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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Subcommittee on Fuel

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Thursday, August 21, 1980
Teton Room
Westbank Motel
Idaho Falls, Idaho

The subcommittee meeting was convened at 8:30 a.m., pursuant to notice, Paul Shewmon, chairman of the subcommittee, presiding.

ACRS MEMBERS:

Dr. Okrent
Dr. Bessette
Mr. Mathis
Dr. Solomon

Designated Federal Employee:

Paul Boehnert

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P R O C E E D I N G S

DR. SHEWMON: The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Fuel. I am Paul Shewmon, subcommittee chairman. The other ACRS members present today are Mathis and Okrent. We also have in attendance Dr. Solomon, a consultant to the subcommittee.

The purpose of the meeting is to continue to review the NRC fuel behavior research branch programs for the annual ACRS report to the Commission and Congress.

The meeting is being conducted in accordance with the revisions of the Federal Advisory Committee Act of the government and the Sunshine Act. Mr. Paul Boehnert, on my left, is the designated federal employee for the meeting.

Rules for participation in these meetings have been announced, and as part of the announcement of this meeting previously published in the Federal Register on August 6th, 1980.

If there is anyone who needs a copy of the notice, please so indicate, and we can provide you with one.

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

We received nc itten comments or requests for time to make oral statements from members of the public, and I guess we will then proceed, if I can get organized.

The program that we will go over today is primarily the PBF program. We have gone over much of the rest of the program at earlier subcommittee meetings.

The PBF program represents a large part, probably the largest single part of the funding of this branch. It has been of some interest to us with regard to the description or the choice of what the experiments and conditions will be, and we will continue that discussion here.

Also, there is a question of the degree to which the PBF program reactor can be used to study more severe accidents, things which have become of interest to the Commission and the committee since TMI-2, and with the proposed rulemaking on so-called Class 9 accidents.

We have not heard anything much about the general plan of the branch for studying the aspects of more severe core damage, and we will get into that also.

One other part of this has to do with tests which are planned or in progress, in progress at the Canadian reactor NRU, planned for ESSOR, and that sort of yo-yos back and forth, depending on the budget these days, among other things. And part of the NRU justification has to do with the mooning which might occur in the event of an accident, and block the core so the coolant could not proceed, and actually that we will get into a little bit today, but it will be the topic of the next meeting of the subcommittee, which will be the 3rd in Washington, D.C.

I guess that's all of my general views. If nobody else has anything, we will proceed.

Are you first on the program then, Pic?

DR. PICKLESIMER: Yes.

DR. SHEWMON: Okay.

DR. PICKLESIMER: Good morning. My name is Picklesimer. I am at the present time the project manager for the PBF program in Fuel Behavior Research Branch, and my topic this morning, for the first presentation, is to give you some of the history in a summary fashion, and some of the major results of the PBF program over this past several years.

Am I speaking loud enough?

(Slide.)

The program will be discussed in detail by the following speakers. Now according to what I can find in our records, the Fuel Behavior Research Branch assumed the responsibility for the PBF program in 1973, when the Division of Reactor Safety Research was formed, as part of the AEC at that time. A program, rather extensive testing program. 160 tests were developed as a test pattern, in consultation with regulatory people. The PBF group which was formed at that time, the technical community, as well as we could get information, and the company at that time was Aerojet, and over a period of several months this was reduced to an essential test, 40-test program.

Now at that time we had scheduled eight power-coolant mismatch tests, six irradiation effects tests, five gap conductance, 10 loss-of-coolant, six reactivity insertion tests, and five inlet flow blockage. That was reduced to 30 tests in November of '78, when five flow blockage and five LOCA tests were deleted from the program.

Now we have added in the last two years two tests for examining the behavior of thermocouples and the effects of thermocouples on fuel rods in quench, because of some problems that were involved in the early LOFT test, and one LOFT lead rod test, which was conducted to examine the behavior of the collapsed cladding and fuel rods for successive tests. And there we found that the tests could be successfully completed using the same fuel rod again and again.

24 of the original tests have been conducted. It has had two of the three added tests. We still have one thermocouple quench test to go, which we hope will go in the end of September.

Five of the original 40 remain to be performed. Two of these will be conducted, LOC-6 which, if we are lucky, will be isothermal runs this weekend, and we hope to conduct the final part of the test the end of next week, and LOC-7, which will be conducted later this year.

Three of these tests have been put on indefinite hold. These are known as RIA-13, 16 and 17.

(Slide.)

Now the test program was developed and modified continuously through the years, with discussions with ACRS, PBF review group, NRR, industry, the vendors, anyone that was interested in discussing the programs.

One of the problems that it is somewhat difficult to realize, when we attempt to make a program change, is the length of time that is required from the time a new test idea is brought forth, and we receive the final reports.

Now these are not tight times, don't try to hold me to them, please. It depends on the test. Planning the experiment, design and construction of the test train can require anything from one to two years.

Preparation of the reactor, insertion and conduct of the test is only one or two months, but then the big part of the work begins to come after that, when you have to conduct the hot cell or radiation examination, the data analysis, and the report and preparation, which takes one and a half to two years on the average, giving us a total time of from three to four years from the original idea to when the final reports are out.

You can't turn that kind of a program around on a month-to-month change of plans.

Now the priorities in the test program have been set over a period of years in concert with NRR, the PBF review group, and they have been approved, for the most part, by ACRS. In 1976, these were in order of priority:

The power-cooling mismatch, LOCA, reactivity insertion accident.

Now because of code development needs we inserted in these at low priorities a gap conductance test, and as the last priority what was called inlet flow blockage.

(Slide.)

DR. SHEWMON: Pic, if you go back to the top of that, you say test programs have been developed and modified. Could you or will you later say something about the goals or criteria that there were for the program, that they were supposed to achieve?

DR. PICKLESIMER: I had not included that in this. The principle goals of the PBF program was to obtain information on safety issues, to answer questions concerning reactor accidents for use in licensing actions, and in code development work.

DR. OKRENT: That's a terribly general goal. Could you be a bit more specific?

DR. PICKLESIMER: Would you --

DR. SHEWMON: One thing which I would like to bring up today is the clearest summary I have ever seen of the PBF program, right or wrong, I don't know, but at least it is concise and clear, better than anything I've got out of the Staff or EG&G, is this thing EPRI put out. It's called "Review of the PBF Programs, the Source of Data for Qualification for Fuel Behavior Codes," and they go over the degree to which the conditions do meet or are co-terminants or overlap with the conditions that are needed for evaluating the DBEs in the SARs. And one criteria could be to provide a better basis for SAR determinations, or it could be not to do that. That is the one of the things I'd be interested in getting into, or hearing your comments on. It can come later, if you want to.

DR. PICKLESIMER: Let me present the major results in this following slide that may present, in an indirect fashion, then some of the objectives.

(Slide.)

One of the concerns that was raised a number of years ago was how long could a rod survive in a power-cooling mismatch where there was film boiling on the surface of the rod.

The regulatory guides at this time require that if the rod has gone into film boiling, it is considered to have failed. The PBF tests assume that the rod can survive the film boiling for several tens of minutes, depending on the power and a number of other conditions involved, so far beyond what was originally estimated, and it indicates that rods can be used again, if necessary, after a small amount of film boiling.

It is not the hazard that had been considered before.

Another concern was the departure from nuclear boiling and the power-cooling mismatch would propagate from rod to rod. It has been shown this does not occur. There is no evidence to indicate this does occur, moving from one rod to another in a large bundle. It seems to go independently.

Another concern was that irradiated rods behave in the same way as unirradiated. Was there a major change in the properties of the rods.

DR. OKRENT: Excuse me. I'm not quite sure what you mean by the second point, when you say there was a concern that DNB would propagate. Was this, in your opinion, some kind of a thermal hydraulic concern, or some kind of a fuel failure propagation concern? Could you be more specific?

DR. PICKLESIMER: I was not involved in the program at that time, and can't give you a completely specific answer. My understanding of the concern at that time was that if you had film boiling develop, departure from nuclear boiling develop in one rod, would that action cause it to occur on a neighboring rod, and then thus move throughout the bundle or throughout the core.

DR. OKRENT: You just told me that something was shown by the PBF test, and I'm trying to ascertain what it is you think was shown, and how it related to some previous concern.

DR. PICKLESIMER: The tests have shown that each rod goes into DNB by itself, on its own. It goes out of DNB by itself, on its own. It is not affected by the neighboring rods.

DR. SHEWMON: I thought most of the tests were done with shrouds around them, to get better flow and to avoid -- well, I don't want to say avoid propagation, but in essence I would think they would strongly inhibit such sort of things.

DR. PICKLESIMER: As far as the single rod test, you are correct; but this was done in a bundle test, without the shroud. It was a nine-rod test.

DR. OKRENT: Again if you're interested in the thermal hydraulic aspects, why in fact would you have looked at NPBF and not have done it electrically, where you'd have a better chance of seeing what's going on, and so forth?

I'm just not sure what it is you think was investigated and what it was that PBF showed.

DR. PICKLESIMER: One of the problems that we have had all along in the out-of-pile programs is to assure ourself that the behavior in and out of pile is the same. We're stating something real. We need confirming test in pile. This is such a test.

DR. OKRENT: Well, let me offer a general request. It would help me as we go through the program, either whether you're talking about result or you're talking about planned experiments, to tell me what it is you think was the important information that was needed first, then how it was thought that this experiment was going to provide this important information, and if it's a planned experiment, what is the chance that it will provide this important information?

Here I'm just trying to find out what it is in fact you think was being sought when this experiment was done, and I really can't tell at the moment.

The reason I ask is you could think in terms of fuel failure propagation, which is certainly a question raised for fast reactors, but if the cladding and one element failed, it could lead to associated cladding failure nearby. But that, in fact, means you are interested in a situation where cladding does fail. If you're talking strictly about a situation, which is what I call thermal hydraulic, not involving cladding failure, that's a different kind of thing, and at the moment I can't in fact tell which it was you think was being investigated.

DR. PICKLESIMER: The question, as I understand it, at that time that was asked, was did film boiling propagate throughout a bundle, once initiated on one rod, and results show that this does not happen. It does not propagate from one rod to another.

DR. SHEWMON: Since the cladding wasn't failed at that time, it was only thermal hydraulics, or did the clad fail during this time?

DR. PICKLESIMER: At the time the question was raised, it was assumed if a rod went into film boiling, it would fail.

DR. SHEWMON: But you said the tests have shown the clad didn't fail when you went into film boiling, and you also said that it did not propagate, and so I'm trying to put words in your mouth, or trying to conclude if indeed it did not propagate during periods when the clad had not failed. Is that correct?

DR. PICKLESIMER: That's correct.

DR. SHEWMON: Okay. It's possible that the EG&G people can come back to what important info is needed on each question. It certainly will be a recurring question today.

DR. PICKLESIMER: That will be discussed by Mr. MacDonald a little later today.

The irradiation effects in the cladding have been shown to be removed once the clad temperatures are above about 900 temperature K, so the cladding then behaves as though it had never been irradiated.

This is not true for the fuel, because this is a buildup of fission products in it.

Another question that has been of concern is in the loss-of-coolant accident, does a rod in pile behave in the same fashion as our out-of-pile or electrically heated rods. And the PBF program has shown that under equivalent conditions the behavior is significantly the same. There is no significant difference.

DR. SHEWMON: Would you tell me what equivalent conditions mean, and especially with regard to pressurization or pressure differential across the cladding?

DR. PICKLESIMER: As best we can tell, the first pressure delta P, first temperature relationship in pile and out of pile is the same. The ballooning strengths are essentially the same in pile and out of pile.

DR. SHEWMON: Now most of the PBF tests were done without appreciable burnup, so there is very little fission gas, and the tests, as I understand it, have been done without internal pressure. Is that right?

DR. PICKLESIMER: No, sir, they have been internally pressurized from low to high pressures.

DR. SHEWMON: Okay. So the fuel often has been pressurized?

DR. PICKLESIMER: Yes.

DR. SHEWMON: And these were in tests of bundles, and you have bundle tests?

DR. PICKLESIMER: No LOCA bundle tests, no, sir. Those were deleted from the program.

DR. SHEWMON: So when you're talking about ballooning here, you're talking about pressurized single element?

DR. PICKLESIMER: That's right. Under equivalent heating rate and steam flow conditions as tested out of pile, the behavior appears to be the same. We see no significant differences.

This gives us considerably more confidence in our out-of-pile tests.

The third problem that was of consideration, several years ago when the program was first set up, reactivity insertion limits. The reg. guides, I believe, for this call for a 280 calorie limit for reactivity insertion in certain kinds of accident situations, to maintain a rod integrity for core cooling purposes.

Some of the late results of the PBF program still have this in question and compare some with one test to another and give us almost contradictory information. We are still discussing the problem.

The gap conductance experience has shown --

DR. SHEWMON: Before you leave that one, there was this analysis, I think, done for you people or NRR at Brookhaven that questioned -- I guess did something that involved reactivity feedback, and showed that indeed the power deposited in the fuel element would probably be appreciably smaller than what had been assigned before.

Has that had any impact on the program?

DR. PICKLESIMER: That's the last reason our last RIA tests are on indefinite hold. We have studied the calculations, we have discussed the problem with NRR. We are in agreement that the most -- that it is unlikely that you car get reactivity insertions of this magnitude in any commercial plant.

Therefore, we have stopped --

DR. TOKAR: I'm sorry to interrupt, Dick, but I'll be discussing that at some length in a few minutes.

DR. SHEWMON: Okay. The work was funded at Brookhaven by Research or NRR?

DR. TOKAR: NRR.

DR. SHEWMON: Well, that will be more believable, too, then. Okay.

DR. PICKLESIMER: The gap conductance measurements have shown that the actual values are considerably larger than they had been estimated in the previous studies.

(Slide.)

DR. OKRENT: What does that mean?

DR. PICKLESIMER: As far as I'm concerned, it means the pellet temperatures would be lower than they would otherwise.

DR. OKRENT: I realize that. But it seems to me the estimate of gap conductance is very dependent on what you assume about what's going on inside the fuel rod.

DR. PICKLESIMER: That's exactly correct.

DR. OKRENT: I guess I'm not sure whether you're trying to tell me that from the PBF test, you know enough to translate a general conclusion to all conditions within a commercial reactor or a well-defined set of conditions within a commercial reactor, or whether it was for this experiment somebody analyzed something and the measurement came out different.

DR. PICKLESIMER: I'm not that familiar with the gap conductance results or the tests themselves. I know that the PBF test with the Halden test have shown that the original models for gap conductance were not correct; that in the commercial fuel operations after not many hours of operation, the original gap that existed between a solid pellet and the cladding has now been distributed between the fragments of that pellet, and consequently the assumptions that have been originally made on the gaps existing between the pellet and cladding are incorrect.

They have been required to change their models, and to improve their calculations.

DR. OKRENT: All right. Let me leave it at that for now.

DR. PICKLESIMER: Now the planned program in the past is coming to an end, and we are moving into a set of new programs, part of which are planned in reasonable detail now, and part of them are not.

The OPTRAN tests, which are the operational transients program, has been planned in reasonable detail. We have at the present time, I believe, seven tests scheduled over the years. These will examine things like the steam isolation valve closure without scram.

There are questions as to what happens to the fuel rod bundle in that case, turbine trip with and without scram, high ramp rate power increase and control rod withdrawal.

The second part of the new test program is that consisting of the severe fuel damage studies, which will be discussed in considerable detail in the following talks.

DR. OKRENT: In the OPTRAN tests, are we going to hear from the NRC what is the information that they think is really needed, and why?

DR. PICKLESIMER: Mr. MacDonald will be discussing that in his presentation.

DR. TOKAR: I'll be discussing some of that, too.

DR. OKRENT: I would like to hear from the NRC, actually, as well as from EG&G, if that is practical, unless the NRC doesn't have these answers.

DR. TOKAR: It's my intention to cover that, I hope to your satisfaction.

DR. OKRENT: Good.

(Slide.)

DR. PICKLESIMER: In severe fuel damage tests that are being planned at this time, not in final detail, by any means, we will be looking at the fuel and rod behavior in bundles. In situations producing severe core damage beyond LOCA, we will be looking at things like the small-break LOCAs similar to TMI-2.

We will be examining the formation of debris in liquefied fuel under various ramping conditions in the bundle. We will characterize the debris that is removed from the test, so that we can construct artificial beds for heat transfer studies, and we will examine the release of fission products in the severe accident situation.

DR. OKRENT: Could I ask a question there? The ACRS, in its last report to the Commission, has recommended that such work is done in PBF and be done as part of some integrated program, that the NRC conduct in the general area of severe accidents.

Is there now some kind of strong coupled coordination between this kind of planning for PBF and other work that is being planned in the Office of Research on severe accidents?

DR. PICKLESIMER: Yes, and there has been since the beginning. I will discuss that in more detail in the second presentation. The answer is yes, it is very closely coupled. We have discussions several times a week with people in the fast reactor groups.

DR. OKRENT: I wasn't talking about fast reactor. I assume the NRC is planning a program on light water reactors.

DR. PICKLESIMER: The fast reactor group .. doing this work, and the light water reactor's part of the work concerns core melt.

DR. OKRENT: If I could ask one other question. You mentioned there is a PBF program review group, or something like that.

DR. PICKLESIMER: Yes.

DR. OKRENT: Do they put out reports or minutes or something?

DR. PICKLESIMER: We put out minutes, yes.

DR. OKRENT: Are they fairly detailed? Do they go into fairly extensive examination in the minutes of just what is being proposed, and what are the merits, and what the recommendations of the review groups are, and so forth?

DR. PICKLESIMER: Not in large detail or considerable detail. There are summary minutes for the discussions, and copies of the handouts in the presentations that were made.

DR. OKRENT: Who are the people on this review group who are not members of NRC or the contractor?

DR. PICKLESIMER: Of the review group itself, the members are required to be NRC members.

DR. OKRENT: Only NRC members?

DR. PICKLESIMER: Yes. Only NRC. We have advisers.

DR. OKRENT: Who are your advisers, then, from outside NRC or the contracto r doing research?

DR. PICKLESIMER: I am sorry. Bob, can you come up with some names?

MR. VAN HOUTEN: Yes. We don't have official advisers in the sense of paid consultants. We have typically the individuals who are the recognized experts within our contracting companies, and we invite the individuals who have an interest in fuel behavior from the vendor groups, but we do not have formal consultants assigned as does the ACRS.

DR. OKRENT: I would suggest that in the near future you consider trying to bring in some people who are neither from your vendor -- from the vendors or from your contractor, and who have some rather breadth and depth in the area for your program and review group, and that they be in the position where they are sort of required to submit in writing their opinion.

I think that would give you maybe another aspect.

DR. SHEWMON: Dave, I suspect that's something we ought to take up with the full committee, because I have been to the pressure boundary group meetings, too, and it's a good show-and-tell amongstthe contractors, but it is not an independent review, and there is certainly no generated document, and I guess I agree with you, because that's sort of more my background from the national labs and other programs, that it is not done throughout, and I suspect if you get something like that set up as enough of a policy matter, it would be worth discussing and seeing if we want to write a letter and make a recommendation.

DR. OKRENT: The advanced code review group does have people from the outside, in fact, and in at least one case they have somebody who was a strong critic of the whole thing. I don't know whether they intentionally got him, but they ended up with somebody and, in fact, I don't think it hurt from the technical point of view to have to stand up to some fairly strong criticism.

DR. SHEWMON: Okay.

(Slide.)

DR. SHEWMON: Let me bring one other point on that. Have the PBF people agreed to any of the things on that last slide, the formation of debris and liquified fuel characterization of debris produced?

I guess if you can be sure that debris was all kept in one spot, they are likely to agree with it, and you might be less interested in whether or not it is simulated. That's sort of a better approach. You know, I'd be interested in --

DR. PICKLESIMER: This will be discussed in considerable detail in Dr. Buescher's presentation. The design of the test train and the design of the tests themselves.

DR. SHEWMON: Okay.

DR. PICKLESIMER: Here is a breakdown -- I'm sorry, not a breakdown, an analysis of the Fuel Behavior Research budget over a number of fiscal years, to give you an idea of how the thrust of the programs have been changing.

In FY '80, more than 70 percent of the Fuel Behavior budget was related to large break LOCA and transient studies.

In '81, that will drop to below 60, and will continue through the years until '84, it will be a relatively small part of the total budget. This is in the phaseout of the LOCA test programs. This is not just PBF, this is the whole behavior branch budget.

Severe core damage studies are starting from a small part, roughly 6 or 7 percent of the total budget in FY '80, to become the major part by 1984.

The steady state and normal operation programs will remain small, less than 10 percent, throughout all of the time. These programs are the type of zircaloy in-pile creepdown studies that Dave Hobson has just completed at Oak Ridge, the work that Tom Kastner has been doing or is doing on stress testing.

The section called "others," I'm not sure what all is in there, but part of that is code development. The cross-over part comes in about FY '83, between the severe core damage studies and the LOCA studies.

(Slide.)

DR. SHEWMON: The top two of those were PBF and the bottom two were non-PBF?

DR. PICKLESIMER: No, sir. That is a different breakdown. The PBF has part of the LOCA and transient studies, but not all of it. MRBT, for example, is included in that.

DR. SHEWMON: What's MRBT?

DR. PICKELSIMER: Multi-rod burst test, that Bob Chapman is running.

Now there has been discussion of the major fraction of the fuel behavior budget that the PBF program constitutes. Here I would like to show you what we see as the ratios in the top curve -- I'm sorry, let me go to the bottom curve first.

Here is shown the ratio of the future budgets to the FY '80 budget, showing that we are dropping from our present budget FY '84 to about 80 percent of our present budget.

DR. SHEWMON: Are those 1980 dollars?

DR. PICKLESIMER: Yes. The part of the budget that constitutes the PBF program remains almost constant throughout that time, at approximately 50 percent peaking in FY '81, because of problems in starting up the severe fuel damage studies.

(Slide.)

Now in the PBF program itself, this gives you the breakdown of the budgets for the different tests, LOCA, PCM, RIA and OPTRAN testing will be decreasing from the present time to where it is only about 30 percent of the PBF budget by 1984.

In contract to that, the severe core damage studies are going from a small amount of a couple of hundred thousand dollars this year to become 70 percent of the funding by 1984.

Now may I answer any other questions?

DR. SHEWMON: I guess we've run out for now. Thank you.

DR. PICKLESIMER: All right.

MR. JOHNSTON: I apologize to the committee that I did not get the opportunity to make any Xerox copies of the viewgraphs I'm going to use in my portion of the talk, which is principally introductory.

However, Mike Tokar who is discussing the PBF program in greater detail, does have copies and pass-outs of his presentation.

My name is William Johnston. I am the chief of the Core Performance Branch in Nuclear Reactor Regulation Office. Until two months ago I was in Research area, and I am still learning my new title.

I am going to give a very short description of the licensing basis that we use in NRR and the objectives, and use that simply as a preliminary to describing what we feel are some of the user's needs that we have related to in-pile program.

(Slide.)

The first one is simply to show the objectives that are included in our field system safety review, and the first one is that the fuel system is noc damaged as a result of normal operation and anticipated operational occurrences.

This is really a statement of the general design criteria No. 10.

The second one -- and that relates to normal operation.

The second one is that if you do have an event which causes damage to the fuel, that (a) it never be so severe as to prevent the ability to put the control rods back into the reactor and stop any reaction that may be going on.

The third is that when you have failure, that you never underestimate the number of rods that may have failed, principally for the purpose of assuring that the radioactive release in doses that would be escaping from the fuel would not be greater than would be calculated by the appropriate groups.

And, finally, that the coolability of the core be always maintained. This is normally defined as maintaining a rod-like geometry.

However, the recent events at TMI and other places have indicated that certainly coolability can be maintained, even in the absence of specified geometric relationships. But that is the way it is presently defined.

(Slide.)

I have a statement of general design criteria 10, which I referred to before. The principal point is that things are designed with an appropriate margin to ensure that the SAFDL, which are the specified acceptable fuel design limits, are not exceeded during the conditions of normal operation, and I will refer to this. This is pertinent when we are talking about the operational transient type thing.

(Slide.)

Each of the four criteria that I have there can be referenced in terms of 10 CFR Part 50 criteria. I have some viewgraphs on it, but I think to save some time I will leave those out and provide them separately.

But basically the way that we operate to ensure that the fuel rod integrity is maintained under normal operation and under transients that are less than the limiting ones we regulate against, fuel element overheating, by setting DNBR limits. It says the DNBR should not go below 1.3 or appropriate agreed-upon numbers in that general range, to ensure that the rods do not overheat and go into DNB, because the assumption has been that if that happens, the rods will immediately rupture and destroy themselves, and that was pointed out in Dr. Picklesimer's talk, that that has not been shown to be the case.

Our second way of controlling overheating is through the LOCA acceptance criteria, which limits the temperature that can be reached, limits the amount of oxidation that can take place, and the remainder of the thermal hydraulic parameters that can draw the limits that we place on the vendors, including the neutronics limits, the F sub Qs, which limit the power distribution and the power shapes within the cores.

The second event we are trying to worry about is the loss of geometry and embrittlement which can, of course, cause the loss of geometry, and the criteria again are the LOCA acceptance criteria, 17 percent of 2200, and in the reactivity initiated area, because of the RIA initiated limits of 270 calories for a rod breakup.

Thirdly, we are concerned about damage from mechanical means. That is principally taken care of by the 1 percent clad strain limit. That particularly reflects internal expansion on the rod.

One that's not totally clear to all of us, why the 1 percent was picked. That's a uniform strain. It doesn't take into account local strains on the rod which may greatly exceed that limit, nor does it cover the PCI situation in which you can indeed have wide failures at nothing anywhere like a 1 percent average clad strain limit.

Principally the 1 percent had to do with the fact that if you had any melting in the fuel, the subsequent expansion might cause internal expansion and cause the rod to fail.

We feel that's an area where we are not real covered at the present time.

The final one is the assembly mechanical damage. This has to do with seismic effects or any type of thing which would cause geometrical changes to the fuel assembly, crushing of the grid, any things of that sort, and that is presently based upon limits on the yield and the ultimate strengths of the zircaloy, of the material that the grids are made of.

DR. OKRENT: Before you remove that, what does the combination of terms mean, fuel rod integrity, then loss of geometry and reactivity insertion limits?

What is the Staff goal, in your opinion, in that area?

MR. JOHNSTON: The Staff goal in all of these is to operate the reactor in such a way that the clad is not breached. In other words, its integrity is not lost.

In the case of the RIA, there are two criteria in Reg Guide 1.77. The first one for boiling water reactors says you shall not permit, I think it's 170 calories per gram of reactivity insertion as a method of precluding rod rupture. That is not a geometry-related criteria, but it is related to dose release from gases that would be in the plenum.

The second criteria for a BWR is that 280 calories not be exceeded, and that does indeed relate to a possible loss of geometry, the rod coming into pieces and so forth.

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In the case of the pressurized water reactors, the first limit on failure to the rod, we use the DNBR criteria, and for the upper limit, for PWR, it's also the 270. So it's to preclude the loss of geometry is the 270 limit.

DR. OKRENT: Now I assume in all cases, certainly in the LOCA acceptance, you don't insist that the cladding not fail?

MR. JOHNSTON: No, that is the limiting case. Category 4, if you like.

DR. OKRENT: All right. Now how about the reactivity insertion accident? I'm not talking about the rod going out a few notches, but the rod drawback in BWR. What is it the Staff thinks is the objective that should not be, you know, overreached, if one should occur?

DR. TOKAR: 10 CFR 100 is essentially what we are trying to avoid exceeding in terms of the radiological dose limit. So, in effect, you can have failed rods doing a rod ejection accident or PWR rod ejection accident, or BWR rod drop, just so long as you don't exceed -- I forget what the accident evaluation branch uses as their internal criteria. A small fraction or whatever.

MR. JOHNSTON: 10 percent, isn't it?

DR. OKRENT: But 10 CFR 100, you are supposed to meet if you release all of your noble gases, and 25 percent of the iodine of the containment. That's a substantial amount of activity.

So again I'm trying to see what it is the Staff thinks it is trying to meet for what I will call the reactivity insertion accident, like the rod drop.

MR. JOHNSTON: The two criteria, the first one, the high limit, is because of a geometrical concern, and we certainly are acting against that one.

The second one is the DNBR, the 170, which is the rupture, which is the Part 100.

DR. TOKAR: I don't want to steal Bill's thunder, but the fact is I'm going to be covering this in fairly great depth in the first part of my presentation, so if you can hold off till then, I think I will satisfy you. I hope.

MR. JOHNSTON: We have a new position on the need for the RIA test and new estimates of what the real activity insertion is in the accident in the real PWR or BWR, so that to some extent, these are a little bit moot at the present time.

DR. OKRENT: Well, they are moot now, but the question is whether the objectives that you are trying to meet, vital objectives, important enough that you should devote whatever it is, the equivalent of a year of PBF time to it.

DR. TOKAR: If you can just wait a few minutes, I'll get to that.

(Slide.)

MR. JOHNSTON: My final viewgraph before Dr. Tokar takes over, is to give you what we feel are some of the upcoming areas in which we shall be having to make licensing decisions or in terms of things that are coming in from the licensees, in the way of requests, or things which we feel need to be done as part of the functions that we have internally.

On the right I have tried to indicate in-pile programs, not necessarily just the ones we are talking about today, but which will provide input to us in resolving these issues, and which I would say these are the basis for user's needs, and in most cases here, there is a user's need which has been written and delivered to Research.

The first one is that all of the reactor vendors are moving to longer time -- to higher burn-ups. There are programs by EPRI and DOE which are supporting this sort of thing, and there is an intent to go into longer cycles, or 12-month cycles.

We are beginning to get requests to increase either the pink pellet burn-ups or the assembly average burn-ups into the 45 and 50,000 megawatt days. Presently they are done around 27 or 35, and we will be needing to make decisions in those areas.

The three issues that I think are important now are the fission gas releases as the burn-up goes up, and the effect of transients which may occur to rods which have the high burn-up.

Therefore, they are both steady state and transient issues.

Regarding fission gas release, pellet cladding interaction is a failure of a mechanism of a mechanical nature which can occur even though you never overheat the heat or go above the DNBR ratio considered safe, and yet that is the situation, that the high burn-up rods find themselves in. The gap is essentially closed.

Operational transients of various sorts will be expected to put through stresses on these rods, and it is uncertain at the present time what kinds of numbers of rods would fail under these conditions, and we feel we need information in that area.

As I say, EPRI and DOE are providing that, and I should have PBF in here. I don't know why I left it off. But that's part of the OPTRAN that relates to the OPTRAN program.

Finally, the cladding attack itself on the cladding corrosion hydriding and crud build-up is addressed by --

DR. OKRENT: Before you go on, now, I would like to explore a general question in this context. I don't disagree that the NRC needs to look at how fuel will behave in normal and expected transient kinds of conditions, to see that either you don't anticipate any fuel failures or a very modest amount or whatever it is, that is thought to be acceptable.

In fact, there was a period some years ago when I was asking the NRC Staff how it was they were ignoring the general design criteria.

What isn't clear to me is how you decide what the responsibility of a utility who wants to do this is, and of course, his back-up groups, and what you consider to be the requirement for the NRC to do something or to prove something, or so forth.

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In other words, it is conceivable to me that it should be the industry that is running these PBF experiments, if they are needed, in fact.

So I would appreciate hearing your point of view on this question.

MR. JOHNSTON: Here I am going to reflect my new position.

DR. OKRENT: I am asking in terms of your new position, indeed.

MR. JOHNSTON: We don't care where the information comes from, as long as we have it and can use it to make an appropriate licensing decision.

Our initial thrust is always that it's up to the industry in their submissions to convince us they have made their case, looking for them to provide the data, and in that sense, indeed we feel that the monkey is on their back to prove to us what it is they want to do is okay.

Now our problem is that we want to have some information in our back pocket with which to make a judgment on what they are giving us. So that gives us an incentive to have additional information coming in from other sources than what industry is presenting to us directly.

And so the answer is both. We feel the main thrust -- they are the ones that want to get the change. Therefore, I think the principal responsibility is indeed on their side to provide it. But we have to make a judgment as to whether they have given us unbiased information, where they have left something out, things of that sort, and that's where we have the requirement for additional information.

A case in point, I think, is the DNBR situation, for example. I think PBF has shown in a number of tests they have run, that rods can indeed operate for periods of time in film boiling, without undue circumstances. That hasn't been done on a large test bed to provide any statistics about it. Industry seems to be very anxious to have adjustments made in the DNBR Limit. They are continually working on the edges of limits of their F sub Qs, and working up against the DNBRs with certain particular plants and vendors.

But the incentive is on their side, I think, to show to us that that rule can be relaxed.

DR. SHEWMON: Let me ask one information question on that. When you say you are picking at the DNBR limits, or FQs, I take it and hope that you are picking at them in the limits of certain transients, and no way during steady state operation, but during any part of the cycle. Is that correct?

MR. JOHNSTON: It's transients, that's correct. No, not normal operation.

DR. SHEWMON: Go ahead.

DR. OKRENT: Well, again, I certainly think you need somehow to have an ability to evaluate what it is that the utility is presenting to you. I am all for that. But that is not, to me, identical to your need to run an experimental program.

If, in fact, you are not satisfied that their work in steady state irradiation has convinced you that for the higher burn-ups they know that the cladding will withstand the transients, for whatever reason, then it seems to me you could pose to them that they haven't made their case, and that it is their responsibility to make the case.

MR. JOHNSTON: That is certainly true. Often, however, there is a time constraint in which some decision has to be made, and we often find ourselves working in that mode.

I think that F here is perhaps the best way to put it, because the modeling codes which we use to evaluate the vendor's inputs which are code-generated inputs, we have to have our own set of codes with which to evaluate their inputs.

Our codes have to be verified. We have to know their limitations. Much of the work being talked about here is providing the inputs to the codes or the inputs to verify and evaluate the codes, and I am talking about the fact that we are in the process of adopting many of the best estimate codes now which have been developed by Research, and which have been verified on the basis of data obtained in these programs.

And that, to me, is the fundamental point.

DR. SHEWMON: Well, it's my feeling as we get to specific cases, we can make this argument more concrete and helpful.

DR. OKRENT: Yes, but see, this is, I think, a very specific case and, of course, it introduces some very difficult technical questions as well.

DR. SHEWMON: You notice that NRC is not any of these up there.

DR. OKRENT: No, but he said they left PBF off.

MR. JOHNSTON: I'm sorry, I left PBF off, but Halden contractors and our subcontractors are providing that information to us. We don't get our information from only our source. We get it from as many sources are are available.

The second part is the clad rupture ballooning and bundle blockage, which will be a subject of the September 3rd meeting, in great detail.

We clearly have a need for new data and data on larger size assembly, and that is why we feel very strongly that both the NRU program and the SUPERSARA programs will provide important and, in fact, invaluable input to our decisions in this area. Not this year, but in about three years.

The ATWS and operational transients are different from A up there, in that again the PCI, the pipe cladding interaction, is the key issue there, and the PBF OPTRAN program is directly related to that.

We also have members of the demo ramp program in Sweden. Our researchers joined that program, in part to get data on the effect of delay on the failures.

The contention has been made that the transients are of such short duration that there is no time for stress cracking corrosion to operate, and that is a debatable point.

This program looks specifically at holding times.

DR. SHEWMON: The demo ramp program is done in a test reactor?

MR. JOHNSTON: It's done in a test reactor, the Stuttgart II reactor.

DR. SHEWMON: If we come back up to PCI, EPRI at least has made up their minds some years ago that all this iodine, how it gets there, is moot. Does DOE have that firm a position in the way they lay out their program?

MR. JOHNSTON: No, DOE is looking at it more from a demonstration technique of what can we do to fix up whatever it is causing the difficulty. They have the programs of putting various materials, zirconium or copper, on the inside of the cladding.

The other parts of the DOE program look at hollow pellets or powder or other techniques which will relieve the stress applied.

DR. SHEWMON: Okay. We'll get later into what PBF might do in this program then.

DR. OKRENT: Will we get later into a discussion of what PBF will do on item 7?

DR. TOKAR: Yes.

MR. JOHNSTON: D, to identify fuel failure mechanisms is a part of our standard review plan, to try to set up acceptance criteria to be used in the licensing, and although in many cases the industry uses bounding type of calculations, without going to specific failure mechanisms, we feel that we have to have enough understanding so we can be sure that failure mechanisms are not ignored, or that we don't understand what it is that is causing the failure.

We presently, for example, are considering some kind of a criterion to determine whether the level of failures experienced in presently operating reactors -- let me put it another way.

A number of rods failing during normal operation in reactors is very small now, the order of a few hundreds of a percent.

On the other hand, we get certain occasions where you begin to get a cluster of failures. We are looking for a criteria to distinguish between random fuel failures, which are statistical in nature, and those that are caused by some mechanism that is beginning to operate, like the waterlogging and the stuff at Maine Yankee, things of that sort.

So partly we need to have some feeling as to what is going on with regard to mechanisms. That's PBF and Halden.

DR. OKRENT: You have lost me on what it is you think this criterion is supposed to do. Is it some kind of a licensing criterion or what?

MR. JOHNSTON: It's a review criterion, in reviewing the SAR reports that come in for new plants as well as reloads, but principally for new plants.

We review their input against certain criteria. In our standard review plan that was recently revised, we have stated some of them, and this is basically back-up for those kinds --

DR. OKRENT: But the vendor designs the fuel not to fail, as best he can, and if he knew of some systematic failure mechanisms, he would try to avoid it?

MR. JOHNSTON: Correct.

DR. OKRENT: Now obviously things don't always go the way the designer intends, but I don't understand really what you're saying is the criterion and how then that impacts on research. So could you take that?

MR. JOHNSTON: All right. The DNB criterion, just to pick on that one again, that simply states that you shall not go into film boiling --

DR. OKRENT: No --

MR. JOHNSTON: There's nothing mechanistic about that. That says nothing -- that has nothing to do with description of what nature is doing under those conditions. It just says don't let DNBR -- it's simply that kind of a statement. There is no mechanistic understanding, no judgment made as to whether that is a conservative criterion. And our point is I think one should go, in this present state of knowledge, go beyond simply statements of that sort which have nothing to do with physical reality.

DR. SHEWMON: Yes, but why, if they are down to one and ten to the thousand failure rate, several people have interest if it goes up to an order of magnitude greater, and your plant is designed to cope with yet an order of magnitude greater than that? Why is this a priority item for the NRC?

MR. JOHNSTON: Things like the baffle jet problems which appeared, for example, are -- I will not say they are earth-shaking, but a general design criteria says that we set the SAFDL limit, which says that we are trying to -- that the acceptable level is to move toward as small a number of failed rods in the reactor as possible.

DR. SHEWMON: I think you and I should keep working on that, but the proof comes in running reactors and learning about, oh, things like the baffle jet problem.

I don't see why that leads to an NRC research need. It leads to an NRC awareness need, I grant.

MR. JOHNSTON: It leads to an awareness that a baffle jet is strictly a mechanical problem, so it may not be the best example I could have picked. But a better one is the failures at Maine Yankee, when apparently there were PCI failures, there was larger gas release into the rods than would have been expected under nominal operating conditions, yet that was the observation. That's a trigger that says there's something peculiar going on there that needs to be looked to further.

Now we generally have to make decisions when those events occur as to whether that reactor can start up again, whether they've got to do a great big program before they can start up.

In order for us to make those kinds of value judgments, we need to have some feeling about what it is that might have gone on. That's the kind of thing we will have to decide whether they can start up again or what they can do before they start up.

We would like to have that background. We go to the literature and current understanding for that.

Item E is the estimate of fuel damage in excess of design basis, and that relates to the TMI situation. We expect that if such an event ever occurs again, that we will have to be making the decisions as to when you can shut the pumps off, whether you can get natural circulation in the system, how you will handle and manage the accident.

We need, therefore, to have understanding about events that go beyond simply the design basis events, be it LOCA or what else it be.

Two items that come up in that have to do with fission product detection, the ability to estimate how severe the damage was by analysis of the fission products, both radio-isotope and chemical, that are in the primary coolant.

The coolability limitation has to do with the knowledge of whether the system has in fact lost geometry and whether that means it has lost coolable geometry or not, and how can one bound the estimates one will have to make under those conditions.

The two programs providing information in that area are the PBF severe core damage studies and the SUPERSARA severe core damage studies, which I guess you will hear more about today. But we feel the need for that kind of information as part of the total understanding of more severe damage than we have been looking at in the past.

Finally, we have been using at present modeling codes in which we put conservative models or we used conservative material parameters in them. The best long-term solution is to work from a best estimate with nominal inputs with uncertainty bounds on the output, and to license against the upper bounds of uncertainty.

We are moving in that direction by modifying codes in a gradual way to become more best estimate. We are beginning to use the research codes FRAPCON and FRAP-T. We need to have a good verification or good understanding of the codes for a variety of conditions, in order to take that next step.

The information for the steady state codes is coming very heavily from Halden reactor projects, and to some extent from PBF, and the information for transient verification is coming, as we indicate there, from PBF, from the NRU program, and from other portions of the SUPERSARA program.

And, Mr. Chairman, these are our estimates of the kinds of needs that we expect to have to respond to in our branch in the future, and we are interested in having that information available as reflected in these programs.

DR. SHEWMON: Okay. Now will Tokar be speaking from the same list?

MR. JOHNSTON: He will be speaking specifically about the things that have the label PBF on them which will cover relatively a large range of what's up there, but not everything.

DR. SHEWMON: And we can get a copy of these summary slides?

MR. JOHNSTON: Yes, you will. I'll leave you the viewgraphs, at least.

DR. SHEWMON: I'm a little surprised at fission product detection. That must have been a point of some concern 30 years ago.

MR. JOHNSTON: That's been a concern of the ACRS for a number of years and PBF installed an on-line detector in PBF about three years ago. The vendors have installed on a guest basis, one of their own systems in PBF for a period of time. It's used for several purposes. It can be used to identify the failures during normal operation, whether PCI or birth defects or whatever, to give you a feeling as to whether it's new or old fission products, or whether it's TRAMP, but the more recent push has been as a part of the TMI accident, because of the difficulties there in determining the amount of core damage through analysis of the coolant. And that is one of the TMI action plan items, is to provide the ability to sample the reactor coolant after an accident, and determine the extent of the damage.

DR. SHEWMON: Without burning --

MR. JOHNSTON: Without burning up a lot of people. And this is one of the places where one can do controlled experiments. You can cause the fuel element to rupture as you wish, and then determine what comes out and what quantities and what isotopes are important.

DR. SHEWMON: Okay. Thank you.

MR. JOHNSTON: Thank you. Dr. Tokar will speak next in regards to the PBF.

DR. TOKAR: Well, if we are keeping up with the agenda schedule, Dr. Johnston and his introductory remarks left me about 10 minutes to give my remarks.

My name is Michael Tokar. I am a senior reactor fuels engineer in the Reactor Fuels Section of the Core Performance Branch, of which Bill Johnston is the branch chief, as he said earlier, lo these many days now. Two months or so.

The material I will be covering in the next few minutes was originally slated to be covered by Ralph Meyer, who is the bureau section leader. So if I don't cover it to your satisfaction, I guess you can take it out on Ralph the next time you run into him at the next meeting. He will be at the meeting on Friday.

MR. JOHNSTON: That's why he's not here now.

DR. TOKAR: What Ralph asked me to cover was the basic question, how is PBF addressing reactor fuel licensing needs.

(Slide.)

One way I thought I would try to give you an answer to that basic question was to first of all give you some historical perspective from the standpoint of where we were, say, two years ago.

Two years ago we were asked to rate the PBF priorities in terms of a program as it was scheduled then, and from a reactor fuels strictly parochial type of view, we rated them in this order:

The RIA, the reactivity initiated accident, was rated No. 1. In our view, we thought that we needed to have that work started and finished as soon as possible, because we had some unidentified deficiencies in our license position in that area.

The second thing on our list was the LOFT lead rod, because we thought the results were needed, obviously to be consistent with the LOFT schedule.

Power-coolant mismatch was No. 3. We didn't see any particular urgency for this test, because we felt the DNB failure criterion was conservative and particularly so for the overheating events.

Flow blockage had even lower priority in our opinion, because flow blockage involves PCM, and as we just said, PCM was covered adequately, we thought, in a licensing way by the DNB criteria.

And the last on our order there was the LOCA test, which we felt given the thermal hydraulic conditions were analyzed relatively completely.

DR. OKRENT: I must say, your interpretation of flow is a completely different one than was the original recommendation from ACRS, that work in this area be done. But let me just note that. It has no relation.

DR. TOKAR: Well, as I said, this was a rating that was, we confess a strictly parochial, narrow view.

DR. OKRENT: Not only that, but the kind of flow blockage people had in mind was -- it wasn't a question of PCM, it was a question of gross overheating, melting, or so forth.

MR. JOHNSTON: The assumption was if you exceed DNB, you automatically get this melting and so forth that you described, and some of the results have suggested that is not the consequence.

DR. OKRENT: Well, again --

MR. JOHNSTON: You are correct, that was the original perception. Also the spreading of it to associated assemblies.

DR. OKRENT: In the BWR, you have closed subassemblies.

MR. JOHNSTON: Yes.

DR. OKRENT: And if for one reason or another you block the inlet flow to -- it's not a PCM question that you're talking about.

DR. SHEWMON: You mean 100 percent?

DR. OKRENT: Or large enough, in fact, that you really get gross overheating and, in fact, there are reports from GE doing analysis on this kind of thing. It's just a completely different question which has mewhat been reinterpreted to something else by the Staff.

DR. SHEWMON: Why don't you go on?

DR. TOKAR: In any event, I'm not responsible for this ranking.

(Laughter.)

So I'm not trying to defend it, okay? And you will remember from Pic's presentation that he had a different ranking in terms of what the PBF review group had ordered these things in, and the reason for that was that we had in effect a vote on how to rank those things, and reactor fuels vote was, shall we say, only part of the total, and obviously other people had a different idea about what they thought was more important.

(Slide.)

And particularly the thermal hydraulics people. So that's why the PCM tests were ranked No. 1 ultimately.

MR. JOHNSTON: The history is that ACRS wrote a letter saying what they felt the priority should be for the PBF test. That was in '72 and '73. The ACRS ranking of priorities was PCM, LOCA and RIA, in that order, and that test plan reflects -- has reflected that all along.

DR. OKRENT: Yes, in PCM, then, in the concept I have just indicated where we're talking about large scale blockage leading to gross overheating, it was a different PCM than you did.

MR. JOHNSTON: The PCM for a PWR was not the same as a BWR issue I thought you were just speaking about.

DR. SHEWMON: Could we go on? What the ACRS wrote seven years ago is of some interest, but not overwhelming.

DR. TOKAR: Okay. I hope what is of some interest today, and what you have already indicated is of some interest is the RIA question, and luckily enough, I think I have enough background to be able to answer that, hopefully, to give you some idea about where we are coming from and how we got to where we are today.

I have given you what our ranking was two years ago. Now the reason we had that ranking in that order with RIA on top stems from the following considerations:

First of all, general design criterion 28, which is in 10 CFR 50, Appendix A, says essentially two things: Protect the pressure system boundary, and also maintain coolable geometry.

Those words, in effect, are that intention, followed up with the Reg Guide 1.77 on PWR rod ejection, which references GDC-28 requirements, and say the failure consequences from the SPERT test in particular are insignificant, below about 300 calories per gram, and therefore 280 calories per gram ought to be good enough.

Those words, in effect, are also repeated in the Standard Review Plan. This is for PWR rod ejection, and BWR rod drop.

And in addition, in those sections are provided failure criteria, that is the so-called incipient failure criteria which, for the BWR case at zero or low power, is 170 calories per gram, and is DNB for PWRs and BWRs at power.

(Slide.)

So one of the major concerns is this coolability limit. The coolability limit, the 280 calories per gram, radial average peak fuel enthalpy number, as I said, was derived basically from SPERT data, but those tests were conducted mainly on unirradiated or low burn-up rods.

And in addition, that 280 calories per gram, as I said, was chosen to avoid prompt rupture and dispersal of molten fuel, and the damage that could ensue from that mechanism to the primary system boundary, and to the core configuration itself.

There are two basic problems with that. First of all, the SPERT data were reported as total energy. That is the integral of the reactivity pulse and what is the fuel enthalpy which allows for heat transfer, and therefore the SPERT, the 280 calories per gram value radially averaged peak fuel enthalpy number, corresponds to about 230 calories per gram radially averaged peak enthalpy.

In addition, more recent tests from the PBF RIA series, the RIA 1-1 test showed that there was some severe fragmentation at energies below this 280 calories per gram number.

DR. OKRENT: Excuse me. Is it the integral or the radial average peak enthalpy that is the number you are referring to in item 2?

DR. TOKAR: That's the radial average peak enthalpy. The actual number that corresponds to the RIA 1-1 test has been estimated in a number of different ways, and I don't know what the last estimate is. Maybe Phil MacDonald can comment on that, but at one point I think it was either 240 or 250, somewhere in that range.

MR. MAC DONALD: 285 calories per gram radially averaged energy, as measured five different ways. And it has a plus or minus about 11 percent.

MR. VAN HOUTEN: And it did a great deal of damage.

MR. MAC DONALD: That represents a total energy insertion of about 330 calories per gram, which would be the comparable number to compare with SPERT publication.

I might add one other thing. Delayed fissions are also important in that difference between total energy deposition and enthalpy. Increase in delayed fissions can be significant in some of these test reactors, and yet play no role to the fuel damage, but contributes significantly to the total energy deposition.

(Slide.)

DR. TOKAR: Okay. As a result, in any event, of what appeared to be a relatively sparse data base that was in support of this 280 calorie per gram number, we issued or drafted, I should say, a user's need from Denton to Levine. This was about two years ago, asking for power ramp data, on high burn-up, that is greater than or equal to 20,000 megawatt per day ton rods, to be tested in this general range of 280 calories per gram, in order to be able to get a better handle on whether that number was in fact a conservative number.

At about that time, or shortly thereafter, the ACRS questioned the need for RIA on the grounds of probability, on the grounds that, as I understand it, the concern was that was a relatively low frequency kind of event, and therefore perhaps the resources to be expended in those kinds of tests could be put to better use on something else.

We considered that argument for some time, and ultimately responded by saying that we couldn't answer the probability question, that if in effect it was low probability, we'd never encounter such an event, and if it were higher probability, of course, we would not be prepared if it came unexpectedly.

And so due to tests, anyhow, to resolve this coolability issue, the document kept bouncing back and forth through the concurrence chain, as we continued to try to get a better handle on this whole issue in terms of its safety significance, and so finally this spring we asked our physicists in the Core Performance Branch to take another look at the coolability issue and the RIA issue in general, and try to get a better handle, a more realistic estimate, if you will, on what the peak fuel enthalpies would really be, and whether, in other words, we were realistically going to expect to come up against this limit in the real case or not.

What happened then was that they had consultants at Brookhaven do some calculations, using the so-called TWIGL code, in which they took into account moderator trip feedback, took into account rod bank -- realistic rod bank motions, realistic rodworths, and so forth, and the ultimate result was that they determined that the 280 calorie per gram number would in fact not be approached when these so-called ultra-conservatisms were eliminated.

And some numbers I might give you for that are for the BWR rod drop case, the range was something like 60 to 100 calories per gram radially averaged peak fuel enthalpy.

For the PWR rod drop case, the numbers, as I recall, were something like 90 to 120 calories per gram peak fuel enthalpy.

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So as you can see, the so-called coolablity damage level was not approached, or would not be approached by so-called realistic estimate.

DR. SHEWMON: When you talked about, we respond that the probability issue cannot be resolved, did that letter ever get out of the concurrence chain?

DR. TOKAR: It was an internal memorandum that was issued at least internally. Whether ACRS ever saw that or not --

DR. SHEWMON: I suspect a better summary would be the document was prepared but never sent; or if it was sent, I'd be interested in seeing a copy. Nobody here ever remembers seeing it.

DR. TOKAR: Whether or not it was ever sent to ACRS formally, I don't know. I do know that a document had been prepared, signed and dated, and so theoretically is in the PDR.

DR. SHEWMON: Ever so often we inquire about things in the PDR that we don't get to see, but go ahead.

DR. OKRENT: Before you go on, it is probably worth nothing that the estimates of probability that were available at the time are those that were made by the Staff, at least for the BWR, and it was the Staff's estimate on probability that were very, very low for the rod drop event. It was the licensing staff, too, I am talking about.

So when you say it cannot be resolved, I don't know quite what that means, because the Staff argued to the ACRS, because this probability is so low, we don't need to do something.

DR. TOKAR: What I meant by that is we are always in sort of a Catch-22 situation on questions like this. On the one hand, we are asked on occasion to make estimates of probability. And on the other hand, we are taken to task by intervenors or whatever on the basis that, well, you don't have enough operating experience or whatever to be able to know for sure that those probability estimates are correct.

I don't know what anyone would have come up with as to an estimate of the probability of TMI-2 before it occurred. I presume it would be very low, and yet it did occur.

Now in hindsight, I presume those estimates are considerably lower. But two years ago --

DR. OKRENT: Nevertheless, I think it's relevant --

DR. TOKAR: I understand that.

DR. OKRENT: I have a different question. You say you want to resolve the probability concern. If you could put that viewgraph back on.

(Slide.)

DR. OKRENT: What I have heard said is what was the coolability concern and, in fact, how it was felt that this related to this limit of calories per gram. You are talking coolability, if I understand it correctly, and it not being the primary boundary.

DR. TOKAR: That's not correct.

DR. OKRENT: Then tell me what coolability you mean.

DR. TOKAR: Okay. I'll try to give you more detail. The 280 cal per gram number, as I understand it, if you look at Reg Guide 1.77, in fact it goes into that, I think, rather explicitly, and I have a copy on the desk there we can look at, if you wish.

But that talks about damage due to prompt fuel dispersal, prompt rupture and dispersal of molten fuel, damage to the primary system boundary, and damage to the core integrity in the sense of being able to maintain rods in rodlike coolable geometry.

That was the intent, as it's been explained to me, by Howie Ritchings, the RIA guru of the 280 calorie per gram Reg Guide 1.77 origins.

DR. OKRENT: What I am getting at is my understanding of an RIA, if it were to occur, and if you were to approach the 280 calorie per gram enthalpy is it would only be a modest part of the core that would reach these temperatures, because it's very much a power peaking situation. So much of the core is not affected to the point of getting to high temperature.

And also even actually there is only part of the fuel that gets this hot, because you are talking about the peak level, and so assuming that you did get damage, it would be in a confined region.

In general, this is an event that is starting from a low power level, so you are not in a position where you have a high stored heat, and so if you asked yourself, what's the coolability question assuming it occurs, and assuming you had some dispersal of fuel, it seems to me you may not be sure there is a coolability problem unless it distorts the core enough you couldn't get your rods in, or there is a large enough pressure pulse to do something else, or so forth.

It's not clear to me that the local damage itself is intolerable. I haven't seen any analyses from the Staff or anyone that say, yes, if you just get this local damage, one set assembly or a fe subassemblies, not over the full set assembly, but in the peak region, that this is intolerable.

So the history was GE was arguing you could use 425 calories per gram, and the Staff and ACRS said we're not willing to go that high. When you're that high, you may have a lot of the core involved, and so the peak is around 280 or 300, you know.

The local damage itself is not likely to be bad enough that you can't tolerate it.

Now what I'm somewhat concerned about, the Staff suddenly seemed to be worried in the local region and said, gee, we have to stay below that, too, without, I think -- at least I haven't seen it -- the benefit of analysis saying we really will damage the primary boundary or really damage our long-term ability to cool the core if we have damage in the local area.

MR. JOHNSTON: I think you raise an interesting point. I think it has much broader implications than just the isolated issue you have raised.

First, let me point out a history thing. As Mike pointed out, the original concern was on the basis of the SPERT test in which as you progressively put energy into it, you saw the rod begin to come into pieces, usually associated with melting of the cladding, and finally melting of the fuel at 280 to 300 calorie range.

I'm not sure if there are any pictures as part of the presentations today that show that sequence. But in those times I believe there was a great deal of concern about fuel coolant interaction when you produce molten fuel under those conditions. You would get your possible pressure pulse which would challenge the primary boundary, which I think was the primary reason for the regulation or the criteria.

Then, secondly, the region that exceeds that is not just the peak pellet, so to speak, but there's some debate about how flat, over what length of the assembly, would you exceed those energies.

Now we have learned from the experiments in Japan and in PBF that the real issue in the RIA, unless you really pull it apart, is really going into film boiling. What happens is you exceed DNB over various lengths of the rod, it's some film blanketing, and essentially sits there and oxidizes, embrittles to varying degrees, and then gets a quench, a sharp quench, when the system rewets, and you do get a loss of geometry. If you like, you get cladding fracture, things of that sort, which gives you your loss of geometry. That can occur at slightly lower input enthalpies, as we now know, than some of those limits. But that was part of it.

And the feeling was, I believe, that we are not allowed to permit loss of geometry in one part of the core, even if it doesn't extend to the whole core. This is the issue you brought up that I find interesting, because, for example, in the case of LOCAs and other accidents, we are not allowed to assume that only a small fraction of a bundle balloons and blocks or does something like that, or one assembly out of the core does it.

We essentially have to try to avoid this occurrence anywhere in the core. That's a degree of conservatism that I think we are operating under. We try to avoid loss of coolable geometry in any portion of the core.

DR. OKRENT: Again I ask what your definition of coolable geometry was.

MR. JOHNSTON: Loss of the ability to model it with the code is what it really ended up.

DR. OKRENT: That's not a loss of coolable geometry.

MR. JOHNSTON: It is as defined for these purposes, as I read the reports

DR. OKRENT: You were telling us how in fact even a TMI core might be coolable.

MR. JOHNSTON: I agree, but we can't give credit for that under the present rules.

I agree with you. I'm just trying to tell you what

I think we are constrained to.

DR. OKRENT: Originally there was concern about pressure boundary from the pressure pulse point of view, and the loss of coolable geometry on a large scale. There was a judgment made you could tolerate a local failure, because once you have a large part of that fuel molten, you have to dissipate -- allow the possibility of some local damage.

But let me leave it that I don't see within the Staff's thinking a deep enough look at -- is it really a threat to the main boundary of loss of coolable geometry if we have the local damage? This is what I'm saying. Maybe you could have made a case, but I haven't seen it in any of the reports, that was the case, and therefore this was really an important concern.

So my own question about why you're doing the RIA experiments was both from that point of view and the probability of argument.

MR. MAC DONALD: Could I clarify a point in the RIA 1-1 test where we subjected the rods to 285 calories per gram? We not only lost rod geometry, but we completely plugged the flow jet. We definitely lost coolability in the particular geometry, and that was a single rod test, inside of a flow channel, but the mechanism of that plugging, which was molten fuel swelling and then freezing, is a mechanism that could be of concern in the loss of coolable geometry question that has nothing to do with loss of rodlike geometry.

DR. OKRENT: Yes, but again, starting from -- especially starting from a low power level, you could plug a certain number of flow channels, I think, and not in any way have inhibited your ability to keep the core in an acceptable condition.

If that is not the case, there is a lot of problems.

MR. JOHNSTON: You're right, but we have always assumed that it was one assembly that was ejected, not all of them. So it's always been a localized occurrence from the very beginning.

Question A had to do, I think, with was there any possibility of it propagating to larger areas, or would it rupture the primary pressure boundary and bring the whole thing into trouble.

DR. SHEWMON: Can you get back onto your program?

DR. TOKAR: In response to Dr. Okrent's basic question as I understood it, I wasn't present at the time someone on the Staff decided that the rod ejection, rod drop area ought to be a design basis accident, and warranted a reg guide and these criteria and so on.

But to the best of my understanding, from discussions I have had with Ritchings, who I guess was involved in that activity at that time, the 280 calorie per gram, as I said, was intended to answer two concerns:

One, the threat to the pressure system boundary; second, the threat to coolable geometry, whatever you want to define coolable geometry as.

And for the first objective we feel, and have always felt that 280 calories per gram met that objective, and there was no concern about that whatsoever.

However, in terms of the coolable geometry issue, we felt there was a concern in the sense of 280 calorie per gram being able to assure that the rods would maintain what we call coolable geometry, which is rodlike geometry -- at least most of us think of it in that sense. If you want to think about it in terms of a small portion of the core versus a large portion of the core, that in fact had not been, I think, as you pointed out, well analyzed in the sense of whether or not you get a loss of rodlike geometry in one area, whether that would propagate to other areas, where the dispersal of molten fuel, if that were to occur, would cause some interaction, that would go through some kind of a domino effect throughout other parts of the core; or whether or not if you had a total loss of coolable geometry in one bundle and it fell down to the bottom of the pressure vessel, whether that would pose some further threat.

Agreed, none of that has been analyzed to the depth, perhaps, that it should have been. Maybe it needs to be, still. It may be an area that we should look at, specifically as part of our fuel failure propagation test at Los Alamos, and if you think that is worthwhile doing, we can advise those people that that is a particular concern of yours.

DR. OKRENT: No, no, please don't attribute any type of concern like that to me.

Why don't you go on.

(Slide.)

DR. TOKAR: This second failure damage threshold we were concerned about in the RIA case was the so-called incipient failure threshold. As I mentioned earlier, there are two criteria for that, the 170 calorie per gram for the BWR and the DNBR failure criteria.

Both of these, however, address only overheating effects such as oxidation and embrittlement, and not the pelletcladding interaction.

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However, as the SPERT test showed with limited data on two rods that were of burn-up, and as the more recent PBF test RIA 1-2 indicated also, PCI appears to the predominant failure mechanism for these kind of scenario event conditions, and that failure threshold also appears to decrease with increasing burn-up. At least the two rods that were tested at about 32,000 megawatt days per ton in SPERT failed -- one of them, I think, at about 140 calories per gram, and another, as I recollect, was estimated to be at about 85.

Therefore, the second part of our draft user's need asked for power ramp tests on relatively high burn-up rods. That is on the order of 30,000 megawatt days per ton, which we asked to be tested at this lower energy level of below 200 calories per gram.

(Slide.)

DR. SHEWMON: We have two criteria here, one the coolable geometry at which 280 has been used as an upper limit; and one incipient failure, for which 170 has been used?

