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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

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Nuclear Regulatory Commission
Room 1167
1717 H Street, N.W.
Washington, D.C.

Wednesday, July 9, 1980

The Subcommittee met, pursuant to notice, at 8:32 a.m.

BEFORE:

MAX CARBON, Presiding

W. KERR

NRC STAFF PRESENT:

PAUL BOEHNERT

ALSO PRESENT:

M. FIRST, ACRS Consultant

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P R O C E E D I N G S

MR. CARBON: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Advanced Reactors. My name is Dr. Carbon. The other ACRS member present is Dr. Kerr. Dr. Plessett will be here soon. Dr. First, a consultant, will also be here soon.

The purpose of this meeting is to review the NRC sponsored research on advanced reactors. The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act, and the Government in the Sunshine Act.

Paul Boehnert is the designated federal employee for the meeting. The rules for participation n today's meeting have been announced as part of the notice of this meeting, previously published in the Federal Register on June 24, 1980.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that each speaker identify himself and speak clearly. We have received no written comments or requests to make oral statements. We will proceed with the meeting in just a moment.

I will call upon Dr. Kelber of the NRC at that time. I would like to suggest, Charlie, that you ask each of the people to stick almost religiously to the time schedule because we have got to be out of here at 1:00.

There is another meeting scheduled in here.

MR. KELBER: We have impressed upon them the seriousness of allowing adequate time for discussion.

MR. CARBON: I would propose to forego the executive session and simply try to move everything up five minutes. So, at 8:35, I will call on you Charlie.

MR. KELBER: I am Charles Kelber, Assistant Director for Advanced Reactor Safety Research. I want to summarize today our understanding of the concerns raised by the ACRS about the work in this area.

(Slide.)

We will discuss what our response has been to that. We have attempted to reapond positively to ACRS recommendations regarding the aims of our research. This response has taken two forms. One, the redirection or change in emphasis on existing work, such as the work with the super-system code and Branda; and two, the development of new approaches that focus clearly on ACRS concerns, such as the fuel testing sensitivity program.

These efforts, we judge to have a high promise of success, if the research is continued at the level of effort requested by the Office of Research. The effort will be concentrated on these concerns.

There will be a certain level of code development and code support to improve computing efficiency. You have heard some of that last week in the discussion of the SIMMER code. The major use of the codes will be their application to these concerns.

We anticipate some testing to continue as well, particularly to improve our knowledge of key processes and development through key models. On the succeeding vu-graphs, we summarize the major efforts, responding to each recommendation.

It is noteworthy to remark in this, the benefit from participation in exchanges with foreigh programs. As early recommendation of the ACRS made several years ago was to derive maximum benefit from such programs. We are obviously doing just that.

As you know, the emphasis on core melt accidents and core disruption accidents varies from country to country, being somewhat similar to our own in Germany, being somewhat similar in England, and noted practically only in passing in France. We have had considerable benefit from the discussions with our foreign partners in this matter, as well as with the broader range of accidents.

The accend delineation phase one report was discussed with you last week. Today, you will hear some about the SSC code and the COMMIX code, which will be used to study a variety of problems in the area of flow transients.

As has been described to you, we are carrying on -- I apologize for the vu-graph, the misprints there are pretty bad. We will be reporting further on the heterogenous versus homogenous core study that has been under way now for some while.

First at Argonne, and now at Los Alamos.

MR. CARBON: Let me interrupt here, if I may. The first recommendation we made when these reports started coming out was to place a lot more emphasis on accident delineation, analyzing accidents of a very broad spectrum. It is quite true that last week, or whenever it was, there was a mention of the accident delineation phase one report. I did not think of it in terms of a discussion. I don't feel I have mush of an understanding or appreciation of what you are doing there.

I wish you would expand on that at the moment.

MR. KELBER: The accident delineation effort started as a response to an attempt by the CRBR project to introduce risk analysis into the licensing stream. It was a defensive reaction.

Up to that time, despite the ACRS recommendation, we have been prohibited from doing this work by the director --

MR. CARBON: Our recommendation was on a generic basis.

MR. KELBER: The effort then started purely as an accident delineation effort. That is, no probabilistic input. This is in keeping with a tradition which has not yet changed within the Office of Research, although it may be changing to restrict participation in these matters to a particular group.

The claim is that peculiar expertise is needed to do probabilistic studies which may be correct. Then, we suffered a number of managerial problems in thes effort, particularly when the -- after the licensing of CRBR was deferred.

The effort within DOE in this area also decreased. We lost some focus. We took some steps to redress this deficiency. We now have, I believe, a first rate manager in charge of that effort, Milt Clauser, from whom you heard last week.

We are making significant progress. We also had some conceptual difficulties. The conceptual difficulties were how to deal with all the phenomena encountered in treatment of core melt accidents; yet significant progress had been made in the discussion of accident initiators and events that might lead up to core melt accidents.

I believe this is where you thought there should be more emphasis. Under Clauser's direction, we have gotten over those road blocks. We believe there has been perspective put into the program. We are now ready to have very wide-spread review of this work.

MR. CARBON: How long has he been at it and how many people does he have?

MR. KELBER: Six months, and three people.

MR. CARBON: Plus himself?

MR. KELBER: Plus himself. He also directs the effort on the CONTAIN code. Now, we did have a draft report which was extensively reviewed. That review was then factored into this latest report, which is now in pretty good shape.

On this we will have a review group meeting. Of course, the ACRS will be notified of that review group meeting when it is scheduled.

We would propose then to discuss these questions in a wider arena, particularly, there are two aspects of the report that I think are very interesting.

I know they are of great interest to DOE as well. That is the sensitivity of the system to the loss of heat sink. Of course, all reactor plants are sensitive to the loss of heat sink. There are many varieties of choices available to the LMFBR to remove residual heat.

DOE has been going through a very considerable study of the way to remove residual heat. They are trying to arrive at criteria to decide which is best. I think it would be a review group meeting for the accidetn delineation report to focus on this issue of how to ensure reliability of residual heat removal. That would be mutually rewarding, both to us and DOE.

I think I look forward to that aspect of it. Somewhat surprising to me, and I think surprising to a number of us, was at the opposite end of the spectrum. The finding that the favored mode for containment failure, though not necessarily from a consequence point of view but from a frequency point of view, was estimated to be base mat melt-through.

This probably contradicts WASH-1400. It certainly contradicts the Zion-Indian Point study, which indicates that base mat melt-through is probably unlikely at all, particularly if you have a thick base mat.

So, we are going to have to look at that as well. I do believe that from the point of view of the design criteria, and accident prevention, focus on how you establish the greatest reliability for residual heat removal is a topic of mutual interest, both to us and Dc .

That, I think, is what the focus of our review group meetings should be. We would anticipate having that as soon as the report becomes widely available. We now have a rather thick draft.

MR. KERR: Is this a review group that is now in being?

MR. KELBER: Do we have a regular review group?

VOICE: We had a review group meeting on the first draft in October --

VOICE 2: January '79.

VOICE: I would estimate we would try to --

MR. CARBON: Excuse me, I am very confused here. I thought this report -- I thought this was six months. You are talking about a review 18 months ago?

VOICE: That is correct. There was a previous draft.

MR. CARBON: He just started on this six months ago.

VOICE: This has been under -- we have had three managers c this problem.

MR. CARBON: I see. Does this report reflect what has been done in the last six months?

VOICE: It reflects the entire work; however, there were enough serious problems raised in the review of 18 months ago that the report was essentially completely rewritten over the preiod.

MR. CARBON: Okay. So, then there is or is not a review group at this time?

VOICE: We would -- to what extent we could reconstitute the same people is problematic.

MR. KELBER: We will get some of the same people.

VOICE: We will get some of the same people and get some new people.

MR. CARBON: Give some examples of how many and who will be on it.

MR. KELBER: Well, the review group itself consists of federal employees. I assume we would have someone from DOE. We usually have had their cooperation in this.

There will be Dr. Curtis, who will be the chairman. We will attempt to get someone from NRR, but as you know, they have no expertise anymore in this area. Whether they will participate in this or not, we do not know.

We would attempt to get someone from the probabilistic analysis staff. Again, in the past, we have had perhaps grudging cooperation. It is improving. We will hope for better.

In addition, we will have a wide range of consultants. one difficulty that we do have is that a great deal of the outside expertise that we do rely on is at Sandia, itself. They have done a lot of peer review, but it would not be reasonable to ask them to take part in a critical review of a Sandia report.

We may have them on hand anyhow to lend expertise. I think we will go to Los Alamos, to Argonne, and we will attempt to get some of the GE people who have been doing work for DOE to come in as well.

We will aslo consult with DOE, whether there are some others that might be appropriate.

(Slide.)

Now, the next issue is natural convection, and do we need a new facility? That will be the focus of a number of -- at least two talks today. We did have a very good specialist meeting at B & L this last February to develop some concerns.

Out of that, we developed the viewpoint that, at least, some DOe work on a new facility was very well thought out. This is the work at AI. We think that they have done a good job of analyzing the need.

It does appear to be reasonably specific to the design. In scale, it is not too much different from the tests, I believe, that you saw, Dr. Carbon, on Carl's work. The unanswered question is to what extent we need full-scale tests.

I know that in a sort of academic or generic way, there are tests being done at Grenoble at essentially full-scale. These are to address problems associated with very low flow rates where the conductivity of the sodium makes a big difference in the behavior.

MR. KERR: I do not remember the language in the ACRS report specifically, but my memory was the last ones adjusted that you determine whether a facility was needed to settle the question. I presume from what you are saying, your determination is yes, a facility is needed.

MR. KELBER: At this time, we see no reason to contradict the AI point of view. The sum total of all of our investigations to date is yes, a new facility is needed. It should be reasonably specific to the design involved.

MR. CARBON: Have you addressed that parcific point? YOu simply did not pass over it and go on to designing a facility?

MR. KELBER: No, we are not going to design a facility. DOE is doing that.

MR. CARBON: Have you specifically addressed the question though of whether a facility is needed?

MR. KELBER: No. We did a review of the AI paper and felt that their analysis was good.

MR. CARBON: Did they analyze whether one was needed?

MR. KELBER: I think, yes. That is the in the proceedings of the Brookhaven report. I guess -- when will that report be out? It should be out very soon.

VOICE: The Chairman should be here shortly.

MR. CARBON: Is that item number two? That is out, I think.

MR. KELBER: I have not seen it, but it should be our very shortly.

MR. CARBON: The papers are out.

MR. KELBER: Yes. I don't know what DOE's feeling is on this to tell you the truth. We have not really discussed with them their plans. I think one of the reasons is that they themselves are trying to make up their minds. We are going to urge them to continue to support that work.

MR. CARBON: Our question was determine whether, also it referred to commercial size. Is this working?

MR. KELBER: The AI work is aimed at the conceptual design stuff.

MR. CARBON: For both loop and pool?

MR. KELBER: The point they make is it should be reasonably specific with the design. I think, therefore, if one were to -- that their argument is that you have at least the conceptual design in mind when you design the loop.

MR. KERR: I think this question grew out of a discussion of the validity of existing or planned codes to analyze the natural circulation situation.

At the time, it was not clear whether the codes needed validation in an experiment that was near commercial size.

Since that time, apparently the conclusion is that, indeed, in order to have confidence in the goals of that regime --

MR. KELBER: Let me comment on the regimes where the size effects are apparently important. The IA paper points out there are two types of regimes. Scaling is not too bad to do.

We are, as you know, cooperating with DOE in making an independent review of the FFTF natural convection tests. That report has been out. The work that is now being done is to relate the values predicted by the code to what will be measured by the instruments.

As you know, there is a tranfer function to the instrument. We want to anticipate that as well, particularly since we have the time.

MR. CARBON: I did not know you were making this review. Who is doing that?

MR. KELBER: Brookhaven.

MR. CARBON: This is a joint NRC-DOE?

MR. KELBER: Yes, DOE has furnished us with the test conditions, the instrument locations, and the other input data. We -- plant description of FFTF.

MR. CARBON: Is this what will be or what was run?

MR. KELBER: What will be.

MR. CARBON: What will be.

MR. KELBER: We have made these prodeictions. The report has been published. So, these are true predictions. We have also done code comparisons. DOE has also done code comparisons. As I say, we are now looking at the question of how the what is actually measured, how it relates to what the code actually does. The two things are not synonymous.

If I can summarize the question of code validation very briefly, it would be this way: The early part of the transition to natural convection, while the flow is still in the turbulent regime, there seems to be little doubt in people's minds that comparisons with tests substantiate the use of the code for larger scale.

As you make the transition into the laminar flow regimes, a variety of scale effects become important. As you go out longer and longer in time, it is -- and particularly as new heat transfer paths become significant or as conductivity paths become significant -- then the validation at small scale becomes less and less assured.

Let me put it that way. Validation on small scale is a usefule basis for extrapolation to large scale. There are also details of design that may heavily influence the output. For example, I am told -- I cannot cite any authority on this -- but I am told that PFR, which is a fairly large-scale machine but not commercial scale, has vortices in the outlet, also arund the entrance to the IHX where the dip heat exchanges are located.

That is a function of design detail. Whether you could reproduce -- expect to reproduce that in a full-scale design, or even one to us is a good question. It certainly influences the tenperature distribution at low flow rates.

MR. CARBON: Let me interrupt.

MR. KELBER: Those experiments are not well instrumented. I know that Dr. Plessett is very pessimistic about our ability to do a great deal with that.

MR. CARBON: Let me interrupt there. I think you said you are told about those PFR tests. Is it correct that the British have been unwilling to give you enough detail to really analyze --

MR. KELBER: This was taken up at the last coordinator's conference that was held this June in the UK. The statement that was made there was that the British side thought that Colin Gradtry and Bill Sha -- Bill is here today -- have an understanding on data transfer. I suspect Bill will be happy to tell you his knowledge of the PFR test in whatever detail you wish.

MR. CARBON: I will ask now or then, do we have enough of the information -- do we have all of the information, so to speak, on their natural circulation test, that we know why they could not predict natural circulation results?

They were unable ahead of time to predict what was going to happen.

MR. SHA: I had two lengthy discussions with Colin Gradtry. They are very open-minded and very willing to give us all the knowledge and results they have on PFR. That is what he promised me.

MR. CARBON: So, they are willing, but we do not have it yet.

MR. SHA: They do not quite understand the phenomenon going on at the PFR. Certain portions, they are still in doubt. That is why they asked us to come in, use the COMMIX code to try to close this gap.

MR. CARBON: Does this imply that there is a real coordinated effort to try between us and them to understand what is going on?

MR. SHA: I think the emphasis is real, but the problem is funding situation. That is the problem. They are waiting -- both sides are waiting to cooperate to a full extent. They are anxious for our help. We would like to understand what is going on over there.

They provided all the data they have, all the knowledge they have. We will give all the code capability to try to have joint efforts to try and understand the situation.

MR. CARBON: The funding problem on our side is NRC funding to support this?

MR. SHA: Yes.

MR. KELBER: As our budget situation becomes clear, we have had discussions with Bill Sha on what is needed to support this work. As our fuding situation becomes clear, we will attempt to see whether we can provide funds for this. When we provide funds for a special corroborative effort, we generally like to have a memorandum with the other partner as to what we are going to do.

We have to justify our expenditures somehow. We would like to have, therefore, at least a memorandum in this repsect. I should say that my opposite number in the UK, Harry Tieg has promoted to another position, as you may know. His position right now is being taken temporarily by Ernie Gilpe. I do not know when they will appoint someone else. When that happens and as our funding situation does become clear, we can put this on a solid basis.

Obviously, we are going to go ahead. We are going ahead mych more slowly than we originally anticipated. There are some real problems here. They are not academic. I was talking with Carl Anderson from Westinghouse, who is in charge of some key design efforts. He feels that understanding these problems, which I believe are characteristic of low flow, is very important from the design point of view since it is under these conditions that many components objected to their highest temperature and to their highest thermal cyclic stress.

MR. KERR: We are all convinced that it is important to understand natural convection.

MR. KELBER: There is a large community of interest between us and DOE and the vendors, also abroad.

MR. KERR: The conclusion is that existing codes probably cannot be dependent upon to predict natural circulation behavior, and an experimental facility is needed.

MR. KELBER: I think that is our present position. I have great hopes that with highly detailed codes like COMMIX and others that it is perhaps -- we can learn enough to construct useful models for what I might terms "production codes."

I might say that Bill Sha has made immense strides in speeding up COMMIX to the point where it is not -- it does not require unreasonable amounts of time. Nevertheless, I think there has to be a distinction between highly detailed codes like COMMIX and production codes, such as SSC.

Whether or not these codes make a good prediction depends very much on whether there exists multi-dimensional effects and paths which are not modelled. If the -- if doing the test or experiments with the reactor -- if the rate is reasonably represented one-dimensionally -- in other words, you can anticipate the flow path when you model it, and you can probably predict natural circulation as well as the other.

MR. CARBON: The evidence would seem to indicate though that the codes are not going to be modelled properly because we do not know how to model them. We don't know what

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goes in.

VOICE: The point is that where multi-dimensional effects such as vortices -- when they become important, clearly a one-dimensional model is sufficient.

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MR. CARBON: We'd better stop. You had better proceed rapidly.

(Slide.)

MR. KELBER: The final effort I want to review with you is our program on safety test -- fuel safety test needs, and review of our testing capabilities. This was a point that you brought home to us about a year or so ago.

At that time, we had received from our contractors a rather extensive discussion of a proposed safety -- fuel safety testing program that covered all parameters and all situations with some attempt at prioritization. It was largely a judgmental attempt.

We decided, in view of your concern which we share, and in view of the rather poor documentation for some of the assigned priorities, we would attempt to do some analytical work to find out what do you really need to test.

Some early work has been done, particularly in cooperation with the English by Harry Hummel, who is here today, and some of the others in that group at Argonne. We are continuing some efforts at Los Alamos and Sandia.

It may well be that the types of tests you want to make are far fewer in number than we previously thought. In particular, we are trying to concentrate some of the work that Harry has done into the description of what the A-3 tests should be a Cabri. That is a test scheduled for late this year.

There is a developing consensus that one wants this test to be large enough to cause a significant failure of the clad, and to summarize what is an extensive piece of work.

What is of interest is how the clad fails. Does it fail by a rapidly prolongating rip, or does it fail at a point, and essentially fuel just widens that breech as it gets expelled?

Now, how successful Cabri would be in determining this is another question. We probably would have at this time -- we would have to pin our hopes on a fairly clued resolution.

We have one of our people working there. We would hope to get as much information as we can on this topic from this experiment. This work is continuing. We hope to be able to report more conclusively to you next year.

Let me summarize that our program as the tools and capabilities to investigate key areas of safety concerns delineated by you and others. Given an adequate level of support, we should be able to continue to produce notable results that help set the stage for any future licensing actions that may arise.

It is not easy to forecast when such actions may be needed. Just a few years ago, we were in a very difficult discussion with respect to licensing CRBR. We do not know when an opportunity like that might present itself again. The estimates range from one year to 20 years. They are as much political as technical in nature. When the opportunity does come up, we should be prepared.

I think that the budget we have requested is a bargain price for making sure NRC has resources available to act, if and when needed. I think that even at a somewhat higher level, it is still a bargain considering the nature of the problem.

That concludes my statement.

MR. CARBON: I am going to ask one quick quesiton. One of our recommendations was to study the advantages and disadvantages of alternate containment design. What have you been doing in that area, just real briefly?

MR. KELBER: We are starting some work under the accident delineation study. That is a natural place to do it because they are also developing the CONTAIN code. We have done nothing but planning in that effort at the present time.

I think the major question that we face in fast reactor containment is dealing with sodium fires. The tendency in fast reactor containment has been to use a design somewhat similar to that of the ice condenser plants, a rather low pressure steel shell.

That, I think, is tenable if you do not have to deal with a sodium fire, or at least if you do not have to deal with a spray fire. There will be a very considerable program in

France, the Esmerelda program on large fires, including spray fires in containments, where they are trying to face up to this issue.

The other side of that coin is what happens when sodium spills on concrete. We are finishing that work. We think we have a good thermal dynamic and chemical model of that process.

MR. CARBON: That answers my question. Bill, any others?

MR. KERR: Will the alternate containment study -- if it is continued to continue -- include features of filter vented containment systems?

MR. KELBER: That is a reasonable way to look at it. Here are the questions you have to look at. Can you suppress spray fires with deflection pans, things of that nature?

Must you have a steel liner in the secondary cells to protect the concrete? Can a simple pressure relief system work effectively? The filters pose a special problem for such a system.

I do not -- in this respect, we may be able to get some help in connection with the filter vented containment studies for the LWRs. I am trying to negotiate an exchange with the Swedish Atomic Energy Commission, who have made a decision to go to the filter vented containment on their reactors.

They are proposing to test filters -- filter efficiencies during the coming year. What I propose to do is to exchange their knowledge on filter efficiencies with our predictions and knowledge on filter loads.

Between the two of us, we ought to come up with a reasonably credible filter design. Whether that can be extrapolated to the sodium case or not, I do not know. DOE has been doing a very significant study at HEDL under Bob Hilliard in this area.

So, between these efforts, we ought to be able to get a reasonable guess on the size of such a system for a sodium plant.

MR. KERR: Our knowledge about loads to which you refer is the kind of knowledge that one gets by running --

MR. KELBER: We have to make some estimate. If you have sodium dumped into the containment --

MR. KERR: I was -- I assumed you were talking about awater reactor. When you are talking about the Swedish work --

MR. KELBER: Yes.

MR. KERR: The loading knowledge that you mentioned is what you get from running --

MR. KELBER: No, we will be doing tests with steam filled atmospheres and aerosols at NSPP starting this fall. We will be -- we also will be doing some aerosol sampling at Sandia.

MR. CARBON: Thank you. We will move ahead. Harry?

MR. HUMMEL: we have prepared a brief presentation this morning at our activities at Argonne. Additional details can be found in the background material we have included in the handout.

Pat Garner will discuss the boiling model development work. I will first cover other aspects of the program.

(Slide.)

Let me remind you what our principal activities are. We are concerned with in-core accident analysis. We are not concerned with out of core development or assessment. Of course, we would use models of the primary loop and so on as they would affect in-core phenomena.

Our principal modelling activities hav- been EPIC, which at the moment pretty well completed the BIFLO modelling code is our principal activity at the moment.

Our cooperative studies include participation with the UK in comparative studies of the WAC group. We have engaged in various assessment activities as shown on the vu-graph.

MR. CARBON: What is the WAC group?

MR. HUMMEL: It is a committee concerned with whole core estimate analysis.

(Slide.)

Our activities are focussed on the initiating phase of whole-core accidents, initial conditions for a transition phase. BIFLO is concerned with looking at incoherent effects within subassemblies. It is currently just looking at radial symmetric effects with the possibility of going to see if skews exist in the future.

We would expect that BIFLO would be incorporated in SAS-4A, which is currently being developed. We would expect them to consider the possible effects of intra-subassembly incoherence on clad and fuel motion, although we do not have any definite plans at the moment for any modelling work in that area.

The first thing we are going to do, I think, is get hold of the LEVITATE code that is being developed by DOE and do studies with this and try to use this in some way to try to assess the importance of these incoherent effects.

It is our thought that as we go through heterogenous designs, that the intra-subassembly incoherence effects become more important because boiling is a slower process and it takes longer to get to the point of flow reversal so that the radial temperature variations could be more important.

Also, clad motion effects become more important for heterogenous design because the lower boiling ramp rates, there is more time for clad motion to occur. So, this could be more of a concern for heterogenous than a large homogenous design.

(Slide.)

The current status of EPIC is pretty well finished. The users manual has just been issued. EPIC is used by the

UK. EPIC was incorporated into SAS/EPIC, which has been the only code so far that has been able to give respectable treatment to the KfK. We have thought about other possible improvements in EPIC such as particularly the plugging model.

We have to consider what is being done with SAS-4A, and whether we want to switch over to that or not. So, we have not made any decision on where to go on that.

(Slide.)

The UK bilaterial program, we have not really been doing too much in the last year. The UK has been fairly active in calculations. They made some corrections in the code, so they re-ran the calculations. I was just over there last month.

We sort of cleaned up a report on the first phase of the analysis. So, we hope that will be issued in the next several months. I think that will pretty well tie it up. The chief thing we have gotten out of it so far has been in the area of trying to understand things about fuel pin failure conditions, and the comparison of SAS and the more appropriate French boiling model was also of some interest.

The UK indicated they were very pleased with the initial development of the program. They are quite anxious to develop it. It is our thought that we would focus for the next couple of years on individual phenomena rather than a lot of whole-core accident calculations, awaiting development of a whole-core accident model.

The two principal areas of concern at the moment are: sodium boiling and fuel pin mechanics. In both of these areas, the UK has excellent programs. The Harwell work and the Winfrith work is of great interest to us.

(Slide.)

The WAC studies, we have finished a TOP calculation. The next step will be an LOF calculation. We expect that next November. The most interesting thing that is coming out of that at the moment is comparison of steady state fuel characterization which is nothing I knew much about, but realizing there is a whole uncertainty of events here -- well, it turns out --

MR. KERR: That just means a desctiption of the fuel at steady state.

MR. HUMMEL: Yes.

MR. KERR: It is an initialization.

MR. HUMMEL: It is a description of the radiation history, which is important for accident analysis because it gives gas distritubtion, the swelling of the fuel clad gap, this sort of thing. It is interesting from that point of view. It turns out that SAS-4A was originally being calibrated against the more detailed code, but LIFE is sort of a first principals code.

It was not too successful in some aspects. So, SAS-4A has been recalculated against the Belgian code. So, naturally, SAS-4A -- the calibrating -- but FRUMP, we had sort of an interesting discussion of this at the June meeting. We had been struggling with the questions of how much fuel swelling you got and how much densification.

Apparently, this is a pretty difficult area. It was sort of an interesting discussion.

MR. CARBON: Where in here do you compare all the codes against real life?

MR. HUMMEL: Well, this -- of course, this has been done -- as I say, some have been pretty well calibrated against experiments.

MR. CARBON: Over a wide range?

MR. HUMMEL: I am not much of an expert on this.

MR. CARBON: Okay.

MR. HUMMEL: I don't really know, but certainly a certain amount of this has been done. I mean, that's -- I think FRUMP has also -- I have bee trying to find out more about FRUMP, and expect to in the coming months.

Certainly, there has been a certain amount of calibration of experiment there also. As I say, a big problem with LIFE. It is sort of a first principal code. They had a lot of coefficients. When they went around to sheck the experiment, things did not work out too well. That is what I have been told.

I am not much of an expert in this area.

(Slide.)

Our assessment activities are designed to be of aid to NRC and the ACRS. There are a couple of areas I have sort of pinpointed here. Charlie alluded earlier to a view of the initial work on studying the question of heterogenous cores and recent conclusions about what would be reasonable there.

A target of about $2.50 is a reasonable range. If you go down to $2.00 -- then you have to start worrying about how much actual expansion you have and what the clad motion does. That sort of thing.

Another study we have done has been concerned with inlet plenum pressure rise during sodium boiling. You do get a large pressure rise.

We have been concerned about whether this is being accurately calculated or not. We found elasticity and compressibility effects were not important. Another aspect is fuel pin failure studies. Last year, we did some calculations, trying to understand mechanical interaction.

Lately, I have been focussing on fission gas models for release of solid fuel and concluded from this that the NEFIG code looks pretty good. It is fast enough to put into SAS-4A.

I think that the background from this work will be helpful in getting parameter studies, because we are starting to find out a little bit more about fission gas release from solid fuel.

It gives us a better idea of what the uncertainty range is and what a reasonable range for parameter studies is.

I would like to conclude my presentation now and turn it over to Pat, unless there are questions.

(Slide.)

MR. GARNER: My name is Patrick Garner. I am with Argonne. I will tell you a little bit about what we are doing in modelling in the BIFLO code for sodium boiling, then the current SAS code fuel assemblies collapsed into one-dimensional treatment.

What we are doing in BIFLO is expanding this modelling to consider several different regions within the fuel assembly. Each having coolant and an associated typical fuel pin. In the typical breakdown we are using right now, it would be about four or five coolant rings from the center, then two coolant rings for the next channel and on out.

This is just typical for now. We are working with a symmetric model for the time being, for development pruposes. We feel we can expand it easily to handle the cases where you would have a power skew across the fuel assembly. In terms of the way we see the modelling progressing or the calculation progressing, in BIFLO you would begin boiling in the high power to flow region.

This boiling would then grow axially and radially as the lower power to flow regions reached boiling conditions.

bfm31

This means you would initiate boiling at a time prior to what you would predict with a one-dimensional code, just using average conditions for the subassembly. It may indicate that you have different times to flow reversal.

It would certainly indicate different times to reach initial clad melting within the subassembly. We have programmed up such a model. Right now it is a stand alone code. We are doing some computational studies and some benchmarking studies.

(Slide.)

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Some of the comparisons we are doing at this time are between the BIFLO code and some experiments done out at Oak Ridge. These are electrically heated pins 36 inches long, fission gas plenum on top. They are currently doing some 61 pin tests which we hope to use in the future.

Other tests which we will be considering for use are 19 and 37 pin tests in the sodium loop safety facility which are run in pile out at Argonne West, and work that is coming in from the Europeans. We are also doing comparisons of BIFLO versus other codes, in particular COMMIX, which you will hear more about today, and the COBRA-4 code. Both of these are subchannel codes so they have highly detailed geometry. It will be very helpful.

We are doing a few comparisons with available SAS calculations and are beginning to get a better feeling for one-dimensional versus two-dimensional effects. A sort of third part of the comparison, I think, involves comparison of the subchannel codes to the experiments.

(Slide)

For example, this is a description of the 19-pin experiment in Thors. These tests are very well instrumented for studying sodium boiling. There are about 100 therm couples concentrated in the region where they expected and did find sodium boiling. In terms of modeling these tests, the subchannel codes such as COMMIX and COBRA would give you a very detailed geometry by modeling all 19 pins, and COMMIX would break this down to somewhere between 32 and 92 coolant channels, depending on which partitioning option you used.

COBRA would give you about 40 subchannels, so you can get very good comparison between the point-wise measurements in the test and the subchannel temperatures you would calculate with the code. It makes the comparison a little bit easier. Then you can take results from the subchannel codes and average them into region-type results that we would then compare with the BIFLO modeling where, for example, we would, say, have only three coolant rings.

(Slide)

The status of where we think things are right now, we have an initial version of the code running and we are doing comparisons against the 19-pin test, both at steady state and for a transient test that went into boiling. There are some future things we have to do. We have an assessment of some assumptions in the modeling. We have to examine the question of how many regions do we need to model within the subassembly to get a good two-dimensional description, how should those regions be assigned.

We need some work on the boiling and nonboiling regions in the code, and we need some work to speed up our boiling computation. It is rolling fast on a per-time-step basis. But we are being limited by material velocities on time step. We are doing studies on other computational methods at this point.

The other steps would be to incorporate this modeling into SAS-4A and examining the impact of the 2D treatment on various accident scenarios. The other things are we need to make the geometry in the modeling more flexible so we can handle the power skews across sub-assemblies and do benchmarking continually at large bunde size.

That is what I have for you. I would be happy to answer any questions.

MR. KERR: Your validation of this version -- one of the validation efforts is a comparison of your code prediction with Oak Ridge experiments?

MR. GARNER: Yes.

MR. KERR: Is that, in your view, a sufficient validation? Will you have the data you need to give you confidence that the code will work in a variety of situations?

MR. GARNER: I think the 19-pin test taken alone will not be sufficient. Combined with the 61-pin test -- I have not seen the test matrix for those experiments to know what they are really going to do on the transient tests, so I guess my answer would be no, it would not be a sufficient validation.

MR. KERR: Will the 61-pin test be done with your validation needs in mind?

MR. GARNER: Will they be done --

MR. KERR: Yes.

MR. GARNER: No, they will not. They are being conducted by DOE.

MR. KERR: Aren't they part of the same government under which we operate?

MR. GARNER: I suppose so.

MR. KELBER: May I comment on this?

MR. KERR: Yes, sir.

MR. KELBER: If after this initial trial it appears that we can get significant benefit from code development or validation from those tests, we would, of course, ask DOE to consider including in their test matrix such tests as might specifically pinpoint problems with BIFLO, and I think they are, in fact, familiar with the work we are doing. I would anticipate no difficulty in getting them to at least consider that question.

Whether they would be able to because of programmatic reasons, I do not know. We are not far enough along yet to come to a specific proposal for tests. I am sure that if we did, they would give it --

MR. KERR: It seems to me --

MR. KELBER: -- significant consideration.

MR KERR: With a test of this magnitude being designed and experimental validation being what I think all of us agree it is, it would be worthwhile trying to have some input into the testing system. It seems to me one of the purposes of a test at this time is to give people information which will assist in modeling.

MR. KELBER: Precisely.

MR. KERR: The earlier that you can get involved in the test planning, the more likely it would seem to me it is that one gets information that is needed. I recogniz your resources are limited and all these constraints exist, but I would think it would be --

MR. KELBER: We will follow that up.

MR. SHA: We do have input to --

MR. KELBER: If you have some material which bears on Bill Kerr's question, you should give it.

MR. SHA: We have input, actually. You see, we have DOE thermal hydraulic working group, and I am one of the members in that meeting. So we have frequently discussed this problem.

MR. KERR: Do you two gentlemen know each other? Have you two met each other?

MR. SHA: Yes.

MR. KELBER: He wants to know if you have discussed in the working group the problems that might be run with Thors that would help the development of BIFLO. That is specifically the question that Bill raised.

MR. SHA: That is regarding the instrumentation in general, how to get the code validation.

MR. KERR: I do not know what sort of information he needs. What I am asking is is there some way you two can communicate so that his needs can become part of the planning for this experiment. I mean the first requirement is that you know each other. If you don't, somebody ought to introduce you.

MR. SHA: I will answer yes because our common interests are the same.

MR. KERR: Okay. But it isn't enough to have a common interest. You have to --

MR. SHA: Yes.

MR. KERR: I feel --

MR. SHA: We actually express what we need through this working group, so we get input from the code validation process.

MR. KERR: And once in a while you talk to each other as well as in working groups.

MR. SHA: Yes.

MR. KERR: You pick up a phone, maybe, or go down the hall to his office, something like that, right?

MR. SHA: Yes. We meet twice a year, and also frequent discussions.

MR. KERR: Okay.

MR. GARNER: Speaking for myself, we have had good relations in talking to the Oak Ridge people so far. The 19-pin tests are completed, the 61-pin tests. They are at the end of our steady state phase I test. They are getting ready to do the transient tests. We will talk to them and see what they are planning to do. Based on what they did on the 19-pin test, they did some nice work, and I expect them to continue with the 61-pin test.

MR. KERR: If they and you talk, it seems to me that it is possible that you might think of some things you need before they run the test that you otherwise might not get.

MR. GARNER: Right, I agree.

MR. CARBON: The BIFLO code is to predict the onset of sodium boiling and the growth of bubbles and so on.

MR. GARNER: That is correct.

MR. CARBON: Does it tell you when you might get into film boiling, some such thing? It has nothing to do with clad melting directly.

MR. GARNER: Up to clad melting, there is a clad and fuel pin temperature calculation with BIFLO. The continuation of the calculation into clad melting is something we hope would be a follow-on to the work we are doing on the boiling conditions right now.

MR. CARBON BIFLO itself right now is specifically the growth of the boiling phenomenon.

MR. GARNER: Yes.

MR. CARBON: Initiation and growth.

MR. GARNER: Yes.

MR. CARBON: Thank you.

MR. SHA: I am Mr. Sha. I work for Argonne National Laboratories. I am very happy to brief you on some of the progress we have made on the COMMIX code. We have two versions of the COMMIX code. One is COMMIX-I, and COMMIX-II is a two-phase flow. I understand that Dr. Kerr said some time ago -- he made a detailed discussion on COMMIX-I and some of the results. For my talk I will concentrate on COMMIX-II, on the two-phase flow area.

COMMIX-II treats the normal equilibrium temperature of both phases. This code has recently added a set of coordinates, so now we have XYZ, and the formulation in the COMMIX code is slightly different than subchannel. In there we use the volume porosity surface permeability and distributed resistance and heat souce.

Recently we added a new solution technique. An IMF solution technique was developed at Los Alamos, but we added a rebalance technique, which is speedup conversion.

Recently we added another new solution technique called the SIMPLER. We actually revised and added an additional feature to this.

The results presented -- we used the SIMPLER solution technique, and this code can treat both continuum or quasi-continuum. They treat all structure with boundary conditions, quasi-continuum, treated the fuel assembly. There are many applications in the use of the COMMIX code. COMMIX-I code is widely distributed. Practically every laboratory in this country has COMMIX-I code, and many laboratories actually use this code.

We have assisted them. If they have problems, they call us and we help them. Another code is the BODYFIT code, which at the present time we are working on the single-phase version only. This code is primarily for fuel assembly and the formulation, the boundary fits, the coordinates transformation, which I will show a little bit later during my presentation.

MR. CARBON: What does BODYFIT do?

MR. SHA: Make transformation for complex geometry. I will show you the slide a little later.

MR. KERR: That is to make a simple problem look complicated?

(Laughter.)

MR. SHA: No, no.

MR. KERR: To make a complicated problem simple.

MR. SHA: After the transformation, now, for laminar flow we are in a position to solve this problem for laminar flow only.

(Slide)

Now, I would like to show the progress in the two-phase flow area.

MR. CARBON: I don't know the purpose in life of BODYFIT.

MR. SHA: The objective of BODYFIT -- for instance, a few applications for small-rod test for two-phase flow. Two-phase is so complicated. You perform this analyis. You have data. You perform the analysis.

MR. CARBON: Two-phase flow to calculate pressure drop?

MR. SHA: Distribution.

MR. CARBON: All right.

MR. SHA: The problem is you have two analyses. One is physical modeling, the flow region, the heat transfer coefficients, all this kind of thing. Another is due to uncertainty in geometry because you cannot treat geometry rigorously. As a result of this, if you use subchannel analysis you cannot treat geometry rigorously, correctly, certainly. The geometry lump into the physical modeling and to the end you could not differentiate. You can fit the coefficient to get a good agreement. Then you move to another situation. You find there is a disagreement, the reason being because we really did not understand this problem fully. There is some uncertainty due to geometry.

MR. CARBON: You are using the word "uncertainty," but you mean, frequently, instead of uncertainty, inaccuracy.

MR. SHA: Inaccuracy may be an appropriate word, but this method separates -- no inaccuracy in the geometry part, so now I can concentrate on modeling. So that is the significant advantage, especially in the two-phase flow area. I think this makes a tremendous contribution.

Another thing you can do is, for instance, subchannel analysis code. They are the two subchannels. Empirical constant for subchannel mixing. Those are emprical constants. If I use the BODYFIT code, I can detail calculation. I can generate those empirical coefficients feeding into the subchannel analysis.

MR. CARBON: Okay, thank you.

(Slide)

MR. SHA: I would like to present some of the results in the two-phase flow area. We are struggling very hard. We are working very hard to have some kind of calculating two-phase flow, sodium two-phase flow. It is a real difficult problem. Lately actually I claim we have made a breakthrough. We can generate numbers. And today I am happy to report some of those. There is a lot of room for improvement, but the breakthrough means we have a stable system and can calculate two-phase flow.

But there is a lot of room for improvement. I mean don't misinterpret this. There is a lot of work ahead of us.

MR. KERR: The problem is not finished yet.

MR. SHA: Far away from it.

MR. KERR: I feel better.

(Laughter.)

MR. SHA: But it is a breakthrough in terms of computation. Now I will show you the three results, three calculations. One is two-phase flow in heated duct. We have experimental data to compare with. Second is NRC standard problem number 1, flashing due to depressurization. The third is the first time, as far as I know in my knowledge for two-phase flow, a calculation for sodium system.

(Slide)

This is the simple geometry of the heated duct.

(Slide)

There are a bunch of data available, one by St. Pierre. This is the comparison between the experimental data and the code prediction for void fraction. The pressure heat flux is shown here. COMMIX-II prediction --

MR. CARBON: This is not meaningful, is it? Simply you have some heat and some boiling, and this is just telling you how much void you have with a certain flow. This does not mean anything, does it?

MR. SHA: This is the initial step. You have to start from a simple case. That is what I am trying to do.

MR. CARBON: You have made no mistakes so far.

MR. SHA: This indicates the physical modeling we use looks reasonable for this. It is initial step for test.

MR. KERR: Two-phase flow of what, sodium?

MR. SHA: This is water, in this case. Water. I will show you later. First two cases are water. The third case is sodium. Lack of experimental data in sodium --

MR. CARBON: It is still just a simple thermodynamic heat balance.

MR. SHA: There is a homogeneous -- it is temperature in the liquid phase, velocity in vapor phase, liquid phase --

MR. CARBON: Is this in a regime where there truly is much slip?

MR. SHA: Yes. I will show you the result. This is 400 psi case, and this is 800 psi case.

(Slide)

We are not doing very well in here, the reason being we do not take into concept of subcool boiling.

MR. KERR: Why not?

MR. SHA: We are going to do that. We just got the result for the last two months. See, we working on this to improve this. This is the actual initiation of two-phase flow calculation -- come on line. Lots of room for improvement.

(Slide)

This is the 2,000 psi case. Again, we are not doing very well. We are not taking the subcool boiling in the calculation. This is the velocity measure.

(Slide)

This is velocity, and -- this is liquid velocity measure. This is slip. This is not simple thermodynamic equilibrium calculation.

(Slide)

This is at 800 psi. As you know, when pressure increases, then slip decreases. This is 800. This is 400. See, the slip is much larger.

(Slide)

This is 400 and 800, and 2,000 we do not have data on, so I cannot really compare. This very simple problem indicates our model works out all right. It looks reasonable, let's put it that way. Again, there is still a lot of room for improvement. For instance, in this case, subcool boiling we should have taken into account.

In the next case we compare NRC standard problem number 1.

(Slide)

This is flashing. The first section looks like this.

(Slide)

At the other end of the pipe -- essentially the pipe is 14 feet long. Pressure was at 1000 psi and 515 degrees Fahrenheit. That breaks the disc, so pressure is decreased. Then follow the pressure distribution and compare with, you know, measurement and prediction. They are the instrument station gs-1, gs-2, gs-3. There are seven instrumentation stations.

We compare two of them because they detail pressure measurement, gs-1, gs-5. This is a difficult problem, flashing due to depressurization.

(Slide)

This is pressure calculation at instrument station number 1, and you notice we did not present time to zero -- maybe a few milliseconds. The next Vu-graph will cover this. So you can compare the calculation --

DR. KERR: That does not look so good to me. Does that look good to you?

MR. SHA: It is a subjective evaluation. It looks to me for first crack as reasonable, but I am not very happy. I have to find out more about -- try to get better agreement. It looks reasonable at the beginning.

MR. KELBER: As a former code developer, I would want to know why the pressure gauge always reads high.

(Laughter.)

MR. KERR: If you take a round pipe and fill it with water and fill it up and rupture the end, you ought to be able to calculate that on the back of an envelope.

MR. SHA: Very difficult.

MR. KERR: These mechanical engineers --

MR. SHA: Mechanical engineers --

(Laughter.)

MR. KERR: Why should it be such a tough problem?

MR. SHA: It is a difficult problem, Professor Kerr, I assure you.

(Laughter.)

MR. KERR: You are pulling my leg.

MR. SHA: We show you another pressure calculation at station number 5. Like I said, we are happy to have this result presented here, but I really am not too satisfied and I have to dig in more to find out a lot of things that are going on which need more investigation. It looks reasonable again.

Now, here is the pressure. Now we talk about different time scale, milliseconds. The previous Vu-graph did not show. Now, in here we use two different time steps in our calculation. One is 10-3, one is 10-4. Results change. And further, 10-5. It would be very close to 10-4. So we stop there. What this Vu-graph shows you is a lot of parameters come into play, the reason being that change in thermal physical properties if we take larger time step.

MR. KERR: What does this show -- it shows me that a lot of parameters come into play. All it shows me is if you use different time steps, you get a different answer.

MR. SHA: A different answer. We examine the reason behind it. The reason is due to flashing. It happens so fast, and when you do calculation assuming the coefficient of the energy equation as a constant for that time step, that may not be good. If you cut it down to small time steps then treat it as a variation of coefficient, although the point I try to make is we have very stable numerical part of the equation, but physics dictates what the physics --

MR. KERR: How do you know that is physics rather than numerics?

MR. SHA: Well, this we have the data to compare for our calculation.

MR. KERR: You are getting a different kind of oscillation depending on the time step you use.

MR. SHA: The trend is the same. Results vary.

MR. KERR: Yes, I agree. In each case you are getting the same number of cycles for oscillation, I guess. But seriously, how do you know it is the physics rather than something that we do with a numerical solution?

MR. SHA: That is what we have to do some time step evaluation. Once we know time step varies 10-5, that did not change it. We follow on the time step at 10-4, so we knew that answer is true answer we are looking for for this particular case.

MR. KELBER: If I may comment on this, there are two aspects to this question. One is does the code contain sufficient description of the acoustic propagation down the pike? That is a relatively difficult problem for a code like this to handle. I think it is clear it can be handled because I think, for example, TRAC handles problems like this reasonably well.

The second question, from a programmatic view, is do we need such capability, and that is a decision yet to be made. I do not know that we are going to have to go into this time regime, so we may be able to make a physical approximation which says --

MR. KERR: I did not express my question well. It is a naive question.

MR. CARBON: It is a good question.

MR. KERR: I wonder how you tell just from looking at that whether it is numerics or physics?

MR. KELBER: I don't think you can just from that, Bill. There are a number of conceptual problems that you have to do to discern the difference.

MR. CARBON: You hit on one question I was wondering about. Suppose you get the code that fits this. Is this kind of test calibration really meaningful for your needs?

MR. KELBER: I suspect that by and large it is not, and I think this is something that Bill Sha is driving at. If we are looking at problems which take place over a longer time scale, the agreement out at the longer time scales may be good enough that we do not have to worry about acoustic effects. I think it is very useful to know what are the boundaries on the models that you do have in your code.

MR. CARBON: All right.

(Slide)

MR. SHA: This is the comparison for the void action.

(Slide)

This we don't have data on. This is velocity distribution at exit for vapor phase and liquid phase. There is no data available.

(Slide)

This is temperature distribution compared with experiment data at gs-5.

(Slide)

Okay. Now, the third problem we talk about is the 7-pin -- we try to predict boiling. This is sodium, and sodium is very difficult to get a solution.

(Slide)

This is test section. It looks like 7-pin. The reason we choose 7-pin is, you know, for validation purposes, relatively cheaper in terms of computer renting time.

(Slide)

This is the flow transient, essentially ten-second transient.

(Slide)

This is temperature calculation of thermocouple number 9. The location of thermocouple number 9 is shown in the previous Vu-graph. It starts from single phase and goes to two-phase. Boiling starts at 9 seconds. We predict boiling starts at 8.8. Actually, experimental data start at 9 seconds.

(Slide)

This is, again, another comparison between the prediction and the experiment data for thermocouple 25.

(Slide)

Now, I guess this is of great interest to us. This shows the radial boiling distribution at exit, at outlet, at end of heat section. This is the center of the rod bundle (indicating). It is conceded incoherency right here.

(Slide)

This is, again, radial voided distribution at the exit. You remember, there is heat section above heat section, which is exit, and so you can see tremendous incoherence exists for this particular --

MR. CARBON: Is this all calculation?

MR. SHA: Yes. Now, I will show you some experimental thing. Again, we are not doing too well but we are not too far off. This indicates measurements, the location of inception of boiling comprising this test. Here our prediction sideline is COMMIX-II prediction, and here at center of rod bundle, this is inlet, this is outlet. This is a voiding pattern. It grows as time goes on.

Now, at the 9.115, we predict inception boiling at this location. The reason is we can improve results very readily. There is one node almost as big as this one. So if we get the fine mesh in there, we can get a better resolution. It is not too bad. We can fairly predict. There is a lot of room for improvement, as I mentioned at the beginning. This is starting to come out results. It is really not bad results for the first time.

(Slide)

Let me go over special features of COMMIX-II code. With this code we can treat the continuum problem as well as quasi-continuum. The code is structured that you do 1-D calculation, 2-D calculation, 3-D calculation, and we can do single-phase calculation and two-phase calculation both at the same time.

We have two fluid model. We now implement homogeneous model to save time sometimes. We have sodium property, we have water property, perform water experiments, perform sodium calculation. We have steady state and we also have transient.

(Slide)

Now, we have spent in the past year all efforts to develop COMMIX-II. Most effort is spent to develop numerical solution technique. We try all kinds of methods. We use line-by-line, plant-by-plant, cell-by-cell, direct inversion. We have various options for explicit and implicit, steady and unsteady calculation.

Now, there is a difference between this code -- instead of steady calculation, we drop time-dependent terms. Many transient calculations include time-dependent terms. In here we actually drop the time-dependent terms.

We have two options for the solution procedure. One is IMF, one is SIMPLER. SIMPLER is the latest newcomer.

We also have a combination of central difference. If my time is up, I can cut off any time.

(Slide)

MR. CARBON: If we could in a couple of minutes or something.

MR. SHA: All right. I will wrap it up.

Those are features in the COMMIX code. We have hex geometry. We have fuel pin model, wire wrap model, duct wall model. We are probing modular form because we recognize all coefficients, heat transfer, so many correlations available.
So if user prefers something else, he can readily implement it in the code.

We have four boiling models, 15 momentum exchange coefficient models. The user can implement it in code.

Let me wrap up and talk about BODYFIT code.

(Slide)

Like Professor Kerr mentioned, the BODYFIT code is very exciting. Basically, it gives you better results, more accurate results. This design only applies to [illegible]le. In BODYFIT-I, actually we use two-dimensional transformation. Actually we use three-dimensional calculation. I remember last time I made a presentation in front of the Committee, I think Professor Kerr mentioned -- I forgot who he is -- wanting to implement wire wrap. If we include wire wrap in the bundle, then transformation is complicated. But we think we have found a way to solve the problem.

So, what we would do, so far BODYFIT-I. If you want to include a wire wrap, we have to have a 3-D transformation. Actually, we did that. I skipped some presentation on BODYFIT-I.

(Slide)

It is not completed yet, but I think we found a way to handle the problem. If you look at rod bundle, this is a unit cell for hex geometry. This is wire wrap. You imagine this is like rubber so wire can squeeze to any location you want and then tries to stretch. That is the basic concept.

This is to show you the mesh at that location, the wire showing there.

(Slide)

This is the equation.

(Laughter.)

This is a different location. Now, if you want to analyze 7-pin, 19-pin rod bundles, you see --

MR. CARBON: Bill, maybe we had better stop.

MR. SHA: What I am saying is the wire wrap model has not been completed yet, but what we found a way to solve this problem. But we are still working on it.

(Slide)

Current status. Right now all men are in COMMIX-II at present time, so COMMIX-I, we will clean up the document and try to release it by December 1980. This is an advanced version of COMMIX-I, which has tremendous -- a lot of people requested this code. On COMMIX-II, as I mention here, all the physical models are semi-complete and semi-implemented. Nothing is complete in two-phase flow because everything is always making improvement, so we have a lot of work that needs to be done, but I show you here far from completion in any one of them.

So, we will try to release the draft version of COMMIX-II by December 1980. A lot of people like to use the code. BODYFIT-I, we finished 3-D transformation. We developed the K-epsilon turbulence model. We implemented fuel rod and duct wall heat transfer model. Now we try to document by September this year, but that version does not have wire wrap model in there.

This the last Vu-graph.

(Slide)

MR. CARBON: Ten seconds.

Validation application. We are going to concentrate on FFTF, and Dr. Kelber mentioned we are working on the PFR, so hopefully we will get additional funding. We work at this. There are two more options we would like to add in COMMIX-IA. One is full core analysis. If you want full core analysis, you need a parabolic approach. You back off. You do not solve this -- you solve an entire --

MR. CARBON: Bill, I have to stop you. Let us read that. We have it here.

I would like to ask Charlie a question. Tell me in sort of an overall statement, where does this work fit in? What is its aim? What is its goal? Why have you had this done? I have some appreciation, but I want to hear what you have to say.

MR. KELBER: The aim of this work is to provide as accurate a calculational tool as we have or we can get to calibrate codes proposed for use in licensing cases. Questions kept arising during CRBR and, to a lesser extent, during FFTF on the adequacy of the representation of two-dimensional effects, particularly in the relatively complex subchannel geometries that we do have.

The COBRA codes at the time used an almost empirical description and even judgment description of interchannel flows, and this is fine for steady state problems. But for transient problems this has caused a lot of problems. I would say there have been many improvements in COBRA-4, and COBRA-4 is getting to have many features similar to those discussed today.

There are some problems when using COBRA-4, however. I think the people working with BIFLO can tell you some of the gory details about using COBRA-4.

MR. CARBON: How long will it be before this work is to the point where it can help in licensing, and then what influence will it ultimately have on licensing?

MR. KELBER: COMMIX-IA is useful now for calibrating codes used to give you a one-dimensional description of flow in a sub-assembly during coastdown to natural convection. So that work is essentially complete.

MR. CARBON: What influence does that have on licensing?

MR. KELBER: The problem here is -- I should say not just a sub-assembly, but also the plena. The question here is what are the maximum temperatures reached during the transition to natural convection and the construction of adequate models to represent that. The SSC work is a one-dimensional code. As you heard from DOE earlier, they are considering the implementation of a similar code with two-dimensional capabilities.

All of these require some extensive modeling. They will be benchmarked against COMMIX-IA, and DOE may even decide to try and incorporate a special version of COMMIX-IA in their new code.

MR. CARBON: But before you really can place great significance on the results from these codes, there is going to be a tremendous amount of validation work, I think.

MR. KELBER: I think so. The boiling problem, I am not sure how far we want to go. That arises from a different consideration. The boiling problem arises from the consideration raised during the CRBR hearings that the natural incoherence in a channel says that the reactivity rate induced by boiling is lower than you would calculate with a code such as SAS because there are a large number of code channels.

They lag behind, and therefore the predictions used by SAS of the reactivity rate from voiding are too high. I think we want to go far enough to have some understanding of what that effect is, and it will be useful in assessing a heterogeneous core, particularly where there is incoherence both on a large scale and a small scale.

I do not think we want to push that as far as we have pushed COMMIX-IA.

MR. CARBON: Did the NRC licensing people urge you to do this kind of work?

MR. KELBER: It is an outgrowth of their concerns. They started with a two-dimensional model. It is part of Professor Theofanis' technical assistance plan.

MR. CARBON: You initiated it.

MR. KELPER: Yes.

MR. CARBON: They did not request it.

MR. [illegible]TBER: By the time they got this work going, they were winding down the efforts of CRBR. I do not think we ever got a user need out of them. During the whole CRBR discussion, we never got a user request, as far as I know. But it would seem to us that if they were spending technical assistance money in this area, it was because they had an obvious need and we felt we had better try and build that on a more permanent basis.

MR. CARBON: Questions, Bill?

MR. KERR: No questions.

MR. CARBON: Fine. Let's go on, then.

(Slide)

MR. GUPPY: I am Jim Guppy from Brookhaven National Laboratories. I want to give a presentation on what we are doing in the Super Systems code at Brookhaven.

(Slide)

I will initially give a brief overview of the scope of the work, the present status, and then give some particulars on some of the results that we generated from one of the versions of SSC and SSC-L for various types of transients that we have conducted, some that are applicable to validation efforts, some that are applicable to normal applications, and then touch on future plans at the end.

(Slide)

For the development program, our basic scope is to develop a series of computer codes that simulate the thermal hydraulics of the entire plant, and with the purpose of studying operational transients and other system-wide transients, and with particular emphasis on natural circulation events.

Included in the modeling are the plant control systems and the plant production systems, and our overall goal is to adequately model all components and processes throughout the plant that are essential to heat removal. Another goal of the code is to execute simulated transients in real time or faster on the machine.

One of the overall uses is that it can be used to do system-wide analyses and to kind of pinpoint some areas where you might get into trouble. There you might want to use some of these other three-dimensional codes that have been discussed for those cases where you get into trouble.

(Slide)

The basic objective of the validation program is to qualify SSC as an independent licensing tool. We are pursuing various -- well, two main avenues of validation. One is by experiment and one is comparison to analytical results, inter-code comparisons.

(Slide)

As I mentioned before, we are developing several versions of SSC. We designate the various versions by tacking on another character at the end. "L" is for loop-type LMFBRs. "P" is for pool-type LMFBRs. Another with "W" is for LWRs, and the last version is SSC-S, where "S" stands for shutdown.

The first three are meant for short-term transients, up to a half-hour of simulation, and SSC-S would then pick up where these others leave off to simulate intermediate and long-term transients where other effects or components that are not important at full power, full flow, such as the shutdown heat removal system, where these come into effect.

(Slide)

Briefly, the background. We have been funded by ARSR since the beginning of fiscal 1976. In other words, late calendar year 1975. The major accomplishments that we have produced are that SSC-L has been operational since September 1977. During the recent fiscal year, we have made two other major milestones, one with the SSC-P code. It became operational in November. The SSC-W code, the first version became operational in March of this year.

(Slide)

Now I am going to start gearing the discussion more towards just the SSC-L code, which has been operational for the longest. I will concentrate on that mostly. We have distributed the code to a number of external users, and in keeping with one of the basic goals of the project, that is, to develop a generic code that is applicable to different system designs.

We feel we have accomplished this in part because we have successfully applied SSC-L to four different plant designs up to present: CRBR, FFTF, the German SNR-300, and also Babcock & Wilcox in some of their conceptual design study applications.

MR. CARBON: You words there were that you successfully applied it.

MR. GUPPY: We have taken plant geometric data and been able to because of the generality with which SSC has been developed. It is not specific to any one plant. That is what I meant. It can be applied through input just by manipulation of the input variables available.

MR. CARBON: Go ahead, then. And after the next slide up there, I want to return to the question of how meaningful is it when you are in the application stage and the development stage.

MR. GUPPY: All right.

VOICE: Dr. Carbon, some of the previous versions had specific plant design detail embedded in the actual coding, to the point that to apply it to a somewhat different system would require a major overhaul of the calculation system.

MR. CARBON: Hold up, if you will. Go ahead, and then I will come back.

MR. GUPPY: What I am trying to say here is that SSC-L has moved out of the developmental stage into more of an applications development verification stage. It is not just purely developmental now. We are trying to apply it to various experimental tests, and the moment we are giving particular emphasis to the FFTF acceptance testing phase.

I wanted to note that I will discuss it in the next slide. Because of the way SSC is constructed, a lot of the validation that is applied to SSC-L is also valid for the other versions of SSC.

MR. CARBON: Then going back to the line that is at the applications developmental verification stage, how much verification have you done? Where does it stand in that regard?

MR. GUPPY: In regard to verification, some of the -- not too far. In summary, not too far. We have validated several components, like the uppper plenum, against experimental data, steam generator against some steady state data. At the moment, as I mentioned, we are giving particular emphasis to being able to predict the FFTF acceptance tests.

I have a report here that I am going to leave that gives our pretest predictions. FFTF has not as yet executed these acceptance tests, which are really of interest to us on an individual component basis. Some of the components have had limited validation done with them. The whole system together, essentially none at this point.

MR. CARBON: Okay, fine.

MR. GUPPY: The validation against experiments --

MR. KERR: Have you had feeback from those users of mistakes, errors, inconsistencies in the code?

MR. GUPPY: Yes. The most prolific users to date have been the Gesellchaft fur Reakforsicherheit in West Germany, and the B&W conceptual design study group, and both of them have given us feedback, not so much as to errors, but perhaps improvements that we could make in ease of the user so that they can use it more readily.

In one instance B&W has applied it quite extensively, and they have supplied us back with steam generator coil modules so we can implement those in our steam generator, in our present steam generator capability. We had a straight tube, but since they are interested in one of their applications on helical coil, they supplied us back with coding to work with that.

MR. KERR: They have not had any mistakes?

MR. GUPPY: Not really mistakes. They have stretched some of the applications, particularly, say, B&W because they wanted to do some --

MR. KERR: Roughly how many FORTRAN statements in the "L" version?

MR. GUPPY: Roughly 25,000, of which about half are in the input processing stage. We do a lot of work to verify and to manipulate the data on input.

MR. KERR: I was just trying to get some kind of idea how extensive -- I would think if they had not found any mistakes, they had not really looked at the guts of the code very carefully. With that many statements, there are almost some mistakes.

MR. GUPPY: Either they have not looked at it that carefully or else they have not exercised it past the points that we have exercised it.

(Slide)

I want to comment briefly on the basic structure of SSC. One way of looking at it is essentially a set of building blocks of models and components that then interconnect together, and then it is basically, in my way of looking at it, how these blocks are interconnected together and what input the user uses. That is what differentiates one version from another.

There is a lot of overlap between the models and the components. A couple of examples would be that for the loop version and the pool versions, once you get past the IHX, everything is the same between the two versions.

F5
JL

(Slide.)

MR. GUPPY: I really will not dwell on this. This is some of the main features. You can look at those for yourself.

(Slide.)

MR. GUPPY: Just looking at an overall schematic, as I mentioned before, we simulate all the major components from the vessel out through the steam generator. This is a little misleading. There is no physical model for the turbine and the condenser. The pump in the steam generator is modeled, and the civility inherent in the steam generating simulation capability is such that we can handle various types of steam generator geometries that come up, plant to plant.

The code is written in variable dimension format such that you can have any number of loops, any number of nodes, any number of pipes. The components need not be in this arrangement. They can be shifted around. In other words, with variable dimensioning, the detail is there if you want it. It it not there if you do not want it, depending upon the transient or the application you are making.

MR. KERR: Considering the complexity of the code, is it likely that other users, that is, users other than the developer, can understand it well enough to use it intelligently?

MR. GUPPY: We have gone, I think, to a great deal of pain to make it user oriented. I skipped over that previous slide.

MR. KERR: I am not interested in your slide. I am interested in your comments.

MR. GUPPY: We have laced it very liberally with comment statements, and the documentation we supply is such, I feel that you can look at the documentation, and we give the various users -- as you know, it is a large code, and it encompasses many pages of microfiche.

The user can take the microfiche. We give them an index. We give them the documentation. It says that such and such a module does such and such. This is the name of the module. We have a naming convention. Very readily the person can take the documentation, then with the microfiche look at the code and, I feel, be able to understand what physical models are there.

MR. KERR: Is it well enough documented so that the user has a fairly good feel for the limitations of the code over what range of variables and situations it makes sense to use it?

MR. GUPPY: I think so. Yes, in my opinion, yes, whether it is one dimensional in the primary and secondary loops, whether it is single phase, and so on and so forth.

analyses. I will show some natural circulation transients and operational transients.

(Slide.)

MR. GUPPY: Various methods of code validation that can be done are line by line comparison with experiment, such as the FFTF acceptance tests. I do not want to mislead you here, because the comparisons that are embodied in this document that I believe are really pre-test predictions, because the tests have not been done yet.

Then, also, I will show some -- the other way that you can validate -- in some aspects, validate a code, but really just to show whether -- in my mind, to show whether there are any gross coding errors or modeling errors is also by intercode comparisons, and I will show comparisons between SSCL and the FFTF -- the Heddle version of FFTF called IANUS.

For the FFTF applications, what we had to do was, since we can use SSCL as is, with the exception of the tertiary system, they used air blast heat exchangers, so we had to devise what they call a DHX, dump heat exchanger module.

(Slide.)

MR. GUPPY: That was then interfaced with the steam generator. Normally, this coding effort is not all wasted, because we will use the same module, this same concept of this sodium to air heat exchanger for shut down heat removal system applications. So, this is what the FFTF looks like with the SSCL simulation.

(Slide.)

MR. GUPPY: The acceptance test that will be run --

MR. CARBON: Let me interrupt a second. On the last slide, the SSCL overview of the FFTF, how many nodal points do you have in the various loops?

MR. GUPPY: That is one of the things that I will discuss a little bit under natural circulation events, but typically we have in the primary system -- well, excuse me. For the FFTF we have -- You asked a premature question -- In the loops, they typically have 100 nodes -- mainly that is because for natural circulation transients you need a lot of nodes, like on the order of 30 to 40 axial nodes, because under these very low flow, low power conditions, you get axial skews, and if you do not have fine enough nodalization, you get non-physical results coming out of it.

MR. CARBON: To get good results that you can rely on, are 100 nodes anywhere near adequate?

MR. GUPPY: We have done parametric studies where we for a given transient -- I will show one of them later on -- for natural circulation analyses, where you get different results for what I call coarser nodalization. We feel that for the natural circulation cases that we have run, about

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100 nodes in the primary loop doesn't -- when we run 100 nodes versus 200 nodes, we get no difference in the computed results.

MR. KERR: I do not have to remind you what that demonstrates.

MR. GUPPY: That demonstrates --

MR. KERR: It demonstrates increasing nodes from 100 to 200 does not cause any change.

MR. GUPPY: Right. It does not invalidate the experiment, however.

(Slide.)

MR. GUPPY: The FFTF acceptance test, this is a summary of the initial conditions. They are going to run four tests that are of interest to us. Natural circulation tests, one starting at 5 percent power, 75 percent flow, another from 35 percent power to 75 percent flow, 75 and 75, and then 100 and 100.

These are the various initial conditions for those tests. To analyze and predict -- to pre-predict the acceptance tests -- I might just as well hand this over -- these tests predictions are summarized in that report.

(Slide.)

MR. GUPPY: We have come up with for the fuel assemblies -- we have divided the core into 18 channels. We have an 18-channel core model along with these, say, roughly

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100 nodes throughout the piping, and the IHX, and so on. This was devised -- they are grouped according to orifice zone. There are three orifice zones in the FFTF subassemblies, and so these are the FFTF assemblies, and these are the SSC corresponding channel numbers.

We have grouped them according to orifice zone, and then, according to power flow ratio. These are the FFTF designation numbers.

What this then shows -- Our Channel 1, what we call SSC Channel 1 is the hot channel in the FFTF. In other words, it has the highest power to flow ratio. There are two what they call fuel open test assemblies which are highly instrumented. There is one in Row 2 and Row 6. These two subassemblies we have modeled alone.

Then I will jus additionally point out that what we call Channel 15 can be construed to be an average channel, average meaning it has that steady state operation. That is what I mean by average.

(Slide.)

MR. GUPPY: I will just show one typical result. IT is discussed in a lot more detail. This is a typical natural circulation transient. You get an initial peak, an initial rapid peak, then an overcooling, and then an undercooling, and natural circulation flow is established. Then you get what is called the second peak, and here it is

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plotted from the 18 fuel channels. There is the hot channel and the average channel.

We have done various parametrics analyses with the SSC version, varying such things as the power of inertia, the core pressure drop, and computed results with and without the flow rate distribution being calculated. Just one minor parametric here shows the response of the average channel when 75 percent of the nominal decay heat was used, The nominal decay heat being 25 full power days, which is what they anticipate they will run the FFTF at to get the -- 25 hours of full power operation before they conduct these tests.

One of the big unknowns is actually the power that is going to be there. We have done some parametrics varying the power and other important parameters. Again, these are all pre-test predictions.

I will skip over to Page 17.

(Slide.)

MR. GUPPY: I will briefly just show now what these next series of vu-graphs embody -- they are now intercode comparisons, and really our basic purpose in running them is to see if we have any gross modeling or coding errors when we compare our results to that of another code, and in this case, it is comparing SSCL to the FFTF -- to the comparable FFTF code, called IANUS, and the cases

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that we have compared on this intercode comparison are normal scram, loss of electric power, pipe ruptures before check valve, after check valve, large and small break, and another what they call tornado.

All of these are not here. A report is forthcoming in the next couple of months that summarizes all these results. I will just show one of them for the normal scram.

(Slide.)

MR. GUPPY: This is plotted here, normalized. The time scale does not start at zero. But this is normalized flow and normalized power. They both start at unity and come down. This is a case of normal scram, typical normalized flow and power, and the IANUS predictions for the power and flow.

(Slide.)

MR. GUPPY: And then, similarly, for the maximum temperature in the core, the discrepancies in the temperatures are attributed to the differences -- minor differences -- although they really are not minor -- differences in flow rate shown on the previous slide.

