and whether this could be reduced by depressurization.

When I talk about depressurization, I will be talking about both intentional depressurization and unintentional depressurization caused by natural circulation.

A second question is, if depressurization does not occur, will there be high pressure melt ejection, and can this lead to unacceptable containment loads due to DCH?

The final question relates to, what is the risk assessment of the depressurization strategy which looks at not only depressurization, but also probability of many other things occurring while the operator may be thinking about this strategy.

This talk will discuss Items 1 and 3, and the other talks will concentrate on Item 2.

[Slide.]

MR. SHOTKIN: Another way to look at this is that there are three phases that can affect an early containment failure from DCH, and this tries to lead you into the way we're using risk to look at this, or probability.

The first question is, what is the probability that there could be a core melt sequence that keeps the reactor coolant system at high pressure? As you might imagine, there's very little probability that this might occur.

The second, and the second box is where I will be concentrating, is what is the conditional probability that the sequence stays at high pressure? In other words, that there is no depressurization.

Finally, if there is high pressure melt ejection or vessel failure, what is the conditional probability that the containment fails due to DCH?

In this third box, there are contained several conditional probabilities which I won't get into. There's the conditional probability of high pressure melt ejection, conditional probability of direct containment heating, conditional probability that there could be hydrogen overpressurization, and finally the structural response itself.

The no depressurization conditional probability consists of two parts, and first is the failure to passively depressurize, which means that outside the operator's control, there could be large pump seal LOCA, there could be a stuck open relief valve, and there could be natural circulation temperature-induced failures, which you have put on the agenda as natural circulation.

Second, there's the failure to intentionally depressurize. That's an operator action.

If the product of these three probabilities is low enough, then the DCH caused containment failure is of little concern, and as we try to resolve the DCH issue or put it to bed, we will be looking at it in a way similar to the way this viewgraph has it.

[Slide.]

MR. SHOTKIN: First, let me talk about intentional depressurization, and this is where we have concentrated a lot of our work over the last few years.

We have used the SCDAP/RFLAP code to make several calculations using the Surry plant as a prototype, and we're looking at the efficiency or efficacy of depressurization during a station blackout transient.

What we found, the conclusion was that the Surry type of plant can be depressurized to approximately containment pressure by latching open two PORVs. So for Surry, the strategy of accident management -- the accident management strategy depressurization looks to be a viable strategy.

MR. KERR: What is meant by the expression "latching open"? Does that just mean opening them and keeping them open?

MR. SHOTKIN: Yes. Opening them and keeping them open.

MR. KERR: Thank you.

MR. SHOTKIN: From the control room.

There are some subtleties to this conclusion. There are two times when you can open the PROVs. First, there is what we call the early depressurization, where the operator initiates the strategy at the time of steam generator dryout, and the second is the late depressurization, which is initiated at the time of core uncovery.

What we have found is that late depressurization is the preferred strategy. It may be counter-intuitive, but the results show that it is the preferred strategy. Why? Because it provides longer time for good things to happen, like AC recovery; the operator can bring in alternate cooling. It also delays the core damage because there is a slower inventory loss and a more effective accumulator injection when the accumulators do come in.

However, one disadvantage to the strategy of depressurization is that steam explosions may occur as the plant is depressurized.

What we have found, though, doing some scoping calculations -- and I see the person who did them is out of the room -- probably just as well to avoid conflict of interest -- but preliminary calculations show that they may be benign and they may not cause a vessel failure.

MR. CATTON: The Germans have considered the same thing because they depressurize their plants, and they have actually calculated loads and are talking about designing their new vessels to accommodate them.

Is this conclusion different? Are they just being conservative, or what?

MR. SHERON: Dr. Catton, if you remember, the

Germans have a sealed lower cavity.

MR. CATTON: I'm talking just about the --

MR. SHERON: They have a high pressure melt ejection.

MR. CATTON: I understand that.

MR. SHERON: Okay.

MR. CATTON: I understand why they depressurize.

MR. SHERON: Okay.

MR. CATTON: It's this last conclusion --

MR. SHOTKIN: Oh, the steam explosion.

MR. CATTON: -- about the in-vessel steam explosion.

MR. SHOTKIN: Oh. Okay.

MR. CATTON: They feel that it's serious and I'm wondering what the differences are. Are they just being conservative?

MR. ELTAWILA: I think it's conservatism in the analysis.

MR. SHOTKIN: From what I have seen of steam explosion analyses --

MR. CATTON: I happen to agree with the conclusion.

MR. SHOTKIN: And the gentleman sitting next to you can confirm the other direction, the one who's not there.

MR. CATTON: Corradini.

MR. SHOTKIN: There are a lot of uncertainties in the ability to predict steam explosions, and based on these uncertainties, we feel that there could be very benign -- there could be a steam explosion, but it will be very benign. But because of the uncertainties, the Germans might have come up with a different conclusion.

MR. WARD: Yes. Well, their conclusion isn't necessarily radically different from yours. I mean, you say may be and may be, you know. So that's kind of -- I mean, your last bullet lacked force.

MR. SHOTKIN: What we'd like to stress is that even though there may be steam explosions, the strategy of depressurization, at least for Surry type plants, is a very viable one, and we would recommend that industry might want to consider that. There are a lot of advantages, and we are pointing out steam explosion as a possible disadvantage.

You know, as we talk of all of our strategies, part of the Commission directive is that we do point out what are the down sides to the strategy.

MR. WARD: Yes.

MR. SHOTKIN: This is one potential down side.

MR. WARD: What do you expect industry -- I mean, what would you expect the people at Surry to do about that?

MR. SHOTKIN: What is going on in accident management is that the industry is putting together a technical basis report where they are looking at this strategy. They are coming up with the positive, the pros and the cons of the strategy.

They are now giving it to the Owners Groups and the Owners Groups will perfect specific strategies that they will then give to the utility, and we would expect the utility as part of their accident management program to strongly consider this as one of their strategies.

MR. WARD: On the previous chart, where you said one of the advantages of late depressurization is it provides a longer time for the -- well, delays core damage because of accumulator injection. Why don't you get accumulator injection if you depressurize early?

MR. SHOTKIN: You do. I have some backup viewgraphs. Actually, I'm sort of glad you asked that question, if you wouldn't mind listening to some more, if you want a good answer to the question.

MR. WARD: Sure. I expected one.

[Slide.]

MR. SHOTKIN: If we look at the difference between comparison of early and late depressurization cladding temperatures, this the late depressurization and this is the early depressurization.

What happens during the early depressurization is that you lose more fluid from the system sooner, so the temperature goes up quicker than in the late depressurization.

Why it doesn't go up as high is that by the time you start the accumulators going on in the late depressurization, the system is more filled with steam, and when the cold water comes in, it leaves a condensation-induced rushing of the water up into the core and cooling it much sooner or quicker.

MR. WARD: Okay.

MR. DAVIS: Excuse me. I can think of another reason why late depressurization may not be as good as early depressurization.

That is that if you let the steam generator dry out, the tubes will begin to heat up, and at the same time, the pressure on the primary side goes up to the pressure relief point, and now you run the risk of a steam generator tube rupture, which can cause a release directly to the atmosphere through the atmospheric dump valves. If you were to depressurize early, that possibility would be, it seems to me, greatly reduced.

Is that an invalid point on this issue?

MR. SHOTKIN: Let's see.

MR. DAVIS: Do you want me to go over it again?

MR. SHOTKIN: No.

MR. DAVIS: Okay.

MR. SHOTKIN: I think it's valid. I'd have to look at the numbers and the timing in order to give you a good answer. I don't have that information on the top of my head.

MR. DAVIS: Okay. I just wondered if it had been considered when you looked at the choices.

MR. SHOTKIN: Have we done anything on that, Duane?

MR. HANSON: My name is Duane Hanson from the INEL. We did look at the heat up of the steam generator tubes, and in both cases, both for the early depressurization and the late depressurization, the temperatures in the steam generator tubes stayed fairly low. So we would think that the probability of failure of those tubes would be about the same for both of those accident conditions.

MR. DAVIS: I guess I'm a little surprised, but I'll let it go.

MR. CATTON: That's only with a great deal of uncertainty that you can say that.

MR. HANSON: There's some uncertainty. I'm only talking about the temperatures, I'm not -- whether the steam generator tubes -- I'd say the likelihood of --

MR. CATTON: It's a RELAP prediction, isn't it?

MR. HANSON: This particular case is a RELAP prediction, but what I was speaking about are some additional calculations that were done on natural circulation on the hot leg. There's an amount of uncertainty in those calculations; yet, the steam generator tube temperatures do stay fairly low compared to, say, the hot leg or the surge line.

MR. CATTON: The early calculations that I've seen that were done under an EPRI contract at SAI showed that they got very hot.

MR. HANSON: I guess I'm not familiar with those calculations.

MR. CATTON: So I think there's quite a bit of uncertainty.

MR. HANSON: There is.

MR. KERR: Mr. Siess.

MR. SIESS: Yes. I'd like to go back to your answer to Mr. Ward's question. I apparently missed your explanation of why accumulator injection causes core damage.

MR. SHOTKIN: Let's see. Did I say that?

MR. SIESS: Yes. It delays core damage because of accumulator injection.

MR. SHOTKIN: It delays the core damage because there's a --

MR. SIESS: Do you want to reword the sentence maybe and try it on me?

MR. SHOTKIN: Sure.

MR. SIESS: What you have up there says core damage because of accumulator injection, and if that's not what it means, could you find some other words?

MR. SHOTKIN: Yes. Let me read what I actually said. I had it written down. It delays core damage because of a slower inventory loss and more effective accumulator injection.

MR. SIESS: Okay.

MR. SHOTKIN: Were there any more questions?

[No response.]

[Slide.]

MR. SHOTKIN: Continuing, concluding with the intentional depressurization, of course, we believe that EOP modifications may be necessary to reliably accomplish depressurization beyond what exists in plants today.

We also think that the support systems, such as the batteries for electricity and air systems, may need to be looked at to make sure they extend over the transient duration to ensure that the PORVs will operate as necessary.

Now, what I have done up until now is just given you examples for one plant, the Surry plant. What we have done is a simple extension of the Surry depressurization results to other PWRs.

What we did is we divided the PWRs into five groups: those Westinghouse plants that had a PORV capacity larger than Surry; those Westinghouse plants that had PORV capacity less than Surry; CE plants that had PORVs; B&W plants; and then CE plants that did not have PORVs. Of course, the CE plants, Combustion Engineering plants that don't have PORVs, this strategy is not a viable option.

We looked at which plants would respond favorably to the strategy, which plants the strategy would not work at all -- of course, that's the CE plants without PORVs -- and which plants would need further analysis to confirm the effectiveness of the strategy.

In summary, most of the Westinghouse plants -- and the way we did it, we looked at not only PORV capacity, but the ratio of PORV capacity to system volume and to core power.

What we found was that for most Westinghouse plants, this is a viable strategy. For the CE plants with the PORVs, this is a strategy that could be considered but would require further analysis to confirm its effective. For the B&W plants, we are concerned that this strategy might not be viable, mainly because of their PORV capacity.

What we are doing is, to conclude this, is we are extending the analysis to the deterministic analysis of depressurization in a B&W plant, and we have initiated that.

We're also going to do a Combustion Engineering plant, since there were some questions of whether it might work for CE.

That completes what I had to say on the intentional pressurization.

MR. WARD: Okay. Now, the self depressurization, though, you know -- if the PORV depressurization isn't viable for the B&W plants or for the subset of CEs, the self depressurization to the surge line overheating or something is -- hot leg, I guess it is --

MR. SHOTKIN: For the B&W?

MR. WARD: Yes. Is that viable?

MR. SHOTKIN: Okay. That's a question that was brought up. We had a review group look at what we had to do for unintentional depressurization.

MR. WARD: Yes.

MR. SHOTKIN: And the question came up, Why didn't the surge line fail during the TMI accident?

MR. WARD: Yes.

MR. SHOTKIN: What we've done -- you know David Bessette. He's been working the better part of the last year looking at the thermal hydraulics of the TMI accident, but just the upper part, you know, not looking at the core and the lower plenum, but looking at the flows in the upper part to see whether there could have been -- why there wasn't a surge line failure at TMI, which is a B&W plant, and whether there could be a surge line failure.

He's preparing that for dissemination, but what his results were that the reason that there wasn't is that people got -- the water got in early enough. If there was a delay in getting the water in, the temperatures probably would have gone up high enough and probably the surge line would have failed at TMI. It was just a question of timing.

MR. WARD: Okay. But what about the CE plants without PORVs?

MR. SHOTKIN: That's a tough one. We haven't faced that one yet. They don't have PORVs, they can't depressurize, and we just leave it --

MR. WARD: Well, what about the surge line, though? I mean, is there --

MR. SHOTKIN: Okay. That's why we're doing this analysis. We're analyzing the B&W plant. I should have added, we're also analyzing a CE plant to look at that.

MR. WARD: Oh, okay.

MR. KERR: Back to the results of this, which will be made available to the plants, what are the plants then expected to do with those results or that information?

MR. SHOTKIN: Now I'm going into the accident management program, which was not part of this committee meeting, but I'd be glad to discuss it.

Again, what's going on is that the industry has the lead in putting together the technical basis for strategies -- they call them high-level actions -- that right now EPRI and NUMARC are working on some reports that they will make available to the Owners Groups.

The four Owners Groups will then take these technical basis documents with the proposed strategies and try to make them into viable strategies and procedures that will be made available to the utilities. Then each utility will look at these and decide how they want to use them in their plant.

What NRC plans to do is to put out a generic letter on accident management. It will probably be delayed. It was originally scheduled for summer of next year. It might come out towards the end of next year.

What we would like to do in the best scenario, we would like to endorse the industry work that is going on now with NUMARC, EPRI and the Owners Groups. But they are looking at strategies like this.

MR. KERR: A plant will have some freedom to decide what it should do about accident management. That will not be mandated by the NRC.

MR. SHOTKIN: That's right. Right now, it's a voluntary effort. It's connected to the IPE. They're supposed to identify vulnerabilities, try to fix them, and come up with an accident management program. So it's part of this ongoing IPE effort.

MR. KERR: It's interesting to me that in some areas, the NRC is willing to be as prescriptive as it is, and in certain other areas of relatively high risk, but not so high since the probability is low, it leaves things up to the utility.

But that's not your problem, I presume; it's a policy decision that was made somewhere else.

[Slide.]

MR. SHOTKIN: Okay. This is -- I'm calling it unintentional depressurization. On your agenda, you had it down as natural circulation.

Unintentional depressurization is caused by natural circulation, the natural circulation of hot gases which transfer energy to the pipe walls in the upper part of the reactor, the hot leg and the surge line going to the pressurizer, and possibly even the steam generator.

If the PORVs are open, there's a natural sucking path that just sucks the hot gases up from the core, goes through the surge line and out the PORV. If the PORV is held open long enough, there's a very good chance that there could be unintentional depressurizer and surge line failure.

When the PORV closes, the natural circulation patterns change, and we've seen this in Westinghouse tests, whereas there is a natural circulation still pulling hot gases out of the core, but goes to the hot leg and then, as it cools, flows back in the bottom of the hot leg. It's not as effective a process as when the PORVs are open.

What typically happens in these analyses is that the PORV will cycle. When the PORV is open, it pulls the hot gases in. When it closes very quickly, the natural circulation pattern starts and you get the possibility of increasing the temperature even more.

MR. WARD: Lou, in the case where the PORVs are on the small side and you don't -- I guess you said B&W plants, where you can't necessarily credit them with the ability to depressurize on their own, are they likely to depressurize due to the surge line overheating?

MR. SHOTKIN: That's a good question, and that's why we're doing the analysis, to find out that answer.

MR. WARD: Okay.

MR. SHOTKIN: It really depends on what they do. Are they going to latch open the PORV? If they do, they'll probably fail the surge line.

MR. WARD: Yes. Okay.

MR. SHOTKIN: Are they going to let it cycle? I mean, I don't know what strategy they are going to come up with, the B&W plants.

Now, what we're doing, we're using the same

SCDAP/RELAP code and the same Surry deck, and based on our review group, we modified it to include two hot legs in the pipe to account for the natural circulation pattern. We put in a special model in the steam generator inlet plenum. We're going to look at the ability of the hot gases to get -- or the temperature to get up into the surge line.

Finally, on my next viewgraph, I'll touch on one of the toughest problems that we had, was how to handle the non-condensable gases like hydrogen that may come out during the accident, and what effect this might have on the natural circulation.

But for this one, for this case, we looked at a pump seal LOCA -- two cases -- pump seal LOCA and one where there isn't a pump seal LOCA.

MR. CATTON: Lou, in your two pipe hot legs, do you include the mixed convexion, or is it just a Dittus-Boelter type calculation?

MR. SHOTKIN: That's a question that I can't answer right now. That's a good question.

MR. CATTON: Because it's going to change the heat transfer.

MR. SHOTKIN: Wait a minute. Let's see if Duane can answer it.

MR. HANSON: Are you asking about the convexion between the stream and the wall? Excuse me. Duane Hanson, INEL.

MR. CATTON: Yes.

MR. HANSON: There is no interaction between the two streams because --

MR. CATTON: No, but there should be an interaction between the hot gas and the steel.

MR. HANSON: Yes.

MR. CATTON: And you get a secondary exercise that augments the heat transfer.

MR. HANSON: In the Westinghouse tests, in their observations at least in their low pressure tests, which aren't as prototypical as some of their later tests, I'm not sure they saw the Eddys in the pipe as it went down. They saw a fairly smooth laminar flow both up and back through their pipes.

To answer your question more straightforwardly, we use strictly the heat transfer correlations in the SCDAP codes, which would be more along the lines of --

MR. CATTON: This is a scaling question, and the magnitude is proportional to the Rayleigh number or Grashoff number based on radius. That means you have to -- this effect gets multiplied in going from one-seventh scale to the full scale by seven cubed.

MR. WARD: What was the scale for the Westinghouse test?

MR. HANSON: The Westinghouse test is a one-seventh scale test.

MR. CATTON: So it goes up like the link scale cubed.

MR. HANSON: Although they looked at their scaling in their hot legs and because of their working fluid, they're not running -- in their later tests, they ran a different working fluid other than steamer water to try and help account for those differences.

MR. CATTON: I understand. But it's the ratio of the Grashoff to Reynolds squared, roughly.

MR. HANSON: Right.

MR. CATTON: And that leaves properties in it and you have these link scales.

MR. HANSON: That's essentially the Richardson number. Right.

MR. SHOTKIN: If there is no pump seal LOCA; in other words, if there's no depressurization, either by opening PORVs or by anything else failing, then there's a very good chance that the surge line failed. In fact, this seems to be the dominant depressurization mechanism.

If there is a pump seal LOCA, then the surge line failure is probably not a dominant mechanism. Of course, the rest of this says there's some uncertainty associated with that conclusion.

[Slide.]

MR. SHOTKIN: Let me go to the evaluation of the effects, unless there's any other questions on that. As I said, one of the big questions that we had was what's the effect of the non-condensible hydrogen that gets into the system, what effect does this have on our conclusions and on the natural circulation.

One of the reasons we were concerned about this is that in our computer codes; in particular, in RELAP, hydrogen and the steam is mixed homogeneously and there is no very good way for the hydrogen to be transported separately.

We had some information on Westinghouse tests th were run. They were low pressure tests, where they did see a separation of hydrogen that looked like there would have to be a separate field of hydrogen in the computer codes in order to handle that.

Then they ran some high pressure tests and it looked like the hydrogen was well mixed. We looked at that and what we found, and we looked at the Rayleigh number, for natural circulation in a PWR, the Rayleigh number under these conditions would be about ten-to-the-fourteenth.

There is a cutoff Rayleigh number of about ten-to-the-ninth, which says that above this, there's an onset of turbulence. In other words, it would be very good mixing c the hydrogen and the steam, and below that number, there would probably be a good chance that it will be well separated.

We looked at the low pressure tests and Westinghouse and the Rayleigh number came out to be one-times-ten-to-the-tenth, which means it's sort of on the borderline of whether it would be stratified or well mixed. But on the high pressure tests, which did show it well mixed, the Rayleigh number came out to be eight-times-ten-to-the-twelfth.

So based on that, looking carefully at that test data and doing some independent hand analyses, we included this first bullet that hydrogen circulating in the reactor vessel under these conditions is not expected to result in stratification in the upper plenum or core regions.

Now, can there be any blockage of flow, and, of course, this could block the flow. Can there be any blockage of flow by hydrogen, and, yes, indeed, there can. Hydrogen circulating in the loops, it can cause flow blockage if there's condensation in the steam generators that could concentrate the hydrogen in the top of the steam generator tubes.

Luckily for us, the computer code can handle that.

[Slide.]

MR. SHOTKIN: Now, let me get to the risk assessment. This is something that we have started recently, maybe a year or so ago, with Sandia. We're using the 1150 models, and I think this is a very important part of our program. It gives a risk perspective to what we're doing, helps tell us when we're going to finish, when we can finish, and brings in just a complete picture that is lacking when we do just these detailed mechanistic calculations.

We're using the 1150 models for Surry as they exist. The results I'm going to tell you today are as they exist, and, of course, later, we're going to modify them. What we found is that there's very little risk reduction in the depressurization strategy for the Surry plant.

Why? Because there are several possibilities that something else might happen that's good. There could be surge line failure, inadvertant depressurization. There could be time to recover the AC power. It also turns out that the Surry containment itself -- now, this is based on 1150 type information -- seems to have a relatively strong containment.

So putting this all together in a total risk perspective says that the depressurization strategy, at least for the Surry-type plant, does not look like it gives a big benefit in conditional probability or in relative risk.

MR. KRESS: Can I ask you a question about that, Lou? This is a philosophical question. It seems to me like you were worried about DCH mostly with this depressurization. With DCH, there were large uncertainties we learned in the core melt progression; large uncertainties in how the stuff behaves as it gets in the cavity, and how it may effect the loads on containment.

With the natural circulation calculation, there are bound to be large uncertainties and questions about whether it really is going to fail the pipe or where it's going to fail it, how big the failure is going to be.

It seems to me like one might jump directly to the conclusion that if, by purposely depressurizing -- you know, that's got very little uncertainty in it. Your calculations for that are pretty good.

It seems to me like one should not really be dealing with inadvertant depressurization, doing natural circulation, if one could really go in and say let's depressurize and reduce the risk by a certain amount, because there's really no uncertainty, very little uncertainty in that action.

[Slide.]

MR. SHOTKIN: Let me show another backup slide. What you're saying makes a certain amount of sense, but then if you do latch open the PORVs, and I've said this several times, you increase the probability of surge line failure. What the calculations show us is that in the early depressurization, which is this solid line here, that you will get surge line failure.

If you open the PORVs, there's a very good chance you will get surge line failure.

MR. KRESS: Does that lead to any increase in risk when you fail the surge line?

MR. SHOTKIN: Any increase in risk.

MR. WARD: So what, in other words.

MR. SHOTKIN: Right. So what. Let me give you a philosophical answer to your philosophical question. I don't think we can regulate on the failure of a surge line. I don't see how, because then somebody will put dynamite around it. I think even somebody suggested doing that as a strategy. If all else fails, blow off the surge line.

I don't think we can regulate like that.

MR. WARD: You don't like that idea?

[Laughter.]

MR. WARD: What is your risk measure here? When you say it reduces risk, what is the risk factor?

MR. SHOTKIN: Actually, with 1150, it is the full-blown off-site release.

MR. WARD: I guess the risk from DCH isn't very big in the first place, right? What I'm driving at is would it be more appropriate to use the conditional probability of avoiding DCH rather than some absolute risk value as a measure for the strategy?

MR. SHOTKIN: Yes. We do use the conditional failure, conditional probability to guide us.

MR. WARD: Given conditions which would otherwise lead to a DCH problem, what is the conditional probability of this depressurization strategy avoiding it? Is that what you're really doing?

MR. SHOTKIN: That's our main measure, the conditional probability.

MR. WARD: All right.

MR. SHOTKIN: That's what I had in this first slide. The second slide is just that we do have -- CP means conditional probability. That's what our measure is.

I didn't finish the risk assessment. I'm sorry. The plans are to extend the Surry risk analysis to all PWRs with large dry containments. Sandia is going to do that. This is not a big effort.

We're going to use the Surry PRA or the risk analysis, but Sandia is quite familiar with the DCH problem, they're quite familiar with the PRA, and they are coming with a proposal that they feel they will be able to give us these quick answers to this problem just using the Surry-type PRA and getting these conditional probabilities.

We might have a problem of extending this to ice condenser containments. But we are considering a simplified study. Again, Sandia, we expect a proposal next week.

Now, what I've said so far is based on the existing 1150 analysis, which does not include many decision points for operator intervention. What Sandia is going to do, even though they tell us that when they go back and make all these good changes, their answers aren't going to change very much, but in order to make these results defensible, they will go back, make changes to the 1150 Surry PRA, put in those operator decision points, and then come up with the final results.

But, again, they don't expect the answers to change.

[Slide.]

MR. SHOTKIN: The final thing I'd like to cover is we have the facility, University of Maryland in College Park. It's a B&W geometry. Several questions came up on what might happen in the B&W plant in this review group that we had looking at natural circulation.

What we decided to do, based on the review group or technical peer review group, their recommendation was to when we finished our testing in UMCP, which we did about half-a-year ago, we would convert the facility to something that was similar to the Westinghouse one-seventh scale test, but it would be -- we'd use the SF6 geometry, but it would be different in that we'd be able to look at transient tests.

We'd have a better scaling of the radiation problems that might occur from the wall to the fluid. What we would do, we'd investigate the natural circulation flow patterns of vapor in the hot leg, and the second -- and this came up, this is a special thing. I don't want to go into it too much here, but as we were looking at the TMI accident, there was a problem in the lead screws just above the core.

There was like a temperature inversion that nobody -- they felt it was non-typical. Some people said it's because of one thing, the hydrogen; some people said it's because of the heat transfer. So we decided that one of the things we could do in UMCP, this is like a side issue, is we could investigate that.

So they have put in lead screw guides that were added into the upper plenum. We're using SF6. We're going to use nitrogen as a non-condensible. We put in new scale hot lets, a new instrumentation system, the core was modified, and we'll be running tests over the next two years to try to answer these two objectives at the top.

Of course, we had a scaling rationale for transients which was developed and reviewed by a group of experts and they approved it.

That's all I have.

MR. KERR: Thank you, Lou. Are there further questions?

[No response.]

MR. KERR: Next, we hear from INEL on lower head failure.

[Slide.]

MS. REMPE: Good afternoon. My name is Joy Rempe, and today I'd like to talk about the NRC-sponsored lower head failure research program. Before I begin my presentation today, I'd like to acknowledge that there are several individuals who have contributed to this research program.

In addition to myself, at the INEL, we are having Susan Chavez, Gary Thimes, Gary Korth, and Chris Allison contributing to this research. In addition, there are individuals at other national laboratories and universities contributing to this research program, such as Jim Sienicki and Kong Wang at Argonne National Lab, and Professor Robert Witt of the University of Wisconsin.

Finally, we're also receiving some help from peer review comments from individuals, such as Steve Hodge and Larry Ott at Oak Ridge, and Marty Pilch at Sandia.

[Slide.]

MS. REMPE: In my presentation today, I'd first like to go over briefly what the objectives of the lower head failure research program are, and the methodology that we've selected for accomplishing these research objectives.

Then I'd like to discuss some recently obtained results we've gotten on the detailed BWR penetration and vessel thermal response analysis, and illustrate how we can apply these numerical results to other LWR designs, using analytically-developed techniques.

Finally, I'll be summarizing with some of the conclusions that we've been able to draw related to which failure mechanism is more likely to occur in a specific reactor design for a particular reactor scenario.

[Slide.]

MS. REMPE: This slide illustrates the postulated end state for the TMI-II reactor debris. Although lower head failure didn't occur during this absolute scenario, the relocation of debris onto the lower head presents several challenges to vessel integrity. First, there is concern that the large mass of debris that relocates down to the lower head could cause the vessel to fail in a global manner via creep rupture.

In addition, there is concern that non-uniform heat sources within this debris bed, as well as the potential for a coherent jet to impinge directly down onto the lower head could cause a localized failure to occur.

Finally, there is concern that the penetrations at the base of the lower head could fail via mechanical mechanisms, such as tube ejection or tube rupture, because there is uncertainty as to which mechanism is more likely to occur for different reactor scenarios and designs.

[Slide.]

MS. REMPE: The NRC has sponsored the lower head failure research program with the following objectives. First, we want to perform a systematic evaluation of plausible failure mechanisms for representative LWR design, thermal hydraulic conditions, and debris states.

Secondly, we're supporting the TMI-II vessel investigation project analyses and sample evaluations. During the last year, the NRC has asked us to participate in the CSNI standard problem on lower head failure. Finally, we would like to obtain a technical consensus related to the nature of vessel failure.

MR. KERR: How does one obtain a technical consensus?

MS. REMPE: The methodology that we're trying to use is obtaining peer review. For example, we issued a draft NUREG last December and we sent it out to different organizations, like Argonne and Sandia and other labs, and asked them to provide comments. We listened to their comments and tried to focus our research in the channel that everyone agrees to.

MR. KERR: And if people don't agree, then you don't get a technical consensus, I presume.

MS. REMPE: So far, that hasn't happened. If it does happen, I guess we'll have to see what happens then.

[Slide.]

MS. REMPE: The methodology that we've selected for accomplishing these research objectives is illustrated by the flow diagram in this slide. We've started out in this research program by performing the task in the upper lefthand corner, which is namely that we perform deluge research to review the range of reactor design parameters, as well as possible debris conditions that could result in lower head failure.

From this broad range of conditions, we've selected specific parameters, such as debris decay heat or reactor vessel pressure, and put them into a thermal analysis to obtain parameters, such as a typical vessel temperature distribution or typical depressurization times.

In addition, at the beginning of this research program, we recognized that there was a need to obtain high temperature creep and tensile data for the vessel steel. The SA-533 Grade B Class I steel undergoes a phased transition at around a 1,000K.

Prior to this research program, there was no high temperature data for the steel above temperatures of around 922K. Recognizing that we did need higher temperature data in order to adequately predict the vessel response, we performed high temperature tensile and creep tests for temperatures up to around 1,400K.

This new data, along with the results from our thermal analysis, are then input into failure mechanism analyses for each of the mechanisms that I discussed earlier. Because there are a broad range of possible combinations of conditions that could result in lower head failure, these initial analyses have relied heavily upon analytical closed-form solution techniques or simplified numerical techniques.

Results are presented in terms of failure maps that have been developed in terms of dimensionless groups, so that a broader range of conditions can be considered simultaneously.

Once all these failure maps are completed, we then compare them to determine which failure mechanism is more likely to occur under a specific accident scenario. In cases where we feel that our analytical techniques may be insufficient, we're going to be going back and performing numerical analyses, where needed.

MR. KERR: Are you going to show us later what a failure map is?

MS. REMPE: Yes.

MR. KERR: Thank you.

MS. REMPE: The tasks that are listed on this slide represent tasks that were performed during the last two years, because this has been defined originally as a two-year research program. And most of the tasks that are in the lighter blue boxes have been completed or are almost complete. We're in the process of finishing up the last task, which is a numerical analysis.

I think that we've obtained some fairly important results form this research program. For example, as I discussed earlier, we do have high temperature creep and tensile data. In addition, we have developed some new models that I think are necessary for predicting vessel failure, such as this localized shear failure model that Professor Woods is developing at Wisconsin, and a model for simultaneously predicting the potential for tube ejection and rupture, along with global vessel failure, which was done by Gary Toonis at the INEL.

However, today, what I'd like to do is discuss some of the recently obtained results from our numerical calculations, and then illustrate how we can extrapolate this result for a specific scenario and design to other reactor geometries by using some of the analytically-developed failure maps that we obtained during the first year of this research program.

[Slide.]

MS. REMPE: Today I'm only going to be presenting results from our thermal analysis of a BWR penetration and vessel response. The calculations that we're performing will include, before they're completed, both a thermal analysis and a structural response analysis.

[Slide.]

MS. REMPE: The objectives of these calculations are listed on this slide. One, we want to assess the relative likelihood of a vessel and penetration failure for a range of accident conditions. Secondly, we want to evaluate the fraction of the corium that is molten at the time of vessel failure, which has applications to DCH calculations.

Last, we are going to be using our analytically-developed failure maps to extrapolate numerically obtained results to other LWR geometries and debris conditions.

MR. KERR: How can you do that second one without knowing more than you know about core melt progression?

MS. REMPE: I am only concerned with vessel failure. So what I've tried to do is bound possible debris conditions. As you'll see later on, with the calculations we're performing, we're taking different debris compositions, as well as coolant pressures, and then specific scenarios can hopefully be classified as similar to the type of calculations that we're performing.

MR. KERR: Then can I conclude that one doesn't really need a detailed picture of core melt projection in order to get information about the time of vessel failure?

MS. REMPE: If you want to look at the results for a particular scenario, I think that's true. My task, again, is only to look at vessel failure, and I'm assuming different inputs for the particular task I've been assigned.

MR. CORRADINI: Would it be fair to say, though, that after you do your analysis, you could go backwards and say how wide of a range of conditions you can live with and still come up with a relatively clear conclusion?

MS. REMPE: I think that we could try and do something like that. I am hoping -- for example, one of the cases we're considering may be similar to the type of conditions that occurred, as far as the debris conditions go, for the TMI type of scenario, although we've looked at a BWR plant.

So I think I could maybe say that this may fall into a typical -- what happened during TMI or maybe similar. But, again, we're still performing the calculations.

[Slide.]

MS. REMPE: The vessel design that we are considering in this analysis is a BWR-4 design, which is shown in the upper lefthand illustration here on this slide. The BWR-4 design is characterized by a slightly thicker base, 20 centimeters as opposed to the side. It's supported by a skirt which is insulated on the inside, as well as the outside of the vessel.

We are concentrating our penetration analysis on the drain line penetration, which is located near the center bottom portion of the vessel. We've selected this penetration for several reasons. First, there are no in-vessel structures associated with this penetration.

For example, a control rod or an instrument tube does have in-vessel structures that would have to be melted through after debris relocated onto the lower head before the melt could enter the penetration.

Secondly, the drain line has a fairly large effective diameter for melt flow. Note that I'm talking about an effective diameter for melt flow, which, for other penetrations, such as an instrument tube, would be reduced by the fact that it has an instrument string that's located within the instrument tube.

The last reason that we've decided to look at this penetration is that it's composed of SA105 and 106 steel. This steel is not a high temperature steel. In fact, it's not recommended for use at temperatures above 811K.

[Slide.]

MS. REMPE: We're using the SCDAP/RELAP5 code to perform our thermal analyses, and we selected this code because it's able to model the time-dependent, as well as the composition-dependent debris relocation onto the lower head. In addition, we can model the heat transfer from the debris to the coolant, as well as from the vessel to the reactor building.

[Slide.]

MS. REMPE: This table illustrates the types of debris conditions that we're considering. As you can see, we are considering three types of debris which correspond to the three types of debris that have been postulated in the recently completed report by Theofanous on Mark I liner failure.

The first case is a case which is primarily metallic in nature. The second is a ceramic debris. The last case is a layered debris bed which corresponds to the debris bed that Hodge hills will occur during a short-term station blackout event. There is metallic debris near the lower head, and on top of that there is ceramic debris.

We're also performing sensitivity studies to consider the effects of power density, debris bed porosity, vessel pressure, and heat removal conditions from the reactor building.

Today, however, I just want to discuss some of the results we obtained for the Case II-2 ceramic debris, which is a ceramic debris. It has a fairly high power density of .4 megawatts per meter cubed and relocates at a vessel pressure of one mega pascal.

MR. CATTON: How do you get 70 percent porosity?

MS. REMPE: What I'm doing is bounding the porosity. There is an upper bound and a lower bound. At the lower bound, if it's all liquid, there's no porosity. The upper bound was a bound that was given to me by people who have looked at the debris conditions from TMI.

MR. CATTON: I would have thought 40 percent would have been a better number.

MS. REMPE: I think 40 percent is what I've been told is an average number. That's what I've been told by other people who are --

MR. CATTON: Almost everything you put in a pile is 40 percent.

MS. REMPE: We wanted to make sure we bounded the possible range of conditions here.

MR. ALLISON: Can I make a comment? This is Chris Allison from Idaho National Engineering Lab. Remember what Joy is trying to do here is we're trying to pick a range of numbers so that when we get better data from the core melt progression program, we hope that, for example, the porosity will fall within the two extremes. So she intentionally picked a high number.

MR. KERR: Continue, please.

MR. CORRADINI: Could I ask a question? What do you mean by -- you said it very quickly -- free and forced? You mean the cooling on the outside of the head?

MS. REMPE: For the free, I'm considering a dry containment building. For the forced, I'm going to have increased heat transfer from the vessel to the containment air.

The final parameters that I select for the forced cases haven't been decided because there's been some questions asked about should we consider a vessel that's cooled by water after flooding for the advanced reactor.

MR. CORRADINI: So right now it's open to be forced, to some extent.

MS. REMPE: Right.

MR. CATTON: How do you know the zero and the 70 percent bound?

MS. REMPE: Well, most people feel it's around 40 percent. If that's the way you feel, you must really think I'm way high for the 70 percent.

MR. CATTON: I don't know. It's just that if there is porosity, then you're going to boil out the water and remelt and slowly heat up, and all kinds of strange things are going to happen.

MR. KERR: Tell him it's arbitrary.

MR. CATTON: It's arbitrary?

MS. REMPE: Yes.

MR. SHOTKIN: You don't argue with the zero, do you?

[Laughter.]

MS. REMPE: One's a little too high to select.

[Slide.]

MS. REMPE: For this case, II-2, the vessel and debris temperatures are at 2,600 seconds as shown in this slide. This two-dimensional output is obtained using the PATRAN code, which has recently been interfaced with SCDAP/RELAP5.

MR. KERR: I'm sorry. What code?

MS. REMPE: PATRAN.

MR. KERR: Thank you.

MS. REMPE: As you can see, at this time period, the vessel temperatures are below 1,225K, which is way below the melting temperature for the vessel steel, which is 1,770K. At a similar time --

MR. CORRADINI: May I ask a question?

MS. REMPE: Certainly.

MR. CORRADINI: What is this tool? Is it just conduction? What is PATRAN?

MS. REMPE: PATRAN is just a display capability. For each node, I predict the temperature using SCDAP/RELAP5 and I put it into PATRAN and it can average it to give me a temperature.

MR. CORRADINI: So SCDAP is actually doing the calculation of what's the heat flux to the wall.

MS. REMPE: Correct. And it is a conduction code. However, it's been modified to include the effects of convection if the debris is molten.

MR. CORRADINI: So some sort of natural circulation.

MS. REMPE: Yes. It's using the John & Reineke data.

MR. CORRADINI: From Germany?

MS. REMPE: I believe so. It definitely sounds German.

[Slide.]

MS. REMPE: The nozzle temperatures at the same time are shown in this slide. As you can see, the nozzle temperatures do exceed the melting point of 1,770K. So for this particular scenario, which was for a ceramic debris, we would predict that the drain nozzle would fail prior to the vessel failing.

However, as you recall, an objective of my research program is to try and find out how all reactor vessel types would respond during different accident scenarios. We don't want to try and perform numerical calculations for all the different reactor designs and debris conditions.

So what we'd like to do is use some simpler techniques to try to extrapolate these results to other debris conditions and reactor designs. So today I'd like to show you how this is done using some of the failure maps that we obtained during the first year of this research program, and considering the Case II-2 debris conditions, which are a ceramic debris that relocates at one mega pascal.

[Slide.]

MS. REMPE: In the first example, we have simply applied the energy conservation equation to try and decide what would happen to tubes that had in-vessel structures. As you will recall, the drain nozzle differed from the other BWR penetrations since it has no in-vessel tube that precludes the melt from directly traveling down into this tube.

So using the energy conservation equation, we're trying to answer the question does the debris have sufficient capacitance that it can melt the tubes that are within the vessel, so the debris can enter the tube and travel below the vessel lower head.

Applying the energy conservation equation to the geometry shown on the left, we obtained the equation on the right, which is simply a ratio of the effective debris-to-tube temperature, and it's equated to the ratio of the tube-to-debris capacitance.

If we plot this effective temperature ratio as a function of the tube mass density in the lower head, we can obtain a failure map such a the one shown in this slide.

[Slide.]

MS. REMPE: The shaded region in this failure map corresponds to the conditions, such as the tube density or the debris or your tube effective temperature, where failure would be predicted to occur.

The failure region is separated from the attacked region by a line that's dependent upon the tube material composition, whether it's Inconel material, which is used for instrument tubes in PWR lower heads, or stainless steel, which is used for control rod guide tubes and instrument tubes in a boiling water reactor.

As indicated in the failure map, light water reactor vessels with lower tube densities would require a lower temperature ratio to induce tube failure. On the lower X-axis, I've plotted typical tube densities for a BWR lower head and a PWR lower head. As you can see, the BWR lower head does have a higher tube density since it's penetrated not only by the instrument tubes, but also by 185 control rod guide tubes, and since, as indicated by the failure map, a higher effective temperature ratio would be required to induce tube failure in a BWR design.

However, for the case II-2 conditions, which are shown by the horizontal bar in this failure map, we would predict that tube failure would occur in both reactor designs, since the X values for this debris condition fall within the failure region.

[Slide.]

MS. REMPE: In the last example I want to discuss today, we've applied the energy conservation equation to equate the heat produced by the debris, decay heat, to the rate at which the heat is transferred from the tube to the containment via radiation, to determine if X-vessel tube failure would occur for other LWR penetrations.

The governing equation which is shown on the right contains terms such as the containment atmospheric temperature, the tube failure temperature, the heat flux from the tube, and radiation heat transfer parameters, along with the tube outer diameter and the tube effective diameter for melt flow.

[Slide.]

MS. REMPE: Using this relationship, we can obtain a failure map such as the one shown in this slide, where, again, the shaded region indicates the combination of conditions where a tube failure would occur.

In this map, the failure region is separated from the attacked region by a line that is again dependent upon the tube material properties. As you can see, the failure region for the SA105/106 steel is considerably larger than that for the other tube materials, and this is because the point where this tube's ultimate material ultimate strength goes to zero is about 400K lower than that for stainless steel or Inconel.

The lower X-axis in this slide shows typical diameter ratios for other LWR penetrations. As you can see, the drain line does, indeed, have the higher diameter ratio, and, thus, it's more likely for failure to occur in this tube.

MR. CATTON: So what did you do? You assumed how far it ran into the tube and then the radiation cooling.

MS. REMPE: I applied a conservation equation between the amount of debris that's in the tube because of its decav heat, and I equated that to the heat transfer via radiation. If you'll write that equation out, you'll see that the link does not matter. It drops out if the melt travels below the lower head.

Now, there's a question of can this debris, indeed, travel below the lower head, and I have --

MR. CATTON: Or could it freeze up in the head itself.

MS. REMPE: Yes. And you can use the bulk freezing or the conduction model and try and predict the distance. I've done that to create another failure map that I hadn't intended to discuss today. However, if you'd like, we can talk about it after I finish with this.

MR. CATTON: I just wanted to know if it's above or below this one.

MS. REMPE: For the kinds of penetrations that we are considering and for the debris conditions we're considering, the melt would travel below the lower head, according to either the bulk freezing or the conduction model. So I believe it would travel --

MR. CATTON: You used one of the classic type of analyses to do this.

MS. REMPE: Right.

MR. CATTON: How far it would penetrate.

MS. REMPE: Right. If you'll recall, although it's never been conclusively proven whether the bulk freezing model or the conduction model is the appropriate one to use, they founded the experimental data. So if I can get it will travel below the lower head with the more restrictive of these models, I believe it would, and that's because this drain line, again, does have a very large diameter for melt flow.

MR. CORRADINI: It would be different if you applied it to the PWR, for example.

MS. REMPE: Right. Again, we can look at that example.

MR. CORRADINI: But the technique is not different.

MS. REMPE: Right. Because we're, again, using dimensionless groups and things like that.

MR. CORRADINI: There are experiments just going to be started at FAI, supported by EPRI, on this, where they are going to drop approximately 20 kilograms of material, aluminum oxide, to simulate the -- to look at sensible heat and essentially the degree at which you'd go before you have failure.

Are you aware of these?

MS. REMPE: Not enough, apparently. I would be interested in it. But, again, my research program hasn't gotten into the aspect of should we validate these models and things like that, and that will be coming later.

MR. CORRADINI: The thrusts of the experiments, as I'm understanding them, are directly at this; not necessarily drain line failure, but any penetration and material going through it and looking for essentially failure after it passes, I think, as Ivan was asking.

MS. REMPE: It's definitely of interest to me.

MR. CORRADINI: I think you ought to at least look at it and see if it's applicable.

MS. REMPE: Okay. For the Case II-2 debris conditions, I've plotted the horizontal line on this chart. As you can see, for the drain line effective diameter ratio, if you take this value of X and intersect it with this horizontal line, you'll see that the point falls within the failure region for the SA105/106 steel.

However, for other penetrations, such as this GE instrument tube, if you intersect this X value with the horizontal line, you'll see that it falls below the failure boundary for the stainless steel material.

Hence, this map not only agrees with what we obtained from numerical calculations, it also indicates that the drain line failure is perhaps a single event and the drain line is more of an outlier as far as LWR penetrations go.

MR. CORRADINI: I have a question that has nothing to do with current reactors, but isn't there -- is this drain line failure a generic thing, that if I go to the SBWR, it appears there, too?

MS. REMPE: I need some help on that, because I'm not very familiar with the SBWR designs.

MR. ELTAWILA: Yes. It's the same drain line.

MR. CORRADINI: Same design.

MR. ELTAWILA: Same design.

MS. REMPE: Are they using the same material, too?

MR. ELTAWILA: I don't know about the materials.

[Slide.]

MS. REMPE: I hope today, although I've only presented a few examples, that I've illustrated how we're performing the lower head failure research program, how we're going to combine analytical and numerical techniques to obtain more general conclusions related to LWR vessel failure.

In particular, for the event we considered today, we saw from our numerical calculations that drain line melting occurs much earlier than vessel melting if ceramic debris penetrates below a BWR lower head.

Finally, when we compared the results from our numerical calculations with our analytically-developed failure maps, we are confirming results that we're obtaining using just the simple governing equations in these failure maps, and, two, we're indicating that the BWR drain line failure is more likely to occur than a failure in other LWR penetrations.

Are there any other questions?

MR. KERR: How much more likely is it to occur than the next most likely?

MS. REMPE: Well, you saw the failure map and --

MR. KERR: I didn't know. Were those failure probabilities that I saw there?

MS. REMPE: No. I am not doing statistics. I'm just developing the models. But it does fall f r the stainless steel instrument tube, which -- it tends to indicate, because of this diameter ratio, that it's in the failure region -- excuse me -- that it's below the failure region, that it won't occur.

MR. KRESS: This is strictly for ceramic.

MS. REMPE: Right.

MR. KRESS: If you had the layered melt, you probably would come to a different conclusion, right?

MS. REMPE: The layered melt has, yes, stainless steel at the base. I am not done with those calculations yet, unfortunately, but we can use the same types of techniques. I just haven't gone through the process.

MR. KERR: At some point, you can predict the time of failure, your earlier slide said.

MS. REMPE: That was using the numerical techniques when we predict the time of failure. Right?

MR. KERR: I don't know. One of your very early slides indicated that you were going to try to predict the time of failure.

MS. REMPE: Right. And using the numerical techniques, we do predict the time of failure. These analytical failure maps do not have a time dependency.

MR. KERR: But your technique, you're convinced, will permit you to -- I ask because one of the objectives of the core melt projection program is to give the time of failure, and apparently you can do this without having a detailed core melt --

MS. REMPE: No. Once again, I'm using bounding conditions so I can develop the models and use these models. I need a specific scenario that has a time dependence associated with it to use these. If you'll give me a scenario in your core melt progression that's time-dependent, I can then put them into the failure maps.

MR. CORRADINI: But to put it another way, though, you're looking at end point conditions. You're saying if given enough time, you'll eventually come to this condition.

MR. CATTON: She's looking at steady-state.

MS. REMPE: Right.

MR. CATTON: If the steady-state temperature is above the melting point, you fail.

MR. KERR: I had assumed that you were able to predict the time of vessel failure after the core melt started or something. You can't do that.

MS. REMPE: After relocation occurs. There are models that we are developing; for example, the one that Bob is doing at Wisconsin. If you look at a global creep rupture type of a failure, that's a time-dependent failure. Given the debris conditions on the lower head, I can give you a failure --

MR. KERR: What is time zero?

MS. REMPE: From my analysis, it's the time that it relocates onto the lower head where it starts happening.

MR. KERR: Okay. I misunderstood your earlier slide.

MS. REMPE: I'm sorry. Yes, that's true.

MR. KERR: No. It's not your fault if I misunderstood it.

MR. CATTON: When you have a 70 percent void, do you melt, then penetrate? From the analysis that you just did, I'm not sure where the 70 percent --

MS. REMPE: The analysis I did, I assumed that there was no porosity. It's liquid at the time it relocates.

MR. CATTON: All right. That's what I thought.

MR. ALLISON: Can I make a comment? Chris Allison, Idaho National Engineering Laboratory. Remember what she's showing here really is a failure time from her numerical analysis and a failure mode for assumed melt compositions amounts of melts.

If you take into account the fact that there's a variation in those due to uncertainly, plus a variation due to sequence and plant design, you will get a range of failure times and a range of failure modes. That's where the probability will ultimately come into it, because it's the probability of having a certain amount of molten material down in the lower head.

Once that occurs, then her analysis will tell you what the consequences of that will be. You have to couple the results from the lower head failure program to the results from the core melt progression program to get the actual amount of molten material coming out and the timing for a given plant.

MR. KERR: I would guess there might even be some uncertainty in her calculations.

MR. ALLISON: I'm sure there are.

MS. REMPE: Thank you.

MR. CATTON: But probably a lot less than some others would say.

MR. KERR: Next, we will hear from Charlie Tinkler. Mr. Tinkler, how much time do you imagine that you need to complete your presentation?

[Slide.]

MR. TINKLER: Good afternoon. I'm Charlie Tinkler. I'm the Section Leader in the Containment Challenges Section in the Accident Evaluation Branch. The purpose of my presentation today is to basically describe the work that's been done on direct containment heating, primarily in the experimental research over the past year.

When we last described this program to the Subcommittee, we noted that the program was on hold, to an extent, in that we had directed our principal contractor, Sandia, to complete and document a scaling analysis which would guide the conduct of future integral tests.

That analysis was completed and was peer reviewed. Some of what you will see is work that was done in preparation for the conduct of integral effects tests. Those tests did not have the benefit of the completed and reviewed scaling analysis, but were intended to provide data from which the scaling analysis could be refined or we believed could be used to provide other insights to guide future integral effects tests.

MR. KERR: I'm not sure what I'm being told. Either they did or did not use the scaling methodology.

MR. TINKLER: They did not in a precise formal way, but we think that the conditions for those tests were --

MR. KERR: They did it, then, in an imprecise informal way?

MR. TINKLER: As it was being developed.

MR. TINKLER: As it was being developed, we did not have the final evaluation for the test conditions. We had some indication as to where it was headed, but we thought that we could explore other issues and there was a need to refine the experimental design of the facility.

The conduct of high temperature reactive melts and expulsion of them into a test vessel is something which involves engineering design and engineering problems. We used that time period, that interval to work out some of those bugs, as well.

But I'd point out that we believe that some of the results of those tests are quite illuminating.

MR. ELTAWILA: You're talking about the separate effects test, not an integral effect test.

MR. TINKLER: I'm talking about what I'll later refer to as limited flight path tests and water-in-the-cavity tests.

MR. SHERON: Bill, those tests were not run with the intention of simulating a DCH event in an actual plant from the standpoint of being scaled. They were more proof-and-principal type of tests. Charlie will tell you a little bit more, but they were not what we would consider an integral test for which we would want to see a more rigorous scaling analysis.

MR. KERR: I would assume, however, if you were using the results to validate a model, that you do need to know something about how you scale experiments to --

MR. TINKLER: Agreed.

MR. KERR: In that sense, you do some scaling analysis at some point, I presume.

[Slide.]

MR. TINKLER: The primary objective of all of this is to develop and validate reasonable analytical methods and/or assessment criteria for evaluating the loading associated with DCH.

MR. KERR: That means scaling.

MR. TINKLER: That means scaling, but not necessarily in the sense that we run a test and we claim that this test reproduces --

MR. KERR: I understand. But to get from your tests via analysis to a reactor means scaling.

MR. TINKLER: Or the use of models which reflect scale dependency.

MR. KERR: Where you have demonstrated that they do, that's correct.

MR. TINKLER: Right. The secondary objective of this is to also assess the loads for selected plants and selected accident sequences.

[Slide.]

MR. TINKLER: The basic approach, as I noted, was to perform a scaling analysis in support of the experimental design, to conduct integral experiments at different scales, to develop data from the tests, and from tests as prototypic as possible.

Testing is being performed in the one-tenth linear scale facility at Sandia, which we refer to as the SURTSEY facility. Counterpart tests are being performed at one-fortieth scale in the CWTI facility at Argonne.

MR. CORRADINI: I have to ask you this. When they first did these tests, they characterized them as one-thirtieth scale. Why did they get smaller?

MR. TINKLER: That's an interesting question, because when we first discussed the prospect of running these tests, it was characterized as a one-thirtieth scale facility. I presume they knew how big their vessel was.

MR. WARD: It's metrification, I think.

MR. TINKLER: When we were done, it was a 2.5 percent linear scale. It was one-fortieth, by my reckoning anyway.

MR. CATTON: Are you going to use this data to test the scaling laws that have been developed?

MR. TINKLER:

MR. TINKLER: Yes.

MR. CATTON: Good.

MR. TINKLER: This is not a situation in which we feel as if it is known with certainty or how one characterizes the processes with certainty. In the scaling analysis for these tests, and I'll be candid about, one postulates the nature of the behavior, and then one conducts tests at two different scales to confirm that that is indeed the nature of the behavior.

MR. CATTON: But when both the scales are so small, it's a leap of faith to full scale unless you have some guiding laws.

MR. TINKLER: I will speak to that a little later, because that is a legitimate concern and a concern that we have shared, that the one-fortieth scale is, in an absolute sense, a small scale.

MR. CATTON: You bet. Even on a non-absolute scale.

MR. TINKLER: It seems relatively clear we're going to talk about scaling at least intermittently throughout this. So let me just put something -- I hadn't included this in the package because it wasn't clear to me how much we were going to delve into scaling.

MR. CATTON. We're always interested in getting to the full-size reactor.

MR. TINKLER: I understand, and so are we.

MR. SIESS: You don't want another accident.

MR. CATTON: That's true.

[Slide.]

MR. TINKLER: As I indicated, we had directed our principal contractor in this area to perform a scaling analysis. This is an area that is I won't say fraught with difficulties, there are disagreements. But the starting point for this scaling analysis was the SASM TPG evaluation of melt mass.

We had to agree on something from which to start. Do we start with 50 percent of the core, 100 percent of the core. Sol Levy's evaluation, survey of various analyses, is a very thorough, very systematic evaluation. From that evaluation, we selected those conditions which are referred to in his report as synthesized conditions associated with RPV penetration failure.