DR. TOKAR: That's correct. 170 and DNB both.

DR. OKRENT: What kind of an event do you want to have on the 170 calories per gram?

DR. TOKAR: That is BWR rod drop at zero or low power, which is the worst-case BWR, rod drop case.

DR. OKRENT: Why only 170 for a rod drop?

DR. TOKAR: What the origins of that number are, are perhaps lost in obscurity.

MR. JOHNSTON: We had those calculations before we made the new ones.

MR. TOKAR: As I say, I think they were established as a result of an estimate that was based on the SPERT test data as being a number that looked pretty good.

DR. OKRENT: Protect against what, though?

DR. TOKAR: Against loss of integrity of the cladding or perforation of the cladding, if you want to call it that.

DR. OKRENT: For a rod drop accident?

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DR. TOKAR: Correct.

DR. OKRENT: Sounds curious, that you are worried about the cladding integrity in a rod drop accident. If that's really what you're worried about.

MR. MAC DONALD: Only as it contributes to fission product release at the site boundary.

DR. TOKAR: I don't understand the basis for your question.

DR. SHEWMON: You've got an improbable event leading to a far from dangerous accident, so why devote great effort to it?

DR. TOKAR: Wait a minute. We already said we couldn't answer the probability question.

DR. SHEWMON: How many years do you go without one occurring?

DR. TOKAR: At the time these criteria were developed, if they had been raised were perhaps not raised at high enough level.

DR. SHEWMON: Do you know how many times we have had RIAs in the last 100 or 500 or 1000 reactor years for commercial power in this country? Or the world?

DR. TOKAR: I think I know the answer.

DR. SHEWMON: I don't know the answer, but I suspect it is zero.

DR. TOKAR: That would be my guess.

DR. SHEWMON: That ought to tell you something about the probabilities, either by a rather unsophisticated argument, wouldn't it? One, even a metallurgist could do -- and I mean myself, not you.

(Laughter.)

DR. TOKAR: Touche.

DR. SHEWMON: Go ahead.

DR. TOKAR: Okay. Where we are at the present time should make you happy. We are agreeing with RES and acquiescing to your wishes, too, I guess, in saying that we should defer the remaining RIA tests, basically on the basis of the recent estimates by our physicists and the physicists at Brookhaven.

We would propose to use or develop new interim criteria which would, we think, be more appropriate, but not all that much different from present criteria for the coolability case, at least, and which the recent physics calculations also show will not be approached or exceeded, and we would therefore have these RIA dollars available for other needs.

With the deferral of the further RIA tests, the low energy, high burn-up fuel failure threshold could be pursued as part of the PCI program, and I hope to get into that, now if we can leave RIA behind us.

(Slide.)

The PCI tests, PBF-wise, are incorporated in the so-called OPTRAN test series. If any of you are able to take the time to read the PCI report I prepared for you last year, you will recall that what we said in that was that PCI has been generally recognized as a potential failure mechanism for several years, but that the industry's approach toward that was directed mainly toward normal operation concerns and development of design remedies, from that standpoint. Whereas our concern in Licensing was focused on a need to predict numbers of rods that would fail for these dose calculations that are needed for accident analysis.

And we also needed to assure, because of the requirements of general design criterion 10, that significant PCI failures would not occur during the so-called moderate frequency events.

As I said earlier, however, the current thermal hydraulic criteria for overheating type damage, that is oxidation embrittlement, don't address PCI.

(Slide.)

Some of the things that have occurred in the recent year or so, we have had developed at Battelle Northwest a model for PCI failure probability prediction called PROFIT. We have sent to RES a user's need about a year ago asking for in-pile ramp data that would address or encompass certain events or conditions which I will get into shortly, and which would therefore check the likelihood for PCI failure under this kind of fuel duty.

We sent at the tail end of last year a user's need to RES, asking for participation in the demo ramp 2 tests, which Bill mentioned earlier, which were intended to check the predicted failure threshold that resulted from the inter-ramp test series, and which would also provide a check on the so-called hold-time question that's associated with PCI stress corrosion cracking type failure.

In the spring of this year, we established a PCI research review group which had two basic objectives: One, which was to better coordinate the analytical and experimental efforts in this area; and second, to try to give us a better access to what we perceived to be a fairly large body of PCI-related type work in the light water reactor industry, domestic and abroad, which we perceive presently as only the tip of the iceberg, where most of the information is in effect not freely accessible to us.

The responsibility for the PCI analytical effort which we had been conducting for the past couple of years has been transferred also to RES and George Moreno will be in charge of that effort, and as I indicated, the operational transient, so-called OPTRAN test series, has been structured and underway now in PBF.

DR. SHEWMON: Before you leave that, is that PROFIT model work still going on at PNL, or is that terminated?

DR. TOKAR: It is not completely terminated. We have a small technical assistance program ongoing this fiscal year, which is due to end at the end of the fiscal year. It had two main objectives this year:

One was to get a better handle on this hold-time issue, because that seems to be a crucial one in terms of whether or not PGI can occur, and during these so-called short-term transients; and the other objective escapes me.

DR. SHEWMON: But what has been transferred to Research, is that primarily the PNL program?

DR. TOKAR: What has been transferred is the general concern or effort in the analytical effort area that may encompass further development and verification of the PROFIT model, as well as some other things or models that might be examined.

DR. SHEWMON: As I recall the PROFIT model was a modification of some correlation which the Canadians had for heavy water reactors, which you hoped would fit PWRs, and didn't too well.

DR. TOKAR: That's not exactly the case.

DR. SHEWMON: If you take a correlation and say your primary interest is what happens in off-normal situations, it seems to me to go from one reactor to another for correlation for normal service to off-normal, you sure have a bunch of mush to start jumping from.

DR. TOKAR: What we had is admittedly a relatively small data base with which to work, but the problem is you have to start with something, and what we were trying to do was crawl before we could walk, and we tried to develop an empirical model based on what were the best available data at the time.

The FUELOGRAM was based strictly on can-do data, not on light water reactor data, and there are approaches somewhat different from the PROFIT approach.

Phil Benkowsky, who was the basic or time developer of the PROFIT model, in addition to the fitting of an equation, using burn-up power and delta power to the existing empirical data, also introduced a concept that was somewhat novel in the sense of being a so-called strain energy failure concept, taking into account indirectly the effects of ramp rates. In other words, power rate increase, and the effect those increases would have on the resultant strain rate in the cladding and ability of the cladding therefore to accommodate the energy that was introduced to it before failure.

That particular parameter, I think, is one that needs further verification, and I would presume, and I expect that Research in their analytical and experimental efforts in the PCI area in the next couple of years will be doing work to obtain a better handle on whether that particular parameter is a good one.

DR. SHEWMON: Okay. Go on.

DR. OKRENT: Before you go on, do I understand what the licensing staff feels is their major need in the PCI area, what it is they are trying to achieve, and what capability they need to acquire?

DR. TOKAR: Yes, I hope to be able to respond to that.

As I said earlier, we know that fuel cladding fails by pellet cladding interaction, even under normal operation, and normal operation power ramps of various magnitudes.

The uncertainty or unknown that we have to try to address, it seems to me, and I think this goes back to a question you asked Bill Johnston about earlier, in terms of whether or what kinds of testing NRR needs -- or NRC needs to fund versus the industry, this is, I think, a good example of that need.

The industry's position is that PCI, as a normal operation, is used strictly and has nothing to do with transient concerns, and therefore its approach has been strictly in -- DOE's, to a large extent, I think, too, in its burial fuels program and other PCI remedy program is directed toward answering normal steady state operation kinds of concerns.

The problem is that for these accidents and transients, where we go or postulate various ramps and various magnitudes, we feel that there is enough evidence to believe that rods -- that cladding will fail under those kinds of conditions, and the basic problem is to try to determine for the dose calculation that goes into the area of accident evaluation work how many of those rods will fail, so that the dose consequences can be estimated.

And, as I said earlier, too, to try to obtain some assurance that for the more moderate event, that PCI failures will not occur.

DR. OKRENT: Which accidents did you have in mind when you said you need to be able to do something for accidents?

DR. TOKAR: The accidents are basically -- are, I should say, transients -- of two types, mainly, and this is why the OPTRAN test series has gotten scoped in the way it has.

In our user's need, we asked to have tests that would simulate a turbine trip bypass BWR.

DR. OKRENT: That's not an accident.

DR. TOKAR: That's a transient.

DR. OKRENT: But you used the term accident. Let's keep the two separate, accidents and transients.

DR. TOKAR: The only accident in the BWR case, if you want to call it an accident, and I guess we have to, is the infamous BWR rod drop that we have just been discussing, and as we said, we are obligated to try to come up with an estimate of what the dose consequence will be for that accident.

In the PWR case, and for the BWRs also, in terms of not accident, but transients now, there is the lesser rod movement problem, and that is that rod withdrawal or improper operation of the control rod. There is another area which is kind of a gray area, and you may want to question that, I suppose, and that is the anticipated transient without scram area.

For the BWR, the same -- the ATWS concerns are in terms of the event scenario quite similar to the turbine trip bypass with scram, the reason being that the type of event is essentially basically the same. You have a closure of a valve which cuts off the flow, and you get a pressure increase which collapses the voids, and you get this power spike associated with it, which we feel is a kind of event scenario which is likely to cause pellet cladding interaction kind of failures.

The PWRs are not that prone, I admit. In the PWR case, the worst PCI event that we can envision at the present time is a PWR rod withdrawal ATWS, because that particular event scenario calls for a relatively sustained power increase modest, it's true, on the order of maybe 10 to 12 or 15 percent in the worst case estimate. But that power is held, according to present event requirements on the order of 10 or 12 or 15 minutes and even if one were to claim that PCI failures were all due to stress corrosion cracking, that combination of event parameters would, we think, potentially lead to pellet cladding interaction kinds of failure.

DR. OKRENT: Let me try it this way:

It seems to me there might be a logic, although I haven't seen it written down, if it exists, that says if you have transients that occur with some frequency, and if the number of failures associated with these transients is a certain amount, and as a result of these, a certain amount of fission products get into the primary system, and this leads to some clean-up problem, and a site boundary level of low release; but nevertheless measurable; and that it exceeds what the NRC thinks is acceptable, you could say -- you can work back from this to maybe some kind of a limit on your frequency release sort of thing from fuel rods, and say, okay, now we want to know that for transients which occur once a year or once in 10 years, whatever it is, we don't get a leak to the primary system because it leads to some event which we consider to be unacceptable. But right now, at least, from what I have read, I cannot see what the threshold level is for concern, and why, with regard to releases and transients.

In other words, how much release has to occur with rare frequency before the NRC thinks this is sort of not what we consider to be acceptable?

I think you don't want to argue that there must never be a failure during transient, since we know we can get failures even without transients, or what you call maneuvering or so forth, in normal operation.

I don't see a logic at the moment for a staff position on what constitutes a larger-than-acceptable number of failures in a transient, and why.

And then I also don't see at all in what I have read how, in fact, the limited number of PBF experiments that you can do will in fact change whatever is your current uncertainty and knowledge in this area, enough to affect such a position, if you had one.

DR. TOKAR: Okay, let me try to -- do you want to try to answer the question?

MR. JOHNSTON: I might know the answer, because we have been kicking this around in the branch among ourselves. As it has been presented to me, we have a concept of ALARA to try to keep the numbers of releases as low as practicable.

There has been a definition, I believe, determined as to what that is. There are requirements on the hold-up systems and the reactors, the size of the filters and so forth, that can hold back a certain amount of activity, such that it won't be released to the public.

We have had instances like at Dresden, where there was a xenon transient which resulted in apparent PCI failures of a number of rods, in which the hold-up system was not able to retain all of the activity, and some of it was released, and I believe exceeded the release limits for a short period of time during that particular transient. That was a maneuvering transient.

The fuel branch then is trying to look into the position that we want to minimize the amount of activity that is put into the primary coolant system -- I'm sorry. We have to review the Chapter 15 transients, which is the kind he was talking about.

We have to look at it from the point of view of the Part 100, the ALARA, and the general concept of trying to minimize the amount of activity introduced in the primary system, lest it get to the outside or get to the people who have to maintain the system.

We are continually trying to lower the dose allowances to the operation people, so there is an incentive to try to reduce those numbers, and try to understand why you get them.

Our present criteria, DNB says that as long as you show that you do not exceed the DNB limit, everything is okay, and the intent for that was to keep fission activity from getting around. But we now have an apparent case in which you can satisfy the DNB limit and still have a way in which you can get potentially large amounts of fission product activity into the system.

So we are looking and saying why are we leaving on one limit, when the intent of it is being bypassed by these PCI type failures?

So we feel we have to -- that's why we have to look at -- we have a new mechanism, if you like, which is causing the Part 100 to be threatened.

DR. OKRENT: Part 100?

MR. JOHNSTON: That's the boundary. I'm sorry.

DR. TOKAR: Let me try to answer the question from another point of view. Bill was talking about ALARA, and ALARA may be a relevant argument, but let me try another one, and that is going back to the general design criteria in Appendix A, which I was not a party to formulating -- and maybe I shouldn't be trying to defend -- but personally my philosophy is at least attuned to some of it, and that is that in terms of potential by multiple fission product barriers, the cladding being in some people's mind at least the first fission product barrier, once you try to assure that that barrier is not breached, so the first step in the way of release of fission products to the environment is not exceeded.

Everything more or less stems from that philosophy, if you will. The consequence of SAFDLS and predicting acceptable design limits, not exceeding those limits. The whole approach toward trying to or being required to calculate what the dose consequences are for these various scenarios and the consequence or the question of probability associated with them falls into that.

Someone in their infinite wisdom -- not I -- decided that for the so-called more frequent events, you should not have any release to the environment at all.

If you follow that -- if you accept that concept, then I think it seems logical that one way of ensuring that is to ensure the integrity of the first fission product barrier, the fuel rod cladding, and that is one reason why we are concerned about the pellet cladding interaction issue, and that is that we see a potential for having the cladding breached in these kinds of event scenarios that involve a power ramp of a certain magnitude.

DR. OKRENT: Again I earlier said I think there is a reason for the NRC Staff to be concerned about the rates at which fuel failed, both in which you would call normal operation and in transients. But I don't think that the NRC has adopted the position there should never be a fuel failure in a frequent transient, because they couldn't, in fact, enforce it. And also they would have trouble arguing why it is logical that this must be never.

So what I am suggesting is that if you don't have one -- and at least I haven't seen it -- there would be merit in developing some kind of -- you might call it philosophical position as to what constitutes --

DR. TOKAR: You know, I'm very happy to hear you say those words, because I just left Bethesda this week, in developing just that kind of internal memorandum, which I hope will eventually get its way through the concurrence chain which addresses just that question.

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DR. SHEWMON: Why don't we leave it at that and get on to OPTRANs, then. We are glad to hear it.

(Slide.)

DR. TOKAR: Okay. The OPTRAN test series, is, as we see it, a test program which will address the issue of what the fuel behavior will be and what we think of as the most severe fuel duty types of events that we need to be concerned about in the PWR and BWR case, and in particular emphasizing PCI.

Bill MacDonald, who will follow me, I guess, will go into the fine details of that program. All I want to talk to at present is the last two tests which have been added to that program, as a direct result of our user's need of last August, and these two tests are the OPTRAN 1-6 and the OPTRAN 1-7 test.

The 1-6 test, as I understand it, will be a turbine trip without bypass simulation, using 3 x 3 hardware, and nine high burn-up rods.

The OPTRAN 1-7 would be a turbine trip with bypass simulating, using high burn-up rods with the same rod configuration.

I think, as I understand it, -- I don't know whether Phil has left the room or if he's still around -- but as I understand it, the parameters of these tests are still being developed.

I had one question about them that I think ought to be considered further, and that is should these be in the boiling transition regime.

It's my understanding from discussions I've had with Phil and with Pic that the latest projection was for them not to be in film boiling or in boiling transition, and that this resulted because of discussions they had with General Electric over a period of several months, in which General Electric convinced them that boiling transition would not be encountered in such events.

The user's need that we developed a year ago, at the time we developed that user's need, did have the intent of having these tests run in boiling transition, the reason being that GE had provided us prior to that a so-called bounding estimate for turbine trip without bypass, where they indicated they would go into boiling transition. It would be in that regime for on the order of five minutes before rewetting, and would encounter peak clad temperatures on the order of 12 to 1400 degrees F.

The ATWS scenario that I mentioned earlier, the turbine trip without bypass, closure times of ATWSs, the latest information we have from GE indicated that they would reach temperatures on the order of 1700, 1600 to 1700 degrees Fahrenheit in those events.

So I guess what I am questioning here is whether or not those tests ought to still be run in boiling transition, or at least be considered to be run under those conditions.

I am not advocating that they necessarily need to be, because I think that in terms of pellet cladding interaction failure, it may well be that if you don't go into boiling transition, and you don't heat up the cladding, you have a less ductile material there, which maybe cannot accommodate pellet cladding interaction stresses, as well as if you were in a higher temperature area, where the material were more ductile.

All I am saying right now is that those issues need to be thought out before the test conditions are set in concrete, and it is my understanding that they are not set in concrete yet.

DR. OKRENT: Before you take that away, let's focus on just one of those. Let's say the OPTRAN 1-7, for example. What is it that the licensing staff thinks is important for them to learn, essential for them to learn, whatever is a fairly strong adjective in this regard?

DR. TOKAR: I would rather answer that in regard to 1-6, because I have a better feeling about that, if you don't mind.

DR. OKRENT: Well, I see both of them there, but let's try 1-6. Go ahead.

DR. TOKAR: 1-6 is one that we had called for in our user's need as of a year ago. What we had hoped to look at were two things:

One was the effect of burn-up, and the propensity for PCI failure. Remember that most of the tests that have been conducted so far have been on relatively low burn-up rods. The SPERT series was mainly low burn-up, except for two rods, with about 32,000 megawatt days. Those rods failed at low energies, and due to PCI. So we were trying to get a better handle on whether or not PCI failures would in fact occur.

Under turbine trip without bypass, again let me go back to the current licensing requirements. Turbine trip without bypass, according to the current Staff requirement, should not result in fuel failure. It's not supposed to have a large enough change in critical power ratio, for example, to fail fuel by that current criteria.

Hence, if there are fuel failures occurring due to PCI, that kind of event scenario is going to cause a problem. What or how we could accommodate that problem you alluded to, of course, a minute ago. I'm not certain I can answer that at the present time. We may have to go back and change our requirements. We may have to require the industry to develop a more PCI-resistant fuel design.

There may be some way we can reinterpret general design criterion 10 to say that with half a percent fuel failure or a tenth of a percent fuel failure, you'd still satisfy SAFDL. You don't exceed the so-called specified fuel design limit, because we now define that design limit to be

X percent fuel failure.

There are a number of ways to go about that. I'm not trying to answer that now.

DR. OKRENT: Well, in fact, you are getting at some of the kinds of questions I have in mind, but there's another one now.

Let's say that you run this experiment and you do get failures. Will it generally be agreed that these are sufficiently representative of the rods in BWRs here that in fact you can translate directly?

And the other part of the question is if you don't happen to get failures in this experiment, do you have any basis for assuming that under another set of irradiation conditions, another initial condition for when this occurs, that you would not get failures?

It is just one possible initial state of these fuel rods out of a myriad of possible conditions of fuel rods.

DR. TOKAR: Let me answer one question at a time, if I may.

The first question was would the fuel rods in test conditions be representative of BWR current design or future design, or whatever, rods? And so would we be able to be comfortable with those results?

It is my understanding-- and again I have to defer to Phil MacDonald's presentation coming up shortly, in terms of the details of the test -- that these rods are going to be supplied by General Electric, and would be representative, therefore, of their rod design, and would have, as I say, sufficiently high burn-up to give us a warm feeling regarding the burn-up question.

So for me to determine beforehand -- I mean right now, this second -- as to whether or not we would be content or completely satisfied with those test parameters or whatever, I am not prepared to do that, because I do not think those tests have been, as I say, fully defined.

DR. OKRENT: Well, let me say I can see that as being very difficult, if not impossible, to decide if you get a negative result, no failures, that you should expect no failures under any other possible situation in reactors.

DR. TOKAR: That was your second question. I had got to that.

Again, I say the turbine trip without bypass BWR event is currently the one which has the largest increase in power or change in critical power ratio, if you will, that is currently postulated, currently modeled, analyzed, by the vendor, as required of him.

And hence, we thought that simulating those kinds of test conditions would hopefully bound anything we currently can conceive of as worst case.

DR. OKRENT: I don't think I made my point clear.

DR. SHEWMON: Let me give a different answer to your question. One PCI comes normally on the scale of the length of the pellet, sc it is whether the pellet breaks up and tends to get out against the cladding. So it is not at all clear it would differ between a three-foot long rod and a 12-foot long rod, and since, I think, it is generally agreed that all clad or all pellets do tend to break up, I am sure EG&G can get themselves a pre-radiation or pre-test set that they will be pretty sure they will be broken up and be in intimate physical contact with the cladding, that I think you can probably make a fair case, that if this withstands it, it would give you a better feel than commercial reactors. And if it doesn't, that indeed you ought to look harder at it.

But the main argument is that PCI comes from the p ellet breaking up and getting in intimate contact with the cladding, so that if you go through an excursion, it now deforms the cladding, and since we are talking about things that happen on a scale on all fuel on the order of an inch, I would think this should be fairly representative.

DR. OKRENT: You may be right, but I have to assume that there are different cladding conditions over 10,000 fuel rods, whatever it is, in an actual reactor, and you have cracks partly through, and a little bit through, and a lot through.

DR. SHEWMON: Over three feet integrates over, you know, a lot of --

DR. OKRENT: Well, in other words, if the Staff is really concerned about no failures in this situation -- and I am not in any sense urging that they should be -- I am saying they should develop some philosophical basis, and I don't think any single test can tell them that there will be no failures. So that was the part two.

DR. SHEWMON: We'll be discussing this later. Why don't you leave this, and actually the last thing in your handout looks enough like Bill's that I would be interested in skipping that, unless there is a particular point you want to make from it.

DR. TOKAR: That's fine with it.

(Slide.)

The only part I think I will talk to, then, is in terms of the extended burn-up test, in the sense that I think one of the things that has to be -- one has to be concerned about is getting specimens, if you will, burn-up rods with sufficient burn-up to be able to address those concerns, and as Pic mentioned earlier, there is a long lead time problem with setting up these tests and so forth. So that one needs to start worrying about that kind of thing now.

Maybe Ralph has talked to you about this earlier, but one of the things we have been kicking around internally, which I think may help in that area, is that we think that it would be a reasonably good idea to have the vendors or the utility or whatever, with each reload put in a certain number, small number of segmented rods, so that over a given period of time, you build up a given inventory of rods or rodlets, if you will, that could be available for doing tests in various kinds of test reactors, and we would like to hear your comments or your opinion in regard to that.

DR. SHEWMON: A segmented rod has a plug or just unenriched elements every three feet?

DR. TOKAR: No, it is disassembable in various lengths along the way.

DR. SHEWMON: So it would have a genuine plug where you just cut through and reweld it?

DR. TOKAR: Right. So you would not lose or change the internal rod chemistry and things of that sort. GE has done this, and does it as a matter of course, as I understand it, for tests that they perform in GETR, for example.

DR. SHEWMON: There may be a surplus of those elements available developing over the parent plan with GETR.

DR. TOKAR: There may be, yes.

MR. JOHNSTON: That's the source of the rods for the OPTRAN test.

DR. SHEWMON: It's a good point. Bring it up before we quit today.

Mr. MacDonald, I have let this run on, maybe because

I couldn't control it, maybe because I didn't want to. I think the main thing we are interested in today is not so much what you're going to be doing, but why, and can you indeed do tests which will speak to the why.

I would hope that in your presentation you would address that and I would also hope that maybe you could cut out some of the detail and help get us back on schedule somewhat.

MR. MAC DONALD: Yes, sir.

DR. SHEWMON: Okay. We'll take our 10-minute break, then.

(Recess.)

T.4

DR. SHEWMON: Why don't you proceed?

MR. MAC DONALD: Okay.

(Slide.)

The title of my talk is the safety and licensing issues that are being addressed by the PBF program. I will go through the highlights of our results, and our plans, and I will be happy to discuss details and differences of opinion on our conclusions.

The results I will be talking about are primarily EG&G Idaho viewpoint, for the most part. They are quite in concert with the NRC Staff, but in a few cases we do have minor differences of opinion.

I think as you appreciate fuel behavior research is important because cladding is the first line of defense. We are interested in this program primarily in defining failure thresholds, damage to fuel during severe accidents, loss of coolability, and other issues, that are prime safety concerns.

We are not interested in this program in simply running tests to get general technical information or in running tests to look at the reliability of fuel during normal operation.

We feel that is the work that is being done, and should be done, by the vendors.

(Slide.)

In our view, the PBF baseline program, the original 40-test program, is basically coming to a conclusion. We have two more LOCA tests. At that point we will then have completed 38 tests in the program, and that original program that was defined some years ago by Dr. Johnston and his staff will be complete. That will be in the spring of 1981.

We will then start a new program responding to new needs as defined by Regulatory, and those new needs primarily center around the area of anticipated transients with and without scram, and severe fuel damage. That is accident scenarios that have been brought to the public attention because of the TMI-2 accident, and also because of the Browns Ferry incident with regard to the scram, or lack of scram.

In the original baseline program, I want to talk to you about the highlights of the results we have gotten, and I want to talk primarily to the areas of power cooling mismatch, reactivity initiated accident, and loss-of-coolant accident, because those are the three key areas that we spent most of the effort and got most of the results that have direct application to licensing and safety issues.

We have, as you know, also one LOFT lead rod test and thermocouples test in support of the LOFT program which have been very useful to LOFT, and we have run gap conductance and thermal performance tests to look at initial stored energy.

I want to be sure when I talk about power cooling mismatch, that you understand that we ran a number of tests there, 16 tests, with both fresh and irradiated rods, and we were interested not only in the behavior of the core due to slight overpower or undercooling conditions during normal operation, which are what we called our mild PCM test, but we also ran tests where we subjected the rods to very severe conditions, looking at category 3 and category 4 accident conditions that were PCM-related.

And so you will see, as I talk about those areas, that there is different kinds of information, and I am not sure that everyone has appreciated that we have run some very severe tests in the PCM program, and we have run some fairly mild tests.

However, I do agree with the comment made earlier that those tests primarily apply to the pressurized water reactor and I would be reluctant to extend our conclusions from those tests to boiling water flow blockage, for instance.

(Slide.)

Okay. Moving on now to talk about PCM, just very briefly. As you know, the present NRC licensing criteria requires that a calculated DNB hour must exhibit a 95 percent probability and 95 percent confidence level --

DR. SHEWMON: Why don't you skip on over these? I think we are familiar with these.

MR. MAC DONALD: I'll go fast, if you'll let me, but I want to make just a couple of points, that no fuel rod will depart from nucleate boiling.

The criteria means that all fuel rods must be operated at DNBR somewhere between 1.13 and 1.32, and what that means is that basically during normal operation and during anticipated transients, you've got somewhere between 13 and 32 percent margin before you start into boiling, even in category 2 events that last only a few seconds. That's a lot of plant margin. In my mind, that's a lot of wasted energy in this country.

And as I say, and the last thing on the slide is that mechanistic damage mechanisms are not presently accepted.

(Slide.)

DR. OKRENT: If I could make one comment in regard to your last statement. Let me assume there may be wasted energy, to use your term. There is a question as to whether it is for the industry to show that --

MR. MAC DONALD: I agree with that. I think we have done in the PCM area, I think we have done the safety side of the job, and industry should show that that margin could be utilized.

DR. OKRENT: No, the question is, should industry have shown that in fact they could make a case for less conservative limits than the NRC decided was okay, based on the information previously available?

MR. MAC DONALD: The way I would put it is I believe that industry -- there is margin there that industry could obtain if they would perform the appropriate research to well define that margin.

DR. SHEWMON: Let's go on.

MR. MAC DONALD: Okay. The questions that we addressed in the PCM program at PBF are listed in this slide in terms of questions.

What is the margin between departure from nucleate boiling and actual fuel rod failure during normal operation, when you get mild overpowers or undercooling situations? Can a coolable geometry be maintained during a severe category 3 or 4 power cooling mismatch accident?

What is the propensity for departure from nucleate boiling and fuel failure propagation from higher powered regions of the core out through the entire core?

And finally, will energetic molten fuel coolant interactions occur during any PCM, particularly high powered PCMs, where there are significant amounts of molten fuel available.

(Slide.)

We have concluded from these tests, as Dr. Picklesimer mentioned earlier, that light water reactor fuel rods can depart from nucleate boiling for significant times and withstand severe damage prior to rod failure.

And, in fact, on the average you are talking about 10 minutes before a rod will fail after it departs from nucleate boiling, give or take a little bit on temperature or power.

That means that the DNBR criteria which now applies to both normal operation and expected transients that last only a few seconds, is very conservative. We have observed the cladding deformation that occurs above 920 K, but the cladding retains a significant ductility to accommodate collapse strains and preclude immediate failure.

Therefore, the primary fuel mechanism is due to oxygen embrittlement of the cladding from the water, the reaction of the zircaloy with the water on the outside, and the UO_2 on the inside.

(Slide.)

This zircaloy oxygen embrittlement is predictable, using temperature-time correlations developed from out-of-pile data at Oak Ridge.

We have also observed in the more severe PCM tests that energetic molten fuel coolant interactions do not occur under such events, and in fact we have taken a rod and subjected it to five times the nominal BWR power levels, with 80 percent of the radius of its fuel molten.

We have held that rod in film boiling till well after it fails, something like eight minutes after it failed. We have seen absolutely no energetic molten fuel interactions during these type of events.

DR. SHEWMON: What do you mean, after it failed?

MR. MAC DONALD: Well, the rod failed, it separated, and we continued to operate it in film boiling with 80 percent of the pellet radius molten, and there were no energetic --

DR. SHEWMON: What failed? Obviously it didn't distribute itself through the coolant.

MR. MAC DONALD: That's correct, but the rod separated, fractured, broke in half, released fission products.

DR. OKRENT: Was that highly irradiated?

MR. MAC DONALD: The lower portion of the rod dropped d own.

DR. OKRENT: Was that a highly irradiated rod?

MR. MAC DONALD: It was a fresh rod with some preconditioning. I would expect some fuel swelling in a highly irradiated rod under such conditions.

DR. OKRENT: Do you know how much fuel swelling you would get with 80 percent of the fuel molten and the cladding in a completely weakened condition?

MR. MAC DONALD: We have run tests with previously irradiated rods where we had something like 40 to 60 percent of the radius molten. These are the IE tests with rods at about 16 megawatt days per metric ton burn-up. We did not fail the rod during operation, however. But, nevertheless, the rod itself, the cladding and the fuel swelled up to about 4 percent in those particular cases.

DR. SHEWMON: What you are suggesting is the clad was molten or removed or ruptured?

MR. MAC DONALD: In the case of the rod out of the molten percent radius, the clad was.

DR. SHEWMON: The clad was what?

MR. MAC DONALD: The clad was removed at the end of the test. We believe parts of the clad were molten during the test from the structure.

DR. SHEWMON: You still haven't told me what failed means. You mean the cladding no longer maintained its --

MR. MAC DONALD: The rod fractured in half.

DR. SHEWMON: Okay.

MR. MAC DONALD: The rod broke in half, and the lower portion of the rod moved downward in the shroud during the test while the rod was in film boiling.

DR. SHEWMON: So your main point is that even though part of the fuel was molten, and even though it ruptured, broke in half, that the fuel solidified and in effect provided a barrier between molten fuel and coolant throughout this; correct?

MR. MAC DONALD: Yes. I suspect the molten fuel continued to be vapor-blanketed. There was no triggering mechanism to break down that vapor blanket, and there were no coolant interactions.

So based on those kind of test results, we do not expect vapor explosions during a power cooling mismatch accident, even though it's a very severe accident. Okay. Also --

DR. OKRENT: Excuse me. I don't expect vapor explosions, but I don't think your tests show it at all, and you are drawing the conclusion, if you suggested it, that your experiment, where you melted somewhat less in the core in an irradiated pin where the cladding didn't get quite as hot, that this somehow bore a relationship to the situation. It seems to me to be tenuous. If you wanted --

MR. MAC DONALD: I am saying, Dr. Okrent, that under a severe power cooling mismatch accident situation, where you have severe overpower of the rod, you have very high power, and therefore a lot of molten fuel within the rod, if that rod breaks apart, water has access to that molten fuel, you will not get a vapor explosion under those conditions.

Now there are other conditions under which you might get vapor explosions. We have created them in a PBF test, but under these type of conditions, this type of accident scenario, based on these results, we do not expect a vapor explosion.

DR. OKRENT: Do we know why PRTR had a problem in one of their molten fuel tests, and they got some fairly high pressures, I think?

MR. MAC DONALD: In the PRTR, they built up pressure within a rod that had molten fuel. They squirted it sideways through their flow tube, if that's the case you're thinking of. We have observed molten fuel measurement within our rods, but when we are in what I call nominal film boiling conditions, that is cladding temperatures below about 1600 K, we have now that molten fuel cladding contact does not result in cladding failure, it results in freezing of the molten fuel on the surface of the cladding.

However, one can go to a more severe core situation and one can penetrate the cladding with molten fuel, and we have developed criteria for such. But for again the category 3 and 4 accidents, that create power cooling mismatch type of situations, as my last point on this slide is, that we also do not expect molten fuel movement through the cladding. That we expect freezing of the molten fuel on the inside of the cladding, and we have observed such. We have a model that describes such.

(Slide.)

I do have, by the way, slides in the carousel that go through much more detail, if anyone would like to, but I'm trying primarily to hit the highlights for you.

DR. SHEWMON: Okay.

MR. MAC DONALD: Okay. My final slide on the PCM area, I would like to mention that we have observed significant grain separation, which we call powdering or desintering, that occurs when the fuel is quenched from temperatures above 1900 K, and occurs in both irradiated and fresh rods.

We believe it does have significance because it contributes to additional fission product release. Also the powder tends to move out through the system. It moves out through the screens that we build to try to contain the fragments. It moves around our loop and I would expect, for instance, in the Three Mile Island core. that there will be significant quantities of powdwered fuel out in the loop areas deposited in various places, based on these test results.

We have also observed in our more mild tests, nine rod cluster tests, where no rod-to-rod DNB or fuel failure propagation, and therefore we don't expect such in a power cooling mismatch accident. That is propagation from a higher power rod to a lowe. power rod.

We have also not observed any loss of coolable geometry. Now we have observed loss of rodlike geometry during a severe PCM test, but we have not observed a flow blockage, a significant flow blockage in any of the PCM tests.

(Slide.)

Moving on to the RIA area -- I will pass through this fairly rapidly, because it was covered this morning. As you know, we are interested in fuel failure thresholds, because that contributes to dose calculations. We are interested in loss of coolable geometry, we are interested in overstress of the pressure vessel primarily because of the SL-1 accident.

(Slide.)

Again, just briefly, the applicable NRC licensing criteria for rod failure in dose calculations. The releases must be within 10 CFR 100, and in calculating those releases one must assume that any rod that is subjected to 170 calories per gram in a BWR fails. Any rod that departs from nucleate boiling in a PWR fails.

In addition, we have a criteria for loss of coolable geometry that says that the radial average peak fuel enthalpy must be below 280 calories per gram, and any rod drop or rod ejection accident. That is one must design the control system so that one can never exceed those absolute limits.

The criteria for 170 and DNB are simply directions on on how to do the dose calculations, and as we mentioned earlier, these values are based on very early SPERT results.

(Slide.)

The PBF results have shown that the mode and the consequences of fuel rod failure are significantly affected by prior irradiation. We have observed that the irradiated rods fail due to pellet cladding mechanical interaction at relatively low energy depositions, well below the 170 calories per gram c riteria.

We have also observed that those failures occur before the rods go into nucleate boiling, and therefore a nucleate boiling criteria has little to do with the failure and, in fact, if you go into nucleate boiling and heat up the cladding, you tend to minimize that kind of PCI failure.

We have also observed that the expansion of gaseous and volatile fission products at higher energies induces extensive swelling of molten irradiated fuel, and that is molten fuel, swelling fuel, will move out of the clad, rupture the cladding, move out onto the flow channel, freeze in the flow channel, and partially or totally blow the flow channel, depending on the geometry configurations.

I might remind you that the UO_2 melts at an energy insertion of 267 calories per gram, and the criteria is 280 calories per gram. So it is a pretty soft fuel thus being driven by the expansion of the volatile and gaseous fission products in it.

(Slide.)

We have concluded here that the 280 calorie per gram and the 170 calorie per gram criterion may be nonconservative because of these results.

DR. SHEWMON: Nonconservative for what?

MR. MAC DONALD: For design of reactor -- for criteria for designing reactors.

We have also concluded that the film boiling criteria which applies to rod failure in a PWR is probably inappropriate, simply because rods experience pellet cladding interaction failure early in the transient before they ever depart from nucleate boiling.

DR. OKRENT: I'm sorry. First tell me what you think the 170 calorie per gram limit is, and how it is used, and why it is nonconservative.

MR. MAC DONALD: A 170 calorie per gram number is part of a dose release criterion where the absolute doses are set out at 10 CFR 100, and your regulations set out directions for calculation those doses. And as part of those directions, you instruct the vendor to assume that any rods that experience 170 calories per gram or greater fail.

What I am saying to you simply is rods fail at lower energies than that, and that number is a poor choice. And that is for BWRs.

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For PWRs, you tell the vendor that he must assume that any rod that departs from nucleate boiling fails. And I'm telling you nucleate boiling has nothing to do with whether or not the rod fails or not. And again that is a poor choice for a regulation.

However, as my last line states, and as Mike discussed earlier, light water reactors are safe because their control systems are designed such that a rod drop, rod ejection accident will insert far less energy than the present NRC criteria.

And as you pointed out, it is a very rare or unlikely event.

So I don't think we have any concern about the safety of the present generation of light water reactors. My concern is that we have made a poor choice in our regulations, and that we should change those regulations, and we have made such a recommendation to Regulatory. That is Ralph Meyer, Mike Tokar, Bill Johnston. And it is my understanding that NRR is in the process of changing these criteria and, in fact, has a draft criteria out for review, at least by some people, and the draft criteria looks quite adequate to us.

(Slide.)

Moving on to the loss-of-coolant accident area, which is the last area --

DR. OKRENT: Excuse me. If the NRR has some criteria in this area that are in draft and they are putting out for review outside of NRR, I would like to request that they send it to the ACRS, so the ACRS can see what it is they are proposing to do.

MR. JOHNSTON: We will certainly be happy to do that, if that hasn't already been done. That was last winter.

DR. OKRENT: If it already exists, then I will ask my engineer to direct me to the document.

MR. JOHNSTON: We will be happy to do that.

DR. SOLOMON: Could I ask a question here just for clarification on one point? You say there are situations in which DNB is overconservative, and rods can operate a long time without failure, and in another place you say it is non-conservative.

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MR. MAC DONALD: Yes, I say it is inappropriate as a criteria, but it is also nonconservative, yes.

DR. SOLOMON: You're saying it's a whole range?

MR. MAC DONALD: I'm saying for different kinds of transients.

MR. JOHNSTON: For a PCI, it's nonconservative. If you're going to get a failure with a PCI, it will occur long before you get to DNB. Therefore, it is inappropriate for the high burn-up rod, in that kind of situation.

On the other hand, when you use a DNBR criteria as determining power levels to preclude going into DNB, it's extremely conservative, where your failure is not due to PCI.

MR. MAC DONALD: In the AIA, it's a very, very fast transient. You interject the energy, the fuel heats up very fast, well before the cladding heats up. It expands out, it contacts the cladding, it takes the cladding to very high stress levels, with a very, very rapid strain rate. And you basically tear that cladding before that cladding gets heated up, and that's the initial failure threshold mechanism.

In the PCM, where things are moving much more slowly, you're heating up the cladding and eventually oxidizing it, embrittling it, and then it fractures.

Okay. Moving on to the --

DR. SHEWMON: If it occurs that fast, I guess I would then be on guard to say -- to have you call it pellet cladding interaction, as you do on a previous slide, because pellet cladding interaction, for example, is not the sort of thing that would happen at mild overpowers, as GE has had trouble with, for example, when they exercised their fuel rods, at least their two kinds of PCI.

MR. MAC DONALD: Yes. We are talking about a different mechanism than the mechanism that is being discussed in the industry for the low transients. I guess at this point we don't believe that fission products have played any role, but the strain rate is key, or radiation damage in the cladding is important in this damage mechanism.

Okay. Moving on to the LOCA test series. As you know, in PBF, we are primarily concerned with fuel behavior during the LOCA accident, not the entire system behavior, and therefore these safety issues that we are interested in addressing are listed here, and they are simply will cladding ballooning during a loss-of-coolant accident lead to co-planar blockage and subsequent loss of coolable geometry?

And as a corollary to that, is the extensive out-of-pile ballooning data that has been generated through the NRC-funded programs using simulators representative of nuclear fuel road behavior.

We have five tests in the LOCA program, as I mentioned earlier. Two remain to be completed. One is on the way at the moment, one will be completed late this fall, and that will then complete the original 40-test program.

The three tests that have been completed to date are the LOC-11 test, which was run at temperatures where we obtained incipient deformation; the LOC-3 test, which was run at temperatures in the alpha plus beta phase transition region, and the LOC-5 test, which was run at low beta temperatures.

You note that all these tests are being conducted at temperature below the NRC licensing limit for the LOCA, and we are primarily interested in the ballooning as it will pertain at temperatures from the point where you begin to get incipient ballooning on up to the NRC licensing limit, and we are interested in whether this ballooning could result in co-planar blockage.

DR. SHEWMON: Were these single element, or cluster?

MR. MAC DONALD: These are single element tests conducted four at a time. Each was in their own shroud. The shroud is fluted to look like -- from a mechanical point of view, it looks like adjacent rods.

And we are choosing, of course, at varying temperatures to run these. Because of the fact that zircaloy changes crystal structure within this region, and therefore its properties change radically as a function of absolute temperature.

I think most of you know that, and I don't need to go into that.

(Slide.)

Okay. The results from the PBF LOCA program today indicate that the deformation of irradiated and fresh nuclear rods is in reasonable agreement with the previously published from the single rod out-of-pile GETR simulator test, primarily the Oak Ridge work.

This means that in our mind, co-planar blockage is unlikely, and that the large NRC out-of-pile base is probably reasonably valid and appropriate for making decisions in the area of co-planar blockage.

We have, however, observed that previously irradiated rods exhibit greater deformation than unirradiated rods.

This is not an effect of irradiation damage in the cladding, but it is due to the fact that previously irradiated rods have slightly different geometries, and therefore exposed to nominally identical thermal hydraulic depressurization conditions, heat up in a little bit different manner, having in fact -- previously irradiated rods have a little bit different heating rate and they have a more uniform temperature distribution along the circumference of the cladding.

In our particular case, both effects tend to result in slightly greater deformation for the irradiated rods than for the fresh rods.

However, in both cases, the results are in general agreement with the data from the -- out-of-pile data work from Oak Ridge. That is our irradiated rods are toward the top of the data scatter, and our fresh rods are toward the bottom of the data scatter.

And as I say here on the slide, we believe that cladding creepdown increases the deformation during the subsequent LOCA, primarily because it results in both the faster heating rate and the more uniform circumferential temperature distribution.

I have also observed fuel fragmentation and relocation --

(Slide.)

-- of these fragmented fuel particles and the ballooning region, which will affect a post-ballooning or reflood behavior slightly, and we have observed smaller fuel particles were produced in the irradiated rods than in the fresh rods.

We have also observed that variation of the initial internal pressure from values representative of beginning of life in a PWR to end of life in a pressurized water reactor had no significant effect on the cladding deformation, in these tests, primarily because heating and circumferential temperature distribution affects appear to be much more important.

DR. SHEWMON: Are you telling me that if you double the pressure, it certainly must expand or fracture, but it doesn't change--

MR. MAC DONALD: Not significantly.

MR. JOHNSTON: Dependent upon what alpha-beta region of the cladding it fails in. If you change the pressure such that you get it to fail in the high alpha instead of the alpha-beta, you certainly get a different answer.

MR. MAC DONALD: That's true, and let me say --

MR. JOHNSTON: It's an oversimplification.

MR. MAC DONALD: Remember that we are running these tests with a temperature history that is predetermined and looks like what you see here. In each of these tests we have two irradiated and two fresh rods, two high pressure and two low pressune rods. They are all seeing basically thermal hydraulic conditions that take them into the high alpha, the alpha plus beta over low beta.

(Slide.)

DR. OKRENT: Does there exist information on the probability per centimeter length of rod, that after it has been irradiated to say 20 to 30,000 megawatt days per ton, that you will find a significant flow in the surface?

(Slide.)

In other words, do we expect to find a flaw one in one hundred times, one in a million times? Do you happen to know that?

MR. MAC DONALD: Let me answer that question by showing you one or two slides that deal with --

(Slide.)

Of these walls that we tested to date, in each case we have an axial power distribution that is flat, and in six and seven rods they deformed basically over this flat axial power distribution.

In other words, there was -- the failure location is shown by the triangle, the length of the ballooning region is shown by the line. You note that in one case, there was a significant difference in the extent of ballooning. In other words, it failed -- it ballooned and failed at a local point; whereas all the rest of these rods are ballooning over about a foot and a half or so.

The reason for that is that on that particular rod, there were fragmentation of the pellets and there was some movement of these fragments, leaving axial gaps, changing the stored energy in here and resulting -- this is a neutron graph of that rod -- and resulting in, therefore, the ballooning occurring at that very local position.

Okay. So that's the only defect, if you like, anomalous behavior that we have seen in the test, and we believe that movement of this stack occurred in part during insertion of these rods into the PBF.

DR. SHEWMON: How long does this test -- I guess I can look that back up.

MR. MAC DONALD: These are about 25 second depressurizations, equivalent to a severe double-ended break.

DR. SHEWMON: Okay. Thank you.

DR. SOLOMON: When you are talking about the deformation, in answer to Paul Shewmon's question, is the deformation the failure of the total deformation of the ballooned region?

MR. MAC DONALD: You are asking how long, over what length of the rod do the rods balloon?

DR. SOLOMON: No. Paul Shewmon asked the question, did the pressure have no effect on the strain to failure? And you said that is correct, that the ballooning occurred over some length, and so you have strained failure --

(Slide.)

MR. MAC DONALD: Here's an example of high pressure and low pressure on a fresh rod. One failed here, and one failed here. And as you see, they ballooned over basically this region in here. This is the diameter, the diametral strain plotted versus the length along the rod.

DR. SOLOMON: So you are talking about the strain to failure, actually?

MR. MAC DONALD: Well, as you see, the red and the black shape, the black being the low pressure rod, and the red being the high pressure rod, are not significantly different.

DR. SHEWMON: What is going to be the thing here is what happens in large assemblies, and how do you get variability; so it's interesting, but not too relevant.

(Slide.)

MR. MAC DONALD: This is a comparison of a previously irradiated rod and a fresh rod at the same pressure, and you note that there the shapes are, in my opinion, significantly different.

(Slide.)

And if you look at the cross sections of those rods, what you will see is in the unirradiated rod, most of the deformation is occurring over in here, the balloon region, the irradiated rod has a more uniform deformation of the circumference, of the total circumference of the cladding, and we believe -- I believe that that is primarily due to variations in circumferential temperature distribution, because in the irradiated rod, after a period of time, pellet cracking, pellet movement, bonding of the pellets to the cladding results in a fairly uniform temperature distribution around that cladding.

DR. SHEWMON: Let's go on. Thank you.

(Slide.)

MR. MAC DONALD: Okay. So I think that basically covers the LOCA Program, and that completes the original test program as we see it in the PBF.

Starting now to the new program, starting in early '81 with the first test being an OPTRAN test, the OPTRAN program has had a lot of very good input from Mike Tokar.

Basically as we see the OPTRAN program and the safety issues that ought to be addressed, they are listed here and they are simply should a reactor be derated following severe operational transient?

Should regulations be imposed to limit pellet cladding interaction in highly irradiated rods?

Should reactors be modified to reduce the probability of a severe anticipated transient without scram?

DR. OKRENT: Before you go, could you tell me how that third one relates to the PBF program, from your point of view?

MR. MAC DONALD: We intend to run tests that are representative of both the anticipated transients with and without scram. I have a lot of question in my mind regarding whether there is going to be rod failures during an ATWS of the type -- you know, turbine trip without bypass without scram, main line isolation valve closure without scram, those types of ATWS events.

I personally am wondering whether or not we, the safety community, are not asking for expensive modifications to the plants to reduce the probability of ATWSs, when we may not have really severe fuel damage during such events.

We don't know at this point. In fact, that is really what my next slide addresses.

(Slide.)

DR. OKRENT: But does that address the major issue with regard to ATWS, whether there is fuel damaged during the event itself?

MR. MAC DONALD: If there is no fuel damage, there would be no release of radiation. Pressure relief valves will take care of any overpressure situation.

I guess in my mind I don't see a problem with an ATWS if there is no fuel damage.

DR. OKRENT: Let me leave it at that for now.

MR. MAC DONALD: You may know more about this than I do. I am primarily fuel behavior-oriented, but in looking at the fuel in a rod -- well, with respect to ATWS, these have been recently classified as design basis event.

In our mind or understanding, the worst-case ATWS event could result in about a 700 percent power increase over about a two to three-second period in a boiling water reactor, and that is primarily due to the loss of the secondary heat sink, collapse of the voids, overpressure situation, primary collpage of the voids.

The power excursion is primarily terminated by Doppler feedback in these calculations. Recent GE proprietary reports that we have seen indicate that 1200 K cladding temperature could occur for a few minutes, and then that might be followed by a rapid power oscillation, accompanied by sloshing of the coolant on a collapsed and embrittled cladding.

The number of fuel rod failures during such event are not known. The magnitude of the damage to the fuel is not known, and it's really hard to estimate.

(Slide.)

DR. OKRENT: Before you leave that point, you referred to some GE proprietary reports. I'm just trying to recall whether the ACRS has copies of these reports. Can the Staff tell me?

DR. TOKAR: I can't speak to whether the ACRS has one report versus another.

DR. OKRENT: Have you seen the reports?

DR. TOKAR: Certainly the proprietary GE reports that came in, I think, at the tail end of last year, whether or how many copies were submitted, and whether GE submitted them to the ACRS, the Staff, as well, I don't know.

MR. BOEHNERT: We got copies of proprietary reports that were submitted in response to Mattson's failure verification.

DR. TOKAR: Then you have copies of those reports that have GE numbers on them, typical proprietary report numbers?

MR. BOEHNERT: Yes, I think we have them.

DR. OKRENT: Fine. But then item says ATWS events have recently been reclassified as design basis events.

DR. TOKAR: I can't speak for that.

DR. OKRENT: Okay.

MR. MAC DONALD: I guess maybe in the process of being classified is the more correct word. It has not been resolved yet, Bill?

MR. JOHNSTON: No, that is still in the form of a Commission paper, and it hasn't been discussed yet before the Commission.

In fact, I think it was scheduled for today or tomorrow, but it is not a position yet. I think that may be the unofficial recommendation, but it is certainly not official.

MR. MAC DONALD: Well, I have to admit I got that out of reading,, and so I'm not always sure of that.

(Laughter.)

(Slide.)

Okay, the kind of transients that we are planning to perform, at least initially, are shown here, in terms of a plot, neutron power versus time.

The OPTRAN 1-1 test will be basically a simulation of turbine trip without bypass, with scram.

The OPTRAN 1-2 test will be a simulation of turbine trip with bypass. As you note, it is a much more mild power transient.

The turbine trip with bypass going up to about 175 percent of nominal power. The turbine trip without bypass going up to about 350 percent of power.

If the reactors do not scram, as I mentioned, these powers continue on up to about 700 percent of nominal power, and then turn over due to Doppler feedback.

These particular histories were produced and sent to us by the General Electric Company. They represent a calculation with the Olden code, which has not yet been fully accepted by Regulatory. They represent basically a better estimate calculation of these events than I think some of us have seen in the past.

If we do not have failure during these types of transients, we have discussed with Dr. Picklesimer going to higher transients, more representative of some of the earlier GE submittals with less conservative models.

The turbine trip with bypass power shape is a power shape that you would expect with a frequency of about 15 times in the lifetime of a fuel rod. That isn't just that particular transient, it is also other transients that would create a similar power shape. But given the statistics published by EPRI for BWRs, we expect this event to occur about 15 times in the lifetime of a fuel rod.

The turbine trip without bypass is an event that you would expect to occur about once in a lifetime of a reactor. To my knowledge, such an event has not yet occurred.

DR. OKRENT: Excuse me. Before you leave that, I guess looking at OPTRAN 1-3, I can't tell why the Staff needs this experience;that is being run routinely on actual plants, so there is some data that you get from the plants. So if that is not sufficient, there must be something else you have in mind that you needed, so it seems to me you should be able to define it specifically, why you need it, and then to show that this experiment will give you the information you need.

Now, in fact, I would apply the same kind of thinking to each experiment and, in fact, I am going to at some point today issue a request -- and I will do it right now, in fact.

I, for one, before the end of the year would like to see from the PBF program in regard to each experiment just why it is needed and how it is going to supply the information that you believe is needed. Because let me say why I have this interest:

I was sort of a father of a similar program on LMFBRs, and I have grown to question how much one learns from this, and at what point you are really getting information that is commensurate with the expense and effort. And I think it is much easier to do experiments that give you information than to do experiments of the kind I have just indicated.

Before I, for one, try to provide a recommendation to the Congress on this program, which will be around January 1, that's when the committee will next write its report on the PBF area, I would like to understand in terms of the kind I just indicated why each experiment is being done, and I, for one, will not be satisfied with general kinds of arguments.

Now I may be a minority of one, but I'll just tell you I sort of feel -- I have been watching this move along in a general way for a long time, and been rather increasingly unhappy, like in the RIA area, but in some of these other areas as well, that they were too vague and not clear to me; that even though you were getting some interesting information, that is commensurate with the effort, and so absent that kind of information, I will sort of have to arrive at a position I can't tell why the NRC is able to say the PBF program should be run, because they have not provided a sufficient basis.

This will apply to the severe core damage, which is a still harder thing, I think, to define, because it is still a more nebulous thing, and yet if you don't try to do this, I think you will end up in a place where you have spent a lot of money and not necessarily gotten to where you want it.

DR. TOKAR: You want this particular response to come from the specific group, that is NRR versus RES?

DR. OKRENT: It is the NRC to whom the request goes, because now, you know, this is Research and NRR, and you have contractors and so forth. But I can't tell, in other words, and I will just choose OPTRAN 1-3 as a good example, but the request is general.

MR. MAC DONALD: Well, let me get back. What I would like to say, just finishing this slide, is that I am showing you the power histories for the OPTRAN 1-1 and 1-3. The OPTRAN 1-2 and 1-4 experience will be turbine trips without bypass without scram or main line valve isolation. Now we are going up to the 700 percent levels for about four seconds, and those are the first four tests in the program.

We will then do a test to look at PWR rod withdrawal without scram, and then we will come back and do some additional BWR collapse transients with higher burn-up.

These tests will be conducted with General Electric fuel rods that have been irradiated in commercial plants for burn-ups up to 20,000 megawatt days. Most of them are about 10,000 megawatt days per metric ton. There are a few up to 20,000 megawatt days per ton. And the fuel rods will include not only the standard product line, but also some of the new PCI-resistant fuel designs that have been developed with Department of Energy sponsorship.

We are hopeful for the later tests with the higher burn-up fuel that we can get similar rod segments from General Electric at high burn-up. If we cannot, our intent is to use very high burn-up fuel out of the BR-3 reactor in Belgium, which is less desirable than using the commercial fuel. The BR-3 reactor is a little less typical although it is a light water reactor.

The key information that we expect to get out of these tests, these two tests, is primarily pellet cladding mechanical interaction failure thresholds and probabilities. So that we can get at the question, how many rods are going to fail corewide during such an event, and is there any safety problem in this area.

For the ATWS events, the primary -- neither of these tests are expected to go into boiling transition, so the only kind of clad damage you are talking about are pellet cladding mechanical interaction high strain rate deformation of the cladding, possible tearing or fracture of the cladding.

The ATWS events will go into boiling transition, and there, as I mentioned earlier, we will get information on the ability of a collapsed and embrittled cladding to withstand the subsequent power oscillations and sloshing that accompany such an event, and will try to determine whether or not we have a safety concern regarding fuel failure and fission product release during the ATWS event.

DR. SHEWMON: If 1-3 is done five times a year on each reactor, do you have the result of what the statistics are on the failures for those?

MR. MAC DONALD: We have not been able to separate out failures during these type of events which have occurred in failures which are occurring due to stress corrosion cracking mechanisms during slow ramps, and the data at this point.

It may be possible to do more in that area.

DR. SHEWMON: So what you are saying is that when you run the turbine trip with the bypass, that the incremental failure rate is down in the range of a few per 10,000?

MR. MAC DONALD: Yes. I am not expecting failures during these tests, and my understanding of the reason for the request for these tests is primarily to show that fact, show it is not of concern.

I am not even sure we are going to get failures during the turbine trip without bypass, frankly, Dr. Shewmon. At least using this best estimate power history.

Now let me say that earlier GE power histories were up to about 500 percent, and we may have to go higher to get failure.

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DR. SHEWMON: I guess another statistical calculation, another way is to say that nine tests compared with 9000 elements, or whatever, and you may not have a great leg up on the statistics.

MR. MAC DONALD: What we would, of course, try to do would be to measure the strain after the test, what the cumulative effects are, and et cetera.

Okay. Moving on just briefly, because it will be covered at length later --

(Slide.)

-- the PBF severe fuel damage test program in our mind will be a series of highly controlled instrumented tests which will address Class 9 safety issues involving core coolability and the ability of the present cooling equipment on light water reactors to respond to severe post-accident cooling challenges.

We have a phase one of this program where we will be running tests up to about 2300 degrees Kelvin, that is slightly above the melting point for the zircaloy cladding. These tests are intended to be -- produce fuel behavior similar to what we believe occurred in the TMI-2 core.

We have proposed to the NRC a phase two, which would be tests with peak fuel rod temperatures up to the UO_2 melting point, and would look at issues of molten UO_2 penetration of such things as the lower vessel, the concrete basemat, or even earth under the reactor.

The phase one tests are in the present program. The phase two tests are proposal at this point in time, and under consideration by the Staff.

(Slide.)

We see the severe fuel damage test as feeding the severe accident analysis activities that will be funded and conducted by the NRC, as the data feeding the mitigation design studies that will be funded by the NRC, and most important, feeding into the rulemaking activities that are expected to be underway some time in the next year or so. I'm not quite sure when.

And I might note that the dates I put on the slide are highly speculative.

And finally, feeding into the regulatory criteria, setting of regulatory criteria, and the analysis appliance, it looks to us like the NRC is moving in the direction of addressing the Class 9 accident and addressing the question of needed mitigation features, and we see the PBF severe fuel damage test as being early input to that process.

DR. OKRENT: What does the term "early" mean?

MR. MAC DONALD: We intend to run the first test in December of 1981. In our opinion, there is no ther facility that can come within three years of that date.

(Slide.)

DR. OKRENT: What will you learn in December of '81 that's really important input to the rulemaking?

MR. MAC DONALD: Well, the purpose of running these tests --

DR. OKRENT: No, no --

MR. MAC DONALD: But it's only as the objectives of the test, the answer to that question, if I don't -- well, I can come back to it, but -- what we want to do is characterize fuel rod damage in terms of UO_2 dissolution movement, freezing, and rod fragmentation.

In other words, given a particular heating rate and given particular quench situations, what does this mass of severely damaged fuel look like? How coolable is it? And we wish to determine the coolability of the damaged fuel bundle.

Now we don't see these results as being the only results, that will answer these questions. We expect that the examination of the TMI core will be an important contributor to providing information in this area, and eventually either LOFT or SUPERSARA tests may contribute, although I don't see those tests in the early future.

But the important questions are how much UO_2 dissolution is there? How much movement, and what does the slag look like after these kinds of events? And how capable is your post-accident heat removal system of dealing with this kind of damage?

I don't think the answers are there, and I think this information will be helpful in deciding whether a post-accident heat removal equipment is adequate, whether we need things like filtered containments, we need things like basemats, or a lot of other expensive proposals. Whether we want to site plants near urban areas. We just don't have a lot of information on behavior of fuel cores at these kinds of temperature regimes.

We have got molten zircaloy, erosion of UO_2, a lot of movement of that liquified material.

DR. SHEWMON: I think we've got your point. Thank you. Why don't you let us read the conclusion, since we have taken an hour on this? I think we can do that.

(Slide.)

MR. MAC DONALD: The conclusions simply reflect what we feel have been the most significant impact of our research to licensing, and I think we have discussed the --

DR. SHEWMON: You've made the point. Thank you.

MR. MAC DONALD: -- those points. And the last page of my conclusions --

(Slide.)

-- simply reiterates that we feel that we need data on both OPTRAN damage mechanisms and failure thresholds, should failure occur, and we need data on core characterization.

DR. SHEWMON: Okay. Thank you.

MR. MAC DONALD: Wait a minute. I would like to make one brief comment before I get off. I would like to reiterate the point that we believe the PBF facility is the only facility capable of providing this kind of information. That if the PBF facility and the associated technical staff was not in place and providing this information, that there would be, I think, a significant void in the overall safety program.

We also the PBF severe fuel damage test as very important and complementary to LOFT and the ESSOR test and, in fact, without PBF, the severe fuel damage test, we don't think that the LOFT and the ESSOR program would move ahead, because they wouldn't have the information to do controlled and well instrumented experimental programs.

Thank you.

DR. SHEWMON: Thank you.

DR. PICKLESIMER: On the last day on the review of the fuel behavior research program, we covered fairly extensively at that time our preliminary plans --

(Slide.)

-- on the overall several fuel damage study. Now I would like to cover today primarily that which is associated with PBF. I plan only a very summary presentation. Dr. Buescher will give you the details on the PBF several fuel damage test.

