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UNITED STATES
NUCLEAR REGULATORY COMMISSION

In the matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS,
SUBCOMMITTEE MEETING ON REACTOR FUEL

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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

Tuesday, April 29, 1980

The Advisory Committee on Reactor Safeguards, Subcommittee Meeting on Reactor Fuel, met, pursuant to notice, at 8:30 a.m., Paul Shewmon, Chairman of the Subcommittee, presiding.

PRESENT:

DR. LAWROSKI
MR. J. CARSON MARK
MR. WILLIAM MATHIS
MR. DAVE OKRENT
MR. A. BEMENT
MR. FRED NICHOLS

P R O C E E D I N G S

8:30 a.m.

CHAIRMAN SHEWMON: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee Meeting on Reactor Fuel.

I am P. Shewmon, Subcommittee Chairman.

The other ACRS Members present today are: S. Lawroski, J. C. Mark, W. Mathis and D. Okrent.

Also in attendance are ACRS Consultants: A. Bement and F. Nichols.

The purpose of this meeting is to begin discussion of the NRC Fuel Behavior Research Branch programs for the ACRS annual reports to the Commission and Congress.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act.

Mr. Paul Boehnert is the Designated Federal Employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on April 14 and April 25, 1980.

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

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

We will proceed with the meeting and I call upon Dr. William Johnston, Chief of the Fuel Behavior Research Branch.

-- change in geometry of the core.

MR. JOHNSTON: Since Three Mile Island we -- within the branch we've developed a code module, you might say, called TMI boil, which was done by George Marino. That covers the oxidation and metallurgical aspects of what happens up to the point of change of geometry. It does not handle change of geometry yet.

CHAIRMAN SHEWMON: Okay.

MR. JOHNSTON: We have no specific code ourselves that handles it. We make use of the marche for alcodes that Batelle Columbus has, which I go through that type of sequence. And through our German exchange we'll word codes like "smelts 'em", "clabbering," and a series of codes that they have which we are getting a hold of.

CHAIRMAN SHEWMON: Is the one -- is the Batelle code a one or two-dimensional code?

MR. JOHNSTON: It's a one-dimensional code I'm sure.

CHAIRMAN SHEWMON: Okay.

Yes.

MR. MARK: Am I right that boil was used in the reactor safety study?

MR. JOHNSTON: The boil code is a part of the Marche Code at Batelle Columbus. The TMI Boil Code is a -- I don't know why we picked the same name, but it -- it's an entirely independent code that was done by George. It has some advances in it that is not in the book, the Batelle version of boil.

MR. MARK: I see. Well, I was aware that that main was used in --

MR. JOHNSTON: I know it.

MR. MARK: -- WASH-1400 for the same calculation.

MR. JOHNSTON: That is correct. And I was simply -- we had a different name for portions of it before, and we changed it. But TMI boil, it was simply meant to cover the TMI boildown. And that was the, I think, the gen -- the genesis of the name.

MR. MARK: Son of Boil, maybe.

MR. JOHNSTON: Son of Boil, yeah.

It has the -- one of the unique features that it has is that it covers the heat exchange between the steam and the cladic in the upper portions of the core. So, that it more accurately models than -- than the original version of boil.

The actual transfer of heat from the lower part of the assembly to the upper part, and it includes the heat exchange in both directions with the steam.

MR. MARK: So, you'd say that it is as good plus some obvious improvements as the older one?

MR. JOHNSTON: That's my understanding of it.

Dr. Marino is here. I guess he could comment --

DR. MARINO: I'd like to add a few more comments. The TMI boil code was done inhouse and is not as sophisticated as I'd like it to be.

This morning I'll mention that we are beginning to plan the small break transient code based on FRAP-T, MEMPRO and FREPÇON at EG&G in physical '81, which will take the best characteristics of TMI boil and those other three codes and hopefully supply us with a very good small break transient code up to and through melting.

Now, if we do the melting part, we are going to coordinate our work with some of the German work. The Nelson code developed a split guard based on Hoggins data at split guard in KFK.

MR. OKRENT: Could I --

CHAIRMAN SHEWMON: Okay.

Yes.

MR. OKREN: -- try to understand a little bit more about the objectives starting at the bottom since somebody has asked up. Just what do you visualize as your objective when you say "utilize models and codes to assess the consequences of severe reactor accidents including core melt events"?

MR. JOHNSTON: It's -- many of the events that we can postulate that may happen in the sequence of a core meltdown; and I use that in a broader sense, cannot be reached explicitly by the experimental techniques that we have available. We don't have a big enough systems, things of that sort.

Therefore, it's been our feeling that we have to take the small scale information that we have available and combine that in the form of model codes, which we will then use to try to describe the larger-scaled events. That -- that's really all I --

MR. OKRENT: Well, what I --

MR. JOHNSTON: -- that means.

MR. OKRENT: -- I'm getting at is I can't define in my own mind what is the objective -- those words are too general for me. So, I'd like to know --

MR. JOHNSTON: Well --

MR. OKRENT: -- what the --

CHAIRMAN SHEWMON: David will spend the day getting into that.

MR. OKRENT: Okay.

No, I --

CHAIRMAN SHEWMON: There are specific items in the program which will -- which will explain it to us.

MR. OKRENT: There are?

CHAIRMAN SHEWMON: Yeah.

MR. JOHNSTON: We will be -- yes. Now, let me -- there are two caveats involved. We will be going into certain aspects of this at the Fuel Behavior Branch as responsibilities for it, and effecient product area today. But the general discussion of this area is reserved, I think, for a meeting that's coming up on May 9th in Chicago, which will be the general discussion of the integrated core metal program. So, that we don't -- had no expectation, at least, of talking about seeing explosions and concrete melt interactions in that aspect of the fuel melt part of it today.

This is a -- we're in a transition I think, in this particular area. And the intergrated efforts that research has been putting together is going to be discussed in toto at that May 9th meeting. We sort of excluded that from today.

MR. OKRENT: Okay. So, that then really --

MR. JOHNSTON: That's our --

MR. OKRENT: Not only not -- not a subject for today's meeting, but it may not be an objective solely within this group; is that what you are saying?

MR. JOHNSTON: It is not an objective solely within this group.

MR. OKRENT: All right.

MR. JOHNSTON: That's correct.

MR. OKRENT: Let me ask the next one up, it says "verified fuel code models with integrated tests." First, what do you mean by "verify"?

MR. JOHNSTON: That's an old word. We now use the word "assess" as a replacement. I think it's semantics. But the point is that when you generate a code at -- at one scale level you have to have some feeling as to what its applicability is to the larger scale.

In fuel we have some advantages, and at least radially we work essentially full scale. Actually we usually do not in terms of the facilities that are available.

But the assessment basically means comparing the predictions of our codes; be it Trapcom or Frap-T, against data which we have obtained from, essentially, the real world of reactors wherever possible. Much of the -- much of the code development is done from tests that are run as separate effects tests, small scale things, and so forth.

Then, we collect an independent data base --

MR. OKRENT: Normally we --

MR. JOHNSTON: -- from commerical reactors and make the comparison between the predictions and the results.

MR. OKRENT: Excuse me.

One of my problems in trying to follow this program is trying to see what the real objectives are and so I think it is important to understand, for me, what they are and that's, again, the -- an insufficiently defined term. Now, the top one says "evaluate fisson product and fuel behavior under normal and accident conditions." That, again, is very general terms. Can you --

MR. JOHNSTON: Well, the A --

MR. OKRENT: -- name more specific objectives than that?

MR. JOHNSTON: The ACRS in 1972 wrote us letters; I didn't -- I realize I left it on the desk -- as well as in your 1977 reports, said that it is our responsbility to find out about all the possible things that might go wrong with the fuel element or the fuel assembly so that we know --

MR. OKRENT: Gee, I hope we didn't say all.

MR. JOHNSTON: -- what -- well, it said a broad spectrum. It said not the LOCA. Everybody else in the country was chasing LOCA's. We were looking at -- at all the other possibilities.

In past years I have started off with a slide that says, "Look, what are the things that can happen to a fuel assembly"? You can have a power change; you can have a loss of flow; you can have an increase of reactivity. Take what the basic parameters that can change that are going to effect the enviroment around the fuel assembly, and if we have an understanding of what happens under those conditions, we've covered basically, we felt, all the things that can affect a fuel assembly.

When we have an understanding of those things -- that's basically what it means. When we started out, for example, we said, "Are there things that are going to happen that we don't -- haven't thought of yet"?

We didn't know whether -- in the beginning whether a power pooling mismatch was extremely critical event or not And the pri -- and one of the purposes of the IMPOWER Program was to exercise the fuels under enough different situations that we felt that there weren't some that hadn't been covered that would pop up and bite us some time in the future.

So, we have had as an objective to do a broad scale evaluation of these sort of events. And I guess that -- that's basically what we have tried to say here.

Now, from -- from the point of view of the people in regulations, the -- the -- much of their work is involved in assessing design, looking at the inputs that come in from the vendors. It has an awful lot to do with -- with normal and slightly off normal situations; the understanding of stored energy, and all the things that go with that-- appellet cladding interaction or the -- the normal failures that we get in reactors under normal operation, called TCI. That sort of thing is all part of what I guess would say comes under normal. But that's very much the bread and butter of the way that it's done in NRR as well as our responsibilities of looking at the more extreme conditions.

We take these -- we've discussed them as particular types of accident in the past, and I think I can go into that a little bit more if you would like me, too. But these are intended to be fairly general. I -- I don't -- in order to put it on a slide I've done that on purpose.

MR. OKRENT: And what is the reason why the NRC is looking at fission product and fuel behavior under normal conditions?

MR. JOHNSTON: The normal amount of person rams that are released in normal operations are at least two times the total amount that was released at TMI. About 2000 person rams is the total dose to the public at TMI. The normal releases from our reactors are somewhere in the order of 5000.

MR. OKRENT: I'm aware of that. But I -- in this research program, I'm still trying to understand at the moment why there is a section which is looking at what we call normal fissions.

MR. JOHNSTON: I have -- when we go into the closed session we will have a category of programs and -- and to summarize it right now I will -- the-- the under -- the --

MR. OKRENT: Do you -- it's not what; it's why. I'm trying to understand why you're giving that.

MR. JOHNSTON: The condition that the fuel is in before an accident initiates influences the sequence of what the fuel does. If the cladding has been damaged by all sorts of power transients and PCI type events in its previous history, we expect that it will probably fail under much milder conditions than if it did not have that previous history. Those are the kind of concerns that are expressed in connection with the -- the high burn-up of fuel which is being carried through by all the vendors at the present time with the aid of EFRI and the DOE -- and DOE.

There are a few issues that we have identified in connection with that program. One of them most certainly is the pelt clad interaction, the previous damage to the fuel.

A VOICE: Fission gas release in what pressure is there --

MR. JOHNSTON: Fission gas release is the other one. How much is in there at -- as the burn-up increases -- the quan -- is the fraction efficient gas produced release itself at a higher rate into the gap.

MR. OKRENT: And it's felt that this --

MR. JOHNSTON: Their point is storing energy. Most of the uncertainties with regards to the LOCA calculations and the power loads permitted in the reactors have to do with what's the initial stored energy. That's strictly determined by the condition of the fuel and the gap, and the amount of cracking in the fuel before the accident begins.

MR. OKRENT: And it's felt that this is an NRC responsibility.

MR. JOHNSTON: I think NRC feels it's very definitely a responsibility.

They must make licensing decisions on just these matters daily.

And one of the points that I want to convey to you today is that the program has been going on for a number of years. We have been recently reevaluating it with the idea of changing the priorities and directions of the program. And in doing that there's -- as a kind of a preliminary to that I'd like to show you a few viewgraphs that I think were presented to you in, I'm not sure exactly, but I think it was either 1976 or 1977, which show what our program was at that time.

And what I would like to do is show you, as I go through this sequence, the kinds of things we were doing then and the results from that, and what we are really going to be talking with you -- what we think we will be doing in the future.

I have three viewgraphs, and basically this is the principal content of our program. And I think it was in either '76 or '77 that we presented it to you.

These three are not in your passout. I just -- I looked these things up yesterday, and I didn't get the chance to stick them in. But I -- it's more that I want to give you an impression rather than a lot of detailed facts, but what I want to point out is that we had a large program in looking at zircaloy. We had intentions of finishing the work in certain time periods, and that's what -- actually this didn't say finish, but it said major results. What I would like to convev to you is that in nearly every case as I go through here that work has been completed. And I will show you a large number of programs which have been finished in the last couple of years showing that we can set goals in this program. We do get significant results and the use -- and the results are being used.

The zircaloy oxidation was, of course, mandated as a part of the ECCS hearing results, and that information is resulted in the Cafcart Fall equation which is becoming standard for looking at high temperature oxidation up over the 2200F.

Properties of zircaloy contianing oxygen and the strength and -- well, this one is the -- Batelle, is the argon program, which has resulted in a new and -- imbrittlement criteria.

Strength and ductility have irradiated, was the Battelle Columbus program. Incidentally, this has been completed since that time. This was subs -- this was completed last year. Strength and ductility was completed last year. That's the Battelle Columbus Program. I'm looking at whether the radiation makes any particular difference on the amount of ballooning that -- and deformation of zircaloy undergoes.

Deformation of reactor operating temperatures was a portion of the Battelle Columbus Program in which we were doing expanding mandrel tests on the inside of the side of the fuel. More for giving us some beginning work on looking at the PCI program and the effect of irradiation on that aspect of it.

Deformation at elevated temperatures is the MRB multi-rod-burst test program at Oak Ridge which is still continuing and is not finished yet. And that's one of the programs that is become of a great deal of interest in the last six to eight months.

Steady state fission gas release is a -- was a collection more of the information from around the world and what's being obtained in industry rather than efforts of our own. We did do that and supply information to the licensing people in that time period.

Transient gas release experimental part of that was completed this past year. That's an argon program using the direct electrical heating types of apparatus.

Pellet geometry and restructing was a prog -- a program that was conducted in part by EPRI at the argon, and also programs that we had going in the Halden Reactor, both sponsored by the Battelle Northwest and by EG&G Idaho. Those tests -- there are a couple more tests still in the reactor in Halden, but a number of reports have come out on the pellet restructing and the effect of this both on -- on gap conductants and on pellet clad interaction.

We're finally reaching a point where we can now use the same code models to describe both the mechanical and the thermal properties of the fuel. We've nearly always -- people have used two separate modules because there was an inconsistency.

The pellet decay heats, the decay heat program that resulted in a new ANS decay heat standard, which was finished in that time span.

Gap conductants out of power was finished in 1979 not 1978 as we said. Actual gas flow was a series of programs that -- done both out of power and in power. We anticipated that the EFA 430 in Halden would be complete by this date. It's not complete. They've gotten the major results, I think, already since it went in a year ago. And the bottom line is that the actual gas flow is -- is rather open as it turns out and not particularly restrictive.

So, that one is -- pellet cladding interactions is one that we did not meet our time schedule on because subsequent to the time that we put this together we had to essentially terminate most of the expectations on that program because of recommendations of the budget review committee within NRC. So, that one we didn't do.

Now, we have yet to do it.

MR. MARK: May I ask, you mentioned the Cafcart somebody.

MR. JOHNSTON: The Cafcart Fall.

MR. MARK: Fall. Equation for oxidation of zirconium.

MR. JOHNSTON: Yes.

MR. MARK: Is that an updating and improvement on --

MR. JOHNSTON: Baker/Just.

MR. MARK: -- what is it? Baker/just.

MR. JOHNSTON: Very definitely. Yes.

MR. MARK: In what way does it give a different picture? The oxidation rates are higher, or lower, or just how do they differ?

MR. JOHNSTON: The oxidation rates are lower. The activation energy is lower. In other words, the slope --

the slope is substantially lower than the Baker/Just slope.

The uncertainty of the data -- the scatter of the data is greatly reduced.

MR. MARK: Right.

Well, now, the Staff, perhaps it's in a different section, has recently made an estimate of oxidation of zirconium in connection with the recommendation on inerting. Did they use the Cafcart Fall, or do they stick with a different -- earlier version?

MR. JOHNSTON: They used Cafcart Fall in this I understand.

Officially for licensing purposes, though, Appendix K they still are required to -- by the rules to use Baker/Just.

MR. MARK: Well, I was suspecting that. But if you were trying to form a real picture you would not do that?

MR. JOHNSTON: That's correct.

I don't want to belabor the -- the points, but the -- FRAP S's has been completed and changed to FRAP-Con and done so in concert with the core performance branch in licensing FRAP -- and that has reached the point now of no further development. It's now in a maintenance mode. The same is true with FRAP-T. The -- all of the LOCA modes and such things are in the FRAP-T sequence, and we are essentially at a point now where we can say the basic code is developed and the point we're at now is merely to clean it up and incorporate minor changes that come in with -- from new data so that we are not in a large development mode there.

The material property correlations are in the same state. They are mostly in. We have statistical uncertainties now ascribed to almost everything in the natural book so that we can quote one and -- one and three sigma uncertainties on the material properties data right down the line.

Efficient product code called TRAP now which does look at the -- more of a core melt situation, particularly inside the primary system is under development at Battelle Columbus. The TRAP that described the LOCA accident was completed in that time span. The continuation of it to go into the core melt is -- is continuing. It's in kind of a interim period right now because we have had to go out for bids on it. And the bids are due in next week. And for about the last eight months it's been in a holding pattern because of our inability to get a new contract -- new contractor with whoever it's going to be that wins the bid.

The molten core concrete area, the intercode was developed back in this time period and it's since been replaced by an improved version called Corecon, which is a much improved version, much more complex and detailed. And that was completed, the Corecon was completed this past year.

Let's see. As far as the verifications are concerned, I think the basic point was that we started in that time frame to do statistical uncertainties in the predictions of our codes, and we've essentially been continuing that since the data base is large enough that we are unable to quote now as a result of our own assessment procedures. The uncertainties at one sigma, at least, on all the aspects of the code predictions.

MR. OKRENT: Let's see. Are you able to predict the t hings like PCI with the FRAP code?

MR. JOHNSTON: PCI we could not do yet in the FRAP code. There is a code called Profit which has been developed through the Tech-assistance Program. We are going to be taking over the work in that area starting physical '81 with the intent of either adding boon module to FRAP or maybe free-standing code which will take care of that problem. We're not the only people working on that.

George Marino.

MR. MARINO: I'd like to add to that that even though we don't have a stress corrision base PCI model in the code, we do look at pellet cladding interaction via transferal of stresses from the fuel to the cladding and entrap -- core entrap team.

MR. JOHNSTON: That's true. We have the mechanical models; we don't have the chemical aspects of it in there.

MR. OKRENT: Well, I know it's in the FRAP code, so I've been trying to see where you think you are, and where you should be, and why.

MR. JOHNSTON: Okay. George is going to go into some depth on the FRAP code. We think it's got the things in it now that it needs to have with possible exception of what we just spoke of. We're not anticipating a great deal of additional development of it.

Probably I should stop going through all this. These -- the unmixed oxide we obviously didn't do because that became a dead issue. The load -- following programs were PCI related thing and we did very little in that for the reasons I mentioned before.

We have been following the program particularly that EPRI has supported, and more lately, DOE in which they are running pilot bundles and -- in the commerical reactors in cooperation with the vendors. There is a detailed poster radiation examination of those pilot assemblies, and we are following that work as it proceeds.

We didn't get any results in that time frame because they -- the people that were running the program didn't organize it in that manner. In fact, they are just now getting to the point where they're putting the -- the data that they have received on some kind of a data acquisition system that will make it more readily available.

MR. OKRENT: Excuse me. If I can interrupt again.

But looking at these charts and seeing the column over on the right that says "major results", if I hadn't been following the program I might get the impression that in fact you'd find your objectives originally and you'd really gotten principal things you were looking for in the years shown at the right-hand side.

A VOICE: It'll keep. Go ahead.

MR. JOHNSTON: I think you'll find that for the most part true.

MR. OKRENT: Well, is there some time today a -- when you will define the thing that you really wanted to know at the beginning of a program and show then how you found this out? That that's different than saying "I ran an experiment, and I got some data."

MR. JOHNSTON: Major results means more than just getting uata. That means getting results from which you can draw conclusions.

MR. OKRENT: Okay. Well, that would help me quite a bit and in particular you could relate these either to a question that you had before you during the experiment or had you learned something nobody anticipated before you did the experiment, I would appreciate that during the day you could point that out to me.

MR. JOHNSTON: Okay. What we are prepared to do today is to do it in every area except these two. These two are subject to later meetings, and I just now got to this on the slide. But I -- I -- to cathcart the zircaloy oxidation is a perfect example of setting a double hoist to -- was to define the extent of oxidation and determine the uncertainties in that number because Baker/Just was a very uncertain number depend -- based upon a couple of points that were taken at the melting point of zircaloy.

The goal of that program was to redefine the rate of zircaloy oxidation as a function of temperature. We did it, and we gave you one sigma -- we gave you three sigma limits of only a few percent uncertainty.

The other part of that program had to do with the rate of diffusion -- the kinetics of diffusion of oxygen in zircaloy because that determines your alpha-beta phase boundries and imbrittlement rate. That kinetics work was done with that specific problem of looking at the 17 percent imbrittlement criteria and whether it was a good basic criteria or not. That was done specifically for that purpose and there are -- again, we've got it to about a 10 percent one sigma, which is a -- an outstanding advance from a kinet -- from a diffusion type of a program.

The decay heat was specifically because of the present condition is to use the ANS plus 20 because the ANS had about a 20 percent uncertainty in it.

As a result of that program, and we ran three different contractors and EPRI ran two, and the result is that the best estimate is less than the ANS number by about three percent. And we now have a three sigma limit on the -- on that work of about three per -- of about eight percent.

We greatly reduced the uncertainty and -- and updated the real numbers -- the best estimate numbers for decay heat.

Now, I can give you that kind of statement for each one of these things.

MR. OKRENT: That would be helpful. I think, in fact, that those two just mentioned are areas where there were goals, and in fact, if I understand the situation, you have in fact advanced the state of knowledge in a significant way. And it would be helpful to me if you could show the same kind of thing in the other areas.

MR. JOHNSTON: Well -- yeah, I think I shouldn't take a great deal more time --

CHAIRMAN SHEWMON: Do all the programs have to be successes? I mean does any other division have that average?

MR. OKRENT: Oh, no, no. But --

CHAIRMAN SHEWMON: I see. Okay.

MR. OKRENT: We might say negative results. That's okay. I mean I --

CHAIRMAN SHEWMON: I'm not saying that they don't have that thousand batting average, but then I just -- some people settle for three hundred.

MR. JOHNSTON: I think I'll stop going through this. I think I -- we've gotten to the point where the two -- the programs that we are not covering I -- I can make similar statements about them.

I would just show one summary of the PCM. Now, in 1972 and in past times, and I'm a really little bit talking about a different program, but one of the big concerns, in fact, number one priority in the ACRS was what -- what are the problems with pellet cladding -- I'm sorry, power cooling mismatch? And what's the possibility of getting a wholesale damage in runaway heatups and clad melting, and that sort of thing?

And the results of that power cooling mismatch program has been to define -- well, first, we didn't find all those terrible things that we were concerned about. Secondly, we were able to come up with what amounts to a failure mechanism under those conditions, which is basically a -- an oxidation of the cladding.

And we were able to take on a time and temperature basis develop a curve which if it exceeds this we can pretty well predict whether the clad is going to fail under power cooling mismatch conditions, or whether it is going to remain ductile and the fuel is not going to fail.

We've taken the atlas calculations, which are used by licensing using steamline break, and the worse case is bounded by this situation here. Before three -- and this was done before Three Mile Island. Now, you know, when you start to put in the multiple failures which were not done at this time, it will change some of this. But using the atlas calculations, which are the standard licensing basis, I believe that the order of 1100 seconds of so is a more -- the longest time that the fuel is predicted to be in steam-boiling and -- I mean in steam heat transfer mode. And that produces a calculated amount of oxidation which is less than our curves, which is a quantitative result which has been the subject of a new Reg, and I think communications with ourselves and been used by licensing. That happens to be the first inpilot program, and it was the highest priority --

MR. OKRENT: Excuse me.

MR. JOHNSTON: -- at that time.

And I think it produced some specific results.

MR. OKRENT: As one who participated in -- in the wording that power coolart mismatch was a high priority, I would say that I had in mind much greater mismatches than you had done in any of your experiments. In fact, the range in which you have looked is not the range in which there was the original interest which dates back to about 1967.

You have not done experiments in that range. So, I -- I don't think you should act as if you are meeting the ACRS number one priority in this area. I think that's incorrect.

MR. JOHNSTON: The power levels at which we have run these experiments have been up to 28 kilowatts per foot. Normal reactors are running at about 8. The majority of those measurements were made at the order of 18 to 20.

MR. OKRENT: I'm sorry. The question --

CHAIRMAN SHEWMON: If I were to criticise the program --

MR. JOHNSTON: Two or three times the --

CHAIRMAN SHEWMON: -- was they blow the damn things up so fast you -- it's irrelevant. But you're saying that they don't blow 'em up fast enough.

MR. OKRENT: No, no. I'm sorry.

The question was do you --

MR. JOHNSTON: We're working at three times the level that a reactor can experience. That seems to me to be -- it's all the capability we have in the plant. It's far more than the capability that any reactor can produce.

MR. OKRENT: I'm sorry. The questions that were of interest back as far as 1967 was where you had enough of a mismatch that in fact you not only melted fuel, but you could fail rods with molten -- fail cladding with molten fuel, possibly getting out. And you have not done that class of experiment.

MR. JOHNSTON: That was the concern that that might be what nature was going to produce. The experiments that we conducted show that nature did not produce that kind of a result, and we couldn't manufacture something that was against nature.

MR. OKRENT: Are you telling me I can't run an experiment in which I melt fuel in a water reactor?

MR. JOHNSTON: You can't do it with the normal power levels that you have in a reactor and have any water in that system.

You can't do it with three times the power levels that you have in the reactor if you've got any water in the system.

I can set up artificial conditions in which I can --

CHAIRMAN SHEWMON: Let's give him a turn for a minute. Yeah, let's --

MR. JOUNSTON: -- produce this sort of stuff. But it's got nothing to do with normal operation or power cooling mismatch.

CHAIRMAN SHEWMON: When power conditions were concerned.

Will you please be quiet for a minute, Bill.

MR. OKRENT: In the first place there was concern about the misloading of one fuel element in the position where you had the wrong enrichment, which would give you, perhaps, a factor of two over whatever you consider normal. And the second was, as you well know, that you might block the coolant coming into a subassembly, in particular, in the BWR design. But this -- a lot of this question arose in connection with Browns Ferry. And you have experiments in your program you haven't reached yet. And to tell me that you are unable to melt fuel in a water reactor is just, I think, inappropriate.

Let me -- I'll use a mild adjective or adverb, whatever it is.

MR. JOHNSTON: We've operated for fifteen minutes with over 80 percent of the radius of the fuel assembly molten.

MR. OKRENT: Of course. And people were running power reactor fuel trying to develop a molten center fuel and -- back in the '60's. And -- over -- and not just minutes. So, that doesn't -- that doesn't answer the kind of issue that people have in mind.

I'm just saying I think you're misrepresenting the concern. The concern was not in the area in which the investigation has been done.

CHAIRMAN SHEWMON: And with that, let's move on to where we should be about now, okay?

MR. JOHNSTON: Okay.

That is in your viewgraph, and I'll just pass quickly. It -- it simply lists the programs that have been completed in the past year. I'll -- it shows that the number of programs have been finished in this area.

The next one is a view graph that I actually showed you last year at this time, but I thought it was still useful, and that is to show that the programs have been conducted under this program were used as far as the NRC's evaluation and understanding of what happened at TMI. And that relates to the decay heat standard, the zircaloy oxidation, the clad ballooning work, the zircaloy imbrittlement, the work on the utetic between UO_2 and zirconium was the result of our exchanges with the Germans. We are able to state that under the conditions of TMI that steam explosions were unlikely on the basis of that work.

All these other things were actually usages that were made of the work that we had done previously.

The point was is that we feel that a good bit of this work has been done and it's time to start looking at priorities again and reassess things.

When we started to do this as a group, we started looking at preceding through the idea of using eventries or consequence diagrams as a basis for determining these priorities. When we did that -- in particular, when we looked at the consequence diagram we were -- always came down to the bottom line that the fission product release, of course, was the basic thing that we were all interested with. And that that should be the focus on any -- any program in reactor safety.

The problem was how do you set priorities in doing that? And we began to put together a consequence tree. Two things happened fairly rapidly. The tree splits into two directions. One of them has to do with the kinds of releases that you get from the very severe accident such as TMI and -- and the ones that are much more severe than we postulate. But right along with it are the releases that you get from normal operations of the plant.

And as I have mentioned before, the releases from normal operation of the plant are actually a good deal larger even than we received from TMI. So, that we can't just out of priority say that releases from normal plant operations are "no never minds" because in terms of contribution to the public risk and some of the public discussions that goes on, a little over-radiation is also an important concern of people. And there's need for data on that so that we carried the consequence thing through for a little bit and decided that that wasn't going to give us a particularly fruitful way to try to establish priorities either because it kept saying we had to do several different things.

So, what we finally did is we -- we used this criteria for setting priorities on our -- on the future work in the program and there are three major ones and three ones that are more administrative, perhaps, or a little bit different from the top three.

This has to do -- is the program going to obtain information which will be used either to establish new licensing criteria or to assess or confirm existing licensing criteria?

The second was that will this information help us to better under -- to improve the response to an accident once it starts or to mitigate or give us opportunities to do something or other to change the direction of it once it starts.

The third one, does it give us information on mechanisms for fuel failure or efficient product release, that being basic understanding that might well be needed to take care of the other two.

Other criteria that we wanted to use was with the data that will be obtained from this particular facility or in this particular program how prototipic of the full-size reactor will it be and what problems will we have in extrapolating or relating that particular work to the actual use?

The second has to do with whether we have user's needs for it or specific requests from ECRS and other groups that provide input and suggestions as to what our program should be.

And the third one, and this was more difficult to apply, but we tried to say does this have a direct relationship to risk reduction because in principal if any of -- anything that meets these criteria should have that, but some are going to be much more directly related to t hat. And so that was a separate item that we added to our discussion.

MR. OKRENT: Excuse me. In our discussions with other groups, in fact, in discussion with NRR about which of the unresolved safety issues and generic items they should work on, the relation to the potential for risk reduction is generally the most important thing. Why is it not the most important thing in your safety research program?

MR. JOHNSTON: Because it -- it -- often -- as we see it, it feeds through one of these others. And we found it a little difficult to say how this would be an independent input to this. And yet there were some members of our branch as we did this that felt it should be in there explicitly. And it served as a lever to take a program which for everyother purpose might have some merit, but had special direct -- for example, a program which has to do with pellet cladding interaction, or a program which has to do with stored energy is -- it's difficult to say that that has a direct relation to risk reduction. It gets added through the operating limits that the reactors are allowed to have and this sort of thing, but it is not a direct relation. It therefore gets no points. On the other hand if it's directly related to fission gas release under, say, TMI type conditions, if it has to do with the bi-pass of the containment of radioactivity, if it has direct steam explosion would be one which has a direct relation because it has something to do with the failure mode of containment. I guess the -- I guess the basic criteria was if this has something to do with a mode of causing the containment to fail following an accident, we felt that it had a direct relation. If it had only an indirect relation to whether fission products get out of the containment, it wouldn't get that -- those extra points.

That's the only way I can answer your question. We tried to distinguish between different aspects of the program in that manner.

MR. OKRENT: I would suggest that there's been a deficiency in your program, in fact, you have not tried to factor in the relationship to risk reduction and why -- you've been giving priority two in the past. I think, in fact, the program reflects it. And your program is not alone. There are a lot of others that -- in the NRC that are like that.

And, for example, you look at "A", Information to establish or assess licensing criteria." Well, there may be some in fact where there is a considerable risk reduction potential, and others where there is very little and yet that could be treated the same because there is a criteria in -- and you say it's the law and we have to meet it, or whatever, but there could be a very different waiting that you gave a series of attention to that lower line.

CHAIRMAN SHEWMON: That's your perception. Tom wants to comment --

MR. MURLEY: Mr. Chairman, I have to make a point here if I could.

I think it's correct that a large part of our program is not directly relatable to risk reduction of our research program. And there's a reason for that. The reason as I see it is that the agency does not license on the basis of risk analysis. It licenses on the basis of technical judgment. And that technical judgment sometimes is based on perception of risk or analysis of risk but in most of the cases it not. The whole ECCS hearing and the LOCA ECCS program we find out, if you believe the numbers in WASH-1400 have very little basis in risk.

Nevertheless, it was a major impact on our -- in fact it shaped our research program 5 and 7 years ago. And we're now finishing that up, and we are changing directions. And I think in the future you will start to see more of our programs are, in fact, going to be based on our perception of risk. But they haven't been in the past. And I don't make any apologies for that. I think it's quite understandable.

And as a matter of fact in the future if we get a request from the licensing staff that -- to do some research because it's needed for their licensing decision making, we will do it.

And what -- even if it doesn't have any, I think -- a basis of risk.

CHAIRMAN SHEWMON: Thank you.

How much more time do you have here?

MR. JOHNSTON: This is the last slide.

CHAIRMAN SHEWMON: Okay.

MR. JOHNSTON: That's the result of our re-prioritization of the program. Our number one priority is to try to look at the -- understand the core damage beyond the LOCA. Following that is the clad ballooning and blockage, fission product release and migration. These are the operational transients that are covered generally in the -- and defined as the clad function in three and not in the ANS catagorization.

This was is last for particular reasons. And I -- I'm sure that r~ ses some red flags in the room, but I think you will near more about that on May 9th.

T' re are -- we have separated fission product release out from the specific core -- the heart of things and that's partly why the change in location of that particular level.

CHAIRMAN SHEWMON: Now, if we placed our your telegraphic style a little bit, the results would be the priority items that you will aim at in the next several years as a result of your reevaluation; is that --

MR. JOHNSTON: Yes. We took all of our programs and essentially developed the rating system based upon those other criteria.

CHAIRMAN SHEWMON: Okay. What is the core damage --

MR. JOHNSTON: One through -- one through thirty-five.

CHAIRMAN SHEWMON: What is the core damage beyond LOCA mean? It -- you have it separate from cool melts.

MR. JOHNSTON: What we tried to do at this point is take the -- roughly cover the understanding of the temperature range from roughly 1200 centigrades to 2700 centigrade, or 1900 centigrade up to the point at which -- well, I guess the best way -- okay, this is the point up to which geometry begins to change.

CHAIRMAN SHEWMON: -- occurs, the temperature is lower in that. There is a change in geometry, but it's not a change in fuel geometry, is that --

MR. JOHNSTON: This occurs -- Not a change in fuel geometry, not a change in location in the modeling sense.

This is in there particularly because the audit curves and calculations and licensing, as concerned about right now.

CHAIRMAN SHEWMON: I'm just trying to find out to what extent the words are mutually exclusive or the items are.

MR. JOHNSTON: Well, we intended this to cover the temperature range, roughly from the point at which severe oxidation begins and goes on up to utectic formation and possible melting of either the -- between the fuel and the clouding or the fuel itself, and the degradation of the core, let's say, to when it begins to fall through a core plate at the bottom.

Now, that helps with mitigation. It helps with understanding of the course of the accident, and presumably it learns something about debris coolability, coolability of the core if it doesn't procede to a full score core melt.

This is more focused on the, I would say, on the mitigation site of an accident sequence than understanding the full core melt thing. That was separated into the other categories.

That's the conclusion of the first part and the next part now is the discussion of the specific programs in the budgets. I believe you changed mode of operation.

CHAIRMAN SHEWMON: We will close the meeting at this point. How do we handle this from here? Is there anybody who does not work for the NRC?

(Whereupon, at 9:30, the meeting went into a closed session and commenced again at 10:35 a.m.)

MR. MARINO: My name is George Marino from the Fuel Behavior Research Branch.

The purpose of the discussions I'll give you today are to give you a brief review of our fuel code programs and our fuel behavior programs.

I'll start with the fuel code development and the evaluation programs and then procede on the agenda into our fuel pellet behavior program.

The objectives of the fuel code development and evaluation are to predict transient and study fuel behavior under normal, off normal, and accident conditions.

Now, we do this to help licensing, hopefully, to evaluate vendors codes, and help them in their general understanding of fuel behavior. We do this also to help us do our pretest predictions and post test predictions for our PBS program and --- program.

And we also do this to provide an integrated easily accessible storage bank of fuel behavior information. and you'll see it comes out in the form of correlation equations.

The first principle model is derived from past, present, and future experimental work.

Now, I consider this second item as fairly important because we do an awful lot of work that comes out in the form of reports that are scattered all over the place, and if we can put it in some item where we can get ahold of it quickly, like the MATPRO handbook, it's very useful to us in research and I think, to the people in licensing.

Now, that was why we're doing the work. I'll try to answer the question of how we're doing the work.

We're doing it by the development of a -- something called MATPRO, which stands for material properties. It gives -- It's a compendium or a handbook of zercoloid and fuel, materials properties and correlations for the fuel and the clad, obviously.

Now, this thing is useful for both our operational codes, the first of which is FRAPCON, which is a study safe code. It contains models to simulate fuel behavior under normal conditions, which primarily help us to have an understanding of the fuel characteristics before a transient, which is very important.

It's also important in the licensing area and in PCI.

The next operational code is the transient code, FRAP-T. This contains -- to simulate -- to show behavior.

CHAIRMAN SHEWMON: Why is it important in PCI?

MR. MARINO: Because the -- interaction is usually for small, 50 percent or hundred percent power changes where we can use the study state code.

It has --

CHAIRMAN SHEWMON: It's also a 3d problem, and when I asked the question a year or two ago, you said that was so difficult, you weren't sure your codes could do it in the foreseeable future.

MR. MARINO: That's still true. We are connecting FRAPCON to what's called an AXI-SIM subcode, which we'll do a two dimensional stress analysis on it, if need be.

The transient code is for past transients. In the past we've been concentrating on loca analyses. They go over a period from 0 to 200 seconds. The code is geared for those kind of transients, and it is not that useful, if useful at all to small break transients that occur over a long period of time, and I'll get into that discussion a little bit later.

We also -- part of how we do this sort of thing, is try to provide links with thermal hydraulics codes.

For example, the track code, the cober code and other codes -- Yes, sir?

CHAIRMAN SHEWMON: Let me sto and ask a general question here. Work of this sort has been going on for the order of 10 years, although I realize this hasn't been in the NRC all 10 years.

Are there any criteria for when it's going to stop or when --

MR. JOHNSTON: You mean the code work in general?

CHAIRMAN SHEWMON: Yeah. I notice you still have it as your highest priority items in those areas and when can --

MR. MARINO: Can I answer that, Bill?

MR. JOHNSON: Yeah, go ahead, and maybe I'll add a comment if you don't say what I want to hear.

MR. MARINO: I truly don't believe that code development or -- I mean, not say development, but code improvement will ever cease as far as we are interested in licensing nuclear reactors.

And, we have to keep our knowledge, essentially the state of the knowledge of the vendors and people who we're trying to license.

And, if they get way ahead of us, --

CHAIRMAN SHEWMON: I suspect you're doing an awful lot more than they are and the question is whether they should end up using your codes to justify for you to evaluate and you know, that gets kind of inbred.

MR. MARINO: That's a serious problems. I think Ralph Meyer might have something to say about that.

But, I think if we're going to license people, we should have at least as much knowledge as the people we're licensing and I just can't get away from that feeling.

CHAIRMAN SHEWMON: That's fine. But, if we do that, and take that criteria, you've probably cut this by an order of magnitude next year because I suspect you already have more knowledge and code modeling than they do, and you could give them a while to catch up, so I don't think that's going to be a criteria to help you.

MR. JOHNSON: Well, we set -- Originally we set the criteria as when to stop developing models as when our ability to describe what was going on was equal to what the experimental input uncertainty existed.

In other words, reactor power is good to about -- I think it's about 4 or 5 percent. In fact, by the time you put a couple sigmas on it, I think it's 9 percent, for example, that is actually used.

When we can predict the parameters that are effected by that to the order of 9 percent, we should quit, because there's no point in developing a code that's better than the data base that you've got.

And, we try to use that as a general criteria as to when we stop developing and when we stop improving and developing models for this code and whether we put different kind of inputs in it.

And this why, -- I think George will show later -- that we're trying to put sigma uncertainties into the various predictions. Now, it has been a basis for quitting and the other point is that we feel in a large number of areas we've essentially reached that area and I think George is going to tell you that we're not embarking in large new code developments. This is mostly a maintenance situation that we think we're in now.

He also said the right words, that we've got to keep up to date with what's going on, but we do not feel we've got major new codes -- major new things to do with the FRAP-T and the FRAPCON anymore.

MR. MARINO: Thank you.

CHAIRMAN SHEWMON: Let me say one other thing, George.

If you can do some things to shorten this in spite of our questions, I'd appreciate it because the agenda, as it's laid out, is longer than we're going to be here, and we aren't up to it.

So, we're going to have to pick up some time.

MR. MARINO: You've seen some of these already.

I'm just providing it for background, just so you'll have them in your handout.

Like I showed you this last year, this is just a schematic of the interaction of the codes.

And, I gave you very extensive descriptions of both the codes last year, and all this viewgraph does is summarize the models in the codes. And, I don't see any need, unless there are some questions, to go into the details on these. I didn't intend to either.

What I want to do is get to the results. Now, the first one you saw was FRAP-T. These are the models in FRAPCON and they're included in your handout just for completeness.

And then there are three on MATPRO, and you've seen -- You've seen many presentations on all the models on MATPRO. These three slides just summarize all the subroutines in MATPRO.

The important thing to note is that if it has a footnote A on it, it's a revised improved model from what you had seen before and it's got a superscript B, it's that brand new model.

So, I'll just flip through these quickly, see if I can make up some time. That's the cladding properties. And, this is the continuation of the cladding properties. The first one was the fuel properties and the gas material properties and supporting materials.

This is all self-explanatory. We do have a program to assist these codes. We talked about this before, and we divided it into two areas, developmental verification for assessment. And like Bill had said earlier, we changed our words from verification to assessment. -- and, independent assessment.

Now, the developmental assessment is just what it says. It's supposed to be able to -- The people who developed the codes, test the code out and makes sure it does what it's supposed to do. They do it against highly characterized data.

Now, they're supposed to catch all errors there and they don't always. We always have problems with this and in fact anybody who develops large computer codes have these problems.

Alot of errors get through here. Some of them get picked up on independent assessment where we compare the code against a large amount of data, not very nearly as well characterized as the data we use in developmental assessment and this gives us an idea of how the code behaves under a wide variety of conditions and I'll show you some results of that.

These people catch errors alot and we feed them back to us. They're corrected in the next version of the code.

We did have some problems, that when an error did occur, an independent verification, it wasn't corrected in the code that was current at that point, and we're taking steps with Tim Howell and EG&G to correct that sort of situation.

And, the related tests providing assessment information are, as I said earlier, the pre and post test predictions for our major experimental programs.

Now, where are the results of some of these things? Well, the latest -- You haven't seen this one yet, so I want to talk about it a bit.

This is the FRAP-T5, standard model errors in the independent assessment.

CHAIRMAN SHEWMON: You may not see it today.

MR.MARINO: I hope you can read it on your pass out. Is it in there? I apologize for the slides. I just got this in a few days ago.

What it does is compare for different kinds of output parameters, sample size it gives a standard error between the predicted -- prediction of the FRAP code, FRAP-T code, this is the transient code, and the major value.

And, at the top we have FRAP-T5 which is our latest version in the code and FRAP-T4, which is what I presented to you last year.

And the thing to note here is the improvement of FRAP-T5 over FRAP-T4, is in the prediction of cladding burst temperature at nonpressure and cladding burst pressure at nontemperature and cladding permanent hoop strain over that of what was available in FRAP-T4.

We are concentrating alot of effort in modeling properly the ballooning behavior of a zercoloid fuel rod under positive internal pressure during a loca or a small break transient.

CHAIRMAN SHEWMON: How do I get impressed by reading what you are pointing at?

MR. MARINO: Last year FRAP-T4 could only predict a cladding burst temperature at nonpressure using the cladding models in MATPRO, which is a deterministic model, not a probabalistic model, so we went 290 degrees kelvin.

FRAP-T5 can do it within 160 degrees kelvin, just from improvements we've made in the cladding behavior models.

CHAIRMAN SHEWMON: And what's Frail?

MR. MARINO: Frail is a probabalistic failure subcode. It's linked to FRAP-T5, or FRAPCON which attempts to predict failure probabilities based on stress to failure at given temperatures, et cetera, over stress, over strain kind of probabalistic ana vses.

And, that information is in Frail, purely imperical, not deterministic.

And Frail actually does a better job because it is fit to a curve for predicting the cladding burst temperature of known pressure.

But Frail will give us nothing in the area of the strain along the whole access of the rod, -- strain.

CHAIRMAN SHEWMON: The standard deviation from Frail, standard error is 94 degrees kelvin?

MR. MARINO: Yes.

CHAIRMAN SHEWMON: And the uncertainty in the clad pressure is 23 mega pascals?

MR. MARINO: Let me explain that.

CHAIRMAN SHEWMON: That's a pretty big pressure, isn't it?

MR. MARINO: These are tests that were done at constant temperature at about 675 degrees farenheit, where you have very high burst pressures.

The cladding burst temperature at known pressure were ramping tests where they put in a fuel gas and ramp the temperature til it bursts. And, these would have burst at something like 2 or 3 mega pascals.

So, yes, sir, I should have pointed this out. These very high pressures are for very low temperature burst tests. Okay.

CHAIRMAN SHEWMON: Thank you for --

MR. OKRENT: Before you run, --

MR. MARINO: Yes, sir.

MR. OKRENT: In a sense, this slide introduces a kind of philosophic question. It seems to me there was good reason for the NRC Staff to somehow develop some sophistication with regard to fuel element behavior.

And, in that sense, I guess I would support some kind of trap kind of program, if that was the way to do it.

So, that seemed to me to make sense. The extent to which one tries to carry this forward as an entity in itself and to do experiments to verify the code or assess the code, or use whatever word you want, it seems to me at that point one has to sit back and ask himself why do I need to do this, what is the reason, where will I be when I'm all done, and so forth, and that's the point at which I myself have questions about the PBF program, both the experimental program, and to some extent the way in which the analytical program has been run and so forth.

And, I would appreciate at some point today, I don't care when, hearing some basis for saying why something of this sort needs to be done.

Do you understand what I'm getting at?

MR. MARINO: I intend to do that.

MR. OKRENT: This are two different things to me, and I don't, in my own mind, automatically say well, we should do everything one can analytically and/or experimentally because there is a need to have some sophistication.

These are two different kinds of things.

MR. MARINO: I intend to answer in the last slide where we intend to go with this development. I also would like to point out. I don't think the PBF program was designed solely to verify or assess the codes.

I think it was designed to give us information on fuel behavior under extremely abnormal conditions and not -- as an adjunct, we can use it to verify the code, that's certainly true, and that's what's being done.

I showed you this slide last year. This is the same kind of comparison for the FRAPCON code, and it gives again, the standard errors on this side, in the sample sides for various output parameters on the left side.

MR. OKRENT: Excuse me. Again, -- See, deviations are given, but I don't know what meaning to attach to these, because you can do a fitting of a set of experiments and get a seemingly good fit.

I can remember back in the middle '50's when we had a very good fit to a series of fast critical experiments with our existing methods and, of course, when we ran a critical experiment, that was substantially different.

CHAIRMAN SHEWMON: That was your mistake.

MR. OKRENT: -- substantially different. We had to change our cross sections. That's equivalent to changing a -- or something.

You know --

MR. MARINO: Well, that's why we try -- That's why this independent assessment has so darn many data points in it. You know, we try to take all the range we could find.

MR. OKRENT: There aren't enough data points in the area of fuel. This is a harder problem than matching the critical mass of a fast reactor. It's about two orders of magnitude harder, I would say.

MR. MARINO: I agree with you, yes.

MR. OKRENT: So, I -- you know, wonder whether it's meaningful to talk about these standard deviations and so forth.

MR. MARINO: Well, I think it is. Because, we've also done some studies where we've perturbed the input. We perturbed the operational input, the materials properties input and what uncertainties they had, and ran an uncertainty response, surface methodology analysis on these codes.

And, it gives errors just in the uncertainty and the input on the same order of magnitude as our standard deviation we're seeing when we compare against data.

And what it's telling us, is that we're not going to get much better than 150 to 200 degrees kelvin predictability on a rod that's in some core somewhere, when we don't know everything exactly.

And, that's a good stopping point to follow and we start getting to the point that we know that we cannot, given the uncertainties in a rod in a core, predict -- If we can predict within the range that -- surface analysis will give us, then our code is good enough.

MR. JOHNSTON: Could I just add one comment on to that. The whole point of doing a diverse program, covering all matters of things is to provide a great -- on our part, to provide a diversity of input for this assessment.

There's not tuning done to the code when we do the independent assessment. That should be made very clear to this committee. That is not a tuned code that you're looking at there, when an independent assessment is done.

It's entirely different data that is used to develop the code and that's been a fundamental point of our program from the very beginning, that there be a different set of data, obtained as broadly as possible, from that that's used to generate, produce the code in the first place.

We've been very careful about that, at least try to be.

MR. MARINO: So, it would be very difficult to tune at the 700 data points in any case, so we don't do that. At least that's our intent.

I wanted to discuss a little bit now about the expected fuel code accomplishments for '80 and '81. We've just completed, as I showed you, the assessment of FRAP-T5.

We planned to complete and complete the assessment of FRAPCON II which will be the last version of the code. We're doing model updating as a result of assessment, and new data will continue after this code is finished on a maintenance basis.

A new version of the code, -- I'd like to say something like FRAPCON II-11, will not be on a yearly basis from now on, it will be made only when we have enough new information to warrant putting out a new version of the code.

We plan to complete and assess FRAP-T6, which is again going to be the last version of the transient code, under the same conditions that I put up here for the FRAPCON 2 code being the last version of the code.

And MATPRO-11, revision 1, was also completed this year. Revision 2 will be out in fiscal '81.

And that is simply updating again the models. And, we are getting some new information in for some new models on cladding creak down, which haven't gone in yet, will go in to revision 2.

But this is phasing down in cost and importance because there's just not much more to do there.

These two codes will be on a maintenance basis.

MR. BEMENT: May I make a point?

CHAIRMAN SHEWMON: Yes, sir?

MR. BEMENT: I'm not clear yet that I've heard a clear statement of what your criteria for code reliability is because it hasn't been made clear the distinction between systematic and random uncertainties and how you next these two to get an overall statement of code reliability through your verification program.

MR. MARINO: We do not -- We hope we see systematic errors when we do our major assessment by plotting things like residual error versus say burn-out.

And, we look for those systematic errors, but we don't --- If we see them, we figure it's in the model and we go back and look at our model, with separate effects, to straighten it our.

MR. BEMENT: I was going back to Dr. Okrent's statement, that the standard deviation or the three signal limits only tell you something about the random uncertainty. It doesn't really tell you whether you understand anything more about nature through the systematic uncertainty.

And, I think, to get an overall quotient or criterian for code reliability, you have to have some way of determining convergence on both uncertainties and I let the matter pass, but it hasn't been clearly stated yet.

MR. MARINO: Well, if we see a systematic error, I think that that's what you're getting at, we will attempt to find out which model is causing that, but it will not show up easily, I agree with you, on a plot of standard error, for a large data code comparison. You will not see that easily unless you do a very fine analysis within that assessment.

CHAIRMAN SHEWMON: Let me change the subject of that last code. Can you tell me -- If we look at FRAP-T6, or FRAP-T5 as you see fit, -- But, what I'd like to do is to get some feeling for what kinds of accidents this is applicable to and to do that, for example, does it get into clad melting?

MR. MARINO: No, it does not.

CHAIRMAN SHEWMON: Does it get into fuel melting?

MR. MARINO: No, sir.

CHAIRMAN SHEWMON: Does it get into change in fuel pellet geometry as a result of gas release?

MR. MARINO: It goes into fuel pellet geometry, as far as fuel relocation and cracking and splitting of the boundaries, yes.

CHAIRMAN SHEWMON: Okay. So, the transients -- The T stands for transient, doesn't it?

MR. MARINO: Yes, fast transients, let me make that more clear.

CHAIRMAN SHEWMON: Okay. But it still doesn't get into a transient such as fizz gas addresses itself to?

MR.MARINO: No, fizz gas -- I'm not familiar too much with fizz gas.

CHAIRMAN SHEWMON: Well, I don't know. What's your version of fizz gas? We were talking about it --

MR. MARINO: That's a gas release code, fast reactor.

MR. JOHNSTON: That's the fast reactor thing that -- looked at it and reported to us last week.

MR. MARINO: Fiz gas is a fast reactor, fission gas release --

CHAIRMAN SHEWMON: You have a transient fission gas release modeling?

MR. MARINO: Yes, this is for PCM type transients, power cooling mismatch.

CHAIRMAN SHEWMON: Okay. But is there a change in the geometry of the fuel pellet in that program?

MR. MARINO: It cracks only. It expands out, gets thermal cracks.

MR. OKRENT: It's just a gut conductance change they look for, but other than that they --

CHAIRMAN SHEWMON: So this is a very mild kind of accident then, one that in no way changes the --

MR. MARINO: As far as the state of the fuel is concerned, yes. The cladding, we do have the deformation of the cladding and the clad ballooning.

CHAIRMAN SHEWMON: Fine, okay.

MR. MARINO: I think we hit on this earlier this morning too. Let's make that clear.

CHAIRMAN SHEWMON: Some students you have to tell three times. I've still got one coming.

MR. MARINO: Okay. The major improvements we expect with --

MR. OKRENT: Excuse me, Paul. You raised the point earlier about work going on under kelver.

And, this relates to the question you had just gotten into. The -- people for 10 years or 15 years or 20 years, depending on when you want to start counting, have been trying to look at the kinds of areas we've just been talking about and they have done it experimentally and they've obviously been trying to develop codes and so forth.

And, if this group is going to try to get into that area, I hope that in some way they build as much as they can on this very considerable body and the first thing that they do is try to see how hard it is.

MR. MARINO: We have looked into that, Dr. Okrent. That's a good point.

We have had --

CHAIRMAN SHEWMON: There you in essence get into core disassembly and how does it disassemble, and I think that was the thrust.

MR. OKRENT: No, no. Even -- Just behavior of fuel rods --

CHAIRMAN SHEWMON: As they change geometry, -- the fuel, not the clad?

MR. OKRENT: The fuel -- The fuel, indeed.

MR. MARINO: We've looked into the SIMI 2 code. We've had some presentations in our office from the people at Lasso who are developing that, and it's a very complicated code, very long running.

MR. OKRENT: Well, there's a SASS series at Argonne and other people have done similar things that deal with the areas Dr. Shewmon is referring to.

MR. MARINO: This is large scale fuel motion that you're talking about.

CHAIRMAN SHEWMON: Okay, go ahead.

I'm almost with you.

MR. MARINO: Okay. The FRAP-T6 will contain a link with Fastgrass which I'll talk about in my next talk here, which is a faster version of the grass code, from A&L.

It's going to have a new ballooning model, based on MRBT results, multi-rod burst test results, which Dr. Picklesimer will talk about right after me.

It'll finally have complete dynamic storage allocation which we hope will make the programs more affordable and easier for other users to use.

This is one of our main concerns with this code, is it's getting so complex that people have difficulty running it and I've been pushing for a year and a half with my people out at EG&G to get this thing more easy to use and they are putting alot of effort in that area right now.

It's going to have an updated failure subcode, prel 6, which I said was an over stress, over strain failure model, which will be compatible with this more deterministic balloon 2 model that we're putting in.

It will have an improved user input and output, a circumferential varying heat transfer coefficient model. Right now we can't model circumferentially varying heat transfer coefficients.

We want that capability. This may help us also in our clad ballooning modeling, and it should have many many other smaller improvements which would bore you if I went into them all.

Completion date for this thing is January 26, 1981.

GAPCON 2 improvements over GAPCON 1, is it will also link with the Fastgrass code. It will also have complete dynamic storage allocation.

It will have the pelet mechanical package from

GAPCON 3 as an option to compare against the FRACASS model from EG&G, and I'll tell you more about that on the next slide.

It will have an improved Inel Mechanical Package. It improved relocation models for both mechanical packages. It will also have as an option to use the A&S 5.4 gas release option.

It will have NRR approved PN model options, so that they can use the code and put in and change the models they want to change and get some analysis out of it and also many others.

This completion date is August 15th, 1980. And, as I said before, MATPRO-11, revision 2, is going to obtain the BCL, Battelle Columbus Laboratories -- properties, work done for Dr. Picklesimer.

The two-stress, two-strain University of Florida data by Mr. Hartley, Dr. Hartley there, revised clad creep and thermal expansion models from the inpile creep data at the -- reactor, which Dr. Picklesimer will talk about later and it'll have an updated hot pressing model from Purdue University, which he's just completing this year.

Completion of this one will be in mid-1981.

And my final viewgraph of this code development is concerned with work plan for fiscal '81 and beyond. Now, here's where we'd like some input, I think, from the

ACRS on this, especially item A.

We want to begin development of a small break, slow transient fuel rod damage code, based on and linkable to what we already have, FRAP-T and FRAPCON.

And the question is to how far to take this. We don't want, at the moment, to take it to large scale fuel melting in motion.

We want it so that it's fairly fast running because these transients are over a long period of time. They're not 200 seconds, they may be two hours. So, we have to change the code so that it can efficiently analyze this transient over that period of time and we can do that.

We have the TMI boil code which Dr. Johnston mentioned at about last year which we can use as a start for this thing, as well as FRAP-T and FRAPCON.

We initially will do it for a single rod, take it right up to the point of clad melting and be able to calculate all the oxidation heat that occurred and all the hydrogen release at that point.

When clad melting occurs and we form the cladding oxide utectic which runs down in the annulus between the fuel and the clad and reacts with the fuel, and we get this candling effect that Hogen in Germany saw.

And then we're going to have some fuel motion to worry about and some new kinds of models to put in there as well as worrying about blockage of the channels.

Now, we intend to coordinate this work with the German work, the Melson code work at Stutgart and what they tend to do with their counter part of our FRAP-T, ES-EST and see where we can put this in here.

Now, we're just in the planning stages. Tomorrow I'm going to talk to the people from EG&G some more about this and think of a single rod code that's fast running initially and maybe have to expand it because of the concern about blockage to a bundle-type code.

But in any event, we want to keep it as simple. as possible and no where near as complex as FRAP-T.

CHAIRMAN SHEWMON: I -- Let me make one comment on this, and others can too. But, it seems to me that by the time you get into that sort of an accident, your primary consideration has to be coolability which is going to get you into geometry changes faster than your number of countries are going to want to get there.

And, I think one of the main points in this area that I'll bring up on the 9th is something that Harold Etherington suggested to me a few months ago, and that was:

Do you know how the fuel comes off of a melting fuel rod. That is, if it comes off sort of like wax drips off a candle, that ends up to one kind of a geometry down in the bottom, relative to other sorts of things.

So, I would guess that, at least my push would be more for things that are likely to be experimental than what you're going to do by incremental steps here where I think you're going to have a fair amount of effort as you suggest and still not be able to do anything that would answer the questions of geometry changing thus the coolability of that fuel.

MR. MARINO: We definitely have to have some experimental programs to tell us what's happening and how dependent the collapsing or the loss of integrity of the rods are on the scenario of the accident.

As Dr. Okrent pointed out today, there's many kinds of small transients can occur and if our final bed of rubble depends on how we got there, then a code like this is going to have to be very very complicated because it will be past dependent.

If we can show from experiment that no matter how you get this cladding up to that point and to interact with the fuel, that the rubble at the bottom that you use for coolability is the same, then we can take this code up to the point of incipient clad melting, the interaction with the fuel and then take the next step is -- We've got a rubble bed, characterized by an experiment.

CHAIRMAN SHEWMON: I don't care how the cladding breaks.

MR. MARINO: Well, it's going to determine the rubble bed you have and the coolability of the core.

CHAIRMAN SHEWMON: I'm not at all sure it is. And if you end up having the clad melt off and your column still stands there, then what comes next?

MR. MARINO: Well, it'll come down. In the small break transient, it will probably hit some water at the bottom of the core, freeze --

CHAIRMAN SHEWMON: Come down?

MR. MARINO: Yeah, in Hogan's experiments, gravity pulled it down the rod and it burst out at the --

CHAIRMAN SHEWMON: In a molten state?

MR. MARINO: In a molten state, yes.

CHAIRMAN SHEWMON: Well, we're getting too detailed. Are there other comments on this before we --

DR. OKRENT: I would like to know in a more general way what the purpose of A, item A is and what are the small breaks that you think you are going to deal with and what are the transients that you think you have to deal with and how this relates then to what code development you think is worth doing.

If there is not a single small break like there was a large loca, --

MR. MARINO: I'm thinking in terms of generic accident in which the cladding will boil -- excuse me, --

coolant will boil up at some slow rate.

Now, many times small transients will cause that. I have not delineated all those transients.

But, like in Three Mile Island, where they had the loss of coolant and they throttled the high pressure injection system --

This code's going to have to be fed information on the water level in the core.

DR. OKRENT: But, you said a generic accident like --

MR. MARINO: What's generic about it is that th water just boils down.

DR. OKRENT: But, my understanding of Three Mile Island was that the water just didn't boil down, that it went up and down in various ways.

MR. MARINO: Right -- That's right.

DR. OKRENT: And, this --

CHAIRMAN SHEWMON: That comes in the second year.

DR. OKRENT: Yes, that has unfortunately, a considerable impact on the fuel behavior itself, as you know, in fact, partly even from PBF experiments, when those more generally --

And so, it seems to me in the absence of some serious thinking and definition of what one is trying to do at the beginning seems to me --

CHAIRMAN SHEWMON: I would suggest they would end up taking a good risk assessment approach and therefore the most probable bad accident, as I recall, involves the loss of cooling ability or ability to put water in the core completely and we assume the operator will do it right if he has the ability, so indeed they may well start back with a small break in power failure and it just boils quietly down and melts.

DR. OKRENT: Well, now, that in fact, would be a well-defined scenario sort of, although when you end up it varies from plant to plant, et cetera.

If you really think that that's what you want to know, then you should say also why. If it's going to go on, item for item, as it were, through melting and so forth, then if this is not -- This is only an intermediate stage, you're not very interested --

So, again, even within that context, one wants to say, what is it one wants to know and why. I'm not saying one shouldn't do such work but the problem is semi-infinit if not greater.

And, I think at the beginning one ought to try to have an idea of what it is you're trying to do and why.

MR. MARINO: Let me make that a little more explicit then. Dr. Picklesimer will be showing you slides this morning, I hope, of severe fuel damage on slow heating rates in the KFK experiments.

And, that gives us some idea of how the fuel breaks down at very high temperatures around 2,000 degrees centigrade.

We want this code to be able to atleast model that, so that we know the kind of state the core will be in if we get a transient that results in slow heating to about 2,000 degrees C.

And, that's what happened, we think, at Three Mile Island, and I think it's important that we are able to analyze the situation as far as core coolability.

We all know that Three Mile Island Unit II was coolable, but nobody knew at the time how coolable it might be. When we assessed the damage, we thought the course off. And, I think we should be looking into it.

DR. OKRENT: I don't believe that after 10 years of work on small break fuel rod damage codes, given another accident in which you don't have all the details in real time, but only days, weeks, and months later, that you'll be able to predict whether things -- coolable or so forth, I'm rather pessimistic about --

CHAIRMAN SHFWMON: You have now heard our comments. You have taken up your allotted time. Let's not assume that the committee's illiterate, so let's say we can read the last two points.

Is there anything that isn't written there that you'd like to say?

MR. MARINO: No.

CHAIRMAN SHEWMON: So, we're ready to go on to the next item, is that right?

MR. MARINO: Yes, sir, that's the fuel pellet programs.

And, the objective of the fuel pellet and fuel rod properties research is first, to provide information on changes to fuel pellets during steady state and transient operation, to improve our models for calculating gap conductance in the fuel rod, and to determine the extent to which fuel pellets effect the transient actual gas flow -- transient actual flow of the gas within the fuel rod.

We apply these results to improving our MATPRO models and also our code models. We're hoping that if we get a large burn up in some of these programs, which I'll tell you about in the -- reactor, that they might shed some more light on the burn up influence on fission gas release, and we're hoping that all of these things will reduce our uncertainties and our stored energies calculations in Appendix K.

The first series of tests I'd like to discuss briefly are the --- tests that are being done via our contractor, EG&G. There are two instrumented fuel assemblies, that's what IFA stands for, 4.29 and 4.30. 4.29 was primarily set up to study the absorption of helium for a pressurized rod under long term study conditions, and also to study gas release under small transients of 50 to 100 percent power changes.

-- 4.30 has just gone in last year. It's an end reactor measurement of transient, actual gas flow and center line temperature, as a function of gas size power and gas flow rates.

And, this is done by putting -- boards of gas connected to the rods so that we can change the gas composition and put pressure differentials across it to measure the rate of flow of gas through the rod after various kinds of burn ups in powers in the transient.

CHAIRMAN SHEWMON: Is helium what most vendors use the pressurize their fuel?

MR. MARINO: Yes.

DR. OKRENT: What does an I.F.A 4.29 or an I.F.A. 4.30 experiment cost in total?

MR. MARINO: Okay. The instrumented fuel assemblies were build when I got on the job. And, all I know is what it's costing us now for data reports and something -- For each assembly, it's something like $40 to $50K a year.

I think Bill Johnston might have an idea of the cost of the assembly.

MR. MARINO: These are accomplishments up to date: 329 and 430. Would you like me to read them, Paul, or do you think I should move it through fairly fast?

CHAIRMAN SHEWMON: You might highlight. You might highlight.

MR. MARINO: Okay. The hearing on helium absorption in the highlight is that there was very insignificant amounts of helium absorption. And so we don't really have to be concerned with that.

They really don't -- They are up to 24,000 megas a day per ton burnout, and they really haven't done enough transient gas release work on it yet. We are waiting for more information this year.

Even 430 began irradiation 11-26-78, and it has already given us some good result on siting relocation during start-up period. We had originally thought that we would completely close the whole gap at the first power ramp, and they are finding that they don't close at all. About 20 percent is still left, or the cracks in the fuel are big enough so that they get fairly good actual transient gas flow.

They have also -- And these are used in verifying the codes because the separator affects things -- have

DR. OKRENT: Round numbers, is it $1 million, $3 million?

DR. JOHNSTON: It depends on the experiment, but it's around $250,000 to $400,000. 4.30 was very expensive and I think it was $385,000. In other words, in the order of $150,000 to $200,000.

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changed the gas mixture up to ten percent zenon, and found that Frap 10 predicted about a 20 percent lower gas induction than was actually observed. Now, this tells us that our gas mixture correlations in the fuel clad gap in Frap T and Frappon may be in error at pressures above one mega pasquel in the gap and concentrations of zenon up to about ten percent. And we'll have to take another look at that.

MR. OKRENT: Excuse me. On IFA 430, might that result not depend on its relevant design and operating conditions and so forth? I mean if you had fuel rod where there was a lot of creep down so that you lost the bulk of your gap --

MR. MARINO: Just from the creep down alone, yes.

MR. OKRENT: And you might get a different result, or if you had a bigger gap initially, you could have it go the other way. I would think that --

MR. MARINO: That's quite the case. The larger the gap, the more relocation you have to start with, yes.

MR. OKRENT: I'm just wondering whether the result you got is applicable generically or --

MR. MARINO: It is for the initial start up.

There is very little creep down at that point and its initial relocation --

MR. OKRENT: Yeah. But is that all you're interested in -- where -- with regard to gas flow?

MR. MARINO: No, it's going to continue. This is going to continue under radiation, and there will be creep down. We'll be studying it as a function of burn-up, yes.

MR. OKRENT: All right. Let me leave it at that.

MR. MARINO: These are the instrumented fuel assemblies we designed to study the fuel rod properties in the steady state condition. It's a matrix of gap size, fueld gas composition and power. And they are just designated by even numbers, and there's a whole part of the matrix to study the stored energy. You've seen this before.

And this is ether 513 which is the same ether 431 which was originally put in to have well characterized fuel rods to use later in PBF tests. We haven't really decided what they'll be used for, but they will be used for transient tests, and so they will be characterized.

Now, these -- this slide shows the accomplishments to dates for either 431, 432 and 513. Remember these were for stored energy calculations and gap conductants. And they found so far no high burnup enhanced fission gas release, but of course, they're not up to where we would expect it yet. It's only 24,500.

No adverse effects noted in two rods that contained densifying fuel. When this test was originally conceived they put in two rods with unstable fuel, and they didn't see any long term advsere effects.

The development of a new model for fuel location --

MR. OKRENT: Excuse me. What does the term adverse mean?

MR. MARINO: It means the rods did not operate at higher than normal operating temperatures of companion rods that had non-densifying fuel. That they did not get more stored energy in them at the same power.

MR. OKRENT: So they are measuring central fuel temperature?

MR. MARINO: Measuring central fuel temperature. Yes, sir. The development of a new model for fuel relocation and effective fuel conductivity and cracked fuel elastic-modulized that P&L is putting into FRAPON 2. We've seen some very preliminary results of this, and they've done a very nice job, and they're lowering

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our experimental uncertainties when we get fuel cladding lockup; when we get a large amount of stress imparted from the fuel even though it's cracked to the cladding.

And this is important in our later analysis of belt-padding interaction. They also found, of course, what you'd expect from this that the fuel conductivity is reduced when it's cracked by 20 percent, and the moduli of the fuel to about 1/40 of solid UO2.

They also have found that except for a very, very small initial gap rod that all these rods after startup and running -- after 10,000 megla a day per ton -- reach essentially the same center line temperature regardless of the initial gap unless it's very small.

And the fuel gap and everything else --that they get very close to a constant number. Yes, sir?

CHAIRMAN SHEWMON: Going back to one, as I recall there's been some disagreement at least between Adrian and Ralph that I think of as to how much of this is burnt up dependent and how much is temperature. And will this tend to settle that, or do you have --

MR. MARINO: We will have detailed temperature histories of all of this stuff very well characterized.

CHAIRMAN SHEWMON: You feel that it will answer that question, Ralph, or do you look into it?

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MR. MEYER: I think it will take tests like this. I'm not sure that this one alone will do it.

MR. MARINO: Yes. We also have E 429 which can stay in longer, too.

CHAIRMAN SHEWMON: Go ahead.

MR. JOHNSTON: No one test answers any specific question as you are aware because it is stocastic thing. But nobody ever said that there was an enhanced burnout below about 30,000 so that the fact that you haven't seen it yet doesn't tell you anything.

CHAIRMAN SHEWMON: The Von-Vogel research man though is to a critical experiment, and I just hope that you were getting enough discussion to make this at least as critical as one could.

MR. JOHNSTON: There are many other fuel elements in reactors right now that are going to 50,000 burnup right along with these. And then all of these are contributing to that information.

This does one thing specifically. It has a special shutter on it so that we can change the power level. We can double the power level. That's been going on now for three years.

That's ether 429, Bill.

MR. JOHNSTON: That's all right. Are you

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talking 30?

MR. MARINO: 31, 32.

MR. JOHNSTON: Oh, I beg your pardon anyway. Well, that's part of the answer, though, to the question anyway is that we have a power -- a way in there of changing power. Up to the levels that it's had so far doubling the power does not give any large increases in fission gas.

CHAIRMAN SHEWMON: Good. Okay.

MR. JOHNSTON: That is measured directly in file.

MR. MARINO: Our last program is the ANS gas release, transient gas release studies at Argonne National Laboratories. First of all, just to update you on the Grass SST development, the final version of Grass Mot 6 has been completed and submitted to the Argonne code center with a driver so that people can now use this code independently of the fuel codes.

Grass SST has undergone verification against the involved radiations, some of the PBF and some of Zimmerman's work on very high burnoff gas rates. And also the BEH transient tests which I'll talk about is part of this thing which was completed in September of 1979.

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Now, this slide shows how Grass is done against the DEH test to date. And it looks pretty good -- it's got major gas release versus predicted gas release with PCN type transients. These things range from 10 degrees K. per second to 500 K. per second transient time.

You'll note the two points that seem to show a underprediction of the gas release. And the reason for this is that when you get above about 25 or 30 percent gas release, the microstructure of the fuel shows very fine microcracks throughout the fuel. This is unconstrained fuel.

And the gas code has no models in there to account for cracking at the grain boundaries due to their reduced strength because of the high concentration of freezing gas bubbles on the grain surfaces.

We are going to try to improve the Grass model. Jeff Rest is working on this for us, hopefully to pull these points on to the line. We think we're about as good as we're going to get down in this region here.

Now, the next item we did was since we tried to connect these codes to our fuel codes, the grass code was so long running that it made connecting them prohibitive in computing time so as Jeff -- Jeff Rest looked at developing a faster version of the code which is not a fast marijuana -- fast Grass -- and he has done that successfully and developed something that's based on less numbers of bubble classification sizes in his very fine model for gas release. And it's 10^{100} times faster in execution than Grass SST.

It's been verified against Grass SST and the data, and the next viewgraph shows you that. And you'll see that even though this code is considerably simplified over the very, very detailed code, this does just as well down here as the long-running code, but a little more poorly up here where we only had bubble size classes of two -- two size classes allowed in fast Grass. However, it is much faster.

Modeling activities planned for the remainder of the fiscal year is to complete Mod 2 of fast Grass which will have only one size class for the bubble, and we expect it to be much, much faster.

I'm still not satisfied with having a code even as fast-running as fast-grass to be our gas release model for best estimates in our Frapon code. I asked him if he can develop a set of algarithms, and a parametric equation, so to speak, by using Grass SST under many, many conditions, and getting a set of algarithms that we

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could put into a fuel good and call it Para-grass which would really enhance the speed. So we're working on that area.

Grass SST calculations will continually to be performed to analyze LWR transients, and ANL will continue to assist EG&G in applying these codes.

Now, the experimental program, as I told you earlier was completed, and they're writing a draft report now. It's coming out May 2 on the analysis of all the DEH tests

The major results of this experimental program are empirical transient gas release correlation was developed for his particular tests, and you should use it with caution. I'll show it to you. Microcracking of the fuel was shown to be very important in gas release rates above 30 percent.

That's a very important part of this thing. The data was used in the verification of the Grass code. And constrained color had significantly less gas release than unconstrained colors, and I'll show you this in the next slide. And this is also an important characteristic of this program.

The program is completed though. A lot of analysis has to be done yet, and Jeff Rest will keep putting all this information into his analysis of Grass.

MR. OKRENT: Would you mind defining the term microcracking as used on that slide?

MR. MARINO: Microcracking -- my understanding of it is the separation of grain boundaries. It's a very fine scale. It's along the grain boundaries. And if you look at the structure, that's what you see. It's not across the grain. It's not trans-granular.

CHAIRMAN SHEWMON: The bubbles all assemble on grain boundaries, and pretty soon it doesn't know whether it's a bubble or a grain boundary, and it breaks. I mean if there's enough pressure in there, you do a stress analysis. It opens up.

MR. OKRENT: I just want to understand the context there. Okay.

MR. MARINO: And if you have constraint on the pellet, it inhibits the grain separation even under your thermal stresses. And you see that -- this is where Steve Gell has plotted all his DEH gas release data. And he attempted to get an empirical correlation based on the maximum temperature gradient in the fuel and the heating rate in the fuel starting from PCM -- starting from the normal condition in a rod and giving it a PCM type transient.

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And he found that for his unconstrained tests -- these are the white circles -- you're going to have to look at your handout. This is not a very good viewgraph. That he can get a correlation pretty good. But when he did his constrained tests which are the dark circles where he put a boron nitrite sheath around it to constrain the fuel from expanding, he got much -- considerable less gas release.

And he saw considerably less microcracking of the fuel as well. I should say that.

MR. OKRENT: Now, there are theories that have been developed at Argonne -- there's a paper by Detrick and Demelfie, for example, and some others where they try to predict when you get microcracking as you've used it, and presumably the theory should indicate the importance of whether the fuel is constrained or unconstrained because this is analyzed -- has that been done, and have they gotten some kind of analytical understanding of the empirical behavior that you're reporting.

MR. MARINO: That has not been done, but it's being planned to be done in Fiscal '81 as far as this program. It has not been done because these tests were just completed a few months ago that really showed the effect there. That's a good point, yes.

And so there's the correlation. I think you should use it with caution. It has to be PCM type transient. I would sooner use the Grass code to make the calculation. Thank you very much. That completes -- yes, sir?

MR. BEMENT: On the constraint test data, if you were to replot them on the previous slide where you have the two points that fell off the curve, does this now draw it into the curve?

MR. MARINO: Yes, it would. I didn't replot them myself, but it would bring them in closer, yes.

MR. BEMENT: In other words, the extension of the low burnout data out to the higher results would closely correlate against constraining fuel.

MR. MARINO: Right. And the Grass code does not have a model for microcracking and gas release due to that. It says the gas atoms accumulate on the grain boundaries. The bubbles form on the grain boundaries, but only at the grain edges when you build up a sufficient concentration of bubbles on the grain edges can you then get the venting of the gas out to the fuel. Any other questions?

MR. OKRENT: I'll make an observation. I have a student who's trying to do this problem for transients

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with a model for cracking in it. I don't know whether it will be successful, but we'll try to, I guess, parametrically put in some kind of constraint effect to see whether it comes out.

MR. MARINO: Yeah. Maybe I can have Jeff Rest contact him since they'll both be working in that area they could correlate some of their work. Anything else?

CHAIRMAN SHEWMON: Okay. Thank you.

MR. MARINO: Thank you.

MR. PICKLESIMEN: I have responsibility for the cladding research programs in the fuel behavior branch. Since there was so much interest a little earlier in what liquified fuel looks like I'd like to take some slides out from this afternoon's presentation and show them to you first.

This is work that was done by Hagan and KFK where he has an eighth rod bundle, and I don't know whether you can see the lower part of this or not. You're looking -- these numbers represent these fuel rods, and you're looking down on the bundle so that in this picture you're looking at this way at Rod 25 -- right in there.

25 is in the middle. 31 is the one that goes up along the slant. 17 is the one that goes up here. Now, this one was heated at two degrees heat per second

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to a temperature of the center 125.

CHAIRMAN SHEWMON: How was heated, and how was it cooled?

MR. PICKLESIMEN: The outer rods -- eight of them or seven of them have tungsten core heaters, EO2 repellants and they're heated and steamed. There is a lumina zirconia blanket, insulating blanket around the outside. Otherwise, it couldn't get up to 2000 C.

The center rod has solid EO2 pellet on it. It is heated only by radiation. This one went up at two degrees heat per second to 2000 degrees C. on the center rod. It was cooled by simply turning the power off leaving the steam off. There was no fast cool down.

Now, you can see the condition under cladding. If you look at this rod here, you're looking at the sign. I'll rotate that 90 degrees, and that is this rod right here. You can see the tungsten wire core and the EO2 pellets.

Now, this other one here -- 32 -- you can see. There -- that's the only one. Okay. Now, there's a good bit of what I call liquified fuel dribbled down in that bundle. Now, the cladding is colder on the outside, and the center, towards the center, will get hotter faster. So that is where your first liquified fuel will

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form. That's where you first detect it. And the zirconiom oxide will form, go in against the fuel, dissolve some EO2 and then find some opening somewhere down the clad where it will come out.

That's what we call candling or liquifying fuel. Now, you notice the shattering that there is here. And this was just on standard steam cooling with the power turned off. If this has been hit with water, I'm sure it would have been much, much finer.

Now, this is a companion bundle that was heated, at I believe, 1/2 degree C. per second. It's either a half or a quarter, and I'm not sure which one it is. I think it's the half. In steam again, the oxide formed on the cladding is much thicker. There is much less zirconium present to form the u-tective, and it forms much less of the liquified fuel.

This bundle broke right in here, and this is what you're looking at here on the higher magnification shot. This is liquified fuel. It has dribbled down to fill up the subchannels.

MR. BEMENT: Do you have any metallography?

MR. PICKLESIMEN: They have metallography on it. I have no reports of that metallography except verbally. I will see this in June in a week-and-a-half

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visit at Carlsrobe. I will be talking to these people. They have a considerable greater amount of work that I'll be able to get my hands on.

And I'll be getting that work from them then. Now, rather large -- I'm sorry.

MR. OKRENT: Now, what is that we should have gathered from the pictures we just saw?

MR. PICKLESIMEN: I'm sorry. I didn't understand your first words.

MR. OKRENT: What should I have learned from the slides you've just shown me?

MR. PICKLESIMEN: You were asking the question earlier of did we have any idea of what the melting fuel looked like when it was coming down the line. That's what it showed you.

MR. OKRENT: In this experiment?

MR. PICKLESIMEN: In this experiment. That's right. They have other experiments that are a different heating rates, different steam conditions -- a wide variety. I have some of the data. I don't have all of it. I'll get the rest of it in June.

We have a fairly large handout of which I included a number of pages for your information. I don't intend to cover them. I'm only going to hit the high-

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lights of this. So we will have to flip a number of pages in the handout. Now, the first program I want to talk about is the multirod first test at Oak Ridge which has turned out a good bit of data. It's being used in a number of studies and so on in licensing and throughout the world.

The objective is to characterize ballooning burst and loss of flow area in bundles. A second objective is to determine the scaling factors going from small bundles to large bundles. How large a bundle must we test to get something that is prototypical of a large bundle?

We were required to do this work initially as a command essentially of the Commission in 1973 to better characterize the ballooning and flow blockage in bundles. A requirement of 10 CFR 50 -- I think it's in Appendix K -- states that the extent of flow blockage shall not be underestimated.

The present embrittlement criteria in 10 CFR 50.46 require better estimates of the rod ballooning and the rupture sizes, rupture strengths to insure that they don't exceed the 17 percent equivalent clad thickness converted to oxide limit.

Now, there have been pre-bundles -- overboards

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have been completed. The data have been determined. There have been flow tests conducted on them, and the data of that is essentially in hand.

There will be an eight bundle burst, we hope, about June 1. The bundle has been constructed. It's not being inserted in the facility, and we hope by mid-July to have a fair bit of information on the 8 by 8 bundle.

We also have constructed a new single rod test facility which is turning out some very important results which uses a heated shroud that is lamped with the specimen in a duel-data track system so that the average over the shroud is within one or two degrees C. of the average temperature on the right.

Now, there are temperature gradients everywhere in this, and this average has to be taken with -- somewhat with a grain of salt. But there are no large temperature differences between the rod and the shroud.

CHAIRMAN SHEWMON: The subassemblies down there or clusters are three feet long -- is that right?

MR. PICKLESIMEN: Yes. The heated link is three feet. The total assembly is six feet. And you have to drop extensions, get your thermal couples out and pressures and so on. The heated length is three feet.

Now, this upper section of this viewgraph shows

3/20 you a typical cross section of a first bundle. Now, all we have done is after the bursting the bundles, these have been flow tested. Then they have been mounted in epoxy, and then they have been cut, and most of these cases, they're a cut of one centimeter increments over the full length of the three-foot heated length.

This shows you what one regions of one bundle which has the maximum number of bursts and the maximum loss of flow area within that. Now, the loss of flow area is defined by the area occupied by the newly expanded cladding at that cross section.

Now, when you plot that for each of the sections along that bundle, then in bundle B-3 which is the last one, it went up at 10 degree C. per second bursting in the neighborhood of 830 degree C., we wind up with loss of flow area now here as much as 80 percent -- 75 percent by a one definition -- 90 percent of one particular point by another definition which I won't go into unless you particularly want to. The average loss of flow area in this bundle is in the neighborhood of 60 percent for the maximum.

Now, when we take this data and plug it into Cobra 4, we then can come up with and predict the pressure drop measurements that was made on that bundle.

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Here are -- the points here are the actual pressure drop measurements. The line is the calculation using the loss of fluid area data. Now, the most significant finding of the recent work in that is they took two rods -- they took two rods from Bundle B-3, the last one that was heated, made single rod specimens out of them, put them in heated shroud, and ran them under the same conditions that the bundle was ran.

