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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE ON ADVANCED REACTORS

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

Tuesday, 7 August 1979

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

MEETING OF

THE

SUBCOMMITTEE ON ADVANCED REACTORS

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

Tuesday, 7 August 1979

The Subcommittee met, pursuant to notice, at 8:35 a.m.

BEFORE:

PROF. WILLIAM KERR, Chairman of the Subcommittee

PRESENT:

Dr. S. Siegel and Dr. R. Seale.

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PROF. KERR: The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Advanced Reactors.

My name is William Kerr.

We have as consultants to the subcommittee Dr. Seale and Dr. Siegel. We are continuing our review of NRC's research program at this meeting. It is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the government in the Sunshine Act, and all applicable rules and regulations associated therewith.

Mr. Savio is the designated federal empl yee. Rules for participation have ..en announced in the Federal Register of July 23rd, 1979. A transcript is being kept. It's requested that each speaker use the microphone. Copies of the transcript will be available a week from today.

We have received no written comments or requests for oral statements.

We will proceed with the meeting. I call upon Mr. Kelber.

DR. KELBER: I have no formal presentation to make, Bill, and if it's all right, I'd like to speak from here.

PROF. KERR: It's all right with me.

DR. KELBER: I am Charles Kelber of the Nuclear Regulatory Commission Division of Reactor Safety Research. I'm the assistant director for advanced reactor safety research.

This morning and afternoon, we will review with you the programs that we have in place at Argonne National Laboratory and at Brookhaven National Laboratory, with a very brief review at the outset of the aerosol release and transport program at Oak Ridge.

The reason for the brief review of that program is that last December, if I recall correctly, we had a detailed review with the subcommittee. And the purpose of today's review is simply to tell you what we did as a result of that review and where we now stand.

I would like to lead off, then, with Mel ...erberg, who is the manager of that program.

Dr. Silberberg. Good morning. My name is Mel Silberberg. I'm chief of the experimental fast reactor safety research branch under Dr. Kelber.

Dr. Kelber has already mentioned the complete review that we had. It was a full one-day review last December. So I just want to highlight the status and accomplishments since then. And I will not go into any detail at all and indicate what some of the near-term plans are for the program to kind of put that into perspective.

I must say that since December, the progress on the program has been considerable.

(Slide.)

This is just a restatement of the objective and scope

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of the ART program in terms of the two key elements -- source term and aerosol behavior in containment, what we call the transport element for radiological consequence assessment.

(Slide.)

Looking at the source term element, the principal facility here at Oak Ridge is the fuel aerosol simulant test referred to as the FAST facility, which is in approximately one-tenth scale, the near-scale version of the reactor vessel. And right now, the facility has been completed since December. It has undergone shakedown tests with UO2 vaporization underwater and we're now into a very intensive series of calibration tests to qualify the instruments that we'll be using, ultimately, in sodium.

And we'll then proceed to the sodium tests. But we will not proceed to the sodium tests until we fully have a good measure and a good account and an understanding of what's happening in the water test in terms of the instruments' performance, their uncertainties, and in terms of characterization of the fuel source, the test bin which is in FAST in terms of temperature distributions.

Some of these characterizations have been going on in cooperation with the Sandia laboratory. That's been going on in a companion facility to FAST called CRI-III.

(Slide.)

Now just to recall again what the FAST facility is --

PROF. KERR: Excuse me. In what sense are they going on in cooperation with Sandia? Can you give me a little more detail?

DR. SILBERBERG: Since the work at Oak Ridge uses electrical heating and condensor discharge, vaporization of the fuel pin, what we want to do is compare the aerosol, at least the primary aerosols from that with the aerosol that would be generated in pile by neutrons by nuclear heating with the special capsule that was in the ACPR around about two years ago.

And the special sampling techniques are used to inter-compare both the Sandia test, and the same sampling test was done at the CRI-III facility at Oak Ridge, and to compare the aerosols and also get whatever information we could on temperature distribution because it's just a little bit easier to determine the temperature distribution on the in-reactor test than in the case of the CDV fuel pin.

So there's an intercomparison going on to get a good feeling for what the range of uncertainty might be in the temperature of the fuel source.

PROF. KERR: Is there something focused on that? Has there been some description of the results published yet?

DR. SILBERBERG: Let's see.

PROF. KERR: Maybe it's too early.

DR. SILBERBERG: It's been discussed. There is a comprehensive topical report that is approaching publication at Sandia. It will probably be out within a few months, I believe.

DR. SIEGEL: Could you expand a bit on the results? Are the condensor discharge particles adequately similar to the reactor-produced particles?

DR. SILBERBERG: Yes. What one looks at, Dr. Siegel, is the primary particles that result from condensation of vapor, which are the very smallest of the primary particles.

Those are coming out very similar. There may be some differences, you know, in the amount of the source, but that may be related to the uncertainties in temperature.

(Slide.)

The facility now is fully outfitted and installed at Oak Ridge. It's quite a comprehensive facility, quite an undertaking. This facility is designed to be qualified for sodium tests after the water tests.

Now as for the future, near-term future in the case of source term --

(Slide.)

-- we'll complete the fast test with water in the '79/'80 time-frame, complete the temperature characterization of the CDV fuel in the same time-frame, and before going over to sodium, test an acoustic system for bubble diagnostics.

The feeling is we have developed at Oak Ridge in cooperation with the University of Tennessee an acoustic system that will be able to track the bubble, hopefully under sodium.

So what one is doing is using the convenience of water and photography to allow us to be able to calibrate the acoustic system to see if we understand what it's doing.

PROF. KERR: What about the bubble? Are you tracking its location?

DR. SILBERBERG: It's size and weight will rise as a function of time.

In other words, is it, as it might condense as it's on its way through the sodium in terms of size history, temperature history before it reaches the surface of the pool and then be able to model that behavior?

PROF. KERR: What will one do with that sort of information?

DR. SILBERBERG: That information as well as the sampling of aerosols that are taken in the upper part of the chamber, we would have an opportunity to see how much of the aerosol source could we account for that would have been depleted by thermo-hydraulic mechanisms on its way up through the column of sodium, and understand the phenomenon that is giving us such behavior.

This would be not with ordinary fuel, but where the fuel sample will have a xenon pressure in it to give us the effective non-condensable too, because that's an important parameter in determining whether or not the bubble will condense.

Hopefully, sometime in Fiscal '80, we will move over to the sodium tests. And, of course, working closely with the experimental program is the source term, model improvement at Oak Ridge, a bubble model which has been developed for the fast experiments and to allow results from FAST to be extrapolated to larger-scale applications.

(Slide.)

Moving over to the next element of aerosol modeling transport, at Battelle-Columbus, there are two components -- the aerosol modeling in the codes and the aerosol property measurements that are separate effects in nature and allow one to get at some of the important aerosol agglomerate properties that are used within the code.

And here, since December, we have released the user manual on the CRAB code, which is a transport reference code that will allow us initially and principally to make a comparison between HAARM-3, which has a log normal restriction in the particle size distribution against a more tedious calculation in the reference code of the non-log normal form of the particle size distribution to determine whether or not it's suitable to use, the HAARM-3 code as a production code and a much slower running code for a variety of accident sequences.

And so this would allow us to make comparisons, numerical comparisons of the particle size distribution effects all the way up to full-size containment cases, something that we don't have the opportunity to do experimentally.

PROF. KERR: Go through this again and tell me -- what you do is probably perfectly straightforward, but tell me how you couple CRAB to HAARM-3.

DR. SILBERBERG: They run independently. What one does -- HAARM-3 assumes an analytical form to the particle-size distribution, a preferred form, which, experimentally, has been shown for the experiments that have been run to date in the literature.

This is acceptable, appears to be acceptable.

When you use that form, you're able to integrate the various algorithms in the aerosol process equations and it makes the running time short and simplifies it.

What one isn't sure about, there may be some conditions where this may not be a conservative assumption. It's possible, perhaps, under certain scenarios you're favoring the larger size end of the distribution, which would tend to give you preferential flow out.

One wants to check this and the only way that we can check this analytically is by going to the CRAB code, using the formulations there that don't require a particle size restriction, distribution restriction in the particular method that's used.

In other words, it just takes whatever particle size [illegible] tribution it computes in each time step and continues on with that.

What one is doing is that for the same set of conditions in a variety of experiments and accident scenarios in containment, com aring the two codes, running them together to see whether there ar substantial differences in behavior that one might note.

PROF. KERR: And if you get differences or you don't know yet whether you get differences.

DR. SILBERBERG: We're just into it now. And the results now indicate that the differences do not appear to be substantive for those scenarios that have been looked at.

But if we did get differences and they were serious enough, we'll have to determine just what those criteria are. Then it would mean for those scenarios, for those conditions, we would probably then have to go to the more detailed code, the referenc[illegible] code.

P[illegible] KERR: How do you know that the more detailed code is the more accurate?

DR. SILBERBERG: The more detailed code would also be checked against experiments for the largest experiments that have been run to date, where HAARM-3 looks very good.

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In other words, the bootstrapping operation, we take whatever information we have and the numberical techniques are checked against that. If it does a good job with that, we say, fine. Then we just keep working together.

Hopefully, for the most part, for accident analysis in LMFBRs, hopefully we could use HAARM-3 for the workhorse for the program.

DR. SEALE: Mel, could you refresh my memory just a bit? In HAARM-3, or in any of these, you mentioned the change in particle size distribution as one moved with the puff in profusion or in its transport.

Does that particle size distribution change just due to depletion, or is particle-particle interaction within the transport medium part of the problem?

DR. SILBERBERG: During the earlier phase Brownian motion, it's chaining because the number of particles are decreasing rapidly within the cloud by particle-particle interaction.

And so that's changing very quickly.

So there's a tendency to asymptote on the particle size distribution. As time moves on in the experiment, or let's say, in containment, in accident analysis, then the heavier particles drop out, fall out. Then the distribution will perhaps change there also, but not all that much.

(Slide.)

Now under transport —

DR. SIEGEL: Could you comment at all on shape factors? Are you going to do so?

DR.SILBERBERG: All I would say on shape factor is that we have measurements underway, a number of which have been completed for the shape factors. First it was for sodium oxide. Then we're completing the UO2.

Now we have mixtures of the two together, the combined conglomerates, and we're looking at the shape factors there, too. And that will allow us to have that available in the code, again to compare with the code agglomeration tests which are going on at Oak Ridge and which I have here on my next slide.

Two noteworthy areas in transport on the experimental side is that the aerosol code verification procedure has been developed, has been written up by Jim Gieseking at Battelle-Columbus. We are now releasing it for peer review. And sometime, either late September or October, we will have an extensive review group meeting with a variety of aerosol experts around the country to evaluate this procedure in terms of what — how we put together all of the experimental information and make judgments against HAARM-3.

I think it's a somewhat unique procedure and it seems like Battelle-Columbus has done a very good job in focusing, I think, on the important points.

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PROF. KERR: Did you say that the report now exists, or is it in preparation?

DR. SILBERBERG: There's a draft report out.

PROF. KERR: You probably ha : given us a reference. But —

DR. SILBERBERG: It doesn't have a number. But what we will do, Dr. Kerr, is we will send the staff here enough copies for the working group, if you like. That will be going out probably within a week.

PROF. KERR: Thank you.

DR. SILBERBERG: Now at Oak Ridge, in our other major facility on the ART program -- namely, the NSPP vessel --

PROF. KERR: NSPP?

DR. SILBERBERG: The Nuclear Safety Pilot Plant.

PROF. KERR: Thank you. It's capable of total coagglomeration?

DR. SILBERBERG: Yes. The aerosols within NSPP are certainly capable of coagglomeration. At the December meeting, there was some discussion as to the nature of the test matrix to nail this area down.

As a result of the feedback that we got from the working group at that meeting, we have gone through the test matrix to minimize the number of tests to those which are essential, which allow us to do a good technical job, minimize the tests. The matrix has been defined, and the first test at high mass rate concentrations for both fuel and sodium oxide was completed.

In order to do this, it required the generation or the development of a high mass concentration and rate fuel aerosol generator.

PROF. KERR: Now what's the meaning of high mass rate?

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DR. SILBERBERG: What one would like to do is deliver a sufficiently high aerosol concentration, mass concentration measured as grams per cubic meter into the vessel and fast enough so that it all isn't starting on its way to fall out while you're putting it in.

You have to, in other words, take into account the fallout characteristics of that particular vessel which is related to the height.

So what one wants to do is in several minutes get up perhaps the order of a kilogram of material or something of that order.

Now the reason why we like to get to high mass concentrations here of the order for fuel of, let's say, ten grams per cubic meter is that in containment, if I take, for example, an LMFBR accident analysis, the one percent source term that's used, that has been used in the past in the core, that amounts to about two grams per cubic meter.

What one would like to do is get a higher concentration in the test vessel so that you can get into the size regime -- in other words, more concentration, larger sizes -- get into the aerosol size regime in terms of agglomeration.

That would be like the containment building, but still be in the smaller vessel.

The most challenging part of aerosol physics that we're now bringing together is that fact that as the aerosols agglomerat and grow to the sizes that one computes in containment, you depart rapidly, severely, from spherical behavior.

This has been the most challenging part of the aerosol dynamics, where the particle agglomerates and shapes depart from spherical.

DR. SIEGEL: Now, you've just sort of dwelled momentarily on the point which I was concerned about on your previous chart, this issue of particle shape. You kind of went over it in passing before this dynamic shape factor.

From what I remember of the story, the shapes of the agglomerated particles are so odd and unusual that it's very difficult to calculate how they'll behave.

DR. SILBERBERG: Well, using two types of techniques, one the Milliken cell, the other an aerosol centrifuge, one can get at the dynamic shape factor which is related to, in effect, what we used to call effective density. But it gives you that part of, in other words, how it's changed due to, if you will, having grown in terms of density.

For sodium oxide aerosols, this is sufficient because it doesn't depart much from spherical, even though its density changes quite a bit.

In the case of UO2, we do have a different shape and there, there is another factor, another shape factor, form factor that's used, which is very difficult to get at. You're correct.

What one is trying to do is, again, by separate effects from data available in the previous vessel tests and some of the tests that we've been doing at Oak Ridge, is to try to work out what might be a reasonable range of values for those.

That is difficult. That is one aspect of it. That is not as clean as we'd like it to be, but it gets us close.

In other words, it will get us down to at least one value that will have one of the parameters that maybe will have a little more looseness on it than others. But with the interval tests that we're running and so forth, we'll at least get some confidence that it's in the suitable r nge, that we're not, you know, in some way out.

That's really the two areas that I think your point brings out.

We are in conjunction with the CSNI aerosol experts group. We're making the international community and our own program —

PROF. KERR: Excuse me. What's CSNI?

DR. SILBERBERG: The Committee for the Safety of Nuclear Installations, sponsored by the Organization for Economic Cooperation and Development in Paris.

To assess, to make an assessment of aerosol instruments that are used in our experiments, so that when they bring together the verification procedures and the data and the codes, we can say also our instrument errors are such so that we have an understanding of what all of the uncertainties are.

This is something that's actually been wanting a long time, really, in aerosol technology for these aerosols.

PROF. KERR: Is this committee made up of committee-type people or scientific types?

DR. SILBERBERG: Scientific people, yes, at least as technical as myself, and maybe more. Eight countries are represented.

(Slide.)

We talked about the NSPP. This is, just briefly, a schematic of the flow sheet. It's an 18 foot high vessel by 10 foot diameter, and capabilities for sodium experiments and UO2.

(Slide.)

Now our near-term plans for transport are to complete the comparisons of HAARM-3 and CRAB, complete aerosol property measurements adding a third component constituent, namely stainless steel in '80, assess with the codes containment mixing and perhaps multiple species. The accountability of multiple species which are now handled in an average way.

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Containment mixing at this point, I don't think one needs to be very profound about it. But the assumption in the code is that everything is well mixed.

And so one is looking at, well, in the real world, things don't become well mixed that quickly, which is a conservatism in the code.

So one has to look at cases if where, let's say, during the first half hour most of the cloud stays pretty low to the floor.

PROF. KERR: That's the second time you used the term "conservative" when referring to the code. Are you trying to design a conservative code or one that shows you what's going on?

DR. SILBERBERG: We have had to use the conservatism in the code because of lack of information, lack of data.

PROF. KERR: Wait a minute. If you have lack of data, you're sure you know -- well, I interrupted.

DR. SILBERBERG: Let me start back again.

When we started out with the code, a number of areas were to be conservative in terms of looking at the formulations and so forth, and how they compare, how they describe accident scenarios.

Some of the items in the code like properties and things like that, and some of the codes where people were using fudge factors, it wasn't clear that those were

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conservative.

That's why we had to go into and make aerosol property measurements.

Now an example — okay, what we would like to do is where we can get the realistic behavior, the physics experiments and so forth, we will use that. We will use that behavior, where we understand it, and that is verified — we'll use that behavior.

Where we cannot get at something, it may very well stay conservative.

Now containment mixing, for example — Dr. Kelber?

DR. KELBER: Let me comment very briefly.

Mel is going into a lot of the details. Our position is the following. We would like to have a code which gives realistic estimate for assessment, but where we know the direction and size of error introduced by various approximations, so that licensing can, in making their assessments, know when they are erring on the side of overestimating the dose and have some idea of about how much they are erring.

Now it's not always possible to do that with great precision, but I think in this case Mel is coming closer than we have in most areas in the past.

And that is what we mean by a conservative code.

PROF. KERR: Well, you're much more familiar with the

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needs of licensing than I am, of course. But it would seem to me that at this stage of development, particularly where one is trying to use calculations to elucidate experiments, one is using experiments to try to determine whether the codes are describing what is going on.

Then an effort would be made to set up an analytical model that would come as close as you could make it come to describing what is going on physically.

At this point, you would not make an effort to make it either conservative or non-conservative, but you will try to make it accurate.

DR. KELBER: That's the inspiration for the CRAB code, which was my initiative. But the point is that there was a need existing within licensing for an assessment of the source from CRBR.

PROF. KERR: Well, there is within 10 CFR 50 a legal requirement that lightwater reactor codes be conservative. I don't know of any such requirement in the fast reactors and it seems to me that if one could avoid such a requirement, it would be desirable to do so.

A conservatism can be introduced, it would seem to me, in other and better ways.

DR. KELBER: I agree with you, Bill. The whole thrust of the HAARM-3 and CRAB development has been to develop an analytic method and data base that would allow you to make a realistic estimate and introduce deliberately whatever conservatisms you wish to for the licensing process.

PROF. KERR: It sure seems to me, the thing that bothers me about conservative codes is that it's easy to get to a situation where you don't know what's going on.

DR. KELBER: Absolutely.

PROF. KERR: And you say, well, we'll make this conservative.

DR. KELBER: Absolutely, Bill. I could not agree with you more. And that's precisely why Mel has gone through this rather deliberate procedure of looking at these various facets in order that we can know what is due to give it a major approximation.

Now there are some things we will never know too well, but we will at least ha e some notion.

DR. SILBERBERG: If I gave the impression that we're talking about conservative codes, that certainly was not the impression I wanted to leave, and if I did, then I'm sorry. That's an error on my part.

PROF. KERR: Okay. I'm probably making too much out of it. You go ahead.

(Slide.)

DR. SILBERBERG: I just want to note that we continue to develop our program interfaces with various foreign activities with the groups and the countries noted here. Some

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of the collaborations here are under discussion. The state of the art report from the OECD-CSNI activities will be published soon and we see the collaborations with the foreign countries as being very complementary and very helpful to our program, and continue to pursue these as we can.

PROF. KERR: Just for my own curiosity, under FRG, you have something called "source terms." Are there experiments similar to those that you described which are being carried on there which would admit comparisons?

DR. SILBERBERG: Okay. In one area, like in the case of the FAST facility, there is nothing comparable at this point. There's some discussions over an FRG that they may want to get into this with us.

The suggestion is that they would perhaps be a lot better joining forces with us than going alone there in terms of time. They are looking at source terms from, let's say, in terms of boiling pools of sodium in terms of fission release — large sodium spilled down the reactor cavity at the time of core melt.

One thing that they are doing is coming up with, I believe, an improved description of the sodium oxide source term. There's one that's been done in a very, very loose way in the past, very empirical. They're trying to make some sense of that.

We will not repeat that work. We will try to use

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what they do in that area, how much sodium oxide burns in the fire.

PROF. KERR: This is some combination of an experiment and analysis.

DR. SILBERGBERG: At our facility, yes, particularly the large sodium fires facility, the FAUNA facility.

Let me say that the largest sodium fires facility happens to be over in Europe.

That's just the way it is.

PROF. KERR: Okay. Now in France, is there anything that is comparable to the FAST facility, or that is closely complementary?

DR. SILBERBERG: Yes. The closely complementary part of the French work is work that they're doing on the HCDA bubble with water only, a series of tests -- the Excobulle tests at Grenoble, which Professor Reynolds was involved with when he was over there on a one-year sabbatical about two years ago.

We continue to have a professional relationship with those people. There is a facility in Cadarache called Caravel, which is something like the FAST facility, but it's only with water, and they're only using a thermite source.

And I think that these may be interesting scoping tests, but the thermite sources give us a problem.

PROF. KERR: This is thermite in contrast with mh

condensor discharge.

DR. SILBERBERG: Yes. It's difficult enough to characterize the fuel sources. I think using thermite, it's even more difficult in this type of test.

PROF. KERR: Now is there close enough communication among these groups that they are reasonably aware of what is going on in France and what's going on at FRG and Oak Ridge, in your view, so that you're getting any input from them that would be helpful?

DR.SILBERBERG: Let me say something, and then Dr. Kelber would like to say something. It's my own personal feeling.

We are trying to develop the relationship that would allow us to do more than just read reports because I don't think that just by reading reports, you can necessarily get that much out of the program.

Ideally, if they were to -- if, in other words -- if we were able to afford, had a large enough program to be able to send personnel there, that would be the best situation, and vice versa.

In the meantime, we try to arrange for periodic visits, for example, at meetings when they're here. We try to set up a collaboration where perhaps maybe someone would spend one or two months there.

Dr. Kelber?

DR. KELBER: We have a problem. The safety collaboration with the CEA is in considerable jeopardy because of a failure of agreement between the Department of Energy and the CEA in France to agree on the details of a safety exchange.

I don't want to go into the complex diplomatic history here, but the resolution of it all is that at the present time, the CEA is bringing pressure not to cooperate extensively with us, except within the framework of the CABRI-NRC agreement.

On a one-to-one basis, we have good relationships. Officially, we have no relationships.

Therefore, we have difficulty in sending people for any prolonged length of time.

Now I am told that the DOE and CEA negotiations will start up again at the end of this months. What the outcome of those will be, I do not know. DOE has been informed at a low level of the effect of their position on ours. They have not had any effective level, taken any steps to indicate that they give any weight whatsoever to our concerns.

And I really do not know what the outcome will be. We have been informed, very unofficially by the FRG that pressure has been put on them to slow down their collaboration with us, which has been very productive because of the nature of their working relationship between the FRG and CEA.

This is an instance where there is simply a lack of internal collaboration on the U.S. side. And if there is a feeling, which I believe there is, that there's considerable technical advantage to be gained from mutual collaboration in the safety field internationally, then I believe that a recommendation from the ACRS that the commission take a more active role in insuring that this collaboration is enhanced, would be very fruitful.

PROF. KERR: What I was trying to determine was whether, in your view, activities and people in the FRG and in France or such, that meaningful collaboration did or could exist.

DR. KELBER: Could, yes. Does exist, no, except within a very restricted field, such as the CABRI/ACRR, where we have a written agreement. It is being honored.

DR. SIEGEL: Does DOE have some sort of legal assigned role in this, or can NRC pursue this independently?

DR. KELBER: We pursue this matter independently; however, DOE has the advantage of a $500 million budget base to work from. And this is a powerful attraction. Their safety program alone is considerably larger than ours. We have pointed out, in fact, that so far as the safety program is concerned, if the CABRI partners were each to contribute $800,000 over a period of two years, we could duplicate at the ACRR most of the test capacity now in existence at TREAT.

What they are looking for, however, is what is coming mainly in the upgrade capacity.

PROF. KERR: I think you answered another question. I interpreted his remarks to mean that they don't have any problems that money couldn't solve. You said something about UK on that slide. I got the impression that they were doing some code development, but no experiments were mentioned.

DR. SILBERBERG: Very little, yes. UK is just over the last two years, just been getting into the aerosol area. They have developed a code of their own, using naturally some of our technology in the code. They have some observations, some ideas on experiments. They're trying to get to some of the separate effects measurements but they're kind of starting from square one, as I understand it.

PROF. KERR: Thank you.

(Slide.)

DR. SILBERBERG: I just want to close by just noting two comments from the Committee on the program, and on the first, the 1978 report, we agree with the comments. We would like to proceed in that direction, but I might say that in view of our current budgetary uncertaintie and constraints, long-range planning is in fact very difficult for us.

And so, we're prepared to move into this area, and as our budgetary situation clarifies, perhaps then we'll be able to in a future meeting discuss what our plans are in this area if we are indeed able to proceed fruitfully in that area on our program.

But I believe that we have the technology that allows us to proceed into that area very well, and it's just a question of program support.

PROF. KERR: Are there questions?

(No response.)

PROF. KERR: Now, you mentioned the collaborative agreement, and Charlie did, between you and the FRG and France. Do you have any collaborative arrangements with DOE?

DR. SILBERBERG: Yes, actually. Very good arrangements. They're an active part of our aerosol review group and exchange of data between facilities, visits and so forth, the information that they're getting in the CSTF, which I just didn't have a chance to go into detail -- the data from that facility plays a very important role in our verification procedures.

PROF. KERR: I should know what CSTF is.

DR. SILBERBERG: I'm sorry. The Containment Systems Test Facility at HEDL, which is a very large facility which is primarily oriented towards air cleaning activities.

PROF. KERR: Is there some relationship between that and the work you're doing at Oak Ridge? Could you just say a little bit about what the relationship is?

DR. SILBERBERG: All right, the relationship is that that's larger scale sodium oxide and the tests at Oak Ridge are primarily fuel aerosol and mixed fuel sodium aerosol oriented. In other words, what the CSTF does, it gives us another chance to test things at a larger level.

PROF. KERR: Now in planning the experiments involving these two facilities, is there a deliberate effort to make the work complementary? Do you plan together in some meaningful way? I'll stop there.

DR. SILBERBERG: We exchange test plans, we discuss tests at meetings, informal discussions. I think the remaining tests that are planned for CSTF and those for NSPP some time this fall as part of this verification procedure, we will get down to the table and actually, perhaps, get involved particularly, I think, more our getting involved in their test matrix or plans at the larger facility and have substantive discussions on things that we would like to see in their test matrix in the context of our verification procedure.

So, there's been good communication and complementary, and I expect that as we move further down the road this becomes stronger.

PROF. KERR: Is there any meaningful way of judging the size of the DOE effort in aerosol transport? As compared to yours, is it much bigger, about the same, or can one make a comparison in a meaningful way?

DR. SILBERBERG: It's almost the same, about comparable in terms of the dollar value, but then again, most of their effort is in the large facility, operating that in the tests there. The DOE is not doing any more in the aerosol area because they put that in their LOA-4 category, and as you look at that, it enjoys perhaps a lower priority than 3 or 2 or 1.

And so, they're satisfied to move on with the technology as it is and look at things like the air cleaning which is certainly very important and very meaningful, and other ways of depleting -- how aerosols might deplete in accident scenarios.

What we would like to do is get a complete handle on the physics, verify the physics.

DR. SEALE: You've indicated that your future plans with aerosols are somewhat hostage to budget uncertainties, and you've also indicated that DOE has at least some of these activities on a relatively like, lower priority in their program.

We've also heard that there's some possibly useful information in the foreign programs, if we could really get effective access to it. Could you tell us what the French program, for example, looks like in terms of level of funding and how it might fit in to filling in the blanks, as it were.

DR. SILBERBERG: Okay. You mean blanks in general in technology or, in other words, in terms of filling in our own blanks?

DR. SEALE: Well, both.

DR. SILBERBERG: First of all let me say that there isn't one aerosol program in the world that puts together in a planned way the experiments and the analysis and actually gets going down the road towards completing the job, what I call verified technology. They talk about it but it's not visible. Ours is visible; we put our cards on the table and we say, you know, we're putting — in terms of verifying — what we're developing.

Now, too, in the case of France their aerosol program is really minimal.

PROF. KERR: Excuse me, Mel. You mean that you think there's activity going on that isn't available to you, or there just isn't organized activity?

DR. SILBERBERG: I don't believe it's as precisely organized as ours, as well organized.

PROF. KERR: I wasn't sure what you meant.

DR. SILBERBERG: I certainly didn't mean that. The big thing about the French program -- well, let me say this, the French are using our aerosol code HAARM-3 for licensing along with one of the German codes. They're happy to use it and that's very well known. They are doing practically nothing on separate effects tests. Their aerosol tests that they did at Cassandra, at Cadarache was strictly done, again, mostly for sodium fires and for air cleaning. The Esmeralda facility, which is under construction now at Cadarache, which is a full-size mock-up of the secondary system of Superphenix. The sodium fire oriented, primarily, they will aerosol measurements, they will try to get involved in those, they will try to predict the aerosol tests.

But there is no concerted effort in terms of aerosol transport in the similar way that we have. As far as the work at Caravelle and Excobulle and so forth, there I feel that clearly more collaboration would be beneficial in that work and as Dr. Kelber pointed out, we'd like to be able to get into that phase once some of these other administrative things get out of the way.

So we see in their work maybe more interest on the source term side and their large facility that gives you another shot at looking at aerosols in large volume.

DR. SEALE: So you'd say that Esmeralda is maybe 10 percent aerosols and 90 percent sodium fires?

DR. SILBERBERG: I think so, yes.

PROF. KERR: Other questions?

(No response.)

PROF. KERR: Thank you, ma'am. I am going to suggest about a 10-minute break between now and the next presentation. We will recess.

(Recess.)

PROF. KERR: May we reconvene, please?

Next on the agenda is a description of AMF fuel failure studies.

DR. KELBER: I'd like to introduce Phil Pizzica from ANL, who together with Terry Hummel has been instrumental in the collaborative studies both with the European community and with the United Kingdom in this area.

PROF. KERR: Can you spell his name? I bet you can't.

DR. KELBER: We can, but he can do it even better.

MR. PIZZICA: We have been involved in cooperative studies of detailed comparative whole core accident analysis studies with two main bodies in Europe, one, the United Kingdom Atomic Energy Authority, mainly with the people at Risley, the other being the Full Core Accident Codes Safety Working Group under the Fast Reactor Coordinating Committee under Euratom, which meets in Brussels, whose participants are the U.K., France, Germany, Belgium, U.S. DOE just recently, and also within the last year, the Joint Research Center, have been doing calculations --

(Slide.)

PROF. KERR: Excuse me, when you say "we," who is we?

MR. PIZZICA: The ARSR, I forget exactly the context --

PROF. KERR: You said "we had cooperated."

MR. PIZZICA: I'm sorry, ARSR, for which I and Dr. Hummel are the representatives.

PROF. KERR: Thank you.

MR. PIZZICA: First of all, I'd like to hit some highlights as to what I think we're getting out of the programs on a technical level. We're pursuing these programs and we exchange ideas and modeling perforce, merely by participating in detailed comparison calculations in these whole core accident studies.

Also, there are other areas of modeling that the Europeans are perhaps somewhat less sophisticated than we are in terms of fast reactor safety modeling, but there are many areas in which they are increasing their sophistication rapidly. In some areas they are not necessarily behind us at all. One of these is pin failure. We have had a very fruitful exchange in that area, and even to the extent that the Europeans may be somewhat behind us in the modeling area, it's still very fruitful to have different points of views on modeling.

I might point out that they have extensive safety experimental programs and they are, of course, building LMFBRs.

PROF. KERR: When you say modeling, I could come to the conclusion that you have all the data you need. You just need to model it.

MR. PIZZICA: No.

PROF. KERR: I shouldn't draw that conclusion?

MR. PIZZICA: Of course not, no, not in our business. I don't mean to give that impression by any means. We work with — of course, we build models according to any existing data base for particular details of the calculation. Obviously, we can't have an integral data base for reactor calculations, but we must be satisfied at the present time and for some time to come, I'm sure, with parametric variations and testing sensitivity for the purpose of whole core accidents studies.

That's simply a fact of life.

DR. KELBER: Phil, may I break in? One of the topics I would like to discuss with you later is the recommendation regarding the definition of in-pile testing of fumigants. The basis for the types of recommendations we will make comes out of the sensitivities and insights developed in programs such as this.

MR. PIZZICA: I would certainly agree with that.

PROF. KERR: I'm not quite sure I understood what it meant, but I presumably will understand it after Charlie discusses it with us.

MR. PIZZICA: We can broaden our perspectives with respect to safety problems, which depend on certain aspects of reactor design, since they have different designs than we do. I've given some examples there. Also, as in any whole core accident calculations can test the sensitivity of final results through parametric changes, we will gain understanding and an idea as to where modeling improvements are needed.

PROF. KERR: I think both broadening and perspective are good words, but can you tell me something that you're doing?

MR. PIZZICA: I did give some examples. For instance, very practically, specifically, they have bottom fission gas lines which increases, of course, the inertial resistance in expelling slug in the voiding process. They'll have a smaller upper slug, decreases its inertial resistance, so you can blow out the upper slug a lot faster and you can void the lower one, compared to a U.S. designed reactor, and that creates very different reactor conditions.

PROF. KERR: Have you concluded that that's good, bad, or indifferent? Or have you reached a conclusion yet?

MR. PIZZICA: I would like to do a lot more calculations before I made any broad statement such as that, such as a conclusion in that area. All I will say is that it changes the scenario and it is an example of how we have been locked into a certain perspective based on a certain type of reactor design, and here's an example of, you know, for example the practical effect of that, you blow out the upper slug, you have a lot of code clamp exposed to sodium vapor much sooner than you do in the U.S. design where it takes much longer to expose that upper slug. That can depressurize at least the upper bubble for a time and that kind of thing, we've seen that sort of thing, but specifically that is what I had in mind.

Low smear density fuel pins is another thing.

PROF. KERR: What is a low smear density fuel pin?

MR. PIZZICA: By smear density, I mean if you took the total volume inside the clamp and the mass of fuel in that total volume, forget about the gap, forget about cracks in the fuel and all the rest, smear the fuel and that's the smear density.

We are typically talking about in a CRBR type of pin, about 85 percent or so, perhaps somewhat more. I'm not exactly familiar with the numbers, but the U.K. number that we have been working with in the U.K. bilateral studies is something like 80 percent, which has considerable significance.

PROF. KERR: The difference between 80 and 85 percent is quite significant.

MR. PIZZICA: In terms of fission gas

pressurization, yes, sir.

PROF. KERR: Okay, thank you.

MR. PIZZICA: Just a last point to emphasize that unless you do these detailed calculations and compare it with someone else's independent detailed calculations — now, I don't meant that their modeling is all independent of ours or they were inspired by the U.S., I don't mean that kind of thing. I mean that if you take a particular reactor calculation and that's an independent calculation, you can often spot difficulties in your own code that you would not spot just looking at your one calculation without anything to compare it to.

I want to refresh your memory, first of all. The group that meets in Brussels by the way, I will abbreviate and call, since it's the whole core accident codes working group, I'll call it WAC from now on, since that's a mouthful.

(Slide.)

I'll refresh your memory. We had a first round of calculations which included an LOF, Loss of Flow calculation. There wasn't anything too interesting about that. All participants got similar results; there was mild disassembly predicted, no loss of flow driven transient overpower was allowed, essentially, if boiling had started in the channel. As it turned out that was the case, in every possible LOF-TOP channel.

By loss of flow driven transient overpower, I mean of course — I probably don't have to say this — but I mean that voiding in higher powered channels will bring the reactor into the vicinity of prompt vertical on to high power levels such that pins in mid and lower powers, sub-assemblies will experience essentially TOP conditions and there may be some or all of the sodium in the channels in those low or mid-power sub-assemblies still.

There was also a one dollar a second TOP calculation and I'll discuss that a little bit later.

(Slide.)

Right now, I want to begin to discuss the loss of flow calculations that we did as part of the U.S./U.K. bilateral studies. First of all, we did a loss of flow calculation, which is a flow rundown, a pump rundown. We first of all excluded the possibility of loss of flow driven transient overpower and let all channels, if you're familiar with the SAS-3D code, be treated with the SLUMPY module after pin failure as opposed to some possibly being treated with the EPIC model which assumes kind of a burst-failure condition as opposed to SLUMPY, which assumes an amalgamation or homogenization of cladded fuel as soon as a given input failure criterion is reached.

And the hydrodynamics are, of course, quite different, because in the EPIC case you preserve pin geometry and in the slumpy case, pin geometry is lost and you assume that the whole channel is available for the movement of materials. Before we allowed any LOF-TOP failures we ran the LOF with and without actual expansion feedback. By with actual expansion feedback, I mean 80 percent of the reactivity feedback, which would have corresponded to free thermal expansion, and that feedback, when it was included, was worth about negative one dollar.

No clad motion occurred before pin failure, if you excluded the loss of flow TOP, you got mild voiding ramp rates in the vicinity of 10 to 15 dollars a second. There was a quite small potential for LOF-TOP conditions in the case that included axial expansion feedback, because if you include negative a dollar from the feedback, you have to increase the voiding of the core to get an extra dollar out of the sodium voiding.

MR. KASLENBERG: Could you briefly review which reactor you're analyzing, you know, what the fission gas plenum is?

MR. PIZZICA: The fission gas plenum is in the bottom in both reactors I will be talking about, both in the U.K. reactors and in the WAC reactor.

The U.K. reactor is 3100 megawatts thermal, approximately 1300 megawatts electric. I have, let me just flash this slide very briefly --

(Slide.)

That's the specs. There's about 3.3 dollars sodium void worth, if you include the upper blanket and negative plus the core positive to the zone core homogeneous and whatever else.

Okay. There's a significantly greater potential for loss of flow driven TOP if you exclude actual expansion feedback, because you need to have voided less of the core, remembering the negative one dollar here. At the point when a power rise is predicted in prompt critical conditions, something like 40 percent of the core is voided here. Without axial expansion feedback, as opposed to 60 percent with, the power conditions are somewhat different.

And these are, again, without loss of flow turbine transient.

PROF. KERR: Did I understand you correctly? Were you making these calculations using SAS-3D?

MR. PIZZICA: I'm sorry. I should have spelled that out. Yes.

(Slide.)

And we're using a version of SAS-3D which has the SAS FCI module, if you're familiar with that, for treating FCI replaced by the EPIC module which treats, as I said, burst failure conditions assuming pin geometry preserved. In other words, kind of a TOP type of pin failure as opposed to a SLUMPY type of pin failure. But with that module replaced, we also have included other options for pin failure and that kind of thing, but that's the major change, essentially it's SAS-3D without.

PROF. KERR: The U.K. people were using a code that they had developed?

MR. PIZZICA: Right, it's called FRAX-2. It doesn't compare in sophistication with SAS-3D. As long as you raised the subject, I'll point out one of the major deficiencies in modeling of that code, that is, it does not treat fuel motion explicitly; therefore, you will get radically different results for TOP conditions from what we would predict.

Obviously, for instance, if a slow TOP gave a top-of-core failure, we would predict some sweep-out and the reactor would shut down, as I will tell you later. And they would predict a prompt burst because they didn't include the sweep-out and they did include voiding reactivity.

DR. SIEGEL: You say they did not include sweep-out?

MR. PIZZICA: They did not, and that's the reason they got top burst. They would not, at least in our best estimate — later we'll develop that.

PROF. KERR: If somebody used a version of SIMMER-2, let's say, would he get anything comparable to what you're getting, or would you want to conjecture?

MR. PIZZICA: I really don't think that's a question for me, not having run SIMMER.

PROF. KERR: It's a question for you. You may not have the answer.

(Laughter.)

MR. PIZZICA: Absolutely right. Perhaps I could leave that to Dr. Kelber.

PROF. KERR: If your answer is, you don't know, that's perfectly satisfactory.

MR. PIZZICA: I'd rather say I don't know, since I haven't run SIMMER.

Okay.

DR. KELBER: The latest quarterly from Los Alamos that will be out in about two months will have a description of a comparison between SAS-3D or 3A, one of the SAS-3 versions, 3D, and SIMMER for a sub-assembly type accident. And the comparisons are pretty straightforward and very similar.

MR. PIZZICA: Very good, I was not aware of the study.

Again, I've already treated this topic, really. I talked about the difference between a SLUMPY type of treatment of fuel and possibly clad hydrodynamics after pin failure and an EPIC type of failure. When we included "LOF-TOP" quote-unquote failures in the mid and lower power sub-assemblies, they were treated with the EPIC module in a kind of burst failure mode where pin geometry was preserved.

Currently the only choice is either SLUMPY or EPIC in the SAS-3D/EPIC code. SLUMPY has the major limitation that it cannot be triggered in the presence of liquid sodium, such that if you wanted to use a code which melted through and there was liquid sodium at a particular node, where you wanted to do this it would be impossible with SAS-3D.

The assumption behind SAS-3D is that would be unlikely, which it may be -- but it's not impossible.

PROF. KERR: Now, I could conclude from somthing like this that you'd have to know a good bit about what is happening to the core in order to use this code, rather than vice versa, which would be that the code would tell you what's happening in the core.

MR. PIZZICA: Right. I would agree. If what you mean is that you have to tell -- just to be very specific here in what I'm saying -- in this reactor, you have to tell it to a certain degree -- to a certain degree it will be obvious which is a SLUMPY channel and which is a TOP channel or an EPIC channel. To a certain degree it will be ambiguous and open to question.

If that's what you meant, I agree. You do have to tell the code what kind of failure occurs in those borderline channels, admittedly. And that is, in fact, the purpose of that slide. For some channels, however — in fact I would say the majority in most of the cases I have looked at, in all the cases I have looked at — the majority, it will be obvious which to use.

That may not be the case for all reactors, for heterogeneous core maybe; I don't know for our homogeneous calculations that is that case. You have a fair amount, for instance in this reactor, of quasi-coherent voiding. In about four higher powered channels the clad is very hot, it's very obvious there's no sodium there. It's very obvious to me that you'd want to use a SLUMPY type module.

It's very obvious in some other channels that you want to use EPIC. There is one that is right in the ballpark of being a borderline channel; in fact, I ran some cases with it as EPIC, some as SLUMPY, and you're right, it's open to question.

PROF. KERR: Now, what was it that you were expecting to learn from this comparative study, or were you just making the comparison to see what you would learn?

MR. PIZZICA: Partially the latter, I suppose. In our business that's always the case, but we can find out what would be a best estimate for neutronic energy deposition, possibly the conditions which would -- the neutronic energy deposition at the time of neutronic shutdown, for a commercial-sized reactor. We don't have too many commercial reactor designs floating around in the U.S. at the moment. That's one advantage of dealing with European countries, by the way.

But we would want to make an estimate of what the conditions would be during and after a disassembly condition that is a prompt burst when the power is rising very rapidly, that kind of thing. And we would want to make an estimate of what the conditions would be at neutronic shutdown for the purpose of either extending our modeling or going into another code.

PROF. KERR: Would you say that what you learned most about how to design a safe reactor from the study, or would you say that you've learned most about how to design a better code?

MR. PIZZICA: That was another point I was going to bring up, is that we can put the code through the motions and test out its capabilities to see where modeling deficiencies occur. If this is done with a commercial-sized reactor, that's a different ball game from CRBR or FTR and that's an advantage.

PROF. KERR: Did you indeed learn some significant things about SAS-3D or 2D?

MR. PIZZICA: Definitely, and I'll list them later in the talk.

Okay. I think we've discussed everything on that slide. I want to emphasize that the clad strength is the main determining factor in choosing between a SLUMPY type of channel and an EPIC type of channel.

(Slide.)

Melt fraction failure criterion was assumed but for the LOF-TOP pins as well as the slumping pins, the higher powered channels. But one could make an argument — I don't mean that this is necessarily the case — but one could make a supportive argument on the basis of burst failure for a 60 percent core height failure, making that at least a possibility.

I don't mean to say it's a certainty by any means, but a possibility, because the LOF-TOP pins are driven to very high fuel melt fractions very quickly and fission gas does not have time to migrate and move from its initial position. It is not released to any great degree from the solid fuel before melt. Almost all of it is brought into the molten fuel cavity with the melting fuel. If you can make an assumption that once the fission gas is brought in with the molten fuel, it's available for pressurization — if you can make that assumption, and if you calculate the hoop stress on the clad in a node-by-node fashion as opposed to averaging over the whole molten fuel cavity, for instance, then you can predict with the fission gas burst failure criterion, failure at the 60 percent of core height, which is the same location.

PROF. KERR: Let me see if I understand what you're telling me. You're saying — are you? — that if the fission gas is at the location of release that there will be corresponding stratifications of pressure, so that if I have a lot of fission gas at this level, I'll also have a lot of pressure, so that I'm in a very non-equilibrium situation, where I have a pressure at this point of the pin markedly different from the pressure at this point.

MR. PIZZICA: That's right.

PROF. KERR: Do you believe that physically?

MR. PIZZICA: There will be, possibly, some redistribution before pin failure.

DR. SIEGEL: What time scale are you talking about?

MR. PIZZICA: Very fast, and that is the point to remember.

DR. SIEGEL: Faster than the shock wave took off.

MR. PIZZICA: No, not that fast; we're talking about tens of milliseconds to reach very high melt fractions from no-melt fraction, 15 to 30 milliseconds depending on the case in there.

MR. KASLENBERG: Phil, on some of these assumptions, did you make them out of convenience or did you try and make them as the most physical, because then they clear up some of the questions, or just because the code can handle that?

MR. PIZZICA: One assumption is made for convenience because we don't have the modeling to model release of dissolved fission gas in molten fuel and we must make this assumption presently, okay.

That's a point, and that's why I want to spell out that assumption. I think that's the key assumption, is it not? Does that help?

DR. SIEGEL: In what time scale will, say, the pressure pulse travel up the annulus?

MR. PIZZICA: Probably somewhat, it could be somewhat faster than the total time scale required to go from zero melt fraction to, say, 70 or 80 percent melt fraction or failure conditions, but I don't think it would be as fast. And this is a guess. I haven't done any calculations on this.

I don't think it would be as fast as when high melt fractions are first attained, when pin failure first becomes a possibility, say in the 40, 50 percent fuel melt fraction range. Going from there to about 70 percent melt fraction only takes you several milliseconds, on the order of five milliseconds. I don't think there's enough time there.

Could I make one last point? Most of the fission gas comes in with unrestructured fuel. There's very little steady state retained fission gas in any but the unrestructured fuel and that is in the outer portion of the fuel and you won't even start to get into that until you get to the very high melt fractions, at which point pin failure would occur.

I think that's probably the best answer to your question. You have to get to 60, 70, 80 percent melt fractions before high pin failure pressure conditions are attained.

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MR. WRIGHT: Pressure propogation through a flow meter is about a millisecond.

MR. PIZZICA: But I presume if he was talking about the convection of the material.

DR. SIEGEL: Within the pin.

MR. PIZZICA: Were you not, because convection of material would take longer.

PROF. KERR: I think the situation is now clear.

DR. SIEGEL: Well, I gather from what you're now telling us that the cladding will fail in the neighborhood of the mounting because of the localized pressure there rather than perhaps the pressure pulse will move up the pin rapidly enough to cause it to fail at the highest temperature points.

MR. PIZZICA: I'm pointing out that this is a possibility only. I'm just saying that it makes 60 percent of core height failure at least credible. I do not say that this is a certainty. I'm not saying that this is our best estimate.

PROF. KERR: You're also saying that it is almost characteristic of the way a code works that this will occur, aren't you?

MR. PIZZICA: Yeah, perhaps, yeah. That may be one implication of saying, go ahead.

DR. CURTIS: A little further down the slide, we have an example of the same code. Rather than applying this pressure differentially, applying it uniformly over the fuel region on an average basis.

MR. PIZZICA: Excuse me, Bob, but that's for the TOP.

DR. CURTIS: It's another problem. But the code quite handily will do either. It will apply differentially at the node where it is released or average it over the fuel column.

PROF. KERR: I guess what you're telling me is that this is a very flexible code, and if you tell it what to say, it will say it.

DR. CURTIS: It will give you back what you've put into it.

PROF. KERR: I think that that's admirable.

(Laughter.)

DR. KELBER: Not only admirable, but it's done because our rate's a reasonable hypothesis. However, there are experimental programs in place that can distinguish the mechanisms that are at work, and we'll discuss those later in connection with the ACRS recommendations.

MR. PIZZICA: But you're right: we're stuck with parametric variations. It's a fact of life.

Well, I have discussed this point at length.

(Slide.)

The conclusions of our LOF-TOP study at least are stated here. If one can allow the initial clad failure to extend axially during the course of the trans. t, if you assume the same criterion for the failure of additional nodes that you assume for the initial failure modes, then our study concludes that no matter what the failure criterion, that melt fraction failure criteria was, we tried a wide variety, from 50 to 100 percent melt fraction criteria, that you will predict no more neutronic energy deposition than in the case which excluded LOF-TOP failures.

If you exclude the possibility of an extension of the clad rip, which is a physical possibility, admittedly, if that is not allowed, then neutronic energy deposition can increase.

DR. SIEGEL: Can you clarify a bit what that phrase, "non-LOF-TOP transient power failure flow" --

MR. PIZZICA: Certainly. By that I mean that all channels or representative pins in a full core accident, SAS-3D calculation, are forced to use the SLUMPY module after pin failure conditions have been attained.

None are allowed to be treated in the burst failure mode that EPIC assumes.

Now EPIC assumes that pin geometry is maintained and that there is a relatively small localized clad rip, at least initially, through which molten fuel is expelled into the coolant channel. But pin geometry is generally maintained. The SLUMPY module in SAS-3D assumes that as soon as the failure conditions are obtained, that clad and fuel are homogenized, if not for the purpose of heat transfer, at least

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for the purpose of hydrodynamics, and that pin geometry is lost and that this amalgamated mass of clad and fuel can move in the whole sub-assembly.

DR. SIEGEL: Is that the differentiation between no and yes?

MR. PIZZICA: No LOF-TOP means that you did not allow any of the burst failure or epic mode failures in the lower and mid-power channels or sub-assemblies. Okay? And you forced all of them to go into the SLUMPY mode, which has different implications for fuel motion, and thus, for reactivty feedback, and it generally predicts a milder disassembly. Okay?

That's the bottom line.

DR. SIEGEL: Five of the veils have been removed, but there's still a couple.

MR. PIZZICA: I'd be glad to go further.

DR. SIEGEL: Go ahead.

PROF. KERR: Veil removal is supposed to be gradual.

(Laughter.)

MR. PIZZICA: So extension of the initial clad failure actually along the pin is a very significant feature to be modelled for TOP or burst failures in LOF-TOP cases.

LOF-TOP failures can lead to an increase in sodium boiling ramp rates in the vicinity of $100 a second.

However, if the axial extension of the clad rip is

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allowed, there will be an absence of fuel compaction toward the center of the core or toward the 60 percent of core height failure location, as well as fuel sweep-out, and perhaps enhanced sweep-out, causing negative fuel ramps very much higher than sodium voiding ramp rates.

But if one is depending on the sweep-out from EPIC, one must bear in mind that plate-out and plugging is not yet modeled in the code and that is an assumption.

MR. KASLENBERG: I have a question. Do we have experimental evidence for rip extension, tear extension? What do you base that on?

MR. PIZZICA: I don't base it on a data base and I certainly didn't use it that way in a specific best estimate sense. I used it as a parametric variation. I included cases with no extension allowed.

PROF. KERR: I don't think that this was meant as a criticism.

MR. KASLENBERG: It was for information.

MR. PIZZICA: I just mentioned that it was included as a parametric variation.

PROF. KERR: Then the answer to his question is no?

MR. PIZZICA: No, it's not based on a data base. I'm sorry. I'm sorry, was the question whether there is such a data base? That I'm not really qualified to answer.

I suppose you could interpret some pin failure results that way. But I think it's difficult to be that precise about the interpretations. But that's simply my guess I'm not an experimentalist and I shouldn't be, really, speaking on that subject.

PROF. KERR: But you're not aware of any such data.

MR. PIZZICA: Not that's precise enough to answer my questions here, no, I'm not aware of it.

DR. KELBER: We hope that the FD-2 series of experiments at Sandia will shed some light on this.

PROF. KERR: Thank you.

MR. PIZZICA: As I pointed out before, the results of the UK calculation were quite a bit different than ours when they included LOF-TOP failures, because they didn't include fuel motion reactivity. They also assumed a melt fraction failure and they increased their energetics significantly from the voiding ramp rates.

PROF. KERR: It was not a surprise that they were different, I presume. Was it a surprise that they were as different as they were, how different they were? Did you learn anything significant from this other than things came out the way that you expected that they would?

MR. PIZZICA: From that aspect of the calculation, it wasn't that surprising. Neither the qualitative nor quantitative aspects of the calculation.

No, I wouldn't say --

DR. SEALE: Could I ask the question in a slightly different way?

I think it's pretty clear that if you put fuel motion in, clad motion, that you're going to get a very different answer, and that's really going to dominate the results from the SAS, whether it's SLUMPY or whatever, calculations.

Was there a calculation done using a precursor of SAS-3D which had the kinds of things treated in it that the UK code had in it, so that you could compare the results of their code and the U.S. code which modelled, essentially, the same phenomenon and didn't model something like fuel motion, which was going to dominate the results, anyway.

MR. PIZZICA: There's a case that I want to discuss in about five minutes. It was for the WAC studies, and we specifically excluded fuel motion reactivity feedback. It was for a TOP, however, and this will give you some idea of what we do with SAS EPIC.

I don't know the specific answer to your question, whether or not an older code was ever used. I'm sure it probably was, but since I didn't run the calculations, I'd hesitate to speak about it.

But I can speak about mine.

(Slide.)

We also ran a 3-cent a second ramp TOP case for the

U.K. environmental studies about rod withdrawal. Boiling had started. The pin fails at the top of the core because, as Bob pointed out, the option was used of average fission gas pressure over the total core height in this case, and the hottest clad temperature was at the top of the core; therefore, it fails.

There were no surprises.

We predict that the sweep-out of the fuel shut the reactor down. Again, this excludes plate-out and plugging, always a qualifier. But since with a top-of-core failure it is probably less important than it would be.

PROF. KERR: What does "plate-out" mean?

MR. PIZZICA: I'm sorry. By that, I mean the adherence of molten fuel which has been expelled into the channel from the pin. The adherence of that to cladding in the exterior of cladding, once it enters the channel, thus limiting its potential for leaving the core.

PROF. KERR: Thank you.

MR. PIZZICA: So we predicted a negative half-dollar reactivity, even including the positive reactivity from sodium voiding. But the excessive negative reactivity -- in other words, the total caused by the sweep-out of the fuel -- added up to a negative half-dollar after just one pin failed.

So we did not continue the calculation. If we had,

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we could only increase the negative reactivity, re-entry of sodium, and further pin failures would have meant the same thing.

Again, the U.K. predicted different results because of the lack of fuel motion modelling. They predicted a prompt burst, and that was predicted by the sodium voiding, excluding --

MR. KASLENBERG: Could you leave that for a second? Is the pin that fails, the high power pin, a high power pin, low power pin?

MR. PIZZICA: The highest power.

MR. KASLENBERG: High burn-up pin?

MR. PIZZICA: They were all homogenized corresponding to mid-cycle. It was all homogenized burn-up.

MR. KASLENBERG: Because usually, calculations that I've seen when you get boiling before pin failure, you're generally dealing with fresh pins. Usually highly irradiated pins fail way before boiling.

MR. PIZZICA: Don't forget that the ramp is very slow. I had never done ramp rates as slow as this before. We're always interested in more spectacular things, I think. But their calculations also began to boil, I believe, before pin failure.

With the full pump head, however, the boiling is always in the upper exit blanket. It never reaches down as

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far as the core.

All I can say is that that's been predicted. This is the only study of a slow ramp rate that I have done.

PROF. KERR: You didn't have any low burn-up pins.

MR. PIZZICA: We did. But since this amount of negative reactivity was predicted with the failure of one of the representative pins, or one of the SAS channels, we did not carry the calculation any further, since it would have just added more negative reactivity.

But it was a whole core accident calculation. It was not a single pin calculation.

(Slide.)

Dr. Hummel did some single pin studies in support of the U.K. TOP and LOF work. These are some of the conclusions which I will merely state -- I don't have time to elaborate much on the LOF calculations. You've probably read them by now. There's a fair amount of coherence in voiding.

The conclusion is that clad motion may have occurred if there was more incoherence in our reactor set-up. But the conditions for clad motion would have been for the power to level off at something less than or equal to six times nominal for at least half a second.

It is quite possible that clad motion would not have occurred if we had included more incoherence. But with

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this reactor configuration, it did not.

If gap conductance is held constant over the transient, the single pin calculations indicate that boiling and clad motion are not very sensitive to that gap conductance.

However, a word of caution. If you assumed load values, gap conductance won't be constant. That's the word of caution. If, for example, one assumed low value for gap conductance, that would increase the fuel temperature, tend to, during the transient, which, again, would increase fuel swelling, which would increase the gap conductance and it would tend to correct itself.

So that you really need a mechanistic calculation of gap conductance. And assuming that it's constant over the transient, which is what we did here, might very well give you misleading results.

But since the calculation of gap conductance is very difficult, it's probably a problem that needs considerable work.

PROF. KERR: What would you say that generally you learned from that calculation that you just described that will guide your further work?

MR. PIZZICA: Well, we learned that we wouldn't have to -- we learned that if, in our whole core accident calculation, we were limited to a constant gap conductance, we would not have to vary that as a parameter and expect that to

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affect boiling and clad motion.

And it's always nice to know which parameters are insensitive because you can eliminate them from consideration.

PROF. KERR: And you consider the constant gap conductance a reasonable hypothesis?

MR. PIZZICA: Not physically, no. That's what I meant by my caveat. But it's probably as good as anything else at the moment. It's so difficult to calculate gap conductance.

PROF. KERR: It seems to me that if you learned that, physically, gap conductance is not constant and that it would be nice if it were, where do you go from there?

MR. PIZZICA: Well, there are, again, certain limits. But to be precise, one has to, I suppose, live with the fact that one cannot make too precise a calculation of gap conductance and one has to live with the necessity for using, not necessarily constant gap conductance, but very approximate calculations of gap conductance at the present time.

And in some cases, constant gap conductance is very worse than assuming those very approximate calculations.

PROF. KERR: What I'm trying to get at is how the whole process works. You've said that you aren't an experimentalist. I gather that you are mostly an analyst.

At some point, one has to take the results of the analysis and say, well, what do I do next? Do I keep writing

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parametric studies or do I conclude that large codes have limitations?

I'd have to have some other method, or are those experiments that need to be done which, if they produced results, would add something to the core of knowledge?

MR. PIZZICA: All of the above.

PROF. KERR: But you're living in a world in which both time and money are limited. What is the process which takes your results and says, okay, here's what somebody ought to do next. Is that Kelber's job or is it a recommendation that you make?

MR. PIZZICA: Partially, I'm sure. His decisions are partially based on our work and his own judgment. We can point the way to a great need, not that this is any surprise to anyone in industry, but a great need for a more precise calculation of gap conductance.

I'm just pointing out —

PROF. KERR: How can you calculate gap conductance more precisely?

MR. PIZZICA: How?

PROF. KERR: If you don't understand the physical phenomenon? Or do you understand the physical phenomenon well enough so that if you just had more computers and more money, you could calculate it as precisely as you wanted to?

MR. PIZZICA: The latter is not the case. Even the most sophisticated gap conduction prediction codes are greatly open, the results are greatly open to question. They are dependent on certain parameters.

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PROF. KERR: Have you thought about whether anybody will ever be able to calculate gap conductance in a satisfactory way?

MR. PIZZICA: Yes, I've thought about it. Maybe Dr. Kelber —

PROF. KERR: I'm trying to get an idea of what your group is up to doing.

MR. PIZZICA: I don't know. I'll give you a personal answer to that. I've thought about similar questions. This isn't the only thing that we had difficulty calculating.

PROF. KERR: No, of course.

MR. PIZZICA: There's a whole category of parameters like that and it's very difficult. All I can say is that we have to do the best we can with what we have. I don't know what else to say to that question.

We, of course, try to incorporate this date, the art in terms of the data base from experimental work in our modelling, and to the extent that that data base is not available, we are forced to make assumptions and to run parametric variations.

But you're asking whether that's meaningful.

PROF. KERR: For example, it might turn out that gap conductance is almost unpredictable from one failure to the next. I don't know.

MR. PIZZICA: It's probably not that bad.

PROF. KERR: If it is, then no matter what sort of model you assume, unless it has some sort of statistical characteristics which say, you know, this is the average behavior, or it's the typical behavior, it isn't a mechanistic behavior.

MR. PIZZICA: Certainly, you're talking about the average pin because you have thousands, tens of thousands of pins in a reactor.

So, certainly, you have a statistical behavior in your favor. You do have that working for you. You don't have to worry about manufacturing effects and that kind of thing and worrying about whether this pin fails there or there.

But — were you going to add something?

PROF. KERR: I was just going to say that at some point, Dr. Kelber or somebody has to make a decision that either the resources need to be put on experiments, or resources need to be put on code development, or resources need to be put on something.

I'm looking for the process whereby one makes that decision. I know that you don't make the decision, but do You make recommendations.

MR. PIZZICA: I, however, can make recommendations in my specific area of expertise and I'm happy to, and I do it all the time. Probably, mainly, my boss, Jerry Hummel does it.

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But the whole picture has to be seen, I think, by Dr. Kelber. And I'm not proficient in many of the other areas. For example, aerosol transport release. So he has to balance funding.

PROF. KERR: Wisdom is a commodity that is in very short supply anywhere. And I'm trying to find out what the source of wisdom is in this area?

DR. KELBER: Bill, you just missed a source of wisdom on the preceding slide. Phil, why don't you put up the preceding slide?

(Slide.)

The final conclusion that boiling clad motion times, which are very significant parameters, are not very sensitive to different assumptions for gap conductance.

MR. PIZZICA: But only if gap conductance is held constant.

DR. KELBER: I understand.

PROF. KERR: That comment really stimulated this question.

DR. KELBER: This leads us down a very interesting path. The question is: Do we have to develop a very sound, well based model with associated experiment tasks for gap conductance over a wide range or not?

This indicates the possibility that that is a lower priority than other parameters. There's a reason for this.

PROF. KERR: What is the antecedent of the "this"?

DR. KELBER: "These calculations." Now the reason that I'm kind of interested in this is two-fold: A, though there are some possibilities by using a type of Fourier transform theory of heat conduction to get some dynamic measurements of gap conductance, these are very difficult experiments. The probabilities of a success are low and we are evaluating that.

The CABRI program is considering it, but we are pessimistic. It's a difficult job, in other words.

Secondly, it is clear that in a mass calculation — that is, in a calculation that represents the average behavior of several thousands of pins, you are not interested in the precise behavior of each pin, but in their average behavior. And it's very likely that knowing all of the details of what happens in any one pin will not tell you a great deal about what happens in the average.

If we find that we are relatively insensitive to this value, we will be much better off experimentally. We don't know the range yet over which we are insensitive. You asked a further question: How do these recommendations get factored into our program?

We depend upon people like Phil and Harry to bring these points. For example, the point about the sensitivity or relative sensitivity to gap conductance and the validity of gh

different approximations to our attention, which they do relatively freely. And we then try to make a decision as to what to support on the basis of the pay-off to the program.

PROF. KERR: Let me see. I read something that says calculations indicate that boiling and clad motion times are not very sensitive to different assumptions for gap conductance held constant over the transient.

Now from a very naive point of view, a constant gap conductance during a transient would be the last thing that I would expect.

That may be as a result of my naivete. So when I read something like that, I sort of conclude that that doesn't really tell me very much about boiling and clad motion because I know that gap conductance is not going to be constant over the transient.

And so I'm puzzled as to what to do next on the basis of that recommendation. It hasn't told me anything that gives me very much guidance.

MR. KELBER: What we are looking at, Bill, is the question of do we attempt to make a detailed measurement of the dynamic gap conductance in a transient?

In particular, do we support that within the context of the CABRI program?

Now there are a number of modelling studies that have to be done. This is one of them. And it says that if you were to assume, as they do in the lightwater codes, a constant value for the gap conductance, and if you then vary that value, you don't find a great deal of difference. It tells you that there is some lack of sensitivity here.

Now we will have to explore in greater detail the lack of sensitivity.

PROF. KERR: To you that says more calculations, more parametric studies.

DR. KELBER: They are relatively cheap and fast, compared to the experiments at CABRI. If we have to do them, we'll do them.

PROF. KERR: But nothing is cheap and fast if you don't get any information out of it.

DR. KELBER: I agree with that, Bill, but we have to know what experiments to do. It is just as dangerous to convince yourself that a certain outcome is desired and is needed and do an experiment which is designed to produce just that outcome.

We have to know what is the crucial thing to test.

PROF. KERR: Please proceed.

(Slide.)

MR. PIZZICA: Our studies have led us to conclude that we have many areas of improvement. The model-loading of clad by solid fuel expansion is an area we would like to improve. Certainly, our studies have shown that it may be

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important transient fission gas release, motion of fission gas in pin after release. We need better data, which is an obvious point, on clad properties and the new low swelling alloys.

And the bottom line has been thoroughly discussed. But an additional point with respect to gap sizes on which gap conductance is also dependent is that failure by differential expansion or mechanical loading of glad is highly sensitive to this.

PROF. KERR: Is that last statement, failure by mechanical load is very sensitive to this, based on calculation

I can conceive of the kind of calculation which might be artificial which would indicate that result, which would not take into account the possible re-adjustment that takes place in the mechanical system if you applied stresses at various points.

Is that conclusion based on computer calculations or is it also a conclusion one would reach if he sat down and thought some about the way in which clad behaves in a transient situation?

MR. PIZZICA: I think the thought process would be so complicated that you couldn't do it without a computer.

So I would say that you need a computer code to do this calculation. It's not just a thought experiment.

It's a calculation that was done with the F-PIN code which was developed.

There are other calculations, I believe, that include this. The Los Alamos fuel failure code, LAFFA, and others.

The calculations showed that this can be a possible cause of clad failure less than .003 centimeters. That was the conclusion of the studies. It was a parametric study done with a theoretical code, and I'm not aware of the details of that.

PROF. KERR: What does one mean by "need better estimates of initial gap sizes"? Does that mean that one needs to try to make measurements as built?

MR. PIZZICA: Initial, I'm sorry, steady state should be the key word there, not cold dimensions. We're talking about the beginning conditions.

PROF. KERR: How does one get such a test?

MR. PIZZICA: Well, I suppose that's better left to an experimentalist. I don't know. I'm sure that we have to have a lot of information about fuel and clad-irradiated and the flux levels that exist and the kind of reactors that we are talking about and not in, for instance, experimental reactors.

We have to have flux levels and we have to have the irradiation times comparable to what they would be assumed to be in the calculation. The irradiation swelling of fuel in

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clad is very significant.

PROF. KERR: The people making this test, this recommendation, view this as something that is attainable in some fashion.

MR. PIZZICA: Possibly, but again, I think that that's better directed at an experimentalist. I'm sure it's attainable to some degree. I wouldn't be able to judge the degree to which that data base is attainable myself.

PROF. KERR: Thank you.

MR. PIZZICA: There's some other material to review on the TOP and the WAC round one, which I won't go into. It was presented before, and there's probably no need to re-emphasize it here. There is a viewgraph telling you the present status, which is essentially self-explanatory, about the present round of WAC studies. And I will skip to this last slide, however, since I'm running out of time.

