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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

EMERGENCY CORE COOLING SYSTEMS

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Holiday Inn
Park Center Plaza
282 Almaden
San Jose, California

Thursday, December 2, 1982

The meeting on Emergency Core Cooling Systems by the Advisory Committee on Reactor Safeguards was convened at 8:30 a.m.

PRESENT FOR THE ACRS:

M. PLESSET, Chairman
Z. ZUDANS, Member
T. THEOFANOUS, Member
V. SHROCK, Member
D. WARD, Member
I. CATTON, Member
J. EBERSOLE, Member
C. TIEN, Member

DESIGNATED FEDERAL EMPLOYEE:

P. BOEHNERT

ALSO PRESENT:

Present for the Industry:
Mr. Sherwood
Mr. Quirk
Mr. Wood
Mr. Sozzi
Dr. Andersen
Dr. Dix
Mr. Potts
Dr. Shiralkar
Mr. Dennison
Mr. Creddick
Mr. Knight

P R O C E E D I N G S

DR. PLESSET: Let's get started. This is Dr. Ward on my left. We have an attendance consultant, Dr. Catton, Mr. Shrock, Mr. Theofanous, Dr. Zudans and Dr. Tien, I think will be here shortly being held up by the traffic and the weather.

The purpose of the meeting today is to discuss with General Electric their Safer, Gestr, ECCS code and the status of proposed revisions to Appendix K of 10CFR50.46. The Committee will also discuss the pros and cons and use of electric versus nuclear heater rod simulators in LOCA tests.

The 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 to my right is the designated federal employee for the meeting.

The rules for participation in today's meeting have been announced as part of their notice of this meeting, previously published in the Federal Register on November 17, 1982.

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

We will receive no written statements from members of the public. We will receive no requests for time to make oral statements from members of the public.

Now, I want to make a comment before we start. As you know, there is a very important test facility here in San Jose at G.E., the FIST facility. I understand we can see that this afternoon at the termination of this meeting which will be about 5 o'clock, but I think we need to know who would like to see this facility now. So everybody up here at the table wants to go. Could you give us an idea of how long it would take before we get back here? If we go out to the plant?

MR. QUIRK: The tour itself will take approximately 30 minutes and I would guess to and from, it's about 30 minutes so all totaled you can do it in about an hour, maybe a little more.

DR. PLESSET: Okay, that's very good. Well, thanks, I'm sure it will be very worthwhile. We have been looking forward to the results in the facility and I'm sure it will be very worthwhile to see it. So, it looks like you've got a good turn out for that. I have no further subsidiary remarks like that to make except I think that the subject of the meeting is an important one and one that we all have to face and that is, what are we going to do about Appendix K and sometime ago, I guess about three or four years ago, the

Staff, the NRC Staff proposed some revisions in Appendix K. At that time, the ACRS indicated they weren't interested in any piecemeal approach but since that last discussion, there have been a lot of developments, our knowledge and experience and the test work has been going on has helped us a great deal to understand how conservative Appendix K is and we have a much better understanding of LOCA, particularly the large LOCA and also the small LOCA so that we should take advantage of this sometime and make an orderly approach to the problem of handling the design basis accident, so-called. I think we all agree that there are many features of it which are unrealistic and evaluation models so-called are terribly conservative and some of the features of it I believe do not necessarily mean conservatisms as far as safety and protecting the public health, so that it's a timely thing that I think should be pursued. I think that not only is it in connection with the Decay Heat ANS plus twenty which we now know is excessive, but other features of the requirements are now known to be not correct and non-physical.

I would think that the whole approach, the whole question of Appendix K could be approached in a much more realistic way and still guarantee a conservative approach to operation and licensing of plants. I think there's an important gain from this if we do it. If we can improve fuel utilization, that in itself is of great value and that would profit the public and everybody else and I think that both G.E. and Westinghouse also, as you may know are proposing and thinking along these lines. Now, I don't want to indicate prematurely that what G.E. is proposing today is what we would like but we should give it considerable attention and I think it's very worthwhile to have this meeting at this time. I'd like to ask if any of the other -- we have a lot of distinguished people here, if they want to make some comments.

DR. CATTON: I'd like to make just one comment.

DR. PLESSET: Dr. Catton is going to make a comment.

DR. CATTON: I think that you're absolutely right the Appendix K modifications. In my view, I really think that all other things should be pursued before Decay Heat because Decay Heat really is the forcing function for the problem and I would hate to tamper with it before I was sure of what I was pushing.

DR. PLESSET: Anybody else want to -- well, it's easy to see, Ivan, why one is tempted to start with Decay Heat.

DR. CATTON: No question.

DR. PLESSET: It's a simple thing that stands out there and everybody's aware of it and it's obvious but I think you have a very good point. I tried to indicate something along that line, that we should look at the whole picture and take advantage of the knowledge that has been gained in other areas which is terribly important and very useful to have built into our consideration of the behavior of nuclear plants and transients of this kind. Virgil?

MR. SHROCK: Could I comment on -- I don't exactly understand -- should I?

DR. PLESSET: Yes, go ahead. Pick it up.

MR. SHROCK: Maybe I should ask Ivan what he means by tampering with it but I guess I have a somewhat different view and that is that the technologies that exist for Decay Heat evaluation today is so far superior to the basis for Appendix K -- that its amazing to me that its taking this long to do something with it in the regulatory process. We had discussion on this at the meeting, at the ANS meeting with some representatives from the Staff and I'm personally very disappointed in the attitude that I find there which is basically, let's see if we can't find a lot of reasons why we should not use it, now that we've developed this better technology, let's look for reasons why we should excuse ourselves from applying it. It seems to me that the regulatory process should be attempting at every stage along the way to use the best available technology and I don't see why we should be motivated to avoid that spirit.

DR. CATTON: Maybe I should clarify that. I agree with you, but on the other hand you don't -- what I don't want to see them do is to give away the margin that we know is there where we know it exists until you quantify it elsewhere. So use the new standard but add 20% to it or some reasonable margin to it.

DR. PLESSET: Well, you see that this is not necessarily a straight forward thing. I can understand Professor Shrock's point. It's just not a sound procedure to ignore what is known or to distort it in an unreasonable way. Dr. Catton also is voicing a sentiment that one hears quite a bit. We've got to be conservative. But I think that being conservative when you are doing something that isn't right is not necessarily a good thing. I can understand that view point though. They want to be sure that we have lot s of margin. Well, in regard to large LOCA I think everybody would agree that we have about a thousand degress Fahrenheit as margin right now. That seems to be rather a significant margin, right?

DR. CATTON: That's right.

DR. PLESSET: Okay, but I think that there are a lot of other features that we haven't touched on in considering the changing in Appendix K. There's a terrible amount of trouble involved in licensing and in operating that could be relieved if we had a more scientifically sound basis for evaluating the performance of these reactors and transients. I think that's a terribly useful thing to keep in mind, that one can improve the behavior of these plants, load following (ph), fuel utilization and these are terribly useful things. They are every day things, really and if Appendix K interferes with an effective development of procedures, then it should be changed and I think we know enough to do it without causing any trepidation among those who say we must be conservative. Well, anyway, anybody else want to make any comments, Jesse?

DR. EBERSOLE: Well, I'm a little bit of an out -- this is not my bag, you know, but I'll make comment anyway. I've been impressed by the intended program of the Germans which is to virtually outlaw the large LOCA and I think that whatever we do here should keep, put the LOCA in a perspective that it was not originally in. It represents only a small part of the reactor safety problem, a very small part and we're really working on the fine structure of that part and we may find some day that the preservation of Appendix K logic is really greatly inhibiting the potential of these machines.

DR. PLESSET: Very good. I think that's a very good point. You're not as much of an outsider as you pretend, sometimes. I think that we've gotten carried away with the large LOCA. I think it was originally put in this way because a lot of people thought that we could never handle it.

DR. EBERSOLE: Yes.

DR. PLESSET: That's such an incorrect way to try to design safety systems. I think that the Germans have said well, this just isn't important. I think the Japanese feel likewise and I think a lot of people in this country agree with you that it's just one of those things that's a distraction.

Now, I think the small LOCA's are a different category and it's not clear at all if Appendix K is very helpful there at all. It may not even be conservative sometimes.

Maybe we should let -- unless there are some more comments from up here, let General Electric proceed with their presentation and I think I'll call on Glen Sherwood who we are glad to see here to do that for us. Would you lead it off, Glen?

MR. SHERWOOD: Thank you. My name is Glen Sherwood. I'm the manager of Safety and Licensing for General Electric and on the behalf of the General Electric Company I'd like to welcome the the ACRS, ECCS Subcommittee to San Jose. We are pleased that you have come to San Jose to discuss the subject which I know you recognize we feel is fairly important to us, namely with the backdrop of the experience which we have had in testing, and model development over the last ten years, we feel that we now have sufficient evidence to request changes in Appendix K and also changes into the licensing evaluation models to give us more operating margin on our BWR plants. We want this operating margin for two reasons. One, which the Subcommittee has already cited because the current limitations are inhibiting the full potential of the plant not only for operation, normal operating in terms of the daily plant operating modes but also the long term utilization of fuel in terms of going from twelve to 18 month cycles which I know you know that both General Electric as well as the other vendors are looking at. But in addition, the limitations which we have on ECCS where most of our operating plants are bumping along at 20° to 100° requires a tremendous amount of effort on our part in terms of reanalyzing each plant at each reload and also change the chasing problems that tend to come up with the Staff when they see a new, sometimes esoteric issue come up in ECCS which requires that we drop everything and look at that problem for anywhere from two to six months. So with the back drop as Prof. Plesset said of a thousand degrees from our two loop test apparatus results, we feel that it is now time to relook at the ECCS situation and make available that operating margin which is really needed for the operating

BWR's. We plan over today and tomorrow to provide a detailed review of the G.E. strategy as well as our technology both from the point of view of testing, as well as our new models. Some of this the Subcommitte has heard last year in Monterey and so we will be repeating some of that for some of the members. However, we do plan to go through in detail in an exhaustive way all of our efforts in the ECCS area in the next day and a half so I will without further introduction turn the presentation over to first, Mr. Joseph Quirk who will discuss our activities with the NRC in terms of proposed changes to the ECCS evaluation models for the BWR and also the Decay Heat and also Mr. Ed Wood who will discuss in detail the ECCS strategy on the part of General Electric. Joe?

(Pause)

MR. QUIRK: Good morning. Joe Quirk.

Good morning. My name is Joe Quirk from General Electric Company. I'm manager of BWR's Systems Licensing.

(Slide)

I have a number of introductory charts that I'd like to kind of set the stage with. The first being the agenda for the two day meeting. As you'll notice, the agenda is slightly different from that handed out by Dr. Plesset before the meeting. It's important to note that all the topics on your agenda, Dr. Plesset, are covered in some form or another. The order of this agenda that we've chosen is slightly different. And the first day, we'll begin with a kind of recalibrating, if you will, the GE ECCS approach. And Mr. Ed Wood will conduct that presentation. We'll then go into an overview of the BWR LOCA technology by Dr. Gary Dix. After a break, we'll pick up and discuss the TRAC model description. We'll follow that after lunch with the GESTR model description and with the SAFER Model description. So on the first day as you see here, it's kind of setting the stage with the overtone of a G.E. ECCS approach followed by a description of the ECCS models.

On the second day, then, we will begin to talk about some of the qualification and evaluation results of these models. I think there's a full two days here and a lot of information. Some of it, a lot of it the Subcommittee has not had the opportunity to see yet. Thus, the purpose of today's meeting is really four fold. We would like to update you on the technical description and the details of our SAFER, GESTR and TRAC models.

(Slide)

We would follow that up with quantifying some of the results. We will also describe our ECCS evaluation methodology and as we -- as already mentioned today, we will give you a status on our Decay Heat exemption submittal.

(Slide)

What I'd like to do is update you from the last time that we have met with you and that was roughly in August of 1981 as shown here where there was an overall ECCS approach presentation given to the ACRS Subcommitte. Since that time, we have submitted the SAFER, GESTR model to the NRC in December. We've also submitted a GESSAR II Decay Heat Submittal at the same time in December of 1981. We followed that up with a meeting with the NRC Staff on the SAFER/GESTR application in January and in June of 1982 we presented the Decay Heat Exemption details to the ACRS subcommittee.

In August of 1982 we presented the SAFER Qualification Results to the NRC, to the meeting in Bethesda. This kind of brings us up to date of activities that have happened since we last met with you.

Activities that are planned in the immediate future include a meeting with the NRC in January to review the application of the SAFER Results and we forecast and look forward to wrapping up and getting approval of our SAFER/GESTR approach in the first quarter of 1983. This was meant to kind of reload the computer banks, if you will, since we last met with you and at this time I'd like to turn themeeting over to Ed Wood who will summarize the G.E. ECCS approach.

(Pause)

MR. WOOD: Good morning. My name is Ed Wood. I'm manager of Core Development of the Nuclear Fuel Engineering Department and I want to talk a few minutes about the analysis approach that we are currently pursuing and I think most of this is a review for you so we probably won't have to dwell too long on it.

(Slide)

I'd like to cover some, what is pure review, go back in the BWR system, our current evaluation model, a little bit as some of what we see as the key issues and then talk for a few minutes about our objectives in our license analysis and then the technical bases or approach that we would take in the new evaluation model, namely the SAFER computer program.

(Slide)

And this is in the category of a review. The purpose I think of pointing this is out is to point out some of the features and remind ourselves of some of the features of the Boiling Water Reactor, specific power we run into 25 to 28 kilowatts per kilogram of uranium. There is complete natural circulation. In fact, it has the capability of 50% power with all reserve pumps off, a dual core spray system in the upper plenum above the core, a coolant injection system either through the recirculation loops as in the case of the BWR 3 and 4, are directly into the bypass region of the core in the case of the BWR 5 and 6 and then with a refloodable core by the use of internal jet pumps such that any kind of major break, the level of drainage would be at the top of the jet pump which assures a refloodable situation in the core. We moved to this design in the 60's and part of the motivation for going there and to making this evolution was because of the issue of LOCA as well as several other things.

(Slide)

Let me talk just a few minutes and I'm not going to spend -- this is a busy chart -- I will not spend much time on it. We have discussed it before. I simply want to make a couple of points on it. This is a schematic representation or a block diagram representation of the current approved evaluation model and the point I think of the chart is that there are basically two bookkeeping systems, what I call the system model, what I call the node heat up model and a number of, a large number of modules that feed that. In our past approach, there has been a concerted effort, a conscious effort, if you will, to look at each one of the individual blocks and for those where there was uncertainty to try to bound each individual module in a conservative manner. Even though it may have meant physical inconsistencies, now, clearly this is in keeping with the intent we feel of Appendix K, the idea being to do a peak clad temperature calculation that truly represents a bounding value, not what one would expect but a bounding value. Let me just take one for instance, and then we'll move on because like I say, we've been through this in some detail before, but let's take for instance these two right here down at the bottom. There is a vaporization correlation in the model and from the output of that correlation we determined the steam flow that's coming up through the upper top plate of the core which in turn is the primary factor in effecting the liquid draining into the core due to the counter current flow limitation process and so there is a vaporization correlation which like I say determines the steam flow which in turn determines the amount of liquid that drain to the core.

On the other hand, there are heat transfer coefficients within the core that determine the heat removal from the core and the cooling process during the rebuilding process. In one case, we take a bounding value on the vaporization because the more you vaporize, the more you restrict the flow and the less fluid that you get in the core. On the other case, we take the other direction and say the lower the heat transfer in the core, the lower the heat removal and the higher the ultimate peak clad temperature will be. In fact, as we all know, these are not independent and so here is a -- I think a clear example of what happens when we take this approach, we do indeed meet the, I think, intent of Appendix K of calculating a bounding value. We violate, however, the conservation of energy. So, and that can be -- those kinds of examples can be repeated. One would ask, and rightly so, why would you ever move into a situation such as this? Why not -- we know what the, you know, some of the basic laws, conservation of energy, conservation of mass. Why not maintain them? Well, perhaps I can answer that a little bit by going back through some reviews as to what the status was at various times in the evolution of the process that takes us to where we are today.

(Slide)

In the 1970's, as I mentioned earlier, we had, our approach in the 60's leading up to 1970 was tc kill the ECCS issue with hardware design and so we did these things that I mentioned earlier. We included the jet pump, we included the core spray system, we included the low pressure cooling injection systems, the intent was to kill the issue with hardware. Having done that, we had some simple bounding models that calculated something like a 1500° peak clad temperature against a then limit of about 2700° and so we said, it's a reasonable approach to overdesign the system with hardware to make it truly safe and not spend much time doing the analysis and so that was the approach. And that went well for awhile. In 1975, due to a number of required changes to the modeling and the limit that was set on allowable, the margin had all disappeared. The key thing that happened here was the implementation of the 71 ANS Decay Heat plus 20% and the imposition or implementation of the counter current flow limitation process at the top of the core. Between the two of those, the calculated margin went down by some 800°, 600° to 800° and then with the limit coming down from 2700° to 2200°, all of the margin had disappeared and so now our evaluation model was calculating in the vicinity of 2200° against the limit of 2200° and there was not realistic technology in place at that time to justify a more sophsticated approach, and so it was a series of events and we, at G.E. and I feel sure at the NRC, too, kept a rather large team of engineering fire fighters to make sure as things -- new things were discovered, that somehow or another the small margin was real. We didn't have the large margin, and so we continued to look at that and assure ourselves that the real margin was still there.

Well, things have changed since then and I think the key thing here is that there just isn't enough technology available in 1975 to do the kind of modeling and demonstrate the qualification of those models to the extent necessary.

As I mentioned, there's been quite a few changes into the 1982 status. We are now in a situation where for the last couple of years -- there have been no plants derated because of ECCS issues. We have received some relief from the regulatory staff in terms of heat transfer coefficient in terms of a little different CCFL correlation, enough to get us out of the derate situation. However, as you have pointed out yourself in your opening remarks, Dr. Plesset, we still are in a situation where the fuel cycle economics, because of local limits are being penalized. A rule of thumb for the BWR with the current G.E. plan and G.E. fuel design and it's current operating situation is that for about every 3% of local margin, you have an impact or an opportunity if you will, of 1% gain in fuel cycle cost. Now, this is a highly non-linear function. One shouldn't extrapolate that to say 30% margin would give you 10% but about the operating point where we are today, that's about the sensitivity of local margin to fuel cycle costs. That happens, keeping the total core power and the average kilowatts per liter constant, not increasing the total power and I'm sure that we discussed this in the past and, but I thought I would mention again that factor.

I think the key things now that have happened is done in these areas here. The technology has advanced substantially since 1975. We now have a vastly improved, as you pointed out already, Decay Heat Model, through the efforts and resources and technical directions of the research side of the NRC, of General Electric and of the utilities themselves through EPRI, there's been a substantial investment in technology resources over the past seven to ten years, really, in the area of experimental and analytical model development and you're going to hear a great deal more detail on that. Dr. Dix will be talking in great detail about the experimental information that we've gained. Dr. Anderson, Dr. Shiralkar, will be talking about the analytical evolution as a result of that and this has been an industry cooperative effort and I think the results are very good. It has resulted in a best estimate system model, namely TRAC, BWR version which you will be hearing about in some detail later and at G.E., we have developed and submitted an improved evaluation model. Now, one of the first questions that might come up and we'll discuss this a little bit later but let me hit it right here is, if you have a best estimate system model, what if you use that as the evaluation model. That's the first question that should come to anyone's mind and it's come to our mind also. I think there's a practical consideration for that. The TRAC computer code is a very good detailed benchmark analysis program. However, it's drawback is that it runs several hours of central processor time on a CVC 7600 for a relatively short real time of transient analysis. In order to do the exploration required for the license and evaluation models, parametric studies in large numbers have to be done. You simply would use up all of our CVC computer and then some if you tried to do this with the best estimate model.

Some of the things that we look at in an individual plant is the parametric studies bearing the parameter of break location, of break size, of initial conditions, of the number of ECCS systems that are available to respond and you end up with a large number of cases, analytical cases that you have to do for each reactor to assure yourself that you have mapped the entire space that would be available, that you want to look at. So what we have chosen to do is to benchmark this license and evaluation model with the best estimate model on the key transients and events of consideration and we have focused on the so-called design basis accident which is the double-ended recircle end break and that will be the focus of the benchmark comparison between these two.

Then we then have a tool that is practical in it's efficiency in terms of computer time that permits us then to do the parametric studies that rightly should be done to evaluate all the possible combinations.

Well, this brings us to what I show here as a current challenge or opportunity. It's an opportunity because of the technology we now have the provides the opportunity to go do something that's better. It is a challenge because there are still -- some resources have to be expended if you go do it. One of the questions that we faced is, if there are no derates to the plant, why go through all the expense and effort on both the part of the vendor and the regulatory agencies of reviewing new models, of approving new models and redoing the analysis, and so it is a challenge because it uses resources and as you wisely pointed out this morning, their resources are in short supply and quite frankly, I get concerned sometimes that we're spending too much of our resources on this issue rather than some of the other broader aspects. So, it becomes a challenge but I think it's a challenge that we've got to take on and have got to resolve, and that challenge is andopportunity is to implement the new license evaluation model and to quantify what the real safety margin is and we will will -- those are the focuses of our two day session with you.

(Slide)

Let me set out here some of our objectives.

DR. WARD: May I ask a question at this point, Ed?

MR. WOOD: Yes.

DR. WARD: The TRAC BWR is a best estimate model?

MR. WOOD: Yes.

DR. WARD: And I understand what you're saying about benchmarking your own model against it, but you keep calling your own model an evaluation model. Why don't you treat your own model as a best estimate model in applying margins or whatever -- explicit margins against that rather than consider it as an EM.

MR. WOOD: Let me go back perhaps and define some terms. Maybe communication, and I think there are some very specific terms or definitions we think of when we think of these terms. Best estimate model has evolved to a definition that says you do the best possible calculation today's technology will permit you to do and I think clearly today that is the TRAC Model. If you -- our evaluation model is not that because in order to get the efficiency enhancement, we have taken advantage of some of the messages that the TRAC calculation and the experimental have told us about the requirements for 3-dimensionality versus 1-dimensionality and so we have shrunk down in areas where we can and gone to 1-dimensional calculations. However, by definition, I think a 1-dimensional calculation couldn't be called a best estimate. Now, that's a fine line on definition admittedly and so that's part of the issue.

DR. WARD: I guess I haven't -- it's probably that I don't understand it but I guess I've seen the best estimate model as a means as an attempt to calculate the mean or median value?

MR. WOOD: Yes.

DR. WARD: An EM model is an attempt to calculate a biased, conservatively biased model. Now, either one of those can be done with broad brush ropes or with fine brush ropes.

MR. WOOD: You're right, and absolutely, if I could get you to hold that until my next chart I think I will answer your question.

DR. WARD: Okay.

MR. WOOD: And if I don't answer it to your satisfaction, please raise it again but I believe I will. Yes, sir?

DR. CATTON: Just a comment. The TRAC model that is now at Los Alamos, runs as fast as you need to have it run. Somehow the numerical algorhythms are different between the TRAC BWR and the present version. It seems to me that's the reason it runs slow. So, you could conceivably change the numerical algorhythm and TRAC BWR and have your fast running code and a good best estimate altogether.

MR. WOOD: You've got a good point there and one of our joint plans with the NRC during the coming year, in fact, I guess a couple of years is to try to do just that, to try to make the BWR version of the TRAC more efficient and there is a rather concerted effort between G.E. and the folks at EG&G at INEL who are going to be concentrating on that, who are now concentrating on it under the sponsorship of the research side of NRC. It's a good point and we're trying to get there. We're not there yet, though.

DR. ZUDANS: Could I add to that? I think the difference really is not big enough for you to be greatly optimistic because a factor of 2 doesn't make much difference if you run one day or two days. It still is a long process. I think what Mr. Wood says, maybe he didn't communicate completely. You could use cruder models with the best estimate codes and do it faster, rather than using evaluation models which you have to adjust for a very specific situation because you can never let your evaluation model fit all the circumstances. You can polish it for one specific transient and it will do all right but for the others it won't do, so I think there is a concept that's something to be looked at.

MR. WOOD: Okay, like I say, let me walk into that in just a couple of minutes and try to go into it in some detail.

Let's go through the objectives if you will, that we laid out and by the way, this is the same chart that we used to discuss with the NRC Staff in January of this year, to what should be the objectives in laying out a revised or any kind of updated evaluation model and I think first of all, clearly we are interested in quantifying the safety margin and assuring that it still exists.

Another part though, that I think of extreme importance to us is that this evaluation model could also be the basis for operational and design decisions. As of today, our evaluation model is not appropriate for this objective right here, namely for, as a basis for a design decision.

DR. CATTON: What you're saying is it's a quasi best estimate model?

MR. WOOD: No, today is not even a quasi best estimate --

DR. CATTON: No, no, the new one.

MR. WOOD: Oh the new -- yes. Of course. Yes, but this one right here, for instance, let me give you a for instance and back up to my last chart, some comments I made.

Because of the bounding of each individual module and thereby doing some violation of the conservation laws, we indeed do come up with a bounding model, but there are two hazards, I think, in that. One hazard is that we, after we use it repetively, we might tend to forget that we have artificially built in the conservatisms and we begin to believe the numbers and then the second hazard in that is that we might tend to make future design decisions based on the outcome of that evaluation model. For instance, in the case that I just mentioned of the previous chart, since the vaporization correlation in the core causes a substantial amount of CCFL at the top of the core, not letting water get in and at the same time we underestimate the amount of heat transfer that is in the core during this refueling process, the evaluation model as it now stands would calculate a lower peak clad temperature if you took all of the ECCS water out of the upper plenum and injected it at the low plenum and you'll calculate a lower peak clad temperature. But I do not believe that you would enhance the safety of the plant and so, that's the hazard, I think of having evaluation models that are not self-consistent because there is great temptation to provide an operating margin to a plant with approved evaluation models by making a relatively simple design change, but that design change would not enhance the safety of the plant. It might even degrade it. So I think that's one of our real concerns and any future evaluation model should correct that discrepancy in our thought process.

I think clearly we would like to have an evaluation model that permits an efficient use of the regulatory and industry resources and this is another way of saying, have a model that has a realistic representation of what's happening so we can focus on the real issues. We'll know what the real issue is on and we can focus our resources on those rather than something else.

DR. SHROCK: May I ask a question?

MR. WOOD: Yes, sir.

DR. SHROCK: I just wanted to clarify to be sure that I heard what I think I heard. You're saying future evaluation models should be required to conserve energy?

MR. WOOD: Yes.

DR. SHROCK: Notice that was not "will". "Should be".

MR. WOOD: Yes. But let me say again, that the approach we took in the 60's, early 70's, resulted in a process completely compatible with Appendix K, because it was, it was for a licensing calculation of this event and it was to bound the value, not to say what the value would be. It also was at a time when the technology was somewhat lacking in being able to understand all the phenomena that one needed to model and so I don't want to be too hard on a lot of us who went down this path. At the time, I think we were doing the best we could with what we had. But times have changed. We now have more and we should change.

(Slide)

Our basis then is going to be very simple and straightforward and you will be hearing the technical basis to this in some detail in a few minutes, but I think the several comments that I've heard physically consistent conservation models should be a requirement. I believe that was almost what it was said a few minutes ago. And the answer is yes, that is, we have formulated them to try to, you know, within the ability of the uncertainty of our computerized calculation to make that happen.

We also should use expected value on the input correlations and I think this is very important because this is a highly non-linear event. If you input different correlations such as Decay Heat, you can change the sensitivity of the behavior of the plant to a lot of other parameters. It's not a linear process. It's highly non-linear with a number of things -- power level with what the ultimate peak clad temperature, what the reflooding time is and if one continues, even with physically consistent conservation models, if one continued to upper bound all of the empiracally based correlations such as heat transfer correlations, such as Decay Heat correlation, such as void (ph) quality (ph) correlation, one could still move the resulting calculations into a regime where the sequence of phenomena calculated might not represent that which you expected to happen. And so, I think it is important to input expected value and of course, the combination of these things says, do a realistic calculation and then one should look at the uncertainties in the calculational, experimental process and compare that with the calculated margin to make sure there is adequate margin to cover those uncertainties. And so, this is the approach that we have taken on the SAFER/GESTR modeling and today we will be carrying you through the models and what the expected value calculations are. We are still working in this area down here as to what the uncertainties should be, the magnitude of the peak clad temperature uncertainties to cover all the uncertainties between the modeling and the experimental data and the qualification.

DR. SHROCK: Could I interrupt you for a moment?

MR. WOOD: Yes, sir.

DR. SHROCK: In connection with the decay heat, I don't regard that as a correlation, clearly in the same sense as heat transfer correlation or correlations of experimental data.

MR. WOOD: That's right.

DR. SHROCK: As Ivan pointed out earlier, it indeed is the forcing function.

MR. WOOD: That's right.

DR. SHROCK: We need to distinguish the forcing function on the problem from the component phenomena such as heat transfer and fluid mechanics correlations.

But now you pointed out that there is sensitivity to the uncertainties or the inaccuracies that you introduced into the calculation to deliberately selected conservative correlations. Now, I've had some difficulty with the presentation you made last June in Idaho Falls on exactly that ground. What you've done with the decay heat evaluation is to remold it into a conservative decay heat curve which goes back then to the older concept that we can define a decay heat curve and apply that in all instances as essentially an upper bound on our forcing function. I think that got you into difficulty previously. If you do it again, it's going to get you into difficulty again. What I read in that report is, here is a conservative evaluation of a decay heat forcing function which we propose to use in our evaluation model and I find that in conflict with the description that you just gave us.

MR. WOOD: And I must confess that we probably still have a lot to learn and decide as to what the trade-off should be on the number of situations we analyzed versus the fidelity which we hold to realism and --

DR. SHROCK: We had difficulty in that meeting understanding exactly what G.E. was asking NRC to approve and for me at least it would be helpful if this brief presentation that's going to be made tomorrow could be preceded by some documentation that would update what it is that G.E. has requested. I'm still not clear that certainly in my own mind, this was not well-defined. I wrote that in my report to the ACRS and I think that it remains in that status. So, if there is an update on it I'd like to know what it is so I can look at it before that meeting tomorrow.

DR. PLESSET: Did you get Professor Shrock's report on that meeting? It was circulated to the NRC.

MR. SHERWOOD: I don't think we did.

DR. PLESSET: Well, we ought to send it to them. The Staff didn't make it available to them.

MR. WOOD: Some of the issues and I will check --

DR. PLESSET: Well, we'll get it to you anyway.

MR. WOOD: Okay, good. Thank you.

Some of the issues that we're looking at in terms of the application of the Decay Heat and I won't pre-empt too much of that and maybe we'll discuss it in some more detail tomorrow, is for instance bundle type dependence. And we've got a large number -- a relatively large number of so-called standard bundles that are slight differences, one bundle to another in terms of average enrichment or local peaking factors that are tailored to specific plants to assure specific cycle energy. One of the questions we ask ourselves is should there be a different calculated decay heat ratio -- I mean, a decay heat model or output for each one of those and we're looking at -- we're concluding that small changes don't make enough difference to make it worthwhile to try to analyze each bundle type within a reactor that you can can kind of come up with a generic bundle type without adding much conservatism at all. Just take the worse one, because the water to fuel ratio and the plutonium conversion ratio changes slightly as you know with a slight change in the nuclear design, but those we concluded were small and so therefore, it made a lot of sense to just take the worst one because we're in fractions of percent. Now, that's one example and then you have to go look at a whole spectrum of other examples and it becomes a judgement call. "This one is big enough to treat separately." I assume that's the issue that you're wanting to raise is, how do you make that judgement call and when do you start using "generic" calculations versus "specific" calculations. Okay. I think I understand that's what it is you're after.

Well, this concludes what I had intended to discuss. I'll say again that where we are to date, we have completed our model, our benchmarking, our evaluation model calculations in some cases so we can give you some specific numbers and you can see some time, temperature results. We are still working in this area doing uncertainty analysis, sensitivity analysis perturbing various parameters to see what the effects are to make sure that we've got the right kind of coverage and margin to cover the uncertainties that one could reasonably assess to be included here.

With that, if there's no further questions, I'll turn it to --

DR. WARD: I'd like to go back just -- I'm kind of slow, maybe at understanding but what you're saying as I understand it now is that what you're calling the proposed evaluation model, the SAFER/GESTR is with the exception apparently of the decay heat curve what I might call a best estimate model. It attempts to calculate the center of a probable distribution.

MR. WOOD: Yes. Our intent was to do that. Now, it's a simplified model compared to TRAC, both in dimensionality and all of the details and so it doesn't fit the classic definition of best estimate but the intent is to calculate your best, I would say, estimate -- the word fits, of what the real number would be. Yes.

DR. WARD: One other question. I guess another approach that you know, you've benchmarked the SAFER evaluation against the TRAC BWR, whatever it is. Another approach would be to define a generic core if that were possible and make the parametric calculations using TRAC BWR with that and then allow for core differences. Is that impractical or that's apparently less desirable for some reason.

MR. WOOD: Well, the reason it's less desirable is that if you're -- like I say, if you're looking at a break spectrum to -- you know, a small value all the way up to a large one -- we run a large number of cases there. And if you're then, like I say coupling that with the location of the break, to do a complete parametric study on one plant is very undesirable. Now, we are however looking at the limiting events, you know. Our best estimate of what the limiting events are and that we will look at with TRAC. And then we will use the SAFER code then to fill in all of the other places just to make sure that we have indeed done the TRAC calculations in the area of where the limiting events are.

DR. PLESSET: Well, let me ask him a question first, Ivan. Just to make it clear -- is your code a 1-dimensional code?

MR. WOOD: It is a 1-dimensional code.

DR. PLESSET: And do you fulfill all the conservation laws?

MR. WOOD: Yes.

DR. PLESSET: Well, the 1-dimensionality is of interest you know. There's a 1-dimensional code for PWR, that's RELAP-5. Did you consider trying to adapt to your needs? Is that an unfortunate question?

MR. WOOD: I would like to let Dr. Shiralkar handle that in his description of SAFER or he can handle it now.

DR. PLESSET: All right, no, no, if he's going to do it later, he can tell us why he didn't think of well, let's take RELAP-5.

MR. WOOD: We did consider RELAP-5.

DR. PLESSET: You did.

MR. WOOD: Yes.

DR. SHROCK: Is your response that it is one dimensional applied to the core or is that channels? Are you working it as parallel one dimensional problems or as a single one dimensional flow for the entire core?

DR. PLESSET: All right, let's let it go then. We'll let it go for now, but you can see we're interested.

MR. WOOD: Yes. I expected that you would be.

DR. PLESSET: Ivan, did you have a comment?

DR. CATTON: With respect to engineering law, sometimes the better -- sometimes engineering models are better than detailed models where all you've done is use the uncertainties at a microscopic level.

MR. WOOD: That's true.

DR. CATTON: So I really wouldn't downgrade the

SAFER kind of model relative to another.

MR. WOOD: Okay. Clearly, our intent here was to develop the best model we could within constraints that resulted in a practical running time and the ability to use it in the design process as a production tool.

DR. CATTON: There's more to it than that. You are actually developing a model at the level of your doing the experiment.

MR. WOOD: Yes, yes.

DR. CATTON: You're not developing a model where you have to go out and run a bunch of other experiments or look for data that doesn't exist and I think that's --

MR. WOOD: That's true.

DR. TIEN: I'd like to make some comments. In relation to, also I would just mention that I think the engineering model are detailed models. It's really a part -- first of your input information and certainties and then the final output, sensitivities margin is wrong (ph). Now when you are developing more and more sophisticated code and also larger and larger like TRAC and so on, I think it's much more important to trace also the uncertainty propagations you know, from different components, different correlations --- another one is wrong (ph) and so it would not get completely loss and also the second point is, in terms of your input and certainties you must weigh certain kind of say, probabilities or some expectations there, otherwise you just use the upper and lower bound. You actually probably propagate that into a very unreasonable you know, degree, and I think it is very important in a large detailed model, you have some kind of at least built in systems so that you can keep track in checking some of the, both uncertainty propagations and final sensitivities.

MR. WOOD: And your point is well taken on the probability of the uncertainty, the various elements of the uncertainties and we have attempted to look at that in terms of our input to try to maintain some balance on what the probability of an input variation and what it's impact on the calculated results are and you're absolutely right. If you ignore -- if you simply perturb input values without regard to the probability of them being that far off, you can lead yourself into an area where again you lose some confidence in your ability to know what the real uncertainty is.

DR. TIEN: I really feel a large code, sophisticated code -- perhaps you should have also some built in relatively approximate, you know, like what, I have just mentioned. Also, so that you can have some kind of comparison. In fact, just like sometimes the engineering model because they have some beauty in this microscopic impactions (ph) and that's, you know, averaged out and actually give you much better -- so if you're having codes which can somehow do something like that and make some internal comparisons which will really serve a lot of good purpose.

MR. WOOD: Yes, one of the things, by the way, in keeping with this line of thought, that we are in the process of doing, have not completed yet, is to looking at the details in the calculated output of the TRAC model versus this engineering or evaluation model and in using then the judgement and the experience of the engineers who have seen the experiments and who understand the phenomena in trying to understand what these differences are and we're in the process of doing much of that right now.

DR. PLESSET: Well, thank you. You can see that there has been a lot of very stimulating thoughts to your presentation. I don't want you to forget what Mr. Ebersole mentioned. A lot of this is in an unrealistic world and we've got to keep that in mind and what we really are maybe going to want eventually is some simple fast methods of analysis which can be built into operator procedures.

MR. WOOD: Yes.

DR. PLESSET: Okay. Well, with that little comment, maybe we can go on.

MR. WOOD: Okay, very good. I'll turn it over to Dr. Dix now who is going to move into the area of looking at some of the results of our experimental data that has come primarily through the joint NRC/G.E. EPRI programs, but also some, quite a bit of the other data.

(Pause)

DR. DIX: Good morning.

(Slide)

DR. DIX: My name is Gary Dix and I am manager of Core Methods in the Nuclear Fuel engineering department. This morning what I'm going to try to do is take about an hour and see if I can capsule for you about ten years of experimental technology development in BWR safety. Now that's going to be a fairly broad brush but I think if I concentrate and just focus on the highlights and not carry you through the ten years but tell you what did we really learn, I think I can accomplish that objective here this morning and give you a good feeling for the experimental background that we have to support the model developments that you'll be hearing about the rest of these two days.

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First I'd like to start off by just characterizing what some of the big experiments that we have for the Boiling Water Reactor are and some of these I'm sure you'll be very familiar with and others perhaps not but I thought I would just go through and give a very brief description of what these are as an initiation point and I'll come back and actually show you some characteristics of some of these subsequently.

One of the main real workhorses that we've had around for a number of years and in fact it's now been replaced is the facility we call the Two Loop Test Apparatus and this facility has been operating actually in various modes. We set it up initially to be a system response facility, a one-dimensional facility where we took advantage of the feature of the BWR that we have channels in the core and therefore each fuel bundle is isolated and operates, communicates only with the plenum at the top and the bottom and thereby that allows us to do some pretty good one dimensional tests of these features by putting in a full scale channel and then if we can simulate by having the rest of the system wrapped around it in a scaled fashion -- if we could simulate the input and output conditions on that channel, then we can get realistic heat transfer performance and flow conditions within the fuel channel, so the Two Loop Test Apparatus was really the first facility that we had that went that direction. We scaled all of the remaining reactor system down so that we could drive this one single channel in a real, real time response.

Now, that facility as I said, has been a workhorse for a number of years. We dismantled that facility and we're just now putting in an upgrade at that facility which we call the full integral simulation test and what we've done is stretched out the two loop test apparatus. One of the compromises that we had in that facility was, it was scaled in volume but not necessarily in vertical height.

With the emphasis following the TMI incident, the greater emphasis now on small breaks and other transients, there are much more of those transient considerations which require gravity driven heads be accurate and therefore you must have full height in order to get a complete realistic simulation of those transients. So basically, the full integral simulation test or FIST as we refer to it is a stretch out and getting rid of those vertical scaling compromises. Of course, since we were putting together a new facility there were several other compromises, the Two Loop Test Apparatus was experimental technology of about 8 or 10 years ago. We have since developed a lot of techniques now for improving how we bring the power in so we can get more realistic simulation at the very top and the bottom of the bundle. We also put in realistic fuel channels, got better heat transfers, so there are a number of rather subtle but general improvements in the simulation. The key one though is it's stretched out and will allow us when we're running these tests which will occur over the next couple of years to confirm our prediction capabilities now with a new facility and with a little more realistic reactor simulation.

Another one you are probably aware of is the Steam Sector Test Facility. This, in the BWR as I said, we have capabilities to actually get realistic fuel channel simulation with one bundle because of the channels that exist in the reactor. But what you miss in these one dimensional facilities is any kind of interaction between the channels and in particular you miss any three dimensional effects. We do things like the coolant injection spargers, the spray spargers are injecting liquids around the perimeter of this large vessel so you would expect to get some significant amount of radial variation in conditions if you had an emergency cooling system coming on, so we developed a very large scale facility and in fact it's placed at a General Electric facility in Lynn, Massachusetts where they had some extra steam coming off a power plant. It's of that magnitude. We have in this facility a 30° pie shaped sector that would be cut out of the Boiling Water Reactor. Actually, it's one of our later -- BWR-6, what we call a 218 plant, a 30° sector of that includes the fuel channels and all of the remaining facilities. The plenum region above the fuel channels, our separator has a lower plenum below it and a simulation of the jet pump so it's a rather complete simulation of a 30° sector. That includes 58 fuel channels or partial fuel channels in this pie shaped sector so we get a lot of opportunity there to look at not only the full radial dimension, what kind of radial effects we might have in the upper plenum, but also a number of fuel channels that can interact.

MR. EBERSOLE: May I ask a question?

DR. DIX: Sure.

MR. EBERSOLE: How do you expect the pie shape sector to get a microscopic picture of the flow distribution brushed and spray when you really don't single out (ph) the circular cross-section? Is the simple reason might be the hottest you have, the greatest --- in the center? It seems like the pie shaped section would automatically give you inaccurate results because you're not synthesizing a circular cross-section.

DR. DIX: I will be covering that a little bit more but let me give you a very brief response on that. With respect to spray distribution, you cannot get a full spray distribution in a pie shaped sector. That's very true. The central region does not have the interaction from adjacent sprays that would be coming out that are missing or from the sprays coming across. With respect to spray distribution therefore, we did not use the 30°

sector as our primary experimental basis for evaluation in development of spray distribution. We used the 30° sector in fact only for confirmation that we knew how to go from an air facility which had the full 360° spray distribution, from an air environment to a steam environment, so we simply for that particular feature used this to check out the analytical models and we set the model up for the 30° sector and then ran the test for the 30° sector.

The primary pay off of the 30° sector was to look at multiple channel interactions on an overall system response and also to look at what happened in the upper plenum when you build up a pool of two phase liquid which is what happens for most of the transient in a Boiling Water Reactor. And for that, the primary area of interest turns out to be right out at the outside of the perimeter of the reactor where you have the very cold liquid being injected into this pool of liquid continuous two phase mixture and it does a rather good job out there. 30° gives you enough region such that the wall effects are fairly negligible.

DR. WARD: Let's see. You said in the Lynn facility you have 58 channels simulated. What's simulated in the channels?

DR. DIX: In this facility, because of the large number of channels, we did not use heated fuel rods. Instead, we used short fuel rod dummy segments if you will, to get the right kind of flow characteristics and then we injected steam into the channels to simulate the vaporization that would have occurred off of heated channels.

We also had reactor hardware at the top and bottom of the pie plate regions where we felt it would be most important to simulate the counter current flow characteristics.

Some other facilities now, the bottom four that I have listed onthe chart are Japanese facilities and you may or may not be familiar with these. We have had very close interaction with these Japanese facilities and in fact as you'll see, there's some close ties between our own facilities and the facilities in Japan. In particular, the one that I have listed, the 18° Sector Test Facility was actually an offshoot that Toshiba developed when they saw the facility that we were developing and we worked with them. The features of the 18° sector are similar to the Lynn facility. However, it's a slightly smaller pie shaped sector. It's only 18°, therefore has a lesser number of fuel channels in it and it operates at atmospheric pressure rather than being a high pressure facility as we have at the Lynn facility. Now, the key feature, however, that you get out of this 18° sector is with the low pressure they were able to put in very large windows in various locations so you can actually look in and see the phenomena going on.

This proved to be very valuable because the instrumentation that was installed into this facility was approximately the same as the instrumentation we had in the Lynn facility and of course, the Lynn facility ends up being in a very large pressure vessel so you're entirely dependent upon the instrumentation to interpret the phenomena.

We had the same instrumentation in the 18° facility plus we have the luxury now at low pressure of having windows so you can actually look in and take photographs and high speed film. We found that fortunately, most of the interpretations that we were making of the instrumentation in Lynn were in fact, fairly straight forward and were very well supported by the visual observations that were made in the 18° sector, so this is a very nice complementary facility.

There is also a 60° sector so we have a slightly larger sector now at a facility in Japan at Hitachi and this one was focusing only on the upper plenum. The Lynn facility and the 18° facility actually had the rest of the system components so you could look at how the system responded. The 60° was just looking at the upper plenum. Results came out quite similar, again quite complementary, too, so we ended up having three sector facilities looking at least at the upper plenum region.

DR. TIEN: Gary, could I ask a question? All this, you know, sector tests, high tests of course assumes sector of symmetry, especially for upper plenum where you have under CCF conditions, based on your experience do you see actually, really, you get very good circle of symmetry or actually the flow situations and resorting in say CCFL breakdown is quite asymmetrical.

DR. DIX: This was one of the key questions indeed, since we are always assuming circular symmetry when you start breaking it up into a pie shaped sector. It turns out, perhaps not surprisingly because the BWR is built with quite good symmetry. Everything is flowing axially is coming in uniformly radially, that the results from the Lynn facility suggest that even the CCFL breakdown happened quite symmetrically. For example, and I'll be getting into this. I'm sort of pre-empting where the conclusions go, but what happens is, when you have a pool of liquid sitting in the upper plenum and you turn on these cold sprays, the cold liquid penetrates down and you get breakdown in the peripheral channels and we found indeed that virtually all of the channels broke down almost simultaneously around the periphery and this was one of the key elements in interpreting were we getting some wall effects or were we inducing some kind of asymmetrical flows and it appeared not, that we would get a very nice breakdown of all the channels and draining entirely in the periphery so that's our primary interpretation, that in fact the symmetry does hold even for CCFL breakdowns.

Okay, I should point out, I implicitly commented on it, that these three sector facilities had a lot of bundles and therefore all of them took the same approach of having steam injection, rather than having heated rods, so the last element then, you might question, we have single bundle heated effects here. We have large numbers of bundles with these facilities. Is there some possibility that when you get multiple bundles and they're heated you get some new phenomenon occurring? And fortunately again, we have a couple of nice facilities in Japan that are addressing that point. One is at the Japan Atomic Energy Research, the JAERI laboratories, is a four bundle system facility built with the same kind of scaling philosophy as the TLTA or the FIST facility, that is, drives the channels, have the rest of the entire BWR system simulated so it puts realistic input and output conditions on the channels, but here instead of having one channel it has four channels.

DR. PLESSET: Aren't they half height?

DR. DIX: I'm sorry?

DR. PLESSET: Aren't they half height?

DR. DIX: Yes. These four channels are running half height and the other facility which then is sort of a counterpart of that and basically the reason for it was to address the question of half height, another facility was built at Hitachi laboratories in Japan which now has two channels and these are full height. In fact, this two channel facility is almost the comparable of the FIST facility. It has some very minor limitations in the simulation of the separator height and the vessel height above the steam separator, but it is almost the full vertical height in addition to -- it does have full vertical height within the fuel channels so two good facilities here to look at multiple channel interactions with heated channels.

