UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

SUBCOMMITTEE MEETING ON

EMERGENCY CORE COOLING SYSTEMS

Vienna Room
Huntington Sheraton Hotel
1401 Oak Knoll Avenue
Pasadena, California

Friday, March 27, 1981

The meeting of the Subcommittee was convened, pursuant to notice, at 9:37 a.m.

PRESENT:

DR. M. S. PLESSET, Chairman,
W. Mathis
D. Ward
I. Catton
Y.S. Chen
A. Acosta
J. Lienhard
T. Wu
P. Boehert
Z. Zudans

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P R O C E E D I N G S

MR. PLESSET: The meeting will come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Emergency Core Cooling Systems.

I am M.S. Plesset, Subcommittee Chairman. Other ACRS members present today, on my left, are Mr. Mathis and Mr. Ward. Also in attendance are our ACRS consultants, Dr. Catton, Professor Linehard, Professor Wu, Dr. Zudans, on my left, and ACRS senior fellow, Dr. Chen.

The purpose of this meeting is to discuss the future test program and the loss of fluid test facility. The meeting is being conducted in accordance with the provisions of the Federal Advisory Act and the Sunshine in Government Act.

Mr. Paul Boehert 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 the meeting previously published in THE FEDERAL REGISTER of March 12, 1981.

A transcript of the meeting is being kept and will be made available as cited 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 will receive no written comments or requests for time to make oral statements from members of the public.

I might qualify that. I expect Dr. Karwat from Munich to be here a little later and I may call on him to make some comments.

Before we go to the detailed schedule of this meeting, let me make a few remarks, primarily directed to the consultants.

It has been pointed out that this subcommittee, like many other subcommittees of the ACRS, act more in a passive or a reactive mode to proposals for safety research and other matters and it has been mentioned that possibly this subcommittee in particular should be in a more active mode.

I would like to suggest to our consultants that they consider this possibility of our being in a more active mode as to the programs and LOCA and transient research and to make some comments -- and I don't want them to do this in a rush; we have time -- but to make some comments on the program as to what it might be -- what it should be, perhaps -- keeping in mind safety research goals and also the needs of the Nuclear Regulatory -- NRR branch -- needs.

So that is the first general comment I am making to the consultants.

The other comment I want to make before we go into the schedule is the following: Last week the -- I believe it was last week -- the full committee wrote its approval for San Onofre Unit Number 2 to go forward in the licensing process.

In the discussion that the full committee had on that, it was brought out that the DNBR -- the departure from nuclear boiling ratio -- for this plant, which is a combustion engineering plant, is 1.19.

I raised the question to the applicant -- Southern California Edison Company -- regarding the apparently very low value for this DNBR and wondered if, as a result of this, there was any consideration perhaps to limiting the modes of operation for this plant.

Well, as you might imagine, this caused a considerable stir not only in the minds and hearts of the applicants, but also the combustion engineering licensing people who were there who maintained that this was a more conservative DNBR than one ever had, which of course is not quite true.

What I would like is for the consultants to consider this question of the DNBR -- 1.19 -- and what they think of it.

Some of you may not know what a DNBR is, but it would be very interesting for you to find out, even if you don't, and for those of you who do, I would appreciate your comments regarding it.

Now this is of sufficient importance that another subcommittee -- in which this subcommittee may participate -- may play a role, but I think it is a significant point and one that we have to keep in mind.

After we get into our regular agenda, I notice that Dr. Karwat is here, I am going to call on him today for some comments. Before that let me first ask, before we go into the regular agenda, if any of our ACRS members or consultants want to make any comments for the record.

Dr. Chen?

DR. CHEN: About the DNBR, 1.19, the combustion engineering have a proposal about 1.13. The NRC staff raised it to 1.19. I think I would like to see the subcommittee have another meeting on this DNBR because it is very important. How did they get to 1.19?

It was dropped from 1.130 down to 1.13.

MR. PLESSET: And then it was raised back up to 1.19. Let me --

DR. CATTON: If I might add one more thing?

MR. PLESSET: Yes, in a minute, but what I was just going to say was, the NRC staff seemed quite content with the 1.19. I didn't know that it had been down as low as 1.13 but anyway, that is an interesting comment. Yes, Dr. Catton?

DR. CATTON: It is just there is also movement afoot within combustion engineering to push to greater burnout and I don't know how that would impact on a decrease of DNBR because the DNBR ration is to avoid fuel damage and if you had more burnout, I would think that would be more critical.

MR. PLESSET: You begin to get a little bit less margin, maybe.

Does anyone else want to make a comment on this point? I would like for our members and consultants to give some thought to this point because we have a little time. Even though the ACRS letter gave approval for the operation of the plant, it will be a little while before they actually go into operation.

Yes, Dr. Zudan?

DR. ZUDAN: Was it made clear during the proceedings in San Onofre, which I didn't participate in, what is the accuracy with which they can measure this DNBR?

MR. PLESSET: That was not gotten into at all.

DR. ZUDAN: I remember participating in the review of System 18 several years ago and there the ratio was 1.3, but they could not prove that they could really measure with good accuracy at .3 and I don't know what has happened in between.

MR. PLESSET: Let me see if I have it straight. What you are saying is that 1.3, plus or minus .3?

DR. ZUDAN: They could not define what the accuracy was of their measurement but that was several years ago.

MR. PLESSET: Yes, that is another interesting point. Did you want to make a comment?

MR. WARD: As I recall, the basis for the 1.19 was a body of new experimental data which was statistically tighter.

I think their point was the confidence limits on the 1.19 are the same as the confidence limits on the previous value of 1.3 or whatever it is.

MR. PLESSET: Yes, I know they talked about the data and the confidence limits and they were 95 percent confidence limits, but that is, to me, just hearsay. I really don't know in detail just how valid that comment was, but that is true, they did say this at the meeting.

DR. CATTON: There is usually a code that is tied in with these confidence limits and if I remember right with Westinghouse, Westinghouse got locked into that particular code because we were unsure how good a particular piece of data was when it was based on a computer code to reduce it.

So if they changed that code, then the whole question of what the DNBR ration should be is opened up again. I don't recall any of this going on with combustion engineering and I don't know that they have as sophisticated a code as the one that Westinghouse had.

MR. PLESSET: I remember, now that you mention that, it was over two years that Westinghouse went into this discussion.

DR. CATTON: And the ECCS Subcommittee was involved with that and there was a great deal of pain in getting to the final position.

MR. PLESSET: Yes, there was a lot of vague discussion.

I guess that is not quite fair, but arm waving and invoking codes and that kind of thing.

Now it is true that a couple of years ago when we went into the Westinghouse DNBR question, that the ECCS Subcommittee was involved. Now last year this should have been done, but for other reasons the ECCS Subcommittee didn't get too closely involved in it, but now I think we will most likely have a joint meeting with the fuel subcommittee on this question, but it would not be amiss for us to get prepared and maybe we might see what data we can get out of combustion angineering and the staff before that.

DR. CATTON: One thing that would be of interest is, I recall a meeting where there was a discussion of using one of the reactors at Idaho for DNBR testing, and of all the reactor vendors, the one that was most interested in getting these tests underway was combustion engineering.

The reason was that combustion engineering is more caught up and the DNBR impacts more strongly on their plant operations than any of the others so they have more incentive to try to push for change.

Further, it was kind of a consensus that the present DNBR is so conservative we ought to be allowed to relax it a little bit, so you wonder just how well this relaxation was monitored when it took place.

MR. PLESSET: I appreciate your comment and I think we have got to be more certain of the background of this point than we are because there are several plants involved.

Next question. Yes?

MR. MATHIS: Mr. Chairman, there have been several cases where, on fuel reload, the DNBR of 1.19 has been used in other cases, so this is nothing unusual.

MR. PLESSET: No.

MR. MATHIS: I thought that ought to be noted.

MR. PLESSET: I don't think it is unusual that it is a little bit of a question in my mind. Just where do we stand. I think that the worst thing that might happen -- but this would be bad enough -- would be that the plant operation would have limits put on it, on the methods of operation.

DR. CATTON: On the other hand, I think that LOFT has demonstrated that you could actually go into fuel boiling and not get the fuel damage that you are forced to face if you exceed the DNBR limit, so there are two sides to the coin and maybe the 1.19 may still be a little bit restrictive.

MR. PLESSET: Yes, that is true.

DR. CATTON: I didn't want to be totally pessimistic.

MR. PLESSET: I don't necessarily feel that it is wrong or bad, but I do think that we should satisfy ourselves of this point.

MR. BOEHERT: There is an ad hoc subcommittee set up to look at the DNBR. It has had a meeting --

MR. PLESSET: Who is on that?

MR. BOEHERT: I think Dr. Schuman is the chairman. I don't know who else is on it. I don't think you are on it.

MR. PLESSET: I think what I would actually rather see than an ad hoc committee is a joint, and you might tell them back there in Washington that I suggest a joint meeting of --

MR. BOEHERT: I know they were in the process of scheduling a meeting.

MR. PLESSET: Well, maybe they should check it out with us because what I propose is a joint meeting of the ECCS subcommittee and the fuel subcommittee and I think this would be more pertinent than an ad hoc committee because I think we -- let me compliment this subcommittee, not for its ACRS members, but for its consultants. I think they have been making a valuable input to the question. That is my view, anyway. I compliment our consultants here.

Let's get back to our agenda and as you know there has been considerable discussion of what should be done with LOFT and the ACRS committee, in its report to the Commissioners last July -- that was NUREG-0699 -- and then in its report to Congress which they made very recently in NUREG-0751 they proposed that LOFT should be shut down in the not-to-distant fugure.

Actually they proposed specifically that the end of fiscal year 1982 be the end of LOFT.

I might add that the Appropriations subcommittee in the House accepted the ACRS recommendation. Going with that was a budget suggestion from the ACRS of $30 million for the fiscal year 1982.

Now the Senate Appropriations subcommittee also accepted the ACRS recommendation. So as of the moment the ACRS recommendation of $30 million terminating budget as far as tests goes will be the end for LOFT.

Now one of the major inputs to this decision was the fact that the LOFT budget had grown to be such a large part of the safety research budget -- like 20 percent -- that it seemed all out of proportion and made it not possible to carry on many items in safety research that the committee thought were much more viable.

Now in some sense I think that this large budget in LOFT was a result of what I would call -- maybe I should call it -- mismanagement on the part of the research office of NRC, which allowed this thing to get a lot of features that had nothing much to do with loss-of-flow testing.

So the way to cure a cancerous kind of growth in this thing, it seemed the best way would be just to get rid of the patient altogether, which is what the ACRS recommended, because sometimes you can't get any surgery without getting rid of the whole body.

Anyway, the Commissioners appointed a review group to review this whole question and we will hear discussion of these points and other related items by Dr. Harold Sullivan from the research office of NRC, so I would like to turn the meeting over to you, Harold, and would you want to continue?

DR. SULLIVAN: Could we first just talk about the agenda a little bit?

MR. PLESSET: Sure, that is on the agenda, the agenda discussion.

DR. SULLIVAN: What we had planned on doing is going through the LOFT special review group report, the recommendations that they made. Then out of that we made a presentation to the Commission and gave them a proposal for LOFT and we plan to discuss that meeting.

On February the 13th we also made another presentation to them which had to do with the budget numbers and we would like to discuss that with you.

The Commission has recently made a decision on the LOFT program and we would like to discuss what that decision means to us.

After that we would like to go into the details of the test matrix with you and then the item that is down, the subcommittee discussion, what we would like out of that is, what you -- look at our presentation of the test matrix and any input that you have to the test matrix so that we can consider those viewpoints also.

Then after lunch probably would be a good time for Dr. Karwat. I would suggest that, if that is acceptable, and then we would close with the water hammer issue.

MR. PLESSET: Fine, I think that is good. Your point about the test matrix reminds me that the committee -- the ACRS -- volunteered to participate with you in making the test sequence and maybe listing in priority so that if it went a little longer, that was all right, and if it went a little shorter, we got the most important tests, at least in our view, over with. So that is fine, why don't you go ahead.

I have an extraneous item, Harold, to call to your attention, an extraneous item in this sense: You may remember that in the review that the ACRS made for the Congress we made some uncomplimentary remarks about the W-Rap(ph.) program. Then I notice that there was a letter sent to Savannah River from Denton which kind of indicated that something might have been along the same line, that the need of NRR may not be met with that development and that they are just giddy-up on it. Is that right?

DR. SULLIVAN: I think the answer is yes. I think they have concluded that any further calculations on that code package, as I understand it, is not in their best interest at the time right now.

I think there are several factors that go into that

One is the recent decision to have a full effort in the licensing area and --

MR. PLESSET: In which?

DR. SULLIVAN: In licensing plants.

MR. PLESSET: To have what?

DR. SULLIVAN: To have a full effort in licensing plants. The first priority is certainly in licensing now and probably that will curtail some of the audit calculations that they had planned.

Another thing is that the package was primarily set up to do large break calculations. They are considering a wider variety of calculations now and we are actually pursuing a number of calculations for NRR in a very wide range of areas and in which the operational transient is considered the small break LOCA's.

They seem to be a lot more interested in best estimate calculations than audit calculations.

DR. CATTON: Could I ask a question?

MR. PLESSET: Yes.

DR. CATTON: What is the procedure by which this is done [illegible] they come to you with a specific request for calculationsn [illegible] d then you have one of your contractors carry it out?

DR. SULLIVAN: There are usually two procedures. One is they have their tech assistance program and if they are short range calculations, the tech assistance program usually takes care of those. If they are longer range more involved calculations, and we have had requests from them to perform a group of calculation -- not just one calculation but more groups of calculations where you are trying to resolve some issue.

DR. CATTON: More generic?

DR. SULLIVAN: More generic, yes.

DR. CATTON: Do you ever give the technical assistance efforts support when they run into minor difficulties with various computer codes that they are using?

DR. SULLIVAN: Yes, both manpower support and financial support. We have in the past.

MR. PLESSET: I guess we can get back to our true agenda item. We have touched on a lot of other things.

DR. SULLIVAN: Starting off, we thought that we would cover the LOFT special review group report and some of the conclusions that they have drawn. The report has actually been published twice, I think, once as a -- quote -- final draft and now it is out in a document. It is NUREG-0758.

The members of the review group are listed here. There were actually two parts to the panel that was formed. There were the members themselves and the affiliations were government agencies.

They consisted of NRR, RAS, and Inspection and

Enforcement. Those were the major offices. Also listed are somebody from DOE and also from NASA.

There were a number of consultants that also were part of the review and they ranged from universities to national laboratories to INPO and EPRI. Also you should note that Dr. Catton was on there and we considered that his participation meant something in terms of the ACRS.

The way that the business of the review group was conducted was that there were a number of presentations by the OAS staff, the NRR staff and also the contractor in Idaho. They considered three main options.

One was called Option A in the report and that was to terminate testing at roughly the end of FY '83 and the cost was around $150 million to complete the program.

There was a B Option which was to complete FY '82 -- testing in FY '82 -- and the cost was around $100 million.

Option C was to complete FY '81 and it was around $50 million.

RES had an original test matrix and the conclusion date from that was late FY'85, so those were the major contributions of the considerations that they looked at.

MR. PLESSET: Weren't there also various possibilities of stand-by maintenance and so on and so forth? Are you going to get into that?

DR. SULLIVAN: Yes. As a good review group should, they didn't hold to those being the only possibilities. In fact, they looked at a number of other options.

If you look at the regulatory needs, they can be divided into roughly four classes and those are to do safety analysis, and basically that was covered in Chapter 4 of the document that they ended up writing.

They also addressed the degraded core cooling issue in Chapter 6, the operational aspects -- and these are not of the program necessarily but of the types of transients -- and those are covered in Chapter 5 of the report, and they also addressed the risk perspective and that was covered in Chapter 7.

The slide you have here is also an outline of the presentation that I will be giving.

By the way, I am here at your request. NRR was unable to attend so a lot of the questions -- detailed questions in the report -- I might not be able to address but maybe Dr. Catton could.

Looking at the general overview, they generally supported more testing in the first class, the safety analysis area. The degraded core area, they looked at mild transients and said that those were a possibility. The severe ones were also a possibility to be run in the facility, but also concluded that the contamination of the facility was a major factor in both he scheduling and the funding that would be addressed by the program.

The operational aspects, they looked at it as an important area. They also concluded that LOFT might not be an optimal place to do that work.

In terms of risk perspective, they addressed the large break area and they said that if the present codes were able to calculate the transient accurately, that they were not risk dominated. They had a very low risk.

The anticipated transients, which we were also performing, they thought the data was useful and they thought that a type of flight recorder system could be installed on the plants that were now operating and provide a much better data set.

The severe transients, they said that they represented a very high risk. They were risk dominated. They questioned the use of the LOFT facility for them and again, in terms of the contamination to the facility also.

The degraded core question is, they didn't see any need to do experimentation in the LOFT facility. They were also a risk dominated portion and they were again concerned about the contamination.

They did look at the LOFT facility and the design of it and came up with the following conclusions:

That the LOFT facility was not expected to perform exactly like a PWR;

That there was scaling and other considerations that they thought would distort the result.

LOFT was scaled for a large break look at transient and they thought that the facility was scaled adequately for large breaks, particularly for code assessment purposes, and made note of the short core;

That the small break simulation precluded some of the key phenomena from occurring that was thought to be observed in a large PWR;

For non-LOCA transients, they said the simulation -- the studies of the simulation -- were not complete and therefore they weren't able to really make a strong comment about the plant's capabilities of looking at non-LOCA transients.

They also concluded that tests were needed.

MR. PLESSET: Which tests? The ones we are here to talk about?

DR. SULLIVAN: Yes.

MR. PLESSET: When they were looking at -- if you look at the bottom line there -- test results expected to be useful, that they considered the cost effectiveness of the testing?

DR. SULLIVAN: I think they also addressed that issue to a limited extent, anyway. They thought that data from operating plants would be much better. We currently don't have the capability of getting that data and they saw there would be a need for it.

MR. PLESSET: Did they consider the contact that this approach might make, that it involves instrumentation to make observations in operating power plants to get data that is meaningful; is that right?

DR. SULLIVAN: They also had looked at that. We have also looked at that question and the current instrumentation is probably not enough to assess a code. It certainly gives us more confidence than we have right now of the code's ability to calculate a transient.

MR. PLESSET: In connection with improving instrumentation in operating power plants, let me mention that there is a program which involves many millions of dollars -- a large part of which is instrumentation. That is the Internation 2D3D program.

As far as I can tell, this isn't contributing anything to the need that we all are aware of, and that is instrumentation that might be adapted to operating power plants. What is your comment on this? I think this is another little elephant that is getting whiter and bigger all the time.

DR. SULLIVAN: Commenting on the instrumentation, other side of the question, first, there is a program in research right now and it is looking at all the instrumentation that we have developed to look at two-phase flow transients, steady state capability and seeing the application of those two power plants to improve the instrumentation, particularly in the area of following transients.

A part of this -- and it was the start of that program -- is the in-vessel liquid level system. There was a lot of work done to look at the types of instrumentation that had been developed for a number -- not only the 2D-3D program -- but a large number of other programs that we have had.

We also should note that the type of instrumentation that we usually develop is truly experimental in nature. A lot of the facilities are limited operation, they are not long-term operation. The instrumentation in them is usually refurbished periodically and periodically means that probably in the order of like a month worth of operating reactor experience. So we did consider that.

We still think that a lot of the technology that we developed in these programs can be applicable to a reactor plant situation and we are pursuing that now.

MR. PLESSET: Go ahead. I didn't want to distract you completely.

DR. CATTON: If I could make a couple of comments on that on the last two items that were on the previous slide.

MR. PLESSET: Yes, go ahead.

DR. CATTON: As far as the small break LOCA's were concerned, there were one or two people who were involved with the group who felt that there just should be no more small break LOCA tests run on LOFT because the LOFT was so atypical in that it had really only one loop. If you were to run any further tests, it would not be to gain information about small breaks. It may be for other reasons.

The last item, which is the non-LOCA transients in accidents, the stat(ment, "not yet thoroughly assessed," is a little weaker than was atter.ded by the people who wrote the report.

It was felt that the atypicalities -- in non-LOCA transients in accidents it was felt that the secondary side was extremely important and that to run tests with the secondary side that really didn't properly represent a power plant might lead to false impressions or whatever and that it would not be useful and that no such tests should be run in LOFT unless they were --

MR. PLESSET: Would y' repeat the comment you made on the small break item? I don't think everybody got it.

DR. SULLIVAN: On the small break LOCA's, it was felt that the atypicalities were overwhelming and that no further tests should be run on LOFT -- no further small break tests should be run on LOFT -- if your intent was to learn about small break phenomena.

Now that wasn't unanimous, but the person who felt most strongly about that was one of the ones who would be more highly respected in that arena.

MR. PLESSET: I think that is worth noting. We have to keep in mind that just because there has been so much talk about LOFT doesn't mean that what you get out of it has much significance. I want to reiterate that. I think that what Dr. Catton has said just kind of gives me a little support in that direction.

Yes, Dr. Zudan?

DR. ZUDAN: Could I ask another question with respect to instrumentation? You said that someone was looking -- at least I understood you to say -- at what instrumentation could be added to power plants to make them a test bed for certain things where there would be separate effects or integral effects.

Has there any thought been given to the possibility of providing a nuclear power plant with a package similar to that used in their plant, flight recorder style?

DR. SULLIVAN: Yes. In fact, the LOFT special review group looked at that question also and decided that that would be one of the things that should be done.

Mr. Chairman, I think also that that was covered in an ACRS meeting with Harold Denton -- I think the Chairman was also there. I am not sure if this was part of the meeting or after the meeting, but he indicated that if we could keep from gold-plating it, that we could probably get something like that in.

We are also pursuing the same interest in research and looking at what it would take to develop a so-called flight recorder and install it on plants. It has the same kinds of problems that the nuclear data link has in its separation from the actual operating system.

DR. CATTON: Didn't EPRI do something like that or ANO?

DR. SULLIVAN: This is BWR's --

DR. CATTON: I don't know which plant that means.

DR. SULLIVAN: That is Peach Bottom, I think.

DR. CATTON: They just brought in a rack with a bunch of digital recording equipment and they got what they felt was data that was good for proofing the retran code and it was basically the matter of the recorder, not necessarily the in-plant instrumentation, and it only cost a little over $100,000.

DR. SULLIVAN: We are looking into that same type of question right now trying to see what you can record to see if it is enough information and what you would actually need to put into the plant if you wanted to improve that.

Like I said, we are having a hard time to keep from gold-plating it also.

MR. PLESSET: We are very much interested in that. We do keep interrupting you. Why don't you go on.

DR. SULLIVAN: The test matrix was also a part of the report and it is in Chapter 4. It was divided into three areas, a high, priority, low and medium.

Starting with the high, there are two large breaks in that s cies. There are two intermediate breaks and two anticipated transients and one is ATWS.

In the medium area there are two large breaks and two small breaks. One of the large breaks is with pressurized fuel.

In the low pressurized class there are three transients. One is an ATWS and one is a steam line break.

What we would like to do is not to discuss these here. We had a proposal that was put before the Commission and what we would like to do is discuss that, which generally covers all of the tests that are here and we excluded some and we will try to indicate why we excluded the ones that we did.

We also looked at what we ended up calling incidentiary (ph.) programs to the LOFT program and the one that you see on this slide is the even factors, or the human machine interface.

There is that program of augmented operator capability in the LOFT program, data find, full criteria that should be addressed in a program like that. Those are also listed on the slide.

They also concluded that criteria 2, 3 and 4 fall short of what they thought would be an acceptable approach. Those are the ability to reproduce the test using a variety of operators, the capability of generating typical transient response and that was, as they pointed out later, that they thought the typicality of LOFT was quite dependent on the type of transients that were being run.

The last area listed is the amenability of the LOFT control room to actually be able to move the instrumentation around tosee what the effect of changing the operator's position would in terms of the controls that he looks at and also the instrumentation that he is able to review.

They recommended that the work be done in an existing facility and also noted that they should be upgraded.

They looked at the degraded core cooling question and noted that NRC had an interest in understanding the phenomena associated with that. They thought there would be a risk reduction if you were able to determine when inadequate core cooling started and what the operator could do to assess it and to start it down a new path that would lead to a condition of either correcting the inadequate core cooling or turn the transient arcund.

They noted that running these tests could jeopardize the future test matrix and also in terms of damaging the fuel itself and also contaminating the facility.

The risk perspective of the LOCA's, anticipated transients and severe action sequence are as listed here in the operational transients.

In general, the tests that were recommended fall under the LOCA and anticipated transients area. The large LOCA, they said, was not risk dominated and that unless the codes predicted the transients very poorly or misled us, that they would not be:

That small breaks were risk dominant and they noted that the facility had performed tests in the past.

The anticipated transients, they noted that they occur in operational plants. They also addressed the area of having a flight recorder or a reactimeter available in plants to record these and it would be a good source of data.

I think it is worth looking at their findings a little bit. Looking at the LOCA area, they said that the transients were needed for code assessment and to gain more confidence in the models and to quantify safety margins.

The anticipated transients were not risk dominated, again; that the data was needed to look at the assessment of industry codes, and that data from power plants would be better but, in lieu of that, they thought that LOFT should run some of these.

The severe accident sequence, again, is risk significant and they felt that the contamination of the facility would preclude testing of the area.

They also made some more general comments: That NRR should take a more active part in defining the activities and the role of LOFT;

That the code assessment should be expedited;

That LOFT is a useful test bed for the instrumentation but one should not justify continuing the program based on that;

That the severe fuel damage experiments and studies are peripheral to the program and they felt that they should be done somewhere else;

That the human factors -- the human/machine interface work that is being performed in LOFT -- should be moved also.

Getting down to the last part of that, which is really the most exciting part of the report, the recommendations, I think if you will just read through that statement what you will find is that they recommended a list of transients be performed, they gave us an order of priority for those transients which we have looked at; that the testing should be conducted consistent with ending the experimental program in FY'83, and that more studies should be done on stretching the program out.

It was our interpretation of that to mean that what they would like to do is stretch the program out such that in case there were any unresolved issues, that they could be addressed at the time that they occurred.

I think that that concludes the comments that I have on the LOFT special review group.

MR. PLESSET: Let me ask you a question. How could it happen that all these inappropriate programs -- like human factors, control and design, etc. -- how could it happen that these would go on and absorb a lot of money? Who was responsible for this kind of uncontrolled -- if I may use the word -- programming?

I thought that this was a facility that was under the control of the NRC but it looks to me that it had a will and life of its own and this is why, most likely, it was spending twice as much money as it should have been spending.

Could you explain to me how this kind of management got going?

DR. SULLIVAN: I think I can at least try to address that. First I would like to say that the LOFT program in Idaho is not out of control. All of the projects that were started in the LOFT program were initiated at NRC, in particular in the research area.

Addressing a number of issues, such as the programs, say the human/machine interface effort. It was initiated shortly after the TMI event. It was recognized that NRC should do research in that area. We had not gotten started in it in any other program and it was initiated in the LOFT program.

I think it is appropriate now to look over all the programs as I think the ACRS also pointed out, and that we should address those issues.

The human/machine interface work is -- I think we made a decision to move that out of the LOFT program. A lot of that work will still be continued. Some of it will still be continued in Idaho but it will not be directly connected to the LOFT program itself.

I think that is what the review group came up with, that the work is applicable, but it is not appropriate that it be tied to the LOFT program.

A number of other things like this were started in the LOFT program and I would say that it was probably research's efforts, or lack of effort, to try to get guidance out of the regulatory staff -- the NRR -- that allowed us to keep adding to the LOFT program in areas that turned out, after a careful review, that other people felt they were not appropriate, and in particular the NRR.

MR. PLESSET: Yes?

DR. ZUDAN: To follow up on this same line, you said that a lot of programs -- peripheral programs -- initiated at LOFT were by the request by RES. Would you identify the programs that were conceived by RES and initiated as compared to those that were conceived by LOFT, suggested to RES and then initiated?

DR. SULLIVAN: Let me see if I can rephrase the question. You are asking which programs were conceived by research and which programs were conceived by the LOFT program?

DR. ZUDAN: Yes.

DR. SULLIVAN: I am not sure that I can go through a complete list.

DR. ZUDAN: I would like to have an answer on three; man/machine interface, augmented operator capabitiliy and tech support. Who initiated those?

DR. SULLIVAN: I would dare say we initiated them all, but I am not sure of that.

MR. PLESSET: Would you identify yourself, please, sir?

DR. LANDRY: Ralph Landry, Research.

The AOC work was all initiated within research. The tech support work, that was initiated from within research too, to be responsive to a program that I&E was overlooking post-TMI. It was within research that both of those began.

DR. ZUDAN: And the man/maching interface?

DR. LANDRY: I am sorry; I switched names on you. I called it AOC, augmented operator capability. That is what we have been calling man/machine within the LOFT program.

MR. WARD: I would like to ask for some clarification from the report. I think it said that $3.8 million was planned for '82 and '83 in this area of AOC. Is that for each year?

DR. SULLIVAN: Was that in the five-year plan that you are looking?

MR. WARD: In the LSGR report.

DR. SULLIVAN: I would have to look at the report. I am just not familiar with that number.

MR. WARD: Is that about the level of the -- of the $40 million of the LOFT budget, is it about 10 percent that is going into the plan for the man/machine interface program? Just what cost are we talking about?

DR. SULLIVAN: I think the figure that you have is out of the five-year plan and we have an active effort in human/machine interface where it consists of the work that is done in LOFT and a number of other contractors are also working on the same problem with us.

One happens to be Idaho also, but it is not connected with the LOFT program, so a number of other contractors that are looking at those types of questions, they range over a variety of areas. I think the number that you have is the total that we are spending in research, but I would need to check that.

MR. PLESSET: Any other comments before we go on to

RES' proposal on all of this? Dr. Catton?

DR. CATTON: I just might record a comment. We, in one of our early drafts of this report, had some very strong comments with respect to the management aspects of LOFT.

Somewhere along the line they got weakened significantly and the feeling was that LOFT was to have been a facility, not a program, and that somehow it became a program and, once a program, it became very easy for someone to have something he wants done, done on LOFT with really no financial responsibility for that task, but the money would somehow be added to LOFT, so LOFT sort of grew.

Then when it comes time to cut back, how do you do it? LOFT was extremely inflexible in this regard.

MR. WARD: Harold, looking at the LSRG, it has addressed emphatically on the LOFT facility. During this deliberation has it encumbered for a comparative study or investigation, such as by comparing the capability whether LOFT is unique or non-unique, such as the semiscale can do some of the things that LOFT can do and in terms of the technical effectiveness and LOFT can do better on this type of problem. Then for the small break and then perhaps the technicality -- as Irvin has said -- might be a more effective thing for the semiscale to do. That is the technical effectiveness.

Then the cost effectiveness. What would be the difference of the cost in supporting the different plants -- man/machine interface and so on.

Then you could draw conclusions, propose some alternatives so that our options might have some bearing on this as a comparative -- it is a question --

DR. SULLIVAN: We have certainly addressed that within research and I am sure that the LOFT special review group did also. An area that I would like to just go through with you is the possibility of studying natural circulation, which we were doing some work in that area.

When we first realized that we should do research in that area, we looked at a wide range of experimental facilities and we have looked at a couple of things.

One is the time frame that we could get the information on. The second one is the cost effectiveness and the quality of the data. Those are the three main things that we were looking at.

We concluded that the experiments that were done at LOFT in the small break area would give us the information as much as we could get. Semiscale was also addressed or looked at and we know there is a problem there.

We also went to facilities that were not ours, such as the Lodi facility and PKL (ph.) and we had asked them to address this same issue. We had also initiated a program in the FLECHT SEASET area to look at this.

So what I am trying to say is, starting at the top, in cost there is LOFT, which is the most expensive, Semiscale, which is probably the second, the PKL program which is part of an international agreement and then the cheapest of the facilities is the FLECHT SEASET and one of the reasons, it is just shared by a number of people -- their costs.

So what we tried to do is put together a complete consistent picture for addressing that in those facilities and that is the kind of things that we tried to go through.

Admittedly, any one of the three factors could override their cost effectiveness. The time frame that you get it on and the quality of the data -- in other words, it may be more cost effective to run it in LOFT because the schedules are critical.

In general we try to do that and I am fairly sure that LOFT review group addressed that issue also.

MR. PLESSET: I think that this is a very important point because the review group itself said that one of the significant contributions that one might get was in code assessment. I think that if we didn't have LOFT, as far as code assessment goes, we wouldn't miss it.

We use it, now that we have got it, to try to justify doing some work in it. You mention natural circulation. Well, they got a lot of data very quickly and very cheaply at PKL and that was that.

For some of the other work, it seems to me, we have to do it all ourselves. Well, there are facilities elsewhere like PKL and it just seems to me that LOFT was designed for an entirely different mission than it was put to and it makes it very awkward and very difficult to do things that are cost effective.

I wonder if the review group kept this in mind enough when they were r viewing it. Well, that is a matter of judgement.

DR. CATTON: If I could make a couple of comments?

MR. PLESSET: Yes.

DR. CATTON: Some of the things that Ted referred to, like uniqueness, I think one of the conclusions the group came to -- none of this is unanimous by the way -- that the so-called "nuclear-ness" of the facility was not really that important because most of the kinds of tests that we run really didn't even involve that aspect in a significant way.

One thing that did come out in this uniqueness category was that there was clearly a need for a large complex messy system and the reason was that most of the things that happe d as a result of running LOFT was, gee, we forgot to put that in the code, not that gee, we didn't understand that.

As far as we could tell, I didn't see any new knowledge coming out of LOFT. Rather it was that the steam generator was more or less important and this was more or less important, the stratified flow bypass; all of these things came into play.

We had some arguments among ourselves about small break. I believe there the conclusion was that a small scale multiple-loop system would probably give you a lot more than LOFT ever could.

I am not sure that that helped.

MR. PLESSET: I think that, for example, if the Japanese build their Rosa 4 (ph.), they could build the whole thing for less than it costs LOFT to run for one year. If we had this and somehow had access to it, it would tell us a lot more and a lot more quickly and in a better way than we could get it at LOFT and at considerably less expense.

Now I don't know if that will be financed or not, but it will be designed with a better understanding of the things of concern to us. It will contribute a lot more to code assessment than LOFT could.

As far as funds go, it is just a different ballpark. Now it is not nuclear, but as Dr. Catton said, that may not be very significant anyway.

DR. SULLIVAN: LOFT has a lot of unique capabilities and those capabilities turn out to be an asset and also a deficit to the program, such as the nuclear-ness, if you address that issue.

It is certainly, you know, going to help where we are looking at transients that you need to have a nuclear core and there are some of those in there.

Now you can argue about if it is appropriate to run those in LOFT or not.

Also the disadvantage is that it costs you a lot. The scale size is the same way. We need the data at large scales. A lot of people have indicated that.

It is also a disadvantage in the fact that it costs a lot and we are not able to instrument the plant to the extent that you would a very small facility. It just takes a lot more instrumentation, period, and the complexity grows as their size grows.

I look at it as we should do a very careful balance of the costs versus the assets of the facility and the large scale and the nuclear core are things that you should weigh very carefully.

You just can't get the size, I don't think, that you have at LOFT out of an electrical heating facility. There just isn't enough power. The Rosa 4 facility is not going to be able to go to a full power stage state, and there just isn't enough power.

So what we are left with is catching the transient n the run, if you want to look at it like that, where the conditions at the start of the transient are going to be slightly atypical.

Now you have to weigh that against the cost and it certainly is a lot of judgement. We have tried + cost benefit studies. The benefit is perceived and out of 10 people you get 10 answers.

DR. ZUDANS: Has anyone ever looked into the possibility of converting LOFT into a power generating facility?

DR. SULLIVAN: Using a power plant?

DR. ZUDANS: Yes.

DR. SULLIVAN: In fact, tnat suggestion has been made either twice or three times, as long as I have been involved with the NRC in one capacity or another. The first time it was early in our career and it turned out that people just couldn't believe that we were asking that.

The instrumentation complexity, the nuclear-ness of the facility and the capital financing that you have in the facility just grows when you try to run experiments in a nuclear plant.

The estimates get larger and larger every time you try to estimate it. It would be a very difficult --

MR. PLESSET: Maybe we should go on, Harold.

DR. CATTON: To be fair, I would like to make one comment. One of the things that the group sort of concluded, the LOFT facility, if possible, should be kept in existence and that a better approach would be a wind tunnel type approach where LOFT was indeed a facility and not a program.

Then the people who want to use LOFT can come up with the money to use it so it is no longer really a budget entry anywhere and if there is no support from the industry to use it, then it will just die a natural death, as it should.

It was also felt that it would only be under those circumstances that you could, indeed, solicit support other than from within NRC. No one from industry would touch it within the present financial structure.

EPRI confirmed that with the $2 to $3 million offer because that was predicated on some sort of an adjustment in the management structure.

DR. SULLIVAN: The thing that we have tried to do is, we have tried to look at the program and decide what tests we absolutely needed out of that facility and looking at its unique capabilities.

We are also very actively pursuing the suggestion that the special review group made that we go out and look for alternate funding sources. So we plan on using it to a termination date on which we would stop funding it.

If there were other financial sources available, I am fairly sure that it would continue operation, as long as people would say that they needed to run tests.

We had thought a lot about -- quote -- selling time on the facility and we are actively pursuing those suggestions.

Maybe we can go on through the next part of the presentation. We had developed a matrix based on a lot of the comments that were made in the LOFT review group report.

We had a very short time turning our proposal around compared to when the report was actually available to us. It was a matter of days, so we considered as many of the alternatives that they presented us as we could.

Certainly a major contribution was the numbers of ACRS meetings that we have had. We also considered that. We tried to come up with a compromise of the approaches that -- there was an NRR need for the program that was not the same as the LOFT review group report.

We also had needs in the area and the ACRS had given us a number of comments so we tried to look at that.

The primary emphasis was on the LOCA's and transients for improving and assessing computer codes. We are trying to take advantage of the facility in terms of its large scale and nuclear core and we tried to address issues instead of trying to set an end date.

We estimated the end date for the completion of testing as January of '83. We looked at the possibility of adding the severe core damage experiments and we would like to show you some budget options that we came up with and finally the commission decision.

DR. ZUDANS: Just one question. Is this RES' response or is it a combined response of RES and NRR?

DR. SULLIVAN: It was RES' response to all of the comments that we had gotten, not only the LOFT review group but from NRR also and the comments from the ACRS, so you will see a lot of compromises in the matrix, I think.

We tried to keep the matrix as consistent with the special review group as possible. We tried to address the high priority test that they recommended. We also included some contingency tests and to give us some flexibility in the matrix.

Maybe I should define contingency tests. They are preplanned tests. They are included in the test matrix. If we decide to run those tests, it is based on a defined criteria before we start the test matrix.

Usually the criteria is the need for the data and also the type and quality of the data that we have gotten out of some previous tests, so they add some flexibility in terms of rerunning or running another test if one of our tests turns out to give us a surprise or the data, we think, could be a better quality.

They would be performed only if required and I would like to underline the only. It is not like we plan to run all these experiments.

Looking at the test description, there is a test description, the priority and the nomenclature. The nomenclature is still the one that is used by the special review group.

If you look at the tests, they address all of the high priority tests that were suggested by the LOFT special review group except for one, and there is a line in the middle of the page that says, no testing below here, and that means that we had not planned on running those.

If we run the four contingency tests -- we estimated that we would only run two of the four contingency tests. The testing program would be completed in mid-FY '82.

The one test that is high priority to the LOFT review group is the upper plenum injection experiment and it is not upper head. We have looked at that experiment and there are some problems with the design of the upper -- the control rods and supports and there is a stress problem and we just feel that we could not run that experiment.

DR. CATTON: Harold, do you have the second intermediate break, the LA-2, as a contingency? I thought that was one of the ones that had a high priority. It was felt by the review group that you needed to have a couple of intermediate break tests.

MR. PLESSET: Are you talking about LA-2?

DR. CATTON: Yes.

DR. SULLIVAN: Yes, and it is noted as a high priority, but it is still a contingency test.

DR. CATTON: So you are really not in agreement with the LOFT special review group's structure?

DR. SULLIVAN: No, if we look at the data -- see there is another intermediate break up above that. If it gave us an indication that we did not need that experimental data, we would not run it. If it indicated we did, we would.

I would like to leave the discussion of the test matrix to Ralph and he will go through the test matrix in a lot of detail.

DR. CATTON: Fine.

MR. PLESSET: Just as a general question, if you take two out of that number -- you have about ten tests, maybe eight, if you drop two of the contingency tests; right?

DR. SULLIVAN: I think if you look at the next slide, that will help you because it has got the dates on it.

MR. PLESSET: Okay, that is what I was getting at.

DR. SULLIVAN: We had estimated on the first slide that we would be completed in mid-FY '83 and that was based on dropping two of the contingency tests. If we run all four you see that it is 6-83.

Now if you got it down where you didn't run any of the contingency tests, the program, I think, is early in '83, very early in FY '83.

We also noted on here, and the thing I would like to point out about the test matrix is, there are two tests in which some clad damage is expected. The first one is the first double asterisk that you see under the priority list and we have calculated that we would not expect significant clad rupture to occur.

It would be like some clad swelling, so that test has the possibility of providing some contamination to the facility.

The last test, we have calculated a clad rupture would occur so it would be an experiment where you would release some radioactivity.

You notice on the right-hand side at the bottom of the page it says that what we have done is limited the burn-up to two weeks and that would be such that we would not contaminate the facility. There would be very little fission products in the fuel itself.

These are new modules. There is a core and we would be replacing some of the modules so we have tried to limit the amount of contamination that you would actually end up with in the facility. It becomes important to us later on.

DR. CATTON: When, in that scheme of things, are you going to put in the central bundle without the external thermocouples?

DR. SULLIVAN: Ralph will cover that. I think his test matrix shows when a number of procedures would be going on.

We looked at -- having a fixed program would have a number of benefits and those are the time of completion of the experimental program would be fixed, there would be an orderly shutdown, there would be greater efficiency and it would lead you to a lower cost.

There are other benefits that are noted on here. One is that it leaves the opportunity for continued testing, it has the contingency tests in there and we could have a very orderly close-out of the program.

The problem areas -- and you should just make note of these -- is that there are -- if we had any significant fuel damage during any of the experiments, there wouldn't be enough fuel to complete all the tests.

We indicated that there was a possibility of fuel damage. We actually have that fuel.

There are a number of others. One of the things is, we felt for the funding plans we felt that the instrumentation development, the human/machine interface and the electrical versus nuclear heater comparison and some calculations of whether power plants ought to be moved out of the LOFT program to get its emphasis focused on getting it completed with the experimental program.

We also covered, with the commission, the possibility of running severe core fuel damage experiment, and that is the L8-4 experiment.

The test is described there. The goals were to look at the progress of the fuel damage through the core, the fission product transport and the fuel fragment transport through the system and to help with the degraded core rulemaking.

We noted that there were a number of reasons why you would like to do it and some which would preclude you doing them. It was in support of degraded core activities, that the experimental program would lead you to a long-term recovery of the core and a look at fuel damage.

We would expect fuel melt to occur. We would expect the facility to become contaminated. We also expected a large increase in cost in decontaminating and decommissioning the plant. Therefore, research did not recommend doing that.

I would like now to turn to some options and this is actually the February of '81 presentation to the commission. There were five options and I would just like to go through those options with you.

The first option is the test matrix and there are two types of test matrices considered. The one that we proposed doing with an uncovered core in the LOFT special review group.

The next one is the estimated end of testing, the stand-by activity in which we looked at three options. One was no stand-by, a minimum support or maintenance for two years and a minimum technical staff for two years.

The assumptions on decontamination were normal, meaning that we would not have any significant fuel damage, and then a significant one if we included the core uncovered.

The next slide, which you will probably be very interested in is, we entered one more column to that, which is the cost.

You note that the minimum cost if option 3. That is, do the test matrix we proposed with no stand-by and having the normal recommissioning.

The next option is -- in terms of cost -- option 1, then the option 2. The most expensive one turned out to be the LOFT special review group and the reason for that, if you look under the column for when testing will be completed. There is the significant cost of keeping the facility operating through that period of time.

DR. CATTON: I thought the LOFT review group recommended the end of fiscal year '83.

DR. SULLIVAN: Yes, and it is probably not correct to say what I did. What I should have said is that what we did is took all of the tests that were recommended by the LOFT special review group, estimated when we could be completed with those, and then estimated the cost.

DR. CHEN: Did you consider high priority, medium and low priority?

DR. SULLIVAN: All of the tests were considered.

DR. CATTON: I might mention that of the three sets of tests -- high, medium and low -- of the low, there was only one person in the entire group who was interested in having them run so it is really not a fair way to do that.

DR. SULLIVAN: Yes, it is really not fair to say that, I agree. I think the only real benefit that I got out of looking at the cost numbers was that it is a lot more expensive to continue testing, even though you didn't contaminate the facility, which was the difference between options 4 and 5.

I left this slide in for the assumptions that we used in the budget because I think they are important. One of the significant things that we should realize is that research did all the estimating. Idaho did not do the estimating.

So what we have now is a program that we have estimated both the dates for completion and the costs. What we are proposing to do now is take that proposal -- the test matrix -- to Idaho and see what the costs are. We have not estimated the new cost of the program as yet.

We are still iterating with the NRR on the test matrix and that is why we are here today, to get your comments on our test matrix.

I have included the costs for options 1 and 2 in the presentation. The cost for options 3, 4 and 5 are also in the hand-out. Let me just briefly go through and let you see what he did.

What we tried to do is break the LOFT program costs down into elements that then we could make a consistent cost comparison between all the options and it turred out to be very difficult to do.

One of the options was to only have a minimal stand-by, which is option 1, so if you look down the left-hand side of the slide, you will see stand-by activity and that is roughly $4.5 million a year. Again, these were our estimates.

In every case the ancillary programs are in the costs but we plan on moving them out. The reason that we left them in all of them is because some of the options, it made a difference in the out years where you had those programs going or not.

MR. PLESSET: Would you tell me what the commission requested in their Congressional -- $44 million, wasn't it?

DR. SULLIVAN: Yes.

MR. PLESSET: As far as you know, that is still what they think they would like to have?

DR. SULLIVAN: Yes.

MR. PLESSET: For FY '82?

DR. SULLIVAN: That is the current proposal. It is $44.5 million; right?

DR. CATTON: There were a couple of big items that I recall. One was TMI upgrading and seismic upgrading. Are they in the $28 million or have they been taken out?

DR. SULLIVAN: Maybe I should have gone through the operating budget assumptions. If you look back through there we were going to make the upgrades through FY '83 and then not do them any more.

As I understand it -- and you can correct me if I am wrong -- the program, we are upgrading at the cost of about $1 million a year, so it represents -- in FY '81 we had $1 million and in '82 and '83 we would also have $1 million each.

DR. CATTON: I just thought it was a larger number than that for those two items, more like a total of $4 million.

DR. LANDRY: The savings on cutting out those upgrades would be something on the order of $6 million.

DR. CATTON: That is what I thought.

DR. LANDRY: I think it is $6 million to $6.25 million that we are going to end up saving by cutting that out. That is one of the assumptions that was on a previous slide, or should have been on the previous slide, as being cut out.

DR. SULLIVAN: But we are still making those at the roughly $1 million a year right now.

DR. CATTON: There is also the rather large instrumentation programs.

DR. SULLIVAN: It is part of the ancillary projects that is noted on these slides. There are other things in there as the human/machine interface.

The second option is roughly the same as the first except that we would incur the cost of having the stand-by for two years and with a minimum technical staff and that was $9 million a year.

DR. CHEN: I have only one comment about the commissioners. They like option 2; is that right?

DR. SULLIVAN: If you flip through your hand-out you will get to the next-to-the-next slide, I think. That is what the commission recommended that we do. They supported the option 2, which is a minimum technical staff.

The supported the estimated end of testing in mid-FY '83. They supported the fact that we had a minimum technical staff available and that we would provide the funding for decommissioning -- decontamination and decommissioning -- after the end of the two-year period.

They also gave further guidance in the fact that the detailed test matrix would be worked out primarily between the research staff and NRR. We are also to solicit some new input.

We would seek new areas of funding. We would also seek a more efficient operation and facility so the way that we are approaching this is that we have the commission decision now. We know what to plan on. We are working on the test matrix.

Once we settle the test matrix we would then go to Idaho and start working on both the scheduling and the cost aspects of the program, we would examire the possibility for LOFT personnel to support the licensing staff in the area of licensing review.

MR. PLESSET: Are there any problems with that last effort, to have personnel employed by EG&G at Idaho contributing to the licensing decisions? Are there legal problems?

DR. SULLIVAN: I can only state what Dr. Henry suggested at a commission meeting; that that had been discussed with him in great detail by a number of lawyers and that there are possibilities of doing that.

They cannot make licensing decisions. That is the job of the regulatory staff.

MR. PLESSET: Yes, that is against the law, isn't it?

DR. SULLIVAN: That is against the law. I understand that they can support them technically and that is what we would be proposing. The law staff is a resource that we see. We have expended a great amount of funding to develop the staff that they have and we would like to see that capability preserved and we are exploring a number of options -- this being one -- but we are exploring a number of other options to make sure that the staff of technical people are preserved in Idaho.

MR. PLESSET: Dr. Zudans?

DR. ZUDANS: There is a precedent that is going on all the time in that way. The technical review can be done by an outside organization and safety-related views of that technical review is done by NRR staff. My organization has a contact like that, too.

MR. PLESSET: Yes, but those are usually fairly well defined tests.

DR. ZUDANS: Oh, yes, very specific licensing.

MR. PLESSET: I guess that is what they would have to do, is make them fairly explicit and specific.

DR. ZUDANS: My question is, what time frame would this come into consideration; af er all the tests are done?

DR. SULLIVAN: We haven't gone into the details of how to execute this. The licensing staff needs help now. We also need the personnel to perform the tests. We hope that there will be some savings by defining the test program in detail and then conducting it.

It will reduce the number of analyses that we have to perform, like you won't be exploring options for new tests, say. So we hope that there is some savings to doing this.

There are also a number of other areas that we need to explore and it is not a question of how many people are working on the LOFT program. It is how to effectively staff the LOFT program to accomplish the objectives that we have set forth by the commission.

MR. PLESSET: I think we are a little bit behind. I think we should take a 15-minute break at this time and come back and go to Mr. Landry.

(A recess was taken.)

MR. PLESSET: Let's reconvene and move forward. Our next item is a presentation by Mr. Landry of the LOFT test program.

DR. LANDRY: I would like to center this talk on three points: some of the factors that went into the test matrix -- and a lot of these Harold has talked about and I won't reiterate everything he said; I would just like to go over a couple of things quickly; the matrix itself as it developed and incorporated the special review group ideas, some comments we had from NRR and some discussions we had with INEL; then I would like to spend some time talking about each test individually, the concerns that we think each test will address and a little description of each test that would occur.

Some of the considerations in establishing the LOFT program are programmatic, scheduling and cost. We have tried to address what we see as experimental needs, or needs for experimental data, and those are needs that Harold had identifed as our understanding of the special review group, our discussions with NRR, some comments we have had from fellow members of research and some things that the ACRS has discussed in the past and has expressed a concern about.

We have tried to take into consideration planning time, make the test schedule as efficient as we could, and make this efficient as far as hardware is concerned, as far as modification, as far as analysis is concerned.

We have tried to minimize the time between the tests, we have tried to get through the tests as quickly as we could and get to the end result as quickly as we can.

Of course we tried to minimize the cost. Some of the discussion earlier centered on what we call the ancillary projects, some of the peripheral projects which have been moved out and which will help cut down the cost of the program, plus we have tried to minimize the hardware changes, which would cut down the cost of the program, and we have added another point.

We have tried to take into consideration manpower. Now this includes both the manpower fluctuations. We have tried to minimize those fluctuations and make the change in manpower as smooth as we can for Idaho, and we have tried to put in a little bit of a human consideration.

We employ, through LOFT, a pretty large percentage of the EG&G staff in Idaho. I guess between the direct and indirect employees, we have about 1,000 out of 4,000 employees with LOFT.

We have tried to make our test schedule so as not to upset those people's lives as much as possible and make the transition for them as smooth as we can.

Then some of the considerations in establishing the test spacing --

DR. CATTON: Are the indirect costs included in the $45 million?

DR. LANDRY: What I call the indirect personnel are I guess maintenance personnel who are working in other areas in addition to working on LOFT. They are technical people -- our technicians -- who do some work for LOFT and do some work for, like, PBF or Semiscale or some of the other programs at INEL.

We have people that are attached directly to LOFT who work full-time for LOFT and we have people attached to INEL who work part-time for LOFT and part-time for other projects. So when you talk about personnel, I have to say there are something on the order of 550 people directly attached to LOFT and approximately 450 indirectly attached to LOFT.

DR. CATTON: But they are part-time so really if you wanted to reduce it to something of a count, it would be something less than the 450?

DR.. LANDRY: Right.

DR. CATTON: Or was it the bigger number you have already done that with?

DR. LANDRY: We took the gross number, the 1,000. I don't know how you scale a person.