(Slide.)

And talk about the modelling needs which have become obvious as the result of our calculations, and I think this goes back to an earlier question. We need better mechanistic modelling of clad failure in the code.

PROF. KERR: What is the significance of the adjective, "mechanistic," here?

MR. PIZZICA: Yes. "Mechanistic" as opposed to, for example, the differential expansion of fuel in the clad leading h to mechanical loading of the clad, as opposed to assuming that clad will fail once a fuel melt fraction, a given fuel melt fraction, has been reached.

Just assuming that there's a correlation between 75 percent melt fraction, that kind of thing. That's not mechanistic. Loading by fission gas pressure, assuming that the hoop stress you calculate with the fission gas is an appropriate failure stress. Okay.

PROF. KERR: In your view, is it practical to achieve this recommendation?

MR. PIZZICA: Yes, it is practical to achieve a better mechanistic model. Again, you probably have questions about the data base supporting that model, and there are many gaps in that data base, certainly.

On the other hand, there is some data base that can be used, and I think that it should be used. We are not making the best use from a modelling point of view of the existing data base at the moment.

PROF. KERR: Is this because you don't have the resources, or somebody hasn't told you to do this, or what?

MR. PIZZICA: We have seen, again, for instance, clad loading by differential expansion. We have seen that that could be a possible mechanism. That wasn't the mechanism we were thinking too much about a year ago before we did the experimental studies.

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Now that gives you one value of parametric studies. It shows you where you're modelling needs perhaps lie.

Yes, we do lack the resources. Everybody lacks the resources to go into all of the areas of modelling that are needed. It's a matter of time and priorities.

This is our laundry list. There's an adequate treatment of ejection of fission gas when the clad is weak; for instance, in a partially voided channel, LOF, the clad temperatures are very hot at the top of the core. There could be enough fission gas pressure to burst that weak clad and release fission gas into the sodium bubble, which would have implications for later pin failure when higher fuel melt fractions are attained in the same pin, because it would mean that less fuel fission gas is available later on to pressurize and, thus, expel it into the channel.

So this has implications for the fuel ejection channel. There's inadequate treatment of melt-through type of failures as opposed to burst failure, which is an ejection of molten fuel, the latter being an ejection of molten fuel through a relatively localized clad rip and melt-through failures being more of a massive disruption of the pin.

There's a need for thermal expansion of fuel and clad after failure because it's very significant for pressure calculation. Fission gas pressure, that is, and we need fuel conduction and a calculation for slow transients, clad temperature calculation, which has a bearing on pin failure, and also, disassembly conditions.

Plate unplugging has been discussed at length. The SAS-3D/EPIC code is totally inadequate where pin geometry is totally lost. In other words, the EPIC model, at least, if not the SLUMPY model, must assume pin geometry. There is no way of treating the transition to a total disruption of the pin geometry in the EPIC model at present.

We need approved treatment of later stages of rapid transient, i.e., disassembly conditions, in terms of the capability of clad to constrain fuel motion and in terms of clad being able to insulate molten fuel from sodium in the cooling channel, in terms of the pressurization and possible rapid expulsion of the sodium in the channel.

These are all open questions.

So I will end the discusion there.

PROF. KERR: When you make these recommendations, have you looked at what SIMMER will do to see whether it will do any of these things that SAS-3D/EPIC should be able to do?

MR. PIZZICA: Have we tested the sensitivity of the TOP calculation? For instance, the plate unplugging, that kind of thing?

PROF. KERR: No. I'm saying have you looked to see whether SIMMER --

MR. PIZZICA: Sorry. SIMMER -- I missed the point.

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PROF. KERR: There does exist another code which —

MR. PIZZICA: Can SIMMER treat these things and need we do it with SAS-3D/EPIC?

PROF. KERR: Could undoubtedly treat these problems. It would treat them in a different fashion. It hasn't even been used in the sense that EPIC is used, would be used for experimental analysis.

I remember listening to a paper by Bill Volk at Los Alamos on the comparison. I remember also the run time was a bit long. The detail might be more. It might, in certain respects, be a more adequate calculation. It certainly would be able to calculate the later stages of an accident and, in fact, all the way to the impact on the vessel.

That stage of the accident I can't begin to treat.

PROF. KERR: You have done through what seems to me to be a useful exercise of comparing the ability of the decay code builders to calculate. With your ability, if you had the resources, do you think it would be worthwhile to go through a similar exercise with the people who put together SIMMER? Or do you think that that's likely to be unproductive?

MR. PIZZICA: It could be productive, I suppose. I don't know if there would be any different conclusions reached, however.

See, the only difference between SAS-3D/EPIC and SAS-3D is the replacement of the SAS-3D module with EPIC. And since apparently there was just a study done of the comparison with SIMMER and SAS-3D, the only difference in such a calculation would be the extent of the reliance on SAS-FCI as opposed to EPIC.

Now if that calculation, which I haven't seen, really does rely on SAS-FCI very much, then it might be fruitful. If not, it would not.

DR. SIEGEL: Bill, I'd like to ask you a question. You say the exercise of examining the relative ability of the two sets of computations to do something, I'm puzzled by your use of the word ability. I'm lost as to what has been compared, they're two different codes apparently, two different computing approaches that come up with some different answers. Has that difference led to illuminating the modeling needs somehow?

MR. PIZZICA: In certain respects, definitely. For instance, I didn't really have time --

DR. SIEGEL: I guess I don't have a good feel for the whole process, in its entirety. I have a feeling that these are two chess playing codes competing against each other, which can't even tell which one won.

(Laughter.)

DR. SEALE: Victory is not the same thing to both codes.

MR. PIZZICA: I would like to give you one specific example. It's very difficult always to talk in generalities but you have to for the sake of compression in a talk. But one specific example that would give you a picture of the value of a comparison calculation is in our WAC studies on round one. All the other participants did switch to a second code to calculate disassembly.

Okay. Now, those second codes either were Venus in the U.K. instance, or were Venus in the Belgian and German instances, a code called CADUS. Now those 2-D codes make certain assumptions. They assume, for instance, that if in a certain cell, a 2-D cell, and remember you're going from one-dimensional representative single-pin geometry to a 2-D mesh, which is a radical change and it's an arbitrary one.

Just halfway in the calculation they assume that if there is sodium in that cell, that the thermal expansion of the fuel, once it reaches zero void fraction, can pressurize that sodium according to the sodium bulk modulus and create single phase pressure in that cell.

DR. SIEGEL: Can create what?

MR. PIZZICA: Single phase pressures in the sodium liquid. And those are very high pressures immediately, as soon as that zero void fraction is reached. If those pressures prevail, all the material again at the switch-over point is free to move, all the fuel, all the clad, all the sodium is free to move under those very high pressures.

Those conditions and results are radically different than what would be predicted which the disassembly calculation done by SAS-3D/EPIC, which does not have a discontinuity in the calculation, but would preserve the pin geometry and calculate at least the beginnings of its disruption.

We haven't gone too far along that road, but I'm just saying we did this comparison in the disassembly phase and saw radical differences in our calculations due to the different modeling. We still preserve the single representative pin SAS type of calculation even after the so-called disassembly switch-over point.

They switched to a whole other code, a very radically different code and we saw drastic differences coming out of that --

DR. SIEGEL: How do you proceed to try and resolve these difference, by simply physical plausibility or what?

MR. PIZZICA: Well, I suppose we're not going to have too much of a whole-core data base. There is a certain element of that, certainly. You have to rely on arguments such as, it's very artificial to move from a calculation which does a single-pin type of geometry and suddenly where the clad isn't moving, the fuel is not as free to move, since it's been constrained by the clad.

Perhaps the sodium motion can move, but it's limited mainly to one-dimensional motion. It's a radical difference to go to that, a 2-D calculation where suddenly everything can move and it can move radially as well as axially, and there are no constraints on its motion radially.

PROF. KERR: Could I interpret your answer to mean that it isn't really clear as to how one resolves the differences?

MR. PIZZICA: Well, there is as I said, an element of judgment, a judgment factor in saying that it's physically plausible to resolve it in the sense of settling a question in physics experimentally. I think it's very difficult to do this on any kind of large scale.

One can do it on a single-pin scale, and say that it stands to reason — I mean, even if disassembly conditions could be modeled in something like TREAT or ACPR, you don't suddenly see a radical change in the configuration of materials after a certain fuel node fraction is attained.

I suppose that's the kind of argument I would present there. It's a limitation that has been problematic, a computer code limitation.

PROF. KERR: Mr. Kelber?

DR. KELBER: One of the side benefits that has come out of SIMMER, which, as you know is basically addressing the transition phase, is that it is capable of addressing some of these difficult computational problems. I would point out that the extension of SAS into the disassembly regime came about as the result of an exercise in wish fulfillment, and that is that all core disassemblies would be very mild, would be essentially the result of fuel expulsion in the upward direction. As it happens, there's a large class of accident scenarios which are like that, and they are very useful. It's an interesting exercise in judgment as to whether you have to abandon that scenario for a more complex one involving the more classical data tape type of accidents.

It's a little early, Bill, to decide what the criteria are. These various comparison calculations enter in to that. SIMMER/SAS calculations that I've told you about earlier are being reported on now and will enter into that, and I would guess that we will, within the next few years, be able to get some better judgment as to how to divide the scenarios in this area. If we have to divide them at all.

PROF. KERR: Thank you.

MR. KASLENBERG: With respect to your shopping list of modeling needs, some of the needs are being addressed at other laboratories and some of the laboratories are NRC contractors, I presume, since I know some of them. I just wondered, are you planning to go ahead and to make all of these changes in SAS-3D, or will you just adopt something?

MR. PIZZICA: To the extent we can adopt it and we agree with it, we certainly would. I have no objection to taking over others.

MR. KASLENBERG: But you do plan to continue to

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try and improve the code system?

MR. PIZZICA: The long-term plans, I suppose are something I couldn't address as well as Dr. Kelber, for instance. To the extent that we intend to improve the code, these are the areas that we would start to work in, at least.

It's not my place really, to assess long-term assignment of priorities, but this is where I would suggest putting one's effort. If one were to improve the code.

PROF. KERR: Are these recommendations made in the context of being familiar with what's being done in other modeling activities or in the context of what you'd do if SAS-3D/EPIC was the only code around?

MR. PIZZICA: Both.

PROF. KERR: I don't understand that answer, I'm sorry.

MR. PIZZICA: Some are based on comparison with other codes and knowledge of other coding efforts, for instance, the mechanistic modeling of clad failure is based on the knowledge that there are codes that calculate differential expansion in mechanical loading of the clad. So we might want to introduce that into the modeling.

A number of the questions which have arisen with respect to disassembly modeling have arisen due to the use of our own code by itself, but nobody thus far has tried to calculate the accident in the same fashion as far as we have, with a representative pin model. And some of the questions which have led to our need for further modeling, in our view, have arisen out of the process of just running our code by itself. That's what I meant by both.

And disassembly is a good example of that, since there's no other code that does that kind of thing.

PROF. KERR: Thank you.

Does that conclude your presentation?

MR. PIZZICA: Yes, it does.

PROF. KERR: Are there further questions?

(No response.)

PROF. KERR: Thank you, sir.

DR. KELBER: The next presentation is by Bob Curtis, Chief of the Analytics Branch, who will discuss some of the modeling efforts that we are sponsoring at Argonne, the so-called FRAM code.

DR. CURTIS: My name is Robert Curtis. I'll try to be quite brief in filling in for Phil on one other activity that's going on at Argonne in Dr. Hummel's group.

Before I do, I think I should remind the group that the SAS code, of course, is not our development; it represents the distillation of perhaps a division of 100 men and has been under development since about 1969 and represents the best judgment of both the modelers and the experimenters at Argonne in that division.

Certainly there are some other activities, but a large fraction of the experimental work and the analytical work that has been done by that division at Argonne is distilled in the SAS code.

PROF. KERR: You are reminding us of this because —

DR. CURTIS: Because I thought that the physical basis for many of the assumptions and alternatives with respect to SAS were left a little vague by the fact that we in our parametric study looked at a wide range of alternative formulations. And the choice of these formulations and the judgment as to which of them to use, if one were sincerely going for a best estimate, is not quite as open as our parametric study might indicate.

In addition to the EPIC work -- which I think has been adequately described -- the SAS like calculations introduce an excessive coherence in terms of reactivity feedbacks, particularly in the way that it treats the sodium boiling and the sodium voiding. This reactivity insertion, which come as the result of the voiding of a channel, which the representative pin provides the reactivity of perhaps pins of sub-assemblies all on the same time scale.

This, in general, is conservative in that it probably overpredicts. If the channels are properly selected, at least, it overpredicts the results. But one cannot a priori be sure of this, particularly when the resulting ramp, as we've seen, is the result of competing effects.

A number of investigators have looked into this problem of coherence and certainly there is experimental evidence in the SLSF and other experiments that the incoherence in the development of boiling and in the voiding does extend the time scale and thus reduce the reactivity insertion.

This incoherence has not been adequately looked at in terms of SAS type analysis. I'd like to detail very briefly our plans for looking at that particular problem.

PROF. KERR: What do you mean by that caveat? I could interpret it as meaning that it has been looked at in some kinds of analyses but not in SAS type analyses.

DR. CURTIS: It has been — what I mean is, it has been looked at both experimentally and analytically in isolation from the calculation of the reactivity feedback associated with the boiling problem. In other words, the boiling problem has been looked at as a separate effect, the voiding and voiding rates and the distribution of the void has been looked at as a separate effect, but it has not been linked in any coherent fashion to a calculation of the reactivity changes associated with this boiling phenomenon.

Clearly, snapshots of reactivity have been taken in calculating them; in other words, a particular void pattern has been calculated. But the problems have not been linked together on the same time scale.

PROF. KERR: By any method, SAS or otherwise?

DR. CURTIS: By any method, to my knowledge that's correct.

PROF. KERR: Thank you.

DR. CURTIS: And it is in the interest of linking these effects together, because the reactivity associated with the voiding is the principal intial driving reactivity of the accidents that we are most concerned about.

PROF. KERR: If one makes physical arguments, does one expect that reactivity feedback will have significant influence on the coherence or incoherence?

DR. CURTIS: Yes, one can make a physical argument that the temperature distribution at steady state, which has been well calculated in a sub-assembly, has a distribution from the sub-assembly center to the sodium film on the wall of about 300 degrees Fahrenheit; in other words, the channel which is between the outer row of pins and the wall, is about 300 degrees Fahrenheit below the hot channel surrounding the center pin.

One would expect that this would influence the development of boiling and that it would stretch out the time scale for the voiding of the channel, and it might very well provide a certain amount of reflexing in this colder area, and perhaps a certain amount of spray cooling and a much slower drying out of residual films than would be on the hot pins if these separate conditions existed in the same sub-assembly.

PROF. KERR: That's an argument which says that one may expect incoherence. My question was, given that one has some picture of that, and one puts the reactivity figure back into the picture, is there some physical argument that says that this may or may not have a significant influence on the course of development of whatever occurs?

DR. CURTIS: I think the key parameter on the severity of the accident is the ramp rate at the time that you are near the problem rate. If you were to void, or calculate that you void, some number of sub-assemblies like 30 in a completely coherent fashion, you've introduced a large amount of reactivity on a time scale associated with what is perhaps happening only at the center of a selected number of these particular sub-assemblies.

And while the total reactivity insertion would be the same, it would be stretched out over a somewhat longer time, resulting in a lower ramp.

PROF. KERR: Let me see if I understood your earlier statement. I got the impression that you said one had a fairly good idea of what would happen to the voiding pattern given the temperature distribution initiation of the accident and some history of the development of the transient, if one ignored reactivity feedback. But that nobody had done a calculation or anything, which would now take the reactivity results from the voids and feed that back in to the description.

Did I misunderstand? I thought that's what you said.

DR. CURTIS: That is essentially what I said.

PROF. KERR: Now, my question was, given that that hasn't been done, is there some physical reason or some plausibility argument that says, if I inserted this feedback, I expect to see something quite different or about the same as what I have seen without putting the reactivity feedback into the picture.

Is my question clear?

DR. CURTIS: No, sir, it's not. Let me see, I think I know. Let me try.

Certainly, there have been calculations, and there is a fairly good understanding of this incoherence from both experiment and multidimensional calculations. Clearly, the reactor physics problem of the worth of voiding sodium in selected regions in the core and the static problem is reasonably well understood. It's as well understood, and there is a substantial data base of critical experiments, which again are static experiments, not that the calculations are very close to the experiments, but there exists a substantial experimental data base, and there are calculations which are reasonably good. Certainly, there is room for improvement in this calculation of the sodium worth.

What has not been done is to find a simple way to take what are essentially two three-dimensional calculations and find a reasonably simple way of incorporating the general conclusions that are drawn from them into an accident analysis code, such as SAS, which will allow repeated examination of multiple cases. And this is the objective of what is about a three-man effort at Argonne in Dr. Hummel's group, that I would bring to your attention.

Our main emphasis here is on the relatively slow low-ramp accident, with the clear understanding that in accidents that happen fast enough, incoherencies are of far less importance because they disappear so quickly that it's not worth investigating them in significant detail. As I said, we plan to devote about three man-years in this next year in an effort to develop a module called "BIFLO." BIFLO will attempt to introduce the multiple dimensional effects of boiling in the SAS to improve the estimation of reactivity feedback during this particular phase of the accident. We hope that it will be sufficiently simple that these effects can be introduced into SAS calculations without greatly expending the running time and destroying the usefulness of the basic tool.

In conjunction with this effort, we will benchmark the BIFLO models against the full multidimensional codes such as COBRA, such as COMMIX, which we have out soon, against the work of Ishi and Chen, and their as yet unpublished work on sodium boiling and voiding at Argonne.

The purpose of this code comparison is, as I said, to see if a relatively simple treatment of incoherence can provide essentially correct neutronic feedback and still become compatible with SAS and the running time that we have an interest in.

I would not like to go into any of the details of the modeling. I will save that for perhaps either next spring or next year when these ideas are a little firmer and a little better delineated.

So, in addition to certain improvements that we plan in EPIC, as Phil talked about, we are looking at the second problem of accident initiation in the slow low-ramp area, the area of the incoherency of boiling and how to properly represent the neutronic feedbacks associated with this. And this work will essentially occupy three man-years for the next fiscal year.

Any questions?

PROF. KERR: Questions?

(No response.)

PROF. KERR: I have no further questions. Thank you.

I am going to declare another 10-minute recess at this point.

(Brief recess.)

PROF. KERR: May we reconvene.

We go now to sodium mixing. But before we do, will somebody remind me what "FRAM" stands for?

DR. KELBER: Fast reactor accident modeling code.

PROF. KERR: I think I knew that.

DR. CURTIS: FRAM is used to account for those modules in different treatments which we have inserted into SAS.

PROF. KERR: And it says "HCDA transaction." I assume that's transition, or do we now have a transaction phase for fast reactors?

DR. KELBER: Where is this?

MR. KASLENBERG: On the agenda.

PROF. KERR: It's not your typo. If it's a typo, it's ours.

DR. CURTIS: We don't have an agenda with those words on it.

PROF. KERR: I am told it should be "transition."

DR. CURTIS: It probably should be deleted altogether.

PROF. KERR: Perhaps we should have a transaction phase.

Okay. Sodium mixing. CD code development.

DR. KELBER: If I knew what kind of transactions you had in mind, we might talk about it later. That probably should have been deleted altogether.

I would like to introduce Bill Sha, from Argonne National Laboratory, to discuss the family of codes in this area.

(Slide.)

DR. SHA: The objective of this program is to develop 3-D transient codes for thermal hydraulic analysis of reactor components with specific emphasis on the natural circulation mode. I deliberately put the "rigorous" in there. I think that the code I am just about to present to you is probably the most rigorous code available on the market as of today.

PROF. KERR: Is "rigorous" synonomous with "complicated"?

DR. SHA: Well, not necessarily, because the code we are developing is pretty clean-cut. And we have added it to clad failure at this moment. So, we clearly have defined the geometry, so in that sense we can --

PROF. KERR: Tell me why you particularly need a 3-D as contrasted with a 1- or 2-D here.

DR. SHA: I guess the previous speaker mentioned about -- ve much emphasized the incoherence of the boiling, and this code actually eventually leads to that calculation.

PROF. KERR: You don't have a lot of boiling in a natural circulation mode, or do you?

DR. SHA: In some cases, it's possible.

PROF. KERR: The reason you need the 3-D is to treat the void location?

DR. SHA: Yes. Another thing is this code is specifically for rod bundle calculations. Now, in any rod bundle calculation, you're dealing with very complicated geometry and the uncertainty of the geometry mixed up with the uncertainty in physical model. Now, we will show you the code we have developed. We completely remove uncertainty from the geometry. Therefore, we can concentrate on modeling the fillings.

Of course, this work is in conjunction with the SSC code, and this is a component code, and that is a system code.

(Slide.)

Two codes we are developing: One is the COMMIX code; one is the BODYFIT code. Of the COMMIX code, we have two versions: one is COMMIS 1; the other is COMMIS 2. COMMIX 1 is a three-dimensional transient single-phase compressible flow with heat transfer. COMMIX 2 is the three-dimensional transient; we use a two-fluid model, so therefore we can treat the nonequilibrium temperature and inhomogeneous velocities.

The formulation we use, again, is different than most people use. We use a porous medium formulation. This code can treat the two classes properly. One that casued a continuing problem, the single code can treat the reactor inlet and outlet plenum piping system. The same code can also treat the fuel assembly. For this discussion we are concentrating on the fuel assembly. And this code has been extended to other components, IHX and steam generator applications.

PROF. KERR: Let's see. A continuum.

DR. SHA: Let me give you the definition of what I mean by "continuum" here. A whole solid structures, internal-external solid structures are treated in the continuum. For instance, you look at the rod bundle. Each rod you treat as the boundary condition. This type of condition, I define as a continuum.

Now, a quasi-continuum, internal structures are homoegenized interflux. And then I call the type of circulation a quasi-continuum. With numerical technique of the use of staggered mesh -- that means we treat the pressure density and temperature at the center of the mesh and the velocity at the side of the mesh. And we use the IMF, which was developed at Los Alamos.

Recently, we added a rebalancing technique which speeds up the computation, and so far this technique looks pretty promising.

(Slide.)

The next code we developed is the BODYFIT code.

DR. SIEGEL: Excuse me. One question. The two-fluid model, is it liquid and vapor only, or do you consider anything like ideal gases?

DR. SHA: At the present time we use liquid and vapor or a water system and vapor. It can certainly easily be extended just for gas.

Our next code we developed is called the "BODYFIT" code, and this is very unique to me. Now we can claim after this code is developed, we can claim for laminar flow and rod bundles, for the first time we can calculate without any exception. This I call the "benchmark" type of the rod bundle calculation. All we require is the geometry of the rod bundls and the thermal physical properties, without any assumptions for laminar flow, because for turbulent flow, you involve the turbulence model, and you have the enclosure model.

At the present time we are concentrating on developing BODYFIT 1 code. It's limited to a single phase, and the formation, we use the boundary fit code in the transformation. I shall explain this a little bit later.

We transformed the rod bundle, complicate the rod bundle geometry into rectangular coordinates; then we solve also the equation from the physical plane to the transfounder plane. The reason we're doing that, therefore all your boundaries of the physical system are corresonding to the grid line. And that's the only way you can solve this problem vigorously without any allowance made for laminar flow. And I think this is the first time for all the calculations that we can claim now for laminar flow we can calculate without any approximations.

PROF. KERR: What are the cases which one needs for the amount of rigor?

DR. SHA: For instance, for the experiments, the small rod bundle experiments in there right now, they're really all mixed up because every code you use you approximate the geometry, and those approximations, for example, mixing between the subchannels, the subchannels are so complicated, so all the uncertainties in the geometry are lumped into the physical model so you don't know what's going on to the end.

So, this code at least can clear up this end. We will at least divorce it from it, and certain techniques of geometry that you normally lump into the physical model. So this is a very important, I think, step forward in this direction for analyzing the small-size rod bundle experiments, especially two-phased flow.

Like I mentioned before, we are only emphasizing. Now in this kind of code, you remember, now, I consider this problem in the fuel rod bundle as a continuum. In the previous slides, in the COMMIX code, I characterized this as a "quasi-continuum." The reason now is in this case all the fuel rods are treated as the boundary, so it's a continuum problem.

(Slide.)

Like I mentioned before, this talk is more or less emphasizing fuel assembly modeling. But let me just show you some capability in our modeling efforts. We can treat wire wrap fuel assembly. We can also treat the grid fuel assembly. I will show you some of the results later on.

We also assess the fuel rod thermal model. We actually attack the clad temperature and fuel rod temperature. Again, I would like to emphasize, although we have fuel rod modeling, our primary objective is the cooling dynamics, so we cannot develop a very sophisticated complicated fuel rod model. That is beyond the scope of our code.

But we do develop -- we have the most efficacious model for the fuel rod model. We also account for the duct wall temperature measured during the transient. Also, in some experiments, you have filler in the test sections. We also model that.

We also model the gamma heating. We have a turbulence model.

PROF. KERR: What is a "filler wire"?

DR. SHA: Filler, in some of the experiments, the hexagon in the rod bundle, we have more coolant at the edge.

PROF. ERR: This is an experiment, not a reactor?

DR. SHA: Yes. You will see the wire analysis, why we developed the technique to analyze the experiments, to see if we can get agreement. But this is optional; you can remove that easily.

(Slide.)

Now, I show you some of the numerical results of the COMMIX 1 code.

(Slide.)

I present here -- you know, we did a lot of calculations. I cannot go over all calculations, but I selected three areas which represent the capability of our code. We analyzed the P-2 LOPI transient, loss of -- this is a very fast tranient; we analyzed that, which also compares with the experimental data.

The next one is the same test section, and then we analyzed natural circulation transient. We also can compare the experimental data.

Finally, both those two fuel assemblies are the wire wrap fuel assembly, and this is a grid fuel assembly. And we just have the experimental data from Germany, Karlsruhe. And the seven-pin grid assembly is the near simulated pvii

loss-of-coolant transient.

MR. KASLENBERG: Do you mean fast thermal transient?

DR. SHA: Yes. Also the velocity. Yes.

Then, also, we recently made a W-1 pretest prediction, and I think the results should come out fairly soon.

(Slide.)

First, let me represent the LOPI transient in the 19-pin rod bundle. The initial power is 730 kilowatts, and the velocity 6.4 meters per second.

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(Slide.)

The transverse set-up in the calculation looks like this. This slice also shows the normalized radial power distribution. It also shows the thermocouple locations.

(Slide.)

The axial model using the COMMIX code calculations looks like this. You have 24 nodes with simulated entrance region, the heated region and the exit region. Again, thermocouple locations are shown here functioning, as I mentioned.

(Slide.)

Now, what I meant by the fast transient, this will tell you. This is the flow structure using the transient, this is the flow rate starting at full flow down to 60 percent of flow within 1/10th of a second and the power from the full power down to 10 percent, a little over 10 percent of power, within again, 1/10th of a second. 1-1/2 tenths of a second.

It's a very rapid transient and this demonstrated code capability is a very difficult calculation.

(Slide.)

Here we show you the prediction for steady state, compared with the experimental data and the expermental data is the circle, and we also calculate in the code, calculate the coolant temperature as well as the wire wrap temperature. The reason you calculate the wire wrap temperature is that the thermocouple are located in the wire wrap and you can see the agreement is, I would think reasonably good.

This is for steady state. Now we show you this in the transient calculation.

(Slide.)

This is the thermocouple 30. Thermocouple 30 is located in here. I'd like to point that out. We did not normalize at the steady state; in other words, we calculated the temperature at the steady state and carried on for the transient. Most of the people represented data that normalized that to this point and then carry it over.

DR. SIEGEL: When you say you calculate for the steady state, what are the input data for that calculation?

DR. SHA: All we need is geometry, inlet velocity, okay, then the inlet temperature, and we know the power. That's all.

DR. SIEGEL: The macroscopic inlet flow rate?

DR. SHA: Yes, that's correct. Total flow rate, of course, we have a flowmeter down below so they know what the total flow is through the rod bundle.

PROF. KERR: Now that's a temperature measured somewhere. Where?

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DR. SHA: This one is the thermocouple 30 measured in here as a function of time during the transient --

PROF. KERR: Somewhere along the wire wrap?

DR. SHA: The wire wrap, yes. The next viewgraph will give you similar information at the different location.

(Slide.)

DR. SIEGEL: Excuse me again, what is that Parameter K?

DR. SHA: K is the axial level. I'm sorry, I should have identified this. K is the axial level.

This is thermocouple 35 at the axial level 13. And this is the calculation and this is the experimental data.

(Slide.)

This viewgraph shows similar information for a thermocouple 20 located in here and axial level 19 is above.

(Slide.)

That's similar information for thermocouple 19, and then even almost at the top of the bundle, axial level 22, thermocouple 19. And the agreement is pretty good.

(Slide.)

Now, the next transient --

PROF. KERR: Is there disagreement within the

Kap

accuracy of measurement?

DR. SHA: Unfortunately, we tried to pin the experiment down. Everything was satisfactory. This is not unusual, it's fairly typical.

PROF. KERR: It's not unusual?

DR. SHA: You look all over. The rod bundle measurements is not easy. So, they won't give you, you know, the flap. We have a report, but never in writing.

DR. SIEGEL: What's on the outside of the wrapper?

DR. SHA: The outside of --

DR. SIEGEL: Is there sodium flowing?

DR. SHA: Yes, sodium flow, another bigger pipe.

DR. SIEGEL: Was that flow interrupted?

DR. SHA: That flow is not interrupted as far as we know. Well, we do have private communication because those people perform experiments so we have close communication with them.

DR. CURTIS: Surrounding the X-scan that you see here is a ceramic insulation and then another can and then a bypass flow which is not interrupted on the outside for the insulation.

There was an attempt to isolate this pin bundle firmly from the bypass flow.

DR. SHA: That's correct.

DR. SIEGEL: So that case where you're looking at thermocouple 35, it really is looking at the inside, not anything on the outside.

DR. SHA: Thermocouple 35? Oh, yes. That's right. That's inside. It's not outside. So far, all I show you here are thermocouples inside the rod bundle.

Now, our next calculation shows natural circulation -- actually, natural circulation started at that higher velocity and went down to a very low velocity and the starting velocity is 3.4 meters per second. So it's certainly equivalent to begin and total power, 32.6 kilowatts and the tested geometry of the rod bundle is identical, as we've shown on the previous one.

(Slide.)

I'll show you the force function for this calculation. See, this is a scale a little bit different. You can see it follows much faster, now we're talking about 10 seconds, so the velocity from full velocity down to five percent of the velocity was in one second. So it's a relatively much slower transient, like I showed you in the previous calculation.

(Slide.)

Now, this will show you the comparison between the experiments and the COMMIX calculation for steady state. As you can see, agreement is very good, within four or five degrees.

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(Slide.)

I don't know whether you can remember that. That's thermocouple 21 at the axial level six. Again, we compare the calculation with the experimental data. Remember, this calculation, it's not easy to get good agreement. It is not a simple calculation. You need a lot of modeling work to get an agreement like this.

PROF. KERR: Near the end, you're getting a disagreement of -- what? -- 10 degrees, eight degrees?

DR. SHA: I think it's probably 10 degrees, something like that. Some of them are better, some are worse, as the next viewgraph will show you. It's much better.

(Slide.)

You see, that's at thermocouple 23, level nine. The agreement throughout the transient is very reasonable.

(Slide.)

This shows similar information at a different location, axial level 13, thermocouple 32.

(Slide.)

This again, agreement not too good towards the end of the transient.

(Slide.)

Now, I show you the third class of transient. It's a slow transient, and those are data from the Germans,

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Karlsrahe, seven pin, and they have very good measurements over there. Those are actually the operating parameters as shown on this viewgraph.

(Slide.)

These are other thermocouple locations in the seven pin geometry and this is the transverse partition used in the COMMIX code.

(Slide.)

This is the axial partitioning used in the COMMIX code. We use a non-uniform mesh; the reason we're doing that is because there is a grid so we tried to put the mesh right on the grid.

(Slide.)

Here is the force and function for flow. It's slowly, linearly coming down. That's the force and function used in the calculation.

(Slide.)

Here, I show you the comparison between the experiments and our calculation for thermocouple 22, located in here throughout the transient.

(Slide.)

The next viewgraph contains a similar calculation, so I'll just flip over.

(Slide.)

This agreement is not too good. Again, if I

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normalized it I would get a better answer on that.

(Slide.)

This is similar information for thermocouple 9 at axial level 17. Now, I move on to the next version of the COMMIX code in the two-phase flow area.

(Slide.)

I show you some of the numerical results of COMMIX-2. At this point I'd like to mention a couple of things. I don't want to say that our actual program is not a big program. Our funding level for this year is only 370K, no comparison with like the TRAC code or something like that. So we are in order of magnitude smaller.

Now, in the two-phase flow area, the two-phase flow area, we are just about at -- all the coding is now ready to do meaningful calculation. And in the last few months and maybe the next couple of months, we will concentrate to develop new solution techniques and we are thinking -- we can make a presentation to you. We can show you, hopefully, some kind of breakthrough much faster in terms of the running time.

At this moment, we're still investigating. But there are some indication that show us it's quite promising.

(Slide.)

Before we do a meaningful calculation, we'd like to check our code, whether our code does a reasonable

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calculation. So we have a couple of analytical results, so we used COMMIX-2 to compare with the analytical results to see how far off -- the first data we'd like to compare is the air flow in a horizontal duct and this, as Dr. Siegel mentioned, we can very easily simulate air. We have the thermal-physical properties. All you have to do is just change the thermal-physical property and you can analyze the system with uniform heat source.

(Slide.)

Schematically, it looks like this. What we're interested in is, the pressure distribution at the center of this duct. We know what the analytical results are so we compare our calculation with the analytical result.

(Slide.)

This is the comparison. Solid line is the analytical result. The dotted line -- you see, we follow very closely, except at the tip points, our calculations are slightly off. So what all this means is that it gives us some confidence. We are doing something not too far off.

(Slide.)

Next comparison we'll be making, again is a simple case where you treat a long square duct. This inlet temperature, one atmosphere inlet velocity, again we used for heat flux. Now, we used two models. We treat continuity and momentum equations for the liquid phase and another set of continuity and momentum equations for the vapor phase. But then we have interphasal friction. If we boost the implement very largely, the two-phase flow model becomes a homogeneous model. The velocity will be the same, and if we have the interphasal heat transfer, the boost is very large and it will become a thermal equilibrium case.

So in this particular case we are deliberating for the very large interphasal friction factor and deliberately input very large interphasal heat transfer coefficient. So as a result of that calculation it should give you the equilibrium homogeneous calculation for two-phase flow.

(Slide.)

If we're doing that, then we can certainly check -- through use of the energy balance -- check our calculation. This is the calculation, the schematic view of the calculation we used in this particular run.

(Slide.)

This is a comparison between the COMMIX-2 calculation, and the theoretical means the energy balance. So again, agreement fairly good.

(Slide.)

The next one is -- since coming in the coolant is subcooled then added heat, so you gradually have a void generation and temperature situation. Again, it looks like a reasonable calculation. Now, like I mentioned before, we

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are right now in the process, in fact, to perform German seven pin two-phase flow calculation at the present time. Hopefully, next time I come here I can present the results on this.

Now, we talk about another code, so this concludes my COMMIX code presentation. The next code is the BODYFIT code.

PROF. KERR: How long does it take to make a run of the kind you just described?

DR. SHA: COMMIX-2, we don't have much statistical information, but the indication to us is very long. That's why we stopped the use developed at Los Alamos. We developed a new solution because in our estimation, if we carried on this type of solution technique, it probably is not attractive in terms of computation time. So we stopped further development, we stop that direction.

Now we try to develop a new solution technique. The indication, like I mentioned before, we're not in a position to make the claim at this time, but indications show it's quite promising.

(Slide.)

PROF. KERR: Am I correct, that as you see it the principal virtue of COMMIX-1 and 2 is that it will permit one to analyze rather carefully and accurately experiments which otherwise you feel are not very well analyzed by

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existing codes?

DR. SHA: This is just about in BODYFIT. BODYFIT, I really believe this is the only way to go in order to understand two-phase flow and to analyze the experiments. Otherwise we are so confused, you know, the sub-channel mixings, sub-channel arbitrary divided, and you don't know what's going on.

PROF. KERR: It is the BODYFIT that you feel — does that accurately —

DR. SHA: We use the boundary-fitted code in the transformation. Now, I think the work with which BODYFIT is most promising, in my opinion, most meaningful really, ought to be in order to understand two-phase flow. Otherwise, I don't believe anybody has analyzed two-phase flow.

Now, go back to COMMIX. COMMIX is essentially an advanced version of sub-channel analysis. All sub-channel analyses, like the COBRA code, they have difficulties to treat the transverse momentum equation, because sub-channel are, you know, in triangular fashion. So the transverse momentum equation, it goes three directions. So the mathematics are not right, and whether to bypass or to make the ad hoc assumptions, but we used the porous medium formulation. That removes that, and that is why I said the COMMIX code the most advanced version of the code as far as I know, in terms of sub-channel analysis.

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PRC.F. KERR: That puzzled me. You started off — this is very grave. At some point you made a transition to a porous medium. I thought you started with the Nadia-Stokes equation and you don't make any other assumptions.

DR. SHA: Now, I do not treat in the COMMIX calculation, I do not treat each pin as the internal boundary. So that's where the porous medium comes in. When you use the porous medium, some kind of averaging process comes through.

PROF. KERR: But that's a more rigorous empiricism.

DR. SHA: Compared to existing codes in that class. Then BODYFIT, for laminar flow this is a benchmark calculation, absolutely no assumptions. The first time.

PROF. KERR: What about measurement?

DR. SHA: Measurement, we've got to have, for turbulant flow, for laminar flow maybe you don't need a measurement. If we have enough, in terms of computation, enough mesh flow to resolve the boundary layers and so forth, yes, I would think that this code is better than the experiment.

Now, let's see, on the BODYFIT code, like I mentioned before, the reason to develop BODYFIT code —

(Slide.)

Again, I'd like to mention there are some limitations. BODYFIT code cannot be used for design calculations. That's impossible. Computer time you would run forever. So BODYFIT code, I see application of BODYFIT code in seven pin, nine pin recurrent maybe up to 37 pin rod bundle, possibly it can handle.

But beyond that is just not possible, til the small rod bundle. Fortunately, most of the rod bundle experiments are small rod bundles so we can analyze lots of data to understand the real physics and the purpose for the coordination formation is really -- you transform the complicated rod bundle geometry into a rectangular code and your computational grid line exactly coincides with the boundary.

So, then, you can specify whatever appropriate boundary conditions on the boundary. Without this, there is nothing you can do. Of course, everything you do you have to pay for it, too, because you're now so much more complicated. You transform the Nodia-Stokes equation physically so that you have to pay. The balance is: Now I can treat the boundary condition correctly, ut using a more complicated equation than the regular one.

So, we tried to validate our code. We have a numerical scheme in our operation. So then we'd like to establish some confidence how good this code is. Again, I tried to validate this code.

(Slide.)

We have to analytic results we can compare with the simpler geometry. We have annular pipe. We know what the velocity should be, and we have analytical results to compare our calculation. This is the annular pipe, and this is after transformation, one transformed here and two outside boundary transformed here, and this is a branch cut AA. AA is in here. So, in other words, you would just cut over here, you flip over to form like this, transformation.

Now, let's see, our calculations with analytical results.

(Slide.)

First, we compare the radial velocity region. This is inside the radius point five. This is outside the radius normalized to the one. So, velocity has to be zero at the solid wall, and you can see the situation. Analytical results is the solid line, and the calculations are the circles in here.

The next one, comparisons of the axial, the center line and velocity distribution, you see the velocity is developed. You reach a certain length. So, this is the analytical results. This is our calculation.

In here, we are not doing too good, but not bad. The reason is we should put more mesh points. We didn't put the mesh points in this particular calculation. When we redo this calculation, we add additional mesh points.

(Slide.)

The next one is in the rectangular duct. This transformation is so simple. You start with a rectangular duct, and then you transform it into here.

(Slide.)

Now, we compare the analytical results with the raw calculation. In this case, we vary with respect to ratio. A is, if we took a cross-section of the rectangular duct, A is the dimension of size, B is the length of the duct. So, we vary this ratio. We have analytical results, and the comparable calculations. Again, we vary from A over B from .25 to one, a wide range of variation. Again, the agreements are fairly good.

Now, we do the seven-pin calculation.

(Slide.)

This is the seven-pin. We actually transform it. It looks like this. You see, this is one in here, and two is outside, representing this line. And all those mesh points, the corresponding mesh points, are shown in here. So, we actually perform calculations and transform the plane.

Now I would like to point out that the results are presented here, just the week before, and Dr. Chen has started to redo his calculation. This turbulent flow involves turbulence modeling, so in there you can sometimes just do the modeling to get better results, because we don't really know what kind of turbulence. Turbulence modeling, by definition -- this is no universal turbulence model.

So, in there, we try to understand, compare the data with the raw calculation, and try to understand the physics, what's going on. So, he tells me he's right now in the process of redoing this calculation, but agreement actually is very good so far. I show you some of the agreement in this table.

(Slide.)

What I labeled the "preliminary results," and those are the actual nodes, and this is the grid. Those are grid fuel assemblies. And this raw calculation compares the measurements, predictions, compares the measurements.

DR. SIEGEL: How are the grids included in the calculation?

DR. SHA: The grids are included. You see, we do not know exactly geometry of the grid. This is preparatory information. So far we cannot get that, but we know what the resistance is. So, we model in the resistance for the grid.

DR. KELBER: Let me break in at this point. This is a very annoying part of our international exchanges that we hope to be able to negotiate out of existence; and that is, information essential to safety-related calculations is withheld because it somehow is proprietary. That is a negotiating -- I feel that is a negotiating ploy on the part of the Franco-German combine, and it is one of our key problems.

DR. SIEGEL: But the grid is assumed to do what? Is that assumed to produce complete mixing at that level?

DR. SHA: Yes. To promote the mixing, not complete. But the intention of the grid is to serve as the supporting structure for promoting turbulence.

DR. SIEGEL: You somehow include some turbulence promotion at that level.

DR. SHA: Yes. We did a model, but that's, again, you see, while we have to compare experimental data, compare with the modeling and try to understand, to improve our modeling.

So, this is the second intervention we're going around. This is the first intervention results. Not too bad, but we have lots of room for improvement.

Now, I just show you -- they probably won't add anything, but this is the sophistication of the data --

(Slide.)

-- This is actually the 3-D velocity plot. This is the seven pins. Unfortunately, the viewgraph is not too good. Those are the seven pins. You see, this is the gap between the pins. The velocity peaks up and goes down. You see, this calculation, the hot-spot factor is given you right away; you don't need additional calculations. It gives you the hot-spot factor right from this calculation.

Now, I show you another, just to refresh your memory. Cross-section BB and the cross-section CC. BB is the center of the pin, and CC passes the three pins.

(Slide.)

This is the velocity profile for BB. This is the pin right here. The velocity actually is zero at the pin edge, is the velocity profile. You can see velocity develop. The velocity profile. And this is turbulent flow. That's why it is still quite flat in between.

(Slide.)

Then, at the cross-section CC, you pass the three pins. You see those are velocity. This holds velocity to zero.

(Slid(.)

Then I show you some temperature distribution at the outlet. You can see the nearby pin is higher.

(Slide.)

Again, the temperature distribution through BB. Here is the drop.

(Slide.)

And through CC.

(Slide.)

DR. SIEGEL: Could you do this calculation if you had wire wraps?

DR. SHA: This calculation is a grid calculation. Now, wire wrap, we have difficulty because the transformation -- so far, you know, we're still investigating. So far, we cannot come up with anything to solve this problem. But because it's so complex a geometry to make that transformation.

DR. SIEGEL: Could you include a flow blockage?

DR. SHA: Oh, yes, that's very easily accomplished. Flow blockage is no problem. And the wire wrap, our current thinking in our approach to this problem, we use the resistance model. In theory, I can meke the transformation for wire wrap, in theory; but that transformation is so complicated, we just don't know how to reproduce it yet.

PROF. KERR: Maybe we should just eliminate wire wrap.

It's too complicated.

(Laughter.)

DR. SHA: Okay. Right now --

DR. SIEGEL: You say that this turbulent flow was somehow, in my naive interpretation of the pictures, it looks like laminar flow.

DR. SHA: In the laminar flow, you see the velocity profile.

PROF. KERR: He didn't show a laminar flow case.

DR. SHA: I didn't show a laminar flow case because the reason is I analyzed this data, and this is the turbine flow throughout the transient. The reason we're doing this is I have the experimental data. We cannot do the laminar flow, but the previous experimental data, the previous two calculations I showed you in the box in the annular, those are laminar. In this we've got good agreement with the analytical results compared to the calculation. So, we feel sort of confident we're doing right.

And now we move to the turbulent flow region. The reason we choose this is because we have the experimental data and also seven pin, you see. Our purpose is to demonstrate our code capability, you know, to develop model to see if it's doing the right thing and to save computer time.

Now, I think this is quite important. I show you the last viewgraph.

(Slide.)

COMMIX code has only a three-year history. It's a relative newcomer to the field. We already have many requests and many have actually used our code. We actually interact, for instance, with Westinghouse for use of our code for design, but they're interested, like, AI also has a code.

PROF. KERR: You don't feel uneasy knowing that people are using your code for design?

DR. SHA: Oh, no. We would encourage them. The reason is we'd get a tremendous benefit out from them. They may point out a certain direction we may overlook. Sometimes they can debug our code. So, we are very much in favor. We press it through NRC approval, and we send it to them right away.

Those installations have our code, and many of them have actually used our code. And I think the spinoff benefit of this program --

(Slide.)

-- Is really tremendous, since we started NRC program to develop the COMMIX code. Now, the DOE actually picked up this code and applied it to different areas. We actually have now developed the COMMIX IHX code. This is really the first time we tackled fast breeder IHX three-dimensional temperature, three-dimensional velocity.

This code has actually been developed, if anybody requests this. We are working on the SG code. And we also now use this code as a base to working in the solar energy area. We have designed the thermocline storage tank, and now they want to promote stratification.

Now, I forgot to mention there is another spinoff from the benefit from this code. Recently, EPRI asked us to give them additional work in the BODYFIT area, to analyze the PWR fuel assembly. Again, they're experimental data, I forgot to mention here.

And that concludes our presentation. Thank you.

PROF. KERR: Further questions?

(No response.)

PROF. KERR: This would seem to me to be a convenient time for a lunch break. Is there disagreement with that conclusion?

(No response.)

PROF. KERR: We will reconvene at 1:30.

(Whereupon, at 12:30 p.m., the meeting was recessed for lunch, to reconvene at 1:30 p.m., this same day.)

end#10

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AFTERNOON SESSION

(1:30 p.m.)

PROF. KERR: Mr. Kelber, may we proceed at this point?

DR. KELBER: Okay. As you can see from the viewgraph package before you, we do have an experimental program in thermohydraulics directed toward certain LMFBR problems at Brookhaven. And it's a small program, but we think it's producing some very useful insights.

To describe it today, Ted Ginsberg is going to make a brief presentation.

MR.GINSBERG: Thank you.

(Slide.)

I'd like to describe the program at Brookhaven national labs directed primarily toward experimental simulations related at the moment towards physical mechanisms involved in core disruptive accidents. Following a brief overview in which I'll indicate the scope of our work, I'd like to discuss with you several of the programs and then summarize some of our findings and accomplishments.

(Slide.)

In general, the objectives of our work are to investigate various thermohydraulic phenomena which are of importance in LMFBR safety analyses and to develop data useful for co-evaluation.

In general, the work that we do at Brookhaven proceeds as follows. This is the way that things have developed historically. We identify a CDA phenomenon which we think is important. We then do some preliminary analytical modelling of the phenomenon.

Using that modelling, we try to develop scaling relationships with which we can take a simulation experiment and extrapolate to a prototypic system.

We then set up our experimental model or experimental simulation with which we acquire our data, see how well it compares with our model. After we have a model that seems to agree reasonably with the data, we can then use our model to predict what's happening in the protypic circumstance.

PROF. KERR: Mr. Ginsberg, I could conclude that you have begun this investigation de nova, and I sort of doubt that. It must have some relationship to other CDA work.

MR. GINSBERG: I'm sorry, I didn't catch the beginning of your question.

PROF. KERR: I say that I could conclude from your opening remarks that you had begun this investigation de nova, and I don't believe that that's really true. The phenomenon at which you're looking must have some relationship to the phenomenon being investigated by others. You must be trying to fill in gaps. You didn't just sit down and look at the CDA and say what phenomena need investigating.

That's sort of what it sounded as if you were doing.

MR. GINSBERG: That's correct. But when I say that we identified phenomena of interest in CDA analyses, this identification is not done in isolation from the rest of the LMFBR community.

PROF. KERR: So you must have picked some or had some assigned to you. Could you give me a little bit better insight -- maybe you're going to do that, and if so, I will wait, on how your work fits into that being done by others.

MR. WRIGHT: A little historical background before Ted arrives on the project.

Brookhaven started out about four years ago primarily concerned with heat transfer problems and post-accident heat removal in convection, natural convection pools, boiling pools. And they applied -- they were essentially working on two-phase heat transfer, two-phase flow heat transfer and an extension, then, into the transition phase phenomena about the time Ted got on the scene was a natural development.

So they, under our direction, moved into areas where their competence fit our needs. They didn't define the problems from scratch.

MR. GINSBERG: I can be more specific now with this slide.

(Slide.)

This slide presents the scope of the programs currently extant at Brookhaven. It's a varied program. Experiments involve mechanisms in the transition phase of the loss of flow accident, post-accident heat removal.

In addition, we are starting up an experiment related to accident energetics. What Bob was referring to was the fact that the program did get started at Brookhaven with this program, which basically asks the question, how is energy transferred from a boiling pool system which is confined in some structure to the bounding walls of that system?

It was a post-accident heat removal problem at the time related to the survivability of retaining structures, which are holding boiling molten pools. The program began with this and it began with this particular experiment because it was related at the time to a problem that came out of licensing efforts; in particular, pseudo-licensing, FFTF.

At the moment, the way things have developed, this boiling pool information is also useful for characterizing certain aspects of the transition phase.

The other program that was begun roughly the same time was the multi-phase fuel relocation and freezing dynamics problem, which basically asks how does hot molten fuel flow through a coal duct structure?

That also was applied to basic issues involved in licensing.

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PROF. KERR: If I am not mistaken, Argonne National Laboratory has also done some experiments related to the transition phase volume. Is there some identifiable relationship between what you're doing and what they're doing?

Are you, in effect, doing work that gives you an independent look at some of the problems on which they have worked, or can one comment on how this is related to that?

MR. GINSBERG: Yes, there definitely is a relationship The relationship comes about in the following way. Their experiments primarily are protypic material experiments and we believe, as such, provides them with a relatively limited scope of tools available for acquiring data.

The experiments that we're doing are with similar materials. It allows us to use what we feel are more detailed instrumentation using similar materials to attack fundamentally similar systems.

I can be more specific as we get to these various items. I can make similar remarks about the other issues, and if you'd like to we can go into that in more detail.

PROF. KERR: As you discuss the various items, I think I'd sort of expect that it would be helpful.

MR. GINSBERG: Okay, fine.

(Slide.)

So these are the programs that we are currently involved with. Core dispersion investigations relate to the question of recriticality during the transition phase. It regards issues regarding fuel compaction and the ability of the steel vapor to keep molten fuel from collapsing on itself.

This multi-phase fuel relocation again is a transition phase-related issuc involving recriticality potential. The potential of reactivity loss of molten fuel as it flows out of the core region. It also involves considerations as to whether or not you would form a bottle coolant during this phase of the transient.

PROF. KERR: In line with your earlier remarks, simulation phase is applied to a multi-locational phase freezing dynamics problem. It also means that you're using simulant materials rather than fuel and, say, sodium.

MR. GINSBERG: Absolutely. That's correct. If you hold on just one moment, I'll give you a typical example of how one program in which from conception to application to the prototypic case —

This, again, this program, heat transfer and internally heated boiling pools was initially a post-accident heat removal-related issue. Now we believe that it has application to the transition phase. We have a program related to HCDA energetics and to the potential entrainment of sodium into the expanding bubble by the mechanism of Taylor instability.

Finally, we are at the moment heavily involved in what we call a transition phase assessment task, in which we are trying to assess what's been done with respect to phenomenology in the transition phase and to put the issues into perspective, how do these various tasks or how does existing information fit into the overall picture of the transition phase accident as it's hypothesized to develop?

So here we're trying to identify key safety issues.

PROF. KERR: Is that primarily a look at experiments that bear on the transition phase or experiments in analysis?

MR. GINSBERG: The assessment includes a look at experiments and analytical models which are used to characterize those experiments.

What we are not doing in this assessment is looking at whole core accident codes. We are not looking at that sort of accident analysis tool. We're looking at phenomenology and the models of those phenomena.

Does that answer your question?

PROF. KERR: I guess it does. But I am puzzled that one is restricting oneself not to looking at whole core accidents codes since I had thought that one of the principal purposes of some of these was to attack the transition phase.

MR. GINSBERG: What we're looking at are perhaps individual models which are in those accident analyses.

PROF. KERR: All right. You're not looking at the

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total part. You are looking at that part of the code that might be in the transition phase.

MR. GINSBERG: That's right. There might be a model describing the flow of molten material through the core. Well, we can take a look at what the model is and see whether it's sound, physically sound.

That's the approach that we're taking.

PROF. KERR: I think I understand the distinction. Thank you.

(Slide.)

MR. GINSBERG: The purpose of our core dispersion investigations are to characterize the flow dynamics of fuel steel boiling pools and to evaluate the impact of these flow dynamics on transition phase recriticality potential.

We have broken these investigations down into two parts. First, a series of adiabatic simulation experiments in which we are using different fluid pair combinations to look at the flow dynamics of two-phased mixtures. I'll describe those experiments in a moment.

Second, we are also involved in a series of volume-heated dispersion experiments with simulant fluids and also simulant heat sources.

So we're looking at two aspects of the problem, the hydrodynamic aspects and the thermohydrodynamic aspects.

(Slide.)

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With respect to the hydrodynamic aspects, we have two tasks. What we eventually would like to do is inject gas in the heavy liquids in a volume-distributed fashion to simulate the boiling process and ask, can we characterize the flow behavior of, for example, mercury air systems and we'll try to relate the behavior of mercury air systems to systems such as molten fuel with steel vapor bubbling through it.

We'll try to make that identification.

The second task arose out of the first. As a result of designing the volume-distributed gas injection tests, certain basic issues came up and we decided that we had to do some preliminary tests to see whether, indeed, it was worth going on to our concept of distributed gas injection.

Because our time is limited, I will in a moment describe these preliminary feasibility tests because it does provide some useful insights into the flow dynamics of volume heated boiling pools.

The objectives here are to understand multi-phase flow dynamics and flow system transitions in fluid systems which we believe are protypic in some sense.

Next, dispersion in internally heated boiling pools. We presented to you last time the results of an investigation using an electrically heated facility. This test program is complete and we have focused this information and the ih

information has been used outside our laboratory.

Next in the series of tests we have planned are the microwave heated high power dispersion tests. I'll describe the rationale of those tests in a moment.

So we feel that we have a coupled program directed toward the thermo and hydrodynamic aspects of boiling pool behavior.

I'd like to now skip the next couple of slides -- they're in your handout -- and go on to show you --

(Slide.)

-- the apparatus that we're using to do our simulation experiments.

In particular, the series of experiments which are preliminary to actually getting into our volume injection system. What you see here is a test rig, a vertical test rig which contains a porous plate at this level through which we inject gas into an overlying layer of liquid.

So we can control the flow through this porous plate. Bubbles then come up through the bottom of this and flow up through this layer of liquid and we try to understand the flow dynamics of this sort of system and apply it to a prototypic boiling pool environment.

This isn't the complete story of these investigations but it's one phase of them. The instrumentation that we have on here is the following. We have pressure taps along

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the axis of this test column. We have the capability of inserting a gamma source here and detector here and make void fraction measurements and we'll get void fraction distributions along the axis and along the transverse dimension.

What you see here is a transversing system capable of moving the source and detector system both axially and transversely.

So we'll be looking at void distributions. What we've done so far is to make static pressure measurements and to use those measurements to interpret what the average void fraction is, or the average void characteristics are.

What you're seeing here are the following: This curve presents the void fraction versus the vapor velocity, the dimensionless vapor velocity in proportion to the volume flux of air bubbling through the system.

Here we have a plot of pressure distribution, pressure around the height of the column.

What you're seeing here is a linear variation of pressure with distance indicating that the pressure is due to the static head of liquid only.

When we take the slope of these characteristic pressure versus distance, we can obtain what the void fraction is.

This is presented up here. This is the void fraction

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versus vapor velocity proportional to the volume flux of vapor.

What one finds is that, initially, at low vapor flux, the data increases — the void fraction increases rather sharply with the vapor volume fraction. And one notices, visually, a bubbly flow regime, something that you would characterize as bubbly flow.

However, you reach a point where the nature of the flow changes. Bubbles agglomerate, become larger, and you get an abrupt change of flow regime together with an abrupt drop in void fraction.

This type of behavior has been observed before in this type of experiment. We also have obtained similar results with electrically heated tests.

What this means to us is that one has to appropriately characterize what the flow regime is and what the coupling is between the vapor and the liquid connected to that flow regime in order to adequately characterize the fuel vapor distribution.

PROF. KERR: The gas and the fluid used in this experiment have been —

MR. GINSBERG: This is water air. We are going to use mercury air later on to mark up the liquid vapor density ratio more closely.

So these are preliminary test results.

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(Slide.)

On these hydrodynamic tests, what we found is that the Churn flow regime is stable through at least three times the Kutateladze limit.

Those of you who are involved in the process recognize this Kutateladze limit. It describes the notion that a boiling steel fuel pool indicate heating conditions is going to be a dispersed fuel pool.

We've shown this with our electrically heated results and it's verified with this experience. It's that this proposed Kutateladze limit really doesn't apply. It doesn't apply to up to three times the Kutateladze limit in this experiment and our electrically heated tests showed that it didn't supply up to roughly five times the Kutateladze limit.

It shows that the flow regime was more stable than was previously thought. I think this is now recognized in the community, although there are some experiments which we believe were begun at Argonne in order to verify the type of experimental results that we've obtained.

In addition, we find the bubbly flow is less stable than in electrically heated tests. The breakdown from bubbly into churn occurs at a low vapor volume flux. It did in our electrically heated tests.

We're not sure yet why that is, although it may be due to some surface tension effects.

DR. SIEGEL: Is there any qualitative definition of what churn flow is?

MR. GINSBERG: Churn flow is characterized by an oscillating, chugging flow behavior in which you cannot really identify individual bubbles.

It's a much more random-type of liquid vapor flow system than bubbly flow, where you see individual bubbles. If you can imagine a dense packing of bubbles breaking down into larger bubbles and those larger bubbles then become chugging. They start to chug and the column starts moving very dynamically.

This has been identified with a churn flow regime.

One of the things I was trying to do that we will do, one of the objectives of the program is to identify flow regimes. One of our tasks is to use this gamma system to perhaps put a quantitative estimate on when the flow regime changes occur.

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The implications are, we believe, that the dispersed flow regime is more limited than it is in the literature and there are bubble stability effects on this boiler potential.

PROF. KERR: What do you mean by the dispersion regime being more limited?

MR. GINSBERG: Going back for a moment to this curve, if we look at the void fraction versus the quantity proportional to the volume flux, if you use the Kutateladze criteria, which supposedly telly you when the churn flow regime breaks down into the dispersed regime, you'd find that it would tell you that that transition should take place further down here -- let's say down around this region.

We have operated up to here in the churn flow regime.

We conclude from this, and we also have data from electrically heated sysems, that the churn flow regime persists to higher values of vapor volume flux than predicted by the Kutateladze limit.

In that sense, it's more persistent in terms of this volume flux parameter.

MR. KASLENBERG: Could you take it one step further, Ted, and tell us what it implies in terms of recriticality, in terms of the licensing or safety point of view?

MR. GINSBERG: Thank you. I'll do that.

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Let's imagine that we go from bubbly flow into churn flow and then into dispersed flow.

(Slide.)

Suppose we go from bubbly flow to churn flow to dispersed flow. It turns out that in bubbly flow, the coupling between the bubbles and the liquid is very tight. They tend to move together. It tends to want to make the system more dispersed; whereas in churn flow, the vapor can slip by the liquid much more easily.

So if you put it in those terms, it can't pool the liquid up with it.

DR. SIEGEL: It cannot?

MR. GINSBERG: It cannot. It's less easy in the churn flow situation than in the bubbly flow situation.

Okay, now when you go from churn to dispersed, we don't have any of this data yet in the dispersed flow regime. If you use previously existing models, slip models for the dispersed regime, they'll lead you to the conclusion that when you go from churn flow to dispersed flow, you want to go to a more compact situation. The vapor slips even more easily by the droplets and you would expect that the fuel vapor distribution in that case will be even more compact than the churn flow or the bubbly flow.

So as you go from bubbly to churn to dispersed, it would appear that your potential to maintain a low fuel density decreases; therefore, your potential for recriticality, it would appear from these types of arguments, would be more highly probable as you go into the dispersed regime.

It's a [illegible]tion of coupling between vapor and liquid. In the bubbly flow regime, the coupling is very tight. In the dispersed flow regime, it's very loose. Therefore, the dispersion characteristics are different. And you can see that on this plot where I'm showing what I call a Pool average void fraction.

And the pool average void fraction is merely, imagine that the liquid level without boiling is roughly up to here. Once you start it bubbling, if the level would go up to twice its initial height, the void fraction would be 50 percent.

So if you use that as a definition of average void fraction, then you find the following.

Here we have our data, and I don't want to go into detail at the moment, but what we do get from this is based upon calculational results, based on models which we've used to predict our data, what we find out from the churn turbulent flow regime, the pool average void fraction for a given volume flux is much lower than it is for bubbly flow.

This means that bubbly flow is more dispersed. It tends to spread itself out more readily than does the churn flow regime. And if I were to plot the same sort of situation

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for dispersed flow, you would get a curve. That's down in this region.

So bubbly flow does tend to give you a more dispersed fuel configuration.

Now I should point out that during the CRBR licensing process, their arguments were completely backwards. The implication was from their arguments that the dispersed flow regime was the flow regime which gave you the least fuel density.

It turns out that that was completely wrong. I think that's recognized now.

PROF. KERR: You think it's recognized, but you aren't sure.

MR. GINSBERG: There hasn't been much done. There is one paper which was presented at the ASME meeting in Atlanta.

PROF. KERR: If your data are valid and are extrapolatable to the core materials, there wouldn't seem to be any doubt.

MR. GINSBERG: The question is what's admitted to in the published literature. And in the published literature there is one article which does finally seem to put the story straight.

I can discuss that article with you later if you're interested.

So I believe the work that we're doing has had an

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impact on the community.

(Slide.)

I'd like now to jump to our program involving dispersion in internally heated boiling pools in which we have done some electrical heating tests in both large aspect ratio geometry and also small aspect ratio geometries.

And we have planned a series of microwave heater tests for a reason that I'll describe in a moment.

(Slide.)

First, just for a brief moment, I'll refer to -- what I have here is a description or a picture of an apparatus which we've used in the past to study the dispersion characteristics of volume-heated pools. It's an electrically heated system, two electrodes penetrating a vessel which is filled with a conducting fluid.

We apply power to the system, and effectively, these initial tests that we did with this, which we have reported in the literature, in these tests we observed photographically what the flow regimes looked like. We also obtained a measure of the pool average void fraction, again, defined by the relative height, non-boiling heights.

PROF. KERR: Is this direct current electricity?

MR. GINSBERG: This was AC.

PROF. KERR: At what frequency?

MR. GINSBERG: This was 60 cycle.

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PROF. KERR: Thank you.

MR. GINSBERG: So we did study the boiling characteristics of this type of system and my purpose now is not to discuss these experiments with you again in detail.

What I would like to show you is how we used this experiment, how we used this simulation experiment together with various assumptions and how we've applied the results to productivity materials.

(Slide.)

Basically, the question I wanted to address is what's the potential for dispersal of boiling pools of fuel and steel, and how do we use this data to make that prediction? If we assume that the assumptions are the following, that the steel is uniformly mixed with the fuel and at saturation temperature, the vapor production is given by this dimension-less vapor flux which we saw briefly beforehand, that the pool is open at the top and that one-dimensional drift flux modelling applies.

We used one-dimensional drift flux modelling to describe the experiments.

What we find from these assumptions is that we end up with what we feel are two scaling parameters. This dimensionless volume flux proportionate to the power density of the fuel and another dimensionless vapor flux which characterizes the transition from a churn flow to a dispersed flow regime.

PROF. KERR: Now do you have evidence that those assumptions make any sense, or do you just not know what else to assume?

MR. GINSBERG: At this point, we don't have any evidence. In the first, for example, we don't have any evidence regarding the separation of steel and fuel. What we do have is information which tells us, for example, that in the churn turbulent regime, we expect a very dynamic mixing situation and perhaps it's reasonable to say that the fuel and steel are homogenously mixed.

We don't have any direct evidence that that's a valid argument.

And one would like to, in the future, address that issue. What's the effect of a second component? There are experiments which are currently being developed elsewhere to address the two-component issue.

PROF. KERR: Okay. I think you're saying that there is some information that leads you to believe that these assumptions are perhaps reasonable. But there is a certain amount of ambiguity.

MR. GINSBERG: Yes, there certainly is.

(Slide.)

MR. GINSBERG: In the next slide, just looking at these two parameters again, we asked, well, what are the prototypic conditions and what are the conditions of our experiment, present water experiments and prototypic single assemble and core-wide molten UO2 steel pools?

First of all, our present water experiments are admittedly single component experiments. In the prototypic situation, they are mutli-component and multi-phase.

So one has to bear that in mind.

Now these two scaling parameters which we derived from our analysis, in our experiments the value of these parameters range from zero to 19.

PROF. KERR: Somehow I got lost. I thought that we were talking earlier about air water experiments. We're not any longer.

MR. GINSBERG: The slide that I put up previously was the electrically heated pool system.

PROF. KERR: Okay, and water is always solid. No vapor.

MR. GINSBERG: That's a boiling system. We're talking about in this slide here —

PROF. KERR: I recognize that you have vapor if it's a boiling system. But you aren't talking about a boiling system.

MR. GINSBERG: Yes, we are, volume boiling system.

PROF. KERR: A single component means it's all water. But some of it can be liquid and some vapor.

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MR. GINSBERG: Yes, correct.

PROF. KERR: Thank you.

MR. GINSBERG: In our experiments, the effect of power density was in this range of nominal fuel power density and the dimension-less velocity ranged from zero to 19 and the other parameter ranged from zero to roughly 5.

In the prototypic circumstance, at one percent power, the value of this parameter was 10.6 and at the upper limit of decay heating, it was 85. Whereas, the value of the second parameter ranged up to roughly 4.5 for 6 percent decay heating for single assembly-sized molten pools; actually, both single assembly and core-wide.

Well, the way we apply our information is as follows. We note that in terms of this parameter, our experimental range up to 19 is roughly up to 2 or 3 percent of the prototypic value. And we would say that, strictly speaking, if we have to apply our information beyond 2 or 3 percent, then it's an extrapolation and we have to make that extrapolation.

Whereas, in terms of the second parameter, we cover the entire range through decay heat.

(Slide.)