Then they have done a strain profile -- section strain profile on both sets of rods with the same heater. These two specimens, one in the bundle and one in the single rod test had the same heater. Now, they are not identical because the rods have to be removed from the bundle specimens. They have to be straightened. They have to be recoated with zirconium oxide spray coating. So they are not quite identical to what they were before.

But if you look at the area under the curves here, and you look at the string padding, you can say that the single rod tests duplicated the behavior in the bundle. Now, the second one here shows a greater deviation in the first points, but again, we've got the same kind of behavior.

And we're convinced that these two specimens, two single rod tests say that the single rod test with the heated shroud is duplicating bundle behavior.

There are two more specimens being run next week or this from a second bundle, and we will find out how well that matches. If this is correct, and our 8 by 8 bundle which will run the first of June shows the same results as our 4 by 4, now we have a scaling factor. Now, we have a test method using single rods to approximate bundle behavior.

All right. The next program I'd like to talk about is one that has been called in the past Mechanical Properties of Zircoloy. It is now being called code verification. Why -- I don't quite know, but it has been. Phase one has been concerned with a study of the embrittlement behavior of zircoloy being oxidized in steam. Again, as a requirement by the Commission in 1973 that we establish more quantitative environment criteria, based on material properties, whether it's in the 17 percent oxidation limit and the 2200 F. heat temperature that has been the present criteria.

Now, phase two which is getting underway now. Phase one is completed. The final reports are being published. I have a copy of one of them in hand. The other one I should have in the mail very shortly. But they should be published and distributed within the next

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month so phase one will be completed.

We do have new quantitative embrittlement criteria. We'll be preparing a research information letter to NRR on that this summer . Phase two, which is just getting started, and is looking at the stress rupture properties of spent LWR fuel cladding to try to understand a different mechanism for pellet clad interaction failures and the stress relation cracking.

This will be done by external pressurization of specimens, an internal manual to load them, and a simulation of the real stress geometry that you encounter in the reactor during a power event, then the manual will be ramped to stress the cladding, and it will determine the time to failure. We will do this in high pressure autoclaves at temperatures like 300 to 350 degrees C.

When we have looked at it -- as a stress rupture mechanism without stress corrodents, then stress corrodents will be put inside the specimens and we will begin. This is being done entirely with irradiated frap.

Now, I'd like to get a couple of slides for the embrittlement criteria. One of the problems that you have with embrittlement of zircoloy by steam at temperatures up to circa 2500 F. is that you also have a

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problem with hydrogen on the inside of the specimen away from the rupture. As you oxidize the inside surface of the rupture area, you liberate hydrogen which diffuses down into the gap and is absorbed on the inside of the cladding at the lower -- and a different level.

All right. Here is the fracture mechanics KlD, fracture characterization, dynamic fracture cut up from this value -- for zircoloy hydrogen for temperatures under 600 K. and for zircoloy oxygen for temperatures under 400 K.

Now, the 600 K. is determined by the solution of this amount of hydride in the zircoloy so that it's no longer is embrittling, and as you can see on a atom percent basis, oxygen is considerably more embrittling that is hydrogen. This means that for the most part when we're looking for embrittlement criteria, we want to look primarily at oxygen.

Now, this is something. We have a different way of plotting the data against temperature for KlD of ten mega pascals in per meter square root against oxygen concentration. And these are the way the data points study for an impact test at .75 meters per second.

This is a drop foot test. Yes?

MR. BEMENT: Can I clarify one point? With regard to the hydrogen data, is it not necessary to take into account hydride orientation or reorientation of hydrides, or is the -- it seems to me the concentration was in the range where you could get significant hydride formation?

MR. PICKLESIMEN: Your hydrogen pickup occurs at temperatures like 800 C., 14 or 1500 F. and higher.

MR. BEMENT: So what you're saying is the solubility of that temperature is such that you don't have to worry about it.

MR. PICKELSIMEN: That's right. And you don't have stress cladding when you're cooling back down because you have ruptured.

MR.BEMENT: But if the hydrogen migrates to cooler regions of the cladding where the temperature isn't quite so high, then you could have some hydride formation or some concentration?

MR. PICKELSIMEN: You're talking about stress oriented hydride?

MR. BEMENT: Yes.

MR. PICKELSIMEN: Yes, it would be possible, but I don't think you can go that kind of distance. You're talking about several feet in that case in a rod that is being heated up in a LOCA.

MR. BEMENT: Well, I'm just trying to recall an experience from pressure tubing in can-do reactors and other reactors where you can get hydride or hydrogen migration over rather significant distances.

MR. PICKLESIMEN: Yes.

MR. BEMENT: And especially in fuel rods where it can go into the weld cap.

MR. PICKLESIMEN: We have looked at this hydride distribution and hydrogen distribution in these specimens. Now, all of the specimens that show the oxgen curve also have hydrogen present. And we have characterized this.

It does go down to regions like two inches away from the rupture, but that's the extent. Now, we have used this data to try to assess the embrittlement criteria for the present and the proposed ones, and the point on this slide I want to show you is this data right here.

The present embrittlement criteria for 70 percent equivalent reacted to oxide and 1477 feet, 2200 F., as to heat clad temperature. We're proposing two embrittlement criteria to be used for different circumstances. For thermal shock resistance, we're proposing that there be at least one-tenth millimeter of wall left in the cladding that has no greater or has less than .9 weight percent oxygen in it.

This insures that there is enough ductile material present in the cladding to withstand thermal shock on quenching. And this is a water quench now; not just steam cooling.

The second one is an impact limit based on handling accidents, bundle drops, seismic events, this, that and the other as best we can up with -- what might happen to an embrittled bundle after the accident is over.

Now, you're disassembling. Or you have an earthquake or whatever. Now, if we have 3/10 millimeter of cladding left that contains less than 7/10 weight percent oxygen, it will withstand a significant amount of impact loading. It will withstand a bundle drop accident accident without shutters.

All right. What they did was to take the Fort Calhoun FSAR, and take the two curves -- this is Exxon reactor -- take the two curves, one for the rupture zone which goes up to this temperature and then comes back down. And then one -- the other for a node that is about a foot away which is the peak clad temperature note which is this -- we took those two cases and calculated using -- I can't remember the code at Oak Ridge that was used for oxidation, diffusion, so on.

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We calculated their oxygen distribution, compared to it the empirical data, and they come up with evaluations like this. For a Fort Calhoun here on a double leg, a cold leg break --the performance limits upon this say that -- let me thing -- which way you come out here -- if this number is one or greater, then this accident analysis met the criteria that is given here.

This one met the criteria. This one over -- this one just barely missed. So that we have a condition here where the 17 percent in this particular analysis now because of the large strains that were present in the cladding under this ramp, and he calculated two surface oxidation. Now, the FSAR only calculated one service because the peak clad node was about a foot way from the other node.

Now, Caster calculated for two-sided oxidation, and with two oxidation, this did not meet the 17 percent equivalent clad reactor. One side at oxidation -- this would be 1.5. It would have met it. So we're looking here at thermal shock. In both pieces the new criteria are well met in this accident, and the fuel handling accident -- it's met in one case and not quite in the other.

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CHAIRMAN SHEWMON: Now, Pick, the -- in the LOCA, you would oxidize much of the length of the sub-assembly or the core.

MR. PICKLESIMER: On the outside.

CHAIRMAN SHEWMON: Whereas your two-sided model, would only come in over the order of inches around the crack -- is that right?

MR. PICKLESIMER: That's right. And since this node -- the peak clad temperature node was about a foot away from the rupture, I think it is not cricket to base that number now on two-sided oxidation, and it needs to be based on one. And if it was one-sided, then that is 1.5.

CHAIRMAN SHEWMON: Fine. Okay.

MR. PICKELSIMER: Now, the phase two part of this study is just getting underway, and it is concerned with the stress rupture program where I won't go through the entire list here -- the program is scheduled to start this year. They should be underway in a few months with actual experimental work. They have the cladding in hand. They will be doing most of their work next year, and in with the stress rupture in 1982 -- FY '82, they should be working with stress corrosion.

Now, I'd like to tell you about the overall

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pellet clad interaction program that :e have been planning. There are a number of components of it. We're going to be looking at kinds of failure by stress rupture in a study which is work that Caster is going to do starting now.

They'll look at the effect of stress corrodents on kinds of failure, and that will be the work he will be doing in FY '82. We'll be looking at spring rate ramping on public clad interaction figure out of pile. This is work that will be done by Phil Pankaskie at present planning at Battelle Northwest.

CHAIRMAN SHEWMON: What's spent fuel cladding?

MR. PICKLESIMER: It is cladding that is removed from spent fuel removed from reactors. H.B. Robinson had about 35,000 megawatt days per ton burnup. And the fuel has been removed from it.

CHAIRMAN SHEWMON: But PCI is not storage pit problem. It's the in reactor transient problem.

MR. PICKLESIMER: That's right.

CHAIRMAN SHEWMON: So what you're doing is saying that this is fuel which represents end of life, and that's what the property measurements would be done on.

MR. PICKELSIMER: Essentially that. We're using

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this as a source of irradiated cladding that has had typical LWR operating conditions, and we're looking strictly now at the cladding features. When we get to stress provoked, then we'll be looking at the other.

CHAIRMAN SHEWMON: Well, if you were writing to the public, I would suggest that you leave spent, I guess --

MR. OKRENT: Why is NRC doing this and not DOE or the industry?

MR. PICKLESIMER: Because the major release from operating BWR's is clad gap -- or gap gases released by public interaction failures during normal operation. That's the greatest activity release to the site from BWR.

MR. OKRENT: I'm sorry. You answered a different question. It must be a different question.

MR. PICKLESIMER: It is a safety question in that we have --

MR. JOHNSTON: Operational transients -- not normal operating.

MR. PICKLESIMER: They determine these transients during their yearly runs.

MR. OKRENT: Why is NRC doing the research and not the industry or Department of Energy?

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MR. PICKLESIMER: Ralph, do you want to answer this. Ralph Meyer.

MR. MEYER: I think the answer is largely a matter of motivation. We see that planning interaction is a failure mechanism analagous to the way we use DNB limits in licensing. And there -- we don't create a lot of enthusiasm in the industry for going after new failure mechanism that may cause some penalties in licensing.

And the industry is very much interested in PCI, but they will argue philosophically that they don't think it's a safety concern so they concentrate their effort exclusively on fuel longevity, and we see a definite safety connection with this failure mechanism and since they don't do it, we feel that we have to.

MR. OKRENT: Again, it seems to me there are two different questions. One is are there safety related issues that arise out of a fuel element failure, and I guess -- I'm not trying to argue that issue.

Certainly one can make a case that this affects dose to workers and so forth. I was asking if the NRC thinks this is a safety question, why nevertheless it's the one that should be doing this particular kind of work which is a rather detailed and specific kind of property measurement thing.

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MR. MEYER: Would you like me to continue to answer these questions?

CHAIRMAN SHEWMON: Why don't you? You're the user in this case.

MR. MEYER: There are only a couple of mechanisms that fail fuel that are related to operating conditions, and one of them we regulate religiously, and that's the portion we boil them. I see the cladding interaction is a nearly complete analogy of that in terms of fuel damage, and there are two reasons for being concerned about those, and included in the safety analysis.

One has to do with the general design criteria that have us insure that during the condition one and two events, the fuel operates according to specified acceptable design limits, and DNVR is one of those design limits, and we think there should be one for PCI.

The other reason is because when you get into the lower probability events, the transients and accidents, where fission products are released you need to make an estimate of the fission product releases. To do that, you need to make an estimate of how many fuel rods of any of the gases.

And if you overlook one of the major mechanisms of failure, then you overlook a source of fission products from a release.

CHAIRMAN SHEWMON: So you see this as a way to help you define how you can set better design limits for the plants instead of -- so you can rely on a more common failure mode that DNB?

MR. MEYER: It doesn't replace in DNB.

CHAIRMAN SHEWMON: I didn't say it did. I just said it's a more common failure mode than DNB?

MR. MEYER: That's correct. We haven't failed many fuel rods by DNB commercial reactors, but this one, we know, works.

CHAIRMAN SHEWMON: This one. What's this one?

MR. MEYER: PCI.

CHAIRMAN SHEWMON: PCI does fail.

MR. MEYER: We know it's a failure mechanism that operates.

CHAIRMAN SHEWMON: Yeah.

MR. MEYER: And the kind of conditions that can be experienced.

MR. OKRENT: The question being answered is really a different question. My question is who should do the kind of research --

CHAIRMAN SHEWMON: Their answer is when it comes to setting criteria, not whether or not they -- how they

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want to phrase the design basis they'll make people react to that the they think the NRC should.

MR. OKRENT: But you can set criteria in a general way, and then the industry has to develop operating modes or whatever.

CHAIRMAN SHEWMON: That's right. You can do that with regard to vintage containments or anything we do research on.

MR. OKRENT: That's true, and you want to have enough knowledge about the situation to know it is you're doing, but I think there is a question as to whether the NRC -- how detailed they get into looking at cladding behavior and so forth and trying to decide under what operating conditions --

CHAIRMAN SHEWMON: So far in this area they have had no criteria. They did come up with some correlation which may work in can-do reactors, but doesn't work exceedingly well out of it. And so there's been virtually out in this area to set criteria on.

MR. PICKLESIMER: The industry has been conducting some research on whiteness of PCI by copper coating and zirconium coding under the underside surface and so on. But they are not that interested in the mechanisms.

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CHAIRMAN SHEWMON: Well, Dave's point is that we shouldn't be that interested in the mechanism either, but we should be interested perhaps in having enough information to set licensing criteria. I'm not sure the two are the same as I understand them.

Why don't you go on?

MR. JOHNSTON: I just want to point out the assemblies to additional time in the reactor to try to reach these 50,000 and 60,000 type burn-ups in which we're dealing with a system now in which the cladding and the fuel are under considerable contact. And we're not sure just what that means by way of failure under rather mild operating transients and variations in power level. That affects the total kinds of releases.

Basically, it provides a basis for what happens under transients.

The other point I wanted to make, the industry has had a particular approach and point of view to the PCI mechanisms and have taken a particular stance that's expressed in the packing that's involved. We've stayed out of that thing for the most part for a number of years. However, the stress rupture point which is something which can be understood only in terms of working with irradiated cladding is a different -- an alternate, if you like -- mechanism of what the failure is. And nobody's looking at it. Industry had the point of view and doesn't particularly want to look at some other points of view. We feel that if we're going to do audit and understand what is being proposed by industry, we've got to have something of our own. Particularly if we think there's an alternate explanation that's not being pursued. We feel we have some responsibility as long as it isn't wild-eyed expenditure of money to check out that other possibility. That's what one aspect of this program is that they've described. It's checking out an alternative idea.

It's not being done by anybody else and yet I think will affect, if it's correct, the basis for some of the criteria.

CHAIRMAN SHEWMON: Thank you, let's go on.

MR. PICKLESIMEN: Very quickly the PCI I discussed a little bit earlier by Kassner to be done in the next three years. The strain rate ramping to failure to be done by Northwestern Planning, it's partly to obtain data for evaluating the project model and get certain material parameters to go into the profit model to see if we can improve it.

The other is that it is a way of ramping the radiated cladding out of pile to somewhat similar to what you would have in pile but without having it go public. It's a much cheaper test.

MR. OKRENT: How much money is going into the kinds of experiments we've just been talking about?

MR. PICKLESIMEN: The money this year -- '81, I'm a little more confident of. At Argon is $350,000. The money for Vatel Northwestern is $100,000.

MR. OKRENT: Okay, that doesn't sound like a lot of money compared to your total budget, but I think it's equal to roughly the total amount of money being spent this year on vented filtered containment by the NRC, just to put something in perspective.

MR. PICKLESIMEN: One of our programs which we are joining is a Demo Rap program which is being done in Sweden by Hilbe Mogart. We'll be one of something like seven or eight participants in this. They will ramp in the R2 reactor pre-iradiated fuel rods having moderately high burn-up, like 25,000 megawatt base per tone. They will ramp these on the base power up until they -- to some higher level. Some of them will fail on the ramp, some of them they will fail after holding at the higher power.

Then we will have straight ramping to PCR failure of the PBF optran tests. Now, these are more of the operational transients where we're looking at things like -- let's say a transient with optran, the turbine trip without bypassing the PWR, a number of these kinds of power ramps that take material off of a cladding fairly rapidly over a fair power insertion.

It's not an NRI8, don't misunderstand me. It's much lower than that and to a much lower level. But it does induce stress in the claddings and can lead to PCI failure. This is seen most commonly in the load falling operations that some reactors have undergone.

We have about seven optran tests total planned. Six of these are what we call the 4X, they are individual rods, four at a time in the test train. We will have one bundle. These will be more or less proof tests, if you wish, for the outpower work.

MR. OKRENT: Excuse me, what will you prove with those tests?

MR. PICKLESIMEN: We expect to establish some curves similar to fatigue failure curves. The stress level versus kinds of failure, or kinds of failure against the stress level induced in the cladding.

Once we know what this curve looks like and we know where the cladding will probably come in hard contact on a power ramp, now we can predict given a given power increment increase predict whether that cladding will fail or not.

MR. OKRENT: If you know everything else including what the source of loading on the cladding is and whether there are other effects besides the pure mechanical effect you're talking about, what will these experiments cost? That you've just shown -- these optran?

MR. PICKLESIMEN: The optran?

MR. OKRENT: Yeah.

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MR. PICKLESIMEN: I don't have a number on that.

MR. OKRENT: About?

MR. PICKLESIMEN: Bob, do you have a number on that?

VOICE: $3,000,000.

MR. OKRENT: That's without charging for operating PBF which is carried as a separate category, but if you put that in it would probably double it I suspect.

MR. PICKLESIMEN: It's without the operating expense, yes.

MR. OKRENT: Roughly 38 percent? Good. I made a wild guess, thank you.

MR. PICKLESIMEN: The last thing I'd like to talk about is studies that we are in planning now, we don't have funds for them in hand, we don't the test procedures developed or anything, but we're calling them -- where they will characterize the properties, behavior and formation of what I prefer to call liquified fuel since we have UO 2 dissolved in the zirconium/zirconium oxide, and this will be over a range of temperatures.

Now, our program will start this year if we get our supplemental money, some of it. We will start in '81 according to the present budget. We will determine r..ctionary composition heat formation in the rea..tion products with zircoloyt, UO 2 and steam. The temperatures from about 1,800 K to about 2,500 K.

One of the things we know nothing about are what are the oxidation rates of this liquified fuel, either solid or liquid. That will have to be determined.

We need to determine the information on viscosity of this as a function of composition so that we can characterize the dribbling rate, the candling rate in bundles.

Unless there are questions, that's my presentation.

MR. OKRENT: What would the effect of long-term irradiation be on the kind of things you've just been talking about?

MR. PICKLESIMEN: The insipient --

MR. OKRENT: Well, you talk about candling.

MR. PICKLESIMEN: We would expect to get into that a little later on in the programs where we would be looking now at fission products that would be say atypical of 40,000 - 50,000 megawatts.

MR. OKRENT: Do you think it might have a very major effect so that --

MR. PICKLESIMEN: I don't know. When a

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few atom parsecs can cut melting points of metals and oxides, I just don't know. I haven't looked enough into what would be in the UO 2 to see what would be the effect at these fission products.

MR. OKRENT: I was thinking about fission product gases and how involved the materials --

MR. PICKLESIMEN: I would expect those would burn out as soon as the fuel liquifies or before.

MR. OKRENT: Yes, I think they would be but they might change the geometric configuration markedly. Okay, let it go for now.

CHAIRMAN SHEWMON: Is that a PBF experiment or will it be? Insipient fuel clad melting?

MR. PICKLESIMEN: No, that's an out of pile test entirely. It's all -- at the present timing it's all laboratory scale, bench scale.

CHAIRMAN SHEWMON: All what?

MR. PICKLESIMEN: Bench scale, laboratory bench scale. We will have to get into bundles a little later, but the test makers and the overall present plan just hasn't been firmed up yet.

CHAIRMAN SHEWMON: Okay, where do we dump into the NRU status in this presentation?

MR. LAWROSKI: Not at all today. It's not part of of the discussion.

CHAIRMAN SHEWMON: That's in August, okay

VOICE: -- money DOE is putting into this.

MR. PICKLESIMEN: No, sir, I don't.

MR. LAWROSKI: Shouldn't we?

MR. JOHNSTON: The number is about $15,000,000 this year.

MR. LAWROSKI: Into which? Into the whole fuels and materials area or into cladding alone?

MR. JOHNSTON: Yes, the only areas that they're working in is that extreme high burn-up and the pilot bundle work with the vendors. The two are basically looking at PCI effects as the bundles go through longer and longer times in the reactor. They're measuring parameters, dimensions and this kind of stuff and looking at the failure modes and carrying on into the high burn-up range, and that's basically the DOE program. It's about a $15,000,000 level.

MR. LAWROSKI: Of course, we have no idea what industry is putting in.

MR. JOHNSTON: Industry is putting in almost the same amount.

MR. LAWROSKI: Oh, it is?

MR. JOHNSTON: Well, I guess I can't say that for sure. Most of the DOE programs are cooperative with industry. And they have to put in something or or other.

MR. LAWROSKI: I see.

MR. JOHNSTON: I have those exact numbers --

CHAIRMAN SHEWMON: There are sub-assemblies that are going in with the zirconium liner and zircoy clad..

MR. JOHNSTON: Yes, that's part of the DOE program. There's two programs -- the one that's not going in with the copper liners, they switched and they're using only the zircoy liners for the large-scale demonstrations.

CHAIRMAN SHEWMON: Zirconium, it's not a zircoy..

MR. JOHNSTON: Quad Cities, all right. They developed both and they made the decision finally when they went large scale to stick with the zircoy editions.

VOICE: Pure zirconium.

MR. JOHNSTON: Pure zirconium, I'm sorry.

CHAIRMAN SHEWMON: There is a difference.

That has been largely a problem with the BWR's but presumably could be picked up in the PWR's?

MR. MARINO: It was in -- it's a question of engineering BWR. They had a considerable amount of failed fuel and they were operating on boron. I don't know why it failed, but it did.

CHAIRMAN SHEWMON: Go ahead.

MR. SHERRY: My name is Rick Sherry, I'm the program manager for the core melt and fission product commission transfer programs that relate to the light water reactors. We'll be presenting the fission product release and transfer programs today and then the core melt programs will be presented by the May 9th subcommittee meeting .

The objectives of the fission product commission transfer program are to develop fission products release short terms for zircoy clad fuel rods under accident conditions and under severe fuel damage and core melt. To develop models to predict the tenuation and transport of fission products within the primary system and the containment; and to provide -- for release from the containment for consequence analysis and for determining the environmental qualifications for engineer safety features and to provide the design requirements for mitigation features, such as vent filters or other types of filters.

These are the programs I'm going to be discussing today. The first five programs are programs that are currently on-going or are programs which have just been completed within the last year.

The next two programs are programs we hope to initiate this fiscal year assuming we get supplemental funding; and the last four programs -- 10 percent -- are programs which we are currently evaluating and may or may not start sometime in the future depending on their merit.

CHAIRMAN SHEWMON: Will you stop while I get oriented here. The program here says Trap Code and Related Studies. You've got Fission Product Release and Transport. Are those the same thing?

MR. SHERRY: Yeah, the Trap Code is the Fission Product Transport Code. A subset of our fission product release.

CHAIRMAN SHEWMON: Developed by whom?

MR. SHERRY: It's being developed by Patel Columbus at the present time.

CHAIRMAN SHEWMON: Okay, thank you.

MR. SHERRY: I want to point out that there are a number of slides in your handout which I will not be presenting to save time.

CHAIRMAN SHEWMON: I hope you also have some upside down and other right side up like Mr. Picklesimen so that you add variety.

MR. SHERRY: Well, I think mine are all probably right side up, I hope.

Starting out with the first one here, this is the trap program. The attempt of this program is to develop a mechanistic first principles computer code to allow the fission product transport within the primary system and container.

The current status is that the primary system model is -- models are essentially complete. We have issued a request for proposals for future code development, and I'll discuss this later.

The accomplishments over the past several years basically the results are these: first of all, the program -- let me go back one second. This program was initially started to evaluate the assumption in the reactor safety study that basically there was no credit given to deposition of fission products within the primary system under core melt accidents. We had initially thought that this assumption was very conservative and we wanted to evaluate it. Results of this program indicate that that assumption was not that bad. The tenuation of fission products within the primary system is not large. It's not on order of magnitude, it may be a factor 2.

However, the program also indicates -- the code indicates -- that the growth of aerosols within the primary system during transport from the core to the containment is important. The aerosols grow from a size of approximately a tenth of a micron to 10 to 20 microns which will affect the subsequent behavior of the aerosols in containment.

The R 2 we have issued basically includes these elements. We want to, of course, improve the Trap Code models. We want to extend the Trap Code to model the containment fission product behavior. We want to put in better models for the fission product release source term from the fuel. And we want to conduct sensitivity analysis and then define verification tests for this code.

MR. OKRENT: Excuse me, is it clear that you need a verification test facility?

MR. SHERRY: No, it's not. We'll be discussing that a little later.

This program at Sandia Laboratory is basically a program to provide some basic data for the Trap Code. We're looking for data on fission product, vapor pressures, compound vapor pressures, and what chemical interactions these fission products may have in the gas stage either with themselves or with the steam or hydrogen.

This program also -- containment pressure experiments are being conducted at Sandia and at New Mexico Tech. We're using a tranporation apparatus at Sandia that meets in a fusion cell at New Mexico

Tech.

We're construcing a small facility to investigate the interactions of fission products in a high temperature, steam environment.

The fission product compounds we've started to test are iodine and CC, primarily the -- hydroxide and --. We plan to go into investigating other fission products including --

This is another small scale experimental series to provide data for the Trap Code. Basically, this program which is being conducted at Patel Columbus Laboratories is directed toward obtaining data on the deposition rates for fission products on high temperature surfaces.

During the past year we constructed a small scale experimental apparatus to do these experiments. We are aging primary system -- samples of primary system components or materials to simulate the reactor environment or their exposure to reactor environment. And we have just begun to do add on vapor deposition experiments. And the remainder of the physical year '80, we plan to do -- vapor deposition experiments. And this program will be completed in '80.

CHAIRMAN SHEWMON: What kind of experiment is this? Will this include the sedium and the other vapor deposition. Go ahead.

MR. SHERRY: What type of experiments?

MR. LAWROSKI: Yeah.

MR. SHERRY: I have a schematic diagram in the -- following I think that slide in the handout, which shows the apparatus.

MR. LAWROSKI: Would you put that slide back up here again?

MR. SHERRY: The last slide?

MR. LAWROSKI: Yeah.

CHAIRMAN SHEWMON: It can be corrolated with what happens in plants?.

MR. SHERRY: These programs, this program and the last program, are geared toward providing data for the Trap Code which models sufficient power transport under core melt accidents. These are providing some of the basic data to develop the models.

This particular set of experiments is geared toward providing the data on the rates at which the fission products will deposit on the primary system surfaces from the steam as they're being transported by the steam. Is that clear?

MR. LAWROSKI: The form of the sesium will be what?

MR. SHERRY: The form will probably be sesium hydroxide. There also will probably be some sesium iodide. Anything else?

CHAIRMAN SHEWMON: What's that under on the budget handout, Bill? What's that slide under on the budget handout?

MR. JOHNSTON: If it's under Fission Product Release and Transport, third category of priority

CHAIRMAN SHEWMON: Okay, but there's nothing at BCL on that list.

MR. JOHNSTON: Well, that program ends in fiscal '80 therefore it's over this year and we didn't even put it on.

MR. SHERRY: As I said, I'm just indicating the results for programs that are to be incomplete for this year..

This is another program that will be ending this year. It was just a one year program. At the request of licensing we have initiated a program to develop models and to investigate iodine transfer and transport under steam generator to rupture accident conditions. This was based on a -- the reason for doing the study and our requesting it was they did a study on this phenomena or this accident and their study indicated there was a potential transport mechanism not considered. That transfer mechanism was

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the effect of atomizing primary coolant during the blow down for the primary system to the steam generator secondary side under the high pressure differential. This could create small droplets which would be capable of being transported along with the iodine they carry through the steam generator and --

CHAIRMAN SHEWMON: What was it atomizing again?

MR. SHERRY: You break a tube. The pressure differential is maybe as high as 1,300 P.S.I. The primary system water is super heated relative to the secondary side conditions and it would rapidly flash and the process is sufficiently violent that this thing could act as a fairly good atomizer.

So basically we've initiated a program to do two things: one, to develop models of iodine transfer within the steam generator and secondary system; and two, to experimentally determine the atomization and to try to clarify it.

The status of this project is that the experimental facility has been designed and is under construction. The iodine transfer models have been developed and are being assembled into a computer code. That's self explanatory.

This program was completed at the end of '79 and the beginning of this fiscal year. It was the Oak

Ridge program to measure fission product release from high temperature fuel within the temperature range of 500 degrees C. to 1,600 degreees C, using commercially irradiated fuel rod segments and it is done in a steam environment.

I just wanted to show you some of the results from this program quickly. I'll concentrate on -- I believe last year I told you that we had taken the fuel rod segments up to 1,200 degrees C. The release of the iodine and sesium was much, much less than the gas release assumption using -- and certainly much less than the terminated term used in licensing calculations.

What I want to just briefly mention now is this temperature regime from 1,200 degrees to 1,600 degrees -- and what these tests indicate that somewhere between 1,300 to 1,400 degrees C. in this regime there is a new mechanism coming into play. And there's a rapid increase in the release of iodine and sesium.

We believe that this mechanism is due to separation of grain boundaries and release of the iodine and sesium was not being released in a similar fashion to the other gases. The krypton release was also measured in these tests. The iodine and sesium tend to follow the release of the krypton.

CHAIRMAN SHEWMON: What percentage of the total fuel inventory is released at 1,600?

MR. SHERRY: This gives the total percentage of the fission products, each specific species. So this is approximately 10 to 15 percent right here.

MR. OKRENT: I'm sorry, are you suggesting that the sesium --

MR. SHERRY: At these temperatures up to here it appears that the mechanism for release of the sesium and iodine may be the same as the nova gases at the very high temperatures. This is what this type of data suggests to me.

CHAIRMAN SHEWMON: And this is in steam?

MR. SHERRY: This is done in steam, yes.

MR. OKRENT: This would say the sesium had moved to the grain boundary and was volatile and stayed volatile just like the xenon and kryton are.

MR. SHERRY: or is moving to the grain boundaries during the test.

CHAIRMAN SHEWMON: Well, it may have accumulated in the same bubbles as the krypton.

MR. OKRENT: Again, that would say it has moved there and stayed there. I thought there wax some sort of a mass migration of sesium in a different way, but let it go for now..

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CHAIRMAN SHEWMON: This inventory gap then is that used in licensing?

MR. SHERRY: This line here is simply the amount of the sesium and iodine that had migrated to the gap during the thermal operations and during the radiation life of these rods.

CHAIRMAN SHEWMON: And that was experimentally determined.

MR. SHERRY: Yes. It represents --

CHAIRMAN SHEWMON: It's an interesting question let me get an answer to the one I'm asking though, will you? That has to do with what is used in licensing. Is it the entire content, or does the gap have any relevance to the licensing rules the way they're written?

MR. SHERRY: I can ask Ralph to answer that.

MR. MEYER: Yeah, it's different for several different accidents. Basically, the assumption is that the gap activity is released and the gap activity is a certain fraction of the total yield. What accident are you thinking of here?

CHAIRMAN SHEWMON: I've learned long ago to ask vague questions when you don't know what you're talking about. I refuse to clarify it any more.

MR. MEYER: There are three different

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perscriptions used in licensing. There's one prescription used for LOCA's, there's one used for reactivity accidents, and there's one used for fuel handling accidents and then that one is implied to a whole lot of different --

CHAIRMAN SHEWMON: Well, the fuel handling presumably is cold, so that would be only the gap inventory.

MR. MEYER: That's correct, but that's the one that's most widely used.

MR. SHERRY: Yes, I think the gap inventory is something like 10 percent, isn't it Ralph? But that's 10 percent of the gases.

MR. MEYER: For some of them it's 10. For the LOCA, for example, you assume -- the effective assumption is that the gap activity is 100 percent and that you release half of that and half of that plates out.

CHAIRMAN SHEWMON: Given these results are any of the regulations you now have conservative relative to them? Or are all of what you have conservative relative to these?

MR. SHERRY: They're very conservative.

MR. MEYER: Yes.

MR. JOHNSTON: I notice there are three or four orders of magnitude.

MR. LAWROSKI: That temperature is temperature of the fuel where?

MR. SHERRY: This is the average temperature of the fuel in cladding.

MR. LAWROSKI: Average?

MR. SHERRY: Yes.

CHAIRMAN SHEWMON: Heated up in steam.

MR. SHERRY: Basically these short segments are cut from rods and are put into an apparatus and they're heated by induction heating.

MR. LAWROSKI: Can you tell me what this average -- what's the total range? When you say you pick a number like 1,300 as a for instance.

MR. SHERRY: What's the difference between the cladding temperature and the fuel temperature?

MR. LAWROSKI: That's an average of what kind of range of temperatures?

MR. SHERRY: By average I meant the temperature of the cladding and the fuel. The heat is being deposited in the cladding and is heating up the fuel rods, but heat-up rate is relatively slow.

MR. LAWROSKI: So it's the average of two large, very different temperatures.

CHAIRMAN SHEWMON: He says it's slow.

MR. JOHNSTON: These are isothermal tests, they're on short segments.

MR. SHERRY: Yeah, over the heated length of the rod the temperature is practically constant.

MR. OKRENT: Two things. First, I don't think you really mean three or four orders of magnitude with regard to the high temperature condition.

MR. SHERRY: We're relative to a a control LOCA okay which is 1,200

MR. JOHNSTON: A controlled LOCA, but there are accident conditions -- I think we want to be careful that we don't use that in a sweeping way

MR. SHERRY: I refer to the LOCA which has been the standard for discussion for the last 10 years.

CHAIRMAN SHEWMON: For the sub-assembly drop.

MR. JOHNSTON: For the sub-assembly drop I think indeed it may be okay. If I look at that figure and go over the highest temperature measurement which looks like 1,600 and something, there's still only on the order of 10 percent of the sesium released according to that.

MR. SHERRY: That's a little deceptive. It's a large scale.

MR. JOHNSTON: I say, roughly. Maybe it's 15 or 20..

MR. SHERRY: Maybe it's 20.

MR. JOHNSTON: Earlier I was asking about whether 1750 which was a cut-off you mentioned was high enough, and I was told I thought well everything of interest is released by 1750 -- sesium and so forth. And if I look through this I find 1750 is really a limit on the experimental equipment which I can understand.

MR. SHERRY: Yes, that's true.

MR. OKRENT: But that's a different answer.

MR. JOHNSTON: I said 2,000 this morning.

MR. OKRENT: I see, I'm sorry. So you have dated it by 2,000.

MR. JOHNSTON: That's the melting point of zirconium.

MR. SHERRY: I have another slide a little further on which shows the results from the -- experiments where they get higher temperature.

MR. OKRENT: Okay, what I'm getting at is that it seemed to me that as I looked at what you were saying that there's some range that you're going to do and there's some range of measurement that's optional. I'm trying to ascertain just what the range is that remains optional.

MR. LAWROSKI: They quit at 1400 before.

MR. OKRENT: Before.

MR. LAWROSKI: Yeah, but that was experimental.

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MR. SHERRY: In this test series we ran four tests about 1200 degrees C. One was a test where we were less than 1400. We had one test at 1600 degrees C. We were only able to maintain the test segment at that temperature for three minutes before the cladding was oxodized and we lost our coupling.

CHAIRMAN SHEWMON: This was in steam.

MR. SHERRY: This was in steam.

This one I'm going to talk about now is basically an extension of that program. To do some additional testing at temperatures of 1200 degrees to 1750. Basically the only difference between the program I've just described and this program will be that the test will be done using an inert atmosphere. We think we can get up to 1750, maybe a little beyond. It's really not possible to use the flowing steam and we'll go up to about 1600 degrees C. and maintain the segment at temperature for any length of time.

MR. LAWROSKI: The three minutes at 1650 a reasonable expectation?

MR. SHERRY: Well, as compared to an accident scenario?

MR. LAWROSKI: Well, compared to what you know about the zirconium. If it all went to oxide that at least changes the scene and that's when they lost their inductive coupling.

CHAIRMAN SHEWMON: I know that.

MR. SHERRY: We're changing metals susceptible to an oxide. We've received a 189 on this and we're basically planning to start this program this fiscal year assuming we get supplemental funding.

MR. LAWROSKI: Where will that be done?

MR. SHERRY: At Oak Ridge in the same facility we had done the past experiments.

Jumping away from fission product release to filter technology, this is another program which we hope to start this fiscal year assuming we get supplemental funds. This was a program requested by licensing, and it's a program to investigate the performance of activated charcoals under radio-iodine retention performance under accident conditions.

If you recall from Three Mile Island, the proponents of the charcoal filter which was pretty horrible, I think the penetration rates were something like 50 percent for the iodines, this is also a continuation effectively of a program which has been funded under our safety division for the past two years to investigate the proposed to charcoals under normal operating conditions. This is the elements of the program.

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MR. LAWROSKI: Now, why isn't that the kind of work that DOE ought to be doing or industry?

MR. SHERRY: Well, I guess we wouldn't do the research if the industry would do it, but this is the information and licensing people feel is needed is not available.

CHAIRMAN SHEWMON: I share with Dr. Okrent many of the questions.

MR. SHERRY: Basically the purpose is to evaluate the acceptable credit that can be given toward the performance of these filters. The charcoal filters are getting a very high rating and credit -- 99.9 percent in iodine retention. If we had a performance after wondering if the performance is anything like the performance at Three Mile Island, there's a substantial margin for error there.

CHAIRMAN SHEWMON: What does weathering mean here?

MR. SHERRY: That's basically exposing the charcoals to a flow of air at high humidity, to contaminants, hydrocarbons, ozone, things you'd expect at a normal air flow through the charcoal base.

I'm going to run quickly through these next four programs which are programs which we have under evaluation now.

We're looking at a program to experimentally determine release of fission products from irradiated light water reactor fuel under melting conditions. This program will basically duplicate the CAFCA SASHA work where they use simulated fuel and activated fission products. Basically the elements of the program are we need to construct a facility similar to the SASHA facility and conduct experiments.

We're currently planning on a start date if needed for these type of tests sometime around fiscal year '82. We want to have an opportunity to look at results coming out of these high temperature tests at Oak Ridge and to get further data from the SASHA tests.

MR. OKRENT: Before you run, if I understand correctly you currently expect to be able to go up to 1750 or a little less with the existent facilities. I'm not urging that you build some new expensive facility to go up to 2000 or 2800 C.

MR. SHERRY: These kinds of tests can be quite expensive.

MR. OKRENT: I realize. Also, I'm not urging that do something that isn't going to begin until FY 83 or FY 85 or FY 87, you know, as things get delayed.

On the other hand, it seems to me that for the kind of decision making that the NRC's going to be involved in with regard to the existing reactors and the kind that's raised by the commissioner's own interest now in what can you do about containing a core melt accident, and this gets into a question of what you buy for different measures and so forth. There could be an interest in what I'll call quasi-accurate -- not accurate results -- or at least knowing whether what's in 1400 is good to a factor of two or so forth on a basis which is before FY 83.

Now, has anybody looked to see whether there is something one can do that's less elegant that might provide a rough corroborative information?

MR. SHERRY: I would say yes, but the Germans are already doing it. The SASHA test program

MR. OKRENT: And you've looked and you don't see anything else that you could do on the short time that would compliment what they're doing?

MR. SHERRY: We haven't even looked if there's any way we could push the temperatures we could obtain in the current Oak Ridge apparatus up to higher temperatures, but it didn't look feasible.

MR. JOHNSTON: Although not specifically designed as part of this thing, two other programs will contribute to it. They're the two IMPOWR programs that we have which will reach those kinds of temperatures, PB 30

and that's the work that's being funded in the SO reactor which will hit those temperature ranges and also the work in PBF that would be done in pile. Those are two in pile tests in which there will be rather fresh fuel, not high burn-up fuel, but the activity will come out and there will be sufficient product detection systems in those two reactors which will give us, so to speak, on-line answers which will not be as quantitative as these might be. But I think they will meet the intent that you asked about. Tha's in addition to the SASHA which you mentioned and he's going to show you something of their out of pile work right now.

MR. OKRENT: Well, that may be so, but my suspicion is that the in pile or the out of pile experiments will only provide meaningful results if they're designed to do it and you look at it critically and review it from that point of view and say yes when I'm all done in fact I will have meaningful results that bear on the decision making processes. Otherwise I agree. You'll make measurements but it's not at all clear that they'll have put you in any position to use them in the way I think they might be needed.

MR. JOHNSTON: I guess I'm puzzled and I guess maybe I ought to ask you a question. Everytime we

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said we define a program to obtain some measurements, I'm having trouble understanding your reception of what we're saying. Because basically -- when we plan something we do plan it to get a particular answer for a particular purpose. I'm confused in the sense that you're not hearing me say that.

When I say we get some data from a program, we define that data. We ask our contractors to get it. It's explicite in their work statement. It's explicite i the work statement as to what we're going to do with it generally speaking, and it isn't just random haphazard data taking that we engage in. That's what's giving me alittle bit of problem.

I did forget one additional one, that is the DEH at Argon which goes right up to melting UO 2 and does look at radiated materials all the way up to the melting points. They do get those kinds of numbers. In fact, in data that was presented earlier you saw 30, 40, 50 percent fission gas release. So it just occurred to me that's an additional piece of data. That work is specific for those kinds of purposes.

MR. OKRENT: Excuse me, fission gas in the normal gas -- xenon, krypton -- is not what you're interested in?

MR. SHERRY: We don't typically look --

MR. JOHNSTON: Not for the stuff other than

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MR. SHERRY: We are --. Now this is a slide showing the results from the German -- program. I think the important thing to notice is the difference between the temperature at which they start getting excellerated releases of iodine -- to what we say in --. This is almost 400 degrees higher.

CHAIRMAN SHEWMON: Is this in steam also?

MR. SHERRY: This is inherent. And I attribute this to the difference in the location of the -- products within the fuel.

So, consequently that's why I agree that additional pressure reading be -- temperatures. Once we get up into the -- fuel becomes liquid. All those differences may --

CHAIRMAN SHEWMON: Are you about done?

MR. SHERRY: Yeah, I've -- let me run through 3 more slides.

This is the Bishop Power Transport Verification Facility that Dr. -- had asked about. We're basically evaluating the need for this facility right now to test our beta product transport codes.

Over the next years we, the NRC and the BFMT and -- will be evaluating the need for this facility. And once -- if we establish a need, we will be developing design requirements. And they will possible beginning construction at the facility for modification of the existing facility such as the --.

MR. OKRENT: You're requesting money in 1982 for that?

MR. SHERRY: We've currently identified some money in the budget. We'll present that.

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MR. OKRENT: You are requesting money for it?

MR. SHERRY: Yes. We haven't discovered to our satisfaction that there is a definite need.

CHAIRMAN SHEWMON: Okay.

MR. OKRENT: That's why it's partly a wish list.

MR. SHERRY: I think it basically depends on the use for which these -- are going to be placed in the future. Whether we sole risk assessment or whether it will be licensing, evaluation, evaluation of mitigation features. That type of thing.

This is a program of -- where we've evaluated basically it's to investigate experimentally the region of -- products from fuel in an environment which would simulate that expected -- and severe accident which is -- reactor.

We think that the preliminary judgement that it's really not a high priority item. We're not planning to fund it. There is some work being done at P & L in this area relative to waste matters.

And it doesn't really look like it -- this is something where you could contain information. You reduce some risks and things like that.

The last item is -- relates to the Three Mile Island data recovery activities. We -- group that's working to develop recommendations for what data should be recovered during the recovery at Three Mile Island.

One of the items under -- we'll look at the types of data -- . Deposition within the containment, -- this type of thing. We're participating in this activity as our number one contract due.

CHAIRMAN SHEWMON: Thank you.

MR. SHERRY: Okay. Well, we have a small amount of money there in case --

MR. MARK: Well, could I ask -- from that -- data you have indications of approximate stuff released by the time the fuel is melted. And what fraction is representative of the decay heat source by the stuff that is evidently left the fuel? Roughly, very rougly?

MR. SHERRY: I guess the gases in the iodine would contribute something like 20 to 25 per cent.

MR. MARK: Well, the cecium is the meter of those.

MR. SHERRY: Right. I guess I can't really give you a -- the answer.

MR. MARK: Well, the answer could be figured out if one sat down with these numbers?

MR. SHERRY: Yes.

MR. MARK: Is there allowance for that when one turns around to discuss melt through?

MR. SHERRY: Yes, there is. When we -- when the penetration time through the reactor vessel and the heat source being recently -- through the decomposition -- that is taken into account.

MR. MARK The stuff is removed from the heat source?

MR. SHERRY: Right.

MR. MARK: Thank you.

MR. SHERRY: I'm not sure how consistently that's applied. It is --

CHAIRMAN SHEWMON: Do you have something to say too before we go on?

MR. PICKLESIMEN: I have a presentation, if you wish.

CHAIRMAN SHEWMON: Well, your compatriot here took up 35 minutes of your 30. What do we have -- tell us a little bit about what you'll tell us.

MR. PICKLESIMEN: What I wanted to discuss is the basic severe and core damage study. It will consist of a number of individual programs.

CHAIRMAN SHEWMON: And this will not be covered in Chicago?

MR. PICKLESIMEN: No.

CHAIRMAN SHEWMON: Okay. Let's get on with it.

MR. PICKLESIMEN: These were the items that were the top priority items on my presentation earlier.

CHAIRMAN SHEWMON: Now, that's under the last -- on the last page here where you've got fuel melt down?

MR. PICKESIMEN: No, no. No, no. It's severe core damage. No, there's __

CHAIRMAN SHEWMON: Well, as far as time. Cut out out about the middle of the presentation -- did only the first 3 or 4 and the last 3 or 4 viewgraphs.

MR. PICKESIMEN: I would like to emphasize that much more is going to be done. Plans are not firm. We have some programs that are in place. We have some programs that are

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fairly well planned out but that are funded. We have programs to be planned.

So, I'm covering a rather broad area here. Not just a few individual programs.

In the types of studies that have to be done, we're going to take a look. Seve:a Core damage. We have one -- the development of core damage, sufficent product dist:ibution resulting from that both in reactors and in the containment. The mottling of severe core damage. Code development for the prediction of core damage. Thermal hydrolics in damage cores. And core melt down and consequences.

Now, the area that we're concerned in fuel -- are these top 4. The thermal hydrolics and damage cores has a provence of another branch in RSR. They have the people that are expert in thermal hydrolics and we are not.

Core melt down is at the present time in the providence of the fast reactor branch. We have some work that's been involved with this but we don't plan to do any extensive work in that area.

CHAIRMAN SHEWMON: Does that actually have anything to do with core melt down or core's after they're moltent and and thrashing around down below?

MR. PICKLESIMEN: What we're seperating at the present time just as a place to seperate their work from ours. ONce the material has dropped out of the full barrel.

CHAIRMAN SHEWMON: So, that last line shouldn't read core melt down. It should read moltent core.

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MR. PICKLESIMEN: It should read moltent core, yes.

CHAIRMAN SHEWMON: Or -- fuel or something.

MR. PICKLESIMEN: And this material that is dropped out of the fuel barrel and is down into the bottom of the primary vessel.

CHAIRMAN SHEWMON: Thank you.

MR. PICKLESIMEN: That's what we're seperating at the present time.

Our thermal hydrolics, there will be some thermal hydrolic data gathered in the study on the development of core energy. But this will be more like pressure drops across the bundle, from end to the other. Things of this type. They will not be -- coeffient type measures.

Now, if you look at the damage that is possible to a fuel rod as you start a heat up. And I'm talking about core now. Assuming that the core were to fall down or is in the process of falling down, as a node on that rod it heats up. The first thing you can have is you can have some -- occurring. Then you can have rupture, then more importantly you can severe oxidation of the pipe. Get to embrittlement, fine oxidation, then total -- oxidation of the --. And now you have a ceramic on a ceramic.

If you're heat up is slow, you will get to this stage rather than --detected formation. If you go fast, this will point. And you'll have oxide on the outside and moltent percodium will react at 1900 degrees Centigrade or that utechnique. Then that utechnique will dissolve, U02 to form what we call liquified

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fuel and if you keep on going up you'll fininaly get to fuel --

Taking those into consideration now, we can do sort of an event free analysis of what kinds of damage will happen on a general area of scenarios. A local rod will heat up at either high rates, medium rates, or low rates. Medium rates I'm saying is someplace in the neighborhood of 2 degrees C per second. Just to have a number to seperate with. We can either have a long time or a short time to--. We can have peak boil temperatures under 1300 C or over 1300 C. And that then allows us to rate the kind of damage that we would expect if we go on any one of these tests.

But you get over to -- finally we would estimate whether there is core geometry lost or whether the core is locked. Some of these will produce locked cores and some won't. In a number of them we'll have a question mark. Whether it will happen or not depends entirely on the scenario you want to pose.

All right. Now we can -- in general seperate the research areas in these in core development into interval affect impile, expile, seperate affects impile and expile, and basic studies impile, expile.

And efficient product release consists distribution I seperated from these as an area where there will be a seperate concentration although most of the information on this efficient product distribution will come out of these studies.

They come out of a different area of data collection. And then of course bottling 3 or 4 differences in the development.

Now, go to the last 5 pages in the handout. Programs that apply to the facts of -- that are needed that have already been completed. We've already had a chance --. The observation of -- by steam to 1500 C has been --. We have a number of studies that do that. WE have a limited amount of at 1800 C.

We're looking at the need to get additional observation data on plutonium at 1800 C. I'm not convinced that it's needed. We'll be looking at this rather hard in the next few months.

Embrittlement of fighting by oxidation has already been completed. The title of reports are almost in hand or in hand.

Spoken study on the -- and liquify of fuel, bundles -- and effective heating rate has already been done in KFK. I'll get more information on that in June when I'm there.

The ZROU -- works about 1500 degrees C has also been done at beta K but it may not be sufficiently material for our needs. We'll have to see.

Now, if our programs that present and planned or in the planning stages are SR with the first test being done in FY 82. There are 32 wide bundles, 6 foot long. Then we will get into, we hope, the revent formation, the liquified fuel formation will come later. That very should not be there. And it does have --

BDF, severe core damage studies, in tone with the other handouts earlier, it would probably be small -- tests. This is what we were referring to, severe core damage. These tests will start at the present time in FY 82. There will be 6 to 8 tests, in 25 to 30 feet wide bundles, 3 feet long. -- formation, liquified fuel formation, boil down -- and --. Now the tests will be varied to give a slow cool down so we can preserve the -- and fast cool down so they can see what happens when they punch it.

And finally in loft, it is being discussed as the final test. But it is being considered as a last test that will be -- to damage study in loft as it's last run. Probably will be posted in 1985 and severity will have to be determined.

MR. OKRENT: Excuse me.

MR. PICKLESIMEN: Yes.

MR. OKRENT: Suppose you had done the experiments you've talked about in TBF on severe core damage, what would you have learned that you now don't know, let's say from other work that's been done on degree bed formation and so forth? I'm trying to see what you think would be the real payoff since this is not a small investment in money, you're talking about.

MR. PICLESIMEN: If we had some of these PDF severe core damage tests already in hand? Is that what you're asking?

MR. OKRENT: Let's assume you've done your 6 to 8 tests.

MR. PICKLESIMEN: Okay.

MR. OKRENT: What do you think will be the real pay off? I agree that you will get data but that's not subject to question.

MR. PICKLESIMEN: What I expect to do is to characterize and to prevent any liquified fuel formation inpile the partial size distribution, if you want. What the compositions are that are present there. 2 people who will have to build beds for thermal hydrolic studies. We will also have a better idea of just what is happening in that disrupting bundle during an accident, like Three Mile Island.

MR. OKRENT: Well, there certainly has been alot of work on debris bed formation in the past so --

MR. PICKLESIMEN: From -- fuel bundles?

MR. OKRENT: From fuel.

MR. PICKLESIMEN: Of these magnitudes that I'm aware of. There's some stuff in the fast breeder program but it's not of this type.

MR. OKRENT: Now, what do you mean of this type? What will you get here as that's unique.

MR. PICKLESIMEN: We have a different size fuel rod, we have different materials and we have different procedure, we have a steam environment rather than the sodium environment.

CHAIRMAN SHEWMON: Have they trickled down fuel subassemblies?

MR. PICKLESIMEN: They have individual fuel --

CHAIRMAN SHEWMON: --

MR. PICKLESIMEN: They have individual fuel rods. I'm not aware that they have bundles. They may have but I'm not aware of it.

MR. OKRENT: Well, let's see. There's a part of the NRC that had to consider debris bed formation in connection with the floating nuclear power plant, for example. And they made some estimates on debris that sizes and so forth and --

CHAIRMAN SHEWMON: They never worry about this stuff until it's down on concrete.

MR. OKRENT: No, no. There are circumstances --

CHAIRMAN SHEWMON: If we have had --

MR. OKRENT: And I -- also if you're interested in debris bed formation, how is it that these experiments will give you meaningful information?

MR. PICKLESIMEN: If we had had this information in hand, we would have been alot more comfortable in understanding what was going on in TMI 2 during the accident and what could be done about it.

MR. OKRENT: Oh, look. I question that in the first place. And in the second place I don't know what the next kind of accident will look like and it maybe so different that whatever you've done that helps you understand TMI 2, would bear no relation to it.

MR. PICKLESIMEN: Well, that's easy to say Steve but it's kind of glib because if we're really talking about what happens when you've got a loca, a small break loca, there really aren't tremendously different scenarios there. You boil the stuff dry. It gets hotter and hotter. You assume maybe you can't keep adding water to it.

UNKNOWN VOICE: Paul. A question.

MR. WRIGHT: Bob Wright, Advanced REactor Safety Research. I have the responsibility for the debris bed work in the fast reactor area. Dr. Okrent, when we look at this in the context of a formal program planning, processes in the water reactors on debris formation look quite different to us in the fast reactor case. You have this oxidizing coolant, you have -- material problem is quite different and it does appear to us that this is an area that we can't just directly apply our elementary R experience.

Incidently the elementary R side I think that reformation of process in characteristic of the degree are probably a weaker link than our knowledge of the coolability of the given configuration.

CHAIRMAN SHEWMON: Do you expect to be committing -- I guess I'm on the wrong slide. Are you still --

MR. PICKLESIMEN: Well, I put the next light on.

CHAIRMAN SHEWMON: Do you expect to be committing to any of those in the coming year? I just think you're going to melt down loft in the next year, on purpose at least.

MR. PICKLESIMEN: ESSOR is already committed. The first test will be this fall, is that not right Doctor?

DR. VAN HOUTEN: It's NRU.

MR. PICKLESIMEN: I'm sorry. I'm sorry. NRU. Excuse me. ESSOR, Bob?

MR. WRIGHT: That's right. It'll be late 82 or early 83 before the first preliminary test.

CHAIRMAN SHEWMON: In ESSOR?

MR. WRIGHT: Yeah.

CHAIRMAN SHEWMON: Well, why don't we put this off until next year. I think there's alot of question about doing any of that. I guess I'm not interested in getting into in great depth here but --

MR. PICKLESIMEN: Well, except the last -- and that's very important.

CHAIRMAN SHEWMON: Oh, no question about that. I hope they get to do that in the next year.

MR. PICKLESIMEN: I'm participating in the 7.2 and 7.4 planning committees.

CHAIRMAN SHEWMON: Okay.

MR. JOHNSTON: Dr. Shewmon?

CHAIRMAN SHEWMON: Yes.

MR. JOHNSTON: We are committing to some of those programs this year and I better make it very clear to you so that we don't mislead you. We're spending money on PBF advance planning right now so and we expect to spend several million dollars in 81 probably on the thing so that --

CHAIRMAN SHEWMON: We'll get into that in August.

MR. JOHNSTON: In fact we are committing on it --

CHAIRMAN SHEWMON: We'll get into that in August.

MR. JOHNSTON: We'll get into this in -- we'll get into both of those in August, that's correct. We'll talk about ESSOR again in August.

CHAIRMAN SHEWMON: Pick said -- Pick said when he started he was going to talk about things we weren't going to cover, I guess I said in the next months meeting.

MR. JOHNSTON: Okay.

CHAIRMAN SHEWMON: It seems to me this is unstructured enough here so that I can -- I agree it's important and let's make a point then of discussing it in August.

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MR. JOHNSTON: Yeah, we'd like to do that very much.

CHAIRMAN SHEWMON: Okay.

MR. PICKLESIMEN: Now here are areas that programs are to be planned. And we have nothing at the present time planned in these areas it's just areas where we know we need to do work or maybe do work. We've got to see what has to be done, how sensitive the program will have to be and what the funding is going to be.

And these are reaction committees who know rebed. There is some evidence that the debris bed in TMI 2 have remelted, at least in part, at about 3 hours and 45 minutes into the accident. Impile seperate affects tests basic to, so on.

None of these programs have been planned at the present time. The brief cool closing studies it is coming from the back of our head that something will have to be done. We don't know how to do it. We don't know how expensive it will have to be.

Efficient product release and distribution will come as part of the other programs. Efficient product tests is again -- we're going to have to have some calibration tests. What has to be done, I don't know.

MR. OKRENT: Excuse me, what's the evidence that there was melting of debris beds? You mentioned that there was evidence.

MR. PICKLESIMEN: I'll cover that this afternoon when I do my -- studies.

MR. OKRENT: Okay. I'll wait.

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MR. PICKLESIMEN: There's a good bit of evidence there, all indirect unfortunately. Now -- of the severe core damage studies, we have programs in place in FY 81. Core damage condition and SR. The initial programs and -- would have been started. Examination of TMI 2 is being planned. We probably won't be in the reactor until 82. PBS severe core damage will be in place in 81, at least in the planning stage and in the test room design.

The programs contained in FY 82 is the modeling of severe core damage. The program is not presently funded in FY 82 are to be fuel --, efficient product release and distribution.

Programs starting after 82 are --

CHAIRMAN SHEWMON: Why don't you let us read that.

MR. PICKLESIMEN: I'm sorry.

CHAIRMAN SHEWMON: If we go back up to insepient fuel clad melting, where would that be done?

MR. PICKLESIMEN: That had not been decided yet. It depends on what kind of a final program we are looking for. I would expect that it would be done at some place like --, Sandia, or Oak Ridge.

CHAIRMAN SHEWMON: Okay. So, when you say it's in place you mean you feel you have a fairly firm budgetory committment that you're not -- that it's in place?

MR.PICKLESIMEN: In 81. Yes, in 81. The details of that though have to be worked out in the next few months.

CHAIRMAN SHEWMON: Okay. Fine.

MR. PICKLESIMEN: Now, the ex-pile program presently in

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the planning stage is the --. The things we will be looking at -- the studying of liquified fuel formation. It will be bench scale. We're looking at reactor --. These probably will be seperate programs.

All right that's it.

CHAIRMAN SHEWMON: Now, thank you. Are we ready for lunch? Okay. Let's adjourn for an hour and I guess we ought to have a talk, Bill, about what we're going to cut out of the afternoon program before we go away.

A F T E R N O O N S E S S I O N

MR. KELBER: The major part of the program that I'm to discuss will be discussed with the ad hoc subcommittee on May 9th.

We are in the process of formulating a program of classifying accident research. A major portion of that program is of integrated fuel -- program that you have heard about.

CHAIRMAN SHEWMON: What does integrated mean?

MR. KELBER: It means that it draws upon all the resources within the division -- within the office of research, including PAS, the work that has been sponsored within the lightwater reactor area, the work that has been in the past under advanced reactor safety restraint.

CHAIRMAN SHEWMON: Fine.

Okay. Go ahead.

MR. KELBER: The logic of the program is dictated by the necessity of answering a series of questions. These are the questions which we believe will be taken up over the next three or four years in the various rule making hearings on cooling degraded cores, on Class 9 rule making, on siting rule making.

If we are lucky we will have some time in order to answer some of these questions.

These challenges have all been identified such as reaching a secondary to either check valve failure or steam generator tube rupture which might come from the quasi static pressure following a steam explosion, can a melted down core breech the pressure vessel and overload the containment and the questions -- the primary question there is the revent coolability and the steam spike.

Current predictions are that the steam spike for example will rupture the containment unless you do something about it.

Can a hydrogen explosion breech the containment? Current estimate for a large dry containment is that a hydrogen explosion per se will not, that you probably can't get it if it's well mixed in the large dry containment.

CHAIRMAN SHEWMON: If we go back to two, whether or not you'll breech the pressure vessel will depend a lot on cooling and I guess in the Indian Point, Zion writeup there was -- maybe it was the Kemmeny Commission Report or -- there was discussion of some reactors were actually designed so you could flood beneath the pressure vessel and cool from there. Is that still part of anybody's procedures even if they had the capability?

MR. KELBER: We are -- we are -- we are speculating on various medigation methods including that one for Zion and Indian Point to make, for example, kind of a poor man's pressure supression pool by flooding the containment to considerable depth with about a million gallons of water.

We are somewhat uncertain about the coolability of the debris that are predicted.

CHAIRMAN SHEWMON: Okay.

Now, this is the debris inside the pressure vessel?

MR. KELBER: Inside the pressure vessel or x-vessel. Frankly, there are very few data to go on.

CHAIRMAN SHEWMON: Um-hum.

MR. KELBER: The current predictions are that if the fragments are as large as they appear to be and if the bed is reasonably well packed, now these are highly hypothetical, then we would for sequences that -- for sequences where the debris beta forms early with relatively high amounts of decay heat, we are pessimistic about the ability to cool -- we think it may melt at least in part.

On the other hand, where you have sequences which go for several hours as you did at TMI 2, for example, it is possible that there will be enough release of fission products and enough decay of what remains behind that it may be coolable.

CHAIRMAN SHEWMON: Um-hum.

MR. KELBER: We just have very few data points to go on although we have some reasonable models at this point.

CHAIRMAN SHEWMON: Yeah.

MR. KELBER: We don't think a steam explosion can breech the pressure vessel let alone the containment but there are some containments where a steam explosion in the sump could generate some sizable concrete missiles and we'd have to look at each case in particular to see whether anything might be endangered by such a concrete missile.

Can a hot core melt the base mat? Well, obviously it can but we don't think it will go through the base mat, if the base mat is reasonably thick. If it does go through, it will go through as a form of slag in solid form. That's our current thinking.

On the other hand, it will generate a great deal of gas in the process and aersols and there may be some benefit to protecting to such gas generation, that has to be evaluated.

Can the containment slowly heat up and be over-pressurized and that brings up the question of long-term protection in case of a loss of power. Sprays help you buy time and there are ice condensers similar will help you buy time in the -- in the near term after a transient but the ice condensers generally speaking are gone by the time your pressure starts to build up. And the question here is how can be stretch that out.

CHAIRMAN SHEWMON: The ice condenser is designed to cope with the loci?

MR. KELBER: Yeah.

CHAIRMAN SHEWMON: Heat only?

MR. KELBER: Yeah.

CHAIRMAN SHEWMON: Then presumably you've got the core covered again?

MR. KELBER: Right.

There are various things I think that one can do with -- one of the principle candidates the we're looking at now, of course, is the filtered vented containment system. But there are others. And I don't think that anybody's ideas should be fixed, I hope they're not fixed at this time.

Can maintenance of vital functions bypass the container -- it's integrity and there I think we refer to the fact that there are lines which obviously penetrate the containment which may have to be maintained such as the letdown line. These are paths for fission products to escapte.

Generally speaking these fission products we expect will be absorbed by the water in the lines and while there may be some release it will be relatively small and tolerable release. But that again remains to be shown. It's certainly a reasonable expectation at this time.

Can failures in instruments and controls compromise a safety systems and that's pretty generally a question throughout. But it in particular it's a question here can we maintain the containment controls.

At this point when you're talking about classifying accidents, your objective is first if you can maintain the debris cooled in the primary coolant system do so. At least it's a good place to have the debris. If you can't, then you've got to protect the containment. And the question is what resources do you have at your hands to do that.

Now, this --

MR. MARK: What's the -- what's the time scale one should have in mind for some of this? I'm looking at number three, for example, now it only took about two weeks for some group to decide that if you had 100 percent hydrogen and if you had high temperature in a small containment, then you could knock it apart even without a hydrogen explosion and that's probably true.

MR. KELBER: Yeah, for the small containment.

MR. MARK: But they invented an impossible senario to get there.

MR. KELBER: Yeah.

MR. MARK: Are you going to be able to say that senario is totally ridiculous, forget it?

MR. KELBER: I would hope so.

MR. MARK: And could you say it in a week or so?

MR. KELBER: I would hope so, but I don't -- I think we will need -- on this one I would hope within a year given the funds and that's the real -- of the matter. It is not a technically -- it is not technically that difficult a problem at least for the large dry containments. For the small containments it may be a question --

MR. MARK: I'm speaking of the small one but in order to get there they really turned on if this and if that and if something else, some ifs were totally impossible.

MR. KELBER: Well, it is true that even the large dry containment we didn't find it easy to get a mixture that would detonate. We -- we had to force the code to pretend that it detonated. We did end up doing isocore burn calculations which are different matter all together. There is some work left to be done there.

It is not a tech -- it is not conceptually complex problem and I would hope that given the funding that within a year something could come of this. And I think it is necessary to do so because the Class 9 rule making hearings, assuming that they start sometime this fall or winter, will demand a series of answers to questions like this and not just for the large dry containment.

Over the next two to three years, I would anticipate they will last at least two years and maybe last three years and if we don't have the answers in hand or at least a good promise of getting the answers in the near future, I think that the results may be a good deal worse than the -- at the ECCS hearings.

Well, this sort of logic has led us to create this structure and this transition here represents the overlap with the program that has been described to you earlier today and I think until -- I think we're going to have to be somewhat patient with our organization until our decision units are rectified with -- that Dr. Murley and Dr. Bubits are doing that now with the Controller and the Commission until the decision units are rectified, until the technical work is carefully planned. There's going to be a fair amount of overlap.

For example, if there's a new loop constructed in PB -- for PBF, we wouldn't make another copy of that loop. We would obviously use that loop but we thing that there is a -- there are impile tests needed in how you form the debris beds and what their charcteristics are and we'll use what's available. But I have -- we think we have to show what is necessary.

CHAIRMAN SHEWMON: Since you're talking about presumably the -- since we've now made sure that control rods go in by -- we have -- why do we need an impile test. 'Cause presumably we're talking about decay heat?

MR. KELBER: Generally -- well, yeah, that's right but generally speaking, when you're talking about molten fuel and debris moving around, one of the technically most satisfactory ways of generating heat is with neutrons.

We have used induction heating of metal spheres, for example, in -- and this is very useful in providing some guidance to the tests and we have, for example, used them in some of the correlations that are being developed at Sandia --

CHAIRMAN SHEWMON: So this presumably would be an undercooled low power core that would melt then.

MR. KELBER: Well, I don't think --

CHAIRMAN SHEWMON: Or subassembly or something.

MR. KELBER: -- I would -- yes, all right. Yeah, a rod really.

CHAIRMAN SHEWMON: A rod, okay.

MR. KELBER: Actually we do, of course, have the D Series tests on debris beds with sodium and we would anticipate translating that into water. That is actually putting water in there and lowering the enrichment but that's at least a year away.

CHAIRMAN SHEWMON: Okay.

MR. KELBER: The integrated fuel melt program, I believe you have heard of at least once and I would say that originally the integrated fuel melt program was pulled -- was designed to pull together what is now going on, what is current in the program and contains some description of additional work that might be coming along later.

As we get our direction we anticipate that this will become more embracing and one of the things that we want to hear from them -- one of the bodies we want to hear from is the ACRS because our minds are by no means closed on this. There are -- certainly the question of scope and the question of level of effort are somewhat open.

CHAIRMAN SHEWMON: Let me assure you that you will.

MR. KELBER: Yeah, I anticipate that.

Under containment response there isn't -- to accident loads there is -- there are two points I would like to make. Code improvements we think are pretty straight forward. There's an excellent LWR containment code beacon which handles the blow down from one con -- from one compartment to another. We have a good modular code contained that we think can be married with beacon and handle the problem.

CHAIRMAN SHEWMON: Come on, we're going too long.

MR. KELBER: Okay.

Structural analysis there's a problem namely when do you -- what is struc -- what is failure of containment. Systems interaction is what I really wanted to emphasize.

One of the things -- one of the lessons that we learned from Zion and Indian Point is that the system interactions if you put in a core catch, put in a filtered vented container or any other medigating system, the system interactions may well dictate what strategy you use to deploy that feature and how successful it will be.

Finally, there's a last item there and that's the LMFBR's, they will show up in this decision unit if they show up at all.

CHAIRMAN SHEWMON: Good. Okay.

Thank you.

Any questions?

Okay. Well, we'll see you in a couple of weeks.

MR. KELBER: Yeah.

MR. JOHNSTON: I'm just going -- I'm just going to talk for a few minutes to introduce the next topic -- core status. I simply want to give you some background that I was chairman of the or task force leader on the SIG Reboven report that looked at the physical status of the accident sequence and the physical status of the plant and some of the what ifs. We're going to talk a little bit about today where we think the plant is at the present time and the sequence of events of the first couple of hours that got it there.

The way that we went about our work was to work with a staff of about five, they were all in a special investigation group plus a number of people from the National Laboratories that helped us. Walt Merkin was on assignment from Sandia Laboratory, for example. There was a special task force under Al Snyder at Sandia that was put together to help us on a number of these areas. They in turn brought in some other groups like NG, Incorporated and several other west coast laboratories that also were involved in it. So it was a wider spreading group.

We made -- had some help EG&G, we had some help from Oak Ridge. These are the people who looked at the data with us and helped us with the analysis and the writing up.

In addition to that we ourselves and Pick in particular made numerous visits up to TMI. We got the raw date, we worked with the actual strip charts, we had the reactivator information directly, we had the radioactive releases and that sort of thing.

And one final point that Pick will not be talking about and that has to do with the hydrogen bubble disappearance and I'll just mention that to you as a highlight, that we got ahold of the actual raw data that the industry was using and so forth in making their calculations of the bubble sizes and we found a number of errors in their calculations and that sort of thing. And we improved upon the equation that they were using to make the calculations, putting in some of the other correction factors.

And in the report which will be coming out this Friday finally on chap -- Volume II of our SIG report, you will see curves which I forgot to bring down that shows that the hydrogen bubble was always decreasing at all times. It was -- there was never any increase followed by a decrease. -- uncertainty of the data analysis. So it was always decreasing and it did go from the order of -- depending on where you want to start counting, from the order of 1,500 cubic feet and it was gone by Sunday noon if you make the solubility corrections.

With that I'll simply --

MR. MARK: The thing you're describing is the Appendix or Volumn II of the Reboven.

MR. JOHNSTON: It's Volumn II of the Reboven report and this will be found --

MR. MARK: And the mistakes you refer to were made by the President's Committee technical?

MR. JOHNSTON: No. No. These mistakes were made by the B&W and the TMI operators -- calculations --

MR. MARK: Did the President's Committee in their Appendix get things straight?

MR. JOHNSTON: The President's Committee I don't believe addressed this.

MR. MARK: They had Appendix Hydrogen.

MR. JOHNSTON: Yeah, but they didn't make these kind of cal -- they didn't recalculate the data.

VOICE: This is the bubble inside the pressure vessel.

MR. JOHNSTON: I read it, it's been a month ago and it didn't speak to this particular point.

MR. MARK: So they --

MR. JOHNSTON: It chewed everybody out for not being up to snuff on the hydrogen and not recognize the oxygen and being a non-problem. They certainly did that.

MR. MARK: Yeah.

MR JOHNSTON: We did -- we did it too in a slightly different way but our -- our contribution I feel was looking at the what the real hydrogen bubble size was and that aspect of it and whether it really disappeared.

Okay.

MR. PICKLESIMER: To attempt to give you a complete detailed explanation of all parts of what we went through and a detailed analysis for estimating the programming at TMI 2 would take an all day presentation.

I can only skim what we get. The data we used was from data acquistions systems like the reactivator, the plant computer and the -- burner, the alarm burner, used the in-house data acquistion system of various types, strip charts, multiple point recorders, log data, anything we could get our hands on.

I'm going first try to describe the damage at three hours, then the damage at four hours and then I'll try to tell you how we came to the conclusions on this and try to cover very quickly the sequence of events from the start of the accident till it was over with about 16 hours later.

Now, here is a view graph of the plant site. Let me get myself oriented properly. I always get mixed up on that.

CHAIRMAN SHEWMON: TMI 2 is on the right.

MR. PICKLESIMER: This is the reactor TMI 2 and these are it's cooling towers and our bases were back here in the background, our base of operations.

All right.

Now, this is something like the TMI 2 core, it's not precise and I understand it but it gives you the basic locations of things.

This is your outlet nozzle, that is the hot leg. Here is the inlet nozzel, the cold legs, the downcomer, the core itself, the instrumentation tubes that go through the bottom of the pressure vessel, 52 of them up through 52 assemblies that are located in a spiral pattern from the center out to almost the core but not quite. I'll show you a full map later that shows that.

Then the upper head structure, the upper in fittings in here which are fairly important. They're stainless steel, 17 lbs., they have about 200 square inches of surface area each and when they get up to 2,000 amps they start oxidizing and start producing hydrogen too.

Your control rod guide tubes and so on up in here and then the upper head. Now --

CHAIRMAN SHEWMON: Those top pieces weighed seven pounds each you said?

MR. PICKLESIMER: The upper in fittings at the top of each assembly weighed 17 pounds.

CHAIRMAN SHEWMON: Okay.

MR. PICKLESIMER: It's about 200 square inches is the best estimate I can come up with, the surface area, 304 stainless.

Now, this is what we believe the -- analysis was the condition of the core at three hours. The first fuel lines had burst about five minutes after the block valve to the pilot operated relief valve that was closed by an operator who finally realized that they had a small break open.

This is tied to evidence from a strip chart recorder of the activity sensing instrument that's located a little bit above the core, I can't remember the exact position but in normal operation it sees nitrogen 16 in the A hot leg of the reactor.

As best we can pin down the timing on the strip chart, there was a halt of activity detected in that instrument at five minutes after the block valve was closed. We believe that's when the first set of rods burst. That's the first time -- crypton inside the primary system to activate that sensor.

By calculations with TMI 2 -- which I won't cover unless there are a bunch of questions as the code that George Marino wrote which has proved to be very, very useful to us, we estimate that all the rods had burst within 20 minutes over the entire reactor core. And that's 20 minutes after the block valve was closed.

The rod burst occurred between one and two feet in the center bundle and between two and three feet in the outer bundle. Now, this is not a grade of compression across the core for the simple reason there is not a graded -- such a graded progression of radial peaking factors on the assemblies.

There are, for example, halfway out on the -- there is a subassembly -- an assembly in there which has a much lower radial peaking factor than either of its neighbors. It's effectively a cold spot.

We don't believe it was actually cold -- blockage. It was near it but not really there.

The maximum temperature reached that we estimate at 4400 F in the upper three feet or more of the core. More than two thirds of the core reached temperatures of that at it may well have gone all -- all the way across, we don't have anyway of really getting a handle on it but at least that much.

And 3,600 F was reached for all the core at least three feet down on the rod and maybe four feet down.

Now, these -- these are as a result of the TMI 2 boil calculations calibrated to all of the other calculations -- hydrogen generation, activity releases, the whole bit.

Now -- by oxidation occurred over the entire core down to a level of four and a half to five feet from the top. Now, this is at three hours. There was later damaging. -- about two feet thick was probably formed with a base at about eight feet from the bottom of the core, about four feet from the top. That -- was about two feet thick and was formed by the formation of liquified fuel and the embrittlement of the cladding and it was aiding by the thermal shock when the pump was turned on at two hours and 54 minutes.

The damaged core was only partly quenched by the water from the steam generator feed because much of that water actually went into steam generator A. You will not find this in the Volumn II report because Chuck -- Bill Johnston and I did not find it until just after Christmas. The report was already written and it was too late to modify it. I'll show you a slide on this a little bit later that will show you how we came to the conclusion at least a fair part of that water went into the steam generator A through it's -- from the downcomer and did not go into the core.

There was not less than 300 pounds of hydrogen then produced by three hours from the oxidation of the -- Now, if I try to make some estimates of the oxidation of the stainless steel, I have to do a hell of a lot of assumptions about one temperature those upper in fittings got to. I know what some of the steam temperatures were coming out of the top of the core. These temperatures were measured. Half the thermal couples -- these temperatures were measured in the upper in fitting itself. So its temperatures had to be close to that on the lower part. I have no idea of what the radiative heat losses were from the top of those upper in fittings up into the upper --

CHAIRMAN SHEWMON: Go back to the top of that for a minute.

MR. PICKLESIMER: Yes.

CHAIRMAN SHEWMON: Are those -- is that supposed to be chronological order?

The maximum temperatures were before the block valve was closed or after?

MR. PICKLESIMER: Oh, no, the maximum temperatures were reached just about the time the pump went on.

CHAIRMAN SHEWMON: Well, when was the pump turned on relative to when the block valve was closed?

MR. PICKLESIMER: The block valve was closed at two hours and 20 minutes and the pump was turned on at two hours and 54 minutes. 34 minutes apart.

CHAIRMAN SHEWMON: Okay.

Thank you.

MR. PICKLESIMER: All right.

Now, here's -- of the idea I have of what the damage is in the core. We have embrittled cladding down halfway or better down the rods. We have liquified fuel formed in debris bed just above that. The rod burst up at this level.

Now, all of this material in here is going into the debris bed. Part of it is liquified fuel, part of it is shattered fuel.

I -- these earlier to show you what I'm talking about as liquified fuel and what some of the rods looked like at the time that they were thermally shocked. We feel that many of the rods looked like this and there was liquified fuel down in the subchannels and this is what the steam and the water hit.

Now, to make our budget calculations I'd like to demonstrate this, thought this unfortunately did not make your handout. I don't know why it was left out but it was. It's not too important. If you have only decay heat as your heat source, you're 15 minutes into the accident, you're at the three foot level, you boil the core down and assuming a constant rate you boiled it down to three feet and now you ask what is the heat -- of the load on the rod that is uncovered at that time.

All right.

That's this point right here. Now, if you have decay heat only we can make some simplifying assumptions as to specific heats, a whole bunch of things, you wind up with essentially a straight line until you get up here to 3,500 F where you have the -- which then goes into the -- and forms liquified fuel.

If you add oxidation heat onto this you start picking up significant amounts of oxidation heat by 1,600 F. It is something like 10 percent of the decay heat in TMI 2 a this particular time. It must be accounted for in the calculations.

By the time you're up about 2,500 F the heat generated by the oxidation is greater than the decay heat itself and the thing is accelerating phenomonon on.

Now, this is one of the reasons why I'm not yet convinced that we must do oxidation -- more oxidation studies between 1,500 and 1,800 degrees centigrade. You're up in this region here. If you've got this kind of thing and you cross that, what difference does it make to you whether you've go 30 seconds different in reaching this temperature. And that's about all it amounts. So I'm not convinced that we have to do that. But we will look at it.

Now, this was some of the first calculations I made last April just a little more than a year ago on this. These are all hand calculations with some graphical solutions, a hand calculator and so on. The top rod, this does not allow for steam heatup of the top part of the rods. I was just not able to do that in a simple calculation. So it doesn't take that into consideration.

I made some simplifying assumptions like 25 percent of the total heat is lost to the rod at that particular vent. When the liquified fuel is formed, you have no more oxidation heat generated at that particular note and now you have only decay heat going at the particular --. I have no way of handling that liquified fuel oxidation.

So the top of the core will heat up along like this and along in here it would take off -- where's my -- point, righ t here it it. Right here. You start oxidation in here and it would come up at this rate.

The one foot level would come up here, with decay heat only it would come through here, with oxidation it would come up to this point. Two feet, three feet and so on.

What it tells you is, with this kind of analysis, is if your burst temperature and this prepressurization was such that I would expect the rods to burst between 1,400 and

1,500 F at these kind of heating rates. Your burst would occur someplace between one and two feet. That's the hot spot on the rod if it gets to 1,500 F first.

All right.

Then this would be later overtaken by rod notes that were lower down because they had more decay heat because of the axial power profile in the rods. So the point that reaches the 3,500 F -- first is the two foot level. Between the two and three. They reach it about the same time.

Now, this is the kind of analyses we were doing.

MR. MARK: Did this --

MR. PICKLESIMER: This printout here is the printout --

MR. MARK: Did you --

MR. PICKLESIMER: Yes.

MR. MARK: -- say that you did not allow for the cooling by steam at the --

MR. PICKLESIMER: Not in that calculation.

MR. MARK: In that calculation.

MR. PICKLESIMER: Not in that one, no.

MR. MARK: Do you have a guess as to --

MR. PICKLESIMER: That's what we want to come up to now.

MR. MARK: Okay.

MR. PICKLESIMER: All right.

Now, this is a printout from the TMI boil code, this does allow radiative heat loss to the steam, it allows heat transfer from the steam back to the rod, it allows for the variation of specific heats with temperature, the variation of steam properties with pressure and temperature. It allows for just about almost anything you could want in thing in a fairly sensible fashion --

All right.

What heat come in with on the same bundle in there, the zero is at the top of the core, one foot, one is the one foot level, two the two foot level and so on. You take a look at this, this plot is almost the same as mine. It's not that much different. So my simplying calculations originally were not that bad. But now we have made these kinds of calculations for many different conditions.

In this particular one, we boil down to 33 minutes to a level of eight feet, held that level at eight feet figuring we had dribble back from the condensers through the cold legs into the core and just held the level constantly. We had no better information to go on.

If we take 20 minutes to go down to eight feet, we change these times by a few minutes. That's all. If we go down to seven feet, we don't get temperatures like 3,600F at three feet. The hottest temperature up there won't even get up to 3,200 if we only boil down to seven feet, now this eight feet.

If we boil down to nine feet, the six foot level up here gets about 3,000.

All right.

Our conclusion that the damage here, the liquified fuel formation down to between three and four and a half is based on these kinds of calculations.

If we boil down to seven feet, we don't get as nearly as much damage that we know happened -- hydrogen, we found activity, anything.

If we boil down to nine feet, we lose far too much. We can -- fall down to about eight feet plus or minus six inches. I don't believe that uncertainly limit myself. I think it's more than that. That's what we draw conclusions from our calculations.

Now, that was at three hours. We have a great deal of evidence that says there was more damaged produced at three hours and 45 minutes and that's what I want to talk about right now, is to characterize the damage -- at four hours.

We have manually read in-core thermal couples that were read between 8:00 and 9:00 o'clock in the morning with -- meters, that indicate temperatures as high as 2,600F indicated by the in-core thermal couples.

If those in-core thermal couples were intact and in existence, that means at 2,600 F temperature was read in the -- in the upper intake.

If that thermal couple was not intact at that point, that temperature had to be down in the bed and that thermal couple had to have been melted and debris formed. You don't have any other choice for the thing.

All right.

There was not just one temperature, there was 12 temperatures above 2,000 F. It took them over an hour to read the 52 thermal couples. So the temperature map I'll show you in just a minute. It took over an hour for them to read and as you go out in the spiral the temperatures get lower for the most part.

All right.

There are -- neutron dectors, seven of them in the instrumentation tubes as in the center of 52 of those assemblies. When those things get above something like 1,000 to 2,000F, they give a signal which causes the plant computer or alarm printer to record them as bad.

In other words, they would be -- have given -- should not have been reading at all. Now, they are reading much too high, they're off scale and the alarm printer --

If we simply take the first time that the Reboven SD&D from level one down at the bottom up to level seven at the top is alarmed as our anchor point for estimating. Then we have 17 SPND's at level one and two were alarmed in about 45 seconds, at 7:45 in the morning.

Now, this means a sudden -- down one foot from the bottom of the core there were temperatures above 1,000F and this is down in water.

All right.

There's only one way you can get that damage -- if you have liquified fuel dropping down in the subchannels just like a lava flow to get down around an instrumentation tube and seal it off from water and then the thing heats up.

So this says that there was more core damage down in the debris bed and below that at that time.

Now, we believe that this liquified fuel that formed in the debris bed sealed that core to level off from steam cooling and form the steam bubble below. This then drove the water levels down further and there was more oxidation and cladding damage as a result of the steam bubble driving the water level lower.

