UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON CLASS 9 ACCIDENTS

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Room 1046
1770 H Street, N. W.
Washington, D. C.

Wednesday, June 24, 1981

The meeting of the Subcommittee convened, pursuant to notice, at 10:10 a.m.

SUBCOMMITTEE MEMBERS PRESENT:

J. KERR, Chairman
C. SIESS
ETHERINGTON
D. W. MOELLER
P. SHEWMON
D.A. WARD

DESIGNATED FEDERAL EMPLOYEE:

QUITSCHREIBER
GRIESMEYER

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P R O C E E D I N G S

MR. SHEWMON: The meeting will come to order.

This is a meeting of the Subcommittee on Class 9 Accidents.

I am Paul Shewmon, Acting Subcommittee Chairman. The other ACRS members present today are: Harold Etherington, Dade Moeller, Dave Ward, and periodically Chet Siess.

The purpose of the meeting is to review the research budget associated with the decision unit on accident evaluation and mitigation.

The meeting is being conducted in accordance with the provisions of the Advisory Committee Act and the Government Sunshine Act. Mr. Gary Quittschreiber is the Designated Federal Employee for the meeting. Dr. Mark Griesmeyer of the ACRS staff is also in attendance right next to me.

The rules for participation at today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Monday, June 8, 1981.

A transcript is being kept and will be available as stated in the Federal Register notice. We request that each speaker identify himself or herself, and speak with sufficient clarity and volume so that he or she can be readily heard.

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

If the subcommittee has no comments or business to take care of before we start, I will call on Dr. Kelber to begin.

MR. KELBER: I am Charles Kelber. I am Deputy Director of the Division of Accident Evaluation in the Office of Research.

The Accident Evaluation and Mitigation Decision Unit is currently a major item in the budget, and it is an area where growth is proposed in the next few years. The focus of this decision unit is severe accidents, their prevention and mitigation.

Most of the work of the decision unit is carried on within the scope of the Division of Accident Evaluation, but there are small but significant elements in other division, most notably Risk Assessment.

The first sub-element, the behavior of damaged fuel, was formerly under the LOCA and other transient decision units. But as the focus changed to the prevention and mitigation of severe accidents, the work was shifted to this unit. Thus, some of the apparent growth in Fiscal Year 1982 is illusory, stemming from an accounting shift.

This sub-element is responsible for the major growth in the decision unit in Fiscal Year 1983, however, while the other sub-elements are projected to grow significantly in Fiscal Year 1984.

It should be noted that there are significant interfaces with other work. The work at LOFT and LOCA and other transient is drawn upon to set the initial conditions for the calculations and experiments performed in this decision unit. Problems are ordered, and priorities set in accordance with the findings of risk analysis as well as licensing case work.

The work is utilized in licensing case work such as the Zion and Indian Point analysis, the Sequoyah and McGuire containment response to hydrogen burning, and similar studies, and in rulemaking. The most current application is to the siting rule with regard to the definition of the source term. The work is oriented to the perceived needs of establishing a technology base for the degraded core cooling and engineered safety features rule.

We do not believe that this is the proper forum for a decision on prevention versus mitigation. It should be noted that the technology base for prevention is generally considered to be in good shape as far as accident phenomenology is concerned, while that for mitigation is thought to be rather poor. Thus, there is a major emphasis in our decision unit on aspects most directly related to mitigation, but our job is to fill in gaps in knowledge. Once those gaps are filled in, a collegial approach will successfully address the mix of prevention with mitigation.

If you want to go into the budget figures on the decision unit itself, I am under instruction to do that in a closed session. So that is at your pleasure.

The various speakers today will be able to address the programmatic areas and their budgets, but for the decision unit itself, because of the stage the matters are at, we are asked to do this in closed session.

MR. KERR: We are here to discuss numbers. So at whatever point in your presentation --

MR. KELBER: I think that this is the appropriate point.

MR. SHEWMON: How long do you expect this to be?

MR. KELBER: Five minutes.

MR. KERR: Let's use whatever procedure we need to go into closed session.

MR. SHEWMON: The closed session will be for ten minutes, so that the people here can come back in.

(Whereupon, the subcommittee went into closed session.)

MR. KERR: Mr. Cunningham, I am told that you are next up.

MR. CUNNINGHAM: My name is Mark Cunningham. I am with the Division of Risk Analysis in the Office of Research. I am here today to talk about those parts of this program within our division which relate to the phenomenology of severe accidents.

Basically, our work in this area comes as a result of three sources: One, our own desires to have work in this area of code development and application, so that we can perform risk assessments.

As you can see, we have been the sponsors and the developers of the MARCH code, the CORRAL code, and applications of these codes to risk related problems, one such program being the Reactor Safety Study and Applications Programs, and then some uncertainty analyses that go along with the code development.

A second area of work, which is within the group, has resulted in what used to be called the Improved Reactor Safety Program.

MR. KERR: Mr. Cunningham, some of these things are so obvious to us, you do not have to talk about them. I am interested, and I think the other members of the subcommittee are also, in the difference between how one gets from '82 to '83. Certainly the MARCH code and the

CORRAL code are not something that was developed in '82.

As you talk about this, I would be interested in a transition, where you go, are you continuing to do the same thing, or are there changes in direction.

MR. CUNNINGHAM: I will try to get into that as we get into the individual programs.

MR. KERR: and how these might be influenced by human beings, if they are, and that sort of thing.

MR. CUNNINGHAM: I will try to address that.

The work that we have on-going, which resulted from the Improved Reactor Safety Program, are programs at Sandia Labs on the filtered-vent containment systems program, the alternate to decay heat removal program, and a smaller program, the molten core retention device program.

The third area, or the third reason that we get into severe accident program is our work in support of the degraded core cooling rulemaking, which is coming.

MR. SHEWMON: Is the molten core retention device a new program, or is that a new name for concrete, or what?

MR. CUNNINGHAM: It is a program to make an initial judgment on the risk reduction potential associated with core catchers, different kinds of core catchers.

MR. SHEWMON: My question was whether it is a new program?

MR. CUNNINGHAM: It is about a year old. It is being used as one input into the work that we are starting, which I will talk about in later fiscal years, in the degraded core research program.

MR. KERR: Mr. Cunningham, can you give me a rough indication of what fraction of that part of the budget you refer to as severe accident mitigation you are talking about? Maybe you don't talk about budget, and you just talk about size.

MR. CUNNINGHAM: I am trying to talk about both.

MR. KERR: I have a line item that says, "Severe Accident Mitigation," and you are talking about some part of that, I think, am I right?

MR. CUNNINGHAM: Of the overall research program, it is a pretty small part of it, I would say.

MR. KERR: I am talking about a fourth section line, the total of which adds up to around $50 million, one of which is called "Severe Accident Mitigation." I think you are talking about a piece of that, or are you talking about all of it.

MR. CUNNINGHAM: I am talking about a piece of it.

MR. KERR: My question is, are you talking about 10 percent of it, 70 percent of it?

MR. CUNNINGHAM: In terms of dollars, 10 percent of that part of the budget, 10 to 20 percent.

MR. KERR: Thank you.

MR. CUNNINGHAM: I will not go into details on this, but basically we have been involved in the past in developing MARCH and CORRAL, and doing risk studies based on these, using these codes.

This shows effectively where the program is at this time. As you are well aware, MARCH is in the public domain, and we have been involved in this fiscal year mostly with follow-on work, after it is released.

MR. KERR: When you talk about follow-on efforts for MARCH users, are you talking about Fiscal '82 or Fiscal '83?

MR. CUNNINGHAM: This is Fiscal '81, then in a minute I will get to where we are going from here in Fiscal '82 and '83.

MR. KERR: Fine.

MR. CUNNINGHAM: This is intended as a background to show what we have been doing to date.

MR. SHEWMON: Is your MARCH code done at Brookhaven?

MR. CUNNINGHAM: The development was done at Battelle-Columbus.

MR. SHEWMON: I am familiar with that. Do you fund at Battelle-Columbus or Brookhaven?

MR. CUNNINGHAM: Battelle-Columbus. The work at

Brookhaven has been funded NRR.

What can be said about what we are doing in later fiscal years, '82 and '83, about MARCH is rather limited right now. It is clear that there are a great number of concerns with MARCH and we intend to pursue them. The mechanism by which we are going to do that is to go through an RFP process that we started a month or two ago. We expect this to be starting sometime in mid-Fiscal '82.

MR. KERR: Would it be accurate maybe to say that you are going to wait to see how much you get, and on the basis of that, and what needs to be done in Fiscal '82, you will do something in Fiscal '83?

MR. CUNNINGHAM: I am not sure I understand you, sir.

MR. KERR: It sound to me as though you are not quite certain as to what you will do in Fiscal '83 with the MARCH code, which is not strange necessarily, since in a sense it is still a study. So you might say, we are going to try to prove it in '82, and subject to the amount we get for additional improvements, we will try to improve it even more in 1983. I think that that is what I am hearing.

MR. CUNNINGHAM: Yes, sir, I think so. We are trying to develop a two-year program of improvements.

MR. KERR: How will you know when you will have it improved enough?

MR. CUNNINGHAM: One part of what we intend to do is to go through some uncertainty analyses with the code as it is going along. There have been identified in the past with the MARCH users, by Brookhaven, and other people that there are clearly a few areas that are of particular weakness, and we will attempt to get those first, then attempt to reevaluate how uncertain the code is after those corrections are made. At that point, we will just have to make a judgment.

MR. KERR: But you will not have finished all the needed improvements in Fiscal Year '82?

MR. CUNNINGHAM: We will not have made all the corrections that we would like in Fiscal '82, and it will go on into Fiscal '83.

MR. KERR: Thank you.

MR. CUNNINGHAM: I would like to get into a little bit of background of some of the other programs we have had, and where we are aiming.

The first program is a filtered-vent containment program at Sandia. To date, we have been working on two particular types of plants. First, the program was involved in and contributed to the Zion and Indian Point concerns that arose in NRR. Since the completion of that, we have been working on a BWR Mark I.

For the rest of this fiscal year and into Fiscal

'82, we are continuing to work on an ice condenser plant, a BWR Mark III design, and a large dry containment design. As that work is completed in Fiscal '82, it will be merged into the degraded core cooling rulemaking research support program that I will talking about in a minute, where we are developing somewhat of an umbrella program, which will encompass the work in this program and other programs in Fiscal '82 and Fiscal '83. I will get into that now.

The second program that we have had ongoing for the last couple of years has been the alternate decay heat removal systems program. To date we have gotten to the point where we have defined what the range of existing systems is throughout the world, and developed some options for study when one goes into the study of risk reduction achieved by such a system.

In '82 and '83, it is my understanding that we will be bringing the this also under the degraded core research program.

MR. KERR: What goal does one have in this program?

MR. CUNNINGHAM: Really, an identification of what the potential risk reduction is associated with the particular kind of decay heat removal system and the costs associated with it also. It is a value impact of sorts, an add-on on a decay heat removal system for particular kinds of plants.

MR. KERR: You will have a spectum on results which will say, System A it will reduce risk by this much, and System D by this much, and it will cost this much.

MR. CUNNINGHAM: Basically, yes.

MR. KERR: I assume that this will vary somewhat from plant to plant?

MR. CUNNINGHAM: It can, very much so, yes. We are going to be looking at different kinds of basic designs of plants, PWRs, BWRs, different containment types, and whatever.

MR. WARD: Are these backfitting concepts or new concepts, or both?

MR. CUNNINGHAM: They could be either.

MR. WARD: How does the program here fit in with the task action plan on the A-48, or whatever, design, or A-45?

MR. CUNNINGHAM: I am not sure what A-45 is.

MR. WARD: Improving decay heat removal systems.

MR. CUNNINGHAM: I am not quite sure of the link. I am sure that this work is being fit it, but I don't know that it is specifically being -- It is not specifically part of the A-45 work.

MR. KERR: How can we get an answer to Mr. Ward's question, and to whom should we address the question?

MR. CUNNINGHAM: One of the people in my division who is responsible for this particular program, Matt Taylor.

MR. KERR: Could you find out for us what the relationship is between A-45 and this work?

MR. CUNNINGHAM: I will do that.

MR. KELBER: For this particular task, I would agree that we can get the answer from Matt Taylor and report it to you later today.

In general, the various items such as A-45, A-44, and so on, all have representatives from the various offices that are doing work on the problem. This work is sometimes tied directly to the task action plan, other times the results are fit in as they arrive. There is no general rule.

MR. KERR: I recognize that.

MR. KELBER: For example, I believe it was Station Black A-44, as I recall correctly, and to SASSO work on Station Black was used as a basis for A-44.

MR. CUNNINGHAM: This is the last of the programs that we have, which relate to the phenomenology of severe accidents. Again, it is a program at Sandia to make an initial evaluation of the potential risk reduction value, and some initial costings on molten core retention devices of different types.

As it sits now, we have completed the risk reduction evaluation. The subsequent work is going to be merged into the degraded core rulemaking support program, which I will talk about just now.

Our program, which we just started, on the degraded core cooling rulemaking, the research support program, is an attempt to bring these various programs together, so that one can make a consistent set of analyses of the risk reduction potential and costs associated with a fairly broad spectrum of prevention and mitigation devices.

These are the options that we intend to look at in the program. As you can see, the vented containment fits in, the decay heat removal systems fit in, core catchers fit, and what-have-you.

We are interested in looking at these options both individually and certain combinations of them, so that one might have a combination of retention devices and a hydrogen control system, or something like that.

MR. KERR: When you say that you are going to look at those, can you give us some idea of what you conceive as the scope of that program in terms of dollars a year, and the number of years, or is that coming later?

MR. CUNNINGHAM: That will come in just a second.

Again, this is what we intend to do to attempt to define options, conceptual designs of some of the options, and analyze what risk reduction value could be associated with the various options.

Parallel, we will be looking at the cost of such options, and then combining them into a value impact survey. Our intent here is to make it somewhat iterative, so there will be a phase one that will do this on a fairly semi-quantitative basis, to narrow the list somewhat to the more promising options, and to allow us to fold in additional research results that will come into play over the next year or so.

This is a schedule for the program. It really it has just begun over the last week or two. The initial work will be traveling. The first phase of the work will be going through in March 1982, and then the second iteration, we expect, will go into the third quarter of Fiscal '83.

The funding levels, our assumptions for Fiscal '82 and Fiscal '83, are about $1 million a year.

MR. KERR: What sort of liaison exists between this activity and that of the industry responsible for the so-called "In Core Program"?

MR. CUNNINGHAM: I know management has been talking to the In Core Program, to Dr. Buell and Dr. Fontana. We intend to make them well aware of what we are doing here. I believe the understanding is that they are pretty much deferring to us in this program. They are not going to be spending a great deal of time and money looking at prevention and mitigation options because this program is in place.

MR. KERR: They are not going to be spending a lot of time looking at mitigation and prevention options; could you elaborate on that and tell me a little more of what you mean by that?

MR. CUNNINGHAM: I don't know whether you have seen the set of tasks that it has set forth.

MR. KERR: I have, and that is why I asked the question.

MR. CUNNINGHAM: A lot of it is analytical work, looking at particular pieces of the severe accident sequence. They are spending some time looking at the phenomena within the vessel, trying to improve somewhat on the codes, or replace MARCH and CORRAL, for example, with something of their own.

I don't think they intend to carry it to the point that they are going to be using these in specific studies, particularly mitigation features, particular prevention features. It is my understanding, at least, that they were going to pretty much leave that to Research.

MR. KERR: Thank you.

MR. CUNNINGHAM: There is one thing that I forgot to mention as we were going through, and the only other --

MR. KERR: Is there some formal or informal mechanism which permits the people or assures that carry on this program know what In Core is doing?

It would seem to me that there is an opportunity that you learn from each other. You may not be doing exactly the same thing.

MR. CUNNINGHAM: Up to now, I believe, the exchange of information has been fairly informal. We are very interested in clarifying that, and making sure that the groups talk to each other.

MR. KERR: So the exchange of information has been effective, as contrasted to being formal or informal.

MR. CUNNINGHAM: I think that it has been fairly effective so far. I think that we might make it a little more formal.

MR. KERR: I. it one of your responsibilities to see that that exchange is effective, or is that somewhere out there?

MR. CUNNINGHAM: If it fits within my responsibilities, it has not been told to me. The formalization of the discussions between In-Core in our division is just in its initial stages right now.

The one thing I didn't mention as I went through, the only other piece of work that we intend to go on to in the next few fiscal years in this area is the improvements to MARCH. I would expect that this will run at a level of four to five hundred thousand dollars a year over the next few years.

MR. KERR: Who is responsible for deciding what it is that one wants MARCH to do? Clearly, MARCH was not originally intended to do what people are now trying to do with it. Improvement, just improvements in the original code intended to do its original task, but a change in an existing code to enable it to do perhaps better what it originally did, but also to enable it to do other things as well.

Who is it that decides what it is that one wants MARCH to do, what group?

MR. CUNNINGHAM: It has been fairly informal actually. I have been involved in the discussions. Jim Meyer from NRR has been involved in the discussions. Some of the people in Dr. Kelber's division have been involved in the discussions.

MR. KERR: Where would we find written in some fashion some idea of what people think MARCH should ultimately be able in an improved MARCH code program?

MR. CUNNINGHAM: I don't think there is really anything written yet.

MR. KERR: When you go out for an RFP, you are going to have to tell people what it is that you want the code to do.

MR. CUNNINGHAM: Yes.

MR. KERR: These RFPs have not been written yet?

MR. CUNNINGHAM: They have not been formalized yet.

MR. KERR: That is for Fiscal '82?

MR. CUNNINGHAM: That is correct.

MR. KERR: Thank you.

MR. CUNNINGHAM: That is all I have, sir.

MR. KERR: Are there questions?

(No response.)

MR. KERR: Thank you, sir.

My agenda shows that Mr. Silberberg is next, and then Mr. Picklesimer.

Let me indicate at this point that I plan to recess this meeting at about 11:25 in order that we may observe the swearing in of the new Commissioner.

MR. SHEWMON: The swearing of the next Chairman.

MR. KERR: Is it being sworn in as the Chairman, or as a Commissioner; I want to be complete accurate about this. Anyway, the swearing in of somebody for something. I don't know how long that will take, but I would judge 20 to 25 minutes. We will reassemble immediately after the formal ceremony has ended.

You are going to talk, according to the agenda, about the behavior of damaged fuel, which is some piece of a line item called damaged fuel, I presume, in the budget process.

MR. SILBERBERG: Yes.

MR. KERR: Can you give me some idea of what piece it is you are going to talk about; is it 10 percent of that item, or 50 percent of that item?

MR. SILBERBERG: All of it.

My name is Mel Silbergerg, and I am Chief of the Fuel Behavior Branch in the Office of Research, Division of Accident Evaluation.

I believe Dr. Kelber this morning mentioned to you that our work on behavior of damaged fuel, as we refer to it, our severe fuel damage program has undergone a rather extensive review with a special task force to look at the entire program, its component parts, what its focus is, and how it relates to the needs within the NRC. That report will be available, at least we will be able to discuss the essence of it at the meeting on July 7th that we are going to have Dr. Shewmon's committee. We would hope by that time that we would even have a draft available for you fairly close to being final.

Let me today just take you through the overall objectives of the program, and an overview of the related information needs, informatin related to the program, and a very brief outline of the major elements and components of our severe fuel damage program.

Mr. Picklesimer will be arriving shortly, and he will then go on from there and give you more detail on the experimental phases of the experimental aspects of the program, and the in-pile and out-of-pile relationship to foreign programs.

As we now see the objective or the severe fuel damage program is on the first vugraph. What we are doing is developing an int grated program that will provide a data base and analytical methodology for understanding and predicting core behavior under severe accident sequence conditions within the vessel.

In other words, the focus of the program is to provide that information which will allow one, working with other phases of the research program, to do three things:

(1) The information needed to terminate the accident in vessel;

(2) Information related to accident management; and

(3) The bottom line, how does one integrate all of this research, although not that we will do the integrating, to allow one to prevent a global core melt. In other words, to be able to terminate the accident within the vessel.

A key part of the informtion needed there must come from the severe fuel damage program.

MR. WARD: Mel, you are saying that you are not going to be looking at how fuel melting behaves with regard to core catcher?

MR. SILBERBERG: Dr. Curtis, who is handling that, will be discussing that this afternoon.

I think that we are interested in the condition of the fuel as it leaves the vessel, if indeed one has given up on the sequence at that point. But having then left the vessel, and the accident management phase, if you will, moves to the containment integrity, then the focus of the research changes, and that is the difference that Dr. Curtis will be addressing.

Let's go to the next slide and look at what we call related information needs. They, if you will, are a subset of three products that are noted on the first slide. We feel that the data and the models that will be evolving from this program are needed for degraded core cooling, and mininum engineered safety feature aspects rulemaking, particularly for rule implementation.

After rulemaking, if you will, is done, how does one implement these criteria, how does one confirm that, indeed, the criteria are satisfactory.

The program plays a role in relation to the accident management procedures, planning and operations, including man-machine interfaces that relate to being able to terminate the accident.

The type of information that one needs:

-- What are the core conditions needed to terminate by simple reflood

-- What are the coolant flows that are needed for termination, stable termination, that is that the temperature is stable, location, and movement of the fuel and its geometry are stable.

-- Are the conditions where refloods worsens the accident? This is one item that we spent some time discussing. Namely, there can be late in a severe accident sequence, perhaps bringing coolant in. If one is convinced of it, and one has evidence for it, that bringing it in too late could aggravate the situation, getting into a steam explosion, or what-have-you, that makes management more difficult in the later sequences of the accident.

Concomittant with the accident management situation, and related again to the MESF will be the question, is the current ECCS adequate, is the design adequate for handling severely damaged core. The answer is, it may be perfectly fine to do that to meet the criteria above in terms of coolant loads and things like that. But if it is not, then this will require some improvement in design.

I mean to note here that there will be others involved in this. But the information from this program provides an important basis for making those assessments.

MR. SHEWMON: Mel, to go back to the last item in that condition where reflood worsens accident, is it put in there to cover all possible contingencies, or do you really think that there are times when the core is still in the vessel, when you would rather turn off pumps and not put water in?

MR. SILBERBERG: That was the intent of my comment. What the details are at this point, I think certainly Dr. Kelber can comment on that.

MR. KELBER: I think at this stage the discussion is largely based on what might be termed informed speculation. But the question is the rate at which one introduces cooling water in severely over-heated core. One may be better off, the speculation goes, introducing water slowly into the lower portion of the core, using steam cooling at first, then accelerating the rate of flooding.

MR. SHEWMON: Then you won't be inhibited by Appendix K.

MR. KELBER: If we are going to do that, we don't need to do it in research.

MR. SILBERBERG: That is a good point.

MR. KERR: Mel, help me a little. I heard what you said and what Mr. Kelber has said. Much of this part is driven by a need for information associated with rulemaking. How do you tell what information is going to be needed other than, as Charlie said, informed speculation.

For example, have you taken the questions that were raised in the notice from the rulemaking, or something like that, that said, we need answers to these questions? How do you get the spectrum of information that you are looking for, from what source?

MR. SILBERBERG: At the first level, we are looking at the --

MR. KELBER: Can I answer pa. t of that?

That was the function of Degraded Core Cooling Steering Group.

MR. KERR: Now that we have that o.. of the way, how do you tell what information you need?

MR. KELBER: The steering group did review the scope of the various rules and the various information needs. Quite frankly, it did not complete the task that I had personally hoped it would, which is to address the relationship of the information needs to the research program that we had suggested.

That function has now been transferred back to the Office of Research. Basically, the steering group associated with the rules had a very broad scope and objectives, and I think almost any research can be related to that scope and objectives so that we have a very difficult in-house problem.

It will get better in the coming years, as the Sandia support work that Mark Cunningham referred to starts to bear fruit, that will help us prioritize the work.

I must say that at the present time the relationship is based largely on what we call informed speculation. It is addressed in somewhat more detail in other areas and this particular area, as Mel is about to tell you, but I must confess that there has been no really intensive effort outside the office of research, to review the relationship of the research program to the rules. In fact, there has been no really intensive discussion at any high level of what the scope of the rules ought to be.

MR. SILBERBERG: Let me suggest that the very question that you just raised, Dr. Kerr, did provide some focus for our discussion on the task force, the so-called Severe Fuel Damage Task force. We grappled with it at some length, and we could get some broad needs. But we came to the conclusion, and I think a very strong conclusion that you will find in the report, that in effect says, in order to allow one to have this program pay off and have it do the kinds of things to be sure, I think there is an awful lot of information that we need.

I am not concerned about in terms of its immediate application today, but what one needs to see is the entire process of how one goes from that point through the middle and later stages of the program, so that when you are done, you come out with the information that is useful, and is of utility in rulemaking as well as implementation of the rule.

We have recommended strongly that this focusing and the interfacing of the elements with this program and others that are needed, be done very early by management. In other words, we are making a recommendation to the management that in effect gets to your point.

MR. KERR: Thank you.

MR. SILBERBERG: I see that Dr. Picklesimer has arrived, and I feel a lot better knowing that I will soon be off.

You will be hearing a good portion of what I have here listed as Scope. What you have in front of you now is the scope of the program. These are the principal elements, but taken together they represent an integrated program with the principal components shown here.

These components are as follows: We have what we call integral in-pile tests which are basically the PBF test, and other tests that are being planned by the European community. This is, if you will, the major portion of the program.

When I say, integral, I mean integral in terms of integrating overall behavior and governing phenomena, not just looking at a specific effect, but where multiple phenomena are being looked at, and then being used to either scope behavior or later on verify more advanced analytical understanding and modeling of the situation.

Part and parcel with that, and closely coupled, are what we call the separate-effect phenomenological experiments which interact with that, and also interact with the model development where you can, at lower cost, look at special effects in the in-pile, like the ACRR program being planned, as well as out-of-pile and laboratory experiments where you can, in effect, cover a lot of things that you would not be covering in the PBF.

Our major analytical effort centers on SCDAP, which is severe core damage analysis package. We will hear more about that on the 7th of July.

MR. KERR: Is there any relationship between that and some part of MARCH?

MR. SILBERBERG: With severe accident SCDAP, We ultimately interface with BA, a portion of MARCH, or some other larger assessment systems code. In other words, where MARCH is now making certain assumptions that say, as I reach this temperature, everything goes through, if you will, and we go to the next phase. This package would allow one to handle it.

George Marino is here. Do you want to add something to that?

MR. MARINO: I can expand a little bit on it. I don't have a detailed presentation. It will be a bundle size that later on, we think -- We are trying to keep the first version of the code --

MR. KERR: I just wondered if you had in mind this as a replacement for some part of MARCH, or is this a separate development which is aimed at bringing out the reactor before it starts melting.

MR. MARINO: It is really a replacement to give us more accuracy. It could be used later on, and it would be quite simple.

MR. SHEWMON: One other question on that. Is there water in the sub-assembly, or is it dry?

MR. MARINO: It will be handle it with the use of a sub-code with a hydraulics package that will be able to handle reflood and refill, boil down, all kinds of things.

MR. SHEWMON: Will it also handle the dry steam assembly?

MR. MARINO: Yes.

MR. KERR: Gentlemen, I don't know what the schedule for the swearing is, but I assume that it is going to start on time. I think that we had better recess.

MR. SHEWMON: All the good seats will be gone.

MR. SILBERBERG: I have a statement, and I would like to come back and make that statement.

MR. KERR: That will be fine.

(Short recess was taken.)

MR. KERR: Let's get going again. Mr. Silberberg had a set of concluding remarks he wished to make.

MR. SILBERBERG: Thank you. The last item on this last viewgraph on scope is something I wanted to say a few words about because I believe it is important. This has to do with the analysis and characterization of the TMI-2 core debris from the TMI-2 core examination. And all of our reviews of the program and all of the work previously that has been done by the people working in the Fuel Behavior Branch prior to my coming onboard has made it very clear.

The information one could get from this examination is of very high importance and potentially high importance and potentially high impact on everything we do in this area. Because first of all, it represents, good or bad, the only whole core experience that we have at this point in severe fuel damage, and hopefully, maybe it will be the only one. But that is all there is. And we will be recommending strongly to our management that somehow we get this particular job off center, and somehow give it more attention nationally.

We think it has got to be removed from the nominal political concerns and what have you that surround it now, and look at it as an important piece of technical information needed in future regulatory research activities and other activities in our program. So I am just highlighting the importance of it.

MR. SHEWMON: When you say getting it off dead center, are you talking about getting on with the cleanup of TMI-2 in general, or questions about what they would do if the core -- .

MR. SILBERBERG: Proceeding in such a way as to get it to the examination; whatever it takes to get it to the examination.

MR. SHEWMON: I am still confused. There's a question of cleaning up TMI-2 so you can get at it. And then there is the question of what people have other plans, or in the rush of things, just want to throw it in a big container and ship it off somewhere.

What your concern is precisely from your provincial -- .

MR. SILBERBERG: The concern is to the latter; that when they do get to that it is done right, and we have the best technology.

MR. SHEWMON: DOE also has an interest, don't they?

MR. SILBERBERG: Yes. A member of our task force is from DOE and he served as a consultant in this area to provide the latest plans; where they were heading, and a lot of this is summarized or will be summarized in a task force report.

MR. SHEWMON: I believe many of us share your concern. Are there others who do not, or do you just want to keep it in front of people?

MR. SILBERBERG: We want to keep it in front of people as loud as we can.

MR. KERR: What fraction of your severe core damage budget for 1983 is being allocated for this?

MR. SILBERBERG: A small fraction, on the order of -- Dr. Picklesimer?

MR. PICKLESIMER: We have scheduled for the TMI-2 core exam about $300,000 dollars for 1983. This is to set up the analytical techniques for analyzing specimens at Argonne. It is not for the removal of the core itself or the in situ examination.

MR. KERR: Does that conclude your remarks?

MR. SILBERBERG: I want to note again the item about the relationship of the work to the rulemaking and the need to integrate these things. We think that is important. I also want to leave with you the important point that the program at this point is at ground zero. There is little or no information in this area.

There is a lot of work we need to do to get the program up to speed to get into the analytical phases; the experimental phases of just being able to get information as we start to focus it later. So this phase is important; independent, if you will, for the moment of the focus.

MR. KERR: Okay. Whose move is it? Is it your move or somebody else's move?

MR. SILBERBERG: To go to the next phase?

MR. KERR: Yes.

MR. SILBERBERG: I believe it is a number of people's moves. We will encourage the speedy arrival of that next phase, but I believe, as Dr. Kelber pointed out, that it will be other people's moves to get to the next phase. I don't know if he wants to add anything to that.

MR. KERR: Well, someone said this morning that Standards was going to be responsible for rulemaking. Do you want to add to that or retract the statement?

MR. KELBER: Let me just say that in my view, the rulemaking activity should be a collegial procedure, guided by Commission determinations as to what our ultimate goal with these rules is. We have not had a satisfactory collegial procedure to date, nor have we had any indication of where the Commission, which has at least up to now been significantly divided on many important questions, would like to see us going.

I believe that there is a chance that in the next several months we will get a clearer definition of our objectives. I, for one, and a number of others I believe led by Mr. Minogue will be moving for a greater collegial discussion of what the rules ought to be. But I must tell you that at the present time, I cannot offer any concrete example of successful action in this area.

MR. KERR: Remind me where the idea that there should be a degraded core rule originated.

MR. KELBER: It originated with the TMI-2 Lessons Learned and the task action plan.

MR. KERR: Did it come from a committee, a task force?

MR. KELBER: The task force that formulated the task action plan. At the time, the concept of the rule was rather ill-defined. And it is only slowly becoming better defined. I have been drafting my own concept of what the rule should be and so have others, but there is, as I say, no coherent body of thought within the Commission and its principal staff as to what this should be.

The interim rule discussed in the Federal Register notice is clearly thought to be insufficient to define the issue.

MR. KERR: Thank you.

MR. SILBERBERG: Thank you.

MR. KERR: Are there any questions of Mr. Silberberg?

MR. SHEWMON: I think with the next speaker we will get down to, indeed, what it is we are talking about in a more nuts and bolts fashion. Or, does that come -- .

MR. SILBERBERG: Most of the nuts and bolts are on the 7th, but we start to get down to where the wrench is maybe.

(Laughter.)

MR. SHEWMON: We used to contract someplace.

MR. KERR: Mr. Picklesimer, you may work from the podium or the table, as you find most comfortable.

MR. PICKLESIMER: If it is more comfortable to you, I would take the viewgraph proceeding.

MR. KERR: Is FBB/DAE something like Order of the Guarter or Royal Society or something?

MR. PICKLESIMER: DAE is the Division of Accident Evaluation. The division of Reactor Safety Research is no more.

MR. SHEWMON: And FBB is Fuel Bundle?

MR. PICKLESIMER: FBB is Fuel Behavior Branch. We are no longer Fuel Behavior Research Branch; we are Fuel Behavior Branch. This morning, yes, it is still morning, I will very briefly cover the generics of the experimental program rather than the details. The details would require a number of hours like four or five for any significant presentation, and that is scheduled to be covered on July 7.

So what I would like to do is give you a background overall view of the overall program. I will be talking only about the experimental program. The overall severe fuel damage program consists of three major parts;

the experimental program, the analytical program, and I have forgotten what we concluded the other was. But I am talking only about the experimental program.

Before I go into what our plans are, I would like to define some things for you. I am going to be talking about what we call liquefied fuel and severe fuel damage. I would like to show you a couple of examples of that, so you will know where I am coming from when I use them.

This photograph was reported in 1977 by Siegfried Hagen at Karlsrue of the work he was doing with experiments involving the very high temperature interactions of fuel and clad. He had electrically heated rods, eight of them in configuration. You are looking down on the top of the rods here. It was an extended three by three bundle, except this bundle was removed so he could site a pyrometer on the center rod to see what its behavior was.

The seven rods in the outer ring are electrically heated. The center rod is not; it is heated only by radiation from the surrounding rods.

Now, he ran the set of experiments; these are about one foot long. He ran a set of experiments with these from different heating rates with different temperatures. All of them were relatively slow-cooled in steam. In this particular case, he was heating at 2 ^{o}C per second. The cladding was not completely oxidized by the time he got to the zirconium zirconium oxide eutectic temperature of 900^{o} Centigrade, so they formed a liquid eutectic between the sequendum and the sequendum oxide which went on to the UO_2 and started dissolving the UO_2, and these fuel pellets you can see right here have what looks like a ceramic glaze on them. It is what we call the liquefied fuel. Part of it ran on down to the subchannels in the bundle, and in these regions you can see how the fracture of pellets occurred, such that you can see the tungsten rods which were his heat source.

This is one of the degrees of damage you can have in severe fuel damage. I would like to show you a larger magnification of one of his bundles. This was heated at one-half a degree C per second, and it formed liquefied fuel only in the center rod, not in the outer ones. This is a blow-up of the bottom region of this section here, and he was trying to disassemble the bundle. In the original photographs and in the specimens I have seen in his laboratory in Germany, this wall is very thin zirconium oxide. These are pores through that oxide; they are not just black spots, they are pores.

There is a fuel pellet on there which has been partially dissolved and the material has run down on the inside. It is down in here and frozen to the plate in the subchannels, just like wax on a candle.

MR. SHEWMON: Remind me, was there any water or steam going through there?

MR. PICKLESIMER: Steam only.

MR. SHEWMON: And enough power to melt it?

MR. PICKLESIMER: Yes. You take it up either to 2000°, or 2050. I am not sure which this experiment was. That was his peak temperature in all of his experiments. Later on, we expect and they expect to have bundles like this, only one meter long, which they will water quench, but that is coming up in the next year or two.

The point I want to make is this liquefied fuel has run down here and filled these subchannels. This material can remelt and drip down further if sufficient heat is,not removed. And it will drip down to a freezing temperature someplace down below in the bundle. But this is what I am referring to when I talk about liquefied fuel. It is a mixture of zirconium, uranium and oxygen of a wide variety of compositions. The lowest melting temperature of any of these liquids can be about 1700° Centigrade, and by 2300° Centigrade, the liquid contains 70 mole percent UO_2, so most of the fuel is in the liquid by 2300° Centigrade.

Now, the philosophy of our experimental approach for the whole experimental program on this is if we are trying to do a set of experiments looking at scenarios to get our experimental data, we would have an infinite number of accident sequences to look at. And no experimental program can even examine a moderate number of these; it costs too much and take too much time.

Furthermore, we think it is not necessary. So we have, in our examination of what the problem looks like, come to the conclusion that there are only a few terminal conditions for that core. There are only so many things you can do to a fuel bundle, and there are only so many states that fuel bundle will exist in after you have done these things. You can heat it fast or you can heat it slow. You can heat it in steam, you can heat it at high pressure or low pressure. You can take it up to the melting point of the zirconium/zirconium oxide eutectic with metal unoxidized, in which case you will form liquid and the liquefied fuel.

If you heat it slow enough, all of the zirconium will have been oxidized and you will form no liquid fuel. You will form no liquid until you have reached about 2700 or 600° Centigrade, much higher in temperature. If you continue to heat, you can melt to UX$_2$; that's 2800° Centigrade.

You can cool it in different ways. You can slow cool it or fast cool it. If you water quench it, you will have a debris bed formed typically by thermal shock unto a brittle cladding; the UO_2 pellets and liquefied fuel that has been frozen. We don't know what thermal shock does to that liquefied fuel after it has been frozen. That is part of our experimental program. But you have only half a dozen conditions that core can exist in in general. Whatever the sequence that go you there. Yes, sir?

MR. WARD: You have concluded, then, that there is no mechanism for, let's say, once liquid flow is re-established, is there any mechanism for debris in small enough particles to be carried out of the core region?

MR. PICKLESIMER: I think certainly it depends on the fragment sizes of the particles you have produced. When I was on the special inquiry group looking into the TMI-2 accident, we had calculations done at Sandia which, as I remember now, I can't be held to the exact number, they concluded that if the UO_2 particles were 100 microns or smaller in diameter, they would stream out of the core. They would not -- on natural circulation now.

They would not if they were larger than that. That is the only information I have. Whether we could form particles of that size by thermal shock of the liquefied fuel or the remaining UW^2 pellets, I don't know. We will have to find that out by experiment.

MR. WARD: Is that one of your six terminal states?

MR. PICKLESIMER: When we have debris in the core, yes, which is a mixture of ZrO_2 and brittle zirconium, fragment UW_2 particles and liquefied fuel, sure. I am not saying what the particle sizes are. People have to find that out.

MR. SHEWMON: The last four and five on that list or three and four have debris bed.

MR. WARD: But debris bed seems to be a debris bed at the bottom of the core region.

MR. PICKLESIMER: Not necessarily. At the bottom, the evidence we have from TMI-2 says we have a debris bed that starts at five feet from the bottom and goes up about three feet thick. So it is suspended up in that core.

MR. SHEWMON: Think of the old down mattress where the feathers came out. Some rose and some stayed.

MR. WARD: But are some out in the piping?

MR. PICKLESIMER: All I can say on this is that in the TMI-2, they have never identified particulate UW_2 in any of the coolant samples they have taken, and they have looked for it. They have never found particulate UO_2.

MR. SHEWMON: Which is one of the bases for saying we had over-estimated the temperature of the core?

MR. PICKLESIMER: No, sir. I think it is over-estimating the fragmentation of the core.

MR. SHEWMON: Come back from retirement in a year and we will discuss it.

MR. PICKLESIMER: As far as we can see, and not only us within the branch, but other people looking at the overall problem, people from the different laboratories looking into this, all of the scenarios we can think of that wind up in severe fuel damage will wind up in one of these states.

If we study these states, then, and the processes that lead to them we should have a viable experimental program that will give us the data we need. Now, here is how we can get at predicting, estimating the type of damage we would expect for a given accident scenario.

Let the systems people tell me which valves will operate and when the pressure will drop to this and when the coolant is going to go. When they tell me what the thermal hydraulic conditions are in the system, I can come back here and go to a particular level on fuel rod or a bundle in the core. They will tell me whether I have high, medium or low heating rates, because I know what my decay heat level is and my thermal hydraulic conditions are.

They will tell me how long it is until coolant gets in to quench the core, so I can go on my heating rates for a long time to quench or a short time to quench, and then knowing what my maximum temperatures could have been because they will have told me what the level in the core was and what my cooling conditions are, I can estimate whether my temperatures got above 1300° Centigrade or not. Then, I can estimate the kinds of damage that could have been done to that, based upon whether or not it went above 1300°; ballooning and bursting, liquidizing and so on.

Any of the scenarios I can think of in a system can wind up taking one of the paths and can be analyzed in this form. This, then, tells us the kind of research we need to do, and this is where we have based our programs.

MR. SHEWMON: Why did you pick 1300 C as a number?

MR. PICKLESIMER: Because 1300° C is a temperature at which the oxidization heat of the zirc alloy and steam begins to be a very significant fraction of the decay heat, at whatever level you have of decay heat beyond 15 minutes in the accident time. If it is as much as 10% of the decay heat level, it can then take over and control the accident rather than decay heat controlling it.

Now, this is assuming you don't get coolant in there to stop it on the way. But the oxidation heat then becomes the controlling factor.

MR. WARD: Can you tell me, unless you are going on with that point, can you tell me what you mean by the row of X's in ballooning and burst? In every case, yes?

MR. PICKLESIMER: Always take this line right here. The temperature is above 1300°, yes, I would expect it to balloon and burst. And oxidation, yes, because at 1300° C, we are at a point where the cladding will dissolve enough oxygen in just a few seconds at that temperature that it is embrittled to thermal shock, and the metal itself will shatter as soon as it is quenched if it has gotten above 1300° C. Whether we have zirconium plus zirconium oxide uetectic or not will depend on the heating rate. We may have total oxidation or we may have some metal left.