Basically, the calculation was done or the assessment of conditions was done for the Surry plant. It resulted in what was characterized as appropriate initial conditions for high pressure melt ejection of 44,000 kilograms of molten corium at 2,500 degrees Kelvin.

Based on system core masses, that's 54,000 kilograms per --

MR. CATTON: With a more complete core melt progression analysis, how much do you think that number will change? It's roughly 50 percent, plus or minus ten percent.

MR. TINKLER: It's a little in excess of 40 percent. I haven't seen anything that would lead me to conclude that more detailed analysis at this point would yield anything more appropriate.

MR. CATTON: Thank you.

MR. TINKLER: Because the SASM TPG activity was underway and we were aware it was underway, we sought to make use of the work that had been done by this group. Ultimately, our contractor is responsible for defending the applicability of their test. So that is a burden they share as well as the NRC. We do retain the right of ultimate refusal on these things.

But where the work was complete, lessons learned from the SASM TPG were incorporated, and there were some important lessons learned; scaling of the fluid flow processes and steam blow-down, RPV and cavity modeling. Incorporation of these lessons learned did have an impact on our facility design. We made changes to our facility design as a result of these kinds of lessons.

The objective of the Sandia scaling analysis was to scale integral tests to produce a pressure increase corresponding to what would be expected at full scale. Now, this is a formidable task. It presumes you know what the pressurization is at full scale.

So as a consequence of that, embedded in the scaling analysis are assumptions. I've cited that there are a number of limitations with this kind or approach. Perhaps limitations is the wrong word, but there are difficulties associated with that.

MR. KERR: In fact, fraught with difficulties was an earlier statement, which I rather liked.

MR. TINKLER: I got carried away. It's late in the day. Excuse me.

MR. KERR: I like that.

MR. TINKLER: But there is uncertainty in the constitutive relationship. I just cited particle size. We could cite entrainment rate, we could site the onset of entrainment. We don't know whether, basically, entrainment is the rollover of a wave, saltation process. These are things we don't know, we may never know, but it's also not clear that we need to know those things.

MR. CATTON: On some of those things, you're quite scale-dependent, too.

MR. TINKLER. They are.

MR. CATTON: You need distance for the wave.

MR. TINKLER: This points to this. One theory would suggest that particle size produced in entrainment scales or changes linearly with scale, that the particles in a one-tenth scale facility are one-tenth the size they would be in a reactor. And that has significant implications for surface area for heat transfer. We changed surface area for heat transfer by a factor of a hundred.

That was one particular difficulty. There are experimental constraints. These basically can be reduced to time and money. It was not practical to build another facility to surround the test facilities at Sandia so that we could use reactor materials.

The Department of Energy has very strict requirements on the use of uranium, whether it be depleted or not. So we were constrained to use other materials. There are simply scale distortions. When you run tests and you're trying to reproduce the Delta P you would expect in a reactor plant, and you're running tests in a smaller length scale facility, the debris doesn't have the same time to interact; can't reach thermal equilibrium with the atmosphere.

It's called interaction time or interaction length.

MR. DAVIS: Excuse me. I'm sorry. Why do you expect the particle size to be dependent on the scale?

MR. TINKLER: I'm not suggesting that I do. I'm just suggesting that there are two theories addressing this issue. One theory is that particle size is independent of scale. Another theory suggests that particle size changes with the scale of the facility.

MR. KRESS: If it's a Weber number effect, for example, then the flow rate you get may depend on the passageway length and diameters and the amount of material that will get out. So you could possibly see an effect of scale.

MR. SIESS: It's either dependent on scale or it's not dependent on scale. You ought to be able to settle that with one experiment.

MR. CATTON: Certainly, if we went to something like 1/400th scale, you'd probably just get one big glop coming out of a small tube.

MR. KERR: Mr. Tinkler, don't ever hesitate, because if you do, this group will think of three questions to ask.

MR. TINKLER: I'm sorry. Excuse me.

[Slide.]

MR. TINKLER: The effect of these limitations/difficulties --

MR. SIESS: Mr. Chairman, I'm having a great deal of difficulty --

MR. KERR: Mr. Siess insists.

MR. SIESS: I'm having a problem following what's on the screen with what's in the handouts. Am I in the wrong notebook or the wrong room today?

MR. CATTON: We asked a couple of questions that brought up some auxiliary viewgraphs, and that's what we're seeing now.

MR. SIESS: How far did you get with your slides before?

MR. CATTON: I don't think he got off the first one.

MR. TINKLER: I think I got passed the first two pages.

MR. SIESS: So I'm not supposed to understand what's up there. That's you guys. Fine. That's all right.

MR. TINKLER: I apologize for that.

MR. SIESS: That's all right.

MR. KERR: If you would just say the word "concrete" once in a while, whether it applies or not, you can keep Mr. Siess' attention.

MR. SIESS: I'm waiting to find out what a SURTSEY is when he gets to that slide.

MR. TINKLER: The effect of these difficulties was ultimately to result in a scaling analysis which said you should use a lot of very -- a simulant with a lot of energy and you should ignore what, in our view, was a principal mitigating mechanism for DCH in terms of debris gas heat transfer.

Basically, what has been done in many of the tests in the past was we studied entrainment of debris out of the cavity and we expelled debris into the open SURTSEY vessel. A casual observer of nuclear power plants knows that the line of site between the operating deck and the reactor cavity is very poor. There are lots of things in between.

If we were dealing with micron-size particles that could follow the flow stream, they might win their way to top, but when we're talking about larger particles that are being carried along with some slip perhaps, it's quite difficult to imagine that that debris would find its way into the bulk of the containment atmosphere, interact with the atmosphere and reach thermal equilibrium.

But when the scaling analysis was driven to reproduce the same differential pressure, it told you this is how you should run your test. You should use approximately 100 kilograms of iron aluminum thermite and you should have an unobstructed flight path into the SURTSEY vessel.

We thought, for the reasons stated on this viewgraph, that that wasn't appropriate as a first integral effects test. All this was discussed --

MR. CATTON: What was the goal of that test? Was that to get all of the stuff into the vessel? An unobstructed flight path would certainly do it.

MR. TINKLER: The goal of that test was to produce the same Delta P that one would anticipate in a nuclear power plant. How you know what that is is another matter. But for the reasons I said -- and, frankly, we didn't think we'd learn anything new by running that kind of test.

At that point, after completing the peer review with individuals that are quite familiar with this issue and scaling in general, we directed the contractors to reconfigure the SURTSEY and CWTI facilities so that it resembled the lower containment regions of the Zion plant.

We were seeking to preserve the geometric similarity of those plants. Other examples I would cite, earlier tests generally were constrained to direct the debris vertically into the SURTSEY vessel. The design cavity shoot is on an incline. It's pointed at a wall below the steel table room.

We made changes to the facility, and these were not minor changes. It was no small effort or small matter to make these changes. There were other changes that I've suggested that we incorporate as a result of the TPG. Geometric similarity for the RPV. It's been pointed out by a number of individuals that it's possible that during a high pressure melt ejection, not all of the thermite will be expelled during the initial depressurization as a result of vessel failure.

You get steam blow-through and some of the debris is retained in the vessel. Earlier tests used a melt generator with a distorted aspect ratio. They were long narrow cylinders with a hole at the bottom, and they generally produced tests in which all the debris was expelled and then all the steam was blown through the melt generator into the simulated cavity.

That generally has the effect of increasing the likelihood and potential for entrainment. We changed the melt generator so that we scale geometrically the radius of curvature of the RPV in both the SURTSEY and CWTI facility. We have, based on preliminary results, seen a difference as a result of that.

Having said all the above, we were faced with a situation where the scaling analysis dictated certain conditions, that we did not think it was appropriate. So we opted for the first test to adopt a simpler approach. One can refer to it as equilibrium scaling, energy-over-volume scaling.

But basically we were trying to put what we thought in a volumetric sense was the appropriate amount of energy into this test vessel. The thermite that we used is less dense than corium. There is no shortage. We're not distorting, on the lean side, the amount the melt simulant. We're probably high by 70 percent for this particular melt.

But in any event, I've just shown it to you here. It's very straightforward. We had 54,000 kilograms of corium from Sol Levy's survey, a ratio of -- this isn't a precise definition of specific heat, but the ratio of the heat capacities was about two-thirds. The length scale is one-tenth. So on a volumetric basis, it's about one-1000ths.

Substitution of the numbers then tells you 54,000 kilograms of corium would be 43 kilograms of thermite. So later on when you see we use 43 kilograms of thermite, this is basically how it was arrived at. Now, we did ask Sandia to look at variations on their integral scaling analysis and they did look at variations in the way entrainment processes can occur.

I don't know if it's serendipitous or not, but if you assume that entrainment is a film process, you arrive at basically the same number, roughly 43 kilograms of thermite.

MR. KERR: Mr. Tinkler, I hate to mention this, but when I look at the number of slides left --

MR. TINKLER: I'll skip through a number of those.

MR. KERR: -- and the clock. Okay.

MR. TINKLER: When you look at this particular quantity of heat capacities, this is an area that is of some dispute still. We do intend on running a test hopefully tomorrow where we measure more precisely the temperature of the thermite as it leaves the melt generator before it's expelled into the cavity.

We've run a number of tests in the past where we think we know this number, but it is a matter of some dispute over many years. We're going to run a test basically just to measure that parameter. We won't inject into the vessel. It could be considered a separate effects test.

But before we submit this test and the counterpart test at Argonne to peer review, we wanted to have that number measured.

[Slide.]

MR. TINKLER: I'll return now to the rest of the presentation, and summarize this past fiscal year's progress and more recent progress. We have completed separate effects tests to investigate the effect of flight path length, the effect of interaction time or interaction length on debris gas heat transfer.

We also conducted a very limited number of tests to investigate the effect of water in the reactor cavity. We've been very good and timely in documenting the results, at least through quick-look data reports that have generally been issued within roughly three weeks of the conduct of a test.

That's not to suggest that we have completely analyzed all the tests results in those. We simply reported pressures, temperatures, hydrogen concentrations.

MR. CATTON: Is the quick-look report available yet?

MR. ELTAWILA: Yes.

MR. CATTON: Could you add that to that other one?

MR. ELTAWILA: Yes.

MR. CATTON: Thank you.

MR. CORRADINI: The LFP and the WC are all following the first four DCH tests that I remember have been documented out by Sandia reports, is that correct?

MR. TINKLER: Right.

MR. CATTON: Is the one-fortieth scale geometrically similar?

MR. TINKLER: There is some distortion in the aspect ratio of the facility, but --

MR. CATTON: Which part? The vessel?

MR. TINKLER: Yes, the vessel. It's a little narrower than the SURTSEY vessel. Actually, it didn't cause a serious problem because it allows us to preserve everything inside the crane wall, and the likelihood that debris takes three or four 90-degree turns to get through the crane wall, which have a maze opening.

MR. CATTON: They had similar problems with the aspect ratio of the reservoir for the fluid before. Have they modified them to get --

MR. TINKLER: Yes. They modified their melt generator in the same way. I talked about this during that momentary diversion there. We did subject this to peer review. We expect to submit the results of these tests and further refinement of the scaling analysis through peer review, after we have counterpart tests.

We had hoped to have the counterpart tests run last week. The latest target date is tomorrow for the one-fortieth scale test.

We noted in earlier discussions that the independent scaling by the SASM TPG and the application of the scaling methodology to DCH is expected to be issued in November. Some of the key appendices still had not been fully complete as of several days ago, but we understand they're going to be done very shortly, and that will be issued as a NUREG document for comment.

The first large scale one-tenth integral effects test was completed on September 13. You can tell I made this viewgraph up earlier. It says conducted 10/17. It may be 10/25.

[Slide.]

MR. TINKLER: I'll just briefly discuss the limited flight path test series. This was with the Surry cavity. We were looking at things like flight path length, hole size, or some variation in the driving pressure of the melt, and these were done with a nominal melt mass of 50 kilograms. This was also done in an inerted Argonne atmosphere.

[Slide.]

MR. TINKLER: You can conceive of this limitation to the flight path as something representing reactor geometries. It could conceivably represent the RHR platform in the Surry plant. Basically, these tests were run by moving this concrete slab at the one meter, two meter, and eight meter elevations.

There were a couple of things that were observed from these tests, and that was while we did see significant effects on pressurization and debris trapping, it did not have a profound effect on hydrogen generation. We generated about the same amount of hydrogen whether we blew it unobstructed into the SURTSEY vessel or whether we trapped it, spread it out against a wall, and then passed steam over it.

MR. CATTON: Does that say that the kinetics are quite fast?

MR. TINKLER: It says they're quite fast for this test. We used chromium. We didn't use zircalloy. That's an issue that we clearly need to look at. Zircalloy has a higher heat of reaction than chromium, but reaction kinetics, other issues aside -- I'm not willing not make that jump at this stage. But it does suggest that reactive metals will react whether or not they're trapped as a thin film on structures or whether they're carried along in the [illegible] steam. It's just very hard to keep very hot metals from reacting.

[Slide.]

MR. TINKLER: I am not going to dwell on this.

MR. KERR: We'll stipulate those tables.

[Slide.]

MR. TINKLER: The summary results, I would point out that you can see the Delta P. The designation "1" means the concrete slab was at one meter above the exit of the cavity, two designates two meters, eight designates eight meters up into the vessel.

You can see that as we increased the flight path length, the debris has a greater opportunity to reach equilibrium with a bulk of the atmosphere. But you can also see that the hydrogen generation doesn't change appreciably.

[Slide.]

MR. TINKLER: This is where we [illegible] --

MR. CATTON: The scaling analysis showed an extreme sensitivity to the vessel pressure, if I remember right.

MR. TINKLER: It does. We don't see quite --

MR. CATTON: Did you see that?

MR. TINKLER: We don't see quite that. We do see sensitivity to vessel pressure, but you've got to be cautious because we are changing a couple other things at the same time.

MR. CATTON: I understand.

MR. TINKLER: But we have improved the database where we do see changes in pressure for this range of melt mass, anyway. Percentage dispersed generally shows that even at these relatively low pressures, these are generally at around three mega pascals, we ejecting somewhere between 40, 50 percent of the debris. We do see one where it's lower. It was a driving pressure a little less than three mega pascals.

MR. CATTON: This is dispersed out of the vessel or out of the cavity?

MR. TINKLER: Out of the cavity.

[Slide.]

MR. TINKLER: I have already said this about five times, that compartmentalization was a dominant mitigator, but this was a test that clearly indicated it, because while it was also a reduction in the overall interaction length, it did, in a sense, simulate compartmentalization.

[Slide.]

MR. TINKLER: The next series of tests were done to briefly explore the effects of water. It has been postulated by a number of individuals and/or groups that water in the reactor cavity would have a beneficial mitigating effect on DCH.

We were seeking to look at this. We were also planning on incorporating small amounts of water into our integral effects test, and, frankly, it was preferred to look at this separately before we ran a first integral effects test, just so we'd have some idea as to what kind of outcome could be anticipated.

At this point, we had switched to the Zion cavity. Melt mass was still kept at 50 kilograms, driving pressure is 4.5, and we basically have a test with three centimeters of water. The logic behind three centimeters of water was it was scaled to correspond to the equivalent water used in an earlier FAI test. But the melt wasn't the same.

[Slide.]

MR. TINKLER: This shows the experimental configuration. The only thing I'd point out is you can see here earlier tests had this transition shoot where we turned everything vertical. We have since eliminated that and it maintained the inclined angle of the instrument tunnel. And you can see the earlier melt generator.

MR. DAVIS: Excuse me. Is there any entrained gas in your molten material?

MR. TINKLER: This has been a subject of some discussion. I'll ask Dana to address that a little bit.

MR. POWERS: As soon as we go to the punch-through phenomena, then there is a lot of entrained steam and hydrogen in the material. When the melt first comes out through the nozzle, it is probably pretty much 100 percent dense But once you get into the two-phase flow through the nozzle, as you get a very thin layer, you start getting both gas and melt coming through the nozzle. Then there's a lot of gas entrained in it.

Of course, when it comes out the cavity region, it's totally entrained.

MR. DAVIS: This may be off-the-wall, but in the reactor case, I would guess that the molten core has considerable entrained gas, fission gases, maybe there's gases from the organic binder that's used in the fuel centering process.

You undergo a very rapid pressure drop and a significant one as the material ejects the vessel. I'm just wondering if that's a mechanism that would assist in the particle breakup. Maybe it's not significant, I don't know.

MR. POWERS: If I could comment some more on that. By the time you get to this stage in an accident, your fission gases are probably pretty much released. They won't contribute much. It's the dissolved gases that you worry about.

The phase that dissolves gases most efficiently is the metal phase, and because the pressures are the same here, we get about the same amount of dissolved gases. We have analyzed how those gases will nucleate bubbles and come out of solution and, in fact, written a report and done some tests on it.

Indeed, they do come out; indeed, they do cause some breakup, but relatively coarse breakup.

MR. DAVIS: Thank you.

[Slide.]

MR. TINKLER: This is just a pressure trace of the downstream receiver volume pressure, the SURTSEY volume pressure. It just illustrates that the test with three centimeters of water on the cavity floor didn't really mitigate it, which shouldn't be surprising because it doesn't possess anywhere near enough stored energy capacity equivalent to that amount of thermite.

At one time, we argued about whether or not this was the -- Sandia and the staff argued about whether or not this was the vaporization of water or the generation of hydrogen.

In general, I think they have concluded that little of the water was vaporized. It was ejected.

MR. CORRADINI: What was the mass of water for three centimeters?

MR. TINKLER: I think it's 3.5 kilograms, something like that. It's incidental compared to the energy of the --

MR. KERR: Mike, you must be recorded and you aren't being when you --

MR. CORRADINI: I'm sorry.

MR. KERR: Would you repeat your question?

MR. CORRADINI: I asked what was the mass of water. It doesn't matter. It's so small that it would have to be enhancement. You have to get above equal volumes before it's going to start being in mitigation. It's the equivalent of an FCI.

MR. TINKLER: Right.

[Slide.]

MR. TINKLER: One of the issues which arose in the wet cavity test was the postulate that we produce energetic fuel cooling interactions, steam explosions, whatever you want to call them. This was the first indication of this. We weren't clear exactly what it was we were seeing. But we did use this as demonstration of the need to provide a little more instrumentation in the reactor cavity to see whether or not we could track a pressure waive by transducer location.

MR. CORRADINI: Your draining pressure, though, was what, 30 bars?

MR. TINKLER: Yes.

MR. CORRADINI: If you go back to the old CWTI tests, they saw equivalent sort of traces.

MR. CATTON: Could this just be choking and unchoking of the cavity?

MR. TINKLER: No. This really isn't choking. The choking phenomena occurs over -- well, actually not a much longer time period, but we do have pressure traces, as you can see from the data reports.

MR. CATTON: When I look at that pressure ratio from the cavity to the vessel and I've got any kind of a reduction in area, it's going to choke.

MR. TINKLER: This portion here generally is representative of the pressure differential we see for dry tests. If you look at a dry test, it looks much like this, without this. You can see a pressure ratio for some of the tests that approach choking conditions out of the cavity exit.

MR. CATTON: Those there at about 27 in time look like they're choked.

MR. TINKLER: I think so. I think we're nominally choked right there.

[Slide.]

MR. TINKLER: Hydrogen production relative to mass of debris dispersal. This is mass of debris dispersal out of the cavity. This is mass of debris dispersal out of the cavity. We don't see a strong function here. We do see --

it's important to note the WC-3 and 1 are Zion cavity and the rest are Surry. But we don't see a marked dependence, anyway.

[Slide.]

MR. TINKLER: This is just a clearer illustration of the effect of flight path length and how much of the energy we're able to impart to the bulk atmosphere.

[Slide.]

MR. TINKLER: It has been suggested that perhaps the reason the same sort of pressure spikes haven't been seen in all the other tests is the frequency of measurement. Some of the tests did not record data as frequently as this data was recorded.

That might explain it. It's not meant to be dispositive in any way. It's just a possible explanation.

[Slide.]

MR. TINKLER: I've talked about this before. Some of the other changes besides the melt mass, the driving pressure, we're now using a nitrogen atmosphere as opposed to an Argonne atmosphere. It's got a little higher specific heat than Argonne, things like that. I'm sorry. This was 3.4 kilograms of water here. That means the earlier test was roughly three times that amount, roughly ten kilograms.

[Slide.]

MR. TINKLER: This is basically to show you the extent of reactor plant simulation that we undertook in this test. It was a rather significant effort. We had to build a false floor at the bottom of the SURTSEY vessel because the radius of curvature at the bottom head didn't allow us to preserve scale. And there were some major gradings that were simulated in this.

This was difficult in SURTSEY. At one-fortieth scale, it was very difficult to build concrete walls, because we tried to use the same materials to build concrete walls at one-fortieth scale.

[Slide.]

MR. TINKLER: This is just another illustration of the structure. You can see generally the flight path. You blow into this region right here, the opening to the seal table room and there's a simulated concrete blowout plug above it. The blowout plug does blow out. We produced a pressurization in the seal table room of about, I believe, 30 psi, something like that.

[Slide.]

MR. TINKLER: Unfortunately, in order to maintain exact similarity between the facilities, we had to distort one area, and that was the length of the shoot. In the Argonne facility, you can't get to these things with the properly scaled length. So rather than have one test with a long shoot, one with a short, we made them both the same length.

We did look at this issue. If anything, the analysis suggested that we would probably enhance entrainment and fragmentation.

[Slide.]

MR. TINKLER: This is the melt generator, the changed, the new melt generator. There were some non-trivial issues to consider when you're conducting these kinds of tests. When we run these tests, we try to preserve the simulated volume of the RCS. We want to use the right molds or mass of steam to drive this melt.

We can do that, but we also wanted to reproduce pressure conditions between the two tests. So we tried to minimize the dead space in the melt generator. For IET-1, the initial thermite elevation is right about here, and it's about 57 percent theoretical density, I think.

So we tried to keep this volume to a minimum. It causes less perturbations on the eventual driving pressures. It's more reproducible. But it does have other effects. This might have the effect, and we were just talking about this today, of directing a jet. It's compensated for the fact by this being a very small hole diameter. If this were the same size as this, I'd have serious concerns about having a pipe exhaust.

MR. CATTON: Even so, I think you probably enhance the gas blow-through with this.

MR. TINKLER: We have thought about that and we are looking into that. It's not a simple matter of simply increasing this volume, because then you distort the steam volume.

MR. CATTON: I understand. You need to put a screen.

MR. TINKLER: Some sort of diffuser, yes.

[Slide.]

MR. TINKLER: But as I said, right now it looks like we're leaving about -- we're getting gas blow-through with about 40 to 50 percent of the melt still in the generator. Before, we used to get gas blow-through with only five percent. But I believe that 40 percent left in the melt generator is a lot closer to reality than five percent, if you're looking at a reactor.

[Slide.]

MR. TINKLER: If everybody wants to know the Delta P, the Delta P was one atmosphere. This does not mean that we're willing to claim that it's one atmosphere in a nuclear power plant. This test was run inerted. We did not allow combustion in the SURTSEY vessel. Subsequent tests will, but for the same reasons why we didn't do everything at once in other tests, we chose to try to separate out debris gas heat transfer from combustion.

MR. CATTON: That certainly is a significant reduction, isn't it?

MR. TINKLER: Yes, it certainly is.

MR. DAVIS: Was there any attempt to calculate that Delta P before the test?

MR. TINKLER: Yes. We did pre-test calculations. I'll let Ken Washington speak to the CONTAIN calculations tomorrow.

MR. KERR: You just want to know if they did or did you want more detail?

MR. TINKLER: Yes, they did.

MR. DAVIS: Do the results suggest that we're over-estimating the pressure from this kind of phenomena or not, or can you say yet?

MR. TINKLER: I think the pre-test calculations that were done for this case were reasonably accurate. That's not to suggest that that's the same for all the other pass calculations.

MR. SIESS: What is that number supposed to mean for me?

MR. TINKLER: This is the pressure in the SURTSEY vessel, the bulk pressure.

MR. SIESS: I know, but that means something to you, when I'm not designing that vessel.

MR. TINKLER: I'm sorry.

MR. SIESS: You said that's the pressure in the vessel.

MR. TINKLER: Yes, sir.

MR. SIESS: That doesn't mean anything to me. I haven't got anything to do with that vessel. I'm looking at nuclear power plants. What is it supposed to mean in that -- is that the pressure in the containment?

MR. TINKLER: Now, if we had run the counterpart test at another scale and we could compare the results from that test to this, I'd be in a better position to answer it. I can only say that depending on how you believe scaling -- some of these phenomenon scale, this can either translate to one atmosphere in a nuclear power plant or roughly two to 2.5 atmospheres in a nuclear power plant.

MR. SIESS: And that's all?

MR. TINKLER: No. This same pressure rise here of one atmosphere could result in a like-pressurization in a nuclear power plant or it could result in a pressure increase roughly two to 2.5 times this large.

MR. CATTON: Is that an upper bound?

MR. TINKLER: That's the upper bound of the scaling analysis I've seen.

MR. SIESS: That's an increase from what?

MR. TINKLER: It's an increase from about two bars starting.

MR. SIESS: Starting at about two bars, and I might end up at 4.[illegible] bars.

MR. TINKLER: Four. Now, this is without hydrogen combustion.

MR. SIESS: Just from DCH.

MR. TINKLER: This is just debris gas heat transfer.

MR. CATTON: That's a significant reduction.

MR. SIESS: It sure is. It is significant pressure. I don't know what you're reducing it from.

MR. TINKLER: I just would point out that it's been conjecture for some time now that the oxidation and combustion of gases as a result of DCH was a principal component to it. But for what it's worth, this is our first large scale demonstration of the effects of reactor geometry.

MR. WASHINGTON: Ken Washington, Sandia Labs. I wanted to just address the earlier question about CONTAIN calculations briefly. I'll talk more about it tomorrow. We did do a pre-test analysis and our best estimate was 20 percent below the one at one atmosphere test results, and our preliminary results from a post-test analysis straddled it with the two calculations that we believe best in.

MR. TINKLER: Thank you.

[Slide.]

MR. TINKLER: Let me just jump ahead here a little bit, Fiscal Year 1992 plans. Like much of what we do, this will be peer reviewed. Interpretation of results is almost as disputable as scaling analysis to design the test. We will reconvene the peer review, design review group to look at the results at the two different scales.

As I say, we expect to have the counterpart test and the test which more precisely characterizes the temperature of the thermite. We hope to complete design integral effects tests in both the SURTSEY facility and the CWTI facility at Argonne.

We're looking to complete Surry integral effects tests. These were selected as representative plants of a large population of reactors. We are also seriously looking now at conducting tests at one-sixth scale. These are the containment facilities that have been used for structural testing, von Rieseman facilities, the large scale stuff.

The use of these facilities allows us to look at issues like the annular gap around the RPV. That has other difficulties, what we do about the insulation skirt and things like that, refueling seal, but it will allow us to at least look at the issue and address the effects of the missile shield above and other structures, because it is, for some plants, an important pathway.

There is also some consideration being given to running UO2 tests at Argonne. Because they're in a different kind of facility, they're able to conduct tests with UO2. So if you think there are serious distortions that arise as a result of using thermite, you could address them by those kinds of tests.

MR. CATTON: On your scaling, you used chemical plus thermal energy to get an equivalence.

MR. TINKLER: Right.

MR. CATTON: But what you're missing is the hydrogen gas production.

MR. TINKLER: For this test. In an inert atmosphere, that was judged to be okay.

MR. CATTON: But the energy scaling was proper. You added a little bit more juice into the thermite to account for the chemical energy.

MR. TINKLER: We added chromium to boost the chemical energy of -- provide some reactive metals. The iron reacts, but it produces so little heat that it's almost inconsequential.

MR. CATTON: I understand.

[Slide.]

MR. TINKLER: I show this as just a proposed strawman test matrix where we look at a number of issues. Again, it will be peer reviewed. We may want to look at other things, like the lower RCS pressures, a range of RCS pressures, other issues.

This one here has been offered as the other mitigating mechanism that we might not be considering, and that is the existence of pools of water on various floors in a reactor containment that provide more mass for energy removal.

MR. CATTON: There is some discussion about filling up the reactor cavity with water to try to stop the vessel failure. You may not stop it. Are any of your tests, are you going to fill the cavity with water?

MR. TINKLER: There are no plans to fill the cavity with water.

[Slide.]

MR. TINKLER: Implications for ELWRs and ALWRs. The Subcommittee is familiar with the general --

MR. CATTON: Are you afraid you're going to blow up the apparatus?

MR. TINKLER: I don't know that we would be addressing anything, any issues specific to a design, frankly, at this stage.

MR. CATTON: I've heard a good bit of discussion about using water.

MR. TINKLER: We've heard the same discussion, but it varies from month to month whether the cavity is going to be pre-flooded up to the nozzle center lines or not.

MR. CATTON: I understand. Until they decide.

MR. TINKLER: Until someone decides.

[Slide.]

MR. TINKLER: I'd also point out that while it has been stated as an EPRI design criteria that one will design a retentive -- one will include a retentive cavity as part of the design, we can only say that we've tested Surry and we've tested Zion and we don't see substantial difference between the two.

There is some difference around the edges, but if one were going to attach a retentiveness factor to it, it would be very difficult to do without experimental data.

More importantly, we don't see much diminution of the hydrogen generation. If you do have a pre-flooded cavity, there is a lack of experimental data relevant to the range of ex-vessel steam explosions or FCIs. As we understand it, there aren't any plans to confirm the extent of retentiveness or the likelihood and consequences of steam explosions. At this point, we have no plans to experimentally pursue that.

That's about it.

MR. KERR: Any further questions?

[No response.]

MR. KERR: Thank you, Mr. Tinkler. We will recess until 8:30 in the morning.

[Whereupon, at 6:02 p.m., the Subcommittee was recessed, to reconvene the following day, October 25, 1991, at 8:30 a.m.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

in the matter of:

NAME OF PROCEEDING: Severe Accidents

DOCKET NUMBER:

PLACE OF PROCEEDING: Bethesda, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Marilynn Estep

Official Reporter
Ann Riley & Associates, Ltd.

Idaho
National
Engineering
Laboratory

NRC-Sponsored Lower Head Failure Program

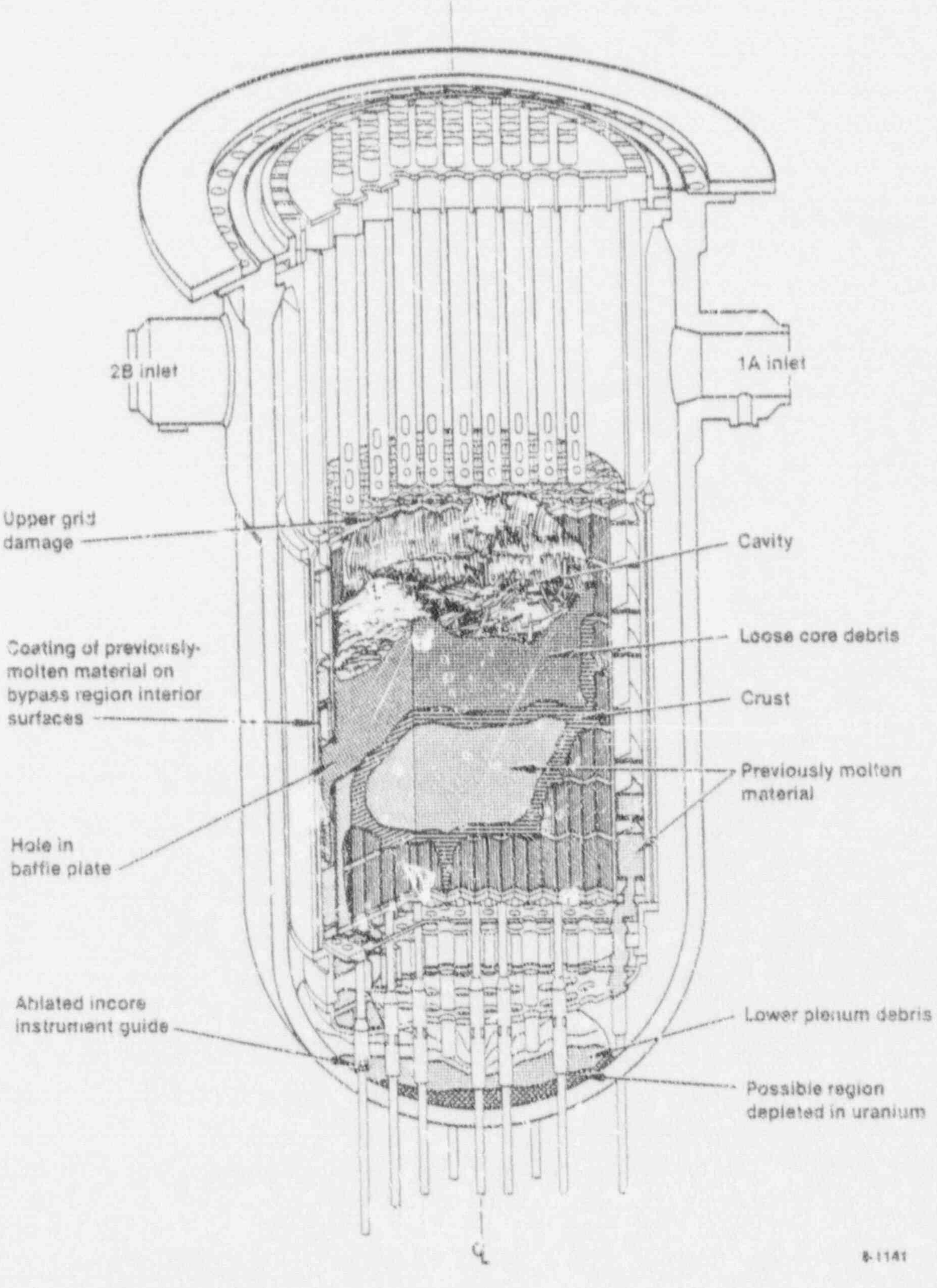
Presented by
Joy Rempe

ACRS Meeting
October 24, 1991

Outline

- Program overview
- Recent results
  - Detailed BWR penetration and global vessel thermal analyses
  - Application of results to other LWR designs
- Summary

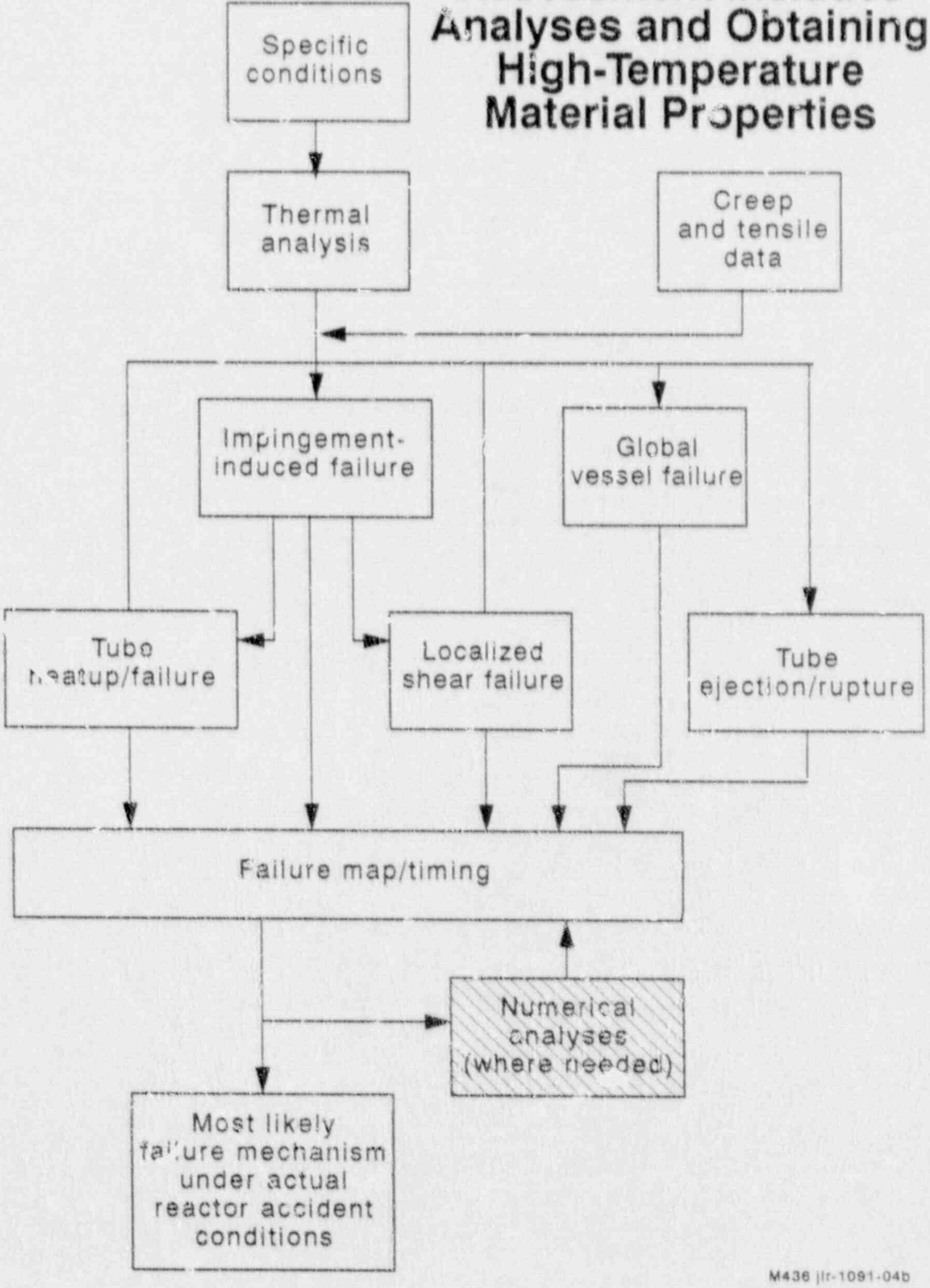
TMI-2 Core End-State Configuration



Program Objectives

- Perform systematic evaluation of plausible failure mechanisms for representative LWR designs, thermal-hydraulic conditions, and debris states
- Support TMI-2 Vessel Investigation Project analyses and sample evaluations
- Support CSNI comparison exercise on lower head failure
- Obtain technical consensus on the nature of vessel failure

Lower Head Failure Assessment Includes Analyses and Obtaining High-Temperature Material Properties



Specific conditions

Thermal analysis

Creep and tensile data

Impingement-induced failure

Global vessel failure

Tube heatup/failure

Localized shear failure

Tube ejection/rupture

Failure map/timing

Numerical analyses (where needed)

Most likely failure mechanism under actual reactor accident conditions

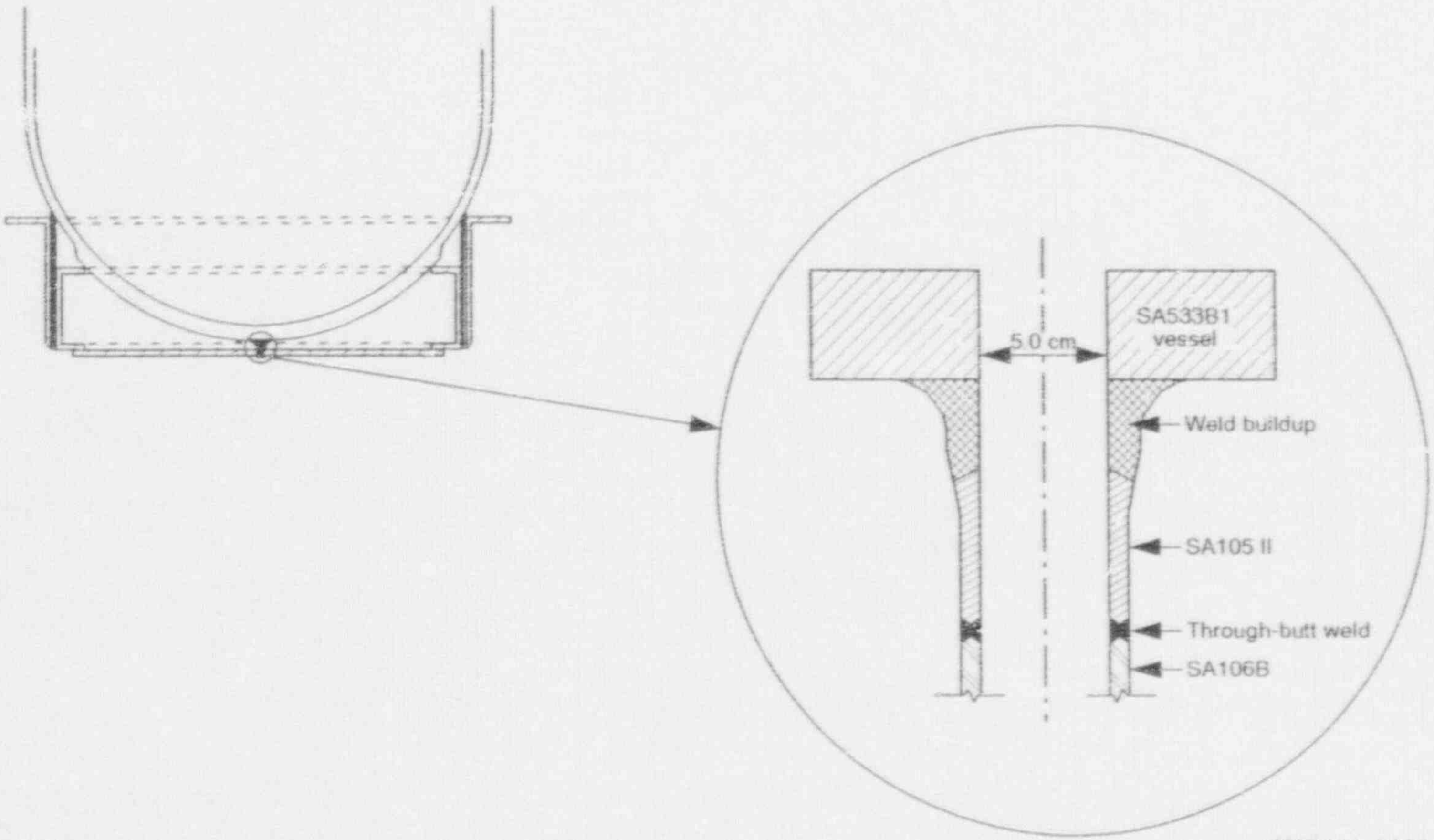
M436 jlr-1091-04b

Numerical BWR Penetration and Global Vessel Thermal Analysis

Analysis Objectives

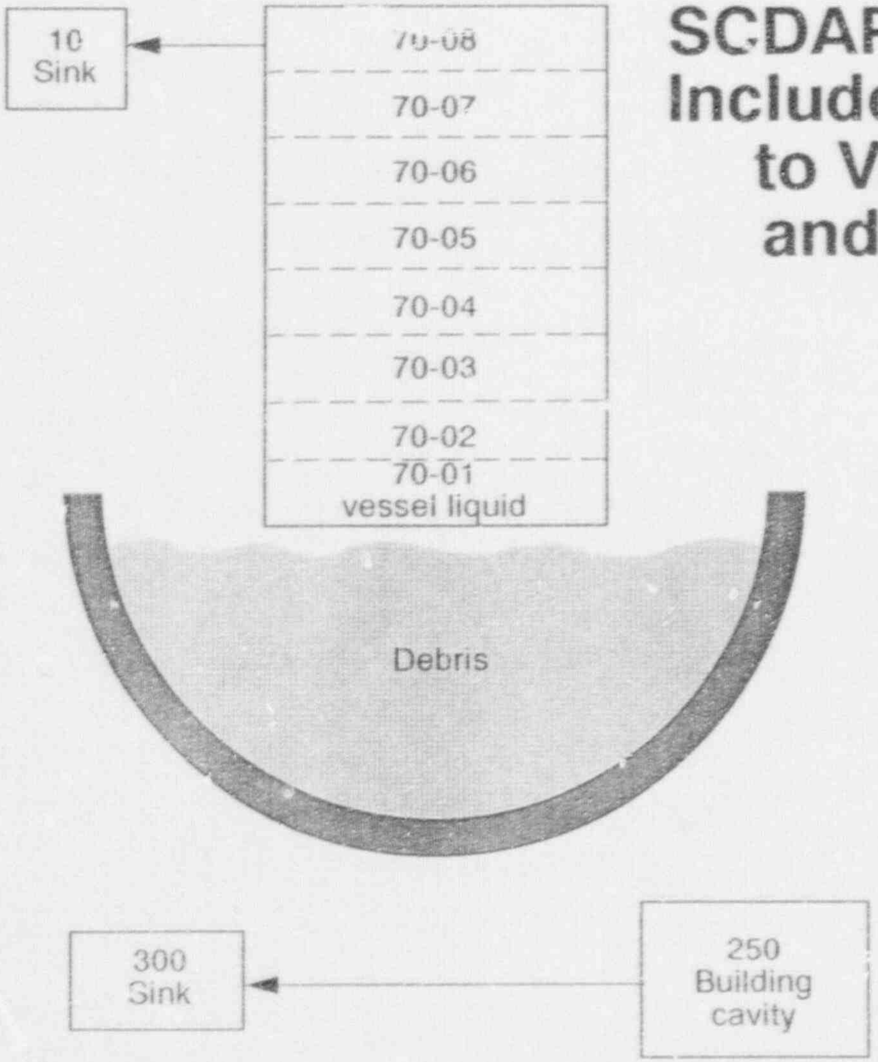
- Assess the relative likelihood of vessel/penetration failure for a range of accident conditions
- Evaluate the fraction of the corium that is molten at the time of vessel failure
- Utilize analytically-developed failure maps to extrapolate numerically-obtained results to other LWR geometries and debris conditions

Drain Line Penetration Likely BWR Lower Head Failure Location

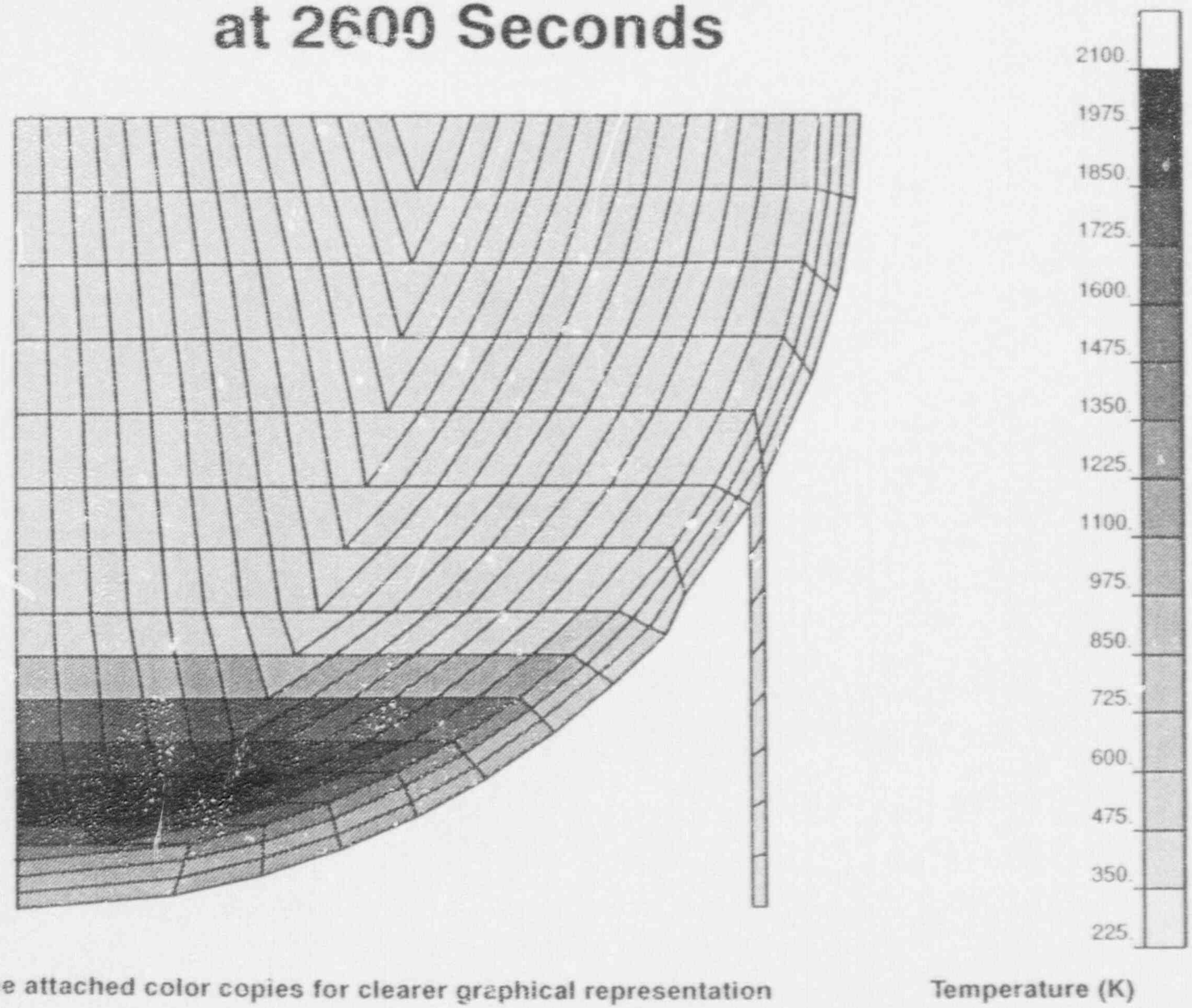


M436 jli 1091-07b

SCDAP/RELAP5 Model Includes Heat Removal to Vessel Coolant and Containment

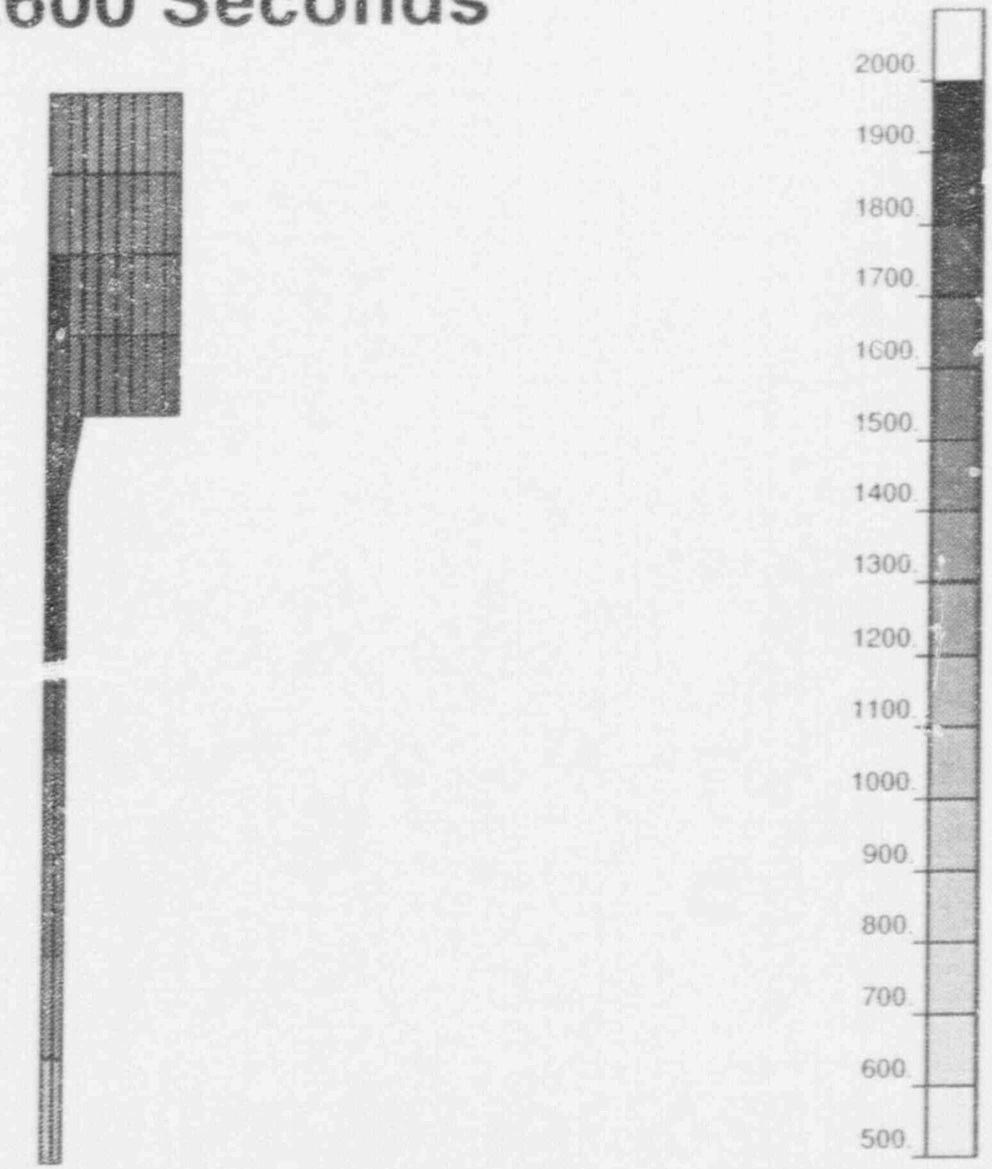


Vessel Temperature Below Melting Point at 2600 Seconds



NOTE: See attached color copies for clearer graphical representation

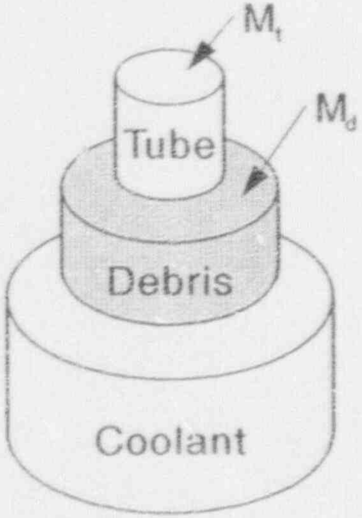
Drain Nozzle Melting Predicted at 2600 Seconds



NOTE: See attached color copies for clearer graphical representation

Application of Results to Other LWR Designs

Energy Conservation Equation Used to Estimate Melt Temperature Required for In-Vessel Tube Melting