At 7:45 in the morning somehow or another this debris bed and sealing layer was penetrated and there was subsequent steam errruption by water coming in from the downcomer into the bottom of the core and up into that --

There is an 80 PSI pressure increase in the entire primary system when it has more than 6,000 cubic feet of vabor space on it. 80 PSI up as fast as a recorder strip chart can take it up. It was something like less than 10 seconds. So there was that particular -- the thing leveled off at about 100 PSI increase and then it turned around and came right back down again.

So we have the SPND's, we have the pressure pulse, we have a number of other indicates plus the temperatures that indicated that there was considerably more damage done at four hours or three hours and 45 minutes.

Now, at that time we estimate that at four hours more than 60 percent of zircoloid in the core had been embrittled or shattered. That doesn't mean oxidized now. It just says that it has been damaged.

I believe the lower surfaces of the debris bed had dropped to about five feet from the bottom of the core and liquified fuel had penetrated within one foot of the bottom of the core in some areas. We don't know how many but we did have 17 SPND's at the one and two and half foot levels go off scale.

Our calculations indicated that from this amount of zircoloid that between 700 and 820 pounds of hydrogen were produced by four hours. And it may have well been more because later we can't estimate that. We have not way of getting at it. There may have been additional hydrogen produced by the oxidation of the stainless steel in the upper in fittings, stainless steel on the control rods inside, we have no way of estimating that.

If I make some simplifying assumptions I come up with something like 50 pounds of hydrogen. In light of our uncertainties here I ignore the 50 pounds.

All right.

Now, we got -- this is a map of the core. Each of these small blocks is an assembly. Each of these colored squares is where there was an instrumentation tube and an in-core thermal couple reading.

The -- on this one is this is at -- between the hours of 6:55 and 7:15 in the morning, two hours and 55 minutes to three hours and 13 minutes of accident time, these thermal couples were all shown by the alarm printer, the red ones to be above 700F. The purple ones were between 650 and 700 where they showed on the alarm printer as coming back on scale. The alarm printer records the first indication -- the first temperature that it sees after it's come back on scale. So this could have been higher earlier. This is over an 18 minute span. I don't know when the alarm printer got to it.

The blue is at 600 to 650 and so on. But you see all of the red ones, those were all over 700F.

Now, this is the data that was read by the instrument men with no -- meter and converted to temperature. Here's a temperature of 2,453 and 2,451, 2,055, 2,655, 2,402, 2,242 and so on.

Now, remembering that they started here reading this one first and went out in the spiral like this to read these two last.

Now, this one was that it had -- was -- thermal coupled, it never did read until much later. Why it read much later we don't know.

It took them more than an hour from the time they started here at number one until they got here. So there was time for cooling down of a bunch of these thermal couples and because these temperatures over in here have dropped, that doesn't mean that that wasn't at 8:00 o'clock, a 2,000F thermal couple. We don't know.

All right.

Now, going here on this plot the particular positions in the instrumentation tubes where the -- at level one and two went off scale at 7:45 in about a 30 second time period. There are -- these -- these two -- this one was already off scale. This one was off scale, this one went off scale. This one was already off scale. All the rest of these went off scale in about a 30 second time period.

Now, to show you how we got to most of this, I need to lay a little bit of background. This is the drawing of the reactor primary system. This is steam generator B, steam generator A, the hot legs, the hot leg temperatures were measured right here. These are the cold legs, the cold leg temperatures were measured right here just below the pump.

The make up lines from the make up pumps injected in the middle of that -- there and -- and in this one over here. One reactor pump was left out of this drawing right here. The one A pump so that you can see some of the other factors.

Some things that are very important here is the surge line right here from the pressurizer enters the hot leg at this point. This is about four feet above the center line of this pipe. This pipe incidentally is 36 inches -- these are 28.

The letdown line comes out of this cold leg, one A cold leg on the A steam generator. This is the pressurizer here, the spray -- the PORV, the stuff opens one of these up here and another point that is critical in the interpretation is the pressurizer spray line that runs from here down to just at the outlet of the two A pump. That pressurizer line feeds a spray of water into the top of the pressurizer to cool it down. It lowers system pressure. That's what it's normal purpose is.

At the time the accident was started, that spray line was operating, it was spraying down the pressurizer. They were were trying to decrease boron level.

Now, I think -- I'll be coming back to this in a minute.

Now, another critical point is here on the pressurizer. Your search line comes in at the bottom here. You have a set of heaters, a thermometer -- a resistence thermometer located about one foot above the top most heater -- ‸ectrical heater in here.

The critical points are your reference line or your pressurizer level indication comes in at this point up here. The reading leg is down here. Since this normally is in steam, you have steam condensed in here to fill this reference leg up to this level. So that maintains a relatively constant position for reading your pressurizer level.

This level sensor down here now reads the level of the water relative to that point. It reads the pressure level difference and that's what the level indication really is.

The critical point on this is if this leg clashes and the water in this leg is lower than the water in the pressurizer, we read a full pressurizer at all times.

All right.

Now, this is a complicated full plot. I don't know have any other way of trying to handle the massive data that has to be looked at here.

Now, I have emphasized on this one and I have a number of errors drawn in because I have emphasized the line, you have a finely scaled print that is considerably more accurate.

Up here at the top we have -- on and off for make up pumps, the core flood -- they misaligned that overlay. These two to the left so that -- is at two hours and 54 minutes.

All right.

Now, here is the system pressure. Part of this is on the reactivator and part of it is on the strip chart and part of it is on plant computer. This is the -- monitor which is the instrument that's located just outside the core that normally reads the activity of the core. I'm sorry, not this one, this is -- startup. The intermediate reads the normal operation.

This is the steam generator pressures -- no, the fill range -- the fill range. This -- these are the pressures. Down here is when the atmospheric steam valve was on. This was when the -- when they were steaming to condensers. This is when the decay heat pumps were on and this is the decay heat plot over the time period.

Now, the times we're interested in run from right here on. At this time 90 minutes into the accident, this is what I think the system looked like.

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Now, those -- I'm sorry --

(Tape one ended at this point.)

MR. PICKLESIMER: -- on both sides, the hot legs, and the indications are by the temperatures up here on the hot legs, that there was one noımal flow, and not reversable in postulate. It's a normal flow.

Water is going up to the top of the hot leg, dripping over into the steam generator and collecting down below. In this case on a recycle, coming back up and drifting back through whole leg, it buckles up.

On the A side where the pump was plumbing it had the -- and what was indeed taken out -- got to be taken out in a let down hind.

The pressurizer was mix phase also. So was the surge line. Didn't have one temperature in here for the surge line at this time which says that it was siphoned on down.

All right. They turned this pump off at 100 minutes into the accident. When that happened this water dropped back in and the steam -- the water separated in here. This one dropped back into the core. This one simply leveled off.

We think then that water at that particular time was right at the top of the core. It may have been there.

A little bit above it or a little bit below it. We can't tell for sure.

DR, SHEWMON: Now, what's the boiling point?

MR, PICKLESIMER: Sir?

DR, SHEWMON: What's the boiling temperature of water at 1100°F?

The boiling point of water at 1100°F and 1100 p.s.i.?

P.S.I., you're right. Pardon me. I mean is 520 above or below it?

MR, PICKLESIMER: I'm sorry. I don't --

AUDIENCE: Look on your saturation curve.

Look on your saturation curve on the --

MR, PICKLESIMER: All right. 1100 p.s.i. is right here. So at this point right in here. And we're boiling at that time, yes.

1100 and 520 should be about the same.

DR. SHEWMON: Fine. Go ahead.

MR. PICKLESIMER: All right. Now, here is a plot of the pressure lozer -- I'm sorry. Of the steam generator level. And of the cold weight temperatures at the time the pump was turned off. The pump was turned off right here.

Now, it has traces for all -- for 2 of the cold weights and both populate under that terminal. The following vest -- we have all four of the hot legs and the cold legs cooling down at the same point within a few degrees of each other, from the time period of abot 4:33 to 5:40 when they turned the pump off.

2/

So they're all cooling down to perilite.

Vis-a-vis, after the pump turns off, the hot leg in B took off and went up.. Now, I think -- I can't win the argument, but I think this is when the pole was first uncovered.

This was the first entry of steam into that hot leg. All right. "A" did not do it. -- About 10 minutes later the hot leg, "A", started heating up, and it didn't stop, to look out for "E" under "F".

So you can argue here -- this had to be the point at this point there had to be core uncovered, because you have steam in that hot leg and it just continued to rise internally.

I will argue that we were uncovered 10 minutes earlier.

Now, the -- well, just to mention the core is boiling down. The pressure is dropping. There are flashing -- and that's the minimum pressure here over about 640 or 650 psi, as best we can figure it.

The close the vlock valve, because the pressure had already started to rise, and had risen from 20 or 30 psi, for full block valve was closed.

The -- once the block valve was closed, the pressure started to rise some. Then at this point there was a very

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definite infraction in the strip joint crisis rose much more rapidly and again at this point, there is a very sharp deflection point in the first occur, and it rose very rapidly from about 1400 psi to over 2000 psi, in just a few seconds.

The temperature shows this -- picks up at 1700 psi, and goes on to maximum, at this point about 2050, and this occurred over about 6 second interval.

Now, it leveled off up there, and let's see -- they had close the block valve here and opened it again at this point to start a blow down. The pump was turned on at this point for this deflection point.

We think that the water hit the hot core, pressurized the system and it's a very rapid rise here. This core is with the pump being turned on.

The pressurizer level indication here had already started to rise. It had dropped down to 300 inches and it rose to almost 385 inches. And that 3.4, 3.5 cubic feet of water -- pressurizer level.

And I have a problem in trying to figure out where that water came from. The hot leg was -- had only steam in it. No water in it.

The pressurizer had to have been dropped down to 350 inches here, and I can't figure out where that 250 something pounds of water came from, on a factor of that pressurizer of --

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Now, the hot legs were increasing in temperature here. The green one, which was the "A" hot leg -- No. I think I'm mixed up on them.

The "B" hot leg was the one that that remained hot, the highest in temperature all the time. It was about 800°F. The "A" hot leg was about 700°F to 750°F. Now this data is recorded on a multiple point recorder which prints out every 2.4 minutes. We have a hell of a time trying to follow this through on a multiple point recorder, because the printer was in very bad condition.

But we are able to go back to the original and pull a bunch of these in critical claims out.

The court imagine now has occurred from this time here to 2054 minutes to give you what I told you earlier as the time at -- the Commission at 3 hours.

Then the pumps -- make up pumps had been taken onto a high pressure injection and immediately thottled back. The hot leg -- the pumps had been swapped "A" to "D" and "B" and "C" going off/on. In this time period, we know that the pumps were on, but we also know that they were followed to a lower flow and we don't know what that flow is. Have no way of getting at it.

Now, the --

DR. SHEWMON: Pick --

MR. PICKLESIMER: Yes?

DR. SHEWMON: Let's go onto the core a little bit more if we could. Get to your bottom line.

MR. PICKLESIMER: All right.

The core damage now here at 4 hours, here is the pressure spike I was talking about that indicates that the core was disrupted at 2:54 . That is coincident with the SP&D's going off state.

We also have an SRM jump at that particular time, would indicates that there was something happened in the core.

This SRM, seeks mostly the level in the down core. In the most part. During this time period -- Now that completes the four hour core damage.

During this time period when they were trying to repressurize, they were bleed and feeding, and this is where I think most of the -- this time period here where most of the hydrogen came out.

Then they opened the block value again, and tried to blow the system down and never got below about 420 psi. And the state down in that temperature range, down in that pressure range, below 600 psi for a good many hours, until they finally started up the steam generators. They post blocked off finally and drove the HPI's in -- to drive the system back full.

One of the principal points is between this time

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here and this time right in here -- There's a 142 gallons of water went on the PWST.

Now here is evidence that indicates the pump throwing water into the --

Unless there are questions, I'll quit.

DR. SHEWMON: Okay. I think we better quit then.

What is your wild guess with regard to how hard it's going to be to pull that stuff out of there?

MR. PICKLESIMER: I think that we can go in on the periphery and start pulling core barrel shapers. And work in from the peripheral sition outside the actual fuel assemblies themselves.

That's what we're thinking about in 7.2 Committee. That's at least one way. If we have to.

DR. SHEWMON: Those will be firm and then you can peal things off into that space --

MR. PICKLESIMER: Providing that the core barrel hasn't dropped. There is a possible that core barrel has dropped and the whole thing is down and cocked. It's a possibility. We don't know.

It will just simply complicate things.

DR. SHEWMON: I dare say. Okay, thank you very much then.

MR. HOATSON: The hand-out that Paul is passing around right now is quite detailed. It's essentially a verbatim account of what I was going to say, so as I skip through these quickly, you won't miss a thing if you read that handout.

I'm going to hit three topics today. These are combustible gas generation and containment, the hydrogen program, and post accident fluent chemistry.

This combustible gas and containment is one of those things that Tom Early was talking about earlier that if LIcensee asks us to do it, we'll do it.

Now this is one of them. We have users aid to investigate the rate of hydrogen production from the sink, galvanized steel particularly zinc primers and orgqanic coatings.

This slide -- the significant thing on this is the amount of zinc in containment. This is from Sana OFRE and it's surprisingly large.

DR. OKRENT: But is it representative of the plants that began construction, let's say, after around 1970 or '72?

MR. HOATSON: As far as I'm aware, only the -- all of the plants have the significant amount of galvanized steel, in cable treadings and galvanized decking and that sort of thing. Quite a bit of zinc and all --

DR. OKRENT: Because they're concerned with this form of hydrogen generation was developed after a SANOFRE

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I -- you're talkinag bout SANOFRE I, I assume? Not II and III?

If you're talking about SANOFRE II and III, then I retract my question. They're pretty new.

MR. HOATSON: I think that was II, but I'm not sure.

DR. OKRENT: Okay.

MR. HOATSON: The program is a rather small one. It's 100 K for this year. We plan to prepare a program plan for the galvanized zinc and perform scopic tests under a variety of chemical conditions, and a temperature of -- and provide for results upon those, primarily a coorosion testing to determine the rate formation of hydrogen from --

DR. SHEWMON: Do you have any idea how many plants have biosulfate in them?

MR. HOATSON: No, I don't. There are quite a few. Base board biosulfate is used in quite a few.

DR. SHEWMON: So it's not B&W, it's Westinghouse, too?

MR. HOATSON: I'm not sure which. There are a number of plants thhat are using biosulfate.

DR. SHEWMON: The ph range quarters 10, is what you think you can get in mixtures of borated sodium hydroxide solutions, or what?

DR. SHEWMON: Now, most of this will be in contact

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with steam, not water. Is that right.

MR. HOATSON: Both. Well, it's spring water and steam, so it's got some both.

DR. SHEWMON: Okay. Go ahead.

MR. HOATSON: We have to look at both. Steam and water phase to determine which is the work base.

Now, we have 149 K with the '81 program, which goes into the zinc primers and then it tests a similar weight of the galvanized and then the planning for the organic components which will involve abbreviation exposure will be done in '81.

The status we have -- user's need. We prepared a scope for 80 and 81 and provided that to the NRR people. We're expecting an endorsement of that split width any day now. The staff has recommended they go ahead, and we should be starting work in June.

The next item is the hydrogen program. Last September I provided the Committee with copies of a trunk. I was quite -- and this is the outline of the items that we plan to include in the hydrogen program. It still looks fairly good.

The status that we provided $100,000 to Sandia to prepare that compendium, and they're in the process of doing that. It's nearing completion. We should have a draft by the end of May and it should be out for distribution in early June.

When we have that in hand, we'll be able to be a little more specific about the program planning.

DR. SHEWMON: What does radiolysis reactor solutions mean?

MR. HOATSON: Radiolysis of boric acid solutions in the primer system and also some solutions in the container.

DR. SHEWMON: But it's not just reactor cooling. It's also after it gets outside?

MR. HOATSON: There are some questions about the rate of hydrogen generation. Some effects -- the effects of fissure products, chemically on the radiolysis, and some.

DR. SHEWMON: Okay.

MR. HOATSON: There are containment volumves, just to give you a little perspective. Each of you are marking on this -- most of those are inerted. The ones that are operating -- I think there are two that are in operating license stage. The recommendation is to inert those. The recommendation of the Mark II is to inert those.

And the other parameters -- to give you an idea of the size, the PWI dry containments are 2 to 2.5 million cubic foot range.

This is a calculation that Charlie Kelpen referred to a minute ago. This is an isoporic, constant -- burring of hydrogen. It drops the hydrogen concentration forces the temperature or pressure that might -- in the containment.

He mentioned that failures of containments do not look likely, although 12% hydrogen will get you about the design pressure. The failure pressure is quite a bit higher. Almost double the design pressure. So it will take about a 28% , 40 % hydrogen to get you to that point.

DR. SHEWMON: How is the failure pressure defined?

MR. HOATSON: That was in zip study. It's failure of the liner, not failure of the concrete.

DR. SHEWMON: The liner is not up against the concrete is that right?

MR. HOATSON: Yes, it is. But the concrete, these pressure will probably have a practice split. And the assumption is that the liner will -- to the atmosphere.

DR. SHEWMON: So it's whenever you get cracking in the concrete, the liner is assumed to have failed?

MR. HOATSON: No. But the cracking of the concrete will occur first, but the failure pressure is about twice the design pressure.

The safety factor of 2.

DR. SHEWMON: Nobody's ever failed one, but that -- somebody else though has calculated or guestimated or something.

MR. HOATSON: Right.

DR. SHEWMON: We don't know how conservative or whatever.

MR. HOATSON: Not really. That was the assumption

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in the --

MR. JOHNSTON: There's a lot more plasticity, of course, in the metal liner than there is in the concrete. So they can calculate the concrete and gradually failing into attention with a metal liner expanding additionally. Now the failure occurs almost at the same time, as far as that goes. I mean the metal liner doesn't carry very much load after the concrete leaves it. But the sequence as the concrete goes first, followed by the metal liner because of the greater expansive and the greater elasticity of the plasticity of --

MR. HOATSON: For perspective, 100% zirconium is about 2200 pounds of hydrogen or 395,000 standard cubic feet. Probably TMI was 135 - 170,000 standard cubic feet. The stainless parts as Pick mentioned a while ago may add 20% to these figures.

If we get to the core melt stage, the core concrete reaction can produce quite a bit of hydrogen. More than the core ziconium.

And the perspective, 100,000 cubic feet is about 4.35% hydrogen which is above the flammability level.

DR. SHEWMON: Tell me again what it is in the core that generates hydrogen.

AUDIENCE: Zirconium and stainless.

DR. SHEWMON: We aren't counting the zirconium

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twice. We had almost all oxydized up there the first time. Pour sorta corn metal didn't we?

DR. OKRENT: Became brittle. It was not all converted to oxide.

DR. SHEWMON: That's just 17%, and now we get the rest of it? Is that the --

AUDIENCE: Yes.

DR. SHEWMON: Okay. Go ahead.

MR. HOATSON: And radiolysis, it takes about 3 - 5 cc of hydrogen per kilogram of water to stop the composition of primary water and a PWR. There are accident senerios which could lead to a loss of dissolved hydrogen.

TMI may have been very close to that. BWR's do not have added hydrogen and they normally decompose water while they're operating, and will do so in accident situations also.

Severe damage accidents can provide a larger fishing products source in the subwater for radiolysis than the design basis accident situation.

DR. OKRENT: When you say TMI may have been close to that, do you mean that they lost a substantial amount of hydrogen but still maintained enough to continue to assure a recombination?

MR. HOATSON: Yes, what we're doing in TMI was essentially boiling the core out the pressurizer relief valve.

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Much of the hydrogen flowed out that way. Must of it went up the hot leg, condensed in the boiler and the steam generator and returned to the core.

If the process continued with no additional hydrogen and we don't know how much hydrogen went into the make up water, the it would have been possible to take all of the hydrogen out of the primary system, or at least get below the level where radiolysis could begin occurring.

How close we were at TMI to that, I don't know. I don't think anyone does.

DR. SHEWMON: That was presumably after the bubbles disappear we got close to --

MR. HOATSON: No, no. Before the bubbles. Once the bubble form, the hydrogen produced from the corrosion of zirconium --

DR. SHEWMON: Fine, okay.

MR. HOATSON: -- would surpress the radiolysis together.

Energy absorption above water is well understood. The G values are fairly well understood in a laboratory basis, but not so well on the dirty conditions that you have in a plant.

Impurities influence it. Vapor/liquid/volume ratios. Chloresence boiling or turbulence in the water, ph, temperature and pressure-- all have an influence.

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DR. OKRENT: Excuse me, if I could ask just one question on this last point.

If we had a period when we were either boiling in the core, or had steam over much of the core and so forth, and they were radiolysis going on at that time, do we know whether the hydrogen and the oxygen formed would be combined before the gases got into the upper region of the vessel?

MR. HOATSON: As one going up, probably not. Because that's simply -- it's happening in a BWR.

DR. OKRENT: In other words, it's not clear to me that the oxygen necessarily recombines as soon as it was made.

MR. HOATSON: No.

DR. OKRENT: And I wonder if anybody's looked to see what would have been the maximum amount of oxygen you could have before the recombination rate was larger than the formation rate, so that there was some maximum steady state level of oxygen that you had in the bubble, assuming there was a bubble in the vessel.

MR. HOATSON: Well the recombination rate is very highly dependent on the amount of hydrogen present. If there's any hydrogen present at all, it will cause total recombination of the oxygen. If it's -- if the hydrogen is absence, then the recomposition will be at the rate --

DR. OKRENT: Well, I'm not sure what you're telling me. Let's see, if I have pure hydrogen, and I add a little bit of oxygen to it. Just in a bottle, it doesn't recombine instanteously, does it?

MR. HOATSON: Not under a radiation condition.

DR. OKRENT: Not under radiation.

MR. HOATSON: No, no.

DR. OKRENT: Well, then there's some mixture which will go spontaneously, but if you just have pure hydrogen with a little bit of --

In other words, so that -- you needed the radiation to get the reaction to go if you had a mixture of hydrogen and oxygen above?

MR. HOATSON: Oh yes.

DR. OKRENT: Now --

MR. HOATSON: And also gas station recombination is quite a bit slower than the liquid.

DR. OKRENT: Well, I'm talking about gas phase recombination and how fast that went and whether we have an estimate --

There probably is one. I just haven't seen it. Of what kind of oxygen levels one might have had.

I'm not convinced it was zero above the core. Okay? It may have been small, but I'd like -- it would have been -- it -- helpful to me to have a feeling, was it .25%, or 2% or whatever number.

DR. SHEWMON: Bill has a comment.

MR. JOHNSTON: I have some information on that. The President's Commission had this work done by two people and we reviewed it. The Argon people did it and also the origin specialist as a consultant in Pittsburgh.

MR. HOATSON: Paul Cohen.

MR. JOHNSTON: Paul Cohen did it.

The maximum estimate between the two of them was .7% oxygen would have been produced during that early part.

.7%. Small fraction. 7/10 of a percent of free oxygen may have been produced during that boiling period --

DR. SHEWMON: That .7% of the volume of gas was oxygen, in the bubble that formed, or what?

MR. JOHNSTON: At the time of the major core damage before very much hydrogen had been produced, .7% of the volume of the gas in the system. I think that's correct -- would have -- could have been oxygen as a maximum. That rapidly disappeared, however, as soon as hydrogen was produced.

Not because of gas face recombination, although that will take place above 600°C or so --

But the point is that the stuff redissolves back in the solution, and your real recombination takes place

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in solution.

So as long as you've got a 2-phase system with gas phase and a liquid that this stuff is soluble and you get your recombination back that way when it gets a chance, and that's very rapid. And it would rapidly clean the oxygen up out of the gas phase under equilibrium conditions, anyway.

DR. OKRENT: Well, I can't tell whether you were talking about the same senerio I was. But I can't recall seeing this in the present, and in the Regovin --

Which appendix is it? I'll go look it up.

MR. JOHNSTON: The chemistry. The one I think they call the chemistry.

DR. OKRENT: I'll go check.

MR. JOHNSTON: It has both Paul Cohen and I think the -- I've forgotten the group at Argon that did it, but John Hunecamp was influential in having that work done.

DR. SHEWMON: Go ahead.

MR. HOATSON: By the way what I'm giving you is a more or less kind of a preview of what's probably going to be on the compenium when it comes out. That's where most of the thing is coming from.

Gamma radiation, boric acid behaves like pure water. -- phase give higher equilibrium, decomposition levels.

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The chemical effects on decomposition are not well understood.

And the present radiolysis criteria for design basis accidents are conservative.

Hydrogen analysis was a difficult area at the time of the Three Mile Island accident. There were a lot of questions about the accuracy of the analysis, and so that there is something probably that has to be done here.

DR. SHEWMON: We'll agree to that. Why don't you just let us run down over it.

I say, we'll agree to that.

MR. HOATSON: In tact, NRC has asked the vendors to add hydrogen analyzers good for 10% by January 1, 1981.

This is just one to indicate that a very low ignition energies are required to ignite hydrogen. However, you can't depend on them. This is a curve from a G.E. report. Here they -- this is --

Well I've said hydrogen along here. The theoretical pressure-wise you would get from a combustion of hydrogen quantities along this line, the dotted line, what was actually seen --

And some of these are rather large scale units. Was that until you got up to 8%, there was little combustion of the -- of all of the hydrogen.

That's probably related to the upward and downward

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flame propogation limits for hydrogen.

But unfortunately you cannot depend on this. If you want ignition, you may get it , but you may not according to this.

DR. SHEWMON: On the previous slide were your units mila jewels?

MR. HOATSON: Yes, mila jewels.

DR. SHEWMON: That's usually a small "m" even in SI, isn't it?

MR. HOATSON: Yes, that typewriter for the view graphs doesn't have a small "m".

DR. SHEWMON: I see.

MR. HOATSON: It's got a small capital "m".

DR. SHEWMON: Only 10^6 differences.

AUDIENCE: Should have been a large capital "J"? wan't it?

MR. HOATSON: These are the commonly accepted flamability limits. The upward propogation is about 4%. Horizontal 6 and downward 9. Upward propogation tends to go up in globules with zones of unburned hydrogen between the globules.

Downward propogation is pretty close to that 8% we were looking at in the last curve and they're probably related.

This is the familiary in Shapiro and Moffet

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triangular diagram. Some of the properties of this -- some of these are difficult to read in textbooks. To read percent hydrogen, that's any line here going from zero up to 100.

Percent air is any line this way. Percent steam is any line that way.

So along this line here, we have mixtures of hydrogen and air. This curve here is the lower flamability limit for hydrogen and air. It runs about 4% here and about 26% air here. Which is equivalent to about 5% oxygen.

The interesting thing about it is that as you add steam to that mixture, the part of your hydrogen stays about the same, and it's the same with oxygen, so that the flamability range doesn't change as you add steam to a mixture of hydrogen and air -- until you get up to about 58%, and then you'll inert it.

The detonation limits have a similar shape, 18%, and 42%, air.

This line here represents a higher temperature and pressure. System 300F and 100psiJ, and it gives you an idea of how the temperature and pressure affect the final ability limits.

These are speed of combustion of hydrogen in air. Lamanor flames are very slow and they lead to causing static loads of containment.

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Even turbular claims are fairly low. 3 meters second and again lead to causey static loads. Accelerated turbulent flames can get up to 200 meters per second and you begin seeing shock waves with these.

And detonations get up to the -- what's known as a chuckman tregay speed of 2000 meters per second. You get a strong impulse loading, plus a strong causey static load.

An area of interest is triggering these into these. It will be done with large ignition sources which might come from a pump motor case and which ignites a smaller volume and then it rushes out into a larger volume. It may trigger a turbulent flame into an accellerated turbulent and give you a shock wave.

Also structure can change a turbulent flame as it flows through and it meets structure in the containment. It may trigger the transition to an accelerated turbulent flame and give you a shock wave.

This is a curve of elastic response of structures to impulse loads, and basically what it says is that at -- below this point here you can go to very high pressures without feeling this structure. The failures are over on this side of the curve. Survival of the structure is on this side.

You can get very high detonation or shock wave

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pressures here as long as the impulse which the integral of the pressure time curve is fairly low.

On the other hand, out here are -- this is the cross static loading area and the container would fail by essentially overpressure on your static load.

Much of the hydrogen area looks like it falls in this area so that we think some of these turbulent -- accelerated turbulent loads have to be settled. Just how large are they and where do they fall on that curve?

DR. SHEWMON: If you're going to say anything about your chemistry program, you better move faster.

MR. HOATSON: All right. I would like to say something about mitigation status because some of these look like they've got a lot of potential.

Talon doesn't. It's costly and it's got corrosion problems. Deliberate ignition. This looks good, but there may be -- the human factors problems on who turns the switch to light it off.

And you need some reliable analyses -- you've got to be able to rely on your analyses to do this, and you've got to have reliable ignition.

Water fog looks very promising. Temperature and pressurizer are greatly reduced. Detonation is inhibited. It raises the lower flamability limit, and only about .05% by volume of water fog and containment is required.

This seems to offer a lot of possibilities. This gives you an idea of what it might do. The top line -- there is no water droplets and here is .05%, which is about 1000 cubic foot of water scattered in containment. And the temperature drop is significant.

And this -- the same thing for pressure. Again for only .05%, the pressure is reduced quite a bit.

Budget for the hydrogen program is all in the supplemental request right now. We don't have any further funds after the funds available through the present compenium work. We have request for \$400K in the supplement and \$600K in '81, plus we have some funds in the chemistry program for radiolysis work which is associated with hydrogen.

Post accident -- in chemistry is 3 parts. The radiolysis work from the hydrogen problem which I earlier discussed.

We're looking at fission products signatures from failed fuel, and also we would like to look at iodine in containment to reduce iodine risk.

The objective of the fission products signature work is to determine if characteristic isotopes signatures result from increasingly severe fuel failure.

Can we draw samples of water during an accident that determine different kinds of fuel failures that might be occuring.

There's a lot of feasibility questions to it. We don't know whether we can do it yet, but we're looking into it.

DR. SHEWMON: Where in the post TMI senerio do we get to where we can take out a sample after an accident without burning up a person everytime we do it?

MR. HOATSON: Well, we -- there's two aspects to that. One is the radiation leve of the sample itself, and the other aspect is drawing a sample in an area that may be higher than the radiation level than it normally is. A laboratory sampling area of some sort.

We're planning to do some sampling and analysis work on the hydrogen program, and I hope we'll be able to take a look at that problem.

But we were only going to be looking at the hydrogen in the things and not all the sampling in the --

DR. SHEWMON: You mean that's a question more for the DOR people than --

DR. OKRENT: Yes. It's not a research problem. It's a plant design.

DR. SHEWMON: I think everybody was disappointed at the exposures they got, but I thought it was more from the sample.

Okay. Go ahead. It's not a research problem.

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MR. HOATSON: I -- would like you to approve the ability to predict post accident containment atmosphere iodine. This is derived from the differences in iodine behavior in TMI, and the predicted iodine behavior in WASH 1400.

And we'd like to start off by identifying which of the iodine factors are most important in reducing the uncertainty? Is it the release fuel, transport condition, water state, two phase, condensation of operation scrubbing, or is it iodine behavior during transport, temperature pressure, chemical form, ph, oxidation reduction potential, impurities, absorption, equilibrium distribution.

Chemical form appears to be an areas that we probably will be looking at. It's confusing to say the least, at the moment.

And the last one is the budget for this work. All of these in the supplement for '80 and in the base budget for '81.

DR. SHEWMON: Thank you.

DR. OKRENT: I have one question.

I would have assumed that the interest in aspects of the hydrogen question, not the corrosion one, but the latter things you were talking about, was sufficiently high that its funding didn't depend on any supplement.

MR. HOATSON: That's where it is.

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DR. OKRENT: I must say I don't understand who's leading the show.

DR. SHEWMON: Go ahead.

MR. JOHNSTON: We took money from other funds to fund the hydrogen book which was $100 - $200 K that Dom mentioned that Sandia is putting together for us.

The other point was that the supplement is supposed to be 100% guaranteed, and it disappears slowly month by month. I mean you think you've got it, and we tell people to start working, and it's getting more and more nebulous.

But if we'd known this in the beginning, I agree with you. We would have done what you suggested. But we wouldn't do it if it weren't necessary.

MR. HOATSON: We have the contractor in a very awkward position right now. He's getting together a pretty good team, and --

DR. OKRENT: I sympathize with him, but I sympathize more, let's say, with those who are going to be scrambling for information.

MR. HOATSON: I hope the compendium is going to provide him at least what information we can find in the literature now. But's it's --

There's a lot of work to be done.

DR. SHEWMON: Now, the handbook -- hydrogen handbook and data base is down here for $500 in supplement, 2'29

881.

DR. SHEWMON: I hope most of that is data. It sounds like a darn expensive handbook. U.C.L.A. could do it for less, I'm sure.

MR. HOATSON: That includes all of the hydrogen program.

DR. SHEWMON: Okay.

DR. OKRENT: Oh, we would want the full amount.

MR. HOATSON: Would you like a promise of a supplement?

DR. SHEWMON: Okay, thank you.

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MR. JOHNSTON: I thought, by way of summary, is to try to reiterate the theme that I talked about in the beginning, and that is that we felt that we have covered a good bit of the things that we set out to do before TMI, and that we're not re-evaluating the program and repriori-tizing it. And we indicated to you earlier the directions that we think are appropriate for us to go. We've suggested the priorities, starting with the core melt -- starting with the severe damage, starting from the point of the loca, and going on from there, as being the high priority area, together with fission products and the clad ballooning as being the top three areas as far as priority, and two of those three need work.

CHAIRMAN SHEWMON: Would you state those again then?

MR. JOHNSTON: The first one on your page, which is the core damage beyond the loca. And then the second one is the ballooning, which is existing. Then the third one is the fission product released in transport. There are a number of new programs in that one, as well as the few existing one. And then --

CHAIRMAN SHEWMON: So you've got both your sections headings and the items within sections, are in severe priority.

MR. JOHNSTON: Prioritized. Approximately so.

The bottom ones, on a given section, are all about equal in priority. But clearly the top two or three or four in a given section are our priority items. I really think that's probably all the time I should take, and that's to indicate that's where our thinking is. We're interested in your responses to it.

CHAIRMAN SHEWMON: Okay, let's stop and talk for a minute on how we get our own prioritization fixed. Now, we have to have something out in the July meeting. Is that right, Tom?

MR. MURLEY: Yes, sir.

CHAIRMAN SHEWMON: And I guess -- well we talk about it at the June meeting?

MR. MURLEY: Yes, sir.

CHAIRMAN SHEWMON: Okay, do you want to do any discussion of that at this meeting, or go on -- I guess the class 9 meeting will have before them the August PBF meeting, we will not.

DR. OKRENT: I'd like to make a couple of comments. I have asked several questions during the day that -- for example, might be interpreted as suggesting that I think we shouldn't do experiments on -- oh, degree formation, or so forth, or a range of things like this. If that interpretation is put to my questions, it's wrong. I do think it's very hard to do experiments of that sort which end up being worth the effort and the money. I think it's easy to do experiments with just the hard work, but it's much harder to do experiments that are worth the money. And this is my concern.

I think if you look at the PBF program so far, which has involved what I'll call easier experiments in general, a considerable number have been off the mark for one reason or another. Experiments are just not easy to do. And experiments you're now talking about are still harder to do even if you've thought it all through.

So there's a lot of money that one's talking about here, and I'm not interested myself in seeing this money spent here, unless we practically have a fair expectation of getting really useful information.

The same goes for the -- what you call the loca experiments. In fact, as you know, I've had less enthusiasm for those, because I haven't seen a real case made that that information we need, and if we get it, it's what Paul called a critical experiment, or something. I haven't seen that case made. I'd like to see the case made.

Now, I acknowledge a couple of areas where I think the problem's been defined. You've done a real job, and it's been a useful technical contribution. But I'm not really fully satisfied in many of the areas that -- and it's not intended to be a slur at the people doing the job.

I think these are very hard to do. I've tried to see this same kind of thing done in area for a couple of decades, and I have an appreciation for how hard it is to do. So you should understand the background from which I'm making comments and introducing questions, and I'm going to continue to be skeptical with that viewpoint. Okay?

So, in other words, I'm willing to give st[illegible] support to an experiment that I'm convinced is likely -- not guaranteed, but likely -- to be meaningful. But if it's just an experiment in the area, is't a scoping experiment, or whatever, I'm not sure that that's the best way to spend the money now, because there's some places I've indicated where I think we're out of balance in here.

CHAIMRAN SHEWMON: Let me bring up one large particular item in this regard. I sort of did a double-take when somebody -- well, when you look in the book and there's the order of $3 million a year down for operational transients, which is, as I understand from this, is for PCI studies. And I guess I would be interested in taking a page out of Dr. Okrent's book at that point and saying, yes, for lab experiments and analysis, yes; but do we really want to spend $10 million trying to figure out PCI limits? Is it worth that much to us? Then getting back, if you could scope things, why can't you encourage the industry to look some at this. And they really bear much of the brunt of that with fuel increased fuel lifetime, or downtime, or something.

MR. JOHNSTON: Would you like me -- just to make a couple of comments. I think in regard to the operational transients, that -- it's not the operational transients during normal operation, load follow type transients, which industry is normally concerned about. What we've defined these things, as the ATWS type transients that are being done and being evaluated in industry as part of the ATWS type thing. So they are transients power excursion, like beyond the normal limits that you would expect, but they're in a class 3, I guess, and maybe class 2 categories that ANS and so forth are used.

CHAIRMAN SHEWMON: Let me come back to my notes here. I've got it under Pick's comment. He was talking about PIC program, went through several things here. And the last item I think before Rick Sherry started was PBF operational transients, $3 million without operating expenses. So --

MR. JOHNSON: That's correct.

CHAIRMAN SHEWMON: But operational transients is primarily connected with a better basis for PCI, or not?

MR. JOHNSTON: No, it's a better basis for the, how does the fuel fail? If a fuel, particularly one with some high burnup in it, undergoes a steamline break in a BWR, for example, which is a calculated power increase momentarily there accompanying the pressure increase, because the voids collapse; you get a power increase which raises fuel power levels and temperatures. There are several others that have been identified. In fact, I can probably get the PBF people here that are sitting in the room to help me out a little bit. But the point is, these are the transients that have to be analyzed from a licensing point of view. From just an operational, or from a systematic point of view, the boundaries have been pretty well defined. They calculate the pressures, and the temperatures, and so forth that will be reached. But what's not known is how much clad damage accompanies that little power rise. It's looking at that kind of thing in PBF that industry can't do. We won't let them do it in a commercial reactor.

CHAIRMAN SHEWMON: No, that's a broader scope. I misunderstood then what we had in mind.

MR. JOHNSTON: I'd like to comment on Dr. Okrent's things for a moment too. We agree with him with regards to many of these experiments. But the big difficulties that we have in conceptualizing some of them is the fact that many of the things we're talking about now seem to have an axial length effect in them. For example, in the case of TMI, it takes maybe five 5-foot lengths to develop the kinds of temperature gradients, such that you have water in one end of the thing, and high temperature fuel at the other end as it boils down. But it takes a number of feet to develop those kinds of gadients and steam conditions that apparently operate.

It's very difficult to simulate that in, say, a three-foot core and determine whether you can really see the effects that you're looking for in that part of the experiment. And I know the PBF people are aware of this kind of a problem too. We're also concerned in the simulation sense that we have to heat these things up with a little bit of reactor power to warm them up. The kinds of temperature gradients and so forth radially in the fuel make a fair amount of difference in the predictions that you're going to have of the way the clad damage gets damaged, and so forth. If you have to use a lot of power to heat it up, you have the usual steep temperature grading; whereas, in reality, it's really the cladding that's driving the temperature because of the oxydation rather than the fuel providing the driving force, once you get up to interesting temperatures.

How can we learn about that aspect of it, because we're not interested in driving the result. We're trying to get the experiment to tell us what it is it wants to do. So we get into some problems of our small size and short lengths, which leads us to look into other places sometimes which are not as well-equipt to do other aspects of it.

Most of this stuff boils down to being a compromise. There are things we don't like about particular experiments, but we can't find alternatives that are better, so we do it, because the feeling is that we need something in the area. But it's an ongoing problem, and I don't think we've ever tried to say that we felt we could solve everything by running some of these tests. But we're just trying to get some feeling about what's going on. I guess that's what I can say on it. I think we're not in disagreement over that.

CHAIRMAN SHEWMON: Carson, do you have --

MR. MARK: There was another point, which I don't want to make an issue of here now. There certainly is a need to sort experiments as between the things which -- for which the NRC is responsible and can make good use of, and things of which it can't necessarily make much use, or could perfectly well be done by someone else. And Dave has made that, I think, several times, though he didn't refer to it again specifically a few minutes ago. And I'm wondering, for my own taste at least, where the degraded performance of filters falls in that kind of a spectrum. You don't really want to understand, nor make any use of understanding, how bad filters can be. It's not a terribly interesting subject, and you know that they can be very bad. And it's really up to the base sellers to say the filter has got to be of such a kind, which we know you can get, and maintained so, that its efficiency doesn't fall below this. And in that case, it's not really terribly interesting to understand how poor it can become with one or another mishandling; or if it is interesting, it's not necessarily for NRC research.

There are things which fall in there where, if it were a comparison between what are the physical range of what can happen, where the hydrogen problem is a little more of that kind, and you do need to understand it, and you can't trust anybody else to bring you the information because he doesn't have it; that would be sort of really in the clear, work deserving attention. The other must surely be somewhere closer to some boundary, and one could sort research projects on that boundary as well.

But I don't want to make a case.

MR. JOHNSTON: Well, it's true. I think Rick tried to give you some of the background. That's a program that we inherited from a different part of our organization. It's one that our licensing people have been asking to have done. But we didn't initiate it. The work in the past with the Naval Research Lab had been, indeed, looking at the degradation of filters under normal operation, if you like, normal exposure to air. Now apparently what it is that we're asked to do is to look into the degrading of these things under steam conditions and more severe conditions. I don't know whether industry can do it or not. I guess the fact -- the real truth is, we didn't look into that. Basically, licensing wanted some information in their own pocket, and they asked us to get it, and it's fairly low-cost. So I guess we -- our management agreed to di it, and it was assigned to this branch. But it is going beyond the normal situation apparently, looking into the effect of these more extreme conditions.

CHAIRMAN SHEWMON: Okay, why don't we take a ten-minute break?

(Whereupon, the proceedings were recessed at 3:55 p.m. for a 10-minute break.)

MR. MEYER: I'm Ralph Meyer, and I'm section leader of the reactor fuel section in NRR. And we were asked to talk about three subjects today. One was out technical assistance work. Another was to discuss some recent fuel failures in operating reactors. And a third subject had to do with cladding interaction, the PCI topic.

We have earlier written a report to this group, and I forgot to get the reference from Dr. Shewmon. But Paul Banard has it. I'm sure he'll get it for you. That part of the proc[illegible] has been cancelled. Mike Tokar, who wrote that repo[illegible] and was to present a PCI talk at the end of the day s[illegible] that we can finish.

Before I begin talking -- I'll talk about the technical assistance, and Dean Houston here will talk about the recent failure experience. And we'll try and do that in short order.

Before I start into technical assistance, there are several miscellaneous topics that I simply want to mention to the subcommittee, not necessarily discuss. I wanted to point out first of all that reorganization that went into effect yesterday has had two effects on the fuel section in the core performance branch. One is that we have -- all of the work that was done in DOR on the fuel aspects of reloads and operating reactor problems, we have inherited none of the people from DOR who worked on that, and we've lost two people from the fuel section. So our fuel effort is going to be rather small for the foreseeable future. And that is bound to have some effect on our communications with the subcommittee.

There are a number of other topics here that I know the subcommittee has an interest in. The second topic, the reactivity initiated accidents, the RIA's, we've talked about off and on during the day. Recently Howie Richings in the core performance branch prepared a memorandum describing some calculations that were done for us by Brookhaven that showed, in fact, for boiling water reactors, that the antholpe that you can deposit in a fuel rod during the rod drop accident is quite small. And it appears that on the basis of the energy that you can insert in a reactivity accident, that we can probably convince ourselves that even if we were to repair what we believe are the nonconservative current fuel damage criteria, that they would not be challenged by the rod drop accident in the BWR, or rod rejection accident in the PWR. And we're going to prepare a recommendation that would, I believe, change our priority on this, where we can probably set it aside as a low priority item.

NOw, we've spoken of that almost as if it's been done. And in fact, it's just a gleam in our eye at this point. But that's probably what will develop with the RIA, and we'll discuss this with you in August if we can get on your program, when you're discussing the PBF program.

CHAIRMAN SHEWMON: Ralph, in two-syllable words, do these things, moderator thermohydraulic feedback, mean that -- as opposed to only hydraulic? That hydraulic has the water going out, and the thermaohydraulic is warmer, so there's less moderation? Or in little words tell me what they did.

MR. MEYER: I can tell you in a word what it is. When you put some energy in, you generate some voids and you get some negative reactivity. And so you reduce the worth of the thing that's trying to put the energy in. And through that feedback effect, they can't get very much energy in by dropping a rod in a boiler.

CHAIRMAN SHEWMON: And the voids in this case are actually steam then.

MR. MEYER: That's correct.

CHAIRMAN SHEWMON: Okay. Thank you.

MR. MEYER: The subject of swelling and rupture during a loca has been discussed extensively with the subcommittee. We've been cancelled from your meetings on several recent occasions. There has been, to this point, really nothing more developed on a schedule for implementation for the model revisions. We have issued the NUREG report with the improvements in it that we discussed with you. We will do some additional discussion inhouse with my research friend before we meet with you in June to discuss this subject.

CHAIRMAN SHEWMON: Okay, do we have a date for that? We're reasonably firm on June?

A PARTICIPANT: Yes, it's the third week of June, on my notes.

CHAIRMAN SHEWMON: Okay. And when does the first NRU shock come?

A PARTICIPANT: October, November.

CHAIRMAN SHEWMON: And the last one comes?

MR. MEYER: Okay. Appendix A, to the standard review plan has to do with the analysis or the mechanical response of fuel assembly -- the response of fuel assembly to mechanical loads that arise during the blowdown of a loca, or during an earthquake. We've discussed this with the subcommittee in detail before. The appendix went out for public comment. It was noted inthe Federal Register in February. Public comment period is just now over. We've only got one comment in our hands so far.

I simply wanted to mention that we had made some progress in getting this out. I don't know now in the uncertainties of reorganization, how the balance of this implementation will go in terms of an actual revision to the review plan. I can tell you that we're going ahead with our review according to this proposed plan, because there is nothing else. We had nothing else on the books to describe that review.

And finally, slightly old subject of fuel bundle liftoff in a boiling water reactor that I believe originated down here. The concern for it originated down here. Was first expressed to DOR, and has been batted back and forth between DOR and ourselves for a couple of years. The last November hired Gus Alberthal to work in the mechanical area. He has started on this liftoff problem. The review is going well now. We'll get a report from GE in October, and we've seen preliminary results, it looks like, that the fuel bundles will chatter a little bit, but they won't lift up enough to come out of the socket. That's what it looks like the answer's going to be.

Unfortunately, Alberthal was taken from the section, so I'm not sure how we'll complete the review. But we'll get something from GE later this year.

Let me now, just quickly through the technical assistance tasks. And I'll simply try and give you an idea what we're doing, and if you want to stop and ask a question, that's all right. Here is a list of the individual tasks, and I have one slide per task that I'll go through, mention what it is. On-call assistance in annual report on fuel performance are two tasks that were contracted by the Division of Operating Reactors, and we've inherited those recently. They fit into our work well, so I'll show how that goes.

The total amount budgeted this year for fuels work is $380 K. I included a summary similar to this from last year to show you that that's roughly the same amount of money that we spent last year on technical assistance in the fuels area.

CHAIRMAN SHEWMON: What's S&L?

MR. MEYER: That's the seismic and loca. I'll go through these one by one. We have two technical assistance programs, called fuel performance code applications.

They are different. There are different laboratories, and they're in fact different programs. This one is at Batell, and it is technical assistance to help us in the review of vender fuel performance codes that are used primarily for the initiation of a loca analysis, the stored energy codes, the ones defense are done in.

We initially had included some money for all calculations for B&W code, and a combustion engineering code. We took that out when we got Alberthal on board to help us with those reviews. And so we have funded general consulting to just sort of help prop us up in doing the reviews inhouse, and a small study on extended burnup problems with fuel performance codes. You've expressed an interest in this. The ATWS DOE program that goes under the NASAT initials has also given us some motivation to try and get a leg up on what kind of problems we're going to run into when we try and do licensing calculations at levels higher than we're accustomed to.

DR. OKRENT: What will they do for you for $30K in that area?

MR. MEYER: Well, they're going to look at the material's properties and at the subroutines that have strong burnup tendencies, and try and point out where we're going to run into big uncertainties in code predictions when we get beyond burnups that we've got in our current data base.

A second task, called fuel failure limits, has been focused almost entirely on the pilot planning interaction problem. During fiscal '79 and earlier we had a joint program with Batell Northwest and Canadien group at Chalk River trying to provide us with some empirical models for predicting probabilities for failures. And we did get those models in fis^al '79. As Bill Johnston mentioned this morning, all of our PCI work is going to be transferred over to research in fiscal '81, and that leaves the current year fiscal '80, which is sort of a transition year, during which we're providing a small amount of money for Batell to document the mechanistic concepts that went into the model that they published in the other report.

CHAIMRAN SHEWMON: Is there anyplace I could get a discussion of the pros and cons, hide and stress corrosion cracking versus any other viewpoints of what causes cracking in PCI?

MR. MEYER: Well, I think the report that Phil Pancaskey is preparing under task 1 is such a report. We do have -- we've already reviewed it for publication. And --

CHAIRMAN SHEWMON: I look forward to seeing it then.

MR. MEYER: -- I believe it'll be out in another month or thereabouts. In particular, Batell is going to look closely at the incubation time, the delay time, the controversial old time that some feel is essential to get the PCI failures. And we'll look at that from the data that we do have to see if indeed the data are unambiguous in showing us the incubation time; or if, in fact, the -- what you interpret as an incubation time might be a rate effect.

Now, Pancaskey has used a concept called strain energy absorption to failure, which he discusses in this report, and he'll be doing some more work on that to see if it -- if he can determine that ratio from the data that we have on the failure rate in the data base. And a small amount of unspecified support in case we have some luck in getting profit mile used in licensing analysis. We would expect to have to ask him a couple of questions.

You've seen this one on previous years, radioactive fission gas release analysis. This is the final year. We've underfunded and piddled around with this one two or three years, and we finally have gotten them enough money to finish, and have the steps to finish this laid out. Our objective here is to do enough calculations to provide a basis for the gas release assumptions that are made in three regulatory guides that are currently used: one dealing with the local, one dealing with the rod ejection accident, and one dealing with the fuel handling accident. And so the calculations will be made of the steady state gap inventory, and then some estimates of the additional release component for a loca transient, for an RIN transient.

Our ultimate use of this would be to try and revise the regulatory guides. Now, this is a DOR program called fuel operational performance. Originally they simply called it oncall assistance, and didn't specify what it was going to be. And then as problems came up, they had them -- they sent them out to Batell, and the problems that have come up so far are, one in connection with Zion extended burnup program. They performed a calculation to look at crud buildup and additional temperature rise across an extra layer of crud going to high burnup. They found that that wasn't very important.

There have been some recent mixed oxide rods put in Genet, and so they did a couple of more calculations with gathcon to look at the average temperatures.

CHAIRMAN SHEWMON: Can I ask that you go faster?

MR. MEYER: Sure. Well, let me just -- I think I don't have to -- DOR has funded Batell to help them do some statistics on fuel failures and to evaluate fuel failures for the purpose of preparing a report. We prepared one report but did not have statistical analysis in it. And we would plan to include that kind of analysis in future versions of the report.

Okay, here's the second fuel performance code application program. This is at Idaho. It's quite different from the first one. Here -- I do want to comment on this one, because in one respect it's the most interesting of the lot. This is our attempt to get a modern symbol code to do loca calculations. This is a modern day 2D replacement, if you want. We're going to take Frap T5, and take the bells and whistles off that we don't need to do the loca analysis, and pay Idaho to run it through something like a licensing review, strip it down, put in some of our favorite assumptions and models in, and end up with a code that we can use inhouse to do the kind of calculations that we'd attempted to do on the swelling and rupture thing a couple of months ago.

So here's a case where we're making a very serious effort to use one of research developed codes, but to simplify it a little bit before we do that.

At Idaho we have some assistance in reviewing topical reports on the seismic and loca mechanical response analysis. That needed a little bit of extra line to finish it, and we've given them some unspecified time to help us respond to comments on the standard review plan appendix, to help us see through this BWR liftoff problem, and other things related to the mechanical analysis. That's a pretty small program.

In that same group that funded, under a separate program letter, is a post blowdown loads study. This is a small task to calculate loads on fuel assembly components from events tnat might happen after the loca heat up this oxydized cladding. This could be something like a pump switchover that we worried about at Three Mile Island. Or if loca is related to an earthquake, it could be an aftershock. And so we're going to make calculations with the audit code that we use for reviewing vender codes and compare those with embrittlement criteria from work done at Argon that Bill Johnston's people have described to you, and see whether there is any cause for concern.

And finally, the last task is also one that ACRF has expressed concern, and this is fuel failure propagation, and it's being done at Los Alamos. It's a two-year program and the $95K covers it for two years. We just went ahead and funded it initially for the whole amount. It includes a very large thermohydraulic component. So TNB propatation is definately one of the things that's being looked at. And this will provide us with an estimate of whether the failure data around the world today, and what's known about failure mechanisms, would indicate any likelihood of provocation.

So that's all I have.

CHAIRMAN SHEWMON: What do those last words mean? Whether the failure propagation data around the world, or failure data would suggest any propagation?

MR. MEYER: I'll have to find those on that slide. This task is not generating any new data. We've got a contractor that has some experience with failure mechanisms from both the mechanical kinds of causes that fuels people are aware of, and the DNB causes. And all I meant to say was that they're going to search the literature and use their experience to see if it's a real worry or not.

DR. OKRENT: What was it that you think the ACRS expressed an interest in?

MR. MEYER: We've had some long discussions about failure propagation here for a year or more now. And whether by failure propagation we meant fission gas impingement on adjacent rods, or molten fuel materials squirting out and plugging up channels so that adjacent rods didn't get properly cooled; or whether, in fact, just a departure from nuclear boiling on one rod would affect adjacent rods. And it was -- as best as I can recall, it was a conclusion of that meeting that we hadn't demonstrated satisfactorily that propagation could be ruled out. And yet we weren't doing anything about failure propagation in the licensing analysis. So --

CHAIRMAN SHEWMON: Must have been a meeting you were in.

DR. OKRENT: I'm not clear what kind of data you think there is around the world that would be useful in answering whatever you think the question is.

MR. MEYER: Mike Tokar is the expert in this area, this program. And we cancelled him for this afternoon's talk. I'm sorry he's not here.

CHAIRMAN SHEWMON: Why don't we wait for the report. My impression is it's a nonproblem, or at least it's one that's been around for a very long time. Nobody's every been able to prove it's not true. And we never will prove something until we see fuel propagation, I would guess, your past reviewer.

DR. OKRENT: I just don't understand what they're going to do by looking at data around the world in regard to the question -- if it's in response to something that they think the ACRS raised. And I suggest you might try to generate some kind of amplified definition of this task over -- it may exist. At least, I'd be interested in seeing an amplified definition to see if, in fact, it does resemble what I think of the areas that the ACRS in the past has expressed interest in.

MR. MEYER: Would you like us to prepare a brief memo to you on that?

DR. OKRENT: If that's convenient.

MR. MEYER: I'm quite sure that if Tokar were here now he could give you the answer.

DR. OKRENT: Fine.

MR. MEYER: Dean Houston now will describe recent fuel failures.

MR. HOUSTON: How much time do we have here? I'm Dean Houston, formerly with the fuel section, and now with the division of licensing. I'll cut this as short as I can, I guess, and we'll just see how long it really runs. I have -- in the handout I have essentially listed the general areas of fuel failures, and included associated core components. I would plan to only discuss just the area of fuel failures, but am prepared to make any comments about the other items if you have any desire.

First here we have a table showing the 1979, as close as we can in 1979, annual operating statistics. Failure here is defined as fuel rods leaking, or structural damage to an assembly component. None of the figures are derived from coolant activity levels. We have 70 different reactors licensed; failed assemblies listed here, the fuel assemblies in those reactors listed here, if you disregard the Three Mile Island, two assemblies which we have estimated here as 150 being failed, you see 116 here containing some kind of failure. Typically these will have two to three rods per assembly that are actually leaking.

What this comes out as in a rod failure percentage in a population of about two and a quarter million fuel rods, you have a rod failure percentage of .015.

Now, in this same population we do have three reactors where the rod failure in a given cycle is something on the order of .2 of a percent, up to .3 of a percent. So there is some sort of a range represented there.

CHAIRMAN SHEWMON: What was your lower limit?

MR. HOUSTON: Well, it's an average for the overall population. It's .015.

CHAIRMAN SHEWMON: Okay.

MR. HOUSTON: And then there are those three ractors in the range of .2 to .3.

Now, next I've put up a slide that mechanism for failure, with the plants in which the failures have

Tape 4

occurred. In some cases the mode of failure is well known, but the exact reason for its occurrence is still unknown, even after extensive investigations. We'll skip TMI 2. We see here that there are two cases of water site corrosion. We always have water site corrosion, but in these cases there's excessive corrosion leading to cladding failure.

First in the PWR's, in the Maine Yankee case, coolant contamination occurred following a changeout of a resin bed in the purification system. I should remark here too that there's been a similar incident where air in-leakage in a purification system occurred at Calvert Cliffs, but no failures resulted. However, there was a heavy corrosion deposit, caused an increased pressure drop across the core, and shifted the peak and the power distribution to the bottom of the core instead of toward the top. They have performed the crud burst procedure, and they're back -- the pressure drop has gone back to normal, and they've been back at 100 percent power for about a month with no noted failure.

In the Maine Yankee case this same type of incident led to a unique crud deposit between the sixth and seven.. spacer grids, and failures there occurred by two assemblies they've identified from corrosion itself. There are five assemblies here that they say are possible PCI's, and I suspect that's because perhaps the power shifted to the bottom of the core. And there's one under the unknown category. They have no real handle on the mechanism.

CHAIRMAN SHEWMON: If we look at those in a different way, which of them, besides the Lacross--and let's scratch the TMI 2, which is a different kind of event--led to enough corrosive activity so that you started giving expect questions, or even increases in primary system activity.

MR. HOUSTON: The only two that I'm really aware of are the Conn-Yankee ones and Lacross where both populations of failures led to an increase -- they were riding about 10 percent of the tech-spec limit. Now, Vermont

Yankee may have had some difficulties here because they're in about the same percentage. About .3 of the core would be represented by leakers, and only in those three cases were there anything above -- anything exceeding 1 percent of the tech-spec limit.

Now, at Vermont Yankee the failures were completely different. They were confined to one reload batch, and only in zircoloid cladding from three or four of the cladding batches. They're typically something like 50 or 55 cladding batches represented in the core at the time. The corrosion product was highly localized in those particular clad batches. Extensive PIE and archive examination, both nondestructive and destructive, has not pinpointed a reason that these cladding batches should be susceptible. There are no other known failures of this particular type, but it did lead to 30 assemblies having two or three failed rods per assembly.

The next one is the stress corrosion cracking. In Conn Yankee, this is in 304 SS, occurred also in just one particular batch of fuel. Here we have sort of a case, the fuel cans were made by Gulf United. The pellets were made to specification by British Nuclear Fuel, and the final fuel rod and assemblies were put together by Babcock and Wilcox. The reason for the stress at end of life burnup was about 33 and a half thousand is not yet specified.

We go on to the -- well, we'll skip the Lacross.

The Lacross is just a carryover from previous PCI problems, and it's listed here mainly because 17 assemblies that were discharged were discharged in the year 1979.

CHAIRMAN SHEWMON: There was a reasonably strict burnup limit put on Lacross when they went back up this last time.

MR. HOUSTON: Right.

CHAIRMAN SHEWMON: How did --

MR. HOUSTON: To 15,000, I believe.

CHAIRMAN SHEWMON: How has performance compared with that? Do you know?

MR. HOUSTON: They have gone through one reactor cycle. They have asked for an extension of the limit to, I believe, another 300 megawatts, something like 15 3, or 15 6. In the sixth operating cycle they had no leakage after they had these 17 removed.

The next case, we have refueling handling that resulted in 11 failed assemblies. Nine of these were at Salem 1. Failure occurred by grit strap damage, and those with strap width pieces missing were not reinserted and considered as failed. Those with minor chinks, or a tab missing, or something like that, were considered reusable in the next cycle, although they did suffer that minor damage, and there were 23 of those. At Maine Yankee there was one assembly twisted, and at Crystal River, there was some kind of an object fell on assembly and did damage to the hold-down springs.

Now, when you go into the unknown category, this is a catchall for leakers with no apparent mechanism. We should have shown -- this is 4, and you could add Trojan to this list, since they called in yesterday and said they had observed one rod that was split open, and it would fall in that same category. The same types of failures have been shown in Fort Calhoun and Rancho Seco on fuel that has been removed, discharged into the pool, and at some time in the examination they have seen only one rod with one failure.

The seven at Brunswick, which would be the seven BWR's here, were first put in a probable PCI category. Since then the full core has been sipped, and the leakers are mostly in old 7 by 7 fuel which, in the previous years, has had a poor performance record. The location of the leakers in the core is not associated with the PCI kind of event. There was a faulty control rod in double notched when they were doing control rod maneuvers. And in previous instances where PCI has been the problem, the leaker fuel has been nicely grouped around the control rod, which gave them the power event. In this case, the old 7 by 7's that are leaking are really not around the control rods. They're scattered throughout the other three quadrants of the reactor. It may be that the individual rod, the control rod problem, has only given rise to the simultaneous release from failures that were already there.

CHAIRMAN SHEWMON: Why don't you move on, hit on high points, or things you think are particularly general.

MR. HOUSTON: Okay, that pretty well takes care of this anyhow. There's PCI. We've talked about that. The vibration treading for Yankee Row is in stainless. There's no apparent reason for that. It's not water-baffled because the baffle there is one piece welded with no joints.

Next, I'd summarize just the common things under one title, stress corrosion cracking. And this is the only one where there has been a lot of failures or potential failures. The two are in fuel, we've talked about Conn Yankee and Lacross. The other ones are in associated core component parts, the Westinghouse upper guide tube pins, which are of incinel; the control rodlet fingers, which are 304 stainless; and the GE control rod cladding. I might point out that in the control rod cladding, the General Electric control rod cladding, they have backed off from what they have considered 100 percent design limit before, to an 80 percent design limit. This doesn't eliminate all of the cracked control rods, but it does eliminate most of them before C washout.

CHAIRMAN SHEWMON: In the middle one there, I guess there is this Japanese reactor and the Westinghouse people are now saying that it couldn't possibly cause any harm if they did break, except in ice condensor -- the ice --

MR. HOUSTON: In the upper head injection plants.

CHAIRMAN SHEWMON: Right. Is that it?

MR. HOUSTON: Right.

CHAIRMAN SHEWMON: Is it your impression that their track record is as good on that as sat as that of Vise? Or have you ever bumped into that in this country? Or is it just one mis-heat-treated batch, or what?

MR. HOUSTON: The problem came up in a foreign reactor with foreign made material, which were made by a different process than Westinghouse makes theirs. Only the foreign made pins broke in that reactor. The Westinghouse pins both foreign made -- or the Westinghouse pins, both in foreign and domestic reg reactors have never shown a failure. However, then, to follow that up, in the same foreign country there was an incidence in a UHI plant where four flaws were found in Westinghouse made -- in a Westinghouse made pin. And so because of the four flaws that were found, Westinghouse has gone back and decided to heat treat all of their incinel at a higher temperature. There's a lot of --

CHAIRMAN SHEWMON: The flaws were stress corrosion cracks?

MR. HOUSTON: It's in a different position than the first failures were noted. It's down in the tongue, or the extension part of the pin, rather than up in the shank. And it hadn't led to failure. But because the flaws were there, Westinghouse decided to go back and heat treat all their incinel at a higher temperature, and then replace all of that in the UHI plant.

CHAIRMAN SHEWMON: And the higher solution to neotemperature would presumably protect things from stress corrosion cracks. Is that it?

MR. HOUSTON: Right. Although they do have a lot of the lulaneal material operating here domestically, and have never seen one of these come apart. And then, just very briefly, here are those single batches that have either had failures or had shown operating anomolies. We've talked about Vermont Yankee and Conn Yankee as failures. There was one batch in Prairie Island 1, the force reload. The entire batch showed excessive rod bowed end of life. No reason given, was the only batch that they've seen this type of behavior.

Surry Unit 2, batch #7 was sabotaged, where workers poured sodium hydroxide on it. Those particular assemblies were taken back. All of the spacer grids except the bottom and the top one, and all of the guide thimbles were replaced in the rebuilt assemblies.

CHAIRMAN SHEWMON: We devote a reasonable amount of time by spells to trying to see that we don't ever get DNB. I guess where we worry about that is in transients. Is that right? And therefore, we're so far away from that with regard to normal operations that you never expect to see it anyway?

MR. JOHNSTON: Or in the misloading of the fuel, which Dr. Okrent mentioned this morning. I think improper enrichment. Where the assemblies unload, you can get DNB and supposedly normal operation.

CHAIRMAN SHEWMON: Okay. But in the -- so many reactor years we have, we've never seen an example you would blame on that. Is that right? Or failure you would blame on that? Or can you say?

MR. HOUSTON: In DNB?

CHAIRMAN SHEWMON: Yes.

MR. HOUSTON: I don't believe we've ever seen anything of that nature.

CHAIRMAN SHEWMON: What would you look for if you did have it? Or what do you think would show itself?

MR. HOUSTON: If you look at the PBF fuel you see a lot of discoloration, crud buildup, even a wasting. I believe you would see those kinds of things if you really had DNB.

CHAIRMAN SHEWMON: Okay.

MR. HOUSTON: And then, finally, I only have this one last slide on generic items that come under this category. Guide tube wear. I don't believe we've seen any new assemblies, new failures in assemblies from guide tube wear. Every PWR vender has a model, has some examination results from their particular assemblies under control rods. And CE has pretty well settled on the chrome-plated stainless steel sleeve to overcome the guide tube problems that they had. The BWR control rod lifetime, which we talked about previously. The BWR water rod wear. This is a matter that they extended the tip on the water rod, and it goes down into a turbulent flow area in the lower tie plate. They did that on a 8 by 8 R assemblies. All 8 by 8 assemblies had a shorter tip and had no wear, so the solution to the problem right at the moment is to cut the tips back to a shorter length.

There may be a problem later on if they go to extremely high burnup, and need the extra bit of the tip to allow differential growth, zircoloid growth, to follow. And the Westinghouse, baffle jetting. This was a problem that was handled in about '75. They thought it was pretty well identified on heat driven joints or sections in the baffles. And because of the flow of the other joints was different, they felt that there was no problem there. So they planed the 8 joints, and then this year in another foreign reactor they saw baffle jetting at one of those other joints. I think there's about 12 or 14, 15 other joints. So what Westinghouse is doing now is going in and cleaning all of the joints in the baffle, both of the original eight locations, and then the following 14 or 16.

And that summarizes where we stand for the given year on fuel failures.

CHAIRMAN SHEWMON: Right on time. I thank you very much. Are there any questions? Okay. Looks like we're in fair shape then. Thank you very much. Meeting adjourned.

(The proceedings were adjourned at 5:05 p.m.)

Now, this is a printout from the TMI boil code, this does allow radiative heat loss to the steam, it allows heat transfer from the steam back to the rod, it allows for the variation of specific heats with temperature, the variation of steam properties with pressure and temperature. It allows for just about almost anything you could want in thing in a fairly sensible fashion --

All right.

What heat come in with on the same bundle in there, the zero is at the top of the core, one foot, one is the one foot level, two the two foot level and so on. You take a look at this, this plot is almost the same as mine. It's not that much different. So my simplying calculations originally were not that bad. But now we have made these kinds of calculations for many different conditions.

In this particular one, we boil down to 33 minutes to a level of eight feet, held that level at eight feet figuring we had dribble back from the condensers through the cold legs into the core and just held the level constantly. We had no better information to go on.

If we take 20 minutes to go down to eight feet, we change these times by a few minutes. That's all. If we go down to seven feet, we don't get temperatures like 3,600F at three feet. The hottest temperature up there won't even get up to 3,200 if we only boil down to seven feet, now this eight feet.

If we boil down to nine feet, the six foot level up here gets about 3,000.

All right.

Our conclusion that the damage here, the liquified fuel formation down to between three and four and a half is based on these kinds of calculations.

If we boil down to seven feet, we don't get as nearly as much damage that we know happened -- hydrogen, we found activity, anything.

If we boil down to nine feet, we lose far too much. We can -- fall down to about eight feet plus or minus six inches. I don't believe that uncertainly limit myself. I think it's more than that. That's what we draw conclusions from our calculations.

Now, that was at three hours. We have a great deal of evidence that says there was more damaged produced at three hours and 45 minutes and that's what I want to talk about right now, is to characterize the damage -- at four hours.

We have manually read in-core thermal couples that were read between 8:00 and 9:00 o'clock in the morning with -- meters, that indicate temperatures as high as 2,600F indicated by the in-core thermal couples.

If those in-core thermal couples were intact and in existence, that means at 2,600 F temperature was read in the -- in the upper intake.

If that thermal couple was not intact at that point, that temperature had to be down in the bed and that thermal couple had to have been melted and debris formed. You don't have any other choice for the thing.

All right.

There was not just one temperature, there was 12 temperatures above 2,000 F. It took them over an hour to read the 52 thermal couples. So the temperature map I'll show you in just a minute. It took over an hour for them to read and as you go out in the spiral the temperatures get lower for the most part.

All right.

There are -- neutron dectors, seven of them in the instrumentation tubes as in the center of 52 of those assemblies. When those things get above something like 1,000 to 2,000F, they give a signal which causes the plant computer or alarm printer to record them as bad.

In other words, they would be -- have given -- should not have been reading at all. Now, they are reading much too high, they're off scale and the alarm printer --

If we simply take the first time that the Reboven SD&D from level one down at the bottom up to level seven at the top is alarmed as our anchor point for estimating. Then we have 17 SPND's at level one and two were alarmed in about 45 seconds, at 7:45 in the morning.

Now, this means a sudden -- down one foot from the bottom of the core there were temperatures above 1,000F and this is down in water.

All right.

There's only one way you can get that damage -- if you have liquified fuel dropping down in the subchannels just like a lava flow to get down around an instrumentation tube and seal it off from water and then the thing heats up.

So this says that there was more core damage down in the debris bed and below that at that time.

Now, we believe that this liquified fuel that formed in the debris bed sealed that core to level off from steam cooling and form the steam bubble below. This then drove the water levels down further and there was more oxidation and cladding damage as a result of the steam bubble driving the water level lower.

At 7:45 in the morning somehow or another this debris bed and sealing layer was penetrated and there was subsequent steam erruption by water coming in from the downcomer into the bottom of the core and up into that --

There is an 80 PSI pressure increase in the entire primary system when it has more than 6,000 cubic feet of vabor space on it. 80 PSI up as fast as a recorder strip chart can take it up. It was something like less than 10 seconds. So there was that particular -- the thing leveled off at about 100 PSI increase and then it turned around and came right back down again.

So we have the SPND's, we have the pressure pulse, we have a number of other indicates plus the temperatures that indicated that there was considerably more damage done at four hours or three hours and 45 minutes.

Now, at that time we estimate that at four hours more than 60 percent of zircoloid in the core had been embrittled or shattered. That doesn't mean oxidized now. It just says that it has been damaged.

I believe the lower surfaces of the debris bed had dropped to about five feet from the bottom of the core and liquified fuel had penetrated within one foot of the bottom of the core in some areas. We don't know how many but we did have 17 SPND's at the one and two and half foot levels go off scale.

Our calculations indicated that from this amount of zircoloid that between 700 and 820 pounds of hydrogen were produced by four hours. And it may have well been more because later we can't estimate that. We have not way of getting at it. There may have been additional hydrogen produced by the oxidation of the stainless steel in the upper in fittings, stainless steel on the control rods inside, we have no way of estimating that.

If I make some simplifying assumptions I come up with something like 50 pounds of hydrogen. In light of our uncertainties here I ignore the 50 pounds.

All right.

Now, we got -- this is a map of the core. Each of these small blocks is an assembly. Each of these colored squares is where there was an instrumentation tube and an in-core thermal couple reading.

The -- on this one is this is at -- between the hours of 6:55 and 7:15 in the morning, two hours and 55 minutes to three hours and 13 minutes of accident time, these thermal couples were all shown by the alarm printer, the red ones to be above 700F. The purple ones were between 650 and 700 where they showed on the alarm printer as coming back on scale. The alarm printer records the first indication -- the first temperature that it sees after it's come back on scale. So this could have been higher earlier. This is over an 18 minute span. I don't know when the alarm printer got to it.

The blue is at 600 to 650 and so on. But you see all of the red ones, those were all over 700F.

Now, this is the data that was read by the instrument men with no -- meter and converted to temperature. Here's a temperature of 2,453 and 2,451, 2,055, 2,655, 2,402,

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2,242 and so on.

Now, remembering that they started here reading this one first and went out in the spiral like this to read these two last.

Now, this one was that it had -- was -- thermal coupled, it never did read until much later. Why it read much later we don't know.

It took them more than an hour from the time they started here at number one until they got here. So there was time for cooling down of a bunch of these thermal couples and because these temperatures over in here have dropped, that doesn't mean that that wasn't at 8:00 o'clock, a 2,000F thermal couple. We don't know.

All right.

Now, going here on this plot the particular positions in the instrumentation tubes where the -- at level one and two went off scale at 7:45 in about a 30 second time period. There are -- these -- these two -- this one was already off scale. This one was off scale, this one went off scale. This one was already off scale. All the rest of these went off scale in about a 30 second time period.

Now, to show you how we got to most of this, I need to lay a little bit of background. This is the drawing of the reactor primary system. This is steam generator B, steam generator A, the hot legs, the hot leg temperatures were measured right here. These are the cold legs, the cold leg temperatures were measured right here just below the pump.

The make up lines from the make up pumps injected in the middle of that -- there and -- and in this one over here. One reactor pump was left out of this drawing right here. The one A pump so that you can see some of the other factors.

Some things that are very important here is the surge line right here from the pressurizer enters the hot leg at this point. This is about four feet above the center line of this pipe. This pipe incidentally is 36 inches -- these are 28.

The letdown line comes out of this cold leg, one A cold leg on the A steam generator. This is the pressurizer here, the spray -- the PORV, the stuff opens one of these up here and another point that is critical in the interpretation is the pressurizer spray line that runs from here down to just at the outlet of the two A pump. That pressurizer line feeds a spray of water into the top of the pressurizer to cool it down. It lowers system pressure. That's what it's normal purpose is.

At the time the accident was started, that spray line was operating, it was spraying down the pressurizer. They were were trying to decrease boron level.

Now, I think -- I'll be coming back to this in a minute.

Now, another critical point is here on the pressurizer. Your search line comes in at the bottom here. You have a set of heaters, a thermometer -- a resistence thermometer located about one foot above the top most heater -- electrical heater in here.

The critical points are your reference line or your pressurizer level indication comes in at this point up here. The reading leg is down here. Since this normally is in steam, you have steam condensed in here to fill this reference leg up to this level. So that maintains a relatively constant position for reading your pressurizer level.

This level sensor down here now reads the level of the water relative to that point. It reads the pressure level difference and that's what the level indication really is.

The critical point on this is if this leg clashes and the water in this leg is lower than the water in the pressurizer, we read a full pressurizer at all times.

All right.

Now, this is a complicated full plot. I don't know have any other way of trying to handle the massive data that has to be looked at here.

MR. PICKLESIMER: -- on both sides, the hot legs, and the indications are by the temperatures up here on the hot legs, that there was one normal flow, and not reversable in postulate. It's a normal flow.

Water is going up to the top of the hot leg, dripping over into the steam generator and collecting down below. In this case on a recycle, coming back up and drifting back through whole leg, it buckles up.

On the A side where the pump was plumbing it had the -- and what was indeed taken out -- got to be taken out in a let down hind.

The pressurizer was mix phase also. So was the surge line. Didn't have one temperature in here for the surge line at this time which says that it was siphoned on down.

All right. They turned this pump off at 100 minutes into the accident. When that happened this water dropped back in and the steam -- the water separated in here. This one dropped back into the core. This one simply leveled off.

We think then that water at that particular time was right at the top of the core. It may have been there.

A little bit above it or a little bit below it. We can't tell for sure.

DR, SHEWMON: Now, what's the boiling point.

MR, PICKLESIMER: Sir?

DR, SHEWMON: What's the boiling temperature of water at 1100°F?

The boiling point of water at 1100°F and 1100 p.s.i.?

P.S.I., you're right. Pardon me. I mean is 520 above or below it?

MR, PICKLESIMER: I'm sorry. I don't --

AUDIENCE: Look on your saturation curve.

Look on your saturation curve on the --

MR, PICKLESIMER: All right. 1100 p.s.i. is right here. So at this point right in here. And we're boiling at that time, yes.

1100 and 520 should be about the same.

DR. SHEWMON: Fine. Go ahead.

MR. PICKLESIMER: All right. Now, here is a plot of the pressure lozer -- I'm sorry. Of the steam generator level. And of the cold weight temperatures at the time the pump was turned off. The pump was turned off right here.

Now, it has traces for all -- for 2 of the cold weights and both populate under that terminal. The following vest -- we have all four of the hot legs and the cold legs cooling down at the same point within a few degrees of each other, from the time period of abot 4:33 to 5:40 when they turned the pump off.

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So they're al. cooling down to perilite.

Vis-a-vis, a'ter the pump turns off, the hot leg in B took off and went up.. Now, I think -- I can't win the argument, but I think this is when the pole was first uncovered.

This was the first entry of steam into that hot leg. All right. "A" did not do it. -- About 10 minutes later the hot leg, "A", started heating up, and it didn't stop, to look out for "E" under "F".

So you can argue here -- this had to be the point at this point there had to be core uncovered, because you have steam in that hot leg and it just continued to rise internally.

I will argue that we were uncovered 10 minutes earlier.

Now, the -- well, just to mention the core is boiling down. The pressure is dropping. There are flashing -- and that's the minimum pressure here over about 640 or 650 psi, as best we can figure it.

The close the vlock valve, because the pressure had already started to rise, and had risen from 20 or 30 psi, for full block valve was closed.

The -- once the block valve was closed, the pressure started to rise some. Then at this point there was a very

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definite infraction in the strip joint crisis rose much more rapidly and again at this point, there is a very sharp deflection point in the first occur, and it rose very rapidly from about 1400 psi to over 2000 psi, in just a few seconds.

The temperature shows this -- picks up at 1700 psi, and goes on to maximum, at this point about 2050, and this occurred over about 6 second interval.

Now, it leveled off up there, and let's see -- they had close the block valve here and opened it again at this point to start a blow down. The pump was turned on at this point for this deflection point.

We think that the water hit the hot core, pressurized the system and it's a very rapid rise here. This core is with the pump being turned on.

The pressurizer level indication here had already started to rise. It had dropped down to 300 inches and it rose to almost 385 inches. And that 3.4, 3.5 cubic feet of water -- pressurizer level.

And I have a problem in trying to figure out where that water came from. The hot leg was -- had only steam in it. No water in it.

The pressurizer had to have been dropped down to 350 inches here, and I can't figure out where that 250 something pounds of water came from, on a factor of that pressurizer of --

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Now, the hot legs were increasing in temperature here. The green one, which was the "A" hot leg -- No. I think I'm mixed up on them.

The "B" hot leg was the one that that remained hot, the highest in temperature all the time. It was about 800°F. The "A" hot leg was about 700°F to 750°F. Now this data is recorded on a multiple point recorder which prints out every 2.4 minutes. We have a hell of a time trying to follow this through on a multiple point recorder, because the printer was in very bad condition.

But we are able to go back to the original and pull a bunch of these in critical claims out.

The court imagine now has occurred from this time here to 2054 minutes to give you what I told you earlier as the time at -- the Commission at 3 hours.

Then the pumps -- make up pumps had been taken onto a high pressure injection and immediately thottled back. The hot leg -- the pumps had been swapped "A" to "D" and "B" and "C" going off/on. In this time period, we know that the pumps were on, but we also know that they were followed to a lower flow and we don't know what that flow is. Have no way of getting at it.

Now, the --

DR. SHEWMON: Pick --

MR. PICKLESIMER: Yes?

DR. SHEWMON: Let's go onto the core a little bit more if we could. Get to your bottom line.

MR. PICKLESIMER: All right.

The core damage now here at 4 hours, here is the pressure spike I was talking about that indicates that the core was disrupted at 2:54 . That is coincident with the SP&D's going off state.

We also have an SRM jump at that particular time, would indicates that there was something happened in the core.

This SRM, seeks mostly the level in the down core. In the most part. During this time period -- Now that completes the four hour core damage.

During this time period when they were trying to repressurize, they were bleed and feeding, and this is where I think most of the -- this time period here where most of the hydrogen came out.

Then they opened the block value again, and tried to blow the system down and never got below about 420 psi. And the state down in that temperature range, down in that pressure range, below 600 psi for a good many hours, until they finally started up the steam generators. They post blocked off finally and drove the HPI's in -- to drive the system back full.

One of the principal points is between this time

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here and this time right in here -- There's a 142 gallons of water went on the PWST.

Now here is evidence that indicates the pump throwing water into the --

Unless there are questions, I'll quit.

DR. SHEWMON: Okay. I think we better quit then.

What is your wild guess with regard to how hard it's going to be to pull that stuff out of there?

MR. PICKLESIMER: I think that we can go in on the periphery and start pulling core barrel shapers. And work in from the peripheral position outside the actual fuel assemblies themselves.

That's what we're thinking about in 7.2 Committee. That's at least one way. If we have to.

DR. SHEWMON: Those will be firm and then you can peal things off into that space --

MR. PICKLESIMER: Providing that the core barrel hasn't dropped. There is a possible that core barrel has dropped and the whole thing is down and cocked. It's a possibility. We don't know.

It will just simply complicate things.

DR. SHEWMON: I dare say. Okay, thank you very much then.

MR. HOATSON: The hand-out that Paul is passing around right now is quite detailed. It's essentially a

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verbatim account of what I was going to say, so as I skip through these quickly, you won't miss a thing if you read that handout.

I'm going to hit three topics today. These are combustible gas generation and containment, the hydrogen program, and post accident fluent chemistry.

This combustible gas and containment is one of those things that Tom Early was talking about earlier that if LIcensee asks us to do it, we'll do it.

Now this is one of them. We have users aid to investigate the rate of hydrogen production from the sink, galvanized steel particularly zinc primers and orgqanic coatings.

This slide -- the significant thing on this is the amount of zinc in containment. This is from Sana OFRE and it's surprisingly large.

DR. OKRENT: But is it representative of the plants that began construction, let's say, after around 1970 or '72?

MR. HOATSON: As far as I'm aware, only the -- all of the plants have the significant amount of galvanized steel, in cable treadings and galvanized decking and that sort of thing. Quite a bit of zinc and all --

DR. OKRENT: Because they're concerned with this form of hydrogen generation was developed after a SANOFRE

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I -- you're talkinag bout SANOFRE I, I assume? Not II and III?

If you're talking about SANOFRE II and III, then I retract my question. They're pretty new.

MR. HOATSON: I think that was II, but I'm not sure.

DR. OKRENT: Okay.

MR. HOATSON: The program is a rather small one. It's 100 K for this year. We p..an to prepare a program plan for the galvanized zinc and perform scopic tests under a variety of chemical conditions, and a temperature of -- and provide for results upon those, primarily a coorosion testing to determine the rate formation of hydrogen from --

DR. SHEWMON: Do you have any idea how many plants have biosulfate in them?

MR. HOATSON: No, I don't. There are quite a few. Base board biosulfate is used in quite a few.

DR. SHEWMON: So it's not B&W, it's Westinghouse, too?