I don't know the exact definition of this high here. I just know if it is above 4° C we will have metal, and if it is two and a half degrees C, I don't know. There are too many things entering into it. We have to do some experimental work.

So I raise the question. It may or may not. We may or may not have liquefied fuel. We may or may not have gotten to UO_2 melt. Tell me what the quench was, how far are we on the decay heat curve.

MR. WARD: You are going to say in every case if the rod uncovers, it is going to ballon and burst?

MR. PICKLESIMER: No, sir. If the temperature doesn't get above 1100F, and the system pressure remains above a few hundred psi there will be no ballooning and bursting.

MR. WARD: Okay.

MR. PICKLESIMER: If the pressure gets up to 600 psi or 700 psi and the temperature is 1000 C, the chances are that the cladding will have collapsed on the fuel, not ballooned and burst. It will have collapsed on it.

All I am saying is we can take this type of damage tree, if you wish, and analyze any of the accident system sequences you want to talk about.

To give you an idea, I won't go to into this in any detail -- .

MR. ETHERINGTON: To be clear, could I ask, in your top line you are saying if the maximum temperature reaches 1300 ^{o} C, then all of those X's will happen?

MR. PICKLESIMER: It should be greater than.

MR. ETHERINGTON: Greater than, yes. But then all of those -- ?

MR. PICKLESIMER: Yes, presuming this high is something like 3 or 4 ^{o} C per second for the high heating rate.

Just to give you an idea of what we are calling parameter space for the experimental programs, I would just like to show you this. I will not go into detail, but some of the things we have to look at are the debris bed production, the types of debris that are produced, the kinetics of the debris production, and this will depend upon the maximum temperature reached, the heating range and the bundling. Debris bed permeability, coolability will depend on flow rates. Heat removal capabilities, this will depend on the particle size and distribution, the types of particles, the pressure deferential, the decay heat level. We can go through the whole system this way.

We also have here some of the types of testing or the facilities where we expect to do experiments. This is just a working diagram we are using at the present time to try to decide what work has to be done first and where is it going to be don .

Now here are the experimental programs we have laid out. In PBF, our Phase I test, we are depending on five 32-rod bundles. We will have two heating rates, slow and fast. These are at the present time chosen to be one-half a degree C per second, and 4 C per second. It could as well be one-quarter and five degrees; it is not that important at this time. We want one giving us complete oxidization by the time we reach 1900° C, that is our slow. We want one that will give us some liquid formation when we get to 1900°.

We will have a slow cool and a water reflood; that is for one test. The last test we have not decided parameters on. The one we are thinking about now is a simulation of TMI-2. The reason for this is in the slow and fast heating we are trying to control the heating independent of the heat introduced by the zirconium oxidation. Therefore, we have to have high power levels for fission heat and we have to have high steam flows. We are trying to keep these two heating rates linear to 1900° Centigrade.

TMI-2 would be what we call free heating. We would put in a constant power and turn the thing loose to go and see what happened.

At our present scheduled funding we would have one test in FY82, one test in FY83 and three tests in FY84. In the accelerated funding schedule we are asking for we would have two tests in FY82 and three in FY83. Phase II tests we are discussing at this time. They have not yet been planned in detail. We do know some of the things we want to include. Some of that will be melting of UO_2, high burnup rods and stagnant steam. These are some of the conditions we know now we want to include in Phase II. We don't know whether we will be talking 5, 10 or 20 tests.

We have to learn on Phase I. We have code development. You heard earlier about SCDAP Mod 0. The first version is to be published in June 1981. Mod 1 with improved models will be due in late FY83. We will add improved mods each year to incorporate whatever fuel damage data we can get as it is developed. George tells me it is FY82 not FY83.

MR. MARINO: The first version in FY82.

MR. PICKLESIMER: I didn't check these data; I should have. This is 82 and this is 83?

MR. MARINO: Yes.

MR. PICKLESIMER: Okay. We have a set of plans for fuel relocation experiments. The separate effects tests, they will be done single rod, and what is being called few pins; few being less than 9 pins.

MR. SHEWMON: What is ACRR?

MR. PICKLESIMER: The test reactor at Sandia that has been used on the fast breeder program, and these are separate effects experiments along the lines of those that have been done for the fast program.

MR. SHEWMON: Do you run steam through that, or can you?

MR. PICKLESIMER: It will be water and steam both. Those will come later. The initial ones will be dry steam only.

MR. KERR: Which one of these experiments would have to be done in reactors? I am not asking which ones are being done in a reactor, but which ones would have to be done in a reactor.

MR. PICKLESIMER: There are a number which have to be done in a reactor because we cannot allow any mechanical interference with the production of the debris beds or the liquefied fuel.

If you remember the first slide I showed you, the Hagen experiments, that debris was held up by the tungsten rods. That was the heat source. We must have some done without this mechanical support. The only place it can be done is in test reactors with fission heat.

We are looking at the possibility of later on doing some of it with decay heat.

MR. KERR: If you are convinced, having done a few of these experiments, that the results are generalizable to all sorts of cores an all sorts of radiations, -- .

MR. PICKLESIMER: I hope. Yes, we expect that.

MR. SHEWMON: He does have a strong feeling, though, that they are more generalizable if you don't have a tungsten rod up the middle of them.

MR. KERR: I am not sure they are.

MR. PICKLESIMER: We will have out of pile tests with the tungsten rods, but we must have checks to tell us whether our out of pile tests are valid or not.

MR. KERR: I guess it depends on how far you take this.

MR. RYE: Mr. Chairman, Bob Rye of the Fuel Behavior Branch, and I have the SCDAP effects of the ACR experiments. They break into two parts; one is the question of the debris formation and relocation, and these are few pin experiments with steam cooling and some, monitored, the unprotected accident sequence debris formation, and some will be done with reflood quenching, which will be examined later.

A separate series of experiments involve the coolability of preformed debris, similar to the work we have been doing in fast reactor safety. In fact, both of these sets of experiments are carry forwards of techniques we have been developing in fast reactor safety and they are analogous to the experiments we have done. But under LWR specific positions -- .

MR. KERR: That is in response to my question of which have to be done in reactors?

MR. RYE: That's right. Also, there are questions about steam and water, also. It's a mix that covers the whole sequence of heatup under steam cooling, reflood quenching, and separate experiments on coolability limits.

MR. PICKLESIMER: Once we are assured we know what the debris looks like in real fuel bundles in the reactor, then we can construct artificial debris beds for out of pile studies and be more confident that our out of pile work can be applied.

MR. KERR: That assumes that if you run two or three experiments you then know what debris always looks like.

MR. PICKLESIMER: No, we know a range of it, and what we expect to do is cover a range. We are not going to try to hit every point in it; we will get certain kinds, certain conditions and terminal states from inpile studies. If this spans the range of debris to be produced, we should have the data we want.

The next set of work is expile. We will be looking at, for lack of a better name, we are calling it fuel cladding interaction studies. Really, what we are looking at is the thermodynamics and kinetics of the reactions between zirconium, Zr, UO_2 and steam and the physical properties of these products.

We need this for insertion into the codes to be able to calculation what the progression of the damage is. At this time, no one has any information on what the solution of zirconium is in liquefied fuel or molten UO_2 and no one knows what the heat of oxidation of that dissolved zirconium is. We must have that information.

Another out of pile study is what we are calling DECCA for deformed core coolability studies. It is an offshoot of the MRBT program at Oak Ridge, where we will look at the behavior of 8 by 8 bundles, full length; that being 12 or 14 feet, during boildown in high pressure, small break conditions. Then, we will remove the deformed bundle from the test facility and instrument it for thermal hydraulic fissions; reinsert it into the test facility and conduct a series of thermodynamic tests looking at the temperatures of the cladding and the heater rods and the steam and steam velocities in the damaged region.

We need this information if we are to predict with reasonable accuracy the amount and types of debris that would be formed by severe fuel damage occurring at a higher temperature. We are participating as one partner in a program underway at Isfra, Italy. It is being supported by the European Economic Committee. It is being conducted in the Essor reactor. They are proposing there will be 10 tests on fuel damage in 32-rod bundles two meters long.

This two meters of length will give us a considerable depth of debris bed liquefied fuel formation and candling, more than we could fit in PBF. We are not sure we can get in PBF under properly controlled conditions beds deeper than six inches. If we are only talking about a 32-rod bundle, this is less than four inches in diameter, and a debris bed four inches in diameter and six inches long does not give us that much confidence. We can extrapolate it to a full-sized bundle. We need length and diameter effects.

Then we have the TMI-2 core examination.

MR. KERR: Excuse me. Give me some idea, if you would, where one might be going. For example, do you, in your planning for this sort of thing, foresee something like an evaluation model for core melt of the kind one now has for ECCS so that one needs very detailed computer codes which describe the behavior of a degraded core? Is that the sort of thing one expects to result from these kinds of studies?

MR. PICKLESIMER: We expect to have a code that will describe the behavior and the condition of the degraded core, but we don't expect it to be a detailed, long-running code. We will keep it as short as we possibly can to make it more nearly interactive with other codes. And with eventually, hopefully, operators. Yes, George?

MR. MARINO: I would like to expand on that a bit. The plans for the code are to develop simultaneously very complex models and simple correlations, to check the correlations against the complex models, to determine how much complexity is truly needed in the code. The objective, of course, is to keep the code as simple as possible and to put bounding limits on each of the models, so we can go into the code with an uncertainty pattern and get the output produce with an uncertainty in the output, because we will never know these things exactly and we know that going in.

We will use correlations from complicated models and put uncertainties on those.

MR. KERR: Then these codes will ultimately be used for what?

MR. MARINO: To analyze what we should expect in a degraded core cooling accident, given any kind of a sequence. There are an infinite number of sequences; the code should be able to cover the range.

MR. KERR: I am not asking a very good question. This is a good academic exercise if one wants to find out how cores behave. But is one expecting that this will be used in a licensing exercise, for example? And I recognize you cannot predict this with much greater accuracy than you can predict the results of the code. But you have some objective in mind for code development. Is it a licensing tool, is it a research tool?

MR. MARINO: It is all of those things. It will be a licensing tool eventually. Initially, it will be a research tool to help us with these experiments.

MR. KERR: But you look at it ultimately as a licensing tool, and one then might have an evaluation model for degraded core performance.

MR. MARINO: As we have developed out other codes, we have intended to go into evaluation models later, and we will do that with this code but that is later on.

MR. KERR: How much later?

MR. MARINO: I would expect in 1984 we would be doing that sort of thing.

MR. KERR: Thank you.

MR. PICKLESIMER: Let me add one thing to that. It is my personal hope that this code can be made simple enough that it can be a realtime interactive code with a safety shift supervisor, for example, on plant during an accident, and he can use it to calculate what the results will be of some of his actions in trying to manage that accident.

Now, whether we can ever get there or not is another problem. But that would be one of the ultimate goals as far as I personally am concerned.

MR. KERR: I must say you have a lot more vision than I do.

MR. PICKLESIMER: There was lots of time at TMI-2 for just such calculations to be made. If they had known enough about interpreting the instrument readings they had at hand, they could have inserted these things int[illegible]at we expect SCDAP to be.

MR. KERR: That is a fairly big if.

MR. PICKLESIMER: Yes, certainly, but we have to have some hope down the line if we are going to try to manage accidents while they are occurring, and that is one of our strong goals.

MR. KERR: Well, as I say, it helps to have vision.

MR. SIESS: It helps to have hope, too.

MR. PICKLESIMER: The TMI-2 examination we hope to experiment in is a cooperative program between GPU, EPRI, NRC and DOE for the examination of the core, the removal of samples and the dispersal of those selected samples to selected laboratories for analyses, examination and so on. I hope to participate directly in that at some future time.

Our contribution to this is in the planning, specifying the types of examinations we want, the types of information we want from it, and our funding is expected to be spent on conducting the analyses of specific interest to us and NRC, from the standpoint of safety, when these analyses are not of interest to the other partners in the examination. That is why our budget level is actually low in terms of total dollars.

We expect most of the examination costs to be carried by EPRI and DOE, and only to work with certain selected samples.

MR. SIESS: You would do those tests to determine what the debris beds look like before you examine the TMI-2 core?

MR. PICKLESIMER: We expect to have at least two tests in PBF before the TMI-2 has removed, and we may well have five.

MR. SIESS: What do you think the chances are that when you see the TMI-2 core you are going to wish you had done something different?

MR. PICKLESIMER: Quite high. We will hope to use the TMI-2 examination to guide part of the tests. That is one of the reasons we need that TMI-2 result as early as we can get it. It is a very important piece of information.

We will have a program on the coolability of the debris produced in light water reactors, both inpile and expile determinations. They will be determinations of the dryout heat in the bed as a function of debris types, particle sizes and so on, and of the depth of the debris. Then we will have a program on the formation and relocation of debris.

This work is primarily in the ACRR. It is a separate effects study more than the integral tests that will be done in PBF and Essor.

Now, for my last slide, last May I participated in an informal meeting in Tokyo, Japan with a Japanese ACRS, and the people from the Japanese Atomic Energy Research, and people from France and Germany on the plans that each of us have for severe fuel damage studies in the near future. These are the major conclusions we reached at that meeting.

The Japanese have no plan to do any experimental work on severe fuel damage. They have several committees that are examining the need for the various types of research work that they need in what they call accidents beyond design basis accidents. They will not use the words severe fuel damage.

The French plan no experimental work on severe fuel damage beyond participation in the super-SARA program. They are examining the modification that would be required to the Phebus reactor to permit severe damage tests to be done in it, but they do not plan to pay for these tests; they expect others to pay for the test trains and the test analyses. They would furnish the reactor site and the neutrons. They expect to satisfy most of their data needs by information exchange with other nations.

The United Kingdom has at this time no active severe fuel damage studies underway except for their participation in the super-SARA program. They are examining their need for severe fuel damage data and are expressing an interest in participating with the PBF study at this time.

Italy has no active severe fuel damage study underway except for their super-SARA program. Karlsrue plans no inpile test except for super-SARA and they are re-instituting research work of Hagen that was stopped in 1979. They expect to do that in the coming year. They will have modified the Hagen facility to take bundles 7 by 7 in size with rods one meter long, and will be able to quench with water.

The discussions I had with them in May in Tokyo indicate they are planning a rather extensive, long-range, out of pile study that we need very badly. We need that information. If they don't do it, we will have to.

We are also continuing the phase diagram and kinetics work that Peter Hofmann has done on the zirconium/uranium oxygen system, and that also we need badly. They want to exchange their out of pile severe fuel damage data with our PBF fuel damage data and SCDAP, and we are interested in exchanging with them. The discussions will proceed sometime this fall. Thank you.

MR. KERR: Thank you, sir. Are there questions?

MR. KERR: Mr. Silberberg, because of scheduling we would like to make the presentations scheduled for later at this time, and if the designated federal employee has no objection, we will arrange that.

MR. SILBERBERG: Thank you, Mr. Chairman.

This afternoon I was supposed to cover an item on the agenda called coordination with foreign programs, and I have here before you the one vu-graph that summarizes this information. A lot of it I believe you have been exposed to before. Some of it is somewhat new.

Our two principal countries in Europe with which we have a very active working relationship in Class 9 accident research are with the Federal Republic of Germany and more recently in Sweden with the Studsvik Energiteknik group. In the Federal Republic of Germany it pretty much runs the gamut of similar programs to ours, some of which you will be hearing about from fission product release experiments in a Sascha facility, a small scale which is, if you will, compared to our own work at Oak Ridge. The severe fuel damage work which has just been described by Mr. Picklesimer. In the area of fuel melt-concrete interactions, the German program is very active with codes like Wechsel, Kavern, and a large-scale melt interaction facility that is now under construction which will be operating in late '82 or early '83, which will complement our own work that is being done at Sandia.

MR. KERR: Is the Beta facility?

MR. SILBERBERG: Yes, the Beta facility. There are some details, some distinct differences between that facility, the experiments in that facility, and our own, but they are quite complementary. And if one wished to get into the details of that, I am sure Dr. Powers could do that for you.

In the area of aerosol behavior in steam atmospheres, we have the NAUA code, which we have which we are setting up in this country to supplement some of our own work, to use it if we can in part or whole. In fact, we were able to obtain some very valuable calculations with NAUA from the Federal Republic of Germany for our recently completed NUREG-0772 which were quite interesting.

In the hydrogen area I am not as familiar with the work as I should be. I believe the principal area we are coordinating with is the RALOC code which has to do with how hydrogen mixes in containment atmospheres.

There is, of course, the work comparable to MARCH, the KESS code which I believe you have heard something about. And in the steam explosion area they are relying heavily on our experimental program, and they are doing mostly modeling work in the area of steam explosions, but they have clearly indicated they are looking to us for the lead.

In Sweden, and I think you will hear a little bit more about some of this work from Dr. Meyers on the 30th, I believe, on what is happening in Sweden relative to the filtered vented containment, the project which they call FILTRA. The Swedes are moving along very rapidly. What they are doing is they are using the analytical techniques available for other countries, the Federal Republic of Germany as well as the U.S. -- the aerosol codes, the MARCH code, things like that -- to use as a reference for design and for assessment. But in the meanwhile they are specifically working with starting to run tests on various types of gravel bed and sand bed concepts in terms of loading capabilities, efficiency and things like that.

I might say they are also getting assistance in that area from the technology that is ongoing at HEDL having to do with the filter gravel bed-sand bed filter work going on up there in relationship to the filtered vented containment for FFTF.

Finally, they are running experiments to see what type of fission product retention, in addition to solid retention, particularly iodine and things like that, one might expect in these beds.

I was impressed with the progress they are making in general. In a very short time they are moving out very nicely, and I would expect if we were to get, as we get more and more involved in filtered vented containment, that we could get a lot of information from their program.

MR. ETHERINGTON: In the Beta facilities are they planning larger scale experiments than those at Sandia?

MR. SILBERBERG: They are about the same scale, a few hundred kilograms different material.

MR. KERR: Is there work complementary to what we are doing, parallel to, duplicative of? How would you characterize it?

MR. SILBERBERG: Where? The Beta facility?

MR. KERR: Yes.

MR. SILBERBERG: Complementary. There may be some overlap, but I would classify it as largely complementary. You might ask others how they feel about that, but that would be my feeling.

MR. KERR: Apparently they are to some extent doing complementary research there, whereas in the steam explosion area they are not. They are going to depend upon our work.

MR. SILBERBERG: I think that is accurate.

MR. KERR: Does that indicate they don't think steam explosion is very important, do you suppose, or that they think we are better at doing steam explosion than --

MR. SILBERBERG: A little of both. I think they may have, as the data now evolves to this point from various work, they may have less concern about steam explosions. I say they may. And I think they are also satisfied with the information they can get from us.

MR. KERR: Thank you.

Mr. Siess.

MR. SIESS: What mechanisms do you use to maintain your contact with what foreign countries are doing in coordination?

MR. SILBERBERG: In the Federal Republic of Germany right now we have a resident engineer on site.

MR. SIESS: A staff member or a contractor?

MR. SILBERBERG: Contractors, Drs. Peck and Corwin. And they focus on several of these areas, obviously not all. That is one mechanism. Obviously, the exchange of reports, and when we can arrange them properly, the, shall we say, periodic visits of staff to staff.

I still think that that is still the best way. In fact, we would want to encourage that in the future.

MR. SIESS: Do you feel that even using the contractor you do maintain research staff awareness of these things in some depth?

MR. SILBERBERG: I think so, yes. I think it is important that --

MR. SIESS: Does your contractor representative send you back detailed reports? Do they come back from meetings with your people, et cetera, et cetera?

MR. SILBERBERG: Yes, yes. Reports are required. There is a requirement that they submit a report, hopefully a detailed report, in order for them to get paid.

MR. SIESS: Is there a corollary requirement that someone read and understand it?

MR. SILBERBERG: Yes.

MR. ETHERINGTON: Mel, I continue to be concerned about the conclusion that melt into concrete will proceed without spoiling. The experiments today have really tended to inhibit spoiling. You cannot pour a melt into a concrete crucible with a band around it and expect that it has the same chance to spoil as the flat slab would have.

My only experience in the steel business is that you do get spoiling where metal penetrates, heavy spoiling. I wonder whether you would find it worthwhile to contact the steel industry and try to get them to make a survey of the occasional accidents that have happened in the steel industry.

MR. SILBERBERG: I believe a lot of this has been done. Perhaps Dr. Curtis or Dr. Powers would care to comment.

MR. CURTIS: The program manager for our core melt program, Dana Powers, is here. He is scheduled to talk about his program for half an hour this afternoon.

MR. ETHERINGTON: We can leave it until then then.

MR. SILBERBERG: I think that is a good suggestion. Thank you.

MR. KERR: Other questions or comments?

(No response.)

MR. KERR: Thank you, Dr. Silberberg.

My agenda shows Mr. Curtis next. Is 15 minutes an accurate evaluation of your presentation?

MR. CURTIS: I hope it is no longer than that.

MR. KERR: Then we will probably stop for lunch after you have finished.

MR. CURTIS: The decision unit we are talking about has four basic line items. The first was fuel behavior which Melvin talked about. The second is a fuel melt behavior. The third, fission product release in transport. And the fourth, severe accident mitigation.

Mel has already talked about the first one. It is my understanding another subcommittee has examined fission products release and transfer, Dr. Moeller's subcommittee, to the point we will not discuss that during the balance of our presentation.

MR. SHEWMON: They studied fission product outside the pressure vessel, I am sure; maybe in containment, and also inside the primary circuit?

MR. CURTIS: It is my understanding that they have discussed that problem to the point --

MR. SHEWMON: Were you there?

MR. CURTIS: Mel.

MR. SILBERBERG: I'm sorry.

MR. KERR: Would you repeat the question, please, Mr. Shewmon.

MR. SHEWMON: Well, the statement was that the subcommittee chaired by Dr. Moeller had gone over all there was to say about fission product transport, and my inquiry was whether or not that covered outside the containment, inside the containment, and possibly inside the primary system.

MR. SILBERBERG: Dr. Shewmon, it was inside the primary system, inside the containment but not outside.

MR. SHEWMON: Okay, thank you.

MR. KERR: Mr. Cunningham told me he was talking about ten percent of severe accident mitigation. Are you going to talk about the other 90 percent, or do you subsume his work as well?

MR. CURTIS: His work is in fact a part of the budget figure that you see, but I do not intend to repeat any of his discussion.

MR. KERR: You are going to talk about accident evaluation and mitigation of program overview. That's this line item, severe accident mitigation?

MR. CURTIS: It includes both the fuel melt program and the severe accident mitigation.

MR. KERR: Okay. And you are going to talk about -- a fuel melt is different from what Mr. Picklesimer talked about. He was only talking about while it was trying to melt, is that right?

MR. CURTIS: The focus of the things you are going to hear about from us are generally after the failure of the pressure vessel.

MR. KERR: Failure of the pressure vessel?

MR. CURTIS: Failure of the primary pressure vessel, yes.

MR. KERR: Okay.

MR. CURTIS: The fuel melt behavior program has basically the following components in it. We have a program on hydrogen which will be discussed with you this afternoon by Dr. John Larkins. I noticed his name is misspelled on the agenda. A program on steam explosions, a program on core melt technology, some work on core debris, though not the in pile portion of the core debris coolability work which has been discussed in the fuel behavior program, and a series of three codes which we intend to discuss.

The codes which are involved in this element are the CORCON code, the CONTAIN code, and our plans for the

MARCH code. We are expecting from Sandia within the month the assessment of the MARCH code which we spent two days on last month, so I would presume not to discuss that at any great length.

MR. KERR: What does assessment of the code mean, Mr. Curtis?

MR. CURTIS: Assessment of the code is an evaluation of the code and a detailed compilation of user experiences to date to indicate areas of appliability and areas of deficiency, and to identify which of those which are labeled deficiencies appear to be amenable to corrections. So I am expecting to get out of that the basis for selecting those issues which we will fix on a near-term basis and those which we will probably defer.

MR. KERR: In order to evaluate deficiencies, one must have some idea of what the code is supposed to do. Where does that description exist?

MR. CURTIS: Cne of the reasons for the assessment is to evaluate if there is a data base which would support an improvement.

MR. KERR: But an improvement, does there exist some description that says this is what we would like for the code to be able to do?

MR. CURTIS: Yes, certainly.

MR. KERR: Where is that?

MR. CURTIS: The anticipated scope and the potential use of the code is another chapter in the assessment document.

MR. KERR: So this document will say what it is, what it would like to do.

MR. CURTIS: This is what it has been used for, and this is the way it has been pushed into further application; and we believe that there are currently a laundry list of deficiencies. Some of these deficiencies are relatively easy to fix. Some are very difficult to fix. Some of these deficiencies can be fixed because there does exist a reasonable data base to extrapolate. Some of these deficiencies probably we should not try to tackle.

MR. KERR: So there will be one chapter which says, in effect, this is what the code ought to be able to do.

MR. CURTIS: There will be a chapter which says at least this is what the NRC is trying to do with the code.

MR. KERR: That is completely different, or it seems to me it could be completely different, because the NRC has just begun to look at the degraded core problem. Is there someone who is saying if we had a code that would be useful to the degraded core problem, this is what we would like to have it do?

Are these the same thing?

MR. CURTIS: I think they're the same thing, and if they're not, we will have to bring them into a better --

MR. KERR: I would anticipate, for example, much of the use of the code up to now has been to design end point analysis.

MR. CURTIS: That is correct.

MR. KERR: I would anticipate the future degraded core rule would go beyond that; hence, it's not obvious to me that what the NRC has been doing --

MR. CURTIS: It has not only been used in design end point; it has been used in the analysis of Sequoyah, McGuire, and a variety of other plants, Limerick.

Dr. Kelber.

MR. KELBER: I would like to just remind you that insofar as MARCH and CORRAL are concerned, there is a formal code document that is distributed along with the code.

MR. KERR: This is the user's handbook?

MR. KELBER: This is the user's handbook. The use of the code was in the characterization of core melt sequences, and I forget the exact wording of the acronym.

MR. KERR: I guess I'm not asking the question well, Charlie. I mean the code was originally put together for a purpose.

MR. KELBER: Yes.

MR. KERR: My impression is it is now being used beyond that original goal.

MR. KELBER: That is a correct impression.

MR. KERR: And indeed, I one had one's druthers, one might like to have an im oved code that would do something or other.

MR. KELBER: That's correct.

MR. KERR: It's that something or other that it seems to me one needs to have in order to assess the code's deficiencies.

MR. KELBER: I think there is a danger here of putting the cart before the horse.

MR. KERR: I agree. It seems to me that is what is being done if one starts improving the code without knowing what the goal is

MR. KELBER: Let me give you my own views, and Bob Curtis, who has much more intimate knowledge of the details, can take up. But my own view is the following: that the need has been amply demonstrated for a code which will at least allow us to, as George Marino pointed out, bound with some confidence the order and magnitude of events that occur in core melt accidents from the point where the cooling point is degraded to the point we have significant loads on the containment with perhaps molten core on the floor of the containment.

There are three portions to the code. These are reproduced in MARCH, but basically they are the portion represented by SCDAP, pronounced SCDAP. As yet unnamed and only in the planning stage, the whole core which would incorporate SCDAP. It might be an enlarged version of SCDAP. It might be a merger of FRAP and SCDAP. But whatever, that whole core meltdown code and the CONTAIN code, that is our ultimate need. We need it if only because we have to answer questions such as the questions being asked at Zion and Indian Point.

But you are right, we will be going beyond that. I think we don't have those codes, but we know what we want them to do.

MR. CURTIS: We have already put MARCH into application in such a variety of ways that I believe we have a first round of improvements which could be focused on improving the quality of the prediction in the places where the code is already in application.

MR. KERR: Bob, it seems to me there is one possible approach to improving the code, and that is to use it for something, and you discover it doesn't do what you want to do for that application, and you say what could I do to make it better. I don't see anything wrong with this. In fact, I think it is quite the way to proceed.

The problem is that that fixup is not very useful unless you are going to use the code for the same thing again. Maybe the answer is no, you don't have it; but I just wondered if someone had made an effort to say we think this is the set of general uses to which a code like this ought to be applicable, these are the needs we have. And maybe indeed MARCH cannot ever be fixed up to do that, and you would have to start over completely.

MR. CURTIS: I believe that is the answer.

MR. KERR: But if it isn't the answer, it seems to me there ought to be some sort of limited goal if you're going to invest very much money in improvements. Otherwise, I don't see how you know when you've got to where you want to get.

MR. CURTIS: I expect the assessment is going to tell me there are a limited number of short-term improvements which can be made to the existing structure, which ought to be made to the existing structure; and that the long-term goal should be to rethink the problem and put together a fresh approach.

MR. KERR: I would guess any organization in the business of developing codes would come up with that result.

(Laughter.)

On the other hand, they do need, and I guess they are getting some input from NRC which will ultimately be the user.

MR. CURTIS: Oh, yes.

MR. KERR: Continue.

MR. CURTIS: I believe I have --

MR. KERR: Excuse me. You mentioned this document that is coming out. I didn't write it down. Does someone have it identified?

MR. CURTIS: A MARCH code assessment being produced by Sandia.

MR. KERR: It will be available?

MR. CURTIS: In draft form within the month.

MR. KERR: Thank you.

MR. CURTIS: In this area there are two other codes. I discussed these with your committee at the time of the March meeting. I have the same presentations here again. I would choose to distribute the vu-graphs and not to go through that discussion again.

MR. KERR: Would it be a better presentation the second time around?

(Laughter.)

About the same?

MR. CURTIS: No. Better.

MR. KERR: Then we'll just take the vu-graph.

MR. SIESS: Could we ask better questions?

(Laughter.)

MR. CURTIS: Perhaps there would be better questions.

But for the record, since these are a part of the decision unit, I will provide you with this data support.

MR. KERR: Thank you.

MR. CURTIS: Our program on severe accident mitigation is rather closely coupled with the other decisions, and it is rather closely coupled in the following fashion. It is closely coupled because the phenomenology associated with severe accident mitigation features is very closely connected with the basic phenomena of the core melt.

Let me make the point in our core melt program we are investigating the phenomena associated with molten cores on concrete. We use the same experimenters. We use in many cases the same devices but substitute for the concrete proposed core-catcher materials, mitigation features.

One day it might be a thuria oxide gravel. Another time we will line it with magnesia bricks. Somebody will come up with a castable ceramic. We will try it out in the same facility using the same experimenters. We have identified for budgetary purposes two separate 189s. One is in severe accident mitigation; one is in core melt behavior. But they are in fact very closely coupled, and as they say, the same research facilities and the same principal investigators are attacking both parts of the problem.

MR. KERR: I'm sorry. You lost me.

MR. CURTIS: I am saying for budgetary purposes we have a separate Fin. number to support work on core-catcher materials from the one that is looking at the core steel melt-concrete interaction.

MR. KERR: The same people are doing the work?

MR. CURTIS: The same people are doing the work in the same facilities, and I will propose Dana Powers will tell you about both pieces of it, realizing, however, that from a funding point of view there is money associated with these in both line items.

The same is true in terms of hydrogen. We have a basic research in hydrogen program. Associated with that program and as an add-on to it, we are looking at the survivability of equipment under hydrogen burn conditions and what can be done to develop suitable standards for the survivability of equipment under hydrogen burn conditions.

MR. KERR: Is that under severe accident mitigation?

MR. CURTIS: The equipment survivability tests, which again are separately funded, are under severe accident mitigation. The basic hydrogen program is under core melt.

As a part of the fission product release of transport we intend to do tests on filters in an aerosol environment. These filter tests will be part of severe accident mitigation and an integral part of the consideration of fuel bed containment. And again, these tests will be separately funded as a part of severe accident mitigation.

MR. KERR: I got the opinion somewhere that the licensing people aren't that sanguine about fuel bed containment.

MR. CURTIS: Right. I suspect if we were not doing the aerosol release and transport experiments and could not put that facility to use we might not be so ready to do some filter tests. But right now we are planning to do filter tests if this thing gets turned off.

MR. KERR: You have a filter facility, sc --

MR. CURTIS: No. We have an aerosol facility.

MR. KERR: And you need something to filter it.

MR. CURTIS: That's correct. So we might just as well, if filters of a certain concept are proposed, we would prefer the test. We have a study at INEL on mitigation features in which we are concentrating on the engineering feasibility of mitigation features across the board with a particular emphasis on what are the problems of backfitting severe accident mitigation features to existing plants.

We have work on containment coolant. In particular, we have recently gotten a user request to put out an RFP to investigate passive containment, which will be a part of this. The RFP --

MR. KERR: Who requested that?

MR. CURTIS: We have an endorsement from NRR. And finally, the analytical part of our severe accident mitigation feature is the SASA program, which is an integrated program being conducted at four laboratories; and I have a half an hour this afternoon to tell you about the SASA program, and I would be happy to do that.

So anyway, those are the basic components of the program. I have Dana Powers to tell you about the fuel melt experiments, the concrete interactions, the core-catcher work. John Larkins will be telling you about the hydrogen control program, the equipment survivability under hydrogen. Rick Sherry will be telling you about the steam explosion program. And I will talk about the SASA program, and then you will have an opportunity to question me about anything we may have left out at the end of the day.

MR. KERR: Thank you.

Are there questions?

(No response.)

MR. KERR: I declare a recess until 2:00 p.m.

(Whereupon, at 1:00 p.m., the meeting was recessed for lunch, to be reconvened at 2:00 p.m., the same day.)

AFTERNOON SESSION

(2:00 p.m.)

MR. KERR: Our next speaker, which involves a slight rearrangement of the printed agenda, will be Rich Sherry, who is going to talk about steam explosions.

MR. SHEWMON: Mr. Chairman, we have Powers and Larkins yet to come, is that right?

MR. KERR: That is right.

MR. SHEWMON: Okay.

MR. SHERRY: My name is Richard Sherry, the program manager for the steam explosion program. The steam explosion program was initiated in 1976. The purpose of this program was to investigate the phenomena associated with the explosive interactions of molten core materials with reactor cooling and to determine the probability that a steam explosion can fill the reactor containment building.

The elements of the current program include small-scale single droplet tests involving droplets on the order of 1 centimeter to try to determine the basic mechanisms of fragmentation; intermediate skill tests involving 5 to 25 kilograms of corium and coriums simulants in the fully instrumented test facility.

Several tests are tests to investigate the phenomena, nonexplosive phenomena of melt-water interactions, primarily to determine mixing phenomena, breakup and steam generation rates.

The analytic part of the program includes development of models for the fragmentation, propagation and bubble expansion during steam explosion events, analysis of response of reactor vessel to the expanding steam explosion bubble, and the response of the containment to shock waves and missiles generated by the steam explosion.

And the ultimate objective of this program is to provide updated estimates on the failure probability of containment due to steam explosion. The accomplishments during the past year in this program include we have conducted a series of small-scale experiments to determine the effect of elevated system pressures on the explosivity of molten materials.

The first test series in the intermediate scale test included five tests. Two steam spike experiments were completed. In these experiments where were initiated as a result of the findings of the Zion-Indian Point study which indicated there was a potential for rapid steam generation following the reactor vessel melt-through leading to a steam spike which might have the potential to challenge the integrity of the containment.

MR. KERR: When you say that was a finding, it was a calculation of the MARCH code, was it not?

MR. SHERRY: That is true.

MR. WARD: A qualified finding.

MR. SHERRY: A qualified finding, very qualified. During the past year a one-dimensional transient propagation model and a two-dimensional empirical expansion model have been developed and used to analyze several tests. These models are also capable of extrapolating the test results to actual full-scale reactor conditions and have been used to evaluate the loadings on the reactor vessel for several actual reactor designs.

A statistical steam explosion containment failure model has been developed and incorporates distributions for such things as the amount of melt and the amount of water which may interact; the energy conversion ratios that may occur during a steam explosion vent; the response of the reactor vessel, et cetera.

Using this statistical model new estimates have been developed for the containment failure probability for a large high pressure PWR design.

I am going to skip the next seven slides. They are devoted to some of the test results and some of the results from the program. And the next slide I am going to be talking on --

MR. KERR: Rich, just a minute.

MR. SHEWMON: Yes. Before you get into that, I read some place recently that the steam explosions and what you refer to as fragmentation here to get good heat transfer occurs only when the droplets are liquid so that they can break up easily.

Did I understand that correctly?

MR. SHERRY: That's the current belief, yes, that the fine fragmentation would only occur --

MR. SHEWMON: In my parlance only solids fragment, or in your parlance or when you do these calculations, it's only liquid droplets that fragment.

MR. SHERRY: We have only seen the fine fragmentation down to the 1 to 200 micron range with the liquid droplets. Solid droplets would fragment through various mechanisms, but not down to the very small sizes.

MR. SHEWMON: Thank you.

MR. SHERRY: This slide shows a comparison of the WASH-1400.

MR. SHEWMON: Let me stay with that for a minute. If you get down some place fine enough then, they solidify instead of breaking up. Is that actually what would limit things if you studied the steam explosion as a function of degree of superheating or temperature difference between the temperature of the bath and the temperature at which it's solidified.

MR. SHERRY: Yes, that's true. There is essentially, we believe, two stages to this. There is, of course, fragmentation and breakup down to perhaps centimeter size and then a fine --

MR. SHEWMON: And that would happen with a solid droplet?

MR. SHERRY: No. I am saying if this is a liquid.

MR. SHEWMON: Okay.

MR. SHERRY: And if it breaks up to about 1 centimeter sizes. If an event does not occur which leads to a fine fragmentation down to the size of perhaps several hundred microns, it is possible that these droplets will be solidified and become nonexplosive, or these droplets could fall. If they dropped into water, there could be the coarse fragmentation, and then these particles could settle by gravity to, let us say, the bottom of the reactor vessel.

MR. SHEWMON: But once they have solidified, they will quit.

MR. SHERRY: They won't participate in the --

MR. SHEWMON: My point is if this stuff candles out or it won't be much above the liquid solid temperature for the oxide when it comes out of the core. What I am asking is have you ever looked in or whether your contractors have looked into whether or not they can argue they would not have enough superheat, that is, temperature difference between the liquid temperature and the solidification temperature to keep breaking it up.

The relaxation time for losing 100 degrees has to get smaller and smaller as you go down in size.

MR. SHERRY: Yes, we are looking at this. In fact, in the past test we used iron aluminum thermites which have a lower melting point than the corium mixtures which we are just beginning to test now. It appears the corium mixtures are much less explosive than the iron aluminum thermites, even when they drop at the same temperature.

MR. KERR: What is the significance of using the term "explosive?"

MR. SHERRY: What's the significance in this case?

MR. KERR: Yes.

MR. SHERRY: It indicates that the interaction when coriums are dropped into the water are much less violent, more characteristic of just rapid boiling than a shock pressurization.

MR. ETHERINGTON: Are the old theories of encapsulization invalid?

MR. SHERRY: The theories of homogeneous nucleation?

MR. ETHERINGTON: Of a capsule of metal being formed with moisture inside and then exploding. It's an old theory. Perhaps it's not valid at all. If you haven't heard of it, perhaps --

MR. SHERRY: No. I have heard of it. The

fragmentation mechanism which we believe actually drives the process is one of vapor film collapse and then local pressurizations due to rapid boiling around the surface of the droplet rather than jetting the water inside of the melt droplet and breaking up in that manner.

MR. ETHERINGTON: By implication you are discrediting the encapsulization theory then.

MR. SHERRY: I wouldn't say that. It may possibly be a mechanism, but I don't think it is the one preferred now, the one we believe.

MR. KERR: You would say it is a bit old-fashioned?

MR. SHERRY: You can say that, I guess.

MR. SHEWMON: But he would be too diplomatic to say that to the most elderly member on the committee, I suspect.

MR. ETHERINGTON: But I was really leading in that direction. I wanted to know --

MR. SHEWMON: Let me come back to my question.

MR. KERR: Excuse me. Could I ask for a line on this encapsulization?

MR. RYE: Bob Rye. I am an old hand in this business of about 20 years ago. The encapsulization model, there is no real evidence now that supports it, and the current mechanisms, as Rick has said, that are looked upon with favor are this multi-stage pre-breakup and then essentially a thermal detonation that propagates as a shock through the premixture.

The actual mechanism of fragmentation that is dominant in the interaction to produce more surface area for the increased heat transfer to sustain the shock is still a subject. It's still essentially unknown. It is a subject of controversy. And that process requires, in answer to Dr. Shewmon's earlier question, that process requires the material to be in liquid form, not solid, to get the very fine breakup, and there is some correlation and a vast range of data between the intensity of the interaction or explosion and the fineness of fragmentation. A more intense interaction tends to be finer fragments.

MR. ETHERINGTON: It sounds as if it's getting simpler.

MR. RYE: No, I don't think it is getting simpler.

MR. SHEWMON: I agree. Everyone agrees, I think, that you have to fragment it to get good heat transfer and to get a violent explosion. My concern and interest though is whether in modeling this and your contractor's enthusiasm to find something to measure you are missing the conditions which are likely to exist in an accident, and that is that you will have relatively little temperature above the melting point of the solid oxide when it falls into the water. And thus, once it breaks into centimeters, millimeters or something, it will quench out, solidify, and by your mechanism then stop.

MR. RYE: My earlier shock work 20 years ago when we didn't quite understand, it showed that essentially the peak pressure is buried with the superheater above the melting point, and that the energy that is available below melting really did not seem to contribute. Certainly we think now that you stop further fragmentation at this point.