DR. CATTON: If he works half time for you and half time for me, I count him as one-half.

DR. LANDRY: But you still effect the families. I am saying we would effect maybe 1,000 families.

DR. CATTON: It is 450 full-time equivalents?

DR. LANDRY: 550 full time and 450 indirect.

DR. CATTON: Okay, that answered my question. The other thing is, is the cost of those people included in the $45 million or is it a hidden cost as well?

DR. LANDRY: It is included in the total budget figure.

Now a number of the considerations in establishing test spacing are self evident. Hardware changes, it takes a certain amount of time to change hardware around. That effects the test spacing. Measurement changes, it takes certain types of equipment and certain types of instruments to measure phenomena for a small break that would not be necessary for measurement in a large break. Or for natural circulation you need a certain type of flow measurement that would not be applicable in a large break. We have tried to take those factors into consideration in the test spacing also.

Safety analysis, that is pretty well self-evident. Most of the tests require more time to analyze and to set the specifications for the test.

The next slide here I will talk about for a while, we have tried to make the test matrix that we have proposed consistent with the description of the tests that the special review group presented and with the priorities established by the special review group and also taking into consideration factors that NRR has expressed to us in discussions with NRR over the test matrix and we have also tried to include contingency tests which would allow for fill-in data if those data were needed. It provides a little cushion that we could remove if we were moving well and we felt we did not need those tests.

Now this test matrix here is the matrix that we went into the commission with on February 11th. Two of the tests that were then identified as contingencies -- the operational transient and the small break after code assessment -- through discussions with licensing -- NRR -- we are now defining as uncontrolled boran dilution from cold shut down and a small break run with pumps on.

Now when we get back into discussion of the individual tests I will talk more about the details of those two tests, but these two, licensing has expressed a need for verbally so we have nailed two of the contingencies pretty well. That allows us two contingency tests now.

DR. CATTON: Will licensing supply you with anything in writing on those needs?

DR. LANDRY: Yes. We are right now in the process of working out an endorsement letter between licensing and research and before that gets finalized we would like to get comments from the subcommittee on the use of the test program and the tests so we can factor those in also.

Hopefully in the near future we can get that endorsement letter finalized, get it over to the NRR for their signature and returned to us so we fix the test matrix.

DR. CATTON: I personally would like to see the description put together and the need by NRR before it goes too far.

DR. LANDRY: I am sorry?

DR. CATTON: I would like to see the reasons that they want particular tests if possible.

DR. LANDRY: On a couple of these I would like to talk about when we get to the individuals.

DR. CATTON: Okay.

DR. LANDRY: Then this was the test matrix that Harold showed earlier. Two of these contingencies are now fixed, at least as far as our discussions with NRR are concerned.

Then some of the benefits of the proposed matrix. We have a predictable shutdown date. We can go through an orderly program. If this endorsement letter finally does get signed off under the memo from the Secretariat on the commission decision from February 13th and the EDO memo outlining the position of the commission, that endorsement letter will then require an agreement between the two office directors -- NRR and research -- before a change can be made in the test program.

In other words, the test program will be fixed and we could not over the phone change the test program.

Of course in here, there is nothing about a meeting in February of '83. A little white out took care of that.

This is a recommended test schedule which we have begun to work out with INEL. We have put in target dates and commitment dates for each of the tests with a brief description of each test. Afterwards I will go into each one in more detail.

What you asked about before was the center fuel module change out. That will occur after the intermediate break L5-1. We will pull the center module which is now in place and replace it with a center module which has 148 fuel pins prepressurized to 350 PSI. This will be all the fuel pins except the peripheral pins.

DR. CATTON: And that had the internal TC's?

DR. LANDRY: They will have the internal TC's.

DR. CATTON: I can never remember what those numbers mean. Will there be a large break one after that?

DR. LANDRY: The next test, L2-5, is the cold leg break. That will be run with loss of off-site power as one of the factors.

DR. CATTON: Good.

DR. LANDRY: You notice these are --

MR. PLESSET: Well, there will still be a lot of fuel rods with external thermocouples, won't there, in that core?

DR. LANDRY: I am not sure of the exact number of internal and external thermocouples that will be in that fuel assembly. I believe you will have the same number of external thermocouples that we now have, 185. I am not sure of the number of imbedded internal thermocouples that we will have. It will be a significant number, though.

DR. CATTON: Now your imbedded thermocouples aren't on the same pin as the external thermocouples, are they?

DR. LANDRY: No.

DR. CATTON: Good.

DR. LANDRY: That is a pretty large perturbation.

MR. PLESSET: It is still not a very thorough exhibition of the effect of the external thermocouples versus the internal. They are only replacing a few.

DR. CATTON: The thing is, what is your contingency if L2-4 shows that there was a significant impact of the external thermocouples.

DR. LANDRY: As far as doing another test?

DR. CATTON: Right. Do you immediately erase all previous data or what do you do?

DR. LANDRY: We don't believe that that will occur, first. A report has been completed now by Bert Tollman which examines a great deal of data from around the world, data comparing internal versus external thermocouples, data comparing nuclear-powered rods with electrically-powered rods.

I am not prepared at this time to go into a lengthy discussions on --

DR. CATTON: I am not prepared to go into a discussion about it either. I have seen parts of the report. I am not sure if I agree with it. I am just asking you, you have to have an awful lot of faith to have no contingency.

MR. PLESSET: Well, I think what it means -- and they may not be able to show this -- they have adjusted codes -- code calculations -- as if the external thermocouples didn't have a significant effect.

DR. CATTON: That is right.

MR. PLESSET: And if they do -- which I am sure they do, to express a positive opinion, that means that this code adjustment is off. I mean, I don't care about LOFT as a facility. That is meaningless, really. What we are really concerned about is developing and assessing codes. It is the only excuse for the thing.

DR. CATTON: And TRAC right now has built into it this iiloeje correlation in order to accommodate the early LOFT tests.

MR. PLESSET: Hes, Hal?

DR. SULLIVAN: What we would like to do is run this test with that module replaced. It has the internal and the external thermocouples in other portions of the core, so we will have direct comparison of internal TC's versus external TC's.

If there is a significant difference, it doesn't mean that we have to go back to ground zero and start all over running the experiments. What it does mean is that we have to undertake a significant development effort to try to remove any bias that may be in the external thermocouple data and it is going to be some work.

We have already started doing that work and one of the tasks that Ralph mentioned was this work that Bert is doing in trying to look at in what portion of the transient is the external thermocouple effecting the [illegible] how much is it effecting the result, and are we going to be able to back calculate, or subtract, the external thermocouple add-on.

MR. PLESSET: I appreciate that. I am sure you are going to try, but what you are going to have is a lot of fuel rods with external thermocouples and you are going to have a few with internal thermocouples. You cannot be sure that you have exhibited the real difference for a core that doesn't have external thermocouples. Wouldn't you say that?

DR. CATTON: If they run exactly the same one and they have replaced a bunch of the pins that have them with those that don't have them, maybe you can make a comparison, but I am still concerned about that L5, the intermediate break.

If there is, indeed, an effect of the external thermocouples, shouldn't you restructure that before you run a test that is questionable in some peoples' minds to test the external thermocouples?

DR. LANDRY: For one thing, we won't have the fuel assembly ready at that point. We cannot put the fuel assembly in until we get a little later in time than that test unless we want to delay that test an awful lot.

DR. CATTON: Is the L2-5 the last test you are going to run?

DR. LANDRY: No, that is just the last one on this page. I have got a second page.

DR. CATTON: I look at the second page and I see a number of tests on it that would not be impacted by external or internal thermocouples. I wonder why you don't put one of them in instead of L5-1.

DR. SULLIVAN: That is a good point. We will go back and look at that. We probably should look at the order. There is a reason why it appears where it is, because we need to look at those intermediate breaks so we could make up our minds about the contingency tests.

I think you have got a point. I think we need to go back and look at that.

MR. PLESSET: To go back to my point, if I have a bundle of rods with internal thermocouples surrounded by bundles with external thermocouples and those run cooler -- or are cooled -- then you are disturbing the others so you are not getting the full effect exposed. Do you see what I am trying to tell you?

In other words, I am downgrading your results already of this in advance.

DR. CATTON: That means they will have to do a lot more homework.

DR. SULLIVAN: I think I understand your question and I know some of the results that are being generated and the results indicate that there is a difference in the temperature of the rods, but the heat release rate is not significantly effected. Therefore the coolant conditions are not significantly effected.

The number of rods with external TC's is a small portion of the total number of rods in the core so if I understand the arguments right, we would not see a big difference in the hydraulics and therefore the gross core would behave in the correct condition.

The question is, are we tuning -- if you wanted to use those words -- or adjusting the heat transfer surfaces in the codes to take care of the external TC's when we shouldn't ought to be doing that.

MR. PLESSET: Right, that is the heart of the matter.

DR. SULLI'AN: That is, hopefully, what we can get out of this changing that module out and putting internal TC's int the core.

MR. ACOSTA: That one module will have the same distribution of external and internal? That one module may not see the same hydraulics. You assert that it will, but it doesn't seem like a real planned test.

DR. SULLIVAN: Let me qualify that. The core in LOFT is not 12 feet in diameter like you think of a large core. It is a small core. The module that would be put in is right beside other modules so they are in relatively close proximity to each other that have external TC's.

DR. CATTON: Is it a repeat of a previous test in all aspects?

DR. LANDRY: This is not a repeat of any previous test exactly.

DR. CATTON: It is not a repeat of any test. I am not sure how you are ever going to iron out the differences.

DR. LANDRY: We won'g be able to iron out the differences by comparing data directly with another test. This is such a different test -- prepressurized fuel, internal and external thermocouples -- that we won't be able to compare it exactly, TC with TC, from another test.

However, we think we have seen enough repeatability -- in other words, we feel that the repeatability has been shown between L2-2 and L2-3 by the magnitudes of the change in temperature compared with the magnitudes of the change in initial conditions, such as linear heat generation rate, power generations.

DR. CATTON: You are not going to be able to get a real solid bottom line, is what it gets down to.

DR. LANDRY: We are trying to operate within the constraints of time and dollars. We could come up with a strong bottom line if we wanted to run forever with unlimited funds but we do not believe that a replicate test is cost effective.

DR. CATTON: On the other hand, I am not sure you will find full agreement with this large number of tests that you have got on your two pages either. I personally would just as soon see you replace the ATWS with a repeat test like L2-3 maybe in order to really get a good bottom line on this.

DR. ZUDANS: Another question. They are either external or internal thermocouples. Are there no rods which have both external and internal?

DR. LANDRY: If I recall correctly, there are no rods with both external and internal thermocouples but they will be mixed. Unfortunately I didn't think that this would come up or I would have tried to get a fuel assembly instrumentation plan to bring along.

I believe what we are going to do is have within -- what we have used as a cluster previously, like four rods in a cluster with external TC's. We will have maybe two with external and two with internal within that cluster so that we are not having a cluster or rods w th external thermocouples and over here a cluster of rods wi internal thermocouples. We will try to mix within a cluster dividual rods with external and internal but we will not h e both internal and external in the same rod.

DR. CATTON: How far apart will hey be?

DR. LANDRY: Whatever the normal tch is, three-quarters of an inch?

DR. CATTON: If you put two pins adjacent to each other and one has external and the other doesn't, the one that doesn't is effected by the one that does, so I am not sure how you are going to do this, but I have another question.

You are using different fuel -- pressurized fuel. You are going to have other things going on. Do you have a fuel model that is good enough for you to resolve the differences between your old fuel and the new fuel even if you ran the same experiment?

DR. SULLIVAN: Let me see if I can get the concern straight. You would like to run a duplicate test so that you would duplicate the conditions -- the thermohydraulic conditions -- that an external TC versus an internal TC would have?

DR. CATTON: Maybe if I rephrase it, it will be clearer. I am concerned about code assessment and I saw what happened with the first test. Now you are going to run another test. I don't want to see the same thing happen. Then we could flip a coin as to which model we use and which code because right now TRAC uses illoeje and that came about because of the early test.

So if you are going to run another test and you want to find out whether those early tests were good, bad or indifferent because of external thermocouples, it ought to be the same test and it ought to be the same fuel, unless your analyis can separate the effects of fuel.

DR. SULLIVAN: I personally would be more concerned if we were trying to reproduce a test because the quality of the reproduction would have to be extremely good before I would believe that we had reproduced anything. I don't think that we can reproduce things down to the extent that we are looking at and the estimates were in 80, 100 degrees. I don't think you can reproduce things that well and try to sort out differences.

What I would believe you could do is if you had the core thermohydraulic conditions seem to be fairly uniform -- particularly in small segments of the core.

I would think that if you had an option to do one or the other, I would run an experiment with external and internal TC's and try to get them to within close proximity to each other to see the difference.

DR. CATTON: But not too close.

DR. SULLIVAN: But not too close, you are right, and that is another thing that we would look at. Maybe one of the things -- I know that there are internal and external TC's close to each other like adjacent channels. I am not sure that they should be a couple of channels away also and I am not sure that it is true.

I wouldn't want an external TC and an internal TC in the same rod.

DR. CATTON: The would measure the same thing.

MR. PLESSET: You have just reinforced in my mind a very severe limitation of LOFT. You said it is very difficult to reproduce an experiment and suppose we were to run it, some of them without external thermocouples as opposed to those where they were there.

It is so difficult to get enough tests to that you really know what you are doing when you are trying to develop a code or assess a code. Isn't that what it boils down to? When they got these results with the external thermocouples at first they said that it didn't matter, that they were right.

Now they begin to admit that there is a possibility that there is a significant effect so that the basis for adjusting the code may be questionable.

Now we want to see, was this readjustment legitimate. It most likely may not have been -- very likely may not have been.

To find this out in LOFT seems to be just about impossible, from what you said. So what does that mean to me as regards the value of LOFT for really making codes assessed? It might be better for me to run a half a dozen tests at Semiscale than try to repeat a test in LOFT and get something out of it. What would you say to that attitude?

DR. SULLIVAN: To the first point -- repeatability -- in all the experimental facilities that we are associated with repeatability, to the extent that we would like to see it, is very very difficult if not impossible, because what we would like to say is that all of the differences are attributed to, say, heater rod design or fuel rods or something and the repeatability of the hydraulics to me are fine tuned things.

We are looking at 100-degree differences.

MR. PLESSET: 100 degrees C?

DR. SULLIVAN: They just put that on the slide and it is a 144 difference max, degrees F.

MR. PLESSET: Degrees F. I wonder about that, but okay, go ahead.

DR. SULLIVAN: I am sure it could be more than that or less. It is transient dependent. That number is not constant, which is also a problem. So we are trying to sort out -- if it was left to me to decide whether to run a test, with the internal and the external TC's to try to sort out the difference in the LOFT facility, I wouldn't do it, personally.

MR. PLESSET: Because it is not good enough to tell you.

DR. SULLIVAN: You may have a point also, but to me it is not worth $5 million to try to sort out the difference of a 100° temperature. It is just not worth that, at least to me.

The reason we are suggesting that we do this is that we would like to look at a new pressurized rod. So we are looking at internal and external TC's as a way to get a handle on what are pressurized rods going to do and that is the only reason for putting those in there.

They are designed and fabricated -- within a short length of time they will be fabricated -- so that the additional expense, if you had to put internal TC's in it, I would probably really question that also.

I think that the difference is probably -- it is not a trivial difference but it is not in the order of magnitude that we are seeing in other areas of research such as whether you uncover the core or not.

MR. PLESSET: I agree with you in large degree but I would have rather have heard this a year ago than today, or two years ago.

DR. SULLIVAN: We are certainly getting smarter.

DR. WU: How about for the same type of experiments if you used the Semiscale, how about the reproductivity? Would that be a sharp relief or would you still feel this same degree of uncertainty?

DR. SULLIVAN: If you asked me this question about two years ago I would have probably have said we could repeat the test very well. Keith Condie did a study that I was associated with and it showed that the effects on heat transfer of very small differences in quality and flow rates are significant effects and they are even beyond our capability of measuring right now.

It is just that we cannot measure under transient conditions that well to sort out the effects of flow and quality on heat transfer.

I would say, first of all, we wouldn't know whether we reproduced it well enough. The instruments just don't have that kind of accuracy.

DR. CHEN: Excuse me, Mr. Chairman?

MR. PLESSET: Yes.

DR. CHEN: I want to inform Harold -- I think he is aware -- that there is a research program going on in Lehigh

Professor John Chen told me that they are starting the external thermocouple effect and I am wondering if that study can quantify the difference on temperature.

He told me that substantial effect -- he told me personally --

MR. PLESSET: How was the study made?

DR. CATTON: It is done at low pressure so it really can't contribute a whole lot more than the one that took place in Denver a year or so ago -- two years ago, I guess.

DR. SULLIVAN: There is a lot of work on external thermocouple design and the LOFT program is doing a lot, PBF has generated some data. The thing we need to do is try to quantify the differences and then if it is significant and we need to correct the data, we need to develop a model and that needs to be inserted into the codes along with the heat transfer surfaces if we are going to look at the LOFT data as code assessment. I don't think we are quite there yet.

DR. CATTON: The use of a code to correct the data just makes me very nervous.

DR. SULLIVAN: I said put the model in the codes when you are comparing it to the data.

DR. CATTON: Developing a model for external thermocouples in order to get around this problem -- right from the beginning we have always had to face the fact we can't measure within those channels. We are always calculating things anyway. Now we are going to calculate things to calculate things with a model to correct the data and I am really concerned about that.

DR. SULLIVAN: I am, too, and if it was the only program that we had I would be even more concerned. There are electrical heated rods at Oak Ridge in Semiscale that are going to give us some valuable insight.

The LOFT program is unique in that it is the only what I call true integral test facility which has the nuclear fuel, which I think we need to look at our fuel codes. So I am looking at it as a very good facility to put the complete transient together.

MR. PLESSET: Let me go back to a question at hand. Before we go on to considering the other tests, presumably the tests on this first page are pretty well set so that there is not much input that you want from us regarding those beyond haggling about these internal and external thermocouples; is that right? These are pretty well in hand?

DR. LANDRY: That is correct. The ones through the remainder of FY '81 we really do not believe we can change at this point.

MR. PLESSET: So that there is not much point in our really looking at those in detail because it is really out of the discussion; is that right?

DR. LANDRY: We can go back and talk about this further.

If you have this concern about L5-1, we can go back and talk about it, but it will be difficult to change the tests at this point for those on this page.

MR. PLESSET: Yes?

DR. SULLIVAN: If there was a concern that you had that was an overriding concern -- we don't think it is cost effective to change.

MR. PLESSET: That is what I would have expected.

DR. CATTON: The feeling of the LOFT special review group was that one area that had been really missed was the intermediate break and you have to take a look at it to see whether there were going to be surprises and if LOFT did nothing else, that had to be done.

MR. PLESSET: That is in there.

DR. CATTON: I think in that spirit, it ought to be done after they had gotten rid of the external thermocouple question.

DR. SULLIVAN: I think you have a point. I think we ought to look at that.

MR. PLESSET: Would that be a really serious dislocation of the time sequence?

DR. SULLIVAN: I am not sure.

MR. PLESSET: That is what I would be worried about. Otherwise, I think you are absolutely right.

DR. SULLIVAN: It is something that we ought to address and let you know.

MR. PLESSET: All right.

DR. LANDRY: It will effect the time sequence in that the bundle -- F1, the insertion date is pretty well fixed. We can't get the bundle, get it prepared and be ready to insert the bundle really before mid-October. Now if we put off the L5-1 test simply because we want to run L2-5 first, we might be able to move another test into that slot, we might not. We would have to talk with Idaho about that.

If we do not, then we would have to have another three or four months added onto the schedule to allow for shifting.

MR. PLESSET: Okay.

DR. SULLIVAN: There is one other point that may be significant. It is that we had put that bundle in there and then you notice that the next test we run is that break and we expected it to get some strain.

We also did not want to burn the bundle up -- it had a lot of burn-up on it. If we moved it we would get more burn-up and we would also run a chance of straining the clad before we would like to.

All I am saying is, there needs to be a lot of careful consideration.

MR. PLESSET: Yes?

DR. ZUDANS: Listening to this conversation, I come with the impression that you will not preserve the thermocouple pressure in any of these tests.

DR. SULLIVAN: I believe we will.

DR. ZUDANS: How? You have a mixture of external and internal in one rod and you have only external n the other; hardly any comparison between the two unless you expect to get from the round that has both, two distinct zones and can make judgement from a single round on two different effects. That is the intention?

DR. SULLIVAN: The intention is to run the internal/external together, to evaluate the difference. If there are significant differences then we need --

DR. ZUDANS: Differences as to what?

DR. SULLIVAN: They both ought to see the same hydraulic test conditions.

DR. ZUDANS: I see you have compared the regions where they are external to internal and see those differences.

DR. SULLIVAN: Right.

DR. ZUDANS: But what you are saying, you run a single test essentially instead of two tests that you cannot very well repeat and if you are lucky, you may have enough clean indication as to resolve this question?

DR. SULLIVAN: Right.

MR. PLESSET: All right, why don't you go on, Dr. Landry?

DR. LANDRY: Does that finish up with any comments on that test?

MR. PLESSET: Let's go on to the next.

DR. LANDRY: The next sequence that I would like to talk about is after we run the L2-5 test through the completion of the program. Now some of these tests -- the first three on here in particular, the LA-10, L9-3 and LA-3, may be shifted around somewhat in timing.

These are rough estimates on dates because we nailed these down really at the last minute. The boron dilution test, we informed Idaho that we wanted to be firm at the last minute, so this is a rough estimate on that date.

The second ATWS test, LA-3, has been put in at this point at this time just as a back-up to the first ATWS. Licensing really has not expressed an interest in running the second ATWS. They have expressed an interest in the first ATWS.

DR. CATTON: That is L9-3?

DR. LANDRY: L9-3, yes. the ATWS with the loss of feed water. Now we put the second ATWS in there as a cushion and as a contingency which may be moved later in time. It may be dropped completely.

DR. ATTON: Now with respect to ATWS tests, there was a good deal of discussion about the non-protecticality. It didn't quite represent a full-scale BWR because the nutronics (ph.) were so different.

I am wondering if somebody has sat down and done calculations to demonstrate why LOFT may be of value in running an ATWS test.

I understand NRR wants the test, but NRR didn't make good arguments for it at the time.

MR. PLESSET: Let me put it a different way. Let me put it this way: that the ATWS test should be omitted altogether unless there is a real strong justification for it.

DR. CATTON: I would like to carry that to the first test, too, the boron dilution. There were some questions as to just what were you going to learn and I frequently see that, hey, that would be a neat test to run, boron dilution.

Suddenly it comes into existence without any study as to what you are going to learn from it.

MR. PLESSET: I would say the first three tests on that page --

DR. CATTON: I would incorporate the fourth one, too.

MR. PLESSET: Well, we haven't gotten down to that one yet. We may get rid of this page altogether.

(Laughter.)

DR. CATTON: I don't want to imply that the test is no good. It is just that because the name of the test is good is not reason enough and that is all I have seen for running the test, is the name.

DR. LANDRY: If you would like to hold the comments on this until we get to the individual discussions, we will go through each test individually. We can go through it in detail then. Let's do that right away.

The only other comment I want to make here is that the last test -- the L2-6 test -- is intended to be a fuel damage test. That would be run with the center module pressurized and that one we plan on either delaying ECC or in some way raising the power to a full 16 Kw per foot and then delay ECC, but we do intend, on that test, to cause ballooning and burst of the fuel.

That would be limited in burn-up so that we don't have a lot of fission products coming out.

DR. CATTON: When you decided to do that, that you are going to go to the point of damaging fuel -- again, I don't think that damaging fuel for the sake of damaging fuel is a good idea. Are you going to actually make a calculation that says, gee, this is the point that we think we will just damage fuel, and then go run that test to that point?

DR. LANDRY: That is one thing that we will be doing. We are not going to go right to that point.

DR. CATTON: If I am getting you out of order, I will just back off and wait.

DR. LANDRY: I will talk about this one for a while. We have looked at the effect of ballooning and the yield point of the fuel.

We feel that we can easily go beyond the burst with this assembly. The intent is not simply to destroy fuel. We feel that it would be helpful to see the manner in which the fuel balloons and bursts, whether it is a coplaner pattern, if it is random or just -- we want to look at the pattern and provide the information to the fuels people.

One of the points that was made by Bill Johnston, who is now over at NRR in charge of core performance branch, was that the LOFT special review group did not ask the fuel people for input, it did not include any fuels people on the special review group or the consultants, and he felt they had some very important points that they wanted to express and wanted to answer, or at least wanted to get some information from a LOFT test.

One of those points is fuel damage, not to the point of melt of the fuel but to the point of bursting the cladding and examining the effect of going through the alpha beta transition region for zircalloy, the effect of the pattern in which the fuel would burst.

We have presented to the commission our view that we would like to include the L2-6 test and they have agreed on that part.

DR. CATTON: As long as we are talking about fuel damage, as we discussed earlier, DNBR is really becoming important and really DNBR is related to fuel damage.

Why is it that somehow a test of that type isn't built into here somewhere? Is it because you can't run those kind of tests? You can't sneak up and hold it right near 1.19?

DR. LANDRY: That is a little difficult to do with a nuclear reactcr.

DR. CATTON: But that is what it is important for and easy tests --

DR. LANDRY: I am not real sure that we could run right to the point of DNB and hold the plant at that point.

DR. CATTON: You keep saying that DNBR, the margin is being pushed down. We hear from PBF that gee, you can go into fuel boiling and it doesn't cause any problems. We hear all of these things and that gets factored back into the licensing arena and the 1.3 starts creeping down. It seems to me that LOFT is a facility that maybe could look at that.

DR. LANDRY: I am not really sure with the instruments that we could measure the right parameters to calculate DNB ratio along the fuel rod or at a given point.

Of course we went through DNB -- on a lot of the tests and for the small break tests we invoked boiling and I guess steam cooling in the L3-6 test, but it is difficult for us to --

DR. CATTON: It seems to me that it should be brought to bear on the DNB.

DR. LANDRY: It is very difficult for us to back out precise heat transfer data from LOFT.

MR. PLESSET: You are not adding to our enthusiasm about LOFT.

DR. SULLIVAN: The DNBR question is not one of reducing the ratio, so much, to me. What they are doing is saying to me that the data base is getting better and better because it is 95 confidence that they are looking for and as long as they can justify that confidence then I think the regulatory staff would let them decrease --

MR. PLESSET: How do they get the data base so that we can believe it?

DR. SULLIVAN: That is the question.

MR. PLESSET: That is the question because if they have data that really is pertinent, no question of the analysis of that data to a 95 percent confidence level, but the question is, is the data relative; right?

DR. SULLIVAN: Right.

MR. PLESSET: That is what is of concern. I don't think LOFT can really add to this question. I agree with you there.

DR. LANDRY: We really don't claim to be a facility for obtaining heat transfer data.

MR. PLESSET: Right, I agree with that, all right.

MR. WARD: It seems to me if LOFT certainly couldn't give you any DNB points -- as you said, that is possible --

about all you could get out of it is to characterize the type of damage that you might get. I don't know if that is of interest or not.

DR. LANDRY: The type of damage that we would get -- I am sorry; did you say damage?

MR. WARD: Yes, fuel damage.

DR. LANDRY: That would require a complete disassembly of the fuel bundle and examination. To do that we are talking about several million dollars just to obtain knowledge of the effect of DNB on cladding.

I really don't feel that we could justify that on a cost benefit basis.

MR. WARD: I am not proposing it. I am just suggesting that that would be the only possible connection of LOFT tests with DNB, it seems to me.

DR. LANDRY: We have obtained some more or less intuitive-type understanding of DNB with the L2-2 and L2-3 tests with the rod length measurement transducers.

We were able to tell by the prediction or the code made of one. We would go through DNB under a large break and correlate that with the measurements on the thermocouples and with the measurements of the rod linear expansion as measured also.

Now that doesn't give us precise data on DNB but it gives us a very good feel for whether the code is predicting it at approximately the right point or not.

There are too many parameters that we don't have, like the center channel flow. We can't measure the precise pressure at a point. The only time we can measure the temperature is when it occurs right at a thermocouple.

MR. PLESSET: Why don't you go on, unless you have another comment.

DR. LANDRY: I would like to talk a little bit about specific tests now. The test coming up in April is to be a piggy-back test looking at the loss of feed water. It will delay the scram for a time so that we challenge the PORV.

With open PORV we will try to bring the plant back down and reflood the steam generator.

This is a concern that was expressed by licensing based on calculations which some of the PWR vendors had done that one of their modes of operating and mitigating the consequences of a loss of feed water is to open the PORV, gag the PORV open and control the plant, but they started calculating core uncovered and problems with controlling plant liquid levels.

So we designed a test or inserted a test to try to answer this concern, or at least show the way LOFT responds so that NRR has a feel for the way that a particular plant responds under these conditions.

DR. CATTON: Where do you expect a surprise?

DR. LANDRY: We really don't. We have done the calculations --

DR. CATTON: If you don't expect a surprise, then what is the justification for the experiment?

MR. PLESSET: Which one are you talking about; L9-1?

DR. CATTON: L9-1.

DR. LANDRY: We don't expect a big surprise, but we expect to be able to see if the calculations which the vendors are doing are reasonable.

They have expressed a possible concern with the response of a big plant and NRR is concerned about the calculations.

DR. CATTON: Who is going to sit down and tie this all together? There is a question about the lack of typical[illegible]ty for an ATWS between a full-scale PWR and LOFT. You e[illegible] no surprises. I don't see any -- somewhere this has to be meshed together.

DR. LANDRY: This isn't an ATWS to begin with.

DR. CATTON: I thought it was.

DR. LANDRY: No, it is just a delayed scram. The scram is only going to be delayed long enough to produce enough heat and enough pressure to open the PROV.

MR. CONDIE: I will talk a little more about this test later on. It isn't a delayed scram. It scrams on high pressure a very short time after the initiation of the test

There were several things in this test that are to be used to confirm things we observed in L3-6 and also things that we didn't achieve in L3-5 so that is the purpose of putting them all together.

MR. PLESSET: Go ahead.

DR. LANDRY: The next test will be a simulation next summer of the turbine trip and the St. Lucie cooldown transient.

Since data exists for two other plants, we feel that by running a test which simulates those two tests -- or those two events -- we will have some basis for understanding the scaling differences between LOFT and a large plant for an operational transient.

Now we don't claim that this is going to answer all the questions and all the concerns, but it will give us a feel for the differences.

If we come back and reproduce those transients very well, then we can't -- we still can't say absolutely LOFT is prototypical under all conditions, but it gives us a little bit better feel for what we are doing with operational transients.

DR. CATTON: But you are not going to run a lot of operational transients so --

DR. LANDRY: But we have run four already.

DR. CATTON: So what this would do would be give you confidence with what you can do with the ones you have already run?

DR. LANDRY: We feel it would give us some confidence in the tests we have already run because we have run loss of feed water, loss of primarily cooling fluid.

DR. CATTON: But your secondary system is so much different than any other plant, again I am not sure what the justification is. Is it just an exercise in modeling?

MR. PLESSET: They will find out that it behaves differently from what was observed at ANO one and what was observed at St. Lucie and they will say, well, they are different.

DR. CATTON: Then Combustion Engineering will argue that none of it makes any difference because it is not prototypic.

MR. PLESSET: Right. So we can anticipate what will happen. Maybe they should spend their -- what does the test cost? What are these tests going to cost, these tests we are talking about?

DR. SULLIVAN: The range in cost in that price range is significant by a factor of two. The latest time that we calculated the number, it is like $1.8 million at a lower limit going up to nearly $4 million at the upper limit.

DR. CATTON: To be fair I might mention that this particular test was strongly argued for by NRR.

MR. PLESSET: But did they really explain why?

DR. CATTON: The explanation that was given that was this particular experiment had been run on Semiscale and that they had data from ANO Two and they would then have LOFT in the middle. LOFT is not going to be here forever. I am not sure what good it is going to do us if we do get the information. NRR indicated that this would be -- at the time a new group member said that these things would be justified and that there would be analysis done to demonstrate this and I still don't see it and I don't see any intent to do it.

One more comment. At the same time, the retake tran exercise that we saw where it was used on LOFT and also ANO and the arguments given to us by EPRI at the time that the single most important aspect of transients is not the primary system. It is all in the controls and the secondary side and everything else, which LOFT really doesn't have.

DR. SULLIVAN: There is a significant effect that you would like to see, and that is the reactor vendors are always saying that LOFT is atypical and it is so atypical that we cannot calculate those transients. In fact, the reason we cannot calculate it is atypical.

I believe that those two transients ought to be run because a lot of the argument will go away on the typicality, not on the secondary side, but for the primary side. If you know the boundary conditions that the steam generator corrected, and that is one of the things that we can control, then the interface is with steam generators and everythinc . ¬ the secondary side which is not in LOFT, there is st the question about, but everything in the prim . is either going to work or it doesn't, and that is of significant benefit too, I believe, because those guys are saying that they can't calculate the primary because it is atypical.

I think that this test will give us some indications of whether that is true. It is necessary but not sufficient to me.

DR. CATTON: That is a different argument. The argument I hear from the vendors is that we can't calculate it because we use EM models and you are asking us to represent something that is physical and we are an appendix case base (ph.) quote Jim Sermack.

DR. SULLIVAN: It depends on which one you get to first.

MR. PLESSET: What what time of day.

DR. SULLIVAN: Right, because when we asked them, why were they not paying more attention to the LOFT data, they said, well, our models are not built for LOFT, they are built for PWR's and all the atypicalities limit our codes to the extent that we can't calculate those.

If it came up that the scaling difference on the primary side -- if you can get rid of that question -- then I think that we would have a much stronger case in saying that, hey, look, there are some problems with your codes, or there isn't.

MR. PLESSET: Okay, let's go on because we have a lot of discussion yet to come on a lot of these tests.

DR. LANDRY: The next test is the L5-1, intermediate break test. We inserted this test because we thought that there was a gap in the knowledge on flow models and transitions that occur for imtermediate-sized breaks.

The data base for large breaks is pretty large now. We have run tests. We have run non-nuclear tests, we have run nuclear tests, Semiscale has run a lot of tests, other facilities have run a lot of tests and we feel that there is a pretty big data base on large breaks.

When we got into the small breaks -- especially L3-5 a 3-6 -- we found out that going into what would be a more typical type configuration -- a flow -- from the primary system to the break -- things that would occur like down an instrument line -- were not well understood for the computer codes or for the models.

We are not sure that the similar-type of surprise would not occur for an intermediate break so we thought that by running an intermediate break something like the loss of accumulator line, which could be a more severe intermediate break, that we would get a little bit better data base, a little bit better understanding of the type of phenomena which occur for an intermediate break.

This will then lead into the sustained core dry out at a higher decay heat level than we have run with the small breaks. With the small breaks so far when we have gone into the part of drying out the core, or uncovering the core, we have been far out in time from shutdown so we have had a relatively low decay heat level.

Now with the large break we had a very high decay heat level, but the core was recovered by the accumulator and by the ECC very rapidly.

We are talking about maintaining the core in an uncovered condition for a longer period of time with an intermediate level, if you will, decay heat.

We want to see the effect of running at that condition and whether we can recover the plant, mitigate the accident by using solely the HPI and the LIP pumps.

MR. PLESSET: How is an intermediate break characterized? How do you decide that it is not a small break, it is not a large break, it is an intermediate break? There must be some way of describing it. Tell me what it is.

MR. LANDRY: I am not really aware of the exact definition. At the time that we did this -- when the EM was being written and worked on -- Harold and I were both in licensing at that time -- the concern was, one was the large break and we were talking about one time to two times the surface area -- or the internal area -- of a large pipe.

MR. PLESSET: We are on the same wage length there. Are you going to tell us?

MR. CONDIE: Yes. I am Keith Condie from EG&G. Our main way of categorizing the small break versus the large break has to do with the core decay heat with normal mechanisms that control that transient.

We approached the large break in the L3-1 transient where we had an equivalent 4-inch break and that perhaps 90 percent or so of the decay heat removal occurred because of energy out of the break and not because of the secondary cooling system.

Pressure dropped so fast that we no longer had a heat sink in the steam generator. Contrast that now to experiment L3-7, a one-inch break, in which probably, in rough terms, probably 90 percent of the decay heat was removed by the steam generator and not by the break.

The pressure decayed slow enough so that it never did drop below that of the secondary so the entire energy was removed from the steam generator.

That is the way we would determine it so what I say is, on the L3-1, with the four-inch break, we approach what would be a larger break in that we could have eliminated the steam generator from L3-1 nd it wouldn't have had hardly any effect on the transient response.

So when we get into the intermediate breaks, larger than four-inches, then I expect the same phenomena would dominate until you approach the 200 percent double-ended break. It is ust a matter of time frame here now as opposed to mechanisms. Does that help?

MR. PLESSET: I was afraid you would say something like that. That is hardly a real physical description that I can follow carefully. Now I get a rough idea of what you are talking about, but I will accept it with a lot of reservations.

DR. ZUDANS: I thought that I knew what it was but now I don't.

DR. LANDRY: I think what you may be talking about, Dr. Plesset, is in the past --

MR. PLESSET: Oh, he has a little more.

DR. ZUDANS: I thought that the small or intermediate or large break related to two things: One is the amount of heat removed by the break; the other is capability to replace the water for the make-up system.

If you have a four-inch break and you say it removes all the decay heat, then any break is considered large break. That is the way you are saying it?

MR. CONDIE: Basically, yes.

DR. ZUDANS. Where does the intermediate fall? If you still need a steam generator to remove the decay heat?

MR. CONDIE: In the large break there is no need for the steam --

DR. ZUDANS: But the intermediate is the one that you need?

MR. CONDIE: For the intermediate breaks we don't need the steam generator either.

DR. ZUDANS: Then it is a large break.

MR. CONDIE: Pardon?

DR. ZUDANS: Then it is a large break, by your own definition. You said it you don't need the steam generator it is a large break.

MR. CONDIE: Because all the energy is removed from the break.

DR. ZUDANS: Where is the intermediate then; below that point or above that point?

MR. CONDIE: I just distinguish between the small break and the large break and the intermediate break is based on our experience with what we call the small breaks. It is one that is going to behalf very much like the large break only over a longer period of time.

In other words, in the depressurization rate, the phenomena is going to be basically the same.

DR. ZUDANS: And when is a small break considered a small break? When does a small break begin?

MR. CONDIE: It is relatively arbitrary. When we decay heat removal mechanism is dominated by the steam generator --

DR. ZUDANS: Where does your make-up capaci y end in terms of break size?

MR. PLESSET: I think he has asked the question that I wanted to bring up.

MR. CONDIT: Where does the make-up?

DR. ZUDANS: At which break size which you qualify as small, intermediate and large, are you no longer able to make up the water that you lose through the break?

MR. CONDIE: You mean without the emergency cooling system?

MR. PLESSET: Let me put it another way. At what break size is the high pressure ejection system not able to keep up with the loss of inventory? Where would you put that one? That is really the bottom line?

DR. LANDRY: That is between a one and a four-inch.

MR. CONDIE: Somewhere in between one and four.

DR. SULLIVAN: I think it is more between one and two, when it gets down to that, and it is plant dependent also.

MR. PLESSET: How is that?

DR. SULLIVAN: It is plant dependent, depending on what the capability of what the high pressure injection is. Some plants can inject right up to the safety relief valve setting.

MR. PLESSET: And with sufficient flow.

DR. SULLIV[illegible] Yes.

MR. PLESSET: Others can't.

DR. SULLIVAN: Yes, but if I remember correctly, it is between one and two inches.

DR. ZUDANS: That is for LOFT?

MR. PLESSET: No, that is in a Westinghouse-type, some of the Westinghouse.

DR. SULLIVAN: Yes, some of the Westinghouse.

MR. PLESSET: When he talks about an intermediate break, he is talking about a loss of inventory, or is he keeping the inventory out? It seems to be some relationship between what the steam generators can do; right?

This is so atypical in LOFT, what am I going to learn?

DR. SULLIVAN: The steam generator heat transfer, I don't believe, is that atypical. At least we can characterize it.

MR. PLESSET: or any pressurized water reactor they are all easily characterized in your mind?

DR. SULLIVAN: No.

MR. PLESSET: Okay, I was going to ask you about that next.

DR. SULLIVAN: I don't believe that is true.

DR. ZUDANS: After this conversation, at least I remain unclear as to what is intended with the definition of small, intermediate and large in terms of the behavior.

In other words, if you have a small break I would assume that you would be able to make up that break.

MR. PLESSET: With no essential loss of inventory.

DR. ZUDANS: That is right, you could maintain inventory. If it is an intermediate break maybe you would not be able to maintain it completely; right?

MR. PLESSET: Without further depressurization, say?

DR. ZUDANS: That is right.

DR. SULLIVAN: In an intermediate break you definitely cannot maintain the system volume up until the time that you get down to very low pressures. The effect that we are looking for is, in a very large break, you have the entrainment (ph.) mechanisms that try to expel the water and now will come a bypass question.

MR. PLESSET: So you are talking about part of the refill?

DR. SULLIVAN: Refill, reflood, if that is the case, all of that question. In an intermediate break, you have another set of questions because you are ejecting -- now the accumulators are injecting into the system that is partially filled with water.

The pressure is maintained at some relatively high level so that the accumulators can't just dump. They may inject some fill and put some more water back in the system -- they may be shut off. Those are the types of questions that we are looking at.

MR. PLESSET: I think it would be better to characterize the tests that way rather than talking about intermediate or small or large. I think that makes it pretty clear what you are after. It does to me, anyway.

DR. ZUDANS: It is better if you say that because a large break is a break that reduces pressure fast enough so that your accumulator --

MR. PLESSET: That is right; that is the important point.

DR. ZUDANS: The intermediate break is somethi , that may or may not do it exactly. That is another category. That makes sense, because a small break is something that cannot remove the decay heat. You need a steam generator. That is another picture again. The pressure may stay up.

MR. PLESSET: I think we clarified it. I learned something and it is a good day when I learn something.

DR. LANDRY: The next break is one that I think we can pretty safely call a large break. It is a full double-line cold leg break. This is the L2-5 break. It is one that will be run with loss of off-site power. In other words, the ECC will be delayed for a time.

We will have imbedded thermocouples in the cladding

I don't mean to reopen that.

We have done some tests -- EG&G has done some tests -- with cladding samples with the thermocouples imbedded. Now the tests have shown that there is a great deal of margin between the burst strength -- the yield strength -- and the type of strain that the cladding will be under in this test.

There is a margin of about twice the pressure or more.

If you are interested, I brought some samples of the thermocouples.

(Samples are distributed.)

This test will be run with the center bundle pressurized to 350 PSI.

The next test that we propose is the operational transient loss of boron, or boron dilution from cold shutdown. NRR raised the concern about boron dilution from cold shutdown because calculations came in from one of the vendors which showed that instead of having the 30 minutes required for loss of criticality, or for reaching criticality, they had three minutes. They found a mistake in the calculation.

Now NRR was concerned about whether anybody was calculating boron dilution correctly so they asked that we run the boron dilution test so that the data would be available for comparison from vendor calculations.

Vendors can be used to calculate boron dilution for

LOFT.

DR. CATTON: What are you going to measure to determine whether or not their codes are any good; just how fast you shut it down?

DR. LANDRY: We are a little uncertain on the details at this time. We are still planning the details of the test. We are looking at trying to measure the wave front, or the clean water -- demineralized water -- moving through the reactor vessel by use of the neutron detectors -- the power detectors -- to measure the neutron flux throughout the core.

One of the concerns NRR has is, does this clean water come in and mix with the borated water or does it come in and stratify and move as a wave?

DR. CATTON: Wouldn't you be better off running that kind of a test as a separate effects test? That is a mixing process and you have to worry about the flow patterns that you have in the lower quantum and it becomes multi-dimensional and all kinds of things.

I can only see you getting the integral effect of all of these things with LOFT and then comes the question again of, gee, your core is a different shape and all these other atypicalities. What you get doesn't matter. I am not going to believe it because I am a vendor and your reactor is just not the same. I can calculate it for mine.

I don't know; how do you address these things?

DR. LANDRY: We discussed those very points with licensing and they feel very strongly that without trying to be prototypical of a large plant, we realize that we have a small diameter, smaller volume, so we will be atypical, especially in those parameters, but they do feel strongly that it is necessary for a vendor to calculate what happens for our particular plant -- for LOFT -- and show mainly, the first point, when we will lose our margin to criticality.

DR. CATTON: I understand that but the thing is, I don't know of vendor codes having the kind of detail in them to allow them to calculate how much boron is where. They have a few nodes and they are all uniformly mixed. They can't follow the front with their codes.

Unless you come up with -- licensing comes up and asks that they are able to do that, I don't know what role your results can play. I am not trying to say the test is a bad test.

MR. PLESSET: I would be willing to say it.

DR. CATTON: I just wrote down some questions that I think ought to be asked on all of these tests. What is the phenomena in question? Second, why do I need the data? Here I am not sure you are measuring the right things that would do me any good if I had to develop a model.

For instance, how will it be used? Are you going to use it in cold assessment?

MR. PLESSET: Dr. Zudans will make a comment too.

DR. ZUDANS: The additional question on the same line of reasoning is this: Since these are small pipes and you prove something with these tests -- for example that there is adequate mixture, no stratification and what not -- there is no way for a positive finding on this particular test to transfer the information to full-sized plants. It is totally impossible.

Even if you find out that it stratifies in this case also, then that might be a question.

MR. PLESSET: Let's not go too long on this, Harold. Go ahead.

DR. SULLIVAN: I guess I am going to change hats on you because I did say -- I got permission that I could sort of speak for NRR.

MR. PLESSET: Yes?

DR. SULLIVAN: Ralph has indicated the question was, there is a problem with a Westinghouse plant and the time they have to respond. We have gone over and discussed this at length -- research has -- with NRR.

We have indicated all of the problems that you have brought up also. We are wondering about the sophistication of their system, we are worried about the data, whether we were going to be able to see the trends in the data. We are not even sure that we can follow the wave through the system.

The thing that we are relying on is a change in core physics. We are worried about the core also, about the atypicality being in the LOFT core compared to PWR.

So there are a number of questions that we -- research -- addressed to the NRR. NRR still thinks that there is no data around on this question. The plant is the largest sized test facility that we have.

We see that one of the possibilities is the 3-D effect and what the 3-D effect is. So there are a number of questions about what happens to the boron or the pure water and how do they separate.

They could separate out on the lower plenum and you would have to fill the whole lower plenum of the reactor up before you would ever get the pure water into the system and the mixing qualities.

We think -- and this is the thing that NRR has impressed on us -- that it is a test that is necessary that they calculate, but it certainly isn't sufficient. It is the only piece of data that we can get.

If you look back at the nuclear-ness of the system, it meets one of our primary objectives of not running the test unless they are using the capabilities of LOFT and the large scale and the nuclear-ness.

So we see that there is a lot of trouble with the experiment. We also see that it has benefits. Now trying to answer your questions, the phenomena that we are looking for is, what happens to the wave as it goes through the system.

MR. PLESSET: What happens to what?

DR. SULLIVAN: The wave. There is a wave of relatively pure water that is going through the system. Do we need it, yes, there isn't any more data and there is a problem in the licensing structure.

The last question, how to use it, you make the vendors analyze this test.

DR. CATTON: I have some more questions.

MR. PLESSET: On this?

DR. CATTON: Yes. I didn't really finish. I think it is, what separate effects data has been brought to bear on a particular test and where is our knowledge so weak that I need to run a large test and I guess what specifically is being confirmed.

What I see here with the boron is, you have a basic process. You have got a flow loop and suddenly you are injecting something in it that is going to, in itself, cause density distributions. This is going to change the flow. I am not sure you understand the basic process.

No separate effects tests have been run. What happens when they dump boron mixture into the lower plenum? Does it settle to the bottom? Just what does it do? That separate effects test doesn't exist, yet you are going to suggest running the integral test. I think it is premature. I think when you run a test that costs millions of dollars you ought to have spent two or three hundred thousand dollars to go through to model the basic process and bring it to bear on your system first.

DR. SULLIVAN: The vendor codes have these models.

DR. CATTON: We know the vneodr codes don't, Harold. They are one dimensional and they are large nodes.

DR. SULLIVAN: The vendors codes have a model in them and the thing that we are trying to do is run a test to see if they can predict it. If you ask me when we get through with this, are we any smarter, I would say yes, but are we smart enough to let the whole question die, and the answer is no.

MR. PLESSET: Is it necessary to have a nuclear facility to get this information?

DR. SULLIVAN: I would say that the nuclear-ness is one of the questions that you are trying to figure out because we are looking at the difference in the reactivity.

MR. PLESSET: But if you know how the boron concentration changed following an injection, I am sure that one could then get this thing that you are talking about.

That is really the question, how the boron distributes itself and what time scale and flow problems do you have and I don't see that you are really going to do this as well in

LOFT as you would in, say, core test facility or something like that. I just think it is doing the wrong thing.

It is just the wrong approach to a mixing problem. If you know how the mixing goes, then the neutron specialists can tell you what happens to the reactivity in a very detailed way, much better than you will get out of this test.

DR. CATTON: In a sense it might be unfair of us to get on Harold like this.

MR. PLESSET: No, no, he is just out there. He put on that hat for a minute.

DR. CATTON: Do you have your NRR hat on?

DR. SULLIVAN: Let me hear the question first.

(Laughter.)

DR. CATTON: I just want to make a comment. NRR is acting very strongly with respect to this boron question wanting to run a LOFT test. I think if they are pushing for it, it is unreasonable of NRR.

MR. PLESSET: Well, let's go on. Maybe Harold will take some message back somewhere.

MR. WARD: May I say one thing?

MR. PLESSET: Yes.

MR. WARD: I think one thing the neutron specialists can't do is back down to the mixing. If they observe the reactivity effects, they are not going to be able to back down and tell you what the mixing is so you really aren't going to learn very much from them.

MR. PLESSET: That is right.

DR. SULLIVAN: We had the physics people in NRR look at it, we have had our physics people, we have had the LOFT physics people, we have had our thermohydraulics experts look at it and there are a lot of questions. I am not saying there is a unanimous opinion.

MR. PLESSET: Very good. Let's go on.

DR. LANDRY: The next test is another controversial one, the ATWS test. Some of the concerns that went into running this test, we have had no ATWS tests run anywhere to date and we felt -- NRR felt -- that it would be important to run an ATWS test and observe the response of the plant.

Now we are aware that there are numerous atypicalities.

DR. CATTON: Thereis a feeling that is very strong that the atypicalities would overwhelm any worth that your atypicalities may have.

MR. PLESSET: This is in the review group?

DR. CATTON: That is right. I wouldn't speak for neutron specialists because some of it is kind of science fiction, I think.

I think that particular aspect, you have to demonstrate convincingly that you are going to contribute to a full-scale plant.

DR. LANDRY: Through this test program that I am talking about today, I would like to emphasize that we are not trying to show a prototypicality, but we are trying to provide a base from which to assess computer codes by calculating our particular plant.

DR. CATTON: I am trying to recall some of the statements that were made. One was that under given circumstances like this the neutronic calculations during an ATWS are not where the problem lies.

Calculating the power that is going to be dumped into the flow is not where the problem is. The problem is determining the hydraulics side.

DR. SULLIVAN: I am not sure that we agree with that.

DR. CATTON: I am not claiming any responsibility for the statement I made either. I am just trying to put out on the table what some of the concerns were and why some people felt that running an ATWS with LOFT really wasn't going to gain all that much.

I am not sure of the cost effectiveness of the test. Does it meet the criteria that you are going to learn something that is worthwhile?

DR. SULLIVAN: There are two distinct questions with almost any ATWS. The first is the neutronics and the other side is the thermohydraulics. The neutronics, we know we have got some problems with it but we think that they are solvable in terms of, we know what characterizes the LOFT core, in terms of the neutronics.

So the question is, are we going to learn anything about the neutronics and we have gotten the physics guys to say yes, we will learn something.

The other side is the hydraulic question and I think there are some questions about the hydraulic side.

DR. CATTON: Yes, and I think I would agree with you. In a situation where you are going to spend all this money -- I think I would personally like to see what it is that you think you are going to learn, but one of the things they brought out -- or that this particular person brought out -- was that your power ascension rate is going to be a lot lower in LOFT than it would be in the other reactor because of the high leakage and the aspect ratio or something.

Now that is going to have an impact on the thermo-hydraulics and what is that impact? How is it going to feed back into what we need to know?

MR. PLESSET: I think it is clear that it is going to be not prototypical. The question is, how badly off will it be. I think it will be pretty badly off and that is what I would emphasize.

DR. CATTON: The other thing is, how much does it matter?

MR. PLESSET: That is the other point. The neutronics people say they will learn something. I think it would be ridiculcus if they said anything else, but that still doesn't mean that what they would learn would be significant.

DR. CATTON: And there is not a concensus amont the neutronics people either.

DR. SULLIVAN: Or the hydraulics people either.

MR. PLESSET: Let's go on. I think we have beat this one around enough now.

