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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON EMERGENCY CORE COOLING SYSTEMS

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Westbank Motel Coffee Shop,
475 River Park,
Idaho Falls, Idaho,

Wednesday, 22 October 1980.

The subcommittee was convened, pursuant to notice, at 8:45 a.m., with Dr. Milton Plesset, Chairman of the Subcommittee, presiding.

PRESENT FOR THE ACRS:

DR. MILTON PLESSET, Chairman
JEREMIAH RAY, Member
WILLIAM MATHIS, Member
HAROLD ETHERINGTON, Member
DR. ZUDANS, Consultant
DR. WU, Consultant
DR. ACOSTA, Consultant
DR. CATTON, Consultant
DR. THEOFANOUS, Consultant
DR. BATES, Federal Employee

PRESENT FOR THE NRC:

Messrs. Sheron, Sullivan, and Lyon

* * *

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PROCEEDINGS

(8:45 a.m.)

DR. PLESSET: The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards' Subcommittee on Emergency Core Cooling Systems.

I am Milton Plesset, the Subcommittee Chairman. The other ACRS members here today are Mr. Ray, Mr. Etherington, and Mr. Mathis; and we have consultants here today: Dr. Zudans, Dr. Wu, Dr. Acosta, Dr. Catton, and I understand that Professor Theofanous will be here a little later in the morning.

The purpose of this meeting is to discuss Semiscale and LOFT programs and plans for those programs and, in particular, recent data on the question of whether it is better to turn off the reactor coolant pumps during a small-break LOCA, or to leave them running.

Dr. Andy Bates is the designated federal employee for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Tuesday, October 7, 1980.

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

It is requested that each speaker first identify himself and speak with sufficient clarity and volume so that he can be readily heard.

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

We will now proceed with our agenda -- and maybe I will have a very few brief remarks to introduce the subject of today's meeting; and I will also call on our subcommittee members, if they wish to make any comments, as well as the consultants.

I think you all have the summary of background material that Andy Bates made available. We have had some discussion of this question of the reactor coolant pump trip, and I think that we have already complimented Brian Sheron on his report that he wrote, which is a very good report, and he has been very actively engaged in the study of this subject.

There was an ACRS letter written on this question, and in it I think that the ACRS view on the matter was made fairly clear. I think that most of you have seen that letter, so I won't quote from it in detail.

Essentially at that time -- which was last July -- the feeling was that -- well, let me quote, briefly: "Speaking for the Full Committee, we do not at this time disagree entirely with the Staff's requirement of prompt coolant pump trip, but in view of the analytical limitations upon which pump trip is based, we believe that the emphasis on the immediacy of the trip and on eventual automatic trip may not be desirable."

I think we will hear more on this subject today. There may be also some pertinent experimental observations which will help people make a decision on this matter.

Let me ask if the subcommittee members want to make any further comment?

MR. RAY: I have none.

MR. MATHIS: Not at this time.

DR. PLESSET: Do the consultants have any special questions that they would like to pose?

(No response.)

DR. PLESSET: If not, I will again apologize a little bit for our being a little bit delayed, but there was a matter of electricity that was involved, and you are all familiar with the problems with that.

Let me call on Brian Sheron to introduce this problem of the pumps-on/pumps-off.

DR. SHERON: Dr. Plesset, my name is Brian Sheron. I am with the Reactor Systems Branch of the Office of Nuclear Reactor Regulation. I have been up here a number of times on this subject. Hopefully I will have a little bit more to tell you today concerning where we've come from and where we are going with this problem.

(Slide.)

The first part of my presentation this morning is going to basically be to bring you up-to-speed with a brief background and history of where we are at today.

(Slide.)

Basically, right after the TMI accident it became apparent that plant operators had never been given any specific guidance on what to do with the pumps during a LOCA, except Westinghouse which had instructions out to their operators to trip them. I believe it was on low pressure, although when I talked with Espezito he said that they were just given instructions to trip immediately.

Previous sensitivity studies that were required by Part II Item 3 of Appendix K certainly address the need to study the effect of pump operation on LOCAs. This indeed was done back in the early compliance days of Appendix K, and it showed that pump-trip assumption was generally the worst case. I believe there are some two-loop plants which showed that pump operation gave a slightly higher peak-clad temperature, but by and large most large-break LOCA calculations show that pumps-trip assumption was a worst case.

This was also consistent with the assumption of a simultaneous loss of off-site power. Most small breaks were not examined, however, in the same detail as large breaks primarily since small breaks were usually not limiting, and that peak-clad temperature calculated using Appendix K was typically around 1800 degrees or less.

An early pump trip was also assumed to be the worst case for small breaks, since it was usually the worst case for large breaks.

On June 5th of '79 right after TMI, the Staff issued a letter to the vendors requesting additional analyses to address various small-break issues which had arisen from TMI. This included studying the effect of the pump trip on small-break LOCA.

B&W came in in early July of '79 and gave a presentation to the Staff. What their preliminary conclusions or the results were showing them was that there was a spectrum of break sizes, break locations, and pump-trip delay times in which the peak-clad temperature was estimated to exceed 2200°F. Now I say "estimated," because they were not doing strict Appendix K calculations, nor were they doing the heat-up calculations that were necessary to show the clad temperature going in excess of 2200°F. They were basically doing their hydraulic calculation with the CRAC code. When they tripped the pumps, they would see how much the vessel was uncovered, and then they could do some quick-and-dirty hand calculations using adiabatic heat-up type models and estimate the time to refill the core and for the fuel to heat up and exceed 2200°F. They also were assuming two HPI trains were available.

Their conclusion was that obviously we believe there are cases that would exceed 2200°F. with two pumps, so there's no sense doing a one-HPI calculation.

The Staff, upon learning this, turned around and told Westinghouse and Combustion of this problem, and they called us back within a short period of time and, I believe it was Denny Ross told us that they didn't know what this strange disease was, but they had it, too.

B&W turned around and issued a letter to all its customers on July 20th of 1979 recommending that pumps be tripped on a low reactor coolant pressure ESFAS actuation signal.

DR. PLESSET: Brian?

DR. SHERON: Yes, sir?

DR. PLESSET: In view of the difficulties of these calculations, I don't think we know yet how to do these calculations, do we?

DR. SHERON: Well, I think that EG&G will probably --

DR. PLESSET: Tell us about that?

DR. SHERON: -- be able to shed a little more light on our capability now that we do have some test data.

DR. PLESSET: I wondered how the vendors could come back so promptly with this assessment or assurance that they had to have the pump trip; that there was this spectrum --

DR. SHERON: Well, their calculations, based on their best judgment on how to model the primary system with the pumps running, indicated that they could produce conditions in the core which they estimated would have the clad temperature exceed 2200°F. So basically this recommendation for early tripping of the pumps was not Staff's idea; this was the industry's.

DR. PLESSET: I understand, and I appreciate that; but I wondered: What does this tell us about their abilities to make these calcualtions? Did they overestimate their abilities?

DR. SHERON: Well --

DR. PLESSET: They come back so promptly and so definitely with this well-defined window, and you wonder: Do they know what they're doing?

DR. SHERON: Well, I guess that was our question.

DR. PLESSET: Okay, so you also share that sentiment to some extent?

DR. SHERON: Yes. I think that's -- Well, right down here (indicating), it's a key conclusion, which I will get to. But the problem we had right at hand with the B&W letter was that it was providing conflicting guidance to their customers.

(Slide.)

On a previous I&E bulletin which went out right the Three Mile event basically said that if for some reason you get a small-break loss-of-coolant accident and your reactor coolant pumps are running, you know, for God's sake don't turn them off; leave them running. And now all of a sudden they get a letter saying that if you get a small-break loss-of-coolant accident, turn the pumps off.

Well, then the phone started ringing off the hook on everyone up to Ed Case. We found ourselves down in his office awfully quick on a Friday afternoon trying to figure out what to do, because they didn't know whether to listen to their NSSS vendor, or to listen to the Staff bulletin.

DR. CATTON: Brian, have any best-estimate calculations been made for the existing plants?

DR. SHERON: Yes, Combustion provided best-estimate calculations in their report, CEN-115.

DR. CATTON: Does their best estimate allow for subcooling?

DR. SHERON: Subcooling at the break?

DR. CATTON: Yes.

DR. SHERON: I would assume as much. Their report, however, did not provide a lot of detail on their calculations as to exactly what their calculations were predicting at the break and the like. Plus, at the time the primary concern, as we saw it, in the analysis models had to do with the flow-regime model; and in fact I think that without -- at the time, without any experimental data, that appeared to be one of the key differences among all the three PWR vendors.

DR. CATTON: Then the Semiscale results apparently pointed out the need to properly predict the amount of subcooling and that they only way you could do that was proper nodalization. And I agree with Dr. Plesset, I don't see how they could have done all those things in time to have made predictions that hold water -- I could have phrased that differently.

DR. PLESSET: Well, anyway --

DR. SHERON: I think that was our conclusion.

DR. PLESSET: Yes. I think everybody but the vendors seemed to have that opinion.

DR. CATTON: And I am still -- Maybe sometime this problem of Combustion Engineering versus Westinghouse coming to different conclusions with basically the same kind of plant --

DR. SHERON: No, they're not.

DR. CATTON: There are enough differences to explain the result? Okay.

DR. SHERON: Yes. I can explain those, if you want and if we have some time.

DR. CATTON: Okay.

DR. SHERON: Let me just run through this briefly here.

Basically what we did is we turned around and issued

Bulletin 79-05C and 79-06C on July 23rd, which you will note was, I believe, one day -- no, that was a weekend -- the 20th was the Friday, and the following Monday, the 23rd, we issued Bulletin 79-05C and -06C stating basically -- endorsing the B&W recommendation for early pump trip.

The reason we could live with this, I guess, was that, number one, Westinghouse was recommending this all along. The reason they were recommending it was I guess primarily because of what Dr. Plesset just brought up, that they said that: We have a lot of experience, and we know that when we trip the pumps that the plants can comply with Appendix K. We understand their behavior a lot better, and it has been studied a lot more than the case with the pumps running.

So they said that they believed the pumps should have been tripped all along, and now B&W was saying that and the Staff basically endorsed that recommendation because now there was never anymore question of compliance with Appendix K with the pumps tripped.

We turned around and I got elected to write up a report, NUREG-0623 which sort of tried to pull all this together as we knew it at the time, and to provide a basis for the actions being taken in the bulletin.

We also concluded at the time that we thought pump trip was probably best to be automatic, primarily because all three vendors were telling us that they had to have the pumps tripped, and that they had to have the pumps tripped for safety reasons. And also because the present requirements were taking credit for operator action in a very short period of time -- much less than the previous Staff estimates that were allowed, which was 20 minutes, I believe, in the Standard Review Plan Section 6.3.

Our key conclusion in NUREG-0623 was that flow regime model assumptions among the three PWR vendors were mutually conflicting. NUREG-0623 has a table, and if you track through the table you will be able to see every difference in every region of the primary loop, where one vendor had homogeneous, where another had separated flow.

As Dr. F sset also said, the ACRS did write a letter on the subject and recommended a restudy of the criteria for pump trip. The Staff agreed that this was certainly an acceptable way to go, and we have presently been doing that and taking a harder look at it.

We have included it in the Task Action Plan -- that's Item 2.K.3.5. What we are doing is including an evaluation of the capability of vendor ECC models to properly predict plant behavior during small breaks with the pumps running. So we are kind of giving them the opportunity to convince us that they know how their plants behave.

We issued a letter on April 15th of 1980 requesting all holders of approved ECC models, which basically brings

Exxon into the fold, to predict LOFT Test L3-6.

Now the Staff met with industry representatives in May of 1980. The purpose was to discuss the status of the pump trip issue, and to receive a briefing by EG&G on Semiscale tests that have been run to date, and to give the industry a chance to express comments, suggestions, and concerns on proposed LOFT tests.

DR. ZUDANS: Brian, on this requirement to predict the L3-6 experiment, what are the chances that the licensees, by using evaluation models, can make a prediction in the best-estimate mode?

DR. SHERON: We didn't ask them to use the evaluation model.

DR. ZUDANS: Then you assume that they will have other tools to do it?

DR. SHERON: Most of them do, yes, sir.

DR. ZUDANS: Okay.

DR. CATTON: So the game plan is to have them prepredict L3-6 to demonstrate that their codes can do the job properly; and then to make the predictions for their own plants?

DR. SHERON: Yes. I'm going to address what we are going to do with all this in a little more detail in the second presentation this afternoon. In other words, this is sort of bringing us all up to today; and then from today on, we will have the LOFT test, we will have vendor predictions, and I will be explaining, I guess, our plans on what we intend to do with them, how we intend to evaluate them, and what we intend to require the industry to do as a result of these predictions.

(Slide.)

On June 26th, Staff issued a letter to all holders of approved ECCS models, basically allowing the blind post-test analysis of Le-6 using actual test conditions. This is sort of a departure from the previous approach on either a standard, or what we call "required problems."

At the May meeting, the industry expressed great consternation about the problem of providing a pre-test prediction and then having a test be run, and the initial conditions were not the same as what was prescribed to them; and that perhaps during the test, a certain number of events occurred which were not spelled out in their pre-test prediction package. For example, some valve sticking open somewhere, or another valve closing when it shouldn't. They said that this usually provides for a poorer prediction than they would like to see, and they said that a lot of people may be making harsh judgments when they're really comparing apples and oranges.

So they're trying to compare apples to apples. They wanted to do a post-test based on the actual test. As you know, we always have the problem of: Well, gee whiz, you know, you're going to have all the test data in front of you and you'll be able to tune your models up, and it really won't be a "blind" prediction.

So what we agreed to was that we would let them do a post-test analysis, but they would document their models with the staff prior to the test. And by "documenting," this would include a printout of the actual input modeling assumptions that would be made, almost to the extent of setting up the code with what they think would be the proper initial predictions, and then running a couple time-steps to show us that the thing initializes and this is what they were going to use; and then, run the test and give them the data of the initial conditions. And that if they -- then, by sending in their final predictions, we could compare their initial to their final to make sure, to convince ourselves that no great modeling changes were made to tune up their model to the data.

And we requested that the models to be used for L3-6 be documented with the Staff by December 3rd. Now L3-6 I understand was scheduled to be run on or before December 17th, but hopefully not before December 3rd or we're going to have trouble.

(Slide.)

Now what does the Staff ask from RES? About the same time that we got the information on the problem from B&W, we sat down with Research to discuss what support they might be able to help us with on these small-break LOCA licensing issues. We requested a number of tests, including some pumps-on/pumps-off. We also recommended three different break size small-break tests to show a small-break when it repressurizes, a small break which sort of hangs up at the secondary side pressure, and then a small break which would depressurize all the way down.

We also, like I said, requested some pumps-on/pumps-off tests. Then we got embroiled in a little question of whether heat losses could be properly quantified from a semiscale system due to the excessive surface area from a scaling distortion.

I think it was around early February the concensus was that the test data from the pumps-on/pumps-off test in the Semiscale would give meaningful information; and that the heat losses could be properly quantified.

Also, Research proposed to run LOFT tests L3-5 and L3-6. L3-5 was a small break on the intact loop with the pumps tripped early. L3-6 would be the same test with the pumps left running -- the "pump" left running.

Research, with their contractor EG&G, ran three small-break LOCA tests for pumps-on/pumps-off problem in the Semiscale; and they also provided supporting analyses of these tests which I believe we'll be hearing about.

(Slide.)

Now the only thing I wanted to do here, this is a review of the phenomena as we understand it today -- just to run through again exactly why these pumps have to be tripped, why a window exists, and then if there is a little bit of time I will try and spell out any differences between say a Westinghouse plant and a Combustion plant.

For small breaks in the cold-leg discharge piping with the pumps tripped early, what happens is: The system will first drain down to loop seal elevation. Once this happens, then steam can pass around the hot leg through the steam generator, around the loop seal, and out the cold-leg break. Once you start to pass steam through a break rather than a low-quality two-phase liquid, you get what I would call "enhanced depressurization effect," and the system depressurizes faster than it was previously. This of course promotes ECC addition, and what happens is that the inventory going in from ECC exceeds the inventory being lost through the break, which is also greatly decreased because it's gone from liquid to steam. And so you get the inventory starting to recover on it.

Now for small breaks in the cold-leg discharge piping with the pumps running, the pumps basically are providing more of a homogenizing effect. The system will initially behave similar to a case where the pumps tripped, because the fluid coming out of the break is still going to be a very low-quality fluid -- although it will be of less subcooling than with the pumps tripped, because you're homogenizing through the pumps with some steam.

DR. PLESSET: You're also more effectively taking heat out of the core, which tends to heat up the liquid -- the mixture. Is that right? How much of an effect is that with the pumps running?

DR. SHERON: I don't really think it's too much of an effect in terms of removing heat from the core, because when the core is covered, the heat transfer -- the pool boiling type of heat transfer is basically a very good heat transfer mechanism at very low power.

DR. PLESSET: I guess it's only a little later that this would be an important effect on the flow that you're generating?

DR. SHERON: Yes. If you could push steam -- as a matter of fact, this is where I guess it was Dr. Catton's question -- one of the big differences between the Combustion calculation and the Westinghouse calculation is based on this very effect.

DR. PLESSET: Okay.

DR. SHERON: What happens, though, with the pump running is you don't get this loop-seal clearing phenomena because you're pumping this mixture around the system. So there is really no distinct liquid level in the system that's draining down, and the like.

What we think may happen is that the pump will continually put some sort of a two-phased mixture to the break location, rather than let it transition at some distinct time into say a low-quality two-phased liquid to steam. It's just going to keep putting liquid there.

We've seen some other evidence both in Semiscale and I think Dr. Griffith's table-top setup which shows pump chugging may have some effect. In other words, you would fill up the loop seal until it hit the suction to the pump, and then you'd kind of push a slug of water through the system; it would clear itself out, and then it would sit there and just be pumping the steam until the loop seal filled up again to the suction, and it would continue to chug -- which would also have some sort of effect on what is seen at the break. We don't have too much information on that right now.

But in any case, we don't think you would see this distinct transition of break flow from a low quality to a high quality; and there would be no distinct decrease in the mass lost from the system. Note that when I talk about draining to a loop seal elevation up here, this is only really for Westinghouse and CE designs. If you look at a lowered loop B&W plant, the loop seals down around the bottom of the core; and if you had of cleared that, you would obviously be calculating that the whole core would be voiding before you could pass steam. Obviously that wouldn't be acceptable, and in fact most B&W calculations show the core doesn't uncover. This is because of vent valves that exist in there and allow steam to pass directly from the upper plenum into that cold leg.

DR. CATTON: Does the LOFT have that problem with the bypass between the down --

DR. SHERON: They just don't have a valve there.

DR. CATTON: But they have bypass difficulties.

DR. SHERON: I don't know. I think that the latest estimates were what, about 6 percent?

MR. SOLBRIG: Very small. About 3 percent.

DR. CATTON: I've heard a great deal of concern about that expressed by some of the vendors.

DR. SHERON: Yes, and EG&G has done an extensive amount of looking at it, I believe, which they would probably be able to address.

DR. CATTON: I would like to hear the vendor arguments addressed.

DR. SHERON: I think there is another question coming up, because I have called all the vendors and they claim that they took credit for the bypass path which they believe exists in their reactors when they did these calculations.

DR. CATTON: It depends if you need it or not.

DR. SHERON: And they claim their bypass flow paths are on the order of a few percent. Their argument was that the initial estimates where LOFT had somewhere around 10 percent, much, much larger than their reactors, and that indeed if they had 10 percent, they would calculate their plants would behave the same way LOFT would in this area.

Now LOFT is basically coming back and saying that no, it's not 10 percent, it's a lot smaller. So then the question is --

DR. CATTON: I would like to hear how they know what it is.

One other thing, your second comment on that previous slide, near the bottom, "No loop seal clearing phenomena" in the transition and break flow. Gee, doesn't that depend on whether your flow is stratified or not stratified, plus the location of the break on the pipe's circumference?

DR. SHERON: Oh, definitely on the location. Remember, I pointed out that these are in the cold-leg discharge piping that I'm drawing these general observations on.

DR. CATTON: Okay.

DR. SHERON: I think it provides the clearest example of the differences of why pumps-on versus pumps-off makes such an effect.

(Slide.)

And again, the flow regime itself, yes, it could have some effect on that. And as I pointed out, we just got some recent information which shows that if one does get this chugging effect in the pump, that too could affect what's at the break. But none of the vendor models, or even our own, I think, can properly predict this chugging that might occur.

This is a little cartoon I drew up which tries to show why leaving the pumps running can get you in trouble versus when they are tripped.

Now let's take the case when the pumps are tripped very early, say at t = 0. What you get is, you get the subcooled flow which -- this (indicating) is the integral mass lost from the system. So as you get the liquid coming out the break and you're draining down, until you drain down to the loop seal and you start to pass steam out the break, now all of a sudden you get a lot of steam out the break and very little liquid. So the mass loss increases. It starts to turn over.

At some point, the primary system pressure drops down to about 600 pounds and the accumulators come on. Now for a CE plant, this (indicating) just goes out a little further until you hit 200.

The accumulators come on, and you start to recover. This is usually for the limiting breaks.

Now with the pumps running, as I said, the first thing they do is they tend to mix up all that fluid in the cold leg near the break, so it's not a subcooled. And because the critical flow goes up as subcooling goes up, because the subcooling is less the mass flow -- the critical flow is less, so you get slightly less mass flow out the break with the pumps running. Okay? And it continues upward -- as I said before, during this (indicating) region, they basically look like the same event whether the pumps are running or whether the pumps are tripped. There's just a slight difference in the mass flow.

But now here (indicating), at this point when the loop seal clears for the pumps-trip case, there is no clearing effect and you continue to push out two-phase fluid. You're pushing it out, and you'll see that the mass lost out of the system goes much higher in the pumps-running case than the pumps-tripped case.

I have penciled in this line (indicating) -- I call it a "critical mass loss limit" -- which basically says that for a given break size and time into the event, et cetera, and if I trip the pumps, would the collapsed liquid coming down produce a core overheating problem in excess of some criteria -- say 2200°F.?

And you can see that there may be a window that would exist then: That if the pumps were tripped whenever the mass loss out of the system was up in this range (indicatin that if those pumps were tripped at any time then, it would collapse down to an unacceptable level of core uncovery and produce excessive heatup.

So what you get, then, is a window in which you don't want those pumps tripped. You see, out here (indicating) is when the accumulators kick on to recover your inventory. The pressure has gone down enough. So this is how you get a window when you don't want to trip a pump. And this is a function, as I said, of break size and break location.

What I have, I think you may have seen these, but Ed Cromm ran these calculations back when for a Westinghouse four-loop PWR: a four-inch cold-leg break in two cases, pumps-off/pumps-on.

MR. RAY: Brian?

DR. SHERON: Yes, sir.

MR. RAY: These curves that you have just showed, are they still for the break in the cold leg?

DR. SHERON: Yes, sir.

MR. RAY: Only the cold leg?

DR. SHERON: Yes.

MR. RAY: Are you going to discuss what happens with a break in the hot leg?

DR. SHERON: I didn't intend to, because it's --

MR. RAY: Is the reaction similar?

DR. SHERON: No -- Well, it depends on the model, okay? Combustion Engineering predicted a hot-leg break would be the most limiting, and Westinghouse predicted a cold leg.

MR. RAY: Do I deduce from this that our only concern is with a break in the cold leg?

DR. SHERON: No. This is strictly to just try and illustrate why a window exists. Okay? It was not to -- There are windows that exist for the hot leg, but again I would like -- bear with me. I'll be able to discuss in a little bit some of the modeling differences that cause a hot-leg break in a Combustion plant to be more limiting than a Westinghouse.

MR. RAY: Okay.

DR. SHERON: Just to show that my little hand-sketch cartoon -- This was a calculation done by Ed Cromm of the Westinghouse four-loop PWR four-inch cold-leg break. He did -- You'll basically see overlays of four calculations, two with the pumps off, two with the pumps on. In one case you will see ECC flow going into the broken loop. In the other case, you'll see no ECC going into the broken loop, which is consistent with a licensing assumption which says that the ECC into the broken loop is assumed to be spilled onto the floor.

DR. CATTON: And their model has no stratified flow?

DR. SHERON: Whose model?

DR. CATTON: Is that correct? The one that is being used for this calculation you're going to show us.

DR. SHERON: No, this is bubble rise. That is stratified flow.

(Slide.)

This is RELAP. These are the break characteristics, and it was located on the center line of the pump discharge leg. The critical flow model used was Henry, Fouskey, and Moody with a CD of 1 and a decay heat of 1.2. So it was along the

EN lines to try and maximize the mass loss.

(Slide.)

This is the effect on the primary system pressure. The bottom curve is numbers 1 and 2, which is the pumps-off case with and without ECC into the broken loop. You can see that they're almost identical. 3 and 4 are with the pumps on, which is consistent -- namely, that you get the sharper depressurization when the loop seal clears; in this case, you don't get it.

(Slide.)

This is the break mass flow out to about 1000 seconds. Again, these raggedy lines (indicating) are 1 and 2, which is the pumps-off, and you can see that there is a very distinct break in the mass flow out the break. Here (indicating) it is more gradual.

MR. MATHIS: Brian, I can't read that. Where does that transition occur in terms of time?

DR. SHERON: I think he has it here. It looks like about 3- I'd say maybe 325, 350 seconds.

(Slide.)

And last, this is very analagous to that cartoon I just put up, which is the integrated mass flow out the break. Curves 1 and 2 are right here (indicating). Curves 3 and 4 are the sort of heavier line (indicating). Again, like the cartoon I just had up, curves 3 and 4, which are the pumps-on, you will note have a lower integrated mass out the break initially than with the pumps-off case, which is indicative of subcooling. So the codes are indeed predicting what was seen in Semiscale, which was a comforting observation.

We are predicting the higher subcooling. There is the cross-over point here, where now with the pumps on the integrated mass loss is greater. And I don't have the curve all the way out, but this would eventually turn over and come down.

That was all I had prepared for my presentation. If you want, I will try to just briefly discuss the problem with the Combustion, say, versus Westinghouse.

DR. PLESSET: Fine. Can you do that now?

DR. SHERON: Yes, I can do it now very quickly, I think.

The way we saw it, there were about two or three key differences in the way the system was modeled, and also the way the vessel is arranged in a Combustion plant versus a Westinghouse.

It is tied to, number one, how you model the hot leg and the uphill side of the steam generator. It is keyed to the pump performance curves; and it is keyed to the vessel geometry.

Now with the hot leg, Westinghouse basically does not have a countercurrent flow model -- a horizontal countercurrent flow model, or vertical, in their hot-leg components or their vertical uphill side of their steam generator. In other words, anything that enters into that hot leg at the vessel cannot find itself back into the vessel unless it goes up and around through the steam generator. Okay? Liquid cannot flow back and steam flow up.

DR. CATTON: That seems to be a rather severe restriction.

DR. ACOSTA: Yes.

DR. SHERON: Well, from the standpoint -- As I understand it, they seem to claim that was imposed by the old Analysis Branch way back.

DR. CATTON: That may be, but has the Staff made calculations with their own tools in both of these cases -- Westinghouse and CE?

DR. SHERON: We've made the calculations with the Westinghouse plant, but not with the --

DR. CATTON: Have you used your own code in LOFT and made a calculation on both plants?

DR. SHERON: Not on the Combustion plant.

DR. CATTON: So there is really no way to tell why they are different, other than discussion.

DR. SHERON: Right. At this point, we just thought that -- Well, number one, at the time we did not do the calculation on the CE plant because we did not have the CE model set up properly. Also, we believed that if we turned around and tried to run a pumps-on/pumps-off comparison for a Westinghouse plant versus a Combustion plant and try and examine their differences, what we would do is we would have three different sets of calculations. Because right now, the Staff knows nothing more than the industry does about how to set up a model with the pumps running.

DR. CATTON: But, you see, you've got a model developed by Combustion Engineering that Combustion Engineering uses to analyze their plant, and they claim that it's the best thing that ever came along.

DR. SHERON: Right.

DR. CATTON: You've got Westinghouse doing the same thing. You've got a table full of differences between their two models, yet you have your own RELAP series here in Idaho. What I don't understand is why you don't use it to do your own calculation in both plants and come to your own conclusions.

DR. SULLIVAN: Brian, I think we'll address that -- or at least I will -- in some detail later.

DR. CATTON: Okay.

DR. SHERON: Let me just go on with these differences--

MR. RAY: Brian, these different models of codes that you mentioned, Ivan, are they different because they are more characteristic of the specific plants? Or is there a different philosophy in the approach to the problem?

DR. CATTON: There is some different philosophy. Like one of the examples that Brian was mentioning is how they handle the hot leg, whether you can have countercurrent flow. One says "yes," and the other says "no." Well, do you really need it? I'm not sure. In some cases --

MR. RAY: It would seem to me --

DR. SHERON: One says "yes"; the other says, "you never allowed us to."

MR. RAY: Well, it would seem to me --

(Laughter.)

MR. RAY: It would seem to me that this fundamental concept, or rather the difference in philosophy might very well be a subject of some research. Which is proper?

DR. PLESSET: It is, but we'll hear more about it, I think, from Harold Sullivan. So let's wait.

MR. RAY: Okay.

DR. SHERON: I think you might, when you hear about some of these LOFT tests, the fact that when they tried to set up a case to get what the people call "reflux boiling," which is basically this liquid down/steam up -- when they thought they had the test set up to get those conditions, it just didn't appear. Okay?

So even though a code may be predicting that the conditions are right for a countercurrent flow in the hot leg, it is not supported by experimental evidence and it is still up in the air as to what is right. But what I am trying to point out is --

DR. CATTON: Well, Harold, what were we looking at when we were up here? Which part of the system did we see this stratified flow in in Semiscale?

MR. SULLIVAN: We were looking at the cold leg.

DR. CATTON: The cold leg?

MR. SULLIVAN: Where the --

DR. CATTON: Okay.

DR. PLESSET: That's the one where they had a movie --

DR. CATTON: Right. And that surface was just beautiful.

DR. PLESSET: Yes.

Well, I think we shouldn't interfere with Brian's presentation.

DR. SHERON: One of the key aspects, though, is that when you calculate countercurrent flow in the hot leg and on the uphill steam generator, you will calculate liquid running back down into the vessel. Okay?

If you have a hot-leg break on the bottom of the hot-leg pipe, that is basically going to keep the liquid source at that location. Westinghouse, by not having the countercurrent two-phase flow in their model, that obviously puts their most restrictive break location in the cold leg. So that's one reason.

Now the other reasons are, as we saw it looking at their models -- and these two are very closely related -- if you look at the Westinghouse vessel versus a Combustion vessel, closer down at the bottom of the core, you've got to look at the elevation from the center line in the hot leg down to where the flow has to take the turn up into the core.

Combustion has a flow skirt with perforated holes around the bottom. I forget the exact dimension of it; it's maybe about a foot or so. What you calculate when you have the pump running is you get a phase separation in the downcover. So you basically have a mixture level in the downcover which is depressed by the pump operating. Okay? When the pump is running, it basically depresses this level down to the bottom of wherever the flow is going to take the turn, and then you are going to pump steam up through the core, under say the bottom of the flow skirt, or wherever, and up through the core.

Combustion calculated that their pump model -- and this is coupled with their pump degradation model, and I tried to do a comparison of the two-phase homologous curves, and I just kind of threw my hands up in agony because it just wasn't really too possible to draw a one-for-one, due to the different characteristics of the pump. They did not calculate they could depress the two-phase level down below this flow skirt.

Now a part of that may be real; the other part may be contrived because they took no credit for the fact that they had little holes in their skirt. They assumed that those holes were plugged up, which they said was conservative. And what they did is, they tried to depress the liquid level down to the bottom of the skirt.

Well, the level that you can depress it down is a function of the pump head --

DR. CATTON: And we don't know the pump head elevation.

DR. SHERON: -- and the elevation. You basically had to push it down so many feet. So here is where the vessel differences come in. If there's a difference in the number of feet between the top of the hot leg and where it has to take the turn, you need a different pump -- develop pump head.

They calculated they could not push that level down enough to pump steam up and under and through the core. Westinghouse could. Therefore, Westinghouse, if you look at the steam flow through the core, it was about a factor of 10 higher than what you would predict if you had not depressed and pumped steam up through the core.

So Combustion's core cooling was only due to boil-off, due to decay heat boil-off steam; whereas, Westinghouse was basically pumping steam through the core to supplement the steam produced from the boil-off. So that is one reason why Westinghouse produced adequate core cooling with the pumps running. And Combustion said: Even if we leave the pumps running -- okay? They didn't even have to trip the pumps -- they said: If we just leave them running, we're going to get in trouble. The reason they got in trouble is because they couldn't depress the level enough.

Now we took a hard look at Westinghouse and, as a matter of fact, I even started doodling around with some elevations from a drawing and I found out that Westinghouse made a mistake when they set up their model, and they missed the elevation in that lower part. They underestimated it by a couple of feet, I think it was, a foot or two.

We called them up, and again they did some arm-waving and some hand calculations and showed us that even if they put the right elevation in, the pump-head characteristic was sufficient to depress the level to pump steam up. So, coupled with the fact that they were tripping the pumps early, we didn't feel at that time it was necessary to make them go back and recalculate everything with the proper elevation.

DR. CATTON: How well do they know the pump characteristics?

DR. SHERON: Well, that's a -- We've got a User Need letter to Research right now looking for that information. They did present some proprietary data from their -- called the "EVA tests" which showed two-phase characteristic homologous curves on a curve-scale test pump.

DR. ACOSTA: This is Westinghouse?

DR. SHERON: Yes, Westinghouse.

But they showed the flow-regime modeling in the cold leg had to be taken into account with respect to pump performance. And then we got into some of the questions concerning a lot of the pump characteristics have to do with flow at side entry versus bottom entry. The entry itself may determine how the flow comes out of the pump. Does it just kind of ride in the lower -- if it's a side entry, does it just kind of trickle in on the bottom of the inlet pipe if it's horizontal and trickle out on the bottom? If it is bottom, do you get the chugging effect -- because you suck it up, and then there's nothing left there to come into the impeller.

So I think the entry conditions, the geometry, can have an effect on this.

DR. ACOSTA: Yes, it would.

DR. SHERON: So that is information that we just don't have right now, I think, to really support the modeling in this area. It's one of the big questions.

But those are the three basic differences, as I saw it, between say Westinghouse and Combustion. But now if you take all three vendors --

DR. CATTON: Let me see if I got them right, then. That is the flow modeling in the hot leg; it's the elevation of the skirt; and it's the pump characteristics. Is that the three?

DR. SHERON: I would say it's more the elevation from the center line of the hot leg to the bottom of the skirt, and the pump characteristics. And then there may have been secondary effects just to phase separation modeling of the various components. Westinghouse had phase separation in the cold leg; Combustion didn't -- they assumed it's homogeneous.

I think if you look at the vendor models in whole, the one that was most different was not really different between Westinghouse and Combustion, but between B&W; basically everything was homogeneous. In other words, it was just a mixed-up system with some average-density fluid chasing around. They didn't have any separation other -- They're putting, as I understand, a slip model in right now in order to better predict the LOFT results.

But then again, they showed the most conservative time required to trip the pumps. And then it has to look and say: Well, am I looking for a best-estimate calculation? Or can I accept something which at least is shown to be conservative?

DR. CATTON: Is this why the bypass from the downflow to the upper plenum is so important, because of this level?

DR. SHERON: Well, for the pumps running case, yes, that is basically an equalizing effect. Okay? And if you have too big a bypass, you will not get that depression because you basically equalize the pressure. You're not trying to balance static heads anymore.

DR. CATTON: Right. Thank you.

DR. ZUDANS: Could you give me some explanation on this integral mass loss from the system? You showed a slide on that?

DR. SHERON: Yes.

DR. ZUDANS: And you showed that if the pumps are running, you continue losing the mass from the system beyond the point -- Well, I am actually referring to this scheme here (indicating), which is basically the same thing.

DR. SHERON: This is basically the same.

DR. ZUDANS: Yes, it is the same except for one point. What controls the accumulator injection? And why is it later in the case of pumps? It should be somehow related to mass lost from the system.

DR. SHERON: The system doesn't depressurize as fast with the pump on.

DR. ZUDANS: Even if it loses mass?

DR. SHERON: What?

DR. ZUDANS: Even if it loses more mass than the case without the pump?

DR. SHERON: No. The depressurization is basically a volume-controlled process. In other words, if you had a container that contained X amount of liquid and X amount of steam, and if you said: I remove one cubic foot of steam --

let me get it straight -- one pound of steam or one pound of liquid, which system would have a lower pressure? And it's the one where you remove the one pound of steam. In other words, you are removing more and more volume with the steam.

DR. ZUDANS: Well, but in this case we are looking -- I see. What you are saying is that if you remove more liquid out, you still retain the volume and therefore the pressure stays up? Right?

DR. SHERON: Yes.

DR. ZUDANS: But if you generate the same amount of heat in average, looking at the whole system, you have the same mass of the fluid, then it would appear that you should have something like the same pressure in either case.

What I'm saying, really, on this window case, wouldn't the accumulator injection occur much earlier in the pumps-on case than is shown there?

(Pause.)

DR. SHERON: Remember, the accumulators inject at 600 pounds, which is right down here (indicating).

DR. ZUDANS: Right.

DR. SHERON: And you can see that if these trends continue, this curve here (indicating) would expect to hit the accumulator-set point first; and that curve is 1-2 with the pumps off.

What is happening is that as you pass steam out the break, pure steam rather than a mixture, you start to depressurize faster. And by depressurizing faster, the accumulator set point is earlier and you recover the system earlier.

DR. ZUDANS: Thank you.

MR. ETHERINGTON: Well, essentially the difference between your cartoon and this case is that in this case you consider the heat balance as well as the mass balance.

DR. SHERON: Well, this cartoon was basically drawn up just to sort of, I would say, amplify this curve -- to sort of amplify where the differences are, and why they occur. In other words, I purposely flattened this (indicating) off, very distinctly, to characterize the transition from a liquid to steam coming out the break. Whereas, in this case (indicating), I purposely derated.

DR. PLESSET: You're going to come back to this? Or are you? Does this complete your part?

DR. SHERON: I have a second presentation I think this afternoon.

DR. CATTON: Just one more thing.

DR. SHERON: Yes, sir?

DR. CATTON: Does NRC plan to do its own independent calculations? If so, when?

DR. SHERON: Yes, we do, and I think Harold will tell you more about that.

DR. PLESSET: Why don't we let Harold Sullivan come in. I think it will help.

DR. SULLIVAN: It might make it worse.

DR. PLESSET: Well, that's a possibility, but --

(Laughter.)

DR. CATTON: We're here to help, Harold.

DR. SULLIVAN: My name is Harold Sullivan, and I am from the Office of Research.

(Slide.)

I have a discussion today on the Research program, and it is an introduction to some talks that are going to be later in the day from both the LOFT and Semiscale.

Brian has indicated that the licensing side asked us to do some research, and to respond to that we planned a two-part program. One was an experimental and the other was an analysis part.

Licensing and NRC found itself with a need to review some vendor analyses of which there are some differences in the plants themselves, which Brian has gone through; and also there were differences in how the vendors used their analytical models to model their plants. And because of those differences, it was difficult to determine exactly if the analysis was predicting what you would expect to occur.

So we formulated this approach. We chose two facilities -- the Semiscale facility and the LOFT facility -- and we will be discussing results from each of those experiments today.

We also have initiated an analysis program assessing the NRC Codes. And NRR has requested that the reactor vendors do an analysis of L3-6. And Brian also indicated that.

I would like to conclude the talk with some very general summary conclusions.

(Slide.)

Looking at the pumps-on/pumps-off experiments that we had planned to do, they were first to provide an experimental data base for the code assessment. We would like to understand some of the phenomena that is also occurring in those experiments, and particularly the effect of the pumps, the core-level swell, the break-flow phenomenon that Brian also addressed, and the two-phase flow conditions in the hot legs.

We also wanted to provide an experimental data base such that the vendors could perform some code assessment also.

And then, as I indicated, there is a required problem.

(Slide.)

We also wanted to address the difference in the scaling that was occurring, and that we were going to run experiments in both a smaller scale experiment LOFT and a larger -- I mean, Semiscale, and a larger experiment in LOFT.

The experiments that were planned to be performed, and we're pursuing those now: The LOFT L3-5 experiment with the pumps off. That has been completed, and Keith Condie will be addressing some of the results of that.

The L3-6 is the next experiment to be completed in LOFT.

The Semiscale facility has performed both pumps-off experiments, pumps-on experiments, and pump-trip at high void. Gary Johnsen is going to be covering those results.

(Slide.)

Looking now -- Turning to the analysis efforts which will probably help address Ivan's question. The purpose of initiating this analysis effort was to provide some code assessment of the NRC Code.

We wanted to have the understanding of the ability of the analytical models in our code to address the phenomenon that were occurring in the experimental program.

We wanted to further address the issue of scaling between the two experimental facilities; and to allow an evaluation of a plant in which we had completed this code assessment process.

DR. PLESSET: When you talk about NRC Code assessment, just what do you mean, Harold?

DR. SULLIVAN: The next slide addresses that.

DR. PLESSET: Okay.

DR. SULLIVAN: It is not a complete code assessment.

We call it a "mini-code assessment." We are trying to see how well the codes predict the experimental data, particularly for the pumps-on/pumps-off experiments.

(Slide.)

The process that we are going to go through is indicated on this slide, and it is just a summary of that process. We plan to do prepredictions and post-test analysis of the Semiscale facility's results. We would like to look at the pump degradation. As Brian indicated, that was one of the areas that NRR had questions about, and we were going to use any new experimental data that we have.

And EPRI has run a set of experiments with Combustion Engineering, and we are going to review that data to see if it would require us to change our degradation model.

We would prepredict the LOFT experiments -- both LOFT experiments -- and then we would take the Semiscale results and the LOFT results and compare those to the experimental data.

After that process, that would allow us to choose "a" code in which we would then put forth the analysis of a plant; but, more importantly, it gives us an audit capability to address some of the questions that we might have during the review of L3-6.

The codes that were to be considered are RELAP4, RELAP5, and the TRAC code.

DR. CATTON: Harold, I can see what your plan is to get to a code, that you could do these kinds of calculations, but I am just frankly surprised that there is not a code existing now that NRC could use to do audit calculations of their own.

I recall a year ago at a meeting in Los Angeles when this same question was raised with respect to a different problem by David Okrent. I don't see any difference between now and then.

DR. SULLIVAN: I think there is a major difference, and you will see some of the calculations that have been done with RELAP. The RELAP5 code has also been used, and you will see some of the results of those.

The major difference I think is that we have the capability, and some of those calculations have been performed not only by the people here, but by Licensing. So it isn't a question of "can we perform the calculations?"; it's a question of we wanted to make some assessment of the capability of the codes to predict the phenomena that are occurring in the experiments.

We ran the experiments, and now -- It was a parallel process; that we were comparing, or calculating the results and comparing them to the test data. So we are further along than what I think your question addressed.

DR. CATTON: Harold is being cautious.

DR. PLESSET: Where will the TRAC runs be made?

DR. SULLIVAN: That is a --

DR. PLESSET: When you try to do this assessment.

DR. SULLIVAN: That is a question that we are addressing now. Either Los Alamos is going to make them, or INL. I don't think they have decided exactly. Some of the calculations have been performed by Los Alamos already, I understand, but I am not sure.

Does that basically address your question?

DR. CATTON: I think so, yes.

DR. SULLIVAN: You might want to bring it up after you see some of the predictions that we have already made.

(Slide.)

In conclusion, there have been six experiments performed in Semiscale. Brian indicated there were three. He was talking about there are three basic different experiments. There is the pumps-on/pumps-off; and the pump trip at high void.

Results from that test series will be presented. The LOFT L3-5 experiment has been completed. The LOFT L3-6 experiment is the next scheduled experiment, and it also is a vendor required standard problem, and NRC is going to review the results of the vendor calculations.

From the experiments that we have completed -- and this is an error on the slide; it also has some words left out -- but from the experiments that have been completed, we have completed some of the code assessment work and you will see those. After you see those, if that doesn't address your questions, we will be happy to try. And you will see the results of those.

From that, we have concluded that the codes do have the capability of predicting the trends in the data. You will see that the magnitudes are slightly off. So that is an area that we will be addressing.

DR. CATTON: Well, Harold, I have a question that's not directly related, but I keep hearing about this big code package called "REM" or "RAM," or something --

DR. SULLIVAN: Yes, "REM."

DR. CATTON: And when you read the descriptions of it, it sounds like it is the answer to all our needs. Why is not something like that used? It's not what it's made out to be?

DR. SULLIVAN: Well, I don't know what you've read, but the --

DR. CATTON: I have three volumes. I've read the summaries.

DR. SULLIVAN: Okay. The REM package is being developed at Savannah River. Basically it is a code that is an Appendix K calculation.

DR. CATTON: So it would just be large break.

DR. SULLIVAN: So it's mainly large break, but it also has the capability of conforming to Appendix K. We are looking at best-estimate calculations. So there is a basic difference.

DR. PLESSET: I thought that this was also supposed to be fast running.

DR. SULLIVAN: The REM?

DR. PLESSET: Yes.

DR. SULLIVAN: Yes.

DR. PLESSET: So that you could run a lot of calculations without a lot of expense and time.

DR. SULLIVAN: It is relatively fast running.

(Laughter.)

DR. PLESSET: Well, maybe they didn't get it as fast as they were talking about.

DR. SULLIVAN: The main purpose of presenting that REM package was to be able to store a calculation at the first, to initialize, and to run all the way through the calculation without having to stop. All the data is transferred automatically between codes, and that was the major goal of that. And also, to make it a code that wasn't an Appendix -- acceptable to Appendix K.

DR. CATTON: It sounds like the REM code package is at least as good as the vendor Appendix K models that they initially used for this pumps-on/pumps-off assessment.

DR. PLESSET: I don't think so, but --

DR. CATTON: Well, it may not be as good, but it is supposed to be.

DR. PLESSET: I don't think it was intended to be like that. I thought that these were just supposed to be fast-running, and possible to make even surveys -- or, as Harold says, to run all the way through from beginning to end easily. But I don't think that they would be useful or suitable for what we are trying to straighten out here. Am I wrong?

DR. SHERON: The package which you're referring to -- which I think is called "WRAP" --

DR. CATTON: It may be WRAP. It starts with a "W."

DR. SHERON: As Harold said, it basically was set up to be a user-oriented compilation of the various codes which the Staff would normally utilize to produce an Appendix K audit.

DR. PLESSET: Just to audit the vendors' submissions, really.

DR. SHERON: Yes, sir. It was for both BWRs, PWRs, small breaks, and large breaks; but it was supposedly a code package which will comply with Appendix K, and will also, as Harold said, allow a user to be able to take a calculation from the start of the event to the full recovery. Whereas, previously, one had to run a version of RELAP to a blowdown, one had to do a hand manipulation of data transfer to the reflood portion, as well as to do adiabatic heatup calculations during refill, input all that, restart the reflood code, get the hydraulics for the reflood, take that level versus time plus the decay heat, put that into another code, and do the heatup calculation. It was a very, very long, time-consuming process, to the point where Staff just totally lost its ability to really do a credible audit calculation for Appendix K.

It is not designed, I believe in at least its present stages, to be something I would want to use to predict say a LOFT or a Semiscale experiment.

DR. PLESSET: It wouldn't help us in this problem we're interested in, as I understand it, this WRAP; right?

DR. SHERON: Right now I think we're trying to understand the phenomenon, and to go with the best-estimate codes. The industry looked at this with their models. As I said, some of the industry looked at it from the standpoint of Appendix K only -- Westinghouse.

DR. CATTON: So there you could have made a comparison with LOFT predictions.

DR. SHERON: Possibly, but I don't think WRAP was up and working about a year ago.

DR. CATTON: Oh, okay.

DR. SHERON: Now Combustion came in with both best-estimate and evaluation model, and they showed a difference, what the difference means.

The B&W did not even do what we would call an Appendix K calculation; they did a quasi-Appendix K.

DR. CATTON: So they weren't even up to Appendix K standards?

DR. SHERON: No, they didn't continue. In other words, they ran their CRAC model, and they used the Appendix-K type of assumptions on heat sources, except, number one, they used two HPI pumps instead of one -- which is the standard loss of single failure. They also strictly did the hydraulics. They did not do the heatup calculation.

As I said before, what they did is, they did some quick-and-dirty hand calculations, and their hand calculations, which were, as they described, on the conservative side, showed that they were going to significantly exceed 2200°F., and so they did not bother to turn around and do detailed heatup calculations based on the hydraulic predictions from CRAC.

DR. PLESSET: Harold, does that finish your presentation?

DR. SULLIVAN: Yes.

DR. PLESSET: Yes?

DR. CATTON: I think what I would really like to hear would be some sort of a presentation by NRR describing in detail just what is their audit capability; how well can they do these things? This is a question that has been raised year after year for the five or six years I've been associated with this subcommittee, and the answer is always the same.

DR. SHERON: Could I propose --

DR. CATTON: I won't pursue this anymore.

DR. SHERON: Right now, we've just -- Jack Gutman(?) from our section has put together a quick-and-dirty memorandum which spells out what he believes are the present audit capabilities within the now Reactor Systems Branch.

I would also like to propose that perhaps as a future subcommittee meeting topic we could come down and tell you exactly what capabilities we have, and what are planned through the technical assistance programs which have been set up for this year.

DR. PLESSET: Right. Actually, we've been already thinking about -- and this could be added to our discussions regarding codes and code assessments in the programs in NRC on this. So I think that is a good point, and I think we will do that --

DR. CATTON: Good.

DR. PLESSET: -- because there are so many different, distinct efforts in code development and code assessment, and how they fit together, and what the staff has been able to do in coping with all of this and using it is a worthwhile subject. We will do that in another subcommittee meeting.

I am glad to hear that Brian has got somebody in his group preparing for this kind of thing. Is that right?

DR. SHERON: Yes.

DR. CATTON: Is it possible to get a copy of this preliminary paper?

DR. SHERON: Yes. I was going to suggest that I will send you a copy of what Jack prepared.

DR. CATTON: Okay.

DR. SHERON: It's not much, but it just points out --

DR. PLESSET: Well, it is a little early, but --

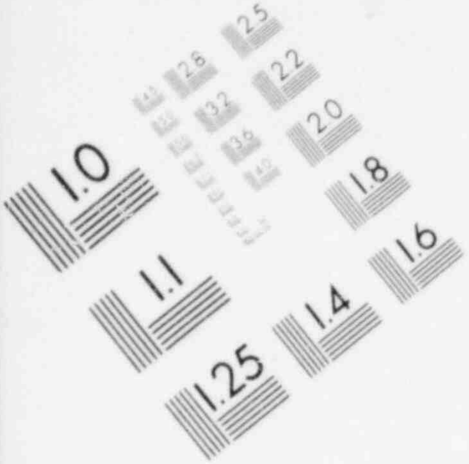
DR. SHERON: It points out what plant techs are available to the Staff right now. And like I said, the plans are for about the next year or so, through technical assistance contracts at the various laboratories, our intent is as a first step to get a plant tech set up for basically every plant type that exists. For example, a Westinghouse four-loop, three-loop, two-loop plant; a B&W raised loop, a B&W lowered loop; BWRs -- three, four, and five, I think -- and Combustion. And then we intend to take it a step further and try and almost have a plant tech available for every operating reactor in the country. It's a rather large undertaking, but we feel it is necessary as a longer term effort.

There is also a question of: Are we just setting up decks to do an audit calculation for Appendix K? Or are we really trying to understand the plant behavior and differences?

DR. CATTON: I would hope it's plant behavior.

DR. SHERON: Yes, it is. We are trying to set up best-estimate codes, as opposed to all licensing types.

DR. PLESSET: Well, thank you, Harold and Brian. I think we will take a short break at this point, a five-minute



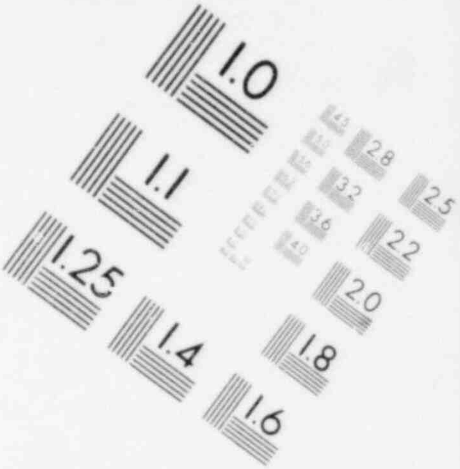
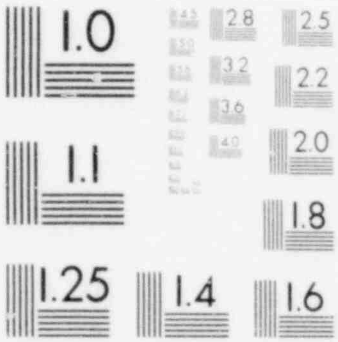
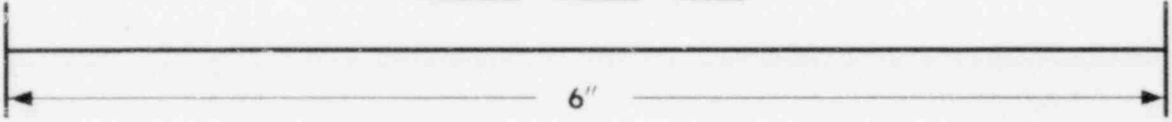
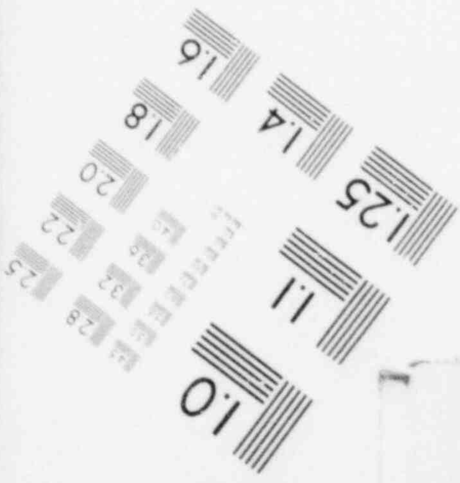


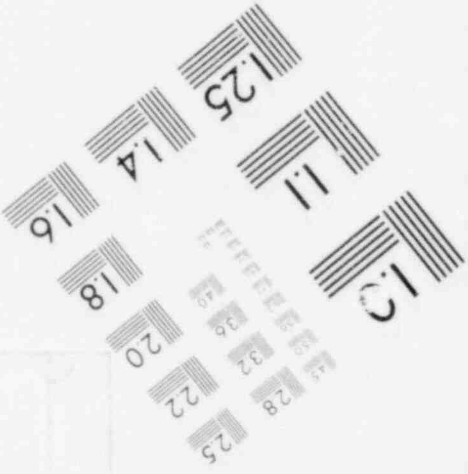
IMAGE EVALUATION
TEST TARGET (MT-3)

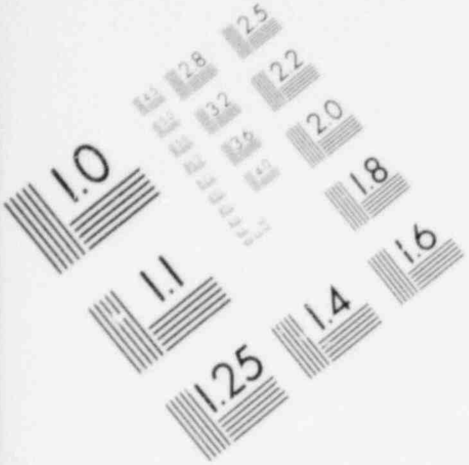




MICROCOPY RESOLUTION TEST CHART







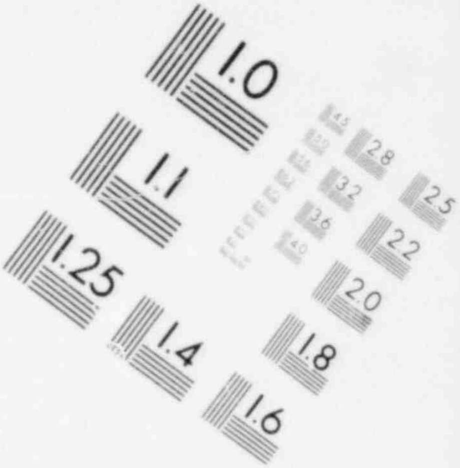
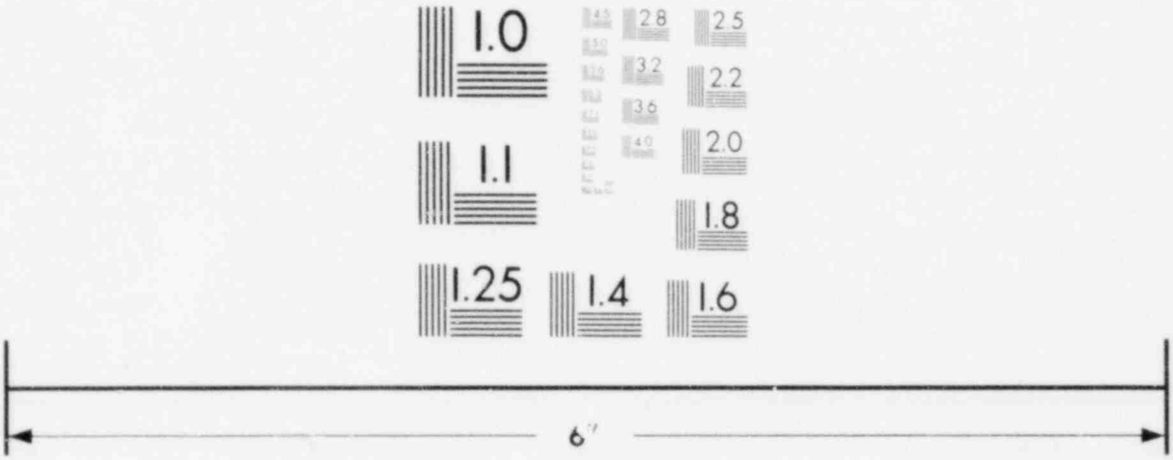


IMAGE EVALUATION
TEST TARGET (MT-3)



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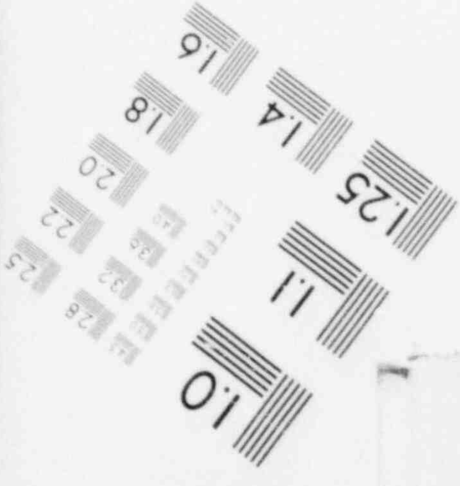

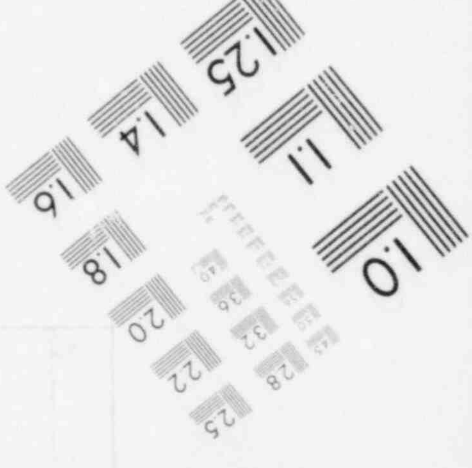

break. It isn't on the schedule, but we ran a little behind anyway.

(Brief recess.)

DR. PLESSET: Let's reconvene.