MR. ETHERINGTON: The thing behind my question was are we looking for a different behavior between a brittle material and a metal like iron? Do you expect them to behave the same way?

MR. SHERRY: A brittle metal?

MR. ETHERINGTON: UO_2 versus steel.

MR. SHERRY: I guess I don't know who to answer that question.

MR. SHEWMON: Apparently the breakup is only when it's a liquid.

MR. ETHERINGTON: But if you pour a metal like zinc into a model, it granulates and can be contorted to shell-like pieces.

MR. RYE: We think that correlates to this premixing and not the thermal process. That would not be explosure.

MR. ETHERINGTON: Then you could not get an explosion?

MR. RYE: In that particular experiment. But on the other hand, the limits under which this could happen, particularly when you furnish a good trigger, is unexposed territory. We don't have the mechanistic model.

MR. SHEWMON: But you do have to keep it liquid apparently.

MR. RYE: All indications are you do not get further fragmentation in the solid state which contributes to the energy discharge.

MR. SHERRY: One further comment on Dr. Shewmon's question. Those considerations have been included in these new estimates for containment failure probability. The observations that the corium mixtures are less explosive than the simulant materials which have been used in the past, and we believe that is due to differences in melting temperature.

MR. SHEWMON: Is there any particular correspondence between the corium temperature you use and the corium temperature you would have good reason to believe would come out of a core or out of a vessel when it fails?

MR. SHERRY: The temperatures of the corium mixtures are fairly difficult to determine, but we believe they are in the temperature range of 3,000 degrees K.

MR. SHEWMON: Steel melts at about 1,800 degrees

K, and it is a little bit difficult for me to see how you would have the majority of your corium at 3,000 degrees K when the steel pressure vessel melted out at 1,800 degrees K.

MR. SHERRY: Well, we are basing the temperature of the material may be at a higher temperature than the melting point of steel at the time the reactor vessel fails.

MR. SHEWMON: But 1,500 degrees K, have you ever done any heat transfer to say that you could have a pool of that temperature in contact with steel and still hold it there?

MR. SHERRY: I haven't done that, but that is only one part of the problem. The most important aspect of the steam explosion problem is the time when the molten core material falls from the core region into the lower reactor vessel head. An assumption is made that there is a pool of material in the core region itself.

MR. SHEWMON: In the core region itself it will not be above its melting point, that you can be sure of, because as soon as it gets to the melting point it starts trickling down, and once it gets to the bottom it pools further. And I guess I will only state this once more and then let it go until next time we get together. But my main point is that if you require superheat to get steam explosions, then I think you ought to look very hard at whether it is physically possible to get superheat; and I would be inclined to doubt it. And I don't see any evidence that you are taking advantage of that or the staff is in their consideration of the question.

Thank you.

MR. SHERRY: Your point is well taken. There is a large degree of uncertainty as to the mechanisms of the meltdown behavior in the vessel itself, whether a pool forms in the region of a core or the material suddenly trickles into the lower reactor vessel plenum is not known.

This slide shows the current estimates for the steam explosion containment failure probability and the estimates which were developed in the reactor safety study.

MR. KERR: What significance do those have in your view?

MR. SHERRY: The significance of these I think we can show on the next slide, if you will.

MR. KERR: Okay.

MR. SHERRY: This slide shows the relative contribution to risk, if you will, of the various containment failure sequences identified in the reactor safety study. This would be the relative contribution of steam explosions to risk using the estimates of steam explosion failure probability in the reactor safety study.

MR. ETHERINGTON: Steam explosion. Are you considering just the static pressure?

MR. SHERRY: The mechanism by which the steam explosion was predicted or thought to fail in the containment in the reactor safety study was through an in-vessel steam explosion leading to reactor vessel failure and generation of a missile with sufficient energy to penetrate containment.

MR. ETHERINGTON: Is it static pressure that generates the missile?

MR. SHERRY: No. It is an acceleration of a slug of material inside the reactor vessel which impacts on the upper reactor vessel and generates a missile.

MR. KERR: Presumably the work up to now has reduced the contribution by what, a couple of orders, three orders of magnitude?

MR. SHERRY: Two orders of magnitude for the best estimate.

MR. KERR: Do you feel pretty confident that has occurred?

MR. SHERRY: I feel pretty confident that has occurred for a PWR with a large high pressure containment design. I am not certain that the same results apply to BWRs with vastly different internal containment designs. We are currently doing a similar analysis with BWRs.

MR. KERR: At what point would you stop worrying about the steam explosion, at what point on that chart?

MR. SHERRY: With the probability of 10^{-4} leading to the contribution of risk on this. I think it would say steam explosions are not really even minor contributors to risk at this point. Other sequences totally dominate.

MR. KERR: So we are continuing to explore this because we haven't looked at all kinds of containments or because we are trying to establish further confidence in the result or what?

MR. SHERRY: I will address that in the next slide.

MR. KERR: All right. I will continue to be your straight man.

MR. WARD: Could I ask a question on that? A few months ago experts in the Swedish technical nuclear community came to the conclusion that was publicized that steam explosions were so improbable that they should not be considered.

Is that the same conclusion you are coming to here?

MR. SHERRY: No. I think their conclusion was --

MR. WARD: Could you give me a couple of minutes on that in layman's terms?

MR. SHERRY: Their conclusions were steam explosions are impossible. What we are saying is steam explosions are significantly improbable that they are not really contributing to risk.

MR. WARD: I am glad Okrent isn't here.

MR. SHERRY: I think there's a distinction there.

MR. WARD: Do you understand their reasoning and differ with it or --

MR. SHERRY: Yes, I do.

MR. WARD: To both questions?

MR. SHERRY: The experts who were involved in the development of the Swedish study met with almost all of the steam explosion researchers we have in the United States as well as in Germany and a number of other places, and basically they tended to use information which -- perhaps I shouldn't say what I was going to say, but in any event, we basically differ with some of their conclusions in the report.

Some of the effects which they indicate contribute to the very low probability or impossibility of steam explosions we had technical disagreements with.

MR. RYE: May I add a slight comment? This has been a very controversial subject for many years, and without using any names, I think the homogenous nucleation hypothesis, that whole area was kind of carried off in this evaluation; and there are many who do not accept this. They had a group of experts which were a one-sided picture.

MR. KERR: To put it so that I can understand, you disagree with the Swedes.

MR. RYE: Yes.

MR. ETHERINGTON: Does the word "explosion" involve an ultrasonic pressure wave?

MR. SHERRY: Yes. There is shock pressurization.

Beginning next fiscal year the title of the program will be changed to molten core-coolant interactions. The reason for this change is to emphasize that we will be investigating nonexplosive core-coolant interactions as well as steam explosion events. This is a redirection of the program. Consequently, there are going to be two tasks in this program, task one being a followon or a continuation of the steam explosion research. The experimental programs will be completed in 1982.

The programs we plan to do next year, the experimental programs are to conduct single droplet tests with the metallic and oxidic components of the corium separately to determine which of the components contribute to the explosion. This deals with the question Dr. Shewmon was raising. Since these two components will have widely different melting points, the one with the higher melting point will not be contributing to the explosion.

We plan to conduct a number of the intermediate scale tests in the fully instrumented test series using corium mixtures, and if funding allows we hope to conduct one or more larger scale tests with greater than 100 kilograms melt to assess the energy conversion ratios and mixing phenomena at a larger scale, and to aid us to extrapolate into full-scale conditions.

MR. KERR: What is an energy conversion ratio?

MR. SHERRY: The fractional conversion of the thermal energy in the melt to mechanical energy by the expansion process. You can view it as the conversion of thermal energy into melt through the acceleration of the materails surrounding the melt.

MR. KERR: It seems to me that would be enclosure dependent at least in large measure.

MR. SHERRY: It is.

MR. RYE: It is extremely dependent.

MR. WARD: What are typical numbers there?

MR. SHERRY: Typical numbers would be on the order of 1 to 2 percent. The maximum theoretical limit is on the order of, I guess, around 30 percent. The analytical programs in the task one steam explosion part of the molten core cooling interaction program will emphasize the application of the 1-D and 2-D models, the continued use and development of the statistical steam explosion containment failure methodology with the emphasis on BWR containment designs.

The second task in this program --

MR. KERR: Let me see. I want to make a

transition. If we talk about the '83 budget, which we are constrained to do, no experiments in '83.

MR. SHERRY: No steam explosion experiments.

MR. KERR: I thought that last slide said the experiments would be or that you said the experiments would be ended in '82.

MR. SHERRY: I indicated the program level had two tasks, a steam explosion task and a task involving the investigation of the interactions between materials where explosions are not predicted.

In task two we will be interested in such events as steam generation rates that break up under debris, debris formation, hydrogen generation, coolant contact. And in task two we plan to continue experiments.

MR. KERR: Okay. So in '83 there will be experiments, but by then steam will no longer be exploding; it will just be forming.

MR. SHERRY: We won't be doing experiments intending to investigate steam explosion phenomena.

MR. KERR: You will not be?

MR. SHERRY: We will not be.

MR. KERR: Okay.

MR. SHERRY: At least we don't plan to at the present time.

As I indicated, in task two --

MR. KERR: The part that you are talking about fits into this, into the part called fuel melt.

MR. SHERRY: Yes.

MR. KERR: And it represents what fraction of that roughly -- 1 percent, 10 percent?

MR. SHERRY: I think it's on the order of 4 or 5 percent.

MR. KERR: Four or five percent, okay. Continue.

MR. SHERRY: Maybe someone could guess since I don't recall what the total number is there.

MR. KERR: The total number is about --

MR. CURTIS: Dr. Kerr.

MR. KERR: Yes, sir.

MR. CURTIS: At this time it is more than that. It comes out to about 10 percent.

MR. KERR: About 10 percent of the FY 83 budget?

MR. CURTIS: No.

(Pause.)

In 1981 it represents --

MR. KERR: No. I'm interested in '83.

MR. CURTIS: His number then is probably about correct.

MR. KERR: All right. Thank you.

MR. SHERRY: That concludes my presentation.

MR. KERR: Are there questions?

(No response.)

MR. KERR: Thank you, sir.

Harold, I want to correct your earlier statement which I assume as a bit of an irony. I don't think things have gotten simpler. I think our misunderstanding is now more complicated than it was.

(Laughter.)

MR. KERR: I show Mr. Powers, who has been patient, to talk about fuel melt experiments.

MR. POWERS: I will talk to the committee about the two programs dealing with materials interactions that can take place outside the reactor pressure vessel that Dr. Curtis mentioned before the lunch break.

The two programs to be discussed are the molten core containment program, which is a study of material interactions with core debris and candidate retention materials and concrete, and how these may threaten the containment integrity. The second is the core retention concept assessment in which the engineering of a core retention device that might either terminate or mitigate the ex-vessel interactions that threaten containment could be used as a mitigation device.

MR. KERR: Mr. Powers, could you also give me some indication of what fraction this 83 line item?

MR. POWERS: No, sir, I couldn't. I simply don't know.

MR. KERR: Can someone give me some estimate of what fraction that Mr. Powers is discussing? I don't have to have it right now, but if you could just give it to me.

Please continue, Mr. Powers.

MR. POWERS: In the course of the presentation I will try to indicate why this work is being done, what I think we know about the ex-vessel interactions.

MR. CURTIS: Fifteen to 20 percent.

MR. POWERS: And finally, where I think this work is going over the course of fiscal year 82 and 83.

To illustrate the threat being considered in the ex-vessel interaction, I have here a rather idealized sketch of a reactor containment. In the course of WASH-1400 it was recognized that once core melt occurred it could progress sufficiently far that the melt could come out of the reactor pressure vessel and fall into the concrete sump.

In WASH-1400 there was a great deal of concern that the concrete would be attacked by the melt, and eventually you would get erosion of the concrete and a loss of containment integrity.

In the course of looking at this work we also recognized there were other mechanisms involved in the ex-vessel material interactions that threatened containment. As the high temperature melt attacks concrete, it liberates quite a lot of gas, quite a lot of aerosols. It produces hydrogen and a lot of heat comes up which can contribute to either overpressurization in the containment or in the case of hydrogen, detonation due to the failure of the containment. And it is in fact these factors of ex-vessel interactions that contribute to an above-ground level failure of containment that seems more of a concern than a simple erosion of the basemat.

So we are studying. We began to look at ex-vessel interactions first because they are the driving force of the reactor accident once the material has left the vessel. In other words, the phenomena associated with these ex-vessel interactions are what drives the accident forward.

Our biggest concern are those phenomena which contribute to an above-ground containment failure. That is either pressurization of the reactor containment or contributing to hydrogen and to a possibility of detonation that would fail containment. To a lesser extent one is concerned about containment failure due to the basement being eroded. However, this would result in a groundwater release of radioactivity which would probably take place on a much longer time scale than above-ground failure and would be more susceptible to intervention.

There is another factor to be concerned with in the ex-vessel interaction, and that is they do interfere with other mitigation devices. For instance, the interactions produce quite a lot of aerosols, quite lot of noncondensable gases. These might interfere with containment filterings, the venting systems or containment coolers.

Finally, the interactions do generate quite a lot of radioactive material in the form of aerosols and to contribute to the release of radioactive inventory into the containment.

MR. KERR: Mr. Powers, tell me what it is, what is the ultimate objective of these studies? What are you going to do with the information you get?

MR. POWERS: The information we get is used in at least two distinct ways. One is for just assessing what kind of problem you face in the event of a severe accident where you overpressurize containment. Do you have so much hydrogen it would detonate?

The second use is to decide whether you need a mitigation device of some type. And if you do decide you need it, how you might go about designing it. It would be concerned about the load. Such a mitigation device might have to handle, for instance, a filtered vent system would have to handle filter aerosols that are radioactive. This interaction produces huge quantities of these aerosols. By understanding it you know how to size that filtering system. For hydrogen igniting systems you need to know what kind of inventory of hydrogen you have to handle.

MR. KERR: Are you attempting to establish a spectrum, an upper bound, what?

MR. POWERS: The experimental work concerns itself with just establishing the kinds of phenomena taking place in sufficient detail that a modeling effort can be expected to yield reasonable results for particular accidents. In other words, clearly in an experimental program we can't do a meltdown as big as the whole plant, but we can do enough exploration of the phenomena to give qualitative models that can be used to extrapolate onto real accident situations, and there they would do a spectrum of accident situations with the modeling.

MR. KERR: Again, this is part of my question. Is an effort being made to find out what is the highest pressure that could be generated by ex-vessel interactions or what is the largest amount of hydrogen, or is it an effort to find out whether hydrogen will be generated?

MR. POWERS: It's the latter category of things that are done in these programs. One of the first findings of the experimental work was quite right, hydrogen was generated, as opposed to WASH-1400.

MR. KERR: Was that a surprise that hydrogen would be generated?

MR. POWERS: It was not considered in WASH-:400.

MR. KERR: That's not the question I was asking. My question is do you have to do a new experiment to determine that hydrogen is generated when you drop hot metal on concrete?

MR. POWERS: I think what you do, you experiment for us to find out how much hydrogen is generated, not only the qualitative b: : the quantitative features, different temperature exchanges with different types of material interacting, different types of concrete.

MR. KERR: And you can pretty well establish by a few experiments something typical so you can model and get a good idea?

MR. POWERS: You try in your analysis to be able to first of all recreate the experiment, to predict the experiment, and know that experiment well enough that you have some confidence that your extrapolation up to several hundred tons of material interacting is in fact reasonably done.

Does that answer the question?

MR. KERR: I'm not sure, but that is because I'm not sure what the answer is.

MR. WARD: Dana, the last item there, you aren't really make any more curies of anything, but new chemical and physical forms?

MR. POWERS: No. I will show you in a few minutes some pictures and you'll see the gases flowing through this melt to the concrete. It will give you much more opportunity to release this reactivity, first of all by sparking it out of the core melt and also by changing the chemical form to a more vaporous situation.

The questions of ex-vessel material interactions come up regardless of whether you provide some protection to your plant through mitigation devices or not. Here I have listed the issues for a plant as it exists now with no mitigation systems, gas generation associated with the ex-vessel interactions, the production of flammable species being transferred up into containment, raising the containment atmosphere temperature and consequently its pressure, the generation of aerosols and basement erosion.

A subset of these become important if you put a filtered vent system on the reactor. You still worry about how much gas generation has to be contended with, the production of flammable gases, and the production of aerosols. Were you to protect it with a core catcher, you would still be worried about upward heat deflects from the melt aerosol generation and the erosion of that.

MR. SHEWMON: Your core-catchers at still dry, is that right?

MR. POWERS: Core-catchers might or might not be dry.

MR. SHEWMON: Here they are dry presumably, but upward heat transfer is by steam or radiation.

MR. POWERS: It could be either one.

MR. SHEWMON: You probably wouldn't generate much H_2 CO of CO_4 though if it was by steam, is that right?

MR. POWERS: If it was by steam you could have quite a little H_2 because of the reaction of water with the core melt materials that produce hydrogen.

MR. SHEWMON: And you think all of that would be so cold you wouldn't burn any oxygen, or it would be completely depleted in oxygen by then?

MR. POWERS: If the melt cover was by steam there would be no oxygen in the interaction zone. There might be oxygen in the containment atmosphere, but your steam pressure would be so high you probably would not be within the flammability limits, so it would just probably burn. Steam suppresses burning of hydrogen-oxygen mixtures fairly effectively.

To give you some idea of what our understanding in the area of concrete interactions are to date, I have plotted here temperatures of a hypothetical core melt versus the time it might interact with the concrete. This time scale is very nonlinear. The temperature scale is approximately linear and is marked by two temperatures, critical temperatures.

One is where the core melt material would be expected to solidify. The other horizontal line is where the concrete would no longer be eroded by a core melt.

For a particular accident scenario we can hypothesize the melt traveling through five distinct temperature phases. When it first begins to interact with the concrete it could be at what we call the ultra-high temperature phase where it has a substantial amount of superheat, and its interaction with the concrete is so vigorous it is difficult to define a distinct --

MR. SHEWMON: Do you ever get into how it got so superheated?

MR. POWERS: We have considered how we would get into this particular situation. It is still an area of great uncertainty how melts coming down onto the reactor pressure vessel will ultimately fail that pressure vessel; but certainly, one of the considerations is that a melt catastrophically fails onto the pressure vessel and forms a crust. You have a substantial --

MR. SHEWMON: How hot was it when it failed down onto it?

MR. POWERS: It could be anywhere between 1,700 degrees Centigrade, which is the lowest eutectics temperature Dr. Picklesimer mentioned this morning, and somewhere between 2,400 and 2,600. There's not a good estimate on what the highest temperature you can get in the core is prior to failure, since we don't know the mechanisms very well.

MR. SHEWMON: It would stay solid until 24 and then all of it would collapse at once at 2,400 degrees C. and go straight down to the bottom? Is that the model?

MR. POWERS: Certainly that model has been presumed. It is not staying solid. What happens is a crust forms around the parameter of the melt. It cruciblizes. There is enough structural material in there --

MR. SHEWMON: Is that the center part where the heat is generated, or do we, for convenience, including the whole core?

MR. POWERS: Most meltdown mechanisms have included pretty much all of the core simply out of ignorance. They didn't know any better.

MR. SHEWMON: Well, out of ignorance of elementary heat transfer, too, because you know the thing will be radiating to the outside to a core which is pretty cold.

MR. POWERS: That's how the crust gets generated.

MR. SHEWMON: So now you have only the center part which is coming out.

MR. POWERS: That's a substantial portion of the core.

MR. SHEWMON: That hits the bottom of the pressure containment and cools.

MR. POWERS: Mechanistically, people -- and again, I think I would class this into what Dr. Kelber called informed speculation -- where they hypothesize that this amount of molten material collecting within this crust [illegible]es some critical limit where the crust can no longer [illegible]d it in place and then fails catastrophically.

MR. SHEWMON: And that drops into the puddle of water.

MR. POWERS: In the lower plenum where there may or may not be water as a liquid pool down there. We have done some experiments on melt streaming onto steel structures of that type and have found that that streaming operation is extremely aggressive, and in fact, we have observed penetration rates such as the pressure vessel walls would be penetrated in something less than 30 seconds. But if one presumes it's not, a crust would form simply because the interface temperature is quite low.

MR. SHEWMON: It would dribble out. I guess what offend me some is I wonder really to what extent you are ignoring the rudiments of heat transfer to get where you want to go, and if you are ignoring all the water that people have had in this darned plant beforehand to make things active enough to do experiments in a convenient period of time.

MR. POWERS: I think not. I think we can come up with mechanisms that are fairly reasonable for having rather high temperatures. I think that is not one of the least justifications or the experimental program Mel Silberberg outlined for you earlier today in that we don't know much about melting within the vessel itself and how it eventually could come out of the pressure vessel.

MR. SHEWMON: I look forward to Wednesday.

MR. KERR: We do know the likelihood of simultaneous melt-through around the circumference of the vessel is fairly low, don't we?

MR. POWER: Simultaneous melt-through around the waist of the vessel?

MR. KERR: Yes. Or is there a fairly high probability of that?

MR. POWER: I have an opinion on that mechanism of failure but it is just an opinion, and there are many advocates of it. Quite a number, including Zion-Indian Point, advocated that as a failure mechanism and it is included in the German risk assessment as a failure mechanism. I personally feel that melt streaming onto the steel itself will fail it locally rather than a simultaneous melt-through around the waistline, but that is just an opinion.

I will emphasize that this is but one scenario for illustration purposes of how the temperature of the melt would behave in the reactor cavity. The areas where experimental effort have been done, both in the United States and in West Germany where the predominant amount of experimentation is done, in those two countries, has been pretty much concentrated in what is labeled here the high temperature phase, progressing, perhaps, into the point where the melt has actually solidified.

In this high temperature phase there is a completely liquid fuel pool attacking the concrete and it is dominated by the gas generation. This influences not only how heat transfers from the molten pool to the concrete; it also affects the upward heat release, the amount of hydrogen production and the amount of aerosol production. It progresses into the solidification phase, which could be quite long, because not only do you have internal heat generation, but you have liberation of the heat of fusion.

There is only a small experimental base. Eventually one would hypothesize the melt to solidify but still be at a temperature above the concrete ablation temperature. In this what we have labeled here the low temperature phase, there is a small experimental data base, mostly scoping pipe experiments, and not enough quantitative data to extrapolate. It is, in fact, this phase which will indicate whether the basemat is eroded or not.

Finally, there is a very low temperature phase at which point the core melt drops below the temperature at which concrete ablation can occur. So there is no longer any erosion taking place but there can still be gas generation taking place.

As I indicated, most of the experiments have been done in the high temperature phase up to now, and this is a photograph of a particular experiment. I think you can see in it most of the safety-related questions addressed in this type of research. There is a tremendous amount of aerosol being created. There are gases coming off the melt which in this open air experiment are flammable and promptly ignite, and there is certainly concrete erosion taking place.

The experimental program we have seeks to address in the next year those areas that have not been addressed up to now.

MR. KERR: When you say in the next year, are you talking about fiscal '82 or '83?

MR. POWER: I am talking about fiscal year '82. In the first item I have culled out here is the attack of the solidified core debris on the concrete. We do know this erosion continues to take place even when the core debris has solidified and temperatures remain above the temperature of concrete melting.

MR. KERR: Is the assumption here there is no water around or will you be doing it with and without?

MR. POWER: Yes, sir, we will be doing it with and without water. I have culled that out in item 4 where we look at both circumstances. The area of water involved in the debris has been categorically neglected in experimental programs largely to date. There has been one scoping experimental series done in Germany, and we have at

Sandia done some small tests involving sodium coolant, but that is clearly one of the bigger omitted areas on this subject of ex-vessel interactions.

The next area I have culled out is large-scale UO_2-zirconium melt interactions on concrete. This is an area which has, again, been neglected. Predominantly experiments have been done with simulant materials like stainless steel or very small UO_2 melts. We now have a capability I will discuss in a few minutes to do larger-scale interactions, and we would begin at this relatively high temperature phase here.

This work complements the beta experiments Mel Siberberg mentioned to you earlier. The beta facility has the capability of working with only steel and steel-aluminum oxide mixtures. We will be trying to do some complementary experiments involving real fuel materials, which is a mixture of UO_2 and zirconium dioxide in the clad.

The third item I have culled out is definition of the aerosol source term. There is a growing data base on how aerosols behave once they are released to the containment but some uncertainty about the aersol's source that gets injected into the containment. We generate the aerosols in this kind of research whether we want them or not, and it is part of this program to characterize that aerosol source.

Aerosols are particularly interesting as far as ex-vessel material interactions. They contribute not only to the radioactive release from the fuel but they also affect the natural or perhaps the designed mitigation systems that might be included to protect the reactor containment.

Aerosol release occurs predominantly by three mechanisms: just the normal vaporization, which was all that was considered in the WASH-1400 analysis; chemical transport, where the chemical species is changed into very volatile materials; and mechanical breakups as gases blow through the melt.

Once these aerosols are released into containment, they can coat any heat transfer surfaces introduced passively or deliberately. They can plug orifices and clog filters that might exist in mitigation systems, and there is some possibility they would alter the atmosphere by condensing steam or interfering in hydrogen burns.

A fourth area I have culled out, which we have already mentioned, is the combined interaction of coolant, core debris and concrete. It is very likely in a real accident the coolant would either be inadvertently added on to the core melt while it attracts concrete or deliberately done so as a mitigation scheme.

We intend to do experiments in three general categories. One category depicted here in the center somewhat schematically is the effect of coolant when a molten mass of material is attacking the concrete. This is clearly a direct fall on where the experimental data base exists today.

Another possibility would be the case of fragmented debris drying out in contact with the concrete while inundated with water. This would be the situation immediately following a steam explosion, which was just discussed. The debris would collect in a pile. It might not be coolable and would begin to re-melt and attack the concrete, which in itself might affect whether it was coolable or not.

The final type of experiment we would do would involve the hot solid debris attacking the concrete with this liquid concrete flowing around shielding the debris itself from an overlying pool of water. Up until now we have talked about the melt-concrete interactions. If one concedes those are not desirable things to allow to occur in a reactor accident, one turns to materials that might be put into a reactor in place of concrete.

There are quite a lot of core retention systems which have been suggested in the literature. As part of these programs we have looked in a scoping sense at several of them and identified two that deserve further study. Here

I have listed some of the more popular schemes which have been suggested which we have examined.

One is to put in a sacrificial pool of borax which would dissolve and dilute the core materials to t point the temperature was low enough to contain by conventional means. Another core retention concept is simply to put a large steel or copper plate down and cool it with flowing water. Castible ceramics have been suggested to replace concrete. They are quite similar to concrete. They are cast just like concrete but they are very refractory.

The combined use of the sacrificial material to dissolve the core debris and a refractory material to contain it has been suggested, and we have looked at this in a scoping sense and find that rather than combining the best features of both concepts, you tend to get a magnfication of the worst features of both concepts. Rubble bed devices consist of refractory gravel material that could be put into existing reactors as a retrofitted device, refractory gravel of the type of thorium dioxide. This would allow you to inject coolant both under and around the melt as well as over the top as a core retention concept, and it could be easily retrofitted into existing reactors.

Finally, magnesium oxide bricks have been suggested.

MR. KERR: Easy to retrofit in what sense?

MR. POWER: In the sense that one does not have to do a lot of construction work in the radioactive environment in the reactor cavity.

MR. KERR: You could just take wheelbarrows?

MR. POWER: That is right. It also allows you to retrofit because there is not a lot of room in existing reactor cavities, and to avoid having to mine out or with some other very elaborate kind of construction effort.

Our initial thoughts on these gravel bed experiments is they would be disrupted by the melt coming out of the reactor pressure vessel, particularly when it came out at high pressure. We have got enough scoping experiments to convince ourselves that is not a major problem, and where it does occur you could by very simple construction get rid of it, and simple construction can be as simple as piling the gravel up at the angle of repose so there is simply no place it can move to.

At any rate, we have identified the rubble bed device and its potential as a retrofit into existing reactors and magnesium oxide refractory materials because of their capabilities for future plants as areas we can concentrate on next year.

MR. KERR: What would the rubble bed be made of?

MR. POWER: Thorium dioxide, so it is heavier than the core melt material and doesn't float through it is the idea, and at the same time for a refractory. You get a certain amount of dilution capability because of the miscibility between the UO_2 and the thorium dioxide for what fraction of it you do melt.

Part of the capabilities we have been developing over the last year to investigate these core retention devices is the large melt facility. We need for this experimental effort to be able to evaluate engineering concepts. That requires fairly big experiments, I think fairly large and prototypic melts. This large melt facility shown in the photograph here would allow us to melt up to about 500 kilograms of this UO_2-CrO_2 mixture.

Some of the elements of the large melt facility are perhaps better seen in this artist's sketch. The upper chamber is a furnace region where we can prepare the melts. The lower region is an experimental chamber. Once the melt is formed, it is tapped with a tapping projectile here and drained into the lower chamber where it can interact with a core retention device or some engineered structure.

We have planned for the immediate future tests with UO_2-ZrO_2 mixture melt, and an MGO core retention device. Again, the emphasis on the test is on the engineering of this device, and here I have listed some of the things we hope to learn from the experiment.

First and certainly not least is material interaction. There does seem to be a strong eutectic reaction between this UO_2-ZrO_2 mixture and MG_2 that affects erosion. We have found in small-scale experiments that bricks are subject to cracking and spalling. We are concerned about this as a failure mechanism and also that the melts can flow through the cracks and penetrations.

MGO bricks are lighter than the core debris and they might have a tendency to float. This is fairly well avoided by construction techniques since MGO is used routinely for steel melts. The melting, crusting behavior of the melt and the aerosol source term are other things that should come from the experiment.

I would conclude my presentation with a summary of the status of the two programs. In the area of core debris-concrete interactions, the data base is predominantly in the area of liquid melt interacting with concrete, and in this situation gas generation is the predominant phenomenon, gas generation both with respect to its mass and the fact that some of it is flammable.

The aerosol source term is still not especially well defined and is a part of the future program. The program will continue on to expand the data base to include hot solid core debris interacting with the concrete, the effect of combined coolant, core debris and concrete interactions, and a large-scale oxide melt interacting with concrete.

In the area of core retention devices --

MR. KERR: What is the significance of the last one, ultra high temperature oxide, concrete uranium oxide?

MR. POWER: Predominantly experiments in the past of any kind of scale at all have been done with steel and they have not included the fuel oxide itself.

MR. KERR: Is that the significance of the ultra high temperature?

MR. POWER: The ultra high temperature is to indicate it is up where the oxide is molten.

MR. KERR: Thank you.

MR. POWER: In the area of core retention devices, several concepts have been examined in this scoping fashion. In the next year we will be looking at the performance and engineering evaluation of two types of devices, rubble bed devices and the refractory brick devices.

MR. SHEWMON: Does the rubble bed device have water in it?

MR. POWER: Yes, sir.

MR. KERR: And the next year is fiscal '82?

MR. POWER: Yes. Yes. The key to the rubble bed device is to have water flowing through this gravel below the core debris itself, so you have coolant all the way around the debris.

MR. ETHERINGTON: I have two questions. The previous subcommittee raised the question of whether there might be any reaction between the UO_2 and iron. If there were, of course, it would affect the melt-through of the vessel and it would also have an impact on these nice layers you have in the model of the melt-through.

I found oxygen pressures of UO_2 from the Sandia publication. I couldn't find oxygen pressures for FeO at high temperatures. I have a very limited library in Jupiter, Florida, and I found that hyperstoic eutectic UO_2 ought to oxidize iron to FeO even with as little UO_2 as .03. I would think that ought to be worth looking at. I got a complete blank when I raised the question at the meeting before.

MR. POWER: There are several studies. They tend to fall in the category of do steel and UO_2 wet. That is usually the way the question is posed, but they are asking the same question you have asked here.

MR. ETHERINGTON: The answer to that question would have a very important bearing on both those two things, the melt-through of the vessel and the behavior of the layers.

MR. POWER: Exactly correct.

MR. ETHERINGTON: Where do they stand? It seems it should be given a lot of impetus.

MR. POWER: There are in the United States two groups I know of who are definitely looking at it. In Germany there is one group looking at this wetting problem. This is, incidentally, out of the fast breeder context and none of the light water context. This work was funded. Where they stand right now is confused. They are looking at stainless steel as opposed to pure iron.

They find there is perhaps a third phase when you interact those two. There is probably a combination of chromium-manganese, manganese oxide, which are much stronger oxygen getters than would be the iron itself, and the third phase does definitely wet UO_2. The metal phase wets it, but the metal phase does not seem to ever directly wet the UO_2.

MR. ETHERINGTON: If you had any incipient reaction with FeO, you would probably get immediate wetting, wouldn't you?

MR. POWER: If you did initially, the chromium takes oxygen preferentially to the FeO of stainless steel, and that reaction seems more critical than does the one with hydrogen oxide. Interestingly enough, there seems to be a difference between the 316-type stainless steels, which have a little molybdenum oxide in them, and the 304s which do not. The 304s are much less susceptible to wetting than 316.

MR. ETHERINGTON: I am glad to hear that is being looked at.

The second question. Are you going to give me some kind of an answer on what evidence there is that we don't get spalling in large-scale bores?

MR. POWER: When we have added high temperature melts onto concrete, we have in every case observed spallation, spallation in a very specialized sense. What we see is the surface material spalls off to a depth of 4 millimeters in every case, and it seems to occur universally across the surface. Once the surface has spalled, we have never observed another spallation event occurring at the location that has spalled.

We have on the staff people working on this experimental project whose job it used to be at one of the specialty steel companies in New Jersey -- it escapes me right now, but his job was when they had a bad pour, he poured it onto the concrete slab and cooled it with a fire hose afterwards.

We talked to him quite a bit about the spallation, and he said his experience and the experience of everyone he had known who had asked about this was yes, there was a surface spallation that caused the metal to bounce around quite a bit. When they got large chunks of spallation it was with some part of concrete that was hit that had no reinforcing bar in it at all, like a step or a lip on this trough they poured into. Otherwise all they saw was local spallation.

We speculate what is happening is in the gravel region below the surface layer of concrete there is enough give that stresses don't build up fast enough to beat the fact that the concrete is starting to melt, and it relieves itself.

MR. ETHERINGTON: I am a bit unconvinced, especially steel manufacturers talking about a few hundred pounds of steel, probably. What I am suggesting is it wouldn't cost you anything except an airline ticket to go to some fairly high technical individual in U.S. Steel and ask him whether he from the knowledge of any of his people can tell you anything about any past experiences they might have had.

MR. POWER: As I said, we have relied on the fellow involved in his. His experience is with melts from 5 or 6 tons up to 20 tons.

MR. ETHERINGTON: That is not a small quantity of steel.

MR. POWER: No, not at all.

MR. ETHERINGTON: Did he pour 20 tons on concrete?

MR. POWER: I never asked him. He said a huge block, and a typical one would be 5 to 20 tons.

MR. ETHERINGTON: You don't pour 20 tons of steel deliberately on concrete ever. Sometimes it runs through the bottom of the furnace and then you have a hell of a mess.

MR. POWER: On occasions when they have melts that had gotten disastrously off chemistry, they have to dump them because there is now way to get back from where you have been.

MR. ETHERINGTON: And if you don't like one man's opinion you can get another man's opinion.

MR. POWER: Yes, yes.

MR. ETHERINGTON: I raised the question in a similar context about designing in connection with a nuclear plant, the design of the refractory superstructure. I suggested going to someone who had designed furnaces, or rather the committee made that suggestion, and we got a rather smug reply giving the credentials of the man they had. But it seems to me --

MR. POWER: It is extremely important.

MR. ETHERINGTON: It seems to me there is a pool of potential information worth tapping. Some of these old practical birds who don't know anything at all about heat transfer may know more than we do about some of these other things.

MR. POWER: Especially in the area of designing refractory brick constructions it is essential.

MR. ETHERINGTON: If that writer comes back I think that question will be raised again because in that particular case there was obviously no expertise brought to bear.

MR. POWER: It is foolish to ignore these people because the practical experience with brick does not get documented. It is not the sort of thing you read about in books.

MR. KERR: Are there other questions?

Mr. Powers, could you tell me how to separate what you are talking about that goes in the fiscal '83 program and the rest of it? Because one of our responsibilities is to try to comment on what you plan to do in fiscal '83.

MR. POWER: I think much of this work we are going to try to -- we always try to complete as much as we can in '82, but some of it will necessarily carry over. The area I expect to have the biggest amount of carryover into '83 will be this area of combined interactions.

MR. KERR: If what you are telling me is that you are going to do in '83 what you don't get around to doing in '82, that is enough.

MR. POWER: That is what it is, and I would say here I expect a lot of this to show up in '83, some work in '82 but most in '83. There will probably be some carryover here in the aerosol source term and undoubtedly down here (indicating).

MR. KERR: So you are going to keep on doing what you have been doing but do it better?

MR. POWER: That's right.

MR. KERR: Thank you.

My agenda shows Mr. Larkin, who has been very patient, and I will ask him to be patient for another ten minutes because I want to have a break. Then Mr. Curtis and that is it; is that correct?

MR. CURTIS: Yes.

MR. KERR: If you will bear with us, Mr. Larkin, I will declare a ten-minute break.

(Recess.)

MR. KERR: Mr. Larkin, the floor is yours.

DR. LARKINS: My name is John Larkins. I am in the Division of Accident Evaluation and I am the project manager for those programs and divisions associated with hydrogen. There are three main programs in hydrogen, combustion gas and containment hydrogen burn survival, which is actually equipment survival, hydrogen behavior and control program, which is the major program. Information is also derived indirectly from programs on molten core interactions, steam explosions and core melt technology, which give us the hydrogen source terms from other areas.

All of these programs also impact on the work on containment analysis and safety margins for containment.

The equipment survivability program is a program which is funded both by the Office of Research and the Office of Nuclear Reactor Regulation, first the experimental portion being funded by Research, and the analytical portion being funded by NRR.

This program was recently initiated to assess the effects of hydrogen burns on equipment, and particularly related to safety-related equipment and equipment associated with the isolation of the containment. Initially the program will focus on the near-term operating licenses of MARK III types of containment, and we will initially just look at the effects of deflagration as opposed to detonation.

Currently we are generating a list of equipment that will be exposed to the various environments from hydrogen deflagrations. The environmental envelope in which this equipment will be exposed will come out of the experimental and analytical program on the hydrogen behavior control.

MR. KERR: Mr. Larkins, if I were trying to fit you into this four-line budget thing I have, are you in Accident Mitigation?

DR. LARKINS: Am I in Accident Mitigation?

DR. CURTIS: Yes. For historical reasons, he is both in Fuel Melt and in Accident Mitigation.

MR. KERR: Okay. I am trying to get some idea of how many bucks he represents. Is there some way I can do that or is that indeterminate or a percentage or something?

DR. LARKINS: I could give you an indication of the level of each of the elements after the presentation if that would help you.

MR. KERR: All right, good enough.

DR. LARKINS: The test will also include the effects of multiple burns to assess the effects of turning on and off igniters at various times, and as I mentioned, we will be using some of the experimental facilities from the hydrogen behavior and control program for this effort. We are assimilating a list of equipment we will be testing, and hopefully we will initiate testing sometime in the beginning of FY 82, and the program will be continuing through FY 83.

MR. KERR: Is this in a sense a kind of an environmental qualification where the environment is burning hydrogen so that you are doing prototypical testing?

DR. LARKINS: Yes, to the best of our knowledge we will be trying to simulate the environments initially from deflagrations for different concentrations of hydrogen, and either simulating or placing the equipment in an actual burn situation or simulating it with radiative heat facilities or things along that line.

MR. KERR: In other areas you are having

manufacturers and utilities and things do a lot of environmental qualification for the things inside containment.

DR. LARKINS: Yes.

MR. KERR: Why are you not having them do this or why are you not doing what you are asking them to do in the others? I am trying to understand how you decide.

DR. LARKINS: There are other programs in the agency which look at an assessment of the qualification testing program done by the industry. However, the industry right now isn't doing very much in this area at all, and there are some questions which must be answered in a reasonable time.

MR. KERR: Is the idea you will do scoping studies here so that you can know what to require industry to do?

DR. LARKINS: That will be part of it. We hope to develop analytical tools so we can do an assessment of the various types of equipment which are in the plant; if there are problems, then require the industry to do further testing themselves.

MR. BASSETT: Dr. Kerr, we are separately looking at the qualification picture in general. Our engineering division is taking a broad-based look to see how effective the qualification procedures that have been used by the industry have been and how much is really being done and how much needs to be done further. This has come up since that started, and we know this has not been addressed in qualification programs. We wanted to see if it falls in the envelope of normal qualification or if it requires extra testing.

MR. KERR: Why are you doing this rather than having the industry do it? Because they are too busy or that you don't trust them or that they cannot do it?

MR. BASSETT: By dividing it first into the normal qualification, we are looking to see whether the industry tests are indeed as represented and if they do represent equipment that will operate under conditions of accident. We have had some insights since TMI that we need to look at some special things, of which this hydrogen is perhaps the most important, and we know they have not looked at the effects of hydrogen.

MR. KERR: I think I recognize and agree with the importance of this. I am trying to get an understanding of how you decide what part of it you do and what part of it industry does. Is this being done on a preliminary basis to sort of establish techniques and approaches and then eventually you are going to require the industry to do qualification testing, assuming you decide a hydrogen burn is a design basis event; they do qualification testing for hydrogen burns, or will your test provide the answers that are needed maybe?