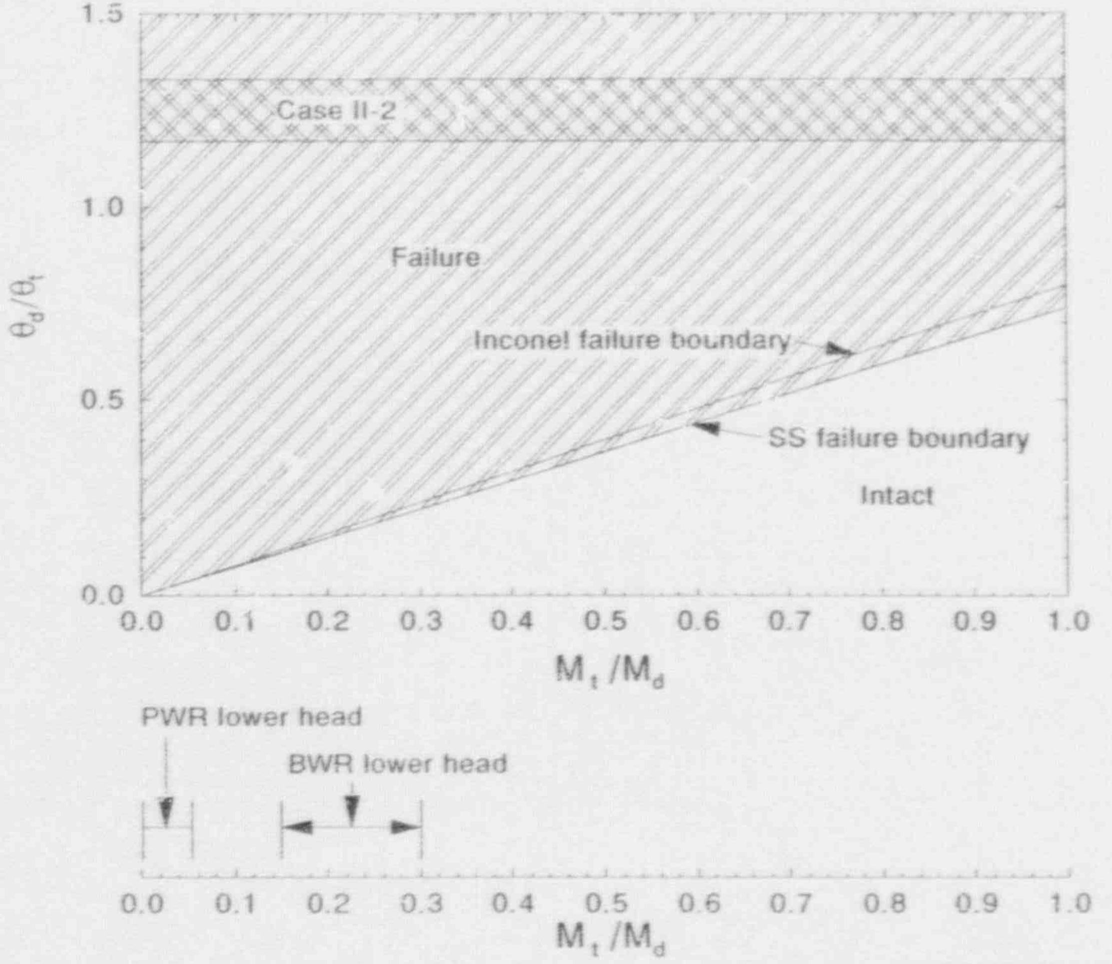
Governing Equation

$$\frac{\theta_d}{\theta_t} = \frac{c_{pt} M_t}{c_{pd} M_d}$$

where:

θ = effective temperature
c_p = specific heat capacity
M = mass

PWR and BWR In-Vessel Tube Melting Predicted for Case II-2 Conditions



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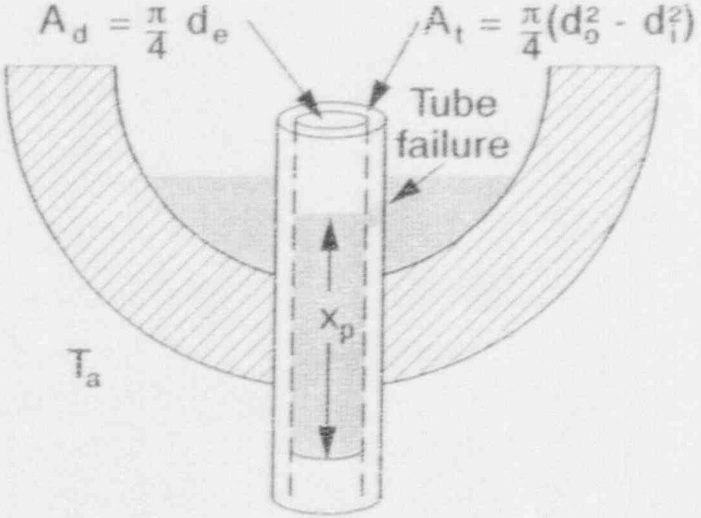
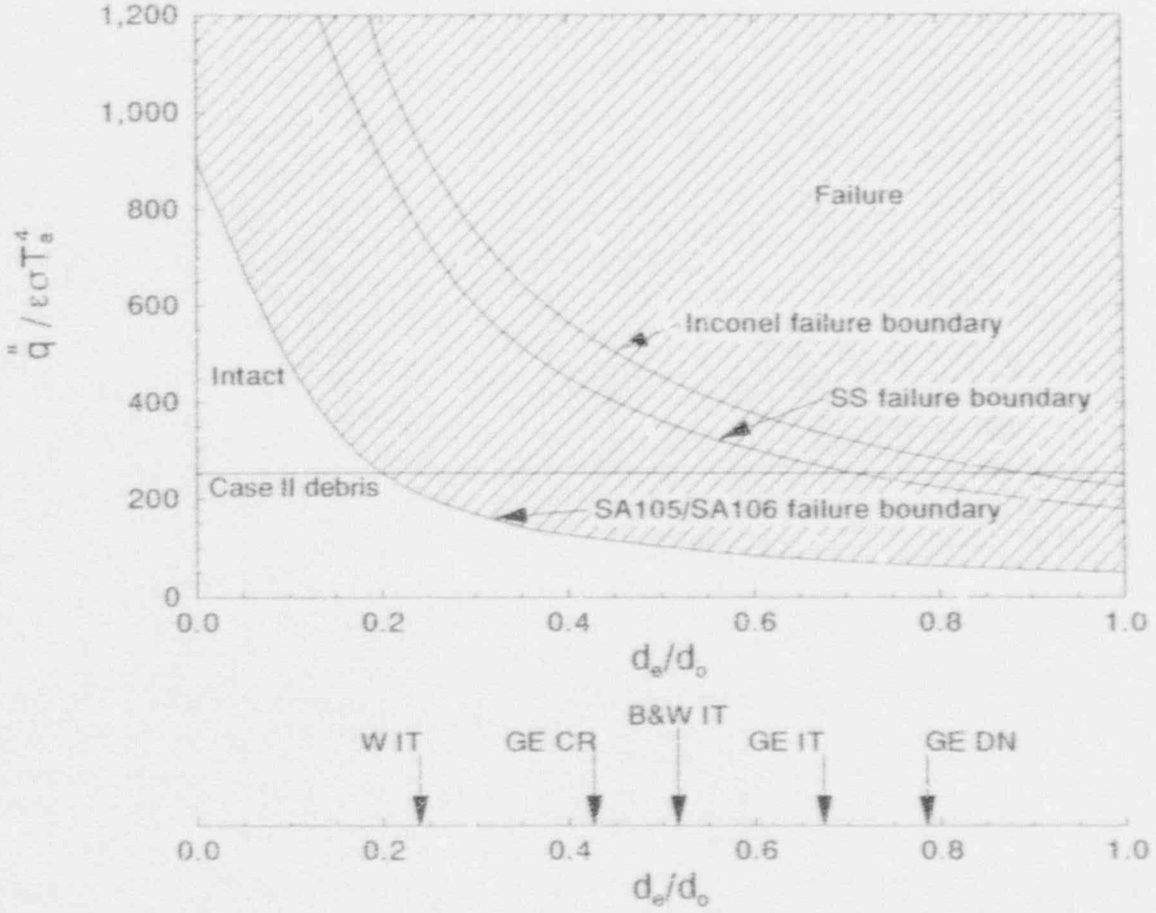
Energy Conservation Equation Used to Predict Ex-Vessel Tube Failure



$$\frac{T_t^4 - T_a^4}{T_a^4} = \frac{d_e}{d_o}\left[\frac{q''_t}{\varepsilon_t \sigma T_a^4}\right]$$

M436 jlr-1091-15b

Only Drain Line Failure Predicted for Case II-2 Conditions



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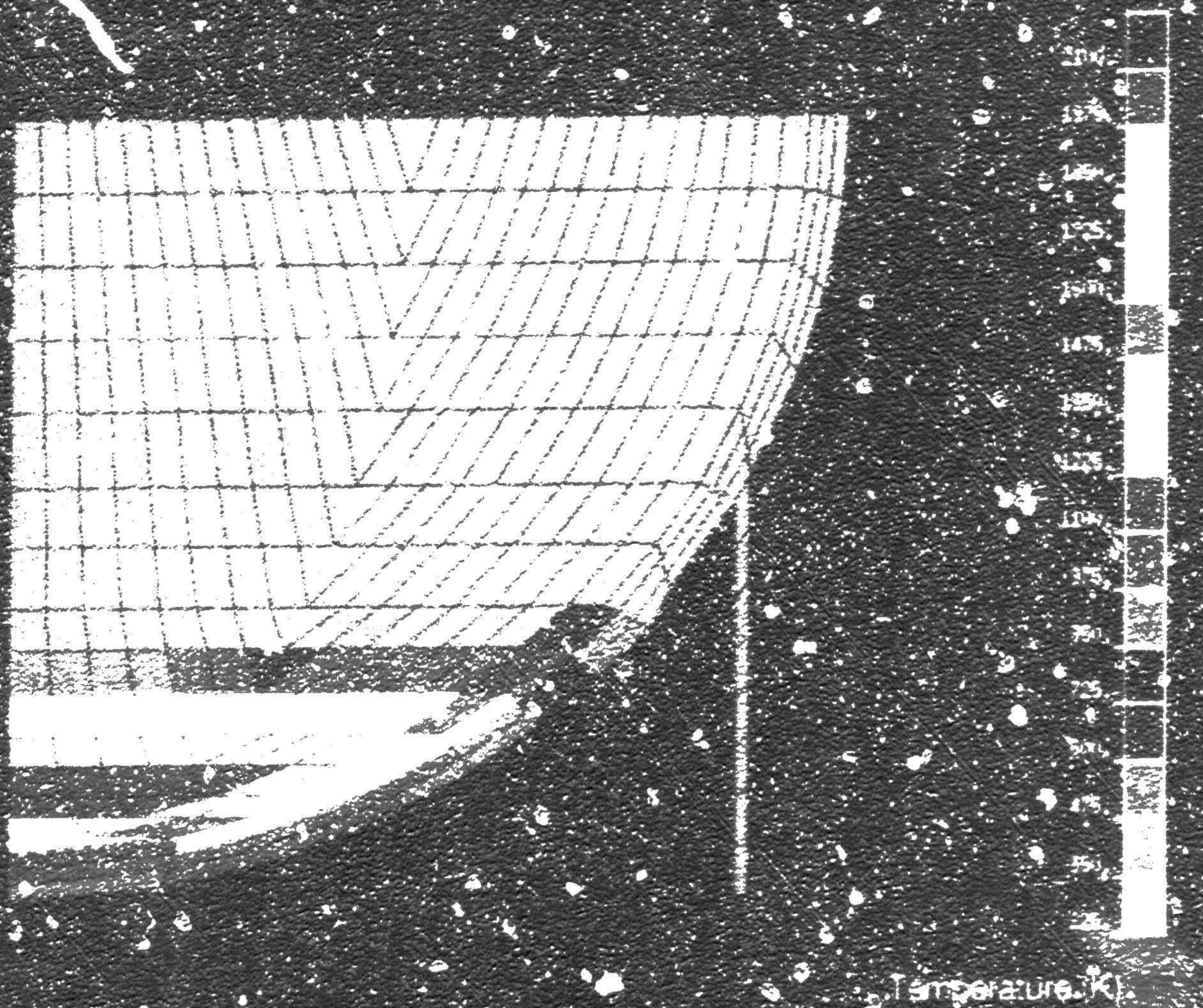
Conclusions

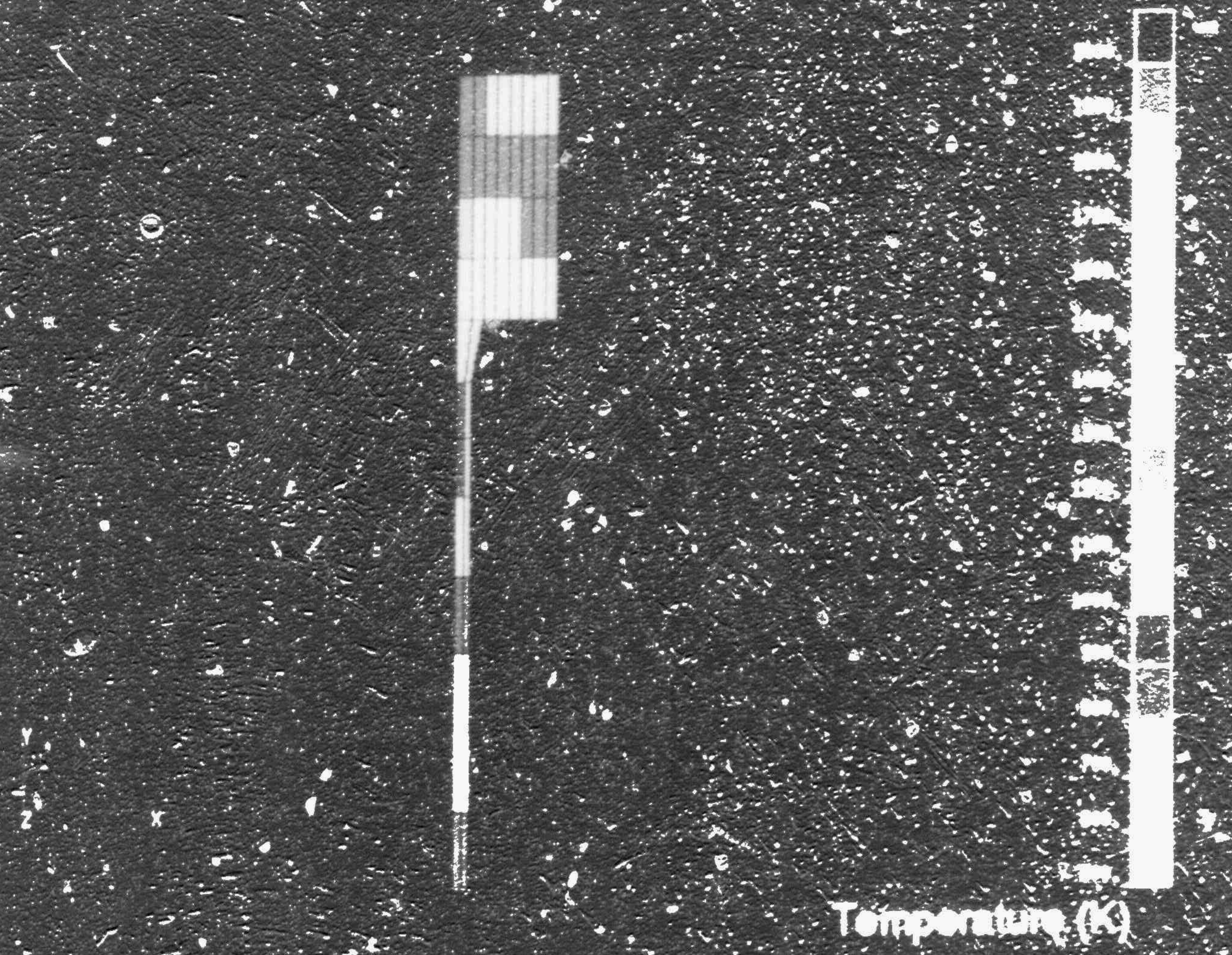
- Numerical calculations indicate that drain line melting will occur earlier than vessel melting if ceramic debris penetrates below a BWR lower head
- Comparisons of numerical results with analytically-developed failure maps
  - confirm results obtained with governing relationships in failure maps
  - indicate that BWR drain line failure is more likely than other LWR penetration failures

Vessel Temperatures Below Melting Point at 2600 Seconds

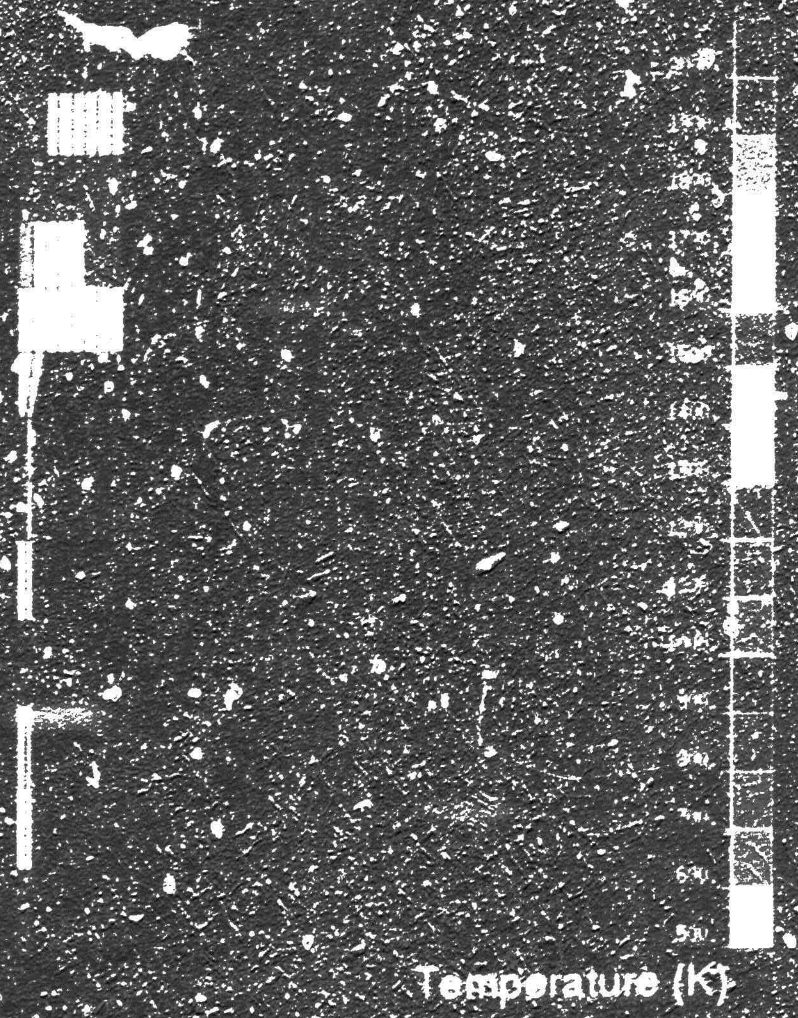


Vessel Temperatures Below Melting Point at 2600 Seconds

Drain Nozzle Melting Predicted at 2600 Seconds



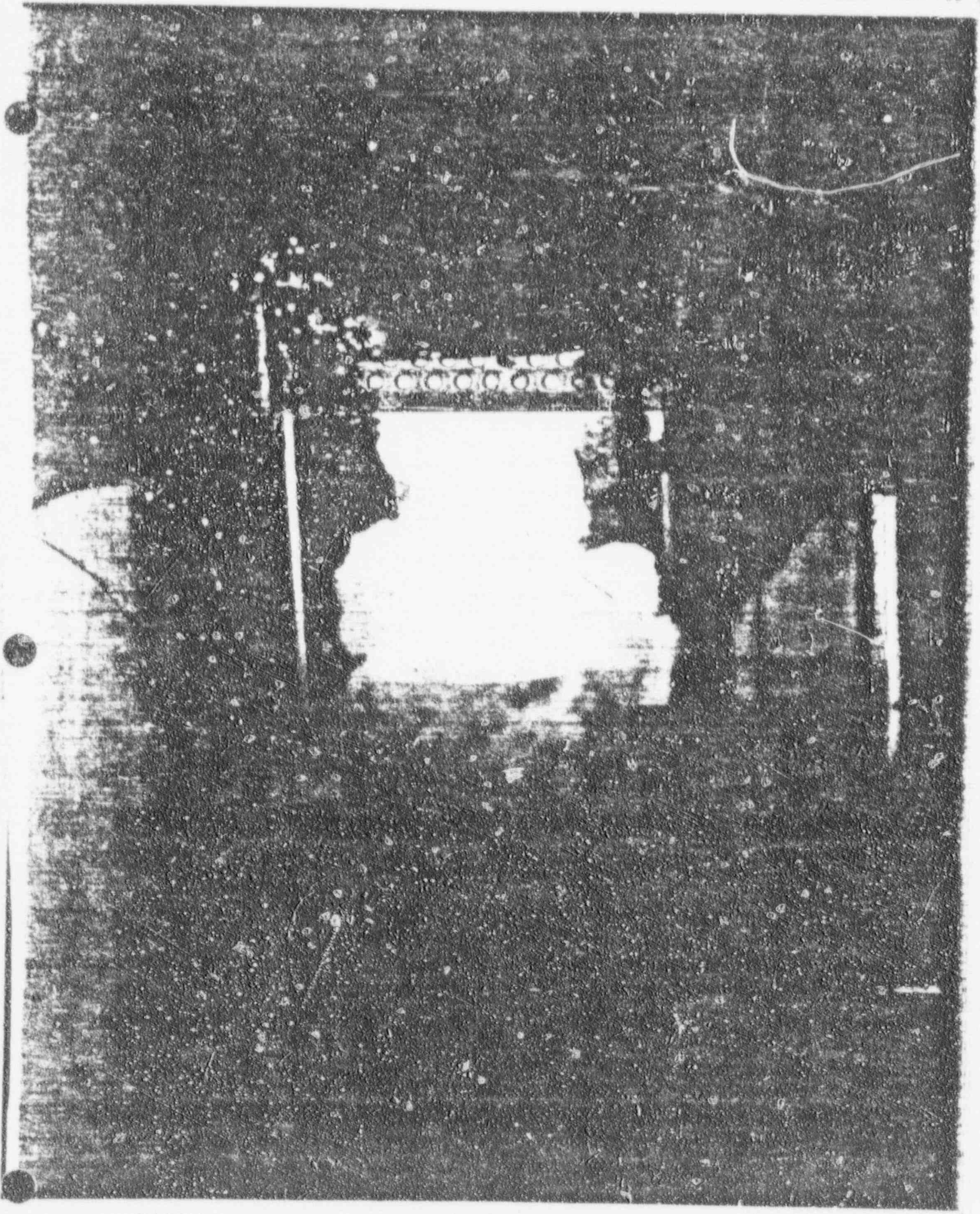
Drain Nozzle Melting Predicted at 2600 Seconds

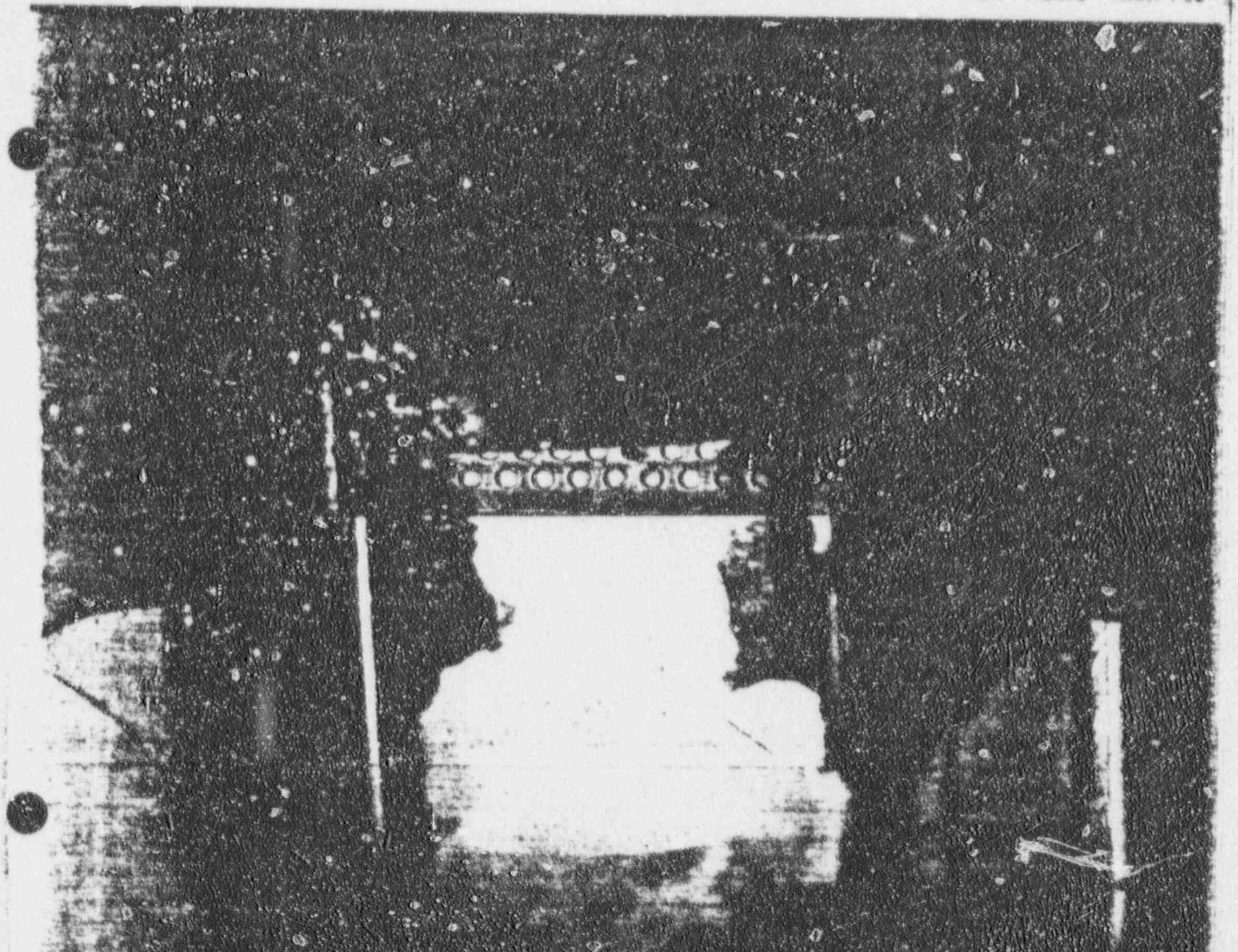


CORE DEBRIS - CONCRETE INTERACTION RESEARCH

D. A. Powers
C. Tinkler
R. Foulds
D. R. Bradley
E. R. Copus

October 24, 1991





THESIS:

SUFFICIENT UNDERSTANDING OF CORE DEBRIS INTERACTIONS WITH CONCRETE IN THE ABSENCE OF WATER HAS DEVELOPED FOR FORESEEN REGULATORY ACTIVITIES.

- **Extensive, Confirmatory Data Base**
- **Predictive Models**

CORE DEBRIS

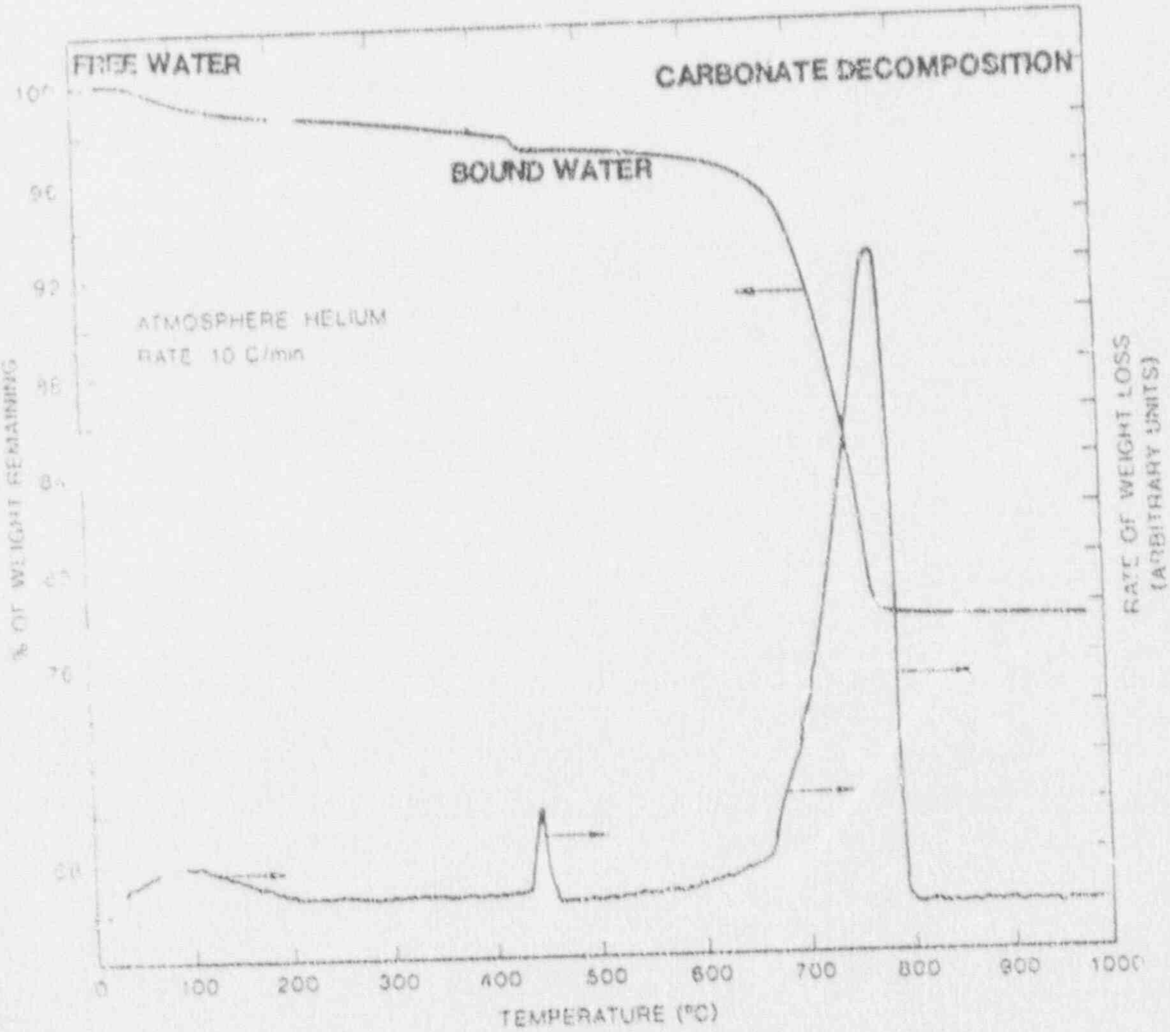
- AT LEAST TWO PHASES
  - METAL (STEEL, UNOXIDIZED CLAD)
  - OXIDE (UO_2, ZrO_2)
- COMPOSITION DEPENDENT ON REACTOR & ACCIDENT
  - 0-80% OF Zr MAY BE UNOXIDIZED
- AMOUNT & TIMING OF EXPULSION FROM REACTOR COOLANT SYSTEM UNCERTAIN
  - TWO MAJOR CASES USUALLY CONSIDERED
- INITIAL TEMPERATURE UNCERTAIN
  - RANGE FROM NEARLY FROZEN SLURRY TO QUITE HOT

CONCRETE

* MIXTURE OF CEMENT & AGGREGATE

- Cement is same for all reactor
- Aggregate is quite variable
- Range spanned by siliceous and calcareous concretes

	SILICEOUS	W/O LIMESTONE	LIMESTONE SILICA SAND
CaO	8.8	31.2	45.4
Al_2O_3	8.3	3.6	1.6
SiO_2	55	35.6	3.6
Na_2O	1.8	0.8	0.1
K_2O	5.4	1.2	0.7
FeO	6.2	1.4	1.2
H_2O	5-8	5-8	5-8
CO_2	1.5	22	36
Melting Range (K)	1400-1700	1400-1700	1650-1850


FREE WATER
CARBONATE DECOMPOSITION
BOUND WATER
ATMOSPHERE HELIUM
RATE 10 C/min
% OF WEIGHT REMAINING
100
96
92
88
84
80
76
68
RATE OF WEIGHT LOSS (ARBITRARY UNITS)
0
100
200
300
400
500
600
700
800
900
1000
TEMPERATURE (°C)

GAS REACTIONS

- WATER VAPOR REACTS TO FORM H_2

$2H_2O + Zr \rightarrow ZrO_2 + 2H_2$

$3H_2O + 2Cr \rightarrow Cr_2O_3 + 3H_2$

$H_2O + Fe \rightarrow FeO + H_2$

- CO_2 REACTS TO FORM CO

$2CO_2 + Zr \rightarrow ZrO_2 + 2CO$

$3CO_2 + 2Cr \rightarrow Cr_2O_3 + 3CO$

$CO_2 + Fe \rightarrow FeO + CO$

SAFETY ISSUES OF CORE DEBRIS INTERACTIONS WITH CONCRETE

- GAS GENERATION
  - COMBUSTIBLE GASES (H_2/CO) ADD TO DEFLAGRATION THREAT TO CONTAINMENT
  - NON-CONDENSIBLE GASES PRESSURIZE CONTAINMENT OVER LONG-TERM
  - H_2 & CO INTERFACE IN NATURAL CIRCULATION HEAT TRANSFER

- BASEMAT PENETRATION & COLLATERAL DAMAGE
  - BASEMAT PENETRATION OF LOWER CONCERN
  - COLLATERAL DAMAGE TO REACTOR INTERNALS IMPORTANT FOR SOME REACTORS

- AEROSOL & RADIONUCLIDE RELEASE

DATA BASE
FOR
CORE DEBRIS/CONCRETE INTERACTIONS

- EXTENSIVE DATA BASE ON INTERACTIONS
- DATA BASE USED TO CONFIRM OR VALIDATE MODELS
- WIDE RANGE OF MATERIALS, CONCRETES AND TEST CONDITIONS BECAUSE OF UNCERTAINTY IN THE IN-VESSEL PROGRESSION OF THE ACCIDENT AND CHANGES THAT OCCUR OVER THE LONG DURATION OF THE INTERACTIONS

MAJOR TEST SERIES

- HOT SOLID TESTS
  - Solidified Steel or UO_2 attacking concrete

- SURC 1/2
  - Sustained, Molten, UO_2-ZrO_2-Zr Interactions With Limestone & Siliceous Concretes
  - 1 Dimensional

- SURC 3/4
  - Effects of Zr on Metallic Melt Interactions
  - 1 Dimensional

- BETA TESTS
  - Metal Melts
  - 2 Dimensional

- ACE TESTS
  - UO_2-ZrO_2 Melts
  - 1 Dimensional

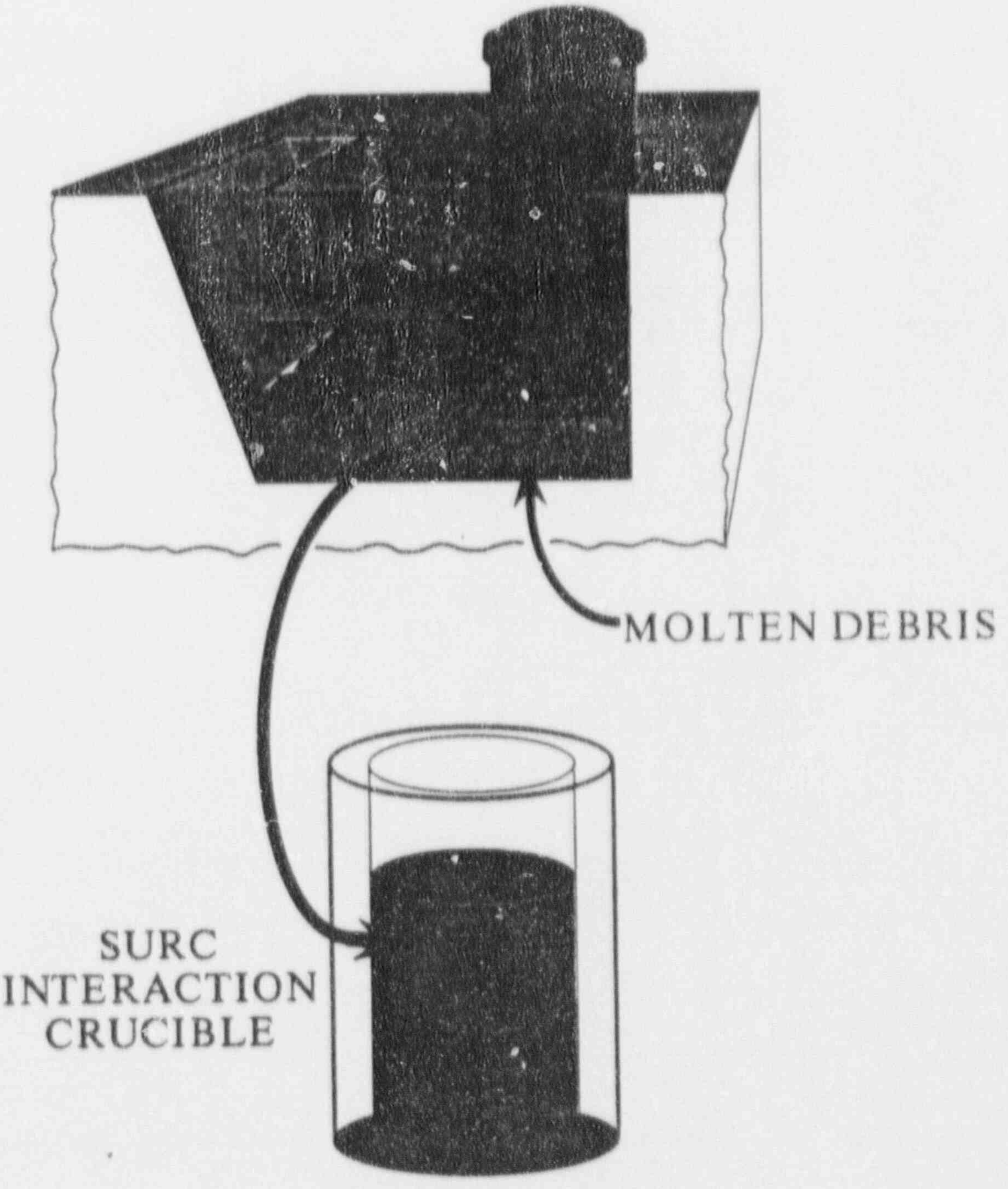
METALLIC TESTS

TESTS	POWER	CONCRETE TYPE	GEOMETRY	RESPONSE REGIME
HSS-1	SUSTAINED	LCS	1D - 15 CM	LONG TERM
FRAG 1, 4	SUSTAINED	SILICEOUS	2D - 20 CM	LONG TERM
FRAG 2, 3	SUSTAINED	LCS	2D - 20 CM	LONG TERM
SWISS 1, 2	SUSTAINED	LCS	1D - 20 CM	QSS POOL
TURC 1T, 1SS	TRANSIENT	LCS	1D - 40 CM	TRANSIENT
SURC 3, QTD	SUS + ZR	LIMESTONE	1D - 20 CM	QSS POOL
SURC 3A, QTE	SUS + ZR	LIMESTONE	2D - 20 CM	QSS POOL
BETA 1, 2	SUSTAINED	SILICEOUS	2D - 40 CM	QSS POOL
BETA3 SERIES	SUSTAINED	LIMESTONE	2D - 40 CM	QSS POOL
BETA5 SERIES	SUS + ZR	SILICEOUS	2D - 40 CM	QSS POOL
SURC 4	SUS + ZR	SILICEOUS	1D - 40 CM	QSS POOL

OXIDIC TESTS

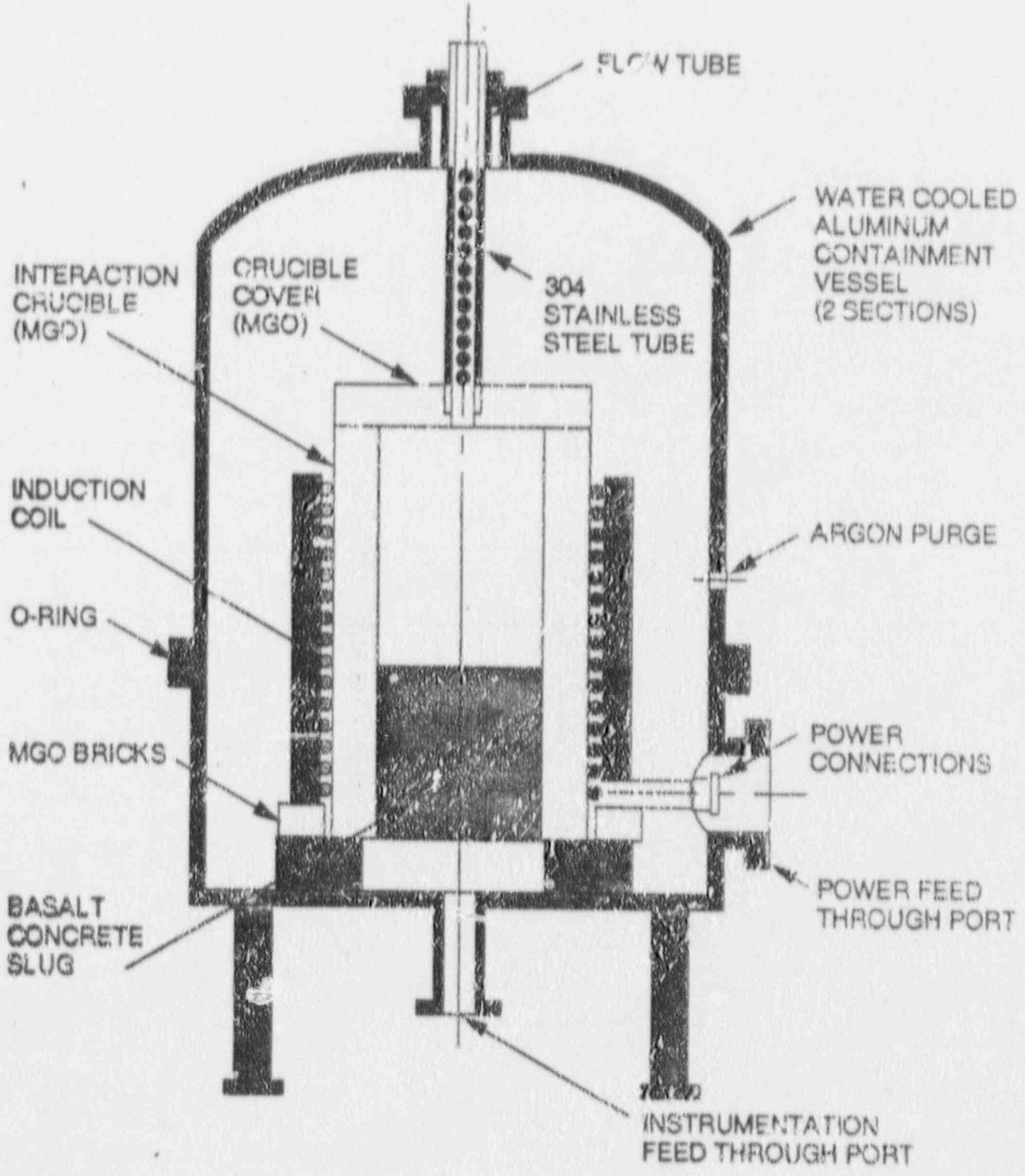
TESTS	POWER	CONCRETE TYPE	GEOMETRY	RESPONSE REGIME
HSS-3	SUSTAINED	LCS	1D - 15 CM	LONG TERM
TURC 3, 3A	TRANSIENT	LCS	1D - 40 CM	TRANSIENT
SURC1.a	SUS + ZR	LIMESTONE	1D - 40 CM	QSS POOL
.b	SUSTAINED	LIMESTONE	1D - 40 CM	QSS POOL
.c	SUSTAINED	LIMESTONE	1D - 40 CM	QSS POOL
SURC2.a	SUS + ZR	SILICEOUS	1D - 40 CM	QSS POOL
.b	SUSTAINED	SILICEOUS	1D - 40 CM	QSS POOL
.c	SUSTAINED	SILICEOUS	1D - 40 CM	QSS POOL
MACE 0	SUS + ZR	LCS	2D - 30 CM	QSS POOL
ACE L SERIES	SUS + ZR	ALL TYPES	1D - 50 CM	QSS POOL

REACTOR ACCIDENT

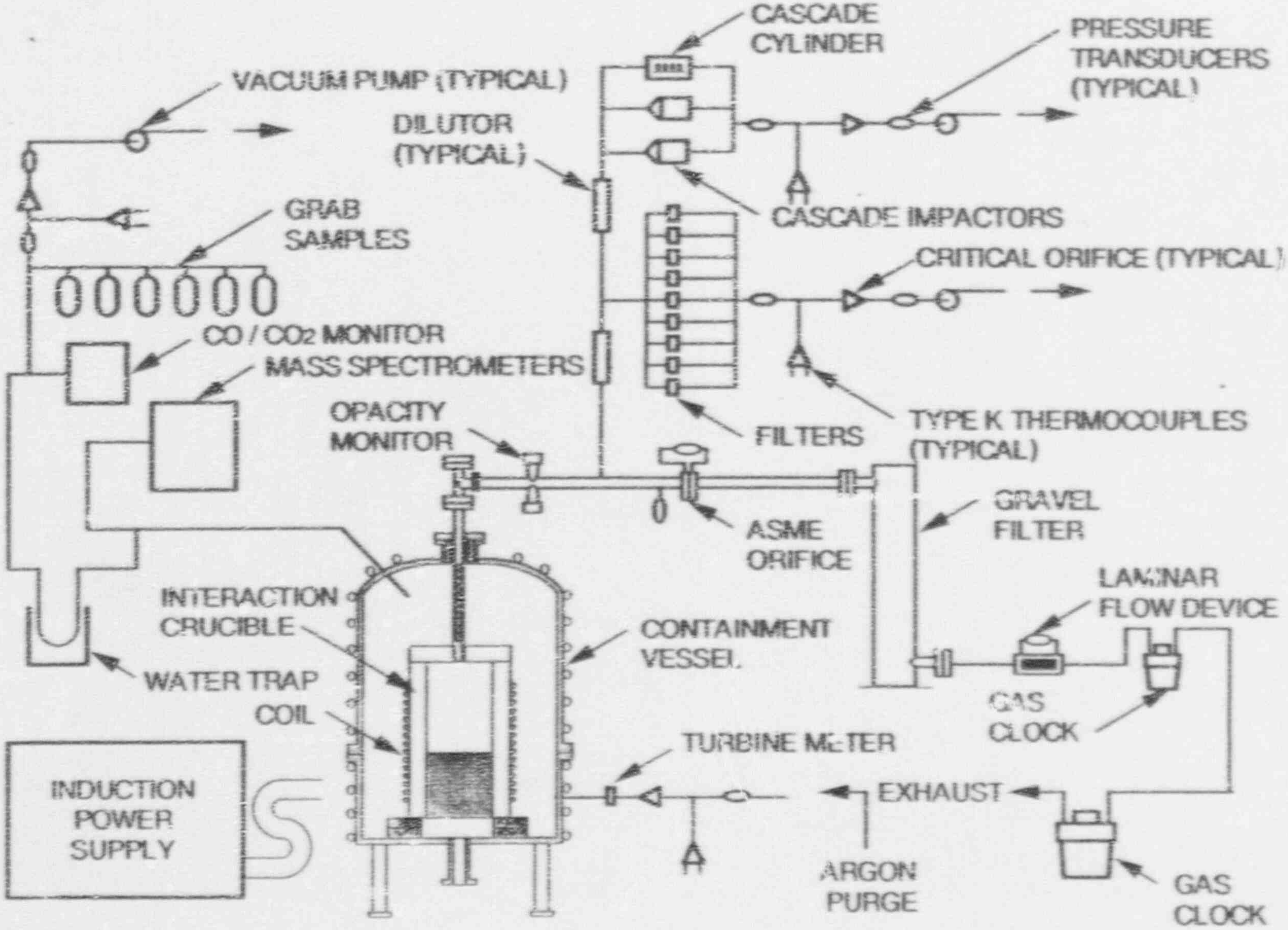


SURC 4 TEST

- OBJECTIVE
  - SHOW THE EFFECT OF Zr ON MOLTEN STEEL/CONCRETE INTERACTIONS
- PROCEDURE
  - STEADY-STATE INTERACTION OF MOLTEN STAINLESS STEEL WITH CONCRETE ESTABLISHED
  - 20 kg. ZrO_2 ADDED
  - TRANSIENT OBSERVED


FLOW TUBE
WATER COOLED
ALUMINUM
CONTAINMENT
VESSEL
(2 SECTIONS)
INTERACTION
CRUCIBLE
(MGO)
CRUCIBLE
COVER
(MGO)
304
STAINLESS
STEEL TUBE
INDUCTION
COIL
ARGON PURGE
O-RING
MGO BRICKS
POWER
CONNECTIONS
POWER FEED
THROUGH PORT
BASALT
CONCRETE
SLUG
INSTRUMENTATION
FEED THROUGH PORT

FLOW TRAIN SCHEMATIC



SURC INSTRUMENTATION

PARAMETER	DEVICES	NO. OF CHANNELS
AEROSOLS	SEDS	10 DP AND DT
	CYCLONES	1
	IMPACTORS	8
	FILTERS	22
	PHOTOMETERS	1 (ON LINE)
GAS CHEMISTRY	MASS SPECTROMETER	10 (ON LINE)
	GRAB SAMPLES	8
	INFRARED DETECTOR	2 (ON LINE)
GAS FLOW RATE	CRITICAL ORIFICE	4
	LAMINAR FLOW ELEMENT	1
	TURBINE	2
	POSITIVE DISPLACEMENT	2
HEAT TRANSFER	K AND S THERMOCOUPLES	40 SIDEWALL
	W/RE THERMOCOUPLES	6 LID
MELT TEMP	PYROMETER	3
	W/RE THERMOCOUPLES	3
ABLATION	K AND S THERMOCOUPLES	72 BASEMAT

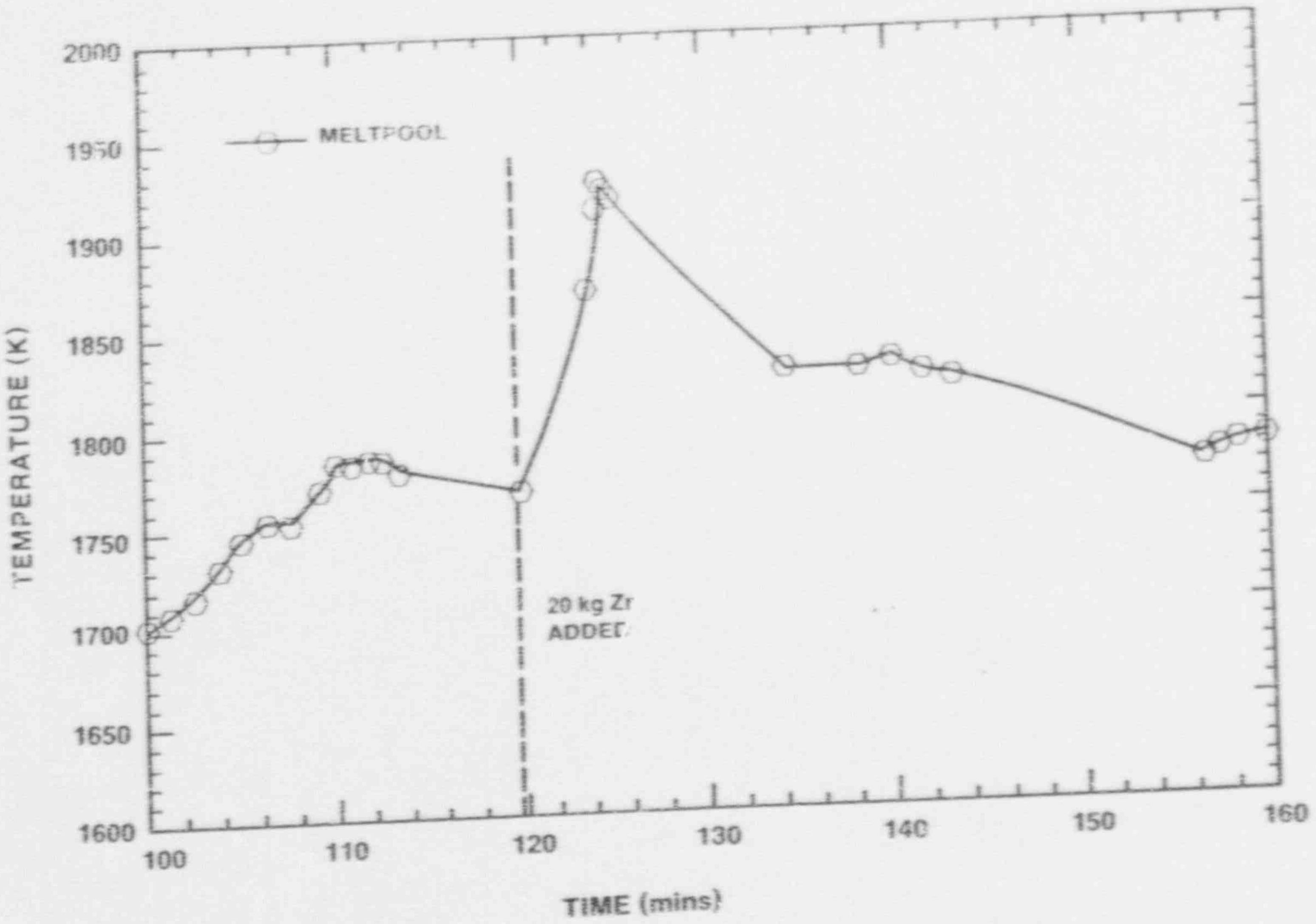
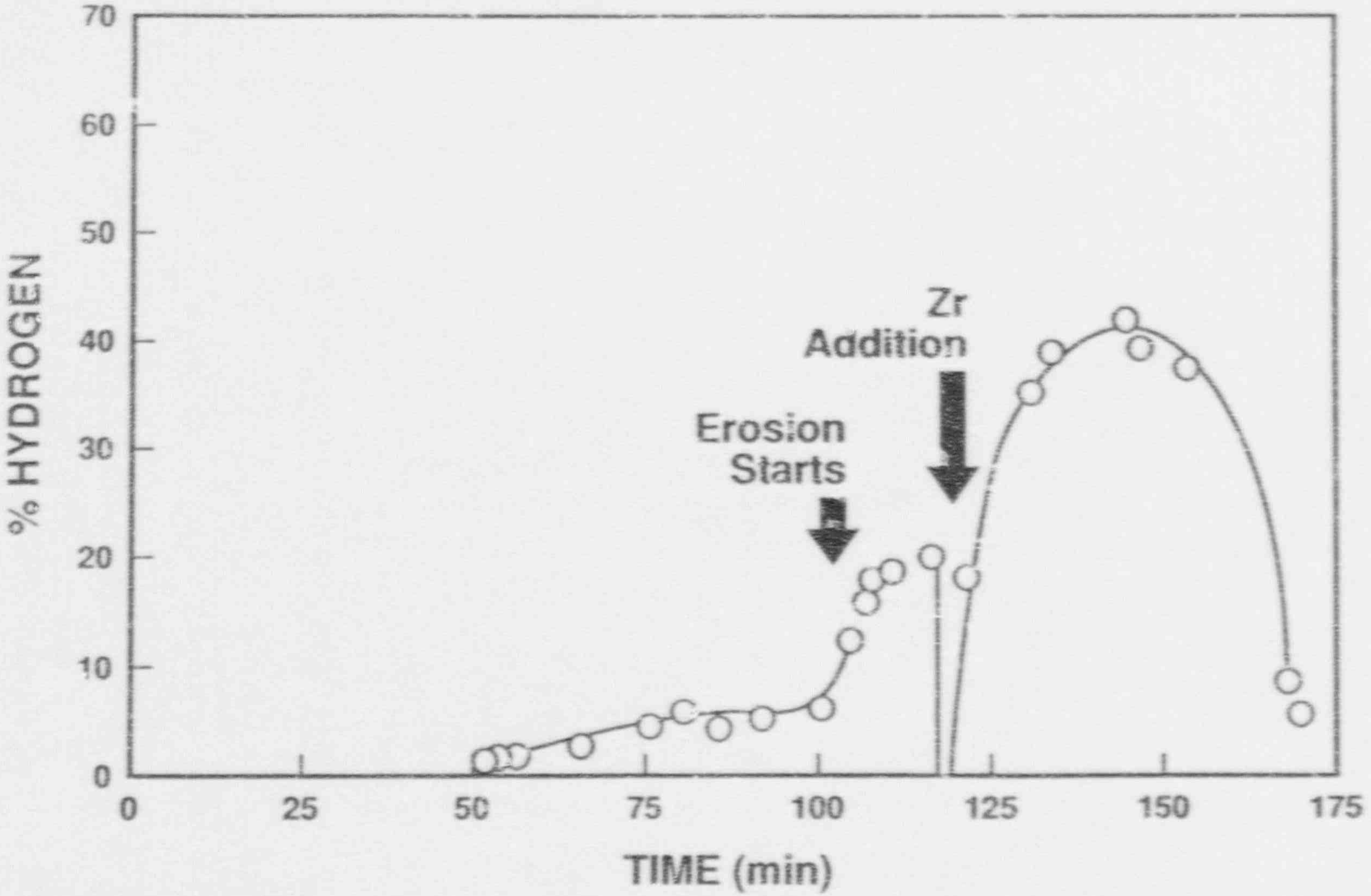
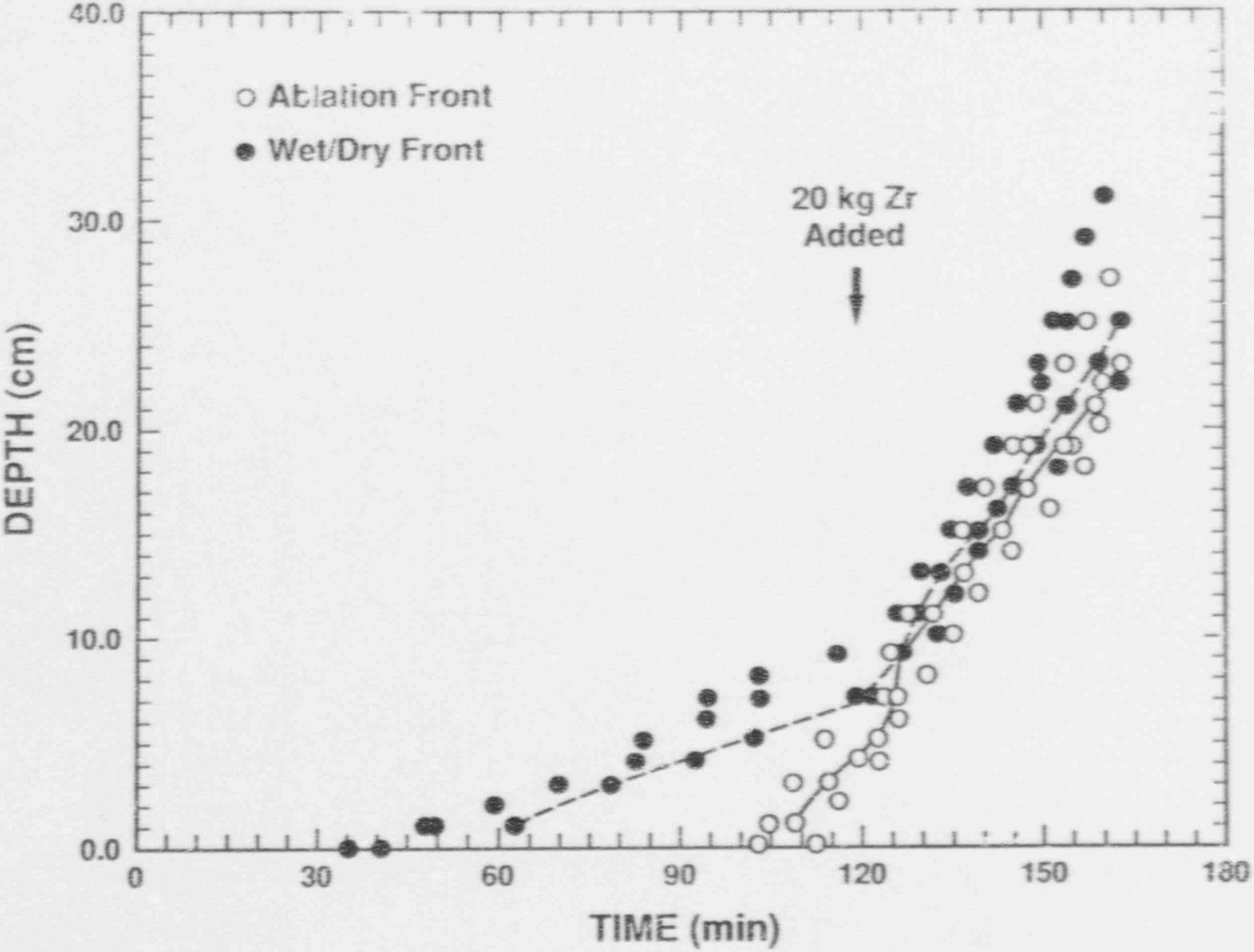