What I'd like to do now is start carrying you through the evolution of the technology and this will go fairly fast but these are the highlights.

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For calibration I thought I would point out what are we really doing in the licensing model, just so that you understand where we're starting from today and this sketch is a fairly accurate picture of the assumptions somewhat implicit in what the phenomena would look like and what the cooling distribution looks like in the reactor. If you literally interpret what's in the licensing model, we spend an awful lot of time in a situation where all of the coolant has drained out of the core region. We have a vaporization going up through the top that restricts and allows only a small amount of liquid to come down. We're putting far more coolant into the upper plenum than is allowed to drain by the calculations so it in a sense is completely filled with liquid, although that doesn't enter in in any practical sense in the calculation. Liquid then slowly runs down through -- a small amount cooling the core, a small amount, and then drains into the lower plenum which is calculated to completely empty out in most cases or in many cases and then slowly refills back up with this limited amount of liquid that's allowed to run down and eventually will fill back up and reflood the channels and cool them off. In the mean time, we have no coolant associated with the steam that's flowing up through here and out the top. We have only the coolant associated with what we refer to as the spray dripping down from above so this is a large part of what we do as far as the physical picture with the current evaluation model.

(Slide)

Now, the technology evolved quite a ways just based on a single channel experiments and in fact, just based on the Two Loop Test Apparatus and some associated separate effects, heat transfer studies, just the key though, single channel model development and we did do a lot of studies. Unfortunately, a lot of the separate effects, heat transfer studies were oriented at that picture that I just showed you, that is, a channel sitting here and having a little bit of liquid coming down from the top and having to be cooled over a very long transient period and then finally reflooded so these kinds of tests -- there's an awful lot of data there that isn't very fruitful in the real world but was very important to our evaluation model. The more important ones are what we call the integral systems tests. What really happens when you cut the thing loose when you cut the thing loose with this being our Two Loop Test Apparatus.

Now, what we've found and these are the two highlights, I guess I would say, is that while the CCFL as you can imagine from that previous sketch is very, has a very adverse effect on peak clad temperature in a licensing calculation, that being that it keeps the liquid from draining down in either putting a lot of liquid in the core to cool it and more importantly so the licensing model slows down the rate at which it fills back up and finally refloods.

In contrast, we found that CCFL is quite favorable, so our old adversary, after the changes in the rules about 1975 has really in fact become our friends and is a very favorable effect for the BWR. I'll show you how that comes about. In addition, and not very surprisingly, we get very high heat transfer throughout the transient and I'll show you how that comes about as well.

First I thought I would just try to give you pictorially now the view of the BWR and most of the transient if you take what we have learned out of single channel tests so this would be the view now. It's evolved to this point from single channel tests and the key thing that happens, we find that vaporization from the lower plenum -- first, I should say the lower plenum retains an awful lot of liquid. In fact, in the single channel test, we found that the liquid moved down to the bottoms of the jet pump diffusers and would stay at that elevation then throughout the remainder of the transient so you have a lot of liquid in the lower plenum and as you depressurize with a break, that liquid vaporizes and the vapor goes partially up and out the jet pumps and partially goes up and through the fuel channel.

But we found, and not again too surprising if you think about what's really happening, we have a fairly tight inlet restriction at the bottom for stability and normal operation and this ends up having a fairly restrictive counter-current flow or CCFL characteristic. So, the vapor that goes up through the channel actually highly restricts the amount of liquid that can run down and what we found is that the channel stays full of liquid, a two phase mixture for quite a long period and in fact, in the single channel tests as I'll show you in a moment, the liquid at some later time does drain out about 40 or 50 seconds after the break would be assumed to occur. By that time, you've gotten rid of all of your stored energy and you're only dealing with the Decay Heat. Then, interestingly when the liquid drains out of here of course, it also drains out of the bypass region. The bypass actually drains into the bottom of the fuel channel. There's some leakage paths here. This drains out. But then when the coolant systems come on, you quickly fill the bypass back up again and what happens is this leakage path now lets the bypass liquid run into the bottom of the fuel channel and once again, our friend CCFL at the bottom doesn't let that liquid drain out and you fill up the channel, even though you haven't filled up the lower plenum.

VOICE: (From audience, inaudible question.)

DR. DIX: Yes. Well, account for -- let me clarify. We accounted for it, we included that in the Lynn tests. In these 1-dimensional tests I'm referring to, you really don't know exactly how to characterize that. In the Lynn tests we did and we found that our characterization in the single channel tests have been quite conservative, that you actually get more drainage into the bypass than what we had assumed.

VOICE: There's plenty of room for water to seep around it.

DR. DIX: Yes, yes.

DR. CATTON: One other thing. When you've got a channel with a lot of fluid in it like that, you're going to have a lot of entrainment (ph) in the steam that's going up through the top. Do you account for the effect of that entrainment on the CCFL?

DR. DIX: We do implicitly by having obtained the data with a similar situation. You're referring back now -- let me try to separate. In our current evaluation model, we use a CCFL correlation that is based on data that had entrainment in it but we did not explicitly account for entrainment. In our TRAC model, we in fact explicitly calculate and account for entrainment so it depends on the two worlds that you're dealing in.

DR. CATTON: I understand. We're headed more towards best estimate on that question.

DR. DIX: Yes. In the TRAC code we account for it.

DR. CATTON: Somehow the proper amount of entrainment, you calculate the proper amount of entrainment and then you have to know what that amount of entrainment will do to the CCFL and put the whole thing together.

DR. DIX: Yes, if you want to do it analytically, that's a correct statement. You could also run an experiment in which you had CCFL and then you -- and run the experiment under the conditions of the -- that the plant would experience and then just grossly correlate the results of that.

DR. CATTON: That's certainly true, providing you know that you have the same circumstances at the same time. I'm not sure that you --

DR. DIX: Sure, there's always a limitation if you try to make that --

DR. CATTON: As you indicated, the fuel is only simulated (ph) and it's not full length and it's probably not heated the same at the same time. All kinds of questions like that would have been raised. We'll come back to this more when you talk about your --

DR. TIEN: Maybe I just ask some information. I understand what you mentions, you -- I guess I was aware of that test you have, actually going through the sector that water was, air going through. Based on that, you know, take into account this entrainment. But we have at Berkeley performed extensive tests in the last two or three years. Prof. Gail McCarthy and so on was entrainment -- and so, interesting enough we find in terms of CCFL breakdown we didn't -- well, we did only the adiabatic tests. We just arbitrarily put the more entrainment particles, particle waters into it. It did not affect that much except not maybe applied to your case, the liquid carry-over has tremendous effect, you know the -- due to entrainment. But in terms of CCFL breakdown, not that much effect. This is just our very recent research information.

MR. EBERSOLE: May I ask a question? Is the top of the diffuses remained at two-thirds core height through all these years?

DR. DIX: Yes, that's correct.

MR. EBERSOLE: Also, I recall way back when, the core spray function was not -- couldn't meet the single failure (ph) criteria and that you might end up with one core spray system out of the two that you had. This left the core spray function per se in the old design, inadequate from the spraying viewpoint to cool the top of the core and you depended on cooling being derived from the two-thirds core height, the flooding mode of cooling and the froth (ph) cooling mode. Yet there was a question at that time whether that cooling essentially would persist in comparatively long times into the shut down because of the actual depression of the power level. Has all that been straightened out over these years?

DR. DIX: Yes.

MR. EBERSOLE: It's just a bit of history to me.

DR. DIX: Okay, let me try to hit on a couple of those points. The spray cooling systems can have a failure of one of the spray systems, therefore you can have operation with only one spray. Under that situation you do get and again we're now talking about the previous picture that I had portrayed with what happens with the bundle sitting empty and liquid only coming from the top. You would still get the coolant coming down from that spray and that does, even under the licensing current model calculate some heat transfer and that allows for a heat up that will go up and indeed, in order to avoid exceeding the 2200° limit, you have to get the reflooding occurring soon enough to turn that around, yet the spray itself would have of course, turn it around eventually, but it would not meet the 2200° Fahrenheit limit if you did not go ahead and calculate a reflood.

Once it has reflooded, the gate flood head that is imposed at two-thirds height that is imposed by the jet pumps will put a similar liquid head in the core and because of the power addition then, you will get swelling and you actually keep the core full until the power drops down to a very low level and it's in the range, I don't recall the number exactly but I think it's less than 10 Kilowatts before you will actually start pulling a level in here, just balancing against the liquid head out here and at that point, the heat transfer just due to the steam that comes off from the vaporization is enough to easily handle that, that very low power level.

MR. THEOFANOUS: Let me ask you, I meant to ask it on this process of accumulating the liquid in the bundle after the bundle has emptied and then from the bypass. Are you planning to discuss this in some detail?

DR. DIX: What I plan to do is show you some data that illustrates that, yes. I can discuss it. Maybe I'll show you that data and if there's some specific questions --

(Slide)

Okay, I think this is covered in the key elements of the world as we see it from the single channel tests. Probably the key item to note is that CCFL at the top now is not very important it turns out to us because the exact rate at which the liquid falls through here under these conditions is probably, it has some minor influence on the heat transfer above the liquid level that's in the bundle, but the bundle is only uncovered for a relatively small time and the steam cooling that you get, even if you didn't have the liquid is quite good so while this is very important in our current licensing model, it turns out to be relatively unimportant in the phenomena as we see it in the experiment, but it's counterpart, the CCFL at the bottom now becomes extremely important.

This is now just some actual data that shows that response that I characterized. On the side is a depiction of the two-lip test apparatus in full height so the fuel channel here is a full height fuel channel. As I indicated, the lower plenum is of course, much shorter than the reactor dimension would be as well as the region above the upper plenum.

What I have depicted on here is an indication of the level in various locations and I'd like to highlight first the level in the fuel bundle itself is this heavier dashed line and you can see that the level stays up -- actually it's up in the upper plenum, stays up for some period of time and let me clarify this a little. What we find is, that the level stays up in the fuel bundle until the lower plenum level moves down to the bottom of the jet pump and that's happening because until the level gets to the bottom of the jet pump, all of the vapors from flashing in the lower plenum is forced to go up through the bundle and there's just no drainage going out that bundle -- it's just sitting there so the level in the lower plenum is dropping down and when the level gets down close to the bottom of here, then we have a path where the flashing vapor can exit out through the jet pump and out the break and at that point then the vapor does that, instead of going up through the channel and the level or at least the reduced amount goes up through the channel and the level in the channel then comes down pretty fast, so it's just a matter of, we've got it bottled up in the bottom until it can clear itself at the bottom of the jet pump. The level in the channel comes down quite fast and then because there is a drainage between the region, the bypass region outside and the channel, the bypass then starts draining into this now empty channel and you see the bypass level comes following right behind it, a little bit delayed.

Now, as I mentioned, the level in the lower plenum went down to the bottom of the jet pump and it just hangs there in the single channel tests.

Out in time we start getting the --

MR. THEOFANOUS: Excuse me. A question. I guess I don't see the -- unless my figure is distorted from the copying, it looks to me like you're draining before the level reaches the bottom of the jet pump. I think the moment that the level actually -- the moment that the level comes -- even begins to decrease a little bit, you already start draining?

DR. DIX: Yes, indeed you are.

MR. THEOFANOUS: I think you said that they start draining after the thing reaches the bottom of the jet pump.

DR. DIX: It starts really coming down when you get down here but it's true -- as soon as you pull a level, you are indeed draining some out of the channel. Your point is well taken. It is not absolutely blocked up. You are draining some.

MR. THEOFANOUS: It looks like you have stopped draining by the time you are half way between the top and the bottom of the jet pump.

DR. DIX: There may be an artist conception problem on the figure but in actual fact, you don't really start draining this out until you can get rid of the vapor going back out the jet pump before it really starts crashing down.

DR. CATTON: The bundle level is actually the solid water level too, isn't it?

DR. DIX: Well, these are from conductivity cells, so we are in fact -- I have plotted here the actual level.

DR. CATTON: It's a nice clean interface that's falling?

DR. DIX: Yes. It is a very clean interface. It just moves down through the bundle, falls down through the bundle.

MR. EBERSOLE: You didn't say what the accident mode was. I assume it was a large suction line break?

DR. DIX: I'm sorry, yes. What I'm showing you here -- most of our work was done and in fact the facility is scaled such that it's pretty accurate for a large, what we call a design basis accident, the suction line break.

MR. EBERSOLE: Suction line.

DR. DIX: Okay, the emergency systems come on then and probably the most important one is the LPCI that--

MR. THEOFANOUS: Excuse me, another question. Isn't that heated? It is, isn't it?

DR. DIX: Yes. Yes.

MR. THEOFANOUS: Don't you have any continous vapor production during this time of draining?

DR. DIX: Sure.

MR. THEOFANOUS: Don't this vapor production of the bundle will push also liquid out both ways?

DR. DIX: Well, you're saying do we get a particularly high pressure drop due to the vapor formation. I think the answer to that is no. We do have vapor formation going on in here but I think you just have the density head pretty much driving it. It's not a huge production. The power is dropping off very rapidly so it's not an explosive character. I think it's pretty much draining under the density head primarily. There is some acceleration component of course, due to the vaporization but I don't think that's a large factor compared to the density head.

MR. THEOFANOUS: I would think that the stem (ph) would be pretty high at this point and you said that the level comes out very very clean in answer to Dr. Catton's question. I just, I can't see that. I don't see a single phase level coming down. I think that should be very much to face (ph) and very much swelled (ph) and trying to, this thing trying to get out and get us some --- maybe I'm wrong but I don't see --

DR. DIX: Well, I'm quoting --

DR. TIEN: Part of the problem, the water level drops very fast if you look at a curve, so you actually have a tremendous volume generated in a very short time.

MR. THEOFANOUS: That's what I'm trying to indicate, I guess and I think that Gary disagrees with that.

DR. DIX: All I'm doing, Theo, is telling you what we see. The single levels are pretty clean. You can tell pretty well when you have, you know, what we're looking at in a conductivity cell is -- you get a mixture of -- you're seeing the two phased mixture. When the bubbles are there, the conductivity cell gives part of the time liquid, part of the time vapor and once it passes, you get a pretty clean signal that it is predominantly vapor. Now, indeed, there's a lot of liquid entrained in that, but when you look at a conductivity cell, you're saying, do you have a liquid continuous region or do you have a vapor continuous region and you can get a pretty clean indication of that from these conductivity cells and it is, I'm sure it's in there pulsing and surging around but it in fact is pretty clean that you can pick out that you're a predominantly a pool where you have a relatively low void fraction or a low fraction of the time you're seeing vapor versus predominantly a vapor --

DR. SHROCK: Gary aren't your probes, aren't they wall electrodes?

DR. DIX: Yes.

DR. SHROCK: So really what you're looking at is the draining of a film and you're not really looking at when it's two phase or a single phase across the channel, when the film drains past the electrodes then it --

DR. DIX: No, I think I probably misanswered -- let me see if I can get a clarification on exactly how far they do penetrate in. We were trying to avoid getting into the film area. Gary, do you know how far in detail those probes are in?

MR. SOZZI: Gary Sozzi from General Electric.

There's a combination of three elements that you can use to detect a mixture level. As Gary pointed out, the conductivity probe -- in conjunction with the conductivity probes, spaced over one foot increments along the 12 foot channel where differential pressure transducers, and also on the heated rods themselves were thermo-couples placed very close to the outside of the cladium (ph) and what you see is a very consistent pattern as the mixture level as indicated here is dropping, you get a corresponding indication in the pressure drop measuring the elevation head and you see it going from a liquid continuous media to a vapor continuous medium and at the same time as the mixture level drops, you start getting an indication of heat up on the rods. To the left of that line, the rods generally are in nucleat (ph) boiling staying well-cooled. And to the right side of that line, you start seeing a heat -- you start seeing heat up on the actual rods themselves so there are really three pieces of information to construct that one line. Does that help?

MR. THEOFANOUS: That helps me. I think without belaboring the point very much, I want to say that the phenomena I think is important and I think we need to understand it and I guess that the information that is given there is not enough for me to understand really what's happening there.

DR. PLESSET: I think that's right, Theo, they've simplified it and condensed it. A lot more goes into it than what we're hearing now.

MR. SOZZI: Maybe one other point that Theo indicated and that is that the -- below this mixture level there is a board fraction. There is vapor that is contained below the liquid level.

MR. THEOFANOUS: Okay, that helps me, too.

MR. SOZZI: It is not solid water. Maybe that wasn't clear.

DR. PLESSET: That makes him happy. Let's go on. I think we're running a little bit behind.

DR. DIX: Okay, let me try to finish up the discussion of this point. As you see, once the emergency systems come on and as I say, a very important one of that is the cooling injection which is putting liquid directly into the bypass region, you're getting more liquid in then than can drain through that leakage hole in the bypass so the bypass starts refilling it's cycles a little bit and fills up and now because of the CCFL that the inlet orifice and the leakage now from the bypass into the channel, then the channel refills even though the lower plenum stays empty so that's a fairly cryptic description of the world that we saw in a number of this single channeled experiments.

DR. CATTON: Does that diagram say that the channel goes basically via the bypass?

DR. DIX: That's correct -- well, it's actually both. Both are contributing.

DR. CATTON: The slope on that level curve looks like it comes mostly from below.

DR. DIX: Mostly it is. Mostly from the bypass. Yes, the bypass is the dominant, the leakage in from the bypass is the dominant effect here.

MR. EBERSOLE: The HPCS is not very important to this transient is it, because you don't even have it in the older plants.

DR. DIX: Well, I'll give you an answer. I can't get from the single channel test -- from further studies that we have done, particularly with the TRAC code, the HPCS does in fact influence the overall transient; to say whether it's important or not, you don't have to have the HPCS but in fact it does change the transient to get cold water in very early in the transient.

DR. TIEN: Gary, may I raise one point here. Maybe you come back, I don't know. It is so important, this lower level CCFL so in your best estimate or the evaluation monitor, you used some kind of correlation. Do you have a lot of experimental data and you know, also kind of a physical model understanding about the lower opening, CCFL type and because most of the data and the literature and so on studied has been mostly on the top, either type plate (ph) or single channel on the top CCFL.

DR. DIX: Yes, since the answer we have in fact taken quite a lot of data -- that particular characteristic is somewhat illustrated here. The entry region instead of being vertical is actually horizontal so the steam flow is actually going in horizontally and the liquid is running out horizontally at this restriction so it's quite different than typically where you have a vertical situation and the two flows are going vertically. We find that in this region you actually have -- the flows are stratified and so you're getting again a different characteristic as far as a CCFL, the phenomenon itself. We have taken a lot of experimental data on it. It turns out that the characteristics are not significantly different, almost surprisingly than the normal vertical characteristics, but they do have some unique features to it associated with the particular geometry and we've had to develop that from a large data base.

DR. CATTON: Is it fair to say that the bottom CCFL is what's allowing that channel to fill?

DR. DIX: Yes.

DR. CATTON: So then your experimental simulation is very important.

DR. DIX: That's correct.

DR. CATTON: Youwould almost want us to use the exact hardware (ph), wouldn't you?

DR. DIX: Indeed. In the Lynn 30° sector, we went exactly to reactor hardware with all of the -- this is a rather complex flow passage in here and so we had to go actually to the actual reactor testings.

DR. TIEN: I don't know whether we're coming back to this topic later again but I would certainly like very much to know more because it's very crucial you know, for the bottom CCFL and whether the data base and also the all the information, you know, is very solid built into the code.

DR. DIX: Okay, we in fact, our primary focus today will be not on the experiments -- I'm trying to give you an overview of the experiments with obviously much simplifications. During the course of the model discussions, we can try to amplify on that point, but we didn't have prepared necessarily a detailed discussion of that so we'll try to amplify it there.

MR. EBERSOLE: Am I understanding that what used to be a ferocious flap about spray distribution on top of the core really didn't have any real meaning?

DR. DIX: For the jet pump BWR's core spray distribution, it has virtually no meaning.

MR. EBERSOLE: Good.

MR. THEOFANOUS: On the same topic, I'm afraid there's a somewhat detailed question again. Your interpretation here is that you have the bundle filling up because of this current limitation. Now, what that means is, another way to look at that is that you're building up pressure or you are able to maintain pressure in the lower plenum. Now, the reason you do that is because presumably the pressure cannot really fall by the venting through the jet pumps. Therefore, you need to be concerned with two things. Number one, you have correctly modeled the venting capability of this lower plenum, vis a' vis the increased wall heating that you have in the small sized lower plenum as opposed to the full scale BWR. Now, of course you realize that over there you have all kinds of other controls and so on, but -- and other structures, but have you looked into that aspect of it, because I think that's important in keeping up the pressure and that's the only reason it's holding up the liquid.

DR. DIX: Let me characterize first that we haven't come to the real world yet. We've made a giant step toward the real world. We're in the 1-dimensional test and the answers and the situation in the lower plenum region is slightly different when you go to many channels, so I'll answer your question about this facility but that's not quite the real world. It turns out what happens here is that you do shove not just vapor up the jet pump but you push a two phase mixture up the jet pump and that's what's balancing the pressure in the core.

DR. PLESSET: We've got a problem, Dr. Dix. You've got a lot of material you may not be able to present because we're running out of time so I'm going to leave it to you how to handle this.

DR. DIX: I will run fast.

DR. PLESSET: Well, you're going to have to leave some things out, I think.

DR. DIX: Okay, let me just very quickly then show you the temperature response and this was referred to earlier.

(Slide)

Now what I have is the level showing how the level in the channels, this is the same figure really, just stretched out slightly in time and you can see what happens to the temperature. I've plotted here the temperature at the mid-plane. We do get in that facility a little heat up just prior to uncovery but when the level passes, you see that virtually all of the rods then start heating up so now you're starting to be cooled in a vapor continuous region. The heat up is not very fast as you can see. On this scale we're somewhere here less than 800°, that they actually rewetted due to the liquid coming down from the top and they heat it up again and finally when this level progressed back through the core then all of the rods quenched and it just followed saturation temperature, so two messages here. Very little heat up until the level falls through so again, as Gary Sozzi indicated, another indication, you have a fairly crisp level that you contract with this temperature response and pretty good heat transfer -- in fact, quite good heat transfer, even when it's uncovered.

(Slide)

I think I'll pass the next one then. It's just a summary of highlights and we've hit those.

(Slide)

Okay, now multiple channel experiments and I've really hit on this. We have two types. The sectors in there were looking for these three dimensional effects, the upper plenum response and what happens when you get a lot of channels interacting, and then the complement as the heated channel facilities that you're interested in -- do you get any unique parallel interactions with heater channels and what finally is the fuel rod temperature when you have multiple channels.

DR. CATTON: We're going to be hearing about the SAFER code and basically the SAFER code is one dimensional. Now, as far as I can understand the upper plenum region is highly three dimensional, essentially sub-cooled out of the periphery and break down of the channels of the periphery earlier than the center. I'd like to how you, the experimentalist resolved that with one dimensional representation, or what could you do to one dimensionalize this problem to stick it into the SAFER code, to represent it?

DR. DIX: It turns out that the SAFER code does that and I'm not sure you'd say we did that on purpose, but it does it in a sense of it time sections it. It trades off time segments for spatial and by that, what happens is that the liquid builds up in a SAFER calculation. The whole upper plenum becomes sub-cooled and the one channel that you have breaks down for a period of time. It drains all the liquid down and then the liquid becomes saturated and it builds back up again so it cycles in time, in effect representing first the periphery if you will, during the break down time and then representing the rest of the core. It wasn't necessarily intended that that was exactly how it would go but that's the way it works when you do it with a one dimensional.

DR. CATTON: When the SAFER code is described, I'd like to dwell on this a little. Really, I don't find your description very satisfying.

DR. DIX: Sure. I simply described the way it turns out working and it works pretty effectively as it turns out but let's do defer that until the SAFER discussion.

(Slide)

I'll pass on the next slide. You have it which is really -- let me just put it up and make sure you appreciate the kind of facility we're talking about. This is a very large vessel. We've enclosed the whole thing inside of a large pressure vessel, the 30° sector and all of the upper plenum, lower plenum characteristics so that we could then run this as a depressurization system and it's focused on only the later part of the transient so we sized this system to run from 150 PSI. We're looking at the period of time after the low pressure system and cooling systems would come on, so it's what we call a REFLOOD experiment. It's only looking at the later part but that's when all the interesting REFLOOD actually occurs.

DR. SHROCK: Excuse me, Gary. Before you take that away this relates to a question that Ivan Catton raised earlier. You're not simulating liquid carry over in this test, is that correct? As it shows in the picture you have only steam injected but there's no liquid passing up with that steam?

DR. DIX: I would say there probably is quite a lot. In the BWR for calibration, the vapor velocities are quite a lot lower than in the PWR systems so the actual amount of entrainment that we get from the experiments we find is relatively low. I think, however, the droplets that are coming down from above, come in with some distribution and I think some probably get turned around and carried back out again, so we're getting whatever you get just from the normal process of the liquid coming down. We are not getting anything --

DR. SHROCK: That's what I have always had trouble with on this experiment. I don't see that through some magic quirk of fate we really are getting the same entrainment situation here that we would get from liquid sputtering off from over-heated rods phase flow coming up from the lower reachers of the thing. So many details involved in what really determines that two-phase flow pattern in the region of this upper tie plate that are not really simulated here and I have some difficulty in accepting the premise that somehow it turns out that the counter-current flow limitation is not different with these different entrainment rates that will exist in the real system.

DR. DIX: Virgil, you have me caught between a rock and a hard spot. I want to give you a very complete answer and I'm caught for time. Let me say, however, that the basis for this is not this experiment. The basis for [illegible] conclusion that you can use an adiabatic bundle and get pretty good characterization of the CCFL effects, it's based upon separate effects tests with a bundle in which we had an adiabatic bundle identical to the one around here and a heated bundle. We also have data from Japan where they have done a similar thing running the identical bundle with and without heat injection so they have vapor injection and then they had heat addition. They get precisely the same CCFL characteristics and this in fact is in our licensing topicals. We have those data compared. I think the answer to that is, we just don't get very much liquid entrainment because clearly, if you get a lot of liquid entrainment, it dramatically changes CCFL. I think the answer is that in the BWR with a relatively low vapor velocities, we get relatively small amounts of liquid entrainment, whether you have heat addition or don't have heat addition.

MR. EBERSOLE: Hang on, just a minute. Will you throw that back just a minute? There's a point unclear to me. I understand, I'm talking to my colleagues, this represents all the plants. It's representative of most of your plants, if not all of them, right?

DR. DIX: The general characteristics -- it's representative of the jet pump plants. There are some earlier plants that did not have jet pumps.

MR. EBERSOLE: I see you have high pressure core sprays? Water, as an input to this experiment?

DR. DIX: Yes, this particular -- the scaling basis of this was a BWR-6 plant.

MR. EBERSOLE: Oh. Then for the other plants you simply don't operate that system when you run an experiment? You don't have high pressure core spray on the old plants because the turbine never works.

DR. DIX: No, we have in this facility, we predominantly were looking at the phenomenon and we took a reference plant. We made a very small attempt to look at other plants by looking also at a BWR-4 and we there simply turned on the low pressure core spray. It's at a different elevation. But the dominant data came for BWR-6 simulation. It turns out it really doesn't matter in this case because this is only 150 PSI facility and the only point of HPCS here was that it was in the elevation of an HPCS but we're not getting any of the effects of what happened exactly with the HPCS earlier in the transient when the pressure is higher.

MR. EBERSOLE: Are you telling me then in modeling the older plants you simply don't turn on the HPCS in your experiments? You do not use it, period?

DR. DIX: In this facility we did not turn on the HPCS to represent the BWR-4's, that's correct.

DR. PLESSET: We have a little misunderstanding up here at the table and you're contributing to it so you can share in it. All the background here is not being completely presented. This is data from a lot of other facilities. As you said, separate effect as the Japanese and so on. I think maybe you're going to have to have another meeting and go into the experimental situation more completely than we have time for today. Now, with that in mind, what can you do? We've got to get on with the main topic. Some of the things we'll have to accept on face because of the other contributions to the data from other facilities. Now, go ahead. You might make a brief comment.

(Pause)

DR. PLESSET: I think the general agreement up here with my colleagues, that we'll have another meeting in which we'll get a more complete background of the data which supports your analysis including all of it, not only this kind of data but separate effects, Japanese data and so on and there will be just nothing else involved, so if you can get them to accept some of your statements and say you'll hear about it at the next meeting, maybe we can wind it up.

DR. DIX: Let me characterize what my attempt and what we had hoped to accomplish with a very short overview and this clearly is a short overview that is trying to highlight the key results and clearly I'm not trying to defend them because that's a -- in fact, maybe I've made a mistake in time here in attempting to -- I think, however --

DR. PLESSET: This is very interesting. You can see that's why it drags on. They want to hear these things in detail.

DR. DIX: I think in order to appreciate some of the features of the model and particularly understand why single one dimensional model may be acceptable or what features are acceptable in the SAFER Model versus TRAC, it would be very valuable to go ahead and show these highlights and that's really the only purpose of this, to set the tone of why and how we made the decisions on reducing the complexity of the model. If I try to go through here very quickly now and not give you the rest of the highlights, I think you will have a deficiency then in the discussions that follow. What I would like to do is maybe ask for your indulgence in not trying to defend all of the issues by going into the background, but I would also like to go through and if you will, expand this a little longer than what the time we had originally planned and we'll make that up in the model development discussions but I think it overall will pay if we go through and you see the highlights -- then you know the whole picture as we at least think we know it from the experiments and you can come back at a later meeting and challenge that but I think it's useful now.

DR. PLESSET: Okay, let's do it on that basis. Go ahead.

(Slide)

DR. DIX: What I've done here then is try to characterize in advance now what have we learned out of these multi-channel experiments and of course, an important element in this is, how do they compare to the single channel. How did the world change, if you will and there are a couple of features. It turns out it didn't change very much in the overall sense, but there are some subtle details that did indeed change. One of the details is that now instead of the lower plenum emptying out to the bottom of the jet pump before the vapor can vent up, what happens instead is that we have a lot of channels here and some of those channels end up being driven into a cocurrent upflow and we actually start venting the vapor out some of the channels so we get a very high velocity vapor flow going out some of the channels. That results then in the level actually not going all the way down so you keep a little bit higher level in the lower plenum. Most of the channels end up in a mode that looks very much like it did in the single channel tests. The dominant flow regime that occurs is what we call the counter-current flow where a level is in the channel and it moves up or down depending upon the rest of the conditions imposed on it. A very important one and one that was a focus of this experiment was that we do indeed -- we built up a pool in the upper plenum. We forced a pool, but you would have a pool in the BWR and when you turn on the cold spray system that cold water comes right down into the peripheral channels and those break down and start flowing with a very high velocity sub-cooled liquid so you get three dominant flow regimes, three flow regimes total, this one being the dominant one which makes as it turns out the system overall is controlled in a response mode very much the same as a single channel but indeed you do have these other two flow regimes that are important to the overall timing and sequence.

There is no question that you could address these loads if you had a three channel model with a little more detail than if you have a single channel model.

DR. CATTON: There are some arguments given by some of the practitioners at General Hydraulics is that happens is that that cold water flows down and right around and out the break and the dry channel stays dry for a very long period of time. If you're not modeling the three channels, you're really can't address that criticism. It's a comment for the SAFER --

DR. DIX: You cannot with a single channel code. That's of course, wny you have the benchmark code so that you can go and evaluate that and that of course, is why we have this experiment with a lot of channels to see if that's what really happens. I guess I'm an advocate that one experiment is worth 1000 expert opinions and this one is worth many thousand.

DR. CATTON: Supposing that that expert opinion is based on an experiment with a simulant fluid. I'm sure you know about the experiment.

DR. DIX: In the sense of getting through this fast, let me proceed.

(Slide)

The liquid level -- this is a typical result of what we see. If we look at the upper plenum, we forced in some cases a two phase mixture to be there at the start of the test and here we turned on the spray and this is now looking at the collapsed level so this is just Delta-P in the upper plenum. We find that there's a little bit of increase in the level and this is occurring while that sub-cooling is working it's way down to the upper tie plates of the peripheral channels. Once that happens, then you get break down. You get very rapid draining and then it stabilizes and the level in the upper plenum sits at some level. This is a collapsed level and hangs there for a period of, the remainder of the transient in fact.

Now, if you look at what's going on at the upper tie plates, these are temperatures measured just below the upper tie plate, so if you're getting sub-cooled liquid down, you'll see that just underneath the tie plate where it penetrates through, and sure enough when the spray comes on this is the peripheral channels -- the thermo-couple reads a very sharp drop in temperature so we're starting to get subcooled liquid draining down and then as the level drops down, the subcooling decreases because the level starts dropping down. It actually uncovers the spray header and you actually start getting a lot of condensation on the spray so that you no longer have the same amount of subcooling. Subcooling comes back up and then it hangs with just a little bit of subcooling penetrating through, enough to keep the liquid flowing down.

(Slide)

One channel away from he sparger show a little bit of a spike and come back up but there's a very sharp gradient in the temperature in that pool when you turn it on. Most of the subcooling is going right down those peripheral channels. All the other channels -- so this is the third row, the third of the center just sat there at saturated conditions and didn't see anything happen when you turned on that subcool spray.

(Slide)

Now, an interesting feature, we ran with three different header elevations. These two are associated with the BWR-6 configuration. This is the high pressure, this is the low pressure. This is the elevation of the low pressure for the BWR-4 configurations so we ran tests looking at what happened in the upper plenum with these three elevations and of particular interest was this steady state pool that remained there and what we found is when the header was high, and in this case we started with a very small pool, it actually built up and had a high residual pool level. When we looked at the intermediate one it had an intermediate pool level and when we looked at the low one it had a very small pool level. In these cases, the initial conditions here, whether they were above or below the header were imposed on the test so the important consideration was where did it end up and we speculated that what was happening is the pool was coming down until it uncovered the header and then you would lose the high amount of subcooling because you get a lot of condensation and therefore you would reduce the subcooling entering here and you would reduce the drainage rate and in fact, this tends to confirm that. We went to the test in Japan, the 18° sector test and ran the studies there and in fact visually you could see that the pool will drain down and you just get a very sharp switch. When the pool level passes and of course, this is not a very sharp pool but in general, if the pool tends to be above, you shield the liquid from the vapor source and therefore this liquid stays subcooled and it goes down and you drain a lot. When the level drops down below, then you get a lot of condensation. You get very little subcooling and so you get little draining, so you have an automatic system here that tends to keep the level just hunting right about at the spray elevation. This is independent of how much. If you put more spray systems in, you simply then drain more so again, it's an automatic compensating, if you will, such that it just holds that pool level sitting right there.

(Slide)

Now, the next slide is simply a schematic of the Two Bundle facility. I'm moving on now to what did we see happening when you did this in a multi-channel with heated channels.

(Slide)

I won't bother to put it up. It's simply a total integral system test that has two full scale channels. Let me just put up the key result of that.

In that two channel test you can see -- I see unfortunately that on my slide some of the numbers came off. I think on the copies, you have the numbers on the scale. The temperatures here are -- this is a 400°, 700°, 1000° so you can see that we did see some differences. They saw two different, slightly different flow regimes. The high-power channel tended to stay full and the lower pbwer channel went into the countercurrent flow mode, the same as we have seen in TLTA so a level drops into the average power, lower power channel. It therefore started heating up earlier even though it was at lower power, didn't heat up fast, again about consistent with the single channel results and then later on the void fraction got high enough in the high power channel and it heated up. It went up a little steeper primarily because of the higher power level and then again they both were reflooded, the same as the reflood characteristics that had happened in a single channel and again, we're seeing temperatures here about comparable to what we had seen in the single channel test. Basically, no surprises.

(Slide)

The four channel facility -- again, I'll pass. The schematic, that's the one at the JAERI facility in Japan. They did a very nice parametric study where they just systematically went through and had various break sizes to evaluate what happens. This is the one now that has four channels. They are only half linked so the exact characteristics won't be as accurate as the others. But the general system response and the response to break size I think is probably fairly representative here and you can see that indeed the BWR basically responds about the same, no matter what the break size is. The smaller the break, the longer the delay before the automatic depressurization system comes on. Once that systems opens up it turns it into a large break and you get a very similar kind of response.

Again, temperatures here got up as high as about 1200° Fahrenheit in this shortened length facility.

(Slide)

So the key highlights then, from the multi-channel facility tests are that when you have a lot of channels, most of them end up responding about the same as they do on a single channel. They go into that counter current mode and they sit there with a level. It turns out there is a subtle interaction between the parallel channels such that the drainage rate is a little slower in the multiple channel than it is in single channel tests but they're basically the same. Subcooled liquids breaks down and drains rapidly through the peripheral channel and you get a few channels with very high vapor velocity, vapor updraft and the leakage path at the bottom, it tends to suck liquid in there and you actually get a two phase mixture but it's a vapor continuous mixture. A residual pool does remain in the upper plenum and it hangs about at the height of this sparger elevation and as indicated from the two heated channel tests, you get low temperatures and the highest temperature measure is around 1200°. We think more typical temperatures for the BWR are more representative and are probably going to be in the range of 800 or so.

(Slide)

They key things then with the brief over view is, I want to highlight that the experiments are almost completed. There are still some wrap up experiments going on in Japan in the two bundle and in the four bundle. We have the FIST facility which will be confirmatory and will also cover a little wider range in the small breaks coming up, but basically the experiments are about wrapped up. We think we understand now what would really go on empirically in the BWR by this consolidation of all the experiments, but we still need, of course, the best estimate model. If you want to extrapolate, because none of the experiments are complete and you've got to take some information from some and some from the other so the ideal is to have a best estimate model that can really model the features of the experiments as they exist, if they have heated channels or if they don't have heated channels, put that in and see how it does against the experiment and then use that model finally to project ahead.

We think the experimental basis is diverse and complete enough to really challenge the model. We think we've got a very wide range of conditions here, none of them perfect, but within this I think we've covered everything that is of concern to the BWR and if we can in fact get good correlation between the model and this broad data range, I think we have good confidence for extrapolations of the reactor.

(Slide)

Let me just introduce the modeling now by reiterating what Ed Wood said earlier, in large measure that our modeling approach is a two-pronged approach. First, get the best estimate model. We think this is the workhorse that you've got to have to extrapolate the experiments to the reactor and really benchmark so we can finally say yes, these effects have been accounted for and we now know what the BWR response would be with high confidence. We've gone after very detailed models. It has the 3-dimensional capability. We're going at this in detail, not only in the model development but also in the qualifications, so we're taking the individual modules out of TRAC, comparing those with separate effects tests and also comparing it with the integral system data so we're trying not just to see, when you package it up does it do a good job on peak clad temperature. We're really trying to dig in and make sure we've got the models as good as possible and as well qualified. And then we'll finally use it to turn around and get our benchmark calculations for the reactor.

DR. SCHROCK: Gary, can I ask on that listing what your view is on the importance of multi-dimensional neutron-kinetics modeling for your small break and operational transient modes?

DR. DIX: Can I separate that question?

DR. SCHROCK: Well, it's a part of the TRAC evaluation.

DR. DIX: For the LOCA evaluations, even including small breaks, we don't believe that the nutronics have any significant impact on it. When you get into operational transients, that's another issue and I guess since this is focusing on ECCS, I'd rather not go into the nutronics needs for operational transience, but for LOCA I think I can respond.

DR. SCHROCK: You think at small break LOCA you'll have no difficulty with point kinetics?

DR. DIX: I don't -- I think we can input -- you're basically driven by Decay Heat even for the small break LOCA's. So I don't think the nutronics are a significant factor even for small break LOCA's.

(Slide)

As Ed Wood said, the SAFER code's primary thrust is to get an engineering tool that's efficient, we can utilize, take advantage of what we've learned about, what the controlling phenomenon are. In TRAC of course, we were developing this in parallel and since we didn't know all the controlling phenomenon, we had to try to go in and put everything in that we knew of to put into the code. In SAFER we haven't done that. In SAFER we've benefitted now from the experiment and from the TRAC calculations and have only out in those features that we think are controlling. It is a simplified model. It has larger nodes, other things, but we have not given up on the basic thrust and I think that was the point brought out earlier this morning of having it be what we call realistic so differentiate from the industry standard of saying best estimate means you throw everything in including the kitchen sink, we're using the term realistic to say we haven't thrown in the kitchen sink but it's still trying to get the right answer. No bias one way or the other necessarily.

And of course our plan is, and are implementing now of qualifying that with both the data and of course, most importantly the best estimate model.

I'll give you now just briefly the status of the TRAC model development per se for LOCA predictions we believe is now completed. There are, of course, models in the TRAC version that were developed under the joint G.E., EPRI, NRC program that are not yet in the released version of TRAC that is in the code center, so what I'm referring to here are the models that have now been developed and will be finally implemented into the released version at some later date.

We are nearly complete with our assessment that is being done with G.E. again within this cooperatively funded program and where we're trying to run this against a very broad spectrum. Indeed, the assessment of course will not really be completed until all the experiments are completed and a lot of people have had a chance at it but we're trying to hit the highlights and make sure that there are no loose ends or further model development needs required and that activity is nearly completed. We basically believe we're there now with best estimate prediction capability and we'll be showing you some of that subsequently and we're now in the process of using this version for quantifying the uncertainty in the SAFER code so the TRAC code is an important part and you'll see how that factors in later to quantifying the uncertainties associated with SAFER.

SAFER itself, we had submitted it to the NRC last December. It is now under close review and I think the word acceptant here, the concept of having a realistic approach to it has been accepted by the NRC. In fact, it's been encouraged by them. I think they also like the idea that you'll have a realistic prediction and then you'll put some kind of an uncertainty adder factor onto that. You can use that realistic calculation though for operator training and design guidance, that sort of thing and I think they like that as well as we do.

The assessment is still ongoing. We're doing the comparisons with data, with SAFER as well and you'll see some of that and this application methodology which now is the question of exactly how do you account for the uncertainties that you would put onto the realistic calculation for a licensing purpose is underdeveloped and it is under discussion with NRC currently. I have to apologize for having let that string out somewhat. I hope that gives a reasonable picture of our perception of what the BWR response is like from an empirical standpoint. I hope it will be useful now as we talk about the model development and the comparisons of the model from the data.

DR. ZUDANS: Of all this discussion, I'm still left with one question. This single bundle test that you showed the results where the CCFL occurred at the bottom, inlet to the bundle. Have you run that case analytically with TRAC and have you been able to show the energy balances and where the additional energy comes from to keep that CCFL at that inlet? Have you analyzed it?

DR. DIX: Yes, yes. TRAC does a good job of predicting that. SAFER does a good job of predicting that. It turns out that it's simply the vaporization from the lower plenum. As you're depressurizing, you're vaporizing. You have a lot of --

DR. ZUDANS: What is the source of this vaporization? Where the energy comes from?

DR. DIX: It's the depressurization, so basically the liquid becomes saturated as you depressurize and that continues to vaporize and flash off as you continue to drop down.

DR. ZUDANS: So wouldn't that situation persist
throughout the entire core in a similar fashion? They would
have the same vaporization rates, plus additional vaporization
due to the Decay Heat in the rod?

DR. DIX: Yes, you do.

DR. ZUDANS: And that ought to kind of keep it
back, push it out rather than in?

DR. DIX: Well, it turns out that the restriction
at the bottom is very, is the most limiting and you have a
lot of liquid in the lower plenum below this restruction
that's flashing, so you have a lot of vapor going up,
so it's a question of where you have the highest velocity
combined with the most restrictive flow passage and that
inlet is very restrictive and all of this vapor from the
bottom has to go through these channels, so that's what
is happening. So you end up still stacking it up even
though you do have vaporization and you actually have a
higher vapor flow rate in the channels but the channels
but the restrictions are much more open.

MR. THEOFANOUS: Gary, you're not flashing at the
kind of sudden --- the pressure by that time should be pretty
low. I think the vapor production at this stage of the
game comes from hitting from the wall. I don't think the
pressure is changing very much in a 150 seconds.

DR. DIX: It is not changing at the same rate, but it's very low, and so, because of the low density of the vapor there you get a very high velocity. So the vapor production is still quite high, Theo. You're right. There is also vapor production due to heating from the wall, but the vaporization is still very --

DR. ZUDANS: That wall heating effect would be quite different in the reactor, because you have a lot less metal volume compared to the pool volume that you have in this model.

DR. DIX: In the one dimensional test, that's right. You always get extra vaporization in the one dimensional test.

DR. ZUDANS: And the reason I asked the question is, maybe this behavior is typical to the test facility rather than to the real reactor and that's the question.

DR. DIX: That's the reason why we went to the very large scale test where there, the vaporization due to flashing is pretty representative because there you have a very large sector and you no longer have this large scaling difference. So that's true, one dimensional tests will always give a slightly different result and that's why you need to go back and have a model that you can set up for that facility and see whether in fact you can predict that result, so it's not reactor-like totally when you go to these very small tests.

DR. ZUDANS: Just one more question. Because you have an automatic depressurization system in the BWR, wouldn't this effect be absorbable in a full-scale plant if you would depressurize it for any reason whatsoever? Wouldn't you show it? It wouldn't be visible in the real plant as well?

DR. DIX: I'm sorry, the effect --

DR. ZUDANS: You have an automatic depressurization system in the BWR?

DR. DIX: Yes.

DR. ZUDANS: That sometimes functions --

DR. DIX: Not if we can help it.

DR. ZUDANS: That never happens.

DR. PLESSET: You don't get this --

DR. DIX: You don't want to do that.

DR. ZUDANS: You don't want to do that.

DR. PLESSET: I don't think it's ever happened.

DR. ZUDANS: I was just asking whether they were handled?

DR. PLESSET: I don't think so.

DR. DIX: I don't think it's ever depressurized. I'm not aware of it ever occurring.

DR. ZUDANS: If it did happen, should you be able to observe this phenomenon?

DR. PLESSET: I doubt it.

DR. DIX: No, you would get some of the same phenomena. You couldn't observe it, of course, because you wouldn't have the instrumentation to measure it. Certainly if you depressurize, open up the automatic depressurization, you will get a lot of this phenomena occurring.

DR. ZUDANS: I'm always tempted to use the reactor for experimentation but I guess it's not practical.

DR. CATTON: Isn't the bottom line in all this that you better characterize that bottom part of those fuel bundles well, particularly bypass and the CCFL? If you don't characterize it well, you're going to miss all this.

DR. DIX: Well, you must have those features. When you say characterize it well --

DR. CATTON: Well, characterize it will because --

DR. DIX: If say, you were off a little bit on the CCFL characteristics there, you would still get this similar kind of thing. You just wouldn't repeat it in detail. If on the other hand you didn't have some of the features like the leakage path between the bypass and the bottom of the channel which some university tests don't have, you would not get the same phenomena.

DR. CATTON: That's true. That's right.

DR. PLESSET: Well, let me just make one remark.

When the Staff finishes their evaluation, I think they will want to come and meet with this committee again and at that time you can see some of the things that have been brought up here. You can be prepared to give them succinct and good answers so there are still some questions left that you didn't have time to answer today, maybe. The fact that your presentation got stretched out is not just your fault. There's a lot of contribution from this table, so we don't blame you entirely, but I do think we're going to expect to come back to these things. I believe the Staff is just about a month away from being prepared to comment on -- beg your pardon? Oh, I understand longer than a month. Do you know when the Staff expects to complete their --

DR. DIX: We are going to be going in meeting with the Staff with our final results in late January and so we're expecting by the end of the first quarter, perhaps.

DR. PLESSET: Okay then, yes. Do we have somebody from the Staff who can tell us?

MR. COLLINS: I'm Tim Collins from Reactor Systems Branch. Our schedule calls for G.E.'s response to our questions on the 26th of January, the date they gave us. Based on that and assuming that it's a nice complete package and it's not tremendously controversial, we think we can complete our evaluation by the third week of March.

DR. PLESSET: Well, that's a little longer than I thought but that's not too far ahead. Fine. Well, so we'll be coming back to some of these things again and with that in mind, we can consider this part of the presentation complete and maybe take a ten minute recess. Off the record.