MR. HOATSON: I'm not sure which. There are a number of plants thaat are using biosulfate.

DR. SHEWMON: The ph range quarters 10, is what you think you can get in mixtures of borated sodium hydroxide solutions, or what?

DR. SHEWMON: Now, most of this will be in contact with steam, not water. Is that right.

MR. HOATSON: Both. Well, it's spring water and steam, so it's got some both.

DR. SHEWMON: Okay. Go ahead.

MR. HOATSON: We have to look at both. Steam and water phase to determine which is the work base.

Now, we have 149 K with the '81 program, which goes into the zinc primers and then it tests a similar weight of the galvanized and then the planning for the organic components which will involve abbreviation exposure will be done in '81.

The status we have -- user's need. We prepared a scope for 80 and 81 and provided that to the NRR people. We're expecting an endorsement of that split width any day now. The staff has recommended they go ahead, and we should be starting work in June.

The next item is the hydogen program. Last September I provided the Committee with copies of a trunk. I was quite -- and this is the outline of the items that we plan to include in the hydrogen program. It still looks fairly good.

The status that we provided $100,000 to Sandia to prepare that compendium, and they're in the process of doing that. It's nearing completion. We should have a draft by the end of May and it should be out for distribution in early June.

When we have that in hand, we'll be able to be a little more specific about the program planning.

DR. SHEWMON: What does radiolysis reactor solutions mean?

MR. HOATSON: Radiolysis of boric acid solutions in the primer system and also some solutions in the container.

DR. SHEWMON: But it's not just reactor cooling. It's also after it gets outside?

MR. HOATSON: There are some questions about the rate of hydrogen generation. Some effects -- the effects of fissure products, chemically on the radiolysis, and some.

DR. SHEWMON: Okay.

MR. HOATSON: There are containment volumves, just to give you a little perspective. Each of you are marking on this -- most of those are inerted. The ones that are operating -- I think there are two that are in operating license stage. The recommendation is to inert those. The recommendation of the Mark II is to inert those.

And the other parameters -- to give you an idea of the size, the PWI dry containments are 2 to 2.5 million cubic foot range.

This is a calculation that Charlie Kelpen referred to a minute ago. This is an isoporic, constant -- burring of hydrogen. It drops the hydrogen concentration forces the temperature or pressure that might -- in the containment.

He mentioned that failures of containments do not look likely, although 12% hydrogen will get you about the design pressure. The failure pressure is quite a bit higher. Almost double the design pressure. So it will take about a 28% , 40 % hydrogen to get you to that point.

DR. SHEWMON: How is the failure pressure defined?

MR. HOATSON: That was in zip study. It's failure of the liner, not failure of the concrete.

DR. SHEWMON: The liner is not up against the concrete is that right?

MR. HOATSON: Yes, it is. But the concrete, these pressure will probably have a practice split. And the assumption is that the liner will -- to the atmosphere.

DR. SHEWMON: So it's whenever you get cracking in the concrete, the liner is assumed to have failed?

MR. HOATSON: No. But the cracking of the concrete will occur first, but the failure pressure is about twice the design pressure.

The safety factor of 2.

DR. SHEWMON: Nobody's ever failed one, but that -- somebody else though has calculated or guestimated or something.

MR. HOATSON: Right.

DR. SHEWMON: We don't know how conservative or whatever.

MR. HOATSON: Not really. That was the assumption

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in the --

MR. JOHNSTON: There's a lot more plasticity, of course, in the metal liner than there is in the concrete. So they can calculate the concrete and gradually failing into attention with a metal liner expanding additionally. Now the failure occurs almost at the same time, as far as that goes. I mean the metal liner doesn't carry very much load after the concrete leaves it. But the sequence as the concrete goes first, followed by the metal liner because of the greater expansive and the greater elasticity of the plasticity of --

MR. HOATSON: For perspective, 100% zirconium is about 2200 pounds of hydrogen or 395,000 standard cubic feet. Probably TMI was 135 - 170,000 standard cubic feet. The stainless parts as Pick mentioned a while ago may add 20% to these figures.

If we get to the core melt stage, the core concrete reaction can produce quite a bit of hydrogen. More than the core ziconium.

And the perspective, 100,000 cubic feet is about 4.35% hydrogen which is above the flammability level.

DR. SHEWMON: Tell me again what it is in the core that generates hydrogen.

AUDIENCE: Zirconium and stainless.

DR. SHEWMON: We aren't counting the zirconium

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twice. We had almost all oxydized up there the first time. Pour sorta corn metal didn't we?

DR. OKRENT: Became brittle. It was not all converted to oxide.

DR. SHEWMON: That's just 17%, and now we get the rest of it? Is that the --

AUDIENCE: Yes.

DR. SHEWMON: Okay. Go ahead.

MR. HOATSON: And radiolysis, it takes about 3 - 5 cc of hydrogen per kilogram of water to stop the composition of primary water and a PWR. There are accident senerios which could lead to a loss of dissolved hydrogen.

TMI may have been very close to that. BWR's do not have added hydrogen and they normally decompose water while they're operating, and will do so in accident situations also.

Severe damage accidents can provide a larger fishing products source in the subwater for radiolysis than the design basis accident situation.

DR. OKRENT: When you say TMI may have been close to that, do you mean that they lost a substantial amount of hydrogen but still maintained enough to continue to assure a recombination?

MR. HOATSON: Yes, what we're doing in TMI was essentially boiling the core out the pressurizer relief valve.

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Much of the hydrogen flowed out that way. Must of it went up the hot leg, condensed in the boiler and the steam generator and returned to the core.

If the process continued with no additional hydrogen and we don't know how much hydrogen went into the make up water, the it would have been possible to take all of the hydrogen out of the primary system, or at least get below the level where radiolysis could begin occurring.

How close we were at TMI to that, I don't know. I don't think anyone does.

DR. SHEWMON: That was presumably after the bubbles disappear we got close to --

MR. HOATSON: No, no. Before the bubbles. Once the bubble form, the hydrogen produced from the corrosion of zirconium --

DR. SHEWMON: Fine, okay.

MR. HOATSON: -- would surpress the radiolysis together.

Energy absorption above water is well understood. The G values are fairly well understood in a laboratory basis, but not so well on the dirty conditions that you have in a plant.

Impurities influence it. Vapor/liquid/volume ratios. Chloresence boiling or turbulence in the water, ph, temperature and pressure-- all have an influence.

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DR. OKRENT: Excuse me, if I could ask just one question on this last point.

If we had a period when we we-e either boiling in the core, or had steam over much of the core and so forth, and they were radiolysis going on at that time, do we know whether the hydrogen and the oxygen formed would be combined before the gases got into the upper region of the vessel?

MR. HOATSON: As one going up, probably not. Because that's simply -- it's happening in a BWR.

DR. OKRENT: In other words, it's not clear to me that the oxygen necessarily recombines as soon as it was made.

MR. HOATSON: No.

DR. OKRENT: And I wonder if anybody's looked to see what would have been the maximum amount of oxygen you could have before the recombination rate was larger than the formation rate, so that there was some maximum steady state level of oxygen that you had in the bubble, assuming there was a bubble in the vessel.

MR. HOATSON: Well the recombination rate is very highly dependent on the amount of hydrogen present. If there's any hydrogen present at all, it will cause total recombination of the oxygen. If it's -- if the hydrogen is absence, then the recomposition will be at the rate --

DR. OKRENT: Well, I'm not sure what you're telling me. Let's see, if I have pure hydrogen, and I add a little bit of oxygen to it. Just in a bottle, it doesn't recombine instanteously, does it?

MR. HOATSON: Not under a radiation condition.

DR. OKRENT: Not under radiation.

MR. HOATSON: No, no.

DR. OKRENT: Well, then there's some mixture which will go spontaneously, but if you just have pure hydrogen with a little bit of --

In other words, so that -- you needed the radiation to get the reaction to go if you had a mixture of hydrogen and oxygen above?

MR. HOATSON: Oh yes.

DR. OKRENT: Now --

MR. HOATSON: And also gas station recombination is quite a bit slower than the liquid.

DR. OKRENT: Well, I'm talking about gas phase recombination and how fast that went and whether we have an estimate --

There probably is one. I just haven't seen it. Of what kind of oxygen levels one might have had.

I'm not convinced it was zero above the core. Okay? It may have been small, but I'd like -- it would have been -- it -- helpful to me to have a feeling, was it .25%, or 2% or whatever number.

DR. SHEWMON: Bill has a comment.

MR. JOHNSTON: I have some information on that. The President's Commission had this work done by two people and we reviewed it. The Argon people did it and also the origin specialist as a consultant in Pittsburgh.

MR. HOATSON: Paul Cohen.

MR. JOHNSTON: Paul Cohen did it.

The maximum estimate between the two of them was .7% oxygen would have been produced during that early part.

.7%. Small fraction. 7/10 of a percent of free oxygen may have been produced during that boiling period --

DR. SHEWMON: That .7% of the volume of gas was oxygen, in the bubble that formed, or what?

MR. JOHNSTON: At the time of the major core damage before very much hydrogen had been produced, .7% of the volume of the gas in the system. I think that's correct -- would have -- could have been oxygen as a maximum. That rapidly disappeared, however, as soon as hydrogen was produced.

Not because of gas face recombination, although that will take place above 600°C or so --

But the point is that the stuff redissolves back in the solution, and your real recombination takes place

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in solution.

So as long as you've got a 2-phase system with gas phase and a liquid that this stuff is soluble and you get your recombination back that way when it gets a chance, and that's very rapid. And it would rapidly clean the oxygen up out of the gas phase under equilibrium conditions, anyway.

DR. OKRENT: Well, I can't tell whether you were talking about the same senerio I was. But I can't recall seeing this in the present, and in the Regovin --

Which appendix is it? I'll go look it up.

MR. JOHNSTON: The chemistry. The one I think they call the chemistry.

DR. OKRENT: I'll go check.

MR. JOHNSTON: It has both Paul Cohen and I think the -- I've forgotten the group at Argon that did it, but John Hunecamp was influential in having that work done.

DR. SHEWMON: Go ahead.

MR. HOATSON: By the way what I'm giving you is a more or less kind of a preview of what's probably going to be on the compenium when it comes out. That's where most of the thing is coming from.

Gamma radiation, boric acid behaves like pure water. -- phase give higher equilibrium, decomposition levels.

The chemical effects on decomposition are not well understood.

And the present radiolysis criteria for design basis accidents are conservative.

Hydrogen analysis was a difficult area at the time of the Three Mile Island accident. There were a lot of questions about the accuracy of the analysis, and so that there is something probably that has to be done here.

DR. SHEWMON: We'll agree to that. Why don't you just let us run down over it.

I say, we'll agree to that.

MR. HOATSON: In fact, NRC has asked the vendors to add hydrogen analyzers good for 10% by January 1, 1981.

This is just one to indicate that a very low ignition energies are required to ignite hydrogen. However, you can't depend on them. This is a curve from a G.E. report. Here they -- this is --

Well I've said hydrogen along here. The theoretical pressure-wise you would get from a combustion of hydrogen quantities along this line, the dotted line, what was actually seen --

And some of these are rather large scale units. Was that until you got up to 8%, there was little combustion of the -- of all of the hydrogen.

That's probably related to the upward and downward

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flame propogation limits for hydrogen.

But unfortunately you cannot depend on this. If you want ignition, you may get it , but you may not according to this.

DR. SHEWMON: On the previous slide were your units mila jewels?

MR. HOATSON: Yes, mila jewels.

DR. SHEWMON: That's usually a small "m" even in SI, isn't it?

MR. HOATSON: Yes, that typewriter for the view graphs doesn't have a small "m".

DR. SHEWMON: I see.

MR. HOATSON: It's got a small capital "m".

DR. SHEWMON: Only 10^6 differences.

AUDIENCE: Should have been a large capital "J"? wan't it?

MR. HOATSON: These are the ommonly accepted flamability limits. The upward propogation is about 4%. Horizontal 6 and downward 9. Upward propogation tends to go up in globules with zones of unburned hydrogen between the globules.

Downward propogation is pretty close to that 8% we were looking at in the last curve and they're probably related.

This is the familiary in Shapiro and Moffet

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triangular diagram. Some of the properties of this -- some of these are difficult to read in textbooks. To read percent hydrogen, that's any line here going from zero up to 100.

Percent air is any line this way. Percent steam is any line that way.

So along this line here, we have mixtures of hydrogen and air. This curve here is the lower flamability limit for hydrogen and air. It runs about 4% here and about 26% air here. Which is equivalent to about 5% oxygen.

The interesting thing about it is that as you add steam to that mixture, the part of your hydrogen stays about the same, and it's the same with oxygen, so that the flamability range doesn't change as you add steam to a mixture of hydrogen and air -- until you get up to about 58%, and then you'll inert it.

The detonation limits have a similar shape, 18%, and 42%, air.

This line here represents a higher temperature and pressure. System 300F and 100psiJ, and it gives you an idea of how the temperature and pressure affect the final ability limits.

These are speed of combustion of hydrogen in air. Lamanor flames are very slow and they lead to causing static loads of containment.

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Even turbular claims are fairly low. 3 meters second and again lead to causey static loads. Accelerated turbulent flames can get up to 200 meters per second and you begin seeing shock waves with these.

And detonations get up to the -- what's known as a chuckman tregay speed of 2000 meters per second. You get a strong impulse loading, plus a strong causey static load.

An area of interest is triggering these into these. It will be done with large ignition sources which might come from a pump motor case and which ignites a smaller volume and then it rushes out into a larger volume. It may trigger a turbulent flame into an accellerated turbulent and give you a shock wave.

Also structure can change a turbulent flame as it flows through and it meets structure in the containment. It may trigger the transition to an accelerated turbulent flame and give you a shock wave.

This is a curve of elastic response of structures to impulse loads, and basically what it says is that at -- below this point here you can go to very high pressures without feeling this structure. The failures are over on this side of the curve. Survival of the structure is on this side.

You can get very high detonation or shock wave

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pressures here as long as the impulse which the integral of the pressure time curve is fairly low.

On the other hand, out here are -- this is the cross static loading area and the container would fail by essentially overpressure on your static load.

Much of the hydrogen area looks like it falls in this area so that we think some of these turbulent -- accelerated turbulent loads have to be settled. Just how large are they and where do they fall on that curve?

DR. SHEWMON: If you're going to say anything about your chemistry program, you better move faster.

MR. HOATSON: All right. I would like to say something about mitigation status because some of these look like they've got a lot of potential.

Talon doesn't. It's costly and it's got corrosion problems. Deliberate ignition. This looks good, but there may be -- the human factors problems on who turns the switch to light it off.

And you need some reliable analyses -- you've got to be able to rely on your analyses to do this, and you've got to have reliable ignition.

Water fog looks very promising. Temperature and pressurizer are greatly reduced. Detonation is inhibited. It raises the lower flamability limit, and only about .05% by volume of water fog and containment is required.

This seems to offer a lot of possibilities. This gives you an idea of what it might do. The top line -- there is no water droplets and here is .05%, which is about 1000 cubic foot of water scattered in containment. And the temperature drop is significant.

And this -- the same thing for pressure. Again for only .05%, the pressure is reduced quite a bit.

Budget for the hydrogen program is all in the supplemental request right now. We don't have any further funds after the funds available through the present compenium work. We have request for \$400K in the supplement and \$600K in '81, plus we have some funds in the chemistry program for radiolysis work which is associated with hydrogen.

Post accident -- in chemistry is 3 parts. The radiolysis work from the hydrogen problem which I earlier discussed.

We're looking at fission products signatures from failed fuel, and also we would like to look at iodine in containment to reduce iodine risk.

The objective of the fission products signature work is to determine if characteristic isotopes signatures result from increasingly severe fuel failure.

Can we draw samples of water during an accident that determine different kinds of fuel failures that might

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be occuring.

There's a lot of feasibility questions to it. We don't know whether we can do it yet, but we're looking into it.

DR. SHEWMON: Where in the post TMI senerio do we get to where we can take out a sample after an accident without burning up a person everytime we do it?

MR. HOATSON: Well, we -- there's two aspects to that. One is the radiation leve of the sample itself, and the other aspect is drawing a sample in an area that may be higher than the radiation level than it normally is. A laboratory sampling area of some sort.

We're planning to do some sampling and analysis work on the hydrogen program, and I hope we'll be able to take a look at that problem.

But we were only going to be looking at the hydrogen in the things and not all the sampling in the --

DR. SHEWMON: You mean that's a question more for the DOR people than --

DR. OKRENT: Yes. It's not a research problem. It's a plant design.

DR. SHEWMON: I think everybody was disappointed at the exposures they got, but I thought it was more from the sample.

Okay. Go ahead. It's not a research problem.

MR. HOATSON: I -- would like you to approve the ability to predict post accident containment atmosphere iodine. This is derived from the differences in iodine behavior in TMI, and the predicted iodine behavior in WASH 1400.

And we'd like to start off by identifying which of the iodine factors are most important in reducing the uncertainty? Is it the release fuel, transport condition, water state, two phase, condensation of operation scrubbing, or is it iodine behavior during transport, temperature pressure, chemical form, ph, oxidation reduction potential, impurities, absorption, equilibrium distribution.

Chemical form appears to be an areas that we problably will be looking at. It's confusing to say the least, at the moment.

And the last one is the budget for this work. All of these in the supplement for '80 and in the base budget for '81.

DR. SHEWMON: Thank you.

DR. OKRENT: I have one question.

I would have assumed that the interest in aspects of the hydrogen question, not the corrosion one, but the latter things you were talking about, was sufficiently high that its funding didn't depend on any supplement.

MR. HOATSON: That's where it is.

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DR. OKRENT: I must say I don't understand who's leading the show.

DR. SHEWMON: Go ahead.

MR. JOHNSTON: We took money from other funds to fund the hydrogen book which was \$100 - \$200 K that Dom mentioned that Sandia is putting together for us.

The other point was that the supplement is supposed to be 100% guaranteed, and it disappears slowly month by month. I mean you think you've got it, and we tell people to start working, and it's getting more and more nebulous.

But if we'd known this in the beginning, I agree with you. We would have done what you suggested. But we wouldn't do it if it weren't necessary.

MR. HOATSON: We have the contractor in a very awkward position right now. He's getting together a pretty good team, and --

DR. OKRENT: I sympathize with him, but I sympathize more, let's say, with those who are going to be scrambling for information.

MR. HOATSON: I hope the compendium is going to provide him at least what information we can find in the literature now. But's it's --

There's a lot of work to be done.

DR. SHEWMON: Now, the handbook -- hydrogen handbook and data base is down here for \$500 in supplement, 2'29

881.

DR. SHEWMON: I hope most of that is data. It sounds like a darn expensive handbook. U.C.L.A. could do it for less, I'm sure.

MR. HOATSON: That includes all of the hydrogen program.

DR. SHEWMON: Okay.

DR. OKRENT: Oh, we would want the full amount.

MR. HOATSON: Would you like a promise of a supplement?

DR. SHEWMON: Okay, thank you.

END OF TAPE 12

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MR. JOHNSTON: I thought, by way of summary, is to try to reiterate the theme that I talked about in the beginning, and that is that we felt that we have covered a good bit of the things that we set out to do before TMI, and that we're not re-evaluating the program and repriori-tizing it. And we indicated to you earlier the directions that we think are appropriate for us to go. We've suggested the priorities, starting with the core melt -- starting with the severe damage, starting from the point of the loca, and going on from there, as being the high priority area, together with fission products and the clad ballooning as being the top three areas as far as priority, and two of those three need work.

CHAIRMAN SHEWMON: Would you state those again then?

MR. JOHNSTON: The first one on your page, which is the core damage beyond the loca. And then the second one is the ballooning, which is existing. Then the third one is the fission product released in transport. There are a number of new programs in that one, as well as the few existing one. And then --

CHAIRMAN SHEWMON: So you've got both your sections headings and the items within sections, are in severe priority.

MR. JOHNSTON: Prioritized. Approximately so.

The bottom ones, on a given section, are all about equal in priority. But clearly the top two or three or four in a given section are our priority items. I really think that's probably all the time I should take, and that's to indicate that's where our thinking is. We're interested in your responses to it.

CHAIRMAN SHEWMON: Okay, let's stop and talk for a minute on how we get our own prioritization fixed. Now, we have to have something out in the July meeting. Is that right, Tom?

MR. MURLEY: Yes, sir.

CHAIRMAN SHEWMON: And I guess -- well we talk about it at the June meeting?

MR. MURLEY: Yes, sir.

CHAIRMAN SHEWMON: Okay, do you want to do any discussion of that at this meeting, or go on -- I guess the class 9 meeting will have before them the August PBF meeting, we will not.

DR. OKRENT: I'd like to make a couple of comments. I have asked several questions during the day that -- for example, might be interpreted as suggesting that I think we shouldn't do experiments on -- oh, degree formation, or so forth, or a range of things like this. If that interpretation is put to my questions, it's wrong. I do think it's very hard to do experiments of that sort which end up being worth the effort and the money. I think it's easy to do experiments with just the hard work, but it's much harder to do experiments that are worth the money. And this is my concern.

I think if you look at the PBF program so far, which has involved what I'll call easier experiments in general, a considerable number have been off the mark for one reason or another. Experiments are just not easy to do. And experiments you're now talking about are still harder to do even if you've thought it all through.

So there's a lot of money that one's talking about here, and I'm not interested myself in seeing this money spent here, unless we practically have a fair expectation of getting really useful information.

The same goes for the -- what you call the loca experiments. In fact, as you know, I've had less enthusiasm for those, because I haven't seen a real case made that that information we need, and if we get it, it's what Paul called a critical experiment, or something. I haven't seen that case made. I'd like to see the case made.

Now, I acknowledge a couple of areas where I think the problem's been defined. You've done a real job, and it's been a useful technical contribution. But I'm not really fully satisfied in many of the areas that -- and it's not intended to be a slur at the people doing the job.

I think these are very hard to do. I've tried to see this same kind of thing done in area for a couple of decades, and I have an appreciation for how hard it is to do. So you should understand the background from which I'm making comments and introducing questions, and I'm going to continue to be skeptical with that viewpoint. Okay?

So, in other words, I'm willing to give strong support to an experiment that I'm convinced is likely -- not guaranteed, but likely -- to be meaningful. But if it's just an experiment in the area, is't a scoping experiment, or whatever, I'm not sure that that's the best way to spend the money now, because there's some places I've indicated where I think we're out of balance in here.

CHAIMRAN SHEWMON: Let me bring up one large particular item in this regard. I sort of did a double-take when somebody -- well, when you look in the book and there's the order of $3 million a year down for operational transients, which is, as I understand from this, is for PCI studies. And I guess I would be interested in taking a page out of Dr. Okrent's book at that point and saying, yes, for lab experiments and analysis, yes; but do we really want to spend $10 million trying to figure out PCI limits? Is it worth that much to us? Then getting back, if you could scope things, why can't you encourage the industry to look some at this. And they really bear much of the brunt of that with fuel increased fuel lifetime, or downtime, or something.

MR. JOHNSTON: Would you like me -- just to make a couple of comments. I think in regard to the operational transients, that -- it's not the operational transients during normal operation, load follow type transients, which industry is normally concerned about. What we've defined these things, as the ATWS type transients that are being done and being evaluated in industry as part of the ATWS type thing. So they are transients power excursion, like beyond the normal limits that you would expect, but they're in a class 3, I guess, and maybe class 2 categories that ANS and so forth are used.

CHAIRMAN SHEWMON: Let me come back to my notes here. I've got it under Pick's comment. He was talking about PIC program, went through several things here. And the last item I think before Rick Sherry started was PBF operational transients, $3 million without operating expenses. So --

MR. JOHNSON: That's correct.

CHAIRMAN SHEWMON: But operational transients is primarily connected with a better basis for PCI, or not?

MR. JOHNSTON: No, it's a better basis for the, how does the fuel fail? If a fuel, particularly one with some high burnup in it, undergoes a steamline break in a BWR, for example, which is a calculated power increase momentarily there accompanying the pressure increase, because the voids collapse; you get a power increase which raises fuel power levels and temperatures. There are several others that have been identified. In fact, I can probably get the PBF people here that are sitting in the room to help me out a little bit. But the point is, these are the transients that have to be analyzed from a licensing point of view. From just an operational, or from a systematic point of view, the boundaries have been pretty well defined. They calculate the pressures, and the temperatures, and so forth that will be reached. But what's not known is how much clad damage accompanies that little power rise. It's looking at that kind of thing in PBF that industry can't do. We won't let them do it in a commercial reactor.

CHAIRMAN SHEWMON: No, that's a broader scope. I misunderstood then what we had in mind.

MR. JOHNSTON: I'd like to comment on Dr. Okrent's things for a moment too. We agree with him with regards to many of these experiments. But the big difficulties that we have in conceptualizing some of them is the fact that many of the things we're talking about now seem to have an axial length effect in them. For example, in the case of TMI, it takes maybe five 5-foot lengths to develop the kinds of temperature gradients, such that you have water in one end of the thing, and high temperature fuel at the other end as it boils down. But it takes a number of feet to develop those kinds of gadients and steam conditions that apparently operate.

It's very difficult to simulate that in, say, a three-foot core and determine whether you can really see the effects that you're looking for in that part of the experiment. And I know the PBF people are aware of this kind of a problem too. We're also concerned in the simulation sense that we have to heat these things up with a little bit of reactor power to warm them up. The kinds of temperature gradients and so forth radially in the fuel make a fair amount of difference in the predictions that you're going to have of the way the clad damage gets damaged, and so forth. If you have to use a lot of power to heat it up, you have the usual steep temperature grading; whereas, in reality, it's really the cladding that's driving the temperature because of the oxydation rather than the fuel providing the driving force, once you get up to interesting temperatures.

How can we learn about that aspect of it, because we're not interested in driving the result. We're trying to get the experiment to tell us what it is it wants to do. So we get into some problems of our small size and short lengths, which leads us to look into other places sometimes which are not as well-equipt to do other aspects of it.

Most of this stuff boils down to being a compromise. There are things we don't like about particular experiments, but we can't find alternatives that are better, so we do it, because the feeling is that we need something in the area. But it's an ongoing problem, and I don't think we've ever tried to say that we felt we could solve everything by running some of these tests. But we're just trying to get some feeling about what's going on. I guess that's what I can say on it. I think we're not in disagreement over that.

CHAIRMAN SHEWMON: Carson, do you have --

MR. MARK: There was another point, which I don't want to make an issue of here now. There certainly is a need to sort experiments as between the things which -- for which the NRC is responsible and can make good use of, and things of which it can't necessarily make much use, or could perfectly well be done by someone else. And Dave has made that, I think, several times, though he didn't refer to it again specifically a few minutes ago. And I'm wondering, for my own taste at least, where the degraded performance of filters falls in that kind of a spectrum. You don't really want to understand, nor make any use of understanding, how bad filters can be. It's not a terribly interesting subject, and you know that they can be very bad. And it's really up to the base sellers to say the filter has got to be of such a kind, which we know you can get, and maintained so, that its efficiency doesn't fall below this. And in that case, it's not really terribly interesting to understand how poor it can become with one or another mishandling; or if it is interesting, it's not necessarily for NRC research.

There are things which fall in there where, if it were a comparison between what are the physical range of what can happen, where the hydrogen problem is a little more of that kind, and you do need to understand it, and you can't trust anybody else to bring you the information because he doesn't have it; that would be sort of really in the clear, work deserving attention. The other must surely be somewhere closer to some boundary, and one could sort research projects on that boundary as well.

But I don't want to make a case.

MR. JOHNSTON: Well, it's true. I think Rick tried to give you some of the background. That's a program that we inherited from a different part of our organization. It's one that our licensing people have been asking to have done. But we didn't initiate it. The work in the past with the Naval Research Lab had been, indeed, looking at the degradation of filters under normal operation, if you like, normal exposure to air. Now apparently what it is that we're asked to do is to look into the degrading of these things under steam conditions and more severe conditions. I don't know whether industry can do it or not. I guess the fact -- the real truth is, we didn't look into that. Basically, licensing wanted some information in their own pocket, and they asked us to get it, and it's fairly low-cost. So I guess we -- our management agreed to di it, and it was assigned to this branch. But it is going beyond the normal situation apparently, looking into the effect of these more extreme conditions.

CHAIRMAN SHEWMON: Okay, why don't we take a ten-minute break?

(Whereupon, the proceedings were recessed at 3:55 p.m. for a 10-minute break.)

MR. MEYER: I'm Ralph Meyer, and I'm section leader of the reactor fuel section in NRR. And we were asked to talk about three subjects today. One was out technical assistance work. Another was to discuss some recent fuel failures in operating reactors. And a third subject had to do with cladding interaction, the PCI topic.

We have earlier written a report to this group, and I forgot to get the reference from Dr. Shewmon. But Paul Banard has it. I'm sure he'll get it for you. That part of the program has been cancelled. Mike Tokar, who wrote that report and was to present a PCI talk at the end of the day so that we can finish.

Before I begin talking -- I'll talk about the technical assistance, and Dean Houston here will talk about the recent failure experience. And we'll try and do that in short order.

Before I start into technical assistance, there are several miscellaneous topics that I simply want to mention to the subcommittee, not necessarily discuss. I wanted to point out first of all that reorganization that went into effect yesterday has had two effects on the fuel section in the core performance branch. One is that we have -- all of the work that was done in DOR on the fuel aspects of reloads and operating reactor problems, we have inherited none of the people from DOR who worked on that, and we've lost two people from the fuel section. So our fuel effort is going to be rather small for the foreseeable future. And that is bound to have some effect on our communications with the subcommittee.

There are a number of other topics here that I know the subcommittee has an interest in. The second topic, the reactivity initiated accidents, the RIA's, we've talked about off and on during the day. Recently Howie Richings in the core performance branch prepared a memorandum describing some calculations that were done for us by Brookhaven that showed, in fact, for boiling water reactors, that the antholpe that you can deposit in a fuel rod during the rod drop accident is quite small. And it appears that on the basis of the energy that you can insert in a reactivity accident, that we can probably convince ourselves that even if we were to repair what we believe are the nonconservative current fuel damage criteria, that they would not be challenged by the rod drop accident in the BWR, or rod rejection accident in the PWR. And we're going to prepare a recommendation that would, I believe, change our priority on this, where we can probably set it aside as a low priority item.

NOw, we've spoken of that almost as if it's been done. And in fact, it's just a gleam in our eye at this point. But that's probably what will develop with the RIA, and we'll discuss this with you in August if we can get on your program, when you're discussing the PBF program.

CHAIRMAN SHEWMON: Ralph, in two-syllable words, do these things, moderator thermohydraulic feedback, mean that -- as opposed to only hydraulic? That hydraulic has the water going out, and the thermaohydraulic is warmer, so there's less moderation? Or in little words tell me what they did.

MR. MEYER: I can tell you in a word what it is. When you put some energy in, you generate some voids and you get some negative reactivity. And so you reduce the worth of the thing that's trying to put the energy in. And through that feedback effect, they can't get very much energy in by dropping a rod in a boiler.

CHAIRMAN SHEWMON: And the voids in this case are actually steam then.

MR. MEYER: That's correct.

CHAIRMAN SHEWMON: Okay. Thank you.

MR. MEYER: The subject of swelling and rupture during a loca has been discussed extensively with the subcommittee. We've been cancelled from your meetings on several recent occasions. There has been, to this point, really nothing more developed on a schedule for implementation for the model revisions. We have issued the NUREG report with the improvements in it that we discussed with you. We will do some additional discussion inhouse with my research friend before we meet with you in June to discuss this subject.

CHAIRMAN SHEWMON: Okay, do we have a date for that? We're reasonably firm on June?

A PARTICIPANT: Yes, it's the third week of June, on my notes.

CHAIRMAN SHEWMON: Okay. And when does the first NRU shock come?

A PARTICIPANT: October, November.

CHAIRMAN SHEWMON: And the last one comes?

MR. MEYER: Okay. Appendix A, to the standard review plan has to do with the analysis or the mechanical response of fuel assembly -- the response of fuel assembly to mechanical loads that arise during the blowdown of a loca, or during an earthquake. We've discussed this with the subcommittee in detail before. The appendix went out for public comment. It was noted in the Federal Register in February. Public comment period is just now over. We've only got one comment in our hands so far.

I simply wanted to mention that we had made some progress in getting this out. I don't know now in the uncertainties of reorganization, how the balance of this implementation will go in terms of an actual revision to the review plan. I can tell you that we're going ahead with our review according to this proposed plan, because there is nothing else. We had nothing else on the books to describe that review.

And finally, slightly old subject of fuel bundle liftoff in a boiling water reactor that I believe originated down here. The concern for it originated down here. Was first expressed to DOR, and has been batted back and forth between DOR and ourselves for a couple of years. The last November hired Gus Alberthal to work in the mechanical area. He has started on this liftoff problem. The review is going well now. We'll get a report from GE in October, and we've seen preliminary results, it looks like, that the fuel bundles will chatter a little bit, but they won't lift up enough to come out of the socket. That's what it looks like the answer's going to be.

Unfortunately, Alberthal was taken from the section, so I'm not sure how we'll complete the review. But we'll get something from GE later this year.

Let me now, just quickly through the technical assistance tasks. And I'll simply try and give you an idea what we're doing, and if you want to stop and ask a question, that's all right. Here is a list of the individual tasks, and I have one slide per task that I'll go through, mention what it is. On-call assistance in annual report on fuel performance are two tasks that were contracted by the Division of Operating Reactors, and we've inherited those recently. They fit into our work well, so I'll show how that goes.

The total amount budgeted this year for fuels work is $380 K. I included a summary similar to this from last year to show you that that's roughly the same amount of money that we spent last year on technical assistance in the fuels area.

CHAIRMAN SHEWMON: What's S&L?

MR. MEYER: That's the seismic and loca. I'll go through these one by one. We have two technical assistance programs, called fuel performance code applications.

They are different. There are different laboratories, and they're in fact different programs. This one is at Batell, and it is technical assistance to help us in the review of vender fuel performance codes that are used primarily for the initiation of a loca analysis, the stored energy codes, the ones defense are done in.

We initially had included some money for all calculations for B&W code, and a combustion engineering code. We took that out when we got Alberthal on board to help us with those reviews. And so we have funded general consulting to just sort of help prop us up in doing the reviews inhouse, and a small study on extended burnup problems with fuel performance codes. You've expressed an interest in this. The ATWS DOE program that goes under the NASAT initials has also given us some motivation to try and get a leg up on what kind of problems we're going to run into when we try and do licensing calculations at levels higher than we're accustomed to.

DR. OKRENT: What will they do for you for $30K in that area?

MR. MEYER: Well, they're going to look at the material's properties and at the subroutines that have strong burnup tendencies, and try and point out where we're going to run into big uncertainties in code predictions when we get beyond burnups that we've got in our current data base.

A second task, called fuel failure limits, has been focused almost entirely on the pilot planning interaction problem. During fiscal '79 and earlier we had a joint program with Batell Northwest and Canadien group at Chalk River trying to provide us with some empirical models for predicting probabilities for failures. And we did get those models in fiscal '79. As Bill Johnston mentioned this morning, all of our PCI work is going to be transferred over to research in fiscal '81, and that leaves the current year fiscal '80, which is sort of a transition year, during which we're providing a small amount of money for Batell to document the mechanistic concepts that went into the model that they published in the other report.

CHAIMRAN SHEWMON: Is there anyplace I could get a discussion of the pros and cons, hide and stress corrosion cracking versus any other viewpoints of what causes cracking in PCI?

MR. MEYER: Well, I think the report that Phil Pancaskey is preparing under task 1 is such a report. We do have -- we've already reviewed it for publication. And --

CHAIRMAN SHEWMON: I look forward to seeing it then.

MR. MEYER: -- I believe it'll be out in another month or thereabouts. In particular, Batell is going to look closely at the incubation time, the delay time, the controversial old time that some feel is essential to get the PCI failures. And we'll look at that from the data that we do have to see if indeed the data are unambiguous in showing us the incubation time; or if, in fact, the -- what you interpret as an incubation time might be a rate effect.

Now, Pancaskey has used a concept called strain energy absorption to failure, which he discusses in this report, and he'll be doing some more work on that to see if it -- if he can determine that ratio from the data that we have on the failure rate in the data base. And a small amount of unspecified support in case we have some luck in getting profit mile used in licensing analysis. We would expect to have to ask him a couple of questions.

You've seen this one on previous years, radioactive fission gas release analysis. This is the final year. We've underfunded and piddled around with this one two or three years, and we finally have gotten them enough money to finish, and have the steps to finish this laid out. Our objective here is to do enough calculations to provide a basis for the gas release assumptions that are made in three regulatory guides that are currently used: one dealing with the local, one dealing with the rod ejection accident, and one dealing with the fuel handling accident. And so the calculations will be made of the steady state gap inventory, and then some estimates of the additional release component for a loca transient, for an RIN transient.

Our ultimate use of this would be to try and revise the regulatory guides. Now, this is a DOR program called fuel operational performance. Originally they simply called it oncall assistance, and didn't specify what it was going to be. And then as problems came up, they had them -- they sent them out to Batell, and the problems that have come up so far are, one in connection with Zion extended burnup program. They performed a calculation to look at crud buildup and additional temperature rise across an extra layer of crud going to high burnup. They found that that wasn't very important.

There have been some recent mixed oxide rods put in Genet, and so they did a couple of more calculations with gathcon to look at the average temperatures.

CHAIRMAN SHEWMON: Can I ask that you go faster?

MR. MEYER: Sure. Well, let me just -- I think I don't have to -- DOR has funded Batell to help them do some statistics on fuel failures and to evaluate fuel failures for the purpose of preparing a report. We prepared one report but did not have statistical analysis in it. And we would plan to include that kind of analysis in future versions of the report.

Okay, here's the second fuel performance code application program. This is at Idaho. It's quite different from the first one. Here -- I do want to comment on this one, because in one respect it's the most interesting of the lot. This is our attempt to get a modern symbol code to do loca calculations. This is a modern day 2D replacement, if you want. We're going to take Frap T5, and take the bells and whistles off that we don't need to do the loca analysis, and pay Idaho to run it through something like a licensing review, strip it down, put in some of our favorite assumptions and models in, and end up with a code that we can use inhouse to do the kind of calculations that we'd attempted to do on the swelling and rupture thing a couple of months ago.

So here's a case where we're making a very serious effort to use one of research developed codes, but to simplify it a little bit before we do that.

At Idaho we have some assistance in reviewing topical reports on the seismic and loca mechanical response analysis. That needed a little bit of extra line to finish it, and we've given them some unspecified time to help us respond to comments on the standard review plan appendix, to help us see through this BWR liftoff problem, and other things related to the mechanical analysis. That's a pretty small program.

In that same group that funded, under a separate program letter, is a post blowdown loads study. This is a small task to calculate loads on fuel assembly components from events that might happen after the loca heat up this oxydized cladding. This could be something like a pump switchover that we worried about at Three Mile Island. Or if loca is related to an earthquake, it could be an aftershock. And so we're going to make calculations with the audit code that we use for reviewing vender codes and compare those with embrittlement criteria from work done at Argon that Bill Johnston's people have described to you, and see whether there is any cause for concern.

And finally, the last task is also one that ACRF has expressed concern, and this is fuel failure propagation, and it's being done at Los Alamos. It's a two-year program and the $95K covers it for two years. We just went ahead and funded it initially for the whole amount. It includes a very large thermohydraulic component. So TNB propatation is definately one of the things that's being looked at. And this will provide us with an estimate of whether the failure data around the world today, and what's known about failure mechanisms, would indicate any likelihood of provocation.

So that's all I have.

CHAIRMAN SHEWMON: What do those last words mean? Whether the failure propagation data around the world, or failure data would suggest any propagation?

MR. MEYER: I'll have to find those on that slide. This task is not generating any new data. We've got a contractor that has some experience with failure mechanisms from both the mechanical kinds of causes that fuels people are aware of, and the DNB causes. And all I meant to say was that they're going to search the literature and use their experience to see if it's a real worry or not.

DR. OKRENT: What was it that you think the ACRS expressed an interest in?

MR. MEYER: We've had some long discussions about failure propagation here for a year or more now. And whether by failure propagation we meant fission gas impingement on adjacent rods, or molten fuel materials squirting out and plugging up channels so that adjacent rods didn't get properly cooled; or whether, in fact, just a departure from nuclear boiling on one rod would affect adjacent rods. And it was -- as best as I can recall, it was a conclusion of that meeting that we hadn't demonstrated satisfactorily that propagation could be ruled out. And yet we weren't doing anything about failure propagation in the licensing analysis. So --

CHAIRMAN SHEWMON: Must have been a meeting you were in.

DR. OKRENT: I'm not clear what kind of data you think there is around the world that would be useful in answering whatever you think the question is.

MR. MEYER: Mike Tokar is the expert in this area, this program. And we cancelled him for this afternoon's talk. I'm sorry he's not here.

CHAIRMAN SHEWMON: Why don't we wait for the report. My impression is it's a nonproblem, or at least it's one that's been around for a very long time. Nobody's every been able to prove it's not true. And we never will prove something until we see fuel propagation, I would guess, your past reviewer.

DR. OKRENT: I just don't understand what they're going to do by looking at data around the world in regard to the question -- if it's in response to something that they think the ACRS raised. And I suggest you might try to generate some kind of amplified definition of this task over -- it may exist. At least, I'd be interested in seeing an amplified definition to see if, in fact, it does resemble what I think of the areas that the ACRS in the past has expressed interest in.

MR. MEYER: Would you like us to prepare a brief memo to you on that?

DR. OKRENT: If that's convenient.

MR. MEYER: I'm quite sure that if Tokar were here now he could give you the answer.

DR. OKRENT: Fine.

MR. MEYER: Dean Houston now will describe recent fuel failures.

MR. HOUSTON: How much time do we have here? I'm Dean Houston, formerly with the fuel section, and now with the division of licensing. I'll cut this as short as I can, I guess, and we'll just see how long it really runs. I have -- in the handout I have essentially listed the general areas of fuel failures, and included associated core components. I would plan to only discuss just the area of fuel failures, but am prepared to make any comments about the other items if you have any desire.

First here we have a table showing the 1979, as close as we can in 1979, annual operating statistics. Failure here is defined as fuel rods leaking, or structural damage to an assembly component. None of the figures are derived from coolant activity levels. We have 70 different reactors licensed; failed assemblies listed here, the fuel assemblies in those reactors listed here, if you disregard the Three Mile Island, two assemblies which we have estimated here as 150 being failed, you see 116 here containing some kind of failure. Typically these will have two to three rods per assembly that are actually leaking.

What this comes out as in a rod failure percentage in a population of about two and a quarter million fuel rods, you have a rod failure percentage of .015.

Now, in this same population we do have three reactors where the rod failure in a given cycle is something on the order of .2 of a percent, up to .3 of a percent. So there is some sort of a range represented there.

CHAIRMAN SHEWMON: What was your lower limit?

MR. HOUSTON: Well, it's an average for the overall population. It's .015.

CHAIRMAN SHEWMON: Okay.

MR. HOUSTON: And then there are those three ractors in the range of .2 to .3.

Now, next I've put up a slide that mechanism for failure, with the plants in which the failures have occurred. In some cases the mode of failure is well known, but the exact reason for its occurrence is still unknown, even after extensive investigations. We'll skip TMI 2. We see here that there are two cases of water site corrosion. We always have water site corrosion, but in these cases there's excessive corrosion leading to cladding failure.

Tape 4

First in the PWR's, in the Maine Yankee case, coolant contamination occurred following a changeout of a resin bed in the purification system. I should remark here too that there's been a similar incident where air in-leakage in a purification system occurred at Calvert Cliffs, but no failures resulted. However, there was a heavy corrosion deposit, caused an increased pressure drop across the core, and shifted the peak and the power distribution to the bottom of the core instead of toward the top. They have performed the crud burst procedure, and they're back -- the pressure drop has gone back to normal, and they've been back at 100 percent power for about a month with no noted failure.

In the Maine Yankee case this same type of incident led to a unique crud deposit between the sixth and seventh spacer grids, and failures there occurred by two assemblies they've identified from corrosion itself. There are five assemblies here that they say are possible PCI's, and I suspect that's because perhaps the power shifted to the bottom of the core. And there's one under the unknown category. They have no real handle on the mechanism.

CHAIRMAN SHEWMON: If we look at those in a different way, which of them, besides the Lacross--and let's scratch the TMI 2, which is a different kind of event--led to enough corrosive activity so that you started giving expect questions, or even increases in primary system activity.

MR. HOUSTON: The only two that I'm really aware of are the Conn-Yankee ones and Lacross where both populations of failures led to an increase -- they were riding about 10 percent of the tech-spec limit. Now, Vermont

Yankee may have had some difficulties here because they're in about the same percentage. About .3 of the core would be represented by leakers, and only in those three cases were there anything above -- anything exceeding 1 percent of the tech-spec limit.

Now, at Vermont Yankee the failures were completely different. They were confined to one reload batch, and only in zircoloid cladding from three or four of the cladding batches. They're typically something like 50 or 55 cladding batches represented in the core at the time. The corrosion product was highly localized in those particular clad batches. Extensive PIE and archive examination, both nondestructive and destructive, has not pinpointed a reason that these cladding batches should be susceptible. There are no other known failures of this particular type, but it did lead to 30 assemblies having two or three failed rods per assembly.

The next one is the stress corrosion cracking. In Conn Yankee, this is in 304 SS, occurred also in just one particular batch of fuel. Here we have sort of a case, the fuel cans were made by Gulf United. The pellets were made to specification by British Nuclear Fuel, and the final fuel rod and assemblies were put together by Babcock and Wilcox. The reason for the stress at end of life burnup was about 33 and a half thousand is not yet specified.

We go on to the -- well, we'll skip the Lacross.

The Lacross is just a carryover from previous PCI problems, and it's listed here mainly because 17 assemblies that were discharged were discharged in the year 1979.

CHAIRMAN SHEWMON: There was a reasonably strict burnup limit put on Lacross when they went back up this last time.

MR. HOUSTON: Right.

CHAIRMAN SHEWMON: How did --

MR. HOUSTON: To 15,000, I believe.

CHAIRMAN SHEWMON: How has performance compared with that? Do you know?

MR. HOUSTON: They have gone through one reactor cycle. They have asked for an extension of the limit to, I believe, another 300 megawatts, something like 15 3, or 15 6. In the sixth operating cycle they had no leakage after they had these 17 removed.

The next case, we have refueling handling that resulted in 11 failed assemblies. Nine of these were at Salem 1. Failure occurred by grit strap damage, and those with strap width pieces missing were not reinserted and considered as failed. Those with minor chinks, or a tab missing, or something like that, were considered reusable in the next cycle, although they did suffer that minor damage, and there were 23 of those. At Maine Yankee there was one assembly twisted, and at Crystal River, there was some kind of an object fell on assembly and did damage to the hold-down springs.

Now, when you go into the unknown category, this is a catchall for leakers with no apparent mechanism. We should have shown -- this is 4, and you could add Trojan to this list, since they called in yesterday and said they had observed one rod that was split open, and it would fall in that same category. The same types of failures have been shown in Fort Calhoun and Rancho Seco on fuel that has been removed, discharged into the pool, and at some time in the examination they have seen only one rod with one failure.

The seven at Brunswick, which would be the seven BWR's here, were first put in a probable PCI category. Since then the full core has been sipped, and the leakers are mostly in old 7 by 7 fuel which, in the previous years, has had a poor performance record. The location of the leakers in the core is not associated with the PCI kind of event. There was a faulty control rod in double notched when they were doing control rod maneuvers. And in previous instances where PCI has been the problem, the leaker fuel has been nicely grouped around the control rod, which gave them the power event. In this case, the old 7 by 7's that are leaking are really not around the control rods. They're scattered throughout the other three quadrants of the reactor. It may be that the individual rod, the control rod problem, has only given rise to the simultaneous release from failures that were already there.

CHAIRMAN SHEWMON: Why don't you move on, hit on high points, or things you think are particularly general.

MR. HOUSTON: Okay, that pretty well takes care of this anyhow. There's PCI. We've talked about that. The vibration treading for Yankee Row is in stainless. There's no apparent reason for that. It's not water-baffled because the baffle there is one piece welded with no joints.

Next, I'd summarize just the common things under one title, stress corrosion cracking. And this is the only one where there has been a lot of failures or potential failures. The two are in fuel, we've talked about Conn Yankee and Lacross. The other ones are in associated core component parts, the Westinghouse upper guide tube pins, which are of incinel; the control rodlet fingers, which are 304 stainless; and the GE control rod cladding. I might point out that in the control rod cladding, the General Electric control rod cladding, they have backed off from what they have considered 100 percent design limit before, to an 80 percent design limit. This doesn't eliminate all of the cracked control rods, but it does eliminate most of them before C washout.

CHAIRMAN SHEWMON: In the middle one there, I guess there is this Japanese reactor and the Westinghouse people are now saying that it couldn't possibly cause any harm if they did break, except in ice condensor -- the ice --

MR. HOUSTON: In the upper head injection plants.

CHAIRMAN SHEWMON: Right. Is that it?

MR. HOUSTON: Right.

CHAIRMAN SHEWMON: Is it your impression that their track record is as good on that as sat as that of Vise? Or have you ever bumped into that in this country? Or is it just one mis-heat-treated batch, or what?

MR. HOUSTON: The problem came up in a foreign reactor with foreign made material, which were made by a different process than Westinghouse makes theirs. Only the foreign made pins broke in that reactor. The Westinghouse pins both foreign made -- or the Westinghouse pins, both in foreign and domestic reg reactors have never shown a failure. However, then, to follow that up, in the same foreign country there was an incidence in a UHI plant where four flaws were found in Westinghouse made -- in a Westinghouse made pin. And so because of the four flaws that were found, Westinghouse has gone back and decided to heat treat all of their incinel at a higher temperature. There's a lot of --

CHAIRMAN SHEWMON: The flaws were stress corrosion cracks?

MR. HOUSTON: It's in a different position than the first failures were noted. It's down in the tongue, or the extension part of the pin, rather than up in the shank. And it hadn't led to failure. But because the flaws were there, Westinghouse decided to go back and heat treat all their incinel at a higher temperature, and then replace all of that in the UHI plant.

CHAIRMAN SHEWMON: And the higher solution to neotemperature would presumably protect things from stress corrosion cracks. I. that it?

MR. HOUSTON: Right. Although they do have a lot of the lulaneal material operating here domestically, and have never seen one of these come apart. And then, just very briefly, here are those single batches that have either had failures or had shown operating anomolies. We've talked about Vermont Yankee and Conn Yankee as failures. There was one batch in Prairie Island 1, the force reload. The entire batch showed excessive rod bowed end of life. No reason given, was the only batch that they've seen this type of behavior.

Surry Unit 2, batch #7 was sabotaged, where workers poured sodium hydroxide on it. Those particular assemblies were taken back. All of the spacer grids except the bottom and the top one, and all of the guide thimbles were replaced in the rebuilt assemblies.

CHAIRMAN SHEWMON: We devote a reasonable amount of time by spells to trying to see that we don't ever get DNB. I guess where we worry about that is in transients. Is that right? And therefore, we're so far away from that with regard to normal operations that you never expect to see it anyway?

MR. JOHNSTON: Or in the misloading of the fuel, which Dr. Okrent mentioned this morning. I think improper enrichment. Where the assemblies unload, you can get DNB and supposedly normal operation.

CHAIRMAN SHEWMON: Okay. But in the -- so many reactor years we have, we've never seen an example you would blame on that. Is that right? Or failure you would blame on that? Or can you say?

MR. HOUSTON: In DNB?

CHAIRMAN SHEWMON: Yes.

MR. HOUSTON: I don't believe we've ever seen anything of that nature.

CHAIRMAN SHEWMON: What would you look for if you did have it? Or what do you think would show itself?

MR. HOUSTON: If you look at the PBF fuel you see a lot of discoloration, crud buildup, even a wasting. I believe you would see those kinds of things if you really had DNB.

CHAIRMAN SHEWMON: Okay.

MR. HOUSTON: And then, finally, I only have this one last slide on generic items that come under this category. Guide tube wear. I don't believe we've seen any new assemblies, new failures in assemblies from guide tube wear. Every PWR vender has a model, has some examination results from their particular assemblies under control rods. And CE has pretty well settled on the chrome-plated stainless steel sleeve to overcome the guide tube problems that they had. The BWR control rod lifetime, which we talked about previously. The BWR water rod wear. This is a matter that they extended the tip on the water rod, and it goes down into a turbulent flow area in the lower tie plate. They did that on a 8 by 8 R assemblies. All 8 by 8 assemblies had a shorter tip and had no wear, so the solution to the problem right at the moment is to cut the tips back to a shorter length.

There may be a problem later on if they go to extremely high burnup, and need the extra bit of the tip to allow differential growth, zircoloid growth, to follow. And the Westinghouse, baffle jetting. This was a problem that was handled in about '75. They thought it was pretty well identified on heat driven joints or sections in the baffles. And because of the flow of the other joints was different, they felt that there was no problem there. So they planed the 8 joints, and then this year in another foreign reactor they saw baffle jetting at one of those other joints. I think there's about 12 or 14, 15 other joints. So what Westinghouse is doing now is going in and cleaning all of the joints in the baffle, both of the original eight locations, and then the following 14 or 16.

And that summarizes where we stand for the given year on fuel failures.

CHAIRMAN SHEWMON: Right on time. I thank you very much. Are there any questions? Okay. Looks like we're in fair shape then. Thank you very much. Meeting adjourned.

(The proceedings were adjourned at 5:05 p.m.)

PRESENTATION FOR ACRS - APRIL 29, 1980

HYDROGEN FROM COATINGS

HYDROGEN PROGRAM

POST-ACCIDENT COOLANT CHEMISTRY

D. A. HOATSON

FUEL BEHAVIOR RESEARCH BRANCH

TALK FOR ACRS

Introduction

I plan to discuss three topics with you today:

VG-1

- Combustible Gas Generation in Containment
- The NRC Hydrogen Program
- Post-Accident Coolant Chemistry

Combustible Gas in Containment

The first item deals with the generation of hydrogen or other combustible gases from galvanized zinc materials, zinc based primers, and organic coating systems. The need for this work arises from inadequacies in the data being used to judge the adequacy of recombiners for accidents which fill the containment with steam, but fall short of the class 9 type event. Reg Guide 1.7 covers the control of combustible gas concentrations in containment following a LOCA, but the generation of hydrogen from zinc and organic coatings is not adequately defined in the guide. The safety analysis reports involve order of magnitude differences in the assumptions for hydrogen generation from these sources.

The use of aluminum has been severly limited in containments, but zinc based paints and galvanized steel are widely used. This viewgraph provides the amounts of zinc from the San Onofre FSAR.

VG-2 - Zinc in Containment

Zinc based paint	850 lb	6700 ft^2
galvanized:		
grating	5000 lb	40,000 ft^2
cable trays	5600 lb	45,000 ft^2
conduits	150 lb	4,200 ft^2
plateforms & stairs	1400 lb	11,000 ft^2

GV-2 (Continued)

decking	2100 lb	26,000 ft^2
pipe hangers	835 lb	6,700 ft^2
polar crane	500 lb	5,000 ft^2
in-core detector system	10 lb	30 ft^2
refuelling equipment	6 lb	60 ft^2
TOTALS	16,450 lb	145,000 ft^2

There is a suprizingly large amount of Zinc in containment. If all of this zinc reacted with steam to yield hydrogen, the concentration in one of the large dry containments of 2M cu ft. could exceed the combustion limit of 4%. When combined with radiolysis and hydrogen from zirconium-water reaction (using the Appendix K design basis x 5 per reg guide 1.7), the rate of hydrogen production from the zinc source and hydrogen and other combustibles from coatings become significant as a basis for design of hydrogen handling systems for design basis accidents.

Our plans for this work are reflected on the next viewgraph:

VG-3 Combustible Gas in Containment

FY 80 - $100K

1. Prepare and Present Program Plan for Galvanized Zinc
2. Perform Scoping Tests
    a. demineralized water baseline
    b. effect of T (130-340F)
    c. effect of pH (4 to 10)
    d. effect of thiosulfate spray
    e. synergistic effects
3. Plots, Equations, Reports of Results

The FY 81 anticipated scope is:

VG-4

Combustible Gas in Containment

FY 81 - $149K

1. Program Plan for inorganic Zinc Primers
2. Test Conditions as for galvanized coatings modified by experience
3. Liaison with Nuclear Coatings Committee (ASTM D-33)
4. Analytical proceedures for combustibles
5. Extend experiments to radiation conditions
6. Plan organic coating experiments

Organic coatings for steel include epoxy polyamides and epoxy phenolics. These are also used for concrete plus a water based epoxy polyamide.

The status of the project is:

VG-5

Users Need #RR-NRR-79-15 received.

Scope of work for FY 80 & 81 - prepared.

User Endorsement - momentarily

Start work - June

TOPICS

- COMBUSTIBLE GAS GENERATION IN CONTAINMENT
- THE NRC HYDROGEN PROGRAM
- POST-ACCIDENT COOLANT CHEMISTRY

ZINC IN CONTAINMENT

ZINC BASED PAINT	850 LB	6,700 FT^2
GALVANIZED:		
GRATING	5,000 LB	40,000 FT^2
CABLE TRAYS	5,600 LB	45,000 FT^2
CONDUITS	150 LB	4,200 FT^2
PLATFORMS AND STAIRS	1,400 LB	11,000 FT^2
DECKING	2,100 LB	26,000 FT^2
PIPE HANGERS	835 LB	6,700 FT^2
POLAR CRANE	500 LB	5,000 FT^2
IN-CORE DETECTOR SYSTEM	10 LB	30 FT^2
REFUELLING EQUIPMENT	6 LB	60 FT^2
TOTALS	16,450 LB	145,000 FT^2

COMBUSTIBLE GAS IN CONTAINMENT

FY 80 - $100K

1. PREPARE AND PRESENT PROGRAM PLAN FOR GALVANIZED ZINC

2. PERFORM SCOPING TESTS
   A. DEMINERALIZED WATER BASELINE
   B. EFFECT OF T (130 - 340F)
   C. EFFECT OF pH (4 TO 10)
   D. EFFECT OF THIOSULFATE SPRAY
   E. SYNERGISTIC EFFECTS

3. PLOTS, EQUATIONS, REPORTS OF RESULTS

COMBUSTIBLE GAS IN CONTAINMENT

FY 81 - $149 K

1. PROGRAM PLAN FOR INORGANIC ZINC PRIMERS
2. TEST CONDITIONS AS FOR GALVANIZED COATINGS MODIFIED BY EXPERIENCE
3. LIAISON WITH NUCLEAR COATINGS COMMITTEE (ASTM D-33)
4. ANALYTICAL PROCEDURES FOR COMBUSTIBLES
5. EXTEND EXPERIMENTS TO RADIATION CONDITIONS
6. PLAN ORGANIC COATING EXPERIMENTS

STATUS

USERS NEED # RR-NRR-79-15 - RECEIVED

SCOPE OF WORK FOR FY 80, 81 - PREPARED

USER ENDORSEMENT - MOMENTARILY

START WORK - JUNE

NRC Hydrogen Program

Last September I provided the Committee with copies of a talk on our proposed Hydrogen Program. The scope of work we envisioned is shown in this first viewgraph.

H1 - PROPOSED SCOPE OF WORK ON HYDROGEN ISSUES

We now have Sandia Laboratories working on the compendium and most of my discussion will be in the nature of a preview of some of the information evolving from their early information gathering efforts. I should caution you that the consequences in terms of reactor safety or future research efforts have not yet been fully digested.

Background

As reference material, which will help to put hydrogen efforts into perspective, the next viewgraph presents pertinent containment parameters.

H2 - CONTAINMENT

In addition to the smaller sources of hydrogen mentioned earlier (dissolved hydrogen, limited oxidation of zirconium per Appendix K, zinc corrosion, and organic coatings), hydrogen is also produced by the substantial core oxidation which can occur in accidents beyond the design basis accidents. These are presented in the next viewgraph.

H3 - HYDROGEN SOURCES (BEYOND DBA)

Clearly on severe accidents, it may be necessary to deal with substantial amounts of hydrogen.

H1 - PROPOSED SCOPE OF WORK ON HYDROGEN ISSUES

Compendium

With regard to project status, we have reprogrammed funds to allow Sandia to start on the compendium. The balance of the funds have been requested in the FY 80 supplement which has not yet received congressional approval.

A draft of the compendium will be released for comment within NRC early in Jun.. The preparation of the compendium will provide an opportunity to update the proposed program scope and identify specific analytical and experimental efforts that may be required.

It is our intent that the first issue of the compendium will summarize available information on hydrogen in an immediately useful form. Subsequently, we will be performing experiments to provide information that is needed to fill the gaps identified and a few years' hence the compendium will be updated and issued in final form.

For now, we expect work will be required in Radiolysis, Hydrogen Analysis, Flammability and Detonation Limits, Detonation Pressure Time Histories, and methods of Mitigation of Hydrogen Problems.

Radiolysis

There are two aspects of radiolysis that have to be considered - decomposition of water in the primary system and decomposition of sump water in containment.

H4 - RADIOLYSIS

Our tentative assessment of radiolysis information is shown on the next viewgraph.

H5 - RADIOLYSIS - STATUS

Radiolysis may be important on intermediate accidents where not much metal/water hydrogen has been generated. We need to get a better handle on the importance of radiolysis before proposing specific experiments.

Sampling and Analysis

There appears to be a need for additional work on analytical determination of hydrogen concentrations under emergency conditions.

H6 - HYDROGEN ANALYSIS

Utilities have been requested by NRC to provide hydrogen analyzers for up to 10% hydrogen by January 1, 1981. One device that Sandia has identified as having some potential is laser Raman spectroscopy and work on it may be proposed.

Combustion of Hydrogen

Ignition

The ignition of hydrogen-air mixtures requires very little energy.

H7 - IGNITION

However, one cannot count on a spark ignition to light off a combustible mixture. One area that seems to need work is the effect of large ignition sources (i.e., an electrical box or pump motor casing) on flame speeds generated.

H8 - NEDO-10812 CURVE

The conservative position appears to be that we had better expect easy ignition, but we can't count on it.

Flammability Limits

The generally accepted flammability limits are presented on the next viewgraph.

H9 - FLAMMABILITY LIMITS

The effect of steam on these limits is portrayed in the familiar Shapiro and Moffit triangular diagram.

H10 - SHAPIRO & MOFFIT TRIANGULAR DIAGRAM

The solid flammability curve is for 75F - 1 atm. The dashed one is for 300F - 100 psig. Note that as steam is added to the H_2-air mixture, the lower limit stays about 4% until there is about 55% steam in the mixture. The upper limit stays close to 26% air (or about 5% oxygen). Also note that mixtures containing 58% steam are not flammable.

Similarly, the detonation limit stays close to 18% H_2 and 43% air (or about 8% oxygen) and mixtures with about 35% steam do not detonate.

Flame Speeds

Hydrogen burns in a number of different flame regimes -

H11 - SPEED OF COMBUSTION FRONTS

These flame speeds are generally, but not necessarily correlated directly to hydrogen concentrations. Large ignition sources can yield turbulent and accellerated flames at relatively low hydrogen concentrations. Also, structural material in the path of an advancing flame front is capable of causing the development of higher speed flame fronts. This suggests that some work on the effect of structures in large scale volumes may be necessary.

Structural Response to Impulsive Loads

The next viewgraph -

H12 - PI DIAGRAM

describes the response of elastic structures to pressure loading and impulse (integral of dP/dt) loading. The structure fails above the heavy curve and it survives below the curve. There is a critical value of impulse at the left of the curve that will not cause failure even at very high instantaneous pressures - because the integral of pressure-time is low due to the short time over which the pressure acts. The message seems to be that all is not necessarily lost even if a detonation were to occur. Pressure-time histories from the hydrogen program will provide better information for such structural analysis.

Mitigation

A number of mitigation schemes have been suggested and some appear worthy of further investigation -

H13 - MITIGATION

To finish the Hydrogen Program discussion, there appears to be a number of areas where research work can lead to a better understanding of hydrogen problems and lead to ways of reducing them. The last viewgraph presents the proposed budget -

H14 - BUDGET - HYDROGEN PROGRAM

PROPOSED SCOPE OF WORK ON HYDROGEN ISSUES

1. COMPENDIUM OF INFORMATION FOR REACTOR HYDROGEN EMERGENCIES.
2. RADIOLYSIS OF REACTOR SOLUTIONS.
3. SAMPLING AND ANALYSIS IN REACTOR EMERGENCIES.
4. FLAMMABILITY AND DETONATION LIMITS UNDER ACCIDENT CONDITIONS.
5. DETONATION PRESSURES FROM HYDROGEN EVENTS.
6. HANDLING POST ACCIDENT HYDROGEN.

CONTAINMENT
(TYPICAL 1200MWe PLANTS)

TYPE	VOLUME	DESIGN P
BWR MARK I	$.3 \times 10^6$ FT^3	62 PSIG
BWR MARK II	$.3 \times 10^6$ FT^3	45 PSIG
BWR MARK III	1.5×10^6 FT^3	15 PSIG/DRYWELL 30
PWR ICE CONDENSER	1.2×10^6 FT^3	12 PSIG (THRU 15 PSIG)
PWR SUBATMOSPHERIC	1.85×10^6 FT^3	45 PSIG
PWR DRY CONTAINMENT	$2.0\text{-}3.5\times 10^6$ FT^3	45-60 PSIG

HYDROGEN SOURCES (BEYOND DBA)
(TYPICAL OF A 1200MWe PLANT)

<u>SHORT OF CORE MELT</u>

100% CORE ZIRCONIUM = 2200 LB H_2 (= 395,000 SCF H_2)
PROBABLE TMI = 750=950 LB H_2 (= 135-170,000 SCF H_2)
CORE STAINLESS PARTS MAY ADD 20% TO ABOVE IF T EXCEEDS ~2000^{o}F

<u>CORE MELT</u>

CORE-CONCRETE REACTION H_2 = 2600 LB H_2 (467,000 SCF H_2) (FROM 2400 FT^3 CONCRETE REACTED)
(FOR PERSPECTIVE 100,000 SCF H_2 IN 2,300,000 FT^3 = 4.35% H_2)

PROPOSED SCOPE OF WORK ON HYDROGEN ISSUES

1. COMPENDIUM OF INFORMATION FOR REACTOR HYDROGEN EMERGENCIES.
2. RADIOLYSIS OF REACTOR SOLUTIONS.
3. SAMPLING AND ANALYSIS IN REACTOR EMERGENCIES.
4. FLAMMABILITY AND DETONATION LIMITS UNDER ACCIDENT CONDITIONS.
5. DETONATION PRESSURES FROM HYDROGEN EVENTS.
6. HANDLING POST ACCIDENT HYDROGEN.

RADIOLYSIS

- IN THE PRIMARY SYSTEM OF A PWR 3-5cc H_2/Kg WATER IS ENOUGH TO ASSURE RECOMBINATION OF DECOMPOSED WATER.
- PWR ACCIDENT SCENARIOS ARE CONCEIVABLE WHICH LEAD TO A LOSS OF DISSOLVED HYDROGEN.
- BWR'S HAVE NO ADDED HYDROGEN AND HAVE A NORMAL DECOMPOSITION DURING OPERATION AND ACCIDENT SITUATIONS.
- SEVERE DAMAGE ACCIDENTS CAN PROVIDE A LARGER FISSION PRODUCT SOURCE IN SUMP WATER FOR RADIOLYSIS THAN DBA SITUATIONS.

RADIOLYSIS - STATUS

- ENERGY ABSORBTION BY WATER IS WELL UNDERSTOOD.
- DECOMPOSITION PER UNIT ENERGY ABSORBED (G VALUE) IS FAIRLY WELL UNDERSTOOD ON A LABORATORY BASIS; LESS WELL IN A PLANT SITUATION.
- G VALUE INFLUENCED BY IMPURITIES, VAPOR/LIQUID VOLUME RATIOS, QUIESENCE, pH, TEMPERATURE, PRESSURE.
- FOR GAMMA IRRADIATION, BORIC ACID BEHAVES LIKE PURE WATER.
- BASE-BORATE SPRAYS GAVE HIGHER EQUILIBRIUM DECOMPOSITION.
- CHEMICAL EFFECTS OF FISSION PRODUCTS ON DECOMPOSITION NOT WELL UNDERSTOOD.
- PRESENT NRC RADIOLYSIS CRITERIA FOR DBA ARE CONSERVATIVE.

HYDROGEN ANALYSIS

- HAVE TO SAMPLE LIQUID AND VAPOR REACTOR COOLANT WITH:
  - HIGH CONTAINMENT AND SAMPLE RADIATION LEVELS,
  - SAMPLES FROM ABNORMAL LOCATIONS (S.G., R.V., PZR, HIGH POINT),
  - AVOID AIR CONTAMINATION.
- NEED IMPROVED METHODS OF ACCURATE ON-SITE ANALYSIS OF HYDROGEN, OXYGEN.
- NEED IMPROVED METHODS OF ANALYZING CONTAINMENT ATMOSPHERE.

IGNITION

H_2 VOL % IN AIR	IGNITION ENERGY, MJ*
7	.6
10	.17
15	.05
20	.025
30	.020
40	.028

* A MATCH IS ABOUT 1000 MJ - A SPARK THAT CANNOT BE SEEN IN A DARK ROOM CAN IGNITE H_2

H8

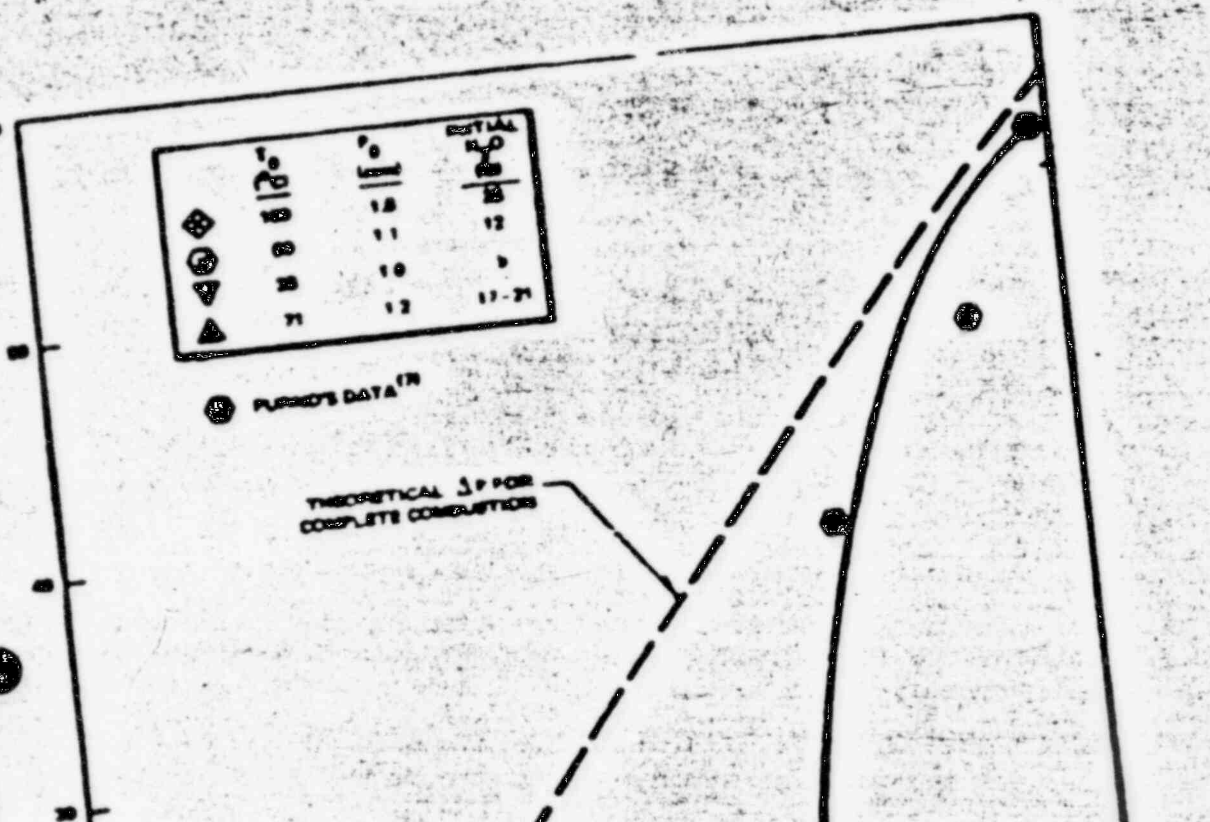


Figure 3-2 Pressure Rise Versus Hydrogen Concentration, Spark Ignition

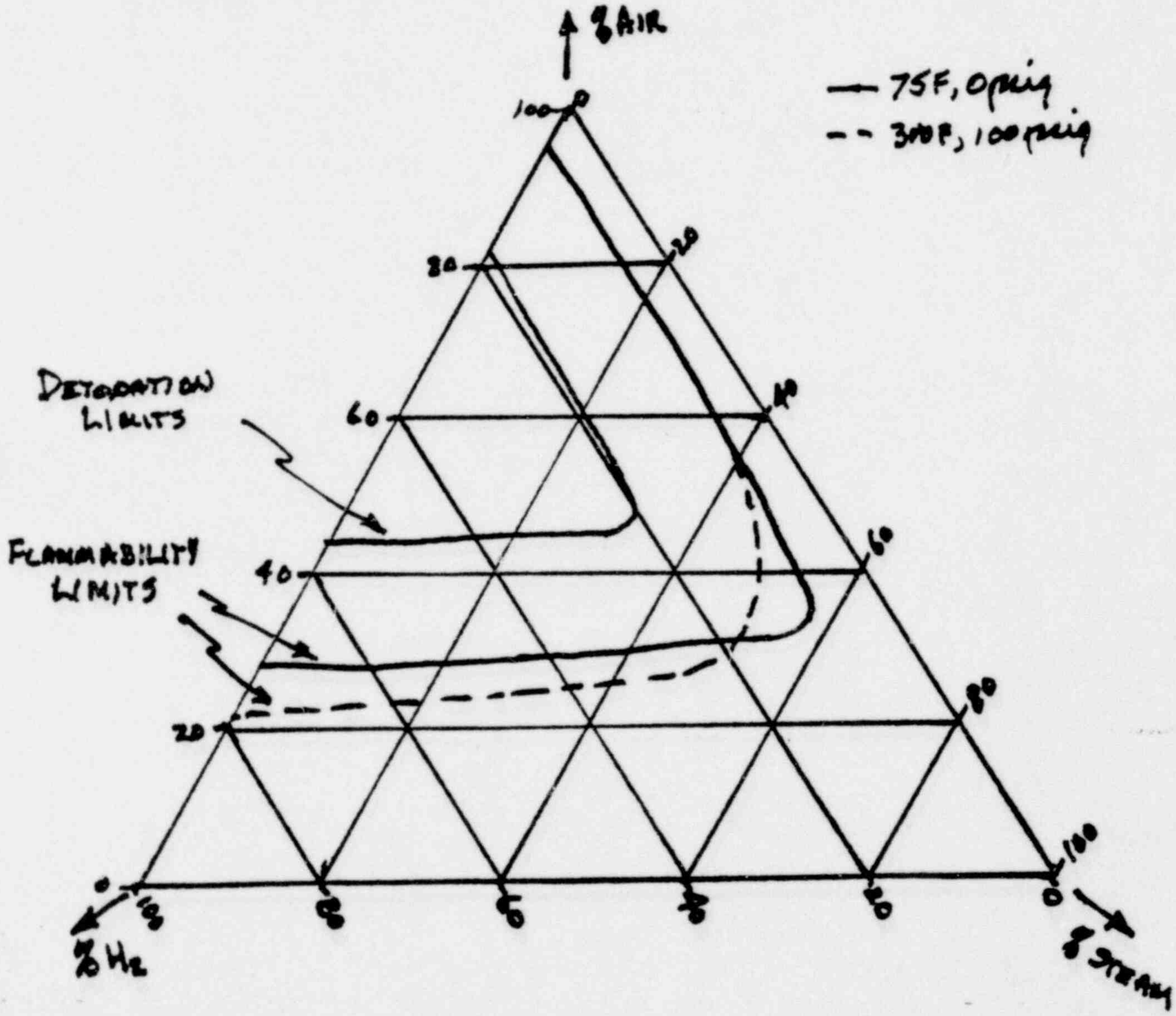
HQ

FLAMMABILITY LIMITS

(H_2 IN AIR, ROOM TEMPERATURE & PRESSURE)

	LOWER LIMIT, %	UPPER LIMIT, %
UPWARD PROPOGATION	4.1	74
HORIZONTAL PROPOGATION	6.0	74
DOWNWARD PROPOGATION	9.0	74

H10

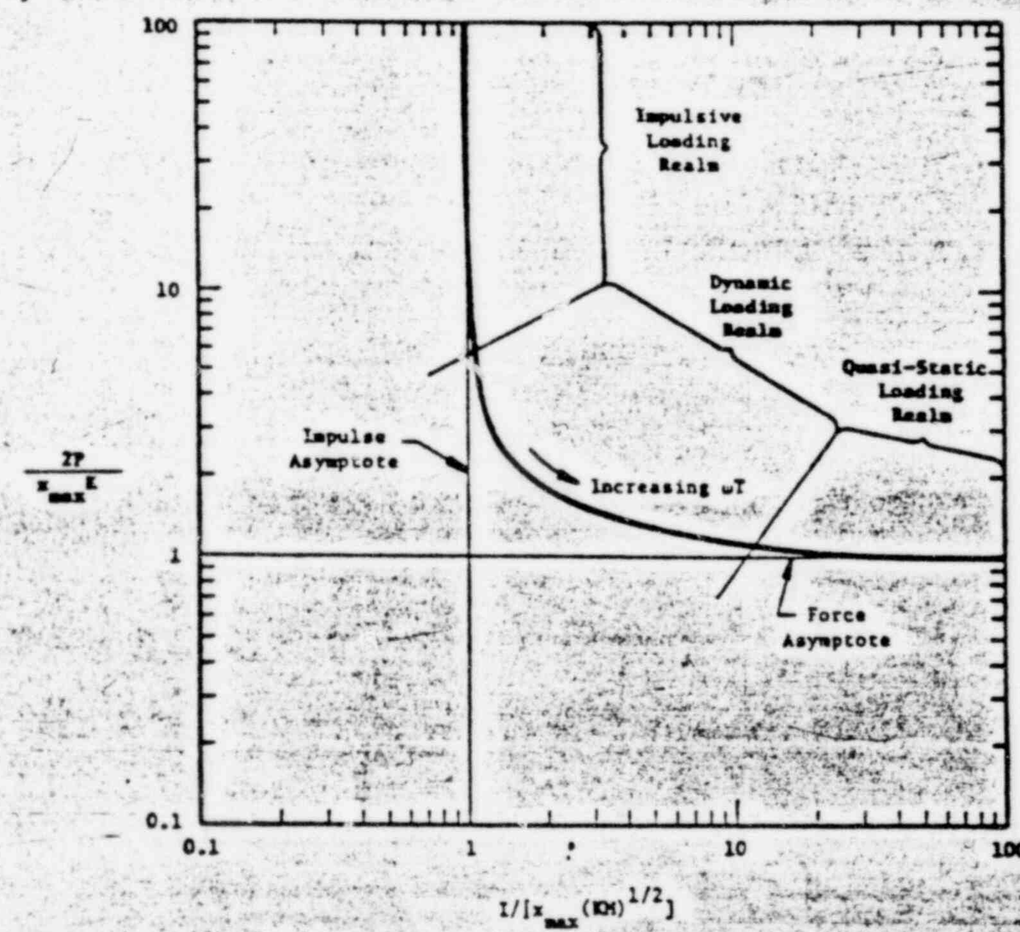


% AIR
— 75F, 0psig
- - 340F, 100psig
100
80
60
40
20
0
20
40
60
80
100
DETONATION LIMITS
FLAMMABILITY LIMITS
% H2
% STEAM

SPEED OF COMBUSTION FRONTS
(HYDROGEN - AIR)

- LAMINAR FLAMES - ½ - 3 M/SEC (QUASI STATIC LOADS)
- TURBULENT FLAMES - 1 - 30 M/SEC (QUASI STATIC LOADS)
- ACCELLERATED TURBULENT - TO 200 M/SEC (DYNAMIC PLUS STATIC LOADS)
- DETONATIONS - 2000 M/SEC (STRONG IMPULSE PLUS STRONG QUASI STATIC)

142



P-I DIAGRAM FOR BLAST LOADED ELASTIC OSCILLATOR

MITIGATION

HALON - FIRE SUPPRESSANT - 20 TO 28% REQUIRED TO INERT - NOT ATTRACTIVE DUE TO COST, DECOMPOSITION PRODUCTS.

DELIBERATE IGNITION - HAS POSSIBILITIES - BURN HYDROGEN AWAY BEFORE HIGH CONCENTRATIONS DEVELOP - HUMAN FACTORS PROBLEMS? RELIABLE IGNITION? STRATIFICATION?

WATER FOG - LOOKS VERY PROMISING - T&P RISE GREATLY REDUCED - DETONATION INHIBITED - RAISES LOWER FLAMMABILITY LIMIT - ONLY ABOUT .05% REQUIRED - SETTLING AND AGGLOMERATION? - NEED EXPERIMENTAL CONFIRMATION - METHOD OF GENERATION & MAINTENENCE

BUDGET - HYDROGEN PROGRAM

FY 80 SUPPLEMENT - $400K (EXCLUDES RADIOLYSIS WORK)

FY 81 - $600K (EXCLUDES RADIOLYSIS WORK)

Post-Accident Coolant Chemistry

My last presentation for today is in the area of post-accident coolant chemistry. The funds for this work are also in the FY 80 supplemental budget request before Congress. There are three items in this category -

CC-1 POST-ACCIDENT COOLANT CHEMISTRY

At this time no committments for this work have been made, but we have been exploring some proposals that are likely to result in research work. I have discussed radiolysis earlier and will not discuss it further here. With regard to fission product signatures -

CC-2 FISSION PRODUCT SIGNATURES

The initial efforts will be to determine if isotopic measurements can resolve these different degrees of failure. Analytical efforts based on theory and fission product release from post-PBF tests will be the first areas investigated and future PBF severe damage tests may offer additional data. There are many complexities that have to be resolved before we are assured of the feasibility of obtaining useful results.

The work on Iodine in Containment is related to a desire to improve estimates of post-accident containment atmosphere iodine -

CC-3 IODINE IN CONTAINMENT

To date we have not received any proposals which appears to meet our needs and we are continuing to seek one.

The budget proposed for this work is as follows -

CC-4 POST-ACCIDENT COOLANT CHEMISTRY

Thank you for your attention.

POST-ACCIDENT COOLANT CHEMISTRY

1. RADIOLYSIS WORK FROM THE HYDROGEN PROGRAM.

2. FISSION PRODUCT SIGNATURES FROM FAILED FUEL.

3. IODINE IN CONTAINMENT.

FISSION PRODUCT SIGNATURES

OBJECTIVE IS TO DETERMINE IF CHARACTERISTIC ISOTOPE SIGNATURES RESULT FROM INCREASINGLY SEVERE FUEL FAILURE -

- PCI CRACKS,
- BALLOON AND BURST,
- PROGRESSING OR STABLE DAMAGE,
- FUEL WASHOUT,
- SMALL FUEL PARTICLES,
- FUEL CRYSTAL STRUCTURE CHANGES,
- $ZR/ZRO_2/UO_2$ LIQUIFIED FUEL,
- FUEL MELTING.

IODINE IN CONTAINMENT

OBJECTIVE - IMPROVE ABILITY TO PREDICT POST-ACCIDENT CONTAINMENT ATMOSPHERE RADIOIODINE.

- WHICH AREAS OF IODINE RELEASE ARE MOST IMPORTANT IN REDUCING UNCERTAINTY IN IODINE RISK?
  - RELEASE FROM FUEL,
  - TRANSPORT CONDITIONS (WATER, STEAM, TWO PHASE, CONDENSATION/EVAPORATION, SCRUBBING),
  - IODINE BEHAVIOR DURING TRANSPORT (TEMPERATURE, PRESSURE, CHEM. FORM, pH, REDOX, IMPURITIES, ABSORBTION, VAPOR/LIQUID DISTRIBUTION).