Now the question was raised this morning about the integration of this planning with other groups. I would like to point out that we are coordinated and integrated with severe accident phenomena and mitigation decision unit. We have worked continuously on both our programs and their programs, experimental plans and so on.

We also are coordinated with the PBF severe fuel damage studies, the ESSOR SUPERSARA program, both in Europe and in our branch with Dr. Van Houten.

We are in contact with the people at PHEBUS and their proposed severe fuel damage studies. We are working directly -- I am -- with the examination groups for the TMI-2 examination, and we are working with the people in LOFT in discussing what possible severe fuel damage tests might be done on LOFT.

I don't know how we could be more coordinated.

(Slide.)

DR. OKRENT: Can I make a suggestion how you might be more coordinated?

(Laughter.)

Since you didn't seem to know how. What I will be interested in learning with regard to the severe fuel damage program, all of this, is how the particular programs, whether they are PBF or elsewhere, how in fact can be expected to affect either the design of existing plants or future plants, or some action that you might take during some future accident or something like this.

If you don't have such a connection, and I think the latter one is a very difficult to make myself, so I think if you are going to make one, it is going to be to design. If you don't have a connection to design, then I tend to view the program as one of getting what I will call general information, but not the design, unless you are somewhat able to bear directly on an NRC direction something doesn't need to be done, and I will accept that as also -- in other words, they don't have to design something. That's also a design question to me.

So maybe I have made a point about a different kind of coordination integration than I see here. If I haven't, I will repeat it.

DR. PICKLESIMER: Well, if I understand your question, you are asking for design answers before we know what the problem is.

DR. OKRENT: No. What I'm saying is you could do a long program here and still have more to learn than you have learned, because it is a terribly complicated problem, and so just getting the information, if it is not going to fit into design or into some specific policy decision, will make it, let's say, interesting, but I don't know whether that justifies the rather large kind of resources that are being proposed.

DR. PICKLESIMER: One of the things we are being asked to look at is the possible means of mitigation of an accident during the progress of that accident. There is no way we can take a look at possible mitigation schemes during the accident if we don't know what the progress of that accident is.

Therefore, we must get this general information first to bound ourselves, to give us some idea for the more detailed experiments that will provide answers later. No one has ever produced in-pile liquified fuel that we can get our hands on. We don't know what it looks like.

DR. SHEWMON: You don't really want to have your hands on it.

(Laughter.)

DR. SHEWMON: I guess I don't know what this statement means.

DR. PICKLESIMER: We have been able to produce out of pile a solid which was formed by the reaction of molten zirconium, molten ZRO_2 and UO_2. We think we know what that looks like if it is done in pile. We don't know that. We have not been able to produce the beds out of pile that would be characteristic of what's done in pile. If we don't know what the debris beds look like, we don't know whether we can cool them or not.

DR. SHEWMON: If you have seen one, you sure haven't seen them all. So that's part of Okrent's question.

DR. PICKLESIMER: If we have seen some, that's better than none.

DR. OKRENT: But you said you hope -- I can't remember whether the term was hope, but, anyway, you think you may be able to develop information that would bear on what somebody would do to mitigate an accident during the course of an accident.

DR. PICKLESIMER: That is correct.

DR. OKRENT: Now if in fact you can do this, you will have earned my compliments, and I will give them to you. But I said earlier I think that is a terribly hard thing to do. If you could just show me on paper one scenario, as it were, of course including the variations therein. Let's say you don't know exactly what the scenario will be -- where in fact given that scenario and given some kind of a program that you have done, this information could somehow be fed in, on a real time basis, to influence what was being done at the plant differently than what they would do with what they had to throw out and so forth.

I think, you know, you may be able to convince me, but if you can't provide one realistic scenario of that kind and show how it relates to the specific experiments that you think you can do, then I will still be in my current position, I guess, where I think it is a very --

DR. SHEWMON: Why don't you take those sage words of advice and try to get on to your presentation, and maybe you'll come to some of them. If not, we will ask you again next time when we see you.

DR. PICKLESIMER: As part of our discussions --

(Slide.)

-- on part of the severe fuel damage studies that we plan to undertake, we have been discussing a program or proposed program, and asking the technical community for their ideas for what the needs have been.

Here is a list of the meetings we have held so far, and the meetings we have in the near future. I won't go into detail, unless you particularly want.

One of our problems in trying to lay out a program plan is to have some idea of what funding we are going to have available. We started out with an idea six months ago of what would be needed. That amount has been cut and cut and cut again. Every time it takes a 25 to 50 percent cut, and we have to rearrange our plans.

(Slide.)

What we are going to wind up with from Congress this year for the FY '81 budget, we don't know. Our present plans on the severe fuel damage studies are concerned with the in-pile and ex-pile integral effects in bundles. That's why the PBF test will be bundles. We are shooting for 32 rods. We will be looking for separate effects tests, and basic studies of properties, mostly out-of-pile tests.

We will be looking for fission products in the fuel and fuel debris for modeling of the code development for core damage.

We expect to detect fission product releases and sample during the release itself, so we have some idea of what is coming off when in the experiment.

Now the discussions for the rest of the day will be concerned primarily with our plans in severe fuel damage studies and the PBF, the ESSOR, SUPERSARA and NRU.

(Slide.)

I would emphasize the severe damage test and PBF will be the first to look at that formation in the liquid fuel formation, will be the first to characterize the debris formed, and to examine a number of the test parameters. Out-of-pile debris formation is hindered by the fact that we must have a center line heater in the rods to get them to this very high temperature which drastically interferes with the destruction of that bundle and the collapse of that bundle. It prevents it.

Therefore, we cannot know what the debris bed looks like under these kind of conditions. We must do it in pile.

In scaling to commercial reactor conditions, our sizes will require extrapolation of the PBF 32 rod data from the three-foot length, where we expect only a fairly short amount of rod to be seriously damaged to the ESSOR 32 rod 1.8 meter test, where we expect to get three to five times as much material damaged through the LOFT test, which is a 15 x 15 six-foot test, as it is done, and e will use TMI-2 as a benchmark.

Now with that availability, PBF will be providing data from FY '82 to '85; TMI-2 about '83 to '84, unless we run into some more; LOFT, if it goes, FY '83 to '85, more likely in '85. ESSOR will begin to provide some data in FY '84, and will be completed about FY '86.

Thank you.

Do you wish to go on now with Dr. Buescher's presentation, or do you want to wait until after lunch?

DR. SHEWMON: No, you were so precise, let's go on and see how he can do.

DR. BUESCHER: Good morning. My name is Brent Buescher, and I'm here to talk to you today about the severe fuel damage test series that we are going to run in the PBF facility.

(Slide.)

Basically I am going to cover the objective of the test series, the scope of the test series, the preliminary test train design that we are currently working on, and finally the expected results that we intend or anticipate getting out of this program.

(Slide.)

As Philip discussed, the test program consists of a series of highly instrumented tests which will address the Class 9 safety issues involving core coolability after severe accidents.

It is divided into two phases, and phase number two, which is the phase I'm going to talk about today, covers temperatures up to 2300 degrees Kelvin, which includes the melting point of zircaloy.

(Slide.)

The key questions to be addressed by this program are what is the nature and extent of fuel bundle damage during the severe temperature transients?

What thermal hydraulic conditions will lead to the formation of a rubble bed and its fragmented fuel rods and fuel pellets?

What thermal hydraulic conditions will result in a layer of frozen slag?

And finally, how coolable is the test bundle after experiencing a severe high temperature transient?

(Slide.)

The sequence of damage that we envision in this test is that the clad will first balloon and rupture. This will be followed by oxidation as it heats further, once the melting point is reached, the remaining zircaloy will melt. It will react with the UO_2 pellets, forming a UO_2 zircaloy zirconium melt.

This will flow down to the lower part of the bundle where it will refreeze during the cooldown or quench. The rods will tend to fragment and form a rubble bed.

(Slide.)

This slide shows the results of some out-of-pile tests run at Karlsruhe, and these are the test conditions used at the bundles. The first two figures show bundles that were tested to about 2300 Kelvin. They were ramped up at 2.5 Kelvin per second.

The cladding was severely oxidized in this bundle, and very little liquification occurred.

In the last two figures, the temperatures reached were about the same, about 2300 Kelvin, but the temperature rise rate was much faster, and the cladding did melt, did react with the UO_2.

DR. SHEWMON: I take it the little "k" for second and the big "K" for temperature should be the same?

DR. BUESCHER: Yes.

DR. SHEWMON: Thank you.

DR. BUESCHER: In the PBF test series, we plan to run five tests and cover -- they will all be tested to a peak temperature of about 2300 Kelvin, and we are going to cover both a fast rise rate and a slow rise rate.

The first has the scoping test. It will be heated up at a slow temperature of .5 K per second. We anticipate getting highly oxidized cladding and we are going to terminate this test with a slow cooldown and try and preserve the bundle characteristics that were present at the high temperatures.

DR. SHEWMON: How much is still solid at 2300 K? What is solid from the original fuel element?

DR. BUESCHER: The fuel pellets on the oxidized zircaloy would still be solid.

DR. SHEWMON: So zircaloy is melted, but zirconium is still solid?

DR. BUESCHER: Right.

DR. SHEWMON: No slag that forms?

DR. BUESCHER: In this particular test, we are going to try and have primarily oxidized cladding, so I expect it to be mostly solid.

DR. SHEWMON: Okay.

DR. BUESCHER: In test number one, we are going to bring the bundle up to a temperature of 4 degrees Kelvin per second, and we anticipate that in a large fraction of molten zircaloy in this test. This will react with the fuel pellets and form the liquid -- or form a slag bed. This test will also be terminated by slow cooldown to try and preserve the structure that occurred during the high temperatures.

Test numbers two and three are going to be repeats of the scoping test with the exception that they will be terminated by fast quench, and this will be done to produce a fragmented rubble.

And finally, test number four -- in test number four, we are going to try to reproduce the temperature history that occurred in Three Mile Island.

(Slide.)

The schedule for these tests is shown in this slide. Right now we intend to run the scoping test in December of 1981, and we will continue to run these tests at approximate six-month intervals, and the final test, test number four, in April of 1984.

DR. OKRENT: A little while ago when I was asking in what way these tests might have input to the rulemaking, it was suggested that you would already have input in 1981. In fact, the answer to the question wasn't really one in terms of how it could affect the rulemaking.

Looking at your full program schedule out through

1984, I will ask you, what input do you think this would have to the rulemaking?

DR. BUESCHER: I think that depends on the schedule for the rulemaking hearing.

DR. OKRENT: Well, I have to assume that the NRC hopes to have completed this rulemaking by 1984. It is now still 1980. I assume we are not talking about 1994, although I can't guarantee it.

DR. SHEWMON: Are you suggesting that the NRC's track record on research is that it is done before, or during rulemakings? Certainly the experience isn't that.

DR. OKRENT: No. I'm trying to understand whether this is an optimum series of tests, or whether it will contribute at all. Where it will contribute is a later question. I earlier phrased it in terms of some scenario, and I have heard now a brief description of what these are, and I am skeptical at the moment about where we will be having run these, and our ability -- either, as I said, to design something differently. I'm certainly skeptical about what you could do in midstream in an accident that you wouldn't already be doing.

So again --

DR. SHEWMON: Let me ask a different question. I keep getting confused on temperature scales. But in WASH 1400, they assumed when they got up to 2100 something or other, that everything was molten. Was that 2100 F, C, or K?

DR. BUESCHER: That was 2200 F.

MR. VAN HOUTEN: That's not WASH 1400. That's Appendix K.

DR. SHEWMON: WASH 1400 also said when they get up to some temperature. Then we'll just approximate it by saying we have a core melt.

MR. JOHNSTON: I assume that if you went beyond the LOCA acceptance criteria, you lost control of the whole thing, it just went on up.

DR. SHEWMON: So one of the things you are doing here or getting out of here is some idea as to how conservative that assumption is on core coolability, beyond where they threw up their hands and quit for conservatism. Is that right?

DR. BUESCHER: That's right. And that is basically the problem that the industry, I think, has, when you get above the design basis accident.

DR. SHEWMON: We can discuss that in committee, but I think we probably have a fair -- it's worth discussion.

DR. OKRENT: If that's all we're going to get out of this, then I might define it as the objective, and I must say it would be a very modest objective, indeed.

DR. SHEWMON: Well, that's why we will have an interesting discussion in committee, and I'm sure that we will find something else that's more of an objective than that.

Why don't you go on?

(Slide.)

DR. BUESCHER: For this test series, all of the test trains are going to be essentially the same design. It's going to be a 32-rod bundle. This will be contained in an insulated flow shroud. It will have zircaloy inner and outer walls, and sandwiched between those walls will be a zinc oxide insulation or insulater.

We are going to run a continuous bypass flow around the test train during the test, and finally we are going to control the flow into the bundle with separate lines coming in at the bottom.

(Slide.)

This slide shows the conceptual design of the fuel rod, the bundle and the shroud. The bundle itself is a 6 x 6 array with the four corner rods left off. It is inside the insulating shroud.

The coolant into the bundle is brought in by these four lines here, the lines from the instrumentation of the bundle would be brought up along the outside of the shroud in the bypass region, and that's because of the high temperatures that occur in the bundles during the transient.

The bypass flow will flow down the outer annulus and back up through the inner annulus, and provide continuous cooling.

(Slide.)

This slide shows an axial profile of the test bundle starting at the bottom. We've got three flood lines coming in. During the test, prior to the transient, we will run about 30 gallons per minute through these reflood lines, and we will keep the bundle full of water, and during the transient we will drop the flow down to about half a gallon per minute, and allow the bundle to boil down and the water level will reach an equilibrium level.

(Slide.)

This slide shows the region immediately above the bundle. During the transient the steam coming out of the bundle will be at about 2000 degrees Kelvin, and it will go up through this zirc oxide pebble bed, and what this will do, this is intended to keep any water entrained within the bundle from falling down into the hot -- or within the test train from falling into the bundle itself.

(Slide.)

The instrumentation on this test is as follows:

We plan to have six center line thermocouples in the fuel rods, 24 cladding ID thermocouples at various axial locations inside the fuel rods, 28 shroud thermocouples. These will be on the outer wall, the inner wall, and in the mid-plane at various axial locations in the shroud. We're going to have 32 coolant thermocouples. They will start down in the bottom, in the entrance water, and all the way up through the sphere bed to measure the temperature of the steam.

We are going to have two flow meters, one to measure flow within the bundle during the high flood parts of the test, and also one to measure the bypass flow, and have 17 pressure sensors. Most of these will be on the fuel rods, so we will measure the pressure within the bundle and within the loop.

We'll have two strings of five SPNDs to measure fission product during the test, two flex flows, and finally we are going to have to put a temperature profile detector which will measure temperature isotherms somewhat below the peak temperature and watch the axial movement of these isotopes.

(Slide.)

This slide shows the loop configuration for these tests. For the nominal test in PBF, we run the water out of the pump into the in-pile tube that goes down through the downcomer, and back into the pump. For these tests we are going to have a separate line coming on through the head to feed water into the bundle, and during the transient we will bring the steam out of the separate line, past the manifold, fission products through the condenser, past our fission product detection system, and we will dump that water into a collection tank.

(Slide.)

In support of the design work, we are also doing some thermal analysis work as conceptual design. This is to evaluate the heat transfer and the test train, establish the linear power requirements for the test bundle, and also to be able to establish the full requirements for the design.

(Slide.)

The analytical model we are using is a BWR TRAC code. We are using the CHAN component of this code. The CHAN model is a fuel bundle within a bypass flow. It treats the whole fuel rod cluster as a single channel, and it has one dimensional thermal hydraulic calculations, which makes it a very fast running pump and versatile code.

(Slide.)

The heat transfer modes considered in CHAN are conductive heat transfer in the fuel rods and across the shroud wall, convective heat transfer in the fuel rods and shroud to both the coolant, both inside and outside the shroud.

Radiation heat transfer is treated from rod to rod, rod to shroud, rod to steam, and shroud to steam, and in the case of two-phase flow, you can also have radiation heat transfer to water droplets, and that also includes heat from metal-water reaction.

(Slide.)

This slide shows the temperatures in the bundle as a function of time for a transient with a flow rate of about a half a gallon per minute. It's equivalent to what happened -- the best estimate of what happened at Three Mile Island.

DR. SHEWMON: Where does the steam water interface end up on that? Some place down on the bottom? Do you have any preheating, or is it just radiation?

DR. BUESCHER: No, the water comes in at basically about 20 degrees subcooled, and then it's boiled off in about the first six inches of the bundle. The peak temperatures -- this slide shows the peak temperatures in the fuel rod which occurred at the top as a function of time, and the peak temperature reached was 2300 Kelvin at approximately 200 seconds, and that dropped off to about 2100 Kelvin for this linear heat rate, because of the metal-water reaction going to completion.

(Slide.)

This next slide shows the same bundle, same calculation. This is at 600 seconds. This is the axial profile temperatures within the bundle, and as you can see, up near the top, the shroud and the fuel rods are within about 100 degrees of each other.

This steam also exits up at about 1900 Kelvin -- 1800 Kelvin.

(Slide.)

This is a map of the fuel temperature within the bundle. This is at the top of the bundle at 600 seconds, and the temperatures are all quite close to one another. They are within about one degree, which is well within the calculational accuracy of the code.

For this particular bundle, we run a uniform enrichment, so we have got a radial peaking factor of about 1.16, something like that. It peaks on the outside.

(Slide.)

Finally, let me cover the results that we anticipate getting from this test series. We anticipate getting zircaloy oxidation rates at temperatures in excess of about 1800 Kelvin, and verification of zircaloy embrittlement and in fragmentation criteria at the quench temperatures.

We expect to get rod fragmentation and UO_2 desintering data from the quenching.

We also expect to be able to characterize UO_2 dissolution by the molten zircaloy in redistribution and freezing --

DR. SHEWMON: If I substituted melting for dissolution, would I get the same thing? Or what is dissolving in what? That's the second time that word has been used.

DR. PICKLESIMER: You don't melt it, you dissolve it, that's what this is.

DR. SHEWMON: It seems to me I've heard of that physical chemistry before. Would you now answer my question.

DR. PICKLESIMER: The UO_2 is dissolved in molten zircaloy. It's not melted. The temperature is 2800 degrees Kelvin.

MR. MAC DONALD: I used the word dissolution.

DR. SHEWMON: Go ahead.

DR. BUESCHER: Then redistribution and freezing of the liquid material.

We also expect to get -- be able to measure the effects of the dissolution of the UO_2 on the fragmentation of the UO_2 and the quenching and the fission product release, since we are going to be monitoring.

DR. SHEWMON: Now that dissolution of zirconium is what some people were talking about when they rather loosely used the word "eutectic" back in TMI-2, and made some of us scurry off, wondering what the hell eutectic was. Is that right?

DR.PICKLESIMER: That was a misuse of the term. Eutectic is a zirconium, ZR_2 material. If these diagrams indicate you can't have a eutectic with a UO_2 at a somewhat lower melting point in temperature, this zirconium -- eutectic -- but eutectic has not been completely demonstrated.

DR. SHEWMON: Okay. Thank you.

DR. BUESCHER: Finally, we hope to be able to determine the hydrodynamic and heat characteristics of the rubble bed in its post-test configuration, and this will be primarily through hot cell examination.

DR. SHEWMON: What will collect that rubble bed? Will it go up or down? Will you end up with the little stuff going up and the big stuff going down, or do you know?

DR. BUESCHER: That's a good question.

MR. MAC DONALD: There's hardly any flow. We're just basically sitting there, steaming off, and so there shouldn't be too much go up, other than the volatile gaseous fission products ought to go up with the steam, and some of the volatiles will play along the line, and some will end up in those tanks connected to the manifold.

DR. SHEWMON: You've got 4/10ths gallon per something or other here that you're vaporizing and heating to 1800 degrees Kelvin. It seems to me that's a fair volume change, and thus ought we to -- what was your flow rate? I guess I'm confusing temperature rise and flow rates.

DR. BUESCHER: The flow rate is a half a gallon per minute of water.

DR. SHEWMON: A half gallon per minute of water converted to 1800 Kelvin, so there's a fair velocity of steam coming out the end of that thing, isn't it?

MR. VAN HOUTEN: At 2000 psi.

DR. SHEWMON: I see. Okay.

Thank you.

Is that it?

DR. BUESCHER: That's it, yes.

DR. SHEWMON: My personal reaction is if you can pull it off, it's quite a shift. I won't say that it all came as a result of an ACRS recommendation, but somehow it is interesting.

Do you have a comment?

DR. OKRENT: I don't have any different opinion that I had before. I think it is not likely to be worth the effort and expense, based on what I have seen so far.

DR. SHEWMON: I guess I would differ from that, because I personally have been particularly offended by the assumption that once we reached 2200 degrees Kelvin, the only line of defense is deciding what to do with that stuff besides fry eggs with it on top of the hot concrete floor. And if, indeed, that is a lousy scenario, it seems to me -- or it may not be a lousy scenario. This is probably the first step I have seen that would tend to begin to tell you indeed what does happen when you get above 2200 degrees F.

DR. OKRENT: I think the questions that are raised by what happens if you get to some point short of 5000 Fahrenheit over the whole core is an important question, but I don't expect to have major contributions to what happens there, or what you would design differently, or what actions you would take, given an accident from these experiments.

So it is not that I am disagreeing with you, that this is a potentially importa.. area to think about. I just don't see big input from what's being proposed here.

DR. SHEWMON: I guess I do, because again my offended sensitivities at the assumption that, gee whiz, once it's 2200 degrees F, it magically and instanteously, at least within some blacked-out period of time, converts to a completely molten mass going through the bottom of the containment or else already gone through the bottom of the containment.

And since these temperatures are twice 2200 degree F almost, the fact that you would have an idea of whether indeed you want to worry about it is all molten steel down there, or whether a lot of it's hung up, or whether it comes apart in coarse particles as distinct from the thinnest and smallest things that anybody at Sandia can think of on a bad day, all seem to me to be very pregnant questions.

I don't know if the theorists have been particularly honest, and it seems to me that this set of experiments might send them back to their computers and straighten them up some.

DR. OKRENT: Well, I'll leave it at that.

DR. SHEWMON: Thank you. I guess if that's it, then I guess we'll adjourn for an hour for lunch, and be back at ten till 2:00.

(Whereupon, at 12:50 p.m., the meeting was recessed, to reconvene at 1:50 p.m., this same day.)

- - - - -

AFTERNOON SESSION

(1:55 p.m.)

DR. SHEWMON: Are you ready?

DR. VAN HOUTEN: Not quite.

(Pause.)

DR. SHEWMON: Please begin. Fire when ready.

DR. VAN HOUTEN: My name is Robert Van Houten. I work for the Fuel Behavior Research Branch, Office of Research, Nuclear Regulatory Commission.

The first slide --

(Slide.)

-- viewgraph in the first page of your handout is the schedule for the ESSOR SUPERSARA severe core damage test program.

As you can see, it is set into two blocks: the test loop and the program.

The test loop shows that the main equipment is being ordered between 1979 and 1981, with assembly in 1982. Most of the major equipment has been ordered, and many of the parts have been received at Harwell at this time.

Out-of-pile commissioning is expected to take place in the middle of 1982. Nuclear commissioning in the following nine months, and large cluster operation then is to begin late in the summer of 1983 through 1986.

For the program now defined under the program section, program verification effectively establishing the consensus program, final details of this should be completed by January 1st, and then the detailed test planning will follow, with appropriate support efforts on test train development and procurement. Then followed by the reactor test from mid-1983 through 1986.

The associated test evaluation and reporting.

(Slide.)

The key elements of the program, as it now exists, are that it will consist of 18 severe core damage tests, of which it is expected that 12 will be the small break TMI-2 type test, and these will be carried in the several tests, first at lower temperatures -- I'll show you that on the next viewgraph -- and then later through clad melting with peak clad temperatures in excess of 2300 K.

Debris bed will be formed and it will be examined. These are 32-rod clusters and the driver core is approximately 1.8 meters in length. We can reasonably go to a fuel length of up to two meters.

In addition to those 12 tests, there will be six severe ballooning flow reduction tests with 32-rod clusters.

DR. SHEWMON: Would you back up for a minute and say a little bit about who is involved in ESSOR and what the criteria were for the design of this test flow?

DR. VAN HOUTEN: Yes. The ESSOR is the European

Economic Community which now consists of 10 countries, if I am successful in listing them all:

Great Britain, France, Germany, Italy, the BENELUX countries --

DR. SHEWMON: That's probably close enough. Go on to the criteria.

DR. VAN HOUTEN: And the program has evolved over the last five years from one in which the Italian government was to take the lead with a small bundle LOCA test program to be followed by the large bundle test program sponsored by the European Economic Community, to one now in which the Italians placed additional monies into the funding of the loop and preparing it for the test series, and at present the program is to be a consensus program for which they have had a series of meetings in April, June, July. There will be another meeting in September, and it is expected that the full program will be set for review by the Directorate in October on the release of the funding.

DR. SHEWMON: That's interesting, but let's go back to the loop, which apparently is reasonably well committed by this time. What are its characteristics?

DR. VAN HOUTEN: All right. Let's move ahead. I do have it at about the sixth slide. I would much prefer, if you don't mind, that I tell you a little bit more about the experiment, so you can see what the loop will do.

DR. SHEWMON: Whichever way you want to do it.

MR. VAN HOUTEN: All right. I'd like to.

The basic program is budgeted for the total cost including the loop of 140 millions through 1986, and the planned NRC cost is 11 millions through the end of this test period, of which some 70 to 90 percent will be spent within the United States.

(Slide.)

The consensus program, the eight tests which have been agreed to, in the June and July meetings, as having highest priority are the eight shown here, in which in the severe core damage mode the initial two tests would be at between the high alpha and the alpha plus beta range to establish maximum ballooning, check out the equipment, determine if there is a difference in the extent of ballooning, and the somewhat slower loading mode which exists in the severe core damage type accident.

The small break, as compared with the large break accident test, which can and will be run in the same loop.

Progressing from those tests in which they will be terminated at substantially the rupture temperature, it will progress test 3 through 1650 K, and then there would be three tests at 1900 K, one of which would be rapid quench, to produce debris, and then two tests at an above 2300 K peak clad temperature; one of which would again be quenched.

Now what were the basic criteria for setting this up? I'd like to skip the slide on the licensing requirements for a minute.

(Slide.)

And move to the criteria, and then to determine what will be the loading placed on the test train and on the loop to determine if it has the capabilities of performing the test.

The criteria agreed to were, first, that the emphasis be on test with cladding, ballooning and rupture in the high alpha where maximum ballooning is expected. Then followed by the continuous cladding rise to the peak clad temperature, which is projected in the later tests to be at 2270 K, and for which we have established the general conditions for the additional four in that group should be established in the September meeting.

Item 2, the tests must involve the creation of in-pile rubble beds. The thermal hydraulic characterization of the rubble bed flow obstructions, where possible. This means running the bed at low power and determining the delta T and delta P associated with this.

Three, the tests must go through drip melting of the cladding.

Four, the fission product release monitoring instrumentation should be added to the loop to provide ad hoc data to check the fission product release codes.

Five, that a particular consensus effort should be applied in obtaining the interaction between drip melting and the thermal hydraulics analogous to full length conditions.

And further, incorporating instrumentation for measuring liquid level on fission product species as the test progresses.

And if we have the time, at the end of my talk, I am going to ask Ed Courtwright to address some of the ways the instrumentation can be used to follow the progress of the accident, and somewhere in this question as to how the data can be used to determine where the accident is, and what recovery procedures are reasonable.

DR. SHEWMON: Could you explain what consensus effort is as distinct from other kinds of effort.

DR. VAN HOUTEN: Well, 10 countries have to agree on the program before the monies are released, and this therefore reflects, as shown in the footnotes, the position of the 10 country representatives at the June meeting as to the listing of priorities.

DR. SHEWMON: Look at the last five. It says particular consensus effort should be applied to obtaining interaction between drip melting and thermal hydraulics.

DR. VAN HOUTEN: Analogous to full length conditions. What that means is that the group will approve a general program and they wish to have the specialists from each country to work, to incorporate the appropriate instrumentation and techniques for providing the wanted information.

DR. SHEWMON: Fine. I guess I just don't understand it. But let's go.

DR. VAN HOUTEN: Well, these aren't my words. This is taken directly from the letter from Mr. Contzen to Dr. Budnitz, reporting on the results of the June meeting.

DR. SHEWMON: Maybe it suffered in translation.

DR. VAN HOUTEN: Now the question of what is the loop and what can it do.

(Slide.)

The general schematic of the facility is shown about two slides further down, and as you can see in the plan view, there is the bunker, the SUPERSARA, and in this bunker all of the out-of-pile equipment is placed, including the analog, electrically heated 32-rod bundle, and the associated pumps, heat exchangers.

(Slide.)

The loop has a number of similarities with the power burst facility, except for the fact that it can handle two meter light fuel rods, and that further it is equipped with an out-of-pile and electrically heated bundle for establishing the thermal hydraulic conditions in a much less expensive mode than in the nuclear mode.

And that further it is equipped with additional heat exchangers and heaters, so that we can control the inlet enthalpy of the coolant, and so that we can control very carefully the depressurization loop rate -- the depressurization characteristics of the loop as needed for small break tests, and that we have the associated heat exchangers for handling the test program.

The general design and flow --

(Slide.)

-- shows the mode for preconditioning, and as you can see, since the out-of-pile bundle is in a leg of the basic loop, the thermal hydraulics in pile and then the out-of-pile bundle are effectively the same. The one difference, of course, is they will not be using pressurized rods in the out-of-pile loop portion. And that because of this difference, obviously, we will not be driving them into a deformed mode.

(Slide.)

In the blowdown phase and the LOCA mode, it shows the associated piping.

(Slide.)

Through reflood. And it was then necessary to perform a study to determine whether or not the small break test could be performed satisfactorily in the loop as it was designed. Specifically in that direction, Jim Broughton and Ed Courtwright and I went to Ispra a year ago June, and we reviewed the basic design elements. We recommended that consideration be given for minor modifications of the loop to accommodate the small break test program, and these modifications have been incorporated into the design, but with the thread that if we wanted any more changes, it would delay the schedule.

(Slide.)

Now the next slide after the loop schematic, the in-pile test schedule section, an indication of the main features that we have hot or cold-leg blowdown capability, we have the ability to control the pressure and depressurization rate. We can reflood at a steady rate or at a programmed rate. We will be able to keep the fission product release within a small portion of the circuit, and further that we do have full two-phase inlet capability.

This is one of the things we don't have in the power burst facility.

(Slide.)

Its cross section is such that, skipping on two slides, you will see a view that is very reminiscent of what Brent Buescher showed you this morning, and it should be, because there has been very close coordination between the two programs.

I think you correctly said that the ACRS could take some credit for the way the small break severe core damage programs are evolving, but let me add that this is particularly so by reason of the fact that some lead work which had been done for the Fuel Behavior Research Branch by Pacific Northwest Laboratories, and then had been applied to what a severe core damage test might look like in the ESSOR reactor, this work done a year and a half ago, was given a great deal of support at the upper levels of management, and the question of can you do something like that in the power burst facility and what are the common features, and with this base of support which was a result of your comments, I believe, why, we then proceeded quite vigorously to develop what you have seen today through Brent's presentation. And you will hear a little bit more about what PNL has done in the joint effort from me, and also from Ed Courtwright.

DR. SHEWMON: TMI-2 may have helped, too.

DR. VAN HOUTEN: Oh, you bet it did.

DR. SHEWMON: Let me come back to a general question. The ESSOR reactor is how much power?

DR. VAN HOUTEN: It can be run at about, I believe it's 110 megawatts.

DR. SHEWMON: Okay. It will be run in a dedicated mode for this exercise?

DR. VAN HOUTEN: That's the reason we see the big gap in the schedule drawing. It will be transferred to a dedicated mode in the next 15 months.

DR. SHEWMON: Okay. And the way you run these tests, you vary primarily the flow rate, not the reactivity in the core; is that right?

DR. VAN HOUTEN: No, we have a problem of a little bit more test core coupling in the ESSOR facility than we do in the power burst facility, and so this does complicate the performance of the test, but, yes.

DR. SHEWMON: Okay. Thank you.

(Slide.)

DR. VAN HOUTEN: This matter of the out-of-pile test section with the electrically heated fuel rod cluster in the pressurized chamber, we can facilitate the loop check-out through valve sequencing response time. The heat transfer c oefficients, the general characteristics for the earlier phases of the small break accident at appreciably lower costs, a nd without contaminating the equipment so much by radiation. So that accordingly minor repairs and adjustments to instrumentation -- because there will be some instrumentation on these bundles -- will be much.simpler.

DR. SHEWMON: Are there heater elements in the center of each rod?

DR. VAN HOUTEN: Yes.

(Slide.)

Now in the small break category of tests, in our meeting of a year ago June, largely with the efforts of Jim Broughton, but with the rest of the support group, we elected to examine these within four basic groups, in which group 3 is the deformation and rupture, preferably at temperatures in the high alpha for the maximum ballooning, followed by the continuing heat-up with internal oxidation through the rupture region, as well as external oxidation, and then followed by fragmentation associated with both slow and particularly with quench cooling.

The category No. 4 is one in which you would have the simultaneous deformation in oxidation, and it is believed that the debris bed characteristics from type 4 and type 3 and then type 2, in which by reason of the nature of the accident, the external pressure would be higher than that within the fuel rods, you would not have ballooning. It is believed that the debris beds in these three cases would be different.

(Slide.)

On that basis, going back to the figure which is about fourth in the handout, it is very difficult to read -- my apologies for that -- but that showed in a tentative test matrix for the test given the highest priority, the planned peak clad temperature, the associated ramp rate, and some indication of the control, what was expected in the final form, then what our major elements prior to removal of the test train from the loop were.

And as you can see here, with the debris beds formed in the several modes, the one in which the peak clad temperatures is about 1900 degrees C -- 1900 K, but with slow cooling to measure the delta P and the delta T, generated across the rubble bed, using low power and flow.

Then in this test, having taken it up to 2270 and slow cooled, similar delta P and delta T measurements across the candle zone of the core, then the rapid quench, similar delta T and delta P measurements.

Now this comes from the consensus letter, so this is the consensus position agreed upon by the 10 countries at this time.

DR. OKRENT: How meaningful do you think a delta P versus flow measurement will be, if you don't have a uniform bed? If you have a bed which has channels, and if it does have channels, you know, what will you do about it, and how could you preclude it from having channels?

DR. VAN HOUTEN: Fortunately George Moreno has the responsibility on the modeling, so we will provide him the data as we examine the bed and extent of channel, and we see how his models will adapt to that.

DR. OKRENT: Well, it's fortunate from your point of view, but I am trying to ascertain whether this particular portion of your experiment is going to provide anything which has any major significance besides somebody trying to calculate the specific measurement.

I agree, if you have the knowledge of the channels and so forth, you can try to predict what it is theoretically, and see if it conforms to the measurement, and it could be a very interesting calculation.

But I don't know that it relates to what I'll call a practical licensing or reactor safety application.

DR. VAN HOUTEN: Well, I have some data from the debris bed formation than much smaller in the power burst facility, and as we can determine the general characteristics of these beds, so that we can make a realistic one, we can perform some out-of-pile tests, and it is figured that the power burst facility test program will continue to be very closely coordinated with the ESSOR SUPERSARA program.

DR. OKRENT: Let me turn the thought around. See, what I'm trying to get at is if one tries to think about what it is might happen in a big reactor and what way one might be trying to use the presence of debris beds and so forth, or measurements of this kind, it seems to me you have a very different situation than trying to do an experiment and make some measurements across that.

It is not clear to me that there is likely to be any major value to this particular set of measurements. If somebody can point it out to me, I would be interested in hearing it. But the fact that you are able to make these measurements doesn't of itself make them of large value. Each measurement sort of -- I suppose it's hundreds of thousands of dollars, or whatever, we're talking. Not that you shouldn't make the measurement if you do the experiment. But this is purported to be one of the things that will come out of it, as if it is going to be an important contribution in some way. And I'm a little skeptical.

DR. VAN HOUTEN: Well, first of all, the idea of determining the channel paths you might have providing analyses of these seems like it would make perhaps not two or three, but half a dozen excellent Ph.D. theses for studies, and then determining a measure for establishing equivalent pressure drop behaviors.

But we will have some in-pile data in which each of the characteristics of the debris bed is changed, by change in the flow rate, you have a chance to increase the flow rate, then drop it back a little bit, and determine what at the then again lower flow rates any changes in delta T and delta P associated. And so I believe you may have more of a chance here, than perhaps as the information is presented in a very preliminary fashion by me is apparent.

(Slide.)

The next several viewgraphs merely give an indication of the analyses which were performed at PNL to determine whether the test loop as designed could indeed perform the wanted test. An indication of the sequence of events, the controlling -- the control parameter, and the other parameters which would be monitored.

This was done for each of the four classes of deformation and loading and heating.

(Slide.)

(Slide.)

And an indication of the parameters which would be examined in the course of the experiment, pressure, temperature, flow rate, power in particular.

(Slide.)

The next graph is listing of the measurement in the instrumentation, and since there is close coordination between the power burst facility and the ESSOR, all the items which we listed in specific instruments for the PBF test have been compared with what was proposed for the ESSOR test series, but even so --

(Slide.)

-- let me show you a bundle location for one of the three sets of tests which could be performed in the ESSOR, indication of where the liquid level probes, where the hydrogen probes, the pressure sensors, the steam probes, the SPNDs and the thermocouples might be located.

That's an expanded view showing the elevation and the position within the array. That's for the small break test, and the -- as you can see, it's changing. We weren't sure we could get them to agree to a 2300 degree K test when this was done. There's very good news that came out of the June meeting. This graph was prepared prior to the meeting.

(Slide.)

This is a test to examine where the instrumentation would be placed in the initial check-out test. The major interest was the extent of ballooning and associated co-planar blockage, if any.

(Slide.)

And similar placement in the separate large ballooning LOCA test bundles, in which placement for thermal hydraulics reasons would be somewhat different.

(Slide.)

The next item in the handout identification of some of the specific characteristics of the instrumentation, in effect we have done a design study, at least well beyond the conceptual stages.

(Slide.)

And, in fact, to give you an indication of what we have been through, the next three or four pages list the accomplishments in the area of thermal hydraulics through the determination of associated steaming rates, temperature profiles within the bundle and shroud in a similar manner as to that shown by Brent Buescher in his presentation for the power burst facility. And we used a somewhat different set of codes to establish the temperature profiles. The idea both to provide confidence in the codes as we were developing them.

(Slide.)

And to provide the needed design data.

So, accordingly, we identified the assembly powers required. One of the major ideas here that we had proposed to them, and it required a change in the licensing practice, was that all these tests be run with fission heat to simulate decay heat. Because then if the reaction should move beyond the planned control range, the reactor power can be cut back, and the additional water can be introduced, and accordingly the test has a better chance of being terminated successfully in a very short time.

Now in the test train concepts, we evaluated the various configurations, materials which might stand at watted heat transfer coefficients.

(Slide.)

Watted insulation resistance, and accordingly we developed a test train concept that was, we believe, capable of performing the full spectrum of the SUPERSARA test, and it would employ a common shroud and a common bundle design with changeable instrumentation packages, and further identified what kind of a test train concept would be compatible with the power burst facility program.

Then in examining the instrumentation, we determined what we believe the fundamental needs would be, what new measurement concepts would be useful. These fall particularly on the question of liquid level.

(Slide.)

And the matter in instrumentation experiment control, the methods for controlling the large break test, we wanted to make sure that the loop could perform these, and that we had an excellent chance of being able to perform all the tests within the envelop of the small break program.

(Slide.)

What we came up was a design in which the end elements, the general modular design, would be applicable both to the power burst facility and to the ESSOR SUPERSARA, and the virtue of this is that it gives us an opportunity to test it out on the power burst facility, so that the probability of success in SUPERSARA will be -- success on the first trial will be much greater.

(Slide.)

The last slide then shows the necessary steps to continue this progress. We will issue a report on all the accomplishments to date, the test train concepts, the thermal analyses, and very important, we prepare a project quality plan, develop a general quality assurance and qualification procedure, and then when we fabricate the components, make sure that we do follow these plans and provide the European community with the same set of information, so that all of the test trains and instrumentation assemblies will be qualified to identical standards.

The U.S. contribution is not one just of designing some hardware. We expect that we will assist them in the licensing of test by test, as well as in the licensing of the new loop and the operation of the facility, and to this intent, we sent them a licensing specialist who spent two weeks there last fall.

We will provide them with appropriate computer tapes, so that they can independently analyze and verify the data we provide them, so that where special studies are performed by a task team of experts, largely within the United States, that they can examine information to sufficient depth that it provides credibility.

We will do the pre-test predictions. We will have a site rep who can help them prepare for the test, and help them plan and execute the post-test analyses which, as Dr. Okrent says, with these debris beds, the later phase of the actual testing and the subsequent analyses are not simple.

DR. SHEWMON: Can we go back to your first viewgraph?

DR. VAN HOUTEN: Certainly.

(Slide.)

DR. SHEWMON: I'm not real strong on English grammar, but I think it's called subjunctive, about what we will do in the future and what we will do, and you are speaking positively as a good advocate should, and using only the future instead of its subjunctive. But where the subjunctive might be more applicable. Would you point out on that and tell me what is being done by whom in this country at this time? And then what you are talking about, what you would like to see done in the future in this country?

DR. VAN HOUTEN: Yes. Well, as I prefaced, the European community is unblocking three full years of funding, so that is in the positive, and the 10 country agreement on the test program is effectively to be written up in October, and then voted on in November for release of the monies in December.

Now the U.S. has -- effectively the Fuel Behavior Research Branch -- has supported this program through a program letter to Pacific Northwest Laboratories to examine facilities, develop multiple module, multiple rod hardware concepts, with modules that would be applicable to a number of facilities.

Many of the elements of this work preceded and led to the development -- the development of the specific design for the NRU test train, and supporting instrumentation. Within this program letter we ask that they do the loop analyses to determine the feasibility of the test, because in this meeting, which Ed and Jim Broughton of EG&G and I attended a a year ago in June, we pointed out a series of, at that time, 10 large break LOCA tests emphasizing the extreme ballooning through a temperature range 1000 to --

DR. SHEWMON: This is all interesting, but tell me, if we go to 1980, which is, I think, the year we're in, give or take what the government talks about, what is going on where out of those black bars?

DR. VAN HOUTEN: All right.

DR. SHEWMON: Any of it? U.S. only.

DR. VAN HOUTEN: U.S. only? As of October 1st, PNL will have used up all of its money on the multi-rod modular test train concept and coordination which has led into this program. But we have a program --

DR. SHEWMON: That's where, that's the black line under program verification?

DR. VAN HOUTEN: This represents the entire European program. This does not represent the U.S.

DR. SHEWMON: That must be part of it.

DR. VAN HOUTEN: The U.S. sent representatives to the meetings effectively to determine whether the program continues to develop in a fashion which is supportive of the other severe core test damage studies which have developed.

DR. SHEWMON: PNL has done some paper studies and some design studies on the loop and the instrumentation, and that also is some place between equipment and program bu8

verification, I take it?

DR. VAN HOUTEN: Yes. It has had a very limited introduction to the test planning, and we are not taking any credit for the efforts on test train development which have been performed to date, and specifically in that direction, then, let me take a moment to show you some of these.

DR. SHEWMON: Now EG&G involvement in this thing is only involving PNL, or primarily that?

DR. VAN HOUTEN: EG&G, under subcontract, has provided special services, for example.

DR. SHEWMON: Subcontract to whom?

DR. VAN HOUTEN: To PNL through our program letter, in providing codes ıch as FRAPT-5 from the CDC 7600 format to the IBM 370 format, which is used by the joint research center.

So, accordingly, we have provided them with support tapes for the FRAPT-5. We have provided them with consulting services on the safety qualification of the facilities.

DR. SHEWMON: But any power, what PNL had, from what Buescher talked about, was when they took Buescher out and bought him a beer at a meeting, or what sort of an action has there been?

MR. MAC DONALD: EG&G people have been involved in supporting the plan along with PNL for a number of years. Myself and my staff have accompanied Bob to Europe a number of times to help the Europeans plan this program and develop this program, and that has continued actually ove r a number of years.

DR. VAN HOUTEN: And specifically we have had joint efforts on the design of test trains by PNL for use in the power burst facility which has led to a series of meetings on design reviews, qualification procedures. It's led the hardware from the RIA through the OPTRAN, and it has led to a series of -- I guess there have been perhaps six visits by EG&G to PNL, and an equal number by PNL back to EG&G, including some joint conferences with European representatives.

DR. SHEWMON: All right. Let me change the subject.

Pic, is your acting branch chief of Research today here?

Will we see any budget discussion, since in effect we are advising on budgets? I don't see anything that looks like that on the rest of the agenda as laid out in front of me.

What I am looking for here is sort of who is doing what with whom for what, and I grant that the Lord hasn't revealed to us just what the budget is going to be for a lot of this. But do you have anything on those words? Or maybe if we could interweave a little bit of that in here, I think it would supplement the plans that he is getting into.

DR. PICKLESIMER: I won't have anything specifically labeled NRU or SUPERSARA, but in the budget figures I have this morning on the severe core damage part, NRU, SUPERSARA, and severe fuel damage for PBF are incorporated in that figure.

DR. SHEWMON: Okay. Fine.

DR. VAN HOUTEN: Effectively everything which has been spent for SUPERSARA to date has had an application to the PBF severe core damage test program.

DR. SHEWMON: And/or the NRU program.

DR. VAN HOUTEN: Yes.

DR. SHEWMON: Okay. Thank you. Go ahead.

Have you finished up with your first presentation?

DR. VAN HOUTEN: Not quite.

DR. SHEWMON: Okay.

DR. VAN HOUTEN: The last two pages on the handout are a condensed summary of the objectives of the program. A description of the experimental facilities as prepared for the CSNI. It is compact. If the rest of the handout is too bulky, I urge that you keep those two sheets as a brief summary, not so detailed, I am sure, as Dr. Okrent would like, as to what is to be done. But since this is a European community experiment, it is hoped the U.S. will be allowed to sign the contracts and participate, because -- it is the bargain of the year on cost. This is 15 millions a year in PBF, plus the very small amount here. And also I believe the Japanese may well be joining the program in the near future.

So those two sheets will provide you a summary of the U.S. view of what the program is and some of the elements of the U.S. support.

Now with this matter of rulemaking and how the information might be used, it seems almost certain that there will be rulemaking on the matter of degraded core cooling, and this rulemaking is very, very likely to establish some criteria, and if they do, we will probably have some provisions for cooling a severely damaged core.

Now I would like to point out to you that if this is so, where, if not for the test in ESSOR as possible -- as a consequence and in conjunction with the tests in power burst facility, would you be able to get an essential block of data that might help you establish the confidence in which the criteria could be applied at full scale?

If there is such a rulemaking hearing, let me suggest some of the words that may be in it, recognizing that there can never be complete assurance that only analyzed events as delineated in a safety analysis report will occur, what additional analysis procedures or design features would you propose to mitigate fuel damage accidents in the range from clad performation without oxidation through a fuel percent clad oxidation through extensive clad oxidation, to full core meltdown? What would you recommend different? And perhaps several of the design features, depending on the severity of core damage to be coped with, and one other element that may appear in any such rulemaking hearing discussion, what aspects of degraded coolant or melted core accidents are sufficiently unknown or uncertain so as to impede mitigating system design and analysis, and thus require additional research or experimentation?

Now if these rulemaking hearings occur in the very near future, as they very well may, even with the program we have, the work will be coming as and after the rulemaking, but we are only eight years behind the ECCS rulemaking hearings and finishing up our work on LOCA.

DR. SHEWMON: Tell me when we will finish that next time.

DR. VAN HOUTEN: That encompasses my part,unless you would like Mr. Courtwright to address some of the ways that the instrumentation might be used to follow.

DR. SHEWMON: I think not for now. What about the NRU LOCA test?

DR. VAN HOUTEN: Give me just a moment to regroup.

DR. SHEWMON: I look forward to the report. I think the report would be a better way to compare systems than a presentation for that sort of thing. This is in regard to Mr. Courtwright, or whatever the name is.

T.7

DR. OKRENT: If I can pass a question for you to reflect on and perhaps give me an answer by the end of the year, together with the other question that I hope you will give me an answer to by the end of the year:

Suppose the Red brigade blew up the ESSOR reactor or suppose a volcano erupted under PBF and you didn't have either of these programs. What vital information would be missed that we would have to get elsewhere?

I don't want the answer now, but if you can tell me by the end of the year, you know.

In fact, if you could define things that fall in that category and make a convincing case, you have won. If you can even define things that have fallen in that category, you have probably won. But if it doesn't get in there, then we are in the gray area, which is what I am talking about.

DR. SHEWMON: You know, one of the things they are selling is the fact that you may not have to make such very conservative procedures if you have some knowledge as distinct from what you have to do. If you write an Appendix K segment and say, you know, it is very nice if they can answer those questions, but I'm not sure it damns their program if they aren't so precise as to be able to answer even half of them.

Let's get on with the NRU.

DR. VAN HOUTEN: Before I do, let me show you what a shroud might look like (indicating) that's been tested.

DR. SHEWMON: A shroud for what?

DR. VAN HOUTEN: A severe core damage test. This is a zirconium,cloth-wrapped zirconium shroud with excellent insulation characteristics, and I believe it represents an approach which will work in both the PBF and the ESSOR testing.

Now you have, in addition to a handout which looks like very much the ESSOR handout, a nice slick looking brochure which was very well prepared by Mr. Chuck Mohr.

DR. SHEWMON: As I understood, you said we have something that looks like this, but isn't the ESSOR?

DR. VAN HOUTEN: It says NRU. One says ESSOR and one says NRU. They each have plots.

DR. SHEWMON: No.

DR. OKRENT: No.

VOICE: There may have been some missed in passing out the handouts. I never did get an NRU, but here is an ESSOR one.

DR. SHEWMON: Okay, we've got some paper we haven't seen yet.

DR. VAN HOUTEN: That's good news. I think, just to be different, I will start with the second slide in the handout.

(Slide.)

Part of the objectives of the LOCA simulation in NRU. Here we are some years after the ECCS rulemaking hearings. We performed bundle tests to establish the turn-around and quench characteristics of bundles. We performed FLECHT tests. We performed multi-rod burst tests with zircaloy clad fuel rod bundles. The FLECHT tests had stainless steel clad, they were nominally thermal hydraulic. This is a chance to make the point that thermal hydraulics and fuel behavior cannot be divorced by reason of either clad deformation, heat transfer across the pellet to clad gap, and accordingly verification of these separate effects out of pile test is necessary.

To date, there has not been an in-pile verification of the FLECHT test. There has not been an in-pile verification of t[illegible]i-rod burst test or the REBEKA test, and with this definition objective to retain our well characterized data set for full length multi-rod bundles under representative heat up in reflood conditions --

DR. OKRENT: If I can give you an example of another thing that hasn't been done, or had not been done prior to July 1, despite a $200 million a year safety research program. It wasn't a good look at the scram discharge systems for BWRs. Now, one can try to ask oneself are we properly oriented in our entire research effort?

In other words, is there perhaps too much in characterizing LOCA and so forth, and maybe too much in some of the fuel areas, compared to some other things? You may not ask yourself that question, but I do to myself, and I think it is partly what is in back of my mind. But I tried to find out in fact are you really going to make an important contribution, if not only within the fuels area, but as compared to other things?

DR. VAN HOUTEN: The question of a proper product mix is indeed a challenge, as with Chrysler and Ford Motor Company.

DR. OKRENT: I think the NRC research program should face the same problem they are facing belatedly.

DR. VAN HOUTEN: I hope you will put us in the category of General Motors in that listing. But with a reference to fuel behavior, because that is where we're at, that is what we are being paid for, as Dr. Picklesimer mentioned, and the other speakers this morning, if you are to have a release of fission products and if you are to know what the release is at any given time during the course of an accident, and since as the accident progresses, the channels of release of these source term fission products beyond the final containment may vary by reason of the condition of the containment at various times and/or the through passages, you have to know what is your damage threshold, what conditions will induce it, what can be done to minimize an acceleration in the rate of release at a particular time during the course of an accident.

You effectively do need to know what is the kind of damage sequence which may reasonably be expected in the associated fission product release.

I didn't mention it, but if we have time this afternoon, I do want Mr. Osetek to tell you a little about the -- he has got a four-minute talk on the fission product detection system as it can be applied to just this topic. It is on the power burst system, and we hope we will be -- if we are allowed to progress on the ESSOR SUPERSARA test program.

In order to then give us a handle on the probabilistics analysis or other techniques that are used to determine the risk by reason of probability and consequences and reasonable projections as to the accidents which might be encountered.

So, you see, each must be done, but this is an important building block, given the remaining million or two of the 13-1/2 millions which are total for this program, we'll be able to give you 90 percent of the data instead of 10 or 20 percent.

(Slide.)

On the matter of schedule, the equipment has been purchased and effectively all of the loop equipment has been received. September is the month of welding and final check-out. I guess the term "beehive activity" at the NRU loop in Canada is a good term in this case. They are exceedingly busy.

DR. SHEWMON: How about the preparation of paper and checking of ASME codes and that sort of thing? The last thing I saw on this was the keepers of the reactor up there after TMI-2 wanted to make sure that this met the Section 3 of the ASME pressure vessel code, and also you were having budget problems which I guess we could have-- or budget control problems.

DR. VAN HOUTEN: Yes, a cash flow problem by reason of the combined escalation and capital equipment costs, and additional instrumentation to respond to an upgrading of -- or revision of the safety practices.

DR. SHEWMON: It's nice you have a lot of welders working. My concern is are they going to say you can't put it in even if you've got enough welders there?

DR. VAN HOUTEN: We worried about that, and Mr. Mohr has been very busy making sure that he had passed all of the safety committees

Chuck, would you like to speak to that?

MR. MOHR: Our preliminary -- what we have had approximately a year of negotiations, the safety analysis report for that, and for these experiments. Our formal submittal went to their ENSEC committee in April. They have had a subsequent response to those questions. The final submittal to them will be on the 26th of August. So it is just in the next few days. The response from ENSEC has been quite favorable with the loop modifications that we have put in place. We feel that everything should be well under control.

DR. SHEWMON: All right. Thank you. Go ahead.

DR. VAN HOUTEN: Thanks, Chuck.

The nuclear commissioning is scheduled for late September and we are on schedule on this program.

Chuck, with respect to shipping the hardware, is the test train going out the front door this week or next?

MR. MOHR: We are going to be delayed. We are going to be shipping on the 11th.

DR. VAN HOUTEN: Oh, oh, bad news. We are 10 days behind.

Now cash flow is this year a problem. We had requested blocks of money at varying levels, because we anticipated that once test trains were fabricated, until we get into the post-test analyses, we would not need large sums of monies.

The Canadians have asked from their share of our contingency funds an appreciable additional outlay this year in order to meet their commitment to get the loop operating this month; and further, we had $400,000 taken from our program for other urgent items last year, and again this year.

So the fact that they have $800,000 less than they had anticipated having at this time, and further that they are in a low cycle this year, we do have a question as to whether or not we can maintain the very optimistic schedule of 18 thermal hydraulic tests at peak clad temperatures of 1400 degrees F through 1800 degrees F in October, to be followed at three-month intervals by rod deformation tests with pressurized bundles, to provide the in-pile verification of the codes for licensing and of the out-of-pile separate effects test.

If we find that we cannot come up with some other monies, we will have about an eight to 10-month stretch-out.

(Slide.)

The next page of the handout just lists some of the items in 10 CFR in which we will have data to add to the data base to determine how satisfactory our existing answers are, in each of these areas, from peak cladding temperature to associated maximum cladding oxidation. Have we predicted it correctly from the other tests? Do we maintain coolable geometry? Are there some surprises insofar as co-planar behavior in this commercial enrichment nuclear test? And so on with the rest of the listing.

(Slide.)

Our review of the general format of the tests, the rods will be heated up in steam with rapidly flowing steam. The steam will then be brought to stagnation at low reactor power levels. The rod temperature will start to climb at the planned rates and then at an appropriate interval thereafter reflood will be initiated at rates of -- depending on the test -- 1-1/4 to 5 and 10 inches per second, to produce the turn-around and the quench in the rods to compare with the separate effects out-of-pile test data.

(Slide.)

General form 18 of these hydraulic tests with unpressurized rods, then five on cladding performance.

(Slide.)

This figure generally is indicated in the next viewgraph, but we expect to use a single test train. We will proceed from the low temperature tests, which have the least possibility of damaging or developing an incipient flaw,proceed through the severe high temperature tests which are nominally peaking at 1800 with a cut-off ceiling of 1900 degrees F, will verify the relationship between the predicted behavior and the actual.

(Slide.)

The next few viewgraphs are just an indication of what the expected sequence insofar as the flooding rates, reflood delay times, the anticipated cladding temperature, peaks, turn-around, and the required powers.

(Slide.)

The second sheet in that group is effectively the same information, but the footnotes may be of interest to you insofar as detailing planned rod powers, outlet pressures, for technical specialists to satisfy themselves as to the validity of the test.

(Slide.)

The general map of the reflood rate, anticipated temperatures, associated delay time is shown in the figure on the next page.

(Slide.)

Now we listed the series of 10 CFR --

DR. OKRENT: Excuse me. Are reflood rates less than one inch per second, not possible, or what?

DR. VAN HOUTEN: They are extremely difficult, and we worked -- Chuck, what is it, about .95 inches per second, looks like the lowest right now?

MR. MOHR: Something like that. We are limited on the amount of available water that we have in the reflood loop. It is a separate pressurization system, a very high pressurization system, and it has to be separate from the loop piping, and as such, with the entrainment and carry-over, we cannot quench it, on these real low loops.

DR. VAN HOUTEN: We realize that this question of must you assume steam cooling when you have a reflood rate below one inch is a very important question, but at this point we cannot provide an answer on that one alone from this test data.

MR. MOHR: If we did, if we decided that we needed to go into that, what we would have to do is add -- there has to be a slight modification to the piping system. So conceivably we could, but when we initially started out on this, we thought that one inch would probably be satisfactory, and so it is possible, but not right now.

DR. VAN HOUTEN: The remaining sheets give you some indication of the topics addressed, peak clad, temperature production, how well have the reflood quench characteristics of oxidized zircaloy rods in pile been predicted by out-of-pile tests?

I would like to move just a little bit to the design that is going to be used on that, move back to those slides, project design features.

(Slide.)

(Slide.)

It is a 36 rod test train with a fuel length of approximately 10 feet. We are using a split shroud concept. The grid spacers are effectively commercial style. The array is that of a 17 x 17. The fuel enrichment is 3.1 percent, and the full description and table within the green covered document covers Chuck's presentation to the European -- to the Germans and Japanese in June, and it -- for your archives -- is the document that I would suggest you refer to, because it has all of the things I have in my handout, and a few more, if you have time for them.

(Slide.)

An indication of what the axial spacing is.

(Slide.)

Where the instruments are placed. This is level 1-9.

(Slide.)

And then levels 10-18 as shown in the preceding slide.

And I believe Chuck has an expanded drawing somewhere to that, which we should view for the SUPERSARA and power burst facility, since we made up similar drawings for each of the programs.

(Slide.)

And the final drawing and indication of how the test train will be transferred from the reactor to the canal, where we will make measurements on the center 12 rod cruciform assembly. We will do rod profiles, and we plan to be able to transfer that center assembly into a small shroud in which we then make first test pressure drop measurements of the ballooned bundle assemblies.

DR. SHEWMON: You say you will be able to do that with the equipment you have?

DR. VAN HOUTEN: This is one of the reasons for potential program stretch-out. We have just -- we have a cash flow problem in building the shroud and closed loop pump.

DR. SHEWMON: To get at the subassembly. We have said it is 12 foot long. There is at least one instrument tube in the bundle which one could then say is equal to the control rod guide tube and the density of spacer grids across this is comparable to a commercial subassembly. Is that right?

MR. MOHR: The Babcock & Wilcox design. In fact, it is a Babcock & Wilcox grid spacer and cladding.

DR. SHEWMON: Okay. So insofar as we know, then, this should do a good job of simulating the kinds or at least many of the kinds of asymmetries that one has in a commercial subassembly?

MR. MOHR: I think as much as possible.

DR. SHEWMON: Fine. Thank you.

DR. VAN HOUTEN: That completes my presentation.

DR. SHEWMON: Okay.

DR. VAN HOUTEN: Do we have time to listen to Mr. Osetek at this point?

DR. SHEWMON: Okay. How many minutes?

DR. VAN HOUTEN: On his dry run, he did six, but in his presentation, I hope you will ask him enough questions to make it 15.

(Slide.)

MR. OSETEK: I appreciate the committee's time. I would like to just briefly describe some of the recent results from the fission product detection system which we are using at PBF.

My name is Dan Osetek. I am currently Project Leader for Fission Project Studies at PBF.

And the system which we have been developing over the past three years has proved quite encouraging at PBF, and we feel that offers some benefits if applied at ESSOR.

(Slide.)

DR. SHEWMON: There was a comment earlier that there is a utility sponsor and an EPRI sponsor, a commercial comparable or similar unit on PBF now, or soon?

MR. OSETEK: There was a Babcock & Wilcox instrument in PBF for about a three-month period, when we did comparative studies. That was a commercial instrument and it proved to have some shortcomings by comparison with the advanced design we are using in PBF.

DR. SHEWMON: Go ahead.

MR. OSETEK: Let me first of all describe the project objectives: to provide test support to PBF in terms of identification of fuel failures and the time of the fuel failure, and then to document fission product behavior. This is in terms of fission product release signatures for the given tests that we are conducting, and finally to compare these signatures with the fuel behavior parameters and fuel condition to contribute to the data base which we feel is important in several areas which I have listed on this slide; that is to assess the consequences of various accidents, accidents which may be similar to the type we are testing at PBF, as well as other accidents, which we may be able to define source terms by extrapolation of our data for code assessment, in helping us to develop and assess fission product behavior codes.

And finally for fuel condition, we have been encouraged by some of our results that comparison of fission products with particular conditions we observe in the test,there may be of some possibility of doing on-line fuel condition working in a power plant.