You can look at the rest of them. I will discuss them if you want, but they are basically to show that for the transients we have analyzed on an intercode comparison basis, the results seem to be -- we do not seem to have any

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gross errors.

(Slide.)

MR. GUPPY: What I want to do now is present some of the other results that are now using CRBR geometry, and this is the simulation capability that we now have with the SSCL and also with the other versions of SSC as they have come of age to handle pipe breaks, scrams, loss of electric power, natural circulation advance, as well as various other operational transients.

The transients I will show in a moment are going to be concerned with natural circulation events and a couple of operational transients, reactivity transients and operator-initiated events.

(Slide.)

MR. GUPPY: We have quite a wide variety of applications work that we are working at, basically, broken down into roughly ten categories, and that is summarized on Page 26.

As I mentioned, I wanted to touch upon natural circulation results.

(Slide.)

MR. GUPPY: And some of the important factors to adequately model natural circulation are pointed out here. The flow rates are small, typically in the 5 to 10 percent range. Frictional pressure losses drastically are reduced.

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Small differences in the locations of the thermal centers are now important. It is necessary to have a detailed accounting of density distribution throughout the system, so you can trap the natural circulation driving heads, and also important are the heat capacity effects, the coolant interacting with the wall, because there are very long piping runs.

(Slide.)

MR. GUPPY: The specific results that I will show in a minute are for an LOEP, for loss of electric power, natural circulation event. I am showing this to show the impact of system nodalization on the results. On the primary side, it is a one-channel simulation, and two cases are presented, one which I call detailed nodalization, and one which I call coarse nodalization.

These are roughly 100 nodes for the detailed node and about ten for the coarse nodalization.

MR. KERR: Mr. Guppy, if one goes back to the previous slide, if I may, for just a minute, is the difference in nodalization an effort to achieve those points that you are making here? For example, does one get a better description of small differences in location of thermal centers with fine nodalization and better track of thermal centers and density changes?

MR. GUPPY: Yes.

MR. KERR: All right.

MR. GUPPY: One other point I did want to make here is that in keeping with trying to make this computationally efficient, I would just point out the results here. This is for a CDC 7600. The transient ran out to 360 seconds of simulation time. For the detailed nodalization case, it required 226 seconds of CDC, 7600 time. Nodalization was 141 seconds. It is what I call real time or better simulation.

(Slide.)

MR. GUPPY: This is the results of the detailed versus the coarse nodalization for the primary loop flow rate. You can see there is indeed a difference in the natural circulation flow rate achieved which is directly attributable to the degree of nodalization that was used.

The next vu-graph on Page 30 shows the temperature respone for the peak coolant temperature, and there is a difference in time to the second peak as well as a difference in the actual magnitude of the peak. It is roughly 39 degrees Kelvin here.

Now, this difference in the temperature is directly an effect of the flow rates being different and the flow rates being different are from such things as are shown on the next slide.

(Slide.)

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MR. GUPPY: What I have plotted here is just the gravitational pressure drop plotted in Newtons per square meter. This is about 9 psi. The transient was run 10 seconds at steady state before the rafter was scrammed. This is for the core gravitational pressure drop.

You will see the differences. They are minor. This is a blown up scale. This is roughly 1,000 Newtons per square meter, but the driving force under natural circulation is on the order of a psi or so.

So, there are differences in the -- there are differences in the various components for the natural circulation head. That is just for the core.

The next vu-graph shows the next gravitational head. It is plotted in Newtons per square meter. There are minus values in the SSC terminology. A minus pressure drop is a pressure gain. You can see that it is on the order of 3,000 Newtons per square meter, roughly one-half a psi or so for the natural circulation driving head.

As is evidenced here, there is less of a natural circulation driving force for the detailed nodalization case.

MR. CARBON: Excuse me, Mr. Guppy. Let me ask you to wind up within five minutes if you could.

MR. GUPPY: Oh, all right. I thought I was allocated 45 minutes.

MR. CARBON: Try and close it off a little sooner

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if you could. We are behind schedule.

MR. GUPPY: Okay. The next few slides were to show -- again, these are intercode comparisons, trying to show some multidimensional effects down at low flow, low power conditions. Intercode comparison 1-D versus 3-D for a section -- horizontal section of pipe, a severe transient, that is about as severe as you are going to get for a natural circulation type of event.

The purpose of showing it was to show that, Number 1, the agreement was not that bad, and also, you can use the one-dimensional results to indicate areas where potentially the three-dimensional code would be necessary.

(Slide.)

MR. GUPPY: Page 34 indicates the input response.

(Slide.)

MR. GUPPY: Page 35 shows for various -- the 17.7 meter long horizontal pipe, these are at five and a half meters, 11.6 meters, and at the end of the pipe, these results are with coolant wall interactions. These are without coolant wall interactions (indicating).

The dashed lines are the SSC one-dimensional results. The solid lines are the tempest code results, which is out at Pacific Northwest Labs, and the dots here are from the comics code results (indicating).

That is the agreement for the one-dimensional versus the 3-D.

The other thing that I wanted to indicate from that was that we did use a correlation that was derived by Jackson and Fewster from water experimental results to derive a dimensionalist parameter in terms of the heat and momentum transfer numbers.

This gamma, which is in this form, and they found for water experiments that if gamma was greater, it should be 1.0 times 10^{-4}. The buoyance effects in a round pipe would be important, and potentially earmarked areas where three-dimensional effects were important.

Page 37, I just show the three axial locations, by using our one-dimensional results, again, where significant buoyance effects could come into play, and if you look at this and go back to Page 34, you can see some of the -- like the five and a half. The thing starts to -- You should note that Page 34 starts at 40 seconds. It does not start at zero.

(Slide.)

MR. GUPPY: You can see that the comparison is poorer during a period when significant thermal effects could be shown.

To summarize that, what I was trying to show there was natural circulation effects, coolant mixing in the pipes and heat capacity effects between coolant and the wall are important. The 1-D results for the case study were in reasonable agreement with the 3-D code results. It was good for a fairly severe case, and also that the 1-D results could be used to predict where 3-D codes could be useful.

(Slide.)

MR. GUPPY: I will not show the specific results of the next few slides. What I will do is just say, we do have operational transient capability, because we have the plant protection system and the plant control system models. We feel fairly generic -- the generic models for input. The user can add more. We do not have -- we do have control systems for the reactor, multiple control banks, pump controls both on the primary intermediate and also on the feedwater and at the turbine and turbine bypass.

In the succeeding pages, are sample results of operational transients that were generated. As I say, we can do operational transient analysis and study the impact of control systems on the plant protection system actions.

Okay. We have some future plans that are noted on Page 43.

(Slide.)

MR. CARBON: Let us just read those, if you will.

MR. GUPPY: In summary, we do have versions of SSCL P&W that are operational. We have a wide range of applications work that is under way. The SSCS work is under way. Code validation work is proceeding. It is not anywhere near complete. We do have continuing effort going on in user support, not only in this country but in foreign countries, and model improvements and extensions are being implemented as required.

MR. CARBON: Fine. Let us take a ten-minute break.

(Whereupon, a brief recess was taken.)

MR. CARBON: Go right ahead.

(Slide.)

MR. KRESS: I am Tom Kress, from Oak Ridge National Laboratories. I manage the aerosol release and transport program. I would like to remind you what the aerosol release and transport program is about.

It is a consequence assessment program for severe accidents, and our studies are focusing in two areas, those relevant to the primary containment and those relevant to the secondary containment.

The primary containment studies we are releasing highly energetic molten U02 under sodium. The study -- principally the transport of this material to the transfer area -- in the secondary studies, we are focusing on validating aerosol behavioral codes under conditions where you have mixtures of nuclear aerosols, sodium oxides, and uranium oxides.

Also, I would remind you the primary containment experiments are conducted in a small scale model vessel about one-tenth the scale of the CRBR. The vessel is about two feet in diameter, about six feet tall, and in this vessel we heat samples of UO2 by electrical means, electricity stored in capacitors, put it in very high molte energy states, on the order of 4,000 joules per gram, and then we study the dynamics of the vapor bubbles that are created to identify the things that occur just to see what happens, and then to quantify the dynamic behavior, primarily to look at the transport of the material.

(Slide.)

MR. KRESS: This is a photograph of the fast sodium facility as it exists today, just to show that it is in place.

(Slide.)

MR. KRESS: This is more illustrative of what we do. This is a diagram of the system. Samples of UO2 are mounted in a low position in the vessel. The discharge condensers -- we put them in a high energy molten state, and these essentially disassemble, much like a small scale reactor event, and the UO2 vapor bubbles grow and form under the liquid, whether it be water or sodium.

We are presently conducting water experiments. The type of measurements we make and the type of information we look for, in water tests, we have a point not shown in which we take high speed motion pictures to try to identify the dynamics of the bubble formation.

There are rapid response submersible pressure transducers. There is a train of thermocouples, a matrix, so to speak, in the path of any rising bubbles that might be created that are intended to respond to a thermal transient so one could map the position of the interface.

There are also -- Not shown is the pressure transducer, and the cover gas space respond to movement of liquid and the compression of the gas and could be used as a measure of the volume of this bubble at any time.

We also take samples of the material that reaches the cover gas, so we can determine the quantity that survived the transport. We look at these samples to determine their characteristics in terms of aerosol sizes.

Finally, we are developing a system in which we mount acoustic transducers on the exterior of the vessel and use pulse echo techniques to image this bubble. This is a development item we are trying to use because we cannot see the bubble with motion pictures when it is under sodium. We use the acoustic device as a measure of the size, position, and velocity of this bubble.

(Slide.)

MR. KRESS: These fast primary vessel experiments are being conducted in three separate phases. First, we are not having any liquid present, just an argon environment. Following that, we are doing disassemblies under water, and then we will proceed to under sodium experiments.

We have presently reached the milestone in which we completed the second phase of the experiment work. This morning I want to discuss what we have found out in these two phases, and the implications -- what I think the implications are.

(Slide.)

MR. CARBON: Is the work that you have done of importance to the LMFBR people?

MR. KRESS: I think it would be, because it essentially is releasing molten fuel under water, and looking at the events that occur. I think they should be interested in it, although LWR's are not projected to get quite this high in energy state. So it does exceed what they need.

First, the system model is one-tenth of the primary vessel. What is this disassembly in terms of scale? Is that the same scale?

MR. KRESS: Linearally, it is the same scale. In terms of surface to volume ratios, the bubble is much different. We produce bubbles on the order of one foot in diameter. The LMFBR postulates that, so surface to volume ratio to scale is one of the diameters -- it is like the one-tenth. The objectives of the non-liquid experiments, the argon environment experiments are first to develop and understand the electrical capacity discharge system for producing these disassemblies.

This was an item that was a development item to begin with. It had never been done before. The use of electrical energy produced high energy bubble states in UO2 had not been done before. We did these argon tests. It is a way to produce UO2 condensation aerosols, and it serves as a way to characterize these condensation aerosols which can be viewed as source aerosols that might get produced at the source, and the argon tests serve as relatively unattenuated tests in the sense it creates maximum amount of vapor without any liquid and structure around to cool the system off. So, it serves as a base line -- not a calibration, but a base line determination of what the maximum amount could have been available to transport to the sodium, and we will use that compared with what actually gets through as a measure of attenuation.

(Slide.)

MR. KRESS: Looking first at the kind of results we got in terms of characterizing the aerosol, we conducted a series of tests at different energy inputs, different energy levels. We selected several of these that span the ranges of energies all the way up to just molten to just

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under the 4,000 joules per gram, and we took photomicrographs of the aerosols produced and determined the size distribution of the primary aerosols, and determined that they were indeed log normally distributed, and they were, as expected, small condensation aerosols on the order of .014 micrometers.

I will discuss possibly the significance of that kind of data in just a moment.

MR. CARBON: I would appreciate it if you would. This is in argon?

MR. KRESS: This is in an argon atmosphere. It is relatively rapid cooling.

Another type of information we get is the quantity of vapor that gets produced. Here I plotted it as a percent of the initial sample. The initial sample is about 20 grams. The percent -- the fraction of that 20 grams that becomes vapor after this high energy material expands down to one atmosphere -- this is a function of the energy output. This upper point here would correspond to just about 3,500 joules per gram in this case. It is just barely above the molten state.

What I plotted here is experimental data in the middle, and it is bracketed with two extremes of analytical calculations. The three points represent a condition -- we have to preheat the samples. These three points are at identical preheats that are comparable to these calculations. The other points are data that essentially had a different preheat history, but they were close enough, we felt they would fall in the same sort of comparative point.

Other data follows in kind of a range around this, the only difference being the preheat. The differences in the calculations involve, first, no changes in physical properties of the liquid as it goes through the melt. That is this curve. But as it goes through the melt, UO2 changes its electrical conductivity, changes its thermal conductivity, and changes its density. If you include what we think are realistic values for those changes, and redo the calculations for the electrical energy discharges, it moves this curve over here.

By putting in appropriate physical properties of the changes we are able to bracket -- these are adibatic expansion calculations. It tends to show an adibatic calculation is an appropriation way to determinen the vapor yield which you would expect.

(Slide.)

MR. KRESS: The implications of these argon tests, looking first at the size distribution, these are condensation aerosols we are looking at, and the production of condensation aerosols is by homogeneous nucleation. The size you get depends on competition between nucleation rates and growth, and a difference on the supersaturation ratio. So, if in the gas tests we think the dominant mode of heat transfer is radiation -- we also believe that will be the case in the under liquid test.

If so, they have comparable cooling rates. You expect to see the same sort of size distribution for the primaries. If when we go to the liquid test we get a different size, it will be an indication to us that perhaps there are different modes of heat transfer. If we get the same size, we feel relatively confident in our assessment of radiation, that that is the major mode, and it gives us confidence in our experiment.

Also, knowledge of these --

MR. KERR: Tell me again why you would get different sizes if you have a different mode of heat transfer.

MR. KRESS: Not a different mode, a different rate of heat transfer. It does not depend so much on the mode as the rate of heat transfer. It depends on the supersaturation transfer. So, it is the rate of heat transfer.

MR. FIRST: The agglomeration rate is the same. If you have more time, you get more agglomeration.

MR. KRESS: Agglomeration is not a factor. It is strictly a production of a fundamental particle from the vapor state to the liquid state.

MR. FIRST: If you have a lot of particles, you are going to get agglomeration nonetheless.

MR. KRESS: We separate those out.

MR. FIRST: This is a calculation.

MR. KRESS: In looking at the data. In looking at the photomicrographs, we do not look at agglomerates. We look at the individual particles.

MR. FIRST: What do you do with the agglomerates?

MR. KRESS: We look at the parts that make up the agglomerates.

MR. FIRST: You try to count the particles in the agglomerates?

MR. KRESS: Yes.

MR. FIRST: What fraction, roughly, of the particles you count are in the agglomerated state?

MR. KRESS: In these experiments, all of them.

MR. FIRST: All of them are? You never see a single one?

MR. KRESS: We collect these samples at some time during disassembly, and they are allowed to agglomerate.

MR. FIRST: How many particles do you get?

MR. KRESS: Thousands.

MR. FIRST: Thousands?

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MR. KRESS: Yes.

MR. FIRST: I don't understand how you can size those optically.

MR. KRESS: It is difficult.

(General laughter.)

MR. KRESS: You count many. That is why we only looked at a selected number. You can use these primary sizes to synthesize properties of agglomerates. Properties of agglomerates -- it is important to know those, and the implications of the yields mostly to understanding our experiments -- they give us the base line data so we can find out how much gets attenuated during transport, but it also shows the relevance of using adibatic calculations to establish vapor qualities on expansion mode of fuel down to some lower energy state.

(Slide.)

MR. KRESS: The scope of the under water experiments, the purposes of these were to validate the particular capacity for discharge design that we would use under sodium by testing it out under water first, and by the use of high speed motion pictures under water, we hope to identify the expansion phenomena to see what happens as the bubble is produced.

We use these high speed motion pictures to correlate our instrumentation from other instruments that are discussed, so that we can be confident that we can interpret the sodium results when we get them.

We would like to quantify the condensation and transport rates under water for comparison with sodium and comparison with our analytical models that we have, and we are trying to develop this ultrasonic imaging system I discussed earlier.

Those are the objectives generally.

MR. KERR: That ultrasonic imaging system measures pressure pulses?

MR. KRESS: Yes, it is a system in which the acoustic transducers are emitters, and --

MR. KERR: You are not measuring something like bubble collapse?

MR. KRESS: We send out a signal and let it bounce off the bubble, and it comes back. It is a sonar type.

MR. KERR: It measures sizes?

MR. KRESS: It measures sizes and positions. We hope -- We are developing that. We hope that is what --

MR. KERR: You hope it will. That makes me feel better.

MR. KRESS: We have some experience with it.

(Slide.)

MR. KRESS: I am not going to dwell on this slide. These are the test matrices of the water tests we

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have completed. We varied the quantity of xenon that was included in with the sample as a non-condensible gas. We varied the water temperature over two levels, one a relatively cool level and another one at essentially boiling point or close to it. We varied the height of the liquid above the sample by two values, something like almost four feet and almost -- a little over two feet above the sample, and the energy input from the capacity discharge was varied.

So, that is the kind of test matrix results I am going to discuss now.

MR. KERR: Why did you measure the high millimeters rather than centimeters?

MR. KRESS: Because millimeters is the ANSI acceptable standard. We are converting over at Oak Ridge.

MR. FIRST: Are these numbers purposeful in all cases, the numbers for energy? Is this just something you got?

MR. KRESS: The energy is. The rest are controllable. The energy is not controllable. We tried to set the system up so we could get energies between 30 and 40 kilojoules. Energies less than that are really not --

MR. FIRST: In your gas pressures, I see you have three decimal places. Were those purposeful also?

MR. KRESS: Not in terms of the decimal places. The important ones are the first two decimal places. Those

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are like .1 -- .1 millipascals in atmosphere --

MR. FIRST: These come from conversions?

MR. KRESS: Yes.

(Slide.)

MR. KRESS: A summary of observations from these tests -- well, maybe I ought to look at this slide here first.

(Slide.)

MR. KRESS: This is the kind of result we might get. These are pressure events produced by the CDV charge as measured by our submersible pressure transducers in the liquid itself. On discharge we got an initial pressure burst hollowed by an expansion of the bubble. That lowers the pressures. It gets lower than normal system pressure. The bubble collapses again and creates a second pulse, and the oscillation continues in ever-decreasing amplitudes.

One of the things we can measure it this period between these pulses and compare that with some analytical models we have, and this is a kind of -- if you vary the system pressure at the start the cover gas pressure from -- this is like the atmospheres. One atmosphere. One atmosphere with a change in temperature of the water and lower than one atmosphere.

You can see how sensitive this period is to these variables. It is a sensitive measure that we can compare

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with our analytical models.

(Slide.)

MR. KRESS: Speaking of the analytical model, this is an example of the UVABUBBLE code being developed at the University of Virginia. It predicts the same pressure pulse. This is the initial prediction. This is in millipascals. This is a bubble expansion. As a parameter, since radiation heat transfer is an important mode in this model, they varied the emissivity of the vapor to see what effect it would have on the second pulse. It had very little influence on this period. It does influence the second pulse considerably.

So, by measuring period, we can check a lot of hydrodynamics. By measuring the pulse, we can check out some of our parameters to see what are the appropriate emissivity values to use in the code itself. Our data so far indicates an emissivity value of about .15 seems to describe our second pulse.

MR. KERR: Does it make sense physically?

MR. KRESS: We do not know yet. It is not -- It has to do with geometry also. We are looking at that to see if it matters.

MR. KERR: So you think these emissivity numbers have some physical significance and are not just a fudge factor which permits you to --

MR. KRESS: Yes, I do believe that. We are not altogether convinced yet that radiation heat transfer is the primary mode, because we do see entrainment of the liquid. These entrained liquid droplets can have a strong influence on heat transfer. In this case, it may not have physical significance.

To summarize the fast water observations, the disassemblies do produce single coherent oscillating bubbles.

(Slide.)

MR. KERR: What is the significance of the word coherent as used there?

MR. KRESS: It did not produce several bubbles, and it did produce a spherical type bubble that was not a jet -- it was definable as a spherical entity.

MR. KERR: I was not being critical. I wanted to know what it meant.

MR. KRESS: Definable as a bubble with a clear interface.

MR. FIRST: These are all just one bubble formation.

MR. KRESS: Yes.

MR. FIRST: Is there somewhere along here you are going to connect this up with a continuous heat source?

MR. KRESS: No. No. We are talking about single bubbles. The films indicate there is enough cooling of this

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bubble so that aerosols are formed in place within the bubble. I guess that should have been obvious, but I was not sure that it would do that.

MR. KERR: What is an aerosol in this sense? You get little droplets?

MR. KRESS: Yes. Droplets and solids.

MR. FIRST: I should think so.

MR. KRESS: These vapor bubbles have been observed to rapidly condense within the first couple of hundred milliseconds during the oscillation phase before they have time to rise significantly through the liquid. They condense completely. We have measured very little transport of any of the UO2 to the cover gas except in very special cases in which we place the water temperature very near the boiling point.

In those cases, we do measure a little bit the transport, and we think this is because of the position of the water. It may produce a more persistent bubble, and these measured bubble oscillation periods which depend on the hydrodynamics and heat transfer have been in generally good agreement with the preliminary calculations using the University of Virginia UVABUBBLE.

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(Slide.)

A quick look at the status of the instrumentation, we have been satisfied with the response of the submersible pressure transducers under water. One comment, they require insulation from ground to prevent interference from electrical discharges.

When we get to sodium, we don't think we can use the ground, so we will lose the first impulse. A good measure of the bubble isolation frequency as compared with what we see on the high speed motion pictute films -- and the measurement of the pressure level is what we would have expected from calculations and by calibration using static pressures.

We think these are now ready for under sodium use.

MR. KERR: The CDV electrical interference comes from a magnetic field?

MR. KRESS: Yes.

MR. KERR: The sodium may provide a good insulation against that.

MR. KRESS: It may. In the water test, we had to ground it. The cover gas measurements, they were effective in measuring the cover gas pressure. We were able to interpret the response as a measure of bubble volume, as compared to the motion picture films.

The particular transducers we had were much too big by a factor of 10, so the sensitivity was not good. We have

bfm2

now changed those out and put in a smaller range unit.

MR. KERR: Were these commercial units?

MR. KRESS: Yes. The ones in the cover gas are a little different than the ones in the submersible, but they are on the same principle.

(Slide.)

I guess I ought to cover the rest of these. The thermocouple array was disappointing because our bubbles condensed so rapidly that they really did not have an opportunity to contact our thermocouple. There were a couple of thermocouples close to the bubble position. There was some limited contact there.

It indicated to us that we have too slow a response of thermocouples to be able to track the bubble. That is the change that is needed. As far as development of the ultrasonic bubble imaging technique, we first tested it out using an auxiliary tank with fixed spheres.

Bubbles, tennis balls, things like this at different sizes and just let them rise up past a fixed transducer. We were able to determine the size and the velocity and the position of these fixed spheres with the ultrasonic devices.

We then attached them to our water system and were able to get them on in time for four tests. In two of the tests, we had faulty mounting. One of the problems is how do you mount these?

They have to be mounted on wave guides. So, mounting bfm

is critical. We had faulty mounting that came off during a couple of the tests. The other two tests, we got signals that were interpretable in terms of the first extension of the bubble size, but the signals -- I think there is still a mounting problem.

We were not able to see the subsequent oscillation of the bubble in these things, although the films show they were there.

MR. CARBON: Tom, why don't you plan to wind up within five minutes and give us a couple of minutes for questions?

MR. KRESS: Okay. Let me switch then to the aerosol part of the program. The secondary containment part is being conducted in our nuclear safety pilot plant vessel. What we do there is we use a plasma torch technique. At the same time, we introduce liquid sodium. We mix these primary LMFBR aerosols in different proportions and in different mass concentrations and make a full battery of measurements of the aerosols so we can validate the computerized models that are being developed for the natural attenuation of these aerosols, with the view in mind of eventually determining quantities that might be released in secondary containment.

(Slide.)

We have completed another major milestone in that part of the program in that we had outlined a test matrix that would scope the parameters for these mixed aerosols. This is a matrix we had outlined which varies the total concentration, the mass ratio of the two different species and the natural size difference between the aerosols by varying the time at which they are mixed.

We are now completing this matrix of mixed aerosols. The typical kind of results we see with the mixed aerosol test -- first, there is a burn to produce U_3O_8. After about an hour's time to allow this stuff to age, and to agglomerate, we introduce sodium.

This is aerosol concentration in the vessel versus time. At this time, there is no abrupt change in slope, a concentration decrease and the U_3O_8 follows the sodium. This is the typical kind of evidence that we get, that the two aerosols are acting together.

They co-agglomerate. They act as a mixed species aerosol. The dashed lines compare them with equivalent runs in which this component was put into the vessel just by itself without the other. So, you can see there is a change in behavior when they are mixed together.

(Slide.)

If you look at a single component, U_3O_8 run the data of the dots. The curve is HAARM-3 calculation. Starting from a known condition of concentration and size distribution, you can see that appropriate choices of the parameters allows you to do a pretty good description of the mass concentration.

You should not really believe that data below 10^{-2}.

This is strictly for U_3O_8 aerosol alone in which these parameters correct for the fact that U_3O_8 aerosols go together in long chains and become fluffy aerosols. These are parameters that correct for that.

If you mix the two aerosols, sodium oxides generally exist as solid spheres rather than as chains. So, you would expect a different behavior. This was a case in which we mixed a sodium to uranium aerosol, with about a one tenth ratio of mass, where one is sodium and ten is the uranium.

You can see in this event, this is what you would use in a code to describe a pure sodium aerosol with a spherical behavior. This is what you would use for U_3O_8, a chain aerosol. You can see a one tenth aerosol, which is primarily U_3O_8.

You can still describe the behavior better by using the U_3O_8 properties.

(Slide.)

Shifting now to a one to one ratio, where they are equally in there in terms of mass, you can see you still do better by using strictly a U_3O_8 properties, sc the chain-like properties of the mixture tend to dominate the behavior. If you go even further to four times as much sodium, four times as much of the sperical property.

You see at this point, you are beginning to part a little bit from the dominance of the chain.

(Slide.)

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It took a four to one ratio of the sodium to get that. So, you do not strictly use a mass waiting system to describe this kind of behavior. There is a dominance of chain-like behavior.

We think this is new information that is of significance.

(Slide.)

I would like to summarize the observations from this program. We feel now that the HAARM-3 code is adequate to describe the single component U_3O_8, or the NA_2O_2 aerosol.

Co-agglomeration is, in fact, the case. These two aerosols when mixed in all proportions -- by this kind of data here, it was shown that U_3O_8 chain-like species appears to dominate the behavior, even up to a four to one ratio of the sphere.

I will skip these, too. We also have shown that these chain-like characteristics of the U_3O_8 tend to disappear if you have a lot of moisture present. I would like to show that before I quit.

(Slide.)

This is what U_3O_8 aerosol looks like in a very dry condition. A chain-like fluffy -- it looks like a cobweb type of thing. If we ran a test, just like this dry test, the same concentration but we introduced a lot of steam into the vessel, about three times the saturation values, it looks like this.

(Slide.)

So, moisture tends to destroy the chain-like appearance.

MR. KERR: The size of the larger particles is about what?

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MR. KRESS: This would be on the order of 2 to 5 microns.

MR. CARBON: We are really out of time. Do you have questions -- I think it would be more important for you to ask questions if you have any, than for more presentation.

MR. FIRST: What is this intended to simulate? That is nor clear.

MR. KRESS: The aerosols?

MR. FIRST: The experiments, what particular accident or sequence are you simulating?

MR. KRESS: We are trying to keep it accident scenario independent.

MR. FIRST: Do you mean this is a pure aerosol study?

MR. KRESS: Right, but we fixed the ranges of our things so that you can postulate releases of sodium into the secondary containment for an LMFBR.

You can postulate releases -- how much UO$_2$ gets in there. You talk about concentration levels on the order of one to ten grams per cubic meter. We are in that range with these things.

In fact, we have exceeded it. We try to produce the aerosols that have the same properties and the same behavior as sodium.

MR. FIRST: Ralph seems to --

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VOICE: Let me clarify something. Up to now, the NSPP tests were working in the dry conditions, fuel aerosol sodium, aerosol under different sequences. In other words, different -- which could imply different sequences the way he has introduced the sodium first or the UO_2 first.

The program would now move over to the more generic core melt aerosol, where later in the accident, when you start to get into a core melt, you start to get moisture from the interaction from the heating of concrete within the containment, which now would bring you into a new condition; no longer a dry condition.

Also, it brings into play large mass quantities of other aerosols besides sodium or UO_2.

MR. FIRST: That is an altogether different regime.

VOICE: That is right. That is the regime that he is just starting to get into.

MR. FIRST: What is the relation to what he is doing, to what you are coming to? This, I do not understand.

MR. KELBER: This regime is the early part of that.

MR. FIRST: IF none of the aerosol gets out of the pool to start with, what are we worrying about? What the particle size of the condensed UO_2 is and whether it agglomerates or doesn't. If it all remains in the pool.

VOICE: No, but --

MR. FIRST: You are talking about an altogether different

bfm9

accident now, which is a vessel release.

VOICE: First, we are starting with the vessel release, which includes fuel and fission products.

MR. FIRST: He is not talking about that. He is talking about a bubble inside the vessel.

VOICE: Assuming failure of the vessel, now you have a source term.

MR. FIRST: But you have an altogether different aerosol generating mechanism. This does not have anything to do with spillin sodium out of your system into the containment vessel -- into the large vessel.

MR. KELBER: Let's back up a bit. In the CRBR licensing, the primary source was from a possible HCDA in which material was transported in the bubble through the sodium out of the upper head by virtue of -- by virtue of the pressure exerted on the upper head, which causes it to lift and open up a path.

That gave the so called one percent source term. We, as well as DOE, have wanted to understand that source term for some while. We are now concluding that work.

There is a second cause of accidents. One which is common to LWR and LFMBRs. That is the core melt accident where we are not concerned with the energetics, but with the fact that eventually you spill the core on the floor and then you have a problem.

That is where we are going. In addition, you do have

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to worry in the case of the LMFBR about how these two situations interact. That is, if there were an end of spectrum accident in an LMFBR an aerosol produced in the course of an energetic recriticality would be expected to escape from the primary vessel.

This comes along after the initial burst. This was another part of the program. This comes during recriticality when almost all the core is molten. There probably would not be a great deal of transport through sodium under these conditions.

That aerosol would then be in the containment during the time that core melt is going on. Then, you have this rather complex species. That is an end of spectrum type accident. I do not know how much priority that is going to have in the scheme of things.

Our first priority in the comin years however is going to be the aerosols produced when you get the core on the floor. As I say, the thrust there will not be toward LMFBRs, but really toward LWRs.

MR. KERR: I misunderstood because I thought his description of the experiment -- he was talking about energy densities that you would only get in some sort of peak pulse situation.

MR. KELBER: That work is now coming to a close.

MR. KRESS: There are two parts of the program. One is the energy density relative to the primary containment, then was the second part which I showed later with the aerosols, which is relevant to the secondary containment.

bfm11

It is a fundamental aerosol study applicable to the secondary containment.

MR. KERR: How do you get to the aerosols?

MR. KRESS: We make them.

MR. KERR: Can you get them from a water reactor core melt down?

MR. KRESS: Oh, yes. You get aerosols produced from concrete.

VOICE: There are -- in the sodium case or the case of water over the debris or melt that is interacted with concrete, you get a sparging of aerosols, fission products as well as fuel coming up through the liquid.

This has been seen in the Sandia experiments.

MR. KERR: You get them when you start getting the concrete interaction.

VOICE: Yes.

MR. KERR: Not before, as far as you know?

VOICE: Some of the more volatile ones would be coming off first, but then the --

MR. KERR: Some of the moe volatile what?

VOICE: Fission products, without having to require the spargin effect, once the gases from the concrete sparge, then they would take the lesser volatile materials too.

MR. FIRST: I do not want to hold up the proceedings here. Maybe somebody would, at the end of this, take a few

bfm12

minutes and try to explain to me what connection the work that you are describing has to what you are talking about.

I cannot see the connection.

MR. KELBER: I think it is extremely clear. I do not understand the confusion. Let me put it very simply. To calculate the radiological source term, you have to know how much aerosol remains suspended. Whether you postulate a certain amount of core evaporization or you rely on the measurements.

However licensing does it, someone has to, as a function of time, know how much radioactivity is available.

MR. FIRST: I don't have any quarrel with --

MR. KELBER: Let me finish please. That is the function of NSPP tests, to check whether or not we have a satisfactory need for calculating. When we started this work, it was not clear that the codes were conservative.

The way we got started, and it was in fact a request -- I cannot remember whether it came from standards or from NRR. It came -- a request from them to do something about the unsatisfactory state of affairs in this calculation.

This calculation is quite independent of whether the aerosol arises from a postulated source, from an actual source, or the core on the floor, which is my own view, or from a release to the primary vessel.

The other question that was discussed, which is what happens --

MR. CARBON: Excuse me just a second, Charlie. Is he answering your question?

MR. FIRST: No, not at all..

MR. KELBER: There is a complete miscommunication.

MR. KERR: Maybe you should get together at the end.

VOICE: Let's try to do it at the end.

MR. CARBON: Do you want to follow anything further at the moment?

MR. FIRST: No. I would like to get a little more detailed explanation. I assume you would like me to respond to this. I do not feel I have enough information.

MR. CARBON: We would appreciate it very much.

MR. FIRST: Since the meeting is brief, I would hope to get the information later. If that is satisfactory with you and everybody else.

MR. CARBON: Fine. Do you have any more questions at the moment?

MR. KERR: I have no more questions at the moment.

MR. CARBON: Fine. Let's move on to the next speaker then.

MR. GIESEKE: I am Jim Gieseke from Battelle-Columbus. We are talking about aerosol code developments and verification qualification as part of an effort which constitutes our efforts in modelling the fast reactor safety study.

(Slide.)

bfm14

The program can be visualized, I think with this sort of a breakdown that shows the major activities divided between the code development and the code verification part of it.

Code development has gone through some analytical work, development of the HAARM-3 code, which incorporates the characteristics of the aerosols, the morfological parameters such as the mobility correction, and collision cross section that were mentioned in the code.

There is some question about the assumption that is integral to the HAARM-3 code, which is that the aerosol is always of a log normal distribution. Because of that, we have moved on to two additional types of codes: The CRAB code and the QUICK code.

CRAB code used a continuous representation of qualification of nodes. There are two QUICK codes, actually.

There is a histogram type approach to divide the distribution up into sections. So, we have really three different analytical solutions as a way to make sure that the analytics are giving us good answers, or at least comparable answers and consistent answers between the codes.

MR. KERR: You get the same results from all codes?

MR. GIESEKE: In most cases we do. We can find extreme cases of concentrations and so on where there are some differences in the codes in support of the analytical work, and also in support of the Oak Ridge -- the NSPP experiments.

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We are doing some fundamental studies in the properties of aerosols and agglomerates. Materials we are worried about are sodium, sodium oxide, fuel materials, structural materials such as steel and the effects of the environment or the gas in which they exist such as argon, air because of its oxidizing effect on the sodium; and water vapor also reacts with sodium and has some effect on the structure.

So, we are looking at the gases. This is work under way. Code verification, we work thorugh sensitivity analyses. We have gone through some code comparisons, as I mentioned.

We have developed a verification plan which provides a very orderly procedure for going through experimental verification of the codes and selection of the experiments that one would like to have. We are in the process of beginning some comparisons with data.

(Slide.)

To put it in a time frame perspective a little bit better, the first item on the list is sort of a special item. The question has come up regarding mixing in the containment. We have a code that is the ZONE code that divides the containment up into three zones.

Zone one, which is near the source. Zone two which is some sort of a natural convection area. Zone three where you can have deposition on the walls.

(Slide.)

It is arbitrary. It could be applicable for a compartmentalized containment. We were looking, in this case, at a sodium fire situation. We estimated mixing rates that go with different zones.

(Slide.)

We get this sort of a result where you see up to the end of the fire or the time where there is a source in zone one. There are some differences between the different zones, but the mass concentration quickly assumes the same result as you get if you assume it is well mixed from the beginning.

MR. KERR: From that I get the impression it occurs at zero time.

MR. GIESEKE: It is very very fast. It is a little bit of function of how fast you want to assume the flow rates are amongst the zones. It never carries that very far in time. It is very relative to what you would expect for a long -- compared to what you --

MR. KERR: Does that picture have any physical significance?

MR. GIESEKE: Yes. It tells you the well mixed assumption is a good assumption.

MR. KERR: I thought you just said it occurs rapidly, if the flow velocities are rapid.

MR.GIESEKE: There is a slight deviation in here. You are saying it is zero. If you get in there, you can see little bits of differences, maybe.

MR. KERR: I am trying to find out whether you have a nodule in the code.

MR. GIESEKE: Yes. We have changed the mixing rates over a much broader rate than you would expect. It would not make any difference on a graph of this sort that you could see. If you took the numbers, you could see a slight deviation.

So, it is essentially a very very rapid mixing process that it carries on.

(Slide.)

The second item on the list is a comparison amongst the codes. I mentioned we mapped out -- we really tried to find areas where they would disagree as a more severe test.

(Slide.)

Just some examples of a typical source of agreement for sodium aerosol case. In most cases, they tend to agree like that.

(Slide.)

Here is another case where we used UO_2 again. It is quite good agreement. We actually have two QUICK codes that have slightly different assumptions of mass amongst the different channels.

MR. KERR: Are these four distinct codes?

MR. GIESEKE: Yes. HAARM-3 assumes a log normal distribution for the particles. CRAB is the collication of the

bfm18

nodes. QUICK uses the histogram approach. There are two QUICKs -- when you take two size ranges, you can take the smallest of the two size ranges. You can agglomerate those to a bigger particle. You get a different size than if you take the larger end out.

So, that was in a different channel. Basically, the difference between QUICK one and QUICK two is how you smear the ranges in the larger size ranges.

What it does show is that for -- those are typical of most cases. There is one other case. This is the biggest disagreement that we were finding. You see, there is a rather significant difference here, even in this short a time.

It does occur after the concentrations drop off a couple of orders of magnitude. We are still in the process of sorting out the difference. This is a high concentration aerosol when this occurs. The differences are related, not only to the high concentration, but also to the spread of the distributions that you have.

MR. FIRST: Why are you using number concentrations instead of mass?

MR. GIESEKE: We use number concentrations. We use mass concentrations. They all come out of the code.

MR. FIRST: My point is that a difference in number concentration may be an insignificant mass concentration since mass is the primary parameter we are concerned with in terms of releases. Is this concealing something that it should not?

MR. GIESEKE: No.

MR. FIRST: Or revealing something that is unimportant?

MR. GIESEKE: No, there is a difference. I would like to show you all -- I think we have 30 curves like this, but I don't think we have time.

MR. FIRST: Are these all on number, or are some on mass?

MR. GIESEKE: Mass, size, distribution, number. We are preparing them in a report right now that is coming out. All I wanted to illustrate with that one vu-graph was that we can find some places that are really unrealistic sort of conditions. You can assume some conditions where the codes do show some disagreement.

MR. FIRST: May I ask another question on this disagreement part which I think that really gets at the essence of it?

None of these numbers have any confidence intervals associated with -- none of these curves have confidence intervals associated with them?

MR. GIESEKE: They are all calculated.

MR. FIRST: There are some uncertainties in the calculations, shall we say? Certainly, there are some uncertainties in the data from which the codes were derived.

MR. GIESEKE: The codes assume a source and some size that goes in there. All of them take the same thing.

MR. FIRST: I understand that.

MR. GIESEKE: It has been set up -- just the question of, for instance, time intervals. We worked it that to where -- beyond any change in results with time intervals.

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MR. FIRST: That is not the question I am asking. The question I am asking basically islooking at your last slide before this where you said you had some significant deviations at some points on the curve. My question is, are these really deviations of significance, or are the error limits of each of these codes such that this is really telling you the same thing?

MR. GIESEKE: I am not sure I understand. The code results -- I could go to 1 millionth of the time interval.

MR. KERR: We will accept as an answer, I have not thought about your question before. I will provide you an answer.

MR. GIESEKE: Okay. Thank you for an answer to the question. I think there was another item.

(Slide.)

We are in the process, as I mentioned, of looking at the properties of mixed aerosols. I think you have seen the difference from Tom Kress of the different types of particles on airborn mass concentration.

This is the same scale sodium oxide aerosol down at the corner relative to UO_2 aerosol. I think it illustrates the difference in the particles.

MR. KERR: Those are the same scale?

nd bfm
c flws

sc 1
fls bm
NRC
Parker
Tape 7
A[illegible]S
7-9

MR. GIESEKE: Yes. The primary sites are much smaller for the UO_2, and the shape of the agglomerate is much different.

(Slide.)

We are in the process of measuring the properties, not only of the individual components, which we have done, but we are in the mixed materials right now.

Another effort, as I mentioned, was the verification plan. We are busily trying to get some consensus among the people working in the area.

As I mentioned, the plan was prepared, and it has been out for comments on an informal basis. We have a meeting scheduled for later this month where we are going to go over our verification plan.

(Slide.)

Just very quickly, we are working down to this sort of an outline. We have gone through sensitivity analyses with the codes. We have tried to decide what sort of agreement the code should have. With experiments we have tried to set ranges for the variables of interest and select ranges for the validation experiments.

Now, the way we selected the ranges for the validation experiments --

MR. FIRST: I do not have that in my folder here.

MR. GIESEKE: I think you have this.

(Slide.)

The shaded areas represent assumed accident conditions plotted with dimensionalist groups representing wall deposition relative to gravitational loss and coagulation relative to gravitation or sedimentation loss, to map out areas where the accidents or assumed accidents are likely to occur and to tell us where to run experiments so we can match it up with the accident.

There are some other considerations, I think in your handout, that are sort of outside this sort of an approach.

(Slide.)

This is based on the controlling mechanisms. Spatial inhomogeneity, as I mentioned, we have been covering with the zone code, and also there is some input from experiments on that, large-scale experiments such as at HEDL, the interaction rates for mixed aerosols; that is basically the NSPP experiments, items 3 and 4. As I mentioned, those are coming out of our experiments at Battelle as well as the NSPP experiments.

Localized thermal effects I think we can handle with a code, but there have also been some experiments in the Netherlands directed towards that question; that is, if you have a hot spot on the floor where you might have some thermal fretting repulsion, that sort of a mechanism is included in the code.

Possible particle heatup or charging because of the specific activity of the material -- this has been evaluated analytically and some conditions mapped out for it. It may or may not be of any importance.

And the question of resuspension has been approached and limited -- from limited, small-scale experiments, as we have done it at Battelle.

(Slide.)

I think the only other thing I would want to do is say where we should get by the end of the year, and other items that are planned or anticipated. We would like to be or hope to be completing our comparisons among the codes, the mapping out of the regions and trying to understand the regions where the codes are in some degree of difference.

The other -- the next item would be we are working towards an improvement of the QUICK code in this case to handle the mixed materials or mixed aerosols a little more adequately. And we are beginning now to do work on comparing experimental results with our verification criteria to see what additional experiments may be needed.

Other efforts that I think are down the line that need to be completed in this whole area, as I mentioned, are improvements, the code to handle the mixed aerosols, that will continue beyond this fiscal year.

(Slide.)

In addition to that, the extension of our measurements on agglomerate properties, both the effects of the materials and of the gases such as water vapor, air, and the efforts toward code verification, as more experiments are completed, and we put them all together to see how the codes do at predicting the experiments.

That completes what I have to say. Do you have any questions, any further questions?

MR. FIRST: What are the criteria for a successful clarification of the code?

MR. GIESEKE: We are trying to set that in terms of -- we arbitrarily have chosen two things that we think are of importance. The first is what people really need in doing assessments of radiological consequences of accidents: the mass of material that has leaked from the containment vessel, and since the timing of a release may be of some question, maybe if there is any containment failure what you really need is sort of two things -- an integrated mass leak as well as an airborne concentration at the time of an opening in the containment, if you want to assume that would occur.

MR. FIRST: I know what the function of it is. What are your criteria for verifying the code? Supposing at the end of your work I said is this code verified or not? How are you going to judge this? That is the question.

MR. GIESEKE: I was trying to get at what parameters you are going to zero in on and use as your index. The first thing, airborne mass concentration, is one parameter that you want to predict.

MR. FIRST: How closely is the question. I am asking you a quantitative question and getting a qualitative answer.

MR. GIESEKE: Let me go one step further. The other parameter of importance we feel, because it leads to understanding of the processes involved, is the particle size.

Now, to answer your quantitative question, after discussions with people that use the codes and expectations or uncertainties in dispersion calculations and that sort of thing, we feel it would be more than adequate -- and the number is not tied down, but we feel the code should predict within a factor of two on the conservative side -- if you do an experiment, the code should be -- the experimental results should be a factor of two higher on the mass concentrations. On particle size I think the code should calculate plus or minus one and a half times the mean diameter.

Those items relate to what the users of the code need in terms of analyzing the whole consequences question.

MR. KERR: Why do you think it has to be a factor of two higher rather than a factor of two lower, for example?

MR. GIESEKE: Maybe it does not have to be. We are still struggling with these particular --

MR. KERR: I am not being critical. I am just asking why.

MR. GIESEKE: I think it should be conservative. I think it should predict on the high side.

MR. KERR: As long as you know it is off by a factor of two why does it matter if it is on the high side or the low side, as long as you know what it is?

MR. GIESEKE: That is okay with me.

MR. KERR: I am not saying that is the way it should be. I am asking why it is you picked the high side as desirable.

MR. GIESEKE: My feeling is the code should be on the .servative side.

MR. KERR: But the code cannot be either conservative or non-conservative. People can be.

MR. GIESEKE: The code can make a prediction in that it overpredicts --

MR. KERR: The code can make a prediction either accurate or inaccurate. What you do with it determines whether the results are used to make a conservative design.

It seems to me -- from my point of view I would want the code to make as accurate a prediction as I could. Then I can introduce a conservatism in the design or whatever I do with it. There may be something I'm missing that says the code should always be conservative.

MR. KELBER: I think, Bill, you are absolutely right. In a way we should view this as a kind of best estimate code. Obviously, if we know, for example, that this code is within a factor of two, even if we do not know whether it is high or low, the regulatory people can use that knowledge in making their assessment. You're absolutely right. No question at all.

MR. KERR: I was not really trying to make a statement, because I have not thought about this.

MR. KELBER: I think as Jim has pointed out, they are struggling. At the end of this month they are going to try to come to some final criteria.

MR. GIESEKE: I would like for the licensing people to tell me what sort of reliability or predictability they want in the code.

MR. KERR: That seems reasonable.

MR. GIESEKE: They will not answer that question.

MR. FIRST: I think that gets basically to the question I was trying to get you to answer, and that is, if you do not have any criteria for validation, how are you going to do it?

MR. GIESEKE: We are setting the criteria.

MR. FIRST: But who?

MR. GIESEKE: I have said them. He disagrees.

MR. FIRST: Nobody is disagreeing with you.

MR. KELBER: Just made an observation.

MR. FIRST: Who should set the criteria? Should this be your function? Should it be NRC's function, whose? And why have we gone 25 years with these models and nobody has set a criteria for validation yet?

MR. KELBER: In the first place, validation is being carried out in a range of activities, in the water reactor program particularly.

MR. FIRST: Are there criteria for that validation?

MR. KELBER: They are oftentimes home-grown. The primary mechanism by which we do it is through the research review group, and quite frankly, it is a function of research at this time by default to make the major judgments as to how accurate the code should be.

Where the final -- where push finally comes to shove is when the code is used in a safety assessment, and someone decides how much safety factor they have to put on the calculated results.

MR. FIRST: There is no way of knowing that without knowing what the reliability of the code is.

MR. KELBER: NRR has yet to face up to this issue.

MR. CARBON: Go ahead.

MR. FIRST: I will try and get some more information on it later.

MR. KERR: Is there someone who could tell us -- maybe you already know what the relationship between his work and the Oak Ridge work. If you already understand it --

MR. CARBON: No, I do not. Would you tell us what the relationship between this work and the preceding Oak Ridge work is?

VOICE: This work has been -- that Jim has described has been aimed at the analytical models used for making predictions of aerosols within containment under accident conditions as a function of time. The fundamental properties -- some of the fundamental properties that go into the code, namely as Jim described on some of the mixed aerosols he has been measuring at Battelle-Columbus, the NSPP work that Tom Kress described is checking in a reasonable scale whether or not the predictions that one would make -- whether or not the analytical methods do represent the -- what is seen in the NSPP for aerosol mixtures.

Now, let me say that in the past there have been tests with sodium oxide and some tests with the UO_2 but in much lower concentrations. And the NSPP has in this nominal test matrix combined the two together, and the two pieces of work will now come together.

Now, unfortunately Jim's work was delayed by five months as explained -- this fiscal year, as I explained in my remarks on June 13 -- in some of the things he was doing on property measurements to keep in phase with the pace of the Oak Ridge work, but his five-month delay in his contract has given him that much delay, so there is some -- he is having to catch up now with -- to put the two together.

MR. KERR: Could you explain that to your class?

VOICE: One program is analytical with some properties --

MR. CARBON: I could make a fair effort, I guess.

MR. KERR: Thank you.

MR. CARBON: Let's go on then to the next discussion.

(Slide.)

MR. GINSBURG: My name is Ted Ginsburg. I am from Brookhaven National Laboratory. I would like to present to you Brookhaven's experimental program.

(Slide.)

In today's discussion, after an overview including the scope of Brookhaven's work, I would like to discuss with you very briefly some of the recent results we have obtained from some of our programs, and then to indicate within the budgetary uncertainties where we are heading.

(Slide.)

This gives you an indication of the scope of the charter for BNL's activities in the area of the fast reactor safety experiments. We are basically charged to investigate various kinds of thermal hydraulic phenomena of importance in fast breeder reactor safety analysis and to apply the phenomenological principles to accident analysis.

(Slide.)

The scope of our activities, as you can see here, vary from experimental simulations of phenomena related to post-accident heat removal, transition phase of the LASL flow accident initiator, and post-disassembly bubble expansion.

In addition, this year we undertook a task involving an assessment of transition phase technology.

MR. CARBON: Hold up on that item. Tell me what the goal of this work is. Where does it fit in the picture? Where does it fit in the NRC picture?

MR. GINSBURG: These experimental simulations are related to safety considerations involved in hypothetical core disruptive accidents. Basically we are asking in these various kinds of simluations how can we obtain information relevant to various processes occurring core disruptive accidents. For example, what happens following a loss of flow accident in an LFMBR, sodium boiling, fuel melting, how do we characterize the resulting processes?

MR. KELBER: This work is related to the SIMMER studies of the transition phase. You may recall during the LASL exposition last week some reference to the Brookhaven work on the transition phase. This is the work being referred to.

MR. KERR: Does this have any relationship to the Sandia work?

MR. KELBER: Yes. This is out of pile. Sandia is in pile. Sandia is designed to test as best one can the various -- what is called their multi-role -- these folks have done a good job in --

MR. CARBON: The goal of this work and the Sandia work is basically the same.

MR. KELBER: They are related to the understanding of the transition phase, that is correct.

MR. CARBON: Out of pile. Sandia is in pile. Both of which are aimed as input to S1 'MER.

MR. KELBER: That is correct. Input or correction.

MR. CARBON: Presumably a basic understanding.

MR. KELBER: That is correct.

MR. GINSBURG: What I would like to discuss with you today is a task that we spent some good amount of time on during the past year, and that is in transition phase assessment.

(Slide.)

Very briefly, however, I provided you with basically a list of major BNL accomplishments during the past year for each one of the tasks that we saw on the previous page. Again, we do not have time to go into each one in detail. I would like to stress the assessment of transition phase technology.

MR. KERR: Mr. Ginsburg, would you tell me briefly what you mean by the transition phase?

MR. GINSBURG: I define the transition phase as that phase of an HCDA initiated by a loss of flow without scram, that period of the accident following fuel disruption during which the core material is molten and is contained within the original boundaries of the core -- of the initial design core. So we have a mass of molten fluid existing with various processes going on within the original confines of the core volume.

MR. KERR: Everything is molten now. There are no solid materials.

MR. GINSBURG: It is not clear.

MR. KERR: I am not trying to put words in your mouth. I am trying to understand what you mean by the transition phase.

MR. GINSBURG: The core proceeds following fuel disruption to a fully molten configuration. During any time slot there may be a small fraction of the core still solid, but it is heading towards a full core melt.

MR. KERR: People refer, I have discovered, to different things when they say transition phase. I want to know what you mean. It is after some major fraction of the core has become molten.

MR. GINSBURG: The core is undergoing a sequence -- the core is being disrupted.

MR. KERR: Yes, sir.

MR. GINSBURG: It takes a finite time to go from a fully solid configuration to a fully molten configuration. In the work that we have done we have considered the transition phase to be that portion of the accident in which most of the core is in a molten configuration. We have not in the work we have done made fine distinctions about when we should go from what I call the fuel disruption part of the accident to a transition phase.

The point is that most of the core is in a molten configuration.

MR. KERR: Say 95 percent of it? I am trying to find out what you mean by "most."

MR. GINSBURG: In the work we have done we have not pinpointed --

MR. KERR: Five percent?

MR. GINSBURG: We have not defined that. In order to define --

MR. KERR: I don't want to be difficult, but it seems to me there is a tremendous difference between a core in which 5 percent is molten and 95 percent is molten.

MR. GINSBURG: In order to go into those kinds of detail to describe the progression from fully solid to fully molten, we need to have an accident analysis such as SIMMER. We did not use SIMMER in our analysis, and therefore, we tried to scope out what we did in terms of broad categories.

We looked at the fuel disruption mode. We looked at the fully molten configuration, and we said for the purposes of our analysis we had to consider a fully molten core. That is what we did.

MR. KERR: Okay. So you looked at the fully molten core.

MR. GINSBURG: We looked at a fully molten core.

MR. KERR: I don't want anything except to know what you are talking about.

MR. GINSBURG: That is what we did in the analysis. We considered the fully molten core. We completed our assessment of the transition phase accident sequence. What we did was to scope out accident sequence paths and to assess the adequacy of current understanding of what we feel are major phenomena.

What we found out as a result of our analysis were the major results -- they were that first of all a concept of fuel dispersal does not rule out the potential for recriticality during the transition phase. And we found that the accident is likely to progress from a -- to a -- to a configuration of corewide fuel motion coherency. I will describe that in a moment.

(Slide.)

Okay. So I will describe some recent results. I would like to stress -- focus my attention on our assessment of the transition phase phenomenology and brief discussion of these remaining tasks to perhaps questions after my presentation.

(Slide.)

The scope of our assessment of transition phase technology was first of all to review previous related work directed toward the transition phase, to assess the state of the art of various phenomena, important phenomena related to the transition phase, to scope out what we feel are reasonable accident sequences for the transition phase, and on the basis of all that work to focus in on research needs and priorities.

The work that we did is primarily related to oxide fuel element FBR and homogenous core LMFBRs.

MR. CARBON: Your work meshes closely with the SIMMER work. Doesn't SIMMER define "transition" totally differently?

MR. KELBER: No, not really differently. The SIMMER code starts with the core at equilibrium and traces it all the way down. Computationally most -- it usually starts at some point when a significant amount of damage has been done. This is the transition from SAS.

The reason it is called a transition phase is this is the phase of the accident in which the core is making a transition from a badly damaged state but where it is still largely solid and largely in its original geometry, to one in which it is fully or nearly fully molten. And either it goes subcritical by dispersal of fuel in massive amounts, or it goes through recriticality.

Now, the definition given to you was, I think, more restrictive than the work that is concerned. The point of the definition given to you earlier today was that the phenomenology that are being looked at are the -- the phenomena being looked at are the phenomena associated with the molten portion of the fuel.

MR. CARBON: Okay. I really do not care much what words are given to this in one sense. I am just concerned that they are not being mismatched between what they are doing at Brookhaven and --

MR. KELBER: Let me say quite frankly and plainly, we do have a problem with parochialism among contractors. It is a serious problem with us, and it extends not only to us but to other places, too.

I think it is incumbent on our project managers in the various organizations, a) to avoid the NIH syndrome and b) to be more cognizant of the work of others. We really knock heads together in the review groups.

MR. CARBON: You can also take the contract away from someone.

MR. KELBER: I hate to do that when good work is being done, but it is a problem. We are not the only ones who face it. We are not the only ones who face it, but it is a problem which we continue to have; and that is that our contractors do not take adequate cognizance of the work by others.

MR. CARBON: Did you want to say something?

MR. KERR: I wanted to say with all due respect to Mr. Kelber, my answer to your question would have been yes, it is different. I don't think there is anything wrong with that as long as both groups know what they are talking about.

MR. CARBON: If they know what they are talking about, it is quite all right.

MR. KELBER: We did bump their heads together at a review group, so at least they know what each other is talking about.

MR. CARBON: Okay. Go ahead.

MR. GINSBURG: In our assessment of major transition phase phenomenology we chose to address ourselves to phenomena which we think -- which we thought -- think had a major impact on the course of the transition phase, and those areas which we concentrated on were the areas of plant dynamics, boiling heat transfer, boiling hydrodynamics, and fuel motion and freezing phenomena.

In the assessment work that we did we attempted to look at the models and experiments available for each one of these areas and to assess the state of the art in each one of these areas; and I have summarized them on this slide.

We feel that perhaps the most important area of phenomenology with respect to the transition phase is with respect to fuel motion and freezing. The state of the art, as we see it, for that area of phenomenology is that we feel that the available evidence seems to indicate that fuel will penetrate -- that fuel penetration into the axial blankets following fuel disruption is limited by freezing of the fuel on the available structural materials.

The freezing mechanisms are still not well understood. However, there are limiting models available with which one can at least bound the observations of freezing rates observed in experiments.

What we feel is necessary to be done in the future are to do prototypic tests to verify the penetrations observed in prior experiments, and these experiments are being planned and implemented at Sandia Laboratories.

MR. KERR: What is meant by saying that the freezing mechanism is uncertain? I would have thought the stuff froze because it got cold.

MR. GINSBURG: The question is how does it get cold and how rapidly does it get cold.

MR. KERR: It gets cold because heat is transferred out of it, doesn't it?

MR. GINSBURG: The fuel material, the multi-phase fuel material is ejected from the molten core region into axial blanket structure, which is indeed coal. The structural material represents the heat sink, okay?

Available evidence seems to indicate that the freezing rates fall between two limits conceptually -- one in which the molten clad material is postulated to stay fixed on the structural material, and a fuel crust forms on that structural material. Fuel crust limits the heat transfer, insulates the rest of the flow from the heat transfer, and freezing rates are low.

The other limiting case is one in which it is assumed that the fuel crust does not form, that there is a large temperature difference available for heat transfer, and that the heat transfer rates are large and freezing rates are very rapid. The available evidence seems to fall between those limits.

MR. KERR: Thank you.

(Slide.)

MR. GINSBURG: In our assessment of the transition phase accident sequences this is the approach that we took. We assumed that the loss of flow initiated would indeed enter a transition phase rather than go directly into a hydrodynamic disassembly, and we attempted to scope the behavior of the molten core configuration on the basis of a single channel, and we attempted to identify mechanisms which would lead to perhaps recriticality events.

We attempted to reassess prior arguments related to fuel freezing -- fuel and steel freezing and their impacts on the transition phase and also to reassess prior fuel dispersion arguments and their impacts on the course of the transition phase.

MR. KERR: Did you find any major errors or major discrepancies in your view between what had been done previously and what your conclusions were?

MR. GINSBURG: Yes, we did. I will point out a couple of them right now.

(Slide.)

First of all, to get straight that question, the second item, we found that the transition phase recriticality events cannot be ruled out on the basis of fuel dispersal arguments. It had been previously proposed in CRBR licensing issues that fuel dispersal would indeed terminate the accident early by transporting fuel from the molten core directly to the sodium plena and thereby lead to permanent subcritical configuration.

We found on the basis of our work, previous experiments, our analysis of the phenomena, that the major retarding effect is the formation of fuel and steel blockages at the axial extremities of the core damage, and that this would limit very early the flow of molten fuel away from the core region. This was a major finding of the study, one in which we -- there is a major discrepancy between previous work and --

MR. KERR: That is an area of uncertainty, or you are sure freezing will occur, and it will prevent dispersal?

MR. GINSBURG: The available evidence, as we see it now, seems to indicate that this fuel freezing will occur. This evidence is based primarily on relatively -- on a series of experiments done at Argonne National Laboratories. We feel the tests need to be verified in more controlled experiments, and these experiments are being planned by ARSR at Sandia Laboratory. This is the way it appears to us today on the basis of the experiments.

Entrapped molten pools existing within the core region does seem to be a likely configuration, and they are likely, according to our analysis, to grow to a whole core scale before blowdown to the sodium plena, which means you go from subassembly pools to large-scale pools before you can eject much of the fuel to a permanent subcritical state. And due to the fact that you do develop large-scale pools, you have to worry about possible coherent core-wide fuel motion during the whole core stage. And you have to worry about recriticality, which we did not do but which the SIMMER code is capable of doing.

So the fuel dispersion process is limited by predominantly the geometry imposed by the axial blockages and by other effects which we have also identified.

(Slide.)

MR. CARBON: We are going to have to close off in about five minutes.

MR. GINSBURG: Okay, fine.

MR. KELBER: While Ted is looking for his slide, let me say the picture he has just given you appears to be a developing consensus. We just received a report from HEDL sponsored by DOE that comes to similar type conclusions based on their work. SIMMER said essentially the same thing.

MR. KERR: What I heard from SIMMER was SIMMER probably treated the plugging very poorly.

MR. KELBER: Yes.

MR. KERR: And they knew that had an influence on the progression of the accident, but they had no idea where the plugging occurred.

MR. KELBER: That is the importance of this work, as well as the forthcoming DOE tests that are now being planned, the RX tests.

MR. GINSBURG: One of the affirmative recommendations that we made out of the work that we did related to the transition phase was that verification of the thermite test results with more controlled systems are indeed called for. The fuel freezing process we feel is a dominant consideration in the transition phase, and even though we have some information, at the moment we need verification.

What I have outlined here --

MR. KERR: You are an experimentalist?

MR. GINSBURG: Yes.

MR. KERR: In your view are there experiments that are likely to give one a good answer to the question of freezing blockage that can be generalized?

MR. GINSBURG: I believe that the available -- that the available evidence, together with the information that is going to come out of the Sandia tests, will give us a larger degree of confidence than we have at the moment.

That does not give you a definite yes or no, but that is my opinion.

MR. KERR. That is enough. I would say that is a good answer.

MR. GINSBURG: This indicates the direction that we see for the future. Of course, we have budgetary concerns, as does everyone else. The assessment report is complete, and we do intend to issue a final report.

The remaining -- this gives you an indication of where we are heading in the remaining tasks that I did not have time to talk with you about today.

Any questions?

MR. CARBON: Anything further?

MR. KERR: I have no further questions.

MR. CARBON: I believe not. We thank you. I thank you all.

I will adjourn the meeting in a moment, but we would like to continue with Dr. First's questions and everything.

The meeting is adjourned.

(Whereupon, at 12:50 p.m., the meeting was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
SUBC. ON ADVANCED REACTORS

Date of Proceeding: July 9, 1980

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.

David S. Parker

Official Reporter (Typed)

Official Reporter (Signature)

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I feel these are adequately documented.

(Slide.)

MR. GUPPY: In the vessel, there is what we call a multichannel capability. These are one-dimensional parallel channels. The user can specify any number of these and build it as shown here. There are five axial sections available for the user, any one of which may or may not be there at the user's specification, such that with this type of arrangement, we feel that any type of fuel assembly, blanket assembly, control rod assembly can be simulated adequately.

We have available the various material properties that the user can call upon to use, or else there is the additional flexibility such that if he wants to put in his own material type, he can.

Again, there is -- you can divide these into any number of axial modes that you want, and radially there is definition available also.

(Slide.)

MR. GUPPY: With that as a little bit of an overview, I want to go into some of the results -- some of the results -- Some are for the FFTF transients, which are in some aspects -- In some aspects, they can be called a validation effort. The ones I will present today are more for intercode comparisons, as opposed to experimental

REACTOR SAFETY MODELING AND ASSESSMENT

PRINCIPAL ACTIVITIES

MODELING

- EPIC CODE FOR FUEL AND SODIUM MOTION
- BIFLO CODE FOR SODIUM BOILING AND VOIDING (DEVELOPMENT AND BENCHMARKING)

COOPERATIVE STUDIES

- UK/NRC BILATERAL PROGRAM
- WAC GROUP STUDIES

ASSESSMENT ACTIVITIES

- LARGE REACTOR STUDIES
- FUEL PIN FAILURE STUDIES

MODELING ACTIVITES ARE RELATED TO INITIATING PHASE OF WHOLE-CORE ACCIDENTS AND TO DEFINING INITIAL CONDITIONS FOR TRANSITION PHASE.

BIFLO ADDRESSES PROBLEM OF RADIAL TEMPERATURE VARIATION IN LMFBR SUBASSEMBLIES, WHICH LEADS TO INCOHERENCE IN BOILING AND VOIDING.

IT IS EXPECTED THAT BIFLO WILL BE INCORPORATED INTO SAS4A.

CALCULATION OF INTRASUBASSEMBLY RADIAL TEMPERATURE VARIATION EFFECTS ON CLAD AND FUEL MOTION BEING CONSIDERED AS NEXT STEP.

INCOHERENCE IN VOIDING IS LIKELY TO BE MORE IMPORTANT FOR HETEROGENEOUS LMFBR DESIGNS THAN FOR CORES WITH LARGE POSITIVE SODIUM VOID EFFECTS, IN WHICH VOIDING PROPAGATION AND FLOW REVERSAL WILL OCCUR MORE QUICKLY. A CAREFUL TREATMENT OF CLAD MOTION IS ALSO MORE IMPORTANT FOR A HETEROGENEOUS DESIGN.

EPIC AND SAS/EPIC STATUS

SOME MODEL INPROVEMENTS RECENTLY MADE AND CODING CLEANED UP TO PREPARE CODES FOR EXPORT.

EPIC USERS MANUAL COMPLETED.

EPIC CURRENTLY USED BY UK AND KFK; WILL BE USED BY JRC AT ISPRA. SANDIA HAS USED EPIC FOR EXPERIMENT ANALYSIS.

SAS/EPIC BEING SENT TO KFK AND JRC.

FURTHER DEVELOPMENTS BEING CONSIDERED ARE ADDITION OF PLATEOUT AND PLUGGING MODELING, IMPROVEMENT IN AXIAL MOTION DISASSEMBLY MODEL, AND MECHANISTIC CALCULATION OF CLAD RIP EXTENSION.

UK/NRC BILATERAL PROGRAM

- REPORT ON CURRENT PHASE OF PROGRAM, INVOLVING COMPARISON OF RESPECTIVE WHOLE-CORE ACCIDENT CODES (SAS/EPIC AND FRAX-2) IS ESSENTIALLY COMPLETE.

- CHIEF VALUE TO US SO FAR HAS BEEN IN AREA OF FUEL PIN FAILURE CONDITIONS. COMPARISON OF SAS AND FRAX BOILING MODELS ALSO OF INTEREST.

- UK INTERESTED IN CONTINUING PROGRAM. NEXT PHASE IS TO FOCUS ON STUDY OF INDIVIDUAL PHENOMENA PENDING FURTHER DEVELOPMENT OF WHOLE-CORE ACCIDENT CODES. IMMEDIATE EMPHASIS ON

    1. SODIUM BOILING - INVOLVING BIFLO AND COMMIX DEVELOPMENT ON USNRC SIDE AND SABRE DEVELOPMENT ON UK SIDE.

    2. FUEL PIN MECHANICS AND CLAD FAILURE - INVOLVING WORK AT ANL, LASL, AND SANDIA ON US SIDE AND WORK AT HARWELL AND WINFRITH ON UK SIDE.