DR. LANDRY: The next one is more of the same. The next test that was put in is a contingency ATWS. We put that in because if something comes up during the first test -- the L9-3 test -- that is a surprise we may want to run another ATWS test. This may be another type of ATWS. Instead of loss of feed water, we may use a loss of off-site power as an initiating event.

That is a very weak contingency at this point.

The next test I would like to talk about is the LA-9 test. This was originally going to be a contingency test but, like the boron dilution, we have defined this test as a small break LOCA, under our discussions with NRR.

NRR has expressed the feeling that they would like to see another small break with the pumps on to confirm the results of the L3-6 test.

There were a number of code changes made after L3-5 and L3-6. They expressed the view that they would like to see another test with the pumps on to see if those code changes had not made the codes break dependent.

That would mean that we would look at running this test with something like the scale of three-inch and two-inch break instead of a scale of four-inch break as we used for L3-5 and L3-6.

MR. ACOSTA: I am confused about that. I thought the policy was to trip the pumps.

DR. LANDRY: Based on the results from L3-6, the policy was confirmed to trip the pumps. Licensing would like to express the view that they would like to see another pumps on test with a different size just to confirm that result and to confirm the independence of the codes.

MR. ACOSTA: I thought on small breaks much hinged on where the breaks was, whether it was a horizontal leg, a vertical leg, top of the pipe, bottom of the pipe and so on. How can it be break independent, particularly with the pump?

MR. PLESSET: You mean it is also location?

MR. ACOSTA: Location, certainly; location dependent.

DR. LANDRY: One of the parts that NRR did ask for was to ask for possibly looking at a different location, but we don't feel that it would be possible on LOFT, or feasible within the time frame and the monetary constraints to put another break at another location than where we have the piece located now for L3-5 and L3-6.

We put that break in a readily accessible and already existing location. To make the system hardware modification to put a pipe break off the bottom of a pipe or off the top of a pipe would mean extensive hardware addition, extensive system modification and in some cases maybe just plain impossible because of location of other equipment.

DR. CATTON: Let me bring up one of the big atypicalities that was pointed out at this point. You are doing pumps off and pumps on with basically a single loop plant.

Now if we think about a plant that has got two hot legs, four cold legs, four pumps, two steam generators, and you put a break in that system somewhere, you can get oscillatory behavior between the various parts of the system and it seems to me that that would play a role in whether the pumps are on or the pumps are off -- which way you should do it.

It may also impact on various kinds of phenomena that are occurring in the plant. This is the point that we have raised and the reason given for not using LOFT for small breaks. Here I see a continuation and a strong desire on the part of NRR, I don't understand it.

MR. PLESSET: I think somebody told NRR there is a LOFT and they have just discovered it.

DR. CATTON: I think further that NRR can request whatever they have from LOFT and with really not much of a responsibility to their request.

DR. SULLIVAN: Again, we are negotiating the test matrix with NRR and we don't always win. The thing that NRR has pointed out is that they made a decision to turn those pumps off based on calculations and those calculations were made for both conditions of pumps on and pumps off.

We now have a reasonably good feeling that the vendors are not going to do a good job with the pumps on.

DR. CATTON: The multiple loop aspect needs to be addressed.

DR. SULLIVAN: That is one of the things but they are also not calculating what we have got either.

DR. CATTON: That is certainly true.

DR. SULLIVAN: So we look at it and -- they are looking at it and saying, we need some more justification. I think they stepped out on a limb pretty far and if I was over there I would like to see another one too.

MR. PLESSET: You would like to see what?

DR. SULLIVAN: Another test.

MR. ACOSTA: Harold, is this a situation where you have to have LOFT? Would not Semiscale, with all the modifications that can be done to give you more flexibility in studying complex hydraulic situations with characterization of the pumps still up in the air --

DR. SULLIVAN: The Semiscale facility has run a whole series of these experiments and they are adding to the data base. There seems to -- even the calculational capability that we have, there seems to be a question, is there something different between the Semiscale test and the LOFT test.

So the test that was run in the pumps on case was not very well calculated by us either. In fact, those that were sitting there watching were surprised, to say the least, and it is one of the cases that we have where we ran a test and were completely surprised.

We went into the test and we knew we had some trouble and we didn't know the magnitude of the trouble and I think we were kind of surprised at the magnitude also.

So it is one of the cases where it would be a repeat test, if you want to call it that, but it would be different conditions. It is probably justified.

DR. CATTON: I was at that test, Harold, and I recall the surprise when things didn't go the way they were anticipated to go. Now the test has been done, the data is available. What has happened? Have any models been changed?

It is my understanding that the steam generator modelling was where it was weak. Has NRR changed any of their steam generator modelling; has anybody else? I mean, why are we running another test at this time?

DR. CONDIE: You are asking two different questions.

DR. CATTON: To me these things need to come sequentially. You learn something --

DR. CONDIE: We have changed our characterization of the LOFT pumps. There was some thought that it could have been the steam generator modelling; that the flow through all the tubes and so forth wasn't taken care of quite correctly and there was some blockage and this caused the uncoupling of the steam generators in the primary system.

DR. CATTON: I don't see anything going on to remedy that. It seems to me before you run another test, you need a separate effects test to look at that steam generator problem.

DR. CONDIE: I think that is in our heat transfer package within the code itself.

DR. CATTON: Has it been tested or are you going to run LOFT again to test your new modelling?

DR. CONDIE: I can address that later as we talk about the L2-5.

MR. PLESSET: Let's let it go until --

DR. CONDIE: We have some concerns in that area, too.

MR. PLESSET: Why don't you go on?

DR. LANDRY: The next test that we included is another contingency test. This would be another intermediate break. We included this test in case we get some suprises with the L5-1 test. We would have the opportunity to run another intermediate-sized break, possibly a pressurizer surge line, a little bit different than the previous L5-1 which would be run with the accumulator line as the rupture. It would give us a little different data and a little different phenomena.

It is a contingency test which may or may not be run.

That brings us to the last test, the L2-6 test. This test is intended to be run with pressurized fuel and here in the slide I have 600 PSI. Idaho has recommended that we use 350 PSI for the prepressurization and we are discussing that point with Idaho, with our fuels people in research and with the fuels people in NRR to determine what would be the best pressure to run the test at.

This test is intended to balloon and burst the assembly.

MR. ACOSTA: This is the last test?

DR. LANDRY: This is the last test that we have planned.

DR. ZUDANS: If I may ask you a question to the previous test. There was a piggy-back test -- L9-1 and L3-3 -- and as one of the concerns listed is a vendor procedure to lock open PROV. Could you explain what that concern really means?

DR. LANDRY: At this time for the loss of feed water where the steam generators dry out, the procedures of the vendors is to open the PORV, gag the PORV open, and in that way relieve the pressure from the primary system, but what they are in effect doing is causing a small break LOCA, which they supposedly have under controlled conditions.

The calculations which they have done show that they go into a rather severe core uncovery. The reason we are doing this test is NRR is concerned about the calculations and the procedures which the vendors have for mitigating the accident.

Basically what we are doing is try to do a test similar to the calculations to show if it is possible to control the plant by the PORV.

DR. ZUDANS: That would the L3-3 test, then?

DR. LANDRY: That is the L9-1 and the L3-3 combined, yes.

DR. ZUDANS: I have one more question only. On the L9-3 which involves the ATWS test, I guess you said that something unforeseen would happen --

DR. LANDRY: On LA-3, the contingency ATWS?

DR. ZUDANS: Yes.

DR. LANDRY: What we were saying with both contingency tests that we now have in -- the ATWS and the intermediate break contingencies -- is that if we arrive at some unforeseen problem with the planned test or if we get some surprises with the planned test, we will have included another slot in which we can do another slot of a similar nature to see if we have had a unique experience or if we have a problem with tests or accidents of that nature.

DR. ZUDANS: I am sorry; I didn't quite finish my question.

If the connection between L9-3 and LA-3 -- on LA-3 you say if something unexpected happens. What unexpected is expected to happen?

DR. LANDRY: That would be if something unexpected happens with L9-3, the first ATWS. If we have a prediction for the first ATWS and something occurs which we could completely not foresee, then we would have a time slot in which we could do another ATWS and do a different type of ATWS to see if that reoccurs.

DR. ZUDANS: Is it not possible that this unforeseen could be quite major and damage your fuel and that no subsequent tests could be done?

DR. LANDRY: That is a possiblity with any test.

DR. ZUDANS: Why not shift this test to the very last?

DR. LANDRY: We don't believe that with the ATWS test that we are going to have severe core damage. We really don't believe we will have any fuel damage.

DR. ZUDANS: To ask the question otherwise, have you already analyzed all of these tests?

DR. LANDRY: No, not in detail.

DR. ZUDANS: Is it planned to be done prior to even selecting the test matrix?

DR. LANDRY: As we move along, yes.

DR. ZUDANS: Not as you move along, prior to selecting the entire test matrix?

DR. LANDRY: The test matrix will be set. We will have the basic analysis before we set the test matrix, but the detailed analysis, we simply cannot do it before we do more tests. We can't do all the analysis and then start doing the tests?

DR. ZUDANS: Why couldn't you? Wouldn't that be a lot more sensible?

DR. LANDRY: If we were going to do that we would have to delay testing for at least a year or more. In that time we would have to do nothing but analytical work. Then we would have to begin the testing and that would put off the test date instead of mid-FY '83 to at least mid-FY '84. That would incur must larger costs because we would have to maintain the staff to operate and perform the tests. We could not put them out on waivers for a year while we sit back and do analysis.

Once we start doing the tests, we would want to keep an analytical staff available in case something occurs that we do not foresee, so we could make changes in test design and test description and to analyze the test results after the tests have been run.

DR. SULLIVAN: There are probably some management problems that we ought to discuss. First, the facility is extensive. It is expensive to do anything. Just to let it sit, it is expensive.

The second thing is, we have pre-planned a set of tests that we are now -- there was an old test matrix that was the one that we presented to FY '85 and we were marching down that test series.

There is some point that we can change and be cost effective. There is a point that we won't be able to change before. There is also pre-planning to see if we can even do the tests that you need, and then before you do the test, there are a lot of detailed calculations that you do for facility's sake and we think that we have designed the stopping point that we can change and we have indicated that.

We have done some of the calculations. Some of those more need to be done and we need to make a decision on the order of tests and the time schedule that they fall into and then the detailed planning.

DR. ZUDANS: Wouldn't it be more logical and more appropriate to have adequate calculations to decide whether or not you even want the test, plus don't you run every one of these tests on Semiscale before you run LOFT anyway?

DR. SULLIVAN: No. We haven't even tried major series. The Semiscale facility is significantly different from LOFT. It hast the two active steam generators. It has both loops that are active now. The loop seals are different, the height of the steam generators is different, the core is different.

DR. ZUDANS: At least I remember in some presentations in Idaho you showed how you ran them on Semiscale just prior to LOFT to make sure that the LOFT test was okay, so it is not exactly true; you don't do it in every case?

DR. SULLIVAN: That is true.

DR. ZUDANS: You do in some cases?

DR. SULLIVAN: Yes.

DR. ZUDANS: The other thing is, before you run a whole LOFT test you certainly must have a complete detailed analysis because you really don't know how to stop your test if you don't do that.

DR. SULLIVAN: I think the definition of a test is bothering you. When we say details, we mean details. I mean there are the safety questions; there are the licensing type issues.

DR. ZUDANS: So your safety analysis report contains adequate analysis to predict gross behavior of your experiment nad if your analysis safety report indicated there was nothing really new to be learned, why would you want to run the test?

DR. SULLIVAN: We wouldn't.

DR. ZUDANS: That is why I said it would be nice to have some analytical results before you even decide whether to run or not to run a test, not necessarily on a matrix because you gave a clear explanation that that is not practical.

DR. SULLIVAN: We will start in the planning of -- once we decide what to do we will then start to plan those experiments. Part of that planning is we do a lot of -- one of the major parts of planning will be to do analyses, and we have done a lot of analysis for these experiments because they were in the old test matrix.

So we have done calculations for almost all of them.

DR. ZUDANS: I see.

MR. PLESSET: Let me try to focus the discussion a little bit and also to plan continuing discussions, When we look at the test set-up for the proposed series, we had one page here that you gave us and then we have two pages a tabular form. Which one should we look at when we tell you which to throw away in our opinion and which to keep?

DR. SULLIVAN: The first test matrix that was presented was the LOFT special review group test matrix, and the one that had the double lines across it where we wanted to stop.

MR. PLESSET: Yes.

DR. SULLIVAN: Then the second one we presented is a more detailed explanation of that.

MR. PLESSET: So what we might then look at, since we might be interested in the details, is look at the two pages of tables.

DR. SULLIVAN: That is the most recent.

DR. LANDRY: That is the one that is current as of last week.

MR. PLESSET: Yes, that is good enough. Let me ask about what would be a suitable sequence because I believe we will be breaking for lunch. If we could look at that now when we have Dr. Landry's presentation fresh by, as we mull it over as we have lunch, go to that. Then go to the presentation of the L3-6 and then we will have Dr. Karwat take advantage of his being here to have him make some comments to us and then we will have water hammer. How does that seem to you? Does that seem reasonable?

If the group here is agreeable, we will have lunch and then discuss this two-page table to see what our comments are. That is really one of the main objectives of this meeting.

Then I will talk to Dr. Karwat to see whether he wants to have some comments before or after Mr. Condie's presentation. That is a minor readjustment.

So let's have a recess for lunch. We are a little behind but fortunately we have a little spare time, so let's recess for lunch for a while and then reconvene.

(Whereupon, at 12:42 p.m., the subcommittee recessed, to reconvene at 1:45 p.m., that same day.)

A F T E R N O O N S E S S I O N

(1:52 p.m.)

MR. PLESSET: The hearing will be in order.

What I thought we would do is look at this two pages of tests, and indicate some that -- get some unanimity regarding keeping or dropping, and because that is one of the main purposes of the meeting.

And after that, I think we will ask Dr. Karwat to make some remarks. He is here as an observer. We are very glad to have him here. He can see how the ACRS, at least the ECCS Subcommittee behaves, and we would like to have his comments.

So we will go to this two pages of tests. I think we can use that -- at the end of looking at those, if there is anything somebody wants to add, we can consider that briefly. I am a little bit doubtful if there is anything we would want to add.

Now, I think that the first three tests are pretty well set anyway. There isn't much to say about them, isn't that right, Harold?

DR. SULLIVAN: Yes.

MR. PLESSET: And the fourth one also, the L5-1/LA-2? That is pretty well committed?

DR. SULLIVAN: It is pretty well committed, but you know, I think that we would like to look at the comments

2 you made about that, about moving it, until after --

MR. PLESSET: Well, maybe we should go back and look at L6 -- the one just ahead of it. It would be a little late to try to indicate that one wants this dropped out, or -- is that correct?

DR. SULLIVAN: Yeah, I am afraid that the L5-1 test is probably the same way.

MR. PLESSET: Same way.

DR. SULLIVAN: But still, we would like to look at it.

MR. PLESSET: Anybody want to make some comments [illegible] on L5-1 and the L8-2? Now is our opportunity to do [illegible] There were some unenthusiastic remarks about it already, is that right, from us?

DR. LIENHARD: About L5-1?

MR. PLESSET: Yes.

DR. CATTON: I think the comment was that maybe it should come after the new fuel is in it.

MR. PLESSET: Oh?

DR. CATTON: But other than that --

DR. SULLIVAN: That is the only thing that I --

MR. PLESSET: Okay. So that is a suggestion. Is that a practical suggestion?

DR. SULLIVAN: If we can look at that, I would certainly like to do that.

3 MR. PLESSET: L2-5.

DR. CATTON: I think with L2-5, there was some interest in that it has the new thermocouples, and --

MR. PLESSET: That is right, that is --

DR. CATTON: -- and being assured that it was close enough to a previous test to make some sense out of it.

MR. PLESSET: I think that was the hope, Harold, that this one you will have some in-core -- you will have some internal thermocouples, and so that would be a very interesting test.

Hopefully it will give a comparison with the one that was [illegible] an old test where all the thermocouples were external, right?

DR. ACOSTA: But Mr. Chairman, the worry is that it will still be inconclusive.

MR. PLESSET: Oh, yes. I appreciate that. We are aware of that.

DR. ACOSTA: So that they should be aware of that.

MR. PLESSET: Right. Harold doesn't agree that it is totally inconclusive.

DR. WU: Some conclusions could be drawn.

DR. ZUDANS: Where are these thermocouples located with respect to axial direction of core, at different -- several locations or just one location?

MR. SULLIVAN: Yes, they are at several locations.

DR. ZUDANS: Several locations.

MR. PLESSET: Several heights.

DR. ZUDANS: So you may be able to have a better observation of differences between in-core -- in and out thermocouple effect in the lower portions of the core, where the cross-flow is less, right? So you might be able to draw a pretty good picture, I think. If there are several axially.

MR. PLESSET: You mean longitudinal or axial?

DR. ZUDANS: Axial.

MR. PLESSET: That is longitudinal.

DR. ZUDANS: You call them longitudinal?

MR. PLESSET: Let us call it longitudinal.

DR. WU: And how about the lateral distance. Are you going to have some -- a variety of distances, or is there a limit?

MR. PLESSET: Limited. It is one bundle.

DR. WU: Yeah, bundle, or --

MR. PLESSET: Just one bundle. It is just one bundle.

DR. SULLIVAN: Yes, that is correct.

MR. PLESSET: So that that is fixed.

DR. ZUDANS: That bundle is not in the center of the core, huh?

DR. LANDRY: The bundle that they were talking about is what we were designating the F1 bundle. That is a central bundle that will be put in -- if we go by this test matrix -- after L5-1. That will be next October through December. That bundle is the first prepressurized bundle.

DR. ZUDANS: And what was the objection of putting it, the F1, earlier? Was it the burnup that you didn't want to get high enough, that is why, or is it that the bundle is not ready?

DR. LANDRY: The bundle has not been prepared. It is -- well, it is in the process of being constructed and assembled right now. It takes about two to three years to build a fuel assembly, and this bundle is right now in preparation or in assembly.

DR. ZUDANS: But it could be made available so that L5-1 could be run with this new bundle, huh?

DR. LANDRY: If we put L5-1 after December.

DR. ZUDANS: Oh.

DR. LANDRY: This fuel assembly will not be ready to be installed probably much before the middle of October.

DR. ZUDANS: The samples that you showed us, the other sample with springlike piece, what is that?

DR. LANDRY: That spring was just a piece to hold the cladding sample in the plastic while the polymer hardened. That is not a part of the fuel.

DR. ZUDANS: Ah-hah.

DR. LANDRY: That was just something to physically hold the sample in place.

DR. ZUDANS: Okay. Now we understand what --

MR. PLESSET: Well, let us go to the next page, then. I think that that one is pretty much --

The next one was LA-10. My feeling was that I would say that this should be scratched. Let me see. I don't want to override anybody else.

DR. ACOSTA: It is hard to understand the nuclear necessity of this, Harold, because this is a purely hydrodynamic problem. Nuclearness in this case is an overall integral measure of the degree of mixing, but it does not get you at the hydrodynamics, so it may be that you could get one answer for an entirely different wrong reason in such a test unless there were internal flow details actually measured. That would require a different kind of a test, it would seem to me.

MR. PLESSET: Is there anybody who doesn't agree that this could be very nicely deleted?

DR. CATTON: Which one are we talking about?

MR. PLESSET: We are talking about LA-10.

DR. CATTON: Oh. Oh, we are on page two.

MR. PLESSET: Yes, we finished with page 1. There is not a lot of latitude there anyway.

DR. CATTON: Yes, I would agree that --

MR. PLESSET: Okay. Anybody -- anybody who is --

wants to defend it?

DR. ZUDANS: Well, I would like just to clarify, to see a clarification. Didn't you say that this is the one that NRR is very strong -- has strong feelings about?

DR. LANDRY: Right, this test is one that NRR has requested. Earlier this morning, when I said that these first tests on this page were questionable as to the dates, one thing I should have pointed out is that in the discussions we have been having with Idaho, we have asked that this test, LA-10, be looked at as a piggyback to another test, not piggyback in the sense that we normally do, on the back end, but piggyback it to the front end of a test.

Since we are going from cold shutdown, we could use this, if it does not violate the tech specs, as a means of starting up the plant to lead into a test that is already scheduled, and that way, this would really be a free test.

What I mean is, instead of normally, like we normally start up the plant, we pull the rods, then deborate to go to power. We looked at this as possibly a way to start up the plant by leaving the rods inserted, deborating to criticality, and then going to the normal startup mode to bring the plant up, and that way it would be piggyback to another test. It would not take up time, a time slot for a test, and would be essentially free. It would be just another means of starting up the plant.

DR. CATTON: I don't think anybody would object to that.

MR. PLESSET: I don't think so. While it wouldn't cost anything test-wise, I can see it costing an inordinate amount of money to figure out what happened.

DR. SULLIVAN: We are concerned about the same thing that you have been concerned about. The overriding factors had been the licensing issue that is at stake, the fact that there is a problem with plants right now, and that this is a test that we could conduct in a relatively large-scale facility, and get some data.

MR. PLESSET: Yeah.

DR. ZUDANS: But could you under even the wildest imagination transfer the results from this to a real plant?

MR. PLESSET: Well, I think that they might get something out of it. That is their hope, and since they think it is not going to cost anything very much in time or -- see, one thing I think we have to be interested in is things that prolong the proposed shutdown date, so that this would -- this test might replace something that would be otherwise more important, and apparently it won't, so I don't think we would get too excited about it, if Research concurs with NRR and seems to be a reasonable thing.

DR. SULLIVAN: There are some constraints that you ought to know about on the test matrix as you go through it. We thought that the last test, which is the pressurized bundle, that test was very important to us.

MR. PLESSET: Which one is that?

DR. SULLIVAN: The very last test on the page.

DR. CATTON: L2-6?

DR. ZUDANS: L2-6.

DR. SULLIVAN: Yes.

MR. PLESSET: Yes?

DR. SULLIVAN: That was the last test in the matrix, and it was very important to us that we do that test, and it not only comes from our concerns, it is the fuel people's concern, and there is an international concern in the same issue.

The international fuels representatives have also highly suggested that we run that test, so it is well supported.

One of the problems that we faced is that that pressurized bundle will not be available much before the date of insertion that you see here.

MR. PLESSET: Which runs it beyond FY '82.

DR. SULLIVAN: So it brings it past FY '82.

MR. PLESSET: But it won't be ready before that, or --

DR. SULLIVAN: Not very much before that.

MR. PLESSET: That is a problem.

DR. SULLIVAN: Yes, that is a problem.

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MR. PLESSET: Well, let us come back to that.

DR. ZUDANS: I don't understand why, now -- here it says in May of '82 you have it, on this table.

MR. PLESSET: No, this is F2. This is a new core. This is a different core.

DR. ZUDANS: Oh, I am sorry.

DR. SULLIVAN: It is a new bundle.

MR. PLESSET: It is a different core.

And what is the other differences between this F2 and the F1?

DR. SULLIVAN: Is that one of them is -- the concern was that we get two points on this curve that he was showing me, that one we get it right before we strain, or you go to burst, and then we wanted to have one that went to burst.

MR. PLESSET: And you wanted a fresh core for that, is that it?

DR. SULLIVAN: Yes, we needed a new bundle.

MR. PLESSET: So you want much that doesn't have much fission product development?

DR. SULLIVAN: Right.

MR. PLESSET: Because of the contamination, okay.

DR. SULLIVAN: And it is at another pressure also.

MR. PLESSET: Oh, is it at a different pressurization?

DR. SULLIVAN: Excuse me?

MR. PLESSET: Is it at a different pressure?

DR. SULLIVAN: I thought it was. He just said maybe.

MR. PLESSET: Oh.

DR. CHEN: It is 600 psi.

DR. LANDRY: This morning, when I was saying that I had this noted at 600 psi, but that was under discussion right now. Idaho has come in and said that they would recommend we run this test with the bundle at 350 psi, instead of 600. The reasoning behind that is that we will be running with a fresh bundle at approximately 1600 kilowatts per foot (sic) under your heat generation rate.

We will be running with -- if we run it at 600 psi, we will be running with what is essentially end-of-life pressurization.

Now, an operating plant does not operate at 1600 kilowatts -- at 16 kilowatts per foot at 600 psi. It operates on the order of six or six and a half kilowatts per foot end-of-life.

The feeling of our people in Idaho is that we would be very unrepresentative if we ran this test at 600 psi, in other words at EOL pressure, with beginning of life or even higher than beginning-of-life linear heat generation rate.

MR. PLESSET: Yes. Okay, go ahead.

DR. LANDRY: Now, if we look at the FRAP prediction, that curve that I put up earlier, that is not in the handout, of the response of zircalloy to temperature and pressure difference, the point at which burst occurs between 350 and 600 psi internal pressure is only about 50 degrees Kelvin different.

Now, both pressures are in the -- or both temperatures are in the 1,000 to 1,100 degree Kelvin range, which is getting into the superplastic range for zircalloy, so they feel that by delaying the burst, using 350 psi instead of 600, we would delay the burst enough in time that we would be sure to go through the alpha-beta transition region for zircalloy, and therefore we would be much more typical of the response of cladding in a large machine.

MR. PLESSET: Well, why do you have an L2-5 and an L2-6? I mean, you have a fresh core, pressurized bundle, in F1, and you are going to run a 200 percent cold leg break, and that is the L2-5.

DR. LANDRY: Correct.

MR. PLESSET: What is the L2-6, then?

DR. LANDRY: That will be the same test, except we will carry it to the point of burst of the core.

MR. PLESSET: Well, I don't see why you couldn't do it right after L2-5. I mean, I don't see anything in between that is significant.

DR. LANDRY: One reason is, we feel a little squeamish about going into that range of bursting fuel without having some feeling for the response of the core pre-burst. By running L2-5 to the point where we may balloon, and we are only saying we may balloon. We are not predicting severe ballooning under those conditions.

That will give us a feel for the way the cladding and the fuel will respond before we go to the point of burst. We would like to have a little bit better feel for the way that fuel is going to respond before we start breaking it.

MR. PLESSET: So you want to run two separate tests.

DR. LANDRY: Right, there will be some differences between the two tests.

MR. PLESSET: Yes.

DR. LANDRY: But the L2-5 will give us a base from which to do the L2-6 test.

MR. PLESSET: But my point is that this could follow immediately on L2-5 without a change of core.

MR. WARD: No, they need months to analyze the first one.

MR. PLESSET: They do?

DR. LANDRY: We do want to analyze L2-5, yes.

MR. PLESSET: You want to analyze, I see.

DR. LANDRY: Plus if we balloon the fuel in L2-5, then the response for L2-6 will not be correct. The cladding will [illegible] -- may be -- I am not a materials expert, but we my end up cold working or annealing out the cladding, taking it to ballooning, then quenching it, and then re-taking it through the transition region, and bursting it.

DR. SULLIVAN: There is also one other important point, is that we need to run that test that is not too severe, because we also need that comparison of external-internal thermocouples.

MR. PLESSET: Right. Okay, well, I see what your problem is, but aside from the L2-6, I don't see any tests on the second page that I would feel you needed. Does anybody see a test on the second page?

DR. CATTON: We don't need that LA-2, the intermediate break, I don't think. It depends what you get on the first intermediate break that they run.

MR. PLESSET: Talk into the mike.

DR. CATTON: The LA-2 test may be needed. They have L5-1, which is an intermediate break, and I think depending on what happens with it, they may want LA-2 as well, but other than that, I agree.

MR. PLESSET: So that what you are suggesting is that there might be some point to the LA-2?

DR. CATTON: Yes.

MR. PLESSET: Anybody else want to plea for any one of the other items on this second page?

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DR. CATTON: I would like to just make a comment on this second page. I have sort of argued against all four, but I am doing it pri--rily based on ignorance, because I have not seen any good justification, so it is a preconceived notion that I have, that these four tests are not worth running.

MR. PLESSET: Well, which four are you talking about?

DR. CATTON: The first four on the second page, the LA-10, L9-3, LA-3, LA-9.

MR. PLESSET: Well, I thought that it was fairly clear to me that this boron dilution test, that that just doesn't fit.

DR. CATTON: That is right.

MR. PLESSET: OKay.

DR. ACOSTA: They get that for free.

DR. CATTON: Well, if they get it for free, it doesn't matter, so we are talking about three tests.

MR. PLESSET: All right, so we will let that -- but the ATWS test I think just doesn't make sense.

DR. CATTON: That is right.

MR. PLESSET: Okay.

DR. CATTON: But if the NRR feels very strongly that it does make sense, gee, I think I would like to hear why, before I continue to say no.

MR. PLESSET: Well, but by the time you find that out, it will have already been decided.

DR. CATTON: If that is the case, then I think it should be no now.

MR. PLESSET: I think that on the basis of what you know now, you have to make some decision.

DR. CATTON: And the decision I would come to now would be the same as the one I came to on the LOFT Special Review Group.

MR. PLESSET: Charlie?

MR. MATHIS: Well, I have got some question on this L9-3. Now, I don't have any quarrel with an ATWS test, but I think the test should be somewhat realistic in its approach, and this particular one where you say loss of feedwater, and then all of the rods failed to go in on a PWR, --

MR. PLESSET: You don't like it.

MR. MATHIS: Well, I don't like it, no. I mean, you are relying on gravity for your rod scram, and I think the odds of that being a real case are kind of remote. Now, maybe there isn't a better way, but my only point is that I don't think that a very realistic kind of setup.

MR. PLESSET: Well, I think -- well, I wouldn't want to disagree with that. My feeling about the ATWS tests is that the conditions are so atypical that if you do get something out of it you don't expect, then it may not mean anything anyway for a full-sized power plant. We decided that in our discussion this morning, and nobody convinced us otherwise, at least not that I heard.

I think the ATWS test, there has to be some very convincing discussion before they should be included. Now, the boron dilution, while you told us that didn't cost anything, so how can one quibble with that. I mean -- now --

MR. WARD : How much will it cost, by the way?

MR. PLESSET: He said nothing.

MR. WARD: Well --

DR. SULLIVAN: We are probably overstating the case.

MR. PLESSET: What is that?

DR. SULLIVAN: We probably overstated the case.

MR. PLESSET: Yes, all right.

MR. WARD: I am afraid they did.

DR. ZUDANS: I would like to talk a little bit more about LA-9.

MR. PLESSET: Well, before we get to that, Zudans, let us --

DR. ZUDANS: Oh, I thought you were ready to go.

MR. PLESSET: Can anybody really make -- aside from what we have heard from the table over there -- a convincing case about ATWS tests? What was the Review Group's --

DR. CATTON: At the Review Group, there were strong arguments against bothering with an ATWS in LOFT, and the only pro position was by the representative from licensing who was there. Nobody else felt that the test was worthwhile, and --

DR. SULLIVAN: I don't know what happened in the LOFT Review Group in the final stages, but it did come out as one of the high priority tests.

MR. MATHIS: By whom?

DR. SULLIVAN: By the writers of the --

MR. PLESSET: By the writers, yes.

DR. CATTON: That is L9-3 and LA-3, ATWS induced by loss of feedwater. Somehow the line between the categories slipped.

MR. PLESSET: Well, let me be a little bit arbitrary, and let me say that I think that I would be concerned as to how NRR would use the results of that test, to be honest with you. I think that they might misuse these results if they did anything with them.

You see what my concern is, it is such an atypical --

DR. CATTON: That is correct.

MR. PLESSET: -- and if they take these things too literally, which they are likely to do, you would have more trouble than you really need.

DR. CATTON: Well, further, I think that the power ascension rate is lower with LOFT than it would be for the full scale, so it wouldn't even be on the right side of things.

MR. PLESSET: I can see a lot of outside people jumping on that, and saying oh, well, this whole ATWS thing is just not a problem.

DR. ZUDANS: But there is one argument for, namely you don't have any other tests at all, with that kind of a condition, and it is interesting to know whether you can get out of it, whatever the comparison.

DR. CATTON: Well, but if you can get out of it on LOFT doesn't mean you can on a full-sized PWR.

DR. ZUDANS: Is that then a positive conclusion or negative?

MR. PLESSET: Well, I think that ATWS has been discussed for ten years that I know of.

DR. CATTON: Twelve, I thought.

MR. PLESSET: Twelve, pardon me, I didn't mean to understate things, and I thought it had been analyzed from that well-known place to breakfast, and that what more is there? I mean, and actually, mitigation features are already decided on.

DR. SULLIVAN: There are some problems with the ATWS calculations.

MR. PLESSET: Sure.

DR. SULLIVAN: And if you look at the pressures that they go to, there is a wide range of pressures that each of the vendors are calculating, okay, so I would say that some code assessment is in order.

The reason that we chose this one was it is one of the worst ATWS cases.

DR. CATTON: But Harold, the problem is the flow through the relief valves and the safeties.

DR. SULLIVAN: Yes, and I --

DR. CATTON: And I think that that needs, again, a separate effects kind of study before you go to LOFT.

MR. PLESSET: With full-sized valves.

DR. CATTON: Full-sized valves, full pressures, the whole business, so that we know what the valve is doing, and then you can ask yourself if you can predict the phenomena correctly or not, and I don't see that piece in the structure of things, and as long as that piece is not there, under no circumstances could I personally recommend that LOFT go ahead with an ATWS test. I don't see that kind of backup coming from anywhere.

Further, I think if you could predict the flow through the valves, the -- a lot of the thermal hydraulic problem would go away.

MR. PLESSET: Harold, I have a much more pleasant suggestion, that you go to Yokohama, Isogo Laboratory, where they are testing full-sized valves, [illegible]l them you are coming, and you want to see the results of some of those tests. That will take care of it.

DR. CATTON: Can we all go?

MR. PLESSET: No, no. Harold will go. In Yokohama. It is right near Tokyo, so don't get upset, it is not -- I think he has gotten the flavor of our feeling regarding ATWS, but --

DR. CATTON: Well, I don't hear any counter-arguments, so --

MR. PLESSET: Well, he did give us some counter. He talked about analysis, and --

DR. SULLIVAN: Again, I don't think that the test is sufficient, but I think it is one of the necessary ones to get, and I believe that it will be valuable in the future that we have that, and it is certainly a place to start.

All the ATWS codes that I know have zero assessment, because there isn't any data, and I feel like that this is a step in that direction, is getting some data.

Now, the next one, the LA-3, which is the contingency test is -- and I think you have got a good point, that we should look very carefully at that test, and decide if it is truly worth running a second, but I think I would support running the first one.

It is the worst case. It -- we need the data, I believe, for code assessment, and --

DR. CATTON: Which test is this, now?

DR. SULLIVAN: It is the one right before it, the L9-3.

DR. CATTON: There is also -- there is a disconnect. To me, code assessment should be done by the peoples in RES to have the charter to do code assessment, and they maintain that there is only one more test they need in LOFT. Now, what is wrong? This question was put specifically to them by the LOFT Special Review GRoup and by the ECCS Subcommittee in Albuquerque, and you were there when your chief of code assessment said that he didn't need it.

DR. SULLIVAN: I believe that if you look back through the experiments that he said that he needed, there were some others in that matrix.

DR. CATTON: Am I right in that recollection? I think.

MR. PLESSET: Well, I really don't know. We could check that out.

DR. CATTON: There was one thing that the people did want, and they wanted another double-ended guillotine break that was different than the ones that had been run in the past.

MR. PLESSET: Well, they are going to have two, I gather, if this thing --

DR. CATTON: Well, we have got a problem, really.

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You want to straighten out the external thermocouple question, yet the code assessors want a test that is different than the ones that have been run before, and I don't see that properly worked into here either.

DR. SULLIVAN: They are different.

MR. PLESSET: You mean the L2-5 and the L2-6, or what are we talking about.

DR. SULLIVAN: Yes. They are different from each other.

MR. PLESSET: Yes.

DR. SULLIVAN: And they are also different from what we have run before. So the only concern I think that we had with some of the questions that you asked, was that you wanted one alike.

DR. CATTON: Yeah. One alike and one different, and that is two.

DR. SULLIVAN: Yeah. Both of ours are different.

DR. ZUDANS: May I ask in the same connection, when I look at L2-5 and L2-6 and recall what you said as a reason for having L2-6, is that mainly to assure yourself there is an adequate difference between the failure margins of pressurized fuel and nonpressurized? In other words, would you say that if you used the same F1 bundle in L6 (L2-6), that the margin of failure between the pressurized bundle and nonpressurized might be too small?

MR. PLESSET: No, well, I think that they had another point, Zudans.

DR. ZUDANS: Well, I am not quite finished with my point.

MR. PLESSET: Yes. Oh, go ahead.

DR. ZUDANS: The point is this, if the margin in L2-6 case of F2 package, between failure of pressurized bundle and nonpressurized is not large enough, then you don't really know for sure that that is the one that will bust, and the other rods, that have a lot more burnup may bust, and you get the fission products anyway.

MR. PLESSET: Both L2-5 and L2-6 are with a pressurized bundle.

DR. ZUDANS: Yes, but the argument of not using the same F1 bundle in L2-6 was that that would be burned up more, and might have gone through cycling.

MR. PLESSET: No, more I thought it was the fact that they might get some clad swell in L2-5 and there would be some annealing, and so on, so that it wouldn't be a proper initiation of a test which would maybe go to rupture, and they had me somewhat convinced. I think that was --

Yes, Charlie.

MR. MATHIS: Well, the way I read this L2-6, it would rupture, and then you would defeat your purpose --

MR. PLESSET: Well, presumably it might, yes.

MR. MATHIS: -- of not contaminating the unit.

MR. PLESSET: Well, but the fuel would be fresh.

MR. MATHIS: Well, very little burnup, but it doesn't take much to cause a real mess to clean up.

DR. LANDRY: If we go into rupture, there will be contamination in the facility, but by using fuel that has not been irradiated for a long period of time, the contamination will not be high in fission products, but what we will have is washout of the UO2 of the primary contaminant.

DR. ZUDANZ: But the question I ask was, do you know what the margin is between failing this F2 or failing something that was there a long time sitting? That is -- you are looking for assurance that if you fail, you fail the F2 and not the other, but do you know what the margin is, how much one can take? You showed in the slide how much the F2 will take, right, how close you come to the failure curve, but do you know what the other fuel elements look like? They may fail before, and then all your hopes are washed out.

That is the question.

DR. LANDRY: I am not really following you. Are you talking about failing the bundle with L2-5, versus failing the bundle with L2-6?

DR. ZUDANS: No, no.

MR. WARD: No, in L2-6, how do you know that the central bundle that you have replaced is the one that is going to fail?

DR. LANDRY: Because that is going to be the only one that is prepressurized?

DR. ZUDANS: But if the other bundle is sitting there for a long time, you don't know that --

MR. PLESSET: No, it is replaced. The pressurized bundle is replaced.

DR. LANDRY: If there is a failure of unpressurized fuel, it is not in a ballooning and rupture. It is collapse on the pellet.

MR. PLESSET: But isn't it correct that the pressurized bundle in F1 will be replaced by new pressurized bundle?

DR. LANDRY: Yes, correct.

DR. ZUDANS: Okay, but the other fuel is not --

MR. PLESSET: That is not --

DR. LANDRY: We are only talking about the central bundle.

MR. PLESSET: That is right.

MR. MATHIS: But you are assuming that nothing is going to happen to the others.

DR. ZUDANS: That is the question. What basis for that?

DR. LANDRY: The experience with the other bundles to date has been that the temperature excursion which they experience has been much smaller than the central bundle in the L2-2 and L2-3 experiments and in the L3 series experiments which we have run.

The peripheral bundles have not experienced a temperature excursion which would cause failure of the cladding.

MR. WARD: Okay. What is that ratio approximately, do you have -- ten percent, 20 percent?

DR. LANDRY: I can't remember the exact numbers, because we did not spend a great deal of time after those two experiments examining and comparing directly the temperatures which the central bundle versus the peripheral bundles experienced, but the peripherals did experience a much lower temperature excursion than the central bundle.

DR. ZUDANS: But you know, they have been in there many more times, and that causes them to accumulate some kind of a damage, and the question I am really asking is whether the margin of failure between the peripheral ones and the new central bundle is large enough for you to be able to assume that the central bundle will fail. That is the question.

DR. LANDRY: Well, the central bundle will be pressurized, so yes, we do --

DR. ZUDANS: But why don't you answer my question? Do you think the margin is large enough or not? I know it is larger.

DR. LANDRY: Yes. Yes.

MR. PLESSET: The answer to that is yes. I think let us leave it at that.

DR. SULLIVAN: We don't know what that margin is, right, I am going to say -- and it is not that the program doesn't know it. It is that we don't know it.

DR. ZUDANS: But the program knows it?

DR. SULLIVAN: The program knows it. Somebody in Idaho knows it.

DR. ZUDANS: Are these bundles inspected before the test?

DR. LANDRY: The peripherals have been inspected following L2-3.

DR. ZUDANS: But will they be inspected --

DR. LANDRY: No, excuse me, following L2-2, after we pulled the central bundle. They were not individually removed and inspected, but they were inspected by TV camera or fluoroscope. They were visually inspected on the peripheral rods, not down within the bundle itself, but the peripherals of the peripheral assemblies were inspected visually.

DR. ZUDANS: And does the test L2-6 call for inspection prior to the test?

DR. LANDRY: We always requalify the core, the whole plant, after each test. The level of requalification depends on the severity of the test. After L2-5, we will undoubtedly do a more detailed requalification of the plant.

To date, with the less severe test, we qualified the core by simply doing reactivity measurements, reactor physics parameter measurements.

MR. PLESSET: Well, let us assume that the answer to that question of concern is that they need not worry about other fuel bundles failing in a significant way. I am not too worried about it. If they do, well, that is a way to shut down the facility. That is what we are concerned about.

DR. LANDRY: We plan to shut down the facility after L2-6 anyway.

MR. PLESSET: Yes.

DR. LANDRY: But yes, we do feel comfortable in the margin between the failure or the -- between the central bundle and the peripheral bundles.

MR. PLESSET: Well, I think you can see that there is not any tremendous enthusiasm, at least that I detect -- well, I would say for the ATWS tests, both of them, nor in my mind, that is one we haven't mentioned yet, the LA-9. I am trying to get us to a point of conclusion here. What was the point of that test? Is there a point? What does the Review Group have to say about it?

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DR. ACOSTA: We wonder, the same question.

MR. PLESSET: Oh.

DR. ACOSTA: Precisely what would be learned?

DR. CATTON: In the review group, I think there was a small group of us who felt that small break tests ought not be run in LOFT at all, because of the atypicalities. Unless you could point to a phenomenon, and I don't see any phenomena with LA-9, so in my opinion, LA-9 should not be run.

I don't see a particular phenomena that it is directed to.

MR. PLESSET: All right, let us go to the other table. Gentlemen, tell us why that was included.

DR. SULLIVAN: In the original test matrix --

MR. PLESSET: It was there. It has been there for a long time.

DR. SULLIVAN: Well, it was in the original test matrix that we proposed as a contingency test.

MR. PLESSET: As what?

DR. SULLIVAN: As a contingency test.

MR. PLESSET: Oh. Oh, so you weren't terribly enthusiastic either.

DR. SULLIVAN: Okay, and the reason that we felt that way was that we had not completed all of the analysis of the small break data that we had. We had not completed the verification or the code assessment process either, so we put in that test in case the assessment came up.

Now, as part of the NRR comments, it is now that small break is now the pumps running case, and we discussed this morning about why licensing .. t that it was required.

I also agree that it, you know, in ter.s of the licensing issue, that it would add some more data for them to look at. Now, I also, looking at it in a research mode, that is a required problem, the previous pumps running case, and I would like to see how well the vendor codes did before I would make a judgment.

If they don't predict it well at all, I would say that we probably should run another one, it should be different, and they should have to predict it.

MR. PLESSET: When does that -- when is that comparison o be made?

DR. SULLIVAN: We are just getting the data. We have gotten the data from two vendors, and in paper form, and we are now trying --

MR. PLESSET: Which tests are they trying to describe?

DR. SULLIVAN: L3-6, which is not on here, it is one of the pump running cases. It has already been run. In fact, it was the one that the LOFT Review Group saw.

MR. PLESSET: Yes.

DR. ZUDANS: In the -- Ralph, when you made the presentation on this test, you added that after six small break test runs, certain deficiencies were found that -- and that afterwards you had to change the LOFT pump characterization, and that this test was needed to confirm that the characterization really, the ne characterization really was such that it wasn't break size dependent.

In other words, you could analyze with the same characterization different sized breaks, and that was indicated as the reason for this test. Now, is that a correct understanding?

DR. LANDRY: That is -- not that we have made t. e pump characteristics dependent n the break, but that we have not inadvertently made e codes de t upon the break.

DR. ZUDANS: N. , how would this one additional test --

DR. LANDRY: By running this test similar to the L3-6 test, but with a different size break orifice, and then predicting this test, we would have a better feel for if we have retained the independence of the computer codes, independence of the break size.

DR. ZUDANS: But you had six other tests that you could compare, right?

DR. LANDRY: Well, but those were under a different configuration. The other small break tests were all run off of the broken loop side of the LOFT facility. L3-5 and L3-6 were run off of the intact loop side, off of the intact loop cold leg.

This test would be run off of the intact loop cold leg also.

DR. ZUDANS: But with a different size.

DR. LANDRY: But with a different size, to make sure we have not made the codes dependent upon the break size.

DR. ZUDANS: Well, my only observation is that I can't see how it will help you to decide that.

DR. SULLIVAN: Keith is going, when he makes his presentation, there is a number of changes that have been made to the codes, based on the previous test. What we would like -- those changes either have been made already, in the process of being made, or we are trying to evaluate whether we should make those changes.

After those changes are made, they will recalculate the original transient, and I would dare say that it would do reasonably well.

DR. ZUDANS: Well, if it did reasonably well on six previous transients, why do you need this extra one?

DR. SULLIVAN: Okay, the question that then you come up with is can this code predict a transient that we have not run yet.

MR. PLESSET: Which code is this code?

DR. SULLIVAN: It is probably going to be RELAP5.

DR. ZUDANS: I see, so the main thing is the blind prediction in RELAP5.

DR. SULLIVAN: And I would daresay this same thing is going to happen to the vendors' codes. That is, that they probably will not predict it well, they will post-predict it well, and the question is still going to be -- and we made a licensing decision, NRC made a licensing decision based on those predictions, and they are -- it adds more confidence that we made the right one.

One of the questions that is open right now is do they have enough time to manually trip the pumps, and we have given them a certain length of time. The question is, are the codes able to calculate those transients well enough to ensure that they should be given that amount of time, so it really is a licensing question and a code assessment question.

DR. WU: Just a very brief question. Has the RELAP5 been used to assess the previous six tests on the small break?

DR. SULLIVAN: Did they?

DR. WU: Yeah, did they.

DR. SULLIVAN: They did.

DR. WU: They did.

DR. SULLIVAN: And it was poor.

DR. WU: Well, then what would be the chance that this new one would be much better?

DR. SULLIVAN: . would say good.

MR. PLESSET: Well, they are learning.

Well, let me -- I think you can see that at best, there is some lack of enthusiasm for some of these, several of the tests on your second page.

Let me point out some other things that might affect it in any case. Now, the way the authorization is in Congress right now, this is all moot, as they say in the law, because it would be shut down at the end of FY '82, but what you are talking about here is running pretty well into FY '83, right?

DR. SULLIVAN: Mid-FY '83.

MR. PLESSET: Huh?

DR. SULLIVAN: Mid-FY '83.

MR. PLESSET: Well, it is 6-15-83, it is well into it.

DR. SULLIVAN: But that is based on us having to run all four contingency tests.

MR. PLESSET: I see, which isn't going to happen, most likely.

DR. SULLIVAN: Which we are going to try to run only two at the very most.

MR. PLESSET: Yes.

DR. SULLIVAN: And there is a possibility of running none of them. Our best estimate was that we would run two, the worst case is to run them all, which is the 6-15 number, and then if we ran none of them, I think we were back -- we had estimated it was at the end of FY '82 if we ran only -- we ran none of the contingency tests.

MR. PLESSET: But that would not allow the F2 bundle to be used.

DR. SULLIVAN: And that would be a real problem.

I think that we would have to do something which we don't understand right now.

The fuels people in both the U.S. and the foreign community are strongly supporting L2-6. It is the final verification that all of the separate effects, all of the blockage work. It is really, you know, that we can put it all together and it works.

It is the cladding swelling and rupture model, the fact of this coplanar/noncoplanar question is at stake, the degree that the subchannels are blocked, and the fact that we can accurately predict the time that rupture occurs, and the extent. Those are major questions that are going to be answered by that experiment.

MR. PLESSET: Yes, I can see that. I would be sympathetic to it, but the timing is difficult.

DR. SULLIVAN: Yes.

MR. PLESSET: You would need to go well into FY '83 in order to accomplish that, L2-6.

DR. SULLIVAN: Well, as you can see -- well, the date is, if you look under column A, it says target date, that is the date it would be run if we did only the two contingency tests.

MR. PLESSET: Which date?

DR. SULLIVAN: It is 11-12-82.

MR. PLESSET: I see. All right, fine.

MR. WARD: Which is in FY '83.

DR. SULLIVAN: Which is in FY '83, be into FY '83.

MR. PLESSET: Yes. Charlie?

MR. MATHIS: Harold, if the fuels people are really interested in this, do you anticipate that if everything goes well that they would go back and reconsider a change in Appendix K?

DR. SULLIVAN: There is a report that you guys have commented on at least once if not a couple of times. I think that that data would provide a strong input into that report.

So, it probably is going to make a big difference in how blockage is considered in LOCA transients.

MR. PLESSET: Well, I think we should move on. Any other comments?

DR. LIENHARD: On what?

MR. PLESSET: On the test program. Do you want to

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wrap it up?

DR. SULLIVAN: Could I just recap what I think I understand about this?

MR. PLESSET: All right, that would be good.

DR. SULLIVAN: We will re-look at L5-1, which is the first page, to see if that should be moved around compared to when we put the pressurized bundle in.

MR. PLESSET: Yes, good.

DR. SULLIVAN: You supported L2-5 with some reservations.

MR. PLESSET: Right.

DR. SULLIVAN: LA-10, you didn't support doing that, but it said that if we could add it on as a front-end test, that it was okay.

MR. PLESSET: It was worth the price you quoted.

DR. SULLIVAN: The L9-3, you did not support, and you said that we should re-look at that, and I wrote down that it needs further justification.

The LA-3 is a contingency test, and based on what you guys have told us, we would have a hard time convincing you that we ought to run it.

The LA-9, you had a lack of enthusiasm, but could see the philosophy --

MR. PLESSET: We could see the reason why it is there, right.

DR. SULLIVAN: Yeah. And I would take that as it is okay to leave it in.

MR. PLESSET: I would say so, yes.

DR. SULLIVAN: The LA-2 is -- there was some lack of enthusiasm, but you would leave it in. Then the L2-6, you supported.

MR. PLESSET: Right.

DR. SULLIVAN: So I would say that what we ought to --

MR. PLESSET: I would like to see it before the end of FY '82, that is all.

DR. SULLIVAN: Yes, right.

MR. PLESSET: So we would be sure to get it.

DR. SULLIVAN: Yes.

DR. ZUDANS: Chairman, could I ask one more question?

MR. PLESSET: Well, let us see if Harold is finished, though.

DR. SULLIVAN: I would suggest that what we do is, the NRR staff is -- certainly has an input into this. We will certainly take the action, to go back and look at the -- their are two questions that we have to answer. One is moving the experiment and this justification of L9-3, and also you would like to see under what circumstances that we would run LA-3, so we will take those as comments, and I would think that we would like to talk to you again sometime, if not as a group, as part of a group.

MR. PLESSET: I think that is very good, Harold. I am sure that the Subcommittee would be very glad to do that, and I think in the next very few weeks we will have a better understanding of how much time this facility is going to have. It might have a lot of time, it might be severely compressed, and the budget may be very skimpy or it may not be, so that I think we should plan to meet again, as you suggest.

DR. SULLIVAN: And we, in that meeting, we would present those two tests in a lot of detail to you, and let you try to see what we are trying to get out of them and why we are supporting them.

MR. PLESSET: Very good.

DR. ZUDANS: On LA-9, just to make sure where we stand, you said that the preprediction all six of these breaks was very poor, and that some model changes were made, and has the analysis of all previous six been done with the changed models?

MR. PLESSET: Zudans, if you don't mind, I would like to have that question discussed after we hear the presentation on the test.

DR. SULLIVAN: I think it will be answered in that presentation.

MR. PLESSET: So let us wait. We are going to have that very shortly.

DR. ZUDANS: Oh, that is just coming up?

MR. PLESSET: Yes.

DR. CATTON: If there is another presentation on these, I would like to hear why the six tests were not predicting well, and what a new test will tell us that will help us fix the code, because I think we already know in many respects what was wrong.

MR. PLESSET: I think we may get back to that after Condie's presentation.

What I am proposing now, if there is no further input to this subject, is that Dr. Karwat can give us some benefit of his attendance at this meeting . He is kind enough to come. Do you want to come up and -- why don't you come up here, or do you want to stay there? It is up to you. Tell us what your thoughts are.

DR. KARWAT: Mister Chairman, thank you very much for giving me the opportunity first to attend this meeting, and to give me an occasion to just make some observations as an unprepared observer for this particular topic.