So we then use the model that we developed to predict the data. It's actually a drift flux model. It wasn't of our invention. And we say that at 1 percent power, applied to prototypic transition phase conditions, the dimension-less velocity is 10.6 and this quantity is .56. And our data lie within this range of dimensionless parameters and we can make a prediction based upon this model. And based upon our information, we say that the flow regime is churn turbulent and that the pool average void fraction is in this range, .6 to .7.

Whereas, if we have to apply this model to 8 percent decay heating, it's out of the range of our experiment, but we can yet extrapolate. And we have to make this calculation, recognizing that it is an extrapolation and we need data with higher values of power in order for our experiment to bound the prototypic circumstance.

PROF. KERR: When I talk about power density here, I'm talking about so many kilowatts per CC, for example. And when I talk about percent of LMFBR, I'm just using the numbers directly. There isn't any scaling involved there.

MR. GINSBERG: That's right, no scaling. But it turns out that they're included.

PROF. KERR: In whatever scaling is necessary.

MR. GINSBERG: It's in that parameter.

MR. KASLENBERG: When you're extrapolating at 8 percent power, you're a factor of a little over 4 between, on your first dimension-less parameter up to 85, from 19 to 85. That's a little over a factor of 4. Would you say that

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with that factor, the void fraction is on the conservative side?

Could you say which side you are in your average void fraction? Do you think you're overestimating or underestimating?

How good would you say that that number is?

(Slide.)

MR. GINSBERG: The predictions were based upon these curves. So our experiments run roughly up to here nd we're trying to make a prediction up around this range.

This turns out that this model shallows out quite a bit. If you were to extend this model over horizontally, you would find that it shallows out quite a bit.

Therefore, I would say that it appears to be a reasonable extrapolation if no flow regime transition occurred

You can perhaps see that from here. This does shallow out significantly.

So the extrapolation from here to here, I believe, seems to be reasonable.

This parameter is roughly up to 20 in our experiments and we have to experiment up to roughly 35. So based upon that model, I believe the extrapolation would not be unreasonable.

(Slide.)

It appears that the dispersal potential by boiling mn

steel is significant. If you're generating the vapor, the Vapor seems to have a potential for keeping this molten fuel boiled up.

The basic issue becomes how much vapor can you generate? That's a question that's tied to the global thermodynamics of the system.

We've had to extrapolate to higher power densities than, strictly speaking, our data allows and we would, therefore, propose that experiments be undertaken at higher power densities in order for us to be more appropriately bracket the system.

Now that exercise that I just described is what I feel is typical of the way that we see things at Brookhaven and we try to apply this sort of reasoning, which we won't be able to go into now, with the various other experiments.

But we do try to do that in each one of our experiments. We recognize the limitations of our experiments and we state what the assumptions are and we do our extrapolations.

(Slide.)

I would like just to make because time is running off on us, to make brief mention in the next of this series of experiments that we have planned related to the dispersion in volume heated boiling pools.

This arises out of our previous conclusion in the previous slide, at least in part, that we want information at higher power densities, at higher volume fluxes than we've obtained.

We would like to be able to go further up the power density scale in order to be able to more reasonably make our predictions under higher power density circumstances.

Well, our previously electrically heated tests have their limitations in terms of being able to apply power them.

It turns out that in an electrically heated system, as you boil up the system and you start to produce droplets, you can't heat those droplets any more because they're separated from the electrodes.

So we need an energy source in which we can remotely, if you will apply the energy to anything that's unconnected in this electrical system. We need a remote source of energy, if you will. And for this, we're looking to microwaves, the application of microwave heating to the simulation of volume-heated boiling pools.

In particular, we're looking to develop information useful for dispersed flow regimes. We're interested in closed pools, in pools with boundary heat losses in systems where the liquid is recirculating in the vessel.

We ran into several basic problems in trying to apply microwaves to these boiling pool studies. These involve the physics of coupling of microwaves to liquids.

Problems involving coupling of the energy source through dispersed flows, problems involving being able to predict the variation of power density in your sample.

These were more or less complex issues relating to the interaction of microwave radiation with liquids of varying kinds of geometries.

Well, we don't have time to discuss this in detail. I would like to say, however, that at the moment, we do have a conceptual design. We've studied the interaction between microwaves and droplets and we believe we have a solution to that. I will describe that very briefly in a moment.

We've done some preliminary modelling and we're prepared to go from our microwave system, and this after a considerable amount of work studying the interaction of microwaves with two-phased fluids.

(Slide.)

Basically, what we were looking for were two items. First, to be able to have a predictable and/or measurable power density distribution within our sample, within our boiling fluid.

Previous experiments, in our opinion, did not have this property, so we did a considerable amount of study, a considerable amount of interaction with vendors and we concluded that microwave oven irradiations are not the way to go.

Doing this experiment in a system like your home oven is not the way to go.

We've concluded in connection with the vendors that we've been talking with to use a system which they call a single mode applicator.

Another way of applying the energy to the system in which what you get is more calculable than will reasonable characterize your power density distribution in this type of environment.

PROF.KERR: Does that assume that power density is independent of the loading in the cavity?

MR. GINSBERG: It doesn't assume that, no. What we are after, we will know how much — one quantity which we'll always know is how much energy is being applied to the system.

PROF. KERR: What you'd like to get ideally, I would think, is the uniform power density in the liquid region.

MR. GINSBERG: That's correct.

PROF. KERR: You're convinced you could get that independently of what the droplet size distribution is, or at least a good approximation of that?

MR. GINSBERG: I believe we can get a much more reasonable approximation of that than in a comparable water system. We have a fluid system chosen to give us a uniform power density independent of droplet size.

PROF. KERR: Do you have some way of telling whether you're really getting it or not? Or are you depending on calculations?

MR. GINSBERG: Well, there are two issues. One of them involving coupling to the droplets, is the power density distribution is independent of droplet size, one issue. The other issue is: Is the power density issue independent of space? Okay? With respect to the former question, we're going on calculation. Is the power density distribution independent of droplet size. We don't think we have a way of determining that. But we do have extensive calculational studies to indicate that we are in the right direction.

With respect to the second issue on power density distribution, we feel we're going to have a way of measuring what the power density distribution is.

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PROF. KERR: So, it's quite a problem.

MR. GINSBERG: Yes, it is a complex one, one which we feel we can understand right now.

And the second issue which you addressed was the distribution of power with respect to droplet size, which I called here. We wanted the system in which the power was coupled to the dispersed flows independent of drop size. We concluded here, after much study, if we go to low index of refraction fluids, we can achieve that.

PROF. KERR: Does that, in effect, mean that if you don't have very much coupling, you don't distort the field very much?

(Slide.)

MR. GINSBERG: The distortion of the field is a second issue. It means --

PROF. KERR: I mean, if you have a uniform distributed electromagnetic field and you start loading it, it seems to me the low index of refraction material is less likely to produce high local loading than a high one, so, you don't get much field distortion. That's all I am interested in.

MR. GINSBERG: What will happen in that circumstance is when you have a low index of refraction material, your exponential decay is not great, your absorption is not great, but your incident fluxes, given a microwave power input, your incident energy influxes will increase to compensate for the small attenuation factor. But as far as distribution is concerned, we are looking to try to measure it. And we feel that's definitely an advance over existing technology and existing experiments.

This, again, gives a schematic of that proposed experiment. Here we have a microwave cavity which separates the whirl from the microwaves. Here is the test vessel which is appropriately again shielded from the environment. This is a loop to catch the condensation and bring it back.

I would like to show you one more experiment which we are currently involved with and actually ending up.

(Slide.)

This is our experiment involved with heat transfer and internally heated boiling pools. This is the exerpiment I had alluded to earlier, which was begun due to a post-accident heat removal issue. What's the rate of heat transfer from a boiling molten pool to surrounding structure.

This is also relevant to the transitional phase as well as to post-accident heat removal. A preliminary report has been issued. This work is being wound up. The purpose was to develop correlations and models to describe the heat loss characteristics of such boiling pool systems, and the information is applicable to both PAHR and transition phase.

(Slide.)

If you will bear with me, the next slide you do not have in your slide pack. So, let me just point out what's going on here.

This is a schematic of the boiling pool apparatus looked at from the top, this from the side. Here we have two electrodes applying neergy to the electrolytic fluid which is brought to boiling, which is brought to the steady-state boiling condition. We have a test plate comprised of a series of pairs of thermocouples along the length of this test panel, in which we measure the rate of heat transfer from this boiling system to that test wall as a function of position.

The temperature information is then converted to heat flux information, and that heat flux information is then converted into numbers, and we come up with heat transfer correlations, both on a local basis and averaged over the length of the pool.

What we show here are, first of all, the behavior of the heat transfer coefficient as a function of depth measured from the plate down. First of all, notice that in this experiment there is more than a factor of two variation in this heat transfer coefficient as a function of distance down that pool, here to here. That's one fundamental finding of this experimental investigation, that there is this behavioral wall.

Now, this experiment was also designed to enable one to vary the angle of inclination of that wall and to measure the effect of that angle of inclination on the heat transfer

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mechanisms. The reason for this was that not all confining walls are vertical. Some confining walls have curvature. So we approached this problem via angle of inclination.

Here you have data plotted as a function of the Nussel number, as a function of teh Grassof number times the Pranel number. This correlation is dimensionless parameters over a range of inclinations and over a portion of the range of boiling flow regimes. Both of these correlations are boundary layer types of correlations.

What we've concluded from this analysis is that the basic mechanism is a boundary layered heat transfer mechanism at laminar and turbulent, and that the effect of the angle of the inclination which is taken into account by the Grasoff number is very simply a question of the direction of gravity relative to the test wall. So, it's a natural convection type of mechanism, and that assumption successfully correlates the data.

(Slide.)

In addition, in this experiment the pool average void fraction was measured. Again, defined in the way I described earlier in terms of the height of the pool. What one finds in terms of the void fraction is a function, again, of this volumetric vapor flux. For bubbly flows, the void fraction increases, and you go into a transition to churn turbulence, and then it collapses again in void fraction as it goes into a

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churn turbulent regime. This corroborates the previous comments I made earlier on the hydrodynamic tests.

So, flow regimes are indeed significant, both for flow properties and, as it turns out, there is experimental data from the heat transfer correlation points of view.

(Slide.)

Very briefly, the conclusions are that the heat transfer characteristics of volume boiling pools depend upon the flow regime, the heat transfer characteristics in the churn turbulent flow regime, by roughly a factor of two above the bubbly flow correlations.

We found that the mechaism of heat transfer and boundary layer flow and bubbly flow regime is a natural convection mechanism. We found that in chrun turbulent flow this is probably not two. The boundary layer appears to be disrupted. As a result, the heat transfer is a factor of two higher than the bubbly flow regime flow.

So, one has to be careful to identify from the heat transfer characteristics of these types of pools to identify what the flow is and to make sure that your correlations are defined to account for that.

Since time is running short, let me skip a couple of our existing programs. I hope what I have said gives you a feel for our approach, but since we do have such a diverse program, I just can't go into all these in detail. So, let's go to a summary of our accomplishments.

(Slide.)

And the results that we've obtained. I said earlier -- I mentioned earlier that one conclusion from our dispersion investigations is that the flow regime under decay heating conditions is likely to be churn turbulence and not dispersed. We also discussed earlier the implication with respect to recriticality potential. We isolated this empirical observation that foam persistence is unresolved. In some cases, the bubbly flow regime was unaccountably stable and led to the existence of a foam regime.

PROF. KERR: What is "foam persistence"? Does that refer to the foaming pool?

MR. GINSBERG: Yes, it is. It is related to that. What we found in some of our experiments, in which the fluid was visually contaminated -- that's our experiments -- that the bubbly flow regime did not collapse to the churn turbulent regime.

PROF. KERR: I don't understand what "visually contaminated" means.

MR. GINSBERG: There were particulates floating in the system that weren't supposed to be there.

PROF. KERR: You mean wood?

MR. GINSBERG: Little pieces of what was apparently corrosion from the electrodes.

PROF. KERR: Oh, okay.

MR. GINSBERG: Under those conditions and under those conditions only, was the bubbly flow regime unusually stable with respect to that volume flux parameter. We were able to maintain a bubbly flow regime without collapsing the churn turbulence to the high values of volume flux. That's what I mean by "unusual persistence." What we found was eventually it would collapse to a churn turbulent flow regime.

Henry's data seems to indicate a much more persistent foam, to much higher vapor volume fluxes. If you would like, I can go into that in a little bit more detail. I do have a slide which does show --

PROF. KERR: I mostly wanted to know what "foam" was.

MR. GINSBERG: Very persistent bubbly flow. It's a dense packing of bubbles which display void fractions in excess of 70 or 80 percent. That is what is called "foam." A very tight packing of bubbles.

PROF. KERR: You've concluded if you want foam to persist, the experiment should be dirty?

MR. GINSBERG: That's one way of looking at it, but it's also something that we isolated as a problem. We really don't know the conditions for foam development. Neither does anybody, in the literature.

MR. KASLENBERG: Don't you need a surfactant for foam and when you have impurities in there which acts as a refractant, which helps to give you the foam?

MR. GINSBERG: I believe that that's true.

MR. KASLENBERG: I thought people accept that generally, people who manufacture detergents and shaving cream.

MR. GINSBERG: I believe that that's true. Argonne has isolated another factor, the question of nucleation site availability, number of nucleation sites.

PROF. KERR: That's another way of saying "dirt," isn't it?

MR. GINSBERG: Not exactly.

PROF. KERR: "Nucleation sites" is more sophisticated. I will withdraw the comment.

MR. GINSBERG: We can discuss it afterwards, if you like.

We have concluded from the dispersion work that if the vapor is available, then that vapor is sufficient in keeping the molten fluid boided up. Our boiling pool heat transfer studies have indicated that in the bubbly flow regime the heat transfer appears to be accounted for by natural convection ideas or enhanced natural convection ideas. And the effect of the inclined wall is merely the effect of the gravity vector relative to the angle of inclination of the plate.

Those two assumptions appear to correlate the data reasonably well.

In the churn flow regime, however, the boundary layer theory and the ideas of natural convection in boundary layer flow does not appear correct. It appears as if the boundary layer is disrupted and the heat transfer is consequently a factor of two higher. That data, the analysis of that data, is incomplete, and we will be presenting that data later on.

The transition from bubbly to churn turbulent is marked by a sharp reduction in void fraction, and we discussed that earlier.

(Slide.)

The other programs which we have not discussed with you today are the multi-phase fuel relocation experiments, and a series of an analytical program at the moment which we hope will develop into an experimental program related to HCDA energetics.

Nevertheless, our conclusions with respect to the work that we've done on multi-phase fuel relocation lead us -- our work leads us to the conclusion -- and I hope this won't be out of context -- but if during this process of flow of molten fuel material from structure, if frozen crusts exists in both the single- and multi-phase freezing appears to be conduction controlled through that crust, then the extent of relocation of molten material depends upon the available pressure drop. It appears to us, therefore, that the issues of crust stability and wall ablation appear decisive and need to be further investigated.

We are currently investigating various stages of two-phase HCDA bubble growth and the impact of Taylor instabilities and sodium entrainment on these various stages of bubble growth.

(Slide.)

Very briefly, our directions, our dispersal work is going to be directed in the near future toward understanding the void distributions as well as average void behavior. We are looking to mercury air experiments to get us some information about ultimate fluid properties. And our microwave tests which we are going to study in both open and closed configurations and with boundary heat losses.

The boiling pool heat transfer work is being wound up. We are completing our data analysis, and we still have some correlations to develop, void distributions to correlate, and our churn flow data to analyze. We're going to be extending our multi-phase fuel relocation work to ablating walls to freezing the flowing fluids with ablating walls; our HCDA energetics work is just under way. And we are currently involved with the transition phase assessment which the ACRS will receive as soon as it's available.

Thank you for attention. Are there any questions?

PROF. KERR: Are there questions?

What questions should we have asked you that we missed?

(Laughter.)

MR. GINSBERG: We can discuss that over a glass of beer, I g[illegible]s.

PROF. KERR: Thank you very much. I have no questions. SSC programs.

DR. KELBER: I would like to introduce Jim Guppy, of Brookhaven Laboratories, who will discuss the SSC work. We will break the presentation in the middle so I can introduce the policy decisions that were made, to introduce the water version of the SSC code.

Before Jim Guppy proceeds, I might say that the preceding work that was discussed by Ted Ginsberg is one of those projects we have which is seriously underfunded. We do our best, but we are distinguishing those projects which are badly underfunded from those which are seriously underfunded.

PROF. KERR: The question I should have asked him is how much money you could profitably spend.

DR. KELBER: We all know that. It's clear that this is one of those programs which had to make a slow beginning, but which should have taken off then at a much more rapid rate, in the sense of being artificially held back.

(Slide.)

MR. GUPPY: I should thank Ted, I guess. I don't know, he used to be a friend of mine, because I thought I was seriously pressed for time before and now he's cut into my time a little bit already.

PROF. KERR: If you look carefully at the agenda, you will see that it runs till 4:00, at which point an executive session is scheduled. Now, the executive session will not last more than 10 minutes. Then the next line says the meeting is anticipated to be completed by 6:00 p.m. So, there is a certain amount of flexibility in the agenda, as it is now written, but there is not much flexibility about 6:00 p.m.

MR. GUPPY: I do not have that much material.

What I would like to discuss with you today is the SSC development and code validation programs at Brookhaven National Labs. "SSC" stands for "super system code," which we have designated as our code.

The presentations --

(Slide.)

-- Will be made by myself; Charlie Kelber, from the NRC; and John Meyer, from MIT. I plan to give, first of all, an overview of all SSC activity, and then talk specifically, first of all, about LMFBR-related activities. Then Dr. Kelber will give a brief introduction to discuss briefly the recent expansion of SSC work to include LWR analysis. And I will talk a little bit about this activity at Brookhaven. And then John Meyer will talk about natural circulation-related phenomena or phenomena and then modeling requirements.

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(Slide.)

The supersystem code development program, the SSC development program is meant to develop a series of computer codes to simulate thermohydraulic analysis in the entire plant including not only the reactor core and vessel components, but also the entire heat transport system through to the steam generator. The codes are designed to study operational and other system-wide accident transients with particular emphasis on natural circulation.

What I mean by particular emphasis on natural circulation is that from the time we first started developing the code, many features and models that were put into the code as well as the code structure, the flexibility built into the code structure was done with natural circulation in mind. The SSC codes will hopefully provide an independent analytical tool that can study a wide variety of potential system designs. Within the scope of SSC also, then, is to develop a generic code that is not tied to any one given plant, so that it can be applied to any either present or future potential system designs.

Plant control systems are included and the plant protection system is also included and is assumed to be operated. This particular assumption then separates the class of accidents that SSC is intended to handle from other LMFBR-related or other accidents analysis codes, in that once you say that the plant protection system when called upon, will function, then that takes us out of the HCDA realm and puts us into this realm here, operational transients natural circulation, transients that involve the whole system.

Therefore, we must be able to model adequately all components and processes that are essential to the ultimate heat removal, decay heat removal.

(Slide.)

The companion effort -- at Brookhaven is the SSC code validation program. The basic objective here is to validate SSC and the validation is basically split into two main areas, validation by comparison to experiment and then by comparison to other codes. Further subdividing is then comparisons on a system basis and on a component basis. We're heavily involved at the moment in following the FFTF pre-acceptance test phase, currently going on out in Richland.

EBR-II is a wealth of knowledge. EBR-II is a pooled type plant. We have established a liaison with the Germans through an international exchange agreement to obtain SNR-300 data when it comes on line. On a component basis, we have validated pumps in DHX models, which are in SSC with LMEC data, a steam generator with AI data. Argonne conducted some in the plenum mixing models tests that we

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have validated our upper plenum model with. We have not done this yet, but the Germans have some SNF-300 prototypes.

PROF. KERR: What is a DHE?

MR. GUPPY: A dump heat exchange where the FFTF has an air blast exchanger. Validation on a system basis can be done by comparing SSC results with the Westinghouse CRBR in-house code called DEMO, and with FFTF using the HEDL, previously Westinghouse code called IANUS.

Then, on a component basis, SSC results can be compared with such codes as COMMIX and/or COBRA. These are three-dimensional codes, but we can use those to compare our one-dimensional results.

PROF. KERR: Have you been describing things that are in progress, are being planned? On that last slide?

MR. GUPPY: This last slide was a mixture of both. These are currently being done. FFTF, EBR-II, SNR-300 are planned. EBR-II, the data is available already, a lot of data is already available. That data is not available because the plant is not on line yet. This has been done, this has been done. The data is available. This has not been done. Some comparisons have been made between these and some 3-D codes, three-dimensional.

(Slide.)

As I mentioned before, the SSC development effort. We envision a series of codes coming out of this development

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effort, and I have listed the four that are currently being worked on here. We skipped the last letter here on the end of it, SCL, PW and S, the SCL code is meant to simulate the short term, up to about half an hour transients in loop type of LMFBRs like Clinch River, where the primary fluid is conducted out of the vessel through pipes to the IHX as opposed to the pool version, designated as SSC-P. This simulates short-term transients in pool-type LMFBRs. Here the hydraulics have to be reworked because the IHXs in the primary pumps are in a huge pool —

PROF. KERR: Can you tell me which of these versions exists?

MR. GUPPY: If you will please bear with me.

PROF. KERR: I'll bear with you.

MR. GUPPY: SSCW is meant to simulate short-term transients in light water reactors. SSCS is meant to simulate intermediate and long-term transients so that these are compatible in scope. SSCS is meant to simulate very long term transients. Here it would incorporate what differentiates this from the other three is that here other heat transfer modes, like heat transfer from the pipe walls, or the vessel wall to the ambient conditions, heat transfer processes that are negligible under higher power high flow conditions.

These models will have to be included as well as auxiliary loops, other loops, auxiliary systems that do not normally operate under normal conditions but are called upon to operate, say, after a half hour or for the longer term.

(Slide.)

Now, as we just saw, the basic SSC development envisions a series of codes, but one way of viewing the structure of SSC is that it basically is a set of building blocks of various computational models and components, like the core, the pumps, the pipes, IHX, steam generator, control systems et cetera. So, it's really a question of how these building blocks are interconnected. This is what differentiates one version from another to a certain extent.

Thus, there is a lot of overlap, and many models and components are identical, between the various versions of the code. For example SSCL and SSCP, SSCL is for loop type LMFBRs, P is for pool type LMFBRs. Once you get to the second side of the IHX, it's identical. A lot of the models for the in-vessel are identical between the L and the P. The way that we run the code, I'll get into that later.

Another example is that the steam generator models are identical, but they could be viewed as physically turned inside out, between the SSCW, which is for LWRs versus that for the LMFBR codes. This is because in light water reactors the primary fuel is inside the tubes, whereas in the LMFBR the primary fuel is on the shell side, but the models are the same. What I was trying to show by that slide is that although there are several series of codes coming out of this effort, that there is a lot of overlap that, you know, it's not a full development from ground zero for each code.

(Slide.)

Now, the next several viewgraphs bring us to where each one of these codes currently is. The SSCL code, this is for the loop type LMFBR, has been operational now for almost two years. It's been operational, we have several users, it's fairly well-documented. I brought those two volumes along. One is a user's manual, one describes the modeling that is involved for the SSCL code. We have, at present, seven users, ourselves, the NRC/ARSR, the German outfit Gesellschaft fur Reaktorsicherheit -- this is a governmental reactor safety agency in Germany. What they're involved in is some aspects of SNR-300 licensing activities.

In addition to having already supplied GRS with a version of SSC we just recently got through an eight-week, you might call, training program for the actual individuals involved in GRS who will be running SSC for them. He was sent here for an eight-week period to learn how to run the code. As a result of his being here, we have generated an input deck that will run the SNR-300 using SSC. That was one of the accomplishments that we accomplished during this person's stay.

The next three users could be considered as, let's say vendor-users. They are involved with design-related studies. Babcock & Wilcox, Combustion Engineering and General Electric are, and will be applying the use of SSC to large scale LMFBR studies. The final one who currently has received SSC is the University of Arizona. They will be involved in these safety-related studies and accident delineation studies on a system-wide basis using SSC.

What we have here, then, is seven users. They have been able to use SSC to three different reactor plants -- excuse me, four. Brookhaven and NRC. We have applied SSC to CRBRP and FFTF. The GRS in Germany have now successfully applied SSCL to their SNR-300 plant and B&W, these latter three users have just recently received it.

Babcock & Wilcox received SSC in January of this year. They have already run through our test cases that we supplied them with, and built their own input deck to simulate their large scale LMFBR design. I have not received an input deck from them, but they say that they're very pleased with it, and have generated their own input deck.

In addition to these seven users here, Argonne National Lab has requested the code, as well as the

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Japanese PNC, have also requested the code. It has not yet been supplied to them. So, the way that I view the actual status of SSCL, then, it's moved out of what you might think of as the pure developmental stage into a more applications/developmental verification stage, where we have various users who are applying the code, supplying feedback to us to help us in our verification effort. And that feeds back into future development.

And as you've seen here, it's being applied to a wide variety of analyses and one of the particular uses that we're putting it to now is, as I mentioned before, to study the FFTF test phase.

(Slide.)

The status of the other three codes is summarized fairly briefly in that the SSCP, we're working toward the first working version. That's expected late this year. The first operating version of SSCW is expected in 1980, early 1980. And the SSCS is currently in the scoping analysis stage.

Right here, now that's kind of the end of the overview. What I was going to do now was to talk specifically about LMFBR-related activities. Then we'll talk at the end about water SSCW, the LWR-related activities.

DR. KELBER: Before Jim goes on, I'd like to interrupt to say that we have heard very enthusiastic remarks from the other users, both from the utility on the code itself, performing analyses. Incidentally, in doing design studies simply using the initialization phase, in addition, the code does set a very good example of documentation and adherence to good coding standards. And we are using it in that sense, with our other contractors as a way of showing them what should be done in this area.

MR. GUPPY: So now I'm going to focus more on the version that currently is operational under the LMFBR-related activities, that's SSCL for the loop type plants and intermingled with what I'm going to say will be something about models, features, capabilities and some results.

Now, SSC is a system-wide code and it contains many models and a lot of features and instead of trying to give a cursory view on a lot of things, what I was going to do was focus on one area. The area that I chose to focus on today is the steam generator area. If you look at the handout that I gave, at the end there are a bunch of viewgraphs that I call supplemental viewgraphs. They're labeled S, S-2, S-3, whatever. And if some of the other aspects of the code -- if you would wish to talk about them, fine. I'd be more than happy to.

But since time is important also, I have chosen to

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focus on our steam generator models and features and some results from there, although supplemental material is available. So, we'll move on to what I have brought with me for the steam generator modeling.

(Slide.)

Now, as I mentioned earlier, under the scope of SSC we tried to maintain a degree of generality as much as possible in the basic structure of the code, not only in the steam generator, but also elsewhere. One of the features is that the code is what is called variable in dimension. What this permits the user to do is to supply or to build any number of loops, any number of pipes within a loop, any number of nodes within a pipe, any number within a heat exchanger, this kind of flexibility.

In the steam generator design, the structure of it, we feel that it can handle any type of potential LMFBR steam generator design. The two basic ones being talked about today are what's called the forced recirculation steam generator design, which is like the Clinch River design, or some of the potential design studies that are being analyzed today use a one-system design. SSC will handle both of those as well as oth .

The modeling is basically a three equation model, equilibrium with slip allowed. And what I've tried to do here is to give a flavor of what is involved in the code.

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We aren't out to re-invent the wheel, as it were. We used tried-and-true methods wherever possible, wherever appropriate, keeping in mind the type of analysis that we want to do with SSC. Wherever appropriate we will use an existing model; however, wherever necessary or appropriate keeping in mind natural circulation type capabilities, we will make improvements to models or incorporate new models as deemed necessary.

In the steam generator, for the thermal and hydraulic solution on the water side, the momentum integral solution procedure, which was first developed by John Meyer, is being used because it is very appropriate for use for the class of accidents that SSC is meant to handle. Certain improvements that we've made t. enhance computer running time have been to make the time advancement implicit. Other extensions to give a better, more adequate representation of natural circulation capabilities is to add what's called the time bearing reference pressure, and also what's called a few-pressure model. In other words, each heat exchanger accumulator has its own reference pressure, which varies in time.

ne correlations that are involved both on the steam side and the sodium side are listed there. This is kind of an overview for some of the modeling that is involved in the steam generator.

(Slide.)

I would like to now just briefly run through one area of validation that we have accomplished with our steam generator model. Here I am going to briefly present some results that were obtained by Atomics International, using their modular steam generator. This was back in 1973. They ran a series of tests. I will show comparisons for what they call their "low-pressure tests," over a fairly wide range of steam generator conditions. And what we are basically comparing is the total heat transfer rate, what SSC has calculated versus what the AI modular steam generator produced.

What we have done is taken our SSC model, put in through the use of inputs and geometric description that typifies this modular steam generator, and then supplied it with inlet conditions. And on the next page are some of the typical results for these power matching.

(Slide.)

What's been plotted here is measured power -- this is experimental power. And if SSC were to produce the same result, then it would lie right along this straight line here. The points show the actual results that were generated over this fairly wide range of steam-generated power. The maximum deviation was 1.2 percent.

But what I tried to show here then is to focus on one component of the steam generator of the whole systems code.

That was really all I wanted to say about code features and models.

As I mentioned in the supplement are others, if you care to discuss some of them.

PROF. KERR: Could you put that slide back on for just a minute?

MR. GUPPY: This one here? These are steady-state tests where, if SSC produced or calculated 10 megawatts --

PROF. KERR: Would you say that that is a difficult thing to calculate or simulate or carry forward?

MR. GUPPY: This is -- well, by being -- using SSC is fairly straightforward. What it involves is going through quite a range of power and quite a range of various heat transfer regimes, and the computation --

PROF. KERR: I am not trying to be critical. I am just trying to get some idea of how significant this particular validation is, how significant you judge it to be. You said something about how you had a lot of doubt and you were delighted to see that it came out this way?

MR. GUPPY: We were delighted that it was in such good agreement.

PROF. KERR: Would you have been decimated had it not come out this way?

MR. GUPPY: What that would have basically meant was that some correlation perhaps was incorrect or something of this nature. But we were pleased that over this fairly wide range of power that we were able to get fairly good agreement.

DR. SEALE: Were there dynamic test data that were obtained and then run also? If so, how did they compare with your SSC results?

MR. GUPPY: We have not yet made dynamic steam generator comparisons to experimental data. There are some from this series of tests. There are others. We have not applied it to that yet. We intend to.

(Slide.)

In the supplement, as I said, are other modeling descriptions from other areas of the system description, as well as some verification. Some of them are in verification with analysis, and some are in verification with experiments.

What I wanted to do now, then, was to tell what can be done with SSC, and then just show a couple of typical results. SSCL can be used to handle sodium pipe breaks in any sodium loop in any pipe. It can simulate reactor SCRAM, either manual or PPS-initiated. Any main motor can be tripped, either all of them or any of them. Pony motors, a feature of LMFBR designs, pony motors can be made to fail. If you take the two of those together, you'll coast the plant down to natural circulation conditions.

So that in addition to these more safety-related transients, the pipe break and natural circulation can also simulate operational transients.

I have listed some of them here, such as reactivity transient, a single pump failure, either a main heat transport system pump or a loss of feedwater pump. Control system malfunctions, operator-initiated actions such as load changes, or perhaps even improper responses to some of them.

All these kinds of things can be done with a system-wide control.

MR. KASLENBERG: Can you do loss of feedwater itself?

MR. GUPPY: You mean a break?

MR. KASLENBERG: Yes.

MR. GUPPY: No. That would be out of bounds.

DR. SIEGEL: Let's pursue that. Does this include in a sodium reactor case, does it include the water system feedwater, turbine?

MR. GUPPY: At the moment, the modeling is to the turbine inlet, and then back to this feedwater supplies. That's how it's done. The feedwater control or turbine valve control, that type of stuff is there. But the turbine model itself --

(Slide.)

-- Quickly, running through a sample transient, the case that we're looking at is a natural circulation transient. This up here describes the degree of system detail. Four parallel one-dimensional channels in the core, 13 axial slices, eight radial nodes. In other words, like 400 stated variables here, the cases that I will show are what I call "flow redistribution." There is a flow redistribution -- I will discuss that in the next slide -- where that is on and off. It's a natural circulation case from 100 percent power and flow. All the main motors were tripped at time zero; in other words, a loss of all off-site electric power. Main pumps were tripped. The pony motors did not come on line. The reactor SCRAMed at three-quarters of a second manually.

Now, the simulation time that we would be looking at the next viewgraph is out to 300 seconds, and for this particular degree of system detail on the CDC 7600, 334 computer seconds were required. So, for this particular case, this particular degree of system detail, we ran a little bit less than one to one. It was 360 seconds of simulation time, which I think is good.

(Slide.)

Just a couple of typical results there. Here is plotted two different things: power in the hot channel -- this is channel one; as I mentioned, this is a four-channel representation. Channel one was meant to represent the hot channel in CRBRP. This is normalized power versus time, normalized power flow rate. Here is the power in that channel normalized. Here is the flow rate in that channel normalized for two different cases: one with flow redistribution; one without flow redistribution.

What flow redistribution means is here is another example of an improvement over existing codes, where we have incorporated an important effect that is important in natural circulation conditions. This is when you're down in the low-flow regime, when the buoyancy is dominating. And what happens here is that hot channels tend to suck more flow at the expense of the cooler channels, and that's what we see here, that when we calculate with flow redistribution again in the hot channel, the flow rate is higher than when flow redistribution is not being calculated.

This is because of the buoyancy-driven regime that you're in. The effect that this increased flow has on the temperature response in the hot channel is shown on the next slide.

(Slide.)

Essentially, more flow means less temperature. And for this case, what we're interested in seeing here is whether sodium is going to boil for this case. And for this particular case, for this particular definition of our hot channel under these conditions, sodium boiling is around 1200 Kelvin. So that you can see that the margin of safety has been increased rather substantially by including the real effect of flow redistribution here. This is to around 110 degrees Kelvin.

These are typical results that are generated in the vessel for SSC. Some other results are in those supplements.

(Slide.)

To summarize for this part of the talk, for a conclusion, I would just like to summarize by saying again that up to here we have been talking about LMFBR-related activities, and the primary emphasis in the latter part of this discussion has been on our loop-type code, SSCL. SSCL has been operational for almost two years. We presently have seven years. It's being applied in many areas, and verification is continually progressing.

We feel that the code is very modular in nature, and it's easily modified. You can get in -- just to give an example -- heat transfer correlations are put in a one-subroutine, and if you don't like that heat transfer correlation, you just have to change it one place. A lot of them you can change on input. Some of them you have to get in and make a one- or two-line change. But it's modular, and it can be easily modified. We feel it's fairly well user-oriented. We have some good documentation. The code is easy to read -- I claim it's easy to read.

There is a bunch of input involved, but we try to make the user code interface as painless as possible, so that it is user-oriented, and we feel that it's computationally efficient.

So, if there are no further questions -- are there any questions on the LMFBR-related activities?

DR. SEALE: Jim, you gave a fairly complete breakdown on some of the assumptions on the steam generator modeling. You also discussed this coastdown situation. Briefly, could you give us a characterization of the heat transfer model you used for natural circulation?

(Slide.)

MR. GUPPY: It's on one of these supplemental pages. Let's see. The heat transfer is what is called the "modified schad correlation." That is the best that we've found to date in that one correlation we feel is valid, both in the turbulent and in the laminar flow regime.

DR. SEALE: What page?

MR. GUPPY: It's called the "modified schad correlation." It's in the open literature.

DR. SEALE: Okay. That's all right. S-4.

PROF. KERR: What do you mean by the statement that you "feel it's valid in both"?

MR. GUPPY: Because the previous BNL staff number had done quite an extensive literature search and prepared the result of this correlation with others, and it was found to give good agreement, not only in the turbulent, but also the laminar flow regime.

PROF. KERR: So, it's more than just a feeling, then?

MR. GUPPY: Oh, yes, right. I am sorry.

MR. KASLENBERG: In one of the viewgraphs, we saw this morning in the COMMIX code, they indicated that it would be used complementary to your SSC. Can you indicate how that's done?

MR. GUPPY: Well, for instance, it can be used, say, to run a pipe to give a three-dimensional feel for such-and-such a transient as to what's going to come out the outlet. Some of the things that are of concern are flow stratification, and how this wave, since we have a one-dimensional, how this wave propagates through.

You mentioned the COMMIX. We have not prepared specifically for COMMIX yet. That's in the process. But one of the supplemental viewgraphs does compare our results to some TEMPEST code results. TEMPEST is a code developed out at the Pacific Northwest Lab.

(Slide.)

Just briefly, the thing that is of interest to us is how does our 1-D analysis, compared to a 3-D analysis, for an almost 18-meter length of pipe, this is the temperature that was put in; it's a relative inlet temperature defined right here. That's the temperature trace; this is the flow trace.

And then, on the next slide, which is S-8 --

(Slide.)

-- We can see the dotted lines in these are SSC and the solid lines are top and bottom of the three-dimensional analysis. The top of the -- at an axial slice, this is the top and the bottom of the cross-section, and this is for two cases: one where the pipe wall heat is transferred to the pipe wall. In other words, not transferred. The adiabatic pipe wall, just looking at the outlet here, this is the top, this is the bottom, this is our SSC one-dimensional result. So, this is the kin

So, this is the kind of thing that a code like COMMIX or another three-dimensional code could be put to use for, to test the results, verify our results.

MR. KASLENBERG: Would you do it to identify a sub-assembly within the context of SSC, or does that become too complicated?

MR. GUPPY: That can be done. I think John Meyer will touch upon some of these kinds of things.

Okay, that was it for LMFBR.

PROF. KERR: Other questions?

(No response.)

PROF. KERR: Let's have a 10-minute break before we go into the next section.

(Brief recess.)

PROF. KERR: Are you still on board?

MR. GUPPY: Yes. But Charlie's going to come up here.

DR. KELBER: You might as well stick around, Jim. I will get back to you very shortly.

Shortly after TMI-2, when we had a chance to catch our collective breaths, a decision was made by RES management to move as rapidly as possible to get a code capable of simulating in at least real time and possible faster the system interaction features of the accident. The eventual aim is to be able to predict the entire phase of the accident, if not in one code, in a series of codes. And it became clear that there were two major considerations:

One was the inclusion of the plant protection system and the control system, on the one hand. On the other was the ability to compute the very complex two-phased flows that are a consequence of a break in the primary system of a BWR.

A survey was made partly by my staff, by the Brookhaven staff, as well as by members of the analysis branch in the LWR section. It became clear that there was no code available that was in any way near the goals set by Sol Levine and Tom Murley. The two codes which came closest to being able to match the operation of the entire plant control system, including its interaction with the plant protection system, were BRENDA and SSC. Both of these happen to be LMFBR codes.

An opinion from the University of Arizona, Dave Hettrick, concerning BRENDA, was that it did not have great potential for extrapolation in this direction. This was based on some fundamental considerations, plus some work that he had been doing with a similar simulation for EPRI concerning water reactors.

This left SSC as a candidate, and here we felt, based partly on the type of consideration that's just been reviewed with you, that there was very considerable potential. Mr. Guppy and his people went into that in some detail. And within a short period of time -- I believe it was of the order of 10 days -- a decision was made to attempt to utilize this potential, realizing that it would be very difficult at these early stages to model the two-phased flows that are a characteristic of BWR systems, but that we would be able in a reasonably short time to have a code that could predict when such conditions would arise.

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There is some potential for extending the treatment to include the two-phased flows, but we would expect that the major tool capable of handling such problems will be the TRAC code that is being modified.

The problem with the TRAC code is, of course, that it has no system for incorporating the plant controls and plant protective system. We assumed they will be adopting a considerable amount of the representation used for SSC.

PROF. KERR: I guess I am having some problem putting your comments in context. When you talk about the difficulty in modeling two-phased flow, it's certainly difficult, but at least it has been attempted with various degrees of success in a lot of codes.

Is there something peculiar about this application that made it more difficult?

DR. KELBER: We do not incorporate modeling of two-phased flow in the primary system for LMFBRs, aside from a small model associated with the core itself. A highly approximate one, not nearly as sophisticated as is necessary for the use with PWRs.

PROF. KERR: Okay. And it was not possible to take some existing module or something and insert it?

DR. KELBER: In principle, I suppose, one could take a TRAC, for example, a fast-running version of TRAC, and use that. It may, in fact, in time evolve that way, that the two codes will be melded together. I think it's too early yet to say. We have to get something running that looks like it has some mix, some proportion of the right capabilities, and is capable of duplicating reasonably well what actually happened at TMI-2 before we can start making further judgments.

Now, this work is being financed and managed by the ARSR office, even though it is light water reactor-oriented. We will be getting, I expect next year, when you discuss this entire program with Dr. Murley, some of these management lines will become a little clearer. I think you will have to bear with us in this respect, as we sort out our problems and our approaches.

PROF. KERR: I gather, when you said earlier that this was not in response to a user request, it was a decision made by RES management.

DR. KELBER: It was a decision made by RES management. No, it was not done in response to a user request, except hallway conversations.

I did want to make clear the status of this because it is a little confusing in the middle of a discussion of LMFRB efforts to have an intrusion from the light water field. We feel it is significant enough, and it certainly takes a significant enough portion of our resources, that we thought it deserved some exposition.

PROF. KERR: When you say "our resources," you mean you had to divert some of your own funds to this?

DR. KELBER: That is correct.

PROF. KERR: They won't, over and above that you got?

DR. KELBER: That is correct.

So, I will turn it back now to Jim Guppy.

MR. GUPPY: So, with that as an introduction, I would like to briefly discuss the newest added version of SSC, designated SSCW --

(Slide.)

-- Tell something about its scope, and a status report as to where we are at this time.

(Slide.)

The scope of SSCW is to develop another version of the SSC code, the super system code, that's applicable to water reactors. Then it can be used as an independent licensing tool for analysis of natural circulation and other systemwide events.

It will be an independent analytical tool, and it will have a strong emphasis on the adequate modeling of natural circulation capabilities.

Now, this, again, is in context with the primary loops, but SSC is a code which is structured specifically to handle natural circulation-type events.

PROF. KERR: That stimulates another inquiry. As I understand SSC -- and I am not understanding it in detail, all that it does -- it is primarily one-dimensional. Now, is it the consensus -- I guess it is -- that the important phenomenon in natural circulation situations in LMFBRs are handleable on a one-dimensional basis?

MR. GUPPY: Yes. It is basically one-dimensional once you get outside the vessel. In the vessel we have what could construed as more than one dimension, particularly in the in-vessel region, because we can specifically represent various assemblies; and if two-phase is to occur in the LMFBR system, which could happen under natural circulation or pipe break -- that's what happens in a regular core -- you can predice whether sodium will boil, to handle it in the LMFBR situation to a limited degree.

So, you're looking for, you might consider, the onset of boiling.

PROF. KERR: How do you decide whether one or more dimensions are necessary in the core? It would seem to me that the core is a fairly crucial region in natural circulation situations.

DR. MEYER: Excuse me, Jim. I will be addressing some comments to that in the natural circulation part of the discussion.

PROF. KERR: I will wait. Continue. You have been preempted.

MR. GUPPY: In addition to accounting for natural circulation-type events within the framework of SSC, it can also be used as a generic tool to analyze other systemwide events where control systems and plant protection systems are operating in influencing the system response.

Since it will have the control of PPS, plant protection systems, modeled, it will have the capability of analyzing various failures on a systemwide basis that could potentially lead to design basis accidents.

MR. KASLENBERG: Does PPS include the high-pressure injection system and low-pressure injection, as well?

MR. GUPPY: It doesn't for LMFBR.

MR. KASLENBERG: I meant for the water. And also, could you simulate an operator intervening -- in other words, shutting it off, putting it on?

MR. GUPPY: Yes.

(Slide.)

For the initial version, which I mentioned before, will be operational early in 1980. I anticipate it would be operational in early 1980. The initial version will be directed toward PWR applications first. And it will be single phase in the primary loop or loop.

But I wanted to point out that this is not necessarily a limitation to restrict it to non-BWRs because, as I mentioned before, SSC can be viewed as a series of building blocks, and we already have a steam generator representation. At the moment, we happen to consider water inside the tubes. It could just as easily be fuel rods instead of tubes. So that extensions to BWRs can be handled within the framework of the SSC structure, but we are directing our initial effort at BWRs first.

PROF. KERR: I had got the impression from Mr. Kelber's earlier comments that one of the goals was to be able to describe the TMI-2 event, which I had understood involves two-phased flow of the primary system.

MR. GUPPY: Yes, so that this initial version will be able to follow TMI up to the point of two-phased appearing in the primary system. If that is as far as we take it, the SSC PWR development still could be used perhaps in setting --

PROF. KERR: I guess I am a little puzzled, because I would have thought that if TMI-2 had stayed single-phased, one would not have needed to develop this code. Yet, you tell me that you're just planning an initial step that will go out to a time at which the second phase began to develop. It isn't clear to me what information that would provide that would be especially useful.

MR. GUPPY: Charlie, do you want to address that?

DR. KELBER: First, there was a realization that in the analysis of the plant and in the analysis of the plant by the operators, there was a lack of awareness that some of the interactions could lead to two-phased flow. So, it was the feeling that having a fast-running code, one that was easily modified, that could indicate when you got to this point was in itself valuable. And I think that is a correct perception.

There are some other points. I think --

MR. RIPP: Lenny Ripp, ARSR. I just got a report which outlines the TMI accident which didn't have the detailed calculations, which does have the pressure, temperature, TMI, as a function of time as best it could be constructed. But it needs much more detail, which we hope to get out of his analysis.

PROF. KERR: I guess I didn't make my question clear. That was not my question. But it seems to be restricting the study of TMI to the single-phase primary as possible.

DR. KELBER: This is not the only activity, Bill. This is something that we can do fairly quickly, and that no other code can do as quickly or as well. That is to determine what are the types of attractions that lead to two-phase TRAC, which is the chosen vehicle -- I think the correctly chosen vehicle -- for the analysis of the two-phased flow.

Clearly, it can do the small-break analysis and is being set up to do that. But it does not have as yet -- it will eventually have -- the same capability as SSC to model the interaction between the control systems and plant protective system, including the safety relief valve.

There is no satisfactory code to handle all aspects of TMI-2 at this time. What we are doing is constructing two codes that are capable of doing the job. Eventually, I believe, there will be a single code. I wouldn't be a bit surprised if it looked very much like TRAC with parts of SSC put on it.

PROF. KERR: I guess at this point I think I understand what you're doing, but I don't understand why you're doing it.

MR. GUPPY: Could I put up the last slide at this moment?

(Slide.)

Maybe this will shed a little more light.

PROF. KERR: Maybe I should just listen. Maybe I am asking questions before I have information with which to ask them. Go ahead.

MR. GUPPY: Well, I will put this up later, but TMI is not the only event that we're going to be using SSC to analyze. We can analyze another whole series where various failures, single failures, multiple failures, that impact on the system response could lead to, say, two-phase in the primary loop. Two-phase in the primary loop, to my mind, is something that you don't want. It could be used to set operational limits.

PROF. KERR: You see, I mean, it depends a little bit on what you mean. You had two-phase in the primary loop all the time in the PWR because you have subcooled boiling. So, I may be asking stupid questions because I don't know how far you're taking this. I was just puzzled a little bit when I saw it was only going to deal with single-phase.

MR. GUPPY: Walt Kato would like to make a comment.

MR. KATO: If I may make a couple of comments.

We have programs in the light water area at Brookhaven which we are also working on, fast-running codes. The problem is there are activities -- code development groups have developed codes which can simulate TMI-2 accidents; however, they're extremely long-running from a computer point of view, and there is a need for a very fast-running code so you can do sensitivity-type studies, basically to change parameters such as operator actions, et cetera, to do survey-type calculations. And that type of code is not available.

The thought was that something like SSC could be quickly converted into a fast-running code, where you can do studies up to the point where you've got boiling.

PROF. KERR: So, you aren't really planning to simulate TMI-2?

MR. KATO: Not initially.

PROF. KERR: What you're doing is studying primary systems to see when you get in trouble with PWRs.

MR. KATO: That's right. Where you have various operator actions taking place. For example, as I understand it, the intent of the operators was to try to get natural circulation started at the very early part of the game. They couldn't because boiling or vaporization had already taken place.

And so, it's in that regime, where, if you have a fast-running code --

PROF. KERR: In that case, Walt, this code wouldn't help. I take it it wasn't that they didn't realize that you have difficulty getting natural circulation started if they'd known what conditions were; it was that they didn't know what the conditions were.

MR. KATO: However, I think the point is that now, having codes, one can take different types of operator action to see --

PROF. KERR: It seems to me what you're telling me is that you are using this route to develop a simulator which will almost operate in real time with which you can simulate various things.

MR. KATO: Precisely.

PROF. KERR: That's different than describing the TMI-2 incident, which is what I thought Charlie said. I guess that's the reason I was puzzled. I didn't understand.

MR. KATO: Well, I think simulate and describe are similar words. But I believe that I said that management felt there was a need for a fast-running code, and that's precisely what this will tell you in response to a variety of situations when you get in trouble. If you get into trouble, this code may not be worth too much, but it will tell you when.

MR. KATO: I might add that there is a great deal of work going on to develop codes which are also fast-running which will now have complete phase separation capability as well.

But that's not the immediate goal of SSCW.

PROF. KERR: I think I understand better what is being attempted.

(Slide.)

MR. GUPPY: So, just to finish out what this initial version that will be available in early 1980 is being directed towards PWR single-phase primary loop, and we're modeling once-throughs first before extending to U-tube written designs.

We are gearing towards B&W types.

PROF. KERR: In the meantime, have you persuaded somebody in licensing or somewhere?

MR. KATO: Pardon?

PROF. KERR: I was asking Charlie if at the same time this development was going on you were also persuading someone in licensing or regulation that they're going to need it when it's available?

DR. KELBER: From the hallway conversations that I have had, I don't think it has very much persuasion. They want us to hurry it up.

PROF. KERR: You do expect a user request?

DR. KELBER: I don't know whether we'll ever get a user request for anything like this. It will just by used. I really don't know whether we'll get it or not.

MR. KATO: There have been tremendous pressures in the last two months to have a fast-running code of this type, from the licensing people.

We have some other work that's going on directly for the Reg staff who are doing this.

DR. KELBER: By the way, the time period we're talking about, for example, in the case of simulating TMI-2, is up to about six to eight minutes after the turbine trip, according to this analysis that Len Ripp referred to. At that time, the system got down tc saturation pressure, and it's not clear precisely would large-scale boiling started. And TMI-2 would be intended to pin that down somewhat further. SSCW would be intended to pin that down further.

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(Slide.)

MR. GUPPY: Before going into the status of exactly where we are with the SSC-W development effort, just looking at various things that are required in the system model in vessel representation, the steam generator representation, the multi-loop representations which SSC is set up to handle -- one of the different features that the BWR plant has is that they have large, oversized -- well, not oversized but large steam generators, for example.

Westinghouse for loop design has a pump for each steam generator. However, TMI has a single pipe leading into the steam generator and then two pumps coming out. These types of systems, these system details will be included in addition to other features like PPS and PCS.

But this is a new feature that has to be addressed.

DR. SEALE: You are going to put a relief valve on that pressure line.

MR. GUPPY: Yes, it's already on line.

(Slide.)

Moving into the exact status of where we are at this point in time, we started this effort -- work was begun in mid-May of this year and as I mentioned a couple of times, it can be viewed as an arrangement of building blocks. SSC-W is up to a certain point now a rearrangement of the blocks.

Examples are in-vessel modelling. The in-vessel

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modelling, the modelling of the core and the fuel rods and the fuel assemblies control rods remains the same. Correlations constitute of relationships have to be changed to water. The LMFBR cover gas model is simply deleted, but then the way that the in-vessel rods and the structure and what not are built, they are handled through input. So that doesn't need to be changed.

PROF. KERR: When you say rod, you mean a fuel rod?

MR. GUPPY: Fuel rod, control rod, whatever is in there. The user built it in inputs. I didn't get into that. It's in some of those supplementals.

PROF. KERR: I just wanted to know what you meant by rod.

MR. GUPPY: Yes. The piping -- the models were identical, the geometry specified through the input. We have deleted the LMFBR, primary loop, and the IHX model pumps.

A description of those is in the supplemental sheet. The models are identical. The actual pump is specified through data that's handled through input.

(Slide.)

Some of the other areas, a new area is the pressurizer. Just briefly touching on some of its main features, the physics is based on the RETRAN formulation. Various things that it will do are listed here. Dr. Seale mentioned the relief and safety valves are in, and a stand

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alone model, since this was a new model, it was developed fist on a stand alone basis. It's currently being interfaced into SSC.

It is operational.

On the next viewgraph --

(Slide.)

-- is showing one result of a verification.

DR. SIEGEL: Excuse me a moment. Is pump cavitation never considered in any of the models?

MR. GUPPY: We haven't extended the model to handle that.

DR. SIEGEL: Even in the LMFBR case.

MR. GUPPY: Even in the LMFBR case. But all operating regimes of the pump are accounted for. All four quadrants of the operating regime are accounted for. We cannot presently simulate cavitation, but you don't have to have cavitation to get into the four regimes.

This is using our stand alone pressurizer model against a shipping port loss of load transient that was from shipping port. It's in the literature from, I think, 1969. This is the response from our pressurizer model to this extensive swing in pressure.

Variously, the sprays are on, the heater's on. That particular stand alone model we can track.

PROF. KERR: Is this considered a good simulation?

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MR. GUPPY: Yes, I think so. That's a fairly wide swing in pressurizer pressure. And the thing that is not known exactly is whether the temperatures come in in a surge line and the sprays -- what's the temperature of the fluid coming in the surge line and the sprays -- to the extent possible?

This to my mind is a fairly good comparison compared to experimental data.

PROF. KERR: What would it look like if your work were not very good? How different would it have to be?

MR. GUPPY: I can't tell you. If you couldn't follow these --

PROF. KERR: You must have had some criteria for deciding that it was pretty good. Was it just eyeball? You looked at it and said that that is pretty good simulation?

For example, there's a shape in both cases in the experiment. It represents some sort of transition, it seems to me.

MR. GUPPY: It's a loss of load transient. Initially, the pressure in the pressurizer rises, the spray comes on. The spray comes on --

PROF. KERR: There's a rather large change in slope there, for example. I don't know whether that's an important phenomenon, but it shows up in the experiment.

If you go into the other experiment, there's a

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similar kind of thing.

Again, I don't know whether that represents a phenomenon that's important. Hence, I don't know. It does not look like a bad simulation to me, but I don't know how you judge. I wonder what criteria you used to determine that it either was a good or not very good simulation.

MR. GUPPY: I would say that it's good. It's not excellent, but it's good. It follows the trends.

Charley?

DR. KELBER: I guess when we first saw this, we looked at the scale over on the left-hand side and felt that the mismatches were within the probable accuracy of the measurement itself.

That's a pretty large scale. That goes from something like 1995 psi up to 2150. And the mismatches are always within a few 10s of psi at the most.

We thought that's pretty good. I would expect, for example, that a gauge reading 2000 psi might easily be in error by 5 percent.

MR. GUPPY: We have done other simulations to confirm the results. Here, I think what makes the comparison good or bad is the temperature of the fluid when the spray is on. It's coming in through the spray line. What's the exact temperature of that.

According to the fellow that did this simulation,

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he used the best temperature data available. If you twiddle that number a little bit, you can get, you know, get it to move in and out.

But he didn't feel that it was worthwhile twiddling that because of the data.

PROF. KERR: I'm really not trying to quarrel with whether it's a good simulation or not. I don't know. I just wondered what criteria you used. Charley seems to say that if it's within 5 percent, it's pretty good. You didn't tell me that, so you must have used something else.

I just wondered what you used. Or maybe the man who was putting it together decided that it wasn't worth twiddling the knobs. People use criteria which is sometimes referred to as engineering judgment, which means, I guess, that you look at it and out of a wealth of experience, you decide that it's pretty good, if that's what you did.

MR. GUPPY: Well, I didn't do it myself. But the fellow that did it thought that it was pretty good.

MR. KATO: Bill, I think that one of the things that we will have to do once the code is assembled, we'll have to see what the sensitivity of the total systems code is to variations like this.

We haven't gotten that far yet.

MR. GUPPY: What the person who made this model did was he compared it to a calculation and he concluded that at least he thought he had it debugged. Then he changed the geometric representation to simulate the shipping port pressurizer and then put in the surge flow, the spray input, and this is the result that came out.

I guess to my mind it follows the trends. Then you'll have to see how the rest of the system --

PROF. KERR: I think what you're telling me is that the man who made the judgment really didn't believe in the experiment. He really felt good about comparing it with another calculation if it fared while the calculation was pretty good.

MR. GUPPY: No, he did the calculation first, then he did the shipping port one.

PROF. KERR: He felt much better about the fact.

MR. GUPPY: Because he knew he could put in the exact input conditions.

PROF. KERR: I understand people who calculate.

(Laughter.)

PROF. KERR: It's a comforting feeling.

MR. GUPPY: The present status on the other major area for the ones through steam generator design is that we have turned the logic inside out, put the primary fluid inside the tubes, switched the correlation over to water, and we've tested the steady state.

Now one of the peculiar features of the TMI-type

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reactor steam generators is that they have what is called an aspirator in which some of the fluid that's going past the bundles gets diverted and mixes with the feedwater coming in.

We're working on incorporating this aspirator in. It's being worked on. But we have, without the aspirator, we've got the steady state steam generator models running.

I had some plots in the next couple of viewgraphs on what that looked 'ike.

(Slide.)

What I wanted to just show here was -- this is without the aspirator. What the aspirator does when the secondary fluid enters the fuel bundle regio , it's already saturated. What this shows is that here in the once-through steam generator, we have four heat transfer regimes that are being handled. This is not a comparison to anything. The outlet temperature is pretty good, but the temperature profile inside is not good because the aspirator is not in there yet.

That just shows that the model is being developed and is coming along.

(Slide.)

The last main feature is the pipe protection and plant control systems. Those are those additional safety and control systems generic to lightwater reactors — are being added as required.

We have a fairly general way of specifying PPS functions and plant control as specified, what's going in and what's going out.

But as one of the people mentioned, something new is this high pressure injection that is added as required.

So that brings me to the last slide that I put up before.

(Slide.)

Which is what we'll be doing once we get the initial version on the road. We hope to have it completed, expect completion in early '80. The first thing that we will apply it to is the TMI-2 event. But we will only be able to follow it out until approximately 8 minutes into the transient until saturation pressure is reached, somewhere in the primary loop.

But that's not the only event that we'll be applying it to. Coast-down and natural circulation type events, and the PWRs are supposed to be designed to handle the loss of all pumping power and make sure that you get natural circulation, and that some potential intervention, either another failure or operator intervention, that this does not preclude you from being able to maintain natural circulation. In addition to those types of transients, operational transients, other failures in the system, either PPS or control system operator intervention -- that could possibly

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lead to design basis events.

Then we will be, just as with the LMFBR-related activities, we will be carrying on a verification effort. One of the particular ones that we're interested in looking at are some that have just recently been done in support of TMI by B&W at their Alliance, Ohio experimental facilities for steam generator and natural circulation related tests.

We'd like to use those to verify our steam generator models.

Other tests that come to mind is a reactor plant is coming on line in Arkansas, alsthough that is a CE design. But they have an ambitious natural circulation test plant during the acceptance testing phase of that plant where they're going to scram it from 80 percent power down to natural circulation.

That should also provide a good test.

So these are the types of applications that this first version will be put through. And then, as with everything else, that wraps up what I wanted to say on the SSC-W version.

Questions, please.

PROF. KERR: Questions?

(No response.)

PROF. KERR: I see none.

MR. GUPPY: At this point. John Meyer will get up and

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talk on some aspects of natural circulation related activities

John has been working with us in a consulting capacity in some of our SSCS. That was the very long-term version of SSC. He's been a consultant with us in that area, although a lot of that work was directed toward the SSCS version of SSC.

Much of John's work and the recommendations that he's come up with are also true for other versions of the SSC code.

I have brought with me, if anyone is interested, a copy of a report that John has written late last year on these SSCS-related activities.

So, please, John --

(Slide.)

DR. MEYER: As Jim has indicated, I will discuss some of the computational methods we hope will be us ful for reactor plant and natural circulation.

As you know, natural circulation processes do contribute significantly to heat removal in a lot of postulated reactor plant situations. And the strength of even the existing SSCL code lies in its treatment of natural circulation.

What I'm going to discuss today are extensions to the existing techniques and extensions which will expand this already considerable natural circulation capability.

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The nomenclature that has already been introduced by Jim Guppy -- the extensions that I'm going to talk about are mostly those to be incorporated in the SSCS version of the code, so I'll talk mostly about SSCS, though many ideas apply also to other versions of the code. And some of them have previously been incorporated into the other versions of the code.

First let's consider computing time.

The L version already is very fast. It requires a few minutes to a half hour of machine time for simulated transients on the order of a minute to a half hour. And I think to really be useful, we ought to try to maintain computing times on this order of magnitude, a few minutes to a half hour a case.

Once they get much longer than that, they become good for check calculations, but not much good for design studies or really understanding the limits of the phenomenon involved.

So that sort of an aim is to get something on the order of minutes to a half hour of computer time. But the job, as was originally discussed with Lee, is that the S version should be used for simulating transients reaching between a half hour and a day.

So this means on the order of a factor of 30 better computer techniques in SSCS than any other version of

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the code.

So this is a pretty dramatic improvement that we need.

Second, and this is the dramatic improvement, second, though, there are various physical processes involved in this long-term cooling that are not important for shorter transients. Conduction becomes much more important, especially in the sodium cooling case. Stratification can become important. In fact, in some cases, could even dominante the situations.

Also, there are additional active components, heater exchangers that are not used during normal operation. Some of the distant metal, for example, in the reactor vessel may become involved in the transient, which is not even, doesn't even change temperature at all in the shorter term transient.

So the conclusion is that -- not the conclusion of my discussion, but the conclusion of my introduction, the conclusion is that we have to strive for some kind of balance in this, a balanced approach in this SSCS.

We have to have enough detail where it's needed for a good physical model. We have to eliminate detail where it's not needed in order that we can obtain high speeds.

We have to use numerical methods that are appropriate for high speed computing. Finally, and this may be the

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toughest one, we have to have sufficient generality to be appropriate for a range of plant designs.

I'd like to, in the rest of this talk today, I'd like to give the present thoughts we've had on achieving this balanced approach to natural circulation.

I've just finished the introduction. I'm going to move on to some of the descriptions of the numerical and physical approaches that we were talking about using. Then we'll get back to the item that I promised from the back of the room — the core model.

Then we'll discuss the plant representation. Then some very brief discussions of comparisons among the various plants and indications of future work that is involved.

(Slide.)

The numerical and physical approach -- some of the distinctions between numerical and physical in what I have here are a little fuzzy. But at any rate, that's taken care of by the fact that I have numerical/physical. I'd like to talk about time constants because certain of these transients, the time constants of particular physical processes are important, both for deciding whether they need to be involved in the physical description and in deciding how to treat them numerically.

One of the thoughts, and you talk about -- when you consider transients on the order of hours or days, you say, well, we can just treat this as a group of steady state calculations or quasi-steady state calculations.

It turns out, and this was a bit of a surprise to me at least, that this is by far not the case. We cannot use the quasi-steady state calculation techniques for reasons I'll describe in just a moment.

I will discuss briefly some numerical methods and then give some numbers for enthalpy transport, which is sort of the support for this statement that we cannot use quasi-steady stated methods.

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PROF. KERR: When you use the term, "time constants," though, is it valid for me to assume that you're dealing with a lump system model of some kind, perhaps either a zero dimension or one dimension, at best?

DR. MEYER: That's why I hesitated to say whether th[illegible] was numerical or physical.

(Slide.)

I'm using sort of an operational definition of time constant. As a function of time, you have some input that goes up in, say, in a linear manner. And then you have some response, the delay between the response an[illegible] the input of the response I'm calling a time constant. We may call that one element in the clad of a fuel rod, for example, or we could call it the entire fuel rod, even though it's sectionalized.

So I'm using it in not a very precisely defined sense, but this is my operational definition that some input — for example, the power goes up in the fuel rod, the heat flux will follow at some later time. And I would use that as a time constant for that fuel rod.

PROF. KERR: It's a perfectly valid definition. If it's your definition, I accept it.

DR. MEYER: That's what I mean then by time constants, an operational definition of time constant. It gets simple in simple cases. It gets fuzzy in more complicated cases. But

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the general idea is this: The question that I had started to raise on the previous slide of whether a computation can be calculated in a quasi-steady state, an equation for that is -- how does this time compare to the time significant changes? If the time constant is very short compared to the time for significant changes, we can probably treat it as a quasi-steady calculation.

If the time constant is very long compared to the time for significant changes, then we cannot treat it as a quasi-steady calculation unless it is very, very long and then doesn't participate at all.

So I'm going through sort of a plausibility argument of how we're considering building the numerics for this long-term version, and this is what I've called a quasi-steady state ratio, which is simply the comparison of time constants to the time for significant changes.

PROF. KERR: You're also going to comment on how you judge qualitatively or quantitatively whether you have to go to more than one dimension.

DR. MEYER: Yes.

PROF. KERR: Okay.

DR. MEYER: I hope so. I'm intending to answer that question yes.

(Slide.)

Question: What is the time for significant changes?

What I have plotted here, this is taken from some Westinghouse calculations for the Clinch River reactor. In fact, all of my reactors today will be from the Clinch River reactor.

This is for a flow coast-down to natural circulation, the first thousand seconds of that transient. This is the exit temperature from the reactor vessel as a function of time. And the scale here is like from 300 to 600 degrees C. You'll see that the curve sort of meanders along and that I've said, well, if I were here, we would really like to know that that particular transient has occurred.

And this is a time of significant changes by about 70 seconds.

So we have to have methods that are able to get detail then on the order of 70 seconds, but we probably don't need to be just a few time steps in there may be significant. May be sufficient to get that detail.

On the other hand, in these particular transients, we could not use 250-second times. We've completely missed that detail there.

PROF. KERR: How do I know that that detail is significant?

DR. MEYER: That's just a judgment. I just looked at this picture and I backed off and said --

PROF. KERR: You're saying suppose that's significant.

DR. MEYER: Suppose that's significant. It may be

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that we don't care about that, that all we care about is that it's going down like this. Maybe we can take much longer time steps, then.

But if we want to know that that's a curve, and I think that there are reasons that we might want to know that that's a curve, because that's where we've gone into the loop and there's a lot of piping out there, we may not want to know where that hot sodium is.

We may want to know where that sodium is a little later.

So that's the kind of thing. You sit back and say, what is significant? What is the time for significant change?

(Slide.)

So numerical methods, as I said, have to be very good. In this situation, one of the questions is how explicit can we make the methods?

By "explicit" I mean that we use old time information to compute what's going to happen at the advanced time to decide how explicit we can make the calculations. Explicit calculations tend to be very fast, tend to be easy to do.

On the other hand, they tend to become unstable if this number becomes too big.

This number is what I've called an explicitness ratio. It's the ratio of time step to the physical time constant or to the time constant of a particular process. If

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this number is very large, then we cannot use explicit calculations. If this number is very small, we can use explicit and simpler and faster calculations.

So that, again, there is a balance that we have to do in this system in designing the computational system.

The final thing, and I've already alluded to it, is how fast is the transit? This is a speed ratio. The ratio is the ratio of time step to significant time like the 70 seconds we had before.