WAC STUDIES

- 10¢/SEC TOP CALCULATION ON IRRADIATED CORE COMPLETED. DEFINITION OF LOF CALCULATION EXPECTED AT NEXT MEETING IN NOVEMBER.
- COMPARISON OF CALCULATIONS OF STEADY STATE FUELS CHARACTERIZATION AMONG UK (FRUMP), BELGONUCLAIRE (COMETHE) AND US (SAS4A) OF CONSIDERABLE INTEREST.

ASSESSMENT ACTIVITIES

- LARGE REACTOR STUDIES

A REVIEW OF AVAILABLE SAS CALCULATIONS ON A RANGE OF CORE DESIGNS INDICATED THAT A SODIUM VOID WORTH IN THE RANGE OF $2-$3 IS A REASONABLE TARGET FOR HETEROGENEOUS CORE DESIGN. CLAD MOTION EFFECTS IN HETEROGENEOUS DESIGNS NEED ADDITIONAL STUDY AS THEY MIGHT CONTRIBUTE TO LOF-TOP POTENTIAL.

PARAMETER STUDIES ON WARD CDS DESIGN ARE BEING STARTED.

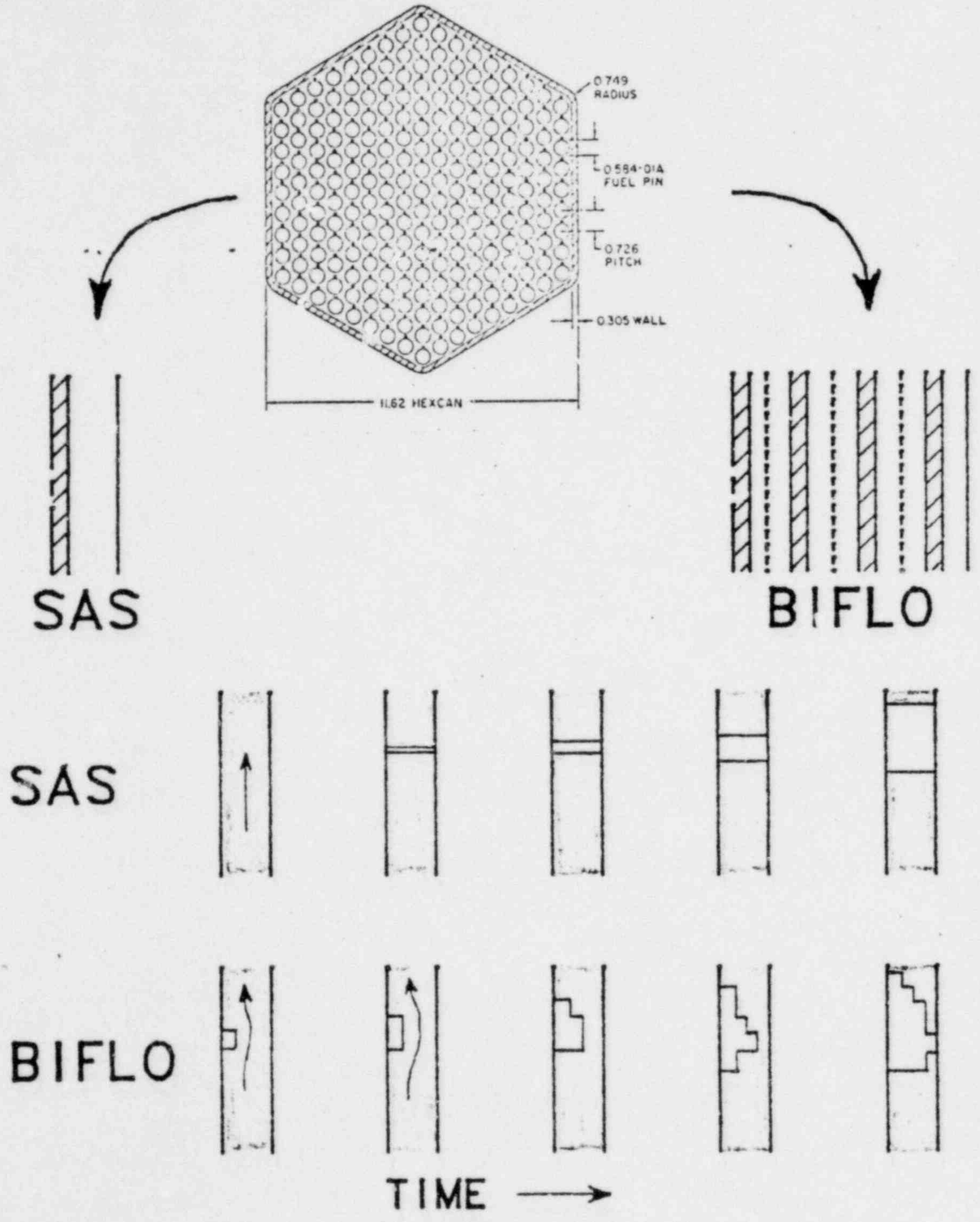
DEPENDENCE OF INLET PLENUM PRESSURE RISE DURING SODIUM BOILING ON ELASTICITY AND COMPRESSIBILITY EFFECTS AND ON LOOP VS POOL DESIGN STUDIED.

- FUEL PIN FAILURE STUDIES

A REVIEW AND COMPARISON OF AVAILABLE MODELS FOR FISSION GAS RELEASE FROM SOLID FUEL IS UNDER WAY. THIS REVIEW LED TO THE DECISION TO INCORPORATE THE NEFIG (UK) MODEL INTO SAS/EPIC.

THEORETICAL AND EXPERIMENTAL RESULTS NOW BECOMING AVAILABLE ON FISSION GAS RELEASE FROM SOLID FUEL WILL BE HELPFUL IN SPECIFYING PARAMETER STUDIES OF FISSION GAS EFFECTS, ALTHOUGH UNCERTAINTIES ARE STILL LARGE. UNCERTAINTY IN FISSION GAS RELEASE FROM OR PRESSURIZATION OF MOLTEN FUEL IMPORTANT IN LOF-TOP EVENTS, WHICH HAVE MILLISECOND TIME SCALE.

SUBASSEMBLY MODELING



BIFLO BENCHMARKING

BIFLO vs. EXPERIMENTS

- THORS
  - 19 & 61 PINS
  - OUT OF PILE
- SLSF
  - 19 & 37 PINS
  - IN PILE
- EUROPEAN

BIFLO vs. CODES

- COMMIX
- COBRA-IV
- SAS

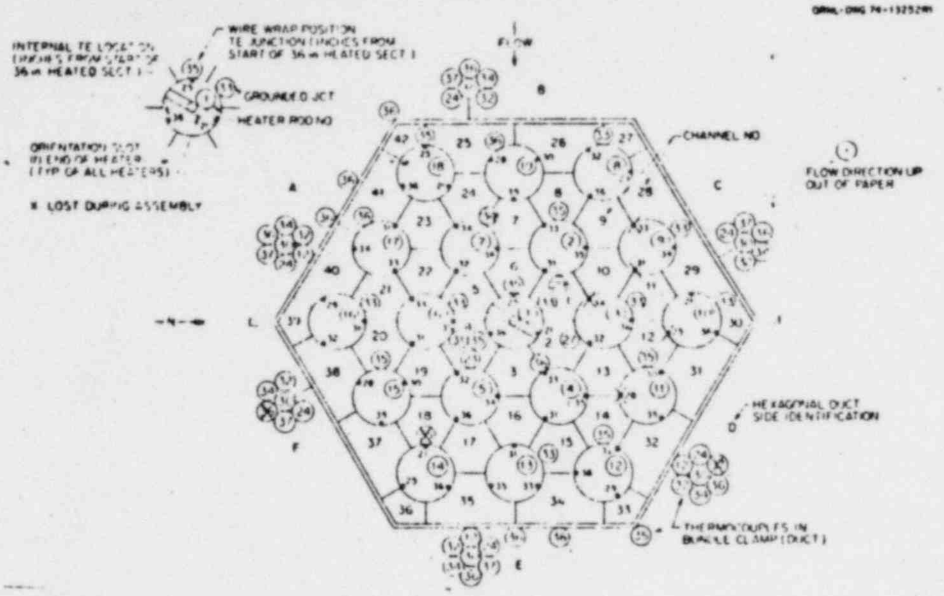
SUB-CHANNEL CODES vs. EXPERIMENTS

- MIXING BETWEEN CHANNELS
- AID IN EXPERIMENT INTERPRETATION

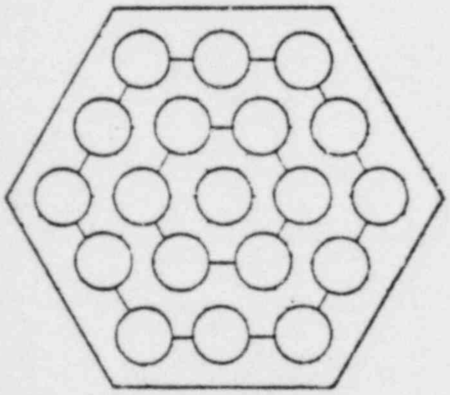
THORS FACILITY
BUNDLE 6A

EXPERIMENT

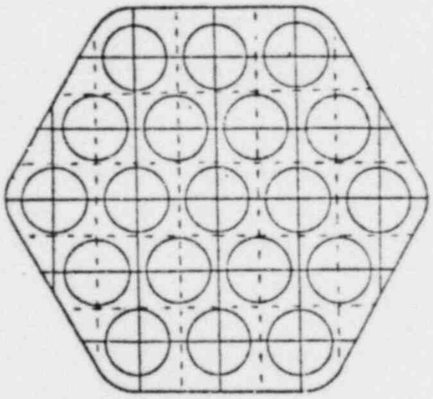
19 PINS (0.91 m HEATED, 1.21 m PLENUM)
100 THERMOCOUPLES



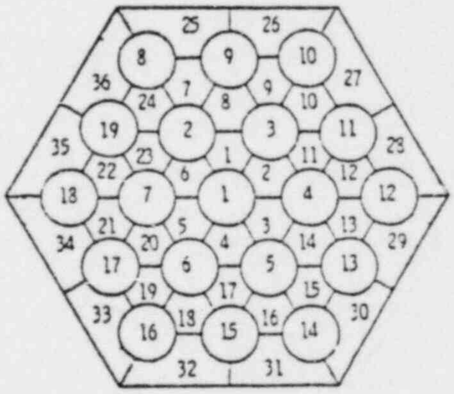
MODELING



BIFLO
3 PINS
3 CHNLS



COMMIX
19 PINS
32–92 CHNLS



COBRA
19 PINS
36–42 CHNLS

BIFLO STATUS

- INITIAL VERSION IS RUNNING
  (stand-alone code)
- BENCHMARKING IN PROGRESS
  (19-pin level)
- <u>FUTURE INVESTIGATIONS</u>:
  - ASSESS MODELING ASSUMPTIONS
  - NUMBER & ASSIGNMENT OF REGIONS
  - BOILING/NON-BOILING COUPLING
  - EFFICIENCY OF BOILING COMPUTATION
  - IMPLEMENT IN SAS4A
  - IMPACT OF 2-D TREATMENT ON ACCIDENTS
  - GENERALIZE GEOMETRY FOR ASYMMETRIES
  - BENCHMARKING FOR LARGE BUNDLES

Benefit to Our Program from Cooperative Studies with Europeans

The exchange of views on fast reactor safety is helpful. The European views are often based on different experience and experimental data (sometimes not available here) as well as on differences in philosophy. The Europeans have their own safety experimental programs which are not necessarily duplicates of the U.S. programs and they are in the process of actually building LMFBR's.

During the course of the calculations, certain effects which had been neglected in our modeling were shown to be important in certain circumstances (e.g. aspects of the thermodynamics of sodium and single-phase regions in the coolant channel separated by a boiling zone).

It was most helpful to compare the results of calculations when very different modeling assumptions were made. This provided greater understanding in areas such as disassembly modeling where our 1-D calculation could be compared to 2-D calculations used by others - other such areas are pin failure modeling, sodium boiling and fuel characterization. The European work in fuel characterization is of considerable interest to us. This is an important area for safety in that transient pin failure is strongly affected by the initial conditions thereby defined.

Different design features of European plants (bottom f.g. plenum, low smear-density pins, etc.) provide a new perspective on our reactor design from the point of view of reactor safety.

Results from Initial Phase of UK/NRC Bilateral Program

In the initial phase of the UK/NRC program, comparison of the SAS boiling model with the simpler one in FRAX showed that the latter gave somewhat higher boiling voiding ramp rates (up to ~$20/sec vs $10-$15 for SAS). Voiding patterns were somewhat different, although LOF-TOP potential was roughly the same. Calculations with SAS/EPIC in general gave more benign results than those with FRAX-2 because of the negative reactivity effect contributed by fuel motion, not taken into account in the UK calculation, for the above core-centerline pin failures assumed in the calculation.

Interesting points of discussion with the UK in the area of fuel pin failure have been the following:

1. Possibility of fuel pin failure by fuel-clad mechanical interaction. Calculations with the FRUMP (UK) code have indicated that this is unlikely because the fuel will soften before enough clad strain occurs to cause failure. US FFIN calculations using irradiated clad properties reflecting the fuel adjacency effect do indicate possible failures of this type. However, the calculated failure times for TREAT TOP experiments are early compared to those measured. A stronger clad must be assumed to match the observed failure terms, and under these conditions the fuel melt fraction is high enough that the fuel softens and failure occurs by fission gas pressure, in agreement with the UK experience.

2. Possibility of clad meltthrough. Based on Petten experiments, the UK incorporates in FRAX-2 a criterion that clad meltthrough can occur at a combination of at least 0.80 fuel melt fractin and 1280°K clad temperature. This was a new idea for us and helped justify use of a fuel melt fraction clad failure criterion, particularly for higher fuel melt fractions.

3. Equilibration of fission gas pressure in a slow TOP. In the FRESS model we make the same assumption as the UK regarding radial pressure equilibration, that all void space and fuel porosity is available to fission gas. The UK assumption of axial pressure equilibration over the core in a slow TOP seemed reasonable to us and was adopted in our calculations for the bilateral program. The situation is less clear for a more rapid TOP or an LOF-TOP.

Continuing contact with the UK in the areas of fuel pin failure and sodium boiling will be helpful to us particularly because of the UK research programs in these areas being carried out at Harwell and Winfrith respectively.

LOF-TOP Potential in Large LMFBR's

A review of SAS calculations on LMFBR's with a considerable range of sizes and reactivity coefficients has been carried out to assess the merits of reduction of the sodium void effect as a way of avoiding LOF-TOP development. It was concluded that there is no significant tendency for such development for an LMFBR with a core sodium void worth of about \$2. For a \$3-\$4 void worth the potentiality depends on the size of the Doppler and axial expansion feedbacks, and also on the degree of incoherence among the channels in power and power/flow, particularly the latter. The amount of incoherence that is realistic from a design standpoint, 10-20% in power and power/flow, is large enough to have a considerable effect on LOF-TOP development. It is important to have an adequate modeling of this incoherence. Assumptions affecting clad motion, including the fuel-clad gap conductance and modeling of clad motion itself, could have a significant effect on LOF-TOP potential in the \$3-\$4 void worth range. For larger void worths LOF-TOP development is likely regardless of design details and parameter assumptions.

The \$2.5 void worth chosen in the ANL heterogeneous LMFBR EPRI design[1] seems to be a reasonable target for LOF-TOP prevention on the basis of current information, assuming that no axial expansion feedback is available. It seems likely that some such feedback will be effective; obtaining more definite information on what fraction of that theoretically possible can be relied on under various conditions would be important in determining the reduction in sodium void effect necessary to prevent LOF-TOP development. Because design penalties, particularly with respect to needed core decoupling, start becoming severe as the soidum void effect is lowered from \$3 to \$2, it is desirable not to lower it more than really necessary. Because clad motion is increasingly likely to occur with a reduced sodium void effect, more parametric studies of clad motion effects, including variation of parameters affecting clad velocity as well as variation of the gap conductance, seem to be worthwhile for heterogeneous designs. It will be helpful to have the improved modeling of clad motion to be available in SAS4A. Increased clad motion could be important from the standpoint of plugging coolant channels regardless of its reactivity effects.

Fuel motion reactivity effects appear unlikely to be important in leading to LOF-TOP conditions. This should become even more evident in SAS calculations when model improvements are made consistent with recent experimental results.

Fuel Pin Failure Studies

The transient fission gas release routine FFRATE in the SAS/EPIC Code has been recoded to include as a third option the British NEFIG model (Ref. Journal of Nuclear Materials 87 (1979) 167-174). In this option, first a user specified amount of grain boundary gas is released linearly with temperature between 1773°K and 2173°K, then the NEFIG model is used to compute the release rate of the intragranular gas between 2173°K and the fuel solidus, and lastly the remaining intragranular gas is released linearly with fuel melt fraction between the solidus and the liquidus.

A review of available models for transient fission gas release from reactor fuel is under way. Codes under study are FRAS3, NEFIG, GRASS-SST, and FASTGRASS. The latter two codes have been developed under the LWR program. NEFIG and FASTGRASS are fast-running codes in which only a single bubble size is followed. With currently-used parameter assumption, NEFIG and FRAS3 give similar results, although there is compensation between different assumptions about fuel lattice vacancy availabiliy and bubble diffusion coefficient. NEFIG simulates the average bubble size of FRAS3 quite well. The reasonable results obtained with the code and the variation in gas release rate possible through input parameter variations led to the decision to incorporate it into SAS/EPIC.

Different parameter and modeling assumptions cause GRASS-SST and FASTGRASS to give lower gas release than FRAS3 and NEFIG. FRAS3 and NEFIG give reasonably good agreement with some of the FGR transient gas release results. Recent HEDL results available in a draft report show a considerable retardation in gas release following an initial heating period of 10-20 sec at 1200°-1500°C. This effect is not predicted by any of the above models.

This work is discussed in more detail in the Physics of Reactor Safety Quarterly Report for January-March 1980, ANL-80-54, NUREG CR-1526 now in press.

Effect of Sodium Compressibility and Steel Elasticity on Inlet Plenum Pressure Rise from Boiling in an LMFBR Loss-of-Flow Accident

The rise of inlet plenum pressure in an LMFBR because of sodium boiling and consequent downward sodium slug ejection can have an important inhibiting effect on the velocity of such ejection, which might in turn have an important effect on an accident sequence. In the SAS code compressibility of the sodium in the inlet plenum is used to smooth pressure fluctuations in calculating the coupling of the in-core sodium flow to the sodium flow in the primary loop. It seemed to be of interest to investigate whether sodium compressibility and structural elasticity effects are of real physical importance in accident calculations.

These effects have been investigated using the one-dimensional Pressure Transient Analysis Code PTA-2,[2,3] using a single channel to model the core. The reactor model used was based on the CRBR, with the geometrical elevations and dimensions taken from the CRBR design. The free sodium surfaces in the reactor and pump vessels have been explicitly modeled. In addition to the loop-type CRBR design, a pool-type reactor has been simulated by using a pipe length between the pump outlet and the inlet plenum of 50 ft rather than 500 ft. The initial coolant flow and the bubble pressure-time history data input to the analysis were based on a SAS-3A calculation of a loss of flow accident for the CRBR.

It was found that the inlet plenum pressure buildup in the loop case was considerably larger than that in the pool case, implying an important difference in the retarding effect of the pressure buildup. This difference was caused by the difference in inertia effect of the two different liquid lengths in the inlet pipe. In either case the effect of sodium compressibility and steel elasticity on the inlet plenum pressure itself was small. For the loop case, however, the pressure difference between core and inlet plenum was relatively considerably greater when these effects were taken into account, resulting in an increase by about a factor of two in lower sodium slug ejection rate (from 1.5 ft/sec to 3.1 ft/sec). However, this ejection velocity was still small compared to that in the pool case, which was 14.3 ft/sec. Thus compressibility and elasticity effects resulted in a small reduction in the difference in plenum pressure buildup effect between pool and loop cases. In the pool case the compressibility and elasticity effects on lower slug ejection velocity were negligible. It does not appear that these effects are large enough to require consideration in accident analysis, although it would be desirable to carry out PTA-2 calculations in which the core is modeled by two or more channels with different pressure-time curves to see if the effects are larger with such a treatment.

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Development of FIFLO to Calculate Intra-Subassembly Incoherence Effects During Sodium Boiling and Voiding in Whole-Core Accident Calculations

Background

The need to consider multidimensional effects in the calculation of sodium boiling and voiding within a subassembly arises from intra-subassembly nonuniformity in the local power/flow ratio which can lead to large radial variations in the temperature. For the case of an FFTF subassembly, Chawla and Fauske[1] reported the results of calculations with detailed steady-state thermal-hydraulics codes indicating differences between the maximum and minimum temperatures at the top of the active fuel region varying between 170 K and 240 K at boiling inception in a loss-of-flow transient. Chawla et al.[2] later calculated the preboiling portions of loss-of-flow transients in a FFTF subassembly using the COBRA-IIIC code and obtained temperature differences at the top of the active fuel region of ~220 K at boiling inception (for a pump coastdown). Relative to a one-dimensional description, radial variation in the temperature within a subassembly is expected to result in earlier initiation of boiling in the higher power/flow subchannels and delayed boiling in the lower power/flow regions. Incoherence could prolong the time following boiling initiation required to attain reversal of the inlet flow and gross bundle voiding. Moreover, the potential also exists for early local dryout followed by cladding melting and motion within the higher power/flow portions of a subassembly. Thus, cladding relocation might begin earlier than would be indicated by a strictly one-dimensional treatment.

Two-dimensional calculations of sodium boiling within a subassembly have been performed by Miao and Theofanous.[3] Their code, HEV-2D, models a pin bundle as a porous medium in a cylindrical (R-Z) geometry. Within boiling regions, they assume that the liquid and vapor phases remain in thermodynamic equilibrium and move with identical velocities (i.e. no-slip flow). For a loss-of-flow transient in a CRBR subassembly, they found significant differences between one and two-dimensional calculations performed with their code. Although their studies predicted that at normal power inlet flow reversal followed boiling inception by ~0.6 s for a one-dimensional calculation, 1.4 s was required to reach flow reversal in a two-dimensional calculation. This effect was partially offset by the fact that boiling was initiated 0.8 s later in the one-dimensional calculation. However, the peak cladding temperature within the subassembly was calculated to be ~200 K higher for the two-dimensional calculation at the time of flow reversal. Several investigators[4],[5],[6] have studied void propagation in pin bundles by treating the boiling region as a blockage and calculating the void growth with a single-phase thermal-hydraulics calculation modified by the presence of the blockage. The calculations of Chen, Ishii and Grolmes[4] performed in this manner suggest that about 0.5 s is required for the void to grow to the radial extent of the bundle for a loss-of-flow transient in a FFTF subassembly.

Multidimensional sodium boiling effects are indicated by wire wrap thermocouple data obtained in the 37-pin P3A and P3 Sodium Loop Safety Facility (SLSF) in-pile experiments.[7] Analysis of the test results indicates differences between test data and SAS which are thought to be due to incoherence effects.[7] For the P3A experiment, it has been reported that better agreement is obtained with multidimensional calculations[8] than with one-dimensional calculations.

Similarly, for the P2 19-pin SLSF experiment, Marr[9] obtained better agreement between test results and two-dimensional calculations than with SAS calculations. Multidimensional effects are also illustrated by thermocouple data obtained from out of pile sodium boiling tests[10] performed in the THORS Facility using a 19-pin electrically heated bundle simulating FFTF geometry.

While studies such as those noted above have indicated that intra-subassembly incoherence efects can strongly influence the boiling and voiding behavior in a subassembly under hypothetical accident conditions, the capability to account for such effects in a whole-core accident analysis calculation does not exist. This is because all current whole-core accident codes for the calculation of the initiation phase (e.g. SAS4A[11]) assume strictly one-dimensional modeling within subassemblies. Consequently, a whole-core accident calculation incorporating the effects of intra-subassembly incoherence has never been performed. To fill this gap, we are developing the BIFLO code to calculate sodium boiling and voiding within a subassembly while modeling the effects of radial temperature incoherence. BIFLO is currently being developed as a stand-alone code with the objective of achieving a computational efficiency high enough to permit its incorporation within a whole-core calculation. When this objective has been achieved, we intend to replace the one-dimensional boiling model in the SAS4A[11] code with BIFLO to create the first core-wide accident analysis capability modeling intra-subassembly incoherence.

BIFLO Modeling

The initial BIFLO version models a subassembly in terms of a multidimensional Eulerian grid containing axial channels which are assumed to be interconnected so that sodium is free to flow between adjacent channels. While the number of channels is currently variable, it is anticipated that for whole core applications the bundle channel representation may be limited to the use of only two or three channels to minimize computational storage. Although the initial BIFLO version has been programmed assuming that each channel corresponds to one or more of the hexagonal coolant rings in a pin-bundle (this geometry is appropriate for our early benchmarking calculations and computational studies), this restriction could be readily removed to permit the treatment of a subassembly subjected to a power skew. In the initial version of BIFLO, different Eulerian numerical methodologies are employed for the calculation of thermohydrodynamic motions in two-phase and single-phase regions of the numerical grid. This permits the computational efficiency of calculations within each type of region to be optimized. In single-phase regions, axial flows are calculated with the aid of an assumption that the local axial pressure drop is approximately the same for all single-phase channels.[12] A transverse momentum equation is not solved in the single-phase regions. Instead, the effects of crossflow induced by the wire wrap spacers is modeled through experimentally calibrated crossflow terms specifying mass transfer between channels. There is also a calculation of crossflow due to flow diversion which can be significant near the boundaries of two-phase regions. Within two-phase regions, the numerical formulation is based on a methodology[13] for calculating multidimensional two-phase flow at saturation. The two phases are assumed to remain in thermodynamic equilibrium at saturation and slip between the phases is modeled with distinct liquid and vapor velocity fields. The effects of radial flow are modeled with the help of transverse momentum equations as well as crossflow terms (analogous to the single-phase crossflow terms) intended to account for the effects of wire wrap induced crossflow. An uncertainty involves the modeling of radial, two-phase frictional effects within pin bundles.

BIFLO Benchmarking

The benchmarking of BIFLO involves a comparison among BIFLO, other codes (e.g. COMMIX, COBRA-IV, and SAS), and experiments (e.g. THORS and SLSF). An important part of this task involves comparison of the codes employing subchannel geometry to the experiments of interest. This process lends credence to the modeling in the subchannel codes, and the results of subchannel codes are more easily compared to the point-wise measurements in the experiments; region-average conditions obtained from the subchannel codes are then useful for comparison to BIFLO results. Confidence in the codes used for comparison is required since for large bundle sizes the benchmarking comparison will be between BIFLO and other codes rather than between BIFLO and experiments.

The COMMIX code was selected to be used for BIFLO benchmarking at the beginning of this work. The numerics of the boiling model were undergoing significant revision at that time, and use of the code was deferred until the improvements could be completed. The boiling model, although still being developed, is ready for limited use at the present time.

The COBRA-IV-I code[14] was the only other subchannel code found to be available which claimed to be able to handle boiling and flow reversal. An IBM-compatible version of this code was obtained from Oak Ridge National Laboratory (ORNL) along with associated preprocessor routines and made operational at ANL. Progress in using this code has been frustrated by convergence and instability problems. (Attempts to eliminate these problems by using the production version of the code, which is maintained by personnel from Battelle Pacific Northwest Laboratories on the CDC computers at Brookhaven National Laboratory, were unsuccessful.) Circumventions have been found to eliminate convergence problems which occur prior to boiling inception. Although there still seem to be instabilities present after boiling inception, there is a more general problem concerning the applicability of the assumptions in the boiling model to sodium systems. The code is, however, being used to examine steady-state and pre-boiling transient cases. Some limited use has recently been made of the COMMIX-1A code[15] (i.e. the non-boiling version) to supplement the results obtainable with COBRA-IV-I. (Indeed, use of COMMIX-1A may eliminate the need for using COBRA-IV-I.)

Comparisons are being made between the several codes and the experiments[9,16] which were run in Bundle-6A in the THORS facility at ORNL. Bundle-6A used 19 wire-wrapped electrically heated fuel pin simulators cooled by flowing sodium. The bundle was instrumented with ~100 thermocouples located on the inner surface of the pin cladding, in the wire wraps, and in the hexcan duct wall. The data records for all steady-state tests and for three transient tests have been obtained from ORNL. The steady-state tests include runs with all pins powered at various total bundle powers and coolant flow rates; analysis of these tests will aid in understanding mixing in bundle geometry. The three transient tests for which the data has been obtained are at different bundle power levels; all three tests experienced boiling and local dryout was indicated in two tests. Selected tests from THORS Bundle-6A are now being analyzed using BIFLO, COMMIX-1A, and COBRA-IV-I. Other test data which may be useful in the benchmarking process are the 61-pin tests now being performed in the THORS facility and the 19- and 37-pin tests in SLSF at ANL.

Future Work

Calculations with the initial version of BIFLO indicate that it runs too slowly after the onset of boiling to be used for whole-core accident analysis applications within the SAS4A framework. It is believed that an improvement in running time of about a factor of ten is required (the present running speed is not a hindrance to performing calculations for a single bundle). It has been determined that the slow running times are due to the necessity of using time step sizes for the two-phase calculation which are limited by a Courant stability restriction based on the two-phase material velocities (i.e. $\frac{U\delta t}{\delta z} < 1$) which results from using an explicit formulation of finite difference terms accounting for convection within the two-phase methodology. Thus, while the current scheme is relatively efficient on a computation per time step basis, too many time steps are required to calculate two-phase conditions over the time scales of several seconds or longer anticipated for accident analysis applications. We note here that when two-phase flow conditions do not exist, the single-phase portions are routinely run with time step sizes exceeding the Courant limit by an order of magnitude or more.

The initial version of BIFLO is currently being used in assessing the adequacy of modeling assumptions incorporated in the code. The comparison with experiments performed in THORS Bundle-6A is underway and has been discussed above. Other BIFLO calculations are addressing the problem of determining the minimum number of channels required to adequately represent incoherence within a subassembly during a loss-of-flow transient. Two modeling assumptions which need to be carefully examined are the assumption of a transversely uniform local axial pressure drop in single-phase regions and the assumption of saturation conditions within voiding regions (particularly at high void fractions). A comparison between BIFLO and COMMIX-2 should provide useful information about the suitability of these and other assumptions. We are considering alternate numerical fluid dynamics methodologies permitting the use of time steps greatly exceeding the Courant stability limit during the two-phase portions of the calculation. The SETS method[17] for the calculation of two-phase flow, recently developed within the TRAC program at the Los Alamos Scientific Laboratory, appears to have the potential of achieving our requirements for fast running times. Other candidate schemes (e.g. Refs. 18-19) will also be examined for possible use in BIFLO. Assuming that our objectives for running times can be met, we plan to write a significantly faster running version of BIFLO for incorporation into SAS4A. Until a faster running version becomes operational, benchmarking activities and computational studies will continue using the current version.

Intra-subassembly incoherence in voiding and dryout gives rise to incoherence in the melting of cladding. Studies[20,21] of cladding relocation in the hypothetical loss-of-flow accident have indicated that multidimensional effects can significantly influence the relocation of cladding. For heterogeneous cores, cladding relocation may assume a relatively greater significance in determining the potential for achieving loss-of-flow driven transient-overpower accident conditions. Development of a cladding relocation calculation modeling the effects of incoherence would be a logical follow-on to BIFLO which would provide the framework within which the development of such a model could be carried out. At the present time, however, no decision has been made concerning the development of a cladding relocation model.

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Statement to ACRS Working Group 6, Advanced Reactor Safety Research
July 9, 1980 by: Charles Kelber

You have asked me to summarize our response to ACRS recommendations concerning the aims of our program.

The Advanced Reactor Safety Research program on Fast Breeder Reactor Safety has attempted to respond positively to ACRS recommendations regarding the aims of our research, and this response has taken two forms:

1. Re-direction or emphasis of existing work, such as work with the Super System Code and Brenda; and,

2. Development of new approaches that focus clearly on ACRS concerns, such as the fuel testing sensitivity program. We judge these efforts to have a high promise of success. If the program is continued at the level of effort requested by the Office of Research ($ 8 million in FY 82), the effort will be concentrated on the ACRS concerns listed in my first viewgraph. While there will be a certain level of code development and support to improve computing efficiency, the major use of the codes will be their application to these concerns. We anticipate some testing to continue,as well, particularly to improve our knowledge of key processes and develop a few key models.

On succeeding viewgraphs we summarize the major efforts responding to each recommendation. It is noteworthy to remark the benefit from particpation in exchanges with foreign programs. An early recommendation of the ACRS was to derive maximum benefit from such programs and we are obviously doing just that.

In summary, the Advanced Reactor Safety Research program has the tools and capabilities to investigate key areas of safety concerns delineated by the ACRS and others. Given an adequate level of support the program should continue to produce notable results that help set the stage for any future licensing actions by the NRC that may arise. Clearly it is not easy to forecast when such actions may be needed. Just a few years ago the ACRS and the NRC generally were in a difficult discussion with respect to licensing CRBR. We do not know when such an opportunity might present itself again-estimates range from a year to twenty years, and appear to be political as much as technical in nature-but,when it does, the NRC should be prepared. The budget requested appears to be a bargain price for making sure NRC has resources available to act if and when needed.

ACRS CONCERNS

- ANALYZE ACCIDENTS OVER A BROAD SPECTRUM, LESS EMPHASIS ON CDA AND CLINCH RIVER.
- INVESTIGATE TRANSITION TO NATURAL CONVECTION - NEED NEW FACILITY?
- DEFINE FUEL SAFETY TEST NEEDS AND REVIEW TESTING CAPABILITIES.

ANALYZE ACCIDENTS OVER A BROAD SPECTURM, LESS EMPHASIS ON CDA AND CLINCH RIVER.

- ACCIDENT DELINEATION PHASE 1 REPORT IN JUNE.
- SHIFT SSC FROM CODE DEVELOPMENT TO APPLICATION - STUDY OF ACCIDENTS WITH SCRAM IN PROGRESS.
- HETROGENEOUS VS HOMOGENEOUS CORE STUDY UNDERWAY.

INVESTIGATE TRANSITION TO NATURAL CONVECTION - NEED NEW FACILITY

- FOCUS ANALYTICAL METHODS ON PROBLEM - SSC AND COMMIX. PRETEST PREDICTION OF FFTF NATCON TEST IN APRIL.
- SPECIALIST MEETING AT BNL IN FEB. TO SHARE CONCERNS WITH DOE AND COMMUNITY.
- CSNI INITIATIVE TO ENCOURAGE INTERNATIONAL APPROACH TO PROBLEMS - SPECIALIST MEETINGS MARCH AND SEPT.
- JOINT ANALYSIS OF PFR TESTS (PENDING).

DEFINE FUEL SAFETY TEST NEEDS AND REVIEW TESTING CAPABILITIES

- SENSITIVITY STUDIES TO REFINE NEEDS.
- CLOSER COMMUNICATIONS WITH DOE FUEL TESTING PROGRAM.
- PARTICIPATION IN CABRI PROGRAM.

SSC DEVELOPMENT AND CODE VALIDATION PROGRAMS AT BROOKHAVEN NATIONAL LABORATORY

PRESENTED BY

JAMES G. GUPPY
ACTING GROUP LEADER

AT ACRS REVIEW MEETING
WASHINGTON, D.C.
JULY 9, 1980

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

SSC PRESENTATIONS

- SCOPE
- PRESENT STATUS
- SSC-L RESULTS
  - FFTF TRANSIENTS
  - NATURAL CIRCULATION TRANSIENTS
  - OPERATIONAL TRANSIENTS
- FUTURE PLANS

SCOPE OF SSC DEVELOPMENT PROGRAM

- SSC SERIES OF COMPUTER CODES SIMULATE THERMOHYDRAULICS OF ENTIRE PLANT INCLUDING REACTOR CORE AND HEAT TRANSPORT SYSTEMS
- SSC CODES ARE DESIGNED TO STUDY OPERATIONAL AND OTHER SYSTEM-WIDE ACCIDENT TRANSIENTS, WITH PARTICULAR EMPHASIS ON NATURAL CIRCULATION
- SSC CODES ARE DEVELOPED TO PROVIDE AN INDEPENDENT ANALYTICAL TOOL APPLICABLE TO A WIDE VARIETY OF POTENTIAL SYSTEM DESIGNS
- PLANT CONTROL SYSTEMS ARE INCLUDED
- PLANT PROTECTION SYSTEM IS INCLUDED
- MUST MODEL ADEQUATELY ALL COMPONENTS/PROCESSES ESSENTIAL TO HEAT REMOVAL
- CODE TO BE FAST RUNNING; REAL TIME OR FASTER

SSC VALIDATION PROGRAM

- OBJECTIVE - PROVIDE A DATA BASE OF SUFFICIENT SCOPE TO QUALIFY SSC AN AN INDEPENDENT LICENSING TOOL
- VALIDATION BY EXPERIMENTAL COMPARISONS
  - ON A SYSTEM BASIS
    - FFTF
    - EBR-II
    - SNR-300 (GERMANY/BELGIUM/HOLLAND)
  - ON A COMPONENT BASIS
    - LMEC - (PUMPS, DHX)
    - AI - (STEAM GENERATORS)
    - ANL - (UPPER PLENUM MIXING)
    - GERMANY - (SNR-300 PROTOTYPE SG)
- VALIDATION BY ANALYTICAL COMPARISONS
  - ON A SYSTEM BASIS
    - CRBRP/DEMO
    - FFTF/IANUS
  - ON A COMPONENT BASIS
    - COMMIX - COMPONENTS, PIPES
    - COBRA - CORE

VERSIONS OF SSC

- SSC-L - SIMULATES SHORT-TERM (UP TO ~½ HR) TRANSIENTS IN LOOP-TYPE LMFBRs
- SSC-P - SIMULATE SHORT-TERMS TRANSIENTS IN POOL-TYPE LMFBRs
- SSC-W - SIMULATES SHORT-TERM TRANSIENTS IN LWRs
- SSC-S - SIMULATES INTERMEDIATE TO LONG TERM (BEYOND ½ HR) TRANSIENTS. IT INCORPORATES OTHER HEAT TRANSFER MODES AND LOOPS

SSC BACKGROUND

- SSC PROGRAM FUNDED BY USNRC/ARSR SINCE FY 1976.
- SSC-L; OPERATIONAL SEPTEMBER 1977
  BUFF BOOK MILESTONE - SEPTEMBER 1977
- SSC-P; OPERATIONAL NOVEMBER 1979
  BUFF BOOK MILESTONE - NOVEMBER 1979
- SSC-W; WORK BEGUN MID-MAY 1979
  OPERATIONAL MARCH 1980
  BUFF BOOK MILESTONE - MARCH 1980

SSC STATUS

- SSC-L
  - OPERATIONAL SINCE SEPTEMBER 1977
  - USERS INCLUDE
    - (1) BNL
    - (2) NRC/ARSR
    - (3) GESELLCHAFT FUR REAKTORSICHERHEIT, W. GERMANY (SNR-300 LICENSING ANALYSIS)
    - (4) B&W (LARGE SCALE LMFBR DESIGN STUDIES)
    - (5) CE (LARGE SCALE LMFBR DESIGN STUDIES)
    - (6) GE (LARGE SCALE LMFBR DESIGN STUDIES)
    - (7) UNIVERSITY OF ARIZONA (ACCIDENT DELINEATION STUDIES)
    - (8) PNC - JAPAN
  - HAS BEEN APPLIED TO FOUR (4) DIFFERENT SYSTEM DESIGNS (CRBRP, FFTF, SNR-300, B&W CDS)
  - AT APPLICATIONS/DEVELOPMENTAL VERIFICATION STAGE
  - BEING APPLIED TO VARIOUS EXPERIMENTAL TESTS (PARTICULARLY FFTF)
  - VERIFICATION OF SSC-L VALID FOR OTHER VERSIONS OF SSC

SSC STRUCTURE

- BASICALLY A SET OF BUILDING BLOCKS OF MODELS/COMPONENTS (CORE, PUMPS, PIPES, IHX, SG, CONTROL SYSTEMS)

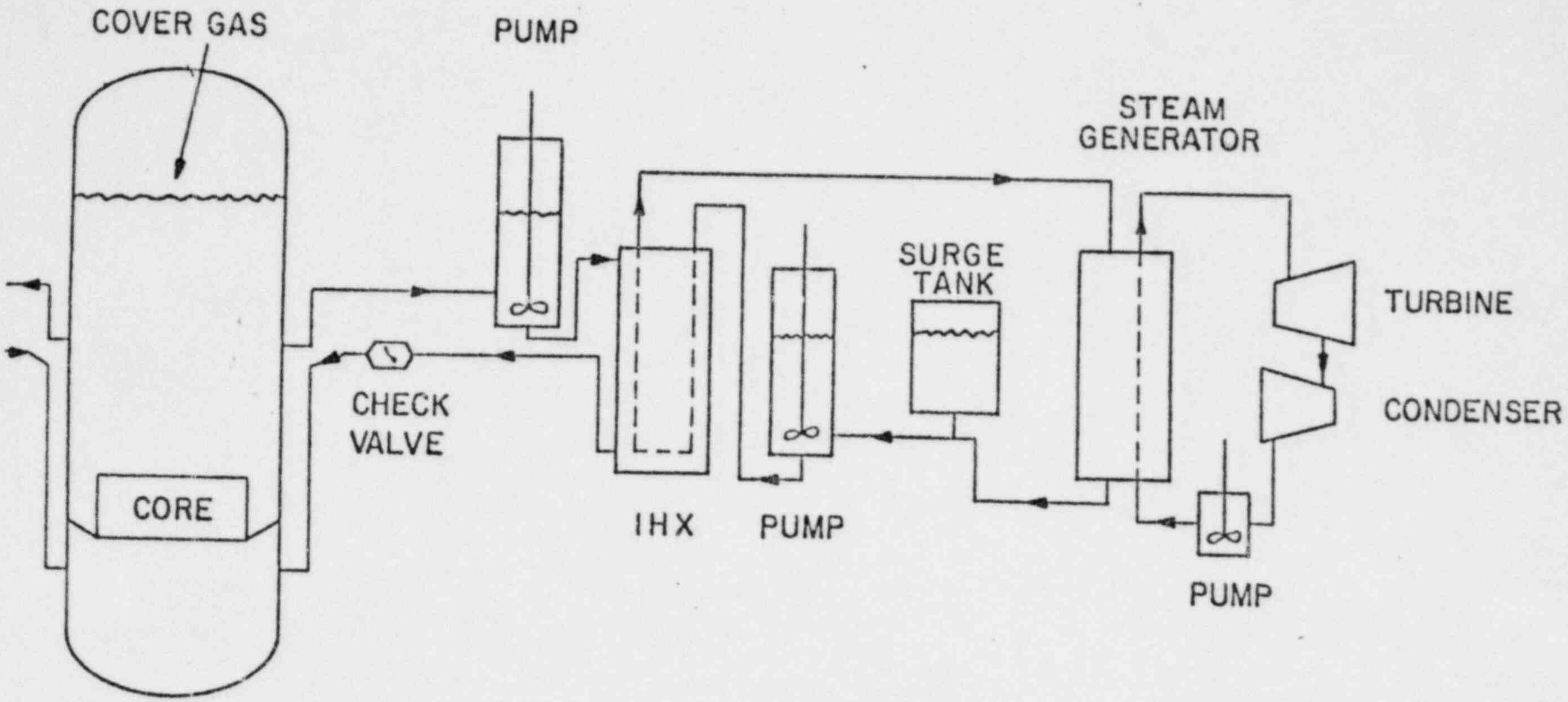
- HOW THESE BLOCKS ARE INTERCONNECTED IS WHAT DIFFERENTIATES ONE VERSION FROM ANOTHER

- THUS, THERE IS MUCH OVERLAP AND MANY MODELS/COMPONENTS ARE IDENTICAL BETWEEN VERSIONS
    - SSC-L AND SSC-P IDENTICAL PAST IHX
    - SG MODELS IDENTICAL, BUT PHYSICALLY TURNED INSIDE OUT BETWEEN SSC-W AND SSC-L, SSC-P

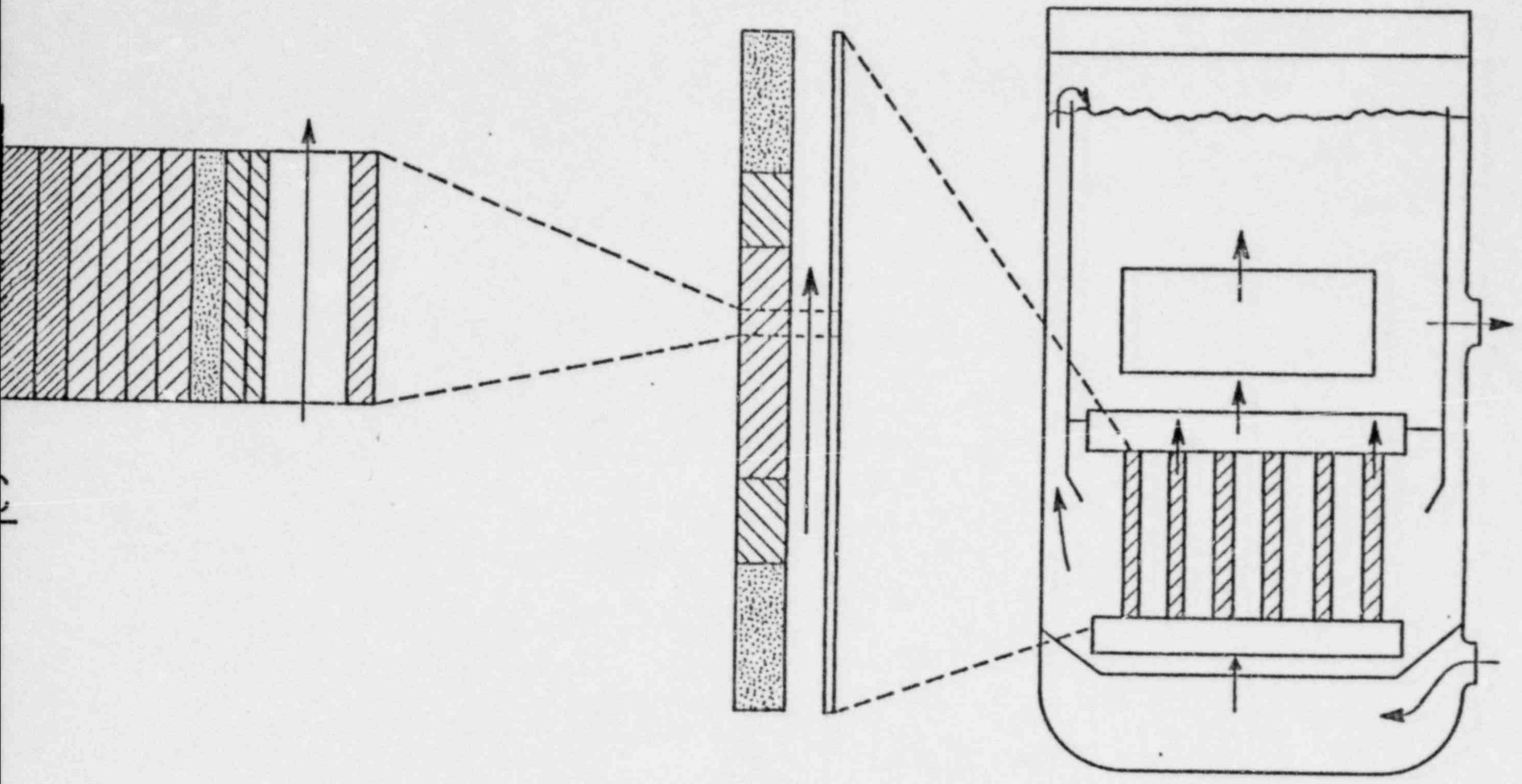
SSC

MAIN FEATURES

- COMPLETELY VARIABLE DIMENSIONED - ANY NUMBER OF USER SPECIFIED LOOPS/PIPES/NODES
- IN-LINE MESH REPRESENTATION
- MULTI-CHANNEL CORE REPRESENTATION
- DETAIL IS AVAILABLE, OR NOT, AS DESIRED (INPUT)
- PRE-TRANSIENT INITIALIZATION
- MULTIPLE TIMESTEP SCHEME FOR TRANSIENT SOLUTION
- DECOUPLED MOMENTUM EQUATION
- COMPUTATIONALLY EFFICIENT (REAL TIME SIMULATION)
- GENERAL SYSTEM TRANSIENT CODE (NOT PLANT SPECIFIC)
- TRANSIENT RESTART
- EXPORTABLE (IBM OR CDC)
- HIGHLY USER ORIENTED
- HIGHLY MODULAR AND READABLE
- EXTENSIVE VERIFICATION OF INPUT
- FULLY DOCUMENTED (INCLUDING USERS' MANUAL)



Sketch of One Set of Loops in an LMFBR System



IN-VESSEL MODELING

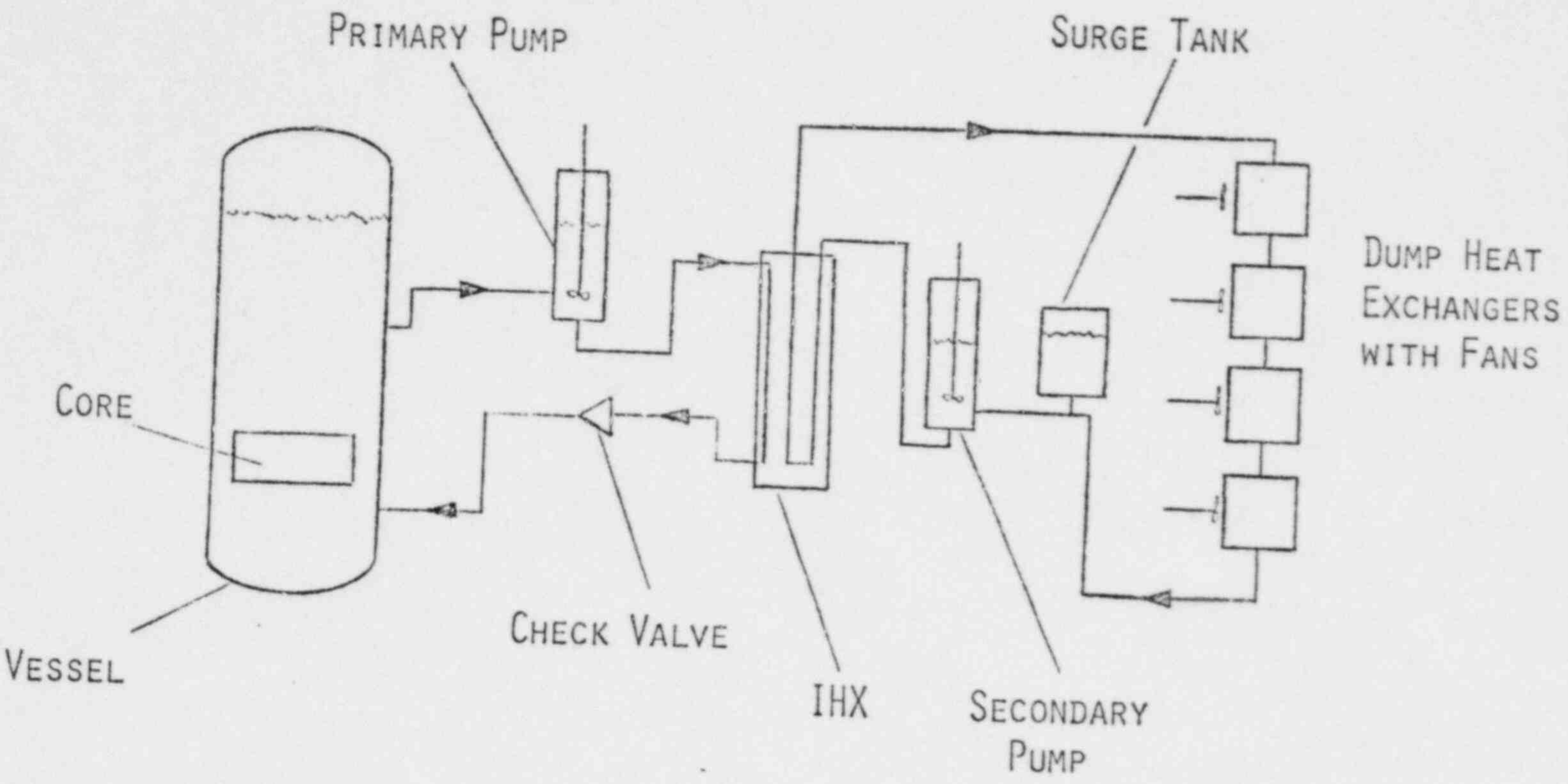
SSC-L RESULTS

- FFTF TRANSIENTS (VALIDATION)
- NATURAL CIRCULATION TRANSIENTS
- OPERATIONAL TRANSIENTS

METHODS OF CODE VALIDATION

- LINE BY LINE
- COMPARISON WITH EXPERIMENT
    - FFTF ACCEPTANCE TESTS
- INTER-CODE COMPARISONS
    - SSC-L WITH IANUS

SSC OVERVIEW OF THE FFTF



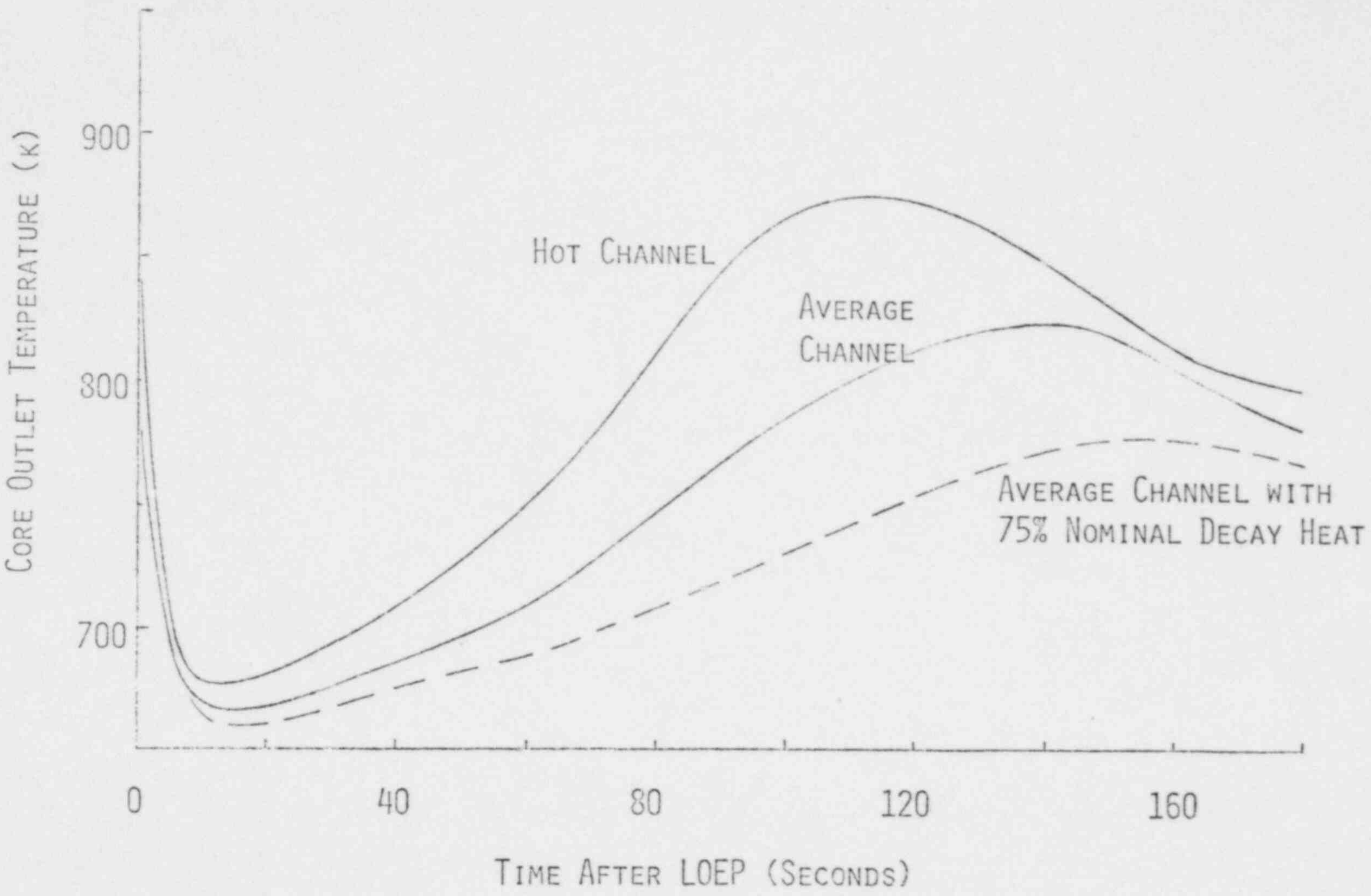
FFTF

SUMMARY OF ACCEPTANCE TESTS

POWER (%)	5	35	75	100
PRIMARY FLOW (%)	75	75	75	100
SECONDARY FLOW (%)	75	75	75	100
PRIMARY COLD LEG (F)	590	625	659	680
PRIMARY HOT LEG (F)	607	745	917	938
SECONDARY COLD LEG (F)	585	595	595	595

Grouping of Fuel Assemblies into Channels

SSC Channel Number		Assembly Designation
1	Orifice Zone 1	2101 (Hot Channel)
2		1201, 2201, 2202, 3201, 3202
3		1202 (Row 2 FOTA)
4		1301, 1303, 1304, 2301, 2303, 2304, 3301, 3303, 3304
5		1401, 1403, 1405, 2401, 2403, 2405, 3401, 3403, 3405
6		1402, 2402, 3402
7	Orifice Zone 2	3508
8		1501, 3506, 3507
9		1505, 1506, 1508, 2506, 2508, 3505
10		1507, 2501, 2505, 2507, 3501
11		1503, 2503, 3503
12	Orifice Zone 3	3609
13		1601, 1609, 2609, 3606, 3607, 3608
14		1605, 1606, 1607, 2601, 2605, 2606, 2607, 3601, 3605
15		1602, 1604, 1608, 2608 (Average)
16		1603, 2602, 2604, 3602, 3604
17		2603, 3603
18		3610 (Row 6 FOTA)



18 CHANNEL SSC SIMULATION OF FFTF

LOEP FROM FULL POWER

COMPARISON OF SECOND PEAK TEMPERATURES
TRANSIENT ACCEPTANCE TESTS (F)

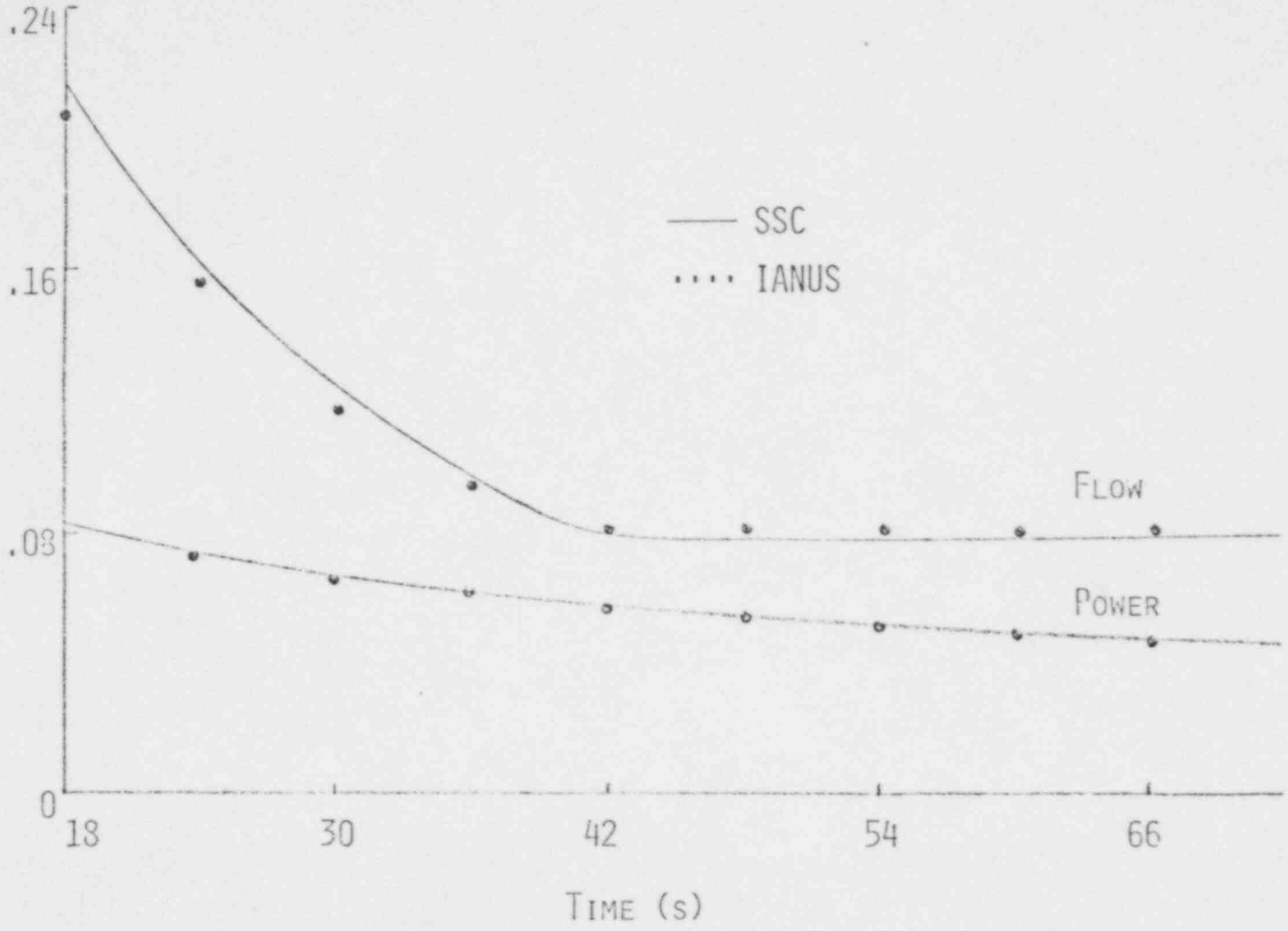
		Ianus/Flodisc	SSC
100 %	AVERAGE ASSEMBLY	975	1023
	HOT CHANNEL	1097	1115
	ROW 2 FOTA	1048	1106
	ROW 6 FOTA	1040	1112
75 %	AVERAGE ASSEMBLY	897	937
	HOT CHANNEL	1002	1015
	ROW 2 FOTA	957	1007
	ROW 6 FOTA	950	1009

FFTF

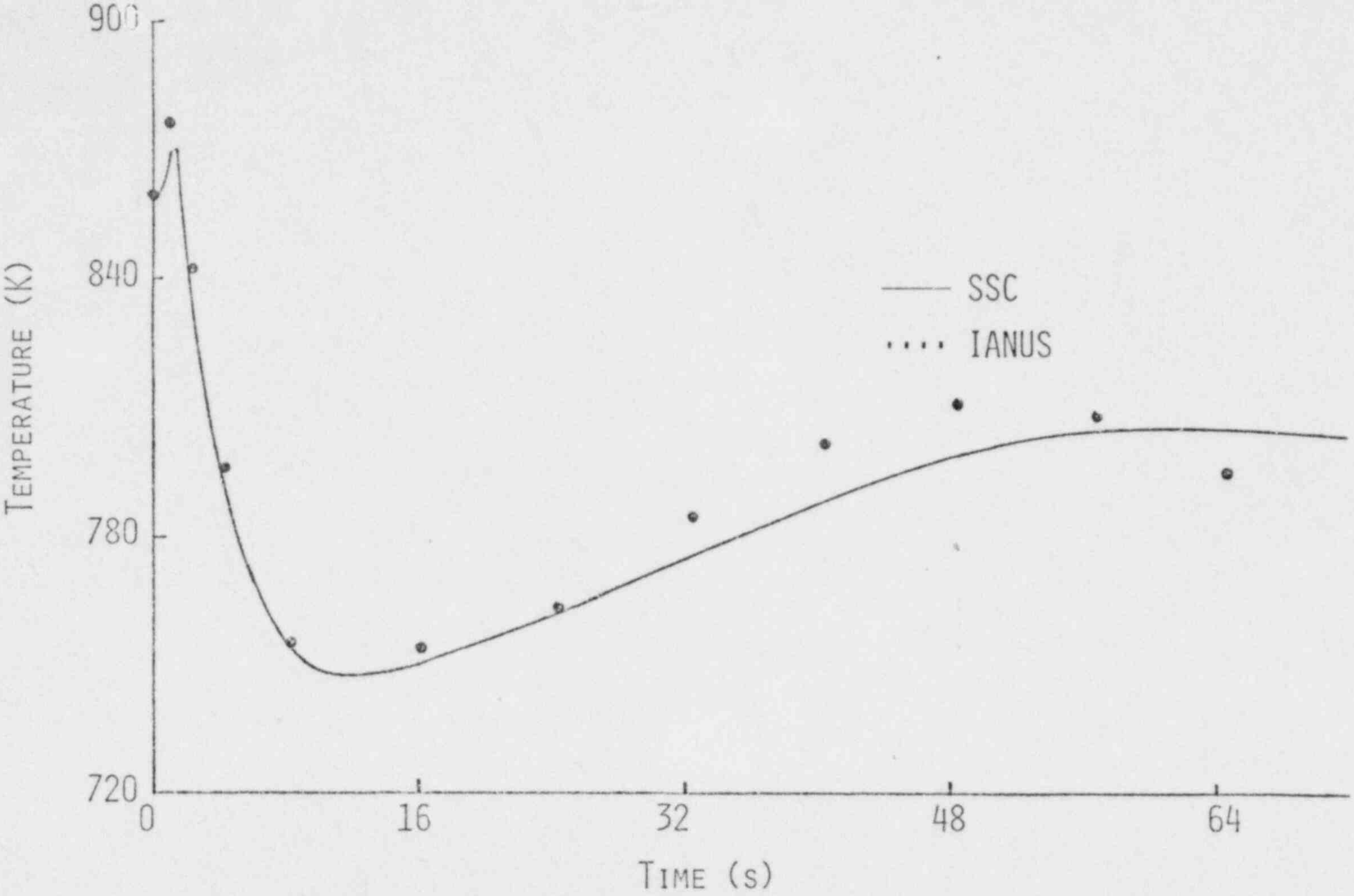
TRANSIENTS SIMULATED

- NORMAL SCRAM
- LOSS OF ELECTRIC POWER
- PIPE RUPTURE
  - BEFORE CHKV
  - AFTER CHKV
  - RVI (SMALL)
  - RVI (LARGE)
- TORNADO