DR. OKRENT: I'm sorry, what aspect of fuel condition are you referring to in a power plant?

(Slide.)

MR. OSETEK: Perhaps, first of all, cladding rupture, whether it occurs, and whether or not there has been some fuel melting, fuel powdering, what perhaps fraction of the core might have been involved in a fuel damage accident.

This is a schematic diagram of the PBF test lab with the fission product detection system sample line shown in red. Any fission products which are released during the test, in the test train, will be entrained in the coolant loop and therefore, by sampling a portion of that coolant and monitoring it with our instrumentation, we can keep track of time-dependent change in fission product concentrations.

(Slide.)

Now the instrument itself consists of some gross monitors and a gamma spectrometer. The gross monitors provide instrumentation to detect gross gamma and delayed neutron activity in the sample, and that provides real time information in the control room. The spectrometer is linked with a separate data acquisition system. It does have some on-line analysis capabilities, but most of the data reduction is done post-test, because in the PBF experience, we are interested in rapid data acquisition as opposed to rapid processing. We want to be able to measure the changes in fission product concentration which may occur in a very short period of time.

DR. SHEWMON: On that diagram, where is the reactor, or is that loop at the left a pipe in the wall in the plant, or what?

MR. OSETEK: Yes, the sample line is shown in red. This represents the red sample line in the previous slide, which was a sample of the loop coolant.

DR. SHEWMON: Okay. Thank you.

(Slide.)

MR. OSETEK: Now what the instrumentation provides us, we call fission product release signature. This is in the form of the release fraction histories or time-dependent release fractions of the various isotopes which are short-lived.

Noble gases, cesiums and iodines.

We are also currently in the research stage of identifying some of the other important fission product species, such as tullariums and some other refractory elements.

(Slide.)

Now to get an idea of how this benefits reactor safety analysis, we are currently doing a comparison of five fuel behavior tests and fission product release signatures from those tests.

I have listed here some of the important fuel behavior parameters from these five tests, starting with fuel temperature, which ranged from 3000 to 3500 Kelvin; the fuel lost percentage, this is in terms of the particulate material which left the test train, was entrained in the coolant stream, and therefore available to produce a fission product signature. This we can see ranged from less than 1 percent in some cases up to 27 percent, in another case. And finally, the fuel melt volumes, which in some tests we saw no fuel melting, but in other tests we saw extreme cases of up to 90 percent fuel melt.

(Slide.)

Now if we look at a comparison of some of these signatures, we see, first of all, a distinct difference in the test in which there was a 90 percent fuel melt, a very large fraction of barium-141 appeared early after the rod failed, as compared to very small amounts in other tests.

Now, not only is the magnitude of the fission product release important --

(Slide.)

-- but so is the timing of the release as shown on the next slide, which compares the time to feach peak concentration of these same five isotopes for the five different tests, and the outstanding features here are the krypton-87 and the cesium-138 isotopes are much longer lived in their source term than are the barium and iodine isotopes shown on the slide.

(Slide.)

Now one last comparison I wanted to offer is what we found to be an unusual behavior of iodine, as compared to the noble gas releases in two pairs of tests, which included fuel melt and no fuel melt.

We notice that in the RIA scoping tests, these are reactivity initiated accident tests for the scoping experiments, no fuel melt occurred in two of these tests; and yet we saw large fractions of iodine released by comparison to the noble gas release fractions. In the fuel melt test we saw 24 percent in PCM 1, and 90 percent in scoping tests, we saw very small fractions of the iodine released by comparison to the noble gas.

We think this may be very important in the severe fuel damage test when we start looking at fuel melt, and just what kind of iodine releases might be expected.

This, of course. is a key safety issue, just how the iodine behaves in an accident.

(Slide.)

DR. OKRENT: Excuse me. Are you going to tell us why that occurred?

(Laughter.)

MR. OSETEK: I cannot be definite. We have some postulated theories that the higher temperatures which occurred in the different coolant conditions which exist around the fuel rod during these high temperature tests may produce certain iodine deposition characteristics, reactions that prevent it from being transported in the coolant, as opposed to the non-melt fuel, where temperatures and conditions are different.

MR. MAC DONALD: We also saw significant differences in the pattern, the fuel and the movement of the pattern, in the cases where you get the melt, and you don't get the melt, but you break up the rods.

DR. SHEWMON: Do you have any idea how much of the iodine you are counting as present is cesium iodine? Is it distinct from an iodine alone?

MR. OSETEK: It's in solution, so we believe it's just iodine ions in solution. If it originated as cesium iodine, that probably is dissolved in the coolant.

DR. OKRENT: Was there more or less iodine -- could I have that viewgraph back, please?

(Slide.)

You have given us relative values, the absolute amount of iodine to the fuel melt case. You said less?

MR. OSETEK: I don't believe so, but we are not able to quantify the absolute fractions in these tests. We will be doing that in future tests, and we are reducing data now on some tests where we will have the absolute number.

DR. SHEWMON: What you show there is counts per second or integrated counts?

MR. OSETEK: No, this is the average of several release fractions which were calculated, as I described previously. These are the maximum release fractions we were able to measure.

DR. SHEWMON: Since you say you can't answerhis question, it seems to me what you are telling me is the only thing you have an answer to his question. What is unity, then, when you say release fraction?

MR. OSETEK: The largest fraction that was observed is unity.

(Slide.)

We have unitized the largest fraction which in the PCM-1 case would have been krypton-85. Therefore, these represent maximum upper limits for the release of fractions.

MR. MAC DONALD: Dr. Shewmon, the information we are giving you now is relative release rates. We are in the process of modifying our system so that we can inject a known quantity in and calibrate -- better calibrate the system and then give you absolute numbers.

DR. SHEWMON: You are saying you have there should have been relative release rates instead of release fractions?

MR. MAC DONALD: Relative to whichever isotope gave you the largest release.

DR. SHEWMON: So this is a time average relative to the maximum; is that what release fraction means?

MR. OSETEK: Yes. Release fraction is the amount that was released from the rod divided by the total amount that was available in the rod. Now because we don't know the absolute value of the concentration in the coolant, we are forced to divide all of our release fractions by the largest fraction observed. We then call them normalized release fractions.

DR. SHEWMON: So all you say really say is that the proportion of iodine and noble gas came out in the two cases?

MR. OSETEK: That's right. We're talking about ratios here and proportions.

DR. SHEWMON: Okay.

MR. MAC DONALD: But we will in the very near future be giving you absolute numbers in this area, to improve on the significant number.

(Slide.)

MR. OSETEK: And when that is accomplished, we feel we will have demonstrated that fission product release signatures are fuel-behavior dependent and measurable, using specialized monitoring techniques.

The fission product release measurements from the PBF test provide contributions to an important data base which can be used for estimating accident source terms,for evaluating safety margins in existing regulatory guides and federal regulations, for preparing safety analysis reports and new regulatory guides, for assessing fission product behavior codes, and finally for on-line fuel condition monitoring in power plants.

DR. OKRENT: Well, that's a fairly ambitious hope, but have you looked at what other -- and if and how some of the isotopes are system-dependent in a different way than others?

MR. OSETEK: Yes.

DR. OKRENT: Is that -- how is that factored into this?

MR. OSETEK: I haven't had time to explain all of the measurements we are capable of making, but with some control over the PBF, we can observe changes in these concentrations as a function of the temperature and pressure of the loop and, in fact, a report that will be coming out in a month or so describes how the iodine is actually at a level of release fraction around 24 percent, and when temperature and pressure of the loop is changed, it has dropped to a level of 2 to 3 percent. So there was either a significant definition or a play-out which occurred as these temperatures changed.

DR. OKRENT: Well, if that is the case in your loop, it may also be the case in some more complicated way or different way in some other system, and if that is handle-able in some generic way, then maybe the methodology has some applicability generically. But unless somebody has a way of assessing what I will call loop effects, all of the loop effects, then it puts somewhat of a limit on the generic usefulness of what you are doing.

MR. OSETEK: I think we will try to attempt to relate the measurements to temperature pressure, flow rate, and key loop parameters that are not unique to PBF.

DR. OKRENT: There may be some chemistry questions, I don't know.

DR. SHEWMON: Actually the amount of iodine in the water at TMI-2 or something showed some anomalous or dropped-off relative to cesium, it seems to me I heard a rumor.

Let me ask more of an instrumentation question, since I feel that's more my specialty than physical chemistry:

How does this system compare with what is currently on operating power reactors? And have you looked at something called Reg Guide 1.97, instrumentation to follow the course of an accident, to know how your system would compare with what is asked for there?

MR. OSETEK: First of all, currently on most power plants, there is only gross gamma monitors, there is no gamma spectroscopic equipment.

DR. SHEWMON: Is that gross on the primary letdown stream from outgassing?

MR. OSETEK: It's usually both. The utility is usually interested in monitoring both the primary coolant and the letdown, and some offgas monitors.

However, the interesting point is in the spectroscopic information that's available and the short-lived fission products which may be released from the fuel rod offer on an order of magnitude more information than a simple gross detector offers.

Another point that has to be brought out is the difference in what we are doing at PBF and what is currently available commercially. This equipment was essentially designed for measuring fission products in near-accident conditions, and you cannot currently buy something like that off the shelf from a commercial vendor.

I have talked to some, and they are working on such designs, and the principal difference lies in the fast electronics and the shielding, the columnator between the detector and the sample line.

DR. SHEWMON: There has been some suggestion maybe what the NRC is asking for, and the reg guide isn't on the market, and that's a point of some discussion --

MR. OSETEK: But I think it could be designed and developed since we have demonstrated at least at PBF it could be done.

MR. MATHIS: With respect to graphic analysis, there, how sensitive is that? Can you detect a minor crack or leak in a fuel rod?

MR. OSETEK: It depends on background conditions. If we have clean coolant to start with, we can measure very small increases which might be indicative of a pinhole leak in the cladding well, evidence of a fuel rod failure has occurred, and there is a large concentration of fission products in the coolant, then you need a substantial change in that level to make a significant decision about additional failures.

However, again, the presence of short-lived fission products and their behavior is going to be key, I feel, to characterizing fuel damage, whether there is just a crack in the cladding, or whether we actually have fuel particulate floating around.

DR. OKRENT: Is the major purpose for this particular program to help the PBF program, or does it have some different primary purpose?

MR. OSETEK: The primary purpose, I feel, is to help characterize fission product source terms from these tests, and then hopefully expand on this to define source terms for an accident of that type. If we can document the fission product releases, then it is available to the NRC for safety analysis.

DR. OKRENT: If I understood your answer, it is not primarily to help analyze the PBF tests. It is for some application beyond that?

MR. OSETEK: Well, if you're going to separate fuel behavior from fission product behavior, I agree that it has some different objectives.

DR. PICKLESIMER: Let me make a partial answer here. One part of the Fuel Behavior Branch responsibility has been to examine the release of fission products from fuel product source terms. This is an excellent area to get such information. It is an add-on to the PBF program rather than a primary thing to the PBF program. It takes up another part of our responsibility.

DR. VAN HOUTEN: And resources.

DR. PICKLESIMER: And resources.

MR. MAC DONALD: It's certainly key to running some of our tests to know where the rods fail and how badly they fail.

DR. PICKLESIMER: As it turns out in the severe fuel damage studies, one of the responsibilities we have in RSR, not just fuel behavior, is in accident mitigation and moderation during the progress of the accident. One of the things we hope to come from this is a way of getting accident signatures so that operators may know what is going on in that reactor. In TMI-2, there were at least a dozen times in the first four hours of that where if the operators had had such kinds of instrumentation, they might have been able to manage the accident better, if they had known what was going on.

DR. VAN HOUTEN: Let me say also that we have a simpler fission product detection system designed by this group and put on the near steady state test in the Halden reactor. So this is not -- our interest in fission product detection system, as Pic said, is not just limited to monitoring a PBF experiment.

DR. SHEWMON: And that does have spectroscopic capabilities?

DR. VAN HOUTEN: Yes.

MR. JOHNSTON: The installation of this particular detector was a part of the original PBF program plans, that had lingered over several years, but then the interest on the part of the ACRS and the ability to follow the course of accidents, which is a generic item of a few years ago, prompted a study that was made in Licensing as to the detectability of BWRs and PWRs and what their ability to detect fission products on line might consist of. It basically prompted the equivalent of a user's need in that time period to get such a device going in the PBF, so we could see where we stood. At that time there was a lot of discussion with both Westinghouse and B&W on that, because Westinghouse was moving to a somewhat similar system for their plants, and B&W was doing it also. That's why the B&W

system also ended up being put on the PBF, to see how they did. The Westinghouse arrangement was never completed to give their system on PBF, but I think Westinghouse feels at least that they have a system presently available to their plants that will give them a pretty good indication of how many fuel rods have failed, and whether they are all failures or near failures.

DR. SHEWMON: And again that was spectroscopic?

MR. JOHNSTON: Yes.

DR. SHEWMON: And B&W had a detector?

DR. VAN HOUTEN: They had an intrinsic -- it wasn't lithium --

MR. JOHNSTON: Anyway, it was spectroscopic. It was germanium. Their system didn't have to be kept within hydrogen temperatures at all times. It could climb up if the power was off. Therefore, you could put it inside a core for periods of time -- not in the core, but you didn't have to have access to it all the time.

DR. OKRENT: Excuse me. Is that in use on any power reactors that you know of, spectroscopic examination of primary system?

MR. JOHNSTON: I'm not up to date on it, but I believe Westinghouse was doing their development on the Ginna reactor.

DR. OKRENT: I mean as distinct from them doing some testing, for example, like Sequoyah, which is the most recent

Westinghouse plant. Did they have a spectroscopic device on their letdown system?

MR. JOHNSTON: I can't answer the question, except it's a TMI action item, which all plants are required to have by January 1st, 1981. Whether Reg Guide 1.97 gets approved or not, there is the requirement for the ability to sample the reactor coolant, not off the letdown line, but directly related to the core itself. Whether they are having it actually in Sequoyah plant, or whether they were finding that they will need a best estimate effort again to get it physically located in there, or getting a waiver of a few months, I don't recall our status.

DR. SHEWMON: Prof. Okrent whispers in my ear that he doesn't think the TMI action plan talks about spectroscopic capability.

MR. JOHNSTON: It doesn't mention spectroscopic by name. What it says is they shall have a means of sampling the coolant activity almost on a real time basis. My memory is having read a couple of them, that most of them are showing a system which has (a) the ability to pull samples off, which would be carried somewhere and analyzed; and secondly, some kind of on-line, onstream gadget. If you can get it in the next room, you can certainly do spectroscopy on it.

Whether you get the short-lived, I'm not so sure.

MR. MATHIS: But the thing you have here is continuous monitoring?

MR. JOHNSTON: That's continuous. You'll get the ones that are shown on his slides, though. I mean he's showing relatively long-lived things. What you have seen there so far.

DR. OKRENT: The reason I asked whether it was aimed at PBF or power reactors is one might have different requirements. In other words, certain things might be what you need for PBF, and there might be rather different things, if you thought you wanted something that quickly translated to somebody taking the design, modifying it into a commercial package and marketing it with the necessary degree of reliability for assurance.

I guess I still cannot tell for sure which way you are headed.

MR. OSETEK: I think the major difference we would find would be in the acquisition and display system, the hardware for collecting the system would be basically what we have at PBF, but just what information you might want to display on line to a power plant or to a control is a difference that would have to be developed. It's a difference that would have to be defined in some way, just what kind of information you would like to have, and the proper data acquisition estimates must then be developed for that.

DR. OKRENT: You are suggesting it wouldn't use a different detection system?

MR. OSETEK: No, not for detection. Only for data analysis and display.

DR. VAN HOUTEN: How about the question of the different columnator for going from a low activity to a high activity level, such as in an accident?

MR. OSETEK: I think it exists maybe with slight modifications in the PBF design, but it's merely a problem of proper shielding and sample line development.

MR. JOHNSTON: Am I wrong, the system that you are having really was not a system that was already in use here at INEL by the physics group, but really PBF took it over and applied it to this case? But am I wrong that this type of electrons is being used in a variety of plants under DOE sponsorship or another part of NRC sponsoring, surveillance and actually operating the plants? Not necessarily -- it may be hooked onto the letdown line?

MR. OSETEK: Fast electronics is in use in other locations, but I'd be reluctant to say it's in use in any power plant.

MR. JOHNSTON: Not that they owned it, but DOE paid EG&G to do surveillance testing in commercial power plants? I think I am right on that.

MR. MAC DONALD: EG&G has a part of our rig in a trailer with this equipment in it that can be and has been taken to other power plants in support of DOE research.

DR. SHEWMON: Okay. Thank you very much. I guess this ends our formal presentation at this point. It is a matter for the subcommittee members to discuss what they want to.

I have got three things here that came out of the discussion. One thing that didn't come first, and that is that before we write our next report, we will have to do that against specific budget information, and I suspect that maybe one month, and at most two months from now, why, there will have to be some specific sort of best estimate budget information coming from you people, and that we will write against that, with regard to our comments on your priorities and budget levels.

Second, we have asked for information on why the NRC needs each experiment, or what the results are that will be of particular value, and I think that is a theme that we will continue to bring up, and that we would like to have some sort of a formalized report on, if we could, or at least a letter.

A slightly longer-term -- and I guess I would say information on why you need it, at least with regard to the things that will be done next year. Let's try to talk about an answer to those questions, at least as a first draft, or within a couple of months.

What may be longer term, because it is not just something that you people can do amongst yourselves, perhaps, is we would like to see a position on what I will call a risk threshold criteria for failure of studies, rather than only a failure criteria, where that is in a sense why you need it, but that is my way of summarizing what Prof. Okrent was bringing up with regard to these points -- well, I have used the word risk criteria instead of failure criteria. It's a variant of why do you want to know? It's some way of getting it. One week doesn't mind or matter much if it doesn't happen very often. Whereas 10 spread all over the inside of the reactor will be a cause of more concern or something.

And you said that something of that sort was being kicked around. We would encourage you in that.

It seems to me the questions of severe core damage is something which the committee will be discussing a fair amount over the next month or two, so I had just as soon let that one go for now, meaning that we will have some input with the discussion, and I am sure it will come up at our committee meetings.

Are there other questions or comments or things that-- let me throw it open at this point and see what other comments members of the committee have.

DR. OKRENT: The only problem with what you might call a deferment of knowing the why of specific core damage experiments that are proposed is, as we have been told, they start experiments down a path three years or something like this before they are done, and they are going to be deciding or they may have already decided in their own minds what the first few years of experiments on severe core damage should be. Either PBF or ESSOR -- I'm not including NRU --

DR. SHEWMON: I am in agreement with that. I waffled, I admit, on the next year, and I would certainly be willing to say wherever a serious effort gets involved or a commitment to an experiment is involved, we would also like to see a document on why you think that will help, or what the justification is for pursuing that, and I don't have any -- I think that is a new request, and we would like to see something on it before we write our report to Congress, which is a month or two, and what experiments that gets in.

It seems to me by the time you are committed to it, you ought to have this done, whether the experiment will actually get run in the next fiscal year or the year beyond it.

Yes?

DR. PICKLESIMER: Will our five-year program recent update satisfy your needs?

DR. SHEWMON: I doubt it, but then you might submit it, and we will see what --

DR. PICKLESIMER: How much detail do you want in this?

DR. SHEWMON: Try us once, and we'll let you know. If you've got that on hand, send it around and you'll get a rebuttal.

DR. PICKLESIMER: All right.

DR. OKRENT: Well, let me take a minute again and maybe repeat myself. I don't think it's easy to do experiments in this field. It is no criticism of the group doing it. I think it is hard for anybody to do experiments in this field that really end up answering important questions.

On the other hand, you know, it does take a lot of resources. I can't remember what it totals up to, but it's about 15 million or something, your in pile program per year. So it seems to me that a program of that magnitude, as contrasted to, let's say, the small one we just heard about -- I assume it's a small one -- on just fission product detection needs to address the question of what is the important information that you really need that we are going to supply?

And if you are unable to do that, I think you have to then maybe sit back and say, okay, we're going to supply information in some other statement, but really someone in the hierarchy has to -- somebody talked about an overall product line for General Motors or Chrysler -- you have to make sure you haven't gotten into an issue where really you have been going down a certain road and putting in resources and not really getting what you might call information that is pretty essential. And there are other places where the Commission could have gotten essential information.

So I'm not in any way saying you can't get useful information. I am not even saying I know what the right experiments are. If I were forced today -- I mean I'd probably have to beg off. I haven't tried to define a set of experiments. I think it is a hard thing to do, but I myself don't feel that the package of experiments in your first 40, whatever it is, have given essential information commensurate with the total level of resources committed. That's a personal opinion.

MR. JOHNSTON: When you were talking this morning, Dr. Okrent, you were asking about the purposes of the PBF program. I was going back and thinking about what we were saying to each other when we were defining the programs in '73 and '74.

My memory is that the concern at that time was that PBF test program be able to cover a spectrum -- cover such a spectrum of conditions that the fuel might see, such that we would develop confidence that there were no failure mechanisms that we were missing in a licensing analysis.

In other words, up to that time, we had thought about LOCAs and RIAs and our power cooling mismatches, and the question was for this new facility, was are there any other failure mechanisms or ways in which the fuel can get into trouble that we don't know about, or haven't thought of yet, and secondly, do any of these things lead to propagation, which would make the accident get worse?

Now what the PBF program -- now that is fairly general, and what was done was a parametric approach was taken. We said what are the parameters that can be varied that would affect the fuel? You can vary the power, you can depressurize the system, you can do various things of that sort, so we arrived at an envelop of typical experiments that would cover those kind of parameters, and that, in essence, is what the 40-test program was reduced down to do.

It was difficult to say precisely which answer we were trying to get when we were trying to make sure there weren't cracks and so forth.

DR. OKRENT: The beauty is in the eyes of the beholder, I guess.

MR. JOHNSTON: Yes. In retrospect, it's nice to comment about what it should do, that experiment, and prove to me precisely what you are trying to get out of it. But I think we couldn't exactly do that. It wasn't really the ground rules at the time the program was added. It was make damn sure we're not missing something that doesn't rear up and bite us when we're not looking. And I think that's what we're trying to do.

DR. OKRENT: Well, let me pursue your general theme a bit. One area of experiments that was in your 40 experiment list that at least to my mind most closely came into the area was there some surprise where things in fact are going to go; whereas in the current analysis, it suggests it would be the flow blockage experiments in the BWR.

But then we really didn't have any experimental phase, and we did have limited information certainly then on coolant interactions, and so forth, between molten UO_2 and water, and so forth.

In fact, in your program, the analytical program didn't model this kind of experiment in your FRAPT, whatever, 5 or 6 doesn't go into that experiment which the LMFBR program is going to do very vigorously for a long period of time.

So your experimental work didn't go into an extrapolation of your experimental program.

MR. JOHNSTON: The only reason for doing that, that had to do with better understanding subsequent to the original concern.

DR. OKRENT: I was just trying to address your point about where might there be sort of a vague area which GE was saying it's okay, but the NRC Staff didn't have its own analyses and so forth. That was the one, I would say, of the things on your list.

So I don't know that it is likely to turn up anything significant if you were to do it. In other words, we've got some background, but --

MR. JOHNSTON: Well, the PCM is somewhat of that category. The DNB, let's put it that way, and the out-of-pile stuff at Columbia. Every time you go into film boiling in a BWR electrically heated rod, you do indeed go way up in temperature and melt your test rod, and we have noticed in doing that some experiment in pile that doesn't happen and the question is, what's the difference between the in-pile and out-of-pile experiments? There's a big difference in the thermal mass of the electrically heated rods and the nuclear rods.

I was talking to people from Columbia about this, because some people still think that's what happens, in a reactor when you go into DNB, and yet there is abundant evidence in pile that that is not what happens.

So we need to know why. We need to be able to articulate why this is the kind of stuff the PBF has shown which we did not expect when we started this program. We really expected the fuel was going to go up relatively fast when we went to DNB, and that we might get fuel squirting out of the cladding.

I remember the discussions, we had the SPERT code to try to model the size of the droplets and particles that were going to interact, and all this sort of thing, but the program didn't seem to produce those kind of results, because that isn't what seemed to happen.

So I think we have learned something there we didn't expect. Now it's in a direction of things being more benign than worse. But that was a hot area when we started, and we didn't really get what I think you people thought we would get.

DR. OKRENT: I don't know whether the area of -- of power coolant mismatch at fairly high powers is precluded from giving you, you know, desirable failure modes. I still have, I guess, the PRTR experiment in mind, if you want to call it that.

I think those are interesting experiments in that area, because they do indicate it doesn't happen every time. You know, each time you get to a partly molten fuel element in the weakened cladding, you are not automatically in an area. So that in itself gives you, I think, a useful perspective. I don't think you have explored the whole area, but I give the program credit for those.

MR. JOHNSTON: We had difficulty in being in the middle in a certain sense in planning these programs, because in order to get those conditions we had to go quite a bit beyond the Chapter 15 type of calculations. The power levels and things that industry calculates for these sort of things, that they thought we were running a program which was too extreme and too unreal and too far away from reality to be of any use. And that is some of the stuff that the EPRI people commented on, in terms of verifying the DBE type of calculations against the PBF results. And, of course, when industry dropped their power ratings from 18 or so kilowatts down to 13 or 14, it also changed the driving forces, if you like, from going on up.

But, nevertheless a large portion of the PBF was going to run on the 18 to 20 kilowatt per foot level. Feeling that we were taking the more extreme position than some people would have liked us to have taken. But perhaps it was not as extreme as other people would have liked us to have taken. We have been running down the middle ground, in many of these tests, since we have had many contending feelings.

MR.MATHIS: Just a couple of comments. I think we have to recognize that any program of this type is going to be a dynamic sort of thing, and we are going to learn things as we go along from one year to the next, and the program is going to have to be adjusted accordingly.

Now any program of this kind, in particular, can be almost infinite in its scope, because as you just mentioned, Bill, you didn't want to have anything fall through the cracks. Well, somewhere in here, you have to draw a line and say here is a reasonable approach that we think is suitable for the particular problem, and this is what we are going to do with what we learned, and that is the other part that I think is missing from this program from time to time.

Pic, you mentioned the program document. I don't think there is enough in there, usually, that says why you really need this. You talk about the needs, but you don't talk enough about the why. And when we get into the budgeting process, the priorities have to be set forth in a reasonable detail, and I know this is a real tough area, but I think we all have to face up to the fact that trade-offs inevitably enter the picture, and you are going to have to be in a position to say this is better than that, and if I have to part with some money or answer questions of that type -- I know I feel that we do not have enough information and we get a lot of the meetings like this, but we do not usually have enough information or enough time to sit down and talk about it enough to really do a good priority evaluation, and I think if you will just keep that in mind, you will help your cause and make our life more simple.

DR. PICKLESIMER: Trying to do a cost-benefit justification on experiments that give benign results is like asking a man to justify every month the premium cost for his life insurance policy. I am not sure that you can do that on a cost-benefit basis.

Now years ago PCM was a major concern to a lot of people. It turns out it is a benign problem, it is not of great importance. Trying to justify the PCM results we have gotten on a cost-benefit ratio with the benign results is an entirely different problem than if we had been able to show that PCM was a severe problem and that the present criteria were non-conservative would have been an entirely different cost-benefit ratio. How I justify this, I don't know. I don't know how to approach the problem.

I can say this was a concern, it turned out to be not of great importance. I can't say it's worth the $10 million of tests or $2 million of tests.

DR. SHEWMON: We won't guarantee that we won't come back and say we told you so, or exercise hindsight. But what we are asking you for here is not hindsight, but a little more foresight, and I guess I would come back to my earlier statement that right or wrong the criteria used by the EPRI report here was the first time I had ever seen a reasonably concise overview.

You know, that's at least one criteria it ought to meet, DBEs. Bill comes up in another one and say, no, that isn't the criteria we thought we were trying to meet at that time, and, fine, they are both right, but a statement of what you feel the basis of the criteria is for justifying the experiments is something which has not come out of this program very easily.

Lord knows the EG&G people generate enough paper, but maybe amongst all those plans and proposals and other stuff -- I know the general conclusion on this side of the table is we've got to have something that comes more cogently to the NRC, and it's much more your responsibility than theirs.

DR. PICKLESIMER: We do this analysis in-house on the justification of our programs within the branch, within the division, within the office.

DR. SHEWMON: Good. Then send it to us and let us comment on it, and maybe we will go from there.

DR. PICKLESIMER: But what we consider adequate justification apparently you don't. I don't know how to satisfy that.

DR. SHEWMON: Try us with a concise statement of what you have in-house, and it may work. At least we guarantee we will get back to you on what it is we don't find there.

Do you have any comments on your early first voyage?

DR. SOLOMON: No.

DR. SHEWMON: Okay. Meeting adjourned.

(Whereupon, at 4:00 p.m., the meeting was adjourned.)

* * * *

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS - Subcommittee on Fuel

Date of Proceeding: August 21, 1980

Docket Number: ____________________

Place of Proceeding: Idaho Falls, Idaho

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Ann Riley

Official Reporter (Typed)

Ann Riley

Official Reporter (Signature)

#1

OVERVIEW OF FBRB PBF PROGRAM

BY

M. L. PICKLESIMER
FUEL BEHAVIOR RESEARCH BRANCH, NRC

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUEL
AUGUST 21, 1980

OVERVIEW OF PBF PROGRAM

- o FBRB ASSUMED RESPONSIBILITY FOR PBF PROGRAM IN 1973 ON FORMATION OF DIVISION OF REACTOR SAFETY RESEARCH.
- o 160-TEST, TEST MATRIX ORIGINALLY DEVELOPED REDUCED TO 40-TEST, TEST PROGRAM IN FY 74
- o 40-TEST PROGRAM (REDUCED TO 30 TESTS IN NOVEMBER 1978) CONSISTING OF:

8	POWER-COOLING-MISMATCH (PCM)
6	IRRADIATION EFFECTS (IE)
5	GAP CONDUCTANCE (GC)
10	LOSS-OF-COOLANT (LOCA)
6	REACTIVITY-INSERTION (RIA)
5	INLET FLOW BLOCKAGE (IFB)
40	

- o TESTS ADDED SINCE ARE 2 THERMOCOUPLE QUENCH TESTS (TC) AND 1 LOFT LEAD ROD TEST (LLR), EACH HAVING MULTIPLE QUENCHES.
- o TESTS DELETED ARE 5 FLOW BLOCKAGE AND 5 LOCA.
- o 24 OF THE 40 ORIGINAL TESTS HAVE BEEN CONDUCTED AS HAVE 2 OF THE 3 ADDED TESTS.
- o 5 OF THE ORIGINAL 40 TESTS REMAIN TO BE PERFORMED. OF THESE 2 WILL BE CONDUCTED (LOC-6 AND LOC-7), AND 3 HAVE BEEN PUT ON INDEFINITE HOLD (RIA-3, -6, -7).

- TEST PROGRAM HAS BEEN DEVELOPED AND MODIFIED CONTINUALLY BASED ON ACRS RECOMMENDATIONS AND PBF REVIEW GROUP, NRR, AND INDUSTRY DISCUSSIONS AND NEEDS.

- TIME REQUIRED FOR COMPLETION OF A TEST IS BETWEEN 3-1/2 AND 4 YEARS FROM CONCEPT AND FIRST PLANNING TO ISSUANCE OF FINAL DATA REPORTS.

PLANNING OF EXPERIMENT, DESIGN, AND CONSTRUCTION OF TEST TRAIN	1-1/2 - 2 YEARS
PREPARATION OF REACTOR, INSERTION, AND CONDUCT OF TEST	1 - 2 MONTHS
REMOVAL OF TEST TRAIN, COOLING, AND TRANSPORT TO HOT CELL	1 - 2 MONTHS
HOT CELL EXAM, DATA ANALYSIS, AND REPORT PREPARATION	1-1/2 - 2 YEARS
TOTAL TIME	3-1/2 - 4 YEARS

- TEST PROGRAM PRIORITIES SET BY PBF REVIEW GROUP IN CONCERT WITH NRR AND APPROVED BY ACRS WERE (IN 1976) IN ORDER, PCM, LOCA, RIA, GC, AND IFB, AS SAFETY ISSUES.

TEST RESULTS HAVE SHOWN THAT:

- FILM BOILING DURING A PCM CAN BE SURVIVED FOR TENS OF MINUTES (FAR BEYOND ORIGINAL ESTIMATES).
- PCM-DNB DOES NOT PROPAGATE FROM ROD-TO-ROD IN A BUNDLE TEST.
- IRRADIATION EFFECTS ARE REMOVED ONCE CLAD TEMPERATURES ARE ABOVE 900K.
- LOCA BALLOONING IN-PILE IS SAME AS EX-PILE UNDER EQUIVALENT CONDITIONS.
- PRESENT RIA LIMITS FOR CORE COOLABILITY ARE OPEN TO QUESTION AND UNDER DISCUSSION.
- GAP CONDUCTANCE IS HIGHER THAN PREVIOUSLY ESTIMATED.

- o NEW TEST PROGRAM CONSISTING OF SEVERE FUEL DAMAGE (SFD) AND OPERATIONAL TRANSIENT (OPTRAN) TESTS.

- o OPTRAN TESTS WILL EXAMINE FUEL BEHAVIOR IN MODERATE FREQUENCY, MODERATE PROBABILITY TRANSIENTS OCCURRING DURING NORMAL TO ABNORMAL OPERATION SUCH AS:

  - o STEAM ISOLATION VALVE CLOSURE WITHOUT SCRAM,
  - o TURBINE TRIP WITH AND WITHOUT SCRAM,
  - o HIGH RAMP RATE POWER INCREASE,
  - o CONTROL ROD WITHDRAWAL.

- SEVERE FUEL DAMAGE (SFD) TESTS WILL EXAMINE FUEL AND ROD BEHAVIOR IN BUNDLES DURING ACCIDENT PRODUCING CORE DAMAGE BEYOND LOCA, SUCH AS:

    - SMALL BREAK LOCA'S A LA TMI-2,

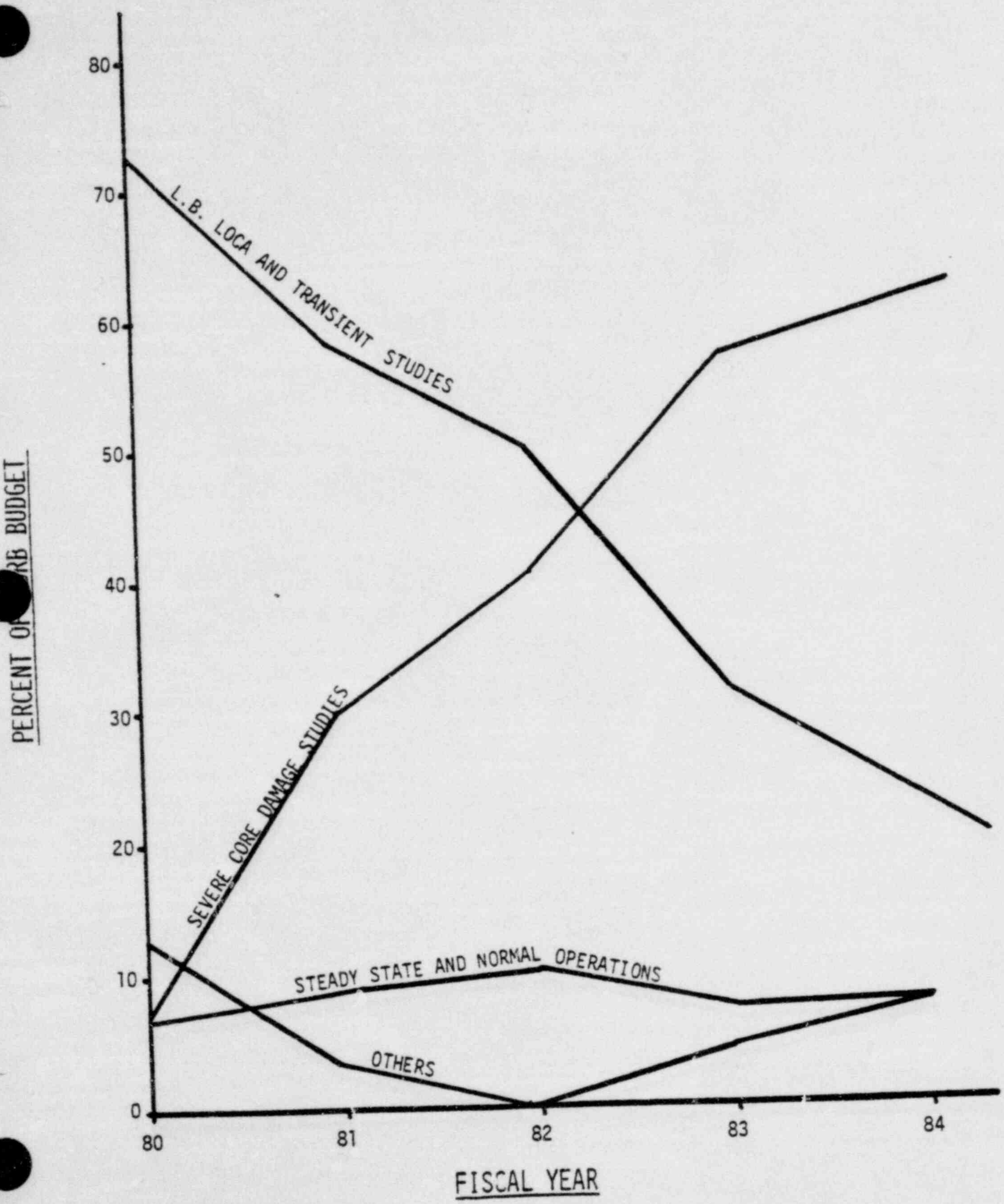
    - FORMATION OF DEBRIS AND LIQUIFIED FUEL UNDER VARIOUS RAMPING CONDITIONS,

    - CHARACTERIZATION OF DEBRIS PRODUCED,

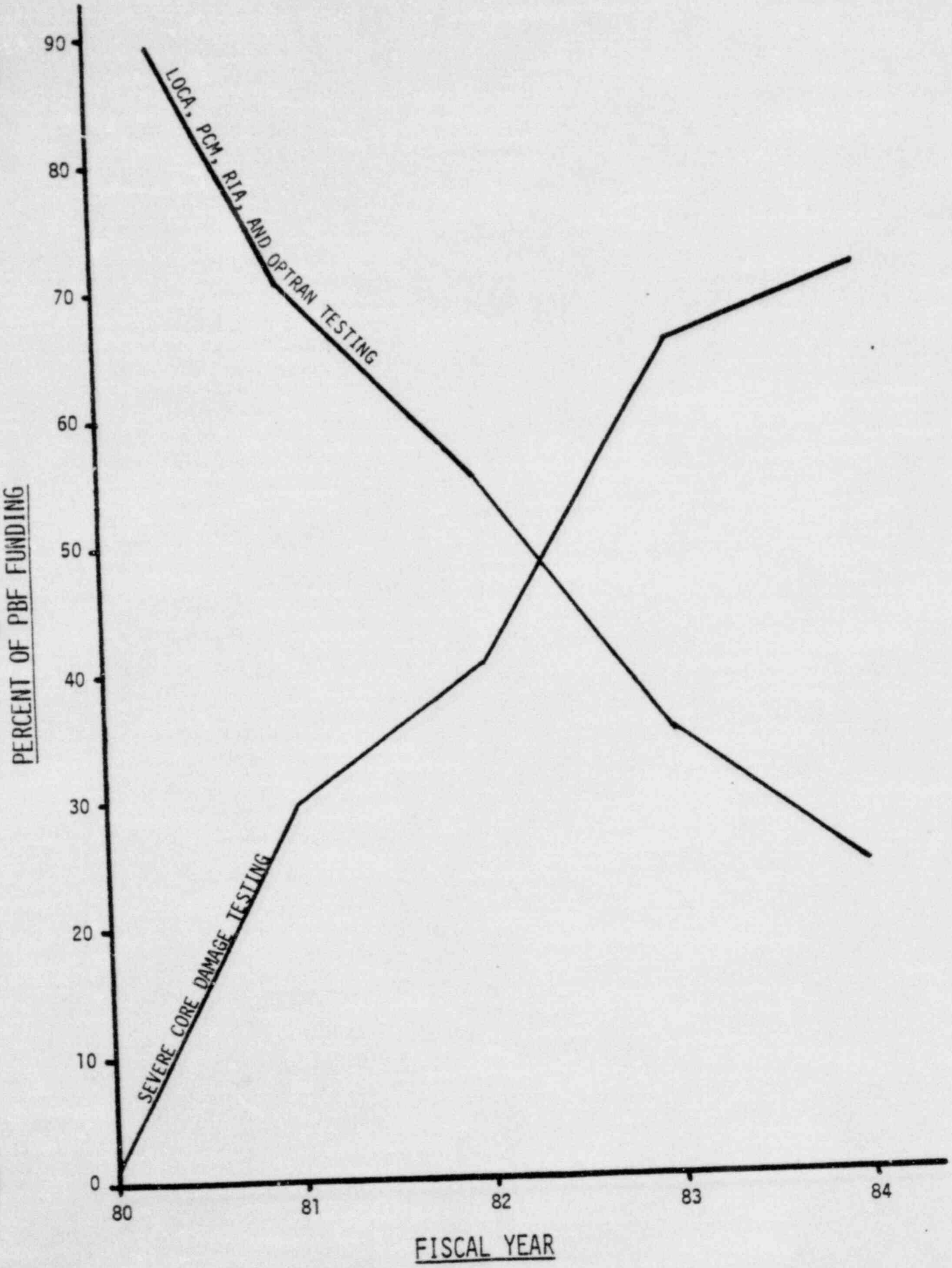
    - RELEASE OF FISSION PRODUCTS.

FUEL BEHAVIOR RESEARCH BRA

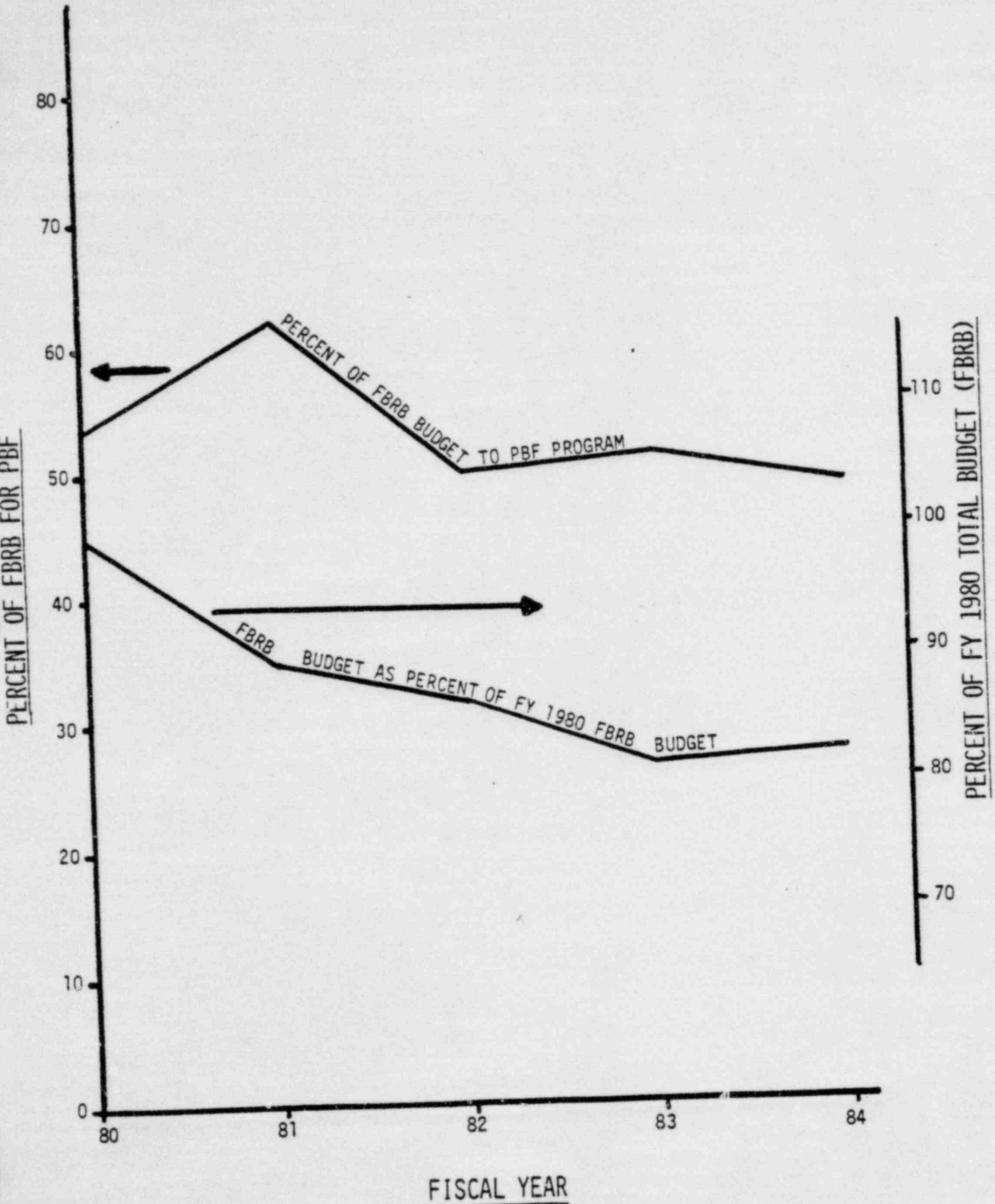
LONG-TERM BUDGET ALLOCATIO



FUNDING ALLOCATIONS IN PBF PROGRAM



ALLOCATION OF FBRB BUDGET TO PBF



Tokar
#2

HOW IS PBF ADDRESSING

REACTOR FUELS

LICENSING NEEDS?

SOME HISTORICAL PERSPECTIVE
(PBF PRIORITIES - 2 YEARS AGO)

FROM A REACTOR FUELS PAROCHIAL VIEWPOINT, THE PBF PRIORITIES WERE AS FOLLOWS:

1. RIA - START AND FINISH ASAP BECAUSE OF IDENTIFIED DEFICIENCIES IN LICENSING POSITION.
2. LOFT LEAD ROD - RESULTS NEEDED CONSISTENT WITH LOFT SCHEDULE.
3. PCM - NO URGENCY SEEN, BECAUSE DNB FAILURE CRITERION WAS CONSIDERED CONSERVATIVE FOR OVERHEATING EVENTS.
4. BLK - LOW "FUELS" INTEREST BECAUSE BLOCKAGE INVOLVES PCM.
5. LOCA - GIVEN T/H CONDITIONS, FUEL BEHAVIOR BELIEVED WELL MODELED FOR LOCA.

RIA

LICENSING BACKGROUND

1. GDC-28 - SAYS PROTECT (1) PRESSURE SYSTEM BOUNDARY AND (2) CAPABILITY TO COOL THE CORE ("COOLABLE GEOMETRY").

2. R.G.-1.77 - (PWR ROD EJECTION) MENTIONS GDC-28 REQUIREMENTS AND SAYS FAILURE CONSEQUENCES ARE INSIGNIFICANT BELOW 300 CAL/G.

3. SRP SECTIONS 15.4.8 AND 15.4.9 - SAY ASSUMED FAILURE THRESHOLDS ARE 170 CAL/G FOR BWRs (AT 0 OR LOW POWER) AND DNB FOR PWRs (AND BWRs AT POWER).

IMAGE EVALUATION
TEST TARGET (MT-3)

6″

MICROCOPY RESOLUTION TEST CHART

IMAGE EVALUATION
TEST TARGET (MT-3)

6"

MICROCOPY RESOLUTION TEST CHART

280 CAL/G COOLABILITY LIMIT

- 280 CAL/G WAS DERIVED FROM SPERT DATA (OBTAINED MAINLY ON UNIRRADIATED OR LOW B.U. RODS)

- 280 CAL/G WAS CHOSEN TO AVOID PROMPT RUPTURE AND DISPERSAL OF MOLTEN FUEL.

TWO PROBLEMS:

1) SPERT DATA WERE REPORTED AS TOTAL ENERGY (INTEGRAL OF REACTIVITY PULSE); NOT AS FUEL ENTHALPY. FOR SPERT, 280 CAL/G TOTAL ENERGY = ~230 CAL/G RADIALLY AVERAGED PEAK ENTHALPY.

2) PRELIMINARY TESTS FROM PBF (RIA 1-1) SHOWED SEVERE FRAGMENTATION AT ENERGIES BELOW 280 CAL/G.

280 CAL/G ACTION AND STATUS

. ~2 YEARS AGO A "USER'S NEED" WAS DRAFTED ASKING FOR POWER RAMP DATA ON HI-B.U. ($\geq$20,000 MWD/T) RODS TESTED AT RADIALLY AVERAGED ENTHALPIES ~ 280 CAL/G.

. ACRS QUESTIONS NEED FOR RIA ON PROBABILITY GROUNDS.

. WE RESPOND THAT PROBABILITY ISSUE CANNOT BE RESOLVED. SO DO THE TESTS TO RESOLVE THE COOLABILITY CONCERN.

. SPRING 1980. REACTOR PHYSICISTS DO CALCULATIONS SHOWING THAT 280 CAL/G WILL NOT BE APPROACHED (WHEN CERTAIN ULTRA CONSERVATISMS ARE ELIMINATED).

RIA INCIPIENT FAILURE THRESHOLD

- CURRENT 170 CAL/G AND DNB FAILURE CRITERIA ADDRESS ONLY OVERHEATING EFFECTS (OXIDATION AND EMBRITTLEMENT), NOT PCI.

- BUT SPERT TESTS ON HI-B.U. RODS (ONLY 2 RODS WERE TESTED) AND RECENT PBF TEST (RIA 1-2) INDICATE THAT PCI IS PREDOMINANT FAILURE MECHANISM AND THAT FAILURE THRESHOLD DECREASES WITH B.U.

- THEREFORE, THE DRAFT USER'S NEED ASKED FOR POWER RAMP TESTS ON HI-B.U. (~30,000 MWD/T) RODS TO BE TESTED AT < 200 CAL/G.

PBF OPTRAN TESTS

LICENSING BACKGROUND

- PCI GENERALLY RECOGNIZED AS POTENTIAL FAILURE MECHANISM FOR SEVERAL YEARS. INDUSTRY APPROACH DIRECTED TOWARD NORMAL OPERATION RESTRICTIONS AND DEVELOPMENT OF DESIGN REMEDIES.

- NRR CONCERN FOCUSED ON NEED TO PREDICT NUMBERS OF PCI FAILURES FOR POSTULATED ACCIDENTS AS INPUT TO DOSE CALCULATIONS.

- ALSO NEED TO ASSURE SIGNIFICANT PCI FAILURES WILL NOT OCCUR DURING "MODERATE FREQUENCY" EVENTS (BECAUSE GDC-10 SAYS SAFDLs CANNOT BE EXCEEDED).

- BUT CURRENT T/H CRITERIA FOR OVERHEATING-TYPE DAMAGE (OXIDATION AND EMBRITTLEMENT) DO NOT ADDRESS PCI.

PCI ACTION

- PROFIT MODEL FOR PCI FAILURE PROBABILITY PREDICTION DEVELOPED AT PNL.
- USERS NEED SENT TO RES 8/79 ASKING FOR IN-PILE RAMP DATA.
- USERS NEED SENT TO RES ASKING FOR PARTICIPATION IN DEMO RAMP-II (TO CHECK PREDICTED FAILURE THRESHOLD).
- PCI RRG ESTABLISHED IN SPRING.
- RESPONSIBILITY FOR PCI ANALYTICAL EFFORT TRANSFERRED TO RES.
- OPERATIONAL TRANSIENT (OPTRAN) TEST SERIES UNDERWAY IN PBF.

OPTRAN PBF TESTS

. CURRENTLY PROJECTS 7 TESTS (EMPHASIZING PCI).

. LATEST 2 TESTS RESPOND DIRECTLY TO OUR 8/79 USER'S NEED.

OPTRAN 1-6: 1 TO 3 BWR/4 TURBINE TRIP WITHOUT BYPASS SIMULATION USING 3 x 3 HARDWARE. 9 HI-B.U. RODS (PROBABLY FROM BELGIUM).

OPTRAN 1-7: ~17 BWR/4 TT WITH BP SIMULATIONS WITH HI-B.U. RODS, 3 x 3 HARDWARE.

COMMENT: WE BELIEVE THESE 2 OPTRAN TESTS REQUIRE FURTHER PLANNING REGARDING THE TEST PARAMETERS (Q: SHOULD THEY BE IN BOILING TRANSITION REGIME?)

OTHER FUELS RESEARCH NEEDS

1. SEVERE DAMAGE BEHAVIOR (BEYOND DBA).
   REASON: TMI-2 SHOWED NEED FOR BETTER UNDERSTANDING OF BEHAVIOR BETWEEN ANALYZED CONDITIONS AND MELTDOWN.

2. CRITERIA FOR SRP 4.2 DAMAGE MECHANISMS.
   REASON: SARs EQUATE FUEL FAILURE WITH DNB, IGNORING REAL MECHANISMS. ALTERNATE LIMITS REQUESTED BY INDUSTRY.

3. CRITERIA FOR EXTENDED BURNUP.
   REASON: CURRENT CRITERIA BASED ON LOW-B.U. EXPERIENCE. EXTENDED B.U. APPLICATIONS EXPECTED.

4. CODE VERIFICATION FOR EXTENDED BURNUP.
   REASON: MOVE TOWARD HIGHER B.U. WILL REQUIRE USE OF CODES BEYOND B.U. RANGE OF DERIVATION.

5. TRANSIENT FUEL BEHAVIOR CODE DEVELOPMENT.
   REASON: USE OF MORE MECHANISTIC CRITERIA WOULD REQUIRE NEW ANALYTICAL TOOLS FOR FSAR PREDICTIONS.

#3- M Do

SAFETY AND LICENSING ISSUES THAT ARE BEING ADDRESSED BY THE PBF PROGRAM

Presented by

P.E. MacDONALD

EG&G Idaho, Inc.

OUTLINE

BASELINE PROGRAM

- POWER-COOLING-MISMATCH (PCM) ACCIDENTS
- REACTIVITY INITIATED ACCIDENTS (RIA)
- LOSS-OF-COOLANT ACCIDENTS (LOCAs)

NEW PROGRAM

- ANTICIPATED TRANSIENTS WITH AND WITHOUT SCRAM (OPTRAN)
- SEVERE FUEL DAMAGE (TMI-2)

NRC POWER-COOLING-MISMATCH LICENSING CRITERIA

- THE CALCULATED DNBR MUST EXHIBIT A 95% PROBABILITY, AT THE 95% CONFIDENCE LEVEL, THAT NO CORE FUEL ROD WILL DEPART FROM NUCLEATE BOILING.

- CRITERION IMPLIES THAT FUEL RODS OPERATED AT DNBR'S LESS THAN 1.13 TO 1.32 ARE IN FILM BOILING AND FAIL.

- MECHANISTIC FUEL DAMAGE LIMITS ARE NOT PRESENTLY ACCEPTED.

POWER-COOLING-MISMATCH SAFETY ISSUES

- WHAT IS THE MARGIN BETWEEN DEPARTURE FROM NUCLEATE BOILING AND FUEL ROD FAILURE

- CAN A COOLABLE GEOMETRY BE MAINTAINED

- WHAT IS THE PROPENSITY FOR DEPARTURE FROM NUCLEATE BOILING AND FUEL FAILURE PROPAGATION

- WILL ENERGETIC MOLTEN FUEL-COOLANT INTERACTIONS OCCUR

RESULTS OF POWER-COOLING-MISMATCH TEST PROGRAM

- LWR FUEL RODS CAN DEPART FROM NUCLEATE BOILING FOR SIGNIFICANT TIMES AND WITHSTAND SEVERE DAMAGE PRIOR TO ROD FAILURE.

- CLADDING DEFORMATION OCCURS ABOVE 920 K BUT CLADDING RETAINS SUFFICIENT DUCTILITY TO ACCOMMODATE COLLAPSE STRAINS AND PRECLUDE IMMEDIATE FAILURE.

- THE PRIMARY FUEL ROD FAILURE MECHANISM IS OXYGEN EMBRITTLEMENT OF THE CLADDING FROM H_2O- AND UO_2-ZIRCALOY REACTIONS.

RESULTS OF POWER-COOLING-MISMATCH TEST PROGRAM (cont'd)

- ZIRCALOY OXYGEN EMBRITTLEMENT IS PREDICTABLE USING TEMPERATURE-TIME CORRELATIONS DEVELOPED FROM OUT-OF-PILE DATA.

- ENERGETIC MOLTEN FUEL-COOLANT INTERACTIONS DO NOT OCCUR.

- MOLTEN FUEL-CLADDING CONTACT DOES NOT RESULT IN CLADDING MELTING.

RESULTS OF POWER-COOLING-MISMATCH TEST PROGRAM (cont'd)

- FUEL GRAIN SEPARATION (POWDERING OR DESINTERING) OCCURS WHEN THE FUEL IS QUENCHED FROM TEMPERATURES ABOVE 1900 K.

- ROD-TO-ROD DNB AND FUEL ROD FAILURE PROPAGATION IS NOT EXPECTED.

- LOSS OF COOLABLE GEOMETRY IS NOT EXPECTED.

KEY REACTIVITY INITIATED ACCIDENT SAFETY ISSUES

- FUEL FAILURE THRESHOLD
- LOSS OF COOLABLE CORE GEOMETRY
- OVERSTRESS OF PRESSURE VESSEL

APPLICABLE NRC LICENSING CRITERIA

- OFFSITE DOSE CONSQUENCES WITHIN 10 CFR 100
    - 170 CAL/G - BWRs
    - DEPARTURE FROM NUCLEATE BOILING - PWRs
- RADIAL AVERAGE PEAK FUEL ENTHALPIES BELOW 280 CAL/G
- THESE VALUES WERE BASED ON RESULTS OF EARLY INEL TESTS WITH UNIRRADIATED FUEL RODS

RESULTS FROM REACTIVITY INITIATED ACCIDENT TEST PROGRAM

- MODE AND CONSEQUENCES OF ROD FAILURE ARE SIGNIFICANTLY AFFECTED BY PRIOR IRRADIATION
  - IRRADIATED RODS FAIL DUE TO PELLET-CLADDING INTERACTION AT LOW ENERGY DEPOSITIONS
  - EXPANSION OF GASEOUS AND VOLATILE FISSION PRODUCTS INDUCE EXTENSIVE SWELLING OF MOLTEN IRRADIATED FUEL

RESULTS FROM REACTIVITY INITIATED ACCIDENT TEST PROGRAM (cont'd)

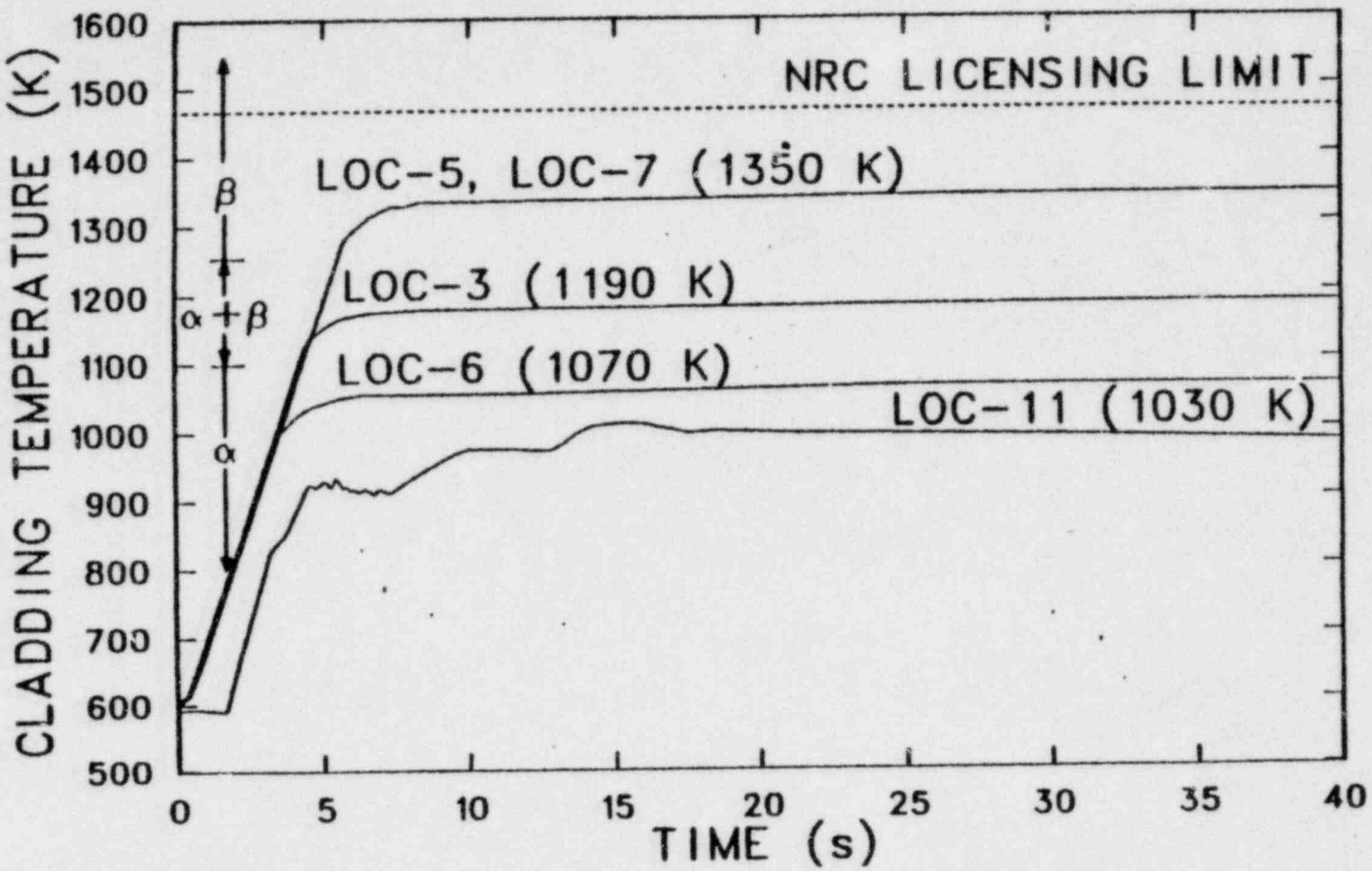
- 280 CAL/G AND 170 CAL/G LIMITS MAY BE NON-CONSERVATIVE
- THE FILM BOILING CRITERIA IS INAPPROPRIATE
- LWRs ARE SAFE BECAUSE THEIR CONTROL SYSTEMS ARE DESIGNED WELL BELOW THE NRC CRITERIA

KEY LOSS-OF-COOLANT ACCIDENT SAFETY ISSUES

- WILL CLADDING BALLOONING DURING A LOSS-OF-COOLANT ACCIDENT LEAD TO CO-PLANAR BLOCKAGE AND SUBSEQUENT LOSS-OF-COOLABLE GEOMETRY?