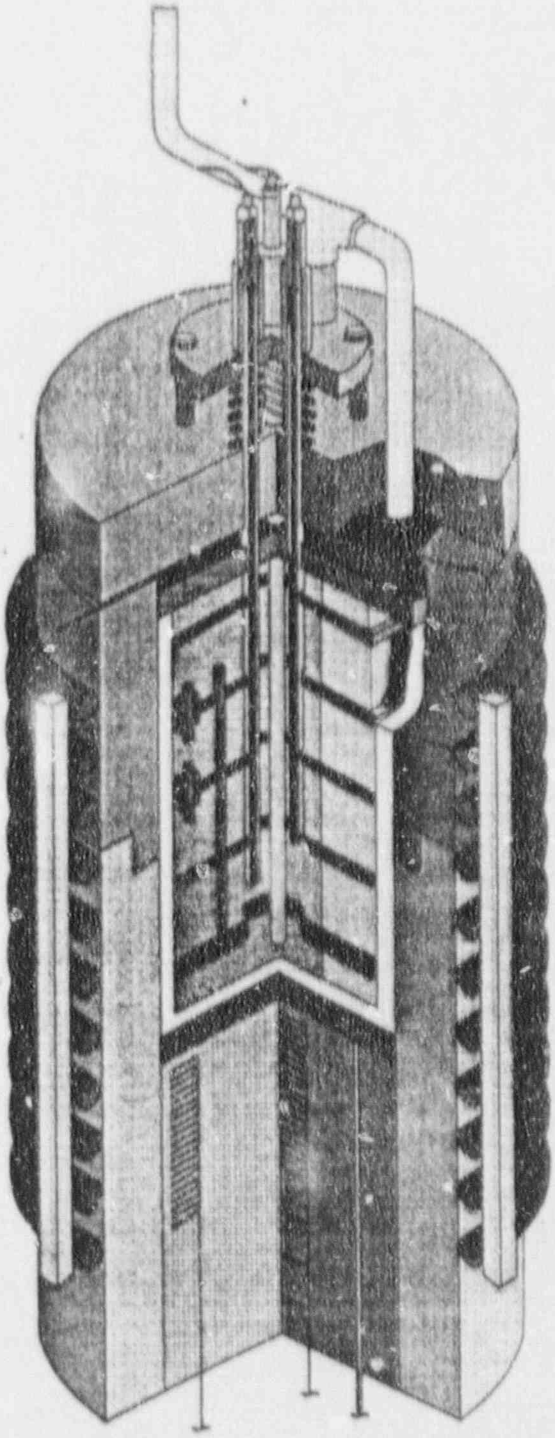
Figure 7.1.7 - SURC 4 Meltpool Temperature as a function of time

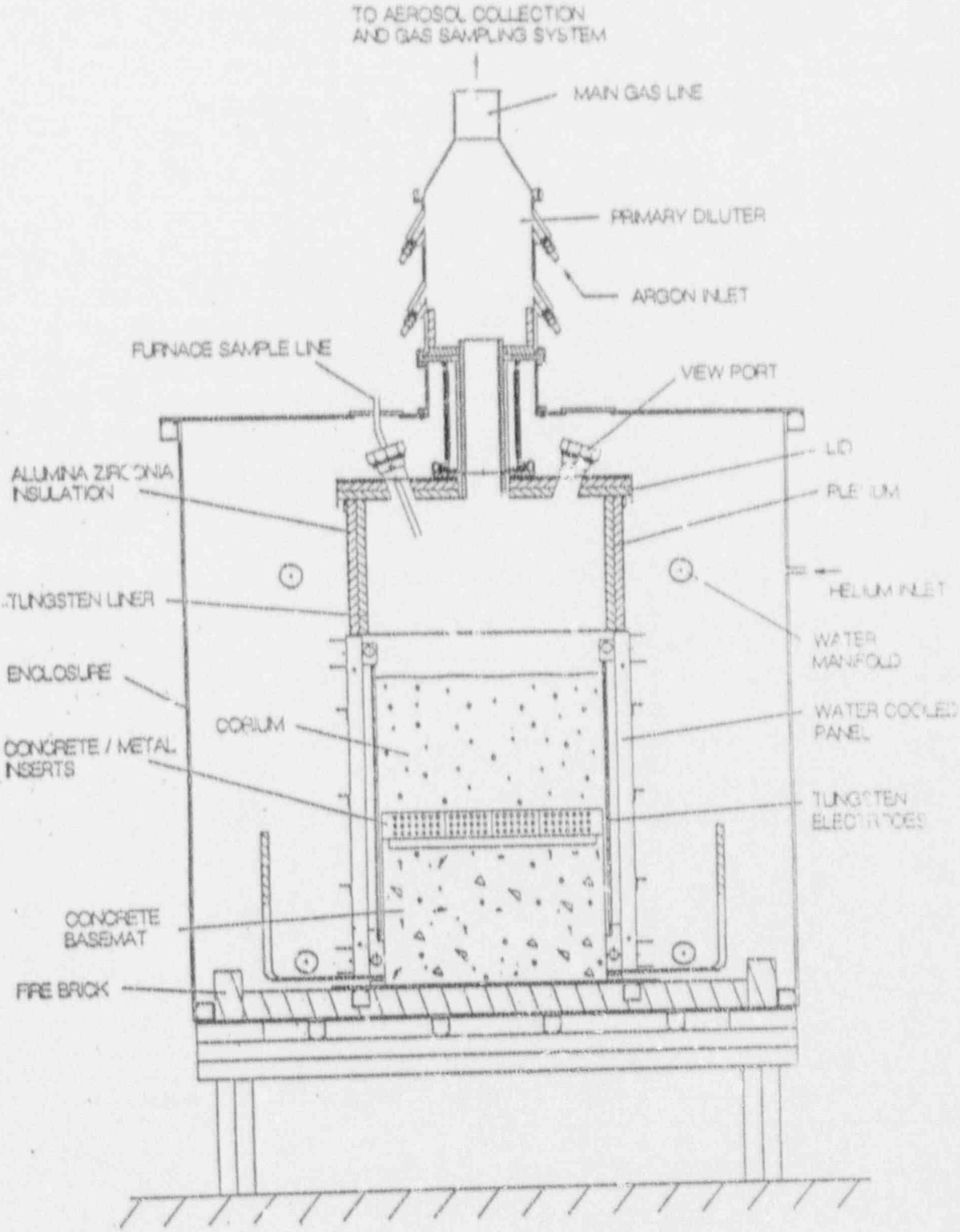
SURC-4 GAS COMPOSITION



400 K AND 1600 K ISOTHERMS







ACE MCCI Test Apparatus

DATA BASE

- THERE IS AN EXTENSIVE DATA BASE
  - Metal Melts & Oxide Melts
  - Solidified Material
  - Effects of Zirconium
  - Various Concretes

- DATA BASE DOES NOT INVOLVE ALL POSSIBLE COMBINATIONS -- CODES NEEDED TO INTERPOLATE AND EXTRAPOLATE

- AREAS OF WEAKNESS
  - Horizontal Attack not as Well Studied as Downward Attack on Concrete

  - Mixed Metal/Oxide Tests With Comparable Phase Volumes Haven't Been Done

  - Very Long Duration (>10 hr.) Tests Have Not Been Done

COMPUTER CODES

CORCON

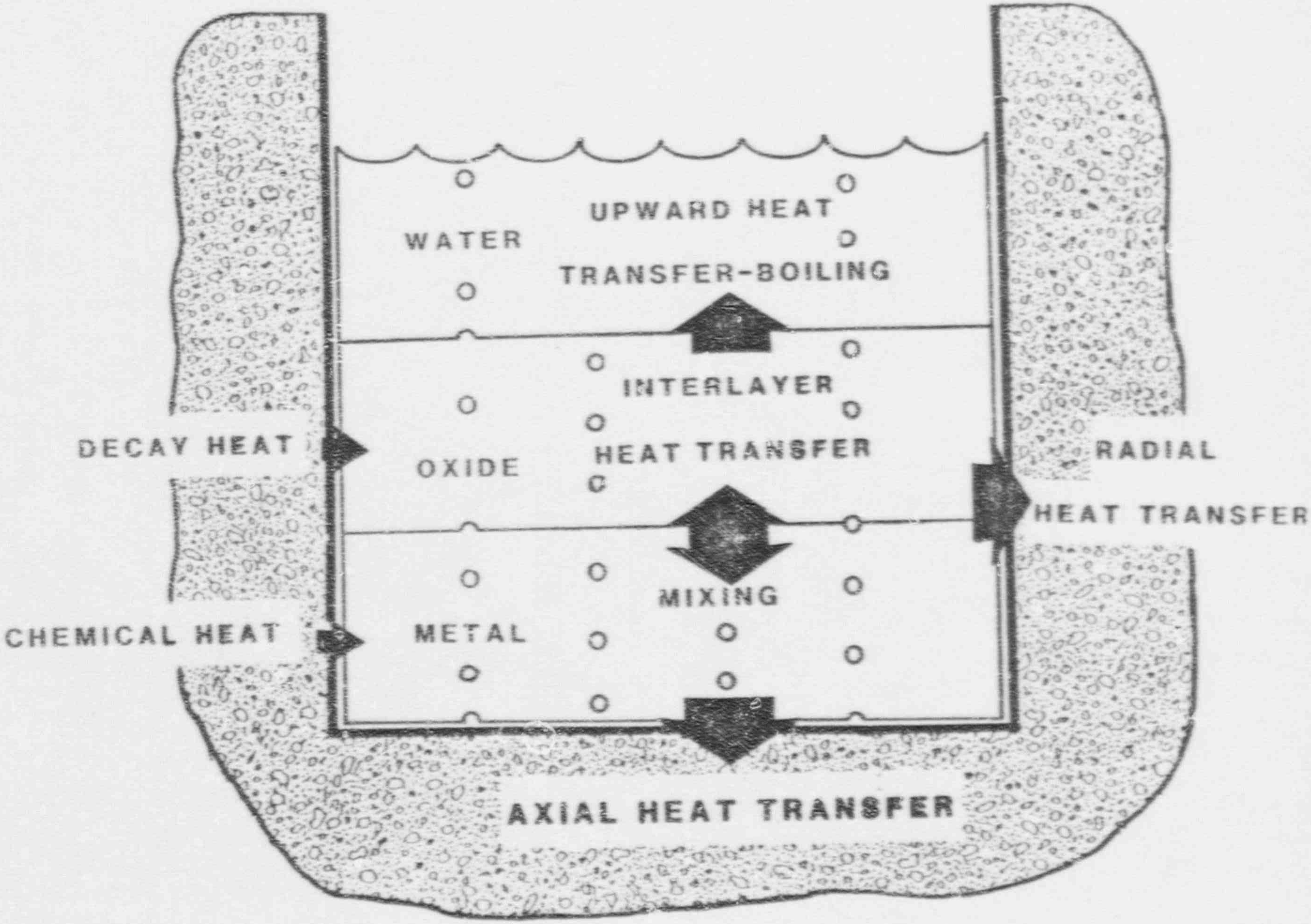
- DEVELOPED BY NRC

WECHSL

- DEVELOPED IN GERMANY

DECOMP

- DEVELOPED BY IDCOR


WATER
UPWARD HEAT
TRANSFER-BOILING
INTERLAYER
HEAT TRANSFER
OXIDE
MIXING
METAL
DECAY HEAT
CHEMICAL HEAT
RADIAL
HEAT TRANSFER
AXIAL HEAT TRANSFER

MAJOR MODELS IN CORCON

- HEAT TRANSFER
  - HEAT GENERATION
    - Decay
    - Chemical
  - HEAT LOSS
    - To Concrete
    - Between Melt Phases
    - To atms. or Water
- CHEMICAL MODELS
  - GAS REACTIONS TO FORM H_2 & CO
  - MELT PROPERTIES
    - Viscosity
    - Density
    - Surface Tension
    - Melting
  - VAPORIZATION OF RADIONUCLIDES AND AEROSOL PRODUCTION

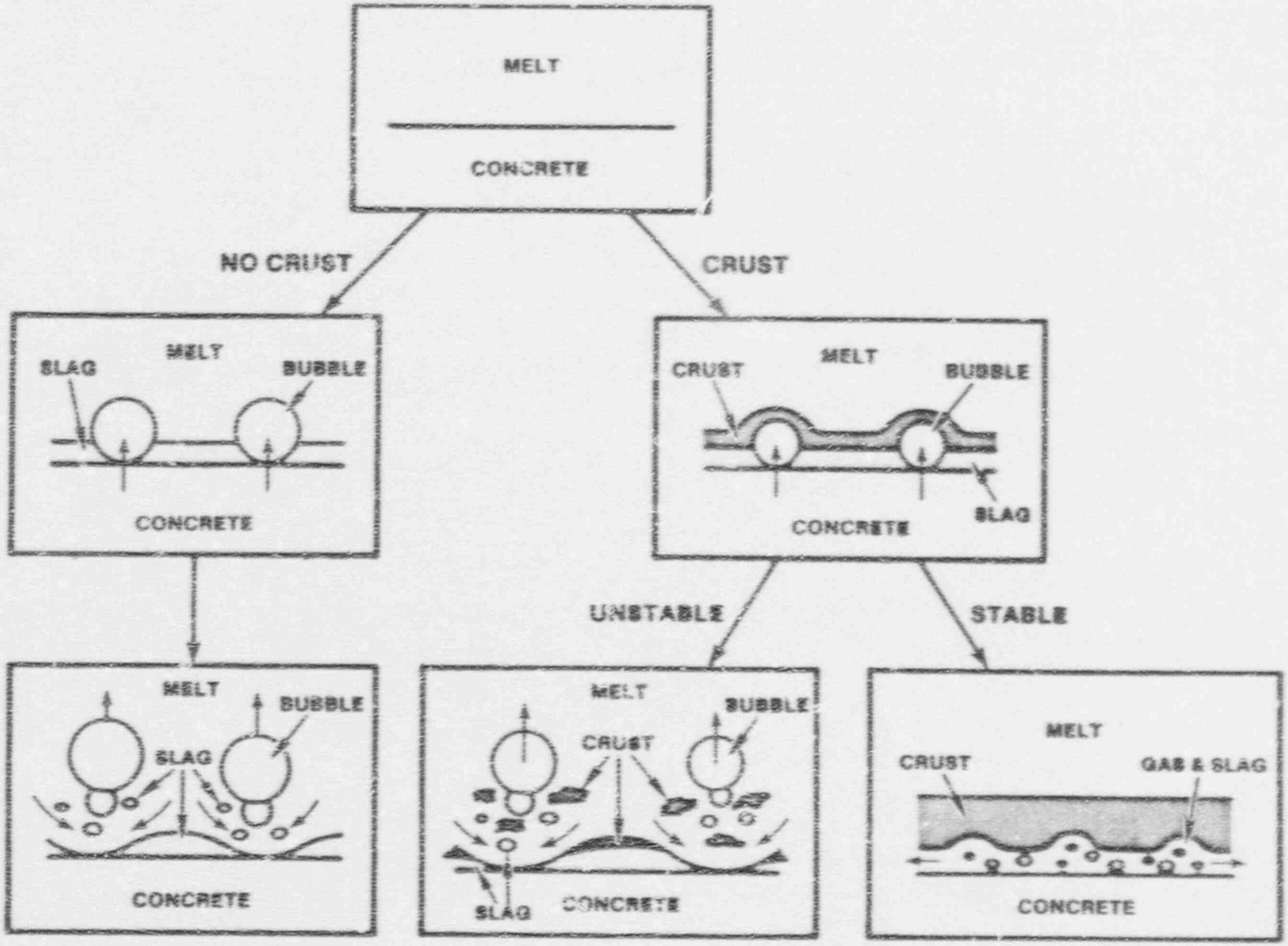
Model Improvements in CORCON-Mod3

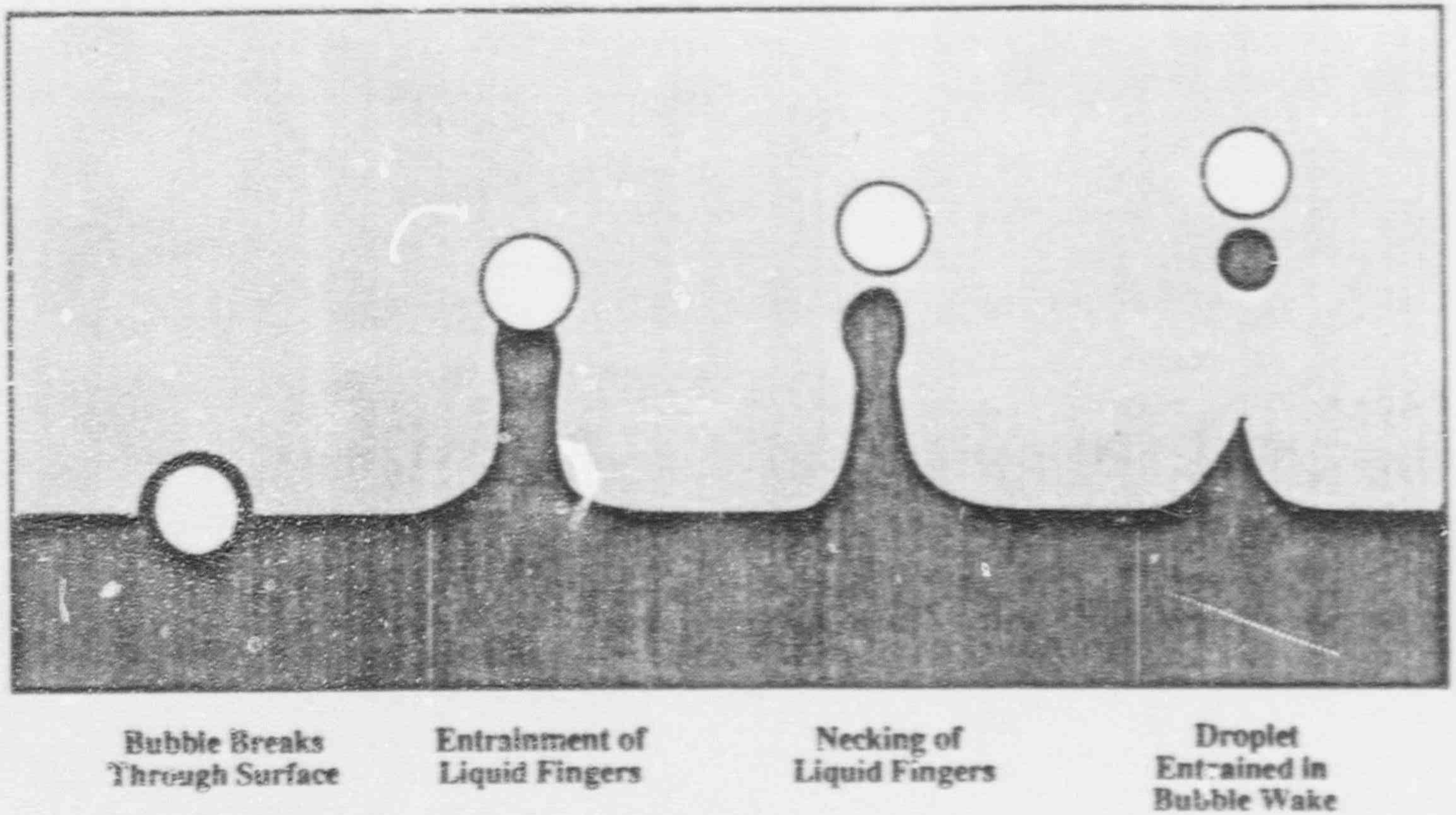
- Improved axial and radial heat transfer models*
- Inclusion of condensed phase chemistry (oxide/metal reactions) *
- Improved coolant heat transfer models
  - Includes effects of subcooling and gas injection on film boiling
- Addition of models for interphase mixing and stratification
- Improved models for bubble behavior (e.g., bubble size, rise velocity, and void fraction)
- Consolidation of CORCON and VANESA into a single code
- Ongoing work: improved phase diagrams, non-ideal solution chemistry

Evolution of Melt-Concrete Heat Transfer Models

- Early MCCI models used empirically determined heat transfer coefficients
  - Impossible to address the range of reactor accident conditions using a few experiments
- Second generation models (CORCON-Mod1, CORCON-Mod2) assumed a stable gas film at the melt-concrete interface
  - Model based on observations of experiments using dry ice as a simulant for concrete
  - These models could not accurately predict the results from early integral experiments
- CORCON-Mod3 assumes intermittent melt-concrete contact and treats transient growth and removal of concrete slag from the interface
  - This model has been validated against results from experiments with metallic and oxidic melts

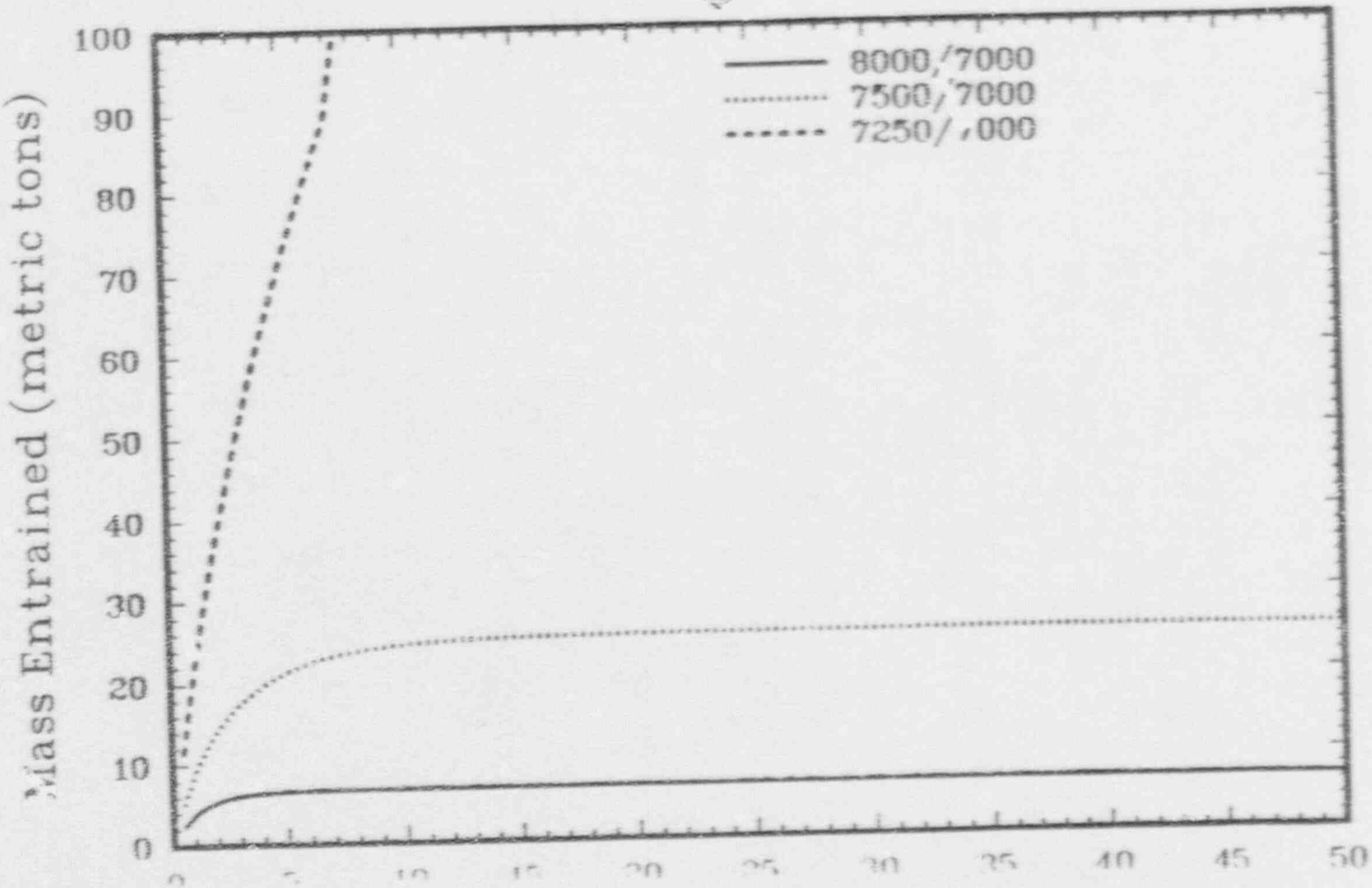
M[illegible]NCRETE [illegible]ANSFER [illegible]NA





Mechanism of Droplet Entrainment

Mass of Entrained Oxide vs. Density Ratio, for $V_g = 10$ cm/s



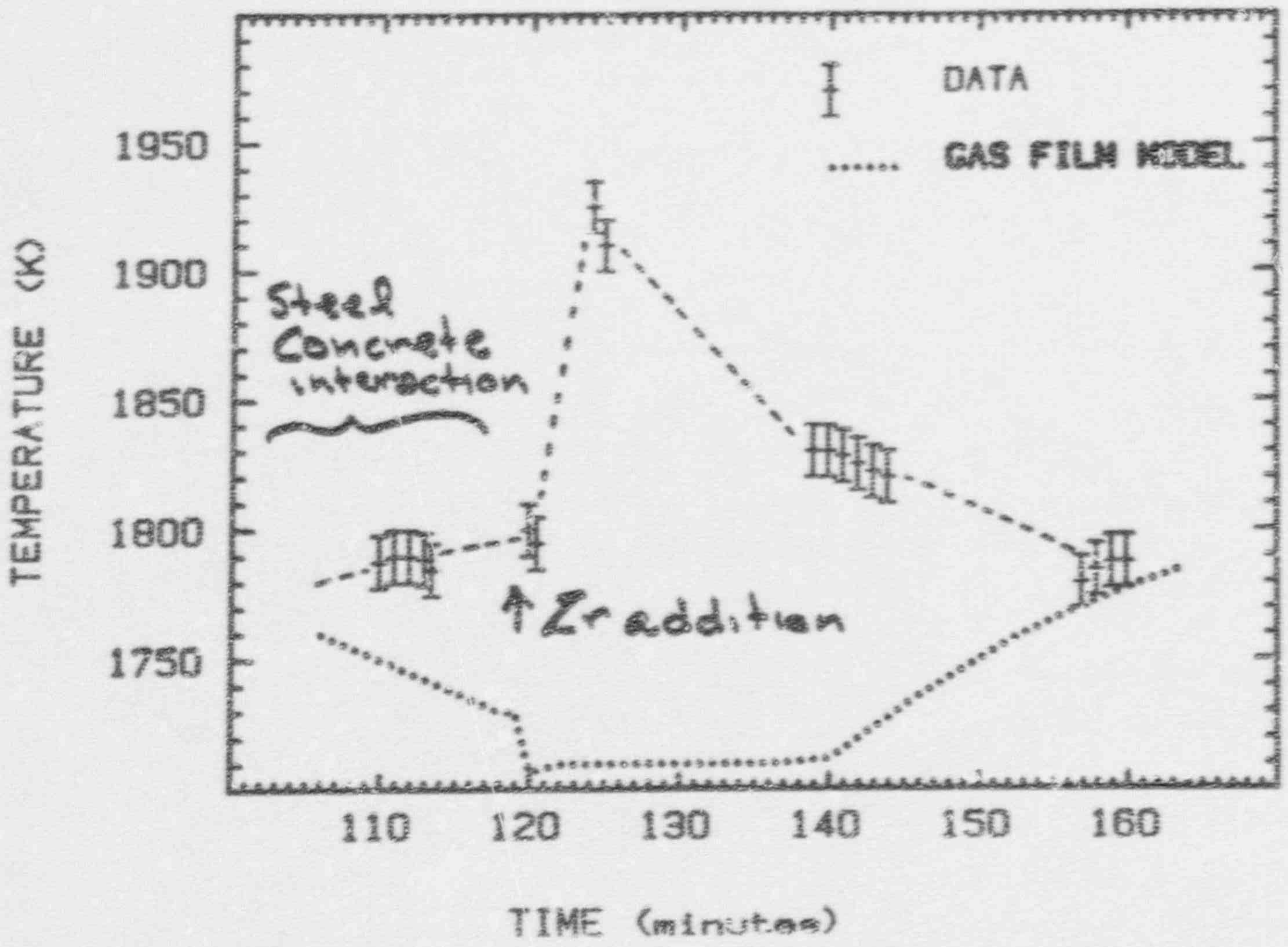
Evolution of Chemistry Models

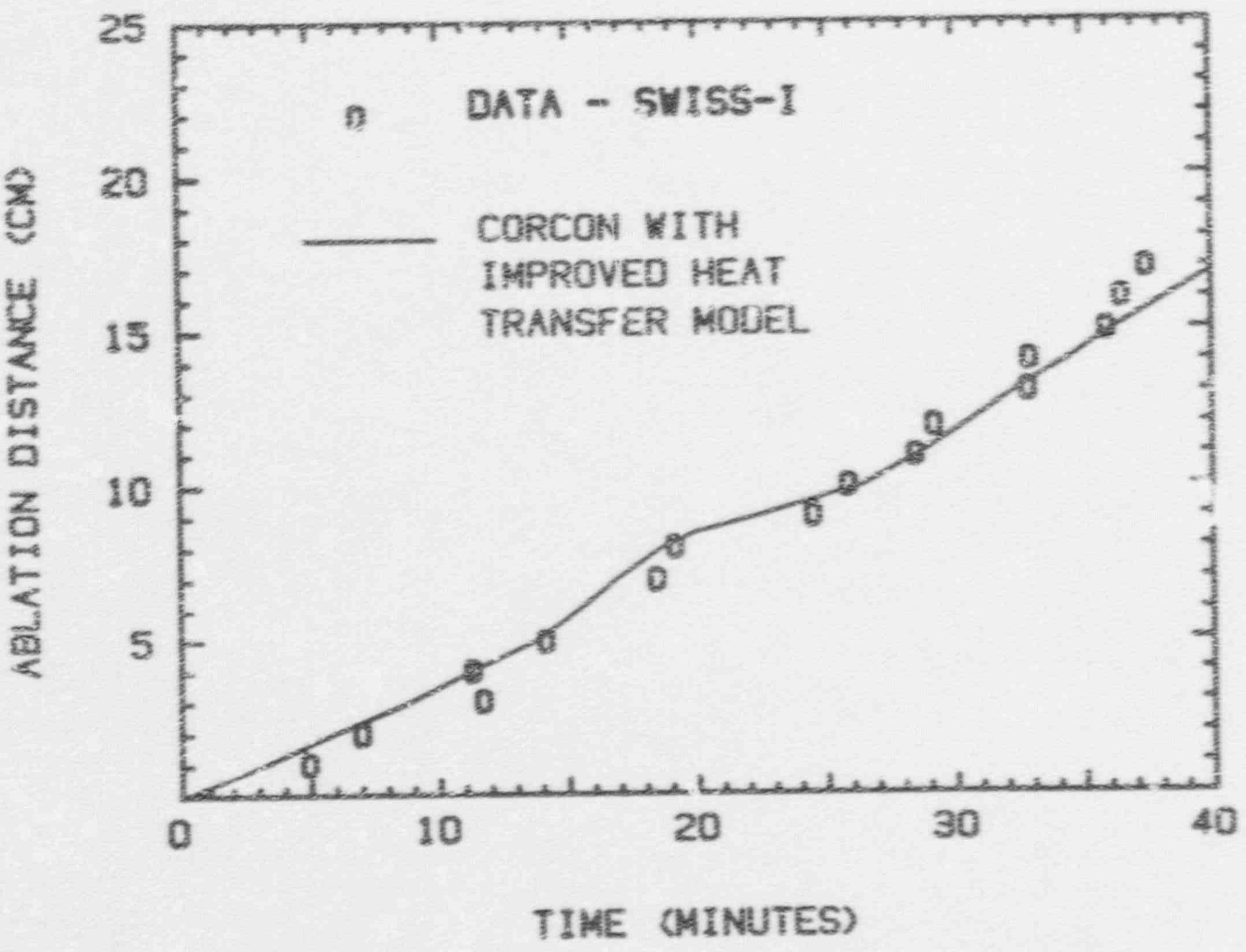
- Early models assumed an hierarchal oxidation of metallic core debris with all reactions going to completion

- Second generation models calculated chemical equilibrium between metallic core debris and gases from the concrete
  - Reactions between metals and oxide were not considered
  - These models could not predict the vigorous condensed phase reactions observed in the SURC-4 experiment

- CORCON-Mod3 treats chemical equilibrium between oxides, metals, and gases
  - The code accurately predicts the chemical energy generation observed in the SURC-4 experiment

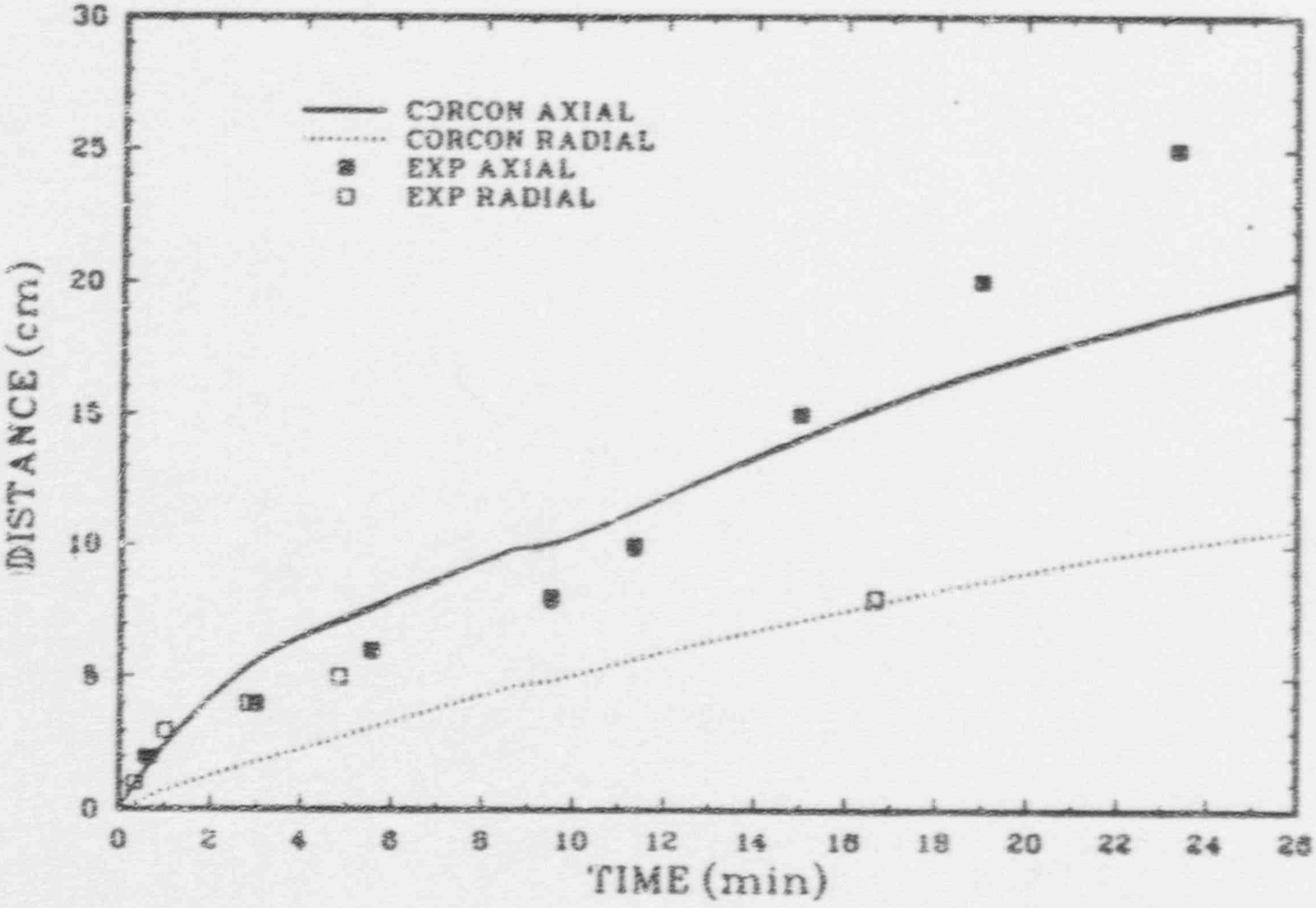
- CORCON-Mod3 currently assumes ideal solution chemistry when modeling melt thermochemistry
  - Experimental evidence suggests that non-idealities may be important
  - Especially true for modeling radionuclide release
  - Non-ideal solution chemistry is currently being added to CORCON-Mod3


DATA
GAS FILM MODEL
TEMPERATURE (K)
1950
1900
1850
1800
1750
Steel Concrete interaction
↑Zr addition
110
120
130
140
150
160
TIME (minutes)

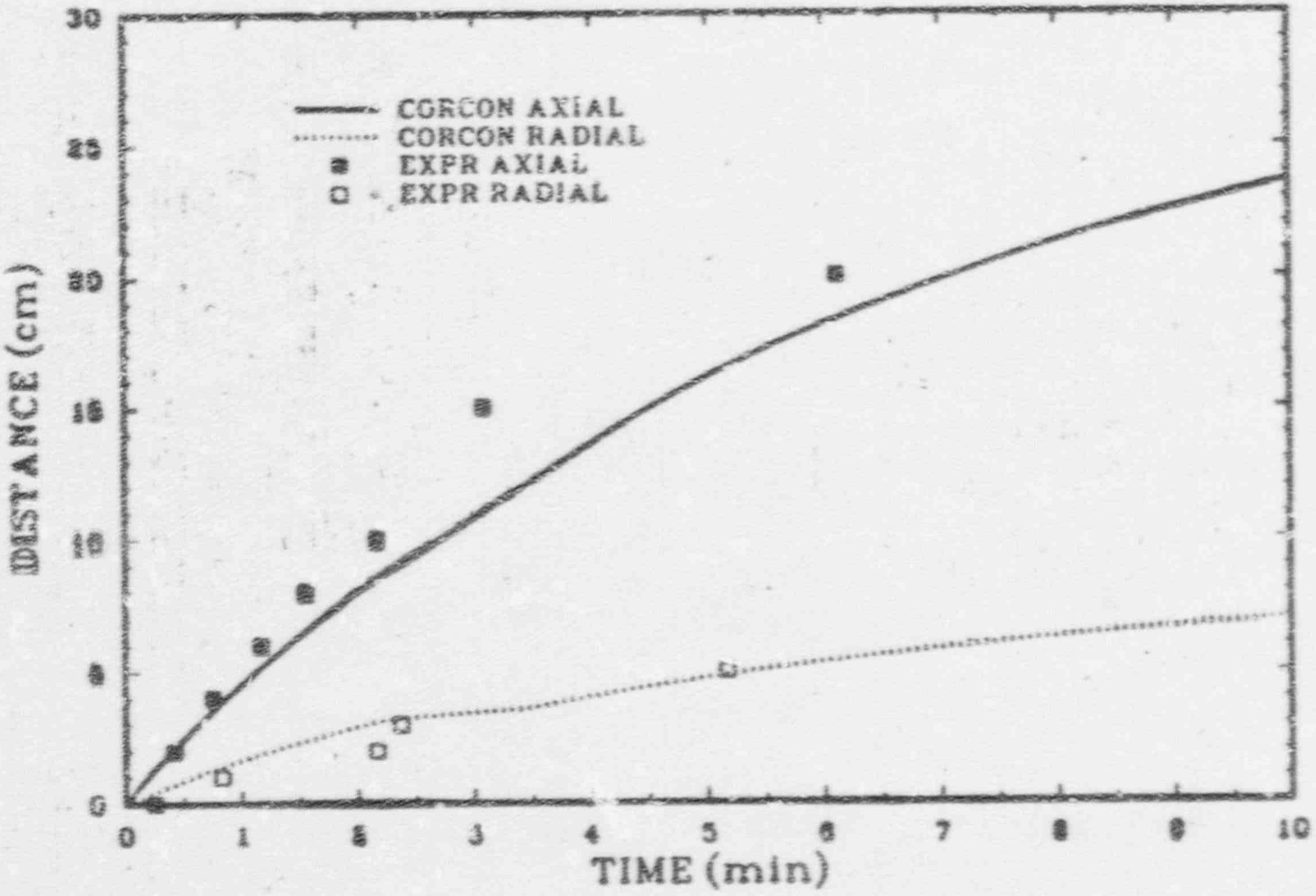


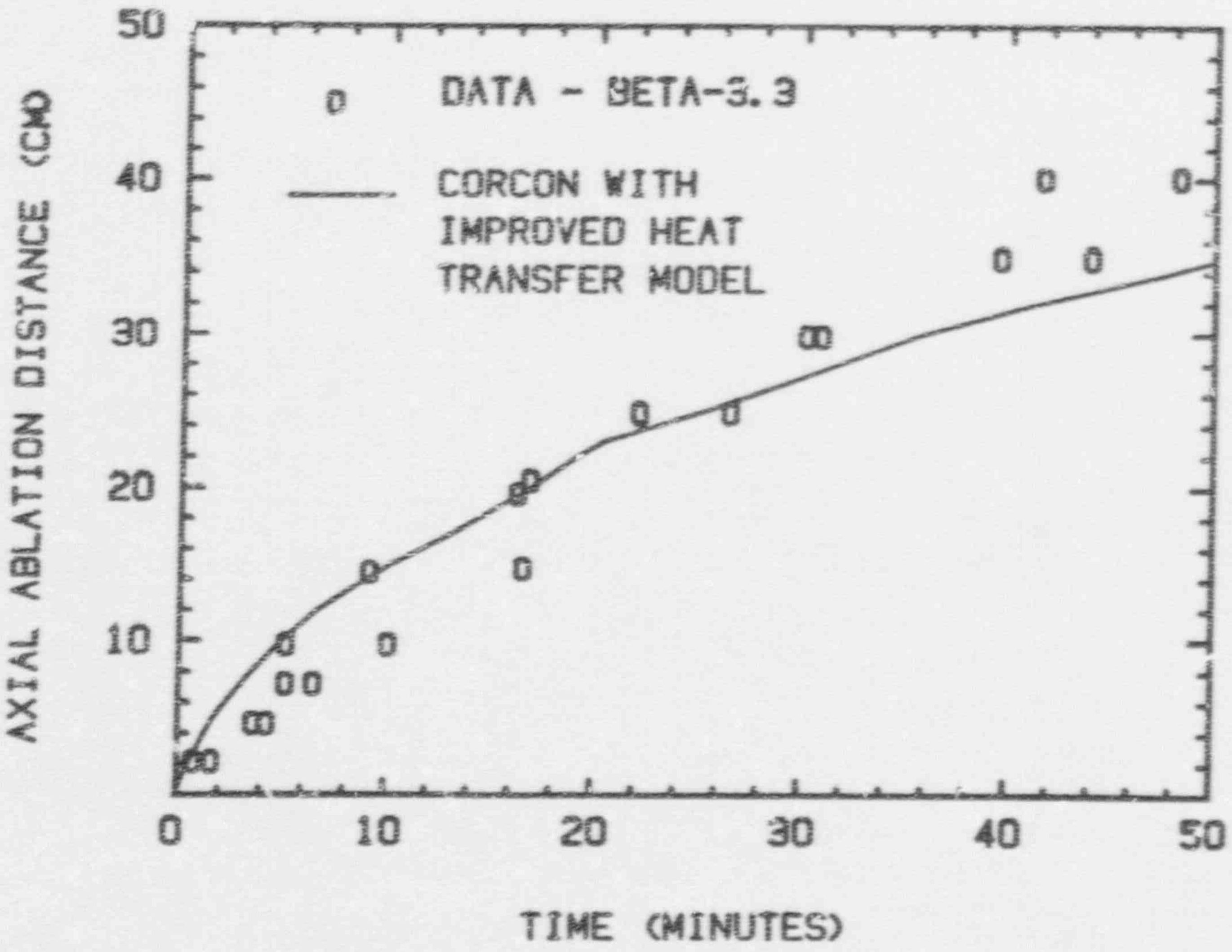
0
DATA - SWISS-I
CORCON WITH
IMPROVED HEAT
TRANSFER MODEL
ABLATION DISTANCE (CM)
25
20
15
10
5
0
10
20
30
40
TIME (MINUTES)

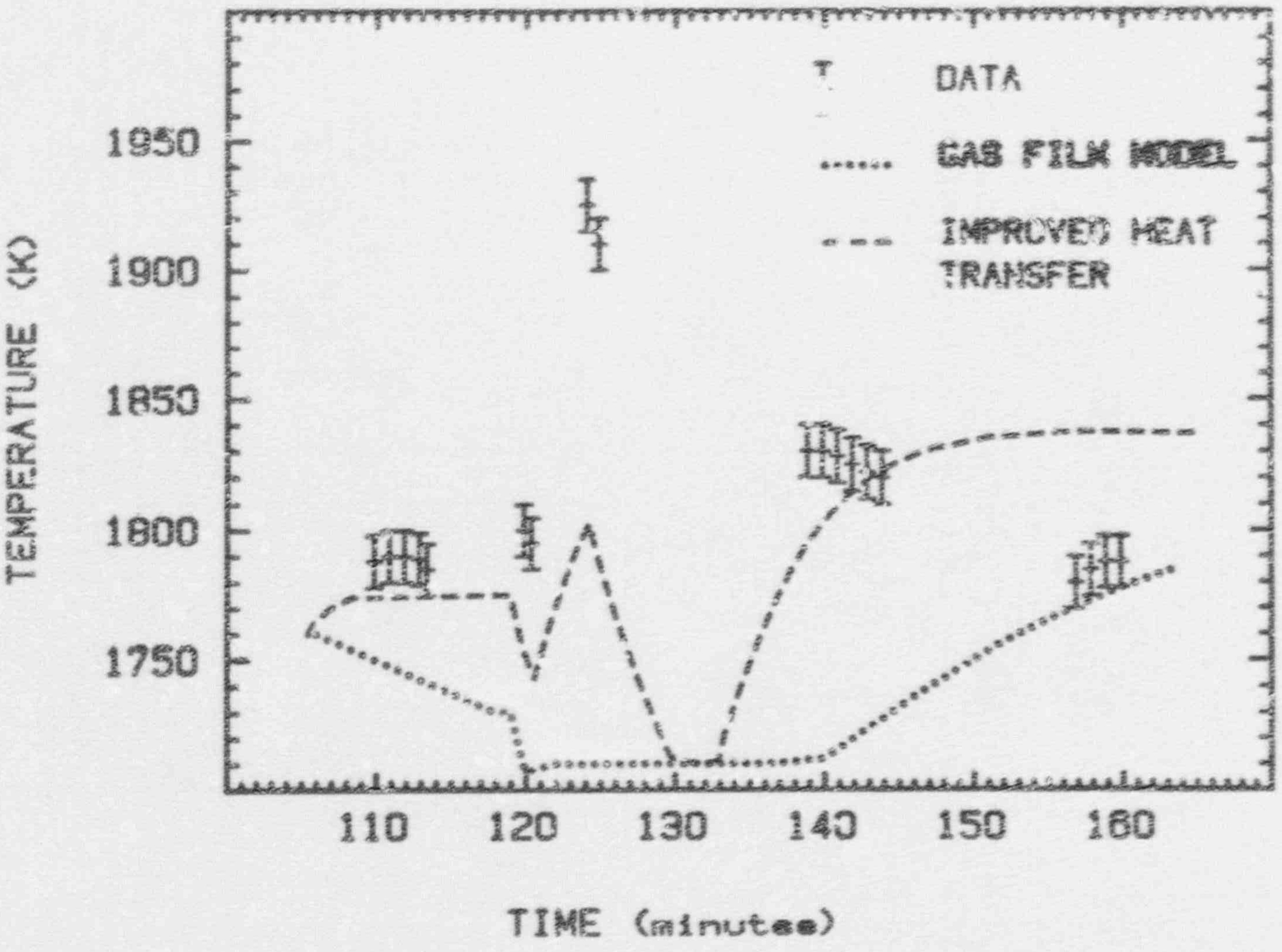
BETA TEST V0.2
ABLATION DISTANCE



BETA TEST V1.7
ABLATION DISTANCE




DATA - BETA-3.3
CORCON WITH
IMPROVED HEAT
TRANSFER MODEL
AXIAL ABLATION DISTANCE (CM)
TIME (MINUTES)
0
10
20
30
40
50
10
20
30
40
50


DATA
GAS FILM MODEL
IMPROVED HEAT TRANSFER
TEMPERATURE (K)
1750
1800
1850
1900
1950
110
120
130
140
150
160
TIME (minutes)