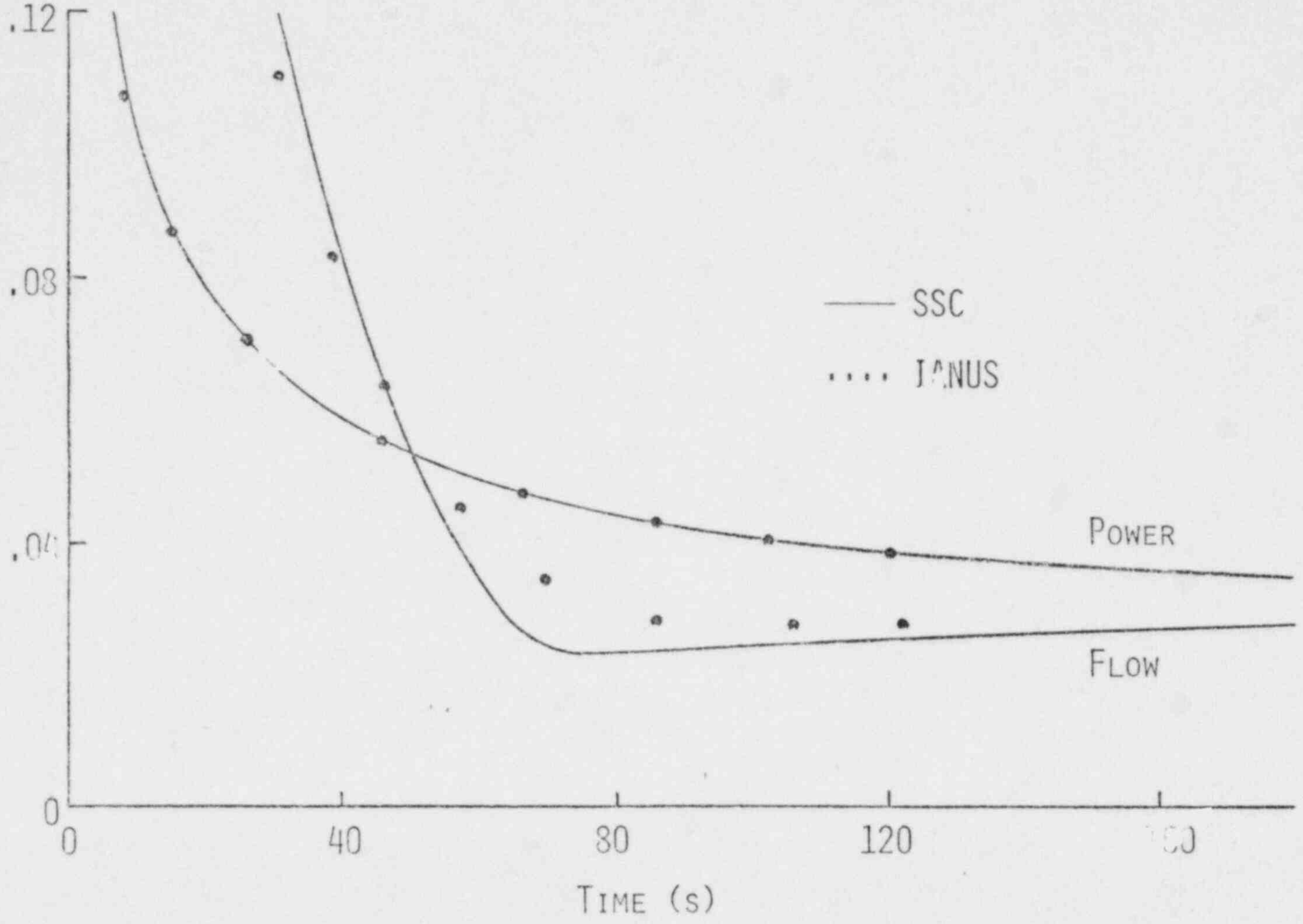
NORMALIZED POWER AND FLOW FOLLOWING NORMAL SCRAM

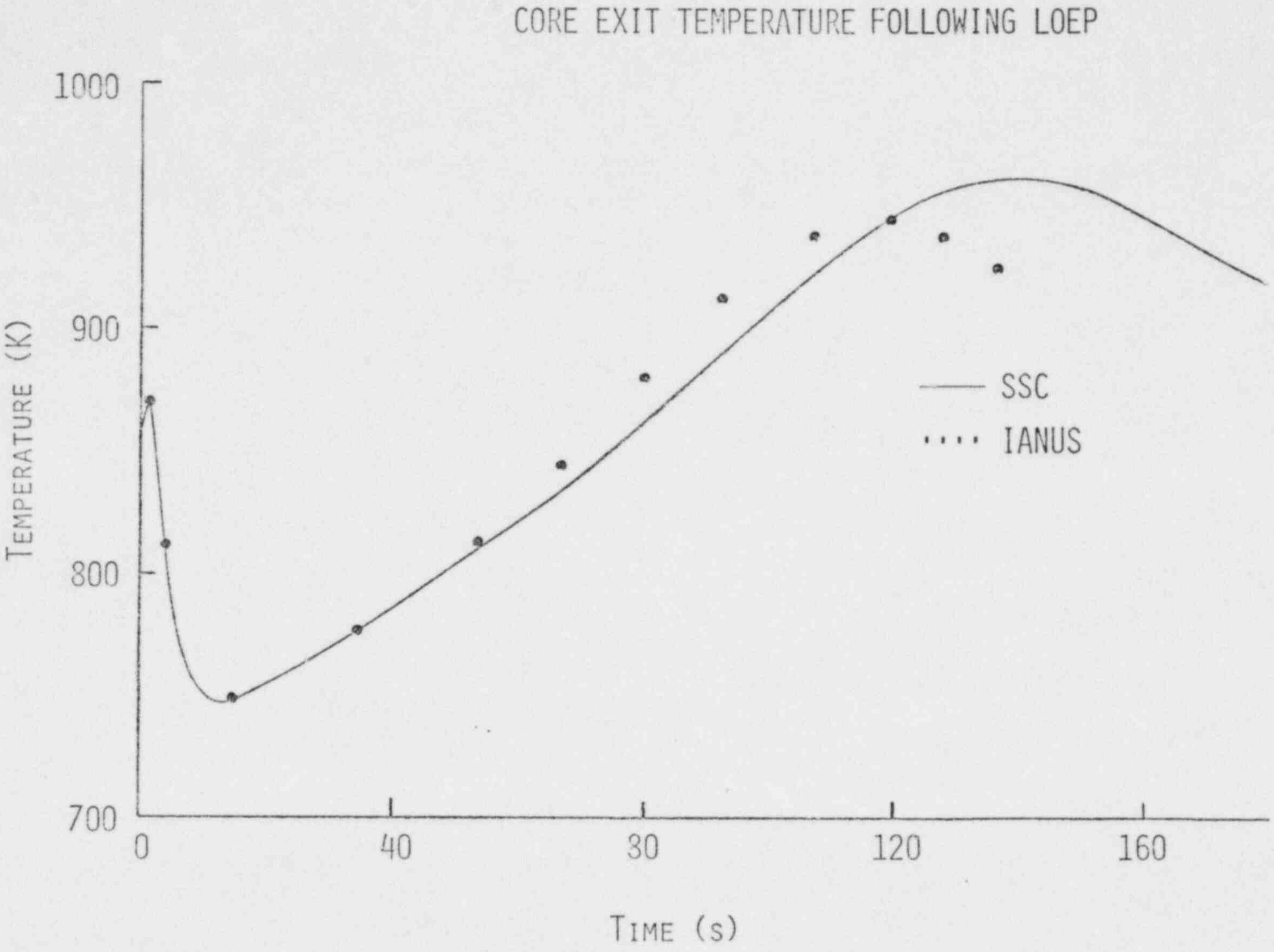


CORE EXIT TEMPERATURE FOLLOWING NORMAL SCRAM

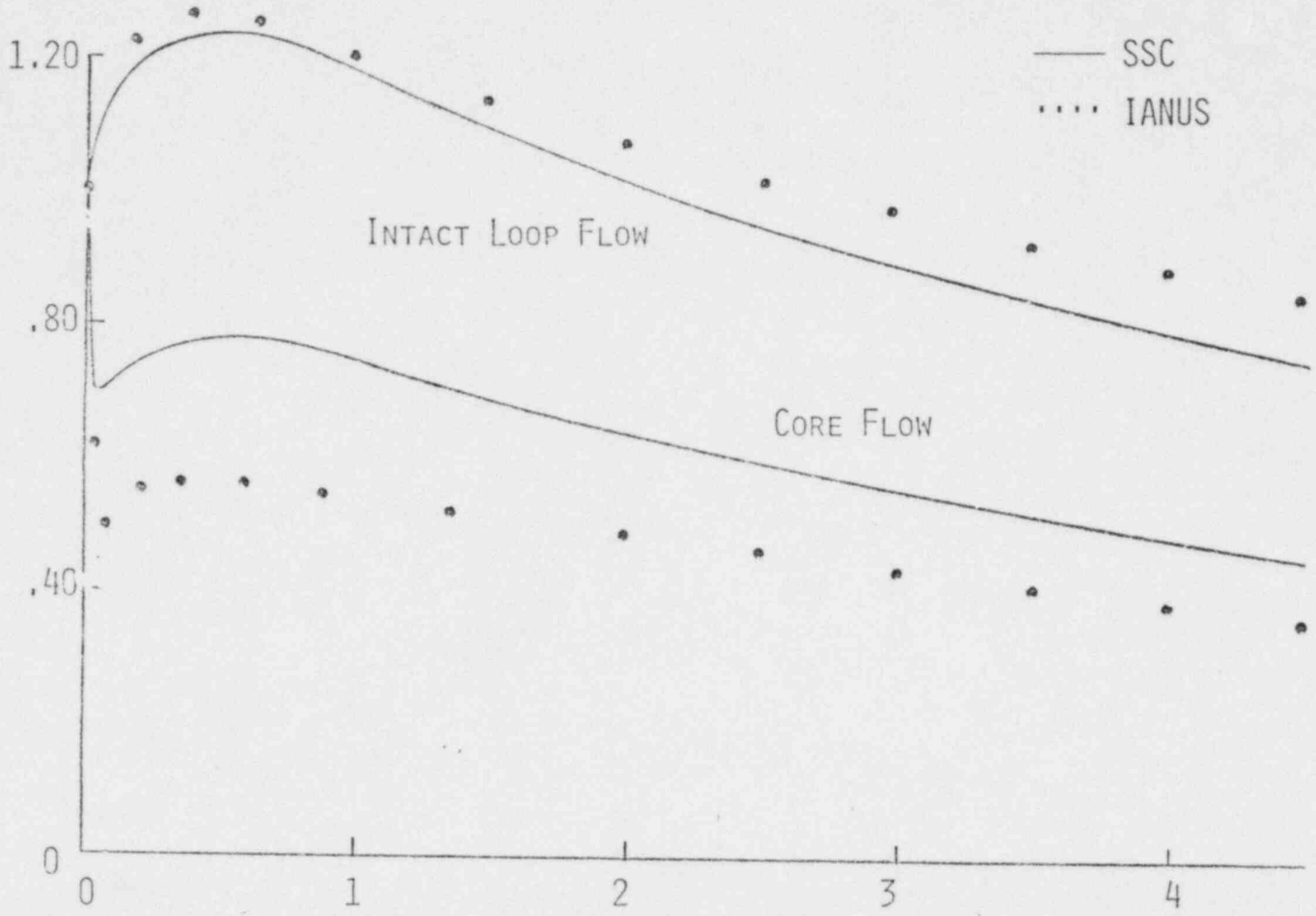


NORMALIZED POWER AND FLOW DURING LOEP

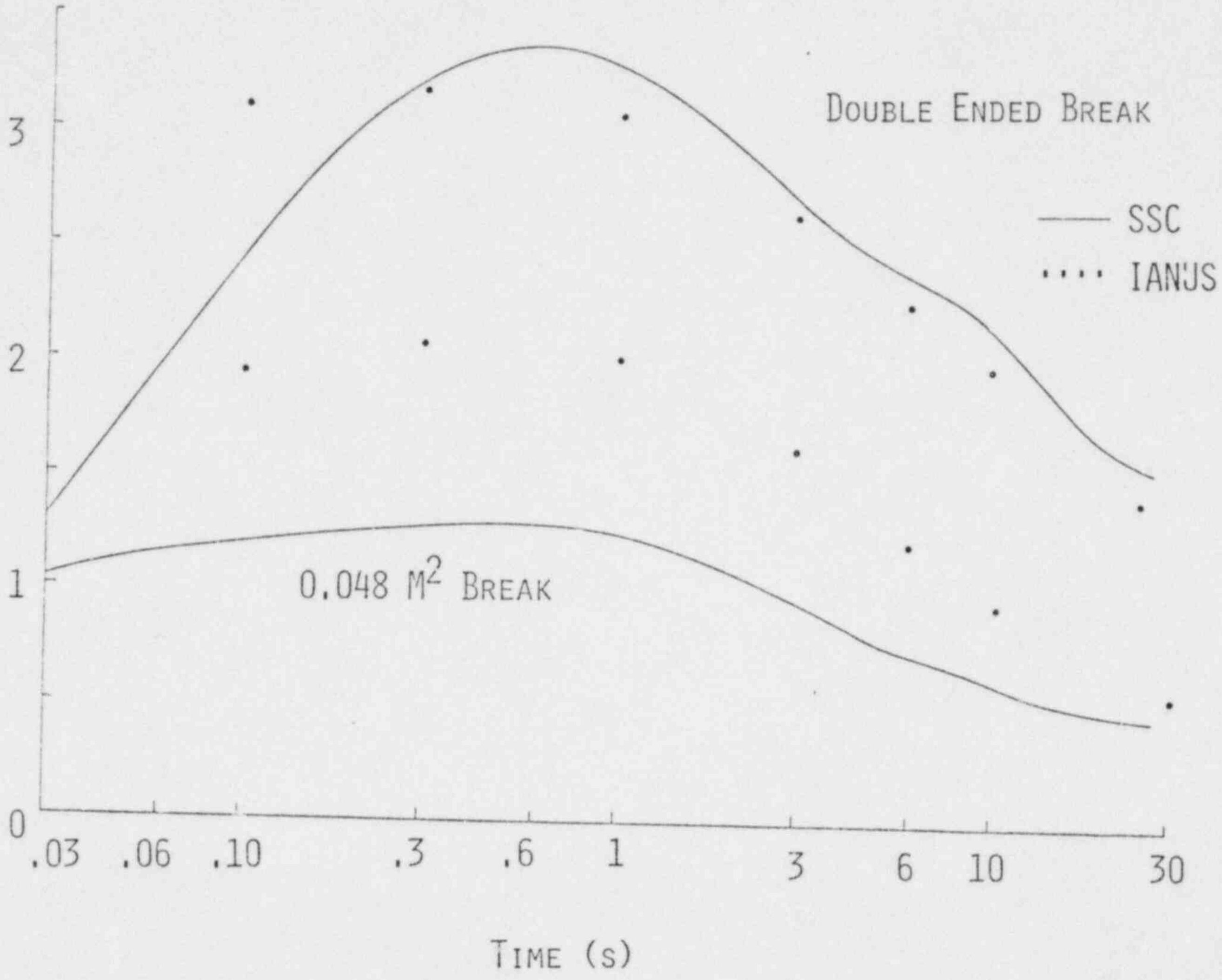



CORE EXIT TEMPERATURE FOLLOWING LOEP
1000
900
800
700
TEMPERATURE (K)
0
40
30
120
160
TIME (s)
SSC
IANUS

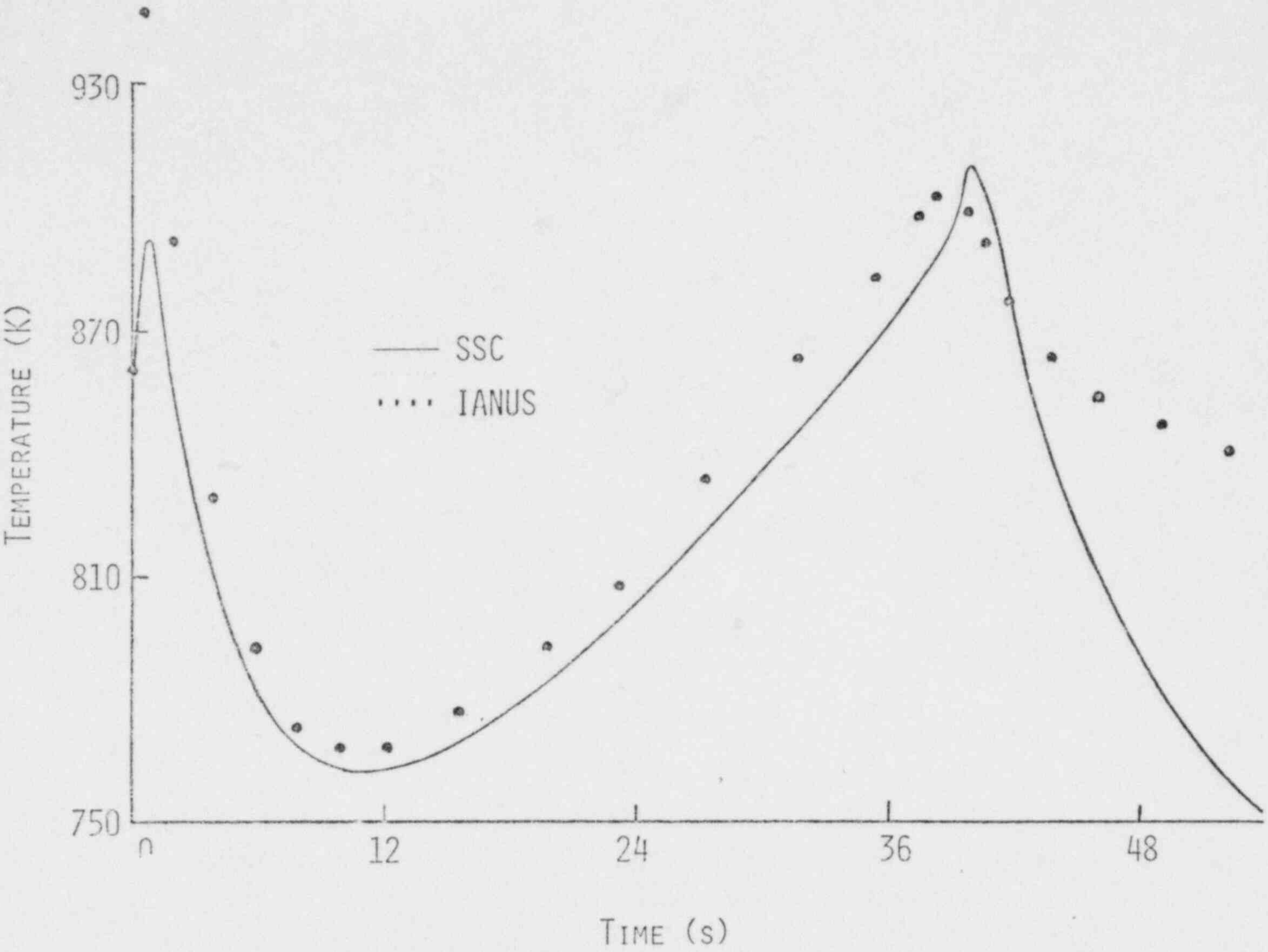
NORMALIZED LOOP AND CORE FLOW (.048 M^2 PIPE BREAK)



TIME (s)


NORMALIZED FLOW THROUGH PIPE RUPTURES

CORE EXIT TEMPERATURE FOLLOWING PIPE BREAK



SSC-L

SIMULATION CAPABILITIES

- ANY SIZE PIPE BREAK IN ANY SODIUM LOOP
- REACTOR SCRAM, MANUALLY AT ANY TIME OR PPS INITIATED
- PUMP MAIN MOTOR TRIP - ANY OR ALL PUMPS, MANUALLY AT ANY TIME OR PPS INITIATED
- PUMP PONY MOTOR FAILURE - ANY OR ALL PUMPS, MANUALLY AT ANY TIME OR PPS INITIATED
- COASTDOWN TO NATURAL CIRCULATION
- OPERATIONAL TRANSIENTS
  - REACTIVITY TRANSIENTS
  - SINGLE PUMP FAILURE
  - VALVE MALFUNCTION/FAILURE
  - CONTROL SYSTEM MALFUNCTION
  - OPERATOR INITIATED EVENTS
  - TURBINE TRIP

APPLICATIONS OF SSC-L

I. NATURAL CIRCULATION ANALYSES

II. PIPE BREAK ANALYSES

III. SCRAM TRANSIENTS

IV. REACTIVITY TRANSIENTS

V. PUMP MALFUNCTION TRANSIENTS

VI. VALVE MALFUNCTION TRANSIENTS

VII. CONTROL AND PLANT PROTECTION SYSTEM MALFUNCTION TRANSIENTS

VIII. COMPUTER TIMING REQUIREMENTS STUDIES

IX. NODALIZATION STUDIES

X. PARAMETRIC STUDIES

IMPORTANT FACTORS NECESSARY TO ADEQUATELY MODEL NATURAL CIRCULATION

- SINCE FLOW RATES ARE SMALL (10% OR LESS) FRICTIONAL PRESSURE LOSSES DRASTICALLY REDUCED (BY FACTOR OF APPROX. 100)
- SMALL DIFFERENCES IN THE LOCATIONS OF THE THERMAL CENTERS ARE NOW IMPORTANT
- TO ADEQUATELY TRACK THE THERMAL CENTERS NEED A DETAILED ACCOUNTING OF DENSITY DISTRIBUTION THROUGHOUT SYSTEM
- ALSO VERY IMPORTANT IN TRACKING THE TEMPERATURE DISTRIBUTION IS THE PROPER ACCOUNTING OF HEAT CAPACITY EFFECTS AND TRANSPORT DELAY

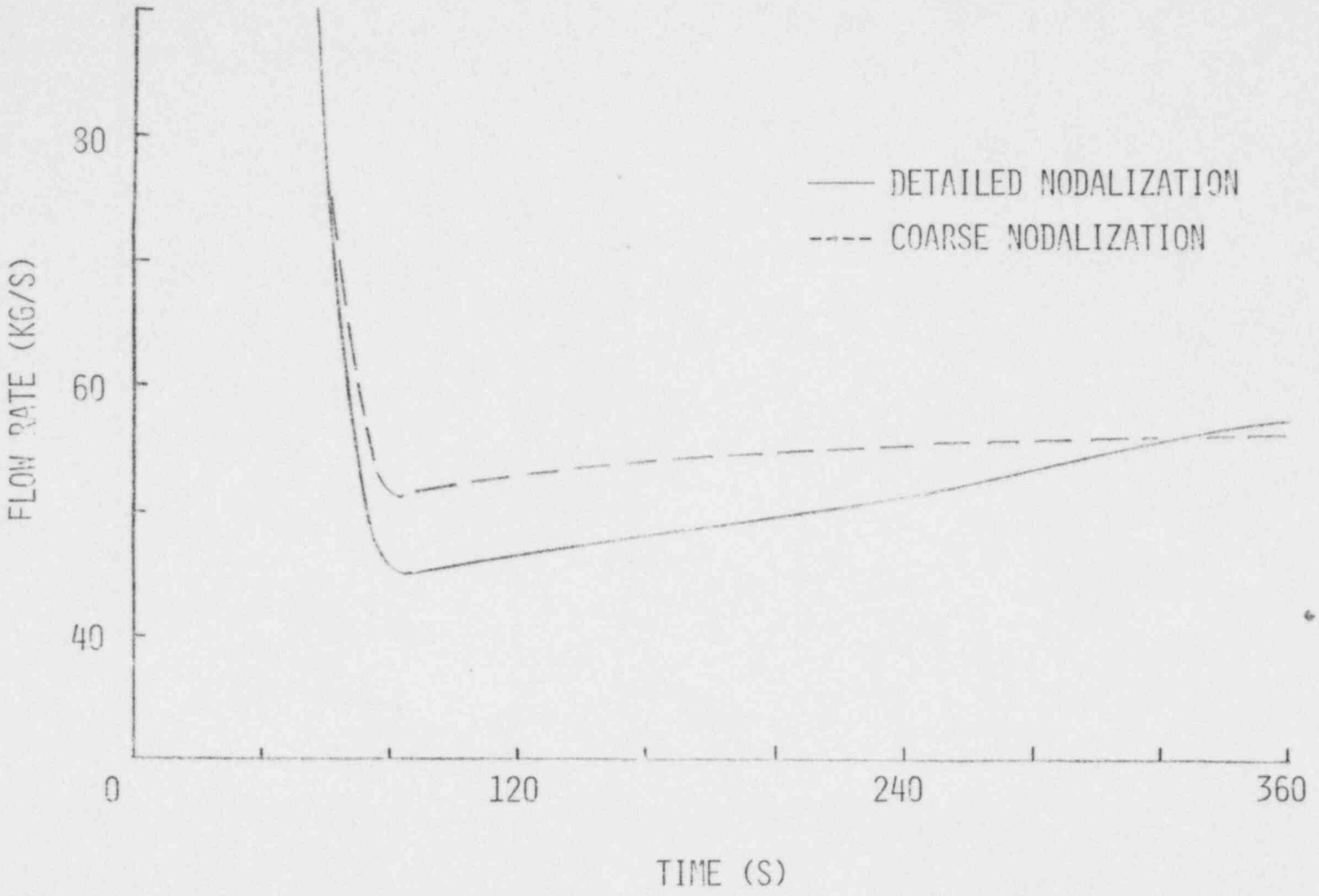
NATURAL CIRCULATION (LOEP) EVENT

- TO SHOW IMPACT OF SYSTEM NODALIZATION ON RESULTS
- CRBRP PROTOTYPIC
- PRIMARY SYSTEM DETAIL

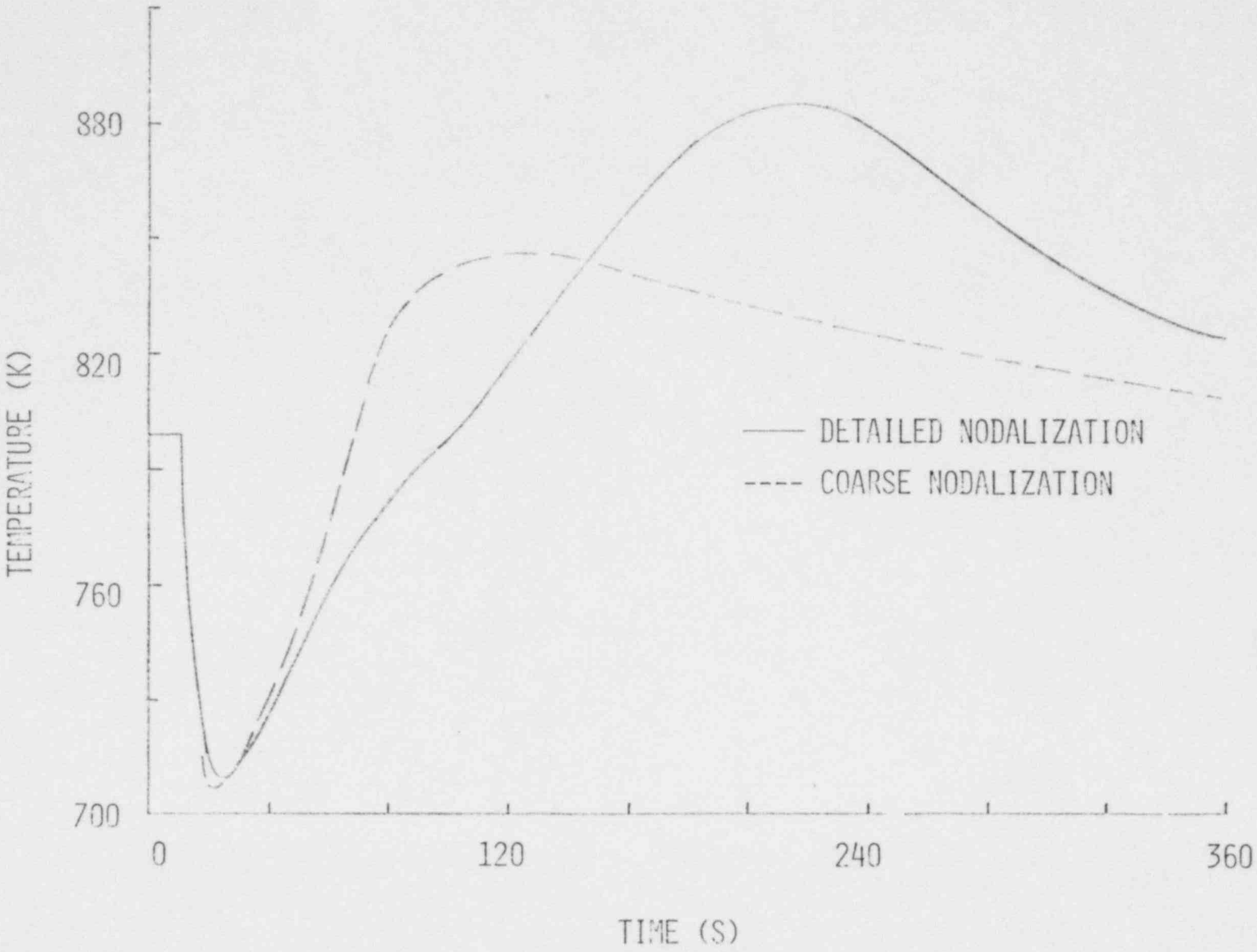
COMPONENT	NODALIZATION	
	DETAILED	COARSE
CORE	18	1
PIPE 1	18	1
PIPE 2	7	1
IHX	40	5
PIPE 3	5	1
PIPE 4	15	1
TOTAL	103	10

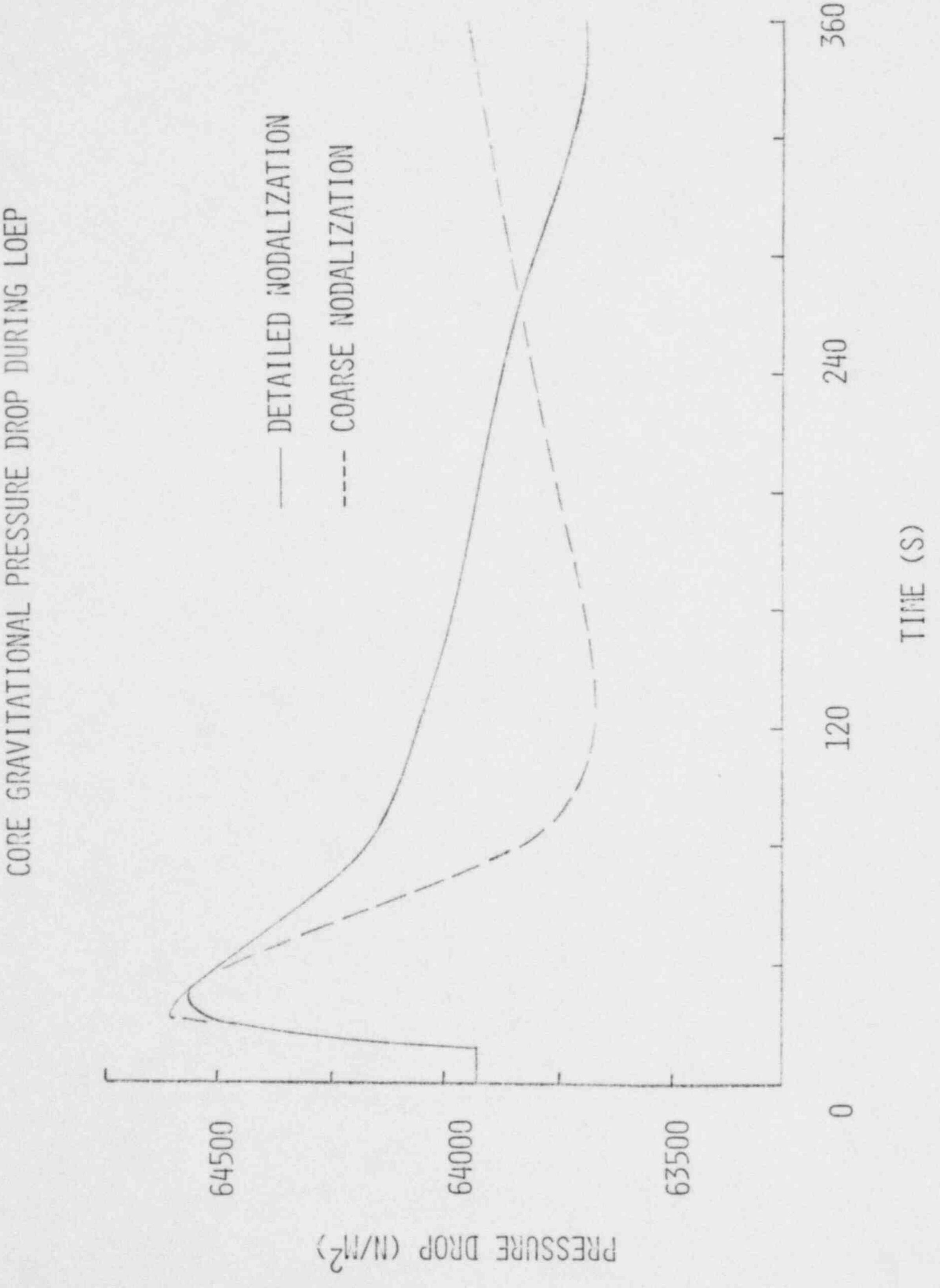
- COMPUTER TIMING REQUIREMENTS, CDC-7600, 360 SECOND SIMULATION
  DETAILED - 226 SECONDS
  COARSE - 141 SECONDS

CRBRP PRIMARY LOOP FLOW RATE RESPONSE TO LOEP

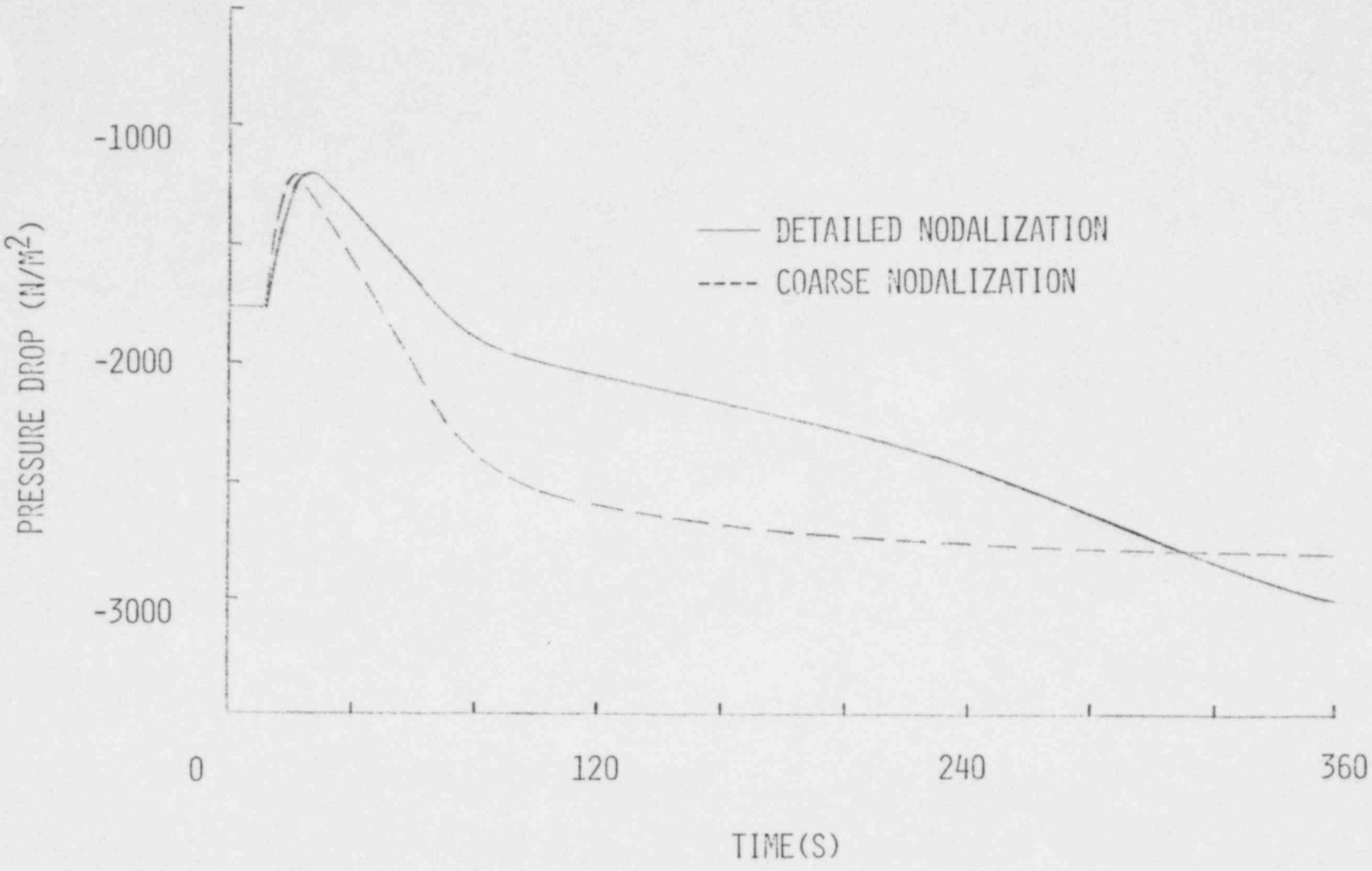


PEAK COOLANT TEMPERATURE RESPONSE TO LOEP



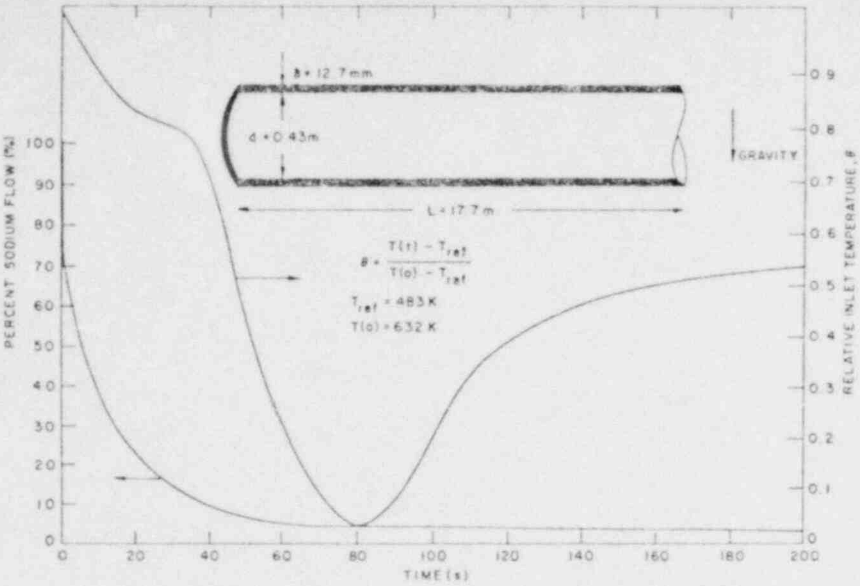

CORE GRAVITATIONAL PRESSURE DROP DURING LOEP
— DETAILED NODALIZATION
---- COARSE NODALIZATION
PRESSURE DROP (N/M²)
64500
64000
63500
0
120
240
360
TIME (S)

NET GRAVITATIONAL HEAD DURING LOEP

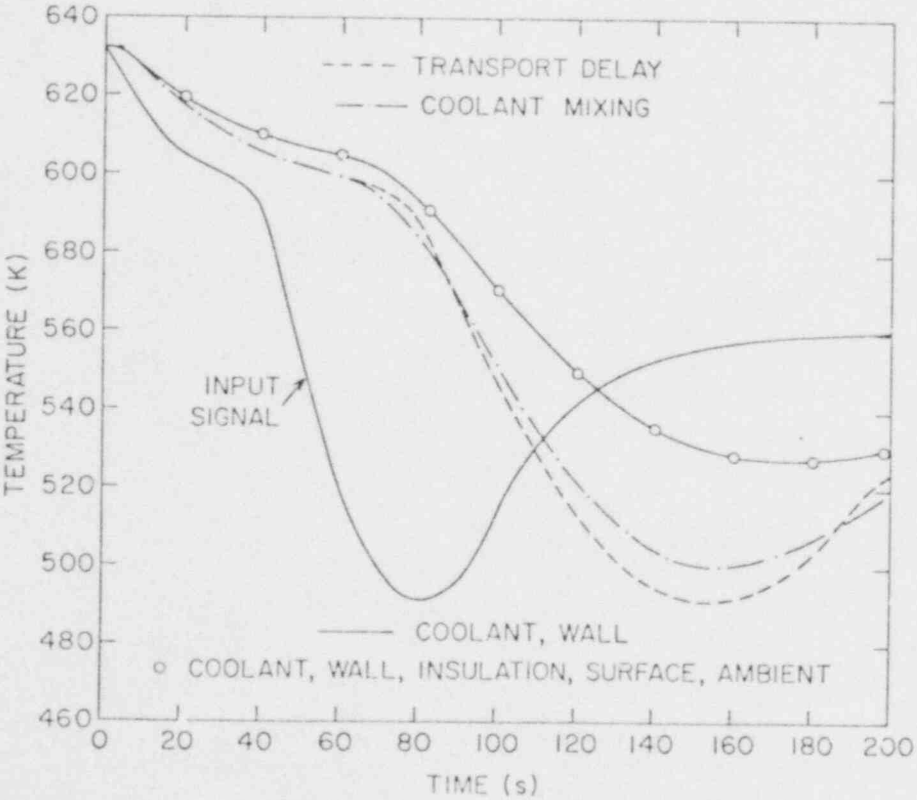


MULTI-DIMENSIONAL EFFECTS

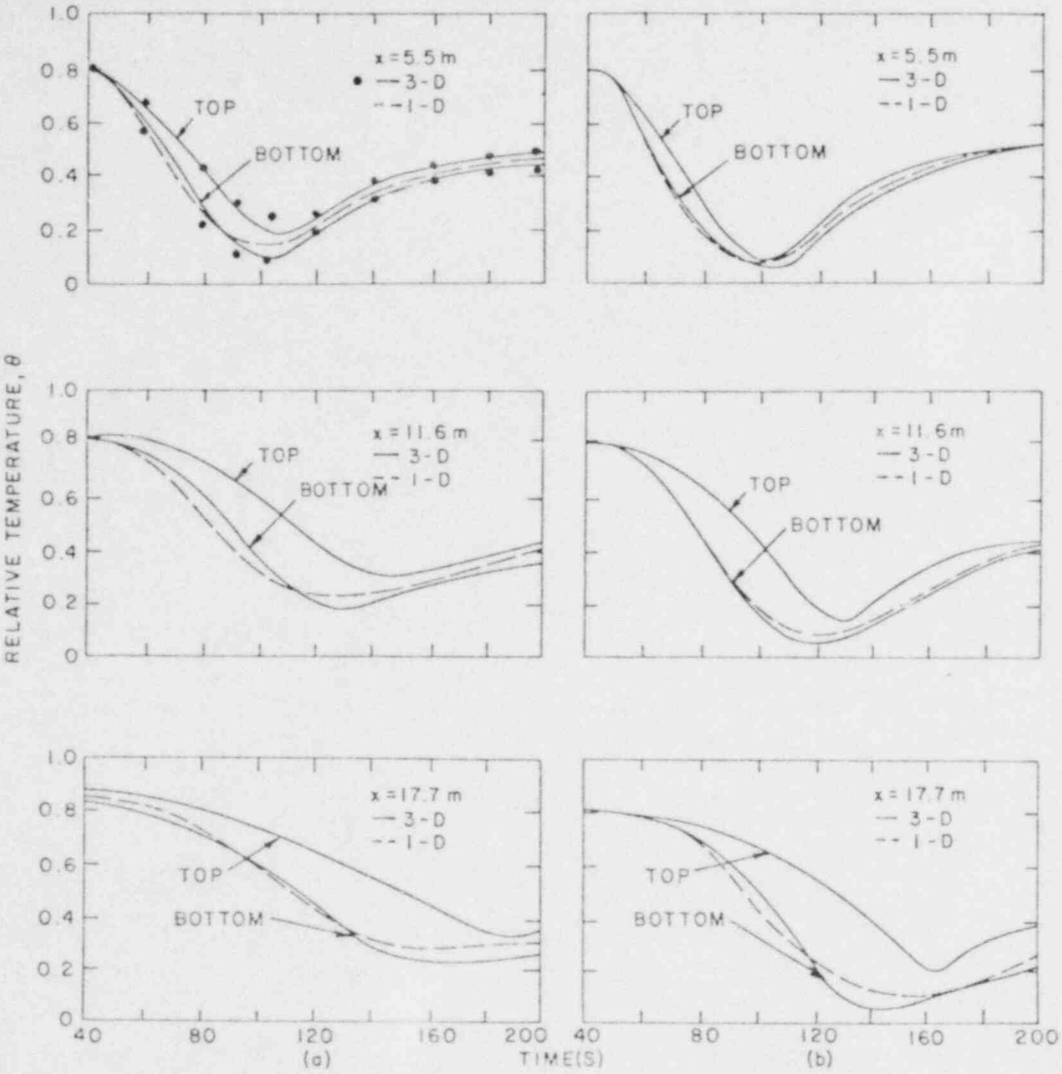
- 1-D vs. 3-D
- SECTION OF PIPE
- SEVERE TRANSIENT IMPOSED
- CAN USE 1-D RESULTS TO INDICATE WHEN 3-D MAY BE NECESSARY



Transient Inlet Flow and Temperature Forcing Functions



Pipe Section Outlet Temperature Response Using Various Thermal Transport Models



Temperature Time Histories at Various Axial Positions and Comparisons to the 3-D Calculations for (a) Conducting Pipe Wall, and (b) Adiabatic Pipe Wall (Note: ● represents the Commix-1A results)

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Transport numbers influencing heat and momentum transport are:

$$Re,\ Gr,\ Pr$$

and other combinations:

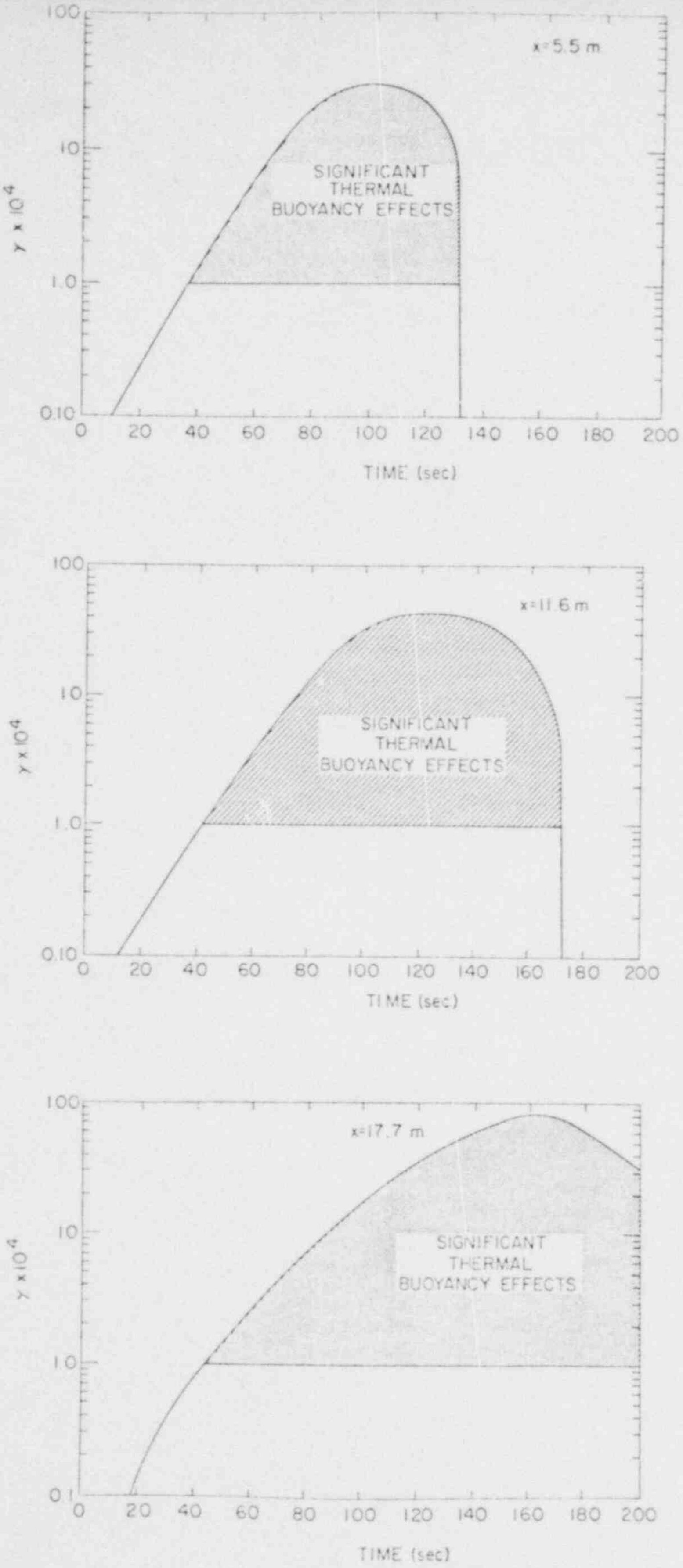
$$Ri = Gr/Re^2 = \frac{\text{BUOYANCY FORCE}}{\text{MOMENTUM FLUX}}$$

For turbulent flow inside round tubes, Jackson and Fewster derived

$$\gamma = \frac{Ri}{Re^{.625} Pr^{0.5}}$$

Available data, (horizontal and vertical pipes) showed the effect of buoyancy to be 10% or more if

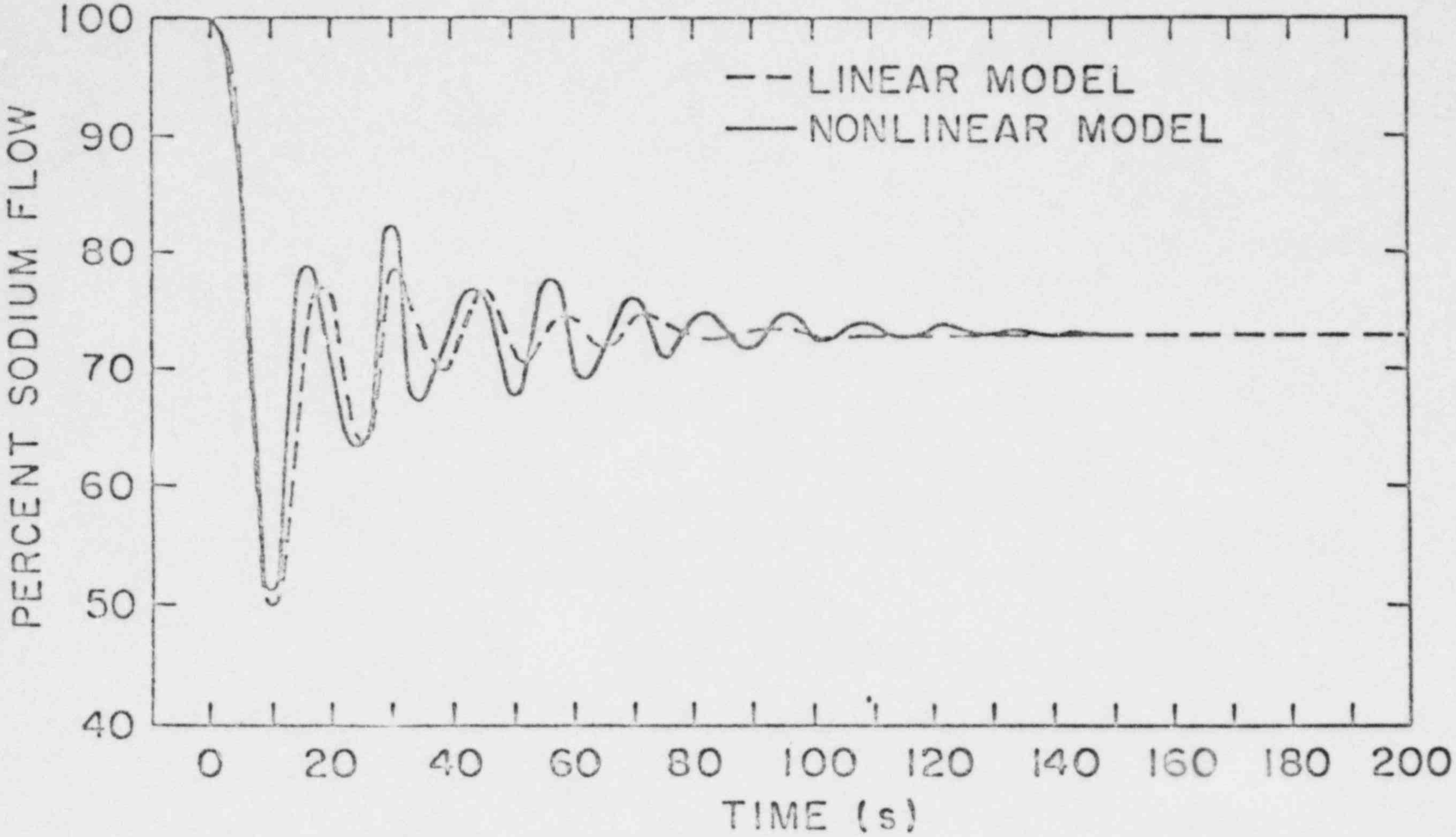
$$\gamma \geq 0(10^{-4})$$



Period of Significant Thermal Buoyancy At Various Axial Positions

PPS/PCS SIMULATION

- GENERIC MODELS FOR PPS FUNCTIONS AVAILABLE (SPECIFICATIONS THROUGH INPUT)
- PRIMARY AND SECONDARY REACTOR SHUTDOWN SYSTEMS
- MULTIPLE CONTROL ROD BANKS MODELED
- PUMP CONTROL
- FEEDWATER CONTROL
- TURBINE CONTROL VALVE CONTROL
- TURBINE BYPASS



FLOW RATE RESPONSE TO 20% STEP CHANGE IN LOAD DEMAND

SAMPLE PROBLEM

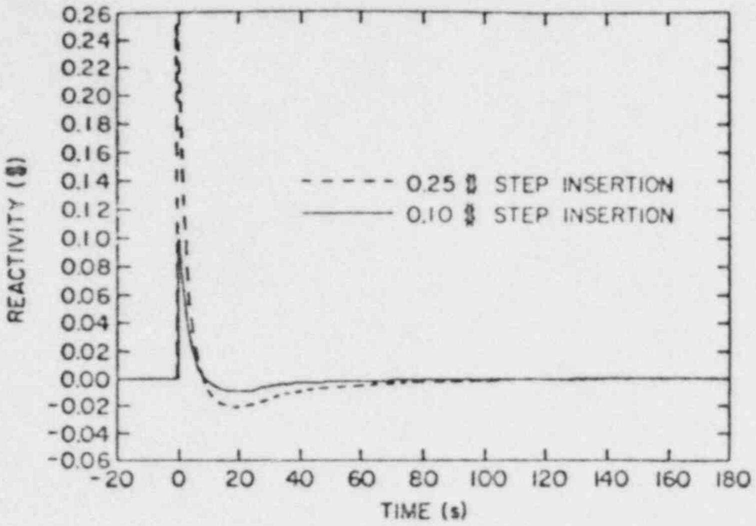
1 LOOP SIMULATION OF CRBRP
5 CHANNEL CORE
DETAILED NODALIZATION

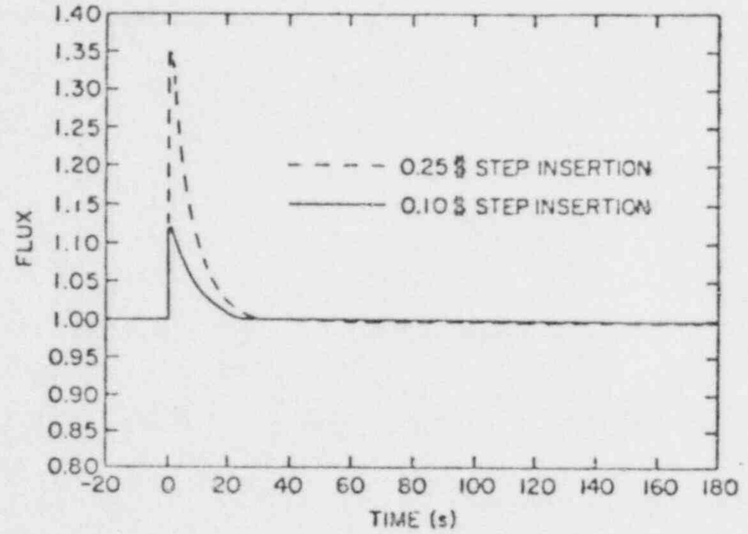
TRANSIENTS

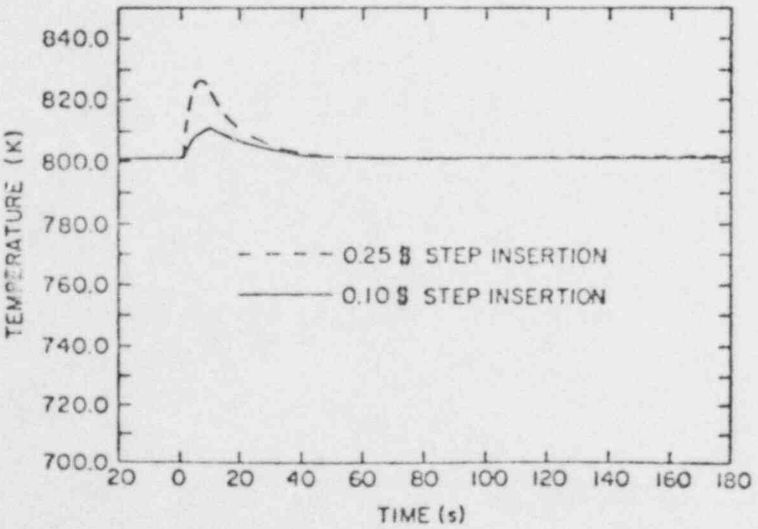
- 10¢ STEP INSERTION
- 25¢ STEP INSERTION
- 10% RAMP CHANGE IN LOAD IN 40 SECONDS

PLANT PROTECTION SYSTEM WAS INACTIVATED

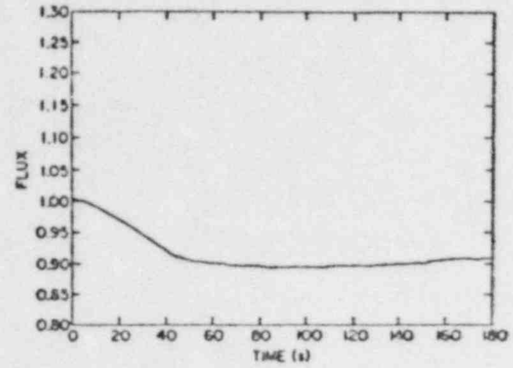
REACTIVITY INSERTION TRANSIENTS

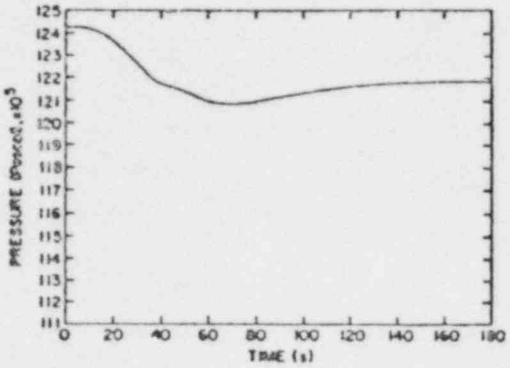


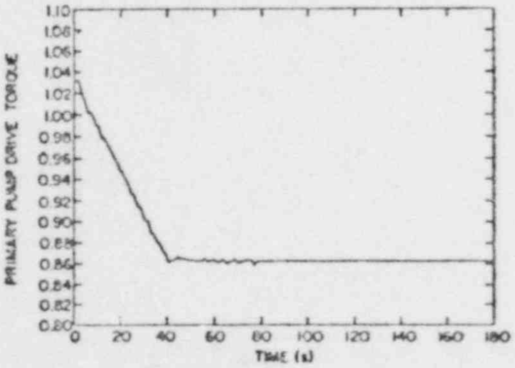


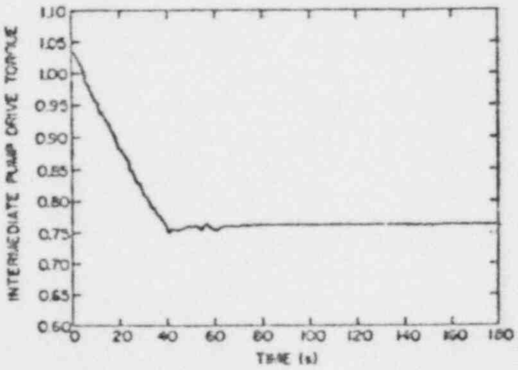


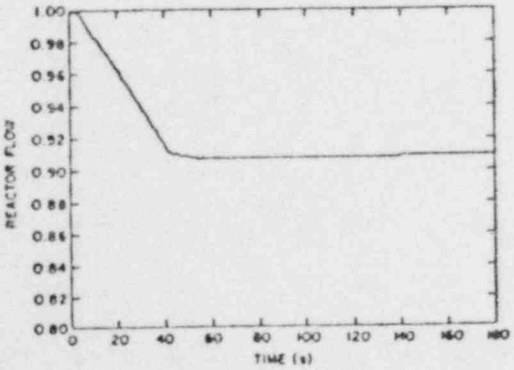
10% RAMP CHANGE IN LOAD

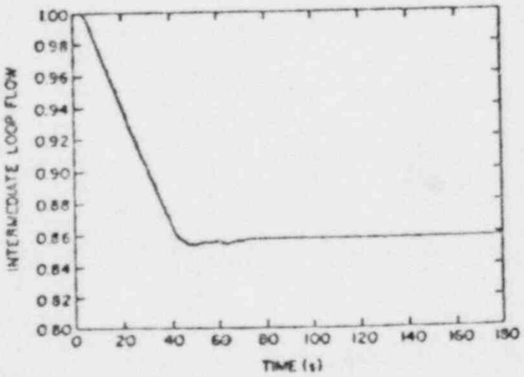












FUTURE PLANS

- RUN MATRIX APPLICATIONS AND DOCUMENTATION
- COMPLETE PPS/PCS INTERFACING AND DOCUMENTATION
- IMPROVED STRATIFICATION MODEL
- COMPLETION OF SODIUM BOILING MODELING
- DRIFT FLUX MODEL INTO SG SECONDARY SIDE
- ADD SAFETY RELATED AUXILIARY HEAT REMOVAL SYSTEM MODELS
- MEANS OF ACCOUNTING FOR INTRA- AND INTER-ASSEMBLY HEAT TRANSFER EFFECTS
- DEVELOPMENT OF SYSTEM-WIDE ACCIDENT DELINEATION APPLICATIONS
- CONTINUE USER SUPPORT

SUMMARY

- VERSIONS OF SSC-L, SSC-P, SSC-W ARE OPERATIONAL
- WIDE RANGE OF CODE APPLICATIONS UNDERWAY
- SSC-S EFFORT WELL UNDERWAY
- CODE VALIDATION PROCEEDING
- USER SUPPORT CONTINUING
- MODEL IMPROVEMENTS/EXTENSIONS BEING IMPLEMENTED

3D-CODE DEVELOPMENT FOR CORE THERMAL HYDRAULIC ANALYSIS

CONTRIBUTED BY

J. G. BARTZIS, B. C-J. CHEN, H. M. DOMANUS, J. L. KRAZINSKI,
C. C. MIAO, W. T. SHA AND V. L. SHAH

PRESENTED BY

W. T. SHA

AT

ACRS MEETING ON JULY 9, 1980
WASHINGTON, D. C.

COMMIX CODE

COMMIX-1: THREE-DIMENSIONAL, TRANSIENT, SINGLE-PHASE COMPRESSIBLE FLOW WITH HEAT TRANSFER

COMMIX-2: THREE-DIMENSIONAL, TRANSIENT, TWO-FLUID MODEL (LIQUID AND VAPOR) WITH NON-EQUILIBRIUM TEMPERATURES AND INHOMOGENEOUS VELOCITIES

COORDINATES: CARTESIAN (XYZ) AND CYLINDRICAL (RZθ)

FORMULATION: POROUS MEDIUM APPROACH -VOLUME POROSITY SURFACE PERMEABILITY AND DISTRIBUTED RESISTANCE AND HEAT SOURCE

NUMERICAL TECHNIQUE:

- STAGGERED MESH
- IMPLICIT MULTIFLUID (IMF) SCHEME WITH REBALANCE
- SEMI IMPLICIT PRESSURE LINKED EQUATIONS REVISED (SIMPLER) WITH REBALANCE

APPLICATIONS:

- CONTINUUM: REACTOR INLET AND OUTLET PLENUM, PIPING, ...
- QUASI-CONTINUUM: FUEL ASSEMBLY, IHX, STEAM GENERATOR, REACTOR INLET AND OUTLET PLENUM

....