- IS OUT-OF-PILE BALLOONING DATA REPRESENTATIVE OF NUCLEAR FUEL ROD BEHAVIOR?

PBF LOCA PROGRAM CLADDING TEMP HISTORIES



RESULTS FROM LOSS-OF-COOLANT ACCIDENT PROGRAM

- PBF CLADDING DEFORMATION DATA ARE IN REASONABLE AGREEMENT WITH PREVIOUSLY PUBLISHED DATA FROM OUT-OF-PILE TESTS

- PREVIOUSLY IRRADIATED RODS EXHIBIT GREATER DEFORMATION THAN UNIRRADIATED RODS

- CLADDING CREEPDOWN INCREASES CLADDING DEFORMATION DURING A SUBSEQUENT LOCA TRANSIENT

RESULTS FROM LOSS-OF-COOLANT ACCIDENT PROGRAM (cont'd)

- FUEL FRAGMENTATION AND RELOCATION WAS OBSERVED. SMALLER FUEL PARTICLES WERE PRODUCED IN THE IRRADIATED RODS THAN IN THE UNIRRADIATED RODS

- VARIATION OF INITIAL INTERNAL PRESSURE FROM 2.4 TO 4.8 MPa HAD NO SIGNIFICANT EFFECT OF CLADDING DEFORMATION IN THESE LOCA TESTS

KEY OPERATIONAL TRANSIENT SAFETY ISSUES

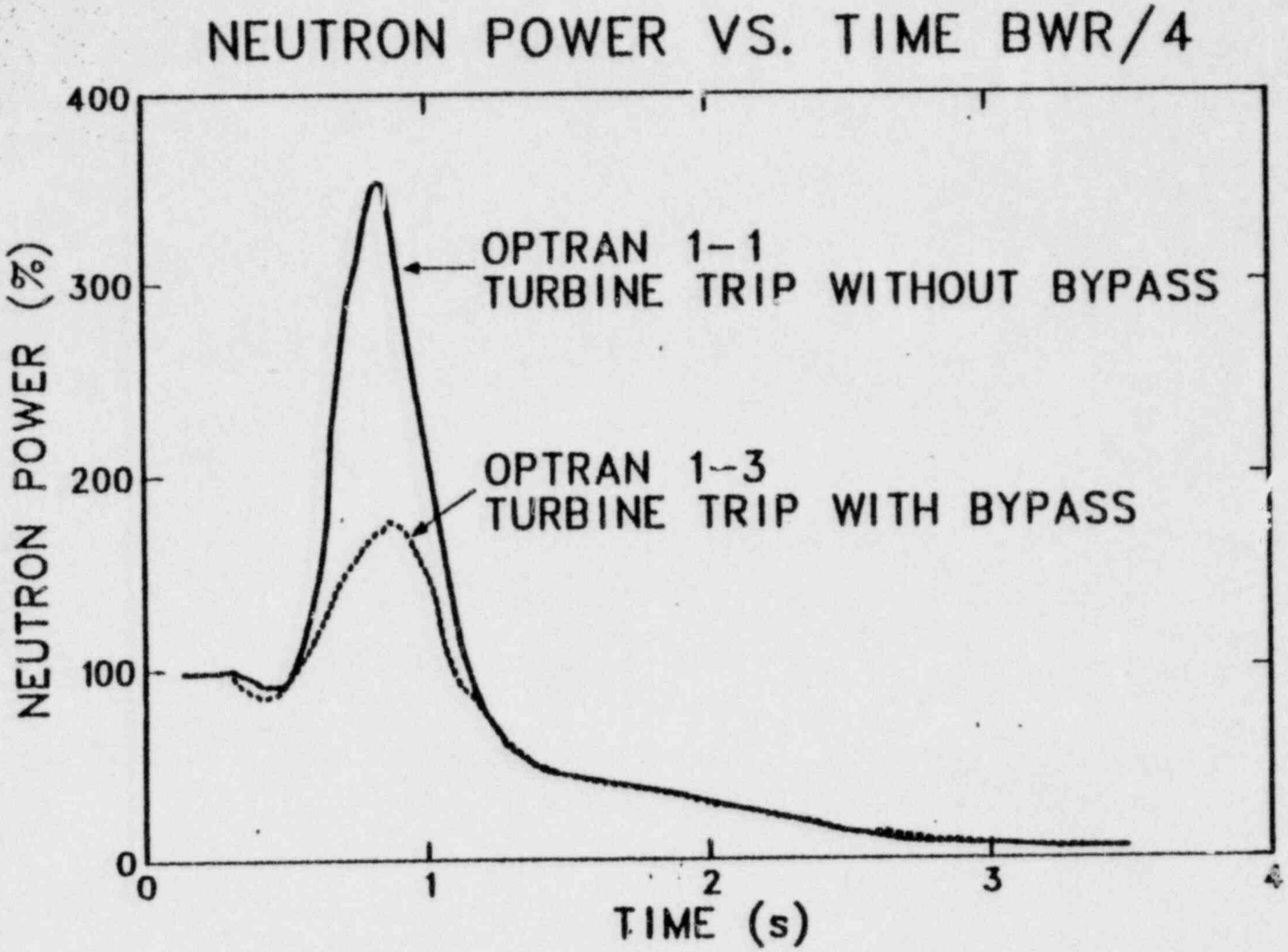
- SHOULD A REACTOR BE DERATED FOLLOWING A SEVERE OPERATIONAL TRANSIENT

- SHOULD REGULATIONS BE IMPOSED TO LIMIT PELLET-CLADDING INTERACTION IN IRRADIATED FUEL RODS

- SHOULD REACTORS BE MODIFIED TO REDUCE THE PROBABILITY OF A SEVERE ANTICIPATED TRANSIENT WITHOUT SCRAM

ANTICIPATED TRANSIENTS WITHOUT SCRAM (ATWS)

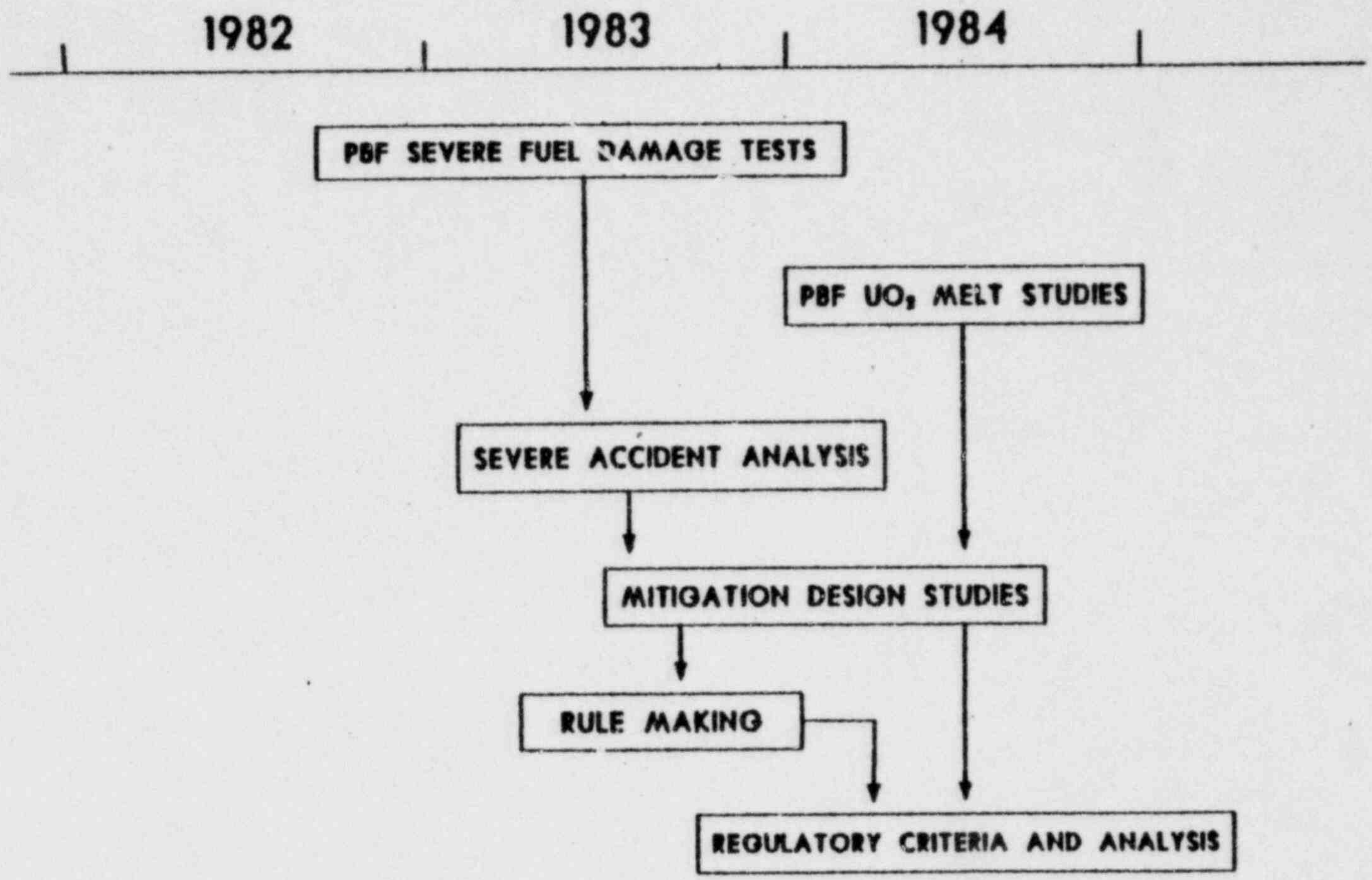
- ATWS EVENTS HAVE BEEN RECENTLY CLASSIFIED AS DESIGN BASIS EVENTS
- WORST ATWS EVENT COULD RESULT IN A 700% INCREASE IN POWER FOR 2 TO 3 SECONDS IN A BWR
- RECENT GE PROPRIETARY REPORTS INDICATE A 1200 K CLADDING TEMPERATURE COULD OCCUR FOR A FEW MINUTES, FOLLOWED BY RAPID POWER OSCILLATIONS ACCOMPANIED BY SLOSHING OF COOLANT ON COLLAPSED AND EMBRITTLED CLADDING
- THE NUMBER OF FUEL ROD FAILURES DURING SUCH AN EVENT ARE NOT KNOWN


NEUTRON POWER VS. TIME BWR/4
NEUTRON POWER (%)
400
300
200
100
0
0
1
2
3
4
TIME (s)
OPTRAN 1-1
TURBINE TRIP WITHOUT BYPASS
OPTRAN 1-3
TURBINE TRIP WITH BYPASS

PBF SEVERE FUEL DAMAGE TEST PROGRAM

A SERIES OF HIGHLY CONTROLLED AND INSTRUMENTED TESTS WHICH WILL ADDRESS CLASS 9 SAFETY ISSUES INVOLVING CORE COOLABILITY

- PHASE I - TEMPERATURES ~ 2300 K
- PHASE II - PEAK FUEL ROD TEMPERATURES $\geq$ 3200 K


1982
1983
1984
PBF SEVERE FUEL DAMAGE TESTS
PBF UO_2 MELT STUDIES
SEVERE ACCIDENT ANALYSIS
MITIGATION DESIGN STUDIES
RULE MAKING
REGULATORY CRITERIA AND ANALYSIS

PRIMARY OBJECTIVES

- CHARACTERIZE FUEL ROD DAMAGE IN TERMS OF UO_2 DISSOLUTION, MOVEMENT, FREEZING, AND FUEL ROD FRAGMENTATION
- DETERMINE THE COOLABILITY OF THE DAMAGED TEST FUEL BUNDLE

CONCLUSIONS

POWER-COOLING-MISMATCH:

- IDENTIFIED SIGNIFICANT MARGIN BETWEEN THE PRESENT NRC CRITERIA AND ACTUAL FUEL FAILURE THRESHOLDS.

REACTIVITY INITIATED ACCIDENT:

- SHOWN THAT THE NRC CRITERIA MAY BE NON-CONSERVATIVE.

LOSS-OF-COOLANT:

- PROVEN THAT CO-PLANNER BLOCKAGE AND LOSS-OF-COOLABILITY IS UNLIKELY.

CONCLUSIONS (cont'd)

OPERATIONAL TRANSIENTS:

- NEED DATA TO DETERMINE DAMAGE MECHANISMS AND FAILURE THRESHOLDS.

SEVERE FUEL DAMAGE (TMI-2):

- NEED DATA TO CHARACTERIZE CORE DAMAGE AND COOLABILITY.

#4

OVERVIEW OF FBRB SEVERE CORE DAMAGE PROGRAM

BY

M. L. PICKLESIMER
FUEL BEHAVIOR RESEARCH BRANCH, NRC

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUEL
AUGUST 21, 1980

OVERVIEW OF SEVERE FUEL DAMAGE PROGRAM

- SEVERE FUEL DAMAGE PROGRAM OF FBRB COOR''NATED AND INTEGRATED WITH SEVERE ACCIDENT PHENOMENA AND MITIGATI DECISION UNIT OF RSR BUDGET. KFK-PNS SEVERE FUEL DAMAGE ST IES, ESSOR SUPER-SARA PROGRAM, PROPOSED PHEBUS SFD PROGRAM, TMI-2 EXAMINATION, POSSIBLE SFD TESTS IN LOFT.

- OVERALL SEVERE CORE DAMAGE STUDY PROGRAM NEEDS WERE SCOPED IN PREVIOUS FBRB BRIEFING.

- SEVERE FUEL DAMAGE STUDY NEEDS BEING DISCUSSED WITH THE TECHNICAL COMMUNITY.
- MEETINGS HAVE BEEN OR WILL BE HELD ON SFD STUDIES:
  - AD HOC FBRB PROGRAM REVIEW OF SCD MODELING, JANUARY 1980
  - AD HOC FBRB MEETING ON SEVERE CORE DAMAGE STUDIES, APRIL 1980
  - NRC/PNS/JAERI INFORMATION EXCHANGE ON CLADDING AND CODES (ONE SESSION OF MEETING), JUNE 1980
  - INFORMATION NRC/PNS/JAERI DISCUSSIONS DURING WRSR INFORMATION MEETING, OCTOBER 1980
  - AD HOC SFD EXPERIMENTERS MEETING, NRC, NOVEMBER - DECEMBER 1980
  - SUPER-SARA PROGRAM PLANNING MEETINGS, JUNE, JULY, SEPTEMBER, AND OCTOBER 1980
- DETAILED PLANS CAN NOT BE COMPLETED UNTIL FUNDING AVAILABLE IS KNOWN.

- FBRB PLANS ON SEVERE FUEL DAMAGE STUDIES CONCERNED WITH IN-PILE AND EX-PILE INTEGRAL EFFECTS IN BUNDLES, SEPARATE EFFECTS AND BASIC STUDIES OF PROPERTIES, FISSION PRODUCT RELEASE FROM THE FUEL ROD AND FUEL DEBRIS, AND MODELING AND CODE DEVELOPMENT OF THE PROGRESS OF CORE DAMAGE.

- DISCUSSION TODAY IS ABOUT THE SFD STUDIES IN PBF, ESSOR SUPER-SARA, AND NRU.

- SFD TESTS IN PBF WILL BE THE FIRST TO EXAMINE DEBRIS BED AND LIQUIFIED FUEL FORMATION, CHARACTERIZE DEBRIS FORMED, AND EXAMINE TEST PARAMETERS.

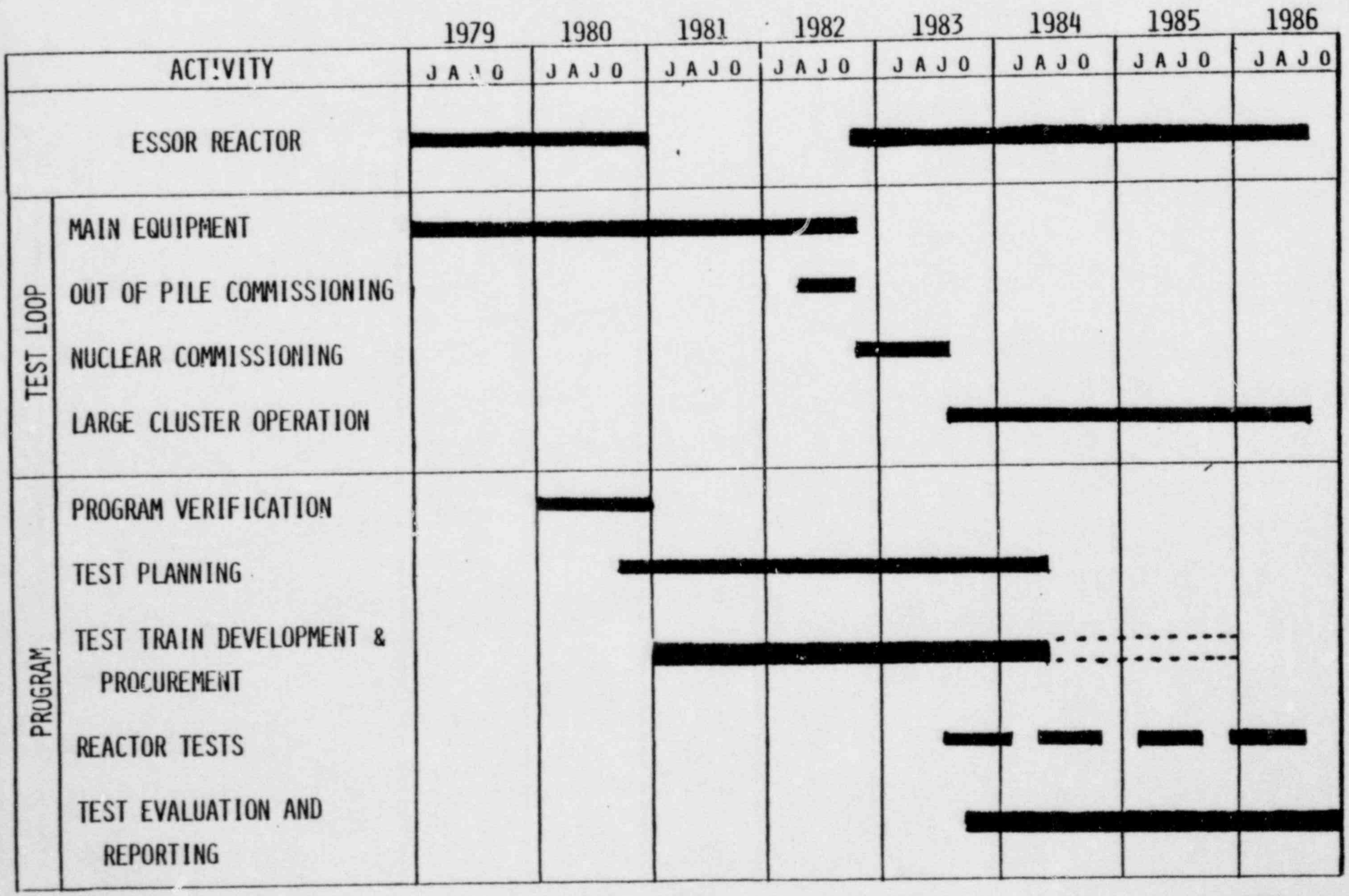
- SCALING TO COMMERCIAL POWER REACTORS WILL REQUIRE EXTRAPOLATION FROM PBF 32-ROD 3-FT TEST TO ESSOR 32-ROD 1.8-METER TEST THROUGH LOFT 15 X 15 6-FT TEST, WITH TMI-2 AS BENCHMARK.

- DATA AVAILABILITY:

  - PBF - FY 82 - 85 SMALL BUNDLE - SHORT RODS
  - TMI-2 - FY 83 - 84 FULL CORE - FULL LENGTH RODS - BENCHMARK
  - LOFT - FY 83 - 85 LARGE BUNDLE - LONGER RODS
  - ESSOR - FY 84 - 86 SMALL BUNDLE - LONGER RODS

#5

Vant Houten

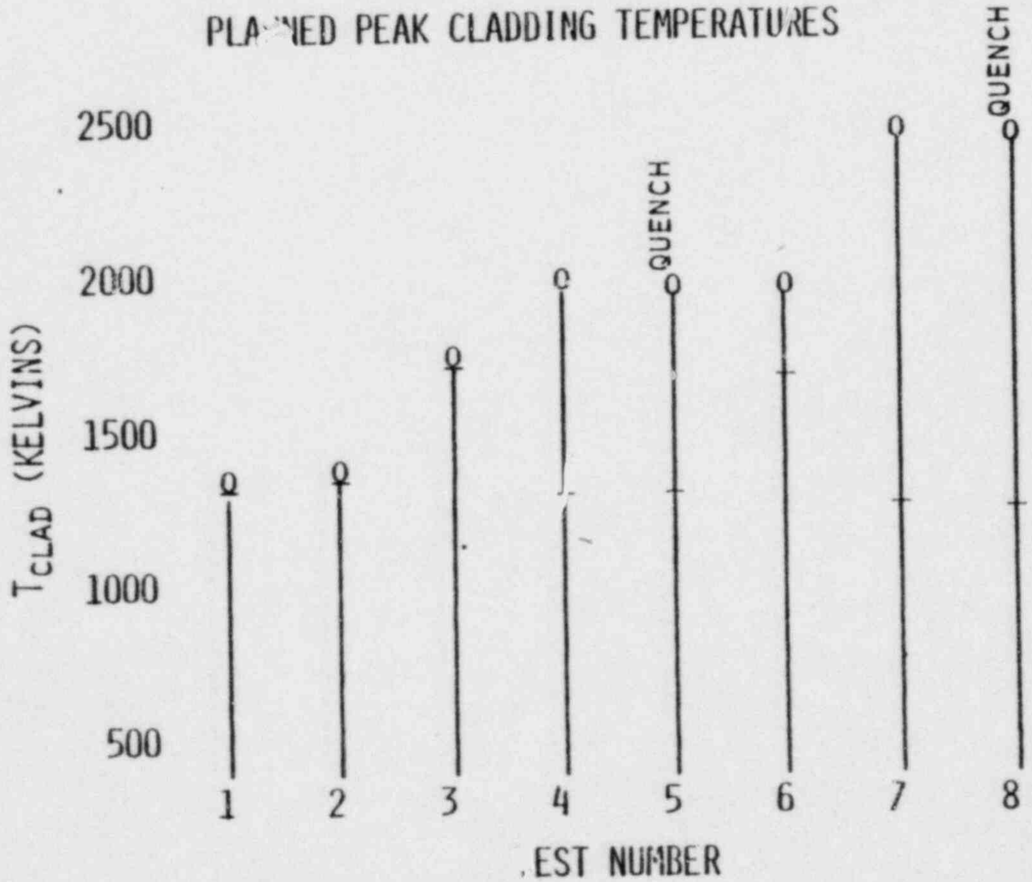


THE SUPER-SARA TEST PROGRAM (SSTP)

THE PROGRAM:	18 SEVERE CORE DAMAGE TESTS (1983-1986) o 12 SMALL-BREAK, TMI-2-TYPE TESTS, TO AND THROUGH CLAD MELTING (PCT>2300K), WITH DEBRIS BED FORMATION AND EXAMINATION (32-ROD CLUSTERS). o 6 SEVERE BALLOONING FLOW REDUCTION TESTS (32-ROD CLUSTERS)
BUDGETED PROGRAM COST: (PRIMARILY EURATOM COUNTRIES)	$140 X 10^6 (TOTAL, 1977-1986)
PLANNED NRC COST: (70%-90% TO BE SPENT WITHIN THE U.S.)	$11 X 10^6 (TOTAL, 1980-1986)

ESSOR-SSTP TESTS

PLANNED PEAK CLADDING TEMPERATURES



LEGEND - o = PEAK CLAD TEMPERATURE

- = CLAD RUPTURE TEMPERATURE

NRC LICENSING REQUIREMENTS

LOCA - SEVERE CORE DAMAGE

10 CFR	ENERGY
10 CFR 50	LICENSING OF PRODUCTION AND UTILIZATION FACILITIES
10 CFR 50.34	CONTENTS OF APPLICATION - TECHNICAL INFORMATION
	APPENDIX A-MINIMUM REQUIREMENTS - PRINCIPAL DESIGN CRITERIA
	CRITERION 35 EMERGENCY CORE COOLING
10 CFR 20	STANDARDS FOR PROTECTING AGAINST RADIATION
10 CFR 20	PERMISSIBLE DOSES, LEVELS, AND CONCENTRATIONS
	20.101, 103, 104, 105, 106 — EXPOSURE AND RADIATION IN RESTRICTED AND UNRESTRICTED AREAS
10 CFR 10C	REACTOR SITE CRITERIA
10 CFR 100.11	DETERMINATION OF EXCLUSION AREA LOW POPULATION ZONE AND POPULATION CENTER DISTANCES

KEY ELEMENTS OF THE SEVERE CORE DAMAGE CONCENSUS PROGRAM
IN SUPER SARA*

1. EMPHASIS WILL BE ON TESTS WITH CLADDING BALLOON AND RUPTURE AT CLAD T $\leq$ 1100K, FOLLOWED BY CONTINUOUS CLADDING TEMPERATURE RISE TO THE PCT, E.G., 2270K (A GRADUAL APPROACH TO THE 2270K PCT TESTS IS RECOMMENDED).

2. THESE TESTS MUST INVOLVE THE CREATION OF IN-PILE RUBBLE BEDS WITH THERMAL-HYDRAULIC CHARACTERIZATION OF THE RUBBLE BED FLOW OBSTRUCTIONS WHERE POSSIBLE.

3. THESE TESTS MUST GO TO DRIP MELTING OF THE CLADDING.

4. FISSION PRODUCT RELEASE MONITORING INSTRUMENTATION SHOULD BE ADDED TO THE LOOP TO PROVIDE AD HOC DATA TO CHECK FPR CODES.

5. PARTICULAR CONCENSUS EFFORT SHOULD BE APPLIED TO:
   - OBTAINING INTERACTION BETWEEN DRIP MELTING AND THERMAL-HYDRAULICS ANALOGOUS TO "FULL-LENGTH" CONDITIONS.
   - INCORPORATING INSTRUMENTATION FOR MEASURING LIQUID LEVEL (AND) FISSION PRODUCT SPECIES AS THE TEST PROGRESSES.

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* J. P. CONTZEN TO R. BUDNITZ, LETTER OF JULY 7, 1980, "MINUTES OF THE FIRST MEETING OF THE SUPER SARA TASK FORCE," (ANNEX 3, PAGE

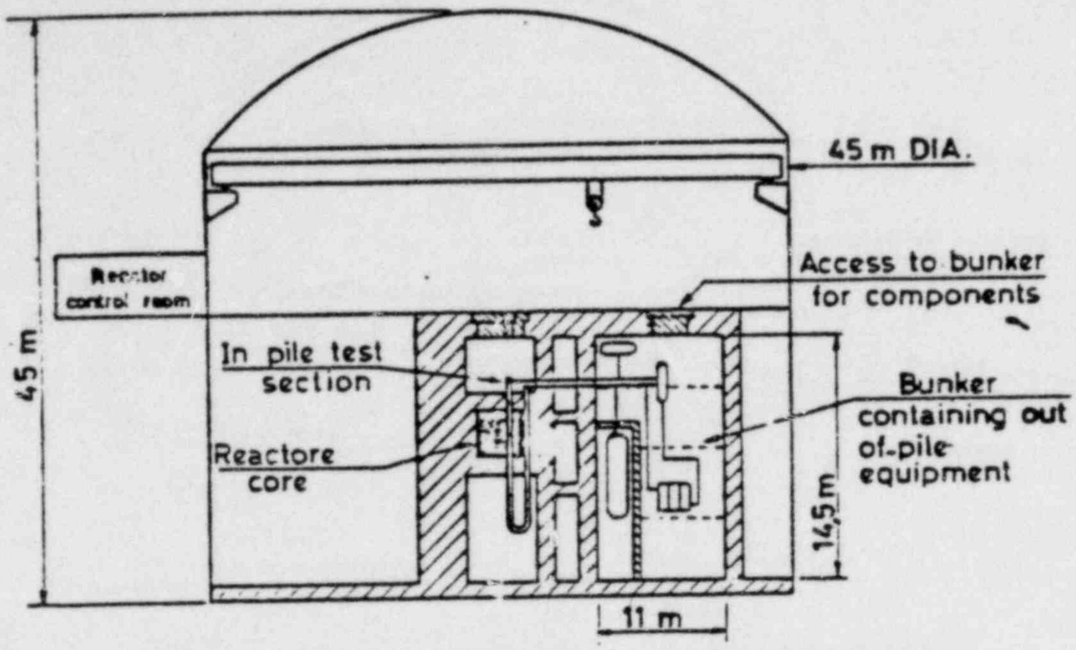
TABLE 2 : SEVERE FUEL DAMAGE : TENTATIVE TEST MATRIX

TEST N°	INTENTIONS	REQUIRED PCT (K)	REQUIRED RAMP RATE TO PCT (K/S)	P_{SYS} CONTROL	TEST TERMINATION AND EXPECTED FINAL STATE OF CLUSTER	FURTHER IN-REACTOR OPERATIONS AND MEASUREMENTS PRIOR TO EXTRACTION OF TEST-TRAIN
1	Clad creep ballooning and rupture in the α or $\alpha + \beta$ phase, attained by boil-off to required PCT, then ramping of P_{SYS}	1080 - 1200	--	~ 8MPa adjusted so that $\Delta P \approx 0$ when PCT stabilised then ramped at -0.005 MPa/s to ~ 4MPa	On attainment of widespread clad rupture, reactor shut down then cluster re-submerged. Deformed cluster configuration "frozen" for PIE.	None
2	Clad creep ballooning and rupture in the β phase with simultaneous oxidation, attained by fast boil-off to required PCT, then ramping of P_{SYS}	1350	max. poss.	~ 8MPa adjusted so that $\Delta P \approx 0$ when PCT stabilised then ramped down in range 0.05-0.005 MPa/s ⊕	After attainment of widespread clad rupture, reactor power and HPI flowrate controlled simultaneously to obtain cooldown without thermal shock. Deformed cluster configuration "frozen" for PIE.	None
3	As test 2, but for <u>high</u> β phase	1850	max. poss.	" "	" "	None
4	<u>Stage 1</u>: clad ballooning and rupture in α or $\alpha + \beta$ phase as in test 1, followed by: <u>Stage 2</u>: further boil-off and temperature rise with extensive external and internal oxidation	1080 - 1200 1600	-- 0.3 *	As in test 1 ~ 4 MPa	On attainment of complete clad oxidation in the zone of the PCT, reactor power and HPI flowrate controlled simultaneously to obtain <u>cooldown without thermal shock</u>. Cluster to retain integrity for PIE.	None
5	As in test 4, but with provocation of rubble bed by fast quench	As in test 4	As in test 4	As in test 4	On attainment of complete clad oxidation in the zone of the PCT, cluster rapidly submerged (HPI flowrate ~10 cm/s) <u>to provoke a rubble bed</u> then reactor scrammed.	Check-out of technology to be developed for instrumenting thermohydraulic characteristics of post-test blockages. Measurement of ΔP and ΔT generated across rubble bed using low power and flow (preferably steam).
6	<u>Stage 1</u>: clad ballooning and rupture in high β phase as in test 3, followed by: <u>Stage 2</u>: further boil-off and temperature rise with extensive external and internal oxidation ending with quench induced rubble bed	1650 1900	max. poss. 0.3 *	As in tests 2 and 3 ~ 4 MPa	As for test 5. Indicates if there are <u>different fragmentation and rubble bed characteristics</u> due to previous β instead of α phase rupture.	As for test 5
7	<u>Stage 1</u>: clad ballooning and rupture in α or $\alpha + \beta$ phase followed by: <u>Stage 2</u>: further boil-off and temperature rise with external and internal oxidation and the formation of a liquid Zr/UO_2 solution to provoke "candling" of the cluster	1080 - 1200 2270	--- 1 ∅	As in test 1 ~ 4 MPa	Immediately the PCT reaches 2270 K, with the <u>cluster "candled"</u> in the zone of the PCT, reactor power and HPI flowrate controlled simultaneously to obtain <u>cooldown without thermal shock</u>. Cluster to retain its "candled" geometry for PIE.	Measurement of ΔP and ΔT generated across "candled" zone of cluster using low power and flow (preferably steam).
8	As in test 7, but with provocation of rubble bed by fast quench	As in test 7	As in test 7 ∅	As in test 7	Immediately the PCT reaches 2270 K with the <u>cluster "candled"</u> in the zone of the PCT, cluster rapidly submerged (HPI flow ~10 cm/s) to <u>provoke a rubble bed</u>.	Measurement of ΔP and ΔT generat[illegible] across rubble bed using low power and fl[illegible] preferably steam

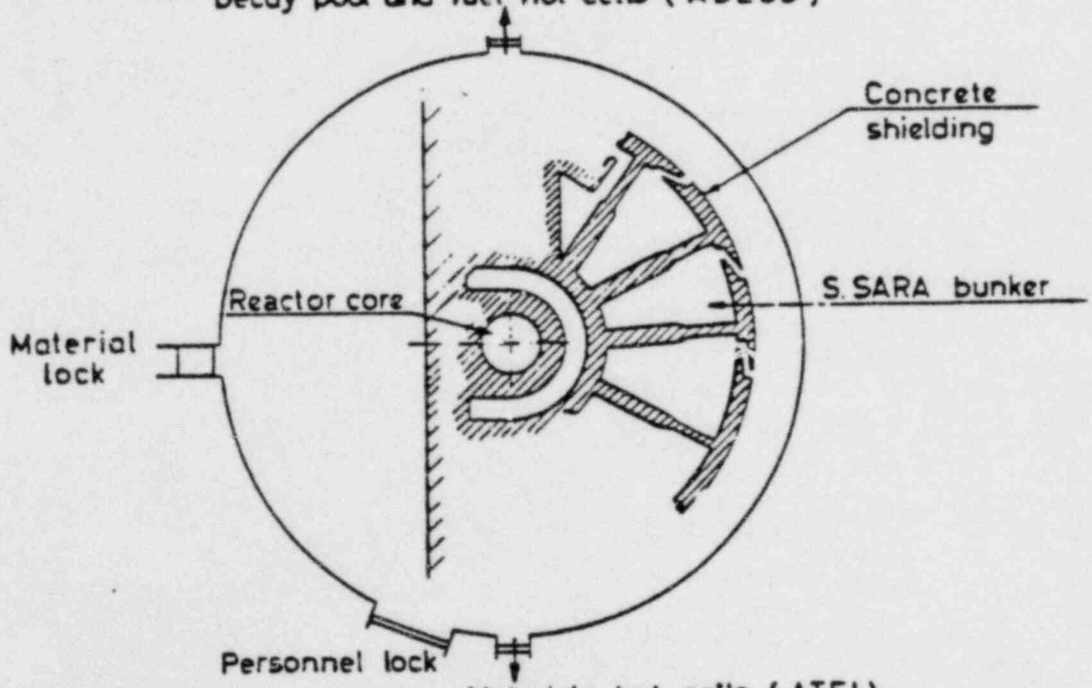
⊕ System pressure ramp chosen in the range - 0.05 to - 0.005 MPa/s to obtain desired amount of oxidation during deformation.

* Temperature ramp rate in stage 2 of tests 4, 5, 6, chosen tentatively to be ~0.3 K/s on the basis of KfK indications that clad oxidation will be completed during the ramp.

∅ Temperature ramp rate in stage 2 of tests 7, 8, chosen tentatively to be ~1 K/s on the basis of KfK indications that some Zr metal will survive to form the Zr/UO_2 molten solution above 2070 K.



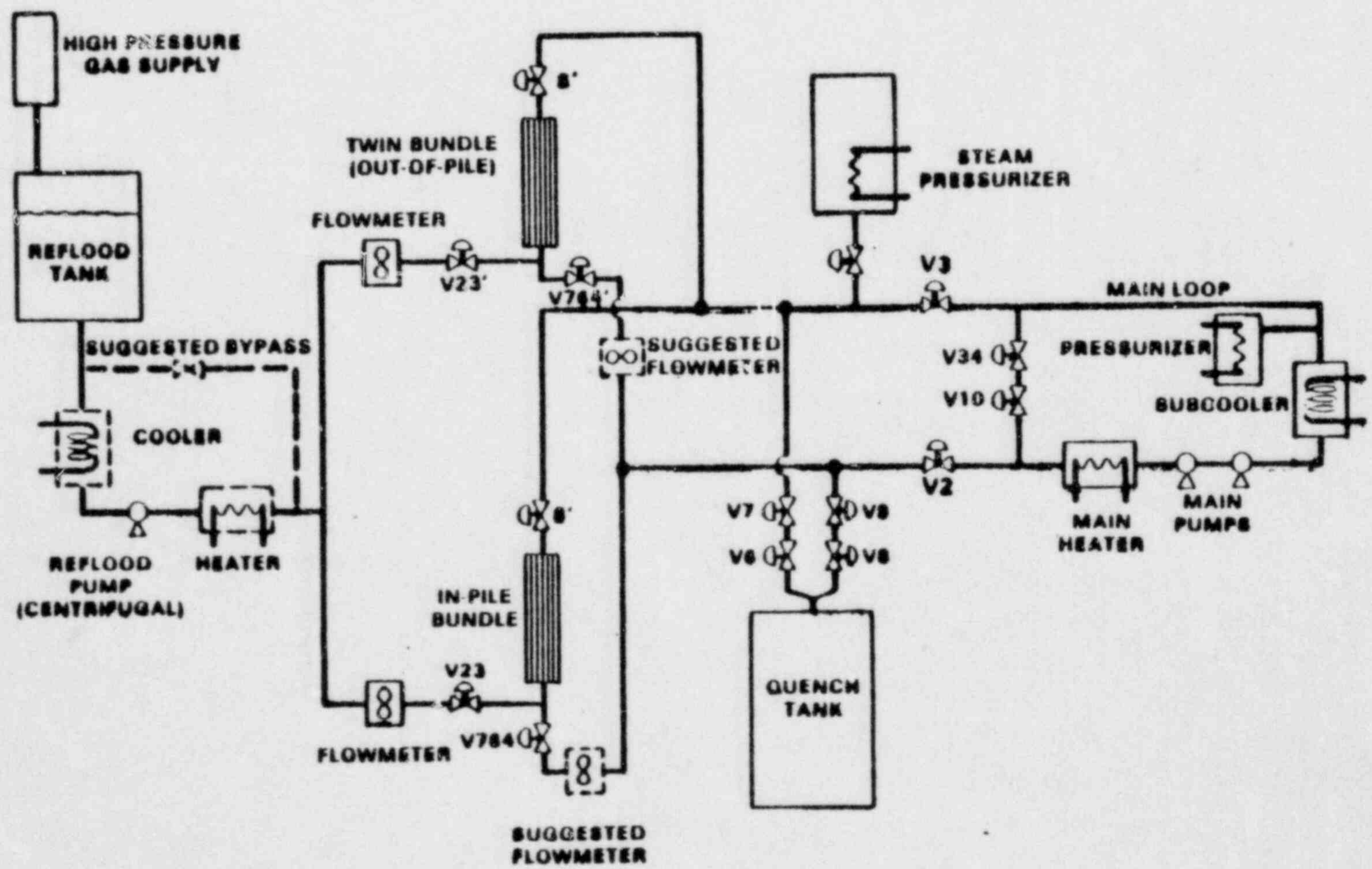
VERTICAL SECTION THROUGH ESSOR REACTOR



HORIZONTAL SECTION THROUGH ESSOR REACTOR

Location of SUPER-SARA Circuit in ESSOR Reactor

SUPER-SARA LOCA SIMULATION BASIC CONCEPT



IN-PILE TEST SECTION

PRINCIPAL OBJECTIVE IS TO PROVIDE A NUCLEAR HEATED ROD BUNDLE AS LARGE AS POSSIBLE (SUGGEST 32 RODS) IN A BASIC CIRCUIT WITH A NUMBER OF SPECIAL FEATURES TO ALLOW LOCA TESTS TO BE CONDUCTED IN-REACTOR

MAIN FEATURES

- HOT OR COLD LEG BLOWDOWN CAPABILITY IN CIRCUIT
- REFLOOD WATER SYSTEM TO PROVIDE A CONSTANT REFLOOD RATE OR, IF DESIRED, A PRE-PROGRAMMED REFLOOD HISTORY
- FISSION PRODUCT RELEASE RESULTING FROM DAMAGE TO FUEL BUNDLE IS RETAINED IN A SMALL PORTION OF THE CIRCUIT

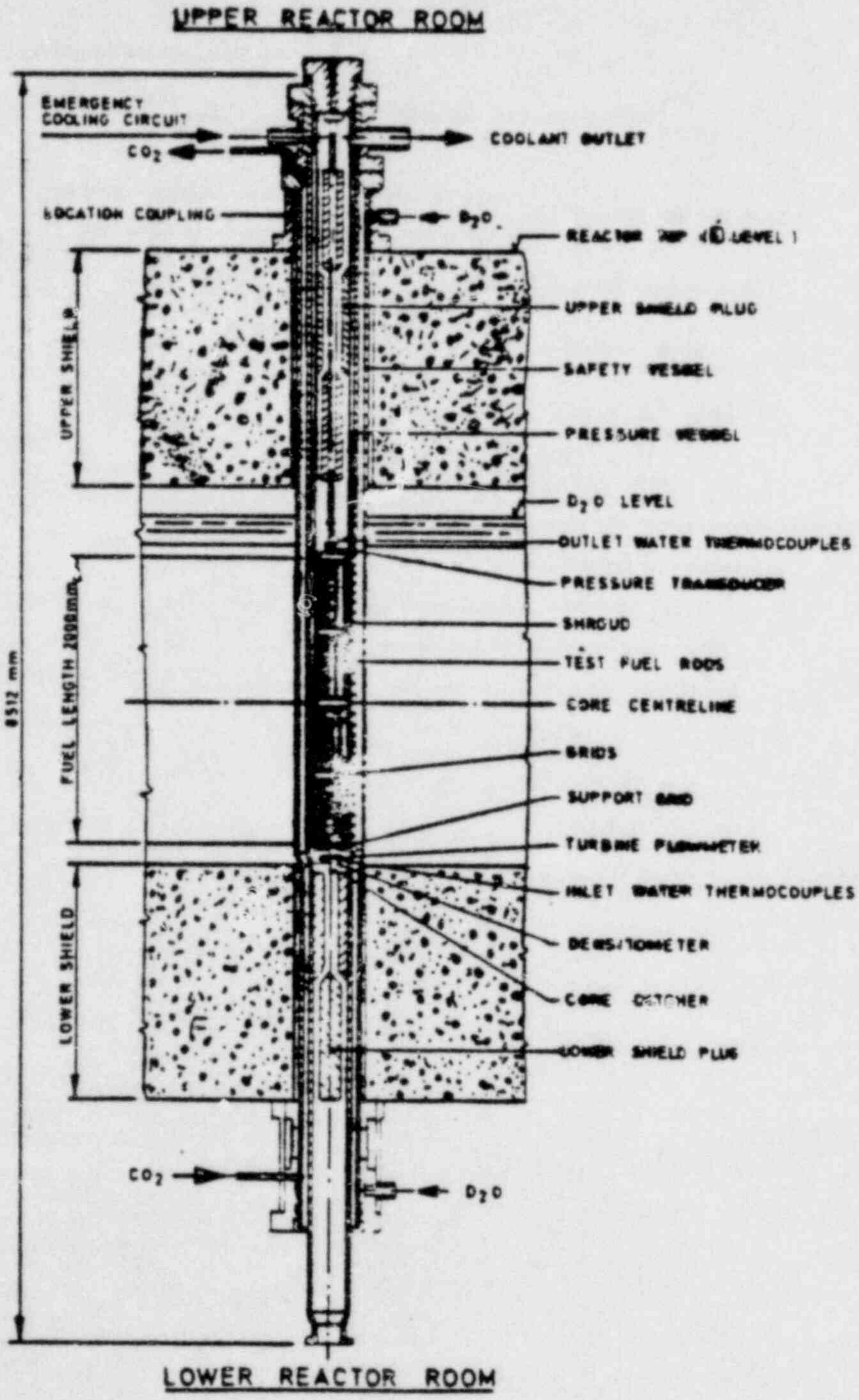
MAIN COMPONENTS

- PRESSURE VESSEL
- SAFETY TUBE
- TEST TRAIN ASSEMBLY INCLUDING FUEL RODS

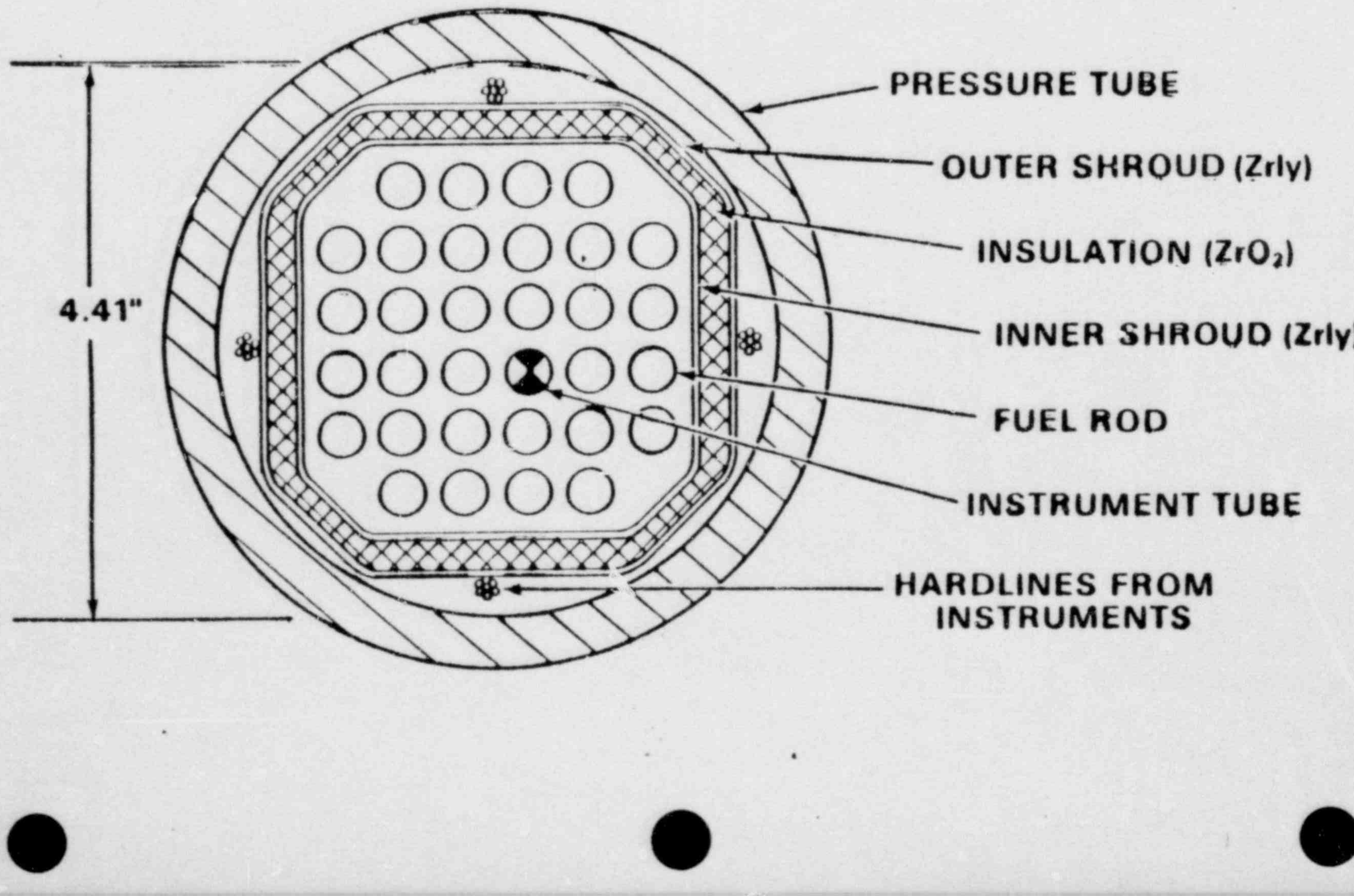
TEST TRAIN ASSEMBLY COMPONENTS

- UPPER STAINLESS STEEL SHIELDING PLUG
- INSTRUMENTED FUEL ROD BUNDLE AND SHROUD FOR TEST CONTROL AND DATA COLLECTION
- LOWER DEBRIS CATCHER DEVICE
- LOWER STAINLESS STEEL SHIELD PLUG

VERTICAL CROSS SECTION OF THE SUPER-SARA IN-PILE TEST SECTION



ESSOR SHROUD TUBE AND INSULATOR CONCEPT (CROSS-SECTION)



OUT-OF-PILE TEST SECTION

PRINCIPAL OBJECTIVE IS THAT, WHERE POSSIBLE, ALL FEATURES OF THE "OUT-OF-PILE TEST SECTION" BE HYDRODYNAMICALLY SIMILAR TO THE "IN-PILE TEST SECTION"

MAIN COMPONENTS

- PRESSURE VESSEL
- ELECTRICALLY HEATED "FUEL ROD" CLUSTER

PRINCIPAL ADVANTAGES OF THE OUT-OF-PILE TEST SECTION

- FACILITATE LOCA CIRCUIT TESTING INCLUDING VALVE SEQUENCING AND RESPONSE TIMES WITHOUT REQUIRING NORMAL REACTOR OPERATION
- CHECK ACTUAL CIRCUIT PERFORMANCE AGAINST THAT PREDICTED BY THEORETICAL ANALYSIS
- TEST AND COMMISSION THE LOCA CIRCUIT AND FACILITATE THE LICENSING PROCEDURES

ESSOR SMALL BREAK

Test Type	1	2	3	4
Test No.	1,2,3*	4,5	6,7	8** thru 13
Rod Behavior	Deformation and Rupture	Oxidation Embrittlement Fracturing Fragmentation	Deformation and Rupture followed by ID/OD Oxidation Fragmentation	Simultaneous Deformation and Oxidation Embrittlement Fracturing Fragmentation

* 3 System pressure oscillating

** 8 Rods preoxidized

SMALL BREAK TEST-TYPE 1 PROPOSED SEQUENCE OF EVENTS

TIME	EVENT/STATUS	CONTROL OPERATION/ PARAMETER	PRIORITY PARAMETERS MONITORED
$t < t_0$	STEADY STATE	MAIN LOOP	MAIN LOOP
t_0	INITIATE TEST	OPEN V10, V34 DECREASE REACTOR POWER CLOSE V2, V3, V764 OPEN V6, V7	ALL
$t_0 < t < t_1$	CLUSTER UNCOVERY (BOIL OFF TO CONSTANT LIQUID LEVEL)	CONTR[illegible] V6, V7 (USE Pclad-Psystem)	LIQUID LEVEL Tcladding Psystem, Pclad-Psystem
$t_1 - \Delta t$	INITIATE REFLOOD	OPEN V23	REFLOOD RATE Tcladding
t_1	CLUSTER UNCOVERY COMPLETE BEGIN SYSTEM STABILIZATION	CONTROL V23, V6, V7 (USE LIQUID LEVEL)	LIQUID LEVEL
$t_1 < t < t_2$	STABILIZATION PERIOD	CONTROL V23, V6, V7 (USE Tcladding, LIQUID LEVEL)	REFLOOD RATE (LIQUID LEVEL) Tcladding Pclad-Psystem
t_2	INITIATE DEPRESSURIZATION	OPEN V6, V7 CONTROL V23 (USE LIQUID LEVEL)	Psystem REFLOOD RATE (LIQUID LEVEL)
$t_2 < t < t_3$	DEPRESSURIZATION	CONTROL V6, V7, V23 (USE Psystem, LIQUID LEVEL)	Psystem REFLOOD RATE (LIQUID LEVEL) Tcladding Pclad-Psystem
t_3	DEPRESSURIZATION COMPLETE	CONTROL V6, V7, V23 (USE Pclad-Psystem, LIQUID LEVEL)	Pclad-Psystem LIQUID LEVEL
$t > t_3$	CLAD DEFORMATION	CONTROL V6, V7, V23 (USE Pclad-Psystem, LIQUID LEVEL	ALL

SMALL BREAK TEST TYPE 3 PROPOSED SEQUENCE OF EVENTS

TIME	EVENT/STATUS	CONTROL OPERATION/ PARAMETER	PRIORITY PARAMETERS MONITORED
$t<t_0$	STEADY STATE	MAIN LOOP	MAIN LOOP
t_0	INITIATE TEST	SAME AS TEST TYPE 1	SAME AS TEST TYPE 1
$t_0<t<t_1$	CLUSTER UNCOVERY (BOIL OFF TO CONSTANT LIQUID LEVEL)	SAME AS TEST TYPE 1	SAME AS TEST TYPE 1
$t_1<t<t_2$	SYSTEM STABILIZATION	SAME AS TEST TYPE 1	SAME AS TEST TYPE 1
$t_2<t<t_3$	DEPRESSURIZATION	SAME AS TEST TYPE 1	SAME AS TEST TYPE 1
$t_3<t<t_4$	SYSTEM STABILIZATION AND CLAD DEFORMATION	SAME AS TEST TYPE 1	SAME AS TEST TYPE 1
t_4	INITIATE TEMPERATURE RAMP	DECREASE REFLOOD RATE (V23) (USE $T_{cladding}$) CONTROL V6, V7	$T_{cladding}$ $P_{clad}-P_{system}$ REFLOOD RATE (LIQUID LEVEL)
$t_4<t<t_5$	TEMPERATURE RAMP	CONTROL V23, V6, V7 (USE $T_{cladding}$, LIQUID LEVEL)	$T_{cladding}$ $P_{clad}-P_{system}$ REFLOOD RATE (LIQUID LEVEL)
t_5	TEMPERATURE RAMP COMPLETE		
$t>t_5$	OXIDATION	CONTROL V23, V6, V7 (USE LIQUID LEVEL, P_{system})	ALL
tmax	TERMINATE TEST	$T_{cladding}$ = MAXIMUM FOR TEST	

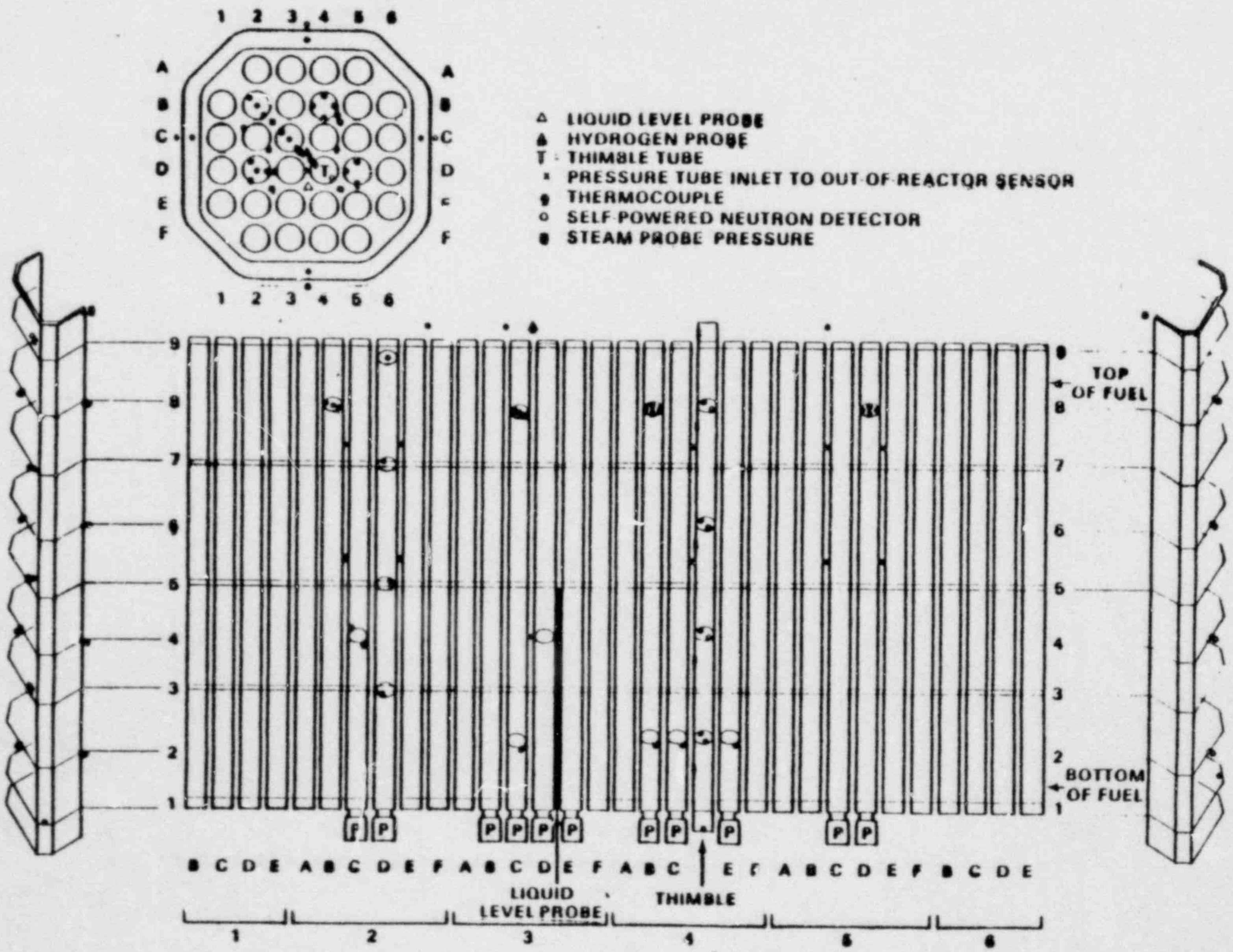
PARAMETERS AVAILABLE DURING TESTING FOR DATA COLLECTION AND EXPERIMENT CONTROL

PARAMETER	DESCRIPTION	POTENTIAL EXPERIMENT CONTROL
PRESSURE	SYSTEM CLADDING	CONTROL DEPRESSURIZATION RATE
	ΔP_c = Pclad-Psystem	CONTROL ΔPc
	ΔP_t = Pinlet-Poutlet (WATER)	PRETEST AND POST TEST WATER FLOW FOR ESTIMATING BLOCKAGE(S) SEVERITY
TEMPERATURE	TEST SECTION INLET AND OUTLET CLADDING AND TEST BUNDLE ARRAY SHROUD REFLOOD INLET DESUPERHEATER SPRAY INLET AND OUTLET	CONTROL REFLOOD RATE HIGH TEMPERATURE ALARM/TEST TERMINATION CONTROL REFLOOD WATER TEMPERATURE
FLOW RATE	MAIN LOOP REFLOOD DESUPERHEATER SPRAY	
LIQUID LEVEL	LOWER PORTION OF TEST SECTION	POTENTIAL CONTROL FOR REFLOOD RATE
POWER	REACTOR TEST SECTION (SPND RADIAL AND AXIAL ARRAY)	

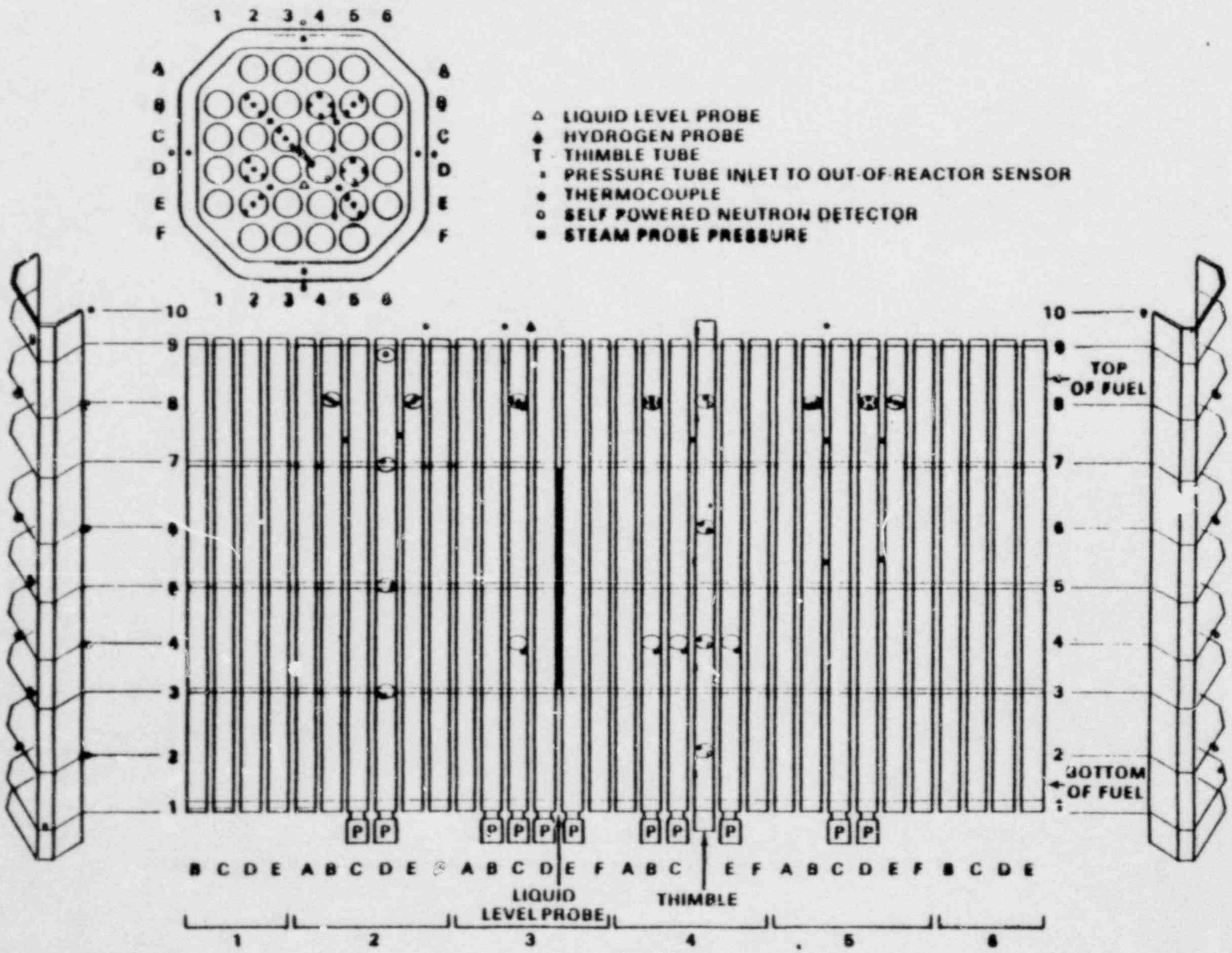
MEASUREMENT NEEDS FOR SMALL BREAK EXPERIMENTS