Again, if we wand to handle that 70 seconds significant time, our time steps can't be larger than, say, 10 to 15 seconds. But if significant time is as long as 70 seconds, we would not expect that we'd have to handle time steps on the order of one second or a half second, we would expect that we would be able to use — from physical reasoning, we'd be able to use longer times.

So it's this kind of balance that I'm trying to indicate. This is our objective — is to get a balance among these various competing things in many times.

SSC-L, of course, the existing code, has been in operation for two years. It has to answer some of the same questions. How explicit? What kind of time steps compared to the speed of the transient? And so forth.

The things that make some of the techniques that have been incorporated in SSC-L that make it run so quickly is that there are strings of calculations. This is the evaluation of having a one dimensional model. There are strings of computations volumes that are connected together in an implicit manner.

For example, the outlet temperature calculation of the reactor vessel all the way around the loop to the inlet to the reactor vessel is handled implicitly.

There's one string of computations.

Then when you get inside the vessel and more detail is needed, you have what are called your explicit connections. The lower plenum and the upper plenum are treated explicitly. They brake the computations, make it very efficient to do the computations, and yet, are sufficiently accurate for the SSC-L type of transients.

One of the questions in our minds that we really don't know the answer to is when we go to these very long-term transients, whether the explicit connectors have been put in the right place or not.

It may be that the explicit calculations should be in a very different place in the long-term transients than in the short-term.

So this is one of the questions that we have asked.

The other SSC-L technique that we had planned to adopt is that different time steps can be used in different parts of the system. The steam generator is calculated on

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one time step and the reactor vessel is calculated on another time step, and they all come together in the master clock.

So there are a few levels of time steps that are used in SSC-L, this type of multi-time step technique.

PROF. KERR: If I had to try to understand the decision process that results from this, it seems to me that in order to determine what time intervals are significant in various phenomena, I have to have a fairly good idea of what's going on.

DR. MEYER: Yes.

PROF. KERR: So that some of these things might have to be decided independently of the code, but on the basis of the physical phenomena and/or phenomenon that are being described. Is that right?

DR. MEYER: That's exactly right.

PROF. KERR: Thank you.

(Slide.)

DR. MEYER: I'd like to display some numbers here for the Clinch River plant that were a surprise to me when I first saw them. These now are enthalpy transport time constants.

In other words, if I take the inlet of the reactor vessel up at some linear rate and then watch where the reaction of the reactor vessel goes up, the delay will be 43 seconds at full flow in Clinch River. It's a very sluggish

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reaction to temperature transients or enthalpy transport transients.

This is pure enthalpy transport delay. There is no metal involved in this or anything. It's 43 seconds. The next step, another 14. Inside the IHX, the intermediate heat exchanger, another 12 seconds, and so forth.

Tht total in the primary heat trensfer system is 76 seconds. The total in the intermediate system at full flow is 94 seconds. The total in the steam generator is 57 and so forth.

Of the time spent by fluid in the reactor vessel, only 1.3 seconds are spent in the core. And this makes an interesting conclusion about the core when we go to the low flow situation. If we go to somewhere near the end of that transient I displayed on the previous figure about 1000 seconds into the transient, the primary heat transfer system flow is about 3 percent.

I think the intermediate is about 2-1/2 and this is about 14 percent.

At any rate, you see the time constant now for transport, enthalpy transport around the primary system is three quarters of an hour, 2500 seconds. The time constant around the intermediate system is over an hour. The steam generator, another 10 minutes.

So this is the support, then, for my contention that gsn

it certainly can't be treated as a steady state. You must treat it as a transient calculation.

On the other hand, if you look at the core here, the core is still at 43 seconds transport time.

PROF. KERR: Tell me how that demonstrates that you can't use a quasi-steady state

DR. MEYER: If we're talking about a transient in which we're interested in things on the order of several hours, and, for example, have operator action within that time, we are not going to settle out to a steady state because it's going to take —

Let's say that the operator puts extra cold feed into the steam generator. That means that that reaction, then will come around through the intermediate loop. It's going to take an hour for it to get clear around the intermediate loop. It's going to take another hour for that disturbance, another three quarters of an hour to get around the primary loop.

So that the effect of that operator action from an enthalpy transport time standpoint will be felt very, very late. It will not settle out to a new steady state until very, very late.

PROF. KERR: I guess to me steady state and quasi-steady state don't mean the same thing. Quasi-steady state I can get by moving from one steady state to another

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steady state in some fasnion, if I don't have things changing too rapidly.

It would seem to me naively that as slowly as these things are changing, one could almost get from here to there by going from one steady state situation to another steady state situation.

You seem to be telling me that that's not practical because it takes so long for change to occur.

DR. MEYER: That's right.

PROF. KERR: You may be right. I just don't understand what it is you're telling me.

DR. MEYER: Okay. I'd like to do it another way. I think one of the things in definitions of steady state, that's one of the reasons I put on the definition before, is the life of significant change.

How does that compare to the total time constants? I'm saying that this is the time constant of the system. If we make a significant change somewhere in the system, change the enthalpy or the temperature somewhere within the system, it's going to take that long for that change to settle out.

Therefore, my definition is not quasi-steady state. We do not reach the new steady state. We must have many of these time constants in order to reach the new steady state, a true steady state where we have the same heat input and the same power going through the IHX and the same power coming

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out of the steam generator.

Now you can reach a situation in which the flow varies in the primary loop almost constantly, and that way it looks like steady state, but it's not a steady state because you're still taking out more or less heat from the steam generator than you were putting in the primary.

That's why I say that it must be approached in some sense as a transient calculation. There may be simplifications but it must be approached as a transient calculation.

The last item on here is to note that this time for the core, 43 seconds, compared to all of these other times in here, indicates that the core, even though the other transport processes are taking very long, that transport through the core is still very rapid. And we may be able to take almost a quasi-steady state treatment of just the transport processes in the core.

We may be able to treat the core much more simply than you would think on the basis of -- well, I don't know. The hope is that we can treat the core very, very simply because of this and that we can use subsidiary steady state calculations with more detail of codes in order to obtain core behavior.

PROF. KERR: Should I conclude, then, that you make the decision on whether the code needs more than a single dimensional treatment by, in effect, observing the time of

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transport or the time constant.

So it's not spatial characteristics that you deem important, but rather, time characteristics.

DR. MEYER: That's correct. And the other item is that what we hope to be able to do is to suggest ways to the user of the code that he can use whatever dimensionality he has -- for example, the COMMIX code -- in order to provide input and to do, for example, steady state calculations and whatever dimensionality is needed for a good description of the code for the COMMIX calculation, and then provide the input information to SSC for these long transients and get the other important features, the important transient features for these.

PROF. KERR: Suppose at some point you were faced with the task of deciding whether the SSC-S, if that's the right nomenclature, code would of itself give results of sufficient accuracy that one would not have to worry about doing experiments?

How would you go about making such a decision?

DR. MEYER: I think that we have to base decisions like that on other studies.

PROF. KERR: Other calculations?

DR. MEYER: Other experiments. Certainly, there are a great number of experiments, intra-construction experiments, EBR-2.

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PROF. KERR: So you would not, at least at the present state of knowledge, be able to extract from the code itself whether the information is valid. You might want some experiments.

DR. MEYER: Oh, definitely, yes. In fact, I think it would be very difficult. I can make a lot of nice pictures and a lot of inter-code comparisons, but I think that there will still be disbelievers. In fact, I think that I would disbelieve a lot it myself.

We have to have the building block experiments. And I'm using building block in a different sense than Jim is. I have it in the notes here somewhere. We need building block experiments. We need building block analyses. The COMMIX code is a building block analysis for individual components.

There are a number of other analyses that are being performed that are building block analyses. We must compare our models to those to find the limitations to this approach.

PROF. KERR: But you would expect that a rather thorough examination of the code would indicate the kinds of experiments or the kind of experiment that one might eventually need.

DR. MEYER: You know, I don't know. I was thinking when you asked the question before about the pressurizer model what I would have answered on that one. I don't think I

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would have answered that. I don't think that I would have said how good is it?

I think what I would have done is say, here we have a code, a pressurizer model. And we followed a Shippingport transient within 20 psi. That's what we've done. We followed this other transient within 17 psi, and so forth.

What I'd like to have is a whole list of things of ways in which this code was compared to experiments and say, here it is. This is how well we can do it. Is this good enough?

If it's not good enough, then we can't help you. And I think it's very important for the user of the code to know how well you can do. I don't think that you can make the judgment. I think it depends on his needs whether 17 psi is close enough or not.

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PROF. KERR: I was faced with the question of whether this was good simulation. My question is how do you judge?

DR. MEYER: In response to that question, I wouldn't have judged. In response to a related question here, of what experiments are needed, what I think we need here is a whole range of experiments and a whole range of these building block things. To be able to say, This is what we can do, this is how we did it, and this is how we delineate the range of validity here, this way, rather than making a judgment ourself as to whether it's enough.

(Slide.)

The core model, then, we hope to make a very, very simple core model. It may be in many of these transients we can use one axial section and one path through the core. It means, though, that the user must supply us with certain information and that's the area in which my present, or up until a week and a half ago, in the water reactor version, was going to try to delineate the way in which information should be supplied for the core model and the range in which those might be valid.

Let me just make a couple comments. These are the areas in which -- these were outlined in the report that Jim handed out as possible areas of investigation. And I've done some of the work in there, and some of the work since

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that time, but let me just give you the results here. It looks like axial conduction in the core, which is a terrible thing to include in the calculations just from a computational standpoint — axial conduction in the core is probably unimportant down to about a tenth of a percent flow. So that's the conclusion of the axial conduction. That's a great simplification.

Buoyancy effect, I will discuss in just a moment. Heat transfer within the assembly, within an assembly, intra-assembly transfer is heat transfer, I think we can combine with the buoyancy effect in some of these subsidiary calculations. Transverse heat transfer between assemblies may be handled in these subsidiary calculations. There may be cases in which we just can't do it in this code, for example, to handle some of the individual assemblies in the XX08, or whatever it was, in the EBR-II, I'm not sure we can handle that very well within this code, the thermocouple response in that assembly.

But, okay, transverse heat transfer, I think we would not have within the code. We could have it in the imput calculations. One of the things, there are several analyses, and I guess some experimental background is, what are the buoyancy effects on local friction factors, because of local velocity changes within the core when we have a buoyant situation. What are the effects on local heat transfer coefficients? Those results, looking them over carefully, indicate that they will not be needed in the core, in SSC-S, if we would have cases in which the friction factor or the heat transfer coefficients are getting into the range where those effects are important. We would have boiling in the core, and when we get boiling it's just like in the water version, there's no good way of handling a lot of boiling in the core.

So that these friction factor effects need not be handled. So we have no axial conduction that need be handled, no local buoyancy-induced changes in friction factors or heat transfer coefficients, and the transverse heat transfer is simplified.

Buoyancy effect, then, we would like to have included in the model --

(Slide.)

--by user-supplied information of this kind. This involves some very simple calculations, hand-held calculator size, from 100 percent down to a tenth of a percent to show what happens from buoyancy effects alone between assemblies and this is sort of another representation in steady state of the kind of effect that Jim Guppy had displayed in one of his earlier slides. At full flow, a hot assembly, here I've chosen one which has an enthalpy rise above the core average enthalpy rise of 1.7.

You see, what happens is that it goes from a transition to that, so that it comes down the entire core. The low flow conditions has the same enthalpy rise, and the transition from the high flow condition to the low flow condition depends on, if it's a high enthalpy rise situation or a low enthalpy rise situation for various transitions. This, then, is a direct measure of the flow moving to the hotter regions.

Similarly, the lower curve here is a family, for the colder assemblies and the flow is being withdrawn from those colder assemblies moving to the hotter assemblies and therefore the enthalpy rise goes up. So out in this kind of region, less than about one percent flow, one has a whole core. They're all there at the same enthalpy rise.

That is also an encouraging simplification. In the distributed material, I also have a similar graph for within an assembly or intra-assembly buoyancy effects. I'm not going to go over that right now.

Now, the conclusions of that --

(Slide.)

-- I hope, and I hope it holds up under more scrutiny, is that the core representation in this code can justifiably be kept extremely simple, one axial section and that's a great percentage of the calculation time, is in the SSC-L within the core. This core model, then, is extremely

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simple.

But where do we put this effort back in? These are some of the items that really need greater attention, probably, than any transients that are already covered in SSC-L. Frank representation, then, first, to get the natural circulation flows -- we need to know density as a function of position around the entire loop, in order to get the driving of the natural circulation.

This takes some special demands. First, we have to know where the heat's taken out of the steam generator. The steam generators may be tens of meters high and where the heat is taken out of that steam generator makes a big difference. If it's taken out high, we'll get good circulation; if it's taken out low, we'll get bad circulation.

It makes a difference, now, what the transient behavior is. If we have an operation in which cold liquid is put on one side of the heat exchanger, removing a lot of heat, that cold liquid then falls down to the bottom of a low region in the core. That's a cold trap and the flow may just stop entirely. This kind of thing has to be studied. That's one of the things that this SSC-S code would be appropriate for, to delineate the types of maneuvers which would lead to cold trapping or to stop the natural circulation of flow.

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And another reason for the transient operation, you remember now, that transport times are on the order of hours here so we have a lot of time for this flow to get down there and stop, and so forth.

PROF. KERR: Will the one-dimensional capability permit one to raise the set of questions to which you refer?

DR. MEYER: That's a tough one. And that, in fact, is what I've called here 3-C, Stratification in Pipes. There is the phenomenon, of course, of hot water — hot coolant, sorry, I keep saying water — but hot sodium going into a pipe with cold sodium will tend to rise up over and move along. Similarly, if cold sodium goes into a pipe it would tend to dive under. If it's in a loop your cold fuel tends to stratify out of the bottom of that.

And I don't think we know the answer to that question. There was a page with six graphs on it that Jim gave before. That was one case in which it looked as though even though the detailed calculations showed stratification in that pipe, that the one-dimensional model gave probably a pretty good density picture in that horizontal pipe. We don't have, I think, though, a good answer to your question in general terms. That's one of the very important things we must do, is to decide when that's important and when it isn't important.

There may be some things one can do with that.

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Some of the water reactor people — I don't remember which code it is — but they tend to run two pipes, one for steam going this way and one for water going this way. Maybe we can do something like that.

This is a situation — I don't know. Plenum and pools, there are a number of questions there. When we talk about local effects in plenums and pools, we no longer have — the buoyancy effects here are very important. We have a cold jet coming into a hot pool. That cold jet will have negative buoyancy and tend to stop and not penetrate the same as if it were a high temperature jet.

So there are a number of plenum and pool treatments that have to be examined by the building block experiments, by building block analyses and compared to whatever techniques we have in these codes, the steam side slip and phase separations. This is important for two reasons, first, to know what the natural circulation flow is on the steam side of any steam generation equipment; secondly, to know where the heat is being taken out of that equipment, to know whether it's being taken out high or being taken out low.

So the same size slip and phase separation, in effect there are going to be special demands on it. I think the conclusions of the study so far — or my conclusions — are that the core needs only to be very simple; that these

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things are still an area that we have to study some more.

(Slide.)

I just have a few words to say about the comparison among the various LMFBR plants and PWR plants. First, there are a number of differences in plant component geometry, both from PWR to LMFBR and also within both of these categories. So there is a generality we need here.

I think that may be a very difficult one for us, because some of these conclusions on relative transport times and so forth may be different for different situations. Fluid properties, of course, are different, especially as we go from the LMFBR to the PWR plants. Enthalpy transport, I have some numbers here. They are sort of intereting. I don't have any conclusions based on these numbers.

(Slide.)

But these numbers now are ones that you've seen before for the Clinch River for the LMFBR. It's only the primary heat transport system. You'll remember that was about 75 seconds at full flow. This PWR is TMI-2 and the numbers are quite different. I don't know what that means, exactly. But we have to rethink it. The time spent within the reactor vessel, for example, is very, very different in the two situations, as I guess all of the numbers really are. We tend to have a lot slower creeping flow through the

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LMFBR and high velocity flow through the PWR situation, even though this is a smaller power facility than the Clinch River.

Let me just make one more comment here. I think I said I have done these calculations and made no conclusions about them, but I think the conclusions have to be made. We have to ask what this really means in terms of the modelling for natural circulation in the PWR system, to see whether we're missing something or not.

(Slide.)

The final illustration, then, is future work. By these A, B, C, and D, I don't mean we'll do A then B then C and D. In fact, work is going on in all these areas already in natural circulation. But we do need to further evaluate the existing SSC methods. For example, it would be very nice, I think, maybe very instructive to just take the existing SSC-L code and just run up the time step to 10 seconds, 15 seconds, or something like that. Some of these studies have been done, but more are suggested.

As we've seen, we do need great improvement in numerical methods and physical models. Maybe improvement in the numerical methods is not exactly right, but ultimate treatments of the numerical methods in order to satisfy these special demands. We must compare it to the building block experiments and the building block computations in

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some detail and these sort of all go together in order to delineate the range for validity of the method. And I think we have to keep that in front of ourselves and in front of potential users at all times.

Do I turn this back, Charlie, to you?

PROF. KERR: Questions?

(No response.)

PROF. KERR: Thank you.

DR. KELBER: I think there are no summary remarks. Are there, Jim?

PROF. KERR: Feel free to say no, if there aren't any.

DR. KELBER: I think that completes the discussion of SSC and its various aspects, and we're ready to go on now to a very brief update of what's going on in the world of NASAP.

PROF. KERR: Proceed.

DR. KELBER: I'd like to introduce Ron Foulds, who has principal responsibility in this area. We were very disappointed in, but understood the priorities, that caused the money that had been set aside for our very small research effort in this area to be diverted to support of TMI-2. The latest word we have is that the Congress has asked that a sum of $125,000 be directed towards the study of fuel cycle effects and that will be handled by NMSS. We will not mount any research program in this area.

MR. FOULDS: Thank you. I'd like to begin by pointing out that we've been engaged in some study and assistance on the NASAP and INFCE work for over a period of years. You may recall we've mentioned this to the ACRS one or more times before — and you may also recall that NRC is participating in INFCE and NASAP mostly from a technical review standpoint. NASAP is the DOE program intitiated in 1977 to assess non-proliferation alternative systems and INFCE is the International Fuel Cycle Evaluation, which was convened in October of '77 among 20-some nations which have subsequently grown to about 53 countries.

The INFCE program was planned to pool the member nations' technical capabilities and develop some consensuses on need for improvement of physical resources and identification of the more proliferation resistant fuel cycles, all within two years. The final meeting of INFCE is now scheduled for February of 1980. DOE plans to complete a report this year on NASAP system assessments and conclusions to be presented to the president and Congress in December.

The NRC has contributed some to this DOE effort. You likely recall that the INFCE organization was developed around eight different working groups as shown here.

(Slide.)

Also, of course, there's a crosscutting technical

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coordinating committee. NRC has participated in most of these areas, these eight areas here, and our Office of Advanced Reactor Safety Research is participating in working group eight. Some details of our effort in the NASAP and INFCE structures have been given to the ACRS in section three of the original and recently updated report by ARSR on the status of advanced reactor safety research.

The INFCE working group eight has focused on three areas.

(Slide.)

Once through reactor fuel cycles, and those are primarily LWRs, HWRs and HTGRs, advanced reactor and fuel cycle concepts, and those are again, primarily LWRs, but the advanced are stretched out fuel concepts, spectral shift control reactors, HWR again, and HTGRs. And then the third section is research reactors.

The United States, Rumania and South Korea have co-chaired this working group, with participation of more than 20 other countries. The final report for this working group is nearly complete.

Reports of other working groups are also very far along. For example, we have heard that working group five's report apparently predicts the installation of some 50 breeder reactors in the world by the year 2000. In general, however, since all INFCE reports are "for official use only" and none are to be released until the final reports are published, the NRC is to limit its discussion of the INFCE details.

Turning to the NASAP effort, it's been and continues to be our intent to provide input to the DOE process of assessing the alternative systems, primarily from a safety and licensing standpoint. ARSR is working closely with NRR on this effort. The schedule developed earlier this year --

(Slide.)

-- is shown here. Now, due to not receiving all the information we had anticipated from DOE as early as indicated on this schedule, plus a stretch-out of our technical capability and resources with Three Mile Island and some other things, why, we provided our comments to DOE in June, only one set of comments, whereas here it indicated that there would be two sets of comments. That's been negotiated out. We haven't received results back from them, and I believe we won't be providing any more comments until their final report, which is now schedules for December of this year.

2-19

The reactor systems to be investigated by DOE and NRC are shown on this slide.

(Slide.)

DOE provided to NRC what are called "PSEIGs," or preliminary system and environmental documents. This sort of thing. Seven volumes of these, plus additional volumes, supporting materials. Each one of these discusses one of these main reactor types.

On here, it indicates that LMFBR, for example, has six variants. That's now been increased to 15. Some of the others have been increased, too, in the number of variants that DOE would like to have set.

NRC is interested in providing comments on these, but it stretches our recources considerably.

The NRC comments have drawn largely on available NRC technical base as accumulated through our own technical expertise, various technical assistance programs of NRR, and the research programs. Since we have already had significant programs under way in LWRs and LMFBRs and HTGRs, the NRC could make the most useful comments in these areas. For the other three concepts, HWR, LWBR -- that's the light water breeder reactor -- and the GCFR, the NRC has less experience. So, we developed small research programs for these concepts as shown here.

(Slide.)

The performance of these programs is consistent with the congressional intent embodied in the budget legislation. Most of it was worked out with NRR personnel.

We received a user request from the NRR on the first three of the four programs. The programs were designed specifically to supply technical assistance to NRR and experimental and to provide information for DOE PSEIGs. The programs were canceled the first of June in order to make the funds available for the Three Mile Island efforts.

The program briefs for these efforts, though, are included in section 3 of our updated report to you.

The current efforts of NRC in all these alternative systems, assessment areas -- that's NASAP, INFCE, and some others -- are being summarized in a report now being prepared. I heard a rumor that you had a copy of the draft. I am not sure whether you do. But in any event, it's soon to be issued. And they're being summarized in that draft, and that's a draft of the submittal to the President and Congress that we are required to prepare. The authorizing legislation for the fiscal year requires a semiannual reporting each of 1979 and 1980, and an annual report in '81 and '82. So, this report is the first of what should be a series of six.

(Slide.)

In summary, our current activities include participation in INFCE working group eight activities, commenting on research needs involved in INFCE proposals, monitoring NASAP activities, and contributing to comments on alternate system proposals.

We've scratched the surface in designating the types of research programs that would be required, and I indicated that to you a month ago. But what I would indicate here is deferred are things that we could do or would expect to be doing, and that's evaluating in some detail the research needs for specific proposals, scoping out tentative reactor safety research programs, and then evaluating the anticipated cost of these various needed safety research programs.

You may ask where all this is headed and what will come of all this. Hopefully, there will be some consensus in the world on the use of nuclear energy in the future. INFCE, by being completed by early 1980, may lay some groundwork for new negotiations under the NPT -- that is, the nonproliferation treaty -- scheduled for review in the spring of '80. On the other hand, there may be some polarizing effect from INFCE. Last week, Nucleonics Week, for example, indicated the USSR, the U.S., and France are still disagreeing over the need for breeders. Likely both, however, those who are in favor and those who are opposed to breeders will find facts in the INFCE reports to support their positions.

In conclusion, I would say that over at ARSR in the Willste Building, we remain prepared to carry out the research technical assistance programs that we have designed for the

HWR, LWBR, and GCFR concepts.

Do you have any questions?

PROF. KERR: Would you say that the INFCE and the NASAP and ORNISAP programs have had any influence on the ARSR program up to date?

MR. FOULDS: In what way? I am not sure what you mean. If you mean has it had an influence on the manpower or if you mean --

PROF. KERR: No. Presumably, these are studying possible fuel cycles in reactors of the future. The advanced reactor safety research is supposed to be aimed at reactors of the future. One might assume that there would be some relationship between the two. I am asking: Is there?

MR. FOULDS: I believe so. Yes. Dr. Kelber wants to make a comment.

DR. KELBER: There is a very modest amount of benefit from what I consider a sheer waste of taxpayers' money. One of the results of a NASAP-related study which probably would have been generated anyhow is that if you very carefully optimize a mixed oxide core of the traditional design but consider larger pin diameters than we have in the past, you can project a system doubling time of somewhere under 12 years. That's a valuable thing to know, because I had, for example, along with a number of others in France and the Soviet Union, long felt that the mixed oxide system would not be a candidate for eventual economic use. If the use of the larger-sized pins appears to be feasible on the basis of pin burnup and safety, then I think that this system stands a very good chance, primarily because of the vast amount of experience and therefore predilection among the designers for the use of the oxide pin.

The alternative system for eventual economic use, according to the best information I have from the Soviets, who have done the most thinking about this, is the metal fuel system, and that, too, was evaluated in NASAP studies. And, of course, it continues to look good.

But you must remember that this is a field in which rationality has had very little play, and I don't know how that will turn out. I think that what we will do, as supplies become available, as our codes come on line, in particular, SSC and COMMIX, is that we will start to take a look at the implications of going to a somewhat larger pin, because these safety implications may have a great deal -- the safety implications I would be looking for are the t. es required for various system actions.

PROF. KERR: But at least for the time being, advanced reactors, to you, will continue to be liquid metal fast breeder reactors?

DR. KELBER: A gas-cooled fast reactor ha certain attractive features, and it's still a conceivable competitor, but the funding isn't there. The practicality is not there.

If you expect these reactor systems to penetrate the market commercially somewhere in the first or second decade of the next century, that means a large number of plants being ordered per year, perhaps on the order of 10 or 20 per year. Then you have to get started building the first demonstration somewhere in the next few years, if you are to be in the race at all.

The Administration has forecast decision dates somewhere after the next election or after the election after that. I think that's a convenient time. It's a little late. I doubt that we will have sufficient resolution of major safety issues to answer all these questions by the time the first demonstration plant does come in, if we are to stay seriously in the race.

I must say that I await with some interest the statements that will be made by the French designers who, right now, are in the forefront, concerning their trend. I would like to know how they feel about larger pins. I have seen no influence in the CABRI program towards testing larger-sized diameter pins. And I think their views on this will be important. So far, they have been content with designing what can only be described as a very good converter, Superphenix. It's probably the world's best converter, and may even actually breed some fuel in the sweet by-and-by, but nobody really counts on it.

So, that's the real influence, is the consideration of larger mixed oxide pins. Carbide, I think is falling into disfavor except in isolated segments of the technology. Most people do not seem to be looking very hard at carbide. Perhaps our own tests had something to do with that.

PROF. KERR: Are there questions, comments?

(No response.)

PROF. KERR: Thank you.

Do you have any concluding remarks, Mr. Kelber, before we go into executive session?

DR. KELBER: I shall be very brief.

The three laboratory efforts discussed here today relate to our entire program in the following sense:

As you know, our program is balanced between issues related to plant safety, and this is reflected very largely in the SSC and COMMIX codes, and the activities associated with them. Those activities are the mainstay of that aspect of our work. To some extent, the work at Argonne on fuel failure mechanisms represents the bridge between that work and the work we are doing on the core disruptive accidents.

Nevertheless, the major focus of the work at Argonne is not on what is the progression of fuel failure, but really is on defining the types of ramp rates and the relationship between the reactivity feedbacks and other core characteristics, and the ramp rates that might be expected in a core melt accident.

In this respect, therefore, the major thrust of that work is towards the core disruptive accident phase of our program. It is important work in the sense that it is an opportunity to make an extremely detailed and checked comparison between knowledgeable groups of the same problem.

One immediate outcome is renewed emphasis on what we call the "FD-2" series of tests at Sandia. As you may recall, these are the tests in which short pellet of clad irradiated fuel is photographed as it melts down under a burst of irradiation. We are now preparing a stage 2 capsule to handle a longer pin and do a better job of photography. Those experiments are almost ready, and we would hope that if you project a meeting for later this year at Sandia that we would have some of those experiments under our belt by that time.

A key question that was raised is the nature of the extension of the rib in the cladding in such failures, and these tests should give accurate visual records of such cladding rib extension if it does occur. Naturally, of course, everybody would like to do this on a full-length pin; that has to wait for the future. But at least these tests should give us some indication of how cladding does fail.

There are other problems being analyzed by these groups. We had considerable discussion of gap conductivity. We are not isolated in this work. This has been of some concern to the analysts reviewing the CABRI program that's trying to make predictions for it. And as a result of that, there has been a discussion of using a dynamic technique to measure the conductivity of the gap. I do not know how well that will work out. We can presumably discuss that at the next CABRI meeting, which is coming up October 3 and 4.

The work on the ART program at Oak Ridge is a major portion of the part of our program oriented towards containment problems. I believe we have made substantial advances in the treatment of aerosol transport through the containment. That program is winding down as we go through the text matrix.

There is a new area of work that is very similar to the area of work being considered now in the LWR program; namely, what is the radiologic source associated with core melt itself. And I do not as yet know what resources we will have to address that problem.

The major thrust of the programs that were not presented today -- namely, the programs at Los Alamos and Sandia -- is in two parts. By far and away, the greatest effort is to the analysis of the core disruptive accident and the associated mechanisms of fuel failure and dispersal that play such a key role in that accident. This includes some experiments related to the thermal hydraulic experiments that Brookhaven discussed today.

We are also doing experiments on ablation and using some of the special techniques developed at Sandia in conjunction with parts of the weapons program, because ablation is an important program.

The post-accident heat removal program at Sandia is an extensive program which has been delayed very much by budget constraints. We have received very active expressions of interest from ISPRA on behalf of the European community to enter into a cooperative agreement with them. Some draft memoranda of understanding have been exchanged. There will be further discussions later this month. And we hope that by early October we can have an agreement in principle on fund-sharing. There has been some expression of interest by the Japanese, and as their budget goes through the budget-approval process, we expect that, too, will come to fruition.

So, that program which centers on the debris bed tests looks like it may become multinational.

I think that puts what you have heard today in the context of our complete program, and I would like to conclude my formal remarks with that.

PROF. KERR: Thank you, Charlie.

Any further comments or questions?

(No response.)

PROF. KERR: I declare the meeting not adjourned, but we are going into executive session, which is still open but is not recorded.

(Whereupon, at 5:25 p.m., the meeting was recessed, to go into executive session.)

end#20

NRC AEROSOL RELEASE AND TRANSPORT (ART)

UPDATE

PRESENTATION BY
MEL SILBERBERG
TO ACRS WORKING GROUP
ON ADVANCED REACTORS
AUGUST 7, 1979

NRC AEROSOL RELEASE AND TRANSPORT (ART)

OBJECTIVE

- o PROVIDE DATA / VERIFIED METHODOLOGY FOR RADIOLOGICAL CONSEQUENCE ASSESSMENT

SCOPE

- o SOURCE TERM - RADIONUCLIDE RELEASE FROM PRIMARY SYSTEM
  - CDA
  - CORE MELT ("FUTURE")
- o TRANSPORT - AEROSOL BEHAVIOR IN CONTAINMENT
  - AEROSOL MODELLING / SEPARATE EFFECTS
  - AEROSOL CODE VERIFICATION

NRC ART PROGRAM

STATUS / ACCOMPLISHMENTS - SOURCE TERM

- FAST FACILITY COMPLETED FOR CDA BUBBLE SOURCE TERM (ORNL)
  - SHAKEDOWN / CALIBRATION TESTS - UO_2 VAPORIZATION UNDER WATER
  - INSTRUMENT QUALIFICATION TESTING
  - QUANTITATIVE ASSESSMENT OF EXP. UNCERTAINTIES AND KEY MODEL PHENOMENA PRIOR TO NA TESTS

- INITIAL CDV SOURCE CHARACTERIZATION TESTS COMPLETED IN CRI-III (ORNL, SANDIA)
  - PRIMARY AEROSOL CHARACTER (ACPR VS. CDV SAME ENERGY LEVEL)
  - TEMPERATURE DIST. CHARACTERIZATION OF FAST FUEL PIN

NRC ART PROGRAM

PLANS - SOURCE TERM

- COMPLETE FAST TESTS WITH WATER (79 / 80)
- COMPLETE CDV FUEL TEMP. CHARACTERIZATION (79 / 80)
- TEST ACOUSTIC SYSTEM FOR BUBBLE DIAGNOSTICS (80)
- INITIATE FAST TESTS WITH NA (80)
- CONTINUE SOURCE TERM MODEL IMPROVEMENTS

NRC ART PROGRAM

STATUS / ACCOMPLISHMENTS - TRANSPORT

AEROSOL MODELLING / CODES (BCL)

- o TRANSPORT REFERENCE CODE (CRAB) USER MANUAL RELEASED
- o CRAB / HAARM-3 COMPARISON IN PROGRESS
  - TEST PARTICLE-SIZE DISTRIBUTION RESTRICTION IN HAARM-3

AEROSOL PROPERTY MEASUREMENTS (BCL)

- o DYNAMIC SHAPE FACTOR
  - UO_2 COMPLETED
  - NA - UO_2 MIXTURES IN PROGRESS

NRC ART PROGRAM

STATUS ACCOMPLISHMENTS - TRANSPORT

AEROSOL CODE VERIFICATION

- o HAARM-3 VERIFICATION PROCEDURE RELEASED FOR PEER REVIEW (BCL, ARSR)

AEROSOL TRANSPORT TEST (ORNL)

- o MINIMUM NSPP COAGGLOMERATION TEST MATRIX DEFINED
- o HIGH MASS RATE FUEL AEROSOL GENERATOR DEVELOPED
- o FIRST HIGH MASS CONC. $NA_2 O_x$ - FUEL AEROSOL TEST COMPLETED
- o AEROSOL INSTRUMENT ERROR ASSESSMENT IN PROGRESS (WITH CSNI EXPERT GROUP)

NRC ART PROGRAM

PLANS - TRANSPORT

- o COMPLETE HAARM-3 / CRAB SIZE-DISTRIBUTION COMPARISONS (80)
- o COMPLETE AEROSOL PROPERTY MEASUREMENTS (ADD STEEL) (80)
- o ASSESS CONTAINMENT MIXING, MULTIPLE SPECIES (80)
- o COMPLETE NSPP COAGGLOMERATION MATRIX (80)
- o COMPLETE AEROSOL CODE VERIFICATION FOR CDA (81)
  - U.S. DATA
  - FOREIGN DATA

ART PROGRAM INTERFACES - FOREIGN ACTIVITIES

- o OECD - CSNI (8 COUNTRIES)
  - STATE-OF-ART REPORT (10 / 79 PUBLIC.)
  - CRITICAL REVIEW AEROSOL INSTRUMENTS
- o FRG*
  - AIR CLEANING EQUIPMENT
  - SOURCE TERMS
  - LARGE SODIUM FIRES (FAUNA)
- o FRANCE
  - HCDA BUBBLE (EXCOBULLE)
  - HAARM-3 SUPER PHENIX SAFETY REVIEW
  - FULL-SCALE SODIUM FIRES (ESMERALDA)**
- o UK
  - AEROSOL CODES

*COLLABORATION DETAILS UNDER DISCUSSION

**AWAITS BROAD EXCHANGE AGREEMENT

8

ACRS REPORTS - ART PROGRAM

- o 1978 REPORT TO CONGRESS (NUREG - 0496)
  - ART PROGRAM CAN PROVIDE IMPORTANT INPUT TO DESIGN OF FILTERED / VENTED CONTAINMENT
  - SUITABLE PROGRAM TO BE CONTINUED WITH THIS OBJECTIVE IN MIND
  - CAN PROVIDE DATA FOR ASSESSING FULL RANGE OF RELEASES

- o 1979 COMMENTS ON NRC RESEARCH BUDGET
  - WORK SEEMS WELL PLANNED AND IS PRODUCING RESULTS

ASPECTS OF INTERNATIONAL COOPERATIVE STUDIES OF VALUE TO OUR PROGRAM

- EXCHANGE OF IDEAS ON MODELING (E.G., PIN FAILURE) WHICH IS VERY FRUITFUL WHEN ARISING OUT OF DETAILED COMPARISON CALCULATIONS ESPECIALLY WITH THE EUROPEAN COUNTRIES WHICH HAVE EXTENSIVE SAFETY EXPERIMENTAL PROGRAMS AND WHICH ARE IN THE PROCESS OF BUILDING LMFBR'S.

- BROADENING OUR PERSPECTIVES ON REACTOR DESIGN WITH RESPECT TO SAFETY MODELING (E.G., BOTTOM FISSION GAS PLENUM AND ITS EFFECT ON VOIDING, POOL-TYPE REACTORS, LOW SMEAR-DENSITY FUEL PINS).

- RECOGNITION OF DEGREE OF SENSITIVITY OF RESULTS TO VARIOUS PARAMETERS IN SAFETY CALCULATIONS (E.G., CLAD RIP EXTENSION DURING TRANSIENT, MODE OF PIN FAILURE).

- GREATER UNDERSTANDING OF AREAS WHERE MODELING IMPROVEMENTS ARE NEEDED (E.G., DISASSEMBLY).

- VERY OFTEN THE NEED FOR MODELING CERTAIN EFFECTS OR IMPROVING MODELING IS NOT VERY APPARENT UNTIL SOME KIND OF DETAILED COMPARISON CALCULATIONS ARE DONE.

US/UK LOF

- WITH (80%) AXIAL EXPANSION FEEDBACK AND WITHOUT (FEEDBACK WORTH ~-1$)
- NO CLAD MOTION OCCURRED BEFORE PIN FAILURE
- NON-LOF-TOP VOIDING RAMP RATES LOW (10-15$/SEC)
- ONLY VERY SMALL POTENTIAL FOR LOF-TOP CONDITIONS IN CASE WITH AXIAL EXPANSION FEEDBACK (~60% OF CORE VOIDED)
- SIGNIFICANTLY GREATER POTENTIAL FOR LOF-TOP WITHOUT AXIAL EXPANSION FEEDBACK
- 260 × NOMINAL POWER WITH EXPANSION FEEDBACK, 770 × WITHOUT (BOTH CASES WITHOUT LOF-TOP)

US/UK LOF

- PROBLEM: SHOULD A PARTICULAR SAS CHANNEL BE TREATED WITH A SLUMPY-TYPE MODEL OR AN EPIC-TYPE MODEL: THIS IS CURRENTLY THE ONLY CHOICE.
- SAS4A AND FRAM WILL IMPROVE THIS SITUATION.
- EPIC PRESUMES PIN-GEOMETRY AND STRUCTURE GENERALLY MAINTAINED.
- SLUMPY ASSUMES PIN STRUCTURE IS DESTROYED, FREE MOTION OF HOMOGENIZED FUEL AND CLAD AND ABSENCE OF SODIUM.
- NOT SIMPLY VOID FRACTION BUT CLAD STRENGTH IS DETERMINING FACTOR, WHICH IS MAINLY DETERMINED BY VOIDING HISTORY.
- POTENTIAL FOR LOF-TOP MUST BE DETERMINED ON A CHANNEL-BY-CHANNEL BASIS.
- CALCULATIONAL RESULTS ARE VERY SENSITIVE TO FAILURE POSITION AND MODE OF FAILURE.
- LARGE MODELING UNCERTAINTIES IN TREATING LOF-TOP PIN FAILURES IN PARTIALLY VOIDED CHANNELS.

US/UK LOF

- MELT FRACTION FAILURE CRITERION ASSUMED.

- LOF-TOP PINS REACH HIGH MELT FRACTIONS ON A VERY FAST TIME SCALE IN THESE CALCULATIONS.

- THEREFORE IT WOULD SEEM THAT FISSION GAS WOULD STAY IN THE FUEL AT THE LOCATION OF RELEASE.

- IF NODE-BY-NODE PRESSURE CALCULATION IS USED, FAILURE WOULD STILL BE PREDICTED AT NODE OF HIGHEST MELT FRACTION IN LOF-TOP PINS.

- THIS IS DUE TO FISSION-GAS RELEASE AT FUEL MELTING IN A FAST TRANSIENT.

- THE ASSUMPTION IS MADE THAT FISSION GAS CONTAINED IN FUEL AT FUEL MELTING IS IMMEDIATELY AVAILABLE FOR PRESSURIZATION.

- ALL THIS LENDS CREDIBILITY TO FAILURE AT NODE OF HIGHEST FUEL MELT FRACTION, TYPICALLY AT 60% OF CORE HEIGHT, MAKING LOF-TOP PIN FAILURES AT LEAST CREDIBLE.

US/UK LOF

- In spite of 60% of core height failure, neutronic energy deposition no more than non LOF-TOP case if failure extension allowed axially along the pin.

- If no extension allowed, neutronic energy deposition can increase significantly with a 60% of core height failure.

- LOF-TOP failures increase sodium voiding ramp rates to ~100$/sec but fuel motion via sweepout can cause negative fuel reactivity ramp rates of several hundred $/sec in the absence of fuel motion toward center of core in the cases where axial exension of failure occurs.

- However, calculation of sweepout in EPIC doesn't take into account plateout and plugging. This is a future modeling effort.

- Results of US LOF-TOP calculation quite different than that of UK since UK doesn't calculate fuel motion reactivity feedback after pin failure.

US/UK TOP

- 3¢/SEC RAMP
- FIRST PIN FAILURE AT ~85 SEC, ~5 SEC AFTER BOILING INITIATION
- 30% MELT FRACTION TOP OF CORE, 70% MAX
- PIN FAILS AT TOP OF CORE, PRESSURE AVERAGED OVER CORE
- -0.50$ REACTIVITY EVEN AFTER EXPULSION OF LOWER SLUG
- REENTRY OF SODIUM COULD ONLY INCREASE NEGATIVE REACTIVITY
- FURTHER PIN FAILURES CAUSED BY FURTHER ROD WITHDRAWAL WOULD LEAD TO MORE NEGATIVE REACTIVITY
- UK CALCULATION PREDICTS PROMPT BURST IN SPITE OF TOP OF CORE FAILURE BECAUSE OF NO FUEL MOTION FEEDBACK IN FRAX AND POSITIVE FEEDBACK FROM SODIUM VOIDING

ONE-PIN BOILING AND CLAD MOTION RESULTS

- NO CLAD MOTION OCCURRED IN OUR WHOLE-CORE LOF CALCULATIONS. ALTHOUGH THERE WAS ONLY $3.3 SODIUM VOIDING REACIVITY AVAILABLE, COHERENCE IN POWER AND IN POWER/FLOW PRODUCED LARGE POWER RISE IN 2.0 SEC AFTER BOILING, NOT ENOUGH TIME FOR CLAD MOTION.

- AUXILIARY ONE-PIN CALCULATIONS INDICATE THAT CLAD MOTION WOULD HAVE STARTED IN 0.5 SEC IF THE POWER HAD LEVELED OFF AT ABOUT 6 TIMES NORMAL.

- THESE CALCULATIONS ALSO INDICATE THAT BOILING AND CLAD MOTION TIMES ARE NOT VERY SENSITIVE TO DIFFERENT ASSUMPTIONS FOR GAP CONDUCTANCE HELD CONSTANT OVER THE TRANSIENT.

FUEL PIN FAILURE CALCULATIONS

Fission Gas

- Failure from fission gas pressure only mechanistic model currently available in SAS/EPIC. This mechanism also responsible for failure in FRAX (UK) TOP calculations.

- Uniform axial pressurization assumed in both cases for 3¢/sec TOP. Leads to failure at core top where clad is weakest.

- Inconsistencies between US and UK in steady-state gas retention, transient gas release, transient release to plenum, cesium pressure. Overall pressure at given fuel melt fraction not too different. Detailed comparison still under way.

Differential Expansion

- Additional studies were made of possible pin failure by solid fuel expansion loading clad.

- This mode of failure had potential for axial center-line failure because the gap may be smallest there. However, molten fuel ejection may not occur immediately because fuel melt fraction tends to be low in failure of this type.

- This type of initial failure may not extend during the transient because the modes of initial and subsequent failures are different.

DESIRED IMPROVEMENTS IN OUR PIN FAILURE MODELING

- MODEL LOADING OF CLAD BY SOLID FUEL EXPANSION.
- IMPROVEMENT OF TRANSIENT FISSION GAS RELEASE CALCULATION FROM BOTH SOLID AND MOLTEN FUEL. MOVEMENT OF FISSION GAS IN PIN AFTER RELEASE.
- NEED BETTER DATA ON IRRADIATED CLAD PROPERTIES. LACK OF DATA ON NEW LOW-SWELLING ALLOYS WILL BE A PROBLEM.
- NEED BETTER ESTIMATES OF INITIAL GAP SIZES - FAILURE BY MECHANICAL LOADING VERY SENSITIVE TO THIS.

WAC - ROUND 1

LOF

- 12$/SEC VOIDING RAMP AT DISASSEMBLY
- NO CLAD MOTION OR FUEL EXPANSION
- NO LOF-TOP ALLOWED FOR BOILING CHANNELS
- MILD DISASSEMBLY CONDITIONS
- QUITE SIMILAR RESULTS FROM ALL PARTICIPANTS

WAC - Round 1

TOP

- 1$/SEC
- 12 GM FUEL IN CHANNEL - CHO-WRIGHT, HEAT TRANSFER
- MANY MODELING LIMITATIONS IMPOSED BECAUSE OF GROUND RULES OF STUDY WHICH LEAD TO CONDITIONS VERY DIFFERENT FROM OUR "BEST ESTIMATE" OF THIS CALCULATION.
- NO FUEL REACTIVITY FEEDBACK BEFORE DISASSEMBLY
- FUEL IN MOTION AT DISASSEMBLY SWITCHOVER POINT CONTRASTING WITH OTHER CALCULATIONS, AND GIVING IMMEDIATE NEGATIVE REACTIVITY
- CONTINUOUS CALCULATION OF NA VOID BEYOND SWITCHOVER
- PRESENT EPIC MODELING UNABLE TO CONTINUE THROUGH DISASSEMBLY
- SAS3D/EPIC MODELS DISASSEMBLY CONDITIONS QUITE DIFFERENTLY THAN OTHER 2-D CODES
- SAS3D/EPIC DOESN'T CALCULATE SINGLE-PHASE PRESSURES LIKE THE OTHER CODES

WAC - ROUND 2

- 10¢/SEC TOP
- BOC CORE, 2 B.U. STAGES
- NO RESTRICTIONS ON MODELING AS IN ROUND 1
- FUEL RESTRUCTURING DATA WILL BE PROVIDED BY SAS3D AND WILL BE CONFORMED TO BY ALL PARTICIPANTS
- EPIC'S CALCULATION OF FCI CONDITIONS AFTER FAILURE MAY GIVE OTHER CODES VALUES FOR THEIR FCI PARAMETERS
- AT PRESENT CONDITIONS ARE BEING COMPARED FOR ALL CODES AT 60% FUEL MELT FRACTION, 60% OF CORE HEIGHT

MODELING NEEDS OF SAS3D/EPIC

- BETTER MECHANISTIC MODELING OF CLAD FAILURE
- INADEQUATE TREATMENT OF EJECTION OF FISSION GAS WHEN CLAD IS WEAK AND THERE IS LITTLE OR NO MOLTEN FUEL
- INADEQUATE TREATMENT OF MELT-THROUGH TYPE FAILURES IN PRESENCE OF SODIUM (IN THE CASES OF PRE-FAILURE EJECTION OF FISSION GAS OR FRESH FUEL FAILURE)
- NEED FOR THERMAL EXPANSION OF FUEL AND CLAD AFTER FAILURE (SIGNIFICANT FOR PRESSURE CALCULATION)
- NEED FUEL CONDUCTION (FOR SLOW TRANSIENTS) AND CLAD TEMPERATURE CALCULATION (FOR CLAD FAILURE EXTENSION AND DISASEMBLY)
- NEED CALCULATION OF PLATEOUT AND PLUGGING
- CODE IS INADEQUATE FOR LONG TRANSIENTS WHERE PIN GEOMETRY IS LOST
- IMPROVED TREATMENT NEEDED OF LATER STAGES OF A RAPID TRANSIENT (DISASSEMBLY CONDITIONS) TO THE POINT OF NEUTRONIC SHUTDOWN

DISASSEMBLY WITH SAS3D/EPIC

- ASSUMPTION THAT ONE-DIMENSIONAL TREATMENT IS ADEQUATE
- CLAD: CAN IT RESTRAIN FUEL, INSULATING IT FROM SODIUM?
  WHEN AND HOW DOES THE CLAD FAIL?
  HOW DOES A CLAD FAILURE EXTEND ITSELF AND HOW RAPIDLY?
  WHAT EFFECTS WILL PREVAIL AND WHAT CAN BE DISCOUNTED BECAUSE OF VERY SHORT TIME SCALE?
- SODIUM: COULD IT POSSIBLY BE PRESSURIZED IN A LOW VOID FRACTION CHANNEL AND BE EXPELLED RAPIDLY?
  WHAT IS POTENTIAL FOR FCI EVEN ON A SHORT TIME SCALE?
  WHAT IS POTENTIAL FOR EFFICIENT MIXING FROM THE POINT OF VIEW OF WORK ENERGY EVEN AFTER NEUTRONIC SHUTDOWN?
- FUEL: WHAT IS THE EFFECT OF FUEL MOTION IN THE CHANNEL?
  HOW DOES FUEL MOVE IN THE PIN UNDER FUEL VAPOR, FISSION GAS OR FILL GAS PRESSURE?
  THERMAL EXPANSION VERY IMPORTANT NOT ONLY FOR PRESSURE CALCULATION BUT FOR DIRECT LOADING OF CLAD.
- THIS TREATMENT VERY DIFFERENT FROM CURRENT TWO-CODE METHODS.

COMMIX AND BODYFIT
COMPONENT COMPUTER PROGRAMS

CONTRIBUTORS

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C. C. MIAO, W. T. SHA AND V. L. SHAH

PRESENTED BY

WILLIAM T. SHA
ANALYTICAL MODELLING SECTION
COMPONENTS TECHNOLOGY DIVISION
ARGONNE NATIONAL LABORATORY

AT
ACRS MEETING ON AUGUST 7, 1979
AT
WASHINGTON, D.C.

689282

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OBJECTIVES:

- TO DEVELOP 3-D, TRANSIENT CODES FOR RIGOROUS THERMAL-HYDRAULIC ANALYSIS OF REACTOR COMPONENTS UNDER NORMAL AND OFF-NORMAL OPERATING CONDITIONS WITH EMPHASIS ON THE NATURAL CIRCULATION MODE.

- TO COMPLEMENT THE SSC CODE

689283

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
- FORMULATION:

  POROUS MEDIUM APPROACH - VOLUME POROSITY, SURFACE PERMEABILITY AND DISTRIBUTED RESISTANCE AND HEAT SOURCE

- NUMERICAL TECHNIQUE:

  STAGGERED MESH

  IMPLICIT MULTIFLUID (IMF) SCHEME WITH REBALANCING TECHNIQUE

- APPLICATIONS:

  CONTINUUM: REACTOR INLET AND OUTLET PLENUM, PIPING, ...

  QUASI-CONTINUUM: FUEL ASSEMBLY, IHX, STEAM GENERATOR, ...

689284

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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

  IMPLICITY MULTIFLUID (IMF) SCHEME WITH REBALANCING TECHNIQUE

- APPLICATIONS:

  CONTINUUM: FUEL ASSEMBLY AS MULTIPLY CONNECTED REGION

FUEL ASSEMBLY MODELLING

- HELICAL WIRE WRAP AND GRID SPACERS
  - FORCES
  - THERMAL
- FUEL ROD THERMAL MODEL
- DUCT WALL THERMAL MODEL
- FILLER WIRE THERMAL MODEL
- GAMMA HEATING
- ONE EQUATION TURBULENCE MODEL

NUMERICAL RESULTS OF COMMIX-1

689287

FUEL ASSEMBLY SIMULATIONS

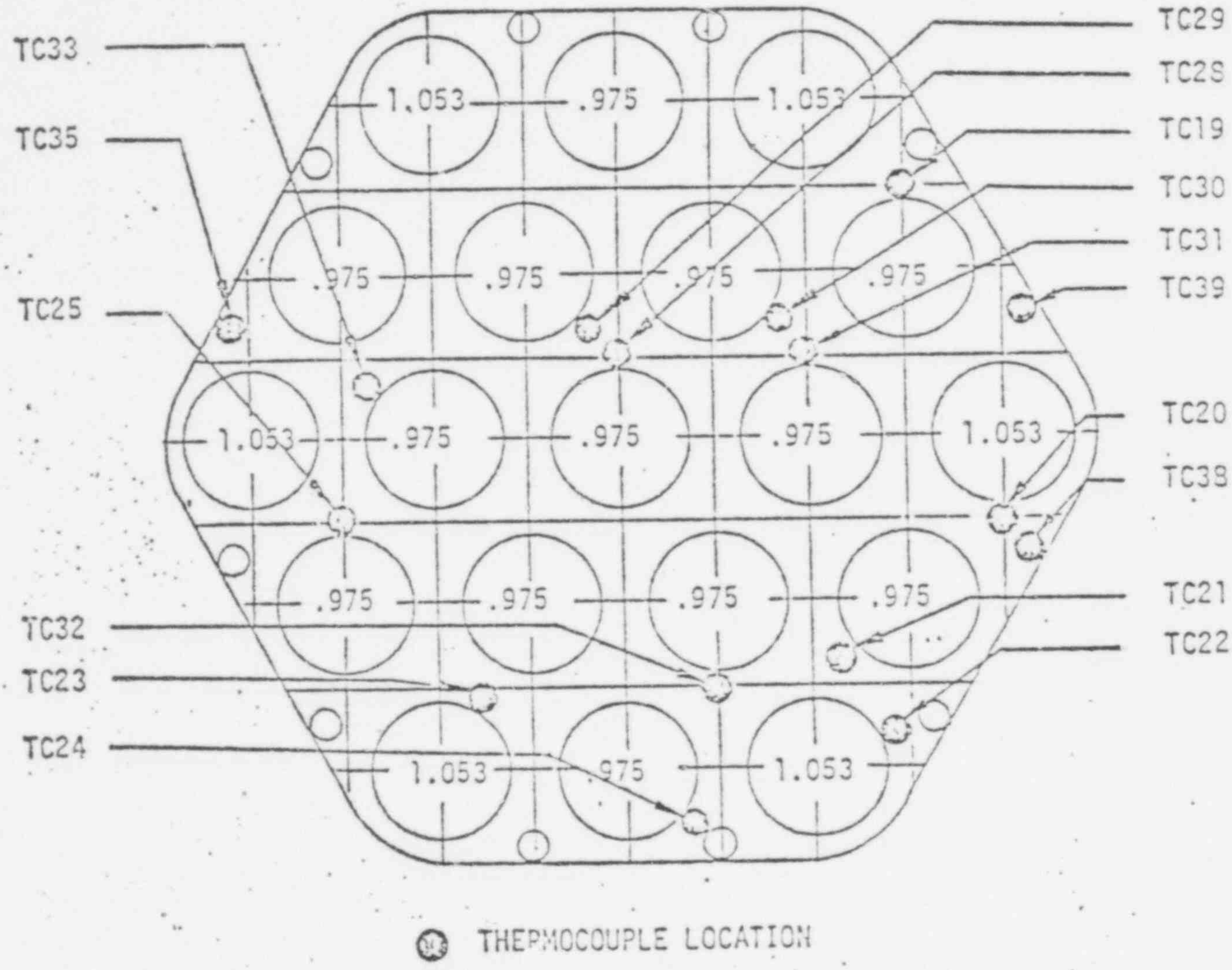
- SLSF P2 LOPI TRANSIENT (FAST TRANSIENT)
- SLSF P2 NATURAL CIRCULATION TRANSIENT
- GERMAN FLOW RUNDOWN TRANSIENT IN 7-PIN BUNDLE (SLOW TRANSIENT)
- W1 LOPI PRETEST PREDICTION

P2 LOSS OF PIPING INTEGRITY TRANSIENT IN 19 PIN BUNDLE

INITIAL POWER = 730 KW

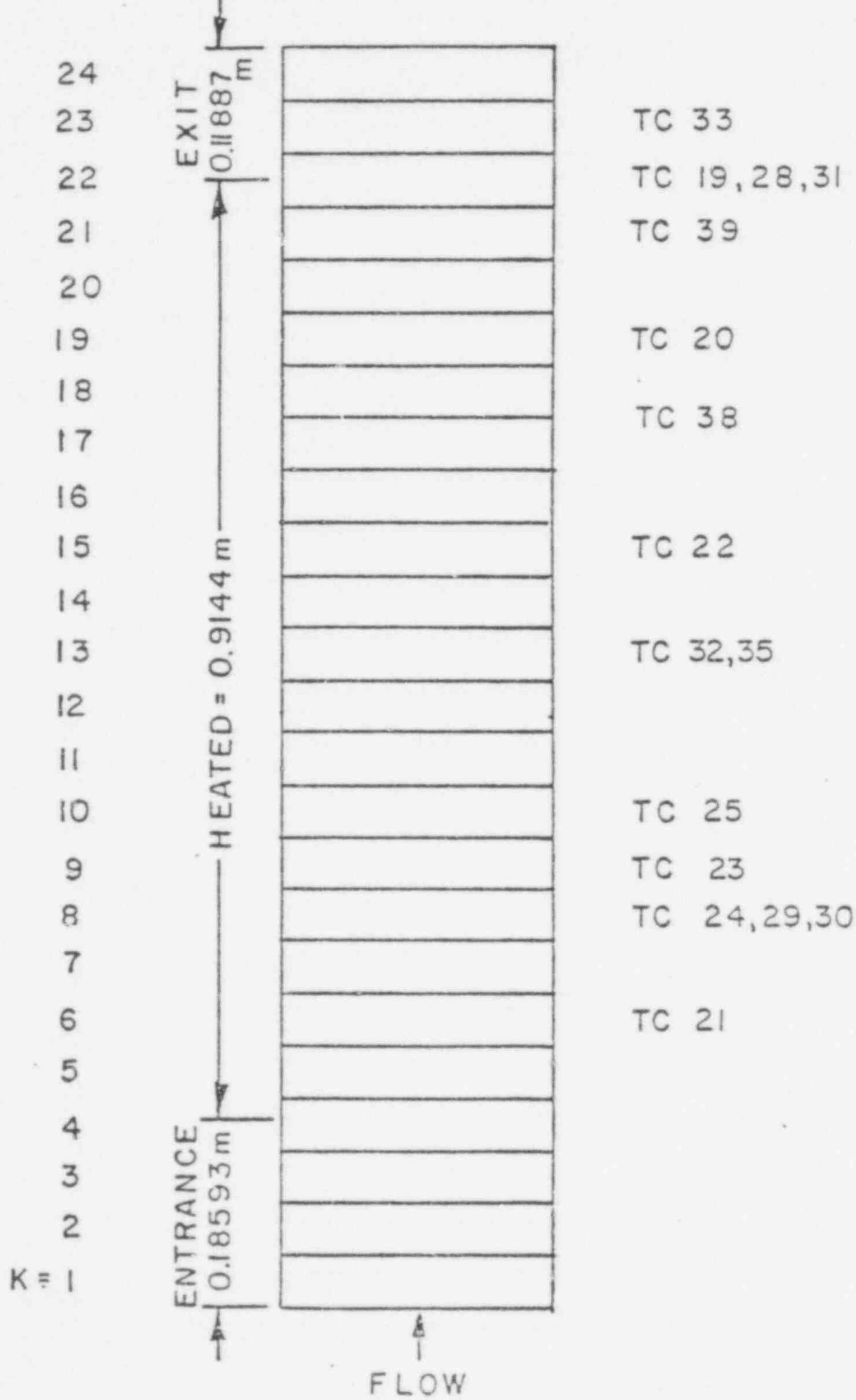
INITIAL COOLANT VELOCITY = 6.4 M/SEC.