We are, not surprisingly, a little bit behind schedule, but let's go to the next important item on the agenda, which is "Coordination of Semiscale and LOFT Test Results, Pumps-on and Pumps-off," and Mr. Leach will make the presentation for us, I believe?

MR. LEACH: Yes. Larry Leach from EG&G, Idaho. I will try to get us back on schedule. I will talk about our tentative plans to close out the issue on analysis of both the Semiscale and LOFT test results, and analysis of PWR. It is really going into a little more detail than what Harold described in general in the overall plan.

(Slide.)

At this point, the prepredictions of all the Semiscale tests have been made with the RELAP4/MOD7 code. The preprediction of the LOFT L3-5 test has been made with the RELAP5 code. And we have post-test analyses of the Semiscale test also with the RELAP5 code.

DR. PLESSET: And they were done here?

MR. LEACH: That is correct.

Incidentally, there is also a pretest calculation with the TRAC code of the L3-5 test, and there will be one of the L3-6 test performed at Lascile (phonetic). I'm not sure about any of the Semiscale tests, if we have a TRAC calculation; Mr. Johnsen can probably answer that.

DR. CATTON: I would think that we should have, to make the story complete.

MR. LEACH: That is correct. The overall objective is basically to have a bakeoff; to have a set of calculations with RELAP4/MOD7, RELAP5, and TRAC, on the representative group of the experiments, if not all; then say: Okay, which is best?

And then go on and do the PWR calculations I will describe with that "best code."

So, the first objective of the analysis effort is to resolve the specific modeling issues raised in NUREG-0623. These are issues such as the stratification or the bubble-rise question.

Based on resolving those issues, then we would put together an optimum model for the PWR based on Semiscale and LOFT test/analysis results. That would say: Okay, the cold leg should be modeled as a stratified model, so we will use that then in the calculation of Semiscale, LOFT, and the PWR.

Then, to evaluate that model by comparison to the Semiscale and LOFT tests -- and this is essentially the mini-code assessment that Harold discussed.

And then finally, with that evaluated model to predict the behavior in a PWR to assist in the evaluation of the calculations performed by the vendors themselves.

(Slide.)

The optimum model calculations would be performed on, of course, the Semiscale tests, pumps-on/pumps-off, hot leg and cold leg break; and on the LOFT tests, pumps-on/pumps-off.

Apparently we have identified kind of a minimum set of analyses that we would perform on one PWR type. That is, looking at the hot-leg and cold-leg break types; the question of the pumps-on or pumps-off; and four different break sizes. We didn't mention that this morning, but there was some difference in the break size that led to this critical window in the different vendor calculations.

Now with a factorial approach to doing these calculations, that would lead to 16 analyses. Now this is not, of course, a complete analysis set of all the questions you can ask on the pumps-on/pumps-off issue. The vendor calculations have to resolve that.

(Slide.)

Some of the things that would not be addressed by this set are the ECC location relative to the break. Now we discussed this morning -- and you will see some more about the effect of ECC subcooling on the break. In the Semiscale test, the ECC was injected into the piping. In the LOFT test, the ECC was injected into the downcomer. So there is not a subcooling effect in the ECC there.

Of course in a PWR, you can have the break either upstream or downstream of the ECC location, and whether the pump is running or not you may or may not get the subcooling effect. One could do a whole spectrum of calculations on that.

The second is the question of the break location around the pipe, particularly with the pumps off. It makes a significant difference whether the break is on the top of the pipe, the side of the pipe, or the bottom of the pipe.

In the Semiscale and LOFT tests, the break is physically located on the side of the pipe for two reasons. One is because it was structurally more convenient to put it there; the other is because it is more probably that, particularly for a break in the cold-leg pipe, that it would happen above the center line of the pipe because that's where most of the nozzles are.

DR. PLESSET: Well, Larry, could I ask you a question about this -- I guess it was on the previous slide. I'm a little slow in catching up with you.

That smallest break, do you get depressurization ... that smallest break?

MR. LEACH: That's the one that would have a very long pressure plateau.

DR. PLESSET: It didn't really fall very much for a long time? Is that it?

MR. LEACH: It would fall relatively, you know, within 10 or 20 minutes, down to the secondary side saturation pressure, and then be on a long plateau.

DR. PLESSET: Well, I was trying to make a distinction in my mind. It's really not kind of a "leak"?

MR. LEACH: That's correct.

DR. PLESSET: Okay.

MR. LEACH: I think it's about a one-inch break in the PWR that the HPI can keep up to.

Brian, you probably know that.

DR. PLESSET: Well, that would be kind of like a leak; but these are really breaks -- very small breaks, on up to a big one.

MR. LEACH: Yes.

DR. PLESSET: Okay, I wanted to be sure I had it straight.

MR. LEACH: Of course, with the set of analyses shown we are not dealing with the design differences between the PWRs. That may be an important enough issue that it should be evaluated, and we are going to look further into this based on the outcome of today's meeting.

Fourth, the explicit treatment of the intermediate pump trip. Our intent is to do calculations with the pumps on throughout, and the pumps off throughout, and generate the kind of map that Brian showed you which will tell you where you would get in trouble and, based on that, make an estimation of which is safer.

Finally, the alternative ECC system availability is included in here. That is, the question of whether you have one HPIS pumps, or two HPIS pumps.

(Slide.)

The most optimistic schedule for getting through this effort is shown here. It is keyed to two things, really. It is keyed to the LOFT Test L3-6, which I show as December 1; Brian pointed out that December 3rd is really our earliest allowable date on that. That is within the accuracy of my dates.

And, the release of the RELAP5/MOD1 computer code, which is scheduled for the 17th of November. Now in practice, the calculations we have been doing with RELAP5 on both LOFT and Semiscale are with a version very close to this version that will be released, but not exactly the same. It is our intent to repeat these calculations with the released version of the code.

However, since it is with a very close version of the code, we feel we have a big leg up on evaluating what are the correct modeling assumptions and therefore should be able to make the evaluation within about three weeks after the LOFT L3-6 test, of what are the best choices to use.

Then we would repeat the Semiscale and LOFT calculations with this optimum code.

Then, based on the results of the bakeoff on which code is best -- RELAP5, RELAP4, MOD7, or TRAC -- we would complete the PWR calculations illustrated. And that would be around May of this year, the earliest that we could complete that. At that time, we would have the information on which to base the evaluation -- or on which NRR can base the evaluation. Of course it would take a few months after that for the report.

I thought it would be worthwhile to have this up front before we went into the comparisons on LOFT and Semiscale of the analysis in order to answer some inevitable questions.

Are there any questions on this that we should address?

DR. PLESSET: I don't have a question on this, Larry. It was very clear. Quite unrelated, is there a BWR version of RELAP5?

MR. NORTH: No.

DR. PLESSET: Would you identify yourself, please?

MR. NORTH: Paul North, EG&G. There have been ones done by private companies on BWR modifications to RELAP5. And while these are not available to us in hands, we are aware of their existence, and I believe they could be made available to NRC or somebody if they should want them.

DR. PLESSET: Well, I heard about that private development that you mentioned, and I wondered if you are going to have your own RELAP5 version for a BWR at some time.

MR. NORTH: It is not currently funded as a direct development, I believe.

MR. LEACH: That is correct, but it is our long-range desire to have that.

DR. PLESSET: Brian, why don't you give them the money?

(Laughter.)

MR. NORTH: Thank you.

(Laughter.)

DR. SHERON: I would point out that RELAP5 is a code that has been funded I believe entirely through the LOFT and Semiscale programs. It is not in the mainline code development of Research. In other words, it is more of a prediction pulled from these two experiments.

We do have a program set up at Brookhaven to develop LOCA models -- small-break, large-break, and transient -- for the BWRs best-estimate codes. I'm not sure right now on the availability of RELAP5 for this. We are, I believe, setting up for RELAP4/MOD7. Again, it is a matter of: Is the code available? Is it amenable to BWR configuration? And there is also that question of the code assessment verification. I don't know if there has been a lot done in, say, PLPA.

MR. LEACH: I think we have made one run with RELAP5. We are of course here developing the BWR modifications for the TRAC computer code.

DR. PLESSET: I was aware of that, Larry, yes. I was just specifically directed toward a RELAP5 version. I knew you were working on the TRAC BWR version.

Well, I was just going to raise a question: Does anybody read the ACRS Safety Budget Reviews?

MR. LEACH: We do.

(Laughter.)

DR. PLESSET: I wonder if it has any effect on anybody?

DR. SULLIVAN: Yes.

DR. PLESSET: That is nice to hear, but were you going to say something else, Harold?

(Laughter.)

DR. SULLIVAN: I assume you are addressing the comments on the RELAP5 program?

DR. PLESSET: Right, as one point.

DR. SULLIVAN: We are looking at increasing the funding that we are putting into that program. Our plans are right now to leave it at looking at PWR analysis. Research is also, as you noted, working on the TRAC BWR code and we hope that that will be our analysis for BWRs. We would like to wait to make a final decision on that effort.

DR. PLESSET: Spoken like a statesman.

(Laughter.)

DR. CATTON: Isn't there a --

DR. PLESSET: That's not a compliment, Harold.

(Laughter.)

DR. CATTON: Aren't there finite funding limitations?

DR. SULLIVAN: There always are, right?

DR. CATTON: That's correct.

DR. PLESSET: Okay. Well, thank you, Larry. We appreciate your helping us with our schedule.

(Slide.)

MR. JOHNSEN: Good morning. My name is Gary Johnsen, and I am the Manager of the Analysis Branch within the Semiscale Program. This morning I would like to review for you the results of experiments conducted in the Semiscale facility to examine this question of what is the effect of primary coolant pump operation on system thermalhydraulics during a small break.

We heard this morning about the technical issues surrounding this question, so I won't go into that. The balance of my presentation -- I will try to adhere to this outline here --

(Slide.)

First, by describing what were the specific objectives for our experiments, and how did we design the test to meet those objectives.

Secondly, to look strictly at the test results themselves, and what is the interpretation of the results that we obtained in the facility.

Then, to move on to the question of how well the codes did -- specifically, the RELAP4/MOD7 code -- in predicting what would happen in the Semiscale facility, and what we learned from that.

And finally, to draw some conclusions relative to this series of experiments.

(Slide.)

Now as has been mentioned earlier by Brian and Harold, the issues bearing on this question are fairly suscinctly contained in NUREG-0623. Our specific objective here was to conduct experiments that looked specifically at the question of what was the effect of running versus tripping the primary coolant pumps during a small break.

Specifically, two subquestions were: What is the effect on primary coolant inventory? And what is the effect on the distribution of the coolant within the system caused by the difference in tripping or running the pumps?

In running these experiments, then, we would be providing relevant integral system data which we could then use to determine what is the best way in which to model a system so that we can predict eventually in a PWR what is the effect of running versus tripping the pumps.

(Slide.)

Now as Harold mentioned earlier, we conducted seven tests in the Semiscale facility: Three cold-leg break tests and three hot-leg break tests.

All of the tests imposed a 2.5 percent break size on the system. By this, we mean 2.5 percent of the total flow area of the cold-leg pipe. This is equivalent to a 4-inch diameter break, if you will, in the side of the PWR pipe.

Now for each of the two break locations, we imposed three different pump-operation scenarios. We tripped at scram or at the beginning of the transient, in effect. We allowed the pumps to run continuously. And we also ran a case in which we tripped the pump at an intermediate point in the transient -- a point at which we had predicted that we would be a maximum void fraction in the system.

Now I will be concentrating throughout the remainder of the discussion this morning on the tests which called for the pumps to be tripped early versus running continuously.

DR. PLESSET: Would you, to just help some of us, translate this 4-inch diameter hole to the Semiscale size and to the LOFT size?

MR. JOHNSEN: In the case of Semiscale, this represents an orifice a diameter of 1/10th of an inch.

DR. PLESSET: 2.8 millimeters? Is that it?

DR. CATTON: 2.54.

DR. PLESSET: No, no, it's not quite a tenth of an inch.

DR. ZUDANS: It's .254.

DR. PLESSET: But it isn't. I think it's 2.79.

MR. JOHNSEN: It was .110 inches that I can recollect.

DR. PLESSET: Okay.

MR. JOHNSEN: .110 is my recollection.

DR. PLESSET: I recollect 2.77 millimeters. Am I right? Or 2.78?

DR. ZUDANS: That would be approximately right.

MR. MATHIS: Is that the equivalent of a 4-inch pipe break?

MR. JOHNSEN: On a scaled basis, that's equivalent to a 4-inch diameter hole.

DR. PLESSET: It's pretty close to 2.8 millimeters in Semiscale.

DR. WU: May I pursue, also, this? The scaling is based on geometric and mass flow, and other factors?

DR. PLESSET: No, they --

MR. JOHNSEN: Scaling is -- in the case of break size?

DR. WU: Yes.

MR. JOHNSEN: The scaling is based on preserving the ratio of the area of the break to the total primary coolant system MOD, so that ratio will be the same.

DR. PLESSET: You preserve area to volume ratio --

DR. CATTON: On geometrically scaled.

DR. PLESSET: Yes.

DR. WU: Geometrically?

DR. PLESSET: Yes. I am sure that they had thought of the question of whether this gets down to a size where other effects could come in.

DR. WU: Yes.

DR. PLESSET: At 2.8 millimeters, it is a fairly small hole. Now LOFT, it's much bigger.

MR. JOHNSEN: Yes. This question was considered prior to running these experiments.

DR. PLESSET: Yes. Okay, I just wanted them to know that you had done that.

MR. JOHNSEN: Yes, we had. In fact, we had done some calibration of these orifices prior to running these experiments to see if we noted any atypicalities with sizes that are larger than that. We did not.

Did you want the LOFT sizes? I think that was slightly over 6/10ths of an inch in diameter.

MR. MODRA: 16.19 millimeters.

DR. PLESSET: What was that number?

MR. MODRA: 16.19 millimeters.

DR. PLESSET: 16?

MR. MODRA: 16.19 millimeters.

MR. JOHNSEN: It is also I think important to note --

DR. PLESSET: Did you get those numbers? I think that some of the committee members might be interested in those numbers.

THE REPORTER: Yes, sir.

DR. PLESSET: Thank you.

MR. JOHNSEN: I think it is also important to note that another ground rule in conducting these experiments was that we did not allow any accumulator injection. By doing so, we provided a more unambiguous means of determining what the inventory of the primary coolant was in the system. So in none of these experiments was accumulator injection included.

However, high-pressure injection was simulated to the extent that one of the two trains was operable.

(Slide.)

Now the actual configuration of the break is shown here on this slide in which we are looking at the break that is inserted in the system. As you will note, the break is physically located on the side of the pipe. It is in fact at the same elevation as the center line of the pipe, and it is communicative in nature.

This particular diagram shows where it sits for a cold-leg break, which is between the pump and the vessel. The hot-leg break would be located between the vessel and the steam generator in-leg.

Now in all of these experiments, what we did was to direct the break flow to a condensing and catch-tank system so that we would have a very accurate determination of the coolant that had left the system from start to finish of the experiment. This measurement then could be corroborated against other measurement techniques to infer what the transient cooling inventory was.

I would like now to turn to --

MR. ETHERINGTON: How does a divergent nozzle like that correlate with a random type of break?

(Laughter.)

MR. JOHNSEN: How random?

MR. ETHERINGTON: You specify the break. What does this correspond with?

MR. JOHNSEN: This is a fairly sharp-edged orifice That is, the entrance is fairly sharp. The L over D of this orifice is fairly close to what was used in LOFT and, in turn, is supposed to be fairly close to what the L over D would be if a primary coolant system pipe had broken. That is, the area was equivalent to this area and the path length to the outside environment, taking those dimensions you would get the same L over D.

Now whether in fact that is a desirable situation is up to some speculation.

DR. PLESSET: So there's some pipe-thickness effect into it, as I understand. Is that correct?

MR. JOHNSEN: That's what I'm trying to imply.

DR. PLESSET: He's trying to get some of that into it somehow.

MR. ETHERINGTON: There's a control parallel section in this cross-section, is there?

MR. JOHNSEN: I guess I don't understand your question.

MR. ETHERINGTON: Well, the cone doesn't go right to the surface. Presumably there's a --

MR. JOHNSEN: Oh, that's correct. There is a straight section prior to the expansion.

DR. CATTON: You're really doing this in two parts, then. You ve put an orifice into the side of that pipe where you're going to know the mass flow. Correct?

MR. JOHNSEN: Well, in fact in these experiments an accurate transient mass flow was not obtained.

DR. CATTON: Oh, I thought you made the flow --

MR. JOHNSEN: Yes, but that really only gave us an end point, as opposed to a good transient to measure it.

DR. CATTON: Okay, but in any event, this a nice -- relatively clean. You could, in another step, relate various kinds of breaks to mass flow and tie the whole thing together for your analysis.

MR. JOHNSEN: Yes; that's correct.

(Slide.)

Okay, I would like now to turn to an examination of what happened in the cold-leg break experiments, first. This slide shows a comparison of the coolant inventory in the pumps-on case versus the pumps-off case.

Now the dotted line represents the results when the pumps were operational; whereas, the solid line represents the case where the pumps tripped early. You can see that the transient inventory was actually lower as the pumps tripped early versus with the pumps running. The difference in the minimum points of coolant inventory in these two experiments is not really very substantial; it only amounts to about 8 percent in the difference between the two values.

However, if one relates that sort of a difference to a change in the vessel inventory, it can be quite significant in terms of either uncovering or not uncovering the core -- and I want to make that point fairly clear.

DR. ZUDANS: How did you get this system mass you just stated? A minute ago you stated you only got an end point.

MR. JOHNSEN: These are the other measurements I alluded to earlier that were indeed corroborated against the end point measurements.

Now the way in which these traces were produced was by using our Delta T measurements from which we can infer the level in the system, in various parts of the system, taken together with our gamma densitometers which give us discrete axial indications of fluid density. Those two types of measurements throughout the system combined were used to arrive at these curves.

Now when the process by which we use those measurements was completed, we then compared the end points to the catch-tank values and found out they were in excellent agreement.

DR. ZUDANS: So this was calibrated against the end point?

MR. JOHNSEN: Yes. The end points were not used --

DR. ZUDANS: I understand.

MR. JOHNSEN: -- to arrive at these --

DR. ZUDANS: It just worked out to be okay.

MR. JOHNSEN: Right.

Now the reason for the fact that with the pumps tripped the coolant inventory reached a lower value we found was directly attributable to the differences in break flow --

(Slide.)

-- which are compared on this slide here. Again, the solid line is the break flow with the pumps off, and the dotted lines are with the pumps running.

Now referring back to what I had said earlier about the transient mass flow measurement, it is true that we don't have a tremendous amount of confidence in the transient measurement which is made downstream of the break, and that is in fact what was used to yield this comparison here. However, qualitatively we feel it is a good measurement.

Now we can see here that in the early part of the transient, up to about 200 or 250 seconds, that the pumps tripped early there is a higher mass flow rate than with the pumps running. After that point in time, there is a slight difference in the opposite direction which ties into what Brian Sheron said earlier this morning: That their calculations have shown that there would be a cross-over point in inventory. In fact, the cross-over did not occur in Semiscale, but there was indeed a cross-over in the discharge to the break.

Now early in the transient, the reason for the difference in these two break flows can be pinpointed to the fact that in the vicinity of the break, the fluid is much more subcooled in the pumps-off calculation than it is in the pumps-running calculation -- when I say "calculation," I mean "test," "experiment."

(Slide.)

Here is a comparison of the degree of subcooling right in the vicinity of the break for those two experiments. Again, the solid line is with the pumps off. One can see that the fluid tends to take on a higher degree of subcooling during the same period of time that we see a greater break flow in the pumps-off case than it does in the pumps-on case. In the pumps-on case, the fluid reaches saturation fairly quickly because of the homogenizing effect of the pump's operation. Where the pumps are tripped off, we see the subcooling in the vicinity of the break for two principal reasons:

One is that it is fairly close to the injection point for the high-pressure injection system, and the subcooling that we see here is in part a consequence of the liquid coming from the ECC injection and pooling in the vicinity of the break.

Secondly, what happens when we trip the pumps off earlier is that we tend to stagnate the fluid that sits in the steam generator, especially the downside of the steam generator tubes, and the fluid becomes more cooled for a period of time when the temperature differential is in that direction than would otherwise occur when the pumps are running.

So for those two basic reasons, we see a greater amount of subcooling near the break, which leads to a higher break flow and a greater mass depletion when the pumps were tripped early.

DR. CATTON: Could you go through the second, again, the steam generator?

MR. JOHNSEN: The second was that when we tripped the pumps early, the fluid tends to stagnate in the system as opposed to causing it to flow. The only inducement for flow essentially is the train behavior, and of course the fact that the fluid is seeking itself towards the break.

Now on the down side of the steam generator tubes, the water that is sitting in there is cooled to a greater extent when the fluid is sitting in the stagnant situation than opposed to when it is flowing through when the pump is running. Its residence time is longer, in effect.

DR. CATTON: Okay.

MR. JOHNSEN: And that is what contributes to the supercooling.

DR. CATTON: You need the Delta T to derive whatever circulation is there.

DR. PLESSET: Do you have any temperature measurements at some points in the core that would give you a difference in temperature for the two cases -- for example, in the period say from 50 to 100 seconds? Do you have any temperature measurements?

MR. JOHNSEN: I don't have the slide readily available, but I can tell you --

DR. PLESSET: Qualitatively.

MR. JOHNSEN: I can tell you that in both tests for the period of time that you cited, the core temperatures followed the saturation temperature associated with the pressure that the system was at. In other words, where there was no core uncovery in that period that you mentioned between 50 and 90 seconds.

DR. PLESSET: My point was directed toward seeing if you could observe a lower temperature in the core with the pumps running versus with the pumps off.

MR. JOHNSEN: The only reason there is in fact a temperature difference at all between the pumps-on and the pumps-off case in the case of the core is by virtue of the fact that the saturation pressure was different. The coolability of the core was not different at all to an extent.

DR. PLESSET: So you wouldn't expect much difference in temperatures?

MR. JOHNSEN: No. None whatsoever. In both cases the decay heat was being adequately removed in that period of time.

DR. PLESSET: Well, but that's a little different from saying that the temperatures are the same.

MR. JOHNSEN: True. But in fact the temperatures were very close together and were virtually the same. The only difference being that in the pumps-on case the saturation -- or I should say, the depressurization was slower, which indeed agrees with what Brian said earlier, and consequently the saturation temperature was somewhat higher than it was in the pumps-off case.

Now --

DR. PLESSET: I think Brian wanted to make a comment.

DR. SHERON: I would just point out that because you are at a low pressure, the saturation temperature is much lower in terms of the clad, let's say. Even though you may say one is warmer than the other, you are well below where the fuel was operating during the steady-state because the saturation temperature is down. If you're down around 1100 psi, your saturation temperature is somewhere around 500 or something, isn't it?

MR. JOHNSEN: Yes.

DR. SHERON: The clad is normally running up around, if you have a hot-leg temperature up around 600, you can be sure the clad is probably running up closer to 7 during normal operation; and during these conditions when the core is covered and you're down in pressure with very low heat generation rates, the clad may be running at, I would say, less than 600.

DR. PLESSET: In any case.

DR. SHERON: In any case, right. So you still are below where you were at steady-state.

DR. PLESSET: Oh, of course. I understood that. But I was just wondering if there was any difference in the heat transfer when you had the pumps running versus when you didn't; that you might have a little better heat absorption from the clad with the pumps running.

MR. JOHNSEN: In that period of time, the nuclear boiling --

DR. PLESSET: It's very effective, in any case.

MR. JOHNSEN: It's very effective in any case.

DR. PLESSET: I think that's really the thing I wanted to hear.

DR. ZUDANS: On this figure, at what time did HPI come on?

MR. JOHNSEN: At about 50 seconds.

DR. ZUDANS: So this subcooling could be --

MR. JOHNSEN: In both cases.

DR. ZUDANS: So the subcooling could be entirely due to that.

MR. JOHNSEN: If one does a simple hand calculation and then tries to attribute the total degree of subcooling to the injection of ECCS, one fails. So that is not the only contributing factor.

The analysis that we went through quite clearly showed that the steam generator heat transfer I mentioned a minute ago was also a contributing factor.

DR. PLESSET: Did you want to --

DR. ACOSTA: Would you go back to your previous slide of mass flow rate against time? What is happening out around 300 seconds or 400 seconds? Would you describe what is happening there?

MR. JOHNSEN: Yes. Of course the dotted line there is the pumps-running case, and indeed you see a general higher mass flow rate if the pump is running. Now the reason for these spikes that you see here is that the pump was exhibiting a slugging behavior during this period of time. This is a behavior that Brian Sheron alluded to earlier that we noticed in Semiscale, and it appeared to be the consequence of the fact that the pump would send a slug of coolant down the cold leg when a sufficient amount of liquid had collected in the pump suction trap. Once that clearing had taken place, the pump would then just be pushing steam essentially. And then this cycle would repeat itself for some period of time. Each time the slug of liquid was in fact pushed down the cold-leg pipe by the pump, the break flow would increase for that duration.

DR. PLESSET: Do these pumps do this indefinitely, Dr. Acosta?

DR. ACOSTA: Well, if they're designed for it; but I don't think they are. Do they happen in full scale? Does this phenomenon happen in full scale?

MR. JOHNSEN: I don't know.

DR. ACOSTA: We've seen no -- in all the traces we've seen this morning of similar pressure time histories, we haven't seen anything like this.

MR. JOHNSEN: I can't answer your question. However, I don't know how many of you are familiar with Dr. Griffith's little benchtop model --

DR. ACOSTA: I'm not.

MR. JOHNSEN: -- which he has constructed out of plexiglass, and which shows the same basic behavior as we noted in the Semiscale.

DR. CATTON: A rather small diameter.

MR. JOHNSEN: That's very small -- a very tiny system -- maybe no bigger than that (indicating).

Now we see that behavior in that system, and we see it in Semiscale. I would say that if we saw it in the LOFT, then my predeliction would be to say that it probably does occur at full scale; because if we have those points to connect there, I think I could be pretty safe in making that assumption. But I can't answer the question.

MR. LEACH: Gary Leach. I think it might be worth emphasizing that the Semiscale pump has been tested in steady-state two-phase flow. For the conditions under which it was slugging here in the test, it did not show that type of behavior in the separate-effects test. And indeed, while it was operating here, pumping with the head degradation, it was quite different than we observed in the separate-effects test.

So I would be very cautious about relating what you see in the separate-effects two-phase flow test to what might happen in the system, if it is truly a system effect that is causing this behavior.

DR. PLESSET: I think that is a good point to remember.

DR. ACOSTA: It probably is a system effect.

DR. PLESSET: Yes.

DR. SHERON: Gary, is the pump in the broken loop a bottom-entry or a side-entry pump?

MR. JOHNSEN: A bottom-entry pump.

DR. SHERON: That -- It's just a gut feeling on my part, but that may be very significant; because the side entry, you could have sort of a continuous layer or level of fluid feeding this pump; whereas, in the bottom entry you have to fill up this loop seal basically until you can draw that suction, and then it cleans it out and then you have nothing there.

One of the points that we learned from this is that the EPRI tests -- and I'm not sure of the EPA tests, as well, from Westinghouse -- which they developed their two-phase degradation models for side-entry pumps, and in fact I would point out a system effect may have a very strong influence on the way that pump behaves. So you just can't take a very controlled test with a side-entry pump and feed it a certain amount of something and say what comes out is the way it behaves in the system.

DR. PLESSET: Right.

DR. ACOSTA: But the same kind of a phenomenon conceivably could occur for a vertically mounted pump, as well. Everything hinges upon the details of the inlet.

MR. JOHNSEN: Just to expand a moment on what Larry Leach said a moment ago, the model that we have for the Semiscale pump was based on tests that were carried out several years ago at Westinghouse-Canada. In those tests, the tests were conducted such that the inlet conditions were uniformly homogeneous, so that the model that was therefore produced utilized essentially a single independent variable being void fraction.

In the actual use of the pump in our facility, we see quite clearly that the topology of the flow at the inlet is not homogeneous, at least for the series this break size. In fact, we see stratification. And therefore, that the results cannot be predicted adequately by the model based on the homogeneous issue.

DR. ACOSTA: I think that is an important observation, Mr. Chairman.

DR. PLESSET: Right. I think so, too. And I think that this just amplifies what Brian was telling us this morning regarding this chugging. And I don't think these pumps are ever designed to do that for any length of time and survive.

What would you say? Does anybody know?

DR. ACOSTA: Is there a pump vendor here?

(Laughter.)

DR. ZUDANS: It's kind of obvious, the mechanical, they couldn't take it forever.

DR. PLESSET: Yes. Would you identify yourself?

MR. QUAPP: Bill Quapp, EG&G. I might comment that on a request to the NRC some months ago, we investigated the possibility of performing some two-phase pump tests on a full-scale Bingham-Wolloman pump identical to that used on TMI-2.

In our discussions with the Bingham-Wolloman company at that time, that vendor claimed that the operation of the pump in terms of the two-phase flow in the pump impeller cavity region, that didn't bother them too much. The main concern was maintaining the integrity of the seal cooling system, which in their pump is totally dependent on the primary system coolant's inventory; and therefore, they claimed to us at that time that's the reason why the TMI-2 pumps did survive for a very long duration of operation during that accident.

DR. PLESSET: But he's talking about survival of the pump in two-phase flow, and was he thinking of this chugging? Or was it just moderately homogeneous?

MR. QUAPP: Survival was discussed there in terms of the mechanical integrity. That is, the thing didn't self-destruct into rubble.

DR. PLESSET: Yes, but was he thinking in terms of what we are asking about now where you have really chugging? I mean, a two-phase mixture that is more or less homogeneous I'm sure that these pumps would not have difficulty; but chugging, I think, is a different question.

Is that right, Dr. Acosta? Would you agree with that?

DR. ACOSTA: I would think so. But I would think this is an issue not normally addressed by pump manufacturers to accept 100 percent liquid and 100 percent vapor ultimately. Whether or not one should design pumps to do this for these kinds of systems is I think a separate issue that ACRS might separately want to take a look at, but it's certainly not the state of the industry.

MR. QUAPP: I think, relative to the question of the system's effects on this phenomena, if during the course of the TMI-2 event the pump current or some of the electrical parameters of the motor were recorded, one could see whether or not this same phenomena occurred in that system. Because we looked at the Semiscale motor current variations as a function of void fraction and related that quite reliably to that told to us by the vendors what would happen as a function of void fraction.

So we may already have the data. I don't know what exists at TMI.

DR. PLESSET: Well, it would be interesting, yes.

Yes?

MR. NORTH: Paul North, EG&G. The question of the chugging -- this may be of interest to the Committee. If you recall, there were experiments done on alternate ECC injection a few years ago with the Semiscale MOD1 system. Injection was done in the pump suction leg, and we observed very similar chugging performance then on the pump with the total system conditions being quite different, and the geometry being somewhat different than this system, because it was the MOD1 system.

So I would expect that, seeing that, that this is far from an isolated occurrence of this kind of behavior --

DR. PLESSET: Yes.

MR. NORTH: -- and it might occur over a range of conditions, in fact, and a range of geometries.

DR. PLESSET: Maybe that's not a very good alternate ECCS system, either.

(Laughter.)

DR. PLESSET: Thank you.

Go ahead. I didn't mean to make it such a lengthy interruption.

MR. JOHNSEN: That's fine.

(Slide.)

As mentioned earlier, another significant aspect that we were interested in was distribution of the coolant as it was effected by pump operation. Here we are looking at a comparison for again the cold-leg pumps-on/pumps-off, the amount of cooling in the vessel itself.

Now in both cases, although it is not illustrated here specifically, in both cases the percentage of the total primary coolant that resided in the vessel increased as a function of time. In other words, as the break experiment proceeded, more of the coolant on a percentage basis remained in the vessel than in the loops. But we can see here that with the pumps running, we had a greater amount of coolant in the vessel, both on an absolute basis and on a percentage basis than we had when we tripped the pumps early.

In the pump-trip case, once the pump suction seal clears in the fashion described earlier by Brian Sheron, we no longer deliver any coolant to the vessel; whereas, with the pumps running we continue to deliver coolant to the vessel throughout the transient.

DR. PLESSET: Does the vessel inventory include the inventory in the downcomer region, as well?

MR. JOHNSEN: It does.

DR. PLESSET: So they're both there?

MR. JOHNSEN: The entire vessel is included here.

(Slide.)

The effect that this had on the actual coolability of the core we can look at here on this slide, where we are comparing the mixture level in the vessel for these two experiments.

Now as I indicated earlier, there was a fairly modest difference in the total coolant inventory between the pumps-on and pumps-off, and it only amounted to approximately an 8 percent difference of the minimal points. However, in the pumps-off case we actually did uncover the core during the course of this experiment. The solid line which represents the mixture level in the vessel for the pumps-off case is shown quite clearly here to descend into the core itself. The top of the core is shown here (indicating). It is just below minus 100 centimeters, zero being the center line of the cold leg. Whereas, in the pumps-on case -- and in fact in this case we're showing the delayed pump trip case instead of continuous pump running -- the mixture level stayed in the vicinity of the hot-leg elevation until the pump was tripped.

This (indicating) is the intermediate pump trip case, and when the pump was tripped, you can see the initial level dropped down slightly. So there was a definitely adverse effect in this case of tripping the pumps early in terms of keeping the core cooled.

(Slide.)

Now I would like to turn to an examination of what happened in our hot-leg break experiments. Here (indicating) is a comparison analogous to the one I showed earlier for the cold-leg break experiments for the total coolant inventory in the system as a function of time.

Again, the pumps-off is the solid line, and the pumps-on is the dotted line. Now here we saw just the reverse behavior of what we had noticed in the cold-leg break test. Here indeed when the pumps were allowed to remain running, more coolant exited the system, and indeed we reached a lower coolant inventory. The difference here was more significant than we had seen in the cold-leg break test. In fact, the difference in the minimum coolant inventory in these tests is about 27 percent.

(Slide.)

Again, the difference in coolant inventory was directly attributable to differences in break flow. Now rather than show a comparison of the break flow, what I can show you here is a comparison of the fluid density upstream of the break, which indeed influenced the break flow.

Here we are comparing the fluid density in the vicinity of the break. The upper curve here (indicating) is the pumps-on case, the dotted line. The lower one of course is the pumps-tripped case. Quite clearly, with the pumps running we delivered more coolant to the hotter portions of the system, including the hot legs, thus increasing the liquid inventory in the hot legs, thus leading to greater discharge of coolant from the hot-leg break.

(Slide.)

In terms of the distribution of the coolant, we also noted in the hot-leg break tests that running the pumps ended up delivering more coolant to the vessel than when the pumps were tripped early. Now on an absolute basis, running the pumps resulted in less mass in the vessel than when the pumps were tripped early, which was opposite from the cold-leg breaks test. But on a percentage basis, if one took the entire amount of coolant in the system and compared how much of it was in the vessel versus the total, that the same trend was borne out here as was borne out in the cold-leg break tests; that the greater percentage of the coolant was in the vessel than was in the loops with the pumps running.

(Slide.)

Now we can kind of summarize the results we obtained in terms of the total inventory --

DR. CATTON: Do you have the core uncovery graph for the hot-leg break for the vessel mixture level?

MR. JOHNSEN: I do have an additional slide on the vessel mixture level. In neither case did it drop below the top of the heated core in the hot-leg break.

DR. CATTON: So if I could summarize what you said, then, for a cold-leg break pumps-off, you uncover the core; hot-leg break pumps-off, you don't uncover the core?

MR. JOHNSEN: That's correct.

So we can summarize the results in terms of cooling of the core in this fashion here --

(Slide.)

-- where we're comparing actually the difference in the transient --

DR. PLESSET: Now let's see. This is with no accumulator?

MR. JOHNSEN: That's right. That point should be made.

DR. CATTON: That's right.

DR. PLESSET: So that doesn't necessarily have significance.

DR. CATTON: If I put more water in, I'll probably be better off.

DR. PLESSET: Well, that's the idea.

DR. CATTON: Sometimes.

DR. PLESSET: But this is without accumulator addition.

DR. CATTON: That's right. But, you see, if you look at some of these curves you get the feeling that for the hot-leg break, gee, clearly the pumps off would be better. But if you ask how much coolant is available in the core, you find out that for the cold-leg break you uncover part of the core if you turn the pumps off; whereas, you don't in the hot-leg break, at least for the cases that were tested in Semiscale. So the difference is not as profound as the graphs make it look.

MR. JOHNSEN: To some extent, I think that's a valid observation. If one compares the coolant inventory for all four cases, if you will, one sees that the minimum inventory reached in three of these experiments is very, very similar.

Those three are: The two cold-leg break tests, and the hot-leg break test with the pump running. The one that stands out quite clear / as being different from the other three is the hot-leg break test with the pumps off.

On a comparative basis, that particular scenario leads to the least mass depletion in the system. The other three -- the minimum coolant inventory is fairly close, with the worst one being the cold-leg break pumps tripped early.

DR. CATTON: Right. That's right. I just wanted to bring that observation out -- or my interpretation of your results.

(Laughter.)

MR. JOHNSEN: Okay, if we then look at all of the results at one time, we can do so in this way. What we are looking at is the difference between pumps on and pumps off for the two different break locations. We are doing so by subtracting the transient coolant inventory in the pumps off case from that in the pumps on case, taking that difference and dividing it by the initial inventory in the system so as to normalize it. Then we can compare the cold-leg break and the hot-leg break cases on one single graph.

Now here you see, as we showed earlier, that in the case of the cold-leg break what happens early in the experiment is with the pumps tripped we have greater mass depletion initially, and then later in the experiment we have greater mass depletion with the pumps running.

Now the greater mass depletion with the pumps running later in the experiment is not profound enough to reverse the trend between earlier in the system, so this curve (indicating) never goes to zero. That is always positive, indicating that with the pumps tripped we have -- or I should say, with the pumps running we have more mass in the system. Just the reverse occurs in the hot-leg break case: That throughout the experiment the pumps running, there is less mass and that trend continues throughout the experiment.

(Slide.)

We can now turn to the question of the calculations themselves and how well did the code do. For all of these calculations, the RELAP4/MOD7 code was used.

First we can look at what the code calculated as the transient cooling inventory for the cold-leg break pumps off. Here we compare that calculation -- which is the dotted line -- with the actual data from that experiment. You can see that the agreement is very, very good.

DR. ACOSTA: Which one is this? Pumps off?

MR. JOHNSEN: This is pumps off, cold-leg break. You can see that the agreement is very, very good. As a matter of fact, the agreement is within the uncertainty of the measurement.

Now it turns out that the code calculated this behavior for the correct reason, as well, which is very heartening.

(Slide.)

I alluded earlier to the fact that the main reason that we have higher break flow in the pumps-off case was attributable to the subcooling of the liquid in the vicinity of the break.

DR. CATTON: You did have to do some study of nodalization in order to accomplish this, did you not?

MR. JOHNSEN: Yes. I probably should have prefaced this portion of the presentation by an explanation of what we are actually looking at here in terms of calculation.

The calculations, by and large, that we are looking at here were made in the post-test mode. They were done after the test was completed. I will show you a little later on in the presentation what our pretest calculation indicated, and wha. changes we had to make in the model to improve the calculation to bring it into greater conformance with the data. And by so doing, what we learned from this.

So as I was about to say, the reason again for the greater mass break flow in the pumps-off case was the subcooling of the liquid; and indeed, the code did calculate that very behavior. We can see that on this slide here which compares the degree of subcooling in the vicinity of the break in the pumps-off cold-leg break test. That's the solid line with the code calculation for the subcooling. You can see it was predicted very, very well. So, too, did the code calculate a greater mass depletion for the pumps running case in the hot-leg break location, and it did so for the correct reason there as well.

(Slide.)

Here we can compare -- or we can show that by comparing the density of the fluid in the hot leg for the hot-leg break case. Here we are comparing two calculations. Now there isn't any data on this slide. What we are comparing is the predicted density in the vicinity of the break with the pumps running in the hot-leg break case versus the density predicted there with the pumps tripped early.

This slide is analogous to the one I showed you earlier which only had data on it. But it does show that the code calculated a greater density than the pumps running in the vicinity of the break, which indeed was what occurred.

We can now go back and look at that slide that I showed earlier. At this time, we can compare what the code calculations showed in relation to the data.

(Slide.)

Again, we have the same curves here shown earlier. The solid curve on the upper part is the difference in the mass observed in the experiment in the cold-leg break. Overlaid with that is what the code calculated. Here we note that indeed the code did calculate, first of all, that there would be more mass depletion with the pumps tripped. Secondly, that there would be a change in the slope of this behavior; that initially we would see more mass depletion with the pumps tripped, and lesser mass depletion later in the transient with the pumps tripped over that with the pumps running.

DR. CATTON: In looking at your hot-leg calculation, it looks like you could be led to want to turn the pumps off, doesn't it? There's a big difference.

MR. JOHNSEN: Yes.

DR. CATTON: Because of the big difference in the calculation.

MR. JOHNSEN: And I do want to anecdotally add here that in the case of the hot-leg break, that this curve here (indicating) is in fact the pretest prediction; it is not the post-test calculation. That is not shown here, and as will become clear in a few moments, the post-test calculation indeed will be quite improved over this one here.

DR. CATTON: I'm trying to get things in perspective now. In the vendor calculations, is their break-flow model similar to the one that you used for your pretest prediction?

MR. JOHNSEN: I would say "no" on a general basis, but the question brings to the front several points. That is, nodalization --

DR. CATTON: Right.

MR. JOHNSEN: -- whether or not phase stratification is used, and certainly that bears on break-flow; and then the break-flow model itself.

In regards to the latter two subjects, the break-flow model itself, there are differences between what we use and what the vendors use -- most notably in the two-phase regime area. I believe all of them use a fairly consistent model in the subcooling part of the transient; but not so in the two-phase. At least the models they use are not best-estimate models as we now consider them.

With respect to the phase stratification, there were different approaches taken. Brian Sheron can probably fill you in better than I can in that regard, but I do know that Westinghouse, for example, uses the stratified representation of the node where the break existed; whereas, B&W and I believe Combustion --

DR. CATTON: But if they stratified, where did they put the break?

MR. JOHNSEN: Well, that, too, was different from the calculations.

DR. CATTON: You could get whatever you wanted.

MR. JOHNSEN: Yes.

DR. ACOSTA: Did any of these calculations show any of the instabilities you observed in your measurements for the cold-leg break, any of this pump chugging?

MR. JOHNSEN: No, none -- none of this behavior.

DR. ACOSTA: Was that because the model was deficient and could not do it?

MR. JOHNSEN: Yes, that's correct. The pump chugging behavior was not predicted by the model.

DR. ACOSTA: But if that is a system characteristic, which I think most people would say it is, why wouldn't that model, if it's a proper model, show that kind of behavior?

MR. JOHNSEN: Well, I can tell you by referring you back to what we said earlier. That is, that the pump model itself was based on homogeneous testing of the pump. Okay? So on that basis alone, it would appear to be incapable of predicting this sort of behavior.

DR. ACOSTA: Well, why? Because if the pump is chugging, if it's alternating between cold liquid flow and full vapor, that certainly was part of your homogolous curve.

MR. JOHNSEN: Yes. Okay, I can see where you're going --

DR. ACOSTA: So that would be tending to focus attention to the system model rather than a pump model.

MR. JOHNSEN: I see what you're saying --

DR. PLESSET: They don't have a --

DR. CATTON: It would never do that.

DR. PLESSET: They don't have a pump characteristic behavior of the kind they need, I think. Is that right? That is, you're forcing it to be a homogeneous flow in the pump.

MR. JOHNSEN: That's correct.

But I understand your point. It's a valid one. The model should predict -- all things being equal, the model should predict this collection of liquid in the pump trap --

DR. ACOSTA: That's right.

MR. JOHNSEN: -- and the buildup, the eventual catching of the pump.

Now the pump model comes into question because the model will have embodied in it some sort of a threshold -- or it should have embodied in it some sort of a threshold void fraction, a stratified condition at which it would catch and deliver the slug of coolant. Now to that extent, the pump is -- the model is incapable of doing that.

DR. ACOSTA: Yes, but --

MR. JOHNSEN: It has a continuous representation of head versus void fraction, which obviously is not the case in the stratified condition.

DR. CATTON: Your nodes near the inlet pump are too big. You'll never get wild variations --

MR. JOHNSEN: That's correct.

DR. CATTON: -- in a short period of time of the void fraction.

MR. JOHNSEN: That's correct.

DR. CATTON: And so you in essence have built in a homogenizer at the inlet.

MR. JOHNSEN: I think that that is true.

DR. PLESSET: Yes.

DR. ACOSTA: Are you, by your calculations, throwing away the phenomenon that you measured?

MR. JOHNSEN: I think the answer to that is largely true; that it is: We are doing that.

DR. ACOSTA: There is one other aspect that might be mentioned here. That is, all of the pump performance that you have mentioned is steady-state performance.

MR. JOHNSEN: That's correct.

DR. ACOSTA: And here the pump is operating in a highly transient mode. Whether or not those operations can be considered equivalent steady-state sequences is something that one would want to think about.

MR. JOHNSEN: Yes. I think that is an important point. All the data is indeed in a steady-state condition. So inertial effects in the pump, for example, are not considered.

DR. WU: Nothing in there?

DR. CATTON: Oh, you don't have a pump inertial model in there?

MR. JOHNSEN: Oh, yes; true. But I am saying that arriving at the data and producing the data which then is fed into the model --

DR. CATTON: Okay.

MR. JOHNSEN: -- inertial effects were not considered. However, the model itself does include inertial effects. The pump inertia itself is factored in.

DR. ACOSTA: But not unsteady behavior.

MR. JOHNSEN: No.

DR. ACOSTA: And it's all quasi-steady.

MR. JOHNSEN: Although in these tests, the pump speed is relatively constant.

(Slide.)

MR. JOHNSEN: Okay, now I would like to touch on a few points in the context of what it is we have learned from looking at and doing these calculations in both the pre- and the post-test manner.

There were three specific things which were mentioned in NUREG-0623 as being of concern in terms of the believability of the vendor calculations. Probably the most significant point is the two-phase pump performance, which we have talked about extensively this morning.

The second is the question of phase separation. How important is it to model phase separation, especially in the pumps-on condition, in order to produce a good calculation? There were different modeling assumptions made by the vendors with respect to phase separation and the degree of homogeneity of the two phases.

Another question that was addressed in NUREG-0623 is: What is the importance of the assumption of phase thermal equilibrium, which all of the codes imply, the vendor codes.

First, looking at --

DR. CATTON: Even for the cold-leg breaks? They would not be able to properly account for the subcooled effect that you found?

MR. JOHNSEN: No, they can because during the period of time when we see this subcooling, the pipe is liquid full, in which case the homogeneous code is fully capable of predicting that.

DR. CATTON: Sure.

(Slide.)

MR. JOHNSEN: In the case of the pump performance itself, we can look at what the effect was on our calculations of modifying the pump head degradation behavior.

Now the pretest calculation, as I said earlier, utilized the old pump model -- I call it the "old pump model" now -- which was based on the Westinghouse-Canada data, which again was homogeneously derived information. When we looked at our test results, we saw quite clearly that in fact the pump had degraded at an earlier void fraction than we predicted with our model.

We attribute this, at the present time, to the effect that I mentioned earlier. That is, the difference between the homogeneous inlet condition and the stratified condition.

Now in an old pump model, we predicted that severe head degradation would begin at a void fraction of about twenty -- a .2 and 20 percent void fraction. But what we observed in the experiment was that the head degradation began at a much lower void fraction, in the vicinity of about 5 percent of the calculation percent.

When we took the test data -- That is, when we took the head versus void fraction characteristics and extracted that from the experimental results and used it to modify our head degradation multipliers and factored that fact into the model and reran the calculation, we indeed got a much different result with respect to the effect of the pumps running in the cold-leg break case. And that is what is illustrated here on this slide, where we are showing the comparison of the pre-test calculation in terms of coolant inventory, the post-test calculation in the middle here (indicating), and the after-the-test data.

Now in terms of end point, the result was indeed significant. So we can see that the sensitivity to the degradation was evident in our system. How one modeled the pump was very significant because in the case of the pre-test prediction, if one compared the pre-test predictions with the cold-leg breaks case pumps-on/pumps-off, one did see this cross-over that Brian Sheron showed earlier that he calculated for the PWR. We did predict that cross-over.

Once we improved, if you will, the pump model, we no longer saw the cross-over; that for the duration of the transient, pumps-off always had a lower cooling inventory than the pumps running.

(Slide.)

Now in terms of the question of the homogeneous representation of the two phases in the spatial sense, here is some data from our pumps-on cold-leg break test, which I think illustrates the point that any model that is going to be used to predict this kind of behavior -- that is, a pumps-running condition -- must account for a separation of the phases; that a purely homogeneous representation which would not allow a slip between the phases would be an inadequate approach to looking at this particular situation.

The way I am illustrating this is by comparing fluid density in the downcomer at two elevations. Again, this is the cold-leg break with the pumps running. The upper curve shows the density reading about midway down the downcomer, and it indicates that throughout this period of the test -- the first 1000 seconds -- that location was full of liquid, pumps running.

However, the second curve here shown is the fluid density very near the top. Actually, about 72 centimeters below the cold leg. It shows significant voiding -- again, this is with the pumps running. And by the time one reaches about 700 seconds into this transient, we see almost pure steam at that location, again with the pumps running.

So this sort of a tremendous difference in fluid densities which is indicative of stratification of the liquid and vapor-phase points could not really readily be calculated by a true homogeneous calculation. One would have to account for phase separation in two phases with the pump running.

Now the third point I brought up was the question of the equilibrium --

DR. PLESSET: There was this question Brian brought up about the depression of the level in the downcomer, and I presume you examined this too, in your tests?

MR. JOHNSEN: Yes.

Now in our case, in our test, what we found was that the downcomer level was never depressed to the bottom of the downcomer; that indeed there always was a finite collapsed liquid level in the downcomer while the pumps were running.

ıalytically we also looked at that question. We said: What would be the effect, or what is in fact the effect of changing the pump degradation on that depression of the downcomer? What we found is that in our pretest prediction we had indeed driven the level all the way to the bottom of the downcomer. That was with the old pump model which produced less degradation than the new pump level did.

When we reran that calculation in the post-test sense, we found that indeed with a more degraded pump we would not press that level down to the bottom of the downcomer.

DR. CATTON: Do you have an equivalent of the downcomer bypass to the upper plenum kind of --

MR. JOHNSEN: Yes, we do.

DR. CATTON: Did you -- at least the calculations, vary the resistance to see --

MR. JOHNSEN: We did not vary it. We merely included that bypass and modeled it based on our knowledge of the resistance of the flow path, which was quite good because it is an intentionally created path. It's not an accidental one.

DR. CATTON: In fact, it seems with your analysis capability you could take a look at how large is "large" when it comes to bypass.

MR. JOHNSEN: In our case, it amounts to 4 percent at steady-state, between the top of the downcomer and the upper head.

DR. CATTON: That's what you have in Semiscale.

MR. JOHNSEN: That's correct.

DR. CATTON: Well, what happens if it were only 2 percent? Or if it were 8 percent?

MR. JOHNSEN: We have not specifically addressed that sensitivity.

DR. CATTON: I think that's relevant for LOFT.

MR. JOHNSEN: Although I can tell you this: That we did perform a calculation with the pumps running. For that particular case, I can tell you what happens, with the pumps running; we took the bypass out completely. What we found was, when we take the bypass out completely, we have more mass depletion with the pumps running in the cold-leg break case, simply because we have created now a short-circuit for the vessel, and we indeed deplete the cold leg, which is the source of the break, more readily than if there is no pipe bypass at all. So there is a sensitivity there; there's no question about it. How great it is I think is probably a function of the system involved.

The third thing I mentioned was the question of phase thermal equilibrium. What we found in our experiments was that -- or our calculations, was that the assumption of thermal equilibrium between the phases was indeed inadequate.

Now I caution you to reconsider the fact that we did not have accumulator injections in these experiments. So had we had accumulator injection, we would have shown that the phase thermal equilibrium assumption falls apart once the accumulator issue begins. But taken in the context of these tests, that is not a significant point. So once the accumulator comes on, the inventory would be restored.

DR. ACOSTA: Could we back up just to the first line here, "two-phase pump performance as a modeling issue"? Would you say again why it is a modeling issue? I mean, you stated that you needed more pump degradation than your pump tests showed, but when you say "modeling" here, it implies to me that there are size and speed changes for similar models; but does it really follow that that's the issue? Or that you have a system that is different here that caused the pump to be different than it would have been in the original test for which your degradation data was taken?

And if that is the case, then it is not a modeling question so much as it is an installation question.

DR. PLESSET: A what question?

DR. ACOSTA: Two-phase pump performance modeling --

DR. PLESSET: Just the one word.

DR. ACOSTA: "Installation."

DR. PLESSET: Oh, "installation."

DR. ACOSTA: Rather than a basic question of modeling the fluid mechanics, I think it is an issue that we should have clear at this stage.

MR. JOHNSEN: Well, indeed we did not calculate, as I said earlier, the chugging here. In the case of the calculations that we did, failure to predict that chugging behavior did not lead to this prediction of the old-world trend that resulted in the experiments.

DR. ACOSTA: Yes. That part is understood.

MR. JOHNSEN: What I am trying to bring across here in making this point is simply that what we found in the Semiscale is that the results of comparisons, calculated comparisons between pumps-on and pumps-off is very sensitive to what one assumes is the correct degradation of the pump. and really no more than that, is what I really wanted to bring across.

DR. ACOSTA: Okay. I just wanted to make the point that it doesn't necessarily mean that your pump performance are size or speed dependent, so much as it is installation dependent. So the modeling issue may not be the pump so much as it is the system in which the pump is placed.

MR. JOHNSEN: Well, maybe I could summarize it in this way: That is, analyst know thy pump.

DR. ACOSTA: Okay.

MR. JOHNSEN: That would include the question of the installation, the point that was brought up earlier about how the tests are run to characterize the pump. I think the point we're trying to make here is that if a vendor comes in and says, "this is how my pump behaves," then one should immediately say: Well, how was the test run in relation to the way the pump is actually installed?

DR. ACOSTA: Well, true, you should do that. But more specifically, is such a test made on a pump at a certain power level -- let me pick a number -- say 100 horsepower, sufficient to characterize the two-phase flow performance with the same inlet setup and so on of let's say a pump that will be operating at 5000 horsepower? That is what I would call the pump modeling question. And that is the question which I don't think your modeling issue is really addressing.

MR. JOHNSEN: No, no, no. When I use the word "modeling" here, I refer only to the analytical modeling of the physical pump, as opposed to the scale question which is really a separate question.

DR. CATTON: Have you taken a thorough look at your modeling of the inlet to make sure you have the proper number of nodes to allow for phase separation, and so forth?

MR. JOHNSEN: Yes.

DR. CATTON: And you don't get slugging when you do that?

MR. JOHNSEN: No. We have not specifically done a detailed nodalization study such as would require looking at expanding a great number of a different number of nodes at the pump. We have not done that.

DR. CATTON: I would be concerned if you didn't have the nodes at the pump inlet proper that you would wind up having nodalization, in essence, homogenizing the flow at the pump inlet, and you wouldn't get the slugging.

MR. JOHNSEN: We did -- I think the important point, though, to note is that we did utilize a phase separation model in the pump inlet. I can show you that --

DR. CATTON: You still have to be careful about the nodalization. You can still lose it.

MR. JOHNSEN: I agree. I agree. But the point I guess I'm trying to make is that one could say: Well, all right, I'm going to use a very large number of homogeneous nodes in the phase slip model which will allow the two phases to separate as they desire at the inlet to the pump section. Or, one could say: Well, perhaps it is adequate to represent that entire leg as one node, but to employ a bubble-rise model, a phase separation model from that node. Those are two different approaches to modeling the same --

DR. CATTON: And I surely don't know which one is correct, but your period of slugging is fairly long; it's 15 to 30 seconds --

MR. JOHNSEN: It is; very long.

DR. CATTON: And I know your time steps must be on the order of hundreds of seconds when you chomp through there.

MR. JOHNSEN: What did you say on time steps?

DR. CATTON: In doing your calculations, your time steps are much shorter in comparison.

MR. JOHNSEN: Oh, yes; absolutely.

DR. CATTON: So I would expect you should predict the slugging if you'd done the nodalization properly.

MR. JOHNSEN: Yes. I wouldn't suppose to tell you that the code calculated every feature of the experiment. Indeed, it did not. What I am merely trying to demonstrate to you here is that, on a qualitative basis, there was agreement.

We didn't have to stand on our heads to get it.

DR. CATTON: I understand.

DR. ZUDANS: One correction. You said that when you made the corrections for the pump degradation, what is that physical explanation? And why did the pump degradation have to be increased as compared to actual test of the pump for two-phase flow?

MR. JOHNSEN: We believe that the reason we had to model the pump with a greater degree of degradation, head degradation as a function of void fraction, is primarily because of the difference in the flow topology at the inlet to the pump in the tests that we ran versus the tests that were used to characterize the pump in the first place.

DR. ZUDANS: Okay, so in a way what you really did, you accounted for some stratification for some transient behavior.

MR. JOHNSEN: Yes.

DR. ZUDANS: If you used the same pump characterization for another test, it would not perform.

MR. JOHNSEN: Well, we have demonstrated in the past that the so-called "old pump model" -- which again is based on homogeneous conditions -- does a very adequate job in fast transients where there is very little phase separation. The head is predicted very well.

DR. ZUDANS: So that means there's nothing wrong with the pump characteristics.

MR. JOHNSEN: Which leads us to the conclusion I mentioned a minute ago, that stratification is the primary culprit.

DR. ZUDANS: That's right.

MR. JOHNSEN: We can then move on to some conclusions based on these experiments and analyses --

DR. PLESSET: He wants to make a comment at this point. Would you identify yourself again?

MR. QUAPP: Bill Quapp, EG&G.

Before Gary gets off the question of pumps, I would just like to add that his comments on pump model and pump applicability for Semiscale should be taken, I think, as applicable to Semiscale.

We have done some additional work looking at some pump behavior in one of the single-phase loops by injection of nitrogen to simulate a void faction, and the sugar cells by densitometer measurements at the fluid at the inlet of the pump was homogeneous.

We then did some data comparisons to get a -- now the Semiscale pump, I forget how many hundreds, or tens of -- it's a small pump by comparison to this other one which was like 500 horsepower. We related this to the EPRI data on their little bit that was published on their pump, which was also of a similar size, and those two had very similar specific speeds and these larger pumps degraded. They were supposedly more representative of reactor coolant pumps than the Semiscale purported to be, and they degraded much slower.

So the point I would like to make here before we -- that I think is relevant to make -- is that I think that the knowledge of the two-phase pump performance, in particular in sizes and design characteristics similar to reactor coolant pumps, is still a fairly major missing area in the public domain of data that can be used in publically available codes.

Now all of the vendors run off and do things in secret and claim they know everything about it, but those of you who have to make judgments in the public domain are relatively limited on data bases on large pumps in geometrically prototypic conditions with, as Gary says, flow topology at the inlet that is representative of the kind of transients that we are discussing.

DR. PLESSET: Thank you.

MR. JOHNSEN: We approach the conclusions, then. We can certainly see that in Semiscale there is definite influence on small-break behavior caused by running the primary coolant pumps as opposed to tripping them early. The opposite effect occurs in the hot and cold leg cases we've seen.

(Slide.)

In addition, we can also see that the continuous operation of the primary coolant pumps tends to deliver more coolant from the cold to the hot portions of the system, which is especially relevant in terms of predicting the coolability of the core in those two different scenarios.

(Slide.)

We have also seen that the RELAP4 code, at least on a qualitative basis, can be made to predict the correct behavior, and really with very little modification, from the pretest predictions that were made.

Furthermore, that our analyses with the code show quite readily that in the case of Semiscale that the case of pump degradation is a significant one. It is an important aspect of the analytical model in terms of predicting the correct behavior.