MR. BASSETT: I think our program will determine whether this is a problem or not, and it would be our intention to require industry to do the necessary qualifications. One of the things that we think we will find out from this program is whether the normal qualification testing fits within the envelope of what would happen with hydrogen. We suspect it will not. That is why we want to [illegible] this program.

MR. KERR: Thank you.

MR. SIESS: Temperature being the prime one which would fall outside the normal range?

MR. BASSETT: I would like John to answer that.

DR. LARKINS: It is temperature initially but we look at pressure effects. When we get into detonations we look at localized pressure effects on equipment, but that is a little further on. A separate element of the program is combustible gas and containment.

Here we are looking at the source of hydrogen from the corrosion and oxidation of zinc and other organic coatings on containment. There has been some work done in this area. However, it is felt by the regulatory staff that work needs to be done in a more controlled manner and also to look at other effects, possibly synergistic effects of things like boric acid, sodium thiosulfate, additives and sprays.

What we propose to do is study these corrosion rates and extend the formation of hydrogen over a series of temperatures associated with post-LOCA conditions.

MR. KERR: Will this be primarily experimental, primarily a collection of information?

DR. LARKINS: Early on they did an assessment of the amount of material in a plant which could generate hydrogen, such as zinc, aluminum and other organic coatings. Right now what we are doing is they have a small experimental program which is looking at the kinetics for the oxidation of zinc with steam to develop baseline kinetics. Then they will move into steam with other additives in it to get other information to develop tables for the formation of hydrogen at various conditions.

MR. KERR: Some of the other activities, did they present or discuss risk production associated with activities? Have they looked at the risk production associated with this activity?

DR. LARKINS: I'm not sure. I can't answer that. I don't know whether anyone has or not. I know this particular activity right now, the amount of hydrogen that can be formed from these materials in containment is the basis for the design or specifications for the hydrogen recombiners, so it may be something they want to associate with that.

MR. KERR: In a sense to determine the capacity requirement of recombiners, do you mean?

DR. LARKINS: Yes, the capacity, the amount of gas recombiners can handle at some period in time. The main part of the program is called hydrogen behavior and control program, wherein we are attempting to assess the rates from hydrogen deflagrations and deformations from several classes of plants and containment types, assessing the adequecy of current equipment and developing new, improved mitigation detective systems, developing a manual on proper strategies and training, and also developing information for codes in addressing the transport of hydrogen containment.

The experiment is both analytical and technical. I will go through the main parts. This is a listing of the accomplishments to date, well, through the end of this fiscal year. We have developed a compendium on hydrogen formation handling, combustion limits, how to handle or treat hydrogen. The Hydrogen Detector Report is being published. The Department of Energy is going to take this information and use it to assess or review different types of hydrogen detectors and make recommendations.

There was a workshop to discuss problems associated with hydrogen, which was attended both by people from the states and from foreign governments.

MR. KERR: Was that the one held in Albuquerque?

MR. LARKINS: Yes, in January, right. A report has been published on the Sequoyah mitigation which looks at different mitigative schemes using things like halon, water fogs, inertings. Both of these other items listed here I will discuss briefly as I go through the presentation.

The experimental program is designed to develop a technical base to develop models to get a better understanding of the phenomena on which we do not have a good handle right now, on things like accelerated flames, to evaluate the equipment survival evaluation schemes, and lastly to get a handle on the effects of scale in going up from the bench type to intermediate and large-scale effects to see how things scale up.

MR. KERR: These are earlier analytic and experimental work on what? Hydrogen burning, hydrogen explosion, hydrogen generation?

DR. LARKINS: Both deflagration and detonation studies, yes. And also things like hydrogen transport.

MR. KERR: I would have thought, except for some of the quirks of reactor containments like steam, there existed quite a lot of work in that field, but apparently it is not applicable or is inadequate or something.

DR. LARKINS: Well, there is quite a bit of work that is applicable but it doesn't cover all of the conditions for mixtures of hydrogen and steam. In the experimental program we use a number of facilities. We have done laboratory scale tests where we have looked at the effects of things like fogs and sprays and foams on inhibiting both the flame propagation and inhibiting the effects of deflagration to detonation.

We have available a 16-foot tank which is part of what we call the Variable Geometry Experimental Series. We have done two series of tests, approximately 35 tests where we vary things like hydrogen concentration. We looked at the effects of turbulence induced by things like fans. We looked at the effects of igniter locations, the different types of igniters currently, modifying the fully instrument test series tank which is currently being used in the steam explosion work to handle both hydrogen, steam and other additives, to study both deflagrations and detonations.

We are putting together a steam/hydrogen jet experimental setup to study auto-ignition, to get a handle on the ignition of gases that could be released from things like high point vents.

MR. KERR: Are you telling me what is going on in fiscal '81 and '82 or what will go on in '83?

DR. LARKINS: There is a series of tests we are completing in here. We are continuing work in the 16-foot tank in '82 and '83, the fits tank in '82 and '83. I think it will become clear as I go through it.

We have completed a series of tests in '81 on the plastic bag setup which allows us to do different geometries.

MR. KERR: I take it since I see McGill down there, not all of this is in one physical location. Is this in four laboratories or two or --

DR. LARKINS: Right now all of this work is centered at Sandia. There are some programs which are not in place which may be other places, but I will discuss that in the end.

The McGill work you point up is to look at the specific problem of accelerated flames. They will be using confined geometries and strong and weak igniters to see what effect this has on accelerated flames.

This last dot down here which says "plant scale" is the potential for looking at things like gas mixing in the plant before the plant starts up, or a plant test using helium to look at transport and gas stratification. These two items, the thunder tube and VGES trench, are things we will assess in '82 as to whether or not we need to do large-scale tests on flame acceleration or trench or large-scale tests on gas mixing stratification.

Some of the things we have done on the small laboratory scale are the water fog experiments. Here we were given a strong consideration for the effects these water droplets have on reducing pressures. In the these small-scale steps we are attempting to characterize the types of fogs and foams you would want to inhibit burning or reduce the pressures and temperatures from the burning of hydrogen.

MR. KERR: Is this because you think someone may decide he wants to control hydrogen for the spray and fog?

DR. LARKINS: Right.

MR. KERR: Does someone propose to do that?

MR. LARKINS: I am not sure. I think some of the utilities are looking at sprays, fogs, and foams; and also EPRI.

MR. KERR: Are they doing some of the work?

MR. LARKINS: They are doing some small-scale tests, yes. Right now, particularly, they are looking at the water fogs.

MR. KERR: Is this work complementary to what the utilities are doing, confirmatory or --

MR. LARKINS: It is really complementary. When I say they are doing work, they are really just starting up. EPRI is just starting their program in hydrogen control, and one of the things they are interested in is what fogs. And we are interacting with them to ensure there is no unnecessary duplication of effort.

MR. SIESS: The normal procedure of the NRC would be for the applicant to propose and the NRC to ask questions, and, if its tests were not good enough, to keep asking questions until he made enough tests to satisfy the NRC.

It seems to me I have seen that procedure in application on Mach 1, Mach 2 hydrodynamic effects. I mean it was 15 years before we asked the right questions, which is worth keeping in mind.

Does this represent a change in philosophy within NRC of runnin tests shared with industry or in running your own tests so you know what questions to ask without waiting ten years for it or something?

MR. LARKINS: I do not necessarily think so. Sam could elaborate on it.

MR. SIESS: I will investigate it at a higher level in management. In fact, I would like to address it to NRR, who presumably sponsors these tests, if they are here.

MR. KERR: We tried, but they were too busy to make it today.

MR. SIESS: I see. They are out licensing, I hope.

MR. BASSETT: You probably heard Bob Minogue address the subject of his desire and the desire of the NRC staff to engage cooperatively with industry and industry organizations in order to assure we do not have unwarranted duplication of tests. And perhaps this is a good precursor for that sort of cooperation.

We have met with EPRi and discussed with them the sharing of expenses for some tests. We do not have a deal with them yet as to whether we will go forward, but at least we know what they are planning to do and they know what we have in mind to do. We have to be extremely aware of the fact that we may appear later in hearings, so we intend to seek this sort of cooperation in the early end of the cycle.

The hydrogen program we are conducting has to do with the various things that turned up at Three Mile Island, in the extent that igniters should be provided or other palliative measures should be provided inside the containment. This program was well underway before the EPRI interest started up. We do not know yet how far we are going cooperatively with that, but we are in discussions with them.

MR. SIESS: That is a good answer, Sam. But let me put the question in another extreme for you. You are talking about sharing with industry. I know what you are talking about, and I approve of it. But the previous policy would have said that not only do we not share with industry but that we do not do any of it; they do it, we just ask questions, we evaluate results -- which is a procedure which has been followed by the NRC in some cases and not others. Okay?

MR. BASSETT: I understand that.

MR. SIESS: Is this a conscious change in direction to share? I could justify this on the basis I suggested earlier, that instead of waiting 15 years to ask questions about the results, by doing some research on your own you get the questions in earlier, which is an argument preconfirmatory -- I do not know what you would call it -- but what I heard, Bob, was in general terms. When I get down to specific cases, I want to see what has consciously been applied there and what the reasoning is.

MR. BASSETT: I think the best context to put it into, Dr. Siess, is in the idea of the degraded core rulemaking operation. We intend to go forward as soon as we can with some proposed rules. We do not find presently available sufficient information on which to base those rules.

If I could have perhaps a minute, I would indicate to you the approach we are taking, which would --

MR. SIESS: Is the rule, for example, going to say use a fog if your research says a fog does the job?

MR. BASSETT: We do not know the answer to that, because we do not know the degree of mechanistic solutions the rule would propose.

MR. SIESS: Not propose. Require. A rule does not propose.

MR. BASSETT: It is a slip. I should have said the proposed rule would state, because it is subject to comment.

MR. SIESS: But the idea that the rule may state some prescriptive style solutions --

MR. BASSETT: There is that possibility. And i order to avoid a completely deterministic formulation of the rule, if we find there are great areas where information would allow us to be much more intelligent than what they say in the proposed rule -- Charlie, would you like to comment?

But before I bring Charlie in --

MR. SIESS: I think what you just said is important, but the more you know, the more intelligent your recommendations are going to be.

MR. BASSETT: I have one more thing I would like to indicate to you. It represents the general approach we are taking toward the formulation of this rule. We see there is a need for information along the line I have just described, and, to that end, John Larkins has an extra assignment as the program manager of the development of the research information for this rule. This goes across our four divisions in research that are interested in developing certain parts of the rule.

John will incorporate that. We think Bernero will be a customer for that sort of information, that he will take the information we derive and apply risk policies to it. And from this accumulation of information and the risk determinations Bernero makes, we will get our first formulations of the rule.

So obviously, it will be a trade-off between making arbitrary, rigid, deterministic requirements and trying to have some insight that might introduce the dimension of cost effectiveness when Larkins has his program in place and has it approved by our NRR friends.

We will go forward and hand the material to Bernero. Bernero will formulate it, with our assistance, into a preliminary rule. That is the general approach we are taking.

MR. SIESS: You used the term "deterministic." I did not. I used "proscriptive" versus "performance." For example, when you became concerned -- and "you" being the staff, not necessarily the research staff -- about main steam line break accidents, as I recall, a letter went out to all the licensees saying, "Recompute your main steam line break and then see if your equipment is qualified for the new temperatures and pressures you get."

Now, I can visualize the Commission following this same procedure, saying, "Recompute the temperatures and pressures for hydrogen burns and see if your equipment is qualified," and it going out of NRR without any research to speak of.

What I see here is a different kind of approach. Right?

MR. BASSETT: Yes, I think it is.

Charlie would like to address this.

MR. SIESS: Charlie.

MR. KELBER: I think there has been a small but significant change -- I think Bob Minogue did comment on it at the initial meeting with the committee -- that the emphasis on confirmatory aspects -- whatever that means, and it means different things to different people -- has in fact decreased considerably, and the emphasis now is on meeting the information needs of the Commission.

The hydrogen program specifically has, however, both aspects to it. The history is that there was what is called an "interim rule" on degraded cores, degraded core cooling, which called for measures to deal with significantly larger amounts of hydrogen than were provided for under Appendix K. And this did, in fact, result in the proposal by TVA to put the igniters or TV torches, as Bob Bernero like to call them, into the containment.

This raised a host of questions and a need by NRR to develop a technical basis to deal with the range of answers and the adequacy of the proposed work, and come to some decision on this schedule by which this issue could be resolved. And we were called into the picture along with the applicant.

The issue also arose in connection with the Zion and Indian Point evaluation, and we are still learning what the real scope of the program is.

Nevertheless, the particular application notwithstanding, there is a clear long-range problem in connection with the final rule itself. And there the needs are the Commission's needs and, in some respects, I think you could say this is research being developed to enable the Commission to ask the right questions of the potential applicants.

And that brings me to the point of the kind of rule. A degraded core cooling steering group had one experience with an attempt to write a proscriptive rule for degraded core cooling and found that that was a very negative experience. It did not work out well. Not that I think it was necessarily a poor job done. It is just a very difficult job to do proscriptively, partly because we know so little and partly because the scope is so broad.

In my view, and I think it is one shared by many, it would be more desirable to write a rule of functional or, as you term it, performance requirement, and to establish these functional requirments in a meaningful way and to be able to evaluate the replies that the applicant claims he has a system which performs these functions adequately. You have to have a very substantial technology base, and that is what this program is really for.

But you are right: There has been a change. And I think the confirmatory nature is becoming of less importance.

MR. SIESS: I think we will probably continue to remind you of that and also to remind the Commission of this, because I do not think they have ever given research that much guidance on what is confirmatory and what is not.

Incidentally, that term "confirmatory" starts looking better now to me. I never did like it. But when you look at your regulatory research as research that enables you to ask the right questions, which is the expression I used and which Charlie used, that means the research makes you smart, what you said a minute ago, essentially.

And the other aspect of it is to enable you to know whenyou are getting the right answers. And that is confirmatory. That gives you the knowledge, the research that gives you the knowledge to confirm the validity of what someone else has done or the solutions they have come up with.

MR. KERR: Mr. Siess, you are getting very academic.

MR. SIESS: You could use the adjective in a pejorative sense, but I am no longer an academician.

MR. KERR: Far be it from me to use it in a pejorative sense.

(Laughter.)

Please continue, Mr. Larkins.

MR. LARKINS: As I was pointing out, water force can have a strong effect on reducing heat temperatures in the hydrogen burn. Part of the risk scale is to determine what kind of fogs can best handle reducing pressures and temperatures.

The next two vuegraphs give a brief illustration of that. Here we have a plot of the temperature profiles of hydrogen burns with various concentrations of suspended water droplets.

Note for a stoichiometric mixture we can get temperatures exceeding 2500 degrees Kelvin with the suspension of .05 percent water droplets. We can step in and reduce that temperature on the order of down to 1000 Kelvin. And the effect is proportional and similar for pressure. For a stoichiometric mixture we get a factor of 8 increase in the pressure over the initial pressure. The introduction of .05 volume percent drop, reduce that by a factor of 6 final pressure to initial pressure.

The problem is getting the appropriate-size droplets and having the appropriate density of water suspended, the scheme being that you would introduce your water fog, burn, and then have your fog dissipate.

I mentioned we had a 16-foot tank as part of a variable geometry experimental series. We have done 35 tests of varying hydrogen concentrations and igniter types. With the introduction of turbulence using fans, we are planning to look at water fog, halons and we will also be doing some of the equipment survivability work in this vessel.

This is a schematic of the vessel in your handout. I will not put it on unless there are some questions.

(Slide.)

The next vuegraph gives you an indication of some of the information which has come out of these two series of tests. The black line represents the pressure calculated from an adiabatic isochoric hydrogen-air burn. The thing I want to point out in here is the black dots represent a burn with the fans on. The open dots represent the burn with the fans off.

The trend is, with the fans on and the introduction of turbulence, you approach the adiabatic isochoric limits, you get much higher pressures and temperatures even with lean air mixtures.

MR. KERR: What does isochoric mean?

MR. LARKINS: Constant volume. I mentioned we are modifying what is called a "FITS tank," or a fully instrumented tank, adding additional diagnostics. This tank would be insulated and heated for steam. We will be also introducing water fog supplies, halons, and we will be doing most of our detonation work in the FITS tank, even though the other facility can withstand pressures up approaching 1000 psi. This tank is better instrumented.

MR. KERR: Did I understand or misunderstand? You can tell me which. The idea is you are trying to see if one had hydrogen in a containment, you could fill it with fog, set it afire, or burn it without detonation, and that would be a way of controlling?

MR. LARKINS: Yes.

MR. KERR: How are you going to decide, when you get through with these experiments, whether or not you can do that?

MR. LARKINS: Well, we will essentially do a series of tests which follow that particular scheme in this tank.

MR. KERR: Yes. But given the data, what process will you follow to determine whether you would recommend that someone use that process in a containment?

MR. LARKINS: I think this will have to be factored into some of the other information that comes out of it. If we determined, for instance, that one can reach temperatures and pressures within the containment system from burning hydrogen, that will have an effect on the equipment viability.

One might suggest in addition to installing igniters, one might want an additional system to inhibit or cut down on the pressures and temperatures the equipment would see.

Does that answer your question?

MR. KERR: What I am asking is has someone designed a system and said, "Ah hah, here are some unknowns, and we need to run this research program to get the answers before we can understand how this system is going to work," or are you saying, "Let's do some research and see if we cannot get some information out of it and maybe having that information we can design a system"?

MR. LARKINS: What I think I am saying is, well, igniters have been proposed for a number of plants. There are some questions --

MR. KERR: Maybe I should simplify it. I think what you have been telling me i you burn hydrogen in the presence of water, it will not get so hot. And that seems like a reasonable statement.

Now, suppose we discovered a quantitative behavior of this in a laboratory setup like this. What do you do with the information in terms of something that has to do with a reactor?

MR. LARKINS: I think you have to factor this into the considerations. If you can potentially reach high temperatures and pressures which affect the safe operation of the plant --

MR. KERR: You and I both know without running experiments that you can get very high temperatures in a hydrogen flame.

MR. LARKINS: Even for lean mixtures.

MR. KERR: That is right, you can. That is now news. And we could also guess with fairly high confidence that if you sprayed water into it it would cool it off.

MR. LARKINS: Yes.

MR. KERR: You are trying to get something more quantitative than that, and I am trhing to get some idea of what are you going to do with this information, how will it be used?

MR. CURTIS: Dr. Kerr, one consideration is containments right now have sprays designed for that purpose. If this work were to discover that by changing the nozzles on the sprays to produce a different droplet size, that the sprays would be far more effective in holding the temperature of a hydrogen burn down, that would be something I would like to know.

MR. LARKINS: One might develop procedures for ignition with igniters and sprays.

MR. KERR: Has anyone looked to see whether the present droplet size is better for taking iodine out and hence you could not change it? Or do you put in two kinds of sprays?

MR. LARKINS: I think both.

MR. KELBER: I happen to know the answer to that one. At least the early calculations were that you would not need to convert all of the nozzles. You would have to convert about one-third, probably. The supply of water and also the mixture of the large spray, which is incidentally better for removing iodine, with the small spray would cause too rapid an agglomeration and sweep-out of the fog.

So there is a fairly complex aerosol problem here. The problem is analogous to the rain, the heavy rain which in fact removes fog. So that you might want to modify all of the nozzles, some still being fairly large, for effective iodine removal, and some being quite small.

MR. KERR: So the possible result is you will know enough so that you can tell people whether to change the nozzles in their existing spray system, assuming they want to use them for cooling burning hydrogen.

MR. KELBER: We want to get both uses out of them. The sprays are veryeffective at cleaning the iodine out of the atmosphere.

MR. SHEWMON: You might have an overkill on the iodine of only a factor of 10 instead of 100 or something if you did not push so much through, I would guess, with the additives they have, I have the impression there is a tremendous reserve of extra capacity.

MR. KERR: Of course, what is going to happen after the research gives this information which is useful, someone in licensing will require the applicant to do the design assuming that water is not available.

(Laughter.)

MR. KELBER: I will not answer that question.

MR. SHEWMON: Charlie, in simplistic terms, to remove iodine, you want a lot of water passing through, and to have a fog you want to have this stuff stay there.

MR. KELBER: Yes. I might just say not iodine but fission products and particulates that get in the atmosphere. The spray, as you may recall from Al Pasedag's report, NUREG-0771, are very effective at cleaning the atmosphere. And I have fallen into the trap of using iodine generically to refer to all fission products.

MR. SHEWMON: Thank you.

MR. LARKINS: That is a good point. The larger droplets tend to agglomerate a lot more quickly than the smaller droplets, which is critical. Clearly, constructing a hydrogen steam jet facility to, say, auto-ignition would have supersteam mixed with hydrogen in various ratios and various flows to see what mixtures ignite over what range.

This will give us information on developing controlled flaring for high point vents. It will give us information on the needs for flame holders, igniters, and various information along that line.

This apparatus should be completed within the next quarter, and work should begin in FY 1982 and continue through 1983. There is a vuegraph in there which gives you the specifics on the steam jet, the experiments, and an indication of the parameters that would be looked at.

There is an analytical experimental effort at Magill University under Professor John Lee, where he is studying this phenomenon of accelerated flames. He is doing a number of small-scale experiments with fixed geometries using both strong and weak sources for ignition. He is looking at the transition from deflagration to detonation for fine gases and he is looking at lean limits of detonatability for varying degrees of confinement associated with this effort.

On accelerated flames we are planning on doing some mock-up tests of various parts of the containment building. One of the first things we want to do is a mockup of the upper plenum region of the ice condenser for Sequoyah to look at the effects of the spacing of the air handlers, the number of air handlers, and the effects of comfined spaces on flame acceleration.

We are planning to do this with a simple setup which would be made of plywood and plexiglas and would be done in the outdoors. The cost is relatively low.

MR. SIESS: Who will do that?

MR. LARKINS: This will be done at Sandia.

MR. SIESS: Sandia actually does things with plywood and plexiglas?

MR. KELBER: But the very best.

MR. LARKINS: I would like to take a few minutes to touch on parts of the program. We are doing some analysis to estimate the quantities and rates of hydrogen generation for various types of reactors. We have completed some work on Zion and Sequoyah. The goals of this work are to become familiar with MARCH, also to develop models to model the effects of steam and other inerting mediums.

MR. KERR: Who is it that has become familiar with MARCH in this process?

MR. LARKINS: This is also Sandia. They will be identifying weaknesses as part of the assessment of MARCH and looking at necessary improvements in the code as it affects the models associated with the generation of hydrogen and investigating the sensitivity to various input parameters.

We have developed models to handle combustion, both deflagration and detonation. And the models for deflagration which predict the adiabatic isochoric temperatures, including the effects of CO_2, CO, and water. We have also added heat-loss models for radiation and convection and the effects of conduction on surfaces and the evaporation of sprays.

MR. KERR: Excuse me. How does one contrast this objective with t'a capability of MARCH CORRAL?

MR. LARKINS: MARCH is a very simple burn model in there which handles only homogeneous gases and just gives you the adiabatic pressures and temperatures and does not count heat losses and things like that. So we want something a little more sophisticated than that.

MR. KERR: The purpose with this is so that you can make --

MR. LARKINS: To better develop, both get a handle on the pressures and temperatures the containment would see, and to envelope the pressures and temperatures equipment would see in the plant.

MR. KERR: Are we talking now about 1982-83?

MR. LARKINS: This is 1982-83, yes.

MR. KERR: And this is being done in Sandia?

MR. LARKINS: Yes. All of this work except the flame acceleration work at Magill University is currently being done at Sandia.

MR. KERR: Thank you.

MR. LARKINS: Included in your handout is a curve of the comparison of one of the models, the detonation model, with various convective terms in it, simply to show the agreement between the experimental results with the model using various convective terms varying from one meter per second to 15 meters per second.

During 1982 and 1983 we will make improvements in the codes I have mentioned, including not homogeneous gases incorporating the burn model and improving the numerics and algorithms in the code.

MR. KERR: This is 1982?

MR. LARKINS: Yes.

MR. KERR: And you will get to 1983?

MR. LARKINS: The last part, as we learn more and get a better understanding, we probably will improve the model system into 1983.

In the area of hydrogn transport, Sandia recently, in the last few months, received the German RALOC Code to handle hydrogen transport. RALOC was originally set up to look at the transport of hydrogen from raolitic decomposition in the containment.

We are also assessing or looking at codes like RISS, CONTAIN, and CONTEMPT, to see how applicable they might be for handling the hydrogen transport model. We will be comparing these with the experiments done at Battelle, Frankfurt. EPRI is currently intending to do some large gas mixing tests at the containment safety facility at Hedl. We would compare these codes with those experimental results also. We should finish this assessment of RALOC and the other codes in the first part of 1982 and develop requirements for changes or needs for developing a new code.

MR. KERR: What is that in-situ testing of helium before licensing refer to?

MR. LARKINS: That is the possibility of getting a utility to agree to do some helium mixing tests in a containment building prior to startup to get a better handle on the gas stratification problem with things like fans on and fans off.

There have been a number of detonation studies done using CSQ to look at the loads on containment both for Zion and Sequoyah. These calculations include global detonation, detonation for various points in the Sequoyah containment using different types of coordinate systems. There is currently a report being written on this analysis. It should be available in the next two or three months.

Lastly, we are putting together hydrogen operator's manual. It is a generic manual, a generic procedures manual for handling hydrogen for various accidents. And the idea is that this could be incorporated into plant-specific procedures for handling hydrogen for various accidents.

MR. SIESS: Is NRC in the business now of writing procedures?

MR. CURTIS: No, sir. We evaluate procedures.

MR. SIESS: But this manual does not sound like an evaluation manual.

MR. LARKINS: The idea is that it would be generic for operator actions and that the utilities could develop specific procedures from the guidance that was provided in this manual.

MR. SIESS: I understand that. I guess if I were in a position of responsibility in the NRC, I would question what kind of liability this placed on the NRC. I read somewhere that, I guess, the general counsel decided that Three Mile Island did not really have their basis to put their claim for $4 million against the NRC for causing the accident. But I wonder what would be the liability position of NRC if they put out a manual and the plant writes their procedures based on that manual and they turned out to be wrong.

MR. CURTIS: My first impression -- and I am not trained in law --

MR. SIESS: I am not objecting to this. But I think I might do a different kind of research if I were going to take responsibility than if I were not.

MR. CURTIS: I do not think this puts us in any more directly liable position than any other guidelines we publish for development of specific procedures.

MR. SIESS: Can you give me an example of guidelines for the development of procedures? Would you call Appendix B to Part 50 a guideline? That is not that specific; is it? I mean that tells you what the plant has to have, but it does not tell you -- the only guidelines I can think of are reg guides, and they simply say these are ways we will accept of complying, and they get pretty prescriptive, I admit, but at least they are not mandatory.

MR. BASSETT: I think the use of the word "envelope" is probably a mistake. It is a frequently made one. I would point out when one assembles a compendium of knowledge in a field where one is lacking, it is irresistible to try to put it in one pile and say this manual is the results of our research.

MR. KERR: It does not say that. It says this is going to be used by plant operators to write a manual on how their people should deal with hydrogen. That is not just putting down the results of research, unless I am misreading the English language.

MR. SIESS: And it would not hurt to put up the next slide, which has more specific information on it, recommends specific operator actions and timing using presently available equipment, recommend specific actions. I cannot find any other way of reading that.

MR. BASSETT: I will not stand behind that

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TEST TARGET (MT-3)

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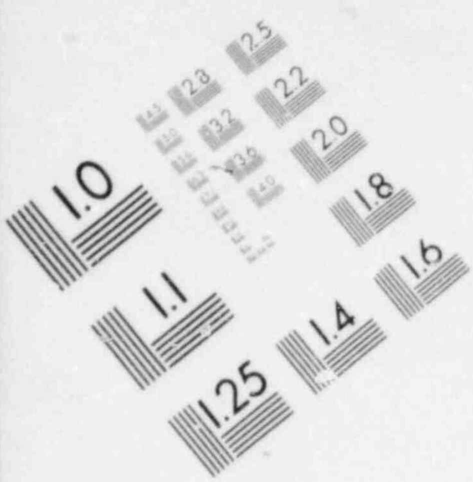
IMAGE EVALUATION
TEST TARGET (MT-3)

6″



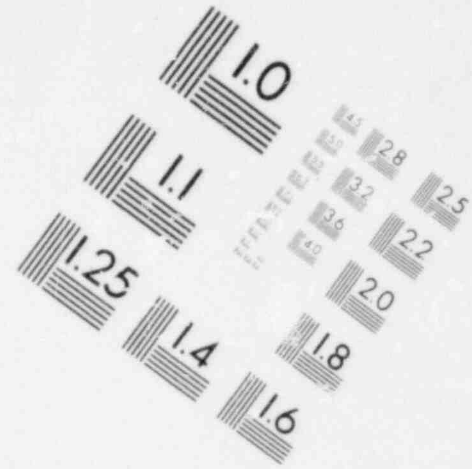
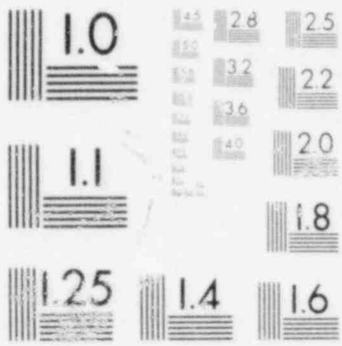
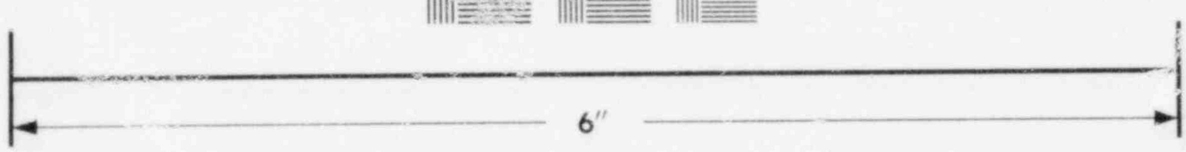


IMAGE EVALUATION TEST TARGET (MT-3)





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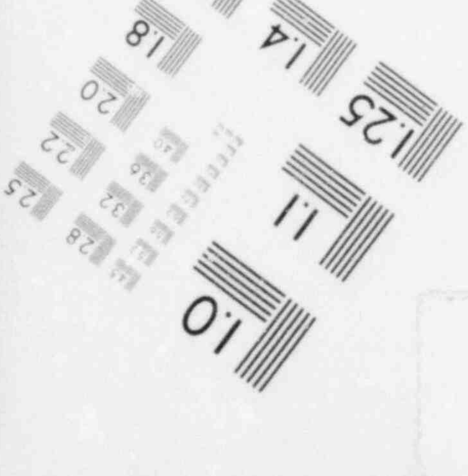

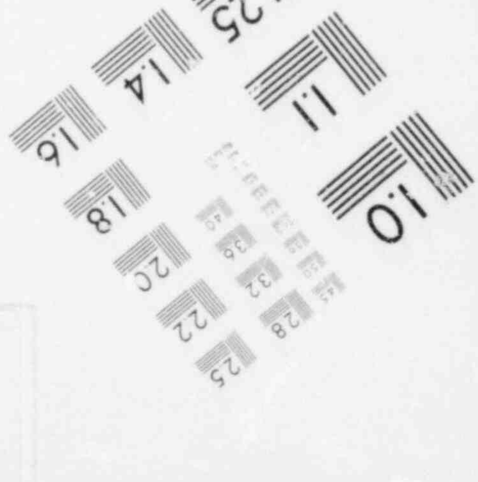

statement. We are in no position to recommend specific actions. We are in the position of providing all the information we get to the operator so they can determine the actions, and we are in the position of passing on them.

MR. SIESS: No problem with that. But be sure you hold your contractors to that philosophy.

MR. BASSETT: Yes, we will be sure of that.

MR. LARKINS: One last comment. Not included in the program now is work on hydrogen formation from radiolysis. There is still some question on the amounts and quantities of hydrogen that can be generated from radiolytic decomposition of water. This is something we are discussing with the regulatory staff to decide whether there is a need to do any work in this area or if there is enough available data to come up with a sound technical position.

Are there any questions?

MR. BASSETT: Excuse me, John. You indicated you would indicate the total amount of dollars of the various programs.

MR. LARKINS: Do you want that on the record or off the record?

MR. BASSETT: I will leave that up to Dr. Siess.

MR. KERR: Dr. Siess wants it on the record.

MR. SIESS: Money?

MR. BASSETT: Yes.

MR. SIESS: We don't care. We will take anything.

MR. BASSETT: We have no objection.

MR. SIESS: If you have got no objection, we have got no objection to hearing it.

MR. LARKINS: On the first vuegraph it is broken down by FIN, the hydrogen behavior control program.

MR. KERR: We are talking about fiscal 1983 now, right?

MR. LARKINS: Yes. That is approximately $2 million. The program on hydrogen from containment or in containment will be phasing down in 1983 and will be less than $150K.

MR. SIESS: What one is it? Give us the FIN number.

MR. LARKINS: I am trying to find it.

MR. SIESS: It is on the first slide.

MR. LARKINS: A1255.

MR. SIESS: That is the $2 million?

MR. LARKINS: Just the equipment survival program is somewhat uncertain. One of the approaches we had hoped to take was to see if we could get some of the vendors to possibly donate equipment. We are looking at various strategies to procure equipment and the types and needs of equipment are still undefined.

I think this number could probably vary from half a million to probably $800,000 in 1983.

MR. SIESS: 1270?

MR. LARKINS: Yes.

MR. SIESS: After 1.7?

MR. LARKINS: Excuse me?

MR. SIESS: You said half a million to --

MR. LARKINS: About 800K. Wait a minute. Let me break this down. This is jointly funded by both research --

MR. SIESS: This is for the two of them then?

MR. LARKINS: Yes. That is for both. The analysis and the experimental portion.

MR. KERR: Any other questions?

MR. BASSETT: We would like to say these numbers are planning figures only, at this state of development.

MR. KERR: That is the only way we will use them.

MR. SIESS: What about the A1246?

MR. LARKINS: A1246?

MR. KERR: Is that not the one you said the hydrogen behavior and control program?

MR. LARKINS: No. That is approximately $2 million.

MR. KERR: What is combustible gas and containment?

MR. LARKINS: That is 150.

MR. KERR: I see. I had them reversed.

MR. LARKINS: That program should be completed in

1983.

MR. KERR: Any other questions?

(No response.)

MR. KERR: Thank you, Mr. Larkins.

Mr. Curtis, the SASA program and other analysis.

MR. CURTIS: The severe accident sequence analysis has the objective of improving understanding of severe reactor accidents and of events beyond the design basis utilizing best-estimate methods and state-of-the-art codes.

The participants are INEL and Los Alamos, who are working on the so-called front end of the PWR severe-accident cycle. This is the part of the accident prior to the loss of core geometry. Sandia is looking at the back end of the PWR accident, which is that part of the accident following disassembly of the core and the nature and type of the containment threat up to the point of containment breach.

The program does not consider in detail the environmental effects of the dispersion of the radioactive material into the atmosphere following the breach of containment. Oak Ridge --

MR. KERR: Excuse me. I apologize, Mr. Curtis, but I am trying to fit this into the context of our budget. Is this part of severe-accident mitigation?

MR. CURTIS: Yes, it is.

MR. KERR: Is it all of severe-accident mitigation?

MR. CURTIS: It is not.

MR. KERR: What fraction?

MR. CURTIS: This program is right now at approximately $2 million. It is projected to continue at approximately the same level of effort, with some inflation, through 1983.

MR. KERR: Thank you.

MR. CURTIS: The utilities that are cooperating with the SASA program include TVA, with the Browns Ferry point, working with Oak Ridge and the Zion Westinghouse plant as the typical PWR.

To give you an idea of some recent progress, this is the product of the INEL program. We had a contractors meeting in very early June, and, of course, this is the progress reported on that. They have been working on PWR station blackout and have been assisting the BWR on station blackout.

The INEL program is largely analysis of the early phases of the accident, using the RELAP Code that was developed there.

Los Alamos has been looking at LOCAs in the PWR system space and a whole series of interfacing systems LOCAs.

MR. KERR: What is a PWR system space?

MR. CURTIS: These are LOCAs out in steam generators in steam lines in the balance of the plant not immediately associated with the main pressurized lines. One of their key interests has been in looking at all sorts of malfunctions in feedwater systems and auxiliary feedwater systems. For example, a U-tube rupture in one steam generator; that is a part of what they have been doing.

MR. KERR: How do they choose these accidents? Do you choose them? Do they choose them? What determines the population of those accident studies?

MR. CURTIS: The accidents are chosen by us after consultation with both risk assessment and NRR. The reason for the emphasis on station blackout is because of the task action plan because Mr. Barenowski and risk assessement has, with respect to station blackout, some of the other earlier accidents had been selected by consultation with NRR, who have asked for some rather specific studies to be done in support of their review of operator guidelines.

MR. KERR: Thank you.

MR. CURTIS: These are recent progress reports that have come out of Sandia. The Zion station blackout, rear end, I guess back end is a better name for it. The report is waiting final review and will bepublished shortly.

(Slide.)

And here is progress which has been reported from Oak Ridge. Oak Ridge has a close working relationship with

TVA at Browns Ferry. They have had an opportunity to compare their calculations to the same problem run on the TVA simulator, which has been a little unique. This is the only case we have of this close a cooperation.

Oak Ridge, in addition to being the only one of the four laboratories to concentrate on the BWR, is also concentrating on the details of fission product release in a more comprehensive way than is Sandia in PWR.

Some tentative conclusions that have come out of these first studies, this is the INEL small-break without high-pressure injection in Zion. The time to core uncovery in a small-break LOCA is quite dependent on the size of the break. I do not suppose that is a surprise.

But what may be a surprise is that if you are able to fully open all of your air duct valves within ten minutes you can prevent core uncovery from this accident.

MR. KERR: What is an air duct valve?

MR. CURTIS: It dumps steam into the air.

MR. SHEWMON: Just for my general education, pursue that one step further. Is it on the secondary side in steam generators?

MR. CURTIS: Yes.

MR. SHEWMON: Okay. So if you have an additional heat sink over there for some reason that prevents core uncovery in spite of the fact you still have this leak. Can you tell me why?

MR. CURTIS: The sequence is you do not open your PORV and depressurize the primary except --

MR. SIESS: Where does your water come from? I mean, to uncover the core you get more water out than you put in; so presumably this either takes less out or puts more in. Which does it do?

MR. CURTIS: It allows you to depressurize and ultimately use the low-pressure injection system in a controlled way.

MR. SHEWMON: Apparently, that line that says "high-pressure injection" is not available?

MR. CURTIS: Yes.

MR. SIESS: But you can depressurize down to the low pressure?

MR. CURTIS: Here is one I copied from a report. It is a little hard to read.

MR. KERR: What you are giving us now is things which have already taken place in the program, and we will get to 1983 shortly?

MR. CURTIS: Yes. Let me expedite getting to 1983. Here is a sample from the Los Alamos report. This is a sequence which starts with the rupture of five tubes in the PWR steam generator. I do not think I will read all of this. But it gives an indication of when the clad excursion begins and then finally when the core is empty of coolant water.

MR. KERR: Did RELAP calculate the reserves?

MR. CURTIS: This is Los Alamos, and Los Alamoses uses [illegible]

MR. KERR: I would think they would know it well enough that they would not do that but --

(Laughter.)

MR. SIESS: Can I back you up a minute? I read the objective, but I guess it is still not quite clear to me what SASA is all about. What is the question you are trying to answer with the SASA studies?

MR. CURTIS: The questions are two-fold: One, we are trying to understand the sequence of severe accidents; in other words, what is going on.

MR. SIESS: How can you get into a severe accident?

MR. CURTIS: How can you get into a severe accident; what kind of failures. We presume that all of these are multiple faults, otherwise.

MR. SIESS: Single failures --

MR. CURTIS: Would be taken care of.

MR. SIESS: Yes.

MR. CURTIS: The specific applications, or at least one of the specific applications of the early part is in support of NRR's review of operator guidelines NRR has required the vendors to provide.

MR. SIESS: In terms of the question, then, it is what can we do to prevent it, how can we get in trouble, and what can the operator do to prevent it?

MR. CURTIS: That is correct. What sort of strategies would be effective.

MR. SIESS: Is the whole thing deterministic?

MR. CURTIS: No. The base cases are generally the result of an unassisted failure. And then operator strategies are factored into the other cases that are being compared to the base case.

MR. SIESS: When one of these sequences is developed, is any attempt made to look at the probability of it going through that particular scenario?

MR. CURTIS: The base-case scenario is a scenario which presumably would result if nothing was done.

MR. SIESS: And then you look at the probabilities doing the right thing?

MR. CURTIS: Yes, then you look at the probabilities doing what else you might have done.

MR. SIESS: Thank you. That helps.

MR. CURTIS: Here are some selected conclusions that come out of the Sandia program which is looking at the back end of the accident. And, again, I think I do not have to read all of them to you.

MR. SIESS: Could I just read the first one? I see containment isolation valve. Is that some specific out of the couple of hundred, or should it be plural? I did not know whether it is a typo or whether it means something I do not understand.

MR. CURTIS: It means the containment isolation valves. Yes, very clearly yes.

MR. SIESS: That is an interesting conclusion or recommendation that someone should actually go around and look at all of the containment isolation valves to see that they are closed. Is that it?

MR. CURTIS: That is one of the recommendations they made.

MR. SIESS: That is so logical I cannot quite see how it would come out of this.