BODYFIT CODE

BODYFIT-1: THREE-DIMENSIONAL, TRANSIENT, SINGLE-PHASE COMPRESSIBLE FLOW WITH HEAT TRANSFER

BODYFIT-2: THREE-DIMENSIONAL, TRANSIENT, TWO-FLUID MODEL (LIQUID AND VAPOR) WITH NON-EQUILIBRIUM TEMPERATURES AND INHOMOGENEOUS VELOCITIES

FORMULATION: BOUNDARY FITTED COORDINATE TRANSFORMATION APPROACH

NUMERICAL TECHNIQUE:

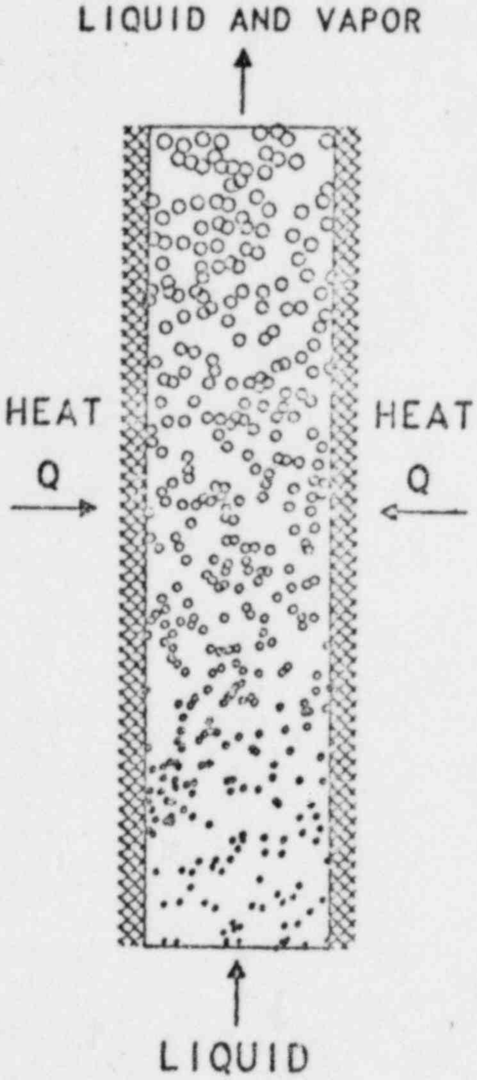
- MODIFIED STAGGERED MESH
- IMPLICIT MULTIFLUID (IMF) SCHEME
  WITH REBALANCE

APPLICATIONS:

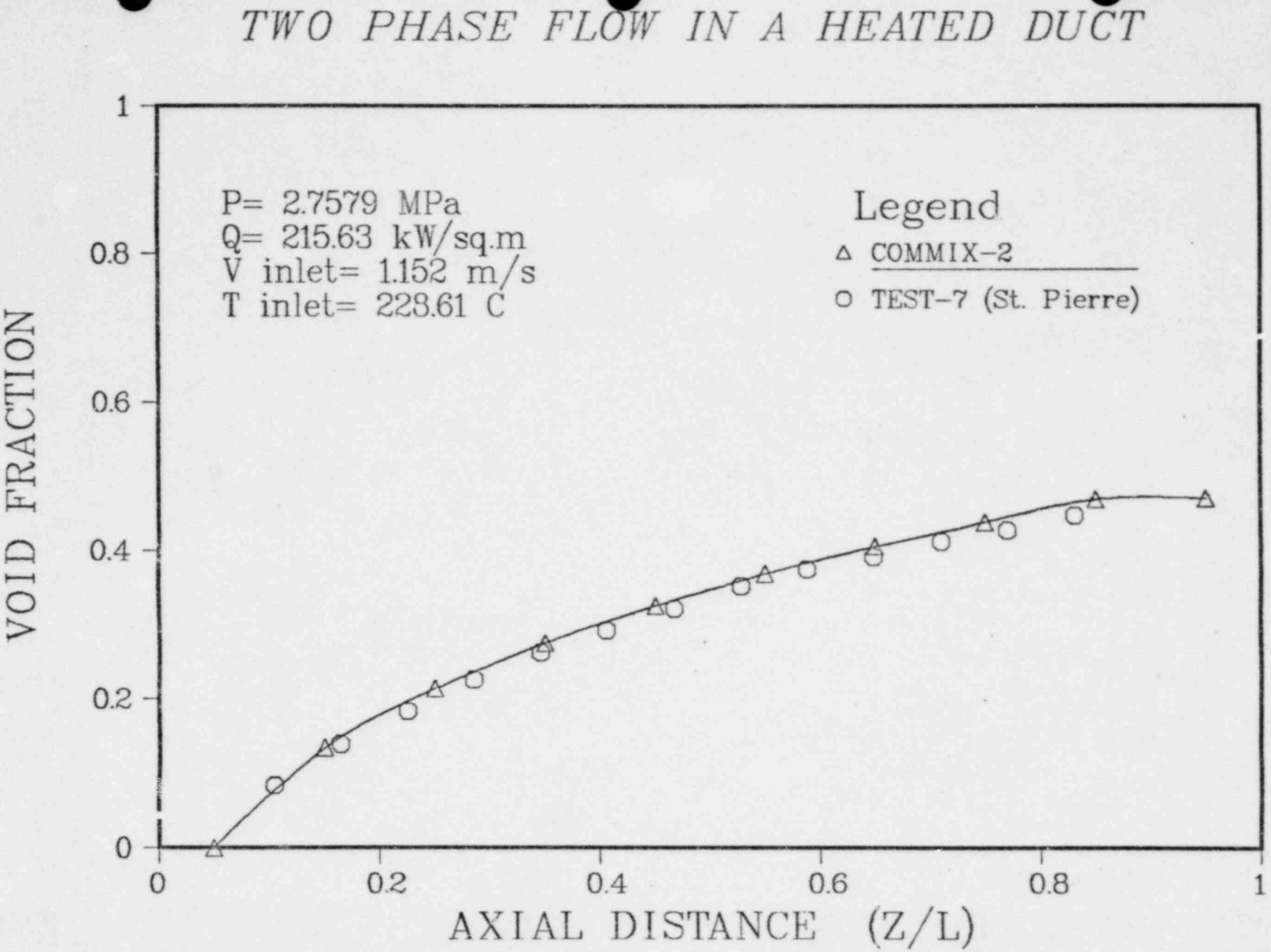
CONTINUUM: FUEL ASSEMBLY AS MULTIPLY
CONNECTED REGION

COMMIX-2 NUMERICAL RESULTS

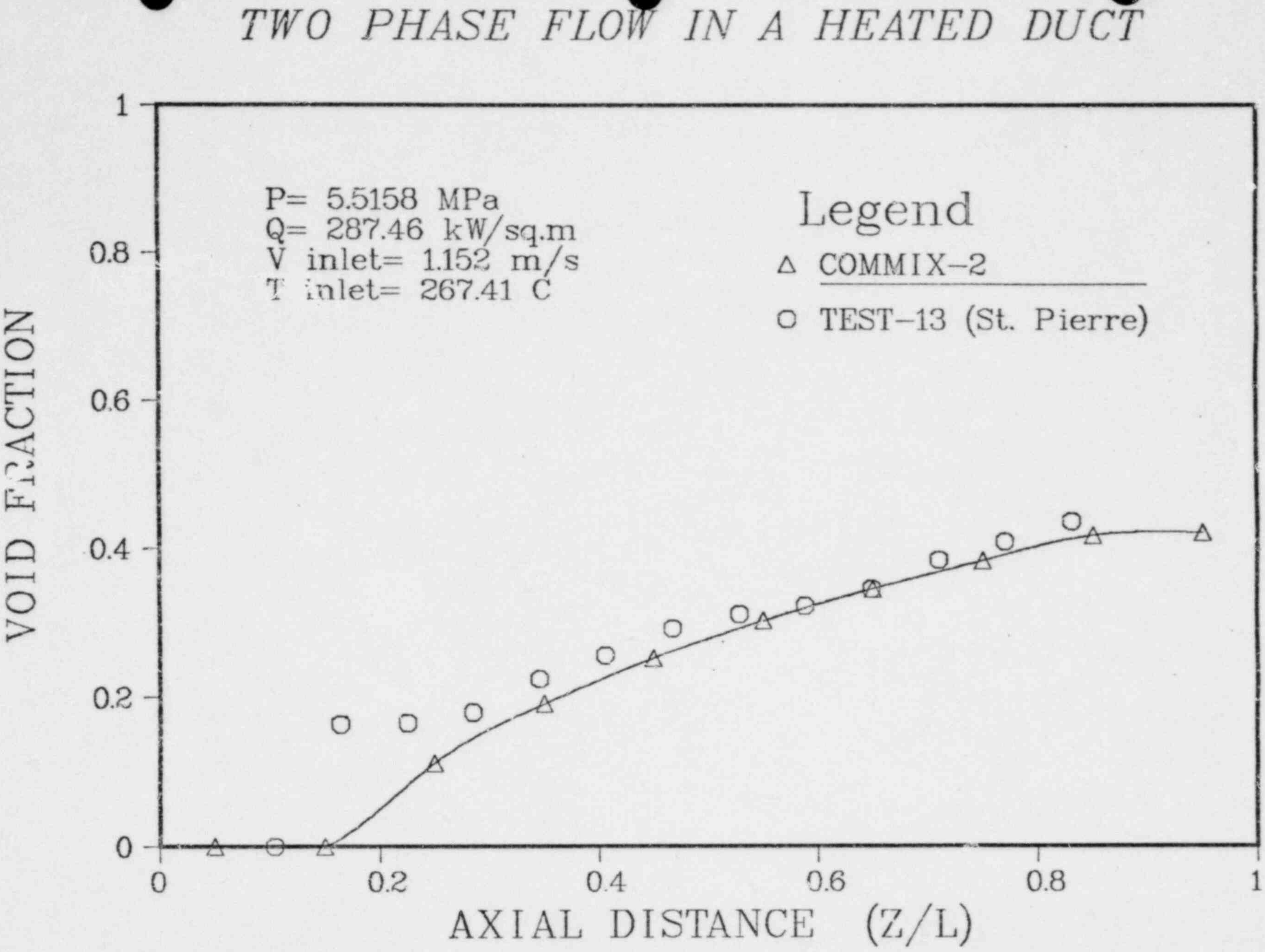
- TWO-PHASE FLOW IN A HEATED DUCT
- NRC STANDARD PROBLEM NO. 1;
  FLASHING DUE TO DEPRESSURIZATION
- FLOW COAST DOWN IN A GERMAN SEVEN PIN
  FUEL ASSEMBLY



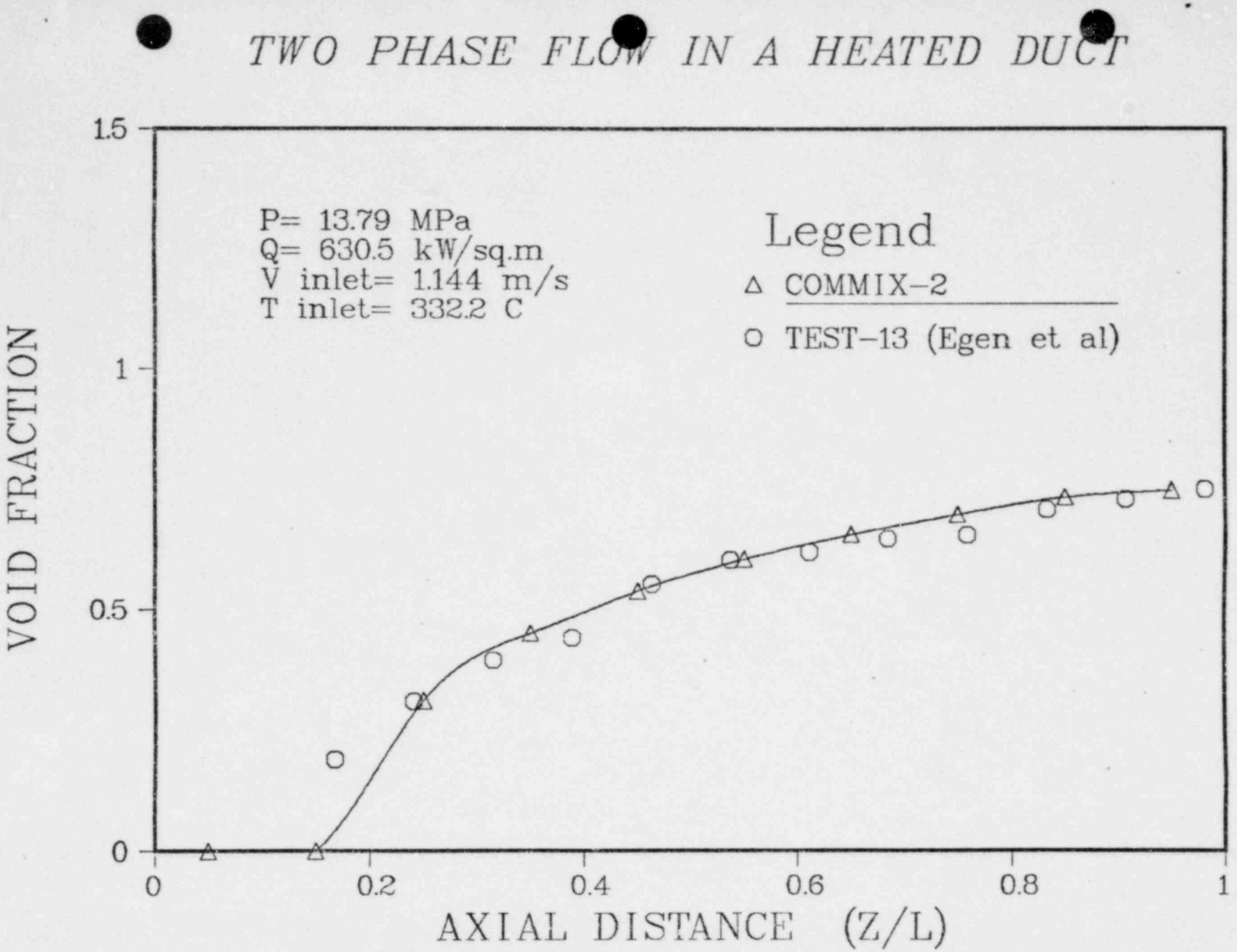
Schematic Layout of two phase flow in a heated duct



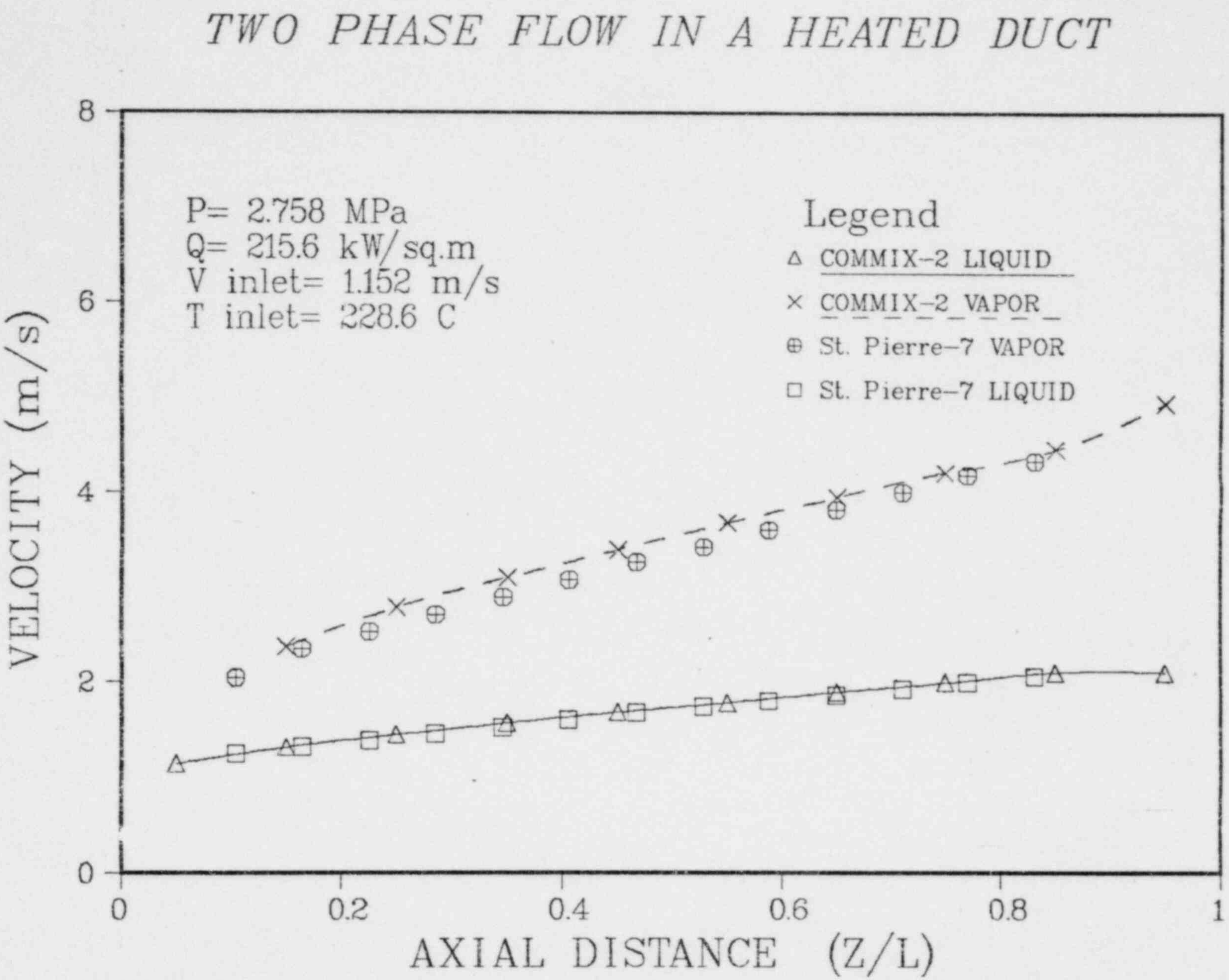
COMPARISON OF THE VOID FRACTION DISTRIBUTION WITH THE MEASUREMENTS
OF ST. PIERRE.



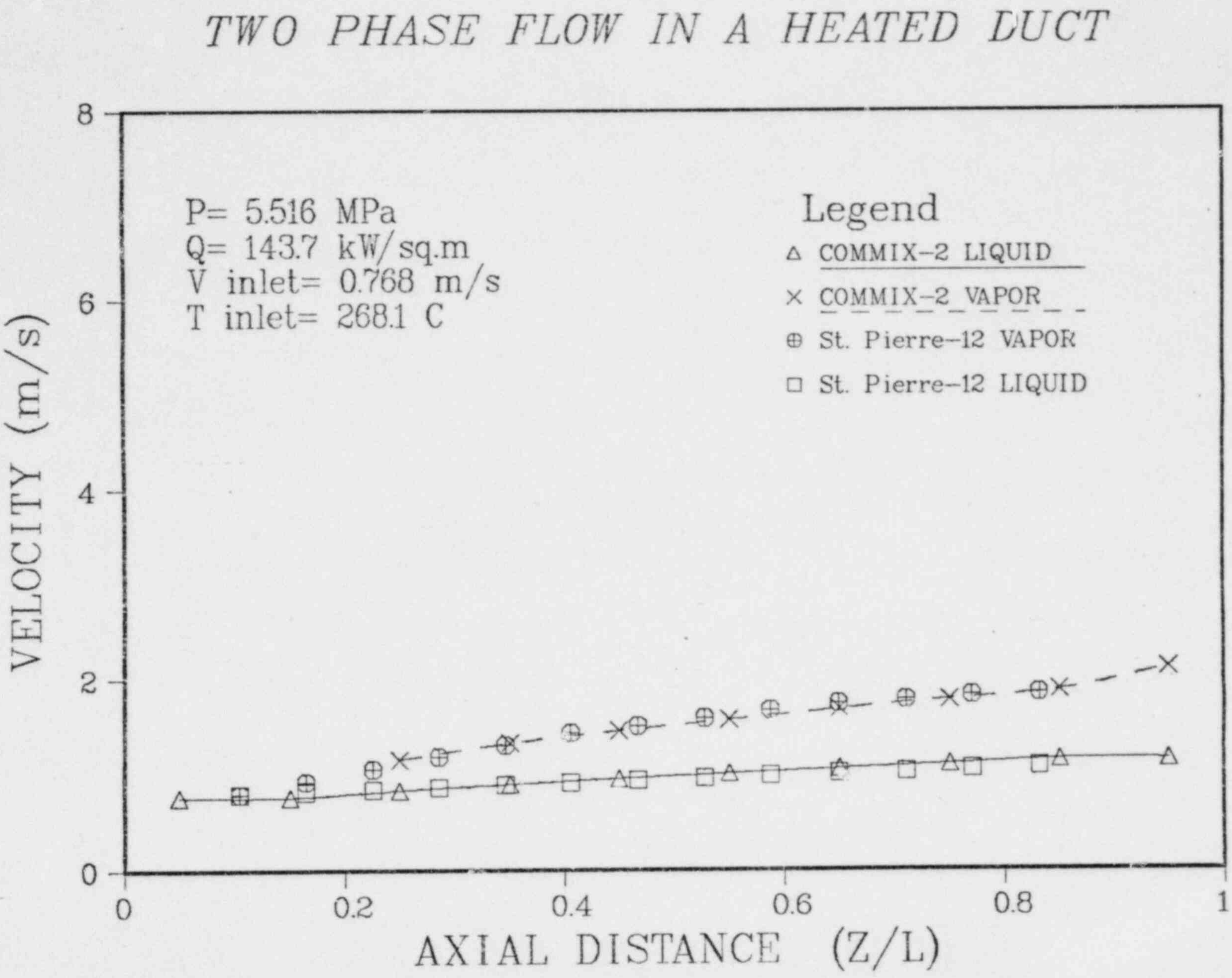
COMPARISON OF THE VOID FRACTION DISTRIBUTION WITH THE MEASUREMENTS OF ST. PIERRE.



COMPARISON OF THE VOID FRACTION DISTRIBUTION WITH THE MEASUREMENTS OF EGEN ET AL.



COMPARISON OF LIQUID AND VAPOR VELOCITY DISTRIBUTION WITH THE MEASUREMENTS OF ST. PIERRE.

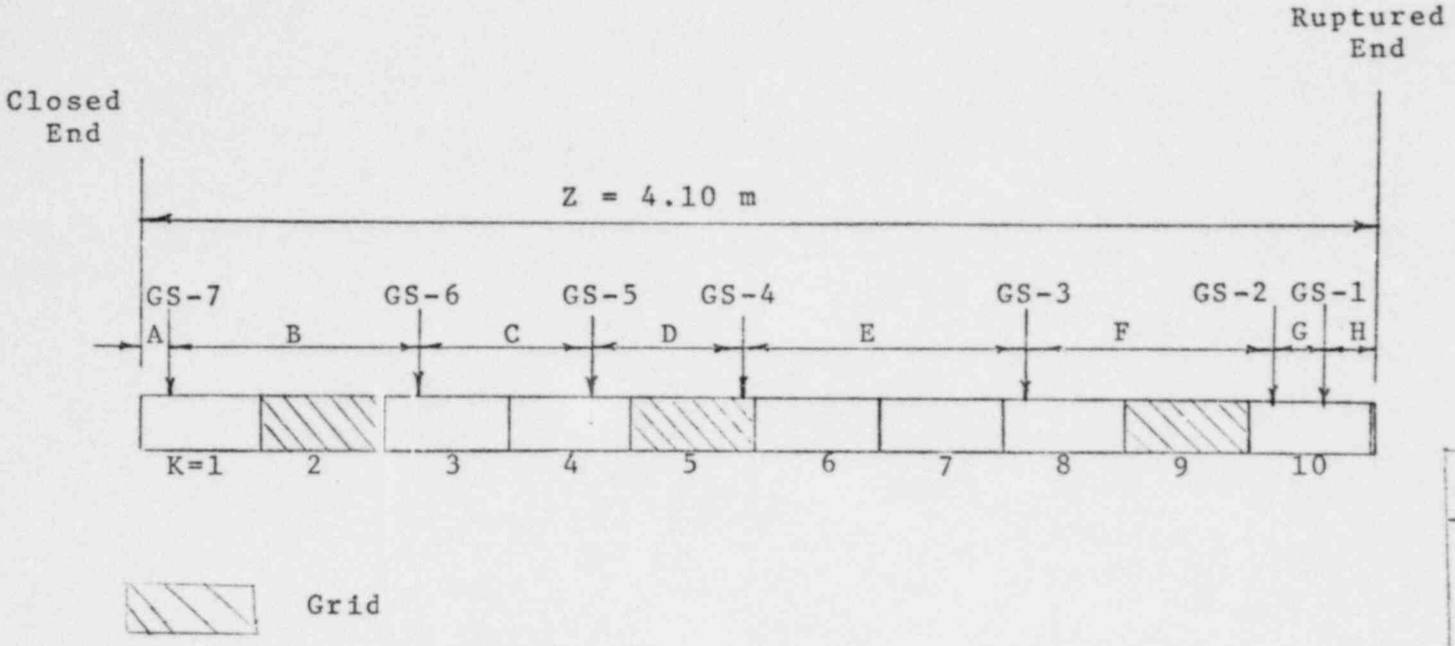


COMPARISON OF LIQUID AND VAPOR VELOCITY DISTRIBUTION WITH THE MEASUREMENTS OF ST. PIERRE.

NRC STANDARD PROBLEM NO. 1

FLASHING DUE TO DEPRESSURIZATION

(TWO-PHASE: TRANSIENT FLOW)

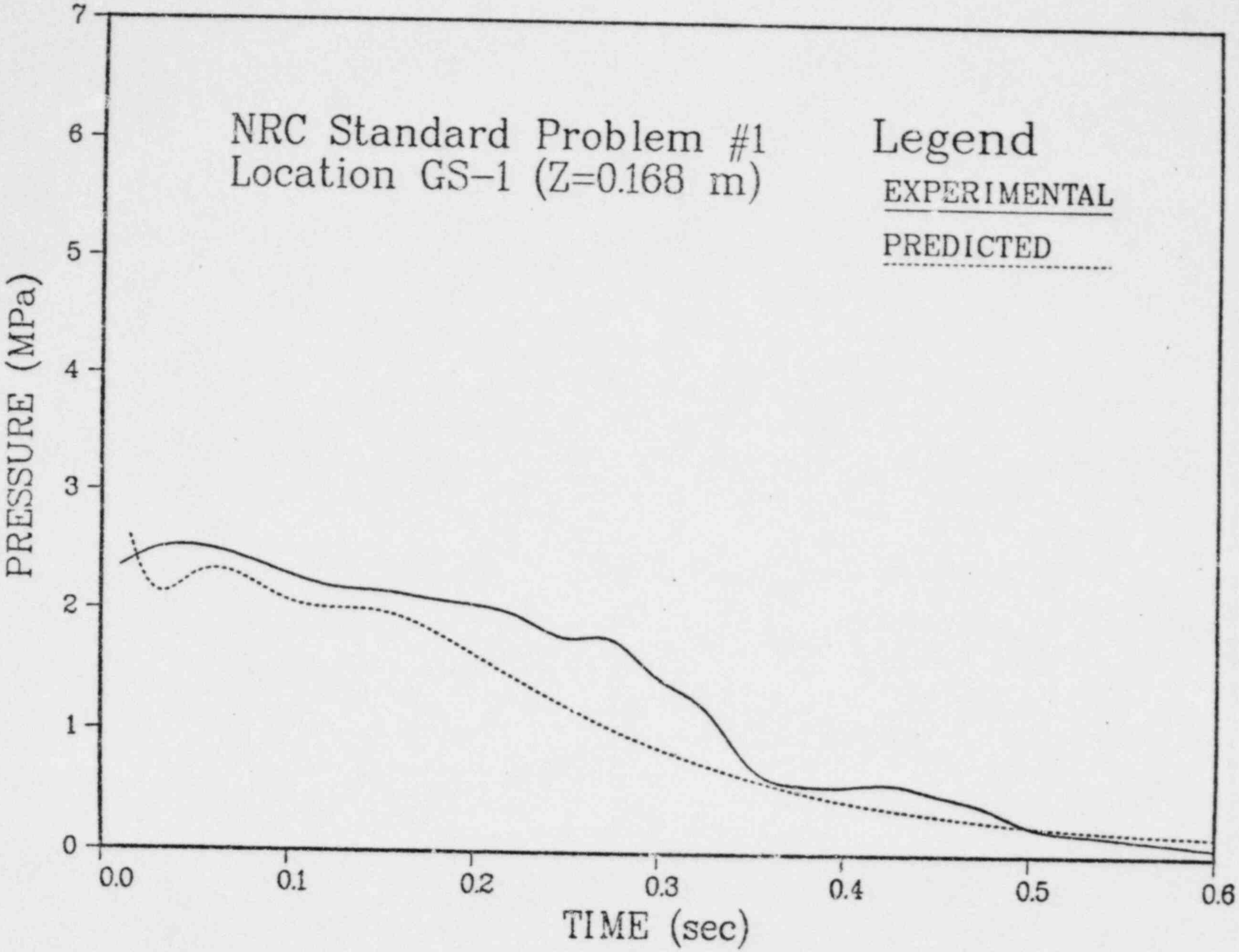


Dimension	m
A	0.079
B	0.835
C	0.555
D	0.555
E	0.911
F	0.835
G	0.158
H	0.168

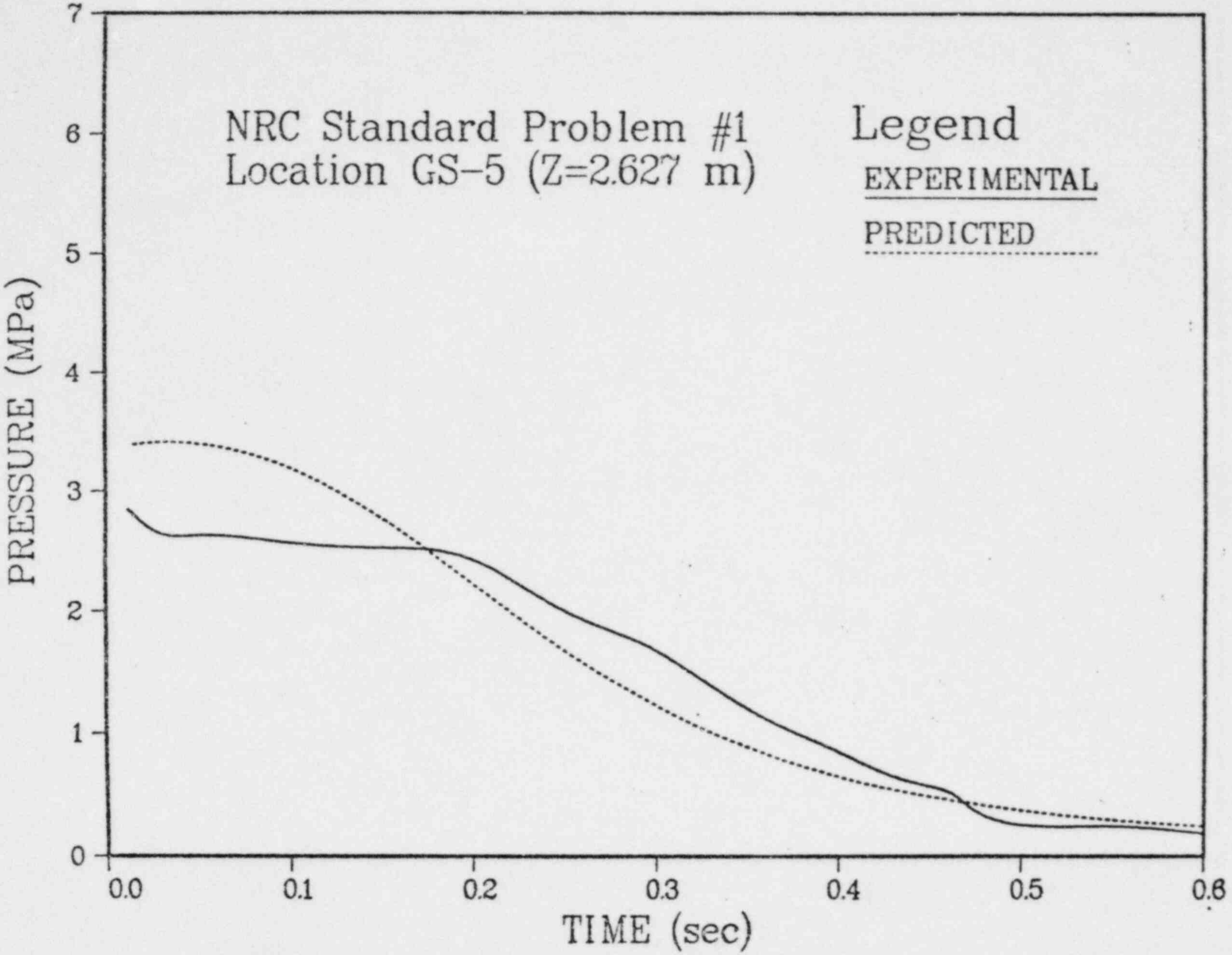
GENERAL ARRANGEMENT OF BLOWDOWN PIPE

(NRC STANDARD PROBLEM #1)

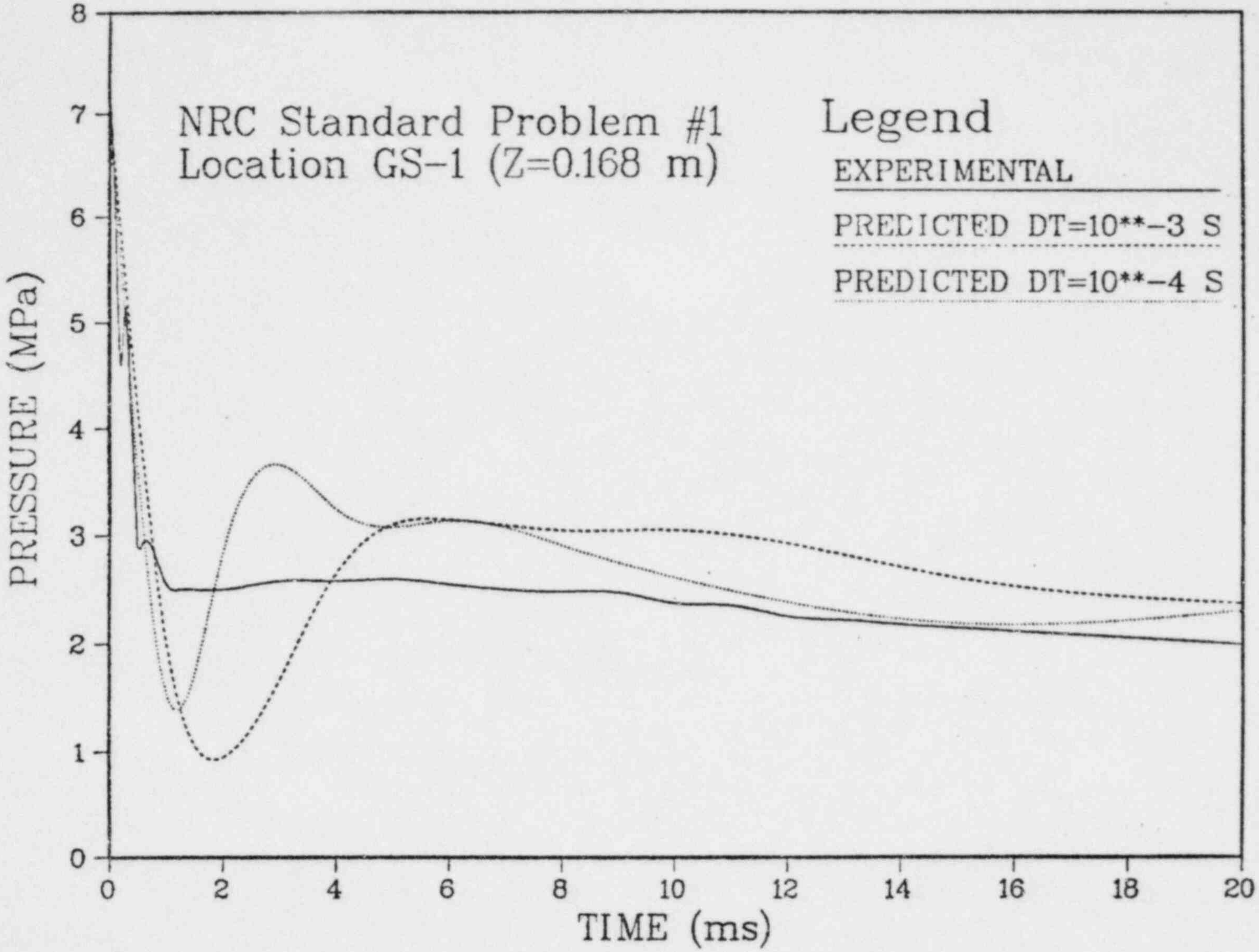
COMPARISON BETWEEN MEASURED AND PREDICTED PRESSURES



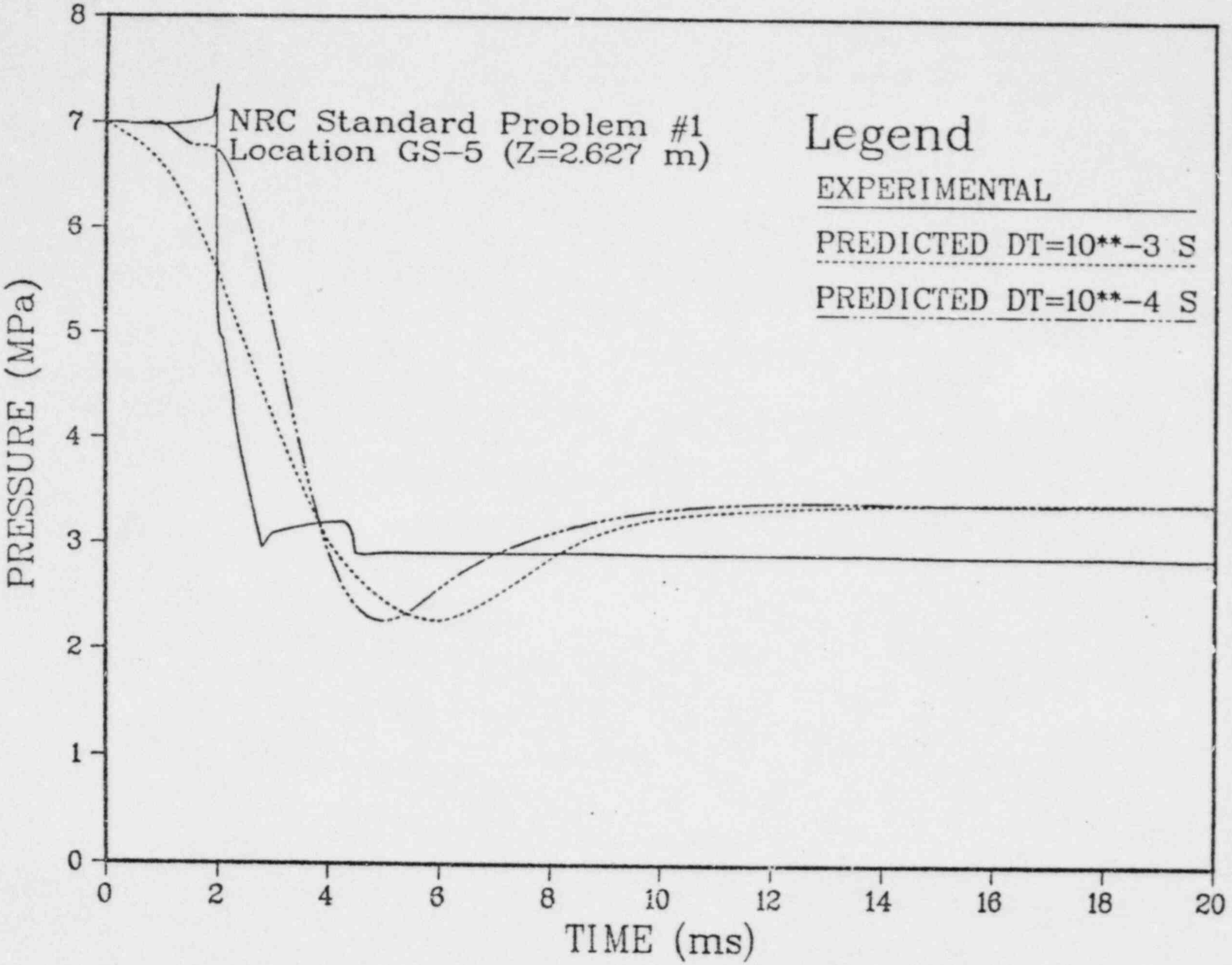
COMPARISON BETWEEN MEASURED AND PREDICTED PRESSURES



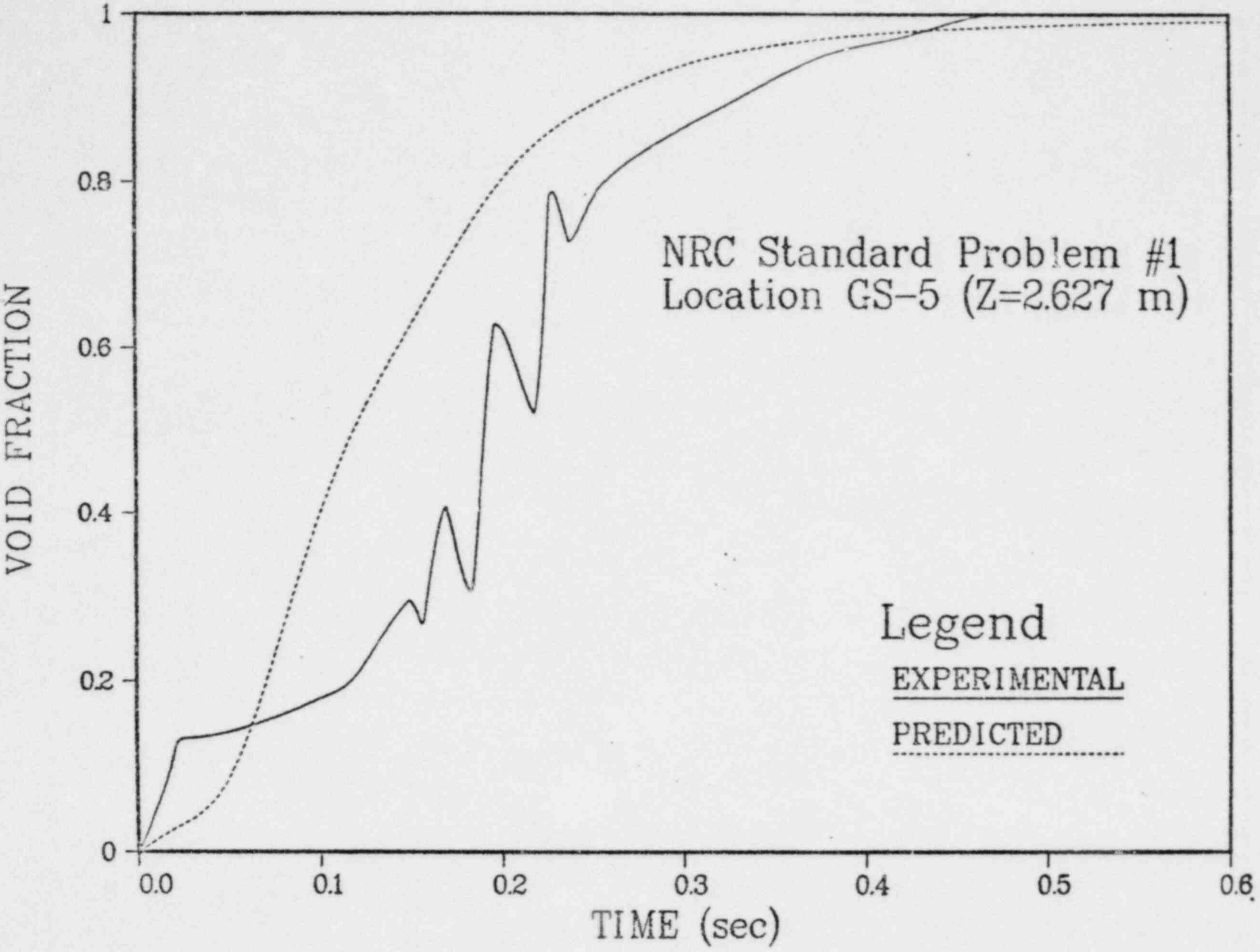
COMPARISON BETWEEN MEASURED AND PREDICTED PRESSURES



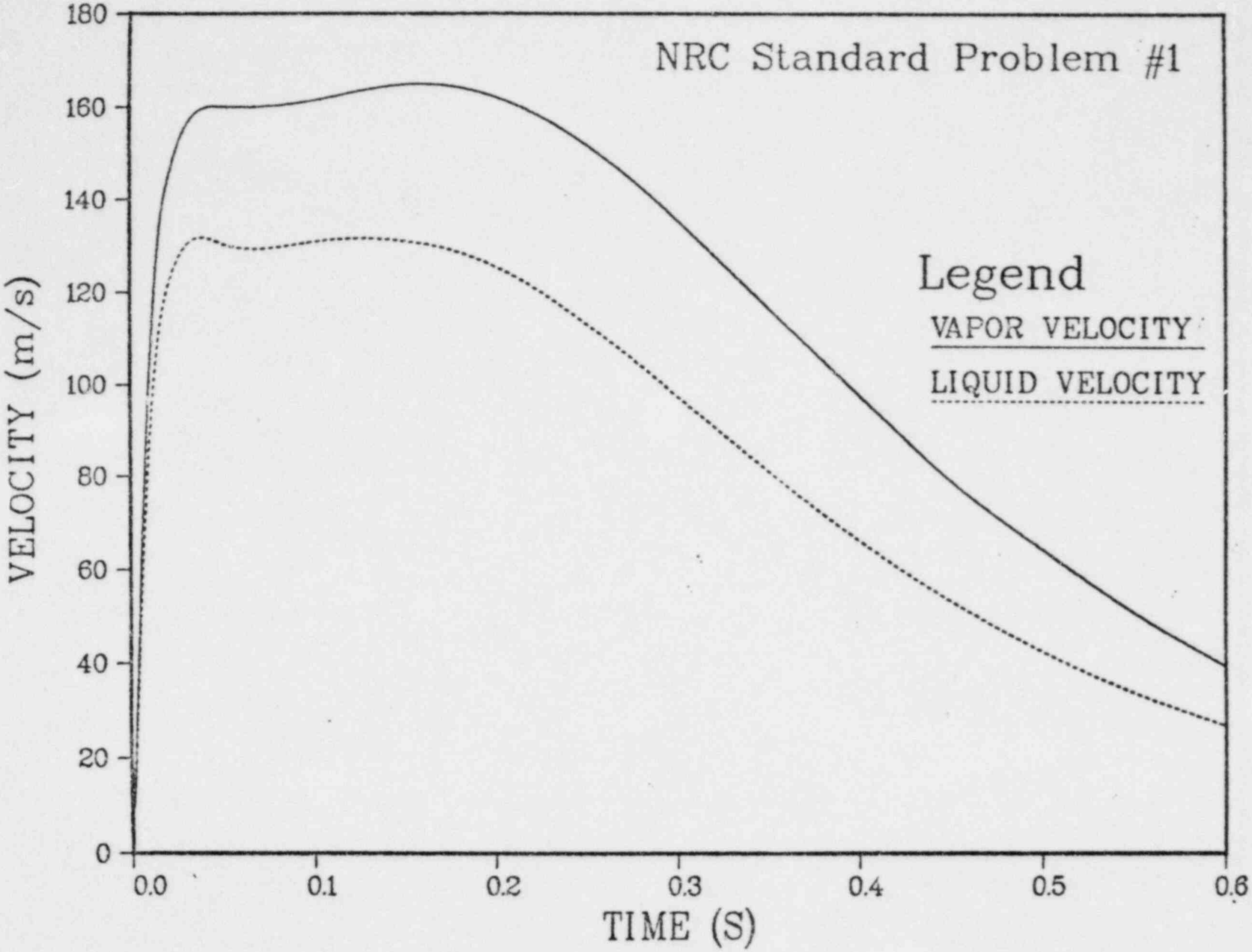
COMPARISON BETWEEN MEASURED AND PREDICTED PRESSURES



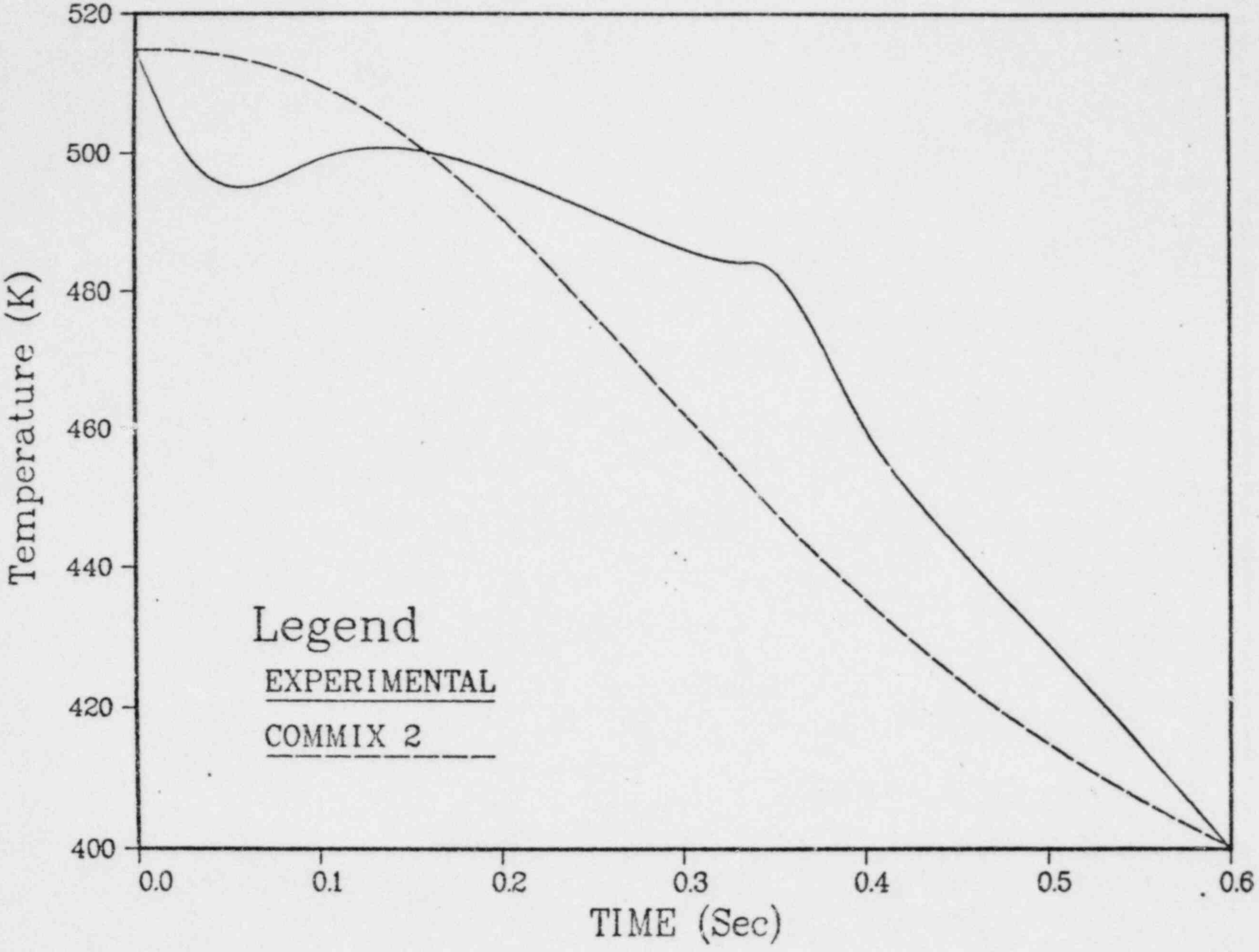
COMPARISON BETWEEN MEASURED AND PREDICTED VOID FRACTIONS



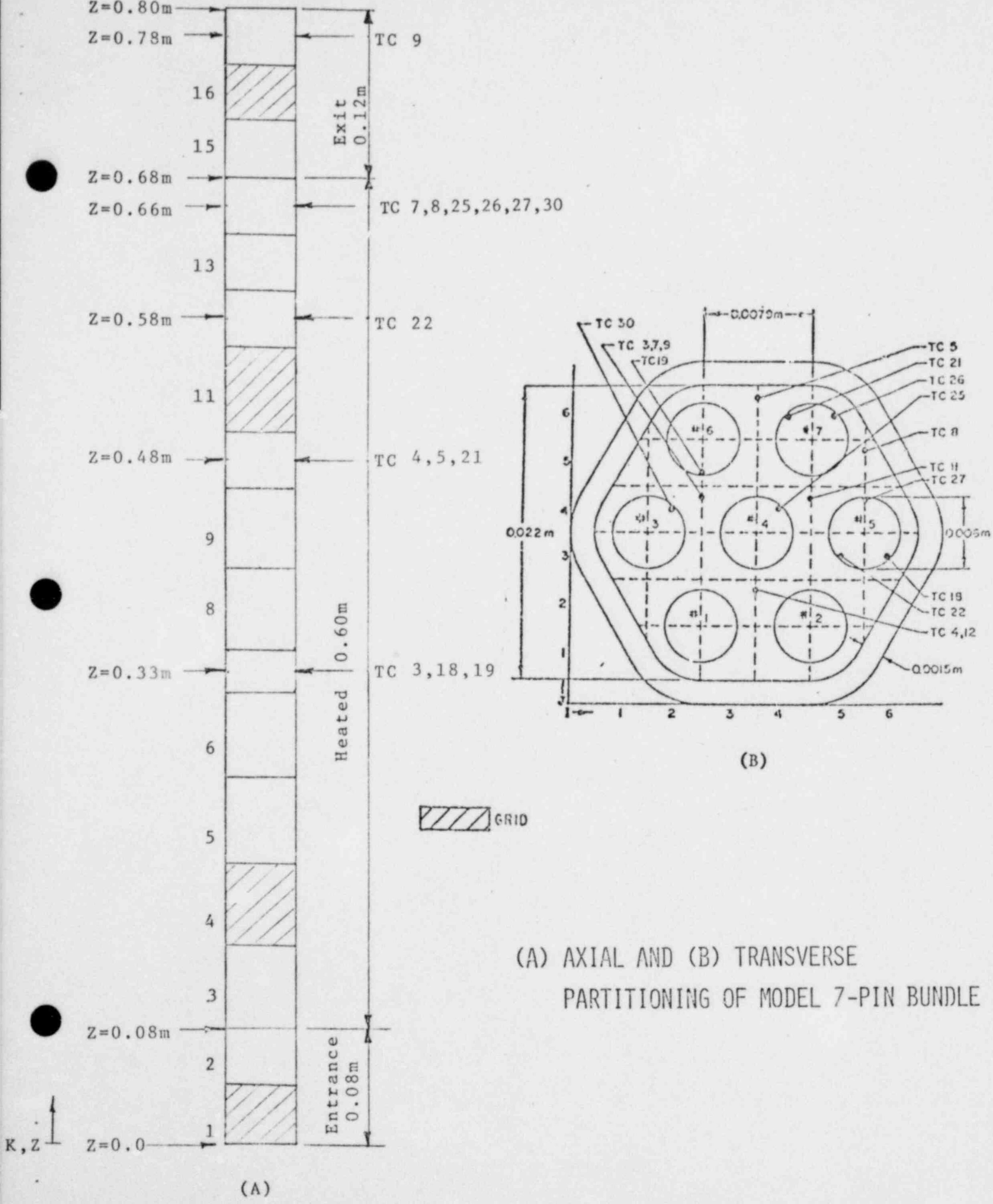
PREDICTED VAPOR AND LIQUID EXIT VELOCITIES



COMPARISON OF PREDICTED LIQUID TEMPERATURE WITH THE MEASURED TEMPERATURE AT GS-5

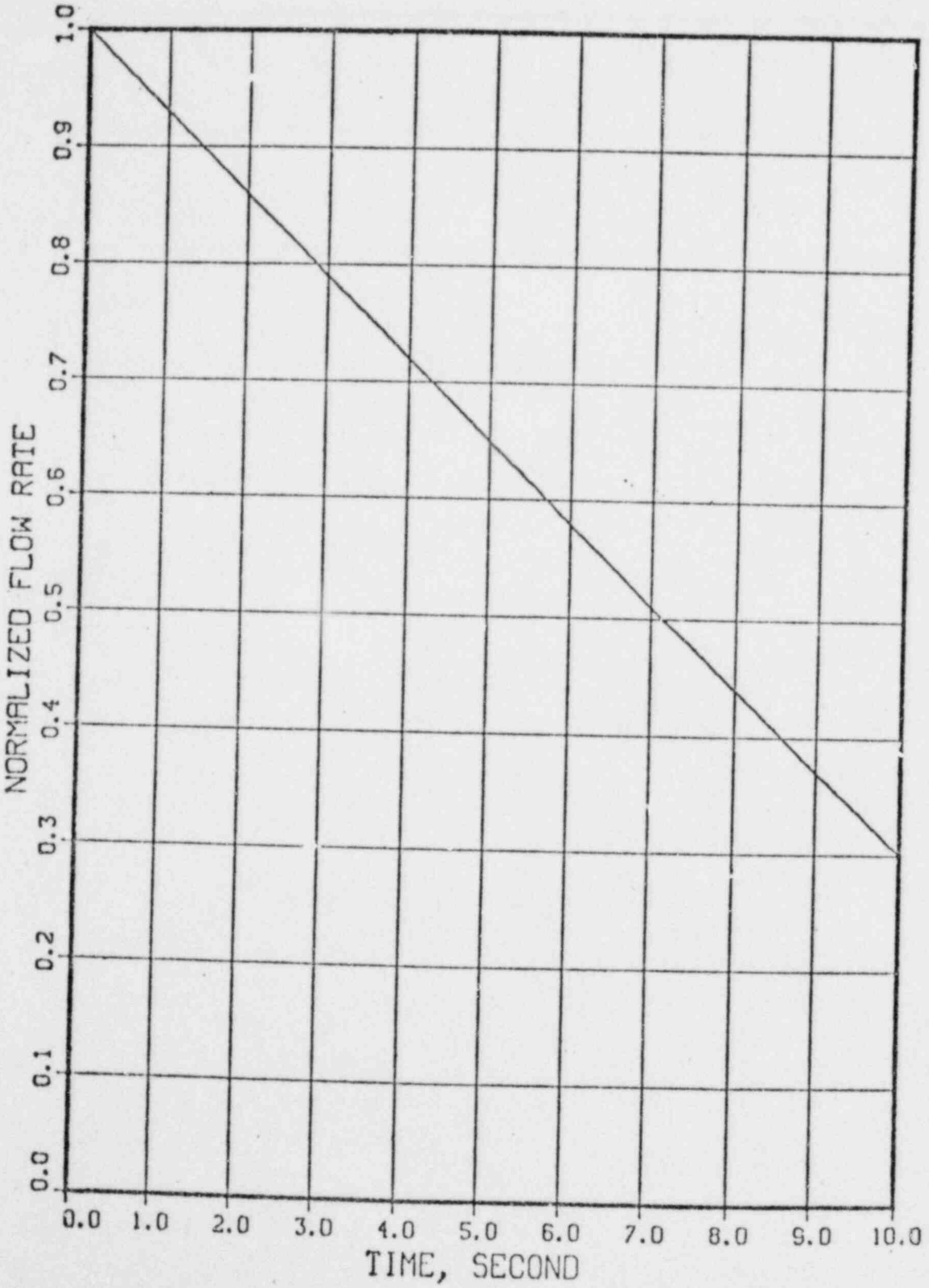


FLOW COAST DOWN IN A GERMAN SEVEN PIN FUEL ASSEMBLY

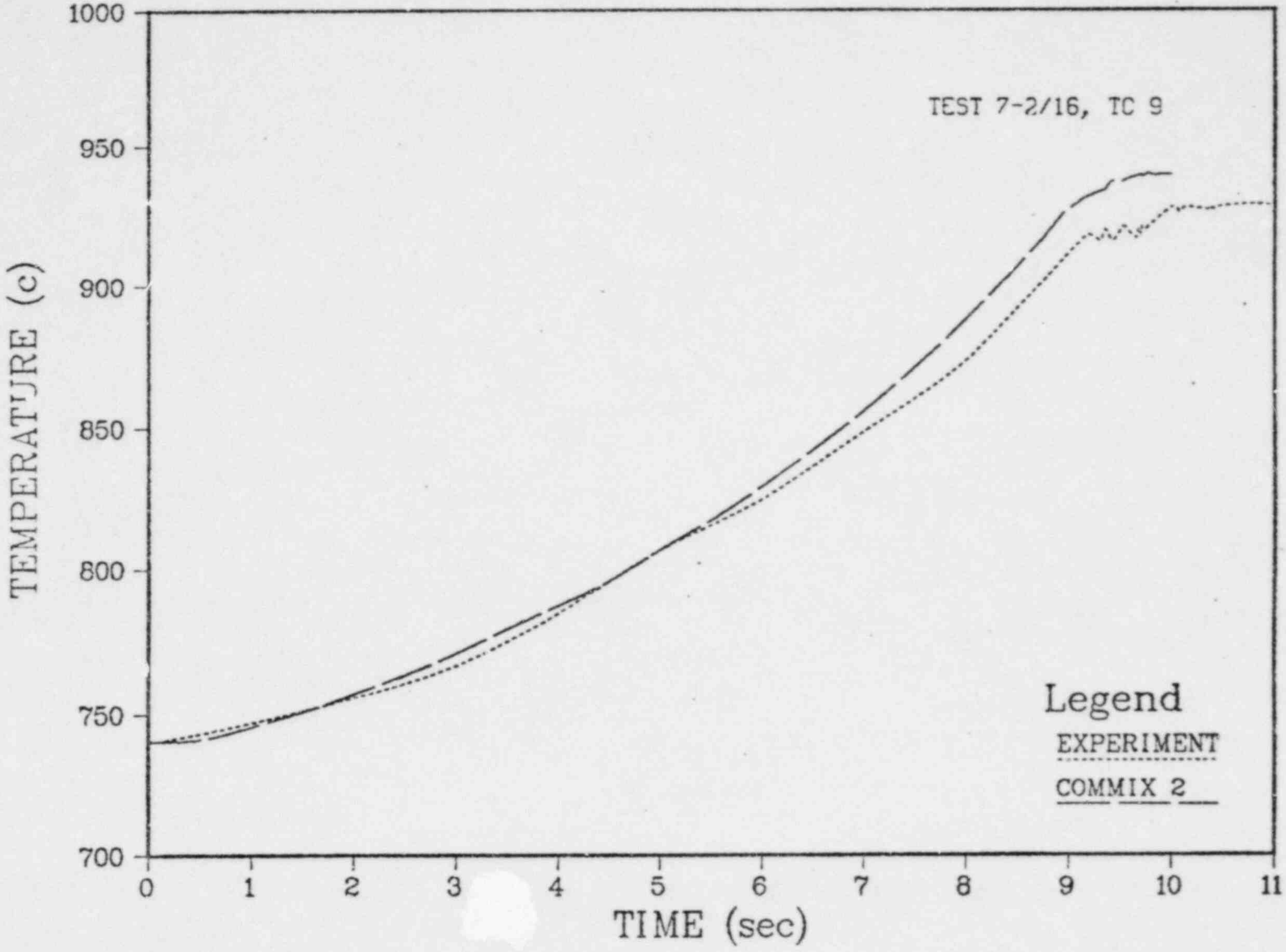


(A) AXIAL AND (B) TRANSVERSE PARTITIONING OF MODEL 7-PIN BUNDLE

TEST 7-2/16, FLOW TRANSIENT

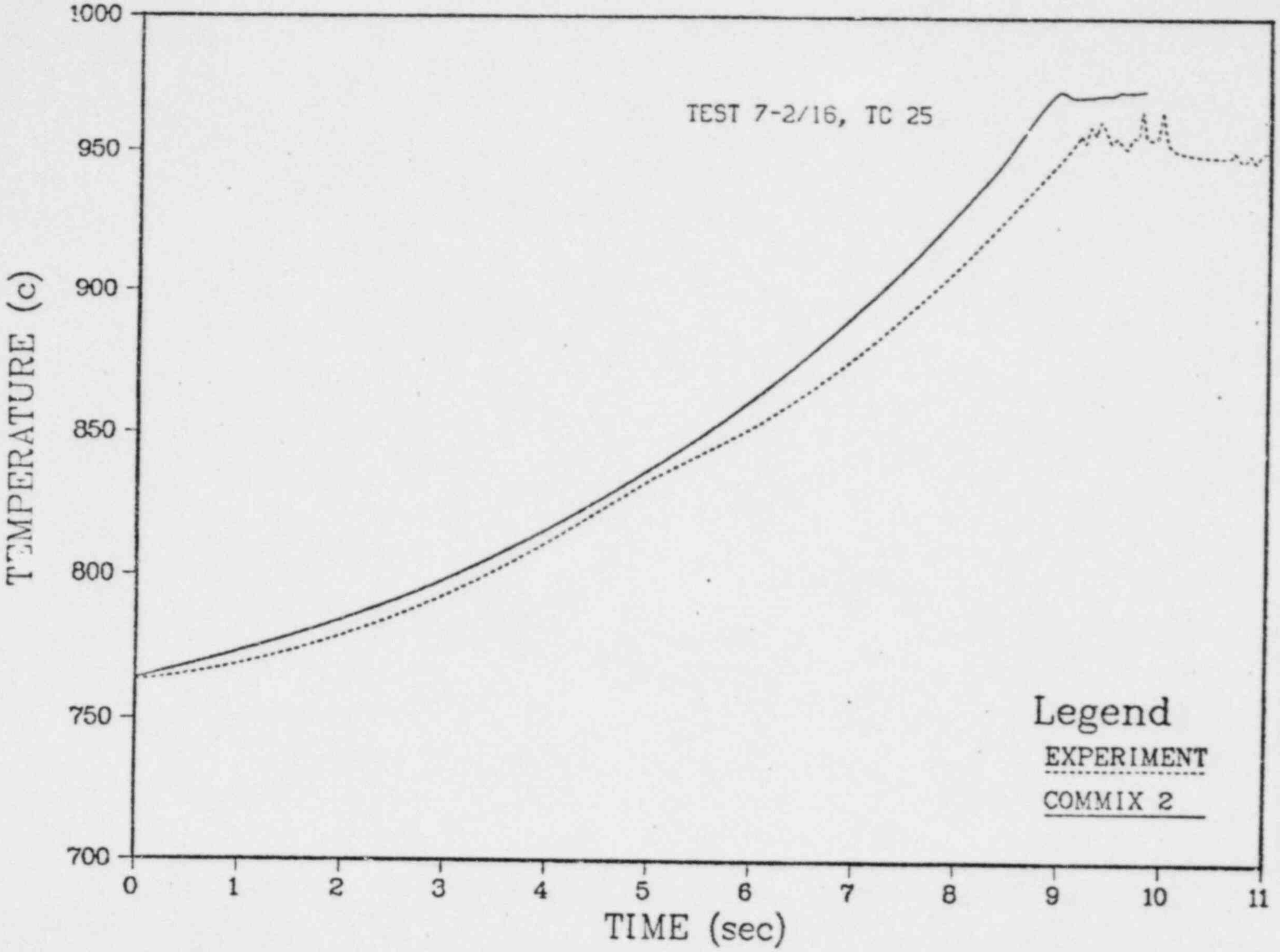


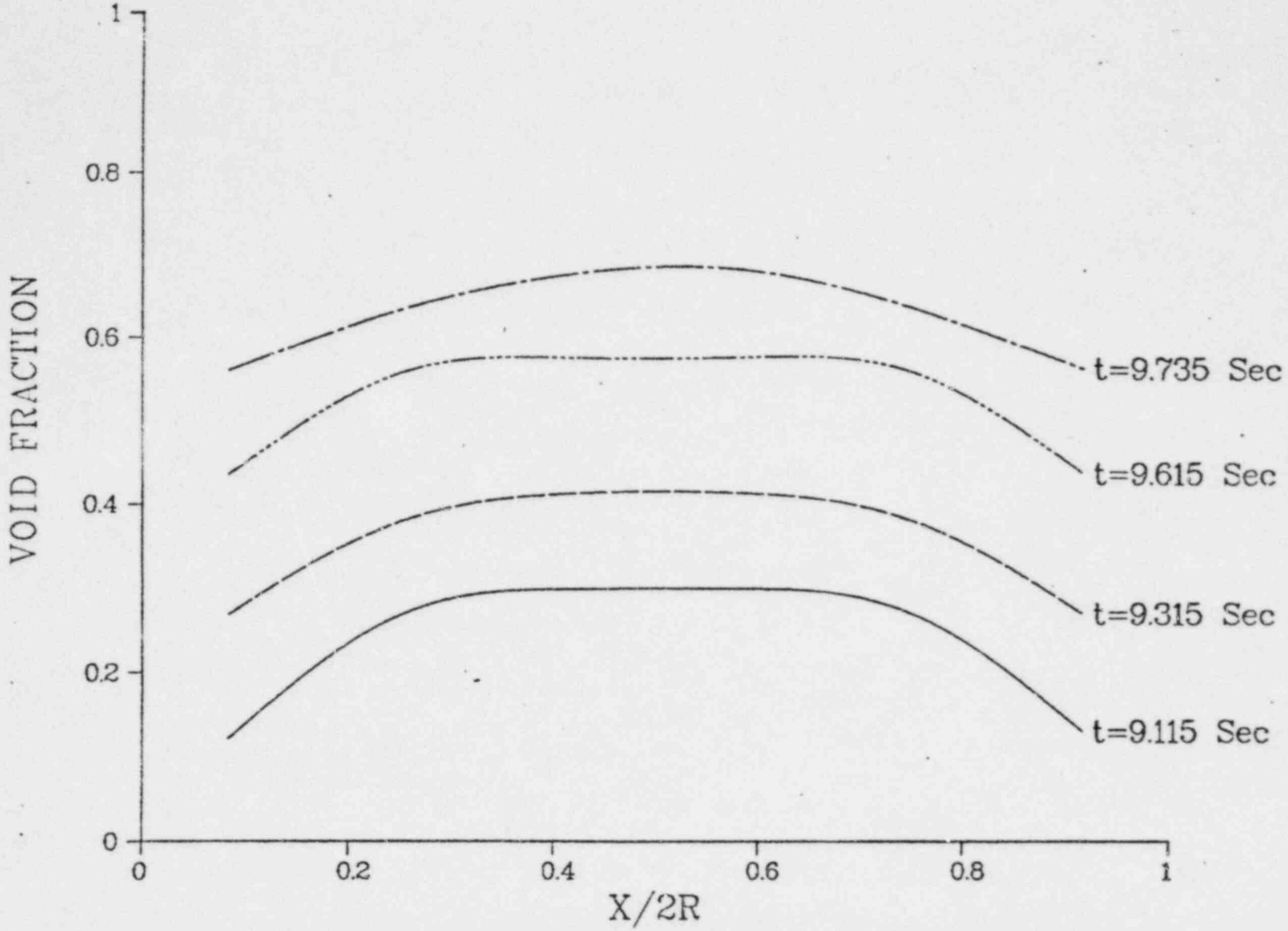
FLOW RUNDOWN TRANSIENT FOR TEST 7-2/16



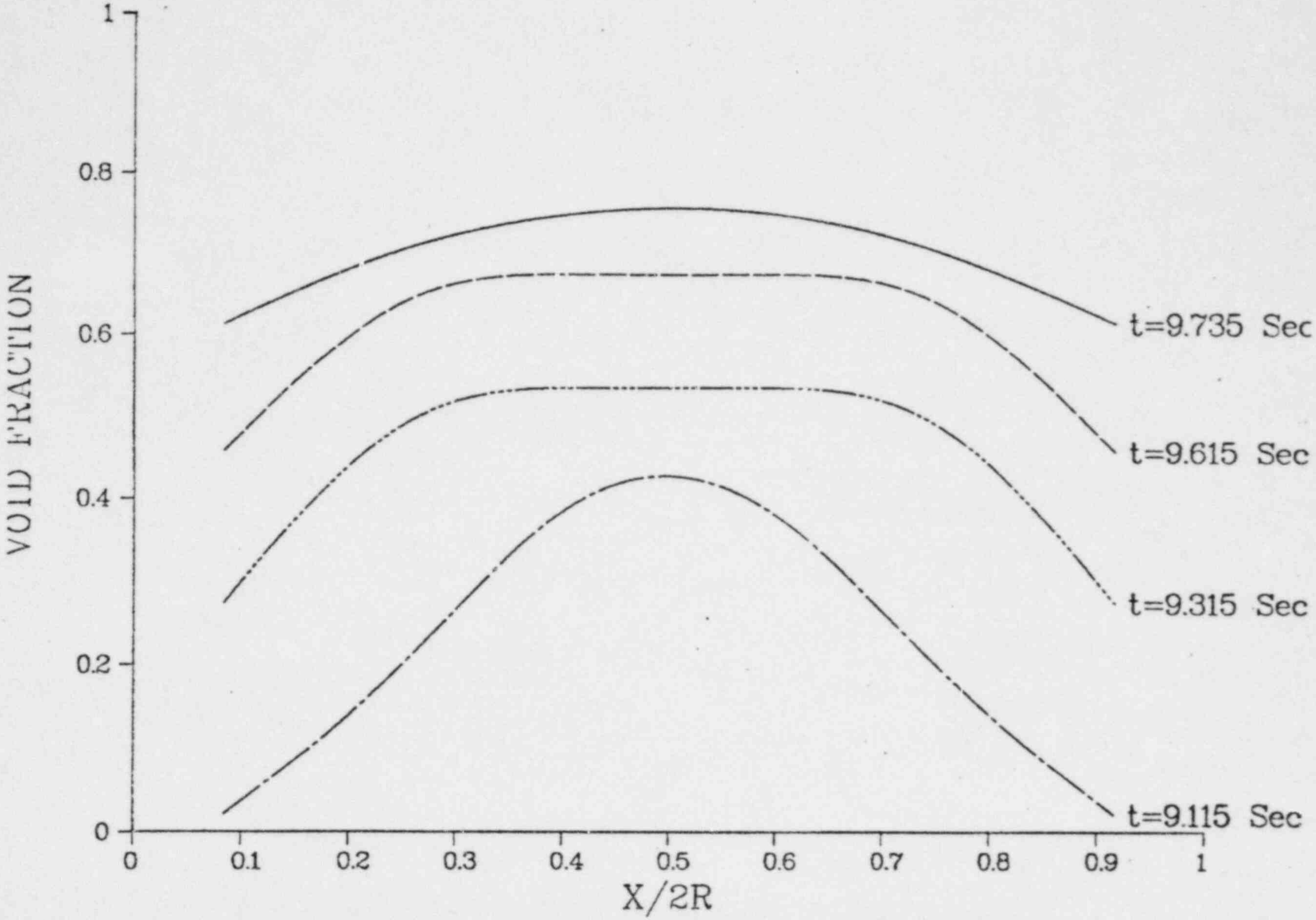
COMPARISON OF EXIT COOLANT TEMPERATURE WITH THE EXPERIMENTAL DATA

COMPARISON OF THE CENTRAL CLADDING SURFACE TEMPERATURE AT THE END OF THE HEATED SECTION WITH THE EXPERIMENTAL DATA

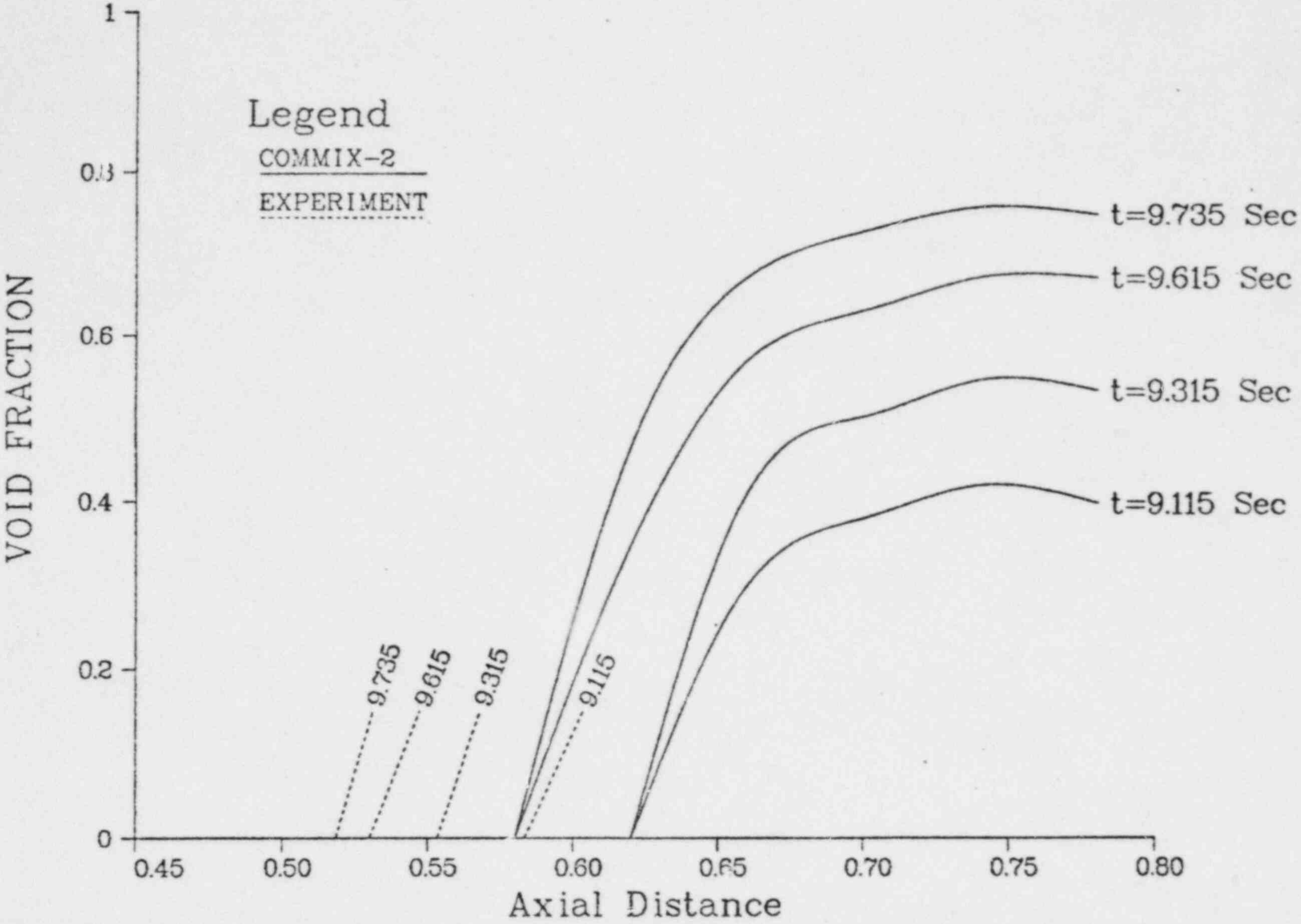




RADIAL DISTRIBUTION OF THE VOID FRACTION AT THE END OF THE HEATED SECTION



RADIAL DISTRIBUTION OF THE VOID FRACTION
AT THE EXIT PLANE



AXIAL DISTRIBUTION OF THE VOID FRACTION
ALONG THE CENTRAL AXIS

SPECIAL FEATURES OF COMMIX-2

CAPABILITIES

- CONTINUUM/QUASI-CONTINUUM
- 1D; 2D; OR 3D
- SINGLE PHASE (LIQUID OR GAS) OR TWO PHASE
- TWO FLUID MODEL / HOMOGENEOUS MODEL
- SODIUM OR WATER
- STEADY/TRANSIENT