MEASUREMENT	INSTRUMENTATION
CLADDING TEMPERATURE	EXTERNAL/INTERNAL TC'S
PEAK CLADDING TEMPERATURE	INTERNAL TC'S
FUEL TEMPERATURE	CENTERLINE TC'S
STEAM TEMPERATURE	STEAM PROBE
CLADDING OXIDATION	EDDY CURRENT PROBE
FUEL ROD PRESSURE	PXD'S
SYSTEM PRESSURE	PXD'S
BUNDEL (AXIAL) ΔP	PXD'S
LIQUID LEVEL	PXD'S/HEATED TC'S/FISSION DETECTORS/PERFORATED MgO CABLE
POWER	SPND'S/SPGD'S/FISSION CHAMBERS
FUEL MOVEMENT	FISSION CHAMBERS
H_2 EVOLUTION	ON LINE GAS SAMPLER
STEAM/H_2 VS TIME	GAS SAMPLES
INCIPIENT MELTING	BI METALIC STRIPS
CLADDING DEFORMATION	- - -

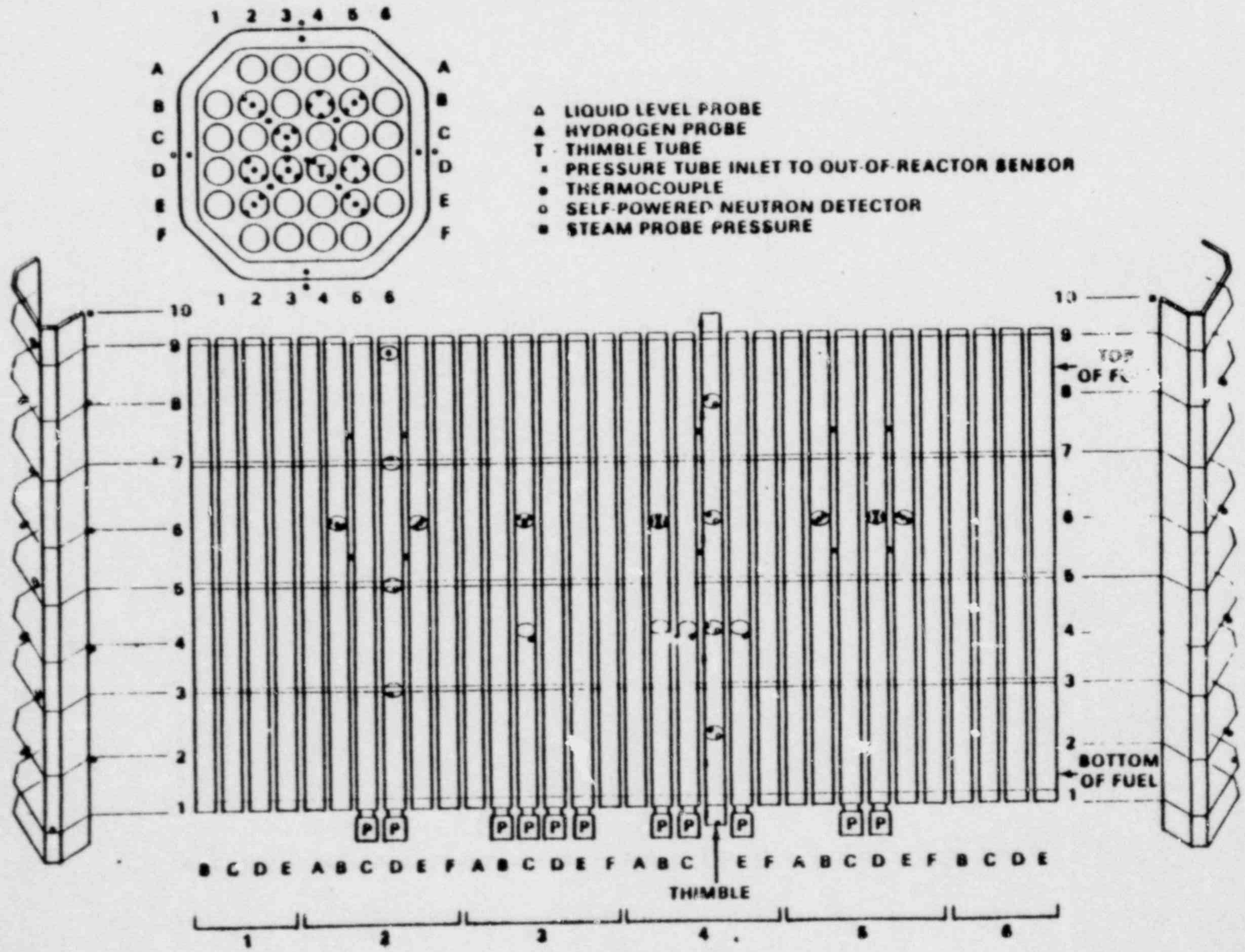
BUNDLE INSTRUMENTATION ESSOR - SMALL AND MEDIUM BREAK-CLAD TEMP. 1360-2170K



BUNDLE INSTRUMENTATION NRC/PNL
ESSOR-SMALL AND MEDIUM BREAK CLAD TEMP < 1100K



BUNDLE INSTRUMENTATION NRC/PNL
ESSOR LARGE BREAK - LOCA - CLAD TEMP. - 1060 - 1500K



NRC/PNL ESSOR-SMALL, MEDIUM, AND LARGE BREAK TESTS (CLAD DEFORMATION AND BALLOONING TESTS) MEASUREMENT REQUIREMENTS

MEASUREMENT	INSTRUMENT	REMARKS
CLAD SURFACE TEMP.	THERMOCOUPLE	INCONEL-600 SHEATH, TYPE K BELOW 1350K; ZIRCALOY SHEATH, W Re TC TO 2100K
FUEL ℄	THERMOCOUPLE	INCONEL-600 SHEATH, TYPE K BELOW 1350K; RE W AUGMENTED SHEATH WRe TC TO 2475K
PLENUM TEMP.	THERMOCOUPLE	INCONEL-600 SHEATH, TYPE K BELOW 1350K; ZIRCALOY SHEATH, WRe TC TO 2100K
PLENUM PRESSURE	EDDY CURRENT TRANSDUCER OR EXTENSION TUBE	TRANSDUCER MUST BE MOUNTED AT BOTTOM OF ROD BECAUSE OF SENSOR TEMPERATURE LIMITATIONS. EXTENSION TUBE ATTACHED TO END CAP AND EXTENDS TO OUT-OF-REACTOR SENSOR
COOLANT TEMP.	THERMOCOUPLE (STEAM PROBE)	INCONEL-600 SHEATH, TYPE K BELOW 1350K; ZIRCALOY SHEATH, WRe TC TO 2100K
COOLANT PRESSURE	PRESSURE TAPS	EXTENSION TUBE TO OUT-OF-REACTOR SENSOR
COOLANT FLOW RATE	TURBINE FLOWMETER	EXTENDED RANGE FLOWMETER LOCATED OUT-OF-REACTOR TO MEASURE REFILL FLOW RATE
NEUTRON DETECTORS	SPNDS	COBALT EMITTER. INCONEL-600 COLLECTOR BELOW 1350K; ZIRCALOY COLLECTOR TO 2160K
LIQUID LEVEL	HEATED TC	INCONEL-600 SHEATH BELOW 1350K. ZIRCALOY SHEATH TO 2100K
HYDROGEN PROBE	—	MUST BE DEVELOPED

TASK D
PRINCIPAL ACCOMPLISHMENTS FY-80 CONT'D

THERMAL-HYDRAULIC ANALYSIS

- DETERMINED STEAM TEMPERATURES AND STEAMING RATES AS A FUNCTION OF ASSEMBLY POWER AND SUPERSARA LOOP — 1/15/80
- OUTLINED STRUCTURE NEEDED FOR A HIGH TEMPERATURE SEVERE DAMAGE FUEL ROD BEHAVIOR CODE — 2/5/80
- USED TRUMP TO EVALUATE RADIAL HEAT LOSSES DUE TO RADIATION IN 32 ROD SUPERSARA TEST BUNDLE — 2/20/80
  (EXAMINED ALTERNATE SHROUD CONCEPTS AND MULTIPLE RADIATION SHIELDS)
- RADIATION LOSS CALCULATIONS AND STEAMING RATES INTEGRATED WITHIN TRUMP — 3/20/80

TASK D
PRINCIPAL ACCOMPLISHMENTS CONT'D

THERMAL-HYDRAULIC ANALYSIS

- DEVELOPED A THREE DIMENSIONAL TRUMP MODEL, USING 1/4 SYMMETRY, INCORPORATING METAL-WATER REACTION AND ROD-STEAM HEAT TRANSFER. THIS MODEL WAS USED TO SELECT A SHROUD DESIGN AND TO REVISE THE PREVIOUS ASSEMBLY POWER ESTIMATES NEEDED TO ACHIEVE PEAK CLADDING TEMPERATURE — 4/3/80
- USED 1/4 SYMMETRY MODEL IN TRUMP TO DETERMINE POWERS NEEDED TO ACHIEVE 2300°K PEAK CLADDIING TEMPERATURES IN A 32 ROD BUNDLE FOR PBF — 4/10/80

TEST TRAIN CONCEPTS

- EVALUATED VARIOUS SHROUD CONFIGURATIONS AND SELECTED MATERIALS CAPABLE OF WITHSTANDING THE MOST SEVERE EXPERIMENTAL CONDITIONS — 3/27/80

TASK D
PRINCIPAL ACCOMPLISHMENTS CONT'D

TEST TRAIN CONCEPTS

- DEVELOPED A TEST TRAIN CONCEPT CAPABLE OF PERFORMING THE FULL SPECTRUM OF SUPERSARA TESTS USING A COMMON SHROUD AND BUNDLE DESIGN AND CHANGEABLE INSTRUMENTATION PACKAGES — 4/10/80
- DEVELOPED A TEST TRAIN CONCEPT SUITABLE FOR PBF SMALL BREAK TEST PROGRAM THAT WOULD UTILIZE THE SAME 32-ROD BUNDLE AND SHROUD DESIGN AS SUPERSARA — 4/12/80

INSTRUMENTATION

- IDENTIFIED FUNDAMENTAL INSTRUMENTATION NEEDS FOR SMALL BREAK TESTS — 2/1/80
- IDENTIFIED SEVERAL NEW MEASUREMENT CONCEPTS AND INSTRUMENTATION DEVELOPMENT REQUIREMENTS — 3/6/80

TASK D
PRINCIPAL ACCOMPLISHMENTS CONT'D

INSTRUMENTATION

- IDENTIFIED INSTRUMENTATION PACKAGES FOR FOUR BASIC TEST CATEGORIES: 4/10/80
  - LARGE BREAK ESSOR TESTS
  - SMALL BREAK ESSOR TESTS $<1100°K$
  - SMALL BREAK ESSOR TESTS $>1350°K$
  - PROPOSED PBF TESTS $\sim 2300°K$

EXPERIMENTAL CONTROL

- METHODS FOR CONDUCTING AND CONTROLLING THE LARGE BREAK ESSOR TESTS WERE FORMULATED 2/15/80
- TEST SEQUENCING AND CONTROL PHILOSOPHIES FOR THE SMALL BREAK ESSOR PROGRAM WERE EVALUATED 4/10/80

TASK D CONT'D

FUTURE MILESTONES

- MEET WITH ESSOR STAFF TO DISCUSS PROGRAM OBJECTIVES AND SET SCHEDULES
- ISSUE A REPORT ON SMALL BREAK TEST TRAIN CONCEPTS
- ISSUE A REPORT ON THE THERMAL ANALYSIS AND POWER REQUIREMENTS FOR THE SUPERSARA TEST MATRIX
- EVALUATE FEASIBILITY OF NEW INSTRUMENTATION CONCEPTS
- PERFORM PRELIMINARY MATERIAL STUDIES ON SHROUDS AND TEST TRAIN STRUCTURAL COMPONENTS
- EVALUATE DE-SUPERHEATER AND FALL BACK BARRIER CONCEPTS
- PREPARE A PROJECT QUALITY PLAN
- ISSUE A DESIGN BASIS AND CRITERIA REPORT FOR 32 ROD SMALL BREAK TEST TRAINS
- ISSUE A DESIGN SUMMARY AND FABRICATION PLAN

INDEX

	CLASSIFICATION: 11.1
TITLE (ORIGINAL LANGUAGE): Severe Core Damage Tests in ESSOR Super Sara Test Loop	COUNTRY: USA
	SPONSOR: USNRC
TITLE (ENGLISH LANGUAGE): Severe Core Damage Tests in ESSOR Super Sara Test Loop	ORGANISATION: Pacific Northwest Lab., BMI
	PROJECT LEADER: R. Van Houten (NRC)
INITIATED: 1977 / COMPLETED: 1986 / STATUS: In Progress / LAST UPDATING: August 1980	SCIENTISTS: E. Courtright, F. Panisko, J. Pilger, G. Hesson, M. Cunningham

Description

1. General aim:

   a. To provide support for the Euratom Joint Research Center (JRC) ESSOR Super-Sara Severe Core Damage Fuel Assembly Test Program (SSTP).

   b. To provide information for evaluating fuel cluster damage behavior during the course of an accident such as TMI.

2. Particular objectives:

   In-reactor severe core damage tests are needed to characterize the extremes of clad ballooning in severe core damage and to provide information on the chemical and physical nature of the axial interactions between high temperature fuel and cladding and grid spacers. These data are needed to evaluate fuel cluster damage during the course of an accident such as Three Mile Island (TMI) and to provide guidance in selecting allowable safe shutdown and recovery procedures. The ESSOR tests will provide the first data to confirm axial fuel damage interactions postulated from results of short core fuel damage tests such as those to be performed in PBF.

3. Experimental facilities and programs:

   The ESSOR is a heavy-water moderated reactor. The Super-Sara Test Loop (SSTL) has a vertical in-reactor test section capable of holding a 36-rod assembly of PWR-type test rods with a fueled length of up to 2 meters. The SSTL is equipped for testing at pressures to 15 MPa and above; at temperatures and power levels typical of current commercial PWR and BWR reactors. The loop is designed to perform a variety of severe core damage tests, including small-break LOCA, LOCA, and flow-blockage tests. The loop is equipped with a side leg fitted with an analog electrically heated fuel rod simulator bundle for establishing thermal hydraulic parameters for the in-reactor nuclear heated test bundles.

Accepted test plan guidelines show 5 to 7 severe core damage large break LOCA tests and 13 to 15 severe core damage small break TMI-type tests. The large break tests will emphasize the study of clad failure under extreme clad ballooning accident conditions. The small break tests will examine the conditions at which clad melting is initiated with clad temperatures to 2300K and above. The tests will also examine the axial variations in the reactions between steam and the clad, the fuel, the grid spacers, the control rod materials and other structures, and the effects of quenching on the oxidized structure. The resultant debris beds will be characterized.

4. Project status:

Progress to date - Hardware for the test loop has been designed and the major loop components are on order. Test loop completion is scheduled for October 1982 and preprogram nuclear checkouts will be completed by July 1983. The first program test is scheduled for late Summer 1983 and the test series is scheduled for completion in mid-1986.

5. Next steps:

The need for performing one or more BWR fuel cluster flow blockage tests will be evaluated.

6. Relation to other projects and codes:

This is part of the overall NRC program to study severe core damage accidents and establish resultant fission product release source terms.

7. Reference documents:

J. Randles, et.al., "The Super-Sara Test Program," in preparation.

8. Availability:

Euratom JRC, Ispra, Italy

#6

Thermal Fuels Behavior Program

PBF Fission Product Detection System

Presented by
D.J. Osetek
Project Leader

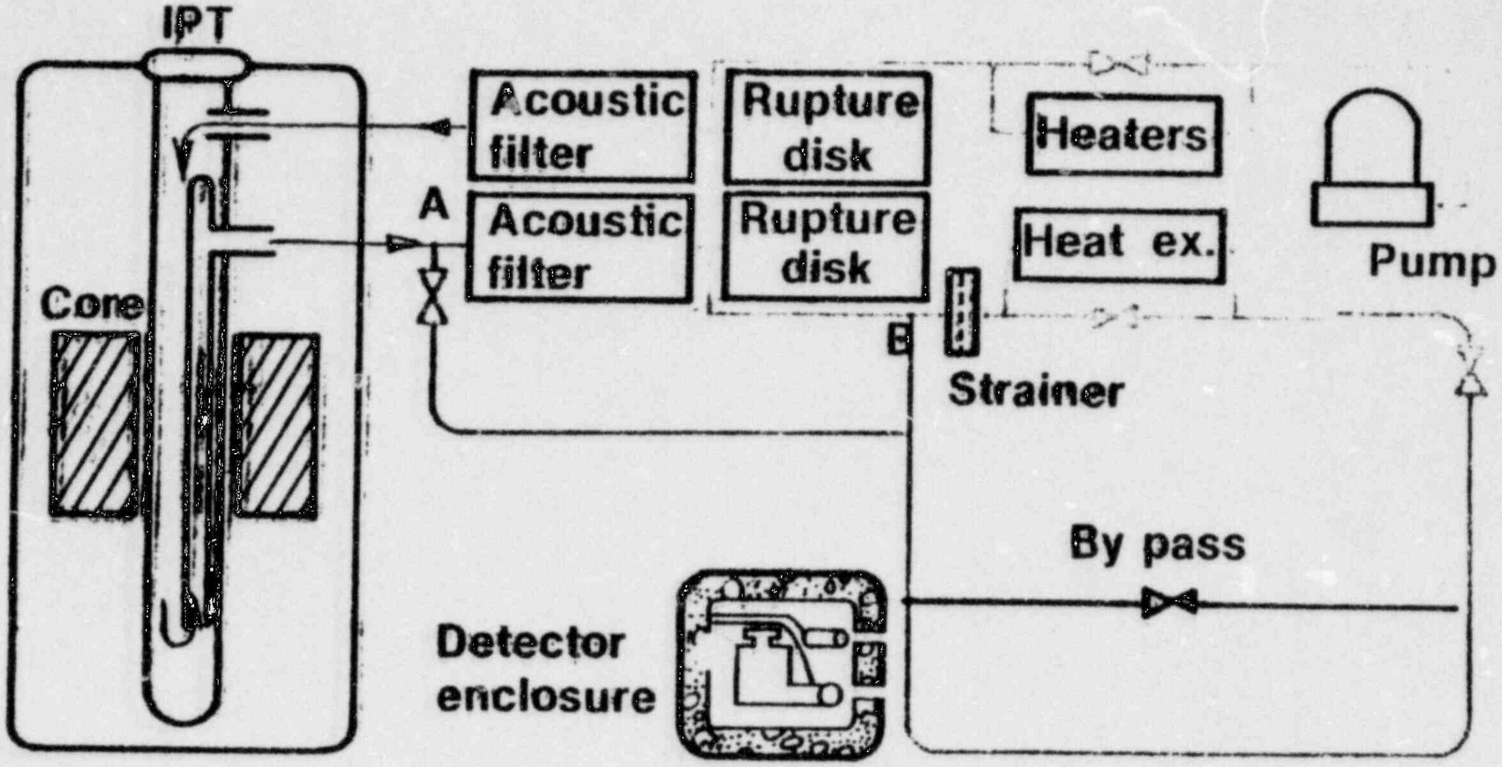
ENERGY SAFETY ENVIRONMENT
IDAHO NATIONAL ENGINEERING LABORATORY

EG&G Idaho, Inc.

PROJECT OBJECTIVES

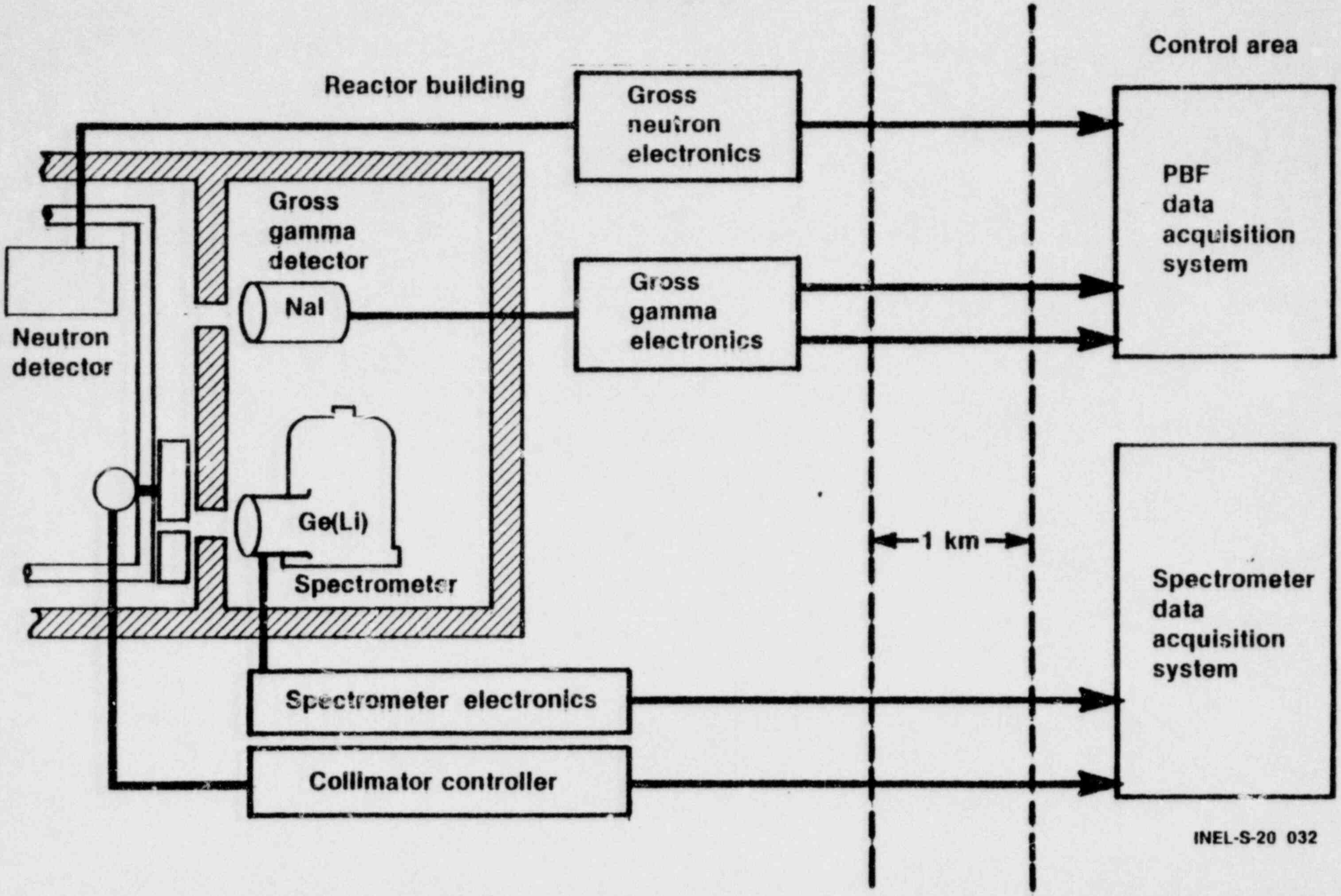
- PROVIDE TEST SUPPORT
- DOCUMENT FISSION PRODUCT BEHAVIOR
- BUILD DATA BASE FOR:
    - ACCIDENT CONSEQUENCE ESTIMATES
    - CODE ASSESSMENT
    - FUEL CONDITION MONITOR

PBF Test Loop Schematic with FPDS



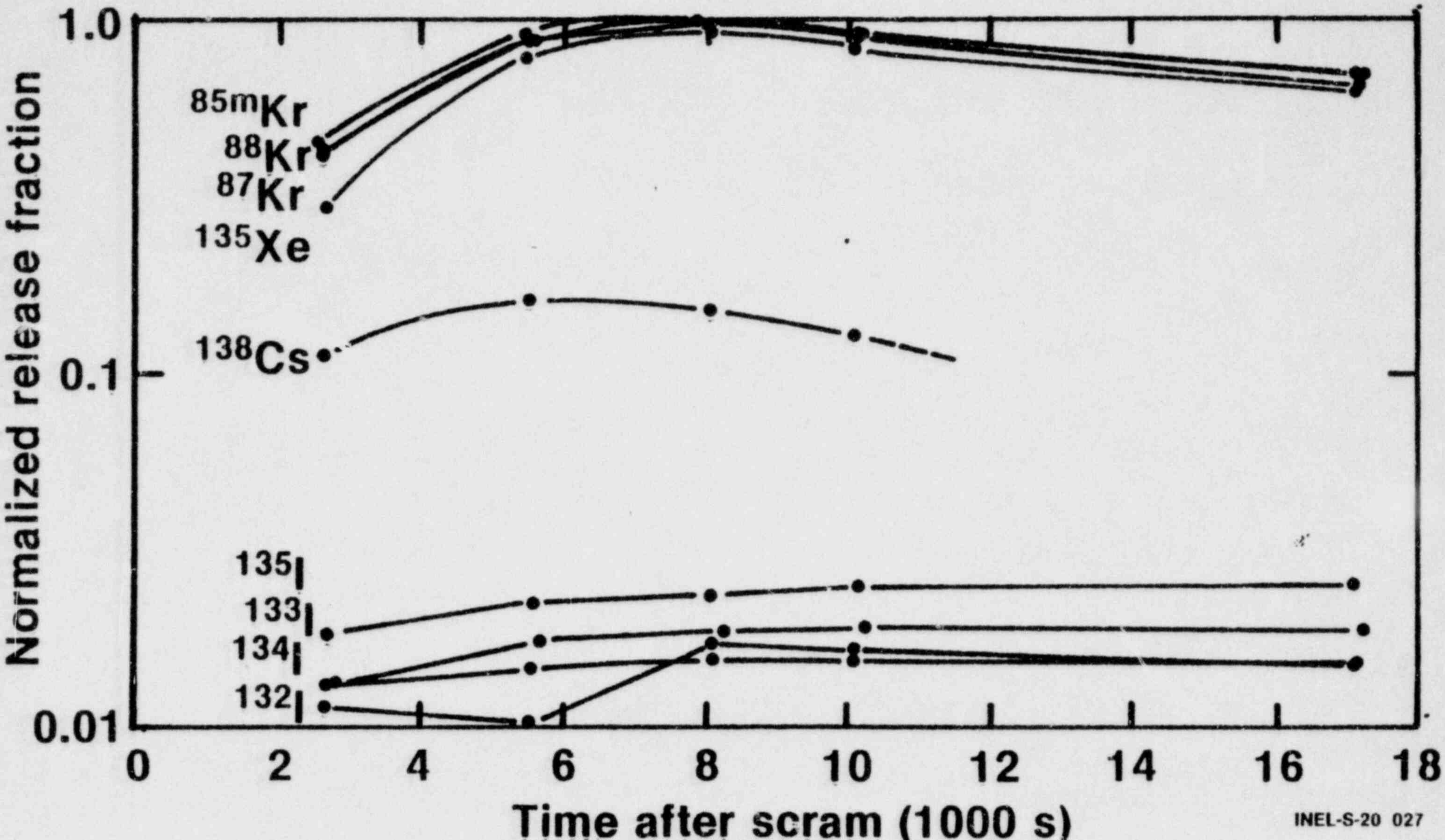
INEL-S-20 030

Fission Product Detection System Instrumentation



INEL-S-20 032

PCM-1



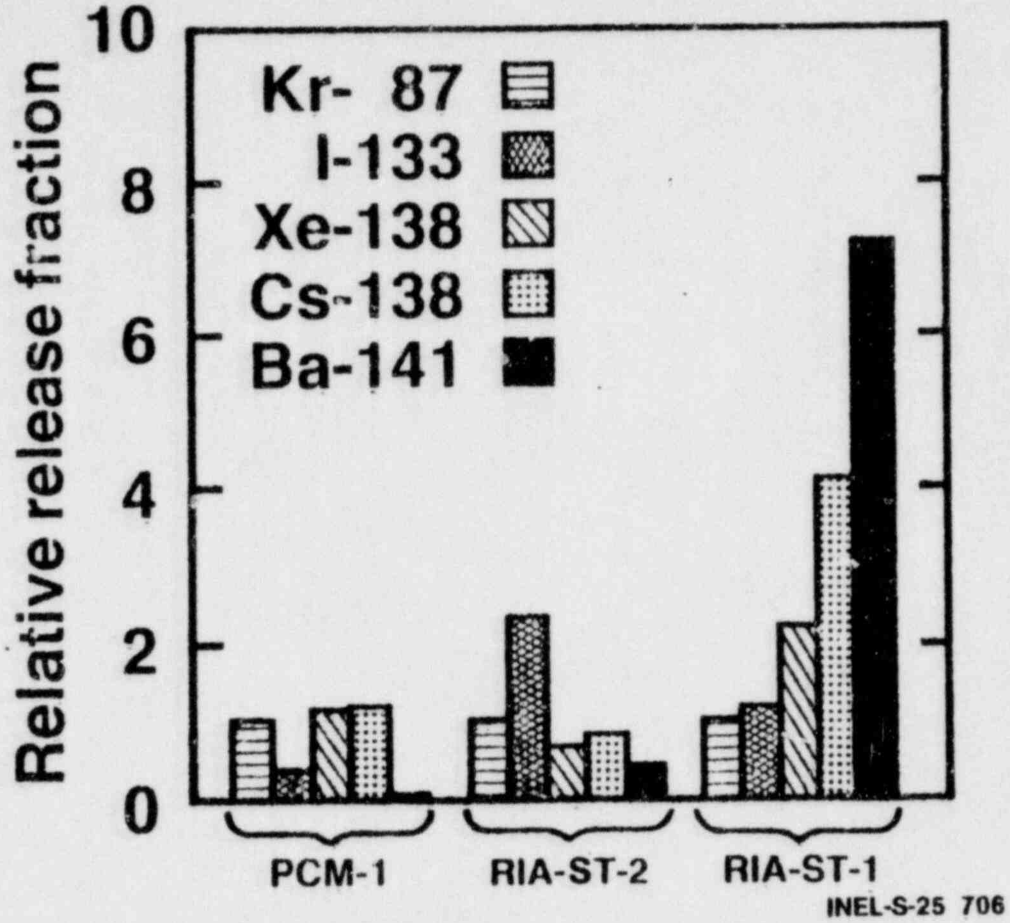
INEL-S-20 027

Fuel Rod Damage

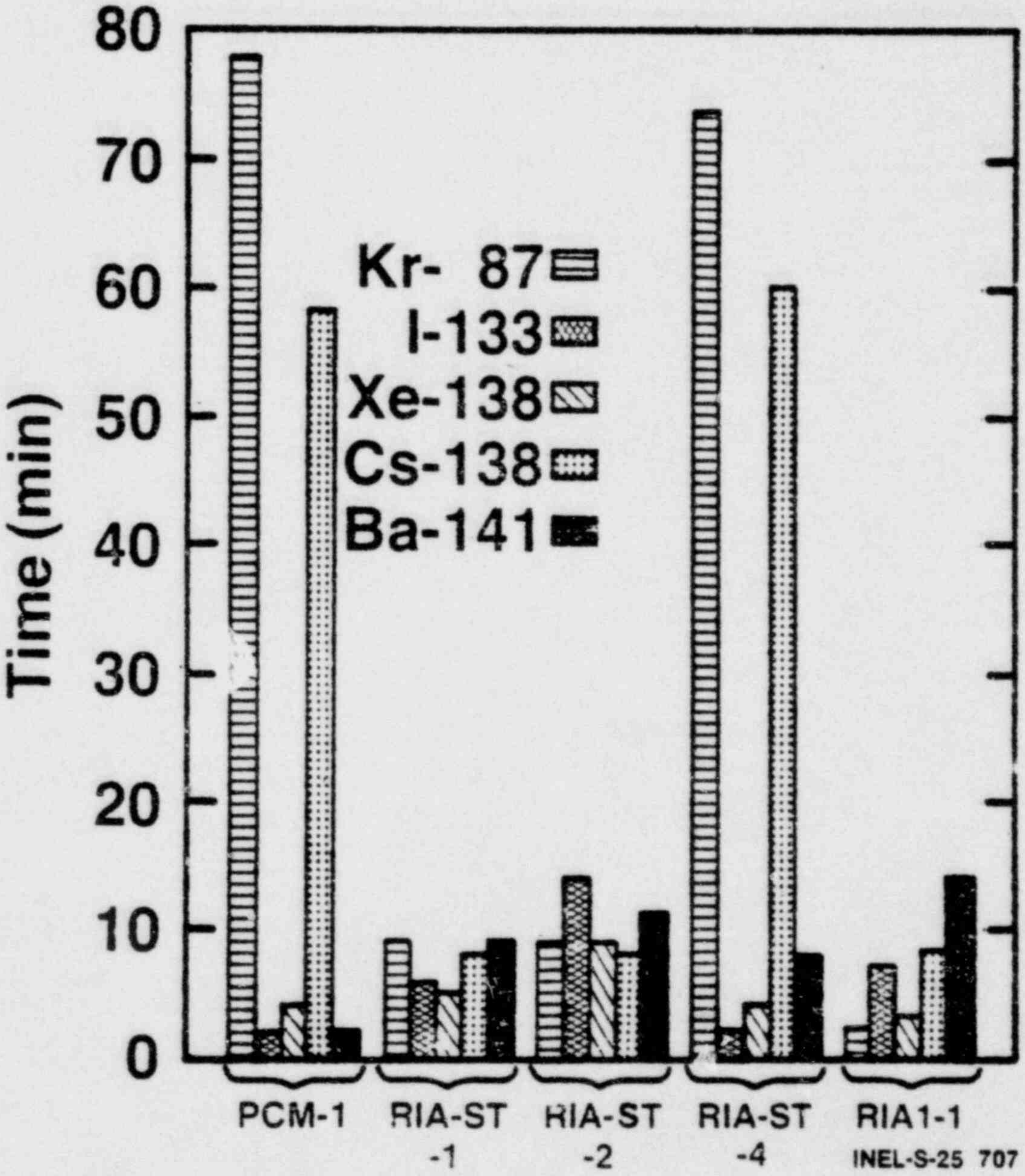
Test	Max Fuel Temp (K)	Fuel Loss (%)	Melt Vol (%)
PCM-1	3100	24	25
RIA ST-1	3000	10	0
RIA ST-2	3000	15	0
RIA ST-4	3500	<1	>90
RIA 1-1	3100	0-27	1-4

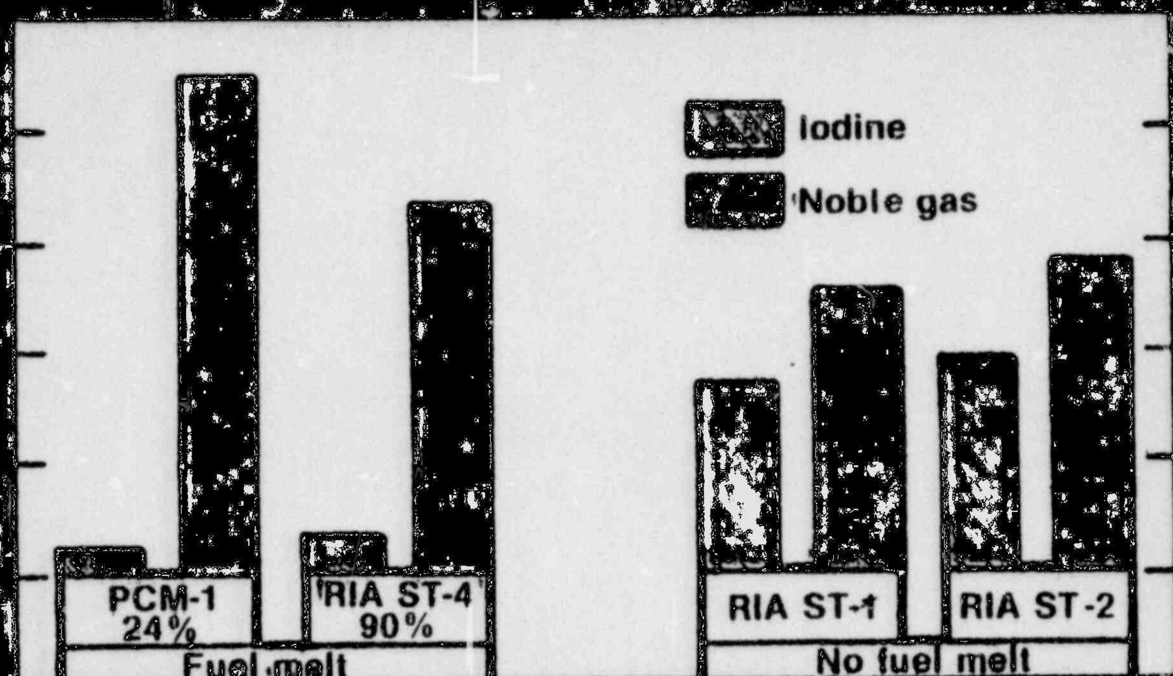
INEL-S-25 710

Relative Peak Burst Release Fraction Normalized to Kr-87



Time After Failure of Maximum Activity Concentration (min)




Iodine
Noble gas
PCM-1
24%
RIA ST-4
90%
Fuel melt
RIA ST-1
RIA ST-2
No fuel melt

INEL-S-27 005

PBF FISSION PRODUCT DETECTION SYSTEM

CONCLUSIONS

- FISSION PRODUCT RELEASE SIGNATURES ARE FUEL BEHAVIOR DEPENDENT AND MEASURABLE USING SPECIALIZED MONITORING TECHNIQUES

- FISSION PRODUCT RELEASE MEASUREMENTS FROM PBF TESTS PROVIDE IMPORTANT CONTRIBUTIONS TO THE DATA BASE FOR:

    - ESTIMATING ACCIDENT SOURCE TERMS

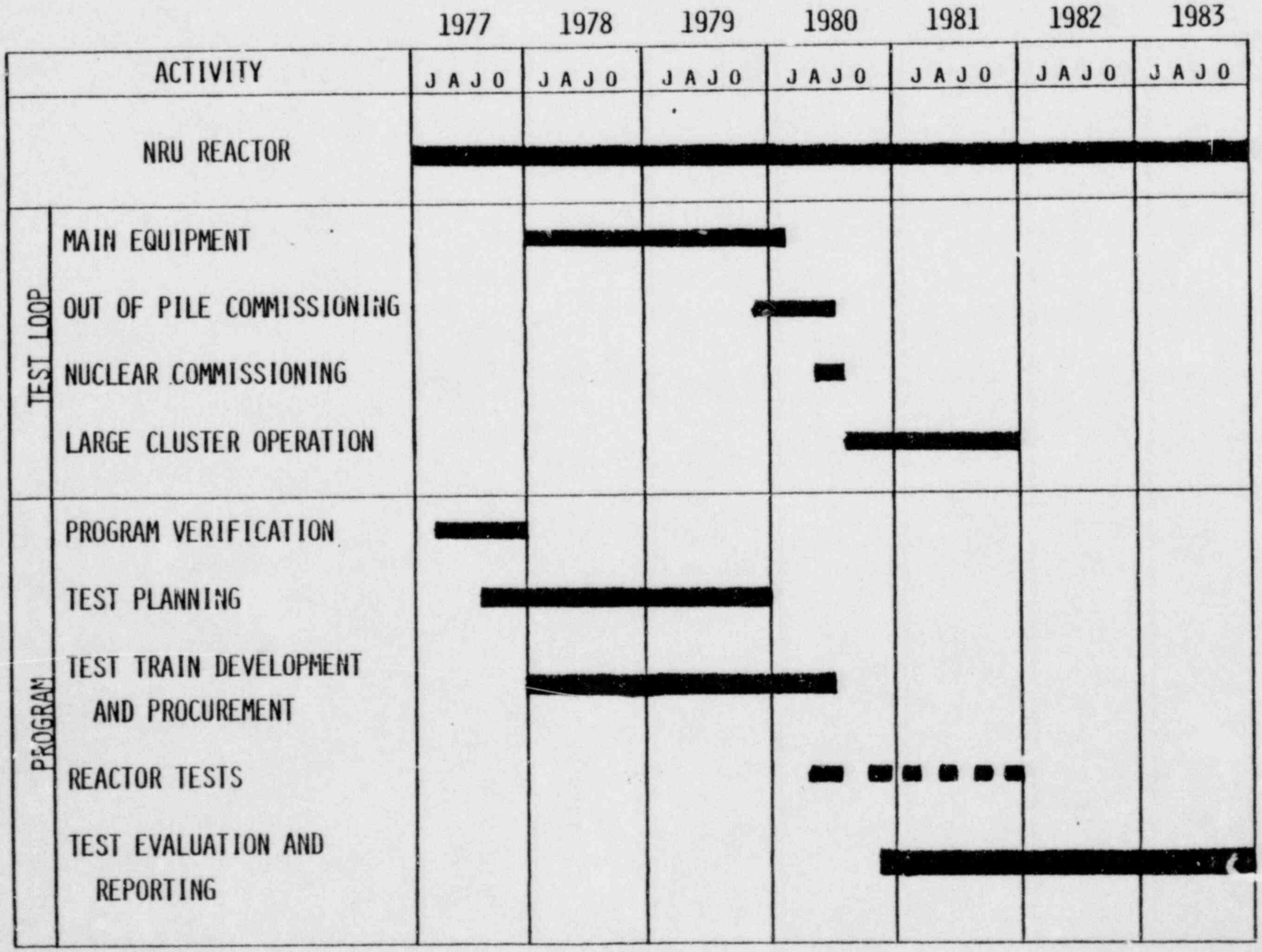
    - EVALUATING SAFETY MARGINS IN EXISTING REGULATORY GUIDES AND FEDERAL REGULATIONS

    - PREPARING SAFETY ANALYSIS REPORTS AND NEW REGULATORY GUIDES FOR ACCIDENT EVALUATIONS

    - ASSESSING FISSION PRODUCT BEHAVIOR CODES

    - ESTIMATING CORE FUEL CONDITIONS FROM ON-LINE MEASUREMENTS

5 Cont.
Van Houten



LOCA SIMULATION IN NRU

OBJECTIVES

- DEVELOP A WELL CHARACTERIZED DATA SET FOR FULL LENGTH, MULTIROD BUNDLES UNDER REPRESENTATIVE HEATUP AND REFLOOD CONDITIONS

10 CFR - COMPARISON WITH NRU TEST RESULTS

1.	10 CFR 50.46 (b)(1)	Peak Cladding Temperature
2.	10 CFR 50.46 (b)(2)	Maximum Cladding Oxidation
3.	10 CFR 50.46 (b)(3)	Maximum Hydrogen Generation
4.	10 CFR 50.46 (b)(4)	Coolable Geometries
5.	10 CFR 50.46 (b)(5)	Long Term Cooling
6.	10 CFR 50.46 (b)(5)(2)	Evaluation Model Assessment
7.	10 CFR 50 Appendix (K)(I)(A)(2)	Fission Heat
8.	10 CFR 50 Appendix (K)(I)(A)(5)	Metal Water Reaction
9.	10 CFR 50 Appendix (K)(I)(B)	Swelling and Rupture of the Cladding and Fuel Rod Thermal Parameters
10.	10 CFR 50 Appendix (K)(I)(C)(1) (C)(2)	End of Blowdown (Droplet Entrainment)
11.	10 CFR 50 Appendix (K)(I)(C)(2)	Frictional Pressure Drops (Reactor Core) Two Phase Friction Multipliers used to Computer Maximum Clad Temperature
12.	10 CFR 50 Appendix (K)(I)(C)(3)	Momentum Equation Within Fuel Bundle During Reflood
13.	10 CFR 50 Appendix (K)(I)(C)(5)(a)	Post CHF Heat Transfer Correlations
14.	10 CFR 50 Appendix (K)(I)(D)(2)	Post Blowdown Phenomena; Heat Removal by the ECCS Containment Pressure
15.	10 CFR 50 Appendix (K)(I)(D)(3)	Calculation of Reflood Rate for Pressurized Water Reactors
16.	10 CFR 50 Appendix (K)(I)(D)(4)	Steam Interaction with Emergency Core Cooling Water in Pressurized Water Reactors
17.	10 CFR 50 Appendix (K)(I)(D)(5)	Refill and Reflood Heat Transfer for Pressurized Water Reactors

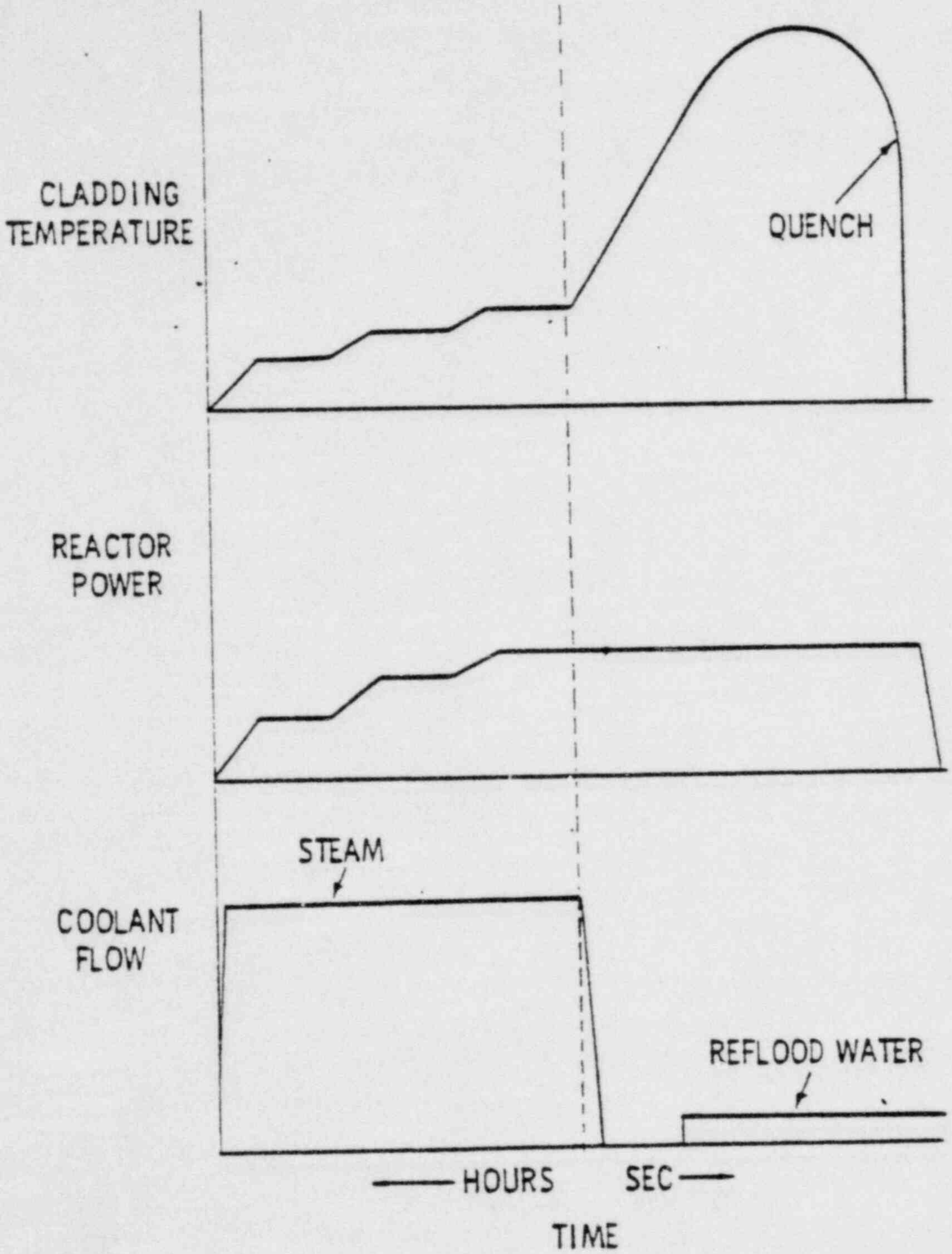
SCHEDULE

October 1980	THERMAL-HYDRAULIC TEST
January 1981	MATERIALS TEST NO. 1
March 1981	MATERIALS TEST NO. 2
May 1981	MATERIALS TEST NO. 3
July 1981	MATERIALS TEST NO. 4
October 1981	MATERIALS TEST NO. 5

SCHEDULE B
PLANNED FUNDING

OCTOBER 1980	THERMAL HYDRAULIC TEST
FEBRUARY 1981	MATERIALS TEST 1
DECEMBER 1981	MATERIALS TEST 2
JULY 1982	MATERIALS TEST 3
SEPTEMBER 1982	MATERIALS TEST 4
DECEMBER 1982	MATERIALS TEST 5

HEATUP/REFLOOD PHASE



TEST MATRIX

TEST NUMBER	NO. OF TRANSIENTS	PRESSURIZED RODS	TYPE
1	18	0	THERMAL HYDRAULIC TEST SERIES
2	1	11	CLADDING PERFORMANCE TEST SERIES
3	1	11	
4	1	11	
5	1	11	
6	1	11	

TEST DESCRIPTION

THERMAL HYDRAULIC TEST SERIES

- 18 TESTS PLANNED USING 1 TEST TRAIN
- PROCEED FROM MOST SECURE LOW TEMPERATURE TEST TO MORE SEVERE HIGH TEMPERATURE CONDITIONS
- EVALUATE RELATIONSHIP BETWEEN PREDICTED BEHAVIOR VERSUS ACTUAL
- SAFETY ASSESSMENT BASED ON A "LEARN AS YOU GO BASIS"

NRU LOCA PROGRAM
MATRIX FOR THERMAL-HYDRAULIC TEST SERIES

TEST NUMBER	FLOODING RATE (in./sec)	REFLOOD DELAY TIME (sec)	HEATING RATE (^{0}F/sec)	CLADDING TEMP. MAX (^{0}F)	PEAK ROD POWER (kW/ft)	PRESSURE (PSIA)	INLET SUBCOOLING (^{0}F)
1-1	10	0	15	< 1400	0.6	40	140
1-2	5	0	15	< 1400	0.6	40	140
1-3	2	0	15	< 1400	0.6	40	140
1-4	5	10	15	< 1400	0.6	40	140
1-5	5	25	15	< 1400	0.6	40	140
1-6	10	34*	15	< 1400	0.6	40	140
1-7	2	10	15	< 1400	0.6	40	140
1-8	1.25*	0	15	1600	0.6	40	140
1-9	5	42*	15	1600	0.6	40	140
1-10	2	25	15	1600	0.6	40	140
1-11	1.45*	10	15	1600	0.6	40	140
1-12	0.95*	0	15	1800	0.6	40	140
1-13	5	58*	15	1800	0.6	40	140
1-14	2	40*	15	1800	0.6	40	140
1-15	1.45	25	15	1800	0.6	40	140
1-16	5	32*	15	1400	0.6	40	140
1-17	2	14*	15	1400	0.6	40	140
1-18**	2	10	15	< 1400	0.6	40	140

*VALUE SELECTED FROM EARLIER TESTS IN THIS SERIES

**REPLICATE OF TEST NUMBER 7

PROTOTYPIC[a] THERMAL-HYDRAULIC TEST SERIES PLAN

TEST DAY	TEST SERIES NUMBER	REFLOOD RATE m/s	REFLOOD RATE in./s	REFLOOD DELAY TIME, s	HEATING RATE K/s	HEATING RATE °F/s	MAXIMUM CLADDING TEMPERATURE K	MAXIMUM CLADDING TEMPERATURE °F
2	101	0.254	10	0	8	15	1033	1400
2	102	0.127	5	0	8	15	1033	1400
3	103	0.051	2	0	8	15	1033	1400
3	104	0.127	5	10	8	15	1033	1400
3	105	0.127	5	25	8	15	1033	1400
4	106	0.254	10	34[b]	8	15	1033	1400
4	107	0.054	2.1	10	8	15	1033	1400
4	108	0.037	1.45[b]	0	8	15	1144	1600
5	109	0.127	5	42[b]	8	15	1144	1600
5	110	0.051	2	25	8	15	1144	1600
5	111	0.042	1.65[b]	10	8	15	1144	1600
6	112	0.127	5	58[b]	8	15	1255	1800
6	113	0.061	2.4	60[b]	8	15	1255	1800
6	114	0.039	1.55[b]	25	8	15	1255	1800
7	115	0.028	1.10[b]	0	8	15	1310[c]	1900[c]
7	116	0.127	5	32[b]	8	15	1033	1400
7	117	0.051	2.25	14[b]	8	15	1033	1400
7	118	0.051	2.1	10	8	15	1033	1400
7	119 - 124[e]			TBD				

(a) IN ALL TESTS, THE PEAK TEST FUEL ROD POWER IS 1.80 kW/m (0.55 kW/ft), SYSTEM OUTLET PRESSURE IS 0.28 MPa (40 psia), INLET REFLOOD SUBCOOLING TEMPERATURE IS 78K (140°F) AND THE INITIAL FUEL ROD PRESSURE IS 0.10 MPa (14.7 psia) AT STP.

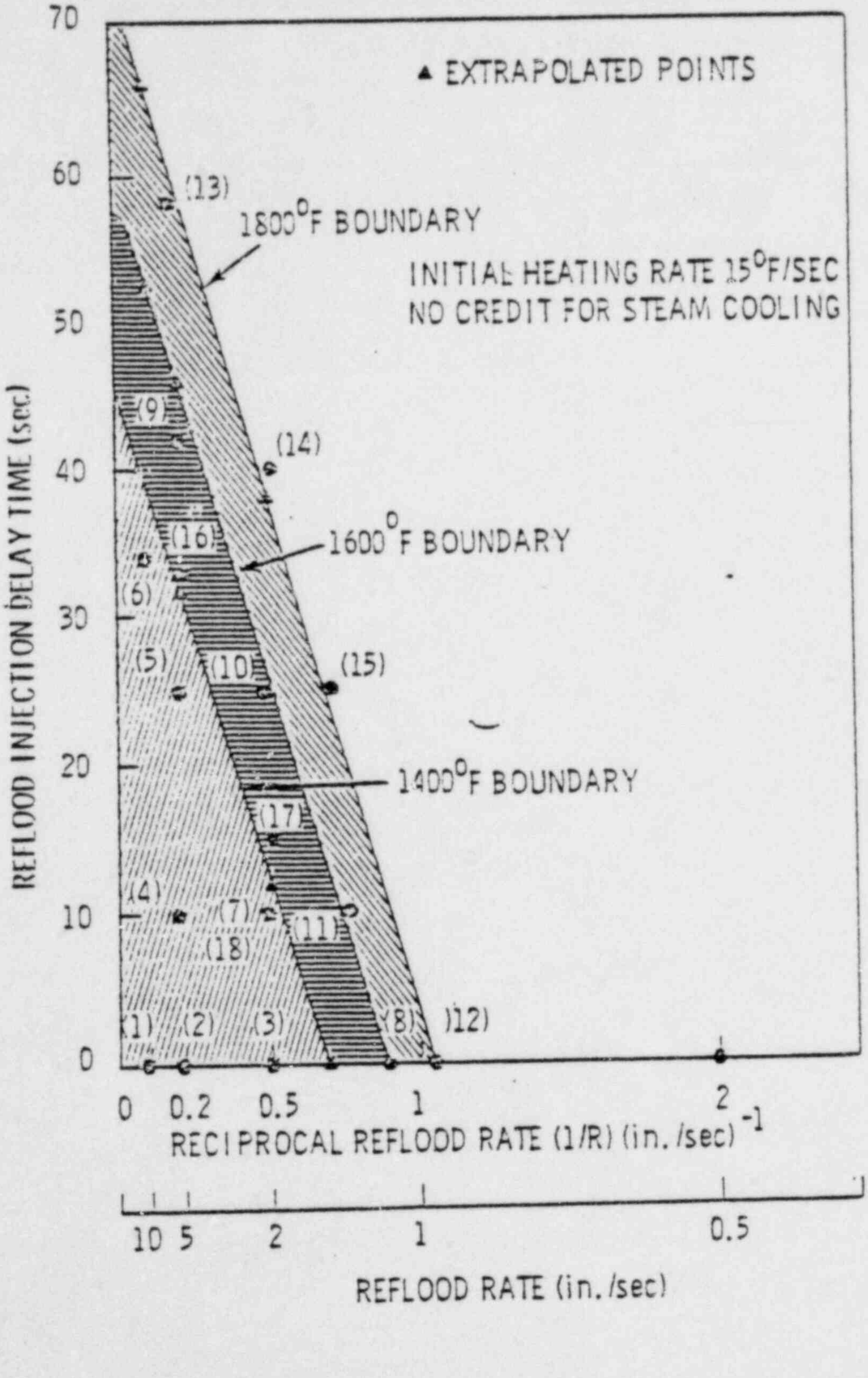
(b) FINAL VALUE SELECTED FROM EARLIER TESTS IN THIS EXPERIMENT.

(c) CLADDING TEMPERATURE MAY EXCEED 1255K (1800°F) BASED ON PARAMETERS EXTRAPOLATED FROM EARLIER TESTS IN THIS EXPERIMENT. FOR SAFETY PURPOSES 1310K (1900°F) IS USED AS THE MAXIMUM.

(d) REPLICATE OF TEST NUMBER 1-7. 1310K (1900°F) IS USED AS THE MAXIMUM.

(e) TO BE DEFINED BY TEST RESULTS OF, AND WITHIN THE OPERATING ENVELOPE CONDITIONS OF PREVIOUS TESTS IN THIS SERIES (IF TIME IS AVAILABLE).

TEST SEQUENCE MAP FOR NRU LOCA THERMAL-HYDRAULIC EXPERIMENTS



CLADDING MATERIALS DEFORMATION TESTS 1-5
TEST OBJECTIVES

- EVALUATE THE EFFECTS OF DEFORMATION ON REFLOOD HEAT TRANSFER CHARACTERISTICS
  APPENDIX (K) (I) (D) (5), APPENDIX (K) (I) (C) (5) (a)
  AND APPENDIX (K) (I) (B)

- EVALUATE THE EFFECTS OF RUPTURE IN THE α, $\alpha + \beta$, β TEMPERATURE RANGES

- EVALUATE THE ID/OD CLADDING OXIDATION
  10CFR 50.46 (b) (2)

- EVALUATE THE FUEL ROD LENGTH EFFECTS AND NUCLEAR HEATING EFFECTS ON BLOCKAGE
  APPENDIX (K) (I) (B) AND APPENDIX (K) (I) (C) (2)

- EVALUATE THE EFFECTIVE REFLOOD AND QUENCH RATES
  APPENDIX (K) (I) (D) (3)

TEST DESCRIPTION

MATERIALS TEST SERIES

- 5 TESTS USING 5 TEST TRAINS WITH SOME REUSED COMPONENTS
- COVER TEMPERATURE RANGE OF α TO β
- USE SELECTED REFLOOD AND DELAY TIMES
- SAFETY ANALYSIS BASED ON THERMAL HYDRAULIC TEST RESULTS

MAJOR SAFETY ISSUES

ADDRESSED BY THE NRU PROGRAM

- Address 17 Items in 10 CFR
- Peak Cladding Temperature 50.46 (b) (1)
- Evaluation Model Assessment 50.46 (b) (5) (2)
- Swelling and Rupture Appendix (K) (I) (B)
- Flow Blockage Appendix (K) (I) (C) (2)
- Droplet Entrainment Appendix (K) (I) (C) (1) (C) (2)
- Reflood Rate Appendix (K) (I) (D) (3)
- Refill and Reflood Heat Transfer Appendix (K) (I) (D) (5)

GENERAL SAFETY ISSUES

- PEAK CLADDING TEMPERATURE PREDICTION
- REFLOOD/QUENCH CHARACTERISTICS OF OXIDIZED ZIRCALOY RODS
- FULL LENGTH SIMULATION WITH GRID SPACERS
- FLOW BLOCKAGE AND CLADDING DEFORMATION
- DELAY TIME AND REFLOOD RATE PARAMETERS
- LIQUID ENTRAINMENT DURING QUENCH

THERMAL-HYDRAULIC

- QUENCHING OF ZIRCALOY VERSUS INCONEL
- QUENCH FRONT VELOCITY VERSUS LIQUID LEVEL
- ENTRAINMENT VERSUS CLADDING TEMPERATURE
- GRID SPACER EFFECTS
- PEAK CLADDING TEMPERATURE
- STEAM TEMPERATURE
- HEAT TRANSFER COEFFICIENTS DURING REFLOOD
- OXIDATION EFFECTS ON PEAK CLAD TEMPERATURE

THERMAL HYDRAULIC TEST
OBJECTIVES

- DEVELOP RELATIONSHIP BETWEEN HEAT TRANSFER ON QUENCH, REFLOOD RATE, AND TIME TO INTRODUCE REFLOOD WATER ON UNDEFORMED BUNDLES.
  10CFR50 APPENDIX (K) (I) (D) (5)

- EVALUATE THE FULL LENGTH EFFECTS TO COMPARE WITH PBF AND MRBT RESULTS.