Secondly, that the data alone tells us that the use of a purely homogeneous model is probably inappropriate for a pumps-on calculation. I might add, in a parenthetic way, that the analytical model we used for the pumps-on and the pumps-off was identical. We made no changes in nodalization, phase separation assumptions, or any other aspect of the model other than the fact that the pumps were on in one case and they were off in the other.

Then on a general basis, we can also say that the results in Semiscale, which are by no means purported to be typical of a PWR, but in any event, in Semiscale we see that the results tend to suggest that the influence of primary coolant pump operation indeed may be sensitive to what assumptions were made with respect to where the break is and where the ECC injection is. These are points that Larry Leach mentioned earlier -- that there may be not an unambiguous answer to the question of should one trip the pumps or run the pumps -- but, in any event, the data that we have provided here is intended primarily to provide some guidance as to the modeling, what is the important basis for modeling this sort of behavior.

And, that taken together with the LOFT results which will be discussed later on today, a more robust data base exists from which to determine what is the ideal code, what is the ideal modeling philosophy that should be applied to answer the question for a PWR.

DR. CATTON: Could you put the previous slide back on?

(Slide.)

I guess I am one of the ones that have been critical of comparison of Semiscale with PWRs, and I am going to ignore my own criticism. I think this is a little bit incomplete. Your conclusion of less mass depletion for the cold-leg break should also indicate that you had core uncovery for the cold-leg break with pumps off; whereas, the greater mass depletion for the hot-leg break did not lead to core uncovery with the pumps off.

MR. JOHNSEN: Yes.

DR. CATTON: Okay, so if one were to make a conclusion based strictly on the very limited experiments and the very nontypical system that you ran, you would have to conclude that pumps-on would be the way to go.

MR. JOHNSEN: Yes, but I -- rather than --

DR. CATTON: I just wanted to cast doubt at the present, not to conclude anything.

(Laughter.)

MR. JOHNSEN: If I overemphasize the fact that indeed we uncovered the core with the pumps tripped and we did not with the pumps running in the cold-leg break case -- if I were to overemphasize that, I may be implying that, gee whiz, take that and run with it --

DR. CATTON: Oh, I don't want you to imply that.

MR. JOHNSEN: -- and go tell your operator what to do.

DR. CATTON: I don't want you to do that.

MR. JOHNSEN: And we in no way want to imply that. The primary reason for running these experiments is to gain an understanding of what happens that can be predicted.

DR. CATTON: I understand that.

DR. PLESSET: I think we have to understand that one of the big and important uses of Semiscale is code assessment, and not to tell you how to run a PWR. I don't think that you would want to say that. I think that's just what you were emphasizing.

MR. JOHNSEN: Yes.

DR. CATTON: He was running backwards.

DR. PLESSET: No, no. Well, you didn't want us to run back so fast.

DR. CATTON: That's right.

DR. PLESSET: Harold?

DR. SULLIVAN: Dr. Plesset, we realized when we started these experiments that there were atypicalities in the Semiscale facility, and we tried to address those. One of the reasons that we did not have accumulator injection -- and if we had, this trend may not hold that Dr. Catton is talking about -- you have to keep in mind that we ran these tests to get some test data to address the codes, as the ACRS has suggested on many occasions, and we fully agree with that.

It is a great temptation to look at the results and to try to translate that into what you ought to do for a PWR. And we certainly support that you should not do that. And we ran the tests in such a way that they are atypical, too, compared to what would have happened, or even trying to further simulate a PWR. And the primary reason for doing that was to obtain a better data set; that we thought that after we got through running the experiments that the data would be better.

So we tried to optimize the test to get the data, and not to try to exactly duplicate the conditions that might occur in a PWR.

DR. CATTON: I'm just trying to bring some balance to bear. We have been hearing about results of codes that some of us don't believe that are pointing to a -- or coming to a rather strong conclusion about what one should do.

Now there is an experiment -- and granted this experiment is not in any means a representation of the system that the codes have been using. The conclusion is different.

MR. JOHNSEN: Yes.

DR. CATTON: So my own feeling is, I don't know where we're at. And I don't really believe the codes that much.

MR. JOHNSEN: I think where we're at right now is we have some data from Semiscale; we have learned what we have to do to model what happens in Semiscale; we can examine if in fact those things we had to do were embodied in the vendor calculations. I have already suggested to you one area where clearly the vendor's calculation is inappropriate -- the use of a totally homogeneous model which one of the vendors employed.

So we have learned something from that. We have learned thatthe subcooling is a significant thing -- at least in Semiscale. One should attempt to allow the model to calculate that with proper nodalization.

DR. CATTON: I am looking forward to the next round of calculations and, gee, I would really like to hear the vendors' input.

DR. PLESSET: Well, Larry Leach?

MR. LEACH: Just one brief alternate interpretation of this, maybe to add to the confusion. We didn't show you the temperature data, but the temperature never increased above about 1600°F.; even though the core was partially uncovered, there was adequate cooling.

So another interpretation, if you wanted to go for the data, is that it really doesn't matter.

DR. PLESSET: Well, this is a good point, Larry.

DR. CATTON: Well, you shot that full of steam, so as long as you've got some water in the core you're going to be boiling. You're going to steam-cool those --

MR. LEACH: Sure.

DR. CATTON: If you shut that off, that's different.

DR. PLESSET: But if it turns out that way, as you said it might, this is of some great help to people like Brian Sheron, because then he doesn't need to tell these fellows to have an immediate pump trip; they can do it at leisure, and they can figure out: Do they have a break? Or do they not have a break?

And if they have a few minutes to do this and it doesn't matter, I think that's important. And this is what Brian would eventually like to get: That he doesn't need to worry about this terrible window that the vendors have come up with, and they can take their time about taking some impetuous actions, or nonaction, if you like.

Maybe that's what will happen. You wouldn't mind that, would you?

DR. SHERON: No. I think, hopefully, you will hear something to that effect this afternoon on how we take all this. I would just point out that I think it is significant, after Dr. Catton says he doesn't know where he's at, this is one case -- I think probably the first case -- where we intend to take a result from Semiscale, take it to LOFT, and to then apply it to a specific licensing decision; where we are actually going to take these results and carry them right through from the Semiscale, to the LOFT, to the big PWR with just the codes today that we have.

So I think that we are putting a very strong reliance on the codes' capability to predict this type of phenomena. And I agree that we certainly are not locking at Semiscale and running off and saying, obviously; it's wonderful to keep the pumps running because it's good for you; Semiscale says that all the calculations that were done previously for PWRs are wrong and hogwash -- That is certainly not the conclusion.

DR. PLESSET: Yes.

DR. ZUDANS: With this calculational model that you now have generated and tuned up to Semiscale, have you attempted to analyze a PWR to see whether it changes the window and all that thing? Or do you plan to wait until LOFT is completed until you do a final tuneup, so to speak?

MR. JOHNSEN: We have not done what you just asked.

DR. PLESSET: Well, I think Brian kind of outlined the program --

DR. ZUDANS: But why wait that long? Why not use this model that seems to be able to predict a complicated --

DR. PLESSET: Well, it's not going to be too long.

DR. SHERON: There are other factors which influence this window. Okay? I have written a short, internal memo on the subject with regard to the accumulator injection. All the vendors calculate that the window is bounded at least on the large-break spectrum by the accumulators coming on and immediately turning the temperature around.

As Gary pointed out, these tests were not designed to look at the nonequilibrium effect of the accumulator injection. We already know that there is a big, open question in this whole area. And in fact, that most of the vendors, we found out that we can inject as much or as little water as we want just by changing the size of the volume into which we inject the code.

A lot of the vendors inject into the downcomer. I believe LOFT is going to inject into the downcomer, for a different reason, but -- So I think you have to keep in mind that the thing that affects this window is not just issues addressed in this set of tests.

DR. ZUDANS: No, I --

DR. SHERON: And in fact, that the accumulators inject a lot slower -- which I believe they do -- in this rapid self-feeding where you put a little bit of water in, you get rapid condensation, it lowers the pressure locally and sucks water in; that if you do have a nonequilibrium, you inject slower and the temperature transient does not come up and immediately drop like a shot, like the vendors predict, but eventually; it's a slower turnaround. And if you're sitting up above say 1800°F. or so, you have circ water reaction taking place which is a significant contributor to the clad temperature, to the heatup, and if the turnaround is slower than this rapid-shot quench which the vendors calculate, that you would extend this heatup period and you could probably extend this window to some unknown degree just by that alone.

DR. ZUDANS: Yes.

DR. SHERON: We also don't know the window very well because we haven't fine-tuned the break size. The vendors only did calculations at specific intervals, and we've already found out that Westinghouse, for example, in extrapolating their data showed that the window supported a pump trip of ten minutes into the event. And in fact if you try to extrapolate back by break size, you can show that there may be a break size that they didn't specifically analyze that one can interpolate a pump trip time of something on the order of five minutes. Okay?

Granted, now, you're saying I have to get that very exact break size to produce a problem, but there's a whole host of other items which can really make you wonder that this envelope or this window in which one has to trip the pumps may have a very, very large uncertainty on it today.

DR. PLESSET: Well, I was going to try to encourage Dr. Zudans to be a little patient, because we're not going to have to wait a long time to get that.

DR. ZUDANS: No, I don't think -- the point didn't come across. I understood from the presentation that you just gave that the main reason for the disappearance of this window was in fact the pump degradation.

MR. JOHNSEN: You mean from the pre- and post-calculations?

DR. ZUDANS: Yes, and that was the sole reason for the window to disappear. My question is: If that is the case --

DR. PLESSET: I would say that's an oversimplification.

DR. ZUDANS: He can answer if that is the reason or not.

DR. PLESSET: Is it an oversimplification to say that?

MR. JOHNSEN: It is, because this so-called "window" that Brian mentioned really doesn't exist in Semiscale, because if one looks at the results of the cold-leg break tests, one sees that in neither case -- pumps-on or pumps-off -- would dangerous temperatures have resulted at the point at which the accumulator would have come [illegible] had we indeed allowed it to come on.

DR. ZUDANS: Those are test results, not the analysis results?

MR. JOHNSEN: The analysis also in a pre-test fashion did not predict a window, or it did not predict, I should say, the occurrence of a dangerous situation should the pumps be tripped later in the transient.

DR. ZUDANS: But if a cross-over occurs --

MR. JOHNSEN: A cross-over in inventory did occur in the pre-test; it didn't occur in the experiment.

DR. ZUDANS: Okay. And the only reason why the cross-over did not occur in post-test is because the curve was further degraded, the pump performance.

MR. JOHNSEN: Exactly.

DR. ZUDANS: That's the point I want to make: That if that's the case and you would run with the same model now a power plant and you find no cross-over there, too, then you would have answered at least some questions, not all.

DR. PLESSET: No, I don't think you would accept that kind of treatment for answering a question --

DR. ZUDANS: Not the whole question; part of the question.

DR. PLESSET: Yes, Harold?

MR. SULLIVAN: We believe that there are several issues. Some of them have been addressed by Semiscale, and some of them have not. The pump degradation model I indicated was one of the issues that we were trying address outside of our two experimental programs that we have going.

We believe that it's appropriate to continue down the process that we're doing. Like you indicated, it is not an extremely long process. We believe that we will be misled the least by going down this process and trying to resolve the issues in somewhat of an orderly fashion.

DR. ZUDANS: I didn't suggest to resolve the whole issue. I just wanted to resolve one point.

DR. PLESSET: He didn't want to introduce too much disorder --

(Laughter.)

DR. ZUDANS: Just a little.

DR. PLESSET: Just a little.

Well, thank you very much. I think that was a very helpful presentation, and we've got to keep our perspective on Semiscale and not say it's a PWR, and I think we know that.

We'll go on to the next item on the agenda, "LOFT L3-5 Small Break Pumps Off Test Results and Analysis."

(Pause.)

DR. PLESSET: Mr. Lienbarger is not going to make the presentation? Is that it?

MR. CONDIE: I'm sorry. I thought you were informed on that agenda change.

DR. PLESSET: That's all right.

MR. CONDIE: Yes. It is the same presentation.

DR. PLESSET: I just wanted to see him; that's all.

(Laughter.)

MR. CONDIE: He just got up and left.

DR. PLESSET: Oh.

(Laughter.)

MR. CONDIE: He had been here. He is going to be making a presentation tomorrow on the additional small-break tests that we've run in LOFT. I will limit my discussion today to the L3-5/L3-5A test that was completed approximately three weeks ago, and give you some of our initial results and analysis that we've completed.

(Slide.)

The L3-5A is one of a pair of tests that we will -- have been and will be conducting in the LOFT facility to address the question of pump operation during a small-break accident.

I will discuss our test objectives in L3-5 and L3-5A, which is a carryon to L3-5. I will talk about the system configuration for this test; the test scenario and event sequence that occurred. I will discuss the cooling mechanisms during the course of the transient, and the mass distribution and inventory during the L3-5 portion of that transient. I will address briefly our experimental predictions or pretest predictions for this test for both the RELAP5 calculations that we did here at EG&G, and the TRAC calculations that were performed at Los Alamos; and some of the conclusions.

(Slide.)

The objectives for this test, L3-5/L3-5A, we have two objectives, one for each of the phases of the experiment.

L3-5 is a small-break test in conjunction with future test L3-6, which as indicated will be run the first part of December, to evaluate the system effects of primary coolant pump operation during a small-break LOCA.

We continued on with experiment L3-5A to evaluate the plant recovery by isolating the break and reestablishing the steam generator as a heat sink in the natural convection mode. I will discuss some of those cooling mechanisms here, and primarily look at mass distribution in the early part.

(Slide.)

The LOFT system -- this shows an isometric -- was configured differently for the L3-5 test than any of our other small-break or large-break tests, for that matter. The previous small-break tests were run by blowing down the broken cold leg, which is this loop here (indicating). The orifice would have been here (indicating) in the cold leg, and the quick opening blowdown valve in the suppression tank.

In this experiment, we moved the break location to the intact cold leg and have a line that comes off the intact cold leg and a blowdown orifice about six feet away from this pipe, with piping then that comes back into the suppression tank as it was here (indicating).

The reason for this was based on the calculations that the vendors submitted, and the fact that it would produce the most difference between the pumps-on and the pumps-off test. And it did, in our calculations.

Now we indicate an ECC injection location here (indicating). There is an injection location, and we introduce HPIS there in the second part of the experiment. But during the L3-5 experiment where we will be comparing to the pumps-on/pumps-off, the HPIS was injected into the downcomer in that region approximately opposite to this cold-leg penetration to the vessel.

As in Semiscale, we did not use accumulator injection. We do have one high-pressure injection train. As I mentioned, it is introduced into the downcomer.

(Slide.)

The configuration of the orifice is shown in this slide. This is the intact cold leg, and the piping that I indicated comes off the side of that pipe at the center line and about six feet down here is the orifice that represents the pipe size. It is configured on a scaled basis to represent a four-inch break in a large PWR, or approximately 2-1/2 percent break.

Downstream of that orifice, we have several measurement devices intended to determine the flow out of the break -- a flow homogenizer, which sets up the flow for us to measure with a drag stream and a turbine meter in that region. We also collect the fluid into the suppression tank and can measure the levels there to give you a measurement of the mass leaving the system.

(Slide.)

I will now discuss just the sequence of events associated with this test. The slide shown here only shows the first 180 seconds on a transient that was probably -- that was about 12,000 seconds long. I will show you the rest later. A lot happened in the early part of this experiment, to help you familiarize with it.

This is the primary system pressure as a function of time, and it characterizes what you might say is the signature of this experiment.

The plant was scrammed prior to opening the break -- in this case, approximately 5 seconds before the plant was scrammed manually, which tripped the main steam valve closure and the feedwater was tripped at that time -- the feedwater to the second area.

Once it was confirmed that the control rods were at the bottom, then the break was initiated manually and that is what we define as "time zero," where we initiated the break.

The pump trip occurred at that point also. We are looking, then -- to make sure you realize -- at the pumps-off condition. The plant pressurized very rapidly, and at about 1900 psi, or about 5 seconds later, the HPIS system was initiated.

The system depressurization continued -- this (indicating) being the curve caused by a generation of vapor in the hot leg and upper plenum. In fact, by 22 seconds the pressurizer was empty and the pressure control was maintained by the void in the upper plenum in the hot leg.

(Slide.)

This shows the same scenario, only carried out to the 12,000 seconds in the transient. The pressure continued to drop until about 750 seconds. The primary pressure was actually less than the secondary pressure, and the heat transfer then to the steam generator was ceased at this point. Depressurization continued, then, until we reached the low pressure of 300 psig, a predetermined point at which the break was isolated and the HPIS was turned off.

By definition, this point of break isolation is the termination of test L3-5 and the initiation of L3-5A. With the isolation of the break, the pressure in the primary system continued -- or started to rise. At this point where you see a change in slope (indicating), is a point where the primary system has now reached the same pressure as the secondary system and allowed heat transfer to the secondary system, causing that to slant.

At 5000 seconds, or about 30 minutes after this period when we started steam generator cooling again, the secondary system was controlled by a feed-and-bleed operation in which we controlled the temperature. We bled steam from the secondary system at a predefined temperature or pressure rate.

This (indicating) indicates the pri ary system following that secondary system based on that pressure descent. At about 6000 seconds, we turned HPIS on, and the system continued to depressurize until about 11,000 seconds the system was actually subcooled, which was the criteria for terminating the experiment.

(Slide.)

I indicated in this slide a relative difference between the primary and secondary system. It is shown better in this slide. The secondary system is indicated by the numeral "2" and the primary by "1."

As I indicated, at about 750 seconds the primary system pressure dropped below the secondary, eliminating the steam generator as a heat sink; and from that point until it reestablished out here at 3800 seconds, that entire decay heat in this region (indicating) then was removed from mass in energy exiting the break.

And then at this point in time (indicating), of course the break was closed and the entire decay heat then was removed from the steam generator for the rest of the transient.

So that the test does give us a good test to compare, then, to the pumps-on tests that we'll run later, and it is a good test of heat transfer/heat removal mechanisms, as well as mass inventory and different natural circulation mechanisms.

(Slide.)

I will discuss now the distribution of the mass in the system. We have in LOFT several gamma densitometers which we can determine the densities at various points in the system. This is the hot leg densities. We had a three-beam gamma densitometer located in that hot-leg piping as it leaves the reactor vessel. The "B" beam is in the middle and it crosses the pipe at about 45 degrees. It goes right through the center. The "A" beam is on the bottom, and it's about 15 degrees below that. And the "C" beam is on top of that at about 22 degrees.

So from these three beams it is possible to determine the flow stratification that exists in the pipe as a function of time.

This, as I indicated, is the hot leg. We see the top beam density dropping very rapidly in depressurization. That is caused by the voiding or flashing in the hot leg, and only takes part of the top of the pipe. Not until the actual liquid level of the upper plenum begins to drop do we see the entire hot leg void; but by about 700 seconds here, you see from these measurements that the hot leg is voided virtually completely through most of the transient until we start out here after HPIS is on and the plant fills up.

(Slide.)

The cold leg, I have the same plots for that. It is shown on this slide. Again, although later, but still the same effect on the top of the pipe as the fluid reaches saturation temperature and we get some flashing in the top of the pipe. But note, as you go down in the pipe farther that there's a definite stratification that exists across that cold leg pipe until such time as the break is isolated at about 2200 seconds. Then you see the collapse of the higher density liquid in the lower part of the pipe.

Now this is caused by -- near this location, we're bringing in fluid from both directions, and some of it coming from the vessel itself. And once that break is isolated, then there's no longer that ability to bring that colder fluid from the reactor vessel, and it goes up through the core.

Now the cold leg voids out at this time. Later, again as HPIS comes on, it fills up at a different rate than does the hot leg and shows much more stratification than we see in the hot leg.

(Slide.)

This slide shows the mixture level in the core. By that -- this measurement is taken by probes that sense the existence of moisture. So this does not mean that we have complete liquid, solid liquid from the bottom of the reactor vessel clear to this (indicating) location. It does mean that there is liquid there, and that it can cool.

The nozzle center line is indicated at zero, with these elevations in feet above the core. The top of the core is indicated here (indicating), approximately seven feet below the nozzle, to give you a perspective.

The dotted line shown here (indicating) indicates that the liquid level may have been higher, but this particular probe was failed. I have implied here that the liquid level did not drop below these levels, when in fact it did.

This is the last probe we have from thermocouple measurements in the core -- or in the upper plenum, excuse me. We indicate that in this time frame here, about 2000 seconds, that the liquid level, mixture level dropped to about 2 feet above the core. So it did drop another foot or so below what was indicated here (indicating).

This test shows that even, then, with our pumps off that at no time did we uncover the core in this experiment. It does show that we have a pretty good handle on what our mass inventory is in the reactor vessel itself, and we're using some of these same mechanisms. Our analysis is continuing to quantify the distribution of the mass in other components throughout the system.

DR. CATTON: How are you going to define the existence of a window?

MR. CONDIE: The existence of the window can only be defined by comparing this test to another test, to the pumps-on test.

DR. CATTON: But in the pumps-on test, if there is a window, the liquid level will drop way down into the core and you'll get very high temperatures.

MR. CONDIE: That's correct.

DR. CATTON: And you're going to subject LOFT to those circumstances?

MR. CONDIE: The next test is L3-6, that's right.

You know, we really can't talk about trends or the effects of the pumps on with this one test.

DR. CATTON: I understand.

MR. CONDIE: Our purpose here is to show you our ability to determine what the mass inventory is and location for this test, intending to do the same for the other, and compare those inventories.

(Slide.)

This slide shows the mass inventory as a function of time. Note the change in scale here to 2000 seconds. So this is just until the break was isolated. After that time of course inventory is constant.

We drop down to about 60 percent of the original mass inventory at the minimum point in this experiment. That is really not too much mass lost from the system. In fact, the -- however, we did drop the level in the reactor vessel considerably.

The comparison of that level with the amount of mass that was lost from the system indicates that quite a distribution -- a distribution of quite a bit of liquid in other parts of the system. So there is liquid in the loop seal, the steam generator, and what have you, and our analysis is continuing to quantify where this mass is throughout that transient.

DR. PLESSET: I would like to follow up this question that Dr. Catton just posed about this next test with the pumps running, what you will do. I mean, suppose you find that the level is approaching the top of the core in the test, what will you do? Continue the test?

MR. CONDIE: We have designated the next test, L6-5, and we also have an additional follow-on test tacked on to that one that we will purposely try to bring the level below the top of the core and investigate other cooling mechanisms as a follow-on to that test -- cooling mechanisms with the core uncovered will be the most severe transient we've looked at in LOFT at this time.

DR. PLESSET: So that will not be the next one.

MR. CONDIE: In conjunction with L3-6. It is the same test. Like I indicated here, we have L3-5 and L3-5A. Our next test, the first part is to look at exactly the same conditions here as we had in -- but with the pumps on, and we fully expect to continue on and decrease the level in the core.

DR. PLESSET: I think that Larry Leach wants to make a comment.

MR. LEACH: Yes. I would just like to make two points, if I could.

One, Dr. Catton mentioned the core heating up with the pumps running. I think it is important to point out that nobody predicts that to happen. It's only if you get the case of running the pump out to the low void and then the pump were to trip that they could get the heatup. So we wouldn't expect that to occur even in a PWR. Furthermore, it's the LOFT calculations don't show that.

But if you had a safety concern about the experiment, we monitor during the experiment the in-core thermocouples. There are numerous criteria for terminating the experiment. One of them would be a rise in the in-core themocouple. I think it is 1000 degrees Fahrenheit, is it? So if the temperatures were to go up to 1000 degrees Fahrenheit, we would terminate the experiment, which means primarily increasing the ECC flow rates. So we really don't feel you're in trouble.

DR. CATTON: So they're just going to slide up next to the window.

DR. PLESSET: Well, not necessarily. It depends on how the temperatures go.

DR. CATTON: That's true.

DR. ZUDANS: But on the basis of previous presentations, we do not expect the core to uncover even as much in the pumps-on as it did with the pumps off, if we have any kind of a capability to transfer the previous result to this. So you do not expect the level to drop below this level; you expect it to be above. So there is no such thing as a "window"; right?

DR. PLESSET: Well, you said it.

(Laughter.)

DR. PLESSET: I don't know if Brian would consider that the end of his concern.

(Laughter.)

DR. ZUDANS: I don't know. We'll see.

DR. CATTON: Gee, it makes one want to be here for the test.

DR. PLESSET: Yes.

DR. ZUDANS: That's right.

MR. CONDIE: Let me go on now with discussing now the heat transfer and cooling mechanisms during the transient.

(Slide.)

This is kind of a busy slide, but let me go through it carefully with you. I have plotted four temperatures on this slide. As indicated, the solid line that drops below is the primary system saturation temperature and corresponds to the temperature of the pressure decay term you saw before. Note we're only looking at the first 4000 seconds -- that period of time where the transitions from various natural circulation modes take place.

The dotted line is the secondary system saturation temperature. So in this period, as we indicated, the primary is above the secondary. Here (indicating) it's below. And here again (indicating) it's above.

These other two lines, as indicated, are the steam generator inlet temperature and the steam generator outlet temperature on the primary side, indicating, as I'll talk about later, what the flow mechanism might be.

Initially, the inlet temperature to the steam generator is higher than the outlet, the pumps coasting down here, and we get the establishment early of single-phase natural circulation, and then as the system goes saturated we develop a smoothe transition into two-phase natural circu'ation.

In a period here of about a couple-hundred seconds or so, we get -- it's difficult to see -- but we get an inversion between the inlet and outlet temperatures in the steam generator. They're very, very close. But the outlet temperature indicates a couple of degrees higher than the inlet, indicating a higher void fraction in the outlet than in the inlet.

We have interpreted this to mean the existence, or the potential existence of reflux cooling during that window that exists from about 200 to 6- or 700 seconds, right in here (indicating).

As we loose cooling to the steam generator as this pressure drops below, the temperatures invert again and through this, then, the inlet is lower than the outlet. Well, at this point in time they switch, and the inlet then is higher than the outlet. But this is a fairly stagnant region because there is no heat transfer to the secondary. There is some heat transfer back on the secondary system to the primary system during that period of time.

In fact, we see where the temperatures even indicate that they go a little above the saturation temperature in the secondary. Whether that's just a measurement of radiation we're not real sure, but we do indicate that we get heat transfer now from the primary to the secondary because, at this point in time, even before these pressures equilibrate, we get an inversion again. We see that the inlet temperature to the steam generator goes to saturation, and it is another couple of hundred seconds before the outlet to the steam generator goes to saturation, indicating a redistribution here and a nice smoothe transition to two-phase natural circulation for the remainder of the experiment.

The conclusions here being, then, that we've traversed several modes of natural circulation cooling from single phase, two-phase, refluxing, loss of natural circulation in this region (indicating), then a smoothe reestablishment of two-phase natural circulation in the latter portion of the experiment.

(Slide.)

I will now discuss the experimental predictions that we made prior to this experiment, which subject we are all concerned about. As I indicated, these are made prior to the experiment and so published. As you see from the plot of primary system pressure as a function of time, I show here just out to 3000 seconds. The data is shown as line number "1."

The RELAP5 calculation done at EG&G is line number "2," and the TRAC calculation is line number "3." You will note the much, much more rapid depressurization rate in both of the calculations as opposed to the data. The trend is the same -- that is, the calculations depressurize to the point where the 300 psi is reached and the break flow is terminated and show the increase in pressure; but the time is compressed by a factor of two, at least in this region, compared to the data.

This pressure phenomena can be directly related to the calculation of the break flow. As shown in this plot, the data being number "1" where we have this much smaller mass flow out the break than was calculated by RELAP5 or by TRAC. So this indicates one of the weakest areas, or the most important areas for the calculation of system pressure response. That is, the knowledge of the boundary conditions directly related here from the inability here to calculate the break flow back to the pressure.

DR. CATTON: Could you give us a little information on the codes, now? Does the RELAP5 and the TRAC have stratified flow in it, or not?

MR. CONDIE: The RELAP5 code does have stratified flow, yes.

DR. CATTON: Does TRAC?

MR. CONDIE: I'm not -- I think it does. I'm not sure on this particular calculation.

DR. CATTON: So one of those things that was found in the Semiscale test -- the need for stratification -- is in RELAP5 already, and yet we're still seeing poor predictions.

MR. CONDIE: That's correct.

DR. CATTON: Okay.

MR. CONDIE: That does indicate -- We're at a point here in this experiment where we're -- if you look at break flow as a function of density, or subcooling, or what-have-you, is you switch from a subcooling down to the saturation, a very high flow rate when you have a high density, the flow rate dropping off very rapidly as you increase the void fraction. We're in this point in here where there may be a big error in the model which, based on our single-effects test doesn't appear to be that the model is that far off, but that the density of the fluid upstream of the orifice that we're feeding into that model is probably off some and, for these conditions, we're very, very sensitive to those densities.

So even though we apply the stratification model, if we don't feed that break orifice exactly the right density fluid, we don't predict --

DR. CATTON: Well, if the surface is at the wrong place relative to the break, you may feed it steam where you should be feeding in water -- the level.

MR. CONDIE: The level, that's correct. We don't calculate that level, or the subcooling even in the liquid portion early in the transient, and then the mass flow is completely different. But this does point out an area for much research.

I do want to point out, as I indicated earlier, the existence --

DR. CATTON: What kind of "research"? If you can't calculate the levels in the pipe when it's stratified flow properly relative to the break, you're going to have terrible errors. I'm not sure that indicates a need for more research; maybe more work on your code, or a better measurement --

MR. CONDIE: Well, a better measurement so you can quantify that mass distribution.

DR. CATTON: Can you find that surface from the data when it's stratified? Do you know where it's at?

MR. CONDIE: We have -- In the area where we have the gamma densitometers, as I indicated, we have three beams, and some of them four, which is a shot directly down. By simulating all that data and applying some assumptions, we are able to predict or imply where that level is and what the density is below and above that. But it isn't a straightforward measurement, at least in LOFT. Now we could have some other tests where you could actually visually --

DR. CATTON: Particular things came out of the Semiscale tests. I'm wondering, is that going to influence your instrumentation in LOFT at all? I would think you would want to be looking more specifically for stratification than in the past.

MR. CONDIE: Chuck would like to address that.

MR. SOLBRIG: Chuck Solbrig, EG&G. I think that traditional models have to be included in the code which can more accurately represent things such as froth flow. We seem to think in terms of steady-state flow regime maps, and steady-state flow regime maps may not be applicable in all cases here.

I think we have instrumentation in terms of our gamma densitometers that pretty well determine what the distribution is within the pipe. The codes such as RELAP5 and TRAC -- the one-dimensional version of TRAC -- really do not represent the perpendicular flow distribution, and I think that that is what Keith is probably referring to that additional analytical research needs to be done, and additional analytical models need to be developed to represent that.

(Slide.)

MR. CONDIE: We indicated from the experimental data we had inferred the existence of reflux cooling early in the transient for a period of several hundred seconds. We did predict this to occur with RELAP5 prior to the experiment, and I just wanted to show this.

Our definition of "reflux cooling" -- and there seem to be several around -- but we define "reflux cooling" where we have a definite condensation of the vapor to liquid in the steam generator, and that liquid falling back down the inlet side of the steam generator, making its way back to the upper plenum with countercurrent vapor flow in the top of the pipe in the reactor vessel.

We have predicted that in the RELAP5 prediction of this experiment. As shown here, the vapor velocity is positive throughout this whole period of time from zero to 800 seconds, but note that at about 150 seconds the liquid flow goes negative, or is returning back to the reactor vessel, and maintains a constant negative velocity until about 500 seconds, in which case it basically goes to zero there. So there's a very long period of established reflux cooling, and we feel that we have indications that that phenomenon did occur in the early part of this experiment.

(Slide.)

In conclusion, this experiment answered a part of the pumps-on/pumps-off set of tests. We feel we will be able to, and have done in part, quantified the primary system mass inventory and distribution of mass throughout the primary system for the pumps-off case. Our analysis, as I indicated, is still continuing.

We have demonstrated in this test decay heat removal mechanisms and smoothe transitions from one mechanism to another.

These include: Single-phase natural circulation; two-phase natural circulation; and the indicated existence of reflux flow.

Even though the code predictions have compressed the time frame because of the high mass flux prediction, we feel that we've predicted the major phenomenon and in the proper sequence for these tests -- at least the transitions from one cooling mechanism to another cooling mechanism in using the code.

That concludes my presentation.

DR. CATTON: I don't think I would agree with that. Factors of two on pressure and factors of five on mass flow are not reasonable.

MR. CONDIE: I say we have predicted the phenomena and the sequence. If we define "phenomena," as I will do quite tightly here, the heat transfer and fluid flow mechanisms that we're continuing in the primary system.

We do have a lot to do in order to predict the mass flow from the system to get to proper mass inventory. I agree with that.

DR. PLESSET: You don't have the accumulators blocked out?

MR. CONDIE: The accumulators were locked out, as they were in the Semiscale test.

DR. PLESSET: I just wanted to be sure that I had that clear.

DR. ZUDANS: And the analyses here are the pre-test analyses?

MR. CONDIE: That's correct.

DR. ZUDANS: Okay. And you are doing something more now to the analyses?

MR. CONDIE: Yes, we are.

DR. ZUDANS: So until you reach such a point, you can't tell what effect LOFT has on your codes.

MR. CONDIE: That's true.

DR. ZUDANS: It may be that it is the same effect as was found in Semiscale, and it may be something else.

MR. CONDIE: It appears that in Semiscale the codes did a much better prediction of the break flow than we have done with this test. Whether it's just the particular conditions where we ran this test are at a case in our critical flow where we're much more sensitive than in Semiscale, we haven't answered that question yet.

DR. ACOSTA: Have you taken your measured break flow and, with that, redone your analysis to predict pressure? And would that be better?

MR. CONDIE: Not yet, and that is in progress. We will drive the RELAP model with the time-dependent mass and energy flow from the system, so that will become a boundary condition.

DR. ACOSTA: If that were in reasonable agreement, then the horrible comparison shown here would not be so --

MR. CONDIE: I agree.

DR. ACOSTA: -- would not be so depressing.

MR. CONDIE: But we have not been able to do that. That is part of the analysis and it will continue.

DR. ACOSTA: Why do you think there is such a great difference?

MR. CONDIE: Between?

DR. ACOSTA: Between the predictions of break flow and what you have measured.

MR. CONDIE: As near as I can speculate now, it is that our prediction of the fluid density that goes into that break is not proper, and we just happened to be at a situation where the break flow is extremely sensitive to upstream densities.

DR. ACOSTA: You think it is a highly localized phenomenon, then?

MR. CONDIE: That's very possibl . I've said the-predicted that much better in Semiscale than we did in LOFT. Our RELAP4 models, which are very similar to that that was used in Semiscale, which we had prepredicted this test with in the planning stages a long time ago, show basically the same trend as our RELAP5 calculation. And noting, also, the TRAC calculations predicting the same thing indicates that it may be a very localized -- or particular local conditions that we see in this particular break size and break location.

DR. PLESSET: They have a very special kind of a break installation, you remember.

DR. ACOSTA: Yes. It's just that if one wishes to make comparisons with the pumps-on experiment, that if it is so sensitive that you are liable not to get anything out of that comparison.

MR. CONDIE: The pumps-on experiment could change that set of conditions such that we would do a much better job of predicting the pumps-on than we do the pumps-off.

DR. ACOSTA: I think someone wanted to make a comment back here.

MR. QUAPP: Bill Quapp, again, EG&G.

Relative to this question of predicting the break flow, we have been discussing a potential research program -- this is kind of directed to Dr. Catton's comment on "what research?"

That is, to do a very extensive separate-effects program measuring the critical flow out a break from a pipe tangent to a large pipe, a small pipe tangent to a large pipe, as a function of flow regime in the pipe. In this case, there was no flow regime, so you could almost consider that to be the relative ideal case where you do have a two-phase boundary, but at least it is moving only in one direction instead of various combinations of anular mist, chug flow turbulence of the different types of flow regime effects. But there is a variable break size in the small-break as well as angular orientation.

And very quickly, the permutations become huge in terms of understanding them, but the point is that I think the break flow calculation capability in the small-break scenario, when one envisions this, there's a wide range of possibilities and separate-effects tests can be used to narrow that down.

DR. CATTON: There's an aspect that has nothing to do with the separate-effects tests. What I was referring to, if you have a one-inch break in an eight-inch pipe and you can't predict where that surface is in the stratified flow within one inch, you're going to get somewhere between zero and full flow out the break, and you don't know where the hell you're at, and no amount of research on break flow is going to answer that question. You've got to be able to calculate that stratified flow correctly. Research and experiments is not going to answer it.

That is why I asked the further question about what you are doing to know where that level is in the pipe in LOFT and know if that's your problem. If the level drops below the break, you've got pure steam out that hole. So one inch out of eight, that means you've got to know it better than 10 percent, and probably more accurate than that to get good results.

MR. QUAPP: I think it's even more important than that. Some work that Dr. Leahy has done has shown that it isn't simply a question of the level dropping below the break, but that steam can tunnel through a liquid interface at elevations where the pre-liquid interface is still above a break.

So my point would be that I think we need some separate-effects experiments that can give us greater insight: When does the criteria of the steam tunnel through? And under what flow regime conditions would you then have to calculate? Because if the tunneling starts at three-fourths' full pipe for a half-full break elevation, it isn't a matter of calculating the liquid level accurately as much as it is understanding the localized phenomena of critical flow out of break in the pipe tangent to a large pipe.

DR. CATTON: I guess I would first like to be convinced that I can calculate where the level is; then I would worry about these other aspects. I believe Zuber had a very nice set of viewgraphs describing this phenomenon.

DR. PLESSET: Yes, I think we can get misled, or maybe too excited about something like this tunneling thing. I think that Catton's point is a basic one: That you have to be able to tell the stratification levels; and if you can't do that, you're in a poor situation. Right?

DR. CATTON: That's right; the other information. Knowing more about the break flow doesn't help a whole lot.

DR. PLESSET: Because you can get almost discontinuous changes as the level were to change.

DR. ZUDANS: Did I understand you correctly saying that you will do, or are doing another analysis where you would prescribe the break flow as a boundary condition?

MR. CONDIE: That's correct.

DR. ZUDANS: What is the point of that? Because it tells you, if it brings your result -- Oh, yes, there is some point. If it brings your results in better agreement, you know that that's a critical item.

MR. CONDIE: That's right.

DR. ZUDANS: But it will not make your code useable to PWR.

MR. CONDIE: Oh, that's correct.

DR. ZUDANS: Until you solve the problem that Ivan is pointing out to you.

DR. CATTON: At least they'll know, if they do that, they're in better focus.

DR. ZUDANS: Well, that's okay. So that's not for the purpose of improving the code; it's only to isolate what makes the big difference. Right?

MR. CONDIE: That's right. We can rule out the heat transfer, or whatever it is that may be making those big differences.

DR. ZUDANS: But you still have to solve the other problem of how to make your code more applicable to your model.

MR. CONDIE: Well, then you need to go the one step farther as to how you apply that to an unknown break location and size.

DR. PLESSET: Well, are there any other comments?

(No response.)

DR. PLESSET: If not, I think we will take a break for lunch and reconvene in an hour.

(Whereupon, at 12:45 p.m., the meeting was recessed, to reconvene at 1:45 p.m., this same day.)

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AFTERNOON SESSION

(1:47 p.m.)

DR. PLESSET: Let's reconvene and continue with our program.

The next item on our agenda is a presentation by Brian, again, and the NRC Plans for Resolution of Pumps-On/Pumps-Off Issue.

Brian?

(Slide.)

DR. SHERON: What I intend to address, I guess for the next hour, is where we are going to go from here, and how we are going to get out of this mess with pump trip. I'm kind of getting tired of working on it.

(Laughter.)

DR. SHERON: There's got to be a way out.

MR. RAY: You have a lot of company.

MR. MATHIS: Good for you, Brian.

(Slide.)

DR. SHERON: First of all I wanted to start out by saying that, okay, we've run a bunch of tests in Semiscale, and what have we in NRR learned from all of this?

Well, one, I think Gary Johnsen already pointed out what they have concluded and we've concluded the same thing. That is, we have gained assurance that 1-D equilibrium models, of course being used by the industry as well as the staff, should be reasonable able to predict qualitative behavior in small breaks in the PWR with the pumps on. By that, I mean that the major phenomena will be predicted properly and that we have some sort of assurance that what the vendors have been giving us is consistent with what we would expect to see if we had much better experimental data available.

The testing showed that the initial inventory behavior prior to accumulator injection appears to be fairly consistent with large PWR predictions of inventory behavior; that an accurate quantitative prediction of small break with pumps on appears highly dependent upon certain modeling aspects -- for example, pump two-phase degradation, the break flow subcooling; and that the predicted strong dependency of break flow subcooling in assumped pump operation for large PWRs is confirmed by the tests.

Many of the modeling concerns identified in NUREG-0623 have been borne out by these Semiscale tests.

So I guess to summarize what all that says: It has confirmed a lot of our previous skepticism of vendor models. It has also given us some confidence in other aspects of the models with regard to the inventory behavior.

You have to remember that despite the fact that the industry used different modeling techniques, there is a common thread running through all the calculations. Number one, they all concluded that the pumps had to be tripped; that they all predicted that this window existed; and that given the fact that they all do this independently, the window size -- at least with regard to break size -- was fairly consistent, somewhere between .02 and .2 square feet.

So keeping that in mind, and looking at the way the models are predicting Semiscale, at least we don't feel right now that the vendor predictions are so far off that we should just throw them out the window.

DR. ZUDANS: Brian, didn't the Semiscale have to adjust the pump deterioration curve to get anything close to the tests? And if you looked at the pre-test results, then they would be more or less like the vendors' analysis today and prove to be not valid?

DR. SHERON: Well, it depends on what you mean. I think Gary pointed out that they certainly did increase the degradation of the pumps from the post-test analyses from what they used on their pre-test, and that gave them a little better agreement with the inventory prediction later on out into the event.

I think that only goes to show that there is a very, very small sensitivity here -- and let me not say it's specifically on the pumps, but let's say on the inventory predictions. We took a look at Zion, for example. It's a classic Westinghouse four-loop. If you look in the core region, if you took a slice across the core, you will find out that --

and I am including the downcomer, as well -- the volume in the core in the downcomer region is something like 100 cubic feet of fluid inventory volume per foot of core in that region. Then you look at the volume in the primary system, it's about 12,000 cubic feet. You look at the volume of the vessel, maybe it's 4000, 4500 cubic feet. And you look at a small break that uncovers a few feet down into the core, and you ask yourself how much inventory has been pushed out of the system to bring it down to that point -- and not even including the ECCS water from the HPI that has been injected and has run out the break -- but just saying, physically I had to remove at least say something on the order of 9- or 10,000 cubic feet of water to get down to that level. And then you say that if I missed that by within 100 cubic feet, okay, I'm talking an error of maybe one foot of core uncovery or something like that. Okay, you've got to do the inventory down to a couple-percent accuracy or you're going to have some hellacious changes in clad temperatures, let's say, in the amount of core uncovery.

So what I am saying is that the accuracy required appears to be a very sensitive -- I'm sorry, not the "accuracy," but the inventory appears to have a very high sensitivity to [illegible]n factors in the calculation; and that minor uncertainties, or that in certain parameters even a minor change in the minimum inventory can lead to a major change in the predicted clad temperature or the predicted amount of core uncovery.

DR. THEOFANOUS: Yes. I think that is a very, very important statement, Brian, and I want to ask you: What does this imply? Because I think that has some very important implications.

DR. SHERON: What does that imply?

DR. THEOFANOUS: What is the implication of what you just said?

DR. SHERON: I'll get to that.

(Slide.)

DR. PLESSET: Are you implying, Brian, that if you get down to this level of inventory where, quote, "one foot of core is uncovered," that that's horrendous? Because I don't believe that.

DR. SHERON: No, I'm not saying one foot is uncovered. I'm saying that if you've uncovered down, say, to the seven-foot elevation --

DR. PLESSET: So you're talking like about five feet?

DR. SHERON: Yes. I mean, if you look at the typical small break in a Westinghouse, a limited small break in a Westinghouse plant, a three-loop plant, the minimum level that gets uncovered is down to about five feet, I believe, below the top of the core. You're talking clad temperatures upwards of 1300 degrees or so.

I'm saying, if you miss that inventory by a small amount, you may uncover four feet, you may uncover six, I don't know, but the difference in clad temperature may be a couple-hundred degrees, just based on a slight error in inventory.

DR. CATTON: And, gee, if LOFT's prepredictions are showing factors of 2 to 5 on pressure and mass flow, I think you're going to miss the inventory by quite a bit.

DR. SHERON: Yes.

DR. CATTON: So I don't think I really follow what you're trying to say. I thought you were trying to say that the vendors --

DR. SHERON: Well, let me jump the gun here --

DR. CATTON: -- you said the vendors were doing well, and yet I see calculations with TRAC and RELAP5 that they're not doing well by your standards. Are the vendor codes better?

DR. SHERON: We don't know yet. My guess would be, no, they're probably about the same.

DR. CATTON: So they may even have a, rather than 2 to 5, a 4 to 10?

DR. SHERON: I wouldn't want to venture to second-guess the industry at this time.

DR. CATTON: I don't, either. I was just trying to figure out where you were.

(Laughter.)

(Slide.)

DR. SHERON: Well, basically where I am is that with regard to pump operation based on a review of all the information to date, the ACRS recommendations, we have reached a conclusion that we believe that the pumps should be tripped in the event of a LOCA, which I think is what I was trying to point out originally.

Quite honestly, I think there are a lot of factors that come into play in these calculations which we're just not doing to ever get an answer for that is going to satisfy us.

The question of the pumps, which was discussed extensively, I don't think there's anything in the works right now that is going to put that to bed in the near future.

Some of these other areas, the break flow, how it is affected by the azimuthal location of the pipe -- that's just a big questionmark right now.

DR. PLESSET: Well, you kind of stacked the cards in the direction of the conclusion. Nobody would disagree, I would think, with the statement that the pumps should be tripped in the event of a LOCA.

DR. SHERON: Let me carry it --

DR. PLESSET: If you don't have a LOCA --

DR. SHERON: Okay, we're going to get to that.

First let me just give the reasons why we've concluded this, because obviously the other alternative was to say: Let's try and keep the pumps running during a LOCA.

One we have previously discussed is that the pumps were not designed to perform for extended periods in a two-phase fluid, and we feel that that might lead to a higher likelihood of failure. Even though they may be able to run forever in a two-phase mixture, it is my understanding that that was never a design basis for those pumps, and I doubt the manufacturers would be willing to extend their warranties, et cetera, if the pumps were to be expected to run in that region.

DR. ACOSTA: What do the manufacturers say about running in this region?

DR. SHERON: We didn't really ask the manufacturers.

DR. ACOSTA: So this likelihood of failure is your feeling?

DR. SHERON: Beg your pardon?

DR. ACOSTA: This higher likelihood of failure is your feeling.

DR. SHERON: Well, I pointed that out. It has to do with the question of slugging.

DR. ACOSTA: Yes, but can you quantify that likelihood?

DR. SHERON: I can only quantify it by saying that if I make a set of conditions a design base for a piece of equipment, it implies that that piece of equipment has to meet certain criteria. For example, if I say a piece of equipment has to be designed for a safe-shutdown earthquake, that means something very specific. That means that when they do their seismic analysis it applies to certain ground acceleration, et cetera, which that piece of equipment must show it can withstand.

Now when I say that they aren't designed to operate in a two-phase region, I am saying that when the pump designer sat down he was never told when he designed this pump it must be able to operate in this region for X period of time.

DR. ACOSTA: Sure. That was probably not in the original specs.

DR. SHERON: Correct. Now I'm not saying they can't; I'm saying that I'm implying it because if it's usually not in the design base, I have I guess a lot less confidence perhaps that it would operate successfully in that region if it hadn't been designed to operate in that region.

DR. ACOSTA: It would seem to be a good idea to ask them that.

DR. SHERON: Well, I guess you would ask them only if really there was an intent to want to run them in this region. I think we have concluded that we don't believe that the pumps should be run in this region.

DR. THEOFANOUS: Do you think they would know the answer to that, even if you asked them? Do you think they would know?

DR. SHERON: I don't know; they might.

DR. THEOFANOUS: I would kind of doubt it, because I don't think we have any experience with that.

DR. SHERON: Correct.

DR. THEOFANOUS: Maybe they would try to take a guess at it, but I don't think they would be able to tell you.

DR. ACOSTA: I think it would be okay to turn the pumps off, if you really feel that there is a safety risk, and if you call it that, that's fine.

DR. SHERON: Let me --

DR. CATTON: You see, Brian, the problem I am having is in the face of codes that can't come within factors of 2 to 5 when compared with LOFT, and these are supposedly our best-estimate codes as developed by the NRC program. I have a little bit more doubt about the codes that are coming from the vendors, because they are based on EM models, and they do as little as -- they're worried about Appendix K. And the decisions or conclusions based on that seem to be without foundation -- even though they all got the same, their codes were all the same starting point. They were all EM models for Appendix K at the beginning.

So I would expect them to predict the same thing. And then we have stratified flow indications from LOFT, Semiscale which is smaller we have more reason to believe it's homogenized flow, it's stratified. I'm just convinced it's stratified flow in the full PWR, yet the codes don't do that; it's, for the most part, homogeneous flow. I don't know how you can come to a conclusion based on all of these -- particularly the 2 to 5 factors --

DR. SHERON: Well, the evaluation model for the codes are all based on a stratified flow run in the primary system because they always assume that the pumps were tripped at the initiation of the event. Okay? So from that standpoint, they have always assumed they had stratified flow in all regions around the primary system.

DR. CATTON: Well, that's news to me.

DR. SHERON: As a matter of fact, I don't know of any vendor that had a homogeneous representation.

DR. CATTON: So they have defined water counterflow in the hot leg and the cold leg?

DR. SHERON: No, no, I'm not talking counterflow for the phase separation.

DR. ZUDANS: Brian?

DR. SHERON: Yes, sir.

DR. ZUDANS: I think that the fact that there is a stratified flow at the pump, even less than that, we have observed that the pump gets water slugs, the first reason alone is good enough to say "you shall shut the pumps down," because I am quite convinced that there is no pump designed that can take the beating for a long time. That is the only good reason. The rest of the reasons are not important.

MR. MATHIS: Well, what do we define as a "long time," on "extended period"? Are we talking minutes, or hours?

DR. ZUDANS: You have to observe the vibrations of the pump in actual installation. They have noise monitors, they have vibration monitors, and the pumps start beating like crazy you'd better shut it down or you won't have it.

DR. SHERON: The point is that if -- at least our feeling is that if you're running through a small break and if these pumps did start vibrating and were doing something they weren't supposed to do, that we would prompt an operator to try and turn them off, and it would probably be during the periods of high void fraction, which is when you don't want to turn them off.

Plus, we're also saying that if I want to leave these pumps running, I'd better really be able to calculate down to a very high degree of accuracy mass in the system. I'm saying that, based on everything we've seen today, I'm not convinced that we're going to get there in the near-term, if ever. And it is a matter of diminishing returns, too: How accurate can a code really get.

Some of the other points on why we believe they should be tripped in the event of a LOCA is that if you look at small breaks and you look at adequate core cooling, you find out that most inadequate core cooling actions, or "scenarios," I should say, are initiated by small breaks. In other words, in order to get inadequate core cooling you have to somehow lose inventory from the system. And in order to lose inventory from the primary system, you need some sort of a break in the primary system.

So -- and one of the things, if you look at inadequate core cooling guidelines that are coming out of the better shops right now, one of the actions that they instruct the operators to do is to turn the pumps back on, which basically is a last-ditch effort to try and force some sort of coolant through the core, be it steam or liquid, and hope that the pumps will be able to stay running and pump something through the core to keep it cooled.

If you trip them early, I feel there is a greater likelihood that they will be available later on in the event somebody has to turn them on, rather than if you're running along and they start vibrating or doing something that wasn't expected, and they do fail in some mode; or if the operator turns them on, and then all of a sudden say we don't know how to calculate these things very well, and covers the core, the liquid collapses back and covers the core further than what we thought and we would start seeing excessive heatup, and he tries to start them and they don't start.

So that was one other consideration here.

Another is that you look at best-estimate analyses to date and you can show that the most probably small breaks -- and what I mean by "most probable" is that I guess I'm trying to say that one would think a small break would occur on some sort of an ancillary line and would have a higher probably of breaking on some sort of an ancillary line coming off a main coolant pipe, as opposed to some break going on the side or the bottom of the pipe.

One can see a weld failing, or something, as a more probable cause than catastrophic failure of a primary coolant pipe. And in fact I think it was mentioned that most of these penetrations are on the upper half of the pipe.

If you look at those calculations from a best-estimate standpoint, you see that most of them probably don't even uncover the core. And if they do, it is just for a very, very brief period, just the loop seal clearant aspect, which is a short spike into the core and then back. You don't even see a heatup on the fuel.

So from the standpoint of, gee, does leaving the pump running cool the core better? Our feeling is: Probably not, because the core will probably remain covered for all but the most limiting type of break. So you're not going to challenge the fuel. You probably would not even have significant fuel damage in one case, whereas in the other you would.

Now also, both the best-estimate and evaluation analyses show that the core is protected -- and by "protected," these are the requirements of 50.46 -- for all small-break LOCAs with the pumps tripped. We're still faced with the question that if I have a pump-trip delay during some undefined window period, an EM analysis is going to show that the results are probably unacceptable.

Also, due to the code uncertainty, the question of whether even best-estimate analysis is acceptable is still sort of unclear at this time.

DR. ZUDANS: Is it expected that the best series of Semiscale and LOFT will shed more light about the existence of this so-called "window"?

DR. SHERON: Yes. I think that through the modeling comparisons to the data, and examining how well we can predict the two tests, we will have a much better understanding of how well the models predict the test; and then we can make a determination of whether we believe they're predicting the PWR very well.

DR. ZUDANS: Okay.

DR. SHERON: And a lot of the modeling concerns which I referred to in NUREG-0623 are being resolved as either legitimate concerns, or, no, they're not a concern, the codes do a very good job of it, and we don't have to worry about that item.

DR. ZUDANS: So at such time you would be able to analyze the range of break sizes and conclude whether or not there is a window? And supposing you find out at the end of this exercise that there is no such thing as a window, would you still think it is prudent to shut the pump down immediately? Or wait until it gets into mechanical problems?

DR. SHERON: I would still recommend the pumps be shut down. Notice, I didn't say "immediately."

DR. ZUDANS: In that case -- Oh. Okay.

DR. SHERON: Let me carry this a little further.

MR. ETHERINGTON: Supposing after all you decided that the pumps should be left running, from a licensing point of view do you think that would force you to make a requirement that the pumps be capable of running under these ill-defined conditions of two-phase flow?

DR. SHERON: I guess if it was determined that the pumps should be left running for some reason, I'm not sure. We would probably --

MR. ETHERINGTON: Do you understand my question and the dilemma it may put you in?

DR. SHERON: Yes. I guess -- I think that the problem when Appendix K first came about was because the vendors could not demonstrate that their pumps would run entirely through a small-break LOCA. As a consequence, that is when we started postulating that the failed at some period into a LOCA, and that is how we started to define this window.

And I would probably say that unless there was sufficient evidence that convinced us that the pumps would run all the way through, we would probably then say that they would have to demonstrate that they could tolerate the pumps failing at some time during the event.

DR. WU: And further, Brian, you would still like to have the option remain open in Item 2? Namely, later on to turn on the pump if it should be so advised.

DR. SHERON: Yes.

DR. WU: That is, even --

DR. SHERON: As a last-ditch effort it one gets into trouble with core cooling, or something.

DR. WU: As a last-resort effort.

DR. SHERON: Yes.

DR. WU: Even to face the risk of the mechanical vibrations of the pump.

DR. SHERON: Well, if for some reason you're draining down the vessel uncontrollably, and for example you had a small break and, the classic is, the ECC does not come on, or it is somehow degraded to the point that you can't recover inventory properly, yes, I would say turn -- you know, it's better to run the risk, okay, of perhaps failing the pumps by starting them up in a two-phase mode and trying to get something through the core and cool it, okay? As opposed to saying: Gee, I'm liable to fail my pumps so I'll just let the core melt, or something like that.

DR. WU: Then it would be quite important to determine or ascertain such a criteria under which the pumps should be tripped.

DR. SHERON: Yes. And that is being addressed as part of the operator guideline review that we're doing now on inadequate core cooling. That is, what really constitutes inadequate core cooling versus what constitutes a small break where the operator should keep his hands off everything and let the systems do what they're designed to do.

What we have concluded is that we do need a better criteria for when the pumps should be tripped. I believe in a previous meeting with the subcommittee or the Full Committee I had discussed the four events which have occurred at nuclear power plants since Three Mile in which the operators tripped the pumps because a low-pressure ESFAS signal appeared on their board.

All of these events were not LOCAs -- I'm sorry, one, Prarrie Island, which was a steam generator tube rupture, is a form of LOCA -- but three of them were depressurized -- secondary side induced depressurized transients overcooling.

Two of them involved a steam dump valve sticking open to the condenser when it should have closed and produced basically small steam line break. One was the Crystal River event, which I'm sure you're familiar with. The other was the Prarrie Island steam generator tube rupture event.

The conclusion that we reached was that, although the pumps were tripped, there was no -- there was never any question that the safety of the plant was jeopardized any more than had the pumps been left running. What we did conclude was that there was a degradation in the operator's ability to properly control system pressure during the recovery phase, primarily because one loses the pressurizer sprays when the man turns the pump off.

Some plants have auxiliary spray capability off of the charging pumps, others don't. I think I remember seeing a letter from the committee recommending that all plants have this capability.

We are presently looking into that. I don't have anything to report at this time, because it does constitute a new requirement and we're kind of gun shy of forcing a new requirement without thorough study.

But we did conclude that if one can keep the pumps running for as long as possible before one has to make a decision to turn them off, so much the better for the operator's ability to have his sprays available and have forced cooling.

I would note here that Westinghouse has a criteria which is different from the other two vendors. Westinghouse made a counterproposal after the bulletin came out to trip the pumps on a lower pressure. This was their whole formula, which we agreed at mutually, on what that pressure is. It is basically defined by the secondary side safety relief valve set points.

Typically the plants -- and it also accounts for uncertainty as one works one's way back through the instrumentation, through the physics of the primary to the secondary side pressure differences, et cetera, and one comes up with set points in the range of say 1350 to 1450 psi.

Now Westinghouse has a low-pressure ESFAS signal of something around 1760, I believe, psi. B&W plants and Combustion plants are all down around anywhere from 15- to 1600 or so, 1650 psi.

Westinghouse took a look at the transients that have occurred to date when the operators have tripped the pumps, and they concluded that had those plants been using Westinghouse criteria, all but I think except one event would have not required the operators to trip the pumps. So that was encouraging because that's exactly what we're trying to achieve. by recommending new criterion.

So based on what we see today, we have no basic hangup with the Westinghouse criteria which is at a low pressure. We think that precludes a very high fraction of all depressurizing events except for the more severe ones like the steam line break or something.

We will probably require the licensees with CE and B&W reactors to revise their present criteria in order to reduce the frequency of reactor coolant pump trip for non-LOCA depressurizing transients.

I might point out at this time that B&W is presently recommending as part of their ATOG program to revise the criteria, and they are proposing that the pumps be tripped on a loss of subcooling, based on the subcooling need, and we are looking at that right now. But that is basically an indication that the system has gone in a two-phase when we would like to have those pumps tripped. And I haven't heard anything from Combustion.

(Slide.)

So if we are going to make them trip the pumps, why do we want them to predict L3-6, then?

We are not asking them to predict L3-6 just because we think that they should predict the test for the sake of the test. Every test that we ask the industry to predict is usually well thought out and there is usually some unique characteristics of it that make it a desirable test.