POST-ACCIDENT COOLANT CHEMISTRY

	FY 80 SUPPL.	FY 81
FISSION PRODUCT SIGNATURES	$200K	$200K
IODINE RISK	$200K	$200K
RADIOLYSIS	$100K	$200K
	$500K	$600K

MECHANISMS FOR FAILURE

	PWR	BWR
SMALL BREAK LOCA (TMI-2)	~150	--
WATERSIDE CORROSION (M-Y, V-Y)	2	30
STRESS CORROSION CRACKING (Conn-Yk)	36*	--
SCC + PCI (LaCrosse)	--	17*
HANDLING (Salem 1, M-Y, Crystal River)	11	--
UNKNOWN (Ft.Calhoun, Rancho Seco,M-Y,Brun 2)	3	7
POSSIBLE PCI (M-Y)	5	--
VIBRATION - FRETTING (Yk-Rowe)	4*	--
	212	54

* - STAINLESS STEEL CLADDING

CLASS 9 ACCIDENT RESEARCH: PROGRAM LOGIC

CLASS 9 ACCIDENTS CHALLENGE CONTAINMENT.PROGRAM OBJECTIVE: DETERMINE BEST ESTIMATE OF RISK ; ANALYSIS AND ASSESSMENT OF SPECIAL FEATURES.

NATURE OF CHALLENGES IDENTIFIED IN WASH-1400:

1. CAN PRESSURES IN PRIMARY SYSTEM BREACH THE SECONDARY? (EVENT V; SG TUBE RUPTURE)
2. CAN A MELTED DOWN CORE BREACH THE PV AND OVER-LOAD THE CONTAINMENT? (DEBRIS BED COOLABILITY; STEAM SPIKE)
3. CAN A HYDROGEN EXPLOSION BREACH THE CONTAINMENT? (HYDROGEN LOADS; HYDROGEN CONTROL; CONTAINMENT RESPONSE)
4. CAN A STEAM EXPLOSION BREACH THE CONTAINMENT? (EXPLOSION EFFICIENCY; PV LOADING)
5. CAN A HOT CORE MELT THE BASEMAT? (CORE CONCRETE INTERACTIONS; CORE CATCHERS)
6. CAN THE CONTAINMENT SLOWLY HEAT UP AND BE OVER PRESSURIZED? (AUXILIARY SPRAYS; FVCS)
7. CAN MAINTENANCE OF VITAL FUNCTIONS BYPASS CONTAINMENT OR THREATEN ITS INTEGRITY?
8. CAN FAILURES IN I & C COMPROMISE SAFETY SYSTEMS?

IMAGE EVALUATION
TEST TARGET (MT-3)

6″

MICROCOPY RESOLUTION TEST CHART

CLASS 9 ACCIDENT RESEARCH: ADVANCED SAFETY TECHNOLOGY

TRANSITION TO DEBRIS BED FROM COOLABLE CORE:

ANALYSIS..............................

OUT OF PILE TESTS......................

CONSTRUCT IN-PILE LOOP.................

IN-PILE TESTS..........................

INTEGRATED FUEL MELT PROGRAM :

(INCLUDES DEBRIS BED COOLABILITY IN-PILE LOOP, ABOVE; FUEL MELT INTERACTION WITH STRUCTURE; AEROSOL RELEASE AND TRANSPORT-THE RADIOLOGICAL SOURCE TERM; STEAM EXPLOSIONS: ENGINEERED SAFETY FEATURES FOR MITIGATION OF ACCIDENTS, INCLUDING DESIGN ASSESSMENT; SYSTEMS INTERACTIONS ANALYSIS AND RISK REDUCTION AND COST STUDIES.)

CONTAINMENT RESPONSE TO ACCIDENT LOADS :

CODE IMPROVEMENTS......................

STRUCTURAL ANALYSIS....................

SYSTEMS INTERACTIONS...................

LMFBRS.................................

(INCLUDES ALL TECHNOLOGY SPECIFICALLY AIMED AT LMFBRS WITH NO OBVIOUS APPLICATION TO LWRS)

PRESENTATIONS TO THE ACRS

REACTOR FUELS SUBCOMMITTEE

BY THE

CORE PERFORMANCE BRANCH

REACTOR FUELS SECTION

APRIL 29, 1980

UNITED STATES
NUCLEAR REGULATORY COMMISSION
WASHINGTON, D. C. 20555

MISCELLANEOUS TOPICS

1. SMALLER NRR FUELS EFFORT.
   WAS 11; NOW 4.

2. RIA PRIORITY TO BE RECONSIDERED BY NRR.
   SEE HOWARD RICHING'S MEMO OF APRIL 15, 1980.

3. NO FURTHER PROGRESS ON SCHEDULE FOR ECCS MODEL REVISIONS.
   NUREG-0630 HAS BEEN ISSUED.

4. SRP-4.2 APPENDIX A OUT FOR PUBLIC COMMENT.
   FEDERAL REGISTER, PAGE 23939, FEBRUARY 27, 1980.

5. GOOD PROGRESS ON BWR FUEL LIFTOFF ISSUE.
   SEE GUS ALBERTHAL'S MEMO OF APRIL 28, 1980.

NRR FY-80 FUELS TECHNICAL ASSISTANCE

TASK	FINANCIAL NO.	LAB	BUDGETED
FUEL CODE APPLICATIONS PROGRAM	B-2170	PNL	$40K
FUEL FAILURE LIMITS	B-2171	PNL	$45K
RADIOACTIVE FISSION GAS RELEASE	B-2169	PNL	$50K
DOR FUELS ON-CALL ASSISTANCE	B-2151	PNL	$30K
ANNUAL REPORT ON FUEL PERFORMANCE	B-2320	PNL	$30K
FUEL CODE APPLICATIONS PROGRAM	A-6268	INEL	$50K*
FUEL ASSEMBLY S&L RESPONSE	A-6157	INEL	$20K
POST-BLOWDOWN FUEL LOADS	A-6269	INEL	$20K
FUEL FAILURE PROPAGATION	A-7116	LASL	$95K
		TOTAL	$380K

* INCREMENTAL FUNDING. ONLY $25K AUTHORIZED AS OF APRIL 15, 1980

NRR FY-79 FUELS TECHNICAL ASSISTANCE

TASK	FINANCIAL NO.	LAB	BUDGETED
FUEL ASSMBLY S&L RESPONSE	A-6157	INEL	$ 60K
FUEL CODE APPLICATIONS	A-6167	INEL	$ 75K
FUEL INTEGRITY PROGRAM	B-2150	PNL	$190K
GE FUEL CODE AUDITS (FY 78 SUPPL.)	B-2150	PNL	$ 12K
DOR FUEL OPERATIONAL PERFORMANCE	B-2151	PNL	$ 60K
		TOTAL	$397K

*LATE FY-78 SUPPLEMENTAL FUNDING, WHICH WAS NOT INCLUDED IN FY-78 BUDGET SUMMARY.

PNL FUEL PERFORMANCE CODE APPLICATIONS PROGRAM (I)

(PNL B-2170)

TASK 1	AUDIT CALCULATIONS FOR TACO-2 AND FATES-REV. ($55K PROPOSED)	-0-
TASK 2	EVALUATION OF EXTENDED BURNUP CODE PROBLEMS.	$30K
TASK 3	GENERAL CONSULTING	$10K
	TOTAL	$40K

LWR FUEL FAILURE LIMITS

(PNL B-2171)

TASK 1	DOCUMENT MECHANISTIC CONCEPTS USED IN PROFIT PCI MODEL.
TASK 2	DETERMINE VALIDITY OF INCUBATION DELAY TIME FOR PCI FAILURE.
TASK 3	DETERMINE SEAF RATIOS FOR CANDU AND INTER-RAMP DATA.
TASK 4	PROVIDE TECHNICAL SUPPORT FOR IMPLEMENTING PROFIT IN LICENSING.

TOTAL $45K

RADIOACTIVE FISSION GAS RELEASE ANALYSIS

(PNL P-2169)

TASK 1	STEADY-STATE RELEASE COMPONENT	\$16K
TASK 2	LOCA TRANSIENT COMPONENT	\$16K
TASK 3	RIA TRANSIENT COMPONENT	\$18K
	TOTAL	\$50K

FUEL OPERATIONAL PERFORMANCE

(PNL B-2151)

ON-CALL ASSISTANCE

TASK 1	CORROSION CALCULATIONS
TASK 2	MIXED-OXIDE FUEL TEMPERATURE CALCULATIONS
TASK 3	REPORT: ASSESSMENT OF CURRENT ONSITE (POOLSIDE) INSPECTION TECHNIQUES FOR LWR FUEL SYSTEMS
TASK 4	(OTHER, AS NEEDED)

TOTAL $30K

FUEL OPERATIONAL PERFORMANCE -- GENERIC

(PNL B-2320)

ANNUAL REPORT OF OPERATING REACTOR
FUEL PERFORMANCE

TOTAL $30K

INEL FUEL PERFORMANCE CODE APPLICATIONS PROGRAM (II)

(INEL A-6268)

TASK 1	SUBMIT FRAP-T5 FOR DSS REVIEW	$ 1K
TASK 2	RESPOND TO DSS QUESTIONS	$18K
TASK 3	MAKE CHANGES IN FRAP-T5 IN RESPONSE TO DSS POSITIONS	$26K
TASK 4	ESTABLISH FRAP-T5 EM ON INEL CDC COMPUTER	$ 5K
	TOTAL	$50K

FUEL ASSEMBLY SEISMIC & LOCA RESPONSE

(INEL A-6157)

TASK 1	TOPICAL REPORT EVALUATION (SUPPLEMENT)	$10K
TASK 2	ON-CALL ASSISTANCE	$10K
	TOTAL	$20K

POST-BLOWDOWN (LOCA) FUEL LOADS

(INEL A-6269)

TASK 1	CALCULATE POST-BLOWDOWN LOADS ON FUEL ASSEMBLIES	$15K
TASK 2	CONVERT EMBRITTLEMENT CRITERIA INTO ALLOWABLE LOADS	$ 5K
	TOTAL	$20K

FUEL FAILURE PROPAGATION

(LASL A-7116)

TASK 1	LITERATURE SURVEY	$10.8K
TASL 2	FUEL FAILURE MECHANISMS AND MECHANICS	$19.5K
TASK 3	CONSEQUENCE OF LOCAL FAILURE	$37.0K
TASK 4	ESTIMATION OF LIKELIHOOD OF FAILURE PROPAGATION	$27.7K
	TOTAL	$95K

1979

ANNUAL OPERATING STATISTICS

	FA	FAILED	W/O TMI-2
26 BWRS	14,342	54	54
44 PWRS	7,334	212	62
TOTALS	21,676	266	116 (1)

(1) FUEL ROD FAILURE - TYPICALLY 2-3 RODS/ASSEMBLY

MECHANISMS FOR FAILURE

	PWR	BWR
SMALL BREAK LOCA	~150	--
WATERSIDE CORROSION	2	30
STRESS CORROSION CRACKING	36*	--
SCC + PCI	--	17*
HANDLING	11	--
UNKNOWN	3	7
POSSIBLE PCI	5	--
VIBRATION - FRETTING	4*	--
	212	54

* - STAINLESS STEEL CLADDING

STRESS CORROSION CRACKING ITEMS

- CONN-YK FUEL CLADDING 304SS
- LACROSSE FUEL CLADDING 348SS
- W UPPER GUIDE TUBE PINS INC. X-750
- W CONTROL RODLET FINGERS 304SS
- GE CONTROL ROD CLADDING 304SS

SINGLE BATCH PROBLEMS

VERMONT YANKEE	#3	CORROSION (30/136)
PRAIRIE ISLAND 1	#4	EXCESSIVE ROD BOW
CONN-YANKEE	#8	SCC (36/48)
SURRY 2	#7	SABOTAGE (64 REWORK)

GENERIC ITEMS

PWR	GUIDE TUBE WEAR
BWR	CONTROL ROD LIFETIME
BWR	WATER ROD WEAR
W	BAFFLE JETTING

OUTLINE

I. PCI LICENSING CONCERNS AND THEIR RELATIONSHIP TO THE FAILURE MECHANISM (SCC, THRESHOLD STRESS, HOLD-TIME ETC. -- RELATIONSHIP TO SHORT-TERM TRANSIENTS).

II. HISTORY OF NRC PCI COMMUNICATIONS WITH INDUSTRY.

III. CURRENT PCI LICENSING CRITERIA

IV. DSS TECHNICAL ASSISTANCE PROGRAMS

V. USER'S NEEDS
   - A. PCI DATA ON HI-BURNUP FUEL
   - B. RIA DATA ON MODERATE-TO-HIGH B.U.

VI. OTHER DOMESTIC AND FOREIGN PROGRAMS

VII. EXTENDED B.U. CONSIDERATIONS

VIII. RECENT DEVELOPMENTS
   - A. DEMO-RAMP II
   - B. RRG
   - C. PROFIT CALCULATIONS AND COMPARISONS

RELATIONSHIP OF LICENSING CONCERNS TO

PCI FAILURE MECHANISM

. GENERAL CONCENSUS EXISTS THAT PCI LIMITS REACTOR POWER CYCLING, BUT OPINION DIFFERS OVER SAFETY SIGNIFICANCE.

. DIFFERENCE OF OPINION STEMS FROM DISAGREEMENT OVER FAILURE MECHANISM.

. INDUSTRY POSITION IS THAT PCI FAILURES ARE CAUSED BY SCC.

. CURRENT SCC THEORY REQUIRES EXTENSIVE HOLD-TIME (ASSOCIATED WITH THRESHOLD FAILURE STRESS). ACCORDING TO INDUSTRY VIEW, PCI FAILURES WOULD NOT BE EXPECTED DURING SHORT-TERM TRANSIENTS AND ACCIDENTS (E.G., ROD WITHDRAWAL, TT W/O BP) BECAUSE HOLD-TIME IS TOO SHORT (SECONDS, NOT MINUTES OR HOURS).

. WE BELIEVE THE HOLD-TIME, THRESHOLD STRESS CONCEPTS ARE NOT YET PROVEN AND, THAT THE SAFETY SIGNIFICANCE OF PCI MUST BE ADDRESSED.

HISTORY OF PCI COMMUNICATIONS WITH

INDUSTRY

- 1972 TO 1976 - PRIMARILY ON INFORMATION-GATHERING PERIOD. PREDICTIVE PCI MODELS FOR LWR TRANSIENTS AND ACCIDENTS WERE NON-EXISTENT.

- 1977 TO PRESENT - MORE AGGRESSIVE POSTURE... AECL/PNL COOPERATIVE PROGRAM SERVED AS FOCAL POINT OF EFFORTS TO PROVIDE PCI MODELING CAPABILITY... PROFIT MODEL DEVELOPED... USER'S NEEDS SENT TO RES... READY TO IMPLEMENT PCI ANALYSIS REQUIREMENT.

CURRENT PCI LICENSING CRITERIA

1% CLADDING STRAIN

. VENDORS ARGUE THAT FUEL ROD CLADDING WILL NOT FAIL BY PCI BECAUSE 1% CLADDING PLASTIC STRAIN IS NOT EXCEEDED.

. BUT OPERATING EXPERIENCE HAS SHOWN THAT PCI FAILURES MAY OCCUR AT TOTAL STRAINS <<1%.

. THEREFORE, SOME OTHER APPROACH MUST BE DEVELOPED FOR PCI (ALTHOUGH 1% STRAIN MAY CONTINUE TO SERVE AS A DESIGN LIMIT FOR OTHER APPLICATIONS).

CENTERLINE UO_2 MELTING

. RELATED TO 1% PLASTIC STRAIN IN ANALYSIS OF "UNCONTROLLED CONTROL ROD ASSEMBLY WITHDRAWAL"... THE CENTERLINE MELTING RESTRICTION IS INTENDED TO PRECLUDE THE SEVERE PCI THAT WOULD OCCUR DUE TO UO_2 VOLUMETRIC EXPANSION ON MELTING.

DSS FY 79 TECHNICAL ASSISTANCE PROGRAM

PNL/AECL COOPERATIVE PROGRAM

- NUREG/CR-1163
- PROFIT
- PCI OGRAM

SCANDPOWER POSHO ANALYSIS OF BWR TT W/O BP

- FIRST TIME POSHO USED TO ANALYZE FAST TRANSIENT.
- 150 CASES CALCULATED -- NO. OF CRACKS DEPENDENT ON INPUT -- VARIED FROM NONE TO THOUSANDS.

INEL FRAP-T ANALYSIS OF FUEL DUTY DURING TRANSIENTS AND ACCIDENTS

- CODE HAS MANY OPTIONS
- RESULTS OF STUDY INDICATED THAT FRAP-T NOT YET READY TO BE USED IN LICENSING ANALYSES OF DESIGN TRANSIENTS AND ACCIDENTS INVOLVING STRONG MECHANICAL INTERACTION.

USER'S NEED REQUESTS

PCI DATA ON HI-BURNUP RODS

. AUGUST 1979 MEMO REQUESTS RES TO DEVELOP AND CARRY OUT EXPERIMENTAL PROGRAM ON HI-BURNUP (≥30,000 MWD/T) RODS UNDER CONDITIONS REPRESENTATIVE AT (1) BWR TTW/OBP AND (2) PWR ROD WITHDRAWAL ATWS.

. FORCUS ON HI-BURNUP REFLECTS OUR CONCERN THAT FAILURE PROPENSITY GENERALLY INCREASES WITH BURNUP.

RIA DATA ON MODERATE-TO-HI-BURNUP RODS

. USER'S NEED MEMO FOR RIA DATA HAS BEEN DRAFTED BUT NOT YET SENT.

. ASKS RES TO TEST ≥20,000 MWD/T BURNUP RODS UNDER CONDITIONS REPRESENTATIVE OF PWR ROD EJECTION AND BWR ROD DROP.

. TWO SEPARATE CONCERNS: (1) PCI DAMAGE THRESHOLD (170 CAL/G?); (2) COOLABLE GEOMETRY (280 CAL/G).

. PRIORITY CURRENTLY UNDERGOING REASSESSMENT.

EXTENDED BURNUP CONSIDERATIONS

- COMMERCIAL REACTOR PCI FAILURE DATA INDICATE B.U. EFFECTS SATURATE AT ~5 TO 10 GWD/T.

- SPERT HI-B.U. DATA (2 RODS) INDICATE FURTHER REDUCTION IN FAILURE THRESHOLD WITH INCREASING BURNUP.

- CUMMULATIVE DAMAGE EFFECTS POSSIBLE.

- NO APPARENT B.U. "CLIFF".

- VENDORS (DOE & EPRI INVOLVEMENT) HAVE A LARGE NUMBER OF EXTENDED B.U. PROGRAMS (FOR STEADY-STATE OPERATION).

- BEFORE LICENSING FOR EXTENDED B.U. WE WOULD EXPECT.

  (1) LEAD BUNDLE EXPERIENCE.
  (2) RESULTS OF FURTHER ANALYTICAL AND EXPERIMENTAL WORK.

<u>MAJOR UNCERTAINTY</u>: TRANSIENT PCI BEHAVIOR -- HOW WILL EXTENDED B.U. RODS BEHAVE DURING A POWER-INCREASING TRANSIENT AT END-OF-LIFE?

RECENT DEVELOPMENTS

DEMO-RAMP II

. 8-12 STANDARD BWR (8X8) RODS WITH BURNUPS >25,000 MWD/T WILL BE RAMPED IN THE R2 AT STUDSVIK.

. MAIN OBJECTIVE: DETERMINE SHAPE (OR EXISTENCE) OF FAILURE THRESHOLD FOR SHORT RAMPS.

RESEARCH REVIEW GROUP

. OBJECTIVE: DEVELOP A COORDINATED EFFORT REGARDING THE PLANNING OF FUTURE PCI ACTIVITIES.

PROFIT CALCULATIONS AND COMPARISONS

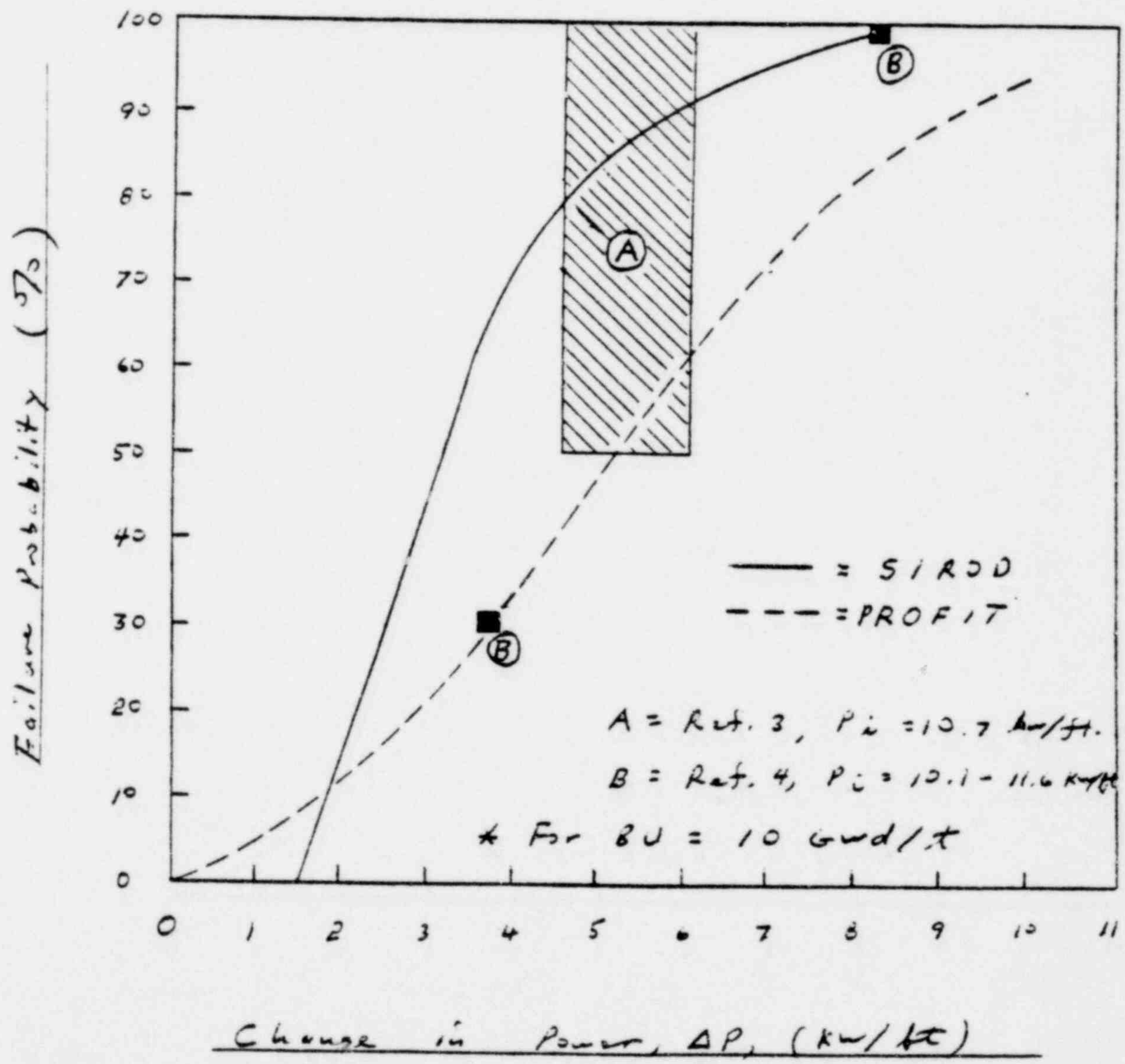
. EPRI-OWNED DATA COMPARISON (5/79) OF 43 PWR & BWR RODS RAMPED IN A EUROPEAN TEST REACTOR." 18.09 ROD FAILURES ARE PREDICTED VERSUS 18 OBSERVED."

. COMPARISONS WITH SIROD AND PREFAIL.

. COMPARISONS WITH RISO AND INTER-RAMP DATA.

. PREDICTIONS OF BWR MSIV CLOSURE, TT W/O BP ETC., PWR ROD WITHDRAWAL ATWS, AND STEAMLINE BREAK.

Comparison of PROFIT Calculations of Failure Probability with SIROD*



PROBABILITY OF FAILURE ESTIMATES

for a

BWR MSIV ATWS

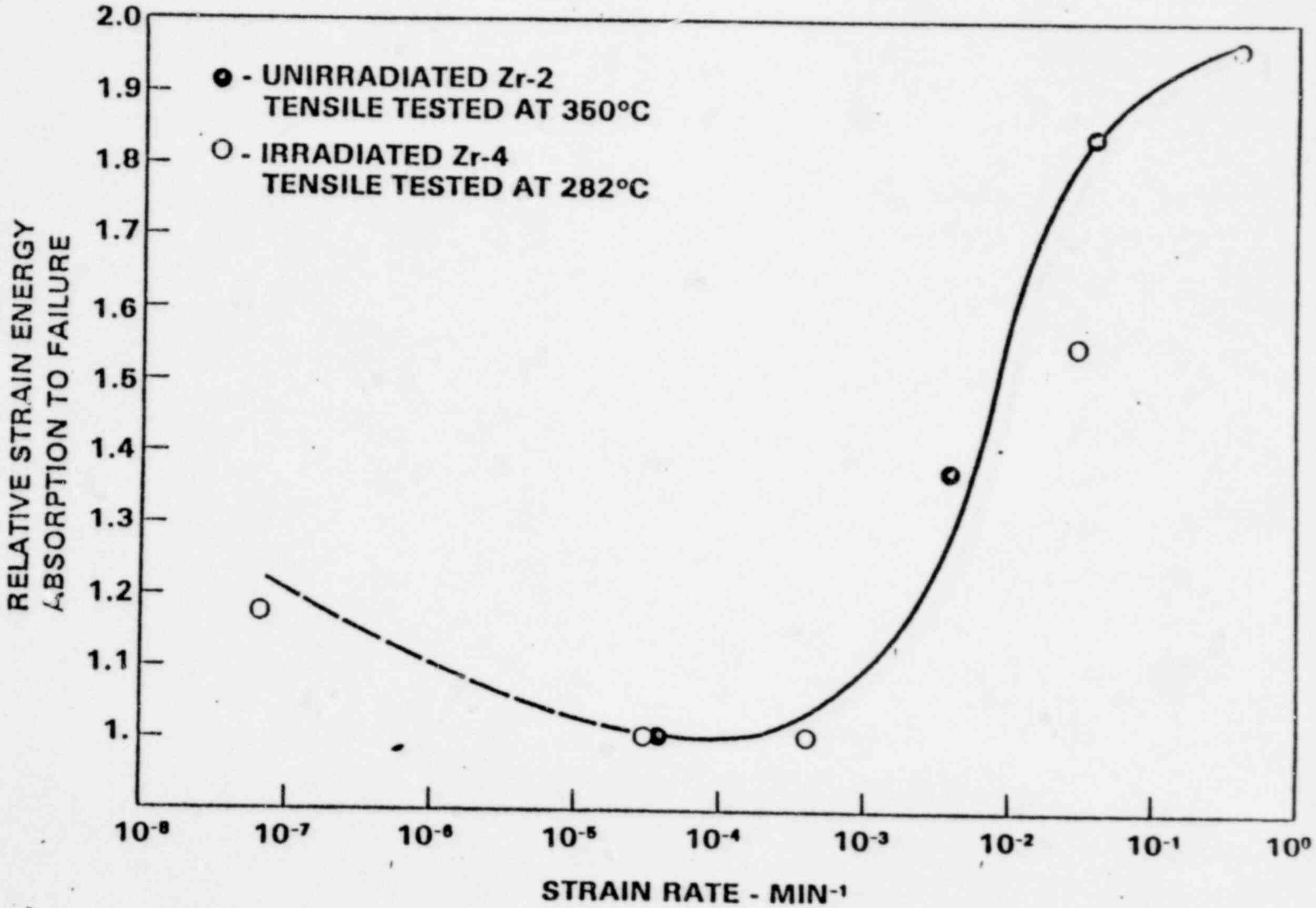
CASE 1. No Power Ramping Rate Correction
($SEAF_o/SEAF_c$ = 1.0)

Bu GWd/TM	Pi Kw/ft	Δ P Kw/ft	POF %
2.0	3.0	1.5	.014
5.0	3.0	1.5	0.47
10.0	3.0	1.5	1.36
2.0	7.0	3.22	1.16
5.0	7.0	3.22	8.23
10.0	7.0	3.22	13.80
2.0	10.0	4.[illegible]	9.15
5.0	10.0	4.17	26.36
10.0	10.0	4.17	32.75
2.0	14.0	5.41	41.8
5.0	14.0	5.41	54.2
10.0	14.0	5.41	54.3

CASE 2. Assumed Power Ramping Rate Correction
($SEAF_o/SEAF_c$ = 1.7)

2.0	3.0	1.5	0
5.0	3.0	1.5	.008
10.0	3.0	1.5	.05
2.0	7.0	3.22	.018
5.0	7.0	3.22	.57
10.0	7.0	3.22	1.5
2.0	10.0	4.17	.48
5.0	10.0	4.17	3.96
10.0	10.0	4.17	6.51
2.0	14.0	5.41	7.5
5.0	14.0	5.41	16.0
10.0	14.0	5.41	16.1

THE EFFECT OF STRAIN RATE ON THE RELATIVE STRAIN ENERGY ABSORPTION TO FAILURE AS DETERMINED FOR AVAILABLE UNIAXIAL TENSILE DATA. (REFS:).



TMI-2 CORE STATUS

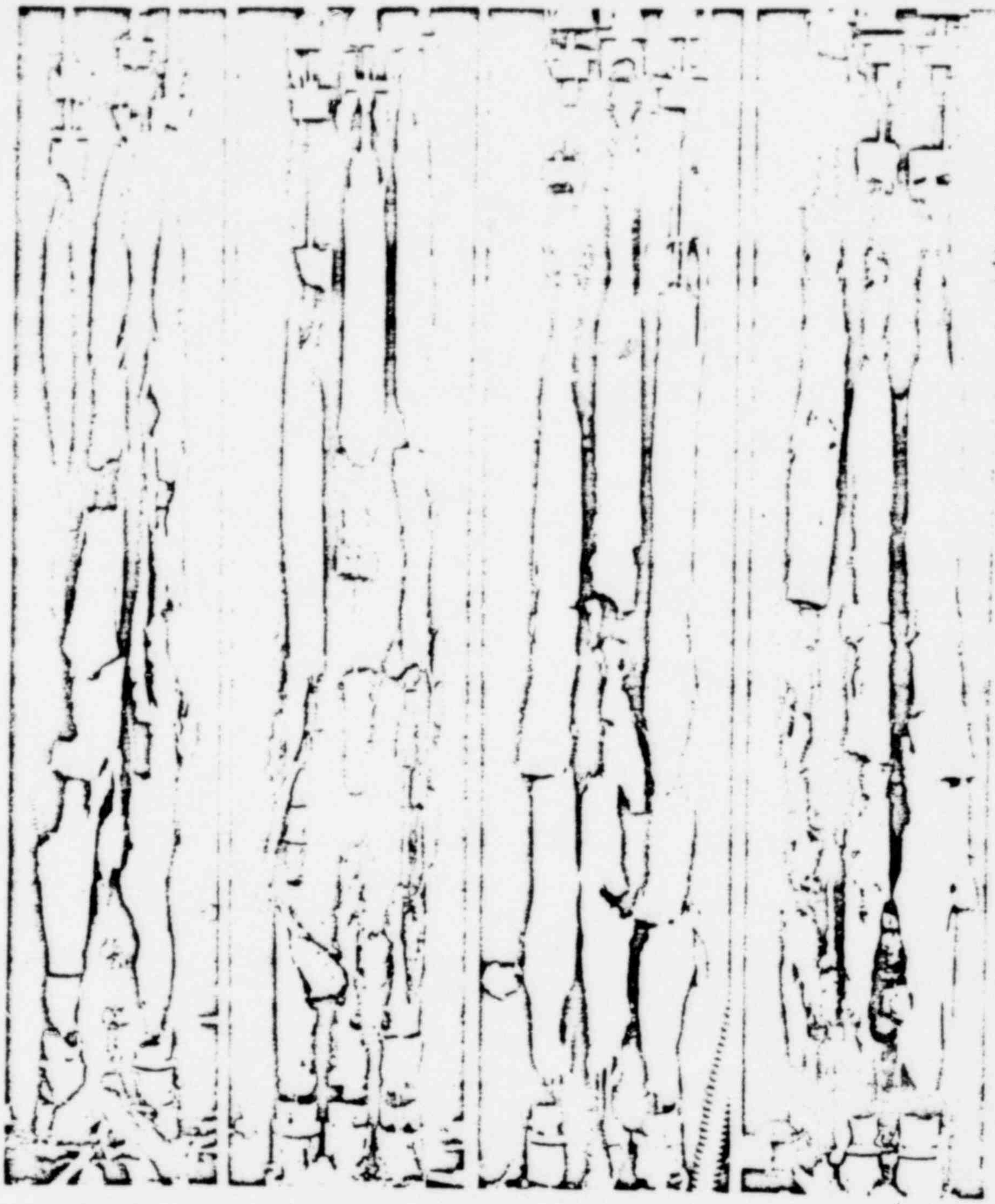
M. L. PICKLESIMER, FBRB/RES

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUELS

APRIL 29, 1980

CORE DAMAGE AT THREE HOURS BASED ON TMIBOIL AND SYSTEM ANALYSES

- FIRST FUEL RODS BURST ABOUT 5 MINUTES AFTER BLOCK VALVE FIRST CLOSED.
- ALL FUEL RODS WERE BURST WITHIN 20 MINUTES AFTER BLOCK VALVE CLOSED.
- ROD BURSTS OCCURRED BETWEEN ONE AND TWO FEET DOWN AT THE CENTER OF THE CORE AND TWO AND THREE FEET AT THE PERIPHERY.
- MAXIMUM TEMPERATURE OF ABOUT 4400°F WAS REACHED IN UPPER THREE FEET OF MORE THAN TWO-THIRDS OF THE CORE, AND 3600°F WAS REACHED FOR ALL OF THE CORE AT THREE FEET DOWN ON THE FUEL RODS.
- EMBRITTLEMENT OF CLADDING BY OXIDATION OCCURRED OVER THE ENTIRE CORE DOWN TO A LEVEL OF ABOUT 4½ FEET FROM THE TOP OF THE CORE.
- A DEBRIS BED ABOUT 2 FEET THICK WAS PROBABLY FORMED WITH A BASE AT ABOUT EIGHT FEET FROM THE BOTTOM OF THE CORE OVER THE ENTIRE CORE AIDED BY THERMAL SHOCK OF EMBRITTLED CLADDING AND "LIQUIFIED FUEL" AT 2 HOURS 54 MIN. WHEN THE RC-P2B WAS STARTED.
- THE DAMAGED CORE WAS ONLY PARTLY QUENCHED BY WATER FROM THE OTSG B, AS MUCH OF THAT WATER ENTERED THE OTSG A THROUGH THE DOWNCOMER AND THE NO. 1 COLD LEG OF OTSG A.
- NOT LESS THAN 300 POUNDS OF HYDROGEN HAD BEEN PRODUCED BY 3 HOURS FROM OXIDATION OF ZIRCALOY FUEL CLADDING.



33	26	19	19	18	17	17		31	31	32	33
32	25	18	26	25		18	25	32		25	26
31		17	33	32	31	19	26	33	17	18	19

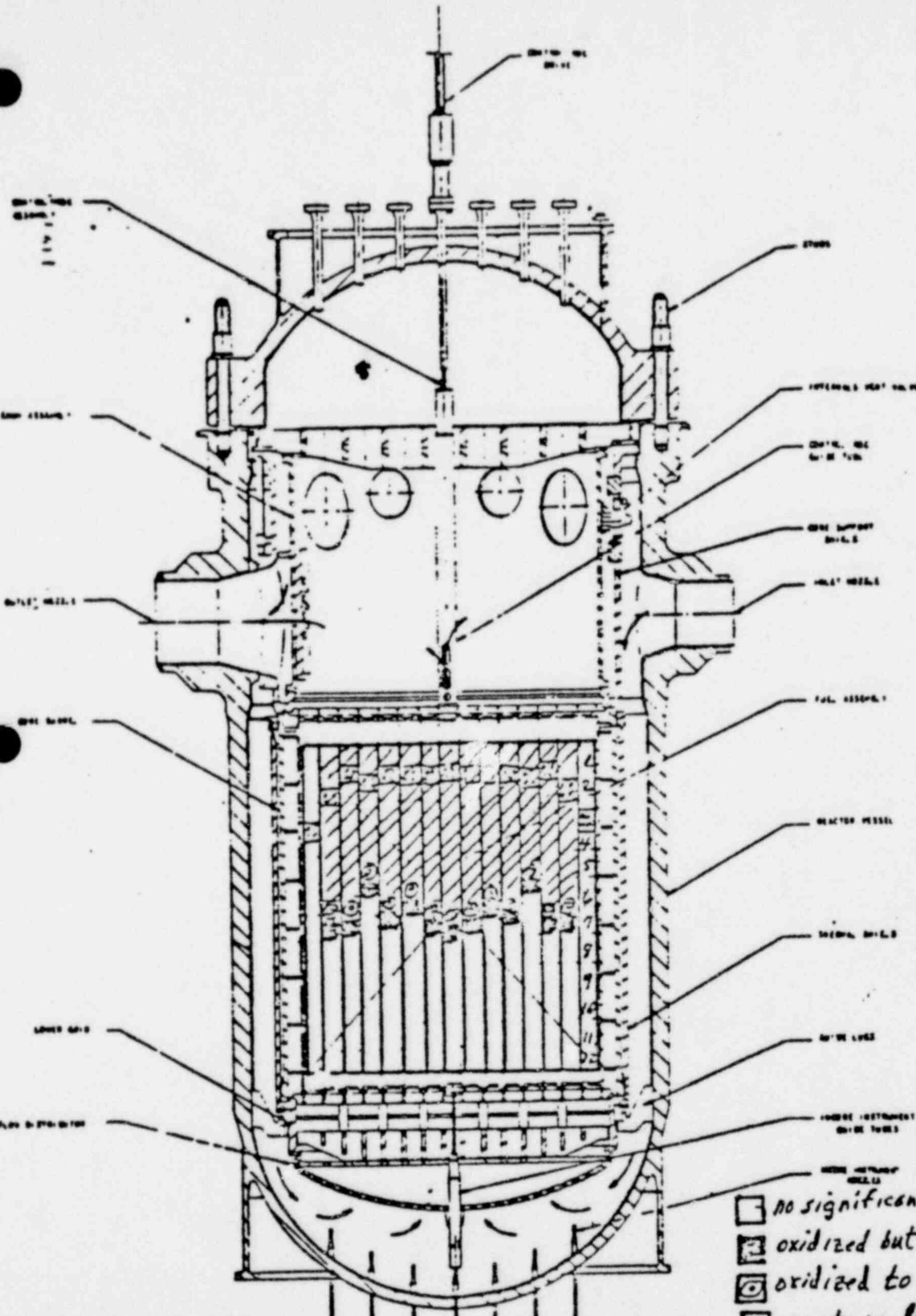
PNS 4321 GfK IT

ABB. 4241-2

DIE VIER SEITEN EINES IN DAMPF MIT 2 °C/SEC AUF 2000 °C ZENTRALSTABTEMPERATUR AUFGEHEIZTEN BÜNDELS. DER ZENTRALE VOLLPELLETSTAB IST VON 7 BEHEIZTEN BRENNSTABSIMULATOREN UMGEBEN. KERAMIKFASERISOLATION.

PNS 4321 GfK IT

ABB. 4241-4: LINKS: BÜNDEL NACH ENTFERNEN DER BEIDEN BEHEIZTEN BRENNSTABSIMULATOREN 17 UND 31. RECHTS: VERGRÖSSERTER MITTLERER BEREICH DES BÜNDELS NACH DEM AUSBAU.



no significant oxidation
oxidized but not brittle
oxidized to brittleness
rupture location
severely oxidized and α-Zr + UO_2 liquid phase formed

ESTIMATED MAXIMUM DAMAGE
TO TMI-2 CORE CLADDING AT 3 HRS.
Decay +oxidation heat.
Heat loss at 25%. Decay
heat at 1% full power
Elevation in feet from
top of core.

REACTOR VESSEL & INTERNALS-GENERAL ARRANGEMENT

THREE MILE ISLAND NUCLEAR STATION UNIT 2

FIGURE 4.2-3
AM. 50 (12-8-76)

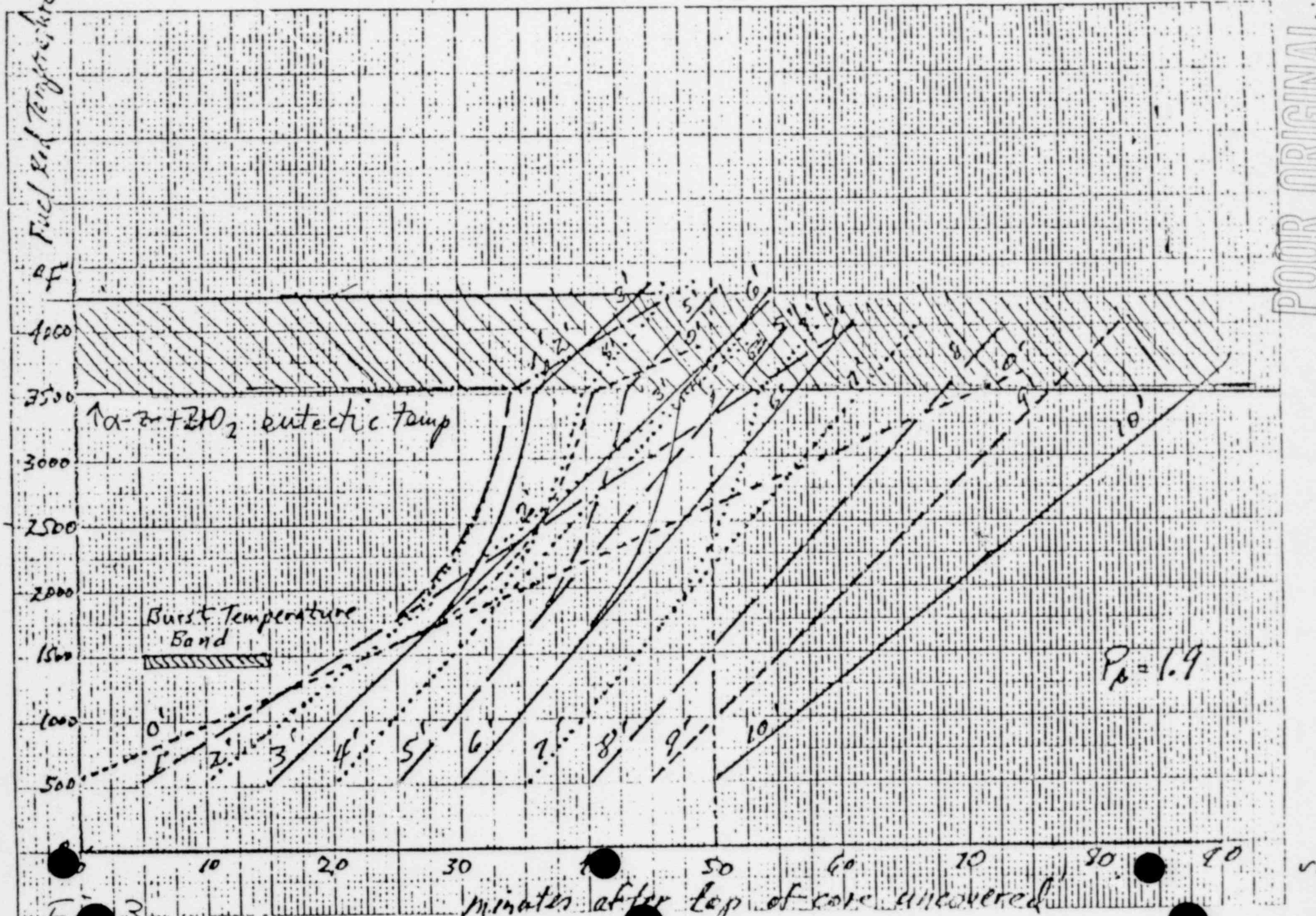

Fuel Rod Temperature
°F
4000
3500
3000
2500
2000
1500
1000
500
0
↑α-Zr+ZrO2 eutectic temp
Burst Temperature
Band
$P_p = 1.9$
0
10
20
30
40
50
60
70
80
90
minutes after top of core uncovered

Fig 3

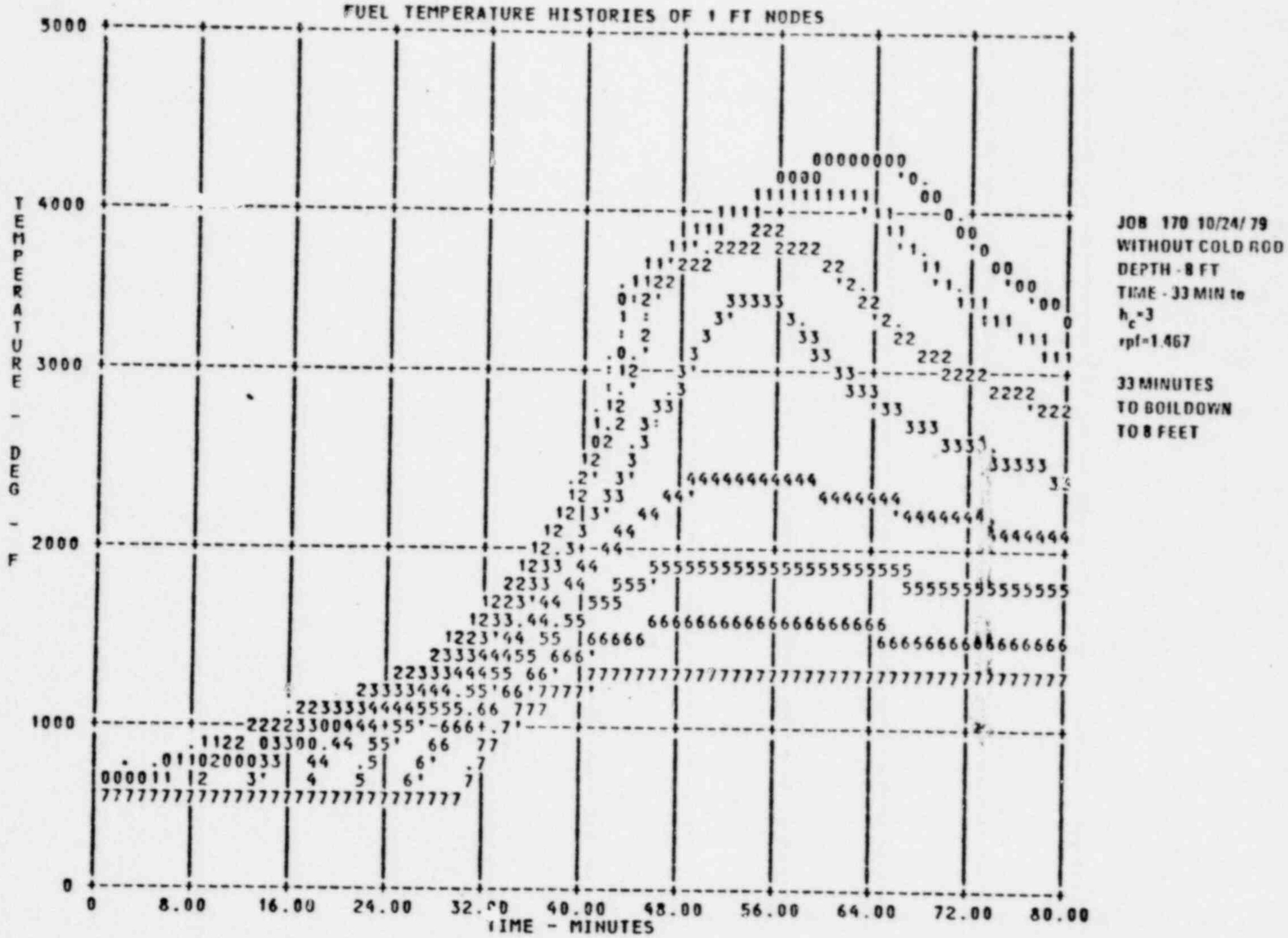


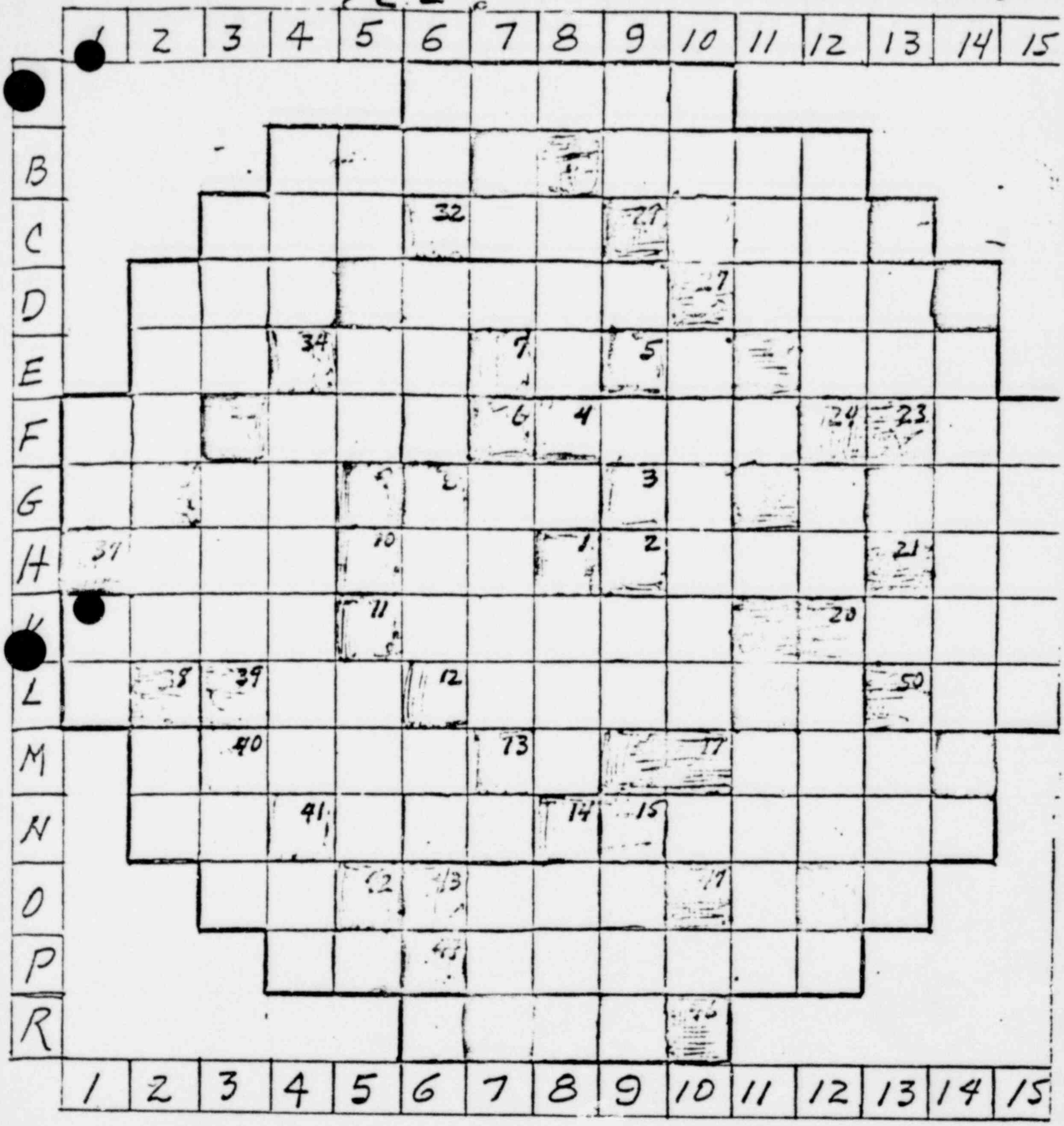
FIGURE II-31. Fuel Temperature Histories

CORE DAMAGE AT FOUR HOURS BASED ON ANALYSIS OF SYSTEM AND ALARM DATA

- MANUALLY READ IN-CORE THERMOCOUPLES INDICATED TEMPERATURES AS HIGH AS 2600°F AT TOP OF THE CENTER OF THE CORE.
- SPNDS AT LEVELS 1 AND 2 ALARMED AT 7:45 O'CLOCK (3:45 ACCIDENT TIME).
- MORE LIQUIFIED FUEL HAD FORMED IN THE DEBRIS BED, SEALING IT FROM STEAM COOLING, AND FORMING A STEAM BUBBLE BELOW THE DEBRIS BED.
- DEBRIS BED DISRUPTURED AT 7:45 O'CLOCK BY A STEAM ERUPTION PRODUCED BY LIQUIFIED FUEL PENETRATING SUBCHANNELS BETWEEN FUEL RODS TO A LEVEL BELOW ONE FOOT FROM BOTTOM OF THE CORE.
- ESTIMATE THAT AT FOUR HOURS, MORE THAN 60% OF THE ZIRCALOY IN THE CORE HAS BEEN EMBRITTLED OR SHATTERED, THE LOWER SURFACE OF THE DEBRIS BED HAS DROPPED TO ABOUT FIVE FEET FROM BOTTOM OF THE CORE, AND LIQUIFIED FUEL HAS PENETRATED TO WITHIN ONE FOOT OF THE BOTTOM OF THE CORE IN SOME AREAS.
- A TOTAL OF 700 AND 820 POUNDS OF HYDROGEN HAD BEEN PRODUCED BY OXIDATION OF ZIRCALOY AT FOUR HOURS.
- ADDITIONAL DAMAGE HAD TO HAVE OCCURRED TO THE STAINLESS STEEL UPPER END FITTINGS, INCLUDING OXIDATION, BUT THE DEGREE OF DAMAGE CAN NOT BE ESTIMATED AT THIS TIME.

3/28/79 Text - ... T/c = ...

06:55-07:13



1 [] ... > ... °F

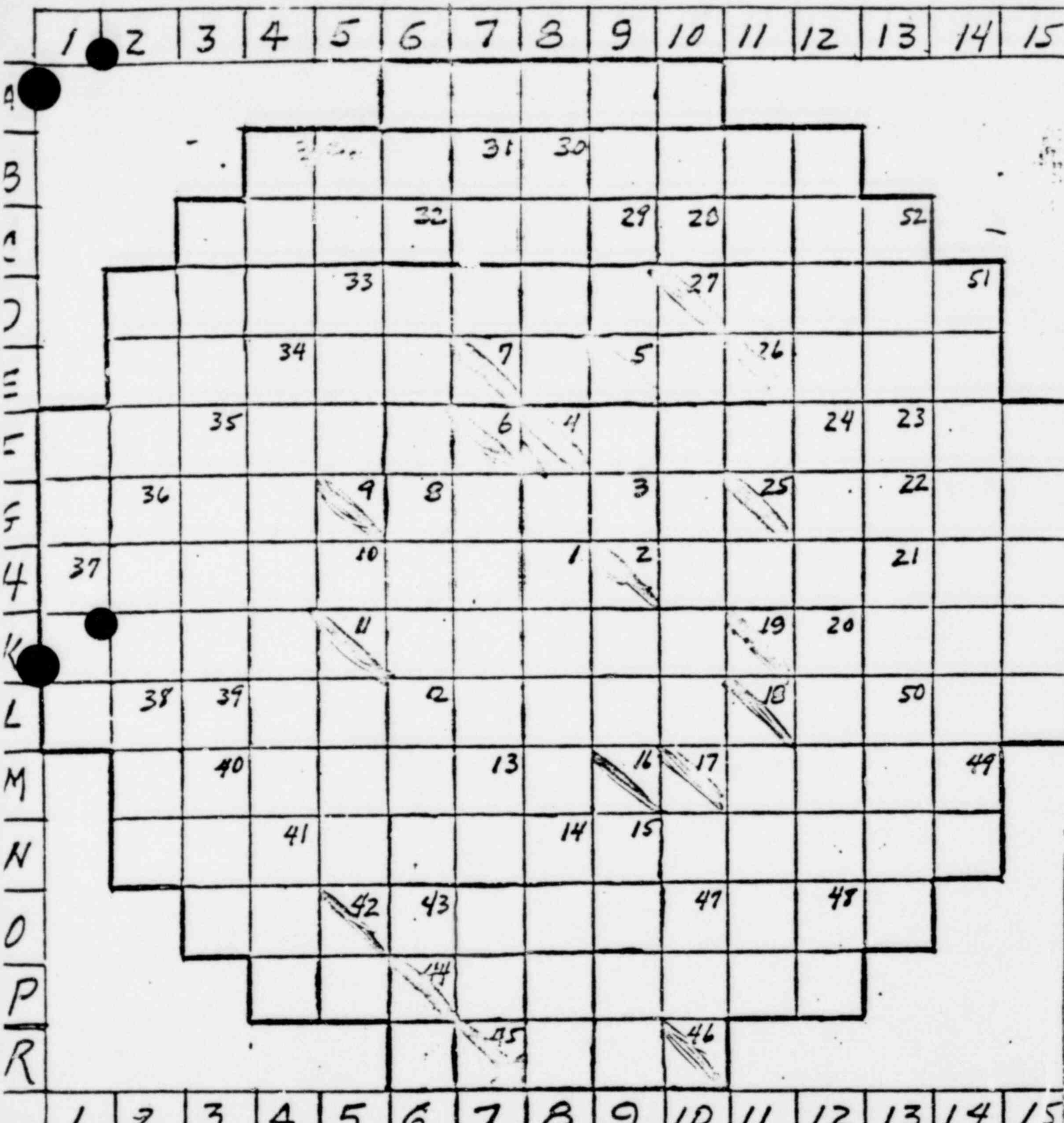
2 [] T/C 650-700°F

3 [] T/C 600-650°F

4 [] T/C 550-600°F

3/28/79 06:55-07:13

IN-CORE T/C's



SPND's at level 1 & 2

off scale at 07:45

off scale at 07:15

off scale at 07:00

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
A															
B							31 355	30 375							
C						32 545			29 1035	28 375			52		
D					33 1275					27 575				51 295	
E				34 1075			7 2055		5 2655		26 405				
F			35 165				6 2441	4 2453				24 405	23 625		
G		36 455			9 2352	8 1849			3 1930		25 1951		22 305		
	37 335				10 2527			1 1370	2 2251				21 1927		
K					11 1886						19 705	20 1775			
L		38 445	39 1575			12 457					18 375		50 1855		
M			40 395				13 2253		16 2402	17 425				49 435	
N				41 485				14 673	15 2242						
O					42 425	43 535				47 1175		48 385			
P						44 375									
R							45 425			46 550					
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15

FIGURE II-28. Temperatures Measured by Incore Thermocouples on March 28, 1979, 8:00 a.m.-9:00 a.m., Using Fluke Meter at Computer Terminal Board

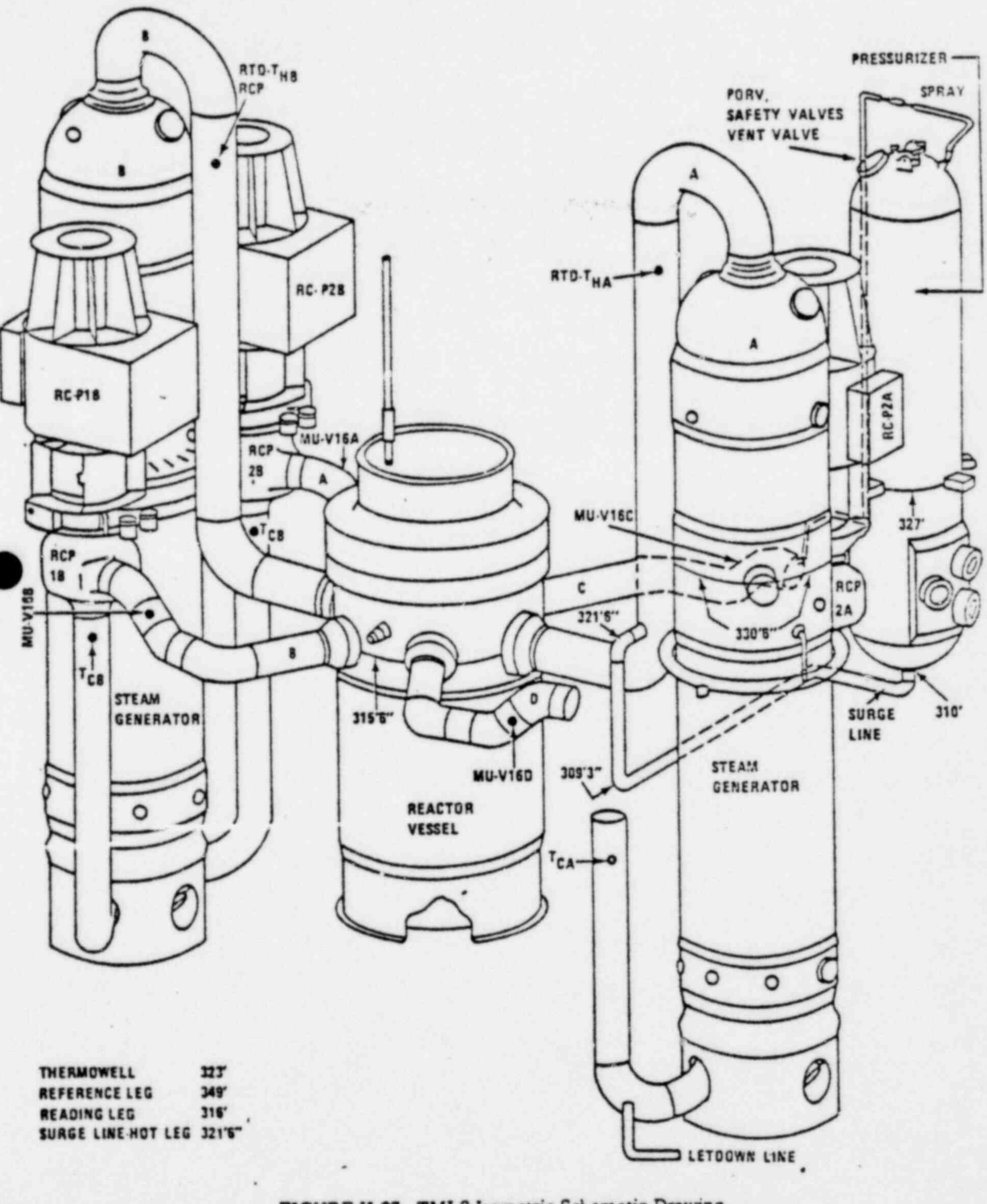


FIGURE II-23. TMI-2 Isometric Schematic Drawing

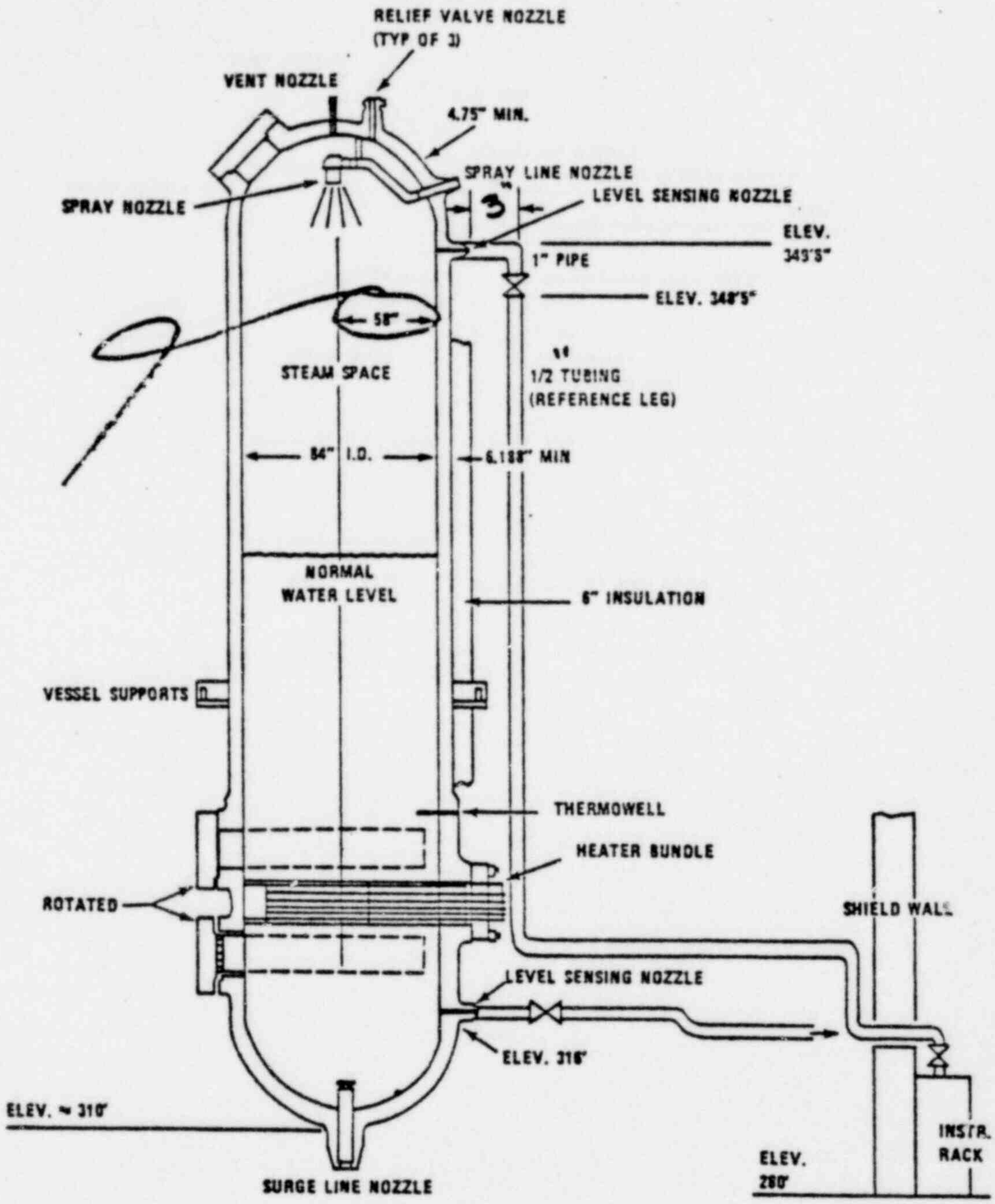


FIGURE II-25. The Pressurizer

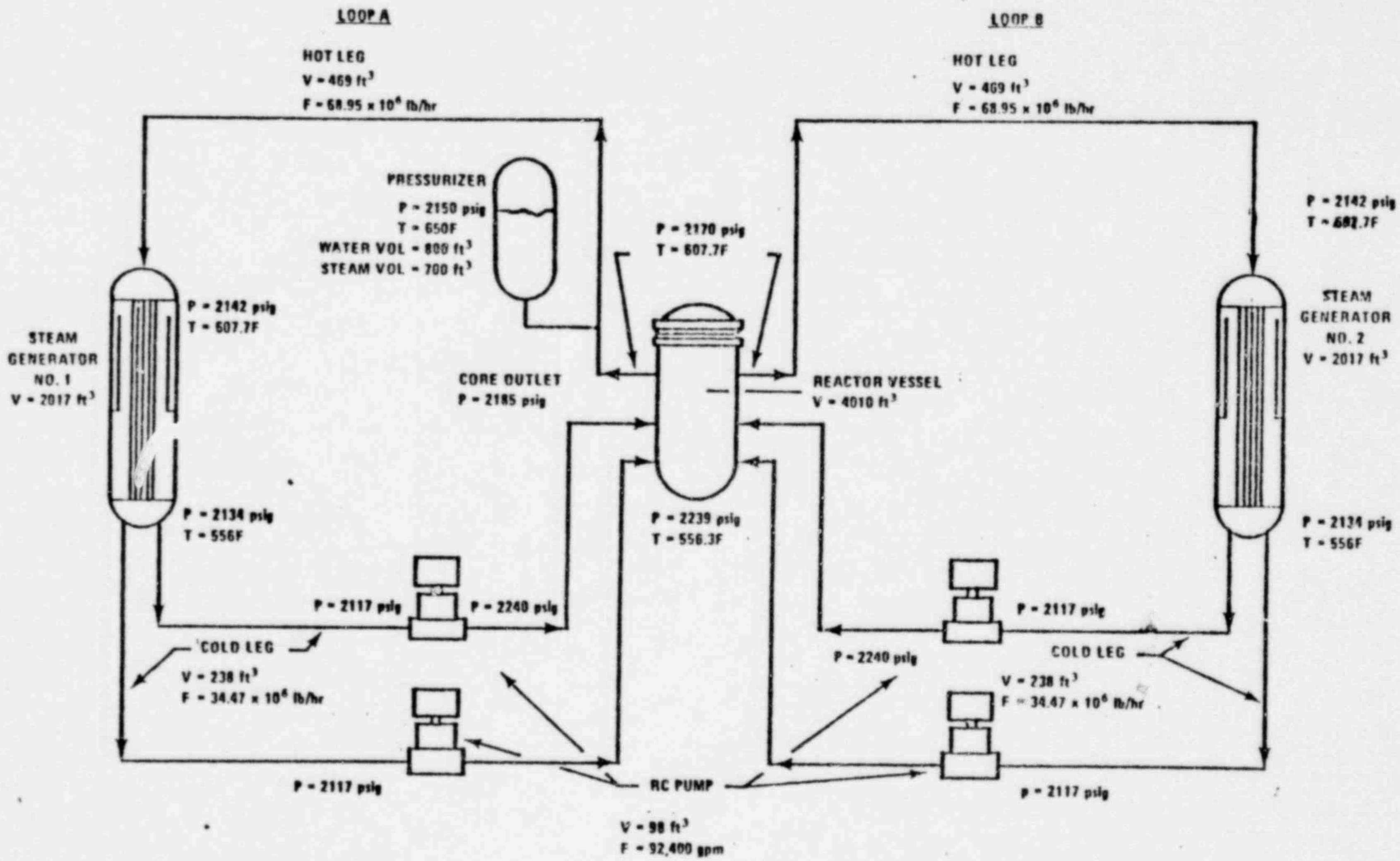
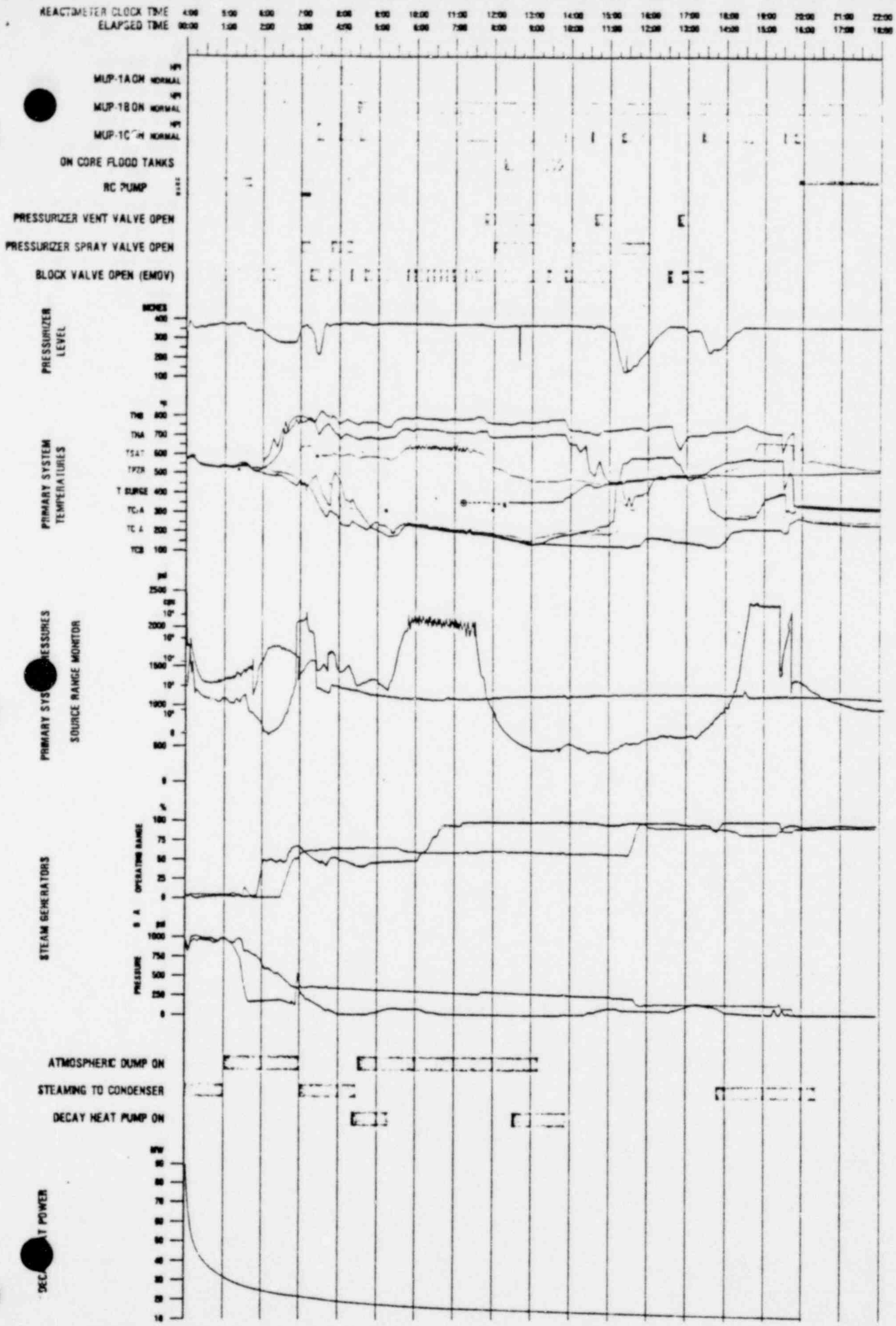


FIGURE II-24. Coolant System Flow Diagram



COLOR PLATE III. PLOT OF SYSTEM PARAMETERS FOR THE FIRST 16 HOURS OF THE TMI-2 ACCIDENT.

POOR ORIGINAL

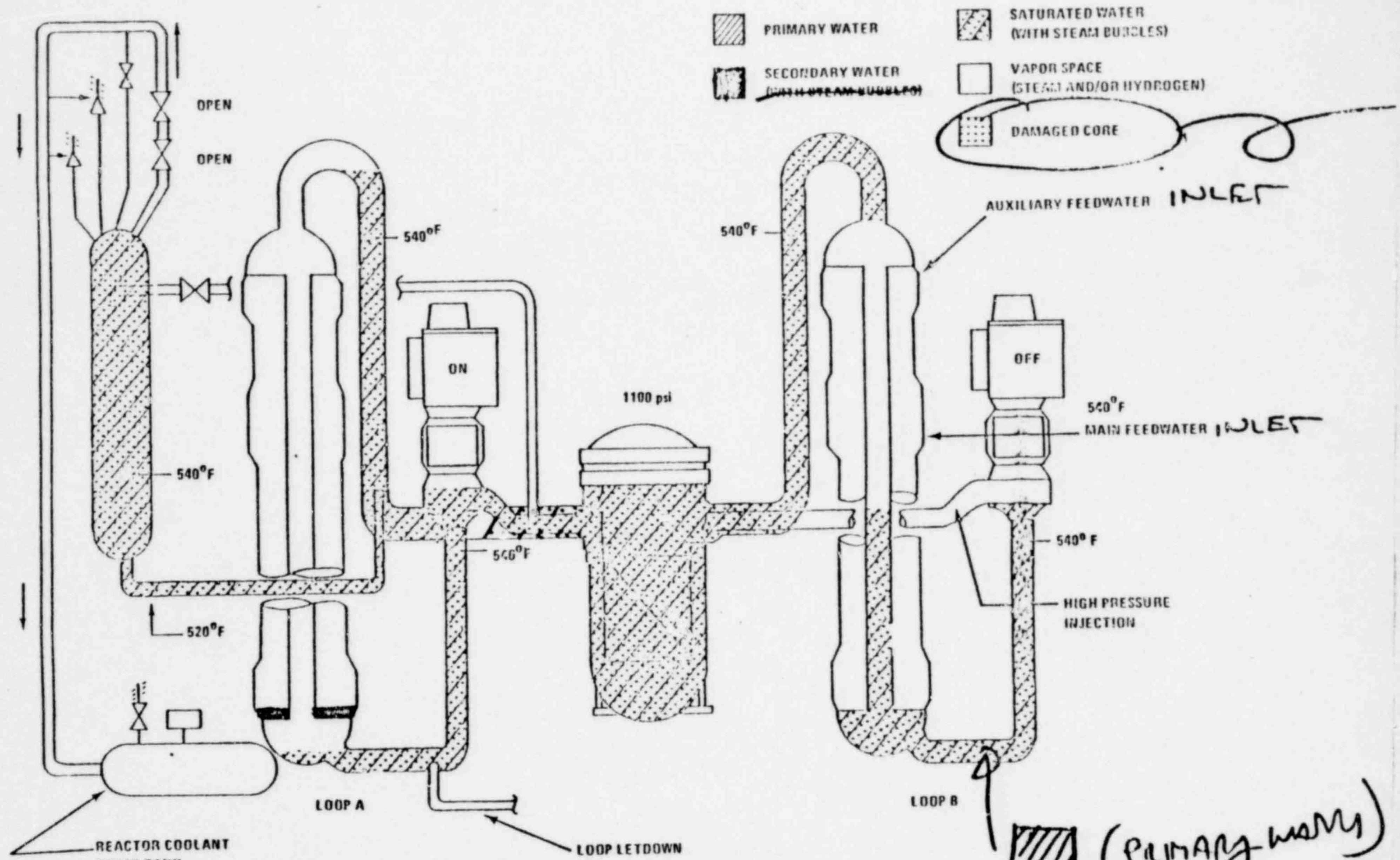
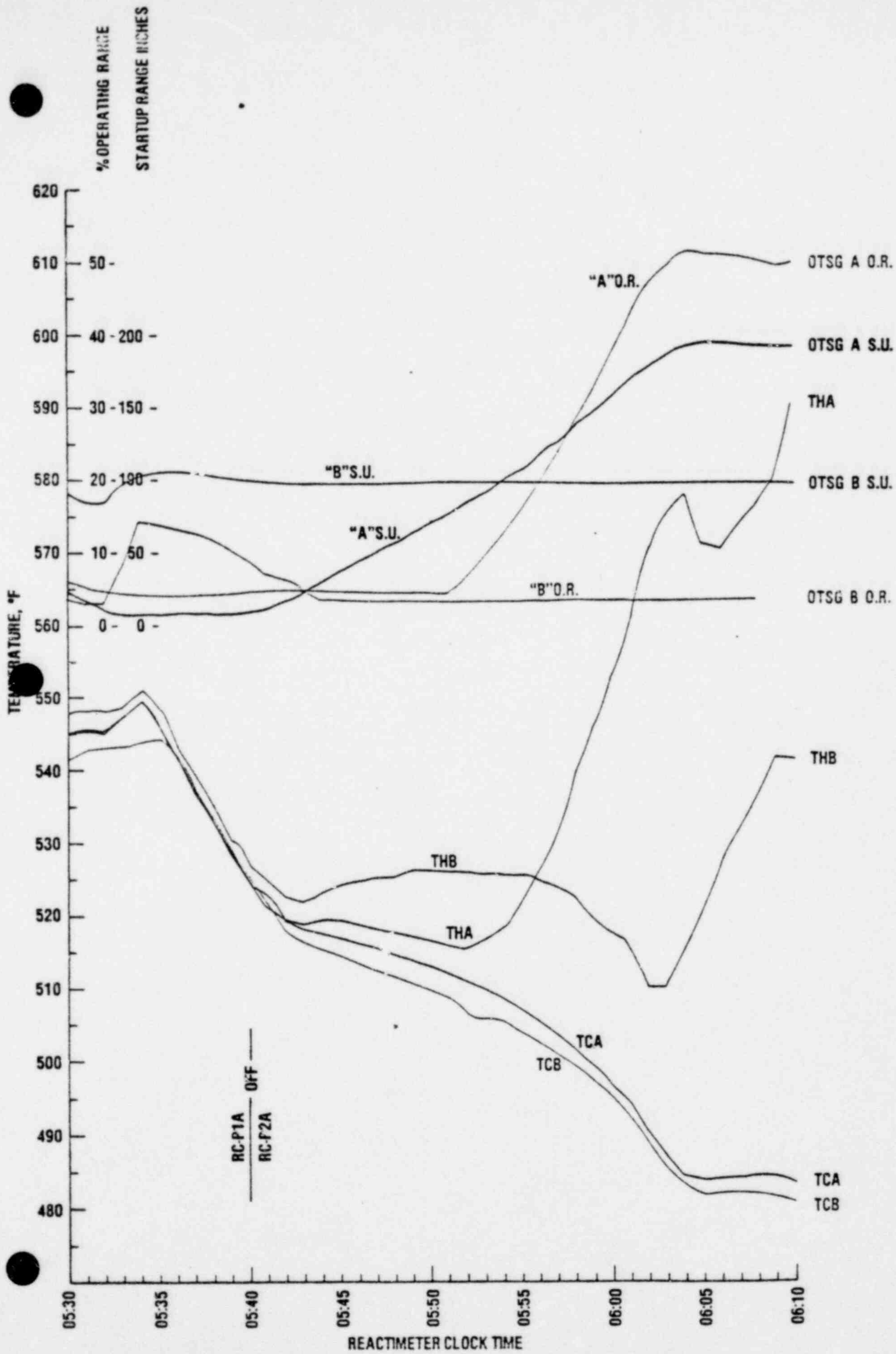
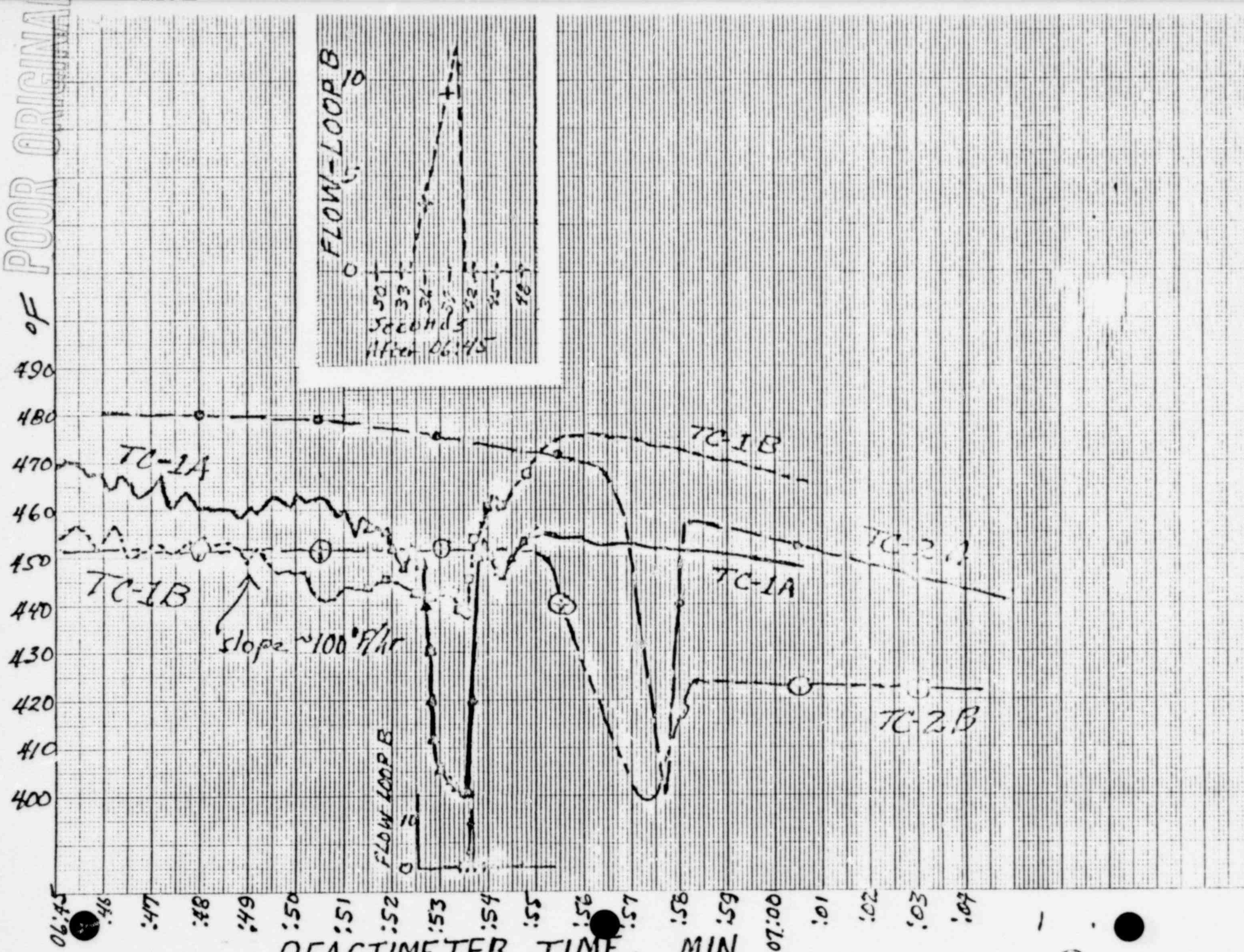


FIGURE II-29. Reactor Primary System at 90 Minutes



COLOR PLATE V. EXPANDED TIME PLOT OF REACTOR LOOP TEMPERATURES AND STEAM GENERATOR


FLOW-LOOP B
%
10
0
50
33
34
22
45
48
Seconds
After 06:45
°F
490
480
470
460
450
440
430
420
410
400
TC-1A
TC-1B
TC-1B
TC-2A
TC-1A
TC-2B
slope ~100°F/hr
FLOW LOOP B
10
0
06:45
:46
:47
:48
:49
:50
:51
:52
:53
:54
:55
:56
:57
:58
:59
07:00
:01
:02
:03
:04
REACTIMETER TIME, MIN

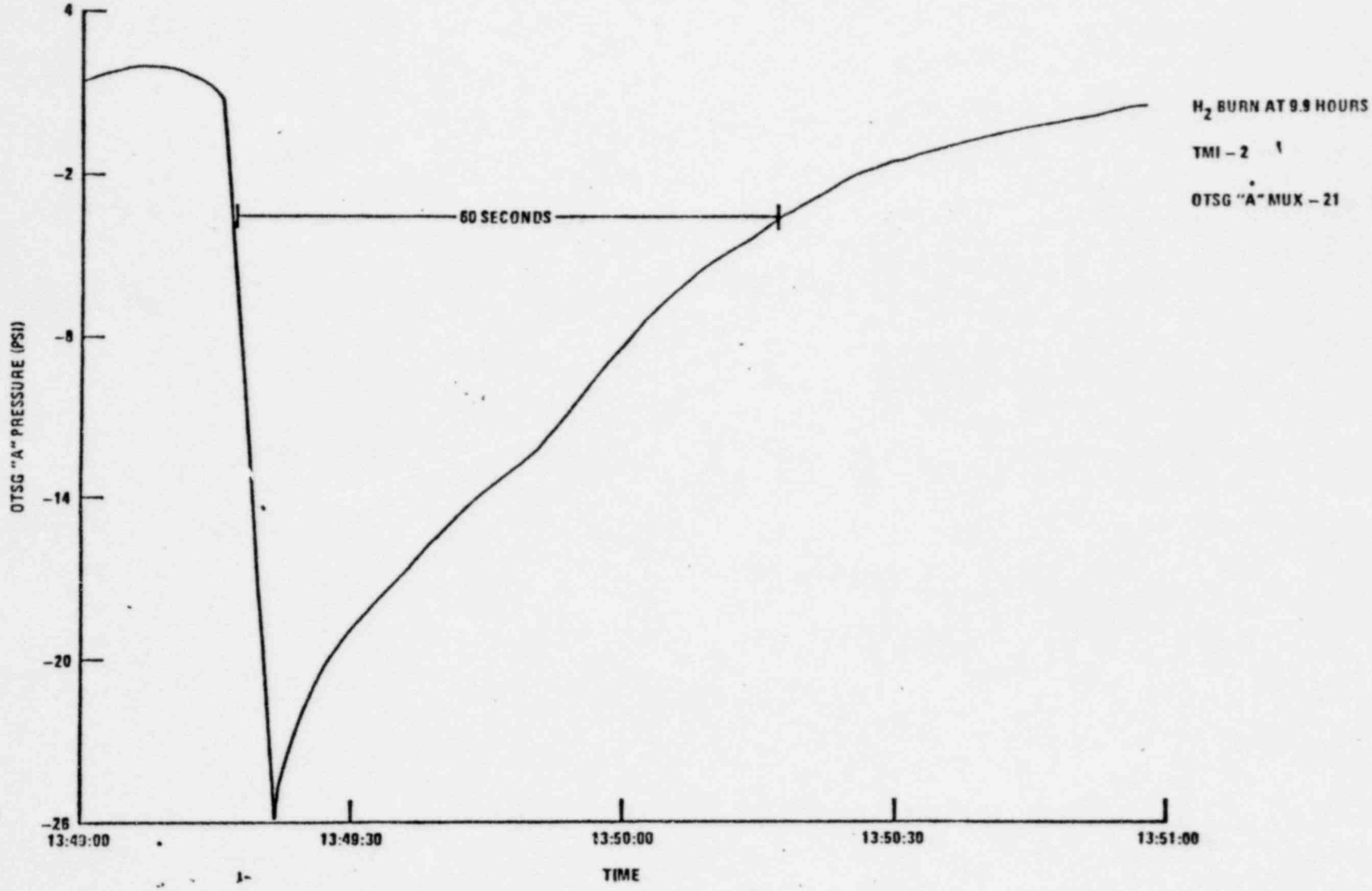
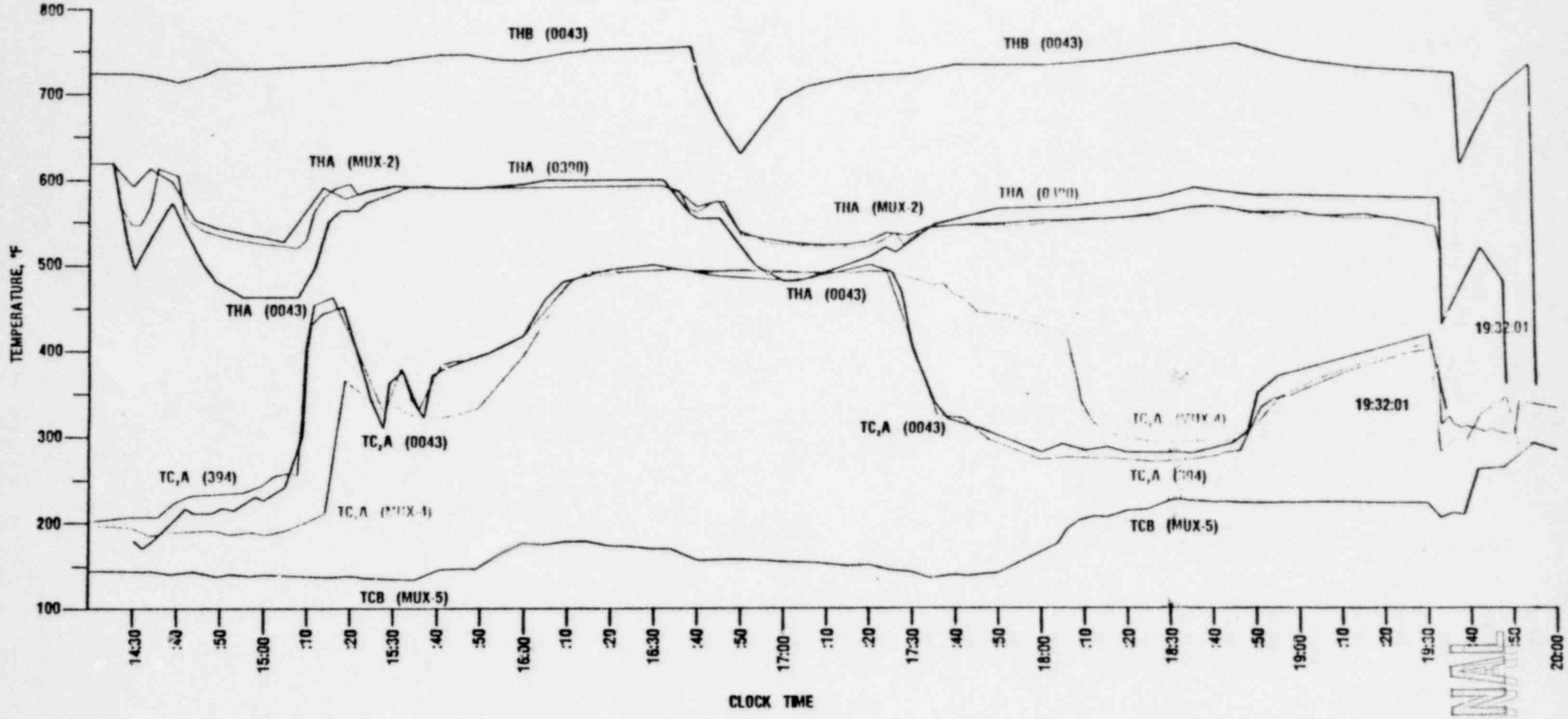


FIGURE II-27. Hydrogen Burn at 9.9 Hours



COLOR PLATE IV. HOT AND COLD LEG TEMPERATURES IN THE LATER HOURS OF THE TMI-2 ACCIDENT.