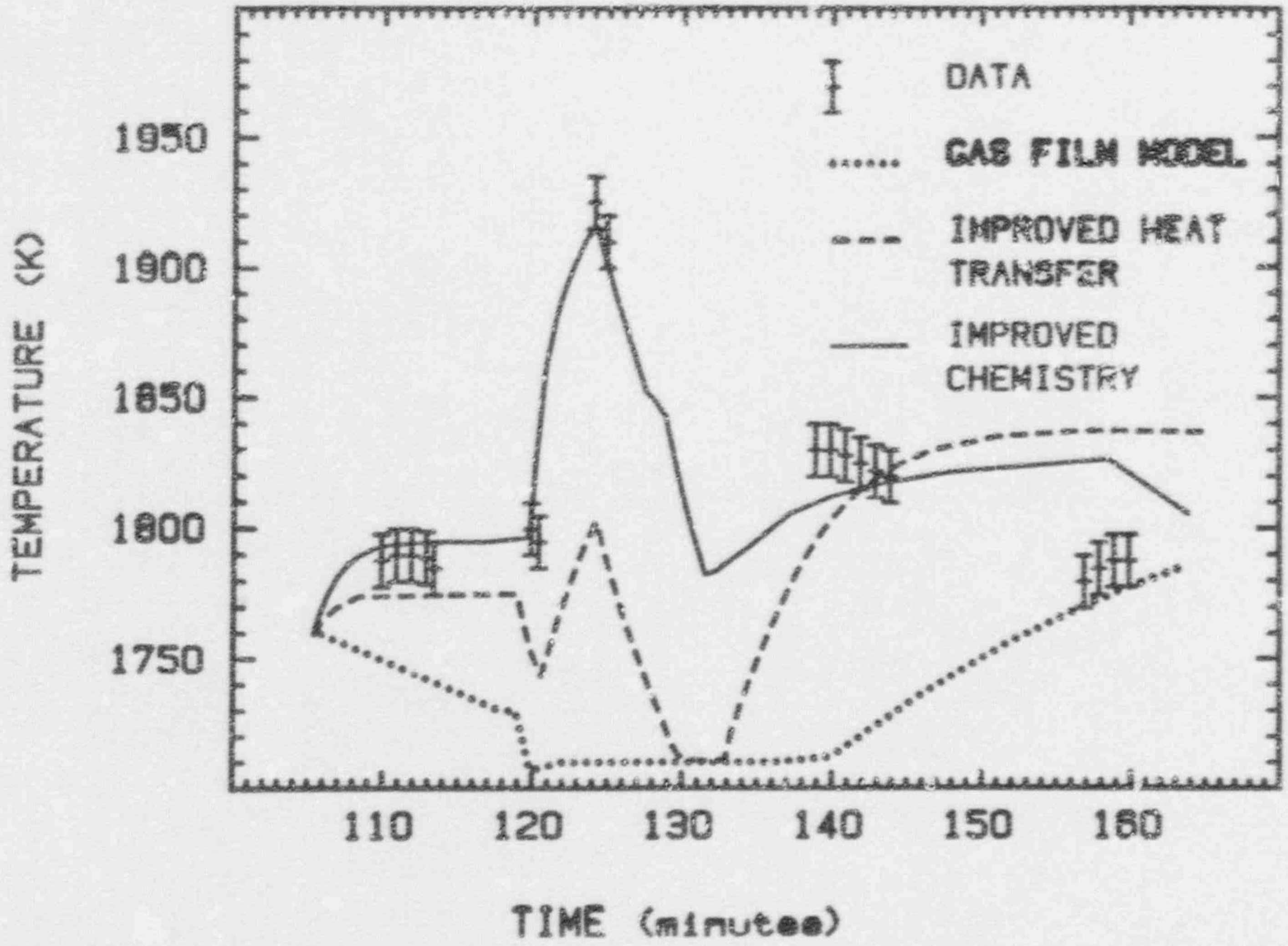
ADDITIONAL HEAT SOURCE

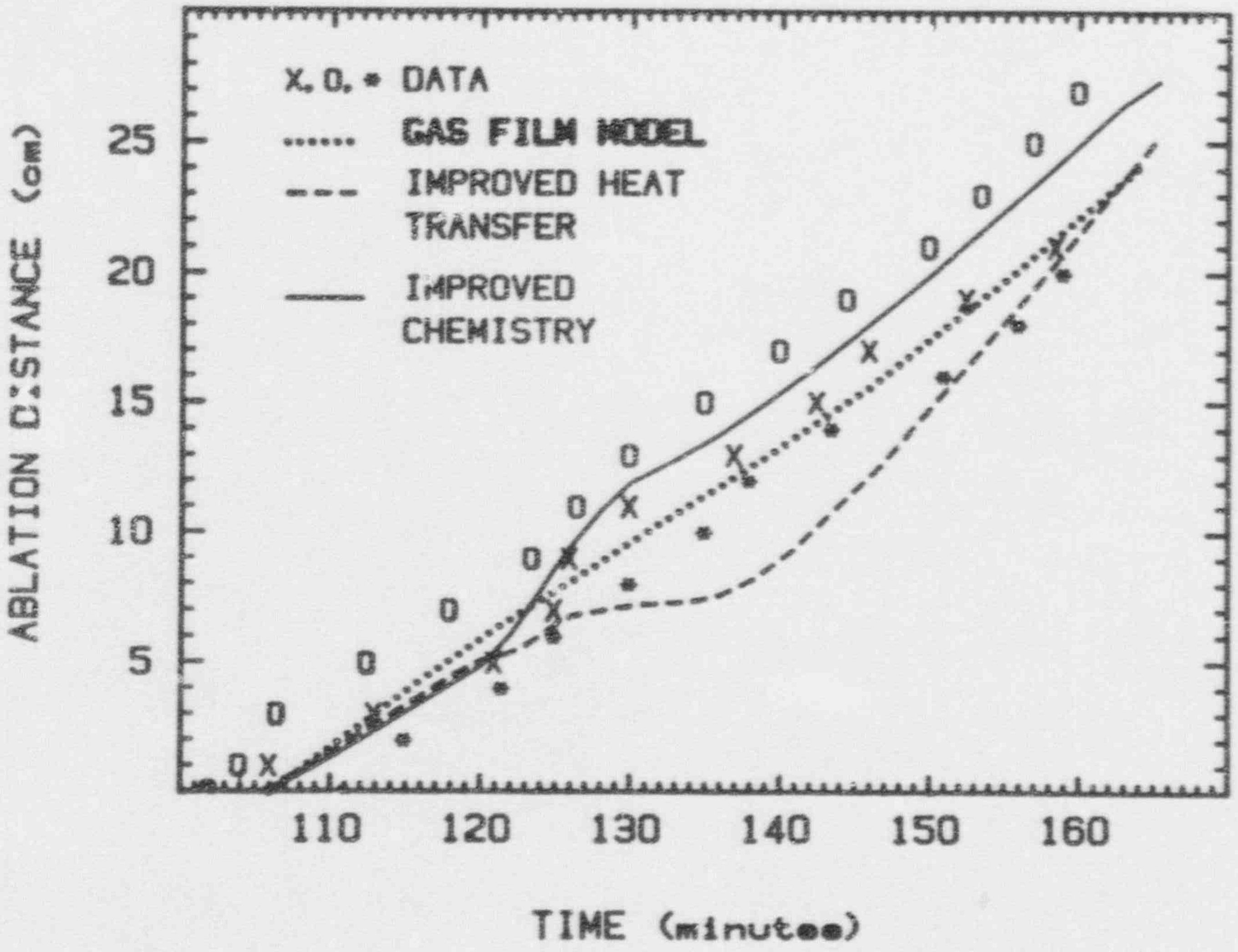
$Zr(sol'n) + SiO_2 \rightarrow ZrO_2 + Si(sol'n) + 28$ kcal/mole Zr

$Si(sol'n) + 2H_2O(g) \rightarrow SiO_2 + 2H_2 + 88$ kcal/mole Si

INCREASES THE RATE OF CHEMICAL ENERGY PRODUCTION BUT DOES NOT CHANGE THE TOTAL AMOUNT OF ENERGY PRODUCED CHEMICAL

HIGH RATES OF CHEMICAL ENERGY RELEASE PRODUCE TEMPERATURE TRANSIENTS IN MELT

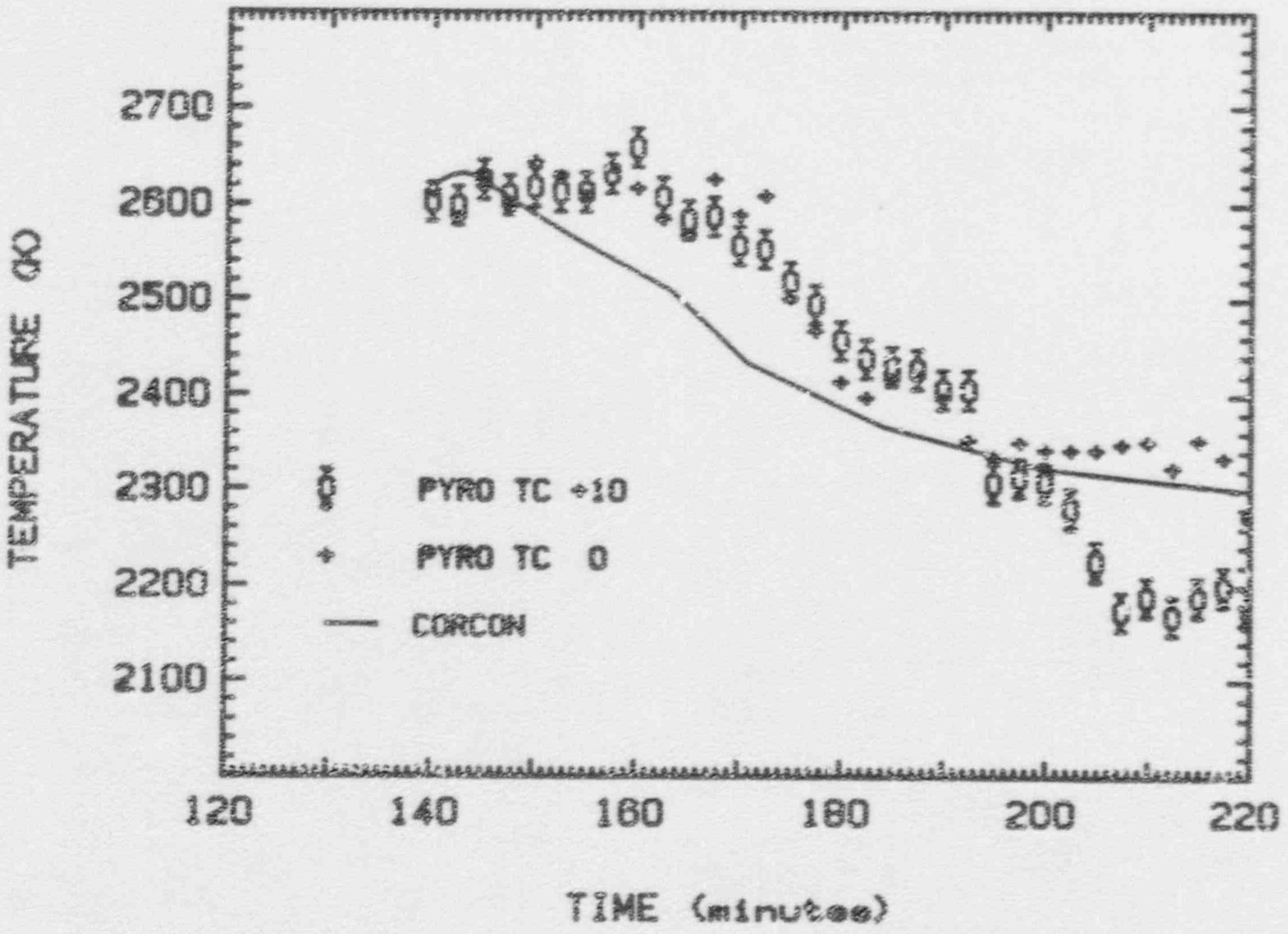

DATA
GAS FILM MODEL
IMPROVED HEAT TRANSFER
IMPROVED CHEMISTRY
TEMPERATURE (K)
1950
1900
1850
1800
1750
110
120
130
140
150
160
TIME (minutes)

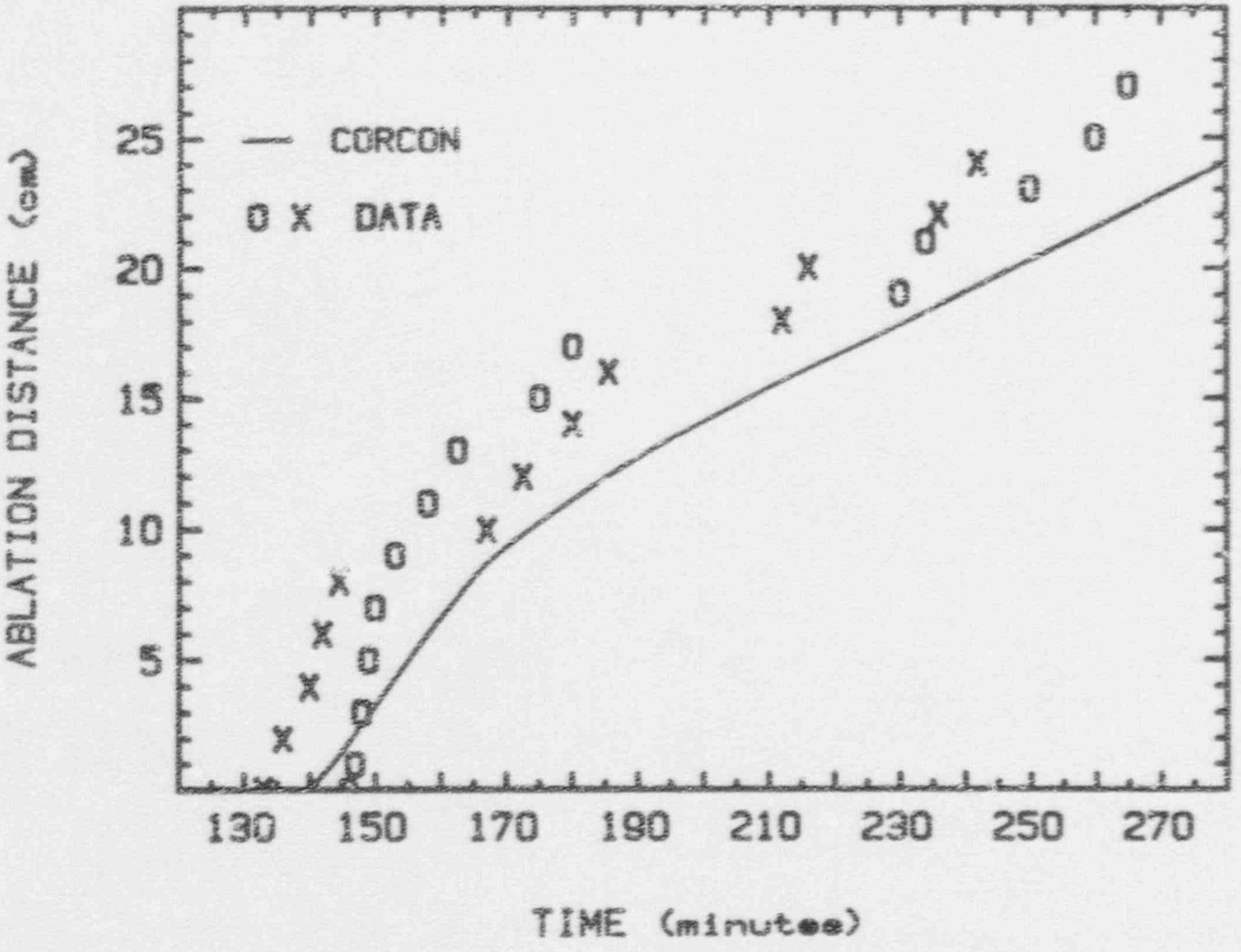

X, O, * DATA
GAS FILM MODEL
IMPROVED HEAT TRANSFER
IMPROVED CHEMISTRY
ABLATION DISTANCE (cm)
5
10
15
20
25
110
120
130
140
150
160
TIME (minutes)

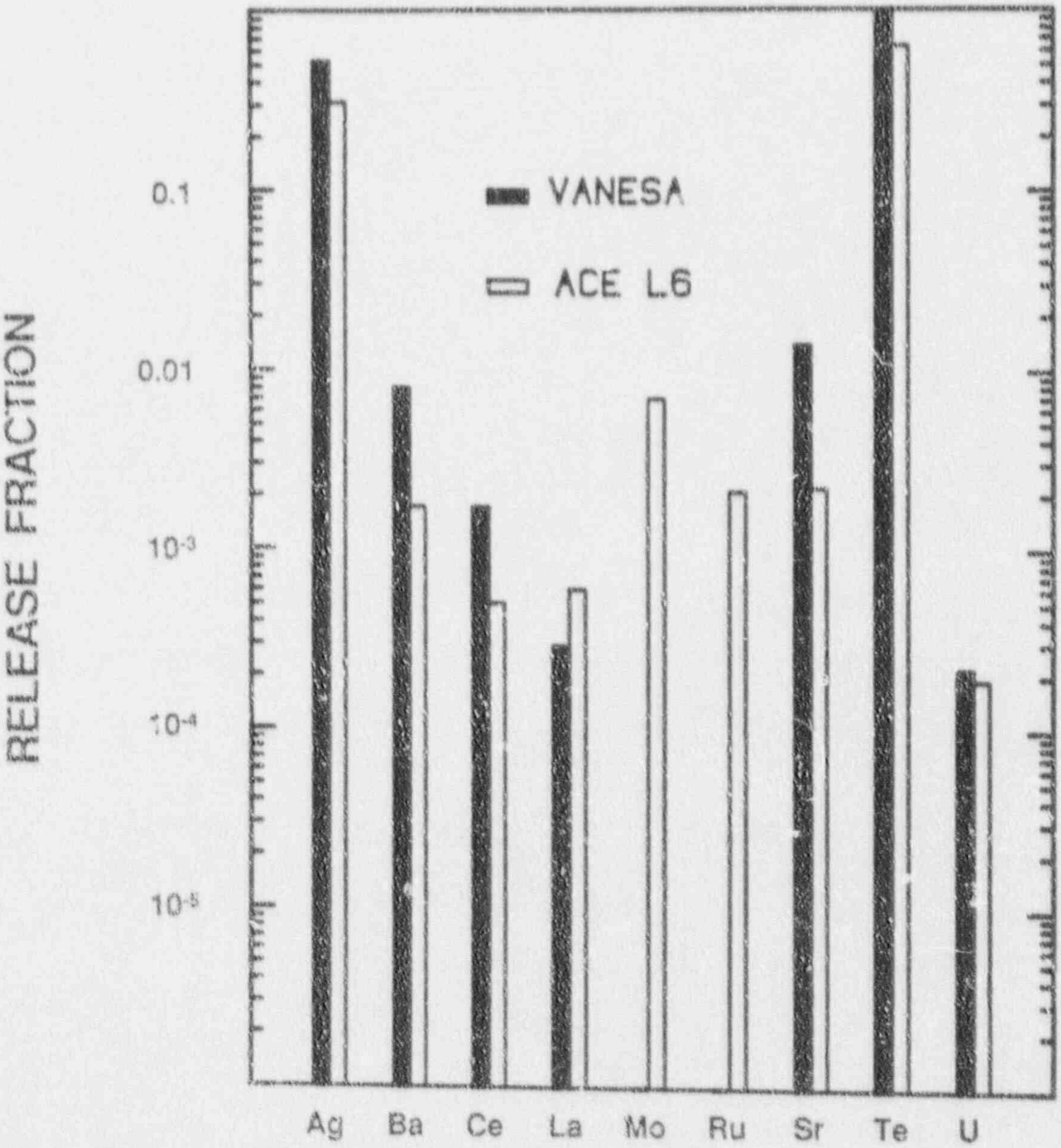
EXTENSION OF MODELS TO OXIDES

* MELT/CONCRETE INTERACTION MODELS HAVE BEEN DEVELOPED BASED ON MOLTEN STEEL INTERACTION DATA

* CAN THESE MODELS PREDICT MOLTEN OXIDE INTERACTIONS WITH ONLY CHANGES TO MATERIAL PROPERTIES?


2700
2600
2500
2400
2300
2200
2100
TEMPERATURE (K)
120
140
160
180
200
220
TIME (minutes)
PYRO TC +10
PYRO TC 0
CORCON


— CORCON
0 X DATA
ABLATION DISTANCE (cm)
25
20
15
10
5
130
150
170
190
210
230
250
270
TIME (minutes)


RELEASE FRACTION
0.1
0.01
10-3
10-4
10-5
■ VANESA
□ ACE L6
Ag
Ba
Ce
La
Mo
Ru
Sr
Te
U

Note on Accuracy

For a General Reaction

$$A(l) \rightleftharpoons A(g)$$

$$P(A) = X(A)\,\gamma(A)\exp[-\Delta G(A)/RT]$$

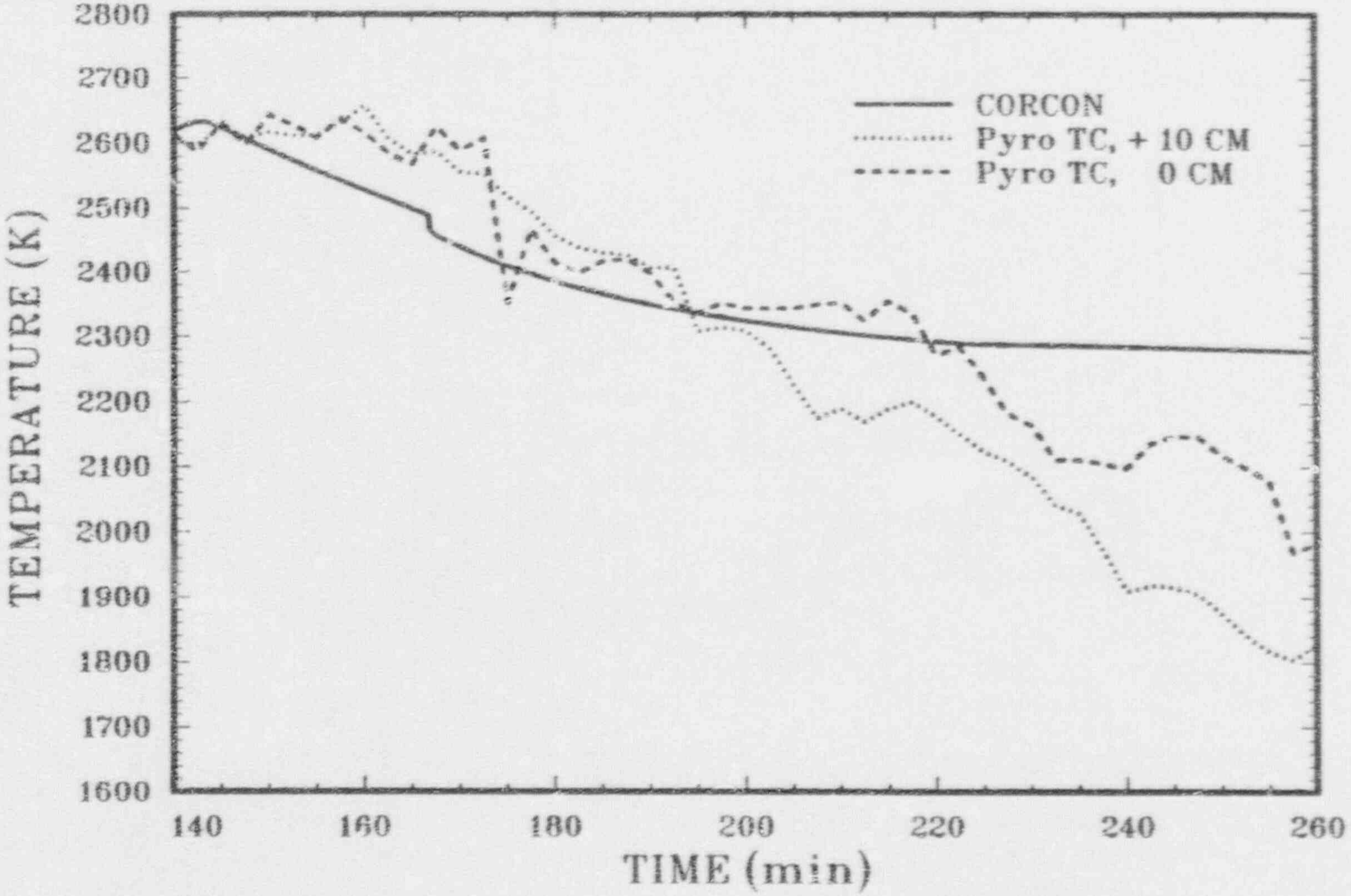
$$RT\ln\gamma(A) = f(X,T)$$

Typical uncertainty in $\Delta G(A)$ = +/- 5000 cal/mole

Uncertainty in $F(X,T)$ of +/- 1000 cal/mole won't alter significantly the uncertainty in $P(A)$. <u>We don't need highly accurate solution models.</u>

+/- 5000 cal/mole at 1800K amounts to a factor of 4 uncertainty in $P(A)$. Don't expect predictions better than a factor of 2 to 4.

SURC-1 COMPARISONS
MELT TEMPERATURE



IMPROVED CHEMICAL MODELS

* NON-IDEAL SOLUTION THERMODYNAMICS TO IMPROVE RELEASE PREDICTIONS

  - Sub-regular Metal Phase
  - Associated Solution of Stoichiometric Species for Oxide

* CHEMICAL POTENTIALS OF LIQUID PHASE CONSTITUENTS TO PREDICT PHASE RELATIONS

  - More Eutectic-like Interaction of Concrete & UO_2.
  - Data Base Being Developed an ANL & BCL

CONCLUSIONS ON THE STATUS OF MODELING CORE DEBRIS/CONCRETE INTERACTIONS

* PREDICTIVE MODEL

- Treats all essential phenomena
- Heat transfer modeling well developed
- Some refinements to chemical modeling possible

* LARGE DATA BASE FOR COMPARISONS

- Many tests still need to be predicted with the model

CORCON-Mod3 Validation

The following nine experiments have been selected for validation of CORCON-Mod3:

SWISS-2:	Molten steel, LCS concrete, water addition
SURC-1:	Molten oxide w/Zr, limestone concrete
SURC-2:	Molten oxide w/Zr, siliceous concrete
SURC-3:	Molten steel w/Zr, limestone concrete
SURC-4:	Molten steel w/Zr, siliceous concrete
ACE L2:	Molten oxide w/Zr, siliceous concrete
ACE L6:	Molten oxide w/Zr, siliceous concrete
MACE/ST:	Molten oxide w/Zr, LCS concrete, water added
BETA 5.1:	Molten steel w/Zr, siliceous concrete, 2-D crucible

OVERVIEW OF SEVERE ACCIDENT RESEARCH
FUNDING AND PROJECTIONS

BRIAN W. SHERON
DIRECTOR, DIVISION OF SYSTEMS RESEARCH
OFFICE OF NUCLEAR REGULATORY RESEARCH

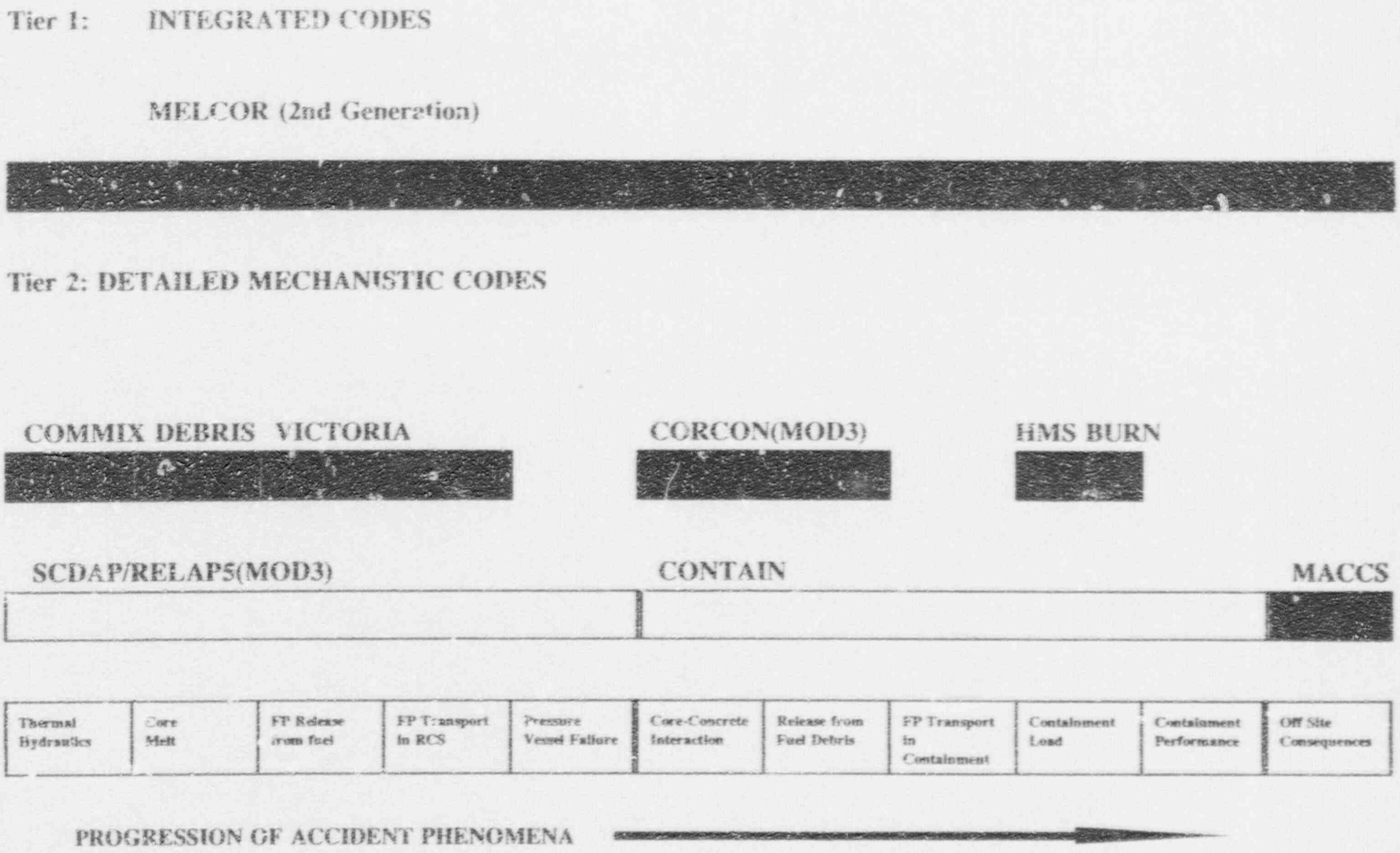
PRESENTED TO

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SEVERE ACCIDENTS SUBCOMMITTEE

OCTOBER 24, 1991

- STAFF REVISED SEVERE ACCIDENT RESEARCH PROGRAM IN 1989
- OBJECTIVE WAS TO EMPHASIZE RESOLUTION OF ISSUES THAT CONTINUED TO CONTRIBUTE TO LARGE UNCERTAINTY IN EARLY CONTAINMENT FAILURE
- THIS APPROACH APPEARS CONSISTENT WITH ACRS PHILOSOPHY ON CONTAINMENTS AS DOCUMENTED IN MAY 17, 1991, ACRS LETTER TO CHAIRMAN CARR
- OTHER AREAS WERE SCALING OF SEVERE ACCIDENT EXPERIMENTS AND CODES



Tier 1: INTEGRATED CODES

MELCOR (2nd Generation)

Tier 2: DETAILED MECHANISTIC CODES

COMMIX DEBRIS VICTORIA

CORCON(MOD3)

HMS BURN

SCDAP/RELAP5(MOD3)

CONTAIN

MACCS

Thermal Hydraulics	Core Melt	FP Release from fuel	FP Transport in RCS	Pressure Vessel Failure	Core-Concrete Interaction	Release from Fuel Debris	FP Transport in Containment	Containment Load	Containment Performance	Off Site Consequences

PROGRESSION OF ACCIDENT PHENOMENA

- TWO EARLY CONTAINMENT FAILURE ISSUES WERE MARK I LINER AND DCH

- PEER REVIEW OF THEOFANOUS' MARK I REPORT AT HARPER'S FERRY IN JULY 1990 IDENTIFIED 4 AREAS FOR FURTHER CONFIRMATORY WORK

- CONFIRMATORY WORK IN PROGRESS. UPON COMPLETION OF CONFIRMATORY WORK, REPORT WILL UNDERGO FINAL PEER REVIEW (FACA REVIEW, LIKE 1150)

- DCH TESTING AT SNL AND ANL RESTARTED. PLAN FOR RESOLUTION OF DCH ISSUE BEING DEVELOPED. DRAFT RESOLUTION PLAN TO BE DEVELOPED BY DECEMBER 1991.

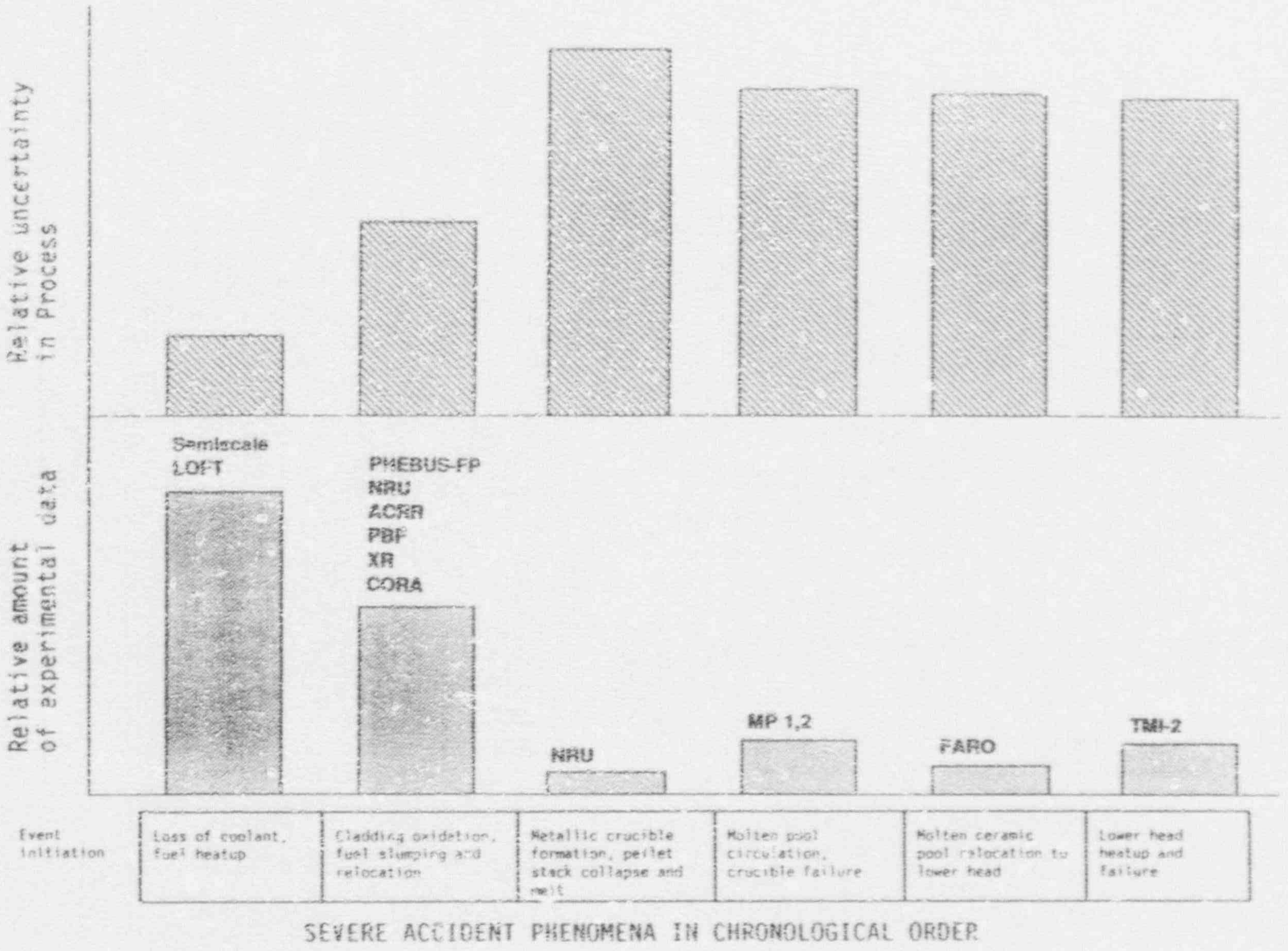
SCALING

- SEVERE ACCIDENT SCALING METHODOLOGY (SASM) REPORT COMPLETED AS DRAFT.
- WILL BE PUBLISHED AS DRAFT NUREG FOR COMMENT
- REPORT WILL BE PROVIDED TO SEVERE ACCIDENT RESEARCH CONTRACTORS FOR USE IN DEVELOPING SCALING RATIONALE FOR PROPOSED SEVERE ACCIDENT EXPERIMENTS.

CODES

- STAFF HAS IN PLACE A PROGRAM TO PEER-REVIEW ALL MAJOR THERMAL-HYDRAULIC AND SEVERE ACCIDENT CODES.
- PURPOSE IS TO DETERMINE WHAT AREAS OF MODELING ARE GOOD ENOUGH, WHAT AREAS NEED FURTHER WORK, ADEQUACY OF DOCUMENTATION, HOW WELL DOES CODE MEET OBJECTIVES
- PROCESS IS EXPENSIVE AND TIME CONSUMING, BUT RESULTS ARE WELL WORTH IT.

- PRIMARY EMPHASIS IN REMAINING SEVERE ACCIDENT RESEARCH IS UNDERSTANDING CONTAINMENT LOADS
- TWO MAJOR PHENOMENOLOGICAL AREAS GOVERN LOADS
  - AMOUNT, TEMPERATURE, COMPOSITION, AND EJECTION ENERGY OF MELT
  - INTERACTION OF MELT WITH CONTAINMENT
    1. DCH
    2. CCI
- DCH BEING HANDLED BY SEPARATE, FOCUSED PROGRAM
- CCI RESEARCH COMPLETED, FOCUS NOW ON DEBRIS COOLABILITY
- FOCUS OF REMAINING EFFORT IS ON IN-VESSEL CORE MELT PHENOMENA. MAJOR PEER REVIEW OF THE IN-VESSEL MELT PROGRESSION PROGRAM IS BEING UNDERTAKEN


Relative uncertainty in Process
Relative amount of experimental data
Semiscale
LOFT
PHEBUS-FP
NRU
ACRR
PBF
XR
CORA
NRU
MP 1,2
FARO
TMI-2
Event initiation
Loss of coolant, fuel heatup
Cladding oxidation, fuel slumping and relocation
Metallic crucible formation, pellet stack collapse and melt
Molten pool circulation, crucible failure
Molten ceramic pool relocation to lower head
Lower head heatup and failure
SEVERE ACCIDENT PHENOMENA IN CHRONOLOGICAL ORDER

- SUBSTANTIAL RESEARCH ON EARLY PHASE WORK HAS BEEN DONE
- LATE PHASE (CRUCIBLE FORMATION, PELLET STACK COLLAPSE, MOLTEN POOL CIRCULATION AND CRUCIBLE FORMATION, MELT RELOCATION AND VESSEL HEAD HEATUP AND FAILURE) IS STILL VERY UNCERTAIN
- DIFFICULT TO CONSTRUCT AND RUN RELEVANT EXPERIMENTS
- TWO EXPERIMENTS PREVIOUSLY DEFINED
    - EX REACTOR EXPERIMENTS (OUT OF PILE)
    - MP EXPERIMENTS (IN PILE)
- RECENTLY UNDERWENT PEER REVIEW
- WE NEED TO DETERMINE IF, IN FACT, MEANINGFUL EXPERIMENTS CAN BE RUN THAT WILL SUBSTANTIALLY REDUCE LATE PHASE UNCERTAINTIES

- FOR CURRENT PLANTS, CERTAIN ACCIDENT MANAGEMENT STRATEGIES HAVE LARGE UNCERTAINTIES AND EXPERIMENTS ARE FOCUSED ON REDUCING THESE UNCERTAINTIES:
  - QUENCHING OF A DAMAGED/MOLTEN CORE
  - DEBRIS BED COOLABILITY
- EFFORTS ARE NOW UNDERWAY TO DEFINE APPLICABILITY OF CURRENT SEVERE ACCIDENT DATA BASE TO ADVANCED PASSIVE REACTORS
  - CODE MODIFICATIONS UNDERWAY TO MODEL AP600 CONTAINMENT AND EXPECTED PHENOMENA
  - CONTAIN
  - COMMIX

OTHER ACTIVITIES

- RESEARCH ON HIGH TEMPERATURE HIGH SPEED HYDROGEN COMBUSTION BEING PERFORMED COOPERATIVELY WITH JAPANESE (NUPEC) AT BNL
- WORK TO BETTER CHARACTERIZE FCIs; SERG RECOMMENDED CONDUCT OF EXPERIMENTS AT INTERMEDIATE SCALES (100 KG) TO CHARACTERIZE FCI MIXING AND EXPLOSION YIELD
- FISSION PRODUCT WORK TO SUPPORT SOURCE TERM REVISION EVENTUALLY FINISHED. REMAINING WORK IS PHEBUS FP PROGRAM SUPPORT.

BUDGET PROJECTIONS FOR SEVERE ACCIDENT RESEARCH

ACTIVITY	FY92	FY93	FY94
CORE MELT PROGRESSION	$3,050	3,630	4,630
FUEL COOLANT INTERACTIONS	$ 600	1,200	1,200
FP BEHAVIOR	$ 850*	1,100*	1,100*
DEBRIS COOLABILITY	$ 800	1,000	500
DIRECT CONTAINMENT HEATING	$2,800	1,000	200
HYDROGEN COMBUSTION	$ 370	795	500
CODES	$4,552	4,152	4,947
SUBTOTAL	13,022	12,877	12,877
ALWR	$1,110	5,500	5,500
TOTAL	14,132	18,377	18,377

* INCLUDES CASH CONTRIBUTION AND IN-KIND CONTRIBUTION TO PHEBUS.

UPDATE ON MARK I LINER FAILURE ISSUE RESOLUTION

FAROUK ELTAWILA
ACCIDENT EVALUATION BRANCH

ACRS SUBCOMMITTEE ON SEVERE ACCIDENTS
BETHESDA, MARYLAND

OCTOBER 24, 1991

MARK I DRYWELL SHELL FAILURE

- Issue: As a result of core degradation/melting and subsequent RPV failure molten core debris spreads over the drywell floor coming in contact with the steel containment shell. In the absence of cooling, molten debris may raise local shell temp above melting temp above melting temp or sufficiently high to induce creep-rupture failure.

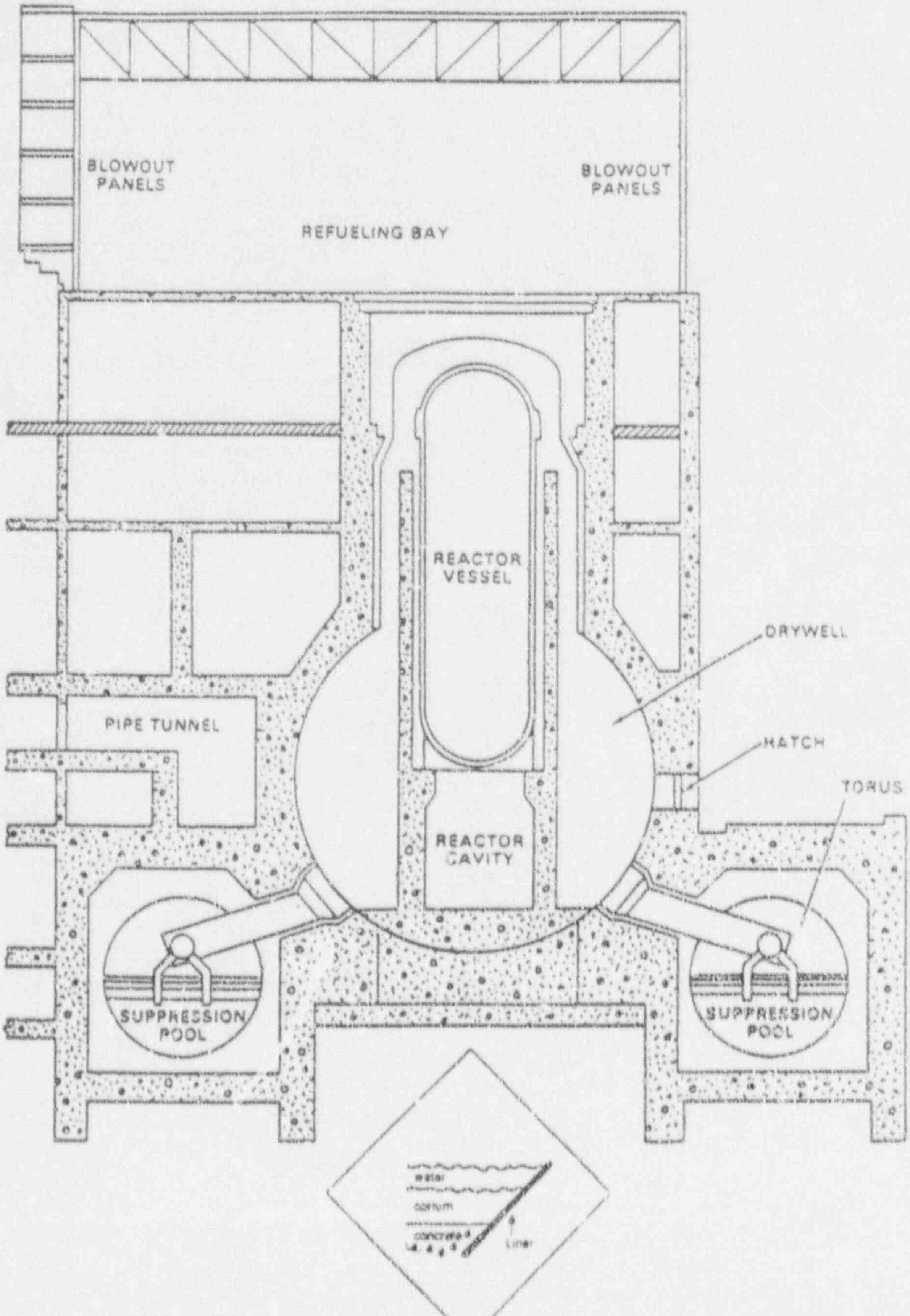
- Background:
  - Various analytical treatments of the issue by NRC contractors and industry have led to different conclusions regarding the survivability of the drywell shell under a variety of severe accident conditions
  - NUREG 1150 Elicitations

    Dry Condition

    - 5 out of 6 experts believe that liner will fail

    Flooded Drywell

    - Divergence of Quantitative Judgement by Experts
    - Aggregate probability is high even though 4 out of 5 experts assigned very low probability to liner failure


BLOWOUT PANELS
BLOWOUT PANELS
REFUELING BAY
REACTOR VESSEL
DRYWELL
PIPE TUNNEL
HATCH
TORUS
REACTOR CAVITY
SUPPRESSION POOL
SUPPRESSION POOL
water
corium
concrete
Liner

NUREG-1150 EXPERTS ON THE MARK I LINER FAILURE ISSUE

David Bradley - Sandia National Laboratories,

Michael Corradini - University of Wisconsin,

George Green - Brookhaven National Laboratory,

Michael Hazzan - Stone & Webster Engineering Corp.,

Mujid Kazimi - Massachusetts Institute of Technology, and

Raj Sehgal - Electric Power Research Institute.

CONTAINMENT PERFORMANCE IMPROVEMENT PROGRAM

STAFF APPROACH

BALANCED APPROACH
TO
REDUCE OVERALL RISK

ACCIDENT PREVENTION

REDUCE THE LIKELIHOOD OF AN ACCIDENT OCCURRING

ACCIDENT MANAGEMENT

CONTROL THE COURSE OF AN ACCIDENT AND RETURN PLANT TO STABLE STATE

ACCIDENT MITIGATION

REDUCE THE CHALLENGE TO CONTAINMENT AND THE MAGNITUDE OF RADIOACTIVE RELEASES TO ENVIRONMENT

SUMMARY OF STAFF RECOMMENDATION
FOR MARK I

(Discussed with ACRS subcommittee on Containment on December 6, 1988, and ACRS full committee on December 15, 1988, January 13, 1989)

- Accelerate staff actions to implement station blackout rule.

- Require alternate water supply for drywell spray/vessel injection with pumping capability independent of normal and emergency AC.

- Require hardened venting capability from wetwell (able to withstand severe accident pressures). Isolation valves to be remotely operable independent of normal and emergency AC.

- Require enhanced ADS reliability. Additional power and/or nitrogen supply and cable reliability.

- Require implementation of improved EPG's (Rev. 4 of BWROG).

NUREG/CR 5423

THE PROBABILITY OF LINER FAILURE IN A MARK I CONTAINMENT

APPROACH

- GENERAL PROBABILISTIC FRAMEWORK THAT DECOMPOSES PROBLEM INTO A FEW ESSENTIAL PARTS.
- CONSIDER EACH PART INDEPENDENTLY AND QUANTIFY BASED ON AVAILABLE INFORMATION.
- SYNTHESIZE TO OBTAIN BOTTOM LINE RESULTS AND AREAS OF SENSITIVITY.
- REVISIT EACH PART AS APPROPRIATE AND AS NEW INFORMATION BECOMES AVAILABLE TO CONFIRM JUDGMENT.
- CONSIDER ONLY LOW PRESSURE SCENARIO.

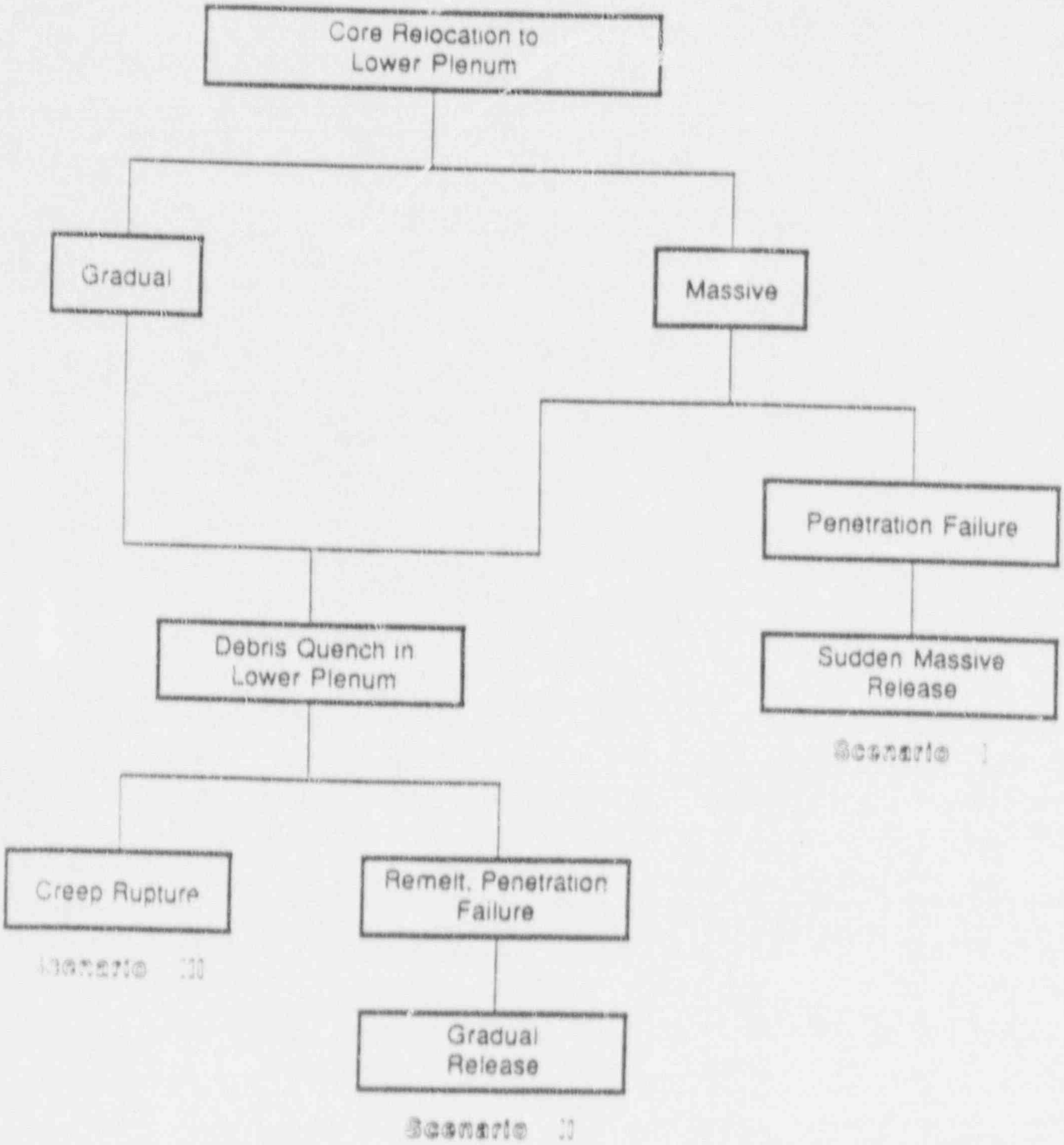


Figure 3.3. Logic diagram of phenomenological bifurcations leading to melt release from the reactor vessel.

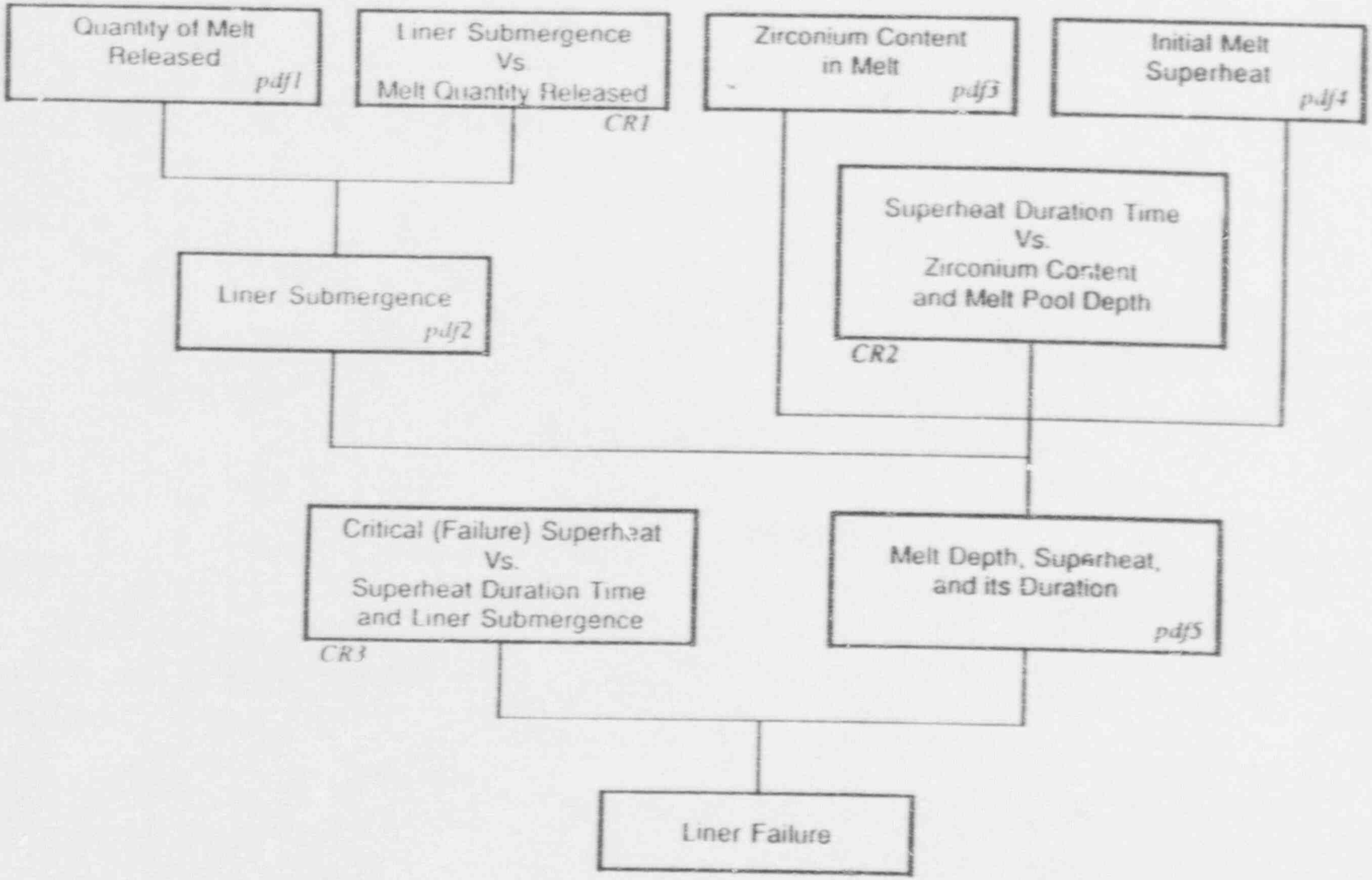


Figure 2.2. Schematic of the decomposition of the Mark-I liner attack issue. "Liner Submergence" and "Melt Pool Depth" are related by a factor of ×1.4, and they are used synonymously.