For example, you will note that we didn't ask them to predict L3-5. That is because L3-5 is very similar to L3-1 which they were asked to predict. We didn't think that we would learn very much by having to predict L3-5.

We agreed that the manual-trip option is by far the most desirable from the standpoint that it doesn't require any more hardware on the plants to be added. It does put much more control of the plant in the operator's hands, in that he is the one that decides when those pumps are to go off.

However, the industry has not produced any models for use as a benchmark against the applicable data to support this recommendation. So what we would like is that the licensees show that the operator has sufficient time to recognize the event and take the proper action, which is to trip the pumps.

If you look at the previous vendor analyses, you find out what kind of time we are talking about. Westinghouse, on a best-estimate basis, concluded that an operator would have probably greater than 10 minutes, based on the most limiting small break.

Combustion claimed that on a best-estimate basis for the most limiting small break, they said the operator had 10 minutes.

B&W did not do any best-estimate analyses, so we really don't know what that time is.

For an evaluation model, Westinghouse showed that their limiting small break for the three-loop plant required operator action within 10 minutes.

Combustion, for an EM calculation, showed an operator at six minutes he was to trip the pump.

And B&W, with their homogeneous model and two HPI pumps, I believe, calculated something like two minutes for the break.

Now remember, this time that the operator has to trip the pump is a function of the break size and location; and that this is for one particular break size and location the most limiting; and that for a different break size, the operator would have a different amount of time available.

We have taken a look at all of this. What e have concluded is that we can accept a manual pump trip in lieu of automatic, provided each licensee can demonstrate that with the revised criteria for pump trip -- namely, if Westinghouse were below pressure, if B&W supports a loss of subcooling, et cetera -- or, assuming at least ten minutes for operator action, whichever is larger.

In other words, if the criteria here show that, for example, they would not lose subcooling until 12 minutes, then we would say: Okay, based on that you should show us that your operator, if he tripped the pump at 12 minutes, the results are okay with respect to Appendix K.

If their criteria would require the operator to trip the pumps at less than ten minutes -- in other words, for a limiting small break they said: We would predict that subcooling would be lost using an Appendix K model say within five minutes, and the operator would trip the pumps, then we would say: No, you must assume ten minutes, which is the minimum time requirement we would allow for operator action.

I might point out that this is at least ten more minutes than were previously given for licensing calculations. So this is a deratchet, or whatever you want to call it, since the Standard Review Plan says we should assume 20 minutes.

So we believe 10 minutes is a sufficient time for an operator to identify an event and react accordingly. We would like the industry to show us they can meet Appendix K with that assumption for the range spectrum of break sizes and break locations for small breaks.

Now, what if they can't meet the criteria? In other words, what if they say: We can't meet Appendix K unless we assume the operator trips the pump in less than 10 minutes. If we assume that he trips it in 10 minutes, that we are going to exceed Appendix K.

Okay, well, all is not lost. We will consider a manual trip acceptable, provided the licensee can determine that a failure of the pumps to trip are required, due to operator error, trip circuit malfunction, et cetera. And, that a delayed pump trip until the worst time into the accident would not produce an unacceptable consequence using a best-estimate model and assumptions.

What that says is that if the operator -- the vendor says: I need the operator to trip the pumps very early in order to meet Appendix K. Then what we want him to show us is that: Okay, supposing an operator made an error and didn't trip them very quickly the way he was supposed to, show me on a best-estimate basis that even if he messes up and trips them at the wrong time, that all is not lost and we're not going to get in trouble on a best-estimate basis.

Obviously trying to meet this one may be a little more difficult, because it puts a little more burden on the vendor because he now has to demonstrate an acceptability of the best-estimate model.

Our present thinking right now is that if the industry is unable to demonstrate both items one and two, that we feel the only resolution to this would be that they should install an automatic pump trip that doesn't rely on very, very rapid operator action. That does not necessarily have to mean that the pumps are going to be tripping off all the time for any little perturbation. They can obviously put in some sort of a pump trip system that trips on a low pressure, on the loss of subcooling, something like that.

But what it would mean is that they would be putting additional hardware into the plant and the like.

(Slide.)

The benefits, as we see it, is obviously if they can manually trip it then they're not going to be putting in automatic trip circuits. Plus, it retains an additional degree of plant control with the operator, rather than having them sit around and watch these pumps trip off when you may not want them to.

The drawbacks: Well, obviously we're going to put a little more confidence in our analytical models than may be required had we gone the automatic route. And if the operator is going to have to trip them at some prespecified point in the event, perhaps on a loss of subcooling, perhaps on a low pressure, we may have to say, you know, is he going to sit there and look at a little meter and a guage goes down, or should we have an annunciator wired into the control room. There are probably too many in there now, and we're just adding another one.

But we think on balance that the manual trip is probably the most desirable way to go. But we need greater assurance that the industry understands the way these plants behave when the pumps are running for any extended period of time into the event.

That is basically where we plan to go. I would say that we intend to look at the vendor predictions of LOFT and try and make a determination on the acceptability of their models. I envision it as being an iterative process; it won't be a clean, just take a look at the comparisons and say "yes" or "no"; but, rather, we'll probably have to call them in and thrash out why there are differences, are they attributed to the fact they just don't know how to model, is it a code deficiency, did they not include a certain piece of hardware, let's say, that was in the LOFT system that should have been included. They may have to go back and do a recalculation to make their case.

But in any event, we would like to use the LOFT calculations as a basis for determining the acceptability of their models to predict the plant behavior with the pumps on. And presuming they do that well, then we would ask that they turn around and apply that same model to the specific large plants to demonstrate they can meet one of those two criteria. If they can do that, they're home free.

DR. PLESSET: Thank you, Brian.

I am going to ask for comments, but before I do I would like to say that I think you're a fairly reasonable fellow

(Laughter.)

DR. PLESSET: And I think that my reaction to your proposal is a reasonable one, and it's about as good as one can do and it's quite good enough.

DR. SHERON: I would point out that there are some, I guess, some hanging threads unanswered about Appendix K.

DR. PLESSET: Yes, but don't let that be too much of a --

DR. SHERON: Well, I've had discussions with our attorneys, and they basically didn't see any problem with accepting 10 minutes' time for operator action with regard to complying with Appendix K. So that aspect of the criteria, I think we feel comfortable with.

The second part, the credit for operator action, if it's very, very short, okay, if somebody comes in and says: Look, I've got to have an operator trip that pump within two minutes or all is lost. Well, then we're trying to impose options here that may be a little more difficult.

DR. PLESSET: Well, I will say again that I think this position, if it is adopted by NRR, seems quite a reasonable one to me. But I would like some of the other people to make some comments.

MR. ETHERINGTON: Could I ask a question?

DR. PLESSET: Yes, Harold.

MR. ETHERINGTON: I didn't quite understand the Westinghouse criterion based -- trip criterion based on steam generator pressure. If it falls below -- What is the criterion?

DR. PLESSET: I think, Harold, that is not the steam generator -- it's just a pressure point in the primary.

MR. ETHERINGTON: No, I thought it was the steam generator safety valves.

DR. PLESSET: But it's that same pressure in the primary, when it's reached in the primary.

MR. ETHERINGTON: Yes, that's what I wanted to get clear.

DR. SHERON: I think I can explain it very quickly with one of the slides here.

DR. PLESSET: But maybe we'd let him answer it. That was what I thought.

MR. ETHERINGTON: That's what I thought, but I wasn't sure.

(Slide.)

DR. SHERON: Okay, here is a predicted pressure plot for a small -- let's take the one with the pumps on, or leaving the pumps off; it doesn't matter. What they're saying -- and if you look at the inventory in this cross-over plot you'll note that the divergence starts at a little bit past 400 seconds. Okay? This is basically the question of of loop seal clearing versus --

MR. ETHERINGTON: What are the two curves?

DR. SHERON: Okay, the bottom one is with the pumps off, small break pumps off; the top one is the small break pumps on.

MR. ETHERINGTON: But what pressure is that?

DR. SHERON: This is the primary system pressure.

MR. ETHERINGTON: The primary system.

DR. SHERON: So I think the key is that, you'll note here, that when you clear that.

DR. PLESSET: But what was the pressure there?

DR. SHERON: Well, what is happening here in this calculation is you assume you've lost off-site power, or at least you assume you've lost your condenser and your heat sink. So what you're doing is you're steaming off the secondary side off the safety valves, and the primary side is going to depressurize to slightly above the secondary side pressure.

MR. ETHERINGTON: Okay.

DR. SHERON: So you'll note that's a little bit below 1300 pounds. That's because the secondary side is assumed to be sitting at the secondary side safety valve set point, which is usually around 11- or 1200 pounds.

Now you will note here, you remember at the point you get in trouble when you trip the pumps is -- and again remember this is the curve with the pumps off; and this is the acceptable calculation. In other words, this does not produce horrible results.

It's only this one (indicating), and when you trip up here (indicating). Okay? So obviously, by just looking at the situation you can say: I only get in trouble when my system inventory is lost way up in this (indicating) region. And if I believe this calculation, then it says that at this point, at the cross-over, okay, which you will note is slightly beyond the 400 seconds, which is representive of when the loop seal clears. Okay? Because that's why this turns over. The loop seal is clearing and now you're just pushing steam out the break.

You can say that, obviously the time when I want that -- the time when I have to trip - when I don't want to trip the pump is after I have cleared the loop seal in the other case. Okay? And if you go back over here, you will note that the pressure then is below the secondary side safety set point. In other words, the pressure would have had to have dropped below the secondary side safety valve set point before the time is when I don't want to trip the pumps. Which basically says that if I trip the pumps any time before, I'm okay; I don't run into trouble.

So the way we got up this formula for Westinghouse was we said that based on the fact that if the pumps are tripped any time the pressure is at or above this plateau level, which is based on the secondary side safety valve set point, you're okay.

So what we did is, we backed off and we said: Okay, I don't like being on a plateau because there's just too much time in here. Okay? So we tried to back off a little bit and make sure that the operator then would have a criterion for tripping the pumps somewhere in here (indicating), before he got on this plateau.

We can define the pressure by saying: Okay, you take the secondary side safety valve set point, which maybe is 1150 pounds. Now back off the pressure drop on the line because those safety valves don't sit right on the generator, there's X number of feet of piping connected to them. So there is a pressure drop there of maybe 20 psi.

Now you can add that on. Now you say: Okay, the secondary difference in pressure may be another 30 psi. So now you go from 1150, plus 20, plus 30. Now you've got to back off to where the primary system pressure is and say there's a pressure drop between there. And then you tack on an uncertainty on the primary system pressure measurement, and on top of that there is an uncertainty on the secondary side safety relief valve set point.

You add all them up together, and you come up with a pressure that would be indicated to the operator in the control room, which is the primary pressure. That number should be somewhere, I think it's coming out on the order of 1350 to 1450 psi.

DR. PLESSET: Otherwise, they'd be tripping at 1800 or thereabouts. Right?

DR. SHERON: Right. Well, I think 1760. What is the set point?

DR. PLESSET: Something like that.

MR. ETHERINGTON: But as long as there's so much time on the plateau, why don't you let them use some of that time to --

DR. PLESSET: That's a good question.

Did you get the question, Brian?

DR. SHERON: Yes. Primarily because when you're out here in this plateau, okay, we're not really sure how well we understand that plateau. And as a matter of fact, at the time we set the criteria, we had a lot less confidence than we do today on the capability of the models to predict this small-break behavior with the pumps running.

So our theory was, let's fade off the plateau, but let's stay as close to it as we can. And this, again, gave them -- In other words, for any transient that was not a LOCA -- it's obvious that if you have a LOCA you're going to trip the pumps, whether you're here (indicating), or whether you're here (indicating). Okay?

So what we were trying to do was to say, we don't want to trip the pumps unless we're sure it's a LOCA. If it's some sort of a secondary side depressure transient, we don't want to do it.

Well, if you take a look at everything that occurred to date, take a look at steam line break events, very few events drop below around 1600 or 1500 psi. They come down, and then they turn right around and go back up. And the only ones that really get you down here (indicating) that are not LOCAs are a major steam line break, double-ended, which is very -- that's a limiting fault, a very low-probability event. And even that one takes you down to about 700 pounds, and I don't think there's any -- and you're going to flash in the primary system, and you lose subcooling, and you're going to trip the pumps whether you like it or not, because the operator is just not going to know what he's got.

So I agree that this was really margin, you might say, from a standpoint that if an operator didn't trip here (indicating), even if he was riding out here (indicating), he would still have plenty of time to trip it before he was in that trouble point, which is out here (indicating) somewhere.

DR. PLESSET: Any other questions, Harold?

MR. ETHERINGTON: No.

DR. PLESSET: Charles?

MR. MATHIS: No. He's answered mine.

DR. PLESSET: Let me turn to the consultants to see if they want to make any comments.

Yes, Theo?

DR. THEOFANOUS: I only simply wanted to say that I find, like yourself, the position very reasonable and I agree with him.

DR. PLESSET: Well, very good. I just want to make one comment, though, maybe of a general nature, which goes back to the comment I made this morning about continuing RELAP5.

Now so far the ACRS has been very polite about this, and I wish to have this work continue to be supported, and I would say "supported explicitly," not by some slush fund, or on the corner, or whatever. Because it bears on the point that keeps coming up: Do you want greater confidence in analytical models? And I think that RELAP5 should be continued and improved. It is a useful thing. I would like that message to get back, somehow, that when do we start being polite about it?

Maybe one of the consultants would like to agree or disagree.

DR. CATTON: Well, I have been following the advanced code program. RELAP5 at one time was a part of it, and I think that in spite of its successes it's been given a back seat. And now that I hear that it's almost going to be dropped into a crack, I think that's very upsetting. It's almost in the fact of success you throw away what's good.

DR. PLESSET: Theo?

DR. THEOFANOUS: Yes, I also have some feelings about RELAP5. I have been on record for a long time now that RELAP5 has a very, very useful role to play, and I want to reiterate that on this occasion.

DR. PLESSET: Okay, Dr. Sullivan, do you get the picture?

(Laughter.)

DR. SULLIVAN: We have not dropped itin a crack. It is a line item in the Semiscale program's budget -- and it is not a trivial amount of money, either.

DR. PLESSET: But say as compared to the total amount of money you fellows have been spending on the code in general, it isn't all that big an item.

DR. SULLIVAN: We will take the action item to review it and get back with you.

(Laughter.)

DR. PLESSET: Okay. Well, we'll appreciate it.

Well, thank you again, Brian. We seem to have come to a concensus that you're not being at all unreasonable, and on the contrary you're being rather reasonable, and we hope that the code people, the code assessment people and the test people will continue to support you.

Well, I think that was one of the objectives of this meeting, to get some view of what NRR was doing and how they were going in this direction in their thinking, and you've given that to us, which is very helpful.

MR. ETHERINGTON: Is there any time schedule on this, Mr. Chairman?

DR. PLESSET: The question was: What is your time schedule about implementation of these ideas?

DR. SHERON: What we are tentatively planning now is for the vendors to submit their predictions by December 3rd-- Well, I shouldn't say their "predictions" -- their documented models that they intend to use to predict LOFT by December 3rd.

EG&G, as I understand, will be running L3-6 somewhere, I presume, very closely after December 3rd, if not before, if the vendors submit their information prior.

We have allotted approximately four weeks, three to four weeks for EG&G to reduce the data into the appropriate -- well, I should say the initial conditions, et cetera, and to understand that if the valve was left open, or vice versa, the flow was halfway through the event, and to send out to the industry the actual conditions within three to four weeks. That puts us somewhere right around the end of Christmas.

We have asked the industry to submit their predictions in approximately four weeks after they receive the test predictions. That, again, is now towards the end of January.

We are now looking for something on the order of, I would say, six to eight weeks for the Staff and for EG&G and RES to kind of massage and assimilate all the information that we have, and to look at the comparisons to determine whether we have to call a vendor in to explain what he'd done, to get any recalculations done that are necessary. But hopefully to come up with some sort of a concensus on the capability of the vendors' models somewhere in the time frame of April 1st.

If we can do that by then -- and again that may be slightly optimistic -- I would then envision that we would inform the industry on the acceptability of their particular vendor model and request them to provide the necessary calculations to demonstrate conforming to the criteria which I proposed up there.

That time frame, I'm not quite sure. I think we would probably want one more meeting with the industry to get a better feel on what they intended to submit. Westinghouse has indicated they believe they could submit some generic analyses that would cover a range of plants -- say all the three-loop plants -- which might suffice, rather than have each utility come in with its own plant-specific calculation. It cuts time required for calculations, and thus doesn't require as many calculations.

Again, though, that may have to be thrashed out with the industry at a future meeting on what is the best way to proceed; plus, to get an idea of how they can respond, since they are under a lot of pressure to meet a lot of other NRC requirements across the board.

So I would envision that this whole thing might be wrapped up by next summer.

DR. PLESSET: Which is almost instaneous for a project.

(Laughter.)

DR. SHERON: Well, we're trying to keep it to within two years.

DR. PLESSET: Good.

MR. ETHERINGTON: If trip is manual, do you propose to develop criteria for operator action in case he overruns his deadline?

DR. SHERON: Yes. I'm sure that would be included as part of his procedures. I imagine they would contain appropriate warnings that if -- Well, if there was indeed a real concern about tripping at the wrong time, I would envision that there would be some warning which says that if they fail to trip when they were supposed to, that they should probably just leave the pumps running and leave them alone, as opposed to tripping them at the wrong time and running that risk.

Again, that is something that would have to be a little more thought-out, I believe. There are a lot of other similar ancillary types of concerns that don't really crop up here.

One is the question of keeping the pumps running for an extended period of time beyond an ESFAS signal, because you get continuing isolation. When you get continuing isolation, a lot of plants right now isolate many of the cooling lines which are on these pumps. And if you isolate them, you will wind up destroying the pump by taking away the essential cooling water to the motor bearings, to the pump seals; and we found out that it is very plant specific.

If you look at the St. Lucie event, you find out that they had a single failure in the component cooling water which forced them to trip their pumps.

I believe some of the plants have taken action now that they do not isolate the cooling lines on a low-pressure ESFAS signal. I think Westinghouse plants do not isolate their pump cooling lines on a Phase A isolation.

So those are some of the other concerns we're going to have to dig into, I think, as well as the operator guidance. Another question is the whole question of how do you trip the pump-trip circuitry itself? Is it sufficient? Or does it need to be upgraded any?

In other words, can you assure that the pumps will always trip when you push the button? That has to be addressed I believe in a little more detail.

DR. PLESSET: Okay, Harold?

MR. ETHERINGTON: Yes.

DR. PLESSET: Well, thanks again, Brian. We appreciate your presentations. They have been helpful.

I would like to go on to the next item which relates to another -- which really begins another general topic. That is, one of the purposes of our visit was to get an idea of the Semiscale program and the LOFT program in connection with the safety research review, and we are going to hear a review by Paul North on the Semiscale test program.

(Slide.)

MR. NORTH: Good afternoon. My name is Paul North, and I will be talking to you about the Semiscale experiment program. As Dr. Plesset said, this is the start of a new topic, so I would like to invite you to sit back in your chairs for the moment and take a deep breath, and get some intellectual fresh air flowing through. Because you have been focusing very narrowly on a particular licensing issue right at the moment, and I want you to look back a little bit and start asking some more general questions.

DR. PLESSET: Paul, we have been urged to be more global.

(Laughter.)

DR. PLESSET: Even caustic.

(Laughter.)

MR. NORTH: Good. If you can in fact stand back and ask the question, as Larry Leach recently has: If I accept the basic definition of "risk" as being a product of probability and consequence, if I take that viewpoint, then what are the kinds of things that I should be looking at in Semiscale? Where is it that Semiscale can make a contribution where it can address relatively high-risk items?

It turns out that, if you want to look at this area, you should be focusing your attention on small-break transients and other transients as opposed to the large-break transients.

So I think there is at this point a well-founded movement in Semiscale in the direction of analyzing small-break transient events, rather than focusing our attention on the large-break LOCA.

(Slide.)

Given the fact that one wants to undertake research in the direction of the small-break transient behavior, there are a number of items that one must consider in making plans for an experiment program along these lines.

First of all, we want the experiment program to be responsive to high-priority licensing needs. Semiscale has the advantage that it has the capability to respond very rapidly to questions that might be raised, and therefore obviously one should make an attempt to direct one's research in this area in the direction that we'll address, the high-priority needs, the high-priority questions first and take advantage of the fact that you can make rapid progress and rapid experimental process with this experimental system.

By these high-priority licensing needs, I mean usually needs that relate to specific questions that have to do with specific plants, or specific licensing issues. You have been having exactly a discussion of one of these kinds of things for most of the day.

There are also, then, in terms of the small break and transient plans some general thermal-hydraulic research needs that one can address. Here, I am directing our attention to items that transcend particular plants or particular licensing considerations and are germane to PWR safety considerations over a range.

Third, we must then consider what modifications we should be making to the system in light of the fact that we want to orient our research in this direction and with these specific immediate objectives. What modifications do we have to make to a system that was originally designed for different purposes in order to make it attractive and appropriate in terms of doing research in this area?

Then, also, are there areas where we can coordinate with other programs both in this land and outside this land to make sure that the research that we do has the greatest impact on the questions that we are trying to address.

What I am going to do in my formal presentation is to address each of these topics as we go down.

(Slide.)

So turning to the high-priority licensing needs first: We have worked with the Research and Licensing plans of NRC to define what the high-priority licensing needs are in terms of areas that could be addressed by Semiscale, at least a range of priority licensing needs only some of which may be addressed within Semiscale.

There are in fact about four or five potential candidates, and I am going to focus our attention on three which we have selected to do some research on during the next fiscal year.

First of all, there is a question relating to behavior of integral systems with small-break loss-of-coolant accidents with and without upperhead injection.

The basic objectives here are: That we need to provide data for the assessment of vendor code capability so far as the prediction of UHI performance is concerned under small break and transients, and the fact that small break transients are relatively probable.

We also want to get some comparative data which will allow us to assess the effect of UHI in these kinds of transients.

The experiment needs here are that we indeed conduct small break integral experiments with and without upperhead injection. We will be planning to do these during this fiscal year.

(Slide.)

The second question of an immediate licensing concern --

DR. THEOFANOUS: Could I ask you a question, please?

MR. NORTH: Yes. Go ahead.

DR. THEOFANOUS: On the previous slide, you say that you want to provide data to assess the vendor codes for upperhead injection --

MR. NORTH: To allow the vendor --

DR. THEOFANOUS: -- to allow it to be assessed. Whom do you envision to carry out this assessment? The vendors themselves? Or the NRC --

MR. NORTH: Probably that question could be more appropriately answered by Brian Sheron. We would provide the data to NRC and say this is the way the thing behaves; we'll obviously do some analysis ourselves. But as for --

DR. THEOFANOUS: I mean, to know whether you are already coordinated in some way, or unilateral? You say, I'm going to take the data and provide data --

MR. NORTH: This is an expressed need on the part of Licensing. They have a vendor who is performing calculations on these kinds of plants, and they want to be able to get some understanding -- some assessment of whether he is able to do that very well.

We will do our own analysis, but I don't think it is appropriate for us to, in effect, assess in a direct sense any vendor code.

DR. THEOFANOUS: No, I don't see your saying anything you will do as far as your codes.

MR. NORTH: This is a definition of experiments that we will conduct.

DR. SULLIVAN: I may be able to clear this up a little bit.

DR. PLESSET: Yes, go ahead.

DR. SULLIVAN: There is a requirement for Semiscale to do pretest analysis for all of the experiments they conduct, all of the new experiments. They would certainly be doing those with our code.

The item that -- how it got to be so prominent is that it was actually requested by the ACRS in a previous meeting. So we took that item in which we were trying to be responsive. It is also needed by the NRR staff in trying to provide some data for small breaks in UHI plants.

DR. THEOFANOUS: Well, I was asking this question because I am a little bit concerned about the ability of the vendor codes in fact to calculate upperhead injection.

DR. PLESSET: Yes. I think other people are having that concern, too, Theo.

DR. THEOFANOUS: Yes.

And then I think the question of assessment, you are somehow -- you see, you talk about assessing a code only when you have confidence that you have a tool that in fact can do the particular job, and then you say: Okay, now I'm going to assess it.

But if, to start with, you don't expect that this code can in fact calculate in the best-estimate sense the particular phenomenon, and in fact you don't use the code in that way, you use the code only from the point of view of getting some input to make a licensing decision as far as the acceptability of a particular system --

DR. PLESSET: You have a very good point, Theo. Let me indicate, as a non-expert, what I think might be attempted. That is, now they have their own codes -- RELAP5, TRAC -- and they will do some experiments in Semiscale and make some predictions. They will most likely get some kind of ideas as to what to do to the codes to make those predictions better.

Now I don't know whether that's "code assessment" or "code development," but whatever it is.

Yes, Brian?

DR. SHERON: I might point out what I guess I would envision at least Licensing would be using the information for.

We do have a Technical Assistance Program at Sandia which was specifically set up to put together an upperhead injection model. I also -- with RELAP there is also -- what is it? The TRAC/COBRA, TRAC modeling effort going on to look at this.

I would envision either -- we still haven't, I guess, trashed this out among ourselves, whether we need to impose this as what we call it. We may call it a "required problem" for certain vendors to assess their codes; or whether we would perhaps run tests to evaluate the capability of our own codes to predict it. And if we came up with any glaring deficiencies I think it would be fair to say that I would see no reason why the vendor codes would not have the same deficiencies.

That would then be a starting point to address the vendor codes.

DR. PLESSET: Theo had a point, I thought, that was important, which indicated some lack of confidence in the vendor codes in this area. My thought was: Well, that could very well be. I suspect he's right. But the real way to guarantee your self-assurance about this kind of performance at a UHI plant for a small break or a medium break would be to have codes that you had worked out yourself and had assessed and tested yourself, and then you will have some confidence in what those codes tell you. I think this is really the way to get this assurance. Would you agree with that?

DR. THEOFANOUS: Yes; right.

MR. NORTH: There are two things to use in the code. There is the fundamental capability of the code itself and the physical models and numerical techniques that are used within the code. There is also then the expertise with which that tool is employed.

DR. PLESSET: Absolutely.

MR. NORTH: The latter is very much almost as important as the former, in many cases.

DR. PLESSET: Yes.

MR. NORTH: So I think that having experiments like this available is addressing both of those areas.

DR. THEOFANOUS: I don't have any problem with the experiments. I think it is very good to hear of the experimental data. My problem was with this wording there that we're going to have this data to provide the means by which the vendor codes can be assessed. Let me say, this word "assessment" is thrown around these days very widely back and forth, and I don't think we are prepared -- we're not ready yet, I believe, to assess even the advanced codes, much less to assess -- In other words, we are not able to address something that deserves some data assessment, much less something that might not deserve assessment.

We hear these days, for example, that we run Semiscale, LOFT, small-break tests and discover we cannot calculate because we don't have nonequilibrium flow, we don't have suppression, and so on. And these things are coming out in the literature as if it were a complete surprise to us that the vendor codes cannot calculate separation.

Well, what is new? The point is that there are certain things that you know, you expect, they're there, there's not too much you can do about it.

Now you run a test, and if you put that test in the perspective of assessing a tool that did not do the job, then you imply later on when the comparison is made that that tool has failed you in some way. I think that is what I am concerned about. And that is not necessarily the case, because you can use a tool that is not describing in complete perfection every little detail in the system, and still you can get all of the insight that can help you in fact, as we have done for so many years, to reach licensing decisions.

And if you put that tool suddenly into this kind of a perspective and this kind of an overall light, I think that it is a disservice from many points of view -- from the point of view of the tool, from the point of view of the licensing authorities, and from everybody's point of view.

MR. NORTH: I don't think that I meant that full of an assessment of the code in the sense that we've been used to dealing with development of assessment of the advanced codes.

DR. THEOFANOUS: Essentially it implies a comparison, and if your comparisons aren't very good --

DR. PLESSET: He's getting alergic to that word, and I don't blame him.

(Laughter.)

DR. PLESSET: Right?

DR. CATTON: I think the best thing is just not to pay any attention to it.

(Laughter.)

DR. CATTON: I think the interaction between experiment and code development is a very good one.

DR. PLESSET: Yes.

DR. CATTON: However, I do wonder a little bit about the one-dimensional -- what is in essence a one-dimensional facility looking at UHI when as far -- it's my feeling that UHI is going to be multi-dimensional. Now you're going to take your almost one-dimensional experimental results and, through your interaction with the codes, come up with an almost one-dimensional representation of your almost one-dimensional experiment, and now apply it to a multi-dimensional process in a full-scale BWR, and I am a little concerned about resolving that. That's just a comment.

MR. NORTH: I believe it's a valid comment.

DR. PLESSET: Yes, Gary.

MR. JOHNSEN: Gary Johnsen, EG&G.

I would just like to make one point in connection with what Dr. Catton said. That is, that the vendor codes are all one-dimensional.

DR. CATTON: I understand that, too.

MR. JOHNSEN: Consequently, if they can't predict what will happen in the one-dimensional system, they haven't got a chance.

(Laughter.)

DR. CATTON: On the other hand, if they do I still don't know where we are at.

MR. JOHNSEN: But we're closer.

(Laughter.)

DR. THEOFANOUS: But I think you can count on them not being able to predict. I think you can count on that. But the question is, are they predicting important things? I think that's the question, and I think that's where I have problems with it. You can count on not getting good agreement. However, the question is, why the agreement is an acceptable agreement. And I don't think we can even begin to tackle that question. And I think if you don't address that question this way, you're going to end up with experimental data --

DR. PLESSET: Don't say "you," he's not guilty.

(Laughter.)

DR. THEOFANOUS: You will find a disagreement there, and you will look at it --

DR. PLESSET: Harold?

DR. SULLIVAN: I think that we're getting a little confused about the UHI large break, which is a real problem and in trying to get a code to calculate that phenomenon, because there you have lots of subcooled water injecting into a core that has a lot of steam in there and there's a lot of condensation effects. These transients are going to be small breaks, so they're going to be relatively slow. And I would expect the code to do much better at these calculations than they did in trying to calculate the large-break transients.

DR. THEOFANOUS: I hope you're right.

DR. SULLIVAN: I think it is something that needs to be investigated, too. These are certainly more probable, for instance.

DR. CATTON: So you think the small breaks will be much more one-dimensional in character within the core?

DR. SULLIVAN: I even think the plant is going to be much more one-dimensional, and Semiscale is certainly going to be that way.

DR. CATTON: Well, when I think about one of those horizontal lines with the water running back into the core, and the steam coming up out of the core and across and the UHI fluid coming down in the top, that doesn't look very one-dimensional to me.

DR. SULLIVAN: Okay, but the accumulators are at -- what is it?

(Pause.)

DR. PLESSET: He has a point about it's not one-dimensional, but that came up already this morning. Right? And I don't think we can talk about "you," as though it is his fault.

DR. CATTON: Oh, I'm not pointing the finger.

DR. PLESSET: Okay.

MR. NORTH: I have the word "assessment" on several other slides.

(Laughter.)

DR. CATTON: We will ignore it.

MR. NORTH: I would like you to read it as "to investigate."

DR. PLESSET: All right, we'll make that editorial change.

(Slide.)

MR. NORTH: We were talking about high-priority licensing needs. The second one that was identified to us was the behavior of plants during very rapid cooldown decrease in pressure under natural circulation conditions.

Here, one can get a situation in which saturated fluid exists in the upper plenum, and as the pressure is reduced a vapor bubble forms in that region. So there is a need to provide data on bubble formation within the system, and also dissipation. How you get rid of that bubble. So that we can see whether the codes can indeed predict that kind of behavior.

Also, there is a need to investigate within a system the effects of different techniques for pressure reduction if you do get a condition in which that bubble is existent.

The experiment needs are in fact to conduct rapid cooldown integral experiments within our system. We are undertaking preliminary analyses to determine whether we can undertake this kind of experiment with reliability. At the moment we are planning on undertaking such a test, and if it proves that we can do it with some validity then we will go ahead.

(Slide.)

The next item under specific high-priority licensing needs relates to the effects that incondensible gas has on natural circulation which may occur in the primary system. The immediate concern being, can you sustain natural circulation in conditions where you have a degraded core and some incondensible gases present in the primary loop.

This in fact is going to be included in the general research that we'll be conducting on natural circulation, so I want to turn our attention now to the general thermal hydraulic research needs that we will be addressing and the kinds of experiments that we will be undertaking there.

(Slide.)

Here, the general research needs relate to the overall assessment of thermal-hydraulic behavior, rather than particular licensing needs.

(Slide.)

The question you can ask is: If you get a loss of forced convection coolant, no matter whether you have that in the context of a small break or not -- in fact, you can propose it with either one -- how does the system behave? What kind of flows, heat transfer, et cetera, do you get?

This leads us to an examination of natural circulation within a system, either alone or else in association with small breaks, either one.

Again, the objectives are to provide data to allow us to assess the capability to calculate the various natural circulation regimes that might exist, and also the transition from one regime to another and back again as conditions are changed within the primary system.

We also want to investigate the effects of various secondary conditions, pressures and levels within steam generators; and also, as I mentioned previously, what does the presence of incondensible gases in the primary do to the behavior of the primary fluid.

DR. ZUDANS: In this instance in natural circulation, even if you succeeded to calibrate your computer codes to cope with what you observed in Semiscale, do you believe that you could then use those codes in a full-sized power plant?

MR. NORTH: Certainly we're going to learn things out of Semiscale that I think will be profitable, which is the basic thrust of the thing.

DR. ZUDANS: That, I agree.

MR. NORTH: I would be hesitant to say that you can take some model that's been tuned -- and I don't like that word, and I would hope we wouldn't do that -- but if somebody were to tune a model with this result, and then try and run out and stick it on a PWR without understanding the physical phenomenon that he's dealing with --

DR. ZUDANS: I guess the understanding of physical phenomena would really help you to learn how to transfer your tools developed for this model to a real plant. But I have a strong feeling, and I'm not really speaking from personal expertise, that there is nothing you can model as far as natural circulation is concerned, you will have to go to full size.

DR. CATTON: I don't think I would really agree with that.

DR. ZUDANS: Fine. If you disagree with that, it's fine. That's your privilege.

DR. CATTON: There are some parts of natural circulation that you don't need to run a Semiscale to predict --

DR. ZUDANS: That's correct.

DR. CATTON: -- for a single phase.

DR. ZUDANS: You run it in an actual power plant before you start it.

DR. CATTON: It runs as a reflux boiler under nice conditions without too many noncondensibles, gee, I think we can do that, too. It's sort of in concert with the rest of the system --

DR. ZUDANS: I'm not against this thing, because it will teach you something, but don't put too big hopes on it.

MR. NORTH: I'm glad you make those comments, because in fact we have divided the experiments into a couple of different phases. The first of these phases uses a single loop in steady-state.

(Slide.)

What one would normally think of as a broken loop would be blanked out and I would only use the intact loop. Obviously we're talking here about steady-state experiments in attempting to establish a series of steady states, and also watching the transitions between those steady-states.

So these are kind of different experiments to what we would normally conduct in Semiscale. There is a slightly different thrust, and the objective is to establish the various circulation regimes and, again, we want to learn how we can establish those, as well as the conditions under which we establish them within Semiscale. And to see whether there are effects such as hysteresis in terms of the transition from one regime to another, and see whether we have the ability to calculate that behavior. Because I think we need to see whether we can do this in a simple loop before we then try to impose things like a small-break imposed flow and ECC injection on top of something that we would see in this line.

So this is a step-by-step progression in terms of the experiments that we will conduct. And here again we look at the effects of incondensible gases.

(Slide.)

Then in the second step, we plan now to introduce effects such as unbalanced conditions between the loops. With a single loop, you guarantee that you're representing in fact a completely balanced condition. It's a very simple condition. You can learn some fundamental things which you can calculate.

Now you can go in and see if you can unbalance the system by having different pressures or levels in the steam generators, for example. You can also take a look at integral effects that you can conduct transient experiments now doing blowdowns with our without ECCS.

So there is a steady progression in terms of complexity. I anticipate we may modify these plans more in terms of what we have learned in other experiments in terms of what we will do at the end.

(Slide.)

The other general area that we want to undertake research on has to do with the station blackout transient. This is a term that we use. It represents the complete loss of AC and DC power, and the failure of any diesel systems, things like that, to come on for some period of time.

Again, we want to provide data to see whether we can calculate the major phenomena that are associated with this particular transient. We have conducted lead-in type experiments with the MOD3 system along these lines.

In doing these experiments, we have learned some things about how we need to conduct future experiments and how we need to modify the system to make it better for this kind of research, and now we want to go back and redo those kinds of experiments and see if we can get some data to help determine how we can predict these blackout transients, and also to determine the effects of recovery techniques that might be used, assuming the diesels become available at some point during the transient.

At that point, then, what does the operator do to effect the best recover-?

DR. ZUDANS: What would happen in the Semiscale for this particular transient? Could you walk through the scenario?

MR. NORTH: Not exactly. Perhaps Gary Johnsen might be able to, because he has been more familiar with the experiments that we've conducted before.

MR. JOHNSEN: I think I will pass the buck back to Tom Larson who will discuss the lead-in experiments.

MR. NORTH: Okay. That's a good point, because --

DR. ZUDANS: He will be discussing that?

MR. NORTH: He will be discussing these in the following presentation in some more detail. So in fact you will see it in quite a lot of detail.

(Slide.)

Now the fact that we are wanting to conduct these kinds of experiments which are markedly different than the system was preliminarily designed for means that we've got to undertake some system modifications. These are in fact in process right now.

They have several objectives. First of all, if we are going to deal with research where the driving potentials are relatively small, where in fact gravity has provided the driving potential in many cases, then the maintenance or preservation of prototypic elevations within the system becomes much more important.

Consequently, we now have an intact loop steam generator which is a full-height steam generator just like the one we had in the broken loop before. It is scaled on a similar basis.

Also one of the things that we discovered in attempting to conduct the original lead-in experiments in this kind of environment -- that is, the small-break type of environment -- is that we need to pay more attention to the secondary fluid volume, the secondary fluid conditions, for these longer slower transients the secondary fluid conditions become more and more important.

Therefore, in order to help us represent the volume of the secondary fluid, we have put filler pieces in the steam generator. So these steam generators are now dryer, essentially, than they were before.

We also have to pay special attention to what are the experiment boundary conditions.

DR. CATTON: Excuse me. What are the filler pieces made of?

MR. NORTH: Steel. Filler pieces that are put in the secondary side of the steam generator to reduce the fluid volume.

DR. CATTON: Does that shift your amount of sensible heat?

MR. LEACH: They're not solid.

MR. NORTH: They're hollow; right.

DR. CATTON: Okay.

MR. NORTH: They're hollow units. And I don't think that should be a problem.

DR. CATTON: If they're hollow.

MR. NORTH: We do have to pay attention to boundary conditions for these experiments in ways that we did not before. I would like to address the "energy" and "mass" boundary conditions that we now have to attempt to sustain.

Again, with the longer, slower transients with slower driving potentials, small energy losses now become relatively important. This is a classic case of the experimentist dilemma, in a way, because those energy losses occur in Semiscale to a degree which is out of proportion with ones that you would have in a large system, for two quite distinct reasons.

The first is relatively easy to cure. That is, it has to do with the ratio of system surface area to system volume, and you can cure that by insulation and direct approaches like that.

The second is a little bit more difficult, and this is where the experimentist's dilemma comes home to roost. Every time you put an instrument in there which is cold, in order that you get information out of the system, you now perturb the system in a much more serious way than had been done previously because you've got an energy extraction through that instrument. And there are other things, because, for example, our pump cooling is probably much more significant than would occur in a full-scale plant.

So we have to pay attention to energy extraction. We also have to pay attention to how we insulate the plant to try to cure the first problem that I mentioned. So we go into a lot of extra trouble with external insulation, internal insulation, and also surface strap-on heaters.

You will hear, again in the experiments that Mr. Larson will describe, that we have in the past used the core as a way to make up energy in the system. This has its drawbacks. One cannot represent certain behavior as realistic when one does that.

So now we have strap-on or guard heaters and heater tape which is placed on various parts of the system to try to replace energy into the system, or stop it from -- well, essentially that's correct -- replace energy in the system in various quantities.

So we've got to be very careful how we operate these things and how we analyze the results, and understand that. It is an attempt to get over the kinds of restrictions that I have just been describing in terms of these new experiments.

So far as the mass boundary condition is concerned, we have undertaken a fairly aggressive leak prevention program. We have to make the system very tight, because small leaks over a long period of time in an experiment again now becomes significant.

We have gone through all our seals, pumps, and instruments and replaced them all. We have reworked the heater rod seals on the bottom of the vessel. In fact, recently in assembling data we'd gotten to the point we didn't even have a helium leak on that unit. It remains to be seen, when we put the whole system under pressure with hot water in it, whether that kind of quality is retained. But we are paying attention in an aggressive fashion to the mass boundary conditions through a leak-prevention program.

Also, I'm aware that we have a lot of work to do on instrumentation for the particular experiments. We have instrumented the steam generator, and we will be placing instruments in the pantlegs, which are the vertical pipe sections which come down out of the steam generator, and attempting to get appropriate information and instrumentation into these experiments.

So that's our system modification.

DR. CATTON: Do you have the heat-loss problem in hand?

MR. NORTH: Let me answer that in two sections. I can't give you a definitive answer to that until we put the system up and measure and see what we have actually got. I believe that we will have substantially reduced the energy loss over the MOD3 system because of the extra insulation and work that we've done on that.

Until we actually conduct some experiments to determine how best to use the heat, the guard heaters, and whether we need additional insulation on the places yet where we haven't got it, I'll resist the temptation to give you a definitive answer.

I believe we've got it under control, in that we are aware of it and we are working on it aggressively. It is not an inconsiderable problem, incidentally. If you go around and look at all the connections to Semiscale, all the brackets and extensions and bonds that are on that thing, it is a difficult problem and I expect that it will require some more work.

(Slide.)

One of the four items that I mentioned at the beginning of my presentation was that we wanted to make sure that we were coordinated with other programs both here at this lab -- which is relatively easy -- but also elsewhere around this country and in the world.

There will in fact be a meeting next week in Washington, D. C., in which we, with NRC, the representatives from LOFT and Semiscale, will be talking to representatives from Germany and Japan, and I will be in fact going over much of the same material that I have just presented to you indicating where we are going with our experiments, and seeing if we can coordinate them and get better understandings by sharing results out of the various experiment systems.

(Slide.)

So, my main conclusion is --

DR. PLESSET: I might mention that this "ROSA-IV," they sometimes refer to as "LSTF," "large scale test facility," or as "Lon Sun Tong"(?) facility.

(Laughter.)

MR. NORTH: I was unaware of that definition.

I think it is quite clear now that we have a formal, and even an intellectual commitment to small break and transient experiments over and above the exigencies of Three Mile Island.

We have conducted exploratory experiments with our whole system. These are completed. We've learned some things about how to conduct these kinds of experiments, and where we go in the future, and we're applying that.

We have system modifications in process. Those will be completed. The final SO test system operating test will be conducted in the middle of next month, and that is when we will have an operating system again.

System modifications are in process. The experiments that we are proposing to conduct during the next fiscal year we believe are responsive to licensing needs -- high-priority licensing needs. And as Dr. Sullivan pointed out when he made his comments, those needs are in fact focused to us through Licensing, but are in fact a result of the expressions of many people, and the ACRS is considered in that.

We believe that our experiments that we've proposed also address general research needs that are germane and important in terms of the kinds of research where we can have an impact with Semiscale. So we are going to be at pains to coordinate with other experimental programs to make sure we derive the maximum benefit from the experimental efforts and the funds that are expended in the various laboratories.

If you have any questions over and above those you've already asked, I'll be delighted to try and answer them.

DR. PLESSET: Yes?

DR. ZUDANS: I would just like to ask one more. In particular, if you start working on natural circulation and then you will use your surface heaters, you will introduce energy in many different places, isn't that going to create a complete havoc?

MR. NORTH: Have you ever been a lawyer?

(Laughter.)

DR. ZUDANS: No.

MR. NORTH: I'm not sure whether it will create complete havoc. It will influence the boundary conditions, and we must do it carefully. But to stand back for the moment, we've done experiments in which we know we lose energy in a variety of locations and the concern is that these may in fact be induced natural circulation behaviors that are unrealistic unless we attempt to address it in some way.

DR. ZUDANS: Yes, that's correct.

MR. NORTH: Again, all I can say to you at this point: Until we have conducted the initial experiments with the guard heaters, we do have to learn how to apply those guard heaters and how to interpret the results when we get them out.

I am ready to admit that we may have the potential that we can drive the experiment with the guard heaters.

DR. ZUDANS: Certainly in the natural circulation mode.

MR. NORTH: Yes.

DR. ZUDANS: Thank you.

DR. PLESSET: Yes?

MR. LYON: Warren Lyon, NRC.

There is one aspect of the test series and the testing that's coming up that Paul did not touch on that is directly applicable to that question.

After the guard heaters are installed and the system is in complete operation, there will be a period of a month to a month-and-a-half devoted to checking out the interactions of the guard heaters, the heat losses, study of where those heat losses occur, how they are distributed, and pretty much a development of the understanding of the influence of the heaters in exactly the kind of situation you are concerned with.

DR. ZUDANS: Well, it's not going to be easy because you have very many of them, many different locations. There are large chunks of metal next to the heater and all of that, so you have a formidable problem there to say the least.

DR. PLESSET: Well, there's formidable talent there, also.

DR. ZUDANS: There probably is an implication that it will lead you nowhere eventually -- it can lead you nowhere, let's say.

DR. WU: Paul, do you plan to carry out the pre- and post-test calculations for some of these major programs?

MR. NORTH: Yes. We have in the past always carried out pretest analyses, and we will continue to do that. And of course our research doesn't finish at the point that we gather data and ship it off to somebody else. We in fact usually spend a substantial period of time in attempting to analyze behavior in an experiment series, and later put out formal topical reports on that. So definitely we will be attempting to predict the behavior before it occurs; we will be analyzing the behavior afterwards -- not only with the codes, but also hopefully with some grey matter between an ears.

DR. WU: The physics.

MR. NORTH: And try to determine what is going on.

DR. CATTON: Separately?

DR. WU: Physics.

MR. NORTH: As separate from the code, yes.

DR. PLESSET: Well, thank you, Paul. I think we are scheduled to have a break at this point, so let's take a 10-minute break.

(Brief recess.)

DR. PLESSET: I believe we have a presentation by Mr. Johnsen -- Oh, Mr. Larson is going to give it. The floor is yours.

MR. LARSON: Thank you, Mr. Chairman.

Good afternoon. My name is Larson. There was a change in the agenda. I am doing this presentation for Gary Johnsen.

(Slide.)

This afternoon I am going to cover the remainder of the program that Gary did not discuss this morning. He primarily went over the pumps-on and pumps-off results. What I am going to talk about are the remainder of the tests that were conducted in fiscal year 1980.

This includes primarily various small-break tests and also what we term "blackout simulation" -- actually, two blackout simulations.

The test objectives vary from running counterpart tests -- counterpart tests to LOFT, that is -- to running scoping tests, and also in a couple of cases evaluating licensing concerns.

I wish to stress that all of these experiments were conducted in the MOD-3 system. I believe yesterday you gentlemen had an opportunity to view the Semiscale system.

(Slide.)

What you saw was actually the MOD-2A system. This slide depicts the MOD-3 system which everyone here has seen before and I don't wish to belabor the point any except to make it clear that the experiments that I'm going to be discussing were conducted in the MOD-3 system.

Also at this point in time, due to time constraints, I think I am going to refrain from talking about scaling -- which I usually have a tendency to do. That was discussed last year in this meeting, to some extent. I will briefly touch on it in discussing some of the test results, and if there are any questions I would be glad to try and answer them.

(Slide.)

This slide depicts four of the experiments that were conducted in our small-break experime s. Test SB-2, which you may have heard about earlier this morning, was conducted primarily as a counterpart to LOFT test L3-1. The objectives of the experiment were basically to identify any problems that may perhaps occur in the conduct of LOFT test L3-1.

The LOFT people and analysts were worried about such things, for this experiment, of: Can I expect core uncovery? Or do I not need to worry about core uncovery? And if I expect core uncovery, how much do I expect?

So the primary objective was to run this experiment and actually provide them with some input. Also, along the same lines, the experiment was conducted with initial conditions that were similar to audit calculations that were conducted by our code assessment people with the lab equipment. By so doing, we thought we could get some ideas on how well the results from Semiscale compared to these audit calculations.

Now you will see in this first line across the slide here, four experiments. I think a moment ago I said that SB-2 was conducted as the LOFT counterpart test; that's incorrect. Actually, test SB-4 is the LOFT counterpart. The "A"s behind the SB-2.2A and SB-4.4A simply designates that these were tests conducted to help assess what we include for augmenting inner-core power to make up for heat losses. And I will be discussing very briefly the results of those investigations.

Also due to time constraints I am not going to say much more about test SB-2 and SB-2.2A. As far as data is concerned, there were some conclusions that came out of these tests that I think are worth going over at this particular point in time.

As you probably recall from earlier this morning, an additional objective of running these experiments SB-2 and -2.2A, and also -4 and 4A, was to gather similar hydraulic data from a small break experiment in which the break flow rate was larger than the HPIS flow rate. For these experiments, we conducted that at a factor of larger than approximately 2 to 10 over the duration of the transient.

The thought behind running the experiments of this type was to give the system every chance possible to see core uncovery. In other words, we were trying to give the system every chance to uncover the core.

Well, by so doing you might expect that under those circumstances the system would undergo a continuous depressurization, and if the steam generator heat transfer was not an effect then you would expect to see continuous depressurization in the system at this time. No repressurization such as you may expect was seen. Indeed, that is what happened on SB-2 and -2A. The effects of the accumulator injection and the LPIS injection was minimal. There was no uncovery of the core on SB-2. There was a slight uncovery of the core on SB-2A, and we thought that that was primarily due to augmented core power.

We learned some things about modeling heat losses and how important they are to the codes. The overall response of the experiments was at least similar in trend to the results of the audit calculations that were performed here. These were audit calculations performed for a lot of the pressurized water reactors.

As I said earlier, the primary objective of the test SB-4 was to assess the conduct of LOFT test L3-1. By that, I mean LOFT test differences in geometry relative to the PWR that may be expected to influence test results.

We modified our system slightly to try to assess those differences. Now we couldn't modify all the things to make the MOD-3 system look like LOFT, but we did what we could and I'll get to that in a moment.

The test was therefore, in terms of counterpart, test L3-1. As I said earlier, 4-A was the same as 4 except for the power augmented to help us account for heat losses and assess the effects of heat losses.

Test S-TR-1 and S-TR-2 were what we termed "station blackout experiments." I think it is a bit of a misnomer on our part to call them "station blackouts." What they really were were complete loss of AC and DC power simulations. That is different, I think, from the industry jargon of "blackout," but hereafter I will probably refer to them as "blackout experiments."

These were the experiments that I spoke of that were primarily scoping tests. Paul alluded to them earlier. We ran the experiments primarily to help us assess what kind of problems we would be facing in conducting these kinds of experiments. In other words, slow transients, anticipated-type transients in the MOD-2A system. We knew of numerous problems in the scaling nature of the MOD-3 system that would affect the results of this kind of a test; but there are also things that we thought perhaps we didn't do, didn't know, and would be therefore worthwhile to conduct these experiments.

We did get some interesting data, and some surprises. Other useful things that were gained from running these experiments were that we got data for instrument ranging. We also got data and some ideas as far as new instruments that would be required to measure the types of phenomena that we were looking for and trying to measure in these experiments.

We also learned, I think, a great deal in terms of what kind of thermal-hydraulic behavior we would anticipate in modeling.

The last experiment that I will be discussing today is a standard problem test S-07-10D. The original objective of this experiment was quite simply to provide data that would help the NRC in assessing codes that are used in small-break licensing. I think based on the discussion I heard earlier, I've got some slides that you are probably going to be quite interested in because they show some of the results of vendor calculations for this experiment.

There were some additional stipulations on the conduct of S-07-10D, and in fact later on it will become clear why this test has a "D" behind it. The stipulations were that, number one, the core become uncovered during the transient; number two, that a definite mixture level develop in the core, and, in the ultimate, the NRC would like to have had that mixture level sink somewhere below mid-point of the core; the third thing was to get some decent subcooled break flow data for small break. That is something we've not been able to do in the past, although we have tried on an earlier stated problem.

This was a 10 percent break. You might be asking the question: Well, why did you run a 10 percent break? Why not a 2-1/2 percent break?

The answer to that question is: At the point in time when we ran this standard problem -- which the first one was like a year-and-a-half, two years ago -- we did not have the instrumentation, facilities, and the like to be able to get good measurements on a break size smaller than 10 percent. So there was a chronological consideration. And in fact, code calculations said that a 10 percent break would give us the information we would want. Those were the three stipulations.

(Slide.)

I would like to proceed now with a discussion of tests SB-4, and to a lesser extent 4A. This slide simply shows the initial conditions. The only thing to really note here is that the Delta T in the LOFT experiment was set to the 20 degrees K. Nominal Delta T in a PWR at steady-state operations is at about 37K. This is one of the conservatisms that the LOFT people were effecting to try to mitigate the severity of this transient.

We, in trying to run a counterpart test, did the same thing. There are three hardware modifications that I think are worth mentioning here. If you saw LOFT yesterday and you saw Semiscale, you may have realized that they don't look anything alike, really. We have two loops that are both active; LOFT only has one. In fact, for this experiment L3-1, it was not a communicative break; it was a centerline break.

Pump suction in LOFT is different from a scaling standpoint than it is in MOD-3. So what we basically did is modify our pump suction on our intact loop, put the MOD-1 pump suction leg in -- it's shorter in elevation, and by "elevation" I mean the bottom of the pump suction trap, to the cold-leg centerline than is the MOD-3, which is scaled for the PWR.

The second thing we did was to install a valve between our broken-loop pump discharge and the break. What that allowed us to do at steady-state was to close the valve and then essentially after the transient started to have a noncommunicative break, a single-ended break through the cold leg.

The third thing we did was to build an orifice that was of the same design as the LOFT orifice. It's a bell-mouthed orifice that had the same L/D, et cetera.

Now there are a couple of differences between the LOFT system and the MOD-3 system other than geometric size that we could not do anything about. Core length is one of the things. We have a 3.66 meter core; LOFT is 1.66. In the final analysis, that's kind of a conservatism on our part, and I think you will see why in a moment.

Another thing we couldn't do anything about is heat loss. Based on analysis done in the past, our heat loss is considerably larger on a percentage basis than it is in LOFT. That was the reason for running the test SB-4A. Again, we augmented the core power and analyzed what that did to the test results.

Another difference that we couldn't do anything about relates back to the core Delta T here. Ideally what we would have liked to have done in test SB-4 is to scale our core flow rate from LOFT and scale our core power from LOFT. We could not do that; our pumps don't have that capacity. What we did was run our pumps flat out, and reduce our core power to get the right Delta T -- because it was our feeling that core Delta T and core outlet temperature were probably the most important things in running the small break, rather than the initial core level(?).

Well, the first thing that -- the phenomenon that I am going to look at in comparison is: What does the pressure look like?

(Slide.)

If you take a close look at this slide, there are actually three curves on it. There's the LOFT L3-1 result; there's the SB-4 result; and one of the predictions that LOFT made for L3-1, essentially the RELAP4/MOD7 issue. I understand they have RELAP5 figures that look better than this, but for the moment we will just concentrate on the Semiscale curve, the solid line.

What you will see, it is evident that the depressurization with time is continuous; therefore, the new criteria that the break flow rate be larger than the HPIS flow rate was apparently satisfied here, and indeed analysis of the data shows that it was, by as much as a factor of 10 higher.

We did not get any plateaus. The analysis of steam generator heat transfer indicates that the system did not hang on the steam generators, and in fact the break was the dominating thing in depressurization. I believe I've heard John Lienbarger in his analysis of the LOFT test allude to the same thing, that for this break size, 2.5 percent, you really don't need the steam generators to cool down; the break is sufficient to do that.

Just as a point of interest, the saturation was reached in the hot leg at about 40 seconds. The accumulators in the Semiscale system came on at about 550 seconds. At that point in time, on the pressure curve you can't even see the accumulator injection, and in fact when the water all gone and the nitrogen projection was much further out in time, and it had little effect on the system's behavior.

It is pretty obvious, just by looking at that plot, that the Semiscale result agrees pretty well with the LOFT result. Keep in mind that the Semiscale test was run before LOFT. What we did was analyze briefly the Semiscale data, come up with some conclusions on how severe the LOFT test might be, and then feed that back to the LOFT people.

I only show these comparisons here now because at this point in time the LOFT test has obviously been run and you can actually see how good those comparisons are. In fact, the Semiscale result compares better to the LOFT result than does the LOFT pretest prediction.

If one considers all things to be equal in the two systems -- in other words, the heat transfer from the secondary to the primary, and it's not a dominant effect -- you might look at this plot and say: Well, gee, my break flows must be scaled pretty well; perhaps heat loss is not a big thing; and therefore I would expect the mass distribution in time for these two systems to be about the same. And indeed if they are and the mass distribution within the system is the same, then maybe I can conclude that if core uncovery did not occur in Semiscale, then you won't see it in LOFT.

(Slide.)

Well, this slide shows the comparison of the mass distribution or the mass inventory based on 100 percent being the initial steady-state value, with time for the first 600 seconds. That is the important time frame because after 550 seconds the accumulators came on in both systems, refilled the systems, the systems depressurized, and the LPIS came on. So there is very little potential for any kind of core heatup after that point in time.

In fact, analysis of the Semiscale data showed that there was no core heatup even for that period of time before the accumulators came on. In fact, the highest void fraction we ever saw at the outlet of the core was about 50 percent.

So based on this result and the fact that the core did not uncover in the Semiscale, one might conclude that, gee, the LOFT test would be relatively safe in terms of core uncovery; that they should not expect any cladding temperature heatup; and in fact when they ran the experiment, that's what they found.

There is one thing to consider here, though. That is, what do heat losses do to you? We ran an experiment to address that, SB-4A. The manner in which we accounted for heat loss was, as Gary mentioned earlier, simply to increase the core power, and it did in fact affect the thermal-hydraulics in the system fairly substantially.

As you might expect, adding core power to the tune of about 80 kilowatts for the time period between 40 seconds and 600 seconds, gives you more steam generation. The pressure is higher with augmented core power. That makes the break flow higher. That makes the mass inventory less at any point in time. And what that eventually did was allow us to uncover the core in test SB-4A. The core uncovered and stayed uncovered for about 200 seconds.

The accumulators came on at about 770 seconds, and the core was completely recovered by 850 seconds. So you can see that augmenting the core power shifted things in time, and also allowed us to uncover the core.

Now we felt at that point in time that the manner in which we increased the core power to account for heat losses was pretty conservative. We thought it was an outer bound.

The fact that we have a 12-foot core, and only uncovered the top half during that experiment, and in the process of uncovering that top half we had to be rather conservative in making up for heat losses, and we thought that that fact indeed further verified our original result by running SB-4 and that the LOFT test would be rather benign. It probably would not expect a core uncovery. And indeed that is what happened.

There might be a couple of other conclusions that a person would want to draw from this. That is, here is a Semiscale system and a LOFT system. There have been certain scaling laws applied to get from one to the other geometrically, and you have compared some results from counterpart tests which do indeed agree pretty well. That may imply for this particular break size that your scaling is good. I think that is probably partially true, but one still has to consider the effects of heat loss. I hope we will be able to put a definitive answer on this kind of question -- Is our scaling good? Or are there problems with it? -- in the MOD-2A system when, again hopefully, we've got a method whereby we can realistically account for the heat losses and then perhaps do this same sort of thing.

Well, again, the three conclusions that we reached from running this set of experiments was that: The original criteria that you want a continuous depressurization was satisfied by selecting a break size such that the break flow was at least a factor of 10 higher than the HPIS.