SPECIAL FEATURES OF COMMIX-2 (CONT-1)

FORMULATIONS AND SOLUTION PROCEDURE

0 GENERAL FORMULATION

PERMIT: LINE BY LINE
PLANE BY PLANE
CELL BY CELL
DIRECT INVERSION

0 EXPLICIT AND IMPLICIT OPTION

0 STEADY STATE (TRANSIENT TERMS DROP OUT)
UNSTEADY STATE (TRANSIENT TERMS RETAINED)

0 SOLUTION PROCEDURE
SIMPER, IMF

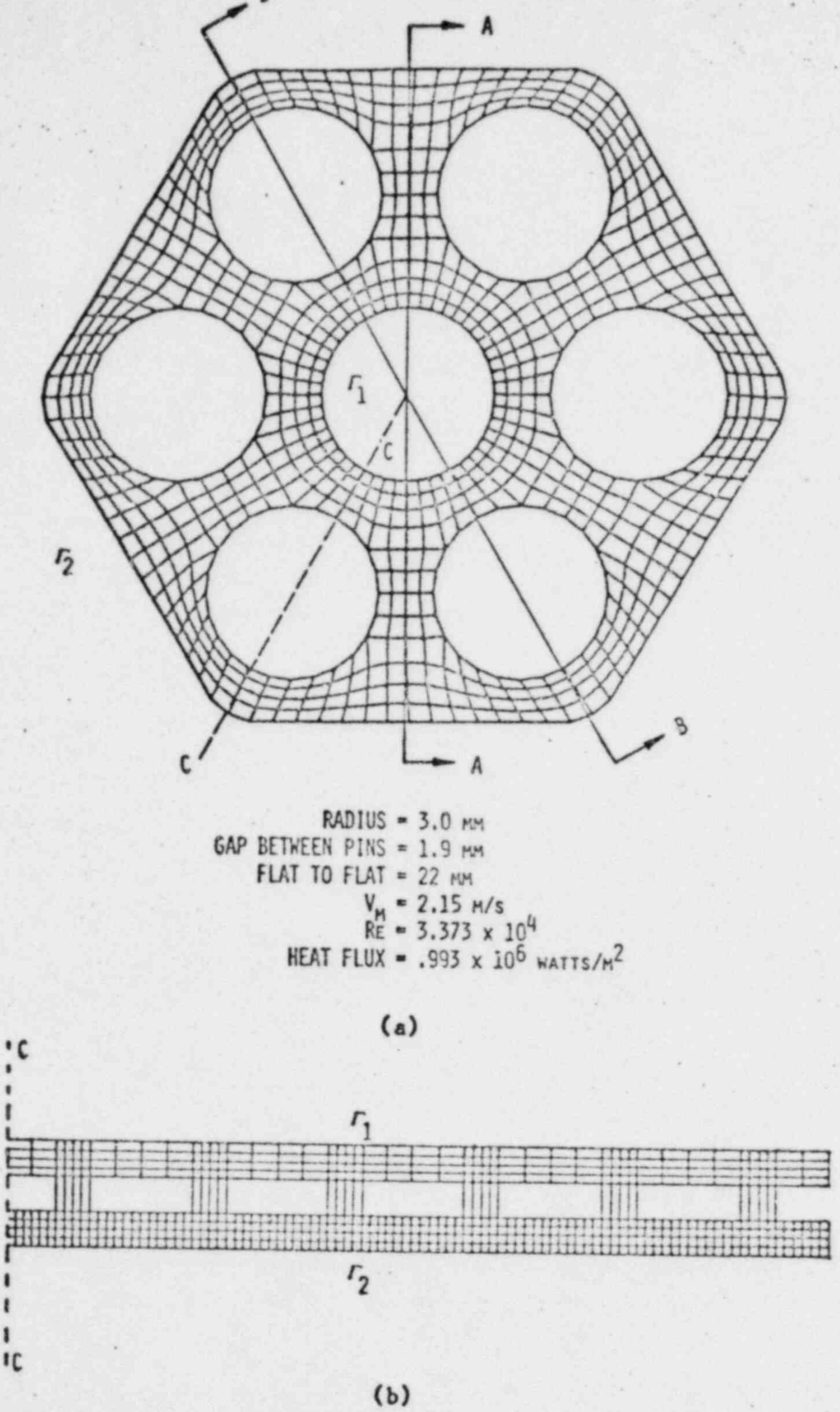
0 COMBINATION OF CENTRAL AND UPWARD
DIFFERENCING SCHEMES

SPECIAL FEATURES OF COMMIX-2 (CONT-2)

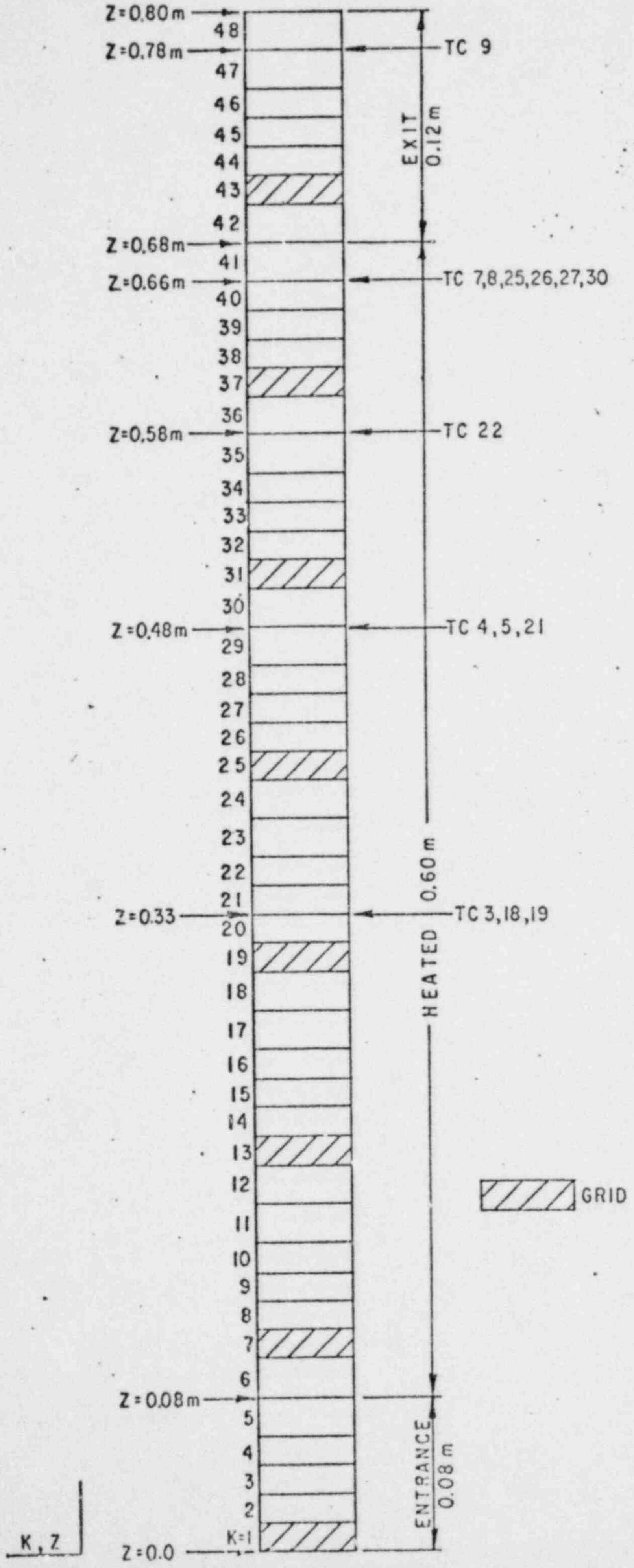
GEOMETRICAL FEATURES AND MODELS

- HEX GEOMETRY
  - FULL PIN PARTITIONING
  - QUARTER PIN PARTITIONING
- FUEL PIN MODEL
- DUCT WALL MODEL
- WIRE WRAP MODEL
- BOILING MODELS (4)
- WALL FRICTION MODELS (5)
- MOMEMTUM EXCHANGE COEFFICIENT MODELS (15)

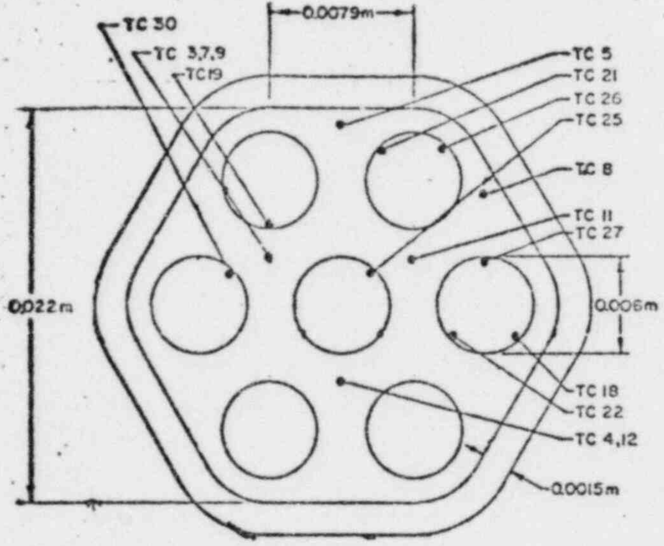
BODYFIT-1 CODE



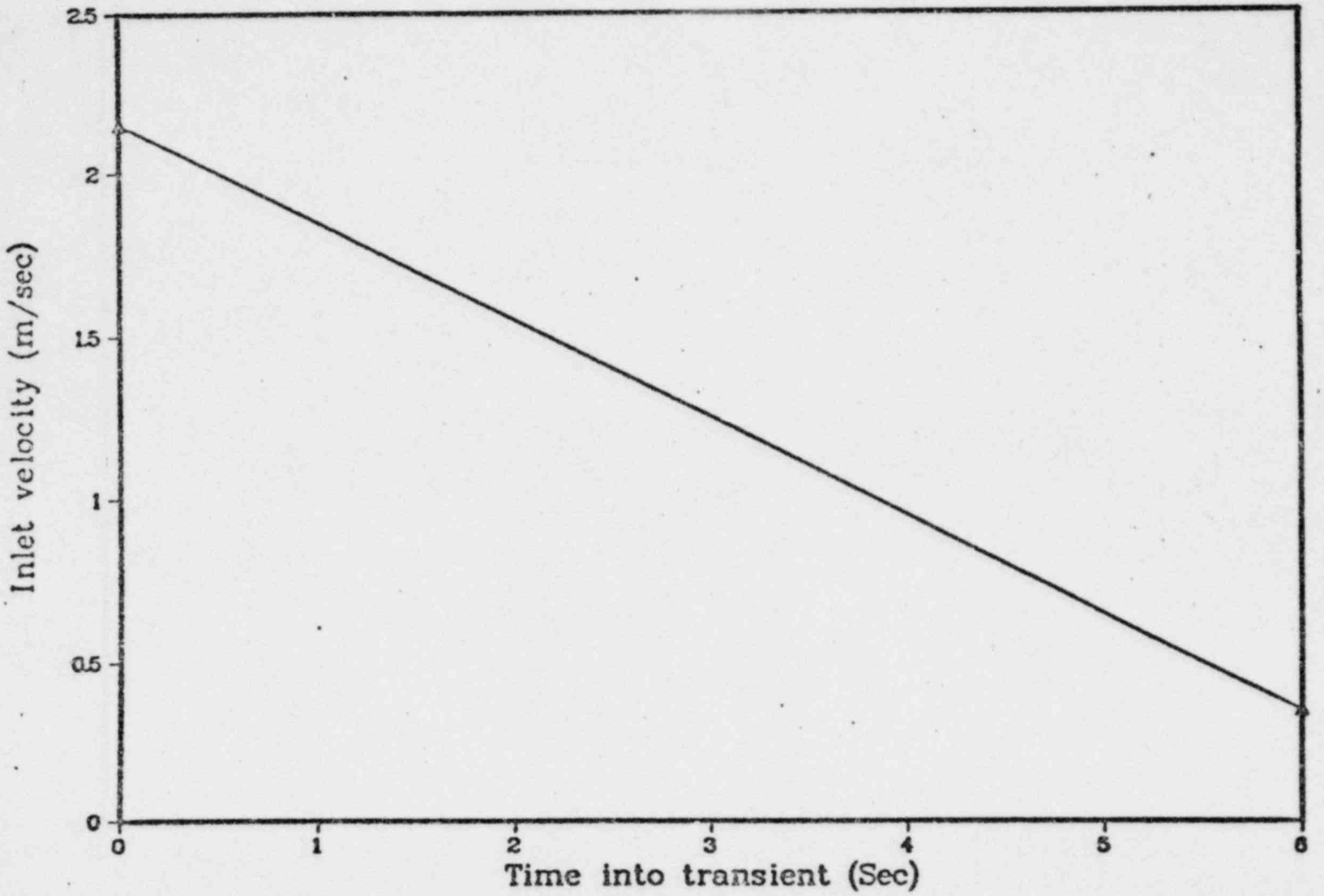
(a) Physical Plane and Dimensions,
(b) Transformed Plane. ANL Neg. No. 116-79-192R1.

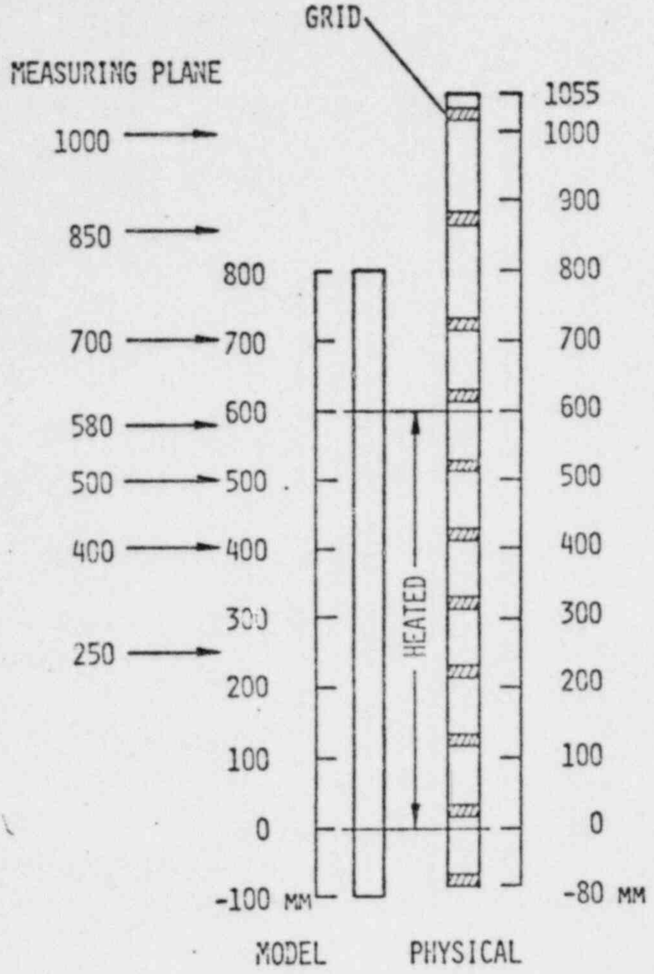


Z=0.80m
48
Z=0.78m
TC 9
47
46
45
44
43
EXIT 0.12m
42
Z=0.68m
41
Z=0.66m
TC 7,8,25,26,27,30
40
39
38
37
36
Z=0.58m
TC 22
35
34
33
32
31
30
Z=0.48m
TC 4,5,21
29
28
27
26
25
24
23
22
21
Z=0.33
TC 3,18,19
20
19
HEATED 0.60 m
18
17
16
15
14
13
12
GRID
11
10
9
8
7
6
Z=0.08m
5
4
3
2
ENTRANCE 0.08 m
K=1
K, Z
Z=0.0

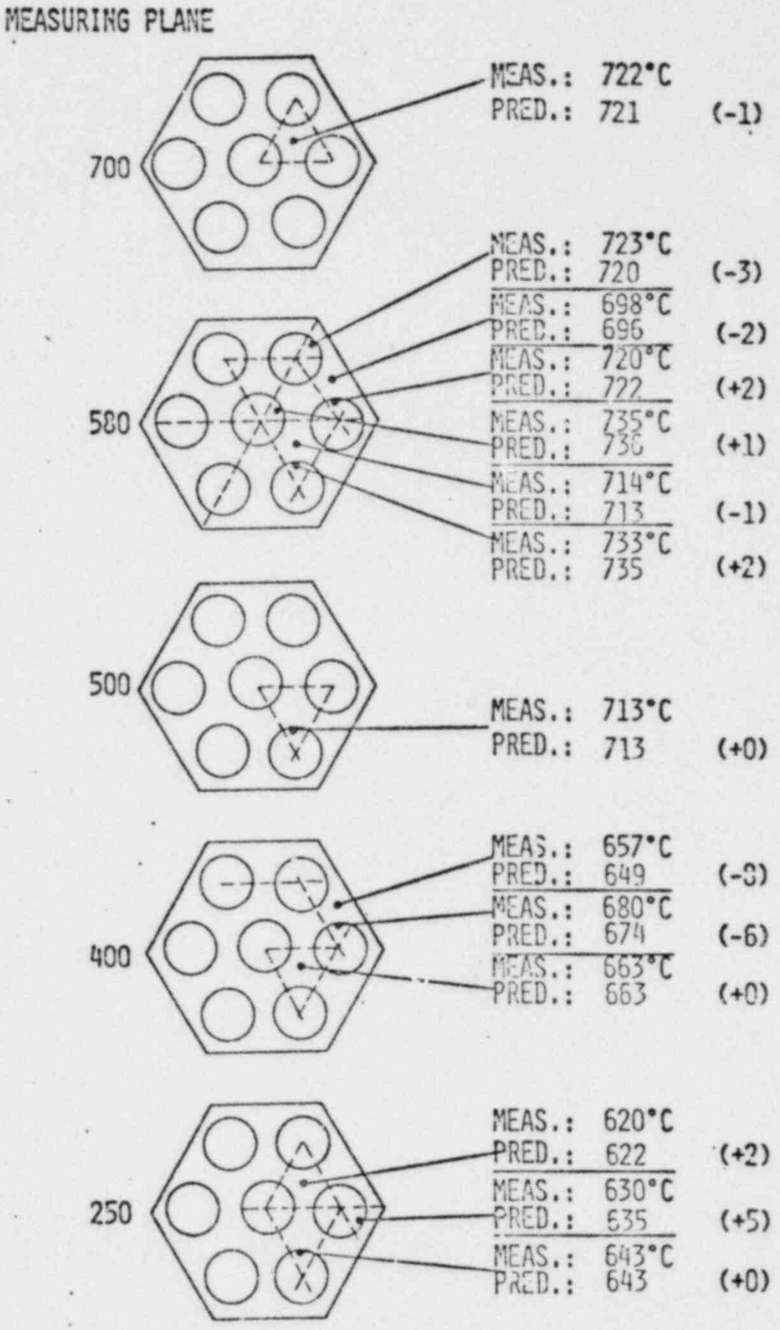

TC 30
TC 3,7,9
TC19
0.0079m
TC 5
TC 21
TC 26
TC 25
TC 8
TC 11
TC 27
0.022m
0.006m
TC 18
TC 22
TC 4,12
0.0015m

FLOW HISTORY

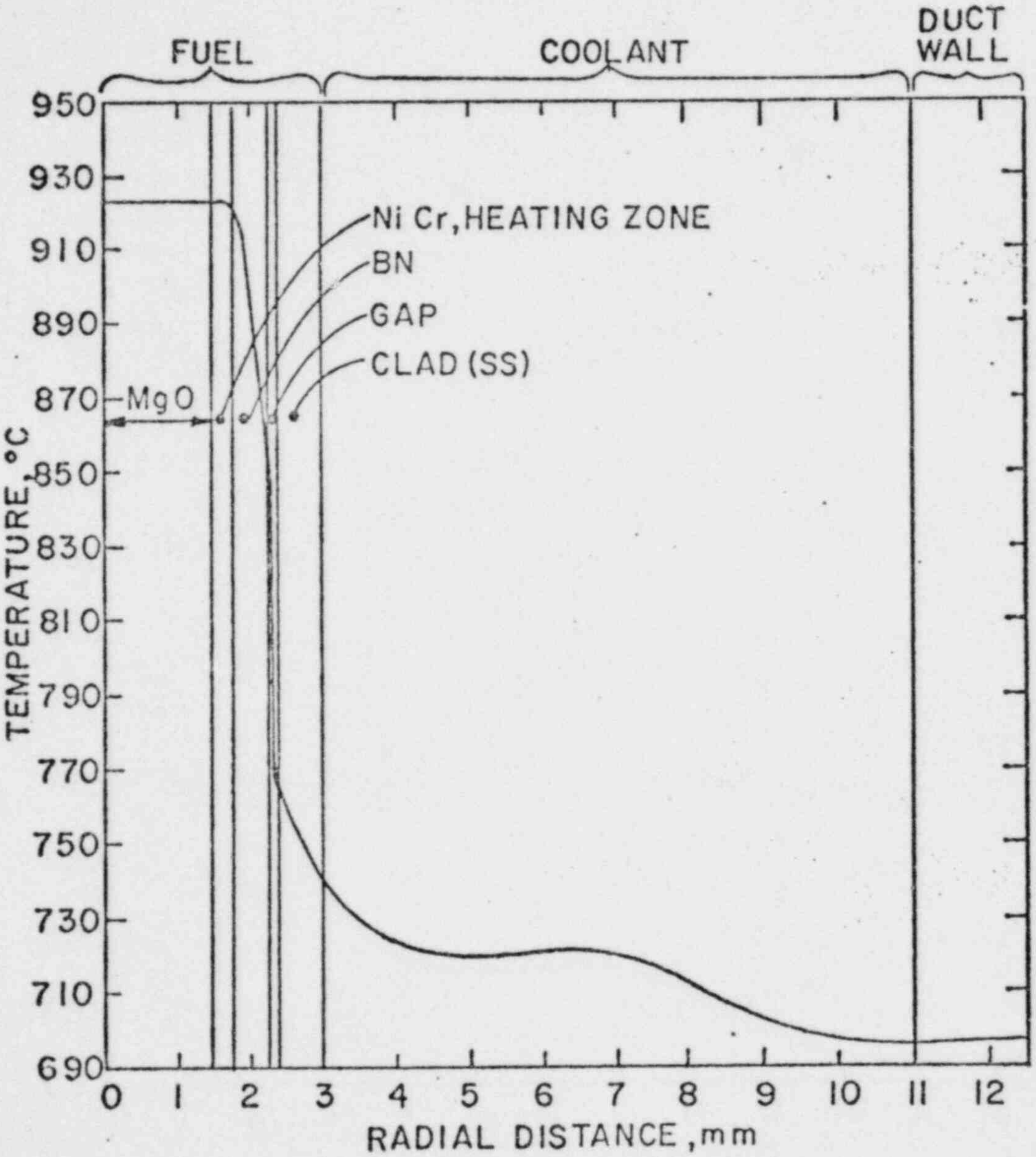


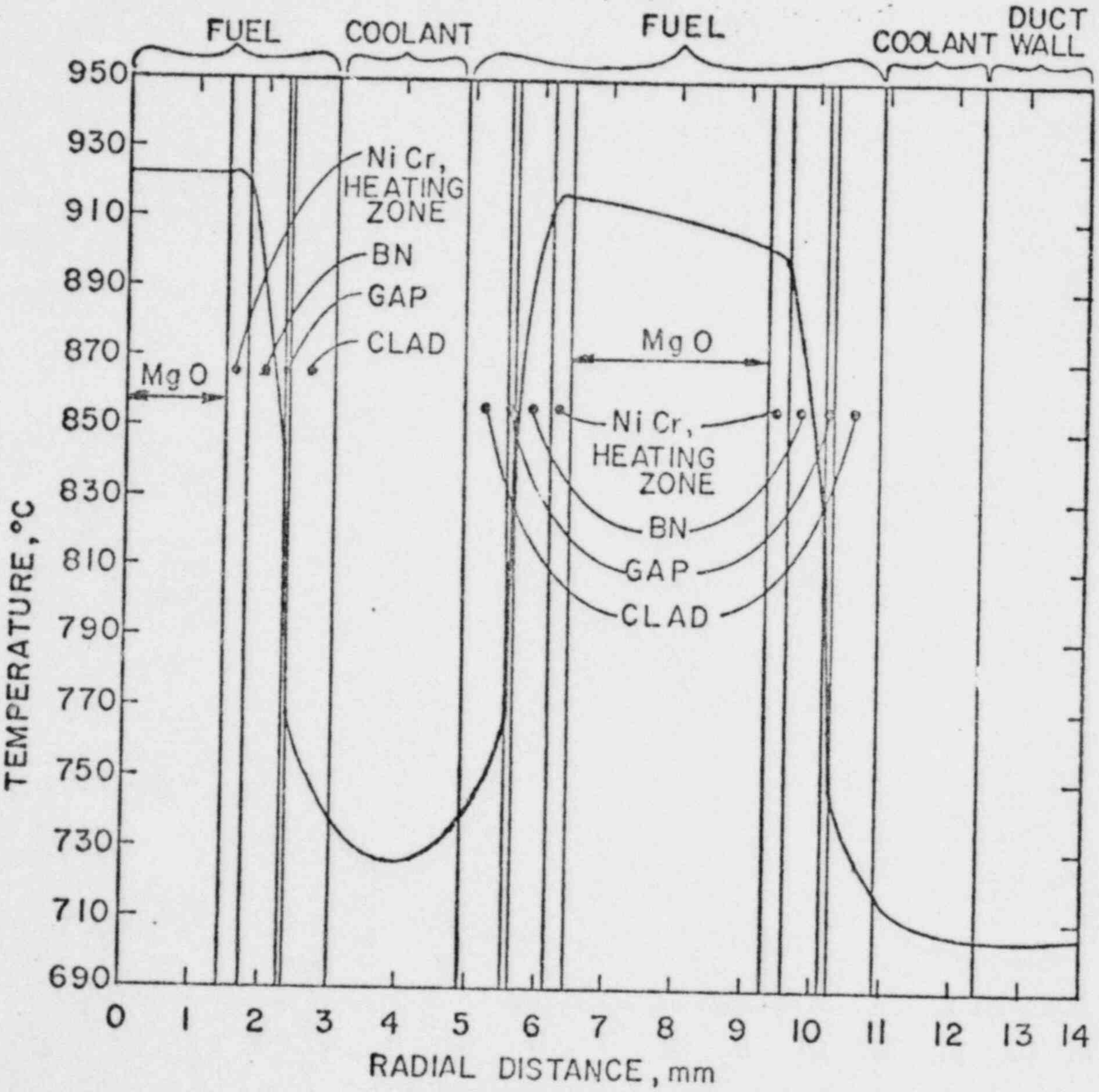


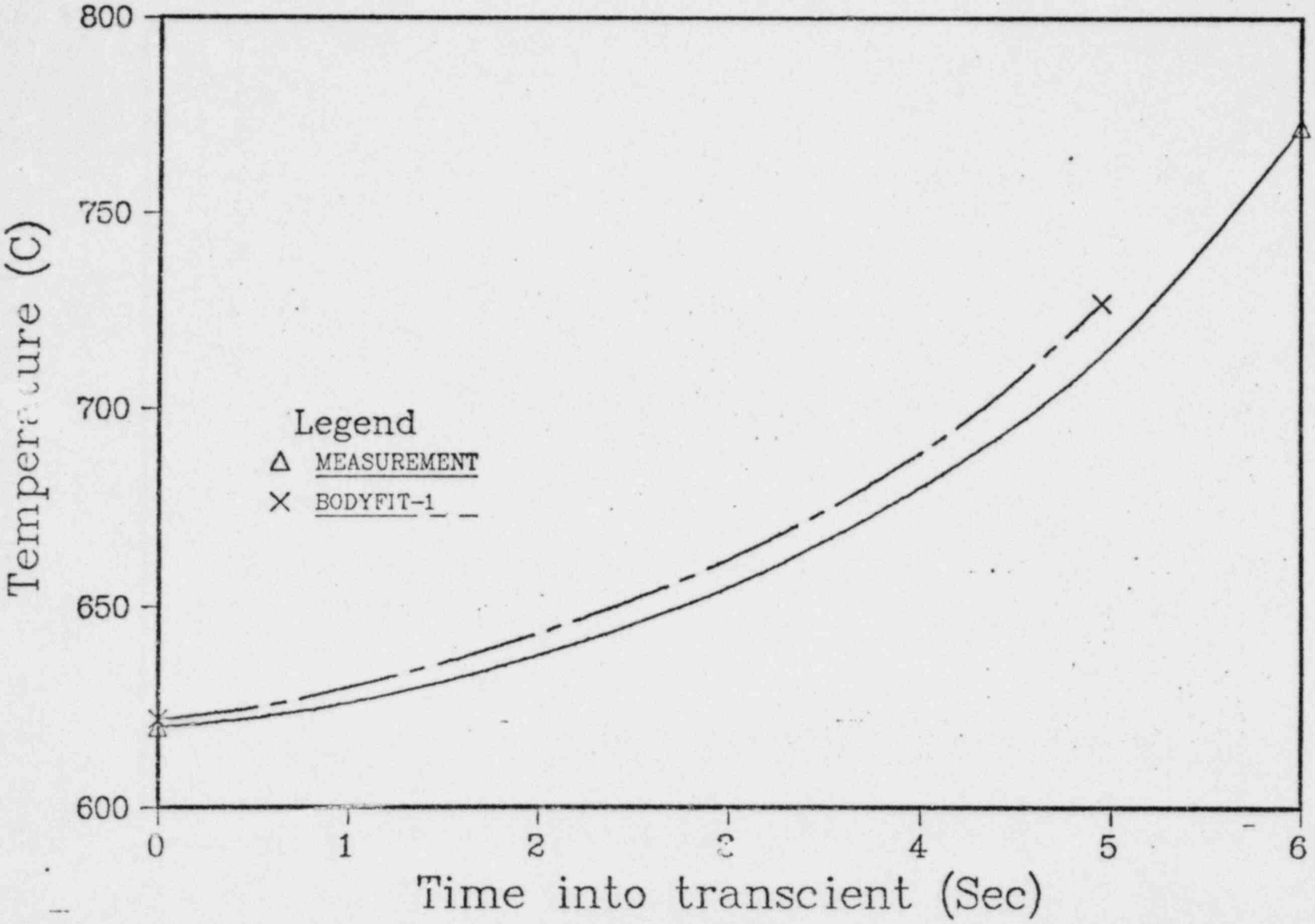
MEASURING PLANE
GRID
1000
850
700
580
500
400
250
800
700
600
500
400
300
200
100
0
-100 MM
1055
1000
900
800
700
600
500
400
300
200
100
0
-80 MM
HEATED
MODEL
PHYSICAL



MEASURING PLANE
700
MEAS.: 722°C
PRED.: 721 (-1)
580
MEAS.: 723°C
PRED.: 720 (-3)
MEAS.: 698°C
PRED.: 696 (-2)
MEAS.: 720°C
PRED.: 722 (+2)
MEAS.: 735°C
PRED.: 736 (+1)
MEAS.: 714°C
PRED.: 713 (-1)
MEAS.: 733°C
PRED.: 735 (+2)
500
MEAS.: 713°C
PRED.: 713 (+0)
400
MEAS.: 657°C
PRED.: 649 (-8)
MEAS.: 680°C
PRED.: 674 (-6)
MEAS.: 663°C
PRED.: 663 (+0)
250
MEAS.: 620°C
PRED.: 622 (+2)
MEAS.: 630°C
PRED.: 635 (+5)
MEAS.: 643°C
PRED.: 643 (+0)

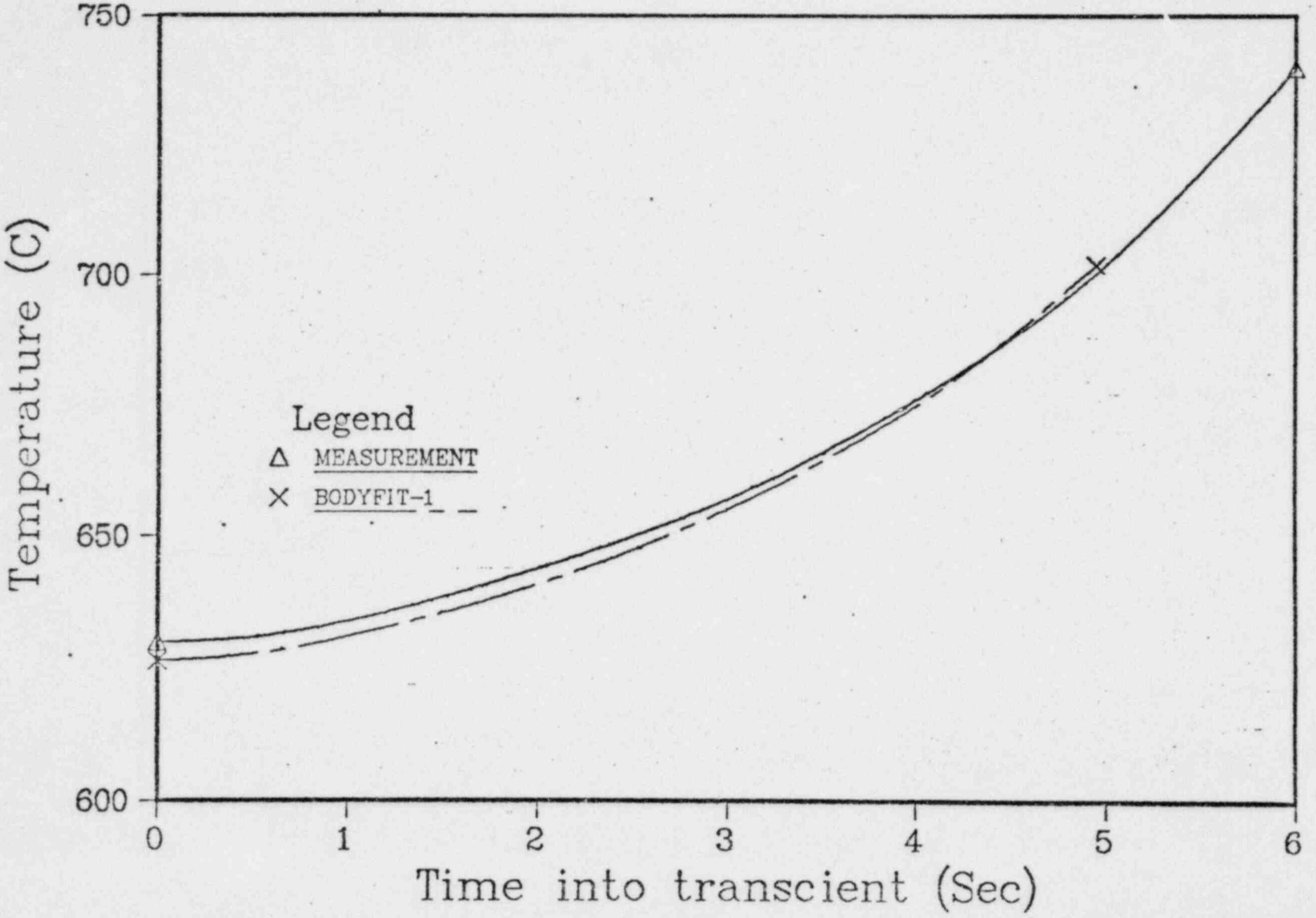

FUEL
COOLANT
DUCT WALL
950
930
910
890
870
850
830
810
790
770
750
730
710
690
TEMPERATURE, °C
MgO
Ni Cr, HEATING ZONE
BN
GAP
CLAD (SS)
0
1
2
3
4
5
6
7
8
9
10
11
12
RADIAL DISTANCE, mm

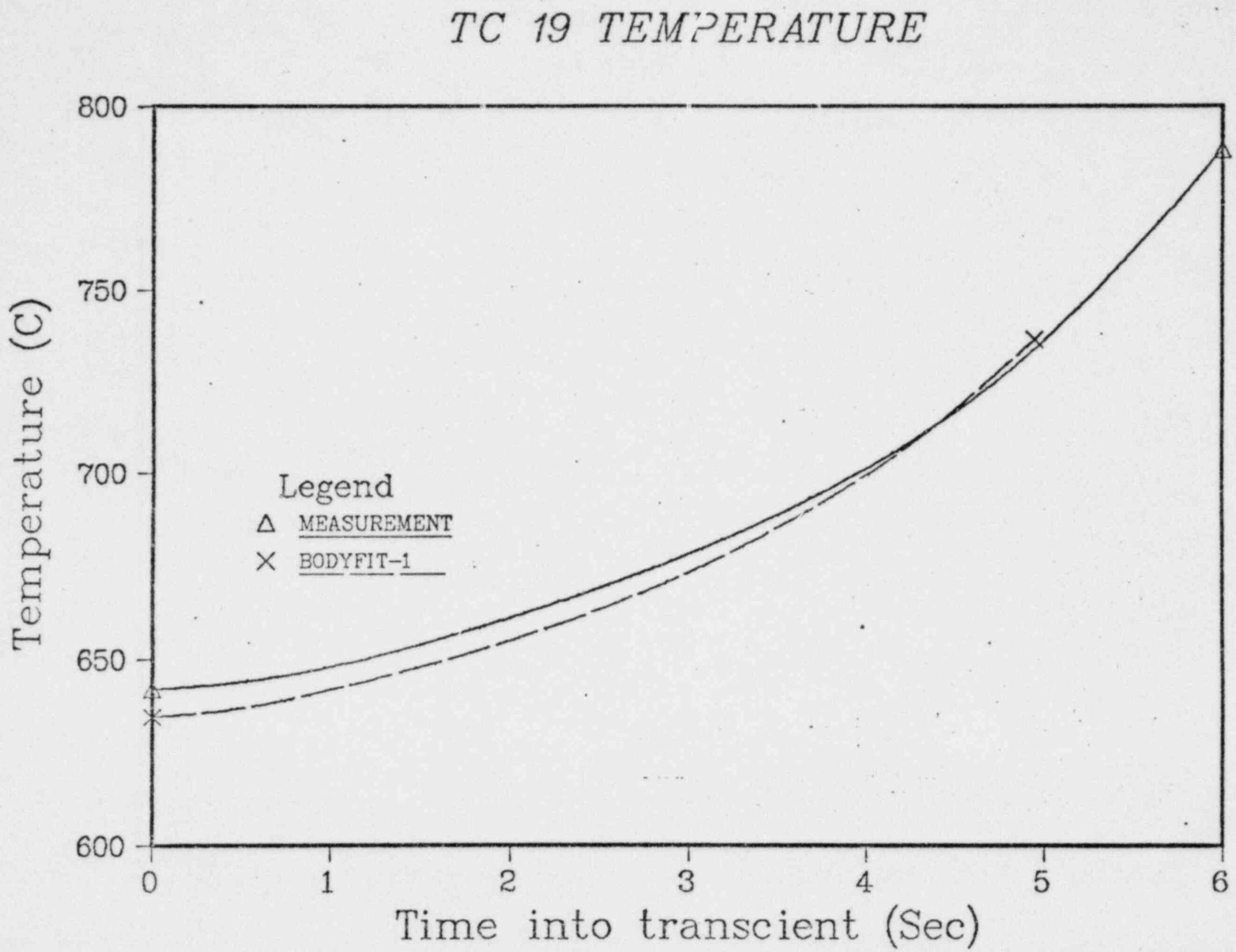

FUEL
COOLANT
FUEL
COOLANT
DUCT WALL
Ni Cr, HEATING ZONE
BN
GAP
CLAD
Mg O
Mg O
Ni Cr, HEATING ZONE
BN
GAP
CLAD
TEMPERATURE, °C
950
930
910
890
870
850
830
810
790
770
750
730
710
690
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
RADIAL DISTANCE, mm



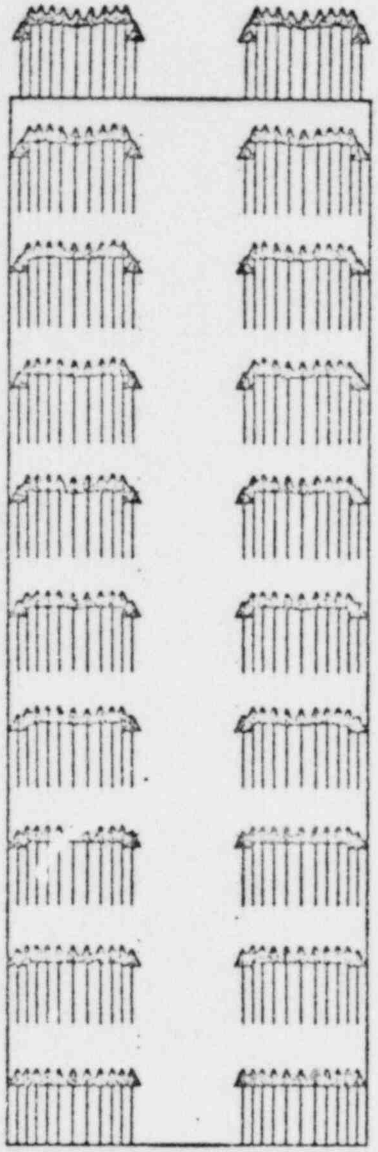
TC 3 TEMPERATURE

TC 18 TEMPERATURE

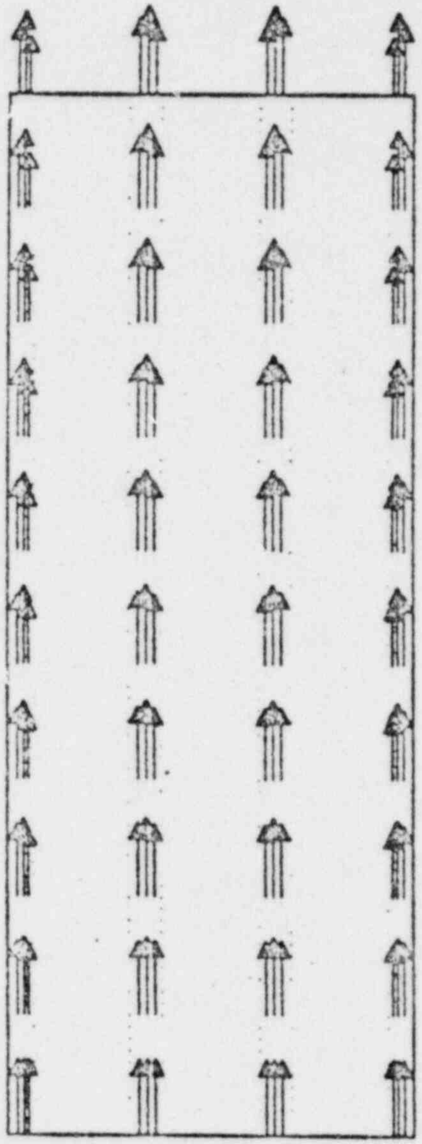




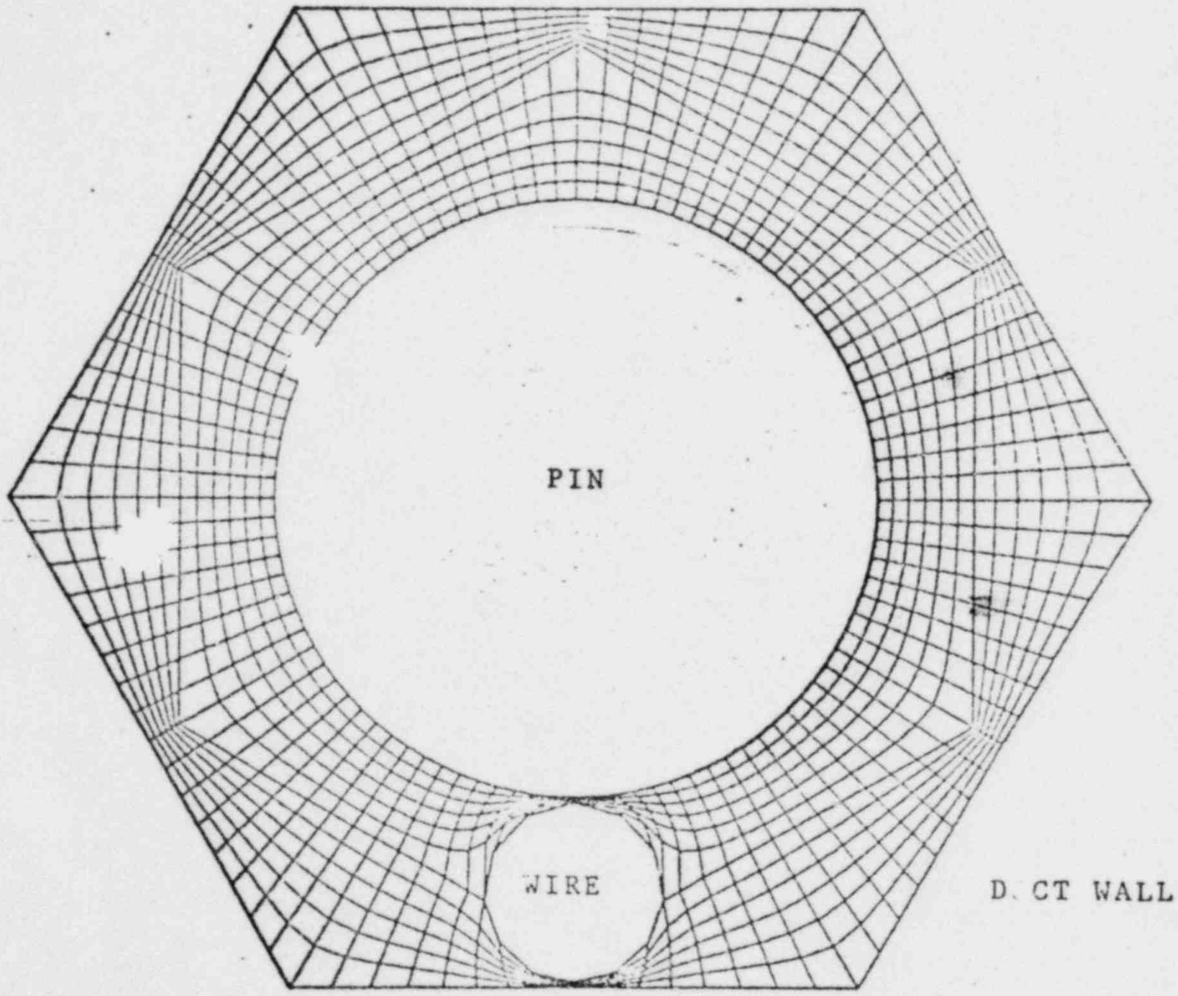
TC 19 TEMPERATURE
800
750
700
650
600
Temperature (C)
0
1
2
3
4
5
6
Time into transcient (Sec)
Legend
△ MEASUREMENT
× BODYFIT-1



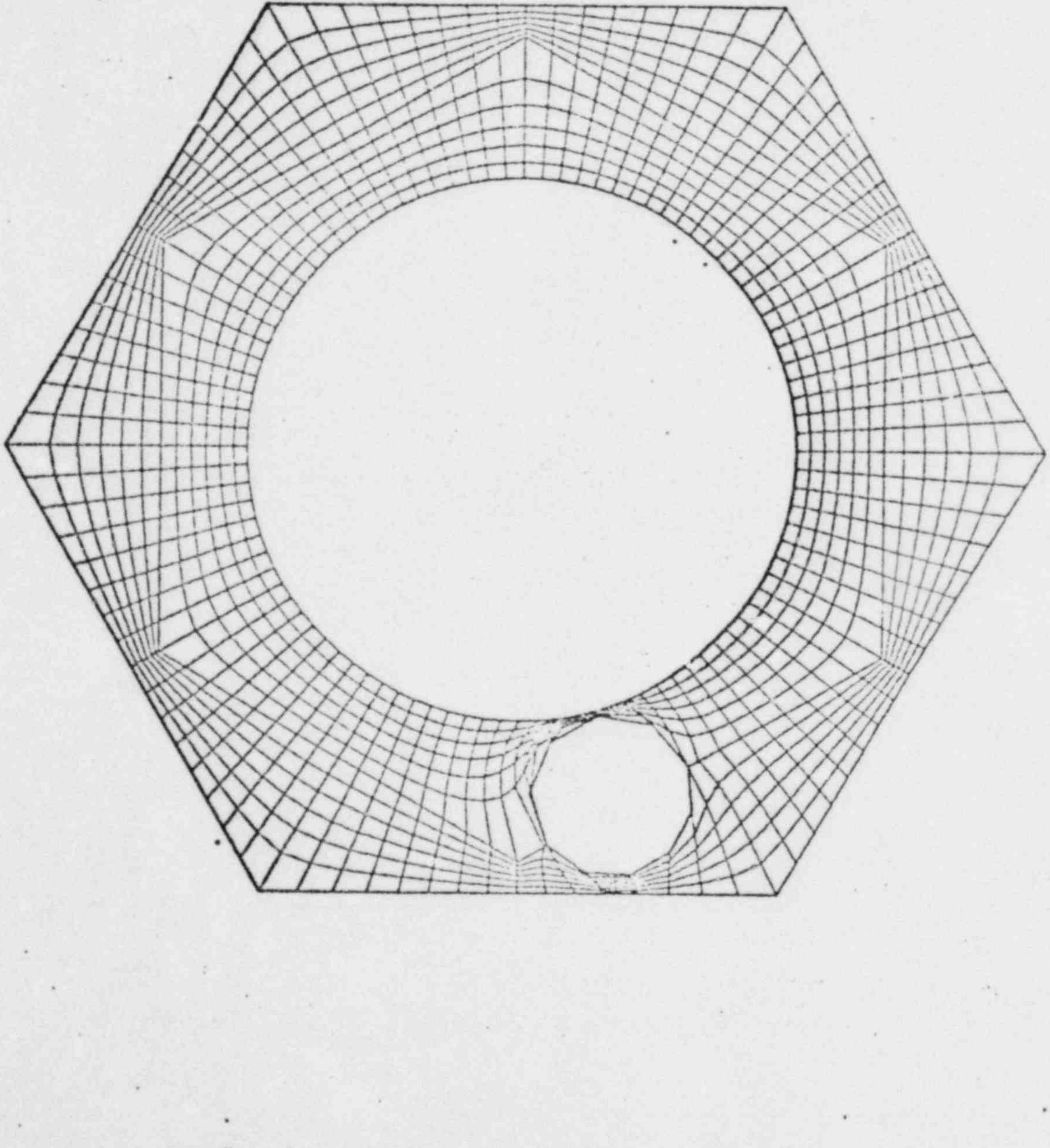
AXIAL VELOCITY ALONG SECTION AA



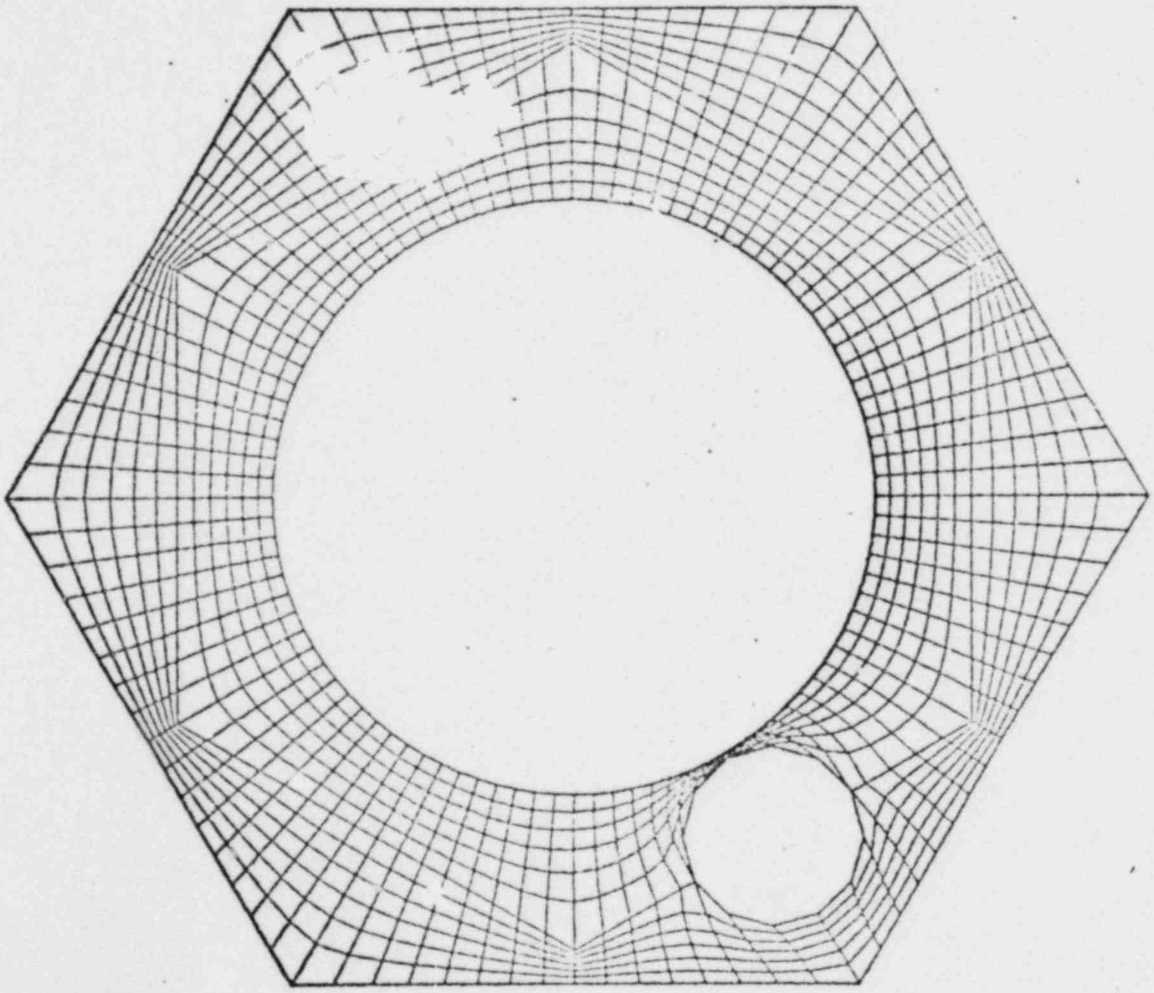
AXIAL VELOCITY ALONG SECTION BB



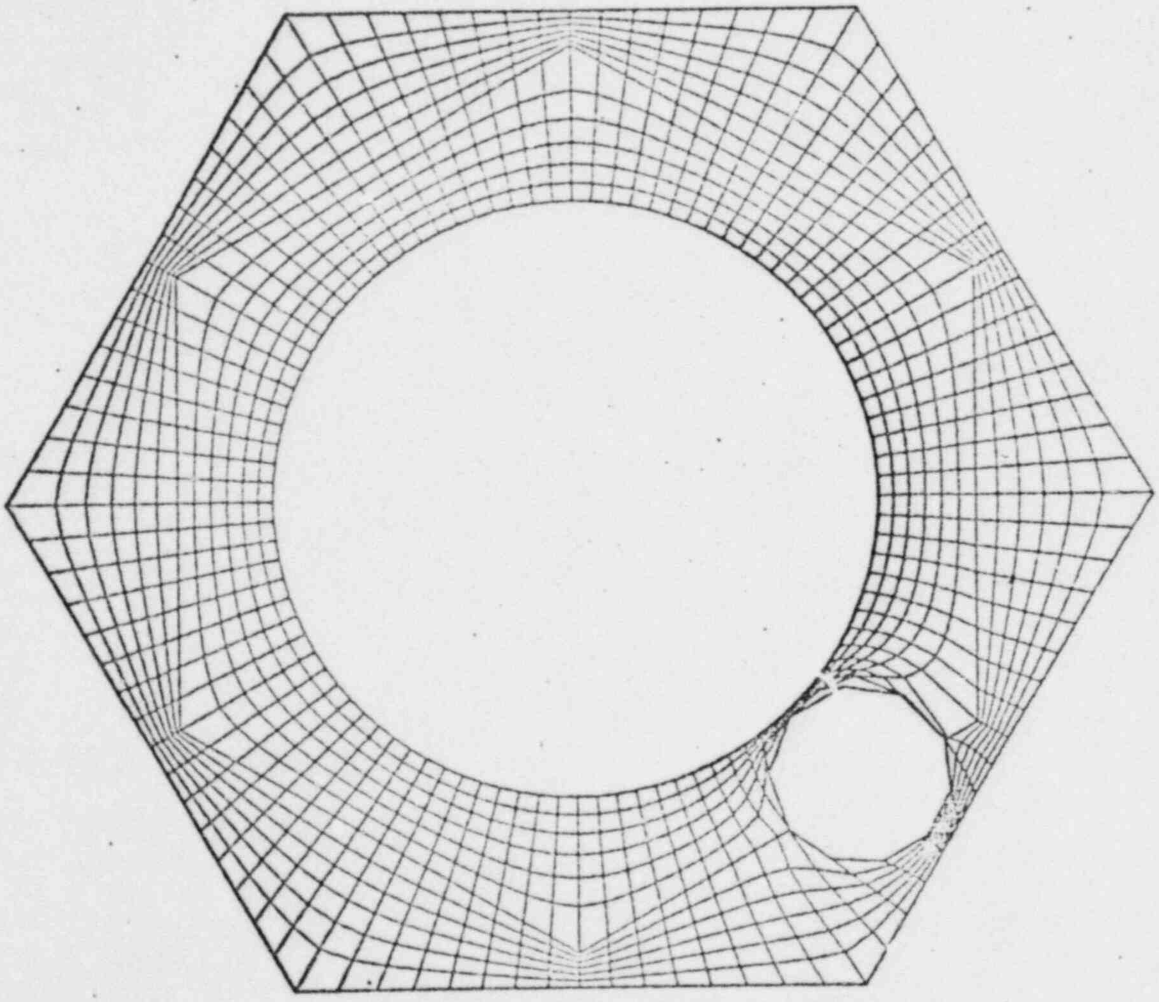
Coordinate Lines for Wire-Wrapped Assembly
at Elevation Level One



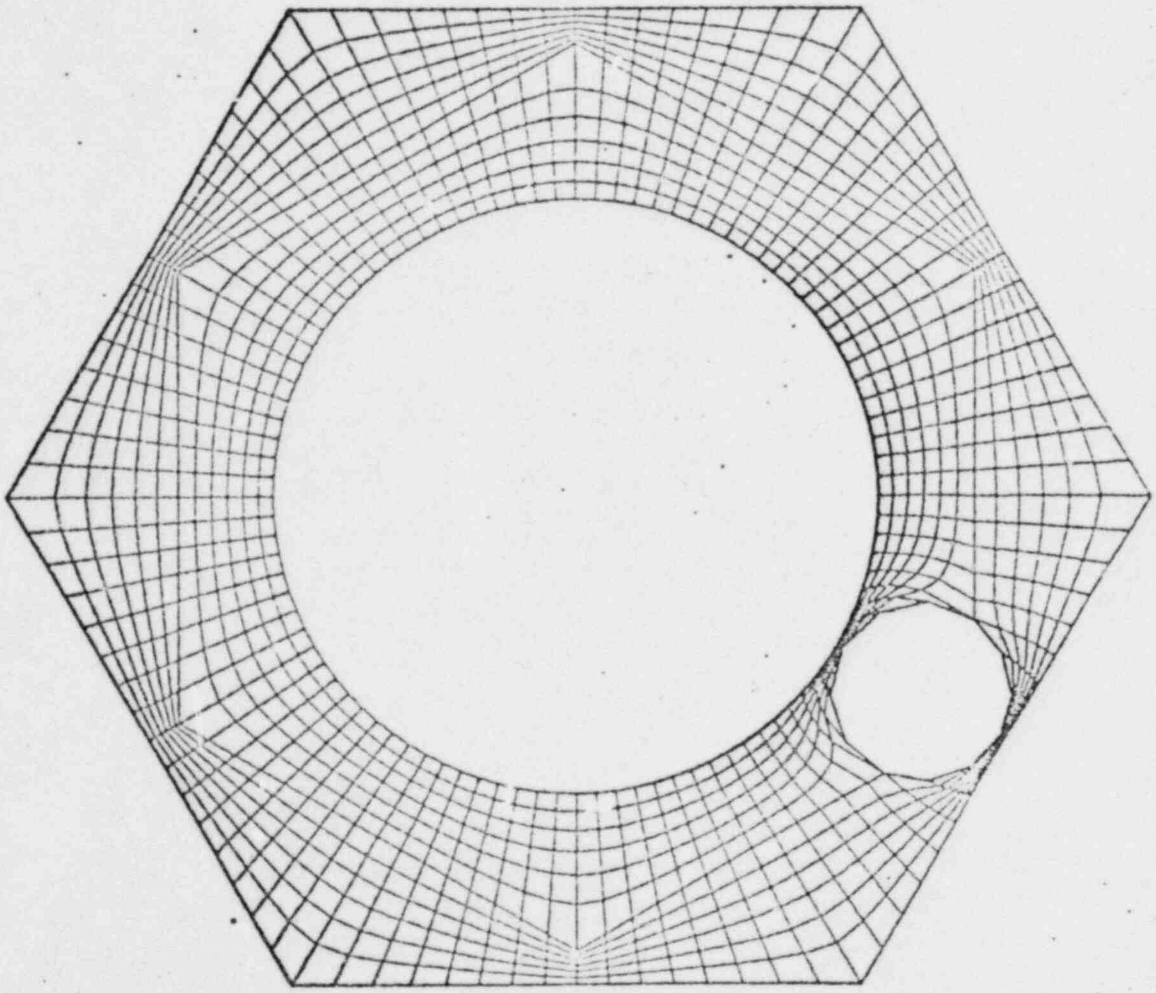
Coordinate Lines for Wire-Wrapped Assembly
at Elevation Level Two



Coordinate Lines for Wire-Wrapped Assembly
at Elevation Level Three



Coordinate Lines for Wire-Wrapped Assembly
at Elevation Level Four



Coordinate Lines for Wire-Wrapped Assembly
at Elevation Level Five

CURRENT STATUS

- COMMIX-1A (ADVANCED VERSION OF COMMIX-1)
  - CLEAN-UP, DOCUMENT AND RELEASE (12/80)
- COMMIX-2
  - INTERFACIAL COUPLINGS:
    - MASS:
      - EVAPORATION COMPLETED
      - CONDENSATION IMPLEMENTING
    - MOMENTUM:
      - INTERFACIAL FRICTION COMPLETED
    - ENERGY:
      - INTERFACIAL HEAT TRANSFER IMPLEMENTING
  - WALL HEAT TRANSFER COMPLETED
  - WALL FRICTION COMPLETED
  - FUEL ROD AND DUCT WALL HEAT TRANSFER MODEL COMPLETED
  - WIRE WRAP MODEL IMPLEMENTING
  - DOCUMENT AND RELEASE (DRAFT VERSION) (9/80)
- BODYFIT-1
  - 3D TRANSFORMATION COMPLETED
  - K-ϵ TURBULENCE MODEL COMPLETED
  - FUEL ROD AND DUCT WALL HEAT TRANSFER MODEL COMPLETED
  - DOCUMENT AND RELEASE (9/80)

FUTURE PLAN

- COMMIX-1A
  - VALIDATION AND APPLICATIONS
    - FFTF
    - PFR
    - ETC.
  - WHOLE CORE ANALYSIS
  - PARABOLIC APPROACH
- COMMIX-2
  - IMPROVE SOLUTION TECHNIQUE
    - SHORTENING RUNNING TIME
    - OPTIMIZING STORAGE
  - IMPROVE PHYSICAL MODELING
    - PARAMETRIC STUDY
  - VALIDATION AND APPLICATIONS
- BODYFIT-1
  - WIRE WRAP MODEL
  - VALIDATION AND APPLICATIONS

BNL LMFBR EXPERIMENTAL PROGRAM: PRESENTATION FOR THE ACRS

July 9, 1980

Washington, D.C.