(Laughter.)

Unless it just turns out -- because I really do not see how the containment isolation valve being open contributes so much to a possible coremelt. I can see how it could contribute to an off-site dose, but not a coremelt. But these are stopping with coremelts; are they not?

MR. CURTIS: No. Sandia is looking at the so-called back end, in which all of the modes of containment failure are a principal ingredient of the study: hydrogen burns, overpressurization, failure to isolate.

MR. SIESS: Okay. Now I understand.

MR. CURTIS: The inventory you saw during the steam explosion.

MR. SIESS: Yes. How often do they want those valves checked? At each accident or --

(Laughter.)

-- weekly?

MR. KERR: These are interesting examples, but we have these. So why don't you give us an idea of where you are headed for 1983?

MR. CURTIS: Fine. This program says 1982, but it is going on into 1983. We believe that when the current round of publications on Zion are complete that we will have studied Zion adequately and we should move on.

Our next step -- and the letter has been written by the director of the Division of Licensing to Arkansas Nuclear, and we will begin a corresponding analysis of station blackout, small-break LOCA, feedwater transients on the Arkansas Nuclear 1 plant, which is a B&W plant, which is significantly different both in system design and other details that it will be interesting to compare it to a large dry Westinghouse design. And we expect to begin that in fiscal 1982.

MR. KERR: At this time we have WASH-1400, we have

RSMEP, which I do not quite understand. We have IREP, we have IREP, INREP.

MR. CURTIS: The reason for picking this is it is an IREP plant in which the IREP has been essentially completed. This means that we do not do the event fault tree. We do not look at the probability of getting into these early initiators. We take from them the probabilities of getting in and do detailed plant-specific calculations at a level of detail at least one cut below what was done in IREP.

MR. KERR: Are the results of this meant to be generically useful, plant-specifically useful, or vendor-generic, or what?

MR. CURTIS: The results should at least be vendor-generic.

MR. SIESS: Is that true? Are these sequences --

MR. CURTIS: Parts of it are true.

MR. SIESS: You can tell the sequences. I would expect a Westinghouse or B&W to be different from a GE, but are you sure that a Westinghouse with a balance of plant from one AE might not have differences with a Westinghouse balance of plant with another AE?

MR. CURTIS: I am sure they would.

MR. SIESS: What do you do with this when they get through, since it only applies to te particular plant you look at?

MR. SHEWMON: Checking the valves in the containment multiply to several AEs.

MR. SIESS: I would suspect that one might be generic. What are you going to do with this when you get through with it, assuming you get through with it in 1983 -- back to Bill's question?

MR. CURTIS: The lead plant for developing operator guidelines for design-basis accidents is Arkansas Nuclear.

MR. QUITTSCHREIBER: ANO-1 Unit 2 was a Combustion Engineering plant.

MR. CURTIS: Yes. This one applies contrary to. We have been asked to do some specific review.

Yes?

MR. KELBER: Let me add a little bit. There is, as Bob Curtis pointed out, a very specific end use, and that is in the development of guidelines for the review of emergency procedures. And for this reason the program is keyed to the plants that are going to be relief plants.

I would expect that from the differences between these various lead plants, both we and the operators will begin to understand types of questions that arise from having different types of balance of plant installations.

As a side remark, I would indicate that the recommendation that one check the containment isolation valves, it stems from the fact that closing these valves in the absence of off-site power may be less than well assured. I think it depends upon the availability of stored air which may not become available if the power is lost.

There is a further logic to this program that we are beginning to explore. It was most succinctly derived from a remark Jesse Ebersole made in a committee meeting I think last month, and that is if you have a combination of faults that puts you into an emergency then there are some ten or so systems in any plant where a single active failure can put you into a very severe problem.

And it seems to me we can develop a logic of attacking the multiple failure problem from that partition. Ten is a large but manageable number of systems to examine.

MR. KERR: Charlie, does the IREP not show this sort of thing up?

MR. KELBER: As Bob has mentioned, IREP and INREP will be developing weak points, and we have used that plus the vital-area study.

MR. KERR: But I thought this was something beyond IREP.

MR. KELBER: It is.

MR. KERR: What is the "beyond"?

MR. KELBER: The corrective action.

MR. SIESS: But if it is plant-specific, how do you do it on 70 plants?

MR. KELBER: We will not be able to do it all. The operators will have to do a great deal.

MR. SIESS: You are going to ask the applicants and licensees to do SASAs on their plants; is that one of the things you will do?

MR. KELBER: It is a possibility yet to be explored. I think at this time we have to see what the guidelines for emergency procedures look like. It is a possibility yet to be explored. I would think it would be in the interest of at least some of these to do just that.

MR. KERR: Is this a user request from NRR or somebody?

MR. CURTIS: Yes, it is.

MR. KERR: So NRR plans to have emergency procedures developed based upon the SASAs?

MR. CURTIS: I believe it is the other way around. The vendors are charged with providing NRR with the emergency procedures. NRR has to evaluate these procedures, and they would like --

MR. KERR: When I say "beased on the SASAs," I mean a SASA will give you an indication of whether the procedures are any good or not.

MR. CURTIS: Precisely.

MR. KELBER: There is another application going beyond that, which is the rulemaking. The SASA is a key integrating tool in judging the adequacy of various provisions. I have said before that I favor a functional rule, and a functional rule may certainly require significant amounts of implementation. I find it difficult to conceive of a fully automatic system that would cover all eventualities in these relatively infrequent and difficult-to-predict accidents.

MR. KERR: What would be wrong with an approach which asked a licensee to do a SASA and then, on the basis of that, write a procedure?

MR. KELBER: That may well be a development depending upon NRR's views and the operator's views. That could very well happen.

MR. CURTIS: They are being required to do a study comparable to the IREP studies which identifies the dominant sequences but does not do detailed thermohydraulic or other calculations to verify this event tree approach to the sequence. SASA is -- in IREP it is enough to know how the systems interact. In SASA we have to know how big the pipes are.

MR. SIESS: I would like to mention, Charlie, the statements you attributed to Mr. Ebersole was indeed made at the last meeting. But as I recall, it was also made about three years ago by Mr. Ebersole, I think, at the second meeting at ACRS he attended. You might have heard it for the first time last month but --

MR. KELBER: It is not the first time I heard it, but the connection with SASA is becoming clearer in my mind, and I would like an opportunity to think this through, because I do believe we need a rationale for and a rational way to scope the multiple-failure problem.

MR. SIESS: When he first mentioned that the answer was that is a multiple failure and we do not consider multiple failures. That has obviously changed, and we all recognize that.

MR. KELBER: Yes.

MR. KERR: I thought the fault tree or event tree pattern was capable of dealing with multiple failures.

MR. KELBER: It deals with them in depicting their relative likelihoods. What it does not deal with is the question of how you recover and what system should you deploy to recover from these, what systems have value.

MR. KERR: It seems to me what you recover from depends upon where you are, not how many failures you had to get there. So it seems to me when you talk about recovery you do not care.

MR. KELBER: No; you have to know what systems you have available as well.

MR. KERR: That is a part of where you are.

MR. BASSETT: Some of the failures, however, eliminate options in recovery.

MR. KERR: But that is a part of where you are.

MR. KELBER: Not in the probabilistic area. In the probabilistic area it is always going forward. Once you have gotten to a certain branch, it does not matter.

MR. KERR: I am simply saying if you are in a particular situation and you know what equipment you have, you can figure out how to get out of it independently of how you got there.

MR. SIESS: That is what SASA is doing.

MR. KELBER: That is what SASA is doing.

MR. CURTIS: Let me give a sample of a rather specific nature. One of the calculations that has been done, the Los Alamos calculation, in particular, set out to investigate what fraction of feedwater capability must be restored in order to allow recovery. And it came out with an answer, say, 30 percent is adequate. Then you look at the specifics of the auxiliary system and you see how are the ways that 30 percent of the auxiliary feedwater or the nominal feedwater supply could be supplied with some combination of these components. This is something that a probabilistic study, an IREP study, would say either the feedwater is on or the feedwater is off and would not address the quantitative parts of it.

I propose to dispense with any summary unless there are questions the committee would like to ask.

MR. KERR: Are there questions the committee would like to ask?

(No response.)

MR. KERR: I see none. Are there questions anyone else would like to ask?

(No response.)

MR. KERR: I see none.

Let me thank you then for the information.

And I declare the meeting adjourned.

(Whereupon, at 5:00 p.m., the committee was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS/Subcommittee on Class 9 Accidents

Date of Proceeding: June 24, 1981

Docket Number: ____________________

Place of Proceeding: Washington, D. C.

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

Patricia A. Minson

Official Reporter (Typed)

Patricia A. Minson

Official Reporter (Signature)

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Sharon Filipour

Official Reporter (Typed)

Sharon Filipour

Official Reporter (Signature)

Severe Accident Research

Division of Risk Analysis

Background

- Risk assessment code development & application
  - MARCH code
  - CORRAL code
  - Reactor Safety Study Methodology Applications Program (RSS-MAP)
  - Uncertainty Analyses
- Improved Reactor Safety
  - Filtered-Vent Containment Concepts Program
  - Alternate Decay Heat Removal Concepts Program
  - Molten Core Retention Device Program
- Degraded Core Cooling Rulemaking Research Support

Severe Accident Research
Division of Risk Analysis

- Risk Assessment - Related Code Development Application

Present Status

- MARCH code
  - public release - October, 1980 (NUREG/CR-1711)
  - follow-on efforts to assist MARCH User's Group, etc.
- CORRAL code
  - upgraded version due to be available late this year
- RSS-MAP
  - 2 plant risk studies published (NUREG/CR-1659)
    - Sequoyah
    - Oconee
  - 2 additional reports pending
    - Grand Gulf
    - Calvert Cliffs
- Uncertainty Analyses
  - reports pending

SEVERE ACCIDENT RESEARCH
DIVISION OF RISK ANALYSIS

- RISK ASSESSMENT - RELATED CODE DEVELOPMENT AND APPLICATION

PLANNED ACTIVITIES

- IMPROVEMENTS TO MARCH CODE
    - RFP PROCESS UNDERWAY TO OBTAIN CONTRACT FOR CODE IMPROVEMENTS

Severe Accident Research
Division of Risk Analysis

- Improved Reactor Safety
- Filtered-Vent Containment Concepts Program

Present Status

- input to Z/IP considerations (NUREG/CR-1410)
- BWR Mark I analyses nearing completion

Planned Activities

- Analyses to be performed
  - ice condenser
  - BWR Mark III
  - large dry containment
- Merger under DCC rulemaking research support program

Severe Accident Research
Division of Risk Analysis

- Alternate Decay Heat Removal Concepts Program

Present Status

- COMPLETION OF STUDY OF EXISTING SYSTEMS AND DEVELOPMENT OF SYSTEM OPTIONS (NUREG/CR-1556)

Planned Activities

- MERGER UNDER DCC RULEMAKING RESEARCH SUPPORT PROGRAM

Severe Accident Research
Division of Risk Analysis

- Molten Core Retention Device Program

Present Status

- Report in preparation describing initial evaluation of potential risk reduction value

Planned Activities

- Merger under DCC rulemaking support program

Severe Accident Research
Division of Risk Analysis

- Degraded Core Cooling Rulemaking Research Support

Objective: To provide an assessment of the values and impacts of a set of degraded core prevention and mitigation features, for use in support of the degraded core rulemaking.

Severe Accident Research
Division of Risk Analysis

- Degraded Core Cooling Rulemaking Research Support

Prevention and Mitigation Options

1. Additional or alternate means of active or passive containment heat removal;
2. Additional or alternate means of containment atmosphere particulate capture;
3. Additional or alternate means of containment atmosphere mass removal, i.e., filtered or unfiltered vents, large and small vent areas;
4. Increased containment volume, pressure capability, or additional pressure suppression features;
5. Containment atmosphere combustible gas control, i.e., deliberate ignition, inerting, or fire suppression;
6. Core retention devices, i.e., wet or dry, active, passive, or no additional cooling;
7. Missile shields for vessel rupture due to steam explosion, vessel thermal shock, or melt-through at elevated pressure, or for ex-vessel steam or combustible gas explosions;
8. BWR containment spray system;
9. PWR primary system depressurization, e.g., automatic or manual, with or without additional relief capacity, and with or without associated pressure suppression or radioactivity removal features;
10. Add-on decay heat removal systems; and
11. Prevention concepts tailored to specific accident sequence vulnerability, e.g., fixes for event V and S_2C in Surry or S_2HF in Sequoyah.

Severe Accident Research
Division of Risk Analysis

- Degraded Core Cooling Rulemaking Research Support

Method of Analysis

1. Use RSS & RSS-MAP results to define accident groupings.
2. Analyze impact of each option (or combination of options) on each accident grouping to define "value" of option.
3. Perform estimations of cost ("impact") of each option (or combination of options).
4. Combine results into value-impact survey.
5. Iterate

SEVERE ACCIDENT RESEARCH
DIVISION OF RISK ANALYSIS

- DEGRADED CORE COOLING RULEMAKING RESEARCH SUPPORT

SCHEDULE

- PROGRAM LETTER AUTHORIZING PROGRAM INITIATION - JUNE, 1981

- COMPLETION OF FIRST ITERATION AND REPORT PUBLICATION - MARCH, 1982

- COMPLETION OF SECOND ITERATION AND REPORT PUBLICATION - 3RD QUARTER, FY 83

BEHAVIOR OF DAMAGED FUEL

OBJECTIVE

- o PROVIDE DATA BASE AND ANALYTICAL METHODOLOGY FOR UNDERSTANDING AND PREDICTING CORE BEHAVIOR UNDER SEVERE ACCIDENT SEQUENCE CONDITIONS.

    - ACCIDENT TERMINATION IN-VESSEL
    - ACCIDENT MANAGEMENT
    - PREVENT GLOBAL CORE MELT

BEHAVIOR OF DAMAGED FUEL

RELATED INFORMATION NEEDS

- DATA BASE AND MODELS FOR DCC AND MESF RULEMAKING AND RULE IMPLEMENTATION.
- SEVERE ACCIDENT MANAGEMENT PLANS/OPERATIONS.
  - CORE CONDITIONS FOR TERMINATION BY SIMPLE REFLOOD
  - COOLANT FLOWS NEEDED FOR TERMINATION
  - CONDITIONS WHERE REFLOOD WORSENS ACCIDENT
- DESIGN OF IMPROVED ENGINEERED SAFETY FEATURES.
  - ADEQUACY OF CURRENT DESIGNS FOR SEVERELY DAMAGED CORES
- IN-VESSEL FISSION PRODUCT/HYDROGEN.

BEHAVIOR OF DAMAGED FUEL

SCOPE

- o INTEGRAL IN-PILE TESTS (PBF AND ESSOR SUPER-SARA-EEC):
  - SCOPING TESTS TO DETERMINE OVERALL BEHAVIOR AND GOVERNING PHENOMENA.
  - VERIFICATION TESTS OF THE INTEGRAL CODES DEVELOPED.
- o SEPARATE-EFFECTS PHENOMENOLOGICAL EXPERIMENTS FOR THE DEVELOPMENT AND VERIFICATION OF MODELS.
  - IN-PILE EXPERIMENTS (DEBRIS FORMATION, RELOCATION, AND COOLABILITY) - ACRR.
  - LABORATORY EXPERIMENTS (OXIDATION, CLAD BALLOONING, AND MELT PROGRESSION).
- o ANALYTICAL MODEL DEVELOPMENT:
  - SEVERE CORE DAMAGE ANALYSIS PACKAGE (SCDAP) FOR USE IN THE ANALYSIS OF ACCIDENTS AND INTEGRAL EXPERIMENTS.
  - PHENOMENOLOGICAL MODELS FOR USE AS MODULES IN SCDAP AND ANALYSIS OF SEPARATE EFFECTS EXPERIMENTS.
- o ANALYSIS AND CHARACTERIZATION OF THE TMI-2 CORE DEBRIS FROM TMI-2 CORE EXAMINATION PROGRAM.

SEVERE FUEL DAMAGE STUDIES IN THE

FUEL BEHAVIOR BRANCH, DAE

BY

M L. PICKLESIMER, FBB/DAE

JUNE 24, 1981

PHILOSOPHY OF THE EXPERIMENTAL PROGRAM ON SEVERE FUEL DAMAGE (SFD)

- THERE ARE AN INFINITE NUMBER AF ACCIDENT SEQUENCES THAT CAN LEAD TO SEVERE CORE DAMAGE - NO EXPERIMENTAL PROGRAM CAN COVER ALL.
- THERE ARE ONLY A FEW TERMINAL STATES OF SEVERE DAMAGE IN AN LWR CORE, EASILY DEFINED AND DESCRIBED.
- THESE TERMINAL STATES ARE:
  - FUEL ELEMENTS BALLOONED AND BURST, BUT NOT EMBRITTLED
  - CLADDING OXIDIZED AND EMBRITTLED, BUT NOT MELTED, FRAGMENTED INTO DEBRIS OF FUEL PELLETS AND OXIDIZED CLADDING
  - CLADDING MELTED IN UPPER PART OF CORE TO FORM "LIQUIFIED FUEL" BY REACTION WITH THE UO_2 AT ABOUT 1900°C (3450°F), "CANDLED" AND FROZEN INTO SUBCHANNELS AT LOWER LEVEL, DEBRIS BED FORMED
  - UO_2 MELTED IN UPPER PART OF CORE (TEMPERATURES ABOUT 2800°C (5000°F), LIQUIFIED FUEL FORMED AT LOWER LEVEL, DEBRIS BED FORMED
  - CORE MELTED, COLLAPSED ONTO BOTTOM OF THE PRESSURE VESSEL
- ALL SCENARIOS LEADING TO SEVERE CORE DAMAGE WILL END IN ONE OF THESE TERMINAL STATES.
- THE FBB SEVERE CORE DAMAGE PROGRAM IS BASED ON THE EXPERIMENTAL EXAMINATION OF THESE TERMINAL STATES AND THE PROCESSES LEADING TO THEM.

LOCATION ON FUEL ROD UNCOVERED

HEATING RATE, T	TIME TO QUENCH	MAX TEMP 1300°C	PROBABLE DAMAGE — FUEL ROD: BALLOONING AND BURST	EMBRITTLEMENT OXIDATION	TOTAL WALL OXIDATION	$ZR + ZRO_2$ EUTECTIC	LIQUIFIED FUEL	UO_2 MELT	CORE: CORE GEOMETRY LOST	CORE BLOCKED
HIGH	LONG	YES	X	X		X	X	X	X	X
		NO	X	?	?					
	SHORT	YES	X	X		?	?	?	X	?
		NO	X	?						
MEDIUM	LONG	YES	X	X		?	?		X	X
		NO	X							
	SHORT	YES	X	X		?	?		X	?
		NO	X							
LOW	LONG	YES	X	X	X			?	X	?
		NO	X							
	SHORT	YES	X	?						
		NO	X							

X = OCCURS

? = DEPENDS ON DETAILS OF ACCIDENT SCENARIO

"PARAMETER SPACE"
EXPERIMENTAL SEVERE FUEL DAMAGE STUDIES

Phenomenon	NRU	PBF	ESSOR	LBT	Ex-Pile	Sandia	ACRR	Data/Info Needed	Variable	Range
1. Debris Bed Production	?	X	X		X			A. type & mixture	1. max temp	A. 1900-2300°C
	?	X	X		X			B. kinetics	2. heating rate	B. 0.5-4°C/sec
									3. quenching rate	C. slow/fast
									4. bundle length	D. 3 ft-2 meters
2. Debris Bed Permeability, Coolability	?				X	?	X	A. Coolant flow rates	1. particle size and distribution	A. fine/coarse
	?				X	?	X	B. heat removal capability	2. types of particles	B. Zr, ZrO_2, UO_2, etc.
									3. pressure differential	C. [illegible]-ft H_2O
									4. decay heat level	D. 0.1-10%
3. Liquified Fuel	?	X	X		X	?	X	A. kinetics of formation	1. temperature	A. 1900-2300°C
									2. ratio of metal liquid to fuel	B. 0.1-1
									3. fuel fragement size	C. 0.1-10 mm
									4. composition of liquid in contact with fuel	D. Zr-ZrO_2 eutectic to equil. with fuel
		X	X		X	?	X	B. candling and freezing behavior, viscosity	1. temp. of liquid	A. freezing point to 2300°C
					X				2. temp. of solid surface	B. 1500-2000°C
									3. composition of liquid	C. Zr to equil. with fuel
		X	X		X			C. fission product retention, rate of release	1. temperature	A. 1900-2300°C
		X	X		X				2. composition of liquid (Zr/U)	B. ?
									3. f.p. species & conc.	C. low to high burnup
									4. time	D. ?
					X	?		D. Oxidation rates, thermodynamics of reactions	1. composition (Zr/U/O)	A. phase diagram
					X	?			2. temperature	B. 1600-2300°C
									3. H_2/steam ratio	C. ?
4. Remelt in Beds	?	X	X					A. Kinetics	1. bed composition	A. Zr to UO_2
									2. stratification	B. ?
									3. temperature	C. 1500-2300°C
									4. blockage	D. ?
5. Bundle Blockage	X							A. Blockage fraction configuration, coolability	1. Burst temp	A. 700-1000°C
	X								2. bundle size	B. 1-64 rods

2. FBB/RES EXPERIMENTAL PROGRAMS ON SEVERE FUEL DAMAGE (SFD)

- o PBF-SFD TESTS: PHASE I - FIVE 32-ROD BUNDLE TESTS, SLOW AND FAST HEATING, SLOW COOL AND WATER REFLOOD. PRESENT FUNDING SCHEDULE - ONE TEST IN FY 82, ONE TEST IN FY 83, THREE TESTS IN FY 84. ACCELLERATED FUNDING SCHEDULE - TWO TESTS IN FY 82, THREE TESTS IN FY 83. PHASE II TESTS NOT YET PLANNED, BUT TO INCLUDE MELTING OF UO_2, HIGH BURNUP RODS, STAGNANT STEAM.

- o CODE DEVELOPMENT: SEVERE CORE DAMAGE ANALYSIS PACKAGE (SCDAP) MOD 0 DEVELOPED IN FY 81, MOD 1 WITH IMPROVED MODELS DUE IN LATE FY 83, IMPROVED MODS EACH YEAR FOLLOWING INCORPORATING SFD DATA AS DEVELOPED.

- o ACRR-FUEL RELOCATION EXPERIMENTS: SEPARATE EFFECTS TESTS EXAMINING CLAD-FUEL INTERACTIONS AT HIGH TEMPERATURES, LIQUIFIED FUEL FORMATION AND FLOW, DEBRIS BED COOLABILITY AND DRY-OUT. FIRST EXPERIMENTS IN LATE FY 82.

- o EX-PILE FUEL/CLADDING INTERACTION STUDIES: THERMODYNAMICS AND KINETICS OF INTERACTIONS BETWEEN ZR, UO_2, AND STEAM, PHYSICAL PROPERTIES OF THE PRODUCTS.

2. FBB/RES EXPERIMENTAL PROGRAMS ON SEVERE FUEL DAMAGE (SFD) (CONTINUED)

- DECCA (DEFORMED CORE COOLABILITY) TESTS: BALLOON AND BURST BEHAVIOR OF FULL-LENGTH 8 X 8 BUNDLES OF ELECTRICALLY HEATED FUEL ROD SIMULATORS DURING BOILDOWN IN HIGH-PRESSURE SMALL-BREAK LOCA'S, COOLABILITY OF DEFORMED BUNDLE REINSTRUMENTED FOR THERMAL-HYDRAULICS TESTS.

- ESSOR-SUPER-SARA SFD TESTS: TEN TESTS ON SFD IN 32-ROD BUNDLES 2 METERS LONG, DEEPER DEBRIS BEDS AND CANDLING THAN PBF-SFD TESTS, EVALUATING LENGTH EFFECTS AND ADDED SMALL BREAK LOCA SCENARIOS. EARLIEST SFD TEST COMPARABLE TO PBF-SFD IS 1986 AT PRESENT SCHEDULE.

- TMI-2 CORE EXAMINATION: COOPERATIVE PROGRAM GPU/EPRI/NRC/DOE FOR EXAMINATION OF CORE, UPPER INTERNALS, DEBRIS, ETC. NRC DEVELOPING TECHNIQUES AND EQUIPMENT FOR ANALYZING SPECIMENS OF SPECIFIC INTEREST TO SAFETY, DOE/EPRI FOR OPERATIONAL AND DESIGN INTEREST. FISSION PRODUCT DISTRIBUTION IN PRIMARY SYSTEM AND CORE OF MAJOR INTEREST.

2. FBB/RES EXPERIMENTAL PROGRAMS ON SEVERE FUEL DAMAGE (SFD) (CONTINUED)

- LWR DEBRIS COOLABILITY: IN-PILE AND EX-PILE DETERMINATIONS OF DEBRIS BED COOLABILITY, DRY-OUT, PERMEABILITY AS FUNCTIONS OF TYPES AND DEPTHS OF DEBRIS.

- LWR DEBRIS FORMATION AND RELOCATION: IN-PILE SEPARATE EFFECTS STUDIES OF FUEL-CLADDING INTERACTION, CANDLING, LIQUIFIED FUEL FORMATION AND MOVEMENT.

1. FOREIGN PROGRAMS ON SEVERE FUEL DAMAGE (SFD)

- JAPANESE PLAN NO EXPERIMENTAL WORK ON SFD. SEVERAL COMMITTEES ARE EXAMINING NEEDS FOR RESEARCH WORK ON ACCIDENTS BEYOND DBA'S.

- FRENCH PLAN NO EXPERIMENTAL WORK ON SFD AT THIS TIME BEYOND PARTICIPATION IN THE SUPER-SARA PROGRAM. THEY ARE EXAMINING THE MODIFICATIONS REQUIRED TO ALLOW SFD TESTS TO BE DONE IN PHEBUS. PLAN TO OBTAIN NEEDED SFD DATA BY INFORMATION EXCHANGE VIA INTER-NATION AGREEMENTS.

- UK HAS NO ACTIVE SFD STUDIES UNDERWAY EXCEPT FOR PARTICIPATION IN THE SUPER-SARA PROGRAM. THEY ARE EXAMINING THEIR NEEDS FOR SFD DATA AND MAY BE INTERESTED IN PARTICIPATING IN THE PBF-SFD PROGRAM.

- ITALY HAS NO ACTIVE SFD WORK UNDERWAY EXCEPT FOR THE SUPER-SARA PROGRAM.

- FRG/PNS PLANS NO IN-PILE TESTS EXCEPT FOR PARTICIPATION IN THE SUPER-SARA PROGRAM. PNS IS REINSTITUTING THE 1977-1979 WORK BY HAGEN WITH MODIFICATIONS TO 7 X 7 BUNDLES 1 METER LONG WITH QUENCHING CAPABILITY. ALSO CONTINUING PHASE DIAGRAM AND KINETICS WORK ON U-ZR-O SYSTEM BY P. HOFMANN. PNS WANTS TO EXCHANGE EX-PILE SFD DATA FOR PBF-SFD DATA AND SCDAP.

Jay - 2
Silberberg

COORDINATION WITH FOREIGN PROGRAMS

- o FEDERAL REPUBLIC OF GERMANY
  - FISSION PRODUCT RELEASE (SASCHA)
  - SEVERE FUEL DAMAGE (OUT-OF-PILE TESTS, EX-MEL)
  - FUEL MELT/CONCRETE INTERACTIONS (WECHSEL, KAVERN, BETA FACILITY)
  - AEROSOL BEHAVIOR IN STEAM (NAUA)
  - HYDROGEN (RALOC)
  - CORE MELT ACCIDENT SYSTEMS CODES (KESS)
  - STEAM EXPLOSION (MODELING)
- o SWEDEN (STUDSVIK ENERGITEKNIK)
  - FILTRA PROJECT
    - FILTERED VENTED CONTAINMENT SYSTEM DESIGN
    - GRAVEL BED AND SAND BED LOADING/EFFICIENCY
    - FISSION PRODUCT RETENTION IN BEDS

1a, 2 2
Curtis

CORCON DEVELOPMENT

MAJOR ACCOMPLISHMENTS - FY81

- CORCON - MOD1
  - COMPLETED
  - DOCUMENTED
  - DISTRIBUTED
- INITIATED CODE COMPARISON TESTS ANALYSIS
- INITIATE MOD1 SENSITIVITY STUDY
- DEVELOP RUDIMENTARY LONG-TERM MODEL
  - SOLID-DEBRIS/CONCRETE INTERACTION

MOLTEN CORE - CONCRETE INTERACTIONS

PROGRAM MANAGER: M. BERMAN

PROJECT LEADER: R. K. COLE, JR.

PRINCIPAL INVESTIGATORS:

R. K. COLE, JR: MODELLING AND CODING

D. P. KELLY: MODELLING AND APPLICATIONS

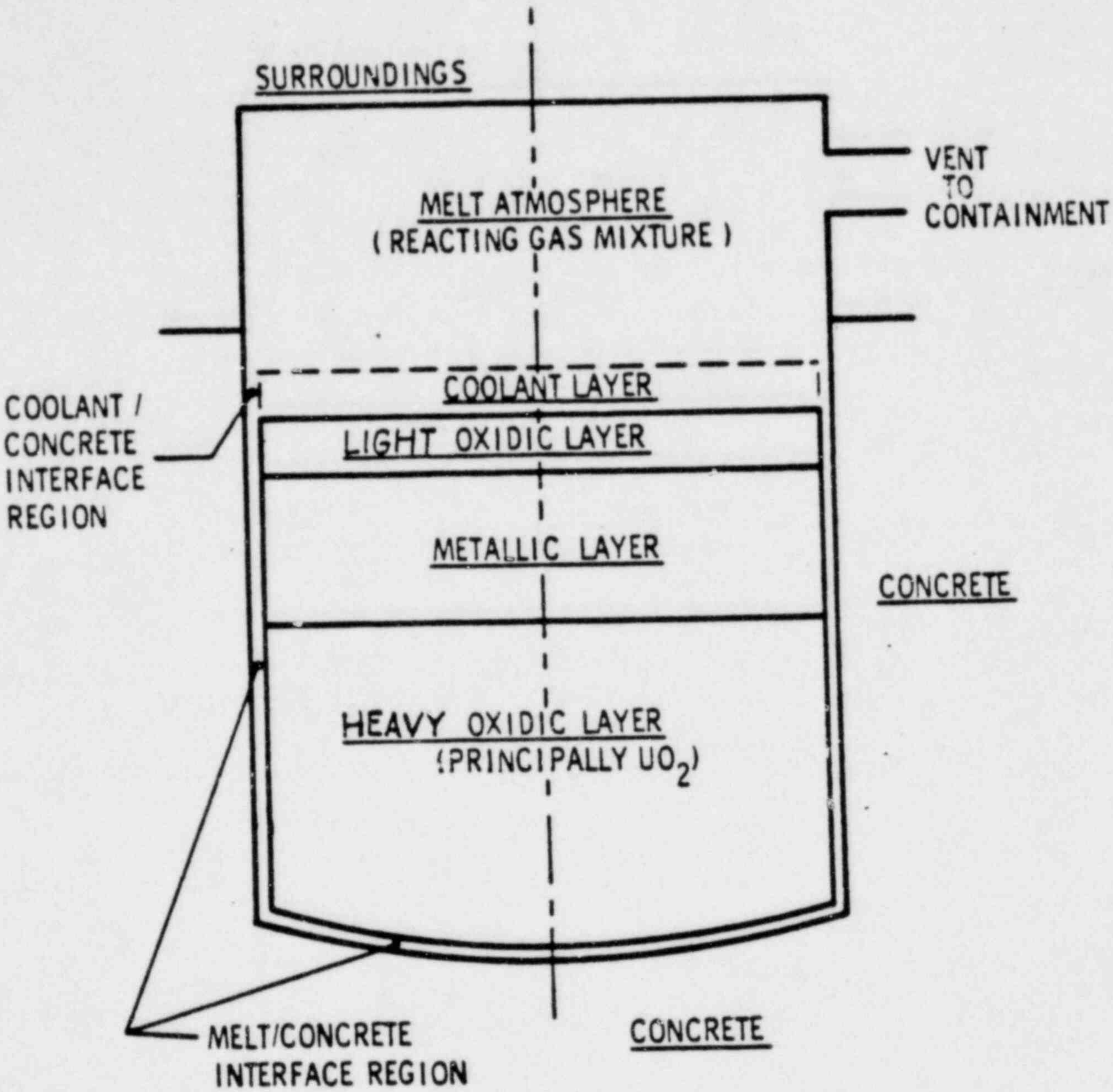
M. L. CORRADINI: CODE APPLICATIONS

CORCON PRINCIPAL FEATURES

FEATURE	MOD0, MOD1	MOD2
COOLANT	NONE	LAYER IN POOL VARIOUS DEPOSITION MODES
ATMOSPHERE	"NONE"	REACTING GAS MIXTURE MASS AND HEAT TRANSFER
SURROUNDINGS	UNIFORM TEMP. BLACK BODY TEMP. INPUT VS. TIME	UNIFORM TEMP. GRAY SURFACE ABLATION, MASS AND HEAT TRANS.
LONG TERM INTERACTION	NONE	RUDIMENTARY MODELS

CORCON CODE DEVELOPMENT

FEATURE	MOD0	MOD1, MOD2
MELT INTERNAL HEAT TRANSFER COEFFICIENTS	CONSTANTS	MODELS BASED ON EMPIRICAL CORRELATIONS
FISSION PRODUCT DECAY HEAT GENERATION	METAL AND OXIDE POWER INPUT VS. TIME	MODEL BASED ON FISSION PRODUCT CONCENTRATIONS POWER INPUT VS. TIME
SOLIDUS, LIQUIDUS TEMPERATURES	SUBROUTINE SOLLIQ FROM INTER	MOD1: NEW BUT SIMPLE MODEL MOD2: IMPROVED MODEL


SURROUNDINGS
VENT TO CONTAINMENT
MELT ATMOSPHERE
(REACTING GAS MIXTURE)
COOLANT LAYER
COOLANT / CONCRETE INTERFACE REGION
LIGHT OXIDIC LAYER
METALLIC LAYER
CONCRETE
HEAVY OXIDIC LAYER
(PRINCIPALLY UO_2)
MELT/CONCRETE INTERFACE REGION
CONCRETE

CORCON MOD2

1. IMPROVEMENTS OVER MOD1 RESULTING FROM SENSITIVITY STUDIES, COMPUTATIONAL MODIFICATIONS, INPUT FROM EXTERNAL USERS, NEW EXPERIMENTAL RESULTS.
2. ATMOSPHERE/SURROUNDINGS MODEL.
3. COOLANT LAYER.
4. IMPROVED GAS FILM MODEL.
5. RUDIMENTARY LONG TERM PENETRATION MODEL.

MOLTEN FUEL CONCRETE INTERACTIONS (MFCI) STUDY

OBJECTIVE: DEVELOP AND VERIFY A MODEL OF MOLTEN CORE MATERIAL/CONCRETE INTERACTIONS.

CAPABLE OF PROVIDING QUANTITATIVE ESTIMATES OF:

- NATURE AND RATE OF GAS EVOLUTION
- RATE AND GEOMETRY OF MELT PENETRATION

APPLICATION: SENSITIVITY STUDIES

INTERPRETATION OF EXPERIMENTAL RESULTS

PROVIDE EXPERIMENTAL DIRECTION

ANALYSES OF REACTOR FUEL-MELT ACCIDENTS

INCORPORATION IN CONTAINMENT ANALYSIS MODEL

Page 2
Curtis

CONTAIN

A Unified Computer Model for
Reactor-Containment System Analysis

Under Development at Sandia National Laboratories

Sponsored by U.S. Nuclear Regulatory Commission

CONTAIN Design

To accomplish its broad objectives, CONTAIN uses:

- General-purpose physical models
- Flexible, modular code structure

CONTAIN Code Characteristics

- Multiple compartments
- Reference cell
- Modular phenomenological models
- Dynamic storage allocation
- Flexible (input) problem description
- Extensive print and plot options

CONTAIN FEATURES

- STATE-OF-THE-ART PHYSICS MODELS
  - -- GENERAL CAVITY DEBRIS-POOL MODEL (SINTER)
  - -- MULTI-COMPONENT, SECTIONAL AEROSOL MODEL (MAEROS)
  - -- GENERAL, DETAILED FISSION PRODUCT DECAY AND TRANSPORT
- MODULAR STRUCTURE
  - -- MODELS READILY UPDATED
  - -- PHYSICS READILY ALTERED
- FIRST VERSION IS OPERATIONAL

Development Schedule

CONTAIN I – Ex-vessel containment

Version A

- Generic Containment Models
- Completion date: June 1, 1981

Version B

- Breeder (sodium) models
- Completion date: September 1981

Version C "Complete"

- Verified models
- Safety system models
- Release date: September 1982

Models in CONTAIN IA

Pool

- 1D multi-component flow
- Melting, freezing, boiling and condensation
- Heat transfer to pool sides
- Water migration in concrete
- Debris-coolant interactions
- Debris-concrete interactions
- Sodium pool fire
- Sources for atmosphere
  - Gases, Aerosols
  - Fission Product, Heat

Atmosphere

- Flow between cells
- Two-phase thermodynamics
- Condensation on structures
- Heat transfer to structures
- Hydrogen burning
- Purge gas
- Aerosol Dynamics
  - Flow, Agglomeration
  - Settling, Condensation
- Fission product dynamics
  - Flow, Release
  - Decay, Aerosolization

Additional Models in CONTAIN 1B

Pool	Atmosphere
Sodium-concrete interactions	Sodium spray fire
Debris-bed dynamics	
Zircalloy-steam interaction	Iron-steam interaction
Improved debris-concrete interaction	

ADDITIONS IN CONTAIN IC

- COMPRESSIBLE-FLOW POOL MODEL
- AEROSOL DEGRADATION OF HEAT SINKS
- FISSION PRODUCT RELEASE MODELS
- SAFETY SYSTEMS
  - CONTAINMENT SPRAY
  - ICE CONDENSER
  - CORE CATCHER
  - SUPPRESSION POOL
  - HYDROGEN BURN SUPPRESSION
  - FILTERED VENT
- ADDITIONS AND IMPROVEMENTS IDENTIFIED DURING APPLICATIONS AND VERIFICATION TESTS

Tay - 3
Sherry

STEAM EXPLOSION PHENOMENA

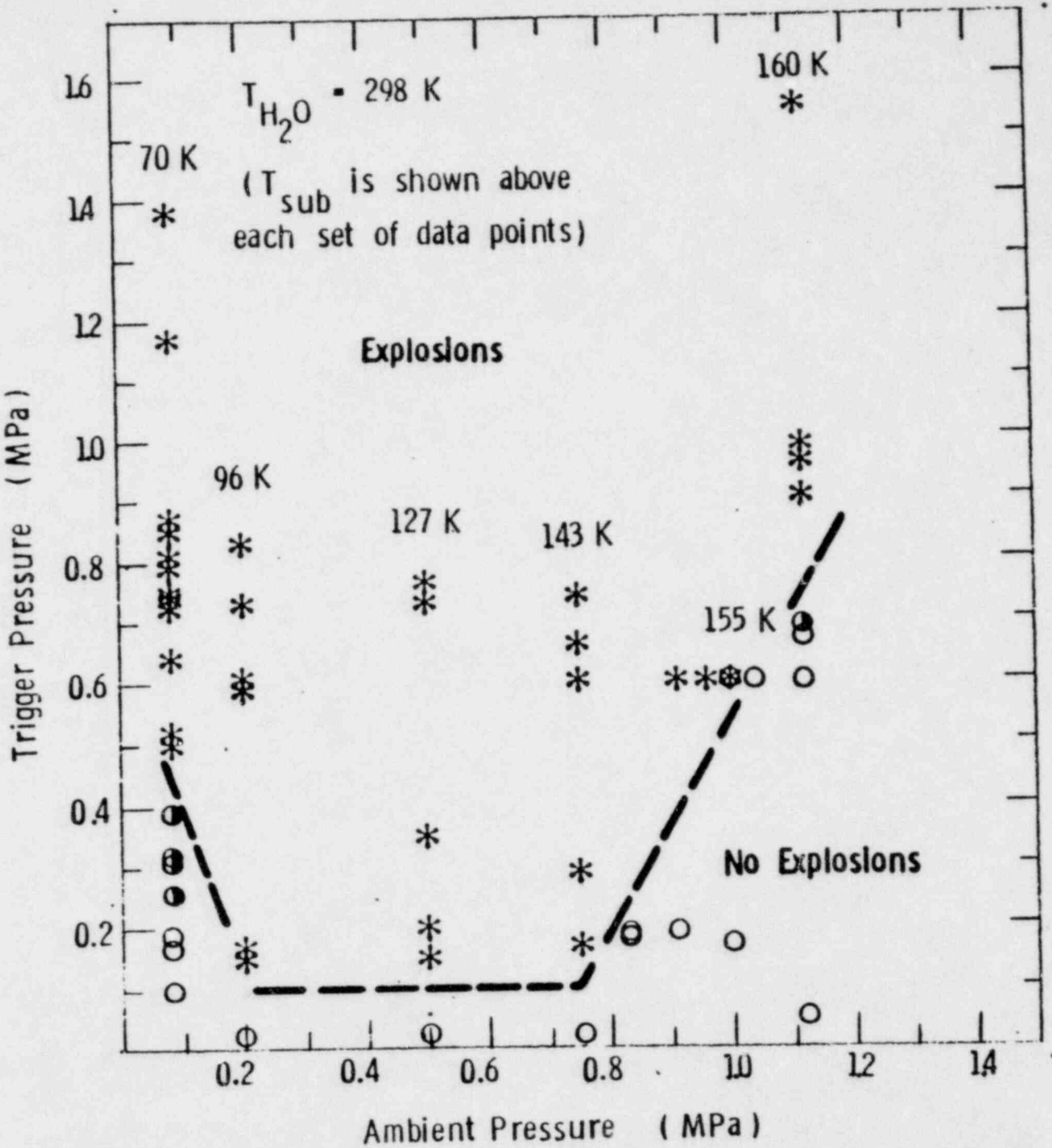
<u>OBJECTIVE</u>: TO INVESTIGATE THE PHYSICAL PROCESSES ASSOCIATED WITH THE INTERACTIONS OF MOLTEN CORE MATERIALS WITH REACTOR COOLANT. TO DETERMINE THE PROBABILITY THAT A STEAM EXPLOSION CAN FAIL THE REACTOR CONTAINMENT BUILDING.