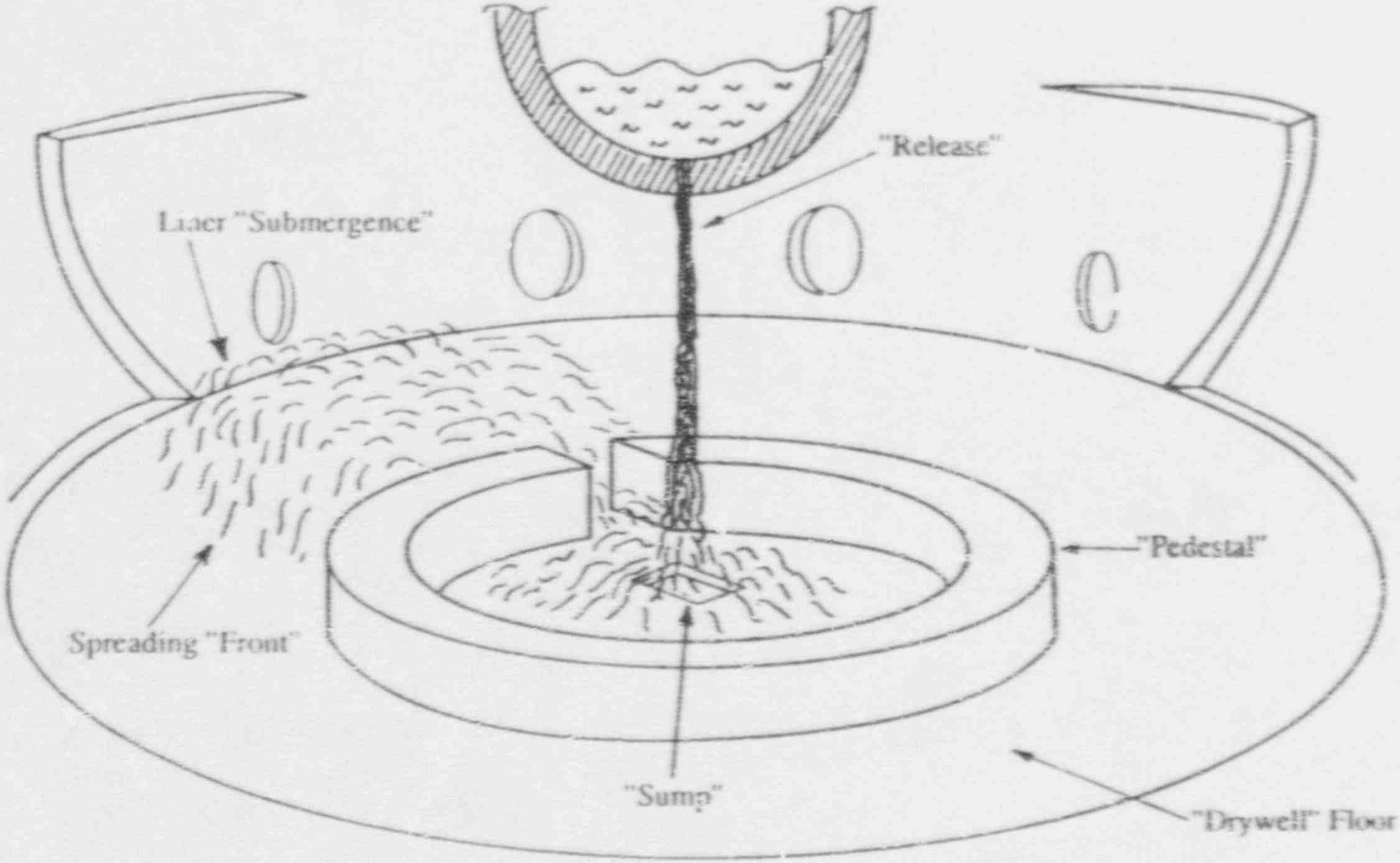


Figure 2.1. Illustration of melt release-spreading event.

- MELT RELEASE PDF1, PDF3, PDF4

  BASED ON SIMPLIFIED CALCULATIONS, AS WELL AS RESULTS FROM BWRSAR AND MAAP CODES.

- MELT SPREADING CR1

  BASED ON SCALED EXPERIMENT FOR MASSIVE RELEASE.

- CORIUM SUPERHEAT CR2 (CORE-CONCRETE INTERACTION)

  - MODEL BENCHMARKED ON EXPERIMENTS
  - PREDICT RATE OF SUPERHEAT LOSS FOR VARIOUS CONDITIONS
  - PREDICT RUNAWAY REGIME CRITERIA (NOT MET FOR PRACTICAL CASE)

- LINER ATTACK CR3 (HEAT TRANSFER WITHIN THE LINER)

  - MODEL BENCHMARKS
  - EXPERIMENTS FOR HEAT TRANSFER COEFFICIENT
  - KEY EFFECTS - TRANSIENT, 2-D

- INTEGRATION

IMPORTANT OBSERVATIONS

- MELT SUPERHEATS ARE LOW AND SHORT-LIVED DUE TO INTERACTIONS WITH COOLANT OR CONCRETE; SUSTAINED SUPERHEATED CONDITION IS UNREALISTIC ASSUMPTION
- TRANSIENT LINER HEAT-UP COUPLE CRUCIALLY WITH TRANSIENT SUPERHEAT
- METALLIC MELT VOLUME CAN BE LARGE BUT SLOW RELEASE RATE; FAVOR RETENTION IN PEDESTAL; LONG TIME TO FAILURE
- OXIDIC MELT CAN BE BOUNDED; RAPID RELEASE
- FUEL COOLANT INTERACTIONS. DURING RELEASE AND SPREADING, PROMOTE MELT QUENCHING
- LINER FAILURE CAN BE LOCALIZED; PATH COULD BE EASILY PLUG

REVIEW OF NUREG/CR-5423 STUDY

- CONSIDERABLE PEER REVIEW THAT INCLUDE THE NUREG-1150 EXPERT PANEL ON MARK I LINER ISSUE, INDUSTRY, UNIVERSITIES, NATIONAL LABORATORIES AND INTERNATIONAL RESEARCH

- NATIONAL LABORATORY RESEARCH DIRECTORS ASSOCIATED WITH THE SEVERE ACCIDENT PROGRAM

- NRR, AEOD AND OTHER RES DIVISIONS

- COMMENTS RECEIVED AND AUTHOR RESPONSE WERE INCLUDED IN APPENDIX TO THE REPORT

- HARPERS FERRY WORKSHOP JULY 1990

  - STRONG SUPPORT OF METHODOLOGY/FRAMEWORK
  - RESIDUAL ISSUES:
    LINER FAILURE CRITERIA
    TIME DURATION OF MELT SUPERHEAT
    MELT SPREADING TO LINER
    INITIAL MELT RELEASE QUANTITY AND COMPOSITION

  - WORKING GROUP MEETINGS DECEMBER 1990, JANUARY 1991

  - FOLLOW UP WORK IN ABOVE AREA @ SNL (ANATECH), SNL, ANL, ORNL (RPI)

- UPDATE RESULTS FALL 1992

LIST OF REVIEWERS

1. H. Alsmeyer and P. Hofmann (Kernforschungszentrum Karlsruhe (KfK)
2. David R. Bradley (Sandia National Laboratory)
3. G.A. Greene (Brookhaven National Laboratory)
4. Robert E. Henry (Fauske & Associates, Inc.)
5. Stephen A. Hodge (Oak Ridge National Laboratory)
6. Stan Kaplan (Pickard, Lowe and Garrick, Inc.)
7. Mujid S. Kazimi (Massachusetts Institute of Technology)
8. T.S. Kress (Oak Ridge national Laboratory)
9. Salomon Levy (S. Levy Incorporated)
10. D.J. McCloskey (Sandia National Laboratories)
11. Mohammed Modarres (The University of Maryland)
12. F.J. Moody, S.A. Wilson, et al. (GE Nuclear Energy)
13. Paul North (Idaho National Engineering Laboratory)
14. Michael Z. Podowski (Rensselaer Polytechnic Institute)
15. Dana A. Powers (Sandia National Laboratories)
16. William H. Rasin (Nuclear Management and Resources Council)
17. B.R. Sehgal (Electric Power Research Institute)
18. B.D. Shipp (Battelle Pacific Northwest Laboratories)
19. J.J. Sienicki, M.T. Farmer, and B.W. Spencer (Argonne National Laboratory)

WORKING GROUP 1 - LINER FAILURE CRITERIA

MEMBERS: DR. KAMAL BANDYOPADHYAY, BNL; DR. ROBERT HENRY, FAI;
MR. DONALD HORACEK, CBI; DR. Y. RASHID, ANATECH;
PROF. PAUL SHEWMON, ACRS

SUMMARY OF CONCERNS - CHECK CR3

CAN LINER FAIL BEFORE MELTING?

I.E., BY LOCAL STRAINS AND CREEP OR EUTECTIC DISSOLUTION

- FAILURE TEMPERATURES AS LOW AS 900°C WERE SUGGESTED AT HARPERS FERRY
- GERMAN DATA CITED FOR DISSOLUTION

WORKING GROUP 1 - CONCLUSIONS & RECOMMENDATIONS

- SCOPING FINITE-ELEMENT ANALYSES (UCSB) SHOW THAT LOSS OF STRENGTH IS NOT A PROBLEM - CREEP AND LOCAL BENDING REMAIN TO BE ANALYZED

- DETAILED FINITE-ELEMENT ANALYSES NEEDED FOR CREEP

  CONSTITUTIVE LAWS TO BE DERIVED FROM ON-GOING EXPERIMENT AT INEL

- NO NEED FOR INTEGRAL EXPERIMENTS

- GERMAN DATA ON DISSOLUTION INAPPLICABLE

WORKING GROUP 2 - CORE CONCRETE INTERACTION

MEMBERS: DR. BRADLEY, SNL; DR. ROBERT HENRY, FAI;
DR. DANA POWERS SNL; DR. BRUCE SPENCER; ANL

SUMMARY OF CONCERNS - CHECK CR2

CHECK ESTIMATES OF TIME DURATION OF SUPERHEAT BY CORCON-MOD3

- CORCON-MOD3 IS A SIGNIFICANTLY IMPROVED VERSION OF CORCON, NOT YET AVAILABLE OUTSIDE SNL

WORKING GROUP 2 - Conclusions & Recommendations

- General Trends of CR2 in NUREG/CR-5423 Confirmed. Quantitatively, Time of Duration of Superheat Lower by 1/3

- The NUREG/CR-5423 Driving Force for Heat Transfer to Liner ($T_M - T_{M,LIQUIDUS}$) was Questioned and after Extensive Discussion, Confirmed

WORKING GROUP 3 - CORIUM SPREADING UNDERWATER

Members: Dr. Fred Moody, GE; Dr. Michael Podowski, RPI;
Dr. Dana Powers, SNL; Dr. Bruce Spencer, ANL

Summary of Concerns - Check CR1

Validity of Author's Froude Scaling for Scenario I Spreading Questions

Underwater Lava Flow Behavior

i.e. ("Causeways") was Suggested as Relevant

WORKING GROUP 3 - CONCLUSIONS & RECOMMENDATIONS

- GROUP SUPPORTED ON-GOING MELTSPREAD EFFORTS AT ANL TO CHECK CR1. REVIEW OF HEAT TRANSFER MODELS IN MELTSPREAD RECOMMENDED

- FROUDE SCALING CONFIRMED GREENE'S CORRELATIONS NOT PERTINENT TO THIS PROBLEM

- PRESENCE OF FLOOR-MOUNTED OBSTACLES AROUND DOORWAY NEED TO BE EXAMINED. IF SIGNIFICANT, QUANTITY EFFECT BY FROUDE-SCALED EXPERIMENTS AS IN NUREG/CR-5423

- EXAMINE BALLISTIC SCENARIO OF MELT SPLASHING ON UPPER (ABOVE WATER) PARTS OF LINER APPEARS EASY TO BOUND

- FOR "LAVA ANALOGY" CONCERNS:

  (A) FLOW "FOCUSING." THE HIGH HEAT TRANSFER COEFFICIENTS IN NUREG/CR-5423 COVER THIS EFFECT.

  (B) SLOW MELTING/FREEZING CYCLES. THIS IS SCENARIO II BEHAVIOR ALREADY DISCUSSED IN NUREG/CR-5423. VERY LONG-TERM BEHAVIOR.

WORKING GROUP 4 - INITIAL CONDITIONS

Members: Dr. Vernon Denny, SAI; Dr. Robert Henry, FAI;
Dr. Steve Hodge, ORNL; Dr. Michael Podowski, RPI;
Dr. Rod Schmidt, SNL

Summary of Concerns - Revisit pdf1, pdf3, pdf4

Since Melt Release Process is Difficult to "Predict," It Was Suggested to Re-examine the Quantification of Melt Quantity, Composition, and Superheat, to Ensure That it Reasonably and Conservatively Envelops the Behavior

WORKING GROUP 4 - CONCLUSIONS & RECOMMENDATIONS

- SCENARIO I & II CONFIRMED. SCENARIO III WAS REFERRED TO ON-GOING WORK AT INEL ON LOWER HEAD FAILURE. PROBABLY UNIMPORTANT SINCE FAILURE MANY HOURS AFTER CORE SLUMP.

- UNANIMITY THAT QUANTITY, COMPOSITION AND SUPERHEAT OF MELT RELEASE IN SCENARIO I OF NUREG/CR-5423 ARE CONSERVATIVE

  SCENARIO I IS MOST LIMITING

- POSSIBILITY OF A SCENARIO-I BEHAVIOR, BUT WITH SIGNIFICANTLY HIGHER STEEL CONTENT NEEDS FURTHER EVALUATION. THE PROPOSED MECHANISM IS UPPER INTERNALS MELT-IN AS FOUND IN SOME PWR HIGH PRESSURE SCENARIOS.

CONCLUSION

NUREG/CR-5423 ANALYSIS METHODOLOGY:

- PHYSICALLY BASED DECOMPOSITION ON CAUSE-EFFECT BASIS
- EXPLICIT TESTABLE MODELS AND JUDGMENTS
- MULTIPLE SCENARIOS (SPLINTERS)
- EXPERT PARTICIPATION - OPEN AND PUBLIC
- FINAL REPORT WILL BE PEER REVIEWED
- BRIEF THE ACRS SEVERE ACCIDENT SUBCOMMITTEE AND NSRRC
- RECOMMENDATION TO THE COMMISSION

APPLICABILITY TO ELWRs AND ALWRs

- Information generated to address fuel melt quenching, adding water to degraded core, and FCI energetics can be used for ELWRs, ALWRs as well as current generation LWRs

- Ex-vessel debris coolability -- assess applicability of MACE test conditions to ELWRs and ALWRs; debris depth and composition

IODINE CHEMICAL FORMS IN LWR ACCIDENTS

by

Richard Lee
Accident Evaluation Branch
Division of Systems Research

ACRS Severe Accidents
Subcommittee Meeting
Oct. 24, 1991

Iodine Chemical Forms in LWF accidents

Perspective and Scope of Study

- Background
- ORNL analysis on iodine source term
    - Calculational approach:
    - Data Input
    - Evaluation of iodine chemical forms entering containment from the RCS
    - Evaluation of changes in iodine chemical forms that occur in containment
- Conclusions
- Regulatory Applications
- Further Assessment

ACRS Severe Accidents
Subcommittee Meeting
Oct. 24, 1991

BACKGROUND

USNRC Licensing Source Term

- Source term is the release of fission product <u>into</u> the containment and potentially available for release to the environment.
- includes timing, form and quantity of fission products.
- Design basis accident source terms (TID-14844) used in licensing in three distinct ways:
  - for siting evaluations as required in 10CFR100,
  - to define radiological environment conditions for certain plant systems, and
  - to assess effectiveness of plant mitigation features.

BACKGROUND (continued)

Present specification for iodine chemical forms (Reg. Guides 1.3 & 1.4)

Regulatory Guides 1.3 & 1.4

Assume 50% of iodine core inventory is released into containment, but 25% plates out and the remaining 25% is available for leakage.

Iodine chemical form at t=0

91% elementary iodine

5% particlulate iodine

4% organic iodide

Iodine chemical form at t>0

No change in chemical form.

BACKGROUND (continued)

Why consider a change?

- TID-14844 largely based on experimental results of heating UO_2 pellets (late 1950's)
- Results from TMI and post-TMI research.
- Iodine was shown to be predominantly in the form of CsI in the RCS, in contrast to the I_2 assumption of WASH-1400.
- Several chemical reactions with CsI can produce modest amounts of HI and organic iodine forms.
- Sensitivity studies show that the magnitude of airborne iodine is not very sensitive to assumed chemical form in the RCS.
- Aqueous chemistry in containment can produce some I_2 depending in temperature, radiation and pH.

SECY-90-103 (May'90): investigate options for early completion of research activities on chemical form of iodine and other areas in TID-14844.

ORNL ANALYSIS

- Calculational approach:

  (a) Calculate the iodine release from the RCS into the containment.
  (b) Calculate the iodine behavior in the containment. Two time-dependent sources of iodine entering the containment will be considered, one directly from the RCS and the other from the water pool that accounts for the revolatilization of iodine that was dissolved in water.

ORNL ANALYSIS (continued)

Data Input

- RCS iodine chemical form was analyzed for seven severe accident sequences from BMI-2104 and NUREG-1150. The seven sequences were for:
    - TMLB'(Surry) - (PWR, Station Blackout)
    - AB (Surry) - (PWR, LOCA/No ECCS)
    - TC (Grand Gulf) - (BWR-III, ATWS)
    - TQUV (Grand Gulf) - (BWR-III, No makeup water)
    - TC2 (Peach Bottom) - (BWR-I, ATWS)
    - AE (Peach Bottom) - (BWR-I, LOCA/No ECCS)
    - TBA (Sequoyah) - (PWR/Ice Condenser, Station Blackout)

- Obtain as a function of time:
  - fission product (I, Cs) release rates from the core
  - associated flow rates of H_2 and steam
  - gas-phase temperatures and temperature gradients

ORNL ANALYSIS (continued)

Iodine chemical forms entering containment from the RCS

- The chemical forms of iodine in RCS is closely tied to the chemical forms of Cesium (Cs).

  $CsI + H_2O \rightleftharpoons CsOH + HI$

- Considered reactions of CsOH with structure surfaces and revaporizationof CsI from RCS surfaces.

- Results:

  Iodine entering the containment from the RCS is
  - 95 % particulate CsI.
  - 5% (I^- and HI), no less than 1% either I^- or HI.

ORNL ANALYSIS (continued)

Iodine behavior in containment

- Considered Iodine Revolatilization from water pool in containment.

- Phenomena considered which affect the form of iodine in containment.

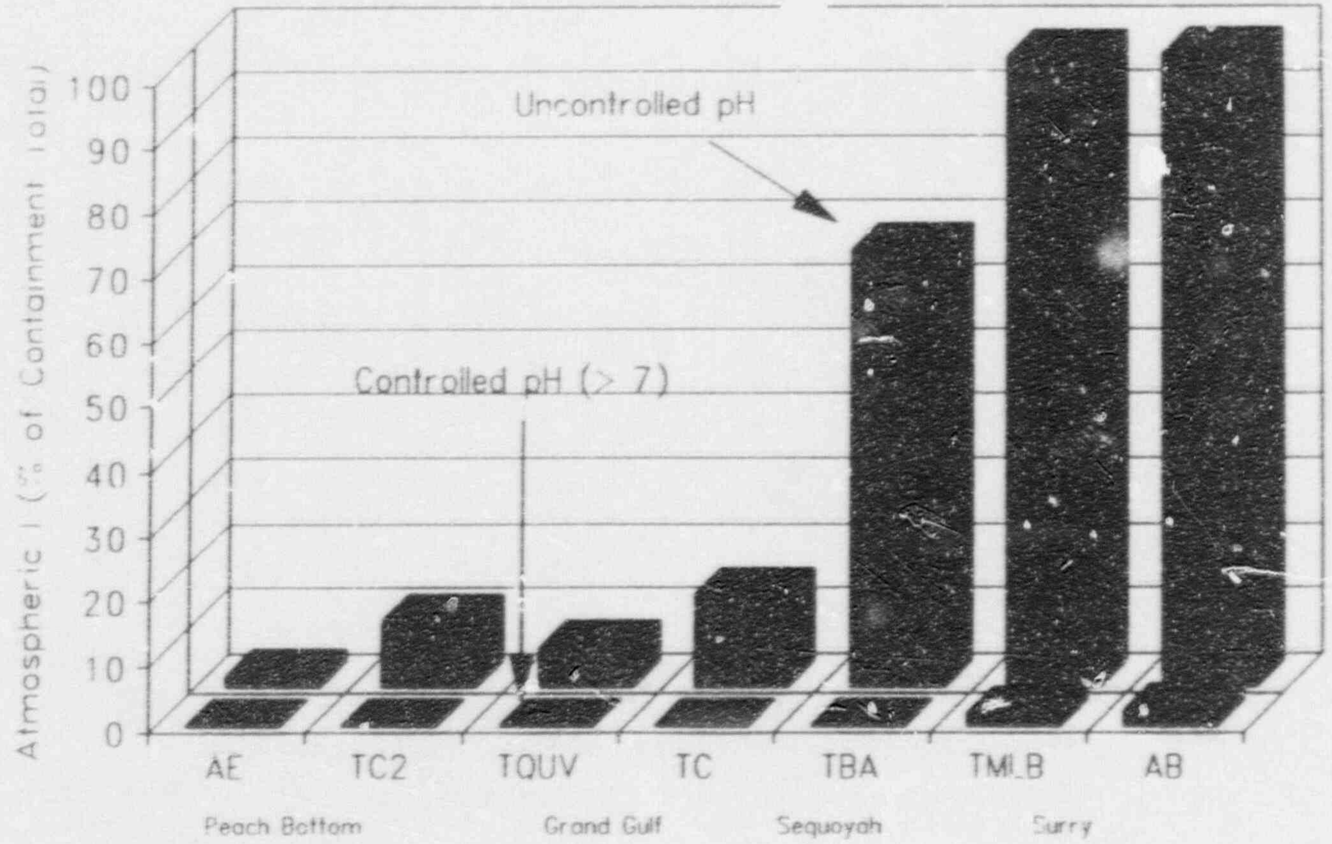
  - Radiolytic conversion of I^- to I_2
  - Gas-liquid partitioning of volatile iodine
  - Formation of organic iodides

- Iodine entering containment dissolves in water pools or plates out on wet surfaces as I^-. Iodine behavior <u>within</u> containment depends upon time and pH of the water solutions.

- If pH is maintained at a value of seven or greater, then the amount of iodine in solution which converts to elemental and organic iodine later in the accident sequence will be very low.

- If pH is not controlled, radiation levels in water pools are sufficient to reduce pH and significant amounts of dissolved iodine will be re-evolved as elemental iodine.

ORNL ANALYSIS (continued)



Additional Atmospheric Elemental Iodine Released

CONCLUSIONS

- Major plant mitigation systems have been optimized to deal with iodine, especially in elemental form.
- Our improved understanding of iodine chemical forms needs to be combined with recent research insights on severe accidents and source terms to provide an integrated understanding for plant operators, designers and regulators.
- Proceeding on a program with this objective.

REGULATORY APPLICATIONS

TID-14844 Update

- **Work is underway to update the technical basis for the source term based upon current severe accident research insights.**
- **Effort is expected to reflected in changes in:**
  - **timing of release**
  - **composition and magnitude of release into containment**
  - **iodine chemical forms**
- **Draft of updated TID report to Commission by December 1991.**

REGULATORY APPLICATIONS (continued)

Some potential implications of iodine chemical forms

- pH control already reflected in U.S. criteria for containment spray systems.
- **Need for charcoal absorbers questioned in safety grade filters. Some questions to be resolved:**
    - **Degree of conversion to I_2 in transport to control room.**
    - **Degree of conversion to I_2 in spent fuel pool.**
    - **Decomposition of CsI trapped on particulate filters and conversion to I_2.**
    - **Effect of hydrogen burns on CsI and conversion to I_2.**
- **Uncertainty in degree of long-term iodine re-evolution in BWR suppression pools without pH control.**

FURTHER ASSESSMENT

PHEBUS-FP AT CADARACHE

The PHEBUS-FP Program includes in-pile severe fuel damage experiments and study the behavior of fission product during their transport in the reactor and containment systems.

- **NRC has entered into an agreement with the Commissariat A L'Energie Atomique of France (CEA) covering participation in the PHEBUS-FP Program**

- **Project is conducted by the Commissariat A L'Energie Atomique of France and the Commission of the European Communities.**

FURTHER ASSESSMENT (continued)

PHEBUS-FP AT CADARACHE (continued)

- Phase-I consists of the preparation phase whereby the facility and experiment design is being formulated.

  Duration: Two-year period from Oct. 1, 1989 to Oct. 1, 1991.

- The experimental phase is conducted under Phase-II.

  Duration: Five year period, Oct. 1, 1990 - Sept. 30, 1995.

  Testing: Six tests. First one scheduled for October 1992. One test per year.

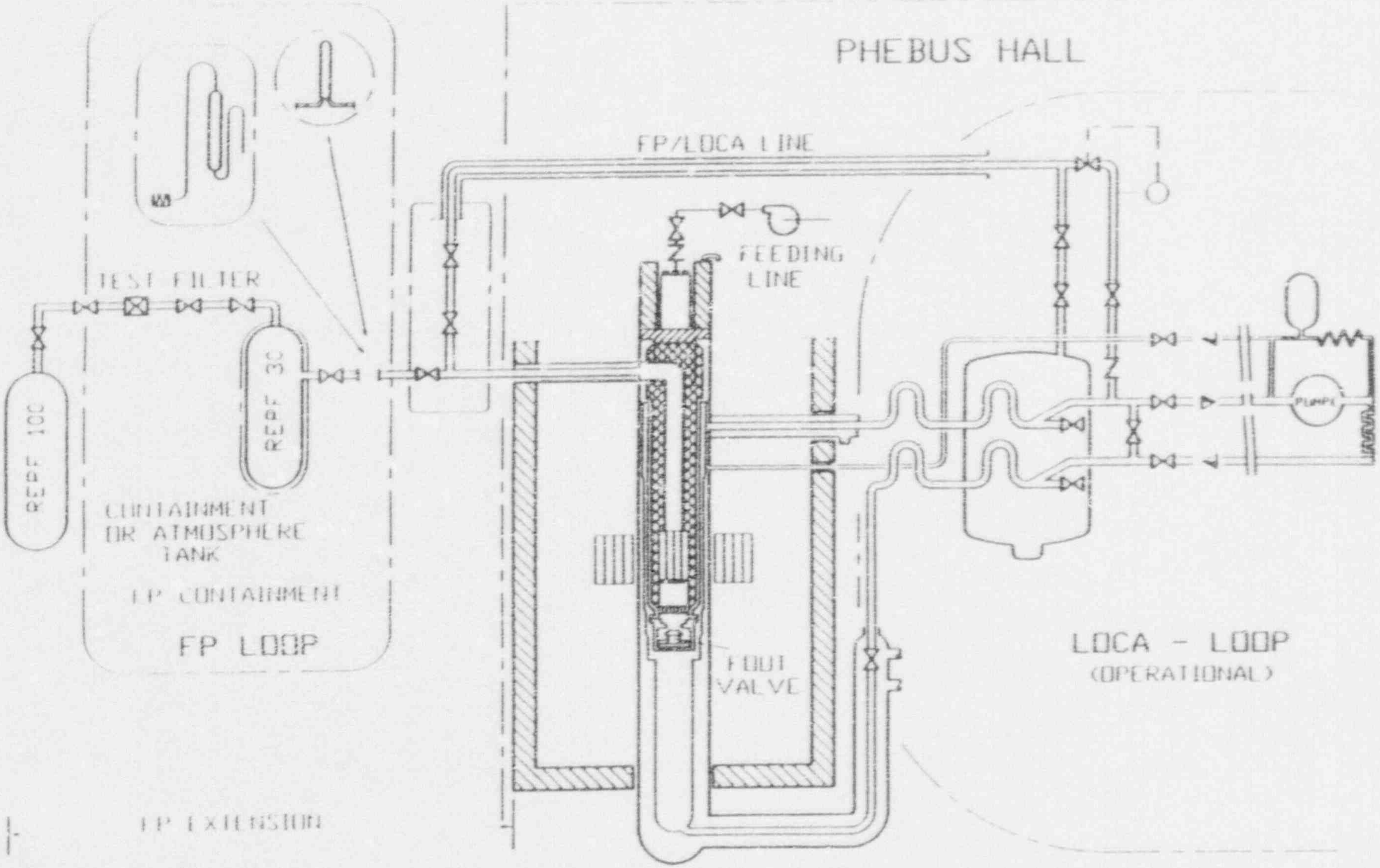
FURTHER ASSESSMENT (continued)

PHEBUS-FP AT CADARACHE (continued)

- PHEBUS-FP will provide integral experimental data to check our analytical models for fission product behavior and transport in the reactor and containment systems, and for iodine chemistry in the containment.
- Expect to obtain information on core melt progression.
- In-pile testing with 21-rod bundle of high burnup fuel.
- Test matrix: (as of 9/91)

1	Cold leg break, fresh fuel
2	Cold leg break, irradiated fuel
3	V-sequence
4	TMLB', dry
5	TMLB', wet
6	Bypass

FURTHER ASSESSMENT (continued)



FURTHER ASSESSMENT (continued)

NRC activities:

A coordinate group consisting of experts from different laboratories was established to assist NRC in the review and formulating recommendation to PHEBUS-FP. Area of technical expertise are:

(a) **INEL - SCDAP/RELAP5, instrumentation and testing.**
(b) **SNL - VICTORIA, CONTAIN and severe accident phenomena (chemistry, aerosol, etc.,) in the RCS and containment.**
(c) **ORNL - TRENDS and severe accident phenomena in the containment.**
(d) **Battelle - specific issues (e.g. BR3 fuel, material interaction with fission product) as required.**

- **In FY91, INEL has completed the design for an on-line aerosol monitor to be used at PHEBUS-FP. A report entitled, "A Review of the PHEBUS-FP Test Program," was completed and provided assessment and recommendation of the PHEBUS-FP instrumentation plans, test matrix, test objectives, and testing.**

FURTHER ASSESSMENT (continued)

- In FY92, the construction of the on-line aerosol monitor will be completed and installed in PHEBUS-FP.
- NRC will use VICTORIA and SCDAP/RELAP5 for pre-test analyses.

CORE MELT PROGRESSION: STATUS AND FUTURE PLANS

PRESENTATION TO THE ACRS SUBCOMMITTEE

ON SEVERE ACCIDENTS

By
ALAN M. RUBIN
ACCIDENT EVALUATION BRANCH
DIVISION OF SYSTEMS RESEARCH

OCTOBER 24, 1991

CORE MELT PROGRESSION RESEARCH

BACKGROUND

- NUREG-1365, "REVISED SEVERE ACCIDENT RESEARCH PROGRAM PLAN," ISSUED 1988
- IN-VESSEL CORE MELT PROGRESSION (SARP LONG-TERM ISSUE L2)
  - OBJECTIVE: CONFIRMATORY RESEARCH TO IMPROVE UNDERSTANDING AND REDUCE UNCERTAINTY
- PLAN TO UPDATE SARP
  - FOCUS OF RESEARCH HAS BEEN ON EARLY PHASE MELT PROGRESSION
  - FEWER EXPERIMENTS AND ANALYTICAL DEVELOPMENT ON LATE PHASE MELT PROGRESSION
  - IDENTIFY AND PRIORITIZE NEEDED CORE MELT RESEARCH

CURRENT PLANS

MELT PROGRESSION EXPERIMENTS

- EX-REACTOR EXPERIMENTS (SNL)
  - DESIGNED TO SEE IF CORE BLOCKAGE OCCURS IN DRY CORE BWR SEVERE ACCIDENTS
- ACRR MP EXPERIMENT (SNL)
  - ADDRESSES INTERACTION OF METALLIC CRUST WITH MELT POOL AND CRUST RELOCATION DURING A SEVERE ACCIDENT
- FLHT-6 TEST AT NRU REACTOR (AECL)
  - BWR SEVERE FUEL DAMAGE TEST TO PROVIDE DATA ON FULL LENGTH FUEL ROD BEHAVIOR FOR BWR GEOMETRY FOLLOWING COOLANT BOILDOWN
- CURRENT EXPERIMENTS DO NOT ADDRESS ALL MELT PROGRESSION ISSUES (I.E., MOLTEN POOL CONVECTION DETERMINES HEAT TRANSFER FROM POOL TO CRUST, AND HENCE AFFECTS CRUST FAILURE)

CORE MELT PROGRESSION ANALYSIS/MODELING

- ANALYSIS OF RESULTS OF EXPERIMENTAL DATA (LOFT FP-2, CORA, PBF, ACRR, NRU, TMI-2 CORE EXAMINATION)
- IMPROVE PHENOMENOLOGICAL MODELS IN SEVERE ACCIDENT CODES (E.G., SCDAP/RELAP5, MELCOR)

RESEARCH PLAN FOR MELT PROGRESSION

- OBJECTIVES
    - TO DEVELOP COMPREHENSIVE CORE MELT PROGRESSION RESEARCH PROGRAM
    - TO IDENTIFY AND PRIORITIZE RESEARCH NEEDED TO IMPROVE UNDERSTANDING OF CORE MELT PROGRESSION (AMOUNT, COMPOSITION AND TEMPERATURE OF MELT IN LOWER HEAD AT TIME OF LOWER HEAD FAILURE)
- DESCRIBE CURRENT UNDERSTANDING OF IN-VESSEL CORE MELT PHENOMENA
- IDENTIFY ONGOING RESEARCH PROGRAMS THAT ADDRESS THESE PHENOMENA
- EXAMINE APPLICABILITY OF EXISTING EXPERIMENTS/DATA
- IDENTIFY NEW RESEARCH NEEDED TO ADDRESS UNCERTAINTIES AND IMPROVE UNDERSTANDING OF MELT PROGRESSION PHENOMENA TO AN ACCEPTABLE LEVEL
- IDENTIFY PHENOMENA FOR WHICH IT MAY BE IMPRACTICAL TO PERFORM EXPERIMENTS

RESEARCH PLAN FOR MELT PROGRESSION

- Preliminary draft for review - May 1991
- Information copy provided to ACRS
- Consultants Technical review of plan (meeting August 13-14, 1991)
- Prepare revised draft based on comments from initial review
- Plan to have revised draft reviewed by
    - Consultant peer review
    - ACRS
    - NSRRC
- Incorporate results into updated SARP

CORE MELT PROGRESSION: STATUS AND FUTURE PLANS

R.W. WRIGHT
ACCIDENT EVALUATION BRANCH

MEETING OF THE ACRS SUBCOMMITTEE ON SEVERE ACCIDENTS
BETHESDA, MARYLAND
OCTOBER 24, 1991

In-Vessel Core Melt Progression

- Melt progression describes the state of the reactor core from core uncovery to reactor vessel meltthrough, including hydrogen generation.

- Improved knowledge of the state of the core at vessel failure will provide the initial conditions for confirmatory assessments of the safety margins in the core-melt threat to containment integrity.

  - melt mass
  - melt composition (metal content)
  - melt temperature (superheat)
  - rate of melt release
  - time of release in a given sequence

- Melt Progression provides the conditions for in-vessel fission-product and aerosol release, transport, and attenuation.

- Melt progression provides the core conditions for assessing accident management strategies.

- Only the characteristics of the melt released from the reactor core will be addressed here.

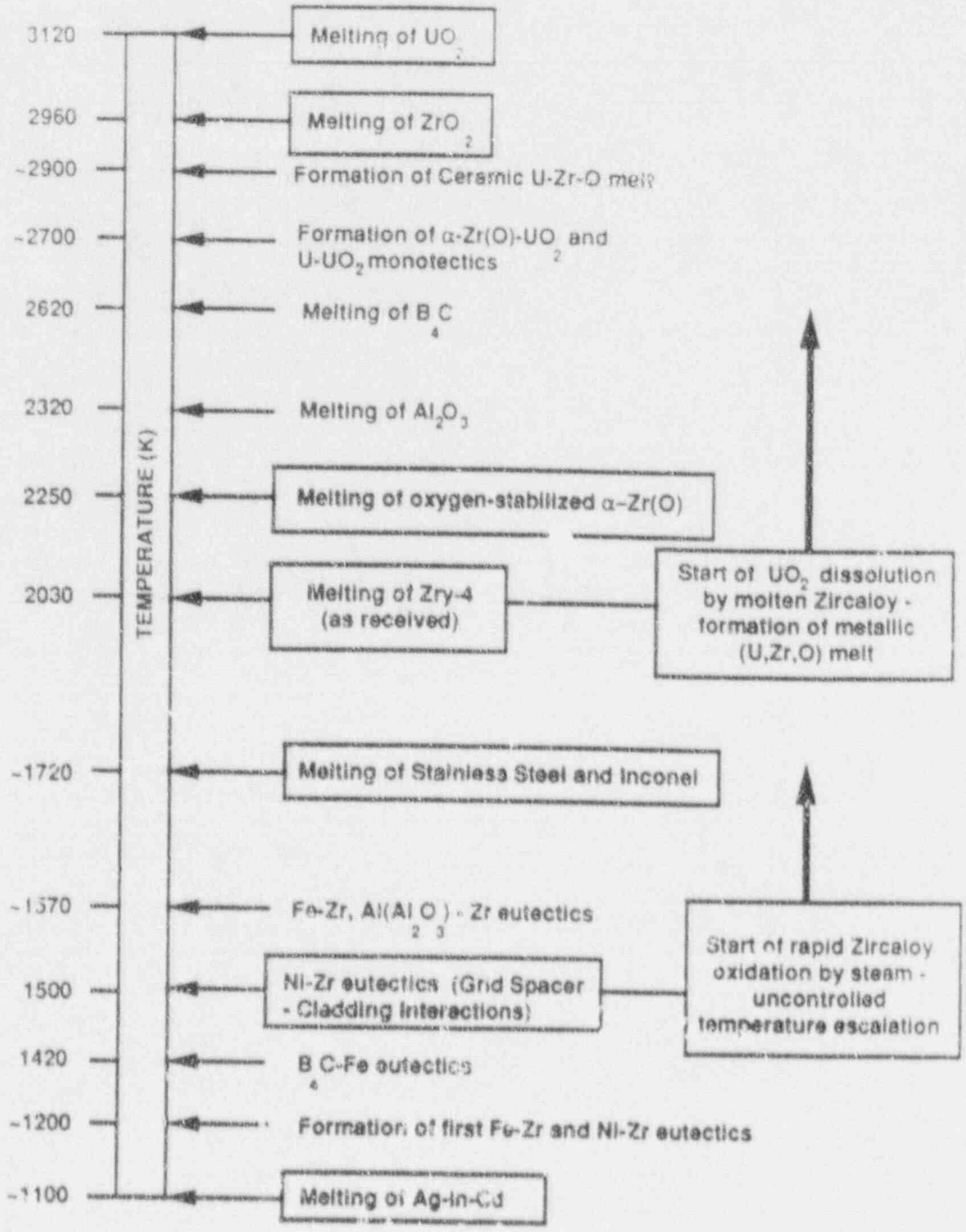
  - Lower plenum melt-coolant interactions and melt-vessel interactions are discussed elsewhere.

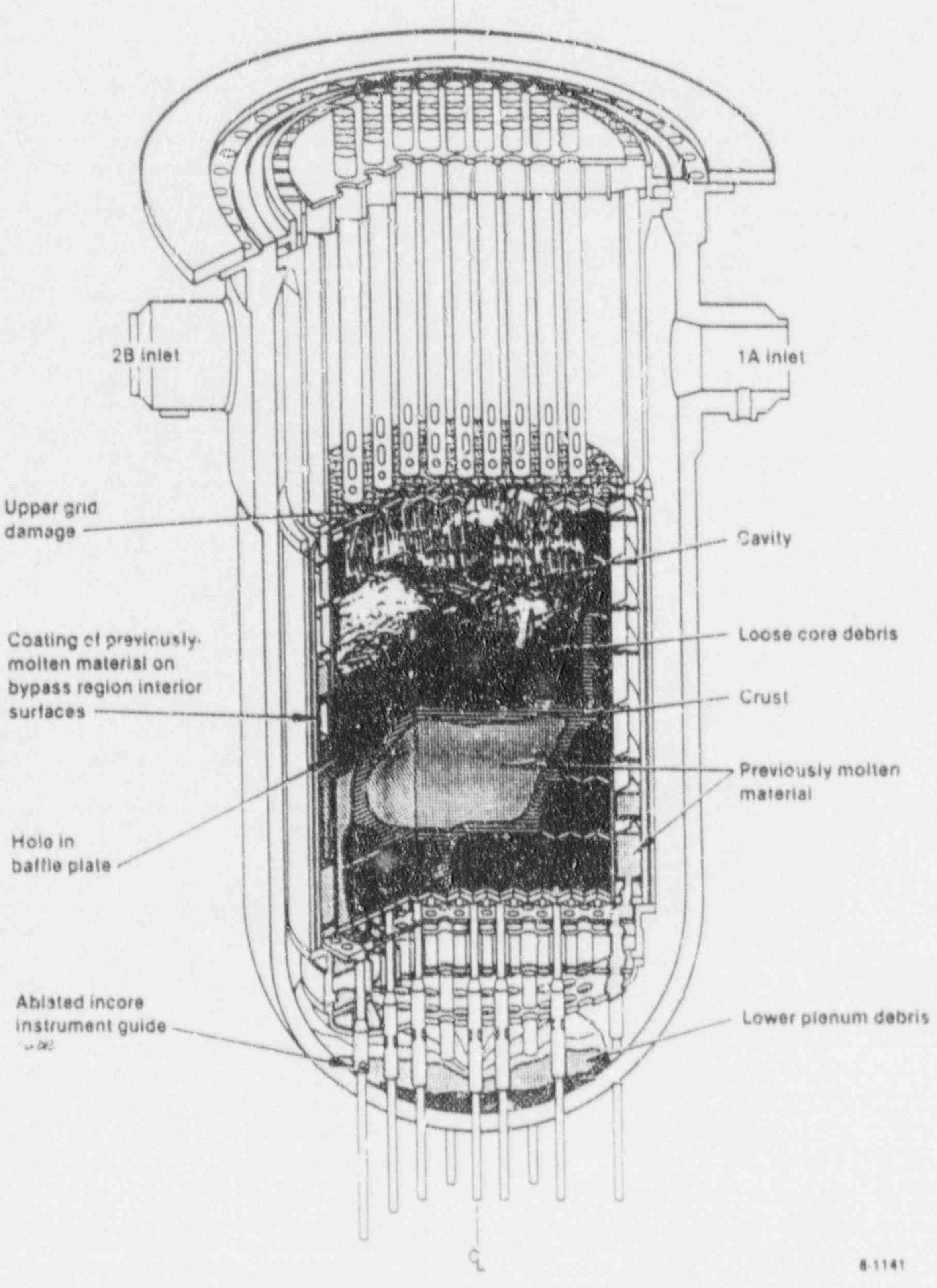
MELT PROGRESSION RESEARCH

- MELT PROGRESSION RESEARCH IS A PART OF LONG-TERM CONFIRMATORY RESEARCH IN TERMS OF THE REVISED SARP (NUREG-1365).
- NEVERTHELESS, IT IS APPROPRIATE TO FOCUS ON THE KEY UNCERTAINTIES OR ISSUES THAT REQUIRE RESOLUTION IN ORDER TO PROVIDE A MECHANISTIC UNDERSTANDING OF THE CHARACTERISTICS OF THE RELEASED MELT.
  - UNPRIORITIZED "LAUNDRY LISTS" OF TECHNICAL UNCERTAINTIES ARE NOT USEFUL FOR RESEARCH GUIDANCE.
- THUS, IN-VESSEL MELT PROGRESSION RESEARCH IS FOCUSED ON:
  - TWO KEY ISSUES FOR RESOLUTION
  - ADDITIONAL SECONDARY UNCERTAINTIES
- THE KEY ISSUES ARE:
  - CONDITIONS FOR THE OCCURRENCE OF A BLOCKED CORE (LIKE TMI-2)
  - CERAMIC POOL MELTTHROUGH FROM A BLOCKED CORE, THRESHOLD AND FAILURE LOCATION

MELT PROGRESSION RESEARCH NEEDS

- NEED A TECHNICAL BASIS TO DETERMINE THE CHARACTERISTICS OF THE MELT RELEASED FROM A BLOCKED CORE AND FROM THE REACTOR VESSEL.
    - SUBSTANTIATE THE TMI-2 LOW MELT RELEASE FRACTION (~20%) FOR GENERAL APPLICATION RATHER THAN ASSUMED 75%, 100%, ETC.
    - VERY LOW METAL CONTENT IN RELEASED CERAMIC MELT.
    - IMPORTANT FOR THE ASSESSMENT OF DCH, MELT-CONCRETE, ETC.
- VALIDATED MODELS ARE NEEDED TO APPLY TMI-2 PHENOMENOLOGY OVER THE RANGE OF BLOCKED-CORE ACCIDENT CONDITIONS.
- ALSO NEED TO DETERMINE THE ACCIDENT CONDITIONS, IF ANY, UNDER WHICH THE CORE IS NOT BLOCKED SO THAT METALLIC AND LATER CERAMIC MELT DRAIN FROM THE CORE (AND BWR CORE PLATE) AS FORMED.
    - IN DRAINAGE CASE, FORM FROZEN DEBRIS LAYERS IN THE WATER FILLED LOWER PLENUM, ACCORDING TO THE TIME OF MELTING, THAT SLOWLY MELT ON DRYOUT.
    - GET MOSTLY METALLIC MELT POOL AT LOWER TEMPERATURE AT VESSEL FAILURE.
    - PRIMARY CONCERN IS BWR "DRY CORE" CONDITIONS FOLLOWING ADS BLOWDOWN.


TEMPERATURE (K)
3120
Melting of UO_2
2960
Melting of ZrO_2
~2900
Formation of Ceramic U-Zr-O melt?
~2700
Formation of α-Zr(O)-UO_2 and U-UO_2 monotectics
2620
Melting of B_4C
2320
Melting of Al_2O_3
2250
Melting of oxygen-stabilized α-Zr(O)
2030
Melting of Zry-4 (as received)
Start of UO_2 dissolution by molten Zircaloy - formation of metallic (U,Zr,O) melt
~1720
Melting of Stainless Steel and Inconel
~1570
Fe-Zr, Al(Al_2O_3) - Zr eutectics
1500
Ni-Zr eutectics (Grid Spacer - Cladding Interactions)
Start of rapid Zircaloy oxidation by steam - uncontrolled temperature escalation
1420
B_4C-Fe eutectics
~1200
Formation of first Fe-Zr and Ni-Zr eutectics
~1100
Melting of Ag-In-Cd



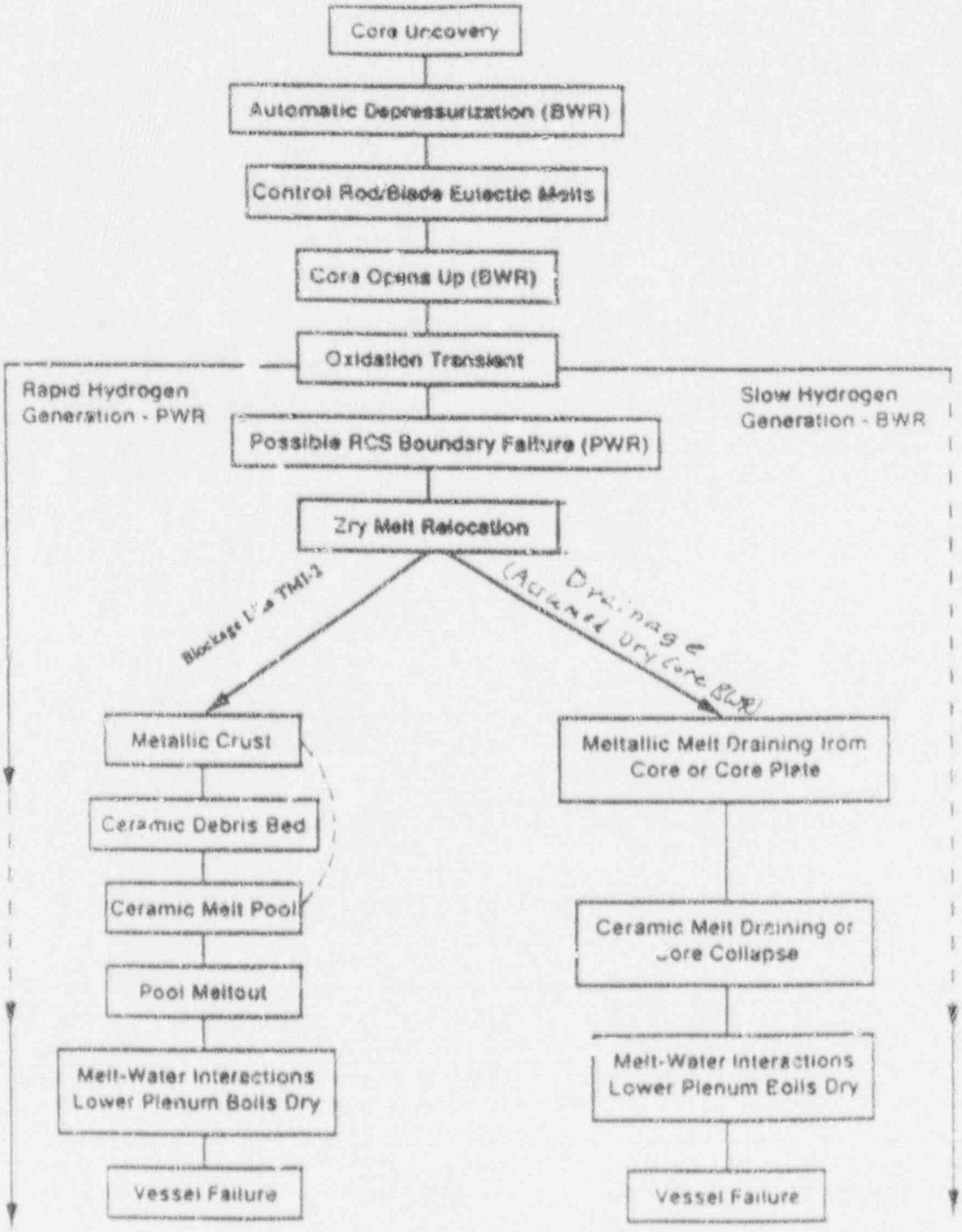
TMI-2 Core End State Configuration

CORE MELT PROGRESSION: STATUS OF CURRENT UNDERSTANDING

- REASONABLY WELL UNDERSTOOD PHENOMENA IN EARLY (METALLIC MELT) PHASE
  - CLAD BALLOONING
  - INTACT-CORE-GEOMETRY OXIDATION HEATING AND HYDROGEN GENERATION
  - UO_2 LIQUEFACTION (DISSOLUTION) BY MOLTEN ZIRCALOY
  - EUTECTIC MATERIAL INTERACTIONS AND RATES AMONG UO_2, ZRO_2, ZRY, AND CONTROL MATERIALS AND THEIR OXIDES
  - OPENING UP OF THE COMPARTMENTALIZED BWR CORE EARLY IN A BWR ACCIDENT BY THE EUTECTIC INTERACTION OF CONTROL-BLADE MATERIAL WITH ZRY CHANNEL BOX WALLS
  - MOLTEN ZRY RELOCATION IS A NONCOHERENT, NONCOPLANAR, RIVULET- FLOW PROCESS THAT DOES NOT BLOCK STEAM FLOW AND HYDROGEN GENERATION. IT IS NOT A FILM FLOW PROCESS
- GENERAL UNDERSTANDING OF LATE (CERAMIC MELT) PHASE
  - BASED PRIMARILY ON TMI-2 CORE EXAMINATION
  - RESULTS ALSO GENERALLY APPLICABLE TO PWR UNRECOVERED ACCIDENTS
  - CERAMIC MELT POOL GROWTH AND MELTTHROUGH FROM BLOCKED CORE
  - REFLOODING PROBABLY STOPPED DOWNWARD POOL AND CRUST RELOCATION TO GIVE SIDE MELTTHROUGH AT TMI-2
  - LIMITED MELT MASS RELEASED FROM CORE (20% AT TMI-2)
  - LOW METAL CONTENT IN CERAMIC MELT POOL
- HYDROGEN GENERATION AND STRONG HEATING OF UNCOVERED CORE FROM ZIRCALOY OXIDATION BY REFLOOD STEAM (LOFT FP-2 AND CORA)

MELT PROGRESSION: HAVE THESE DIFFERENT ACCIDENT CONDITIONS

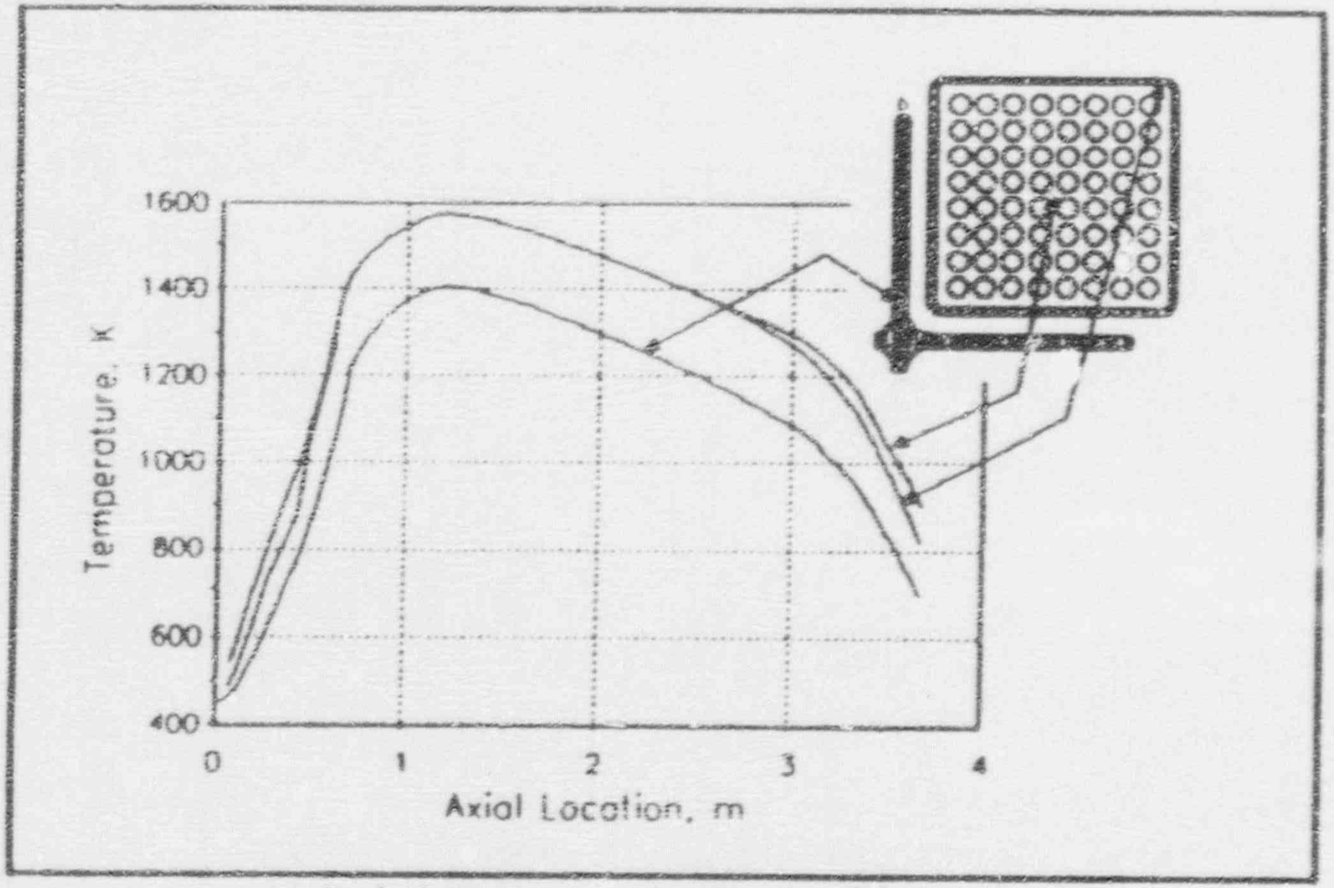
1. WET CORE COOLANT BOILDOWN
   - BLOCKED CORE LIKE TMI-2
   - CASE OF <u>ALL</u> EXISTING EXPERIMENTAL INFORMATION, PBF, ACRR, NRU, CORA, LOFT FP-2
2. DRY CORE FROM ADS BLOWDOWN
   - U.S. BWR EMERGENCY OPERATING PROCEDURES
   - QUESTION OF METALLIC (AND LATER CERAMIC) MELT DRAINAGE OR BLOCKAGE UNDER THESE CONDITIONS
   - TO BE ADDRESSED IN NEW EX-REACTOR EXPERIMENTS ON METALLIC MELT BLOCKAGE OR DRAINAGE
3. EARLY-PHASE REFLOOD THAT PROMPTLY QUENCHES THE CORE
   - OXIDATION HEATING BY REFLOOD STEAM, HYDROGEN, FISSION PRODUCTS, FRAGMENTATION
4. LATE-PHASE REFLOOD THAT DOES NOT STOP MELT POOL GROWTH
   - TMI-2 ACCIDENT CASE
   - REFLOODING AT TMI-2 STOPPED DOWNWARD CRUST RELOCATION GIVING MELTTHROUGH OUT THE SIDE OF THE CORE



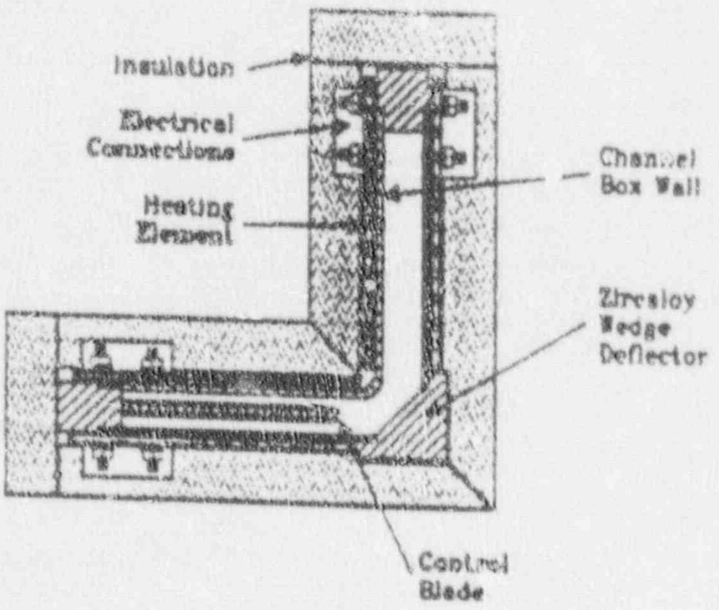
Core Melt Progression Sequence Showing Blockage or Drainage Paths

CONDITIONS FOR THE OCCURRENCE OF A BLOCKED CORE

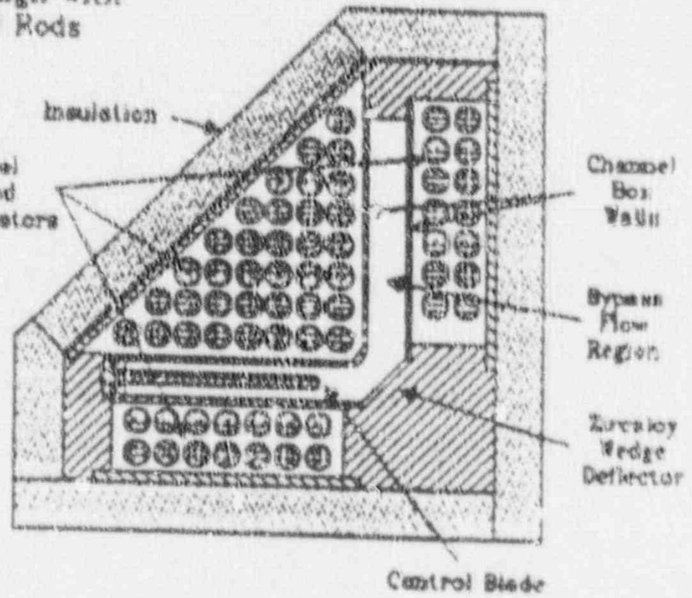
- HAVE MAJOR TECHNICAL DISAGREEMENT ON WHETHER FOR DRY-CORE CONDITIONS, PARTICULARLY FOR BWRS, HAVE METALLIC MELT BLOCKAGE OR DRAINAGE FROM THE CORE (AND BWR CORE PLATE).
  - ALL BWR TESTS, ACRR DF-4 AND CORA INDICATE BLOCKAGE, BUT ARE ALL FOR WET CORE CONDITIONS.
  - PRIMARY INTEREST: BWR DRY-CORE SEQUENCES.
- MAIN EXPERIMENT: EX-REACTOR METALLIC MELT RELOCATION AND BLOCKAGE EXPERIMENTS FOR BWR DRY-CORE CONDITIONS.
  - FULL RADIAL SCALE SECTION OF LOWER QUARTER OF BWR CORE AND CORE PLATE WITH PROTOTYPIC INITIAL TEMPERATURE DISTRIBUTION.
  - DRIBBLE OF METALLIC MELT OF PROTOTYPIC COMPOSITION, TEMPERATURE, AND RATE INTO PRE-HEATED LOWER QUARTER OF A SIMULATED BWR CORE. INTERNAL HEATING OF THE MELT NOT NEEDED.
  - CHARACTERISTICS OF PROTOTYPIC POURS TAKEN FROM EARLIER CORA RESULTS AND ANALYSIS.
- ASSESSING POROUS MEDIA MODELING (ANISOTROPIC PERMEABILITY) OF THE PROCESS OF METALLIC MELT RELOCATION AND BLOCKAGE FORMATION.
- ADDITIONAL INFORMATION FROM:
  - CORA TESTS; OXIDE FILM EFFECTS, LOW HEATING RATES (LONG TERM STATION BLACKOUT), CHECK OF BOILOFF VS. INPUT STEAM FLOW.
  - FINAL NRU TEST: LENGTH EFFECT METALLIC RELOCATION DATA IN 14-ROD BWR CORE GEOMETRY.