(Whereupon, a ten minute recess was taken.)

DR. PLESSET: On the record. Let's reconvene and continue. I believe we're going to go into the SAFER model discussion.

MR. QUIRK: We have on the agenda, James Anderson will talk about the TRAC model description.

DR. ANDERSON: Okay, what I'd like to talk about is the model development of the TRAC code and I'm going to talk about some of the developmental assessment we have made that is part of the development of the code.

(Slide)

This is a development which has been ongoing for a couple of years now and it's a joint development project which involves the Idaho National Engineering Lab. What we've been doing here at G.E. is jointly sponsored by the Nuclear Regulatory Commission, the Electric Power Institute and General Electric. And it's part of the Refill/Reflood program. The objective of being to develop a best estimate model, describing the phenomena in the BWR viewing the loss of coolant accident and of course, our objective allows us to use the model to demonstrate two safety margins in the BWR. Where we are right now is that the development of the model is complete. The developmental assessment has demonstrated good agreement of the data. The qualification of the code which you'll hear about later also demonstrates good agreement.

(Slide)

Some of the current capabilities of the TRAC code, it has 3-dimensional hydraulic model and that's primarily the 3-dimensional calculation of the fluid dynamics in the vessel of the lower and the upper plenum and the bypass region of the vessel component.

The power component such as the fuel channels are still one dimensional in the code. The fluid model is the two fluid model which solves the conservation equation for mass momentum and energy for both the liquid and the vapor phase. As such, it allows us to model the countercurrent flow and it also allows us to simulate some of the dynamic (ph) non-equilibrium.

We modeled the heat transfer during the various phases of the LOCA and in particular we have good models for the reflood phase and the heat transfer during the later part of loss of coolant accident, including the reflood heat transfer, spray cooling and heat transfer at future radiation.

The code which is in a modular structure has component models for all the major BWR components. Together with the multi-dimensional hydraulic in the vessel, the code also allows for multiple channel calculation and it allows us to simulate the three different flow phenomena which were observed in the Lynn test facility.

The constitutive correlation which basically controls the wall friction, the interfacial shear, the wall and the interfacial heat transfer has been developed based on the state of the art knowledge and provides for good predictive capability of the individual phenomena. What we believe we have in the tri-code now is the best available benchmark tool for BWR calculations.

(Slide)

The approach we have taken in the development of the models is to first develop the models for the individual phenomena in the BWR and to develop models for the specific BWR components such as fuel channels, jet pumps, steam separators.

We started out by assessing the model by seeing how well we can predict basic effects test and once we accomplished that, we continued to more complicated tests where you get system interactions. A lot of the developmental assessment will now be shown to cover the basic effects test and very little of the system effects test. There will later on be as probably qualification, a presentation which would go more in depth on system effects tests.

The final use of the code would be to apply it for BWR predictions to get the two best estimate prediction of what's happening in the BWR.

(Slide)

As I mentioned, the development has been ongoing for several years now. It started back in 1979. Of course, the BWR version of TRAC is based directionally on the PWR version which was developed in Los Alamos which started even earlier than that. But the development of the BWR version, the first version was available, we call it TRAC B01, was developed by G.E. in 1980 and the qualification of the model was complete in 1981.

In 1981 then came out TRAC BD1 from Idaho which contained a lot of the models which were developed for the TRAC B01. Idaho released a new version in 1982 which was called BD1 Version 12 and just lately we have finished TRAC B02 which is based on BD 1 Version 12 and it includes all of the models we have developed here at G.E. and this is the version of the code I will be talking about. Of course, this is not duplicate efforts here and in Idaho. These models will make it into subsequent versions from Idaho.

DR. SCHROCK: Jens, could you comment on that notation? B0 refers to what? The zero is different from the pressurized water reactor notation. What does it signify here?

DR. ANDERSON: Okay, this just signifies the first version.

DR. SCHROCK: So now you've got a B02.

DR. ANDERSON: This is the second version. Our approach was to develop an --

DR. SCHROCK: It doesn't distinguish fast from detailed?

DR. ANDERSON: No, no, it's just -- we had the program which was over four year and we decided let's get a code which has most of the BWR features built in already so we can use it and that was completed in '82. It helped us in deciding how to conduct subsequent experiments. We also had a very detailed review of the modeling capabilities at this time and it helped us to decide on where we need to additional development for the final version so it's just succeeding improvements of the code. They are both detailed versions. However, it's a good question. In Los Alamos they developed the two step method which they implemented into TRAC PF1 which allows it to run much faster. We have recently implemented to the two step methods into TRAC B02 or a slightly modified version of the two step message and we are right now in the process of testing this out and it does show that we can run the code much faster.

DR. WARD: How much faster, Jens?

DR. ANDERSON: Okay, we have not completed the assessment of that but, so I cannot give you a good answer. It depends very much on the amount of detail you want from the simulation. The main thing that would control how much faster the calculation could proceed would be not stability which is limiting now what accuracy of the prediction. We have run cases which are in order of magnitude faster than the detailed version of the code.

DR. CATTON: Doesn't that make it almost as fast as SAFER then when you do a factor of 10 faster? No?

DR. ANDERSON: No.

DR. CATTON: Oh, okay.

DR. ANDERSON: No, because there's still a lot of detail in the code which we do not have in the SAFER code. If you want to go down and look at the calculation in kind of computer time per time step, you get down to the same order of magnitude in the computer speed and if you run the code with very few notes you can make it run very fast but then you tend to lose the detailed simulation.

DR. ZUDANS: I have a question on that. In this two step method, the accuracy is at issue and it's also strictly problem dependent now. How are you going to assess what benefits you can get out of this faster method because you are losing accuracy if you go too large times the estimate. Do you have some criteria built in there already?

DR. ANDERSON: No, as I mentioned, the implementation and the testing out of the two step method is not complete yet so we have not completed that phase but eventually we will have to determine it by looking at the convergence as we make the time step smaller and we have to look at the comparison of the data to see how large a time step we can get away with and still get a decent good prediction.

DR. ZUDANS: Another question, B02 G.E. version, is that supposed to be released by someone at some time?

DR. ANDERSON: What we are doing -- see, this is -- development is going on in cooperation with Idaho National Engineering Lab and we have taken the latest version BC02, we took the latest version that was released from Idaho in May this year and we implemented into this version all the models we have developed at G.E. Now, these models we are making available to Idaho and they will later on release a version which is called TRAC BD2 which will contain most of the models we have developed here at General Electric.

DR. SCHROCK: Is B02 going to be a released code?

DR. ANDERSON: It's not going to be released to the, like the Argonne computer library. We're going to release the models to Idaho who then has the official responsibility for the, as the NRC subcontractor that develops TRAC, the BWR version of TRAC and they will implement the model and release the code to the computer library in Argonne.

(Slide)

What I would like to talk about is some of the model development we had made towards developing a BWR version of TRAC and as I started out by saying the code originates from the PWR version so we did the development along two lines. One was that we developed models for the components that were unique to the BWR, the component margins that were not simulated in the original code and that includes, like fuel channel, jet pump and so on and similar, the other line we took was we looked at some of the basic models in the code, constitutive correlation and we looked at which phenomena were in particular important for the BWR and we took a hard look at the models and developed basic models for what we saw which was very important for the BWR and this outlined some of the major basic models which we have developed. We developed a new model for the interfacial shear which was primarily geared towards having good predictive capability for the void fraction. Again, together with the development of models for the interfacial shear, we also developed a new flow regime map. These models were primarily developed at G.E. We improved the models for the heat transfer and the code primarily in the area of having a model for the boiling transition that was better at describing the phenomena as we see them in a boiling water reactor. Primarily it was having a boiling length type correlations with crical heat flux. We included a model for sub-cool boiling. We include models for some radiation heat transfer which could be important for spray cooling type heat transfer. We also made a number of modifications to the interfacial heat transfer. We included models the countercurrent flow limitation effect as you would see it in the upper tie plate or at site entry orifice. A model for the choked flow was implemented by Idaho and finally the last basic models we implemented into the code was the model for the two-phase level, in particular, an accurate modeling of the two-phase level in the downcomer region is important for the early pressure response following a LOCA.

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(Slide.)

The major component models we developed for the code. A fuel channel component was developed in Idaho and that's basically a pipe component with fuel rods inside the pipe component and it allows for heat transfer from the outside similar to the heat transfer between the channel wall and the bypass region of the vessel.

The jet pump component was developed. It was based on the T component in the code. We developed model for the steam separator. It's the same predicting good predictive capability for the carry over and the carry under in the steam separator.

The model for the steam dryer is implemented and we implemented a model for the phenomena in the upper plenum and I'll get back to these models later on with some more details.

Idaho has implemented the number model which are not really important for LOCA simulation, but in case you want to apply the code for other purposes, they had the model for the control system and model for the boron injection and reactivity feedback due to the fuel temperature and the moderated density.

(Slide.)

What I would like to do is to go a little in detail. Describe some of the models and just some of the results of the developmental assessment we did as part of the development of the models.

(Slide.)

Let me start out with the jet pump model.

The jet pump model is based on the T component in the code having the primary side of the T simulate the suction down to the discharge line and the secondary side simulating the guideline and the basic part of the model is the conservation and momentum for the mixing process.

The momentum equation as it is formulated in TRAC is not on the conserving form and particular in the jet pump the mixing and the operation of the jet pump is entirely dominated by the concentration and the momentum.

We implemented that in the code, but it's also dominated by the various losses that occur and the various part of the jet pump there are losses associated with the mixing processs. There are losses associated with the various bends and area changes in the jet pump and we correlated these losses and implemented them and we tested out the jet pump model not only for normal operation, but you have the drive line, the suction and the discharge and depending on the various possible combination of inflow and outflow, you can have a total of six flow machines and we correlated and tested the model for all six machines.

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(Slide.)

Most of the data which are available are taken for a one-sixth scale jet pump the same which was the sized jet pump which was used in the TLTA experiment. It's plotted in terms of M ratio -- as function of M ratio. The M ratio is the ratio of the suction flow to the drive flow. The N ratio is the difference between the discharge minus suction pressure divided by drive pressure minus discharge pressure.

The points here represent calculations made with the TRAC code. This is the type -- And the line here is the best fit to all the available data that were taken and you can see it covers M ratio from about minus two to three and this is for drive flow in this quadrant here being normal operation.

This here is for negative drive flow. And you can see that quite good agreement is obtained.

(Slide.)

We also ran a test for two phase condition. We had some data available where -- And these were available for normal operation and again you can see that solid line is TRAC and the points are data and we've got quite good predictions.

DR. TIEN: That's a same scale?

MR. ANDERSON: That's the same scale.

DR. TIEN: Do you have any other data which shows --

MR. ANDERSON: We have some data for full scale jet pump in normal operation and we had compared to go to this also and they show recent good agreement in the similar scale that these were. I didn't bring them here.

(Slide.)

The steam separator model was designed to calculate the pressure drop in the steam separate and calculate the carryover and the carryunder through the separators.

Now for LOCA, what is particular important is an accurate prediction of the carryunder. The carryunder the enters the downcomer, mixing in the downcomer region. It controls the amount of subcooling that exists in that region and viewing the depressurization following a LOCA that controls when flashing of the liquid will start.

The model is the mechanistic model for the phenomena in the separator and what is solved is the continuity equation for the mass of the liquid and the vapor and we solve the momentum equation both in the axial direction and in the angular direction.

You have to realize how the separator operates. You have a vein at the entrance to separate which spins the liquid and the centrifigal force forces the liquid to flow upperward as a film on the inside or the outside wall, actually, but the inside of the wall in the separator.

So what we're solving is both the axial and the angular momentum equation and what we used as tuning parameters for the model was the radial void fraction and velocity profiles in the separator.

(Slide.)

MR. CATTON: How many nodes do you have in that particular model?

MR. ANDERSON: That's most of the TRAC components. Separated model is this one dimension, but the action under nodes can be determined by the use of them more often.

We find that we can do a good simulation of the separator by something like four to six nodes, actually. If you want more nodes, you can have that.

This is a comparison of what we can obtain with this model. This is a comparison of carryover. The solid line here is the data and the doted line is the prediction using the TRAC model.

Similar here is the comparison of carryunder. The solid line is data and the dotted line is the prediction.

DR. THEOFANOUS: What pressure was that that was obtained there?

MR. ANDERSON:I think this is obtained at a normal operation pressure and these are the conditions right here are typical of normal operation of the BWR separator.

(Slide.)

The steam dryer model which we have simulating the dryers at the top of the vessel is a relatively simple model and basic function is to simulate the pressure drop in the dry and the separation of the moisture.

No separate component was developed for the steam dryer, but it was integrated as part of the vessel component.

(Slide.)

And the basic concept of the dryer model is that where the dryer function as the dryer is a function of the inlet steam flow. For a given steam flow there is a -- for the moisture can be separated out from the dryer and it's basically a line like this that is the function of the steam flow.

So the model is very simple. Below the solid line we have complete separation and above the dotted line, the separation process breaks down.

(Slide.)

We developed the model for the phenomena in the upper plenum and that's quite --

DR. PLESSET: Before you go into that, could you tell me where the data came from for that separator behavior?

MR. ANDERSON: Okay, we had taken some data for full size steam separator.

DR. PLESSET: At operating pressure?

MR. ANDERSON: At operating pressure and they are published in various documents. I do not remember the reference.

DR. PLESSET: We've never seen it before. Has it been proprietary, is that it?

MR. ANDERSON: The data which we have used has been published in -- I think it's in journals or various meetings.

DR. PLESSET: Oh, it is.

MR. ANDERSON: There is one thing that I should mention is that this program -- the development of the TRAC codes since its jointly sponsored by EPRI and NRC, the data which we're using in developing of the code are available

Let me go on to the upper plenum model.

It's quite a sophisticated model. It has three basic models. It has a spary distribution model and the one thing that is important here is where the two-phased level is in the upper plenum and following Gary Dix' presentation, you saw that you reach a situation with a two-phase level which sits right around the sparger.

If the two-phase level is below the sparger, then we go in and we have a model for spray distribution in the upper plenum.

If the level is below the sparger, then it's essentially a submerged jet that's injected into a pool of liquid in the upper plenum and we have a separate model for that.

DR. EBERSOLE: Isn't the phrase spray distribution misleading in fact because it doesn't mean anything anymore?

MR. ANDERSON: Well, it's not important for the jet pump plans. There are some earlier BWRs with non jet pumps and there it could be important.

We have for the pools we have implemented a model for turbulent shear and mixing which controls the gross flow in the upper plenum in the pool.

We used a 16 degree sector test data to tune the model and I'll show you a few of those results and we have qualified it against the SSTF data -- steam sector test facility.

DR. CATTON: Could you give me one to two sentences as to why the spray distribution is more important for non jet pump plants. I'm missing something.

MR. ANDERSON: If you have the LOCA -- the circulation line break in the non jet pump plan, that's a direct circulation. You take the liquid out of the downcomer and inject it into the lower plenum. So in those plants you have a break directly leading into the lower plenum.

So you cannot do what you can in the jet pump plant flooded up to two sorts core level, because you have a break in the lower plenum. So the non jet pump plant you rely on the spray cooling alone.

DR. CATTON: Okay, I understand.

DR.THEOFANOUS: What do you show on the vertical axis in the previous slide that you already just took off.

MR. ANDERSON: This one?

DR. THEOFANOUS: No, I thought you showed the one with some traces.

DR. PLESSET: That's coming.

(Slide.)

MR. ANDERSON: This one here.

DR. THEOFANOUS: No, no.

DR. PLESSET: It's the one that you were going to show.

MR. ANDERSON: Oh, the one that I was going to show. Okay, this is coming here.

(Slide.)

This is an example on the comparison with the 16 degree sector test and the 16 degrees as far as the upper plenum, it's smaller than the SSTF test. It only covers 16 degree pie sector, but it's not full size.

We have a spray sparger sitting here and it injects liquid in here and what I'm showing is the void fraction measured or calculated in the upper plenum in these five rings.

What we did in the code was that we simulated this pie shaped sector with six radial rings.

Steam was injected from below and we had the upper tie plate here with CCFL.

The test was run in what we call a C factor which is equal to 1.24 and that means that we had 24 percent more steam being injected than what could be condensed by the subcooling of the spray water.

So based on the experience from single bundle tests, there should be no subcooled CCFL breakdown in this facility. However, what you find is that because of the -- dimensional effect and the parallel channel effect, you get subcooling enough to break down the CCFL in the peripheral bundles and what I'm showing is we started the code with a pool of liquid in the upper plenum similar to how the experiment was conducted.

The experiment was run with saturated water here until a steady state pool developed in the upper plenum and then at a given time subcooled water was turned on and that is zero in the time scale.

What I'm showing here is the calculated void fraction in this region. It started out with void fraction around 60 to 70 percent which was typical of what was measured while saturated water was injected.

Now, what you can see is that this curve here which represents the void fraction in this region here very rapidly drops down to a very low value as you get the subcooled water coming in here condensing the steam.

At this point here you get a breakdown and the drainage of the upper plenum. Of course after you drain the liquid, you get a higher void fraction in the region.

DR. TIEN: Could you say a few words of how you take care of the turbulent mixing between different rings?

MR. ANDERSON: It's a very simple turbulence model based on the pump and mixing length theory. We have one of the parameters we could tune in the code was the mixing length and we ended up with a typical mixing length in the order of an inch that would give good agreement with the data.

DR. TIEN: Is that a reasonable value for this particular type of flow situation?

I'm trying to see whether there is some kind of physical or reasonable estimate instead of totally adjustable constants.

MR. ANDERSON: Well, I think it's reasonable giving the size of the upper plenum, but of course, it was one of the parameters which we used to tune and we ran a parametric spectrum. If we had no trouble in mixing at all, we could get into a situation where we could get very rapid -- motion in the upper plenum and we've got a lot of mixing which would give a uniform distribution of the subcooling and preventative breakdown. If we had a avery large mixing length, we would basically stop gross motion in the upper plenum when we got too early a breakdown.

DR. EBERSOLE: I'm having a little trouble concluding something about the non jet pump plants. From what I'm hearing, it looks like you've got a lot of trouble with them. Because you don't have the cooling mode from refilling, which you can from the others and we heard that the spray function, first of all, it's not single failure proof and it's not effective even if it was.

Where do you stand on the safety of the old non jet pump plants?

(Pause)

MR. DENNISON: Basically the BWR-2s, they depend on the two core spray systems and the BWR two core spray was designed -- there nozzles are different designed than the three and four later designs and also the five and six are a different design.

DR. EBERSOLE: What nozzles?

MR. DENNISON: The spray nozzles on the actual --

DR. EBERSOLE: We just heard that it doesn't make much difference why you design the spray nozzles because everything floods out at the top anyway because of counter-core impedence.

MR. DIX: Maybe I should try since I apparently lead to the confusion on this.

The earlier plans, where you postulate a break in the bottom, you do not have the vapor trapped in the lower plenum that has to exit up through the bundles. In fact, it could exit out the break and therefore, you do not get the same kind of pool build up in the top of those plants.

Any plant that would have a break in the bottom, would not have the pool. So the characterization that we gave and apparently I didn't say it clearly enough is the phenomena that I discussed of all of the characteristics are relevant for the jet pump plants. The non jet pump plants, the response is much simplier if you postulate the worst break inthe bottom. It can simply drain out and then you do sit there and you cool them with core spray.

In the non jet pump plants, the older plants, the power density is much lower and even though they're cooled only with core spray, the peak clad temperature still stays below 2200 degrees. But there is no question that the temperatures in the old plants that can have a bottom break would be higher than they would be in the jet pump plants.

DR. PLESSET: Let me go back to the one inch -- That seems a little small to me and gives you better mixing than maybe you're really going to get. That's what I'm a little bit troubled by. I think that's what Dr. Tien was getting at.

DR. TIEN: I was thinking, if I remember, you have some -- you can visualize the full pattern the kind of eddy size should be the really reasonable estimate of this mixing -- if you call that way and so that's what I was trying to see whether that --

MR. ANDERSON: You have to realize that when we run the code, we use -- we can not go down and have nodes the size of one inch node. We use very large nodes even though it's a three-dimensional code. It's maybe in the order of several feet. So it's really questionable how accurately we modeling the two phase, the turbulent mixing. It gives us a tuning parameter and we tune that to give the good comparison to the data and this particular case, the test flow down-- CCFL flow down and was tested six seconds into the transient which is about what we see in the calculation.

We got similar results on some of the other --

DR. CATTON: That makes your tuning node size dependent and so once you've tuned it, you can't change it. Unless you retune it.

MR. ANDERSON: Well, we found that we get good agreement also with the data and the SSTF test facility where we used it with one node size.

DR. CATTON: Did you retune?

MR. ANDERSON: No. We used the same value. As part of the developmental assessment, we developed a model and we found out what our recommended values are. Those were then used in the qualification process which we will describe later.

MR. CATTON: I missed the name of the last facility. What did you mention?

MR. ANDERSON: I did not mention a facility. Oh yes we have run comparison also against data from the SSTF or the Lynn test facility which is much larger test facility and we did not use the same node size in this facility and we still get reasonably good agreement with the data.

DR. THEOFANOUS: This breakdown process, is it pretty continuous or is it happening in dumps. Do you get periodic behavior?

MR. ANDERSON: You do get a periodic behavior because what you see is as you get a breakdown in the two phase level in the upper plenum drops. As you uncover the sparger, then you start getting rapid condensation. You get steam available for the condensation process and you lose the subcooling and you build up the level again and you may get a subsequent dump.

DR. THEOFANOUS: Well, as a general comment, I want to say that -- this is myself -- I'm not getting here any substantial information on some of the important phenomena.

Earlier we said okay. With the experiments, we're going to leave it for another time, but here we are discussing an important part of your model and you want presumably to have some input from us. You want us to think about and you're showing us the previous slide which doesn't contain any of the important physics that you know are present in there.

This is just a comment. I'm not really happy with what I'm hearing from you.

MR. ANDERSON: I had not planned on a presentation of that level of detail here. Because clearly I could not do that within the time that is allocated.

(Slide.)

What I show here is the comparison of the spray distribution model. These are data from the horizontal spray test facility. The circles are the data which is the amount of liquid available as the function of the distance from the spray nozzle location in the solid code with prediction by the model in TRAC code.

DR. SCHROCK: What is the dimension there? I'm not sure on what you're plodding.

MR. ANDERSON: This is the liquid downflow. THe liquid that actually wets the upper tie plate that could be in kilos per second.

DR. SCHROCK: So it's mass flow rate per --

MR. ANDERSON: It's mass flow rate down per channel as function of the distance going away from the nozzle location.

DR. THEOFANOUS: Is that one instance in time and what instant in time?

MR. ANDERSON: This was a steady state test where you just had spray distribution and you measured what the actual distribution was as function of the distance from the nozzle.

DR. THEOFANOUS: In those tests, do you have data of the temperature distribution in the upper plenum?

MR. ANDERSON: This was --

DR. THEOFANOUS: Not in this one. In the previous one. In the previous... Do you have that information on temperatures as a function of time and position -- How do you compare -- How do you TRAC cores that gives over a long period of time. Not only five seconds, but over a long period of time, how are you able to reproduce the periodic behavior and mixing grossly from one part of the pool to the other.

I guess the moment you have breakdown, you should be getting a lot of radial flow from the higher void fraction regions going over to the radial part -- to the outside and that will again submerge eventually the -- submerge the nozzles and then you're going to start developing a subcooled region again and then you're going to get a breakdown again.

Are you able to predict any of that?

MR. ANDERSON: We do have data for the temperatures in the upper tie plate and I do not have them here and they show reasonable good agreement where we've compared the actual calculated subcooling to the measured subcooling.

We have been able to show that we can predict the subsequent build up of the level and following breakdown -- If you want more detail, I'll have to come back to it at a later time.

DR. CATTON: Are the units on that previous figure meters?

MR. ANDERSON: Yeah, the actual distance is meters.

DR. CATTON: What's the vertical scale?

MR. ANDERSON: It's the mass flow down.

(Slide.)

Let me go on and talk about some of the basic models we have developed.

We developed a new void fraction prediction model and the essential part of that was a new model for the interfacial shear tied together with a modefied flow regime map and in the light of trying to stay on schedule which I think I'm already behind, let me just show you some of the results we have obtained.

(Slide.)

This is a comparison, again, some of the void fraction data taken in the FRIGG 36 rod bundle which shows the void fraction as function of the actual distance measured from the inlet to the bundle.

The solid point data and the line is the calculation with the TRAC code. It covers two different pressures and powers and two different inlet subcoolings.

One case here is virtually saturated at the inlet.

(Slide.)

This is another test which is a single tube test where we have highly subcooled inlet and what is plotted is the void fraction as function of the equilibrium quality.

The dotted line is the data and the solid line is the TRAC code.

(Slide.)

Part of the model of the model was the CCFL prediction and we obtained good CCFL prediction partly through the interfacial shear model which is tuned to give agreement with counter and flow data when you're at the counter -- and flow machine.

And by having good models with the condensation and heat transfer and subcooled liquids, it's essential for the prediction of subcooled CCFL breakdown.

DR. CATTON: This is CCFL at the top with the bundle.

MR. ANDERSON: This is CCFL where it happens in the code. We can test for CCFL at site entry or we can test for CCFL -- in the bundle. We can test for CCFL at the upper tie plate. The code would allow us to do it anywhere in the system.

DR. CATTON: You indicated that your predictions were good. Are the predictions universally good?

MR. ANDERSON: Most of the data which are available are for the upper tie plate and there we get good agreement partially because we have used CCFL data for the upper tie plate in the development of the model.

The model is a good correlation.

DR. TIEN: When you use site entry CCFL, do you have a different constance from the top CCFL?

MR. ANDERSON: You can apply in the code -- you can apply two different values for the CCFL constant.

DR. TIEN: But still --

MR. ANDERSON: And you can apply different value for the site entry orifice and the upper pie plate.

DR. TIEN: How do you get those values, the constants -- tuned values?

MR. ANDERSON: The values are obtained from experiment.

DR. CATTON: You're going to describe the experiments that those values were obtained from for the side entry orifice?

MR. ANDERSON: No, I'm not going to describe those.

DR. TIEN: I worked on this area before. I got very much confused. This subcooled CCFL, very recently I saw from a paper from Japan and infact Taiwan also they did some experiments. They show quite different characteristics now from what this energy balance thing. In fact, now they show other agreement with a previous model that I proposed.

I was appending my model before, but now some new phase and they said they agreed. So I don't know if you're aware of these new results.

MR. ANDERSON: I've not seen them.

DR. TIEN: There was some controvery several years ago on this thing, but now there were some experimental -- added on to the controversy also.

(Slide.)

MR. ANDERSON: This is -- shows an example of what the Model and TRAC will do. This is a test for CCFL at the upper tie plate and what is plotted here is along the horizontal lines is a steam flow injected and here we have the liquid downflow. The units are pounds per hour.

5,000 pounds per hour liquid was injected.

Now, this line here represents the saturated CCFL as given by the -- correlation. We ran TRAC both with subcooled and saturated water. We ran it with saturated water we got to the right on this code.

We then ran it with about 100 degree fahrenheit water which is a little more than a 100 degree subcooling and what we find is that all the liquid get down until we get to the point where we have so much steam that we can not condense all the steam at which point we get back on the CCFL line and that is in agreement with the data for saturated and subcooled CCFL.

(Slide.)

We have developed improved model for prediction of the heat transfer and bundle and there are two things that are important in accurate prediction of the heat transfer.

One is an accurate prediction of the hydraulic conditions in the bundle and that is again controlled by the flow regime map that we're using and how accurately we can predict the void fraction.

We demonstrated through assessment of the void fraction model that we can predict a hydraulic conditions adequately and the rest that is left in good prediction of the heat transfer is good prediction of the wall heat transfer and the interfacial heat transfer.

DR. CATTON: Before we get too far away, I would just like to make sure that I understand about the CCFL. You showed us a nice figure with data and flow rates and predictions. It looks very good. Can you show us a similar figure for the CCFL configurations at the bottom of the bundle?

MR. DIX: Excuse me. Can I make a comment?

One of the comments here is that we have developed an open presentation to try to present to the overview of this and we have included as much data as is openly available.

There is, I don't believe, any inlet CCFL data thatis non proprietary data and and that's our difficulty here in bringing it out in this environment. So if you want to see that kind of data and the comparisons, we would have to have a proprietary meeting at some point for that.

DR. CATTON: There's another way, too. I don't know how much interest there is, but I personally would like to see that, because as your earlier figures show, what goes on at the bottom is really important. Probably more important than the top.

If it would be possible, if you could communicate proprietary information to Paul and then he could give it to me and I could take a look at it or whatever the committee chairman would like.

DR. PLESSET: I gather that several members would like to see it. So maybe you would like to discuss it.

So maybe at the next meeting we could plan a closed session. It might be useful anyway. Maybe we should leave it that way. Is that agreeable that you give it to us in close session?

MR. SHERWOOD: We thought we would chat with you during the lunch break in terms of how to handle some of the other questions that came up during this and also Gary Dix' earlier presentation. So why don't we discuss the mechanics then of trying to come to grips with these other questions.

DR. PLESSET: No closed discussion at this meeting, but maybe at another one.

MR. ANDERSON: The main improvements in the heat transfer as I mentioned earlier has been in the wall heat transfer in terms of subcooled boiling. The boiling consistent correlation which consists of the boiling length correlation.

And we included the correlation for our model for thermal radiation heat transfer in the bundle.

(Slide.)

This is a comparison of the data from one of the Oakridge film boiling tests which was at a given time a step increase in the power which forced the bundles to go into film boiling and what we see here, the circles are the data and the solid line is the calculation.

This is the measured wall temperature in the electrically heated rods and this test shows very good agreement with the data.

(Slide.)

This is a prediction of one of the BDHT experiments. Again, it shows the comparison of measured and calculated wall termperature as function of time.

You get an earlier boiling transition and subsequent -- eventually you get into film boiling. Again, we get reasonably good agreement.

(Slide.)

This here is a comparison of the termal radiation model. What it is is an experiment where low steady state power was applied to all 64 rods in the bundle. The outside channel wall was kept cold and the experiment was conducted until the steady state temperature profile was obtained.

DR. CATTON: This is a dry bundle?

MR. ANDERSON: This is a dry bundle inside. It's only a test of the thermal radiation model. The basic mode of heat transfer is thermal radiation.

DR. CATTON: Do you measure the emissivity?

MR. ANDERSON: The emissivity which was used is .7 which is a good emissivity for the stainless steel rods that were used in this test. They were slightly oxidized on the surface, because of the high temperature that was attained and .7 is a good value for that.

DR. THEOFANOUS: Why do you say that? The -- stainless steel has a very low emissivity to start with and you know that it's going to go up and it's going to go up to one.

Now, why .7 is a good value between point two and one. This is a different parameter and not a good value particularly unless you measure it directly.

MR. ANDERSON: You're right. You get different results as you change the emissivity.

DR. TIEN: In fact, this is even a paper that I wrote. So maybe I can mention this. It is -- that's for -- stainless steel that emissivity is well known. The more perhaps -- if you vary the emissivity, you will not be able to fit the data. That's important. I think if you just simply vary the emissivity, it would not be able to fit the data in terms of distribution.

DR. THEOFANOUS: What does that mean?

DR. TIEN: That means that you will not be able to simply say has a floating tune constant to get a distribution. If you change the emissivity, you change the whole distribution of the temperature prediction. You will not be able to get a good fit.

DR. THEOFANOUS: So this value works well. That's all it says.

DR. TIEN: Yes.

DR. PLESSET: I think more than that that if you change it, it's not going to work well.

DR. TIEN: That's what I was thinking.

DR. PLESSET: I think that's a good point.

DR. ZUDAN: Is this .7 independent of heating rate in the element. I'm sure it is not.

MR. ANDERSON: Well the .7 value is used for all the rods which did not have --

DR. ZUDAN: It's for one experiment, right? If you would change the heating rate in the element or the temperature --

MR. ANDERSON: It's not a function of the heating rate, it's a function of the surface condition of the rods. Of course, if you have -- if you start out with nice and shiny rods, you have a much lower imissivity as you conduct experiments and you get more oxidized on the surface, then you get to .7 emissivity.

DR. SCHROCK: Could I ask one last question? On your radiation model, your network analysis presumes each rod is isothermal, isn't that correct?

MR. ANDERSON: Each rod is assumed to be isothermal.

DR. SHROCK: So for rod number one which clearly is the least well represented by that assumption, what do you know about the amount of circumferential variation and temperature that that rod actually experiences and where have you measured the temperature?

MR. ANDERSON: I do not remember what the actual location of the thermal couple was. I don't believe that the circumferential temperature variation is very large.

DR. TIEN: Usually the temperature is almost uniform. The conduction is so strong. The question is very well taken. Although the termperature is uniform - the rod, actually you have very very uniform radiocity and that's where the new factor .5 comes in actually as -- to take care of very uniform radiocity -- uniform heat flux even though you hae the same temperatures because of the rods facing very different environments. You have to take them into account.

MR. ANDERSON: What we have is a first order amocotropic transport correction on the radiation model and we're using it controlling for factors this new factor and the value to be used is four and five which can be showed to be the one theoretically would use for cylindrical rods.

When that accounts for the very non uniform radiocity which you have along the perimeters of the rods.

(Slide.)

Let me just show you a few data on critical flow and level swell.

(Slide.)

This is a comparison to the good old Edwards blowdown test. The triangles are the data and the dotted line is the TRAC BO2 calculation.

(Slide.)

This is a comparison of the level swelling -- level swell test facility and this was a four foot vessel which was filled initially with liquid -- at an actual elevation of four and a half foot. And it was blowndown to a steam line and what we have measured or compared is the actual void fraction profile as the two phase level swelled up following the depressurization.

The circle are the data and the dotted line going through the triangles are the actual calculated actual void fraction profile.

The data indicates that this is where the two phased level is. This is where we calculate the two phase level.

So both the Edward's void fraction profile and the two phased level position is well calculated.

(Slide.)

So if I can summarize my presentation, the development of the BWR version of TRAC for LOCA application successfully completed. From the developmental assessment that we have conducted, we have obtained good agreement with data and we have included enough different testing to developmental assessment to make sure that we have captured all of the major phenomena which you expect in the boiling water reactor.

Thank you.

DR. SCHROCK: Could I ask one question about the reflood applications?

I know that there has been some difficulty at INEL and handling the reflood problem due to the uncertainty of the flow regime just ahead of the quench front and the amount of liquid carryover and the impact that that has on the cursory cooling.

You never really addressed that specifically as I heard your presentation and I think that it is still a fairly unresolved issue. There was discussion on it at the Advance Code Review Group meeting last summer and I think it's a little surprising to me that what I hear you saying is that we've got it all well in hand and we think we have adequate physical representations.

I think that problem is one in which a two fluid model has some severe difficulties because you have liquid droplets, some of which are moving upward and others are falling down simultaneously and essentially continuously with time. That situations prevails for a significant time, I should say.

The modeling of that with the two fluid set of equations is not an obviously simple problem. So I guess -- I would just like to hear your reaction to whether or not that is an area in which the fundamental knowledge is adequate and the codes are already in good shape.

MR. ANDERSON: I agree with you. There are limitations of the two fluid model. That basically means that you have one liquid field and that can either go up or it can go down.

The example you mentioned with droplets, some going up and some going down, we cannot do that. The comparison we have with data from the TLTA test facility shows that we can reasonably well predict the behavior of the bundle.

We will not deny that there are certain details though which we cannot handle.

DR. SHROCK: There have been difficulties with TRAC BD1 predicting Chen's data for example. Isn't that right?

MR. ANDERSON: Yes. That is a very low pressure reflux test.

DR. SHROCK: Granted, but if the modeling is on firm ground, it ought to be able to cope with a low pressure situation as well as higher pressure situations.

MR. ANDERSON: Well, it gets into this problem of having just one liquid field available, because you have a situation where you have large spectrum of droplets being produced. Some are small and some are large and this test all the small droplets are being carried up and the large droplets would fall backdown and will subsequently breakdown and carried up as small droplets.

So in order to model that test accurately, you need to be able to simulate the small droplets that are carried up. Now, if you want to model other tests where you have liquid coming in from the top such as from CCFL at the upper tie plate, what is important there is to be able to model the largest spectrum or the part of the spectrum that contains the larger drop that will penetrate into the bundle.

Having only one liquid field, you have to make a choice which one do you want to have and what the choice was in the TRAC code was to model the droplets representing the larger end of the spectrum and that's why we had a difficulty predicting Chen's experiments.

The only way I really see to get around that is to go one step further and have a three fluid model.

DR. SHROCK: But as Ivan suggested, maybe you're just buying new problems. You'll have to provide more information then in the way of constituative equations in order to do that.

That may not be the answer, but my purpose in raising the question here is only to perhaps shed a slightly different view than the one that I seem to be getting from your presentation which seemed to say to me that all of the physical effects that are important are being adequately modeled in the BWR TRAC codes and I don't see that as yet at that stage.

DR. CATTON: I think this is an example of more detail than you can handle. You don't have the information to describe the detail you're trying to build into the code and that just leads to trouble.

MR. ANDERSON: There are limitations in the model.

DR. CATTON: As a matter of fact, I don't think any of the advance codes do a very good job of reflux for that same reason and I would be surprised if the TRAC BWR would do any better than the others that try to devote more attention to that particular problem.

DR. PLESSET: I think these points that Shrock and Catton mentioned are correct. I have an optimistic feeling however that a kind of a smoothing and integrating effect in a large facility and it may not have a significant effect on the final answers that one gets.

Now, we may not be describing details correctly, even, but it doesn't make all that much difference in the end. Let me stimulate some discussion on that point.

DR. SHROCK: On this one, the things that I've seen would suggest to me that in fact, it is important that the constituative equations currently in BWR TRAC are not adequately handling that reflood problem and that something has got to be developed that will do it better.

I'm not advocating a three fluid model, but what I am saying is that we should recognize the shortcomings of what we have presently and find a way to do the problem that is adequate. I would thoroughly agree that some kind of a smoothing technique is a better route to pursue rather than try to chase after a three fluid model which ties you to constituative equations that you're never going to get.

DR. PLESSET: I think that's a good place to leave it.

Let's have a recess until 1:30 for lunch.

(Whereupon, the hearing was recessed for lunch.)

AFTERNOON SESSION

1:30 p.m.

DR. PLESSET: We will reconvene and then recess to take our tour to see the facilities. So then we'll come back here and go into session then at 3:00 p.m.

(A recess.)

3:00 p.m.

DR. PLESSET: I think our next item is GESTR, Mr. Potts, is that right?

(Pause)

MR. POTTS: My name is Gerry Potts. I'm the manager of the fuel rod thermal mechanical design unit in the nuclear fuel engineering department of GE.

What I will do is give a brief description of the GESTR LOCA model.

(Slide.)

I'll start off with a background, what it is, what it's function is. Give a description of the various phenomena that are considered and then discuss the experimental qualification performed.

(Slide.)

GESTR-LOCA is a mechanistic fuel rod thermal mechanical performance model. It analyzes an individual fuel rod. It divides the fuel rod up into a number of axial nodes to adequately describe the axial power distribution and divides the N sincle node into a number of radial rings to adequately describe the radial temperature distribution.

It's function in the loss of coolant accident analysis sequence is to initialize the conditions at the onset of transient and that is the fuel stored energy cap conductant imputs and the inventory of fission gas that is released from the fuel pellet to the void space.

The application of this model is to both UO_2 and gadalinia fuel.

It's applicable to zircaloy cladding and our barrier cladding where we have a thin zirconimum liner on the ID of the clad and the GESTR - LOCA in conjunction with the SAFER will replace GEGP/SAFE and reflood in the loss of coolant accident analysis sequence.

The status is that the model is fully developed. The qualification is complete. THe LTR was submitted in December of '81 and we just finished the second round of NRC review questions.

(Slide.)

Here I'd like to walk through the various models -- component models that are in GESTR. All of these models are to the greatest extent possible independently derived calibrated to test data and they're combined and qualified to integral fuel rod experiments.

We start with the thermal model. The temperature solution starts off in the coolant saturated conditions works inward to the cladding accounting for the resistances to heat transfer due to the accumulation of crude or oxide on the outer surface of the fuel rod.

We then calculate the gap conductance and use a modified version of the Ross and Stout gap conductance model and calculate fuel temperatures accounting for any flux suppression in the pellet.

The mechanical model is an elastic/plastic model. We have elastic/plastic properties and account for any radiation effects such as the hardening of the cladding strength, increased hardening and the annelling of the hardening with a radiation as the temperatures get higher.

The individual expansion models include thermal expansion, irradiation growth of the cladding, the radiation swelling of the pellet due to the accumulation of solid and gaseous fission products in the fuel matrix, N reactor fuel densification, cracking up and outward movement of the pellet call relocation, fuel and cladding creep, mechanical densification or hot pressing and fuel cladding axial interaction.

This fuel cladding axial interaction accounts for the fact that pellets when they're randomly loaded into a very long fuel rod are going to be off center to some extent and they're going to develop axial forces and even though there is no hard radial contact, the pellets will be locked in the fuel rod and will cause large axial stresses.

The mechanical model itself is a finite element model. Let me just briefly walk through hat.

(Slide.)

This is the way we idealize the behavior of the fuel rod. At very low powers, the radial temperature gradiant is sufficient to cause radial cracks that extend all the way to the center of the pellet. We also develop transverse cracks through that radial temperature gradiant.

As we go up in power or as the expansion mechanisms contribute, these radial cracks begin to close and the stiffness of the pellet is originally very low when it's highly cracked and so it takes a very small interface pressure to being closing those cracks.

As the cracks begin to close it takes a higher and higher interface pressure to close those cracks and the pellet stiffness becomes greater. When all cracks are closed, radial transfer cracks its effectively -- the stiffness of a right circular cylinder. A solid right circular cylinder.

The cracking -- we account for that cracking explicitly and the crack front can be at various radial locations.

DR. EBERSOLE: Pardon me. What keeps the pellet concentric with the cladding in both the original and the broken state?

MR. POTTS: It's modeled that way.

DR. EBERSOLE: In reality, it can't be that way, can it?

MR. POTTS: It no doubt is not in real life. It is modeled that way for our purposes. If it is in a non concentric geometry, as you begin to expand, it's probably will start shifting over before you get into any real hard radial contact and tend to line, but for the purpose of our calculations, we assumed that it is concentric.

(Slide.)

Finite element model divides the pellet -- this is a cross section of a fuel rod -- divides the pellet into a number of radial elements. Cladding into a number of radial elements -- rings. We have an element right here at the interface. The stiffness of this element is set equal to zero prior to any hard radial contact and once there is contact, we set that stiffness equal to infinity so that the pellet and clad move together.

Circumferential symmetry. The clad and the pellet are modeled such that they are mechanically coupled as a plane strain calulation and we account for the Poison effects between the pellet and the clad.

(Slide)

The model also has the capability to determine local mechanical conditions. The hour-glassing of the pellet that can cause ridges have pellet-pellet interfaces and also the local strain concentration adjacent to a pellet radial crack.

DR. ZUDAN: Could I ask a question on the model?

MR. POTTS: Yes.

DR. ZUDANS: The rod is modeled axially by a number of such elements, right?

MR. POTTS: That's right.

DR. ZUDANS: Than I assume then that the calculation is incremental.

MR. POTTS: Yes, that's right.

DR. ZUDANS: How do you model the cladding? You didn't say anything. Is it modeled as a finite element, too?

MR. POTTS: Yes, it is.

DR. ZUDANS: And assumes that it's an axiomatic deformation.

MR. POTTS: Yes.

(Slide)

The mechanical and the thermal models are coupled and is coupled through the gap conductance.

Initially we have to assume a state of the pellet clad gap or a state of the closure of these radial cracks. With that gap assumption, we calculate that gap conductance. That defines then what the fuel temperatures are which then defines what the expansion is, the state of radial crack closure and an update of what the actual mechanical gap is. That gap gets fed back into the thermal solution and we iterate and converge to have a consistent set of thermal mechanical calculations.

Once that internal interation is performed, we then at each axial location determine the amount of fission gas release and then also calculate the internal pressure in the rod.

DR. TIEN: Could I ask several questions about this cladding. You always have some eccentricity, right? The gap resistance is quite important in terms of the temperature gap across the gap.

MR. POTTS: The eccentricity of the pellet within the --

DR. TIEN: Right.

MR. POTTS: Yes.

DR. TIEN: You have various gap thickness.

MR. POTTS: Yes.

DR. TIEN: However, is that correct, because of the conduction around in the circumferential direction will smooth out that so that you can get still a good fuel temperature?

MR. POTTS: Well, the non-concentric case is actually the most conservative case thermally. I think Bettel wrote a report some years ago. They did a very detailed study on that. Because of the increase conduction locally, it's more than offsets the degradation and conduction due to the wider gap around.

So for the purpose of our assumption, our analysis, we assume that it's concentric. We account for the eccentricity effect in terms of this axial locking that I alluded to.

(Slide)

With respect to experimental qualification, the thermal model is calibrated and qualified to continuous in-reactor measurement by central fuel thermal couples in the fuel column. One of the primary data sets used here is the NRC state of the art reg, IFA431,432 Holden Regs radiated out to exposures of about 25,000 megawatt days per ton.

Mechanical model is qualified to diameter change measurements, both mid-plane at a pellet -- mid pellet location and local measurements ridge heights.

Also we qualify to length change measurements. These are measurements both taken on fuel rods that are radiated basically in steady state conditions and we also look at the deformations on regs that are instrumitted so that we can see what what the length change or diameter change is during a power ramp.

Fission gas release model is qualified to a large number of experimental and commercial fuel rod data points all obtained from puncture of the rod and collection of the gas.

The fuel rod internal pressure is qualified to continuous in-reactor pressure transducer measurements. These are data that are obtained from the Holden Reactor.

DR. ZUDANS: With respect to this eccentricity -- the question was raised. Wouldn't this produce a hot spot on one side of your cylinder of cladding and also cause some bowing of the fuel element -- the whole rod or is not an important aspect?

MR. POTTS: It's an important aspect in terms of if there is an eccentric pellet clad condition during the transient and that hot spot can contribute to the amount of deformation that you can get. For example, the clad ballooning that can be experienced at high temperatures.

For steady state conditions, it's not really a significant effect.

DR. ZUDANS: The model does not include it?

MR. POTTS: Does not consider explicitly the thermal effects of eccentric pellets.

DR. ZUDANS: Then you perform stress calculations and thermal calculations in a couple fashion, right?

MR. POTTS: Yes, that's right.

DR. ZUDANS: They both matched in time, right?

MR. POTTS: That's right.

That's all I plan to say. If there are any other questions.

DR. PLESSET: I don't think so. Thank you very much, Mr. Potts.

And if we can go on.

(Pause)

MR. QUIRK: Our next speaker is Brot Shiralker and he'll be talking about the SAFER model description.

MR. SCHIRALKER: Good afternoon.

(Slide)

I'll be describing some of the models in the evaluation model -- in the evaluation model that we are proposing.

By way of introduction, let me say that again what was said this morning -- our overall direction has been to progress from the very conservative evaluation model we have today and go to where it is more physically realistic in qualified models. And the objective of this work is to quantify the true BWR safety models and also something which has not been stressed very much today, I think, is try to establish really when scenarios for design and operator guidance. And these tend to be mainly in the nature of small breaks -- small events with some possible degradation of systems.

I would like to point out that while we're primarily talking about SAFER, the approach we're proposing is really more than that. It's a combination of SAFER and TRAC to back it up, to calculate uncertainties that you might have in SAFER.

This is why we spent some time this morning talking about TRAC. TRAC is an important part of the overall process and I think tomorrow we'll be describing some more of how we plan to use it in the overall process.

(Slide.)