MR. PLESSET: Yes, this was a spontaneous thing that develop in the past days.

DR. KARWAT: I realize that the working method of this group is really the same as we have it. You know that we have also a kind of review groups advising the Research Ministry on various proposals with respect to reactor safety research, but these are not the same review groups, or the members do not necessarily belong to the Reactor Safety Commission, which reports to the Federal Ministry of the Interior, which is responsible for licensing.

Here, I observe it is a little different. You are reporting to the NRC as well as to the Congress, and our review groups report only to the Ministry for Research and Technology, but the working principle is obviously exactly the same, the discussion of a lot of pros and cons for each individual proposal.

Now, let me just go into, immediately into the problem which was discussed here, my observations, for instance, one comment, perhaps, with respect to the discussion which went on just before on the merits of L2-6 and L2-5.

As Chairman for the CSNI working group on emergency core cooling, I think with also the necessity which was also already mentioned here, to have at least one more large break test which is challenging the codes, which is challenging also the fuel codes.

Whether it shall be two, as it was discussed here, L2-5 and L2-6, is something which probably can be discussed once we have an impression what came out from L2-5, so we are carefully observing what is going on here.

Now, let me just -- give me a few chances to make some general observations. Of particular interest for me was the discussion about the benefits of LOFT with respect to small break LOCA situations.

I think there are certain atypical features of the LOFT, but there are also typical feature of the LOFT, and one typical feature of the LOFT facility, from my feeling, is that we have to expect a typical systems interaction, which does not necessarily mean that the components are typical, but the fluid dynamic interaction which we could observe from this integral type of test in a nuclear environment to me seems to be of merit.

Also, there might be deficiencies in having everything measured, which we like to measure, and on the other hand, we always talk about replacing LOFT experiments by Semiscale tests, or making much benefit from Semiscale tests.

Certainly the possibilities to measure with the Semiscale are larger, because it is out-of-pile, and it is perhaps easier, it is cheaper to perform tests, but I see also there some untypical effects, which in particular with respect to small breaks might give some difficulties, and these are the heat losses, and for instance microscopic bypass effects, which are very difficult to be measured, and which on the other hand could impair the results of small break tests in particular, and so for me it is not solved which type of test is of more benefit.

I see advantages in both directions. I think we need both types, last not least, also from the point of view of scaling. We have the problem of extrapolating test results, whatever the scale is, into the full-sized system.

One two-thousandth is about Semiscale. One to 25 or 50 is about LOFT, and 1 to 1 we have to extrapolate from them. And here again, I see some problems with respect to application of codes. The codes, as we have them now, they require input data for known design features, and we know this. We know the length of the system, we know the diameter, the volume, and so on. We know about pumps' characteristics, and this is supposed to be an input as given for the facility, as well for the reactor, as well for the experimental system.

And we have some less-known features which we also have to input, and these less-known features are associated with the fluid dynamic process itself. We have to make use of flow charts. We have to make use of interfacial relationships, of heat transfer correlations, and so on. We could talk for a long time at this point, and I think that we have a certain chance of running into misinterpretations if we derive such recommendations for using these types of input into the codes, if we derive this type of input of information only from very small scale tests. I think personally, it is

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my personal opinion, these scaling-dependent, probably scaling-dependent correlations, we use correlations, empirical correlations, are more difficult to be extrapolated over three magnitudes of scaling than over two magnitudes or one and a half magnitudes of scaling, and this is again from my point of view, and I recommend in favor of a larger-scale small break LOCA test, whatever the facility is, just one reason to take it into account.

In this context, I am thinking on possibilities to do a very general, a new very general scaling study. We have certain ideas about how to scale experiments that are really primarily related with respect to holding volume to power constant in order to preserve the transients, which is certainly correct.

But I think it would be worthwhile for the nuclear community -- this can be done in European countries as well -- to perform a general study on scaling of transient two-phase flow experiments, and derivation of conclusions from such small-scale or intermediate-scale experiments.

This could be helpful, in particular in relation to future discussions, and I am sure there will be future discussions in two years again, on what are the appropriate scaled experiments, and how can we assure ourselves that extrapolation from smaller-scaled experiments is justified, and I think 1 to 1 experiments are prohibitive. Sometimes we have a chance to get one. I think TMI was one, but this is probably a very expensive, and of course not an envisaged type of procedure, to solve these problems, but TMI can show us a lot of things, and this just reminds me again of the question I still have with respect to behavior of fuel under abnormal conditions. You are now in the process of degraded core, establishing degraded core rules, and such things.

I think small break LOCA tests in particular should be -- you have now proposed to do, or recommended to do two at least, and I would like to draw the attention of everybody who is in charge of these two tests, that there could be an integral possibility if we would install at the LOFT site a very elaborated gas sampling system to study at least qualitatively the question of steam phase radiolysis. This could be done qualitatively in the first stage. One could take perhaps more benefit from these tests in also addressing this question.

I studied these questions in the last six months in detail, and for instance, I found that all the correlations which describe the oxidation of zirconium are to 99 percent performed out of pile, and I am wondering whether it is worthwhile to study possible interactions of the radiolytic decomposition of steam in a radiation field, and this has to be an appropriate radiation field, with an appropriate spectrum of radiation, to study this interaction with the chemistry of the zirconium oxidation.

And therefore, coming back again to the LOFT tests, I would recommend to think about the possibilities to make use of these two experiments also this direction, and even if it is in the first step in a qualitative way, I think more detailed investigations probably could also be done in the PBF facility, which was not addressed here at all, and I think combination of PBF experiments with LOFT experiments could be complementary and perhaps resolve some of the questions which can only be studied in LOFT in a very integral way, but not amenable to cubic measurements, perhaps it is more favorable also to think about PBF.

I think that is all that I can say at the moment, and I thank you very much for giving me this opportunity.

MR. PLESSET: Well, thank you, Dr. Karwat. Do any one of you want to ask Dr. Karwat a question? He is a little more sympathetic to LOFT, but then he doesn't have to pay any of it either.

DR. ZUDANS: Dr. Karwat, you said that one to one scale experiments are not feasible. I still feel that they are feasible unless you bring the facility to a state that you cannot recover from. You can do tests without damage, in particular separate effects, on a real power plant. It is maybe the question of instrumentation, so you don't need a Three Mile Island to solve your physical phenomena. Would you not agree with that?

DR. KARWAT: If I understood this at the moment, well, I think 1 to 1 scaled experiments have of course different meanings. In certain types of experiments, a 1 to 1 scale is already acheived if we for instance study the behavior of a fuel bundle with full lengths, but here we are addressing the 1 to 1 scale problem of integral experiments showing the interaction of a total fluid dynamic system, and this is certainly a different approach which has to be considered if we would like to arrive at a 1 to 1 scale of an experiment.

I think that 1 to 1 scale experiments, if they are really implying the whole process, they are not feasible, because then we cannot only start with doing 1 to 1 refueling experiments, we should then also start to do 1 to 1 blowdown experiments in a 1 to 1 scale, in order really to fill up the complete field, and this is not feasible.

With respect to what is going on in TMI, TMI was an accident, and this happened in full size, and what we are going now to do, or to try, is we want to make the most benefit of this accident in understanding it, and trying to understand it requires that we certainly have to leave the concept of best estimate -- of evaluation model type of prescriptions, because here we are faced with some part of reality, and if we conclude that something which we have to do in our analysis in order to end up with known facts, say, this is -- we use the conservative assumptions, and this conservatism overnight has become to realism, and then what is the margin of conservatism which is left? This is probably the problem.

I don't know whether I have at the moment addressed what I understood when I said --

DR. ZUDANS: Well, I guess you gave me more than the question. My question was rather simple. If you talk about 1 to 1 scale testing, I talk about actual nuclear power plants.

DR. KARWAT: Yes.

DR. ZUDANS: Now, I understand that a full blast blowdown is not feasible. I understand that no test is feasible or transient is feasible that creates a risk that you are not sure you can control.

DR. KARWAT: Yes.

DR. ZUDANS: But cannot you as two-phase experts devise all kinds of small transients that you could check individual effects.

DR. KARWAT: I think now I got the point. I think what we are obliged to do is, we have to demonstrate our capability to understand the fluid dynamic process, for instance, or all the implications with all other processes.

DR. ZUDANS: Right.

DR. KARWAT: And this has been at the moment done

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in the scale, roughly, we can say we have done this in 1 to 1000, if we remember all the years of experimental evidence we had in Semiscale.

But in order to make extrapolations, I need at least a second point in the scaling map, and what we are working on at the moment, and we are working on when we discuss this type of experiment in LOFT, we are working on acheiving this in a scale 1 to 50, and this is a first requirement, that we have two points.

If we have two points, we can draw a straight line and can say, okay, we have some reasonable feeling about extrapolation over the next magnitudes, and then with some fantasy, we could even say that it is not a straight line, but perhaps something of a curve, but at least we need two points which are reasonably apart from each other, then we can say we have good justification to extrapolate over the next magnitudes. That is what I have in mind.

MR. PLESSET: Well, I think this discussion is very helpful. Dr. Zudans has always been interested in getting operating power plants to do little experiments. More power to him. I mean, go talk to Southern California Edison.

DR. ZUDANS: Aren't you doing that in Germany already?

DR. KARWAT: If I may, might I add one thing?

MR. PLESSET: Yes.

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DR. KARWAT: I think certain experiments even can be performed in real plants, but they cover only a very small part of the two-phase flow questions if we are talking a moment about two-phase flow.

They could help us to conclude the picture, and fill up the picture, and give us a certainty that we for instance understand transient models, transient models which are also of importance if we talk about ATWS transients.

MR. PLESSET: Yes.

DR. KARWAT: Thank you.

MR. PLESSET: Yes, well thank you.

Let us take a short break, 10-minute break, because we have two topics yet to go into, so let us reconvene and try to conclude our proposed agenda in a reasonably consistent with the time schedule, so let us do that.

(Brief recess.)

MR. PLESSET: Let's reconvene, and we will now have Mr. Condie give us a presentation of the L3-6 results.

DR. CONDIE: Thank you, Mister Chairman.

I have prepared a somewhat lengthy presentation here today that is going to cover, or can at least cover these topics, but it would be up to the Committee if they want to go through them all real fast, or eliminate a certain amount of them.

The handout is much more detailed than the presentation, so I will be skipping through some slides, but first of all, I want to give you the results for the L3-1 L8-1 experiment that was performed, the last one we did in LOFT, the pumps on companion to the pumps off experiment. We will go through that in a little detail and have the movie to show the thermocouple response.

And then I have got some comparisons, the pumps on, pumps off, those two tests, L3-5 and L3-6, and summarize the conclusions that we reached from those two LOFT tests, and then I can go into the code calculations of the pumps on/ pumps off tests, both pre- and post-test predictions on both of those tests, and then some conclusions and I have just a slide or two for the -- to give you a preview of the LOFT L9-1/L3-3/L8-1 test that will be coming up in another couple of weeks.

Now, that is quite a bit, if we get bogged down in some discussion, although I have noplace to go myself.

MR. PLESSET: Well, how long do you think it would take?

DR. CONDIE: It would take about 45 minutes if I was not interrupted with any discussion.

MR. PLESSET: All right. That is all right. But if there is something you can leave out, feel free to do so, but your outline is fine.

DR. CONDIE: Okay. Just to bring you up to speed on the LOFT system, then, here is a schematic. I will remind you that the test L3-6 was a pumps on test with a break here in the intact cold leg, as opposed to the previous small break tests which had their breaks here in the cold leg broken loop.

L3-5 and L3-6 had equivalent to a four-inch LPWR broken in the cold leg.

The data I will be showing you has some uncertainties. I don't have them shown on every slide. There is a couple of slides here that so as you look at some of the data and get an idea of what our calculated uncertainties are.

Six degrees on temperature, about 32 psi on pressure, 10 pounds on density. Our differential temperature is a lot better with one degree, mass inventory, about 600 pounds, differential pressure, 0.3 psi, and quite a little discussion there on break flow uncertainty that I will leave to your perusal.

The initial conditions for both L3-5, 5A and L3-6 are essentially the same, 2154 and 2168 psia to start, with a cold leg temperature of about 545 degrees F, which gave us a delta T across the core of 32 to 35 degrees, a flow rate of 3.8×10^6 pounds per hour, and basically at 100 percent power on the plant, at 50 megawatts.

This slide now shows the pressure response of both the primary system and the secondary system for this experiment, and I will use it to kind of give you an

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introduction to what the test is like.

The test was initiated by scramming the plant about five seconds before time zero here, and once the plant scrammed and we knew that the rods were in their bottom location, then the break was opened because of the depressurization.

HPIS was allowed to come on as a function of pressure, although the accumulator was valved out of the system, so it doesn't come on until later.

Pressurizer emptied at about 20 seconds. We saturated the upper plenum here in about 50 seconds, and reached the saturation point, causing the primary pressure to level off.

The pressure continued to decay with a significant amount of heat loss to the steam generator. As you will note, the secondary pressure came up and the primary and secondary are very close together, but with the primary a little bit higher.

Then at about 1100 seconds, the primary pressure dropped below the secondary and decoupled from the secondary system. This is a rather uneventful time, just depressurizing. The target pressure for pump trip was approximately 300 psi, and we were carefully watching the inventory at that time.

The pressure continued to drop, such that at about

300 psi, the pumps were tripped, and then within less than about a minute, the thermocouples started to go up at almost an adiabatic rate, until within about 50 seconds, the maximum -- we had reached a point of about 600 degrees, at which time, the ECC system then was allowed to come on.

It was set to go on at 600 psi, but was valved out up to that time, so it was sitting there now with a 300 psi head on it. HPIS, both trains were allowed to come on and quench the core very rapidly, began to fill it up, and so here only a couple of minutes later the accumulator was shut off and so was the HPIS at its high flow rate.

Then the pressure started to go up a little bit.

We note that the very rapid increase in temperature as we shut the pumps off is shown on this next slide. This is -- overlaid here are several fluid temperatures in the core, as well as selected thermocouple temperatures, and indicating here through the -- clear up to the pump trip, the thermocouples showed nothing more than saturation.

And then, once the system voided, after the pumps were shut off, you can see the sharp increase, and this is typical of all hot thermocouples in the core.

This rate here is about -- the heatup rate is about four and a half degrees F per second, as compared to our calculation of the adiabatic heat-up rate of somewhere between 5 and 6 degrees F per second, so it didn't go up quite adiabatically.

MR. WARD: Could some of that be lag?

DR. CONDIE: Pardon?

MR. WARD: Is any of that just lag in the thermocouple, or is that --

DR. CONDIE: No, we don't think so.

MR. WARD: Okay.

DR. CONDIE: There was still cooling just a little bit, as the pumps were coasting down. The pump trip occurred, then as they were coasting down, the temperature started to take off, and they had reached their limit of 600, which was the time that we were going to initiate ECC, while the pumps were still coasting down, so there was just a little bit of cooling that was going on there.

Now, I am going to show you a movie here that will show all of the -- well, the thermocouples on some of the rods in the center assembly as a function of elevation. It will start out kind of slow here, and we will speed it up through this region, and then we will slow it back down again when we get over here near the time that we trip the pumps.

Shown also on that will be the saturation temperature so you can see where we are relative to that. It will take about five minutes here. Question?

DR. ZUDANS: When was this experiment terminated here? Right about at this time? When did you shut off the

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blowdown, the break, when was it closed?

DR. CONDIE: Oh. At the same time we turned the accumulators on, we isolated the break.

DR. ZUDANS: Where is that, right here --

DR. CONDIE: Right in here. It is all in real tight. As we go through this sequence, I will try to tell you some of the events that went on. Within about 200 seconds here, the accumulators went on, we reached the maximum temperature, the pumps went off, and I will try to kind of step you through it as we go.

MR. PLESSET: I think we should hold the movies until he can get the lights, so you might go on a bit if you have --

DR. ZUDANS: In the meantime, I will ask a question. Later on, I am sure you -- if you are going to tell about this, then say so. You got into a natural circulation or something after you isolated the break, or what did you do?

DR. CONDIE: Well, we isolated the break, shut the pumps off. Then the accumulators came on, and then we went back onto the -- the HPIS was in there, and then we just went onto a residual heat removal program, and the test was actually terminated at that time, so although about -- with very low HPIS on, and the residual heat removal system, the pressure did go up and about 2500 seconds after -- about 2000 seconds after the temperature excursion, we did go back into natural circulation, because the pressure had come up enough to get up above the secondary, but that -- I don't think we even show that. That wasn't part of the experiment.

Now, in the L3-5 experiment, we make a real effort to get back into natural circulation, to reestablish natural circulation after we close the break. I presented, in Idaho Falls, the results of the L3-5 experiment. Don't have too much detail here, except as they show up in the comparisons to the L3-6 stuff, I might point that out.

Any other questions?

DR. ZUDANS: The initial conditions for these two, L3-5 and L3-6, they are similar?

DR. CONDIE: Yeah, they tried to be exactly the same. They both had basically the same number of hours of operation prior to the scram so the decay curve was very, very close. The initial flow rates and temperatures were within a degree or two on the temperatures. The only -- the initial sequence was exactly the same, the only difference being that in L3-5, we shut the pumps off immediately on scram.

DR. ZUDANS: This break, was it big enough to remove the residual heat?

DR. CONDIE: Through the break?

DR. ZUDANS: Yes.

DR. CONDIE: Yes, this was the same size of break as L3-1, that is, two and a half percent, four inch equivalent

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rate. The steam generator does remove a significant amount of heat just because it is there, but it isn't essential in cooling the plant, as long as the break is open.

DR. ZUDANS: So, the break was big enough to remove the residual heat?

DR. CONDIE: Yes.

DR. SULLIVAN: Mr. Chairman?

MR. PLESSET: Yes.

DR. SULLIVAN: Dr. Karwat will be leaving us shortly, and I would just like to thank him also for coming.

You also mentioned that -- something about paying bills . The Germans are in the LOFT program, both financially, and support the program in terms of sending people to Idaho.

They have made a significant contribution to the LOFT program in both the use of their funding in the extension of the program, and also the people have made a contribution. They have also done a lot of analysis work in Germany that we really have appreciated.

MR. PLESSET: Well, I appreciate your mentioning that, and I want to have Dr. Karwat take that message back. They could make an even bigger contribution to the future program if they would cancel UPTF.

DR. KARWAT: That is not within my power.

MR. PLESSET: I know. What have you got to say to that, Harold. You didn't expect that, did you?

60 DR. SULLIVAN: Could I reserve comment?

MR. PLESSET: Yes, you can. Yes.

Well, thank you again, Dr. Karwat. We appreciated it, and look forward to seeing you again.

DR. KARWAT: Thank you.

DR. CONDIE: I can go on and show you a little comparisons, L3-5 and L3-6.

MR. PLESSET: Well, why don't you go on, and then we will come back to the movies. Why don't you get to the comparisons.

DR. CONDIE: I will show a series of slides here that show the comparison between the L3-5 and the L3-6 experiment, and start that out with pressure.

The pressure response in the two experiments was very similar, at least over the period of time to the time we reached 300 psi. L3-6 depressurized just a little faster here in this time frame and then flattened out while L3-5 was shaped the other way, but for all intents and purposes, very very little difference in the pressures.

The difference comes, though, here in the flow rate, or the break flow rate between the two. Initially, we show that L3-5 put out quite a little bit more flow out of the break, but that only lasted for the first 100 seconds or so, and then you will see two, line two, is the top line, and now that is the break flow for L3-6, number two, whereas number one is for L3-5.

You will see over this entire range of the experiment that there was much much more fluid left the system in L3-6, the pumps running case, than in was in L3-5. That resulted in an inventory in the primary system, shown in this slide, where you see L3-5, the final mass inventory, was something like 38 percent, 32 percent of the original on L3-5, and on L3-6, we lost all but 12 percent of the mass in the primary system, and that is what we see as the major difference between the two experiments, is that mass inventory.

MR. PLESSET: Let us see, two is --

DR. CONDIE: Two is L3-6, and one is L3-5.

DR. CHEN: What is the break flow uncertainty, 15 percent or ten percent?

DR. CONDIE: The break flow uncertainty is in the neighborhood of 15 percent. There is a slide in there, in fact, you have it, Dr. Plesset, that shows the integrated mass, the actual value with uncertainty on it, to show that it is well within the range -- well, the difference is outside the range of the uncertainty on there.

DR. ZUDANS: Look at this one a little bit. In both cases, the HPIS was initially on, and added some mass to the inventory, did it not?

DR. CONDIE: That is correct.

DR. ZUDANS: Was that addition the same in both cases or different? Do you have any measure on how much was added in one, and how much was added in the other?

DR. CONDIE: Well, this plot here integrates that and takes that into account. We take the initial inventory, subtract the break flow and add in the HPIS. The HPIS is -- the positive displacement is a function of pressure, and the pressure curves that you saw are fairly close. There is very little difference in the pressure, so the amount of fluid the HPIS added between the two was very little, but it is included here anyway.

DR. ZUDANS: And how are these curves generated? Is this a calculation of some kind of a measurement of collected blow-out fluid?

DR. CONDIE: Yes, we have -- from an integral base, all the fluid that leaves the system is collected in the blowdown suppression tank.

DR. ZUDANS: Okay.

DR. CONDIE: So from beginning to end we have a very good measure of the total mass inventory. This blowdown suppression tank inventory is determined by liquid level in the suppression tank, so during the transient itself, that isn't very accurate. We have drag disks and turbine meters downstream of the break itself, so we can determine the instantaneous flow rate from those, and using the two together, come up with the break flow and the system inventory, so after that quiescence after the experiment is over, we have a very good measurement on the final inventory.

DR. ZUDANS: Okay.

DR. CONDIE: This next slide is a slide of the mass inventory in the system, and I show it to show you a range of time or inventory here that which if we had stopped the experiment and collapsed all the liquid, where we would have uncovered the core.

This band includes the uncertainties, so we would expect that in this L3-6 experiment that somewhere after about ten minutes, if we had shut the pumps off, we would have collapsed the liquid to somewhere below the core, to such that by about 26 minutes, we would have completely uncovered the core.

But in this experiment, we did not, as you noticed, see any temperature excursions during that period of time, indicating, of course, that the core was being cooled, even though the total mass inventory was not sufficient to cover the core had it collapsed, but that the high void steam cooling was very efficient in removing the decay heat from the core, and as somewhat of a surprise to us, the pumps continued to pump the two-phase mixture much more efficiently than we had originally thought it would.

MR. PLESSET: Were the pumps operating fairly smoothly with the two-phase mixture?

DR. CONDIE: The pumps operated very smoothly. We saw some cavitation at about 35 percent void, and I will show you that in a minute. There was accelerometers on the pumps. We didn't see any excessive accelerations or problems with the pumps.

The bearings in the pumps are cooled by external primary coolant pump injection, and that was continued while the pumps were running, so that didn't depend on the high void mixture.

DR. ZUDANS: Do you have a curve like this for L3-5 as well?

DR. CONDIE: A movie?

DR. ZUDANS: No, no. This type of -- this figure.

DR. CONDIE: Well, L3-5 never did uncover, okay. L3-5 during that experiment --

DR. ZUDANS: This didn't uncover either. It is only a calculated uncover.

MR. PLESSET: Well, if the pumps had stopped and had collapsed, the core would have uncovered. The pumps were never running in L3-5, and it never uncovered, in that sense.

DR. ZUDANS: Well, but they reached also saturated state, and there was some fluid, there would be some collapse there. In other words, you don't have a graph like this?

DR. CONDIE: No, in L3-5, there was a definite

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collapse level after a period of time, after the initial blowdown, and when we were at saturation, L3-5 was nothing more than a pot of boiling water, and all the liquid had collected into the core, and at no time did it get -- it got within about a foot or two, maybe, of the top of the core, but other than that it was covered during the whole experiment and the intent of the experiment was not to uncover the core.

DR. ZUDANS: Okay, well that answers my question. Thank you.

DR. CONDIE: One of the reasons, or the primary reason, I guess, why the break flow was much higher in L3-6 than it was in L3-5, is shown on this plot where we have the density in the spool piece just upstream of the break, and you see that with the pumps running, it created a significantly higher density of fluid available for the break, and since it was choked during that time, the higher density fluid took out much much more mass than we did with this low density fluid in L3-5.

A lot of that has to do with the homogeneity of the fluid in the cold leg where the T to the break was located. In L3-5, that was stratified in there, so that only the vapor went out the T, and so it was originally much higher -- or a lower density fluid going out the break.

So this would have some implications as far as location of a break, a small break, just exactly where the break occurred, as to whether or not these would be typical densities upstream of that break.

This slide here shows what -- I will parenthesize this, the system, the average system void fraction in L3-6. A plot like this isn't available for L3-5 because we had a very definite level between the liquid and the vapor, but in L3-6, as I mentioned, we had a very homogeneous situation throughout the system, and the average void, we calculated it in two different ways.

The top curve, as shown here, was calculated from the density data. That is, we looked at all the densitometers, and kind of averaged them and weighted them, whereas if you took this bottom line, it was -- we just took the mass inventory and divided it by the volume and came up with a density.

We think that this top one is more accurate, the reason being is that there is a couple of places in the system that the liquid can collect, even though the flow paths are homogeneous, and that would be in the steam generator simulator, in the broken loop there is a piece of piping from there down to the isolation valve that would contain liquid and subtract from that inventory that would be voided, as well as some in the lower plenum, so you can see that into this experiment about half-way that we are up to almost a hundred percent void in the system, yet we are still cooling the core.

One thing that is very interesting is between the two, is the ability to determine an absolute liquid level, to know where you are. These plots here, I have got two, one for L3-5 and one for L3-6, show conductivity probes at elevations above the core. This is in the upper plenum. These first three are above the hot leg nozzle and the last couple or three are below the hot leg nozzle but still above the core.

You can see from these voltage readings that this top one, about 400 seconds, voided, the increase in voltage here to about 10 volts indicates to wetting of that probe, and see a very definite liquid level as time goes on, as it drops. As I say, there are all above the core, so we don't indicate anything, even though they all show voided, it is still above the core.

But take a look here at the L3-6 one now. You -- even though remembering the system void plot, in this region, out in here, we are basically between 95 and 100 percent void, but yet these thermoconductivity probes, none of them have indicated that they are totally dried out. They still are being some moisture on them, and there is no way to determine what a liquid level is, or what your inventory would be.

I'll talk a little bit here now about the pumps, as we didn't have a very good idea of the pump curve, or the pump performance curve for LOFT. They were put in the system a long time ago before we were interested in small breaks and nobody thought about pumps on and pumps off and they were -- we kind of scavengered them, so we haven't done any tests on these pumps, didn't know exactly what the two-phase degradation of those might be. This -- I am sorry, this is almost near the -- this slide is just about at the back of the -- I rearranged it a little after I looked through this.

But we have been using some Semiscale data as shown in number two here for analysis, and curve number one is some CE data, the EPRI CE pump data that you have heard a lot about. There is a lot of difference here.

These scattered data here are the data that we got from this experiment and have extracted after it was over, and these are fairly new. You can see the head, pump head as a function of void fraction here stayed fairly high, until about 35 or 40 percent void. Then it dropped down and stayed fairly constant until we were almost single phase, and then it went up. We have taken a little artistic liberty here to make sure it goes through the head ratio of one when it is single phase.

Two here is the Semiscale pumps. Notice the big drop-off in head, and that is the data that we had in the RELAP5 model for the experimental prediction, and that is why when we predicted these first, we didn't show a continual effect of the pumps circulating the fluid. We had predicted a situation very much like L3-5, in that once the pumps cavitated, right here, we basically had a liquid level occur in our prediction, and you can see why there because of this curve.

We have since, and I will show you some plots, have included the CE EPRI pump curve in our computer modelling, and I have got some results of that, and going from this curve to this curve, that significantly changed the prediction of the experiment. We are just in the process of including this curve right here into our model. We expect that that will improve the prediction much more. This is not really a cold problem. It is an input problem we have.

There is just a little data in here out of our large break test, too, just a few points as we got in a couple of seconds there as the L2-2 and L2-3 was in progress, the pumps were still running in those experiments.

DR. CATTON: Is there something universal about this curve now?

DR. CONDIE: Universal about this curve? No, this is a LOFT curve.

DR. CHEN: How about the real plant pump? Closer to what?

DR. CONDIE: Closer probably to the CE data, right here.

DR. CATTON: But you really don't know.

DR. CONDIE: I don't really know, no. That is one of the things that we are going to have to do in our typicality studies is get some actual pump curves and see how they actually effect --

DR. CATTON: Where are you going to get them?

DR. CONDIE: From the vendors, we hope.

DR. CATTON: For the LOFT pump?

DR. CONDIE: No, no, no, no. For other pumps, when we look at other pumps. We run -- we model quite a few other plants in our process of looking at -- after these experiments we will run this, like, on the Zion, for example, our computer code, RELAP5, to see the effect of this same transient on a large plant.

DR. CATTON: Who do you do this for? Is this done for Research or for NRR?

DR. CONDIE: It comes under the LOFT budget. Now, we don't do all of that. The code assessment people do quite a bit of it, too, and that comes --

DR. CATTON: You have code assessment people within LOFT?

DR. CONDIE: No, we share information. We are in the same area, and if the code assessment people have set up a Zion plant, for example, for some reason, we will just run that as a check on the scaling and typicality.

DR. CHEN: Would you say that the CE pump will cavitate much earlier, about 0.25?

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DR. CONDIE: The what?

DR. CHEN: The CE pump curve will cavitate much earlier than the LOFT cavitation will occur --

DR. CONDIE: With the CE pump data in here , we were still able to predict the homogeneous high void cooling that occurred in the experiment, as opposed to when we had the Semiscale pump curve in there. Once it cavitated, we settled the liquid out into the bottom of the reactor vessel, and then boiled it off, and that was the big difference between the experiments, so in summary --

DR. CATTON: Are those points measurements of the delta P across the pump?

DR. CONDIE: Pardon?

DR. CATTON: Are those data points that you had on the previous figure --

MR. PLESSET: Those are all data points, aren't they?

DR. CONDIE: Where, here?

DR. WU: The pump head measure.

DR. CONDIE: Yeah, delta P.

MR. WU: Delta P.

DR. CATTON: Across the pump?

DR. CONDIE: Yeah.

DR. WU: Across the pump how far away from the blade section?

MR. PLESSET: That is across the pump, isn't it?

DR. CONDIE: Oh, no, we don't have any delta P measurements directly across the pump.

MR. PLESSET: That is what you want, really.

DR. CONDIE: No, I take that back. We have delta P measurements right across the pump. It is the density that we don't have right close to the pump, so we have density upstream of the pump a little ways, and we have density downstream of the pump, oh, about that far, and so the uncertainty in this really comes in with the density that goes into these head curves, but the delta P is a good measurement across the pumps.

MR. PLESSET: That you have with some accuracy.

DR. CONDIE: Right.

MR. PLESSET: Yeah, okay.

DR. WU: I think on your suction side, and then pressurized the side of the blades, that you cavitation phenomena and also the F variation must come up somehow, so it can't be that homogeneous, right?

DR. CONDIE: I guess I didn't understand your question.

DR. WU: Oh. As your pump is running, and when you have a certain df fraction fluid intake, and it is just across, or the distribution around the pump blades, there must be some variation, and a noticoable variation.

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MR. PLESSET: There would be a lot of variation, yeah.

DR. WU: Lot of variation.

MR. PLESSET: Well, I think --

DR. WU: The cavitation would be the worst --

MR. PLESSET: Well, we should go on, this is just incidental, but of some interest. Okay.

DR. CATTON: Wouldn't this curve change if you changed the flow regime, if you had stratified flow entering the pump, or something different, so that is not a unique motion, so it is somewhat experiment-dependent.

MR. PLESSET: Location dependent on the pump, inlet and outlet.

DR. CONDIE: These pumps, we come into the bottom of them from the loop seal and vertical, so that would be pretty uniform, as -- coming into those pumps as opposed to a horizontal inlet in the pump where you can actually see some stratification. It made quite a little bit of difference on how the inlet then pump occurred. sic

Let me summarize here the results of our pumps on/pumps off experiments. With the pumps running, we had a higher break flow over the majority of the transient, thus resulting in a much lower system mass inventory. However, we had a much more uniform flow distribution throughout the primary systems with the pumps running, and resulting in no trackable liquid level that you could measure.

The cooling continued in the core at this high system voids, because of the effects of the pump. These experiments here do support the decision by NRR to shut the pumps off during a small break LOCA. We basically have confirmed that window that the vendors proposed, wherein if a pump trip was delayed, it could result in a rapid core uncovery, and that is exactly what we got.

MR. PLESSET: But did the window occur anywhere near what they had predicted, the vendors?

DR. CONDIE: In terms of time, or void fraction?

MR. PLESSET: Yes, in terms of time.

DR. CONDIE: I could find that out. I couldn't answer that.

MR. PLESSET: I think that is the important -- do you remember?

DR. CHEN: You mean the vendors' codes have that down pump predictions on this test?

DR. CONDIE: The vendors basically said yes, we should shut the pumps off because they had proposed that this window would occur, and that it would be a more severe transient to leave the pumps running, and then for some reason the pumps have to shut off later on, for pump failure or loss of power, or something, so the decision was basically shut them off.

75 MR. PLESSET: There is a detailed report of this. We shouldn't stop on this, the vendors predictions.

DR. CHEN: I know that, but I am asking if they had conducted a test.

MR. PLESSET: Oh, no, they have no tests.

DR. CHEN: No, no, calculation for L3-6.

MR. PLESSET: That was just a calculation. They don't run any tests.

DR. CHEN: No, no. A calculation, they have done that.

DR. SULLIVAN: I am sure you are aware that that was a required problem --

MR. PLESSET: Yes.

DR. SULLIVAN: -- of the vendors. The results are just now coming in. There is a lot of questions about what they change. We had a very elaborate system set up to find out what they changed and why they changed it.

MR. PLESSET: You mean, this is changed from the first report that we saw -- well, Sheron summarized it in his report, oh, over a year ago. He had their predictions at that time. Did they change them?

DR. SULLIVAN: No, this is the --

MR. PLESSET: This is the vendors?

DR. SULLIVAN: Yeah, that was L3-1. We are talking about the L3-6.

DR. CONDIE: We probably missed something here. We asked the vendors, or NRC asked the vendors not to pre-predict the experiment, because the experiment -- a lot of money.

MR. PLESSET: No, this is a generic -- this is kind of a generic result that they had given some time ago.

DR. SULLIVAN: That is L3-1.

DR. LANDRY: Are you talking about the predictions that the vendors did --

MR. PLESSET: Yes.

DR. LANDRY: -- at their own plants?

MR. PLESSET: Yes.

DR. LANDRY: Right.

MR. PLESSET: Right. These were specific, and kind of generic to their own plants.

DR. LANDRY: Right, and they found a window in which --

MR. PLESSET: They found a window.

DR. LANDRY: -- in which they could not shut the pumps off.

MR. PLESSET: And my question is, how well did they predict the time of occurrence of the window, because that is pretty important.

DR. LANDRY: There is a little difference in the way this test was run, and the way that calculations predict that window. That window was using the assumption that the accumulators came on at the normal set pressure of each vendor. That would vary from I believe it is 250 or 350 psi up to 600 psi.

We valved out the acculators in L3-6 and did not allow the accumulator to come in throughout the whole transient.

MR. PLESSET: Yes.

DR. LANDRY: So we went below 300 psi without the accumulator feeding any extra water to the system. That would throw us off from that window.

MR. PLESSET: That is true, but what I wonder is, I mean, one can take this into account roughly, is what I am trying to say, because the important factor here is how much time do you have to decide whether to turn the pumps on or off and the vendors were leaning toward a relatively short time, and it isn't that short, quite clearly.

See, this is an important aspect of -- what is the problem? The problem is, does the operator have enough time to decide he has a real small break LOCA or not? The chance of his having another kind of transient which would drop the pressure other than a LOCA, a small break LOCA, is much greater than the LOCA.

The question is, does he have time to try to diagnose it? According to the vendors' calculations as I remember them, they didn't, but I don't believe it.

DR. LANDRY: We did not do a comparison between that window and timing of the vendors we are talking about for the large plants, with our results for L3-6.

MR. PLESSET: No, but I think the whole question is kind of an important one in determining --

DR. LANDRY: Licensing may have.

MR. PLESSET: See, the vendors want to turn the pumps off very promptly, because that is part of their general procedure, pumps off right away, and they like that. There is no discretion involved, but it might not be the safest thing. This is why there was some sentiment for letting the operator have a little time, and it is clear that he does as far as LOFT goes. It may not apply to the real world, but --

DR. SULLIVAN: That was the purpose of this required standard problem, to see if the calculations they had performed were realistic, and what we were going to try to do is to assess the code that they were using against a LOFT transient, and they -- well, they may have resisted, but they did agree finally to do that.

MR. PLESSET: You haven't seen that yet.

DR. SULLIVAN: And we have not seen that yet. After we see those calculations, we will be able to make a decision if they know that time very well. And it may give us some more data to give them more time to make that decision.

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MR. PLESSET: Okay. All right.

DR. ZUDANS: Question here. This figure that you show that indicated essentially the window that we are talking about shows here in about ten minutes you begin uncovering the core.

DR. CONDRIE: That is right.

MR. PLESSET: If you shut the pumps off.

DR. ZUDANS: They were not shut off here.

MR. PLESSET: No, no, but --

DR. CONDIE: If you were to shut them off --

DR. ZUDANS: Now, you end this figure as ending here. What happened from this point on? You still would uncover the core.

DR. CONDIE: Oh, yeah, I am just saying by that time, the last part, it would be completely uncovered.

DR. ZUDANS: So according to this, you have ten minutes in LOFT, right?

DR. CONDIE: That is right.

DR. ZUDANS: And your accumulator wouldn't have done anything, because it wouldn't have come on. This pressure was still much higher than 600 psi at that point.

DR. CONDIE: At that point, yes.

DR. ZUDANS: So the accumulator wouldn't have helped you any.

MR. PLESSET: It wouldn't have changed the picture, right.

DR. ZUDANS: So you still have just that time.

DR. CONDIE: That is right.

MR. PLESSET: Well, it is also related to the question, should it be automatic pump trip, or should we let the operator use some discretion, and it is still an open question, I guess, to NRR.

DR. SULLIVAN: I don't think that it is --

MR. PLESSET: Not open, they have settled on pumps off?

DR. SULLIVAN: They have settled on pumps off.

MR. PLESSET: Automatic trip?

DR. SULLIVAN: No, manual trip.

MR. PLESSET: Manual trip. For a while they were talking about automatic trip.

DR. SULLIVAN: Yes, and -- that was one of the purposes of the required standard problems.

MR. PLESSET: Yeah.

DR. SULLIVAN: Was to show that you can calculate well enough that you would allow the guy to have enough time to make some decision, and it is in the order of minutes, I think.

MR. PLESSET: Yes.

DR. SULLIVAN: It is not like run over and do it, which I think was in the direction that you were leaning also, the ACRS was leading.

MR. PLESSET: Yes.

DR. SULLIVAN: They wanted to give him enough time to diagnose what was going on, and then to trip the pumps manually.

MR. PLESSET: Yes.

DR. SULLIVAN: If you realized that you had this problem.

MR. PLESSET: Okay. We can go on, I think.

DR. CONDIE: I would really like to show the movie. I don't know if we are going to get any closer to getting the lights off or not.

MR. PLESSET: Let us try it. All right, let's go.

DR. CONDIE: A two and a half percent small break, pumps on, it is equivalent to the four-inch break in a large plant. This gives you the test sequence. As I indicated, the reactor scrammed just prior to time zero, which is the break flow initiation. HPIS came on very early. We tripped the pumps, as you will see, 2371 seconds. I will try to point these points out to you as we go.

About 25 seconds after that, the first fuel rod acheived CHF, and then HPIS -- or ECC came on.

Okay, here we are sitting at steady state, or not a steady state, a nontransient situation. This is time after rupture. These are thermocouples with a space -- this is saturation temperature. The red ones are one rod, the white ones another rod, and the green ones another one. There is three different fuel rods involved here.

DR. WU: Are we looking at data?

DR. CONDIE: Everything on this is data, yes.

And we should be going back to time zero here very shortly. This is the center module. Okay, we are counting down, now, so we are here sitting at close to power. We have scrammed now. Some subcooling occurs. Now we have opened the break, see, and the saturation line is dropping back. HPIS is on by now. We are starting to pump saturation in the whole system. Note the thermocouples getting closer to that line. We will start boiling here now in a few seconds.

Now, we have speeded up. That is to get you through this in a hurry. There is not much that goes on here for quite a while.

Saturation line dropping down with the pressure. I can guarantee that the night of the test, it was going a lot slower, and the suspense was much greater.

We will skip a few hundred seconds here now and go back into slow motion, and let us pick it up just before -- the pumps are off now.

MR. PLESSET: How long have they been off?

DR. CONDIE: About 15 seconds.

MR. PLESSET: Okay.

DR. CONDIE: They went off at 2379 and so about -- so now you can see the temperature excursion going up. We initiated ECC at 600, approximately 600 degrees F. This is actually about real time, I guess.

Now the accumulator should be on. See it drop the saturation. Now, notice the points -- notice the points from the top down, and now as the ECC comes in, we are subcooling from the bottom up.

The introduction of the accumulator into the lower plenum caused it to condense and to pull that much -- enough liquid condense out of the upper part of the system, the upper head, the hot leg, and even as far as the steam generator, you know, you never know, came down and quenched that from the top down, and then you could see that occurred then just as the ECC hit the bottom of the core.

DR. LIENHARD: It is worth noting that everything we have been watching since the film came back on is in real time.

DR. CONDIE: Yes, right, that is real time. In fact, there was only -- we were watching this on a monitor, not quite this particular format, but there was only one or two people in the whole audience that picked it up that it quenched from the top down. Most of them were expecting a bottom-up quench and they saw this curve here, the subcooling and the quenching from the top down occurred so fast that most people missed it. It wasn't -- we saw this film after and could hardly believe it.

We have shut the accumulator off now, and also the HPIS has been down to one train, and it has reduced just enough to keep the core -- we don't want it to go solid.

DR. LIENHARD: Why was it a surprise that it quenched from the top down? The water came in from the top.

DR. CONDIE : No, the water came in from the bottom.

DR. LIENHARD: Excuse me.

DR. CONDIE: That is why it was a surprise. The accumulator dumped intc the down-comer, and so it entered the core from the bottom.

DR. LIENHARD: And there was some film boiling convection, I suppose, at the bottom as it went up.

DR. CONDIE: Yeah, there was enough -- starting to condense at the bottom and drew that fluid out of the top, and there was enough velocity on that that it quenched it as soon as it hit the top from there on down.

DR. ZUDANS: Do you have a significant amount of bypass to the top from this --

DR. CONDIE: From the top?

DR. ZUDANS: Yeah.

DR. CONDIE: From where, to the break?

DR. ZUDANS: No, to the top of the fuel.

DR. CHEN: From the pump -- to the upper plenum bypass.

DR. CONDIE: Which bypass are you talking about?

DR. ZUDANS: Well, when you come in, some of the fluid, instead of going downcomer, can also bypass that to the top.

DR. CONDIE: That is in the neighborhood of three percent leakage there.

MR. PLESSET: That is a leakage. Really, it is pretty small.

DR. ZUDANS: It is three percent leakage could have quenched the top rod.

MR. PLESSET: Well, I doubt --

DR. CONDIE: Well now, we injected this directly into the downcomer, not into the cold leg.

DR. ZUDANS: Oh. Oh.

MR. PLESSET: So there was no leakage -- yeah, okay.

We are going to get the lights on, and we can continue. Those are very beautiful presentations, very nice. Very good. Fantastic.

DR. ZUDANS: Do you have a similar film for L3-5?

DR. CONDIE: Well, it is not as dramatic, of course, because the thermocouples don't do anything, and one thing we do have for L3-5 is a liquid level movie, and that is what is important to L3-5. I do have it here. I didn't intend to show it. In fact, we also have one that splits it and shows the void fraction in L3-6 in the same time frame that we see this liquid level develop in L3-5. I didn't intend to have this all movie.

MR. PLESSET: That is all right. That is for another time. We can go on.

DR. CONDIE: Is there any question on that, the film or the data itself? I am going to go in now to our predictions of the both the L3-5 and the L3-6 experiments. I am sure there will be some questions that come along.

I don't want to belabor you with a lot of plots about what our predictions did, because most of that is past history, but in a conclusion -- in summary, I should say, the experimental predictions exhibited a much much too high depressurization rate. They brought us down to the 300 psi point in about half or two-thirds of the time, depending on which test we saw on the data.

Mostly because they overpredicted the break flow in L3-5 and in the early part of L3-6, we had too high of densities -- or two low of densities in the hot leg and generally too high of densities in the cold leg.

A question that was brought up earlier was about the primary and secondary being thermally coupled in L3-6. We continued to have primary and secondary heat transfer in our calculations through the whole experiment, and the pump two-phase degradation curves were too high. We talked about that a little bit, and we did have one error in the code that caused very large mass errors to occur in L3-5, and I don't think we want to go into a lot of detail on that.

To show you that the RELAP5 code was not the only one that had problems, this is a plot of the experimental prediction for L3-6 versus data. The data here, RELAP5 code is the dotted line, and the TRAC code is the dashed line underneath, so the TRAC code also has the benefit of the L3-5 experiment prior to their calculation of L3-6, so we are talking about some very fundamental problems in our models.

The break flow is also shown here with RELAP5 and TRAC with L3-6, and TRAC predicted much much higher flow rate than even RELAP5 and then the data is clear down here for L3-6, so it isn't just a matter of one code not being able to handle some of the phenomena that we see here.

We are still in the process of doing post-test analysis on both these experiments and we will be for quite some time, but up to this point in time, we have made some changes that have helped. They fall into this category. The initial conditions are always just a little different than what we had in the experimental predictions, so we changed those.

The initial sequence, the scramming of the plant, the opening of the blowdown valves, that is a manual thing and varies from test to test two or three seconds, and so that has quite a little bit of effect on the decay heat when the break opens.

We have talked in times past quite a little bit about the steam control valve leakage on the secondary side. I don't have any slides here to show you, but we have basically concluded that that leakage is not a very important problem, and it doesn't account for most of the problems we have with the secondary pressure response. It is -- well, I will talk about the heat transfer problems we think are more important on that.

We had our pump inertia wrong in our EP for L3-5. We fixed that. L3-5 after that, we found the mass error and fixed that in the code, so that is a definite improvement there that speeded up the running time as well as fixed the mass error.

The break geometry, we have done more to try to model it. That break comes off of that large pipe, that large 14-inch pipe, with about a one and a half inch pipe, and we have looked at different ways that can be modeled, and we have also improved the two-phase degradation curves, although we haven't modeled what we think are the best ones there.

So with those changes I will show you some differences there, both L3-5 and L3-6, the differences from our experimental prediction to our post-test. The post-test here we show isn't very far out in time, but represents where we are today.

You will note that we have improved the early pressure response. One here is the data. Two is the experimental prediction, and three is our post-test prediction.

We pick up the early transition to saturation now very well and the early pressure response here, but notice we deviate from the pressure about 500 seconds, and it looks like we are still on this same trend that we were with almost the same slope in the EP.

The L3-6, still basically the same thing, we are faced with the same problem there. We improved this initial time in this region, but here you see after 700 or 800 seconds, we continue to decrease the pressure, and also on L3-6, I will show you the primary and the secondary pressure response in our latest post-test prediction.

Note that the primary and secondary temp pressures are staying very close together here, indicating this close thermal response. If you will remember, the first slide I showed you on L3-6, the data, that that primary pressure dropped below secondary pressure, and then they started to divide, and stayed decoupled, but we haven't been able to achieve that yet in our analysis, and we think that that has to do with heat transfer problems that I will mention in a minute.

The break flow for L3-5, our prediction was way high, as shown by line 2, and we have decreased that some with line

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3, but still we haven't satisfactorily characterized that break flow.

Consequently, it didn't change the inventory much. Our original prediction for L3-5 on the mass inventory was clear down here, and -- as opposed to the data.

In time, at 300 psi, there wasn't too much difference in our final mass inventory, as you will note, but the time it took to get there was much, much less.

I am whipping through these very fast, and I hope you feel free to ask any questions or stop me as we go. The same is shown here for L3-6, but note, even in our post-test, we have increased the break flow and gone the wrong way in this post-test analysis we have increased the flow.

Now, we haven't done anything at this point to the break flow model or the multipliers. We are working on those things internal to the code right now, but it is interesting in this post-test, by changing the mass -- or the pump curves, we have now come in on the inventory. The data is number one, this is the primary system mass inventory, and we do have a calculation that is very similar to this, that sic goes out farther than time. It goes clear out. It follows this inventory, and actually gives us the very very rapid temperature increase when we shut off the pumps, but that was -- I don't have it in this calculation, but it does show that we have characterized a little better the break flow or the mass inventory.

I indicated earlier our problem with densities. We think that is where the problem with the break flow was, of course, was in not predicting the upstream density correctly, and one of the reasons why we don't predict the loop densities correctly, we think has to do with the steam generator interfacial drag, and condensation phenomena.

That is, you will notice that -- you have got to change your thinking a little bit, the pumps are shut off, and in this region, we are in a natural circulation mode. Note, this is the hot line. The data, number one, shows that we stay fairly high density out here until about 500 or 600 seconds.

It is during this period of time just a little bit beyond 500 seconds that the primary drops below the secondary and we lose that natural circulation, but note in our prediction, we drop down in density very, very early. We pick up a little bit of density, raise and then drop back down, but basically in this region where we are in natural circulation, we are not predicting the proper density in the hot leg or the cold leg.

We feel, perhaps, that there is more refluxing going on in the experiment than we predicted in our data. We did predict in L3-5 a significant amount of refluxing to occur. That is, the two-phase mixture went into the steam generator, condensed and dropped -- flowed back directly to the -- back into the core, or the upper plenum.

That would account for this density staying considerably higher in that hot leg, and we predicted some of that refluxing to occur, but probably not enough, and we are looking carefully now at the interfacial resistance between the vapor and the liquid. Dr. Banerjee is consulting with us on that, and he has a little refluxing facility up at Santa Barbara, and we are looking at that as a way to help qualify these interfacial drag terms.

I do want to show now, though, the trends of our post-test prediction relative to the data -- I mean, relative to each other, not the data. This is a prediction of L3-5 versus L3-6, and you will notice as we go through these, we have picked up the basic trend of the difference between the two experiments. L3-5 depressurized a little faster during the early part of the transient than did L3-6. We have got that difference.

MR. WARD: You do? I don't see the difference there.

DR. CONDIE: Pardon?

MR. WARD: I don't see much of a difference there.

DR. CONDIE: There wasn't much difference in the data either, if you will recall.

MR. WARD: Oh, right at like 50 seconds, or something.

DR. CONDIE: Yeah.

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MR. WARD: Oh, all right. Okay. Go ahead.

DR. CONDIE: Out in this time frame, L3-2 or L3-5 did depressurize faster than L3-6, and then they were very similar.

The break flow, very close to the same earlier, but notice number 2 is right up here, and it is a significantly higher break flow than L3-5, as shown in plot number one, so we have got that trend correctly.

The next slide shows a reactor vessel mass, not the primary system mass that we have kind of been talking about. From the data, we can't get the individual component masses, but we can with the prediction, so it makes it quite convenient to look at individuals, but if we sum just what is in the reactor vessel, you will note, as we saw on the data, that the reactor vessel mass stayed fairly constant in L3-5, that is, that we had a reactor vessel full of water, and it was boiling off and dropping down some.

In L3-6, that dropped down much faster so that we had this uniform high void mixture in the core.

DR. ZUDANS: What would be the boiloff rate of that water if you can't remove the heat anyplace, either reflux or otherwise?

DR. CONDIE: Oh, if it is just sitting there boiling off?

DR. ZUDANS: Right, how far would it be from that curve one?

DR. CONDIE: Oh, it is very slow. See, in our initial calculation, that is what we had, and I was trying to think, it took, oh, you were losing something like three kilograms a second.

DR. ZUDANS: So it would be close to that curve one, then.

DR. CONDIE: Yeah, it would just be -- it would be kind of like this.

DR. ZUDANS: Oh, above the curve.

DR. CONDIE: Well, it would -- yeah. Up to about here we had natural circulation. In this region, we didn't, but the break was open, so it would be pretty similar to this curve right there, yeah.

Now, this next plot shows a ratio of the reactor vessel mass to the total system mass, so as in L3-5, with the pumps off, the total system mass is decreasing, but most of it, a larger and larger percentage of it ends up in the core, see, and fills it up.

In L3-6, it is virtually homogeneous throughout the system, and so as the system mass depletes, so does the core, and therein then is the difference between the pumps on and the pumps off.

We have got a ways to go in our pumps tests, and now these are a few of the regions that we want to look at.

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We have been occupied as an industry over the past few years looking at heat transfer, blowdown heat transfer, reflood heat transfer. We find in these small breaks where natural circulation occurs, where the steam generator heat transfer is important, that we have as an industry neglected the less popular heat transfer modes, condensing and free convection.