689289



TRANSVERSE PARTITIONING OF
LOPI FUEL ASSEMBLY MODEL

689290



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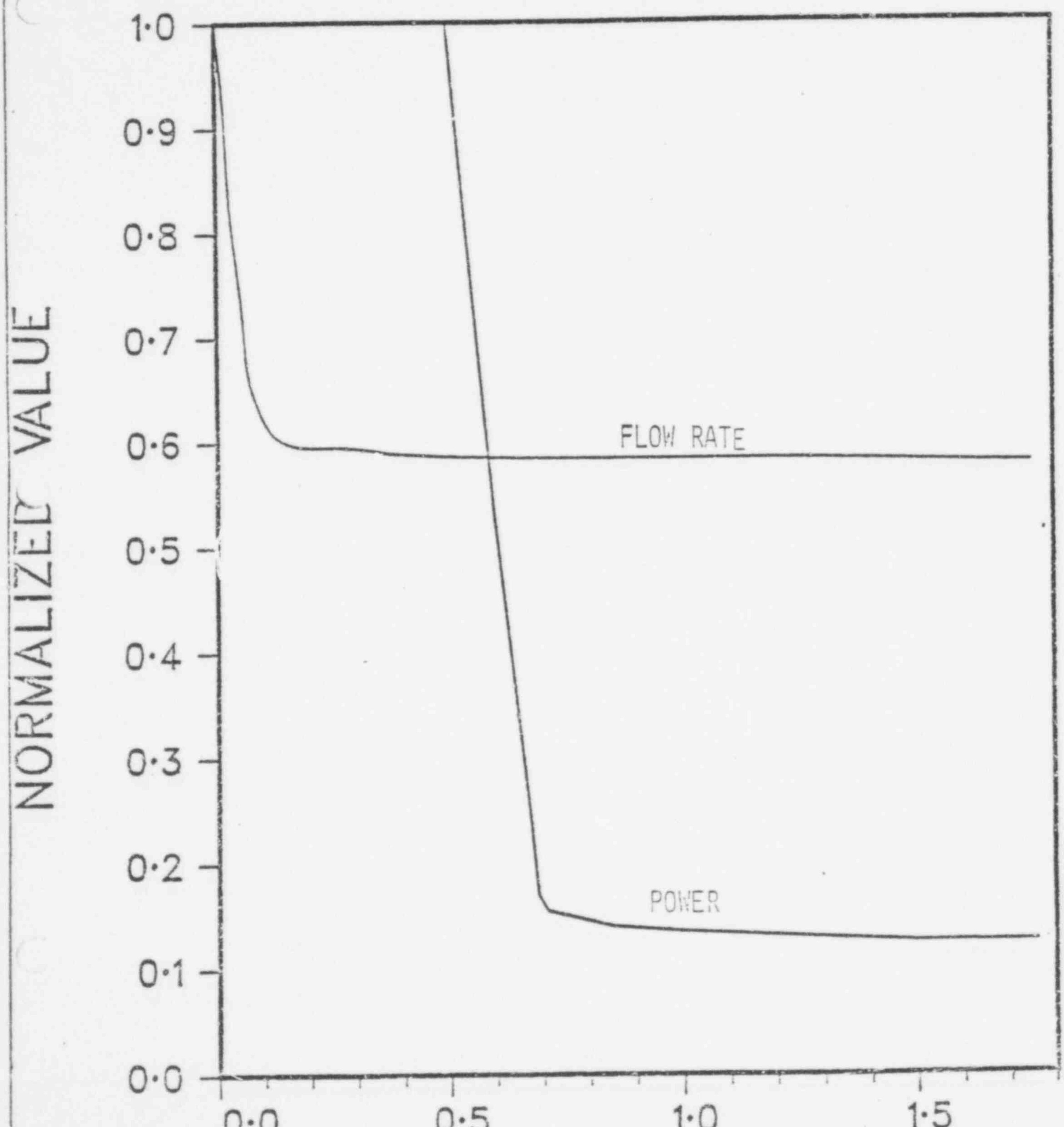
AXIAL PARTITIONING OF FUEL ASSEMBLY

FLOW-POWER HISTORY



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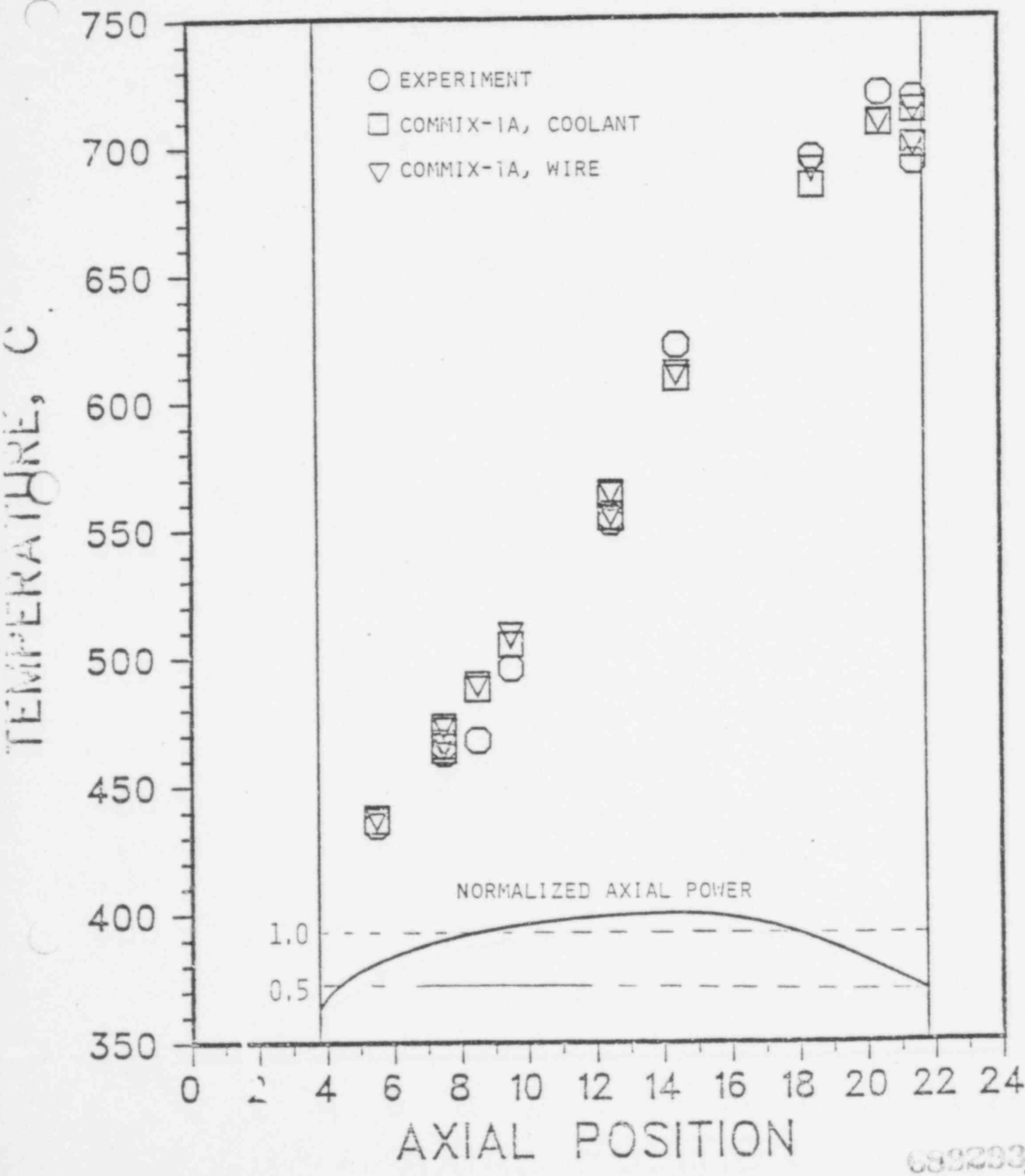
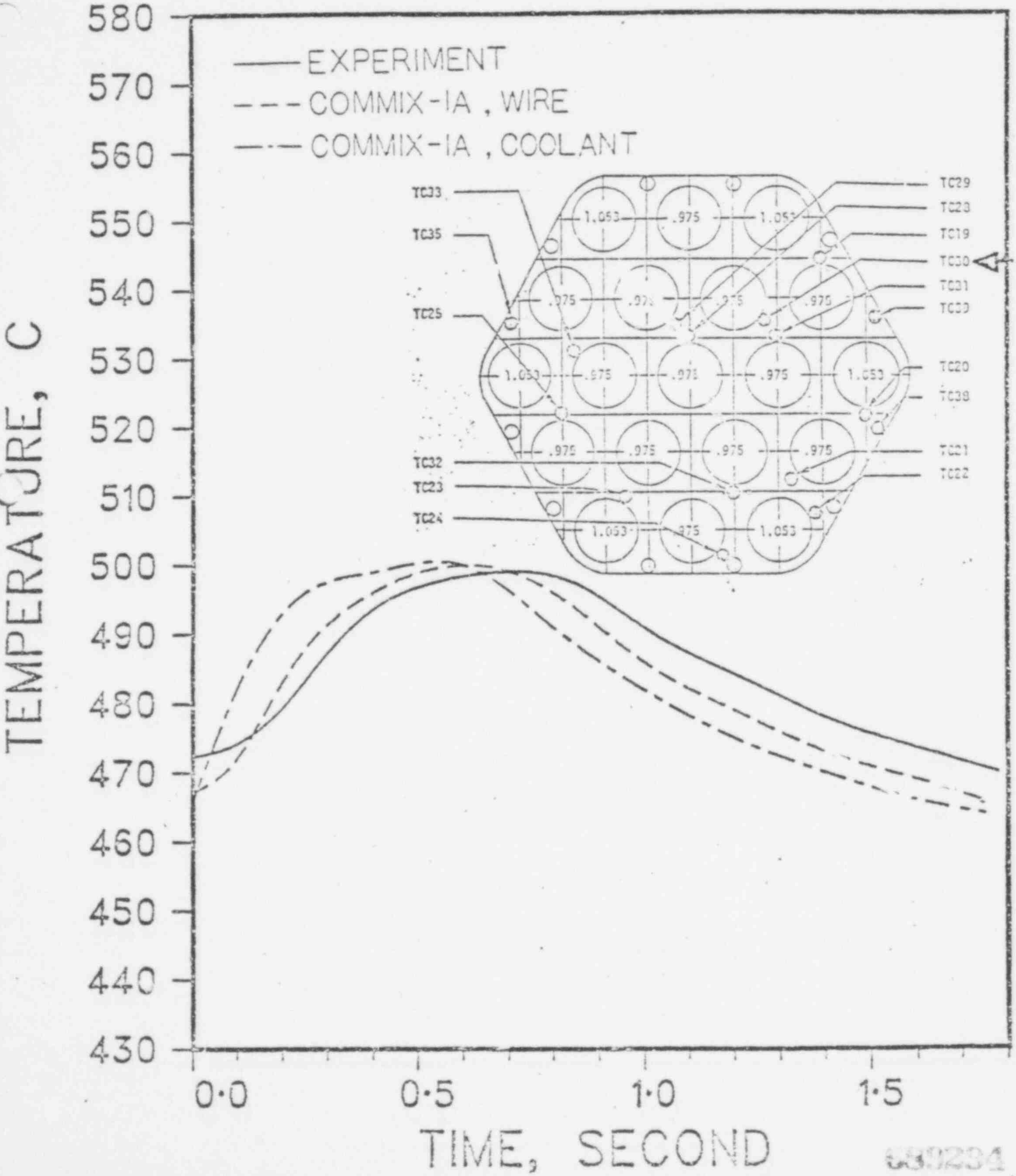
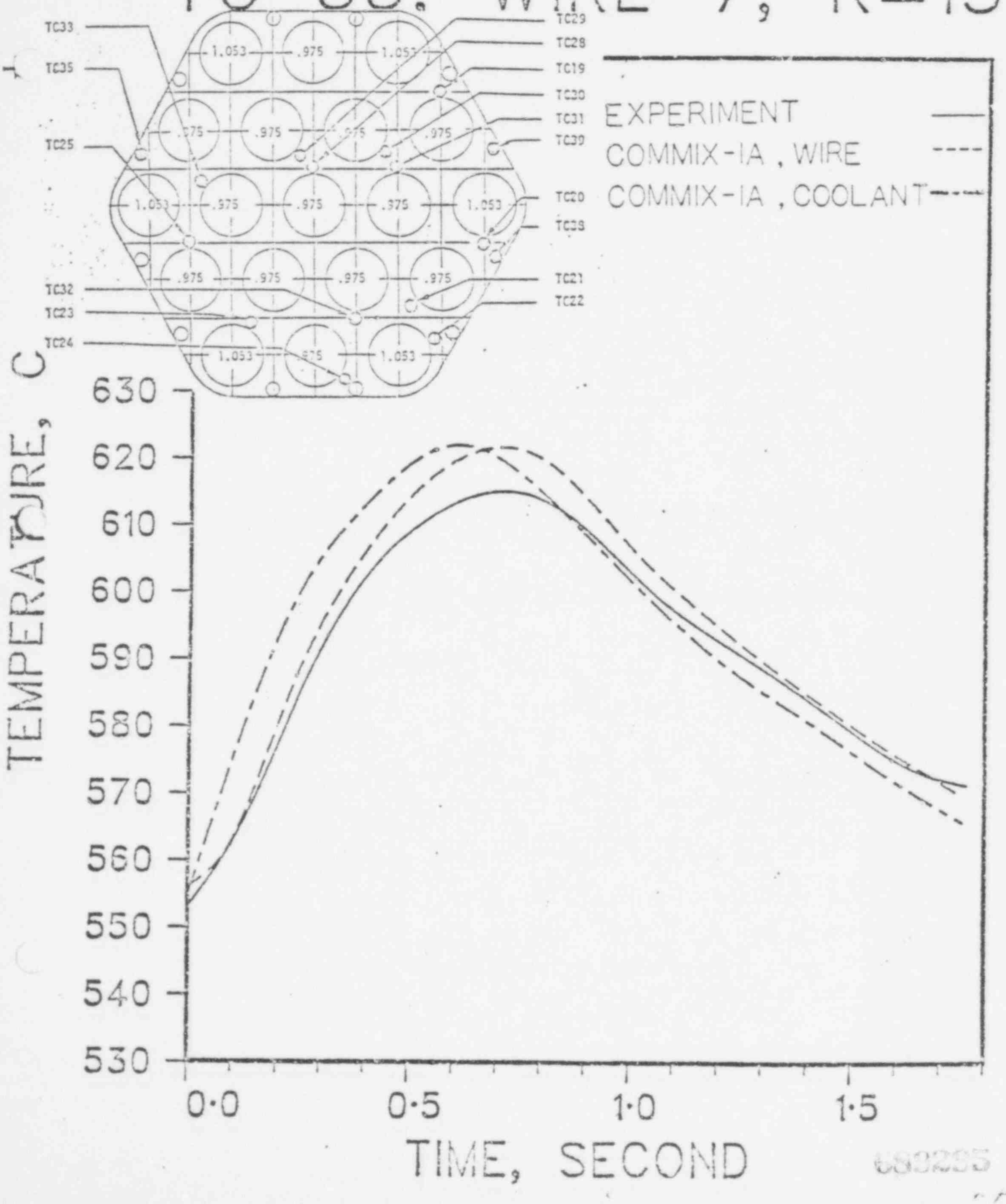


STEADY COOLANT TEMPERATURES

TC 30, ROD 15, K=8



TC 35, WIRE 7, K=13



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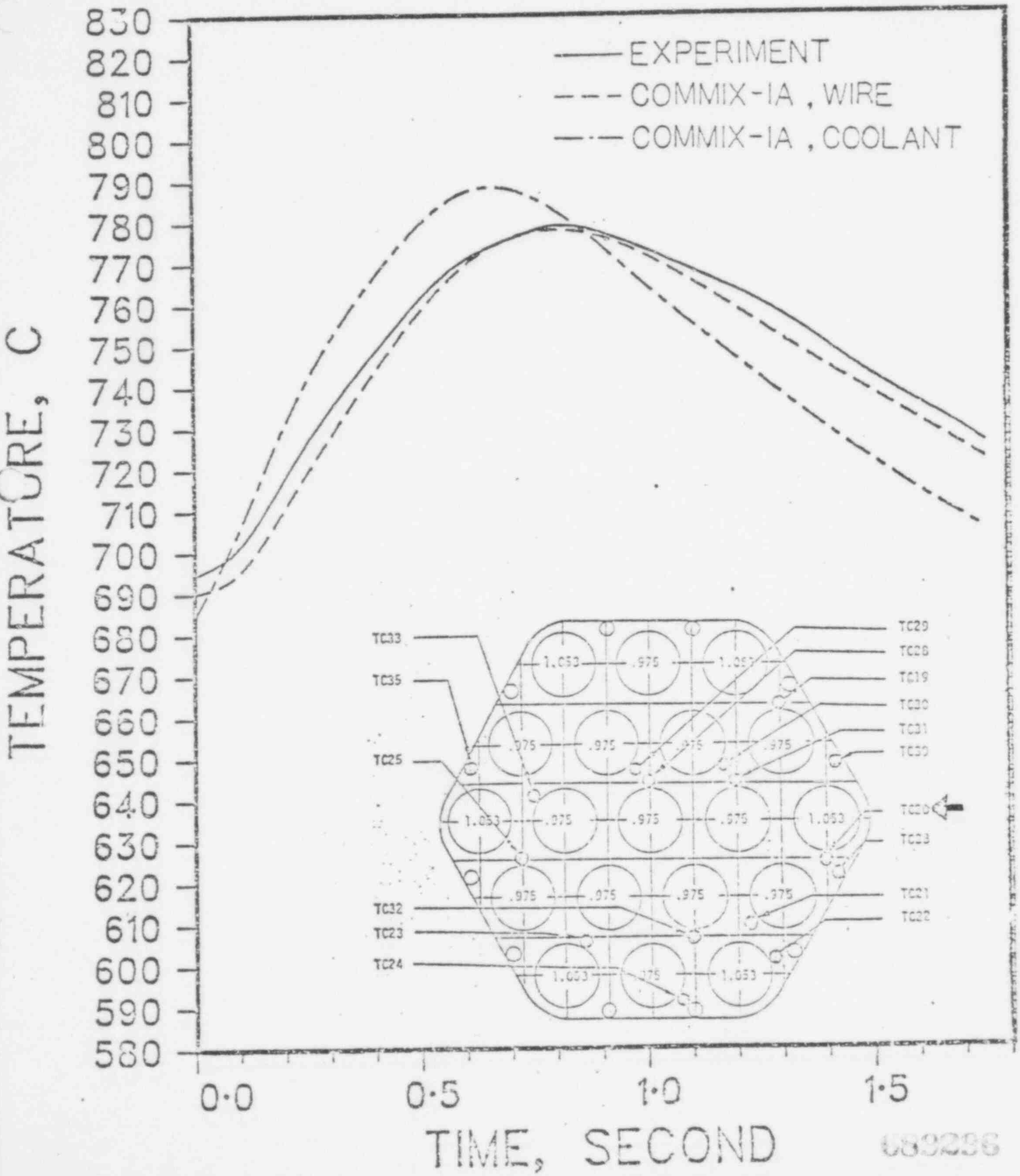
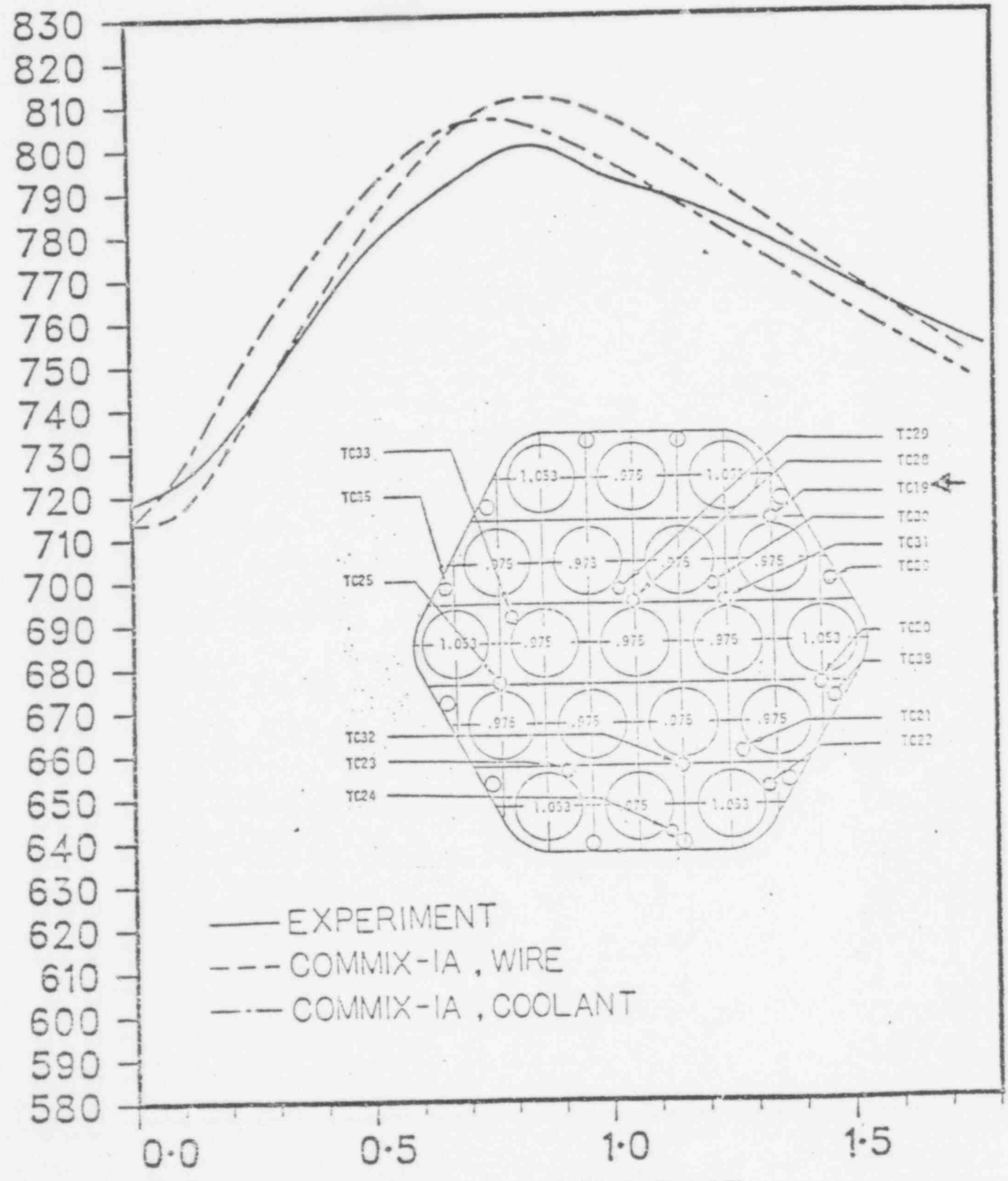
67

TC 20, ROD 12, K=19



TC 19, ROD 16, K=22



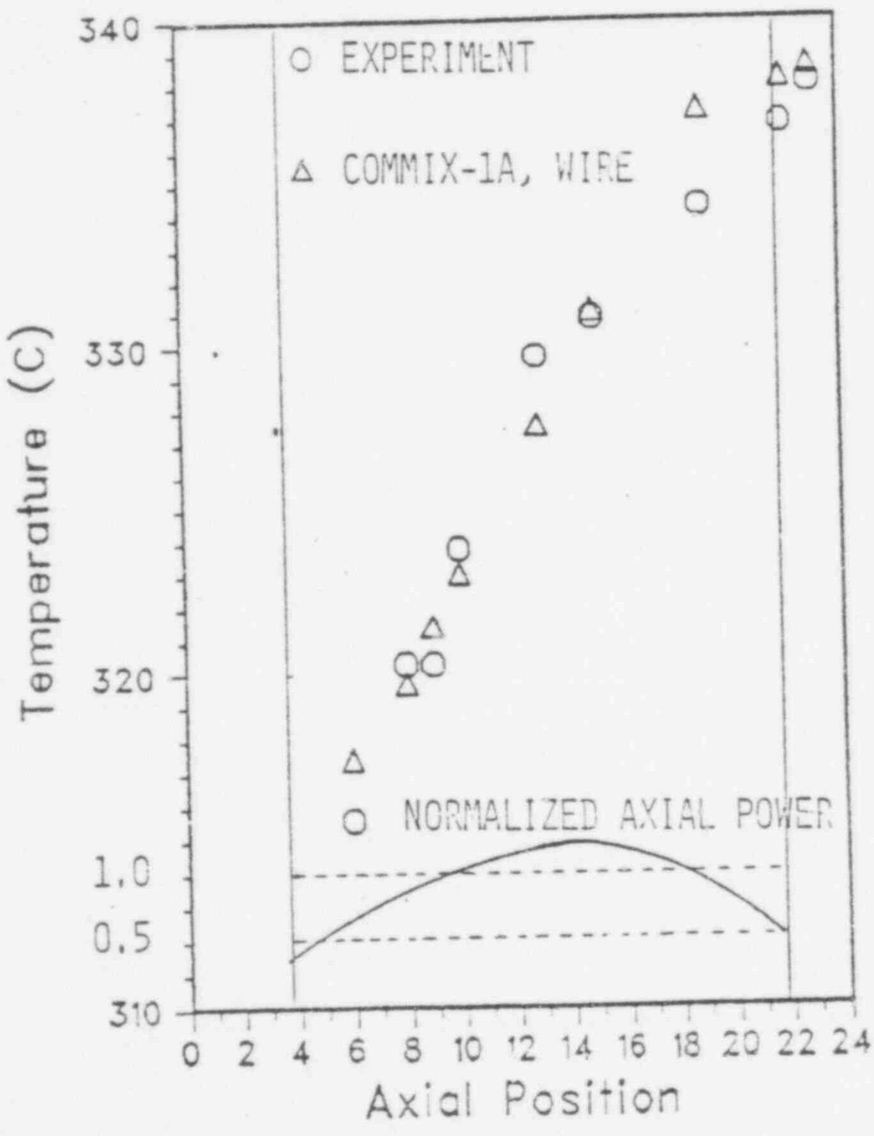
P2 NATURAL CIRCULATION TRANSIENT IN 19 PIN BUNDLE

6 MW ETR POWER

TOTAL BUNDLE POWER = 32.6 KW

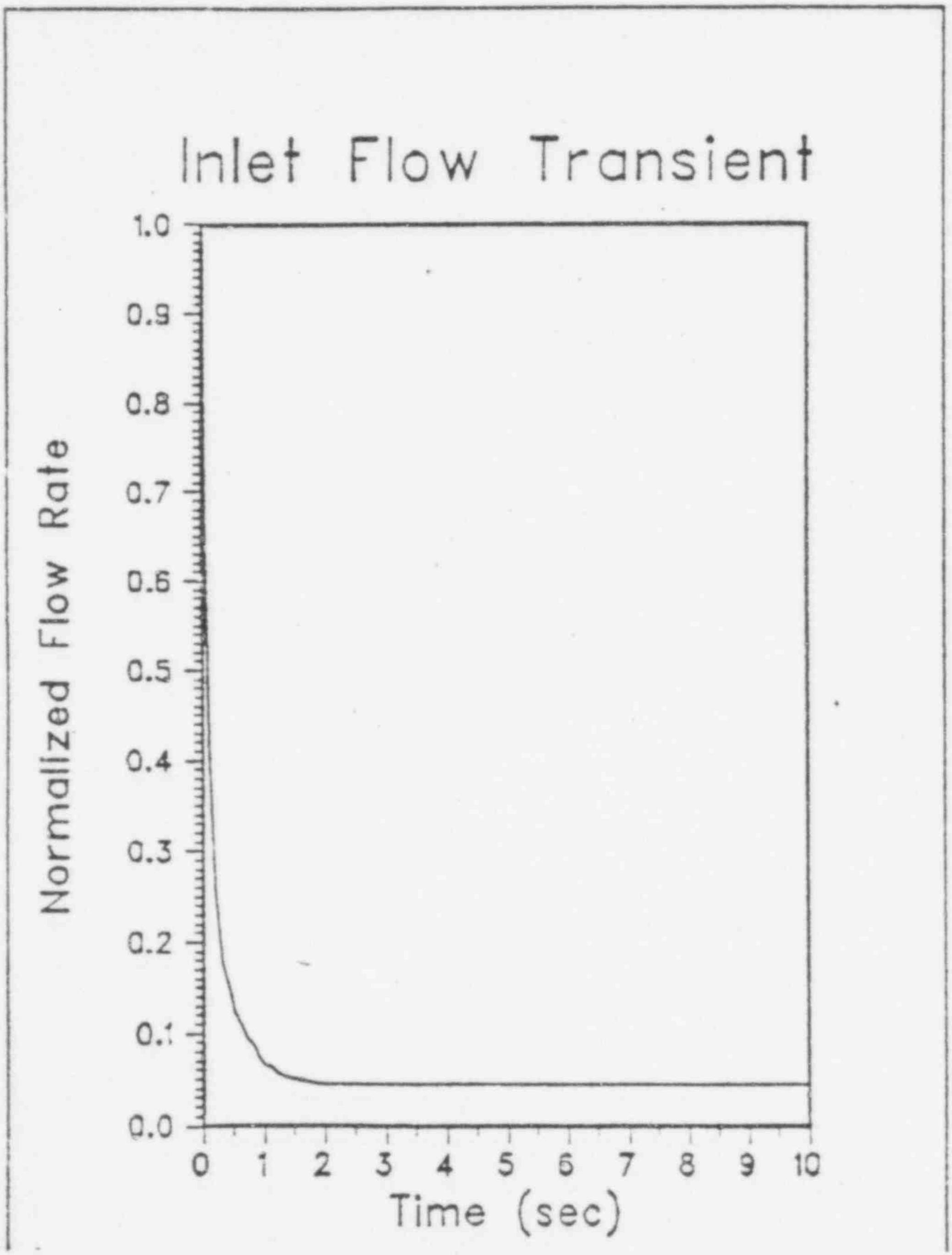
INITIAL COOLANT VELOCITY = 3.4 M/SEC.

Steady Wire Temperature



689209

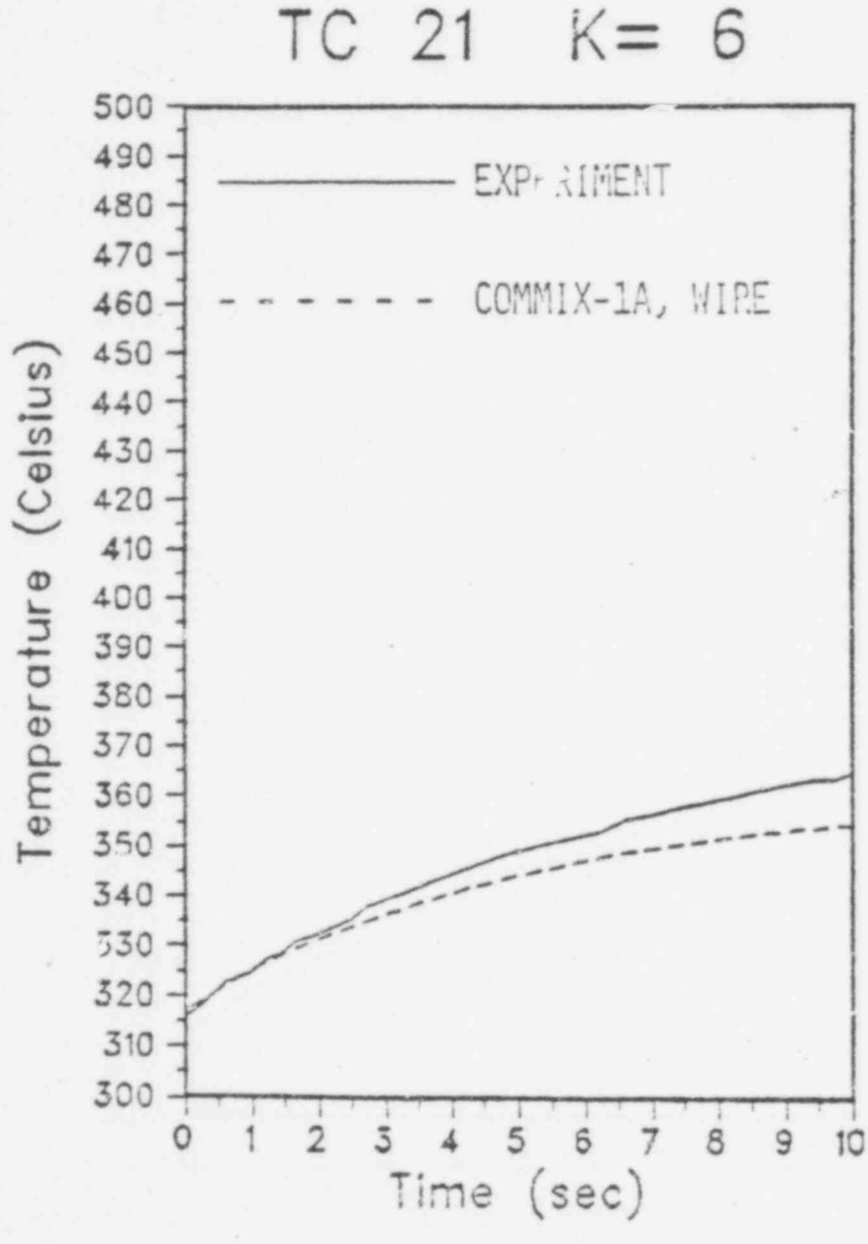
21



Inlet Flow Transient
Normalized Flow Rate
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0.0
0
1
2
3
4
5
6
7
8
9
10
Time (sec)

689300

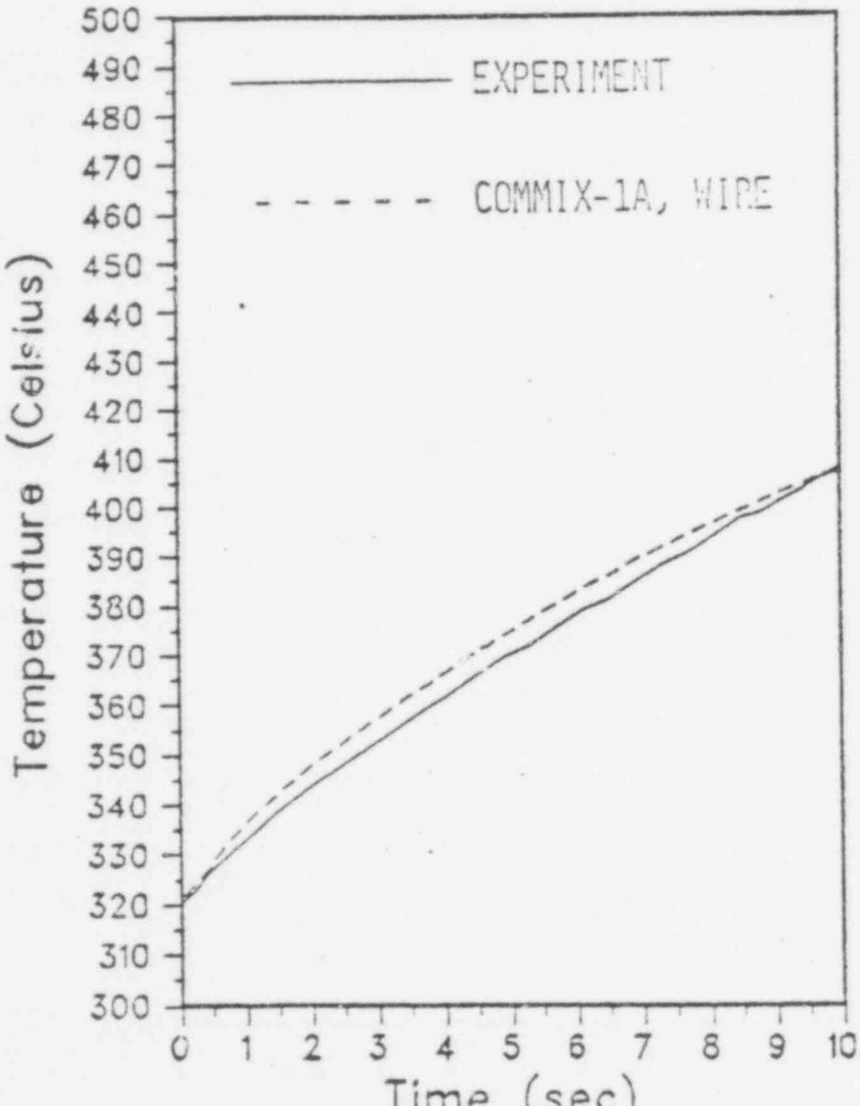
42



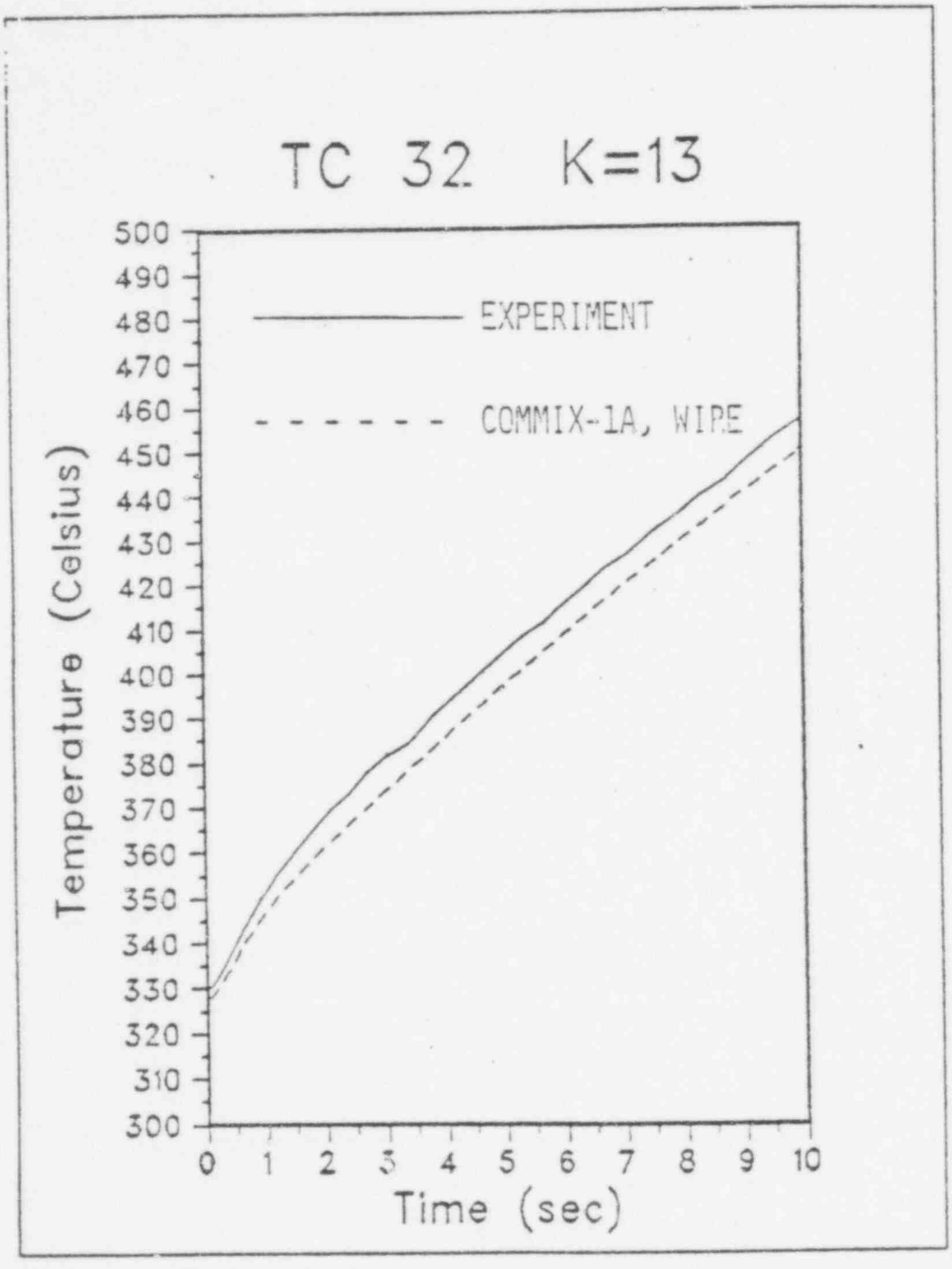
TC 21 K= 6
EXPERIMENT
COMMIX-1A, WIRE
Temperature (Celsius)
Time (sec)

8893301

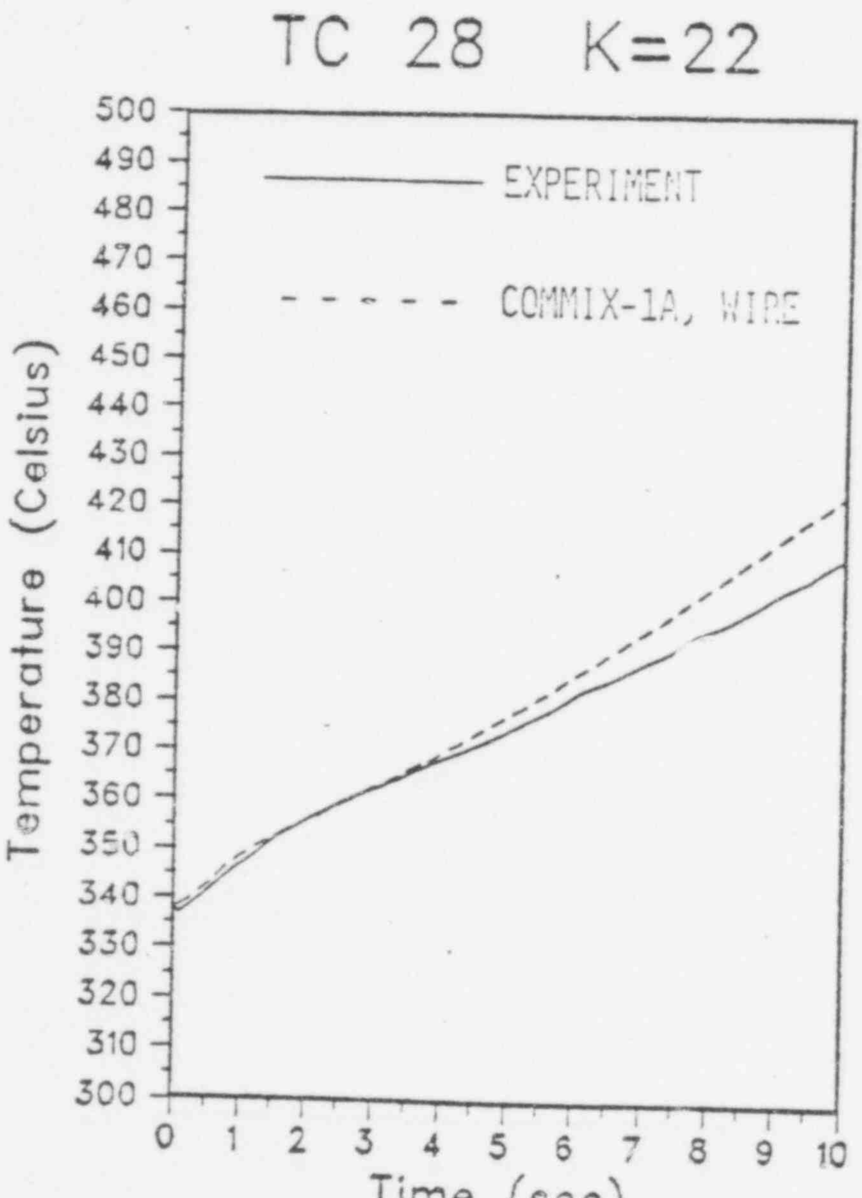
TC 23 K= 9



689302


TC 32 K=13
EXPERIMENT
COMMIX-1A, WIRE
Temperature (Celsius)
Time (sec)
500
490
480
470
460
450
440
430
420
410
400
390
380
370
360
350
340
330
320
310
300
0
1
2
3
4
5
6
7
8
9
10

699303

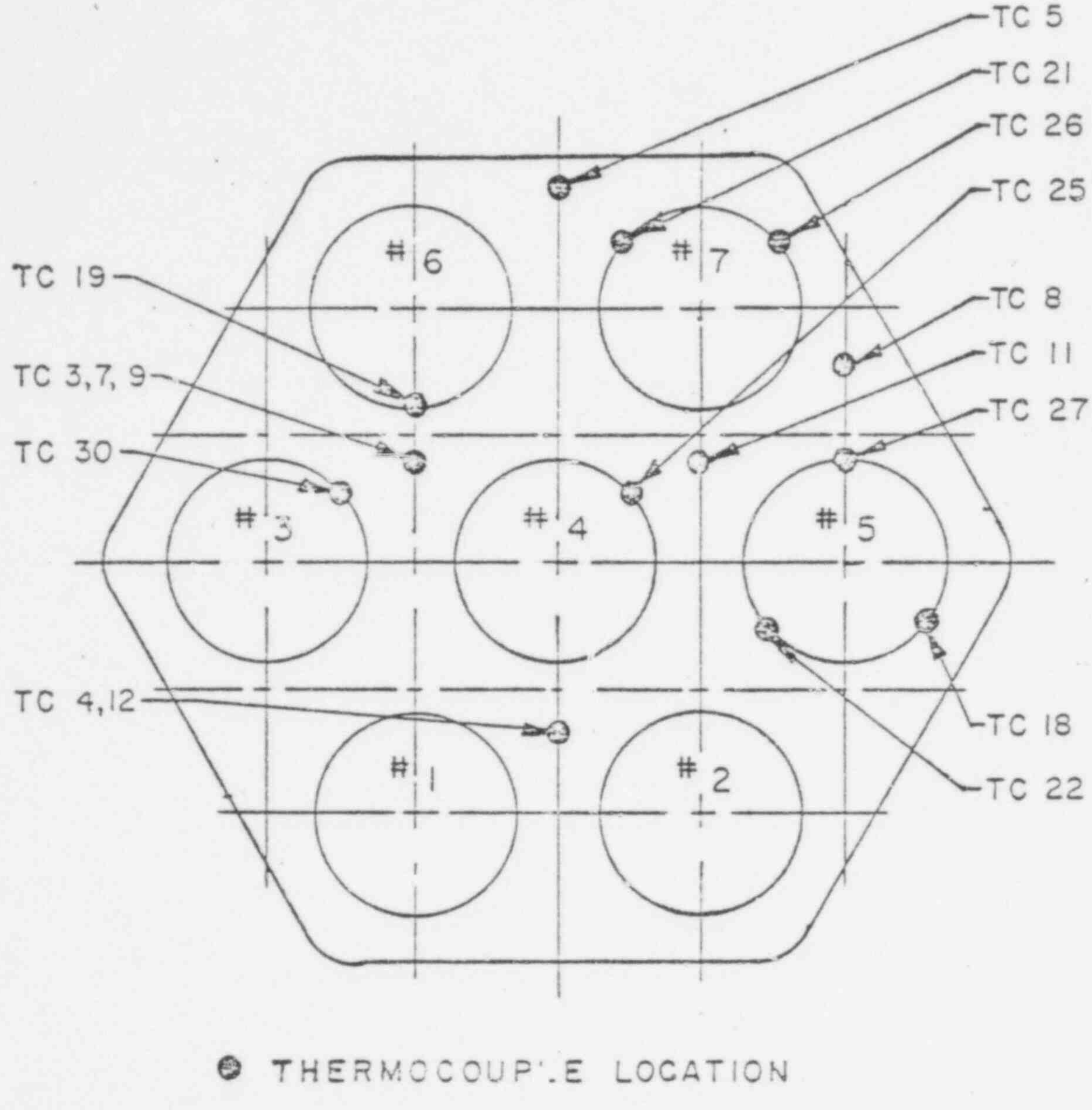


TC 28 K=22
EXPERIMENT
COMMIX-1A, WIRE
Temperature (Celsius)
500
490
480
470
460
450
440
430
420
410
400
390
380
370
360
350
340
330
320
310
300
0 1 2 3 4 5 6 7 8 9 10
Time (sec)

689304

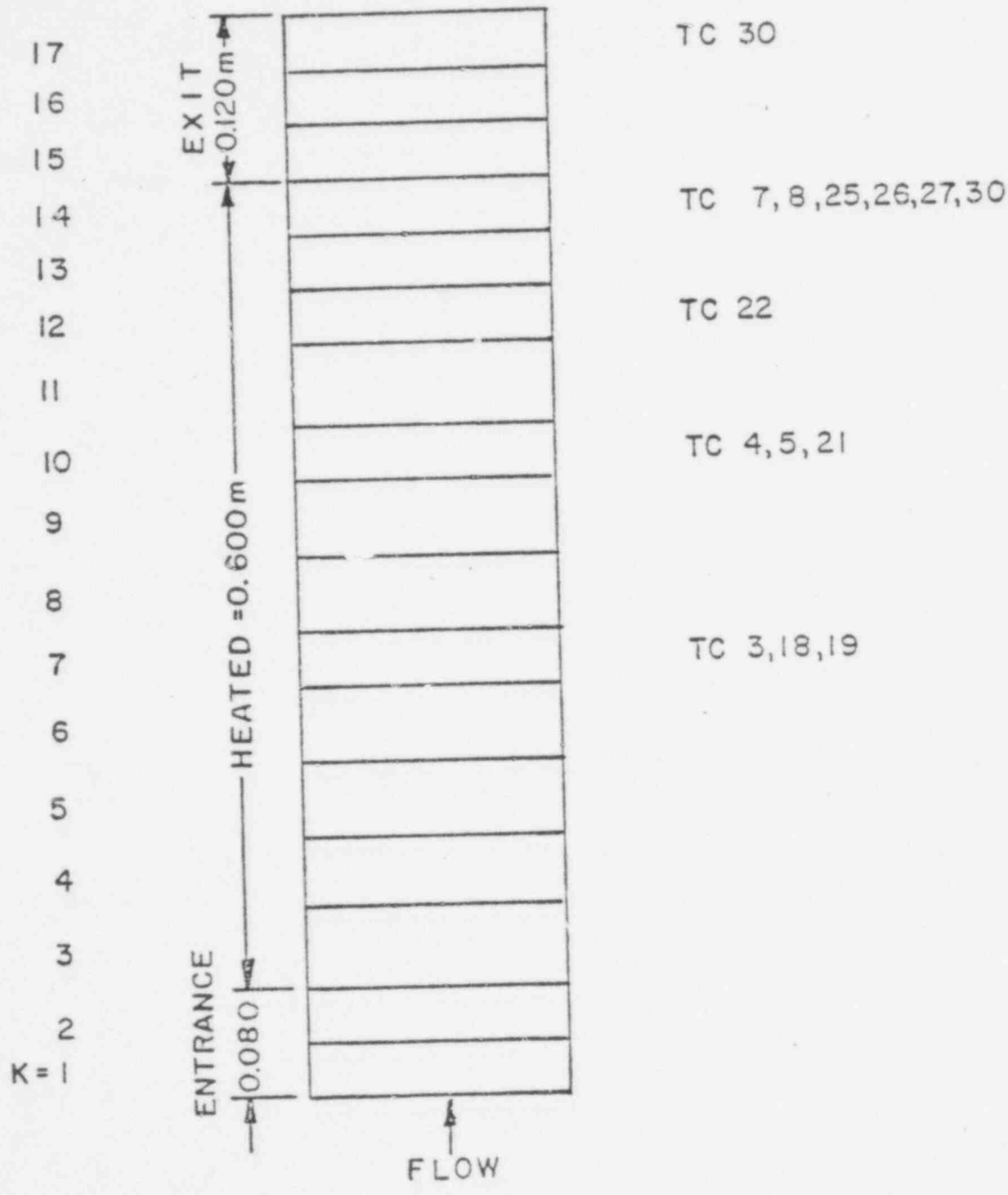
FLOW RUNDOWN TRANSIENT IN 7 PIN BUNDLE

- TEST NUMBER 7-2/24
- CONSTANT POWER = 78.6 KW
- UNIFORM AXIAL POWER
- UNIFORM RADIAL POWER
- INITIAL COOLANT VELOCITY = 2.15 M/SEC



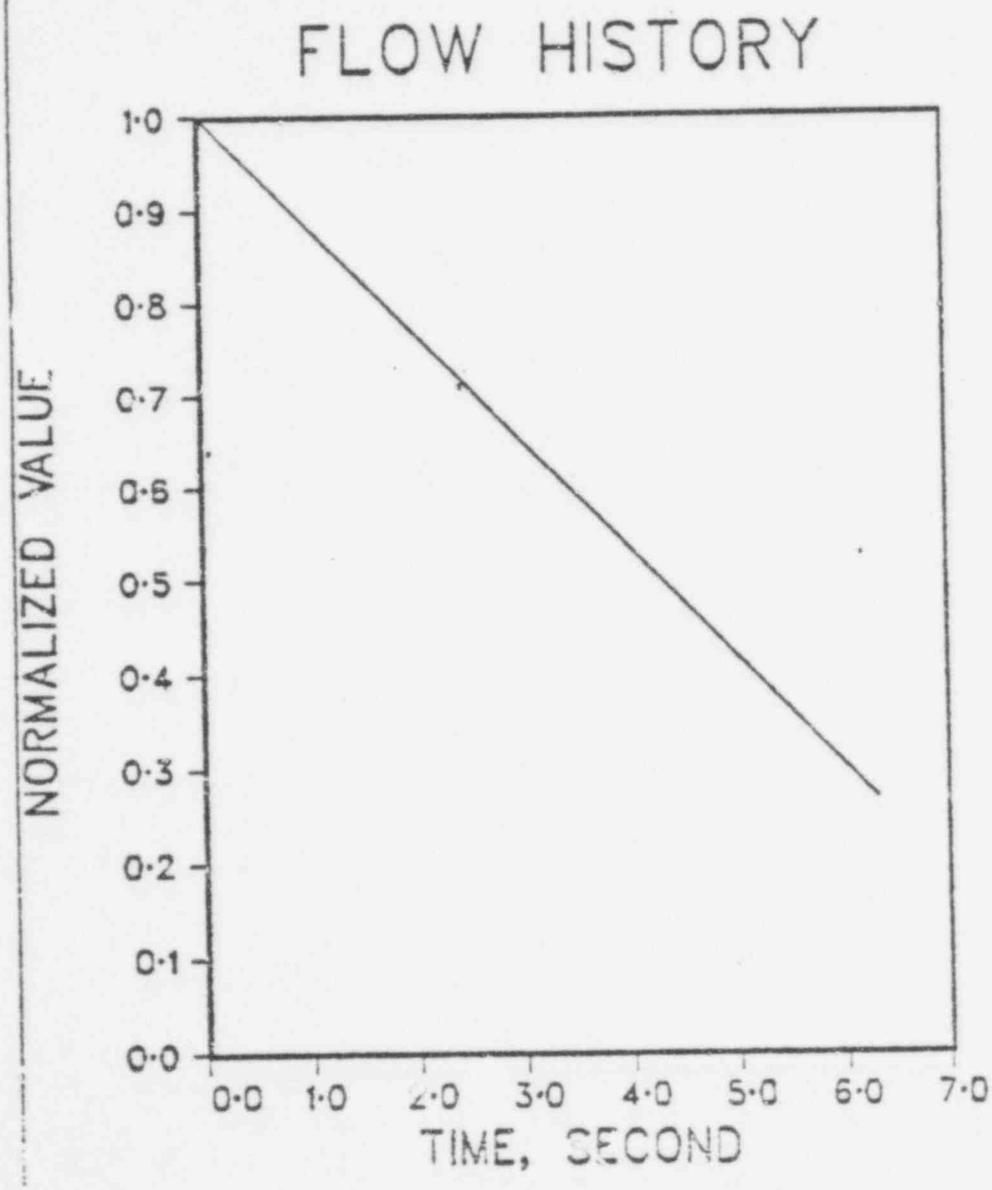
TRANSVERSE PARTITIONING OF 7 PIN FUEL ASSEMBLY

689306



AXIAL PARTITIONING OF FUEL ASSEMBLY

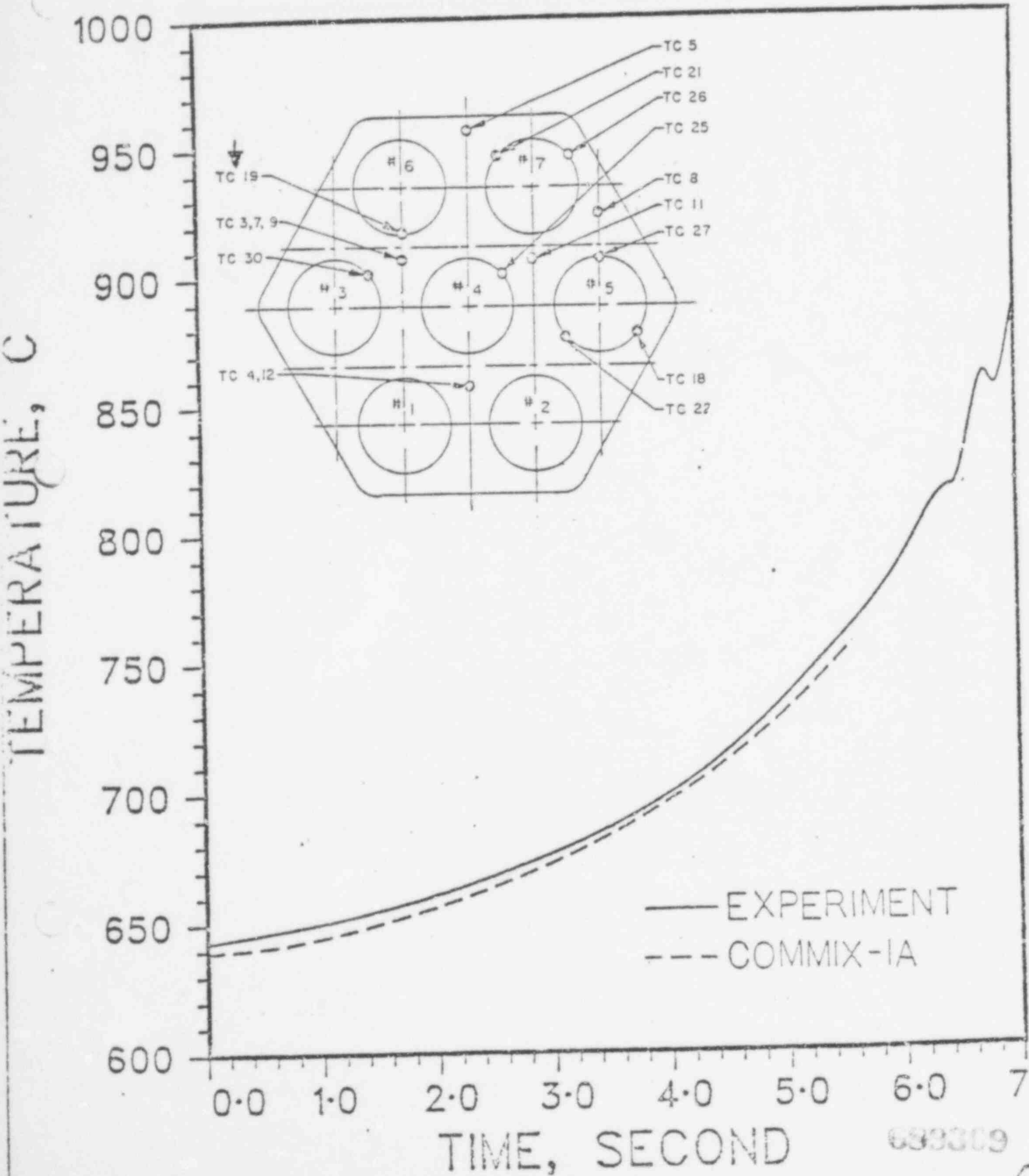
689307


FLOW HISTORY
1·0
0·9
0·8
0·7
0·6
0·5
0·4
0·3
0·2
0·1
0·0
NORMALIZED VALUE
0·0
1·0
2·0
3·0
4·0
5·0
6·0
7·0
TIME, SECOND

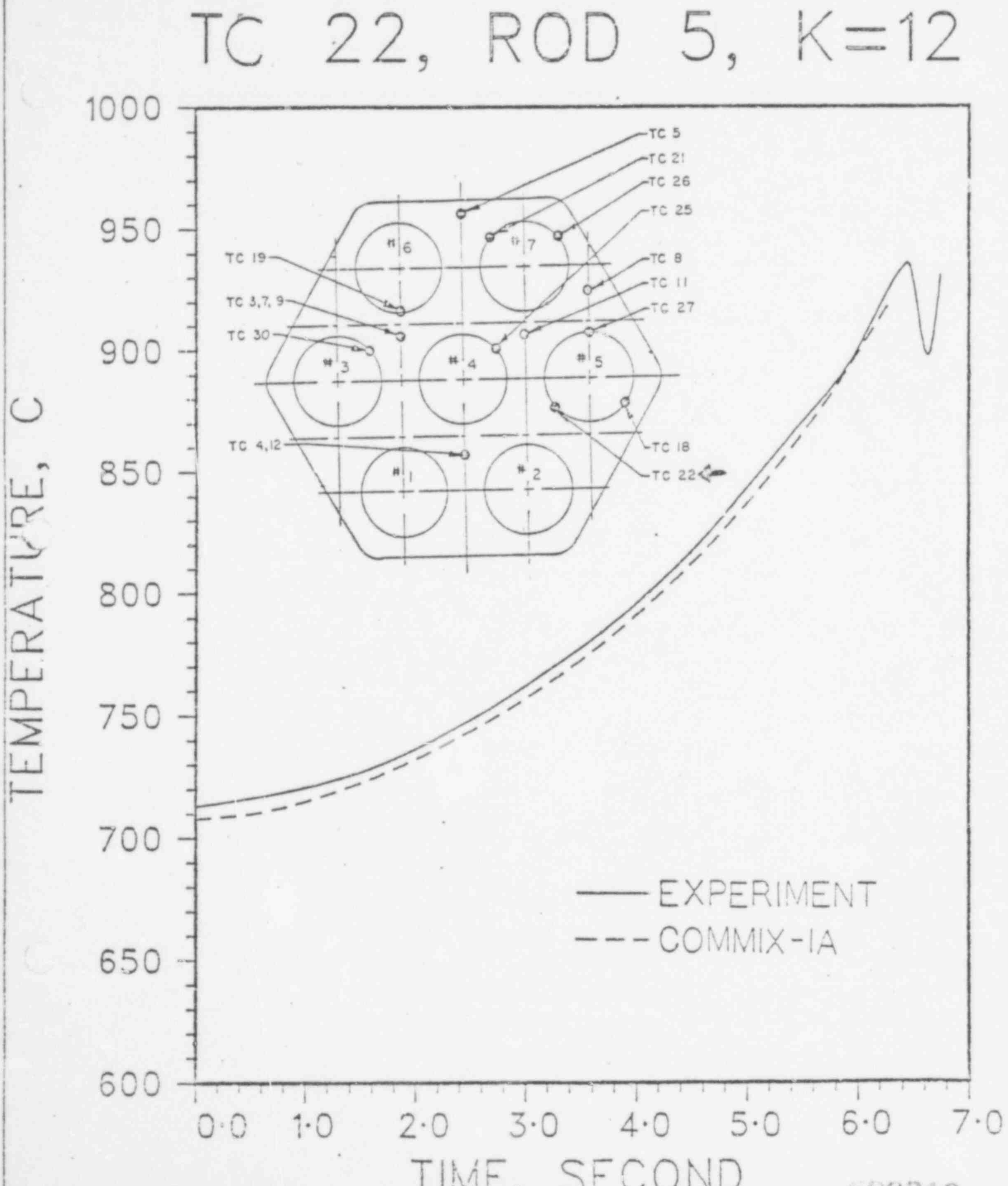
689308

50

TC 19, ROD 6, K=7

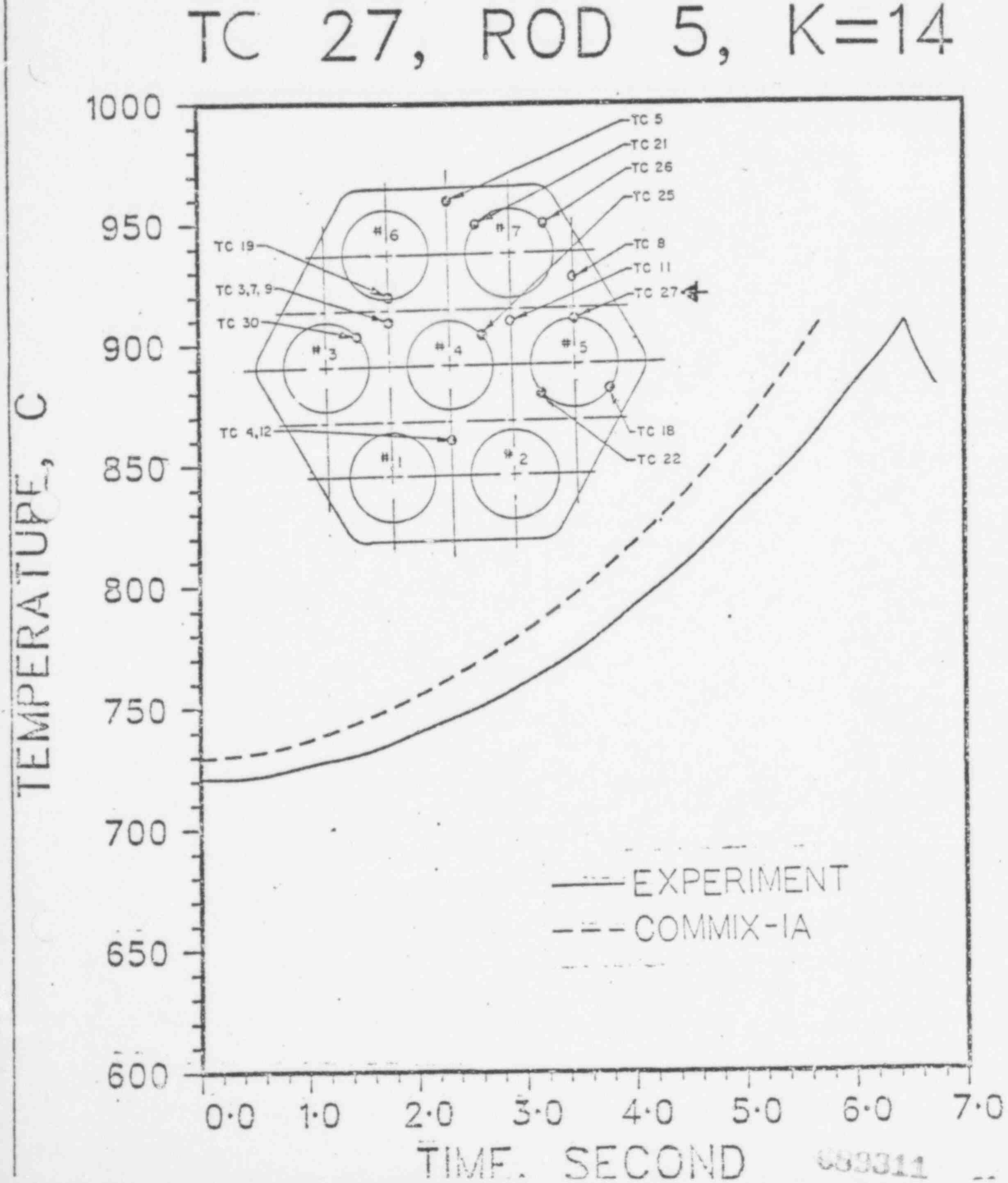


689309

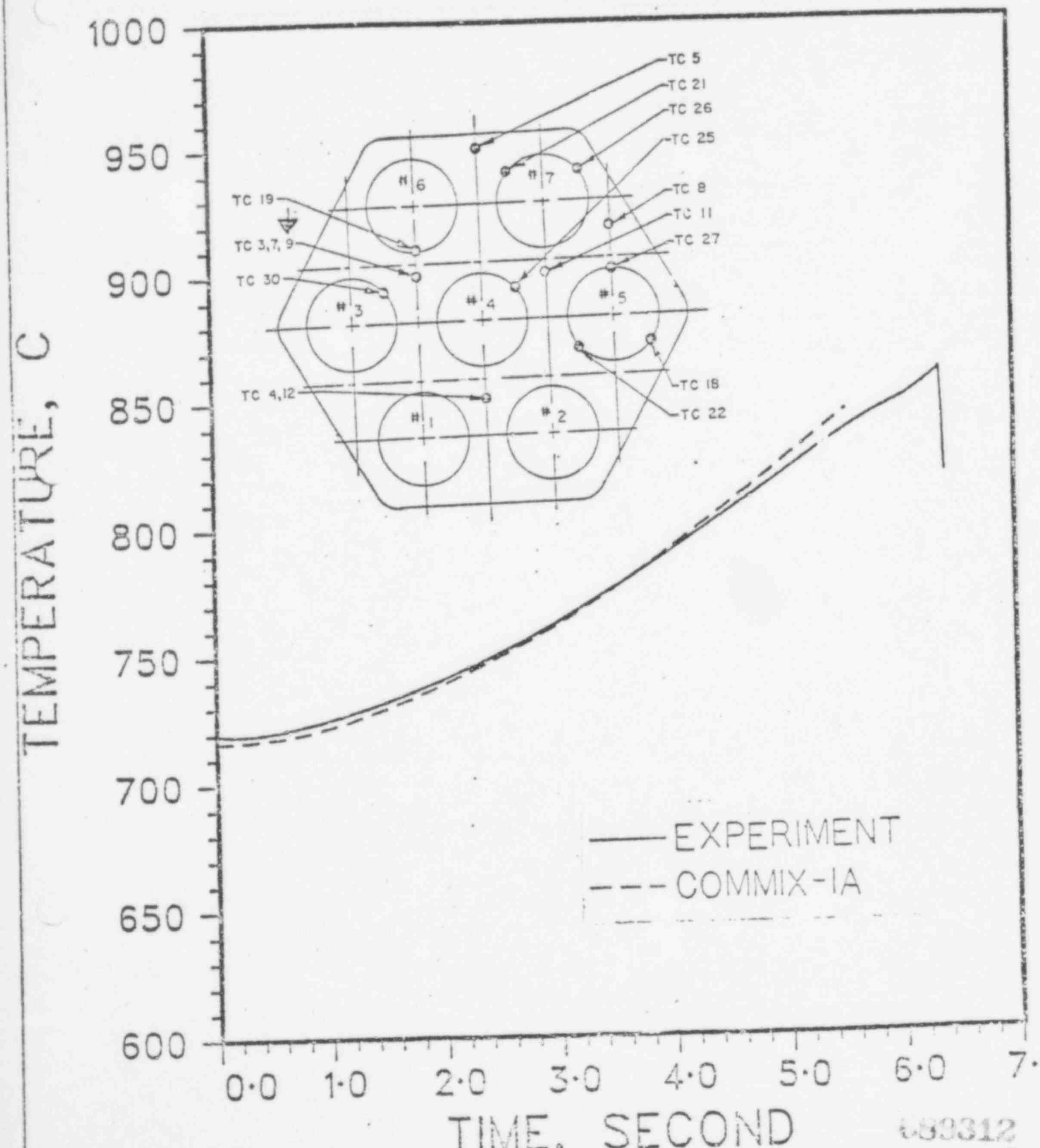


TC 22, ROD 5, K=12
TEMPERATURE, C
TIME, SECOND
1000
950
900
850
800
750
700
650
600
0·0
1·0
2·0
3·0
4·0
5·0
6·0
7·0
TC 5
TC 21
TC 26
TC 25
TC 19
TC 8
TC 3,7,9
TC 11
TC 27
TC 30
TC 18
TC 4,12
TC 22
#6
#7
#3
#4
#5
#1
#2
EXPERIMENT
COMMIX-IA

689310


TC 27, ROD 5, K=14
TEMPERATURE, C
TIME, SECOND
1000
950
900
850
800
750
700
650
600
0·0
1·0
2·0
3·0
4·0
5·0
6·0
7·0
TC 5
TC 21
TC 26
TC 25
TC 19
TC 8
TC 11
TC 3,7,9
TC 27
TC 30
TC 4,12
TC 18
TC 22
#6
#7
#3
#4
#5
#1
#2
EXPERIMENT
COMMIX-IA

TC 9, K=17



689312
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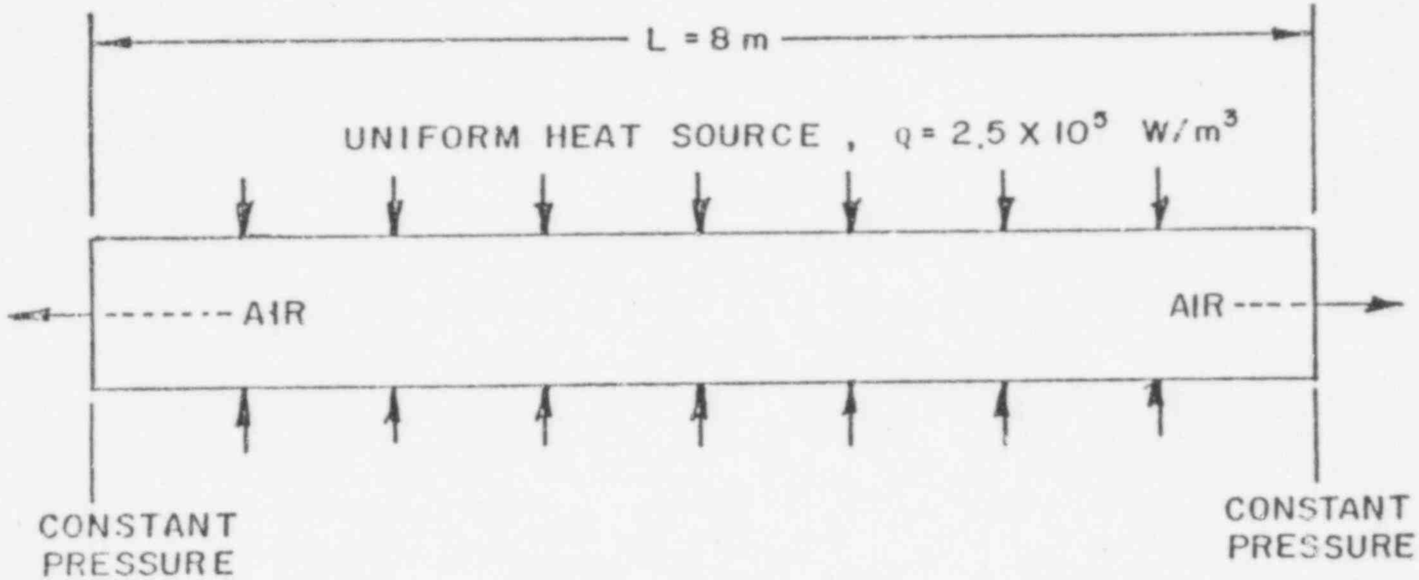
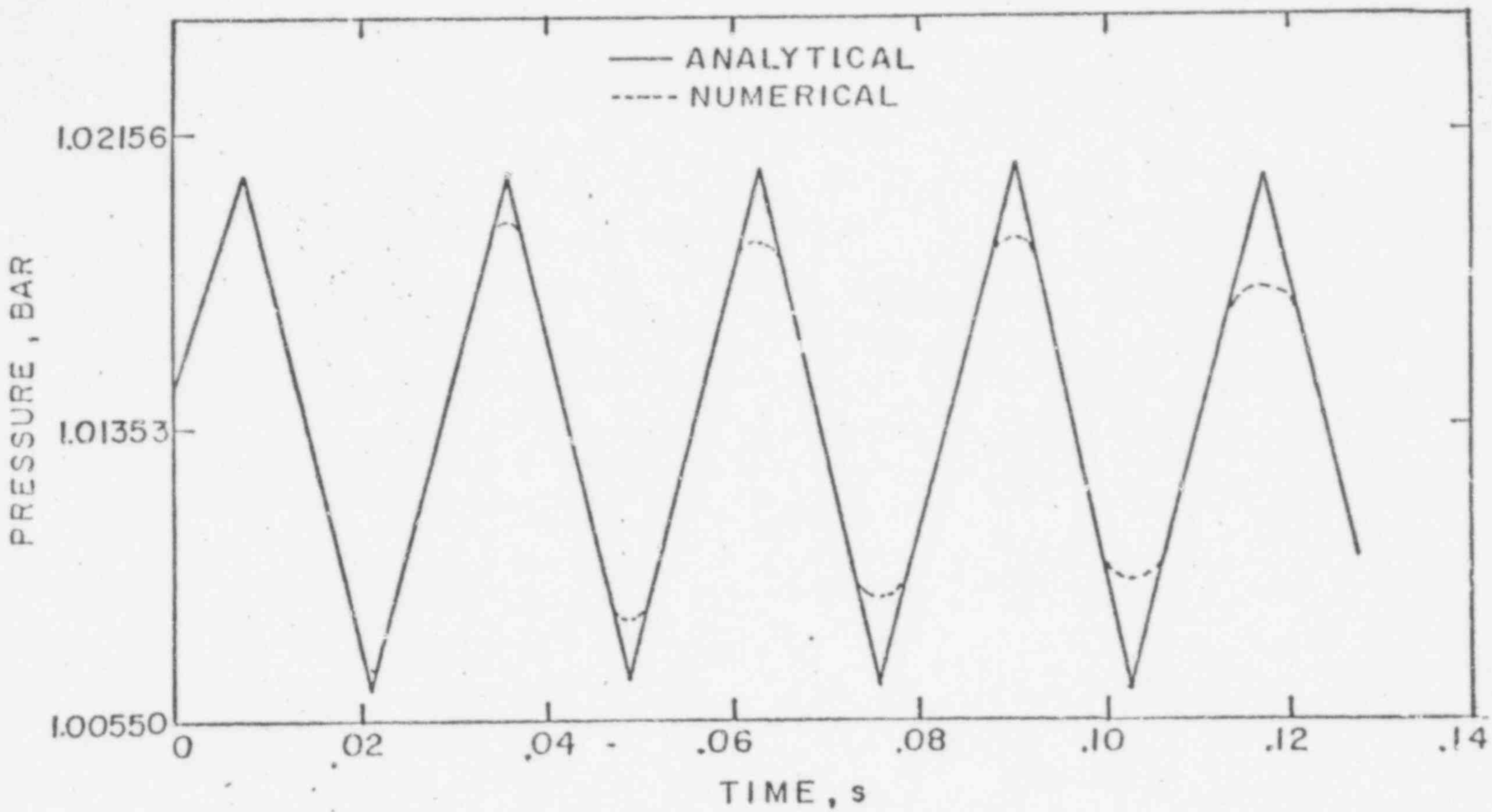
NUMERICAL RESULTS OF COMMIX-2

AIR FLOW IN A HORIZONTAL DUCT WITH UNIFORM HEAT SOURCE



DUCT WITH UNIFORM HEATING

(INITIAL PRESSURE = 1 ATM.; BOUNDARY PRESSURE = 1 ATM)



VARIATION OF PRESSURE AT THE CENTRAL PLANE
DUE TO UNIFORM HEATING

TWO PHASE FLOW IN A VERTICAL DUCT WITH UNIFORM HEAT FLUX

GEOMETRY: 0.1 M x 0.1 M x 0.9 M LONG SQUARE DUCT

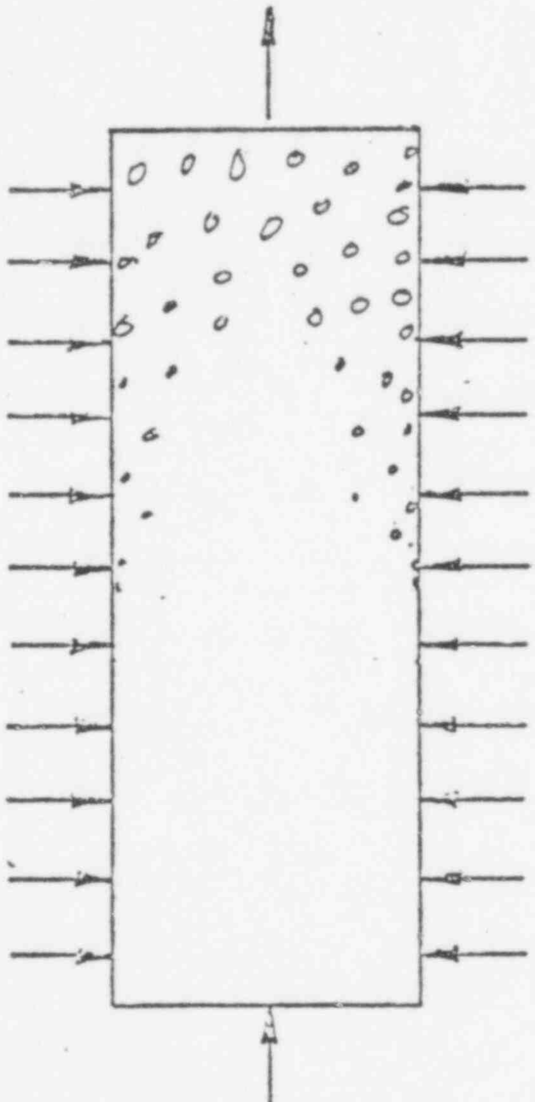
INLET TEMP: 99.75°C

INLET PRESSURE: 1 ATM

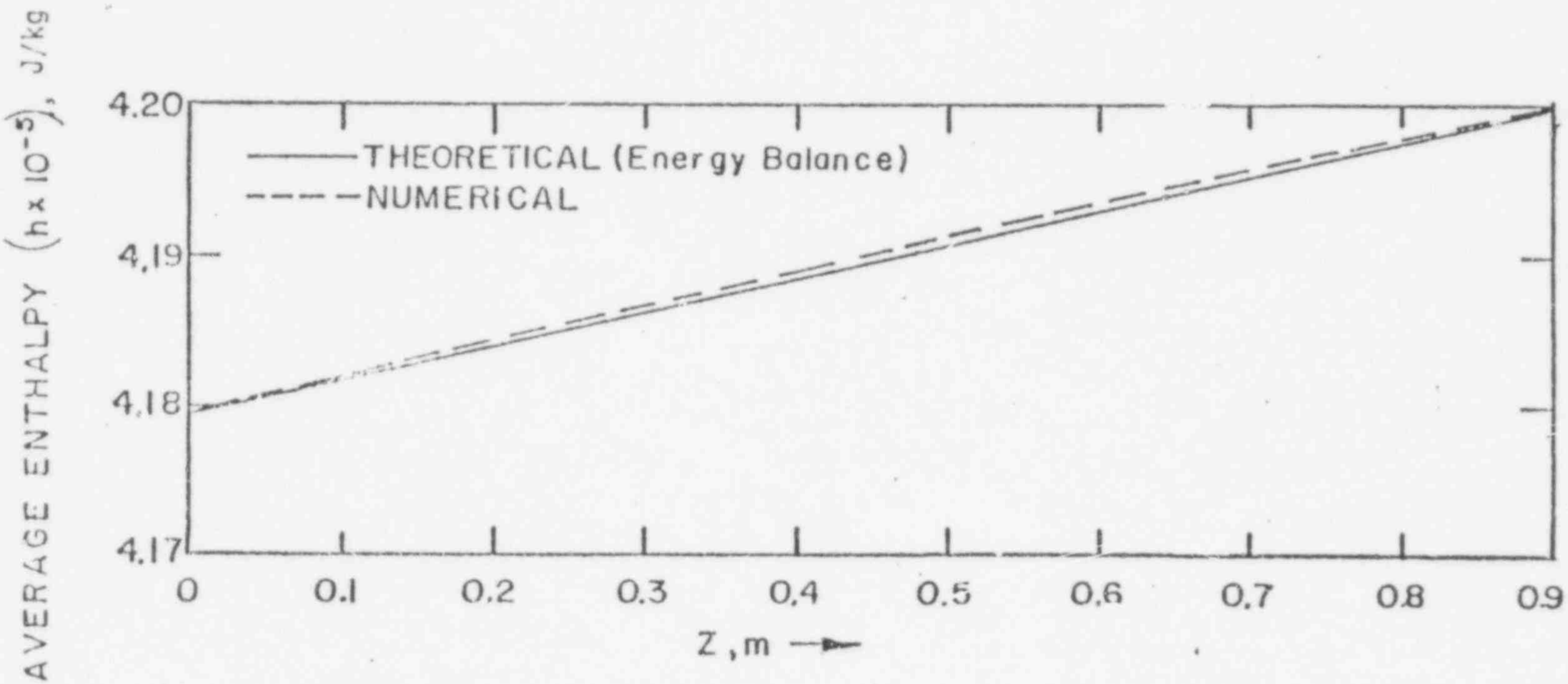
INLET VELOCITY: 1 M/S

SURFACE HEAT FLUX = 5.375×10^4 W/M^2

FLUID = H_2O



VERTICAL DUCT WITH SURFACE HEAT FLUX.