- COMPARE EVALUATION MODEL WITH EXPERIMENTAL RESULTS.
  10CFR 50.46 (b) (5) (2)

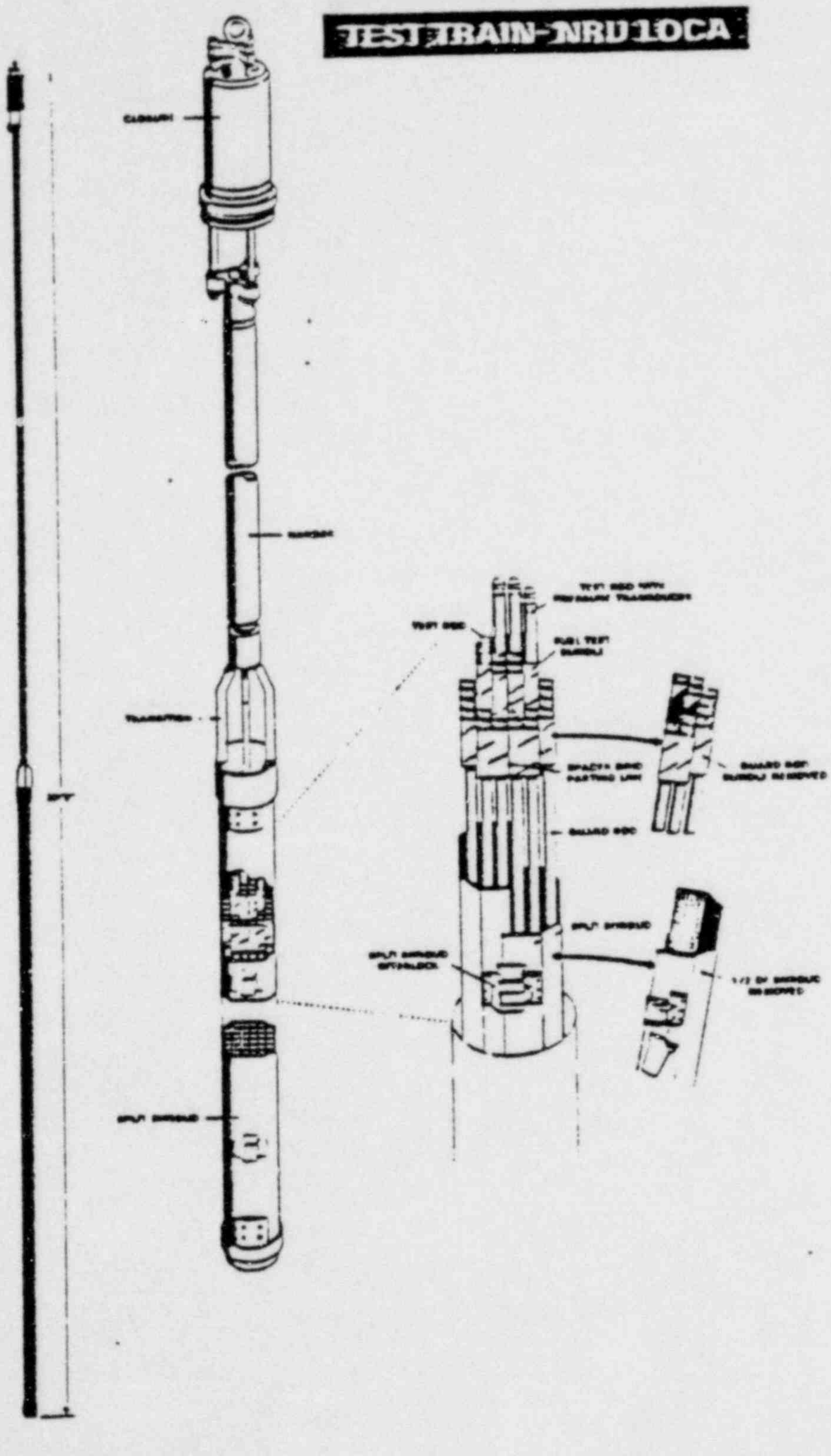
- DEVELOP RELATIONSHIP OF THERMAL HYDRAULIC BEHAVIOR FOR USE IN SELECTING CLADDING MATERIALS DEFORMATION TEST CONDITIONS (5 TESTS)

MATERIALS

- RUPTURE CHARACTERISTICS
- BALLOONING AND BLOCKAGE
- AZIMUTHAL TEMPERATURE EFFECTS ON RUPTURE STRAIN
- FUEL RELOCATION EFFECTS ON QUENCHING
- GRID SPACER EFFECTS ON BALLOONING

PROJECT DESIGN FEATURES

TEST TRAIN-NRU LOCA

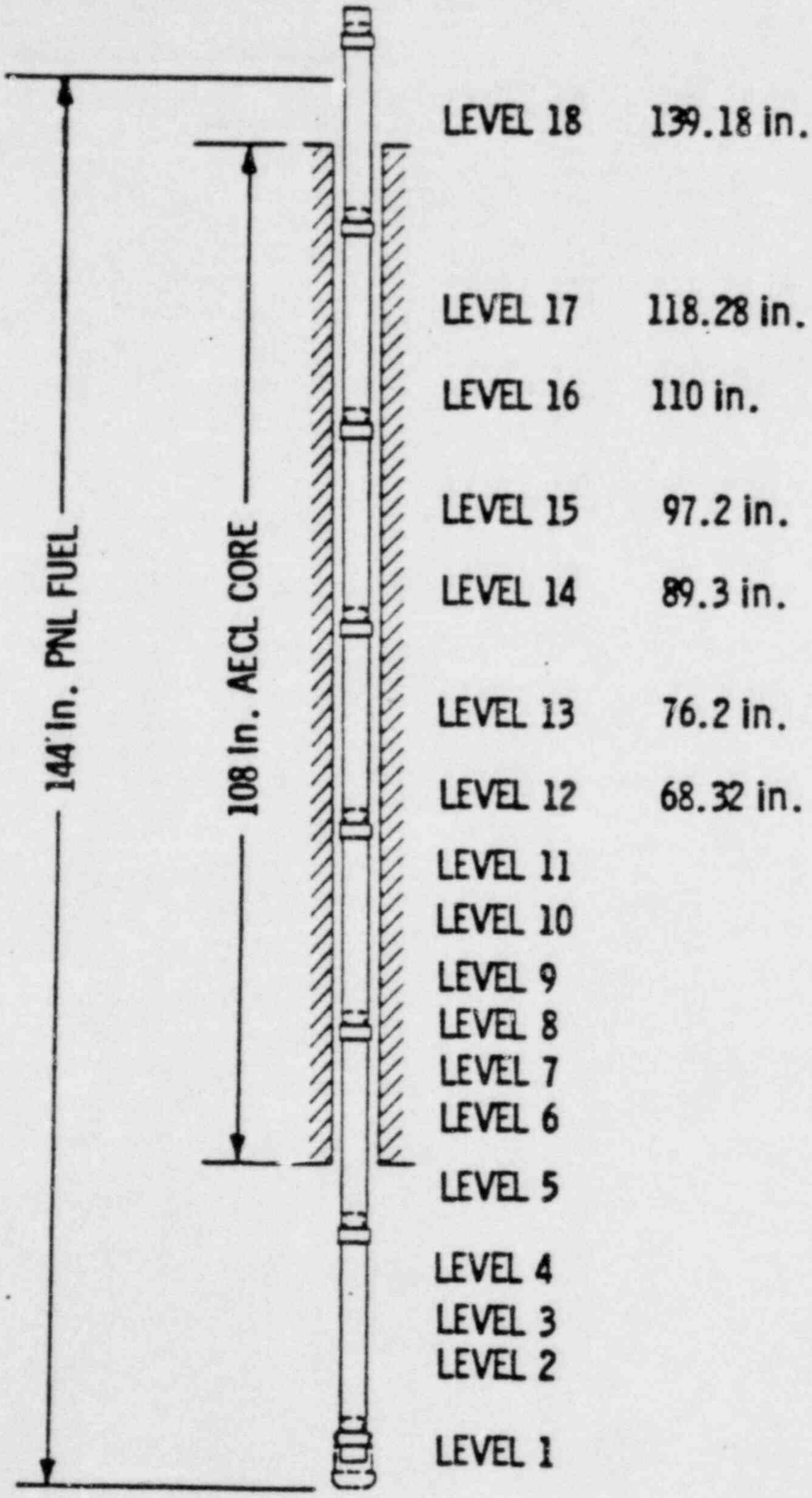


THE FUEL ROD DESIGN VARIABLES

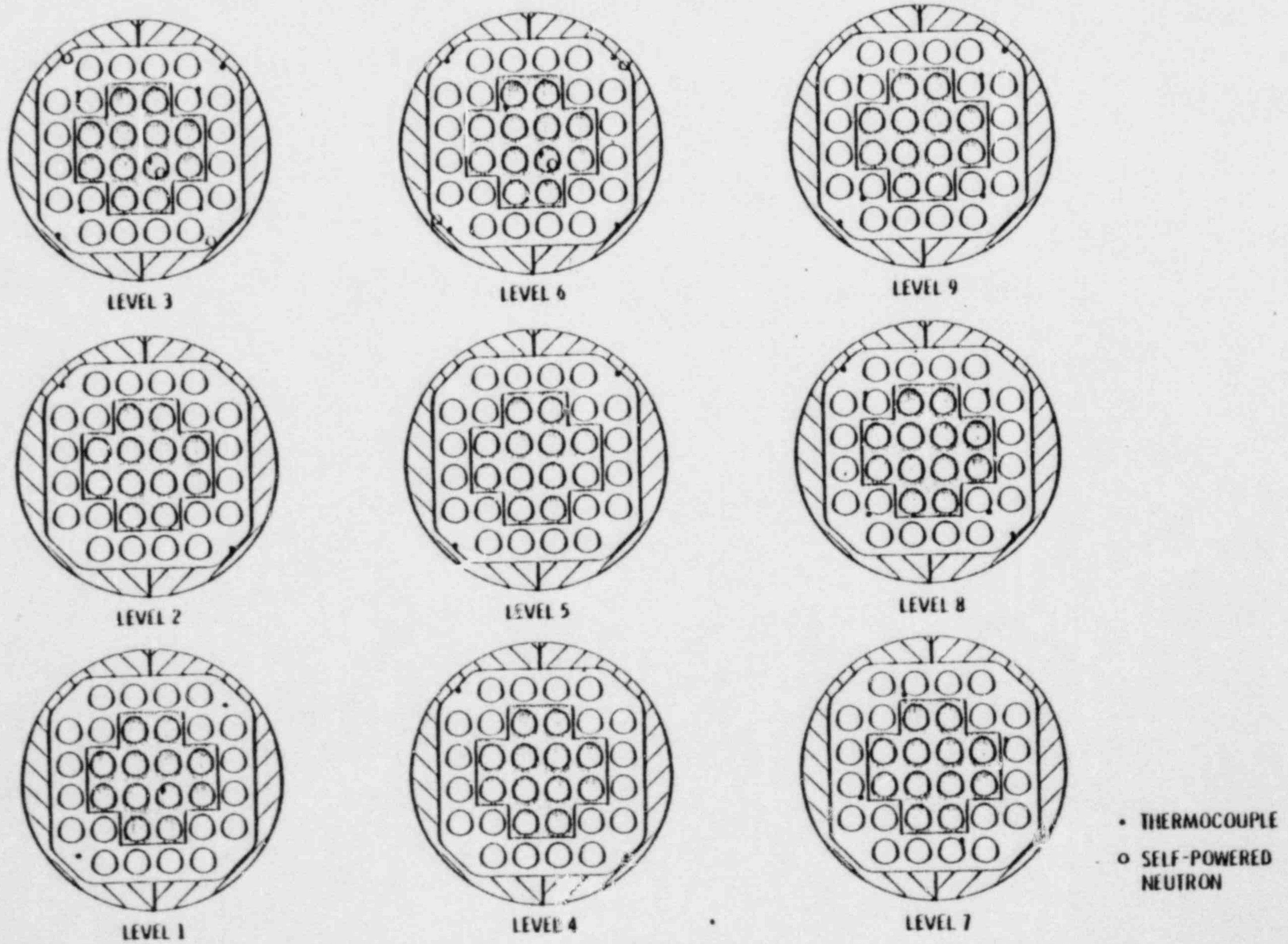
CLADDING MATERIAL SPECIFICATION	= ZIRCALOY-4
CLADDING OUTSIDE DIMENSION	= 0.379 IN (0.963 CM)
CLADDING INSIDE DIMENSION	= 0.391 IN (0.841 CM)
PITCH	= 0.502 IN (1.275 CM)
FUEL PELLET DIAMETER	= 0.325 IN (0.826 CM)
FUEL PELLET LENGTH	= 0.375 IN (0.953 CM)
ACTIVE FUELED LENGTH	= 144.0 IN (365.76 CM)
TOTAL SHROUD LENGTH	= 170.125 IN (423.1 CM)

TEST TRAIN INSTRUMENTATION

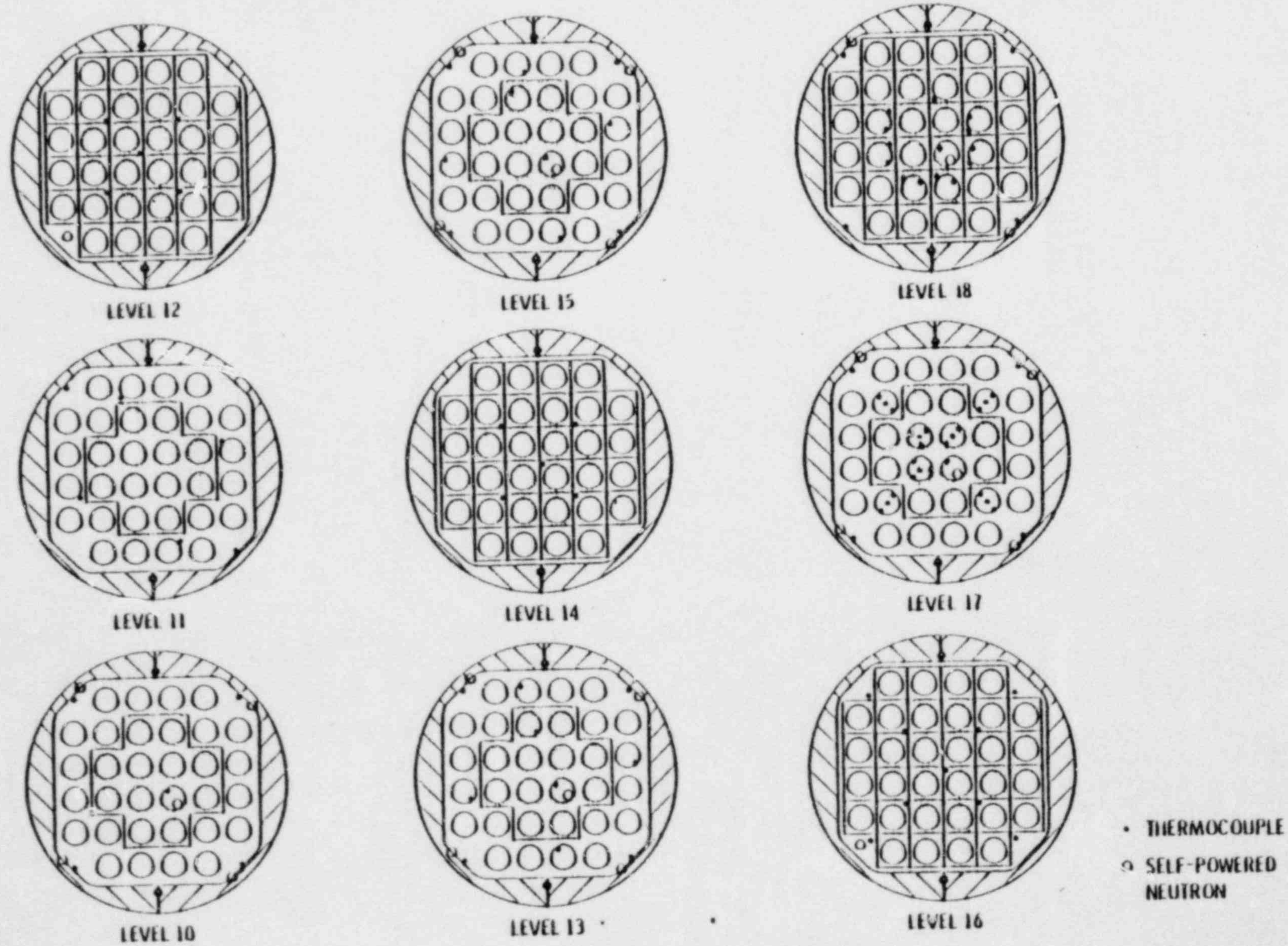
	TC'S	SPND'S	PRESSURE TRANSDUCERS	PRESSURE SWITCHES
	----	------	------------	---------
FUEL ROD			2	9
FUEL CENTERLINE	7			
PELLET-CLADDING GAP	32			
CLADDING O.D.	27			
STEAM PROBES	18			
THIMBLE	8	7		
SHROUD	38	23		
CARRIER	6	1		
HANGER TUBE	4	-		
	-----	-----		
TOTAL	140	31		


144 in. PNL FUEL
108 in. AECL CORE
LEVEL 18 139.18 in.
LEVEL 17 118.28 in.
LEVEL 16 110 in.
LEVEL 15 97.2 in.
LEVEL 14 89.3 in.
LEVEL 13 76.2 in.
LEVEL 12 68.32 in.
LEVEL 11
LEVEL 10
LEVEL 9
LEVEL 8
LEVEL 7
LEVEL 6
LEVEL 5
LEVEL 4
LEVEL 3
LEVEL 2
LEVEL 1

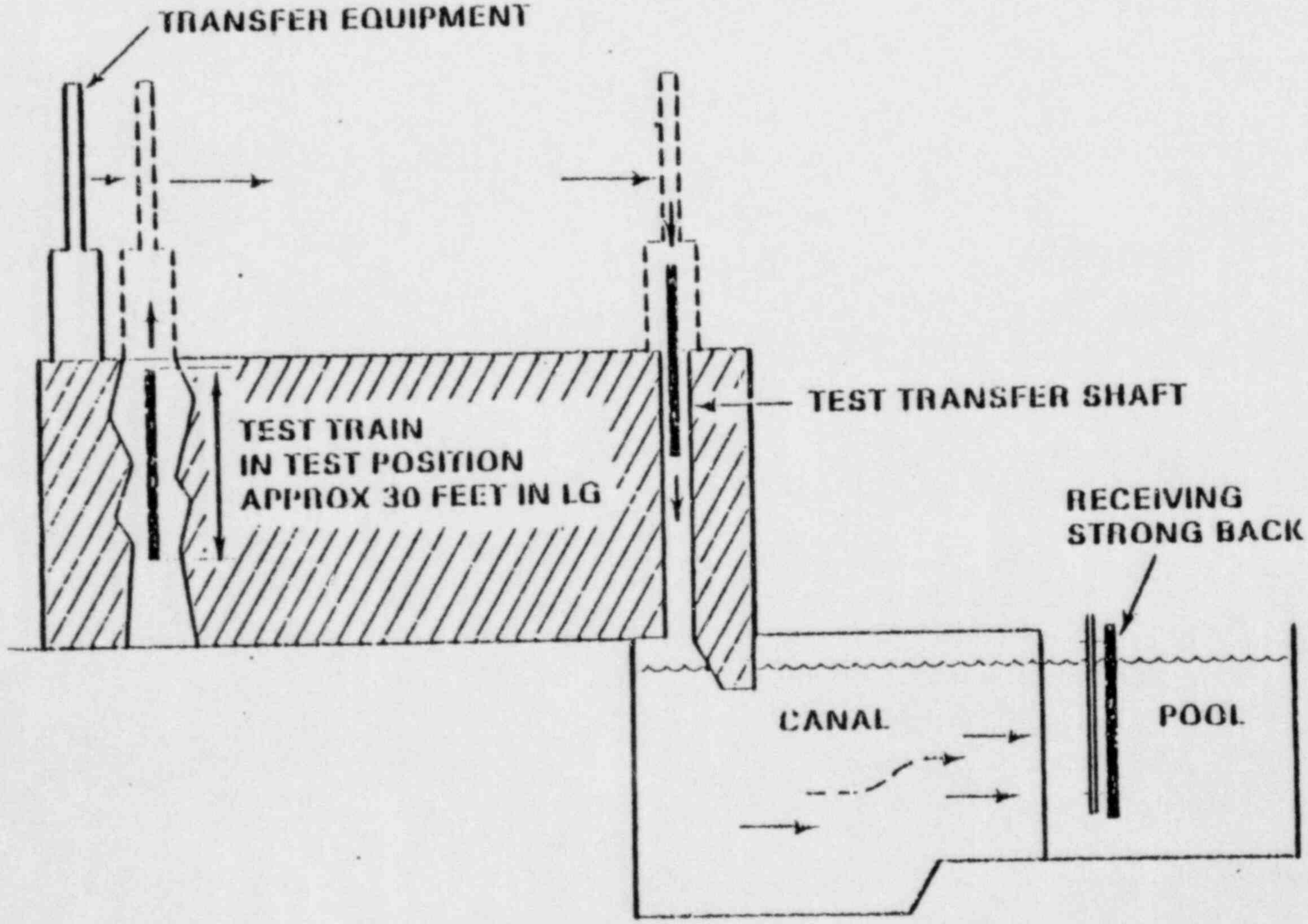
THERMAL HYDRAULIC INSTRUMENTATION ARRAY



THERMAL HYDRAULIC INSTRUMENTATION ARRAY



TEST TRAIN FLOW



ELEVATION

INDEX

	CLASSIFICATION: 11.1
TITLE (ORIGINAL LANGUAGE): LOCA Simulation in NRU	COUNTRY: USA
	SPONSOR: USNRC
TITLE (ENGLISH LANGUAGE): LOCA Simulation in NRU	ORGANISATION: Pacific Northwest Lab., BMI
	PROJECT LEADER: R. Van Houten (NRC)
INITIATED: 1977 / COMPLETED: 1983	SCIENTISTS: R. Goodman, C. Mohr, J. Pilger, G. Hesson, R. Cunningham, F. Panisko
STATUS: In Progress / LAST UPDATING: August 1, 1980	

1. General Aim:

To provide well characterized in-reactor test data on the ballooning and rupture of prototypic fuel rod clusters during LOCA heatup and reflood.

2. Particular Objectives:

In-reactor LOCA heatup and reflood tests on prototypic fuel rod clusters are needed to permit the accurate prediction of:

a. cladding damage thresholds,
b. the progress of clad ballooning and any associate of impeding of coolant flow,
c. resultant clad rupture and release of the fuel rod fission product inventory into the coolant space,
d. clad oxidation and associated release of gaseous hydrogen into the coolant space, and
e. production of debris by quench shattering oxidized cladding during reflood and related impeding of flow by debris collected on grid plates.

3. Experimental Facilities and Programs:

The NRU reactor is a heavy water moderated and cooled reactor. The effective core height is 10 feet. Peak reactor thermal power is 135 MW. At this power the peak thermal neutron flux at the U-2 position exceeds 7×10^{14} nv (neutron centimeters per cubic centimeter). The U-2 loop is a vertical test loop with a nominal 10 cm inside diameter. The loop can be filled with steam or with light water or heavy water at pressures to 10 MPa. The tests will first be run in steam at a nominal pressure of 0.2 to 0.3 MPa and then reflood water will be introduced to terminate the test. In October 1980, approximately 20 nuclear heated tests will be run with unpressurized fuel bundles to establish the thermal-hydraulic relationships. Peak clad temperatures of 1040K to 1255K will be achieved in these thermal-hydraulic tests. Five destructive over-temperature fuel bundle ballooning and flow restriction tests are scheduled thereafter with about a 4-month interval between any two destructive tests. A 32-fuel rod test array of full-length (nominal 3.67 meter) commercial enrichment (3.1 percent enriched) 17 x 17 PWR-type rods, complete with prototypic inconel spacer grids will be used for each in-reactor experiment.

4. Project Status:

Progress to date - Hardware for the required test loop and facility modifications have been received and are being emplaced. Parts for the first fuel rod test assembly have been received and are being inspected. The data accumulation computer system is being emplaced at the reactor and components for post-test examination of the fuel assembly are being fabricated. Safety analyses are receiving final review. There have been no changes in the program schedule in the past 15 months despite escalations in facility safety requirements as a result of concerns raised after the Three Mile Island accident. Initial nuclear heating tests should begin in October 1980.

5. Next Steps:

Performance of the planned 20 thermal-hydraulics tests and the five clad ballooning tests will require at least 16 months. Post-test analyses and preparation of a summary report will require an additional 18 months. The possible need for a subsequent, coolant boilaway long bundle severe fuel damage test series is being studied.

6. Relation to Other Projects and Codes:

This is part of an overall NRC program to study fuel behavior under a wide range of accident conditions and to establish resultant fission product release source terms and allowable safe reactor shutdown and recovery procedures.

7. Reference documents:

a. S. W. Heaberlin, et.al., "Design Basis Neutronics Calculations for NRU-LOCA Experiments," NUREG/CR-1025 (PNL-3113), August 1979.

8. Availability: NTIS

LOCA SIMULATION IN THE NRU REACTOR
PACIFIC NORTHWEST LABORATORY

August 21, 1980

C. L. Mohr
Pacific Northwest Laboratory

R. Van Houten
U.S. Nuclear Regulatory Commission

LOCA SIMULATION IN THE NRU REACTOR

By

C. L. Mohr
Pacific Northwest Laboratory
Richland, Washington 99352

Sponsor: Fuel Behavior Research Branch
U.S. Nuclear Regulatory Commission

A. SUMMARY

The NRU test series will begin early in FY 1981, and will continue on into FY 1982. The tests provide a well characterized data set for comparison of nuclear heated versus electrical heated tests. They provide initial conditions for translation by computer analysis techniques of the short rod data obtained in the Power Burst facility to more prototypic full length assembly analysis.

Two main types of information will be obtained from these test series, real time thermal hydraulic thermal response data and post test deformation data. Botn data sets will be on computer magnetic tape and will be presented in computer aided graphics form. The thermal hydraulic test data will augment the current electrically heated bundle tests data base that is being developed in the U.S. and Europe. The thermal hydraulic test data to be obtained from the NRU tests include:

- quench front velocity, (thermocouples on the cladding, water tube and shroud)
- water entrainment (thermocouple)
- effects of grid spacers (thermocouple)
- burst pressure and time (pressure transducers/pressure switches)
- dry out (thermocouple)
- comparison between fueled and non fueled rod heat transfer (thermocouple)
- froth front indication (SPND)
- azimuthal cladding temperature measurement (thermocouple).

The deformation data obtained from post test measurements will include:

- bundle geometry (LVDT MEASUREMENT)
- fuel rod profilometry including location and orientation within the bundle (LVDT MEASUREMENT)
- bundle deformation (photography)
- ailed P.I.E. of selected ruptured zones

- detailed results of bundle blockage
- detailed plots of fuel rod strain.

B. RESEARCH OBJECTIVES

INTRODUCTION

The LOCA simulation project is being conducted in the National Research Universal (NRU) Reactor located at Chalk River Nuclear Laboratory [Atomic Energy of Canada, LTD (AECL)] by Pacific Northwest Laboratory. The project is sponsored by the Fuel Behavior Research Branch of the Nuclear Regulatory Commission and has the major objective of evaluating the thermal hydraulic and the mechanical deformation behavior of a full length fuel rod bundle during the heatup, reflood and quench phases of a LOCA. The test will be driven by low level fission heat and will simulate the temperature gradients in the fuel typical of a LOCA.

SCOPE AND OBJECTIVES

The current scope of the program calls for six full length test assemblies to be irradiated. The geometric configuration of each assembly represents a 6 x 6 segment of a 17 x 17 PWR fuel bundle. The test will be performed in the U-2 loop of the NRU Reactor. The data that will be obtained will include:

- temperature distribution in a full length bundle as a function of time,
- interaction between thermal-hydraulics and cladding deformation,
- quench front propagation,
- quenching characteristics of nuclear heated, zircaloy clad rods for comparison with electrically heated, inconel or stainless clad rods,
- temperature-stress-time of cladding deformation,
- distribution of cladding strain within bundle and information on failure propagation,
- axial distribution of diametral strain in test fuel rods, and
- flow area reduction from cladding expansion.

The data will be used to assess various calculational models for reactor safety analyses and to assess conclusions derived from the large series of electrically heated tests and smaller scale, in-pile tests being conducted elsewhere.

C. EXPERIMENTAL PROCEDURES

TEST DESIGN FEATURES

A schematic of the overall test train is depicted in Figure 1.

The total length of the test train, including both the closure region and the test region, is 9.18 m (30 ft, 1-1/2 in.).

The closure region will provide the pressure boundary between the test train and the pressure tube of the loop. It includes penetrations for 183 instrumentation leads.

The hanger tube is used to suspend the test bundle and shroud from the closure plug. The instrument leads will be attached to the hanger to protect them during testing and transport.

The shroud will suppport the fuel bundle, serve as a liner during experimental and transfer operations, and provide proper flow distribution during various stages of the experiment. The shroud is fabricated from stainless steel. It consists of two halves clamped together at 17.78-cm (7-in.) intervals and attached at the end fittings. The split shroud design will make it possible to disassemble and reassemble as well as examine the test train under water. The shroud assembly is approximately 4.27 m (14 ft) long and is instrumented with 22 self powered neutron detectors (SPNDs) and 38 thermocouples.

The fuel bundle consists of a 6 x 6 array of fuel rods using a 17 x 17 PWR assembly design basis with the four corner rods removed for easier insertion in the shroud. This provides a basic test array of (6 x 6) - 4 (or 32) rods. The outer row of rods, including the corner rods of the next inner ring, will not be pressurized and will serve as guard rod heaters during the test. The test section consists of 11 fuel rods and one instrument thimble tube arranged in a cruciform pattern. The test rods will be unpressurized for the first test series; subsequent tests will use pressurized rods. The bundle is designed to enable reuse of the guard rod heaters, and the guard rod array can be separated into two sections. The cruciform array can also be divided into segments to aid poolside inspection and removal of the instrumented thimble tube. The cruciform test assembly will be replaced after each test. The fuel rod design variables are listed in Table 1.

TEST FACILITY FEATURES

The tests will be conducted in the U-2 loop of the NRU located at Chalk River. Table 2 summarizes the U-2 loop capabilities.

Preliminary neutronics and thermal-hydraulic calculations performed by Battelle indicate the facility could provide sufficient nuclear power in the proposed fuel bundles to provide peak cladding temperatures of 1477°K (2200°F)

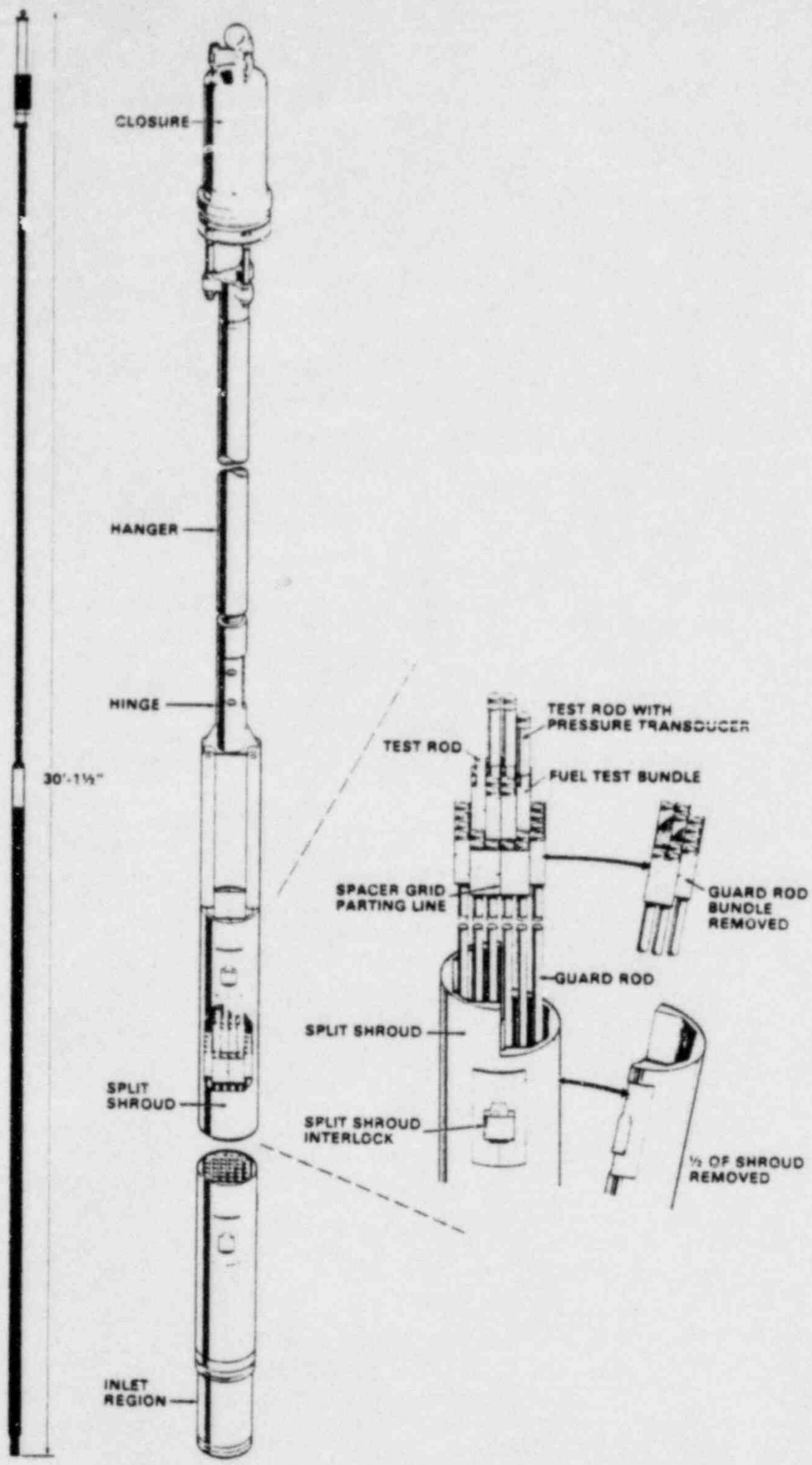


FIGURE 1. Schematic of the NRU Test Train

TABLE 1. The Fuel Rod Design Variables

Cladding Material Specification	=	Zircaloy-4	
Cladding Outside Dimension	=	0.963 cm	(0.379 in.)
Cladding Inside Dimension	=	0.841 cm	(0.331 in.)
Pitch	=	1.275 cm	(0.502 in.)
Fuel Pellet Diameter	=	0.826 cm	(0.325 in.)
Fuel Pellet Length	=	0.953 cm	(0.375 in.)
Active Fueled Length	=	365.76 cm	(144 in.)
Total Shroud Length	=	423.1 cm	(170.125 in.)

TABLE 2. NRU U-2 Loop Capabilities

Parameter	Range	
Coolant Flow	20.16 Mg/sec	(160,000 lb/hr)
Coolant Pressure	10.34 MPa	(1500 psi)
Coolant Inlet Temperature	589^{o}K	(600^{o}F)
Steam Supply	Eight 240 kW Generators	
Steam Pressure, Saturated	0.69 MPa	(100 psig)
Heat Rejection Capacity	8 MW	
Thermal Flux	2.4×10^{18} $nm^{-2}s^{-1}$	
Axial Flux Profile	Cosine, 7.20 m Period	
Test Section Length	8.8 m	(29 ft)

at heating rates approaching 28^{o}K (50^{o}F/sec). These capabilities provide an operating band that covers the major areas of interest for contemporary LWR fuel systems.

Integral with the reactor building is a large spent fuel examination pool or rod bay. This area has direct access through appropriate fuel handling machinery to the reactor top for easy insertion and removal of the test train. The rod bay area will be used for examination of the test trains including detailed rod and bundle profilometry after each test. The individual test train assembly will be disassembled and reassembled in this area.

TEST ENVIRONMENT AND COOLANT CONDITIONS

The test coolant and loop operating conditions are separated into the preconditioning, pretransient and transient phases. During the preconditioning phase, the fuel will be cycled to typical LWR reactor powers. The loop will be filled with circulating pressurized water. The loop operating conditions for this phase are shown in Table 3.

Upon completion of the preconditioning phases of operation, the loop plumbing will be changed to be operated as a once through steam filled loop. This phase of operation sets the initial conditions for the subsequent transient test phase. Table 4 summarizes the steam conditions in the test section during this phase.

The transient stage of operation is the last of the three operating phases. During this phase of operation, the steam flow during the pretransient period will be suddenly terminated thus initiating the transient or test phase.

The test train will be allowed to heat up in the stagnant steam until a pre-selected delay time has been reached. The reflood water will be introduced turning around the temperture transient and quenching the rods and shroud. Table 5 lists the range of main control variables and the maximum trip conditions that will be used.

MEASUREMENT TECHNIQUES

The test train instrumentation includes self powered neutron detectors (SPND), thermocouples, steam probes and pressure transducers and/or pressure switches (see Table 6 for a listing of instruments). These instruments are to be monitored on a real time basis by the data acquisition system. The data received will make it possible to determine the temperature, power and time history of the test train as well as providing an indication of when cladding rupture occurred.

The thermocouple measurements will provide the main source of the thermal hydraulic information. Measurements of quench front velocities and indications of water entrainment from the steam probe (thermocouple) instruments will be provided by the thermocouple instrumentation. Thermocouples will be placed inside the fuel rods on the cladding inner surface. Thermocouples will also be placed to measure azimuthal temperature variations.

The SPNDs will provide relative power measurements within the fuel bundle during steady state operation. The SPND devices are cobalt detectors and should also be able to detect the froth front that will be present during the reflood phase of the transient.

TABLE 3. Preconditioning Operating Conditions

	10.34 MPA (1500 psi)		900 psi	
Average Coolant Outlet Flow	16.94 ℓ/sec	0.598 ft^3/sec	13.34 ℓ/sec	0.471 ft^3/sec
Average Coolant Outlet Velocity	4.10 m/sec	13.45 ft/sec	3.23 m/sec	10.60 ft/sec
Coolant Inlet Temperature	517°K	471.0°F	485°K	407.0°F
Average Coolant Outlet Temperature	560°K	548.5°F	529°K	493.4°F
Total Power	2.60 MW	--	2.60 MW	--
Maximum Heat Flux	143.2 W/cm^2	0.454×10^6 Btu/hr/ft^2	143.2 W/cm^2	0.454×10^6 Btu/hr/ft^2
Average Heat Flux	76.0 W/cm^2	0.241×10^6 Btu/hr/ft^2	76.0 W/cm^2	0.241×10^6 Btu/hr/ft^2
Maximum Linear Rod Power	43.30 kW/m	13.2 kW/ft	43.30 kW/m	13.2 kW/ft
Average Linear Rod Power	21.3 kW/m	7.0 kW/ft	21.3 kW/m	7.0 kW/ft
Maximum Cladding Surface Temperature	590°K	602.0°F	551°K	547°F
Maximum Fuel Temperature	1883°K	2930.3°F	1667°K	2541°F
Maximum Linear Bundle Power	0.125 MW/m	0.409 mW/ft	0.125 MW/m	0.409 mW/ft
Average Linear Bundle Power	0.066 MW/m	0.217 mW/ft	0.066 MW/m	0.217 mW/ft
Maximum Shroud Temperature	572°K	570°F	546°K	524°F
Outlet Pressure	10.34 MPa	1500 psia	6.20 MPa	900 psia
Pressure Loss	0.104 MPa	15.1 psi	0.087 MPa	12.4 psi
Maximum Pressure Tube Temperature				
At Test Assembly Location	358°K	545°F	558°K	545°F
Above Test Assembly	573°K	572°F	573°K	572°F
Axial Peaking Factor	1.51		1.51	
Radial Peaking Factor	1.24		1.24	
Minimum DNBR Ratio	2.96		3.51	

TABLE 4. Pretransient Operating Conditions

Average Coolant Outlet Flow	0.359 kg/sec	2850 lbm/hr
Average Coolant Outlet Velocity	94.2 m/sec	309 ft/sec
Coolant Inlet Temperature	$436^{o}K$	$325^{o}F$
Average Coolant Outlet Temperature	$622^{o}K$	$660^{o}F$
Maximum Heat Flux	7.03 W/cm^2	22,288 Btu/hr ft^2
Average Heat Flux	4.12 W/cm^2	13,071 Btu/hr ft^2
Maximum Linear Rod Power[a]	2.13 kW/m	0.648 kW/ft
Average Linear Rod Power	1.25 kW/m	0.38 kW/ft
Maximum Cladding Surface Temperature	$700^{o}K$	$800^{o}F$
Maximum Fuel Temperature	$746^{o}K$	$883^{o}F$
Maximum Linear Bundle Power	65.91 kW/m	20.09 kW/ft
Average Linear Bundle Power	38.65 kW/m	11.78 kW/ft
Maximum Shroud Temperature	$627^{o}K$	$670^{o}F$
Outlet Pressure	0.276 MPa	40 psia
Pressure Loss	0.058 MPa	8.4 psi
Maximum Pressure Tube Temperature		
At Test Assembly Location	$493^{o}K$	$428^{o}F$
Above Test Assembly	$588^{o}K$	$600^{o}F$

(a) The outer guard rods have the largest linear power.

TABLE 5. Controlled Variables for Transient and Trip Conditions

Control Variables		
Reflood Rate	1.3-25.4 cm/sec	(0.5-10 in./sec)
Reflood Temperature	325°K	(127°F)
Maximum Linear Heat Rating Per Test Rod	1.8 kW/m	(0.55 kW/ft)
Maximum Linear Bundle Heat Rating	55.9 kW/m	(17.1 kW/ft)
Temperature Ramp Rate	8.3°K/sec	(15°F/sec)
Delay Time to Start of Reflood	0-60 sec	(0-60 sec)
Outlet Pressure	0.138-0.276 MPA	(20 to 40 psia)
Trip Conditions		
Maximum Clad Surface Temperature	1310°K	(1900°F)
Maximum Fuel Centerline Temperature	1338°K	(1950°F)
Maximum Pressure Tube Temperature		
At Assembly Location	727°K	(850°F)
Above Assembly	727°K	(850°F)
Maximum Average Coolant Outlet Temperature	727°K	(850°F)

TABLE 6. Test Train Instrumentation

No.	Description
31	Self-Powered Neutron Detectors (SPND)
120	Thermocouples
18	Steam Probes
11	Pressure Transducers

The pressure transducers consist of one pressure measurement transducer and 10 pressure switches. The pressure switch will provide an indication of when rupture of the cladding has occurred while the pressure transducer will provide a detailed pressure versus time history.

The instruments are located spatially throughout the test bundle at specific axial locations or levels. Figure 2 shows the different instrumentation levels where Figures 3 and 4 show the distribution within the fuel bundle and shroud where the SPNDs and thermocouples are located.

Post test measurements on the deformed fuel bundle will include detailed axial profilometer measurement of both the bundle and the individual fuel rods. This information will be collected by computers and will allow spatial reconstruction of the bundle deformation by computer aided graphics techniques.

D. PROPOSED TEST MATRIX

The proposed test matrices for the NRU test series are broken into thermal hydraulic tests using a single assembly and five separate fuel cladding performance tests. The thermal hydraulic test series will consist of 18 separate transients using a single test assembly. This assembly will use unpressurized fuel rods and will concentrate on evaluating the thermal hydraulic behavior of quench front during reflood and heat transfer parameters within the fuel bundle. Table 7 shows the parameters that will be used in this test series.

The fuel cladding performance test series will use test rods pressurized to 3.1 MPa (450 psia). The parameters of reflood rate and delay time will be selected based on quenching characteristics developed during the thermal hydraulic test series. The parameters will be chosen to provide cladding rupture at 1033°K (1400°F), 1144°K (1600°F) and 1255°K (1800°F). Table 8 shows anticipated range of system variables to be used for this test series. A total of five tests will be performed using five separate test rod assemblies with each test consisting of one transient each.

E. RESULTS

Two main types of information will be obtained from these test series, real time thermal hydraulic-thermal response data and post test deformation data. Both data sets will be on computer magnetic tape and will be presented in computer aided graphics form. The thermal hydraulic test data will augment the current electric heated tests data base that is being developed. This data includes:

- quench front velocity, (thermocouples on the cladding, water tube and shroud)
- water entrainment (thermocouple)
- effects of grid spacers (thermocouple)
- burst pressure and time (pressure transducers/pressure switches)
- dry out (thermocouple)
- comparison between fueled and non fueled rod heat transfer (thermocouple)
- froth front indication (SPND)
- azimuthal cladding temperature measurement (thermocouple).

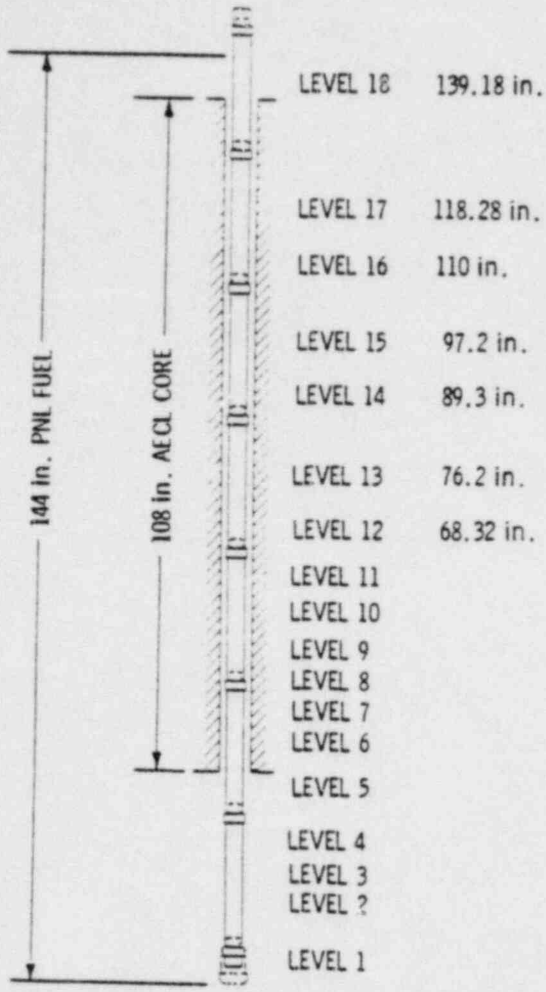


FIGURE 2. Test Train Instrumentation Axial Location Levels

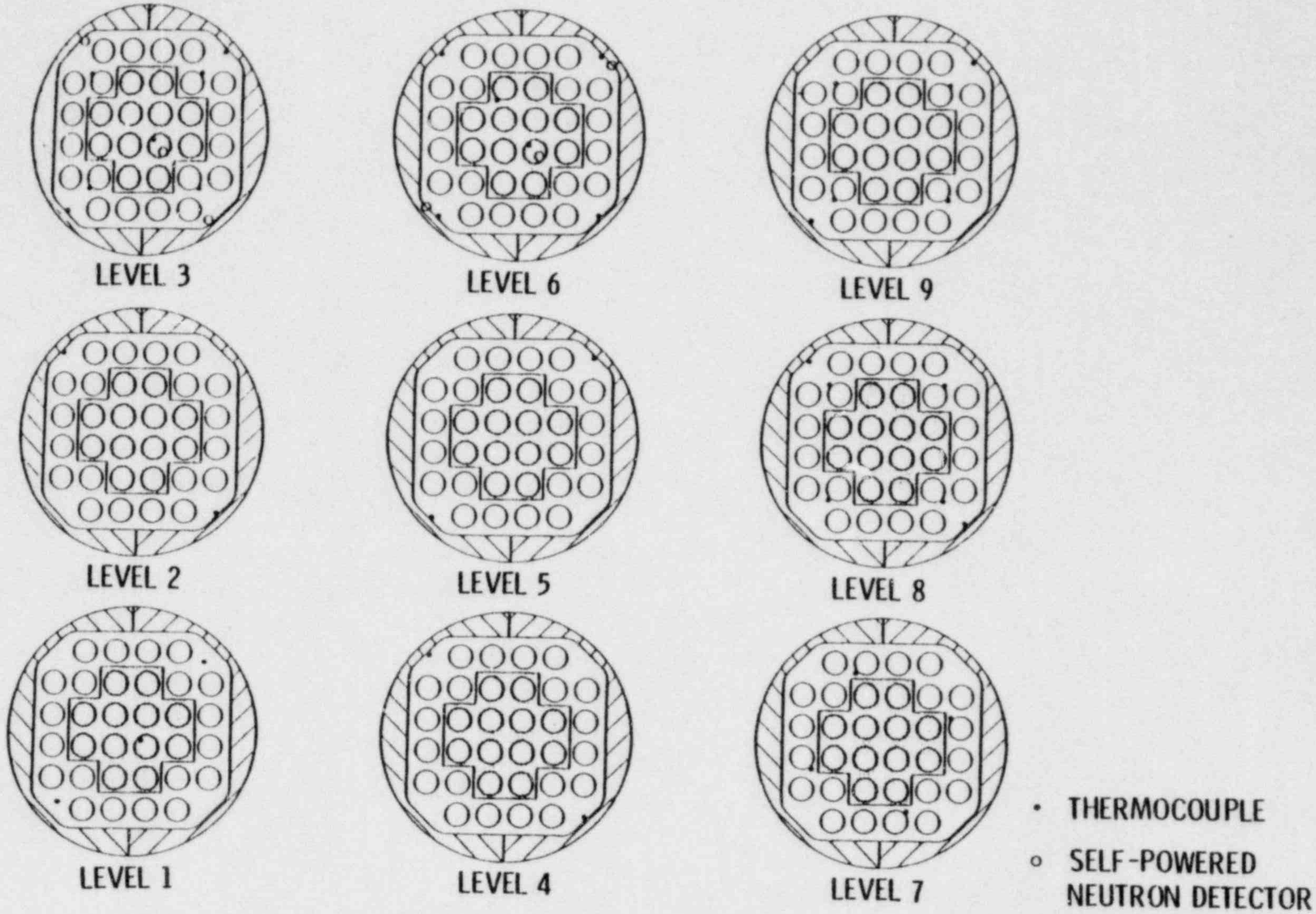


FIGURE 3. Test Train Cross Sections Showing Instrumentation Distribution for Axial Levels 1 through 9

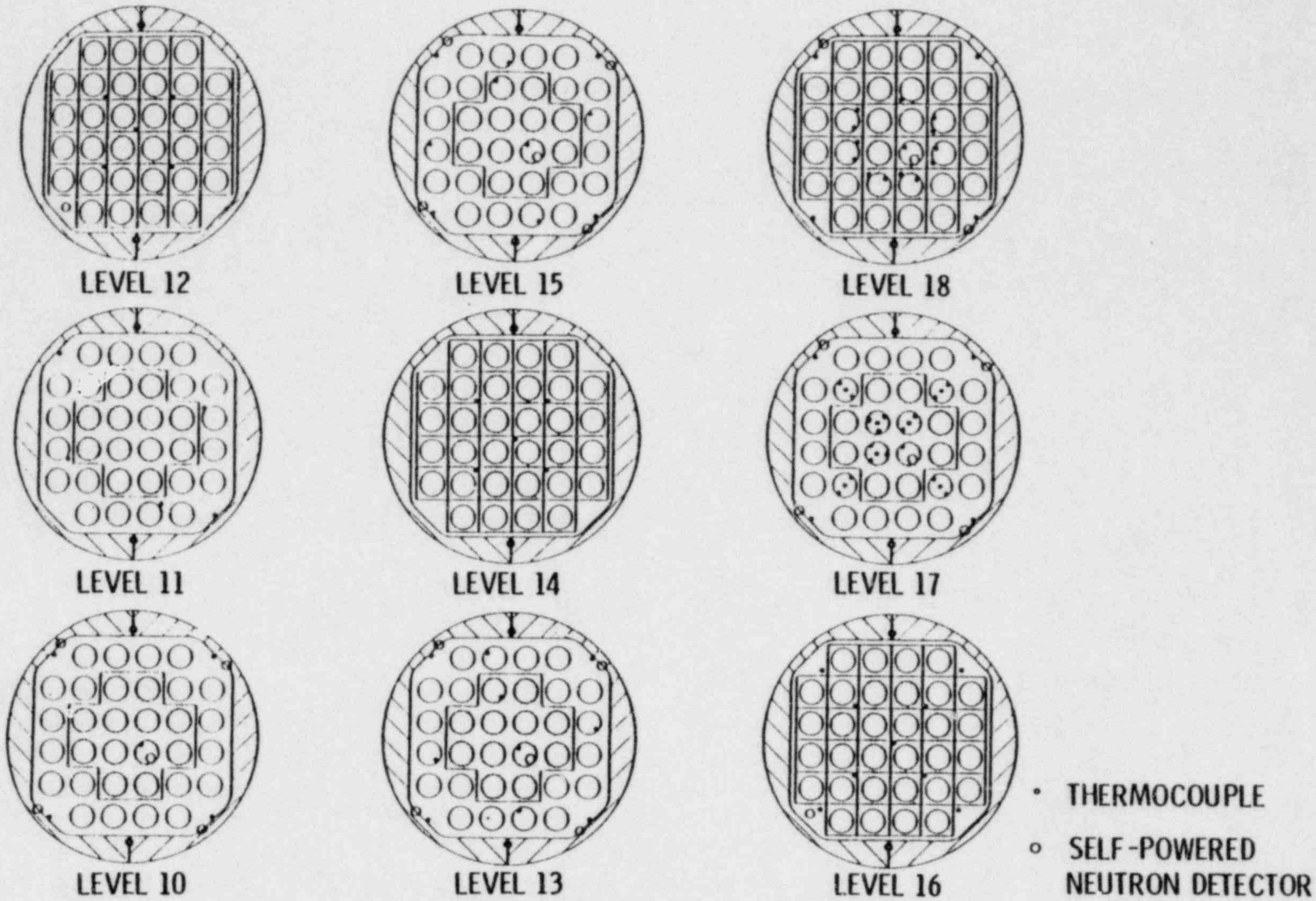


FIGURE 4. Test Train Cross Section Showing Instrumentation Distribution for Levels 10 Through 18.

TABLE 7. Prototypic[a] Thermal-Hydraulic Test Series Plan

Test Day	Test Series Number	Reflood Rate		Reflood Delay Time, s	Heating Rate		Maximum Cladding Temperature	
		m/s	in./s		°K/s	°F/s	°K	°F
2	101	0.254	10	39	8	15	1033	1400
2	102	0.102	4	39	8	15	1033	1400
3	103	0.076	3	33	8	15	1033	1400
3	104	0.051	2	7	8	15	1033	1400
3	105	0.047[b]	1.85	0	8	15	1033	1400
3	106	0.254	10	53[b]	8	15	1144	1600
3	107	0.102	4	53	8	15	1144	1600
4	108	0.076	3[b]	46	8	15	1144	1600
4	109	0.051	2	27[b]	8	15	1144	1600
4	110	0.037[b]	1.45	0	8	15	1144	1600
4	111	0.064	2.5[b]	60	8	15	1255	1800
4	112	0.051	2.0	50[b]	8	15	1255	1800
5	113	0.038[b]	1.5	22[b]	8	15	1255	1800
5	114	0.032[b]	1.2[b]	0	8	15	1255	1800
5	115	0.028[b]	1.1[b]	0	8	15	1310	1900[c]
5	116	0.076	3	33[b]	8	15	1033	1400
6	117	0.051	2	7[b]	8	15	1033	1400
6	118[d]	0.047	1.85	0	8	15	1033	1400
6	119-124[e]	----------	----------	TBD[e]	----------	----------	----------	----------

(a) In all tests, the peak test fuel rod power will be 1.80 kW/m (0.55 kW/ft), system outlet pressure will be 0.28 MPa (40 psia), inlet reflood subcooling temperature will be 78°K (140°F) and the initial fuel rod pressure will be 0.10 MPa (14.7 psia) at STP.
(b) Final value will be selected from earlier tests in this experiment.
(c) Cladding temperature may exceed 1255°K (1800°F), based on parameters evaluated from earlier test results. For safety purposes 1310°K (1900°F) will be used as the maximum.
(d) Replicate of Test Number 105.
(e) To Be Defined by test results from, and within the operating envelope conditions of, previous tests in this series (time permitting).

TABLE 8. Fuel Cladding Performance Tests[a]

Test Number	Flooding Rate, m/sec	Flooding Rate, in./sec	Reflood Delay Time, sec	Heating Rate, °K/sec	Heating Rate, °F/sec	Maximum Cladding Temperature, °K	Maximum Cladding Temperature, °F
2	0.127	5	32[b]	8	15	1033	1400
3	0.051	2	12[b]	8	15	1033	1400
4	0.025-0.036	1-1.4[c]	0	8	15	1033	1400
5	0.051	2	25[b]	8	15	1144	1600
6	0.051	2	40[b]	8	25	1255	1800

(a) In all six tests, the following conditions will be met: peak rod power 1.97 kW/m (0.6 kW/ft), system pressure 0.27 MPa (40 psia), inlet subcooling temperature 78°K (140°F) and the initial rod pressure 3.1 MPa (450 psia). The actual value of the rod pressure will be adjusted to give an NRU hot operating pressure equivalent to that of a modern PWR. The option exists to change the rod pressures of tests 5 and 6, depending upon results of tests 2, 3 and 4.
(b) Target, actual value selected from previous tests to obtain desired cladding temperatures.
(c) Target, lowest rate practical.

The deformation data obtained from post test measurements will include:

- bundle geometry (LVDT MEASUREMENT)
- fuel rod profiliometry including location and orientation within the bundle (LVDT MEASUREMENT)
- bundle deformation (photography)
- detailed P.I.E. of selected ruptured zones
- detailed results of bundle blockage
- detailed plots of fuel rod strain

F. DISCUSSION AND CONCLUSIONS

The NRU test series will begin with actual testing in October 1980. The test program will continue on into 1981 with the final tests being performed in early 1982. The current schedule calls for thermal hydraulic data to be available in early 1981 with deformation data in mid to late 1981.

The data from the NRU program are aimed primarily at large break LOCA conditions. The tests will provide a well characterized data set for comparison of nuclear heated versus electrically heated tests. They also provide the opportunity to evaluate computer codes associated with the heatup and reflood phases of a LOCA and in addition provide basic fluid dynamics data on entrainment for long fuel rods.

The effects of geometry, size and nuclear heating are significant in simulating accident conditions. The NRU test will provide the first opportunity to evaluate the combination of these effects in a single test. The major questions associated with the FLECHT data and the German REBEKA tests should be answered with these data.

The results of these tests also provide information related to high damage effects by evaluating the potential heat transfer modes during the quenching of hot oxidized rods. Entrainment, the effects of mass flux, the combination of grid spacers and deformed and ruptured cladding all provide the initial conditions for subsequent high damage conditions. Although the temperature ranges of these tests are much lower than the high damage area conditions, the initial conditions, the effects of geometry between long and short rods will be applicable. Many of these points are unique to the NRU test series and equivalent data will not be available from any other test program either planned or presently under way.

G. PERTINENT REFERENCES

Mohr, C. L., et al. 1980. Safety Analysis Report: Loss-of-Coolant Accident Simulations in the National Research Universal Reactor. NUREG/CR 1208, PNL 3093, Pacific Northwest Laboratory, Richland, Washington 99352.

Hann, C. R. 1979. Program Plan LOCA Simulations in the National Research Universal Reactor. PNL-3056, Pacific Northwest Laboratory, Richland, Washington 99352.