<u>ELEMENTS OF CURRENT PROGRAM</u>:

- SMALL SCALE SINGLE MELT DROPLET TESTS
- INTERMEDIATE SCALE (5-25 KG) TESTS IN THE FITS FACILITY
- STEAM "SPIKE" TESTS IN THE FITS FACILITY
- DEVELOPMENT OF STEAM EXPLOSION FRAGMENTATION, PROPAGATION AND EXPANSION MODELS
- ANALYSIS OF REACTOR VESSEL AND CONTAINMENT RESPONSE TO STEAM EXPLOSIONS
- ESTIMATES OF CONTAINMENT FAILURE PROBABILITY

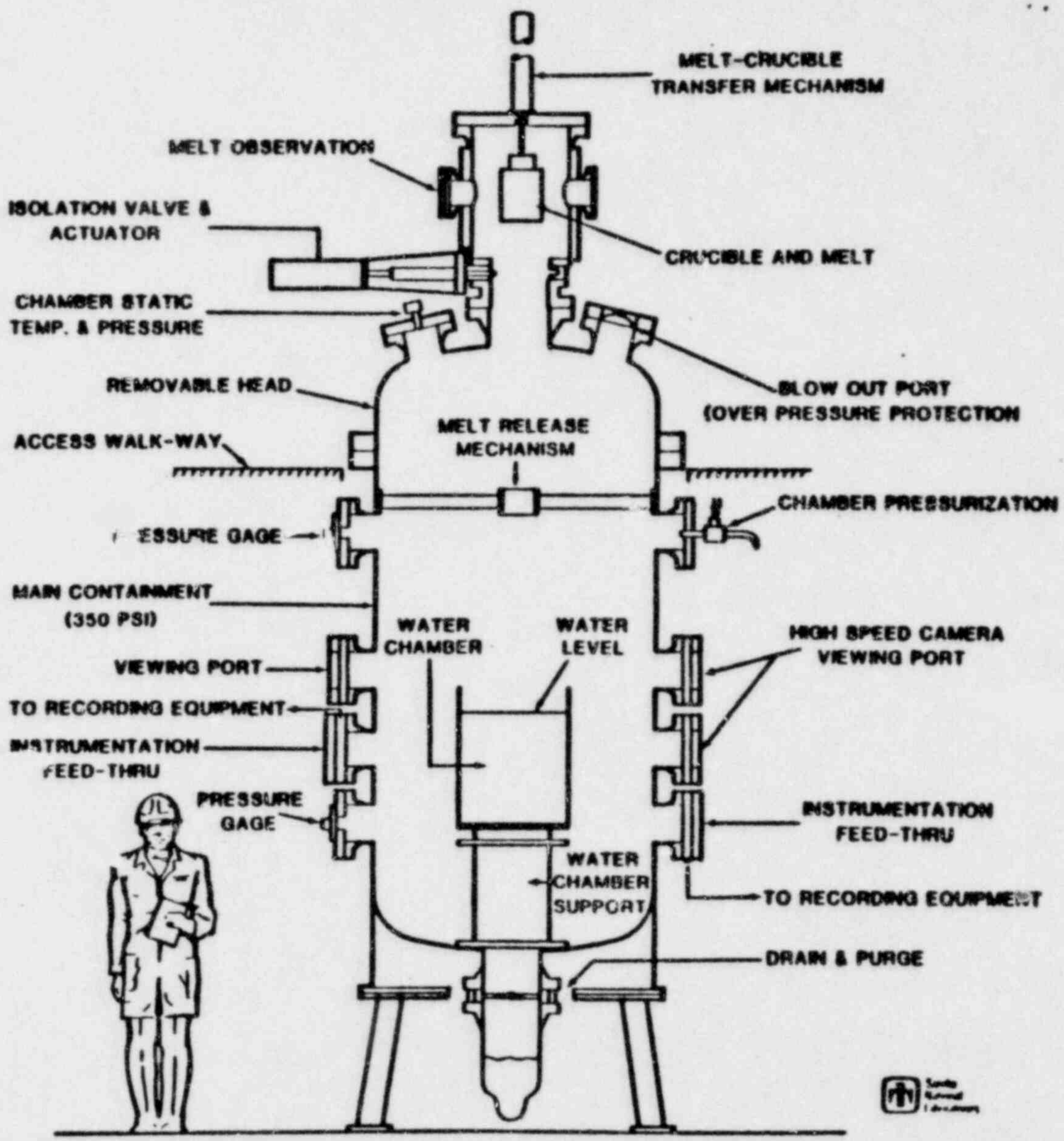
ACCOMPLISHMENTS DURING PAST YEAR

- SMALL SCALE EXPERIMENTS HAVE CHARACTERIZED THE MELT EXPLOSITIVITY AS A FUNCTION OF AMBIENT PRESSURE
- FIRST INTERMEDIATE SCALE STEAM EXPLOSION TEST SERIES IN THE FITS FACILITY COMPLETED (5 TESTS)
- TWO STEAM SPIKE EXPERIMENTS COMPLETED.
- A 1-D TRANSIENT STEAM EXPLOSION MODEL AND 2-D EMPERICAL MODEL HAVE BEEN DEVELOPED AND USED TO ANALYZE TESTS
- A STATISTICAL STEAM EXPLOSION CONTAINMENT FAILURE MODEL HAS BEEN DEVELOPED
- IMPROVED ESTIMATES OF CONTAINMENT FAILURE PROBABILITY HAVE BEEN DEVELOPED.



T_{H_2O} = 298 K
(T_{sub} is shown above each set of data points)
70 K
96 K
127 K
143 K
155 K
160 K
Explosions
No Explosions
Trigger Pressure (MPa)
Ambient Pressure (MPa)
1.6
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0.2
0.4
0.6
0.8
1.0
1.2
1.4

STEAM EXPLOSION RESEARCH FACILITY



FITS-A SERIES DATA SUMMARY

Expt.	Fuel Mass Kg	Water Mass/Temp Kg/C	Entry Velocity M/S	Purpose and Major Result
FITS1A 3/18/80	2.1	90/10	6.2	System checkout, Partial reaction, Velocity effect suspected
FITS2A 4/24/80	3.0	152/13	4.2	Surface triggered reaction .03 sec after entry
FITS3A 6/25/80	5.5	226/24	5.0	Propagating reaction
FITS4A 7/23/80	5.5	226/24	~7.0	P = .94 MPa, Delivery system failure, disperse melt no-explosion
FITS5A 9/10/80	5.5	226/24	5.7	No spontaneous explosion, Explosion triggered by external source

IN-VESSEL (FITS) RESULTS

- Velocity/Shape Effect on Explosivity Observed

  FITS1A
  FITS2A

- Debris Distribution gives an Indication of Degree of Reaction

FITS1A	2000 μm	Partial Reaction
FITS2A	260 μm	Surface Trigger
FITS3A	155 μm	Propagating Reaction
FITS4A	>3800 μm	No Explosion
FITS5A	155 μm	Triggered: High Pressure Environment

- High Ambient Pressure may Suppress Spontaneous Explosions

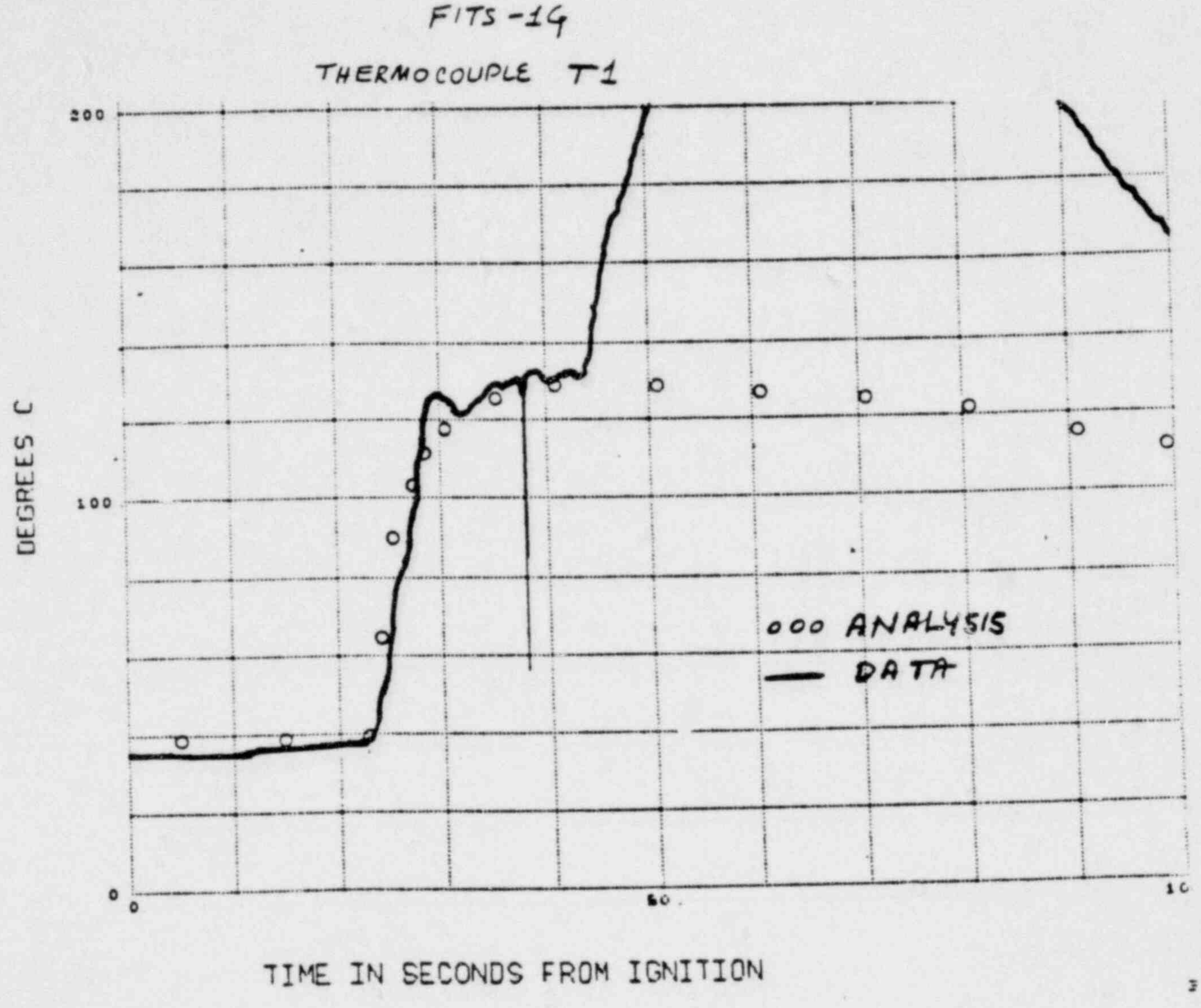
  FITS4A: Pamb = 0.94 MPa
  FITS5A: Pamb = 1.01 MPa

- Explosions can be Triggered at High Ambient Pressure

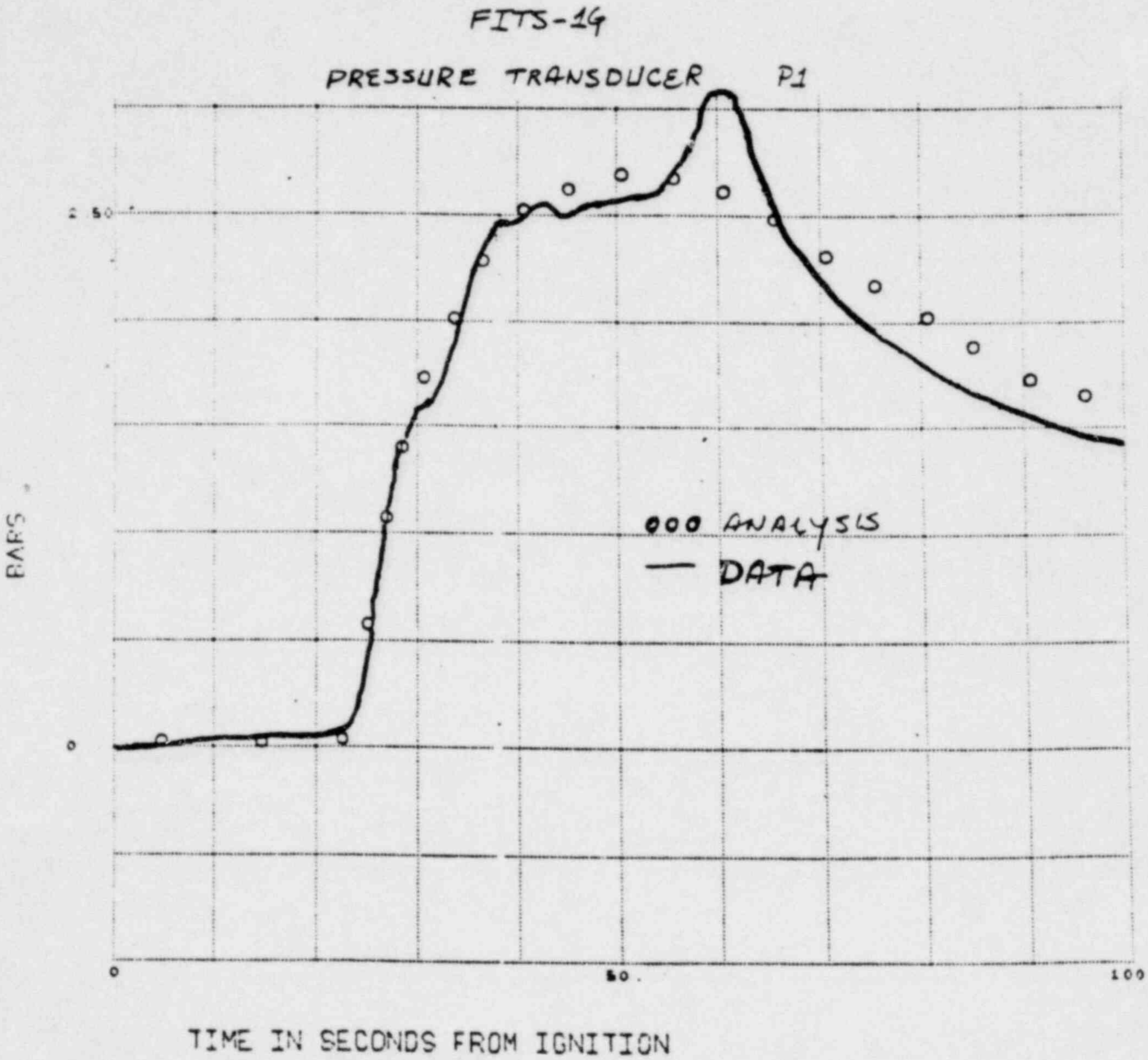
  FITS5A

- Melt Fragmentation Prior to Explosion (mixing phase) is not Affected by Chamber Ambient Pressure

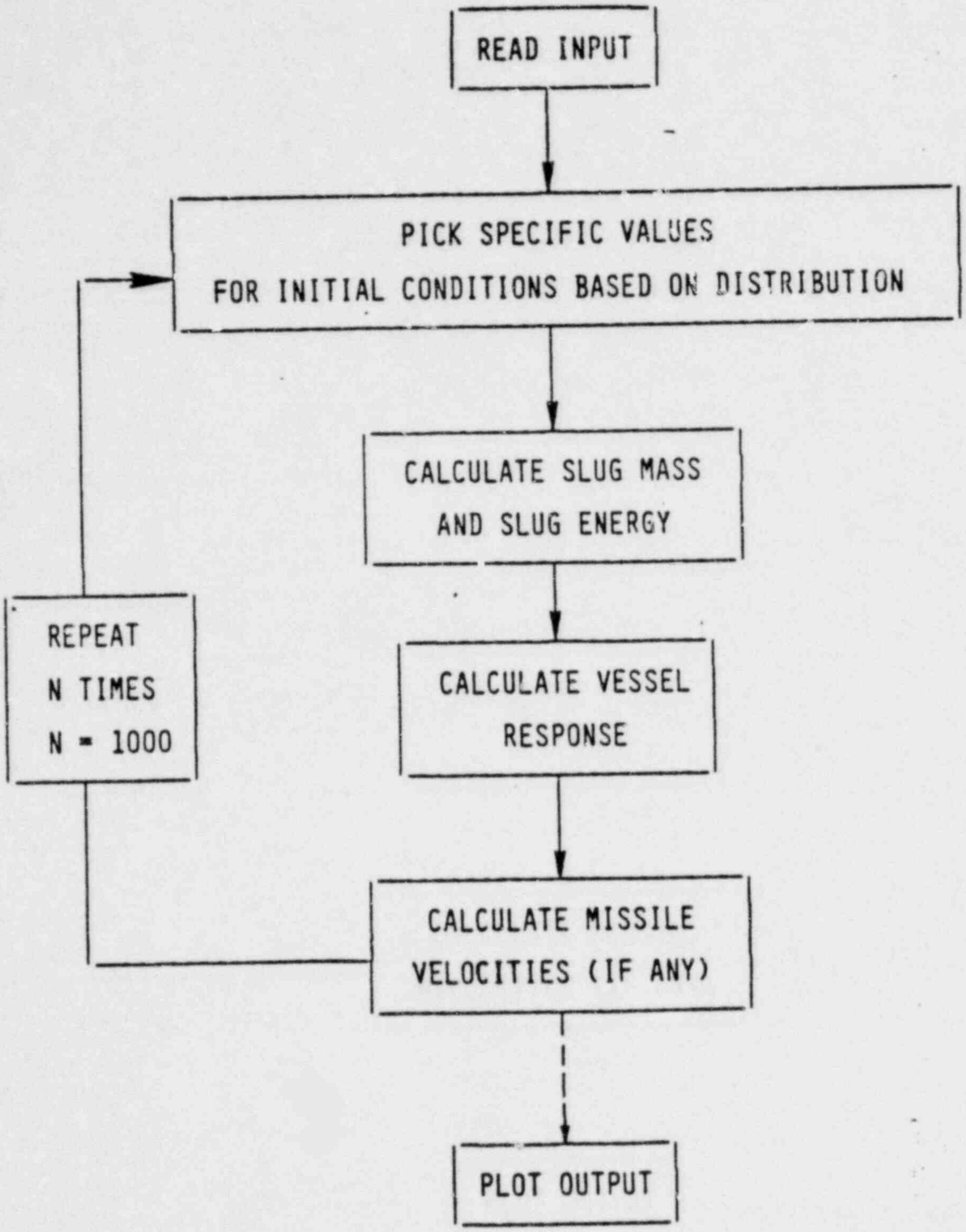
  FITS5A



FITS-1G
THERMOCOUPLE T1
200
100
0
DEGREES C
0
50
000 ANALYSIS
DATA
TIME IN SECONDS FROM IGNITION


FITS-1G
PRESSURE TRANSDUCER P1
2.50
0
BARS
000 ANALYSIS
— DATA
0
50.
100
TIME IN SECONDS FROM IGNITION

MONTE-CARLO SOLUTION TECHNIQUE

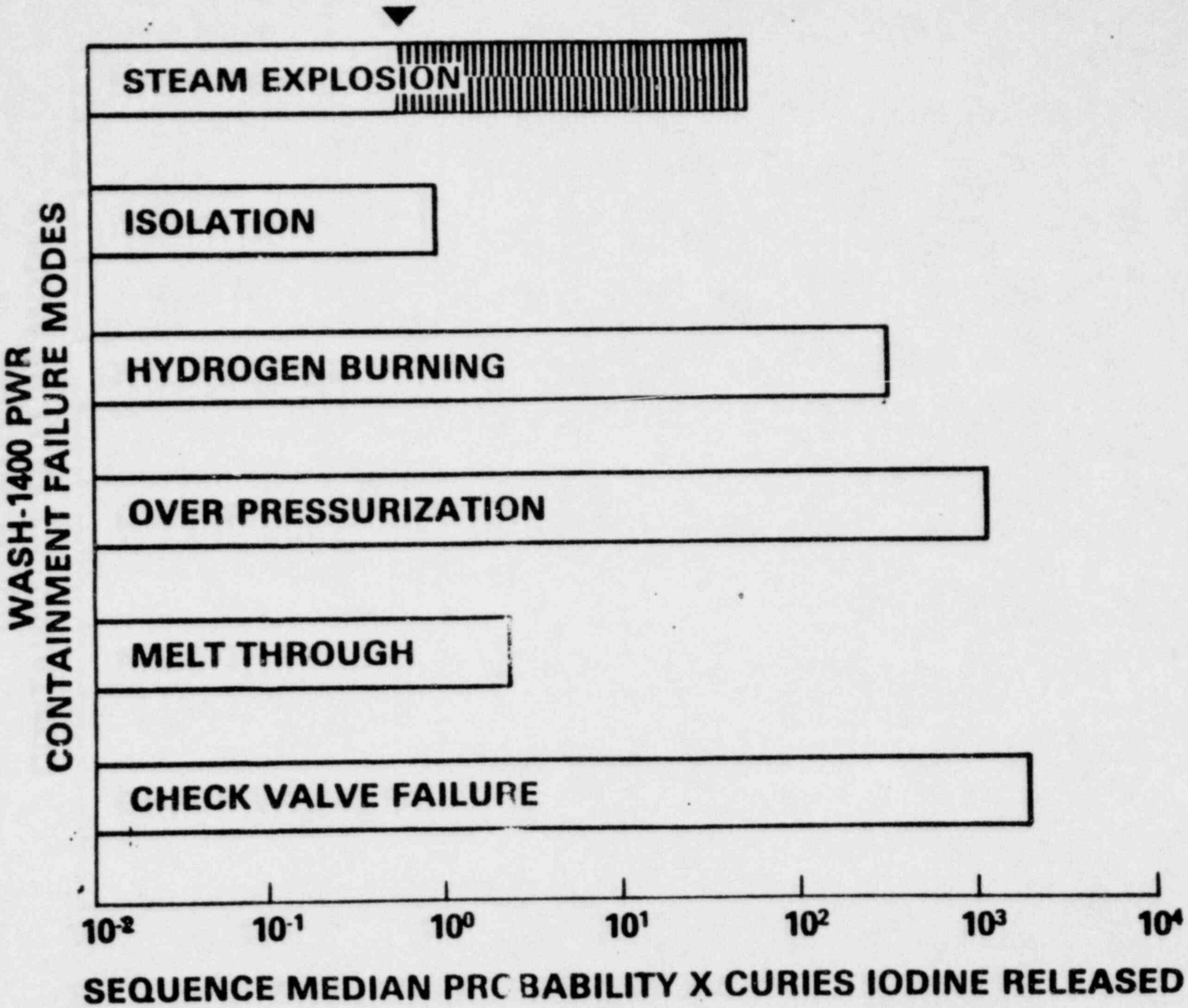


REVISED STEAM EXPLOSION CONTAINMENT FAILURE ESTIMATES FOR PWR WITH LARGE-HIGH PRESSURE CONTAINMENT

	WASH - 1400		CURRENT	
	BEST ESTIMATE	UPPER BOUND	BEST ESTIMATE	UPPER BOUND
P_{α}	.01	.1	.0001	.01
P_{FCC}	1	1	1	1
P_F	0.1	0.3	0.5	1
P_C	0.1	0.3	.0002	.01

WHERE

$$P_{\alpha} = P_{FCC} \times P_F \times P_C$$


WASH-1400 PWR
CONTAINMENT FAILURE MODES
STEAM EXPLOSION
ISOLATION
HYDROGEN BURNING
OVER PRESSURIZATION
MELT THROUGH
CHECK VALVE FAILURE
10^{-2}
10^{-1}
10^{0}
10^{1}
10^{2}
10^{3}
10^{4}
SEQUENCE MEDIAN PRC BABILITY X CURIES IODINE RELEASED

MOLTEN CORE - COOLANT INTERACTIONS

- NEW PROGRAM TITLE IN FY-82 EMPHASIZE NON-EXPLOSIVE INTERACTIONS AS WELL AS STEAM EXPLOSION EVENTS

WORK SCOPE

TASK 1 - STEAM EXPLOSION RESEARCH

- EXPERIMENTAL PROGRAMS (TO BE COMPLETED IN FY-82)
  - SMALL SCALE SINGLE DROPLET TESTS TO DETERMINE EXPLOSITIVITY OF DIFFERENT COMPONENTS OF CORIUM
  - INTERMEDIATE SCALE (10 - 25 KG) FITS TESTS TO INVESTIGATE MIXING PHENOMENA, PROPAGATION, ENERGY CONVERSION RATIOS AND EXPANSION BEHAVIOR IN WELL INSTRAMENTED TEST FACILITY USING CORIUM MELTS
  - LARGE SCALE (> 100 KG) TEST(s) TO ASSESS ENERGY CONVERSION RATIOS AND MIXING PHENOMENA AT LARGER SCALE.

MOLTEN CORE-COOLANT INTERACTIONS

- ANALYTICAL PROGRAMS (WILL CONTINUE BEYOND FY-82)
  - DEVELOPMENT AND APPLICATION OF THE 1-D TRANSIENT EXPLOSION MODEL AND 2-D EMPERICAL MODELS WILL CONTINUE
  - USE AND DEVELOPMENT OF THE STATISTICAL STEAM EXPLOSION CONTAINMENT FAILURE ANALYTICAL MODEL WILL CONTINUE.
  - EMPHASIS ON ANALYZING BWR MARK I, II, AND III CONTAINMENTS.

MOLTEN CORE-COOLANT INTERACTIONS

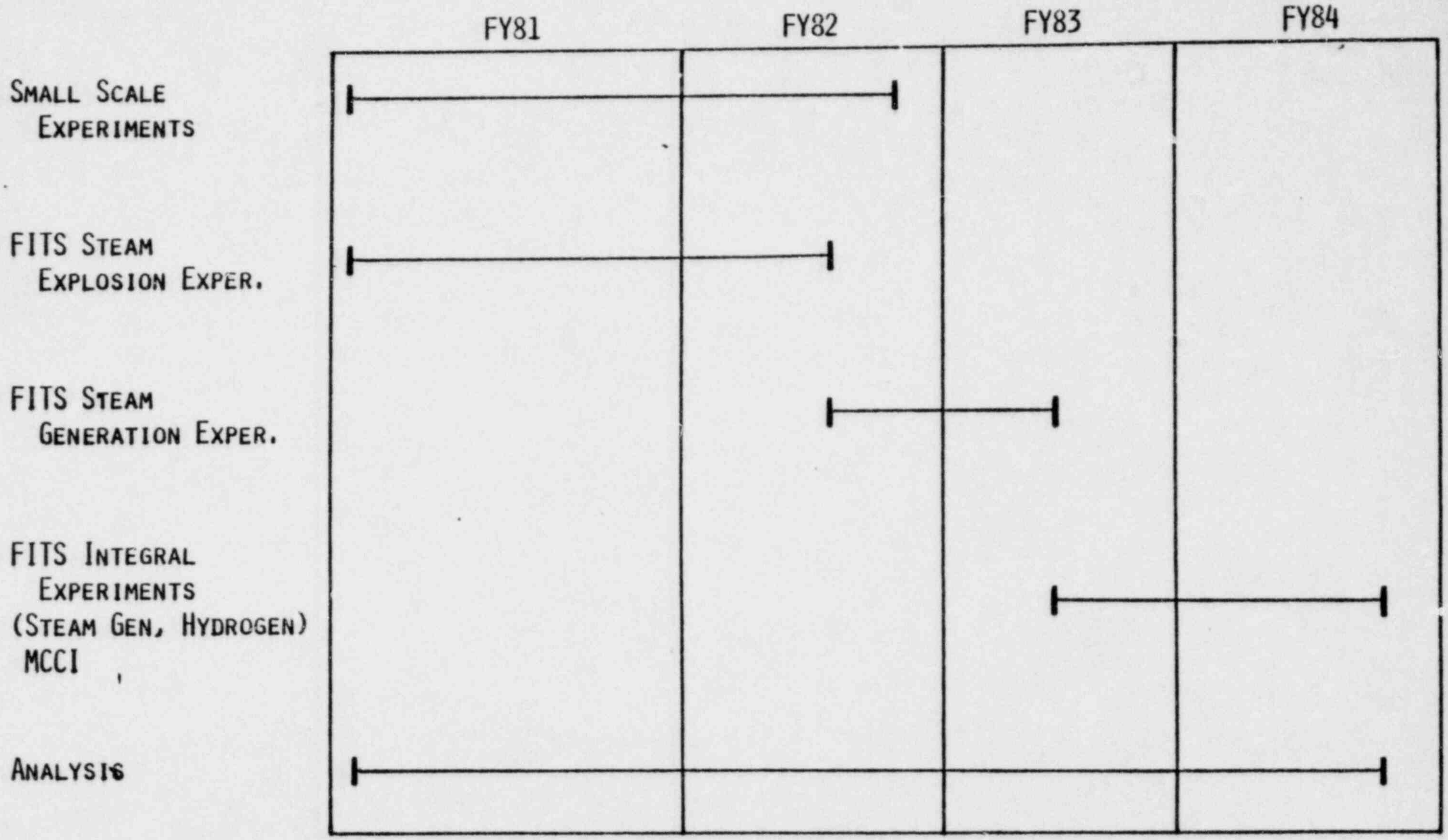
TASK II NON-EXPLOSIVE DEBRIS/COOLANT INTERACTIONS

- EXPERIMENTAL PROGRAM
  - EXPERIMENTS WILL CONTINUE IN THE FITS FACILITY TO INVESTIGATE:
    1) STEAM GENERATION RATES,
    2) HYDROGEN GENERATION RATES (AND COMBUSTION),
    3) MIXING AND BREAK UP PHENOMENA,
    4) DEBRIS FORMATION AND MOVEMENT, AND
    5) INTERACTIONS WITH THE CHAMBER BASE.

- ANALYTICAL WORK
  - THESE TESTS WILL BE COMPARED WITH EXISTING MODELS (E.G. MARCH AND CONTAIN)
  - WHERE CODES ARE INADEQUATE MODEL DEVELOPMENT WILL BE INITIATED (IN CONJUNCTION WITH ONGOING CODE DEVELOPMENT PROGRAMS)

STEAM EXPLOSION RESEARCH PROGRAM

LONG RANGE PLANNING



Tape 3 Powers
8 4
/

STATUS OF
EX-VESSEL CORE DEBRIS INTERACTIONS
STUDIES

Ex-vessel core debris interactions that pose a threat to containment integrity are being studied in two programs. The Molten Core Containment Program deals with core debris interactions with concrete and material interactions of debris with candidate core retention materials. This program provides quantitative experimental data on basemat erosion, gas generation, hydrogen production, aerosol generation and heat release to containment. The second program, Core Retention Concept Assessment, deals with the engineering core retention structure to either prevent or mitigate the effects of ex-vessel interactions with concrete.

The interaction of core debris may be broken down into several temperature regimes. The regime where the core debris is molten has received the most attention. Gas generation during this phase of the interaction is the predominant safety concern. Analyses indicate that gas production during the interaction, when combined with steam generated during the in-vessel portion of an accident may be sufficient to overpressurize the containment. Flammable gases, H_2 and CO, produced during the interaction also contribute to the detonation hazard.

Aerosols produced during ex-vessel interactions have not received much attention. These aerosols not only contribute to radioactive release, they can heighten the threat to containment by interfering with either passive or active containment cooling. Aerosols will coat heat transfer surfaces, plug filters, and clog orifices. Whereas understanding of aerosol behavior in containment is good, the definition of the aerosol source term remains unclear. Obtaining this definition is an objective of the Molten Core Containment Program.

Phases of core debris interactions with concrete that have not been adequately studied are:

(1) attack on concrete by very high temperature ($>2600^{\circ}C$) oxide melts

(2) attack on concrete by solidified or partially solidified core debris

(3) effects of coolant on core debris interaction with concrete

These phases of the interaction are now being studied as part of the Molten Core Containment program. Concrete will be exposed to 200 Kg melts of UO_2-ZrO_2 using the Large-scale Melt Facility described below. Current analyses show that debris, even after solidified, will remain hot enough to erode concrete and generate gas for many days. A systematic experimental analysis of this attack process is now underway. The study will generate the data for modelling this phase of the ex-vessel interactions.

Coolant may be inadvertently dumped onto core debris while it attacks concrete or it may be deliberately added as a mitigation measure. Three principal modes of combined coolant-core debris-concrete interactions may be identified: (1) coolant on fully molten debris, (2) coolant and solid, fragmented debris, and (3) coolant on solid debris and liquified concrete. Scoping experiments with these three modes will be conducted in the Molten Core Containment program.

Core retention schemes to either prevent or mitigate ex-vessel material interactions have been widely suggested. Several of these concepts have been investigated in a scoping sense by the Molten Core Containment program and the Core Retention Concept Assessment program. The rubble bed retention device seems particularly attractive for retrofitting existing reactors. Both these concepts will be intensively and systematically investigated as part of the Core Retention Concept Assessment program.

A major test of the engineering of refractory brick retention devices will be conducted with the Large-scale Melt Facility. The facility allows large core debris melts (>200 Kg) to be prepared at temperatures in excess of $2700^{\circ}C$ and deposited on test structures. Questions of core debris material interactions with MgO, MgO brick stability, floatation and cracking will be answered with this test. Aerosol generation and heat partitioning will also be addressed.

Scoping experiments have shown a rubble bed of ThO_2 gravel is a feasible core retention concept. Systematic study of the performance and engineering of this concept is also part of the Core Retention Concept Assessment program. This study is closely coupled with other NRC-sponsored research efforts such as core debris coolability and the interaction of coolant and core debris.

Both the Molten Core Containment and Core Retention Concept Assessment programs are yielding significant data pertinent to reactor safety concerns. The results from these efforts are being produced in close conjunction with the needs of the licensing activities of NRC and the consideration of severe reactor accidents.

STATUS OF
EX-VESSEL INTERACTIONS PROGRAMS

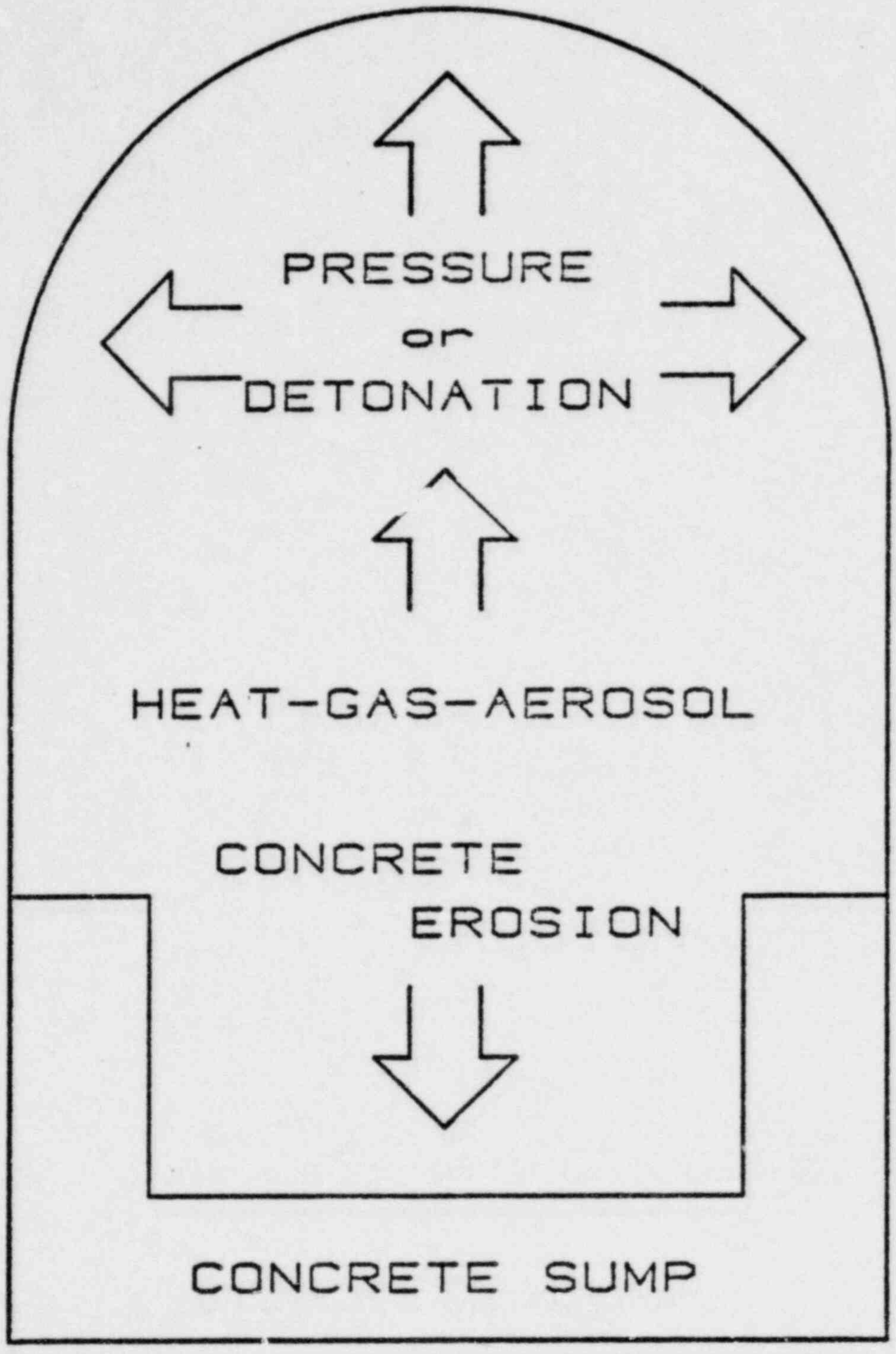
OBJECTIVES

MOLTEN CORE CONTAINMENT:

STUDY OF MATERIAL INTERACTIONS OF CORE DEBRIS WITH CONCRETE AND CANDIDATE RETENTION MATERIALS THAT MIGHT THREATEN CONTAINMENT INTEGRITY

CORE RETENTION CONCEPT ASSESSMENT:

DEFINITION OF THE MEANS TO ENGINEER A CORE RETENTION DEVICE THAT WOULD ELIMINATE OR MITIGATE CONTAINMENT THREATENING INTERACTIONS OUTSIDE THE REACTOR VESSEL


PRESSURE
or
DETONATION
HEAT-GAS-AEROSOL
CONCRETE
EROSION
CONCRETE SUMP

WHY EX-VESSEL INTERACTION STUDIES

- DRIVING FORCE FOR THE LATER STAGES OF AN ACCIDENT.
- THREATEN ABOVE GRADE CONTAINMENT FAILURE.
- THREATEN CONTAINMENT INTEGRITY BY BASE MAT EROSION.
- THREATEN OTHER MITIGATION DEVICES SUCH AS FILTER AND CONTAINMENT COOLERS.
- PROVIDE A LARGE RADIOACTIVITY SOURCE.