Presented By

T. Ginsberg

Program Contributors: G. A. Greene, N. Abuaf, G. Zimmer, BNL; J. Chen, Lehigh Univ.; M. Kazimi, MIT; J. Moszynski, Univ. of Delaware.

Brookhaven National Laboratory
Department of Nuclear Energy
Experimental Modeling Group
Upton, New York 11973

TODAY'S PRESENTATION

- Overview
- Recent Results
- Future Directions

BNL FAST REACTOR SAFETY EXPERIMENTS

- Objectives
  - Investigate thermal-Hydraulic Phenomena of Importance in Fast Breeder Reactor Safety Analysis.
  - Application of Phenomenological Principles To Accident Analysis.

SCOPE OF BNL PROGRAMS

SIMULATION EXPERIMENT	ACCIDENT PHASE	ISSUE
Transition Phase Assessment		Identification of Key Safety Issues
Heat Transfer In Internally Heated Boiling Pools	PAHR Transition Phase	Structure Coolability; Impact on Dispersion
Multiphase Fuel Relocation and Freezing Dynamics	Transition Phase	Recriticality; Reactivity Loss; Bottled Pool
Core Dispersion (Hydrodynamic and Volume-Heated)	Transition Phase	Recriticality; Fuel Compaction; Steel Vapor Dispersion
HCDA Energetics: Entrainment by Taylor Instability	Post-Disassembly Bubbly Expansion	Sodium Working Fluid Potential

MAJOR BNL ACCOMPLISHMENTS

TASK	ACCOMPLISHMENTS	IMPLICATIONS
o ASSESSMENT OF TRANSITION PHASE PHENOMENOLOGY	- Report completed. - Accident sequence paths traced. - Adequacy of understanding phenomenlogy assessed.	- Fuel dispersal does not rule out potential for recriticality. - Accident likely to progress to greater fuel motion coherency.
o BOILING POOL HEAT TRANSFER	- Volume-heated boiling and non-boiling experiments completed. - Boundary layer correlations derived.	
o FUEL RELOCATION	- Two-phase gas-liquid tests completed. - Scoping slurry freezing tests carried out.	- Dispersed phase may accelerate fuel freezing process in blanket structure.
o HYDRODYNAMIC DISPERSAL	- Two-phase air-water experiments with gamma densitometer initiated.	
o DISPERSION IN VOLUME-HEATED BOILING POOLS	- Study of microwave heating simulation method completed. - Conceptual design of microwave irradiation facility completed.	
o HCDA ENERGETICS: ENTRAINMENT BY TAYLOR INSTABILITY	- Literature review completed. - Analysis of Taylor Instability growth in two-phase HCDA completed.	- Potential for entrainment by Taylor mechanism limited by heavy phase (UO_2). - Work potential enhancement by sodium via Taylor entrainment limited by presence of heavy phase.

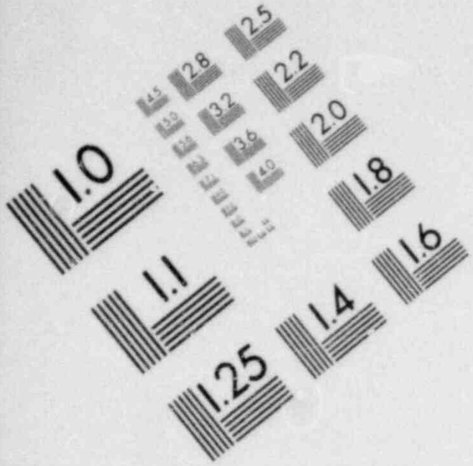
RECENT RESULTS

- Assessment of Transition Phase Phenomenology
- Boiling Pool Heat Transfer
- Fuel Relocation
- Hydrodynamic Dispersal

TRANSITION PHASE ASSESSMENT

SCOPE:

- REVIEW PREVIOUS WORK
- PHENOMENOLOGY
- ACCIDENT SEQUENCES
- RESEARCH NEEDS AND PRIORITIES



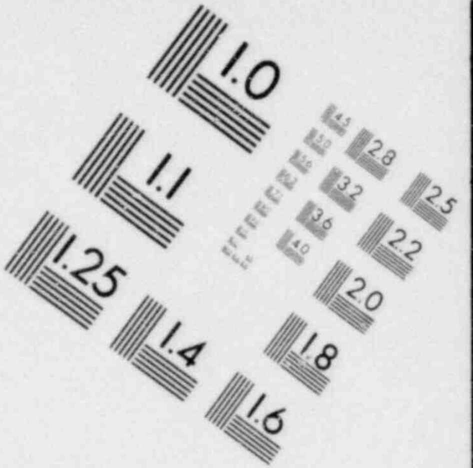
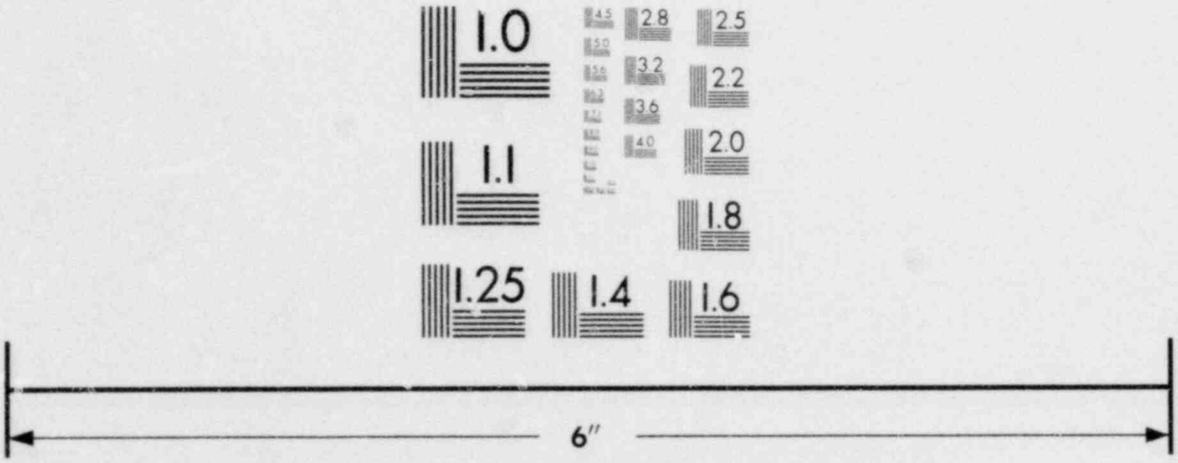
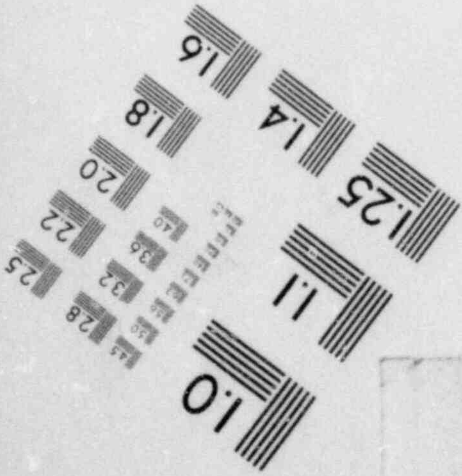
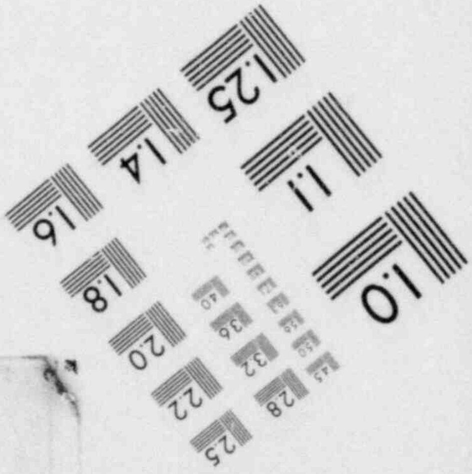


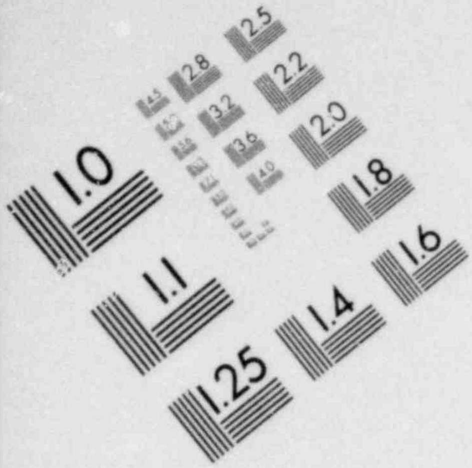
IMAGE EVALUATION
TEST TARGET (MT-3)



MICROCOPY RESOLUTION TEST CHART







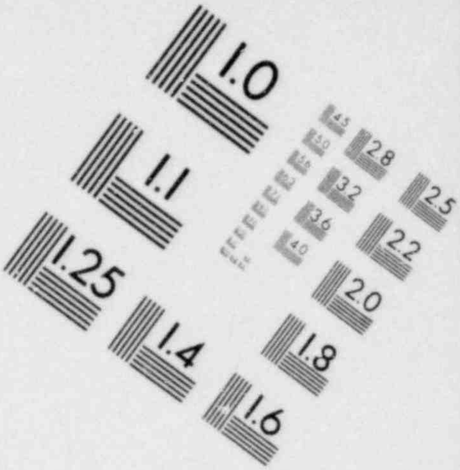
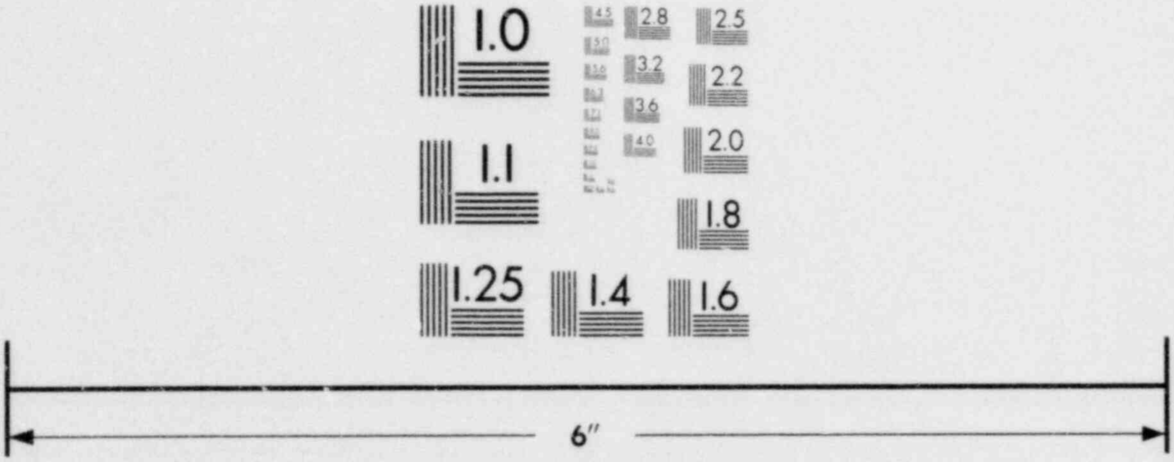
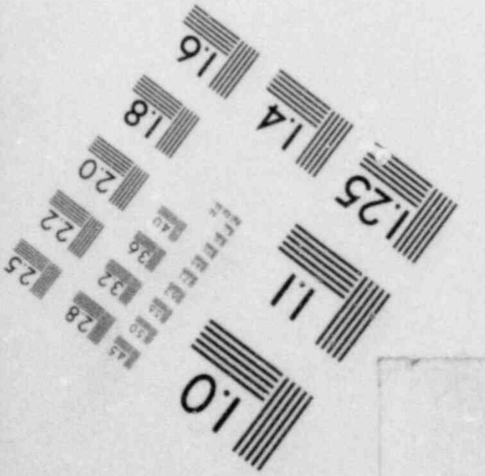
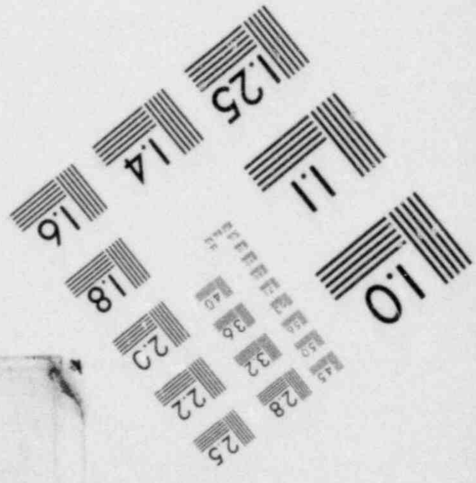


IMAGE EVALUATION TEST TARGET (MT-3)



MICROCOPY RESOLUTION TEST CHART





TRANSITION PHASE PHENOMENOLOGY

Phenomena	State-of-Art	Future Research Needs
Clad Dynamics	Available Clad Motion Models Appear Adequate. Plug Formation Likely.	In-Pile Large-Bundle Tests to Substantiate Small-Scale Results.
Boiling Heat Transfer	Heat Transfer Flow Regime Dependent. Correlations Available for 1-Comp. Bubbly Flow, Small L/D.	Extend to Churn Regime. Subassembly Scale Pools. Multi-Comp. Larger Power Densities.
Boiling Hydrodynamics	Drift Flux o.k. for Average Void Churn Regime Dominant for Decay Heat Conditions.	Multi-Comp. Pools. Local Void Transients. Larger Power Densities.
Fuel Motion and Freezing	Fuel Penetration into Blankets Limited by Freezing. Freezing Accelerated by Voids and Particles Observed. Freezing Mechanism Still Uncertain. Freezing Rates Bounded by Limiting Models.	Prototypic Tests to Verify Penetrations. Slurries. Entrainment. Crust Stability.

TRANSITION PHASE ACCIDENT SEQUENCES

- Assume Entry to Fuel Disruption Phase
- Scope Behavior of "Representative" Channel
- Identify Potential for Recriticality
- Reassess Fuel & Steel Freezing Arguments and Impact on T.P.
- Reassess Dispersion Arguments and Impacts
- Identify Distinctions Between FFTF & CRBR Behavior

CONCLUSIONS

- Likelihood of Development of Transition Phase is Design-Dependent.

- Transition Phase Recriticality Events Cannot be Ruled Out on Basis of Fuel Dispersal.

- Early Termination of Transition Phase Progression by Blowdown to Sodium is Unlikely.

- Entrapped Molten Pools are Likely. They are Likely to Grow to Whole Core Scale Before Blowdown to Sodium Plena.

- Coherent Core-Wide Fuel Motion During Whole-Core Stage is Possible. Recriticality Energetics Must be Carefully Assessed.

- Fuel Dispersion is Limited By
  - Geometry Imposed by Axial Blockages
  - Boundary Heat Losses
  - Pressurization
  - Quenching by Cold Steel
  - Flow Regime

RESEARCH NEEDS AND PRIORITIES

- Is Transition Phase Likely for Particular Design?
- Verification of Thermite Test Restuls with Gas-Free UO_2 and Separate Effects Tests.
- Whole-Core Pool Stability
  - How Much Mobile Fuel?
  - Bound Recriticality Energetics.
- Continue Search for Recriticality Mechanisms and Estimation of Energetics.

VOLUME — HEATED BOILING POOL BEHAVIOR
VOID FRACTION, HEAT TRANSFER, DISPERSION

OBJECTIVES

- Measure Local Boundary Heat Flux as Function of Wall Angle and Flow Regime
- Measure Average and Local Distribution of Void Fraction as Function of Dimensionless Power and Flow Regime. Investigate Effect of Aspect Ratio
- Identify Applicable Flow Regime Transition Criteria:

    Non-Boiling To Bubbly Flow

    Bubbly To Churn-Turbulent

- Compare Results to Previous Investigations and Develop Correlations and/or Models Applicable for Performing Analyses Related to PAHR and Transition Phase Conditions

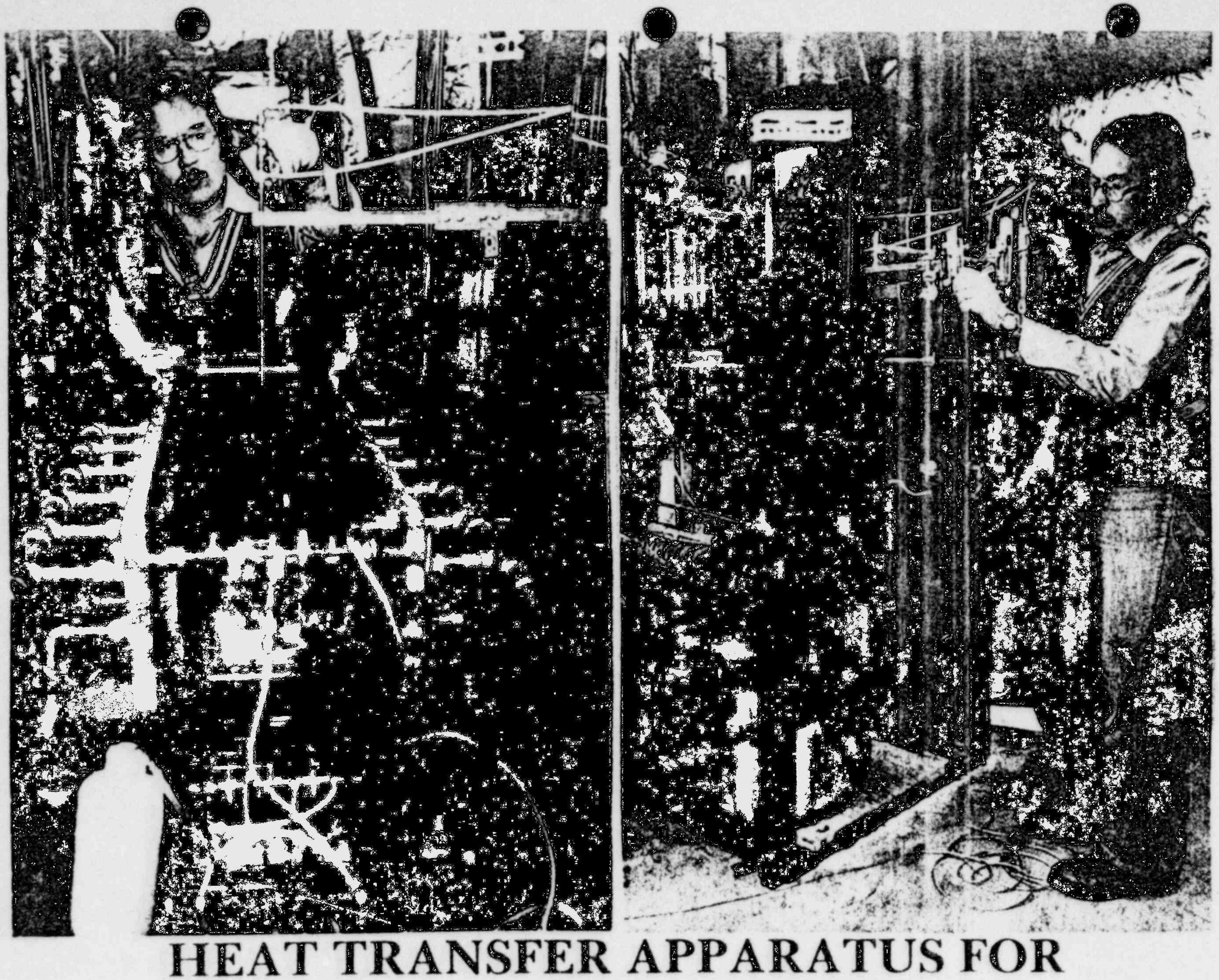
APPARATUS

- High Density, Non-Dispersed Small L/D Pools for Heat Transfer Studies Simulating Whole Core & Plenum Geometries
- High/Low Density, Large L/D Pools Simulating Subassembly Geometries

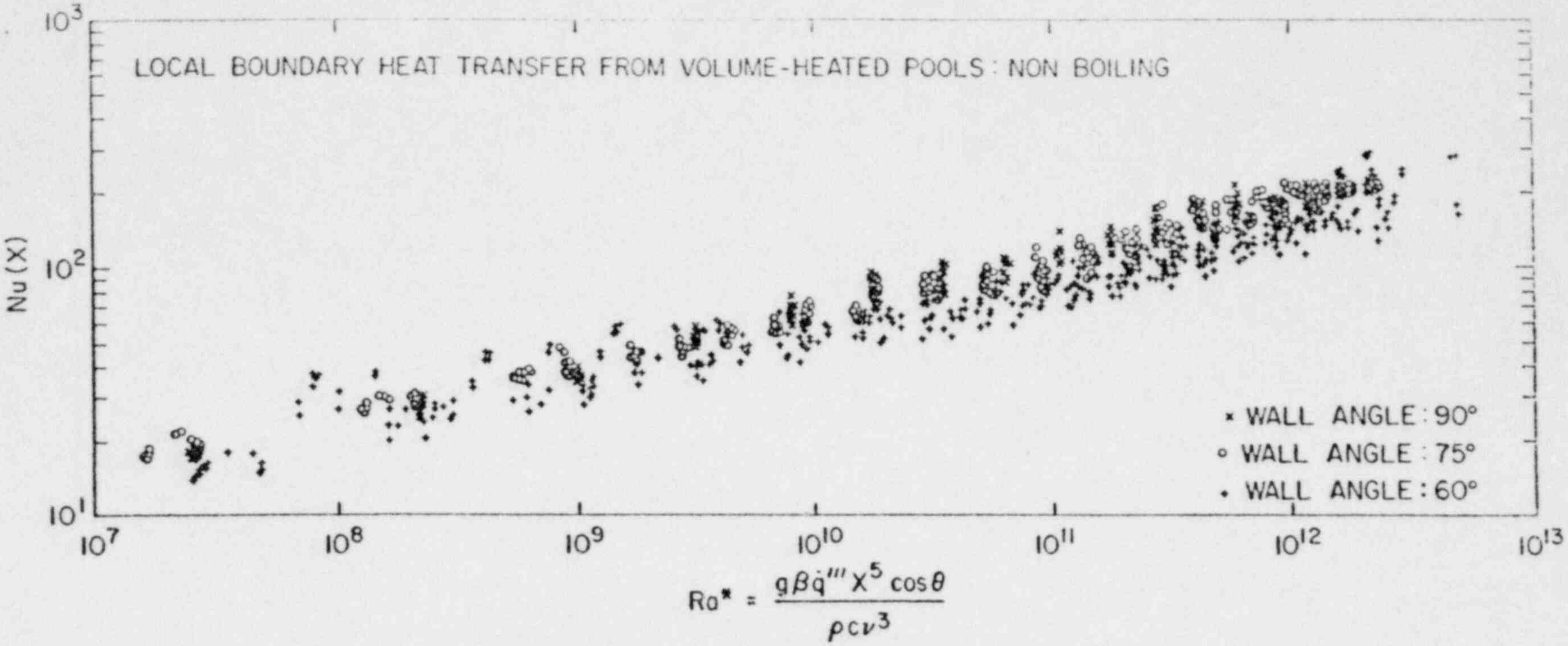
  * Joule Heating

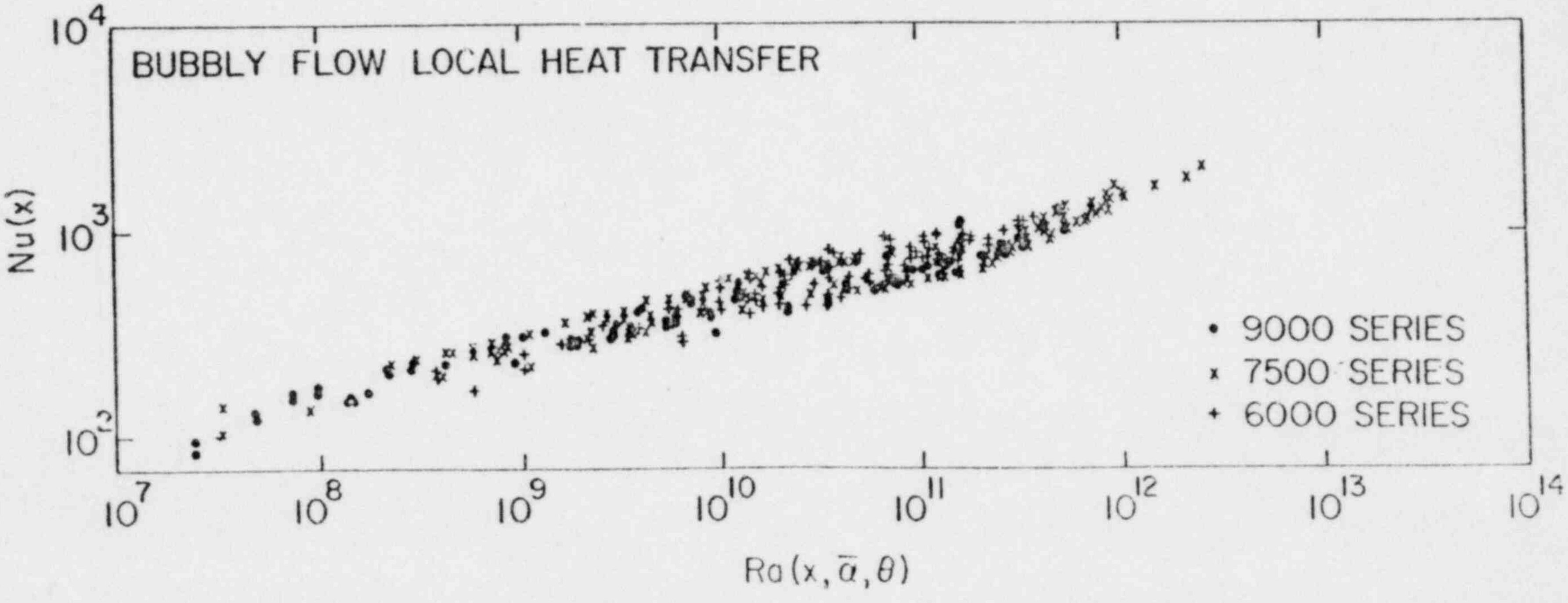
  * Hydrodynamic Simulation

  * Microwave Heating



HEAT TRANSFER APPARATUS FOR
BOILING POOL STUDIES


LOCAL BOUNDARY HEAT TRANSFER FROM VOLUME-HEATED POOLS: NON BOILING
Nu(X)
10^3
10^2
10^1
10^7
10^8
10^9
10^{10}
10^{11}
10^{12}
10^{13}
$Ra^* = \frac{g\beta\dot{q}'''X^5\cos\theta}{\rho c\nu^3}$
× WALL ANGLE: 90°
○ WALL ANGLE: 75°
+ WALL ANGLE: 60°


BUBBLY FLOW LOCAL HEAT TRANSFER
Nu(x)
10^4
10^3
10^2
10^7
10^8
10^9
10^{10}
10^{11}
10^{12}
10^{13}
10^{14}
$Ra(x, \bar{\alpha}, \theta)$
• 9000 SERIES
x 7500 SERIES
+ 6000 SERIES

BOILING POOL HEAT TRANSFER

- Conclusions

Bubbly Flow

- Boundary Heat Transfer Mechanism Resembles Natural Convection by Void-Induced Buoyancy.
- Heat Transfer Correlation Developed.
- Local Heat Transfer Greater Than Previous Estimates by Factor of 2.

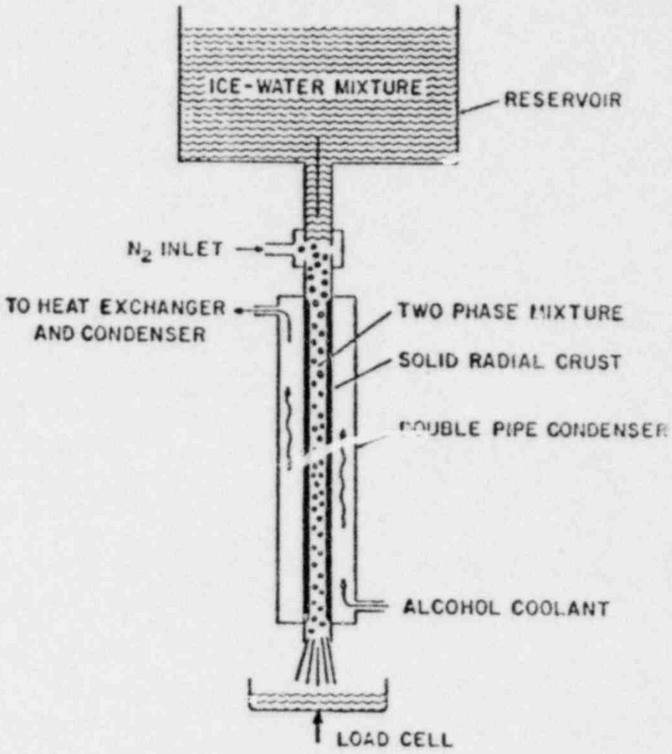
Churn Flow

- Boundary Heat Transfer Mechanism Uncertain.
- Average Heat Transfer Greater Than Bubbly By Factor of Two.
- Transition From Bubbly to Churn at $J_{G\infty}/U_{\infty} \approx 1$
- More Work Needed.

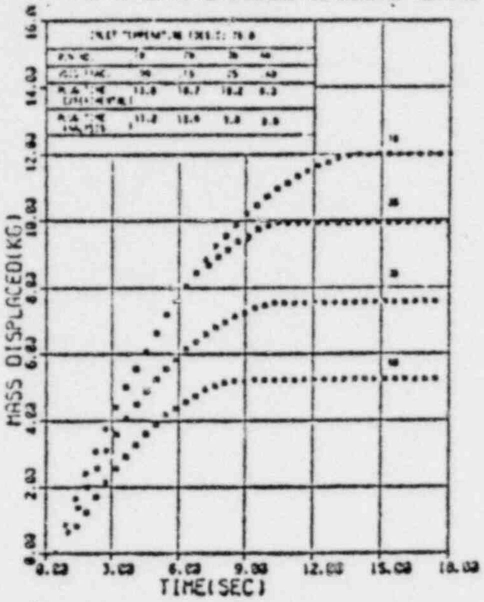
FUEL RELOCATION DYNAMICS SIMULATION STUDIES

OBJECTIVES

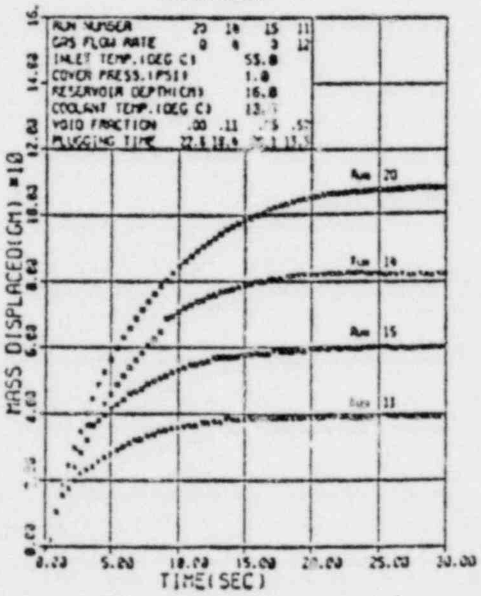
- Predict Potential for Formation of Frozen Barriers in Normally Open Flow Channels in the Core of an LMFBR Following a HCDA, Both During Transition Phase and PAHR.
- Predict Potential for Formation of a "Bottled" Core During Transition Phase Which is of Interest in Pressure-Driven Recompaction and Recriticality.
- Predict the Possible Distribution of Fuel During Transition Phase.
- Determine Subsequent Effects Upon Pool Hydrodynamics and Boundary Heat Transfer from Fuel Pool Remaining in the Core.



TYPICAL RESULTS



Woods Metal-
N_2
$P_r = 0.02$



Paraffin Wax-
N_2
$P_r = 40$

LIQUID-VAPOR-SOLID FREEZING TESTS

55 C PARAFFIN-WOOD PULP

RUN	GAS FRACTION	SOLID FRACTION	PLUGGING TIME*	MASS DISPLACED**
1	.00	.00	27.	143.
2	.00	.00	27.	117.
3	.10	.00	24.	81.
4	.10	.00	22.	73.
5	.00	.10	NO PENETRATION OF TUBE***	
7	.00	.10	NO PENETRATION OF TUBE	
8	.00	.10	NO PENETRATION OF TUBE	

* SECONDS

** GRAMS

*** THE TUBE WAS IDENTICAL TO THE ONE USED FOR TWO-PHASE PARAFFIN TESTS.

MULTIPHASE FREEZING DYNAMICS

- CONCLUSIONS
  - FUEL MOTION IN ABSENCE OF MELTING WALL DOMINATED BY CONDUCTION HEAT TRANSFER.
  - PRESENCE OF VAPOR IN A FLOWING-FREEZING LIQUID ACCELERATES FREEZING RATE.
  - MECHANISMS OF MECHANICAL CRUST BREAKUP ARE UNCERTAIN.
  - CORRELATIONS FOR MULTIPHASE FLOODING AND ENTRAINMENT DO NOT EXIST.
  - PRESENCE OF SOLIDS IN A FLOWING-FREEZING LIQUID APPEARS TO ACCELERATE FREEZING RATE SIGNIFICANTLY GREATER THAN VAPOR.
  - MORE WORK IS NEEDED IN THE FOLLOWING AREAS:

    SLURRY FREEZING PROBLEMS

    CRUST INSTABILITY MECHANISMS

    TWO-PHASE FLOODING AND ENTRAINMENT MODELS

DISPERSION: TASKS AND OBJECTIVES

- HYDRODYNAMIC DISPERSION
  - TASKS

    VOLUME DISTRIBUTED GAS INJECTION

    PRELIMINARY FEASIBILITY TESTS
  - OBJECTIVES

    FLOW REGIME TRANSITIONS AND MULTIPHASE
    FLOW DYNAMICS IN PROTOTYPIC FLUID SYSTEMS
- DISPERSION IN INTERNALLY HEATED BOILING POOLS
  - TASKS

    ELECTRICALLY HEATED DISPERSION TESTS (COMPLETE)

    MICROWAVE HEATED HIGH POWER DISPERSION TESTS
  - OBJECTIVES

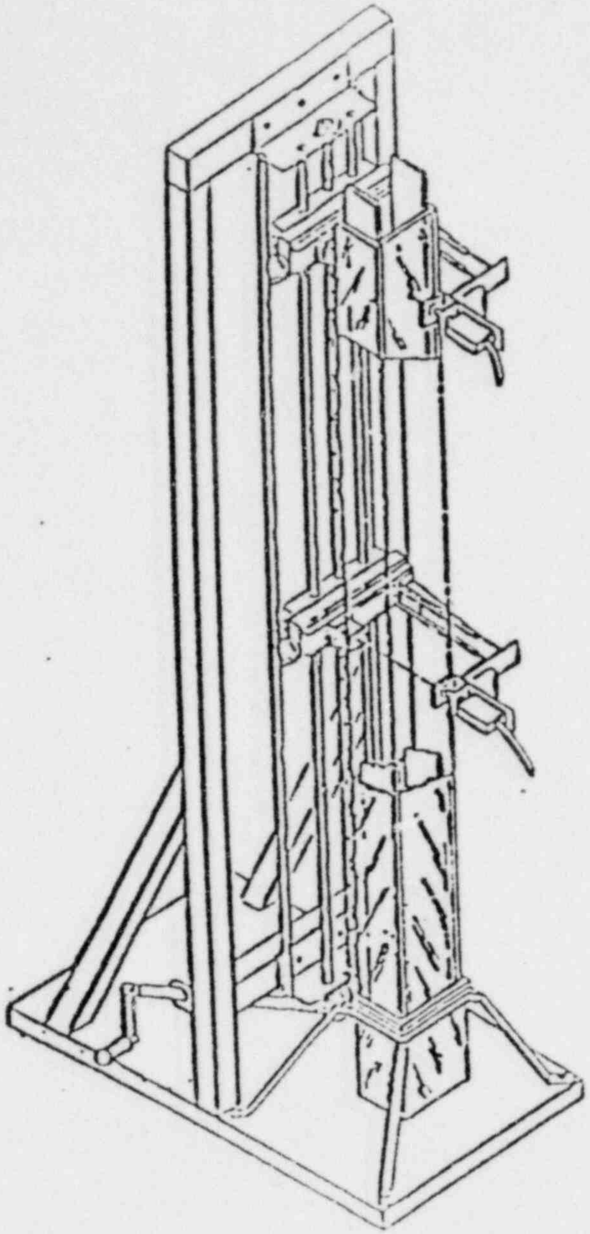
    FLOW REGIME TRANSITIONS AND MULTIPHASE

    FLOW DYNAMICS IN INTERNALLY HEATED POOLS

    OPEN, CLOSED, HEAT LOSSES, REFLUXING

HYDRODYNAMIC DISPERSAL

PRELIMINARY FEASIBILITY TESTS



TEST VESSEL AND GAMMA DENSITOMETER SYSTEM

PURPOSE

- Obtain More Definitive Visual Void and Instrumented Void Dynamic Behavior.
- Provide Design Evaluation Information for Hydrodynamic Dispersal [illegible]s.
  - Without Injector Tube Simulations
  - With Injector Tube Simulations

STATUS

- Tests Complete.
- Data Analysis in Progress.

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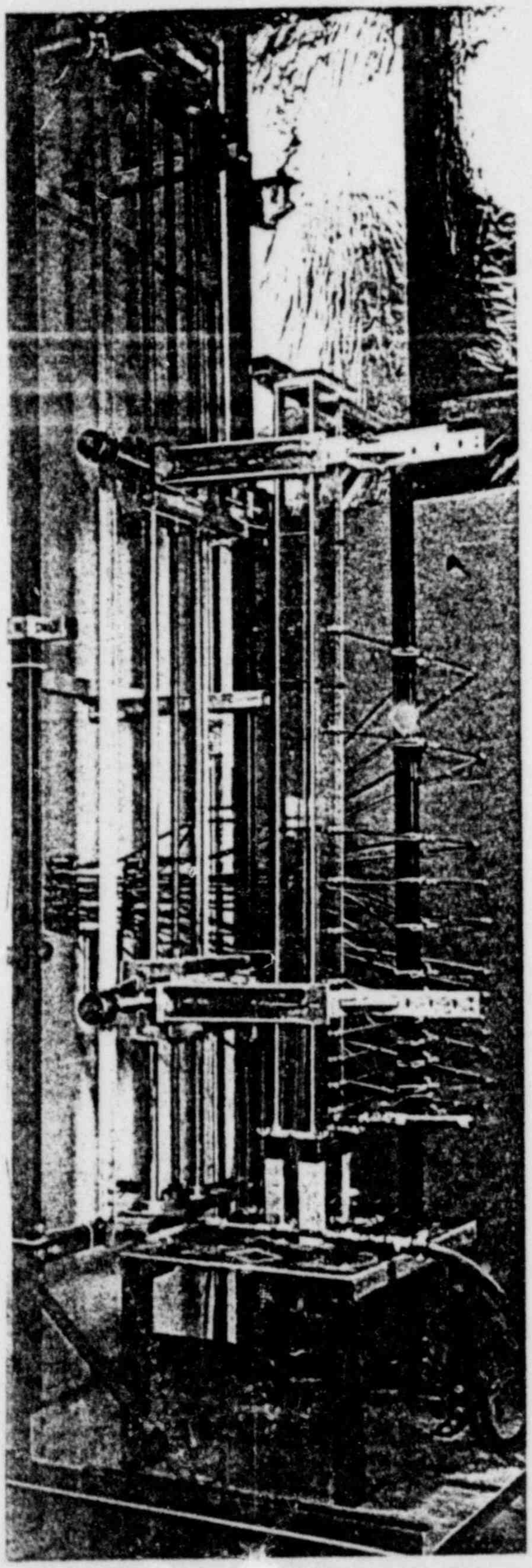
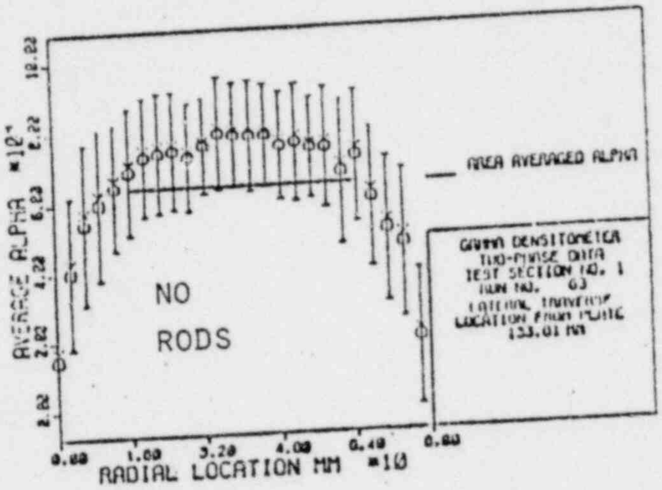
ASSOCIATED UNIVERSITIES, INC. aui

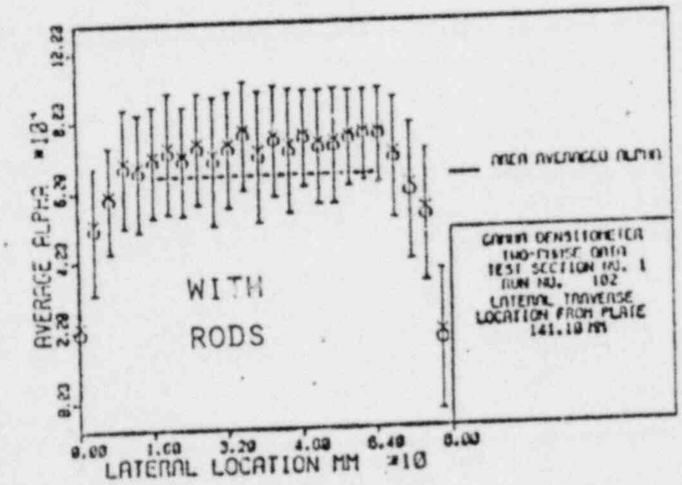
HYDRODYNAMIC SIMULATION

APPARATUS



TYPICAL RESULTS





HYDRODYNAMIC DISPERSAL

- CONCLUSIONS (PRELIMINARY TESTS)
  - ANALYSIS OF DATA FOR EFFECT OF INJECTOR TUBES UNDERWAY AT PRESENT.
  - CHURN FLOW REGIME STABLE TO GAS FLUX APPROXIMATELY 3.5 TIMES KUTATELADZE PREDICTION.

FUTURE DIRECTIONS

TASK	FUTURE WORK
ASSESSMENT OF T.P. PHENOMENOLOGY.	ISSUE FINAL REPORT.
BOILING POOL H.T.	COMPLETE.
FUEL RELOCATION.	ISSUE FINAL REPORT IN TWO-PHASE TESTS. INITIATE STUDIES OF SLURRY BEHAVIOR, CRUST STABILITY EFFECTS.
HYDRODYNAMIC DISPERSAL.	INITIATE STUDY WITH ALTERNATE FLUID SIMULATIONS.
DISPERSION IN VOLUME-HEATED POOLS.	MICROWAVE SIMULATIONS.
HCDA ENERGETICS: ENTRAINMENT BY TAYLOR INSTABILITY.	ISSUE FINAL REPORTS.

AEROSOL RELEASE AND TRANSPORT (ART)
PROGRAM AT ORNL

PREPARED BY

T. S. KRESS

ART PROGRAM MANAGER

PRESENTED TO

ACRS SUBCOMMITTEE ON ADVANCED REACTOR SAFETY

WASHINGTON, D.C.

JULY 9, 1980

PRINCIPLE AREAS OF THE AEROSOL RELEASE AND TRANSPORT PROGRAM AT ORNL

OVERALL OBJECTIVE:

TO PROVIDE DATA AND ANALYTICAL METHODS FOR USE IN ASSESSING RADIOLOGICAL CONSEQUENCES OF SEVERE ACCIDENTS

PRIMARY CONTAINMENT STUDIES:

INTERACTIONS OF ENERGETIC MOLTEN UO_2 WITH SODIUM TO DETERMINE THE CHARACTERISTICS OF BUOYANT TRANSPORT OF RADIONUCLIDES WITHIN THE PRIMARY VESSEL (QUANTITY AND CHARACTERISTICS OF MATERIAL RELEASED INTO SECONDARY CONTAINMENT)

SECONDARY CONTAINMENT STUDIES:

BEHAVIOR OF NUCLEAR AEROSOL MIXTURES IN THE CONTAINMENT ATMOSPHERE (VALIDATION OF MODELS FOR NATURAL ATTENUATION MECHANISMS TO ESTABLISH RELEASE FROM THE SECONDARY CONTAINMENT)

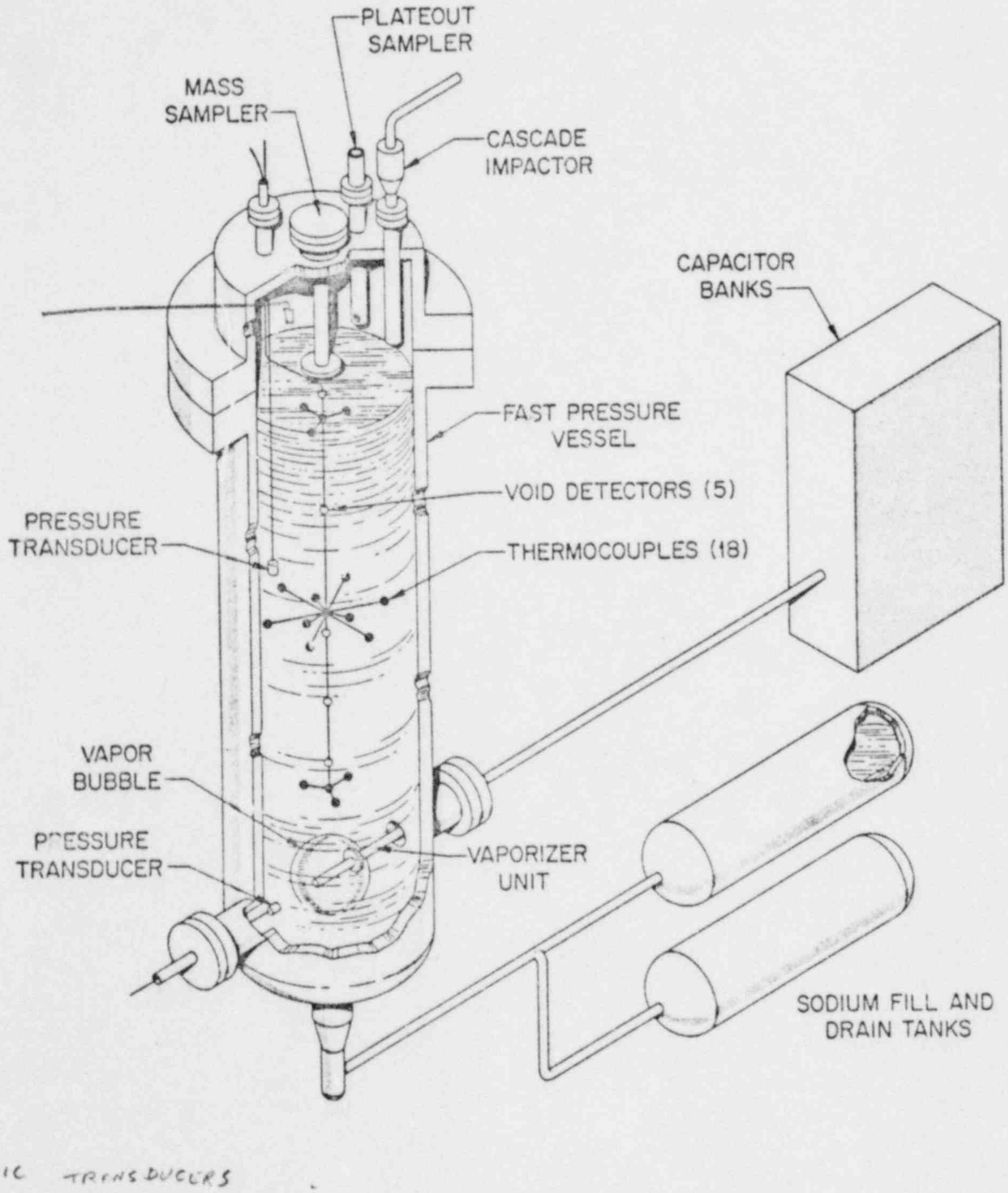
THE IN-PRIMARY VESSEL EXPERIMENTS ARE CONDUCTED IN THE FAST FACILITY

- A SODIUM SYSTEM WITH A MODEL VESSEL ABOUT 1/10 THE SCALE OF AN LMFBR PRIMARY VESSEL
- SMALL SAMPLES OF UO_2 (TYPICALLY ~20 G) ARE ELECTRICALLY HEATED VIA DISCHARGE FROM CAPACITORS TO MOLTEN ENERGY STATES OF $\lesssim$ 4000 J/G
- THE RESULTANT VAPOR BUBBLES ARE OBSERVED FOR:
  - PHENOMENA IDENTIFICATION
  - DYNAMIC BEHAVIOR
  - CONDENSATION AND/OR TRANSPORT OF MATERIAL THROUGH THE LIQUID TO THE COVER GAS

UNION CARBIDE

ornl

THE UNDER-SODIUM TESTS ARE CONDUCTED IN THE FAST FACILITY



+ ULTRASONIC TRANSDUCERS MOUNTED ON VESSEL EXTERIOR

THE FAST EXPERIMENT PROGRAM USING THE CAPACITOR DISCHARGE VAPORIZATION (CDV) TECHNIQUE TO PRODUCE UO_2 DISASSEMBLIES IS BEING CONDUCTED IN THREE PHASES:

1. DISASSEMBLIES IN ARGON ENVIRONMENTS
2. UNDER WATER
3. UNDER SODIUM

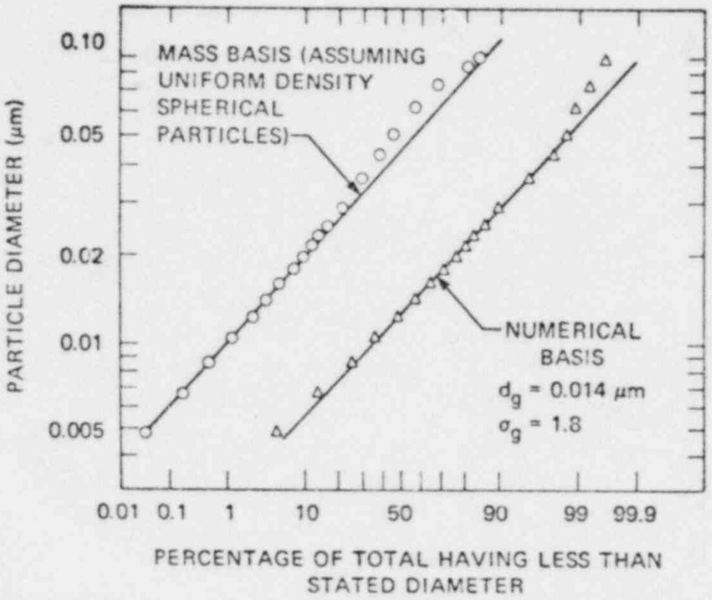
OBJECTIVES OF THE FAST "ARGON" EXPERIMENTS

- TO DEVELOP AND UNDERSTAND THE CDV SYSTEM
- TO PRODUCE AND CHARACTERIZE UO_2 CONDENSATION AEROSOLS
- TO MEASURE THE VAPOR YIELDS AS A FUNCTION OF THE ENERGY STATE OF THE SAMPLES

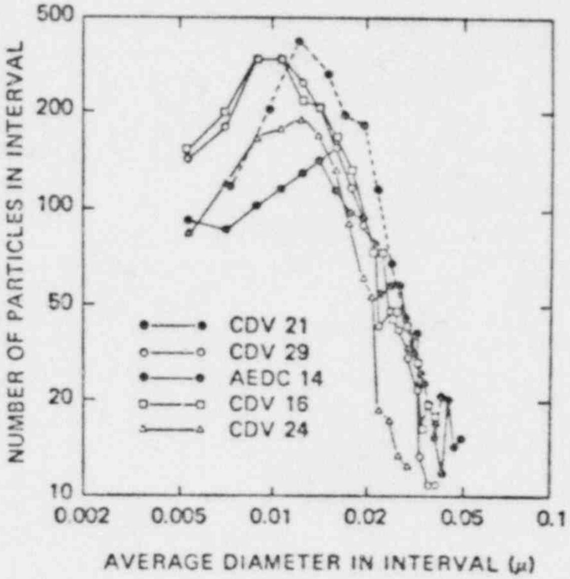
UNION CARBIDE
ORNL

THE PRIMARY SIZE DISTRIBUTION OF CDV-PRODUCED AEROSOL WAS LOG-NORMAL; THIS DISTRIBUTION WAS INSENSITIVE TO THE TEST SAMPLE ENERGY STATE AFTER CDV

CUMMULATIVE SIZE DISTRIBUTION



SIZE DISTRIBUTION FOR FIVE TESTS



EXAMPLE OF MEASURED YIELDS IN THE FAST CDV ARGON EXPERIMENTS (☒, •) COMPARED TO ANALYTICAL PREDICTIONS; — ○ - WITHOUT CONSIDERING DENSITY & ELEC. CONDUCTIVITY CHANGES THRU MELT
— o — CONSIDERING DENSITY & ELEC. CONDUCTIVITY CHANGES THRU MELT.

IMPLICATIONS OF THE ARGON TEST RESULTS

- THE SIZE DISTRIBUTION IS A FUNCTION OF THE LEVEL OF SUPERSATURATION OF THE VAPOR (COMPETITION BETWEEN HOMOGENEOUS NUCLEATION AND CONDENSATIONAL GROWTH). IF THE DOMINANT MODE OF HEAT TRANSFER IS RADIATION, THEN WE WOULD EXPECT TO OBSERVE SIMILAR SIZES AND DISTRIBUTIONS IN THE UNDERWATER AND UNDER-SODIUM TESTS – AND WOULD EXPECT THE SAME IN THE REACTOR.

- PENDING FURTHER EXPERIMENTS TO VERIFY THE CALCULATED CDV ENERGY DISTRIBUTION, THE MEASURED YIELDS MAY VALIDATE THE INHERENT CONSERVATISM OF THE PRESENT PRACTICE OF MAKING EQUILIBRIUM ADIABATIC CALCULATIONS TO ESTABLISH THE VAPOR QUALITIES ON RAPID EXPANSION OF MOLTEN FUEL.

- KNOWLEDGE OF THE PRIMARY SIZES CAN HELP UNDERSTAND (AND SYNTHESIZE) AGGLOMERATE PROPERTIES FOR USE IN AEROSOL BEHAVIOR ANALYSES.

- THE "BY-PRODUCT" RESULTS OF CHANGE IN ELECTRICAL RESISTANCE OF UO_2 ON MELTING MAY INCREASE THE UNDERSTANDING OF THE MECHANISMS FOR CHANGES IN THERMAL CONDUCTIVITY.

OBJECTIVES OF THE FAST UNDER-WATER EXPERIMENTS

- TO VALIDATE THE CDV DESIGN FOR UNDER SODIUM USE
- TO IDENTIFY THE EXPANSION PHENOMENA THROUGH THE USE OF HIGH SPEED MOTION PICTURES
- TO CORRELATE INSTRUMENT RESPONSES WITH VISUAL OBSERVATIONS FROM THE FILMS
- TO QUANTIFY THE CONDENSATION AND TRANSPORT RATES UNDER WATER
- TO DEVELOP A NEW ULTRASONIC IMAGING SYSTEM SUITABLE FOR TRACKING BUBBLE MOTION UNDER SODIUM

Appendix B

Actual Conditions For Under-Water Tests Discussed In This Report*

Test	P_A(MPa)	P_X(MPa)	T_W(K)	L(mm)	E_{CDV}(kJ)
FAST 22	.122	.135	298	1120	35.7
FAST 23	.123	.135	298	1120	36.5
FAST 24	.122	.135	298	1120	23.4
FAST 25	.128	.513	298	1120	35.2
FAST 29	1.029	1.063	298	1120	32.6
FAST 30	2.045	2.073	298	1120	40.7
FAST 31	2.094	2.025	298	1120	30.9
FAST 32	.307	.335	298	1120	36.0
FAST 34	.126	.149	298	1120	35.2
FAST 35	2.059	2.094	298	1120	32.6
FAST 36	.025	.513	298	1120	34.6
FAST 37	.025	.513	298	1120	28.9
FAST 40	.108	.513	298	1120	56.3
FAST 41	.122	.513	364	1120	29.4
FAST 42	.108	.513	298	1120	21.4
FAST 43	.111	.513	364	1120	32.1
FAST 44	.122	.513	339	1120	41.1
FAST 45	.123	.513	298	1120	35.9
FAST 46	.122	.513	363	1120	41.4
FAST 51	.123	.513	298	710	25.0
FAST 52	.122	.520	298	710	26.6
FAST 53	.122	.513	298	710	15.6

*P_A = argon gas pressure

P_X = xenon gas pressure

T_W = water temperaure

L = water height

E_{CDV} = CDV energy input

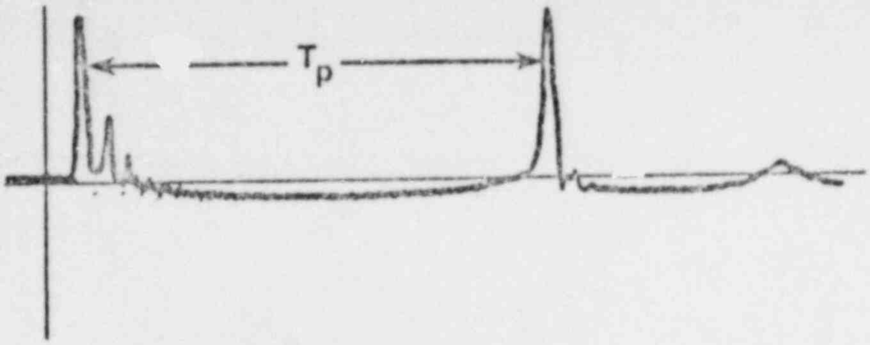
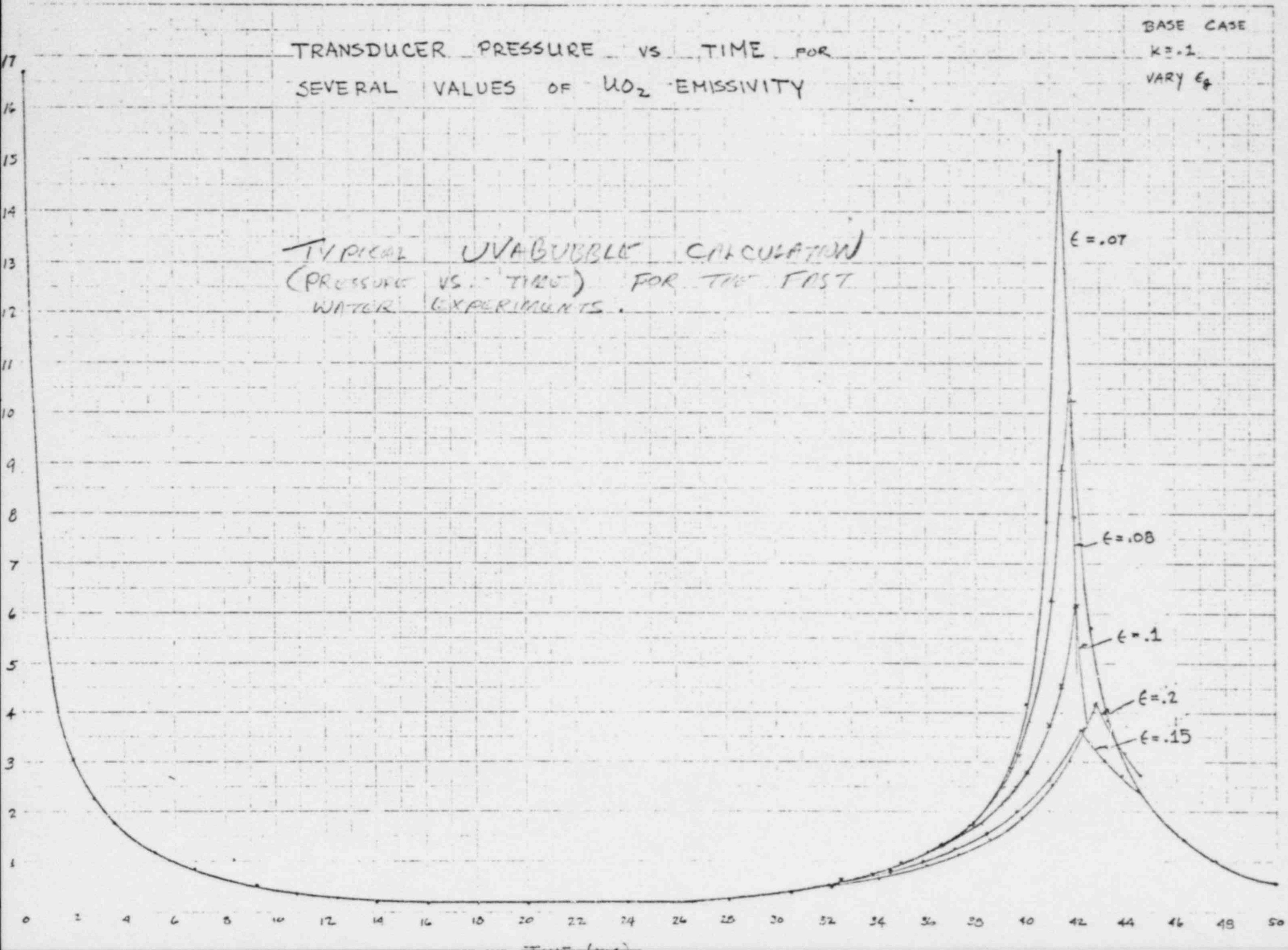


Fig. 6. Typical FAST Under-Water Test Pressure vs Time Data (from FAST 22). Pressure Pulses Shown Occurred At 0.9, 58.3, and 86.8 ms After Sample Breakup And Had Magnitudes Of 1.45, 1.40, and 0.23 MPa, Respectively. Pressure Was Measured $\sim$23 cm From Test Sample. Time Between First Two Pulses Is Time Noted As T_p In Text.

Measured Initial Bubble Periods T_p For Various Test Conditions (For 1.12 m Water Height)

Test Conditions	Initial Bubble Period T_p (ms)
$P_w = P_x = 2.02$ MPa $T_w = 298$ K	$\sim$4
$P_w = P_x = .101$ MPa $T_w = 298$ K	$\sim$50
$P_w = .101$ MPa $P_x = .505$ MPa $T_w = 298$ K	$\sim$55
$P_w = .101$ MPa $P_x = .505$ MPa $T_w = 363$ K	$\sim$80
$P_w = .025$ MPa $P_x = .505$ MPa $T_w = 298$ K	$\sim$180



TRANSDUCER PRESSURE VS TIME FOR
SEVERAL VALUES OF UO_2 EMISSIVITY

BASE CASE
K = .1
VARY ϵ_f

TYPICAL UVABUBBLE CALCULATION
(PRESSURE VS. TIME) FOR THE FAST
WATER EXPERIMENTS.