ENTHALPY DISTRIBUTION IN A VERTICAL HEATED DUCT

19

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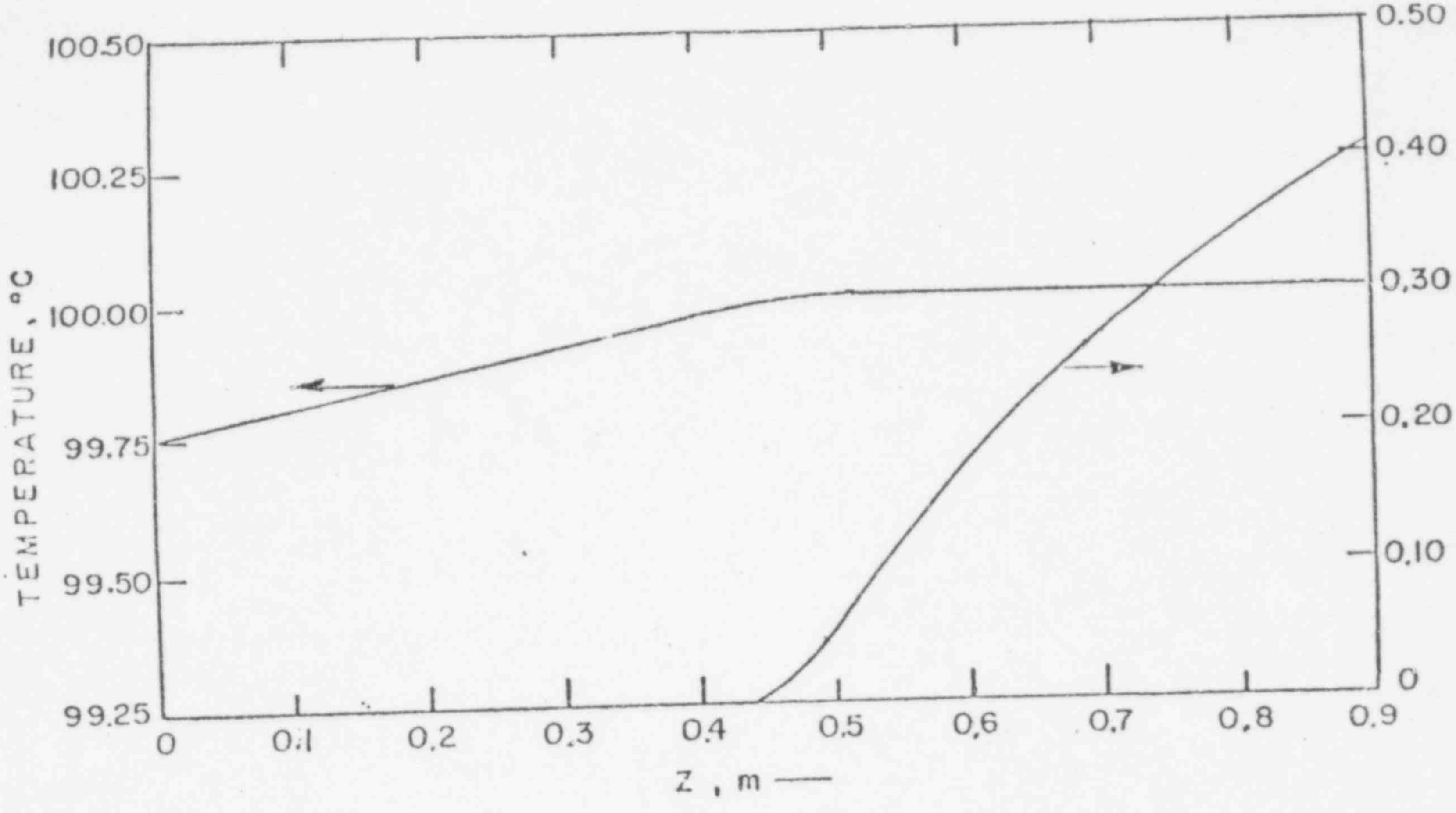


VARIATION OF TEMPERATURE AND VOID FRACTION IN A VERTICAL HEATED DUCT

NUMERICAL RESULTS OF BODYFIT-1

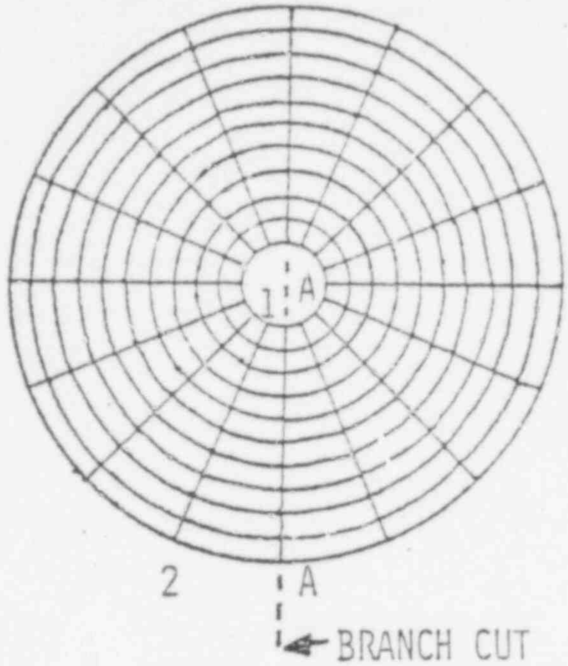
COORDINATE TRANSFORMATION

- Complicated Rod-Bundle Configuration Transforms to Rectangular or Cylindrical Coordinates with Uniform Mesh

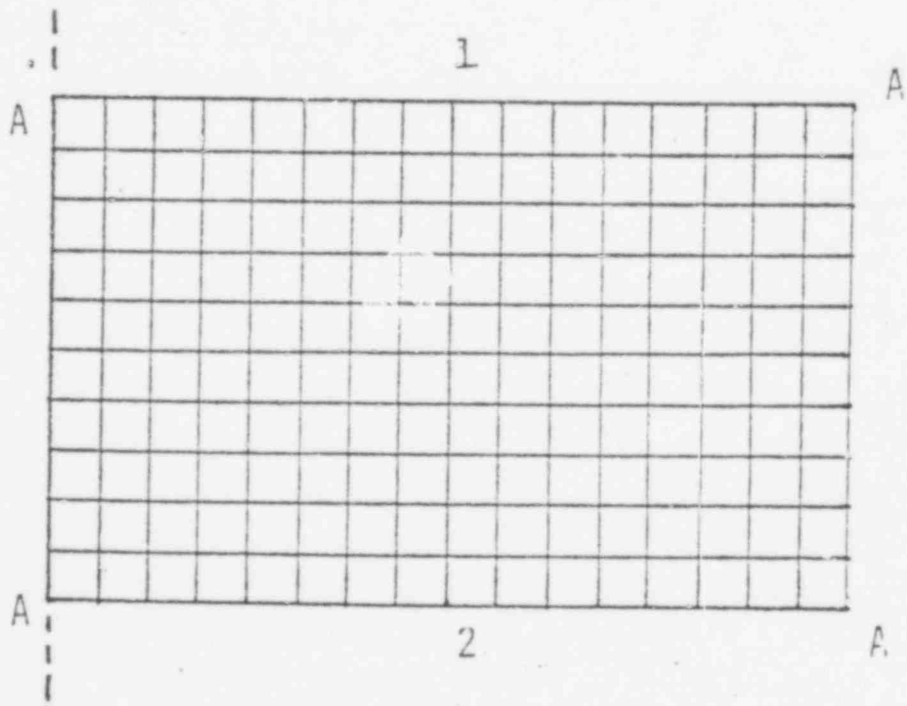
- Internal and External Boundaries Become Coincident with Coordinate Lines

PROBLEMS CONSIDERED

- Square Duct with 4 Fuel Pins
  - Isothermal Case
  - Heat Flux Case
- Hexagonal Duct with 7 Fuel Pins
  - Isothermal Case
  - Heat Flux Case



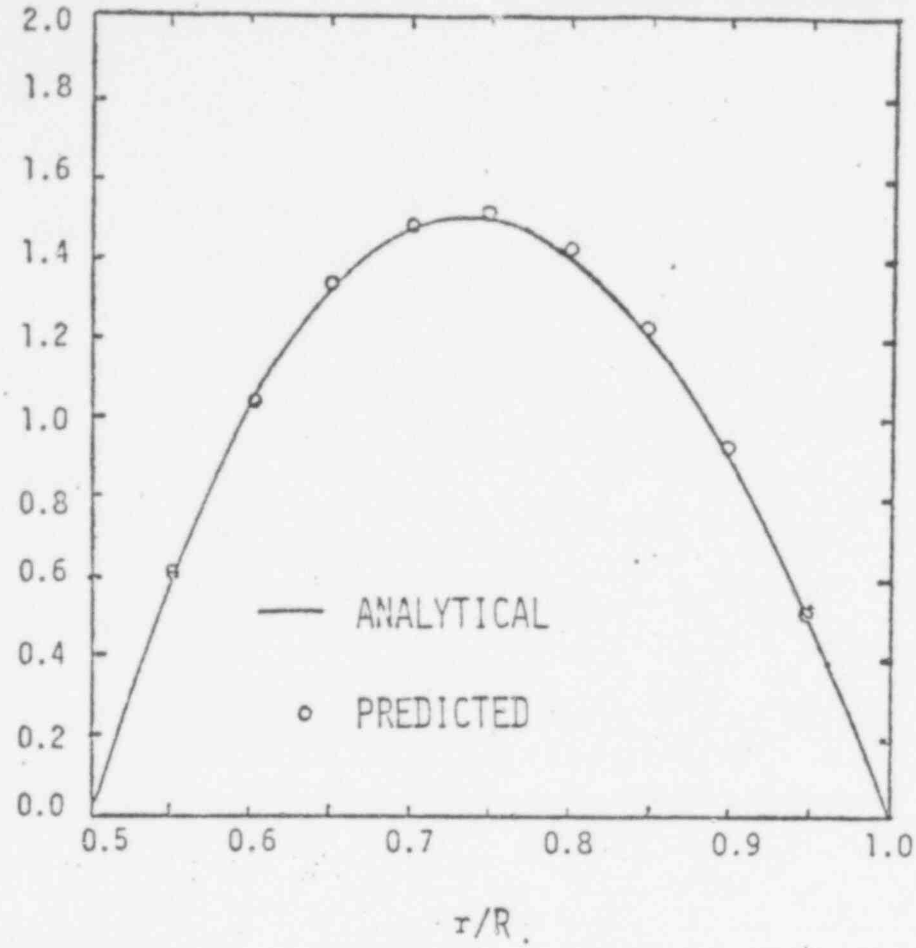
COORDINATE LINES FOR THE CONCENTRIC RING BEFORE THE TRANSFORMATION

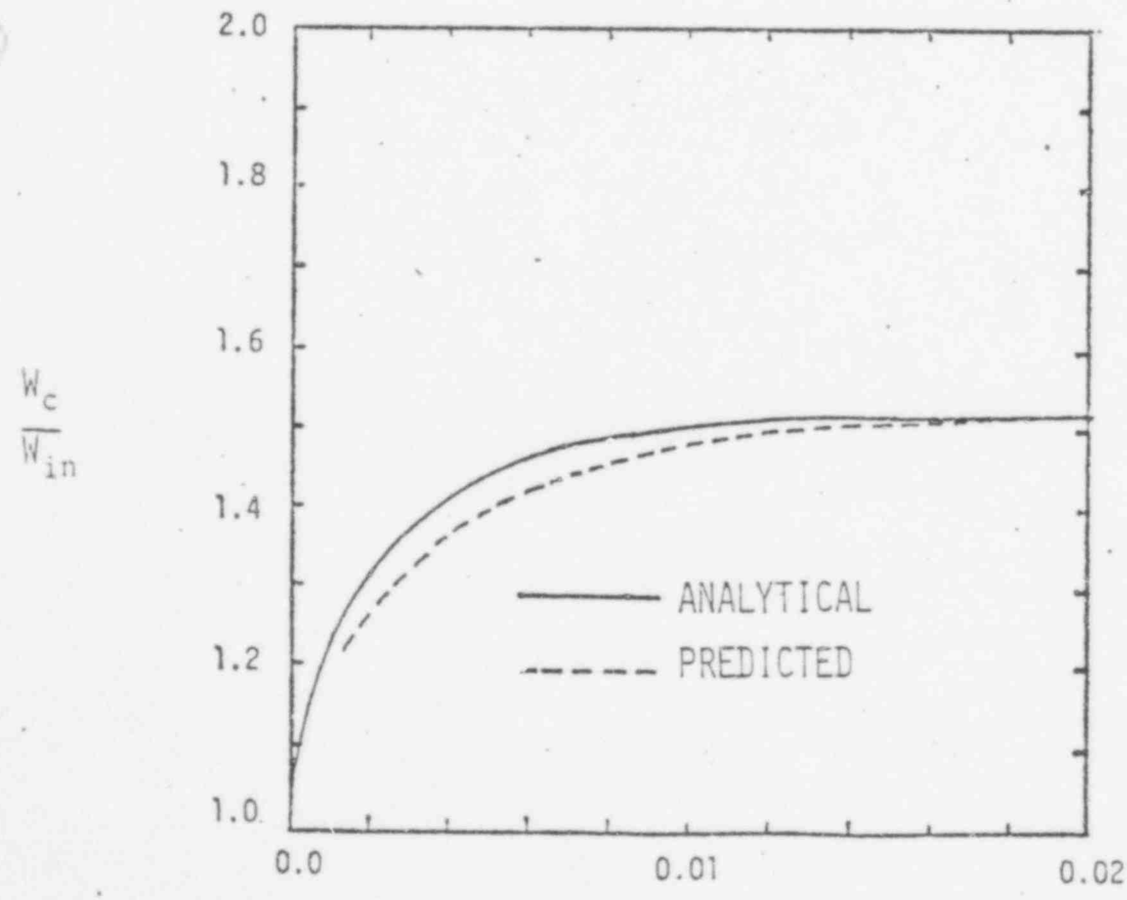


COORDINATE LINES FOR THE CONCENTRIC RING AFTER THE TRANSFORMATION

689324

66





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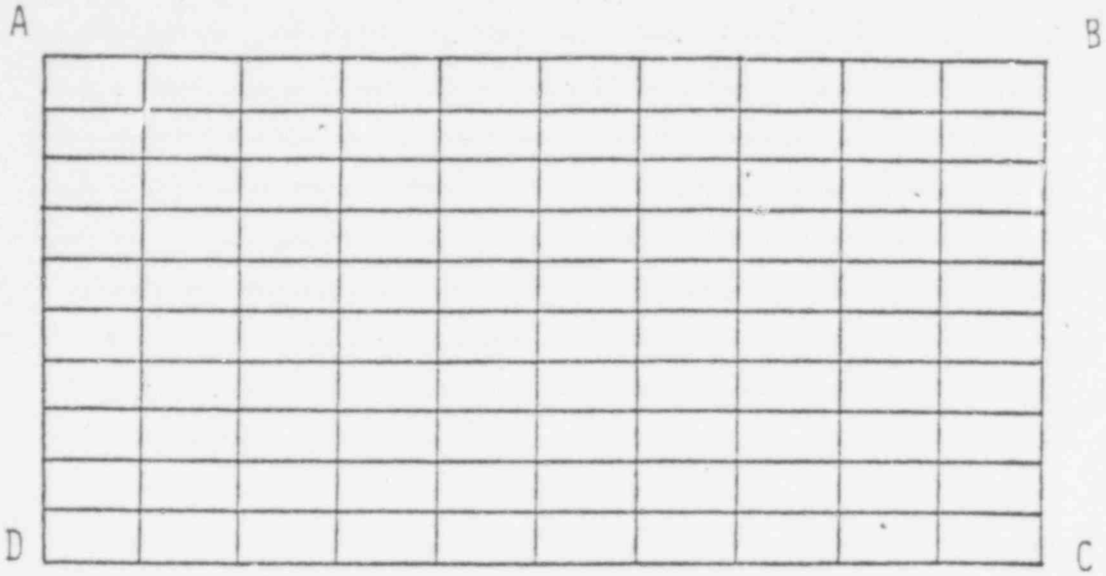
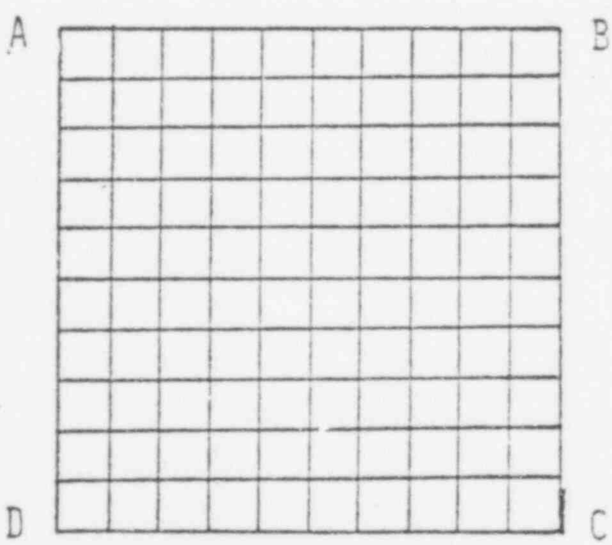
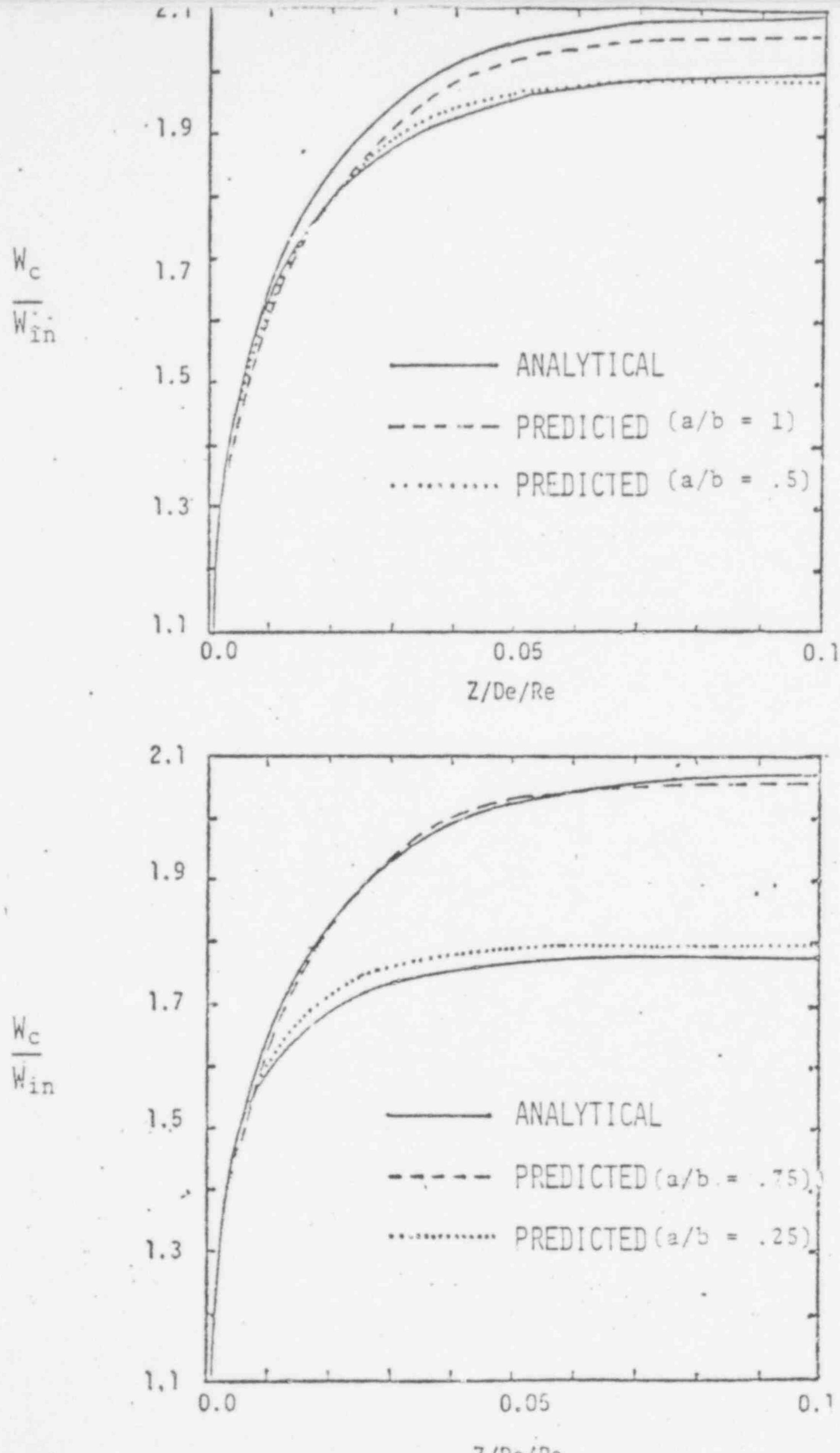
RADIAL AND CENTER LINE VELOCITIES FOR
THE CONCENTRIC RINGS WITH THE ASPECT RATIOS OF 0.5



COORDINATE LINES FOR THE RECTANGULAR DUCT
BEFORE THE TRANSFORMATION



COORDINATE LINES FOR THE RECTANGULAR DUCT
AFTER THE TRANSFORMATION



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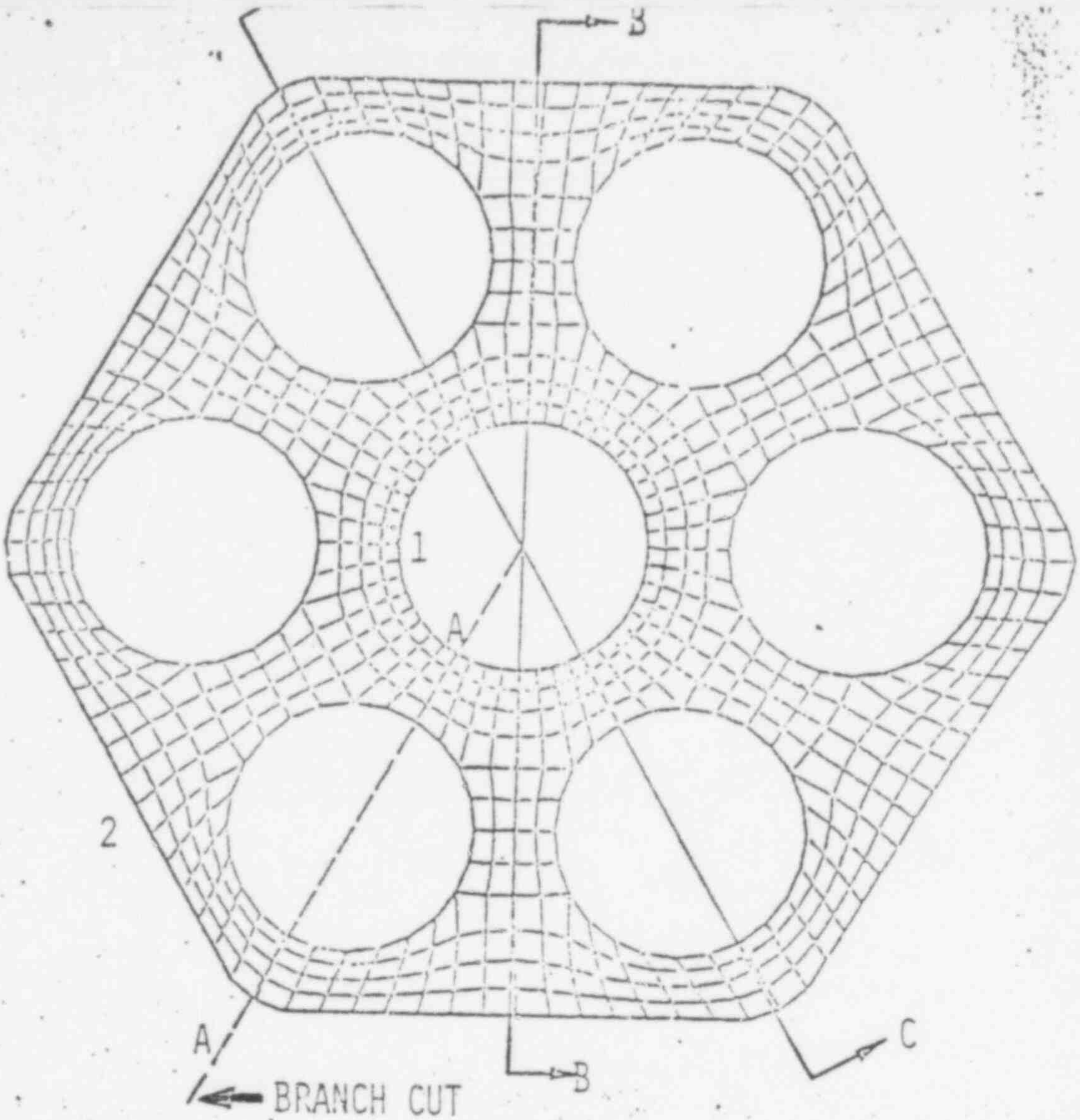
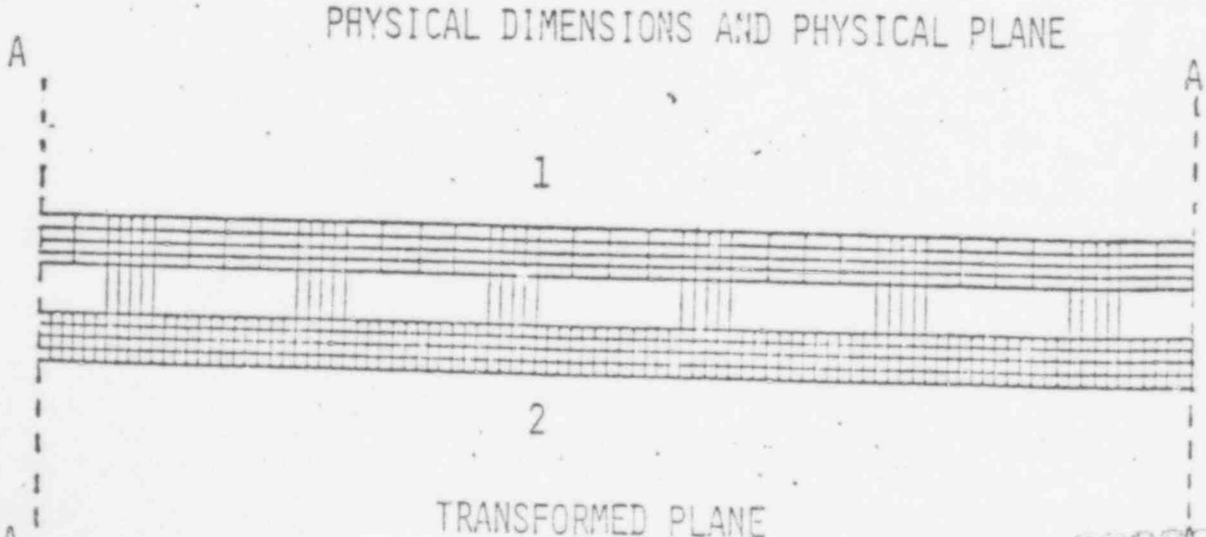
CENTER LINE VELOCITY FOR RECTANGULAR
DUCTS OF VARIOUS ASPECT RATIOS



Radius = 3.0 mm
Gap between pins = 1.9 mm
Flat to flat = 22 mm
V_m = 2.15 m/s
- Re = 3.373×10^4

PHYSICAL DIMENSIONS AND PHYSICAL PLANE

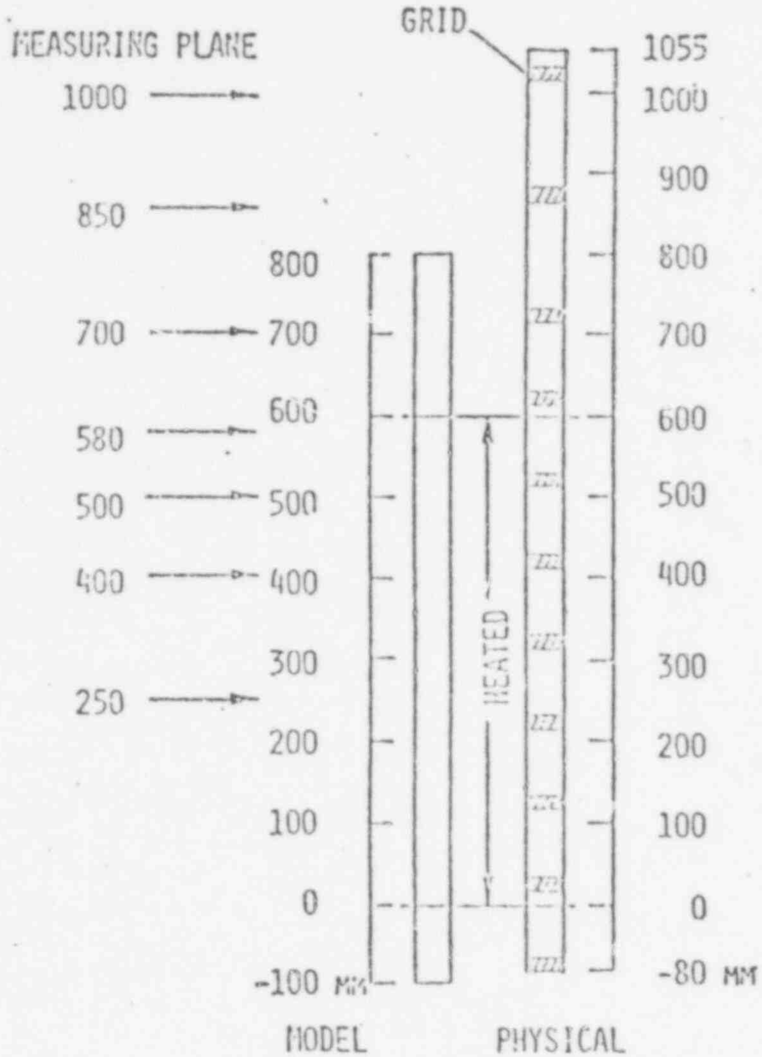


TRANSFORMED PLANE

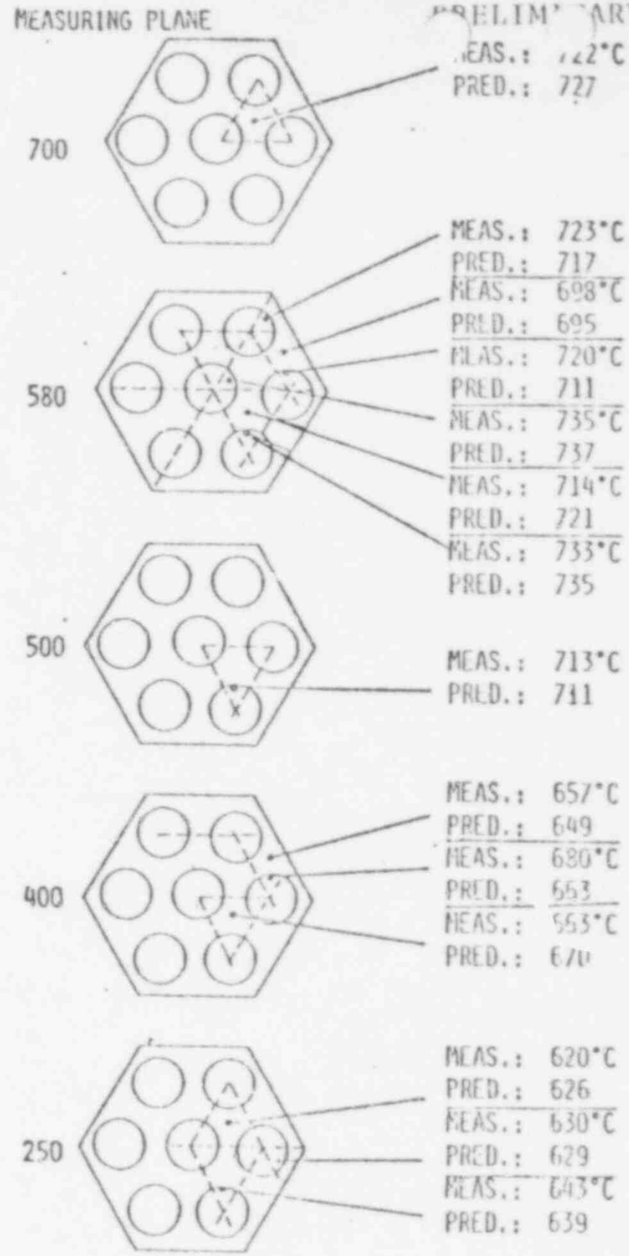
6893A28

70

PRELIMINARY RESULTS



PHYSICAL MODELING



COMPARISON BETWEEN MEASUREMENTS AND PREDICTIONS

14

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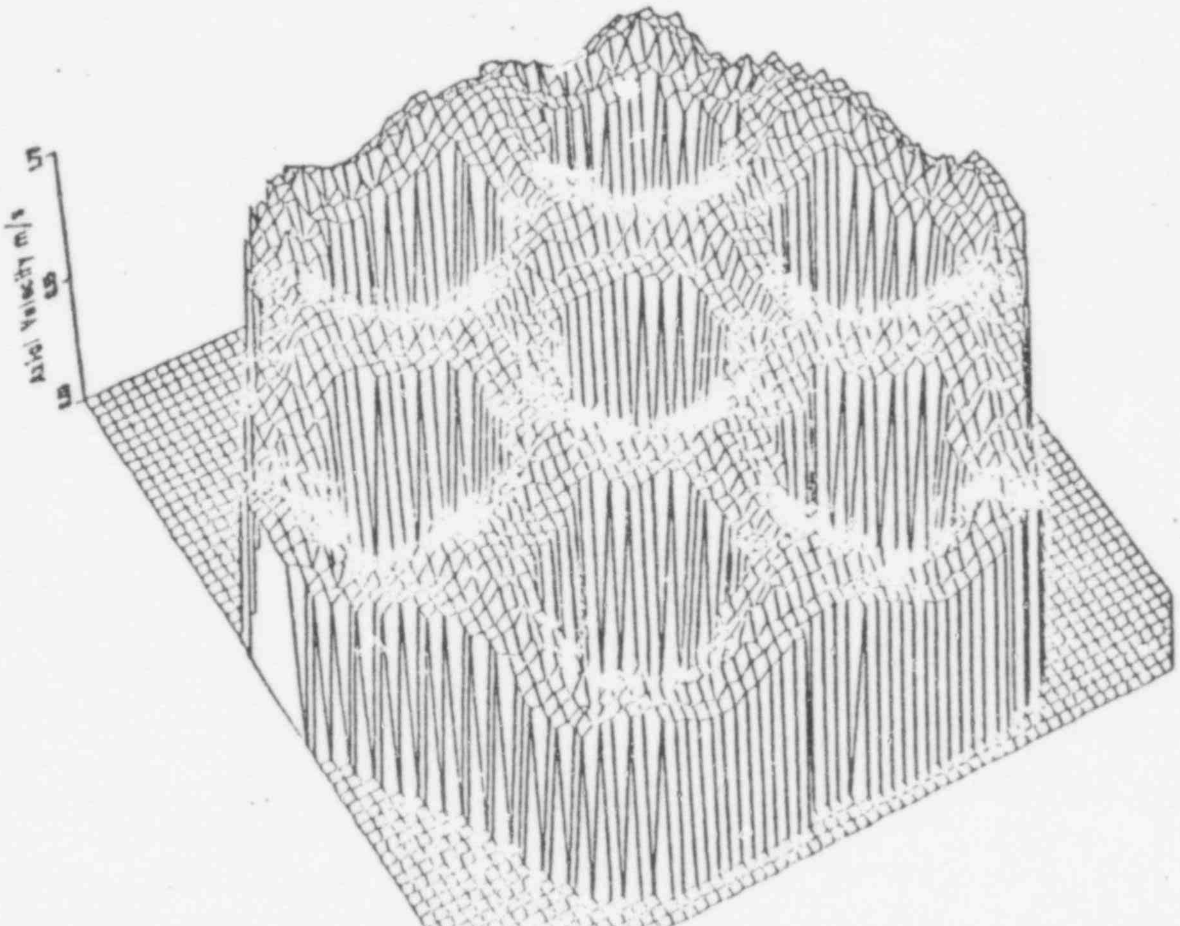
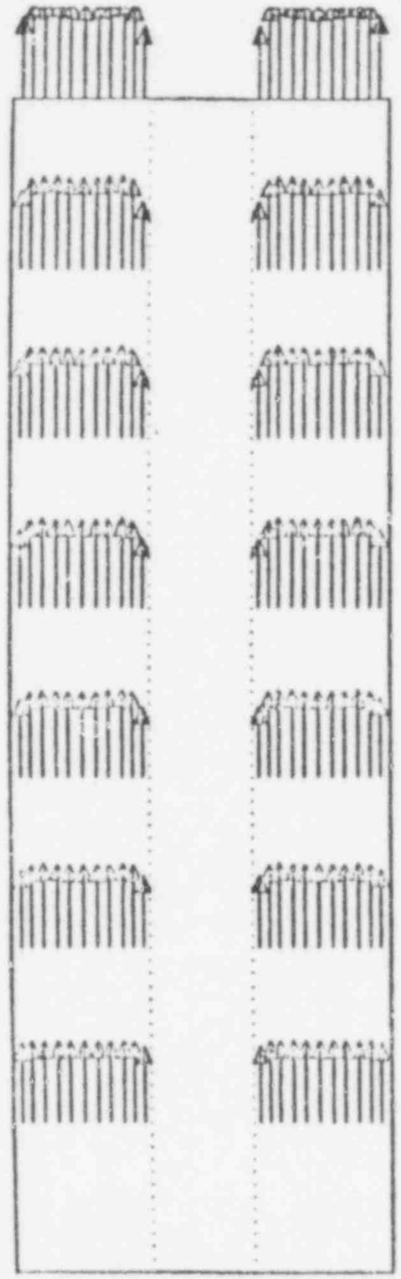
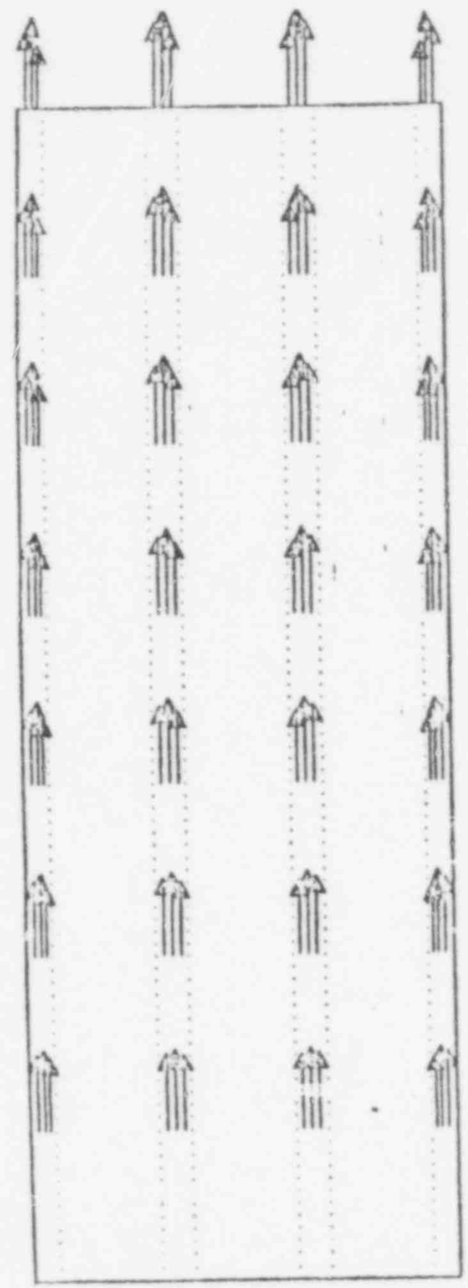


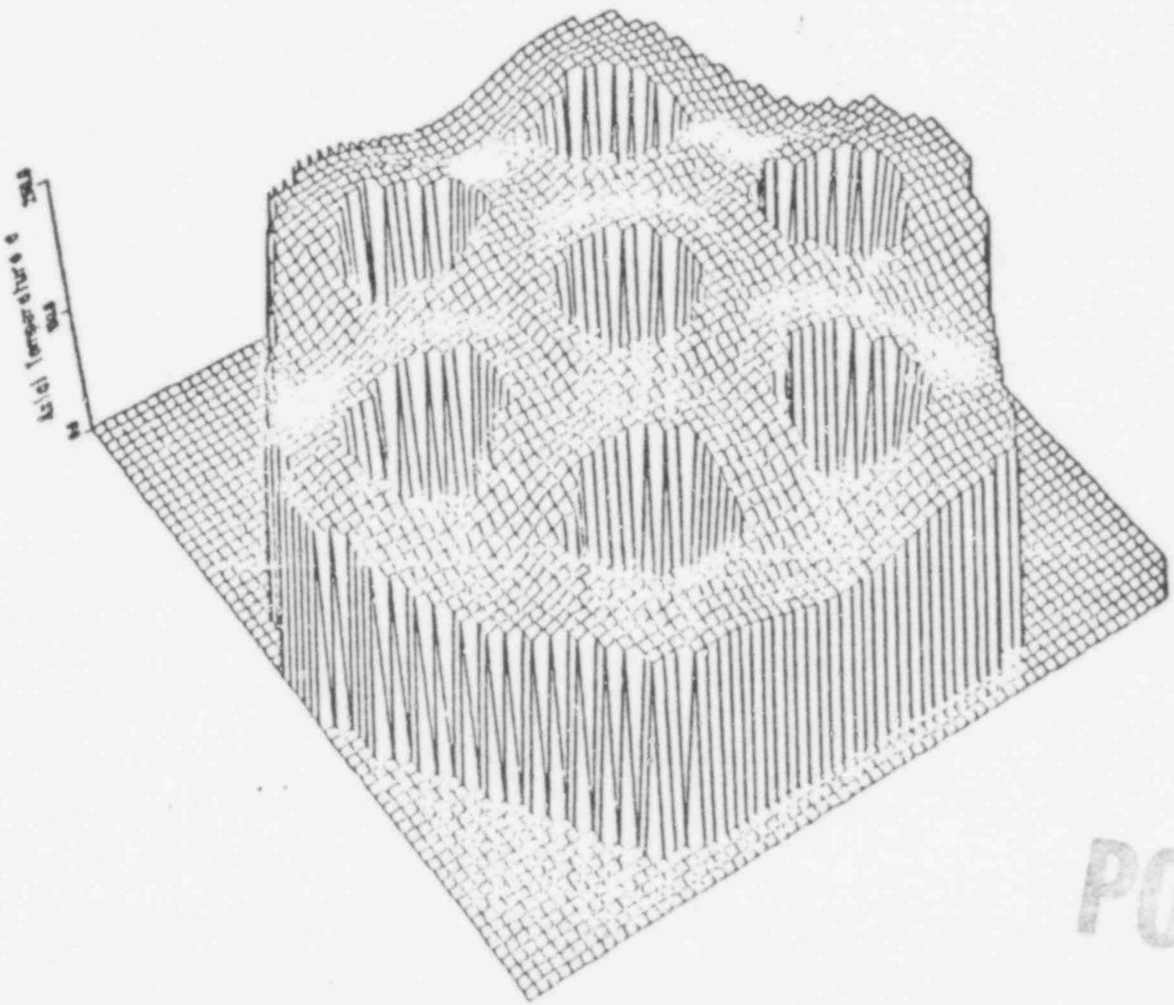
AXIAL VELOCITY DISTRIBUTION AT ELEVATION 800 MM



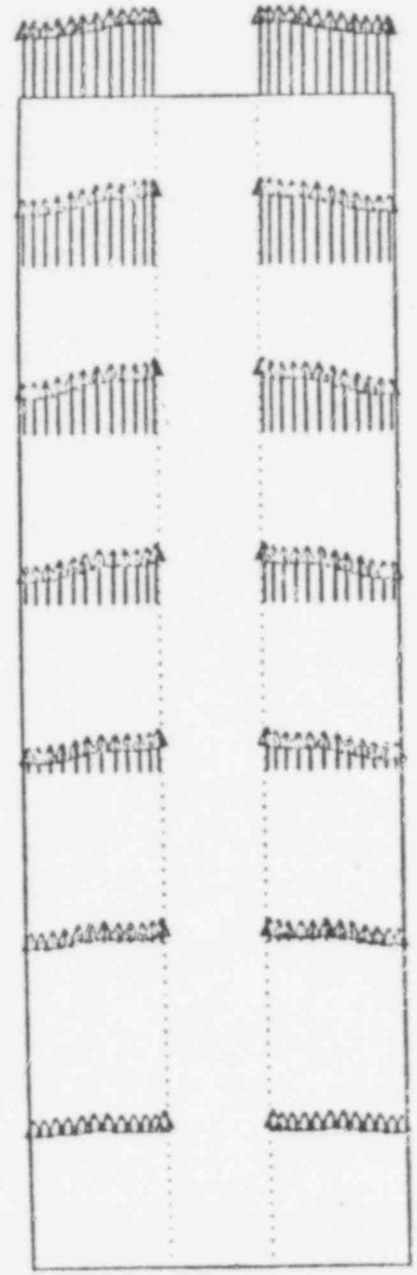
AXIAL VELOCITY DISTRIBUTIONS ACROSS B-B



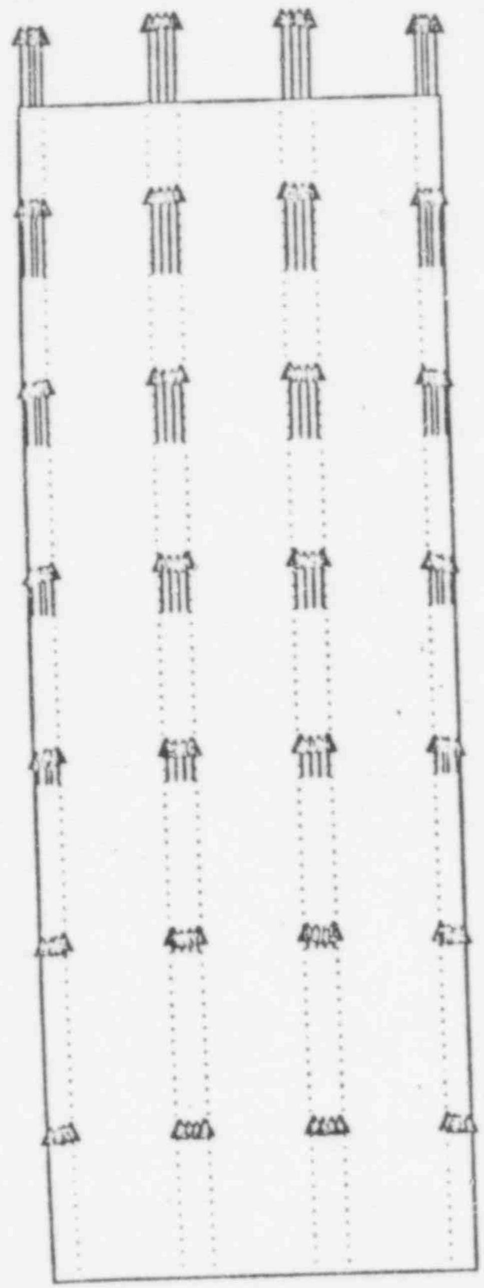
AXIAL VELOCITY DISTRIBUTIONS ACROSS C-C



TEMPERATURE DISTRIBUTION AT ELEVATION 800 MM



TEMPERATURE DISTRIBUTIONS ACROSS B-B



TEMPERATURE DISTRIBUTION ACROSS C-C

AVAILABILITY OF COMMIX-1

- NRC APPROVED
- TAPE SENT TO

1) UK AEA (ENGLAND)
2) ATOMICS INTERNATIONAL
3) WARD
4) GE
5) MIT
6) BABCOCK & WILCOX
7) BROOKHAVEN NATIONAL LABORATORY
8) GENERAL ATOMICS
9) ISPRA (ITALY)
10) UNIVERSITY OF ARIZONA
11) FOSTER WHEELER

S?IN-OFF BENEFITS FROM NRC SPONSORED

COMMIX CODE DEVELOPMENT WORK

DOE SPONSORED PROGRAMS:

I. LMFBR IHX APPLICATIONS:
COMMIX-IHX CODE

II. LMFBR STEAM GENERATOR APPLICATIONS:
COMMIX-SG CODE

III. SOLAR APPLICATIONS:
COMMIX-SA CODE (THERMOCLINE STORAGE TANK)

79

BNL LMFBR EXPERIMENTAL PROGRAM

PRESENTATION FOR THE ACRS

AUGUST 7, 1979

WASHINGTON, D.C.