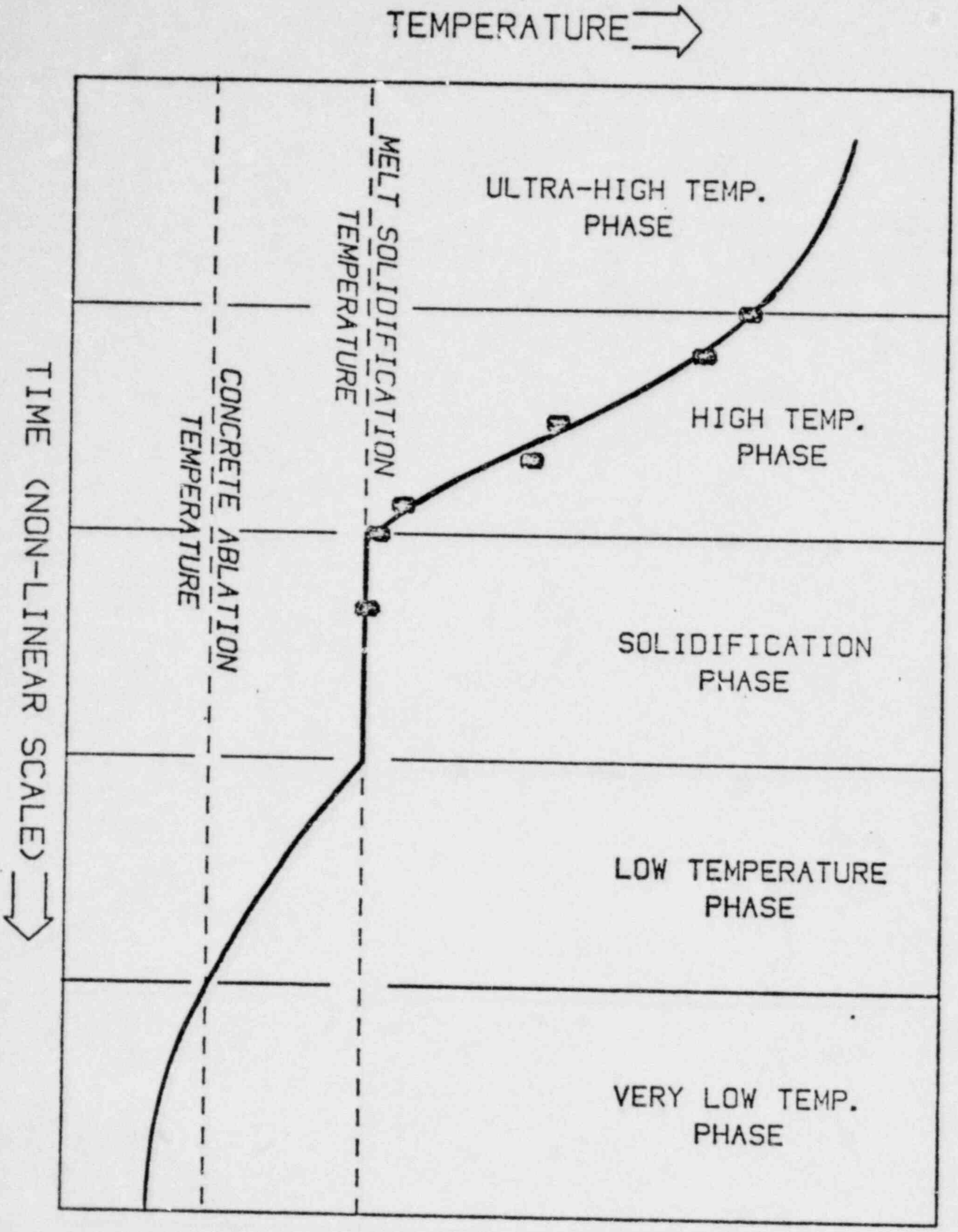
TECHNICAL ISSUES RAISED BY EX-VESSEL INTERACTIONS

CORE CATCHER SYSTEM		*UNPROTECTED PLANT*		*FILTERED VENT SYSTEM*
		⇓		⇓
		GAS GENERATION	⇐	GAS GENERATION
⇓		H_2, CO, CH_4 PRODUCTION	⇐	H_2, CO, CH_4 PRODUCTION
UPWARD HEAT TRANSFER	⇒	UPWARD HEAT TRANSFER		
AEROSOL GENERATION	⇒	AEROSOL GENERATION	⇐	AEROSOL GENERATION
BASEMAT EROSION	⇒	BASEMAT EROSION		

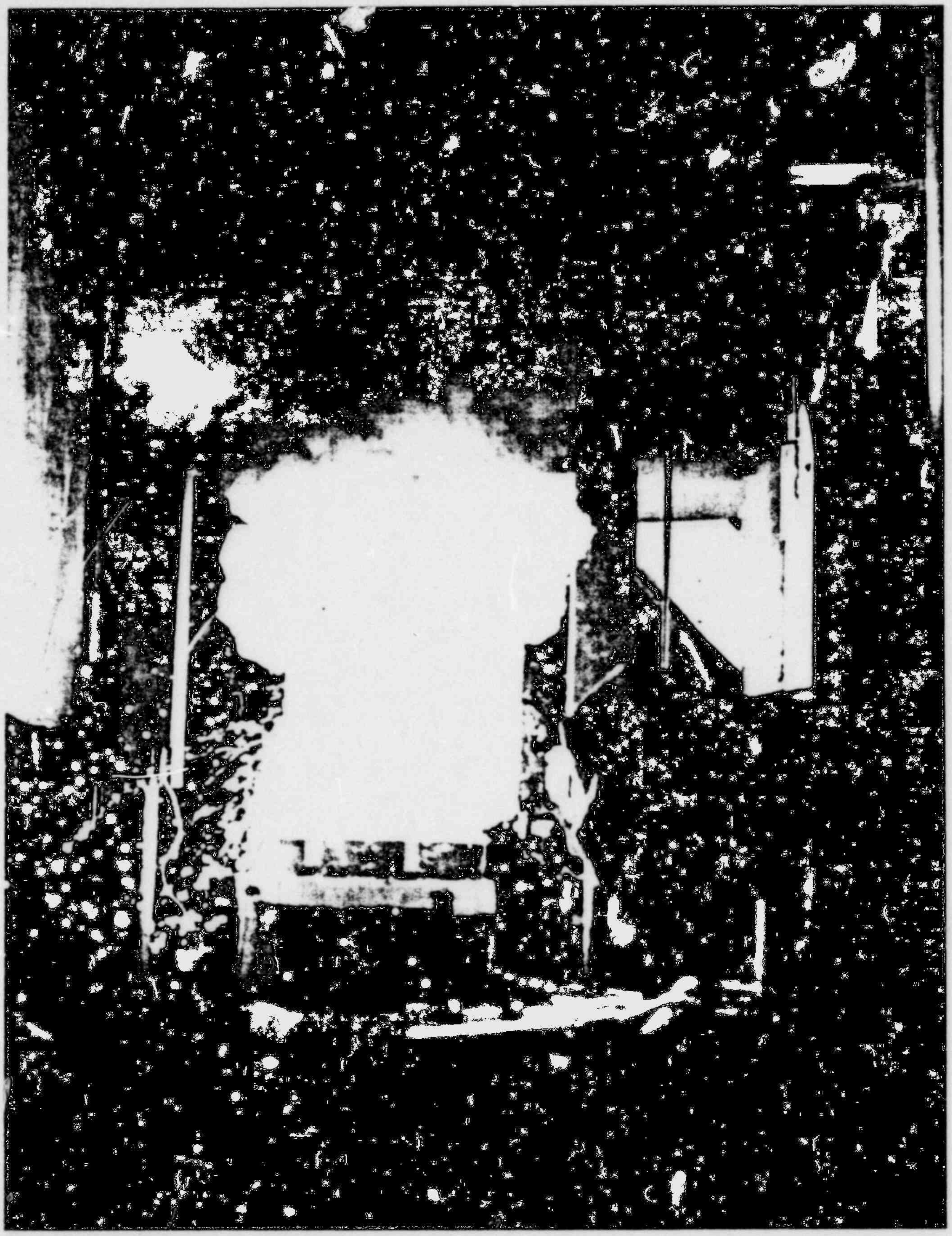
IMPACT OF AEROSOLS SOURCE TERM DURING EX-VESSEL INTERACTIONS

- <u>RADIOACTIVE RELEASE FROM FUEL</u>
  - -- VAPORIZATION
  - -- CHEM TRANSPORT
  - -- MECHANICAL

- <u>AFFECT BOTH NATURAL AND DESIGNED MITIGATION SYSTEMS</u>
  - -- COAT HEAT TRANSFER SURFACES
  - -- PLUG ORIFICES
  - -- CLOG FILTERS
  - -- MAY ALTER ATMOSPHERE

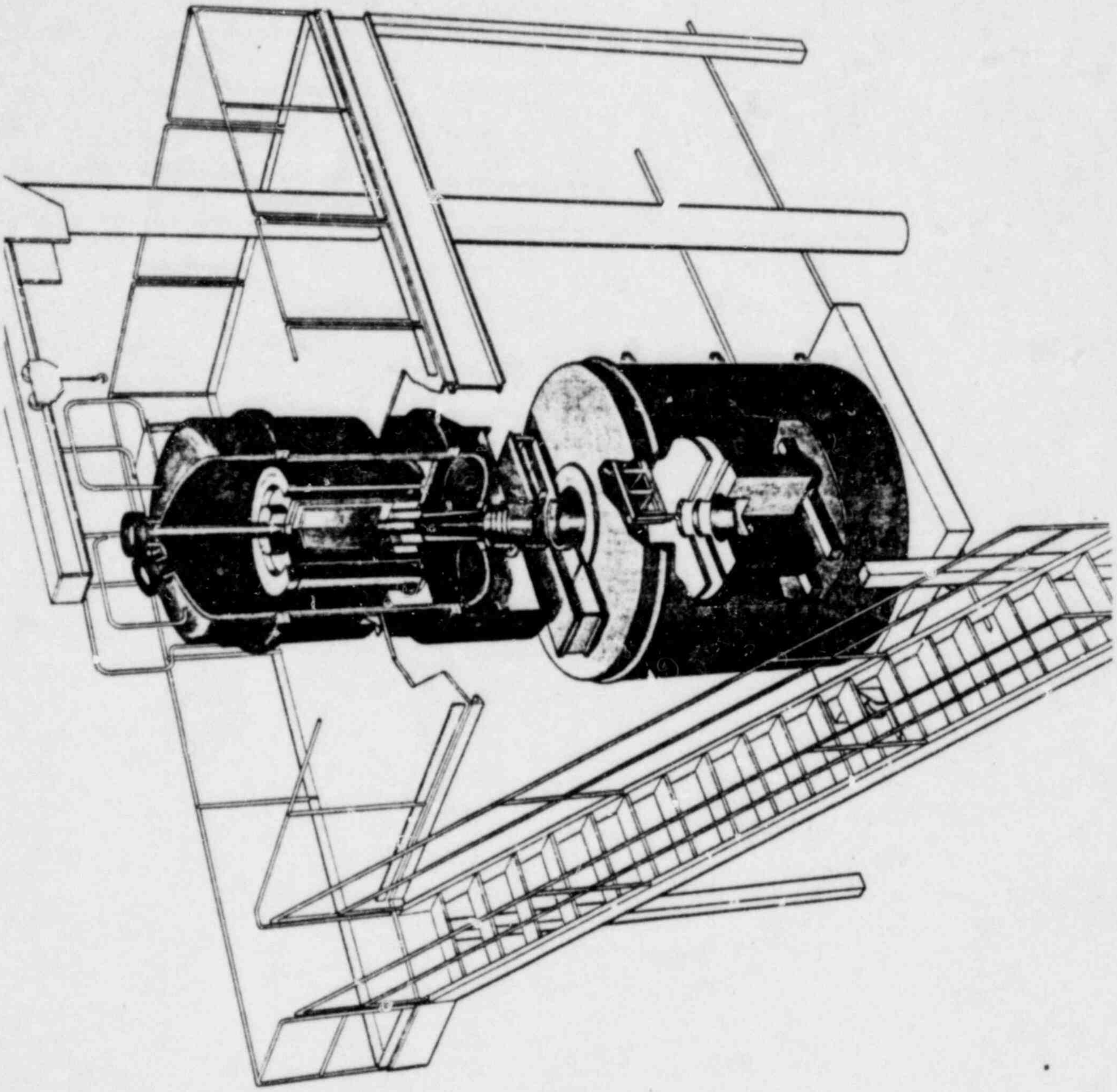


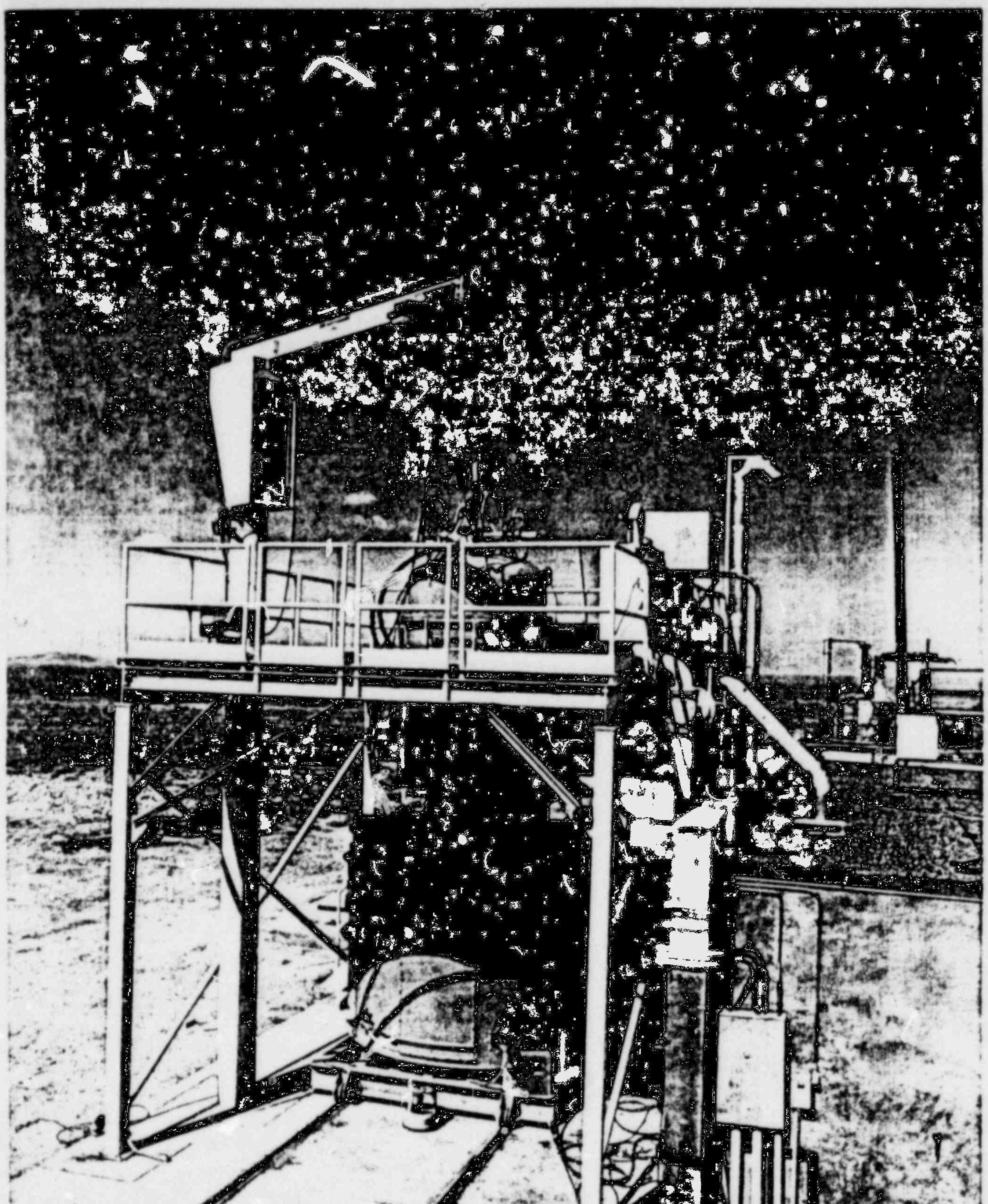
SCHEMATIC MELT TEMPERATURE HISTORY



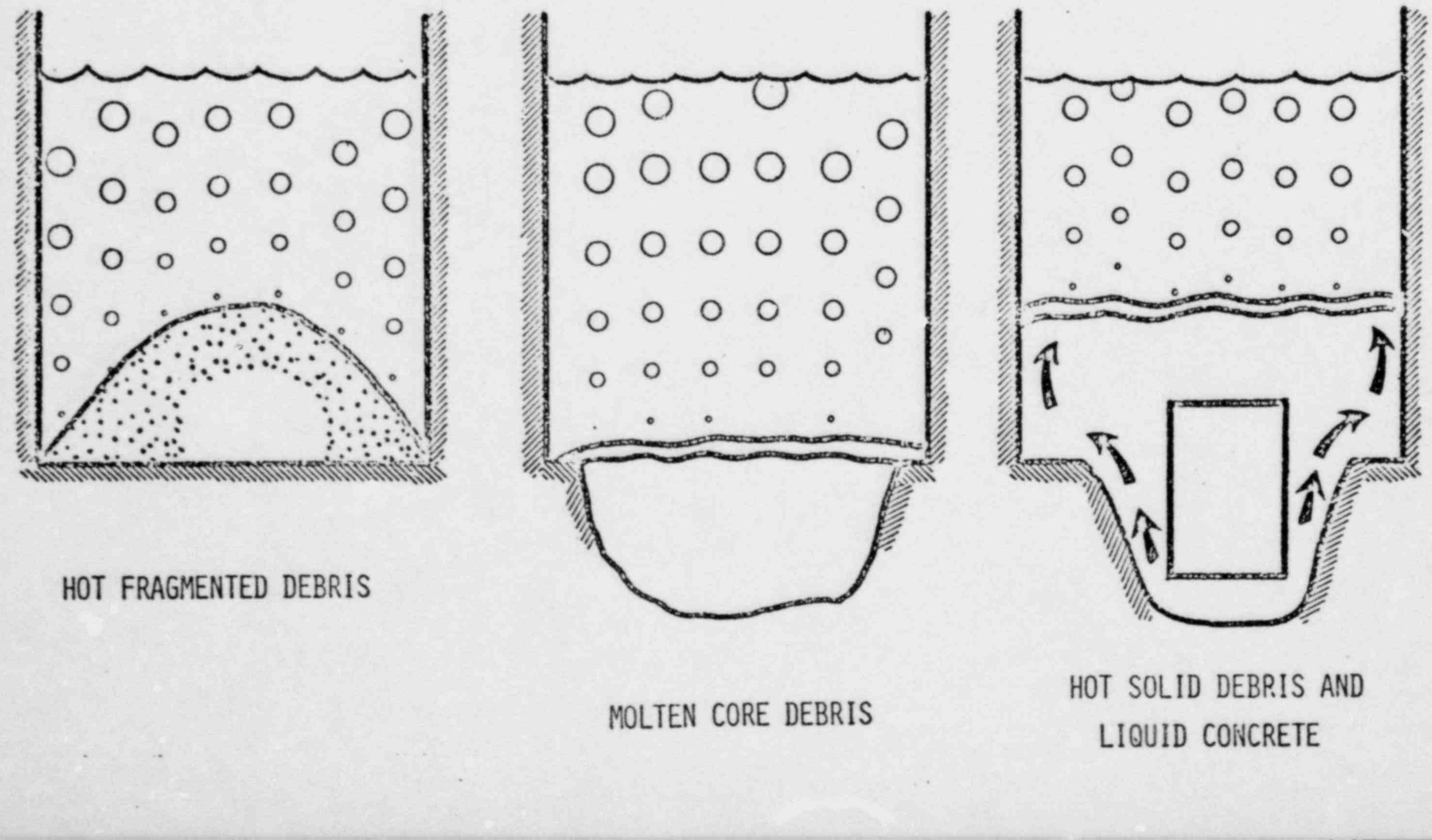
ACTIVITIES UNDERWAY FOR CORE DEBRIS/CONCRETE INTERACTIONS

- SUSTAINED, HOT, SOLID CORE DEBRIS ATTACK ON CONCRETE
- LARGE-SCALE UO_2-ZrO_2 MELT (200 KG) ATTACK ON CONCRETE
- DEFINITION OF AEROSOL SOURCE TERM
- COMBINED INTERACTION OF COOLANT-CORE DEBRIS AND CONCRETE
- EXPERIMENTS TO AID DEVELOPMENT OF THE CORCON MODEL





MODES OF COMBINED COOLANT-CORE DEBRIS-CONCRETE INTERACTIONS



RETENTION SYSTEMS EXAMINED

- SACRIFICIAL BORAX
- COOLED STEEL PLATE
- CASTABLE CERAMICS
- SACRIFICIAL LAYER OVER MgO BRICKS
- RUBBLE BED DEVICES*
- MgO BRICKS*

*CONCEPTS THAT WILL RECEIVE INTENSIVE STUDY

SCHEDULE FOR TESTS AT THE LARGE MELT FACILITY

- CHECKOUT TEST TiO_2 MELT - MgO CRUCIBLE
  JULY, 1981
- 200 KG UO_2-ZrO_2 MELT - MgO CRUCIBLE
  AUGUST, 1981
- 200 KG UO_2-ZrO_2 MELT - CONCRETE CRUCIBLE
  JANUARY, 1982

WHAT WILL BE LEARNED FROM THE LMF TEST OF UO_2-ZrO_2 MELT INTERACTION WITH MgO

- MATERIAL INTERACTION WITH POROUS BRICKS
- CRACK AND SPALL OF BRICKS
- CREVICE PENETRATION BY MELT
- BRICK FLOATATION
- MELT CRUSTING AND HEAT PARTITIONING
- AEROSOL SOURCE TERM

SUMMARY

CORE DEBRIS/CONCRETE INTERACTIONS:

- DATA BASE IS FOR MELT/CONCRETE PHASE
- GAS GENERATION IS QUITE IMPORTANT
- AEROSOL SOURCE TERM NEEDS DEFINITION
- PROGRAM WILL EXTEND DATA BASE
  - HOT, SOLID DEBRIS/CONCRETE
  - COOLANT/CORE DEBRIS/CONCRETE
  - ULTRA HIGH TEMP. OXIDE/CONCRETE

CORE RETENTION DEVICES:

- SEVERAL CONCEPTS EXAMINED
- PERFORMANCE AND ENGINEERING STUDIES OF:
  - RUBBLE BED DEVICES
  - REFRACTORY BRICK DEVICES

Tape 4
Laskins

NRC HYDROGEN PROGRAMS

DIRECT:

- COMBUSTIBLE GAS IN CONTAINMENT
  RES-A1255

- HYDROGEN BURN SURVIVAL
  ANALYSIS AND MODELLING - NRR-A1306
  EXPERIMENTS - RES-A1270

- HYDROGEN BEHAVIOR AND CONTROL PROGRAM
  RES-A1246

INDIRECT:

- MOLTEN CORE INTERACTIONS
  RES-A1019

- STEAM EXPLOSIONS
  RES-A1030

- CORE MELT TECHNOLOGY
  RES-A1218

- CONTAINMENT ANALYSIS
  RES-A1198

- SAFETY MARGINS FOR CONTAINMENT
  RES-A1249

NRC EQUIPMENT SURVIVABILITY PROGRAM

OBJECTIVES:

- DETERMINE THE THERMAL AND MECHANICAL LOADS WHICH COULD BE DELIVERED TO EQUIPMENT DURING HYDROGEN COMBUSTIONS IN TYPICAL LWR ACCIDENTS.

- DETERMINE THE EFFECTS OF THOSE LOADS ON EQUIPMENT, I.E., SURVIVAL, DEGRADATION OF FUNCTION, OR FAILURE.

NRC EQUIPMENT SURVIVABILITY PROGRAM

MAJOR END PRODUCTS

1. ANALYTICAL TOOLS FOR CALCULATING THE THERMAL AND MECHANICAL ENVIRONMENTS LIKELY TO RESULT FROM HYDROGEN DEFLAGRATIONS AND DETONATIONS.

2. PREDICTIONS OF THE EFFECTS OF COMBUSTION ON EQUIPMENT.

3. EXPERIMENTAL INVESTIGATION OF THE EFFECTS OF TRANSIENT LOADS ON REPRESENTATIVE PIECES OF SENSITIVE EQUIPMENT.

COMBUSTIBLE GAS IN CONTAINMENT

OBJECTIVE: DETERMINE THE RATES AND QUANTITIES OF HYDROGEN WHICH CAN BE GENERATED BY THE CORROSION OF ZINC AND OTHER COATINGS IN CONTAINMENT AFTER A HYPOTHETICAL LWR ACCIDENT

REASONS FOR PROGRAM

- RATES OF H_2 GENERATION ARE NOT WELL CHARACTERIZED
- QUANTITY OF THE H_2 GENERATED CAN BE SIGNIFICANT
- NON-DEGRADED CORE ACCIDENTS

SOURCES OF HYDROGEN

FROM CORROSION

- ZINC-BASED PAINTS/PRIMERS
- ZINC GALVANIZING
- ALUMINUM
- ORGANIC MATERIALS
- OTHER MATERIALS

QUESTIONS TO BE ANSWERED

- INVENTORY IN "TYPICAL" CONTAINMENTS
- RATE OF HYDROGEN EVOLUTION

HYDROGEN BEHAVIOR AND CONTROL PROGRAM

OBJECTIVE:

QUANTIFY THE THREAT POSED BY HYDROGEN RELEASED DURING LWR ACCIDENTS AND GENERATE INFORMATION, PROCEDURES AND EQUIPMENT CONCEPTS WHICH WILL PREVENT OR MITIGATE THAT THREAT.

MAJOR END PRODUCTS:

1. ASSESSMENT OF THREAT FOR SEVERAL CLASSES OF PLANTS.
2. ASSESSMENT OF ADEQUACY OF EXISTING SAFETY SYSTEMS AND MITIGATION STRATEGIES.
3. IDENTIFICATION AND CONCEPT DEMONSTRATION OF IMPROVED MITIGATION AND DETECTION SYSTEMS.
4. PUBLICATION OF OPERATOR STRATEGIES, TRAINING AND EMERGENCY MANUALS.
5. CODES FOR ADDRESSING THE TRANSPORT AND COMBUSTION OF HYDROGEN IN CONTAINMENTS.

HYDROGEN PROGRAM: ACCOMPLISHMENTS THROUGH FY81

- COMPENDIUM PUBLISHED.
- HYDROGEN DETECTOR REPORT TO BE PUBLISHED.
- HYDROGEN WORKSHOP HELD, PROCEEDINGS TO BE PUBLISHED.
- SEQUOYAH MITIGATION REPORT PUBLISHED.
- COMPREHENSIVE PROGRAM PLAN DEVELOPED.
- ACCIDENT ANALYSES FOR VARIOUS LWR CONTAINMENTS INITIATED.
- DEFLAGRATION, DETONATION, HEAT TRANSFER CODES TO BE DEVELOPED.
- RALOC CODE OBTAINED, EVALUATION BEGUN.
- CSQ REPORT ON ZION/SEQUOYAH TO BE PUBLISHED.
- WORK INITIATED ON HYDROGEN EMERGENCY MANUAL.
- EXPERIMENTAL TEST PLAN DEVELOPED, PRESENTED, APPROVED.
- LABORATORY EXPERIMENTS INITIATED ON FOGS AND FOAMS.
- VGES 16'-TANK TS #1 AND 2 TO BE COMPLETED AND DRAFT REPORT WRITTEN.
- H_2 STEAM JET FACILITY TO BE DESIGNED, CONSTRUCTED, AND TESTING INITIATED.
- FITS TANK TO BE MODIFIED, TESTING INITIATED.
- EXPERIMENTS ON ACCELERATED FLAMES INITIATED AT MC GILL UNIVERSITY.

HYDROGEN EXPERIMENTAL PROGRAM

OBJECTIVES:

1. CONFIRM EARLIER ANALYTIC AND EXPERIMENTAL WORK.
2. PROVIDE A DATA BASE FOR MODEL DEVELOPMENT AND ASSESSMENT.
3. ANSWER QUESTIONS TOO COMPLEX TO MODEL ANALYTICALLY.
4. EVALUATE EQUIPMENT SURVIVAL AND MITIGATION SCHEMES UNDER CONDITIONS SIMULATING LWR ACCIDENTS.
5. INDICATE THE EFFECTS OF SCALE TO PERMIT RESULTS TO BE EXTRAPOLATED TO REACTOR-SCALE ACCIDENTS.

HYDROGEN EXPERIMENTAL PROGRAM

- LABORATORY-SCALE
- VGES 16-FT TANK
- FITS TANK
- STEAM:HYDROGEN JET
- VGES PLASTIC BAG SETUP
- VGES ACCELERATED FLAME SETUP
- THUNDER TUBE
- VGES TRENCH (LARGE-SCALE)
- MC GILL TESTS
- PLANT-SCALE

WATER FOG LABORATORY EXPERIMENTS

OBJECTIVES:

1. INVESTIGATE THE CHARACTERISTICS AND DENSITIES OF WATER FOGS IN SMALL SCALE MODELS OF REACTOR SYSTEMS.

2. DEVELOP AND TEST INSTRUMENTATION TECHNIQUES FOR MEASURING MEAN FOG DENSITIES AND DROPLET DIAMETER DISTRIBUTIONS.

3. CHARACTERIZE FOG DEPENDENCE ON NOZZLE TYPE, MANIFOLD CONFIGURATION, FLOW RATE AND HEIGHT TO COMPARE AGAINST ANALYTIC MODELS AND TO PROVIDE BENCHMARKS FOR LATER COMBUSTION EXPERIMENTS IN VGES AND FITS.

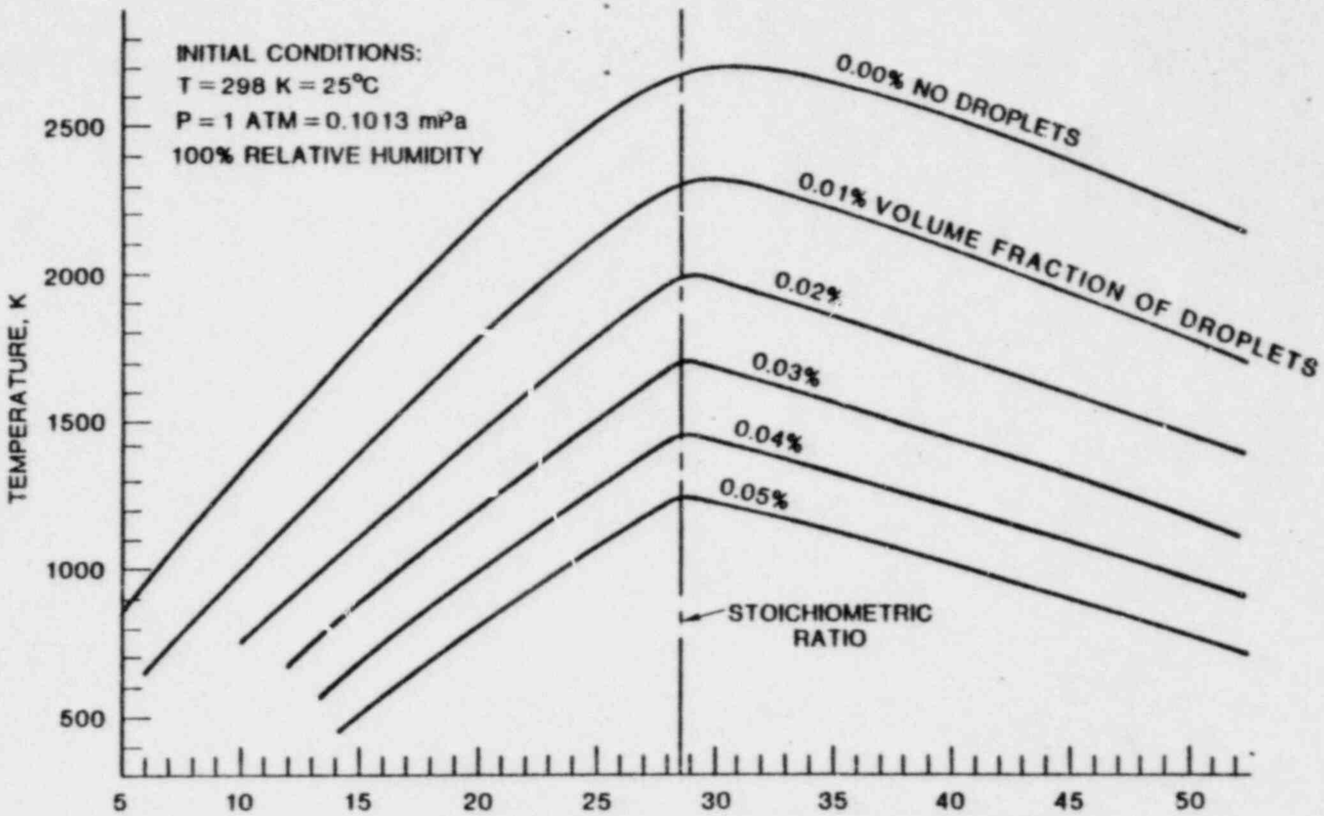


Figure 5.1. Effect of Droplet Vaporization on Adiabatic Isochoric Hydrogen Combustion

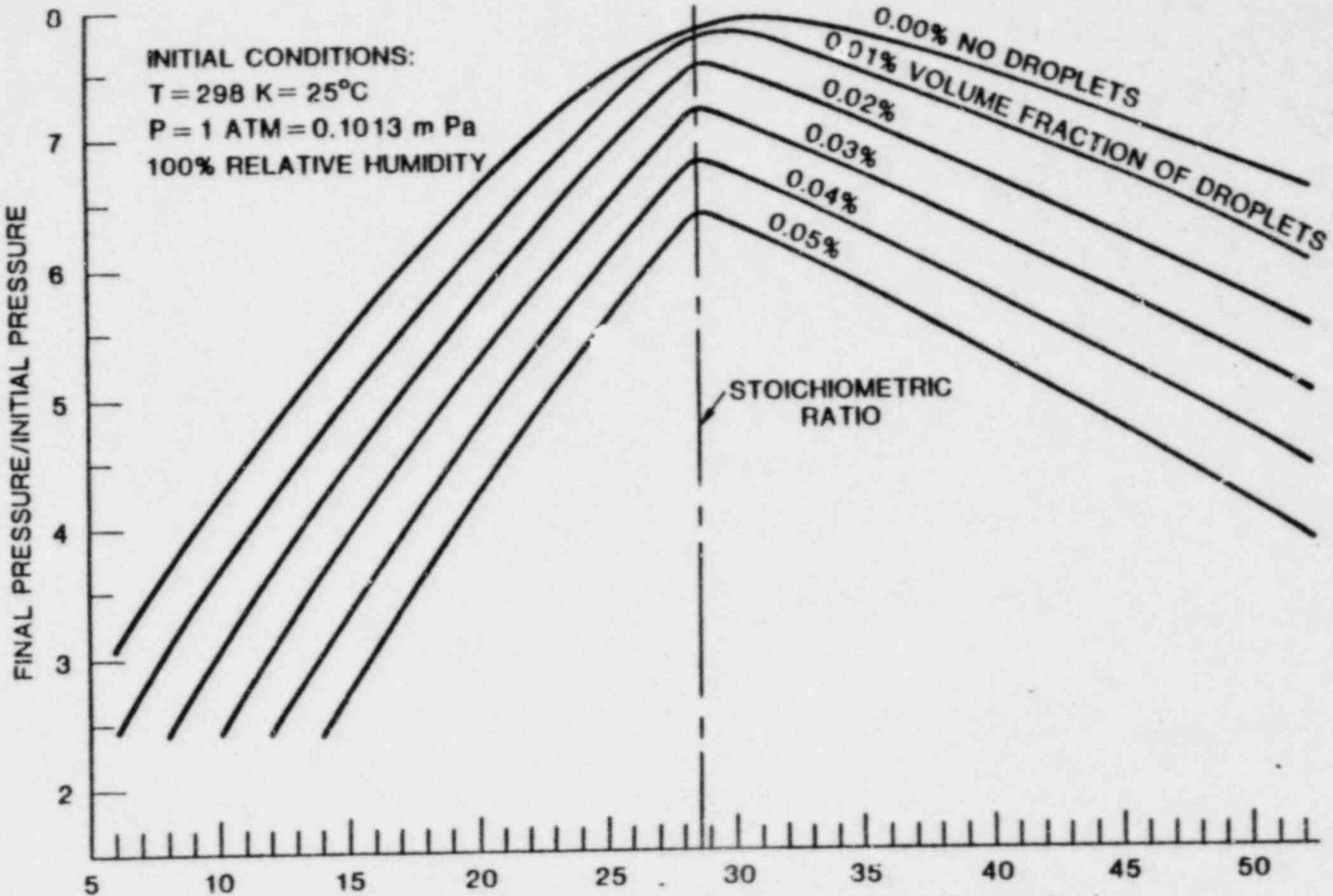
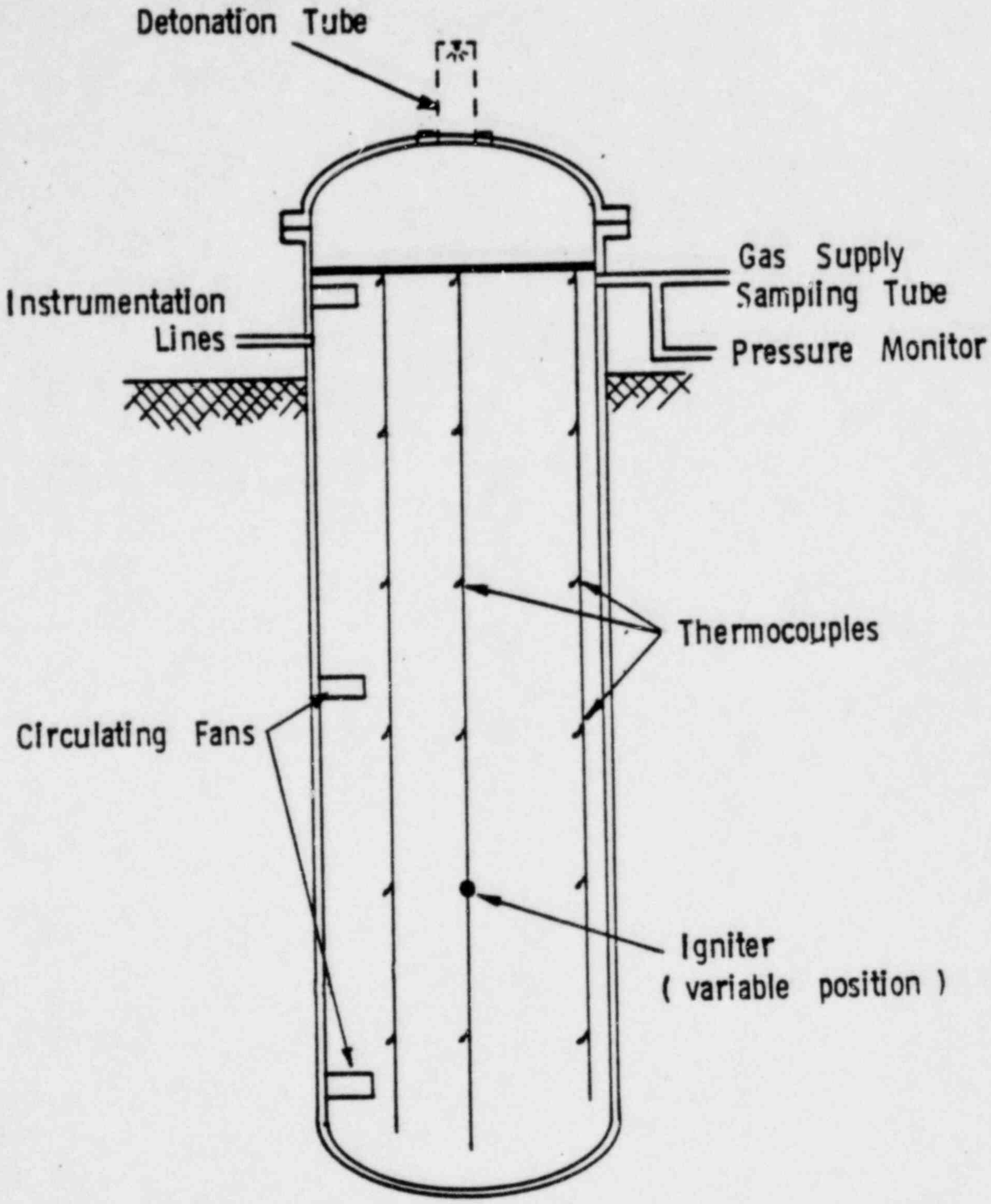


Figure 5.2. Effect of Droplet Vaporization on Adiabatic Isochoric Hydrogen Combustion.