SUMMARY OF FAST UNDER-WATER TEST OBSERVATIONS

- A CDV DISASSEMBLY PRODUCES A SINGLE COHERENT OSCILLATING BUBBLE
- INSPECTION OF THE FILMS INDICATES THAT AEROSOLS ARE FORMED WITHIN THE BUBBLE
- THE UO_2 VAPOR BUBBLES RAPIDLY CONDENSE (IN 100-200 MSEC DURING THE OSCILLATION PHASE)
- TRANSPORT OF UO_2 TO THE COVER GAS HAS BEEN VERY SMALL AND HAS BEEN OBSERVED ONLY IN TESTS IN WHICH THE INITIAL WATER TEMPERATURE HAS BEEN SET AT NEAR THE BOILING POINT
- THE MEASURED BUBBLE OSCILLATION PERIODS (FUNCTION OF THE HYDRODYNAMICS AND HEAT TRANSFER) HAVE BEEN IN GENERAL AGREEMENT WITH ANALYTICAL PREDICTIONS USING THE UVABUBBLE MODEL AT THE UNIVERSITY OF VIRGINIA

STATUS OF THE UNDER-WATER TEST INSTRUMENT DEVELOPMENT OBJECTIVES

- FAST RESPONSE SUBMERSIBLE PRESSURE TRANSDUCERS
  - EFFECTIVE OPERATION UNDER WATER
  - REQUIRE INSULATION FROM GROUND TO PREVENT CDV DISCHARGE INTERFERENCE
  - GAVE GOOD INDICATION OF BUBBLE OSCILLATION FREQUENCY
  - GAVE GOOD MEASURE OF PRESSURES PRODUCED BY CDV DISCHARGE AND/OR FCI
  - NOW READY FOR UNDER SODIUM PERFORMANCE TESTING

- COVER-GAS PRESSURE TRANSDUCERS
  - EFFECTIVE OPERATION IN WATER TESTS
  - RESPONSE CAN BE INTERPRETED AS A MEASURE OF BUBBLE VOLUME VS TIME
  - A SMALLER RANGE UNIT WOULD BE MORE SENSITIVE - THESE ARE BEING INSTALLED FOR SODIUM TESTS

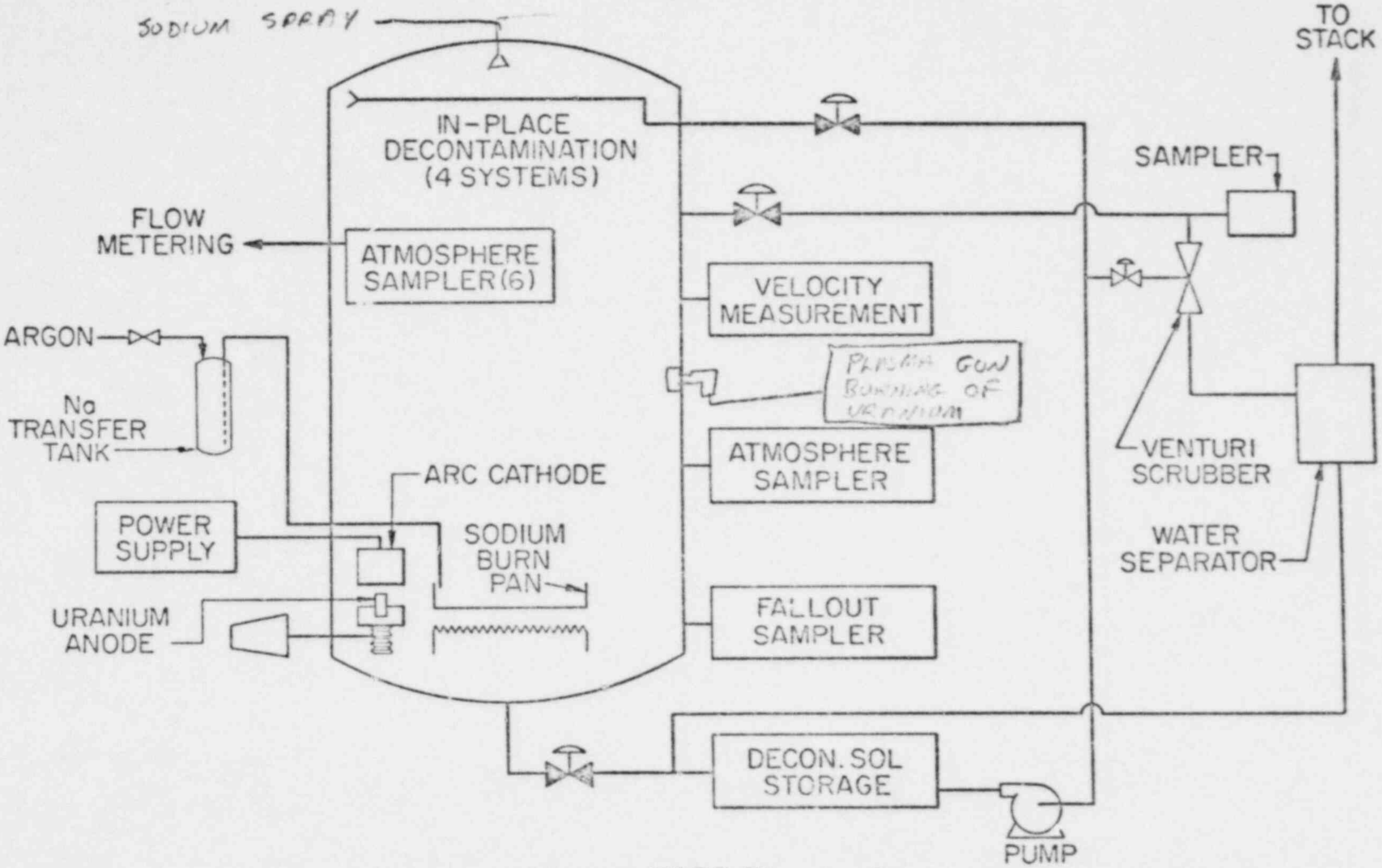
- THERMOCOUPLE ARRAY
  - LACK OF A RISING BUBBLE IN THE WATER TESTS PREVENTED CONTACT WITH MOST OF THESE
  - LIMITED CONTACT WITH NEAREST TC'S INDICATED THAT RESPONSE IS TOO SLOW TO INDICATE BUBBLE INTERFACE POSITION AS HOPED FOR

- ULTRASONIC BUBBLE IMAGING
  - FEASIBILITY DEMONSTRATED IN AUXILIARY VESSEL USING RISING SPHERES AND A SINGLE SUBMERSED TRANSDUCER
  - TWO CDV TESTS WITH ONE TRANSDUCER MOUNTED ON THE OUTSIDE OF THE FAST VESSEL PRODUCED SIGNALS INTERPRETABLE IN TERMS OF BUBBLE SIZE; TWO OTHER TESTS INDICATED FAULTY MOUNTING.

- ULTRASONIC BUBBLE IMAGING (CONT'D)
  - ADDITIONAL DEVELOPMENT NEEDED:
    - WAVE-GUIDE MOUNTING ON FAST VESSEL TO ALLOW USE WITH HIGHER TEMPERATURE (SODIUM TESTS)
    - TESTING OF SIMULTANEOUS OPERATION OF MULTIPLE ARRAYS OF TRANSDUCERS

- EXPERIMENTS IN THE NSPP AND IN CRI-II ARE CONDUCTED BY SIMULTANEOUSLY BURNING URANIUM AND SODIUM TO PRODUCE AIRBORNE MIXTURES OF U_3O_8 AND NA_2O_2 AEROSOLS SIMULATING SECONDARY CONTAINMENT CONDITIONS FOR SEVERE LMFBR ACCIDENT CASES. THE OBJECTIVES OF THESE TESTS ARE TO:
  - PROVIDE EXPERIMENTAL VALIDATION TO THE HAARM-3 AEROSOL CODE
  - ESTABLISH VALIDITY OF CO-AGGLOMERATION ASSUMPTION FOR MIXTURE AEROSOLS
  - CHARACTERIZE MIXTURE AEROSOL PARAMETERS
  - PROVIDE COMPARATIVE EVALUATIONS FOR DIFFERENT SIZE VESSELS (SCALING TO CONTAINMENT SIZE)

ORNL-DWG 75-9647



NSPP Flowsheet.

A test matrix is proposed that would scope the major parameters

Test	Mixing Time	Total Mass Concentration (µg/cc)	Mass Ratio (U_3O_8/Na_2O)	~No. Density Ratio (U_3O_8/Na_2O)	Size Relationship (Mass Equiv) D_f/D_s
1.	Simultaneous	~20	10/10	~2×10^3	1/20(Source Aerosols)
2.	Simultaneous	~20	18/2	~2×10^6	1/20(Source Aerosols)
3.	Simultaneous	~20	2/18	~2×10^2	1/20(Source Aerosols)
4.	U_3O_8 First	~20	10/10	~1	~1/1.55[a]
5.	Na_2O First	~20	10/10	~3×10^5	1/100[b]
6.	Single Component (U_3O_8)	~10	—	—	—
7.	Single Component (U_3O_8)	~18	—	—	—
*8.	Single Component (U_3O_8)	~2	—	—	—
*9.	Single Component (Na_2O)	~10	—	—	—
10.	Single Component (Na_2O)	~18	—	—	—
11.	Single Component (Na_2O)	~2	—	—	—
Any additional tests	Simultaneous	Vary up to max. obtainable	1	~2×10^3	1/20(Source Aerosols)

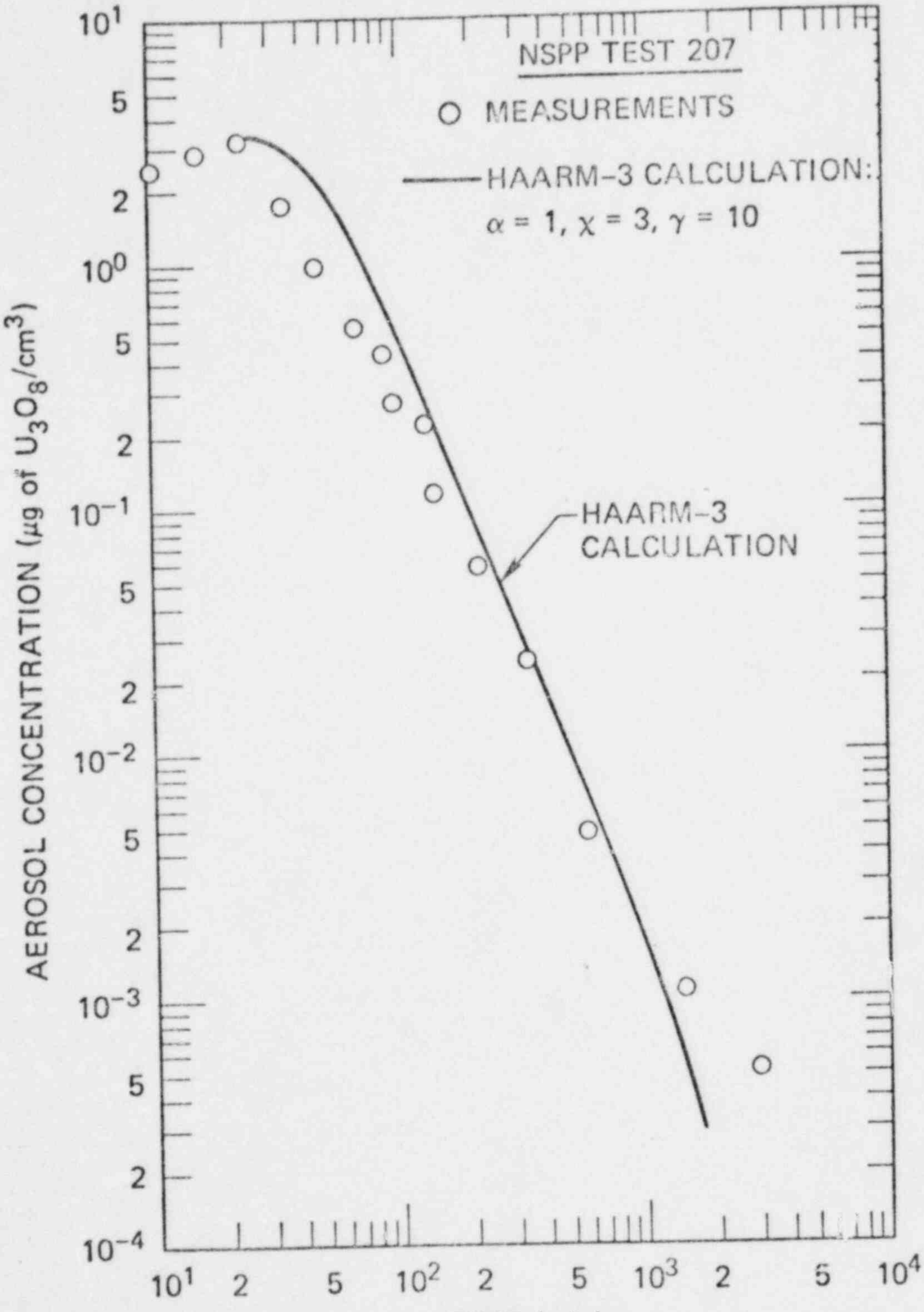
---

*Test already completed

[a] Approximate size ratio for both number density and mass conc. ratios to be 1

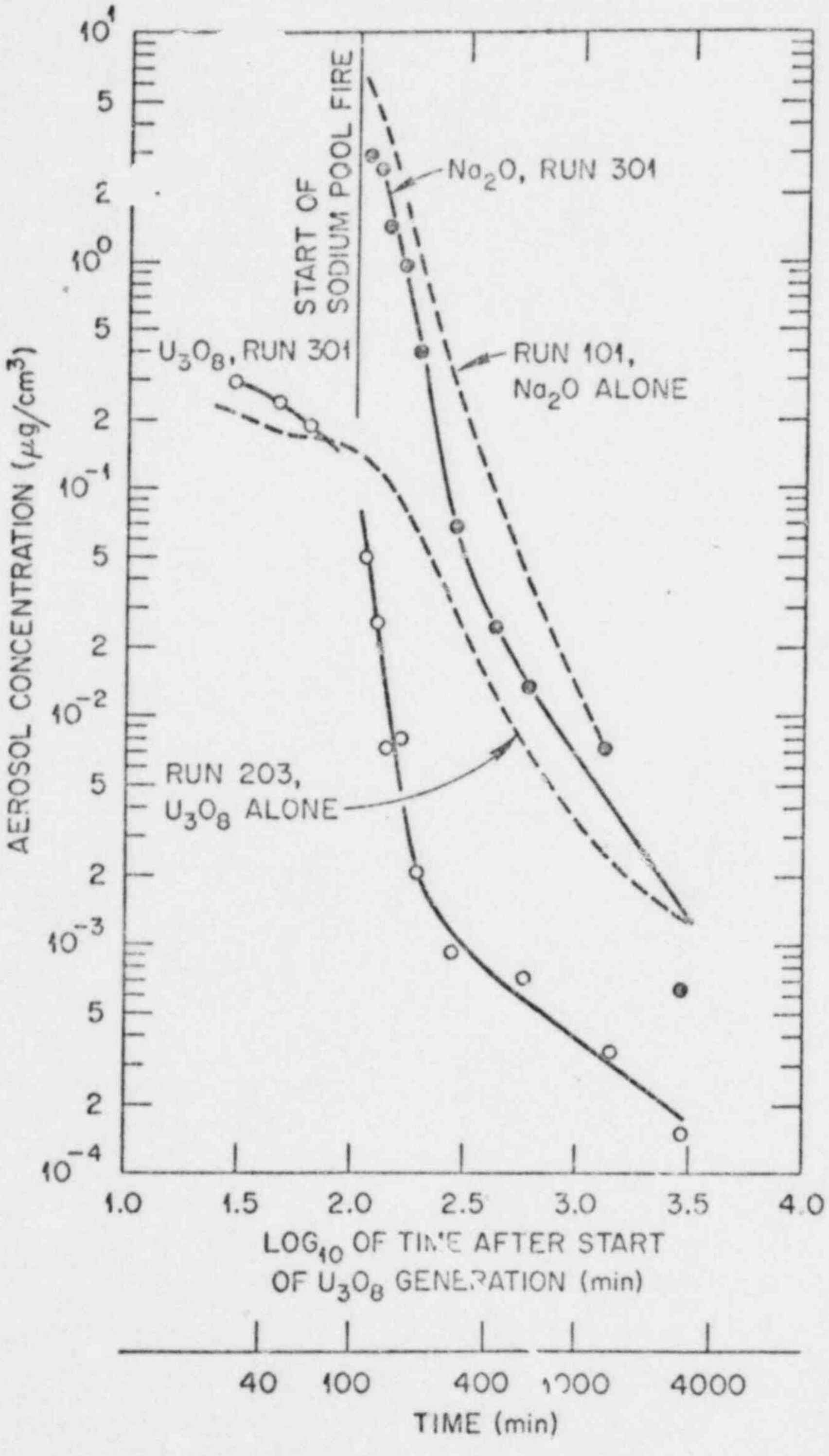
[b] Maximum size differential for this conc. level

THE ABOVE NSPP TEST MATRIX FOR SCOPING MIXED AEROSOL BEHAVIOR HAS BEEN COMPLETED,

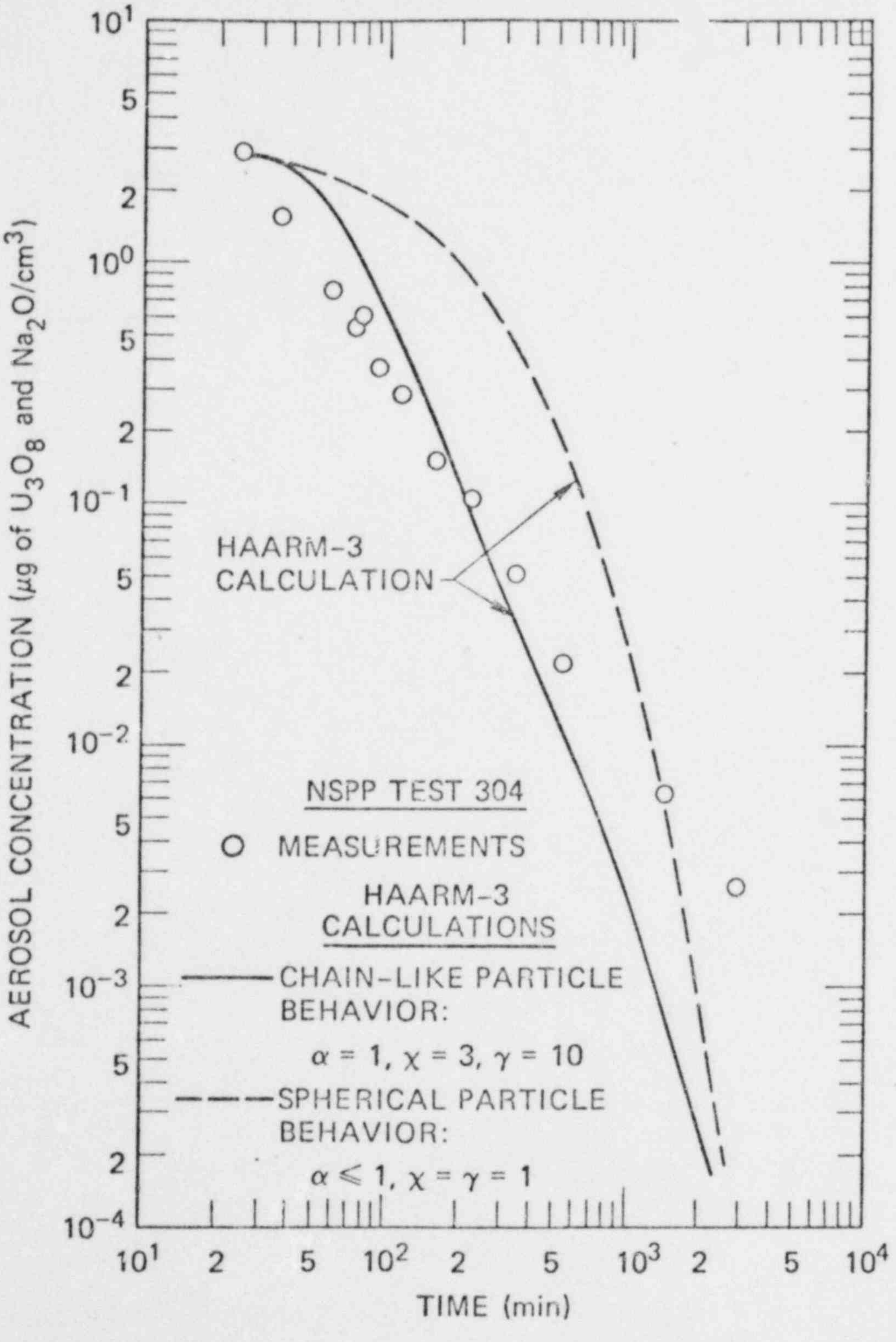


TYPICAL COMPARISON OF HAARM-3 AND NSPP EXPERIMENT WITH U_3O_8 AEROSOL ALONE.

ORNL-DWG- 78-16901

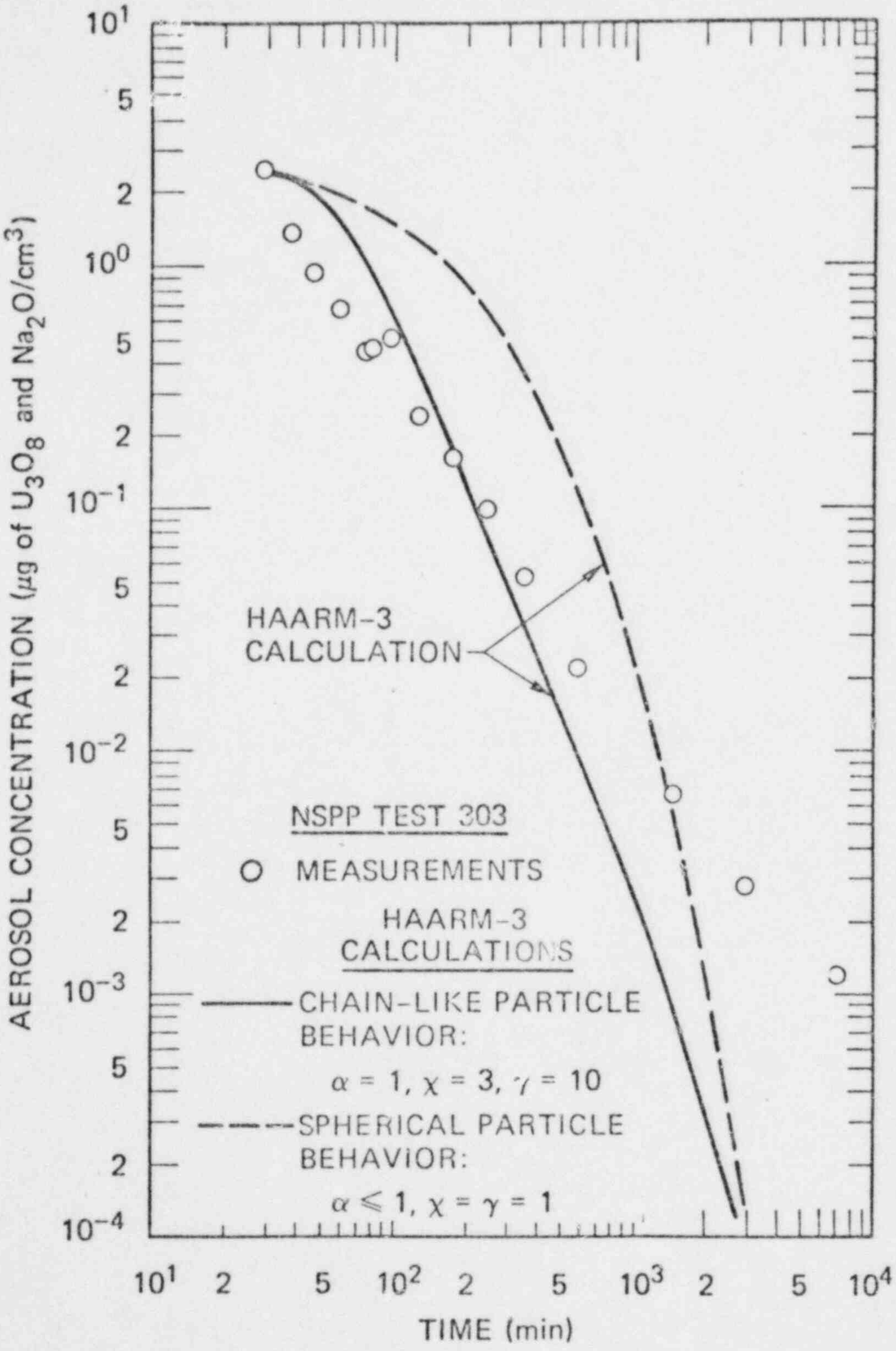


Comparison of Mixed Oxide Aerosol Behavior with Single Component Aerosol Behavior,



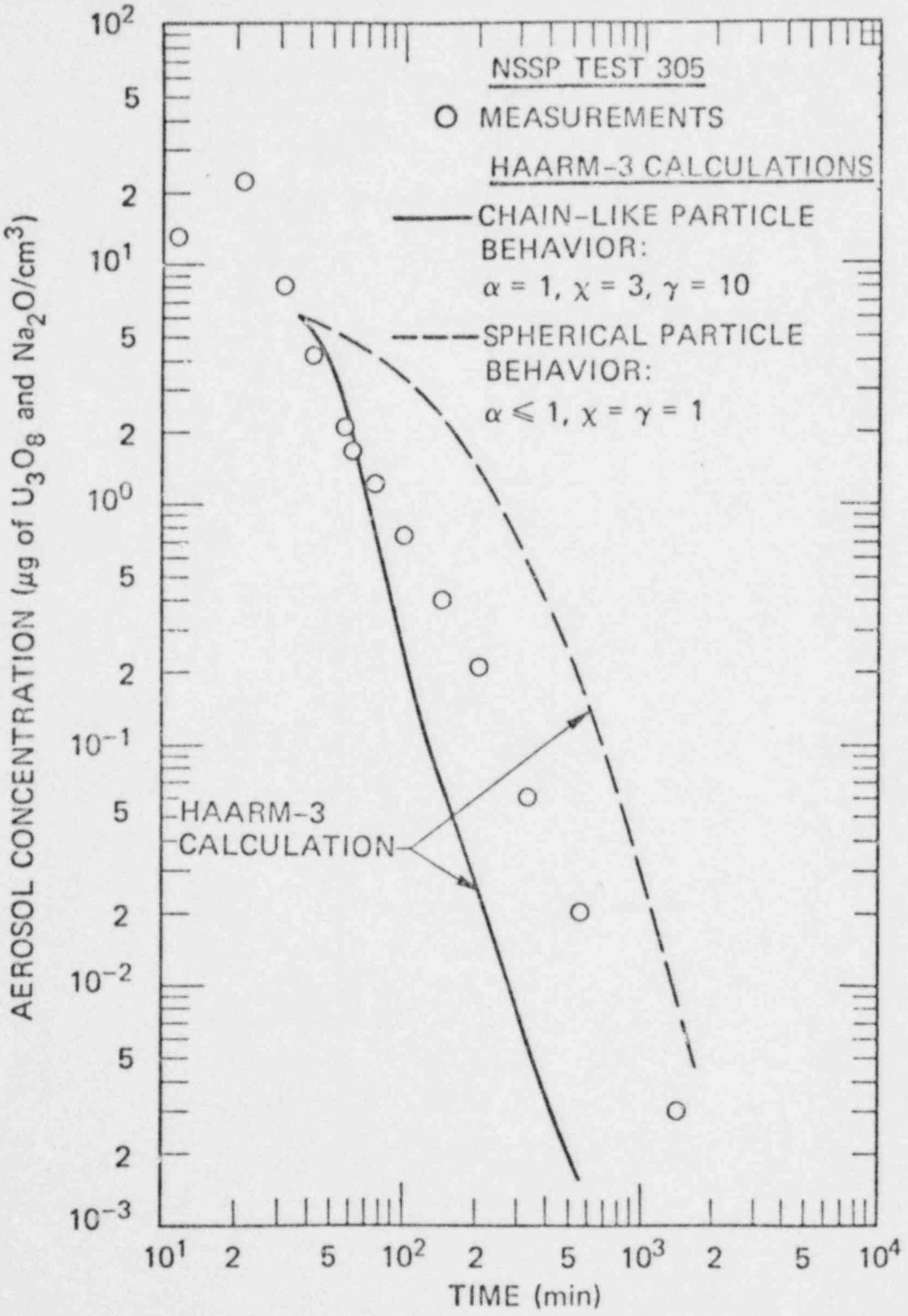
COMPARISON OF HAARM-3 WITH MIXED AEROSOL DATA FOR AN EXPERIMENT WITH THE MASS CONCENTRATION RATIO (Na_2O_2 / U_3O_8) ~ 1/10

ORNL-DWG- 80-11628



COMPARISON OF HAARM-3 WITH MIXED AEROSOL DATA FOR AN EXPERIMENT WITH THE MASS CONCENTRATION RATIO (Na_2O_2 / U_3O_8) ~ 1/1.2

ORNL-DWG- 80-4



COMPARISON OF HAARM-3 WITH MIXED AEROSOL DATA FOR AN EXPERIMENT WITH THE MASS CONCENTRATION RATIO $(Na_2O_2/U_3O_8) \sim 3.5/1$

Simultaneous generation

SOME OBSERVATIONS FROM THE NSPP MIXED AEROSOL TEST PROGRAM

- AEROSOL BEHAVIOR MODELING (HAARM-3) IS ADEQUATE FOR DESCRIBING SINGLE COMPONENT U_3O_8 OR NA_2O_2 AEROSOLS
- CO-AGGLOMERATION OF U_3O_8 AND NA_2O_2 AEROSOLS IS EVIDENT FOR MIXTURES IN ALL PROPORTIONS AND PARTICLE SIZES
- ON MIXING A CHAIN-LIKE (U_3O_8) AEROSOL WITH A SPHERICAL (NA_2O_2) ONE, THE CHAIN-LIKE SPECIE APPEARS TO DOMINATE THE BEHAVIOR OVER A WIDE RANGE OF CONCENTRATION PROPORTIONS
- THE BASIC MODEL ASSUMPTIONS CRITICAL FOR SCALING TO LARGER SIZE VESSELS APPEAR TO BE SATISFIED:
  - THE WELL MIXED CONDITION
  - GRAVITATIONAL FALLOUT IS GIVEN BY

$$\dot{M}_r \cong V_{sr} A\ Cr$$

- U_3O_8 AEROSOLS PRODUCED IN A MOIST ENVIRONMENT LOSE THEIR CHAIN-LIKE AGGLOMERATE APPEARANCE

PROPOSED FUTURE DIRECTIONS FOR NSPP/CRI-II

- VALIDATION OF CODE UTILITY FOR DESCRIBING GENERIC CORE MELT AEROSOL BEHAVIOR (INCLUDES MOISTURE AND AEROSOLS EMANATING FROM FUEL-CONCRETE INTERACTIONS)
- GENERIC AEROSOL INTERACTIONS WITH VENTED/FILTERED CONTAINMENT COMPONENTS AND CONTAINMENT COOLING SYSTEMS
- CORE-MELT FISSION PRODUCT AND AEROSOL SOURCE TERMS

AEROSOL CODE DEVELOPMENT AND QUALIFICATION

Aerosol Measurements and Modeling for Fast Reactor Safety

J. A. Gieseke,
Program Manager

Battelle Columbus Laboratories

notes for presentation to ACRS

Washington, D.C.

July ~~10~~, 1980
9

Aerosol Measurements and Modeling for Fast Reactor Safety

- Code Development
  - Analytical
    - HAARM-3
    - Reference codes (CRAB, QUICK)
  - Experimental Aerosol Properties Measurements
    - Na_2O_x
    - Fuel materials
    - Structural materials
    - Mixtures (Effect of gas--argon, air, water vapor)
- Code Verification
  - Sensitivity analyses
  - Code comparisons
  - Verification Experiments plan
  - Comparisons with data

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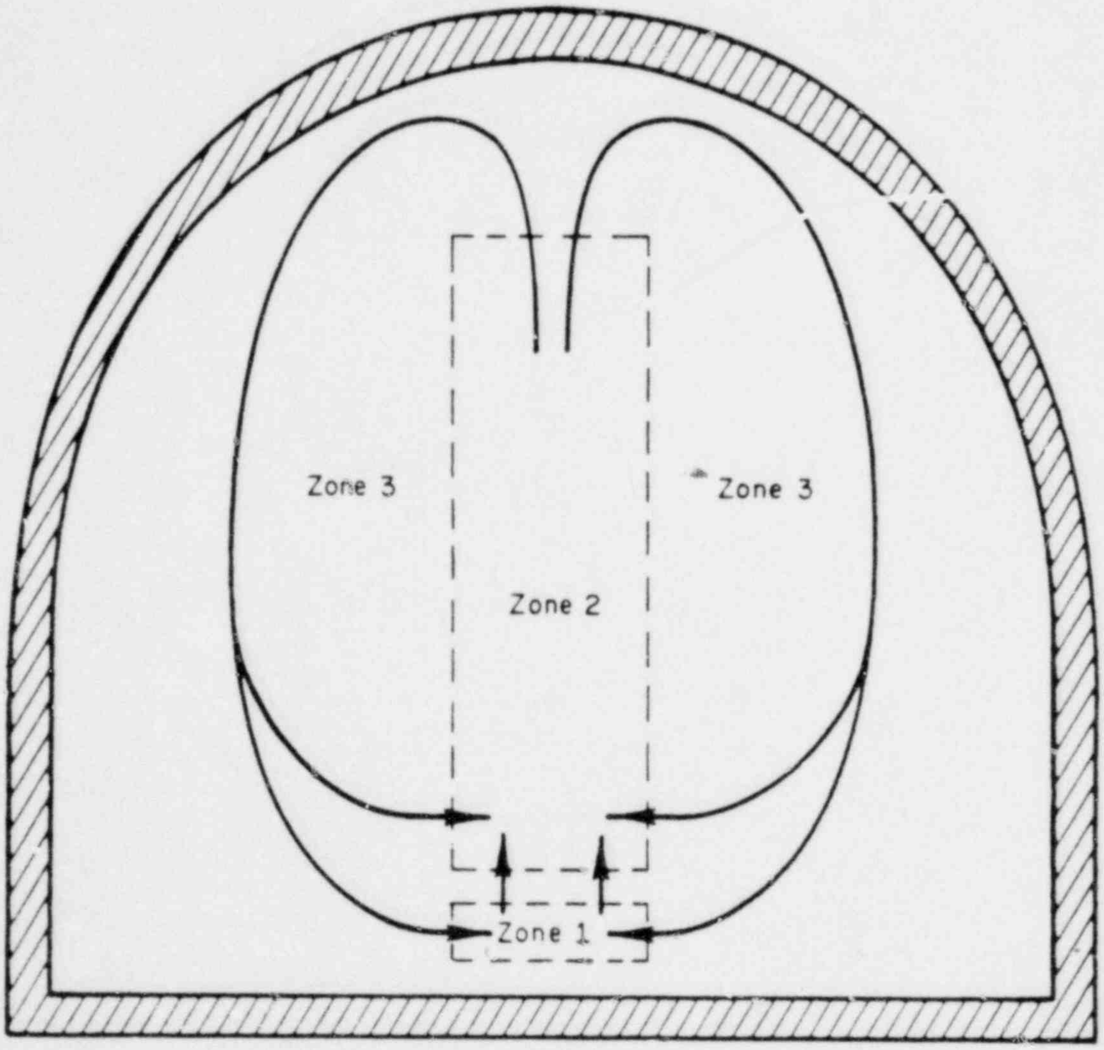
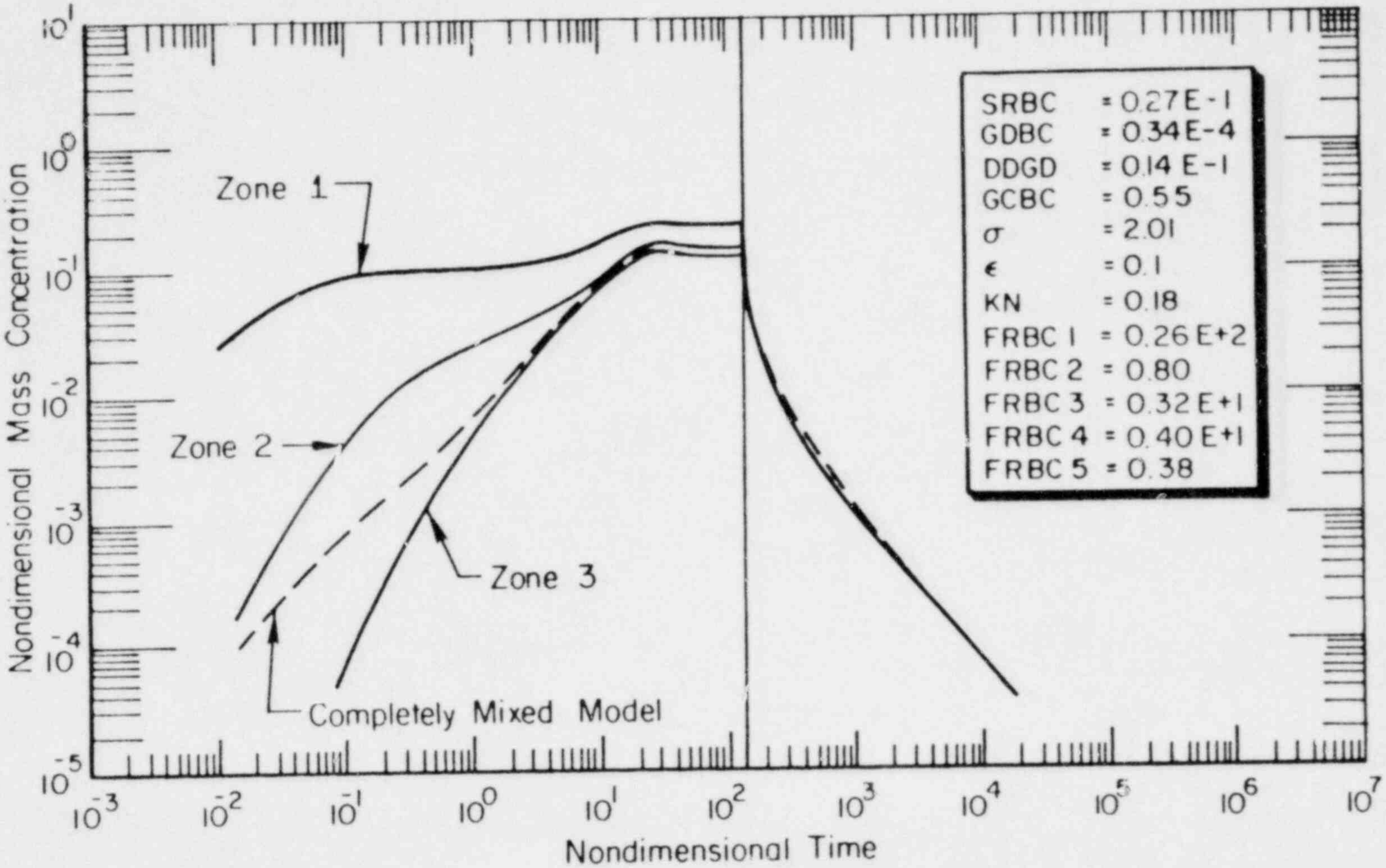
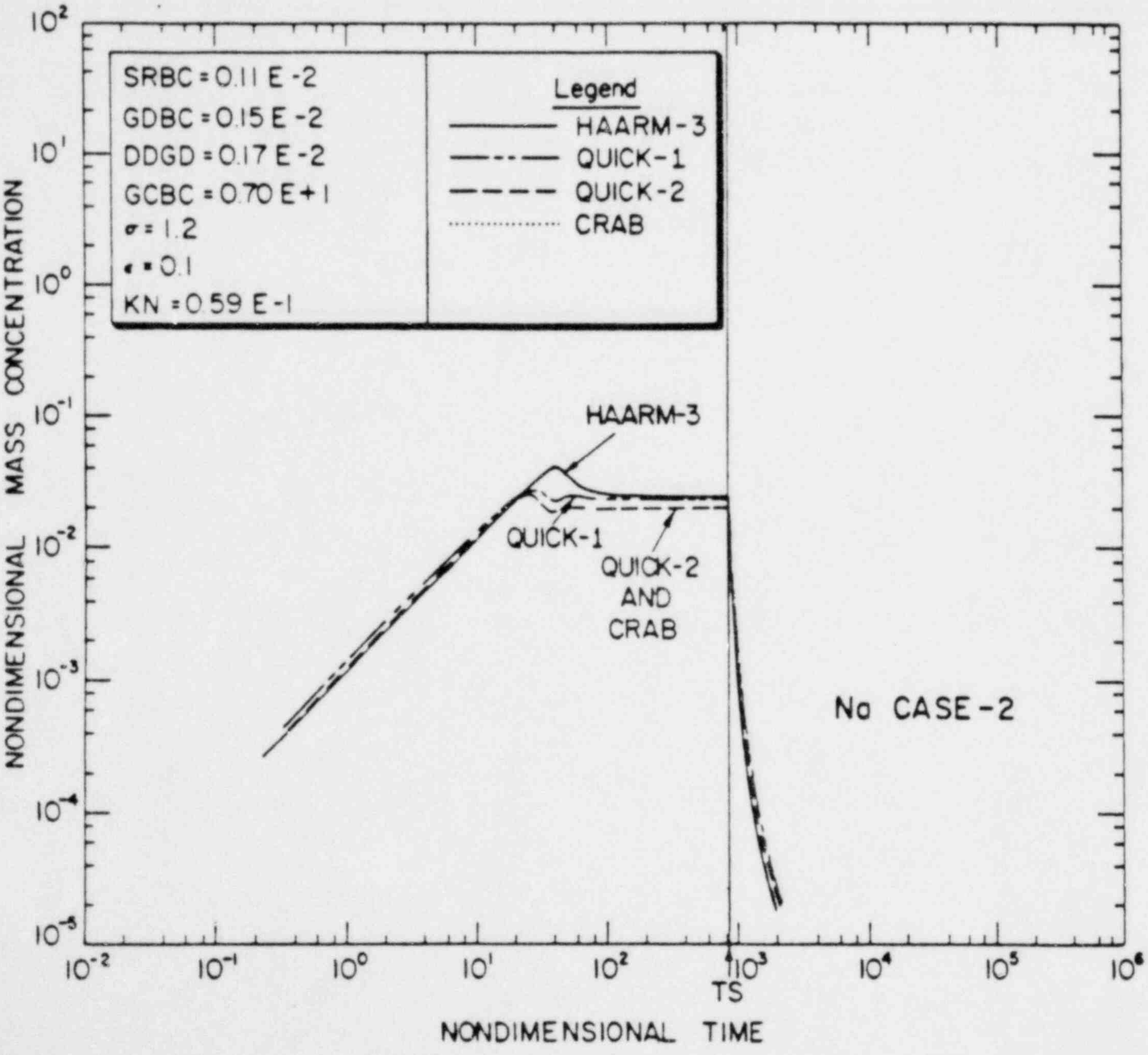
FY 80 ACCOMPLISHMENTS TO DATE

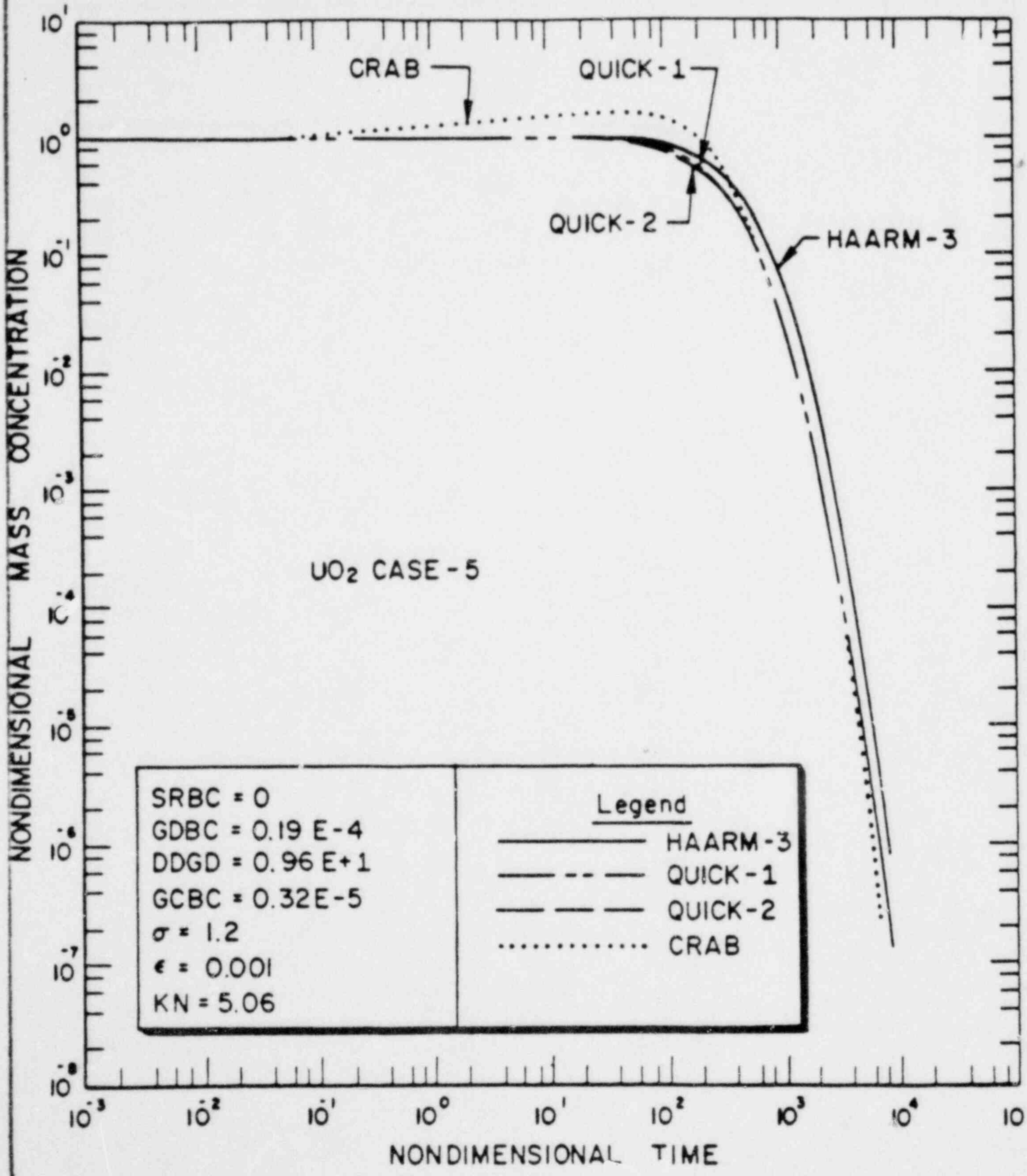
- COMPLETION OF ZONE CODE
- COMPARISONS AMONG HAARM-3, CRAB AND QUICK CODES
- INITIAL PROPERTIES MEASUREMENTS FOR MIXED AEROSOLS
- COMPILATION OF EXPERIMENTAL DATA RELATIVE TO VERIFICATION PLAN

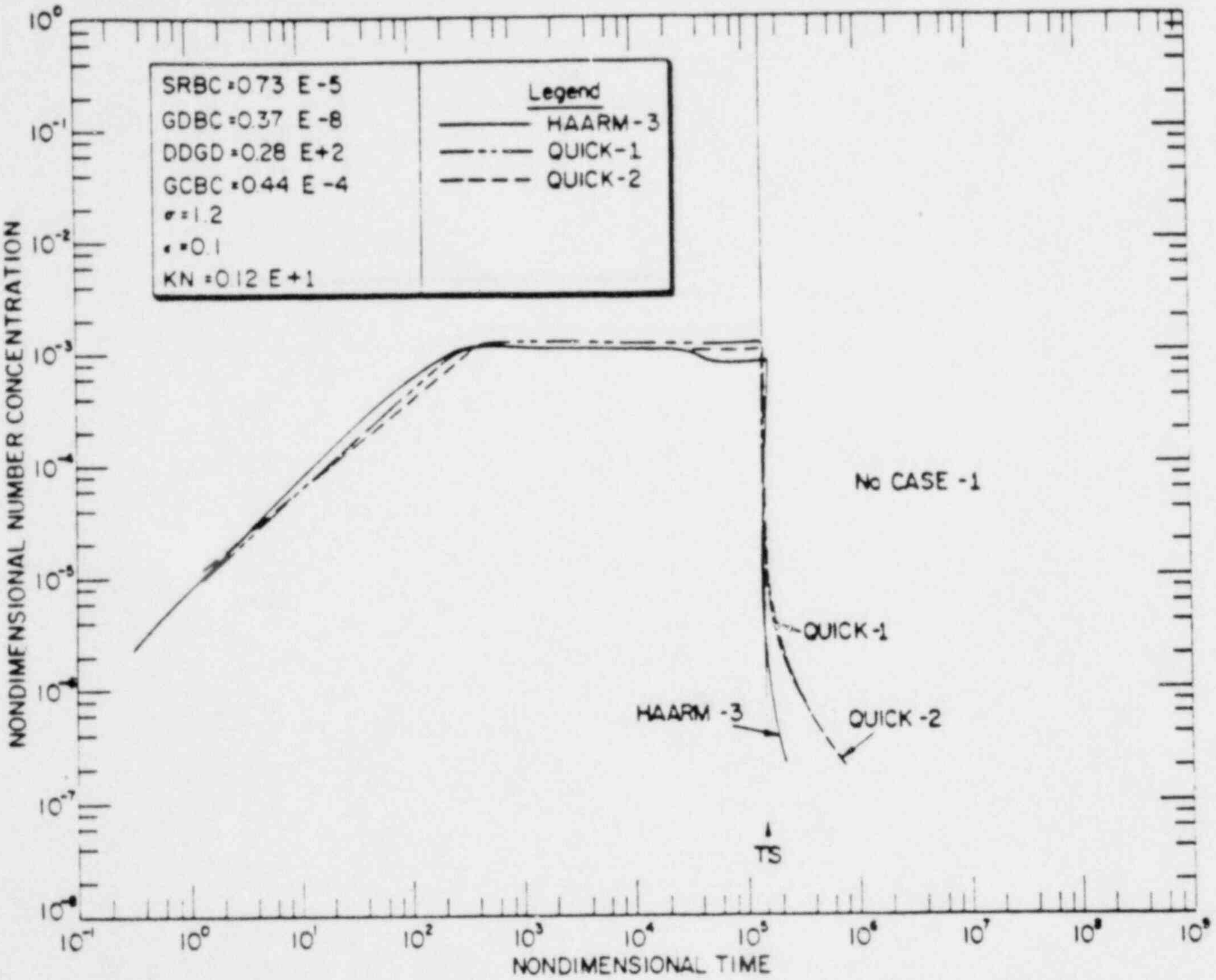

Zone 3
Zone 3
Zone 2
Zone 1

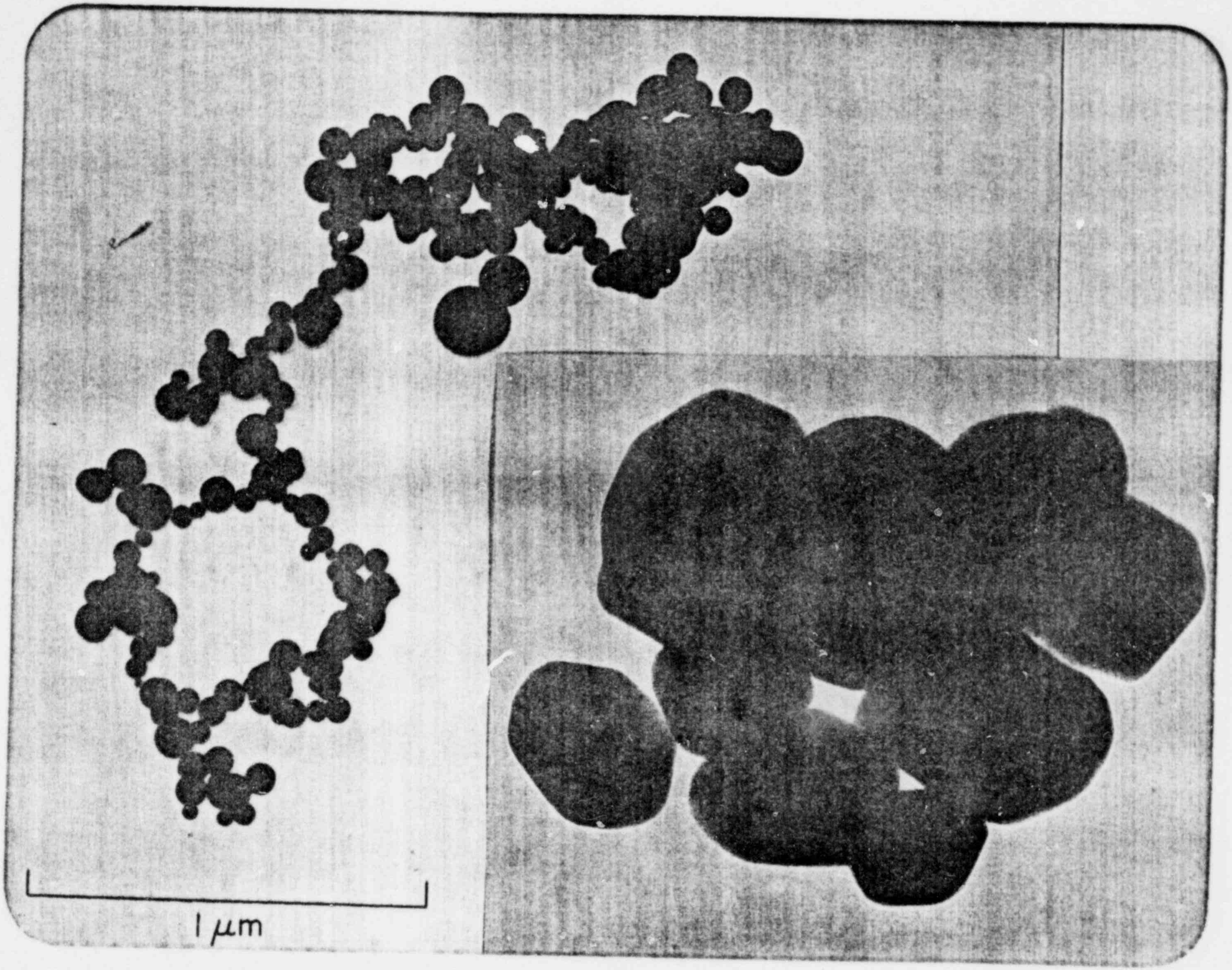

Zone 1
Zone 2
Zone 3
Completely Mixed Model
SRBC = 0.27 E-1
GDBC = 0.34 E-4
DDGD = 0.14 E-1
GCBC = 0.55
σ = 2.01
ϵ = 0.1
KN = 0.18
FRBC 1 = 0.26 E+2
FRBC 2 = 0.80
FRBC 3 = 0.32 E+1
FRBC 4 = 0.40 E+1
FRBC 5 = 0.38
Nondimensional Mass Concentration
10^1
10^0
10^-1
10^-2
10^-3
10^-4
10^-5
Nondimensional Time
10^-3
10^-2
10^-1
10^0
10^1
10^2
10^3
10^4
10^5
10^6
10^7



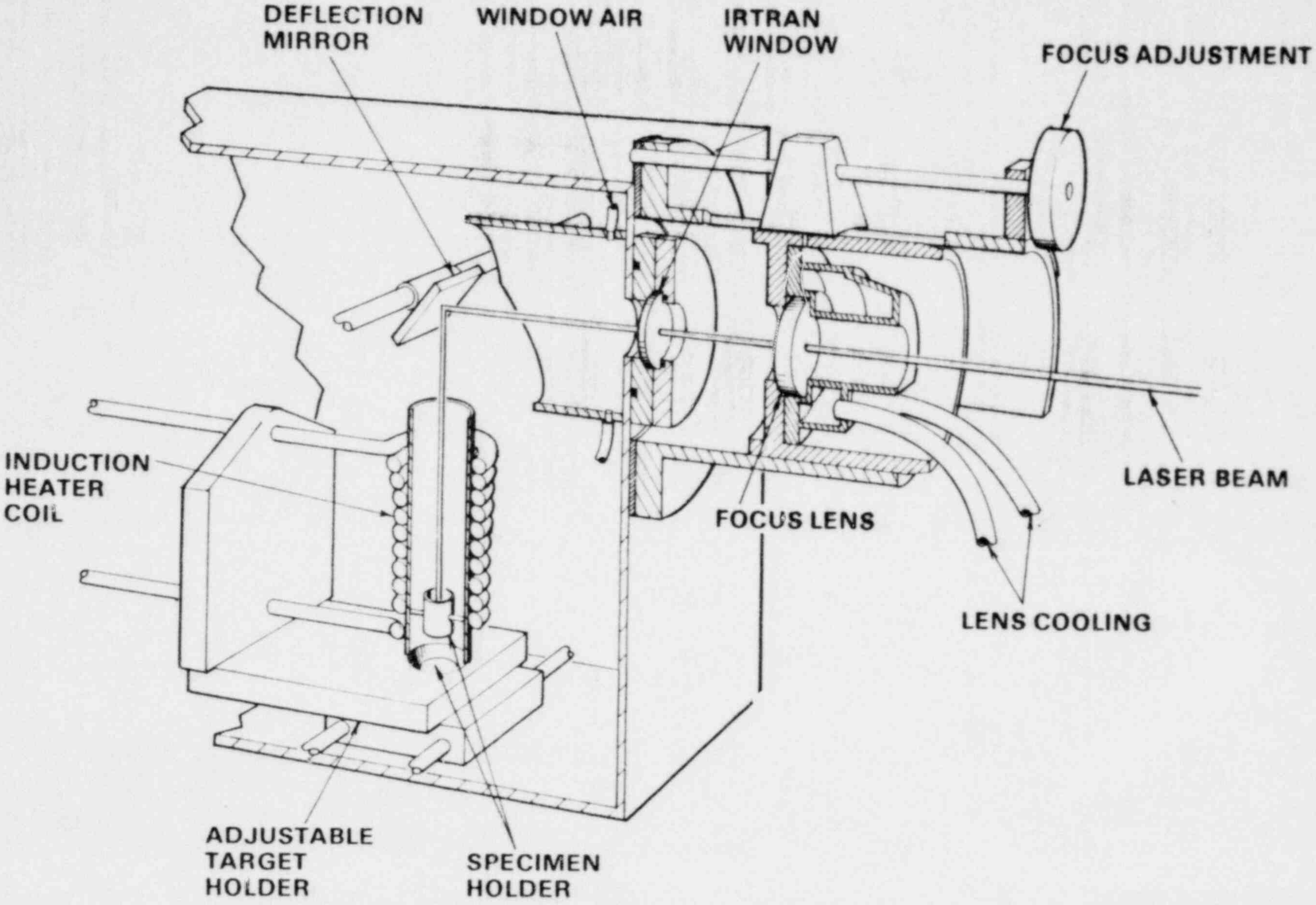
SRBC = 0.11 E -2
GDBC = 0.15 E -2
DDGD = 0.17 E -2
GCBC = 0.70 E + 1
σ = 1.2
ε = 0.1
KN = 0.59 E -1
Legend
HAARM - 3
QUICK - 1
QUICK - 2
CRAB
HAARM-3
QUICK-1
QUICK-2 AND CRAB
Na CASE - 2
NONDIMENSIONAL MASS CONCENTRATION
10^2
10^1
10^0
10^-1
10^-2
10^-3
10^-4
10^-5
10^-2
10^-1
10^0
10^1
10^2
10^3
10^4
10^5
10^6
TS
NONDIMENSIONAL TIME

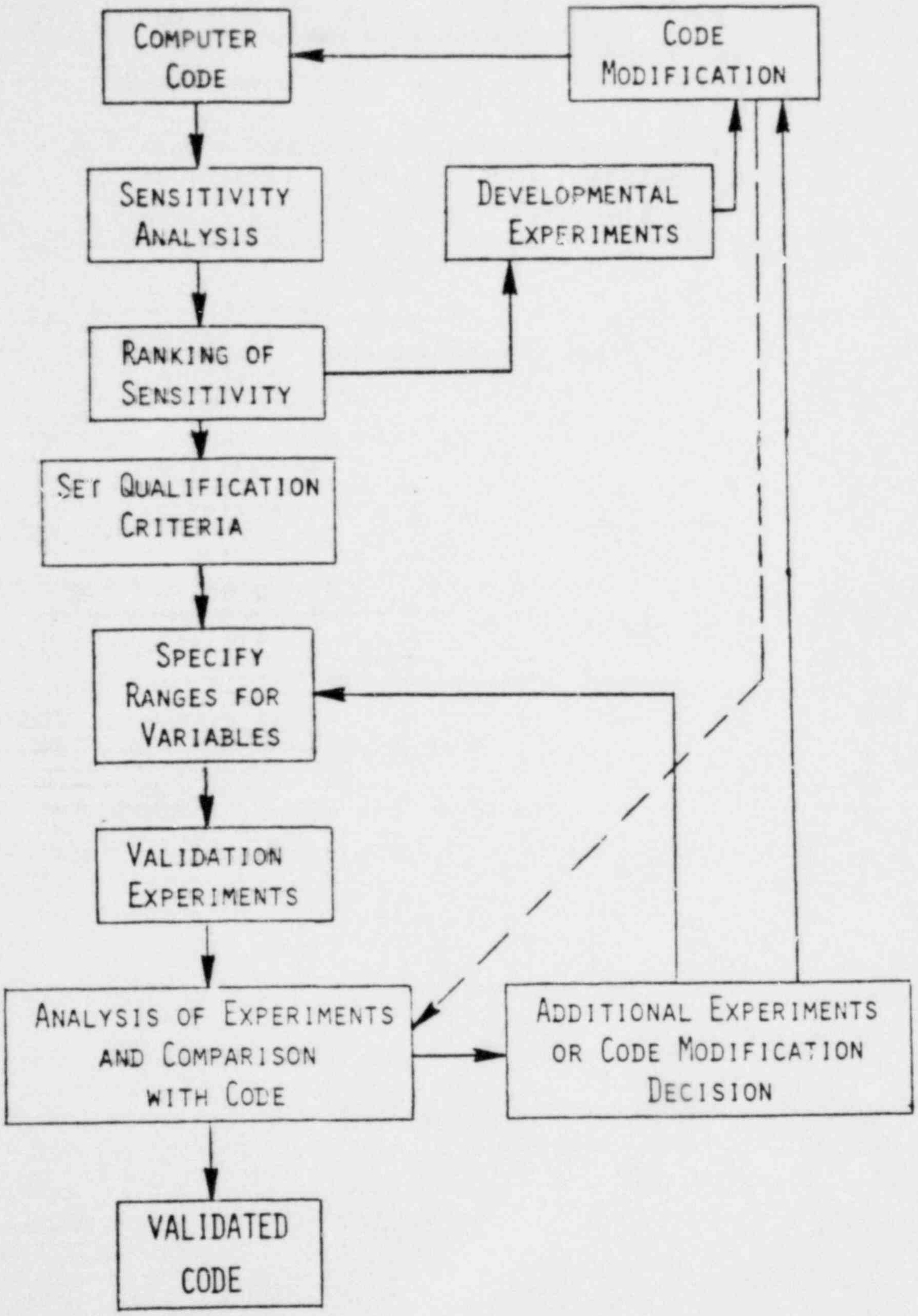

CRAB
QUICK-1
QUICK-2
HAARM-3
UO₂ CASE-5
SRBC = 0
GDBC = 0.19 E-4
DDGD = 0.96 E+1
GCBC = 0.32E-5
σ = 1.2
ϵ = 0.001
KN = 5.06
Legend
HAARM-3
QUICK-1
QUICK-2
CRAB
NONDIMENSIONAL MASS CONCENTRATION
10¹
10⁰
10⁻¹
10⁻²
10⁻³
10⁻⁴
10⁻⁵
10⁻⁶
10⁻⁷
10⁻⁸
10⁻³
10⁻²
10⁻¹
10⁰
10¹
10²
10³
10⁴
10
NONDIMENSIONAL TIME


SRBC = 0.73 E -5
GDBC = 0.37 E -8
DDGD = 0.28 E+2
GCBC = 0.44 E -4
σ = 1.2
ε = 0.1
KN = 0.12 E+1
Legend
HAARM-3
QUICK-1
QUICK-2
No CASE -1
QUICK-1
HAARM -3
QUICK -2
TS
NONDIMENSIONAL NUMBER CONCENTRATION
NONDIMENSIONAL TIME
10⁰
10⁻¹
10⁻²
10⁻³
10⁻⁴
10⁻⁵
10⁻⁶
10⁻⁷
10⁻⁸
10⁻¹
10⁰
10¹
10²
10³
10⁴
10⁵
10⁶
10⁷
10⁸
10⁹

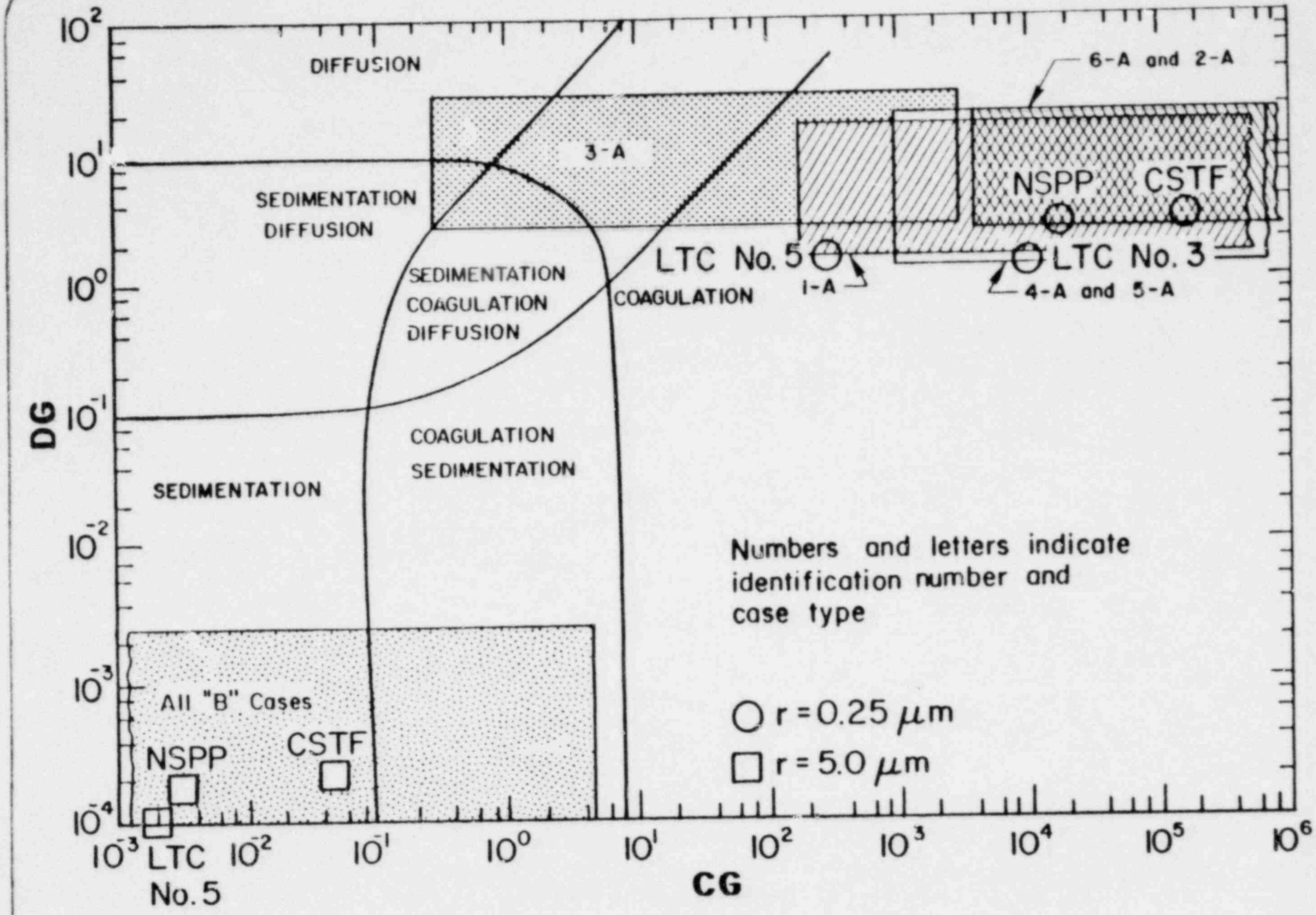

1 μm

CUTAWAY VIEW OF LASER COMBUSTION CHAMBER





CODE VALIDATION PROCESS



CONTROLLING MECHANISMS FOR SELECTED EXPERIMENTS

SPECIAL CONSIDERATIONS

(1) Spatial inhomogeneity

(2) Interaction rates --
mixed aerosols

(3) Aerosol properties --
mixed aerosols

(4) Effect of ambient gas

(5) Localized thermal effects

(6) Particle heating or charging

(7) Resuspension

ACCOMPLISHMENTS EXPECTED FOR REMAINDER OF FY 80

- COMPLETION OF CODE COMPARISONS
- IMPROVEMENT OF CODE TREATMENT FOR MIXED AEROSOLS
- COMPARISON OF EXPERIMENTAL RESULTS WITH VERIFICATION CRITERIA

ANTICIPATED EFFORTS FOR FY 1981

- Measure aerosol properties
    - Effect of mixed materials
    - Effect of ambient gas
- Code verification

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