We concluded that we didn't expect any core uncovery in LOFT. And as I mentioned earlier, late in time after the accumulators came on, it was evident that LPIS would be of sufficient volume to keep the core covered.

DR. THEOFANOUS: Just a point. When you put additional power in the core to take care of the heat losses, also another effect would be introduced, that you augment level swell in the core. So then you had better cool it. So it can work both ways -- if you put in more power, you get better cooling for a certain portion of the experiment.

MR. LARSON: For certain time periods, you --

DR. THEOFANOUS: For certain periods, right.

MR. LARSON: And we did in fact see that time frames were shifted.

That concludes what I have to say about the 2-1/2 percent break test.

(Slide.)

I would like to proceed on to the standard problem test. I think you will find some of these slides pretty interesting. This slide again just shows the initial conditions. They're basically scaled from steady-state PWR operating conditions -- 37 degrees core Delta T, 2 megawatts, which is our scaled full-power value.

I promised earlier to indicate why this test is called test "D." Again recall that we had three secondary objectives, if you will. They were to, number one, make sure the core uncovered; number two, hopefully uncover the core and develop some sort of a collapsed liquid level that dropped at least to mid-plane or below; and number three, to get some good break flow.

The first time we ran this test, we satisfied none of those objectives, and in fact didn't even get any very good data -- we had some data system problems.

We reran the experiment again and had some other problems with steam generator steam valves, and the like. That's up to "B."

We ran "C" and had some other problems. So that's why we've got this "D." This test was run in fact after all the standard problem participant calculations had been submitted. It was run just last April, in fact. It was at that point in time that I think it officially turned into a standard problem; before that, it was known as a "pseudo-standard problem." That's the reason for the test D.

In fact, on the first experiment we ran, they weren't total wastes. What it really showed us was that on a 10 percent break, all the ECC comes on at the nominal set points and nothing happens. You might have expected that from the 2.5 percent break LOCA is presented and everything works as planned, that nothing happens.

What we eventually had to do on test "D" was delay

ECC initiation. All right, what we did is manually turned the ECC on after the core had uncovered and heated up to some preset level. In fact, because of that, the test wasn't all that exciting. Again, based on the 2.5 percent results, you would expect a continuous depressurization; indeed, that is what happened.

We did satisfy all the test objectives, though, and we got one other piece of information. We did satisfy the standard problem requirements. Number two, we did get some uncovered-core heat transfer data. The problem is with the quality of the data. All it really was able to tell us -- and we've just written a report on this -- is that we were able to determine what it was that we didn't have that we really needed to really answer the problem about uncovered-core heat transfer.

So in some future tests that we're going to run on LTSF, we've factored in this information and we will make use of it.

(Slide.)

I have some comparisons --

DR. THEOFANOUS: Can you be more specific on that?

MR. LARSON: Vapor temperature measurements -- local flow conditions.

DR. THEOFANOUS: And you are going to measure the steam flow?

MR. LARSON: We're going to try in the LTSF system, separate effects, and I hope we're going to do the nine-rod bundle test. We think we would like to have LOCA flow conditions, and I don't think we're going to be able to get them; but we can sure get a vapor temperature measurement.

The problem with tests like S-07-10D are thermocouples in an uncovered state are magnified by rod radiation, conduction losses, and all kinds of things. You just can't put an accurate enough handle on it to back out all those --

DR. THEOFANOUS: So how are you going to do this?

MR. LARSON: There's a couple of different things we're contemplating. One is perhaps the Westinghouse aspirating steam flow. And Ralph Nelson, I believe, is trying John Ginn's technique. Those work nicely. I would hope we would think about using those in these tests that we have proposed for LTSF, the bundle tests.

Before I go to the comparisons, there are a couple of other conditions that are reasonably important. This test was set up so that everything scrammed on pressurizer pressure. The core scrammed on pressurizer pressure; the pumps tripped on pressurizer pressure; and the feedwater valves and the steam valves were controlled on the pressurizer pressure, also.

MR. ETHERINGTON: So there is no distinction between when in your first slide, and then after in the other two?

MR. LARSON: The trip set points were at 12.41 megapasses for all three, so they tripped after that point, essentially right at that point.

(Slide.)

Before we get too tangled up in these comparisons, there are a couple of points I would like to make. That is, the comparisons I am going to show are extremely preliminary in nature. There has not been much analysis done yet as to determine whether or not the comparisons are really valid.

What we've got here are calculations that were conducted by the standard problem participants. They were sent to our code assessment people. They've made the overlays and these are the overlays. There hasn't been any analysis done yet, and we don't know the particulars of any of the calculations, but they're pretty interesting.

There is a draft report in preparation right now that will address some of these comparisons in much more detail than I am, but just for the sake of argument I know there's a lot of interest in how good did the vendor codes do, and how good did TRAC/RELAP do, et cetera. So I think it is worthwhile presenting them.

This slide just shows the comparison of the upper plenum pressure to calculations submitted by INEL and

Los Alamos -- Los Alamos with TRAC-P1A; INEL with RELAP4/MOD6.

The hump you see out at approximately 500 seconds in data, as I mentioned earlier, is the result of turning on the ECC at that point in time. What you see basically is that both calculations follow the trends in the data, but both of them underpredicted the data; and in fact, by and large the same can be said for those calculations submitted by the vendors. Everybody underpredicted the test data.

The obvious result is that, by and large before ECC came on, that depressurization continues. As I said earlier, the hump at 500 seconds is caused by manual activation of the ECC at some specified peak cladding temperature.

One would probably guess that since all of the codes are underpredicting the pressure, that they're probably overpredicting the mass inventory in the system. You might guess, further, that if that's the case then they're probably going to predict cladding temperature heatup too early relative to the data. And indeed, that's exactly what happened, as depicted by here for the TRAC and the RELAP/MOD6 calculations.

Everybody predicted that the cladding would heat up earlier, and only a few predicted that the peak temperatures would be what they really were.

(Slide.)

This next slide shows the same calculation for the vendors. You can see that the Westinghouse calculation was fairly accurate for peak temperature, but again everybody is early in time. That suggests that there's a problem in calculating the break flows, although that's not been verified yet.

There are I think a couple of points to be made about this. One is, if you recall last year at the reactor safety meeting there was some analysis presented on our TMI simulations. The result was that we do not ever see core uncovery in Semiscale until the collapsed level has dropped down at least to mid-plane in the core.

The results from S-07-10D here verify that same thing. The results from more recent small-break experiments verify that also. Simply stated: Until the collapsed level -- at least the decay heat levels -- collapsed level drops down to the mid-plane in the core, you're not going to see any heatup. But once it does, then you start to see heatup in the core and on down.

If we take a peak at the calculations that were done here and look at their collapsed levels and their mixture levels, there doesn't seem to be any uniform relationship between the calculated heatup and the core levels, at least in the calculations. So I think the comparison of the test data and the calculations here has probably got some useful information to be gained.

There is one other point I would like to make before we leave this subject. That is, we ran a similar experiment some years ago as standard problem 4, I think, another small break which was S-02-6, comparisons of all the calculations and the data on that experiment showed that this agreement that was similar in nature to what we see here. I would hope that this information was sufficient to allow people to make some judgments about how good or how bad the codes are, and perhaps do something about it.

DR. THEOFANOUS: Well, that is exactly -- I think I should ask a question now, because that is exactly the kind of thing that I had behind my mind for asking the question earlier.

In this particular case, there it is. You've got it. Now what do you do with it? In contemplating this kind of situation, I asked the question: You were going to run some UHI tests, and you're going to find similar things, so what do you do with that?

I think we keep coming to situations where we figure out some experiments to do, we get some disagreement. Then when we see it, we say, okay. And nothing is being done about it.

So can you tell me specifically, now that we see this disagreement, what it is that this suggests, if anything?

DR. PLESSET: He didn't say "okay," yet.

DR. THEOFANOUS: "Okay" to what?

DR. PLESSET: The situation.

DR. THEOFANOUS: Oh, he did say it, because he said: There it is, and let other people decide what this means.

DR. PLESSET: Oh.

DR. THEOFANOUS: And I think that that's easy enough to say -- let other people decide what it means -- but I would like to hear what you think this means.

DR. PLESSET: Brian is going to venture an opinion.

(Laughter.)

DR. THEOFANOUS: Brian is very virtuous today.

DR. SHERON: I don't want to be volunteering, but I think the question is probably fairly directed to Licensing rather than to Research or Idaho.

DR. PLESSET: He's not going to let them off the hook.

DR. THEOFANOUS: No, it's not directed to Licensing, but please, be my guest.

DR. SHERON: Well, I think, though, that the ultimate responsibility of looking at this data and making some sort of a judgment on it, on the acceptability of vendor models, lies with Licensing.

What we would intend to do with this -- now I should point out that, as Tom said, the report is still in preparation, so we haven't really had any of this data in our hands for any period of time to look at -- is to first try and understand why there are differences. Now one of the key things that has been coming up repeatedly, whenever you try to compare a code prediction to an experiment, is that it all seems to always draw back to "how well did I predict the break flow?" If I miss the break flow, it seems like everything else falls apart.

I have just -- we have been toying considerably, I guess, with the idea of, for example, on the LOFT test L3-6, the possibility of perhaps, rather than just giving the industry a break size, maybe we should give them the actual measure of break flow, to eliminate that uncertainty out of the procedure and let them drive their calculation with measured break flow and then see how well we do -- since break flow, when you look at a licensing calculation, requires that they analyze the spectrum, for the very reason that you just don't know what the break sizes ought to be; you can't divine a break size. For that matter, you can't even -- if you look at it, the break flow is basically a combination of a critical flow times an area, which you can say that looking at a spectrum encompasses both the spectrum of area plus the uncertainty on the critical flow itself -- although it may not be that clear when one is dealing with subcooled versus saturated areas.

That is one way I think we have been looking at trying to get a handle on this. It is very difficult to sit down and just look at those curves and say: Well, obviously the vendors did a lousy prediction, so we ought to slap some penalty on them for their bad calculation.

DR. THEOFANOUS: That's exactly right. That's exactly the reason I raised the question, because if you -- if somebody who I guess is not very much involved with the licensing process looks at calculations like that, they will think that the licensing people are crazy that they license plants.

On the other hand, I share your feeling that the disagreement here is certainly much more than what it really is.

DR. SHERON: And it also makes a very good case for why there's an evaluation at all.

DR. THEOFANOUS: That's right. On the other hand, from another point of view, I want to remind you, Brian, that you already have made your judgment concerning cores. It's not a matter of you making a judgment now. The fact of the matter is, already you have licensed plants, and already the judgment has been made.

Now I think, concerning your introductory remarks, what you were saying really is whether you want to reevaluate your judgments. But the judgment already has been done, and you in fact have approved licensing.

That's why I think the question is more pertinent to the people that in fact are in the business of running tests and exploring, research-wise, surprises, possible surprises, and possible difficulties. And that's why I think the question is more relevant to ask to EG&G rather than to licensing.

I think when you consider that in some depth you will come up with the answer that it is not probably too important in terms of disagreement.

DR. SHERON: We've made a judgment that we believe conservatisms posed by those aspects of Appendix K applicable to small breaks -- which are basically the heat sources and the single-failure criteria -- impose a large enough margin to hopefully bound the uncertainties that we are seeing here.

DR. THEOFANOUS: Yes.

DR. SHERON: And as a matter of fact, there is an item in the Task Action Plan under the 2K-3 items -- I forget the exact number -- but it says that the staff is going to perform an assessment of the uncertainties in small-break calculation methodology, as we understand it today; in which case, information like this would be extremely useful.

And from that, we will try to make a determination whether the conservatisms in Appendix K are sufficient to bound the uncertainties. And we've pointed out that if they're not, if our judgment is that when we finish this assessment that we believe that the uncertainties on small-break analyses are larger than the margin afforded by Appendix K, I believe we said that we would probably try to go to the Commission and ask for some sort of an interim rule to make sure that the licensing calculations indeed would bound the uncertainties.

Now that is in the Task Action Plan. It is an item that is supposed to be picked up, I believe it's starting in FY '81, which we've got now. We do have a Technical Assistance Program identified to start this work, and I'm sure we would be back to the committee later on to report on our progress.

DR. THEOFANOUS: Well, I agree with all that, and I share many of your views, Brian. I think it is very admirable that we can have tests like that, and we can go back and look at them and make adjustments and so on.

However, one thing that I think really bothers me is that -- and unless you've been paying a lot of attention to that -- is that whatever assessments, or whatever considerations you are saying that you are going to be doing now, it seems to me that we would be much better off, all of us, if we had gone through that exercise before those tests, so that we are not in a position where always we are rationalizing disagreements.

We seem to always be coming to the position where we are rationalizing, in a way, disagreements. And I think from what you are describing the process is going to be that. And I think this is a good example where one wouldn't have to rationalize disagreements. I think that if you go through that exercise concerning the break flow, and how actually Licensing does this, taking inputs from the core, and then goes about the process of deciding the acceptability of the plant, if you could go through that, then you wouldn't have -- you wouldn't be in the position now, at least in the eys of many, that you are rationalizing what is apparently a rather -- as we heard a minute ago -- a rather large disagreement.

I believe that you will be able to rationalize it. I think, furthermore, you will be able to foresee. And I would rather see you foreseeing it rather than rationalizing it away after the fact. I think it gives a very bad image, and we keep doing that for many, many years, and I think that at some point we should take the position of acting instead of reacting to codes. And I think that's the way I interpret now what you are telling me when I look. That's my only problem with all that.

MR. RAY: Have these comparisons been made available to the vendors? And have you had any reactions?

MR. LARSON: To my knowledge they have not. As I said, the report is still in draft form.

MR. RAY: It would seem to me, in my own limited background, that there's remarkable agreement in the magnitude of the core heater temperatures between Westinghouse and the two tests. I wonder what the significance of the earlier appearance of the peak is? Does that have any major importance?

MR. LARSON: To me, it does.

MR. RAY: What is it? Can you tell me?

MR. LARSON: The timing is wrong.

MR. RAY: Yes, but does it mean anything physically to the reactor?

DR. PLESSET: It could; we don't know.

MR. LARSON: You worry about timing that temperature, or those sorts of things.

MR. RAY: Well, you don't know yet. Is it a fact that you haven't analyzed it yet, so that you don't know?

MR. LARSON: Well, I'm not in the business of analyzing these, period, like I said earlier, but I would be interested in this.

DR. PLESSET: I think Sullivan can answer that. Go ahead, Harold.

MR. LARSON: There are two more points to make here. One is that these are preliminary comparisons. The people who are working on this have not in fact verified that the calculations are all that consistent with the tests. That's the important thing to keep in mind. It may be that some of the calculations should be redone.

MR. RAY: Excuse me. On this last point that you made, did you consider at all carrying out the test beyond the termination here of the 750 seconds on the possibility that you might see a second peak like the Westinghouse did?

MR. LARSON: In fact, we did carry out one of the tests much beyond that and the LPIS rate was high enough to keep the core covered and we did not see a second peak.

DR. PLESSET: Harold?

DR. SULLIVAN: I would just like to point out that it is of significance, at least to us, and the fact that the models did not predict the -- even though they got the magnitude right, the timing was wrong -- in the fact that the power is at the wrong -- it's at the wrong power at the time that they predicted the temperature, it's at the wrong pressure, also.

DR. PLESSET: Yes.

DR. SULLIVAN: Which means that the level swell is probably out; which means that probably the masses are probably in error. So it is important, we feel.

DR. PLESSET: It is something that I would expect, offhand, that they shouldn't take any pride in that they're "somewhere in the ballpark" of the peak. Because the conditions under which that peak occurs are quite different from the conditions under which the observed peak appears, and I think this doesn't give one confidence at all. And of course Combustion was way off in every respect. Maybe their model is even better than Westinghouse's because it does a little better in other respects. But I think that it doesn't give us reassurance.

MR. LARSON: I alluded to that earlier when I said that if you peaked at the level calculations that the levels calculated in the core-heatup sense calculated are not consistent with the phenomena that caused that to happen in the test.

DR. PLESSET: Yes.

MR. LARSON: Therefore, people are getting the right answers for the wrong reason, and that's not a warm feeling, either.

DR. CATTON: RELAP4 nor TRAC did very well, either, which was the previous slide. I believe those runs were by you people here, weren't they?

MR. LARSON: TRAC was Los Alamos.

DR. CATTON: TRAC was done by Los Alamos? But you ran the RELAP4/MOD7 yourself?

MR. LARSON: I didn't --

DR. CATTON: Well, somebody within your group. That gives one even less comfort.

DR. ZUDANS: If I may, I would like to return back to Gerry's question. I think there is something significant in Westinghouse's being able to produce that shape that is very similar. I do not disagree with the comments that followed the discussion, but there is something good about that model that makes the shape come out the same, even if it's the wrong time. Maybe it's worth investigation to find out what it is that makes it repeat the slope. It raises the same way and drops the same way.

DR. THEOFANOUS: Well, the powers are actually similar, so that it's going to have the right kind of slope. When you lose cooling, it's going to heat up; and then you quench, and it cools it right back down.

DR. PLESSET: Yes, that is to be expected.

DR. CATTON: The accumulators come on.

MR. RAY: Well, getting back to Theo's question, it seems to me that a good thing to consider doing, Brian, would be to tie in with Westinghouse. It might very well be they had a hell of a lot better model than RELAP or TRAC. And if you could establish what it is that makes it come on earlier and they correct that, you may have in your hands a good model.

DR. PLESSET: Well, no, that's not his job to fix up Westinghouse's code.

(Laughter.)

DR. PLESSET: That's not his job.

MR. RAY: No, but the information might very well be communicated to them.

DR. PLESSET: Oh, they'll get the information;

no question about that.

DR. SULLIVAN: There is a formal process that we are going through here. One of the things that you ought to keep in mind, these calculations are approximately two years old. So the standard problem was run a considerable length of time ago. We feel that we have improved our calculations, and I am sure Westinghouse feels the same, and the rest of the vendors also.

The process is that we all decide on an experiment and there are pretest predictions for it, and when the experiment is then completed, at some later date the results are compared to the data, and then there is another meeting with each of the reactor vendors, or everybody that was calculating the results, and they're discussed.

A lot of the times, there are post-test analyses made of those in which the reactor vendors are trying to iterate on their answers to improve their models. And that is where a lot of modeling improvement has taken place in the past. So it isn't a closed issue. It is an open-ended iteration with the reactor vendors, and they are certainly going to be aware of the results as they are presented.

(Slide.)

MR. LARSON: I guess we are ready to go on now to discuss our blackout simulations. I believe that there was a question asked earlier, and I mentioned that I was going to discuss the blackouts, and what the test procedure is, and to walk you basically through the test, and I will indeed do that.

Again I want to clarify that our definition of a "blackout" is "complete loss of AC or DC power." What that means to us is -- what initiates a transient is pump trip core scram, and closure of the steam and feedwater valves, and also the assumption that auxiliary feedwater is not available.

What this also means is that you do not have a PORV that's operable. You would assume that the relief valve on the pressurizer -- not the safety, but the relief valve is electromatic, so that it's not available in a loss-of-power.

So that you are basically allowing the system to do is, at decay heat, boil steam generators dry, start to boil the primary dry until the pressure comes up to the safety set point of the pressurizer, and then start to lose mass from the primary. And then obviously, if you wait long enough, the core is going to uncover and you're going to get a heat level.

We ran two tests. They were similar. As I said earlier, they were scoping tests. We ran one and had some problems and ran another one in an attempt to correct some of the problems.

I will be discussing, at least earlier, primarily the results from the second test. The two are similar, so that's no big deal. Later on, with some code comparisons, I'll be discussing the first test.

The initial conditions, again, are typical of steady-state operation in the reactor. We start decay power in the core after a 3.4 second delay time to simulate rod drop. Feedwater valves close by 5 seconds, again to simulate essentially the valve closure time.

(Slide.)

There are three other things in terms of operating conditions that probably should be mentioned. That is, the primary pumps were tripped to zero speed at 60 seconds. We knew beforehand that we had a problem in the steam generator secondary scaling. You've probably heard a thousand times that our volumes in MOD3 were oversized by a factor of 3 in the intact loop, and something like a thousand in the broken loop. We realized that in the conduct of this test and we therefore before we ran the test said: Okay, we'll allow a scaled volume to boil away in each generator, and then we will manually drain the rest out of the generators. By so doing, we were removing a large heat sink that we thought would do nothing but cause us problems.

We also recognized the heat loss problem and its potential effect, and therefore elected to augment the core power during the transient to sink up to that, at least until the core uncovered. Then we realized we had to go back to decay heat.

(Slide.)

This slide typifies the pressure response to the system throughout the duration of this transient. You can see it's fairly long at 14,000 seconds.

The results in both the tests are characterized by three distinct periods of time. The first is basically from 0 to 5400 seconds -- that's roughly the time period required to boil off the scaled amount of fluid from the secondary side of the steam generator.

The time period from there to the pressure peaks at somewhere around 176 minutes, 10,500 seconds, is what we termed the "repressurization period." In other words, that's the time period when the generators are now dry, the system pressure is just coming up because you're generating voids, you're approaching the pressure at which the safety in the pressurizer is going to open.

Primary boiloff occurred between 10,500 seconds and about 12,000 seconds. Core uncovery started at somewhere around 11,500 seconds. You will see an interesting phenomena somewhere around 12,000 seconds there. You might ask what happened. Well, that is I think depicted fairly well on the next slide, which shows the comparison of saturation temperature and upper plenum vapor temperature.

(Slide.)

Now this thermocouple is situated in a spot that it's not affected by rod radiation, so we feel that this is a fairly good indication of the superheat in the upper plenum.

What you actually see here is that, as I said earlier, the core started to uncover about 12,000 seconds. We got a sustained core uncovery at about 12,100 seconds. After that point in time, we see that the vapor in the upper plenum and in fact in the loops is fairly well superheated, and in fact at somewhere around 12,300 seconds there's 225 degrees of superheat.

We have some thermocouples in the Semiscale system that are silver-soldered in, and 825 degrees K happens to be slightly above the melting point for silver solder. And the effect, what happened, as depicted on the previous slide, was the rapid pressure changes and we had an unscheduled blowdown through the melted TC quirk.

We have since got some better silver solder and we will run some more tests of this type. The punch line is here, and that is: Gee, at that point in time, we were just getting ready to start a recovery procedure. What we were going to do in this experiment is, first, try to do a feed-and-bleed operation; and if that was not successful, then we were going to go and start refilling the steam generators and cool the system down that way. That was one of the objectives of the test.

Well, we didn't realize it, but the message here is that if you can't start any kind of a recovery procedure, there is the potential that during this kind of a transient you can be at high pressure -- high pressure being 16.2 megapasses between 400 psi. The significance -- and you may be at such a point where your structures can't stand it. So there is another limit to worry about other than cladding integrity perhaps. That is, material strength.

You might be wondering how the break size we blewdown to actually compared to the PORV area, and in fact what we blewdown to was about 14 times larger in area than what the PORV area is. So we really can't come to any conclusions about what would have happened if we would have seen that the PORV was unavailable for starting to recovery.

(Slide.)

I would like to turn your attention now to some code predictions that were made in the post-test sense. The timing is going to be shifted relative to test STR-2. That's not really relevant. We did not have an unscheduled blowdown in the test TR-1.

(Slide.)

This slide shows the comparison of calculated and measured upper plenum pressure. There are some obvious differences. I think the trends are the same, but we think that heat loss and heat-loss modeling is a big problem in this kind of transient. We don't think anybody would argue with that.

What happened in the code was that the system dropped down to a particular pressure, and it hung on the steam generator secondaries, and that's why the pressure is flat for that time period from 1000 to about 4000 seconds; whereas, in the system it continued to depressurize, again because of secondary heat losses, and primary heat losses. There were also some problems with secondary heat transfer lodged in the code.

Strangely enough, even though the pressure response is significantly different out at 9000 seconds, the code did predict that the core would start to uncover, and in fact that is the point in time when the core started to uncover in this experiment.

So what it really says is that the integral of the mass out to the safeties in the pressurizers in the code for the time period between 4500 seconds and 9000 seconds was about the same as the mass out from 8000 to 9000 in the test.

What that really says is that there are considerable differences in fluid conditions and calculation relative to the actual data. We are still analyzing that. Again, we're blaming heat losses for a lot of our problems and, again, in the future in the MOD-2A system we hope to run what is a better test, or at least a more representative test with the same sort of thing.

(Slide.)

The next slide just shows a comparison of what the code calculated for the loop flow rates with what we actually measured. Now please keep in mind, when we say "measured" here, but if you know what the uncertainty is, we're really down in the mud with this particular measurement. Although the fact that the code is calculating something in the same ballpark gives you sort of a warm feeling.

The important thing on this slide is that you've got natural circulation even after the steam generators are drained. Now that's not too surprising. There's a Delta T around the system, and you're going to expect natural circulation for awhile even when the generators are dry.

We think our magnitude is affected by our heat loss, and in fact it is interesting that natural circulation increases after the generators are dry. That is primarily because the hot leg is approaching saturation and the void fractions are going up. Note that that is volumetric flow.

(Slide.)

The next slide just shows the comparison of the pressurizer levels calculated and measured. We have to ignore the two panel humps in the data. It's just really a product of all the sense lines are set up -- in fact, they're higher than the actual pressurizer pressure. In general, the code is predicting the right trends in the pressurizer's behavior. An important thing -- an interesting thing to note is that the codes calculated that the system stayed in a single-phase state, whereas in the test it did not. We actually got a bubble in the upper plenum and we did go two-phase, as is obvious from some previous slides. We think the fact that the code did not go two-phase was related to the secondary steam generator. Again, it's something we're looking at.

(Slide.)

In conclusion, all of the experiments we have done over the past year, I think based on results from test SB-4 and -4A, we concluded that LOFT test L3-1 was safe, and it has now been confirmed, and the test was run in fact a long time ago.

We got some warm feelings about scaling, comparing the results from SB-4 and L3-1. We did see, just in the last slide, that large heat losses in Semiscale do apparently affect natural circulation. It's been brought up that that's going to affect things in the future when we're trying to do separate-effects natural circulation. We are aware of that. We're putting the guard heaters on the loops and we hope to run the appropriate test to calibrate the heaters, and hopefully insulate the system and do away with the heat loss problem.

But we know we're not going to do away with it, but we hope we can mitigate it.

(Slide.)

We did see a significant disagreement between the calculations and the standard problem data.

(Slide.)

I think, anyway, that we have provided some good data for code analysis and code improvement. Lastly, our blackout simulations provided us with some surprises. We think that pressure temperature limits on the primary structures are of a concern in that kind of transient. And in the simulations that we're going to do in the MOD-2A system, we hope maybe to be able to put a handle on how much time a person or an operator has before he has to start system recovery to avoid that sort of thing.

That concludes my presentation. I would be happy to try and answer any questions as long as they are not related to S-07-10 and the vendor calculations.

DR. PLESSET: Well, thank you. I think you have handled a lot of the questions already, and we don't need to impose on you.

If there aren't any pressing questions, we will go to some kind of concluding remarks which I will make, and then I will ask for others, if they're available.

Well, I think that you are all aware that the

ACRS for some time has been benevolently inclined toward Semiscale tests. I think that they are going to continue with that sentiment. As a matter of fact, there were statements to the effect that they would like to see a MOD5 Semiscale to be a separate facility, because it would be very helpful, we thought, in connection with a rather different type of system; and that the kind of tests you might get in a MOD5 would help, certainly help in connection with the B&W-type plant, get some familiarity with the ICS, and related problems.

Now I don't know, but it doesn't seem to have stimulated much of a positive reaction in Research. We will still be somewhat favorably inclined to the MOD5. Now this is not related, quite, to Semiscale, but it's a similar kind of thing.

A certain high-placed person in this business, who doesn't feel himself or herself -- I won't specify sex -- highly qualified in technical details, asked me, as not being too highly qualified in maybe being able to give a straightforward answer, why there were so many of these test programs all directed toward pressurized water reactors. You've heard of LOFT, and Semiscale, and LOBI, and UPTF, and so on and so on. What about boiling water reactors? There seems to be not nearly that kind of attention.

So I said: Well, there is a TLTA. But I said, of course it's no good; that the height relationships were all wrong. Well, this was like throwing gasoline on a fire, as you might imagine.

We have made remarks about wanting an improved TLTA and wondered what kind of reaction. We have both Harold Sullivan here, and Brian Sheron, so we can chew on them in the absence of higher persons, to bother about these two things.

Sheron, do you want to make any comment before we let Sullivan defend himself?

(Laughter.)

DR. PLESSET: Or try to.

DR. SHERON: With regard to that remark about the BWRs, I hate to start right in on the PWRs, but we do -- NRR does have the user need letter in preparation right now. As a matter of fact, it actually more than in preparation; it's preparing for signoff.

You're aware that any time we want any large-scale -- or any change in funding or direction in our Research program, we originate what we call a "user need letter," which is a letter from Harold Denton to, I guess, primarily office director to office director, identifying a specific user need.

We have prepared one. As I said, right now it is somewhere before our chief, I believe, for the Semiscale program as a whole.

DR. PLESSET: Oh, yes. That would be very interesting.

DR. SHERON: For the MOD-2A system, which is the one presently upcoming, we have basically identified our testing needs in the form of a table of recommended tests that we would like to see. We have assigned very general priorities in terms of their order of being run -- sort of an A versus B -- but we don't have any outstanding priority which says you have to run this one first, this second, et cetera. But, rather, to ask Research and EG&G to propose a test schedule which basically makes the most optimum utilization of the facility in terms of you don't want to run a small break and then tear it up for a large-break configuration, and then tear it down and put it into a small break again.

So we basically asked them to try and arrange a test matrix guided by our priorities for testing. And I think, as Paul North has indicated up there, they had covered most of them.

I will point out that we have not 100 percent abandoned a large-break in these facilities. There are a number of large-break tests which are, I think, still needed to be run in the area, for example, of UHI large break. That still hasn't been run yet. Also, the repeat of our favorite test 2-07-6, which produced those downcomer oscillations that needs to be repeated.

But again, our emphasis was more in the small-break and operational transient areas, as well as on these more of a degraded condition. For example, in the noncondensibles aspects of steam generator heat transfer one gets significant amounts of noncondensibles associated with the damaged core. How does this in turn affect the primary system coolant?

Now this has all been identified as the first part of the user need letter. The second part identified a need for Research to do a -- a request for Research to do a very definitive study on the MOD5 concept with regard specifically to -- we've identified a number of data needs unique to the B&W-design reactor which we feel cannot be properly addressed by the present Semiscale or LOFT systems, primarily dealing with the effect of vent valves on small break. As I have mentioned, there is not this loop seal clearing process or phenomenon. It just doesn't occur in the B&W plants because of the vent valves. I think this need to be perhaps understood a little better.

The other aspect is what we refer to as the "Michaelson phenomena," which is the collection of steam at the top of the hot leg U-bend or candycane and producing a temporary interruption of natural circulation during certain small breaks, and the repressurization of the primary system during this period produces very wierd behavior in the sense of what an operator sees, as well as to the question of reestablishing natural circulation.

There is also -- additionally, on the secondary side of the B&W plants, the steam generators with their low primary system inventory are extremely sensitive to secondary side upset transients. This was borne out, I guess, by the information from what we call the Tedesco Task Force last spring, which culminated in NUREG-0667, which presented 22 good things to do in B&W plants to try and reduce the sensitivity through secondary side transients.

One of the things -- or some of the recommendations would be very amenable to testing in such a MOD5 facility. We recommended moving the location of the auxiliary feedwater nozzles from the bottom of the steam generator -- I'm sorry, from high up on the steam generator in the lower group configuration to a bottom entry to slow down the cooling rate.

There are a number of other recommendations that could be studied here in such a facility. We have had discussions with RES on the MOD5 facility. I am aware that there are a number of options that could be done. Each has an associated different cost, and also an associated degree of the amount of data one can get from such a facility. In other words, one may want to look at modifying the present Semiscale facility, or a new facility with a different cost and a different degree of data.

That is what we have requested Research to do a definitive study on to come up with some sort of a cost/benefit recommendation on what might be the optimum way to get the data that we're looking for.

DR. PLESSET: Well, that is very interesting. You asked them to make a study, and they could "study" it for a long time.

(Laughter.)

DR. SHERON: I guess what we're really saying is that we are asking Research -- that we've identified some very definitive data needs with regard to B&W plants which we feel we need experimental data for code verification and code assessment purposes. Quite honestly, I think that puts the burden on Research's shoulders to come up with a plan on how to get us that data, and what the best way to do that is.

DR. PLESSET: Okay. I understand. Let me follow along with one more thing before we ask Harold to explain the situation of why he isn't building one right now, MOD5 that is.

What about TLTA? What is your feeling there? Do you feel needs there?

DR. SHERON: Yes. I think the need is there for an upgraded TLTA facility, as well. I don't think any definitive user need letter is presently in the works, although one is planned, I believe.

We also had to keep in mind that there is a finite amount of dollars available to do all this stuff, and when you come into Research and just say: We must have an upgraded TLTA, a B&W Semiscale, and the like, it again probably has to be assessed in the context of the overall program.

Harold can probably discuss this a little better, but within the context that Research has a fixed amount of dollars to spend, there are other programs which obviously may be in the planning stages which would suffer if we say we must have these other two facilities.

Now there are other options available which certainly should be considered. There can be cooperative programs on the idea of FLEC, with some EPRI or B&W, or industry vendor owner groups participation. These are certainly options, just as they are for an upgraded TLTA.

Again, I think that some sort of a need should be looked at by Licensing for an upgraded facility, and I guess based on the number of factors, if it's warranted, a user need letter should start to be prepared -- although, again, it is something that does require a lot of consideration on our part, first, prior to sending someone off on a giant study to see what is feasible.

DR. PLESSET: Okay. Well, Harold, do you want to make a brief comment?

DR. SULLIVAN: It might not be really brief, but I will try to keep it that way.

Currently, as you well know, there are a number of issues that are open in Research. There is the "new BWR facility," and I would like to bring you up-to-date. The present TLTA is now completely tested --

DR. PLESSET: It's finished, then?

DR. SULLIVAN: We have finished testing in that facility. We have asked GE to give us a cost estimate of what they can do within the current contract dollars that we have with them. There is money left in the contract to do more testing, or to do a modification to the facility. We are pursuing that with them.

Of course, it is a three-party agreement. All three parties have to agree on the direction that we intend to take. Our preliminary assessment of what could be done within the current dollar range that's in the contract is that we would -- probably wouldn't support that option, because we believe that we would like to have a much upgraded facility, and we don't believe there's enough money left in the contract to do that.

So if we went that route, it would be a new contract with the three parties, or that we would have to go to some other option. That is the first one.

Now let me get back to what I would just like to cover in general a little bit. There is the MOD5 facility. You know that there were a lot of questions about the cost/benefit of doing that system, and we are still pursuing that.

The third area that we are concerned about is the continued testing in the current Semiscale facility, which is roughly the Westinghouse design. We are trying to weigh all three of those options off to see the best way to spend the funding that we have.

Now let me go back to the BWR. Since we talked to you last, there has been a meeting that we held in Bethesda in which we called GE in, the utilities were invited, NRR was there, the lab here was there, and we had some people do some studies for us to try to identify the areas that we should do research in if we had a new facility, to try to define what the new facility should look like.

We came out of that meeting with a lot of the problems that are in BWRs cannot be addressed by an electrically heated experiment, or it would be very difficult. There are things like the containment aspect; the power and the effect of power -- the void effect on the power in the coupling of the fluid to the power. That is one of the things that we think the facility would be very limited in, and we would be trying to program to power in -- particularly for transients that have overpower, that is predicted to be overpower.

We also went through the multi-bundle option. Basically, we came up with that we have a set of requirements for the new facility, and we can go through those with you at some later time. I just don't have it with me.

DR. PLESSET: Okay.

DR. SULLIVAN: But basically it was large breaks, small breaks, and transients. There are a number of transients we thought we could address.

So -- and we also decided that the multi-bundle option was probably not worth pursuing because of the work that the Japanese have done, and we understand that -- from you, today, that they have a two-bundle full-height facility.

So that is about where we stand on that option.

Looking now --

DR. PLESSET: Before you leave that, Harold, we are having a meeting in San Jose in December. Maybe you might be able to give us a presentation on this subject. Would that be too soon?

DR. SULLIVAN: We are to receive the cost estimates for -- or what they could do with the steam within the current contract dollars, and it is supposed to be the end of this month -- in fact, it is supposed to be at the same time the water reaction safety meeting. So it looks like that we would be able to tell you at least whether we're going to have a new contract or not, or what the option is.

DR. PLESSET: All right. Well, that would be very welcome, if that is convenient for you.

DR. SULLIVAN: Of course we also have, following several other possibilities, as you know I&L has presented us with a proposal to build that same facility here. The cost estimates are a lot higher. Basically it's in two areas, we think.

One is, the cost-sharing ratio is -- we're paying for it all if it's built here, unless we can get EPRI to come in with us, and that is a possibility.

DR. PLESSET: Yes.

DR. SULLIVAN: The facility seemed to be a lot bigger. The instrumentation was better. The facility was designed to be a more permanent facility than the one we're looking at at GE. So there are several options that we are trying to pursue.

We need to either close out the present contract that we have with them, or extend it, or decide to go some new option.

Looking at the --

DR. CATTON: Harold, was the possibility of taking advantage of what I see as three different requirements in one place considered? The fact that you would have the MOD5, your current Westinghouse, and the new BWR facility -- if it was in one location, couldn't you make instrumentation common, power supplies common --

DR. SULLIVAN: We said --

DR. CATTON: -- and then in that sense come up with a more cost-effective program?

DR. SULLIVAN: We certainly looked at that in the case of the MOD5 system. We have just about decided -- and, Larry, you might want to comment on this -- but there is a space limitation where the Semiscale facility is now. There is another pit that is right beside it.

DR. CATTON: That's what I thought.

DR. SULLIVAN: So we could build something in that other pit, I guess, so that we would have two facilities. But you went to the BWR, you would need three.

DR. CATTON: Dig another hole.

(Laughter.)

DR. SULLIVAN: I think another hole would be very expensive.

Well, that was the BWR. The MOD5 system, you know that we have started the design work on the MOD5 system. Due to funding limitations, we have stopped that. That does not mean that we are not actively pursuing that effort. In fact, we have also had a meeting that Brian Sheron attended, and several other people, in which we again discussed the options and the cost/benefit for doing it.

Out of that meeting, we decided that if we did it, it probably ought to be done right, and we were talking about a single-loop facility, a separate-effects experiment, and we wanted to go and have the full representation with the integrated control system, and that was of course the more expensive way to go also.

So we are generating an in-house memo to the office director now to specify what we have learned, and what we think that it's going to cost. At his request, we have also pursued alternate funding sources, both within the US and outside of the US, to help with the funding of that.

A lot of this does not look very promising on the alternate funding, but it is a definite -- I think if we say that there is a definite need for it, then I think the funding issue might clear up also in the alternate funding area. So we have pursued it. It is not a closed issue at all.

So we are going to present these three areas to the office director, and I think that sometime in the near future we will be able to tell you a lot more about the areas. We have taken your comments to heart about the BWR area, and that does seem to be one of the areas that the Research is definitely slanting to the PWR side, and we are concerned about it also -- particularly since we are looking at maybe closing out the TLTA facility.

That would make it even more lopsided, so we are concerned about that. That is one of the primary things we are weighing in trying to make a decision on which of these options should we follow, and how should we spread the funding that we have.

DR. PLESSET: Well, that helps a great deal, Harold. And if you feel that it is suitable and helpful to give us an uptdate at our December meeting, we will make a place for it on the agenda. That meeting in San Jose is December 10 and 11. So you might make a note of that, and we would welcome hearing about it, because we are, as you know, very much interested.

Well, I think that we have a day tomorrow which is a little bit shortened because of transportation problems, and I would like for you all to be very fresh and suscinct and alert tomorrow. And until then, let's recess.

(Whereupon, at 5:14 p.m., the meeting was adjourned, to reconvene at 8:30 a.m., Thursday, October 23, 1980.)

* * *

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

ACRS - Sub on Emergency Core Cooling Systems

in the matter of: To discuss Semiscale and Loft Programs and Plans for those programs

Date of Proceeding: Oct 22, 1980

Docket Number:

Place of Proceeding: Idaho Falls, Idaho

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

Jane N. Beach

Official Reporter (Typed)

Jane N. Beach

Official Reporter (Signature)

REACTOR COOLANT
PUMP TRIP DURING
LOCAs

PRESENTATION FOR ACRS ECCS
SUBCOMMITTEE MEETING
OCT. 22-23, 1980

Sheron #1

BRIEF BACKGROUND AND HISTORY

- o AFTER TMI-2 ACCIDENT, IT BECAME APPARENT THAT PLANT OPERATORS HAD NEVER BEEN GIVEN SPECIFIC GUIDANCE ON PUMP OPERATION DURING LOCA'S. (EXCEPTION WAS W WHICH INSTRUCTED OPERATORS TO TRIP PUMPS IMMEDIATELY)

- o PREVIOUS SENSITIVITY STUDIES REQUIRED BY II.3 OF APPENDIX K ADDRESSED EFFECT OF PUMP OPERATION ON LARGE BREAKS - SHOWED PUMPS TRIPPED ASSUMPTION GENERALLY TO BE WORST CASE. THIS WAS ALSO CONSISTENT WITH ASSUMPTION OF SIMULTANEOUS LOSS OF OFFSITE POWER.

- o SMALL BREAKS NOT EXAMINED IN SAME DETAIL AS LARGE BREAKS SINCE
  - SMALL BREAKS USUALLY NOT LIMITING
  - EARLY PUMP TRIP WAS ALSO ASSUMED WORST-CASE FOR SMALL BREAKS

- o STAFF ISSUED LETTER TO VENDORS ON JUNE 5, 1979 REQUESTING ANALYSES TO ADDRESS VARIOUS SMALL BREAK ISSUES, INCLUDING EFFECT OF DELAYED RCP TRIP ON SBLOCA.

- o B&W PRESENTED PRELIMINARY RESULTS TO STAFF IN EARLY JULY, 1979, WHICH SHOWED THERE WAS A SPECTRUM OF BREAK SIZES, LOCATIONS, AND PUMP TRIP DELAY TIMES IN WHICH THE PCT WAS ESTIMATED TO EXCEED 2200°F. MODEL USED WAS EVALUATION MODEL BUT WITH 2 HPI TRAINS AVAILABLE.

- o STAFF NOTIFIED W AND CE TO SEE IF THEY HAD SAME PROBLEM. THEY TOLD STAFF THAT PROBLEM WAS APPLICABLE.

- o B&W ISSUED LETTER TO ALL ITS CUSTOMERS ON JULY 20, 1979 RECOMMENDING RCP TRIP ON ESFAS ACTUATION ON LOW RC PRESSURE.

#2

- This conflicted with early IE Bulletin that said pumps should be left running.

- Staff accepted vendor recommendations and issued Bulletins 79-05C and 79-06C on July 23, 1979 to trip pumps.

- Staff performed followup evaluation in NUREG-0623, providing basis for bulletins. The staff recommended that the pump trip be automatic, since all of the vendors stated pump trip was necessary.

- Key conclusion of NUREG-0623 was that flow regime model assumptions among the three PWR vendors were mutually conflicting.

- Based on ACRS recommendation, staff agreed to restudy criteria for RCP trip, including need for automatic trip.

- Staff reassessment and need for predictions of LOFT L3-6 Documented in Task Action Plan (item 2.K.3.5)

- Staff reassessment included evaluation of capability of vendor ECCS models to properly (i.e., best estimate) predict plant behavior during SBLOCAS with pumps running.

- Staff issued letter April 15, 1980 requesting all holders of approved ECCS models to predict LOFT Test L3-6.

- Staff met with industry representatives in May, 1980 to discuss status of pump trip issue, receive briefing by EG&G on Semiscale tests, and express comments, suggestions, concerns on proposed LOFT tests.

- On June 26, 1980, staff issued letter to all holders of approved ECCS models allowing "blind" post-test analysis of L3-6 using actual test conditions. Requested that models to be used for L3-6 be documented with staff by Dec. 3, 1980.

- L3-6 scheduled to be run on or before Dec 17, 1980, but not before Dec 3, 1980.

#4

RES SUPPORT

- o In July, 1979, staff met with RES to discuss research support to NRR on SBLOCA licensing issues.
- o NRR requested scoping tests in Semiscale, provided heat losses could be properly quantified.
- o RES proposed LOFT tests L3-5, L3-6.
- o RES (EG&G) ran three SBLOCA tests for pumps on/off problem.
- o Provided analyses (RELAP) of tests.

OVERVIEW OF PHENOMENA AS UNDERSTOOD TODAY

o FOR SMALL BREAKS IN THE COLD LEG DISCHARGE PIPING WITH THE PUMPS TRIPPED EARLY,

- SYSTEM WILL DRAIN TO LOOP SEAL ELEVATION*
- ONCE STEAM CAN EXIT BREAK VIA PATH FROM CORE THROUGH STEAM GENERATORS, ENHANCED DEPRESSURIZATION BEGINS - ECC ADDITION INCREASES SIGNIFICANTLY - INVENTORY RECOVERS.

o FOR SMALL BREAKS IN COLD LEG DISCHARGE PIPING WITH THE PUMPS RUNNING

- SYSTEM WILL INITIALLY BEHAVE SIMILAR TO CASE WITH PUMPS ,RIPPED
- NO LOOP SEAL CLEARING PHENOMENA, NO DISTINCT TRANSITION OF BREAK FLOW FROM LOW QUALITY TO HIGH QUALITY. NO DISTINCT DECREASE IN MASS LOSS FROM SYSTEM.

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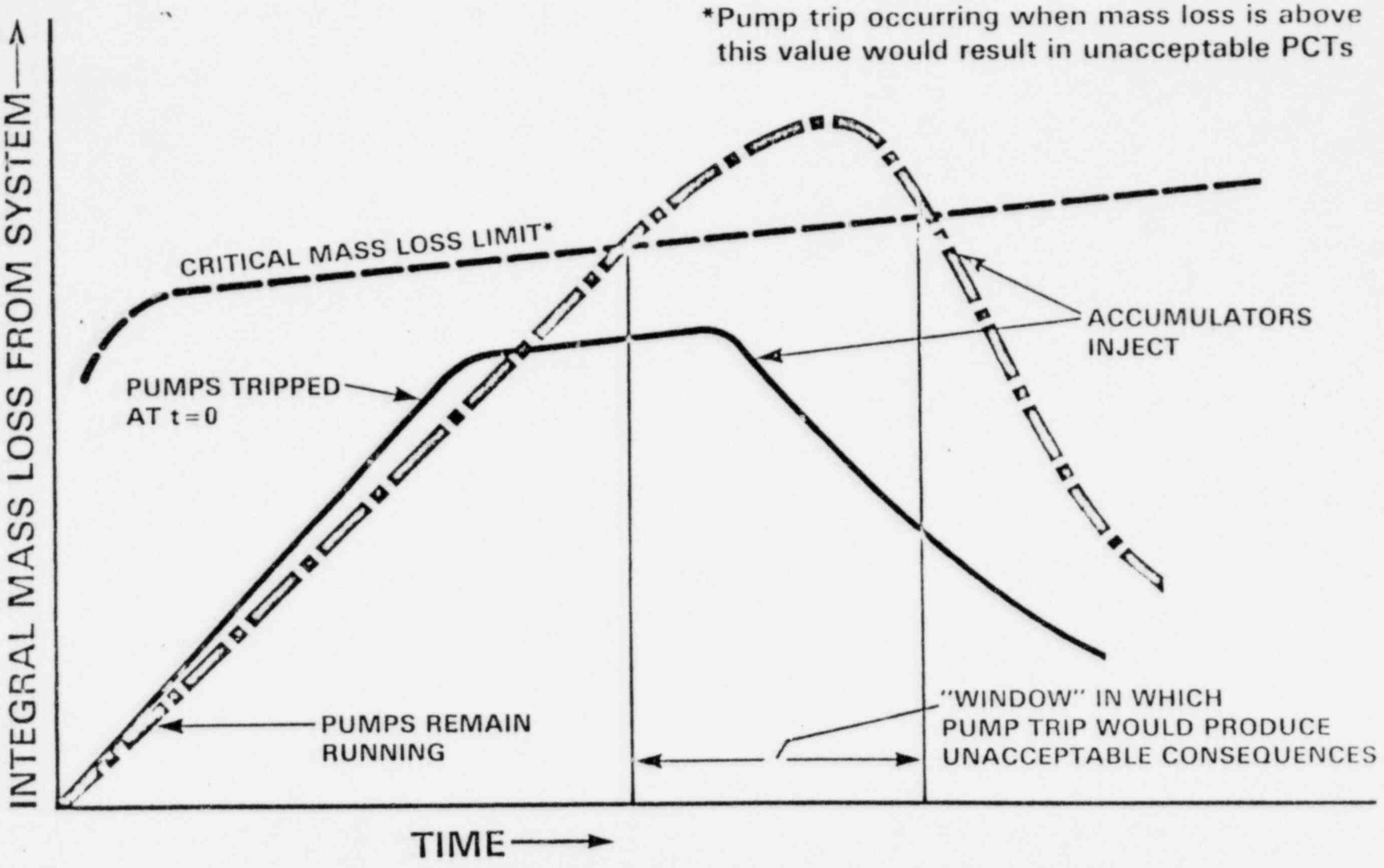
* FOR W AND CE DESIGNS. VENT VALVE OPERATION FOR B&W PLANTS PRECLUDES LOOP-SEAL CLEARING PHENOMENON.

#6



SYSTEM INVENTORY BEHAVIOR DURING SMALL-BREAK LOCAS WITH AND WITHOUT RC PUMP OPERATION

#7

PUMPS ON/PUMPS OFF ISSUE

RESEARCH PROGRAM

- EXPERIMENTAL PROGRAMS
  - SEMISCALE EXPERIMENTS
  - LOFT EXPERIMENTS
- ANALYSIS PROGRAM
  - NRC CODE
  - VENDOR REQUIRED ANALYSIS OF LOFT L3-6
- CONCLUSIONS

Sullivan #1

PUMPS OFF/PUMPS ON EXPERIMENTS

PURPOSE

- PROVIDE A EXPERIMENTAL DATA BASE FOR CODE ASSESSMENT
- UNDERSTANDING OF PHENOMENA INVOLVED
  - EFFECT OF PUMPS
  - CORE LEVEL SWELL
  - BREAK FLOW
  - TWO-PHASE FLOW IN HOT LEGS
- EXPERIMENTAL DATA BASE FOR VENDOR CODE ASSESSMENT
- EXPERIMENTS IN DIFFERENT SCALE SYSTEM

PUMPS ON/PUMPS OFF EXPERIMENTS

LOFT - L3-5 PUMPS OFF EXPERIMENT
- L3-6 PUMPS ON EXPERIMENT

SEMISCALE - PUMPS OFF
- PUMPS ON
- PUMPS TRIPPED DURING TRANSIENT

#3

PUMPS OFF/PUMPS ON

ANALYSIS EFFORT PURPOSE

- PROVIDE NRC CODE ASSESSMENT
- UNDERSTANDING OF ABILITY OF ANALYTICAL MODEL TO PREDICT EXPERIMENT
- FURTHER UNDERSTANDING OF SCALING
- CODE ALLOWS THE EVALUATION OF PWR SYSTEM EFFECTS

#4

PUMPS ON/PUMPS OFF

ANALYSIS EFFORT

- PRE AND POST-TEST ANALYSIS OF SEMISCALE
- MODIFICATION TO PUMP MODEL DEGRADATION MODEL
- PRETEST PREDICTIONS FOR LOFT EXPERIMENTS
- COMPARISONS OF LOFT AND SEMISCALE PREDICTIONS
- AFTER CODE ASSESSMENT ANALYSIS OF PWR
- CODE USED: RELAP4
  RELAP5
  TRAC

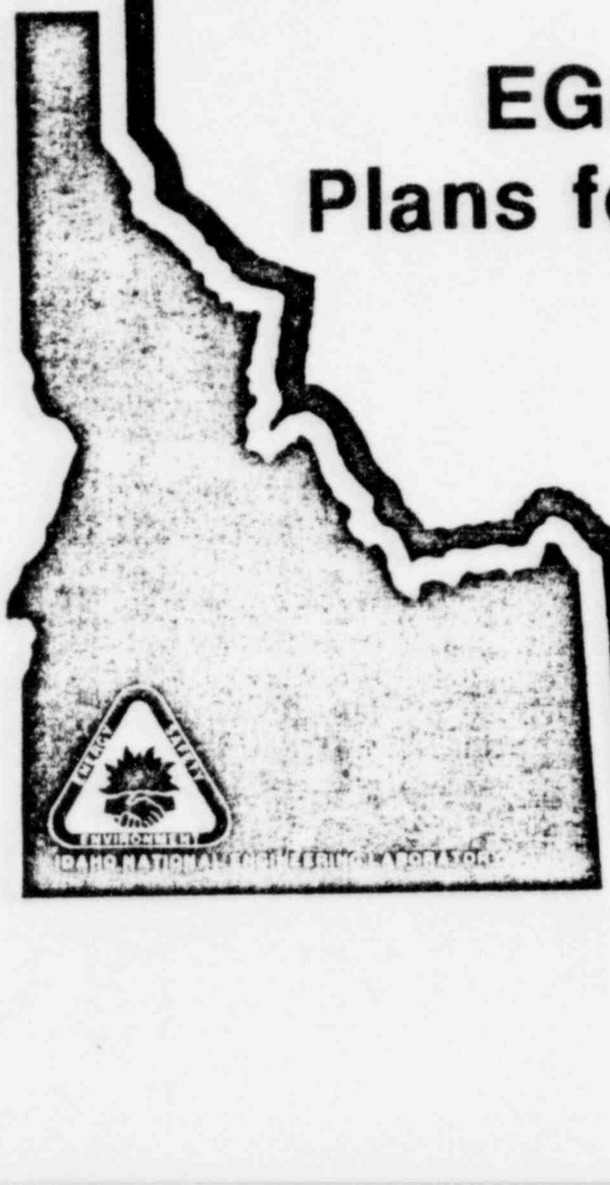
#5

CONCLUSIONS

- SIX SEMISCALE PUMPS ON/PUMPS OFF EXPERIMENTS

  EXPERIMENTS COMPLETED
- LOFT L3-5 PUMPS OFF EXPERIMENT COMPLETED
- LOFT L3-6 PUMPS ON EXPERIMENT IS A VENDOR REQUIRED PROBLEM
- ANALYSIS OF ALL EXPERIMENTS COMPLETED WITH NRC CODES HAS BEEN PERFORMED
- CODE HAS ABILITY TO PREDICT DATA TRENDS

#6



EG&G Idaho Analysis Plans for Pumps On/Off Issue

by

L.P. Leach

EG&G Idaho, Inc

Leach . #1

Objective of Analysis Effort

1. Resolve specific modeling issues raised in NUREG-0623
2. Select optimum model for PWR based on Semiscale and LOFT test/analysis results
3. Evaluate optimum model by comparison to Semiscale/LOFT tests
4. Predict PWR behavior

Optimum Model Calculations

1. Semiscale Tests S-SB-P1, P7, P3, P4
2. LOFT Tests L3-5, 6
3. Four-loop PWR - 16 cases
    - Hot and cold leg breaks
    - Pumps on/off
    - 0.5% (2 in.), 1% (3 in.), 5% (6 in.), 10% (9 in.) break sizes

#3

Issues Not Addressed in Calculations

1. ECC location relative to break
2. Location of break around pipe
3. PWR design differences
4. Explicit treatment of intermediate pump trip
5. Alternative ECC system availability

#4

Schedule for Pumps On/Off Analysis

Release of RELAP5/MOD1	November 17, 1980
LOFT Test L3-6	December 1, 1980
Optimum model completion	December 21, 1980
Complete Semiscale and LOFT CL calculations	February 15, 1981
Complete PWR calculations	May 1, 1981
Complete reports	August 1, 1981

#5

RESULTS OF SEMISCALE
PUMPS ON/OFF
EXPERIMENTS

BY

G. W. JOHNSEN

ACRS MEETING
IDAHO FALLS, ID
OCTOBER 22, 1980

EG&G Idaho, Inc.