POOR ORIGINAL

THE BABCOCK & WILCOX CO.

Date ______
Time 1025
RC Temp ______
RC Press ______

SPND ~~STRING~~ NUMBERS AND LOCATIONS - 177 FA CORE

STRING NO

3/29/79

H2 = ... °F
G7 = 558
D13 = 579
E7 = 576
K11 = 58...

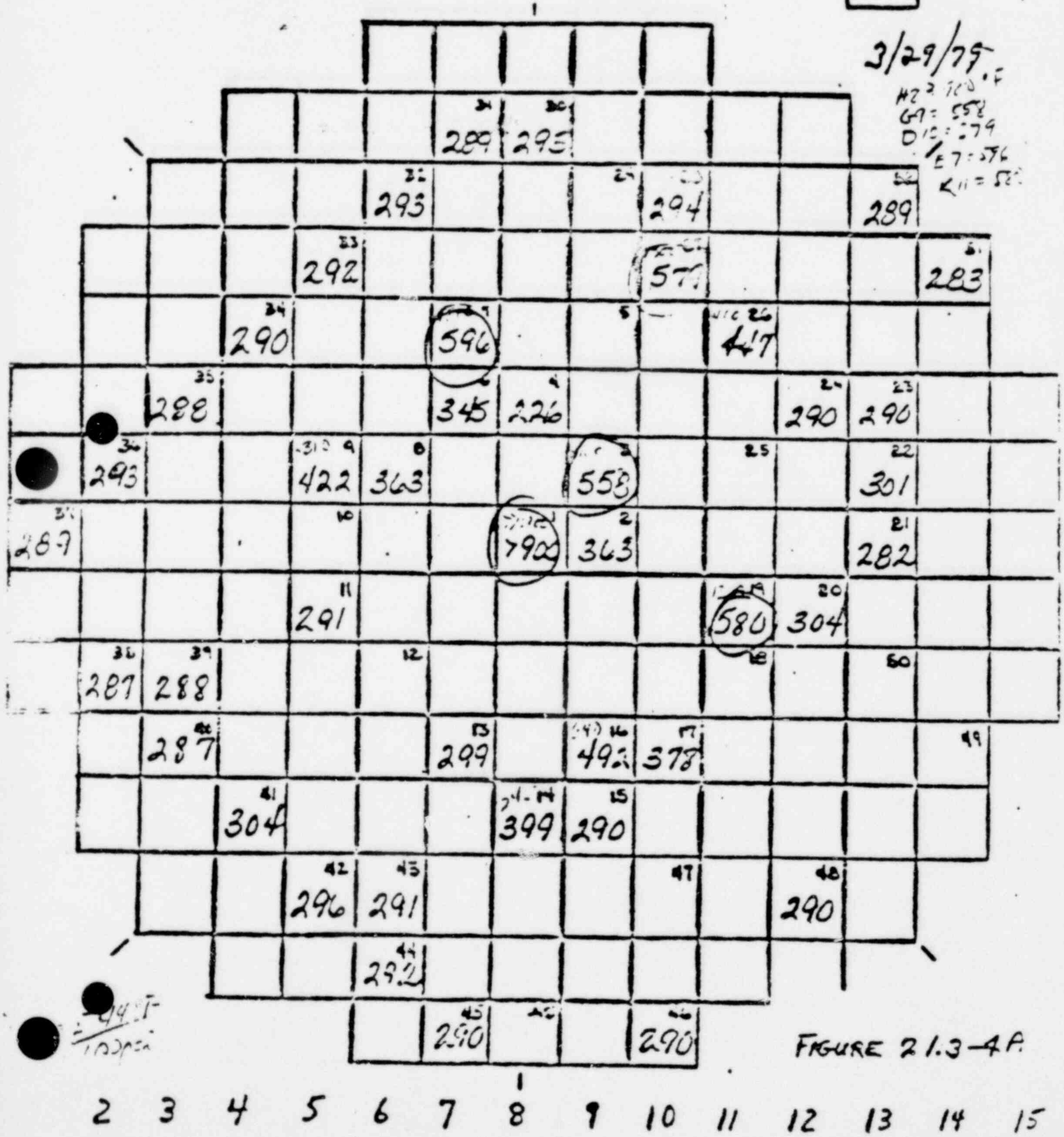


FIGURE 21.3-4A

FUEL BEHAVIOR RESEARCH PROGRAM

OBJECTIVES

- EVALUATE FISSION PRODUCT AND FUEL BEHAVIOR UNDER NORMAL AND ACCIDENT CONDITIONS
- DEVELOP PHYSICAL MODELS THROUGH LAB SCALE SEPARATE EFFECTS TESTS
- VERIFY FUEL CODES AND MODELS THROUGH INTEGRATED TESTS
- UTILIZE MODELS AND CODES TO ASSESS THE CONSEQUENCES OF SEVERE REACTOR ACCIDENTS INCLUDING CORE MELT EVENTS AND TO AID IN THE DESIGN AND EVALUATION OF MITIGATION FEATURES

PRIORITIES

BASED UPON:

A. INFORMATION TO ESTABLISH OR ASSESS LICENSING CRITERIA

B. INFORMATION TO IMPROVE RESPONSE TO OR MITIGATE ACCIDENTS

C. INFORMATION ON FUEL FAILURE MECHANISMS OR FISSION PRODUCT RELEASE

SECONDARY: RELATIVE PROTOTYPICALITY OF DATA
SPECIFIC REQUESTS (NRR, ACRS, ETC.)
RELATION TO RISK REDUCTION

RESULTS

- o CORE DAMAGE BEYOND LOCA
- o CLADDING BALLOONING AND BLOCKAGE
- o FISSION PRODUCT RELEASE AND MIGRATION
- o OPERATIONAL TRANSIENTS - CLASS I, II, AND III
- o FUEL MELTDOWN

FUEL BEHAVIOR TASKS COMPLETED THIS YEAR

FIN #	LAB	TASK DESCRIPTION
A2017	ANL	PHASE 1 - ZIRCALOY EMBRITTLEMENT STUDIES
A4068	BCL	STRENGTH AND DUCTILITY OF IRRADIATED ZIRCALOY
B0124	ORNL	ZIRCALOY CLADDING CREEPDOWN STUDIES (COMPLETION SECOND QUARTER FY 80)
B5948	U. FLA.	TRUE-STRESS TRUE-STRAIN STUDIES
B2043 (TASK B)	BNWL	EX-REACTOR GAP CONDUCTANCE MEASUREMENTS
A2016 (TASK A)	ANL	DEH FISSION PRODUCT RELEASE STUDIES
A1019	SANDIA	MOLTEN CORE INTERACTIONS (EXPERIMENTAL)
B0127	ORNL	FISSION PRODUCT RELEASE FROM LWR FUEL
A2029	ANL	VAPOR EXPLOSION TRIGGERING
B6274	U. MO	GAP CONDUCTANCE STUDIES
A4078	BCL	VAPOR DEPOSITION EXPERIMENTS FOR TRAP
B6706	BCL	IODINE TRANSPORT MECHANISMS

RELATION OF FBRB PROGRAM TO TMI NEEDS

- ASSESS FUEL BEHAVIOR AND CORE DAMAGE FOR FEASIBILITY OF NATURAL CIRCULATION
- DECAY HEAT STANDARD
- ZIRCALOY OXIDATION KINETICS AND H_2 PRODUCTION
- CLAD BALLOONING PREDICTION
- ZIRCALOY EMBRITTLEMENT
- ZR-UO_2 REACTION (GERMAN EXCHANGE)
- STEAM EXPLOSION UNLIKELY
- FUEL AND CLAD THERMAL PROPERTIES (MATPRO)
- TEMPERATURE ESTIMATES FROM FISSION PRODUCT RELEASE
- FAST RUNNING HEAT BALANCE CODE
- SUPPORT NRR (CPB) ANALYSIS

DETAIL BUDGET - FUEL BEHAVIOR

(CONTINUED)

FIN #	CONTRACTOR	TITLE	FY 80	FY 81	FY 82
FISSION PRODUCT RELEASE AND MIGRATION					
*B6747	IN PROCUREMENT	FISSION PRODUCT TRANSPORT ANALYSIS	75	3-5MY	SAME
-----	UND.	TMI FISSION PRODUCTION IN CONTAINMENT	(175)	85	UP
B0127	ORNL	FISSION PRODUCT RELEASE AT HIGH TEMPERATURES	(365)	400	UP
A2016	ANL	TRANSIENT FISSION GAS RELEASE AND MODELING	150	105	UP
	NRL	IODINE FILTER AFFECTIVENESS TESTING	(110)	115	SAME
*-----	---	FISSION PRODUCT TRANSPORT VERIFICATION FACILITY	---	---	UP
*-----	---	FISSION PRODUCT RELEASE FROM MOLTEN FUEL	---	---	UP
A1227	SAN	SEPARATE EFFECTS STUDIES FOR TRAP	150	210	COMPLETE
-----	---	LEACHING OF FISSION PRODUCTS FROM FUEL	---	100	SAME
-----	---	MITIGATION OF LIQUID PATHWAYS	---	---	UP
		RELEASES (CONTAINM[illegible] BYPASS)		2280	UP

DETAIL BUDGET - FUEL BEHAVIOR

FIN #	CONTRACTOR	TITLE	FY 80	FY 81	FY 82
CORE DAMAGE BEYOND LOCA					
B7084	ANL	EXAMINATION OF TMI FUEL	(350)	500	UP
B5702	PNL	CORE DEGRADATION IN ESSOR	---	1695	UP
B7100	SAN	HYDROGEN HANDBOOK AND DATA BASE	(500)	800	DOWN
UND.	EG&G	SEVERE CORE DAMAGE - PBF	(1900)	2135	UP
B7281	---	INCIPIENT FUEL-CLAD MELTING	---	300	SAME
B2372	---	POST-ACCIDENT COOLANT CHEMISTRY	---	400	DOWN
-----	---	DEBRIS COOLABILITY STUDIES	---	---	UP
-----	---	MODELING OF SEVERE CORE DAMAGE	---	---	UP
B7200	---	REACTOR CHEMISTRY	(400)	400	UP
				6530	UP
	UND.	INLET FLOW BLOCKAGE TESTS	----	1000	UP
CLADDING BALLOONING AND BLOCKAGE					
B2277	PNL	LOCA BUNDLE REFLOOD IN NRU	3015	1875	UP
B0120	ORNL	MULTIROD BURST TEST	960+(250)	900	DOWN
-----	UND.	RESIDENT ENGINEER-CADARACH, FRANCE	100	155	SAME
~~A6041~~	~~EG&G~~	~~LOCA TEST IN PBF~~	1150	1175	DOWN

DETAIL BUDGET - FUEL BEHAVIOR

(CONTINUED)

FIN #	CONTRACTOR	TITLE	FY 80	FY 81	FY 82
OPERATIONAL TRANSIENTS AND INITIAL CONDITIONS					
A6050	EG&G	FRAP AND FRAPCON CODE DEVELOPMENT	690	730	DOWN
A6046	EG&G	CODE ANALYSIS AND ASSESSMENT	245	260	SAME
A6041	EG&G	OPERATIONAL TRANSIENTS - PBF	3000	3060	DOWN
B2043	PNL	EXPERIMENTAL SUPPORT AND DEVT. OF SINGLE ROD FUEL CODES	430	570	UP
A2017	ANL	STRESS RUPTURE OF IRRAD. CLADDING	450	370	UP
B5531	NRC HQ	HALDEN PROJECT MEMBERSHIP	477	490	SAME
A6041	EG&G	PCM, RIA TESTS IN PBF	2761	2880	DOWN
-----	UND.	MODELING OF OPERATIONAL DAMAGE TO ZIRCALOY	----	----	UP
B7202	UND.	LONG BUNDLE TESTS	----	----	UP
-----	UND.	RESIDENT ENGINEER - NSRR JAPAN	----	(150)	SAME
VARIOUS	EG&G	PBF OPERATION AND SUPPORT	6012	5721	DOWN
B6746	-----	H_2 GENERATION IN CONTAINMENT	100	149	UP
				15230	DOWN

DETAIL BUDGET - FUEL BEHAVIOR
(CONTINUED)

FIN #	CONTRACTOR	TITLE	FY 80	FY 81	FY 82
FUEL MELTDOWN*					
A1030	SAN	STEAM EXPLOSIONS	500+140	915	DOWN
A1019	SAN	MOLTEN CORE/CONCRETE INTERACTIONS	194+(56)	210	DOWN
-----	UND.	FUEL MELTDOWN SYSTEMS CODES	----	(200-500)	UP
-----	UND.	FUEL MELT MITIGATION FEATURES EVALUATION	----	(300)	UP
-----	UND.	RESIDENT ENGINEER - KARLESRUHE	100	130	SAME

* INCLUDED IN INTEGRATED FUEL MELTDOWN PROGRAM.

FUEL CODE DEVELOPMENT AND EVALUATION

OBJECTIVES:

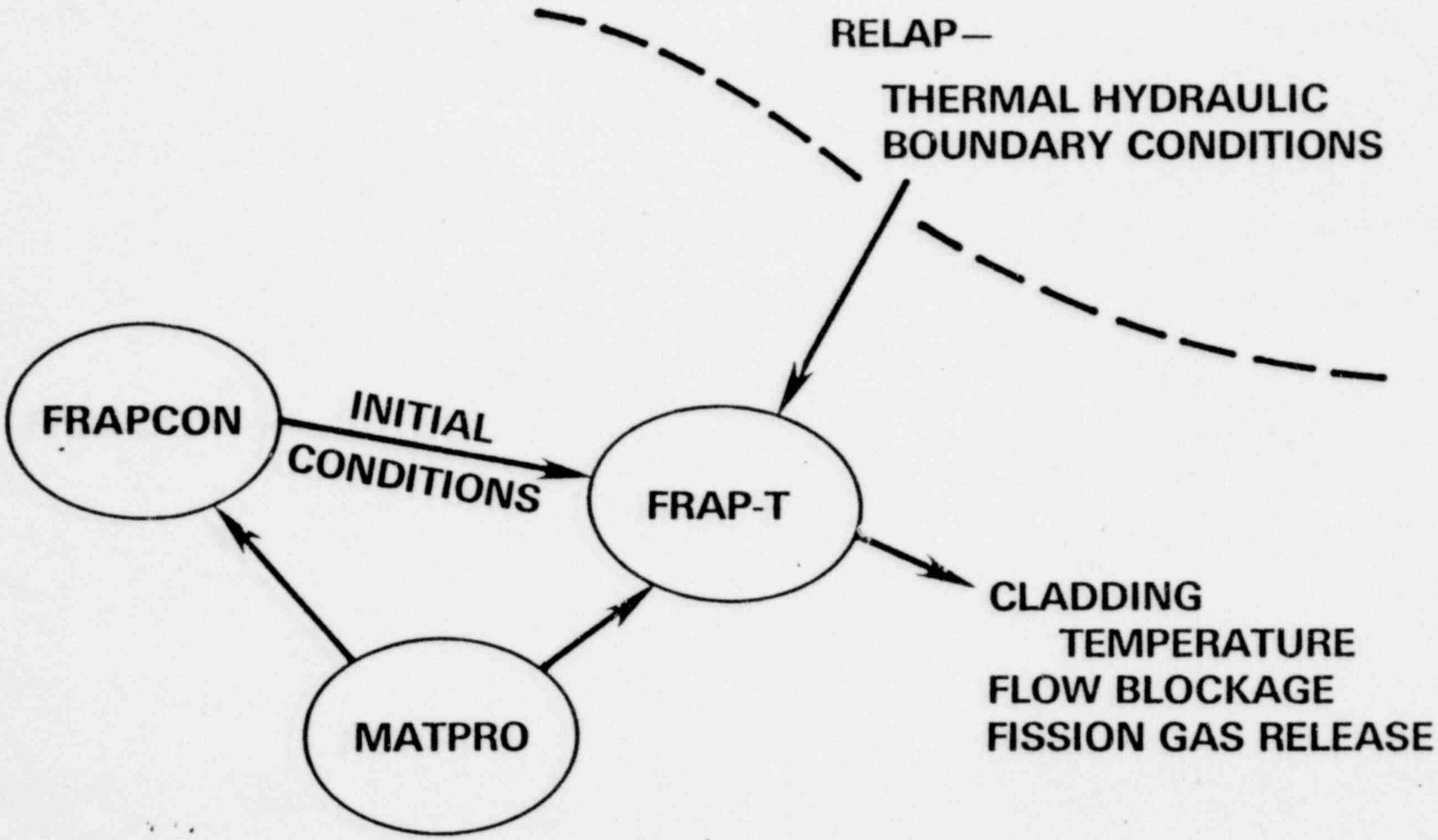
- Predict Transient And Steady State Fuel Behavior Under Normal, Off-Normal, And Accident Conditions.
- Provide An Integrated, Easily Accessible Storage Bank Of Fuel Behavior Information In The Form Of Correlation Equations And First-Principle Models Derived From Past, Present, And Future Experimental Work On Nuclear Power.

FUEL CODE DEVELOPMENT AND EVALUATION (Cont.)

THESE OBJECTIVES ARE ACCOMPLISHED BY:

- MATPRO—Material Property Correlations (Fuel and Clad)
- FRAPCON—Steady State Code; Contains Models To Simulate Fuel Rod Behavior Under Normal Conditions
- FRAP-T—Transient Code; Contains Models To Simulate Fuel Rod Behavior Under Transient Conditions
- Link With Thermal/Hydraulic Codes

FRAP CODE SYSTEM



FUEL MODELING CODES

OBJECTIVES

To Predict Transient Fuel Behavior During Off-Normal and Accident Conditions and at any Time During the Useful Life of LWR Fuel Rods.

CAPABILITIES

FRAP-T: Best Estimate Computer Code That Calculates the Thermal and Mechanical Response of a Fuel Rod During LOCA Type Transients.

It is Capable of Describing the Following Phenomena:

Cladding Deformation	Pellet Temp. Distribution
Cladding Ballooning	Failure Models (FRAIL Subcode)
Cladding Surface Temp.	Two Dimensional Heat Generation
Stored Energy	9 Modes of Rod-Coolant Heat Xfer

FUEL MODELING CODES (CON'T)

FRAPCON: Best Estimate Computer Code That Calculates the Thermal and Mechanical Response Characteristics of a LWR Fuel Rod Operating Under Steady State Power Conditions.

It Can Supply the Hot State Values of:

Radial Temp. Distribution	Cladding Deformation
Stored Energy	Fuel Deformation (Non-Mechanical)
Total Fission Gas Release	Gap Size and Gap Conductance
Gas Pressure & Comp.	Cladding Corrosion and Hydriding

PROPERTIES INCLUDED IN MATPRO

PROPERTY	SUBCODE
FUEL MATERIAL PROPERTIES	
1. SPECIFIC HEAT CAPACITY	FCP
2. THERMAL CONDUCTIVITY[A]	FTHCON
3. EMISSIVITY[A]	FEMISS
4. THERMAL EXPANSION[A]	FTHEXP
5. ELASTIC MODULI	FELMOD, FPOIR
6. CREEP RATE	FCREEP
7. DENSIFICATION	FUDENS
8. SWELLING	FSWELL
9. PRESSURE SINTERING	FHOTPS
10. RESTRUCTURING[A]	FRESTR
11. FRACTURE STRENGTH	FFRACS
12. FISSION GAS RELEASE	FGASRL
13. CESIUM AND IODINE RELEASE	CESIOD
14. VAPOR PRESSURE[B]	FVAPRS

[A]REVISED AND IMPROVED MODEL

[B]NEW MODEL

PROPERTIES INCLUDED IN MATPRO (CONT.)

PROPERTY	SUBCODE
CLADDING MECHANICAL PROPERTIES	
1. SPECIFIC HEAT CAPACITY AND THE EFFECT OF HYDRIDE SOLUTION ON THE SPECIFIC HEAT	CCP, CHSCP
2. ZIRCALOY THERMAL CONDUCTIVITY AND ZRO_2 THERMAL CONDUCTIVITY	CTHCON, ZOTCON
3. ZIRCONIUM DIOXIDE EMISSIVITY	ZOEMIS
4. THERMAL EXPANSION	CTHEXP
5. ELASTIC MODULI	
YOUNG'S MODULUS FOR ISOTROPIC CLADDING	CELMOD
SHEAR MODULUS FOR ISOTROPIC CLADDING	CSHEAR
CLADDING ELASTIC MODULUS	CELAST
6. AXIAL GROWTH	CAGROW
7. CREEP RATE[A]	CCRPR, CREEP
8. PLASTIC DEFORMATION[A]	CSTRES, CSTRAN CSTRNI, CANISO[B]

[A]REVISED AND IMPROVED MODEL

[B]NEW MODEL

PROPERTIES INCLUDED IN MATPRO (CONT.)

PROPERTY	SUBCODE
CLADDING MECHANICAL PROPERTIES (CONT.)	
9. ANNEALING	CANEAL
10. TEXTURE FACTORS	CTXTUR
11. MECHANICAL LIMITS[A] AND EMBRITTLEMENT[B]	CMLIMT, CBRTTL
12. CYCLIC FATIGUE	CFATIG
13. COLLAPSE PRESSURE	CCLAPS
14. LOW AND HIGH[A] TEMPERATURE OXIDATION	CORROS, COBILD
15. HYDROGEN UPTAKE	CHUPTK
16. MEYER HARDNESS	CMHARD
GAS MATERIAL PROPERTIES (APPENDIX C)	
1. THERMAL CONDUCTIVITY	CTHCON
2. VISCOSITY	GVISCO
SUPPORTING MATERIAL (APPENDIX D)	
1. PHYSICAL PROPERTIES[A]	PHYPRP
2. LINEAR INTERPOLATION	POLATE

[A] REVISED AND IMPROVED MODEL

[B] NEW MODEL

APPROACH TO CODE VERIFICATION

DEVELOPMENTAL VERIFICATION

- Iterative Process During Code Development
- Comparison of Predictions and Data for Standard Problems and Separate Effects Experiments Using Limited Amount of Data

INDEPENDENT VERIFICATION

- Performed on Frozen Versions of the Codes
- Utilizes Much Larger Data Base Than Developmental Verification Effort; Data Primarily Derived from Integral In-Reactor Experiments Conducted at Several Facilities
- Iterates with Code Development Until Prediction/Uncertainty Agrees With Experiment/Uncertainty

RELATED TASKS PROVIDING VERIFICATION INFORMATION

- Pre-Test Predictions of Integral Accident Test Results
- Post-Test Recommendations for Code Development

FRAP-T5 STANDARD MODEL ERRORS

Output parameter	Sample (rods/pts)		Standard error
			$\left[\sum_{i=1}^{n} (P_i - M_i)^2/n-1\right]^{0.5}$
CHF power at known flow	30/87		0.04 kW/CC channel
CHF flow at known power	30/87		390 $kg/s\text{-}m^2$
Initial fuel centerline temperature at scram	21/32		250 K
Fuel thermal decay constant during scram	21/32		5.7 s
Equilibrium fuel centerline temperature during scram	21/32		57 K
		MATPRO	FRAIL
Cladding burst temperature at known pressure	(155/155)	160 K	94 K
Cladding burst pressure at known temperature	(61/61)	16 MPa	23 MPa
Cladding permanent hoop strain	(327/327)	32% cladding OD	33% cladding OD

FRAP-T4 STANDARD MODEL ERRORS

Output parameter	Sample (rods/pts)		Standard error
			$\left[\sum_{i=1}^{n} (P_i - M_i)^2/n-1\right]^{0.5}$
CHF power at known flow	18/87		0.06 kW/CC channel
CHF flow at known power	18/87		400 $kg/s\text{-}m^2$
Initial fuel centerline temperature at scram	21/32		280 K
Fuel thermal decay constant during scram	21/32		5.4 s
Equilibrium fuel centerline temperature during scram	21/32		54 K
		MATPRO	FRAIL
Cladding burst temperature at known pressure	(158/158)	290 K	Not Analyzed
Cladding burst pressure at known temperature	(64/64)	34 MPa	Not Analyzed
Cladding permanent hoop strain	(370/370)	57% cladding OD	Not Analyzed

FRAPCON-1 MODEL ASSESSMENT — SUMMARY OF STANDARD DEVIATIONS BETWEEN MEASUREMENTS AND PREDICTIONS

Output Parameter	Sample Size (# of Rods/# of Points)	Standard Deviation FRAPCON-1
Fuel Centerline Temperature	32/274 (Pressurized Rods)	294K
	61/472 (Unpressurized Rods)	170K
Released Fission Gas	145/145	15.9%
Rod Internal Pressure	20/330 (Unpressurized Rods)	1.38 MPa
	28/285 (Pressurized Rods)	1.93 MPa
Gap Closure Heat Rating	88/88	11.4 KW/M
Axial Fuel Thermal Expansion	18/160	0.37%
Permanent Fuel Axial Deformation	97/354	0.45%
Permanent Cladding Hoop Strain	154/358	0.47%
Permanent Cladding Axial Strain	96/119	0.15%
Cladding Surface Corrosion Layer	40/69	5.8 micron
Cladding Hydrogen Concentration	33/46	37.2 ppm
Gap Conductance	17/112 (Unpressurized Rods)	10821 W/m^2K
	20/115 (Pressurized Rods)	21200 W/m^2K
Fuel Off-Centerline Temperature	20/111	208K

EXPECTED FUEL CODE ACCOMPLISHMENTS IN FY 80/81

A. ASSESSMENT OF FRAP-T5 COMPLETED

B. COMPLETION AND ASSESSMENT OF FRAPCON-2 - LAST VERSION OF CODE - MODEL UPDATING AS A RESULT OF ASSESSMENT AND NEW DATA WILL CONTINUE. HOWEVER, A NEW VERSION I.E., FRAPCON-2 MOD 1 WILL NOT BE MADE UNTIL SUFFICIENT CHANGES TO THE MODELS WARRENT IT.

C. COMPLETION AND ASSESSMENT OF FRAP-T6 - LAST VERSION OF CODE.

D. MATPRO-11 REVISION-1 COMPLETED

E. <u>MAJOR IMPROVEMENTS EXPECTED</u>:

<u>FRAP-T6</u>: LINK WITH FASTGRASS GAS RELEASE MODEL FROM ANL, A NEW BALLOONING MODEL BASED ON MRBT RESULTS, COMPLETE DYNAMIC STORAGE ALLOCATION, AN UPDATED FAILURE SUBCODE (FRAIL 6) COMPATIBLE WITH BALLOON-2, IMPROVED USER INPUT AND OUTPUT, θ-VARYING HTC MODEL, AND MANY OTHER SMALLER IMPROVEMENTS. COMPLETION DATE JANUARY 26, 1981.

<u>FRAPCON-2</u>: LINK WITH FASTGRASS, COMPLETE DYNAMIC STORAGE ALLOCATION, PELET MECHANICAL PACKAGE FROM GAPCON-3, IMPROVED INEL MECHANICAL PACKAGE, IMPROVED RELOCATION MODELS FOR BOTH MECHANICAL PACKAGES, ANS 5.4 GAS RELEASE OPTION, NRR-APPROVED EM MODEL OPTIONS, AND MANY OTHERS. COMPLETION DATE AUGUST 15, 1980.

<u>MATPRO-11 REVISION-2</u>: INC BCL ANNEALING PROPERTIES, TRUE STRESS/STRAIN U.F. DATA, REVISED CLAD CREEP AND THERMAL EXPANSION MODELS, UPDATED HOT PRESSING MODEL. COMPLETION MID 1981.

WORK PLANNED FOR FY 81 AND BEYOND

A. BEGIN DEVELOPMENT OF A SMALL BREAK (SLOW TRANSIENT) FUEL ROD DAMAGE CODE BASED ON AND LINKABLE TO FRAP-T AND FRAPCON.

B. CONTINUE TO IMPROVE THE MOST CRITICAL MODELS IN FRAP-T AND FRAPCON; NAMELY, FUEL RELOCATION AND CRACKED FUEL THERMAL AND MECHANICAL PROPERTIES, CLAD BALLOONING, PCI FAILURE ANALYSIS, AND LINKS WITH T/H CODES SUCH AS TRAC AND COBRA.

C. COORDINATE WITH NRR PERSONNEL TO PLAN AND ACHIEVE FUEL ROD BEHAVIOR STUDIES PERTINENT TO LICENSING STUDIES USING THE ABOVE CODES.

OBJECTIVES OF FUEL PELLET AND FUEL ROD PROPERTIES RESEARCH

- Provide information on changes to fuel pellets during steady-state and transient operation
- Improve models for calculating gap conductance in a fuel rod
- Determine the extent to which fuel pellets affect the transient axial flow of gas within a fuel rod

APPLICATION OF RESULTS

- Improved input data for fuel code calculations (MATPRO)
- Licensing evaluation of burnup influence on fission gas release
- Reduced uncertainties in stored energy calculations (Appendix K)

PROGRAMS TO STUDY FUEL ROD PROPERTIES

Halden Tests (EG&G)

IFA-429 — In-Reactor Measurement of Helium Absorption, Steady State and Transient Fission Gas Release, and Fuel Centerline Temperature as a Function of Burnup, Power, Gas Pressure, and Pellet Cladding Gap.

18 PWR — Type Rods-Pressurized to 375 psi — 25 cm Long.

IFA-430 — In-Reactor Measurement of Transient Axial Gas Flow and Centerline Temperature as a Function of Gap Size, Power, and Gas Flow Rates Plus Two Rods Unpressurized Instrumented for Fuel Temperature Measurements.

ACCOMPLISHMENTS TO DATE FOR IFA'S 429 AND 430

IFA-429 - HELIUM ABSORPTION REPORT ISSUED. RESULTS: AMOUNT OF HELIUM ABSORBED REDUCES PRESSURE BY AN INSIGNIFICANT AMOUNT (1.5%). PERIODIC POWER INCREASES (UP TO 50%) DID NOT DRIVE OUT THE ABSORBED HELIUM. BURNUP IS NOW AT 9000-24000 MWD/MTM. IRRADIATION WILL CONTINUE THROUGH 1980 AND INTO 1981. A PIE REPORT WILL BE ISSUED ON TWO RODS REMOVED AFTER 8000 MWD/MTM AND TWO RODS AFTER 30,000 MWD/MTM. IN 1981 OR 1982 THE TEST TRAIN WILL BE REMOVED AFTER 50,000 MWD/MTM.

IFA-430 - BEGAN IRRADIATION 11/26/78. PRELIMINARY RESULTS INDICATE THAT AT POWERS WHERE REDUCED GAS FLOW WAS EXPECTED, AN EFFECTIVE GAS GAP OF GREATER THAN ONE-HALF THE INITIAL WAS PRESENT INDICATING THAT THE FUEL CRACKS ARE NOT TIGHTLY CLOSED. PRESENT BURNUP IS 3000 MWD/MTM.

DATA USING DIFFERING GAP GAS COMPOSITIONS OF He AND Xe (UP TO 10% Xe) HAVE VERIFIED THE MODELS IN FRAP-T FOR GAP CONDUCTANCE TO PRESSURES OF 1.0 MPA. AT PRESSURES > 1.0 MPA AND Xe CONCENTRATIONS OF 10%, THE CODE PREDICTED ABOUT 20% LOWER GAP CONDUCTANCE THAN OBSERVED.

IRRADIATION WILL CONTINUE THROUGH 1980 AND 1981. DATA WILL BE CONTINUALLY COLLECTED AND REPORTED.

PROGRAMS TO STUDY FUEL ROD PROPERTIES (CONT'D)

Halden Tests (PNL)

IFA-431/432/527 — In-Reactor Measurement of Centerline Temperatures (Both Ends) as a Function of Burnup, Power, Gap Width, and Gas Composition. Six 50 cm Unpressurized BWR Type Rods Each Assembly.

IFA-431: PIE Complete, Peak Burnup 5000 MWD/MTM. Reports Issued: NUREG/CR-0318, NUREG/CR-0332, NUREG/CR-0749, -0797.

IFA-432: Presently In-Reactor, Average Burnup 24,000 MWD/MTM. 16 of 26 Instruments Still Working. To be Discharged From Reactor in CY 1981. Reports Issued: NUREG/CR-0220,-0560,-1139

IFA-527: Xenon Filled Rods to Determine Pellet Relocation Effects. To go In-Reactor MAY 1980.

PROGRAMS TO STUDY FUEL ROD PROPERTIES (CONT'D)

Halden Project Sponsorship

IFA-513 — Same as IFA-431 Except: He-Xe Gas Mixtures; Longer Length; Continuously Recording Pressure Transducers; Intermediate Power; and One Rod Pressurized to 45 psi Helium. Began Irradiation 11/78. Rods Will be Used Later in PBF for RIA and LOCA Tests. Decision Regarding Removal/Continued Irradiation to be Made CY 1980.

Reports Issued: NUREG/CR-0862, NUREG/CR-1077.

ACCOMPLISHMENTS TO DATE FOR IFA'S 431, 432 AND 513

A. BOL MEASUREMENTS OF TEMPERATURE, POWER, AND CLADDING ELONGATIONS RESULTED IN:

1. NO HIGH BURNUP ENHANCED FISSION GAS RELEASE NOTED TO DATE (24,500 MWD/MTM).

2. NO ADVERSE EFFECTS NOTED IN TWO RODS CONTAINING DENSIFYING FUEL.

3. THE DEVELOPMENT OF A NEW MODEL FOR FUEL RELOCATION, EFFECTIVE FUEL CONDUCTIVITY, AND CRACKED FUEL ELASTIC MODULI WHICH WILL BE TESTED IN FRAPCON-2.

4. CRACKED FUEL CONDUCTIVITY WAS REDUCED BY 20% AND THE ELASTIC MODULI TO ABOUT 1/40 OF SOLID UO_2 FOR 80% FUEL RELOCATION AT 30 KW/M.

5. THE RESULTING FUEL/CLAD GAPS HAVE REMAINED ESSENTIALLY CONSTANT SINCE.

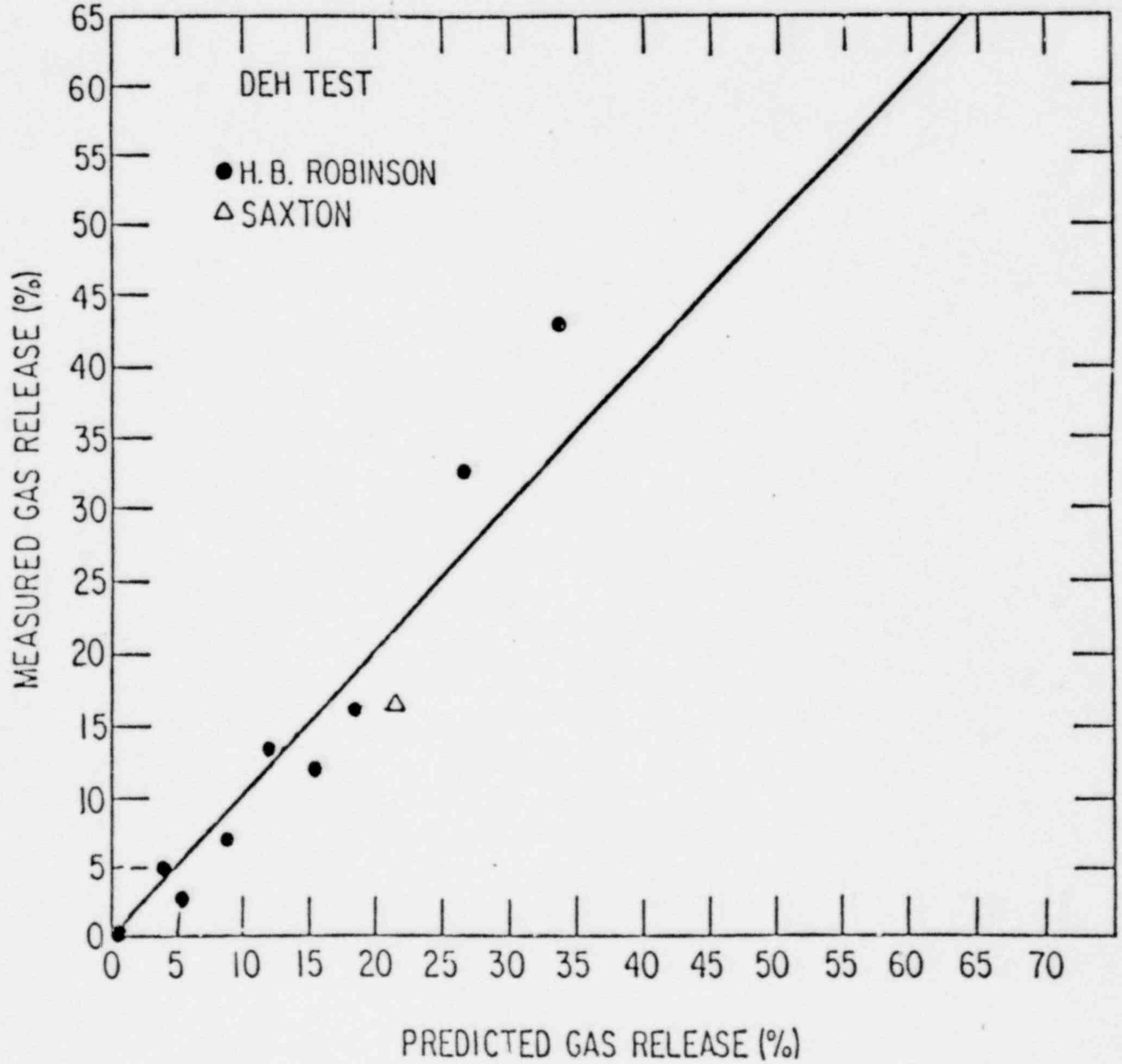
B. PIE COMPLETE ON 431. IFA-432 WILL BE REMOVED IN SUMMER OF 1981.

PROGRAMS TO STUDY FUEL ROD PROPERTIES (CONT'D)

EX-REACTOR TRANSIENT GAS RELEASE - ANL

GRASS-SST DEVELOPMENT

- THE FINAL VERSION OF GRASS-SST, MOD 6, HAS BEEN COMPLETED AND IS BEING SUBMITTED TO THE ARGONNE CODE CENTER
- A GRASS-SST USERS MANUAL HAS BEEN COMPLETED AND IS CURRENTLY AVAILABLE IN DRAFT FORM
- GRASS-SST HAS UNDERGONE VERIFICATION AGAINST IN-PILE IRRADIATIONS, HIGH BURNUP GAS-RELEASE TESTS, AND DEH TRANSIENT TESTS ON IRRADIATED FUEL.

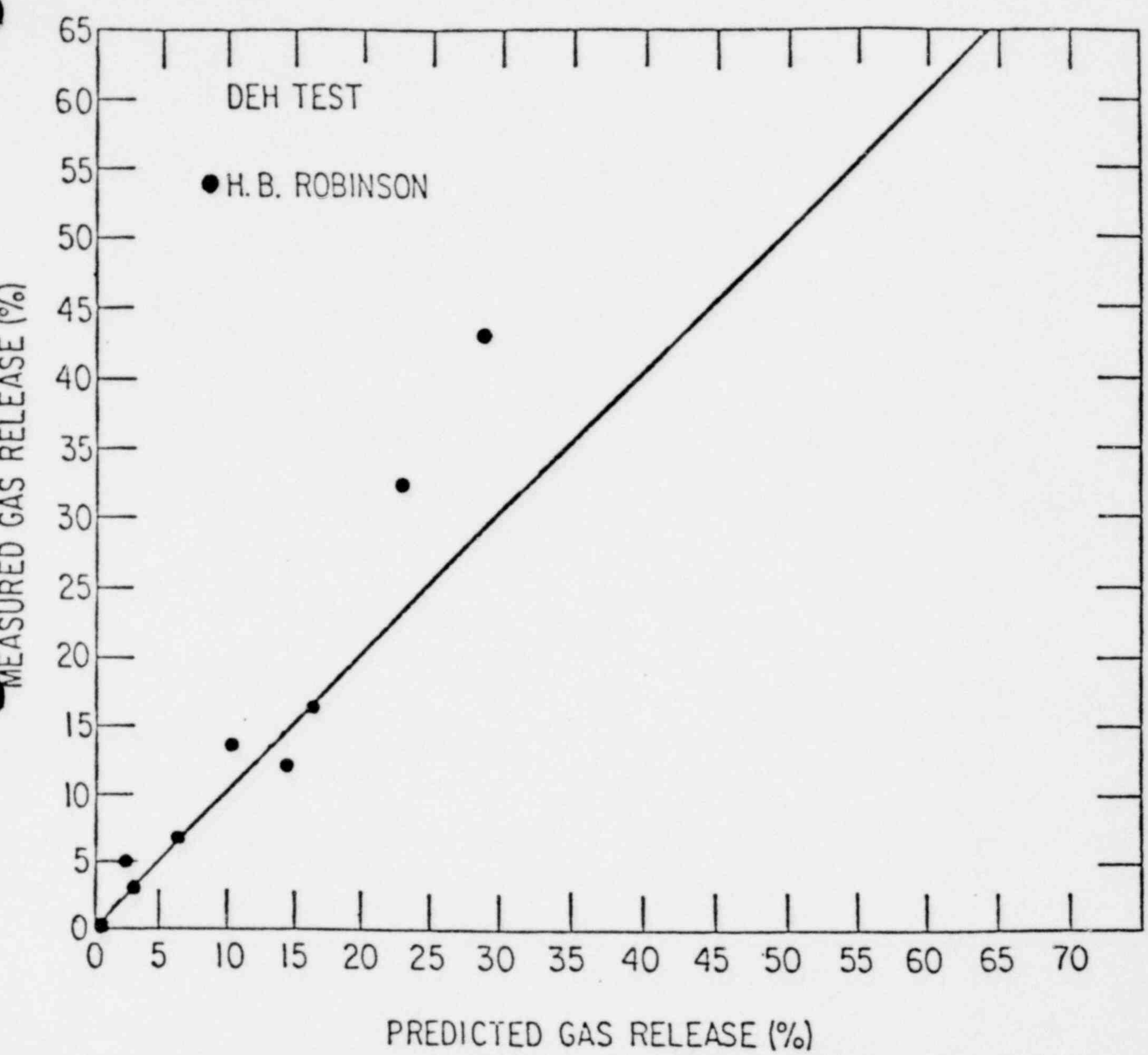


GRASS-SST-PREDICTED TRANSIENT GAS RELEASE

ANL GAS RELEASE (CONT'D)

FASTGRASS DEVELOPMENT

- FASTGRASS-MOD 1 WAS DEVELOPED, VERIFIED AND TRANSMITTED TO EG&G FOR INCORPORATION INTO FRAP
- FASTGRASS-MOD 1 IS 10-100 TIMES QUICKER IN EXECUTION THAN GRASS-SST-MOD 6
- FASTGRASS-MOD 1 HAS BEEN VERIFIED AGAINST GRASS-SST, AND AGAINST HIGH BURNUP AND DEH TRANSIENT TEST DATA
- FASTGRASS-MOD 2 IS UNDER DEVELOPMENT AND WILL BE AVAILABLE FOR INCORPORATION INTO FRAP BY SEPTEMBER 1980. FASTGRASS-MOD 2 WILL BE SIGNIFICANTLY QUICKER IN EXECUTION THAN FASTGRASS-MOD 1



FASTGRASS-PREDICTED TRANSIENT GAS RELEASE

ANL GAS RELEASE (CONT'D)

MODELING ACTIVITIES PLANNED FOR THE REMAINDER OF THE FISCAL YEAR AND BEYOND

- o FASTGRASS-MOD 2 WILL BE COMPLETED AND TRANSMITTED TO EG&G FOR INCORPORATION INTO FRAPCON AND FRAP-T
- o PARAGRASS DEVELOPMENT HAS BEEN INITIATED. WORK PLANNED FOR THE REMAINDER OF THE YEAR WILL BE THE IDENTIFICATION OF THE KEY PARAMETERS REQUIRED FOR AN ACCURATE REPRESENTATION OF STEADY-STATE AND TRANSIENT GAS RELEASE AND SWELLING FOLLOWED BY GRASS-SST PARAMETRIC ANALYSES ON THESE PARAMETERS. THESE PARAMETRIC ANALYSES WILL BE USED TO GENERATE THE PARAGRASS CORRELATIONS
- o GRASS-SST CALCULATIONS WILL CONTINUE TO BE PERFORMED TO DETERMINE THE RESPONSE OF FISSION GAS DURING LWR TRANSIENTS
- o ANL WILL CONTINUE TO ASSIST EG&G IN THE INTEGRATION OF GRASS-SST AND FASTGRASS INTO FRAPCON AND FRAP-T

ANL GAS RELEASE (CONT'D)

EXPERIMENTAL PROGRAM

STATUS AND RECENT PROGRESS

I. ANALYSIS OF DEH TESTS RESULTS HAS BEEN COMPLETED. FINAL REPORT ON EXPERIMENTAL PROGRAM HAS BEEN WRITTEN. DRAFT WILL BE OUT MAY 2, 1980.

RESULTS:

- EMPIRICAL TRANSIENT FISSION-GAS RELEASE CORRELATION DEVELOPED.
- MICROCRACKING WAS SHOWN TO BE IMPORTANT IN GAS RELEASE RATES > 30%.
- DATA USED TO VERIFY GRASS CODE.
- CONSTRAINED PELLETS HAVE SIGNIFICANTLY LESS RELEASE. EFFECT WILL BE INCORPORATED IN GRASS MODEL.

EMPIRICAL FISSION-GAS RELEASE CORRELATION

ZIRCALOY CLADDING RESEARCH

FUEL BEHAVIOR RESEARCH BRANCH, RES

M. L. PICKLESIMER

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUEL

APRIL 29, 1980

MULTIROD BURST TEST PROGRAM, ORNL

OBJECTIVE:

- CHARACTERIZE BALLOONING, BURST, AND LOSS OF FLOW AREA IN BUNDLES OF LWR FUEL ROD SIMULATORS DURING REFILL-REFLOOD AFTER A LOCA AS FUNCTIONS OF HEATING RATE, MATERIAL PARAMETERS, ROD-TO-ROD INTERACTION.
- DETERMINE SCALING FACTORS FROM SINGLE ROD TO 8 X 8 BUNDLES.

JUSTIFICATION:

- DEGREE OF CONSERVATISM OF PRESENT LICENSING CRITERIA NOT ESTABLISHED QUANTITATIVELY FOR MANY ACCIDENT SCENARIOS, PARTICULARLY SLOW HEATUP DURING REFILL-REFLOOD AFTER LOCA.
- REQUIREMENT OF 10 CFR 50. THAT THE EXTENT OF FLOW BLOCKAGE NOT BE UNDERESTIMATED.
- PRESENT EMBRITTLEMENT CRITERIA IN 10 CFR 50.46 REQUIRE BETTER ESTIMATES OF RUPTURE STRAINS TO ENSURE 17% EQUIVALENT CLADDING THICKNESS OXIDATION LIMIT NOT EXCEEDED.

SUMMARY OF TESTS PERFORMED TO DATE

TEST GEOMETRY	HEATING RATE (°c/s)	SHROUD HEATED	NUMBER OF TESTS	BURST TEMPERATURE (°c)
SINGLE ROD	28	NO	54	690-1170
	10		4	760- 800
	5		3	770- 790
	~0		4	760- 820
4 X 4 BUNDLE	30	YES	1	865
	30	NO	1	857
	10	YES	1	764
SINGLE ROD	28	YES	3	760- 940
	10		5	760- 900
	5		3	765- 775
	1		6	825- 970
	~0		6	760- 810

ACCOMPLISHMENTS IN 1ST HALF OF FY 1980

1. COMPLETED SIMULATORS AND ASSEMBLED B-5 (8 X 8) TEST ARRAY
2. COMPLETED MULTIROD TEST FACILITY EXPANSION FOR B-5 TEST
3. PERFORMED 13 SINGLE ROD HEATED SHROUD TESTS
4. PUBLISHED B-3 DATA REPORT
5. PUBLISHED B-1 & B-2 FLOW TEST ANALYSIS REPORT
6. PUBLISHED MRBT FUEL SIMULATOR DEVELOPMENT REPORT
7. PREPARED DRAFT OF MRBT THERMOMETRY REPORT
8. PERFORMED ANALYSIS IN SUPPORT OF LICENSING LOCA MODELS
9. INITIATED SUBCONTRACT FOR B-5 FLOW CHARACTERIZATION

ORIGINAL OBJECTIVES, PARAMETERS, AND TEST MATRIX
DESIGNED TO CONFIRM LICENSING EVALUATION MODEL BASES

- ▶ REPRESENT PRE-TMI PRECEPTS AND SCENARIOS
- ▶ CONCENTRATE ON RAPID HEATING BEHAVIOR
- ▶ ASSUME A FEW SCOPING TESTS WILL SHOW INSIGNIFICANT EFFECT OF SLOW HEATING
- ▶ NEGLECT THERMAL-HYDRAULIC EFFECTS ON DEFORMATION

INITIAL SINGLE ROD UNHEATED SHROUD TESTS SHOWED

- ▶ OPTIMISTIC RESULTS FOR EXPECTED DEFORMATION
- ▶ NEGLIGIBLE EFFECT OF HEATING RATE

4 X 4 BUNDLE TESTS WITH AND WITHOUT HEATED SHROUD SHOWED

- ▶ GREATER DEFORMATION THAN ANTICIPATED
- ▶ DEFORMATION SENSITIVE TO HEATING RATE
- ▶ 4 X 4 BUNDLE PROBABLY NOT REPRESENTATIVE OF LARGE BUNDLE WITH RESPECT TO ROD-TO-ROD INTERACTION

RECENT SINGLE ROD SCOPING TESTS WITH HEATED SHROUD SHOW GREATER THAN ANTICIPATED INFLUENCE OF HEATING RATE AND THERMAL-HYDRAULIC CONDITIONS

- ▶ HEATED SHROUD DECREASES ROD POWER REQUIREMENTS
- ▶ DECREASING ROD POWER INCREASES TEMPERATURE UNIFORMITY
- ▶ DECREASING HEATING RATE ENHANCES TEMPERATURE UNIFORMITY
- ▶ INCREASING TEMPERATURE UNIFORMITY INCREASES DEFORMATION
- ▶ MAGNITUDE AND DISTRIBUTION OF DEFORMATION STRONGLY INFLUENCED BY THERMAL-HYDRAULICS
- ▶ BURST TEMPERATURE CORRELATION DEVELOPED FROM UNHEATED SHROUD TESTS APPEARS TO UNDERPREDICT HEATED SHROUD TEST RESULTS
- ▶ NEW KFK DATA APPEAR CONSISTENT WITH THESE RESULTS

CURRENT MRBT TEST PLANS

- B-5 (8 X 8) BENCHMARK TEST
  - LATERAL CONSTRAINT, UNHEATED SHROUD, ORNL SIMULATORS
  - ALL RODS PRESSURIZED & POWERED THE SAME
  - TEST AT ~800°C WITH 5 K/S HEATING RATE
  - DETAILED FLOW CHARACTERIZATION INCLUDING VELOCITY PROFILES
- B-4 (6 X 6) COLD-ROD TEST
  - LATERAL CONSTRAINT, UNHEATED SHROUD, SEMCO SIMULATORS
  - THREE RODS UNPRESSURIZED AND UNPOWERED
  - TEST AT ~765°C WITH 1 K/S HEATING RATE BY 11-30-80
  - NO FLOW CHARACTERIZATION
- B-6 (6 X 6) ALPHA + BETA TEST
  - SAME DESIGN AND SIMULATORS AS B-5
  - ALL RODS PRESSURIZED & POWERED THE SAME
  - TEST AT ~900°C WITH 10 K/S HEATING RATE BY 9-30-81
  - NO FLOW CHARACTERIZATION
- 20 SINGLE RODS TESTS
  - 16 SCOPING TESTS TO CONTINUE EXPLORATION OF HEATED SHROUD EFFECT
  - 4 WITH JAERI SIMULATORS
- CONCLUDE EXPERIMENTAL WORK WITH B-6 TEST

EXPECTED ACCOMPLISHMENTS IN 2ND HALF OF FY 1980

1. CONDUCT B-5 (8 X 8) BURST TEST BY 6-1-80
2. SHIP B-5 TO SUBCONTRACTOR FOR FLOW CHARACTERIZATION
3. CONDUCT ~7 SINGLE ROD HEATED SHROUD TESTS
4. COMPLETE SIMULATORS AND ASSEMBLE B-4 (6 X 6) TEST ARRAY

EXPECTED ACCOMPLISHMENTS IN FY 1981

1. COMPLETE B-5 FLOW CHARACTERIZATION BY 12-30-80
2. COMPLETE B-5 STRAIN AND BLOCKAGE MEASUREMENTS BY 9-30-81
3. PERFORM B-4 TEST AND OBTAIN ~50% OF STRAIN DATA BY 9-30-81
   (OMIT FLOW CHARACTERIZATION)
4. FABRICATE AND TEST B-6 (6 X 6) BUNDLE BY 9-30-81
   (OMIT FLOW CHARACTERIZATION)
5. CONDUCT ~10 SINGLE ROD HEATED SHROUD TESTS BY 9-30-81
6. TERMINATE TESTING AFTER B-6 & START REDUCING STAFF

ABOVE ACCOMPLISHMENTS ASSUME AVAILABILITY OF 250K FY 1980 SUPPLEMENT AND 1050K IN FY 1981

CONCLUSION

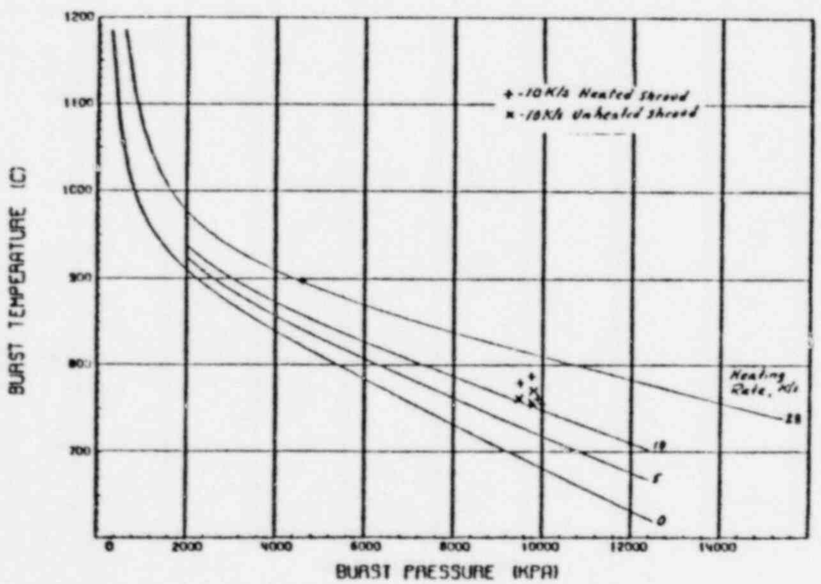
SINGLE ROD HEATED SHROUD TESTS MODEL MOST ASPECTS OF INDIVIDUAL RODS IN BUNDLES FOR COMPARABLE TEST CONDITIONS

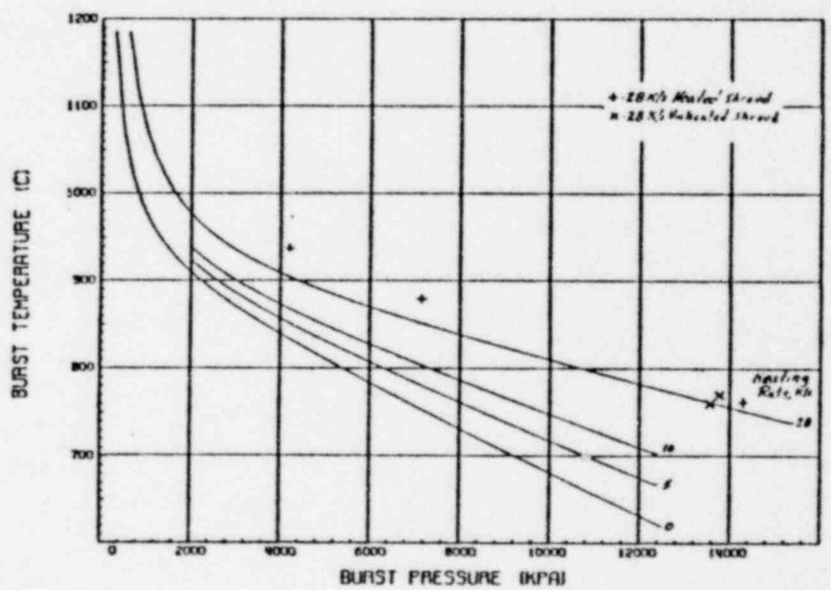
- ▶ POWER INPUTS AND, HENCE, TEMPERATURE GRADIENTS MODELED
- ▶ BURST STRAINS ARE TYPICAL
- ▶ DEFORMATION PROFILES IN REASONABLE AGREEMENT
- ▶ ROD-TO-ROD INTERACTION ON DEFORMATION PROFILE NOT INCLUDED IN SINGLE ROD TESTS

SINGLE ROD HEATED SHROUD TESTS ARE MOST COST EFFECTIVE METHOD OF EXPANDING DATA BASE

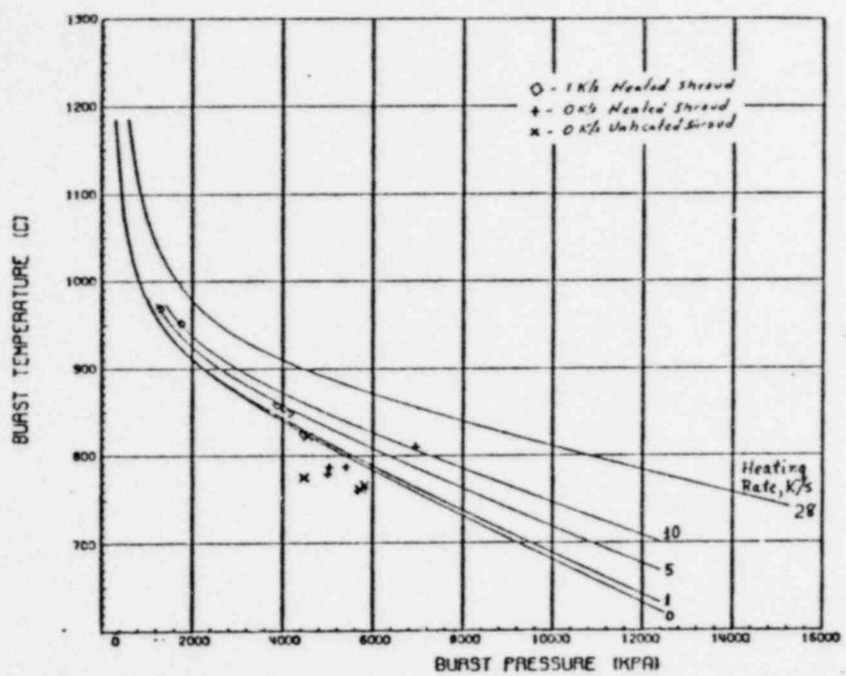
- ▶ RELATIVELY SIMPLE TO BUILD, TEST, AND EVALUATE
- ▶ EASY TO INVESTIGATE VARIOUS PARAMETERS
- ▶ NECESSARY TO INTERPRET BUNDLE TESTS
- ▶ ALLEVIATE NEED FOR MANY BUNDLE TESTS

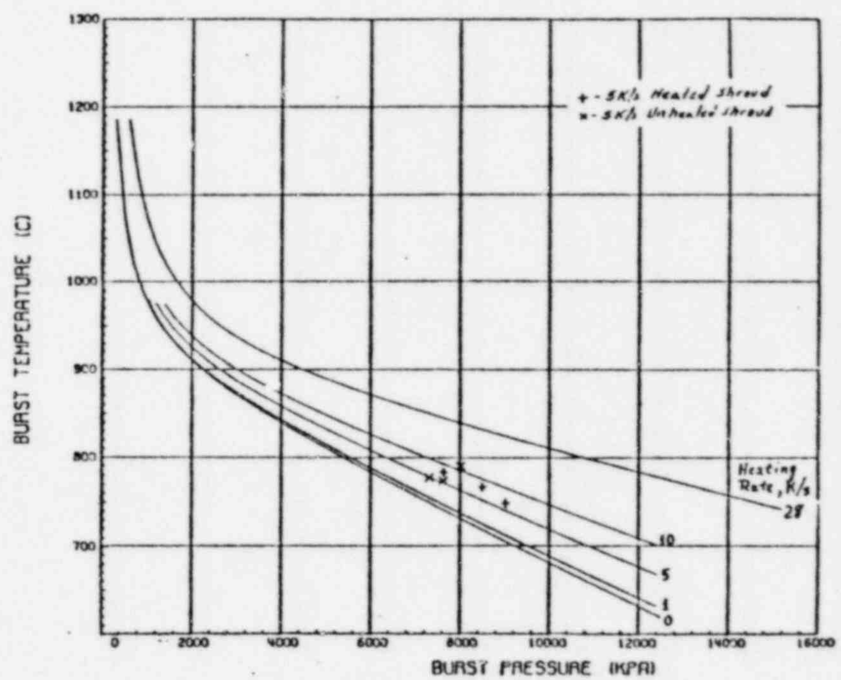
BURST TEMPERATURE CORRELATION TENDS TO UNDERPREDICT HEATED SHROUD TEST RESULTS BY 15-30°C FOR 10 AND 28 K/S TESTS IN 750 TO 950°C RANGE AND OVERPREDICTS NUMBER OF FAILURES





BURST TEMPERATURE CORRELATION PREDICTS HEATED SHROUD RESULTS REASONABLY WELL FOR 1 AND 5 K/S TESTS





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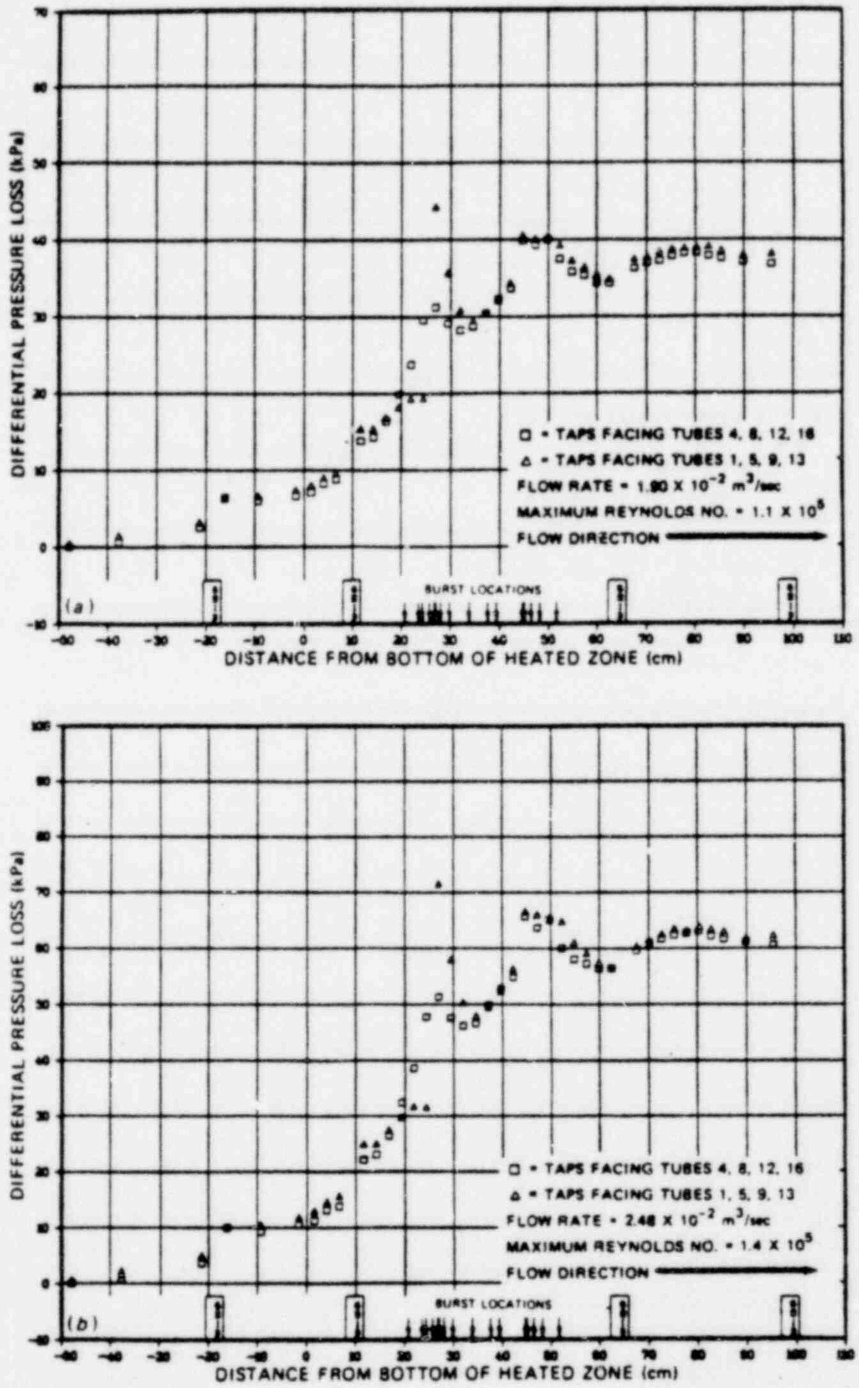


Fig. 130. Measured pressure loss profiles of bundle B-3 in shroud 3; (a) Re = 1.1×10^5 and (b) Re = 1.4×10^5.

POOR ORIGINAL

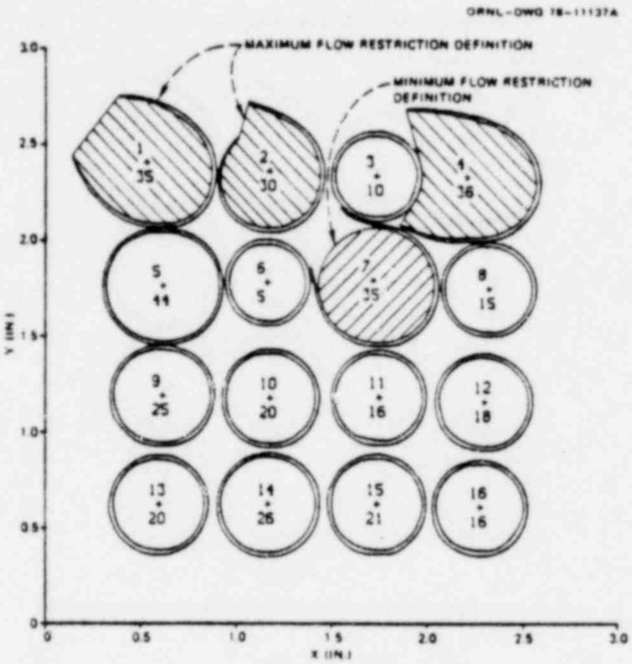


Fig. 124. Example of computer simulation of bundle cross section showing definitions of maximum and minimum flow restrictions for burst tubes.

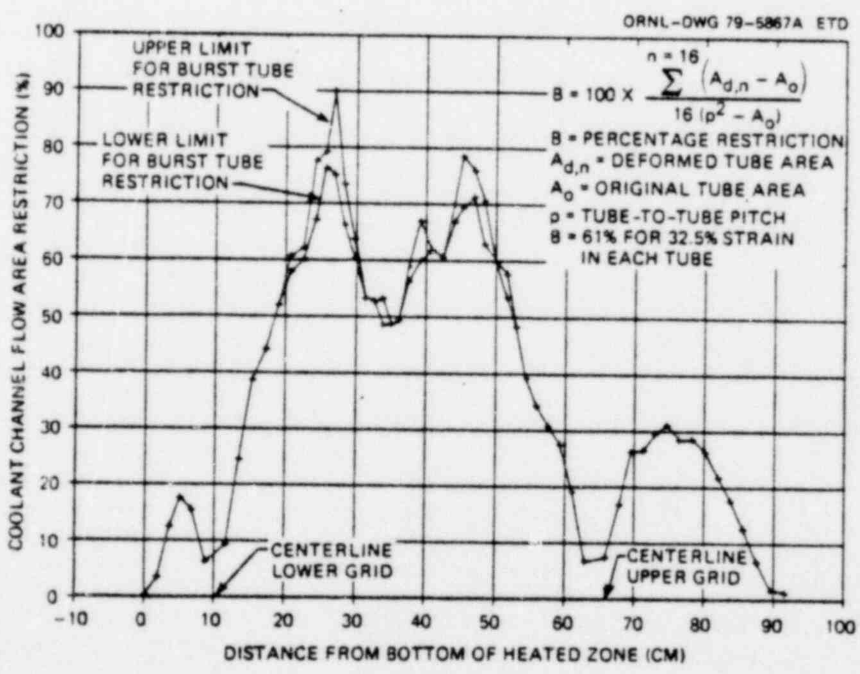
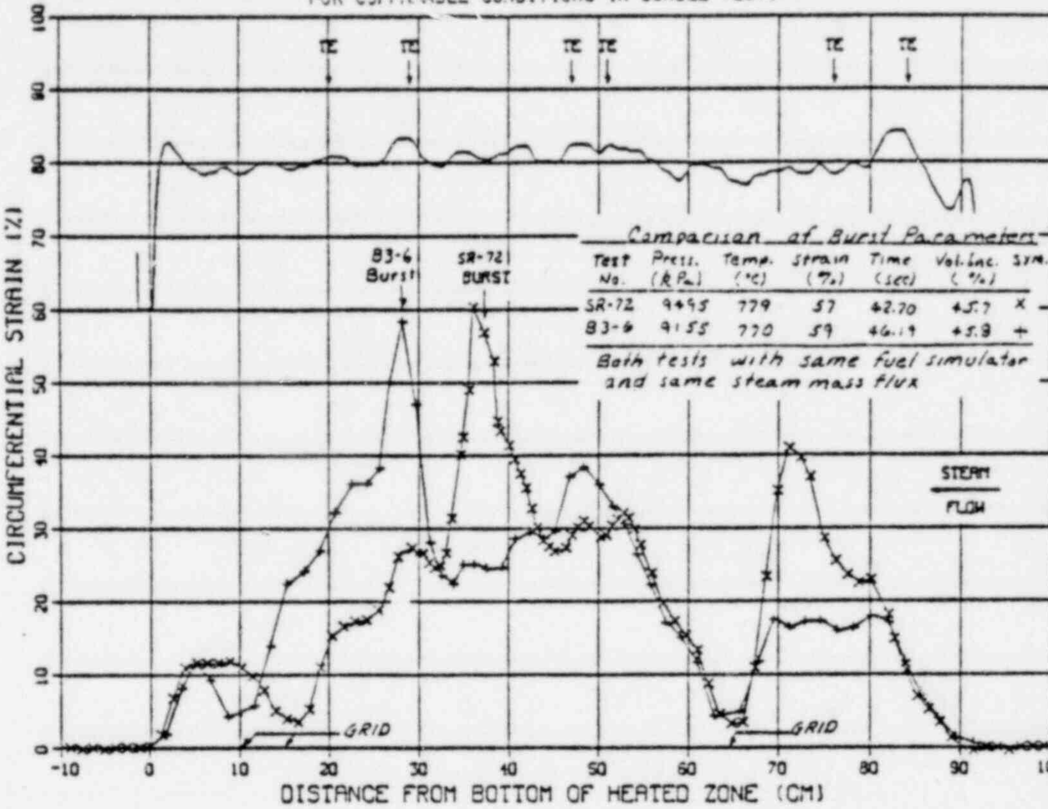
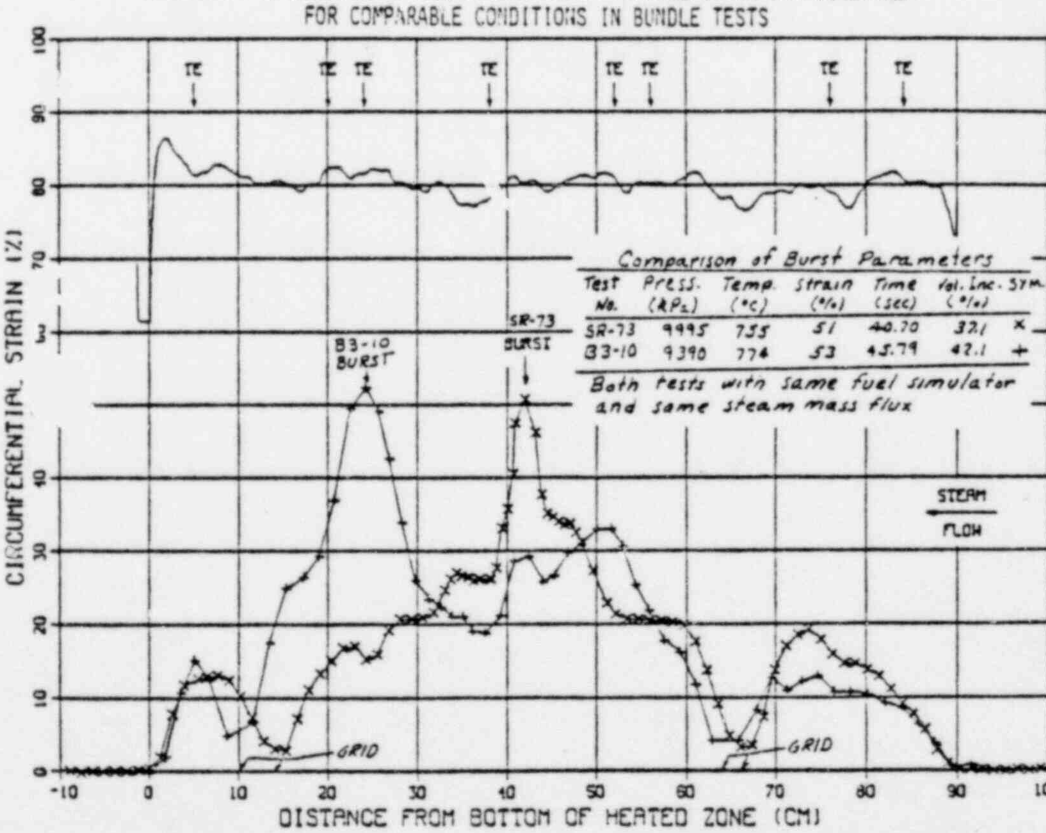


Fig. 125. Coolant channel flow area restriction in B-3 based on rod-centered unit cell and estimated upper and lower limits of burst tube flow restriction.