If I may, I'd like to give you a little background about where we are in the -- in terms of our evaluation model. Today's evaluation model we know from experience and the comparison with data has some significant non-realistic bounding type assumptions and these manifest themselves mainly in two ways.

One the inventory distribution is not correctly predicted. There is more inventory in the core than -- in the lower plenum than what the current evaluation models predict and secondly the heat transfer models are unduly conservative. Like there is no credit at all for steam cooling. No transition boiling.

So, when we started on SAFER, our objective was to get to a more realistic state of affairs through improved nodalization in some ways, hydraulic model improvements and more realistic heat transfer.

I think that this kind of summarizes the main directions that we have gone in making the improvements.

(Slide.)

Though we talk about SAFER as being our evaluation model, it's really not all of the evaluation model. It's a part of it. For those of you who are not familiar with what the evaluation model really looks like in total, today's method is really a combination of about six different computer codes.

There's the short term system blowdown method called LAMB, which is somewhat in the relap category. It's a homogenous code which calculates the pressure and velocities in different regions of the vessel.

There's a short term hot channel heat transfer calculation code and the primary function of that is to calculate boiling transition early in the transient and blowdown heat transfer -- in the transient.

And then we switch to a long term inventory calculation and SAFE and REFLOOD were utilized for that purpose. Those are the long term inventory codes.

Finally there is a fuel rod heat up calculation which is called CHASTE and this looks at 64 of full range of rods inside the bundle and there's a detailed radiative heat transfer calculation and the detail of fuel rod model calculation to look at the final peak clad temperature.

And associated with these models is a fuel rod gap conductance stored energy model which is called GEGP.

So that's our present line up.

The new method we're going to is we're still retaining LAMP and SCAT for the short term calculation. SAFER primarily replaces the SAFER and REFLOOD long term system inventory calculation.

CHASTE code is still available if needed, but the primary area where CHASTE provides a benefit is in the radiative heat transfer and unless the temperatures get up high enough over say 16 - 1700 degrees fahrenheit, there is little purpose in going to the small detail calculation.

So that's why we have it if needed.

And finally the integral part of this calculation is going to be a new fuel rod model, the GESTR model which Mr. Potts just described. Some more mechanistic fuel rod model and goes with the improved models in the LOCA area.

In the third column we have TRAC which in principle can perform all of these functions and we're using it to calibrate the performance of these set of models and primarily SAFER, because SAFER now does most of the calculation.

DR. WARD: TRAC does not have the fuel rod model?

MR. SHIRALKAR: At present, TRAC does not have a dynamic gap conductance model. That's true.

DR. WARD: So when you use TRAC, you run the GESTR or how is it used?

MR. SHIRALKAR: You really need to look at the sensivity of what the gap conductance is and we're finding that the sensitivity of the store energy is fairly small when we're able to remove the stored energy early in the transient.

So we initialize it to get us about the right stored energy and get on with that.

DR. ZUDANS: That raises still another question. How doyou use TRAC to validate GESTR? How do you use TRAC to evaluate your evaluation model? To evaluate your evaluation model.

MR. SHIRALKAR: We're evaluating primarily, I think the hydraulics models. We're evaluating the total core system performance. We can make sensitivity studies with the GESTR model in SAFER and show that it's not a very important fact.

MR. CATTON: In other words, a simple fuel rod model would probably do?

MR. SHIRALKAR: Yes. As long as your temperatures -- as long as you move stored energy early in the transient and you're not talking about performation and so on. I think, yes.

DR. CATTON: Are you going to compare SAFER with TRAC BWR -- to demonstrate that you do or do not need to consider multi channel and --

MR. SHIRALKAR: I'll show you some comparisons later. Let me get into that later as you go along.

DR. CATTON: I was just hoping that you would answer the question yes or no.

MR. SHIRALKAR: I cannot compare SAFER model to channel, because SAFER which I haven't showed you yet -- but SAFER has an average core and it has a hot channel in parallel with it. It's driven by the average core.

DR. CATTON: I understand that. If you run TRAC BWR with multi-channel which you have the capability of doing, then you can compare the results coming from SAFER with it.

MR. SHIRALKAR: That's the purpose,yes. That's the reason for calibration.

We're in the process of doing that and we have some comparisons, but we're not at the point where we can show you all the comparisons. We're just not there yet.

DR. TIEN: I would like to come back to this fuel rod model. Is there any reason not to incorporate a fuel rod model into the TRAC? Let's put it that way.

MR. SHIRALKAR: There is no real reason. We'll probably will at some point. It's just more expense added to the code. I think that the people in Idaho are or have linked the -- code with TRAC for the fuel rod model but that is a linking of two really large codes and if we went -- we would probably go in the direction of a simplified dynamic gap conductance model for TRAC eventually.

DR. CATTON: The bigger uncertainties are usually the thermal hydraulics.

DR. TIEN: I still come back to this basic philosophy. I think that in the big codes somehow we should a simplification also so you don't have to run always -- the very complex picture of models. So that in some cases you can see the model. Not crucial components you can see the model.

DR. SHIRALKAR: I agree. And I think that SAFER is playing that role in many ways. Because in SAFER we can make sensitivity studies. We can vary parameters and look at the importance of the parameters in a fairly easy and efficient manner and I believe that the primary response is reasonably good in SAFER so that we can rely on these sensitivity studies, but your point is well taken.

(Slide)

I will rush very quickly through this. It's a flow chart which just expands a little bit on what I just said. Basically here is our long term inventory codes and here are our short term codes and the final output is a peak clad temperature that comes out of the CHASTE code.

What we're proposing basically is to replace the left-hand side of this page with the SAFER calculation.

(SLIDE)

So once we do that, the new formulation will look like this.

The only reason we really need the LAMB and SCAT with the SAFER code is because SAFER does not have a very good recirculation line model. Very early blowdown process we were simulating with these codes and the primary input that comes to SAFER is the time of boiling transition. That's the only input that you need.

We can do that with this code, but we believe in the more accurate estimate from here and once we have that, then we can run with SAFER coupled with the GESTR cap conductance code that feeds in and recalculate the core and recovery time, the heat transfer coefficients following boiling transition, the vessel pressure, ECC flow rates, core reflooding time and peak cladding temperature.

DR. ZUDANS: Could you walk through the process of sequential process on this chart, because there are many hours and I could start most any place and get to the end.

I'd like to see how it's done and where you start first, which pieces going parallel and where do you meet and what decisions you make.

MR. SHIRALKAR: First we run the LAMP code. This is a short term system response code. It's a homogenous code and the primary outputs from it are the core average pressure, inlet flow and inlet enthalpy.

DR. ZUDAN: As a function of time or given time step?

MR. SHIRALKAR: Function of time.

DR. ZUDAN: So you complete the analysis --

MR. SHIRALKAR: Complete the analysis.

DR. ZUDANS: It's not a coupled analysis.

MR. SHIRALKAR: It's not coupled.

We complete the analysis and we use that to run a more detailed single channel model called SCAT.

MR. ZUDANS: Now you feed in the time history that you've got into this --

MR. SHIRALKAR: That's right. We feed in the inlet enthalpy, inlet flow and the pressure and calculate from that a more detailed heat transfer response.

The primary thing we're looking for is the time of boiling transition or critical heat flux, if you will.

DR. ZUDANS: That's the next step that you do?

MR. SHIRALKAR: Yes. So that is what we feed the SAFER calculation.

DR. ZUDANS: It doesn't look like it's feeding. It's feeding the output.

DR. CATTON: Oh the arrow is going wrong.

MR. SHIRALKAR: That's an error.

DR. ZUDANS: Do you see why it was necessary?

MR. SHIRALKAR: Yes. That arrow should go that way.

DR. ZUDANS: What's next?

MR. SHIRALKAR: So then we perform the long term calculation which start at time zero. But what we do is we provide this input to tell it when to consider it has got a boiling transition.

DR. ZUDANS: You have to have a time scale detailing that resolution sothat you can come in appropriately with this time that you got from LAMB and SCAT step, right?

MR. SHIRALKAR: This is already done, so we have that as an input.

DR. ZUDANS: What's next?

MR. SHIRALKAR: There is no feedback here.

So that's been done and we're on the long term code with that as being one of the inputs.

DR. ZUDANS: You run the entire history again. You run the entire long term time history with SAFER at that point.

MR. SHIRALKAR: We only have one input coming in now. That is one number the time at which we got boiling transition in the hot bundles.

DR. ZUDANS: The output is what ever parameters you compute verses time.

MR. SHIRALKAR: Yes, from SAFER.

DR. ZUDANS: So there is no coupling between the fuel code and this one contrary to what you told me --

MR. SHIRALKAR: No, there is no coupling.

There's an input to SAFER and then the SAFER takes over and does the entire calculation, the transient and ultimately right now we have found it sufficient to stop here at this point, because the peak clad temperatures have been sufficiently low, but if they were at a high enough level, we would continue the process and give it more detail radiated heat transfer calculation which is in the CHASTE code.

In which you input the convective heat transfer coefficients as a function of time.

DR. ZUDANS: You've completed SAFER analysis. You have time history of those things that you list in the output. That includes cladding temperature as well.

MR. SHIRALKAR: That's right.

DR. ZUDANS: Now, you did not even look at the fuel itself. Does not the fuel calculation to GESTR -- to the flooding temperature.

MR. SHIRALKAR: Oh, yes.

DR. ZUDANS: When did you run that analysis in this timeframe?

MR. SHIRALKAR: It's within SAFER. When I go into SAFER alittle bit, I can show you what the fuel rod model looks like. There is a fuel rod model within SAFER which uses the GESTR cap conductance model.

DR. ZUDANS: Are you now telling me that when you run SAFER time history you couple it with GESTR?

MR. SHIRALKAR: Yes.

DR. PLESSET: If he needs to.

MR. SHIRALKAR: Coupled in the sense that it's initialized from GESTR. GESTR is a steady state calculation.

DR. CATTON: So does that give you the initial gap conduction?

MR. SHIRALKAR: It gives you the inital stored energy, the initial gap conduction and the initial gas pressure -- the amount of released products.

MR. CATTON: Can I interrupt --

DR. ZUDANS: Go ahead, I think --

DR. CATTON: Is that at time zero? I thought I understood when I read through your report. Now, I'm really confused. Do you take LAMB to just find this time of boiling transition or do you just take it to give you a set of conditions at a given time from which you start SAFER?

MR. SHIRALKAR: SAFER starts at times zero.

DR. CATTON: Then what is this time used for?

DR. ZUDANS: Only to get time of boiling --

MR. SHIRALKAR: The SAFER has the recirculation model which we believe is not sophisticated enough. The noding is not sufficiently good for the calculation of boiling transition. So we override water boiling transition times calculated in SAFER by a more accurate time that goes from LAMB.

DR. CATTON: But it is not done in a coupled way.

MR. SHIRALKAR: No,it's not.

We just think that that is a more accurate input, so we just use that.

DR. CATTON: But stored energy is a function of that time.

MR. SHIRALKAR: The stored energy is --

DR. CATTON: How do you make sure that those are compatible? You do your calculations with LAMB.

MR. SHIRALKAR: Yes.

DR. CATTON: You get the time.

MR. SHIRALKAR: Yes.

DR. CATTON: Associated with that time is a certain amount of the stored energy that is gotten out of the fuel. Now you're going to take just that time and go into a different code and when you get to the same time you should have all of the same numbers or else there is some incompatibility.

MR. SHIRLAKAR: As far as the fuel is concerned, it will.

DR. CATTON: Then you really don't need to run the first part at all. You can just put the time in straight on.

MR. SHIRALKAR: That's right. The fuel model is complete in SAFER.

DR. CATTON: You run LAMB because it is more accurate for the short term.

MR. SHIRALKAR: RIGHT.

DR. CATTON: But the only piece of information that you use from it is the time.

MR. SHIRALKAR: Yes.

DR. CATTON: Now you use a code thatis less accurate because you're more interested in the long term.

MR. SHIRALKAR: Yes.

DR. CATTON: How are you assured that at that time if you calculate with LAMB your sets of calculations are compatible.

MR. SHIRALKAR: The only things of concern is --

DR. CATTON: Or doesn't it matter?

MR. SHIRALKAR: It doesn't matter, because your nuclear --

DR. CATTON: I thought time to transition did matter.

MR. SHIRLAKAR: It does matter, but you see before that is the nuclear boiling. Soyou don't care what has happened before that.

DR. TIEN: Maybe the easiest way -- what he says sounds physically reasonable but qualify it. Maybe you should go one -- loop to see if they're all consistent.

MR. SHIRALKAR: In fact, we have done that. We have found that --

DR. CATTON: If you've done that, then the answer is that they are compatible.

MR. SHIRALKAR: I'm telling you the process. What the way we're going it.

DR. ZUDANS: There is nothing compatible for --

MR. SHIRALKAR: But in fact we have found that we can use the SAFER calculation itself and really not even rely on LAMB because they're very close. That's been our process as we've explained it before and -- but yes , they're close in terms of timing.

And what happens before that doesn't really matter, because you have nuclear boiling before that.

DR. CATTON: I guess I don't understand why if you run LAMB you still transfer those or what you have at hand is initial conditions and continue at that point with SAFER.

DR. ZUDANS: That would make a lot more sense.

DR. CATTON: It makes more sense.

DR. CATTON: Either that or throw LAMB out.

DR. PLESSET: No, that's essentially what he does, I think.

DR. CATTON: He recalculates from time zero up to the time of transition. The time of transition being calculated by LAMB. Right?

MR. SHIRALKAR: Right.

But they're very close and I think the way we're going I think is the process of not even relying on LAMB.

DR. CATTON: Everything is probably just fine. I just have a fundamental problem of throwing away what you say is best and replacing it with what you say is second best. If thereis a reason to run LAMB during the initial stages, why don't you use the results you get from LAMB and continue from that point in time? Or do you have problems transferring the information --

MR. SHIRALKAR: Well, we might have some problems, but I think the main reason is that I don't believe what has happened before that time is really important.

DR. CATTON: But if you've done the calculations you hae the information.

MR. SHIRLAKAR: I have the information. It's just easier for me to start steady state.

DR. CATTON: I don't want to pursue this.

DR. ZUDANS: Are the models compatible between

LAMB and SAFER?

MR.SHIRLKAR: Which models?

DR. ZUDANS: LAMB and SAFER.

MR. SHIRALKAR: No. LAMB is a short term calculation.

DR. ZUDANS: I mean the physical models.

MR. SHIRALKAR: I think at the very early part of the trend it doesn't make much difference. AT the later part of the transient, LAMB is a homogenious code and it will not do as good a job.

DR. WARD: If SAFER is calculating other parameters than LAMB is right?

MR. SHIRALKAR: Yes.

DR. WARD: That's all there is to it, isn't there? LAMB isn't calculating everything that SAFER is. It does a better job than one number. So that's just plugged in.

MR. SHIRLAKAR: That's the essence.

DR. TIEN: Either you have the input of LAMB into it or go around to see if they are compatible. If you have done several calculations of that type, this is indeed correct.

MR. SHIRALKAR: I still think that it's not very important in terms of what has happened earlier.

DR. PLESSET: Why don't you go on. I think that they're getting reasonably happy.

(Slide)

MR. SHIRALKAR: I think we can go through this one fairly quickly. This describes what we've done. We've been through this before. The fast running model for design application and the application intended for is design operator guidance as is and Appendix K calculations with some kind of uncertainity adder on the results.

DR. CATTON: In making it fast running, you could have done one of two things. You could have simplified the physics or you could develop a fancy numerical algorhythm. What was your basic philosophy?

MR. SHIRALKAR: I think the basic philosophy here is being simplified nodalization and incorporating the phenomena we believe would be most important.

DR. CATTON: So you're not trying to do things like the two step methods or anything like that?

MR. SHIRALKAR: No.

(Slide.)

To summarize the major improvements over what we have today, I think we have made a significant improvement in calculating the inventory distribution.

We have now CCFL being considered at all restrictions including the bottom of the core which we saw this morning is an important phenomena.

We have a calculation of subcooled CCFL breakdown.

We have a drift flux model for sweep flow.

A realistic heat transfer coefficients which include steam cooling and transition boiling.

And with respect to operator guidance, this does not affect the peak clad temperatures, but it is an important feature because it provides complete flexibility for simulating operator actions: shutoffs, starts, restarts and so on.

Now, the core is modeled as an average region, but in parallel to that the high power assembly which is driven by the core delta P and it uses the inlet flows consistent with lower plenum conditions which means that if you have a two phased mixture in the lower plenum, then the inlet to the high power bundle is those two phased conditions. If you have a level in the lower plenum then only steam is allowed to come up.

DR. CATTON: In your average core model, I think I recollect that you have five axial nodes.

MR. SHIRALKAR: Five plus two unheated.

DR. CATTON: Right. Fivein the core and one on either side. I couldn't tell what you did with the hot fuel assembly. Do you also have five axial nodes?

MR. SHIRALKAR: Yes.

DR. CATTON: Your report didn't indicate that clearly.

MR. SHIRALKAR: That's the way it is.

DR. ZUDANS: What is different is that they are fed by the same conditions, why would they come out different?

MR. SHIRALKAR: The power is different and you have --

DR. CATTON: What they're doing is the proper way to do it. They get the average condition in the upper plenum from an average core to propose that on the hot channel.

MR. SHIRALKAR: So there are separate calculations of the inlet flows to the hot channel and separate calculation of inventory and heat up.

(Slide.)

This will give you an idea of some of the sensitivities that you see with SAFER.

DR. ZUDANS: I have a question that bothers me. You said that your inlet and outlet conditions are kept at the conditions of the plenum from the other model and then you feed more energy. Where does this energy go?

MR. SHIRALKAR: Sorry. What do you mean --

DR. ZUDANS: Where would the energy disappear.

DR. PLESSET: No, no. This is how he calculates behavior of the hot channel.

DR. ZUDANS: Yeah, that's right, but the inlets are fixed energy amount and outlet are fixed energy amount.

MR. SHIRALKAR: No, no.

You impose a certain core pressure drop on the high power bundle.

DR. ZUDAN: Just the delta P, not the energy contents.

MR. SHIRALKAR: Just the delta P.

Calculate from that the inlet conditions.

We try to estimate the sensitivies of the various factors on the peak clad temperature. There are a lot of things here, but I think that somethings which you might expect.

The measure of sensitivities are due to the fact -- these features that the CCFL is now considered at all restrictions. That subcooled CCFL breakdown is calculated and and the heat transfer coefficients.

And I would like to point out that the impact of the fuel model is fairly minor.

DR. WARD: What do the dashes mean?

MR. SHIRALKAR: We couldn't really compare it directly with something we had before. We're comparing this with respect to where we were before and we couldn't make a direct comparision.

I would say that the hot channel calculation is really a result of all of these. So what comes out of that is going to be quite different, yes.

(Slide.)

I would like to run through quickly through some of these models.

Under hydraulic models I have listed the nodalization, mass and energy balances, steam slip modesl, CCFL, break flow and overall momentum equations that are solved.

Under special -- we do have special regional models for bypass leakage. Treatment of the upper plenum and the hot fuel assembly.

The external flows which includes all the ECC systems and so on are modeled.

I have some information on the heat transfer models, the nodalization and the heat transfer coefficients.

And the fuel rod models, how they initialize and how the dynamic calculation is done.

(Slide.)

First the nodalization, I can probably best talk referring to the next chart.

(Slide.)

The nodalization is still fairly simple. What we've done is to emphasize the major regions of the reactor vessel that are naturally separated. For example, the lower plenum, the guide tubes are separated by core plate and orificing from the core region. There is a bypass region and actually subdivided core region.

There is a high power bundle in parallel which

I'm not showing.

Upper plenum, the steam dome and outside the downcomer is modeled as two moving regions. The boundary between the subcooling and saturated region is TRAC. This is primarily so that we get a good definition of the breakflow based on the subcooled region movement.

(Slide.)

The mass and energy balances are separately formulated for the subcooled and saturated regions. There is the assumption of thermal equilibrium between the phases, but vapor slip is calculated with respect to liquid by means of drift flux type correlation.

The steam dome is maintained saturated through heat transfer to the liquid in the downcomer and upper plenum regions. The degraded situations such that there might not be sufficient liquid in these regions and super heating of that dome will be calculated.

And we calculate an average pressure rate for the system for thermal dynamic property evaluations. We have differential pressures calculated by the momentum equation at an average pressure rate.

MR. CATTON: In your picture that you had there, I was reading through your report, I couldn't figure out -- one of your nodes is called a lower downcomer or one of your volumes and I couldn't figure out how the subcooled water in the lower downcomer enters the lower plenum. It seems to me that it would flow into the lower -- if you had subcooled water, it would -- if it did get into the lower plenum, it would just go down to the bottom and stratify.

MR. SHIRLAKAR: It goes into the jet pumps, yes. It makes this whatever is inside the lower pump.

DR. CATTON: You mix it with the whole lower plenum.

MR. SHILRALKAR: Yes.

DR. CATTON: In reality if it were subcooled enough, it would stratify in the lower plenum.

Would that have any impact. Have you looked at that? You basically would lose some of the water -- you would lose some of the two phase region.

MR. SHIRLAKAR: Initially when you have sufficient velocities, I don't think you have that problem when the pumps are coasting down. Now, --

DR. CATTON: Stratified flow does a pretty good job of quieting things down and I don't know what the velocities are or anything. It's just that when I look at your picture --

MR.SHILAKAR: The pumping flow through the jet pumps and you're getting a pretty good discharge velocity coming out here and we know from -- you can get stratification when you get down to very low flows at the -- five percent of core flow or something like that.

DR. CATTON: What kind of velocities are you talking about at five percent core flow?

MR. SHIRLKAR: Where? You mean the part of the jet pumps?

DR. CATTON: Out of the bottom of the jet pumps.

We maybe -- That could be a detail that should be gone into at a later time.

MR. SHIRLKAR: I don't have the number off the top of my head.

DR. CATTON: I just want to raise the question at this point.

MR. SHIRALKAR: It's a good question. I think the answer is at least for a LOCA, the region was saturated fairly rapidly and the flow details to the five percent level say at about five or ten seconds and during thattime, I think we have enough flow coming through here to keep the thing fairly churned up.

DR. CATTON: The next time we discuss some of these details of your modeling, I would like to see numbers associated with that and the rationale for assuming that that huge lower plenum is completed mixed and why you can get away with --

DR. PLESSET: I think that's a good point. We can let it go for now.

(Slide.)

MR. SHIRLAKAR: For the calculation of steam slip flows, we use a conventional drift flux model and the drift flux parameters are correlated from steady state data. And at very low flow rates we have found that a transition to the Wilson bubble rise correlation is in better agreement with the data.

Because the lower plenum for example is a large region, what we've done is to assume or actually calculate a quasi study-wide profile within that region. It can be shown to be a function of these parameters.

And then the exit fluxes from the region are related to the exit void fraction. The slip velocity -- on the exit void fraction.

Now we also have a model in case to calculate a level to calculate entrainment if the momentum flux is larger than a certain value which is based on experiments.

(Slide.)

We've talked about counter current flow limiting or CCFL quite a bit and this happens to be one of the more important parameters in the BWR transient LOCA response in terms of determining inventories.

If I could show you the next chart that gives you an idea of the CCFL characteristics at the top of the bundle and the inlet.

(Slide.)

Without showing any numbers here, I realize, but the main message is that the CCFL at the top allows substantially more liquid than does the bottom for a given vapor flow rate. The one at the bottom is more restrictive and tends to accumulate water in the bundle.

DR. THEOFANOUS: Is this also a part of the effect of the geometry?

MR. SHIRLAKAR: The reason for that curve?

DR. THEOFANOUS: Yes.

MR. SHIRALKAR: It's mainly that area.

It's a much smaller whole than the area at the top.

DR. TIEN: So the basic correlation is still about the same, except the area. Is that what you just said?

MR. SHIRLAKAR: There are different values of the constant. The form is similar.

DR. TIEN: How about the constants. Do they vary a lot? You have K_1 and K_2. I can see K_1, you know would be quite different from the top CCFL and the side entries CCFL.

MR. SHIRLAKAR: They turn out to be not very different, but the primary impact is the area difference.

DR. TIEN: Again, you do not want to say the numbers.

MR. SHIRALKAR: I do not want to say the numbers.

DR. CATTON: Do you use an effective density in GESTR that includes entrainment?

MR. SHILAKAR: No, I do not.

DR. CATTON: What are your arguments for not? It's my understanding that you did.

MR.SHILAKAR: It might be a refinement. Normally when you have a large amount of entrainment coming up, the velocity is large enough so thatyou won't get any water coming down. There is a small region where maybe the droplets coming up are able to be entrained and still allow liquid coming down.

DR. CATTON: So you're saying that where the CCFL occurs you probably don't have any water in the steam anyway.

MR. SHIRALKAR: Very little, yes.

DR. CATTON: It's kind of weak, but understandable.

DR. SHROCK: Why do you want D to the ¼ power, multipling every term? Why don't we divide that out?

DR. CATTON: Symmetry.

MR. SHIRALKAR: We could divide it out here, but then you end up with a constant that involves the D to ¼ and this constant then has units of the diameter of the length of the ¼.

DR. SHROCK: As you've got it, it looks like you just straight forward divided out --

MR. SHIRLAKAR: No, if I multiplied it out -- you see I got rid of this D here.

DR. SHROCK: No.

MR. SHIRLAKAR: Yes.

DR. SHROCK: No, you have one on every turn there.

MR. SHIRLAKAR: I have D to the ¼ here. I multiply that guy with this.

DR. SHROCK: No, I'm just looking at the top line of the equation. It's probably a misprint, but you've got D to the ¼ on every turn.

MR. SHIRLAKAR: I think that's right.

The way we're using it, we're using it in the modified form which is not the -- the diameter is really -- it's not the classical Wallace form. It's what you call the modified Wallace in which the diameter drops out of this group.

DR. CATTON: I suspect if Wallace saw that D to the ¼ there, he'd get upset.

DR. TIEN: Not anymore. I think perhaps, I don't know, the reactor industry still keeps calling this Wallace correlation. I think they should just go and say this -- then there will be no confusion.

Just a side comment.

DR. THEOFANOUS: That's not the case. I think this is Wallace' correlation, but let's argue this another time.

I'm interested in going back to Dr. Shrock's point. There's a D to the ¼ there in that term in that equation. It seems to be if you've cancelled it out, you haven't changed anything. Something is wrong. Look at the first equation there. There's a D to ¼ in every turn.

Now you can't cancel it out?

DR. ZUDANS: Let's make sure that it's not a mistake.

DR. THEOFANOUS: It looks like you're telling us that there is something significant to the D to the ¼ in terms and we can't get the significance.

MR.SHIRALKAR: Something very significant. I can write the same thing as the AG' the ½ plus K_1, JF' to the ½ would equal the K_3. The K_3 now is going to dimensional. It's going to have the dimensions of the diameter of the ¼ power. So it's just a way of representing it. If you like I can represent it in this manner.

DR. TIEN: I would like to raise a question here. Really this is -- because you do not have the D dependents, but somehow you do not have -- it appears to me for some reason that you do not want to put the surface tension in there and that's why it has confused so many people.

MR. SHIRALKAR: You're absolutely right. We can easily go back to the KG' and KA'.

DR. TIEN: Can you tell me why you do not want to -- if you don't want to say that, that's another matter, but why you do not want to preserve the tension inside.

DR. CATTON: Or are K_1, K_2 functions of temperature? They may have built it into their correlation.

DR. TIEN: I agree, but you purposely may have --

DR. THEOFANOUS: May I say something to that?

The surface tension entered the picture when there is no characteristic dimension. Now depending on the size of the channel, it can be a range where the parameter is more important and there is another range where the Wallace parameter is more important and why I see there is the Wallace parameter where the diameter being the characteristic dimension.

DR. TIEN: But once you put a D ¼ power there, they cancel out. For the reactor geometry in this case here, and on the other hand you do not preserve the tension. You have some reasons, I'm pretty sure. That confuses a lot of people.

DR. SHROCK: Chang, is JG' is JF' are dimensional quantities and Kudatalotsy numbers are dimensionless. So you can't convert to Kudatalotsy by multiplying through by D to the ¼. You've done nothing to the equation by multiplying through by $D^{1/4}$.

MR. SHIRLAKAR: All we're trying to do is to relate it to the Wallace form.

DR. PLESSET: I think we'd better not pursue this anymore and put it off to the next meeting.

MR. SHIRLAKAR: The vapor flow -- This is just an application thing. The vapor flow that goes into the CCFL calculation is the vapor flow leaving the level adjusted for going to the level of operation above the level of any condensation.

(Slide.)

The break flow model is a conventional moody type choke flow model and we use homogenous flow model for realistic calculations. We can use Appendix K perscribed slip for model for sensivity studies.

The break flow enthalpy in case we have a -- we're going to give a mixing length if two different regions exist. For example, the level falls through a certain mixing length. The mixing length is typically the diameter of the -- the GESTR means the rating of the enthalpy to calculate the --.

(Slide.)

We do not have coupled momentum equations, but overall loop momentum euqations which traces the path through each bank of jet pumps and through the core and down through the downcomer.

The inital pump coastdown transient is approximated. This is where the LAMB would commence.

We solve simultaneously for the lower plenum mass/volume data.

DR. CATTON: When you do the sum of the pressure drops around the loop, don't you get an acceleration term on the mass flow?

MR. SHIRLAKAR: Yes.

DR. CATTON: How come it's equal to zero or is this for steady?

MR. SHIRLAKAR: That's included in an acceleration term.

DR. CATTON: I missed a beat here. When you go all the way around the loop with your sum and pressure drops, you wind up with a DM by DT. What happened to it?

MR.SHIRLAKAR: You mean GD, GT.

DR. CATTON: Well, GT, GD. I don't see it up there.

MR. SHIRLAKAR: Well, that's a component of the pressure drop. If I integrate all around the loop, the pressure across the loop is going to be zero.

It's solved simulataneously with the lower plenum mass and volume balance to calculate the flow splits in the lower plenum and once we have that we can integrate the mass energy grations sequentially through the core and upper plenum regions.

The flow conditions are typically determined by the mixture conditions in the donor region.

(Slide.)

So what we're doing is providing a momentum balance to historic point one and you work your way down through the downcomer to the suction through the lower plenum and up through the core. That's one loop.

The other loop is through the other bank of jet pumps, the broken side.

DR. CATTON: What happened to the bypass?

MR. SHIRLAKAR: The bypass region would be at the pressure balance between the bypass region and the core is solved separately and should be the same.

DR. CATTON: From the earlier description of the experiment that I saw, the bypass fed the core.

MR.SHIRLAKAR: It does.

DR. CATTON: So shouldn't it be here?

MR. SHIRLAKAR: We do solve it, but you see what we're doing is--

DR. CATTON: Am I at a different time in the sequence of things?

MR. SHIRLAKAR: No.

There is another loop between the core and the bypass.

DR. CATTON: I'm doing an electrical network and

I've got to include all paths and I'm not going to get the right answer for the curves. Maybe this can be discussed at the next meeting.

DR. WARD: I thought that to. There is suppose to be leakage in the bottom of the fuel channel from the bypass region, right?

DR. CATTON: It filled up the channel according to the experimental data we saw, because the lower plenum level was well below.

MR. SHIRALKAR: I think what you're saying is true and we do do that. It's not shown on this figure, but essentially what we're doing is we are solving between here and here and that should be shown to complete the drawing.

DR. PLESSET: It's just not on the figure.

DR. CATTON: The major path is left off.

MR. SHIRLAKAR: It's not the major path. It's the leakage path.

DR. CATTON: From what we saw earlier today, that core filled up from the bypass, not from the lower plenum or is this just steam flow or what?

MR. SHIRLAKAR: No, this is a total flow. It's a pressure drop equations.

DR. CATTON: Then I think that you've got to include that in your network.

MR. SHIRLAKAR: I agree. It's included and I'll show it.

DR. WARD: It wasn't shown in your earlier diagram.

As long as you say you include it in your model, okay.

MR.SHIRLAKAR: Yes, it is.

DR. WARD: It keeps showing up missing.

(Slide.)

MR. SHIRLAKAR: In fact that is the next subject that we had and that was the bypass leakage flow and we have a fairly detailed modeling of the leakage flow paths and the flow through the leakage paths. but I agree that that should have been shown together with the other one as part of the network.

(Slide.)

This is a drawing of what the geometry of the leakage paths looks like. This is shown in reverse flow when the bypass is in the mode of feeding the core.

The normal operating condition of course would be all reversed, because then the leakage is from the core to the bypass.

To give you an idea, this is a very fairly complex diagram. This is the fuel channel sitting on top of the bottom tie plate which sits on the fuel support casting which sits in the control dryer tube. And there are a number of leakage paths, but I would like to draw your attentionto just three which are the major ones.

There are holes in the lower tie plate and that provides about 45 percent of the total leakage flow.

DR. CATTON: Is that to scale? What is the width of the bypass channel on that diagram? Is it out somewhere near the E of channel or closer in?

MR. SHIRLAKAR: This is not to scale. Only the fuel part is --this part is to scale. The distance between the fuel bundles -- The gap between the channels is a 12 inch pinch and three-quarters of an inch.

DR. CATTON: So it's somewhere down near the head of the arrow off of chanel one.

MR. SHIRLAKAR: Yes, they're very close. The next channel would be here. This shows the core --

The leakage holes provide about 45 percent of the total leakage flow. You get another 25 percent through these finger spring leakage paths, as we call them, between the channel and the nose piece. So that's a total of about 70 percent and this path here between the bypass, the top of the control rod guide tube and the lower plenum provides about 18 percent.

So these are the three major contributors.

DR. CATTON: Of all of those passages from the bypass, the bundle, from the region below the bundle, which one of them do you think you've got a good handle on for CCFL? Those look like terribly torturous complex passages.

MR.SHIRLAKAR: Well, it turns out that most of the passages --

DR. CATTON: Which ones are most important?

MR. SHIRLAKAR: This one here.

DR. CATTON: That's fairly clean.

MR. SHIRLAKAR: That's two holes in the side -- drilled holes in the side of the tie plate and their facing upwards. So flow is coming in and going up through them. So I think being pretty confident, we do not have CCFL problem in this.

Flow is typically upwards through these paths, the weaker flow.

DR. TIEN: That major leakage of flow, what is the diameter of the hole you mentioned?

MR. SHIRLAKAR: It's a little over a quarter of an inch -- 9/16s or 5/16ths.

DR. SHROCK: Do the arrows indicate the direction of leakage in the normal plant operation or at some point in the transient accident situation?

MR. SHIRLAKAR: This is what we call reverse flow conditions when the bypass pressure drop is higher than before. So the bypass is feeding the core. It's all reversed in the normal flow conditions.

(Slide.)

The next one is the missing core of the network which shows the -- how the flows splits coming up and is tied into the other loop that I showed you before and this basically is modeling three major leakage paths.

One is through these tie plate holes. On e is this one between the lower plenum to the bypass region. That is the control rod guidetube to fuel the forecasting path as shown and this one is a small path which is from the lower plenum to the guidetube.

DR. CATTON: Under the circumstances where I've got primarily steam flow for WRC, does your code actually do a CCFL calculation for all three branches or four branches? What do you do? I would think that you would treat them separately, right?

MR. SHIRLAKAR: We actually perform a CCFL calculation only along the side entry orifice path.

DR. CATTON: Don't you have to check and see whether or not you're going to be -- the steam is going to go up into the bypass region from below as well? When you solve your network, don't you have to do that? You've kind of got like a -- strange kind of nominy or resistance that you've got there.

MR. SHIRLAKAR: I think what we do is when the bypass region does not have an inventory in it, then it's basically a steam split in all directions. But when the bypass region accumulates water, we assume that the steam will not force its way up that region. It would rather go up the open bundle region. These are much smaller paths as compared to the bundle.

DR. CATTON: Don't you need to do the calculation rather than assume?

MR. SHIRLAKAR: I think it's a realistic assumption. You can have it on one side a cold region that is allowing steam flow to go. On the other side, you have a region full of liquid. You're not going to force your steam up in the liquid filled region.

DR. CATTON: I just have the feeling that something that is complex as the geometry that you've got needs a little more attention than an engineering judgment or an assumption and again this probably something that we should continue later.

DR. TIEN: I got lost. Ivan's question can be very easily clearly answered. In those three leakage holes, the characteristic length and size -- the side hole is largest. So really if you get CCFL -- that's your question and before the large hole got CCFL, the others are plugged. But on the other hand, you don't worry about the other smaller ones, so you're really only talking about this large one.

If that's correct. Maybe I'm wrong. I just got a little bit confused by your exchange.

MR. SHIRLAKAR: What we have done, I should say, is look at the pressure differentials across these paths and the pressure differentials are such of the order of PSI, because of the static accumulation in the bypass region that you could not support counter current flow through it.

DR. CATTON: So you essentially have enough of the head in the bypass region that you're going to drive the water through.

MR. SHIRLAKAR: Right.

DR. CATTON: So it's really solid water.

MR.SHIRLAKAR: Yes.

((Slide.)

The upper plenum region as you see from the geometry is divided.

(Slide.)

If I look down upon the upper plenum it looks something like this and in answer to your question previously, we have a large number of bundles. These are controlled rod positions and when I talk about the bypass region its' divided into two regions normally.

There's a region inbetween bundles which is a fairly small gaps and there's a region around the outside of the core geometry which we call the peripheral bypass.

So we try to distribute the water in he upper plenum to these regions. The bundles, the -- bypass and the peripheral bypass based on the CCFL characteristics typically when the flow is going down.

DR. CATTON: You do something to accont for the larger amount of subcooling near the edges for the peripheral bypass?

MR.SHIRLAKAR: No,we're going an average energy balance.

DR. CATTON: But you're treating the peripheral bypass different than you treat the --

MR. SHIRLAKAR: Only for a CCFL calcuation.

DR. CATTON: But you don't account for the temperature difference. It's observed.

MR. SHIRLAKAR: We do not. We do calculate CCFL breakdown when the entire upper plenum is subcooling the average which takes some seconds longer than subcooling just the peripheral region.

(Slide.)

The flow to each region -- flow available is based on the pressure of the upper plenum with static head.

The plenum is empty then and the core spray distribution wouldn't become of some importance.

Based on data that we have, we have a fairly large CCFL coefficient for the bypass. And in experiments performed at length, we've found that we could not obtain

CCFL at the top or the bypass for steam flow rates well above the range that you would normally expect. So you just have very very large CCFL at the top of the bypass for the BWR.

WE do have a model for condensation efficiency of the core spray. If we define a mixing length typically of the order of a fuel nozzle diameters, if the level -- the two phase mixture level is below this mixing length, then we assume that the core spray coming in will be totally saturated by the time that it gets to this mixture level.

At or above the mixture level, we assume that it's just added to the water and mixed the mixture. And inbetween, we just mirror the efficiency minimally.

And what this does in fact is that it does exhibit the effects that was shown this morning. That is when the level drops below the sparger, the upper plenum will saturate and CCFL will be established. If the water level builds up above the spargers, the spray coming in is able to rapidly subcool the upper plenum.

(Slide.)

The high power assembly calculation, the core delta P is imposed on the hot channel. However, there is no feedback from the hot channel in the system. It's treated as a separate calculation. The hot channel is driven by the core delta P.

The inlet flow is consistent with lower plenum conditions. We do a separate calculation of inventory and the heat up and the liquid downflow to the hot channel is based on the upper plenum inventory and spray distribution.

(Slide.)

The external flows deal mainly with the water make up systems. We have flexibility of location and restarts primarily aimed at looking at operator actions for starts and stops.

We have the simulation of the safety relief valves and the ADS function with different groups of different set points to assimilate the pressure response better.

The main steam line flow is calculated before MSIV closure. It's calculated based on a simple turbine admission valve simulation and after that MSIV closure of course is shut off to zero.

(Slide.)

Let me move on to the heat transfer models.

The fuel and cladding models account for the peaking factors, the axial peaking factors, the radial distribution of power within the pellet and gap conductance model that is initialized from the GESTR calculation.

For the vessel an internals --

DR. THEOFANOUS: How much radial distribution is there within the pellet? You show radial distribution of power within a pellet?

MR. SHIRLAKAR: Yeah.

DR. THEOFANOUS: How much of a distribution is there. What is the average from one end to the other? What's the difference.

MR. POTTS: The center relative to the surface is depressed and it is usually depressed on the order of 20 to 30 percent.

MR. SHIRLAKAR: The vessel and internals are modeled with heat slabs which I'll show you in a minute and in the heat source distribution, we account for the decay heat within the fuel pellets. We account for the bundle and rod peaking. We account for gamma smearing which accounts for rod to rod smoothing of local heating factors as well as accounts for some redistribution of energy from the pellet to the cladding and to the water.

DR. CATTON: So you do a little gamma heating of the water.

MR. SHIRLAKAR: A small amount.

And for the model of the metal-water reaction, it's a conventional model and you can use for example the Bakergest coefficients or you can use the coefficient that was developed out of the EPRI program.

The gap conductance is initialized by the GESTR in the detailed steady state model. What we get from GESTR are the fission gas quantities, gap conductance and gap size as a function of kilowatts per foot and exposure.

The dynamic gap conductance calculation is performed during the transient and accounts for changes in internal pressure following perfect gas laws and the gap and changes in the gap and the fuel clad temperatures.

(Slide.)

The core heat slab representation consists of two types of rods modeled in both the high power and the average power bundles. The average power rod is used to calculate the heat flux to the fluid. The highest power rod is used to calculating the peak cladding temperature.

And the heated region of each rod is divided into five actual segments plus the two unheated segments and there is a one dimensional radial conduction calculation with radial noding within the pellet and the cladding.

DR. CATTON: Gee, with the size of the code that you've got, couldn't you take -- ten radial fuel loads is probably twice as many as you need. Couldn't you take those extra nodes and stick them into a couple of extra bundles and do a parallel bundle calculation.

You don't have to answer that.

(Slide.)

This shows the heat slabs that are used. There are four in the vessel wall. It shows the one, two, three and four.

There are heat slabs of various internals. The dryers, the separate upper plenum, the core shroud wall, the nine is the fuel channel. Eight is the control rod guide tube and ten is the control blade.

(Slide.)

The heat transfer coefficients that I used for nuclear boiling. We're very insensitive of value exactly used, so we used a simplified model of a constant value for the nuclear boiling heat transfer coefficient ramped off at very high void fractions.

So film boiling we used Gougall-Rohsenow or modified Bromley correlations. Modified Bromley is only used in what we calculate to be the liquid continuous region below the level.

For transition boiling, we perform an interpolation between nucleate boiling and film boiling and the interpolation is based on the delta T critical heat flux and delta T minimum which comes out of an Allogi correlation

The steam cooling calculation is made using the Dittus-Boelter correlation and accounting for steam super heat as the steam passes up the core.

We have a model to account for the droplet heat transfer the falling liquid from above to he function of liquid downflow, steam inflow and he steam pressure level.

For radiative heat transfer, we have a very simplified conservative model which is why if the radiation were to become important, we'd have to switch to a model like CHASTE.

DR. CATTON: Do you know the emvissivity of oxidized zirk?

MR. SHIRLAKAR: We have good estimates.

DR. CATTON: Have you measured it?

MR. SHIRLAKAR: Yes.

It has been measured and after it's been through a few heatups and we have an oxide film on it, I think the value of .7 is a very reasonable value.

DR. CATTON: .7 seems to be okay for everything.

We ran a steam through a bundle that had zirk in it and you can watch it change from nice shiny to gray and grayer and until pieces start to fall off and I just can't imagine that you can get away with using a fixed value. It somehow the emvissivity would be time dependent. It may not be important. I just mentioned it.

MR. SHIRLAKAR: I think it's time dependent in the early training for sure. We have nice clean rods, but I think that once they've been through a few temperature transients, the effect is much smaller.

DR. CATTON: To the eye they continue to change. I don't know how long that goes on and the infrared and what ever is important, it may not matter.

MR. SHIRLAKAR: It may depend on how high a temperature you take it to and how long, I guess.

DR. TIEN: I would like to come back to that position on boiling and steam cooling. You mentioned before that that it's very important in terms of the right estimate of heat transfer and you find your interpolation and also Dittus-Boilter are actually good enough.

MR. SHIRLAKAR: I'm sorry, Chan. Are you talking about transition now or steam cooling?

DR. TIEN: I talk both. Either one.

First you have a transitional boiling that you use a -- critical and also a minimum with Allogis interpolation formula. That you find the best so far as you can get estimate.

MR. SHIRLAKAR: If you look at the literature you'll find a lot of disagreement transition boiling.

I think the interpolation, the Allogi T minimum is about as representative as the data that you can find, because the Allogi did discuss for typical BWR pressures and flowdown qualities.

The interpolation --

DR. CATTON: In order to do proper interpolation, don't you have to correct the film boiling for void?

MR. SHIRLAKAR: I don't think so. Well, the film boiling coefficient is the function of flow conditions.

It's the function of the Dougall-Rohsenow is basically a Dittus-Boilter type correlation with mixture of velocity.

DR. CATTON: That's right, but I would have thought that you maybe would have used some of the fancy new correlations that came out of the Oakridge studies or are they not applicable here for some reason?

MR. SHIRLAKAR: I don't think we know enough about them yet.

DR. CATTON: You might take a look at them. They've been available for a year or so.

If you don't get the film boiling right, you're not going to get the intersection properly for transition boiling.

MR. SHIRLAKAR: Yes.

DR. CATTON: Then your turnaround would be wrong.

DR. TIEN: In the steam cooling is Dittus-Boelter as good as others? That has been around for a long long time.

MR. SHILAKAR: We've tried Dittus-Boelter. We've tried the Hynaman correlation for steam and they look pretty similar except for very small LODs.

DR. CATTON: I think Oakridge also has some corrections to Dittus-Boelter to account for property variations and things like that that you might also take a look at.

MR. SHARLAKAR: Okay.

DR. CATTON: Gee, as a matter of fact, the Oakridge people are here. You might even ask them for the report.

DR. PLESSET: Let's go on.

(Slide.)

MR. SHIRLAKAR: This is a chart for the heat transfer logic that we go through.

If we are about a mixture level or a very high void fraction, we assume that it's steam droplet cooling. IF not,we do check on a critical quality. If that is satisfied, then we check on the delta T minimum and if that is satisfied then we check on the delta T CHF.

So we fall into nucleate boiling or film boiling along this path or inbetween delta T minimum and delta T CHF, we go to transition one. We start checking for is the temperature low enough to start get into transition boiling and is there enough water available to wet the surface if the temperature is low enough.

DR. CATTON: That means that the way you've put this together, you have to specify a delta T min.

MR. SHIRLAKAR: Yes.

DR. CATTON: Isn't it more appropriate to just intersect heat flux curves and let them fall where they may? I think you'll find that that is not only easier, but it's more accurate.

MR. SHIRLAKAR: I'm not sure that we have good enough transition boiling to dissect it.

DR. CATTON: Well, you have a correlation for film boiling, good bad or indifferent and a correlation for transition boiling --

MR. SHIRLAKAR: No, I don't.

The transition boiling regime is an interpolation between delta T minimum on the film boiling curve and the nucleate boiling curve with delta CHF.

DR. CATTON: I'm just a little bothered by the delta T min because if you start looking around at the data it ranges a tremendous amount, butif you start -- if you take the transition boiling correlation like Su's and maybe correct it for whatever circumstances you have and you take reasonably good film boiling correlations and you intersect them, you'll find that there is a lot more consistency in your results.

MR. SHIRLAKAR: We've considered it, but I've been unimpressed with the transition boiling correlations also.

DR. CATTON: Okay.

(Slide.)

MR. SHIRLAKAR: The fuel rod model is initialized by GESTR in terms of the stored energy, the fission gas quantity and gap size and I'm repeating something that I said earlier here. The dynamic gap conductance calculation accounts for a change in internal pressure from a perfect gas calculation, change in gap. We do calculate a very simplified cladding stress and we check that against a perforation stress to the function of temperature.