VGES: 16 (14) - FT TANK

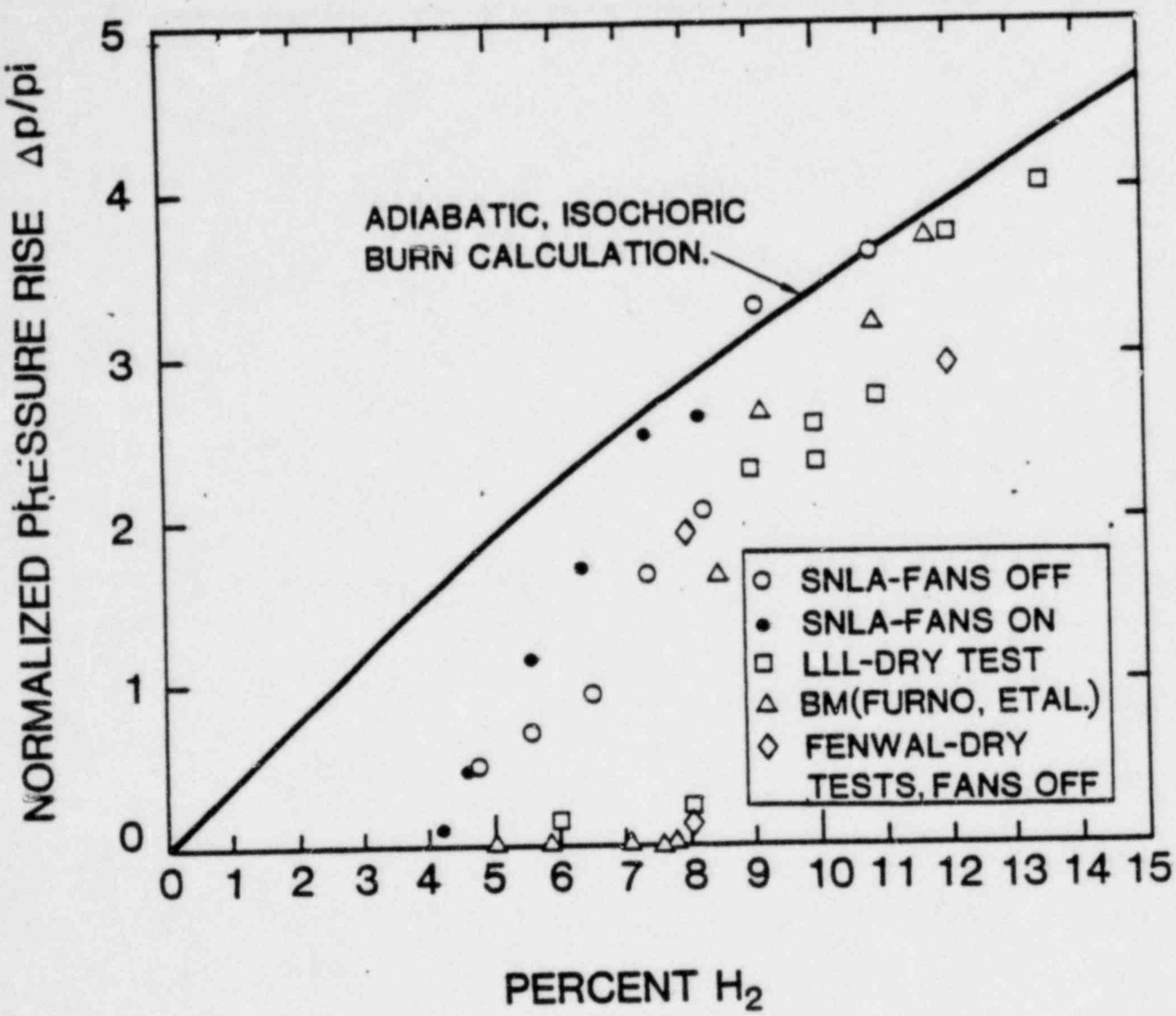
SIZE:	14-16 ft. in length 4 ft. in diameter 175-200 ft^3 (5.0-5.7 m^3) in volume
STRENGTH:	200 PSI (Safety Factor 4)
INSTRUMENTATION:	Pressure Transducers Thermocouples Gas Sampling
INFORMATION:	Scoping
EXPERIMENTS:	Hydrogen Concentration Igniter Type, Location Turbulence Water Fogs Obstacles, Ducts Halon Shock Ignition CO_2 Equipment Survivability Additional Tests (Foams)

VGES: 16 (14) - FT TANK

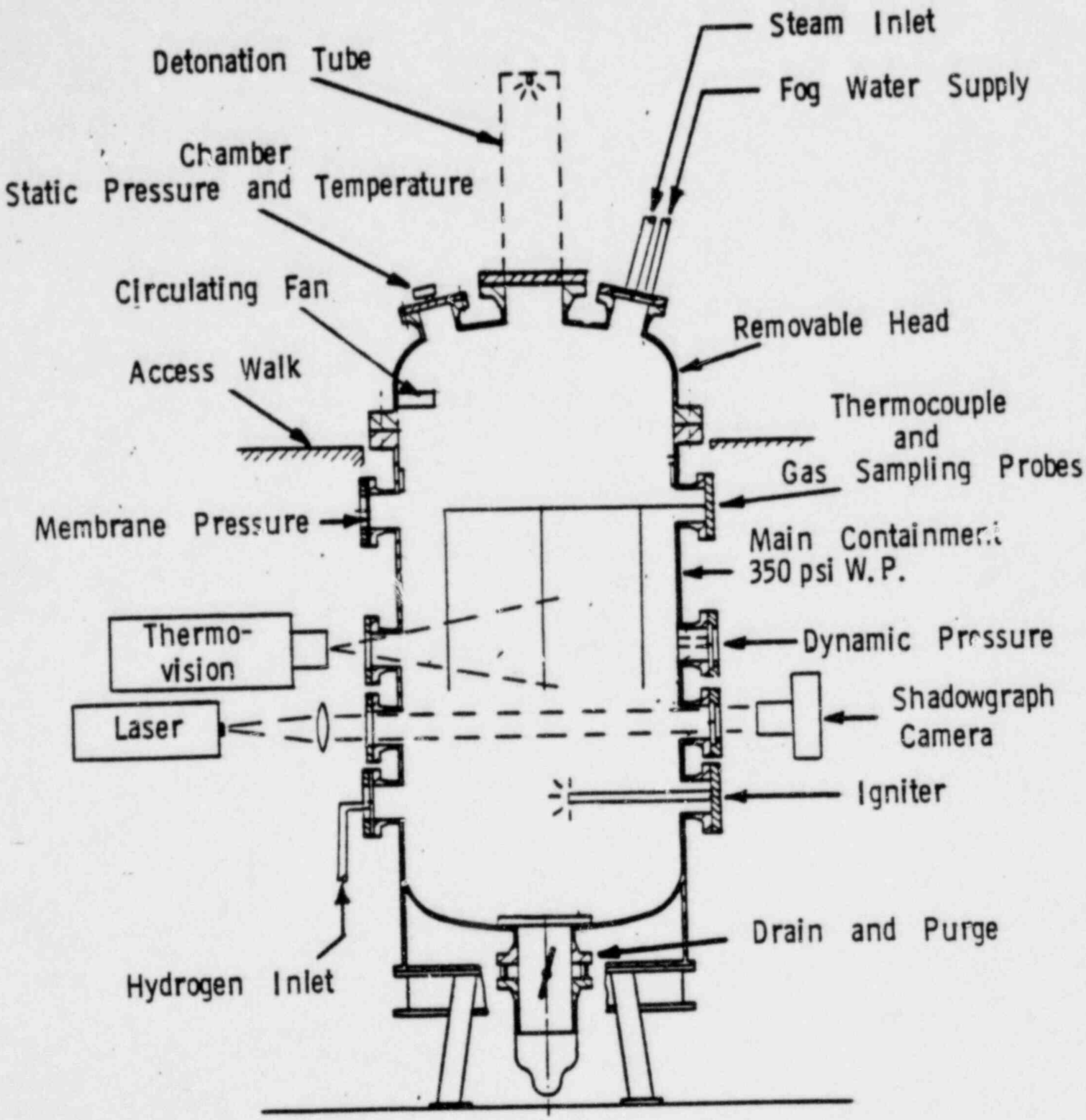


1.22 m (4 ft) diameter . 4.27 m (14 ft) tall

5.0 m^3 (175 ft^3) volume


NORMALIZED PRESSURE RISE Δp/pi
ADIABATIC, ISOCHORIC
BURN CALCULATION.
SNLA-FANS OFF
SNLA-FANS ON
LLL-DRY TEST
BM(FURNO, ETAL.)
FENWAL-DRY
TESTS, FANS OFF
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
0 1 2 3 4 5
PERCENT H_2

MODIFIED FITS TANK



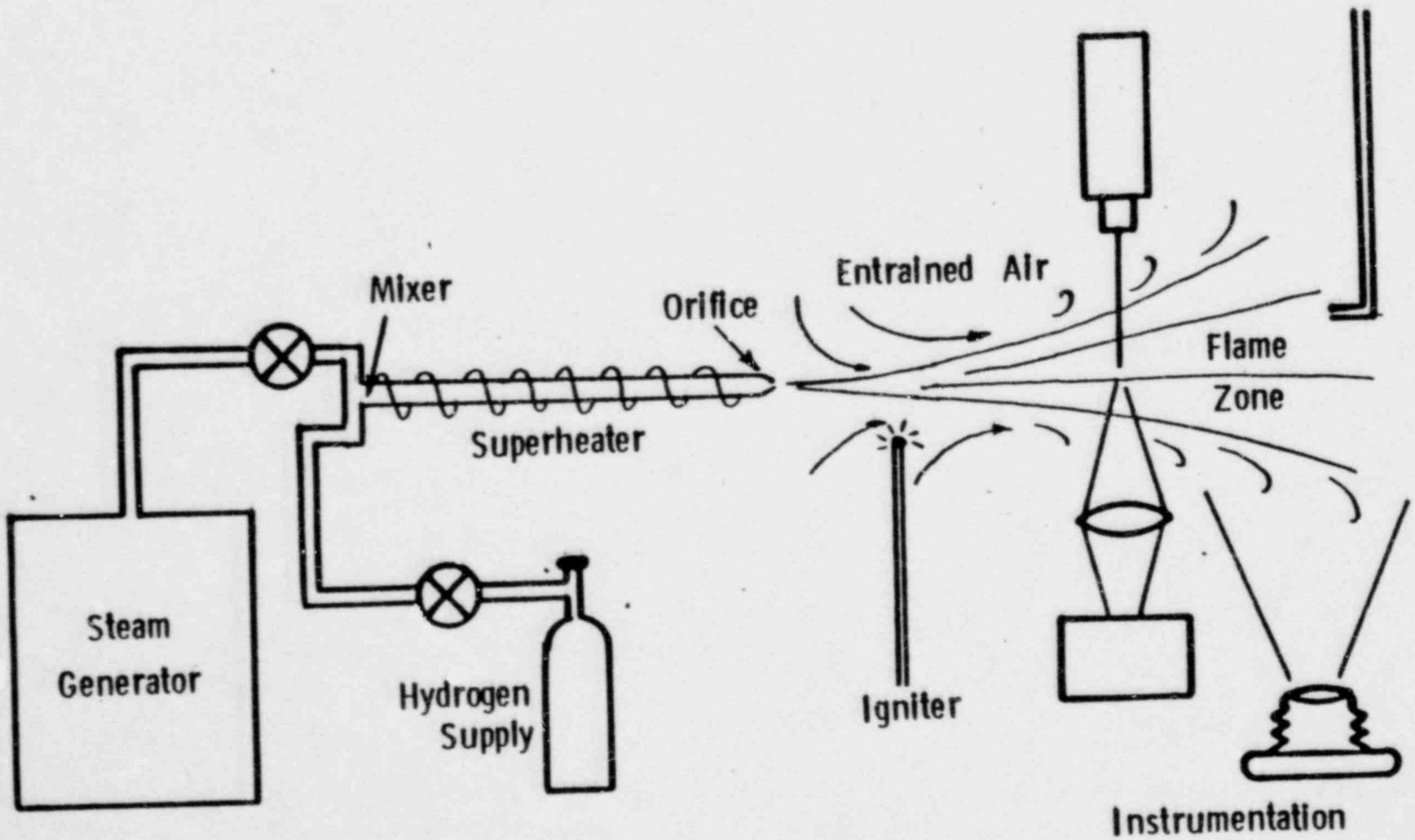
1.5 m (4.9 ft) diameter 3.4 m (11.2 ft) length

5.6 m^3 (195 ft^3) volume

FITS TANK

SIZE:	11 ft. in length
	5 ft. in diameter
	$\sim$200 $ft.^3$ (5.6 M^3) in volume
STRENGTH:	500 PSI (Safety Factor 4)
INSTRUMENTATION:	Pressure Transducers
	Thermocouples
	Gas Sampling
	Laser Shadowgraphy
	Photography
	Infrared Television
	Strain Gages
INFORMATION:	Detailed Data
EXPERIMENTS:	Hydrogen Concentration
	Initial Temperature
	Igniter Type, Location
	Steam
	Deflagrations
	Detonations
	Obstacles, Ducts
	Shock Ignition
	Mitigation Schemes
	Hydrogen Mixing, Transport
	Inhomogeneous Combustion

HYDROGEN:STEAM JET FACILITY



STEAM : HYDROGEN JET

SIZE:	SMALL ORIFICE (0.3-3CM) INITIALLY
FLOW:	CONTINUOUS OR PULSED UP TO 50 LBS/HR STEAM SUBSONIC TO SUPERSONIC
TEMPERATURE:	VARIABLE UP TO 1200 K INITIALLY
PRESSURE:	VARIABLE UP TO 100 PSI INITIALLY
INSTRUMENTATION:	THERMOCOUPLES LASER SHADOWGRAPHY PHOTOGRAPHY INFRARED TELEVISION PITOT PROBES GAS SAMPLING
INFORMATION:	SCOPING AND DETAILED DATA
EXPERIMENTS:	HYDROGEN CONCENTRATION JET TEMPERATURE JET MOMENTUM IGNITION THRESHOLDS AUTOIGNITION FLAMEHOLDERS, OBSTACLES

ACCELERATED FLAMES

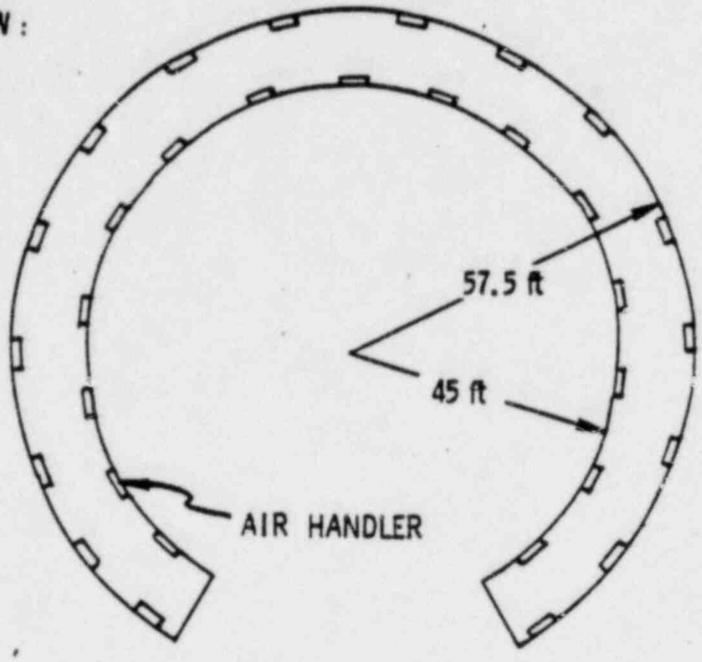
MCGILL SUPPORT, JOHN LEE ET AL.

- LONG TERM EFFORTS INCLUDE SMALL SCALE EXPERIMENTS AND MODELLING TO ADDRESS

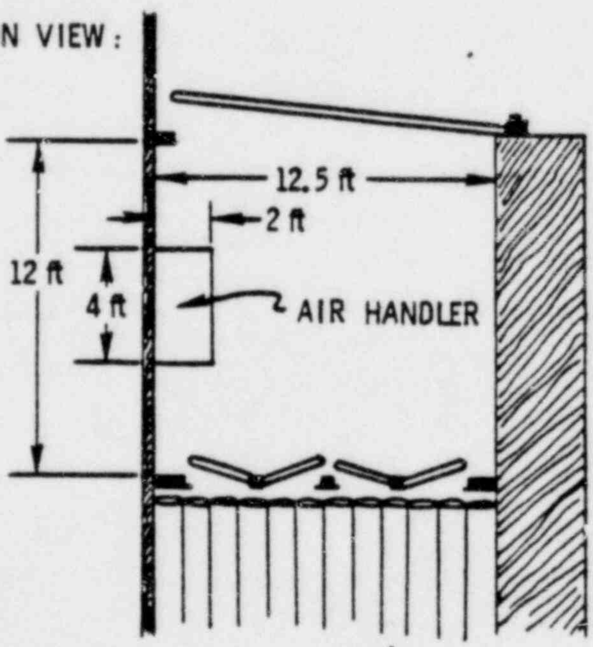
    1. THE EFFECTS OF REPEATED OBSTACLES (DIFFERENT GEOMETRIES AND SPACINGS) AND VARYING DEGREES OF CONFINEMENT ON FLAME ACCELERATION,
    2. THE TRANSITION DISTANCE AS A FUNCTION OF OBSTACLE ENVIRONMENT AND DEGREE OF CONFINEMENT,
    3. STRONG (JET) IGNITION AND DIRECT INITIATION OF DETONATIONS,
    4. THE TRANSMISSION OF DETONATIONS THROUGH TUBES AND OTHER GEOMETRIES,
    5. THE LEAN LIMITS OF DETONABILITY FOR VARYING DEGREES OF CONFINEMENT.

SCHEMATIC OF ICE CONDENSER UPPER PLENUM REGION

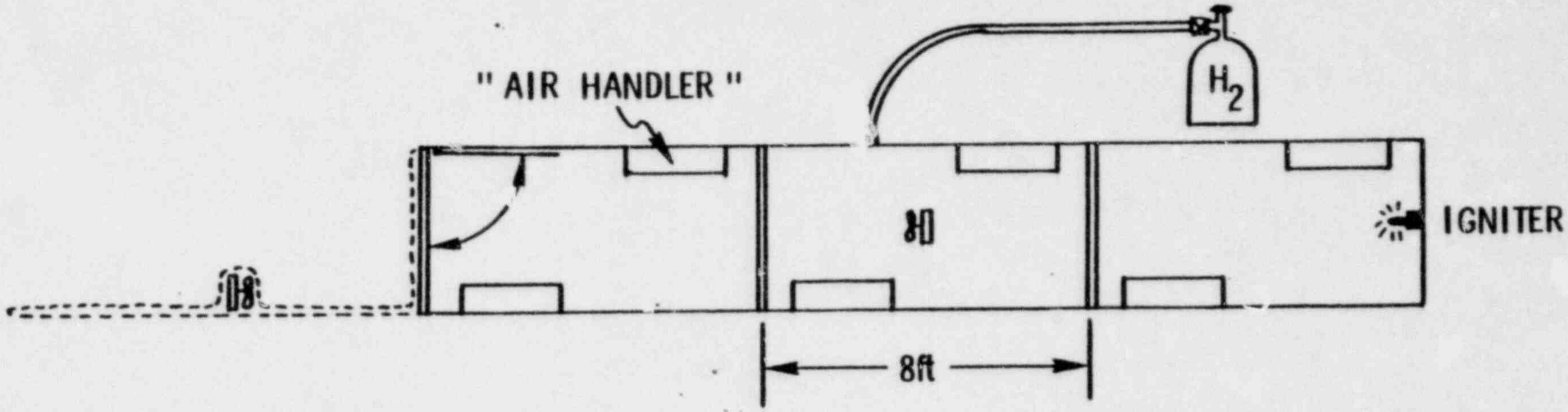
TOP VIEW:



CROSS SECTION VIEW:



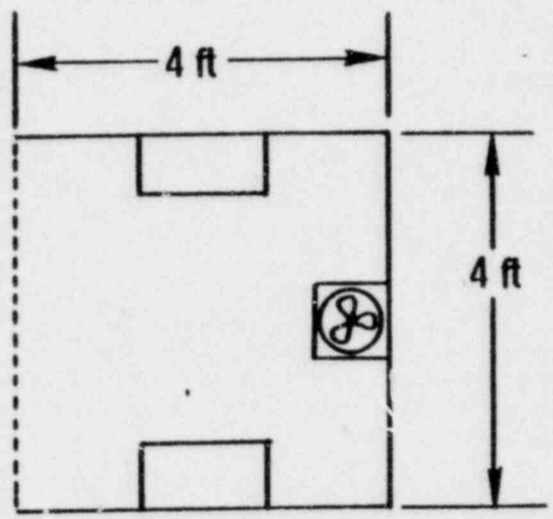
VGES: ACCELERATED-FLAME SETUP
1/3-SCALE ICE CONDENSER UPPER PLENUM REGION



WALL MATERIALS:

——— PLYWOOD

- - - - - - - PLASTIC SHEET



ACCIDENT ANALYSES

OBJECTIVE: ESTIMATE THE QUANTITIES AND RATES OF HYDROGEN GENERATED DURING POSTULATED ACCIDENTS FOR VARIOUS CONTAINMENTS.

- PRESENTLY USING MARCH

- CONTAINMENTS

LARGE, DRY PWR	-	ZION
ICE CONDENSER	-	SEQUOYAH
BWR MARK III	-	GRAND GULF
SUBATMOSPHERIC PWR	-	SURRY

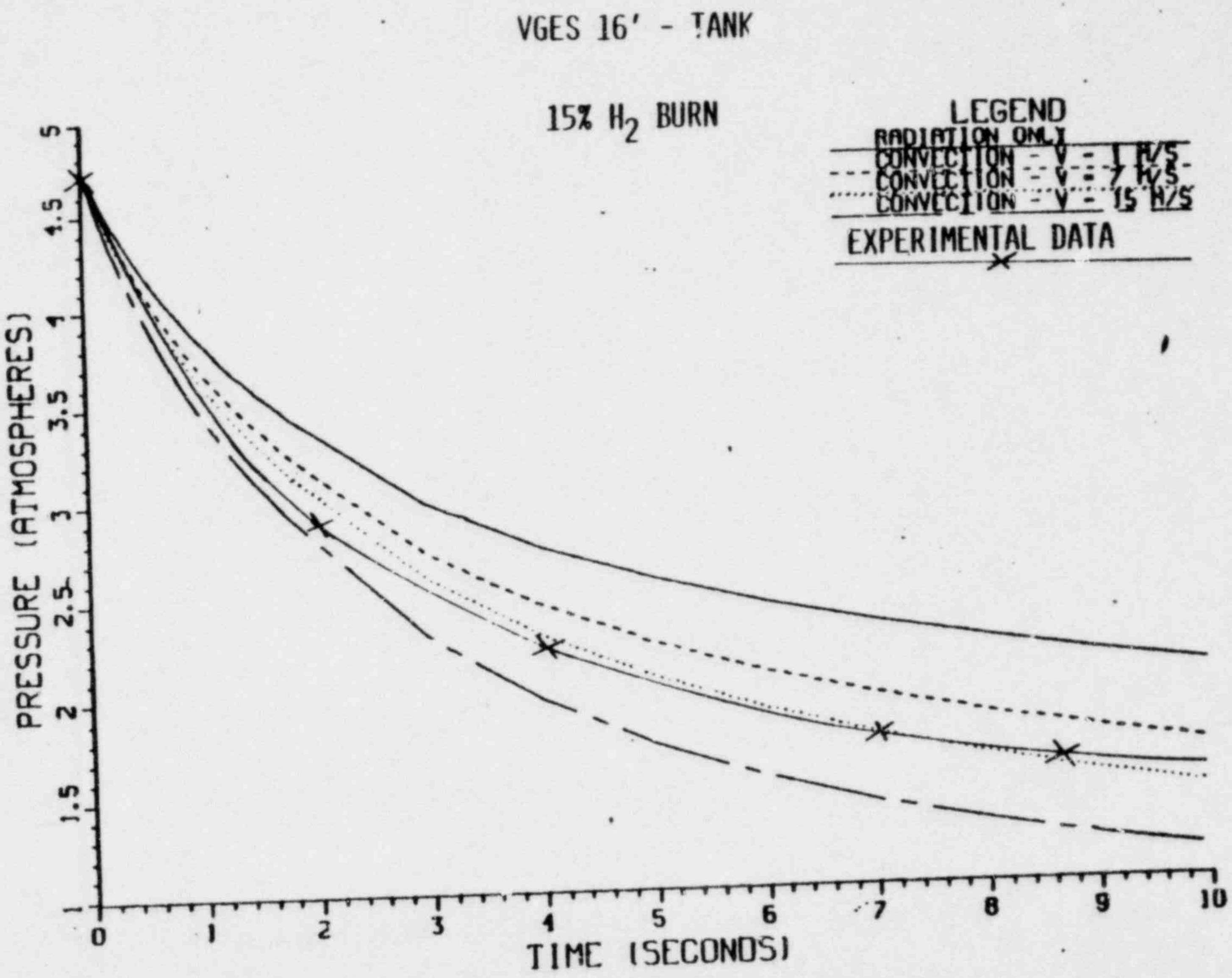
COMBUSTION AND HEAT TRANSFER MODELLING

OBJECTIVE: DEVELOP A CAPABILITY TO PREDICT TEMPERATURE AND PRESSURE HISTORIES IN CONTAINMENT DURING AND AFTER A HYDROGEN COMBUSTION.

- DEFLAGRATIONS: CODE TO PREDICT ADIABATIC, ISOCHORIC Ps AND Ts, INCLUDING CO_2, CO, AND WATER FOG EVAPORATION.

- DETONATIONS: CODE TO PREDICT CHAPMAN-JOUGUET Ps AND Ts, INCLUDING INCREASES AFTER NORMAL REFLECTION.

- HEAT TRANSFER: CODES WHICH ADDRESS RADIATION, CONVECTION WITH OR WITHOUT CONDENSATION, CONDUCTION INTO SURFACES, AND EVAPORATION OF SPRAYS.


VGES 16' - TANK
15% H_2 BURN
LEGEND
RADIATION ONLY
CONVECTION - V = 1 M/S
CONVECTION - V = 7 M/S
CONVECTION - V = 15 M/S
EXPERIMENTAL DATA
PRESSURE (ATMOSPHERES)
TIME (SECONDS)
1
1.5
2
2.5
3
3.5
4
4.5
5
0
1
2
3
4
5
6
7
8
9
10

PLANNED IMPROVEMENTS TO THE CODE

- EXPAND TO TREAT NON-HOMOGENEOUS GASES.
- DEVELOP AND INCORPORATE A BURN MODEL.
- IMPROVE THE EFFICIENCY OF THE CODE.
- CONTINUE TO UPGRADE THE VARIOUS MODELS TO KEEP THE CODE AS NEAR THE "STATE OF THE ART" AS POSSIBLE.

HYDROGEN TRANSPORT MODELLING

OBJECTIVE: DEVELOP A CAPABILITY TO PREDICT THE CONCENTRATIONS OF HYDROGEN, AIR AND STEAM IN CONTAINMENT AS FUNCTIONS OF POSITION AND TIME FOR HYPOTHETICAL LWR ACCIDENTS.

- ASSESSMENT OF RALOC
- ASSESSMENT OF OTHER CODES
- TRANSPORT AND MIXING EXPERIMENTS

1. BATTELLE - FRANKFURT
2. EPRI - HEDL
3. IN SITU TESTING WITH HELIUM BEFORE LICENSING
4. SANDIA EXPERIMENTS

CSQ CODE MODELLING

OBJECTIVE: DETERMINE THE IMPULSIVE LOADS WHICH COULD RESULT FROM DETONATIONS IN VARIOUS CONTAINMENTS DURING HYPOTHETICAL LWR ACCIDENTS.

- MODIFICATIONS TO CSQ
- CALCULATIONS OF GLOBAL DETONATIONS IN ZION
- CALCULATIONS OF VARIOUS LOCAL DETONATIONS IN SEQUOYAH
- REPORT BEING WRITTEN

LWR HYDROGEN MANUAL

OBJECTIVE: PREPARE A MANUAL ON THE HANDLING OF HYDROGEN DURING AND AFTER AN ACCIDENT WHICH CAN BE USED BY REACTOR POWER PLANT DESIGNERS AND OPERATORS AS A BASIS FOR PREPARING THEIR OWN PLANT-SPECIFIC OPERATING AND EMERGENCY MANUALS.

LWR HYDROGEN MANUAL

GOALS:

- ASSESS HYDROGEN-PRODUCING ACCIDENT SCENARIOS FROM OPERATOR'S VIEWPOINT (INSTRUMENTATION).
- PROVIDE INFORMATION USEFUL IN DEFINING "GENERIC" OPERATOR ACTIONS.
- RECOMMEND SPECIFIC OPERATOR ACTIONS (AND TIMING) USING PRESENTLY AVAILABLE EQUIPMENT.
- RECOMMEND POST-ACCIDENT ACTIONS TO DISPOSE OF HYDROGEN CONTAINING FISSION-PRODUCT GASES (PRIMARY SYSTEM AND CONTAINMENT).

SCHEDULE:

DRAFT MANUAL AVAILABLE SUMMER 1982.

SASA OBJECTIVE

IMPROVE UNDERSTANDING OF SEVERE REACTOR ACCIDENTS AND OF EVENTS BEYOND THE DESIGN BASIS UTILIZING BEST ESTIMATE STATE-OF-THE ART ANALYSES AND CODES

SASA PARTICIPANTS

NRC: RES, ASSISTED BY NRR, DRA

CONTRACTORS: INEL, LANL; PWR "FRONT-END" TO CORE DISASSEMBLY
SNL ; PWR "BACK-END" TO CONTAINMENT BREACH
ORNL ; BWR FRONT-AND BACK-END

UTILITIES: TVA (BROWN'S FERRY, GE) } PROVIDE INFORMATION, REVIEW DRAFT REPORTS FOR ACCURACY OF PLANT DESCRIPTIVE INPUT
(ZION; W)

INEL SASA PROGRESS

JANUARY - MAY, 1981

- PERFORMED PWR STATION BLACKOUT ANALYSES
- ASSISTED ORNL IN BWR STATION BLACKOUT ANALYSES
- STUDIED ACCIDENT MITIGATION FOLLOWING A SMALL BREAK WITH COINCIDENT FAILURE OF CHARGING AND HIGH PRESSURE INJECTION FOR THE ZION-1 PWRS
- PRODUCED ACCIDENT SIGNATURE CALCULATIONS TO ASSIST OPERATOR IN PLANT STATE DETERMINATIONS

LANL SASA PROGRESS

JANUARY - MAY, 1981

FOCUS: 1. LOCA'S IN PWR SYSTEM SPACE OF PCS (PV-LOCA)

2. INTERFACING SYSTEM LOCA'S

ACCOMPLISHMENTS:

- CALCULATED "HANDS-OFF" ACCIDENT SCENARIOS FOR PV-LOCA ASSUMING
  1) FAILURE OF PORV TO RECLOSE AFTER LOSS-OF-FEEDWATER PLUS
  2) FAILURE TO INSERT CONTROL RODS
- CALCULATED SCENARIOS ASSUMING U-TUBE RUPTURE IN ONE STEAM GENERATOR (IS-LOCA)
- CALCULATED PLANT RESPONSE TO PV-LOCA'S AND IS-LOCA'S
- DEFINED OPERATIONAL SAFETY QUESTIONS FOR IS-LOCA AND PV-LOCA'S

PROGRESS REPORT - JAN-MAY, 1981

SASA - SANDIA NATIONAL LABORATORIES

1. ZION STATION BLACKOUT
   - -DRAFT REPORT SUBMITTED (NUREG/CR-1988, SAND81-0503)
   - -CURRENT STATUS - AWAITING FINAL REVIEW COMMENTS
   - -EXPECTED PUBLICATION - 6/81

2. ZION SMALL BREAK SEQUENCES
   - -LETTER REPORT SUBMITTED

3. OTHER ZION SEQUENCES
   - -CURRENT STATUS - IN PROCESS
   - -FINAL DRAFT ZION REPORT - APPROX. 7/81

4. MINIMUM CONTAINMENT COOLING
   - -DRAFT REPORT SUBMITTED

5. HIERARCHY OF PLANT STATES
   - -CURRENT STATUS - IN PROCESS

6. MARCH CODE MODIFICATIONS

7. ARKANSAS NUCLEAR ONE, UNIT 1
   - -INITIAL DATA DISTRIBUTED
   - -COMPREHENSIVE PLAN PREPARED

ORNL SASA PROGRESS

JANUARY - MAY, 1981

CONTINUED STATION BLACKOUT ANALYSIS FOR BROWN'S FERRY

- DEVELOPED DRYWELL HEATUP MODEL WITH TVA ASSISTANCE
- COMPARED TVA SIMULATOR, ORNL CALCULATIONS, AND RELAP-IV RESULTS
- COMPLETED ANALYSES OF DRYWELL FAILURE MODES
- INITIATED MODELING OF FISSION PRODUCT RELEASE FROM FUEL AND TRANSPORT WITHIN PLANT

SELECTED TENTATIVE CONCLUSIONS

SASA - INEL

JANUARY - MAY 1981

SMALL BREAK W/O HPI (W ZION):

BREAK SIZE DIAMETER	TIME TO CORE UNCOVERY
1 INCH	2 HOURS
2 INCH	30 MINUTES

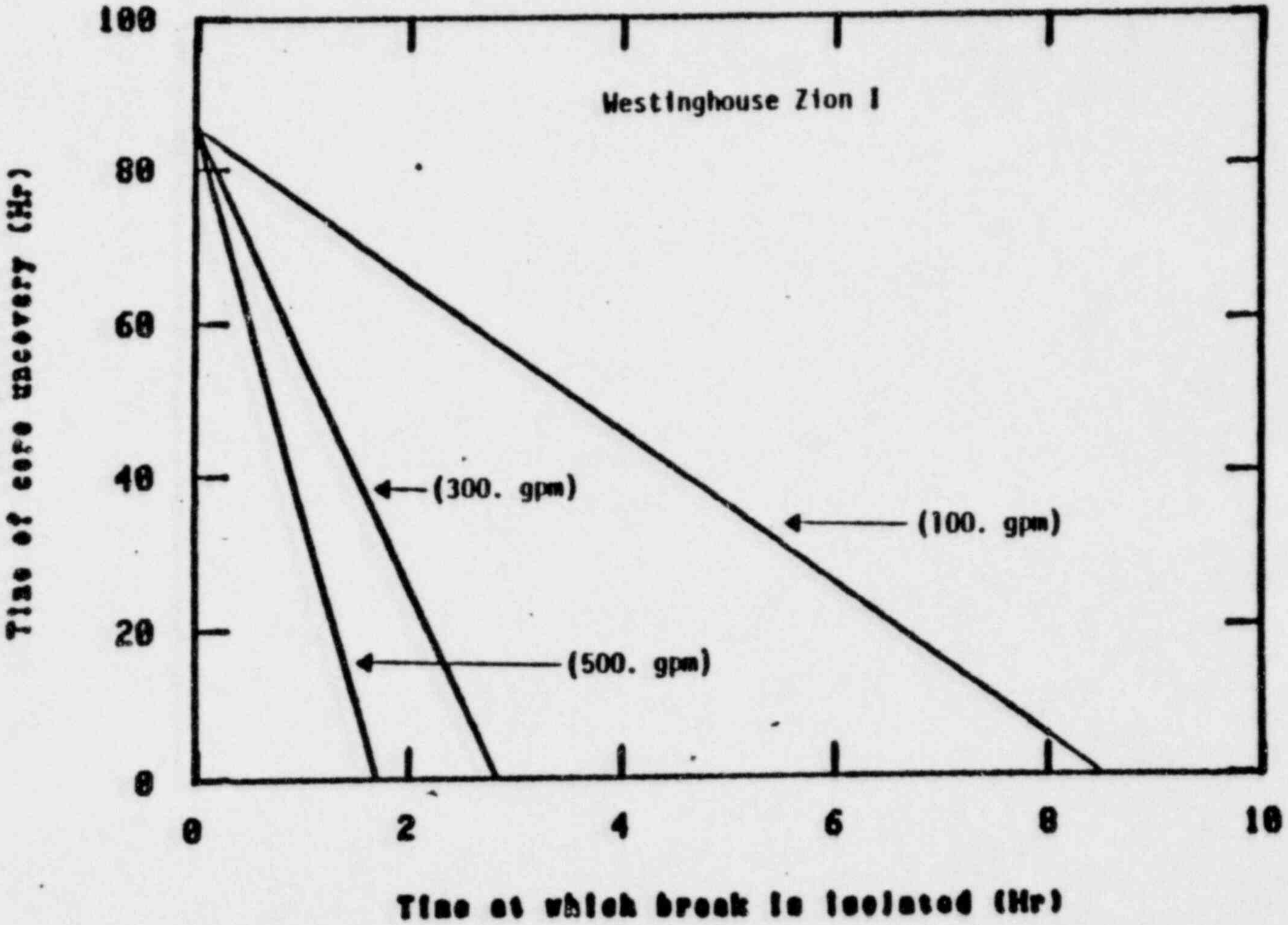
FULLY OPENING ALL ADV'S @ 10 MIN. PREVENTS CORE UNCOVERY FOR 1 IN. AND 2 IN. BREAKS

SELECTED TENT... .E CONCLUSIONS

SASA - INEL

JANUARY - MAY, 1981

Loss of offsite power, failure of all diesel generators, turbine driven auxiliary feedwater operates, primary break starting at zero time and isolated at a later time, additional 10 gpm technical specification limit leakage at all times.

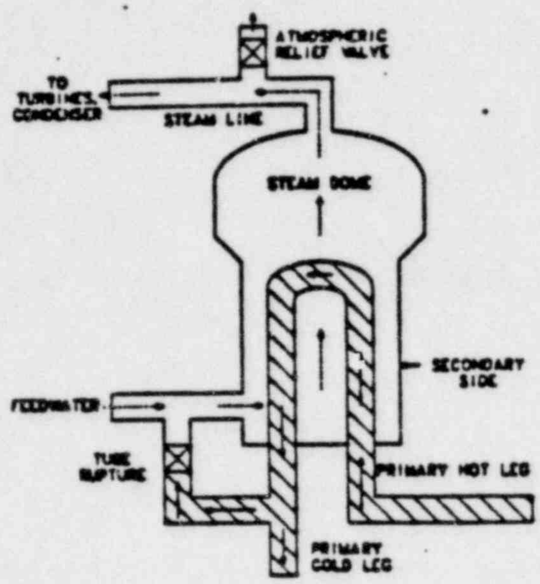


Westinghouse time to core uncovery as a function of break leak rate and isolation time.

SELECTED TENTATIVE CONCLUSIONS

SASA - LANL

STEAM GENERATOR TUBE RUPTURE MODEL



5

EVENT SEQUENCE FOR STEAM GENERATOR TUBE RUPTURE w/o ECCS

TIME	EVENT
0.0	FIVE TUBES RUPTURED
175 s	ECC ACTUATED ON LOW PRESSURIZER PRESSURE; REACTOR SCRAMS; PUMP COASTDOWN INITIATED BY OPERATOR; SWITCH TO AFW; CLOSE MAIN STEAM ISOLATION VALVES
180 s	SG "B" SECONDARY SIDE BEGINS FILLING; ARV SETPOINT REACHED
400 s	"ACD" - SECONDARY SIDE BEGINS TO FILL
1850 s	PRIMARY FLUID REACHES SATURATION; CORE AND UPPER PLENUM BEGIN VOIDING; MAJOR DEGRADATION OF SGs AS HEAT SINKS; PRIMARY PRESSURE SLOWLY RISING
2800 s	UPPER PLENUM AND HOT LEGS VOIDED
3300 s	CLAD TEMPERATURE EXCURSION BEGINS (16.8 K/min)
3800 s	CORE EMPTY

Los Alamos

EVENT SEQUENCE FOR TRANSIENT WITHOUT SCRAM AND ONE PORV STUCK OPEN

TIME (s)	POWER (MW)	EVENT
0	3240	LOSS OF MAIN FEEDWATER.
4	3220	PORV OPEN SETPOINT REACHED.
10	2970	SAFETY VALVE SETPOINT REACHED.
20	2480	PRIMARY PRESSURE PEAKS.
30	2410	AFW BEGINS.
56	2180	PRESSURIZER RELIEF TANK DISK RUPTURES.
170	1360	STEAM GENERATORS DRY OUT.
170-210	1360-550	PRIMARY PRESSURE PEAKS.
213	530	ECC TRIPPED ON BY HIGH CONTAINMENT PRESSURE.

Los Alamos

SELECTED TENTATIVE CONCLUSIONS

'SASA - SNL

ZION STATION BLACKOUT, KEY FINDINGS

1. MANUAL ASSURANCE OF CONTAINMENT ISOLATION VALVE CLOSURE IS RECOMMENDED.

2. REACTOR COOLANT MAKEUP MUST BE RESTORED IN SLIGHTLY OVER TWO HOURS IF IN-VESSEL TERMINATION IS TO BE ACHIEVED.

3. FOLLOWING CORE UNCOVERING, MINIMUM CONTAINMENT ESFs SHOULD BE OPERABLE BEFORE RESTORING REACTOR COOLANT MAKEUP.

4. RELATIVELY SHORT OPERATION OF CONTAINMENT SPRAYS BEFORE VESSEL BREACH CAN SIGNIFICANTLY REDUCE RADIOLOGICAL CONSEQUENCES.

5. CONTAINMENT PRESSURE REDUCTIONS FOLLOWING VESSEL BREACH SHOULD BE CAREFULLY CONTROLLED, PREFERABLY WITH SPRAYS, TO AVOID H_2 BURNS.

6. PROBABILITY OF CONTAINMENT FAILURE DUE TO OVERPRESSURIZATION IS SIGNIFICANTLY LOWER THAN ESTIMATED IN WASH-1400.

SELECTED TENTATIVE CONCLUSIONS

ORNL - BROWN'S FERRY STATION BLACKOUT

NORMAL RECOVERY: SUMMARY

1. SYSTEM: NORMAL RECOVERY POSSIBLE AFTER 5 HOURS IF BATTERIES LAST THAT LONG:

   - LEVEL >200" ABOVE TAF
   - VESSEL PRESSURE ~100 PSIA
   - 41,500 GAL WATER LEFT IN CST
   - POOL TEMPERATURE BELOW 190°F T-QUENCHER LIMIT
   - DRYWELL ATMOSPHERE TEMPERATURE BELOW 281°F DESIGN TEMPERATURE

2. OPERATORS: SHOULD DEPRESSURIZE WITHIN 1 HOUR OF STATION BLACKOUT:

   - PROTECT VITAL DRYWELL EQUIPMENT
   - POSTPONE SEVERE DAMAGE

PRINCIPAL SASA PROGRAM
ASSUMPTIONS - FY 19,2
(TENTATIVE)

1. COOPERATION WITH ARKANSAS POWER AND LIGHT CO. WILL BE ESTABLISHED TO PERMIT PWR ANALYSES ON ANO-1, UNIT 2 (B&W) BY INEL, LANL, AND SNL

2. ORNL BWR EFFORTS WILL INCLUDE SMALL BREAK LOCA OUTSIDE AND INSIDE CONTAINMENT, INCLUDING F. P. TRANSPORT ANALYSIS