STRAIN IN SINGLE ROD HEATED SHROUD TESTS TYPICAL OF STRAIN OBSERVED
FOR COMPARABLE CONDITIONS IN BUNDLE TESTS



STRAIN IN SINGLE ROD HEATED SHROUD TESTS TYPICAL OF STRAIN OBSERVED
FOR COMPARABLE CONDITIONS IN BUNDLE TESTS



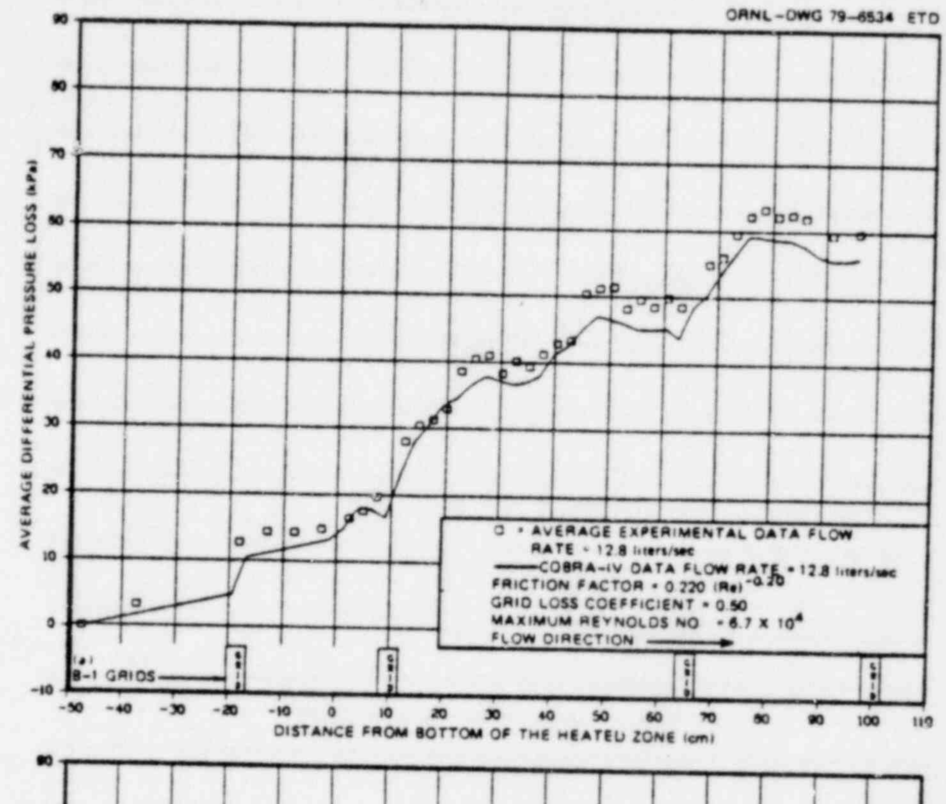


Fig. 5.20. Comparison of B-1/shroud 1 experimental and COBRA-IV axial pressure loss profiles; experimental flow rate = 12.8 liters/sec; minimum restriction definition. (a) Lower-limit; (b) upper-limit correlation values.

MECHANICAL PROPERTIES OF ZIRCALOY

OBJECTIVE:

PHASE I: QUANTITATIVELY CHARACTERIZE EMBRITTLEMENT OF ZIRCALOY FUEL ELEMENT TUBING BY OXIDATION WITH STEAM, RELATE EMBRITTLEMENT TO MEASUREABLE MECHANICAL PROPERTIES OF EMBRITTLED MATERIAL.

PHASE II: DETERMINE STRESS-RUPTURE PROPERTIES OF SPENT LWR FUEL CLADDING UNDER SIMULATED PELLET-CLADDING INTERACTION (PCI) CONDITIONS LEADING TO CLADDING RUPTURE.

JUSTIFICATION:

PHASE I: CHARGE TO RSR BY AEC DURING 1973 RULE-MAKING HEARINGS ON ECCS TO DETERMINE QUANTITATIVE EMBRITTLEMENT CRITERIA BASED ON MATERIAL PROPERTIES TO REPLACE ESTABLISHED CRITERIA BASED ON TEMPERATURE LIMIT AND MAXIMUM THICKNESS OF WALL OXIDIZED.

PHASE II: REQUEST BY NRR TO ESTABLISH DATA NEEDED TO SET LICENSING CRITERIA ON PCI FAILURES DURING NORMAL LWR POWER PRODUCTION AND OPERATION TO REDUCE RADIATION DOSE TO PUBLIC

CLAD PROPERTIES FOR CODE VERIFICATION (A2017)

PROGRAM OBJECTIVES

I. Develop Failure Criteria for Embrittled Zircaloy Cladding Based on the Mechanical Behavior of the Material.

II. Determine Stress-Rupture Properties and Fracture Mechanisms of Irradiated Zircaloy-4 Cladding under Simulated Reactor Operating Conditions.

III. Provide Technical Assistance on Zircaloy Embrittlement Characteristics.

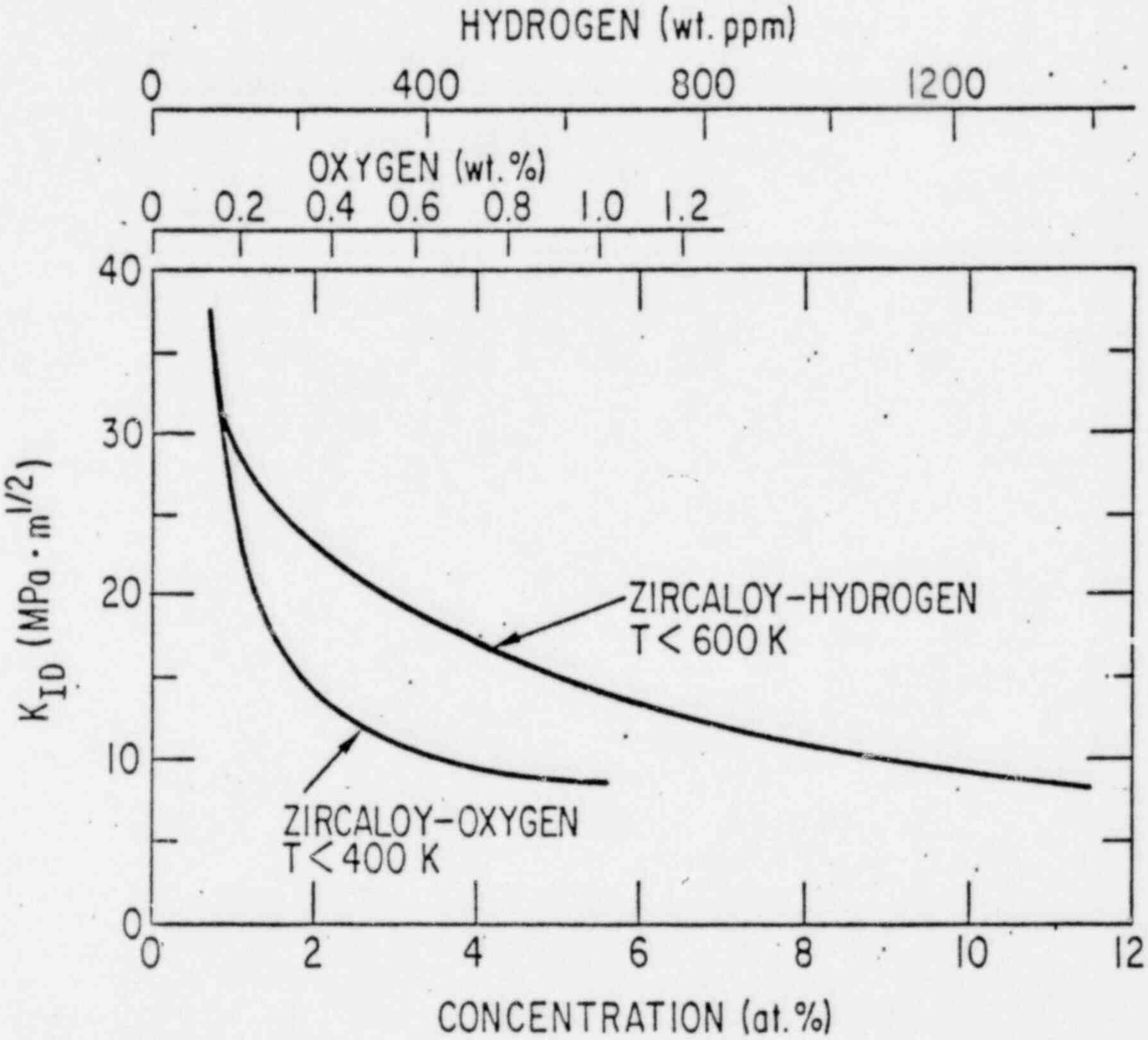
CLAD PROPERTIES FOR CODE VERIFICATION (A2017)

PROGRAM STATUS

I. Final Reports on Zircaloy Cladding Embrittlement and Instrumented Impact Properties of Zircaloy-Oxygen and Zircaloy-Hydrogen Alloys. (COMPLETE)

II. Application of Ballooning and Embrittlement Results to an Assessment of the Margin of Performance of ECCSs in LWRs. (IN PROGRESS)

III. Formulate Experimental Program to Determine Stress-Rupture Properties of Irradiated Zircaloy-4 Cladding under Simulated Reactor Operating Conditions. (IN PROGRESS)



Comparison of the Effect of Oxygen and Hydrogen on the Dynamic Fracture Toughness of Zircaloy at Temperatures of $\lesssim$400 and 600 K, Respectively.

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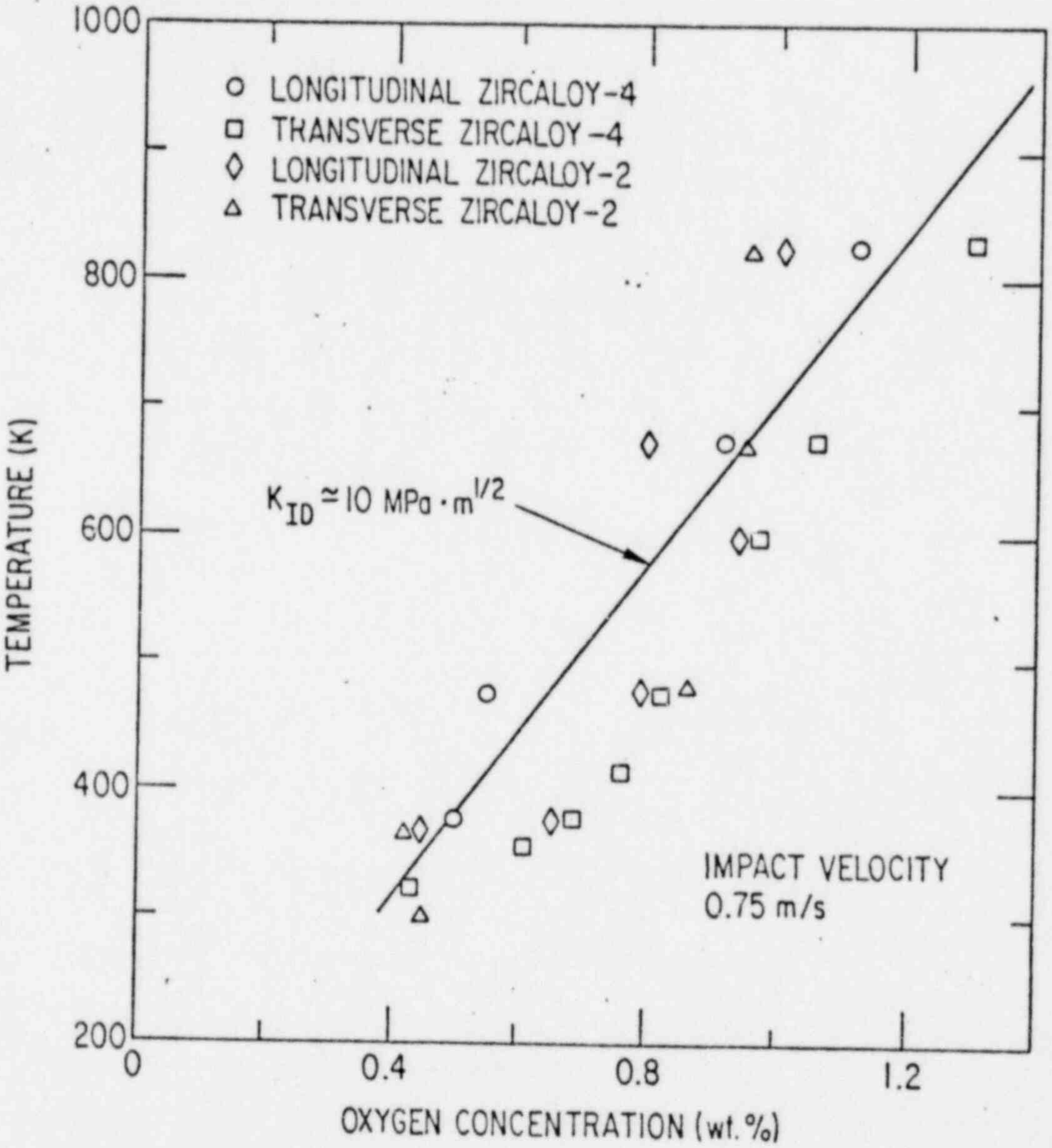


Ductile-to-Brittle Transition as a Function of Temperature and Oxygen Concentration of Homogeneous Zircaloy-2 and -4 Alloys Based upon a Dynamic Fracture Toughness of $\sim 10\ MPa \cdot m^{1/2}$.

ASSESSMENT OF THE MARGIN OF PERFORMANCE OF ECCSs

CALCULATED RESULTS

- Time-Temperature Transient for Rupture and Peak Temperature Nodes (from FSAR)

- Oxidation of Cladding with Different Wall Thicknesses during the Time-Temperature Transient at Several Axial Nodes (from Oxidation Models)

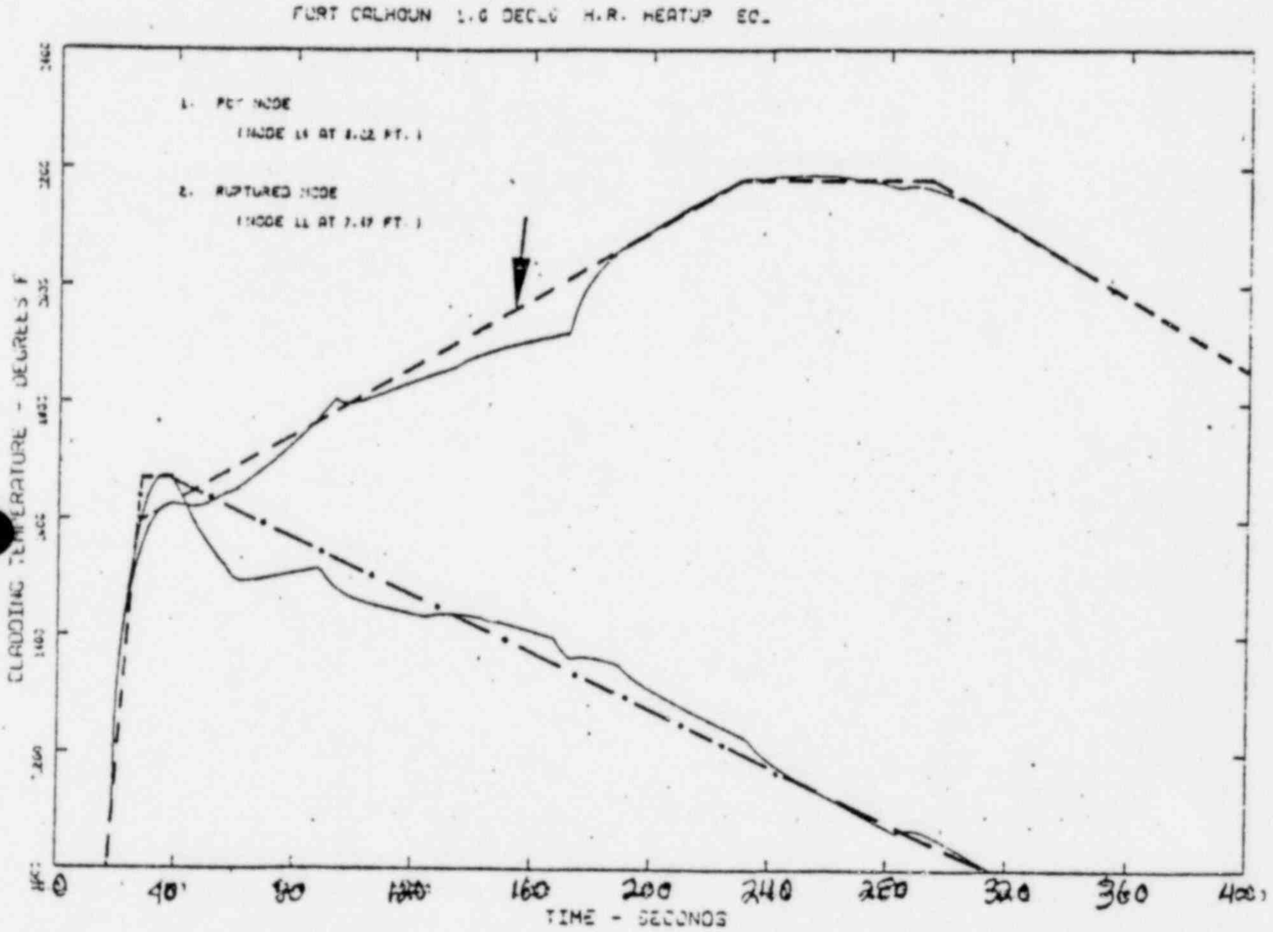
- Compare Oxidation Characteristics with Failure Limits Based on:

  1. ECR Limit of 17%, 1477 K (Present Criteria)
  2. Thermal-Shock Limit, $L_{0.9} \geq 0.1$ mm
  3. 0.3 J Impact Limit, $L_{0.7} \geq 0.3$ mm

  } ANL-79-48, NUREG/CR-1344

- Define Margin of Performance of ECCS Relative to

  1. ECR Parameter
  2. Transformed β-layer Thicknesses of 0.3 and 0.1 mm for 0.3 J Impact and Thermal-shock Failure, Respectively.



Temperature vs Time Transients (Dash Curves) Used in the Calculation of Various Oxidation Parameters for the Peak-temperature and Rupture Nodes of Zircaloy Cladding.

EVALUATION OF ECCS MARGIN OF PERFORMANCE

Plant	Accident	Clad Oxidation Parameters[a]					Performance Limits[b]		
		ECR (17%), s	t_f, s	ECR, %	$L_{(0.7)}$, mm	$L_{(0.9)}$, mm	17% ECR, 17%/ECR	0.3 J Impact, $L_{(0.7)}/0.3$	Thermal Shock, $L_{(0.9)}/0.1$
San Onofre	0.8 DEGPLD	275	500	26.0	0.25	0.25	0.65	0.83	2.5
Fort Calhoun	1.0 DECLG	325	475	22.5	0.28	0.28	0.75	0.93	2.8
	1.0 DECLG	-	100	4.0	0.30	0.30	4.20	1.00	3.0

[a]Values based upon two-side oxidation calculated from the model reported in ANL-79-48, NUREG/CR-1344.

[b]Value of ≥ 1 indicates the performance limit is met.

STRESS-RUPTURE PROGRAM ON IRRADIATED ZIRCALOY

FY 1980

I. Review Past Work on Stress-corrosion and Hydrogen-assisted Cracking of Zirconium-base Alloys

II. Acquire Irradiated Cladding from BCL

III. Design Autoclave Apparatus That Incorporates

a) External Pressure of ~15 MPa
b) Controlled ΔP across Tube Wall
c) Strain Gauges on OD of the Tube
d) Internal Mandrel Loading of the Tube
e) Temperature to ~630 K

IV. Develop Stress-rupture Test Matrix

a) Spent Fuel Cladding
b) Poison Rods

V. Construct and Check Out Apparatus on Unirradiated Cladding

FY 1981

I. Conduct Stress-rupture Tests on Irradiated Fuel Cladding

II. Characterize Fracture Surfaces by SEM and SAM

III. Establish Deformation Mechanisms

STRENGTH AND DUCTILITY OF IRRADIATED ZIRCALOY

- STUDY OF MECHANICAL PROPERTIES OF IRRADIATED ZIRCALOY REMOVED FROM SPENT FUEL ELEMENTS
- DATA SHOW THAT IN PWR SPENT FUEL CLADDING THE IRRADIATION DAMAGE IS ANNEALED OUT ON HEATING TO ABOUT 1200°F AT HEATING RATES TO 50°F/SECOND OR ON ISOTHERMAL ANNEALING FOR ONE MINUTE AT ANY TEMPERATURE OVER 1100°F
- BURST PROPERTIES COMPARABLE TO UNIRRADIATED CLADDING AT 1200°F AND HIGHER
- STUDY COMPLETED ON PWR CLADDING JANUARY 1980, FINAL REPORT IN DRAFT
- STUDY ON BWR CLADDING DELAYED TO JUNE 1980 BY INABILITY TO OBTAIN SUITABLE SPENT FUEL CLADDING IN TIME. SCOPING STUDY FOR COMPARISON TO PWR RESULTS. TO BE COMPLETED IN AUGUST 1980 AND FINAL REPORT ISSUED IN SEPTEMBER 1980.

PLASTIC PROPERTIES OF ZIRCALOY IN TRUE STRESS-TRUE STRAIN-TRUE STRAIN RATE TESTING

- PLASTIC TENSILE STRESS-STRAIN PROPERTIES OF ZIRCALOY FOLLOW A MODIFIED POWER LAW FOR TRUE STRESS-TRUE STRAIN-CONSTANT TRUE STRAIN RATE-TEMPERATURE TESTING TO 600°C IN UNIAXIAL TENSION
- PARAMETERS DETERMINED AND MODEL LAW IN USE IN MATPRO AND BALLOON-2
- STUDY COMPLETED FY 80, FINAL REPORT IN DRAFT

ZIRCALOY CLADDING CREEPDOWN IN-PILE

OBJECTIVE: TO EXAMINE CREEPDOWN BEHAVIOR OF LWR FUEL ELEMENT CLADDING UNDER EXTERNAL PRESSURE IN-PILE

- PROGRAM CONDUCTED BY COOPERATION BETWEEN ORNL AND PETTON, NETHERLANDS
- SEVEN IN-PILE CREEPDOWN TESTS SUCCESSFULLY COMPLETED, EIGHTH NOW IN TESTING
- FIVE TESTS COMPLETED UNDER EXTERNAL PRESSURE TO PLACE CLADDING IN CIRCUMFERENTIAL COMPRESSION
- DATA BEING REDUCED, CORRECTED FOR ZERO DRIFT, SHOW CREEP RATE SAME AT HALF STRESS FOR TENSILE LOADING AT SAME TEMPERATURE
- TESTS SIX, SEVEN, AND EIGHT CONDUCTED WITH STRESS REVERSAL - AFTER COMPRESSIVE CREEPDOWN TO CONTACT MANDREL, EXTERNAL PRESSURE REMOVED, SPECIMEN INTERNALLY PRESSURIZED TO PRODUCE TENSILE CIRCUMFERENTIAL STRESS
- STRESS REVERSAL TESTS SHOW ELASTIC RECOVERY, REDUCTION OF OVALIZATION, APPARENTLY NO BAUSCHINGER EFFECT
- STUDY TO BE COMPLETED IN FY 80 AND FINAL REPORT ISSUED BY SEPTEMBER 1980

PELLET-CLADDING INTERACTION FAILURES IN LWR FUEL RODS

OBJECTIVE:

DETERMINE OPERATIONAL LIMITATIONS ON LWR FUEL REQUIRED FOR PREVENTION OF CLADDING RUPTURE BY INTERACTION BETWEEN THE CLADDING AND SWELLING FUEL PELLETS, DURING NORMAL TO OFF-NORMAL REACTOR OPERATION, ANTICIPATED TRANSIENTS WITHOUT SCRAM, START-UP, AND LOAD-FOLLOWING OPERATION FOR SETTING LICENSING CRITERIA FOR PREVENTION OF PCI FAILURES.

JUSTIFICATION:

PCI FAILURES IN BWRS DURING POWER PRODUCTION CAUSE RELEASE OF NOBLE FISSION PRODUCT GASES (XE AND KR) TO THE STACK GASES, RESULTING IN EXPOSURE OF THE PUBLIC TO RADIOACTIVITY BEYOND "LOWEST PRACTICAL LEVEL". WHILE LESS PREVALENT AND LESS RISK TO PUBLIC IN PWRS, PCI FAILURES HAVE OCCURRED IN SOME, AND RESULT IN UNNECESSARY EXPOSURE TO PLANT PERSONNEL.

PELLET-CLADDING INTERACTION FAILURES

PLANNED PROGRAM

- TIME TO FAILURE BY STRESS-RUPTURE IN SPENT FUEL CLADDING
- EFFECTS OF STRESS-CORRODANTS ON TIME TO FAILURE
- STRAIN-RATE RAMPING TO PCI FAILURE EX-PILE
- STRAIN-RATE RAMPING TO PCI FAILURE IN-PILE

PCI FAILURE BY STRESS-RUPTURE

- o STUDY BY KASSNER, ANL, BEGUN IN FY 80
- o EXAMINATION OF PCI FAILURE IN SPENT FUEL CLADDING BY LOADING SPECIMEN WITH EXTERNAL PRESSURE AND INTERNAL EXPANDING MANDREL IN AUTOCLAVES
- o USING HIGH-TEMPERATURE STRAIN GAGES ON EXTERIOR SURFACE OVER "CRACK" OF EXPANDING MANDREL TO SENSE HOOP STRESS AND INITIATION AND GROWTH RATE OF GROWING PCI CRACK
- o TESTS IN FY 80 AND FY 81 SHOULD HAVE ESTABLISHED STRESS-RUPTURE FAILURE CURVES WITHOUT STRESS CORRODANT
- o TESTS IN FY 82 TO EXAMINE FAILURE WITH STRESS CORRODANTS PRESENT
- o BWR SPENT FUEL CLADDING WILL BE EXAMINED AS WELL AS AVAILABLE PWR MATERIAL (H. B. ROBINSON, MAINE YANKEE, OCONEE)

STRAIN-RATE RAMPING TO PCI FAILURE EX-PILE

- STUDY BEGUN IN FY 81 BY P. PANKASKIE, BNWL.
- OBJECTIVE IS TO OBTAIN DATA FOR ESTABLISHING PARAMETERS IN PROFIT MODEL OF PCI FAILURE, DEVELOPED FOR CPB/NRR IN FY 80.
- SPECIMEN CONSISTS OF TUNGSTEN WIRE CENTERLINE HEATER, UO_2 ANNULAR PELLETS, ZIRCALOY FUEL CLADDING, EXTERNALLY PRESSURIZED IN A LOOP. HEATER WILL BE RAMPED IN POWER TO LOAD CLADDING AT VARIOUS RATES, POWER INCREMENT BETWEEN HARD CONTACT AND RAMP FAILURE DETERMINED. CAN ALSO BE USED FOR TIME TO FAILURE AT PRESELECTED LOADING PAST HARD CONTACT.
- INITIAL STUDIES TO BE WITH UNIRRADIATED CLADDING, THEN IRRADIATED CLADDING, AND WITH AND WITHOUT STRESS-CORRODANT.

STRAIN-RATE RAMPING TO PCI FAILURE IN-PILE: DEMO-RAMP PROGRAM

- NRC PARTICIPATING IN DEMO-RAMP PROGRAM AT STUDSVIK ON HIGHER BURNUP FUEL.
- SELECTED PRE-IRRADIATED FUEL RODS TO BE POWER-RAMPED IN THE R2 REACTOR AT STUDSVIK TO DETERMINE POWER INCREMENT OR TIME TO FAILURE IN FUEL RODS AT ABOUT 25 MWD/T U BURNUP.
- DATA WILL BE COMPARED WITH THAT FOR LOWER BURNUP RODS OF SUPERRAMP AND INTERRAMP PROGRAMS.
- THIS PHASE OF STUDY COMPLETED IN JUNE 1981.

STRAIN-RATE RAMPING TO PCI FAILURE IN-PILE: PBF-OPTRAN TESTS

- PBF-OPTRAN TESTS DESIGNED TO EXAMINE PELLET-CLADDING INTERACTION DURING POWER RAMPING CAUSED BY VARIOUS SCENARIOS OF OPERATIONAL TRANSIENTS LIKELY IN COMMERCIAL LWR POWER PLANTS
- DATA DETERMINED WILL INCLUDE ONE OR MORE OF THE FOLLOWING:
  - INCREMENT OF POWER FROM BASE TO HARD CONTACT BETWEEN PELLET AND CLADDING
  - INCREMENT OF POWER FROM HARD CONTACT TO CLADDING FAILURE
  - CLADDING FAILURE AS FUNCTION OF RATE OF RAMPING
  - TIME TO FAILURE AS FUNCTION OF POWER INCREMENT AFTER HARD CONTACT
  - EFFECT OF BURNUP
  - EFFECT OF REPEATED CYCLING BELOW RAMP FAILURE LIMIT
- SCHEDULE OF TESTS:

  FIRST OPTRAN TEST PLANNED FOR 1980

  FOUR OPTRAN TESTS PLANNED FOR FY 1981

  OPTRAN TEST MATRIX COMPLETED IN FY 1982

  TOTAL OF SEVEN OPTRAN TESTS PLANNED, SIX 4X, ONE 9-ROD BUNDLE

INCIPIENT FUEL-CLAD MELT

OBJECTIVE:

CHARACTERIZE THE PROPERTIES, BEHAVIOR, AND FORMATION OF "LIQUIFIED FUEL" FORMED BY REACTION BETWEEN ZIRCALOY CLADDING AND UO_2 FUEL PELLETS AT HIGH TEMPERATURE

JUSTIFICATION:

REACTION BETWEEN ZIRCALOY CLADDING, STEAM, AND UO_2 FUEL PELLETS CAN CAUSE EXCESSIVE RELEASE OF FISSION PRODUCTS AT TEMPERATURES WELL BELOW THOSE OF UO_2 MELTING, DISRUPTION AND DESTRUCTION OF CORE GEOMETRY, AND BLOCKAGE OF COOLANT FLOW THROUGH A DAMAGED REACTOR CORE. ONLY SCOPING DATA ON THE REACTIONS ARE AVAILABLE, AND QUANTITATIVE DATA MUST BE OBTAINED FOR RULE-MAKING HEARINGS ON CLASS IX ACCIDENTS AND SMALL-BREAK LOCAS SUCH AS TMI-2.

INCIPIENT FUEL-CLAD MELT

- o PROGRAM INITIATED IN FY 81
- o STUDY TO DETERMINE REACTION RATES, COMPOSITIONS, AND HEATS OF FORMATION OF REACTION PRODUCTS BETWEEN ZIRCALOY CLADDING, UO_2 FUEL PELLETS, AND STEAM AT TEMPERATURES FROM ABOUT 1800K TO ABOUT 2500K
- o OXIDATION RATES OF "LIQUIFIED FUEL" WILL BE DETERMINED IN BOTH SOLID AND LIQUID PHASES
- o DETAILED PROGRAM NOT YET FORMULATED

LWR FISSION PRODUCT RELEASE AND TRANSPORT

OVERALL OBJECTIVES:

TO DEVELOP FISSION PRODUCT RELEASE SOURCE TERMS FOR ZIRCALOY-CLAD UO_2 FUEL RODS UNDER ACCIDENT CONDITIONS INCLUDING SEVERE FUEL DAMAGE AND CORE MELT.

TO DEVELOP MODELS TO PREDICT THE ATTENUATION AND TRANSPORT BEHAVIOR OF FISSION PRODUCTS WITHIN THE PRIMARY COOLANT SYSTEM AND CONTAINMENT.

TO PROVIDE ENVIRONMENTAL RELEASE SOURCE TERMS FOR CONSEQUENCE ANALYSIS AND TO PROVIDE FISSION PRODUCT AND AEROSOL LOADING SOURCE TERMS TO EVALUATE ESF AND MITIGATION FEATURE DESIGN REQUIREMENTS.

LWR FISSION PRODUCT RELEASE AND TRANSPORT RESEARCH

EXISTING PROGRAMS AND PLANNED PROGRAMS

FISSION PRODUCT TRANSPORT ANALYSIS - TRAP CODE

SEPARATE EFFECTS TESTS FOR TRAP CODE

FISSION PRODUCT VAPOR DEPOSITION EXPERIMENTS

STEAM GENERATOR TUBE RUPTURE IODINE TRANSPORT

FISSION PRODUCT RELEASE FROM LWR FUEL

FISSION PRODUCT RELEASE FROM LWR FUEL - HIGH TEMPERATURE - NEW

CHARCOAL FILTER IODINE RETENTION PERFORMANCE - NEW

PROPOSED FUTURE PROGRAMS

FISSION PRODUCT RELEASE - MELTING FUEL

FISSION PRODUCT LEACHING

FISSION PRODUCT TRANSPORT VERIFICATION FACILITY

TMI FISSION PRODUCT RELEASE EXAMINATION

FISSION PRODUCT TRANSPORT ANALYSIS - TRAP CODE - BCL

OBJECTIVE:	TO DEVELOP A MECHANISTIC COMPUTER CODE TO MODEL FISSION PRODUCT TRANSPORT BEHAVIOR WITHIN THE PRIMARY COOLANT SYSTEM AND CONTAINMENT.
STATUS:	PRIMARY SYSTEM MODEL ESSENTIALLY COMPLETE. RFP ISSUED FOR ADVANCED CODE.
ACCOMPLISHMENTS:	DEPOSITION OF FISSION PRODUCTS WITHIN REACTOR COOLANT SYSTEM UNDER CORE MELT ACCIDENT CONDITIONS IS <u>RELATIVELY</u> UNIMPORTANT. GROWTH OF AEROSOLS WITHIN RCS IS IMPORTANT.
FUTURE PLANS:	IMPROVE TRAP CODE MODELS, EXTEND TRAP CODE TO MODEL CONTAINMENT FISSION PRODUCT BEHAVIOR, SOURCE TERM MODELLING, SENSITIVITY ANALYSIS, DEFINE VERIFICATION TEST FACILITY FUNCTIONAL DESIGN REQUIREMENTS.
FUNDING:	FY 80-83 -- 10-12 MAN-YEARS

FISSION PRODUCT TRANSPORT ANALYSIS - RESULTS

RADIONUCLIDE DEPOSITION IN RCS - TRAP BASELINE CALCULATION RESULTS

SEQUENCE - DESCRIPTIONS

TMLB' - PWR TRANSIENT WITH LOSS OF SECONDARY HEAT SINK AND LOSS OF ELECTRIC POWER

TC - BWR TRANSIENT WITH FAILURE OF RPS

AB - PWR LARGE LOCA WITH LOSS OF ELECTRIC POWER

FISSION PRODUCT RELEASE PATH(S) TO CONTAINMENT

TMLB' - CORE, UPPER PLENUM, PRESSURIZER, QUENCH TANK

TC - CORE, STEAM SEPARATORS, STEAM DRYERS, UPPER HEAD, OUTER ANNULUS

AB - CORE, UPPER PLENUM, LOWER PLENUM, DOWNCOMER, STEAM GENERATOR

PERCENTAGE OF SOURCE TERM DEPOSITED

	TMLB'	IC	AB
I	5.6	1.9	0.8
Cs	7.5	29.9	61.3
Pu	2.0	4.0	19.0

SEPARATE EFFECTS TESTS FOR TRAP CODE - SANDIA

OBJECTIVE: TO PROVIDE BASIC DATA ON FISSION PRODUCT COMPOUND VAPOR PRESSURES AND CHEMICAL INTERACTIONS IN A HIGH TEMPERATURE STEAM ENVIRONMENT TO SUPPORT DEVELOPMENT OF THE TRAP CODE.

STATUS: VAPOR PRESSURE EXPERIMENTS IN PROGRESS AT SANDIA LABORATORIES AND AT THE NEW MEXICO INSTITUTE FOR MINING AND TECHNOLOGY - COMPOUNDS OF CESIUM AND IODINE BEING INVESTIGATED.

FISSION PRODUCT REACTION SYSTEM (FPRS) APPROXIMATELY 60% COMPLETE.

FUTURE PLANS: VAPOR PRESSURE TESTS ON OTHER FISSION PRODUCT COMPOUNDS WILL BE CONDUCTED AS NECESSARY.

FPRS WILL BE COMPLETED AND TESTING INITIATED.

NON-INTRUSIVE REAL TIME FISSION PRODUCT COMPOUND IDENTIFICATION BY LASER RAMAN SPECTROSCOPY WILL BEGIN IN THE FPRS APPARATUS.

FUNDING: FY 80 - 150K -- FY 81 - 210K

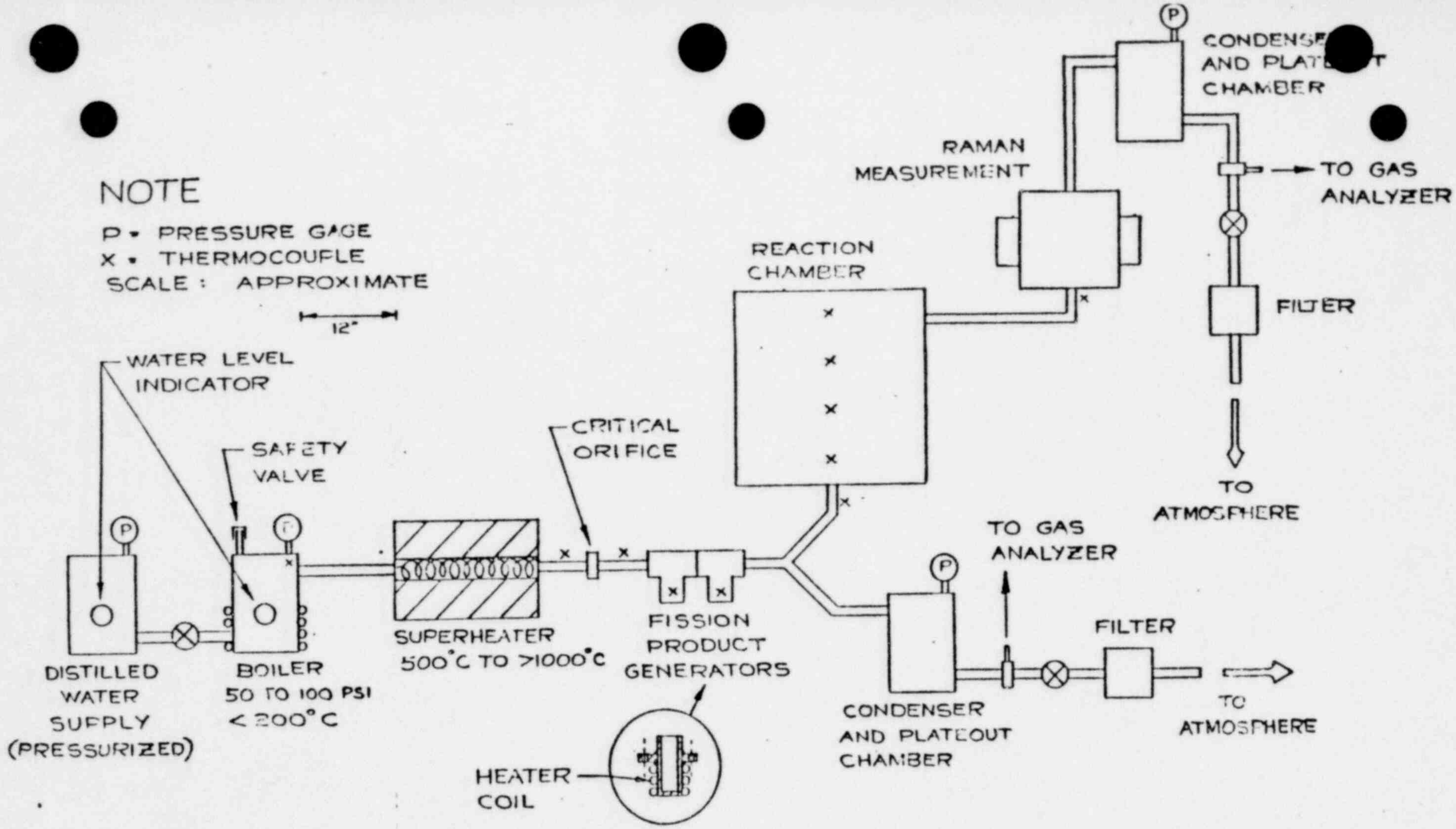


Figure 4. Fission Product Reaction System (FPRS).

ALO
9-10-79

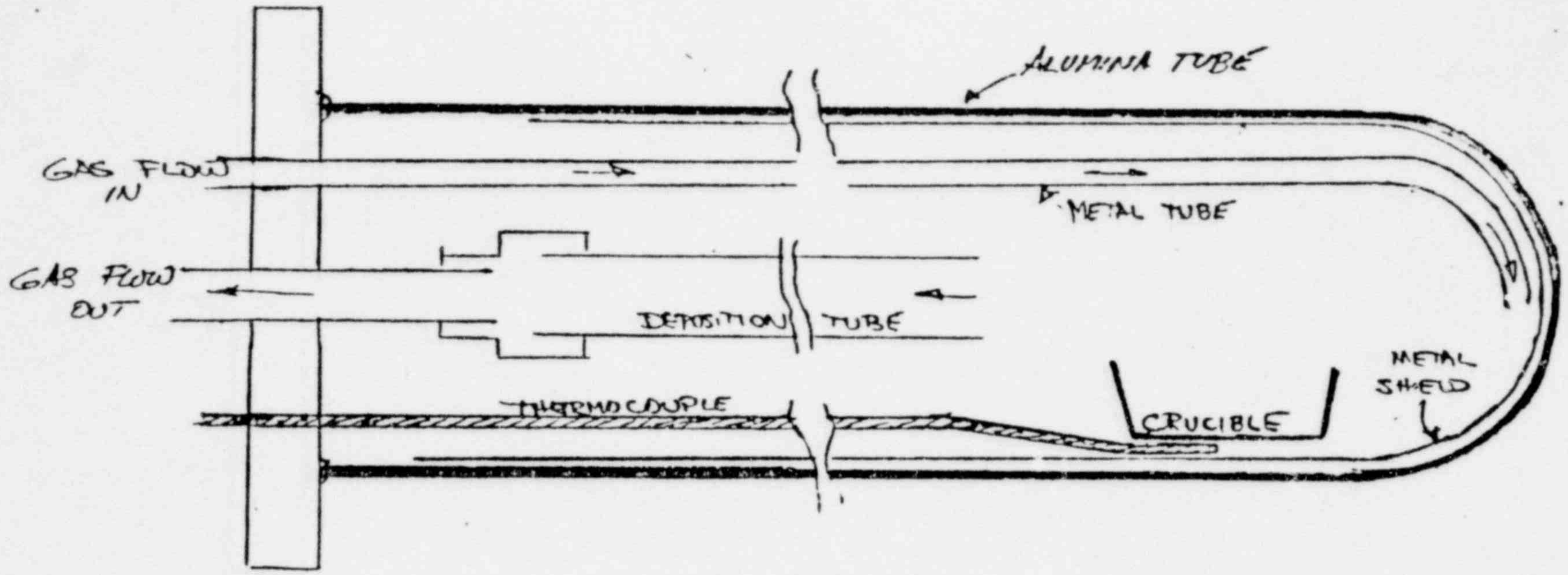


Figure 3. Schematic of Transpiration Apparatus, Sandia Labs.

SEPARATE EFFECTS TESTS FOR TRAP - RESULTS

CsI - VAPOR TRANSPORT UNAFFECTED BY H_2 AND/OR H_2O AT 770°C

CsOH - VAPOR TRANSPORT MEASURED AT 590°C IN PRESENCE OF H_2O (0.8 TORR FOR CsOH MONOMER). THIS VALUE IS ABOUT 10 TIMES GREATER THAN ANTICIPATED BASED ON COMPARISON WITH OTHER ALKALI HYDROXIDES.

CsI - NON-REACTIVE WITH STAINLESS STEELS AND NICKEL (AT 770°C).

CsOH - REACTS WITH STAINLESS STEEL, BUT NOT WITH NICKEL OR COPPER.

FISSION PRODUCT VAPOR DEPOSITION EXPERIMENTS - BCL

OBJECTIVE: TO PROVIDE EXPERIMENTALLY DERIVED FISSION PRODUCT DEPOSITION RATES AT HIGH TEMPERATURE ON PRIMARY SYSTEM SURFACES TO AID IN DEVELOPING THE TRAP CODE. TO DETERMINE THE NATURE OF THE INTERACTION BETWEEN VARIOUS FISSION PRODUCT COMPOUNDS AND PROTOTYPIC SURFACES.

STATUS: CONSTRUCTION OF FISSION PRODUCT VAPOR DEPOSITION APPARATUS IS COMPLETE.

STAINLESS STEEL AND INCONEL DEPOSITION COUPONS HAVE BEEN SUBJECTED TO SIMULATED PRIMARY SYSTEM AGING.

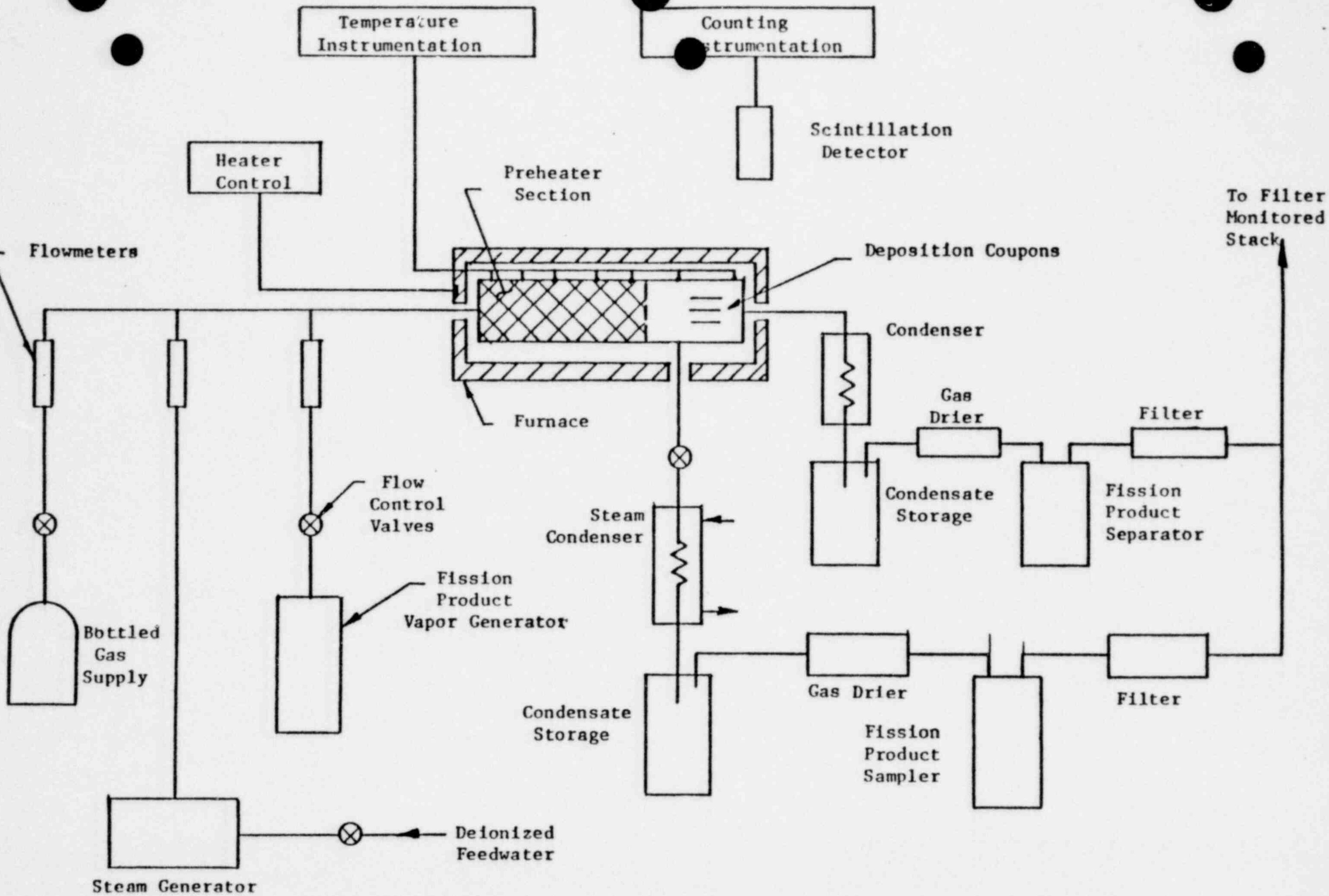
IODINE VAPOR DEPOSITION EXPERIMENTS HAVE BEEN INITIATED AND WILL BE COMPLETE IN APPROXIMATELY 4 MONTHS.

FUTURE PLANS: PROGRAM TO BE COMPLETED FY 80.

CESIUM AND TELLURIUM VAPOR DEPOSITION EXPERIMENTS WILL BEGIN IN APPROXIMATELY 4 MONTHS.

DATA WILL BE ANALYZED AND MODELS DEVELOPED FOR INCORPORATION INTO TRAP CODE.

FUNDING: FY 79 (PART OF TRAP DEVELOPMENT PROGRAM) FY 80 - 95K FY 81 - 0



SCHEMATIC OF VAPOR DEPOSITION APPARATUS

STEAM GENERATOR TUBE RUPTURE IODINE TRANSPORT - BCL

OBJECTIVE: TO DEVELOP MECHANISTIC COMPUTER MODELS FOR IODINE TRANSPORT WITHIN THE STEAM GENERATOR AND SECONDARY SYSTEM UNDER SGTR ACCIDENT CONDITIONS. TO EXPERIMENTALLY DETERMINE THE AMOUNT OF ATOMIZATION OF THE PRIMARY COOLANT DURING BLOWDOWN INTO THE SECONDARY SYSTEM.

PROJECT STATUS: DESIGN OF THE EXPERIMENTAL FACILITY TO MEASURE PRIMARY COOLANT ATOMIZATION IS COMPLETE AND CONSTRUCTION IS UNDERWAY.

THE IODINE TRANSPORT MODELS HAVE BEEN DEVELOPED AND ARE BEING ASSEMBLED INTO A COMPUTER CODE.

FUTURE WORK: PROJECT WILL BE COMPLETED IN FY 80.

THE AMOUNT OF ATOMIZATION AND DROP SIZE DISTRIBUTION WILL BE MEASURED AS A FUNCTION OF PRESSURE DIFFERENTIAL (100 - 1300 PSI).

THE SGTR IODINE TRANSPORT COMPUTER CODE WILL BE COMPLETED AND DELIVERED TO NRC/NRR.

FUNDING: FY 79 - 70K FY 80 - 63K

FISSION PRODUCT RELEASE FROM LWR FUEL - ORNL

OBJECTIVE: TO DETERMINE THE QUANTITY, SPECIES AND CHEMICAL FORM OF FISSION PRODUCTS RELEASED FROM DEFECTED FUEL RODS UNDER ACCIDENT CONDITIONS.

STATUS: PROGRAM COMPLETE.

ORNL-DWG 77-87R

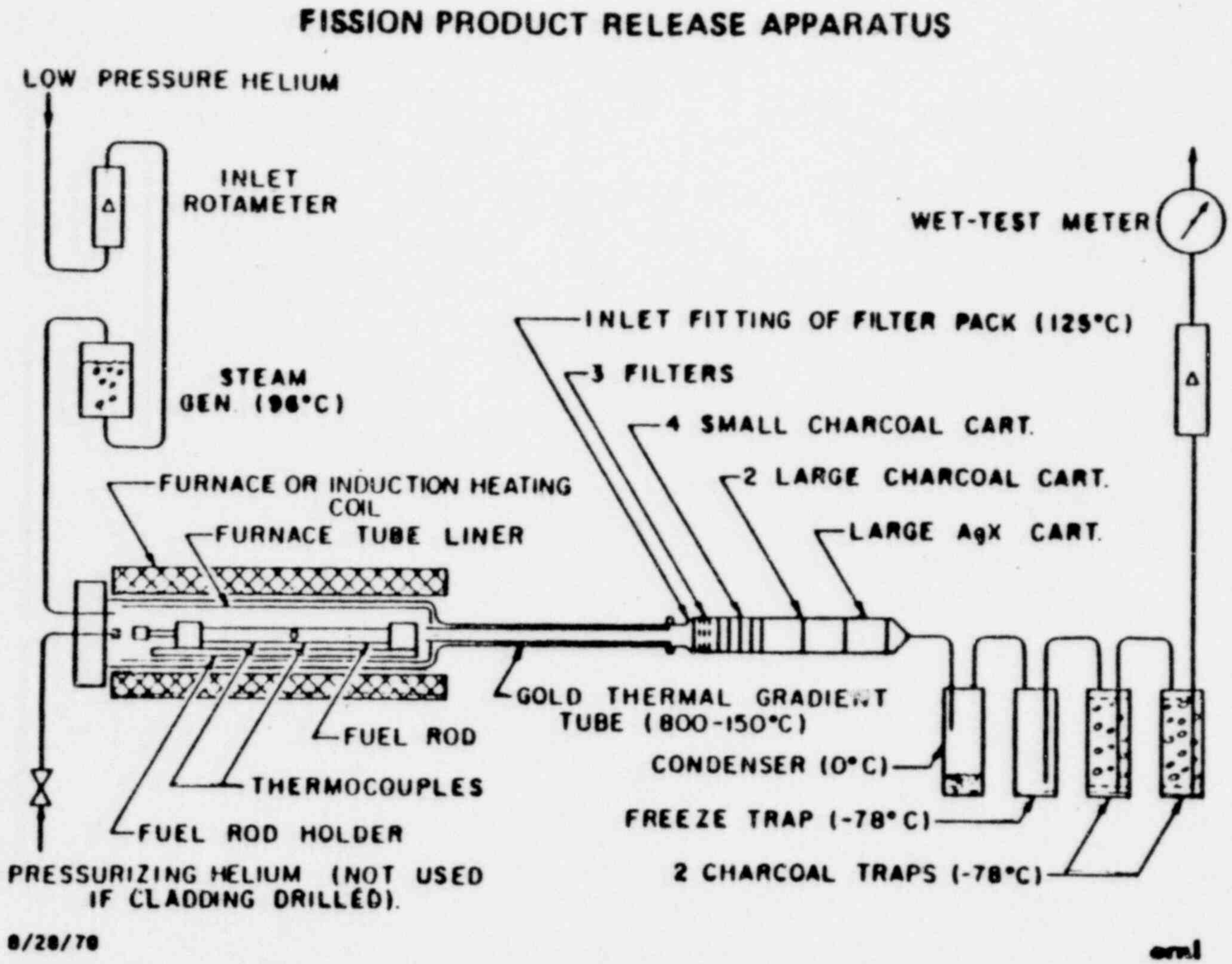


Fig. 2. Fission Product Release Apparatus

SUMMARY OF PREVIOUS WORK

	TEMPERATURE RANGE, °C	ATMOSPHERE[A]	TYPE OF RELEASE	NO. OF TESTS
IMPLANT TESTS	500-1300	S,A	BURST, DIFFUSION	12
LOW BURNUP[B]	700-900	S	DIFFUSION	2
HIGH BURNUP[C] – LOCA	500-1200	S,A,I	BURST, DIFFUSION, GAP PURGE	11
HIGH BURNUP[C] – HIGH TEMPERATURE	1300-1600	S	DIFFUSION	4
HIGH GAP INVENTORY[D] MEDIUM BURNUP	900-1200	S,I	BURST, DIFFUSION, GAP PURGE	4

[A] STEAM, AIR, INERT.

[B] BWR CAPSULE, 6-IN., IRRAD IN GETR.

[C] LOW GAP INVENTORY, PWR, 30,000 MWD/T.

[D] BWR, 12,000 MWD/T.

FISSION PRODUCT RELEASE FROM LWR FUEL - HIGH TEMPERATURE

OBJECTIVE: TO EXPAND THE INVESTIGATION OF FISSION PRODUCT RELEASE FROM DEFECTED LWR RODS WITHIN THE TEMPERATURE RANGE OF 1000°C TO 1750°C.

STATUS: 189 RECEIVED - PROGRAM START AWAITING SUPPLEMENTAL FUNDING AUTHORIZATION.

FUNDING: FY 80S - 365K -- FY 81 - 400K -- FY 82 - [redacted]

FISSION PRODUCT RELEASE FROM LWR FUEL - HIGH TEMPERATURE

SCOPE: MEASURE RELEASE FROM 12 BWR AND PWR RODS UP TO ~1750°C.

DETERMINE Cs, Kr, Ru, Ag, Sb, AND Eu BY GAMMA-SPECTROSCOPY.

DETERMINE I BY NAA.

RATIONALE: CAN USE EXISTING APPARATUS WITH MINOR CHANGES TO REACH ~1750°C.

LIMITATION: MAXIMUM TEMPERATURE IS ~1750°C.

AVAILABLE DISCHARGED LWR FUEL

REACTOR	BURNUP (MWD/T)	DATE OF DISCHARGE
DRESDEN-1, BWR	24,000	SEPTEMBER 1975
OCONEE, PWR	30,000	AUGUST 1977
BROWNS FERRY, BWR	~20,000	JANUARY 1980
POINT BEACH-1, PWR	30,000	NOVEMBER 1975
PEACH BOTTOM-2, BWR	12,000	FEBRUARY 1976
QUAD CITY-1, BWR	(24,000)	
DRESDEN-3, BWR	24,000	1973
H. B. ROBINSON, PWR	30,000	MAY 1974
BIG ROCK POINT, BWR	5,800	MARCH 1974

NOTES: REQUIRE LOW BURNUP FUEL (TMI?). EPRI MAY ASSIST FUEL ACQUISITION.

CHARCOAL FILTER IODINE RETENTION PERFORMANCE - NRL

OBJECTIVE: TO INVESTIGATE THE PERFORMANCE OF ACTIVATED CHARCOALS IN REMOVING AIRBORNE RADIOIODINE UNDER LWR ACCIDENT CONDITIONS. TO ASSESS THE EFFECTS OF IN-SERVICE WEATHERING AND EXPOSURE TO CONTAMINANTS ON THE REMOVAL AND RETENTION OF RADIOIODINE.

STATUS: 189 RECEIVED - PROGRAM INITIATION AWAITING SUPPLEMENTAL FUNDING AUTHORIZATION.

PROGRAM ELEMENTS: EXPOSE SAMPLES OF COMMERCIALLY AVAILABLE ACTIVATED CHARCOALS (IMPREGNATED WITH TEDA, KI_X AND OTHER WIDELY USED IMPREGNANTS) TO WEATHERING AND TO KNOWN ATMOSPHERIC CONTAMINANTS.

TEST THESE CHARCOALS FOR RADIOIODINE RETENTION UNDER THE RANGE OF SEVERE ACCIDENT CONDITIONS INCLUDING:

A. EXPECTED RADIOIODINE LOADINGS,

B. TOTAL RADIATION LOADING, AND

C. EXPECTED TEMPERATURE AND HUMIDITY ENVIRONMENT.

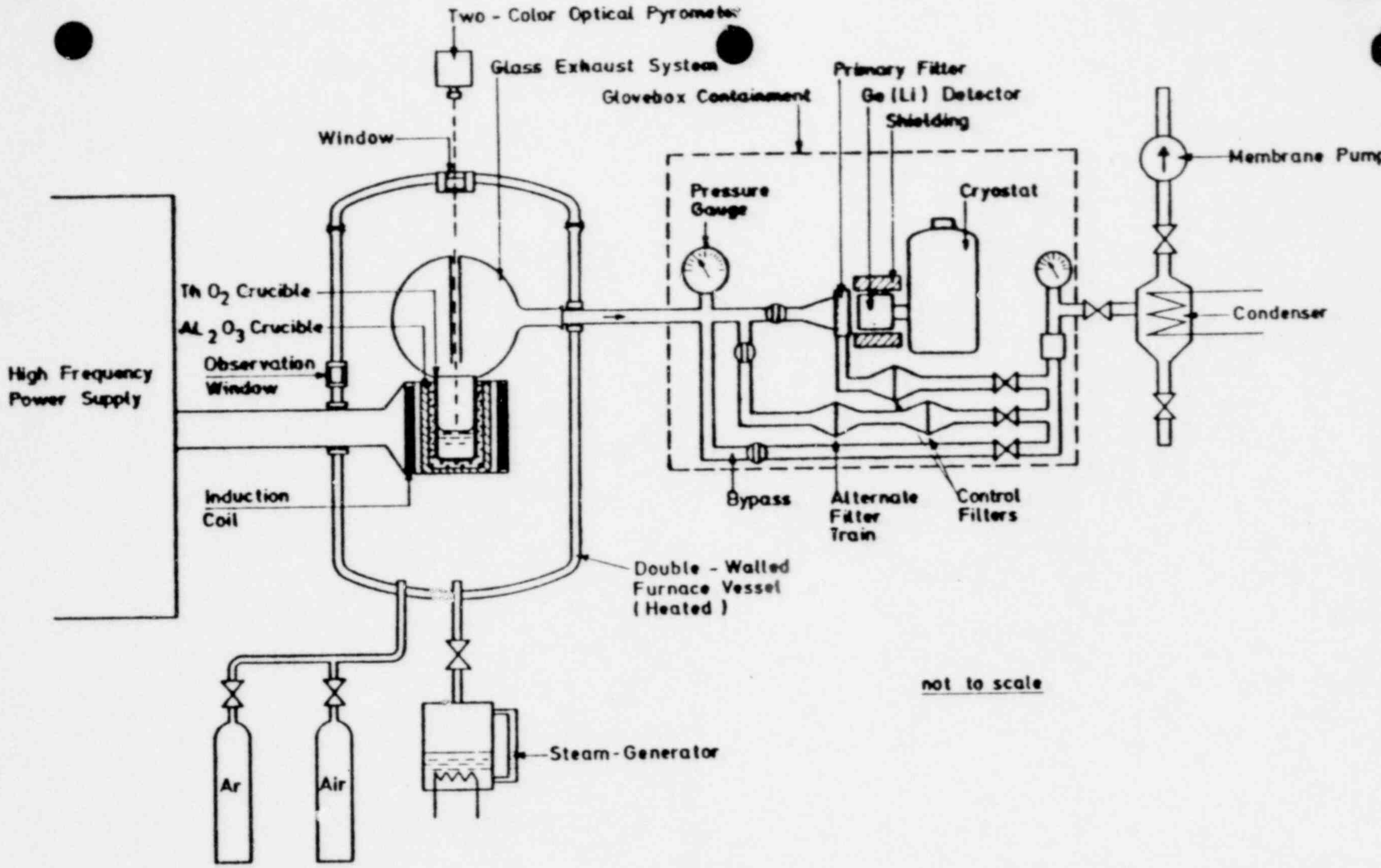
FUNDING: FY 80S - 110K -- FY 81 - 115K -- FY 82 - [redacted]

FISSION PRODUCT RELEASE - MELTING FUEL

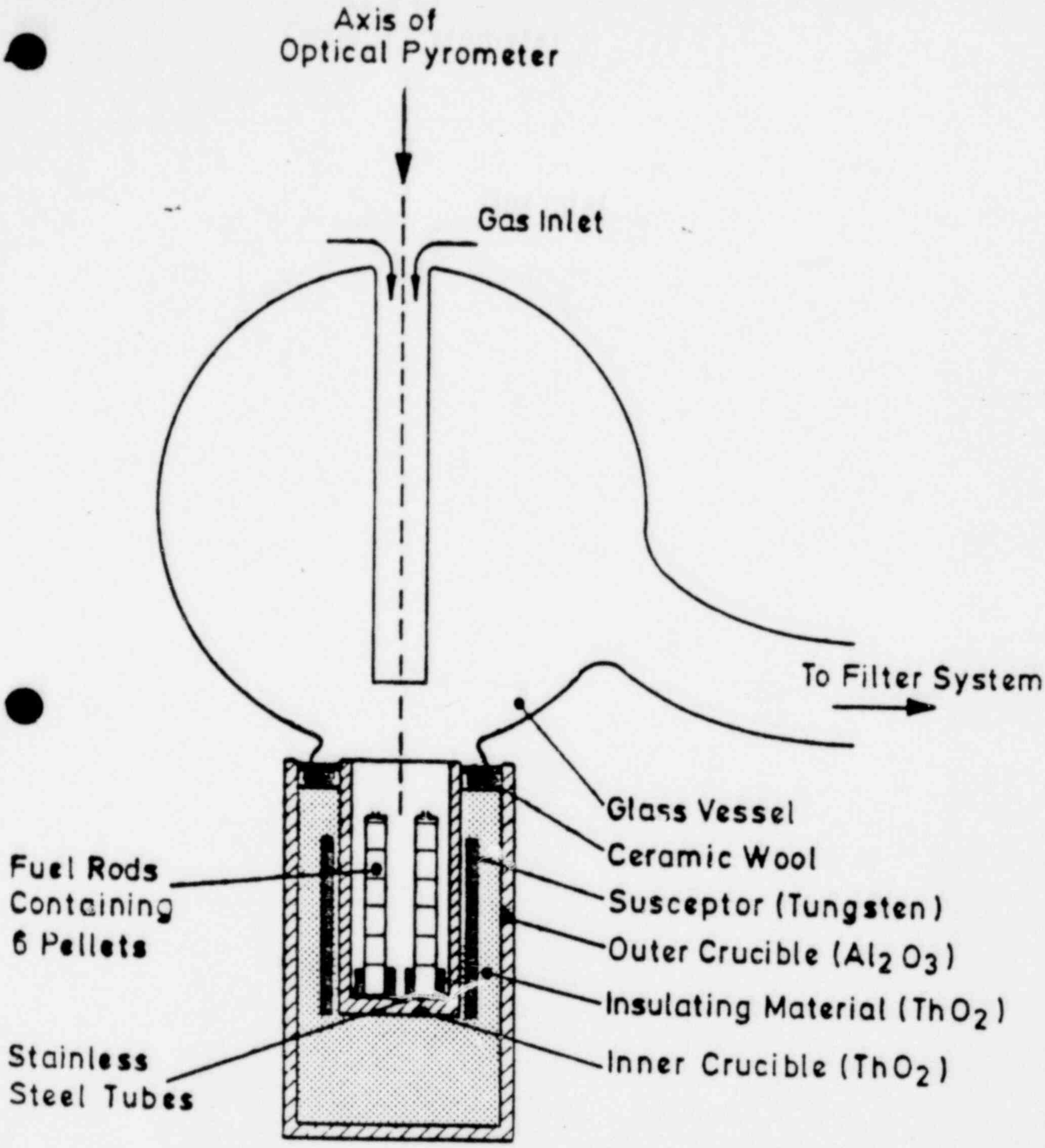
OBJECTIVE:	TO EXPERIMENTALLY DETERMINE THE RELEASE OF FISSION PRODUCTS FROM IRRADIATED LWR FUEL IN THE TEMPERATURE RANGE ~1800°C TO 2800°C.
STATUS:	PROGRAM UNDER EVALUATION.
PROGRAM ELEMENTS:	CONSTRUCT A FACILITY CAPABLE OF TRANSIENT HEATING OF COMMERCIALLY IRRADIATED FUEL ROD SEGMENTS TO MELTING IN A STEAM OR STEAM/H_2 ENVIRONMENT.
	CONDUCT EXPERIMENTS TO MEASURE THE RATE, QUANTITY, SPECIES AND CHEMICAL FORM OF RELEASED FISSION PRODUCTS UNDER HIGH TEMPERATURE INCIPIENT FUEL MELT CONDITIONS.
FUNDING:	FUNDING IDENTIFIED FOR FY 83 AND BEYOND IF NEED FOR PROGRAM IS ESTABLISHED.

CONCLUSIONS ON NEED FOR FP RELEASE MEASUREMENTS IN THE TEMPERATURE RANGE 1800 - 2800°C

- RELEASE DATA FOR A RANGE OF FP's FROM REAL FUEL RESTRICTED TO VERY SMALL SAMPLES AND WERE PERFORMED ~1965.
- OTHER DATA ON REAL FUEL RELATE MAINLY TO NOBLE GAS.
- NEW TECHNIQUES ALLOW LARGER SAMPLES AND BETTER SIMULATION OF CHEMICAL ENVIRONMENT.
- FRG DATA ON FUEL SIMULANTS SHOW QUALITATIVE AGREEMENT WITH FUEL DATA WHERE OVERLAP ALLOWS COMPARISON, BUT SIGNIFICANT QUANTITATIVE DIFFERENCES EXIST.
- FRG TESTS CANNOT INCLUDE NOBLE GAS RELEASE VOLATILE FP (I, Cs) SIMULATION QUESTIONABLE.



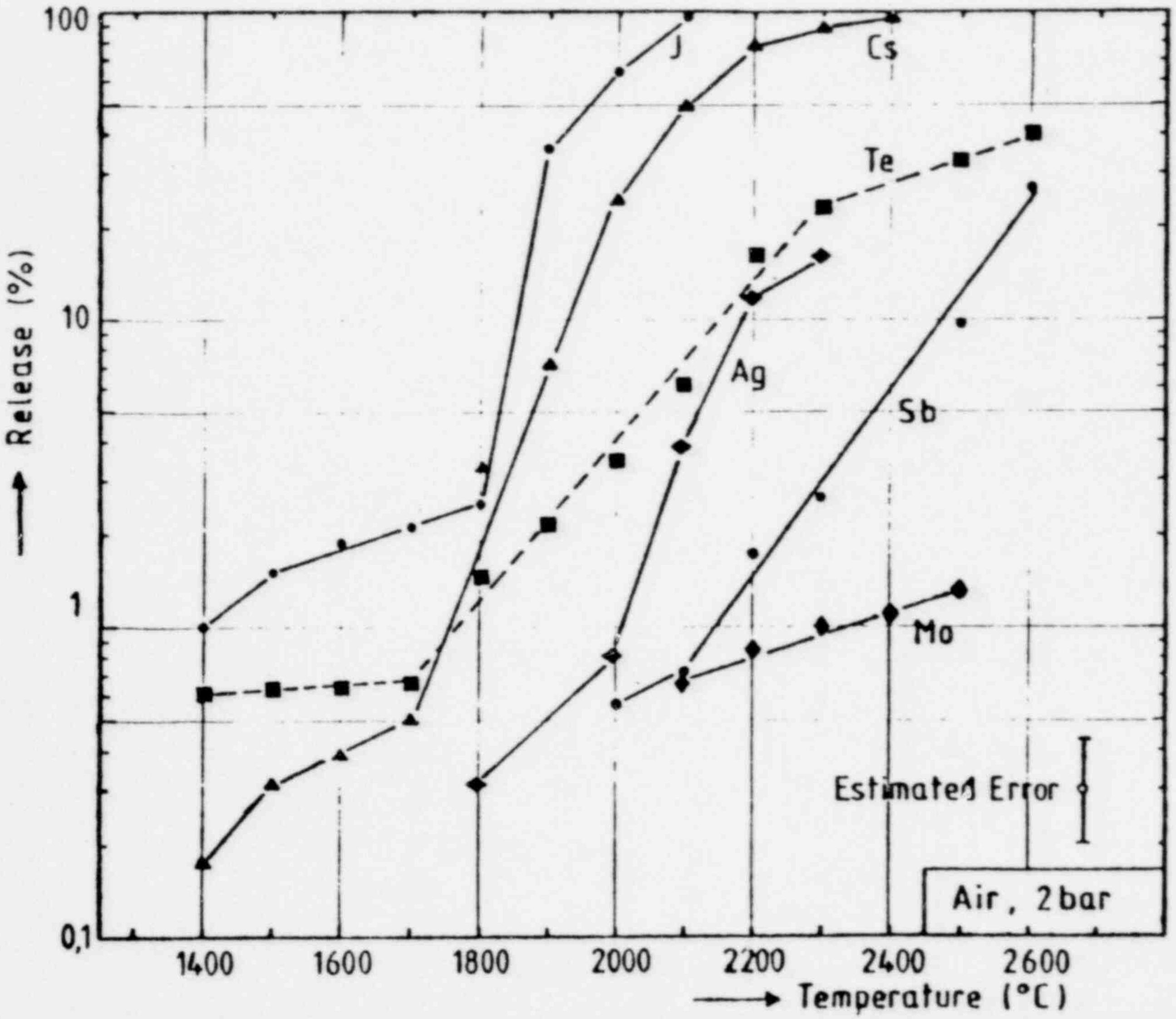
KfK-SASCHA EXPERIMENTAL APPARATUS FOR CORE MELT RELEASE STUDIES



KfK

Crucible Arrangement and Glass Exhaust System

KfK-SASCHA RESULTS



KfK/PNS-4315

Fission Product Release During Linear Heat-up With 110 °C/min

KfK-SASCHA

Plans for Future Experiments

1. Release Tests in Steam

OBJECTIVES: DETERMINATION OF RELEASE RATES AS A FUNCTION OF TEMPERATURE FOR THE MOST RELEVANT FISSION AND ACTIVATION PRODUCTS

FP: I, CS, TE, AG, SB, MO, RU, BA, ZR, CE, ND

AP: FE, CR, MN, CO, SN, ZR, NP

EXPERIMENTAL:

MASS OF SAMPLES	= 150 G OF CORIUM
BURN-UP	= 44 000 MWD/T (FISSIUM)
PRESSURE	= 0.5 , 2.0 BAR
MAX. TEMPERATURE	= 1700, 2000, 2300, 2600 °C
TIME AT T=CONST.	= 30 MIN FOR $T_{MAX} \leq 2300$ °C
	5 MIN FOR $T_{MAX} = 2600$ °C

IN EACH TEST, THE TOTAL RELEASE OF 4 - 6 RADIOACTIVE SPECIES CAN BE ANALYZED QUANTITATIVELY AS F(TIME,TEMPERATURE). THAT MEANS: NOT ALL PARAMETER COMBINATIONS CAN BE REALIZED

2. Aerosol Tests

OBJECTIVES: DETERMINATION OF ELEMENTAL COMPOSITION AS A FUNCTION OF PARTICLE SIZE

EXPERIMENTAL:

USE OF AN 8-STAGE CASCADE IMPACTOR AND HIGH EFFICIENCY GAMMA-SPECTROMETRY TO MEASURE ELEMENTAL DISTRIBUTIONS ON CALIBRATED CASCADE STAGES

PARAMETERS:

STEAM ATMOSPHERE

2 BAR

T = 2000, 2300, 2600 °C

THESE TESTS CAN BE COMBINED WITH FISSION PRODUCT RELEASE TESTS

3. Release During Melt/Concrete Interaction

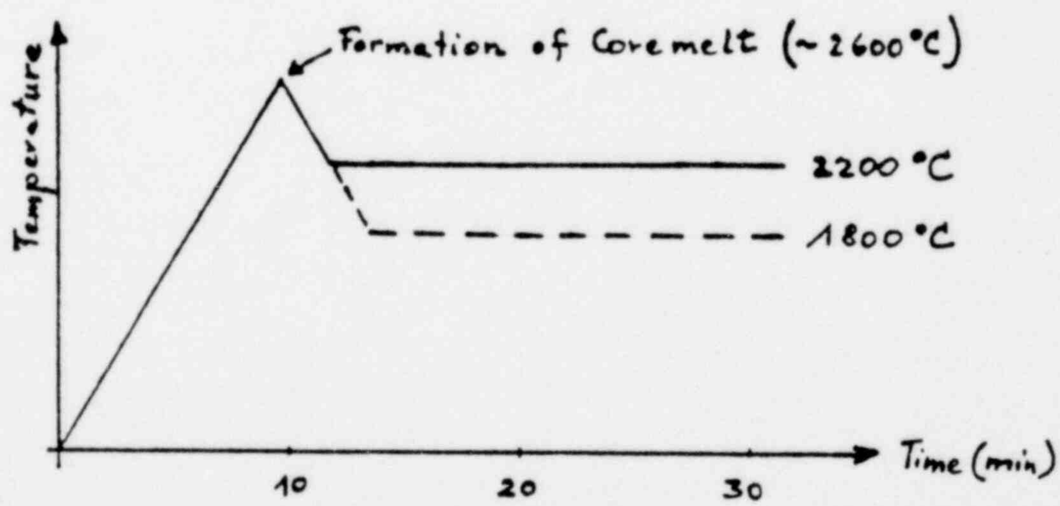
OBJECTIVES: A) TO DETERMINE TOTAL MASS RELEASE AS F(TEMP.)

B) TO FIND OUT, IF THE VOLATILITY OF CERTAIN FISSION PRODUCTS INCREASES WITH DECREASING TEMPERATURE (E.G. MO AND RU MAY FORM HIGHLY VOLATILE OXIDES WHICH ARE STABLE ONLY AT LOW TEMPERATURES)

C) TO MEASURE SIZE DISTRIBUTIONS

EXPERIMENTAL:

MASS OF SAMPLES = 150 G OF CORIUM + 150 G OF CONCRETE
ATMOSPHERE = AIR, STEAM
PRESSURE = 2 BAR
HEAT-UP RATE AND TMAX : SEE BELOW



IF NO SEVERE EXPERIMENTAL PROBLEMS ARISE, THE MASS OF CONCRETE AND CORIUM SAMPLES WILL BE INCREASED TO ABOUT

1 KG OF CORIUM + 1 KG OF CONCRETE

FISSION PRODUCT TRANSPORT VERIFICATION FACILITY

OBJECTIVE: TO CONSTRUCT OR MODIFY AN EXISTING FACILITY FOR THE PURPOSE OF TESTING THE VALIDITY OF CURRENT LWR FISSION PRODUCT TRANSPORT CODES SUCH AS TRAP-MELT, CORRAL, NAUA, ETC. EMPHASIS WILL BE ON CONTAINMENT FISSION PRODUCT BEHAVIOR. HOWEVER PRIMARY SYSTEMS EFFECTS WILL ALSO BE INCLUDED.

STATUS: PROGRAM UNDER EVALUATION.

PROGRAM ELEMENTS: DEFINE FUNCTIONAL DESIGN REQUIREMENTS FOR A FISSION PRODUCT TRANSPORT CODE VERIFICATION TEST FACILITY.

INVESTIGATE THE CAPABILITY OF EXISTING FACILITIES, SUCH AS STCF, NSPP, ETC., IN MEETING THESE REQUIREMENTS.

CONSTRUCT (OR MODIFY) A FACILITY TO PERFORM TESTS.

CONDUCT FISSION PRODUCT BEHAVIOR TESTS IN PROTOTYPIC ACCIDENT ENVIRONMENTS.

FUNDING: FY 82 - [redacted]

FISSION PRODUCT LEACHING

OBJECTIVE: TO EXPERIMENTALLY INVESTIGATE THE LONG TERM RELEASE OF FISSION PRODUCTS FROM SEVERELY DAMAGED FUEL RODS UNDER THE PHYSICAL AND CHEMICAL CONDITIONS EXPECTED WITHIN THE REACTOR VESSEL FOLLOWING A SEVERE ACCIDENT.

STATUS: PROPOSED PROGRAM UNDER REVIEW.

PRELIMINARY JUDGEMENT IS TO NONSUPPORT.

TMI FISSION PRODUCT RELEASE DATA EXAMINATION

OBJECTIVE: TO PROVIDE FUNDING FOR FISSION PRODUCT RELEASE AND TRANSPORT DATA GATHERING ACTIVITIES AND ANALYTICAL SUPPORT DURING TMI RECOVERY.

STATUS: JOINT NRC, DOE, EPRI, GPU DATA GATHERING ACTIVITY UNDERWAY.

DOE HAS COMMITTED TO PROVIDE GOVERNMENT SHARE OF FUNDING.

FUNDING INDICATED BELOW REPRESENTS A CONTINGENCY FOR DATA ACQUISITION AND ANALYSIS NOT AGREED TO BY JOINT COMMITTEE (AND NOT FUNDED BY DOE).

FUNDING: FY 80S - 175K -- FY 81 - 85K -- FY 82 -[redacted]

SEVERE CORE DAMAGE STUDIES

M. L. PICKLESIMER, FBRB/RES

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUELS

APRIL 29, 1980

SEVERE CORE DAMAGE STUDIES

- DEVELOPMENT OF CORE DAMAGE
- FISSION PRODUCT DISTRIBUTION
- MODELLING OF SEVERE CORE DAMAGE
- CODE DEVELOPMENT FOR PREDICTION OF CORE DAMAGE
- THERMAL-HYDRAULICS IN DAMAGED CORES
- CORE MELTDOWN AND CONSEQUENCES

DAMAGE POSSIBLE TO FUEL CLADDING AT ONE AXIAL LOCATION

INCREASING TEMPERATURE →

- UO_2 MELT
- $ZR-ZRO_2$ EUTECTIC + UO_2 LIQUIFIED FUEL
- $ZR-ZRO_2$ EUTECTIC FORMATION
- TOTAL OXIDATION OF CLADDING
- EMBRITTLEMENT BY OXIDATION
- OXIDATION
- RUPTURE
- BALLOONING

LOCATION ON FUEL ROD UNCOVERED

X = OCCURS

? = DEPENDS ON DETAILS OF ACCIDENT SCENARIO

PROBABLE DAMAGE

HEATING RATE, $\dot{T}$	TIME TO QUENCH	MAX TEMP 1300°C	BALLOONING AND BURST	EMBRITTLEMENT OXIDATION	TOTAL WALL OXIDATION	ZR^+ ZRO_2 EUTECTIC	LIQUIFIED FUEL	UO_2 MELT	CORE GEOMETRY LOST	CORE BLOCKED
		FUEL ROD							CORE	
HIGH	LONG	YES	X	X		X	X	X	X	X
HIGH	LONG	NO	X	?	?					
HIGH	SHORT	YES	X	X		?	?	?	X	?
HIGH	SHORT	NO	X	?						
MEDIUM	LONG	YES	X	X		?	?		X	X
MEDIUM	LONG	NO	X							
MEDIUM	SHORT	YES	X	X		?	?		X	?
MEDIUM	SHORT	NO	X							
LOW	LONG	YES	X	X	X			?	X	?
LOW	LONG	NO	X							
LOW	SHORT	YES	X	?						
LOW	SHORT	NO	X							

RESEARCH AREAS IN CORE DAMAGE STUDIES

- IN-PILE INTEGRAL EFFECTS IN BUNDLES
- EX-PILE INTEGRAL EFFECTS IN BUNDLES
- IN-PILE SEPARATE EFFECTS
- EX-PILE SEPARATE EFFECTS
- IN-PILE BASIC STUDIES
- EX-PILE BASIC STUDIES
- FISSION PRODUCT RELEASE AND DISTRIBUTION IN PRIMARY SYSTEM
- MODELLING OF SEVERE CORE DAMAGE
- CODE DEVELOPMENT FOR PREDICTION OF CORE DAMAGE

IN-PILE INTEGRAL EFFECTS IN BUNDLES

o TMI-2 CORE EXAMINATION	TMI-2
o DEBRIS BED FORMATION	PBF, TMI, ESSOR
o DEBRIS BED CHARACTERIZATION	PBF, TMI, ESSOR
o LIQUIFIED FUEL FORMATION	PBF, TMI, ESSOR
o FISSION PRODUCT DISTRIBUTION	PBF, TMI, ESSOR

EX-PILE INTEGRAL EFFECTS IN BUNDLES

- $ZR-ZRO_2$ EUTECTIC FORMATION
- LIQUIFIED FUEL FORMATION
- DEBRIS BED FORMATION, CORE SHATTERING
- "CANDLING" OF LIQUIFIED FUEL

IN-PILE SEPARATE EFFECTS STUDIES

- o SINGLE ROD CLAD AND FUEL SLUMPING
- o SINGLE ROD FISSION PRODUCT RELEASE

EX-PILE SEPARATE EFFECTS STUDIES

- SINGLE ROD OXIDATION WITH AXIAL TEMPERATURE GRADIENT
- EFFECTS OF STEAM FLOW RATES ON SINGLE AND MULTI-ROD POWER RAMPS
- SINGLE ROD FISSION PRODUCT RELEASE

IN-PILE BASIC STUDIES

- CRITICAL EXPERIMENTS ENSURING VALIDITY OF EX-PILE BASIC DATA

EX-PILE BASIC STUDIES

- OXIDATION KINETICS OF LIQUIFIED FUEL
- COMPOSITION GRADIENTS IN LIQUIFIED FUEL
- REACTION KINETICS OF UO_2 AND LIQUIFIED FUEL
- REACTION KINETICS IN MELTING DEBRIS BEDS

FISSION PRODUCT RELEASE AND DISTRIBUTION
IN THE PRIMARY SYSTEM

- o SECONDARY OBJECTIVE OF MOST IN-PILE AND EX-PILE INTEGRAL EFFECTS TESTS.
- o MOST OF THE DATA WILL BE OBTAINED BY DIFFERENCES IN PRE- AND POST-TEST COMPOSITIONS OF UO_2 PELLETS AND LIQUIFIED FUEL.
- o RELEASE RATES OF SOME FISSION PRODUCTS TO BE OBTAINED DURING PBF-SCD AND ESSOR SUPERSARA TESTS.
- o UNDEFINED SEPARATE EFFECTS TESTS SPECIFICALLY FOR FISSION PRODUCT STUDIES

MODELLING OF SEVERE CORE DAMAGE

- EARLY AND CLOSE INTERACTION WITH EXPERIMENTS IN DETERMINING DATA TYPES AND QUALITY TO BE COLLECTED IN BOTH IN-PILE AND EX-PILE RESEARCH PROGRAMS.
- DEVELOPMENT OF MODELS DESCRIBING DEVELOPMENT AND PROGRESSION OF DAMAGE.
- INTERACTION WITH EXPERIMENTERS TO DEVELOP TESTS TO EVALUATE MODELS.
- IMPROVEMENT OF MODELS

CODE DEVELOPMENT FOR PREDICTION OF CORE DAMAGE

- o INCORPORATION OF MODELS INTO A CODE TO DESCRIBE PROGRESS OF CORE DAMAGE.
- o MODIFICATION OF EXISTING CODE OR DEVELOPMENT OF A NEW CODE.