PRESENTED BY

THEODORE GINSBERG

THERMAL HYDRAULIC DEVELOPMENT DIVISION
DEP MENT OF NUCLEAR ENERGY
BRO HAVEN NATIONAL LABORATORY
UPTON, NEW YORK 11973

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

TODAY'S PRESENTATION

- OVERVIEW
- SCOPE
- RESULTS
- PROGRAM SUMMARY

BNL FAST REACTOR SAFETY EXPERIMENTS

- OBJECTIVE
  - Investigate Thermal Hydraulic Phenomena of Importance in Fast Breeder Reactor Safety Analysis
  - Develop Data Base for Code Evaluation
- METHODOLOGY
  - HCDA Phenomena Identification
  - Analytical Modeling
  - Scaling Analysis
  - Experimental Simulation
  - Prediction

SCOPE OF BNL PROGRAMS

SIMULATION EXPERIMENT	ACCIDENT PHASE	ISSUE
• Core Dispersion	Transition Phase	Recriticality; Fuel Compaction; Steel Vapor Dispersion
• Multiphase Fuel Relocation and Freezing Dynamics	Transition Phase	Recriticality; Reactivity Loss; Bottled Pool
• Heat Transfer In Internally Heated Boiling Pools	PAHR Transition Phase	Structure Coolability; Impact on Dispersion
• HCDA Energetics: Entrainment By Taylor Instability	Post-Disassembly Bubbly Expansion	Sodium Working Fluid Potential
• Transition Phase Assessment		Identification of Key Safety Issues

CORE DISPERSION INVESTIGATIONS

- PURPOSE

  - Characterize Flow Dynamics of Fuel-Steel Boiling Pools

  - Evaluate Impact on Transition Phase Recriticality Potential

- METHODOLOGY

  - Model Flow Dynamics With Prototypic Simulant Fluid Systems

  - Model Volume-Heated Dispersion With Simulant Fluids and Heat Sources

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)

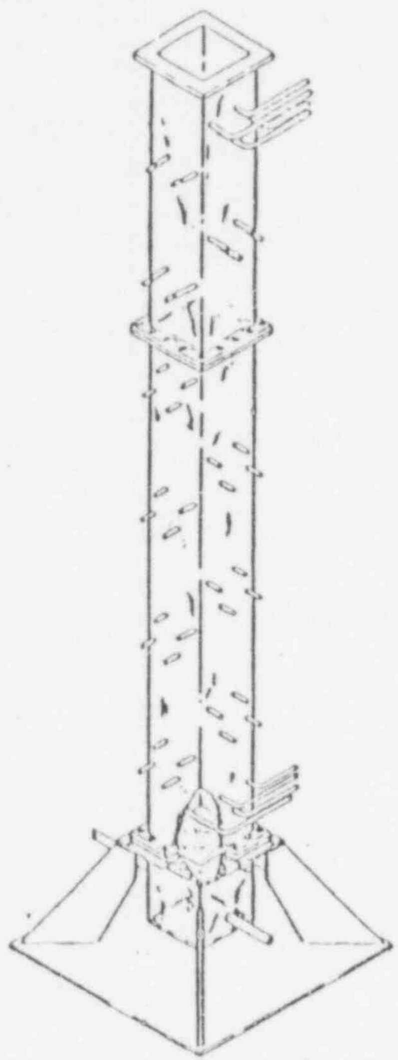
    MICROWAVE HEATED HIGH POWER DISPERSION TESTS
  - OBJECTIVES

    FLOW REGIME TRANSITIONS AND MULTIPHASE

    FLOW DYNAMICS IN INTERNALLY HEATED POOLS

    OPEN, CLOSED, HEAT LOSSES, REFLUXING

HYDRODYNAMIC DISPERSAL



SCHEMATIC OF PROPOSED HYDRODYNAMIC DISPERSAL TEST SECTION

PURPOSE

- Develop Flow Regime Transition Criteria and Void Dynamics Information with Prototypic Dispersion Parameter Magnitudes (Dimensionless) Water-Air and Mercury-Air Systems
- Provide Base Data for Evaluation of the SIMMER Model

STATUS

- Engineering Design Complete
- Preliminary Tests for Injector Disturbance in the Near Future
- Narrow Beam γ-Densitometer Complete

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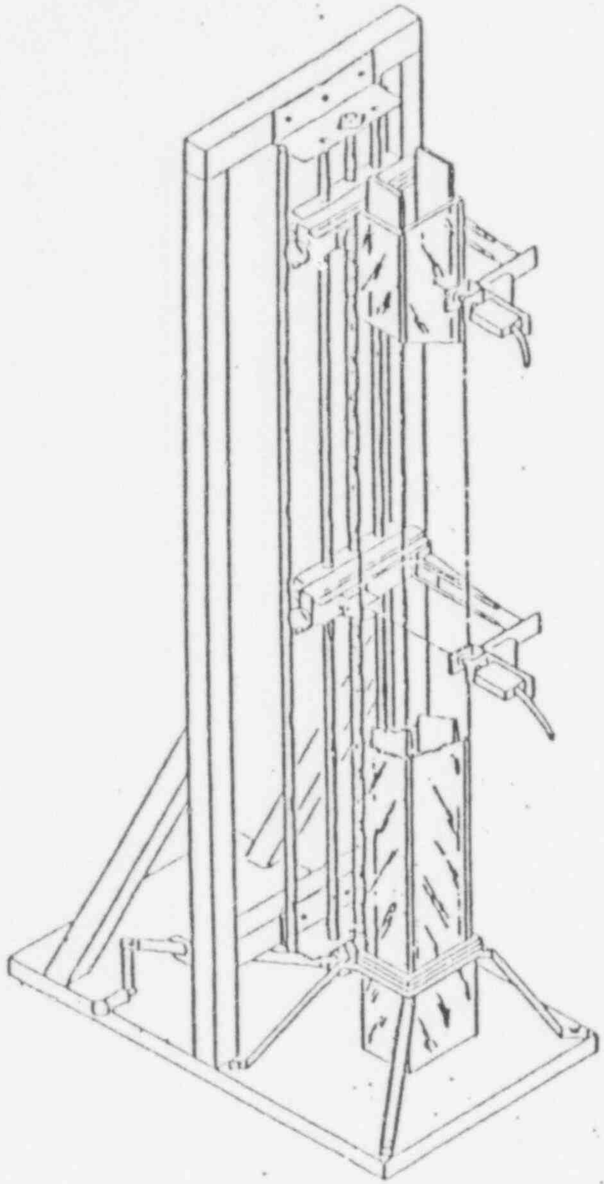
HYDRODYNAMIC DISPERSAL

PRELIMINARY FEASIBILITY TESTS



TEST VESSEL AND GAMMA DENSITOMETER SYSTEM

PURPOSE

- Obtain More Definitive Visual Void and Instrumented Void Dynamic Behavior in Near-Prototype Geometries
- Provide Design Evaluation Information for Hydrodynamic Dispersal Tests.
  - Without Injector Tube Simulations
  - With Injector Tube Simulations

STATUS

- Test Vessel Constructed and Assembled
- Support Structure Complete
- Densitometers Complete
- Preliminary Tests Initiated

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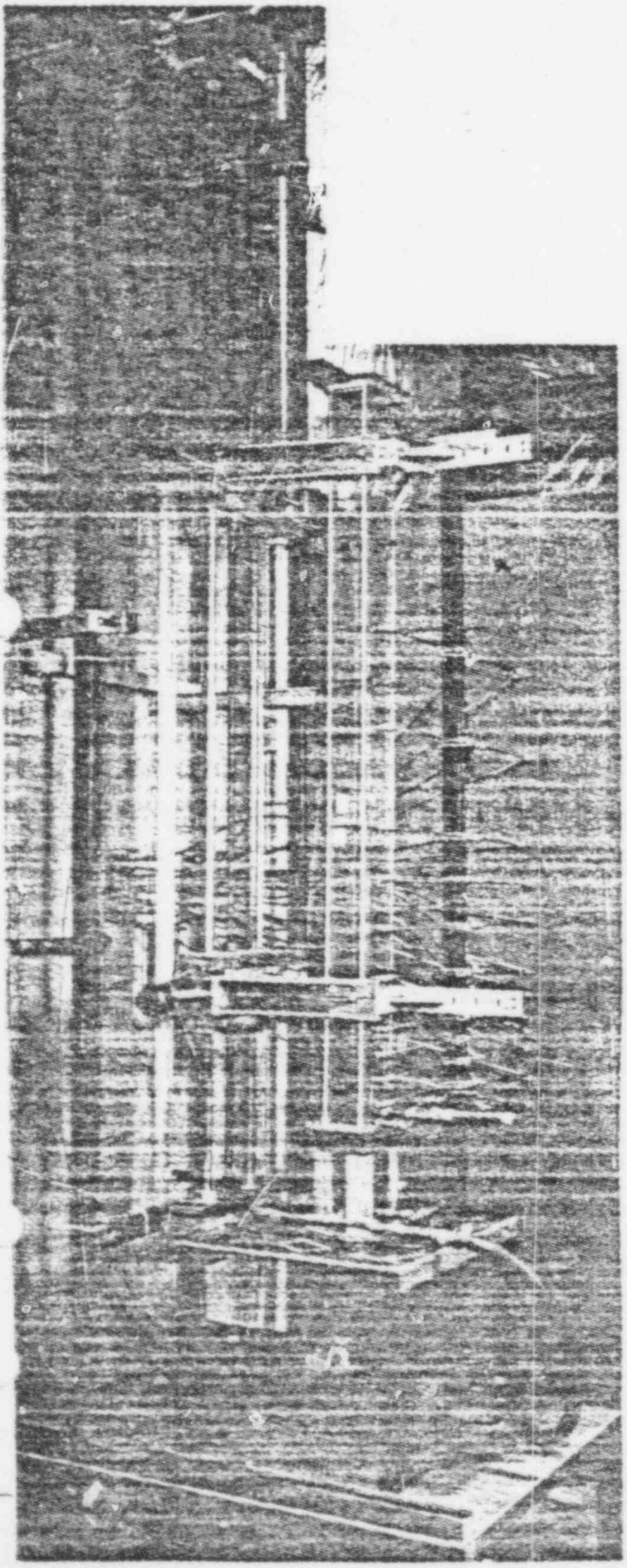
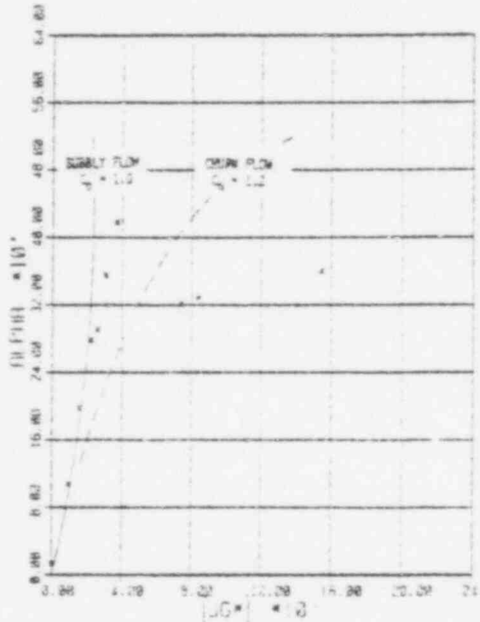
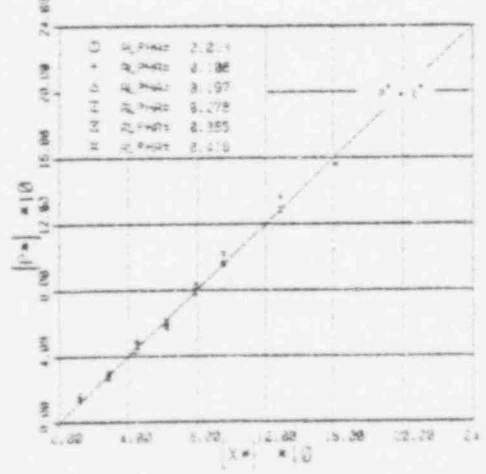
HYDRODYNAMIC SIMULATION — LARGE L/D DISPERSION AND VOID DYNAMICS

APPARATUS



TYPICAL RESULTS





HYDRODYNAMIC DISPERSION

- PRELIMINARY RESULTS

  - CHURN FLOW STABLE TO AT LEAST THREE TIMES KUTATELADZE LIMIT. CONSISTENT WITH ELECTRICALLY HEATED RESULTS

  - BUBBLY FLOW LESS STABLE THAN IN ELECTRICALLY HEATED TESTS

- IMPLICATIONS

  - DISPERSED REGIME MORE LIMITED THAN ASSUMED IN LITERATURE

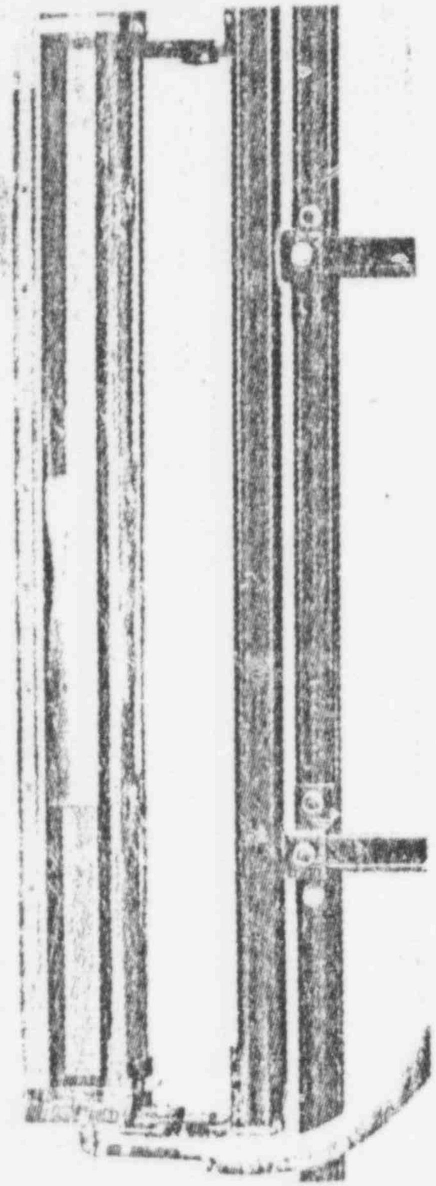
  - BUBBLE STABILITY EFFECTS ON DISPERSION SIGNIFICANT

DISPERSION IN INTERNALLY HEATED BOILING POOLS

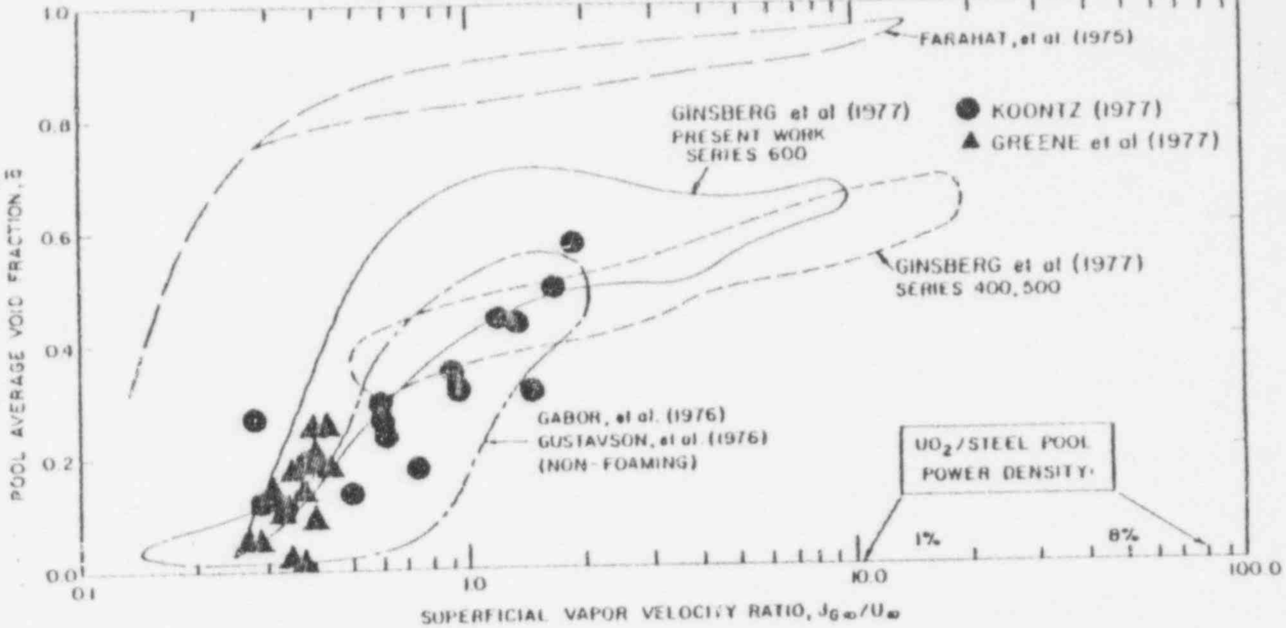
- ELECTRICAL HEATING
  - LARGE L/D
  - SMALL L/D
- MICROWAVE HEATING

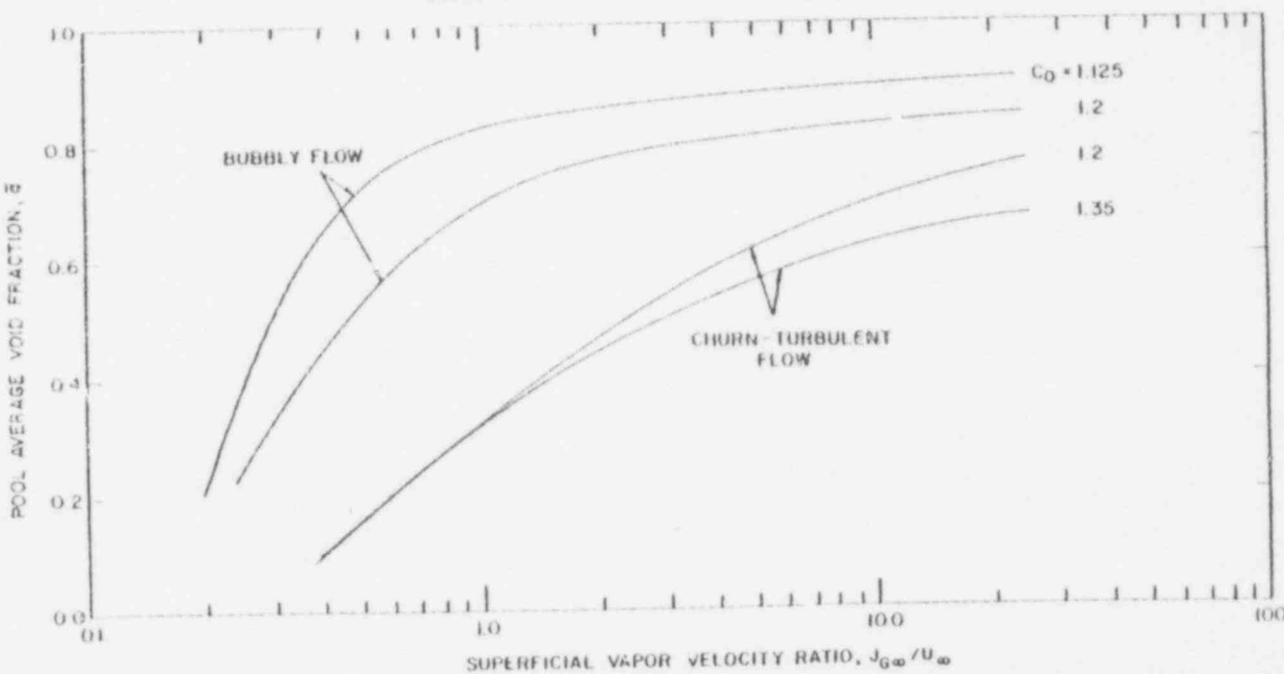
ELECTRICAL HEATING — LARGE L/D DISPERSION AND VOID FRACTION

APPARATUS



TYPICAL RESULTS





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DISPERSION - ELECTRICAL HEATING

APPLICATION OF SIMULANT EXPERIMENTS AND ANALYSIS

- QUESTION:
  - What is Potential for Dispersal of Boiling Pool of Fuel (Heat Source) and Steel (Vapor Source)?

- ASSUME
  - Steel Uniformly Mixed With Fuel and At Saturation Temperature
  - Vapor Production Rate is $J_{G\infty}/U_{\infty}$
  - Pool Open at Top
  - 1-D Drift Flux Model Applies

- Major Scaling Parameters
  - $\frac{J_{G\infty}}{U_{\infty}}$, $\frac{J_{G\infty}}{J_{GK}}$

TABLE 7.1

Comparison of Experimental and Transition Phase Dimensionless Parameters

	Present Water Experiments	Single-Assembly Molten UO_2/steel Pool		Core-Wide Molten UO_2/steel Pool	
Flow Field	Single Component	Multicomponent Multiphase		Multicomponent Multiphase	
Power Density (% of LMFBR nominal fuel power density*)	0 - 3.1	1	8	1	8
$j_{g\infty}/U_\infty$**	0 - 19	10.6	85	10.6	85
$j_{g\infty}/j_{gk}$**	0 - 4.8	0.56	4.5	0.56	4.5
ρ_g/ρ_ℓ	6.2×10^{-4}	2.8×10^{-5}	2.8×10^{-5}	2.8×10^{-5}	2.8×10^{-5}
$j_{g\infty}D/\gamma_\ell$	4.6×10^5	2.9×10^5	2.3×10^6	5.3×10^6	4.2×10^7
H_o/D	4.8 - 9.7	3-6	3-6	0.16-0.32	0.16-0.32

*270 cal/cm^3-s.

**The calculation of $j_{g\infty}$ assumes all energy generated is available for steel vaporization.

EXTRAPOLATED TRANSITION PHASE CHARACTERISTICS

POWER AVAILABLE FOR STEEL VAPORIZATION (PERCENT OF LMFBR NOMINAL POWER DENSITY)	$\frac{J_{G\infty}}{U_{\infty}}$	$\frac{J_{G\infty}}{J_{GK}}$	FLOW REGIME	POOL-AVERAGE VOID FRACTION $\bar{\alpha}$
1	10.6	0.56	CHURN-TURBULENT	0.6 - 0.7
8	85	4.5	CHURN-TURBULENT	0.7 - 0.8

DISPERSION - SIMULANTS

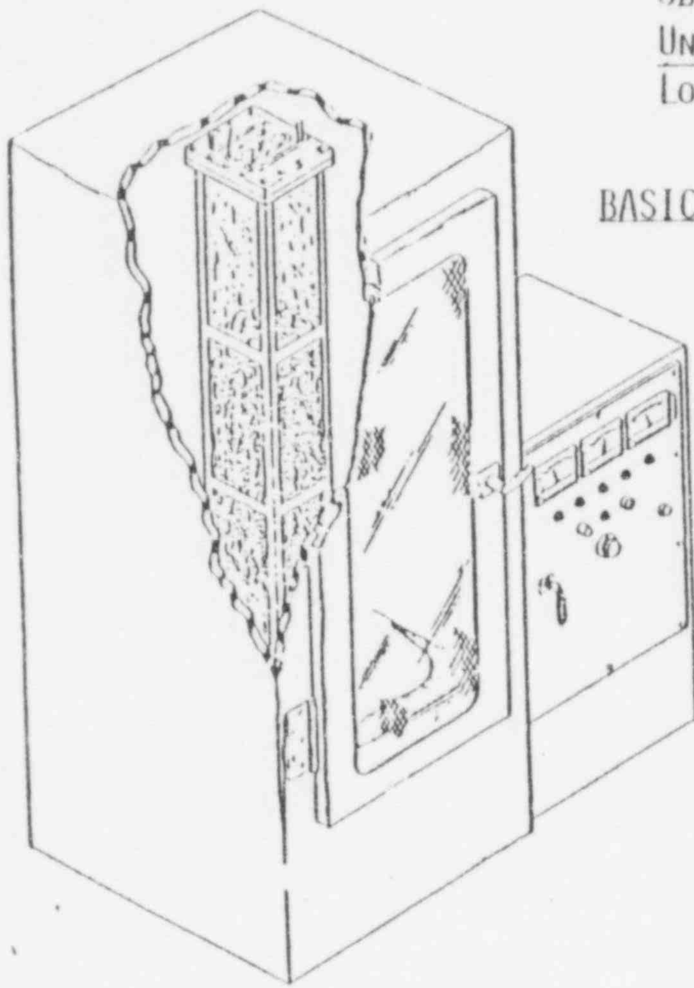
- CONCLUSIONS

  - Dispersal Potential by Boiling Steel is Substantial

  - Extrapolation to Higher Power Required

  - Data to Cover Wider Range of $j_{G\infty}/u_\infty$ Needed

DISPERSION IN INTERNALLY HEATED BOILING POOLS-MICROWAVE



SCHEMATIC OF MICROWAVE DISPERSAL TEST FACILITY

PURPOSE

Obtain Flow Regime and Void Dynamic Behavior With Uniformly Heated Dispersed Flows: Closed, Heat Losses, Refluxing

BASIC PROBLEMS

- Coupling Energy Source to Dispersed Flows
- Spatial Variation of Power Density

STATUS

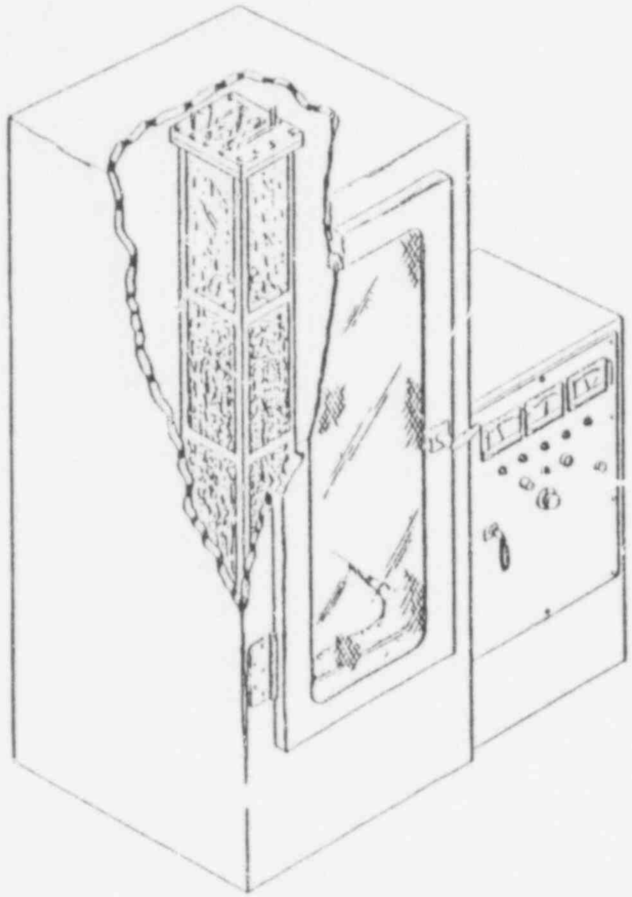
- Conceptual Design Complete
- Microwave Coupling Study Complete
- Transient Pool Modeling Complete
- Prepared for Quotes on Microwave System.

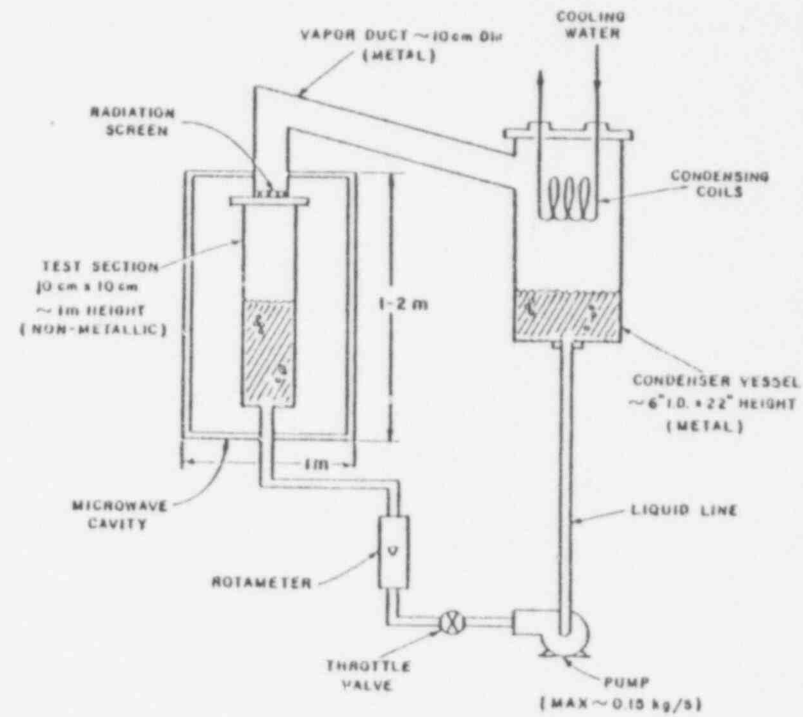
MICROWAVE POWER SOURCE

- PREDICTABLE/MEASURABLE POWER DENSITY
  - Multimode Cavity (Oven)
  - Single Mode Applicator
- POWER COUPLED TO DISPERSED FLOWS
  - Cyclohexane-Ethanol Solution

DISPERSION IN INTERNALLY HEATED BOILING POOLS-MICROWAVE

EXPERIMENTAL CONCEPT





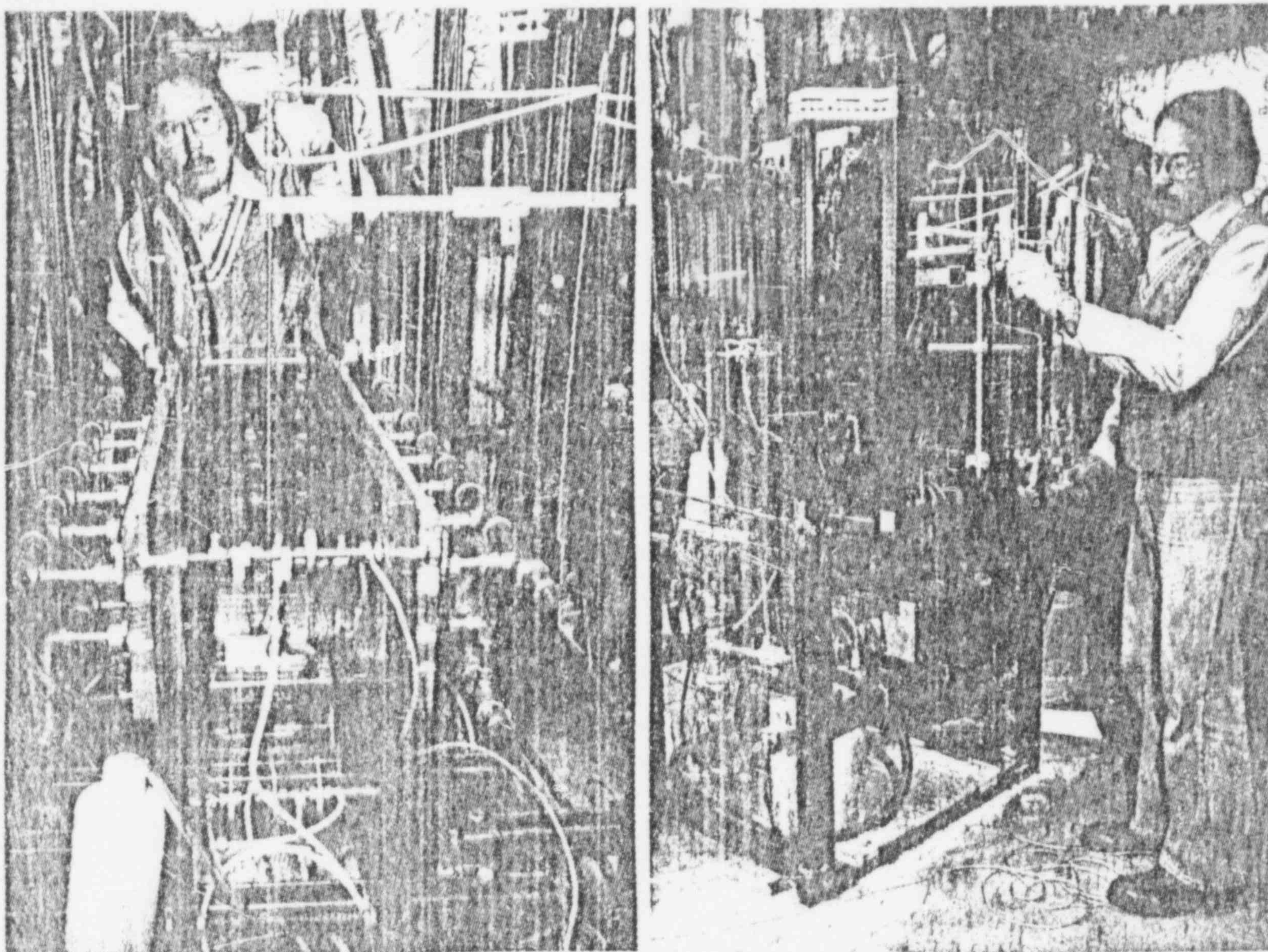
MICROWAVE HEATING TEST LOOP

HEAT TRANSFER IN INTERNALLY HEATED BOILING POOLS

(REPORT ISSUED)

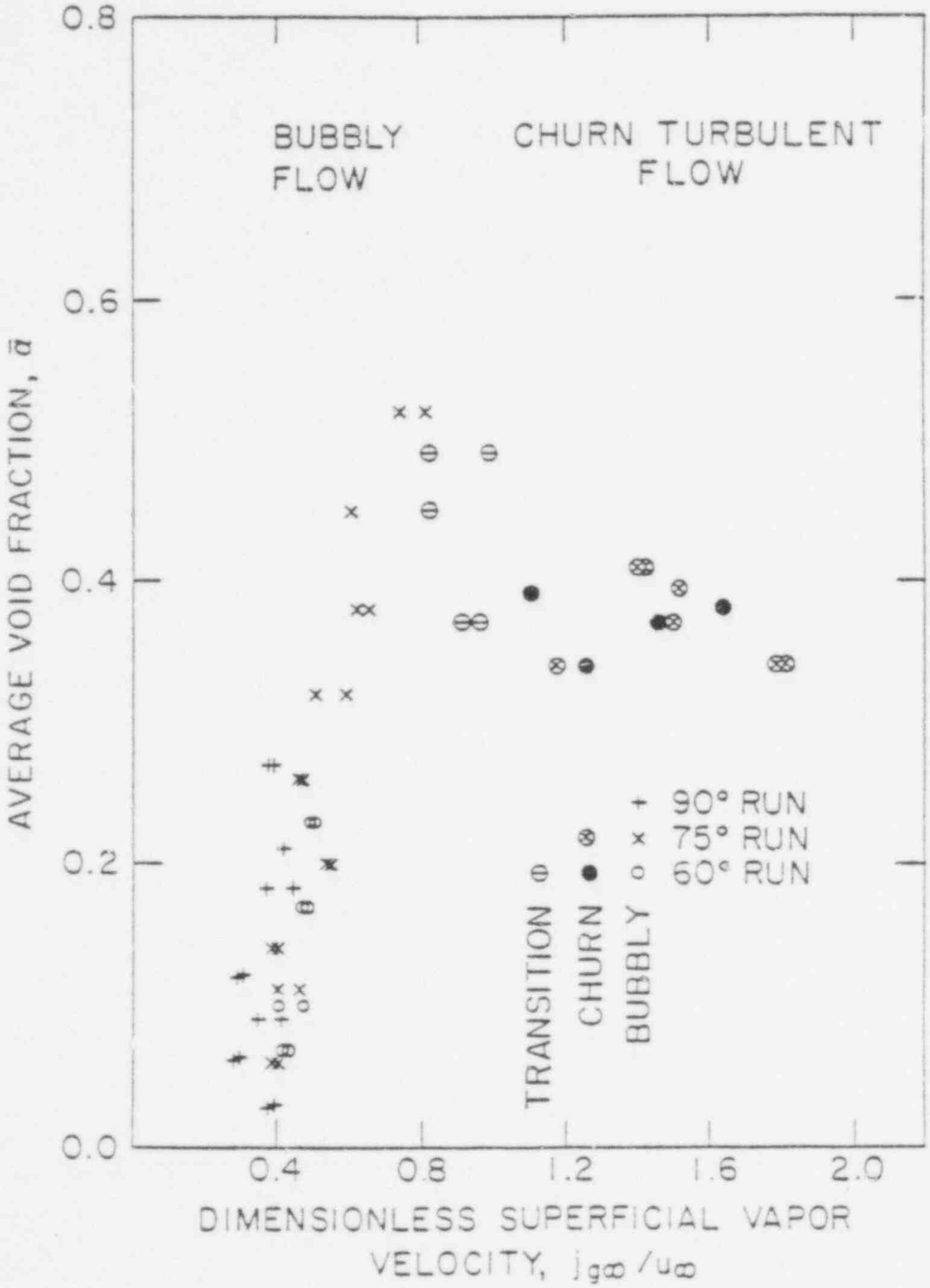
- PURPOSE
  - Develop Correlations and Models to Describe Boundary Heat Loss Mechanisms
  - Applicable to PAHR and Transition Phase

- 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



HEAT TRANSFER APPARATUS FOR
BOILING POOL STUDIES

BOILING POOL VOID BEHAVIOR



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BOILING POOL HEAT TRANSFER

- CONCLUSIONS

BUBBLY FLOW

- Distribution of Boundary Heat Transfer Coefficient Resembles Natural Convection
- Laminar and Turbulent Heat Transfer Correlations Developed
- Local Heat Transfer Coefficient Greater Than Previous Estimates by a Factor ~ 2

CHURN-TURBULENT FLOW

- Distribution of Boundary Heat Transfer Coefficient More Uniform Than Bubbly Flow
- Average Heat Transfer Coefficient Greater Than Bubbly Flow By Approximately a Factor of 2
- Boundary Layer Destruction-Renewal Process Believed Responsible for the Magnitude of the Heat Transfer
- Transition to Churn Turbulent Flow Occurred Suddenly at $j_{g\infty}/U_{\infty} \sim 1$

MULTIPHASE FUEL RELOCATION AND FREEZING DYNAMICS

- PURPOSE

  - Predict Potential for Formation of Frozen Barriers in Normally Open Flow Channels
  - Predict Potential for Formation of a "Bottled" Core During Transition Phase
  - Predict the Distribution of Fuel During Transition Phase

- METHODOLOGY

  - Model the Solidification Dynamics of Multiphase Fluids in a Thermodynamic State Characteristic of Transition Phase and PAHR Conditions in an LMFBR
  - Perform Small Scale Experiments With Simulant Fluids that Span the Range of Prandtl Number of Reactor Materials Appropriate as a Function of Void Fraction

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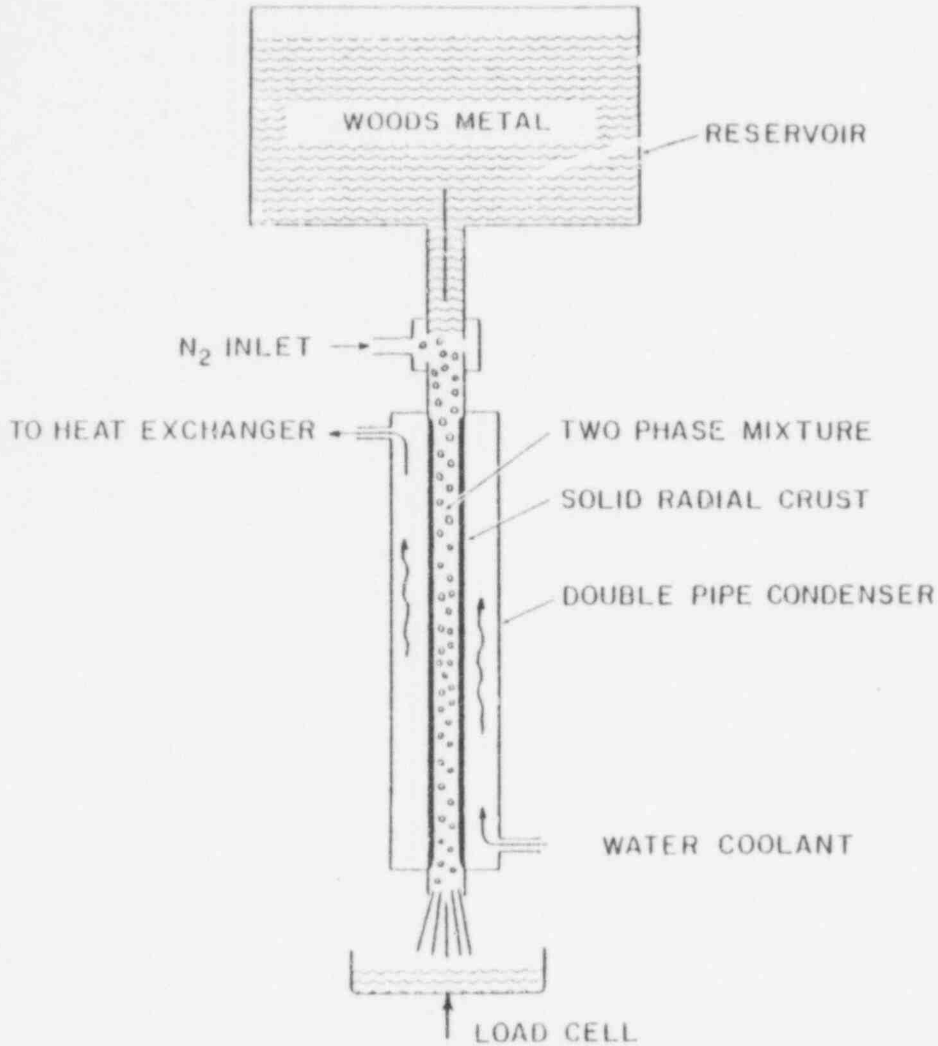
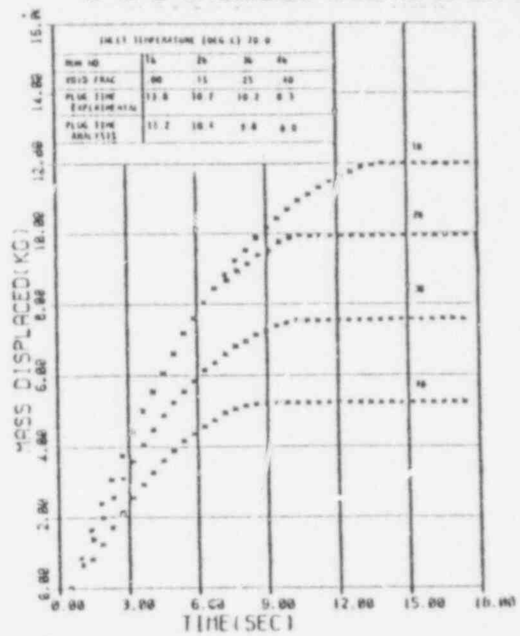
ASSOCIATED UNIVERSITIES, INC.

FUEL RELOCATION DYNAMICS SIMULATION STUDIES

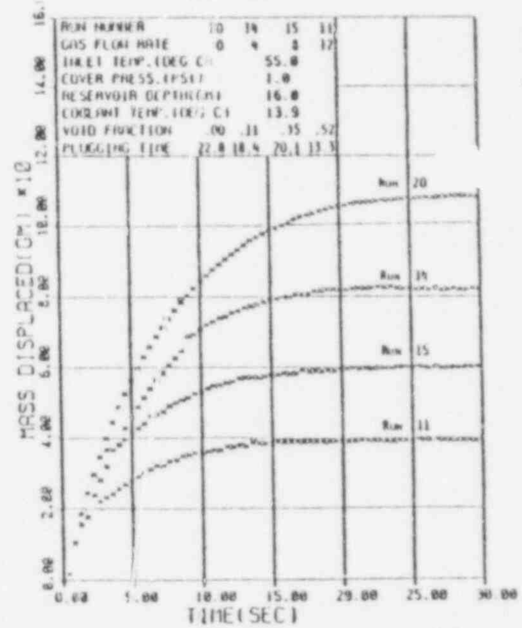


TWO-PHASE TRANSIENT FREEZING APPARATUS

TYPICAL RESULTS

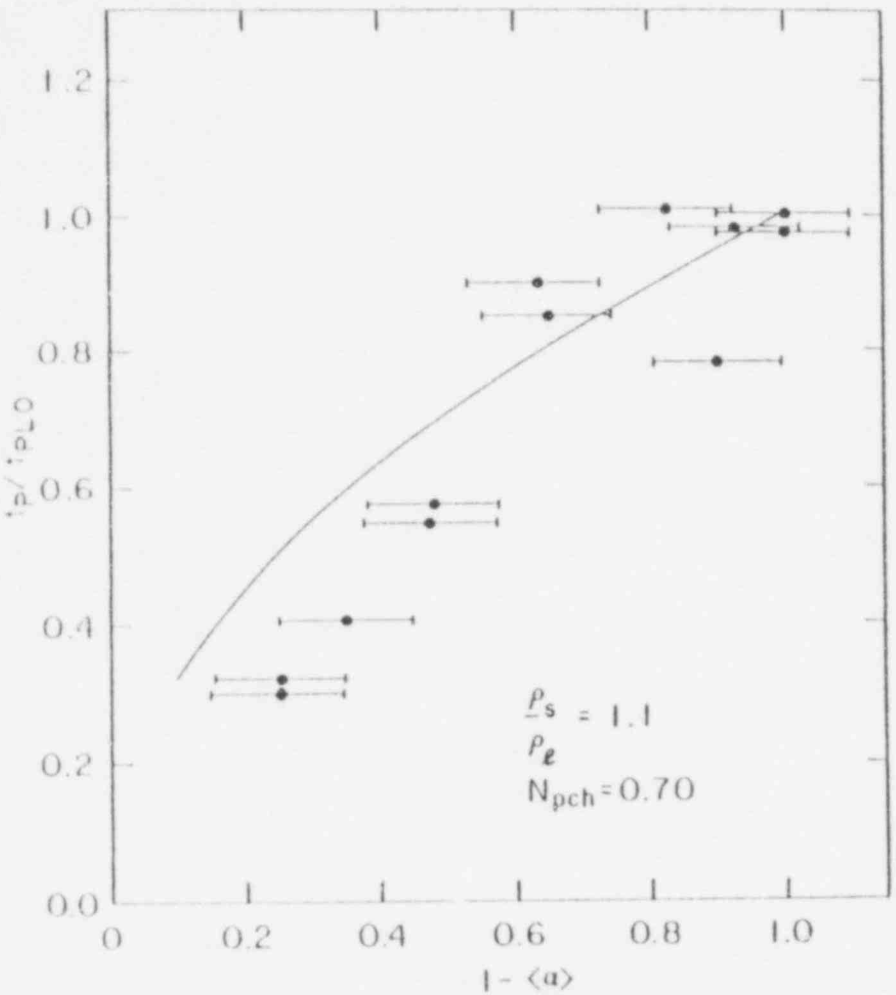


Woods Metal-N_2 $P_r = 0.02$

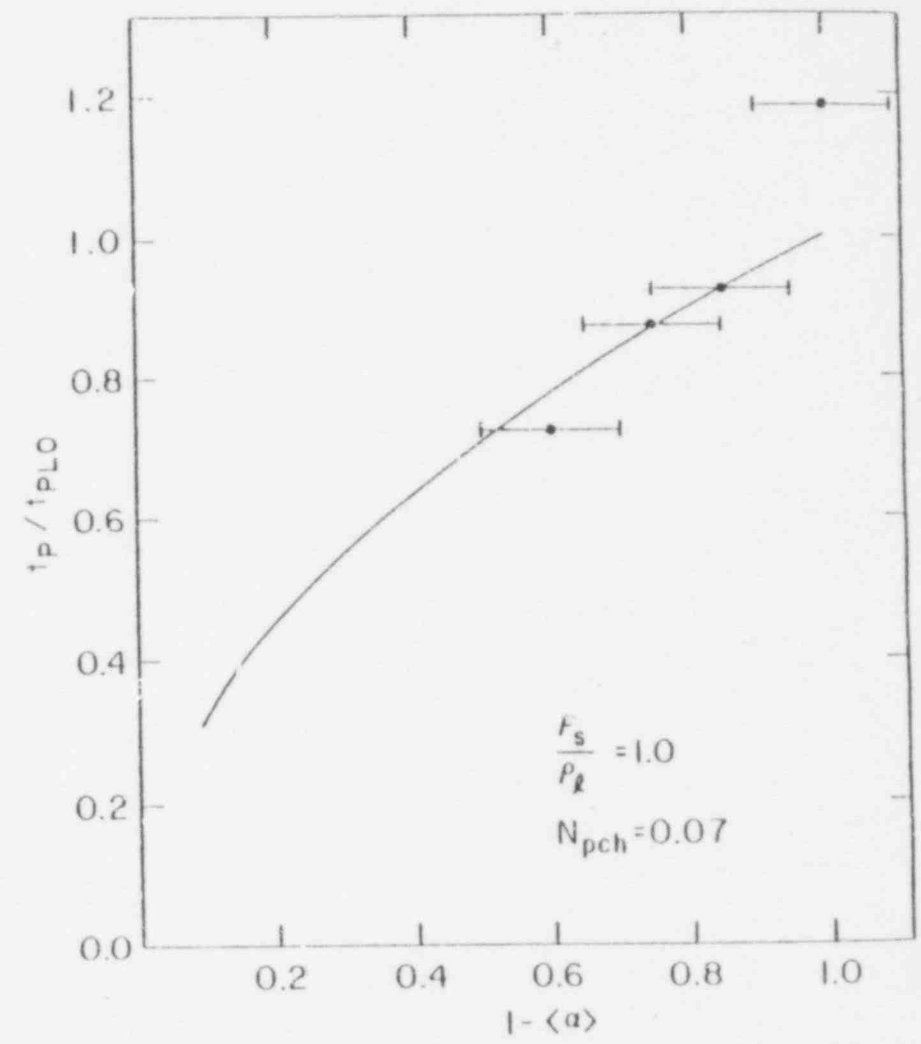


Paraffin Wax-N_2 $P_r = 40$

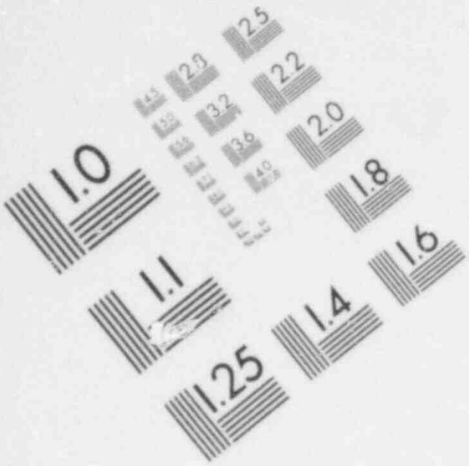
COMPARISON OF TWO-PHASE GAS-LIQUID FREEZING TIMES WITH VARIOUS VOID FRACTIONS FOR LOW AND HIGH PRANDTL NUMBER MIXTURES



WOOD'S METAL – N_2



PARAFFIN – N_2



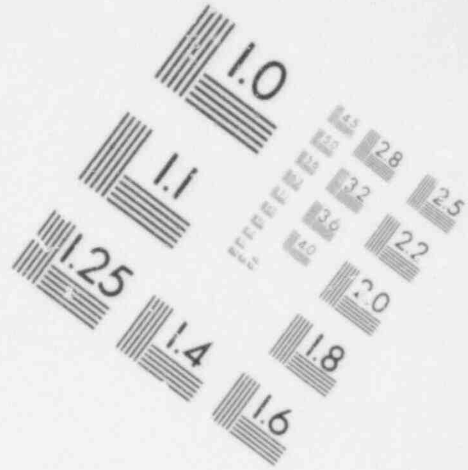
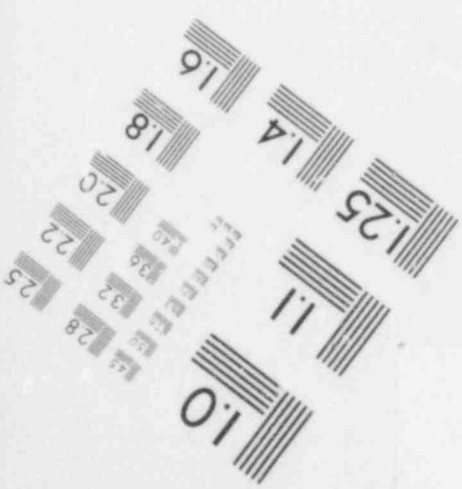
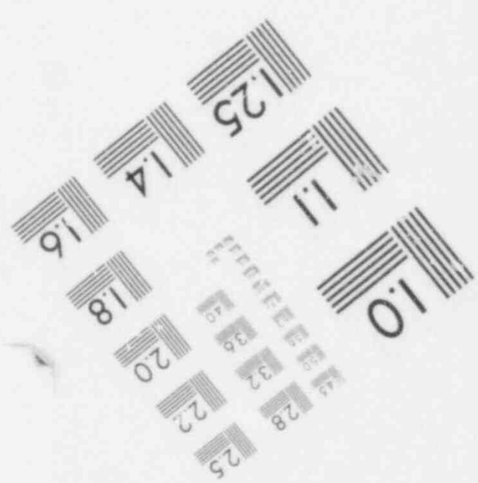


IMAGE EVALUATION TEST TARGET (MT-3)

6″





MULTIPHASE FUEL RELOCATION

- CONCLUSIONS:

  - Rates of Flow and Solidification With Stable Crusts is Adequately Characterized By Hydraulic Pressure Drop and Conduction Through the Crust.

  - Gas-Liquid Mixtures Exhibit Accelerated Solidification Rates Above the Liquid-Only Case.

  - Conduction-Limited Freezing Model Yields Upper Bound on Fuel Relocation Rates.

  - Freezing With Melting Walls in the Absence of Stable Crusts Represents a Lower Bound on Fuel Relocation Rates.

  - Major Uncertainties

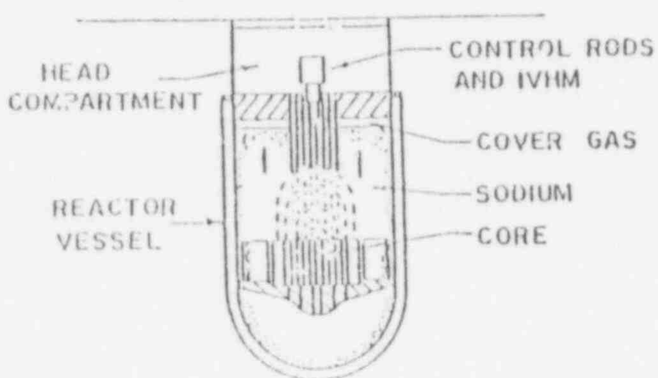
    Criteria for Crust Instability

    Mechanisms of Entrainment of Molten Wall Material

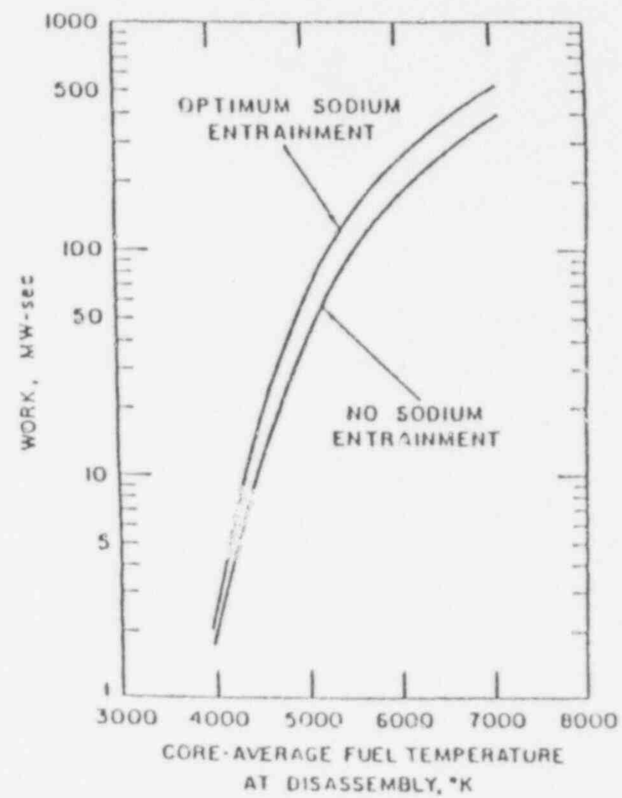
HCDA BUBBLE EXPANSION ENERGETICS
ROLE OF TAYLOR INSTABILITY

ENTRAINMENT EFFECT

REACTOR VESSEL SCHEMATIC



Initial bubble development for energetic LMFBR HCDA.

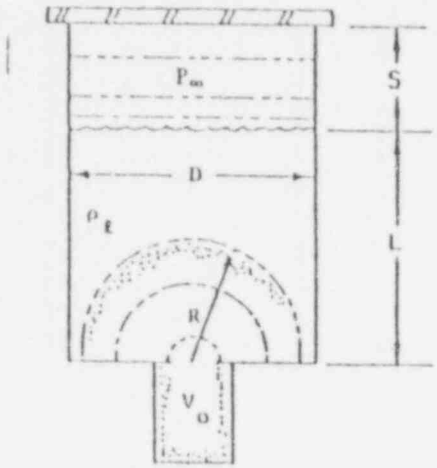


Work potential resulting from a voided-core disassembly.

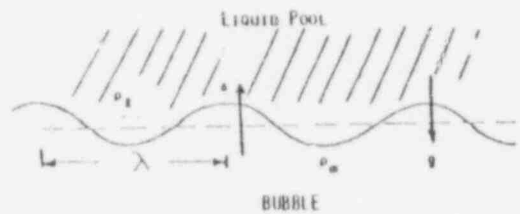
2000002

TAYLOR INSTABILITY MECHANISM

SCHEMATIC IDEALIZED



τ_{IMPACT} = TIME TO SLUG IMPACT



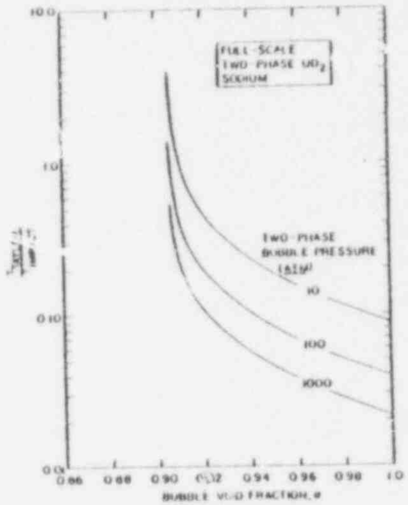
VAPOR: $\rho_m = \rho_v$

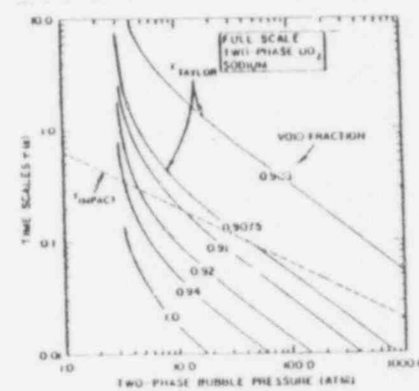
TWO-PHASE: $\rho_m = \alpha\rho_v + (1 - \alpha)\rho_\ell^*$

CRITERION: "Bouyant" Force > Surface Force

$$(g + a)(\rho_\ell - \rho_m) > 4\sigma\left(\frac{\pi}{\lambda}\right)^2$$

τ_{Taylor} = Taylor Growth Time Scale





QUESTION: WHAT IS INFLUENCE OF TWO-PHASE BUBBLE ON TAYLOR INSTABILITY GROWTH MECHANISM?

- DISPERSION

  - Flow Regime Under Delay Heating Conditions is Likely to be Churn-Turbulent, Not Dispersed.

  - Foam Persistence is Unresolved. Perhaps Contaminant and Nucleation Site Dependent.

  - If Vapor is Available, Dispersal Potential is Significant

- BOI[illegible]G POOL HEAT TRANSFER

  - Bubbly Flow:

    Enhanced Natural Convection

    Inclined Wall Effect is $G \cdot \cos\theta$

  - Churn Flow:

    Boundary Layer Perhaps Disrupted

    Factor of Two Higher Heat Transfer

  - Bubbly to Churn Transition

    Marked by Sharp Reduction in Void Fraction

SSC DEVELOPMENT AND CODE VALIDATION PROGRAMS AT BROOKHAVEN NATIONAL LABORATORY

PRESENTED BY

JAMES G. GUPPY (BNL)

CHARLES N. KELBER (NRC/ARSR)

&

JOHN E. MEYER (MIT)

AT ACRS REVIEW MEETING

WASHINGTON, DC

AUGUST 7, 1979

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

SSC PRESENTATIONS

- OVERVIEW OF ALL SSC ACTIVITIES — GUPPY
- SSC LMFBR RELATED ACTIVITIES — GUPPY
- RECENT EXPANSION OF SSC WORK TO INCLUDE LWR ANALYSIS — KELBER
- SSC LWR RELATED ACTIVITIES — GUPPY
- NATURAL CIRCULATION RELATED PHENOMENA AND MODELING REQUIREMENTS — MEYER

SCOPE OF SSC DEVELOPMENT PROGRAM

- SSC SERIES OF COMPUTER CODES SIMULATE THERMO-HYDRAULICS 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 AND IS ASSUMED TO BE OPERATIVE
- MUST MODEL ADEQUATELY ALL COMPONENTS/PROCESSES ESSENTIAL TO HEAT REMOVAL

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 - SIMULATES SHORT-TERM 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 STRUCTURE

- BASICALLY A SET OF B''ILDING 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 STATUS

- SSC-L
  - OPERATIONAL FOR ALMOST TWO YEARS
  - 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)
  - AT APPLICATIONS/DEVELOPMENTAL VERIFICATION STAGE
  - BEING APPLIED TO A WIDE VARIETY OF SYSTEM TRANSIENTS
  - BEING APPLIED TO VARIOUS EXPERIMENTAL TESTS (PARTICULARLY FFTF)

SSC STATUS (CONT)

- SSC-P

  FIRST OPERATING VERSION EXPECTED LATE 1979

- SSC-W

  FIRST OPERATING VERSION EXPECTED EARLY 1980

- SSC-S

  IN SCOPING ANALYSIS STAGE

SSC-L

- MODELS
- FEATURES
- CAPABILITIES
- RESULTS

STEAM GENERATOR MODELING

- GENERIC FOR ANY TYPE SG DESIGN
- THREE EQUATION MODEL, EQUILIBRIUM, SLIP ALLOWED
- MOMENTUM INTEGRAL SOLUTION PROCEDURE (MEYER)
  - EXTENSIONS
    - (1) IMPLICIT TIME INTEGRATION
    - (2) TIME-VARYING REFERENCE PRESSURE
    - (3) FEW PRESSURE MODEL
- CORRELATIONS
  - STEAM/WATER PROPERTIES (THOR, JORDAN)
  - FRICTION FACTOR (W-ARD)
  - TWO PHASE FLOW MULTIPLIER (THOM)
  - HEAT TRANSFER: WATER-SIDE

FORCED CONVECTION	-	DITTUS BOELTER
NUCLEATE BOILING	-	CHEN
FILM BOILING	} -	HADALLER
SUPERHEAT CONVECTION		
DNB	-	AI CORRELATION

  - HEAT TRANSFER: SODIUM SIDE

    GRABER-REIGER CORRELATION

VALIDATION OF STEAM GENERATOR SIMULATION MODEL

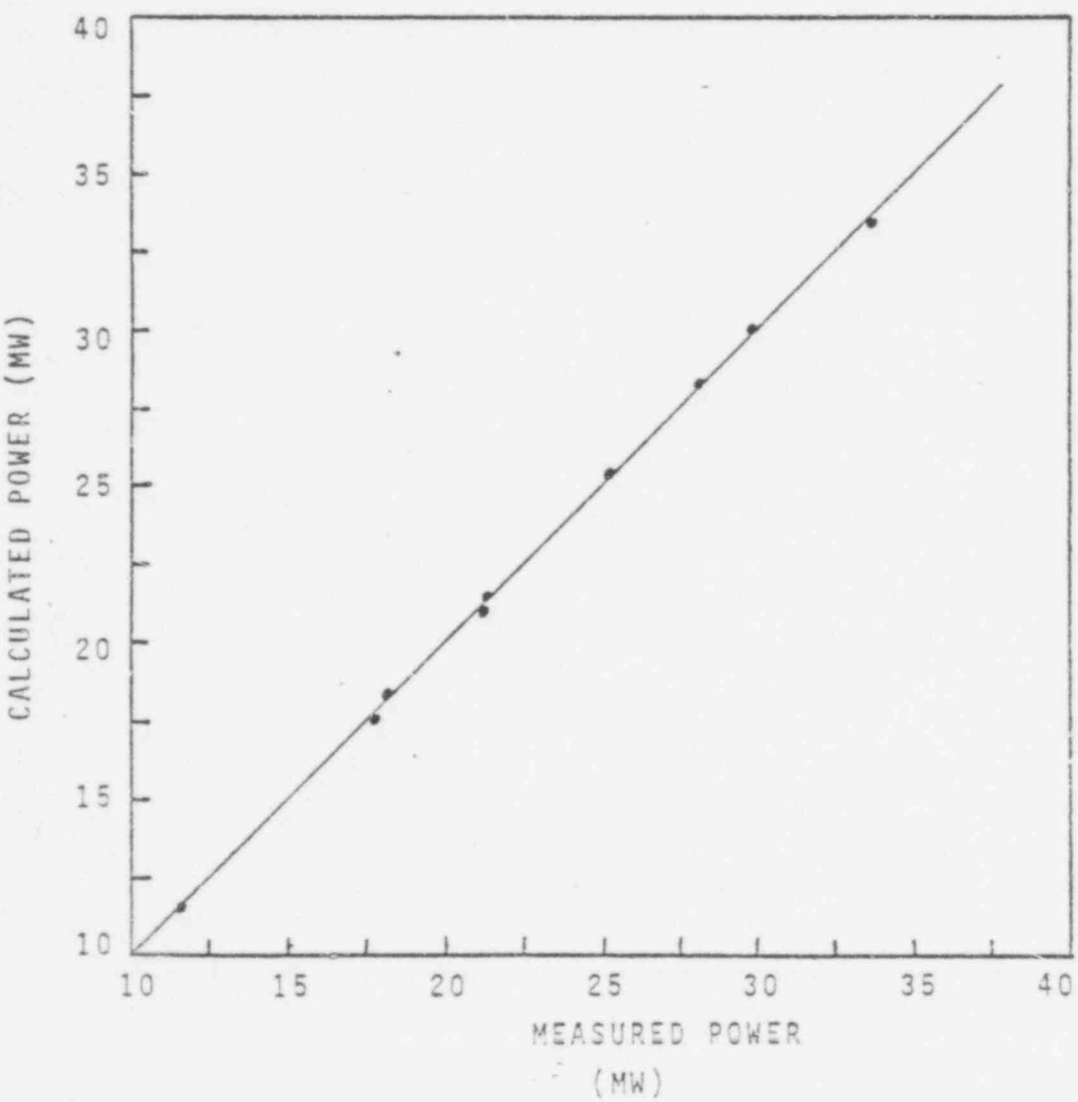
- ATOMICS INTERNATIONAL MODULAR STEAM GENERATOR
  - COUNTER-FLOW SHELL AND TUBE SODIUM HEATED HEAT EXCHANGER
  - STRAIGHT TUBE, HOCKEY STICK DESIGN

- STEADY STATE HEAT TRANSFER TEST DATA
  - TESTS PERFORMED MARCH 21-24, 1973
  - TESTED IN ONCE-THRU MODE
  - LOW PRESSURE TESTS   STEAM PRESSURE 1550-1850 PSIG
  - HIGH PRESSURE TESTS   STEAM PRESSURE 2450-2650 PSIG
  - VARIOUS COMBINATIONS OF SODIUM AND FEEDWATER FLOWS

  REF: TR-097-330-008

- SSC-L SIMULATION
  - INPUT   - MEASURED SODIUM FLOW AND INLET TEMPERATURE
  - COMPUTE - TOTAL HEAT TRANSFER RATE (MODULE POWER)
  - NO "KNOBS" ADJUSTED

SSC-L SIMULATION OF AI-MSG LOW PRESSURE
STEADY STATE HEAT TRANSFER TESTS



MAX DEVIATION 1.2%

890016

CURRENT SSC-L CASE SIMULATION CAPABILITIES

- ANY SIZE PIPE BREAK IN ANY SODIUM LOOP
- REACTOR SCRAM, MANUAL OR PPS INITIATED
- PUMP MAIN MOTOR TRIP - ANY PUMP
- PUMP PONY MOTOR FAILURE, ANY PUMP
- COASTDOWN TO NATURAL CIRCULATION
- OPERATIONAL TRANSIENTS
  - REACTIVITY TRANSIENTS
  - SINGLE PUMP FAILURE
  - CONTROL SYSTEM MALFUNCTION
  - OPERATOR INITIATED EVENTS

EXAMPLE OF SSC-L TRANSIENT

- SYSTEM DETAIL, CRBRP PROTOTYPIC

  - IN VESSEL

    4 CHANNELS, 13 AXIAL SLICES, 8 RADIAL NODES

    FLOW REDISTRIBUTION ON/OFF

  - SODIUM LOOPS

    ONE LOOP SIMULATION

    5 PIPE SECTIONS IN EACH CIRCUIT, TOTAL OF 104 NODES

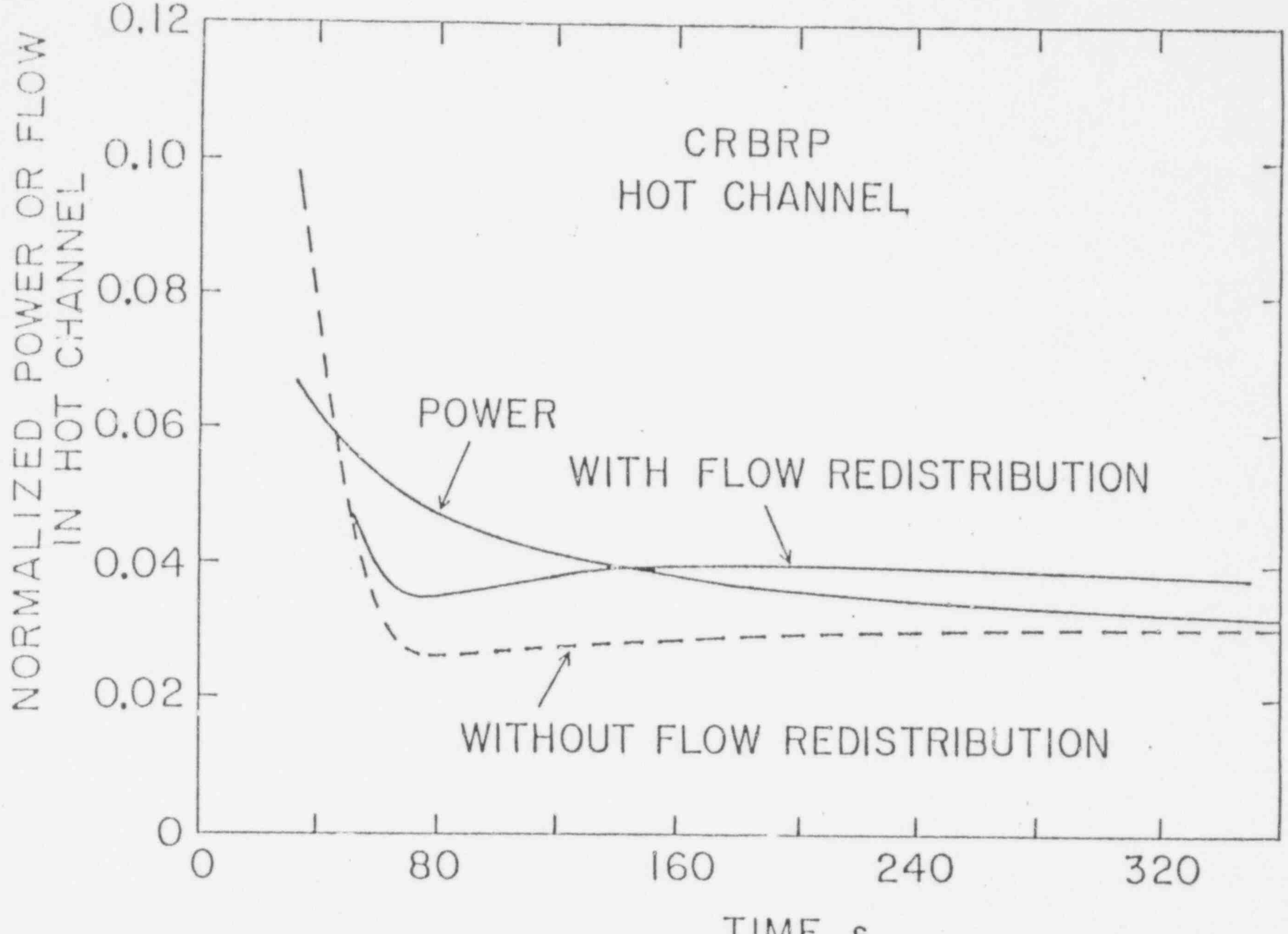
    20 NODES IN IHX

  - STEAM GENERATOR

    6 NODES IN EVAPORATOR, 4 IN SUPERHEATER, TOTAL OF 44 NODES ON WATER SIDE

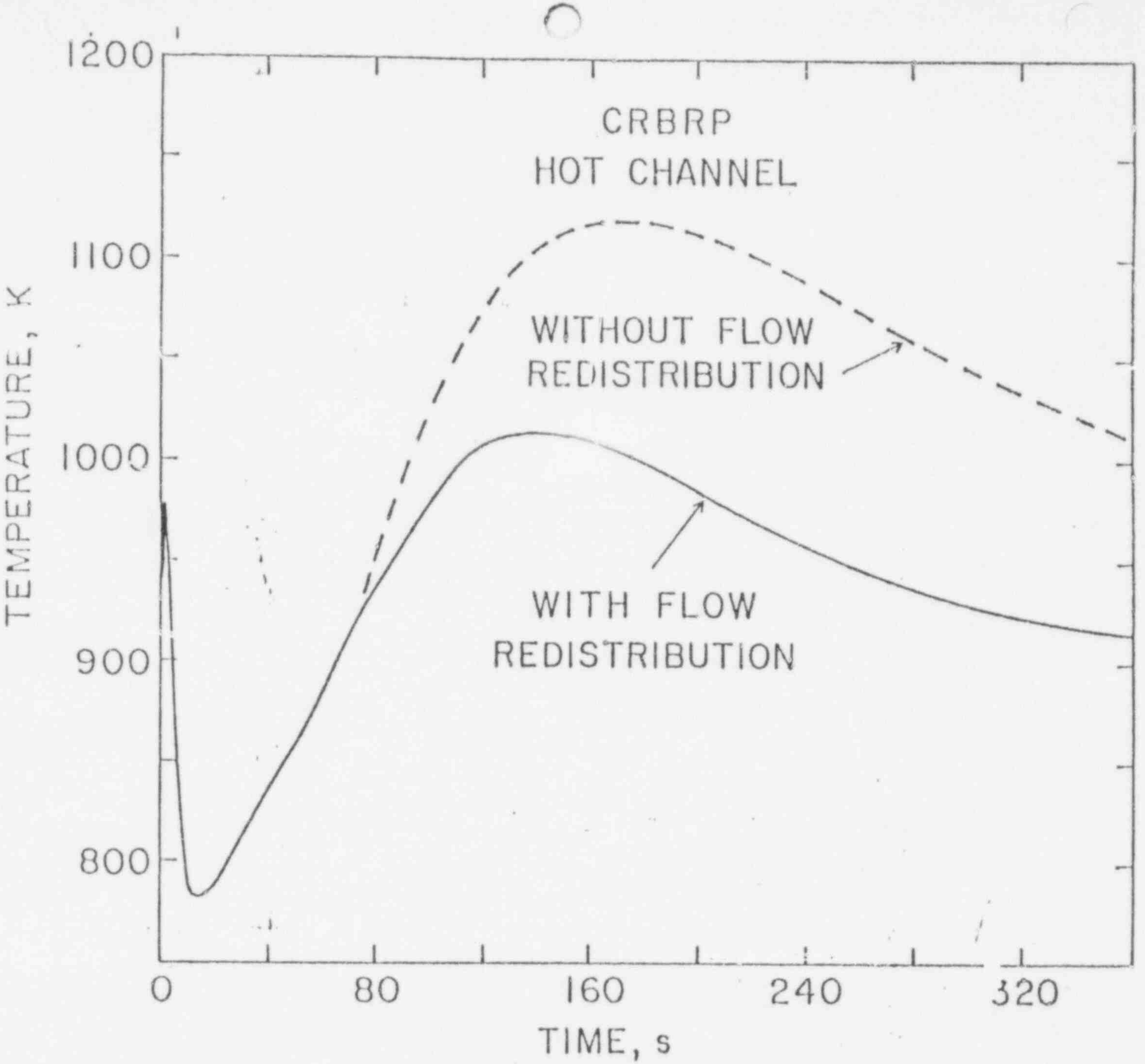
- CASE SIMULATED, NATURAL CIRCULATION

  - PLANT INITIALLY AT 100% POWER AND FLOW
  - PUMPS MAIN MOTOR TRIP, REACTOR SCRAM AT .75 SEC.
  - PONY MOTORS OFF
  - SG RECIRC PUMP TRIP
  - SIMULATION TIME 0-360 SECONDS
  - CDC 7600, 334 CPU SECONDS REQUIRED