On the horizontal parts in the steam generator -- there is a lot of, as you say, offbeat modes that we have not correctly modeled in the code. We have not -- there are not good correlations around for them, and these are the areas that are giving us problems. We start to condense too fast on the secondary side of the tubes, once we drop the primary pressure below the secondary side, and so we are going back -- in RELAP5, we have -- the code has something like 32 distinct heat transfer modes, they are called, and that means that there is a lot of regions where they are switching from one mode to another, and the interface on those mode changes is not well-defined. There isn't much data. That is where we are spending a lot of our time right now. We have talked about the LOFT pump curves and we are going to update that.

We are trying to codify the stratification in that "T". We have got that large pipe, the small pipe, and the codes don't handle well the stratification as it goes around that bend.

One thing that we haven't had in the codes, we have never had in our model -- it is not the codes, it is our model, is we have never modeled the piping heat slabs. It is an economic thing. Back in RELAP4 days, we just didn't have enough time, enough computer capability to put all those heat slabs in. With RELAP5 it has been expensive, and it is running faster, and so we are adding these. We have heat slab now in every volume, or we will have, to account for this -- it is not so much that we are losing heat to the environment, but in these smaller breaks and the longer transients, it gives a lot of time for that heat, stored energy in that pipe, and there is a lot of it, to come back into the system. That will help us on our depressurization rate. That change is just about done.

MR. PLESSET: That is for LOFT. That is not going to be important in a full-sized plant, is it?

DR. CONDIE: Well, there are --

MR. PLESSET: I mean, I am asking.

DR. CONDIE: Not as important, but there are still -- the ratio of surface area, because of the smaller diameter pipes, is larger -- surface area to volume is larger in LOFT than it is in other plants.

MR. PLESSET: Yes. Yes.

DR. CONDIE: But as far as the internals in the core and within the reactor vessel and the steam generator and

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things like that, there would still be a certain effect.

DR. SULLIVAN: It is a good way to find out if it is an orderly -- if you can model it in LOFT, and then leave that model alone in calculating --

MR. PLESSET: Yes, no, I am all for it. I just thought it might not be as significant.

DR. SULLIVAN: No, it -- you would expect it to be much significant in LOFT.

MR. PLESSET: Much more here, yes. And it could give you some significant error here.

DR. SULLIVAN: Yes.

MR. PLESSET: In LOFT, yes. Very good.

DR. CONDIE: Well, LOFT does have those filler blocks, if you will recall, those about 12-inch-think in-canal in the lower plenum. Now, we have always modeled those in our calculations before this time.

MR. PLESSET: Yeah, I see.

DR. CONDIE: And we look at those and there is a significant amount of heat that enters the downcomer from those, but we haven't had the piping in before, so we are going to do that.

I talked a little bit about our interfacial drag problem in the steam generator. We are looking into that, and we will have to look more at the critical break flow, see if there is some reason why we could justify a multiplier on the model. We haven't done that yet.

DR. ZUDANS: I would like to comment at this point.

We heard a presentation this week by a German lady that modeled this critical break flow with a completely different model. She called it 1B, and compared to your results, she got an excellent break flow. I think it was the same test, L3-6.

DR. CONDIE: Was it?

DR. ZUDANS: That is right, and I am sure you can get the paper from the chairman here or from your colleagues that attended the meeting. She had a remarkable break flow comparison to your test.

DR. CONDIE: I would like to see that. One reason why we haven't --

MR. PLESSET: You can get that. She wasn't a German lady.

DR. ZUDANS: She wasn't?

MR. PLESSET: She was a Polish lady married to a Frenchman, but otherwise --

DR. ZUDANS: I thought she was --

MR. PLESSET: That is pretty close, because she is from Karlsruhe, I think.

DR. ZUDANS: She had told the -- spoke in German, so that is why I thought that she was German.

MR. PLESSET: Oh, yes. She speaks German. She lives there now, and --

DR. ZUDANS: I don't know where -- but that was an impressive model.

MR. PLESSET: Yes, interesting. Well, they can get that -- well, you have it, don't you.

DR. CONDIE: One reason why haven't at this point said aha, we need a break multiplier is because we feel that our model is pretty good if we can get the right densities upstream of the darn thing, and we could show you some pretty good pressure responses if we would go ahead and do that right now.

MR. PLESSET: That may be a big part of your problem.

DR. CONDIE: But I think we would be missing the boat, so we are holding off on that.

MR. PLESSET: Yeah, that is okay. Right.

DR. CONDIE: Well, let me summarize real quickly this whole pumps on/pumps off thing. We feel we have confirmed the critical limit for the delay pump trip for PWR small breaks and I think as important as anything, these tests have provided a real challenge for the codes, and we are learning probably more from these two tests, at least I have, in the period of time that I have been in LOFT, we have learned areas that we haven't even considered before.

From that standpoint, they have been extremely beneficial for us in terms of code assessment and understanding small break phenomena. They have certainly been useful in that respect, and so as we improve these, we feel it is important that we have another test in a year or so so that we can really test whether or not we have changed the codes and improved them for the right reason, and not just tuned them up for the particular transient that we are looking at.

MR. PLESSET: Right, well that was a very interesting presentation, and we appreciate it. I think it was very very good, and I am sure that sentiment is shared by all of us.

DR. ZUDANS: Yes, it is a very convincing argument.

DR. CONDIE: I do have a couple of slides on our next test, the L9-1 thing, if you want to take a minute, just tell me.

MR. PLESSET: Well, all right. Let us look at them. It won't take long.

DR. CONDIE: It won't take long. I just want to -- it is coming up in a couple of weeks. It may help you.

Just to summarize, we have been designating tests as a function of the phenomena that we have in them, so in order to describe this test in our nomenclature, we have got three tests. That also helps us look better when we say we ran three tests for $10 million instead of one, but L9-1 portion of this test is a loss of feedwater, then without auxiliary feedwater available, steam generator dries out, and that results in an overpressurization of the primary system and the PORV LOCA occurring.

The L3-3 portion of our transient, then, is recovery from a loss of feedwater LOCA accident, and the recovery procedure in this case is to latch open the PORV to keep the pressure going down.

You see, if the plant is left to take care of itself, the pressure is so high that once the steam generator dries out, none of the emergency core cooling systems can come on. We are up at high pressure. We are generating steam that opens and closes the power-operated relief valve and the pressurizer and depletes the mass inventory without able to turn on the emergency cooling system.

So the procedure herein is to prop open that PORV and make the pressure go down so that the emergency system can operate.

And the L8-1/A is a degraded core cooling experiment. We will let the core blow off slowly, and then a recovery process.

DR. ZUDANS: One question on this middle part. Your PORV is big enough to remove all the heat, all the decay heat, right?

DR. CONDIE: Well, that is one of the objectives of the experiment.

DR. ZUDANS: Oh, you don't know.

DR. CONDIE: We have scaled the size of the PORV orifice on a core power basis to an LPWR, so we will determine from this --

MR. PLESSET: The primary function of locking it open is to get the pressure down. It is still steam. It is not any essential heat going out. What you want to do is get the pressure down so that the ECCS can come on and take care of the heat removal.

DR. CONDIE: Right.

MR. PLESSET: I think that is the purpose of the test.

MR. WARD: The Combustion Engineering system doesn't have PORV's, does it?

MR. PLESSET: They have safeties that will do the same thing, we hope -- we presume.

DR. CONDIE: This -- I have one slide here that shows our experimental prediction for that, and it doesn't go out all the way, but let me just lead you through it just a little bit. We scram the plant -- well, we initiate the experiment by shutting off the main feedwater.

The heat transfer to the secondary decreases, the pressure goes up and that causes a scram on high pressure. The pressure drops because we have scrammed, and then it

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picks up because of the inability to remove the heat. The spray cycle is on and off during this period of time, the sprays in the pressurizer, to hold that pressure constant until the system then is solid.

Then the pressure goes up to the point where it cycles the power operated relief valve. The pumps are running during this period of time also.

Then there is a limit, a temperature limit specified in the experiment such that when the temperature reaches a certain point, we latch open the PORV and then have a pump trip at the same time. That causes the pressure to drop, and it drops, but it drops very slowly in that period of time.

Now, this period of time in the experiment is also specified and after a couple of thousand seconds then we will close the PORV and refuel the steam generator so that we reestablish the steam generator as a heat sink.

So, this is L9-1, this is L3-3. Once the steam generator is full, so that it is at maximum cooling, the pumps are not on, so there is natural circulation. The pressure goes up to such a point that now we are dumping heat to the steam generator again, and the specification is if we are not below 300 psi at this point, then we will feed and bleed the secondary side of the steam generator. We have done that very effectively in other experiments to get that pressure

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down. Then we will do that for a while, holding the pressure around 300 psi, making sure that the primary and the secondary systems equilibrate.

Then we will open the PORV again to create a small break LOCA at low pressure, which will allow the core to boil off and uncover the top of the core, and then at a certain point in time -- not at a certain point in time, but at a certain temperature, once the temperature reaches I think it is 750, before we initiate ECC, we will start the pumps up again, and see if in that condition, the depleted mass inventory, if the pumps can pick up on that, and get into this high void steam cooling mode, and once we demonstrate whether or not that is possible, then we have got the ECC systems, and we will dump that once the temperature reaches a certain point, so it is going to be kind of an interesting test. That is about all I can say about it right now. I appreciate your question.

MR. PLESSET: Thank you. Harold, do you have a comment you want to make at this point?

DR. SULLIVAN: Yes, maybe I could do a little housekeeping.

MR. PLESSET: Do a little what?

DR. SULLIVAN: A little housekeeping.

MR. PLESSET: Yes.

DR. SULLIVAN: The LOFT L3-3 and L9-1, if you look on your schedule, is a date of around 4-15. I would like to invite each of you to come to that experiment if you could. It is probably going to be one of the more interesting experiments that we have tried. All of the cycling that we are going to do and trying to uncover the core and looking at different uncovery modes is probably going to be pretty exciting, so I would like to invite each of you to come.

I would like to talk a little bit about TLTA. You know that we were in a mode of extending the contract with GE to do some more testing in a modified TLTA facility. We are presently in a process of trying to get that done.

The theory is that we need to do something about that contract before the end of this month, and in an extension mode. If we let the contract expire, which it does at the end of this month, we would have to negotiate a completely new contract with them, and it probably would be in the order of eight or nine months delay if we decided to go this way.

Dr. Tong is presently in Japan, and he is reviewing the facilities that you mentioned. He will give us a report back.

MR. PLESSET: I would be interested in what he has to say also.

DR. SULLIVAN: We thought maybe we would come in and give you maybe a half an hour or 15 minutes.

MR. PLESSET: Fine.

DR. SULLIVAN: Something about what he found out.

MR. PLESSET: When will he be back.

DR. SULLIVAN: It is either this week or next week.

MR. PLESSET: Next week. Well, that would be a lot more exciting than the full committee meeting, I think. I would be glad to -- excuse me, gentlemen -- I would be very glad for you and he to tell me what his thoughts are.

DR. SULLIVAN: When will you be in Washington?

MR. PLESSET: Well, the full committee meeting is 9, 10 and 11 of April, and it would be nice if we could get together. 11 is a Saturday. That is no good for you people, but 9 or 10 of April.

DR. SULLIVAN: We will see what we can do.

MR. PLESSET: Fine.

DR. SULLIVAN: We did send -- is it Itachi?

MR. PLESSET: Itachi.

DR. SULLIVAN: That owns the facility, a TWX, and told them that we were interested in doing this. I predicted that there would be one of three responses, no, maybe, or wait, and we got the third response, wait.

MR. PLESSET: Wait. Well, you are pretty shrewd to predict. That is a good pre-test prediction.

DR. SULLIVAN: Changing now to the FLECHT SEASET program, I think the last time we were with you, we indicated that we had a potential cost overrun. That still exists, and a very healthy overrun.

We are currently negotiating with Westinghouse to reduce the overrun. It probably is going to lead to dropping the systems effect experiment completely, so that whole set of experiments. It would then leave the blockage experiments which we are doing now, and the natural circulation. We will keep you updated as to what is going on there.

The Se~iscale mod 5 we are still pursuing the industry support. We were in the wild enthusiasm stage. It has gone on, and we are dragging on then to the slightly wild enthusiasm.

A letter is being generated now to send out. It is going out very shortly, or maybe already has gone out, so we should be getting some kind of an answer back. There will be a meeting --

MR. PLESSET: There wasn't a tremendously enthusiastic response right off, from --

DR. SULLIVAN: We haven't heard from them yet.

MR. PLESSET: Oh, you haven't.

DR. SULLIVAN: The -- in fact, if you or some of the Committee members or the ACRS staff is there, that memo requests a meeting of Research, NRR and the affected parties, and so you might want to attend that, and see some of the questions that are asked, and what our response might be.

MR. PLESSET: When will that be, Harold?

DR. SULLIVAN: I don't have that memo. It was scheduled like for the first part of April, but I am sure that the memo has slipped a number of times, so I think that it will probably be something later than that, so we will keep you informed about that also.

I think that concludes the areas that --

MR. PLESSET: Very good. I appreciate that information, Harold. Would you be able to let Paul Boehert know about this meeting with the affected parties, as you call them?

DR. SULLIVAN: Yes.

MR. PLESSET: Do you have any kind of estimate on what the facility might run to in cost?

DR. SULLIVAN: In terms of money?

MR. PLESSET: Yes.

DR. SULLIVAN: We had estimated it was about $18 million or so, and that we would support the testing for about two or three years at roughly $7 million a year, so the thing we can't get the money for is the capital equipment, and what have asked for is help for that.

We think there is going to be a large number of iterations necessary to make sure that this occurs. We have the time, though. That is also important. There is, you know, within a year, if we could get something done within a year, it probably would be effective.

MR. PLESSET: Yes, very good. Well --

DR. SULLIVAN: We owe you one more thing, in terms of a presentation, I guess.

MR. PLESSET: How long will it take?

DR. SULLIVAN: Not very long at all.

MR. PLESSET: Oh, okay. Let me plead with the group up here not to drag it out by harassing Harold too much.

DR. SULLIVAN: The water hammer is an unresolved safety issue, and it is A-1. The current status of that, is there is no experimental work going on right now by Research.

We have done a water hammer survey, and done some evaluations. That report was done by Research and has been sent to NRR. They will make the final conclusions and write a report that will either address problems in the USI or say that it had been completed.

Our understanding is that NRR currently reviews that as not a -- not representing a serious or immediate safety issue, or safety threat.

The outstanding issues that are left are, we need to wait for the NRR findings. We understand that they are processing that now, so I don't think -- very shortly, they should be coming tc [illegible] with either their findings to resolve the issue or to make some proposal about what to do next.

The only area that we see that we can do work in is in the experimental area, is looking at the collapsing of the steam bubbles, and the heat transfer rate on the surface of that bubble is -- has a very large effect on the amount of a water hammer that you would receive from that. If you had plugged up a pipe, the rate at which that collapses is directly related to if it actually causes structural failure or not, or is just a loud bang.

There are two other slides in the handout, and I didn't make viewgraphs of those. You probably couldn't have read them anyway if we put them up there. The first report that is under the evaluation reports is a NUREG document, and it did a survey of power plants, and it was published in July of '79.

There is a number of other reports. The third one down, review a nuclear power plant and the events of water hammer that could actually occur, that actually occurred, and the potential water hammers.

The next report is a state-of-the-art literature survey on water hammers. Toward the middle of that, and it is called RE-E-79-009, that report, it looked at the analysis tools for predicting the transients, the hydraulic transients, and their applications, and it did some applications work in that report.

The next report under that had -- that was extended into the structural analysis area. So we have looked at void collapsing, the studies of valves closing at high rates and

111 causing the water hammer, check valves closing and power operated valves through the system, and there is a number of reports on that.

The next slide, in fact, the next two slides give you a summary of the result for that, and I will let you read those at your leisure.

Again, the first NUREG document actually was the planning of a lot of the tests that were going on. The water hammer study came up with some interesting conclusions, that the PWR steam generator feed lines were one of the worst places for water hammer as we found out later through an event report.

In the judgment, the computer codes were able to handle the water hammer issue. The steam bubble collapse was noted there, that there was not a lot of experimental data, and that the loads were very definitely a function of the heat transfer rate on that, on the bubble.

I think that Dr. Okrent's question is probably addressed as well as any other in the third report, in which they did look at a number of events in BWR's and PWR's.

The next report said that there was a gap between the amount of computer power that we had and the amount of analysis that had been performed, or the applications, and we also concurred in that.

The next page, the first report looked at the BWR spray lines and said that there was a potential for water hammers in those spray lines.

The next one looked at the opening and closing of the valves in BWRs, and there is a number -- the next two reports, in fact the next three reports all looked at valves in systems opening and closing.

The purpose that I had for giving this rather short introduction is to indicate that we were going to -- NRC or the licensing side of the house will be coming to you with a document that addresses this, and their conclusions from the data that we have gathered.

I think that we have addressed Dr. Okrent's question in some detail. Maybe it is not in the detail that he would like to see it, but we had a number of studies of plants and the potentials for water hammers over the plant.

I think that probably an acceptable thing to do would be to wait and see what NRR comes up with, and then we could -- you could make a recommendation about what you think we should do after that.

MR. PLESSET: Well, thank you, Harold. Actually, things aren't in such bad shape. There are other things in worse shape, right?

Well, I want to thank all of you. We have had a long, hard, and I believe profitable day. I regret that Harold couldn't have come earlier, like last Sunday, but we tried, and we will look forward to the next meeting that you are indicating we should have on the LOFT series.

DR. SULLIVAN: I am not sure how long it is going to take us to get prepared to discuss those, I guess there are three tests, with you. Would you like for us to initiate the next meeting, or --

MR. PLESSET: Yes, we will wait for you to let us know when you think it would be profitable, all right?

MR. BOEHERT: Giveme a call, Harold.

MR. PLESSET: That is the way I would prefer to leave it, and you can tell better both when you have reasonable input, and when you are ready, all right?

DR. SULLIVAN: Fine.

MR. PLESSET: Well, thank you very much, gentlemen. I think that we can adjourn the meeting.

(Whereupon, at 5:02 p.m., 27 March, 1981, the public meeting in the above-entitled matt[illegible] adjourned.)

This is to certify that the attached proceedings before the

Advisory Committee on Reactor Safety

in the matter of: Emergency Core Cooling System Subcommittee

Date of Proceeding: 27 March, 1981

Docket Number:

Place of Proceeding: Pasadena, California

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

George D. Girton

Official Reporter (Typed)

George D. Girton

Official Reporter (Signature)

LOFT TEST PROGRAM
THROUGH FY 1983

PRESENTED TO
ACRS-ECCS SUBCOMMITTEE

MARCH 27,1981

BY
RALPH R. LANDRY

CONSIDERATIONS IN ESTABLISHING LOFT TEST SEQUENCE

- PROGRAMMATIC
  - -- PERCEIVED NEED FOR EXPERIMENTAL DATA
  - -- REQUIREMENTS FOR PLANNING TIME
  - -- GROUPING OF CONTINGENCY TESTS
- SCHEDULE
  - -- MINIMIZE TIME BETWEEN TESTS
  - -- MAXIMIZE PREPLANNING, STAGING OF MATERIALS, ETC.
- COSTS
  - -- MINIMIZE SYSTEM HARDWARE CHANGES
  - -- MINIMIZE MANPOWER LOADING FLUCTUATIONS

CONSIDERATIONS IN ESTABLISHING TEST SPACING

- HARDWARE CHANGES
- MEASUREMENT CHANGES
- SAFETY ANALYSIS CONSIDERATIONS
- PLANT REQUALIFICATION TIME
- PLANT START-UP TIME

RES PROPOSED LOFT TEST MATRIX

TEST MATRIX CONSISTENT WITH LOFT SPECIAL REVIEW GROUP DESCRIPTION

CONSISTENT WITH LOFT SPECIAL REVIEW GROUP HIGH PRIORITY EXPERIMENTS WITH ONE EXCEPTION

INCLUDES CONTINGENCY EXPERIMENTS

RESEARCH PROPOSAL FOR LOFT TEST PROGRAM

TEST DESCRIPTION		LSRG PRIORITY	LSRG NOMENCLATURE
L-OF-FW, SG DRYOUT, LOCK OPEN PORV, SG REFILL		M/H	L9-1/L3-3
COOLDOWN ACCIDENTS ST. LUCIE, ANO		H	L6-7/L9-2
INTERMED. BREAK; ACCUM. LINE, UNCOVERY		H	L5-1/L8-2
OPERATIONAL TRANS	CONTINGENCY*	M	LA-10
LB LOCA; PRES^{D}. FUEL; L.-OF-OFFSITE; OFFNORM. ICs		H	LA-1/L2-5
INTERMED. BREAK	CONTINGENCY*	H	LA-2
ATWS w/L OF FW		H	L9-3
S.B. AFTER CODE ASSMENT PROGRESS	CONTINGENCY*	M	LA-3
ATWS	CONTINGENCY*	H	LA-3
LB LOCA, PRES^{D}. FUEL, L-OF-OFFSITE, DELAY ECC		H	L2-6
NO TESTS BELOW			
LB LOCA, UPPER PLENUM INJ.		H	L4-2
S.B. CONFIRM PUMPS ON/OFF RESULTS		M	LA-5
LB LOCA REPEAT		M	LA-4
ANTIC. TRANS ROD w/D		L	L6-4
ATWS, L OF OFFSITE		L	L9-4
ANTIC. TRANS; UNCONT. B. DILUTION		L	LA-6/L6-6
STEAM LINE BREAK		L	LA-7
STEAM LINE BREAK		L	LA-8

NOTE: THE ESTIMATED DATE OF COMPLETION OF ALL TESTS WILL BE APRIL 84

*PREPLANNED TESTS TO BE RUN ONLY IF REQUIRED. OF THE 4 TESTS, RES ESTIMATES 2 WILL BE NECESSARY.

RESEARCH PROPOSAL FOR LOFT TEST MATRIX

DESCRIPTION		LSRG	DCC RULEMAKING NEED	APP^{X}K NEED	TEST DATE (RES ESTIMATE)
L-OF-FW, SG DRYOUT, LOCK OPEN PORV, SG REFILL		M/H	X		4/81
COOLDOWN ACCIDENTS ST. LUCIE, ANO		H	X		8/81
INTERMED. BREAK; ACCUM. LINE, UNCOVERY		H	X	X	10/81
OPERATIONAL TRANS	CONTINGENCY*	M			12/81
LB LOCA PRES^{D}, FUEL; L-OF-OFFSETE: OFFNORM, ICs		H**	X	X	4/82
INTERMED. BREAK	CONTINGENCY*	H	X		6/82
ATWS w/L OF FW		H	X		9/82
S.B. AFTER CODE ASSMENT PROGRESS	CONTINGENCY*	M	X		12/82
ATWS	CONTINGENCY*	H	X		3/83
LB LOCA, PRES^{D}, FUEL, L-OF-OFFSITE, DELAY ECC		H***	X	X	6/83

CONTINGENCY* = PREPLANNED TESTS TO BE RUN ONLY IF REQUIRED. OF THE 4 TESTS, RES ESTIMATES 2 WILL BE NECESSARY, IN WHICH CASE, FINAL TEST WILL TAKE PLACE IN JANUARY 1983.

**CLAD BURST POSSIBLE

***CLAD BURST LIKELY

BURNUP LIMITED TO 2 WEEKS IN BOTH CASES.

BENEFITS OF PROPOSED TEST MATRIX

- ADDRESSES HIGHEST PRIORITY CONCERNS, INCLUDING LSRG

FIXED PROGRAM MEANS -

PREDICTABLE COMPLETION DATE
ORDERLY SHUTDOWN
GREATER EFFICIENCY
LOWER COSTS

OPPORTUNITY FOR INPUT FROM DCC RULEMAKING BEFORE SHUTDOWN

LEAVE POSSIBILITY OF PREPLANNED CONTINGENCY TESTS

SHOULD LEAVE ONE USEABLE CORE AVAILABLE IN CASE OF REACTIVATION

LOW PROBABILITY OF FACILITY CONTAMINATION

Proposed LOFT Test Program in Response to RES/SRG Recommendations
March 13, 1981

Test ID	A Target Date	A Commitment Date	Description
L3-3/L9-1	4-15-81	4-22-81	• Small hot leg break experiment. Dry steam generator secondary • Loss of all feedwater. Scram on high pressure – delayed
CV leak test	6-19-81	7-02-81	Required test of containment leak integrity
L6-7/L9-2	8-03-81	8-2[illegible]-81	Simulated turbine trip multiple failure continuation of L6-7
L5-1/L8-2	9-25-81	10-26-81	Intermediate size break (accumulator line). Core uncovery at high decay heat level.
Replace A2 with F1	10-15-81 through 12-01-81	11-19-81 through 1-05-82	F1 center fuel pressurized to 350 psi
L2-5	1-13-82	2-24-82	200% cold leg break at 50 MW to produce the worst probable core thermal-hydraulic conditions. No fuel damage

Proposed LOFT Test Program In Response to RES/SRG Recommendations March 13, 1981

Test ID	A Target date	A Proposed commitment date	B Target date	B Proposed commitment date	Description
LA-10	2-24-82	4-14-82	2-24-82	4-14-82	Boron dilution from cold shutdown
L9-3	4-27-82	6-29-82	5-10-82	7-5-82	ATWS - Loss of feedwater
LA-3	0	0	6-07-82	8-9-82	ATWS
LA-9	7-12-82	9-20-82	8-21-82	11-01-82	Small break intact loop cold leg ECC injection in downcomer
LA-2	0	0	11-22-82	2-07-83	Intermediate break
Replace F1 with F2	8-19-82 through 9-22-82	11-11-82 through 12-15-82	12-19-82 through 1-22-83	3-14-83 through 4-15-83	F2 pressurized bundle
L2-6	11-12-82	2-14-83	3-14-83	6-15-83	200% cold leg break double ended at 50 MW

INEL-S-31 107

CONCERNS

- PLANT BEHAVIOR UPON COMPLETE LOSS OF FEEDWATER
- PIPING LOAD DOWNSTREAM OF OPEN PORV
- IS VENDOR PROCEDURE TO LOCK OPEN PORV CORRECT
- BENEFIT OF PRIMARY COOLANT PUMPS RESTART FOLLOWING CORE UNCOVERY
- RETURN OF NATURAL CIRCULATION WHEN STEAM GENERATOR REFILLED

PIGGY-BACKED TESTS L9-1/L3-3

L9-1: LOSS OF ALL FEEDWATER WITH DELAYED SCRAM. CHALLENGES AND OPENS PORV UNDER HIGH PRIMARY SYSTEM PRESSURE. STEAM GENERATOR DRIES OUT AND NATURAL CIRCULATION IS LOST.

L3-3: LOCK OPEN PORV TO REDUCE PRIMARY PRESSURE. RESTART THE PRIMARY COOLANT PUMPS WHEN CORE TEMPERATURE RISES. REFLOOD STEAM GENERATOR SECONDARY TO STUDY REINITIATION OF NATURAL CIRCULATION.

CONCERNS

- o ESTABLISH SCALING BENCHMARK LOFT - PWR
- o RAPID COOLDOWN DATA NEEDED FOR CODE ASSESSMENT
- o UNDERSTAND PWR BEHAVIOR WITH REACTOR VESSEL BUBBLE

<u>PIGGY-BACKED TESTS L6-7/L9-2</u>

L6-7: TURBINE TRIP WITH STUCK-ON PRESSURIZER SPRAY AND STUCK-OPEN ATMOSPHERIC DUMP VALVE. SIMULATES ARKANSAS NUCLEAR ONE STARTUP INCIDENT RAPID COOLDOWN.

L9-2: CONTINUE RAPID COOLDOWN CAUSING REACTIVITY INSERTION AND UPPER PLENUM VOIDING AS IN ST. LUCIE INCIDENT.

CONCERNS

- FLOW MODELS AND TRANSITIONS WHICH OCCUR FOR INTERMEDIATE SIZE BREAKS
- EFFECTIVENESS OF HPIS AND LPIS FOR INTERMEDIATE BREAK
- PLANT BEHAVIOR FOLLOWING ACCUMULATOR LINE BREAK
- CLAD RESPONSE WHEN CORE UNCOVERS AT HIGH DECAY HEAT LEVEL

PIGGY-BACKED TESTS L5-1/L8-2

L5-1: ACCUMULATOR LINE BREAK - INTERMEDIATE BREAK SIZE LOCA WITH NO ACCUMULATOR INJECTION. LEADS TO ...

L8-2: SUSTAINED CORE DRYOUT DURING HIGH DECAY HEAT LEVEL. HPIS AND LPIS INJECTION MUST CONTROL AND MITIGATE EVENT.

CONCERNS

- o PEAK CLAD TEMPERATURE AND BEHAVIOR OF PRESSURIZED FUEL DURING DESIGN BASIS LARGE BREAK LOCA LOSS-OF-OFFSITE POWER
- o CLAD IMBEDDED THERMOCOUPLES TO BE USED TO COMPARE WITH SURFACE THERMOCOUPLE PERFORMANCE

L2-5: LARGE BREAK LOCA AT OFF-NORMAL INITIAL CONDITIONS WITH LOSS-OF-OFFSITE POWER LEADING TO PRIMARY COOLANT PUMP RUNDOWN AND DELAYED ECC INJECTION. CENTRAL FUEL BUNDLE PREPRESSURIZED TO 350 PSI, AND INSTRUMENTED WITH NEW CLAD-IMBEDDED THERMOCOUPLES. PEAK CLAD TEMPERATURE PREDICTED TO FALL BELOW YIELD POINT.

CONCERNS

- o FOUR OPERATIONAL TRANSIENTS HAVE LED TO IDENTIFICATION OF SEVERAL CODE DEFICIENCES AND SUBSEQUENT IMPROVEMENTS
- o TIME TO CRITICALITY FOR BORON DILUTION FROM COLD SHUTDOWN
- o BORATED/UNBORATED WATER MIXING

LA-10: OPERATIONAL TRANSIENT - BORON DILUTION FROM COLD SHUTDOWN.

CONCERN

- o NO ATWS HAS YET BEEN PERFORMED
- o AN ATWS, INITIATED BY A HIGH FREQUENCY EVENT SHOULD BE PERFORMED AND COMPARED WITH ATWS CODE PREDICTIONS

I 9-3: FOLLOWING A LOSS-OF-FEEDWATER, THE CONTROL RODS WILL BE PREVENTED FROM SCRAMMING.

CONCERN

- THE FIRST ATWS, L9-3, MAY RESULT IN UNEXPECTED EVENTS OR LEAD TO CODE CHANGES. TO VALIDATE ANY CHANGES, A SECOND, DIFFERENT ATWS MAY BE NECESSARY

LA-3: CONTINGENCY ATWS TO BE IDENTIFIED.

CONCERN

- SIX SMALL BREAK TESTS HAVE LED TO IDENTIFICATION OF CODE DEFICIENCIES AND SUBSEQUENT IMPROVEMENTS
- CODE IMPROVEMENTS SHOULD NOT MAKE CODES BREAK-DEPENDENT
- PUMPS OFF/ON POSITION [illegible] BE CONFIRMED FOR ANOTHER BREAK SIZE

LA-9: SMALL BREAK LOCA WITH PUMPS ON. BREAK SIZE AND INITIAL CONDITIONS DIFFERENT THAN L3-6 TO CONFIRM CONCLUSIONS OF L3-6 AND CODE/BREAK SIZE INDEPENDENCE

CONCERNS

- THE FIRST INTERMEDIATE BREAK SIZE TEST, L5-1, MAY INDICATE PROBLEMS OR NEEDS FOR CODE CHANGES. AN ADDITIONAL TEST MAY BE NEEDED TO VALIDATE ANY CHANGES MADE

LA-2: CONTINGENCY INTERMEDIATE SIZE LOCA - POSSIBLE: PRESSURIZER SURGE LINE BREAK.

CONCERNS

- HOW CONSERVATIVE IS OUR ANALYSIS OF THE LARGE BREAK DESIGN BASIS ACCIDENT?
- HOW DOES CLADDING FAILURE DEVELOP IN A BUNDLE UNDERGOING MASSIVE BALLOONING AND BURST?
- HOW EFFECTIVE IS THE ECC UNDER THESE CONDITIONS?
- HOW DO THE FISSION PRODUCTS DISPERSE?

L2-6: LARGE BREAK LOCA WITH LOSS-OF-OFFSITE POWER AND EXTENDED ECC DELAY. FUEL BUNDLE IS PREPRESSURIZED TO 500 PSI, AND INSTRUMENTED WITH CLAD-IMBEDDED THERMOCOUPLES. ECC WILL BE DELAYED TO INSURE BALLOONING AND BURST OCCUR GENERALLY THROUGHOUT THE CENTRAL ASSEMBLY.

1-8, 9

OVERVIEW

USI-A1, WATER HAMMER

CURRENT STATUS

- NO EXPERIMENTAL RESEARCH UNDERWAY
- WATER HAMMER SURVEYS, EVALUATIONS AND ANALYSES COMPLETED AND REPORTED TO NRR
- NRR VIEW IS WATER HAMMER EFFECTS AS NOT REPRESENTING A SERIOUS OR IMMEDIATE SAFETY THREAT

OUTSTANDING ISSUES

- o NRR ACTION ON FINDINGS TO DATE
- o WHETHER EXPERIMENTAL EFFORT ADDRESSING STEAM BUBBLE COLLAPSE SHOULD BE PURSUED?

USI-A1 Evaluation Reports

NUREG-0582, Water Hammer in Nuclear Power Plants (July 1979)

CAAP-TR-053 (Rev. 1) Water Hammer Studies (July 1980)

CAAP-TR-042 (Rev. 1) Review and Evaluation of Actual and Potential Water Hammer Events in Nuclear Plants (September 1979)

RE-A-79-044, A State-of-the-Art Literature Review of Water Hammer (April 1979)

NUREG/CR-1606, An Evaluation of Condensation - Induced Water Hammer in Preheat Steam Generators (September 1980)

RE-E-79-009, An Analysis Tool for Predicting the Transient Hydrodynamics Resulting from the Rapid Filling of Voided Piping Systems (February 1979)

RE-A-74-013, An Analytical Procedure for Performing Structural Analyses of Nuclear Piping Systems Subjected to Fluid Transients (February 1979)

RE-A-78-229, An Investigation of the Steam Void Collapse Water Hammer Initiating Event (February 1979)

RE A-78-261 (Rev. 2), An Analysis Tool for Predicting Transient Hydrodynam in Nuclear Piping Systems Containing Swing Check Valves (September 1979)

EG/CAAP-5733, Supplemental Water Hammer Analysis and Systems Review (July 1980)

TABLE 1

OVERVIEW OF KEY FINDINGS (REFERENCE: USI-A1 EVALUATION REPORTS)

Reference	Study Objective	Key Findings Overview
NUREG-0582, Water Hammer in Nuclear Power Plants (July 1979)	Staff review of water hammer events, classification of problems and recommendation for action(s).	Endorsed technical studies contained in Task 4.0 of TAP-A1, recommended reviews of experimental data and analytical methods, revisions of REG guides, etc.
CAAP-TR-053 (Rev. 1) Water Hammer Studies (July 1980)	Followup review on water hammer incidents by EG&G.	1. PWR S.G. feedline water hammer events most damaging. 2. Flow into void lines, water entrainment in steam line and steam bubble collapse noted as occurring in safety systems. 3. Judgment made that computer codes for analyzing certain types of water hammer events exist, although these codes have not been compared with applicable test data. 4. Rate of condensation in steam bubble collapse is not easily determined, and therefore, peak pressure loads from slug inpact is not predictable within current state-of-the-art
CAAP-TR-042 (Rev. 1) Review and Evaluation of Actual and Potential Water Hammer Events in Nuclear Plants (September 1979)	Review and evaluation of actual and potential water hammer events (BWR and PWR).	1. Water entrainment and vapor bubble collapse can result in potentially damaging water hammers. 2. Valve, or flow, instabilities noted as significant problem in PWR and BWR feedwater systems. 3. Recommendations made for design and operational (including operator actions) requirements which would protect nuclear plant against damaging water hammer.
RE-A-79-044, A State-of-the-Art Literature Review of Water Hammer (April 1979)	Review current knowledge from experimental and analytical work reported.	1. Review of computational methods shows a gap between existing analysis capability and application. 2. Advanced best estimate codes have potential to analyze two-phase water hammer phenomena.
NUREG/CR-1606, An Evaluation of Condensation - Induced Water Hammer in Preheat Steam Generators (September 1980)	Evaluate potential for condensation induced water hammer, utilize 1/8 scale W test data.	1. State-of-the-art on condensation heat transfer prevents deriving a credible scaling criteria. 2. W and CE S.G. design review resulted in recommendation that each plant be reviewed separately and that verification tests be performed.

TABLE 1 (CONTINUED)

OVERVIEW OF KEY FINDINGS (REFERENCE: USI-A1 EVALUATION REPORTS)

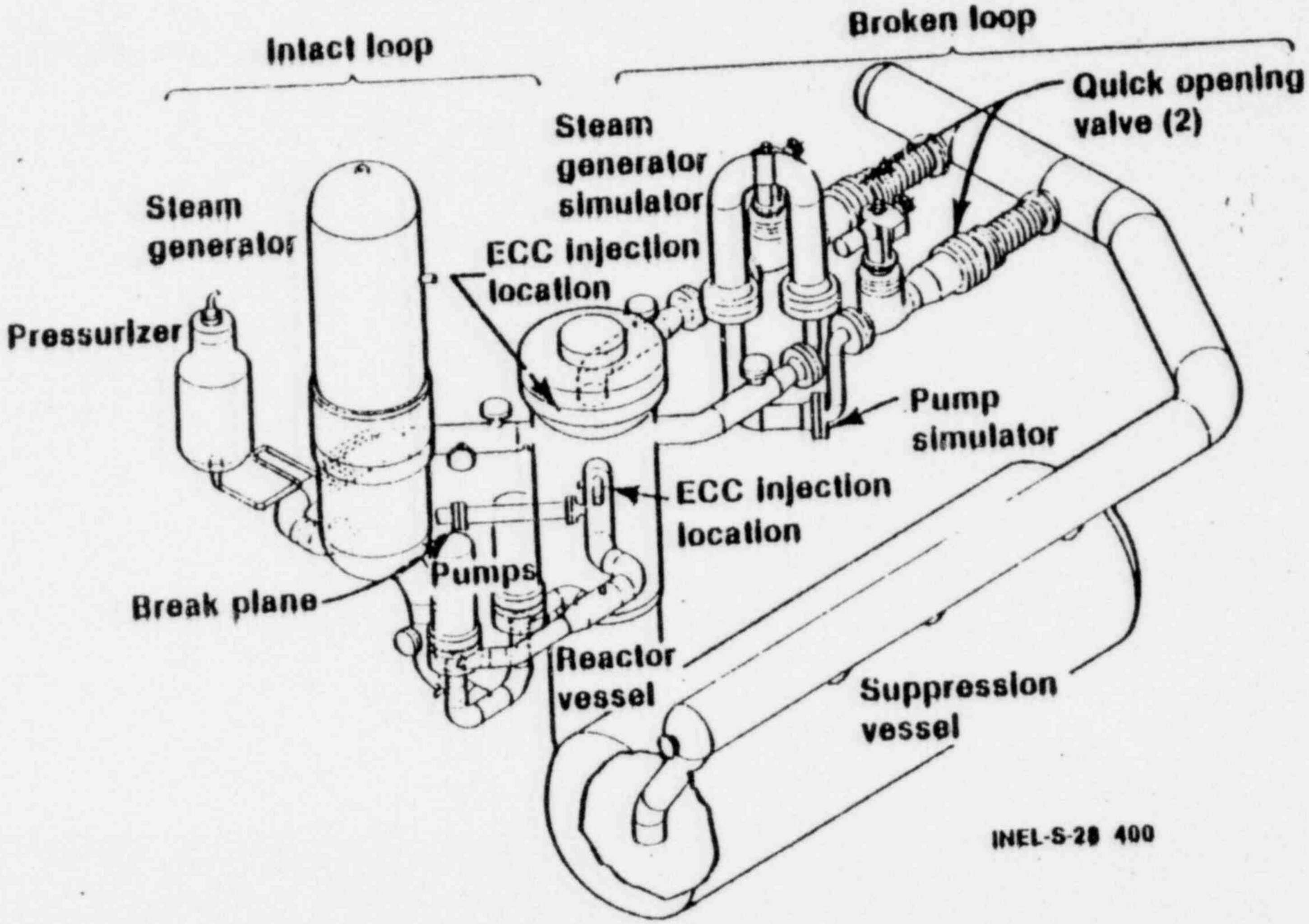
Reference	Study Objective	Key Findings Overview
RE-E-79-009, An Analysis Tool for Predicting the Transient Hydrodynamics Resulting from the Rapid Filling of Voided Piping Systems (February 1979)	Analysis of rapid filling of voided piping systems (BWR core spray line filling was analyzed).	1. Results demonstrate applicability of a modified SOLA-PLOOP hydrodynamics code. 2. Recommendation made for experimental verification of analytical tool.
RE-A-74-013, An Analytical Procedure for Performing Structural Analyses of Nuclear Piping Systems Subjected to Fluid Transients (February 1979)	Formulate analytical procedure to predict structural sequences of fluid transients in nuclear piping systems.	1. Analytical proceedure demonstrated for: a. Sudden check valve closure of a BWR primary feedwater line, and b. Simulated BWR core spray line experiencing an instantaneous valve opening. 2. Findings show significant loads on piping systems
RE-A-78-229, An Investigation of the Steam Void Collapse Water Hammer Initiating Event (February 1979)	Investigate steam void collapse water hammer initiating mechanisms.	1. Use of K-FIX/MOD1 judged inadequate due to treatment of interphase heat transfer and mass transfer. 2. Recommendation made to analyze experimental data with advanced codes like TRAC or THERMIT.
RE-A-78-261 (Rev. 2), An Analysis Tool for Predicting Transient Hydrodynamics in Nuclear Piping Systems Containing Swing Check Valves (September 1979)	Analysis tool constructed for analyzing fluid transients in piping systems having a check valve.	1. RELAP5 adapted to model check valve, BWR primary feedwater line transient calculation performed. 2. Future experimental verification noted as desirable.
EG-/CAAP-5733, Supplemental Water Hammer Analysis and Systems Review (July 1980)	Additional water hammer analysis and system review effort (check valve closure and slug flow in voided lines).	1. Effect of assumed valve opening times (1 versus 5 seconds) has significant effort. 2. Evaluation of 16 water hammer scenarios judged to have a highly unlikely level of occurrence.

LOFT PUMPS ON/PUMPS OFF
EXPERIMENT RESULTS
AND
COMPARISON TO CODE CALCULATIONS

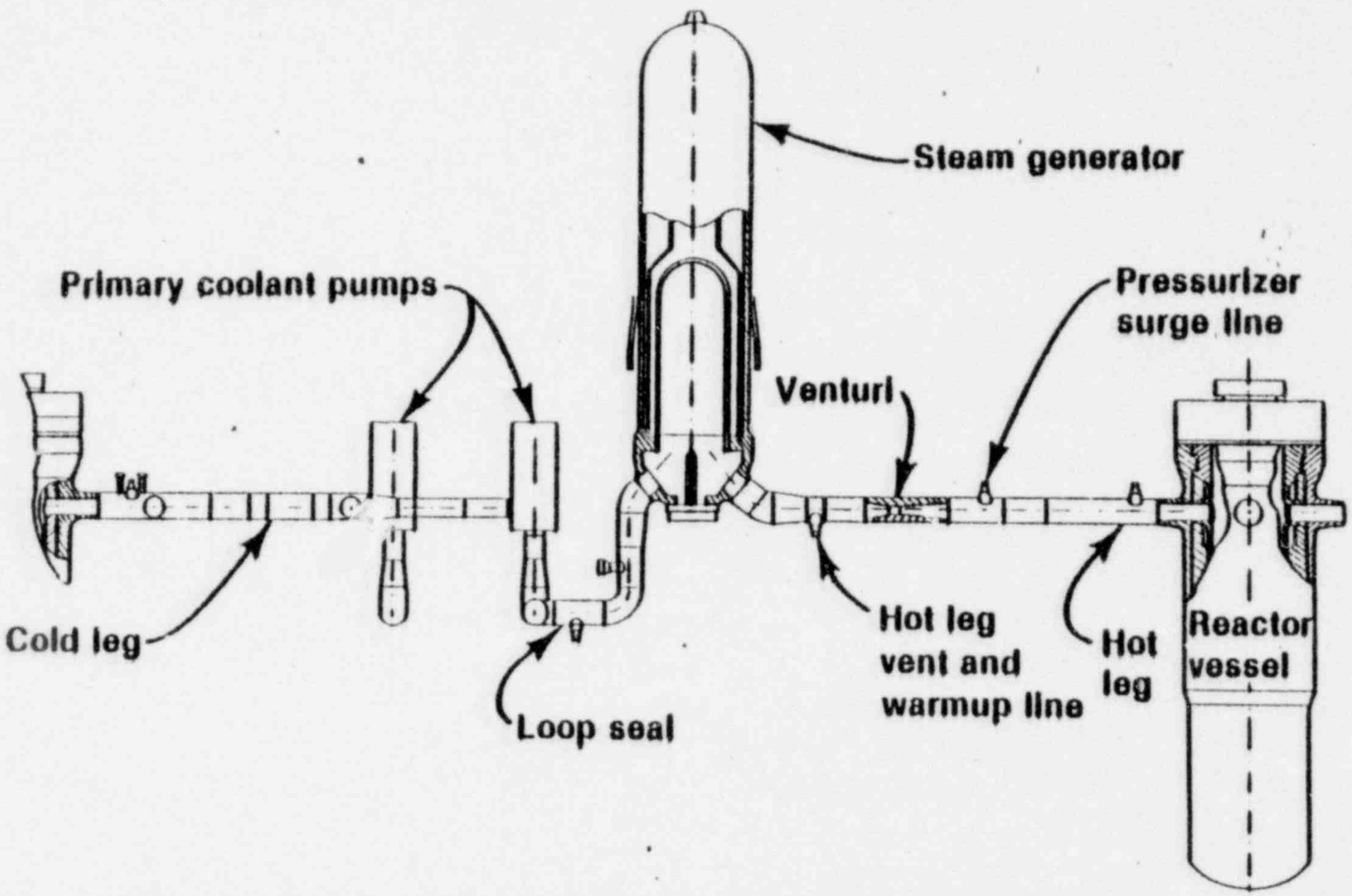
KEITH CONDIE

CONTENTS

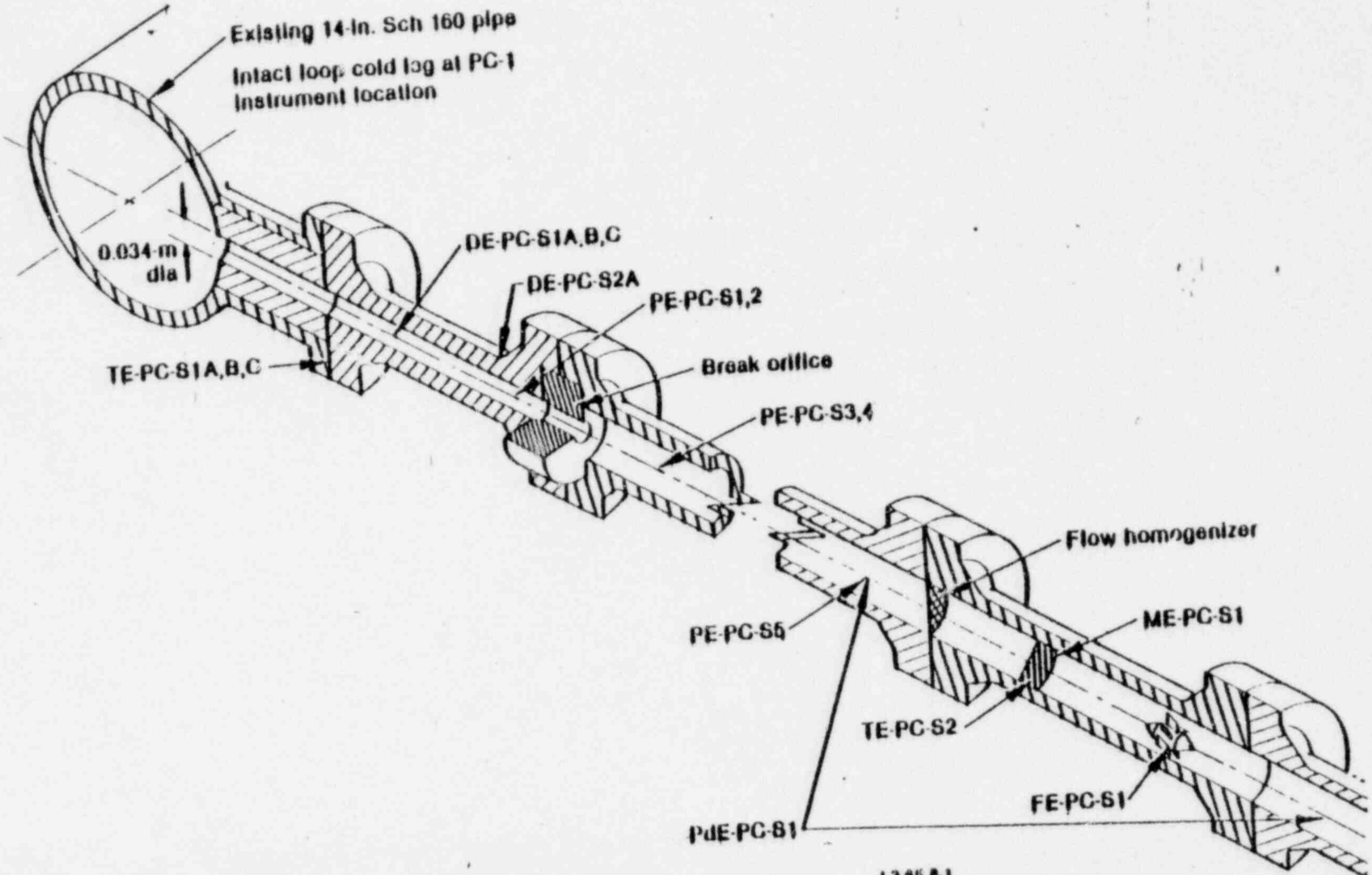
LOFT Primary System



LOFT Intact Loop



INEL-S-25 495-1



LS 6/A 8-1
INEL-A-16 907

L3-5, L3-6 TYPICAL UNCERTAINTIES

TEMPERATURE:	±6°F
PRESSURE:	±32 psi
DENSITY:	±10 lbm/ft3
DIFFERENTIAL TEMPERATURE:	±1°F
MASS INVENTORY:	±660 lbm

TYPICAL UNCERTAINTIES (continued)

PRESSURE
DIFFERENTIAL: ±0.3 psi

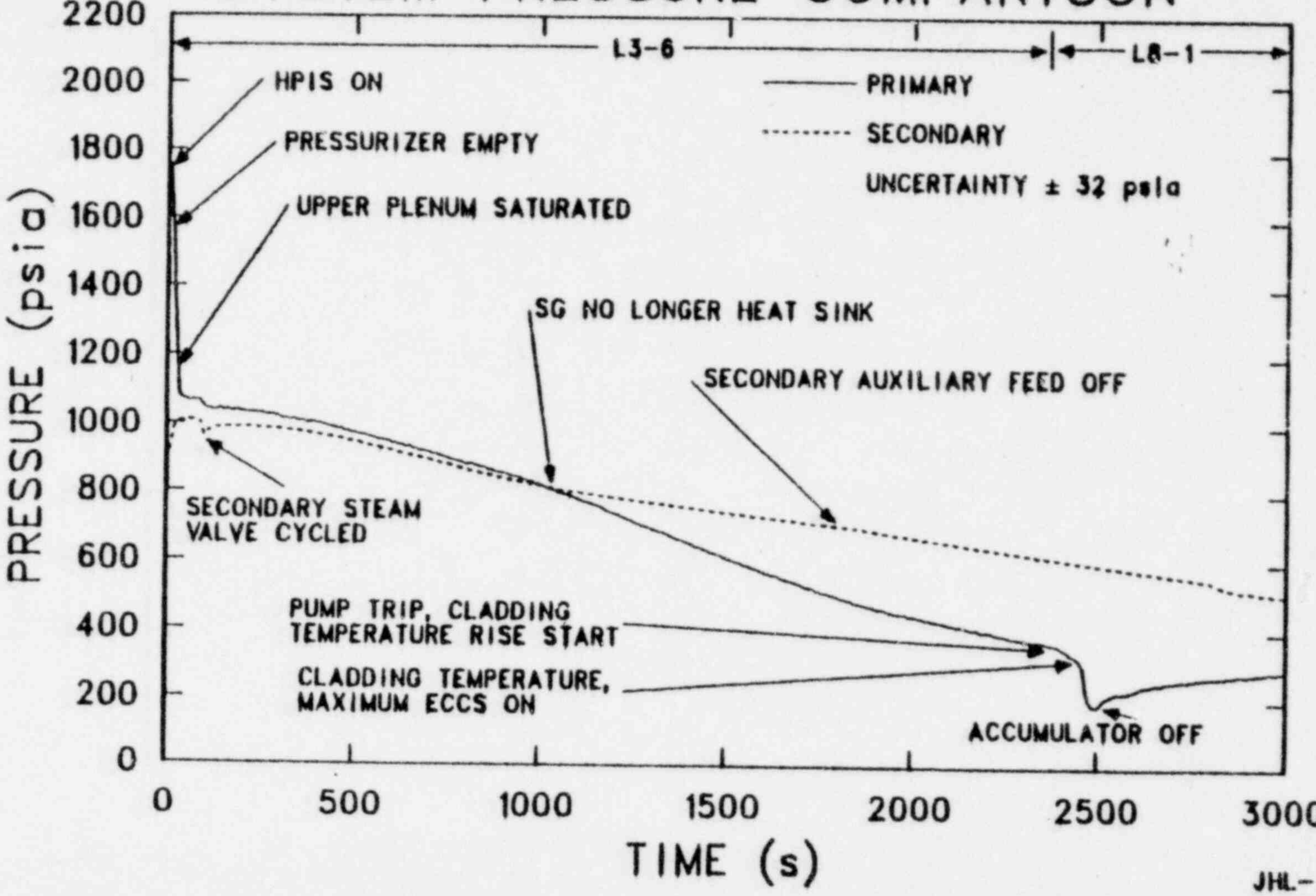
BREAK MASS FLOW:

t = 40-750s	L3-5	25% reading
t > 750s	L3-5	0.45 lbm/s
t = 40-1435s	L3-6	15% reading
t > 1435s	L3-6	0.34 lbm/s