(Slide.)

That's what I had in the way of models.

I'd like to leave you with a couple of comparisons. We're going to talk more about the assessment results tomorrow, but I would like to show you to compare and to close our this segment.

One compares todays evaluation model SAFER and TRAC with the TLTA data. The other one compares TRAC, SAFER and the evaluation model for the BWR large recirculation line break.

(Slide.)

The first one is a TLTA comparison. This is the test apparatus that we talked about this morning. This is an average bundle. Average ECC test case for a big recirculation line suction break.

The experimental data is this dashed curve over here. YOu can see the temperature rises to maybe a maximum for 7 to 800 degrees and then you basically get falling film type of rewet. Falling liquid rewetting from above which turns the temperature around and settles out around saturation temperature.

The TRAC calculation done with B01. This is our older version. It can perdict loads to the high end of the data, follow the heat uprate very closely. Did not catch the downflowing rewet -- liquid flow rewet and eventually turned around and quenched when the bundle filled up.

The SAFER calculation showed a dryout slightly earlier and therefore a slightly higher heatup, but the slow peer is very comparable to both the data and TRAC calculation.

Again SAFER does not have a good enough model to catch the rewetting coming from above and it keeps going until it reflushed the bundle somewhat later.

In comparison, you can look at what the current evaluation model would do. It would get an earlier dry out and never come back. It keeps going. Poor heat transfer and poor inventory ends up at about 2000 degrees,

DR. TIEN: In this case where you compare the TRAC and SAFER with the data, is that the delay is due to primarily the inadequacy of the rewet - the liquid film rewet or liquid downflow rewet.

MR. SHIRLAKAR: Yes. It's the top down phenomena where we're getting some liquid coming down and we're also getting a distribution of liquid coming down within the bundle.

DR. TIEN: If you look deeper, doe that mean actual TRAC hold did not have perhaps far enough node for the fuel rod so that they cannot catch that the rewet?

MR. SHIRLAKAR: I don't believe so. I think there have been so improvements made of the heat transfer models and tomorrow there is a session to look at some of the TRAC BO2 comparisons with TLTA and we can get into that more.

I just want to give you a glimpse of what we're going to be talking about tomorrow in some detail about the assessment part of it.

(Slide.)

The second calculation I have is what I call a typical comparison of large breaks. I would of like to have had a comparison on an identical basis. These are not on an identical basis.

Following a recirculation line break, you can see that the peak clad temperature predicted by current licensing model is around 2000 degrees fahrenheit.

For a comparable calculation, SAFER calculated a temperature of about 1000 degrees.

DR. PLESSET: What's the single failure that's referred to there?

MR. SHIRLAKAR: HPCS failure.

For TRACE BO1, the only run we have to show you today for the big break is not exactly compatable with this one, because this is with all systems on. And also it's with slightly lower peak kilowatts per foot. This is probably a true best estimate in terms of all systems available also and here of course we end up -- the initial dryout happens to be the dryest temperature.

Now, we don't have a comparable case to show you at the present time, but we know it's going to end up somewhere fairly close to SAFER and inbetween these two and my best guess now is about 800 to 1000 degrees of peak clad temperature.

We'll show you some comparisons tomorrow for three different break sizes, but they're smaller breaks. We do not have yet in hand the results for the biggest break.

DR. SHROCK: Could I ask a question on your previous slide?

(Slide.)

Just pursuing Chang Tien's comment about the use of the Dittius-Boetler equation, because that must strike most everyone as being the most out of context use of the correlation that you can imagine. But I know you use it also in TRAC for the wall heat transfer to the steam in your two fluid model and then you have additional heat transfer between the entrained droplets and the superheated steam.

It looks to me like your SAFER is giving you only a part of that story with the same calculation. That may account for the fact that SAFER is giving you a higher temperature than the TRAC B01. But I've looked at this in the context of the BWR TRAC use of the Dittus-Boelter and I'm convinced that it is just chosen out of lack of anything good that's available and it seems to me most inappropriate.

It does seem to me that it's an area where we need to do something to get some better data on that heat transfer to the super heated steam. I think it's a subject that maybe could be discussed in greater depth when we have this fall up meeting.

MR. SHIRLAKAR: I would just like to make one comment though and that is in TRAC, we do account for the presence of droplets in the steam.

DR. SHROCK: I understand that.

MR. SHIRLAKAR: In the wall heat transfer and in the official heat transfer of course, but in the wall heat tranfer, we account for the presence of droplets near the wall accounting the thermal boundary layer.

There's a correlation that modifies the single phase steam calculation to account for that effect.

DR. SHROCK: You see, I don't know how you even evaluate Dittus-Boelter equation is giving you an answer that is any good or not when you have really no very good measurements of what the mix mean temperature is in any clean cut experiment and whenever I raise that question people tell me, oh, but the profiles are just pretty flat.

It's pretty flat. It's maybe not good enough.

(Slide.)

MR. SHIRLAKAR: That closes the segment of my presentation and I leave you with the conclusion that we have greatly improved models in SAFER GESTR compared to where we were in the evaluation model and the large reduction in the BWR LOCA PCTs is primarily due to two reasons. Improved inventory modeling and more realistic heat transfer.

DR. PLESSET: Well, thank you very much. You withstood many interruptions very patiently and we appreciate it and I think you're going to continue tomorrow. Sothat's all we have for you today.

Now, we're going to continue with quite another topic so you can be excused if you don't want to listen to it.

Without a break we'll go on with a topic that has to do with the comparison of nuclear heated rods and electrically heated rods and we have a presentation by I think first, Mr. Knight. Is that correct.

Let's take Mr. Craddick first if that's all right.

(Pause)

MR. CRADDICK: My name is William Craddick. I'm from Oakridge National Laboratory. I'll be describing investigations into the differences in thermal behavior of electric fuel rod simulators and nuclear fuel rods.

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(Slide.)

This work was done some while ago. It's somewhat old work. A few years old that was done as part of the PWR-BDHT separate effects program which was an NRC sponsored program that was being conducted at Oakridge.

I'm here at the specific request of Dr. Longsong Tong. Apparently Dr. Tong felt that bringing this information to the subcommittee's attention would be of value.

(Slide.)

The project lasted for a number of years and addressed a number of different topics and I'm only going to confine my remarks to a narrow segment of things that the project did.

(Slide.)

The project began analytical investigations of the differences in electric and nuclear rods in 1977. The work was first documented at the international symposium on fuel rod simulators which was held in 1980 and it was also been documented in several ORNL reports in the event that anyone wants to read about all this stuff in more detail.

(Slide.)

In the area of nuclear electric fuel rod comparison, the project addressed two objectives within that area. We needed to determine how power should be varied through time in an electric fuel rod simulator to best simulate nuclear rod behavior when we were conducting thermal hydraulic experiments using bundles of electric fuel rod simulators.

And then afterwards, we had to analyze the post test electric rod behavior to determine what we could infer about nuclear rod behavior from the data that we obtained.

To address these two objectives a code called PINSIM was written. PINSIM was essentially a one dimensional transient code that solves a one dimensional transient conduction code in radial geometry and the one dimension being -- cylindrical geometry and the one dimension being radial.

(Slide.)

The calculation of the variation of the electric power that your provide to the PIN for a particular test is very test specific and it's facility specific and for that reason, I'm not going to describe the results of that sort of thing, because I'm not sure that it's very relevant to the larger generic issue. I'm going to talk about the post test analysis that was done.

In post-test analysis, we were addressing the specific question, how would time to DNB of electric and nuclear rods compare if they were exposed to the same hydrodynamic environment. That is for purposes of considering an analytical question, we split up the comparison into first how do the rod behaviors compare with respect to time-to-DNB if they had the same hydrodynamic environment and then separately we addressed the question of was the hydrodynamic environment that we created in the experiment similar or representative of what might have occurred in an actual nuclear reactor during an accident.

(Slide.)

The following type of calculation was the one that we found. We tried different types of calculation and this was the type that we found gave us the most useful results.

First we calculated the electric surface heat flux and the surface temperature of the electric fuel rod simulator from the data. That is what actually came out of the experiment. And we also calculated a sink temperature. Now the sink temperature was just taken as the saturation temperature for the measured pressure through the experiment.

We then used that to compute a heat transfer coefficient. Just divided the flux by the delta T between the surface of the rod and the sink temperature. We then made the assumption that if you had a nuclear rod in the identical hydrodynamic environment, it would then see the same H and the same T_{sink}. That is we're assuming that the H that it would see is in fact determined by hydrodynamic conditions and therefore if you postulate those are the same, the H would be the same.]

Using that then, H and T_{sink} as a boundary condition, we supply that to a calculational model of the nuclear fuel rod and calculated what the nuclear flux, q''_n on the chart here -- calculated what the nuclear flux would have been through time.

We then made the further assumption that the electric heat flux would be equal to the critical heat flux at the moment of DNB. Not any other time, but at the moment when the electric rod underwent DNB, we assumed that it had just -- it's flux had just exceeded the critical heat flux and therefore at that point, that was a reasonable value to assume as an estimate of what the critical heat flux was.

We then prepared our calculated nuclear heat flux to the critical heat flux and it's from that last comparison that we draw conlcusions.

(Slide.)

In particular, if the calculated nuclear heat flux was less than the critical heat flux at the moment of DNB, then we assumed that the nuclear -- we concluded that the nuclear rod would have gone DNB later. It's flux was lower and it wouldn't have exceeded the critical heat flux yet.

Now without using some correlation for CHF, we don't know how much later, but it would have been later. And vice versa, if the nuclear flux had already -- was already higher than the critical heat flux at the moment of electric

DNB, then it would have experienced DNB sooner.

Thus we could determine whether or not the electric rod timed to DNB was or was not a lower bound for what the nuclear rod would have seen.

Now this chart is an attempt to illustrate what I'm talking about. What I've got plotted here is surface heat flux verses time for just some hypothetical case here. If we assume that the curve labeled PIN 1 was the electric -- actual electric flux from the experiment and what we're assuming is that at the moment it starts to drop off, the moment we have DNB -- and I might add that in our actual experiments, this was a much sharper drop than it was on this little sketch, but it clutters up the sketch if we do it sharper. At that moment, it had just exceeded the critical heat flux and that is drawn here as a dotted line, but we have no way of knowing what that is without using some sort of correlation or something except at that point in time.

At that point in time, we're assuming that it had just exceeded it.

If we then take the same heat transfer coefficient and sink temperature and apply that to the nuclear rod. If at that moment we're down here and then if I assume a generally downslopping shape for the CHF curve, I can't have reached it yet, so I would have to undergo DNB somewhat later.

So what we were trying to address is whether or not our electrical fuel rod simulator timed to DNB was or was not lower bound than what a nuclear rod would have seen had it been in a hydrodynamic environment.

That's the method that we used. We analyzed a couple of tests in detail. In one case we found that that particular test was in fact a lower bound. Oh, I might add that the calculation we did for the nuclear rods we did for various different gap sizes down to no gap with no contact resistance which is the lowest resistance that you could possible have, i.e. none -- up to a very wide gap. The idea being to see whether or not the gap size is going to influence it and in one of the calculations we found that no matter what we assumed for gap size, the nuclear PIN would have undergone DNB later and then on another one of our tests that we analyzed, we found that in fact the gap size, depending on what you assume for gap size, you could straddle the electric flux and therefore we couldn't in fact use that as any lower bound on the DNB or anything.

DR. SHROCK: Could you clarify why the nuclear PIN has a trace that is lower than the electrical one?

MR. CRADDICK: Well, I was hypothesizing a particular case that it was. Whether it would or not would depend on the solution to the conduction collision. In other words, if I could postulate that I'm going to imagine it in the same hydrodynamic environment. If I'm willing to assume that the heat transfer coefficient is determined by the hydrodynamic environment. In other words, I'm ignoring surface roughness, variations perhaps, surface containments and that sort of thing. In other words, I'm using essentially the same kind of thing that many correlations that we use in codes do. They jus t look at the fluid conditions and say, okay the H is this.

Well, I'm assuming that if I put it in the same environment, I'll see the same H. And then I just simply take my nuclear rod model and solve for whatever the heat flux would be. Sometimes it would come lower and sometimes it would turn out higher. And what it usually winds up depending on, the relationship between those two is what type of power variation was supplied to the electric PIN.

In the experiment that I described where we concluded that the nuclear flux would always have been lower, we put a lot of energy into that electric PIN in the experiment.

Oh, I might say now that in the nuclear rod, we assumed -- The other thing that I had to have to do in the other calculation is some assumption of what the power does which is generated internally in a nuclear PIN and for that we assumed an ANS type DK curve. Just more or less a standard point kinetics type DK curve for a double ended co -- break-type accident.

So the biggest determining factor on where the electric PIN winds up compared to the nuclear tends to be what you've done with the electric power.

Does that answer the question?

DR. SHROCK: No, but go ahead.

MR. CRADDICK: Okay, I'll try to clarify it later if we have a chance to talk.

(Slide.)

I'm not going to go into the specific calculational results for the two tests. I'm not sure that they're very relevant. The view graphs addressing those are in your handout and it's described in the paper that I believe you all have that we prepared describing what we've done.

Rather I'm going to go on and make some comments about what we would conclude about the general question of how well electric fuel rod simulators simulate nuclear fuel rod behavior.

This is a summary of what I'm about to say. We are doubtful that the benefits of construction electric rods closely simulate nuclear rod behavior is worth the cost.

Now, current electric rods do not in fact, at least by current rods meaning the fuel rod simulators that we have used, do not in fact simulate nuclear fuel rod behavior very well in terms of matching what a nuclear fuel rod would do and that would mean that you would have to develop some sort of new fuel rod simulator if you wanted to match nuclear rod behavior with a fuel rod simulator. You would have to design a new one and build it.

The new electric rods that you might design to simulate nuclear behavior would be expensive and difficult to implement and current electric rods we believe can be used in many cases to bound nuclear behavior.

DR. CATTON: They've already done that, haven't they at Carls--

MR. CRADDICK: Done what?

DR. CATTON: Build nuclear simulators.

MR. CRADDICK: They may have. I don't know.

DR. CATTON: They've done it and they've used them. I don't know what the expense was. I thought you knew. That's why you had expensive up there.

MR. CRADDICK: No, what I have expensive and I'm going to address each of these in turn on the viewgraph. I'm going to describe the ideas that we considered. We spent some time thinking about, could you do a better -- produce a better fuel rod simulator and the version that we came up with that you could do that is expensive and so my statement about expensive is that it's expensive to do the things that we thought of.

I don't know what they did and I don't know how much it cost. So I can't really talk about that, but I'll talk about each of these points a little bit here.

(Slide.)

Now the first thing I said was that the current electric rods cannot match fuel rod behavior and the reason for these is as follows:

First of all you can only match at most with an electric fuel rod simulator, the behavior of a nuclear fuel rod at one level only and the reason for that is that in our rods, at least, the actual electric power distribution is fixed and I mean the relative power distribution. And that is fixed because it is determined by how thick the resistive heater is inside the fuel rod simulator and that's fixed at fabrication.

So the relative shape of the power profile is in other words fixed and it can't vary in time or from experiment to experiment.

However, if you calculate what the electric power you need to supply to the electric rod to simulate a nuclear fuel rod behavior -- if you tackle it what that power is, that will be different at different levels and bundles. And since you can only pick one power transient to follow, that means that most one axial level can be simulated.

Furthermore, even at the one level matching that behavior -- the nuclear fuel rod behavior through time becomes difficult. Our calculation s of the electric power that we would have had to supply to our rods to match nuclear fuel rod behavior indicates that we would have had to have generators with an infinitesimal response time. That is that it could make drastic changes in power in virtually no time and we didn't have generators that could do that and furthermore, part of the time you'd have to figure out a way to get energy out of the rod from the insides. Because energy that you put in in an earlier time step to help match the nuclear rod behavior now has to come out and we didn't have any way to get it out of there. I could put it in there, but I can't get it back out.

So that the fuel rod simulators that we had could not in fact match nuclear fuel rod behavior. So then we spent a little time thinking about how one might build an electric fuel rod simulator that could match nuclear behavior.

(Slide.)

We considered two designs. One of these is a sheath heated PIN. The reason that is is that if you use the sheath itself as a heating element and in our fuel rod simulators we had -- the current ones that we used had internal heating elements. But if you use the sheath as a heating element, then the fact that you got rid of a lot of this thermal inertia between the heat source and the surface allows you to get away from some of these unrealistic power transients and to do a better job controlling the surface behavior of the pin and I'll talk a little bit about why we didn't pursue that.

The other option was to try to produce a pin that had stored energy and thermal conductivity that was closer to that of a nuclear pin in the first place. So we came up with a design for uranium oxide filled pin that would be heated with a platinum tungsten alloy material for the resistive heater and I'll talk a little bit about that in a second.

(Slide.)

The first option that I mentioned was the use of the sheath heated pin and there are several practical problems with the sheath heated design. First of all, if you have a thermal couple located against or on or in the sheath in order to measure your surface behavior, you wind up having that up at voltage because your sheath is up at voltage which means that you have to have isolation amplifiers on tbe thermal couple and those things have slow time responses. So that you would have trouble tracking the type of behavior that occurs during a blow down where things change rapidly. At least in the early stages of a blowdown.

Secondly, if you had a thermal couple inbedded in the sheath, you could perturb the heat generation, because now you've got the termal couple laying in the middle of something that was being used to generate heat and it's not going to generate heat. So you've essentially messed up what you were trying to measure and you were measuring things in an atypical spot because of the presence of the measuring instrument.

Another point is that if you then take your thermal couple behavior from wherever and try to calculate what actually went on right at the surface, that type of calculation is ill posed and ill conditioned in general, but it is not so ill posed and ill condition that you can't solve it. Unless you have a heat source between your thermal couple and the surface and if you put a heat source in here, it becomes so ill conditioned that we didn't see anyway to be able to solve that problem.

So we wouldn't have known how to take the thermal couple data and translate it into what went on in the surface.

And finally if you then took the thermal couple and say, well, you're going to put it on the outside of the sheath and tack it on the outside or something, then it's going to act like a cooling pin and again perturb the thing you're trying to measure.

So for all of those reasons and probably other ones we rejected the idea of using a sheath heated pin.

(Slide.)

Now, the uranium oxide filled rod responded much more like a nuclear rod than the existing THTF heaters.

The THTF stands for thermal hydraulic test facility and that's the facility in which we did all of these experiments. It gives you a much closer match of the internal thermal properties and because of that the needed power variation through time that you would have to supply to the pin to mimic nuclear rod behavior becomes much more attainable. You don't have to jerk the generators up and down so fast. You don't have to find ways to get energy out of the pin and it becomes an achievable power program or power variation.

However, this rod is expensive and this is Dr. Catton's remark about what do you mean by expensive. Well what I mean is that when we did an initial estimate on how much it would cost to produce pins with uranium oxide and in particular platinum at the heating element, we came up with numbers on the order of maybe in the range of $35,000 a pin. The fabrication cost and that's after you spend about 160 or $170,000 to create the facility and perfect the means for manufacturing the pin.

So we're talking about large dollar expenses. The large dollar expense in the parts is mainly due to the platinum.

DR. SHROCK: Well, it's true that they respond more like nuclear rod. It still is different in the sense that you don't have a distributed source.

MR. CRADDICK: That's right.

It is different.

DR. SHROCK: It remains a considerable amount of the problem that you still have with this. What do you do about that?

MR. CRADDICK: What we did -- we didn't try to resolve that, because when we got as far as the expense, we punted the idea.

Let me tell you what we did to look at the question. What we did is that we took a nuclear type power profile of something that you might expect for a nuclear pin. Put it into a nuclear pin model and calculate what might happen to some arbitrary set of boundary conditions. We just picked a set that we thought might represent a particular blowdown. You know, go along with nuclear boiling for awhile, CHF and all of this sort of thing.

Then we applied this same boundary conditions to a current fuel rod simulator and we found that it behaved nothing like a nuclear pin which we already know and then we supplied the same thing to this improved design and the surface flux and surface temperature transient that it underwent was much closer to a nuclear fuel rod, but it was still somewhat different and the fact that the power source wasn't distributed is what we suspected is probably the cause of the difference.

DR. EBERSOLE: Did you say that the principle cost was due to the cost of the platinum?

MR. CRADDICK: Yes, sir.

DR. EBERSOLE: Well, isn't it almost 100 percent salvagable?

MR. CRADDICK: That might be. I'm not myself a person who was personally involved with the disign of that pin, so I'm reporting the results of others and it may well be salvagable. So what it may represent is an initial investment that you can recover.

DR.TIEN: I would like to make a general comment following what Virgil Shrock just mentioned. I think if you want to really simulate a nuclear rod perfectly, it's -- you end up with a tremendous cost and still perhaps don't do the job. It really depends on what kind of experiment or what kind of phenomena that you would like to know.

If you have a limited scope, then you can find maybe an electrically simulated rod can actually do a very good job with relative cost. Of course we did have this rod, but they had electrically simulated rod, but they are manufacturing it in a very different setting. Like what you set up your certain criteria and for different types you can have different ones.

I think that should be really realized.

MR. CRADDICK: If I understand what you just said, we agree with it. That I think what you were just saying or one of the things that you were pointing out is that how well you need to match the behavior depends on what you want to know and I agree with that.

In fact, when we looked at this point, we got to say that this thing was expensive. We now say, what good will it do us and we concluded that it wasn't clear that it would do us that much good at all. In fact, that's the last point I had. It was that electric rod behavior can be made to bound nuclear rod behavior in some respects and I mean electric rod behavior with the electric rods that we have now.

We know that we can do it with time to DNB or we feel we can anyway using the type of methods that I just talked about a suitable supplies of power to the pins -- attainable powers. Because if you just want to bound the time to DNB for example, you can come up with power programs that are attainable and will drive the electric pin in effect to DNB earlier than the nuclear pin would go.

We suspect that you could do the same kind of bounding with other areas. I've listed here quench rate and I'm put a question mark after it, because we never really looked into quench rate, but when we sort of sit back and think about it, it seems to us that you ought to be able to do it there to if you put enough time into developing the right kind of pin and this sort of thing.

So in general, the point that I'm trying to make with this slide is that we feel like certainly in a lot of cases at least, you can bound nuclear fuel rod behavior with electric rods and I'm not sure that that isn't most of the benefit that you want to get anyway.

So that given that we had also come to the conclusion that getting better nuclear fuel rods is very expensive, the ones that would match better, we just didn't see that it was -- it didn't look to us like a profitable enterprise and therefore we didn't pursue it any farther. But in the general question, that's essentially the point that we had to offer and in the investigations that we did, we came to the conclusion that we didn't think that whatever benefits you would get from improved fuel rod simulators would be worth the cost. AT least not as far as the direction of improvement being make them act more like a nuclear rod.

Improvements in the direction of making them -- the fuel rod simulators able to give you information more accurately in terms of measurements correctly and that sort of stuff, that's probably well worthwhile, but not in the direction of just trying to make it more like a nuclear fuel rod.

(Slide.)

So, now I will quickly summarize this. The PWR-BDHT program which has since ended, but at the time we had investigated differences in electric and nuclear fuel

fuel rods, we did this with the PINSIM code. We came to the conclusion that time to DNB can be bounded by tests using electric fuel rod simulators and we're doubtful that the cost of developing what I call here more realistic electric fuel rods, it would be justified by the benefits that you get.

So I realize that I went through that rather rapidly, but we're pressed for time.

DR. PLESSET: That's fine, but before we call on another presentation by Mr.Knight, I would like to just make a comment that I don't think that -- I think that this misunderstood what the view of the ACRS was in this matter. The fact that it may be very very difficult to get an electrically heated rod that will simulate a nuclear rod under all circumstances, I would be perfectly willing to admit that and say so what. I would like for you to -- or not you, but for anyone to describe a facility which is a test facility which will simulate a full scale nuclear power plant. There is no such thing. We've got to use some judgment and make some analysis to get some value out of it.

This is what I had in mind and when I made this comment that it's very difficult to do experiments in nuclear fuel. It's expensive. It meant that you couldn't make very many tests and so on, it was misunderstood by some people in the NRC and they got excited and thought that they had better educate us.

They'llfind that I personally am very difficult to educate in this matter. So let's hear from Mr. Knight. He maybe able to give us more light on this matter.

MR. CRADDICK: While Mr. Knight is walking up, I'll just add to the last comment is that I think that what I was trying to communicate here if I was successful is essentially just what you said.

DR. PLESSET: Oh, good.

MR. CRADDICK: We came to exactly the same conclusions --

DR. PLESSET: I'm glad to hear that.

MR. CRADDICK: -- that using analysis of the type for example that we tried to do and this sort of stuff will work. You don't need to spend a lot of money in other directions.

DR. PLESSET: Very good.

(Pause)

MR. KNIGHT: I'm Thad Knight from Los Alamos and I was requested to give you a short pitch by Harold Sullivan of the research staff of the NRC.

I think the point of this discussion stems out of a session of the LOFT review group that was held back in October in Idaho Falls. There was an afternoon session at which there were a number of presentations that discussed electrical verses nuclear rods and what Harold asked me to do is to give you somewhat of a flavor of that discussion. Okay?

From this point, I developed my presentation for today on fairly short notice and it's based primarily on the presentation that I made then. I will try to incorporate a few of the comments that were made during that session.

(Slide.)

Basically, we consider power generation in the core to be a boundary condition. You can discuss various ways that you generate this boundary condition, but that's what it's really treated as.

The desposition of the energy into the fluid is affected by the heat transfer in the rod and by the heat transfer coefficients to the fluid. The heat transfer coefficients we think are common between the nuclear rod and the electrical rod.

There was some discussion at the meeting concerning whether or not this is in fact true. They thought that heat conduction and/or material properties within the rod might effect the external heat transfer coefficient.

I would maintain that the heat conduction problems and the material properties would effect only the surface temperature which would then feed into the heat transfer coefficient and I think our statement here is still applicable.

The electrical rod at the same surface temperature compared to a nuclear rod with the same surface temperature and the same fluid boundary conditions would have the same H unless you come up with a weird external surface.

DR. TIEN: I think it depends on the condition transient and so on. -- it could get worse.

DR. PLESSET: If you have a very rapidly varying temperature field.

DR. TIEN: Yes.

DR. PLESSET: He would admit that.

MR. KNIGHT: All I'm saying under steady state conditions. Okay? And that's basically what the heat transfer correlations are that we have. They're for steady state conditions.

(Slide.)

The nuclear fuel rods and the electrical fuel rods have different heat capacities and conductivities. This is basically other people have been saying today.

These parameters together with a distributed heat generation effect the amount and the location of the stored energy within the rod. If we assume that the rod configuration remains unchanged during the transient, then we think that you can handle these differences very handily in your normal conduction solution of the fuel rod.

From the center of the fuel rod out to the clad, we think you can handle these types of differences directly with the conduction solution. The hooker is the conductance of the gap and the version of the code that we're using now has and has had for a number of years a very simple dynamic gap conductance model that for LOFT appears to work very well.

DR. SHROCK: Excuse me, Thad. What do you mean by handling? You mean you can calculate what is happening in one rod. You can calculate what is happening in the other rod, but that isn't the issue, is it? The issue is whether rods behave differently in response to the imposed thermal hydraulics.

MR. KNIGHT: That's right, but if I can calculate the thermal hydraulic response with an electrical facility and the thermal hydraulic response of the nuclear facility, then I don't need great big data base of nuclear test. I can get by with a limited nuclear data base and do most of my test with electrical facilities that are normally cheaper and the point that I'm trying to get to is that I think that under the conditions that the rod is not reconfiguring itself, there is no ballooning or swelling or rupture of the clad, a very simple conduction model takes into account the differences -- the heat transfer differences between the nuclear rod and the electrical rod.

At that point, I don't rightly care.

Does that answer your question?

DR. SHROCK: Yes.

Essentially your view is that you want to use the experiment to qualify a code which you'll then use to make predictions about the full scale equipment. It's not important that the experiment was not a complete simulation of the event in the full scale equipment.

MR. KNIGHT: That's right. I think a code is necessary to extrapolate any experimental results to full scale if the results are taken in any other than full scale or full size facility.

Well, I breezed right across a note that I had here. I wanted to make a comment. One of the presentations that was made that afternoon in Idaho Falls dealt with comparing the reflood heat transfer coefficient correlation to some data for which it does a very good job in the same facility that the correlation is based on and then they compared it to some NRU tests which are nuclear powered separate effects reflood test that are conducted in Canada.

Once you take into account in the comparisons that the quench front is moving at different rates, then the heat transfer coefficient that comes out is applicable to either one. If you just straight forward apply the fleck correction to the NRU test, you've got a fairly bad comparison. But when you accounted for the differences in the quench front progression, which is part of the correlation, then the heat transfer coefficient that comes out is about the same and that supports what I said earlier that for a nuclear rod and an electrical rod, we think the external heat transfer coefficients are about the same.

(Slide.)

This just says that if you have cladding, swelling and rupture and fuel cracking that is a dynamic process then you need a more sophisticated model representation of the fuel rod and to date these have not been important in the LOFT test and I made a statement to that effect.

(Slide.)

A problem that seems to be associated with nuclear fuel rods is that generally the state of the fuel at the initiation of the transient is not well established. Therefore, the stored energy in the fuel is not well known and this may effect the peak cladding temperature and the ultimate quenching process.

This makes nuclear tests more difficult to analyze. You can get around it to a certain extent by using fuel codes to give you a good estimate of what the intial conditions are in the fuel, but in the experiments thus far at least the ones that I have looked at, this is not normally been the case.

(Slide.)

This is a figure that I pulled out from another presentation at Idaho and it forms I think the basis of having had that session that afternoon. It shows you in the dotted line the semiscale test S-06-3 heat clad temperature and then the solid line down at the bottom is the measured heat clad temperature for LOFT test L2-3.

S-06-3 and L2-3 are counterpart tests. They were designed to be identical and when they got around to performing both of them, there was a big difference in the measured fuel temperature, the cladding temperature.

And this is where the electrical rods verses nuclear rods problems associated with that question has come about and where I want to go on from here is to describe a little bit of what they did in response to this question and then show you that I think that the advanced codes can calculate both phenomena and that you don't really need to have the large nuclear data base to support your code assessment and your code applications.

(Slide.)

Just very briefly to show you that there are some thermal differences between a nuclear rod and an electrical rod, at least the semi-scale rod. This shows you the radial temperature profile through the rod. The dotted line is the LOFT profile and the solid line is semiscale and because of the differences in the thermal conductivity between the two, the center line temperatures are greatly different and this is part of the concern.

EG&G in operating semiscale recognized that this was a problem.

DR. SHROCK: Excuse me, is that nuclear LOFT --

MR. KNIGHT: Yes.

The LOFT facility is a nuclear reactor.

DR. SHROCK: It had electrical rods at one time.

MR. KNIGHT: No, it did not.]

It has always had a nuclear core or no core at all. They ran some isothermal tests without.

(Slide.)

Semiscale was the experiment facility that was built to support the design and construction --

DR. SHROCK: My point is that the difference between these may relate only partially to the difference in the conductivity of the materials. The other part of it is that of course semiscale has a different source distribution.

MR. KNIGHT: That's true, yes.

DR. SHROCK: That's probably the more important effect.

MR. KNIGHT: Actually not, I don't think.

The source in semiscale is located at this point. It's a coil of wire going up through the center of the rod. The LOFT power profile through here would rise very rapidly to a peak just inside the outside edge of the fuel pellet and then come down slowly. This is much the same thing that

GE was talking about earlier.

DR. SHROCK: That large isothermal region in the middle of the semiscale is because there is no heat transfer through it.

MR. KNIGHT: That's right. That's inside the heat source

(Slide.)

Semiscale in designing their experiments, their counterpart experiments for L2-3, L2-2 and all of them said we recognized that there are differences and we want to minimize those differences. What they did is they had a design temperature curve which is another story on where that came from, but basically they had decided that the solid black line would be -- excuse me -- the dash line is the termperature transient that they wanted to follow with their rod and then they backed out of power history which is this curve that gave them the solid line which was a reasonable approximation.

One of the things that I would like to point out is that the semiscale rod is at full power at about five seconds -- five to six seconds it's at full power and that is important a little bit later on.

(Slide.)

In fact, this is later on.

What I'm showing here are measured temperatures for semiscale and for LOFT. The counterparts and the LOFT rod quenched at about six seconds. Well, up until six seconds, the semiscale rod was still at full power. And the reason that they're running it full power is to try to account for differences in the stored energy.

That is not all of the story. There is some hydraulic differences between the two and I'll touch on that after the next slide.

(Slide.)

This is a fairly busy slide with four curves on it. There is the achieved semiscale temperature trace. The solid black line that is fairly constant at around 30 there. It is the actual power history that they use to drive the semiscale experiment. This dash line down here is the LOFT L2-3 response and then there's a dash power curve. Okay? That is a curve that they backed out that would have been necessary to achieve the LOFT temperature response and part of the problems with this curve are these periods of negative power.

The discussion at this time -- the way I remember it is that they were using the same fluid conditions for semi scale -- for both semiscale curves. The measured curve and their backed out power profile that gives them the LOFT temperature response.

In reading Sanjoy Banjari's report of that session, his recollection is at least in that report, he is claiming that they used the LOFT conditions and if they used the LOFT conditions, it's not fair to compare to this. Okay?

If they used the same -- If this curve makes sense if they used the same fluid conditions from the test and the extrapolation. You're still going to have, I think, this big spike of negative power, if you would, in order to achieve the rewet and part of that is in effect of the gap that is not represented in the semiscale rod.

(Slide.)

What I want to do over the next three slides is very rapidly if I can show you differences in the fluid conditions or at least in the mass flow entering the core.

The first slide here is for LOFT test L2-3. They're calculated numbers from the TRAC PD2/MOD1. The solid line is the calculated mass flow in the intact loop cold leg. The dash line is the calculated break flow for the broken loop cold leg.

The LOFT people claim that this overlap at about five seconds is very significant. What that says is that at about five seconds for a short period of time the intact loop cold leg flow exceeds the broken loop cold leg flow and that difference has to move into the downcomer possibly the lower plenum and into the core.

There's another aspect of it. During this period of time, the liquid inventory in the lower plenum is flashing and swelling up.

(Slide.)

That gets me to the calculated core inlet mass flow per LOFT for L2-3 and I want to ignore everything late in time. It has to do with the reflood process, but beginning at about four seconds, the core flow goes positive and it goes positive because of the lower plenum flashing. It's bigger in the long run than it might have been because of the cold leg -- the intact loop cold leg flow exceeds the broken loop cold leg flow and so conservation says it has to go somewhere.

But there's a big, very large, very long period of positive core flow. This is moving liquid into the core at this time. Okay? And I would like you to note that it goes up to about 80 percent of the steady state core flow.

(Slide.)

Because of differences between the LOFT facility and the semiscale facility, there's a difference in this core flow between LOFT and semiscale. Here the solid line is calculated by TRAC PD2. The dash line is the measurement.

There is a couple of spikes -- there are a couple of spikes in the measured core inlet mass flow. They are of short durations and low magnitude and if you look at the data, it's not clear that they are even real. The calculation does not show a return to positive core flow at all. So during the period of rewet and LOFT test L2-3, also L2-2, there's a positive core flow bringing liquid into the core and the semiscale counter part test, this positive core flow is modest if it exists at all and it's this difference in fluid conditions combined with the differences between the rods that gives you the rewet.

(Slide.)

Let me skip the next slide. All it is is a word slide saying that I'm going to show you results from L2-2, semiscale S)-6-3 and Loft test L2-3.

Let me just show you this one plot from semiscale test S)-6-3. It's a calculation with TRAC PD2 showing clad temperature verses time at the high powered zone for the high power rod.

If you go back and look at the details of the data and the calculation, the time to to CHF is calculated very well. It's somewhat washed out in this figure.

We think the time to CHF is within a tenth -- on the order of a tenth of a second. Anything less than that you can't really sort out from the data.

There is a difference at the peak. We're running about 100 calvin below the peak during blowdown. When we analyze this test and it's been almost two years now, our explanation for that difference had to do with the very high radial peaking of the semiscale core. The center four rods and a forty rod bundle were peaked 50 percent above the remaining rods around the outside.

In the TRAC code, wecan't put one channel around these four rods. It gives us -- the cell sizes are too small and the expense of the calculation becomes great. So we can't realistically do that. And we think that that's where the differences come from.

If I had gotten Longsong Tong's internal memo to the NRC earlier, I would have brought some additional semiscale calculations to show you that we can in fact calculate that blowdown peak temperature. WE did so very well in SO-2-8.

Following the blowdown peak, there's a period of cooling, a reflood peak, a cooling rate or cooling during reflood and then the quench. And we're quenching a little bit early. It's due to the fact that we just never quite got hot enough. The thing that I mentioned before when I showed this slide was that the code calculated all the trends right. The shapes of the curve were very good. If we just -- If we had had the extra 100 calvin here the two lines would probably be indistinguishable. And we think that this is a good comparison combined with the other semiscale and LOBI stuff that we've done.

We think that we can calculate electrical heated rods well.

(Slide.)

LOFT test L2-2 calculated with TRAC PD2 showing this -- this slide shows you cladding temperature as a function of time at about the 30 inch elevation in the center fuel module. It's the high powered zone.

What it's showing you is that we're calculating dryout to occur at about the right time. We're doing a good job calculating peak clad temperature. We calculate the initial rewet that occurs from the bottom up. The timing of this is off a little bit. There was some data shown at the meeting in Idaho Falls that would indicate that the external TCs may give you an early indication of quench by several seconds. This difference between the calculation and the data here is about on the order of what they showed. It maybe gratuitous, but I think it's good.

It shows you that we're calculating a subsequent dryout later in time and rewets and I've shown you several different thermal couple responses that show you what the code is giving you is somewhere in the middle of what is really happening.

(Slide.)

LOFT test L2-3 calculated with TRAC PD2/MOD1 the cladding temperature verses time. Solid line is the calculation. The dash lines are the measurements. Basically the same location as the plot for L2-2.

Again, we calculate time to dryout very well. And this is a dryout problem I think. I go in and look. I haven't looked at all of them, but the ones that I've looked at what I'm hitting in the code is a void fraction limit that forces me over into transition in film boiling as opposed to hitting the CHF limit out of the correlations. The void fraction is getting very high very rapidly and if I didn't hit that I would have hit the CHF limit a little bit later, but not much later.

And that applies to semiscale,too. That's why I don't think there's in these test the electrical and nuclear rods are going to give you a significantly different time to CHF.

The initial rewet is seen again in the data. The code calculates a substantial cooling, but the quench is not complete at this elevation. Elsewhere in the core it is complete. After the inital quench in the data, you get a heat up. The code matches and then the final quench and again we think that this is a good comparison.

(slide.)

The next plot that is in the package would show you that there is a basis for this dip in the calculated temperatures.

The last plot that I want to show you is basically the same calculation that is on the previous plot with the reported uncertainty bounds that LOFT has reported for that test and basically the solid line is the calculation. The lower dash line is the peak clad temperature that was measured. The upper bound is based on some assumptions of when CHF occurs, the cooling after CHf and facts resulting from the external PCs and stuff.

And the bottom line is that we followed the measurement much more closely than we do all of these other things that are based on things that may have resulted from the electrical rod. The gist of what I'm showing you is that the code can calculate the phenomena for the nuclear rod and the electrical rod assuming that the configuration of the rod doesn't change during the transient.

That's a big if.

(Slide.)

For the conclusions, we think that any of the problems associated with nuclear fuel rods can be resolved by adding a more detail fuel rod model to our code and by running a steady state -- excuse me.

We think that any problems associated with the nuclear fuel rods can be resolved by adding a more detailed fuel rod model to our code and by running a steady state fuel rod code to establish initial conditions. Separate effects, fuel tests should be sufficient to support co-development and assessment in this area. What I'm saying is that I don't think that we need to go off and generate a much larger integral system nuclear data base. I think that we have enough. It's been very valuable. I don't think that we want to throw it out and I don't think that we need to spend a lot more money generating a lot more of it.

For the LOFT test to date, the models that are currently in the code work very well. We see no reason to change them.

And that basically concludes what I wanted to say to you.

DR. PLESSET: Thank you very much, Mr. Knight for a nice presentation.

Any questions?

I guess you convinced us all.

MR. KNIGHT: I think that everybody here seems to be of the same mind about it.

DR. PLESSET: Yes.

MR. KNIGHT: The flavor at the meeting in Idaho there were a number of people principally associated with LOFT that wanted to see nuclear experiments continued and I can understand that.

DR. PLESSET: They had a lot of mathematicians and purists and things like that.

Thank you again and we'll recess until 8:30 tomorrow.

(Whereupon the meeting was recessed at 6:00 p.m.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

NUCLEAR REGULATORY COMMISSION - ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

in the matter of:

Date of Proceeding: Thursday, December 2, 1982

Docket Number: n/a

Place of Proceeding: San Jose, California

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Sheila Khalov

Official Reporter (Typed)

Sheila Khalov

Official Reporter (Signature)

J. Quirk

GENERAL ELECTRIC PRESENTATION
TO
ACRS ECCS SUBCOMMITTEE

REALISTIC BWR ECCS EVALUATION METHODOLOGY

DECEMBER 2-3, 1932
SAN JOSE, CALIFORNIA

MEETING AGENDA

DECEMBER 2, 1982

TOPIC	TIME	PRESENTOR
• Introduction	8:30	G.G. Sherwood/ J.F. Quirk
• GE ECCS Approach	8:45	J.E. Wood
• Overview of BWR LOCA Technology	9:15	G.E. Dix
• BREAK	10:20	---
• TRAC-BWR Model Description	10:30	J.G. Andersen
• LUNCH	12:00	---
• GESTR-LOCA Model Description and Qualification	1:15	G.A. Potts
• SAFER Model Description	1:45- 3:15	B.S. Shiralkar

DECEMBER 3, 1982

TOPIC	TIME	PRESENTOR
• Introduction	8:30	J.F. Quirk
• TRAC Qualification	8:45	M.D. Alamgir
• BREAK	10:15	---
• SAFER Qualification	10:25	B.S. Shiralkar
• LUNCH	12:00	---
• TRAC-BWR Calculation Results	1:15	B.S. Shiralkar
• ECCS Evaluation Methodology (Application Approach)	2:15	B.S. Shiralkar
• Decay Heat Exemption Status	2:45	D.K. Dennison
• Conclusion	3:15	J.F. Quirk

GE ECCS APPROACH
COMPLETED AND PLANNED ACTIVITIES

COMPLETED ACTIVITIES

☐ Presented Overall ECCS Approach to ACRS-ECCS Subcommittee	Aug. 1981
☐ Submitted SAFER/GESTR Realistic Model to NRC for Review	Dec. 1981
☐ Submitted GESSAR II Decay Heat Exemption Technical Basis to NRC for Review	Dec. 1981
☐ Met with NRC and Reached Agreement on SAFER/GESTR Application Approach	Jan. 1982
☐ Presented Decay Heat Exemption Technical Details to ACRS-ECCS Subcommittee	June 1982
☐ Presented SAFER Qualification Results and Application Plans to NRC	Aug. 1982

PLANNED ACTIVITIES

☐ Meet With NRC to Present Final SAFER Qualification and Application Results	Jan. 1983
☐ Obtain NRC Approval (SER) of SAFER/GESTR	Mar. 1983

MEETING PURPOSE

- ☐ PROVIDE TECHNICAL DESCRIPTIONS OF SAFER, GESTR-LOCA, AND TRAC-BWR ECCS MODELS
- ☐ PROVIDE ECCS MODEL QUALIFICATION AND ASSESSMENT RESULTS
- ☐ DESCRIBE ECCS EVALUATION METHODOLOGY
- ☐ OUTLINE CURRENT STATUS OF THE GESSAR II DECAY HEAT EXEMPTION

GE DECAY HEAT EXEMPTION PROPOSAL

☐ ALLOW AN EXEMPTION TO SECTION I.A.4 OF APPENDIX K AND REPLACE THE SPECIFIED 1971 ANS + 20% STANDARD WITH A GE CORRELATION BASED ON THE ANSI/ANS-5.1-1979 STANDARD

☐ LIMITATIONS

- Applicable for Appendix K Analysis
- Envelope Conditions
  - 14.4 kw/ft (Peak Pellet)
  - 57 kw/liter (Core Average)
  - 28 kw/kgu (Core Average)

☐ RESULT

- 200°F to 400°F PCT MARGIN

EXEMPTION STATUS

- TECHNICAL BASIS SUBMITTED (DEC 1981)

- TECHNICAL DETAILS PRESENTED TO ACRS (JUNE 1982)

- NRC GENERIC REVIEW COMPLETED - QUESTIONS ON EXEMPTION APPLICATION INFORMALLY SUBMITTED TO GE (JUNE 1982)

- NRC APPLICATION QUESTIONS WILL BE ANSWERED DURING FIRST PLANT SPECIFIC ANALYSIS

SUMMARY AND CONCLUSIONS

- TECHNOLOGY PROGRAMS HAVE CONFIRMED LARGE REALISTIC BWR LOCA MARGINS
- REALISTIC EVALUATION METHODS HAVE BEEN DEVELOPED
- REALISTIC METHODS ARE BEING QUALIFIED WITH EXPERIMENTAL DATA AND BENCHMARK ANALYSIS (TRAC)
- APPLICATION WILL ACCOUNT FOR MODELING BIASES, PLANT AND MODEL UNCERTAINTIES, AND APPENDIX K REQUIREMENTS
- ECCS APPROACH PROVIDES UTILITIES WITH OPTIONS FOR COST EFFECTIVE ALLOCATION OF RESOURCES
  - Decay Heat Exemption
  - Improved Evaluation Model