Johnsen #1

OUTLINE

- OBJECTIVES AND TEST DESCRIPTION
- TEST RESULTS AND INTERPRETATION
- CODE ANALYSIS AND RESULTS
- CONCLUSIONS

TEST OBJECTIVES

ASSIST IN THE RESOLUTION OF

NUREG-0623 ISSUES:

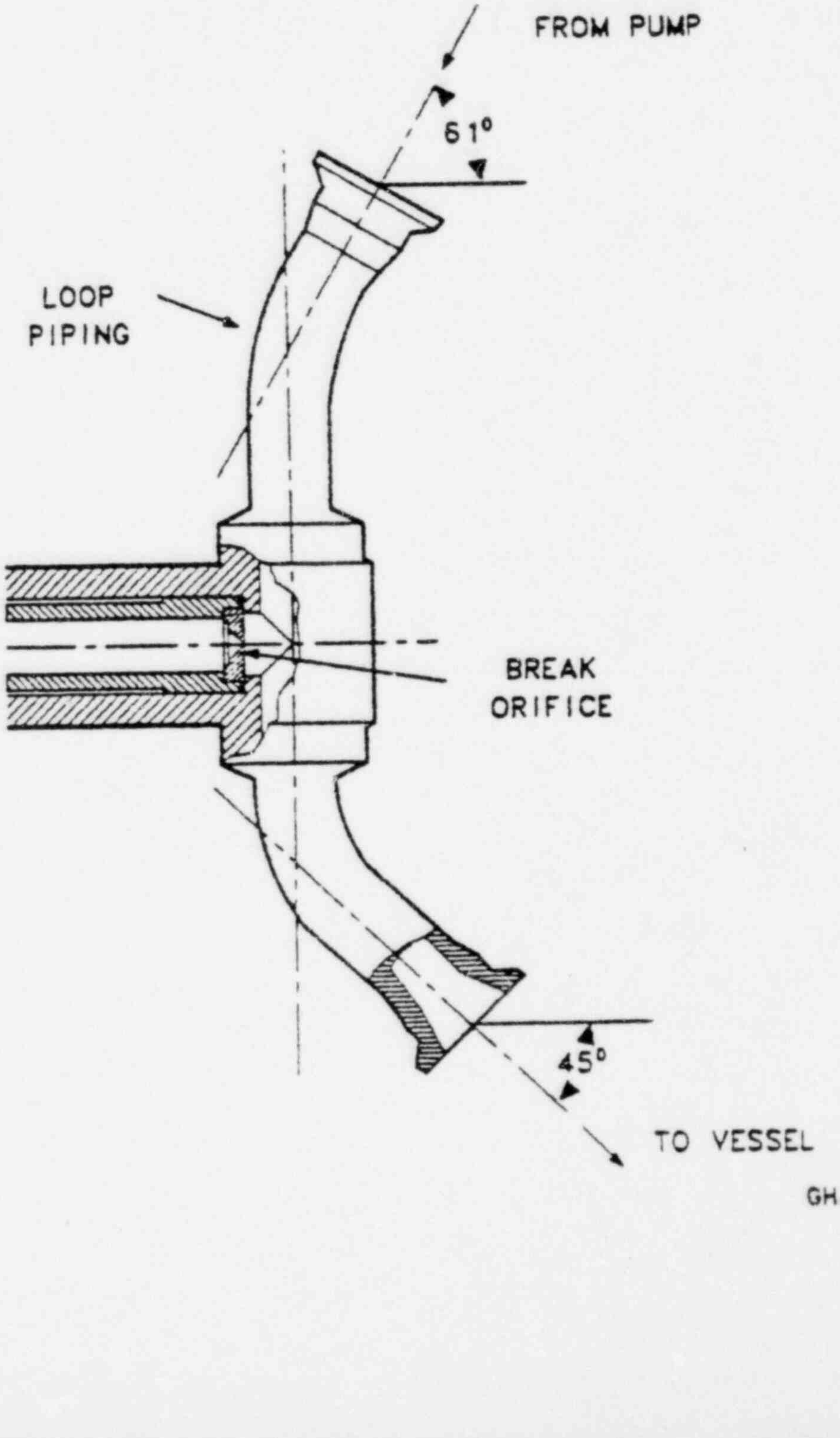
- DETERMINE THE DIFFERENTIAL RESPONSE CAUSED BY CONTINUOUS PUMP OPERATION VERSUS EARLY PUMP TRIP DURING A SMALL BREAK
- PROVIDE RELEVANT INTEGRAL SYSTEM DATA TO ENABLE ASSESSMENT OF COMPUTER CODES

TEST MATRIX

TEST	BREAK/LOCATION	PUMP OPERATION
S-SB-P1	2.5% COLD LEG	TRIP AT SCRAM
S-SB-P2		CONTINUOUS
S-SB-P7		TRIP AT 3.3 MPa
S-SB-P3	2.5% HOT LEG	TRIP AT SCRAM
S-SB-P4		CONTINUOUS
S-SB-P6		TRIP AT 3.3 MPa

GHJ-3

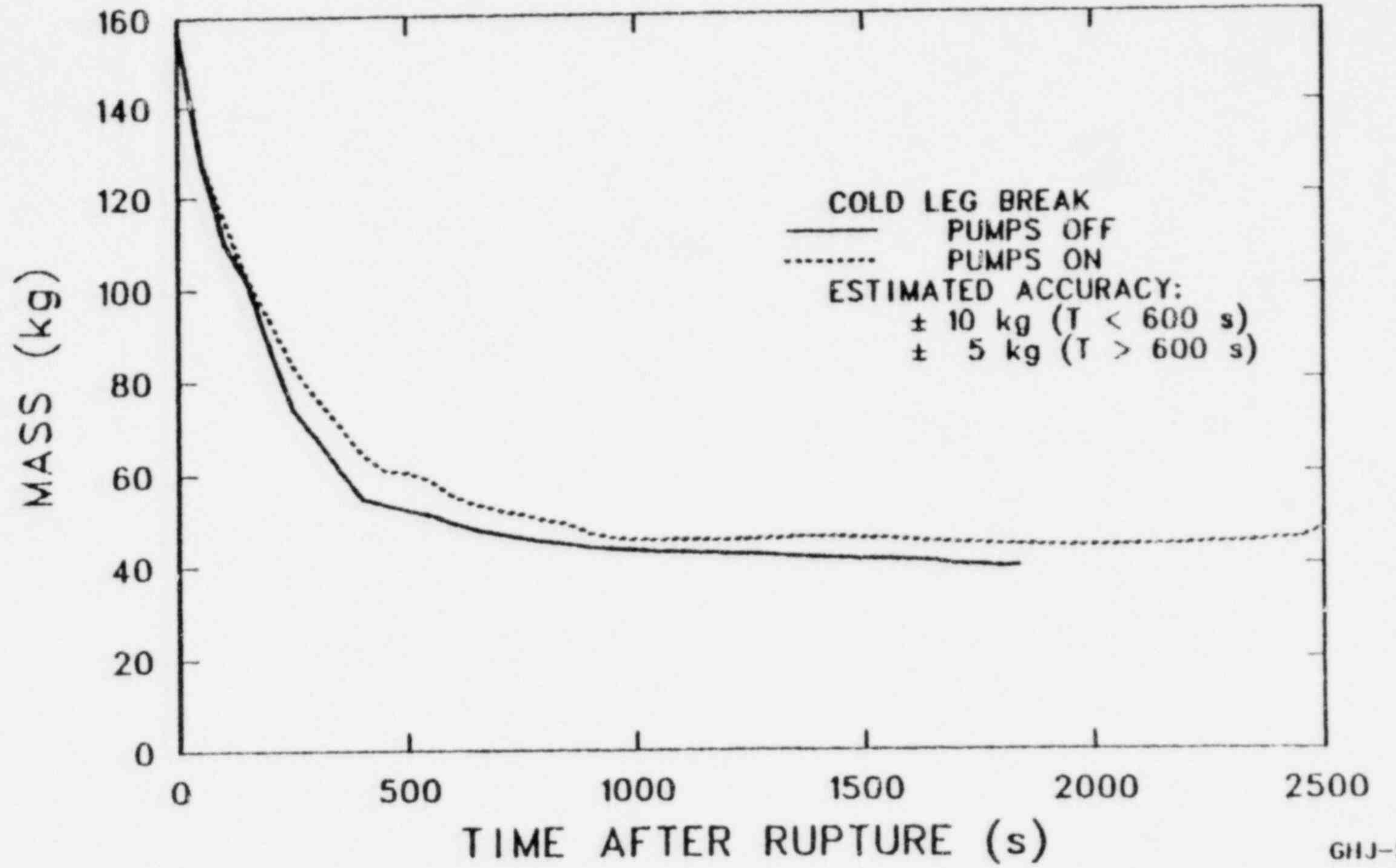
COLD LEG BREAK CONFIGURATION - PLAN VIEW



GHJ-4

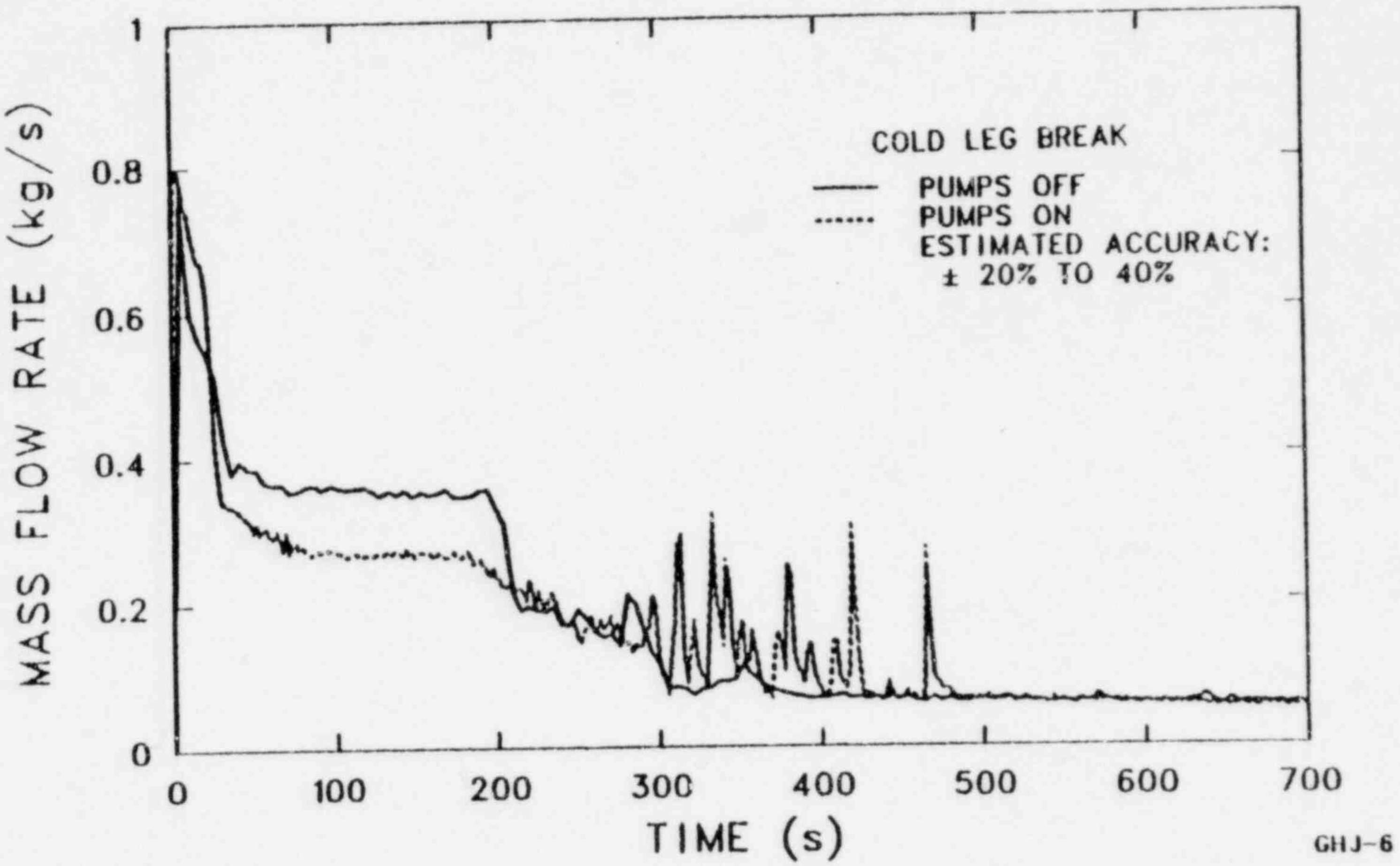
5

SYSTEM MASS INVENTORY

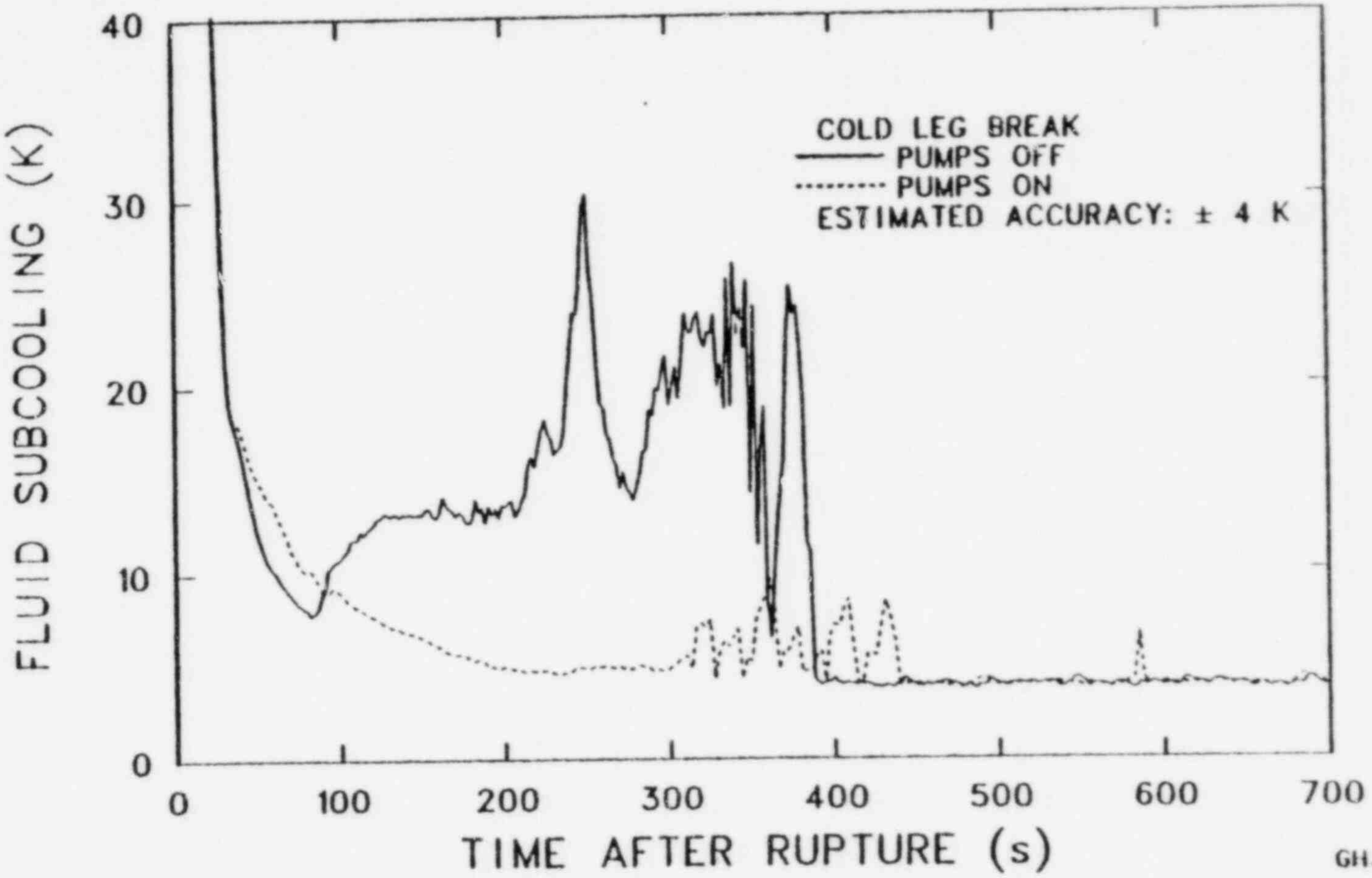


GHJ-5

BREAK FLOW

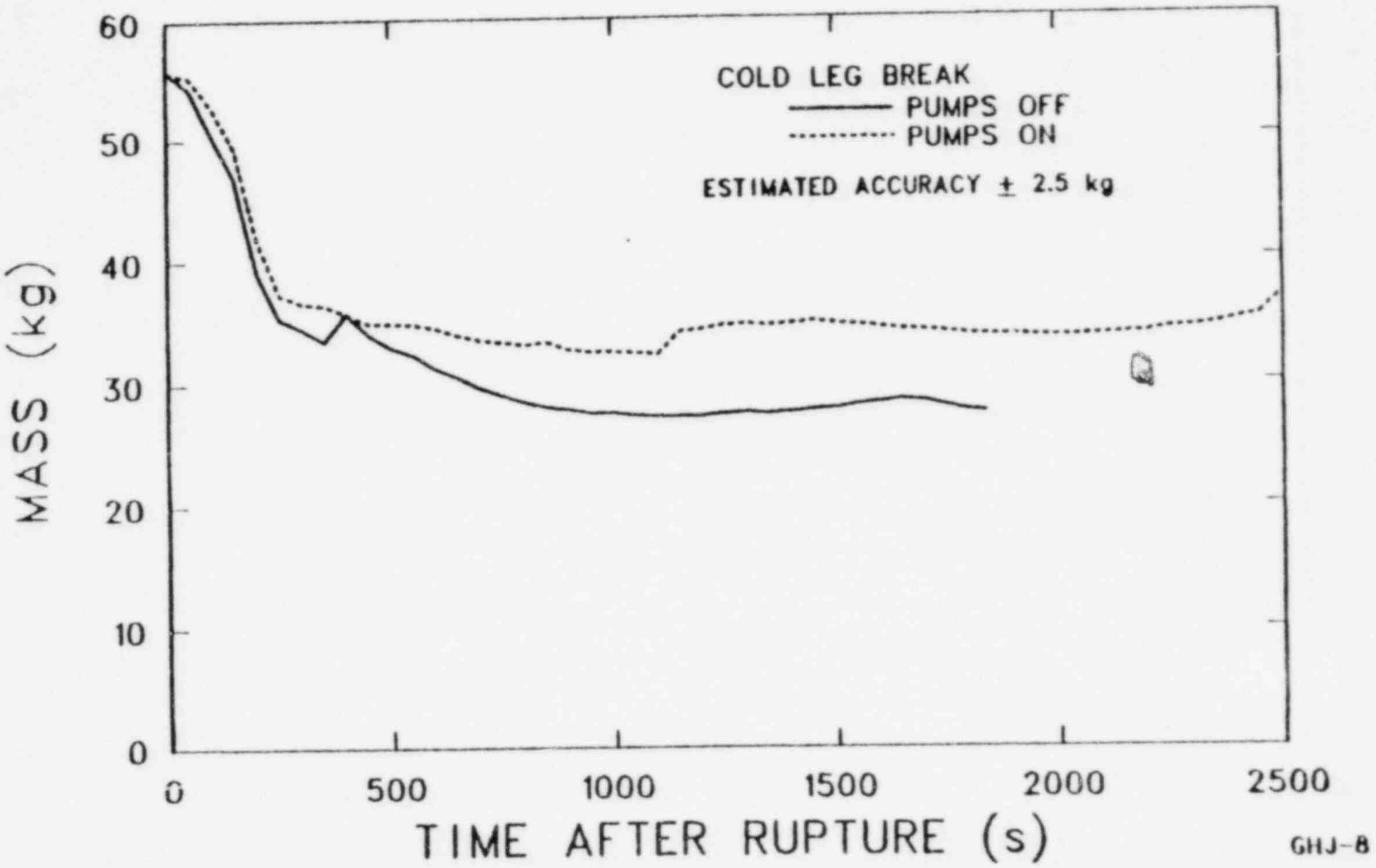

MASS FLOW RATE (kg/s)
1
0.8
0.6
0.4
0.2
0
0
100
200
300
400
500
600
700
TIME (s)
COLD LEG BREAK
PUMPS OFF
PUMPS ON
ESTIMATED ACCURACY:
± 20% TO 40%
GHJ-6

FLUID SUBCOOLING IN BROKEN COLD LEG

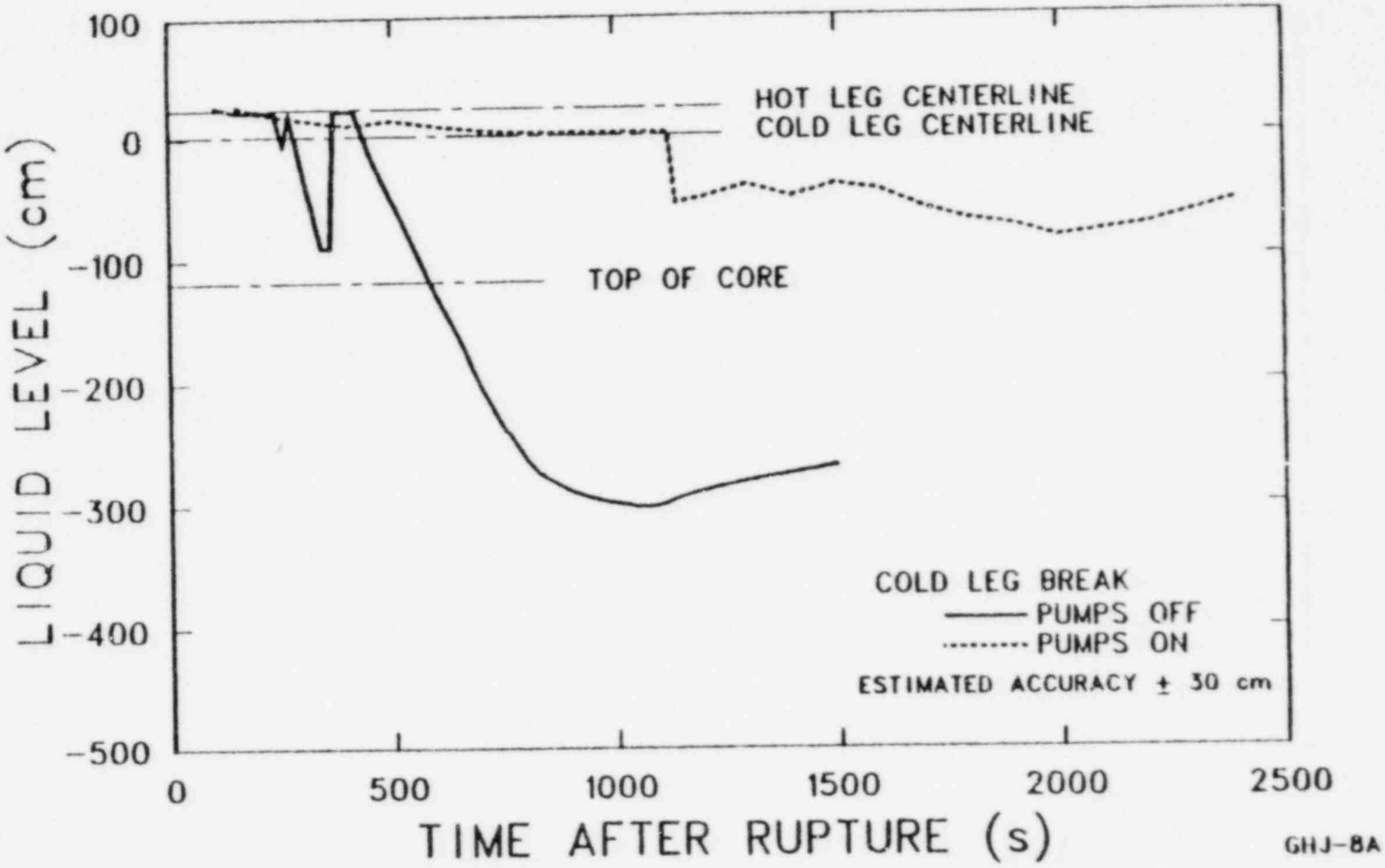


GHJ-7

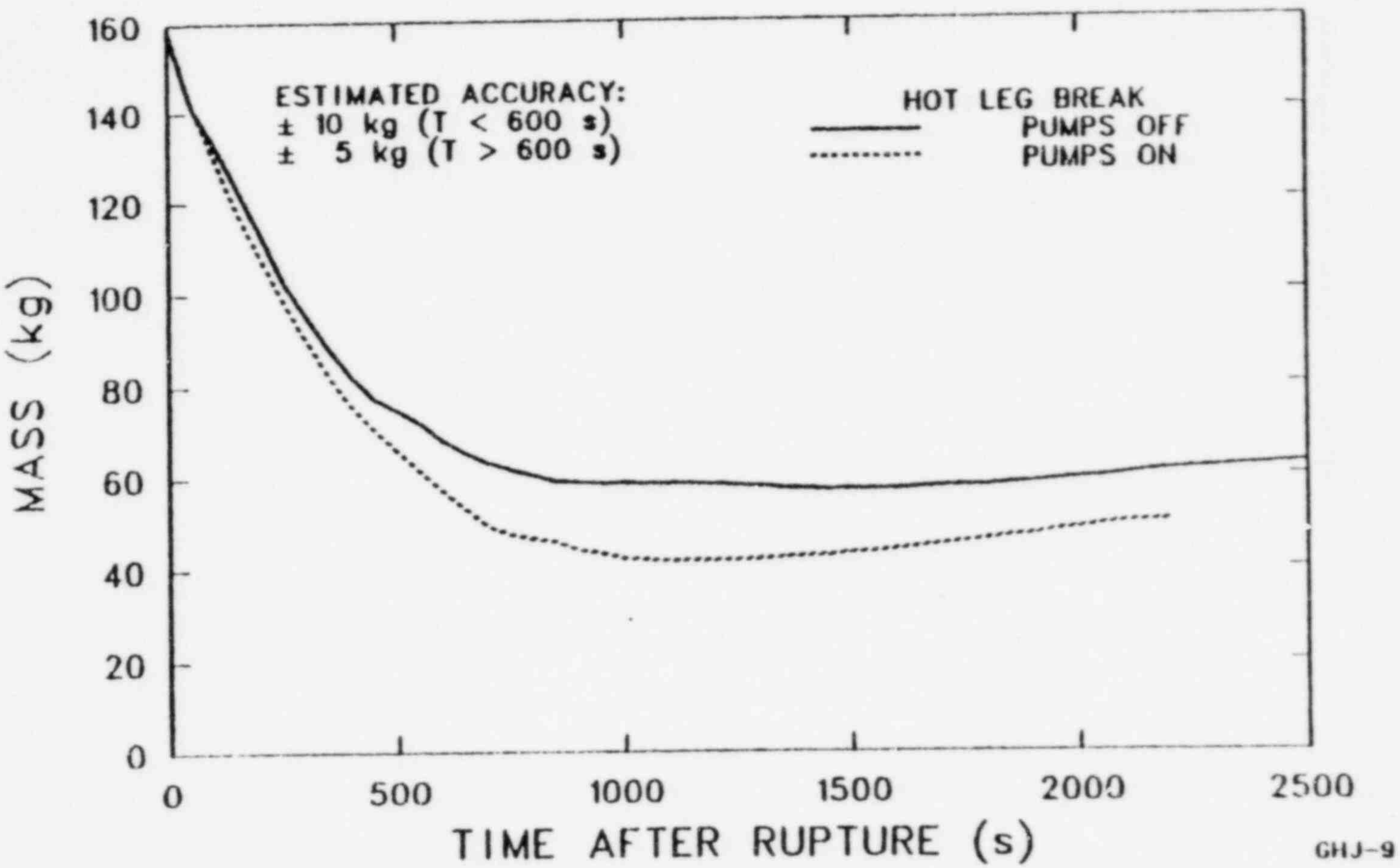
VESSEL MASS INVENTORY



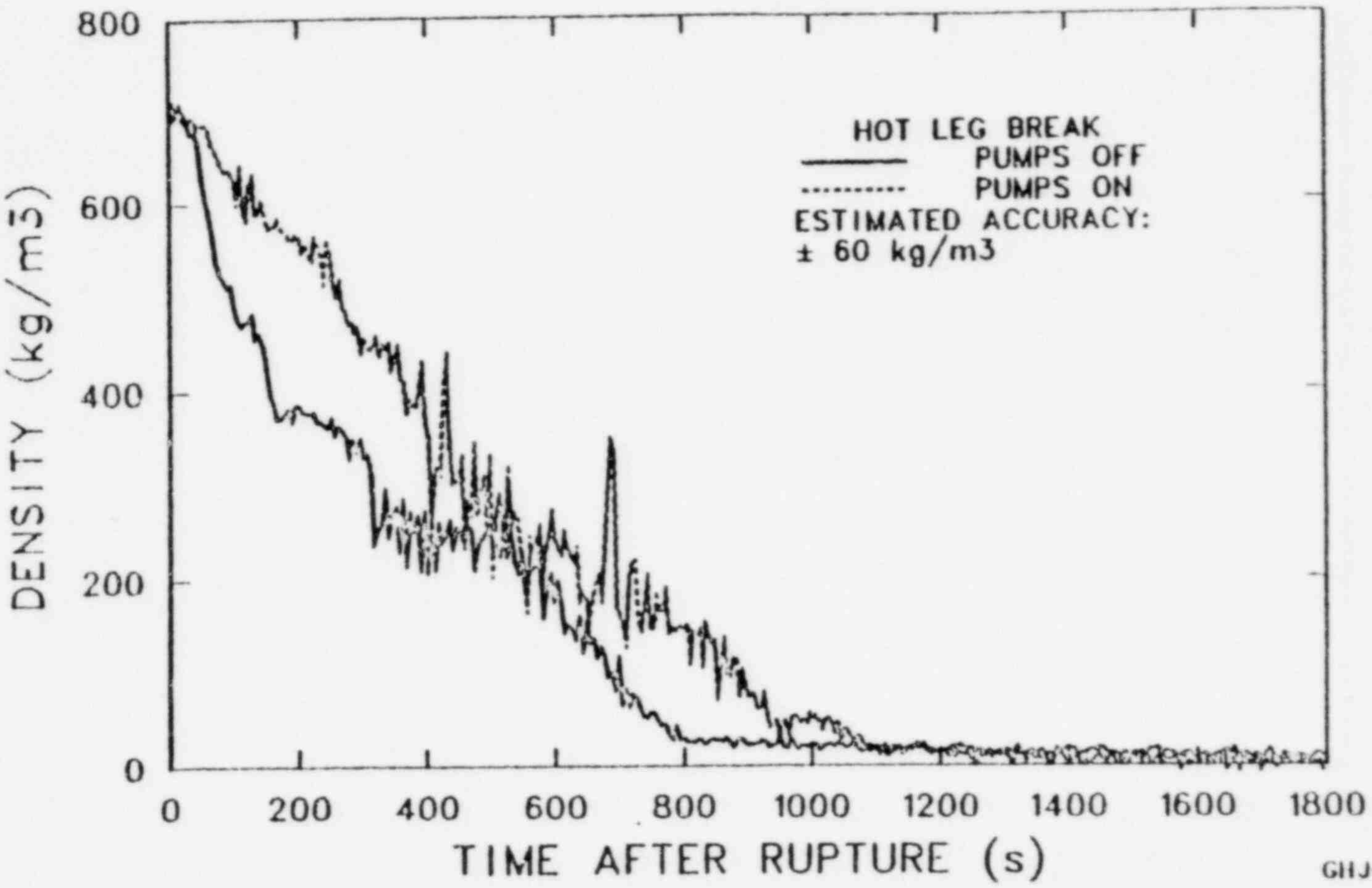
VESSEL MIXTURE LEVEL



SYSTEM MASS INVENTORY

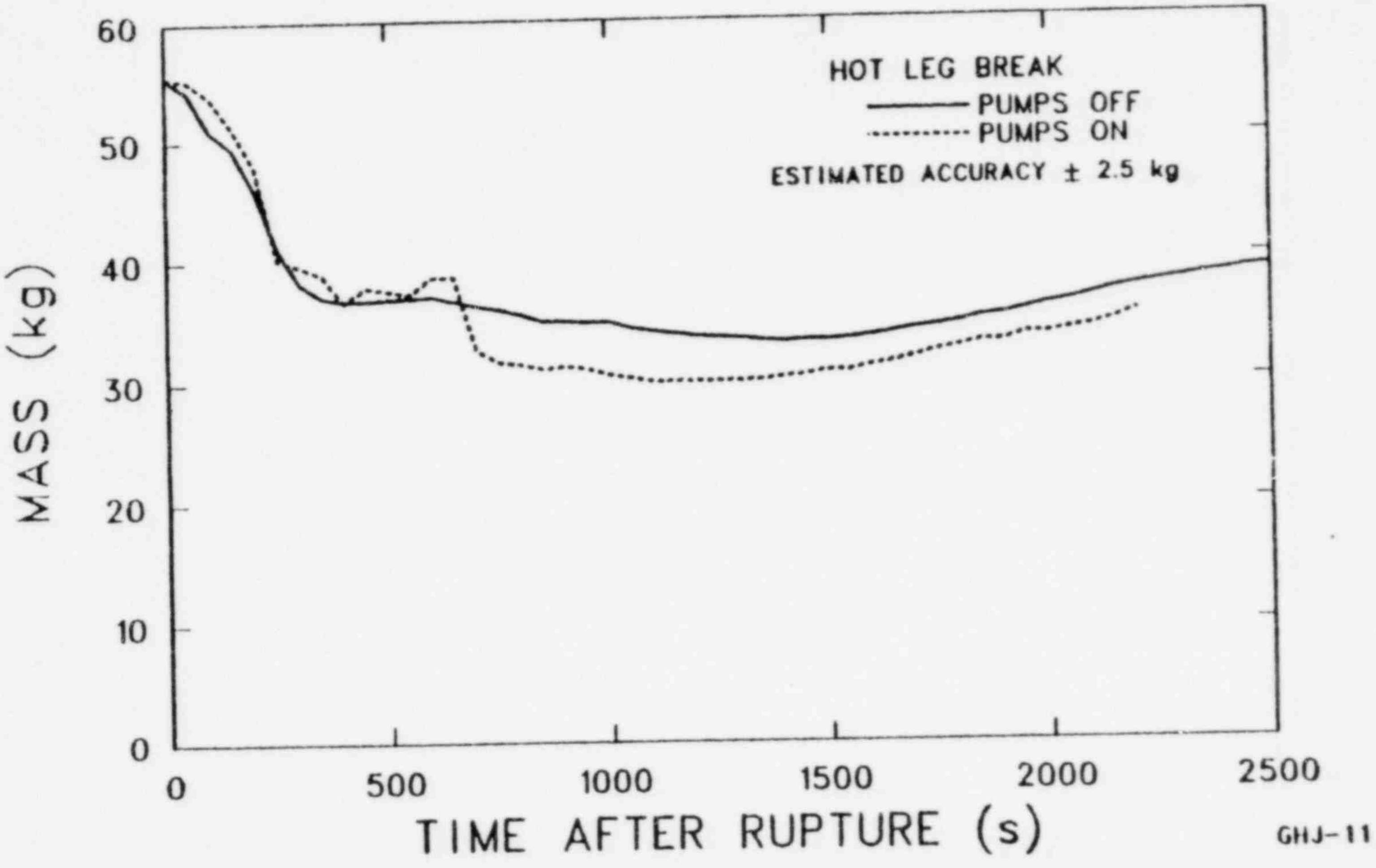


BROKEN LOOP HOT LEG DENSITY

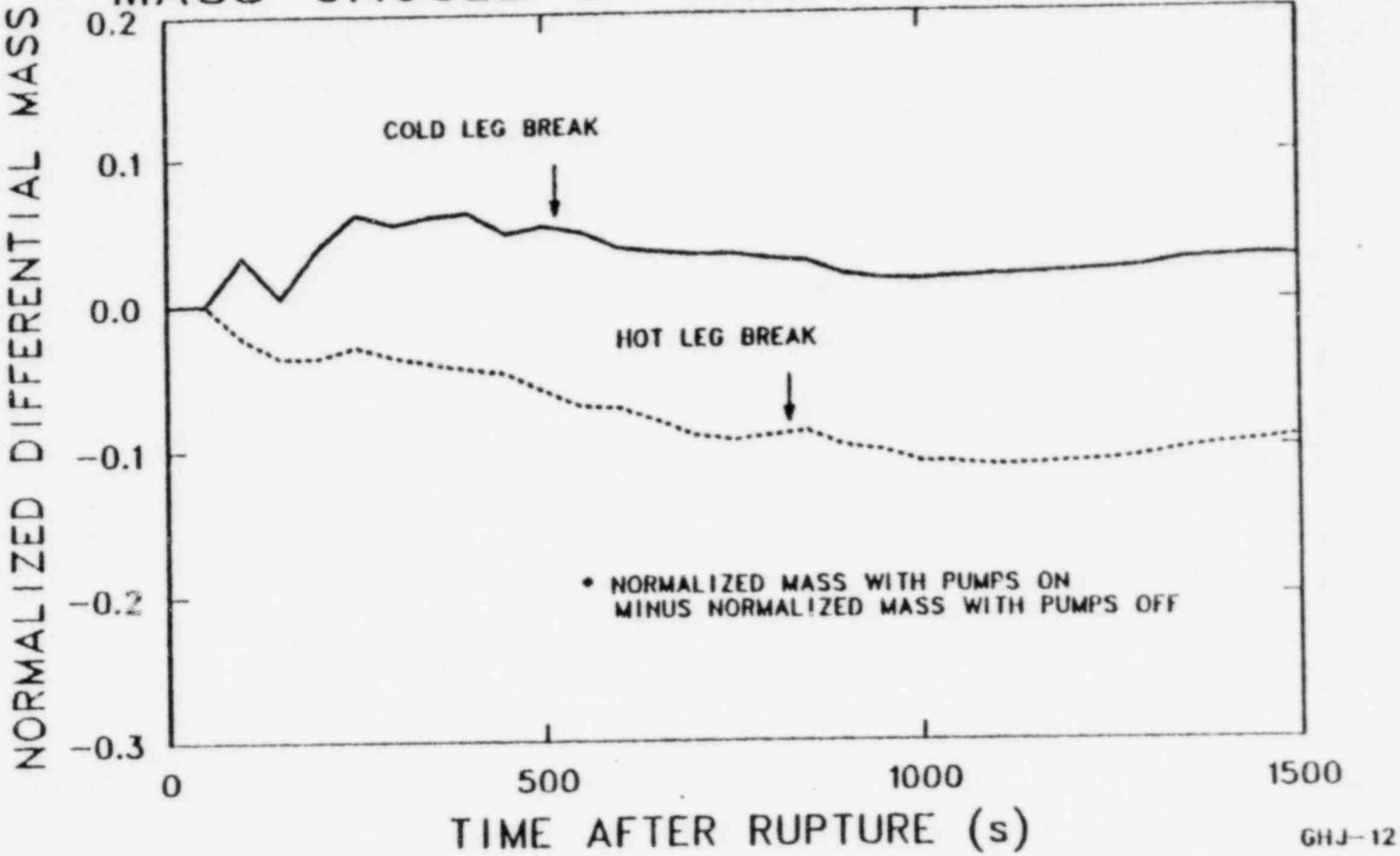


GHJ-10

VESSEL MASS INVENTORY

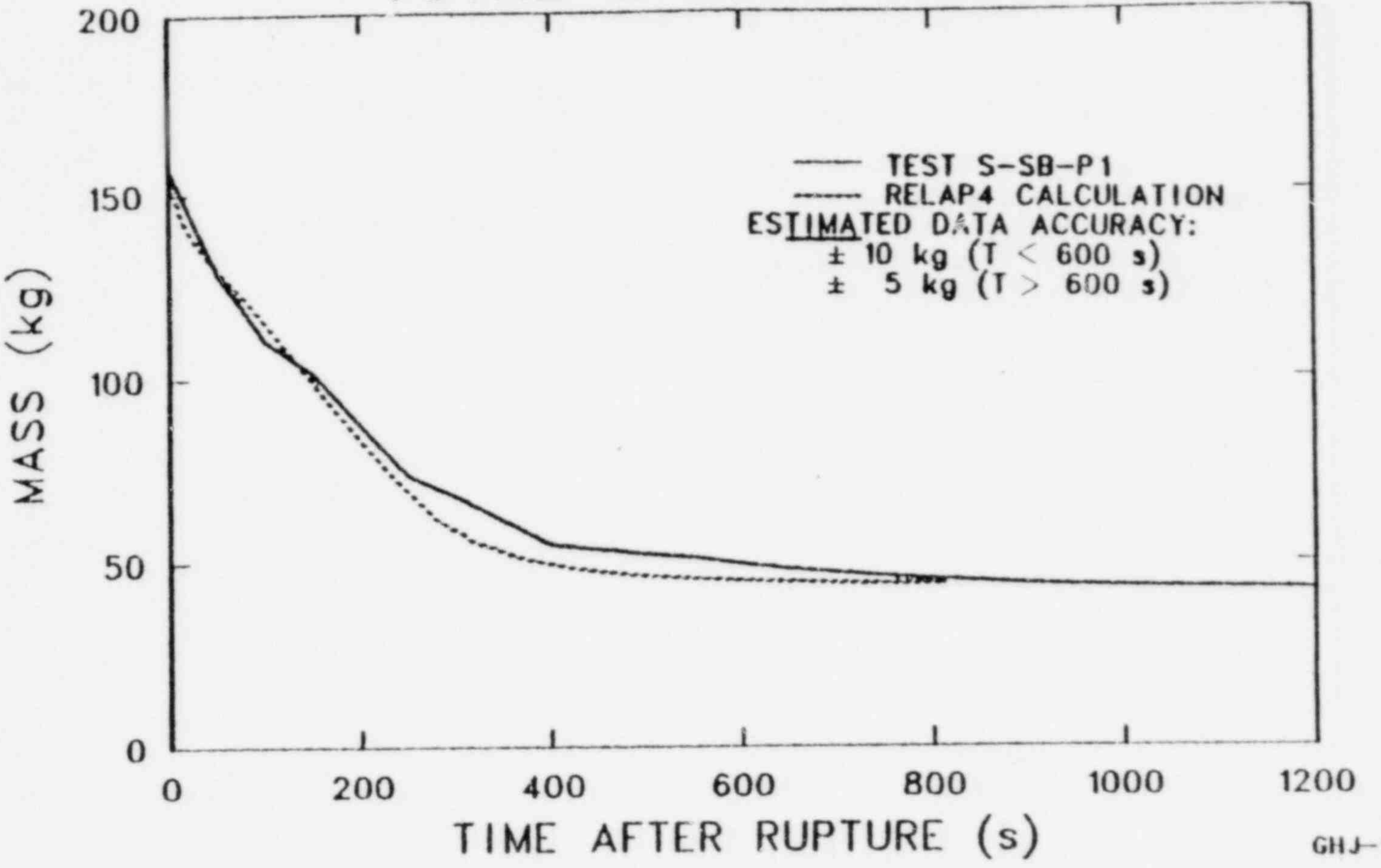


DIFFERENCE IN SYSTEM COOLANT MASS CAUSED BY PUMP OPERATION *



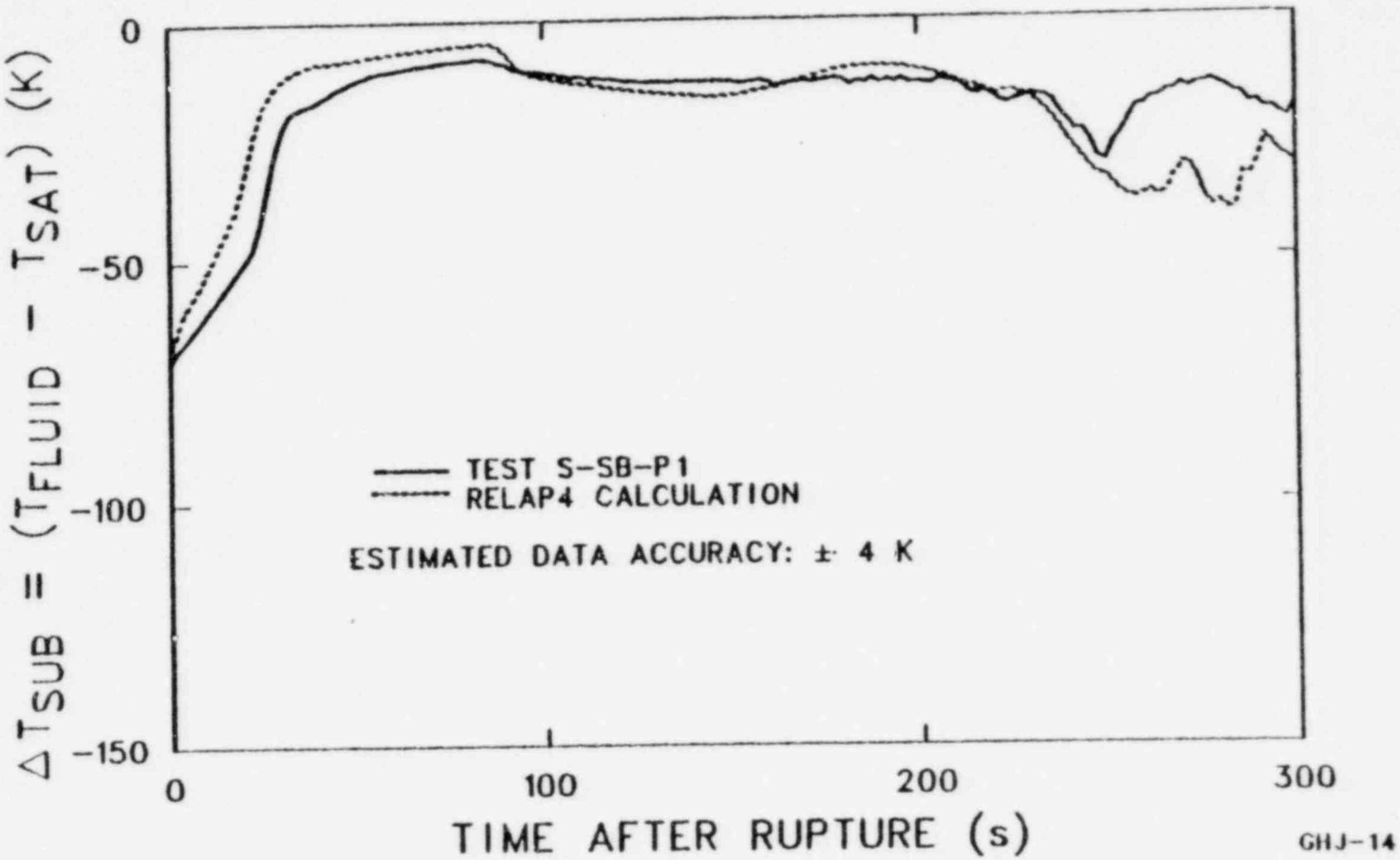
* NORMALIZED MASS WITH PUMPS ON MINUS NORMALIZED MASS WITH PUMPS OFF

CALCULATED AND MEASURED TOTAL SYSTEM MASS

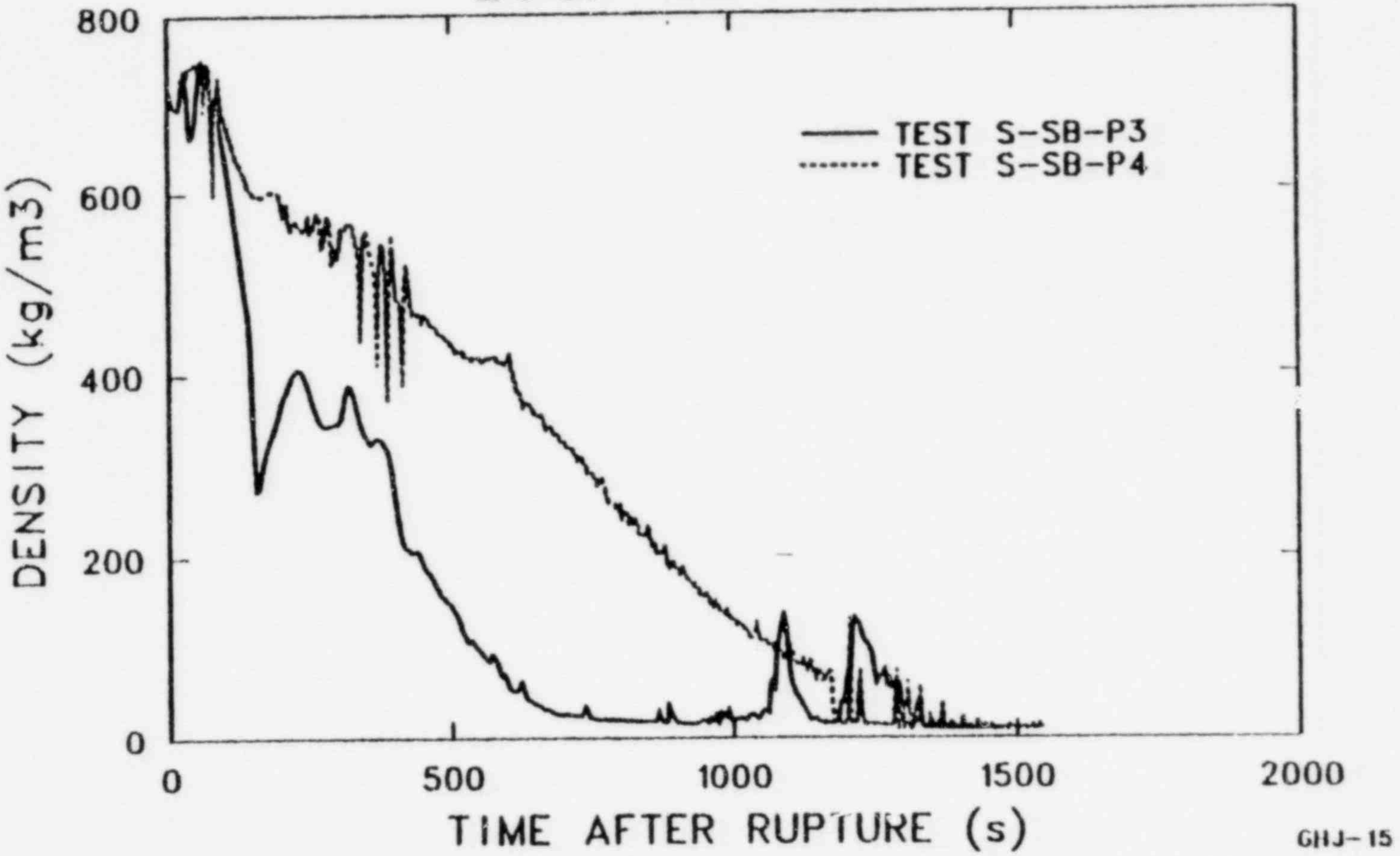


GHJ-13

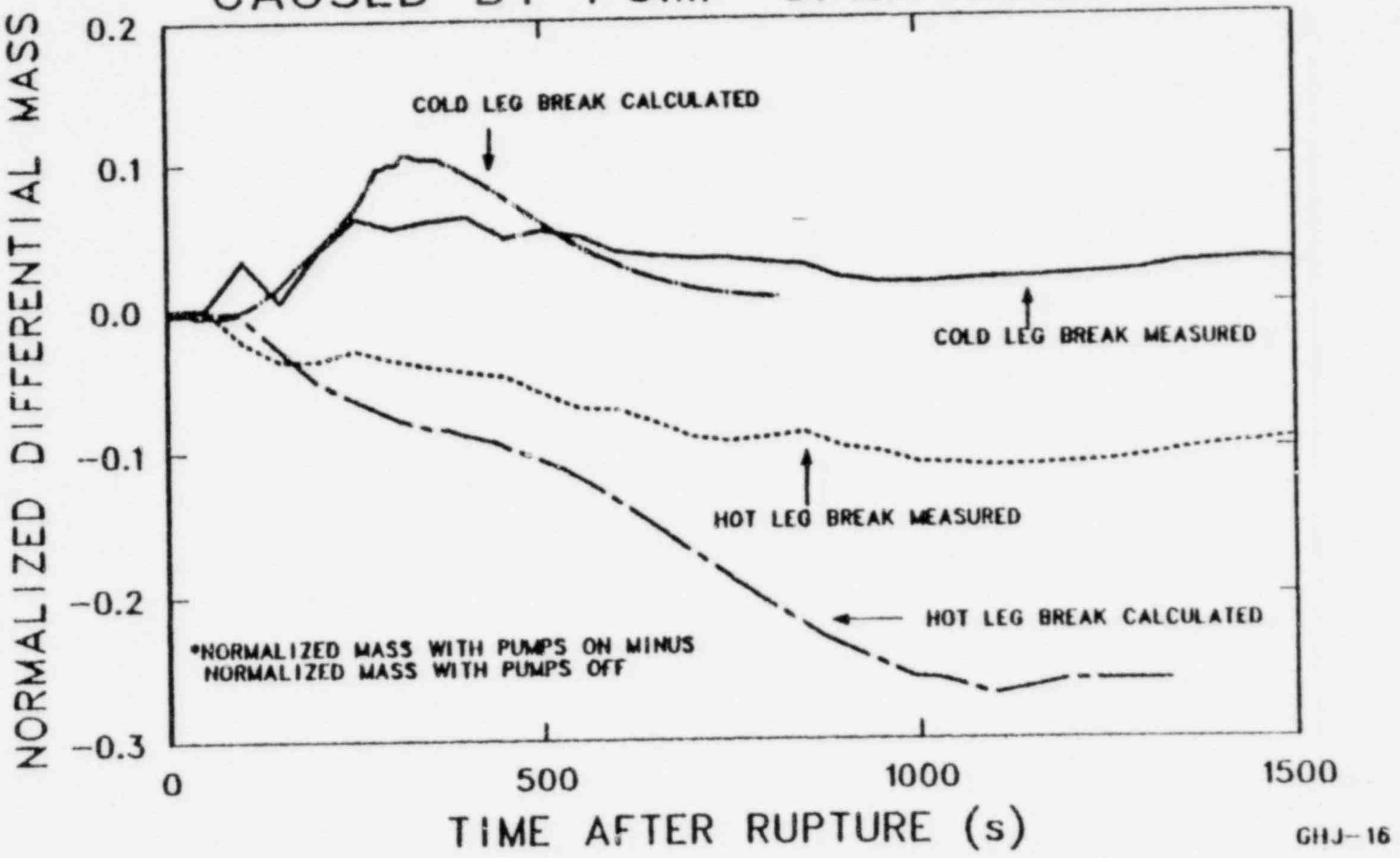
SUBCOOLING IN THE BROKEN LOOP COLD LEG



CALCULATED DENSITY IN BROKEN LOOP HOT LEG



DIFFERENCE IN SYSTEM COOLANT MASS CAUSED BY PUMP OPERATION *

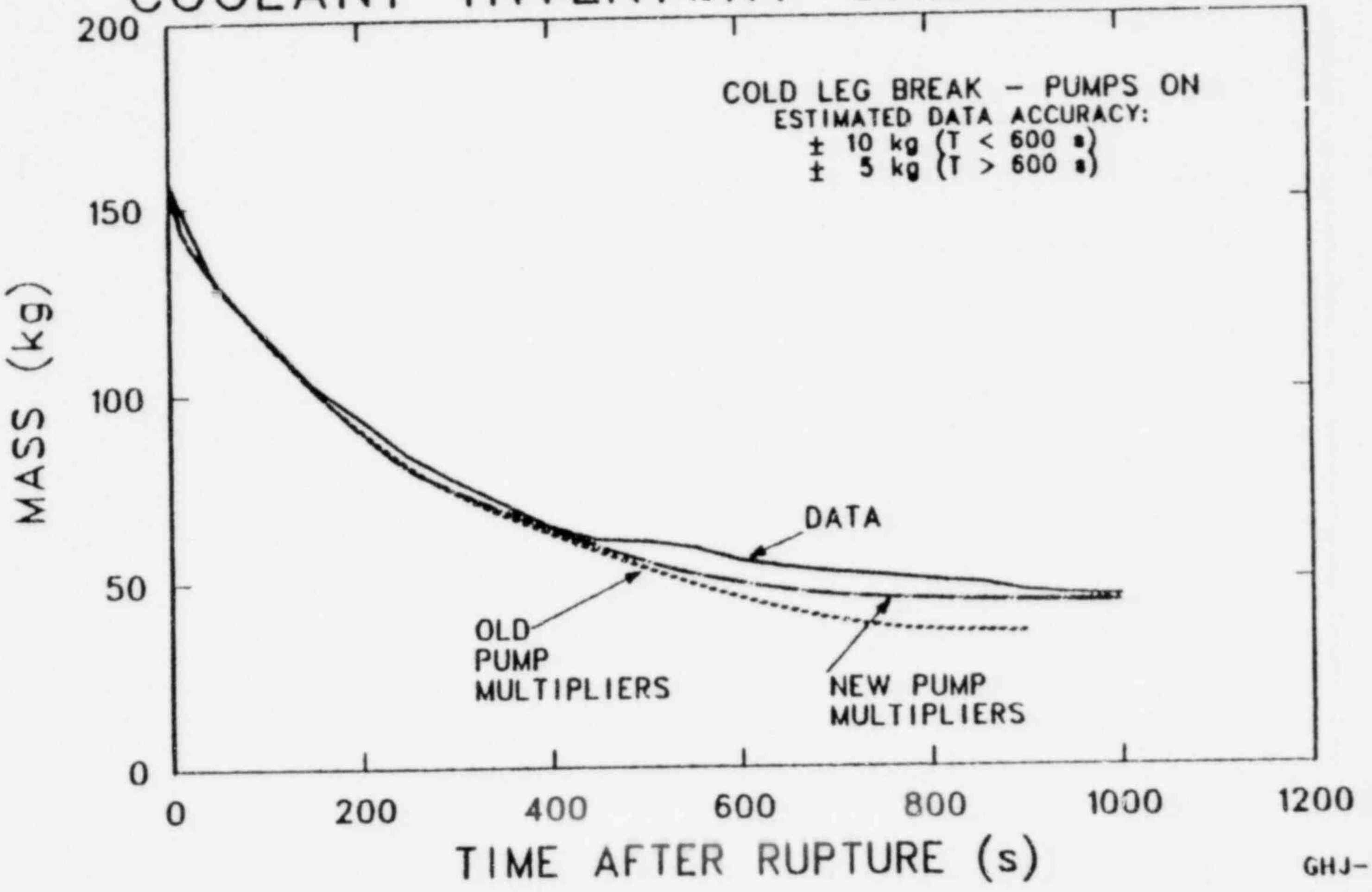


MODELING ISSUES

- TWO-PHASE PUMP PERFORMANCE
- PHASE SEPARATION
- PHASE EQUILIBRIUM

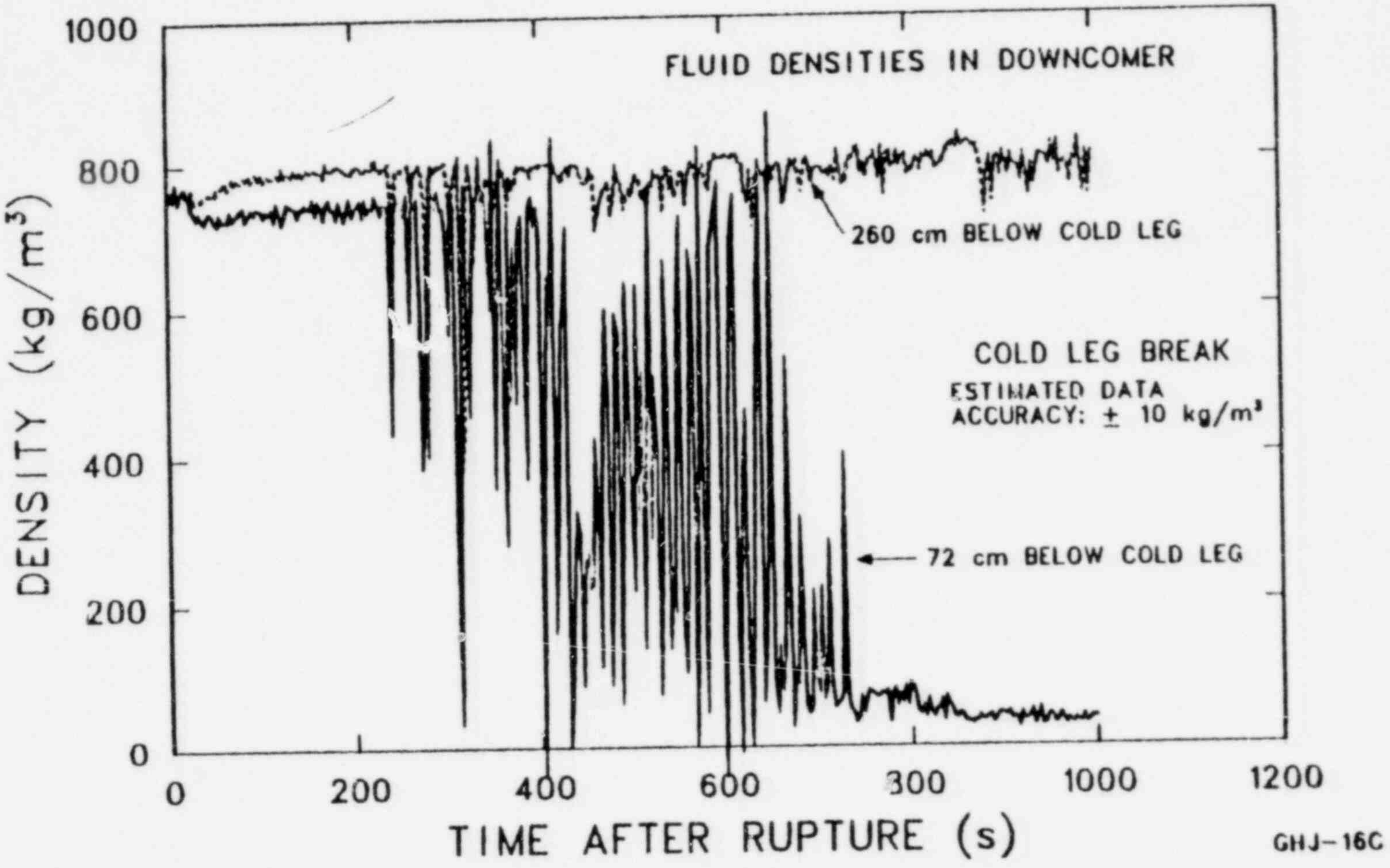
GHJ-MA

EFFECT OF PUMP HEAD MULTIPLIERS ON COOLANT INVENTORY CALCULATION



GHJ-16B

DOWNCOMER PHASE STRATIFICATION – PUMPS ON



Dr. Bates:

This page should be replaced with
the first page of the conclusions.