INITIAL CONDITIONS

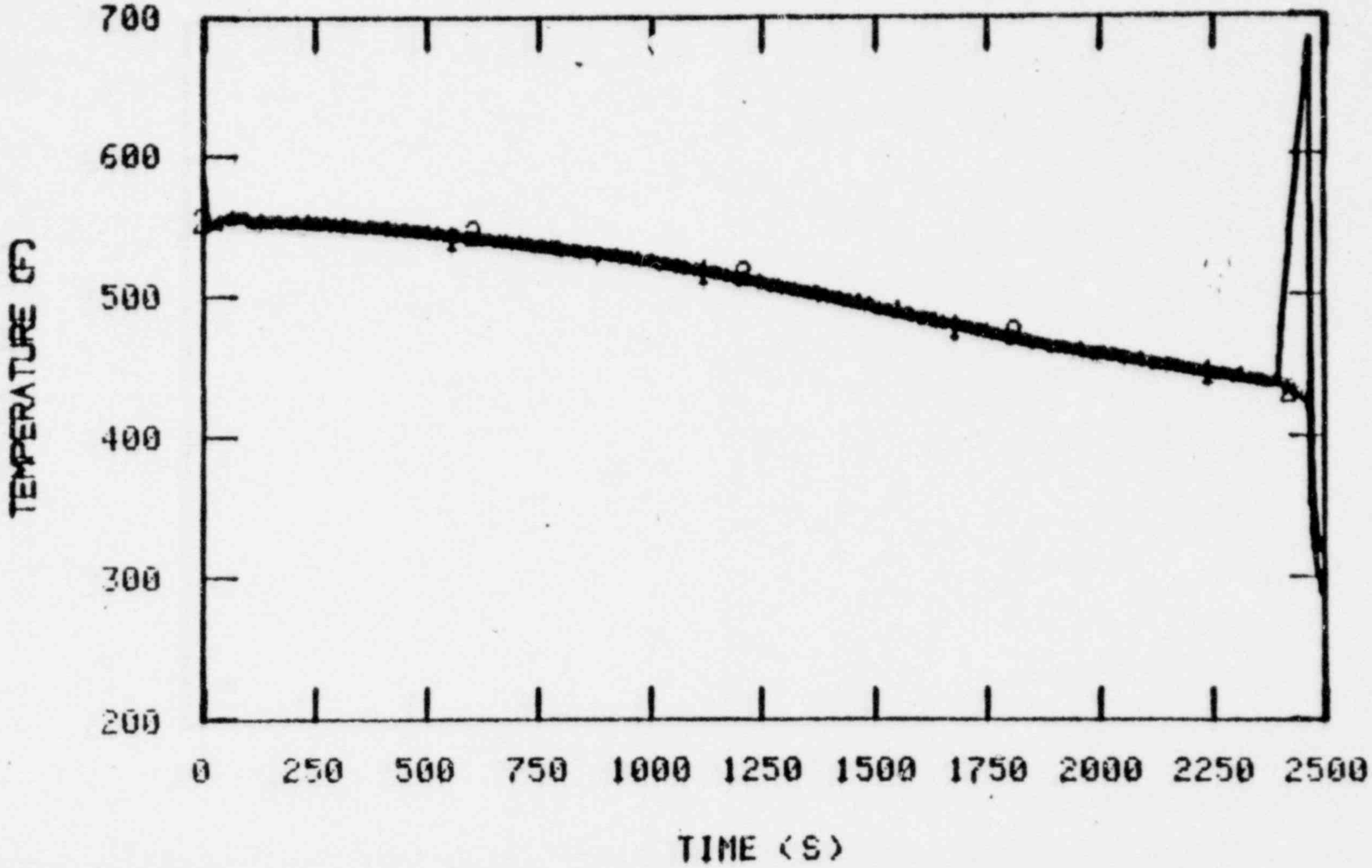
PARAMETER	L3-5/5A	L3-6/L8-1
PRESSURE (PSIA)	2154	2168
COLD LEG TEMP. (DEG. F)	545	544.5
CORE ΔT (DEG. F)	32	34.6
PCS FLOW RATE ($X\ 10^6$ LB/HR)	3.8	3.8
CORE POWER (MW)	49	50

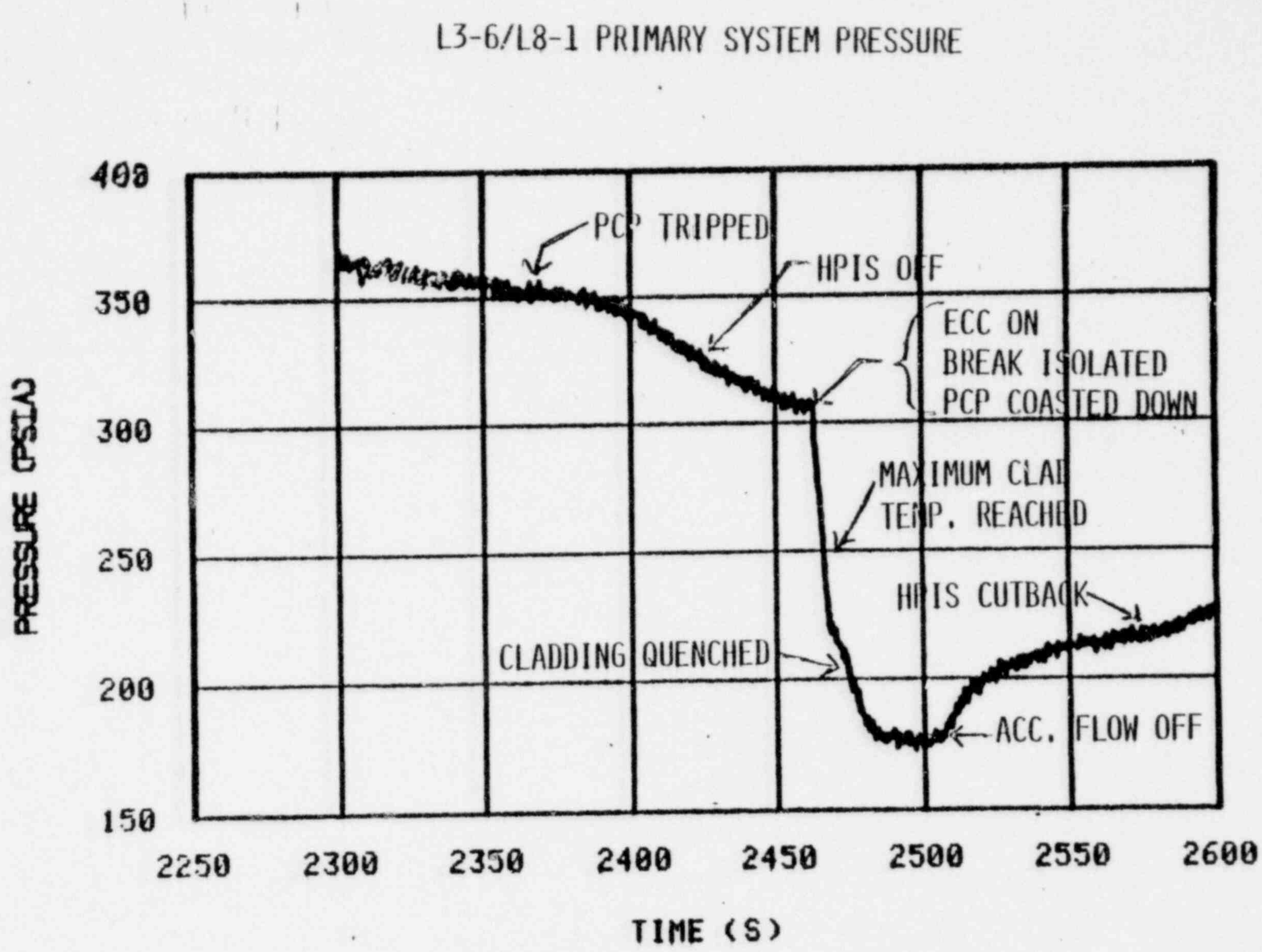
L3-6/L8-1 PRIMARY AND SECONDARY SYSTEM PRESSURE COMPARISON



JHL-181-4

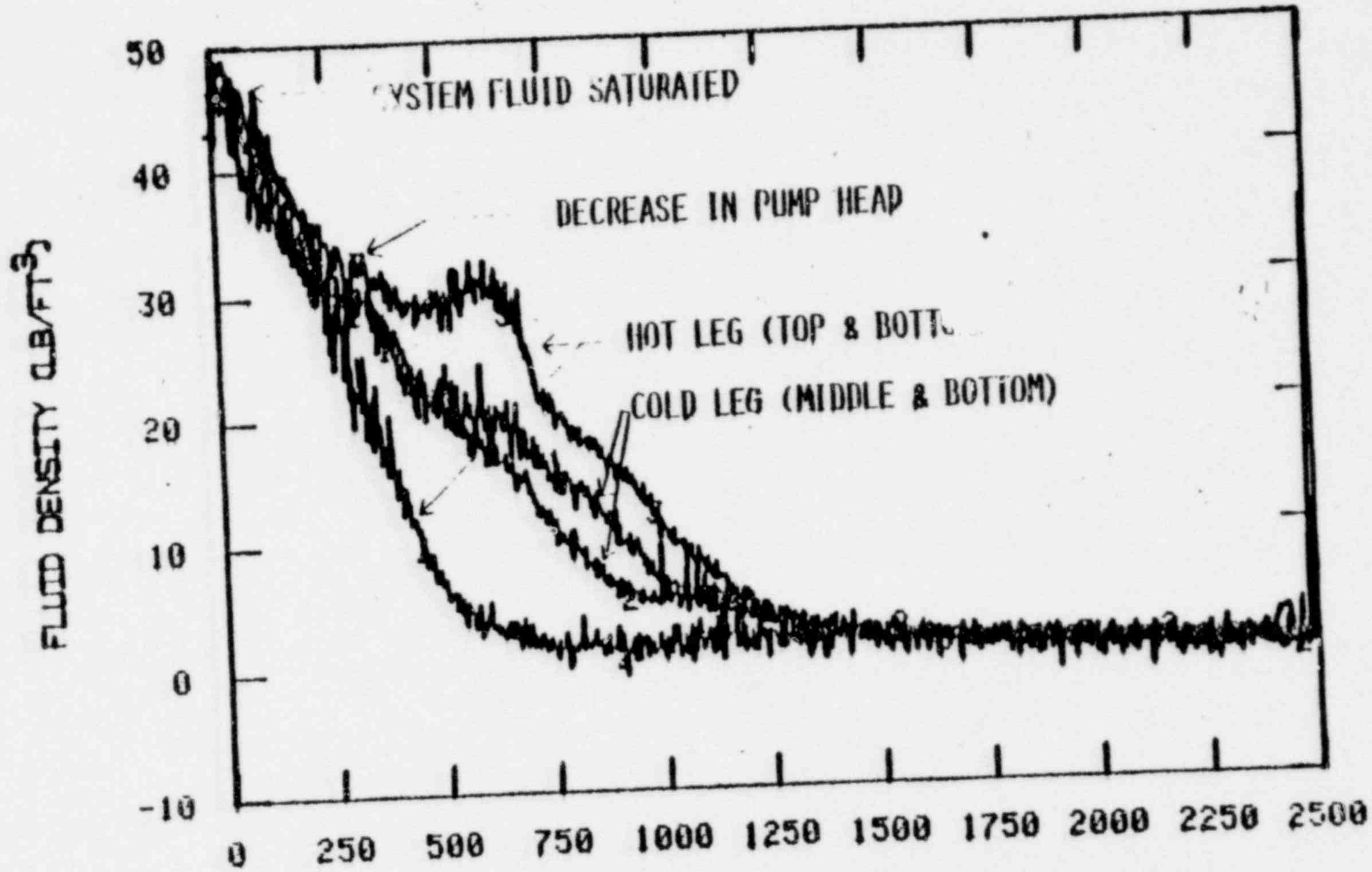
L3-6 FUEL CLADDING AND FLUID TEMPERATURES



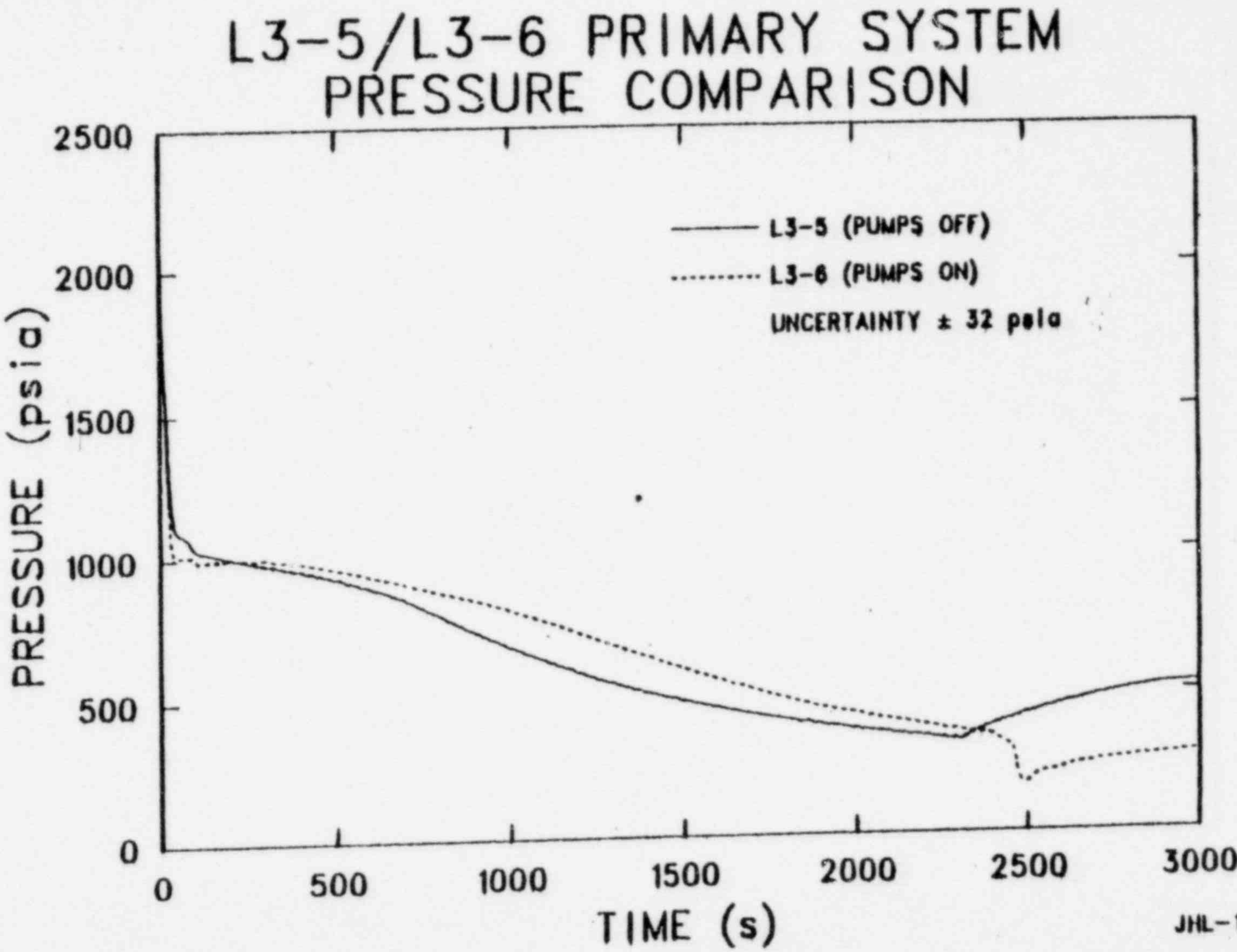


L3-6/L8-1 PRIMARY SYSTEM PRESSURE
PRESSURE (PSIA)
400
350
300
250
200
150
2250
2300
2350
2400
2450
2500
2550
2600
TIME (S)
PCP TRIPPED
HPIS OFF
ECC ON
BREAK ISOLATED
PCP COASTED DOWN
MAXIMUM CLAD
TEMP. REACHED
HPIS CUTBACK
CLADDING QUENCHED
ACC. FLOW OFF

INTACT LOOP HOT LEG AND COLD LEG DENSITIES

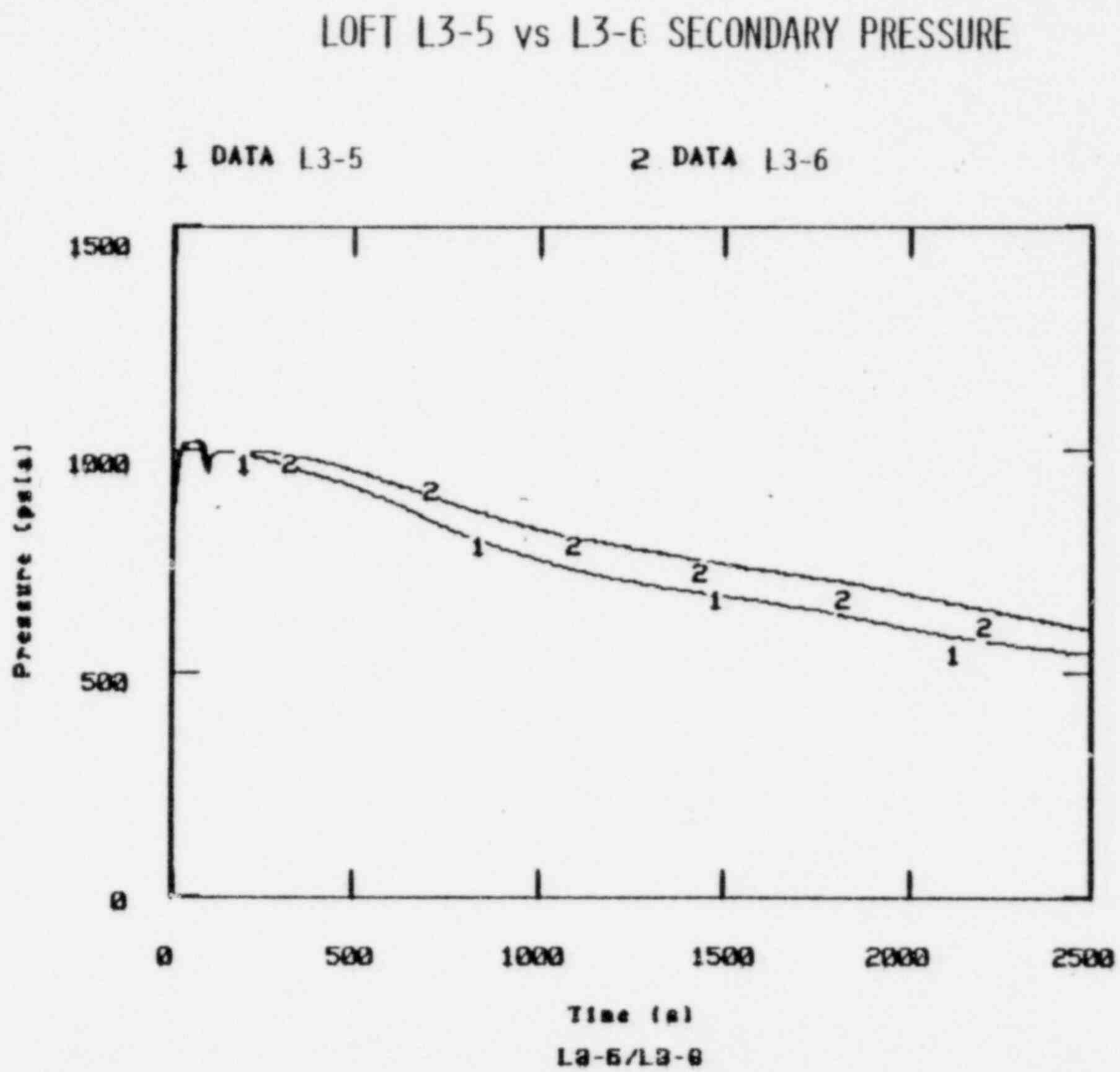


L3-6



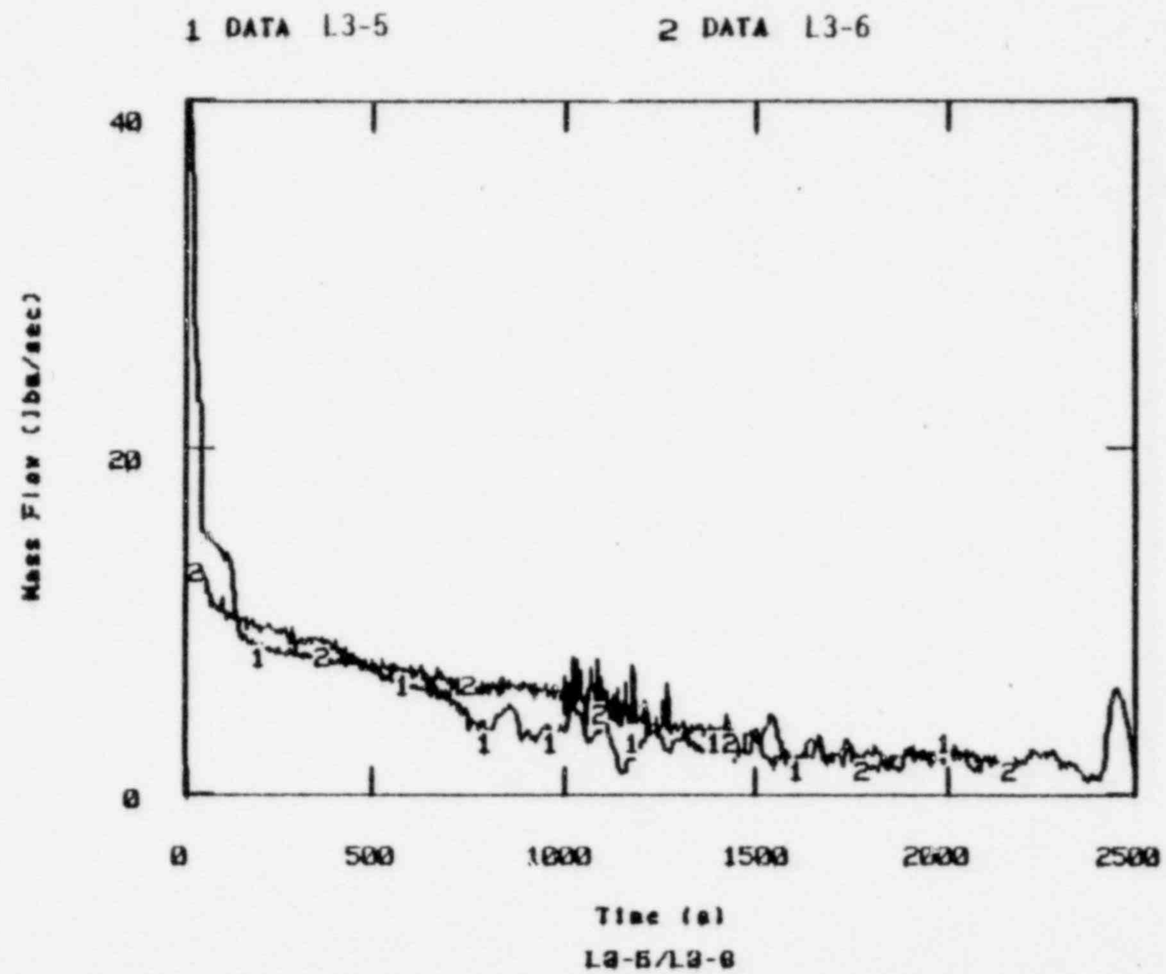
L3-5/L3-6 PRIMARY SYSTEM
PRESSURE COMPARISON
L3-5 (PUMPS OFF)
L3-6 (PUMPS ON)
UNCERTAINTY ± 32 psia
PRESSURE (psia)
2500
2000
1500
1000
500
0
0
500
1000
1500
2000
2500
3000
TIME (s)

JHL-181-5

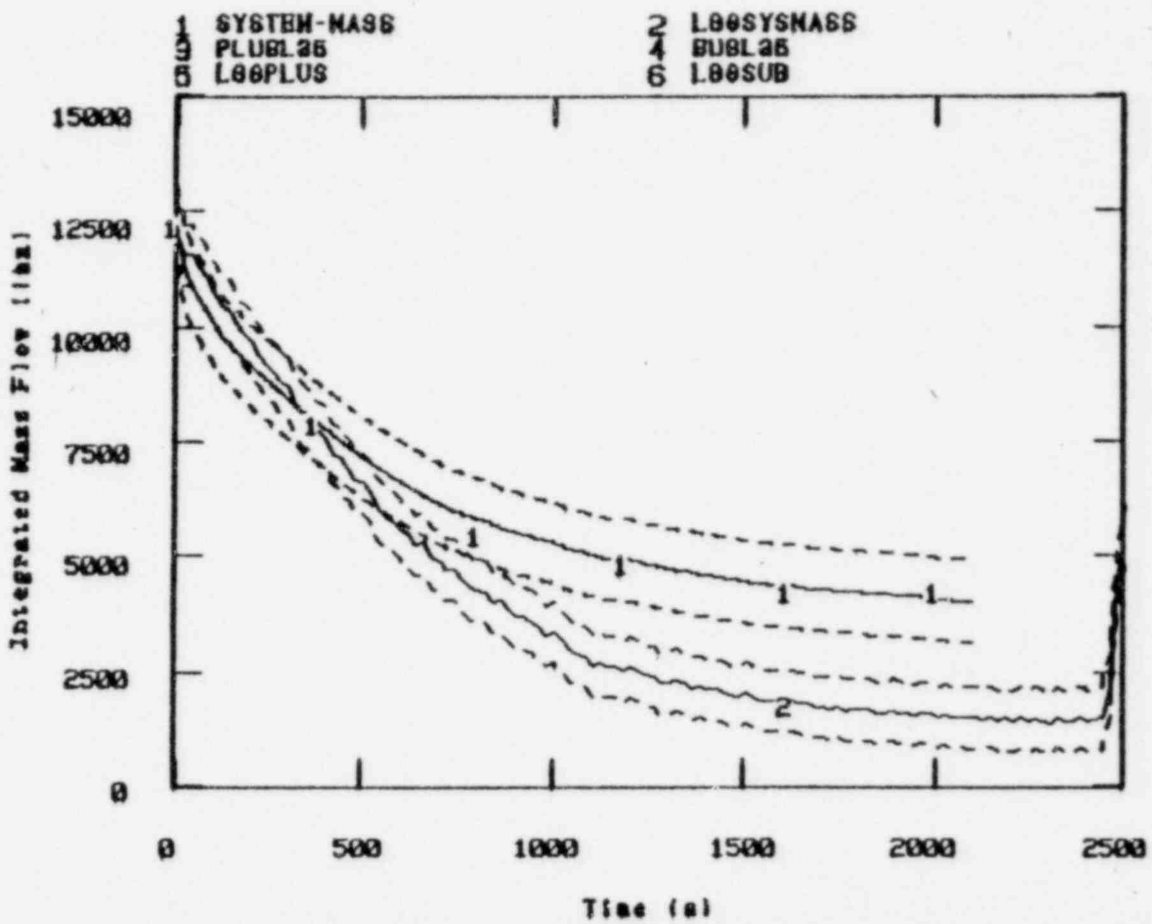


LOFT L3-5 vs L3-6 SECONDARY PRESSURE
1 DATA L3-5
2 DATA L3-6
Pressure (psia)
1500
1000
500
0
0
500
1000
1500
2000
2500
Time (s)
L3-5/L3-6

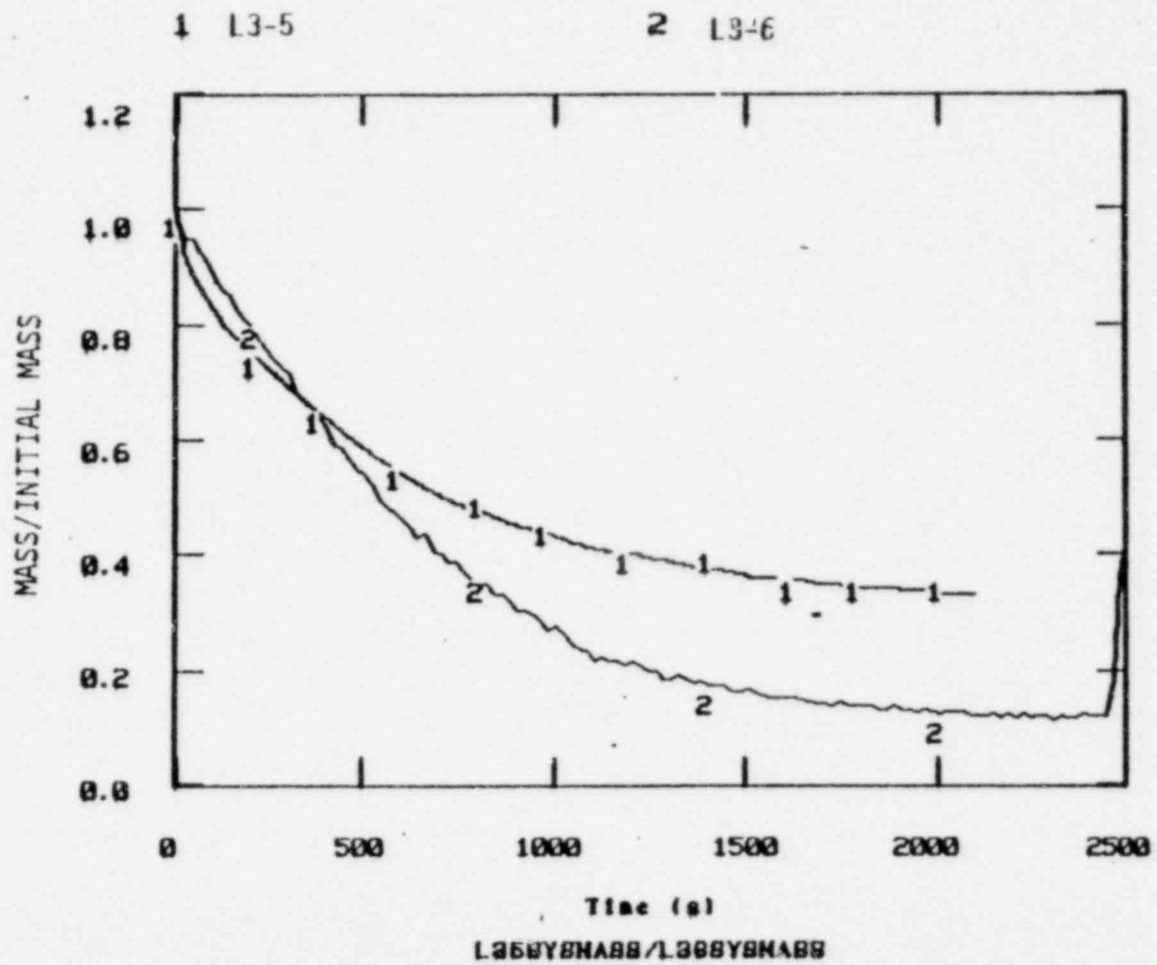
LOFT L3-5 vs L3-6 BREAK FLOW



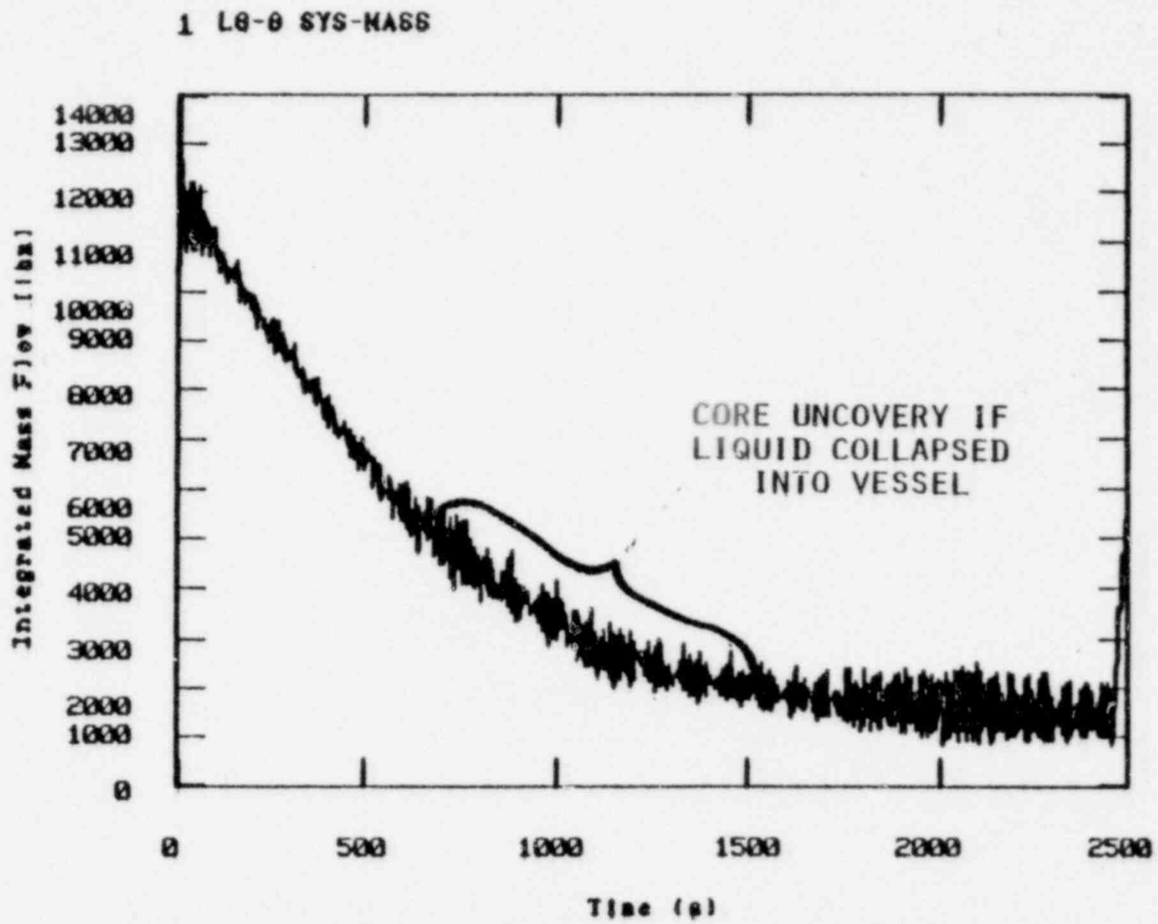
LOFT L3-5 vs L3-6 PRIMARY SYSTEM MASS WITH UNCERTAINTIES

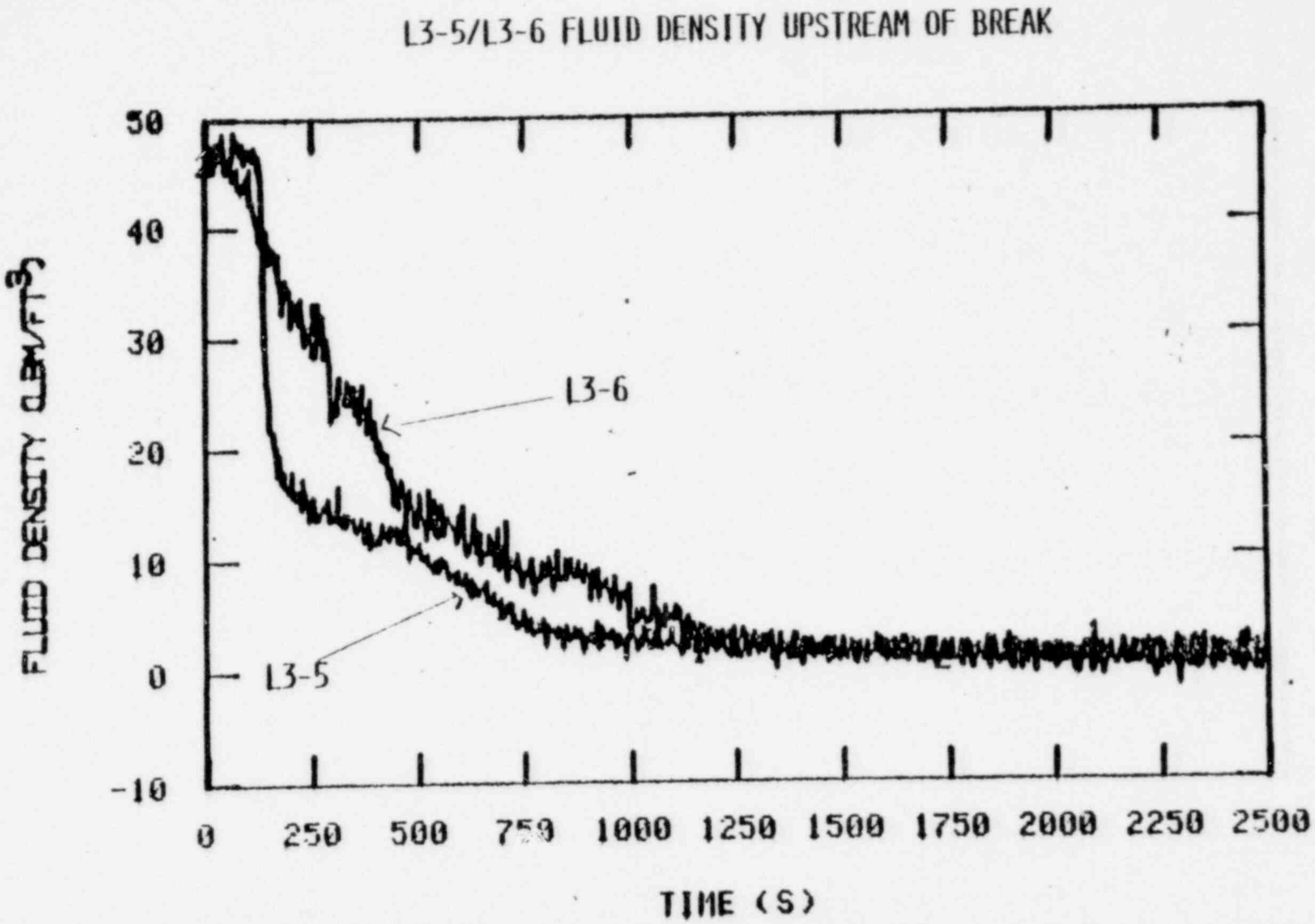


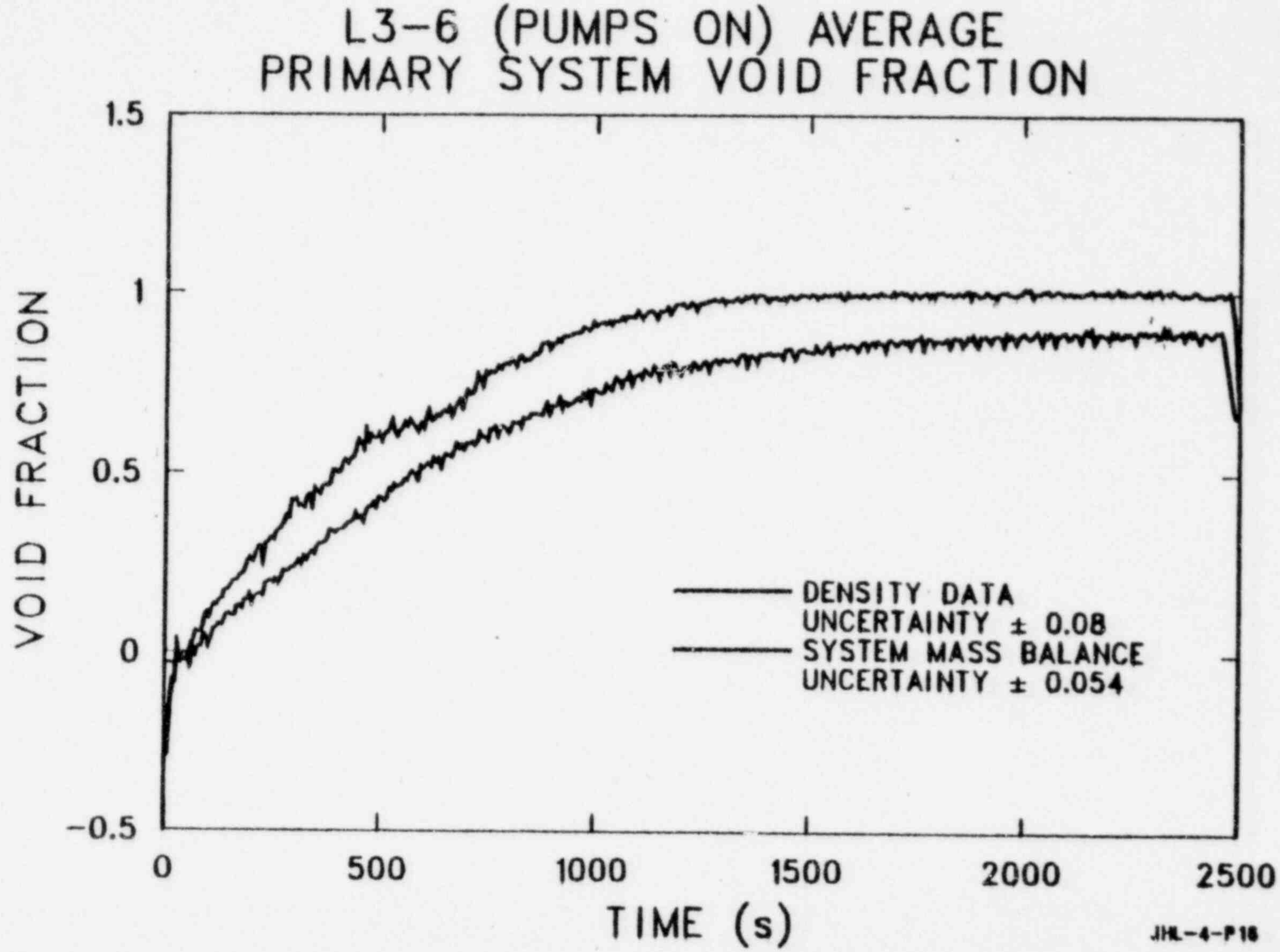
LOFT L3-5 vs L3-6 NORMALIZED PRIMARY SYSTEM MASS

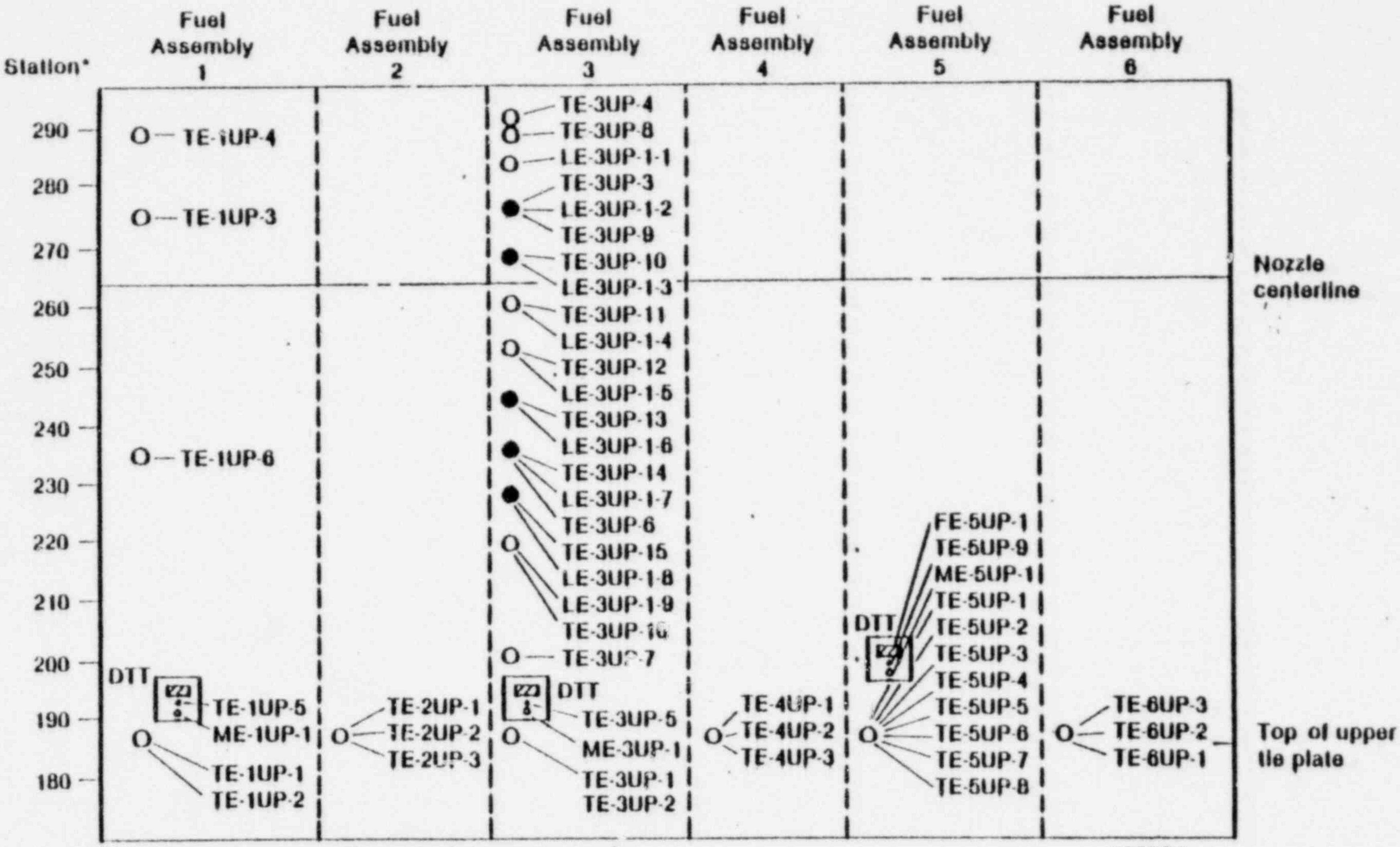


LOFT L3-6 PRIMARY SYSTEM MASS



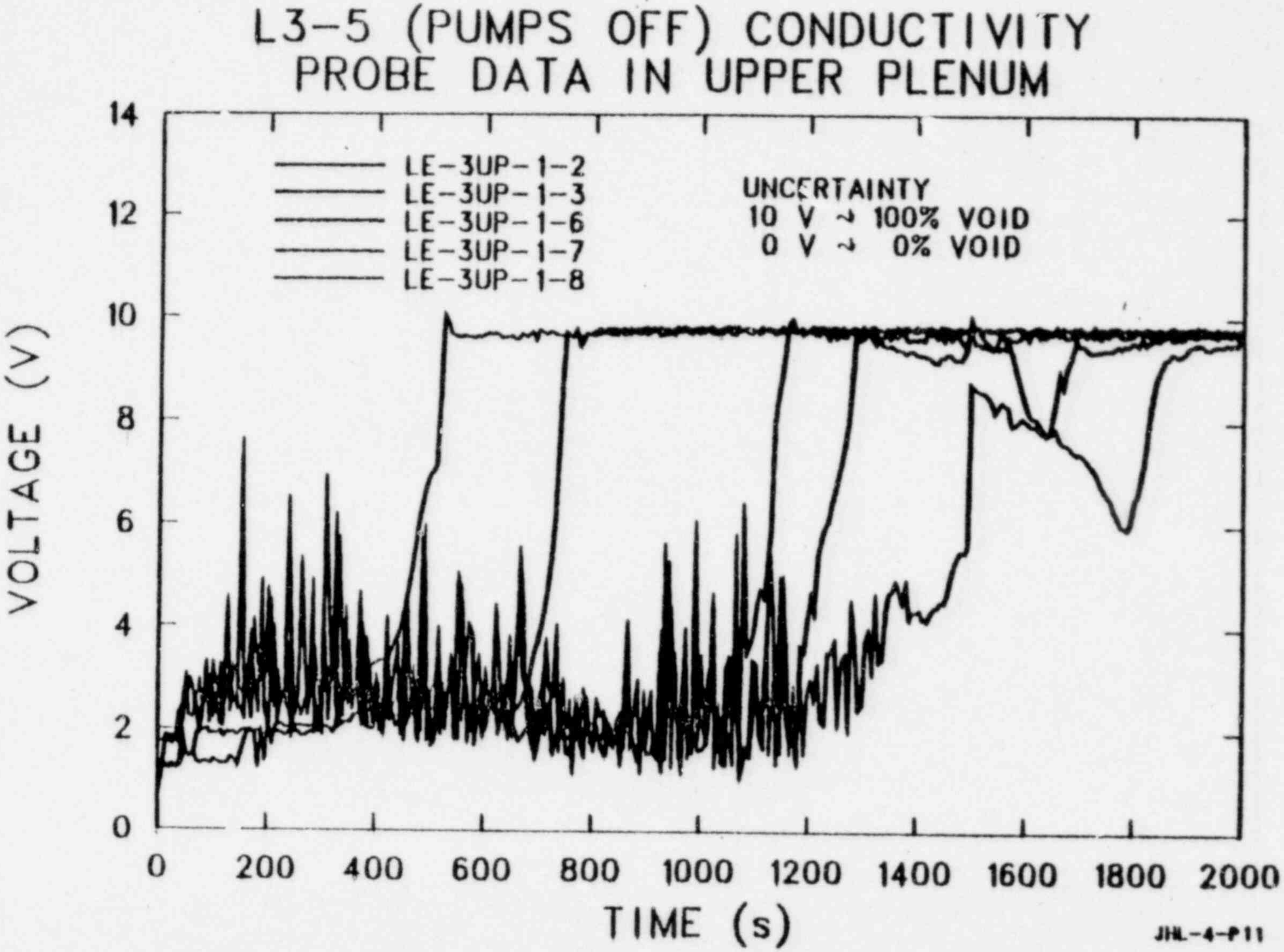

L3-5/L3-6 FLUID DENSITY UPSTREAM OF BREAK
FLUID DENSITY (LBM/FT3)
50
40
30
20
10
0
-10
0
250
500
750
1000
1250
1500
1750
2000
2250
2500
TIME (S)
L3-6
L3-5

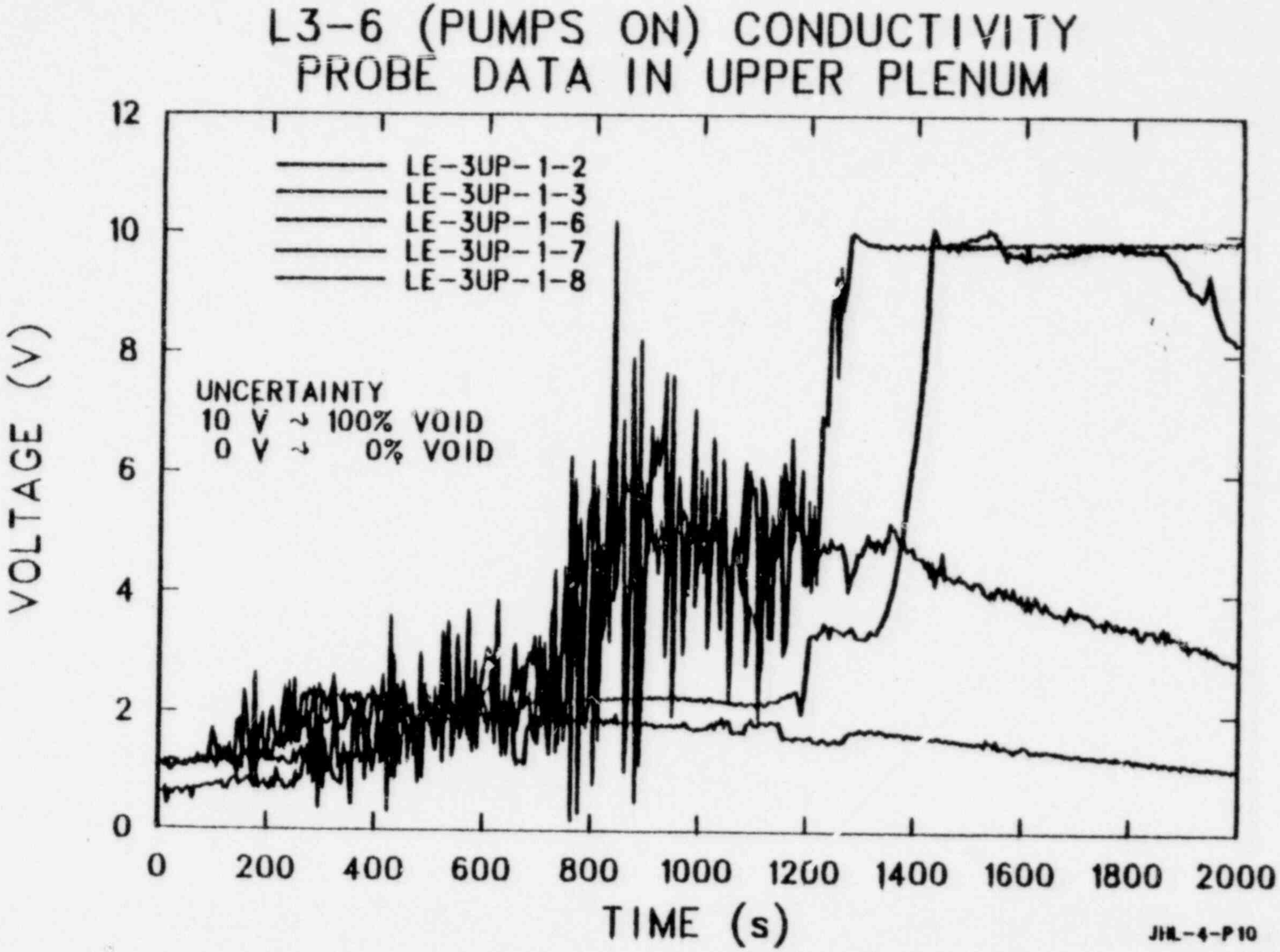

L3-6 (PUMPS ON) AVERAGE
PRIMARY SYSTEM VOID FRACTION
1.5
1
0.5
0
-0.5
VOID FRACTION
0
500
1000
1500
2000
2500
TIME (s)
DENSITY DATA
UNCERTAINTY ± 0.08
SYSTEM MASS BALANCE
UNCERTAINTY ± 0.054
JHL-4-P16



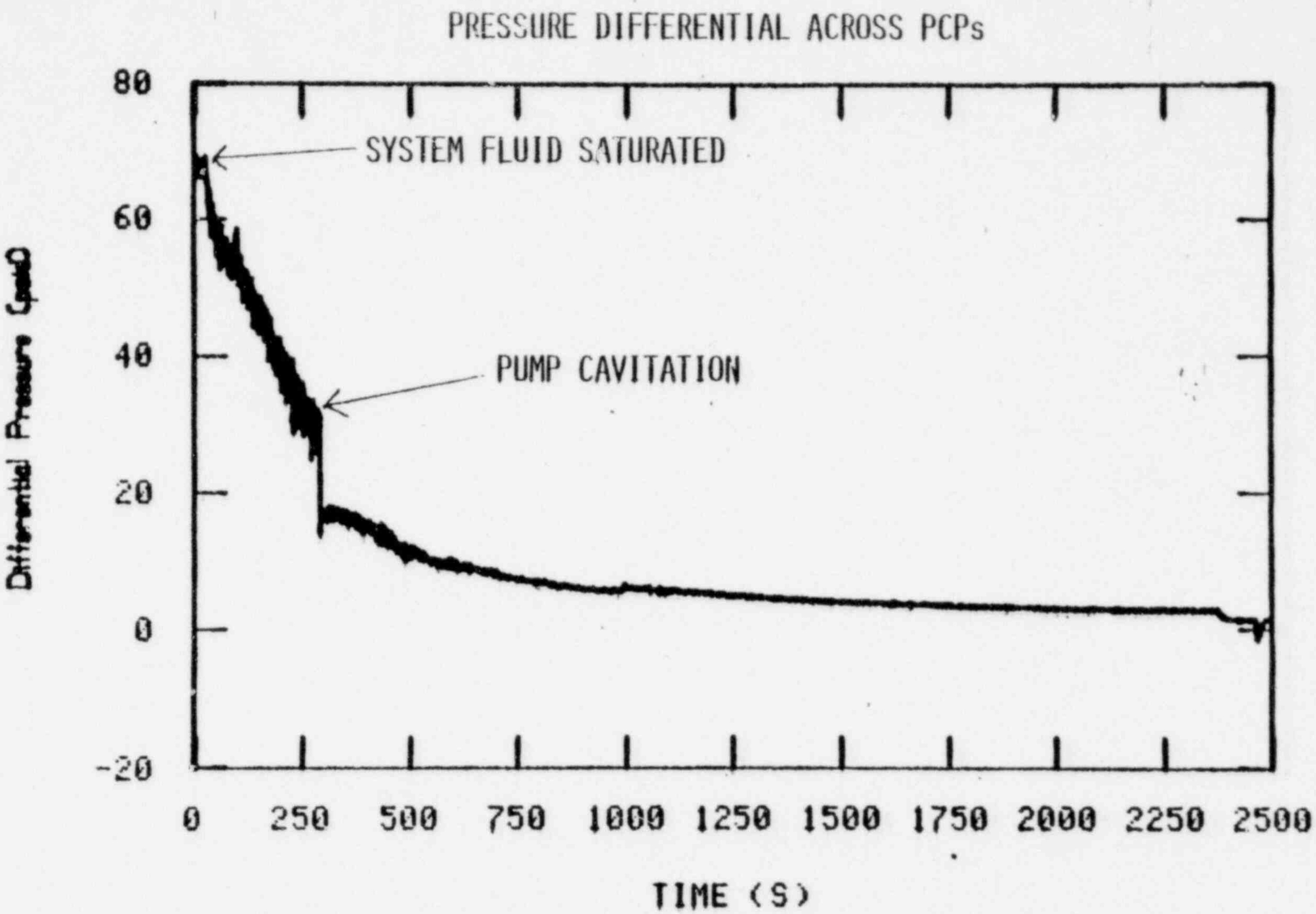
*Station numbers are a dimensionless measure of relative elevation within the reactor vessel. They are assigned in increments of 25.4 mm with station 300.00 defined at the core barrel support ledge inside the reactor vessel flange.