LOCA CAN BE ELIMINATED
AS A BWR OPERATIONAL RESTRICTION

J. Ed Wood
④

GE ECCS ANALYSIS APPROACH

- REVIEW
  - BWR SYSTEM
  - CURRENT LICENSING EVALUATION MODEL
  - LOCA/ECCS ISSUES

- LOCA/ECCS LICENSING ANALYSIS OBJECTIVES

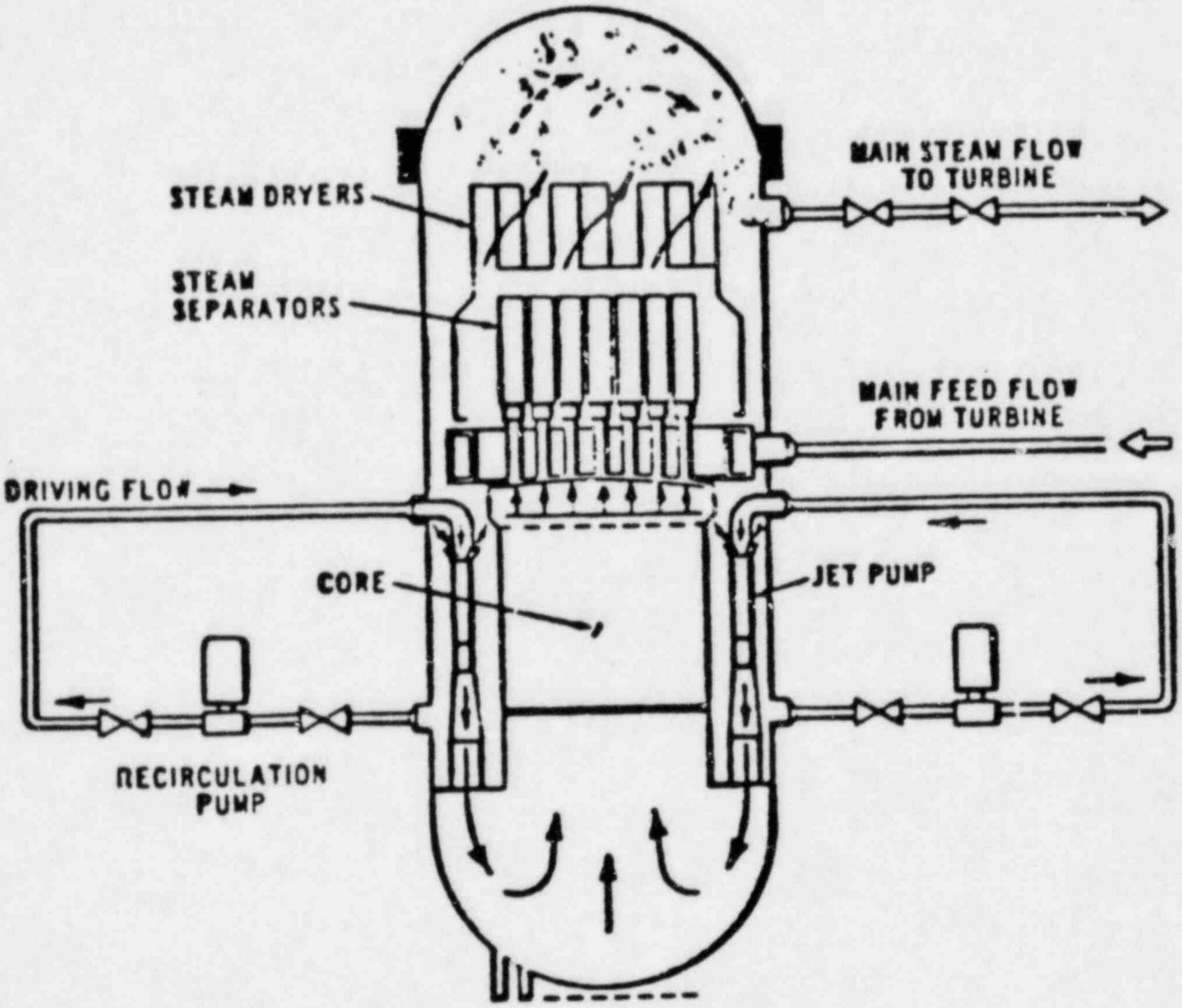
- NEW LICENSING EVALUATION MODEL BASES

JEW:12/2/82

BWR LOCA/ECCS SAFETY DESIGN FEATURES

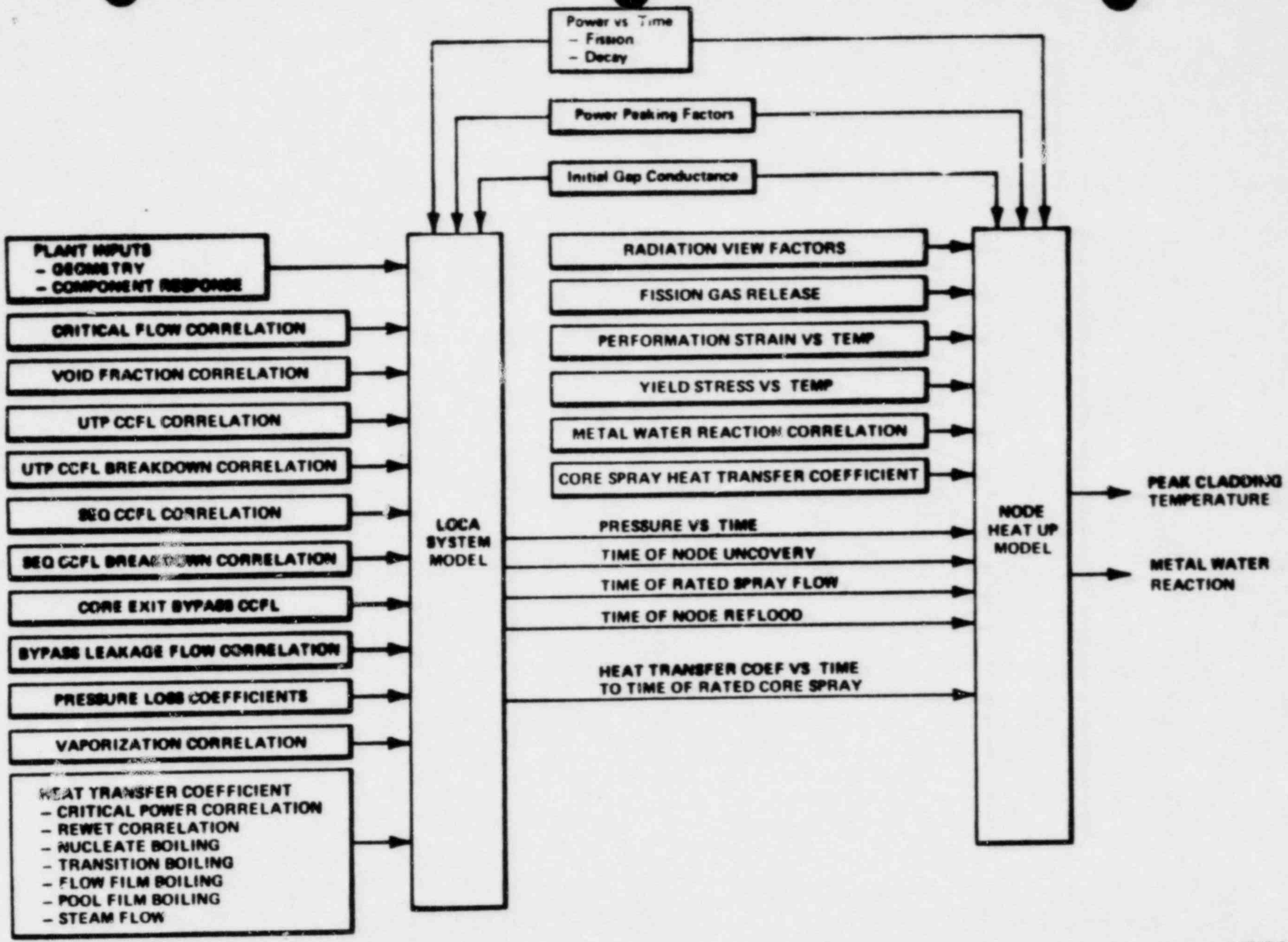
5

- LOW SPECIFIC POWER
- INTERNAL NATURAL CIRCULATION
- DUAL CORE SPRAY SYSTEM
- COOLANT INJECTION SYSTEM
- REFLOODABLE CORE



JET PUMP BWR DESIGNED FOR LARGE LOCA SAFETY MARGINS

JEW:12/2/82


Power vs Time
– Fission
– Decay
Power Peaking Factors
Initial Gap Conductance
PLANT INPUTS
– GEOMETRY
– COMPONENT RESPONSE
CRITICAL FLOW CORRELATION
VOID FRACTION CORRELATION
UTP CCFL CORRELATION
UTP CCFL BREAKDOWN CORRELATION
SEQ CCFL CORRELATION
SEQ CCFL BREAKDOWN CORRELATION
CORE EXIT BYPASS CCFL
BYPASS LEAKAGE FLOW CORRELATION
PRESSURE LOSS COEFFICIENTS
VAPORIZATION CORRELATION
HEAT TRANSFER COEFFICIENT
– CRITICAL POWER CORRELATION
– REWET CORRELATION
– NUCLEATE BOILING
– TRANSITION BOILING
– FLOW FILM BOILING
– POOL FILM BOILING
– STEAM FLOW
LOCA SYSTEM MODEL
RADIATION VIEW FACTORS
FISSION GAS RELEASE
PERFORMATION STRAIN VS TEMP
YIELD STRESS VS TEMP
METAL WATER REACTION CORRELATION
CORE SPRAY HEAT TRANSFER COEFFICIENT
PRESSURE VS TIME
TIME OF NODE UNCOVERY
TIME OF RATED SPRAY FLOW
TIME OF NODE REFLOOD
HEAT TRANSFER COEF VS TIME TO TIME OF RATED CORE SPRAY
NODE HEAT UP MODEL
PEAK CLADDING TEMPERATURE
METAL WATER REACTION
11713-3

JEW:12/2/82

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⑦

LOCA/ECCS ISSUES

- 1970 STATUS
  - BWR DESIGNED FOR HIGH SAFETY MARGIN
  - SIMPLE BOUNDING EVALUATION MODELS
  - 1200°F CALCULATED MARGIN

- 1975 STATUS
  - ALL CALCULATED MARGIN ELIMINATED
  - PENALIZED FUEL CYCLE ECONOMICS
  - MINOR PLANT DERATES
  - REALISTIC TECHNOLOGY NOT AVAILABLE
  - LARGE TEAM OF FIRE FIGHTERS REQUIRED

- 1982 STATUS
  - NO PLANT DERATES
  - PENALIZED FUEL CYCLE ECONOMICS
  - IMPROVED ANS DECAY HEAT MODEL
  - KEY PHENOMENA EXPERIMENTALLY INVESTIGATED
  - BEST ESTIMATE SYSTEM MODEL DEVELOPED
  - NEW LICENSING EVALUATION MODEL SUBMITTED

- CURRENT CHALLENGE/OPPORTUNITY
  - IMPLEMENT NEW LICENSING EVALUATION MODEL
  - QUANTIFY REAL BWR SAFETY MARGIN

JEW:12/2/82

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LOCA/ECCS LICENSING ANALYSIS OBJECTIVES

- ASSURE APPROPRIATE PLANT SAFETY
- PROVIDE BASIS FOR OPERATIONAL/DESIGN DECISIONS
- ALLOW EFFICIENT USE OF REGULATORY/INDUSTRY RESOURCES
- MINIMIZE PENALTIES ON POWER GENERATION COSTS

JEW:12/2/82

(9)

NEW LICENSING EVALUATION MODEL BASES

- USE PHYSICALLY CONSISTENT CONSERVATION MODELS
- INPUT EXPECTED VALUE CORRELATIONS
- COMPARE CALCULATED MARGIN WITH REASONABLE UNCERTAINTIES

JEW:12/2/82

Gary Dix ⑩

BWR SAFETY TECHNOLOGY REVIEW

G. E. DIX

DECEMBER 2, 1982

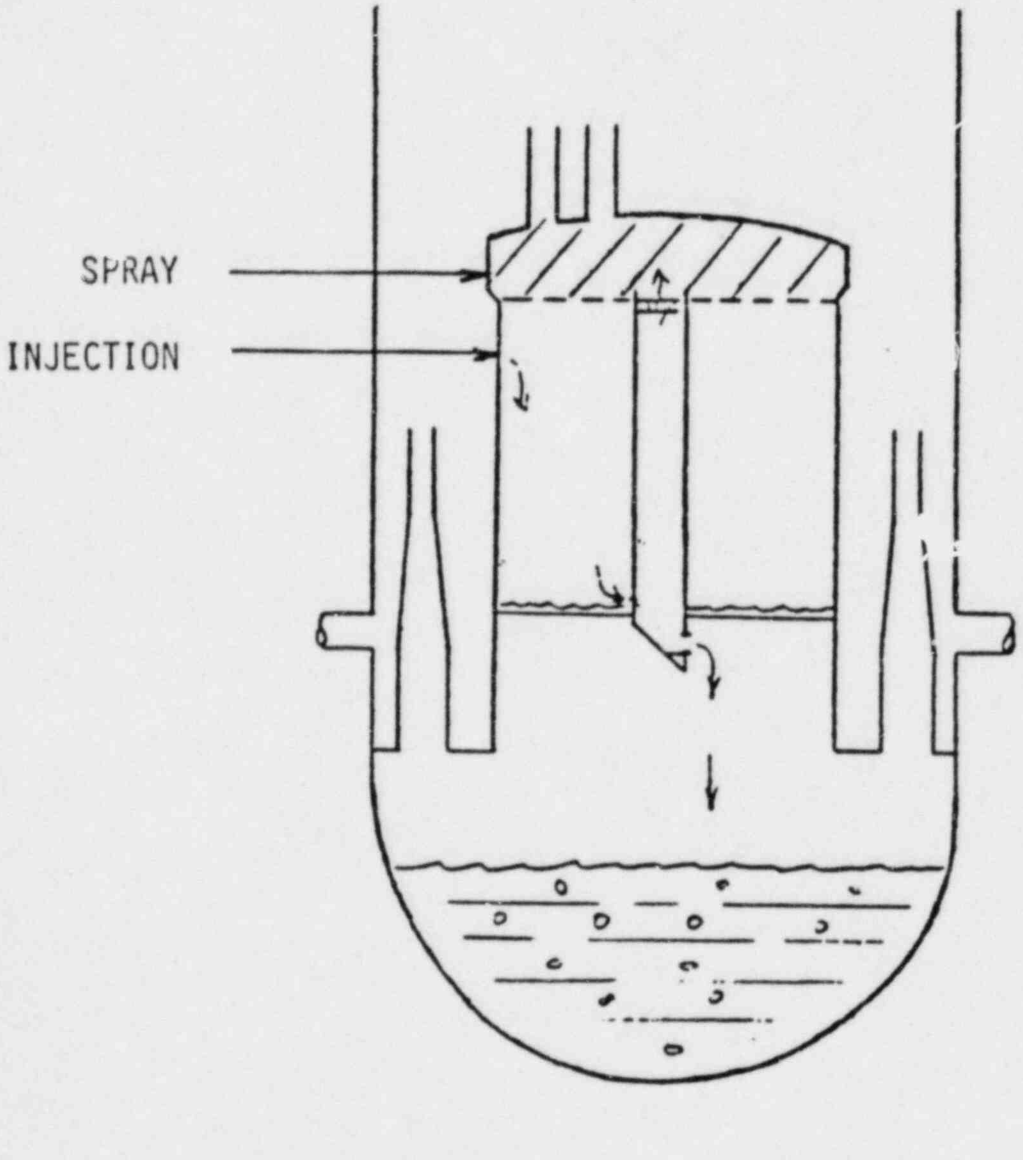
MAJOR BWR LOCA/ECCS SAFETY RESEARCH FACILITIES

- TWO LOOP TEST APPARATUS - TLTA (NRC/EPRI/GE)
- FULL INTEGRAL SIMULATION TEST - FIST (NRC/EPRI/GE)
- STEAM SECTOR TEST FACILITY - SSTF/LYNN (NRC/EPRI/GE)

- 18° SECTOR TEST FACILITY - (TOSHIBA)
- 60° SECTOR TEST FACILITY - (HITACHI)

- BWR FOUR BUNDLE LOOP - ROSA III (JAPAN)
- BWR TWO BUNDLE LOOP - TBL (HITACHI)

LICENSING MODEL

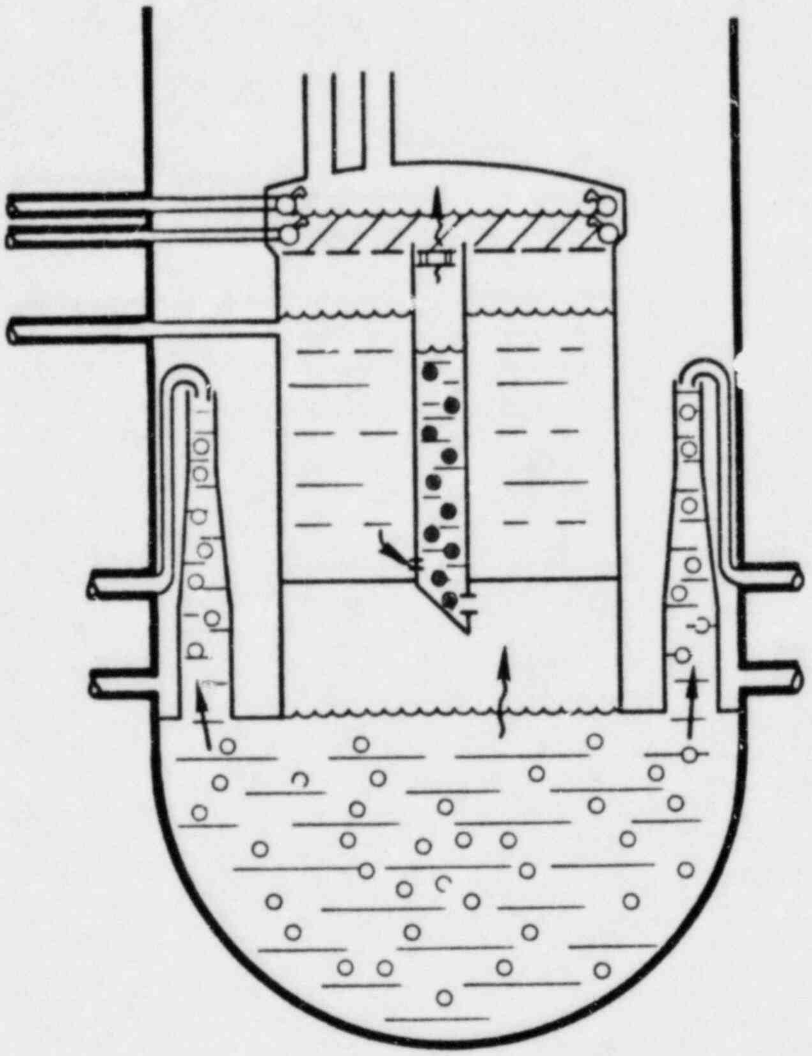


SINGLE CHANNEL EXPERIMENTS

- TWO TEST TYPES
  - SEPARATE-EFFECT HEAT TRANSFER
  - INTEGRAL SYSTEM RESPONSE

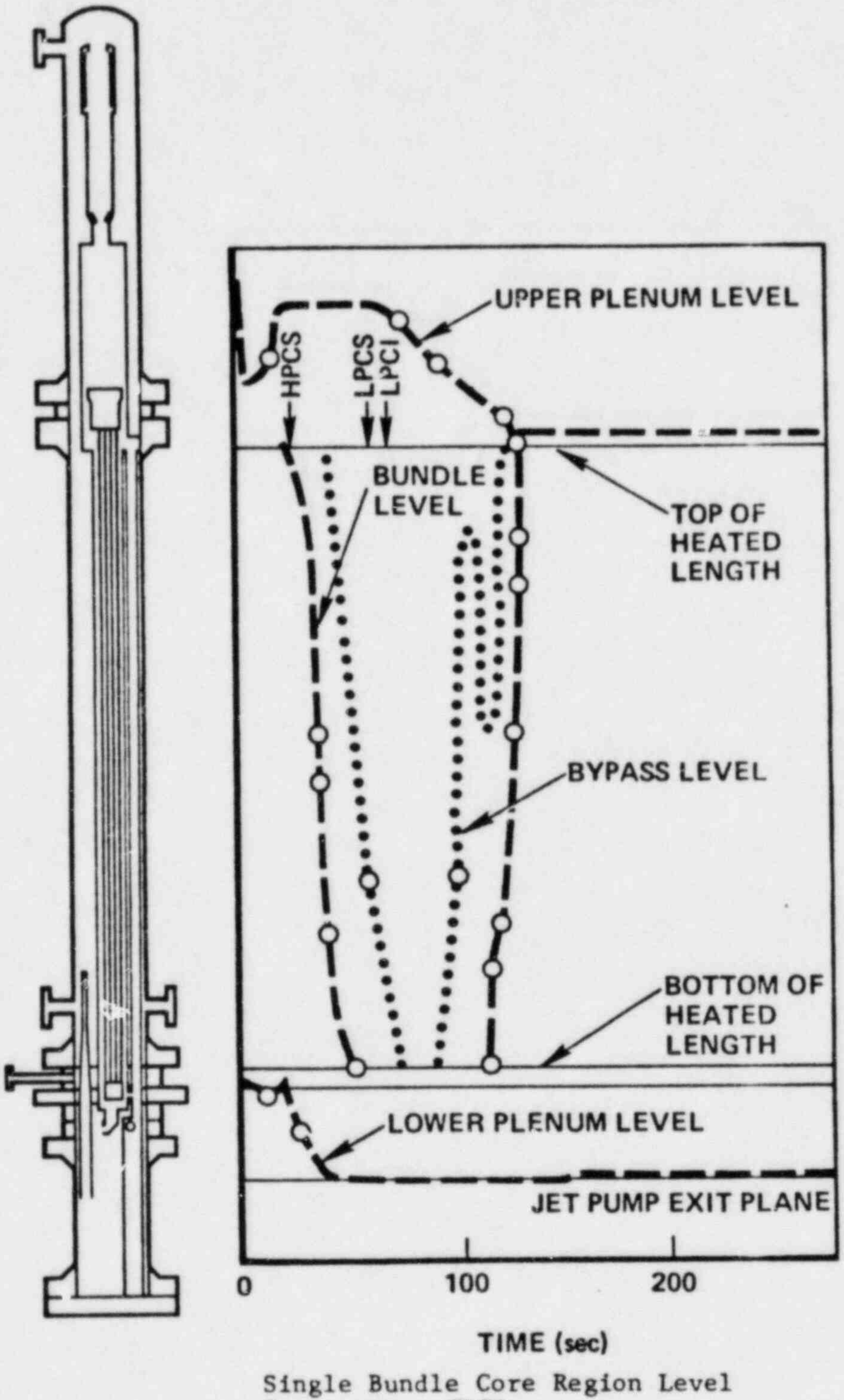
- LARGE MARGINS IDENTIFIED
  - CCFL FAVORABLE
  - VERY HIGH HEAT TRANSFER

(14)



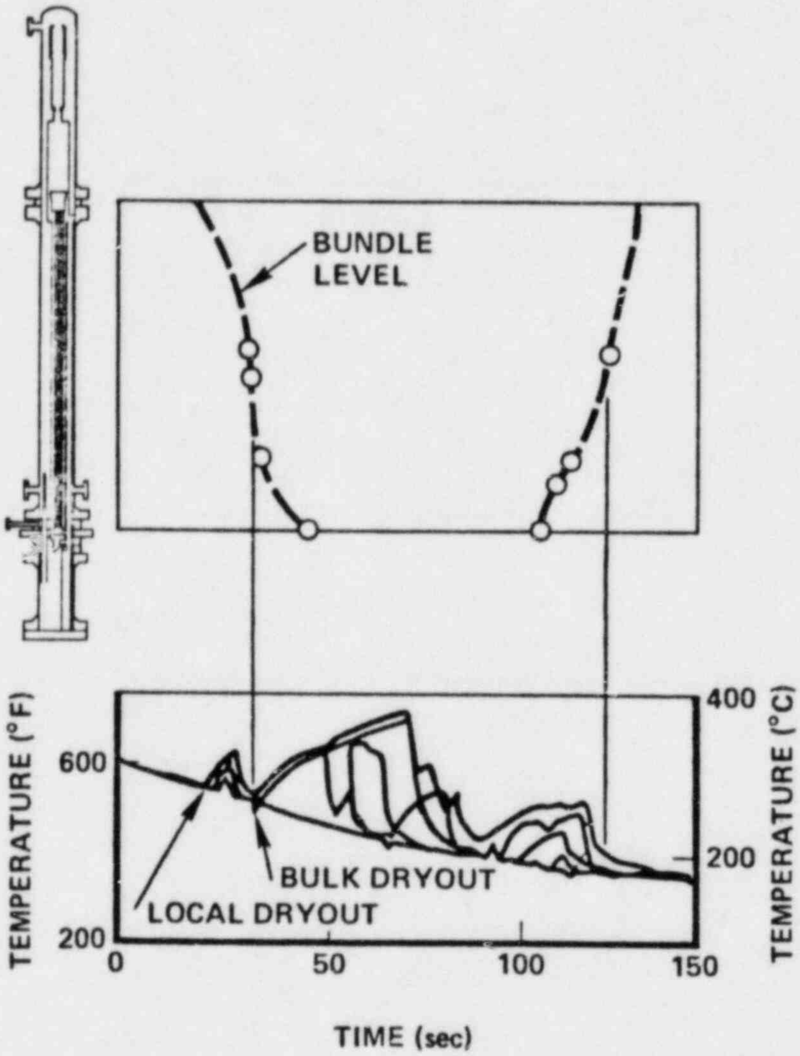
Typical Single Channel Conditions

(15)



Single Bundle Core Region Level Response -- TLTA

16



Core Region Response of
One Bundle -- TLTA

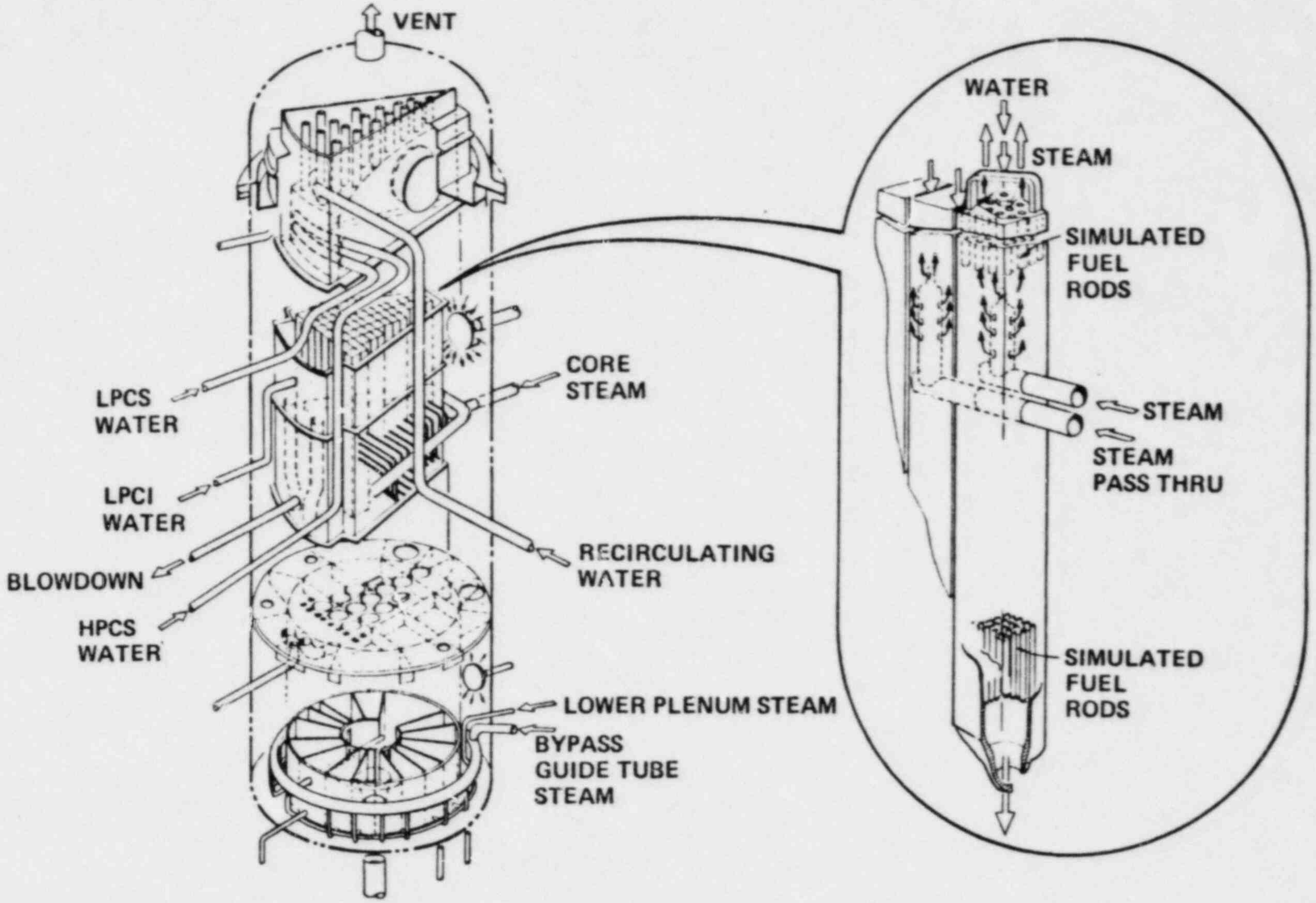
SINGLE CHANNEL HIGHLIGHTS

- INLET CCFL RETAINS LIQUID IN FUEL CHANNEL
- EFFECTIVE STEAM-DROPLET COOLING ABOVE LEVEL
- SIMILAR PHENOMENA FOR SMALL AND LARGE BREAKS
- LOW PEAK TEMPERATURES

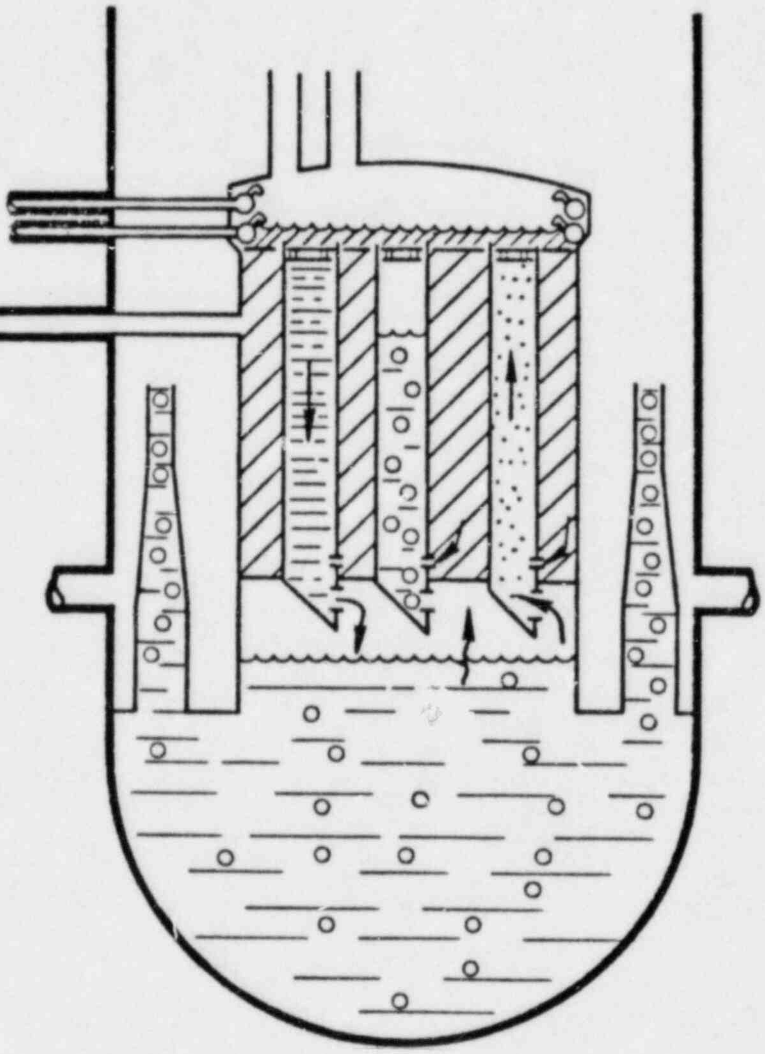
MULTIPLE CHANNEL EXPERIMENTS

- FULL SCALE SECTORS
  - THREE DIMENSIONAL EFFECTS
  - UPPER PLENUM RESPONSE
  - LARGE NUMBER OF CHANNELS

- HEATED CHANNEL FACILITIES
  - PARALLEL CHANNEL INTERACTIONS
  - FUEL ROD TEMPERATURE RESPONSE

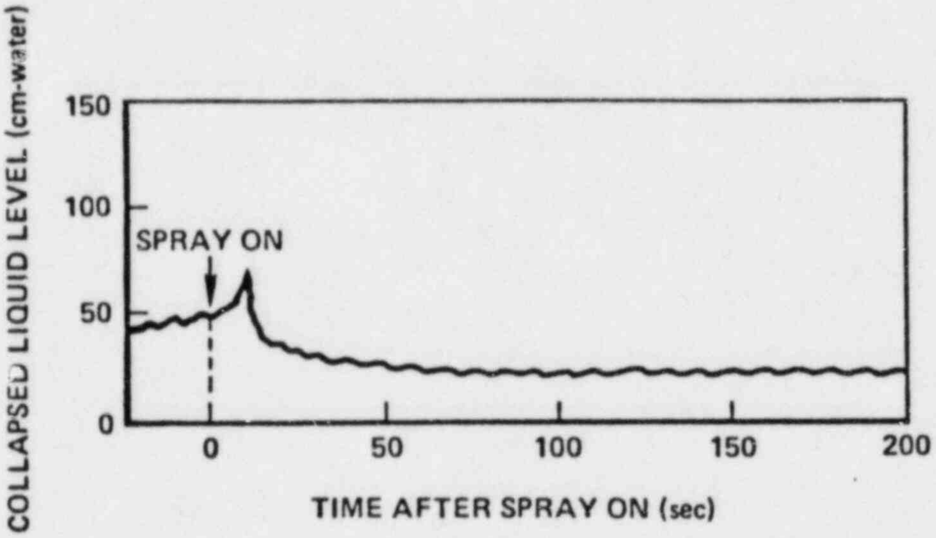


30 Degree Steam Sector Test Facility -- SSTF



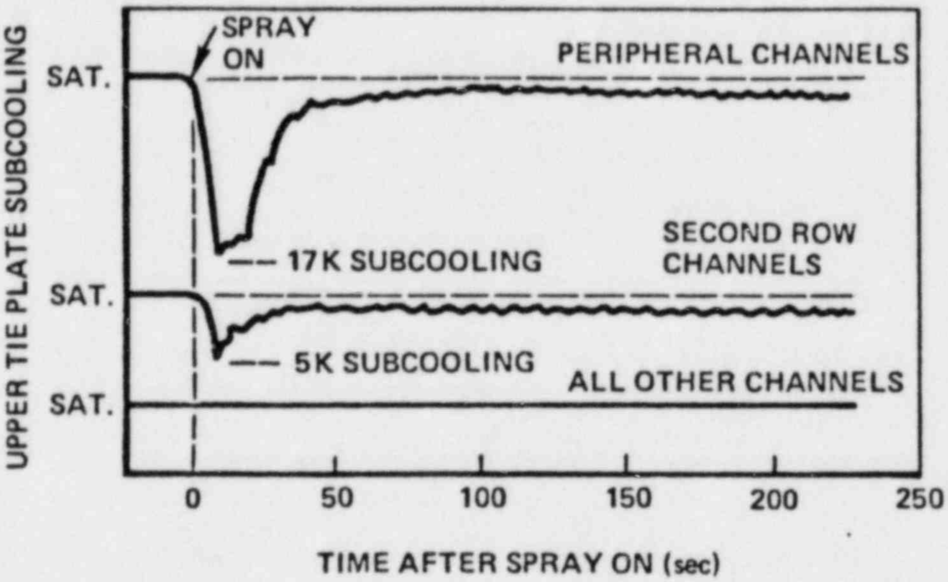
Typical Multi-channel Conditions

20

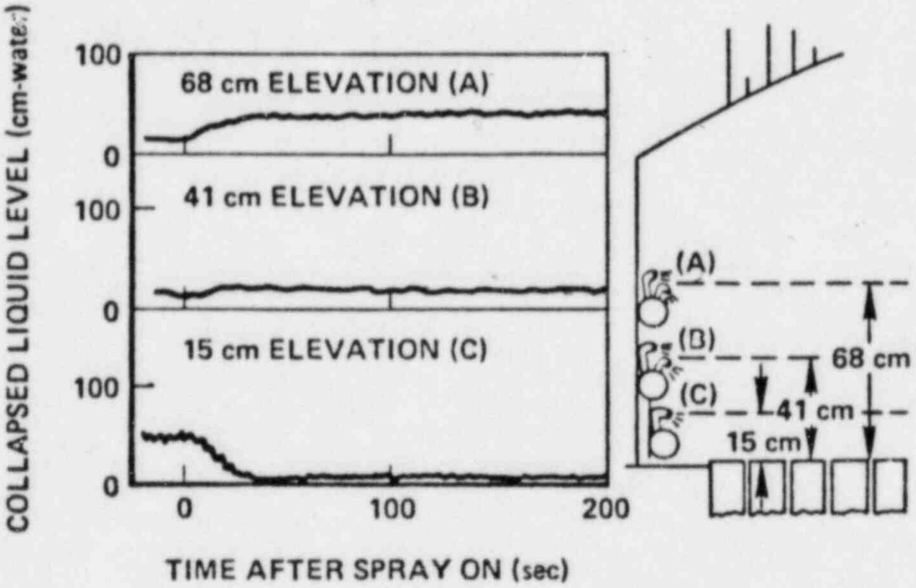


Upper Plenum Response and Residual Pool -- SSTF

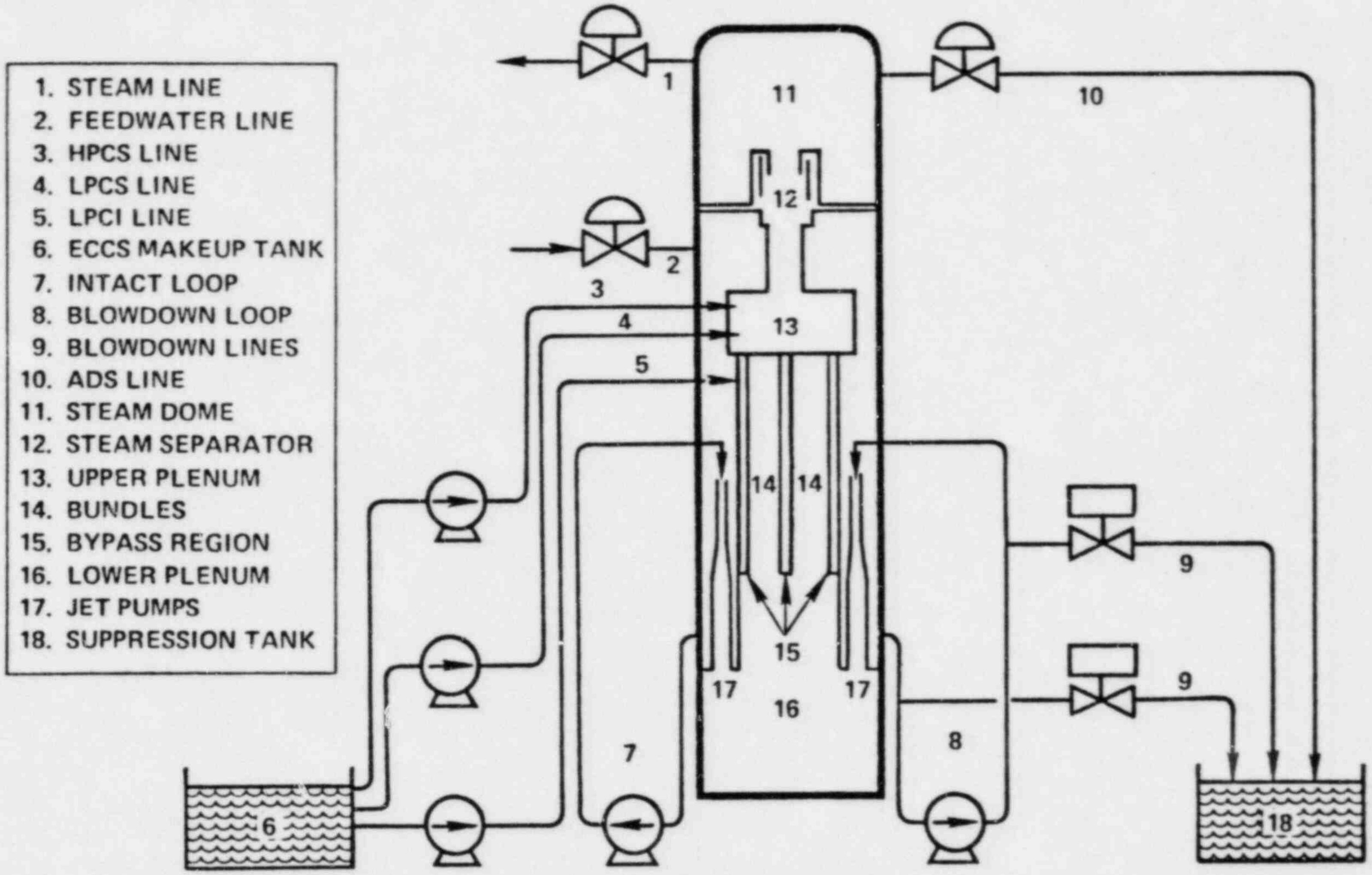
(51)



Subcooled CCFL Breakdown - SSTF

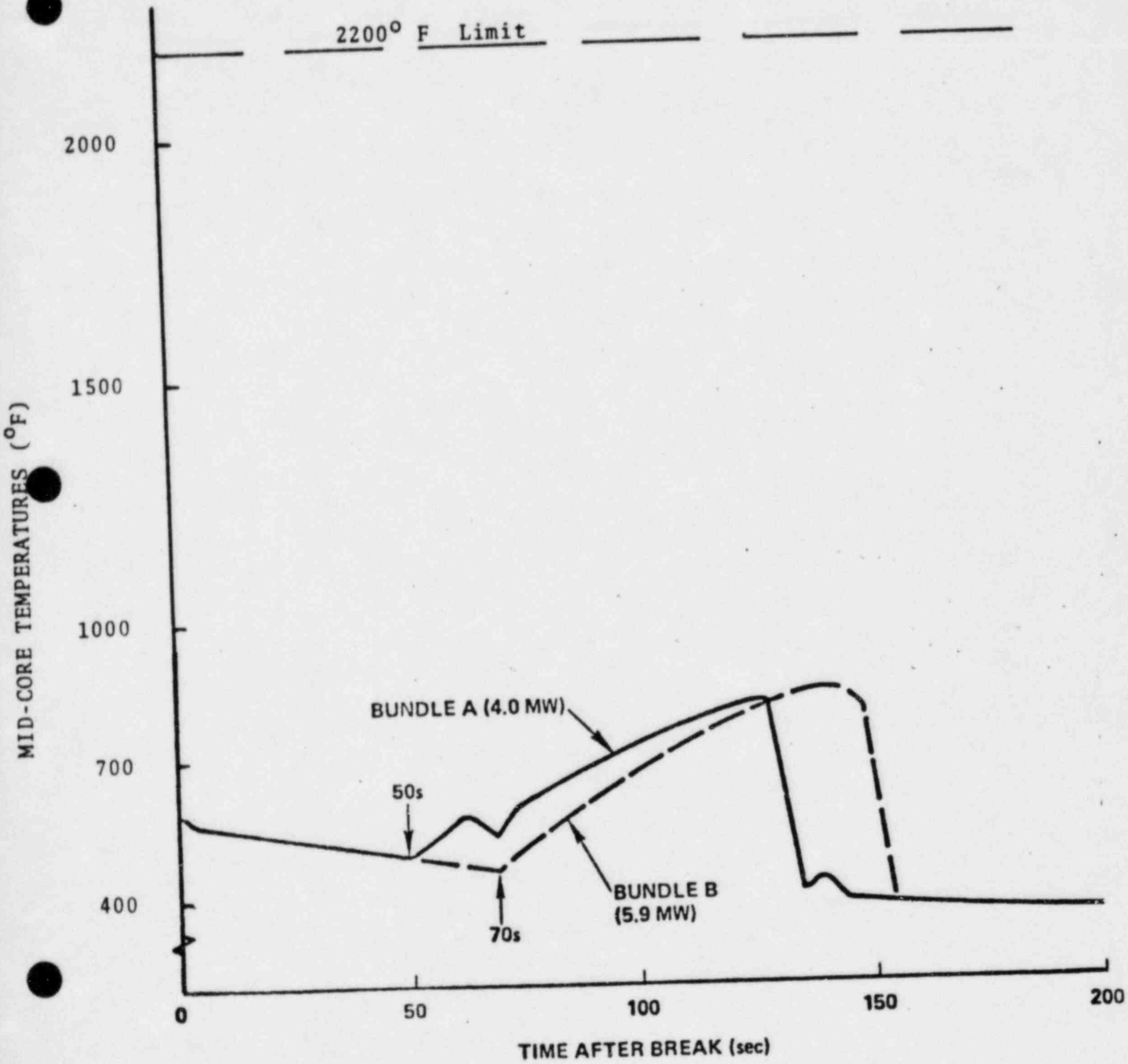


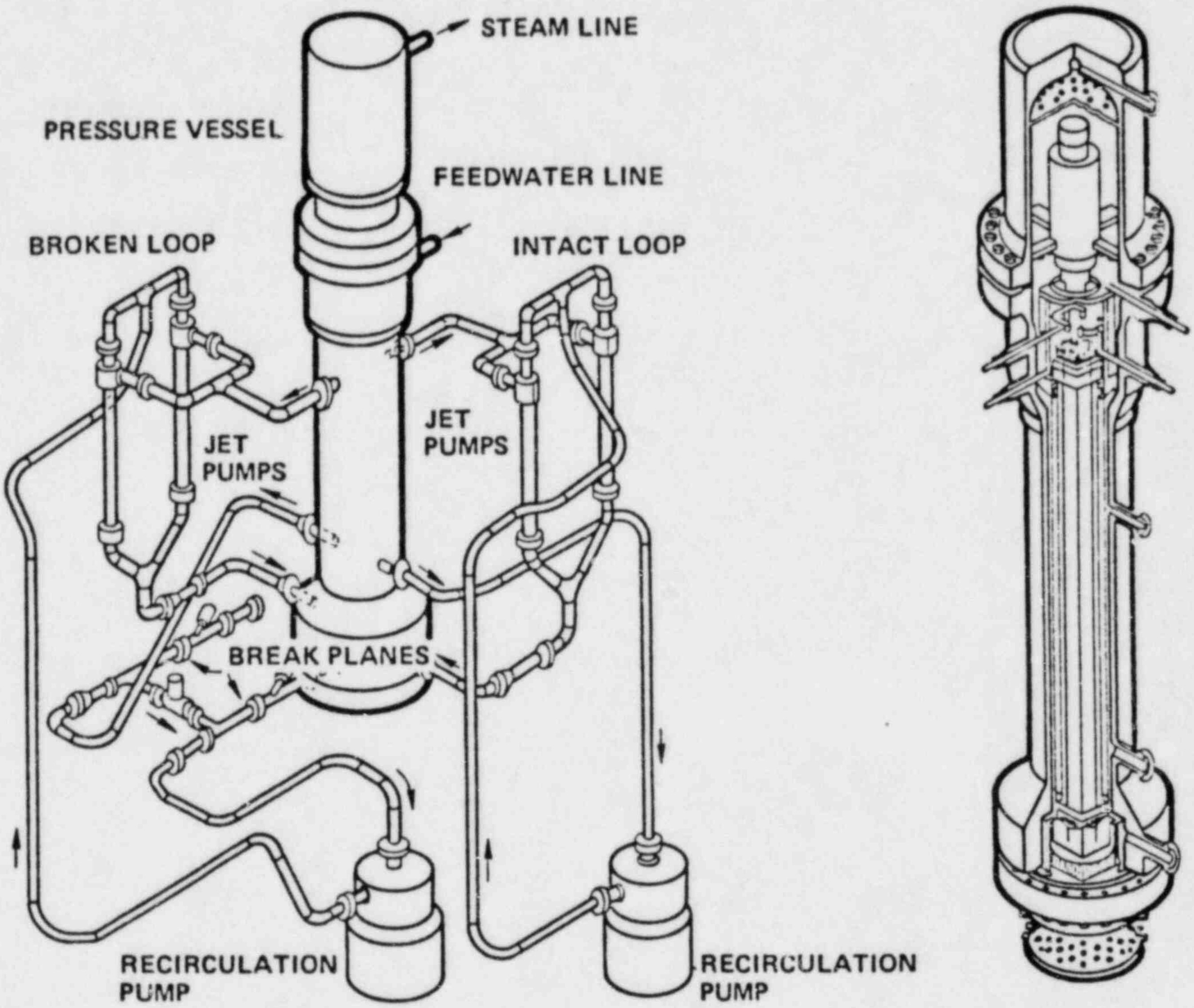
Upper Plenum Pool Sensitivity to Spray Injector Elevation



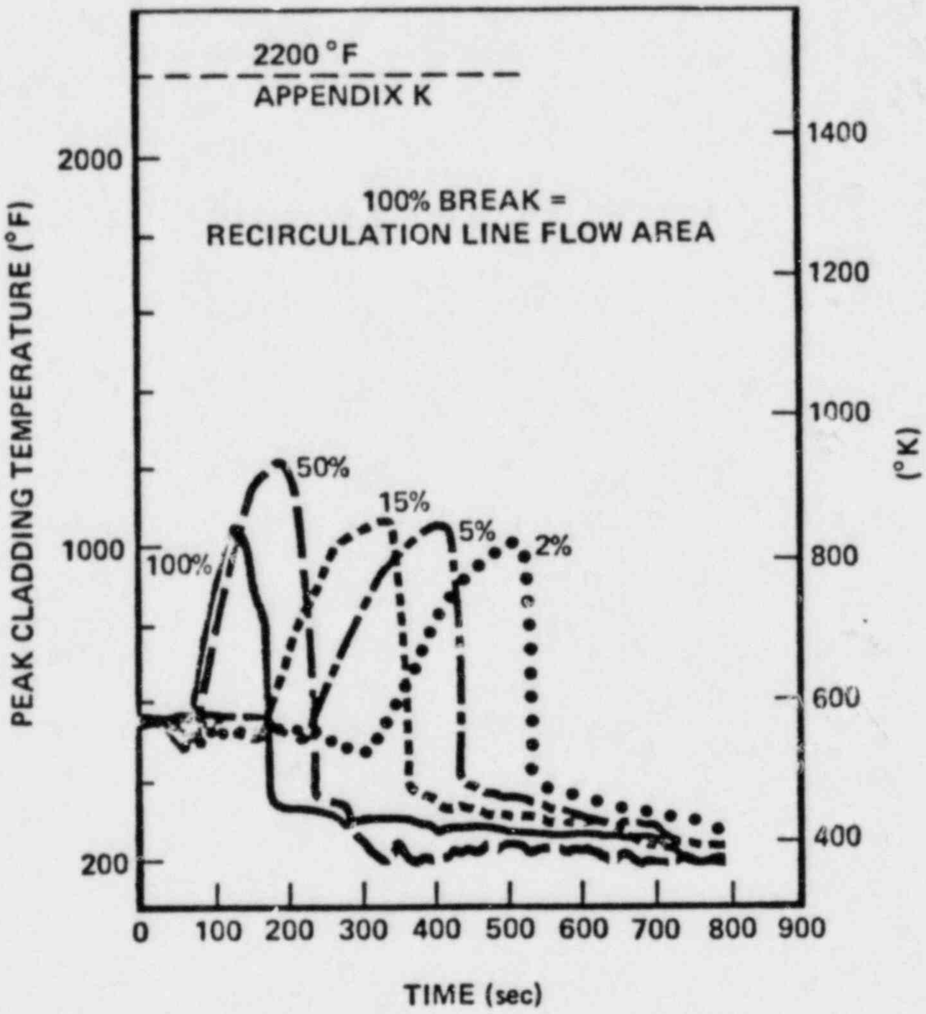
Two Bundle Loop -- TBL

TWO BUNDLE LOCA TEMPERATURES





Four Bundle Rig of Safety Assessment -- Rosa III



Peak Cladding Temperature Response
Sensitivity to Break Size -- Rosa III

MULTIPLE CHANNEL HIGHLIGHTS

- MOST CHANNELS RESPOND AS IN SINGLE CHANNEL TESTS
- SUBCOOLED LIQUID DRAINS THROUGH PERIPHERAL CHANNELS
- HIGH VAPOR UPFLOW THROUGH A FEW CHANNELS
- LIQUID POOL REMAINS IN UPPER PLENUM
- LOW TEMPERATURES FOR ALL BREAK SIZES

EXPERIMENTAL TECHNOLOGY SUMMARY

- KEY EXPERIMENTS OF INTEGRATED PROGRAM NEARLY COMPLETED
- EXCELLENT EMPIRICAL UNDERSTANDING OF BWR LOCA/ECCS RESPONSE
- MODEL REQUIRED TO EXTRAPOLATE FAVORABLE RESULTS TO REACTOR
- EXPERIMENTAL BASIS DIVERSE AND COMPLETE ENOUGH TO CHALLENGE AND QUALIFY MODELS

GE MODELING APPROACH

- Develop Best-Estimate Model (TRAC)
  - Detailed Phenomena Models
  - Three Dimensional Capability
  - Separate Effects Data Qualification
  - Integral System Data Qualification
  - Benchmark Reactor Applications

- Develop Realistic Evaluation Model (SAFER)
  - Efficient Code for Production Use
  - Incorporate Controlling Phenomena
  - Simplify for Efficient Application
  - Qualify with Data and Best-Estimate Model

MODEL STATUS

- TRAC-BWR
  - Models for LOCA analysis developed
  - Assessment for LOCA events near completion
  - "Best-Estimate" predictive capability available
  - Being used to quantify uncertainty in SAFER calculation.
- SAFER
  - Models under US NRC review
  - Consistent use of SAFER/GESTR accepted
  - Assessment for BWR LOCA In Progress
  - Application methodology under development.