Axial temperature profiles for the control blade, the channel box and the fuel rods, indicating maximum lateral temperature variations.



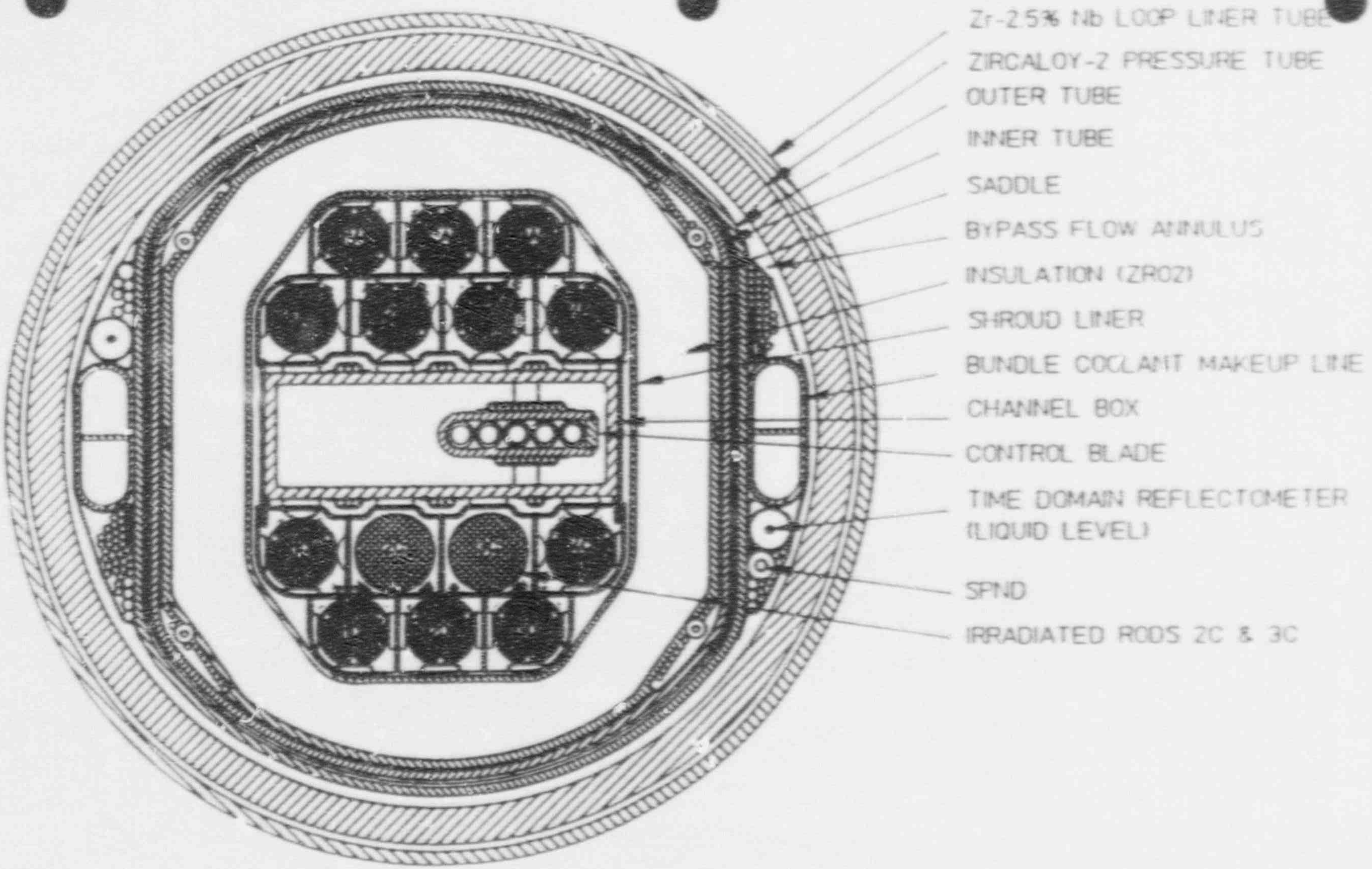
"L" Design with Fuel Rods



Cross sectional views of two test bundle designs to be investigated in the Ex-Reactor experiments.

FULL LENGTH BWR TEST FLHT-6 IN NRU

- PURPOSE:
  - TO PROVIDE LENGTH-EFFECT DATA IN BWR CORE GEOMETRY ON METALLIC MELT RELOCATION AND BLOCKAGE FORMATION AND ON HYDROGEN GENERATION.
  - TO PROVIDE UNIQUE DATA ON FISSION PRODUCT RELEASE AND TRANSPORT IN THE BWR BORON-CONTAINING CHEMICAL ENVIRONMENT.
- FLHT-6 IN BWR CORE GEOMETRY IS THE LAST IN A SERIES OF FULL-LENGTH COOLANT BOILDOWN AND FUEL DAMAGE TESTS IN THE CANADIAN NRU TEST REACTOR.
- FLHT-6 WILL HAVE A B_4C CONTROL BLADE, ZIRCALOY CHANNEL BOX WALLS, 2 HIGH BURN-UP FUEL RODS, AND 12 FRESH FUEL RODS.
  - MOST MELT RELOCATION AND BLOCKAGE DATA FROM END-STATE PIE.
  - FLHT-6 TO BE PERFORMED IN EARLY 1992.



FLHT - 5 TRANSVERSE CROSS SECTION SHOWING KEY IN-REACTOR COMPONENTS

LATE PHASE MELT PROGRESSION

- IN BLOCKED CORE ACCIDENTS, THE MASS AND OTHER CHARACTERISTICS OF THE MELT RELEASED FROM THE CORE ARE DETERMINED BY THE THRESHOLD AND THE LOCATION OF FAILURE OF THE METALLIC AND CERAMIC CRUST THAT SUPPORT THE GROWING MELT POOL.
  - WITH THE SIDE FAILURE AT TMI-2 ONLY HALF THE CERAMIC MELT POOL DRAINED INTO THE LOWER PLENUM.
  - WITH THE REFLOOD WATER BELOW THE CRUST, THE MELT POOL AT TMI-2 MELTED OUT THE SIDE OF THE CORE.
- THERE ARE THREE PRIMARY DETERMINANTS OF THE THRESHOLD AND LOCATION OF POOL MELTTHROUGH.
  1. WHETHER BOILDOWN IS LOWERING THE CORE WATER LEVEL OR IT IS FIXED BY REFLOODING, AS AT TMI-2.
  2. THE SURFACE HEAT FLUX DISTRIBUTION FROM THE INTERNALLY-HEATED MELT POOL.
  3. THE PROCESS OF MELTTHROUGH OF THE COMPLEX ROD-STUB-SUPPORTED METALLIC AND CERAMIC CRUST SYSTEM (WITH A GIVEN HEAT FLUX).
     - MATERIAL INTERACTIONS (EUTECTICS) APPEAR SIGNIFICANT HERE.

EXPERIMENTS ON LATE PHASE MELT PROGRESSION

- POOL MELTTHROUGH FROM A BLOCKED CORE IS A WHOLE CORE PHENOMENON.
- BUT IT IS NOT POSSIBLE TO DO FULL-SCALE WHOLE CORE MELTDOWN SIMULATION TESTS TO SUPPLEMENT THE TMI-2 INFORMATION ON CERAMIC POOL MELTTHROUGH.
  - INTERNAL HEAT GENERATION AND REAL REACTOR MATERIALS AND TEMPERATURES (ACRR MP EXPERIMENTS).
  - FULL REACTOR SCALE (ABOUT $1M^3$ POOL) NEEDED FOR EXPERIMENTS ON MELT POOL THERMAL HYDRAULICS AT RA $\approx 10^{16}$ (REACTOR MATERIALS NOT REQUIRED).
- THEREFORE MUST BREAK PROBLEM INTO SEPARABLE PARTS AND INTEGRATE THE RESULTS BY ANALYSIS - HAVE NO CHOICE.
  - SIMILAR PROBLEMS OCCUR REGARDING MELT ATTACK ON THE VESSEL LOWER HEAD AND EX-VESSEL FLOODING TO PREVENT VESSEL MELTTHROUGH.

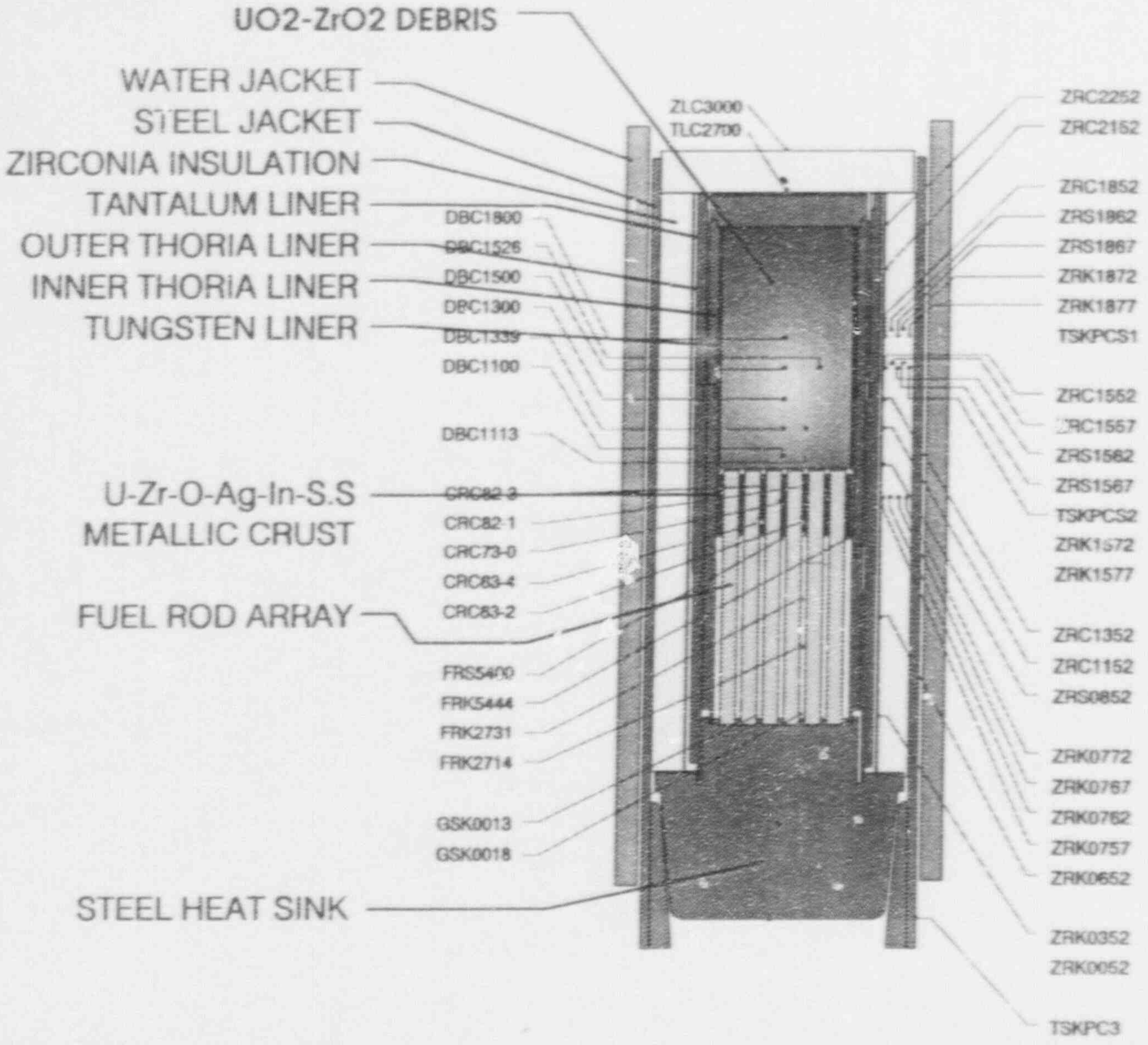
LATE PHASE MELT PROGRESSION: RESEARCH APPROACH

- MELT POOL THERMAL HYDRAULICS
  - WILL PERFORM ANALYSIS (WITH PEER REVIEW) ON WHETHER CURRENT LOW RAYLEIGH NUMBER DATA BASE ON THE SURFACE HEAT FLUX DISTRIBUTIONS OF INTERNALLY HEATED POOLS IS ADEQUATE FOR LATE-PHASE MELT PROGRESSION USE.
- ARE PERFORMING TWO MP (MELT PROGRESSION) EXPERIMENTS WITH INTERNAL HEATING IN ACRR ON THE PROCESS OF MELTTHROUGH OF A CERAMIC MELT POOL FROM A BLOCKED CORE.
  - MP-1 PERFORMED DECEMBER, 1989, MP-2 PLANNED FOR EARLY 1992
- THE DEBRIS POROUS MEDIA MODEL OF LATE-PHASE MELT PROGRESSION TREATS THE MELTING, RELOCATION, AND FREEZING DYNAMICS IN A PARTICULATE CERAMIC DEBRIS BED, INCLUDING THE FORMATION, RELOCATION, AND FAILURE OF THE PRIMARY METALLIC CRUST AND THE SECONDARY CERAMIC CRUST.
  - DEBRIS MODELING, IF VALIDATED BY EXPERIMENT, WILL BE INCORPORATED AS NEEDED (PROBABLY IN SIMPLIFIED FORM) INTO SCDAP/RELAP5 AND MELCOR.
  - DEBRIS AGREED WELL WITH THE MP-1 RESULTS.

THE MP-1 AND MP-2 EXPERIMENTS IN ACRR

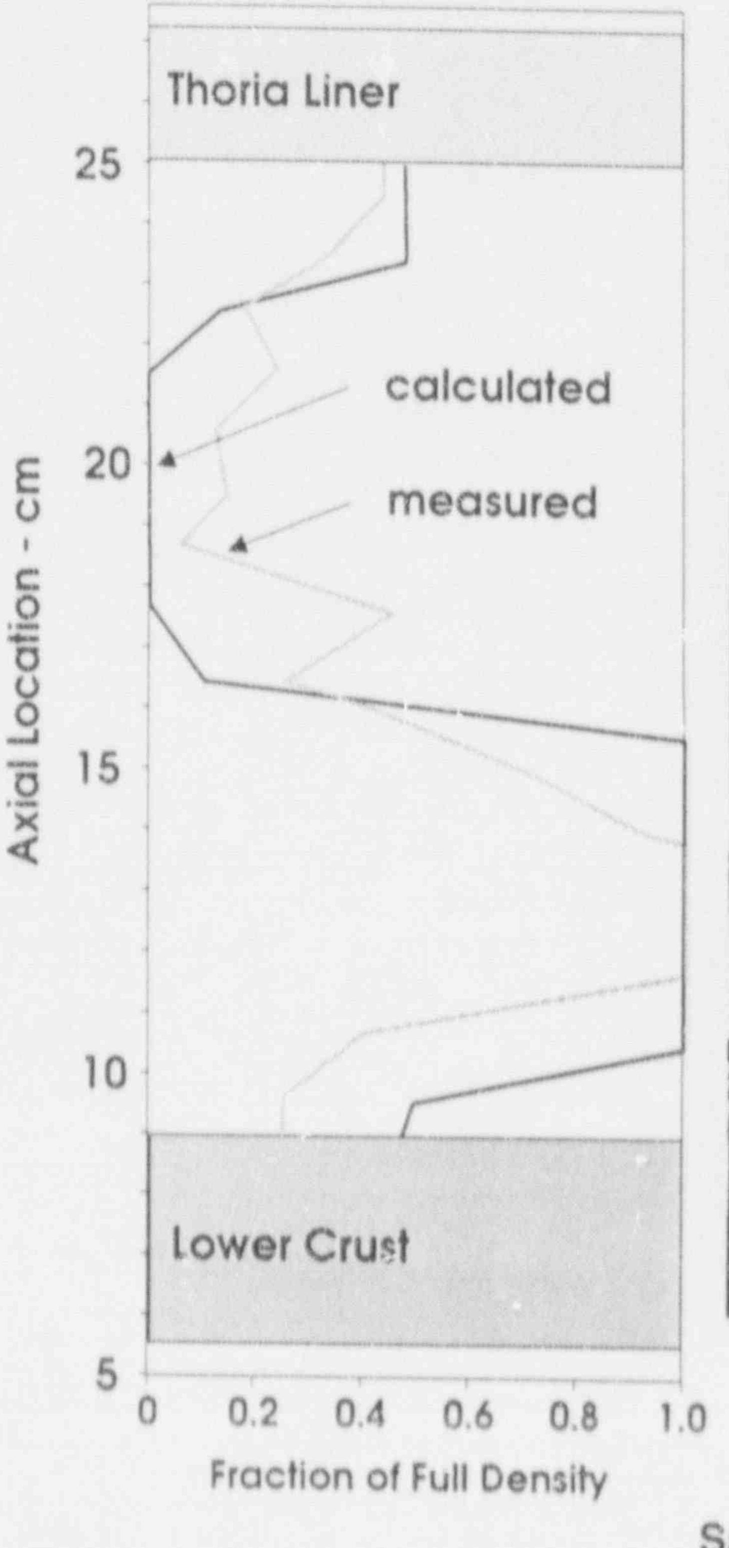
- PURPOSE: TO PROVIDE INFORMATION ON THE KEY PROCESSES IN THE RELOCATION AND FAILURE DYNAMICS OF THE POOL SUPPORTING CRUSTS IN UNCOVERED BLOCKED CORE ACCIDENTS (NO REFLOODING).
  - TO BE USED IN DETERMINING THE CHARACTERISTICS (MASS, COMPOSITION, TEMPERATURE, AND RATE OF RELEASE) OF THE MELT RELEASED FROM A BLOCKED CORE AND LATER FROM THE REACTOR VESSEL.
- PARTICULATE CERAMIC DEBRIS BED WITH A GROWING CERAMIC MELT POOL THAT IS SUPPORTED BY A PRECAST METALLIC CRUST ACROSS 32 FUEL-ROD STUBS.
  - FISSION HEATING IN ACRR PROVIDES INTERNAL HEAT GENERATION IN THE UO_2 IN THE DEBRIS BED, IN THE MELT POOL, AND DISSOLVED IN THE METALLIC CRUST.
  - ON-LINE CHARACTERIZATION OF INTERNAL TEMPERATURES IS PROVIDED BY AN ARRAY OF SPECIAL HIGH-TEMPERATURE THERMOCOUPLES.
  - MOST OF THE INFORMATION OBTAINED FROM POST-TEST EXAMINATION
  - MP-1, EUTECTIC ZR-UO_2 METALLIC CRUST, MP 2100K.
  - MP-2, PROTOTYPIC TMI-2 METALLIC CRUST WITH CONTROL ROD MATERIALS.
- WITH THE RESULTS FROM MP-2, DEBRIS ANALYSIS, AND SENSITIVITY STUDIES, THE LATE PHASE MELT PROGRESSION PEER REVIEW GROUP WILL PROVIDE GUIDANCE ON THE NEED FOR AND NATURE OF FUTURE EXPERIMENTS.

MP-2 Test Section showing Major Components and Thermocouple Locations



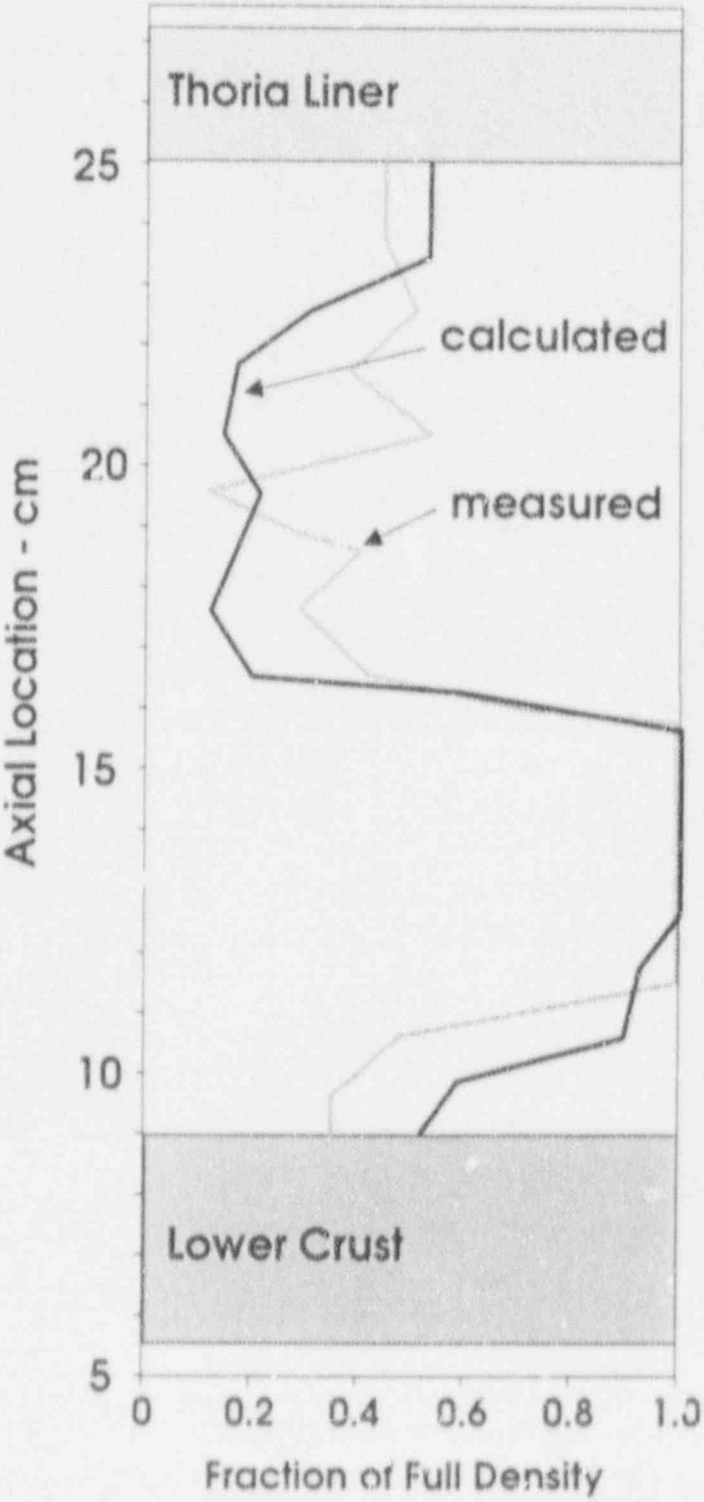
Sandia National Laboratories

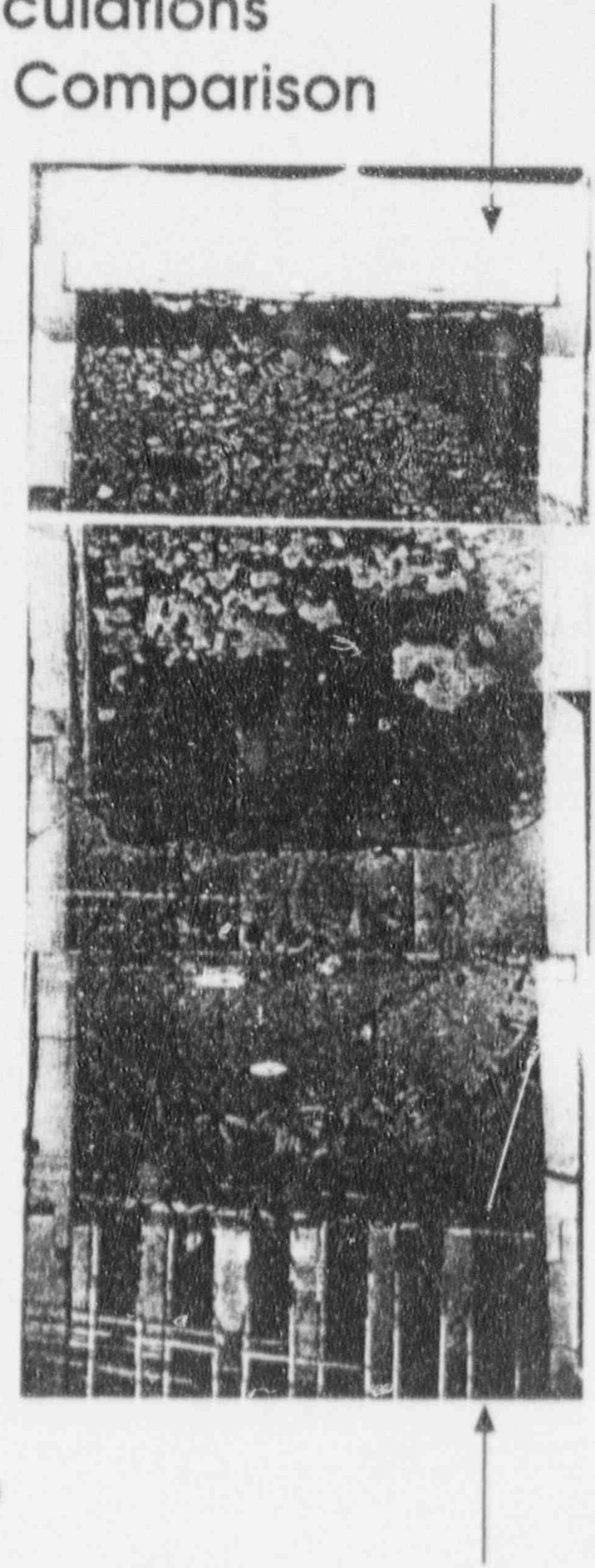
MP-1 Material Density vs. DEBRIS Calculations Centerline Comparison



Sandia National Laboratories

MP-1 Material Density vs. DEBRIS Calculations Off-Centerline Comparison





Summary of Current Melt Progression Uncertainties and Applicable Current Research

Uncertainty	Where Addressed
Early Metallic Melt Progression Phase	
Conditions for Blocked BWR Cores	Ex-Reactor Melt Relocation & Blockage Expts., NRU, CORA (FRG) Modeling: Porous Media, DEBRIS, Other
- Mechanisms in Melt Relocation and blockage formation	Ex-Reactor Melt Relocation & Blockage Expts., NRU, CORA (FRG) Modeling: Porous Media, DEBRIS, Other
- Effects of Zry-C.R. Matl. Eutectics	CORA (FRG)
- Pre-existing Oxide Film Effects	CORA (FRG), Ex-Reactor Melt Relocation & Blockage Expts
- Thresholds of Metallic Melt Reloc	CORA (FRG)
- H_2 Gen from Relocated Metallic Melt	NRU, CORA (FRG)
- Effects of High Burnup Fuels	NRU,
- Hot Gas (Natural Circulation) Failure of the Primary System Boundary	Analysis, Expts.
Late Ceramic Melt Progression Phase	
Ceramic Pool Meltthrough Mechanisms	ACRR MP, Modeling: DEBRIS, Other
- Ceramic Blockage without Metallic Blockage	ACRR MP, Modeling: DEBRIS
- Natural Circulation Melt Pool Thermal Hydraulics	Analysis, Possible Expt
- Transition from Declad Fuel-Rod to Debris Bed Geometry	NRU
- Late Phase High Burnup Fuel Effects	Phebus (France/CEC)
- H_2 Gen from Relocated Melt	None

MELT PROGRESSION RESEARCH: APPLICABILITY TO ELWRs AND ALWRs

- THE GENERAL PHENOMENOLOGY OF MELT PROGRESSION IS ALSO APPLICABLE TO ELWRs AND ALWRs.
    - TO THE EXTENT THAT MECHANISTIC MODELS ARE AVAILABLE, THEY CAN BE USED DIRECTLY FOR ELWRs AND ALWRs.
    - CORE HEAT-UP AND HYDROGEN GENERATION MODELS ARE DIRECTLY APPLICABLE.
    - EXISTING MODELS AND CODES CAN BE USED FOR ANALYSIS OF THESE EFFECTS UNDER THE DIFFERENT ELWR AND ALWR CONDITIONS.
    - SYSTEMS CODES REQUIRE SOME MODIFICATIONS TO ACCOMMODATE CHANGED FEATURES, I.E., REFLECTORS.
- THE LOWER POWER DENSITY IN ALWRs WILL AFFFECT MELT PROGRESSION.
    - LOWER HEAT UP AND STEAMING RATES AND SOMEWHAT LOWER PEAK TEMPERATURES FROM OXIDATION.
    - SOMEWHAT MORE IN-PLACE OXIDATION, LOWER METALLIC MELT MASS, AND LOWER CERAMIC MELT MASS.
    - WOULD NEED TO EXAMINE THE POSSIBILITY THAT THE TMI-2 BLOCKED CORE SCENARIO WITH CERAMIC POOL MELTTHROUGH MIGHT NOT OCCUR UNDER THESE CONDITIONS.
- DO HAVE ONE TEST IN THE GERMAN CORA EX-REACTOR FUEL DAMAGE TEST FACILITY AT A LOW TEMPERATURE RAMP RATE (30%) BUT HIGH STEAM FLOW WITH RESULTS THAT ARE CONSISTENT WITH THIS PICTURE.

THE ACCIDENT MANAGEMENT STRATEGY
OF DEPRESSURIZATION

LOUIS M. SHOTKIN

USNRC

ACRS SEVERE ACCIDENT
SUBCOMMITTEE MEETING

OCTOBER 24, 1991

QUESTIONS RELATED TO RESOLVING THE ISSUE OF DIRECT CONTAINMENT HEATING (DCH) INCLUDE:

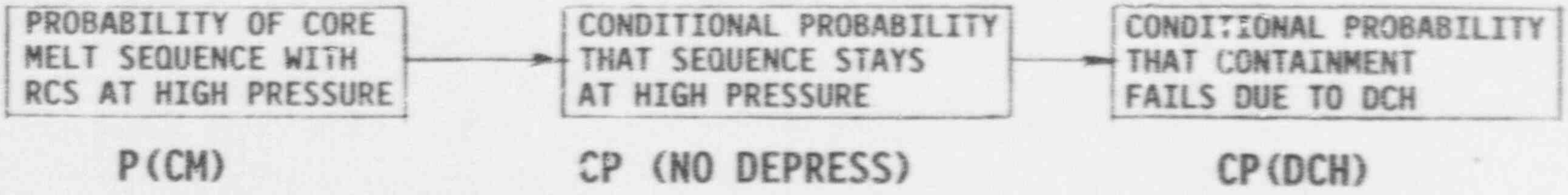
1. CAN THE LIKELIHOOD OF EARLY CONTAINMENT FAILURE DUE TO HIGH-PRESSURE MELT EJECTION (HPME) BE REDUCED BY DEPRESSURIZATION (EITHER INTENTIONAL OR UNINTENTIONAL) OF THE REACTOR VESSEL?

2. IF DEPRESSURIZATION DOES NOT OCCUR WILL THE OCCURRENCE OF HPME LEAD TO UNACCEPTABLE CONTAINMENT LOADS DUE TO DCH?

3. WHAT IS THE RISK ASSESSMENT OF THE DEPRESSURIZATION STRATEGY?

THIS TALK WILL DISCUSS ITEMS 1 AND 3

THERE ARE THREE PHASES THAT AFFECT AN EARLY CONTAINMENT FAILURE FROM DCH



PROBABILITY OF CORE MELT SEQUENCE WITH RCS AT HIGH PRESSURE

P(CM)

CONDITIONAL PROBABILITY THAT SEQUENCE STAYS AT HIGH PRESSURE

CP (NO DEPRESS)

CONDITIONAL PROBABILITY THAT CONTAINMENT FAILS DUE TO DCH

CP(DCH)

- CP(NO DEPRESS) CONSISTS OF TWO PARTS:
  - FAILURE TO PASSIVELY DEPRESSURIZE VIA LARGE PUMP SEAL LOCAs, STUCK OPEN RELIEF VALVES, AND NATURAL CIRCULATION TEMPERATURE-INDUCED FAILURES.
  - FAILURE TO INTENTIONALLY DEPRESSURIZE
- IF THE PRODUCT OF THE THREE PROBABILITIES IS "LOW ENOUGH" DCH-CAUSED CONTAINTAINMENT FAILURE IS OF LITTLE CONCERN.

INTENTIONAL DEPRESSURIZATION

- CALCULATIONS WERE PERFORMED USING THE SCDAP/RELAP5 CODE AND SURRY DECK TO ANALYZE EFFICACY OF DEPRESSURIZATION DURING STATION BLACKOUT.

- THE SURRY TYPE OF PLANT CAN BE DEPRESSURIZED TO APPROXIMATELY CONTAINMENT PRESSURE BY LATCHING OPEN TWO PORVs.

- TWO DIFFERENT DEPRESSURIZATION CASES WERE CONSIDERED. EARLY DEPRESSURIZATION (INITIATED AT S.G. DRYOUT) AND LATE DEPRESSURIZATION (INITIATED AT CORE UNCOVERY). LATE DEPRESSURIZATION IS THE PREFERRED STRATEGY. IT PROVIDES LONGER TIME FOR AC RECOVERY AND ALTERNATE COOLING, AND DELAYS CORE DAMAGE BECAUSE OF ACCUMULATOR INJECTION.

- STEAM EXPLOSIONS MAY OCCUR AS THE PLANT IS DEPRESSURIZED AND PART OF THE CORE RELOCATES TO THE LOWER PLENUM. PRELIMINARY CALCULATIONS SHOW THAT THEY MAY BE BENIGN AND MAY NOT CAUSE A VESSEL FAILURE.

- EOP MODIFICATIONS MAY BE NECESSARY TO RELIABLY ACCOMPLISH DEPRESSURIZATION.

- SUPPORT SYSTEMS (BATTERY AND AIR) MAY NEED EXTENSION OVER TRANSIENT DURATION TO ENSURE THE PORVs WILL OPERATE AS NECESSARY.

- EXTENSION OF SURRY DEPRESSURIZATION RESULTS TO OTHER PWRs WAS PERFORMED.

    - WHICH PLANTS WOULD RESPOND FAVORABLY TO THIS STRATEGY

    - FOR WHICH PLANTS THE STRATEGY WOULD NOT WORK AT ALL

    - WHICH PLANTS WOULD NEED FURTHER ANALYSIS TO CONFIRM THE EFFECTIVENESS OF THE STRATEGY

- ANALYSIS OF DEPRESSURIZATION IN A B&W PLANT IS BEING INITIATED.

UNINTENTIONAL DEPRESSURIZATION

- IT IS CAUSED BY HOT GASES TRANSFERRING ENERGY TO PIPE WALLS, UPPERHEAD METAL, AND S.G. METAL AS GASES EXIT THROUGH OPEN PORVs, OR IF THEY ARE CLOSED, BY CIRCULATING WITHIN THE HOT LEG. THE SURGE LINE FAILS AS ITS TEMPERATURE INCREASES LEADING TO UNINTENTIONAL DEPRESSURIZATION.

- CALCULATIONS ARE BEING PERFORMED USING THE SCDAP/RELAP5 CODE AND THE SAME SURRY PLANT DECK MODIFIED BY INCLUDING TWO PIPE HOT LEGS TO MODEL HORIZONTAL COUNTER-CURRENT NATURAL CIRCULATION.

- TWO CASES ARE CONSIDERED. NO PUMP SEAL LOCA AND PUMP SEAL LOCA.

- IN NO PUMP SEAL LOCA CASE, SURGE LINE FAILS. THIS IS THE DOMINANT DEPRESSURIZATION MECHANISM.

- IN PUMP SEAL LOCA CASE, SURGE LINE FAILURE MAY NOT BE A DOMINANT MECHANISM. THE PRESSURE IS LOWER AND HOT GASES DO NOT EXIT PORVs. HOWEVER, DURING THE RELOCATION OF THE CORE MATERIAL TO THE LOWER PLENUM THERE MAY BE A PRESSURE SPIKE WHICH MAY CAUSE FAILURE OF THE PRESSURE BOUNDARY (SURGE LINE - SINCE IT HAS THIN WALL AND HIGH TEMPERATURES). THE FAILURE DEPENDS ON SELECTION OF SOME CORE DAMAGE PARAMETERS. SENSITIVITY CALCULATIONS ARE UNDERWAY TO BOUND THE PHENOMENA AND IDENTIFY CONDITIONS WHERE SURGE LINE MAY FAIL.

- EVALUATION OF THE EFFECTS OF HYDROGEN ON NATURAL CIRCULATION
  - HYDROGEN CIRCULATING IN THE REACTOR VESSEL IS NOT EXPECTED TO RESULT IN STRATIFICATION OF FLUID IN THE UPPER PLENUM OR CORE REGIONS.
  - HYDROGEN CIRCULATING IN THE RCS LOOPS CAN CAUSE FLOW BLOCKAGE IF THERE IS CONDENSATION TO CONCENTRATE THE HYDROGEN IN THE STEAM GENERATOR TUBES.

PWR DEPRESSURIZATION: RISK ASSESSMENT

- SNL HAS USED NUREG-1150 MODELS TO DETERMINE THE RISK REDUCTION EFFECTIVENESS OF OPENING PORVs DURING A SBO EVENT IN THE SURRY PLANT.

- THEY FOUND VERY LITTLE RISK REDUCTION BECAUSE OF THE POSSIBILITIES OF INADVERTENT DEPRESSURIZATION, AC-POWER RECOVERY AND THE FACT THAT SURRY HAS A RELATIVELY STRONG CONTAINMENT.

- PLANS ARE TO EXTEND THE SURRY RISK ANALYSIS TO ALL PWRs WITH LARGE-DRY CONTAINMENTS.

- A SIMPLIFIED STUDY OF PWRs WITH ICE-CONDENSOR CONTAINMENTS IS ALSO BEING CONSIDERED.

- THE NUREG-1150 ANALYSIS OF SURRY IS BEING UPDATED TO INCLUDE A BRANCH POINT FOR OPERATOR ACTION TO OPEN PORVs. ALSO SURGE-LINE FAILURE WILL BE INCLUDED IN ALL RELEVANT SEQUENCES (E.G., PUMP-SEAL LOCA AND STUCK-OPEN PORV)

UMCP TESTS IN B&W GEOMETRY

OBJECTIVES:
1. INVESTIGATE NATURAL CIRCULATION FLOW PATTERNS OF VAPOR IN HOT-LEG
2. SHOW EFFECT OF NON-CONDENSABLE GAS ON FLOW PATTERNS IN UPPER PLENUM AND LEAD SCREW GUIDES (TMI-2)

FACILITY WAS MODIFIED:

- SF_6 GAS USED (AND N_2)
- STEAM GENERATORS REMOVED
- LEAD SCREW GUIDES ADDED IN UPPER PLENUM
- NEW SCALED HOT-LEGS
- NEW INSTRUMENTATION SYSTEM
- CORE MODIFIED TO SCALE FLOW RESISTANCE

TWO YEARS OF TESTING BEING PLANNED

TRANSIENT SCALING RATIONALE WAS DEVELOPED AND REVIEWED

DIRECT CONTAINMENT HEATING (DCH) RESEARCH - STATUS AND FUTURE PLANS

ACRS SEVERE ACCIDENT SUBCOMMITTEE

CHARLES G. TINKLER
OFFICE OF NUCLEAR REGULATORY RESEARCH
U. S. NUCLEAR REGULATORY COMMISSION
OCTOBER 24, 1991

OBJECTIVES: PRIMARY OBJECTIVE IS TO DEVELOP AND VALIDATE REASONABLE ANALYTICAL METHODS AND/OR ASSESSMENT CRITERIA FOR EVALUATING THE LOADING, I.E., CONTAINMENT ATMOSPHERE PRESSURE AND TEMPERATURE INCREASE ASSOCIATED WITH HIGH PRESSURE MELT EJECTION OF MOLTEN CORE MATERIAL. SECONDARY OBJECTIVE IS TO ASSESS THOSE LOADS FOR SELECTED PLANTS AND SELECTED ACCIDENT SEQUENCES.

- APPROACH
  - SCALING ANALYSIS IN SUPPORT OF EXPERIMENTAL DESIGN
  - CONDUCT INTEGRAL EXPERIMENTS AT DIFFERENT SCALES TO DEVELOP DATA FROM TESTS AS PROTOTYPIC AS FEASIBLE
    - 1/10 SCALE FACILITY AT SNL (SURTSEY)
    - 1/40 SCALE FACILITY AT ANL (CWTI)
  - ASSESS AVAILABLE ANALYTICAL MODELS

SUMMARY OF FY91/RECENT PROGRESS

- SEPARATE EFFECTS TESTS TO INVESTIGATE EFFECT OF FLIGHT PATH LENGTH (LFP TEST SERIES)
- SEPARATE EFFECTS TEST TO INVESTIGATE EFFECT OF WATER IN THE REACTOR CAVITY (WC TEST SERIES)
- DOCUMENTATION OF TEST RESULTS
- COMPLETION OF SNL SCALING ANALYSIS IN PREPARATION FOR INTEGRAL EFFECTS TESTING
    - SUBJECTED TO PEER REVIEW APRIL 1991
    - INDEPENDENT SCALING BY SASM TPG
        - DRAFT NUREG TO BE ISSUED NOVEMBER 1991
- FIRST LARGE SCALE (1/10) INTEGRAL EFFECTS TEST CONDUCTED IN THE SURTSEY FACILITY 9/13/91
- FIRST COUNTERPART INTEGRAL EFFECTS TEST IN 1/40 SCALE FACILITY CONDUCTED 10/17/91

LIMITED FLIGHT PATH (LFP)
TEST SERIES

Purpose: To investigate the effect of the flight path on DCH

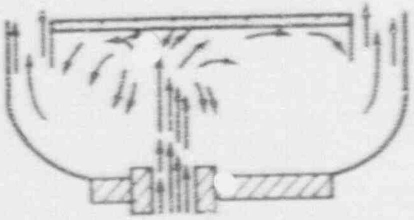
Initial Conditions:

- 1/10th scale Surry cavity
- Melt mass - 50 kg
- Driving pressure - 3 MPa
- Initial Surtsey ATM - > 99.5 Mol. % Argon
  at 0.16 MPa

Initial Conditions Varied:

- Flight Path
- Hole Size

- 103 m^3 Internal Volume
- 1.0 MPa Design Pressure
- Removable Upper and Lower Heads
- Instrumentation Ports at Six Levels



Exploded View of
Debris Flight Path
and Concrete Structure

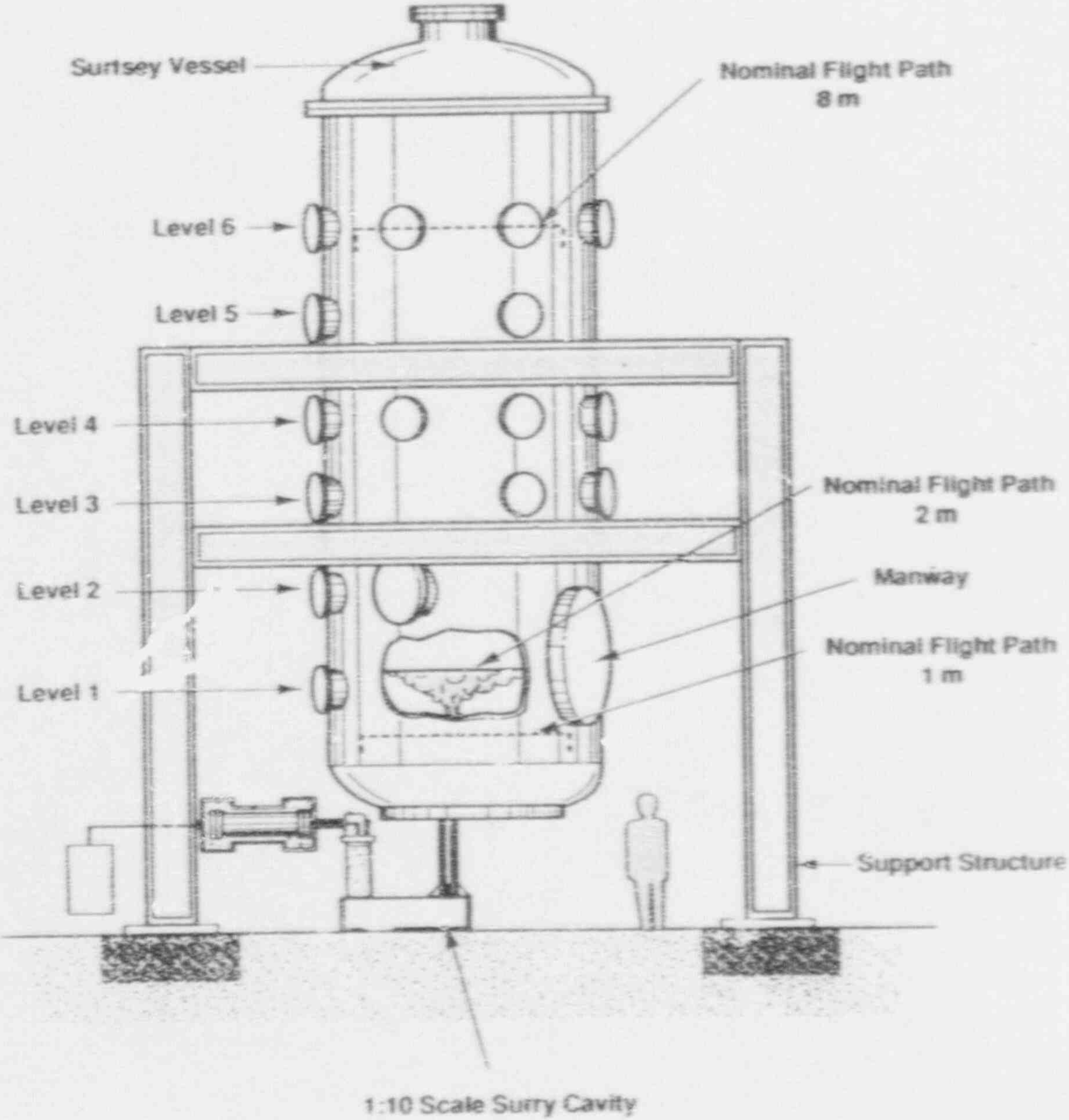


Table 2-1

Initial Conditions for the LFP Experiments

	LFP-1A	LFP-1B	LFP-2A	LFP-2B	LFP-2C	LFP-8A
Flight path (m)						
nominal -	1	1	2	2	2	8
actual -	0.91	0.91	1.85	1.85	1.85	7.70
Mass of the initial charge (kg)	80(a)	50(b)	50(b)	50(b)	50(b)	50(b)
Exit hole diameter (cm)						
nominal -	6.0	3.5	3.5	6.0	10.0	3.5
actual -	6.41	3.5	3.5	5.97	8.57	3.5
Steam pressure at HPME initiation (MPa)	3.7	2.6	3.0	3.6	3.3	2.9
Steam temperature at HPME initiation (K)	585	583	563	606	572	631
Moles of steam driving gas (moles)	262	180	229	249	246	188
Initial pressure in Surtsey (MPa)	0.161	0.158	0.160	0.160	0.160	0.159
Initial gas composition in Surtsey (mol.%) Ar -	99.6%	99.6%	99.7%	99.2%	99.7%	99.5%
N_2 -	0.31%	0.33%	0.26%	0.63%	0.29%	0.38%
O_2 -	0.08%	0.07%	0.05%	0.16%	0.06%	0.08%

Thermite Composition (kg)	(a)	(b)
iron oxide -	54.440	34.025
chromium -	8.648	5.405
aluminum -	16.912	10.570
Total Mass (kg)	80.000	50.000

Table 3-3

Summary of the Results of the LFP Experiments

	LFP-1A	LFP-1B	LFP-2A	LFP-2B	LFP-2C	LFP-8A
Driving pressure at plug failure (MPa)	3.7	2.6	3.0	3.6	3.3	2.9
Moles of H_2O driving gas (moles)	262	180	229	249	246	[illegible]8
Ablated hole diameter (cm)	6.41	3.5	3.5	5.97	8.57	3.5
Time from ignition to HPME (s)	26.75[1]	19.90	21.80	21.57	22.90	23.21
ΔP due to the HPME (MPa)	0.117	0.066	0.102	0.118	0.131	0.172
Moles of H_2 produced (moles)	235.3	128.2	151.4	154.2	184.4	139.3
$H_2O \rightarrow H_2$ conversion efficiency	89.8	71.2	66.1	61.9	75.0	74.1

Notes:

1. The burn time is longer due to the larger initial melt mass.

Table 3-4

Debris Recovery Summary of the LFP Experiments

	LFP-1A	LFP-1B	LFP-2A	LFP-2B	LFP-2C	LFP-8A
Initial thermite charge (kg)	80.0	50.0	50.0	50.0	50.0	50.0
Debris recovered from Surtsey						
- Debris on underside of the structure (kg)	4.95	2.31	3.91	0.92	5.49	2.11
- Debris above the structure (kg)	1.80	0.46	2.18	1.04	5.66	0.59
- Debris on bottom head (kg)	62.71	9.81	25.60	35.13	[illegible].94	20.23
Total debris recovered from Surtsey (kg)	69.46	12.58	31.69	37.08	43.08	22.93
Debris Recovered from Cavity (kg)	26.34	48.79	33.72	23.08	26.41	35.57
Total Debris Recovered (kg)	95.80	61.37	65.41	60.16	69.49	58.50
Percentage dispersed (%)	72.5	20.9	48.4	61.6	62.0	39.2

Notes: 1. The molten mass available for dispersal into the vessel is usually about 20% greater than the initial iron oxide/aluminum/chromium thermite charge due to melting of the inner wall of the crucible, vaporization of the fusible brass plug, ablation of concrete in the cavity, and oxidation of the debris.