LOCA SIMULATION IN NRU

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REACTOR SAFETY

AUGUST 21, 1980
IDAHO FALLS, IDAHO

CONTENTS

LOCA SIMULATION IN NRU

OBJECTIVE

- DEVELOP A WELL CHARACTERIZED DATA SET FOR FULL LENGTH MULTIROD BUNDLES UNDER REPRESENTATIVE HEATUP AND REFLOOD CONDITIONS

LOCA SIMULATION IN NRU

DATA TO BE OBTAINED

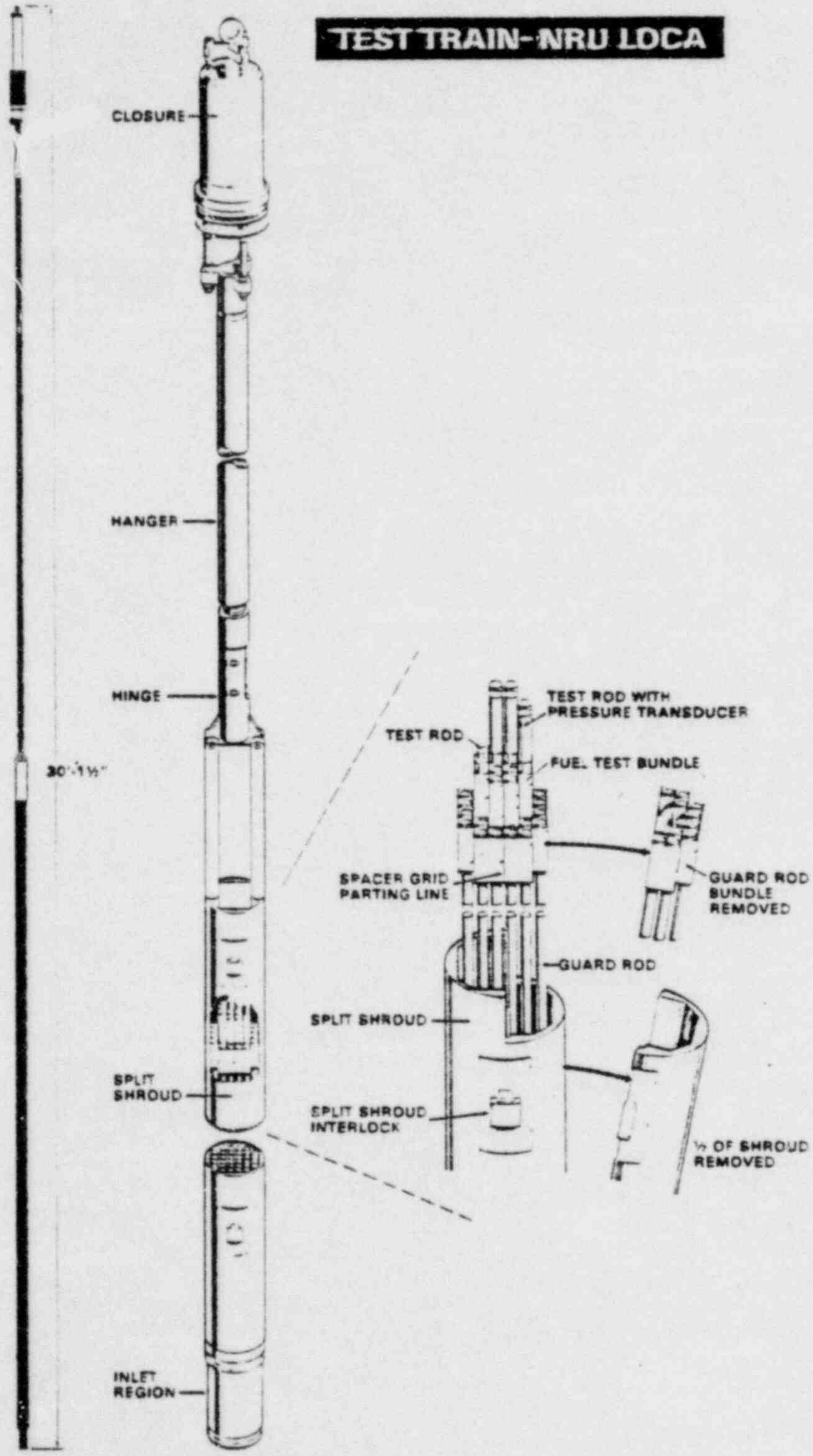
- TEMPERATURE DISTRIBUTION IN A FULL LENGTH BUNDLE AS A FUNCTION OF TIME
- QUENCH FRONT PROPAGATION
- INTERACTION BETWEEN THERMAL HYDRAULIC CHARACTERISTICS AND CLADDING DEFORMATION
- QUENCHING CHARACTERISTICS OF NUCLEAR HEATED RODS FOR COMPARISON WITH ELECTRICALLY HEATED RODS
- QUENCHING CHARACTERISTICS OF ZIRCALOY CLAD RODS FOR COMPARISON WITH INCONEL ON STAINLESS CLAD RODS
- DISTRIBUTION OF CLADDING STRAIN WITHIN BUNDLE
- TEMPERATURE-STRESS-TIME OF CLADDING PERFORATION
- CONFIRM CONCLUSIONS DERIVED FROM ELECTRICALLY HEATED TESTS AND SHORT, SINGLE ROD IN-PILE TESTS

DESIGN VARIABLES

THE FUEL ROD DESIGN VARIABLES

CLADDING MATERIAL SPECIFICATION	ZIRCALOY-4
CLADDING OUTSIDE DIMENSION	0.379 in. (0.963 cm)
CLADDING INSIDE DIMENSION	0.331 in. (0.841 cm)
PITCH	0.502 in. (1.275 cm)
FUEL PELLET DIAMETER	0.325 in. (0.826 cm)
FUEL PELLET LENGTH	0.375 in. (0.953 cm)
ACTIVE FUELED LENGTH	144.0 in. (365.76 cm)
TOTAL SHROUD LENGTH	170.125 in. (423.1 cm)

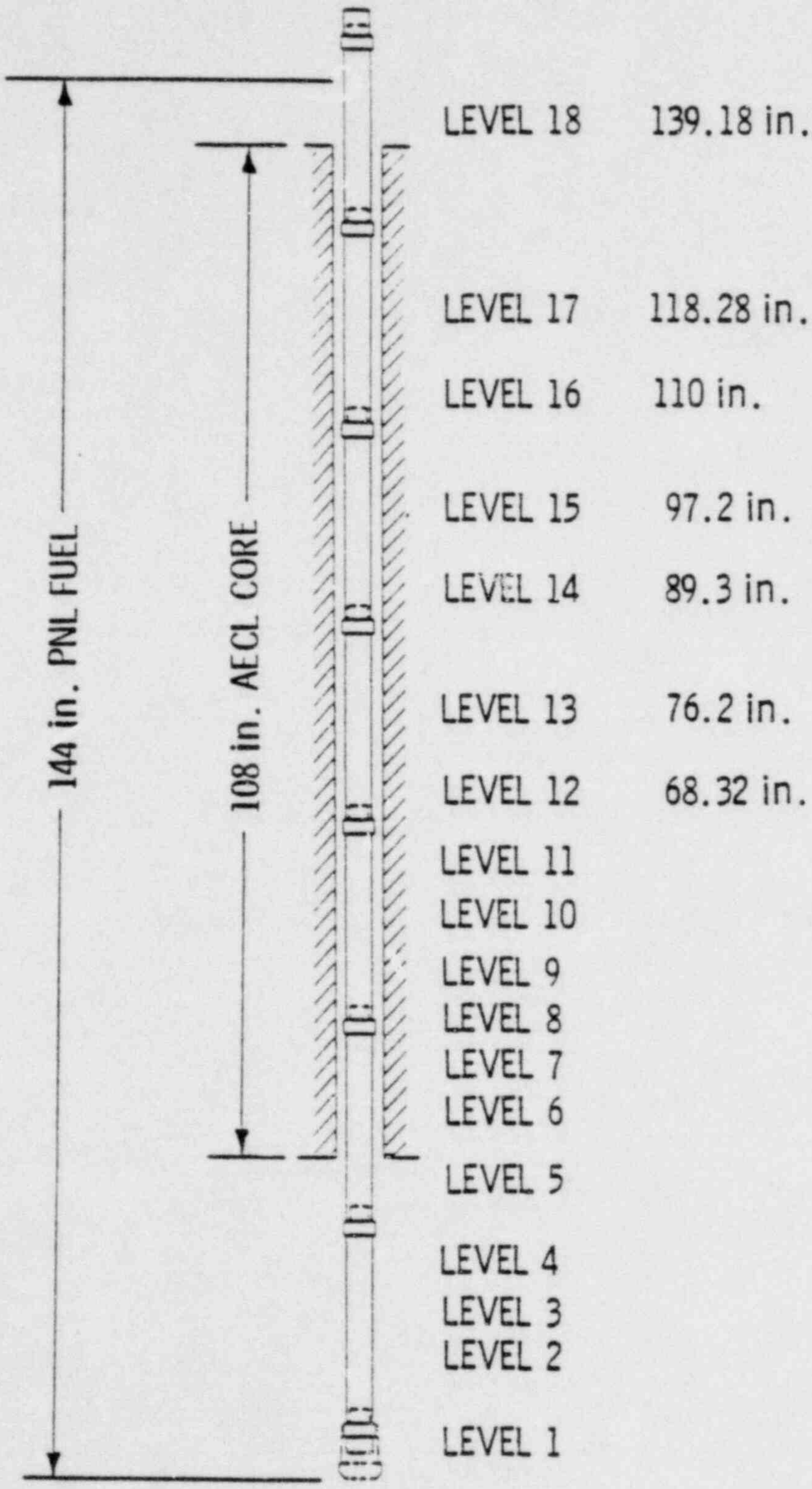
TEST TRAIN-NRU LOCA



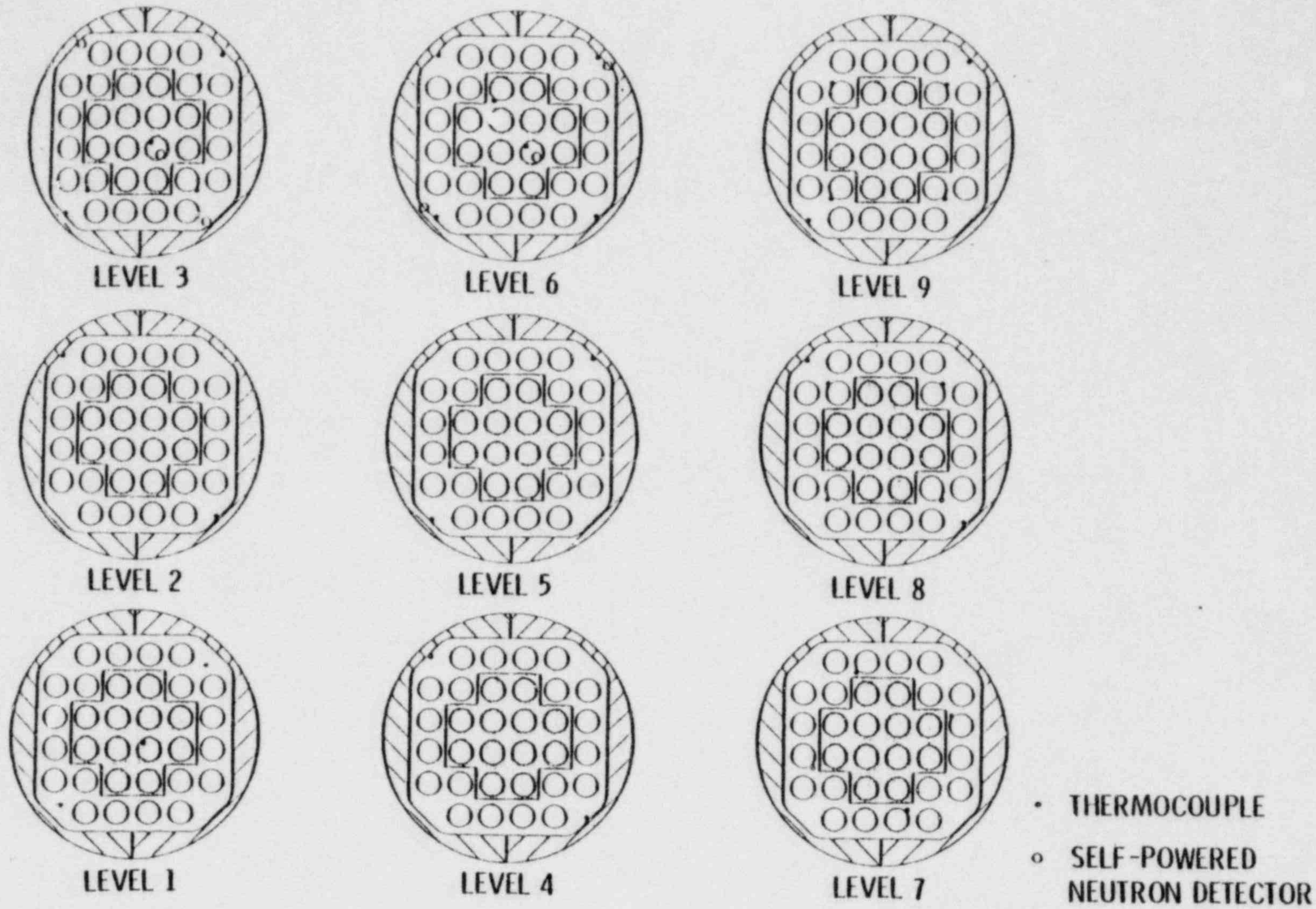
INSTRUMENTATION

TEST TRAIN INSTRUMENTATION

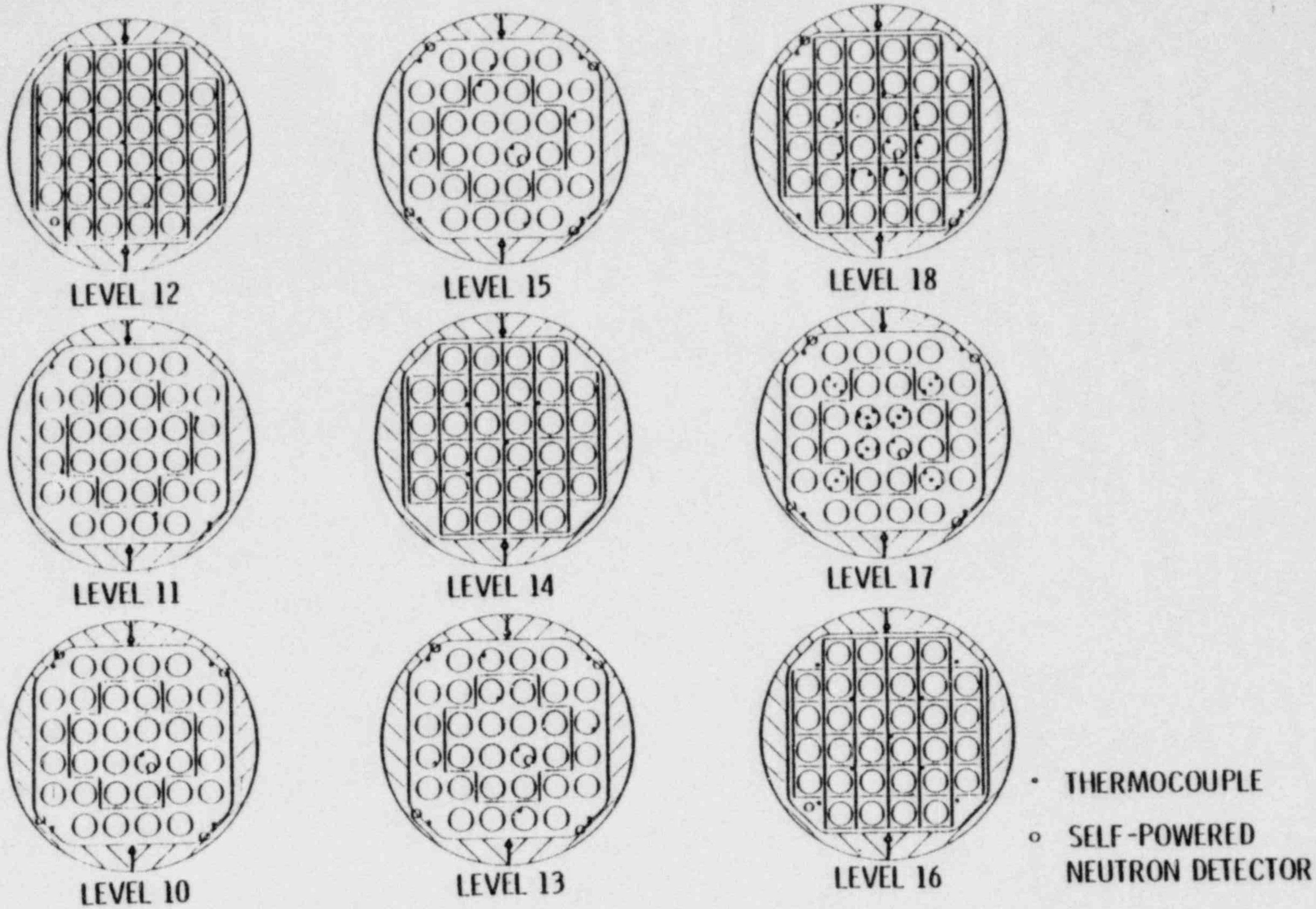
	THERMOCOUPLES	SELF POWERED NEUTRON DETECTORS	PRESSURE TRANSDUCERS	PRESSURE SWITCHES
FUEL ROD				
FUEL CENTERLINE	7		2	9
PELLET-CLADDING GAP	32			
CLADDING O.D.	27			
STEAM PROBES	18			
THIMBLE	8	7		
SHROUD	38	23		
CARRIER	6	1		
HANGER TUBE	4	--		
TOTAL	140	31		


144 in. PNL FUEL
108 in. AECL CORE
LEVEL 18 139.18 in.
LEVEL 17 118.28 in.
LEVEL 16 110 in.
LEVEL 15 97.2 in.
LEVEL 14 89.3 in.
LEVEL 13 76.2 in.
LEVEL 12 68.32 in.
LEVEL 11
LEVEL 10
LEVEL 9
LEVEL 8
LEVEL 7
LEVEL 6
LEVEL 5
LEVEL 4
LEVEL 3
LEVEL 2
LEVEL 1

THERMAL HYDRAULIC INSTRUMENTATION ARRAY



THERMAL HYDRAULIC INSTRUMENTATION ARRAY



TEST DESCRIPTION

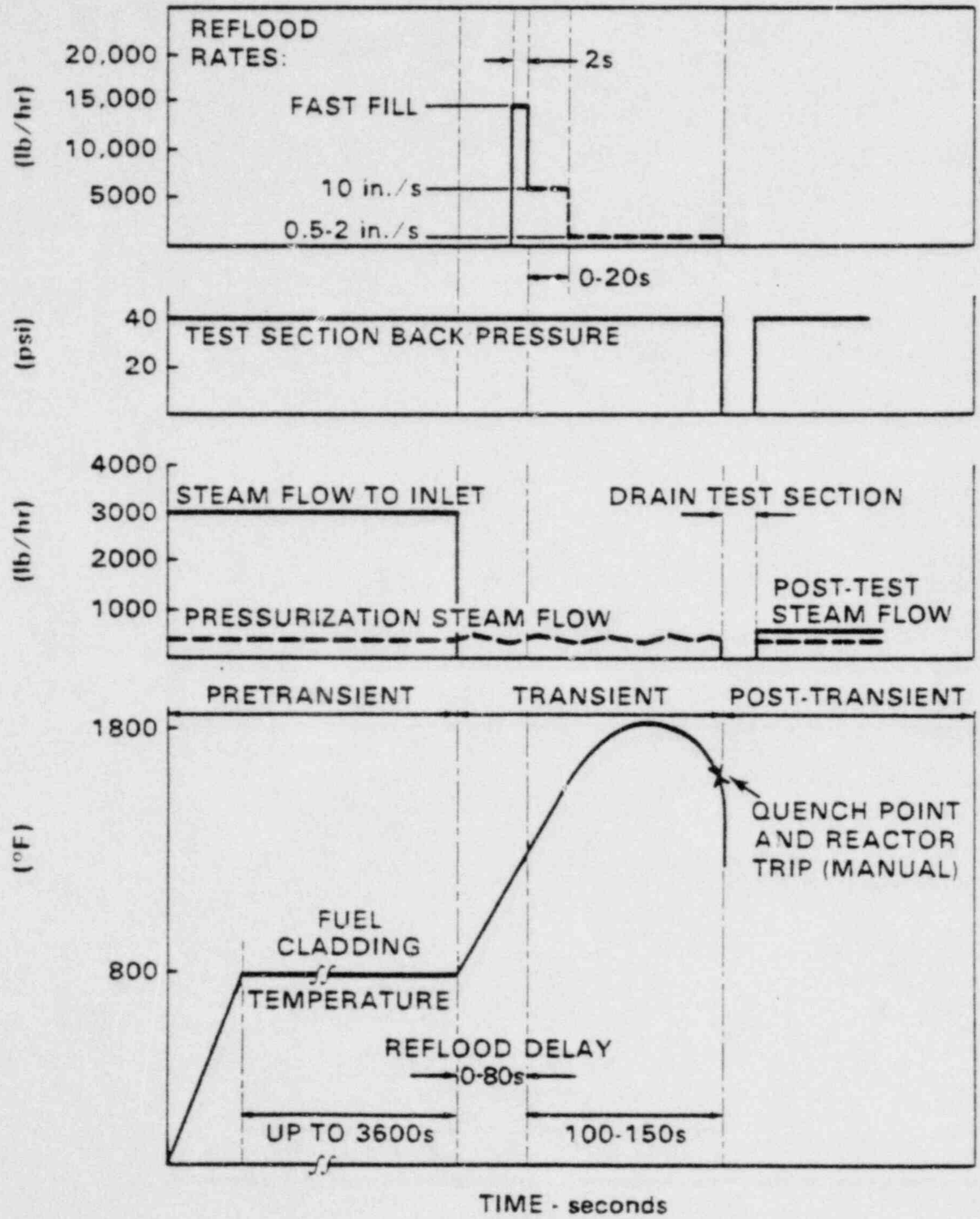
NRU U-2 LOOP CAPABILITIES

PARAMETER	RANGE
COOLANT FLOW	20.16 kg/sec (160,000 lb/hr)
COOLANT PRESSURE	10.34 MPa (1500 psia)
COOLANT INLET TEMPERATURE	589 DEG K (600 DEG F)
STEAM SUPPLY	EIGHT 240 kW GENERATORS
STEAM PRESSURE	0.69 MPa (100 psig), SATURATED
HEAT REJECTION CAPACITY	8 MW
THERMAL FLUX	2.4E18 n/cm^2/sec
AXIAL FLUX PROFILE	COSINE, 7.20 M PERIOD
TEST CROSS SECTION ID	8.8 m (29 ft)

TEST PHASES

* PRECONDITIONING	(WATER)
* PRETRANSIENT	(FLOWING STEAM)
* TRANSIENT	(STAGNANT STEAM)

LOCA SIMULATIONS IN NRU TESTING SEQUENCE



PRECONDITIONING OPERATING CONDITIONS FOR OUTLET PRESSURE OF 1500 psia AND TEMPERATURE DIFFERENCES OF 75°F

VARIABLE	METRIC	ENGLISH
AVERAGE COOLANT OUTLET FLOW	14.29 ℓ/sec.	0.505 ft³/sec.
AVERAGE COOLANT OUTLET VELOCITY	3.46 m/sec.	11.35 ft/sec.
COOLANT INLET TEMPERATURE	243.9°C	471.0°F
AVERAGE COOLANT OUTLET TEMPERATURE	288.5°C	551.3°F
TOTAL POWER	2.60 MW	—
MAXIMUM HEAT FLUX	143.2 W/cm²	0.454×10⁶ Btu/hr/ft²
AVERAGE HEAT FLUX	76.0 W/cm²	0.241×10⁶ Btu/hr/ft²
MAXIMUM LINEAR ROD POWER	4.03 kW/m	13.2 kW/ft
AVERAGE LINEAR ROD POWER	21.3 kW/m	7.0 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE		
CRUCIFORM	311°C	592°F
BUNDLE	317°C	603°F
MAXIMUM FUEL TEMPERATURE		
CRUCIFORM	1164°C	2127°F
BUNDLE	1426°C	2598°F
BUNDLE ORIENTATION	VERTICAL	VERTICAL
MAXIMUM LINEAR BUNDLE POWER	0.125 MW/m	0.409 MW/ft
AVERAGE LINEAR BUNDLE POWER	0.066 MW/m	0.217 MW/ft
MAXIMUM SHROUD TEMPERATURE	306°C	581°F
OUTLET PRESSURE	10.34 MPa	1500 psia
PRESSURE LOSS	0.087 MPa	12.6 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	285°C	545°F
ABOVE TEST ASSEMBLY	300°C	572°F
AXIAL PEAKING FACTOR	1.51	
RADIAL PEAKING FACTOR	1.24	
MINIMUM DNBR RATIO	2.96	

PRECONDITIONING OPERATING CONDITIONS FOR OUTLET PRESSURE OF 1250 psia AND TEMPERATURE DIFFERENCE OF 75°F

VARIABLE	METRIC	ENGLISH
AVERAGE COOLANT OUTLET FLOW	13.45 ℓ/sec.	0.475 ft³/sec.
AVERAGE COOLANT OUTLET VELOCITY	3.54 m/sec.	11.6 ft/sec.
COOLANT INLET TEMPERATURE	230.6°C	447.0°F
AVERAGE COOLANT OUTLET TEMPERATURE	274.4°C	526.0°F
TOTAL POWER	2.232 mW	
MAXIMUM HEAT FLUX	121.9 W/cm²	0.386 MBtu/hr/ft²
AVERAGE HEAT FLUX	65.1 W/cm²	0.206 MBtu/hr/ft²
MAXIMUM LINEAR ROD POWER	36.8 kW/m	11.2 kW/ft
AVERAGE LINEAR ROD POWER	19.7 kW/m	6.0 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE		
WITHIN CRUCIFORM	296.1°C	565.0°F
OVERALL	302.6°C	576.7°F
MAXIMUM FUEL TEMPERATURE		
WITHIN CRUCIFORM	1149°C	2101°F
OVERALL	1411°C	2572°F
BUNDLE ORIENTATION	VERTICAL	
MAXIMUM LINEAR BUNDLE POWER	1.14MW/m	0.348 MW/ft
AVERAGE LINEAR BUNDLE POWER	0.61 MW/m	0.186 MW/ft
MAXIMUM SHROUD TEMPERATURE	290.9°C	555.6°F
OUTLET PRESSURE	8.62 MPa	1250 psia
PRESSURE LOSS	0.092 MPa	13.3 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	302.8°C	577.0°F
ABOVE TEST ASSEMBLY	274.4°C	526.0°F
AXIAL PEAKING FACTOR	1.51	
RADIAL PEAKING FACTOR	1.24	
MINIMUM DNBR RATIO	3.43	

PRECONDITIONING OPERATING CONDITIONS FOR OUTLET PRESSURE OF 900 psia AND TEMPERATURE DIFFERENCE OF 75°F

VARIABLE	METRIC	ENGLISH
AVERAGE COOLANT OUTLET FLOW	13.45 l/s	0.475 ft^3/sec
AVERAGE COOLANT OUTLET VELOCITY	3.51 m/s	11.5 ft/sec.
COOLANT INLET TEMPERATURE	208.3°C	407.0°F
AVERAGE COOLANT OUTLET TEMPERATURE	252.5°C	486.5°F
TOTAL POWER	2.232 MW	
MAXIMUM HEAT FLUX	121.9 W/cm^2	0.386 MBtu/hr/ft^2
AVERAGE HEAT FLUX	65.1 W/cm^2	0.206 MBtu/hr/ft^2
MAXIMUM LINEAR ROD POWER	36.8 kW/m	11.2 kW/ft
AVERAGE LINEAR ROD POWER	19.7 kW/m	6.0 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE		
WITHIN CRUCIFORM	274.2°C	525.5°F
OVERALL	279.8°C	535.6°F
MAXIMUM FUEL TEMPERATURE		
WITHIN CRUCIFORM	1127°C	2061°F
OVERALL	1389°C	2582°F
BUNDLE ORIENTATION	VERTICAL	
MAXIMUM LINEAR BUNDLE POWER	1.14 MW/m	0.348 MW/ft
AVERAGE LINEAR BUNDLE POWER	0.61 MW/m	0.186 MW/ft
MAXIMUM SHROUD TEMPERATURE	269.1°C	516.4°F
OUTLET PRESSURE	6.20 MPa	900 psia
PRESSURE LOSS	0.095 MPa	13.8 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	281.5°C	538.7°F
ABOVE TEST ASSEMBLY	252.5°C	486.5°F
AXIAL PEAKING FACTOR	1.51	
RADIAL PEAKING FACTOR	1.24	
MINIMUM DNBR RATIO	3.66	

PRECONDITIONING OPERATING CONDITIONS FOR OUTLET PRESSURE OF 1250 psia AND TEMPERATURE DIFFERENCE OF 50°F

VARIABLE	METRIC	ENGLISH
AVERAGE COOLANT OUTLET FLOW	20.50 ℓ/sec.	0.724 ft³/sec.
AVERAGE COOLANT OUTLET VELOCITY	5.24 m/sec.	17.2 ft/sec.
COOLANT INLET TEMPERATURE	244.4°C	472.0°F
AVERAGE COOLANT OUTLET TEMPERATURE	273.4°C	524.1°F
TOTAL POWER	2.232 MW	
MAXIMUM HEAT FLUX	121.9 W/cm²	0.386 MBtu/hr/ft²
AVERAGE HEAT FLUX	65.1 W/cm²	0.206 MBtu/hr/ft²
MAXIMUM LINEAR ROD POWER	36.8 kW/m	11.2 kW/ft
AVERAGE LINEAR ROD POWER	19.7 kW/m	6.0 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE		
WITHIN CRUCIFORM	289.4°C	552.9°F
OVERALL	294.2°C	561.5°F
MAXIMUM FUEL TEMPERATURE		
WITHIN CRUCIFORM	1145°C	2093°F
OVERALL	1404°C	2560°F
BUNDLE ORIENTATION	VERTICAL	
MAXIMUM LINEAR BUNDLE POWER	1.14 MW/m	0.348 MW/ft
AVERAGE LINEAR BUNDLE POWER	0.61 MW/m	0.186 MW/ft
MAXIMUM SHROUD TEMPERATURE	289.1°C	552.3 °F
OUTLET PRESSURE	8.62 MPa	1250 psia
PRESSURE LOSS	0.165 MPa	24.0 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	299.2°C	570.5°F
ABOVE TEST ASSEMBLY	273.4°C	524.1°F
AXIAL PEAKING FACTOR	1.51	
RADIAL PEAKING FACTOR	1.24	
MINIMUM DNBR RATIO	3.87	

PRECONDITIONING OPERATING CONDITIONS FOR OUTLET PRESSURE OF 900 psia AND TEMPERATURE DIFFERENCE OF 50°F

VARIABLE	METRIC	ENGLISH
AVERAGE COOLANT OUTLET FLOW	20.22 ℓ/sec.	0.714 ft^3/sec.
AVERAGE COOLANT OUTLET VELOCITY	5.14 m/sec.	16.87 ft/sec.
COOLANT INLET TEMPERATURE	222.2°C	432.0°F
AVERAGE COOLANT OUTLET TEMPERATURE	252.1°C	485.7°F
TOTAL POWER	2.232 M[illegible]	
MAXIMUM HEAT FLUX	121.9 W/[illegible]	0.386 MBtu/hr/ft^2
AVERAGE HEAT FLUX	65.1 W/cm^2	0.206 MBtu/hr/ft^2
MAXIMUM LINEAR ROD POWER	36.8 kW/m	11.2 kW/ft
AVERAGE LINEAR ROD POWER	19.7 kW/m	6.0 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE		
WITHIN CRUCIFORM	253.6°C	488.5°F
OVERALL	262.1°C	503.8°F
MAXIMUM FUEL TEMPERATURE		
WITHIN CRUCIFORM	1124°C	2055°F
OVERALL	1383°C	2522°F
BUNDLE ORIENTATION	VERTICAL	
MAXIMUM LINEAR BUNDLE POWER	1.14 MW/m	0.348 MW/ft
AVERAGE LINEAR BUNDLE POWER	0.61 MW/m	0.186 MW/ft
MAXIMUM SHROUD TEMPERATURE	267.6°C	513.7°F
OUTLET PRESSURE	6.20 MPa	900 psia
PRESSURE LOSS	0.168 MPa	24.3 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	278.3°C	533.0°F
ABOVE TEST ASSEMBLY	252.1°C	485.7°C
AXIAL PEAKING FACTOR	1.51	
RADIAL PEAKING FACTOR	1.24	
MINIMUM DNBR RATIO	4.08	

PRETRANSIENT OPERATING CONDITIONS (SAR)

AVERAGE COOLANT OUTLET FLOW	0.3908 m^3/sec	13.8 ft^3/sec
AVERAGE COOLANT OUTLET VELOCITY	94.2 m/sec	309 ft/sec
COOLANT INLET TEMPERATURE	163°C	325°F
AVERAGE COOLANT OUTLET TEMPERATURE	349°C	660°F
TOTAL POWER	141.36 kW	--
MAXIMUM HEAT FLUX	7.03 W/cm^2	22,288 Btu/hr ft^2
AVERAGE HEAT FLUX	4.12 W/cm^2	13,071 Btu/hr ft^2
MAXIMUM LINEAR ROD POWER[a]	2.13 kW/m	0.648 kW/ft
AVERAGE LINEAR ROD POWER	1.25 kW/m	0.38 kW/ft
MAXIMUM CLADDING SURFACE TEMPERATURE	428°C	801°F
MAXIMUM FUEL TEMPERATURE	473°C	883°F
BUNDLE ORIENTATION	VERTICAL	VERTICAL
MAXIMUM LINEAR BUNDLE POWER	65.91 kW/m	20.09 kW/ft
AVERAGE LINEAR BUNDLE POWER	38.65 kW/m	11.78 kW/ft
MAXIMUM SHROUD TEMPERATURE	354°C	670°F
OUTLET PRESSURE	0.276 MPa	40 psia
PRESSURE LOSS	0.058 MPa	8.4 psi
MAXIMUM PRESSURE TUBE TEMPERATURE		
AT TEST ASSEMBLY LOCATION	220°C	428°F
ABOVE TEST ASSEMBLY	315°C	600°F

(a) THE OUTER GUARD RODS HAVE THE LARGEST LINEAR POWER.

TRANSIENT PHASE OPERATING VARIABLES

REFLOOD RATE	1.3-5.1 cm/sec	0.5-10 in./sec
REFLOOD TEMPERATURE	325 K	127 DEG F
MAXIMUM LINEAR HEAT RATING PER TEST ROD	1.8 kW/m	0.55 kW/ft
MAXIMUM LINEAR BUNDLE HEAT RATING	55.9 kW/m	17.7 kW/ft
TEMPERATURE RAMP RATE	8.3 K/sec	15 DEG F/sec
DELAY TIME TO START OF REFLOOD	0 - 60 sec	

TEST MATRIX

TEST NUMBER	NUMBER OF TRANSIENTS	PRESSURIZED RODS	TYPE
1	18	0	THERMAL HYDRAULIC TEST SERIES
2	1	11	CLADDING PERFORMANCE TEST SERIES
3	1	11	
4	1	11	
5	1	11	
6	1	11	

PROTOTYPIC[a] THERMAL-HYDRAULIC TEST SERIES PLAN

TEST DAY	TEST SERIES NUMBER	REFLOOD RATE m/s	REFLOOD RATE in./s	DELAY TIME, s	HEATING RATE K/s	HEATING RATE °F/s	MAXIMUM CLADDING TEMPERATURE K	MAXIMUM CLADDING TEMPERATURE °F
2	101	0.254	10	39	8	15	1033	1400
2	102	0.102	4	39	8	15	1033	1400
3	103	0.076	3	33	8	15	1033	1400
3	104	0.051	2	7	8	15	1033	1400
3	105	0.047[b]	1.85	0	8	15	1033	1400
3	106	0.254	10	53[b]	8	15	1144	1600
3	107	0.102	4	53	8	15	1144	1600
4	108	0.076	3[b]	46	8	15	1144	1600
4	109	0.051	2	27[b]	8	15	1144	1600
4	110	0.037[b]	1.45	0	8	15	1144	1600
4	111	0.064	2.5[b]	60	8	15	1255	1800
4	112	0.051	2.0	50[b]	8	15	1255	1800
5	113	0.038[b]	1.5	22[b]	8	15	1255	1800
5	114	0.032[b]	1.7	0	8	15	1255	1800
5	115	0.028[b]	1	0	8	15	1310	1900[c]
5	116	0.076	3	33[b]	8	15	1033	1400
6	117	0.051	2	7[b]	8	15	1033	1400
6	118[d]	0.047	1.85	0	8	15	1033	1400
6	119-124[e]			TBD[e]				

(a) IN ALL TESTS, THE PEAK TEST FUEL ROD POWER WILL BE 1.80 kW/m (0.55 kW/ft), SYSTEM OUTLET PRESSURE WILL BE 0.28 MPa (40 psia), INLET REFLOOD SUBCOOLING TEMPERATURE WILL BE 78K (140°F) AND THE INITIAL FUEL ROD PRESSURE WILL BE 0.10 MPa (14.7 psia) AT STP.

(b) FINAL VALUE WILL BE SELECTED FROM EARLIER TESTS IN THIS EXPERIMENT.

(c) CLADDING TEMPERATURE MAY EXCEED 1255K (1800°F), BASED ON PARAMETERS EVALUATED FROM EARLIER TEST RESULTS. FOR SAFETY PURPOSES 1310K (1900°F) WILL BE USED AS THE MAXIMUM.

(d) REPLICATE OF TEST NUMBER 105.

(e) TO BE DEFINED BY TEST RESULTS FROM, AND WITHIN THE OPERATING ENVELOPE CONDITIONS OF, PREVIOUS TESTS IN THIS SERIES (TIME PERMITTING).

CONCEPTUAL FUEL CLADDING MATERIALS DEFORMATION EXPERIMENT(a) PLAN

EXPERIMENT NUMBER	REFLOOD RATE,		REFLOOD DELAY TIME,	HEATING RATE,		MAXIMUM CLADDING TEMPERATURE	
	m/s	in./s	s	K/s	°F/s	K	°F
EXP-CMD-200	0.127	5	32(b)	8	15	1033	1400
EXP-CMD-300	0.051	2	12(b)	8	15	1033	1400
EXP-CMD-400	0.036	1.4(b)	0	8	15	1033	1400
EXP-CMD-500	0.051	2	25(b)	8	15	1144	1600
EXL-CMD-600	0.051	2	40(b)	8	15	1255	1800

(a) IN ALL FIVE EXPERIMENTS, PEAK ROD POWER WILL BE 1.80 kW/m (0.55 kW/ft). THE SYSTEM PRESSURE WILL BE 0.27 MPa (40 psia). THE INLET REFLOOD SUBCOOLING TEMPERATURE WILL BE 78K (140°F) AND THE INITIAL ROD PRESSURE WILL BE 3.1 MPa (450 psia). FUEL ROD PRESSURE PROVIDES AN NRU HOT FUEL ROD OPERATING PRESSURE THAT SIMULATES A CONTEMPORARY PWR. ALTERNATE FUEL ROD PRESSURES MAY BE SELECTED FOR EXPERIMENTS 5 AND 6, DEPENDING UPON THE RESULTS OF 2, 3 AND 4.

(b) TARGET, FINAL VALUE WILL BE SELECTED FROM PREVIOUS EXPERIMENT RESULTS TO OBTAIN MAXIMUM CLADDING TEMPERATURES.

COMPARISON OF PREDICTED RESULTS

* THERM (COMBUSTION ENGINEERING)
* FRAP-T5 (INEL)
* GT3-FLECHT (PNL)

QUENCH TIMES FOR BUNDLE (SECONDS)

REFLOOD RATE (in./sec.)	DELAY, SECONDS 0	10	20	44	60
10	35	55	75		
5	80	100	125		
2	740	765	790	840	870

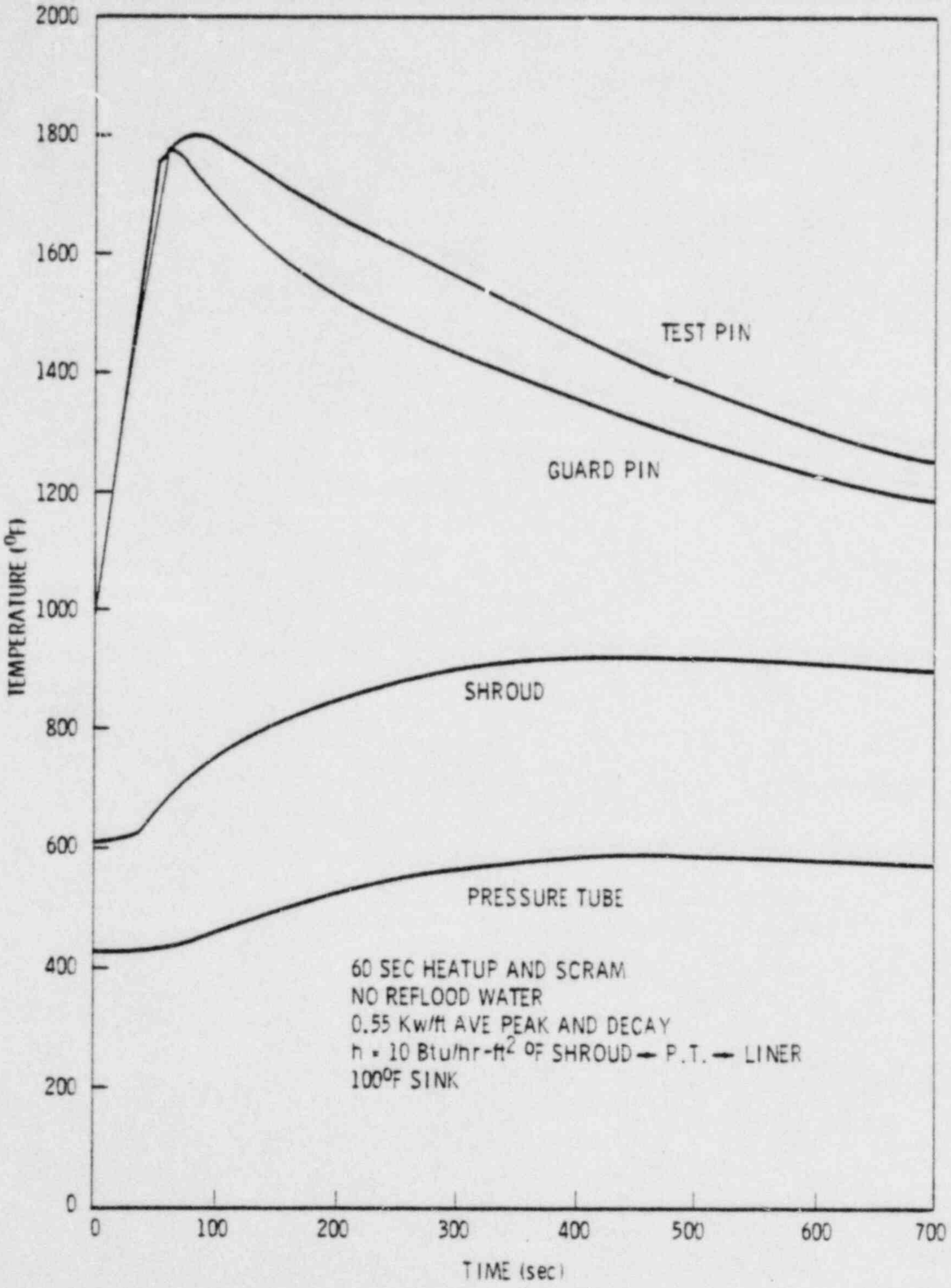
CE-THERM - FROM TABLES OF BUNDLE QUENCH TIMES

30	45	60		
50	70	85		
110	115	125	160	250

GT3-FLECHT - EXTRAPOLATED FROM PLOTS OF CLADDING TEMPS

—	—	—		
40	—	—		
105	—	120	130	150

FRAP-T5 - EXTRAPOLATED FROM PLOTS OF CLADDING TEMPS



Test Assembly Temperature for a SCRAM

PEAK TEMPERATURES

OPERATING MODE	TEST PIN CLADDING	SHROUD	PRESSURE TUBE
PRECONDITIONING	586K(595°F)	578K(582°F)	589K(602°F)
PRETRANSIENT	699K(800°F)	635K(685°F)	583K(590°F)
TRANSIENT	1255K(1800°F)	699K(800°F)	605K(630°F)

EXAMINATION TECHNIQUES

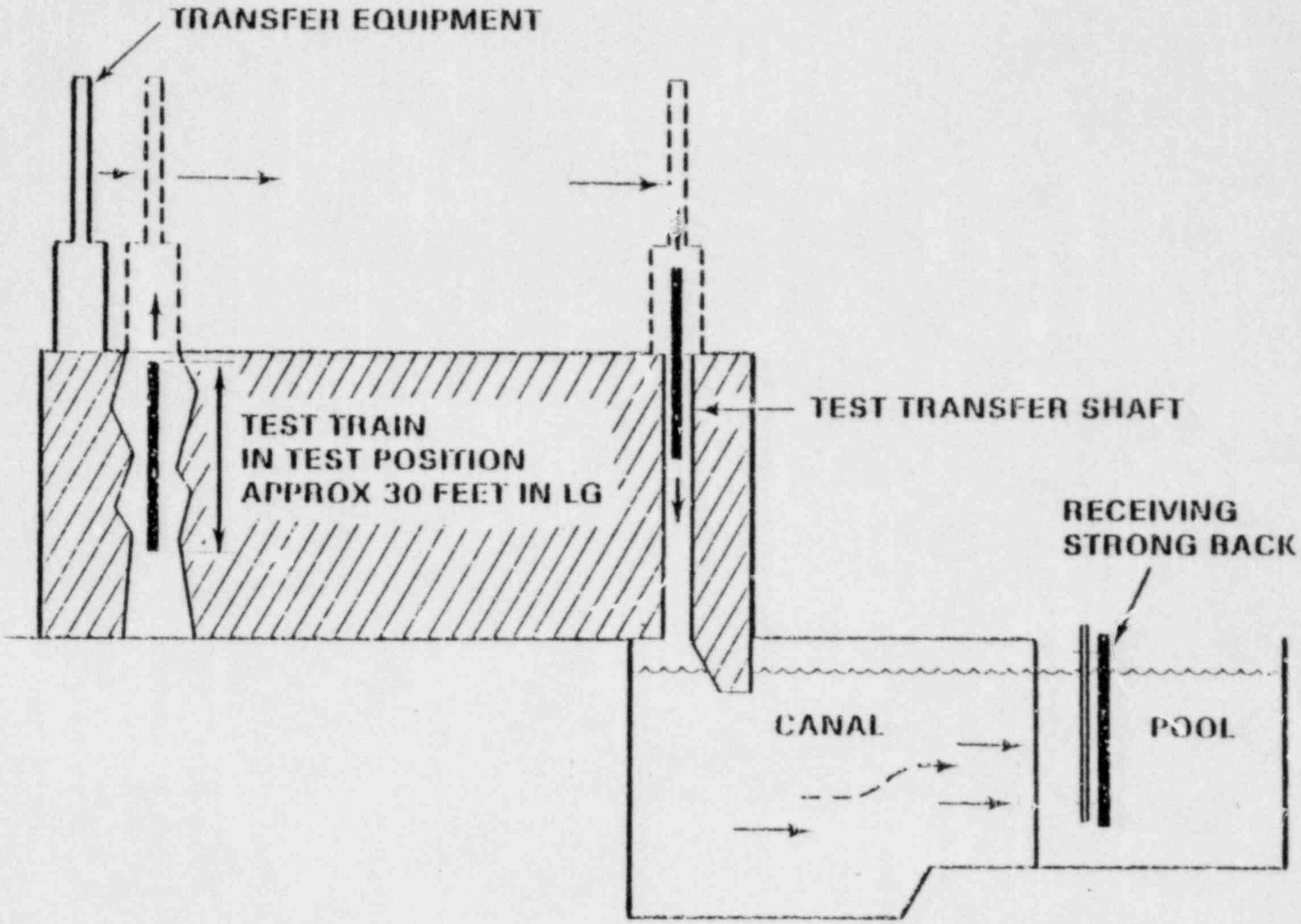
* DERM
  * DISASSEMBLE TEST TRAIN FUEL BUNDLE
  * REASSEMBLE TEST TRAIN FUEL BUNDLE
  * BUNDLE PROFILOMETRY
  * CRUCIFORM PROFILOMETRY
  * SINGLE-ROD PROFILOMETRY

* PHOTOGRAPHY

* GAMMA SCAN

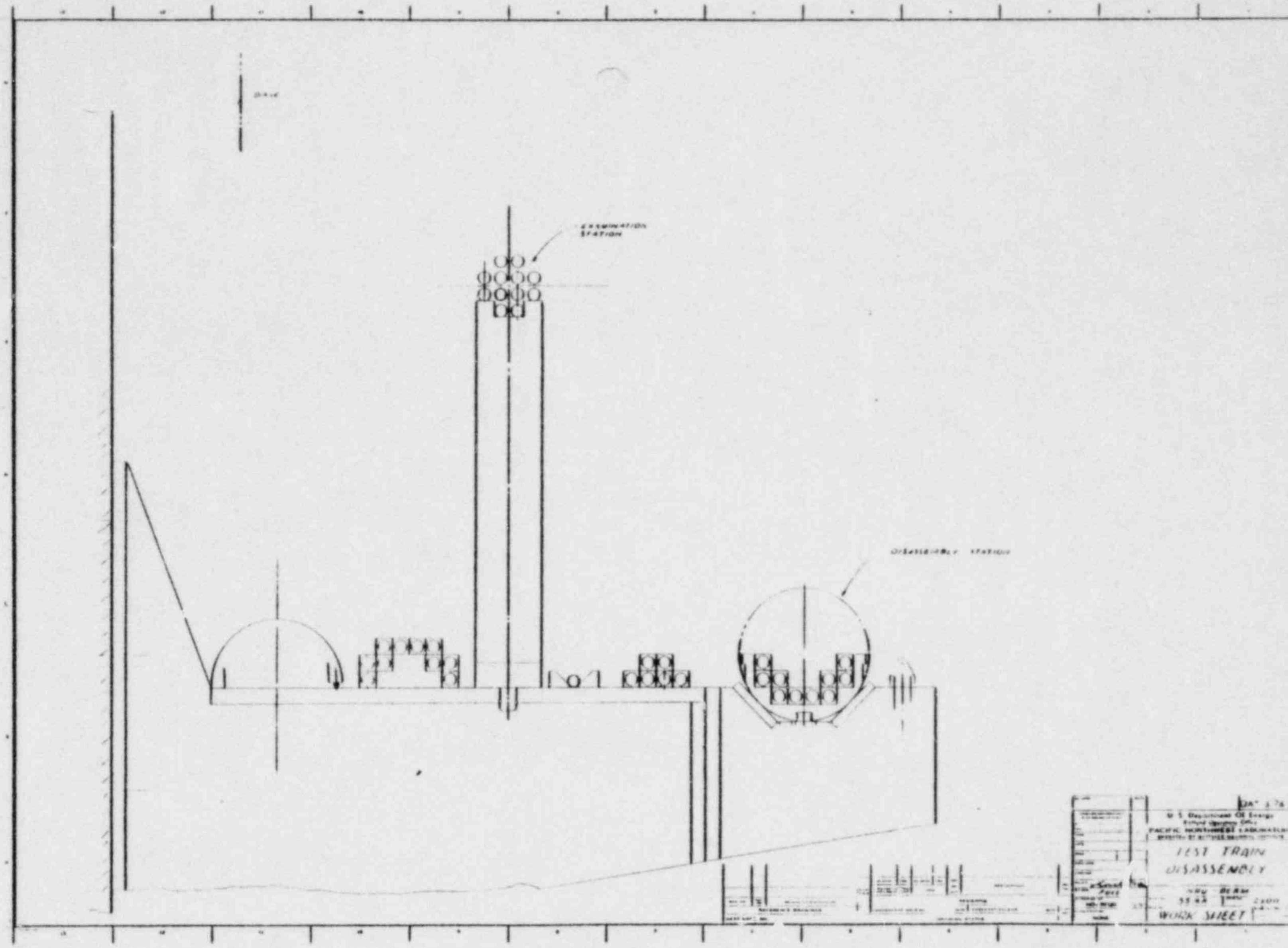
* DETAILED PIE

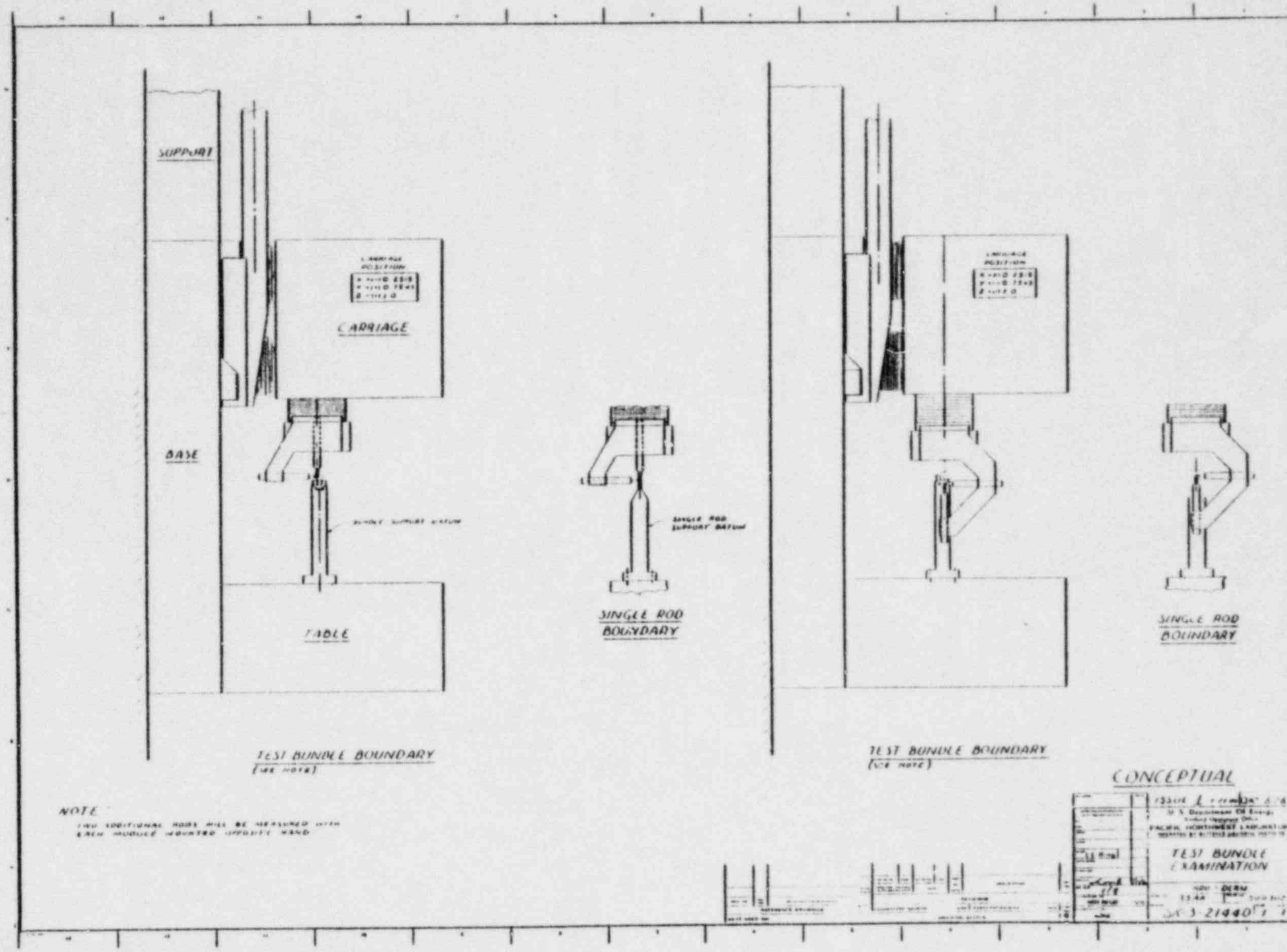
TEST TRAIN FLOW

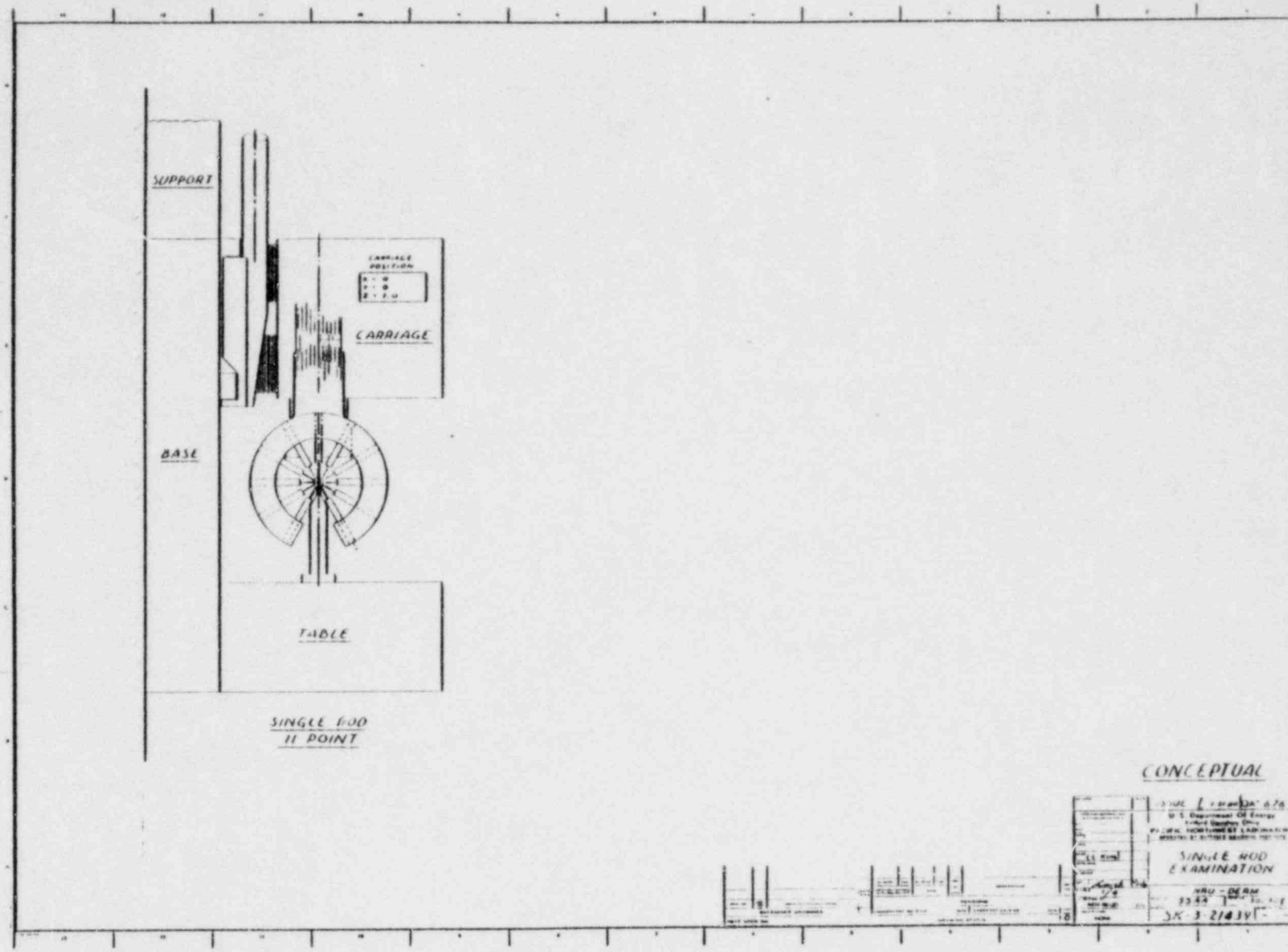


ELEVATION

TROUGH
CARRIAGE
BASE
EXAMINATION ZONE
OPENING
ELEVATION
(LOOKING EAST)
U.S. Department Of Energy
PACIFIC NORTHWEST LABORATORY
DCRM
LAYOUT
WORK SHEET


EXAMINATION STATION
DISASSEMBLY STATION
U. S. Department Of Energy
PACIFIC NORTHWEST LABORATORY
TEST TRAIN
DISASSEMBLY
WORK SHEET


SUPPORT
BASE
CARRIAGE
TABLE
SINGLE ROD SUPPORT DATUM
SINGLE ROD BOUNDARY
TEST BUNDLE BOUNDARY
(SEE NOTE)
SINGLE ROD BOUNDARY
TEST BUNDLE BOUNDARY
(SEE NOTE)
NOTE
TWO ADDITIONAL RODS WILL BE MEASURED WITH EACH MODULE MOUNTED OPPOSITE HAND
CONCEPTUAL
U S Department Of Energy
PACIFIC NORTHWEST LABORATORY
TEST BUNDLE EXAMINATION
SK-3-21440


SUPPORT
BASE
CARRIAGE
CARRIAGE POSITION
TABLE
SINGLE ROD
11 POINT
CONCEPTUAL
U. S. Department Of Energy
SINGLE ROD
EXAMINATION
SK-3-21434

APPLICATION OF EXPECTED RESULTS

* VALIDATION OF FLUID ENTRAINMENT MODELS (APPLICATION OT BOTH LARGE AND SMALL BREAK LOCA HEAT TRANSFER ANALYSIS)

* EVALUATION OF QUENCH FRONT VELOCITY MODELS

* EVALUATION OF CLAD BALLOONING MODELS

* FLOW BLOCKAGE MODEL DEVELOPMENT

10 CFR - COMPARISON WITH NRU TEST RESULTS

1.	10 CFR 50.46 (b)(1)	Peak Cladding Temperature
2.	10 CFR 50.46 (b)(2)	Maximum Cladding Oxidation
3.	10 CFR 50.46 (b)(3)	Maximum Hydrogen Generation
4.	10 CFR 50.46 (b)(4)	Coolable Geometries
5.	10 CFR 50.46 (b)(5)	Long Term Cooling
6.	10 CFR 50.46 (c)(2)	Evaluation Model Assessment
7.	10 CFR 50 Appendix (K)(I)(A)(2)	Fission Heat
8.	10 CFR 50 Appendix (K)(I)(A)(5)	Metal Water Reaction
9.	10 CFR 50 Appendix (K)(I)(B)	Swelling and Rupture of the Cladding and Fuel Rod Thermal Parameters
10.	10 CFR 50 Appendix (K)(I)(C)(1) (c)(2)	End of Blowdown (Droplet Entrainment Threshold)
11.	10 CFR 50 Appendix (K)(I)(C)(2)	Frictional Pressure Drops (Reactor Core) Two Phase Friction Multipliers used to Compute Maximum Clad Temperature
12.	10 CFR 50 Appendix (K)(I)(C)(3)	Momentum Equation Within Fuel Bundle During Reflood
13.	10 CFR 50 Appendix (K)(I)(C)(5)(a)	Post CHF Heat Transfer Correlations
14.	10 CFR 50 Appendix (K)(I)(D)(2)	Post Blowdown Phenomena; Heat Removal by the ECCS Containment Pressure
15.	10 CFR 50 Appendix (K)(I)(D)(3)	Calculation of Reflood Rate for Pressurized Water Reactors
16.	10 CFR 50 Appendix (K)(I)(D)(4)	Steam Interaction with Emergency Core Cooling Water in Pressurized Water Reactors
17.	10 CFR 50 Appendix (K)(I)(D)(5)	Refill and Reflood Heat Transfer for Pressurized Water Reactors

PROGRAM SCHEDULE

OCTOBER 1980	-	THERMAL HYDRAULIC TEST SERIES
JANUARY 1981	-	START MATERIALS TEST SERIES
JANUARY 1982	-	COMPLETE MATERIALS TEST SERIES
JULY 1982	-	COMPLETE PIE WORK
JANUARY 1983	-	FINAL REPORT