J. G. Andersen

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BEST ESTIMATE MODEL DEVELOPMENT

- General Electric Actively Involved in TRAC-BWR Development (In Collaboration with INEL).
- Work Sponsored by NRC (RSR)/EPRI/GE Under BWR Refill/Reflood Program.
- Objective: Develop Best Estimate Model to Quantify True Safety Margins.

TRAC-BWR Successfully Completed

Good Agreement with Data Demonstrated

CURRENT CAPABILITIES OF TRAC-BWR

- Model Capabilities
  - Three dimensional hydrodynamics
  - Full two-fluid model for entire LOCA transient
  - Mechanistic calculation of non-equilibrium conditions
  - Detailed reflood phase models for radiation heat transfer, spray cooling, channel, and rod quenching
  - BWR component models
  - Multiple channel calculation
  - Realistic constitutive correlations for flow regime map, shear, and heat transfer.

Best Benchmark Tool for BWR Calculations.

TRAC-BWR DEVELOPMENT APPROACH

- Develop Detailed Models for Individual Phenomena and Components.
- Predict Basic Effects Tests.
- Predict System Tests.

↓

Apply for BWR Predictions

TRAC-BWR MODEL DEVELOPMENT HISTORY

TRAC B01 (GE)	1981
TRAC BD1 (INEL)	1981
TRAC BD1 Version 12 (INEL)	1982
TRAC B02 (GE)	1982

Final Version: TRAC B02

- Based on TRAC BD1 Version 12.
- Includes all models developed at GE.

MAJOR BASIC MODELS FOR TRAC-BWR

• Flow Regime Map	(GE)
• Interfacial Shear	(GE)
• Heat Transfer	(GE/INEL)
- Boiling transition	
- Subcooled boiling	
- Thermal radiation	
- Interfacial heat transfer	
• CCFL	(GE)
• Choked Flow	(INEL)
• Two-Phase Level Model.	(GE)

MAJOR COMPONENT MODELS FOR TRAC-BWR

• Fuel Channel	(INEL)
• Jet Pump	(GE/INEL)
• Steam Separator	(GE)
• Steam Dryer	(GE)
• Upper Plenum	(GE)
• Control System	(INEL)
• Boron Injection	(INEL)
• Reactivity Feedback.	(INEL)

35

MODEL DEVELOPMENT

AND

DEVELOPMENTAL ASSESSMENT

36

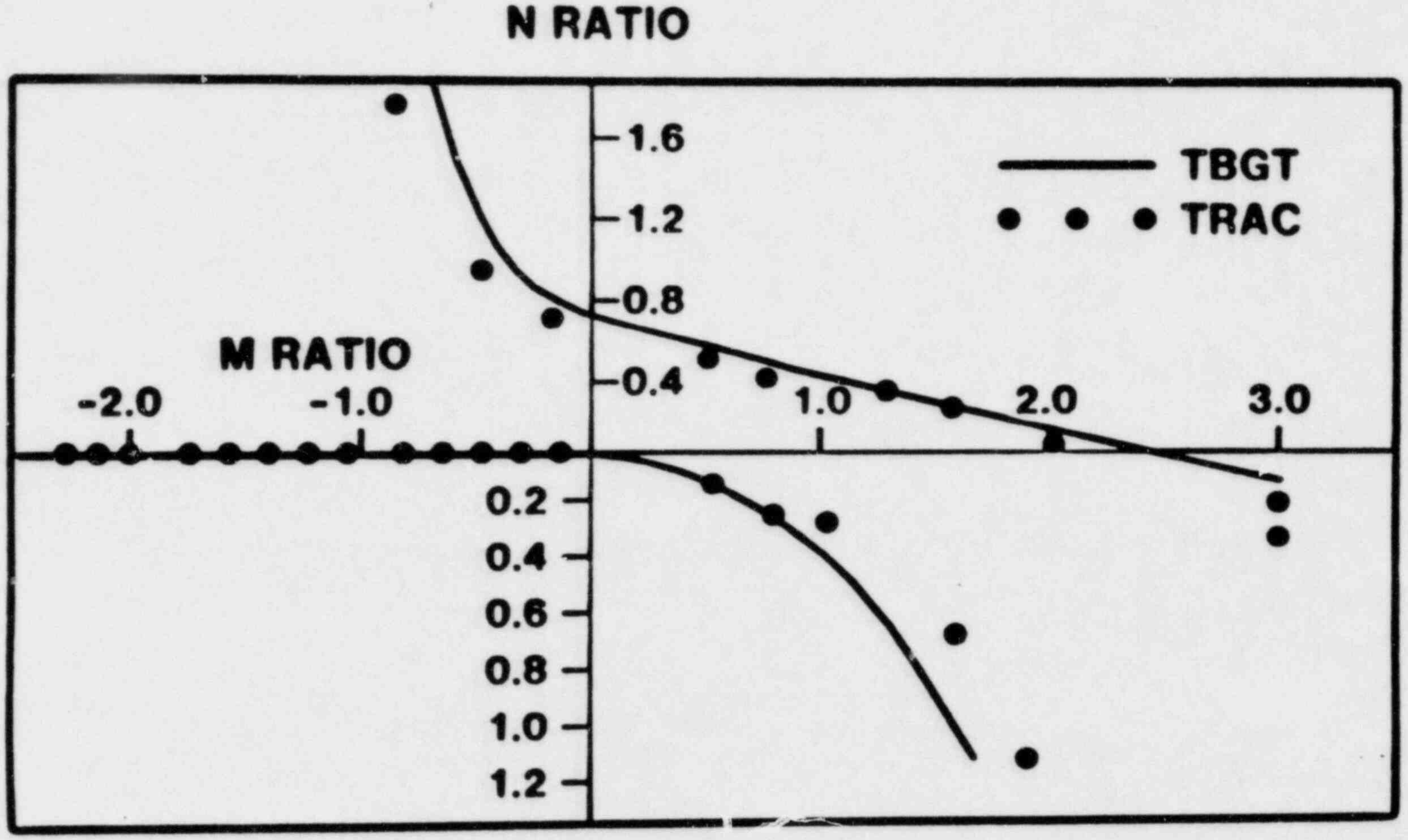
TRAC JET PUMP MODEL

- Conservation of Momentum for Mixing Process.

- Irreversible Losses
  - Mixing
  - Bending
  - Area changes.

- All Six Flow Regimes.

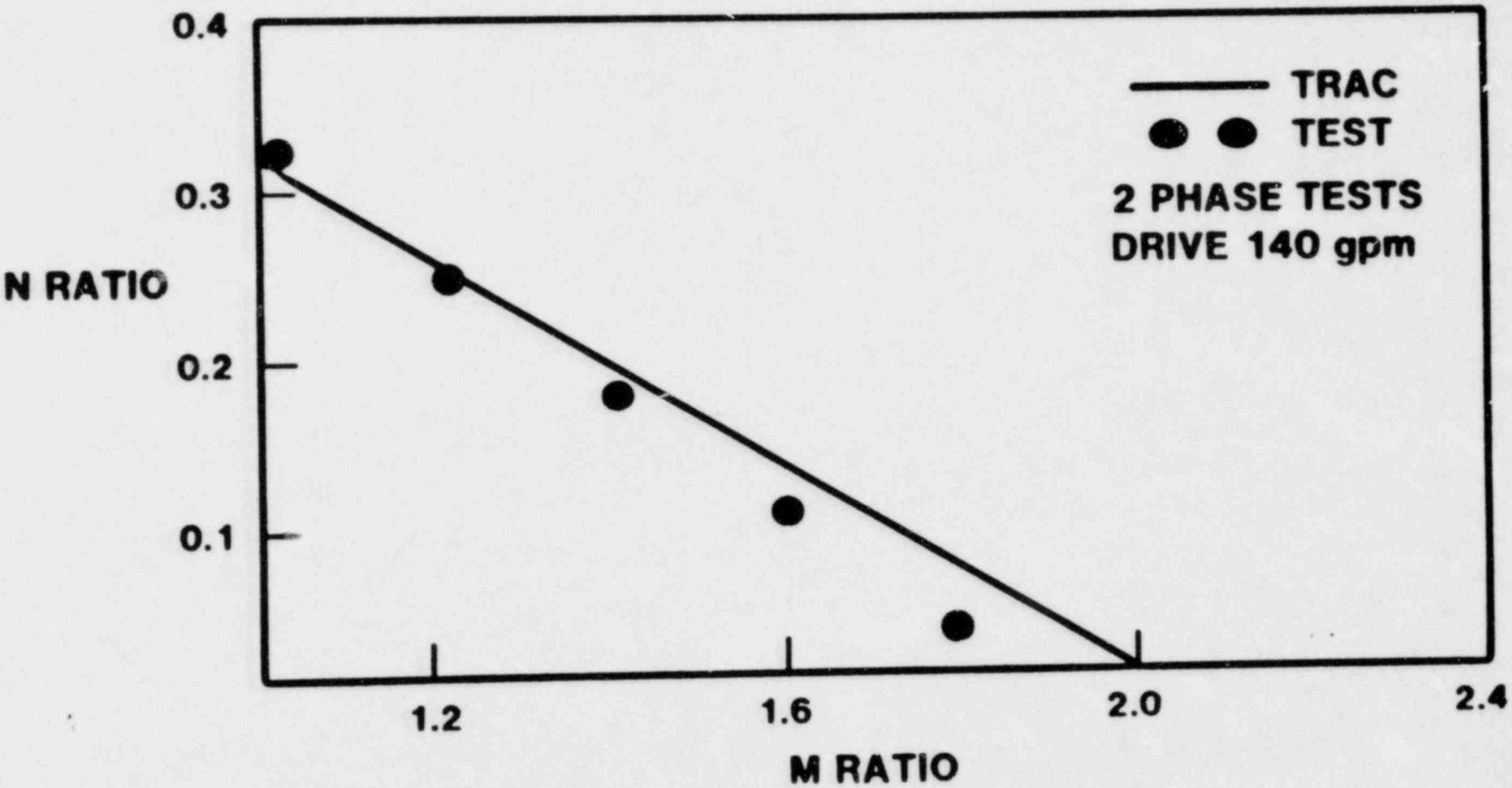
⅛ SCALE JET PUMP SINGLE PHASE TEST



Jet Pump Performance Predicted in All Four Quadrants

37

JET PUMP 2 PHASE TEST



Two-Phase Jet Pump Performance Accurately Predicted

STEAM SEPARATOR MODEL

- FUNCTION

  To calculate pressure drop, carryunder and carryover in the flow through the steam separator.

- MECHANISTIC MODEL

  - Solved

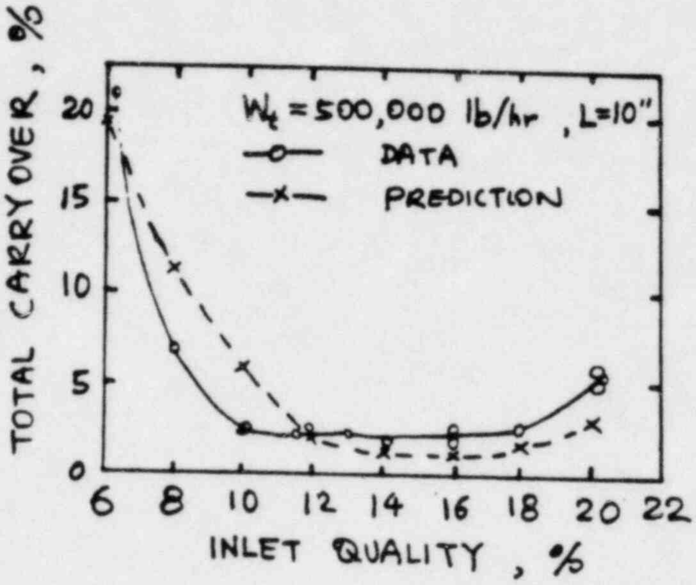
    Water mass
    Vapor Mass
    Axial momentum
    Angular momentum

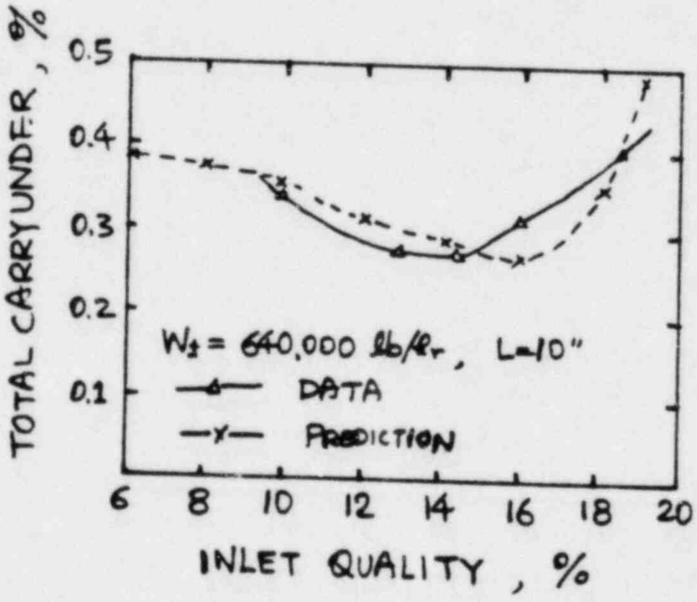
    } Conservation equations for the separator barrel

    Pressure drop equation in discharge passage
    Pressure drop equation across the water layer.

  - Tuned

    The parameters controlling the radial void and velocity profiles.





SEPARATOR CHARACTERISTICS WELL PREDICTED

STEAM DRYER MODEL

- Function

  To simulate pressure drop and separation of moisture in flow through the steam dryer.

- TRAC Modeling

  Structured as an integral part of the VESSEL.

- Dryer Pressure Drop

  Calculated based on input loss coefficient.

$$K_{SD} = \frac{2\rho_v \; \Delta P_{SD}}{(W_v/A_{SD})^2}$$

- Dryer Separation Capacity

  Function of steam velocity and moisture content at dryer inelt.

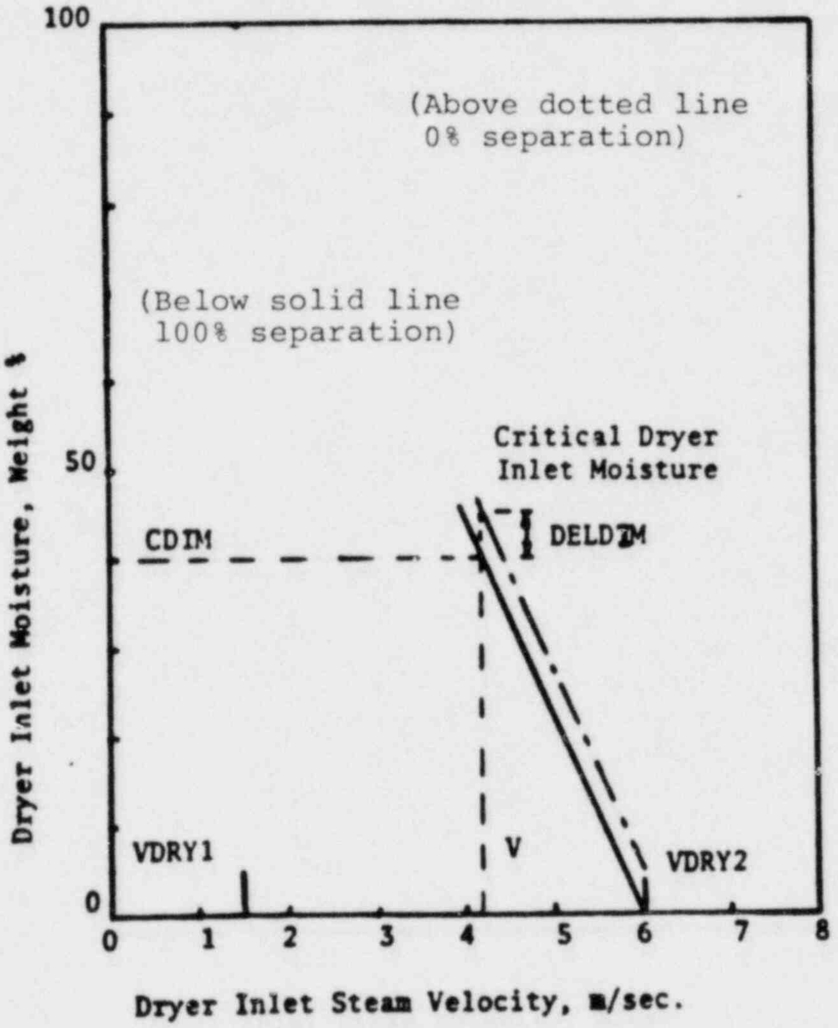


FIGURE 22. Dryer Separation Capacity as Function of Inlet Moisture and Steam Velocity

UPPER PLENUM MODEL

- Spray Distribution

  Calculate ECC distribution, when upper plenum two-phase level is below the ECC sparger.

- Submerged Jet

  Calculate ECC penetration, when ECC sparger is covered by two-phase level.

- Turbulent Shear and Mixing

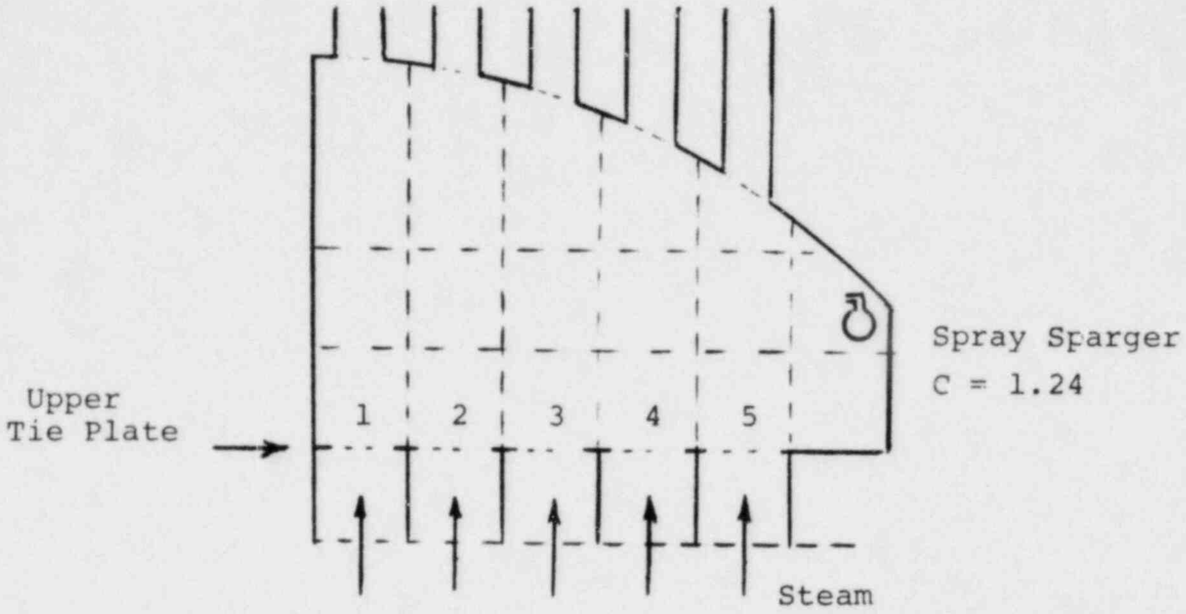
  Controls gross motion in upper plenum pool.

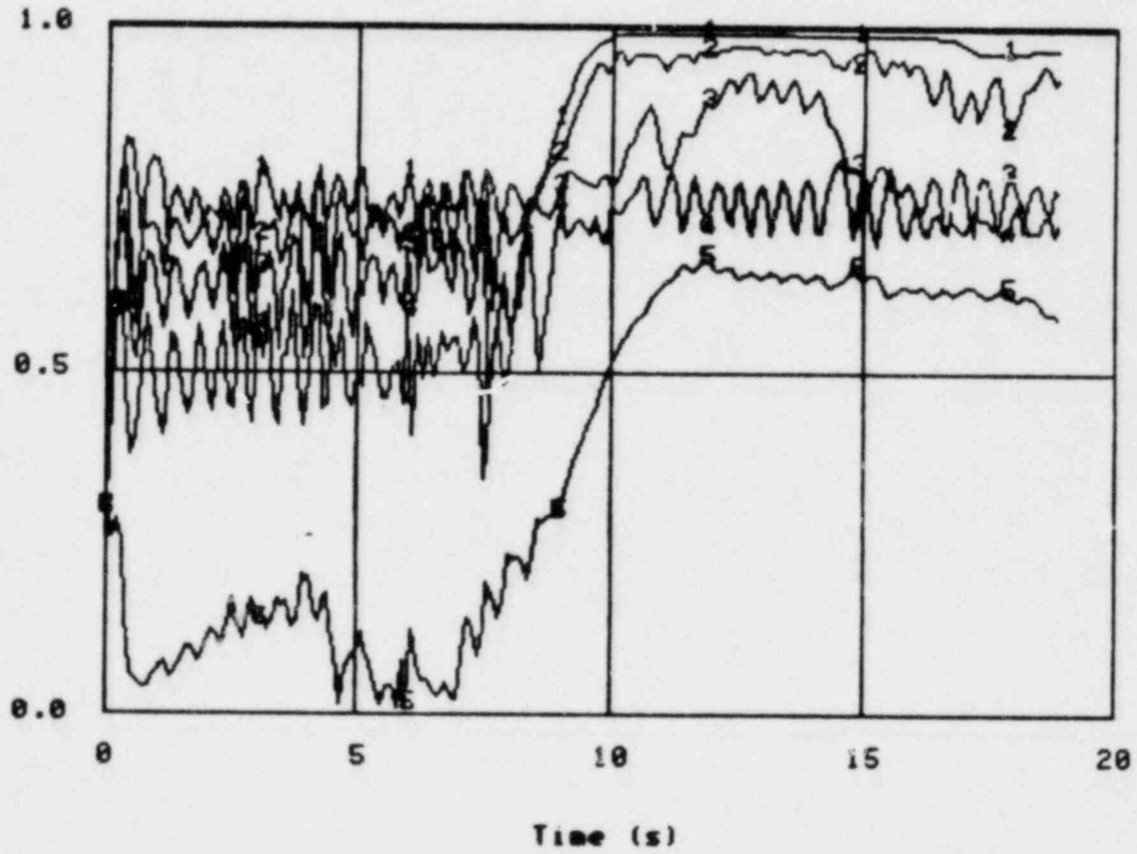
- Tuned to 16° Sector Tests

  Qualified on SSTF results.

UPPER PLENUM MODEL

16° Sector

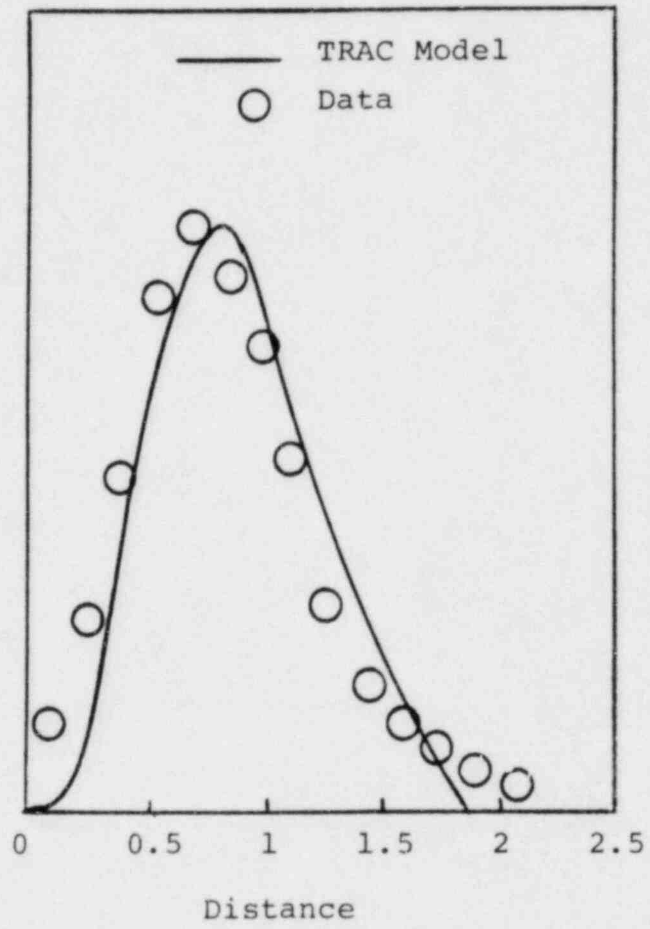




Data: CCFL Break Down at t = 6 seconds.

(45)

UPPER PLENUM MODEL

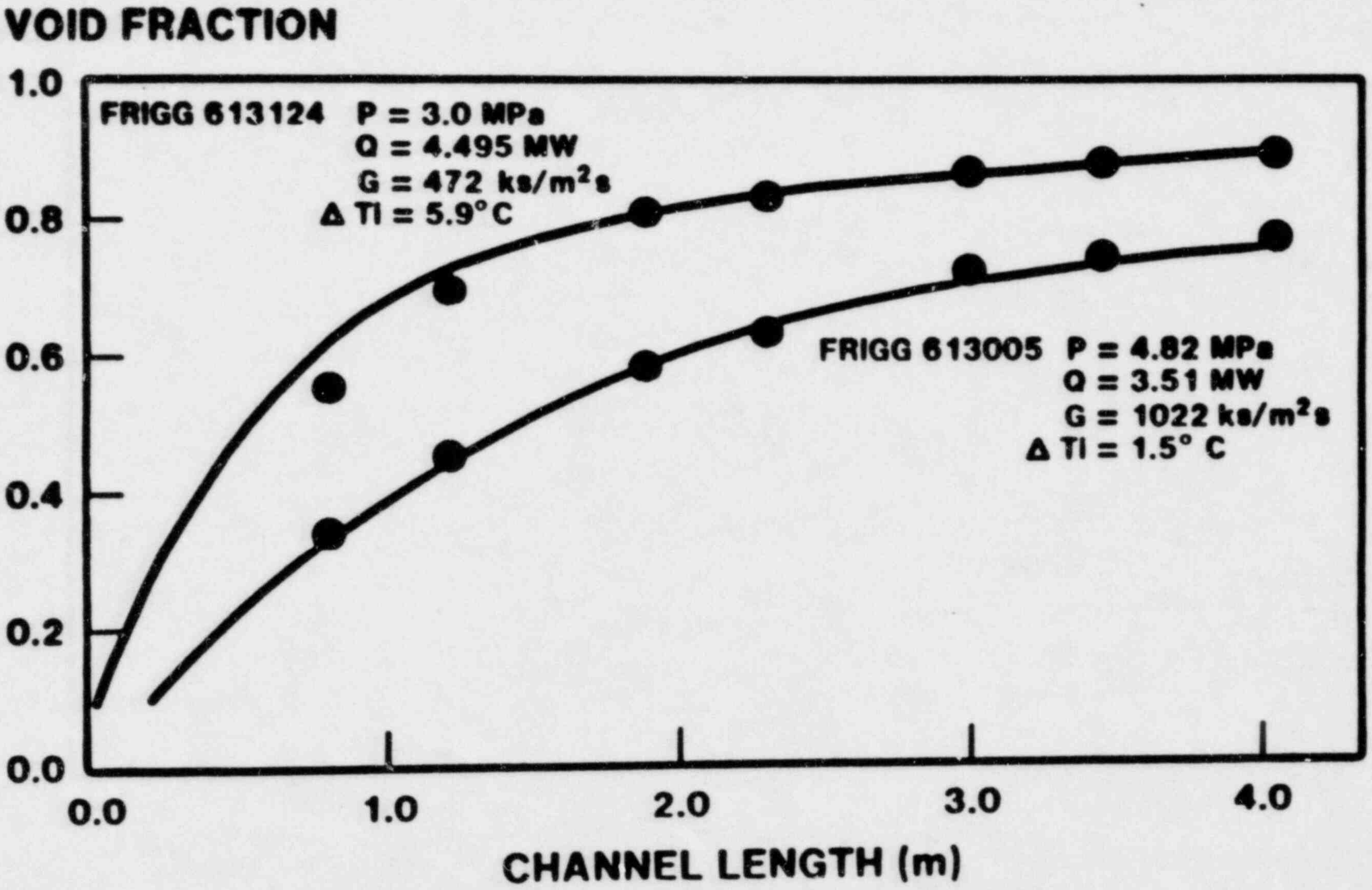


Horizontal Spray Test

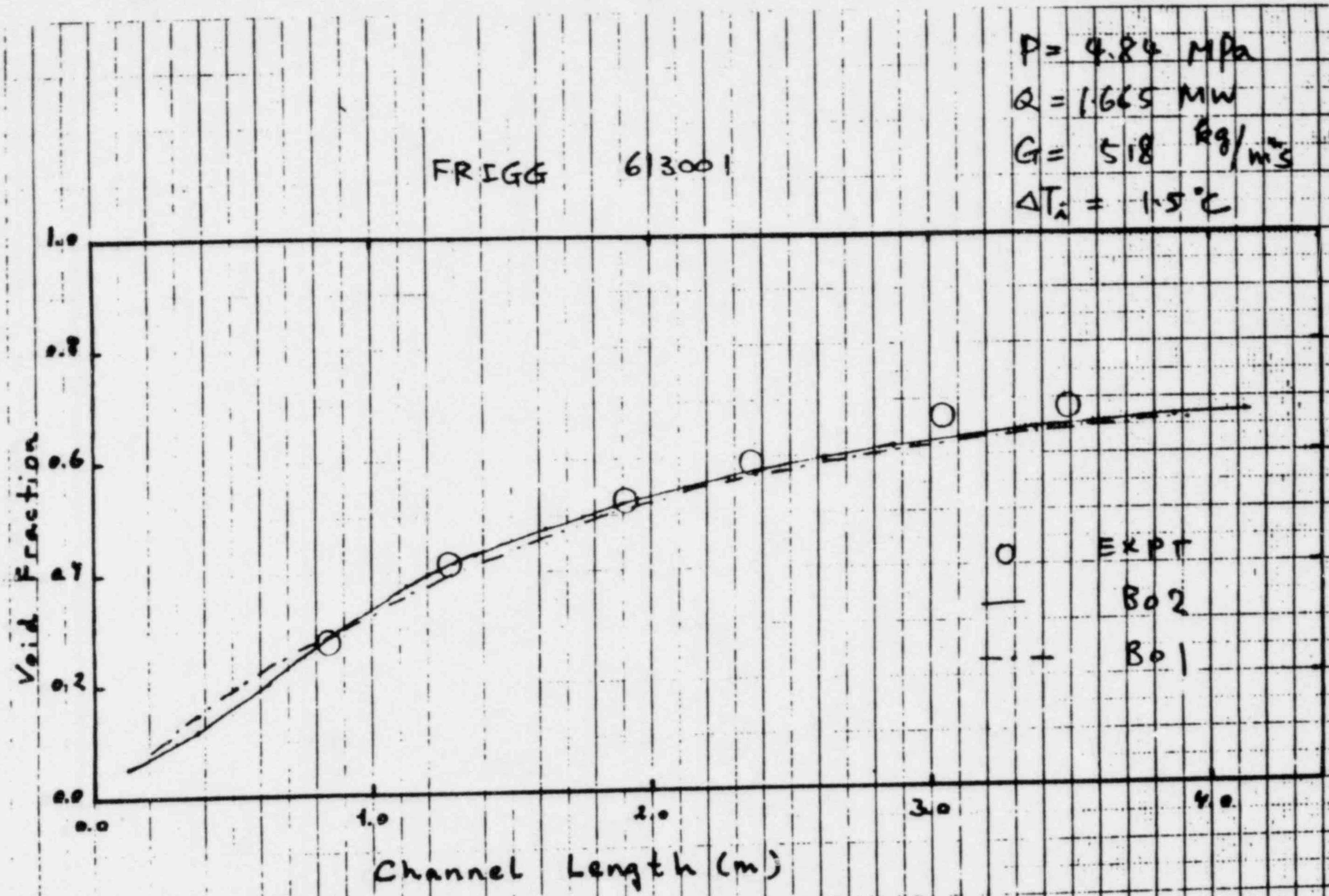
VOID FRACTION PREDICTION

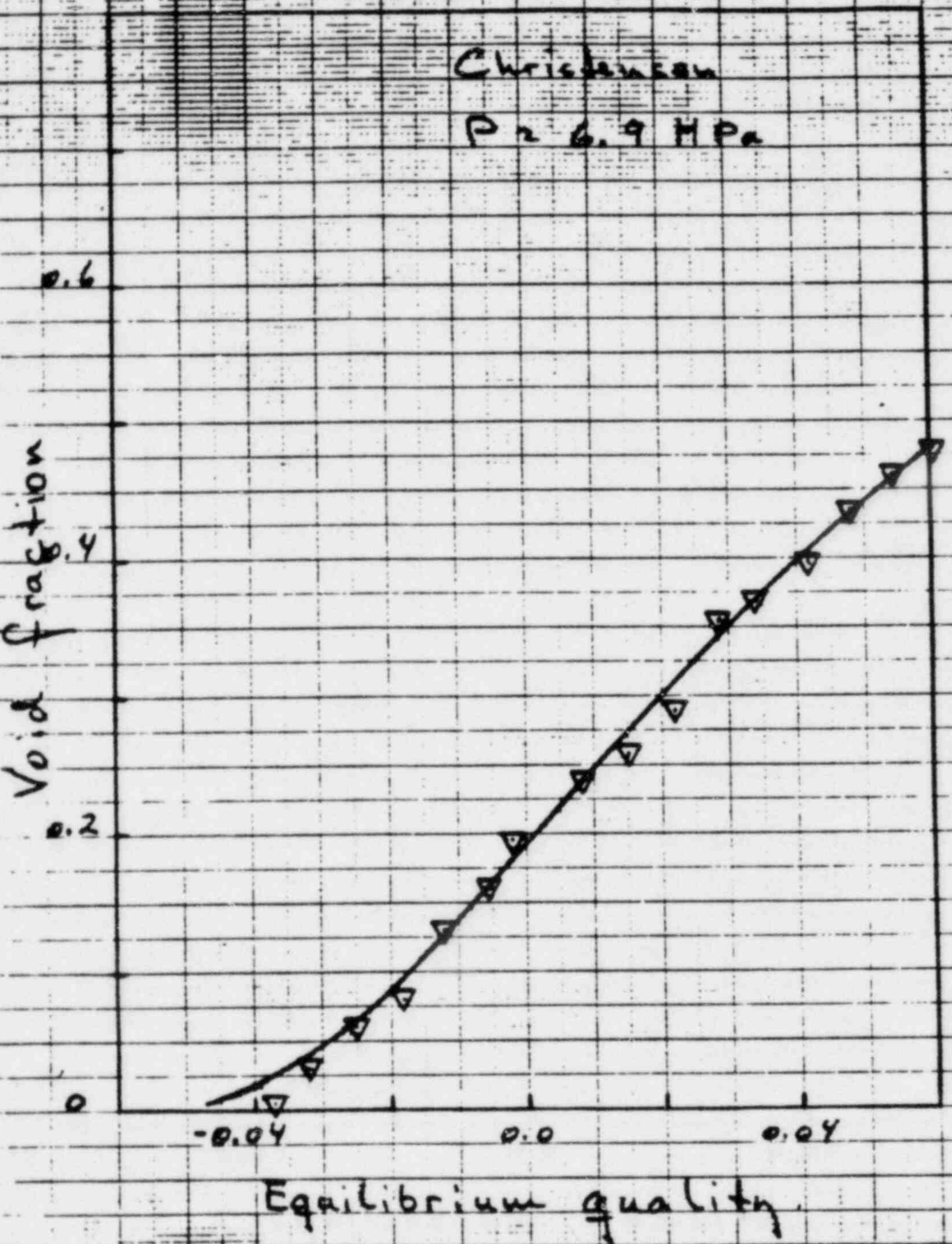
- Flow Regime Map
- Interfacial Shear
- Interfacial Heat Transfer
  - Condensation
  - Subcooled boiling.

FRIGG VOID FRACTION COMPARISON



ROD BUNDLE VOID FRACTIONS ACCURATELY PREDICTED

FRIGG 613001
P= 4.84 MPa
Q = 1.665 MW
G= 518 kg/m²s
ΔTᵢ = 1.5 °C
○ EXPT
— Bo2
-·- Bo1
Void Fraction
1.0
0.8
0.6
0.4
0.2
0.0
0.0
1.0
2.0
3.0
4.0
Channel Length (m)


Christensen
P = 6.9 MPa
Void fraction
0.6
0.4
0.2
0
-0.04
0.0
0.04
Equilibrium quality

TRAC D02

CCFL PREDICTION

- Interfacial Shear

- Condensation Heat Transfer
  Subcooled CCFL Breakdown.