SIGNIFICANT RESULTS OF THE LFP TESTS

- CONTAINMENT COMPARTMENTALIZATION WAS A DOMINANT MITIGATOR OF DEBRIS/GAS HEAT TRANSFER
- STEAM REACTS QUICKLY WITH METALLIC DEBRIS IN THE CAVITY TO FORM HYDROGEN

Wet Cavity (WC)
Test Series

Purpose:

to investigate the effect of water in the cavity on DCH

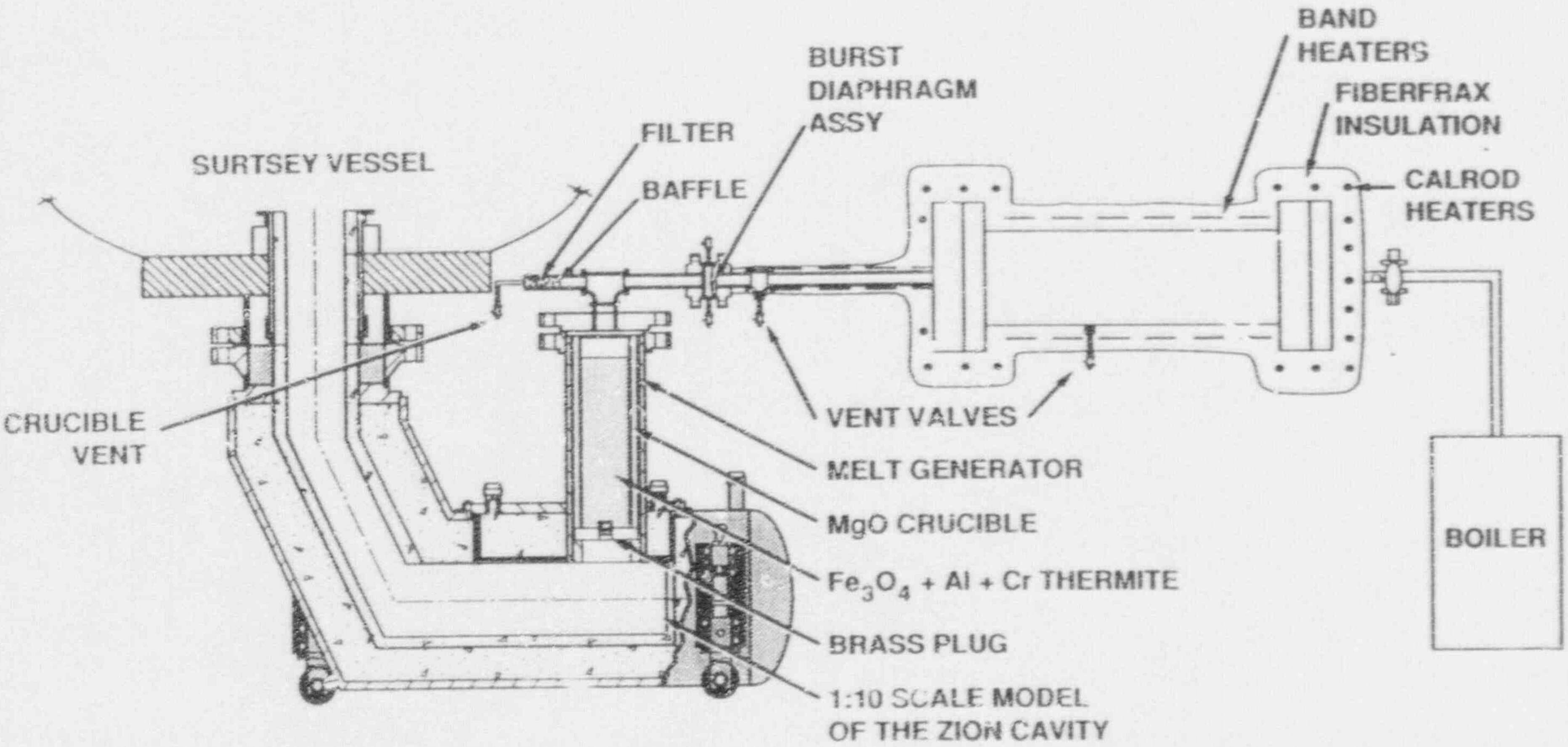
Initial conditions:

- 1/10th scale Zion cavity
- Melt mass - 50 kg
- Driving pressure - 4.5 MPa
- Initial Surtsey atm - >99.5 mol.% argon at 0.16 MPa
- Flight path - 8 meters

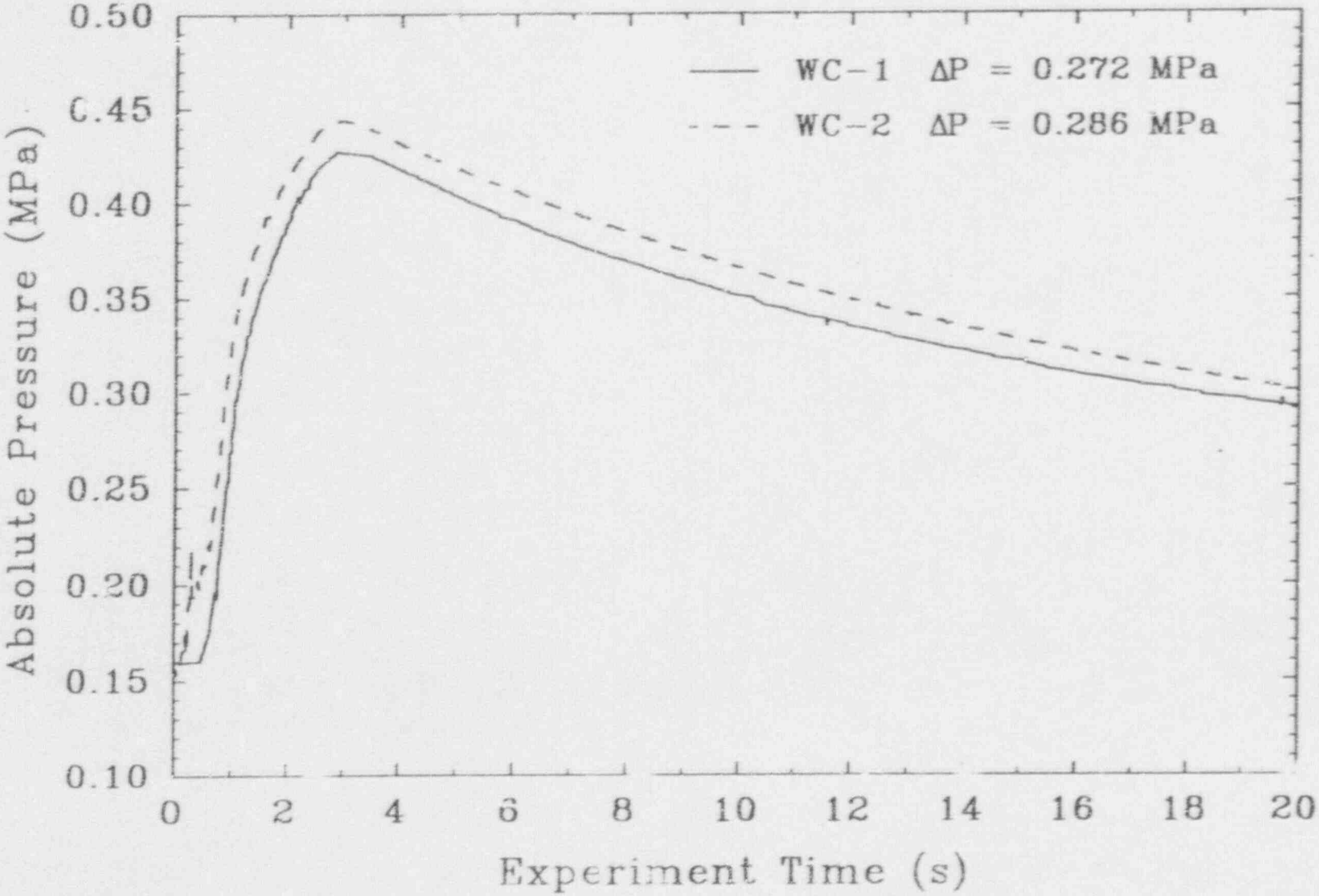
Initial conditions varied:

	Water depth	Final hole diameter
WC-1	dry	3.5 cm
WC-2	3 cm	3.5 cm
WC-3	dry	10 cm

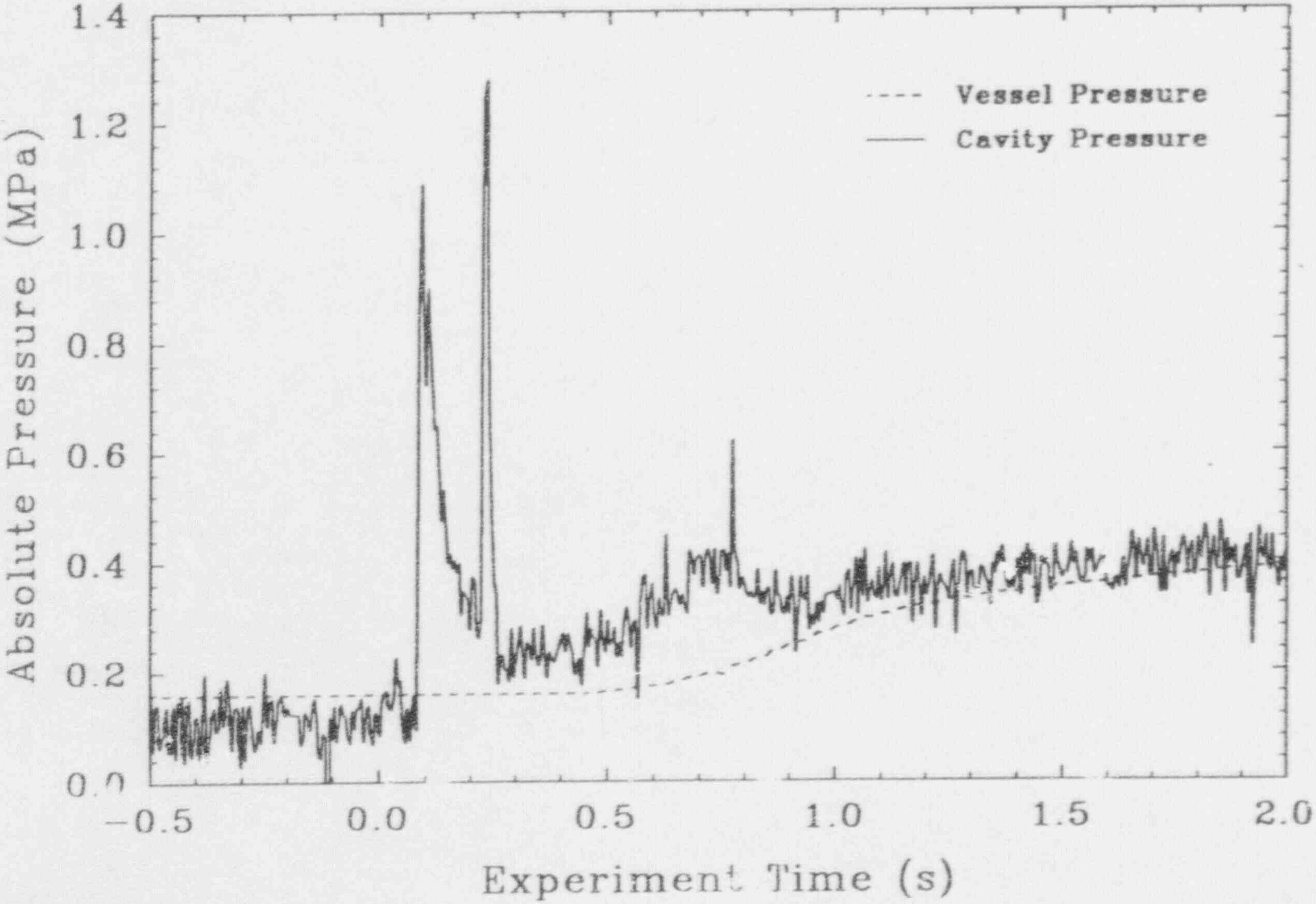
EXPERIMENT SETUP FOR SURTSEY WET CAVITY TESTS



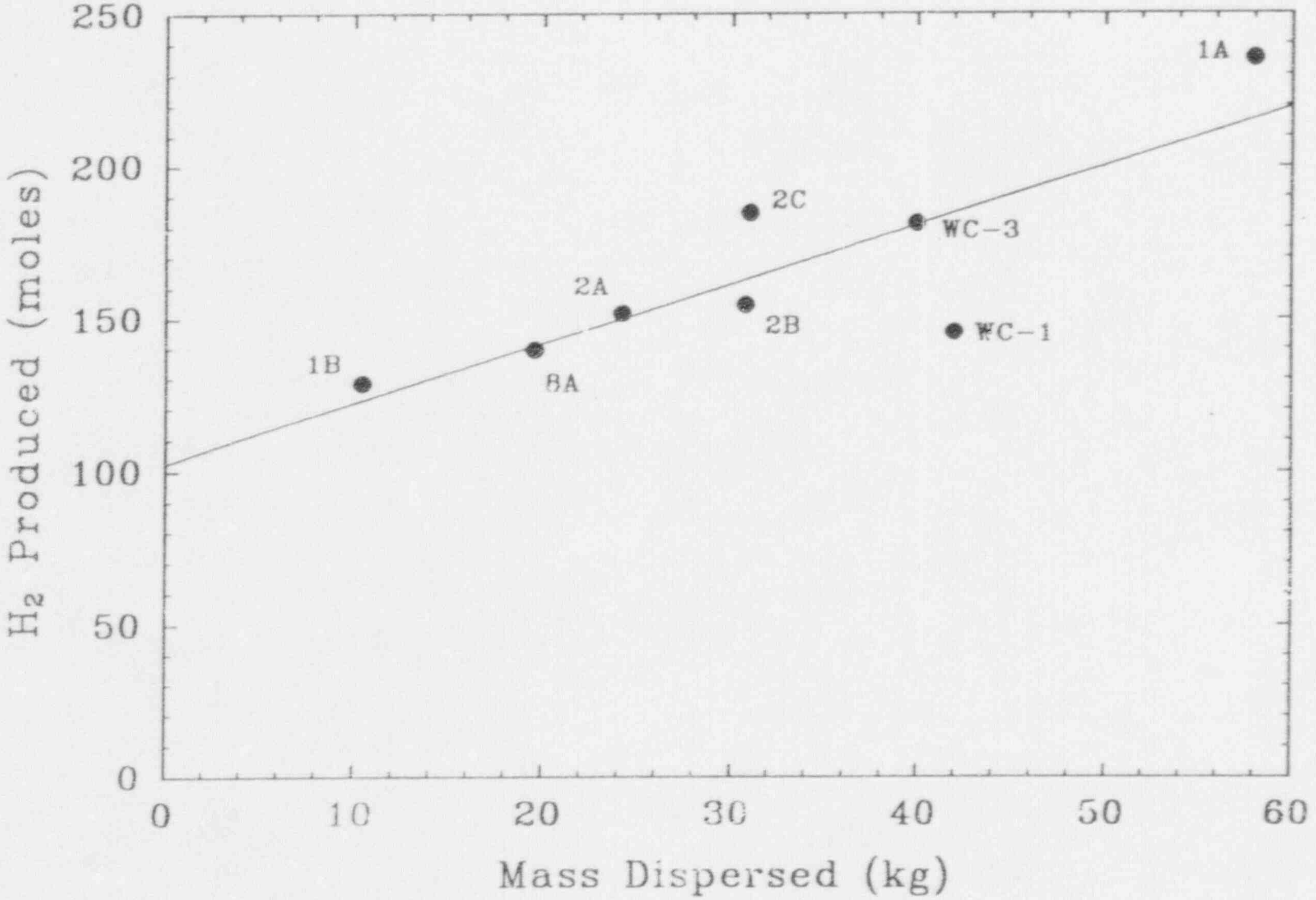
Comparison of WC-1 and WC-2 Vessel Pressure Versus Time



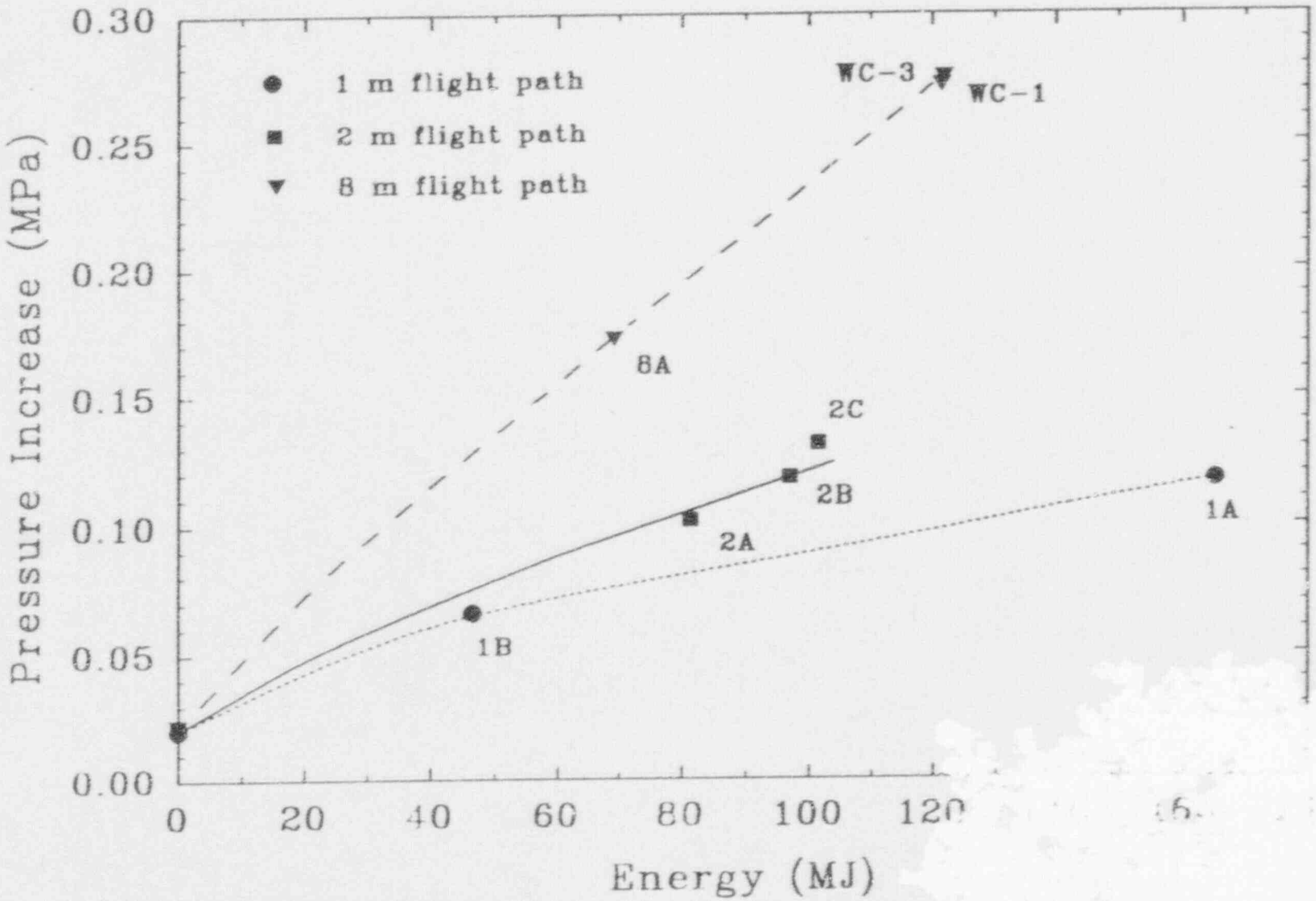
Cavity and Vessel Pressure Versus Time
for the WC-2 Experiment



Hydrogen Production Relative to Mass of Debris Dispersed



Peak Pressure Increase in Surtsey as a Function of Total Available Energy



EXPERIMENTS WITH WATER IN THE CAVITY

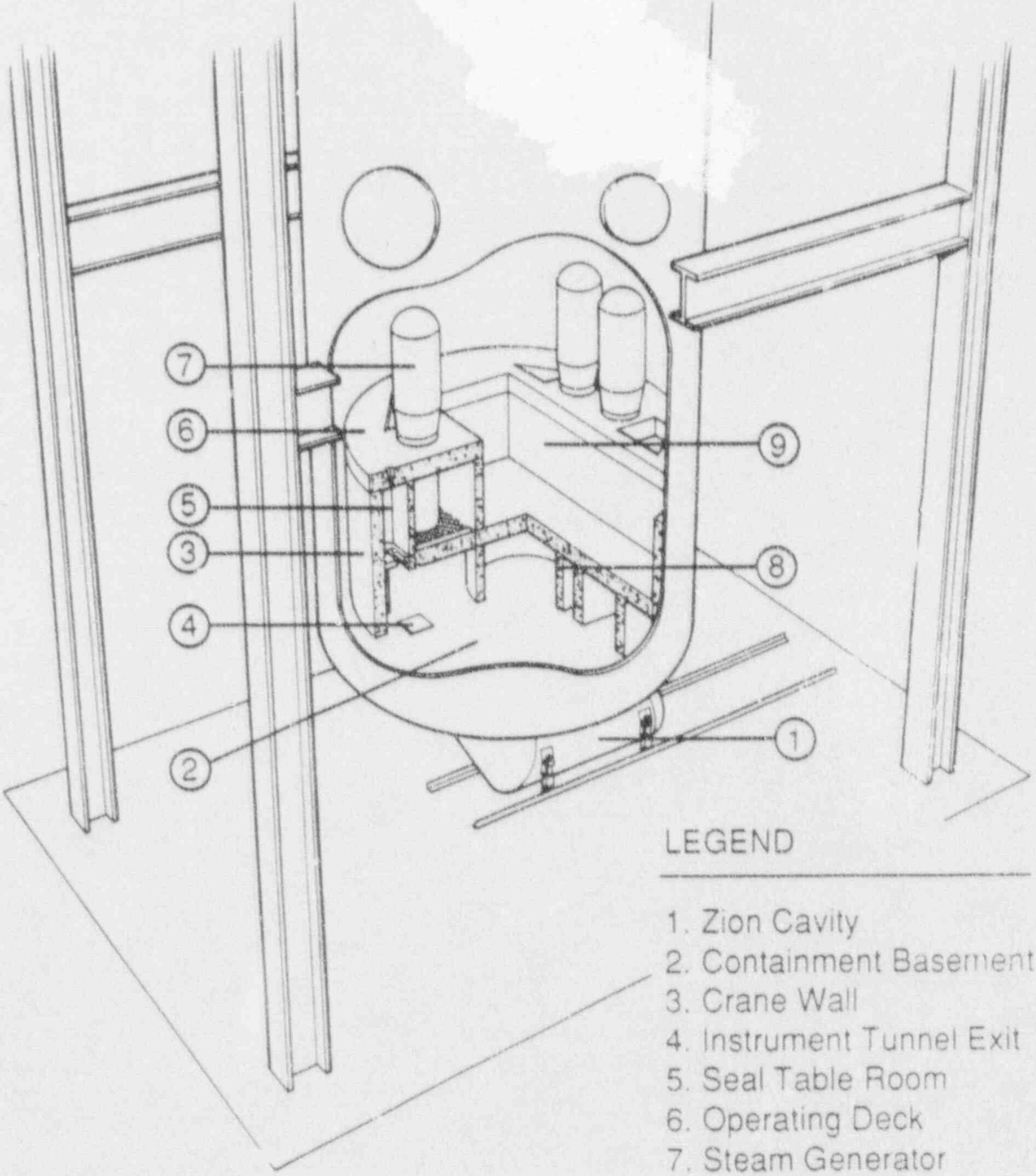
	PRESSURE INCREASE	HYDROGEN PRODUCTION	FCI
ANL	$\Delta P \uparrow$	$H_2 \uparrow$	NO
FAI	$\Delta P \uparrow$	$H_2 \uparrow$	NO
SNL/WC	$\Delta P \uparrow$	$H_2 \uparrow$	YES

SNL IET-1

PURPOSE: TO INVESTIGATE THE EFFECT OF PHYSICAL SCALE ON DCH IN A ZION-LIKE GEOMETRY

INITIAL CONDITIONS:

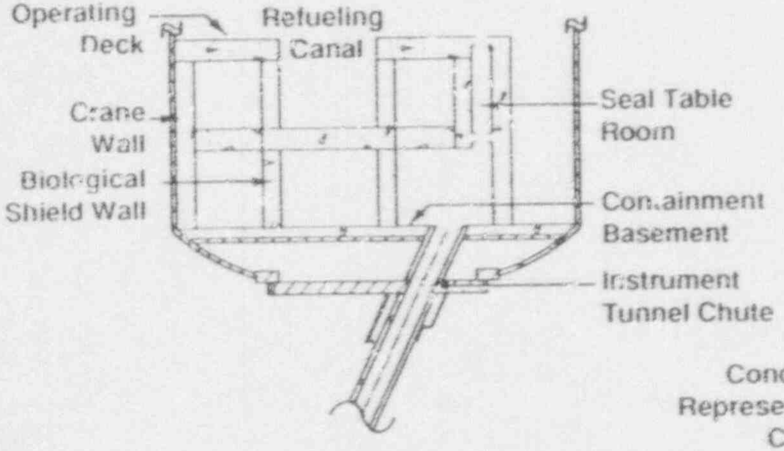
- 1/10TH SCALE ZION CAVITY AND SUBCOMPARTMENT STRUCTURES
- MELT MASS - 43 KG
- DRIVING PRESSURE - 7.1 MPA
- INITIAL SURTSEY ATM - >99.96 MOL.% NITROGEN AT 0.20 MPA
- CAVITY WATER - 3.48 KG (0.9 CM DEEP)



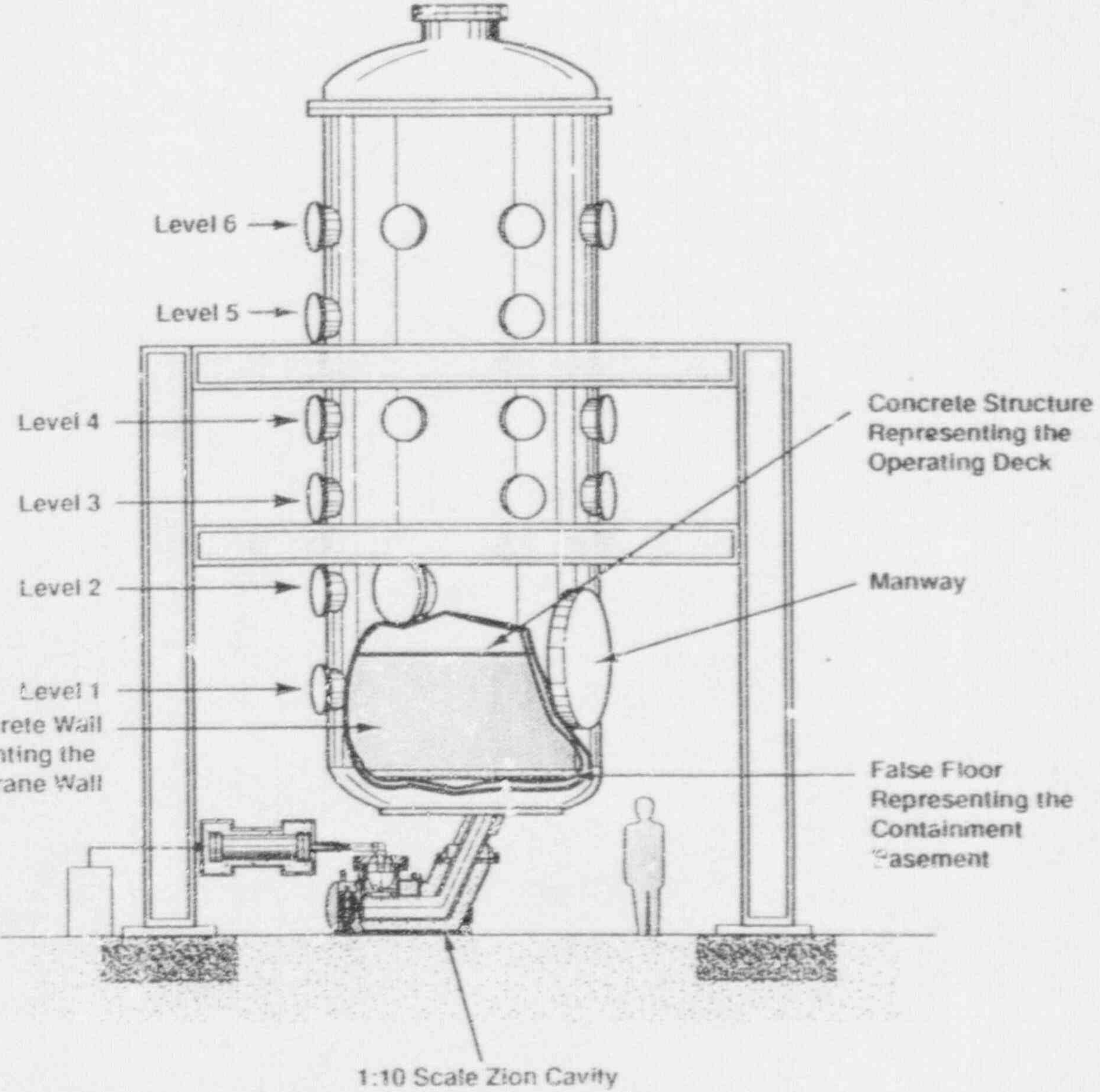
LEGEND

1. Zion Cavity
2. Containment Basement
3. Crane Wall
4. Instrument Tunnel Exit
5. Seal Table Room
6. Operating Deck
7. Steam Generator
8. Biological Shield Wall
9. Refueling Canal

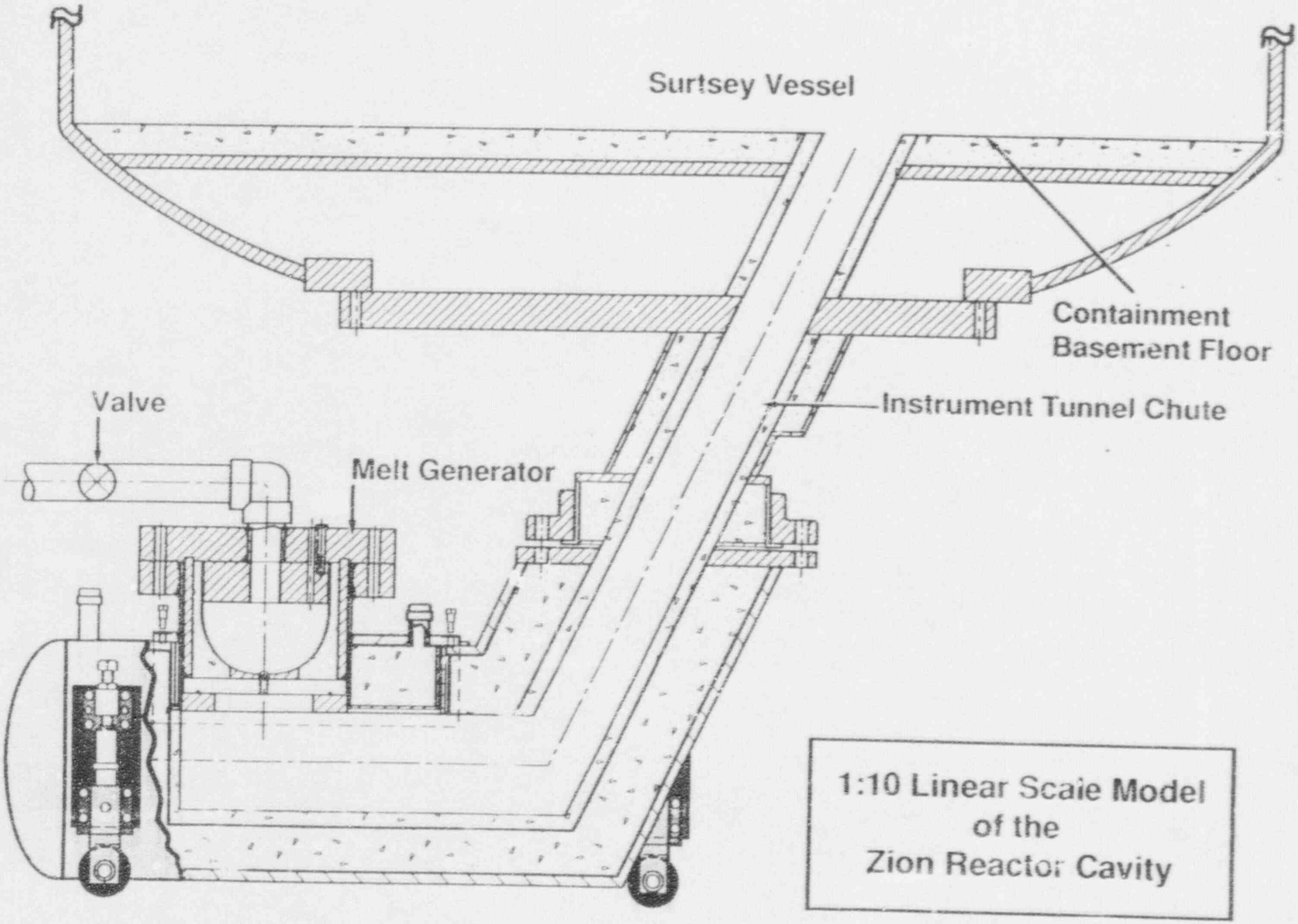
- 103 m^3 Internal Volume
- 1.0 MPa Design Pressure
- Removable Upper and Lower Heads
- Instrumentation Ports at Six Levels



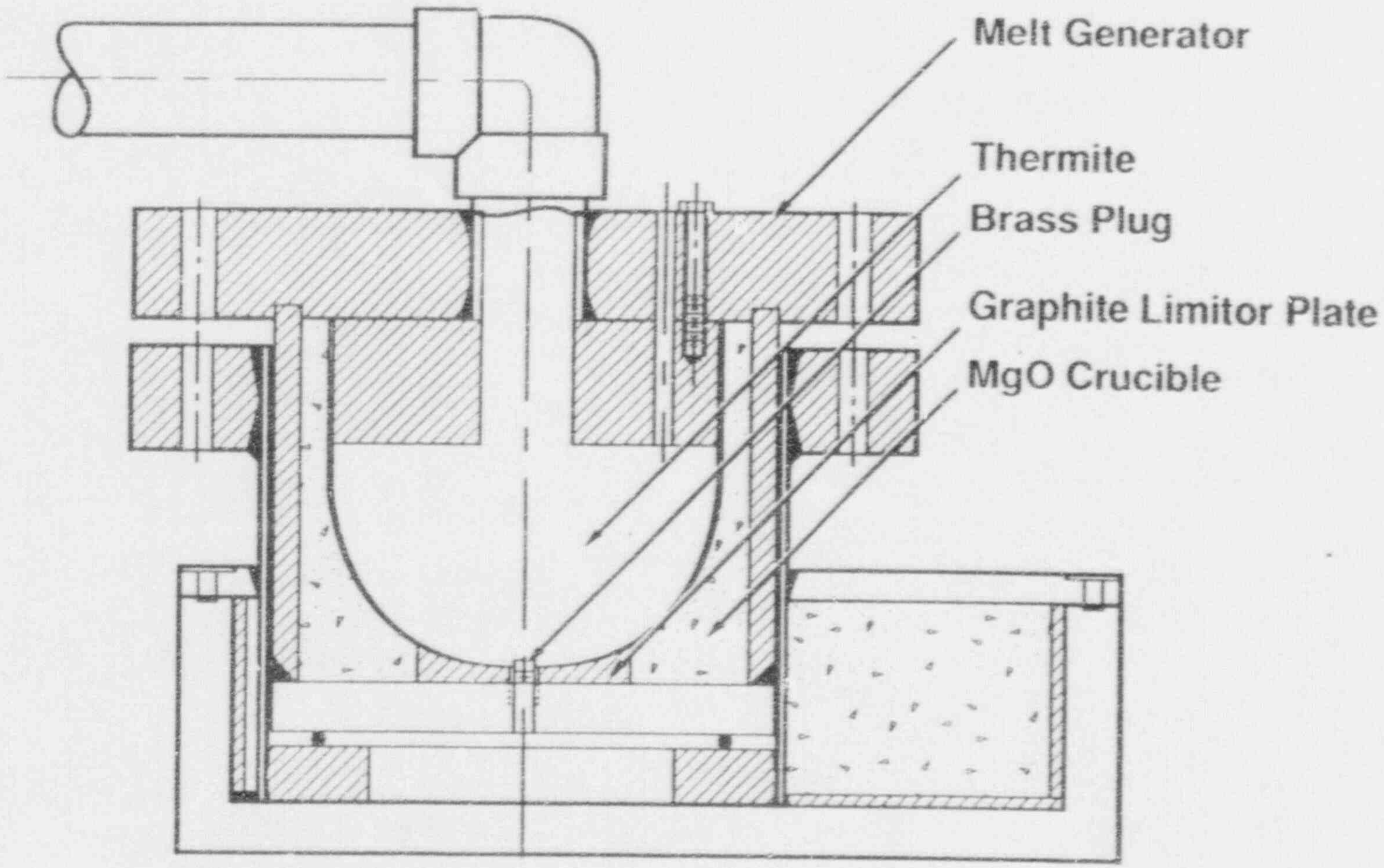
Exploded View of Subcompartment Structures



Surtsey Vessel, and HPME Delivery System and Subcompartment Structure

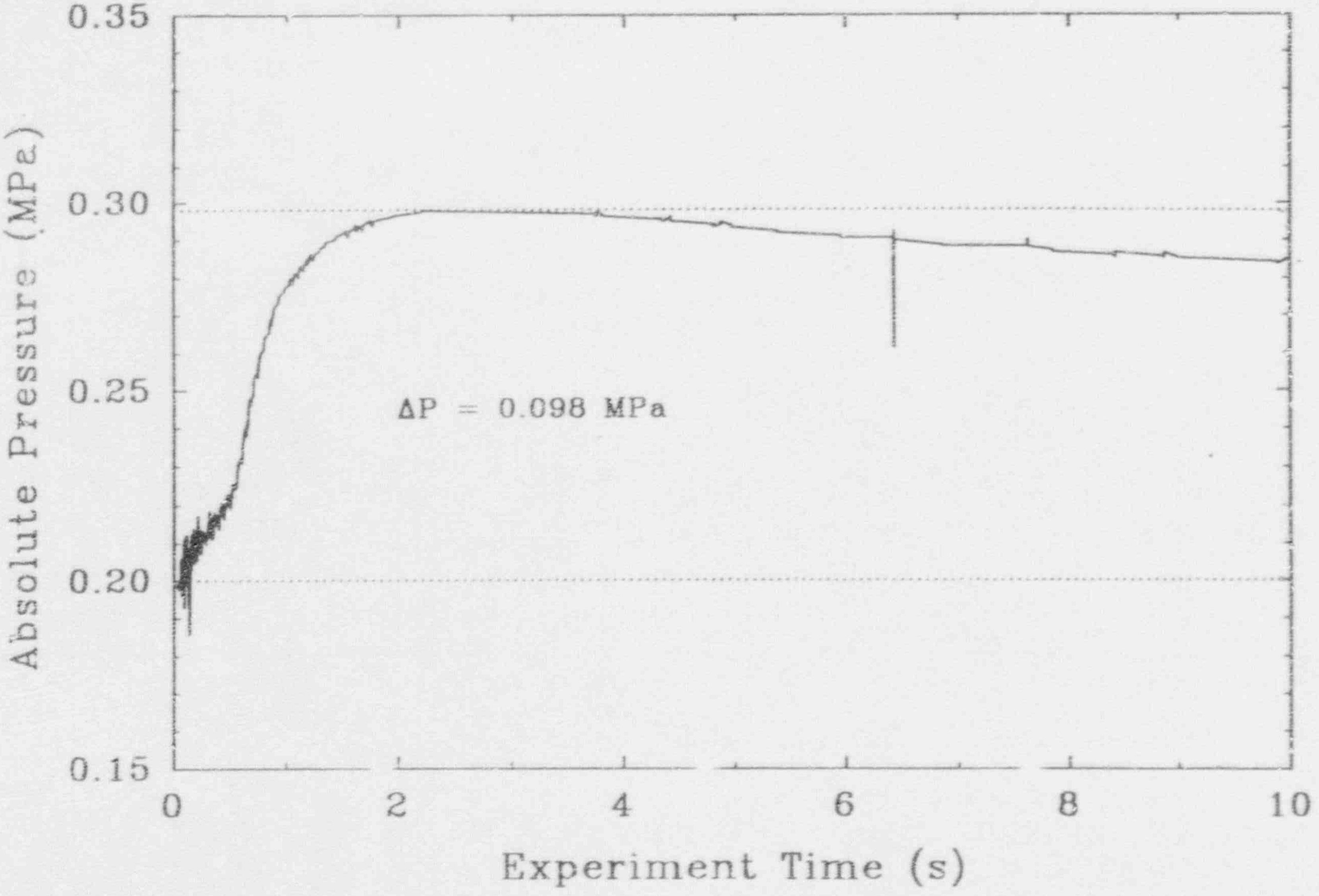


1:10 Linear Scale Model
of the
Zion Reactor Cavity

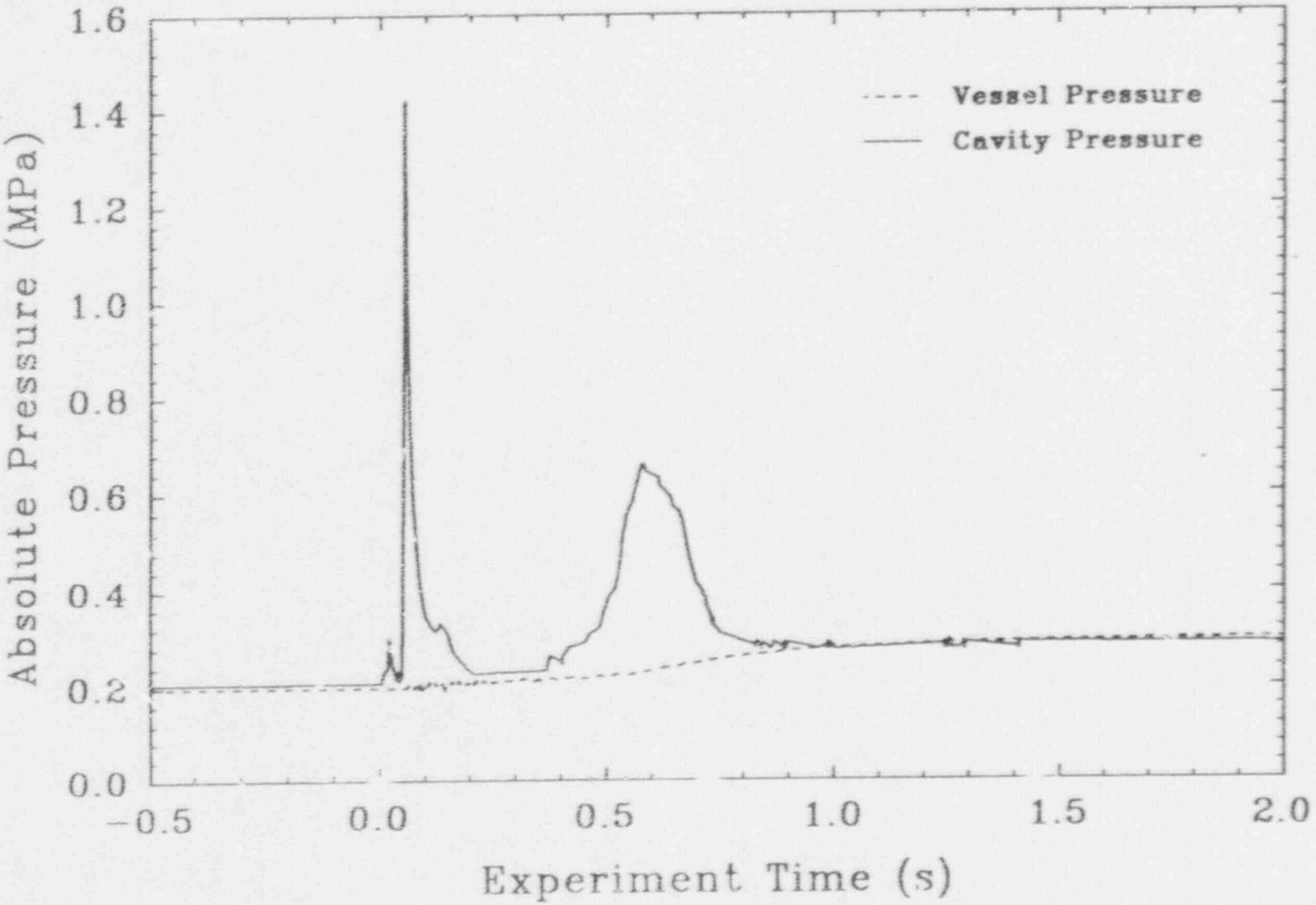


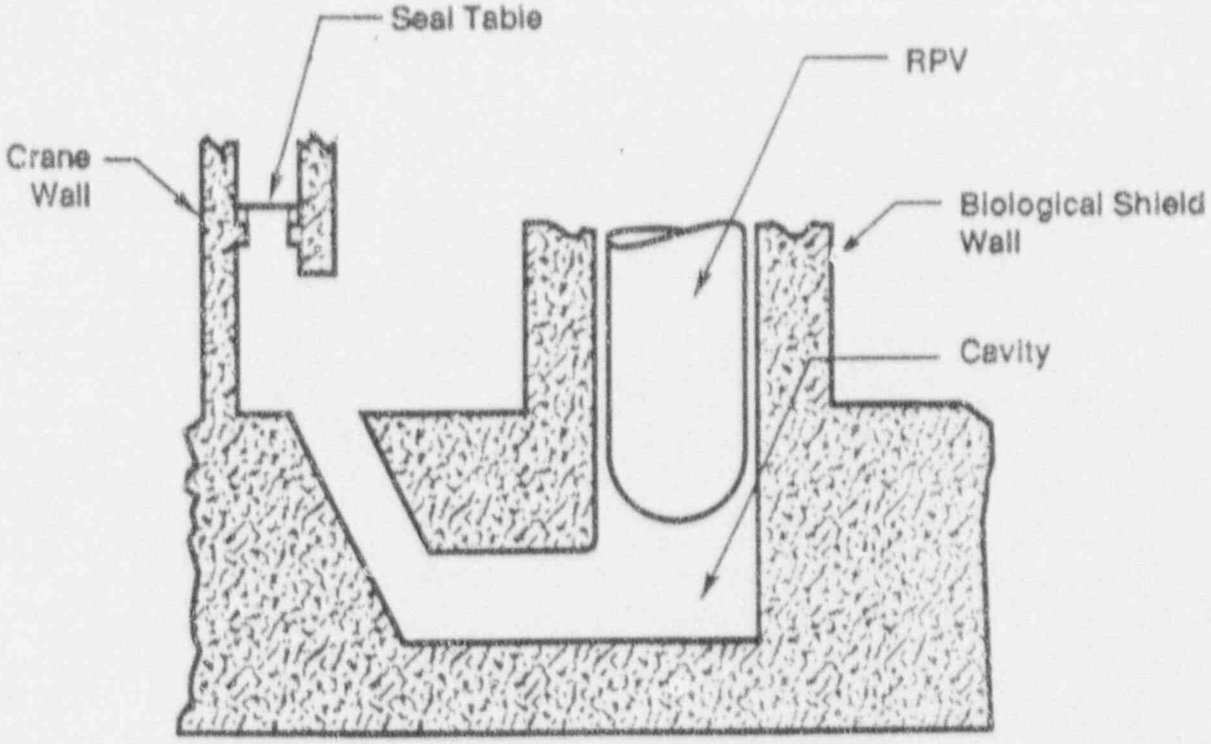
Melt Generator and MgO Crucible
Containing the Melt Simulant

Vessel Pressure Versus Time in the IET-1 Experiment

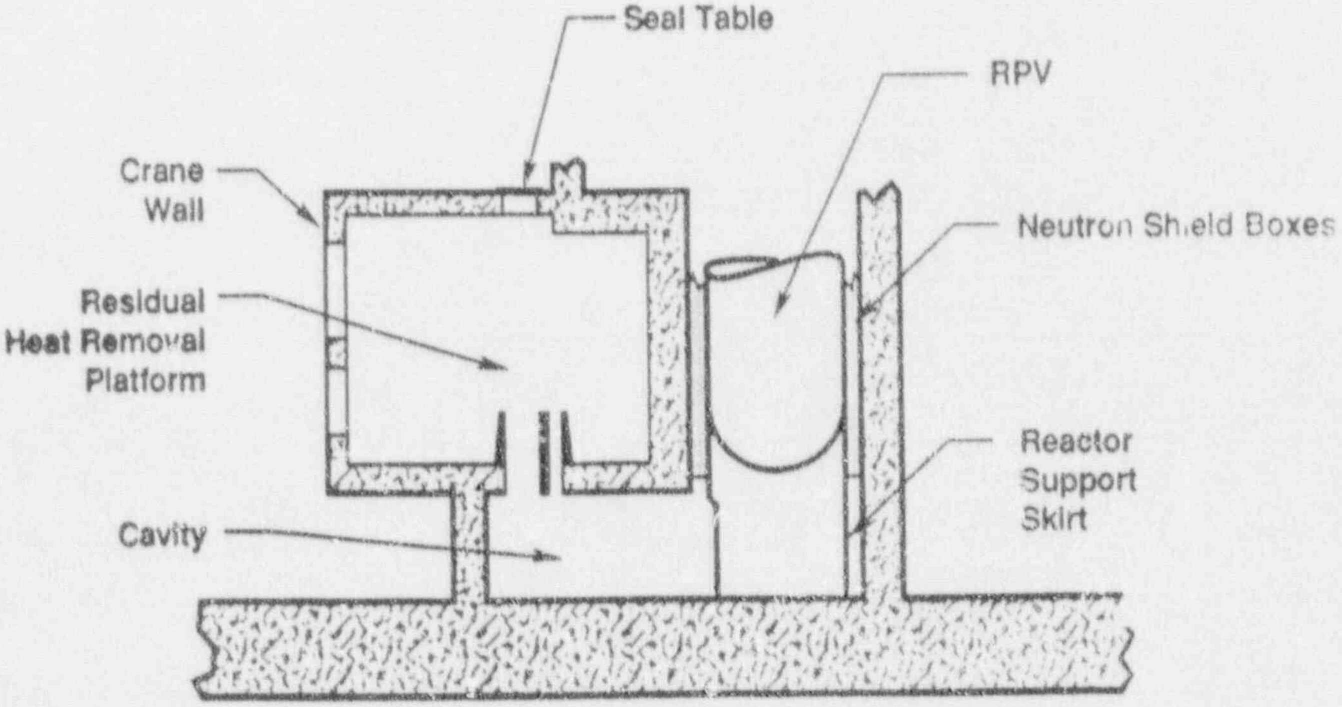


Cavity and Vessel Pressure Versus Time
for the IET-1 Experiment





Zion Cavity



Surry Cavity

FY92 PLANS

- Reconvene Design/Peer Review Group to assess results of first IET's, scaling issues, recommendations for future tests
- Complete Zion IET's
  - SURTSEY
  - CWTI
- Complete Surry IET's
  - SURTSEY
  - Containment Technology Test Facility (CTTF) 1/6 scale
- Consideration being given to conducing UO_2 tests at ANL
- Validation of analytical models

PROPOSED SNL FY92 TEST MATRIX		
Test	Date	Description
IET-2	10/91	Thermite Melt Temperature
IET-3	11/91	Zion Geometry Reactive Atmosphere
IET-4	12/91	Zion Geometry Large Hole
IET-5	2/92	Zion Geometry Containment Water
IET-6	5/92	Surry Geometry Inert Atmosphere
IET-7	6/92	Surry Geometry Prototypic Atmosphere
CIF IET-8	5/92	Surry Geometry IET-6 Counterpart
CIF IET-9	7/92	Surry Geometry IET-7 Counterpart

WILL BE PEER REVIEWED

DCH IMPLICATIONS FOR ELWRS/ALWRS

- ELWR/ALWR APPROACH IS TO REDUCE BOTH THE LIKELIHOOD AND CONSEQUENCES OF HIGH PRESSURE MELT EJECTION AND DCH
    - DEPRESSURIZATION CAPABILITY FOR PWR
    - INCORPORATION OF DEBRIS RETENTIVE CAVITY
- OTHER FACTORS
    - ELIMINATION OF RPV BOTTOM HEAD PENETRATIONS
    - MAY RESULT IN INCREASED FAILURE AREA AND AGGRAVATED HPME
- WATER FILLED CAVITY AT TIME OF VESSEL FAILURE (IF VESSEL FAILS)
    - INCREASED POTENTIAL FOR EX-VESSEL STEAM EXPLOSION

- Existing Information
  - "Retentiveness" of cavity can only be judged in a crude qualitative sense
  - Lack of experimental data relevant to range of ex-vessel FCI's
- No industry plans to experimentally confirm postulated behavior
- No NRC plans to experimentally confirm postulated behavior