L3-6/L8-1
INEL-A-16 380


L3-5 (PUMPS OFF) CONDUCTIVITY
PROBE DATA IN UPPER PLENUM
LE-3UP-1-2
LE-3UP-1-3
LE-3UP-1-6
LE-3UP-1-7
LE-3UP-1-8
UNCERTAINTY
10 V → 100% VOID
0 V → 0% VOID
VOLTAGE (V)
0
2
4
6
8
10
12
14
0
200
400
600
800
1000
1200
1400
1600
1800
2000
TIME (s)
JHL-4-P11



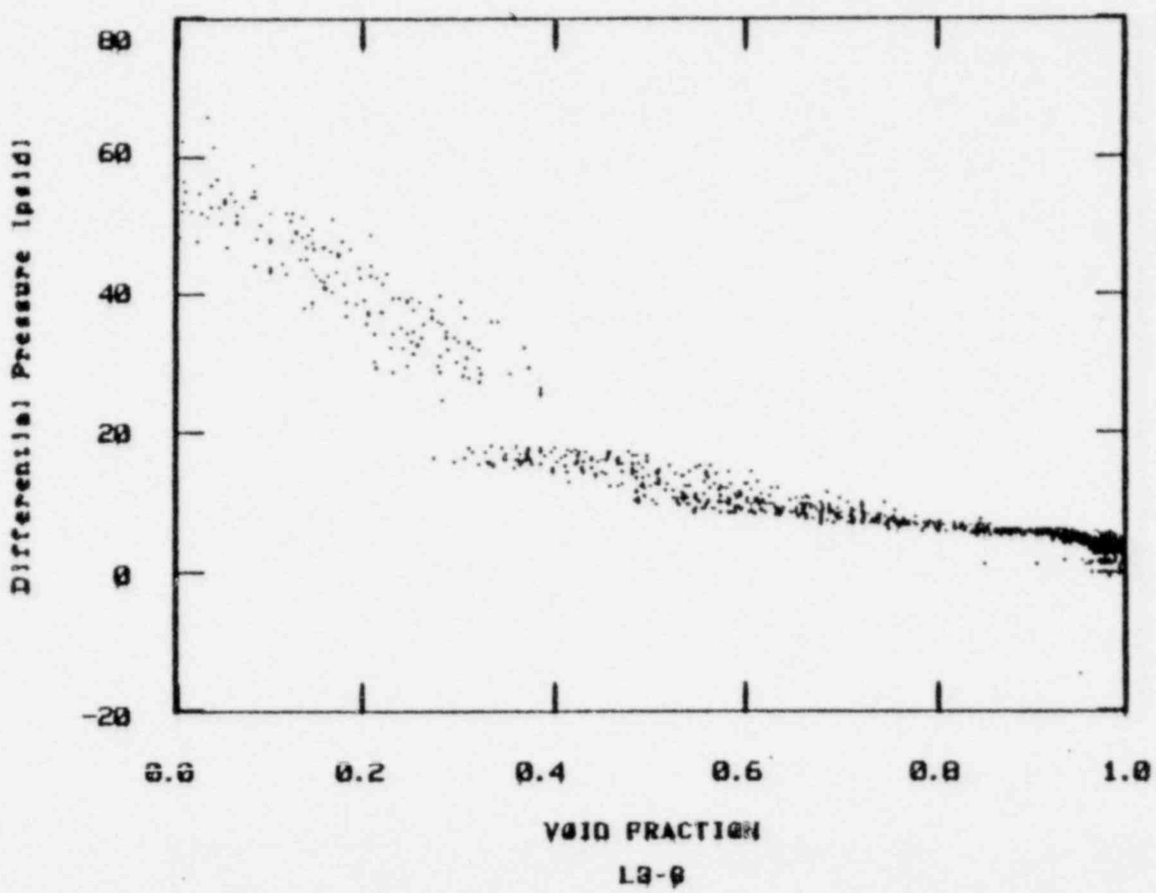
L3-6 (PUMPS ON) CONDUCTIVITY
PROBE DATA IN UPPER PLENUM
LE-3UP-1-2
LE-3UP-1-3
LE-3UP-1-6
LE-3UP-1-7
LE-3UP-1-8
UNCERTAINTY
10 V → 100% VOID
0 V → 0% VOID
VOLTAGE (V)
TIME (s)
0
2
4
6
8
10
12
200
400
600
800
1000
1200
1400
1600
1800
2000
JHL-4-P10



PRESSURE DIFFERENTIAL ACROSS PCPs
80
60
40
20
0
-20
Differential Pressure (psid)
SYSTEM FLUID SATURATED
PUMP CAVITATION
0
250
500
750
1000
1250
1500
1750
2000
2250
2500
TIME (S)

L3-6

LOFT L3-6 PUMP ΔP vs SYSTEM VOID



PUMP DELAYED TRIP SUMMARY

ASSUMED PUMP TRIP TIME (min)	CORE LIQUID LEVEL
0 to 2.5	Transients almost identical
10 to 17	Core uncovery initiation
19.5 (accumulator actuation pressure reached)	Core 9/10 - 1/2 full
22 to 26	Core completely uncovered

JL-4-8

PUMPS ON/PUMPS OFF SUMMARY

WITH PUMPS RUNNING:

- HIGHER BREAK FLOW, LOWER SYSTEM INVENTORY
- MORE UNIFORM FLUID DISTRIBUTION
- NO TRACEABLE LIQUID LEVEL
- COOLING CONTINUED AT HIGH SYSTEM VOIDS

LOFT EXPERIMENTS SUPPORT PUMPS OFF PROCEDURE

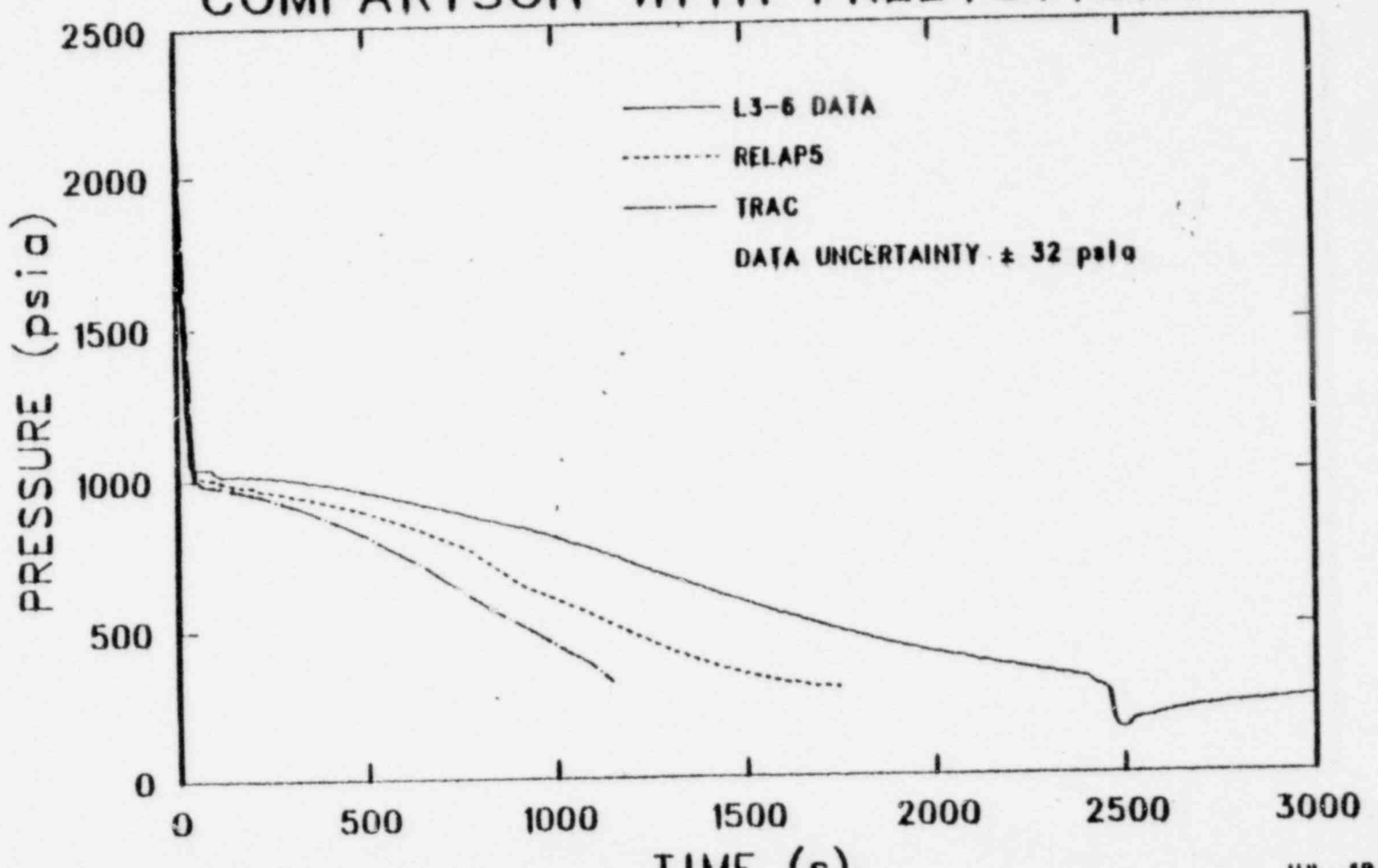
SUMMARY OF EXPERIMENTAL PREDICTIONS

- SYSTEM DEPRESSURIZATION RATE TOO HIGH
- OVERPREDICTION OF BREAK FLOW - L3-5 AND EARLY L3-6
- EARLY HOT LEG DENSITIES TOO LOW
- PRIMARY AND SECONDARY THERMALLY COUPLED IN L3-6

SUMMARY (continued)

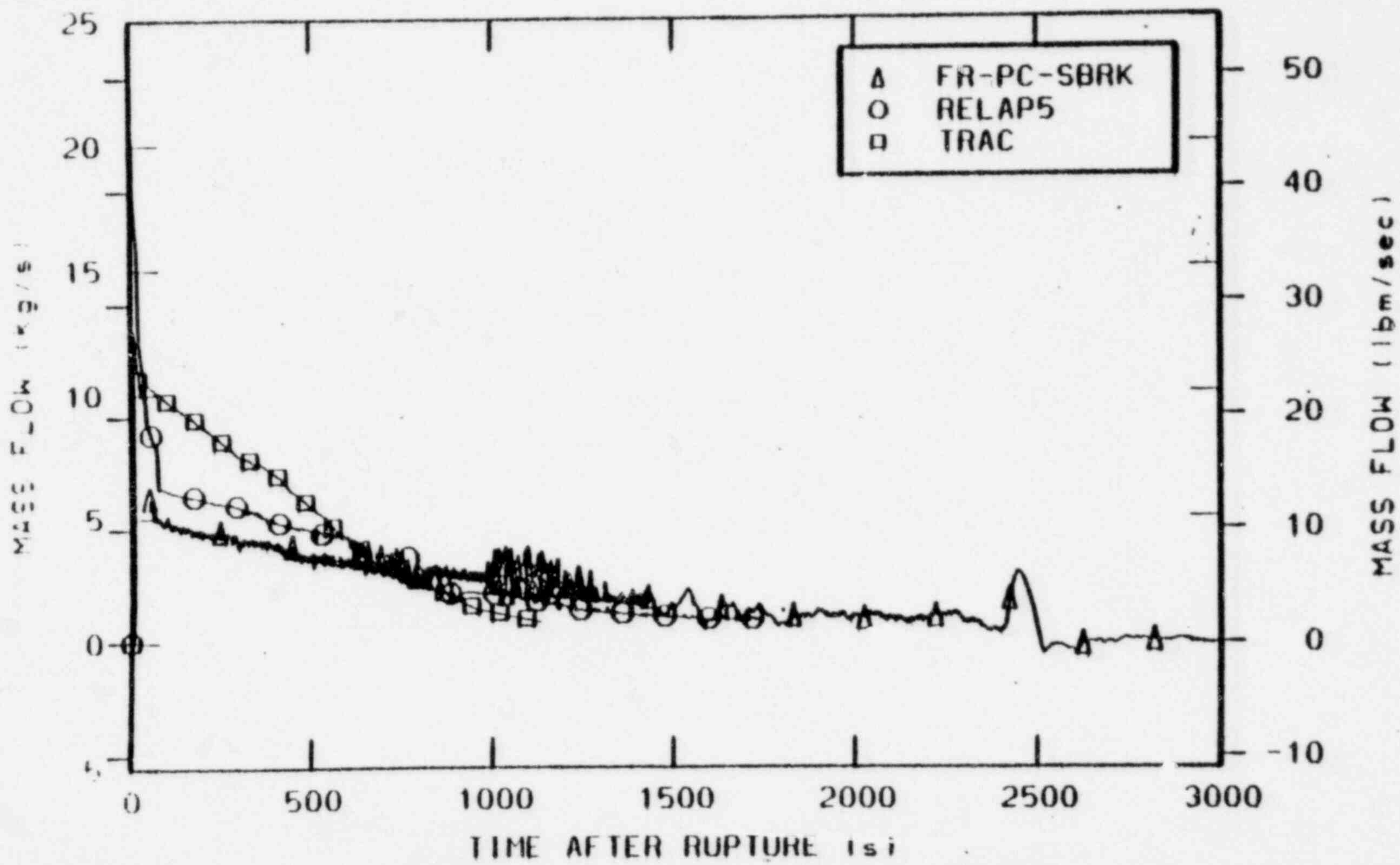
- PUMP TWO-PHASE DEGRADATION TOO HIGH
- LARGE MASS ERROR IN L3-5

L3-6 PRIMARY SYSTEM PRESSURE COMPARISON WITH PREDICTIONS



JHL-181-14

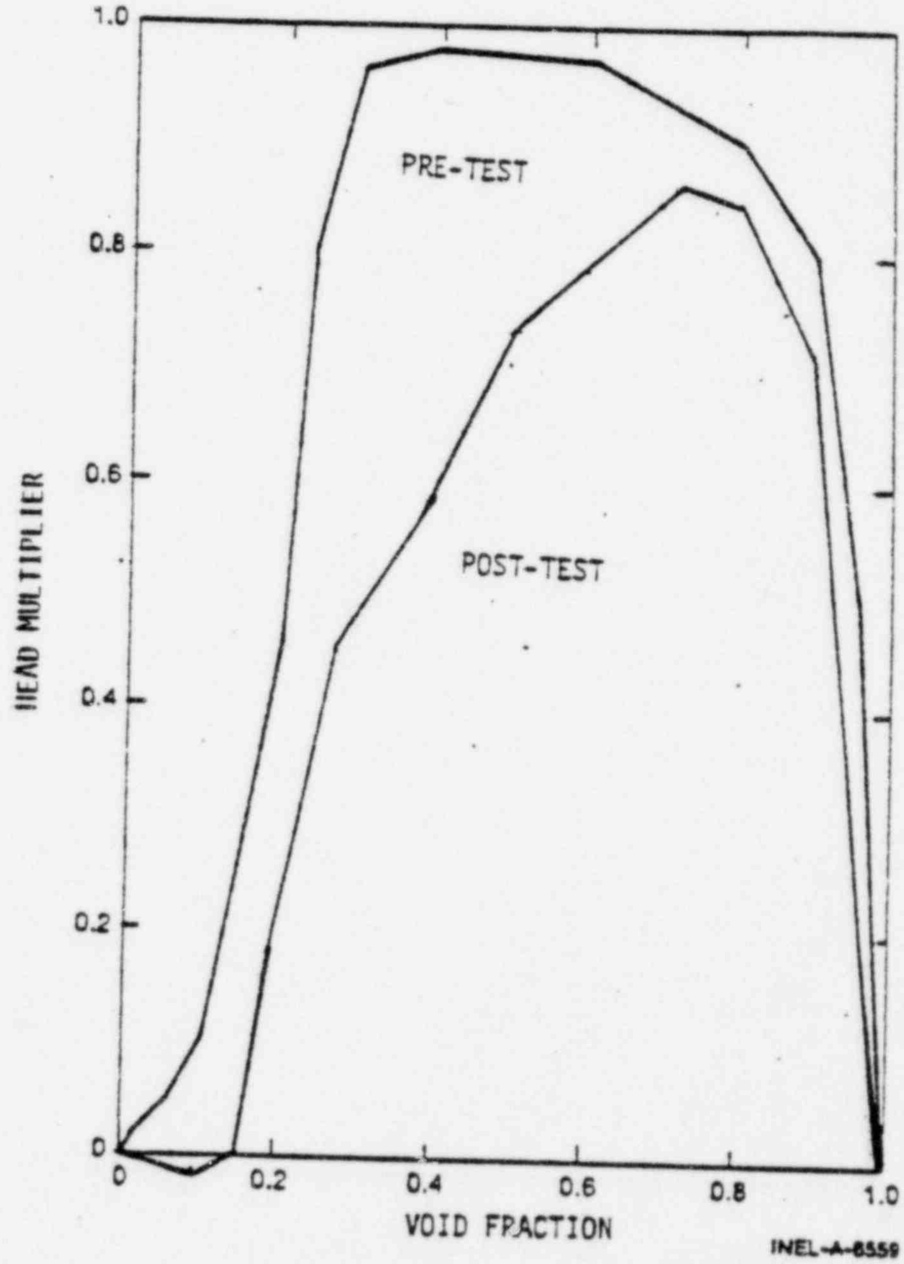
L3-6 CALCULATED BREAK FLOW - COMPARISON WITH DATA



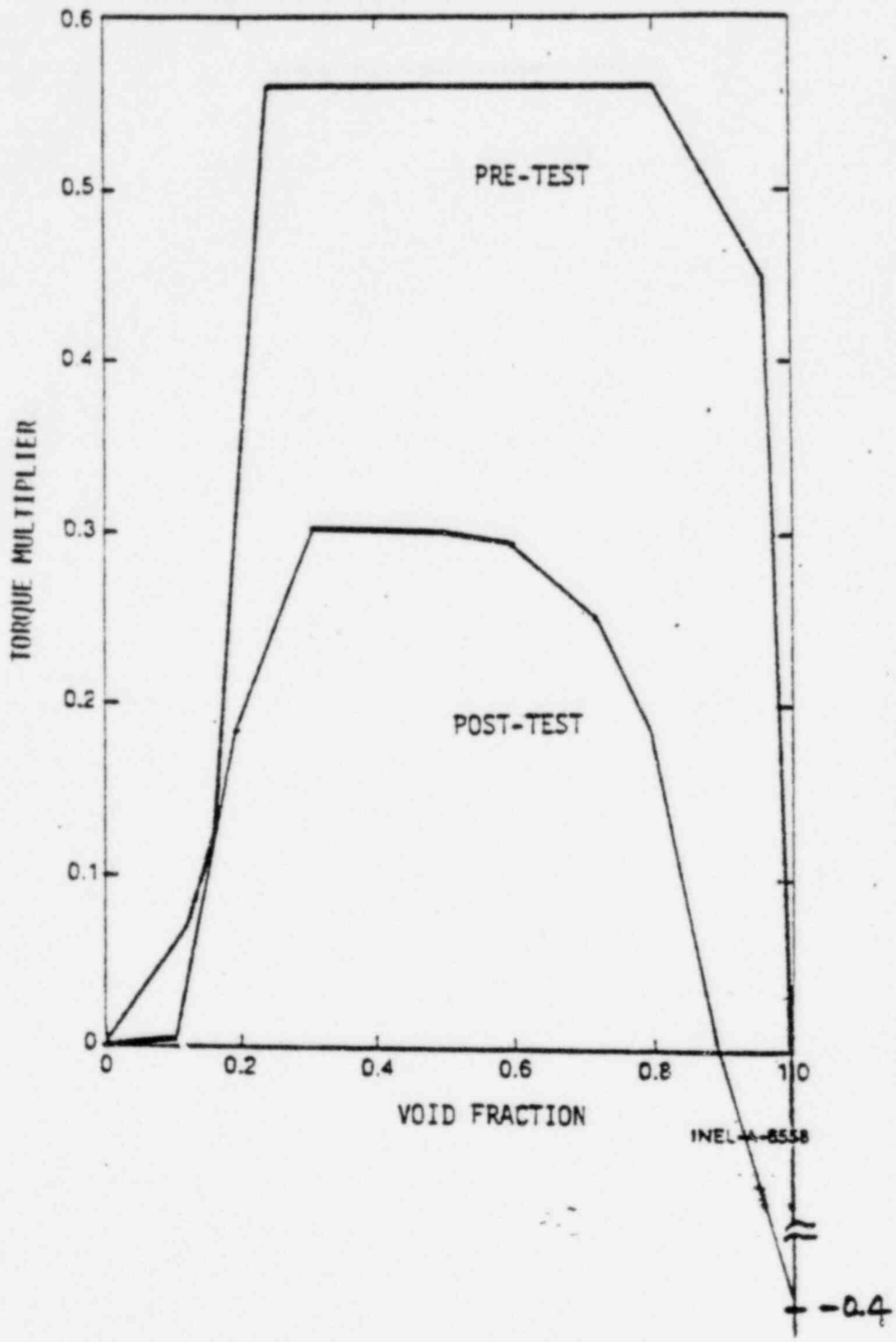
CHANGES FOR POST-TEST ANALYSIS

- o INITIAL CONDITIONS
- o INITIAL SEQUENCE
- o STEAM CONTROL VALVE LEAKAGE
- o PUMP INERTIA FIX
- o MASS ERROR FIX
- o BREAK GEOMETRY
- o IMPROVED TWO-PHASE DEGRADATION CURVE