Jane Beach 22

CONCLUSIONS (CONT'D)

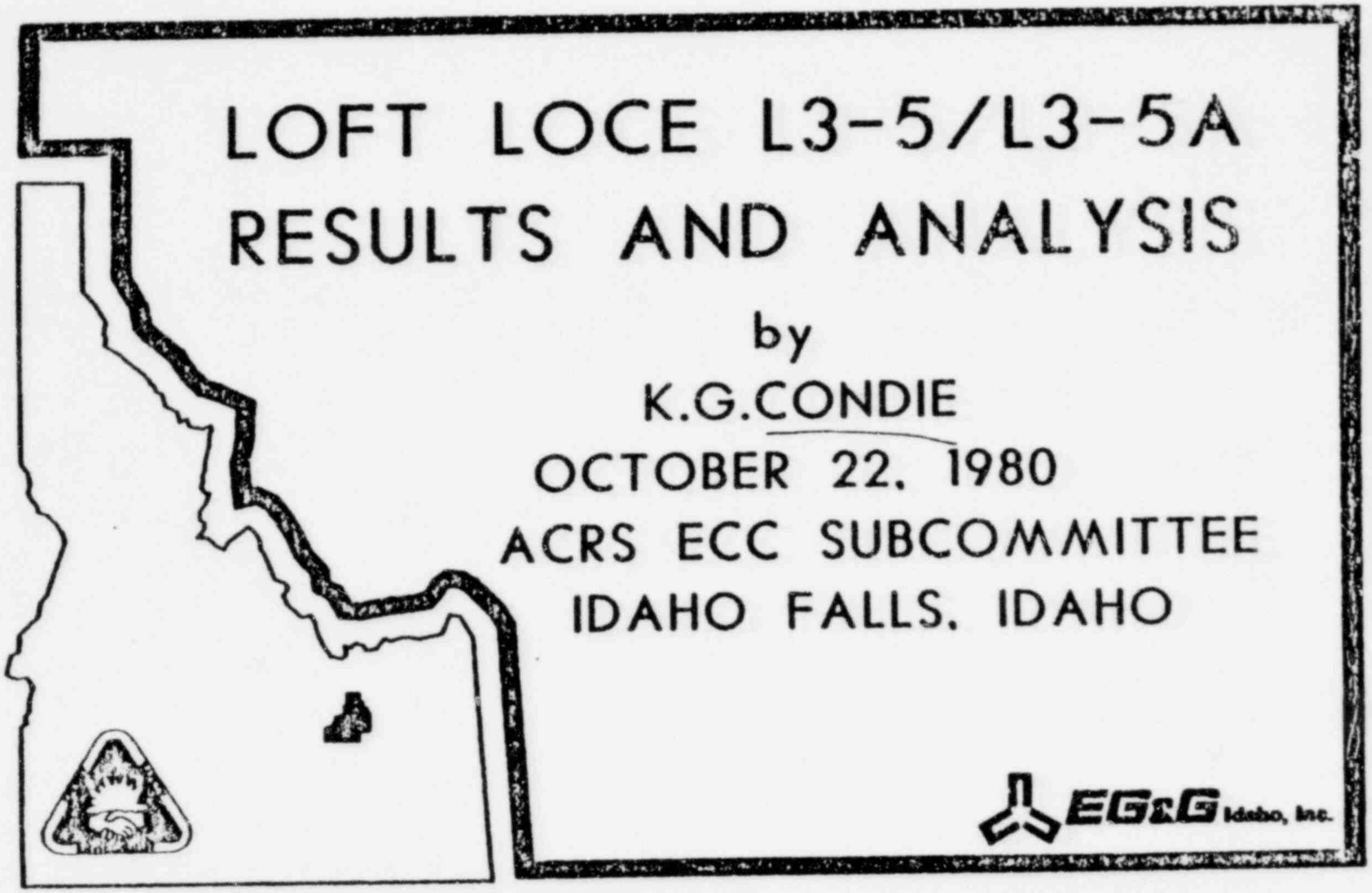
- RELAP4 CODE CAN CORRECTLY CALCULATE DIFFERENTIAL TRENDS CAUSED BY PUMP OPERATION

- SIGNIFICANT MODELING ISSUES CONFIRMED.

    - PUMP DEGRADATION
    - PHASE SEPARATION

- OVERALL RESULTS SUGGEST SENSITIVITY TO ASSUMED BREAK CONFIGURATION AND SCENARIO

GHJ-18



LOFT LOCE L3-5/L3-5A RESULTS AND ANALYSIS

by

K.G.CONDIE

OCTOBER 22, 1980

ACRS ECC SUBCOMMITTEE

IDAHO FALLS, IDAHO

EG&G Idaho, Inc.

Condie 1

LOFT LOCE L3-5/L3-5A RESULTS AND ANALYSIS

- TEST OBJECTIVES

  L3-5
  L3-5A

- SYSTEM CONFIGURATION
- TEST SCENARIO AND EVENT SEQUENCE
- COOLING MECHANISMS
- MASS DISTRIBUTION DURING L3-5
- CODE PREDICTIONS
- CONCLUSIONS

EXPERIMENT OBJECTIVE

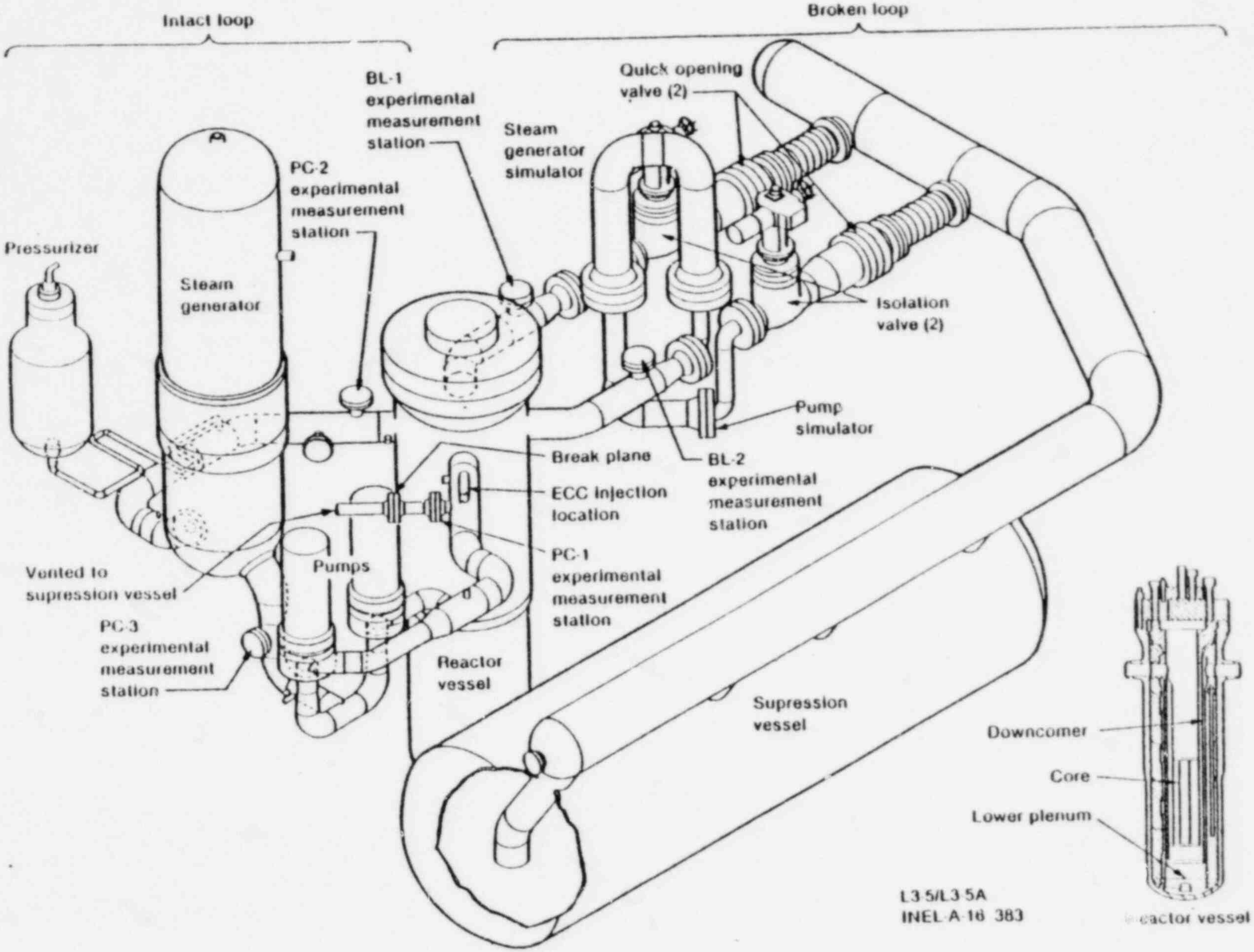
L3-5

IN CONJUNCTION WITH FUTURE TEST L3-6 EVALUATE THE SYSTEM EFFECTS OF PRIMARY COOLANT PUMP OPERATION DURING A SMALL BREAK LOCA.

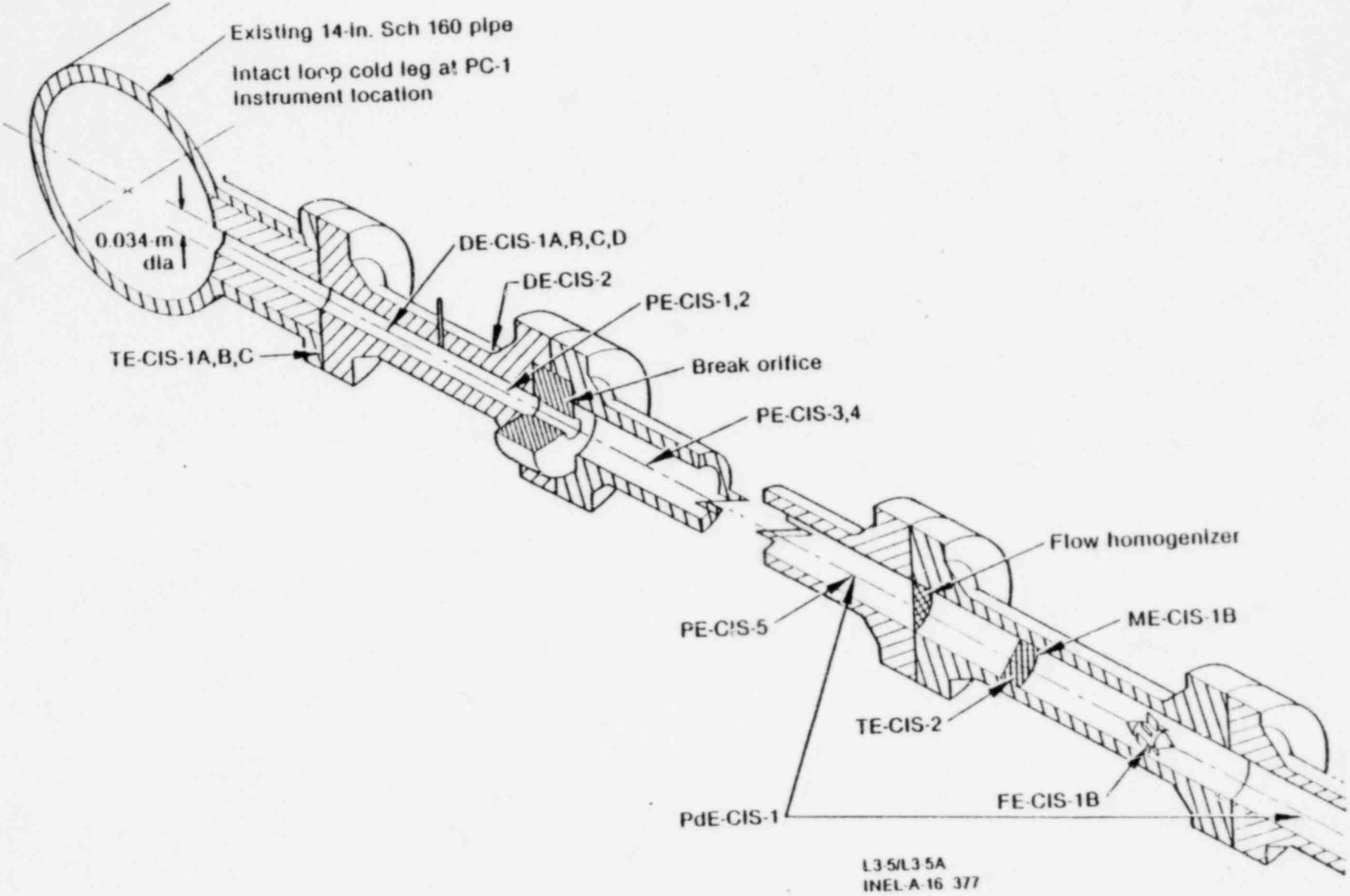
L3-5A

EVALUATE PLANT RECOVERY BY ISOLATING THE BREAK AND REESTABLISNING STEAM GENERATOR AS HEAT SINK USING NATURAL CIRCULATION.

LOFT PRIMARY COOLANT SYSTEM

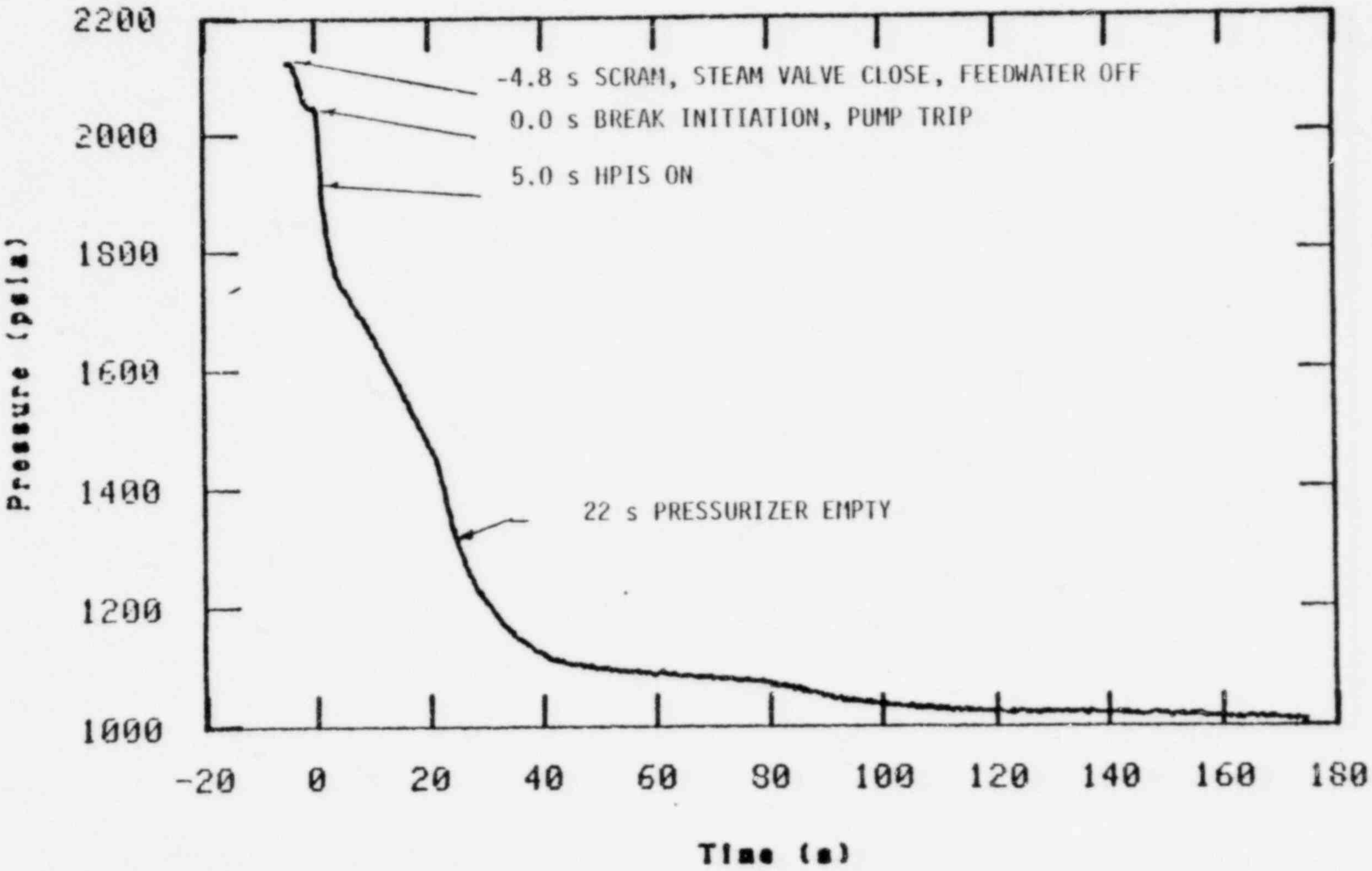


LOFT L3-5/L3-5A BLOWDOWN SPOOL PIECE

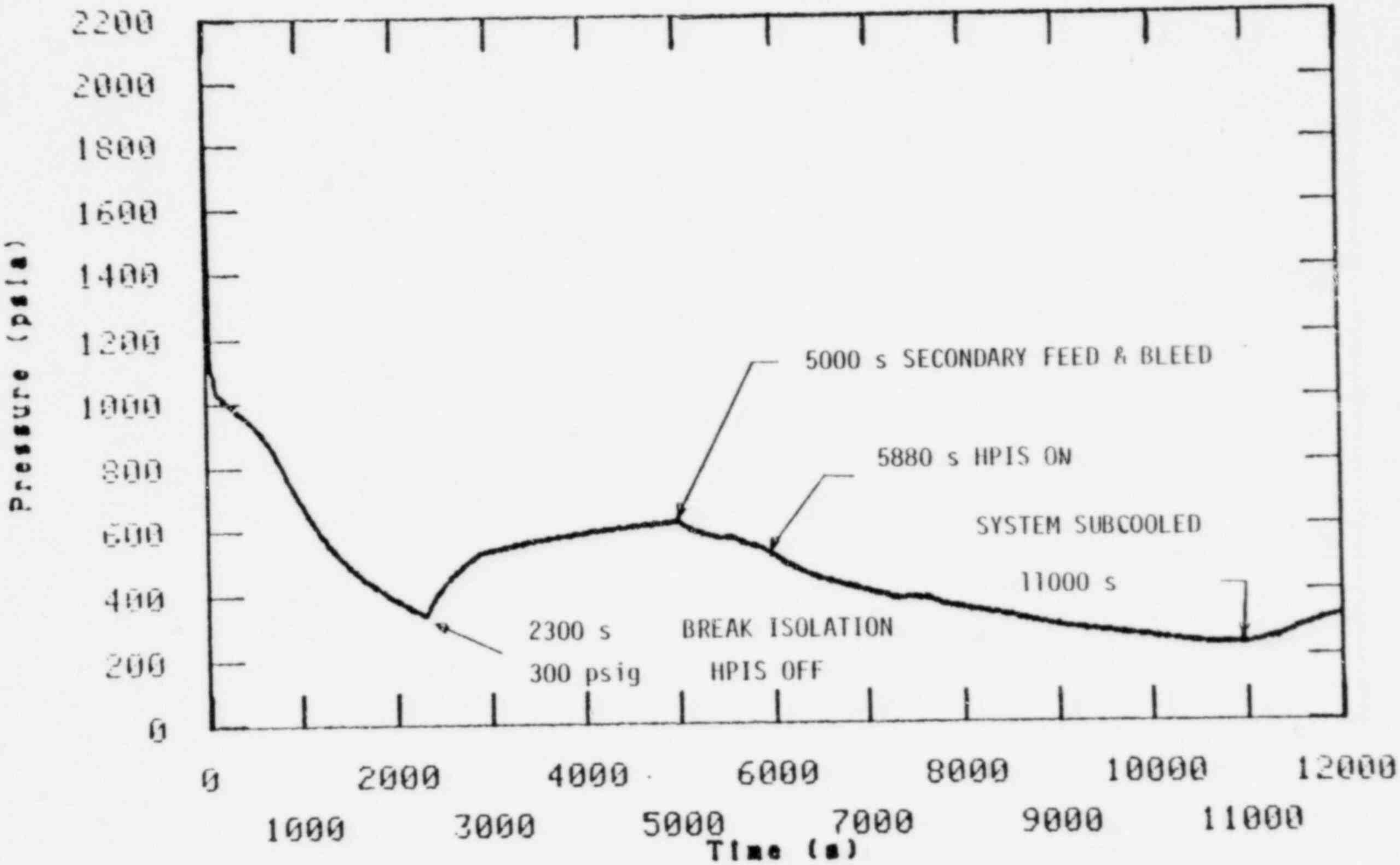


L3-5/L3-5A
INEL-A-16 377

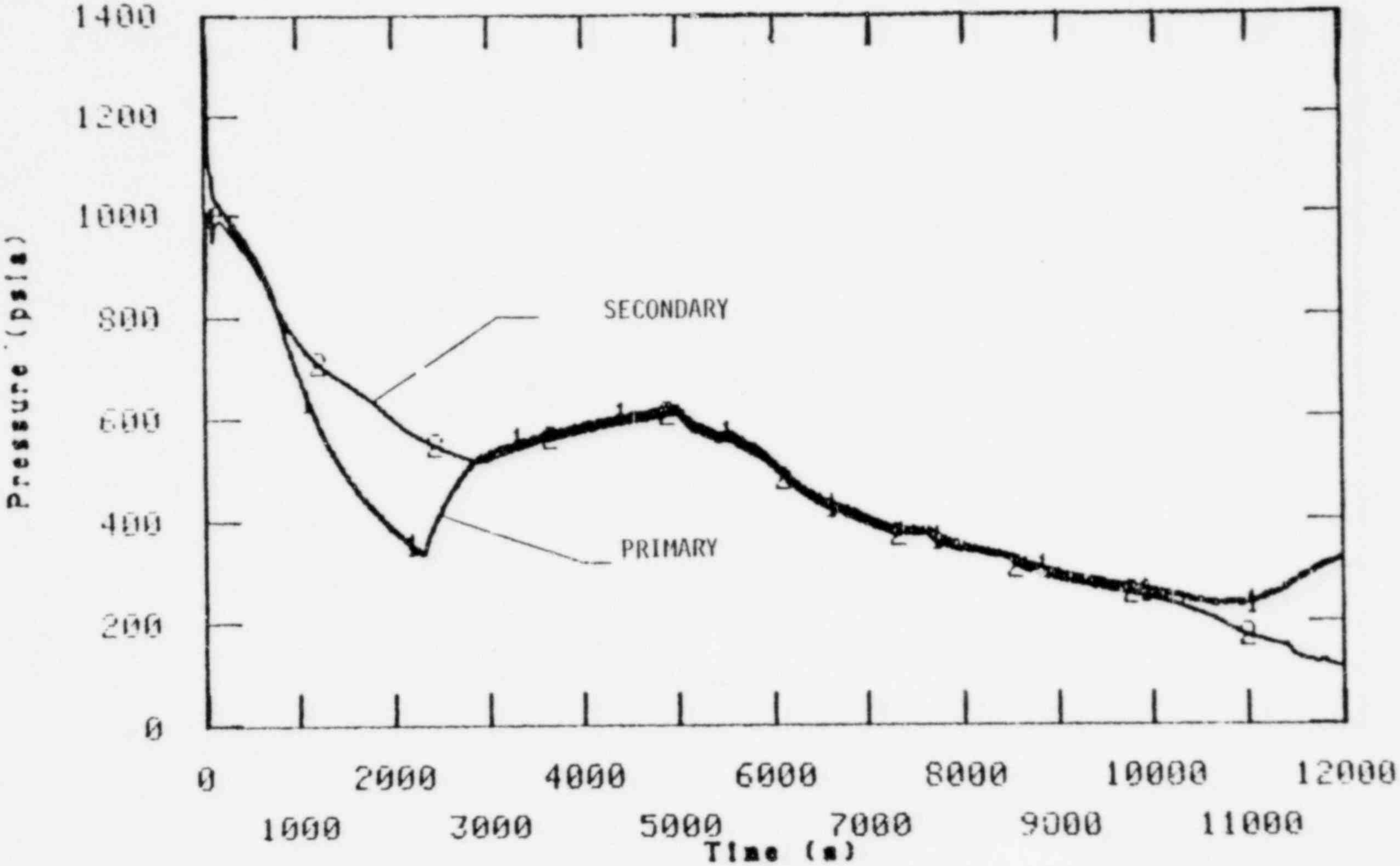
LOFT L3-5 PRIMARY SYSTEM PRESSURE



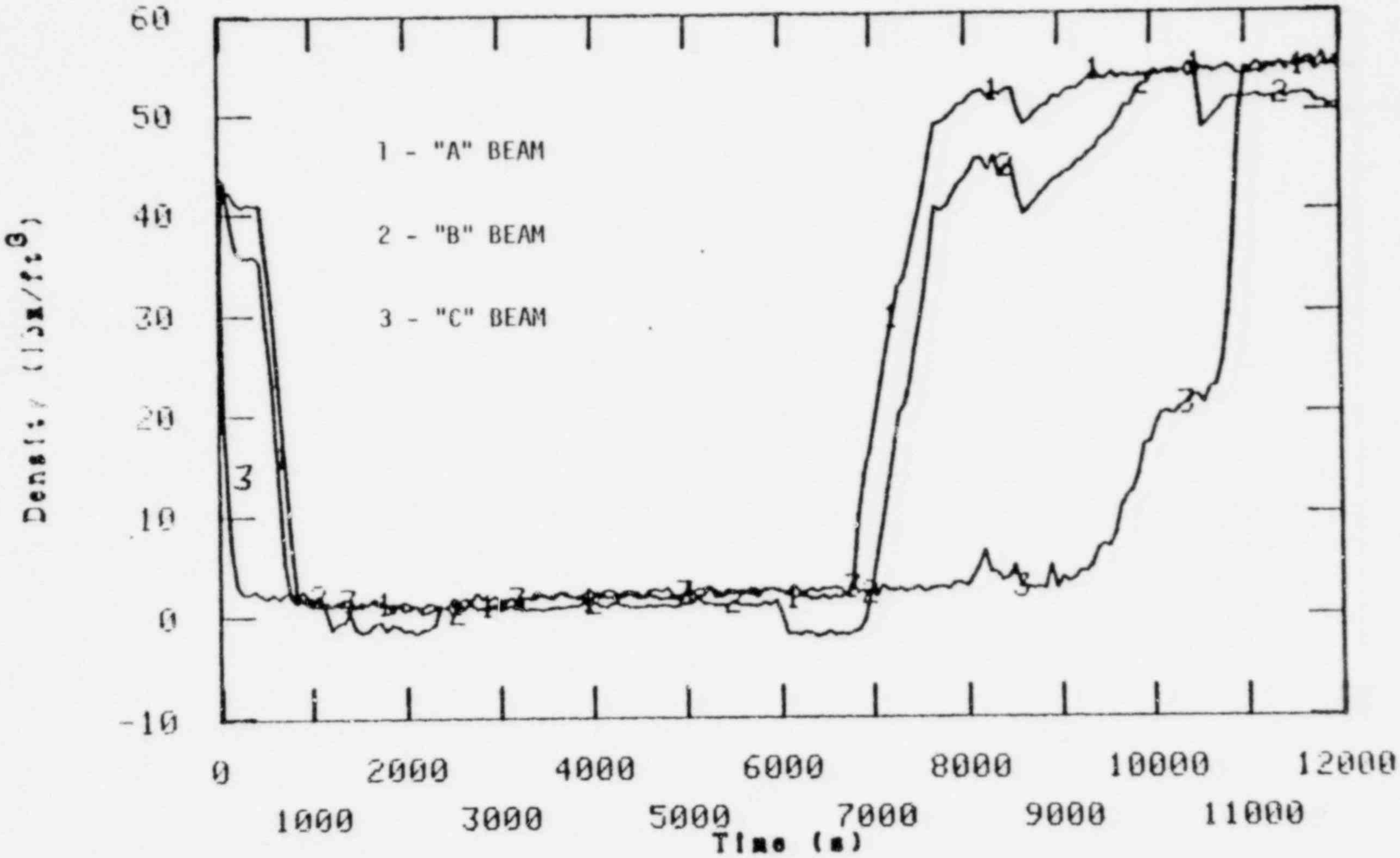
LOFT L3-5/L3-5A PRIMARY PRESSURE



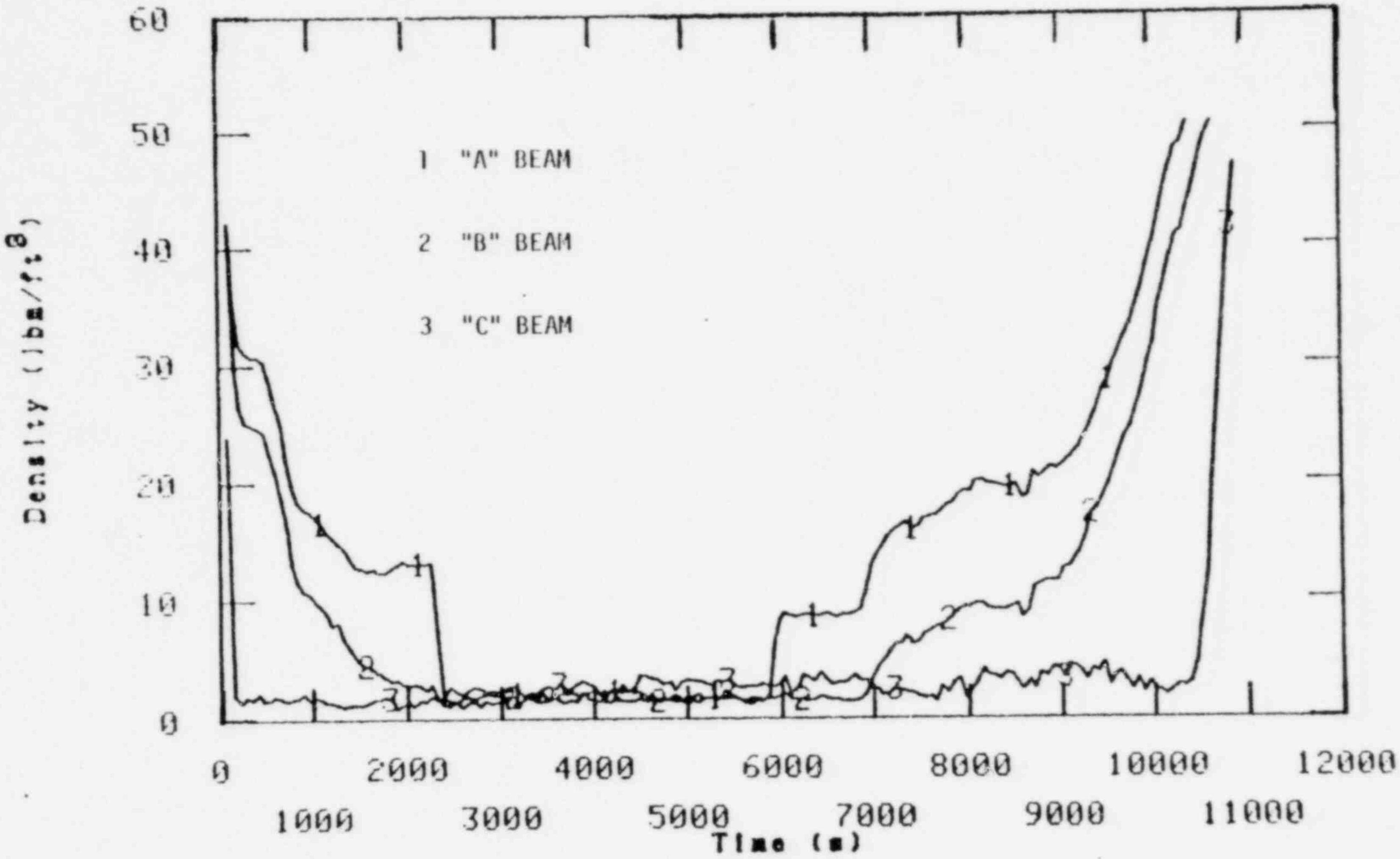
LOFT L3-5/L3-5A PRIMARY AND SECONDARY PRESSURE



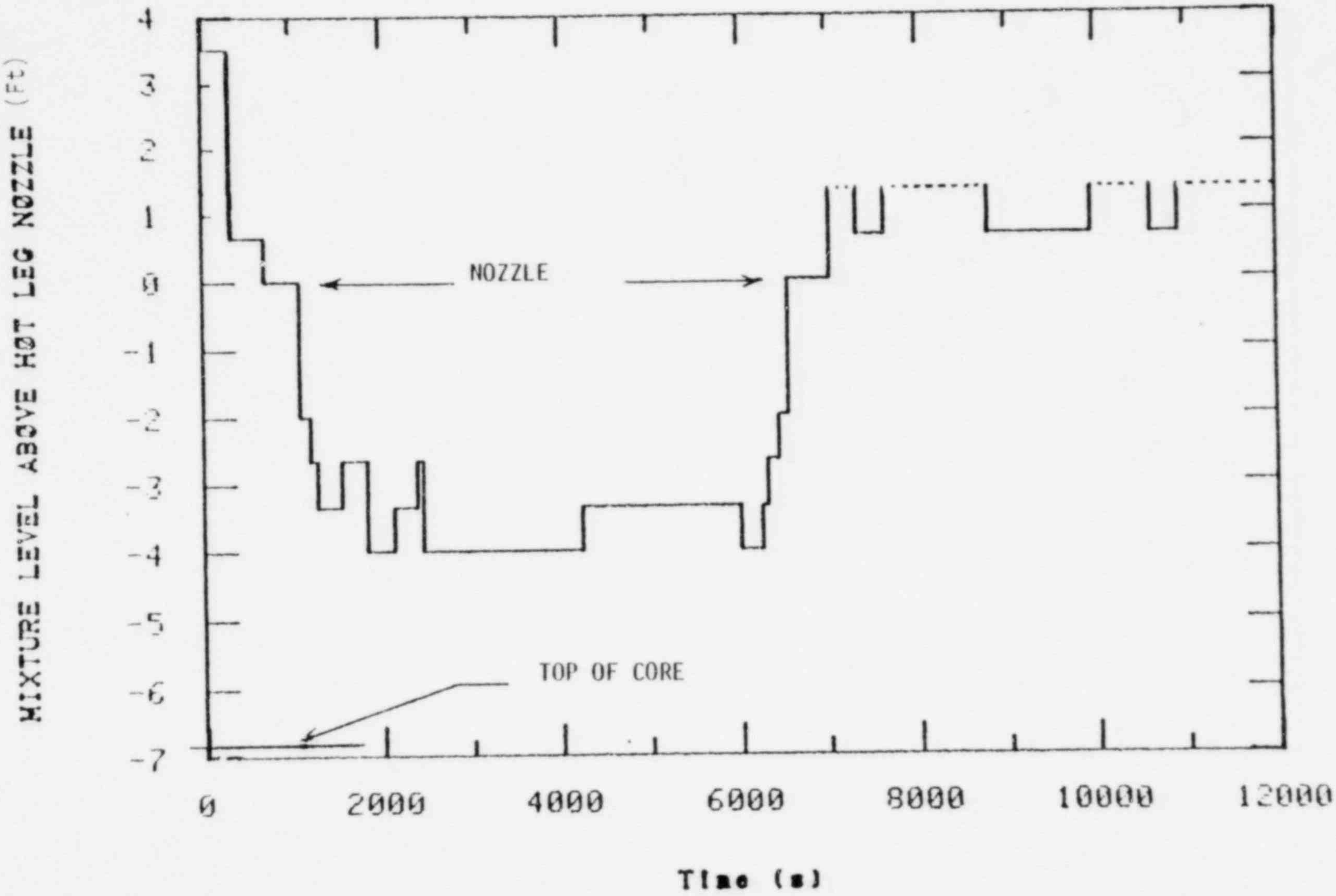
LOFT L3-5/L3-5A INTACT HOT LEG DENSITIES



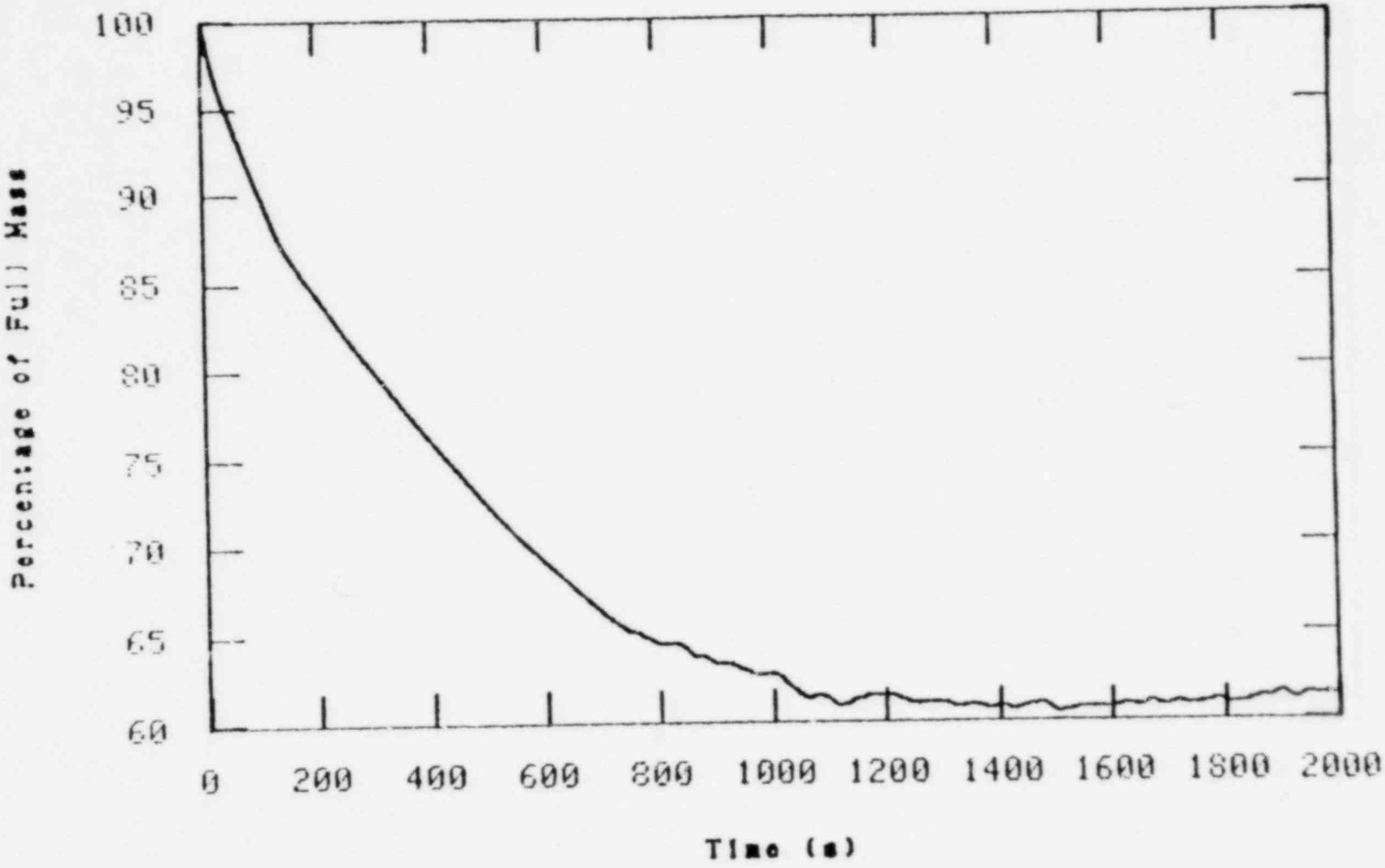
LOFT L3-5/L3-5A INTACT LOOP COLD LEG DENSITIES



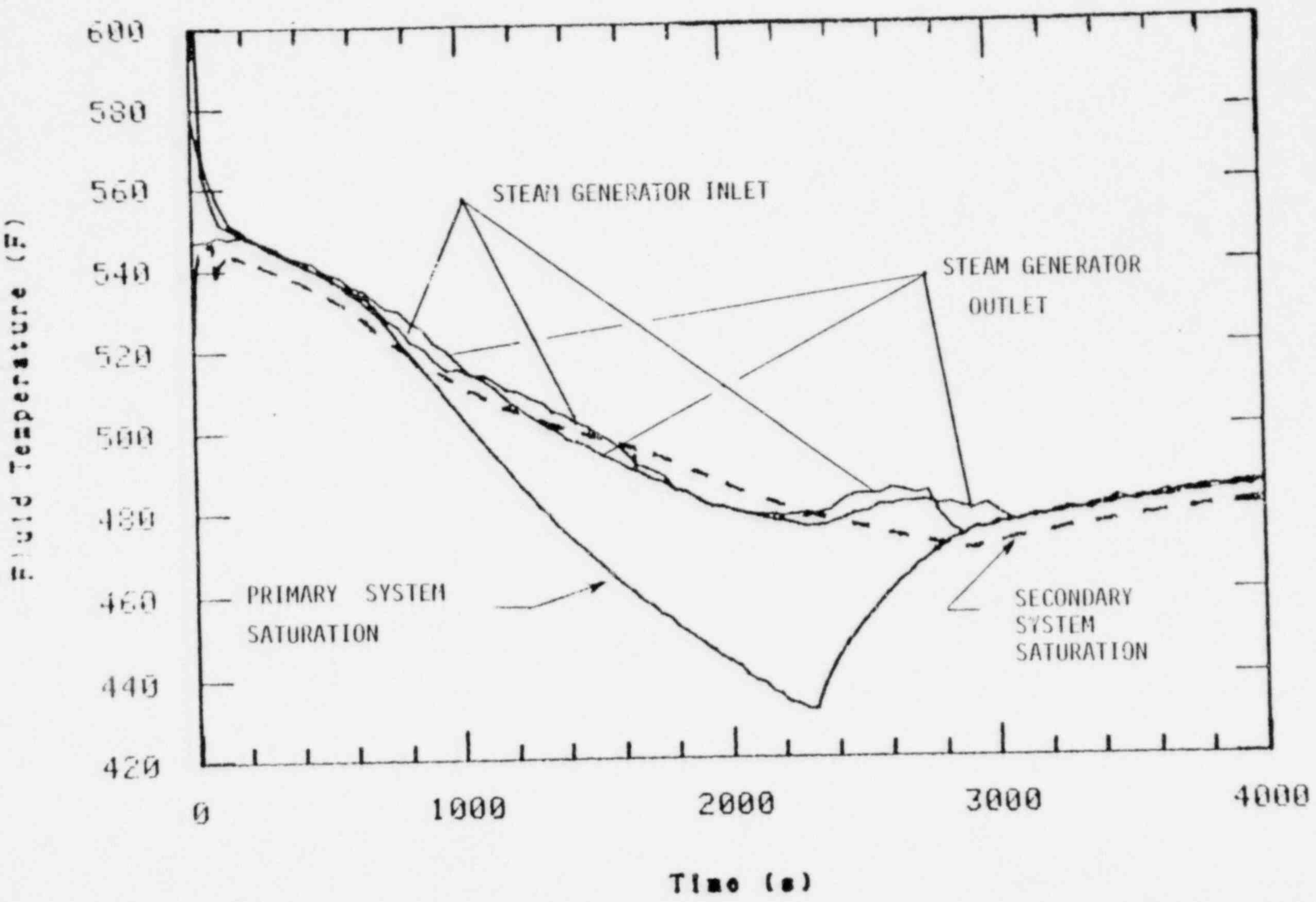
LOFT L3-5/L3-5A REACTOR VESSEL MIXTURE LEVEL



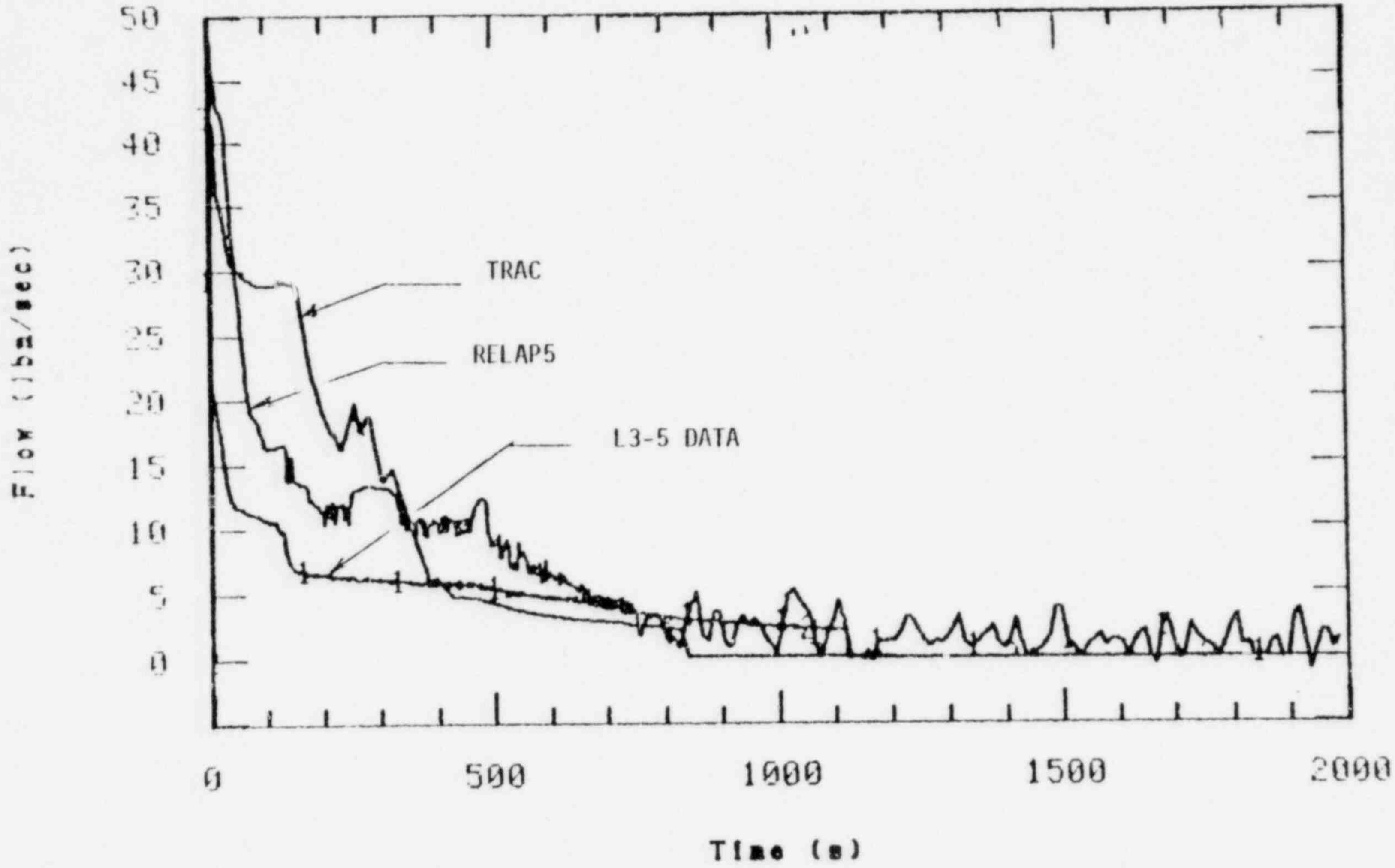
LOFT L3-5 PRIMARY SYSTEM MASS INVENTORY



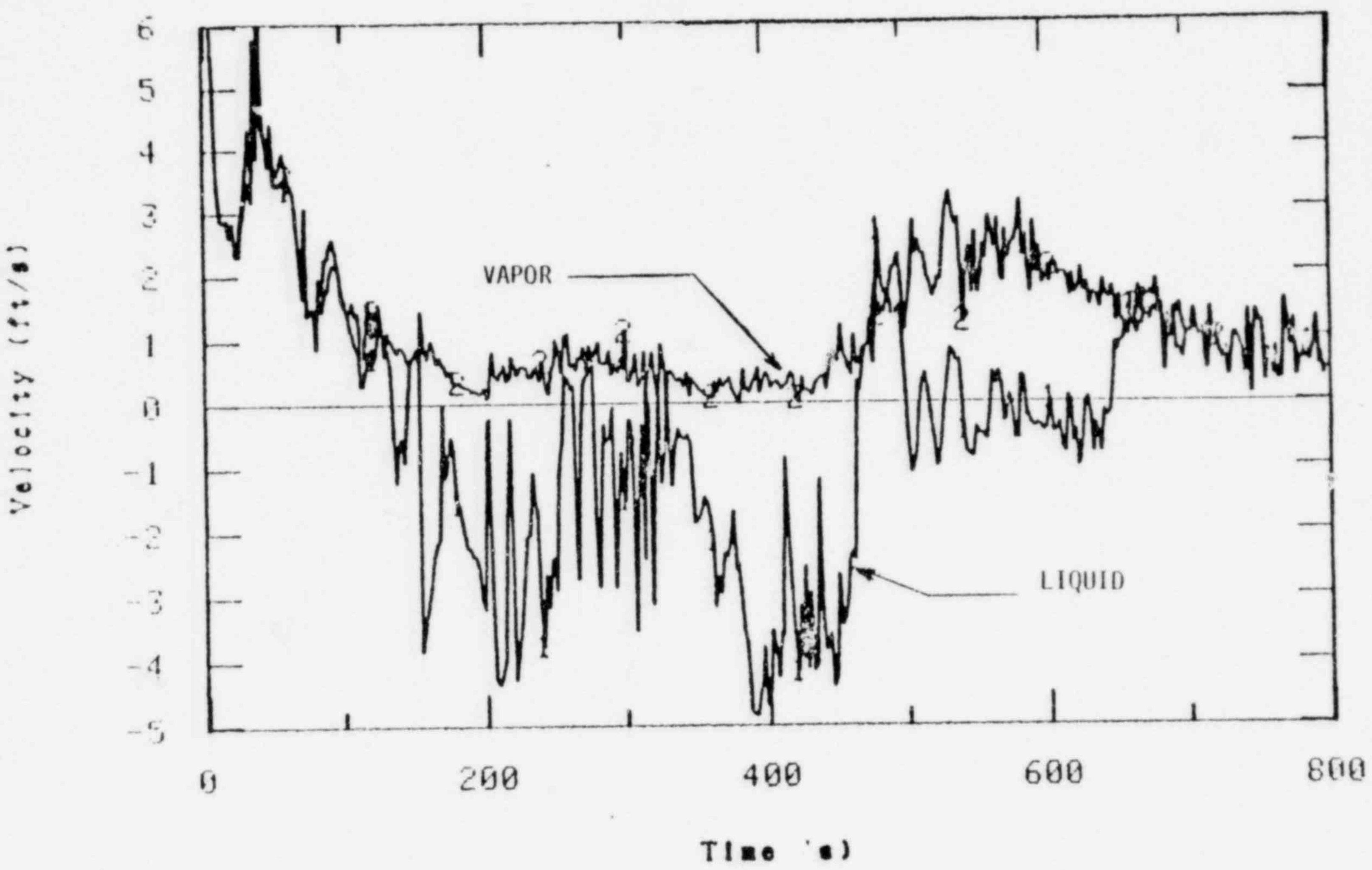
LOFT L3-5/L3-5A TEMPERATURES



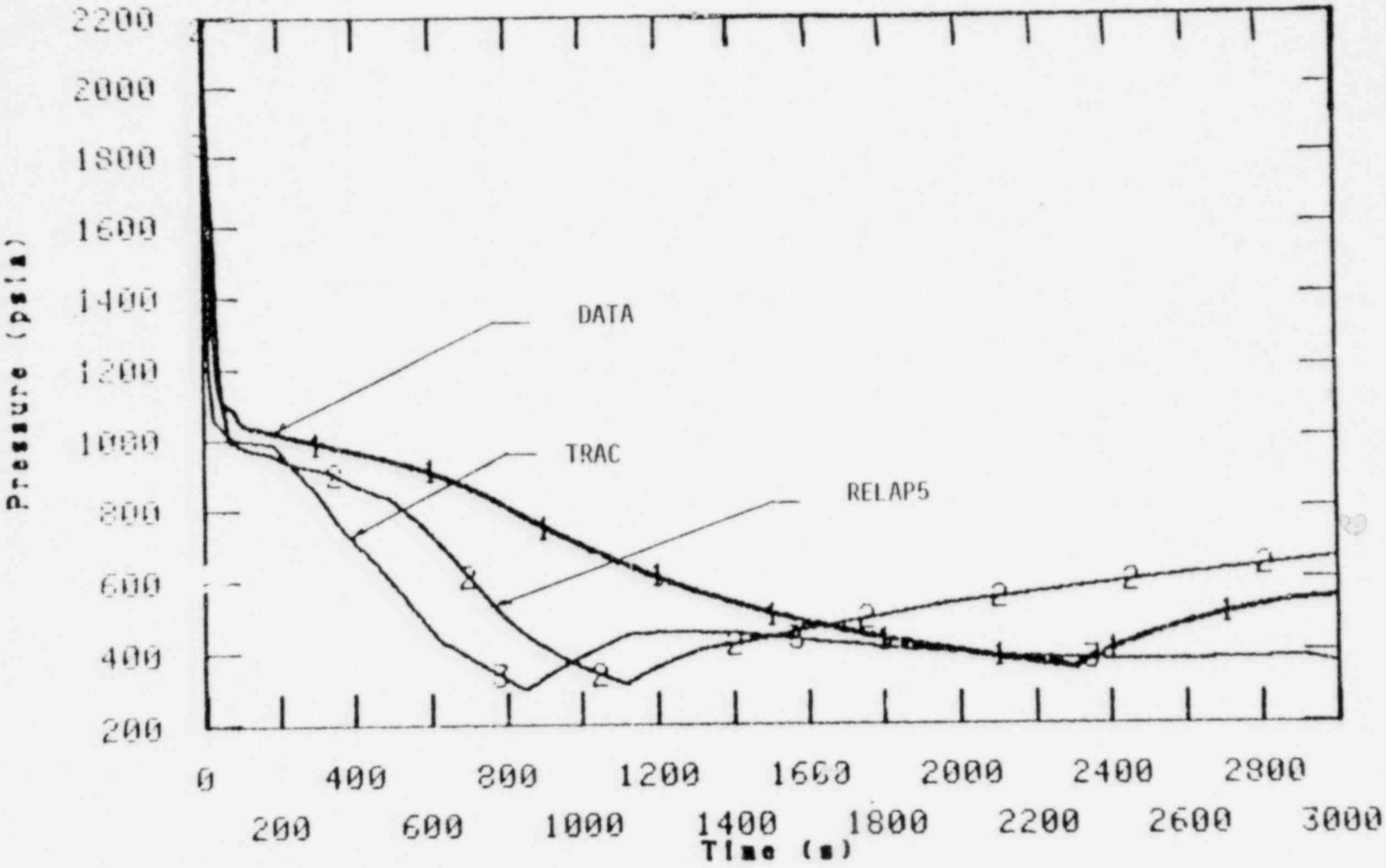
LOFT L3-5 PREDICTED BREAK FLOW



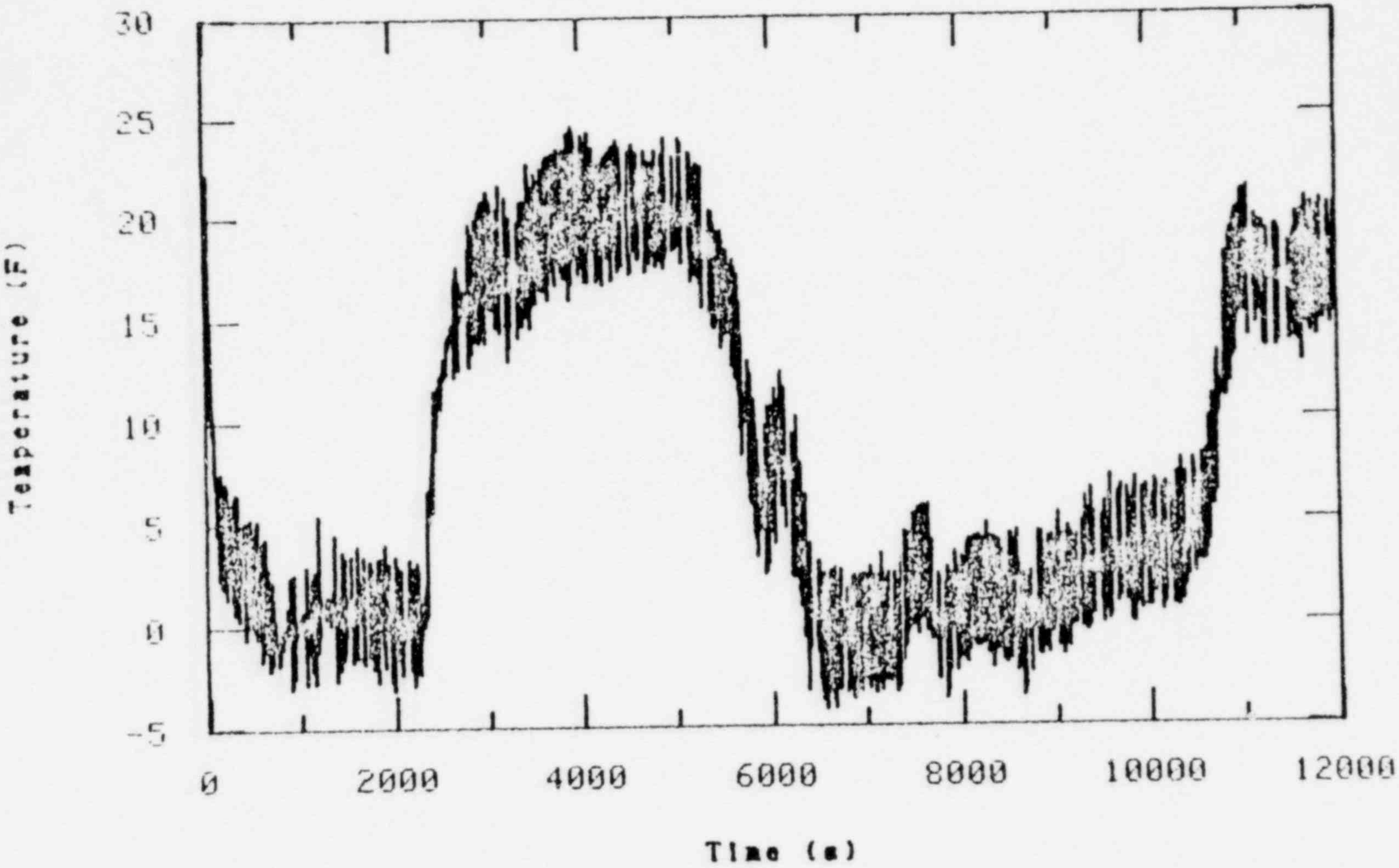
RELAP5 PREDICTED HOT LEG PHASIC VELOCITIES FOR LOFT L3-5



LOFT L3-5/L3-5A PREDICTED PRIMARY PRESSURE



LOFT L3-5/L3-5A CORE TEMPERATURE DIFFERENCE



CONCLUSIONS

- QUANTIFIED PRIMARY SYSTEM MASS INVENTORY AND DISTRIBUTION FOR PUMPS OFF CASE.
- DEMONSTRATED DECAY HEAT REMOVAL MECHANISMS AND TRANSITIONS.

    SINGLE PHASE N.C.
    TWO PHASE N.C.
    REFLUX FLOW INDICATED

- PREDICTED MAJOR PHENOMENA IN SEQUENCES.