APPLICABLE PROGRAMS COMPLETED

- OXIDATION OF ZIRCALOY BY STEAM TO 1500°C, LIMITED DATA TO 1800°C (ORNL, KFK, JAERI, EPRI, AECL)
- EMBRITTLEMENT OF CLADDING BY OXIDATION (ANL, AECL)
- SCOPING STUDY ON FORMATION OF LIQUIFIED FUEL, BUNDLE DISRUPTION, EFFECT OF HEATING RATE (KFK)
- ZR-O-U PHASE DIAGRAM ABOVE 1500°C (MAY NOT BE SUFFICIENTLY DETAILED) (KFK)

IN-PILE PROGRAMS PRESENTLY PLANNED OR IN PLANNING STAGE

- ESSOR - FIRST TESTS IN FY 82, 32 ROD BUNDLES, 6 FT, BALLOON AND BURST FIRST, DEBRIS BED FORMATION, AND LIQUIFIED FUEL FORMATION. LATER, REFLOOD CAPABILITY.
- PBF - SEVERE CORE DAMAGE (SCD): TESTS STARTING IN FY 82, 6-8 TESTS 25 OR 32-ROD BUNDLES 3 FT LONG, DEBRIS BED FORMATION, LIQUIFIED FUEL FORMATION, BOILDOWN, QUENCH, REFLOOD CAPABILITY.
- LOFT - SEVERE CORE DAMAGE BEING DISCUSSED AS LAST TEST, SEVERITY TO BE DETERMINED, PROBABLY POST 1985.
- EXAMINATION OF TMI-2 FUEL.

EX-PILE PROGRAMS PRESENTLY IN PLANNING STAGE

INCIPIENT FUEL-CLAD MELT

- o LIQUIFIED FUEL FORMATION - BENCH SCALE, REACTION KINETICS WITH UO_2, "CANDLING," REMELT BEHAVIOR, COMPOSITION GRADIENTS, VISCOSITIES.
- o OXIDATION OF LIQUIFIED FUEL - BENCH SCALE, OXIDATION KINETICS OF SOLID AND LIQUID ZR-O-U COMPOSITIONS.

MODELLING OF SEVERE CORE DAMAGE

PROGRAMS TO BE PLANNED

- o REACTION KINETICS IN MELTING DEBRIS BEDS
- o IN-PILE SEPARATE EFFECTS TESTS
- o IN-PILE BASIC STUDIES
- o EX-PILE SEPARATE EFFECTS STUDIES
  - SINGLE ROD OXIDATION WITH AXIAL TEMPERATURE GRADIENT
  - EFFECTS OF STEAM FLOW RATES
  - SINGLE ROD FISSION PRODUCT RELEASE
- o DEBRIS COOLABILITY STUDIES
- o FISSION PRODUCT RELEASE AND DISTRIBUTION
- o FISSION PRODUCT SEPARATE EFFECTS TESTS
- o CODE DEVELOPMENT FOR PREDICTION OF CORE DAMAGE

SCHEDULING OF SEVERE CORE DAMAGE STUDIES

PROGRAMS IN PLACE IN FY 81

- o CORE DEGREDATION IN ESSOR
- o INCIPIENT FUEL-CLAD MELT
- o EXAMINATION OF TMI-2 FUEL
- o PBF-SEVERE CORE DAMAGE

PROGRAMS BEGINNING IN FY 82

- o MODELLING OF SEVERE CORE DAMAGE

PROGRAMS NOT PRESENTLY FUNDED IN FY 82

- o DEBRIS COOLABILITY STUDIES
- o FISSION PRODUCT RELEASE AND DISTRIBUTION

PROGRAMS STARTING AFTER FY 82

- o IN-PILE BASIC STUDIES
- o CODE DEVELOPMENT FOR PREDICTION OF CORE DAMAGE
- o PROGRAMS NOT FUNDED IN FY 82, CUT FROM FY 81 BUDGET, OR NOT PREVIOUSLY FUNDED.

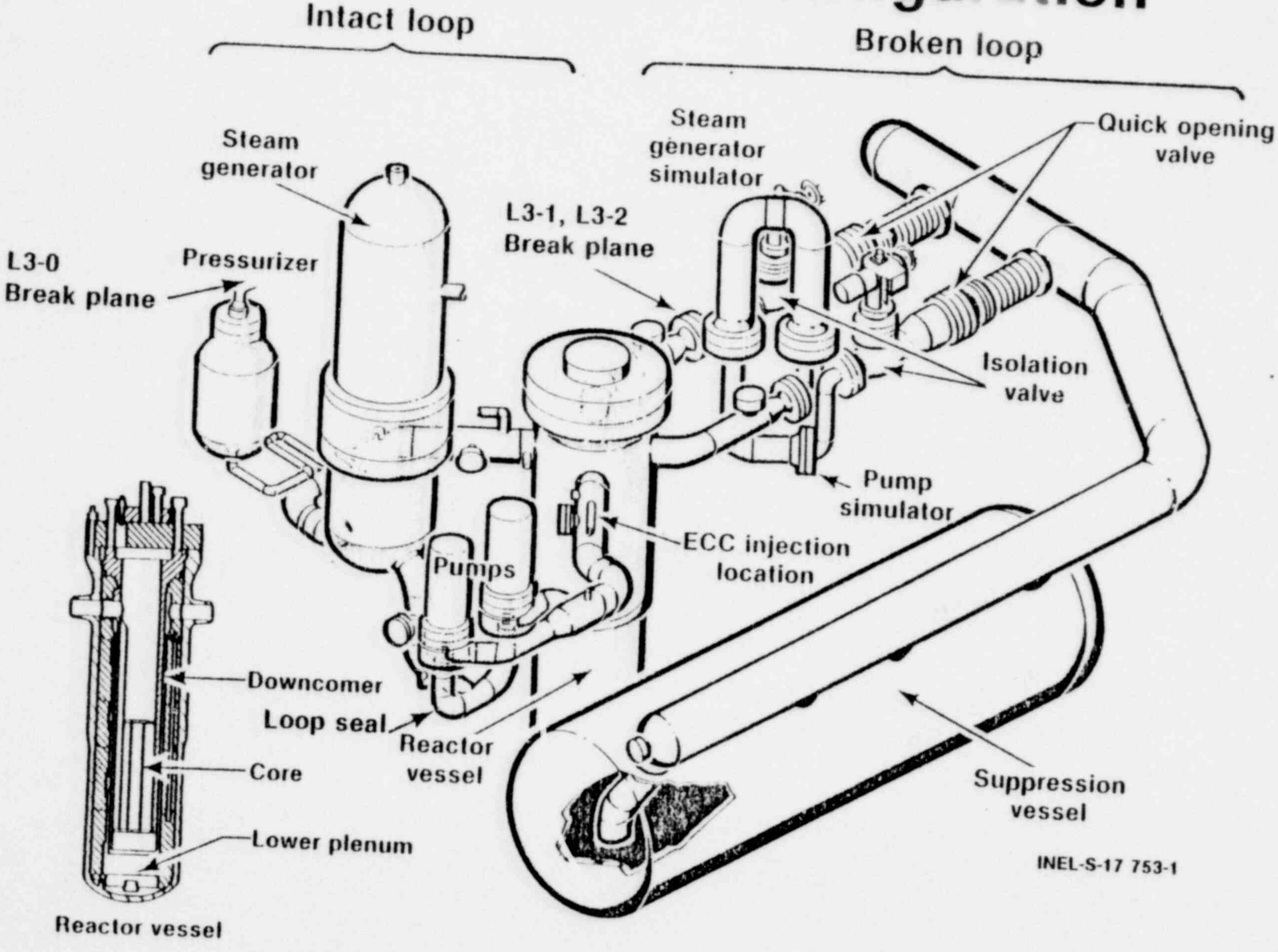
CONTENTS

- BACKGROUND AND ISSUES
- L3-1 RESULTS
- L3-2 RESULTS
- PLANS

EG&G Idaho

LOFT

LOFT System Configuration



NATURAL CIRCULATION ISSUES

- MODE OCCURRENCE
- TRANSITION STABILITY AND REVERSIBILITY
- MODE INTERRUPTION OR CESSATION AND REESTABLISHMENT
- INDICATIONS USED TO VERIFY ABOVE

LOFT

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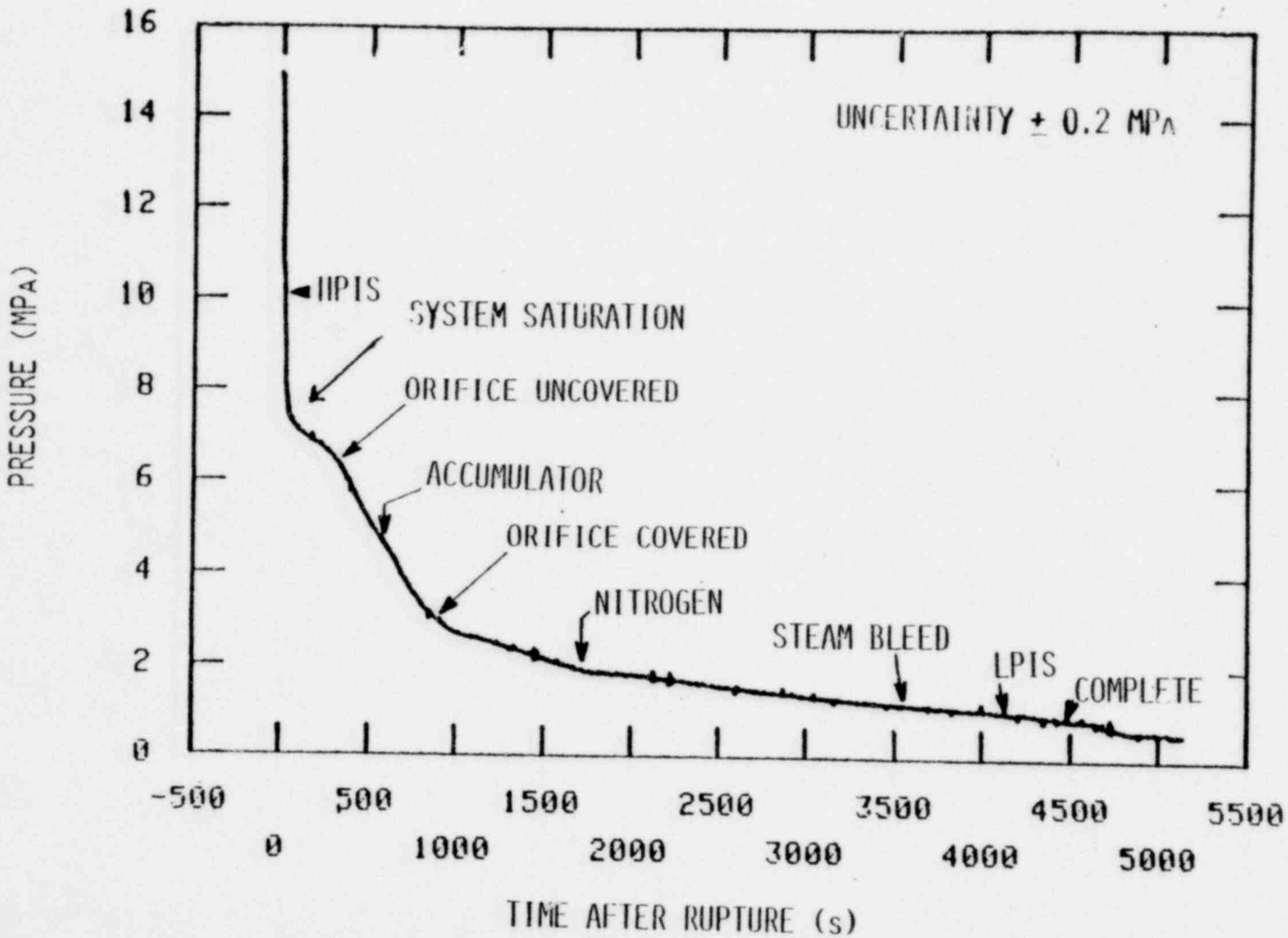
LOCE L3-1

Reactor MLHGR:	52 kW/m
Break location:	Cold leg
Break size:	2.5% of Primary Coolant pipe area

HPIS flow < Break flow

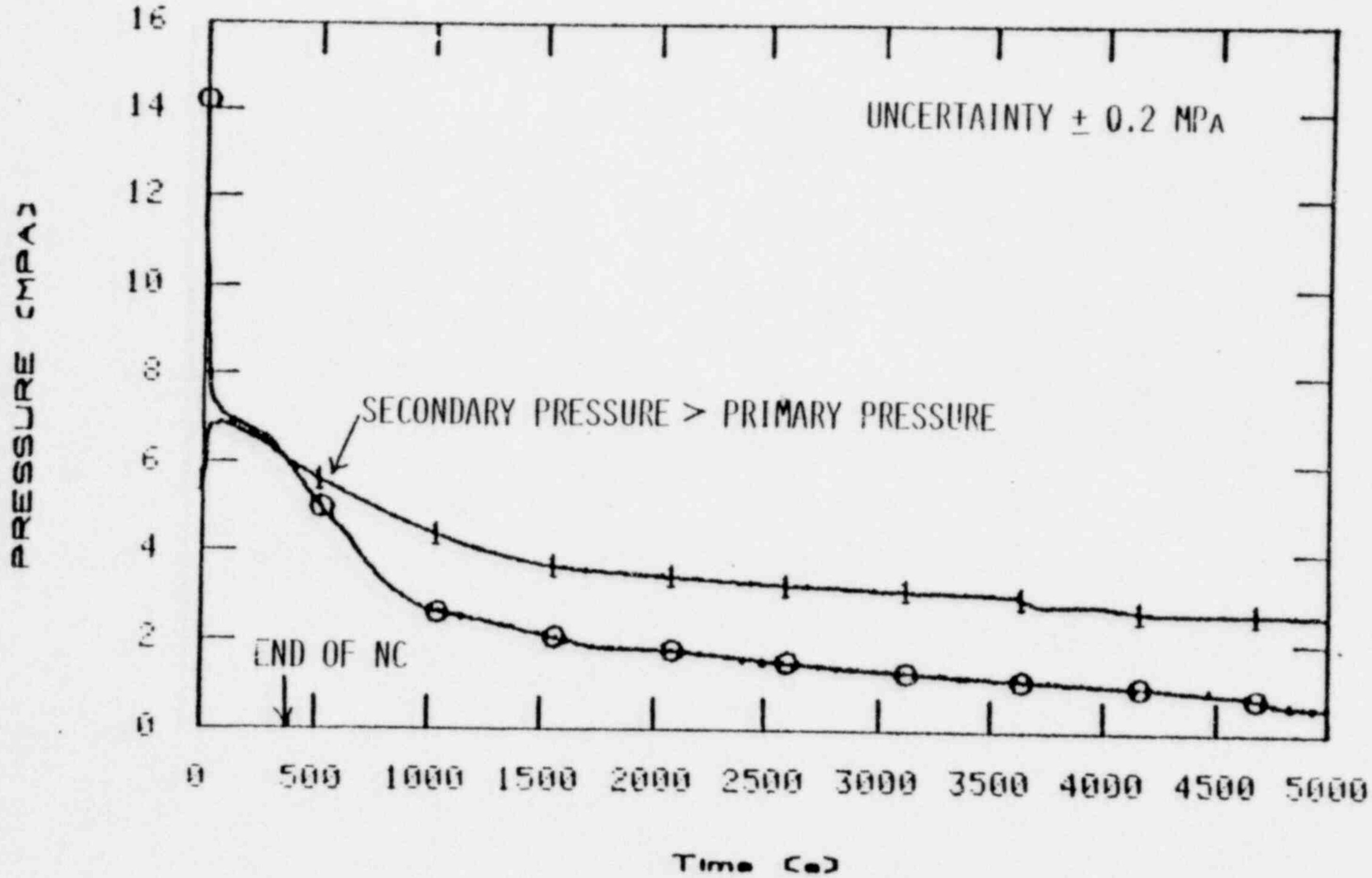
INEL-S-25 051

L3-1 SYSTEM PRESSURE



COMPARISON OF PRIMARY AND SECONDARY PRESSURES

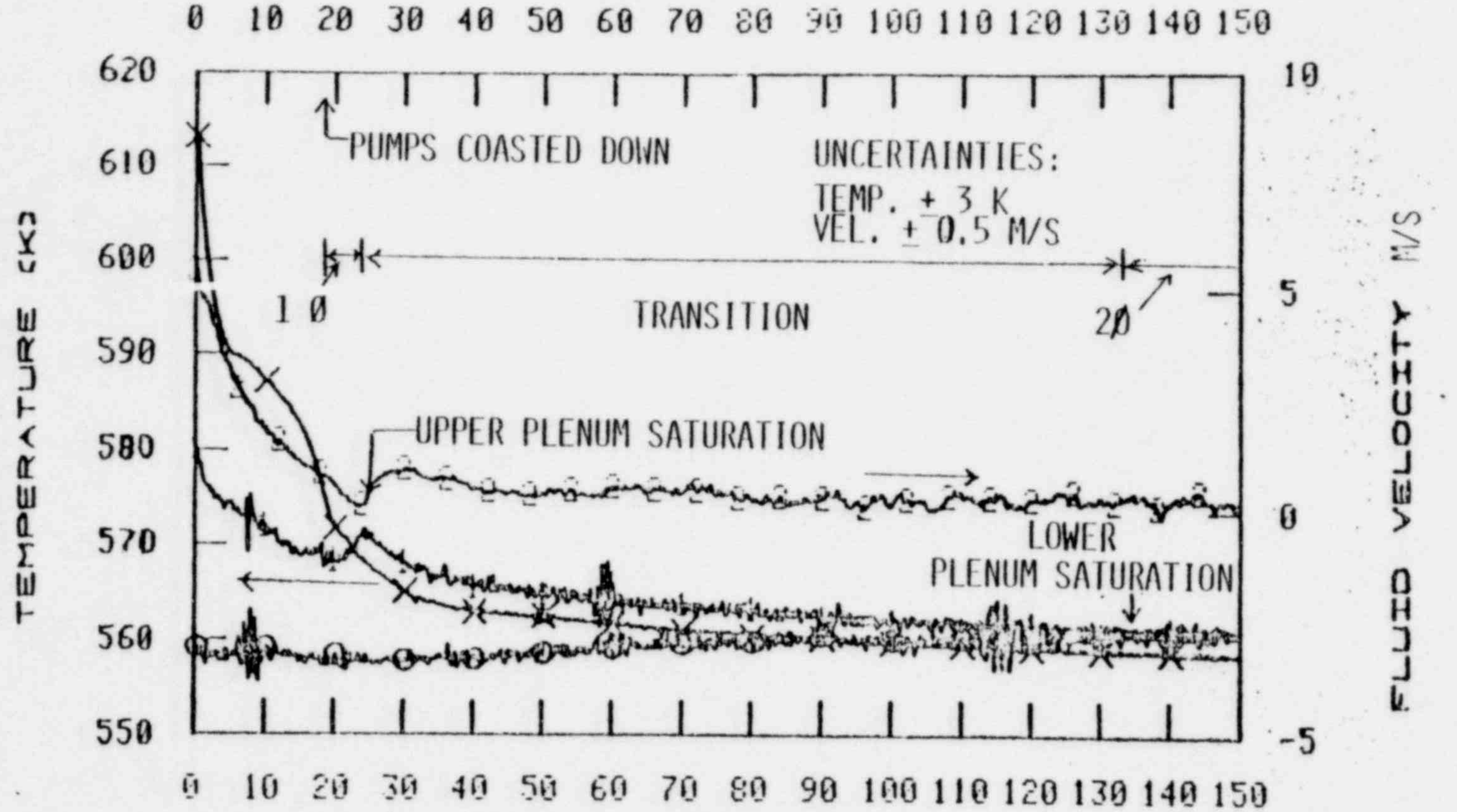
EXPERIMENT L9-1



O-INTACT LOOP REFERENCE PRESSURE
1-STEAM GENERATOR PRESSURE

COMPARISON OF UPPER AND LOWER PLENUM FLUID TEMPERATURES
AND UPPER PLENUM FLUID VELOCITY

EXPERIMENT L3-1



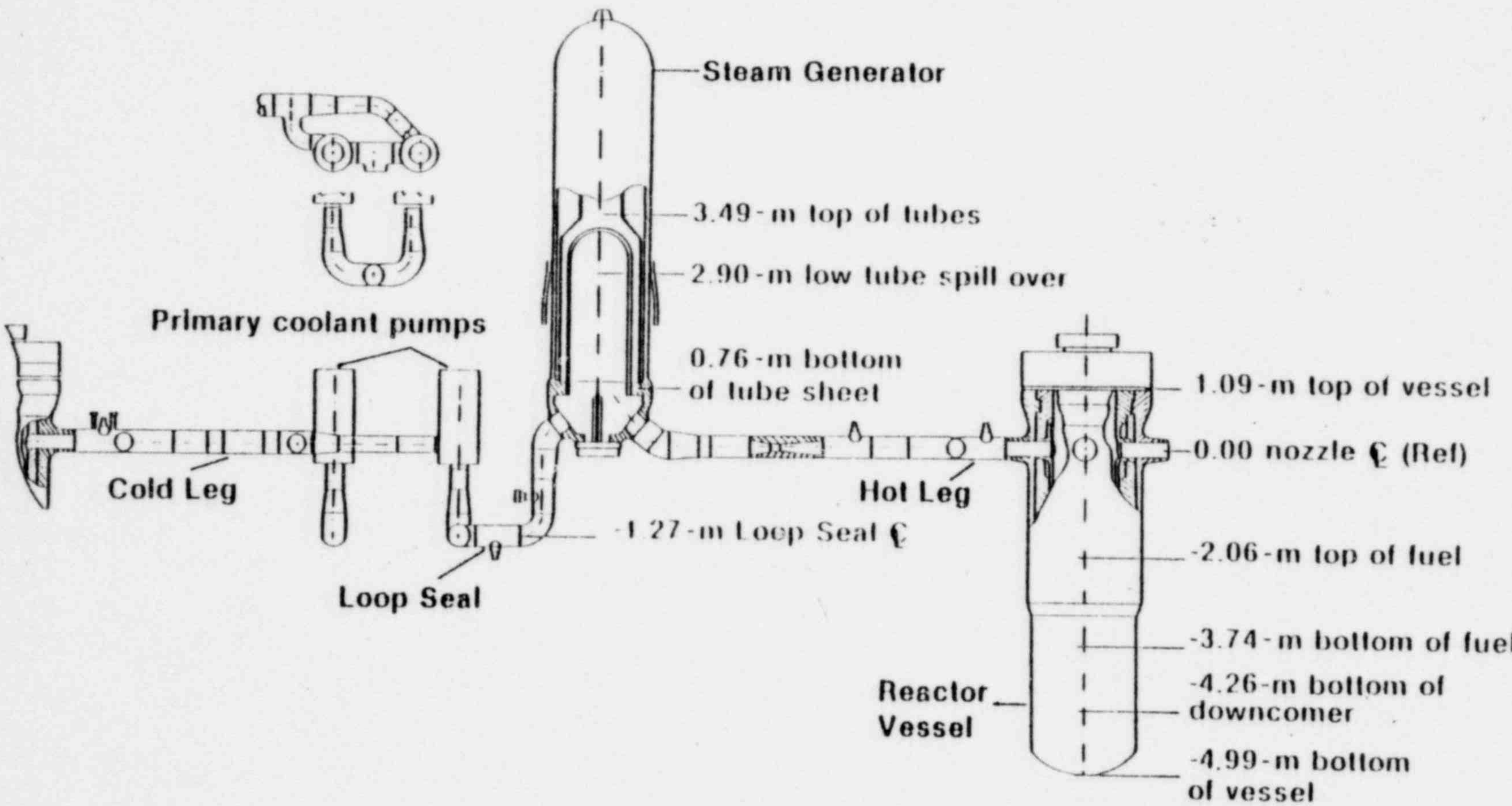
Time (s)

X–SATURATION TEMPERATURE

1–UPPER PLENUM TEMP, O–LOWER PLENUM TEMP

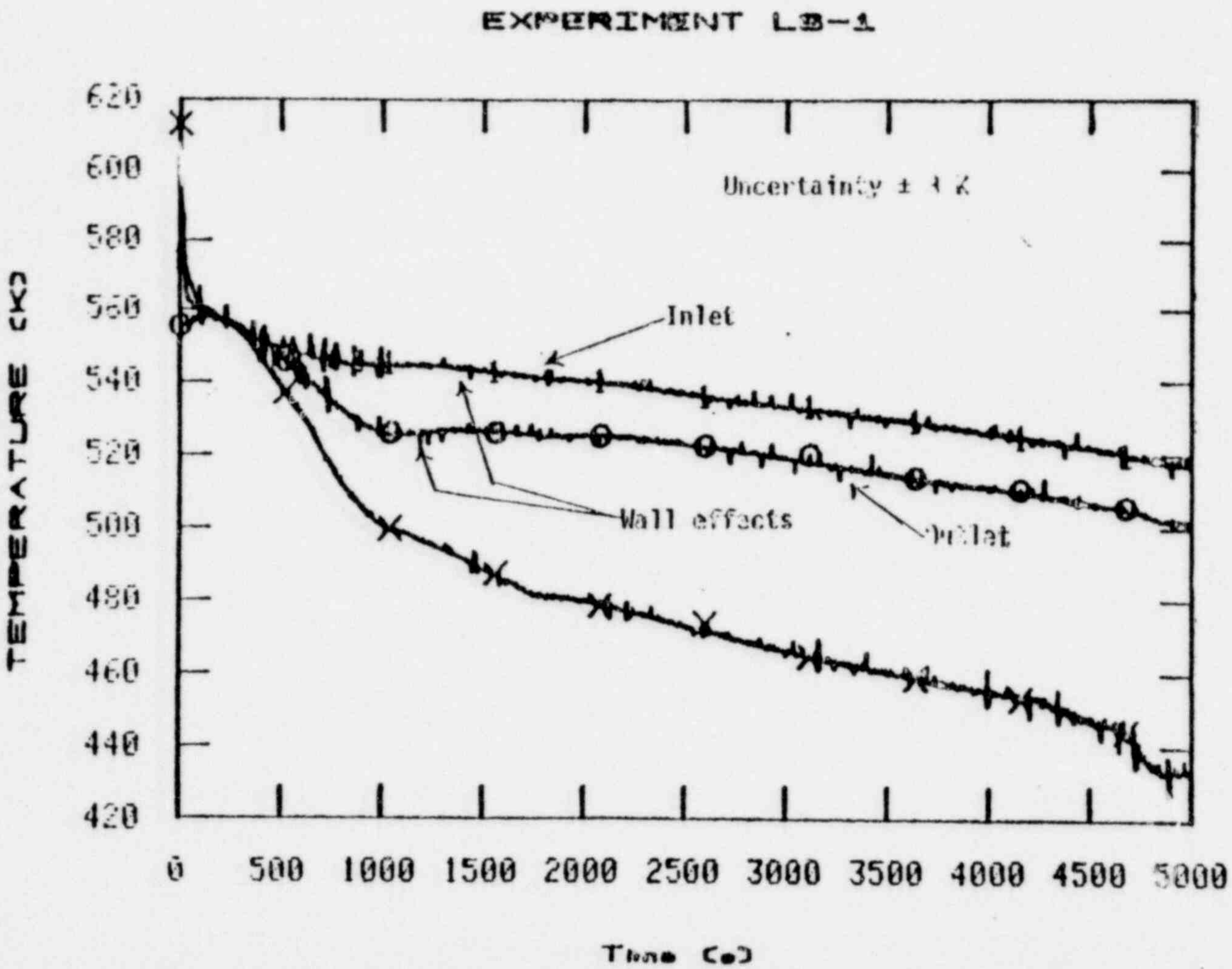
2–UPPER PLENUM FLUID EXIT VELOCITY

LOFT Intact Loop Elevations



INEL-S-25 495

COMPARISON OF THE PRIMARY SYSTEM STEAM GENERATOR INLET AND OUTLET TEMPERATURES



X—SATURATION TEMPERATURE
1—STEAM GENERATOR INLET TEMPERATURE
O—STEAM GENERATOR OUTLET TEMPERATURE

L3-1 CONCLUSIONS

- SINGLE PHASE NC NOT FULLY ESTABLISHED BEFORE TRANSITION TO TWO-PHASE NC BEGAN
- TRANSITION FROM SINGLE PHASE NC TO TWO-PHASE NC APPEARED STABLE

LOFT

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L3-1 CONCLUSIONS (CONTINUED)

- TWO-PHASE NC TERMINATED AT OR BEFORE 350 S AND DID NOT RESTART. (EFFECTIVE FOR ONLY ABOUT 7% OF TRANSIENT DURATION.)

- SECONDARY FEED-AND-BLEED, WHEN $P_{SEC} > P_{PRI}$, WAS INEFFECTIVE.

EG&G Idaho

LOFT

LOCE L3-2

Reactor MLHGR: 52 kW/m

Break location: Cold leg

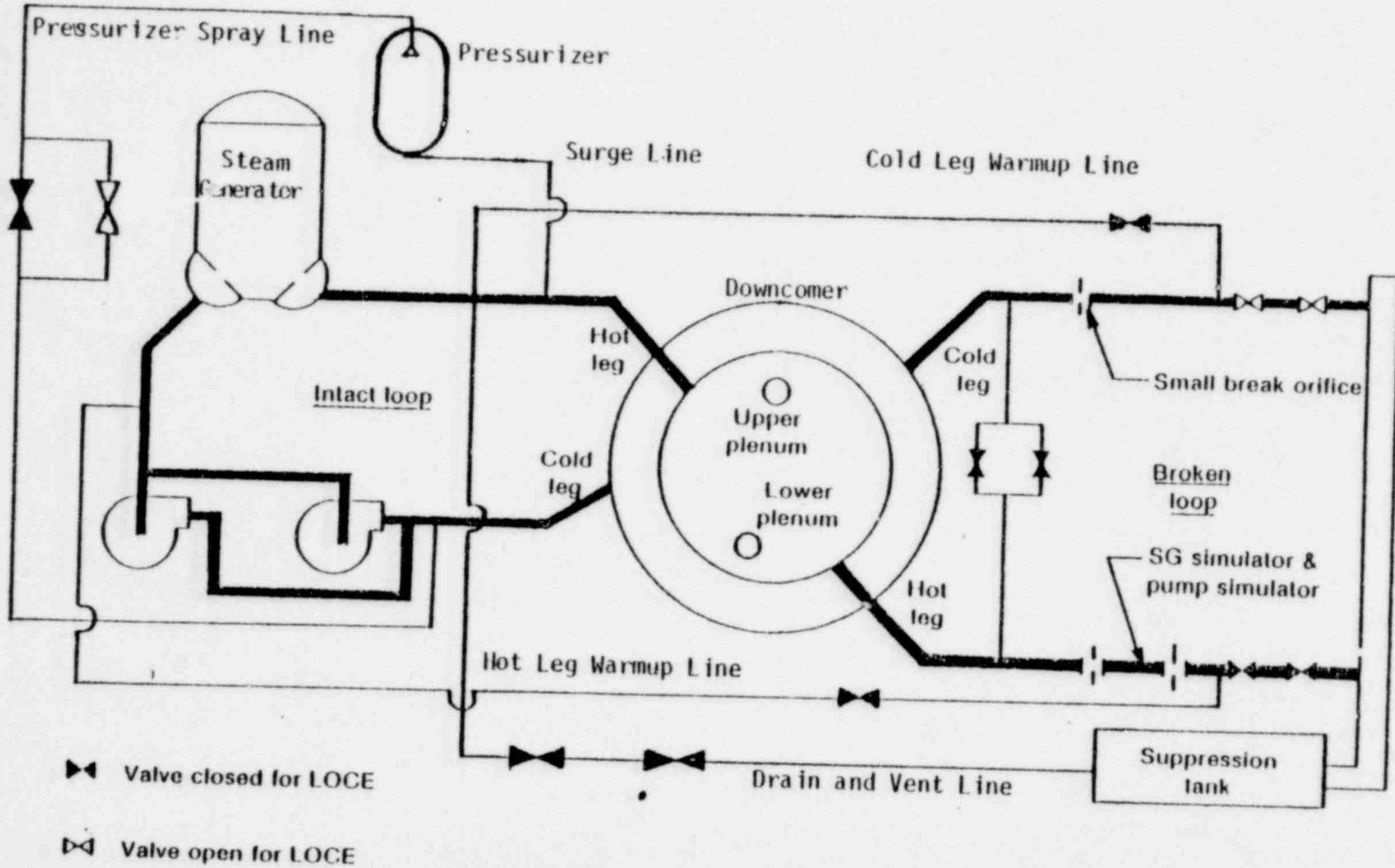
Break size: 0.16% of Primary Coolant pipe area

HPIS $\cong$ Break flow

INEL-S-25 050

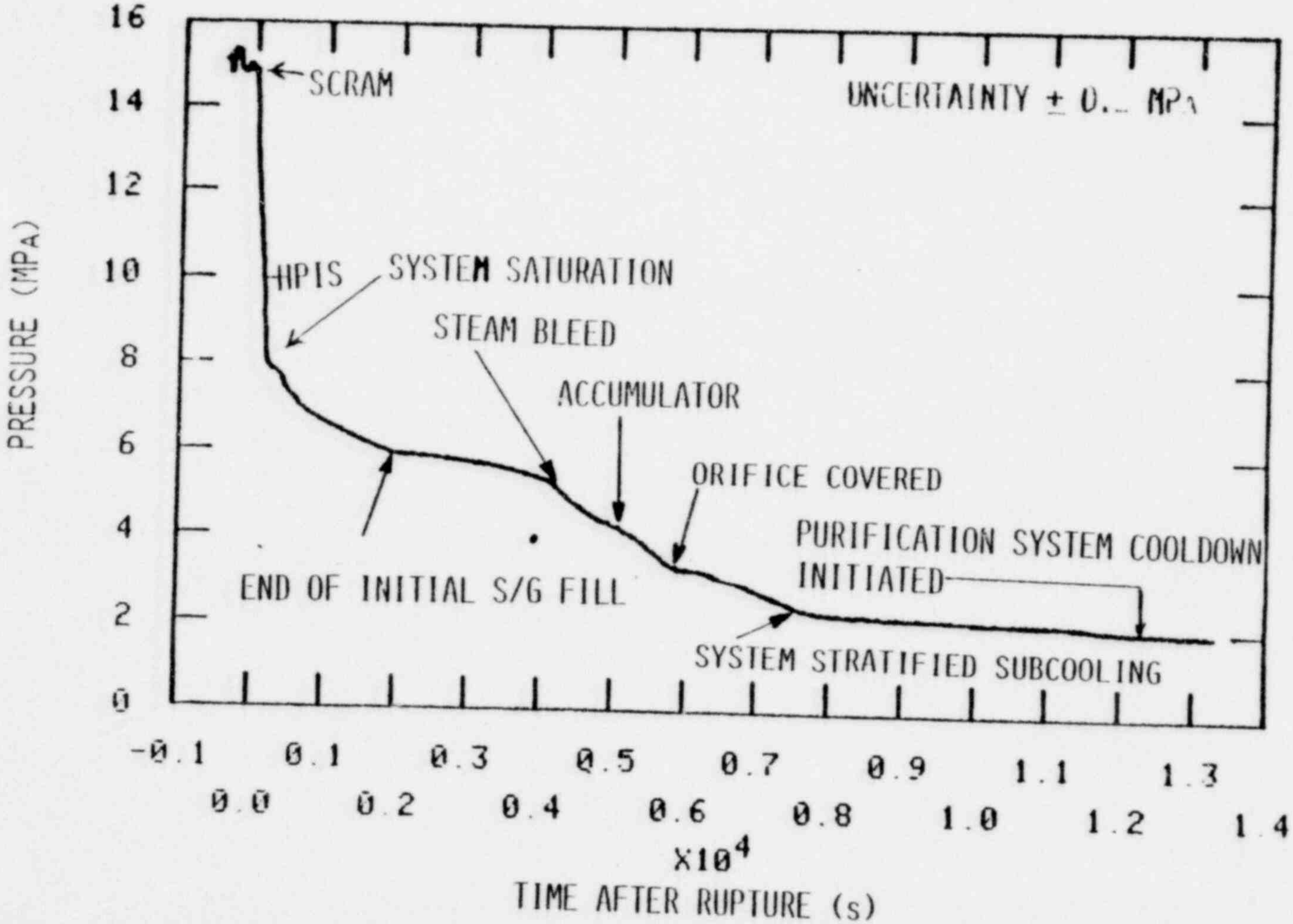
IDAHO NATIONAL ENGINEERING LABORATORY

LOFT SCHEMATIC



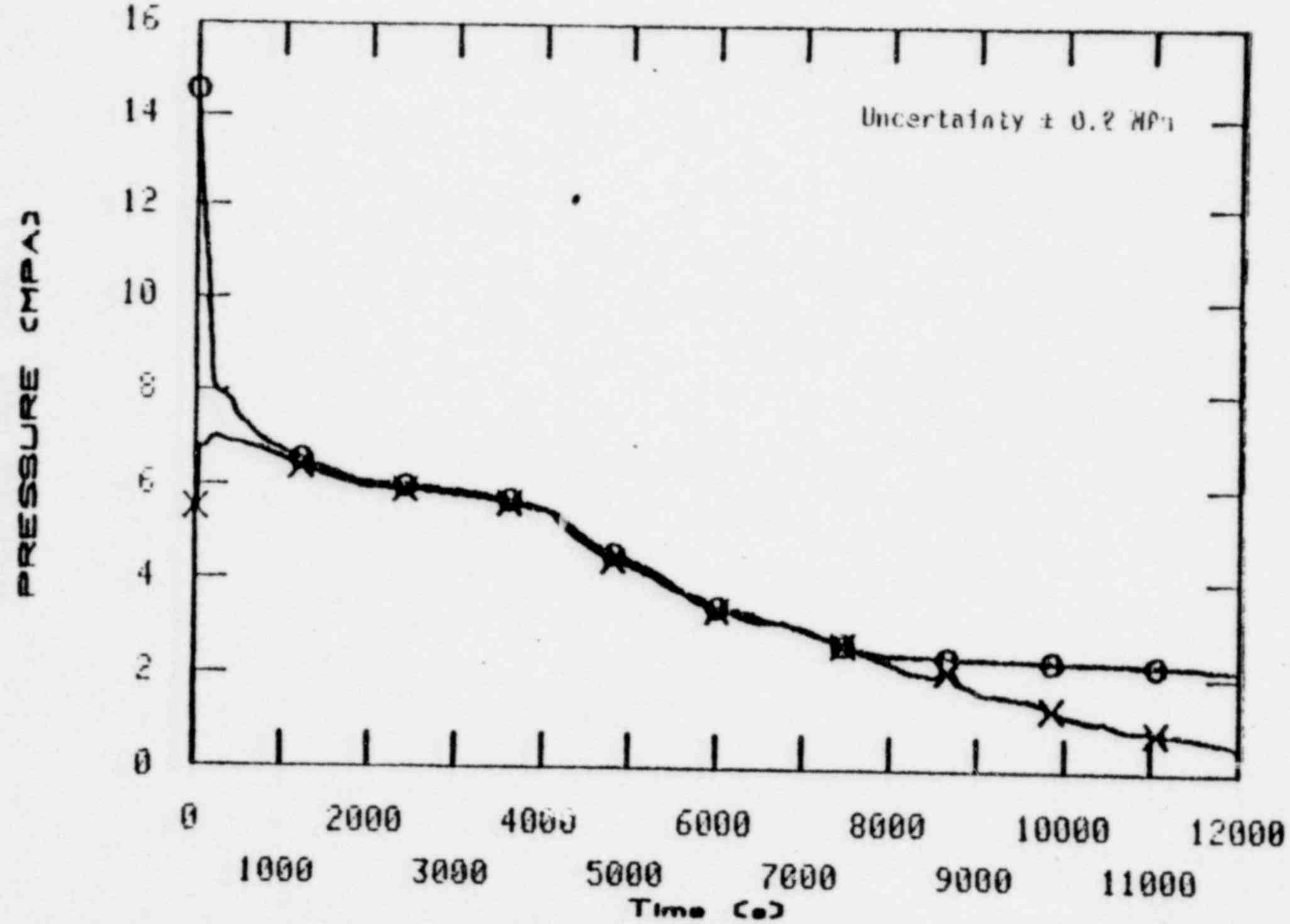
EG&G Idaho, Inc.

L3-2 SYSTEM PRESSURE

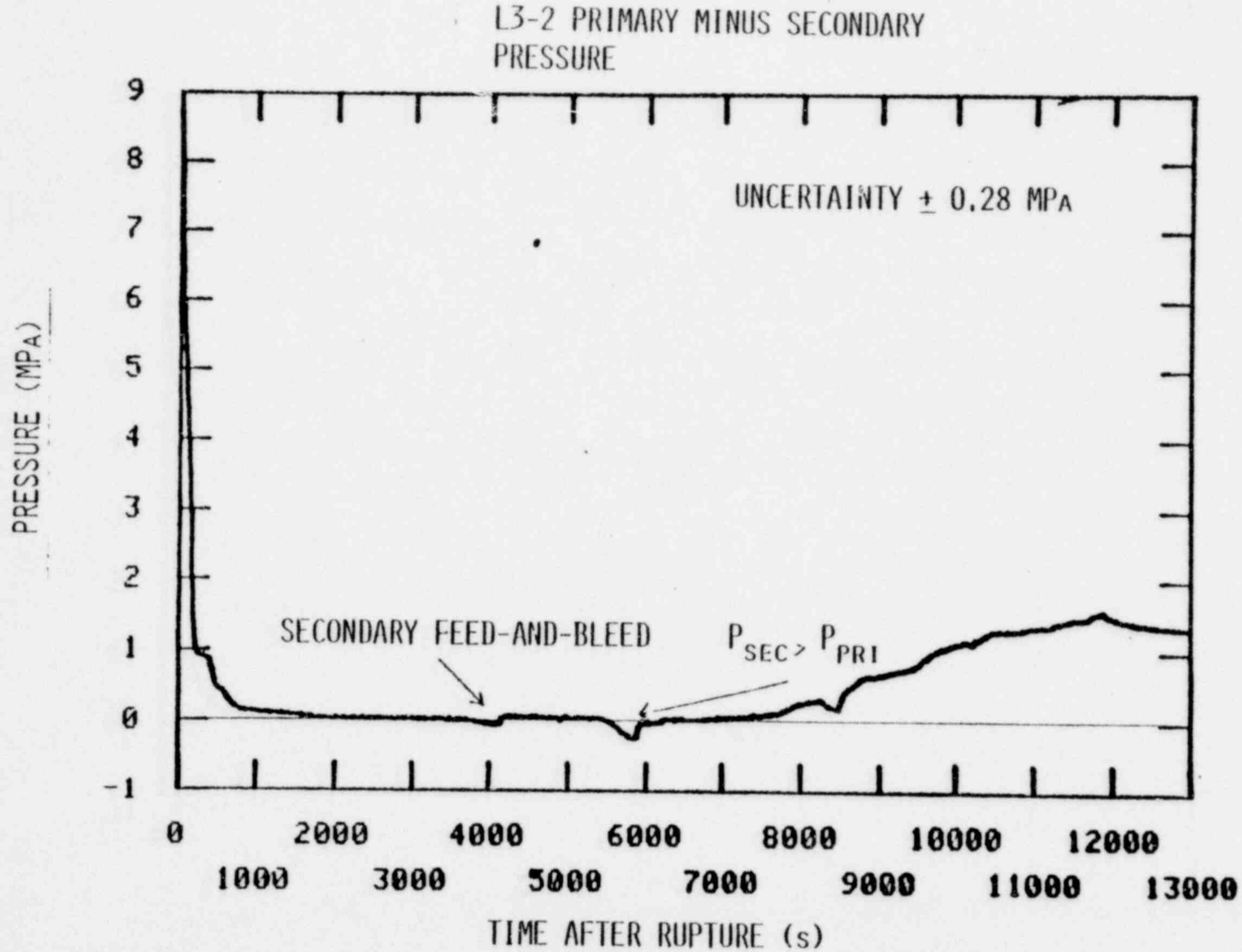


COMPARISON OF PRIMARY AND SECONDARY SYSTEM PRESSURES

EXPERIMENT L8-2



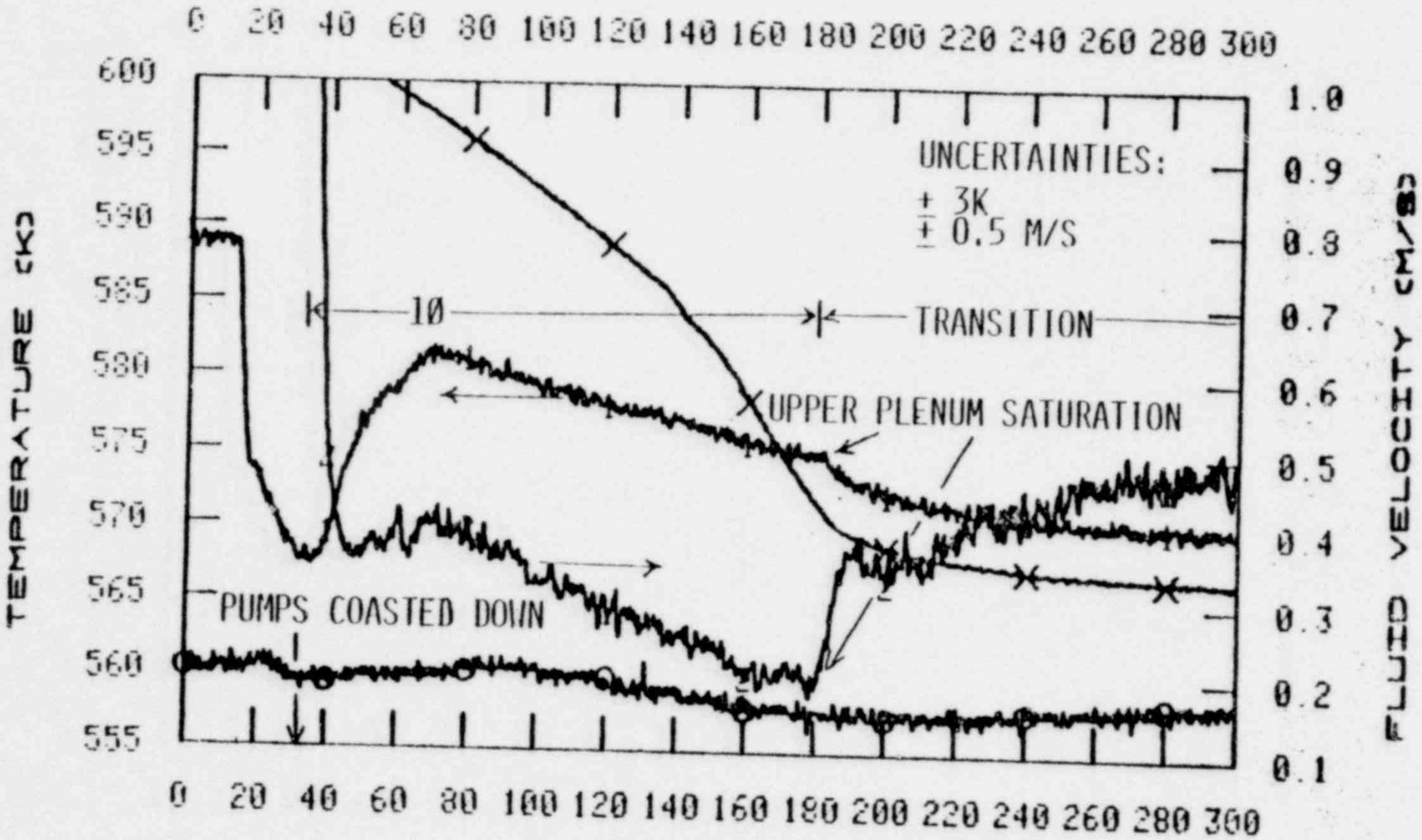
O—INTACT LOOP REFERENCE PRESSURE
X—STEAM GENERATOR OUTLET PRESSURE



L3-2 PRIMARY MINUS SECONDARY
PRESSURE
UNCERTAINTY ± 0.28 MPa
SECONDARY FEED-AND-BLEED
$P_{SEC} > P_{PRI}$
PRESSURE (MPa)
9
8
7
6
5
4
3
2
1
0
-1
0
1000
2000
3000
4000
5000
6000
7000
8000
9000
10000
11000
12000
13000
TIME AFTER RUPTURE (s)

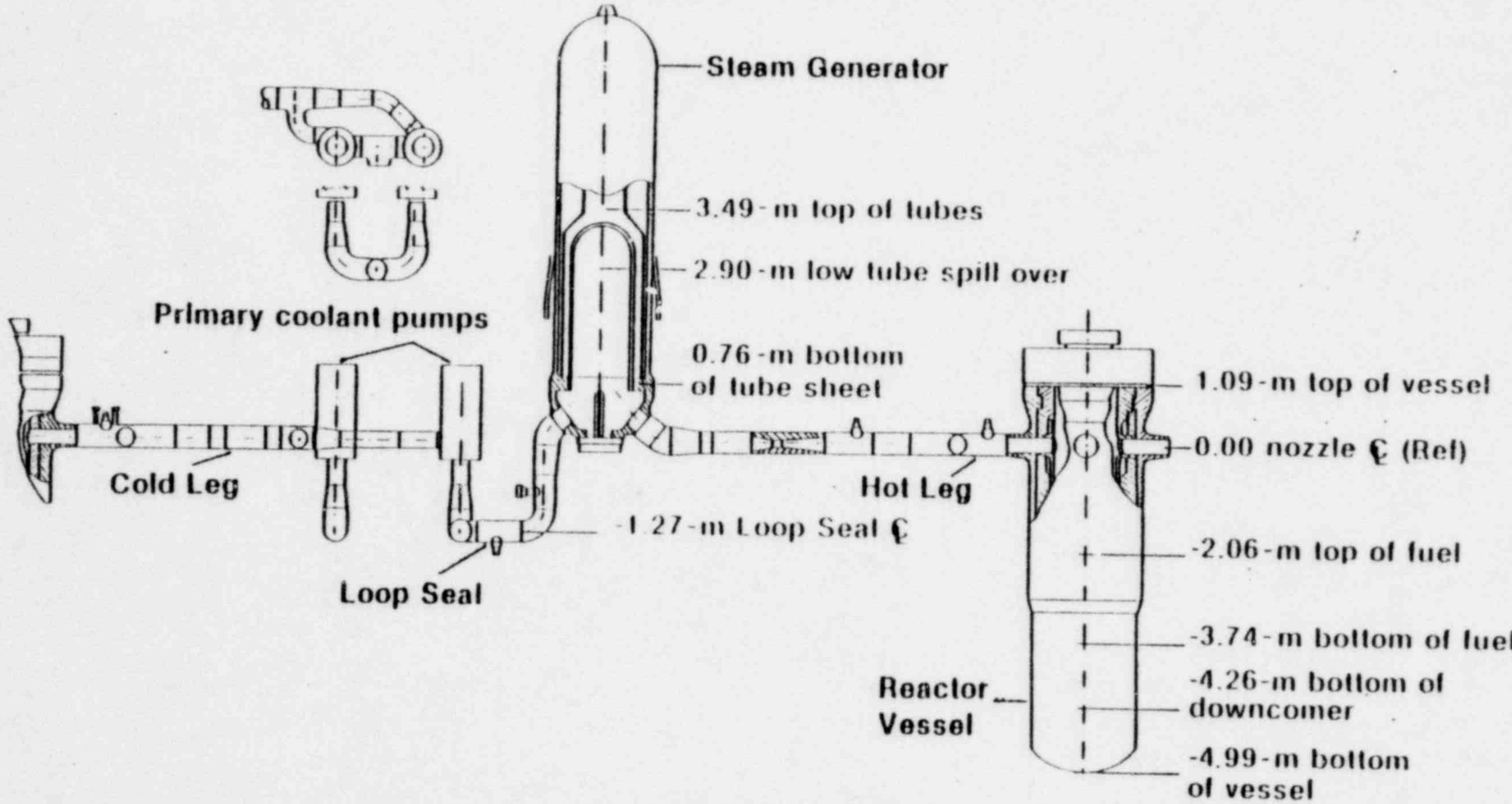
COMPARISON OF UPPER AND LOWER PLENUM
FLUID TEMPERATURES AND UPPER PLENUM FLUID VELOCITY

EXPERIMENT L3-2



X–SATURATION TEMPERATURE
1–UPPER PLENUM TEMP, O–LOWER PLENUM TEMP
2–UPPER PLENUM FLUID EXIT VELOCITY

LOFT Intact Loop Elevations

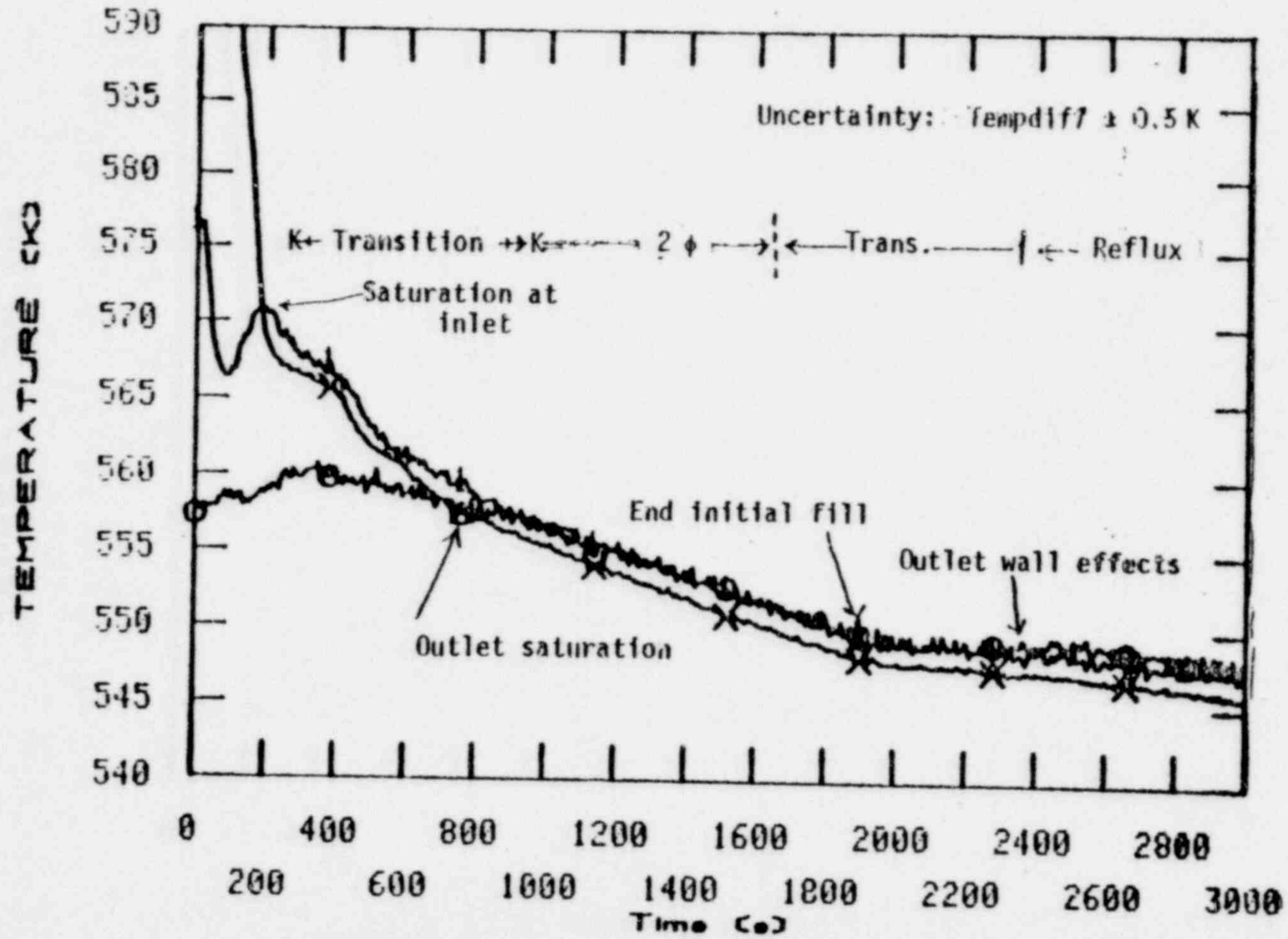


.NEL-S-26 495

COMPARISON OF PRIMARY SYSTEM INLET AND OUTLET

STEAM GENERATOR FLUID TEMPERATURES

EXPERIMENT L9-2

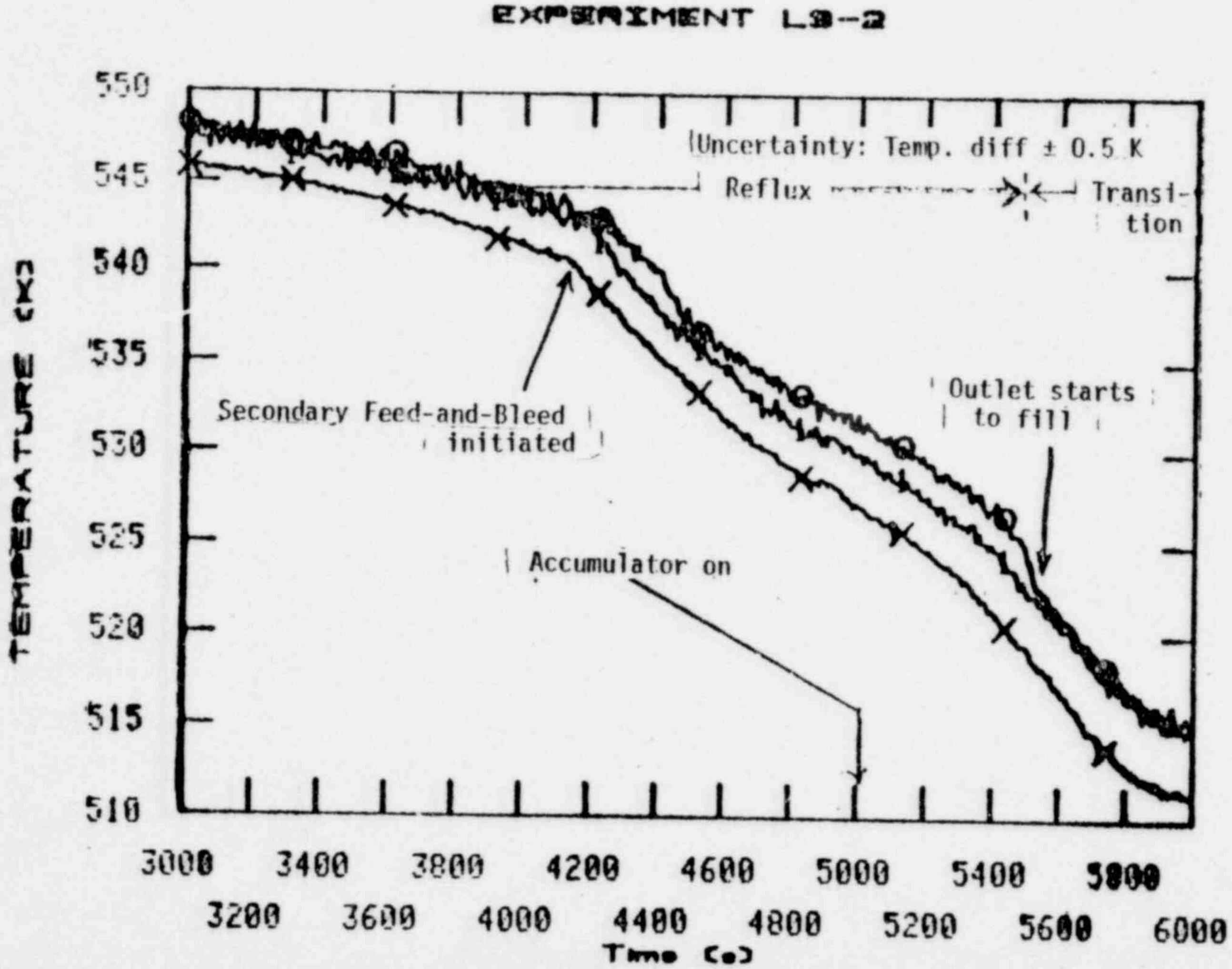


X—SATURATION TEMPERATURE

1—STEAM GENERATOR INLET TEMPERATURE

O—STEAM GENERATOR OUTLET TEMPERATURE

COMPARISON OF PRIMARY SYSTEM INLET AND OUTLET STEAM GENERATOR FLUID TEMPERATURES



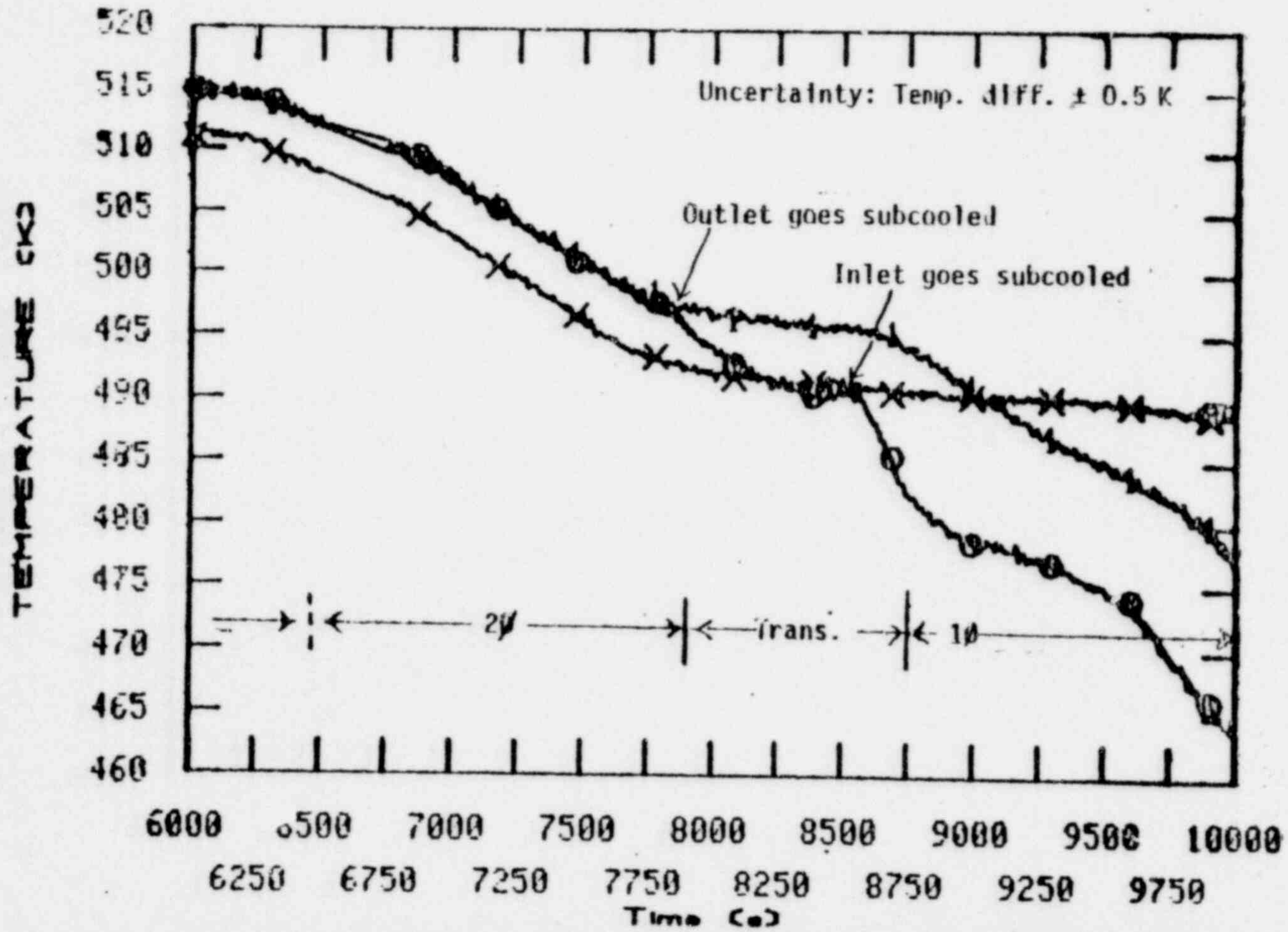
X—SATURATION TEMPERATURE

1—STEAM GENERATOR INLET TEMPERATURE

O—STEAM GENERATOR OUTLET TEMPERATURE

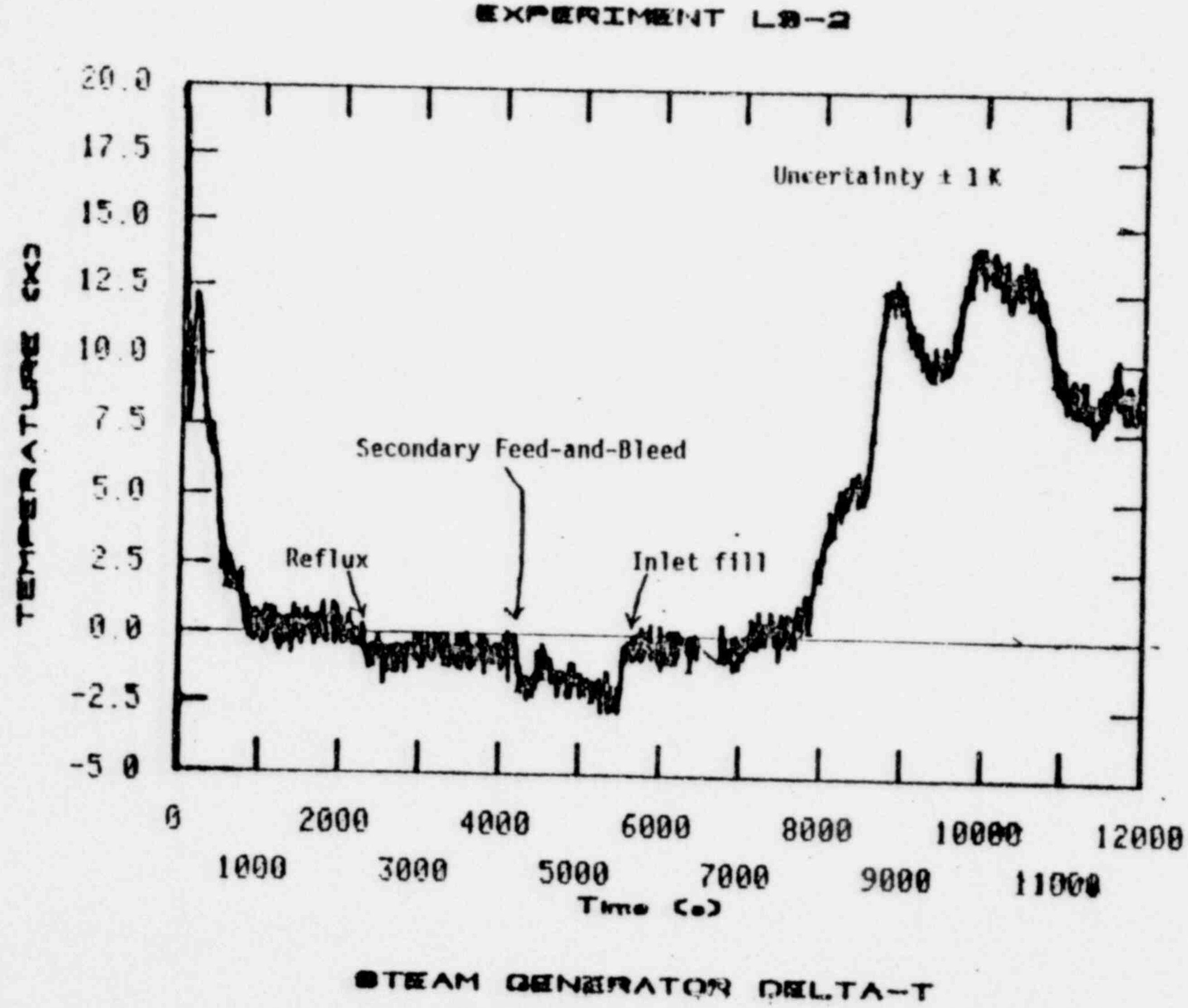
COMPARISON OF PRIMARY SYSTEM INLET AND OUTLET
STEAM GENERATOR FLUID TEMPERATURES

EXPERIMENT L8-2



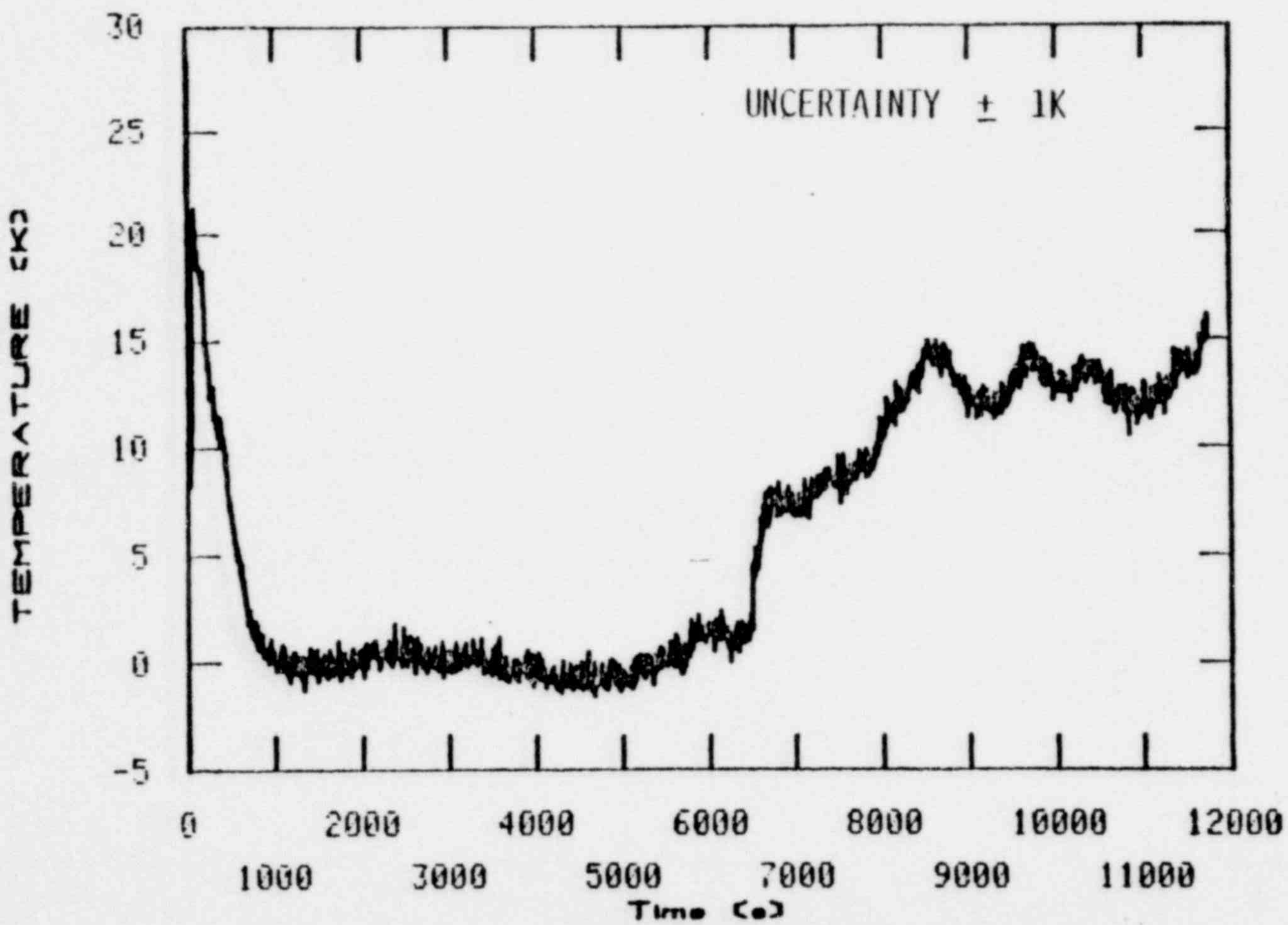
X-SATURATION TEMPERATURE
1-STEAM GENERATOR INLET TEMPERATURE
O-STEAM GENERATOR OUTLET TEMPERATURE

STEAM GENERATOR INLET MINUS OUTLET FLUID TEMPERATURE



CORE ΔT ACROSS CENTER FUEL MODULE

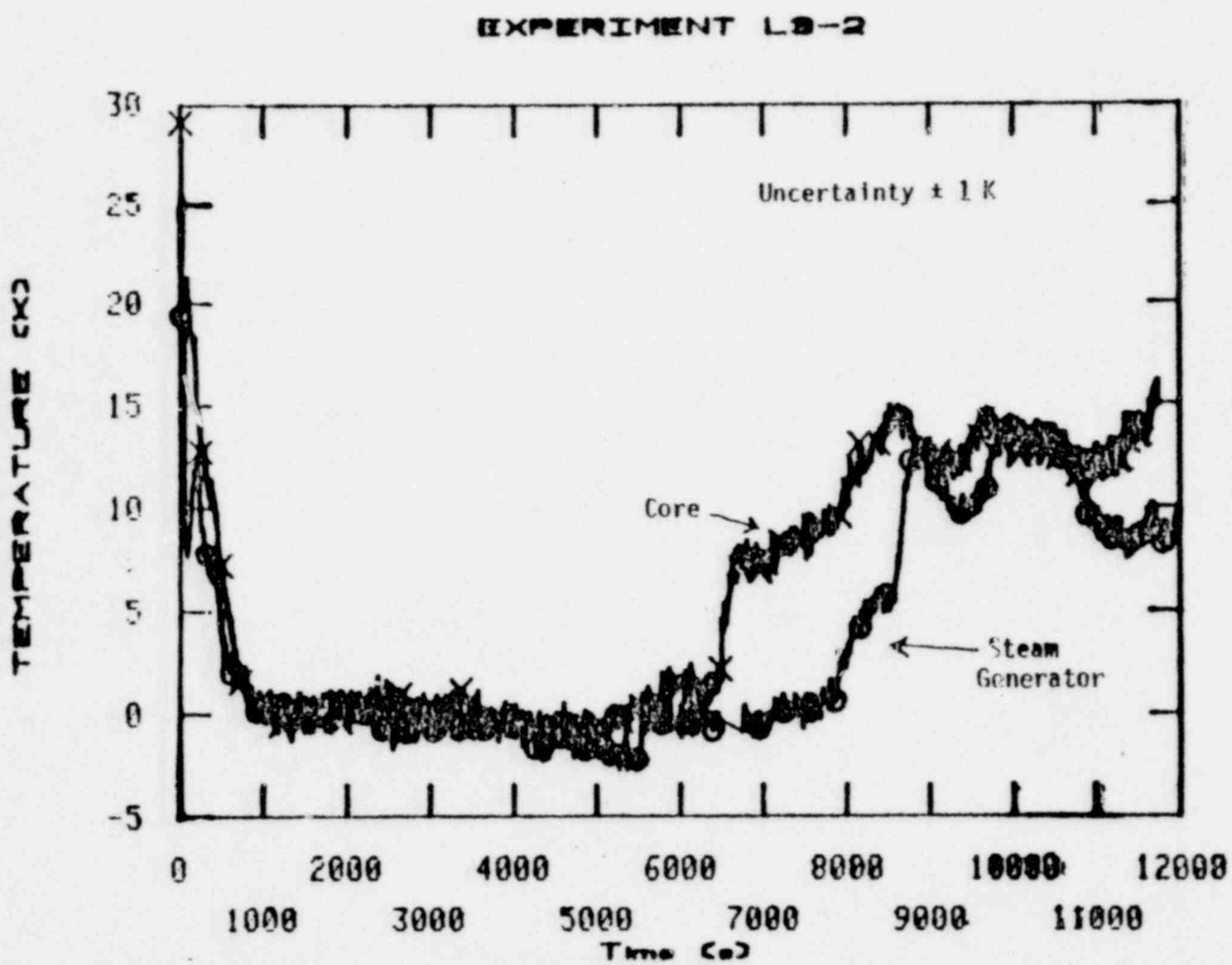
EXPERIMENT L8-2



CORE DELTA-T

COMPARISON OF CORE AND STEAM GENERATOR Δ T

IN THE PRIMARY SYSTEM

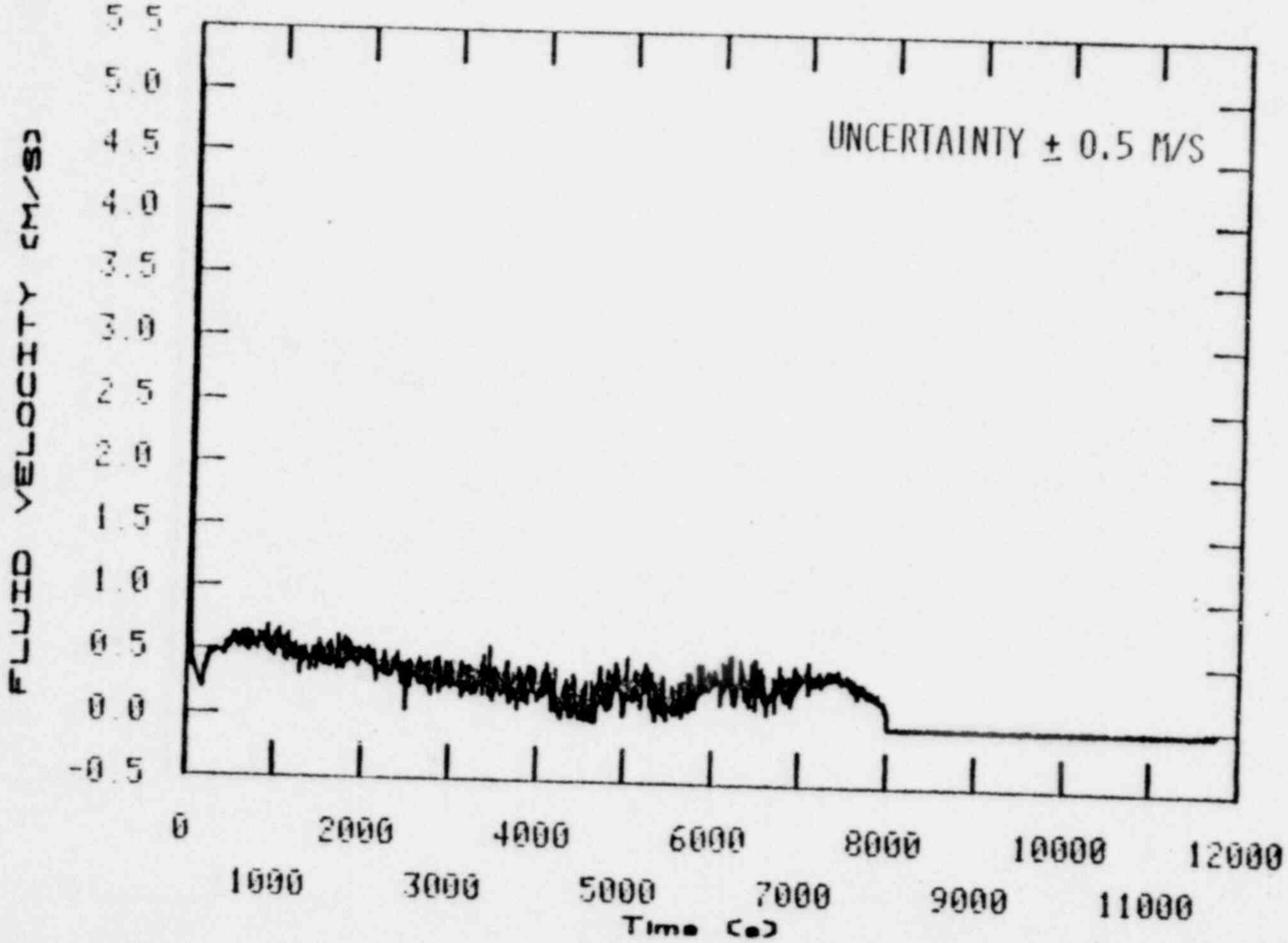


O-STEAM GENERATOR DELTA-T

X-CORE DELTA-T

TURBINE METER FLUID VELOCITY ABOVE CENTER FUEL MODULE

EXPERIMENT L9-2



UPPER PLENUM FLUID EXIT VELOCITY

L3-2 CONCLUSIONS (CONTINUED)

- SECONDARY FEED-AND-BLEED WAS EFFECTIVE IN LOWERING PRIMARY SYSTEM PRESSURE

- MORE SUBSTANTIAL EVIDENCE OF REFLUX NEEDED

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LOFT

L3-2 CONCLUSIONS (CONTINUED)

- MODE TRANSITIONS WERE GRADUAL AND APPEARED STABLE
- EXACT TRANSITION TIME BETWEEN REFLUX AND TWO-PHASE NC WAS NOT MEASURED
- NC WAS SUSTAINED THROUGHOUT TRANSIENT, EXCEPT POSSIBLY FOR A 200 S PERIOD
- HEAT TRANSFER, DURING NC, EFFECTIVE COOLING MODE

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LOFT

L3-2 CONCLUSIONS

- SINGLE-PHASE NC FULLY ESTABLISHED BEFORE TRANSITION TO TWO PHASE
- THE TWO-PHASE NC MODE TRANSITIONED TO A MORE DOMINANT REFLUXING MODE BY 2400 S
- THE SEQUENCE REVERSED AND SINGLE-PHASE NC WAS REESTABLISHED BY 8500 S. (BEFORE THE PLANT WENT "SOLID")

EG&G Idaho

LOFT

PLANS

TEST ID	CHARACTERISTICS
L3-7	0.16% BREAK, COLD LEG
	TURN OFF HPIS
	BREAK ISOLATION
L3-4	SMALL BREAK, PORV

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LOFT

PLANS (CONTINUED)

TEST ID	CHARACTERISTICS
L3-3	0.16% BREAK INITIATE FROM LOSS-OF-FEED WATER PRIMARY FEED-AND-BLEED

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SUMMARY CONCLUSIONS

- EVIDENCE OF ALL THREE NC MODES
- TRANSITIONS APPEAR STABLE AND REVERSIBLE
- MODES OVERLAP DURING TRANSITION - ONE DOMINATE MODE PHASING OUT AS ANOTHER PHASES IN

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SUMMARY CONCLUSIONS (CONTINUED)

- STEAM GENERATOR HEAT TRANSFER EFFECTIVE DURING ALL THREE MODES
- SECONDARY FEED-AND-BLEED EFFECTIVE
- FUTURE PLANNED EXPERIMENTS WILL PROVIDE MORE INFORMATION ON NC AND PRIMARY FEED-AND-BLEED

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