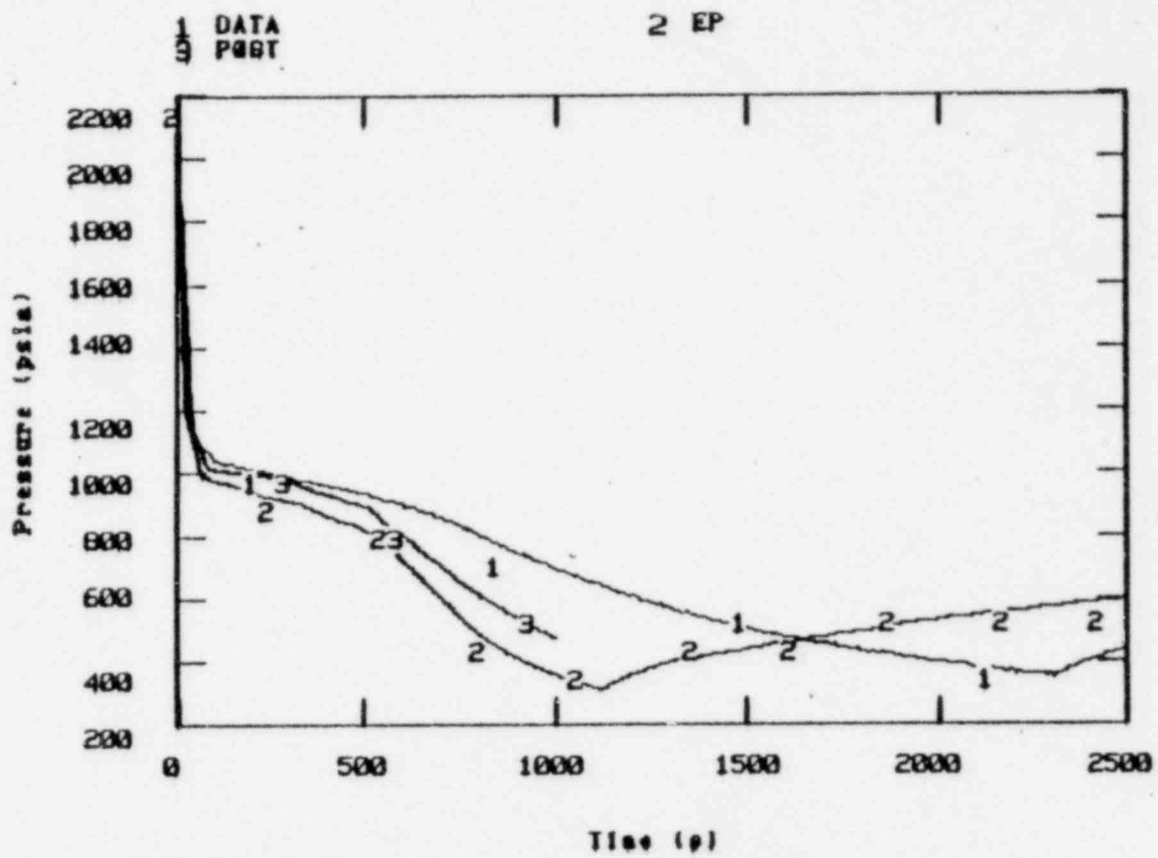
LOFT PUMP HEAD MULTIPLIER



LOFT PUMP TORQUE MULTIPLIER



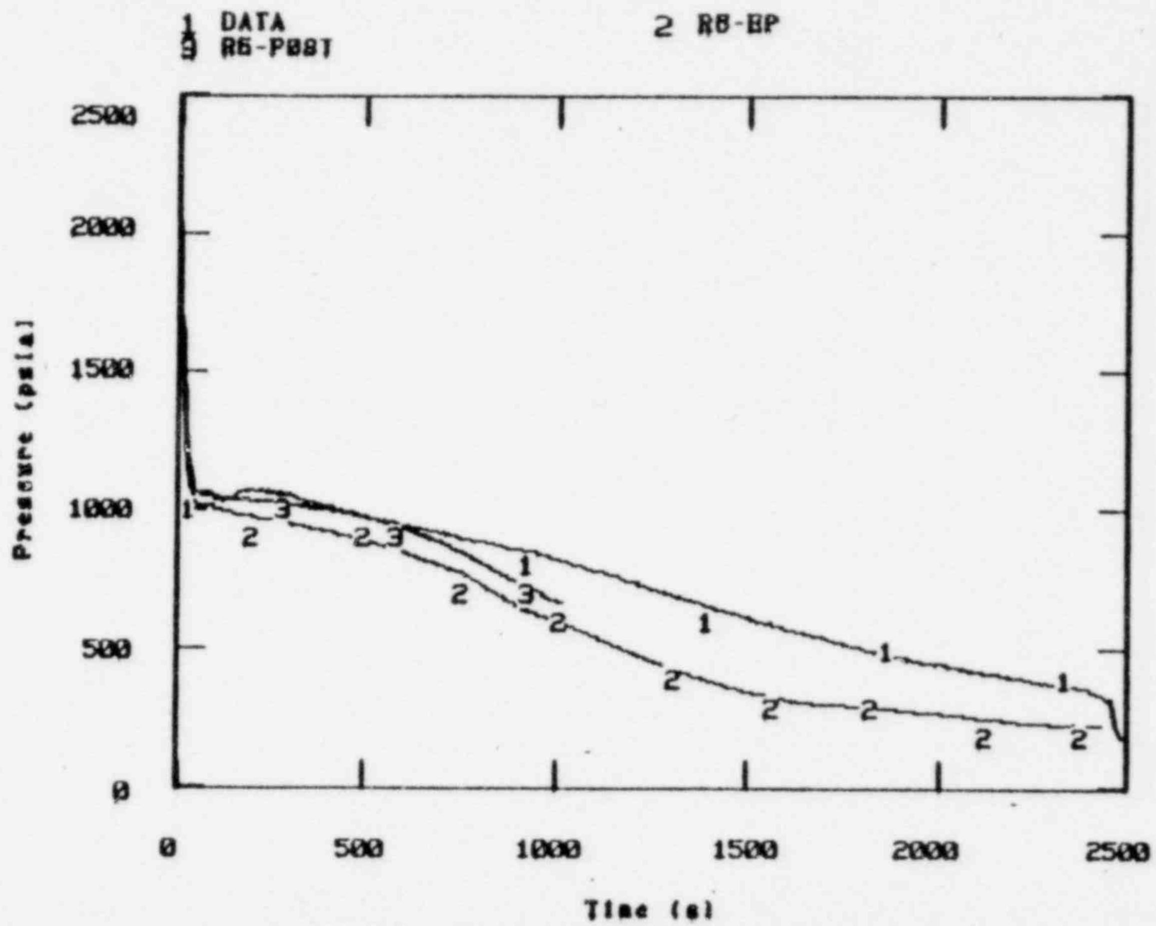
LOFT L3-5 PRIMARY SYSTEM PRESSURE



L8-6

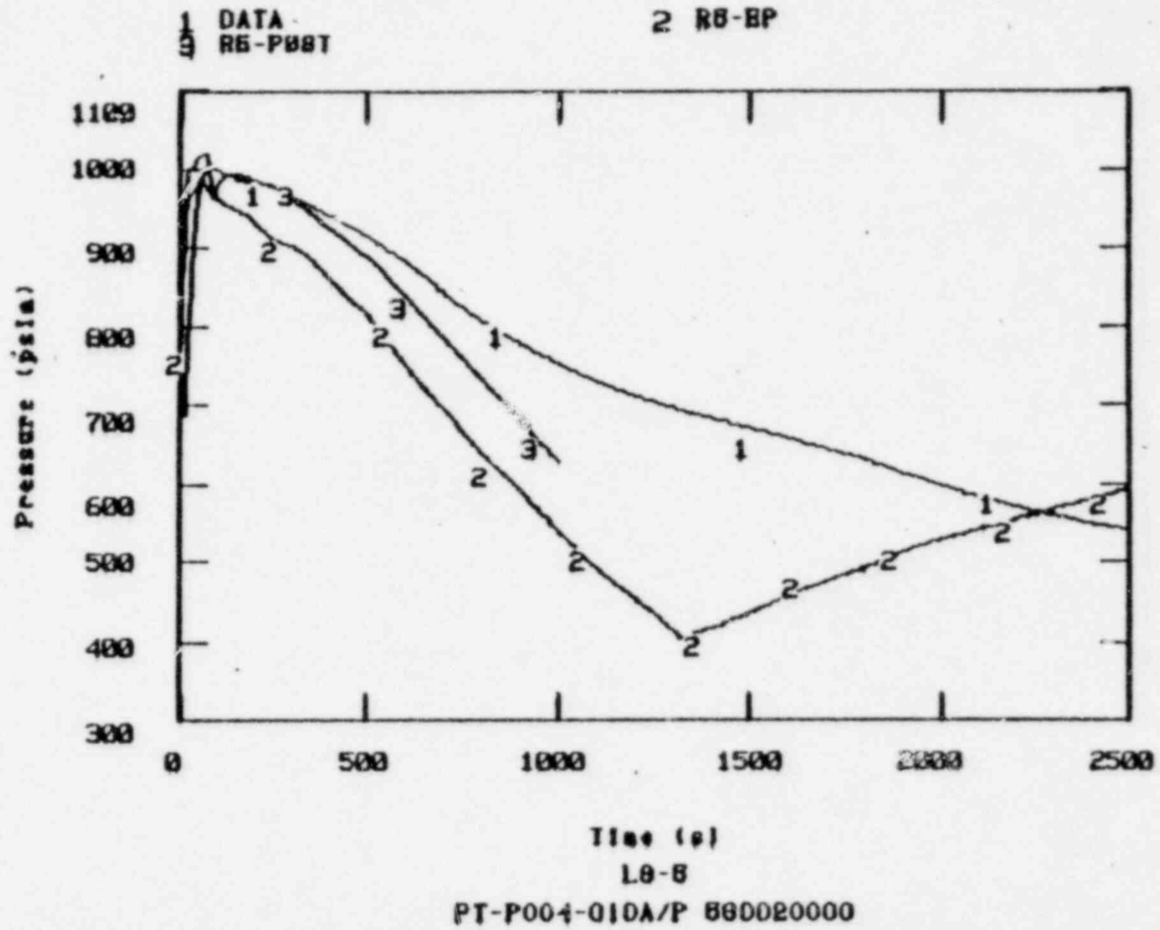
PE-1UP-001/P240010000

LOFT L3-6 PRIMARY SYSTEM PRESSURE

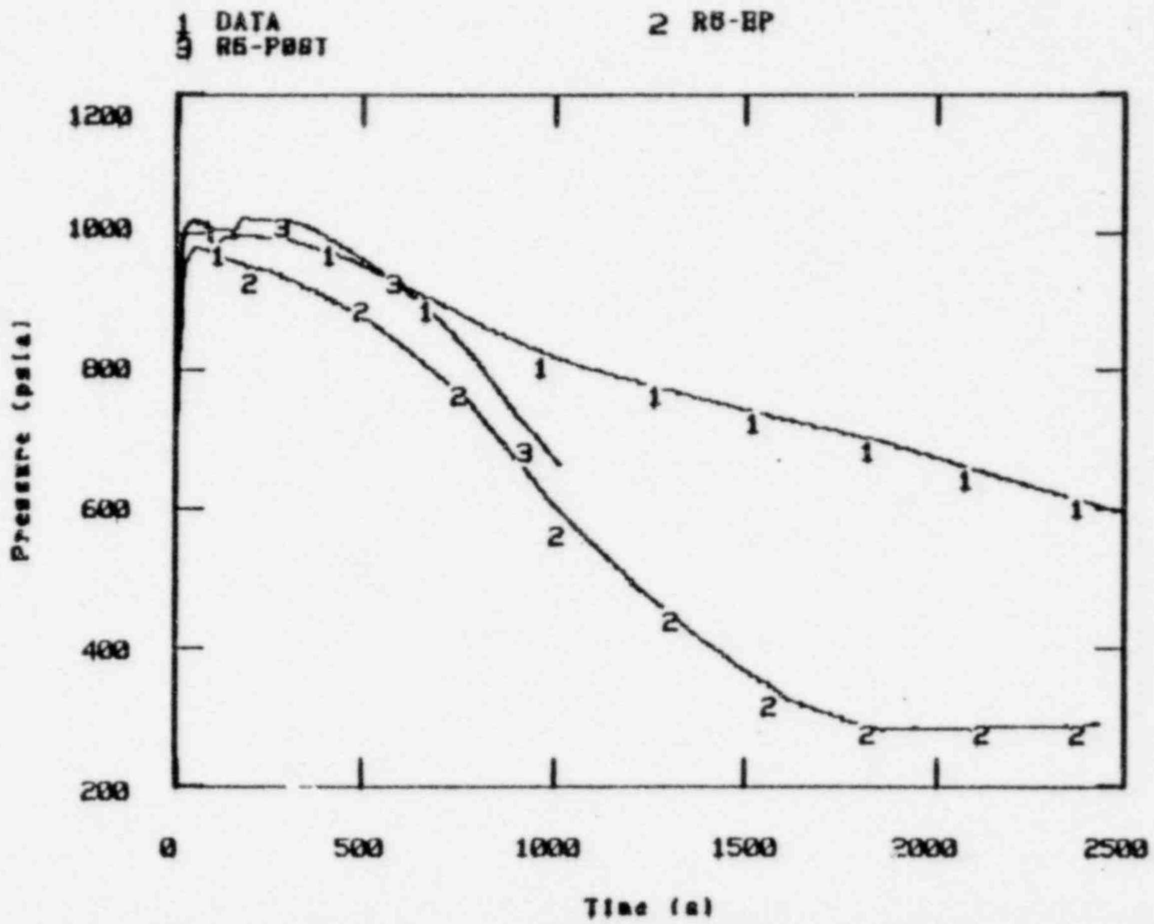


PE-1UP-001A/P2400I0000

LOFT L3-5 SECONDARY SYSTEM PRESSURE

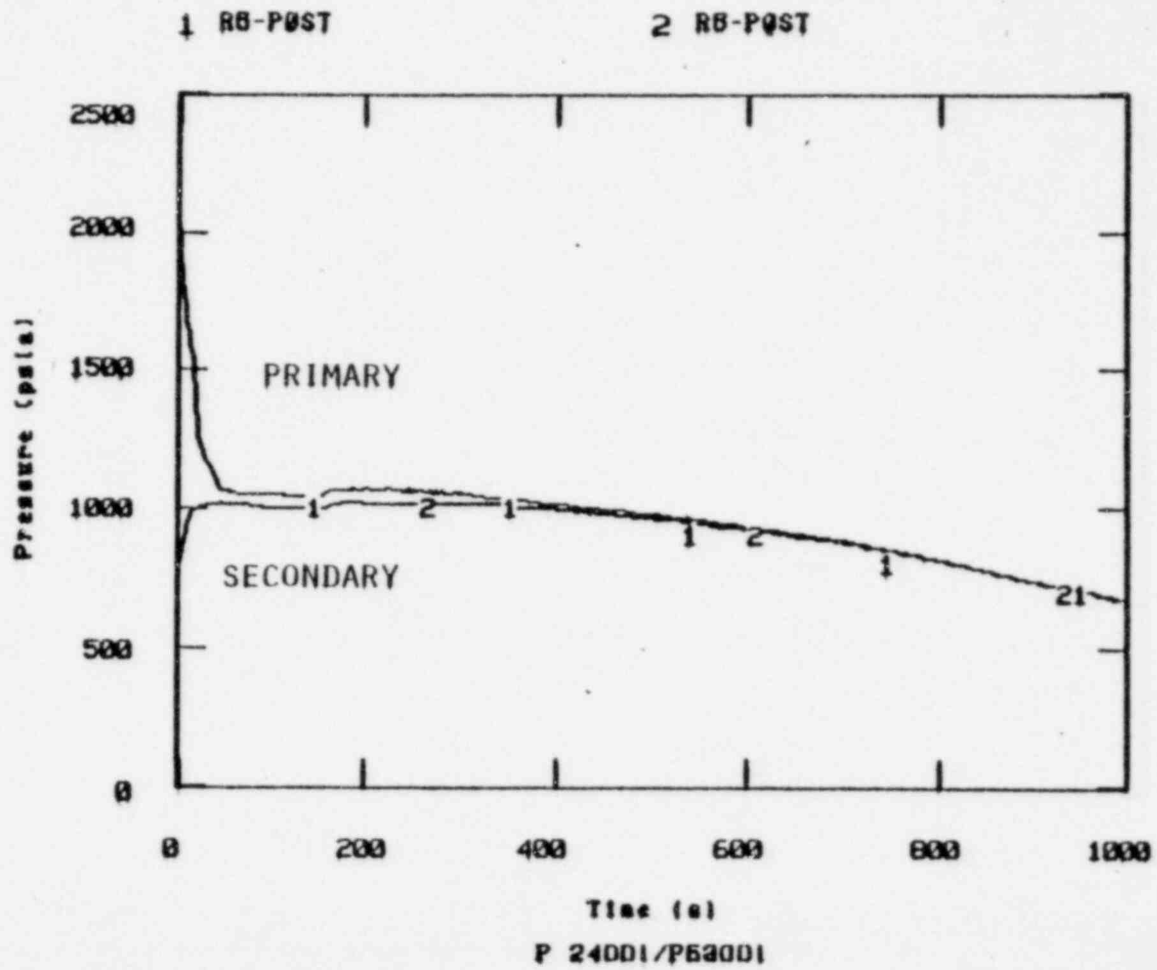


LOFT L3-6 SECONDARY SYSTEM PRESSURE

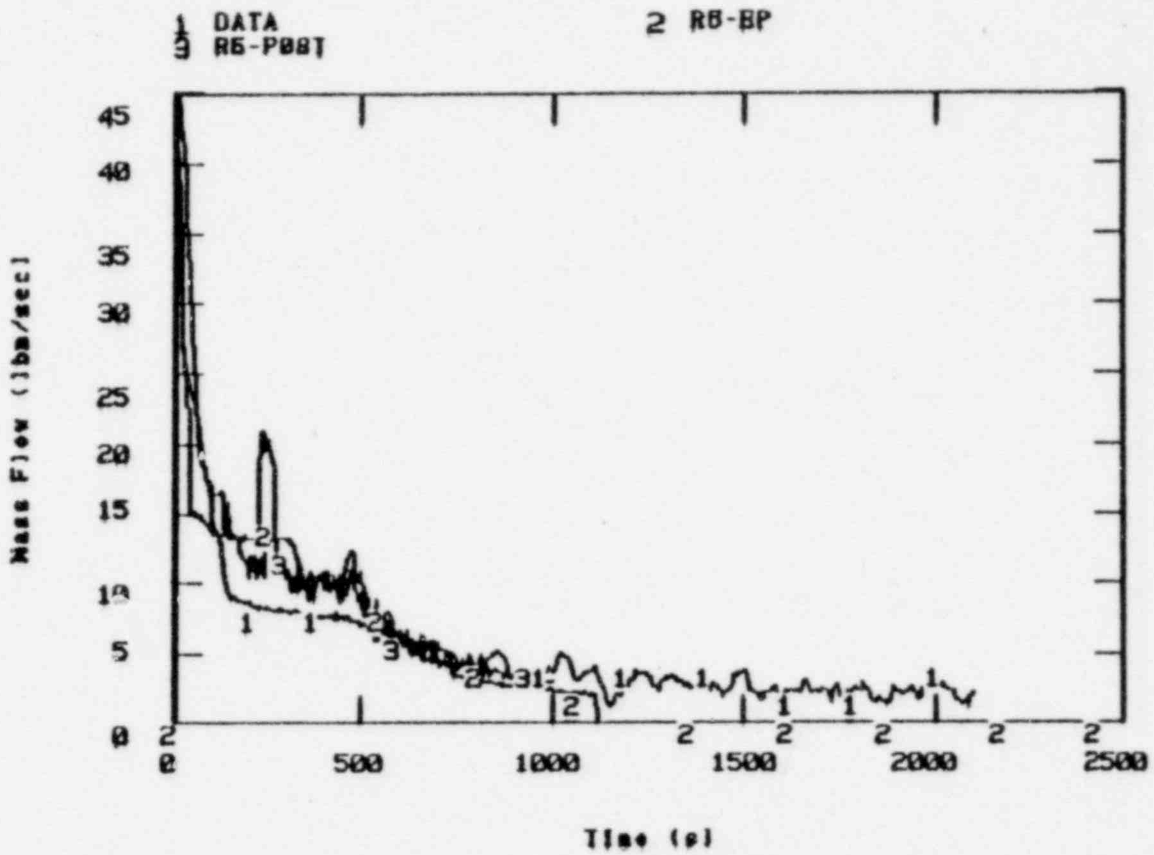


PT-PDD4-010A/P520020000

RELAP5 CALCULATION OF LOFT L3-6 PRIMARY AND SECONDARY PRESSURE



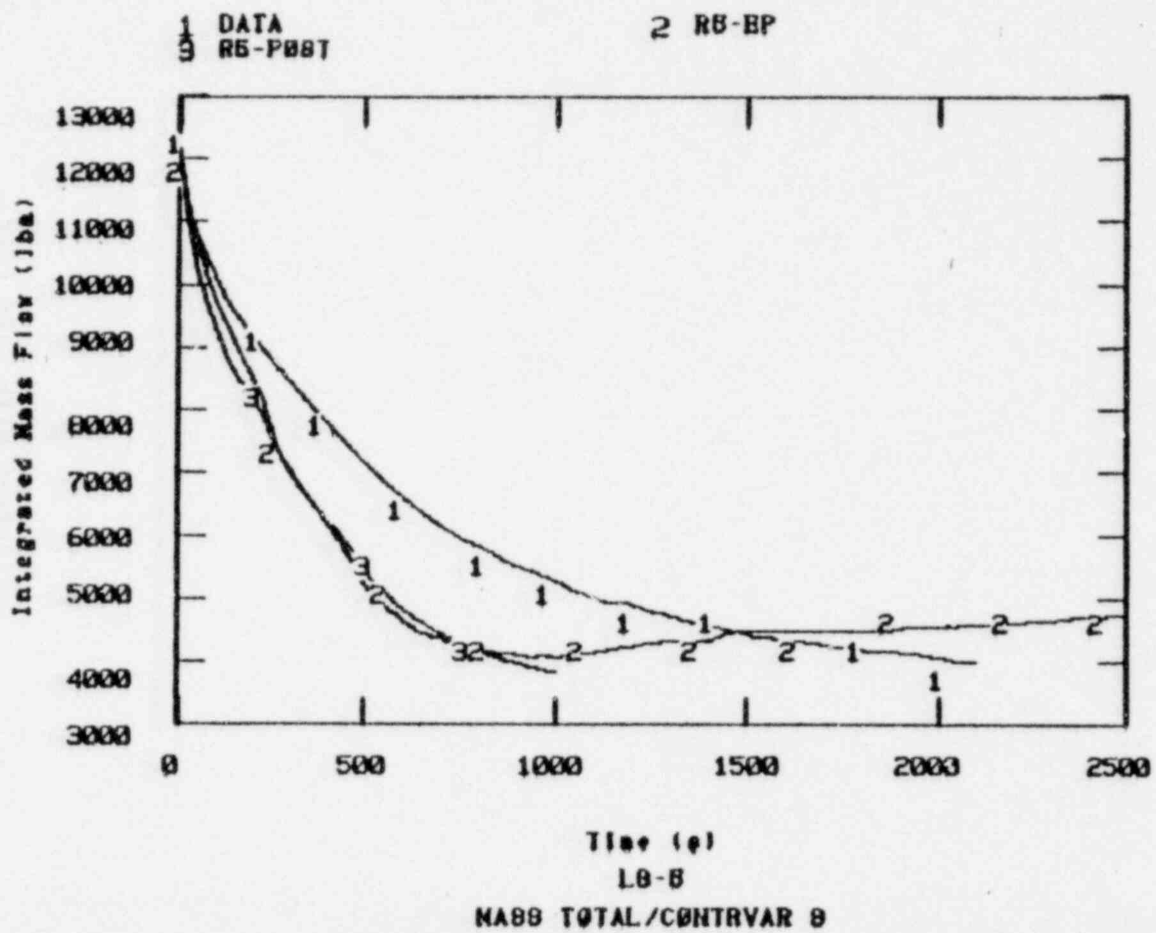
LOFT L3-5 BREAK FLOW



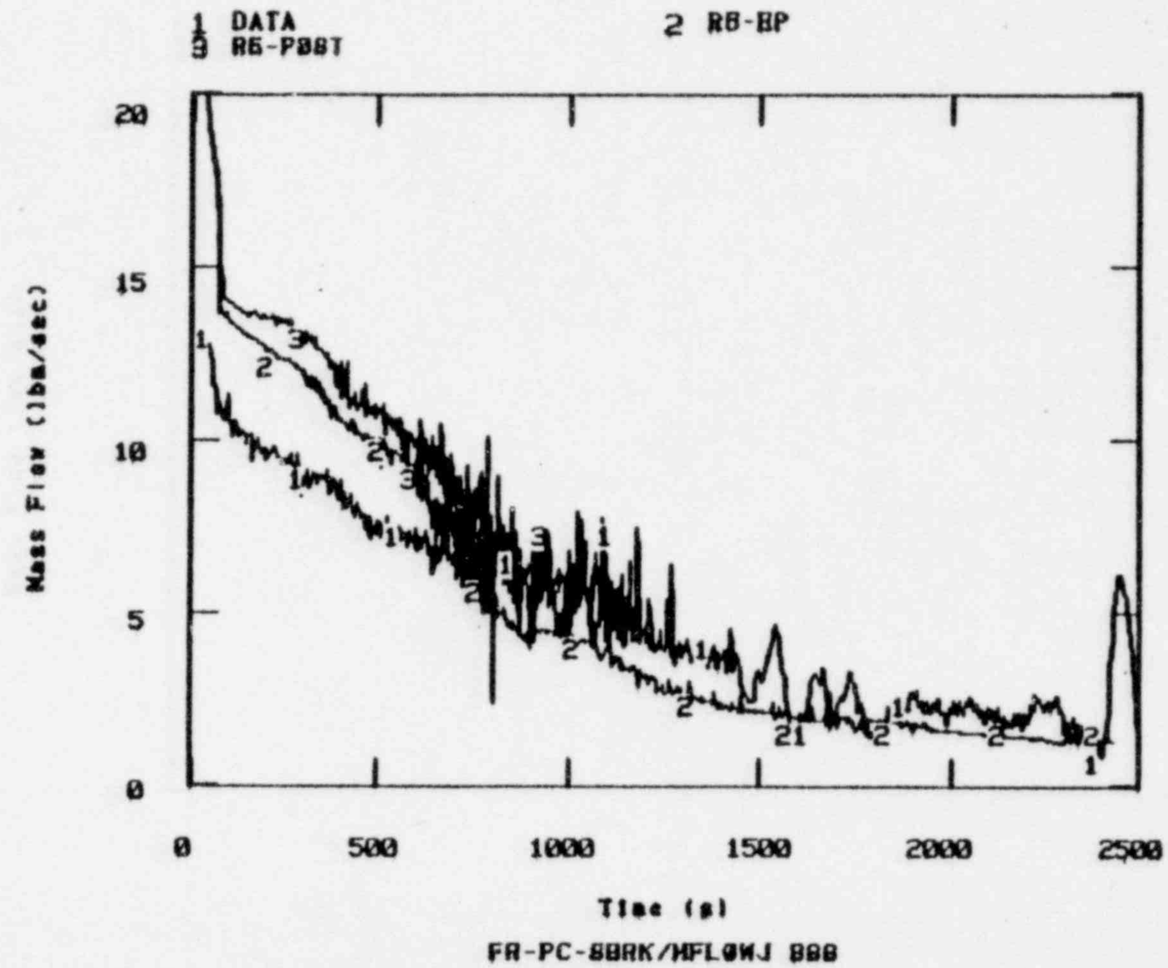
L8-5

BREAK FLØW/MFLØWJ000

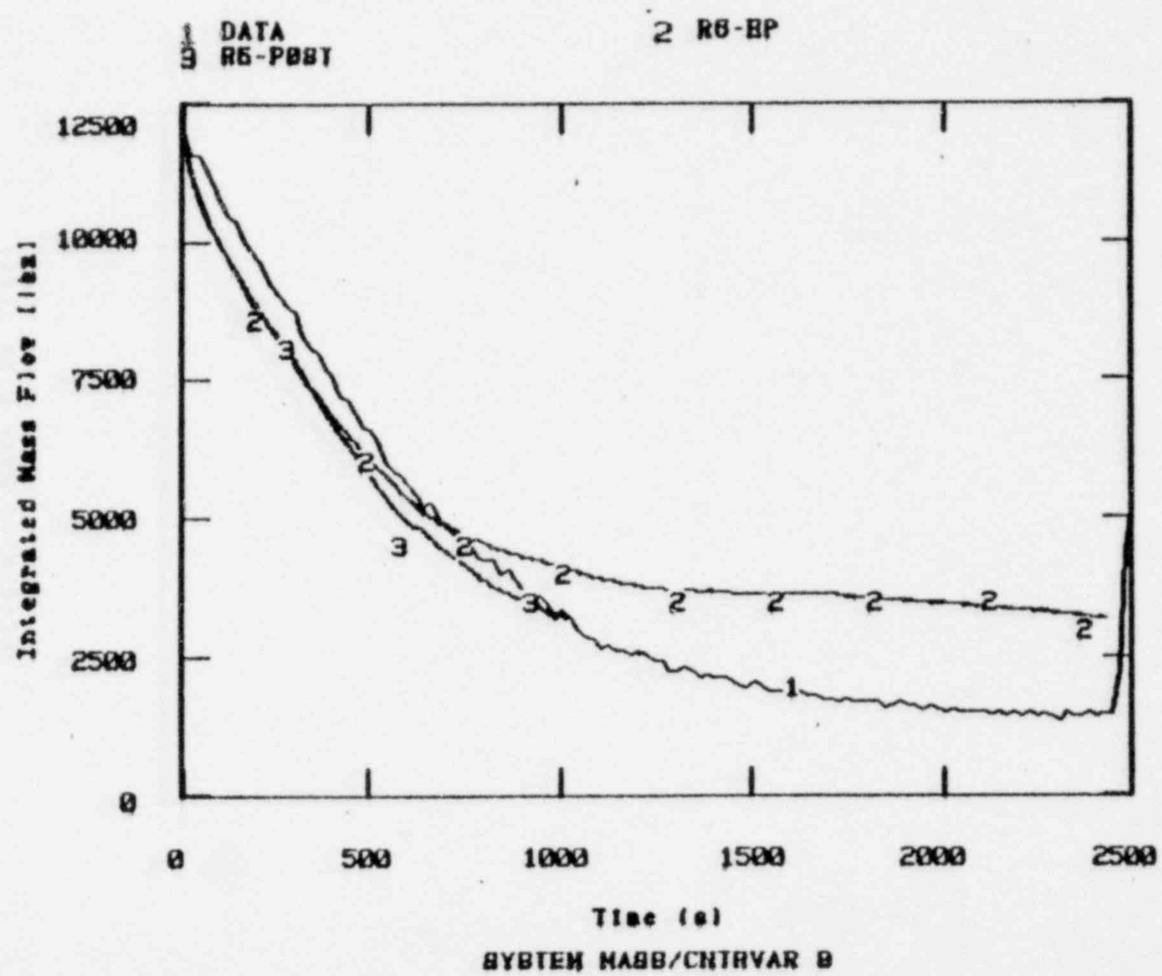
LOFT L3-5 PRIMARY SYSTEM MASS



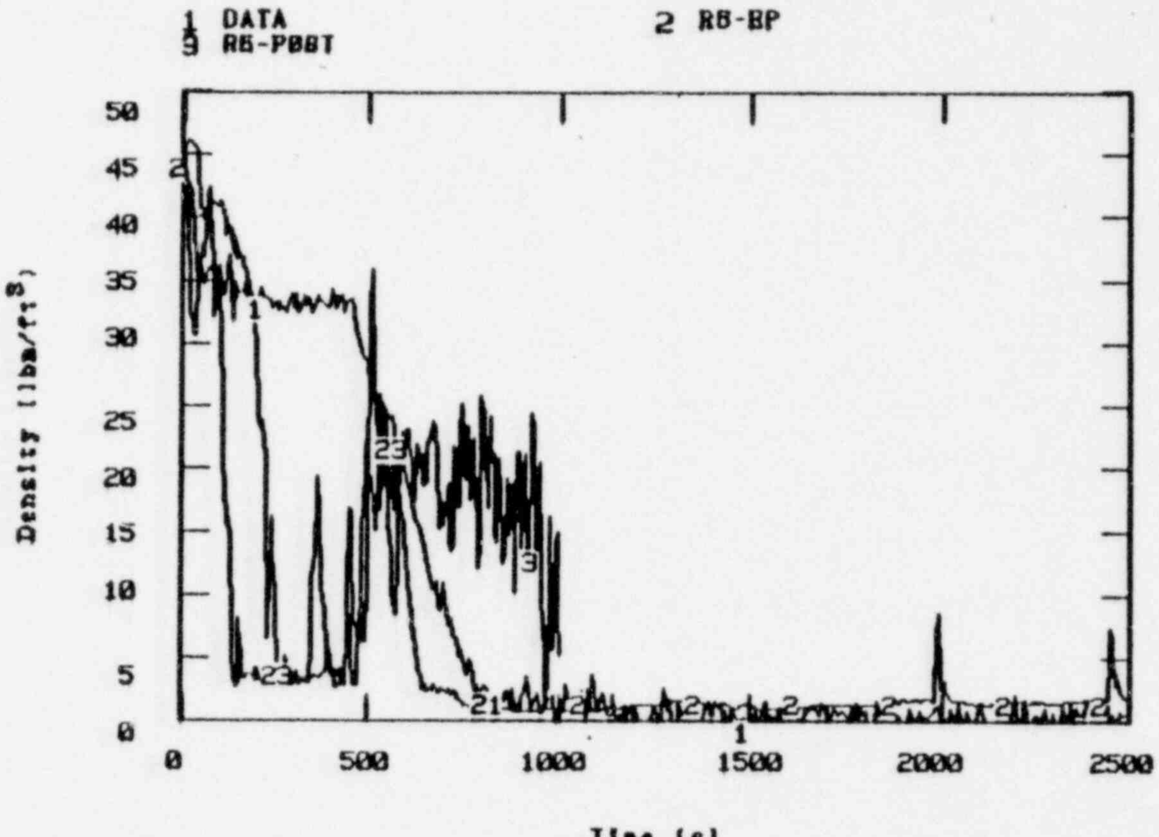
LOFT L3-6 BREAK FLOW



LOFT L3-6 PRIMARY SYSTEM MASS

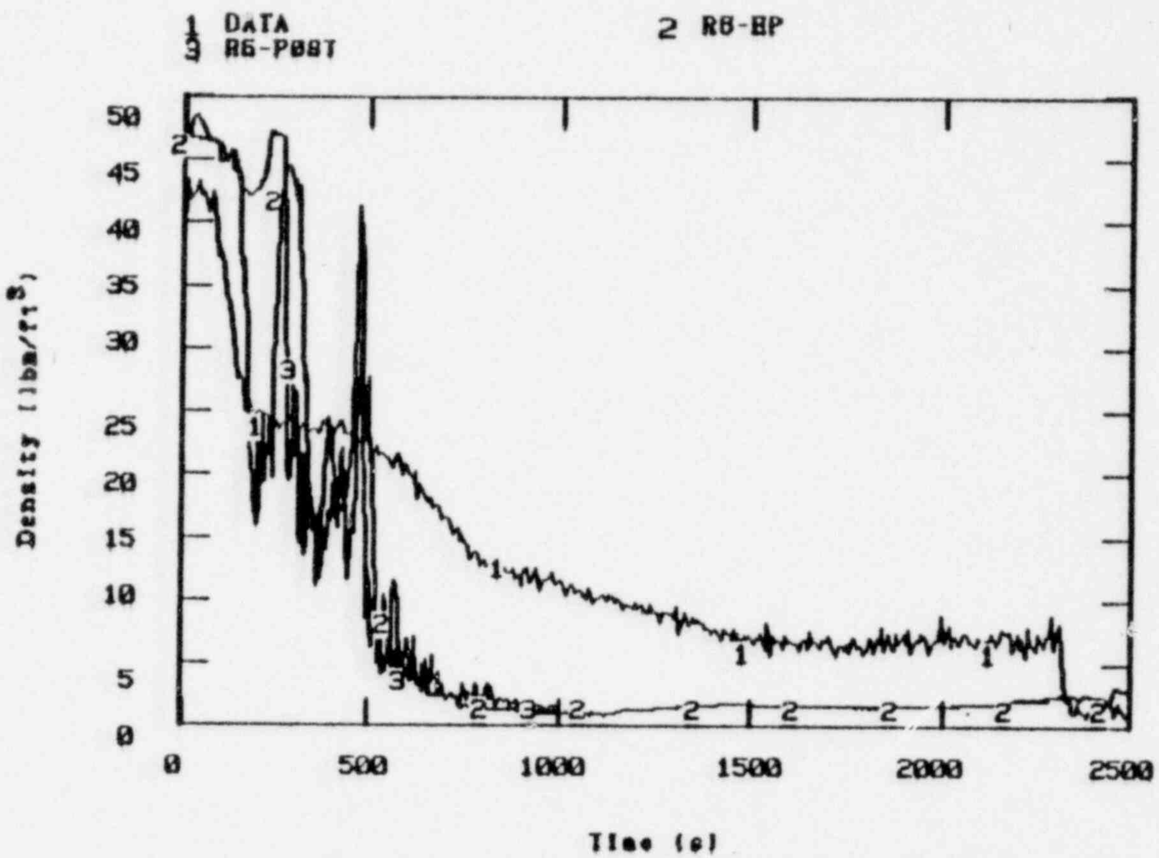


LOFT L3-5 INTACT LOOP HOT LEG DENSITY



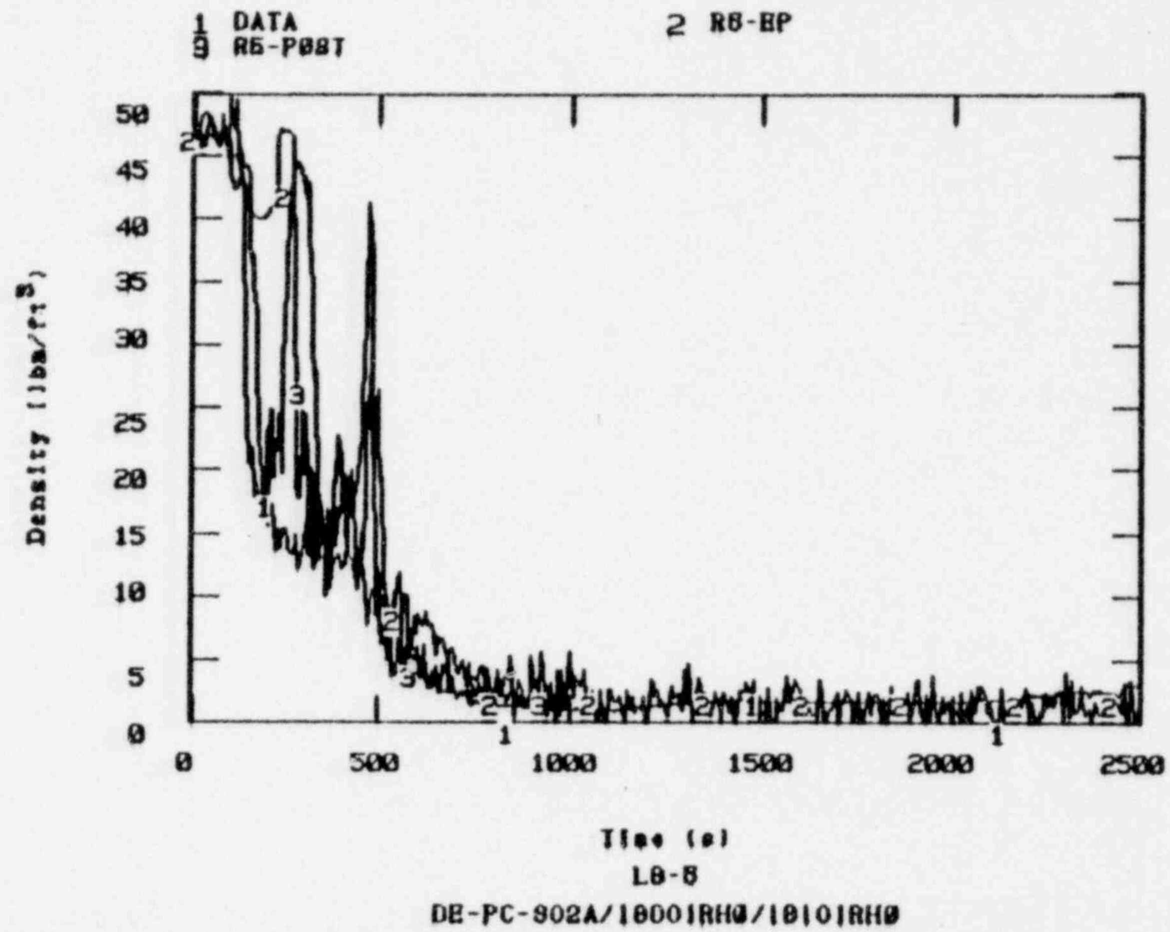
L8-8
DE-PC-208/RHØ 108010000

LOFT L3-5 INTACT LOOP COLD LEG DENSITY

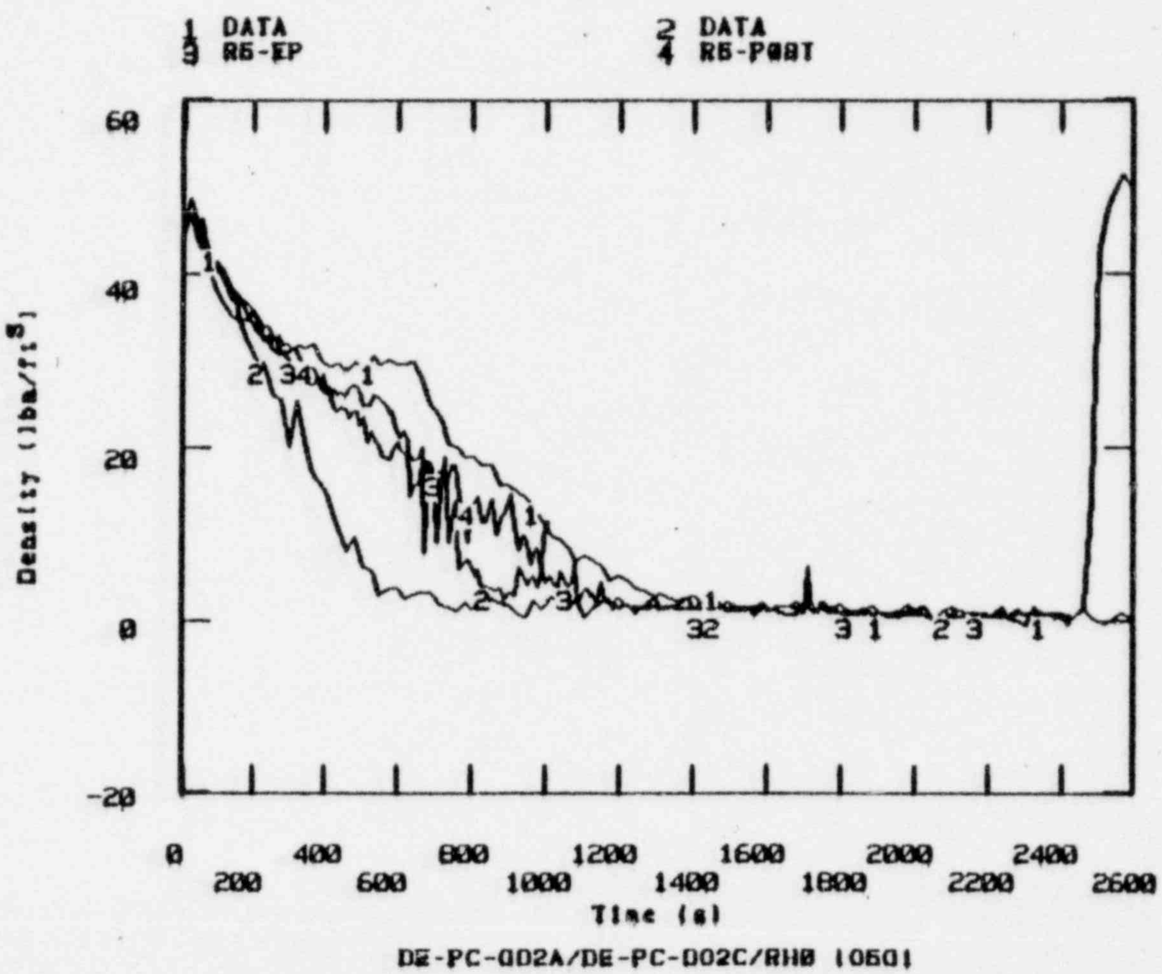


L8-8
DE-PC-11B/RHO 100010000

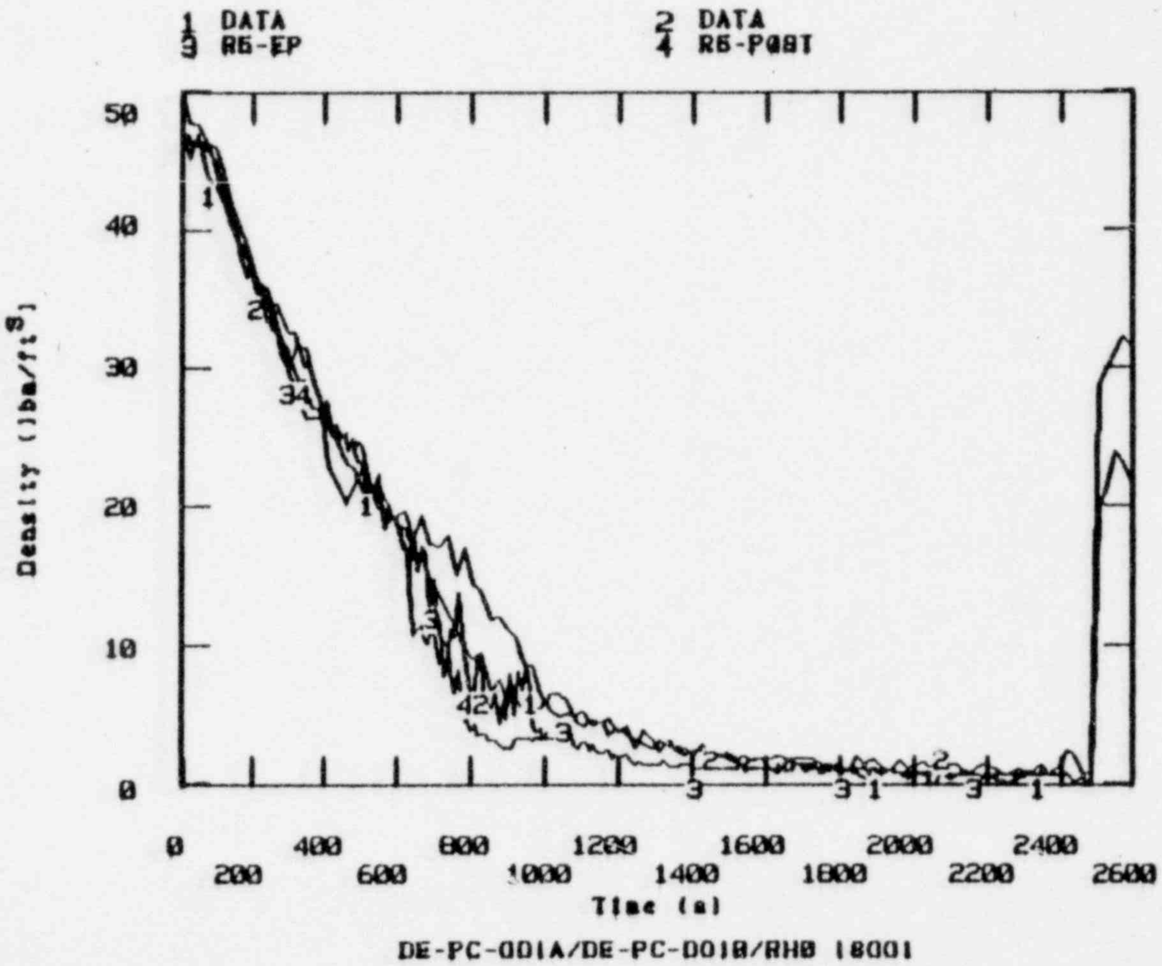
LOFT L3-5 BREAK SPOOL PIECE DENSITY

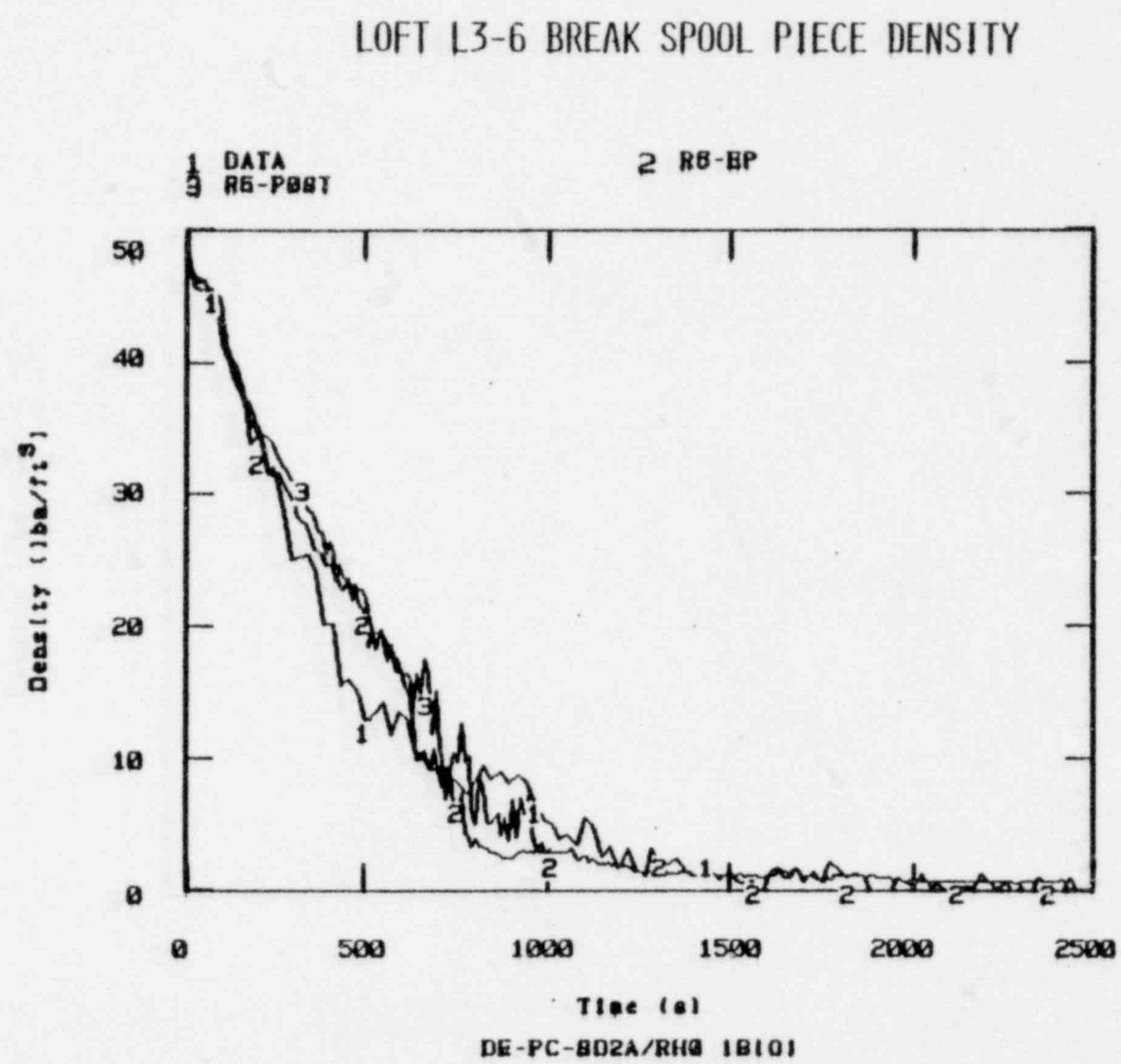


LOFT L3-6 INTACT LOOP HOT LEG DENSITY

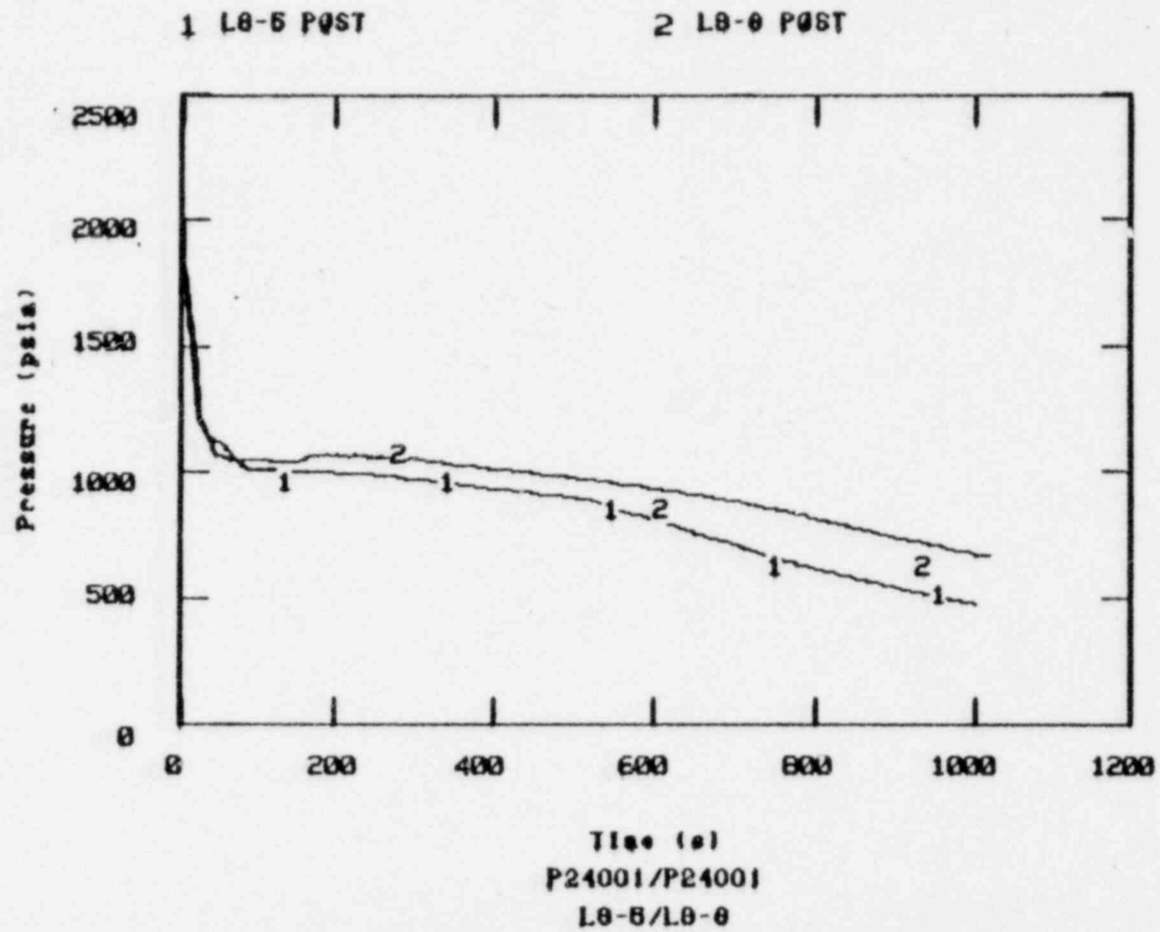


LOFT L3-6 INTACT LOOP COLD LEG DENSITY

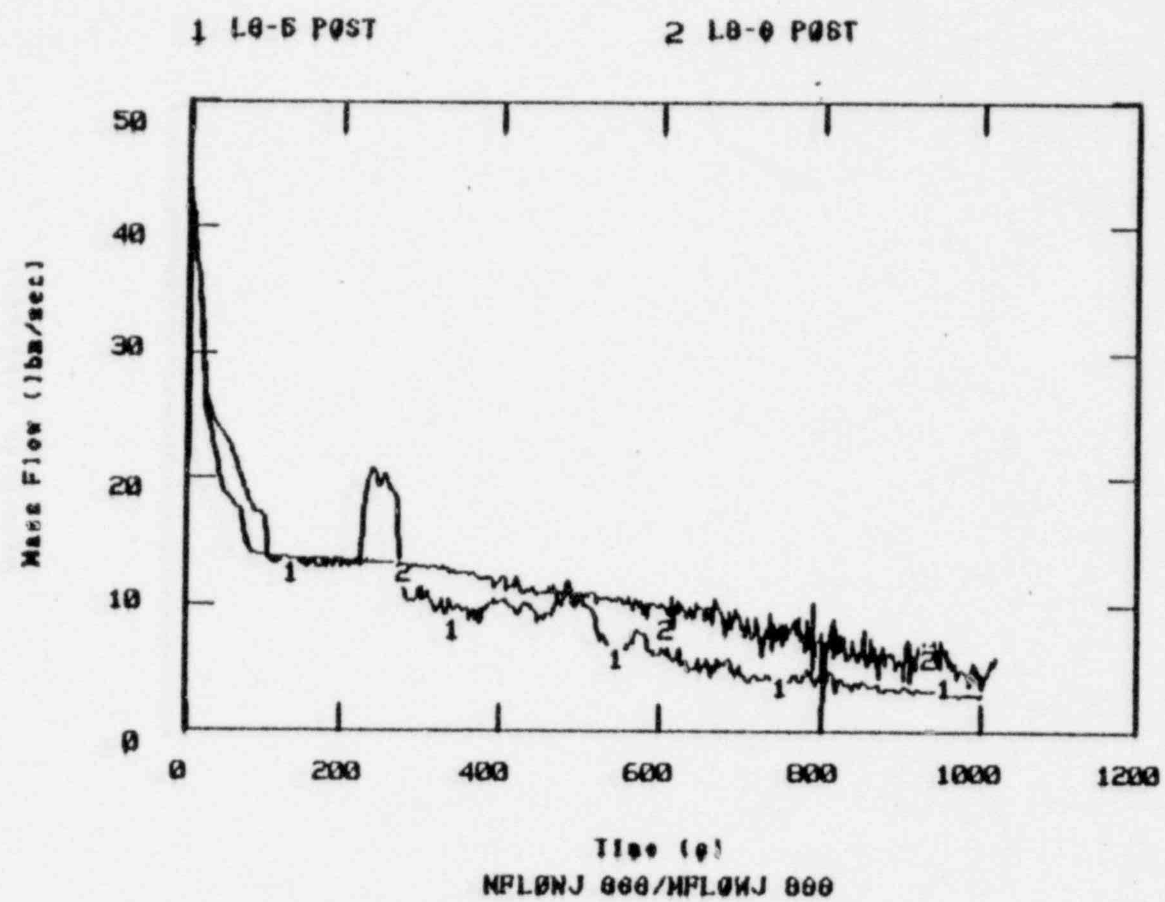



LOFT L3-6 BREAK SPOOL PIECE DENSITY
1 DATA
2 R6-BP
3 R6-POST
50
40
30
20
10
0
Density (lbm/ft3)
0
500
1000
1500
2000
2500
Time (s)
DE-PC-BD2A/RHO 1B101

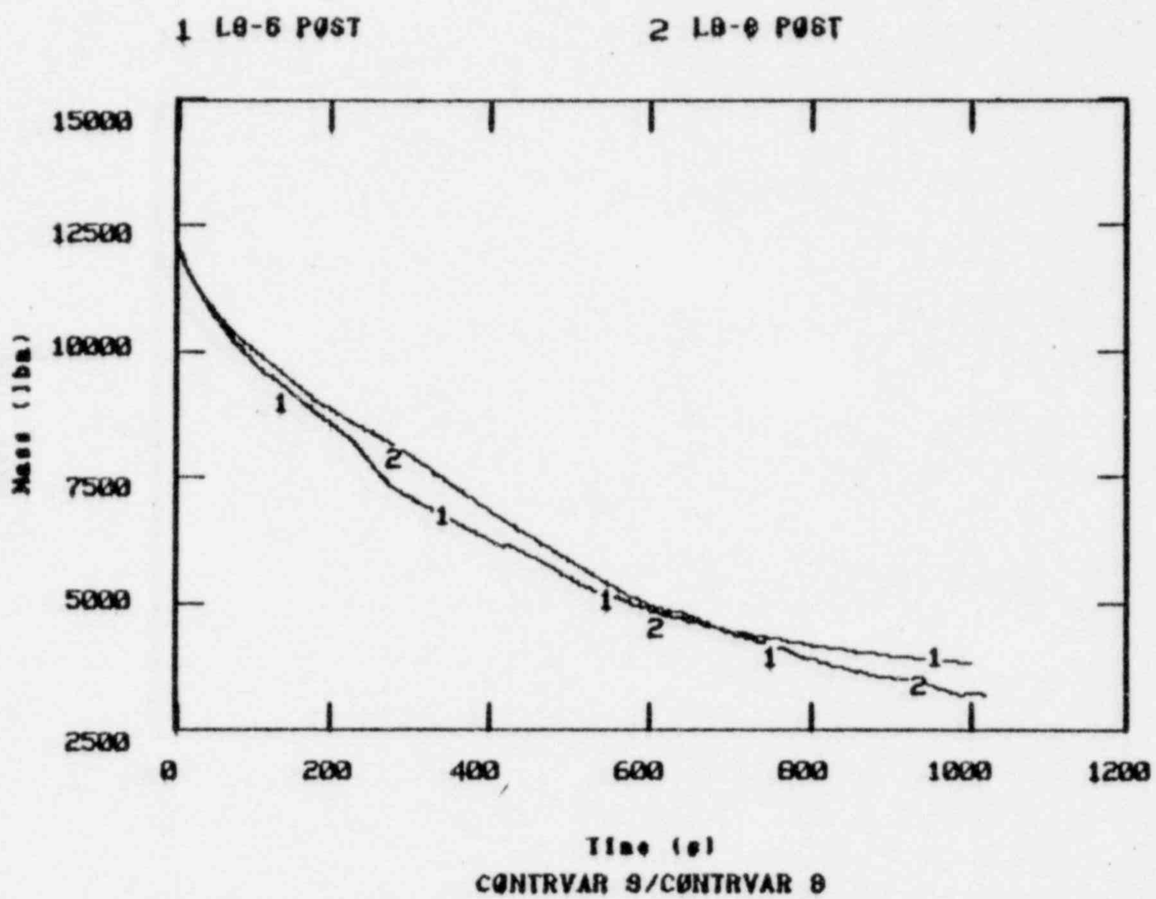
L3-5 vs L3-6 CALCULATED PRIMARY PRESSURE



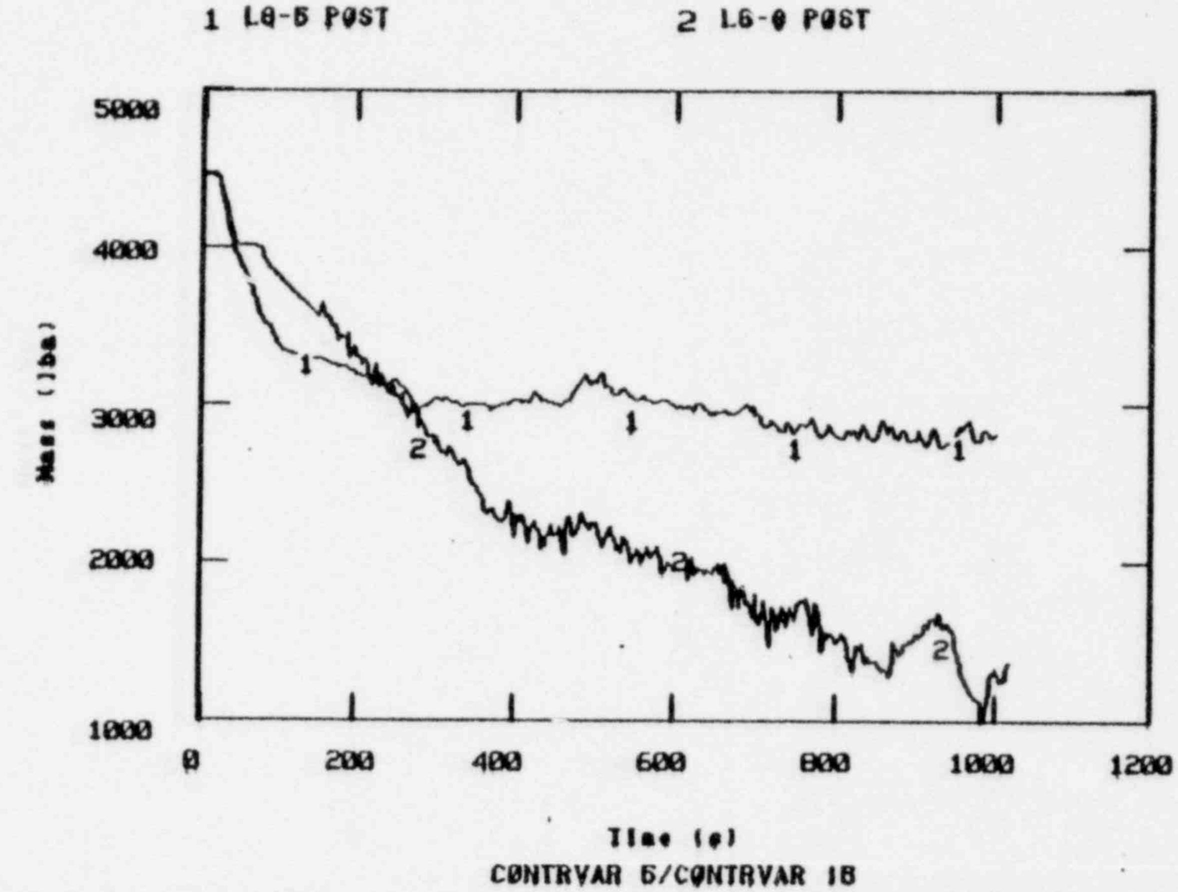
L3-5 vs L3-6 CALCULATED BREAK FLOW



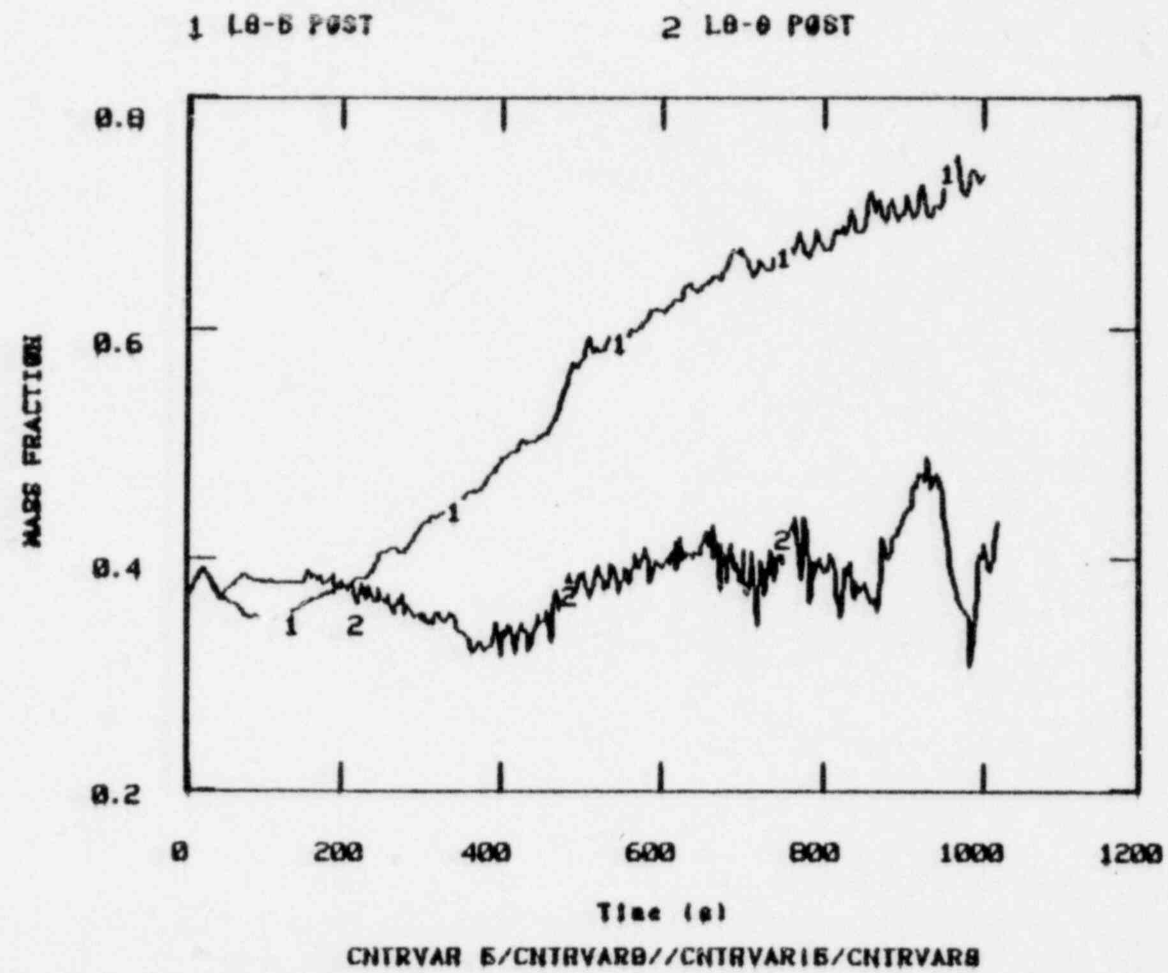
L3-5 vs L3-6 CALCULATED PRIMARY SYSTEM MASS



L3-5 vs L3-6 CALCULATED REACTOR VESSEL MASS

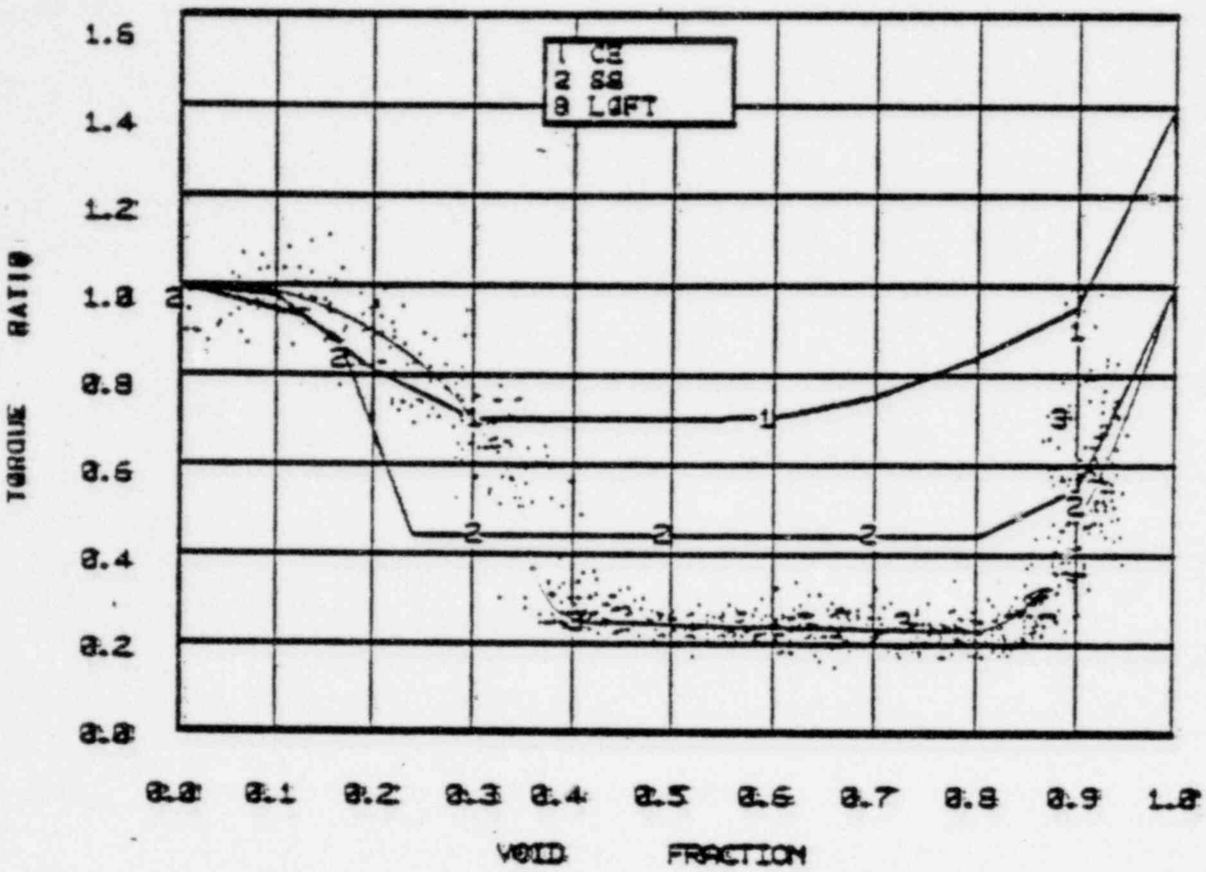


CALCULATED REACTOR VESSEL MASS/PRIMARY SYSTEM MASS - L3-5 vs L3-6

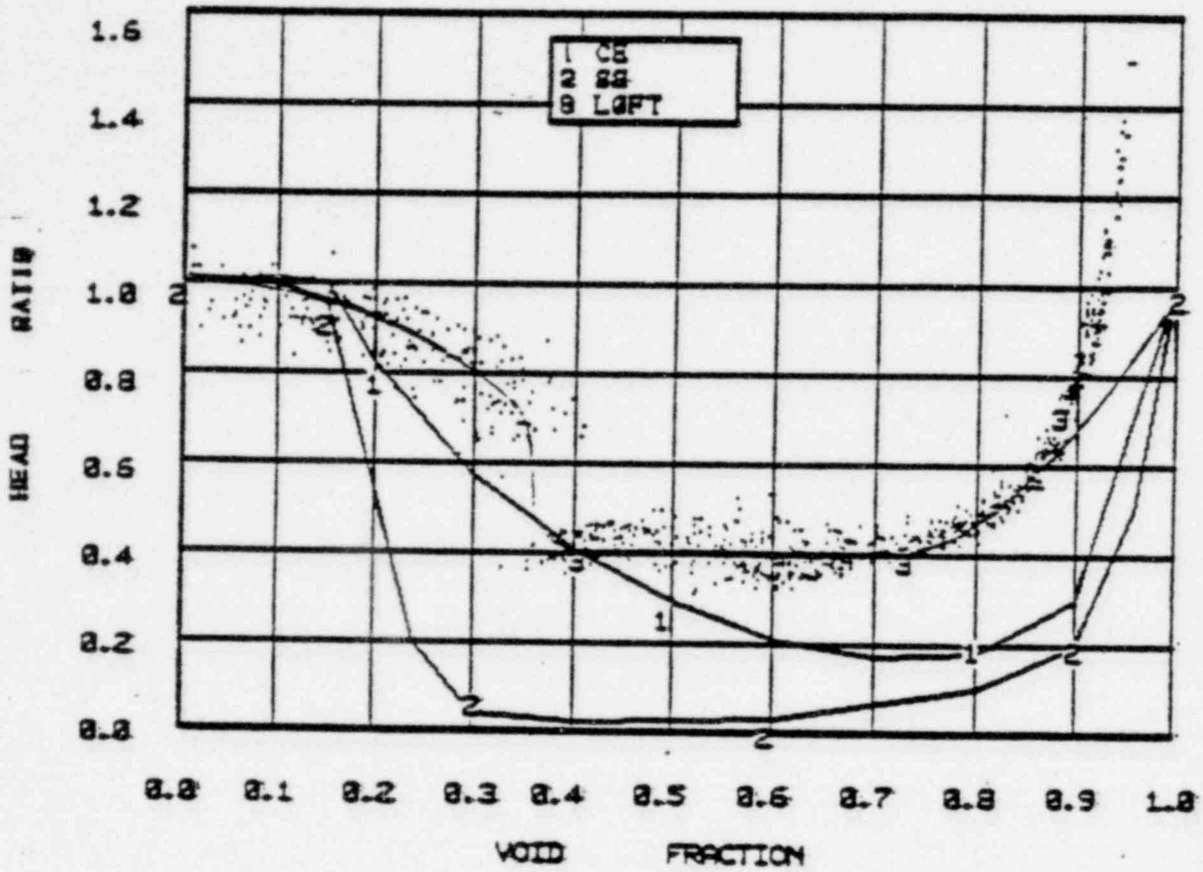


POTENTIAL CHANGES FOR IMPROVED CODE CALCULATIONS

- LOW FLOW REVERSE HEAT TRANSFER
- LOFT PUMP CURVES
- STRATIFICATION IN TEE JUNCTION
- ADDITION OF PIPING HEAT SLABS
- IMPROVED INTERFACIAL DRAG
- CRITICAL BREAK FLOW MULTIPLIERS



TORQUE RATIO VERSUS VOID FRACTION FROM
LOFT TEST L3-6, CE PUMP DATA, AND
SEMISCALE PUMP DATA



HEAD RATIO VERSUS VOID FRACTION FROM LOFT TEST L3-6, CE PUMP DATA, AND SEMISCALE PUMP DATA

CONCLUSIONS

CONFIRMED CRITICAL WINDOW FOR DELAYED PUMP TRIP FOR PWR SMALL BREAK

HAVE PROVIDED IMPORTANT DATA FOR ASSESSMENT OF CODES