RESOLUTION
PLAN FOR
PUMP TRIP DURING
LOCAs

Presented to ACRS ECCS
Subcommittee
October 22-23, 1980

Sheron #2

WHAT HAS BEEN LEARNED FROM PUMPS ON/OFF RESEARCH TESTING IN SEMISCALE?

o STAFF HAS GAINED ASSURANCE THAT 1-D EQUILIBRIUM MODELS SHOULD REASONABLY PREDICT QUALITATIVE BEHAVIOR OF SBLOCAs IN PWRs WITH PUMPS ON

o TESTING SHOWED THAT:

  o INITIAL INVENTORY BEHAVIOR (PRIOR TO ACCUMULATOR INJECTION) IS CONSISTENT WITH LARGE PWR PREDICTIONS OF INVENTORY BEHAVIOR

  o ACCURATE, QUANTITATIVE PREDICTIONS APPEAR HIGHLY DEPENDENT UPON CERTAIN MODELING ASPECTS, FOR EXAMPLE:

    - PUMP TWO-PHASE DEGRADATION
    - BREAK FLOW SUBCOOLING

  o PREDICTED STRONG DEPENDENCY OF BREAK FLOW SUBCOOLING IN ASSUMED PUMP OPERATION FOR LARGE PWRs CONFIRMED BY TESTS.

  o MANY OF MODELING CONCERNS IDENTIFIED IN NUREG 0623 BORNE OUT BY SEMISCALE TESTS.

BASED ON REVIEW OF ISSUE TO DATE, STAFF HAS REACHED CONCLUSION THAT PUMPS SHOULD BE TRIPPED IN EVENT OF LOCA.

<u>REASONS</u>

- o PUMPS NOT DESIGNED TO PERFORM FOR EXTENDED PERIODS IN TWO-PHASE FLUID/HIGHER LIKELIHOOD OF FAILURE
- o SMALL BREAKS ARE INITIATORS OF SCENARIOS LEADING TO INADEQUATE CORE COOLING. EARLY TRIP HELPS ASSURE PUMP AVAILABILITY LATER ON AS MEANS TO TRY AND COOL CORE IN EVENT OF ICC.
- o LIMITED NUMBER OF "BEST ESTIMATE" ANALYSES PERFORMED TO DATE SHOW MOST PROBABLE SMALL BREAKS DO NOT SIGNIFICANTLY CHALLENGE CORE INTEGRITY WITH PUMPS TRIPPED.
- o BOTH BE AND EM ANALYSES SHOW CORE PROTECTED FOR ALL SBLOCAs WITH PUMPS TRIPPED. UNACCEPTABLE PUMP TRIP DELAY "WINDOW" EXISTS WITH PUMPS RUNNING FOR EM ANALYSES. ACCEPTABILITY OF BE ANALYSES WITH PUMPS ON/OFF DELAYED TRIP STILL UNCERTAIN.

- o WHAT IS NEEDED IS BETTER CRITERIA FOR WHEN PUMPS SHOULD BE TRIPPED.
- o BOTH CE AND B&W TO DATE HAVE RETAINED ORIGINAL IE BULLETIN (79-05C,06C) CRITERIA OF ESFAS ACTIVATION ON LOW PRESSURE AS ORIGINALLY PROPOSED BY B&W.
- o <u>W</u> TRIP CRITERIA IS ON LOW PRESSURE DERIVED FROM STEAM GENERATOR SECONDARY SAFETY VALVE SETPOINT.
- o BASED ON EXPERIENCE TO DATE WITH NON-LOCA DEPRESSURIZING TRANSIENTS, STAFF BELIEVES <u>W</u> CRITERIA ACCEPTABLE.
- o WE WILL PROBABLY REQUIRE LICENSEES WITH CE AND B&W REACTORS TO REVISE PRESENT CRITERIA IN ORDER TO REDUCE FREQUENCY OF RC PUMP TRIP FOR NON-LOCA DEPRESSURIZING TRANSIENTS.

THEN WHY REQUIRE LICENSEES TO PREDICT L3-6?

- STAFF AGREES THAT MANUAL PUMP TRIP IS DESIRABLE. INDUSTRY HAS NOT PRODUCED MODELS BENCHMARKED AGAINST APPLICABLE DATA TO SUPPORT THIS RECOMMENDATION.
- LICENSEES MUST SHOW THAT OPERATOR HAS SUFFICIENT TIME TO RECOGNIZE EVENT AND TAKE PROPER ACTION (TRIP PUMPS)
- PREVIOUS VENDOR ANALYSES SHOWED THE FOLLOWING.

VENDOR		W	CE	B&W
MIN. TIME AVAILABLE TO TRIP RC PUMPS	BE	>10M	10M	?
	EM	10M	6M	2M*

*EM ANALYSIS BUT WITH 2 HPI'S

- STAFF WILL ACCEPT MANUAL PUMP TRIP PROVIDED EACH LICENSEE CAN DEMONSTRATE THAT
  - WITH REVISED CRITERIA FOR PUMP TRIP (E.G., LOW PRESSURE, LOSS OF SUBCOOLING, ETC..), OR ASSUMING AT LEAST 10 MINUTES FOR OPERATOR ACTION (WHICHEVER IS LARGER) APPENDIX K LIMITS CAN STILL BE MET.
- IF CRITERIA ABOVE CANNOT BE MET, (I.E., MUST TRIP IN < 10 MIN. TO MEET APPENDIX K), STAFF WILL STILL CONSIDER MANUAL TRIP ACCEPTABLE PROVIDED LICENSEE CAN DETERMINE THAT
  - FAILURE OF THE PUMPS TO TRIP WHEN REQUIRED (DUE TO OPERATOR ERROR, TRIP CIRCUIT MALFUNCTION, ETC.) AND
  - DELAYED PUMP TRIP UNTIL "WORST" TIME INTO ACCIDENT WOULD NOT PRODUCE UNACCEPTABLE CONSEQUENCES USING "BEST ESTIMATE" MODELS AND ASSUMPTIONS.

PRESENT STAFF THINKING IS THAT INABILITY TO DEMONSTRATE ITEMS 1 AND 2 WOULD REQUIRE AUTOMATIC PUMP TRIP.

BENEFITS OF ABOVE APPROACH

- ELIMINATES NEED FOR AUTOMATIC TRIP CIRCUITRY
- RETAINS ADDITIONAL DEGREE OF PLANT CONTROL WITH OPERATOR

DRAWBACKS

- REQUIRES GREATER CONFIDENCE IN ANALYTICAL MODELS
- MAY REQUIRE SPECIAL ANNUNCIATOR/ALARM IN CONTROL ROOM

POOR ORIGINAL

Semiscale Experiment Program

Presented by

Paul North

EG&G Idaho Inc

North #1

Small Break and Transient Experiment Plans Must Consider:

- High priority licensing needs
- General thermal-hydraulic research needs
- Required system modifications
- Coordination with other programs

High Priority Licensing Needs

INEL-S-28 420

Integral small break LOCA with/without UHI

- Objectives
  - Data for UHI vendor code assessment
  - Data for comparative analysis of effects of UHI
- Experiment Needs
  - Small break integral experiments with and without UHI

INEL-S-28 421

Rapid cooldown on natural circulation

- Objectives
  - Provide data on bubble formation and dissipation for code comparisons
  - Investigate effects of different techniques for pressure reduction with bubble, e.g., CE drain and fill method
- Experiment Needs
  - Rapid cooldown integral experiment

5

Effects of incondensible gas on natural circulation

Included in general research on natural circulation

General Thermal-Hydraulic Research Needs

Natural Circulation Associated with Small Breaks

- Objectives
  - Provide data to support assessment of capability to calculate three circulation regimes and transition between regimes
  - Examine effects of various secondary conditions and of presence of incondensible gas in primary

Single Loop Experiments
Steady State

- Six tests
- Establishment of three circulation regimes and transitions
- Effects of steam generator secondary conditions
- Effects of incondensible gas

INEL-S-28 426

Integral Experiments

- Three tests - 1 steady state, 2 transient
- Effects of unbalanced steam generator secondary conditions
- Small break transients with/without ECCS

Station Blackout Transient

- Objectives
  - Provide data to support assessment of capability to calculate major phenomena associated with transient
  - Examine effects of recovery techniques assuming availability of diesel power

System Modifications

- Preservation of Prototypic Elevations - Intact Loop Steam Generator
- Secondary Fluid Volume - Steam Generator Filler Pieces
- System Boundary Conditions - Energy
    - external insulation
    - internal insulation
    - surface heaters
  - Mass
    - leak prevention program
- Instrumentation - Steam Generator
  - Pantlegs

Coordination with Other Programs

October 31, 1980 Meeting

- NRC (EG&G) - LOFT, Semiscale
- FRG - PKL, LOBI, UPTF
- JAERI - ROSA-IV, TPTF, ROSA-III

Conclusions

- Commitment to small break and transient experiments - exploratory experiments completed
- System modifications in process
- Proposed experiments - Responsive to licensing needs
  - Address general research needs
- Coordination with other experiment programs

KEY RESULTS OF SEMISCALE TRANSIENT THERMAL-HYDRAULIC TESTS

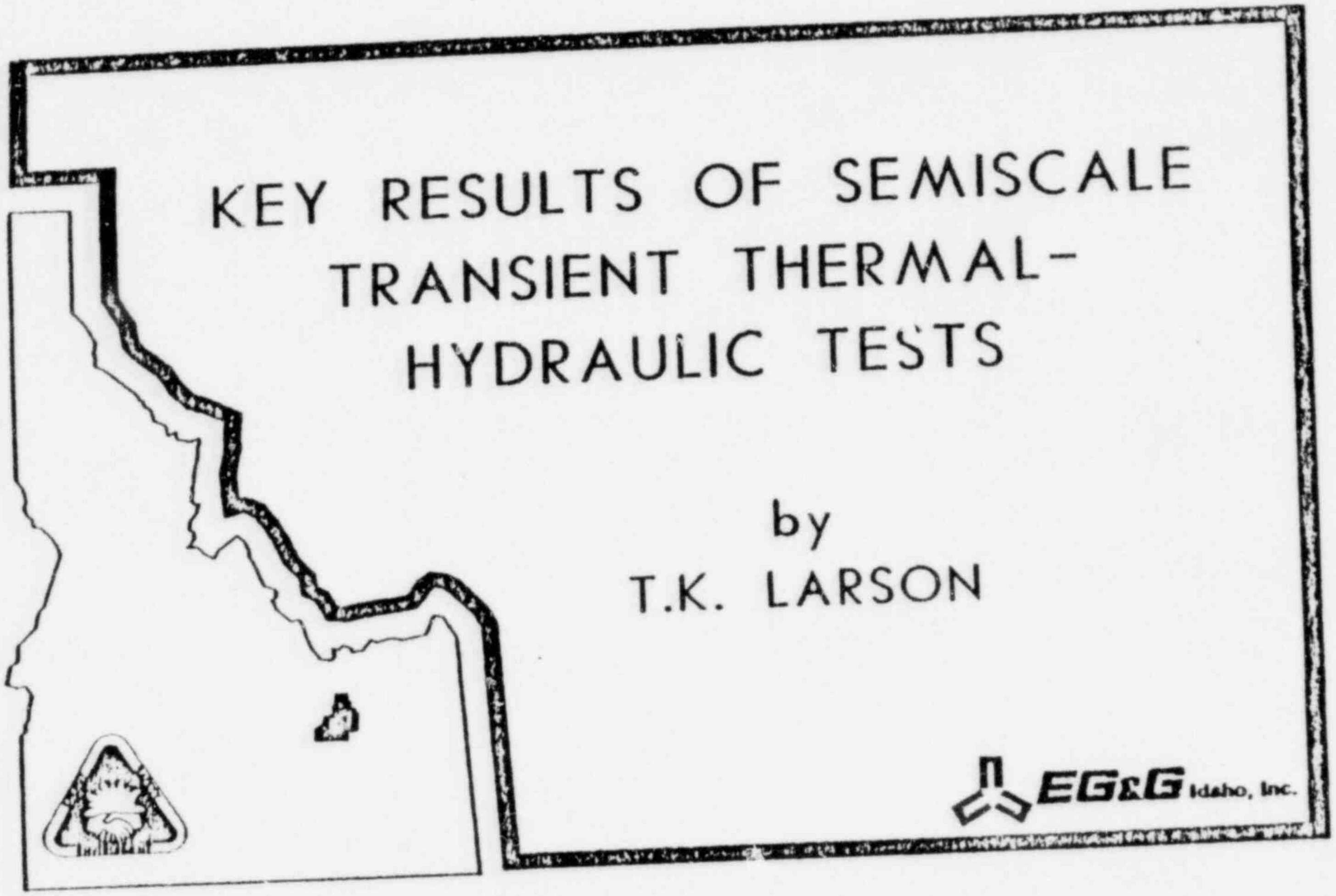
BY

T. K. LARSON

ACRS MEETING
IDAHO FALLS, ID
OCTOBER 22, 1980

EG&G Idaho, Inc.

Larson 1



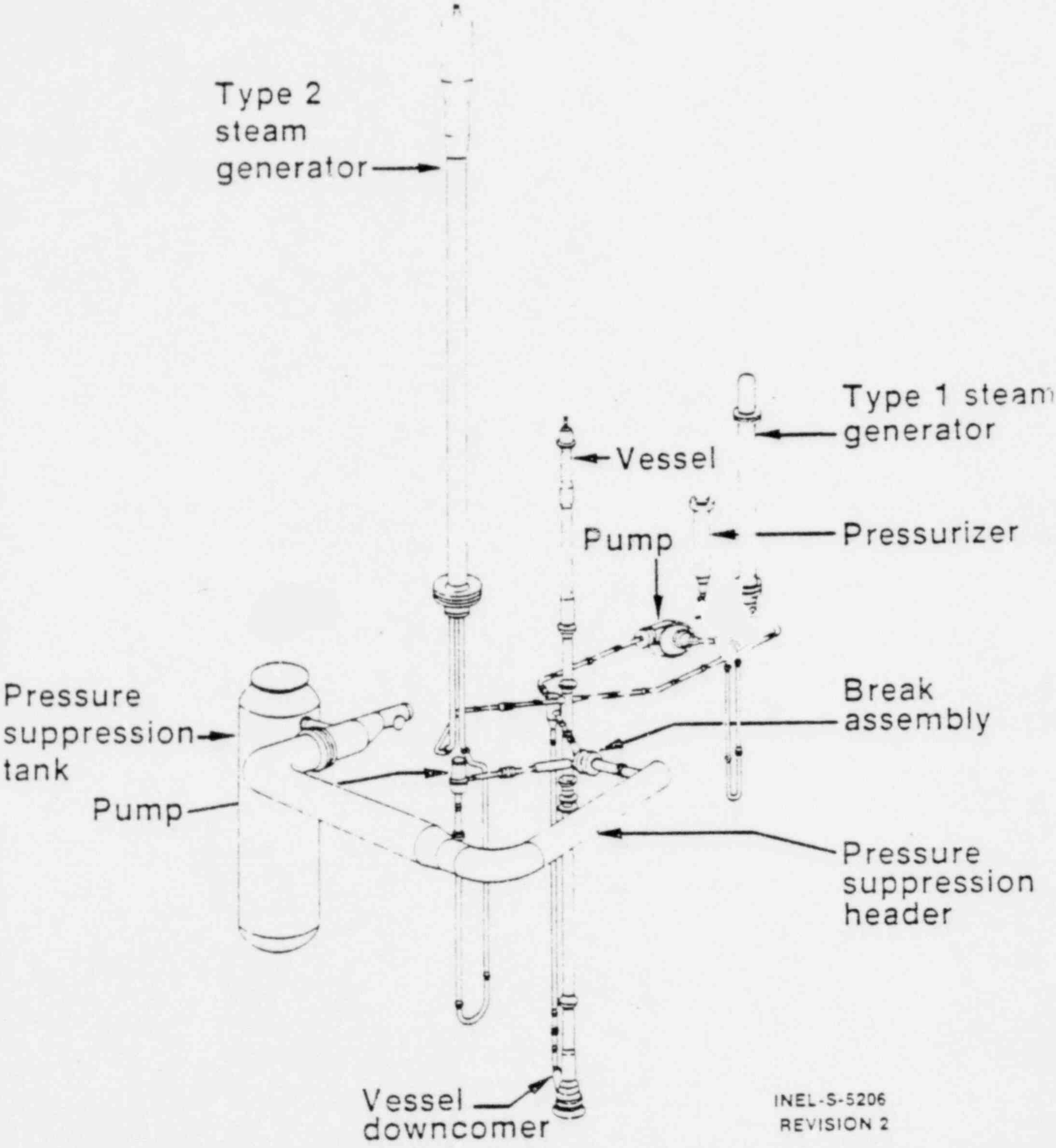
KEY RESULTS OF SEMISCALE TRANSIENT THERMAL-HYDRAULIC TESTS

by

T.K. LARSON

EG&G Idaho, Inc.

Semiscale Mod-3 System



INEL-S-5206
REVISION 2

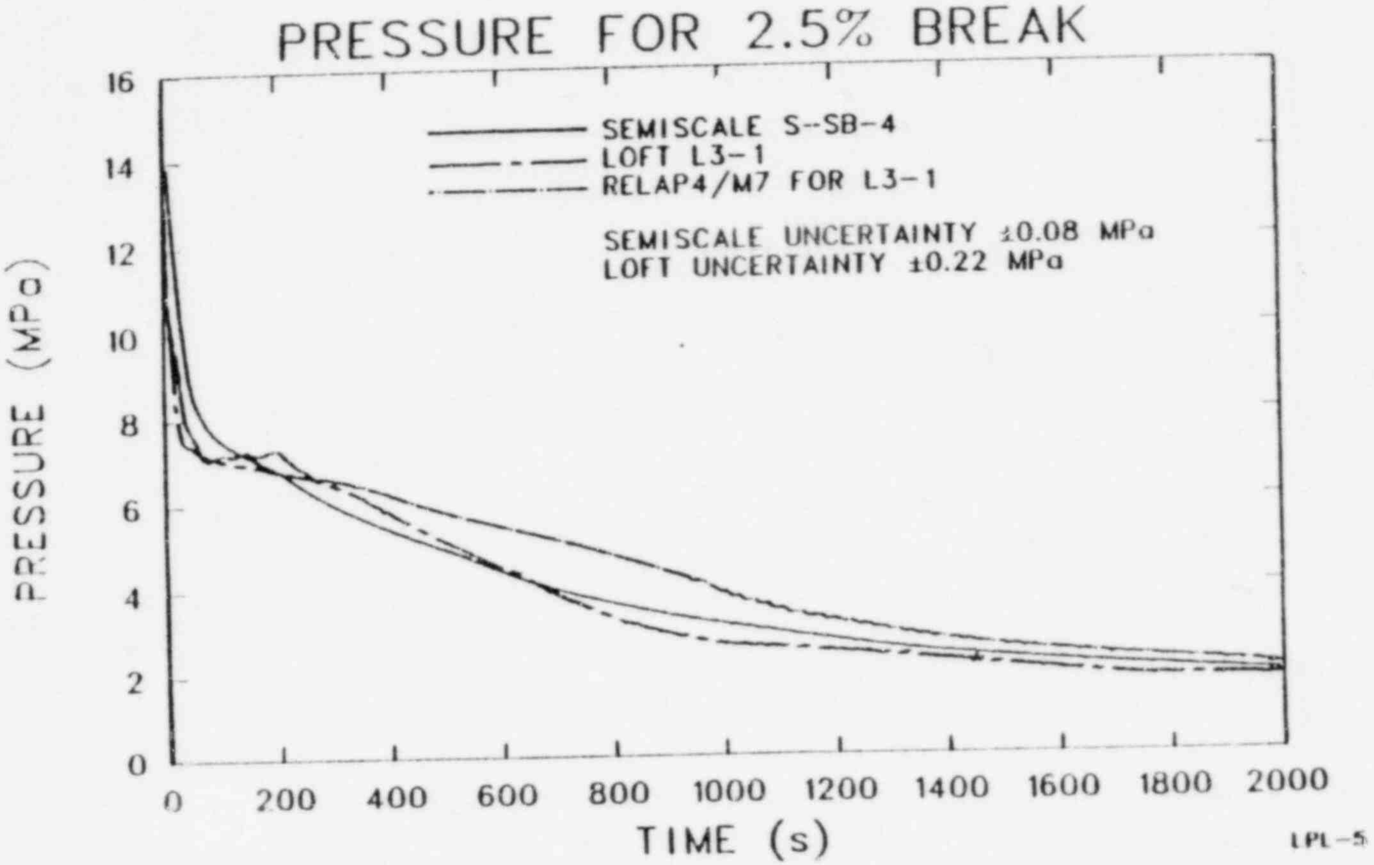
SEMISCALE MOD-3 TESTING
(FY-1980)

TEST	TYPE	OBJECTIVE(S)
S-SB-2.2A, 4.4A	2.5% COLD LEG BREAK	LOFT TEST L3-1 AUDIT CALCULATIONS
S-TR-1.2	STATION BLACKOUT	SYSTEM OPERATION THERMAL HYDRAULIC BEHAVIOR
S-07-10D	10% COLD LEG BREAK	NRC STANDARD PROBLEM

LPL-3

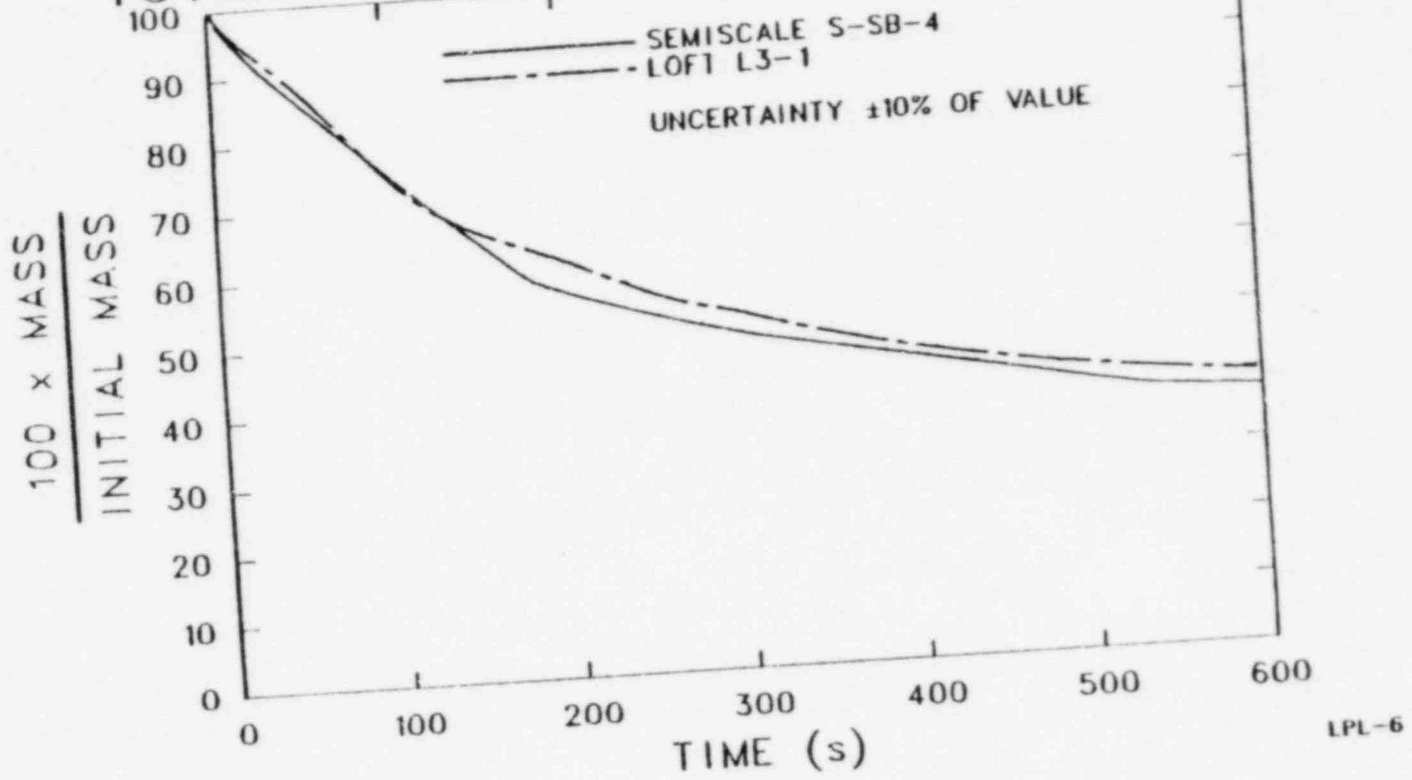
INITIAL AND OPERATING CONDITIONS

PARAMETER	S-SB-4/4A	LOFT L3-1
POWER (MW)	1.2 / 1.2	49
PRESSURE (MPa)	14.8 / 15.1	15.0
COLD LEG T (K)	558.2 / 558.7	554
△T (K)	20.0 / 19.2	20
BREAK SIZE, LOCATION	2.5% COLD LEG	2.5% COLD LEG
HPIS	1 TRAIN	1 TRAIN
LPIS	1 TRAIN	1 TRAIN



PRESSURE FOR 2.5% BREAK

TOTAL SYSTEM MASS FOR 2.5% BREAK



TEST S-07-10D

INITIAL AND OPERATING CONDITIONS

- INITIAL POWER - 1.94 MW
- INITIAL PRESSURE - 15.7 MPa
- CORE ΔT - 35 K
- CORE FLOW - 9.72 kg/s

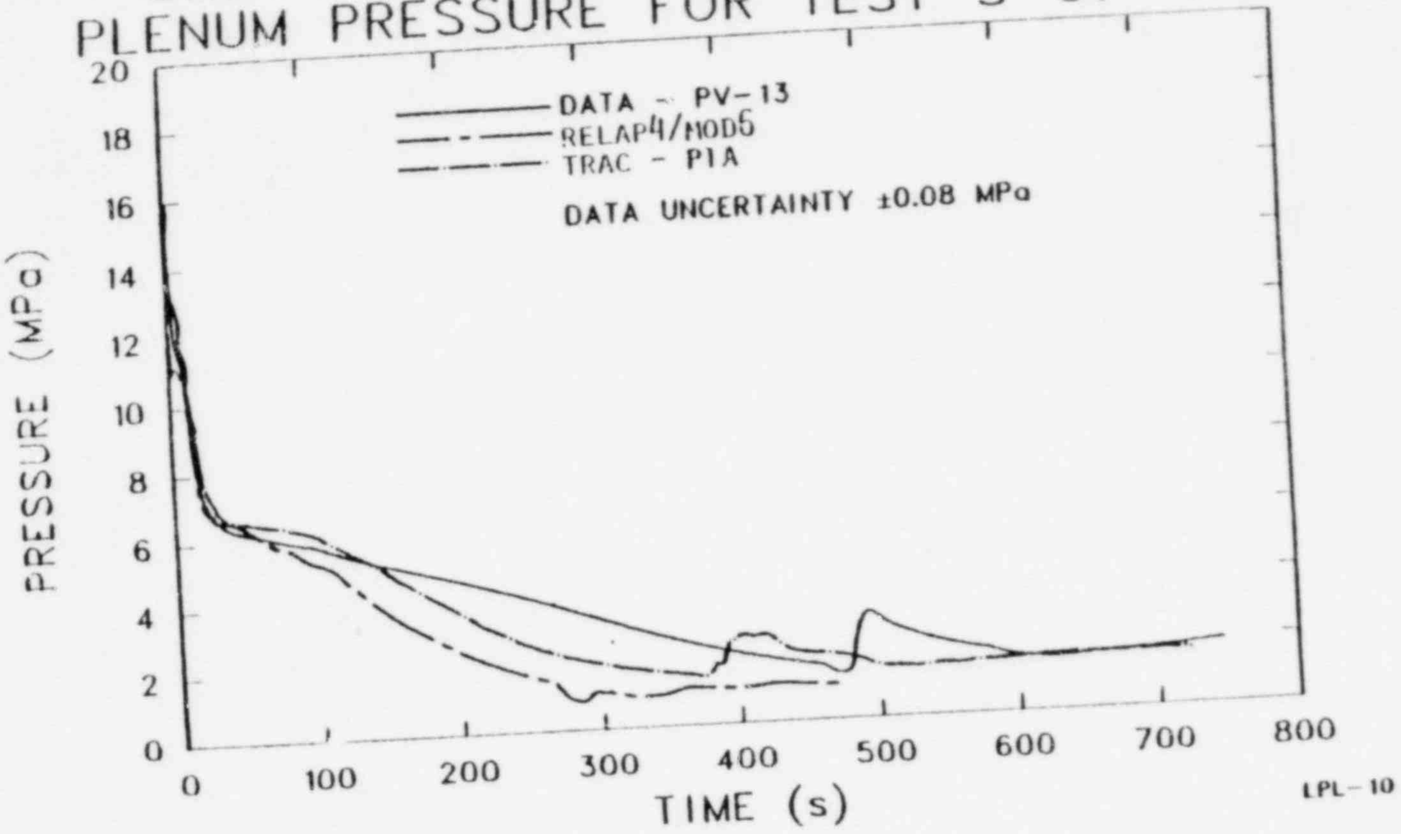
TEST S-07-10D (CONT.)

INITIAL AND OPERATING CONDITIONS

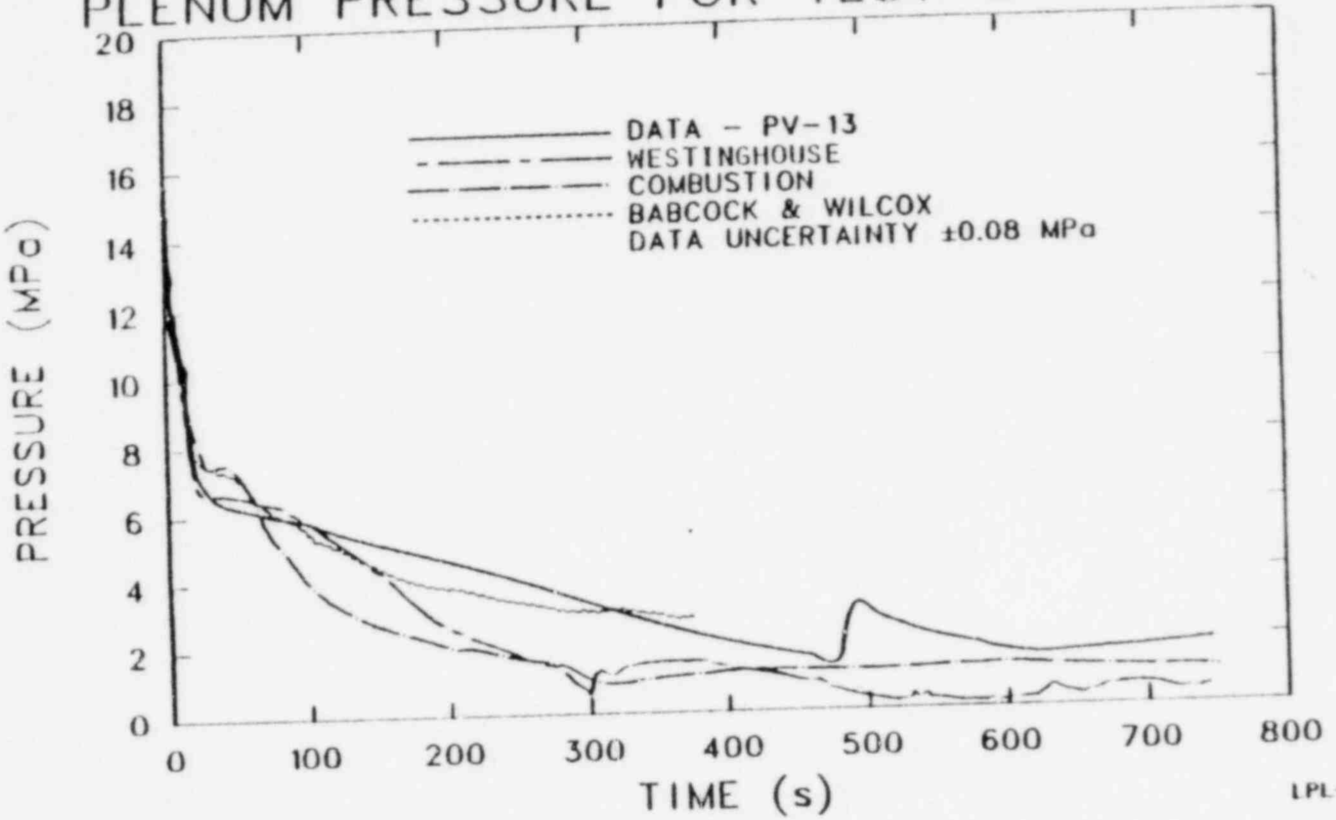
- CORE SCRAM WHEN PRESSURIZER PRESSURE REACHED 12.41 MPa
- PUMP TRIP AFTER PRESSURIZER PRESSURE REACHED 12.41 MPa
- FEEDWATER AND STEAM VALVE TRIP AFTER PRESSURIZER PRESSURE REACHED 12.41 MPa

L2L-9A

CALCULATED AND MEASURED UPPER PLENUM PRESSURE FOR TEST S-07-10D

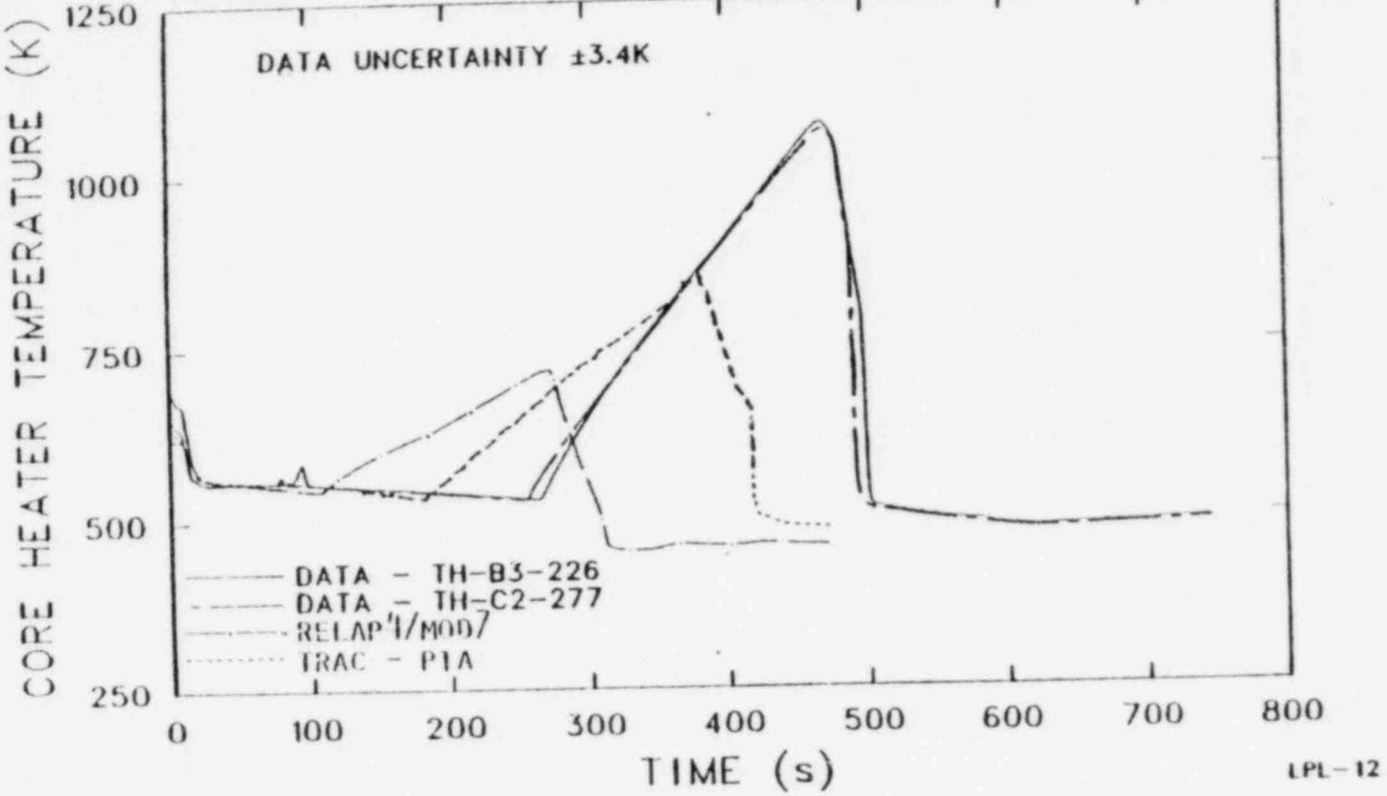


CALCULATED AND MEASURED UPPER PLENUM PRESSURE FOR TEST S-07-10D

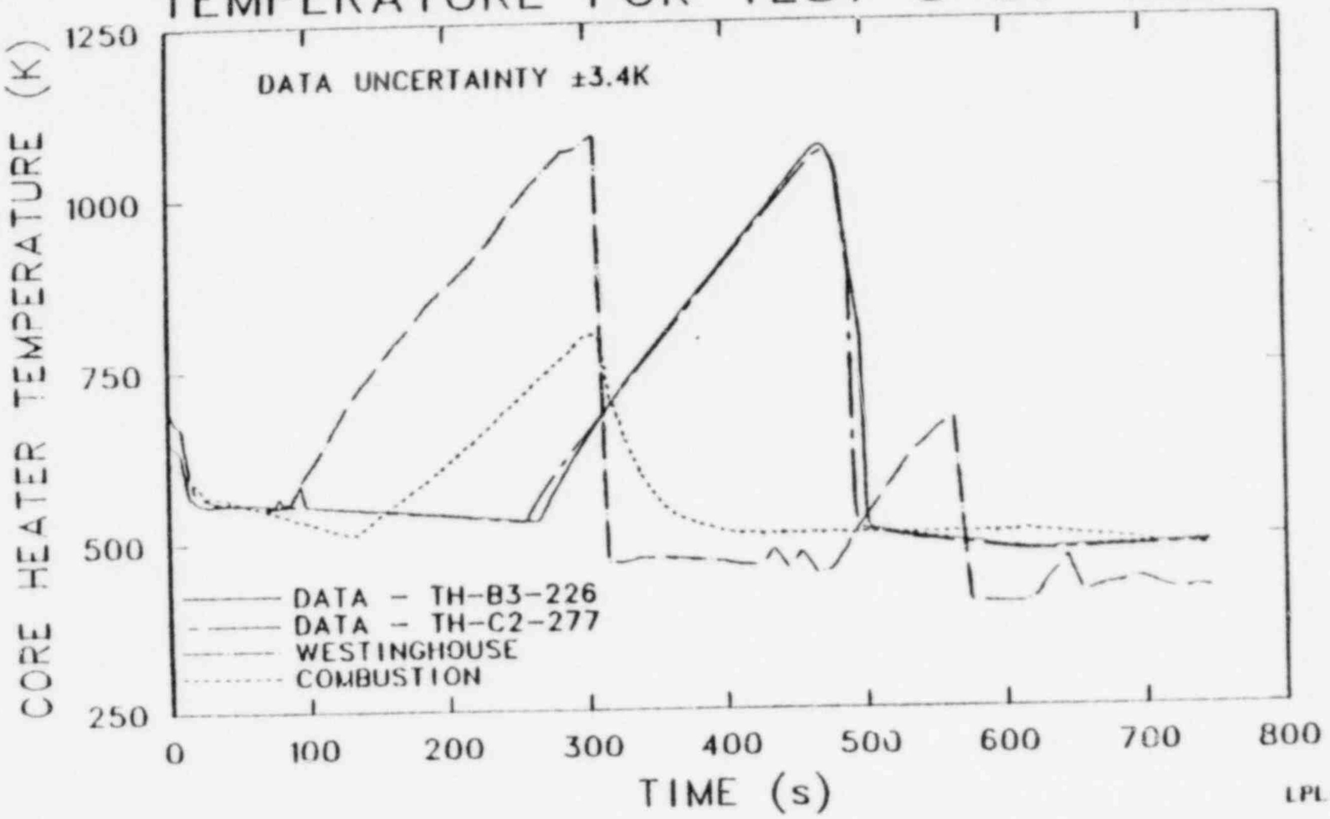


LPL-11

MEASURED AND CALCULATED CLADDING TEMPERATURE FOR TEST S-07-10D



MEASURED AND CALCULATED CLADDING TEMPERATURE FOR TEST S-07-10D



LPL-13

BLACKOUT SIMULATIONS
TESTS S-TR-1, S-TR-2

INITIAL AND OPERATING CONDITIONS

- INITIAL POWER - 1.97 MW
- CORE ΔT - 34 K
- CORE FLOW - 11.7 kg/s
- CORE POWER DECAY BEGINNING AT 3.4 s
- FEEDWATER VALVE CLOSED AT 5 s

LPL-14

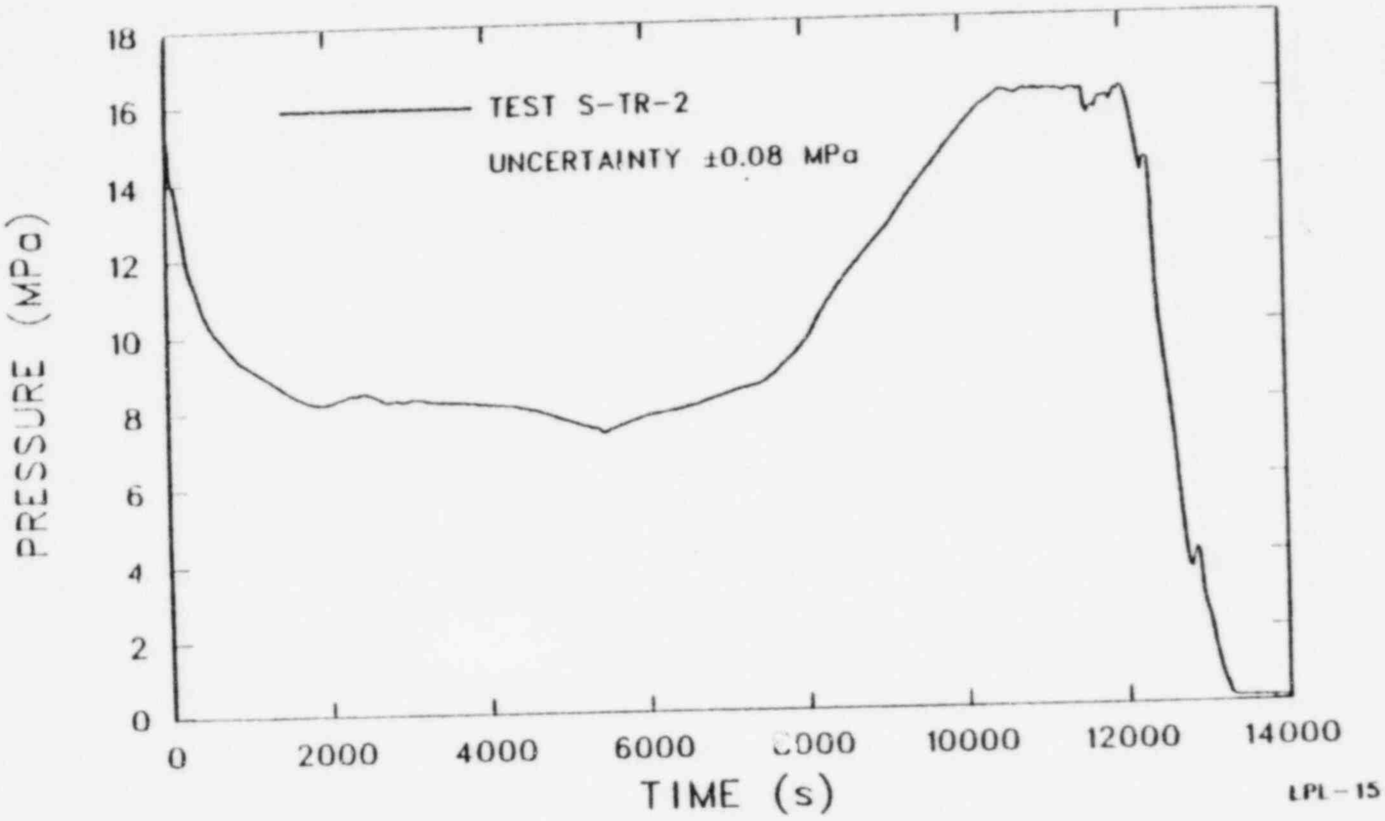
BLACKOUT SIMULATIONS
TEST S-TR-1, S-TR-2 (CONT.)

INITIAL AND OPERATING CONDITIONS

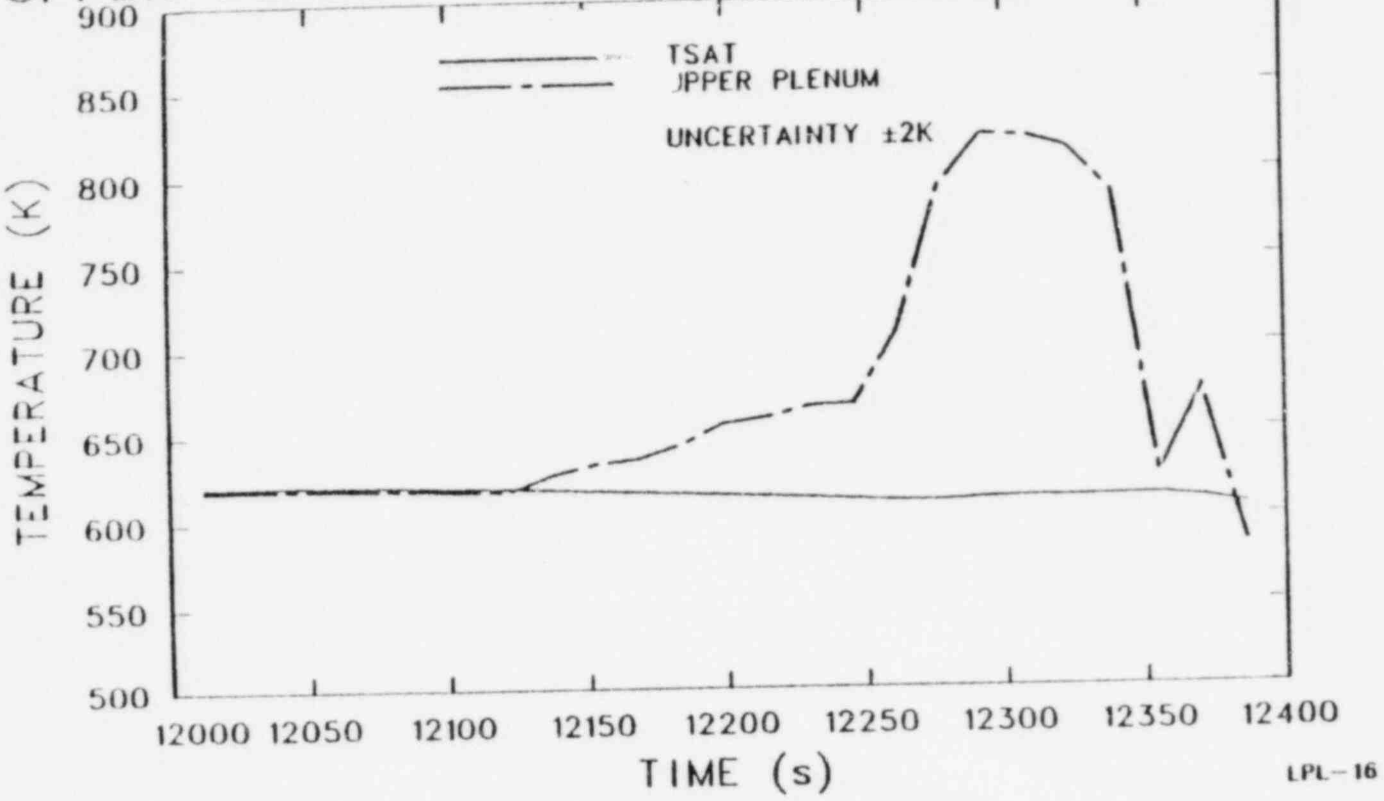
- PRIMARY PUMPS TRIPPED AT 60 s
- STEAM GENERATOR SECONDARY VOLUMES DRAINED AT 57 MIN
- CORE POWER REDUCED TO DECAY HEAT AFTER CORE UNCOVERS

LPL-14A

SEMISCALE "BLACKOUT" SIMULATIONS

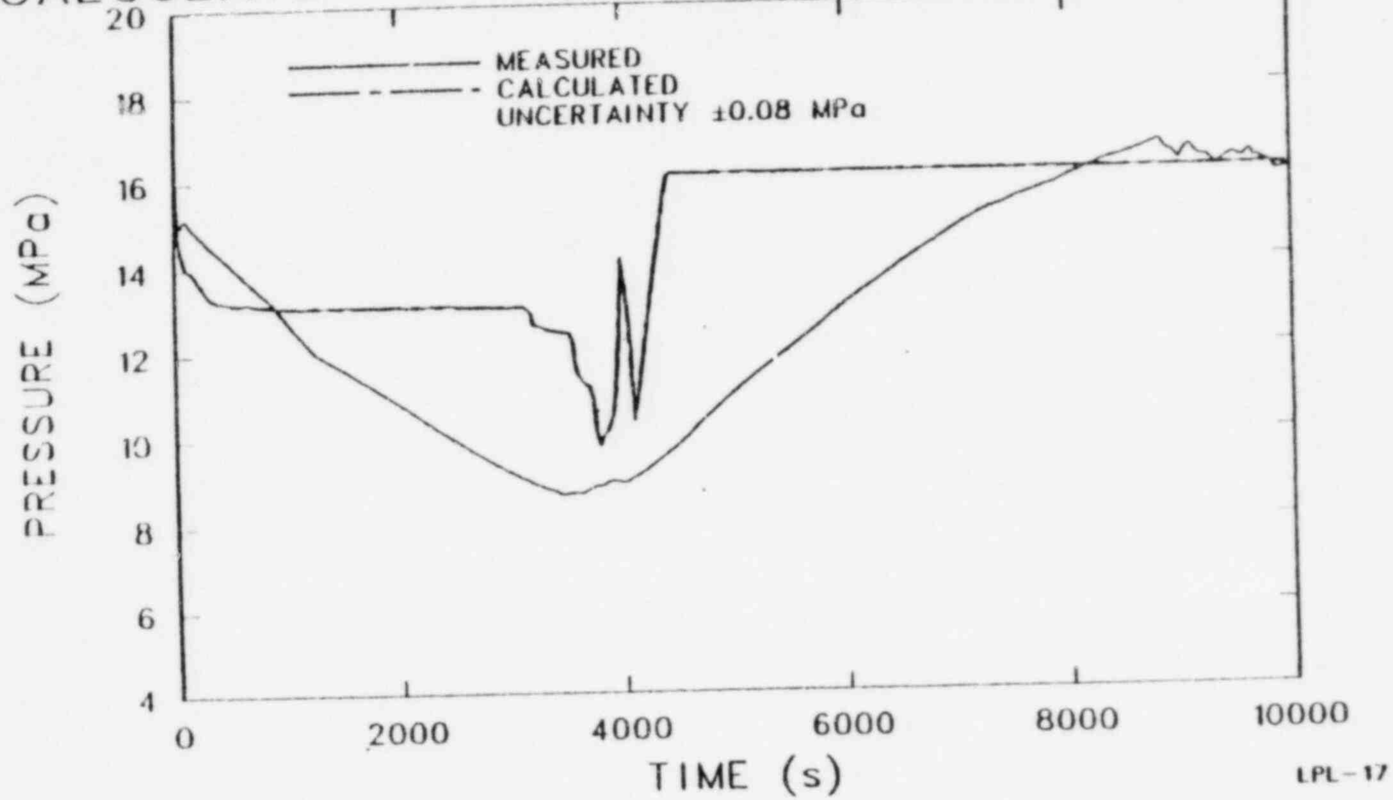


SEMISCALE "BLACKOUT" SIMULATIONS UPPER PLENUM FLUID & SATURATION TEMPS.

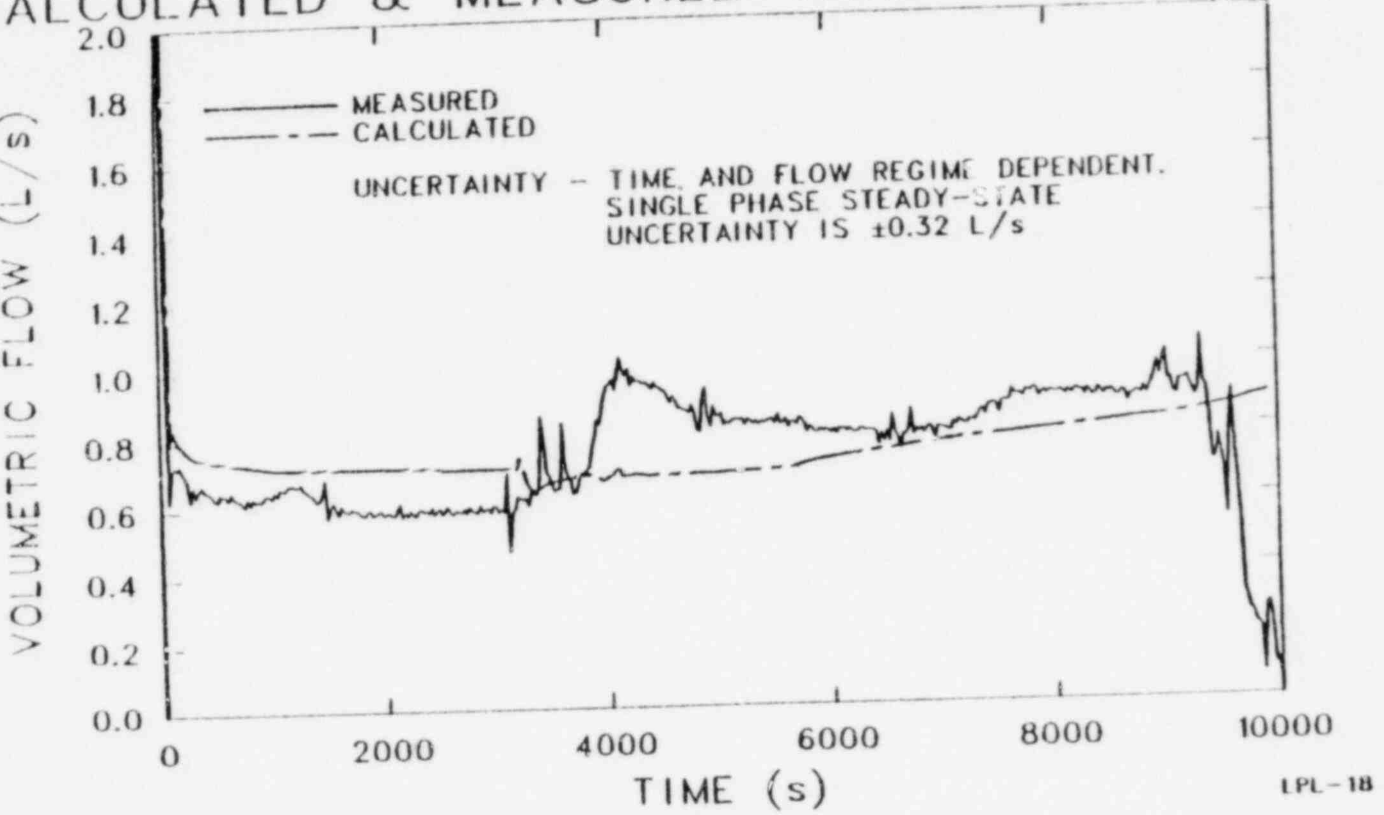


LPL-16

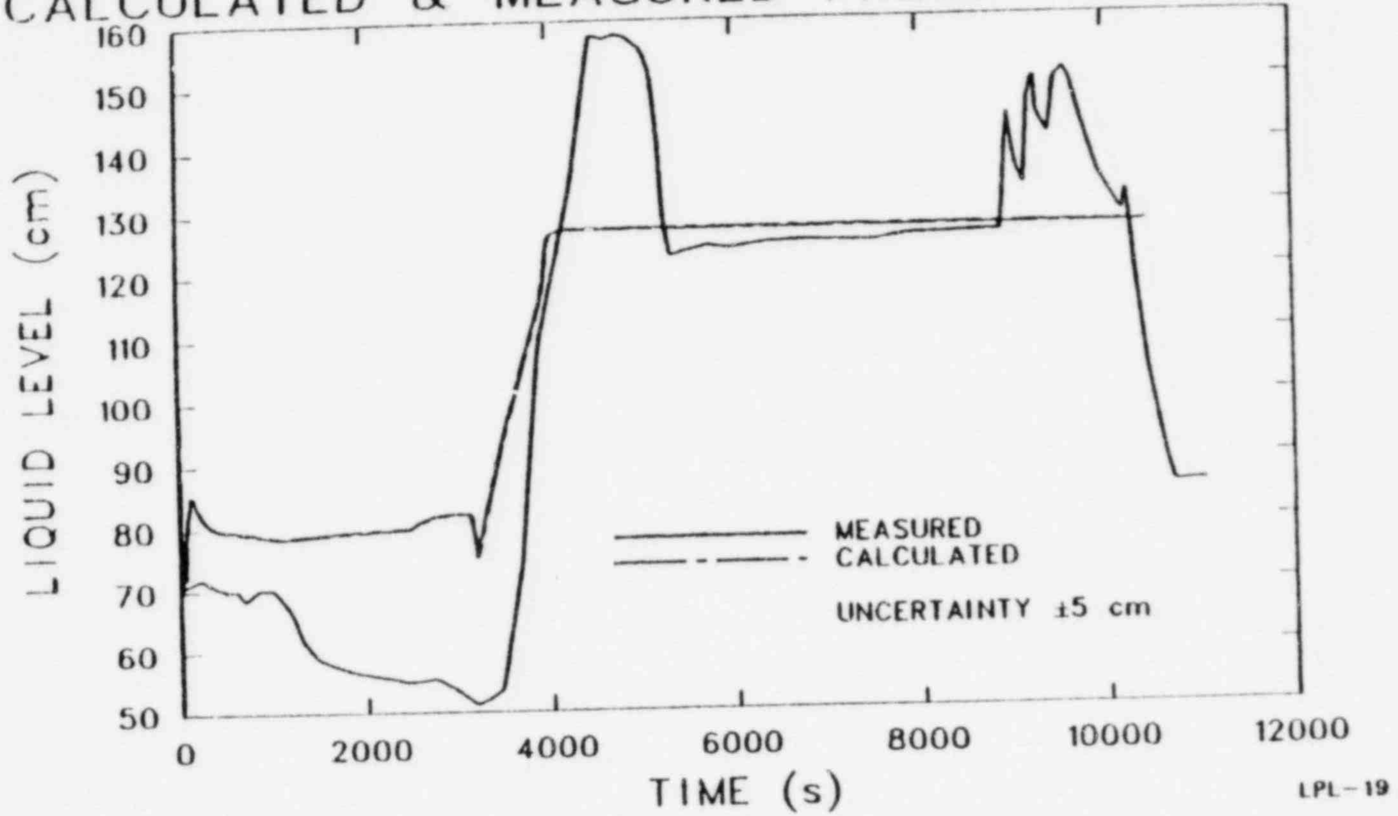
SEMISCALE "BLACKOUT" SIMULATIONS CALCULATED & MEASURED PRIMARY PRESSURE



SEMISCALE "BLACKOUT" SIMULATIONS
CALCULATED & MEASURED INTACT LOOP FLOW



SEMISCALE "BLACKOUT" SIMULATIONS CALCULATED & MEASURED PRESSUIZER LEVEL



CONCLUSIONS

- LOFT TEST L3-1 SAFE (CONFIRMED)
- S-SB-4 COMPARED WELL TO L3-1 ⟹ GOOD SCALING
- LARGE HEAT LOSS IN SEMISCALE AFFECTS NATURAL CIRCULATION

LPL-20

CONCLUSIONS (CONT.)

- INCREASED CORE POWER TO OFFSET HEAT LOSS ONLY PART SATISFACTORY
- SIGNIFICANT DISAGREEMENT BETWEEN CALCULATIONS AND DATA FROM S-07-10D

CONCLUSIONS (CONT.)

- SMALL BREAK ANALYSIS CAPABILITY IMPROVED
- STATION BLACKOUT EVALUATIONS REQUIRE NON-EQUILIBRIUM ANALYSIS; EVALUATION OF P/T LIMITS

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