Response of Hot Channel Power and Flow Rate (Coastdown to Natural Circulation)

690019



RESPONSE OF HOT CHANNEL SODIUM TEMPERATURE
(COASTDOWN TO NATURAL CIRCULATION)

SUMMARY

- SSC LMFBR RELATED ACTIVITIES
- EMPHASIS TODAY ON LOOP-TYPE CODE (SSC-L)
- SSC-L
  - OPERATIONAL ALMOST TWO YEARS
  - SEVEN USERS AT PRESENT
  - BEING APPLIED IN MANY AREAS
  - VERIFICATION PROGRESSING
- SSC FEATURES
  - MODULAR
  - EASILY MODIFIED
  - USER ORIENTED
  - COMPUTATIONALLY EFFICIENT

SSC-W

- SCOPE
- STATUS REPORT

SSC-W

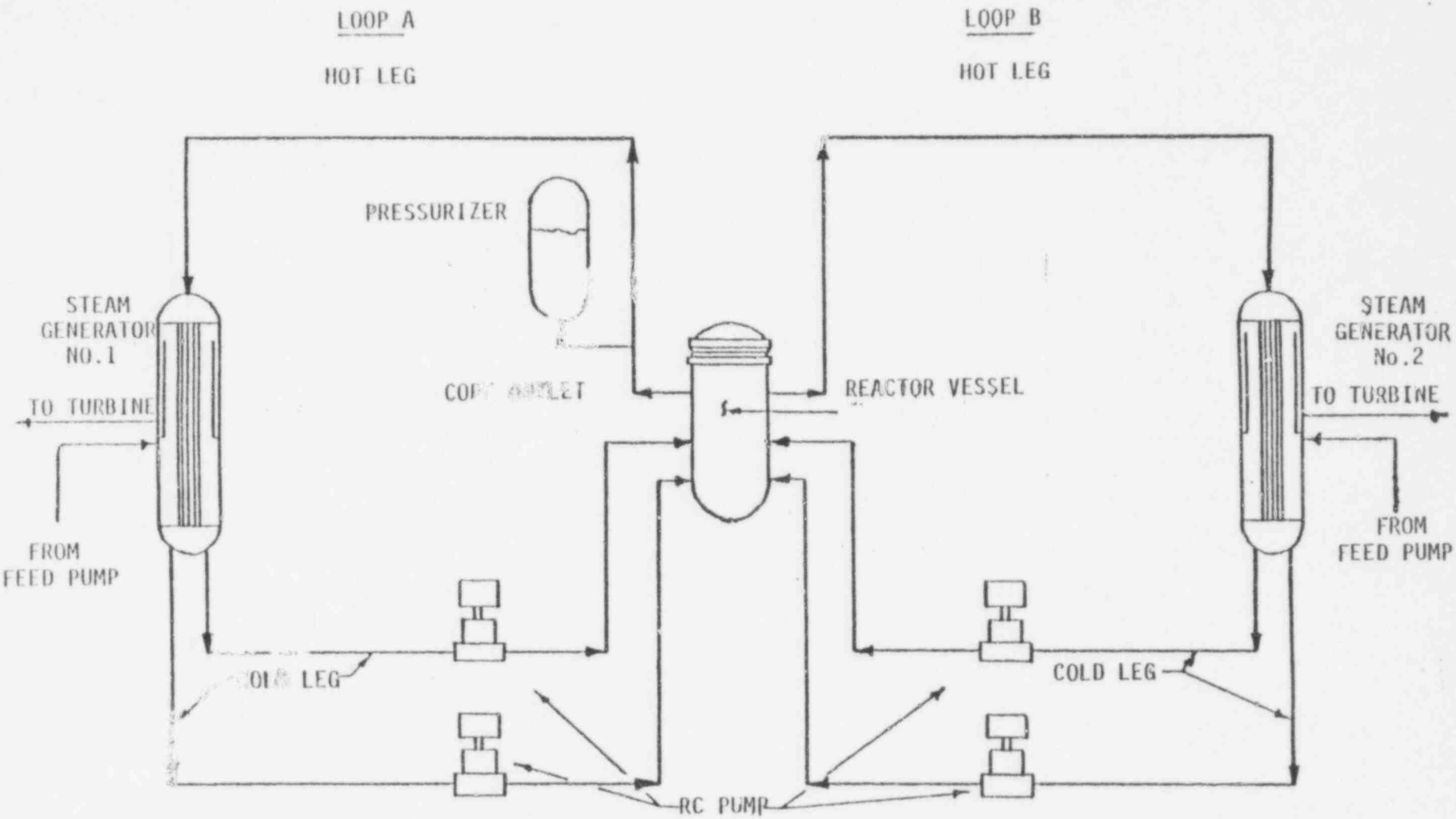
SCOPE

- DEVELOP A VERSION OF THE SUPER SYSTEMS CODE (SSC) APPLICABLE TO WATER REACTORS CAPABLE OF PROVIDING AN INDEPENDENT LICENSING TOOL FOR DETAILED ANALYSIS OF NATURAL CIRCULATION AND OTHER SYSTEM-WIDE EVENTS.
- DEVELOP INDEPENDENT ANALYTICAL TOOL
- MODEL NATURAL CIRCULATION
- DEVELOP GENERIC TOOL TO ANALYZE SYSTEM WIDE EVENTS WITH CONTROL SYSTEM/PLANT PROTECTION SYSTEM OPERATING
- CAPABILITY TO ANALYZE SINGLE/MULTIPLE FAILURES IN SYSTEM THAT CAN LEAD TO DESIGN BASIS ACCIDENTS

SSC-W

ASSUMPTIONS FOR INITIAL VERSION

- DIRECT DEVELOPMENT TOWARD PWRs FIRST
- SINGLE PHASE IN PRIMARY LOOP(s) (NOT A LIMIT FOR BWR DEVELOPMENT)
- MODEL ONCE-THROUGH SG DESIGNS FIRST, THEN EXTEND TO U-TUBE DESIGNS


LOOP A
HOT LEG
LOOP B
HOT LEG
PRESSURIZER
STEAM GENERATOR No.1
TO TURBINE
FROM FEED PUMP
REACTOR VESSEL
STEAM GENERATOR No.2
TO TURBINE
FROM FEED PUMP
COLD LEG
RC PUMP

SSC-W STATUS

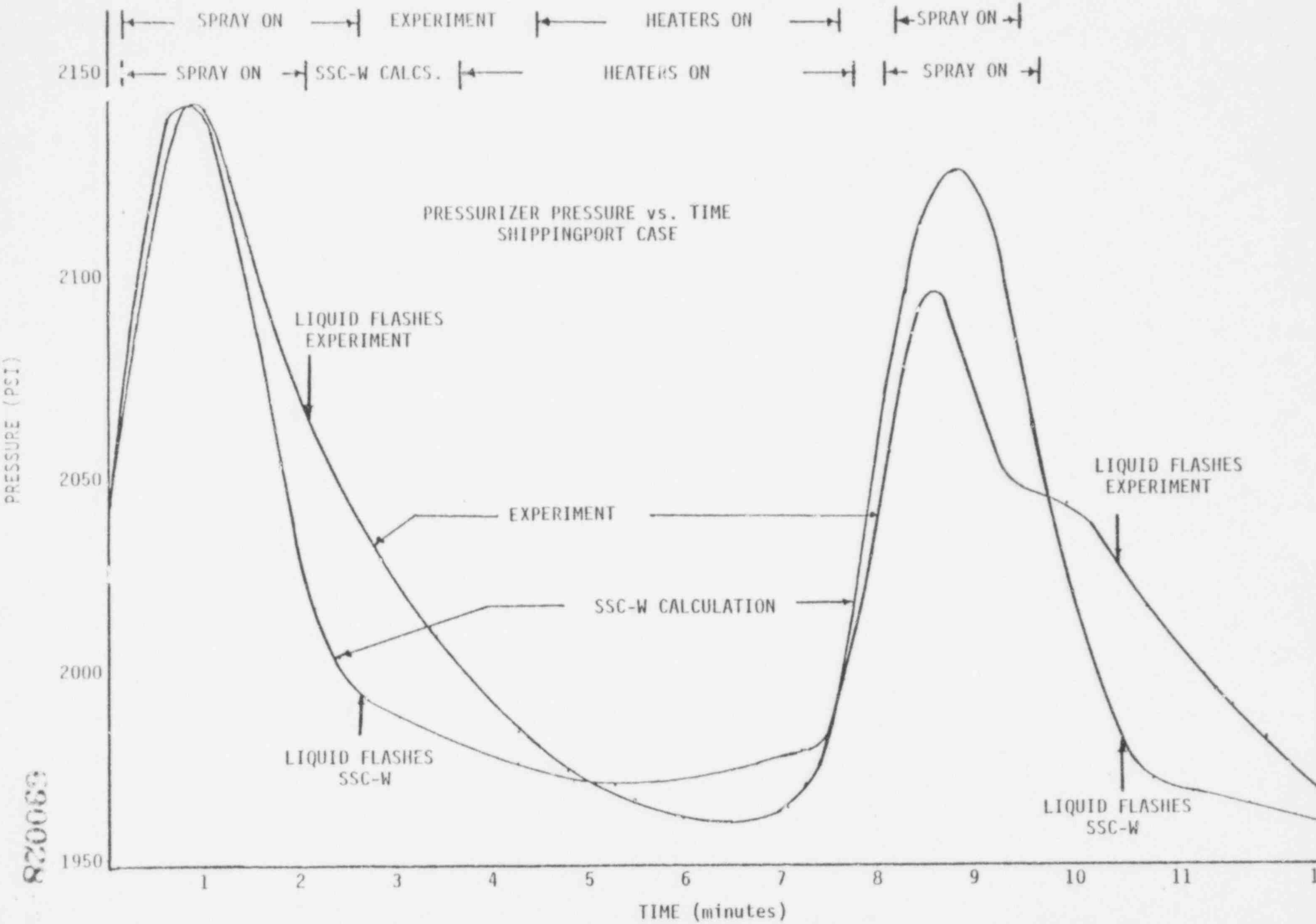
- WORK BEGUN MID-MAY 1979
- SINCE SSC CAN BE VIEWED AS AN ARRANGEMENT OF BUILDING BLOCKS, SSC-W IS TO A CERTAIN POINT, A RE-ARRANGEMENT OF THE BLOCKS.
- IN-VESSEL MODELING SAME AS SSC-L
  - CORRELATIONS/CONSTITUTIVE RELATIONSHIPS MUST BE CHANGED TO WATER
  - COVER GAS MODEL DELETED
  - ROD PROPERTIES HANDLED THROUGH INPUT
- HTS PIPING
  - MODELS IDENTICAL
  - GEOMETRY SPECIFIED THROUGH INPUT
  - DELETE LMFBR PRIMARY LOOP AND IHX
- PUMPS
  - MODELS IDENTICAL
  - PUMP DATA SPECIFICATION HANDLED THROUGH INPUT

SSC-W STATUS (CONT.)

- PRESSURIZER
  - PHYSICS:
    BASED ON RETRAN FORMULATION,
    TWO REGION NON-EQUILIBRIUM.
    RAINOUT & FLASHING,
    SPRAY & HEATERS,
    SURGE FLOW,
    RELIEF AND SAFETY VALVES
  - MATHEMATICS (BASED ON NAHAVANDI'S WORK)
  - STAND-ALONE MODEL OPERATIONAL

- ONCE THROUGH STEAM GENERATOR
  - LOGIC TURNED INSIDE OUT (PRIMARY IN TUBES)
  - PRIMARY SIDE CORRELATIONS SWITCHED TO WATER
  - SECONDARY SIDE DNB CORRELATION MODIFIED (BIASI CORRELATION)
  - STEADY STATE TESTED (WITHOUT ASPIRATOR MODELED)

- PPS/PCS
  - ADDITIONAL SAFETY AND CONTROL SYSTEMS GENERIC TO LWR-S ARE BEING ADDED AS REQUIRED


SPRAY ON
EXPERIMENT
HEATERS ON
SPRAY ON
2150
SPRAY ON
SSC-W CALCS.
HEATERS ON
SPRAY ON
PRESSURIZER PRESSURE vs. TIME
SHIPPINGPORT CASE
2100
LIQUID FLASHES
EXPERIMENT
PRESSURE (PSI)
2050
LIQUID FLASHES
EXPERIMENT
EXPERIMENT
SSC-W CALCULATION
2000
LIQUID FLASHES
SSC-W
LIQUID FLASHES
SSC-W
1950
1
2
3
4
5
6
7
8
9
10
11
12
TIME (minutes)

8200028

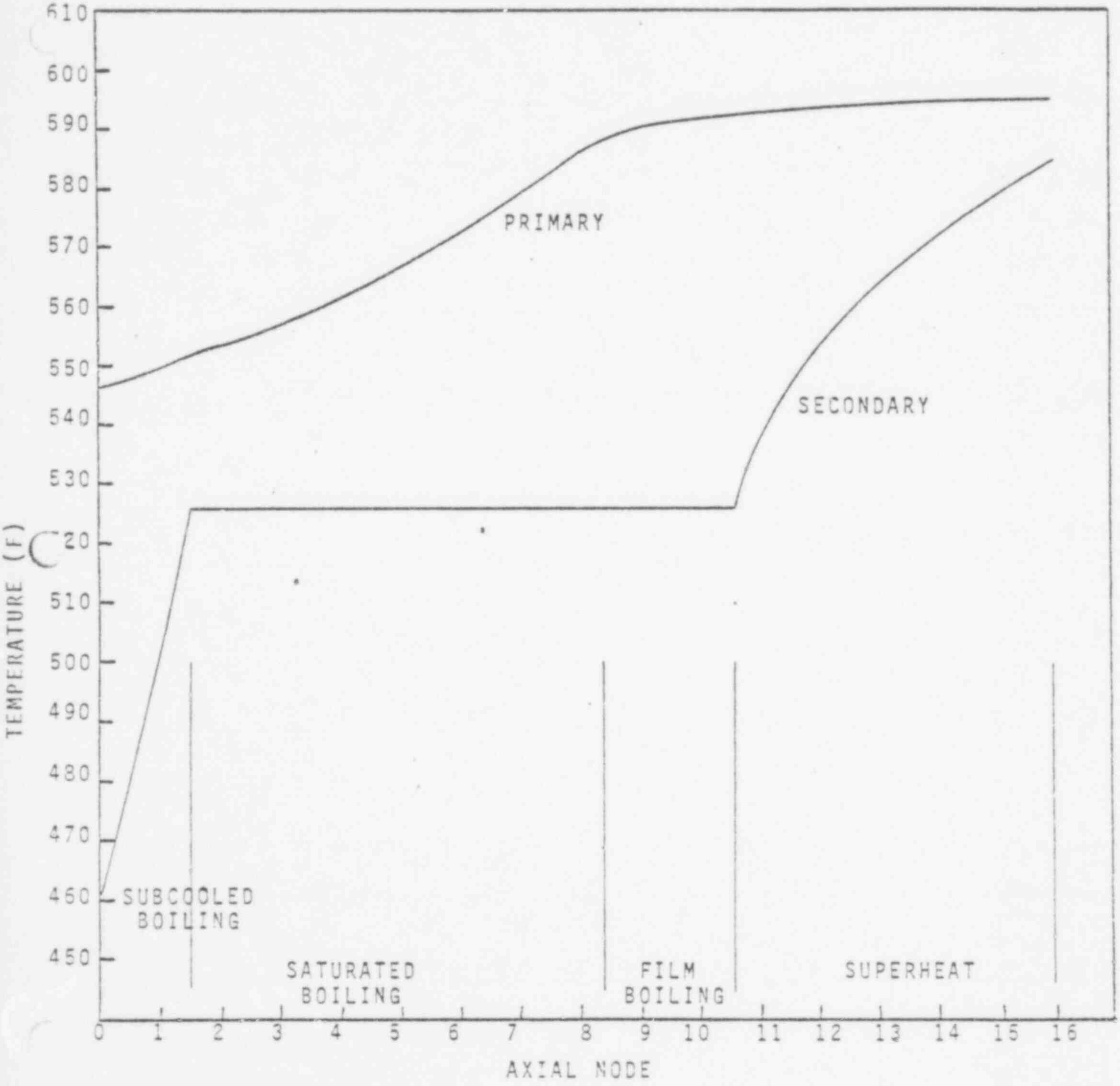
TMI-2 OTSG

SPECIFIED CONDITIONS

- PRIMARY FLOW — 1.379×10^8 LBS/HR
- PRIMARY OUTLET TEMPERATURE — 556 F
- SECONDARY OUTLET TEMPERATURE — 593.65 F
- TOTAL POWER — 2772 MW

CALCULATED CONDITIONS

- STEAM FLOW — 5.9259×10^6 LBS/HR
- PRIMARY INLET TEMPERATURE — 605.74 F



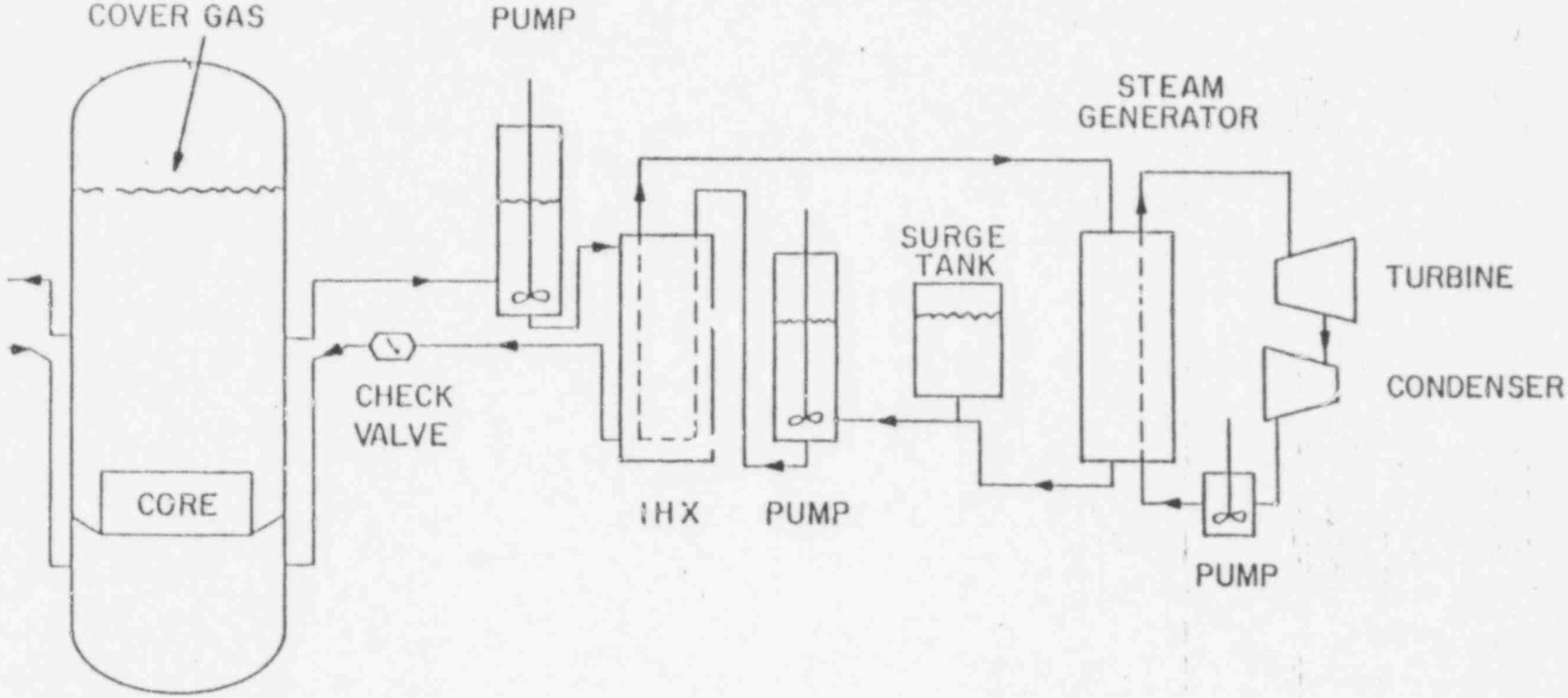
610
600
590
580
570
560
550
540
530
520
510
500
490
480
470
460
450
TEMPERATURE (F)
PRIMARY
SECONDARY
SUBCOOLED BOILING
SATURATED BOILING
FILM BOILING
SUPERHEAT
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
AXIAL NODE

8300030

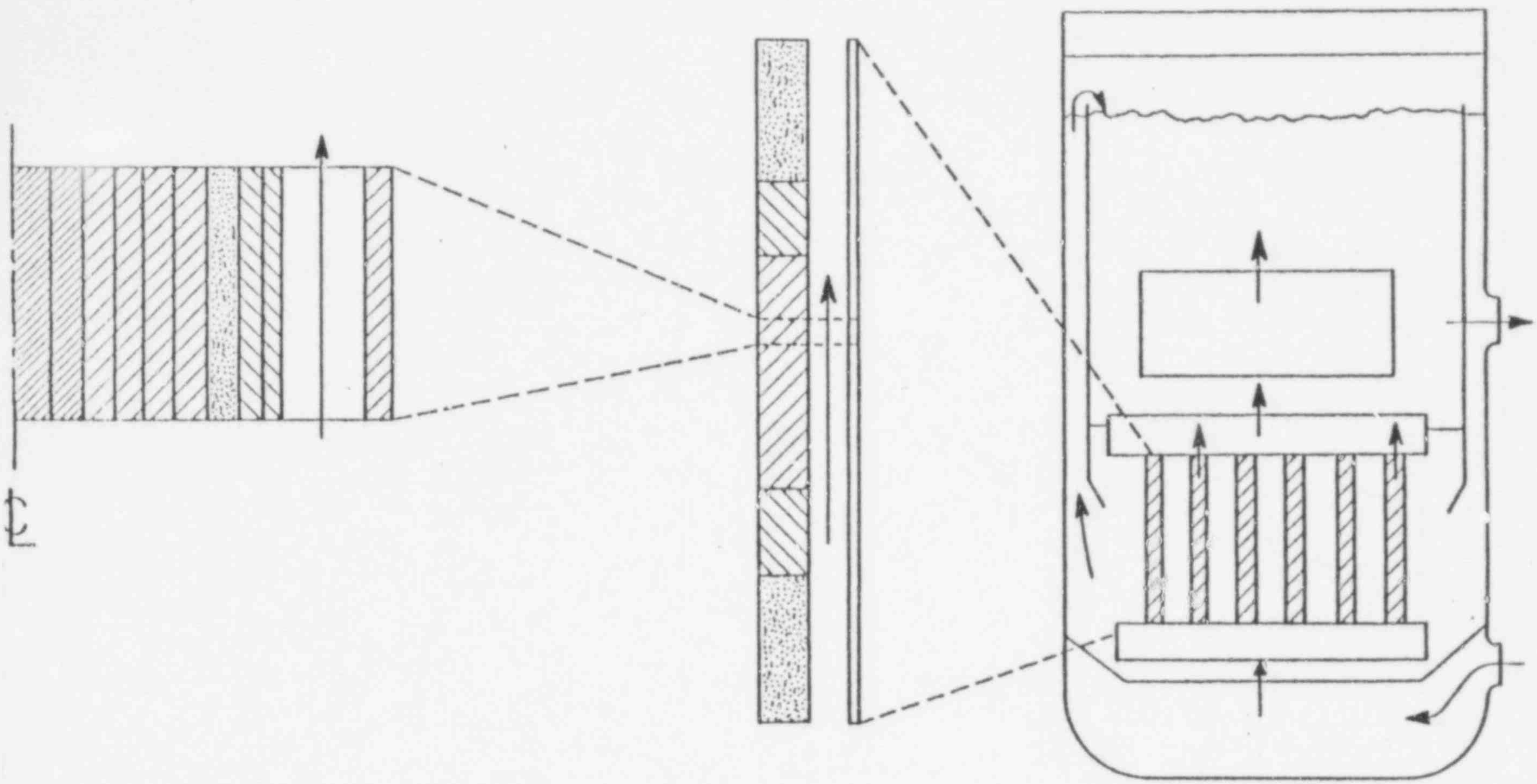
SSC-W FUTURE PLANS

- INITIAL VERSION EXPECTED EARLY 1980
- APPLICATIONS TO B&W PLANTS
  - TMI-2 EVENT
  - LOEP EVENT, COASTDOWN TO NATURAL CIRCULATION
  - OPERATIONAL TRANSIENTS, INCLUDING SINGLE/MULTIPLE FAILURES IN SYSTEM THAT CAN LEAD TO DESIGN BASIS EVENTS
  - VERIFY BY COMPARISON TO NATURAL CIRCULATION TEST RESULTS

SUPPLEMENTAL VIEWGRAPHS



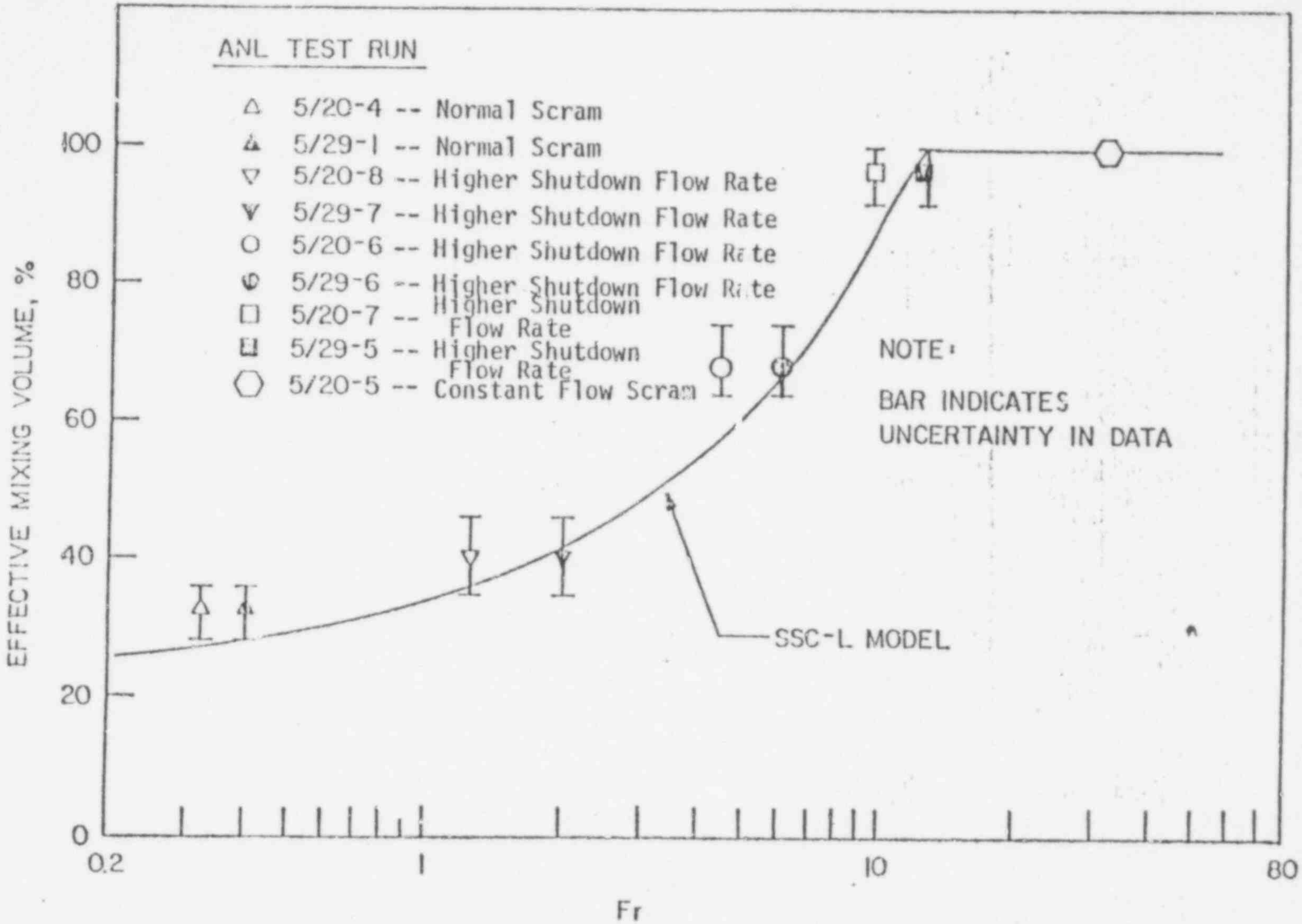
Sketch of One Set of Loops in an LMFBR System



IN-VESSEL MODELING

IN VESSEL MODELING

- POINT REACTOR KINETICS (NAHAVANDI)
- REACTIVITY FEEDBACK EFFECTS (SAS FORMULATION)
- ROD HEAT CONDUCTION (CRANK-NICHOLSON SOLUTION PROCEDURE)
- TRANSIENT BUOYANCY-INDUCED INTER-ASSEMBLY FLOW REDISTRIBUTION (FLODISC)
- REVERSE FLOW ALLOWED BOTH CORE-WIDE OR INDIVIDUAL CHANNEL BASIS
- DETAILED ACCOUNTING OF ALL PRESSURE LOSSES AND TEMPERATURE/DENSITY PROFILES
- CONSTITUTIVE RELATIONSHIPS AND CORRELATIONS
  - FUEL/BLANKET (LIFE-II, HEDL)
  - SODIUM (GOLDEN & TOKAR)
  - FRICTION
    - TURBULENT FLOW IN BUNDLES (NOVENDSTERN)
    - LAMINAR FLOW IN BUNDLES (HEDL/W-ARD)
  - HEAT TRANSFER - (MODIFIED SCHAD CORRELATION)
- TWO ZONE MIXING MODEL IN UPPER PLENUM (YANG)



Comparison Between Effective Mixing Volume Calculated by SSC-L Model and ANL Experimental Values.

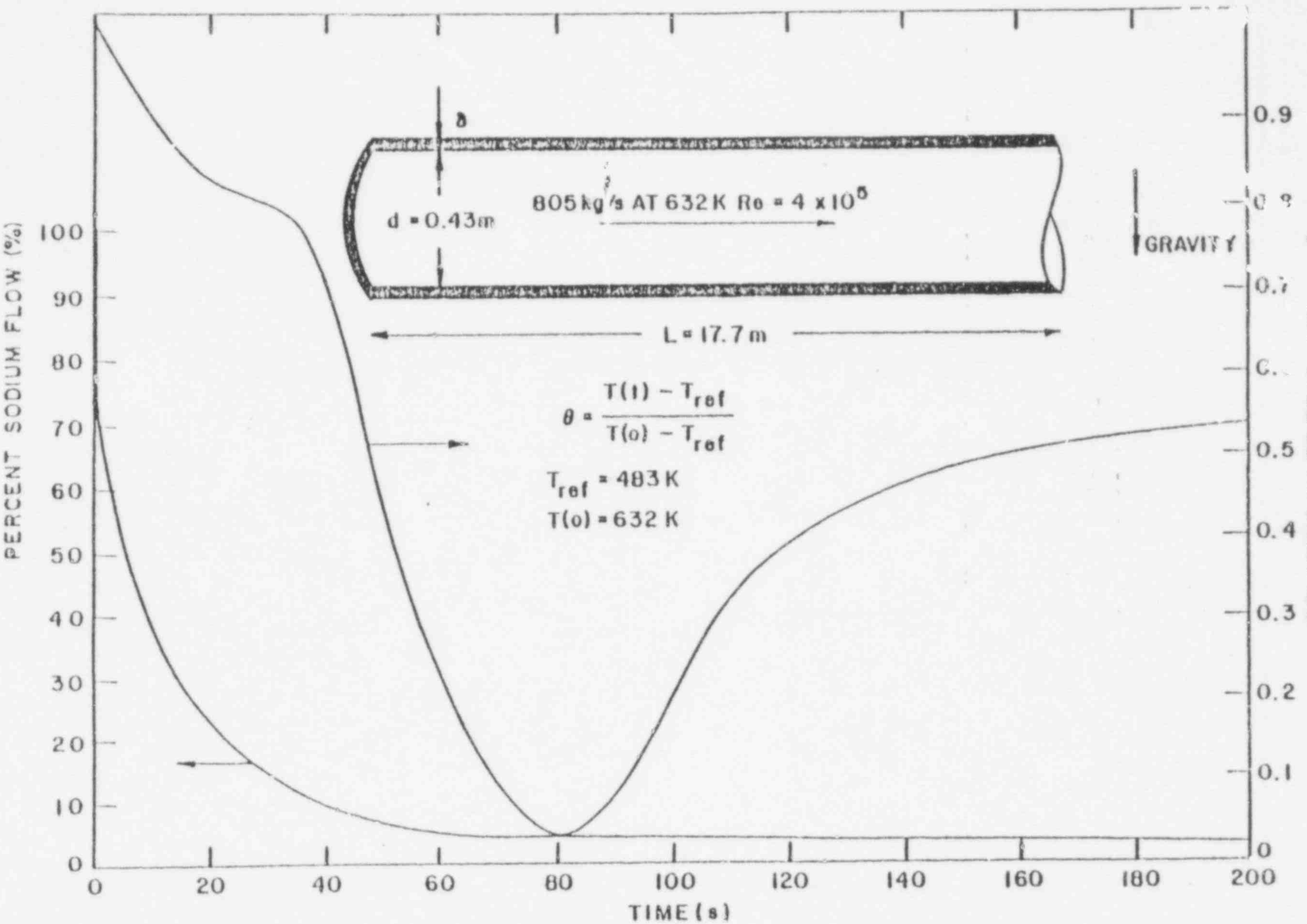
HEAT TRANSPORT SYSTEM MODELING

- PIPING
  - COOLANT, WALL MODEL
  - TRANSPORT DELAY INHERENTY MODELED
  - NODAL HEAT BALANCE METHOD
  - COOLANT EQUATION IMPLICIT, HEAT FLUX EXPLICIT
  - MARCHING PROCEDURE
  - HEAT TRANSFER - AOKI CORRELATION
  - VARIABLE FRICTION FACTOR, APPROXIMATION TO COLEBROOK CORRELATION (PROVEN)

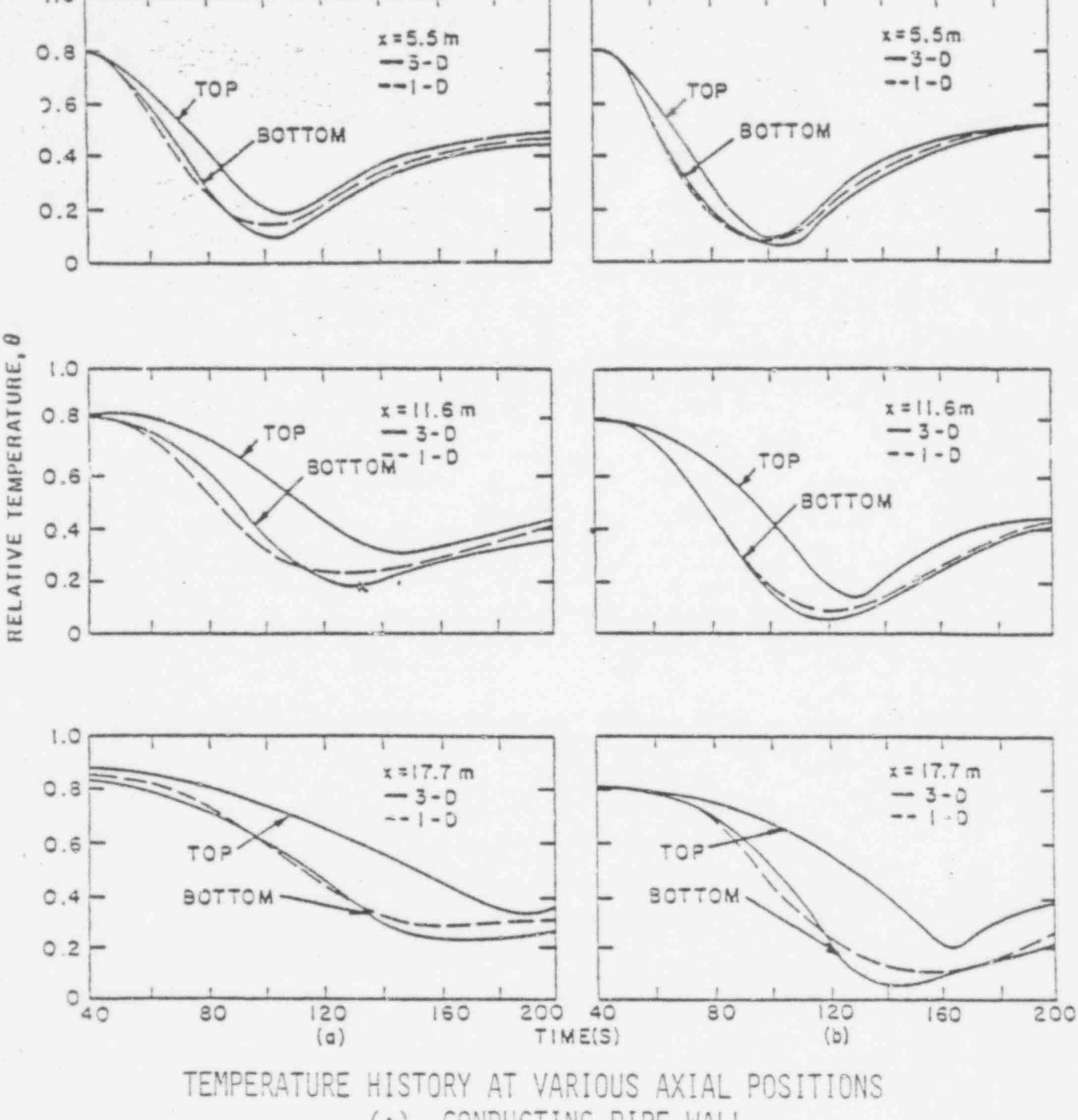
- IHX
  - PRIMARY COOLANT, WALL, SECONDARY COOLANT, SHELL
  - APPROACH SAME AS FOR PIPING
  - HEAT TRANSFER

    TUBE-SIDE: AOKI CORRELATION

    SHELL-SIDE: GRABER-RIEGER CORRELATION



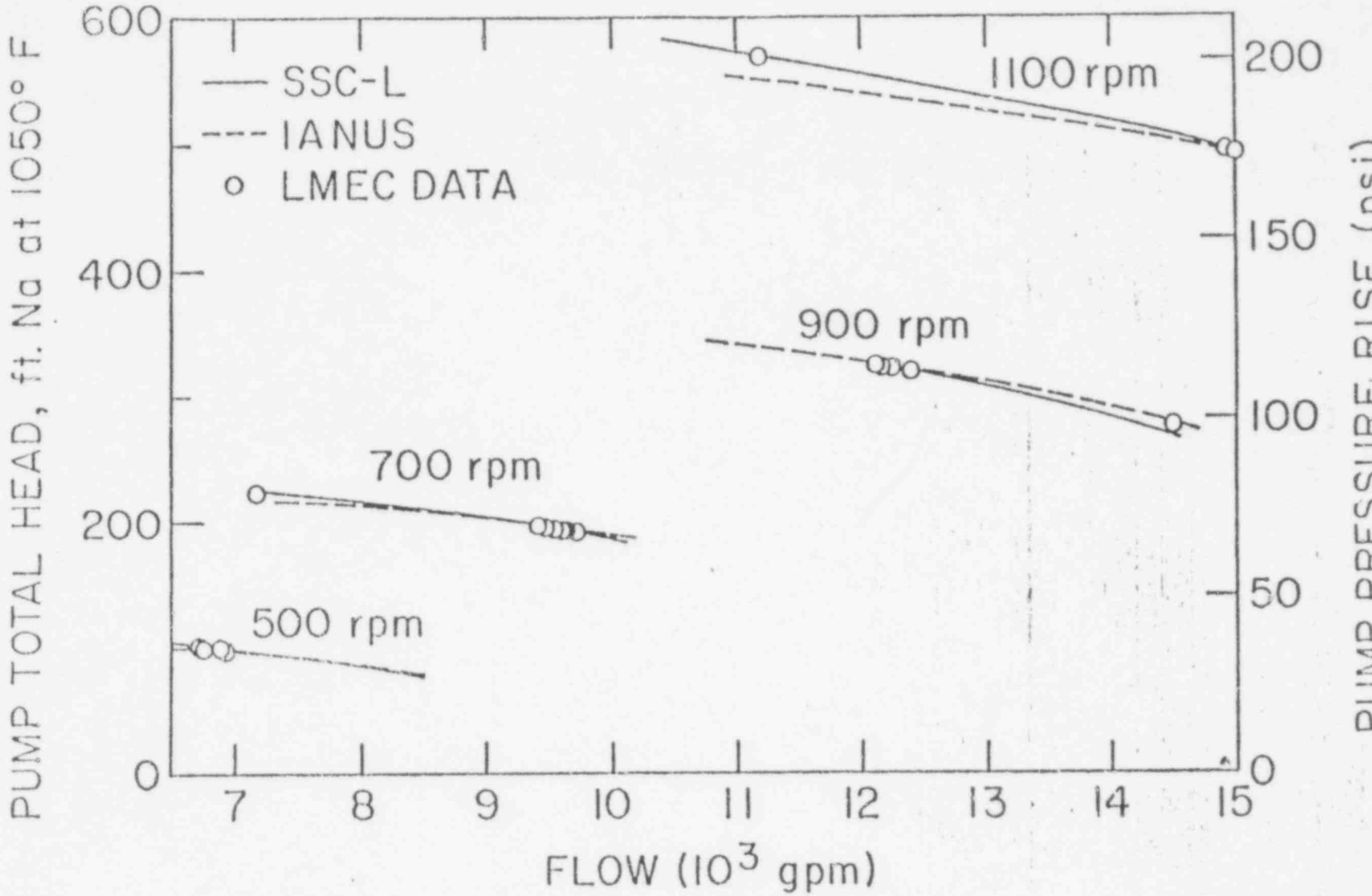
PIPE SEGMENT INLET FLOW AND TEMPERATURE BOUNDARY CONDITIONS FOR TEST CASE.



TEMPERATURE HISTORY AT VARIOUS AXIAL POSITIONS
(A) CONDUCTING PIPE WALL
(B) ADIABATIC PIPE WALL

HTS MODELING (CONT)

- PUMP
  - HOMOLOGOUS MODEL
  - ALL OPERATING REGIMES ACCOUNTED FOR
  - REQUIRED HEAD & TORQUE POLYNOMIALS DERIVED FROM INDEPENDENT MODEL TEST RESULTS (STREETER & WYLIE, AND DUNSKY)
  - RESULTS IN EXCELLENT AGREEMENT WITH VENDOR DATA



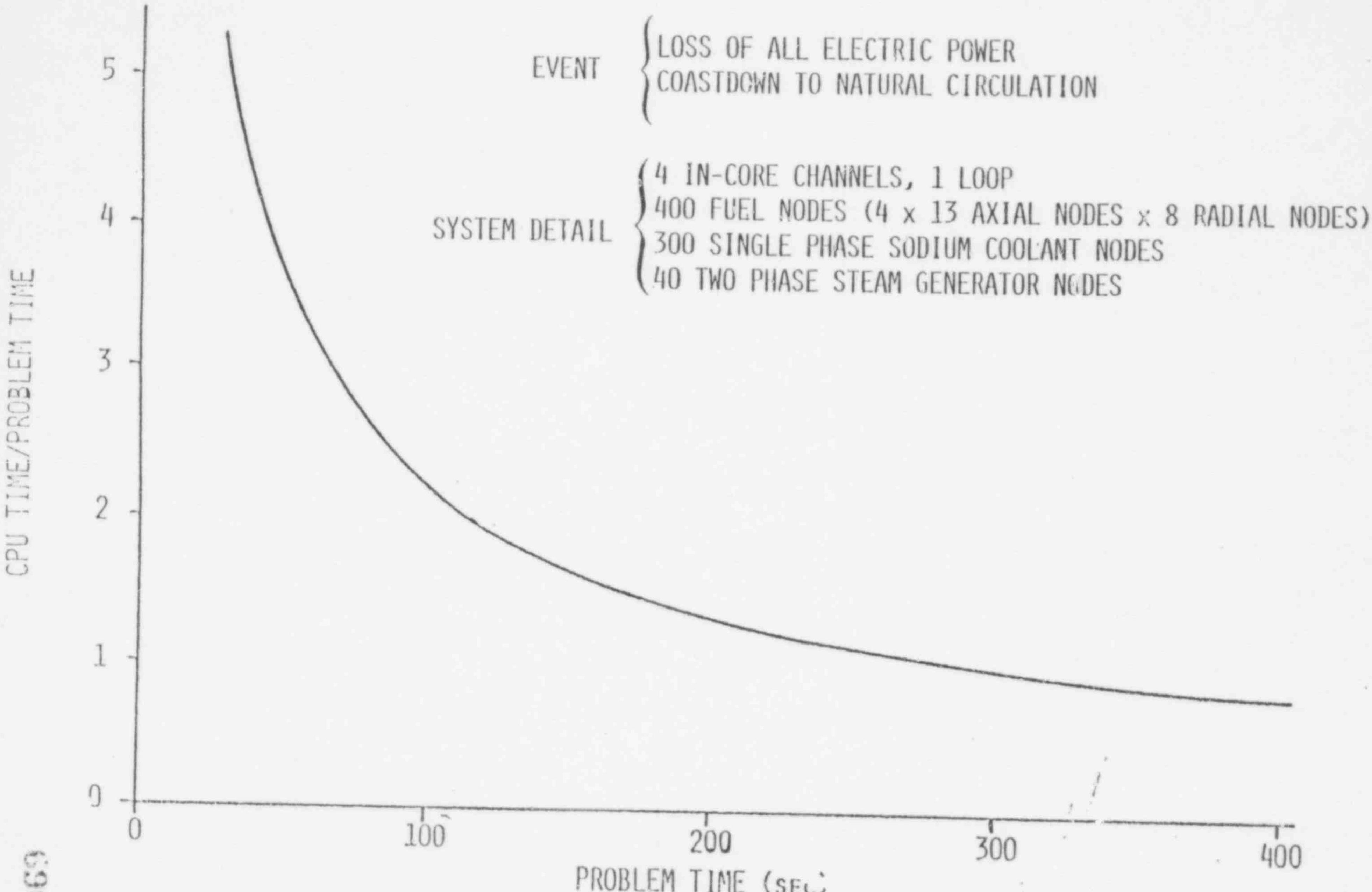
A Comparison of Pump Performance

690041

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

630042



RATIO OF MACHINE TIME (CPU) TO PROBLEM (SIMULATION) TIME

690043

COMPUTATIONAL METHODS FOR REACTOR PLANT NATURAL CIRCULATION

J. E. MEYER

AUGUST 7, 1979

1. INTRODUCTION
2. NUMERICAL/PHYSICAL APPROACH
3. CORE MODEL
4. PLANT REPRESENTATION
5. COMPARISON AMONG LMFBR AND PWR PLANTS
6. FUTURE WORK

2. NUMERICAL/PHYSICAL APPROACH

A. Time Constants

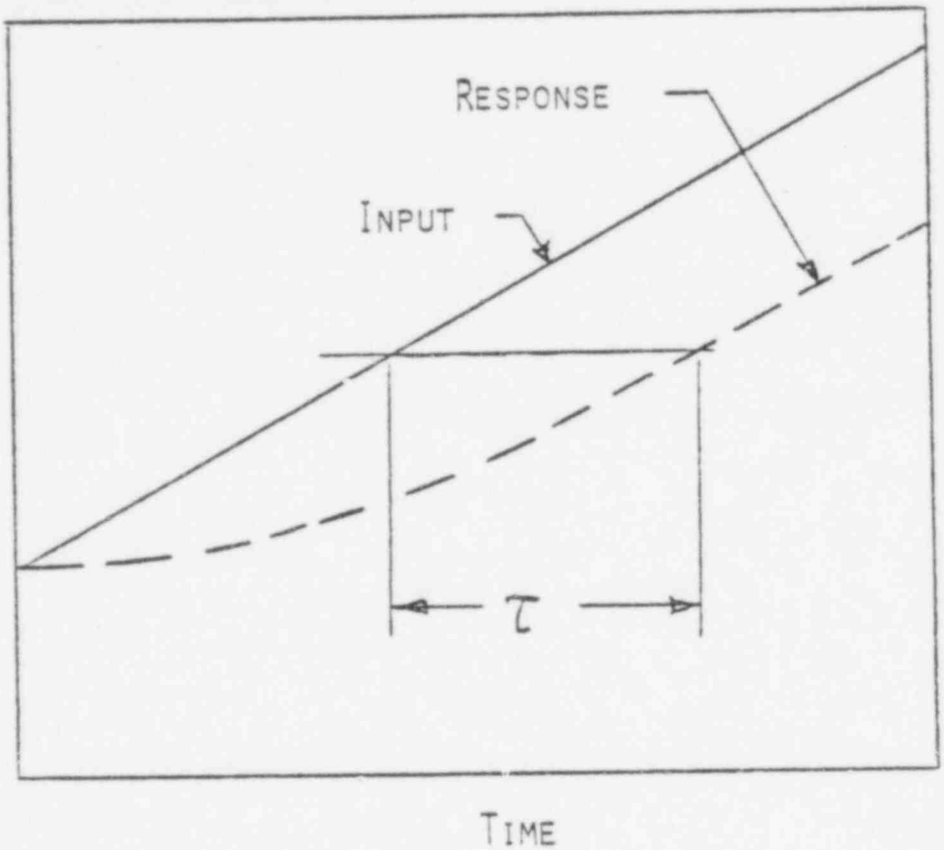
B. Quasi-Steady

C. Numerical Methods

D. Enthalpy Transport

2A TIME CONSTANTS

- OPERATIONAL DEFINITION



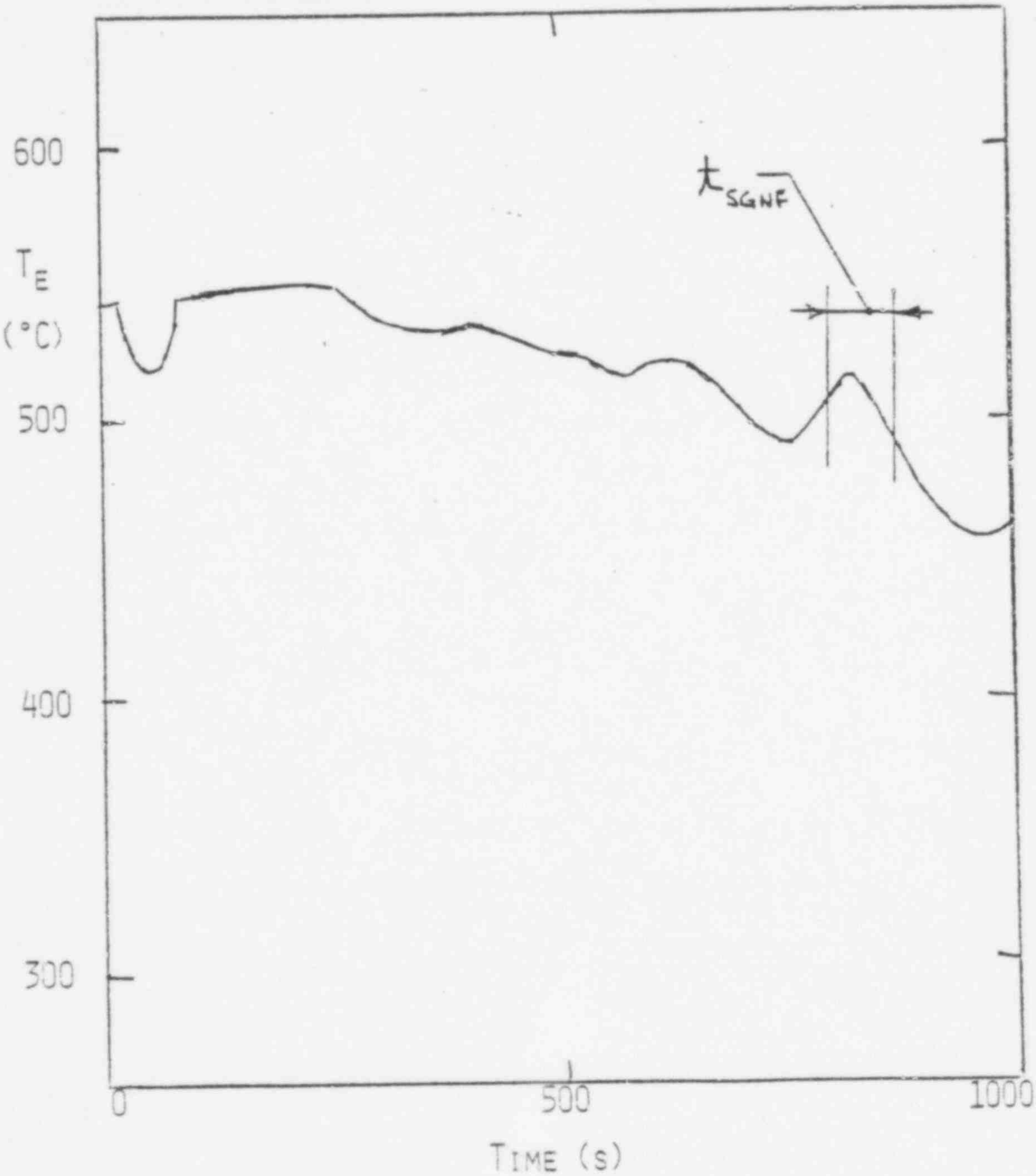
2B QUASI-STEADY?

- EQUATION

$$R_Q = \left[\frac{\tau}{t_{SGNF}} \right]$$

2B QUASI-STEADY (CONT'D.)

- DEFINE t_{SGNF}



T_E = REACTOR VESSEL EXIT TEMPERATURE

630047

154

2C NUMERICAL METHODS

- EXPLICIT?

$$R_E = \left[\frac{\Delta t}{\tau} \right]$$

- SPEED?

$$R_S = \left[\frac{\Delta t}{t_{SGNF}} \right]$$

- SSC-L TECHNIQUES

  - IMPLICIT STRINGS/EXPLICIT CONNECTORS

  - MULTI-TIME STEPS

2D ENTHALPY TRANSPORT (LMFBR)

PHTS Component	100% Flow	3% Flow
In Reactor Ves.	42.8s	1420s
R.V. to IHX	14.3	480
In IHX	12.2	410
IHX to R.V.	6.2	210
Total PHTS	75.5s	2520s
Core	1.3s	43s
Total IHTS	93.6s	3740s
Total SGS	56.8s	543s

3. CORE MODEL

A. AXIAL CONDUCTION

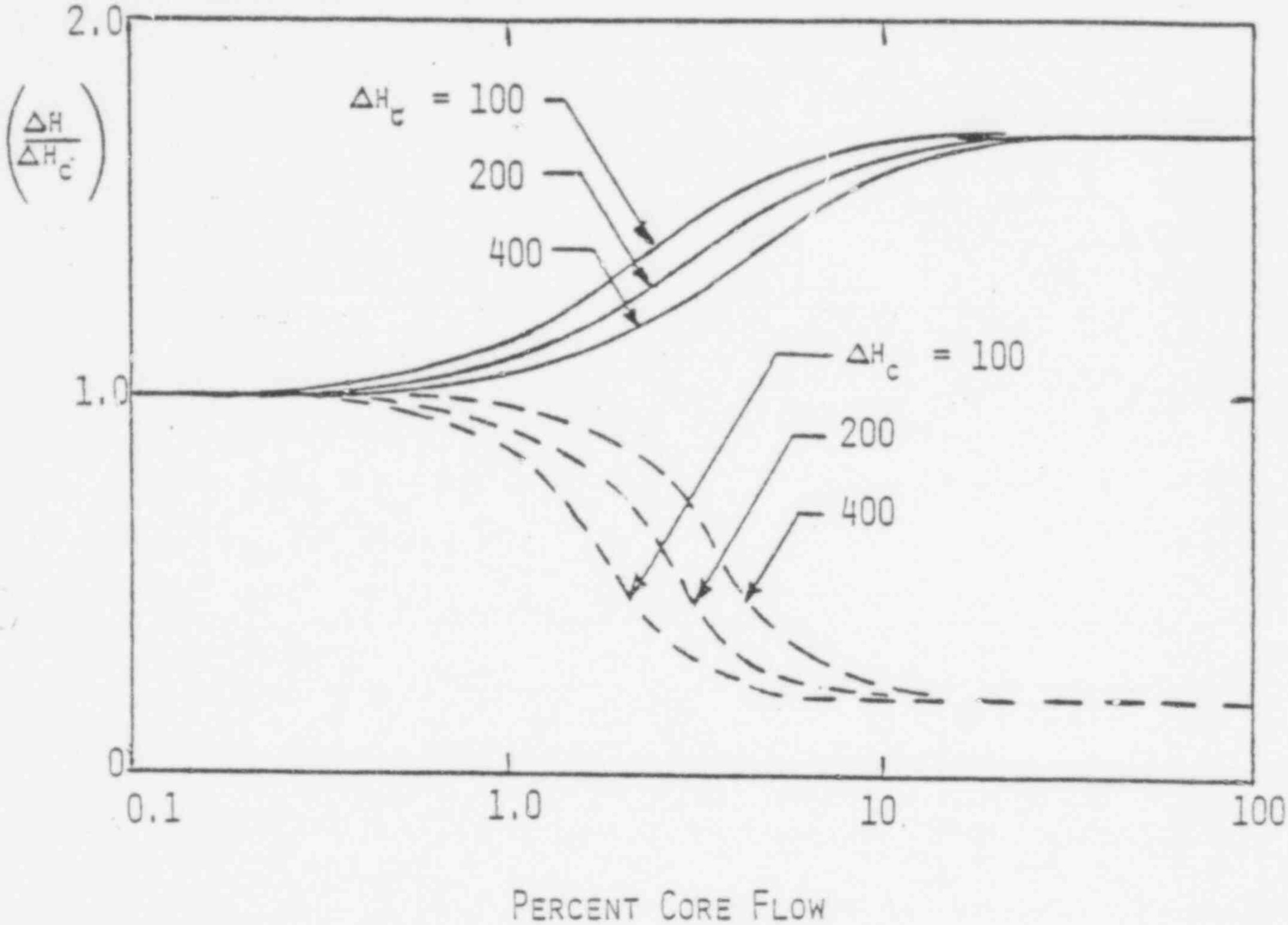
B. BOUYANCY EFFECTS

C. TRANSVERSE HEAT TRANSFER

D. FRICTION FACTORS AND HEAT TRANSFER COEFFICIENTS

3B BUOYANCY EFFECTS (CONT'D.)

- INTER-ASSEMBLY



ΔH = ASSEMBLY AVERAGE ENTHALPY RISE (KJ/KG)

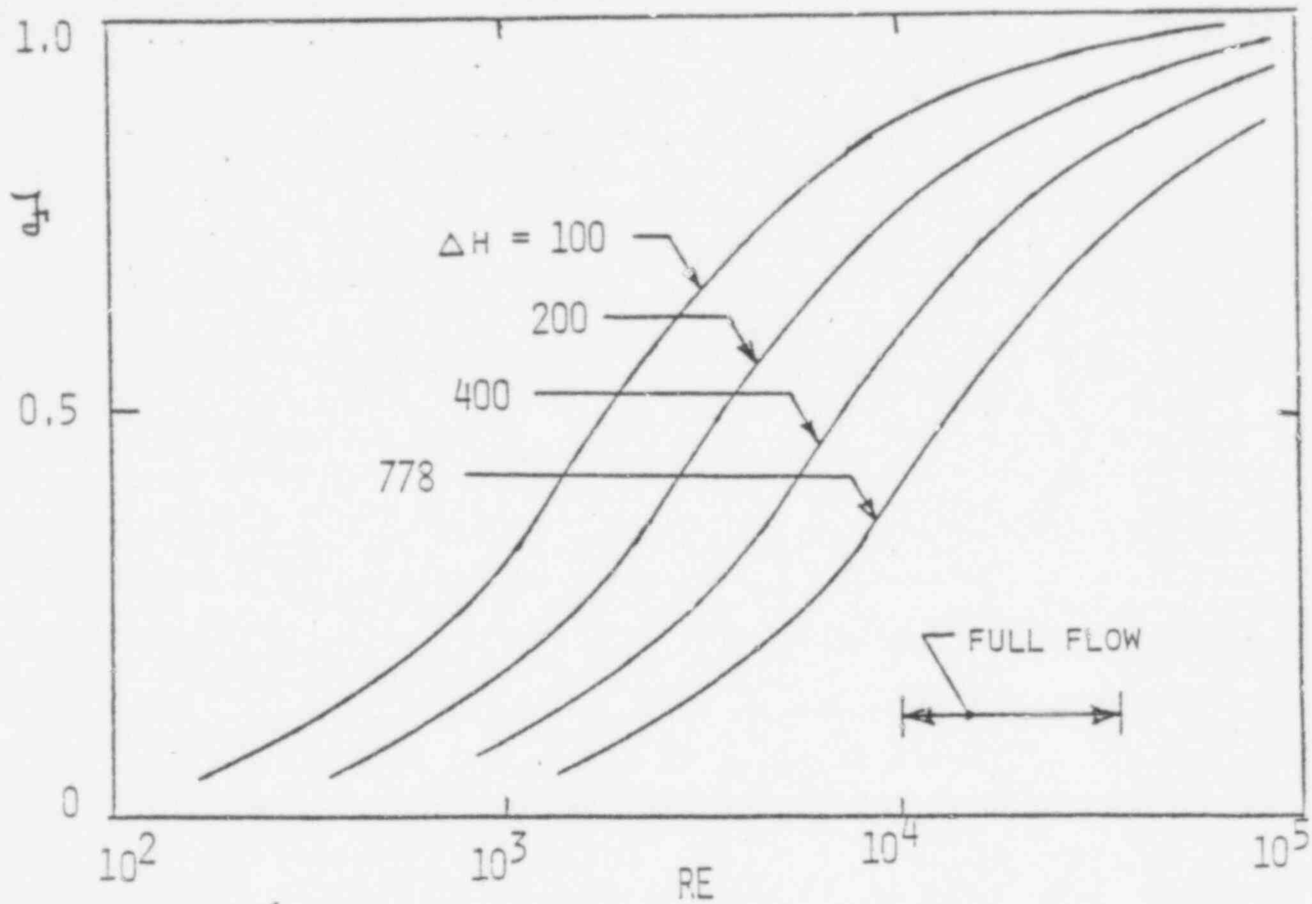
ΔH_c = CORE AVERAGE ENTHALPY RISE (KJ/KG)

——— = HOT ASSEMBLY

- - - - - = COLD ASSEMBLY

3B BOUYANCY EFFECTS (CONT'D.)

- INTRA-ASSEMBLY



ΔH = ASSEMBLY AVERAGE ENTHALPY RISE (KJ/KG)

RE = REYNOLDS NUMBER BASED ON ASSEMBLY AVERAGE FLOW RATE

$$\mathcal{F} = \left[\frac{\left(\text{ENTHALPY HOT CHANNEL FACTOR}\right) - 1}{\left(\text{POWER HOT CHANNEL FACTOR}\right) - 1} \right]_{\text{INTRA-ASSEMBLY}}$$

4. PLANT REPRESENTATION

A. Natural Circulation

B. Plenum and Pool Treatments

C. Stratification in Pipes

D. Steam Side Slip and Phase Separations

III. COMPARISON AMONG LMFBR AND PWR PLANTS

A. Plant Component Geometry

B. Fluid Properties

C. Enthalpy Transport

5C ENTHALPY TRANSPORT

(FULL FLOW, TIME IN SECONDS)

	LMFBR	PWR
IN REACTOR VESSEL	42.8	3.4
RV TO IHX/SG	14.3	1.0
IN IHX/SG	12.2	4.7
IHX/SG TO RV	6.2	1.6
TOTAL PHTS	75.5	10.7
CORE	1.3	0.7

6 FUTURE WORK

A. Further Evaluate Existing SSC Methods

B. Improve Numerical Methods and Physical Models

C. Compare to Building Block Experiments and Computations

D. Delineate Range of Validity

INFCE WORKING GROUPS

1. FUEL AVAILABILITY
2. ENRICHMENT AVAILABILITY
3. SUPPLY ASSURANCES
4. REPROCESSING Pu HANDLING, RECYCLE
5. FAST BREEDERS
6. SPENT FUEL MANAGEMENT
7. WASTE MANAGEMENT AND DISPOSAL
8. ADVANCED FUEL CYCLE CONCEPTS

8300058

NASAP

REVIEW SCHEDULE

- ALL MAINLINE REACTOR PRELIMINARY SAFETY AND ENVIRONMENTAL INFORMATION DOCUMENTS (PSEIDs) ARE DUE AT NRC BY 2/9/79 (THE LWR-VARIANT IS IN)
- ROUND ONE COMMENTS (ON DOE DRAFT NASAP REPORT) DUE 4/15/79
- ROUND TWO COMMENTS (ON DOE NASAP REPORT) DUE 6/15/79
- DOE REPORT TO THE PRESIDENT AND TO CONGRESS DUE 12/24/79

8309059

NASAP

"MAINLINE" NASAP REACTORS TO BE REVIEWED

1. LIGHT WATER REACTOR (LWR) (THREE VARIANTS ON CONVENTIONAL PWR)
2. LIGHT WATER BREEDER REACTOR (LWBR) (THREE PREBREEDER/BREEDER PAIRS)
3. LIQUID METAL FAST BREEDER REACTOR (LMFBR) (SIX VARIANTS)
4. HEAVY WATER REACTOR (HWR) (A C.E. VARIATION OF THE CANDU)
5. HIGH TEMPERATURE GAS COOLED REACTOR (HTGR) (LOW ENRICHMENT FUEL)
6. GAS COOLED FAST REACTOR (GCFR)

ALTERNATIVE SYSTEM
RESEARCH PROGRAMS
PROPOSED

HWR

- PRESSURE TUBE MATERIALS EVALUATION
- SEISMIC ANALYSIS

LWBR

- CORE PERFORMANCE ASSESSMENT

GCFR

- CORE RETENTION ASSESSMENT

INFCE & NASAP ACTIVITIES

7906061

CURRENT

- PARTICIPATE IN INFCE/WG-8 ACTIVITIES
- COMMENT ON SAFETY (RESEARCH NEEDS) INVOLVED IN INFCE PROPOSALS
- MONITOR NASAP ACTIVITIES
- CONTRIBUTE SAFETY RELATED INPUT (COMMENTS) TO ALTERNATE SYSTEM PROPOSALS

DEFERRED

- EVALUATE SAFETY RESEARCH NEEDS FOR SPECIFIC PROPOSALS
- SCOPE OUT TENTATIVE REACTOR SAFETY RESEARCH PROGRAMS
- EVALUATE ANTICIPATED COST OF NEEDED SAFETY RESEARCH FOR PROJECTED CONCEPTS