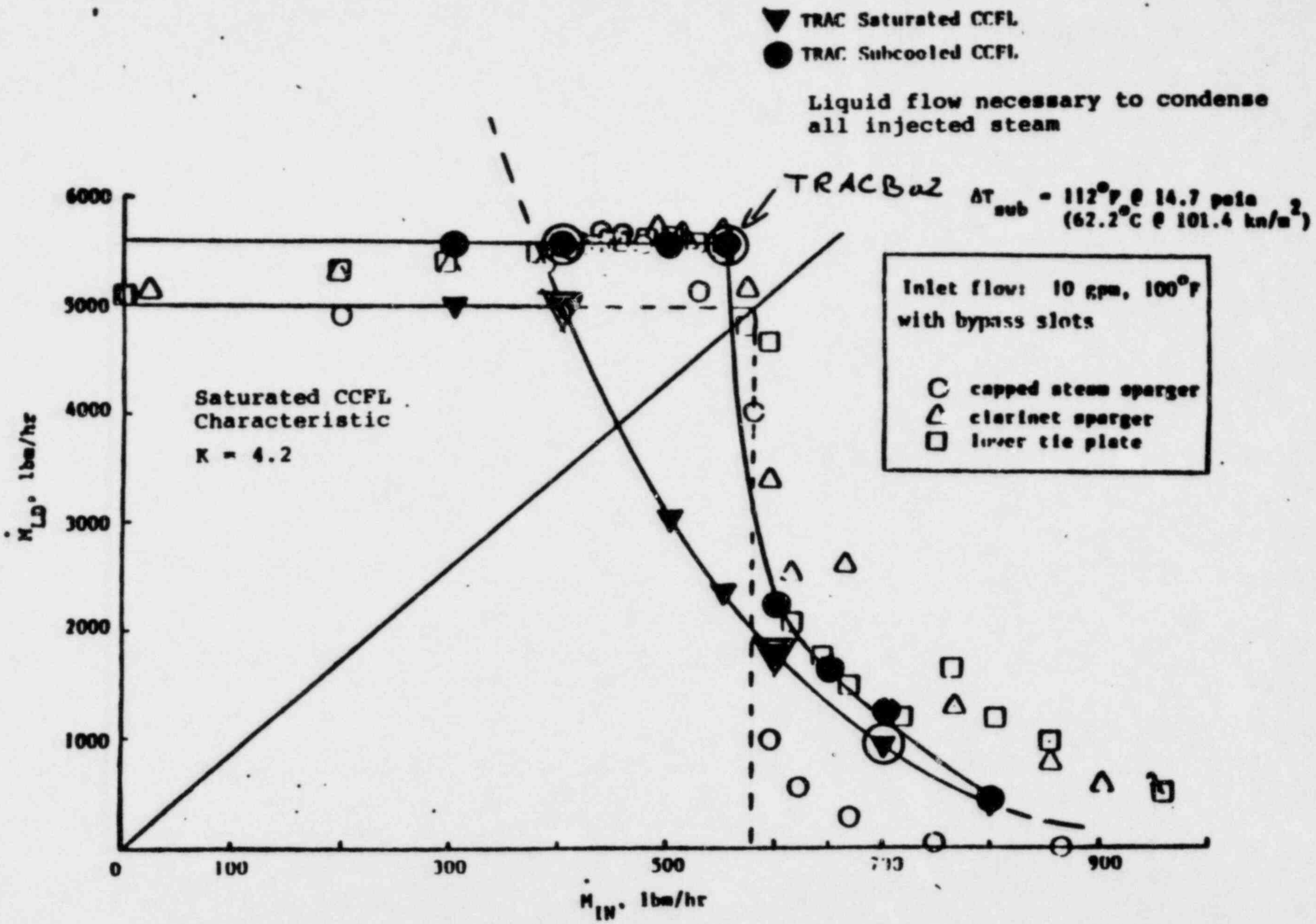
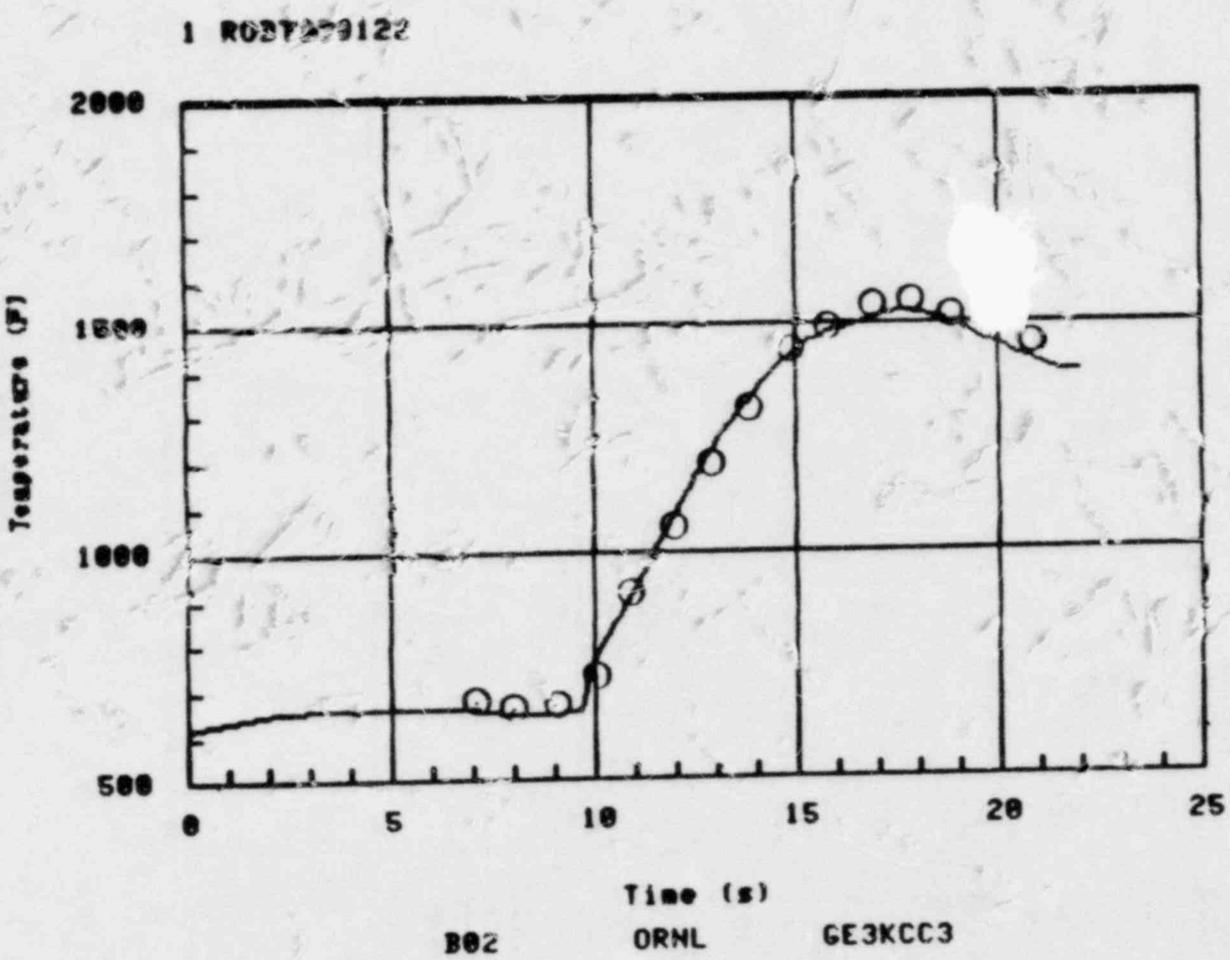


FIGURE 3.2.1 - Counter-Current Flow Limitation

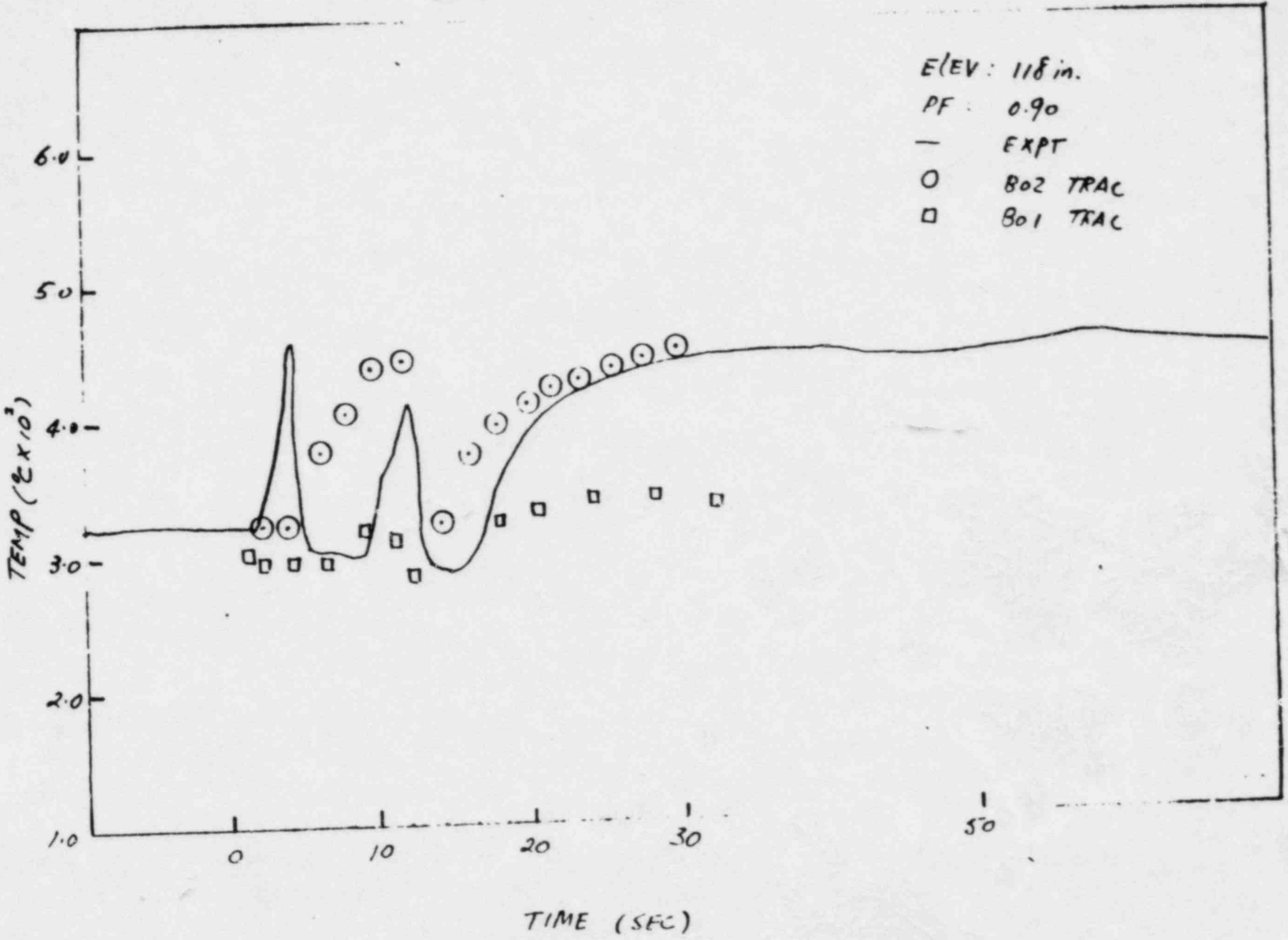
50

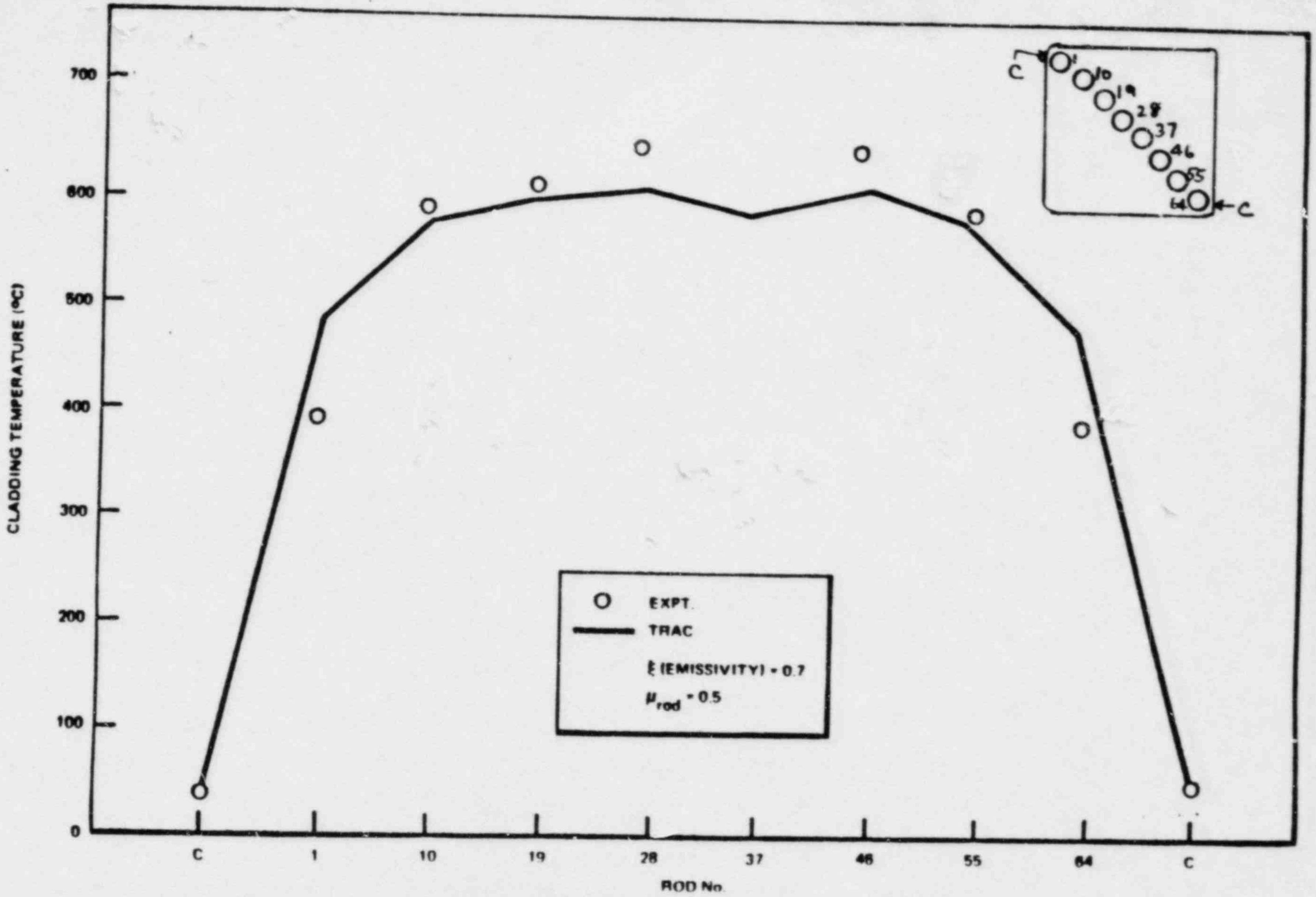
HEAT TRANSFER PREDICTION

- Hydraulic Conditions
    - Flow regime map
    - Void fraction prediction.
- Heat Transfer
    - Wall heat transfer
    - Interfacial heat transfer
    - Thermal radiation.


1 RODT[illegible]9122
2000
1500
1000
500
Temperature (F)
0
5
10
15
20
25
Time (s)
B02
ORNL
GE3KCC3

BDHT- TRAC COMPARISON





RADIATIVE HEAT TRANSFER WELL PREDICTED

CRITICAL FLOW

LEVEL SWELL

56

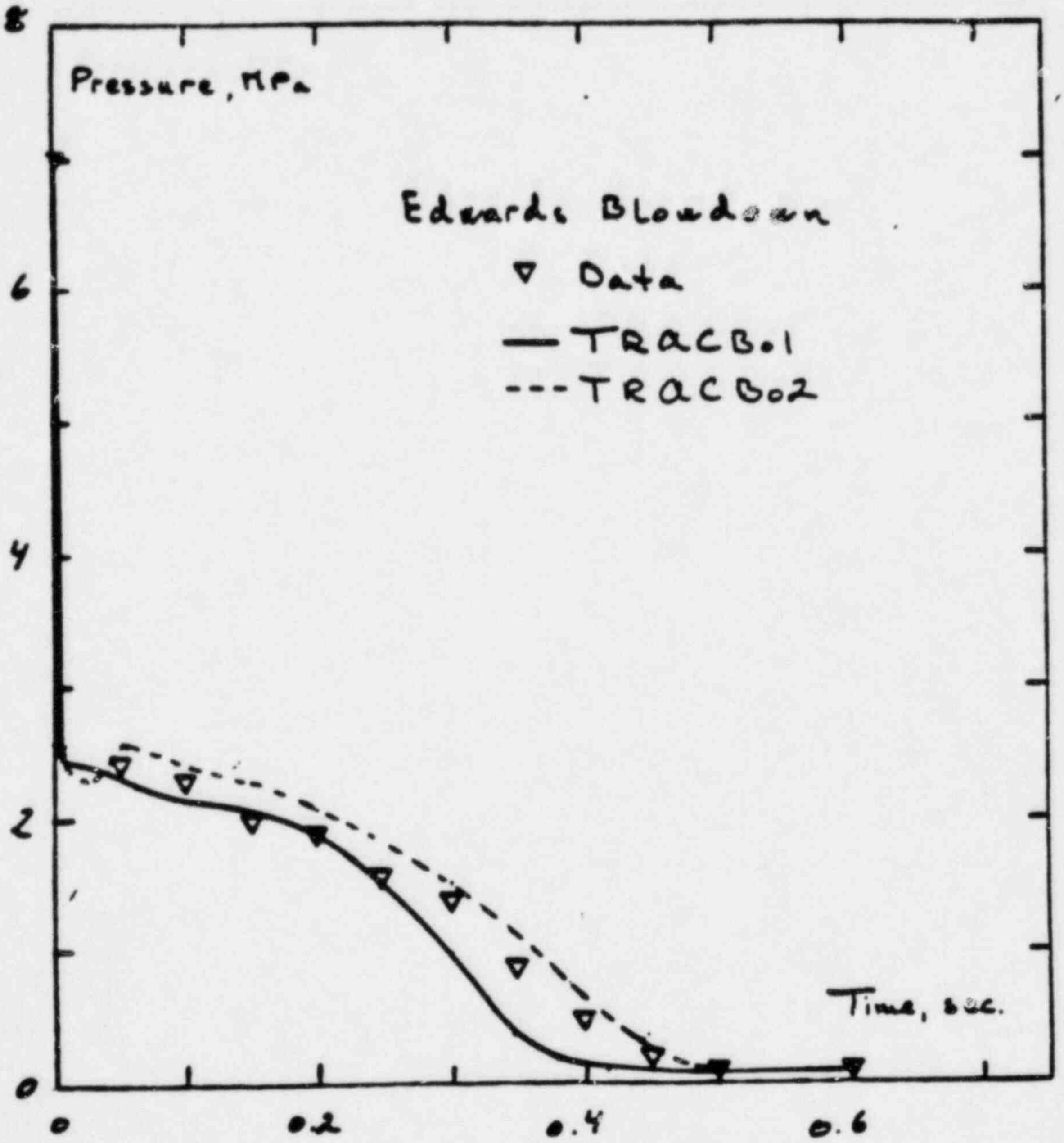


FIGURE 3.3.1 - Edward's Blowdown Pressure at Gauge Station #1

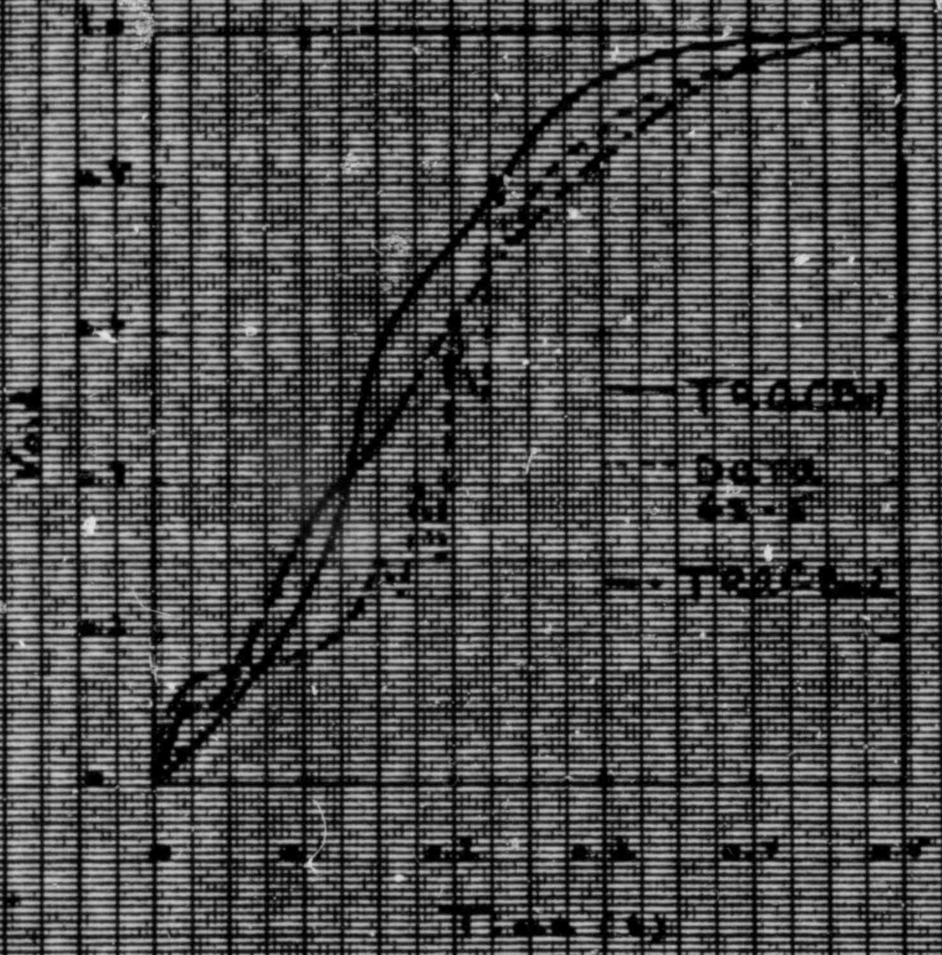


FIGURE 3.3.2 - Edward's Blowdown Void Fraction at Gauge Station 5

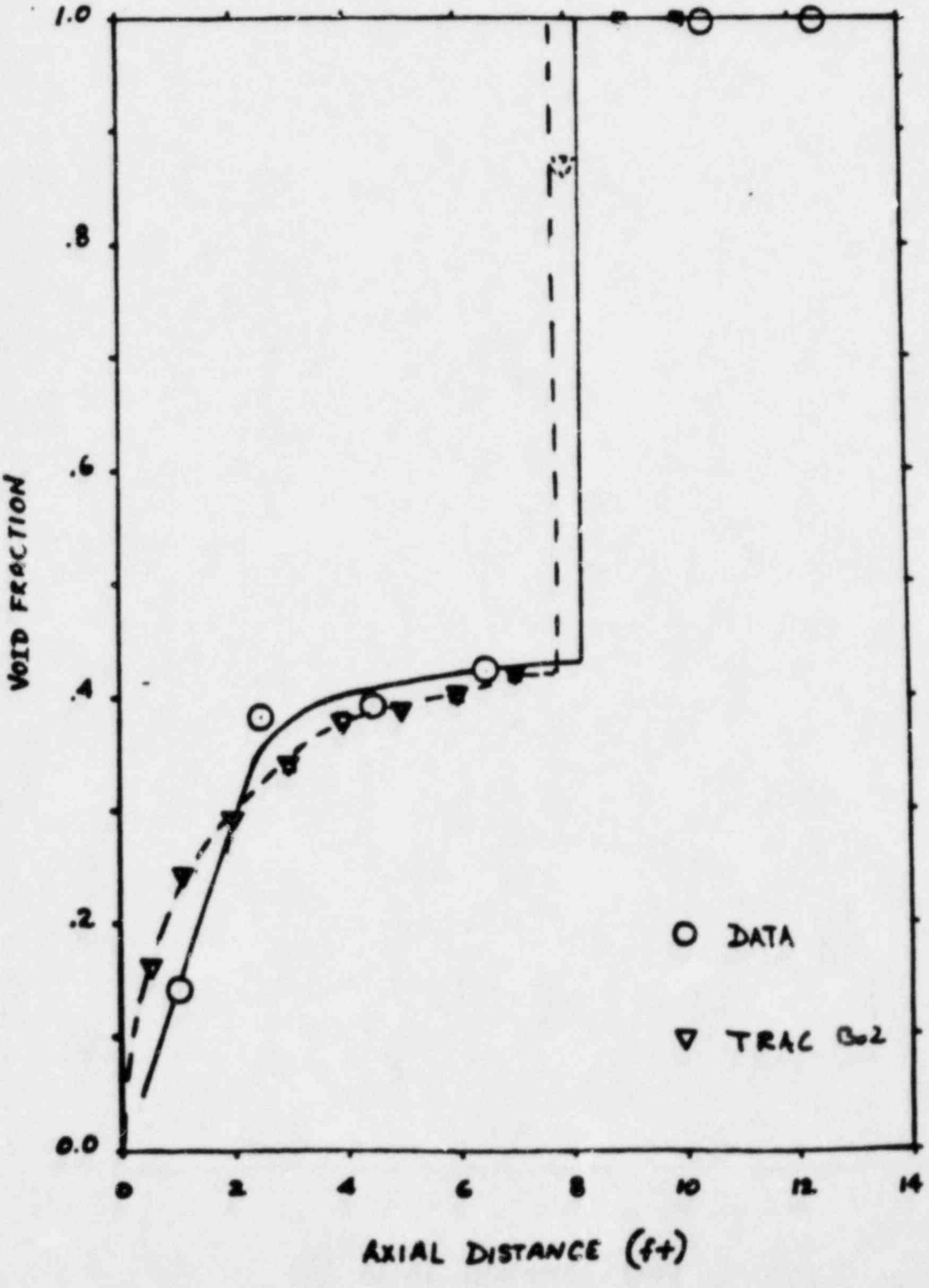


FIGURE 4.22 PSTF (5801-15) Void Fraction Distribution

TIME = 2 SEC

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SUMMARY

- TRAC BWR Successfully Completed for LOCA.
- Excellent Agreement with Data.
- TRAC-BWR Captures all Major Phenomena in the BWR.

ELECTRICAL VS NUCLEAR RODS

Thad D. Knight
Safety Code Development
Energy Division
Los Alamos National Laboratory

We consider power generation in the core to be a boundary condition. Deposition of the energy into fluid is affected by heat transfer in the rod and by the heat-transfer coefficients to the fluid. The heat-transfer coefficients are common to both nuclear and electrical rods.

Rod Heat Transfer

Nuclear fuel rods and electrical rods have different heat capacities and conductivities. These parameters together with distributed heat generation affect the amount and the location of the stored energy. If we assume that the rod configuration is not changing, these differences can be modeled easily in the heat-conduction model of the rod.

Additional Phenomena in Nuclear Rods

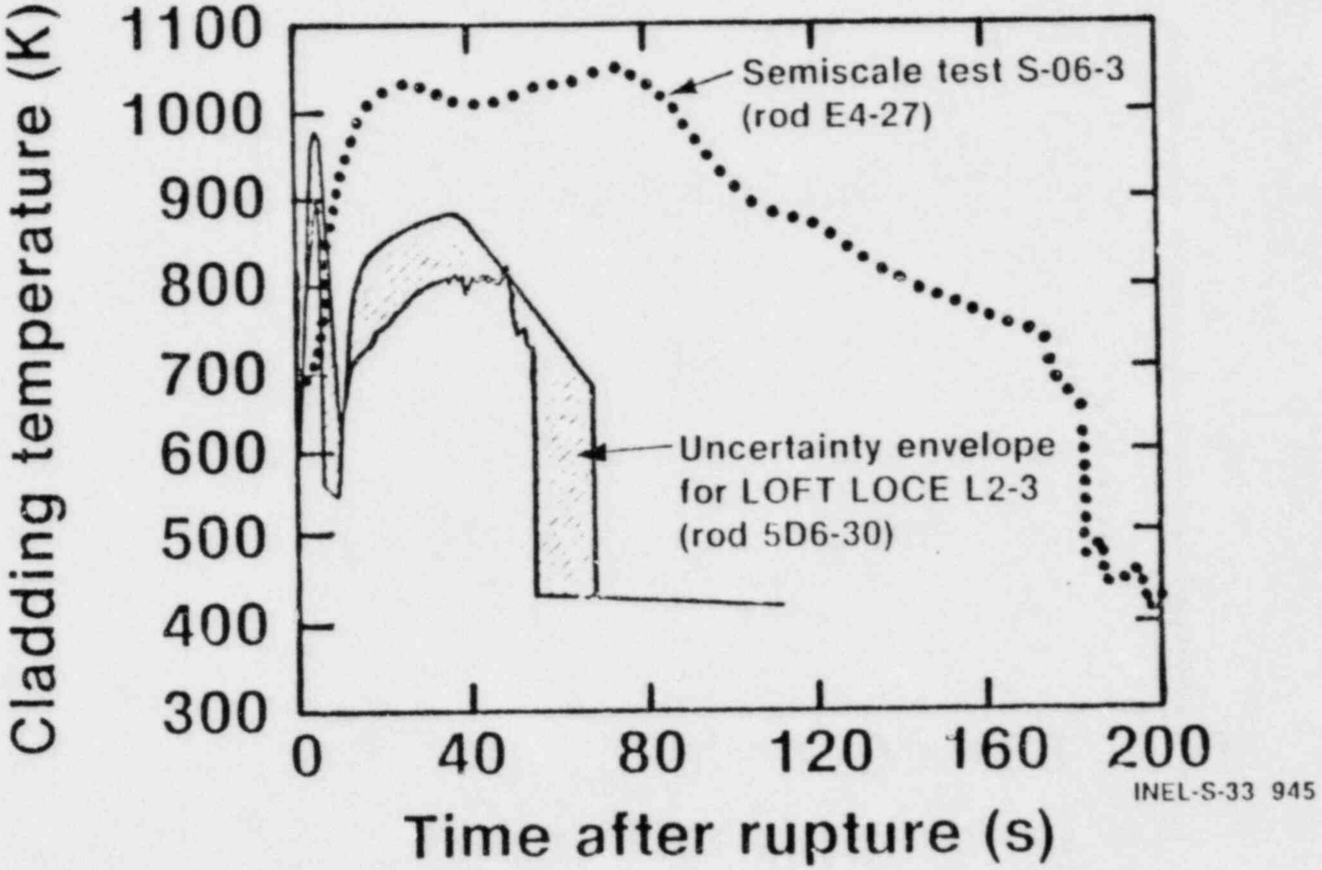
The nuclear fuel rod can exhibit cladding swelling and rupture and fuel cracking. These phenomena can change the heat-transfer characteristics of the fuel rod but have not been important in LOFT tests to date.

Problem Associated with Nuclear Fuel Rods

Generally, the state/condition of the fuel at the initiation of a transient is not established well. Therefore, the stored energy in the fuel is not well known and may affect peak cladding temperature and quenching.

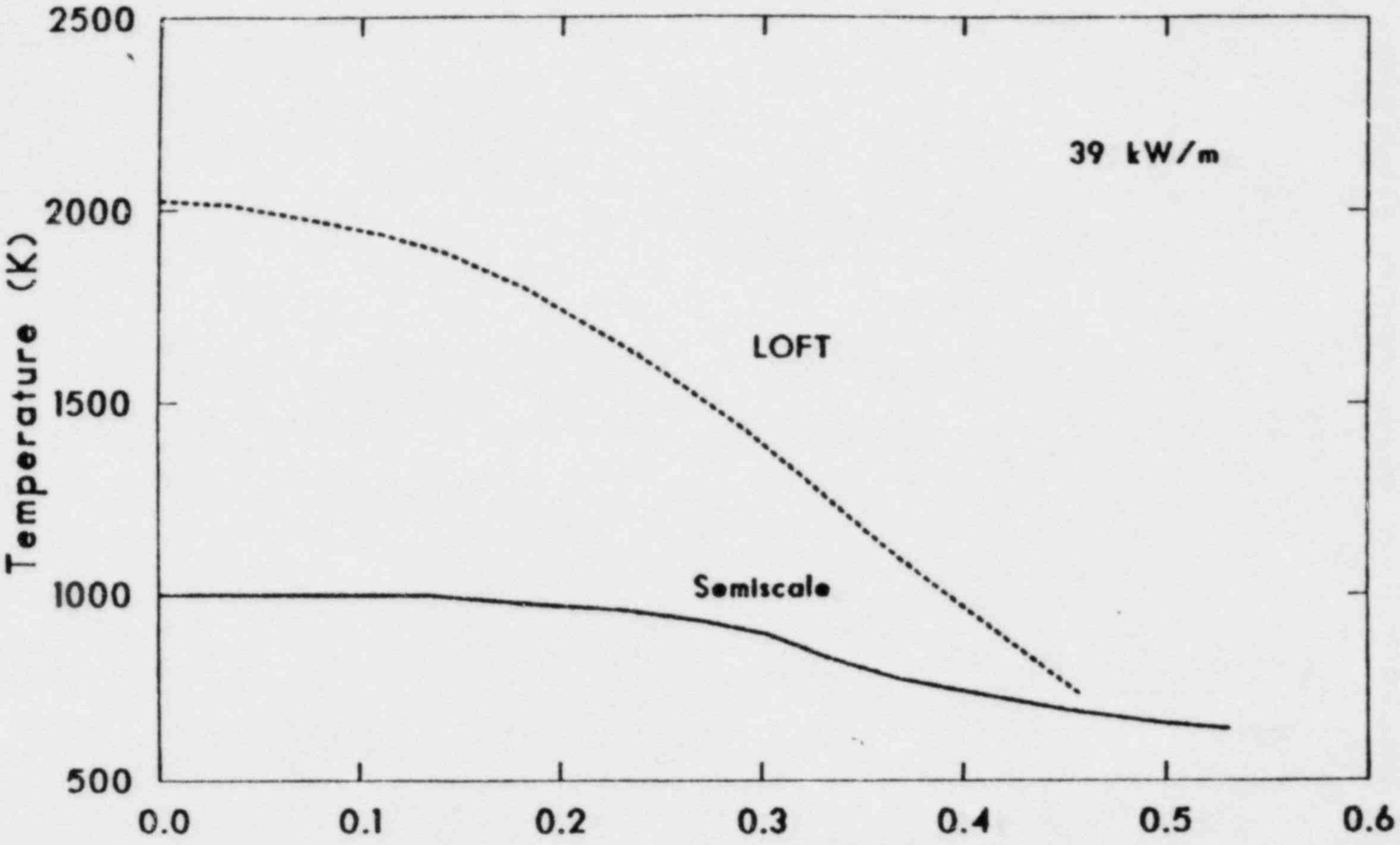
6

Comparison of LOFT and Semiscale (S-06-3, L2-3)



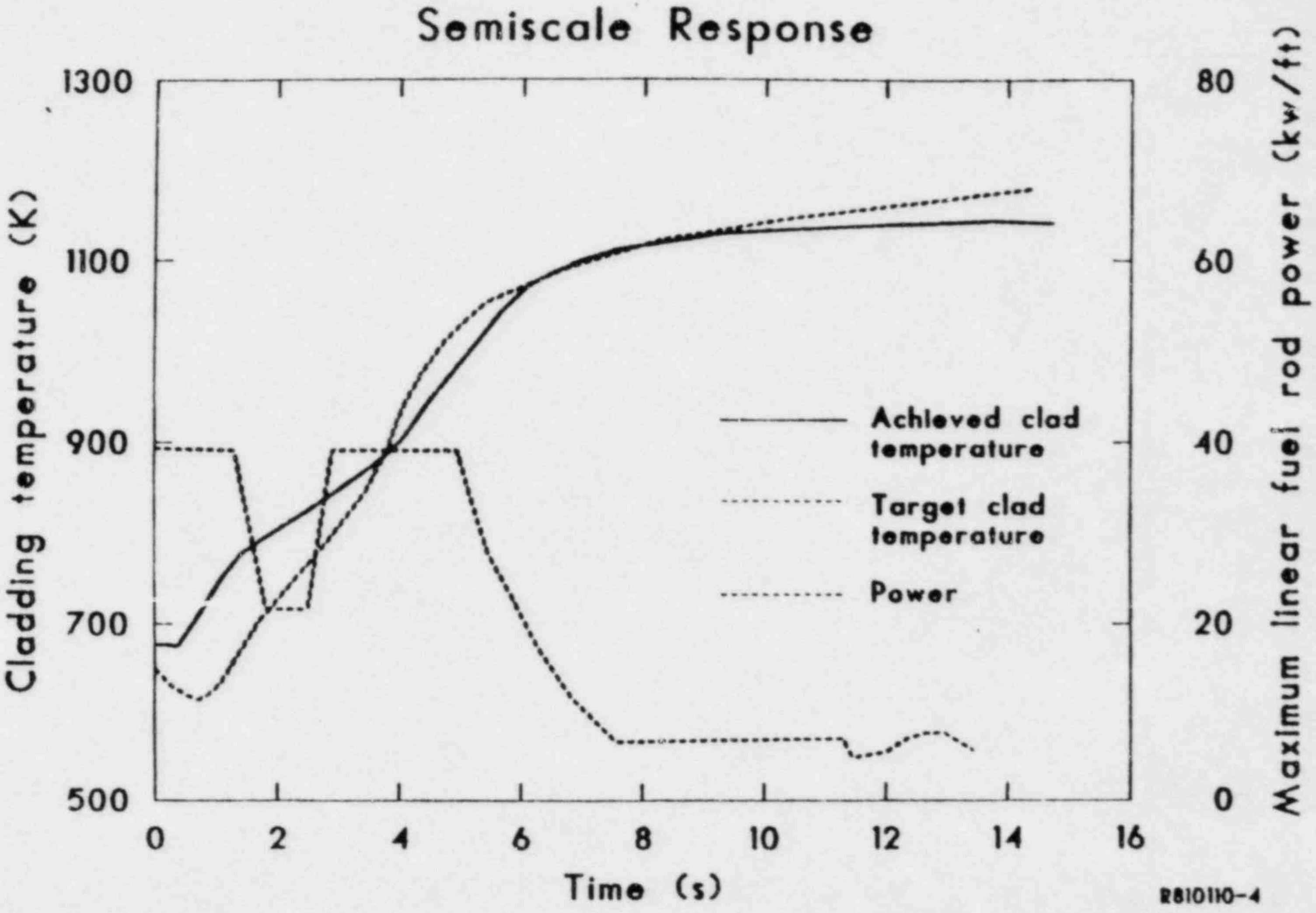
⑦

Rod Temperature Profiles



R810110-6

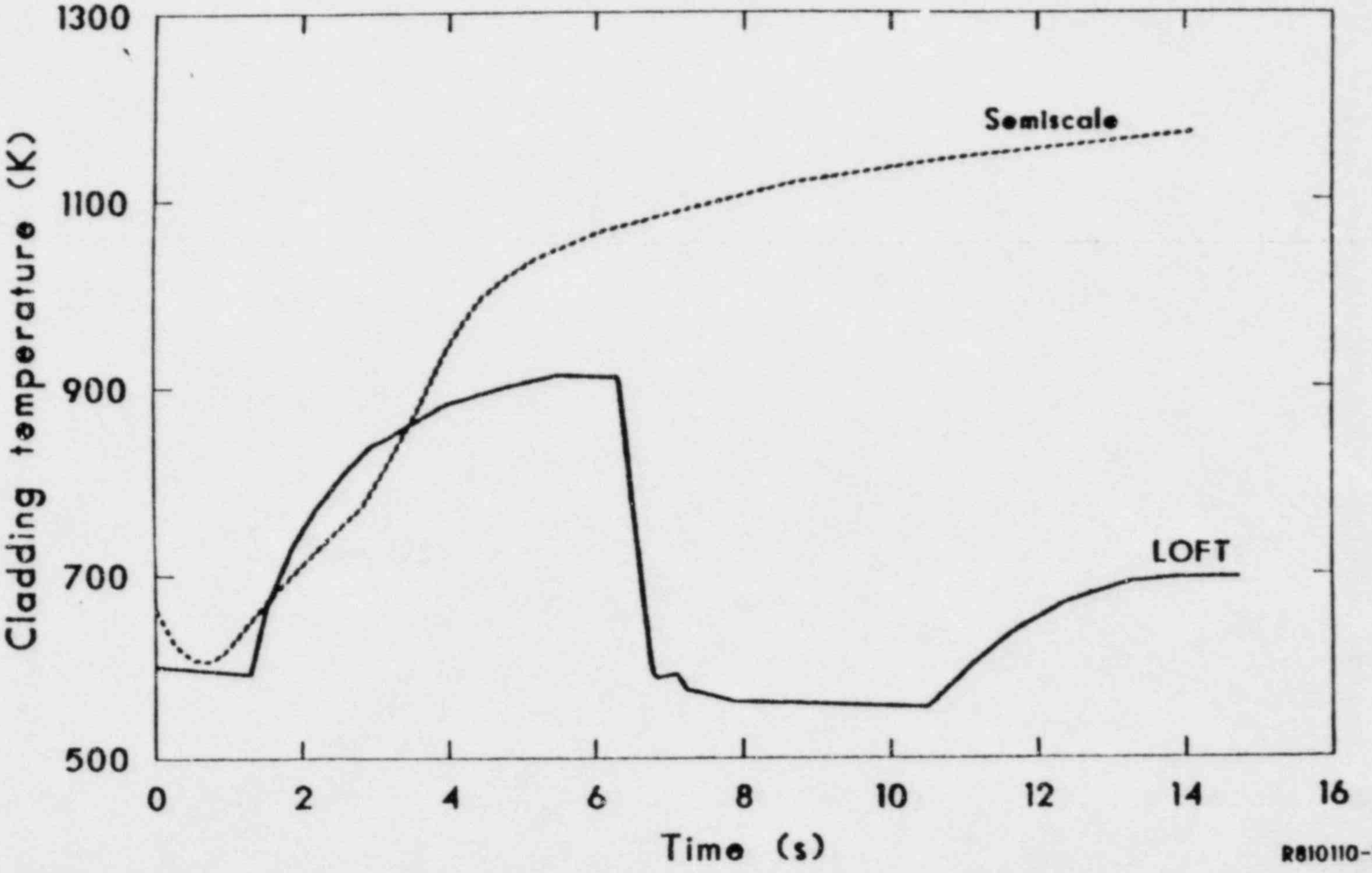
8


Semiscale Response

R810110-4

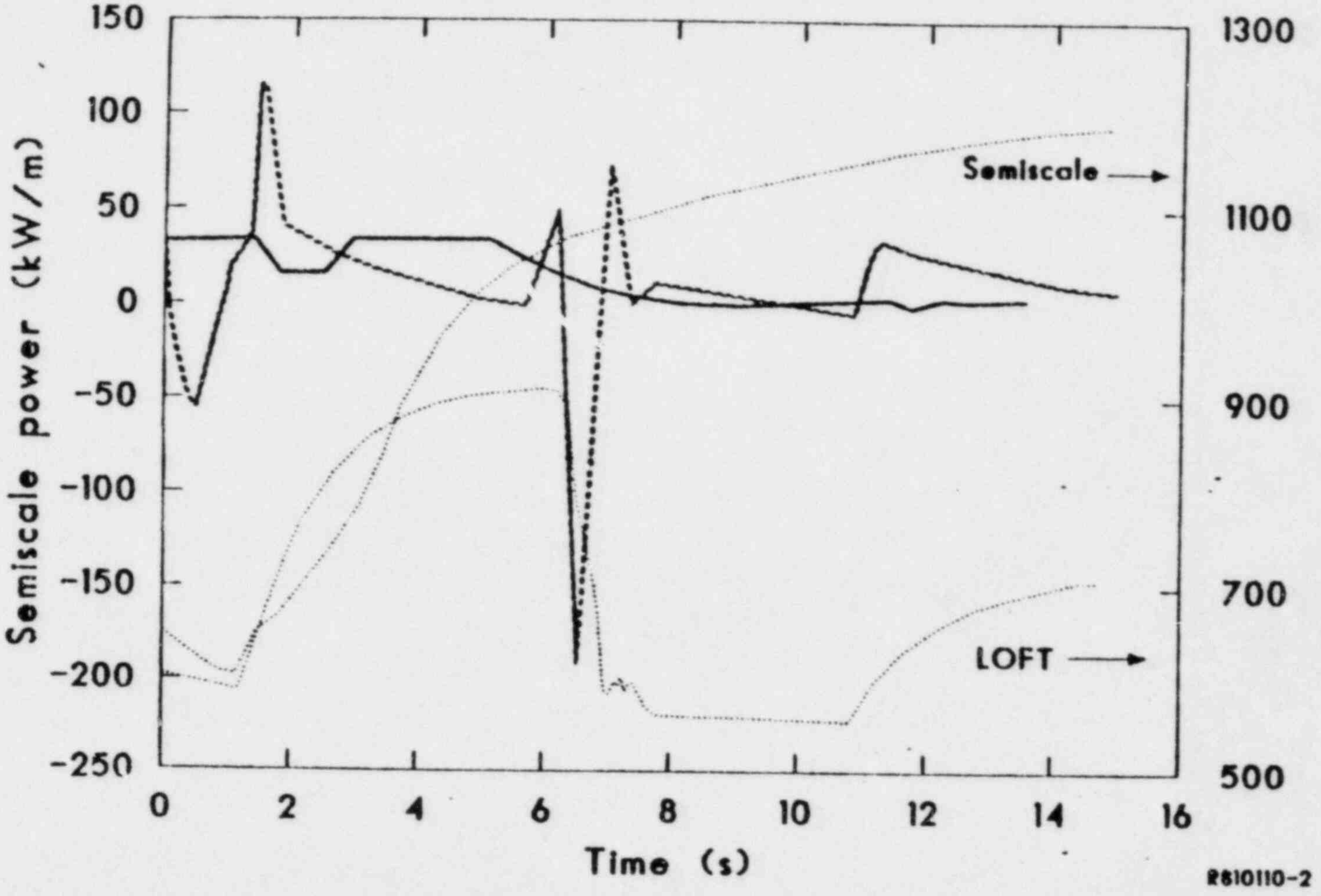
9

LOFT and Semiscale Response

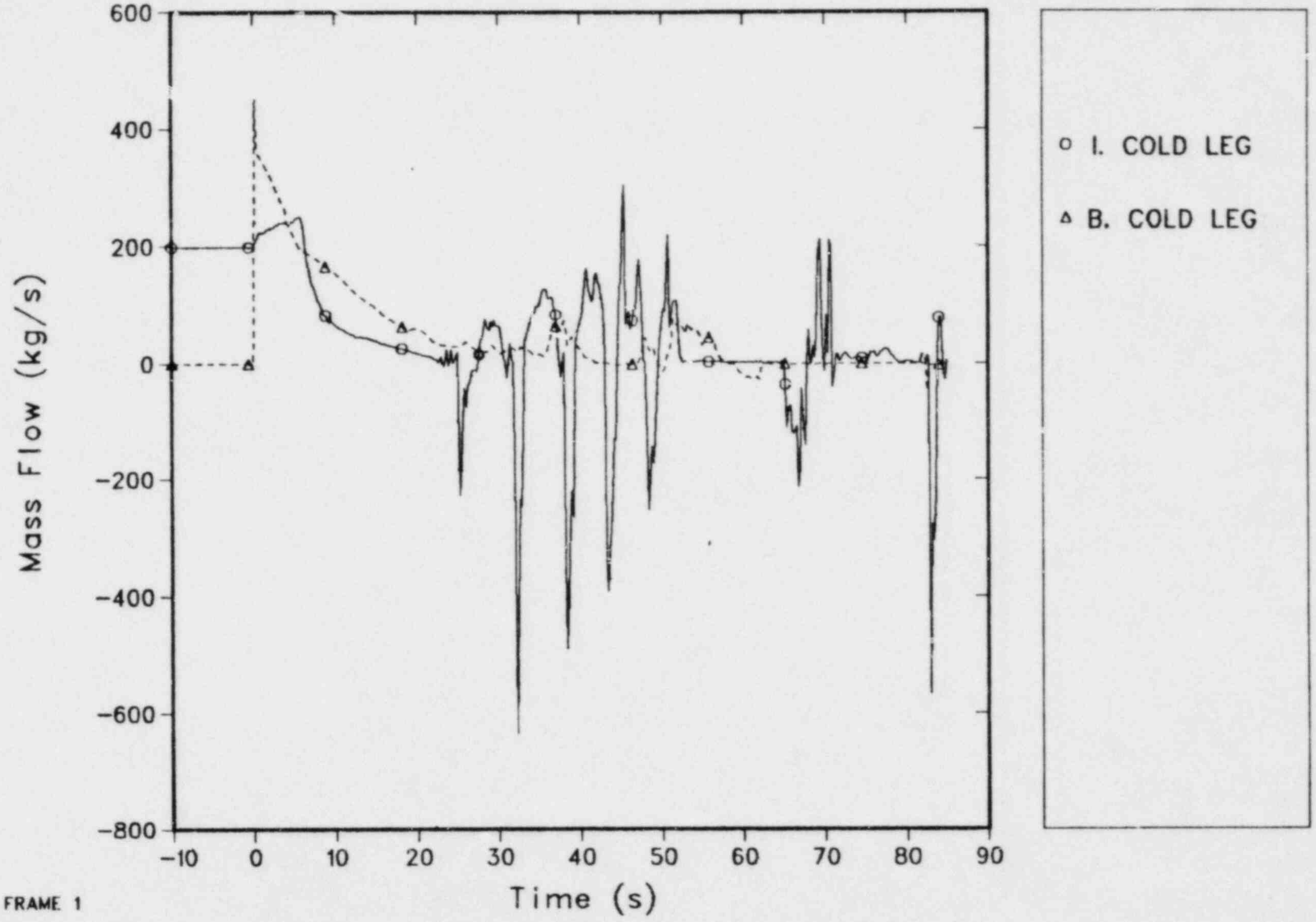


R810110-3

Semiscale Rod Power

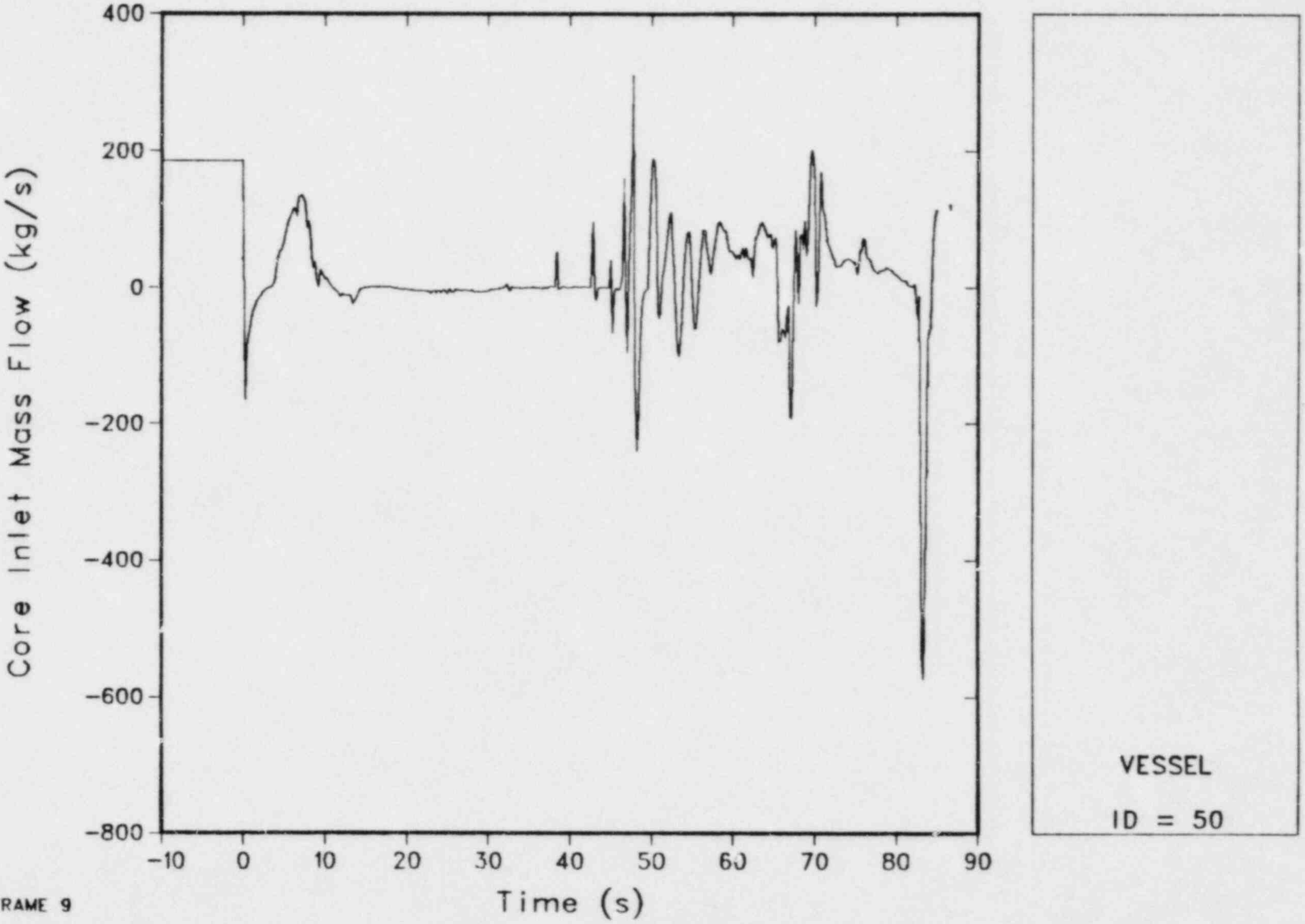


LOFT TEST L2-3 BASE CASE
TRAC-PD2/MOD1



FRAME 1

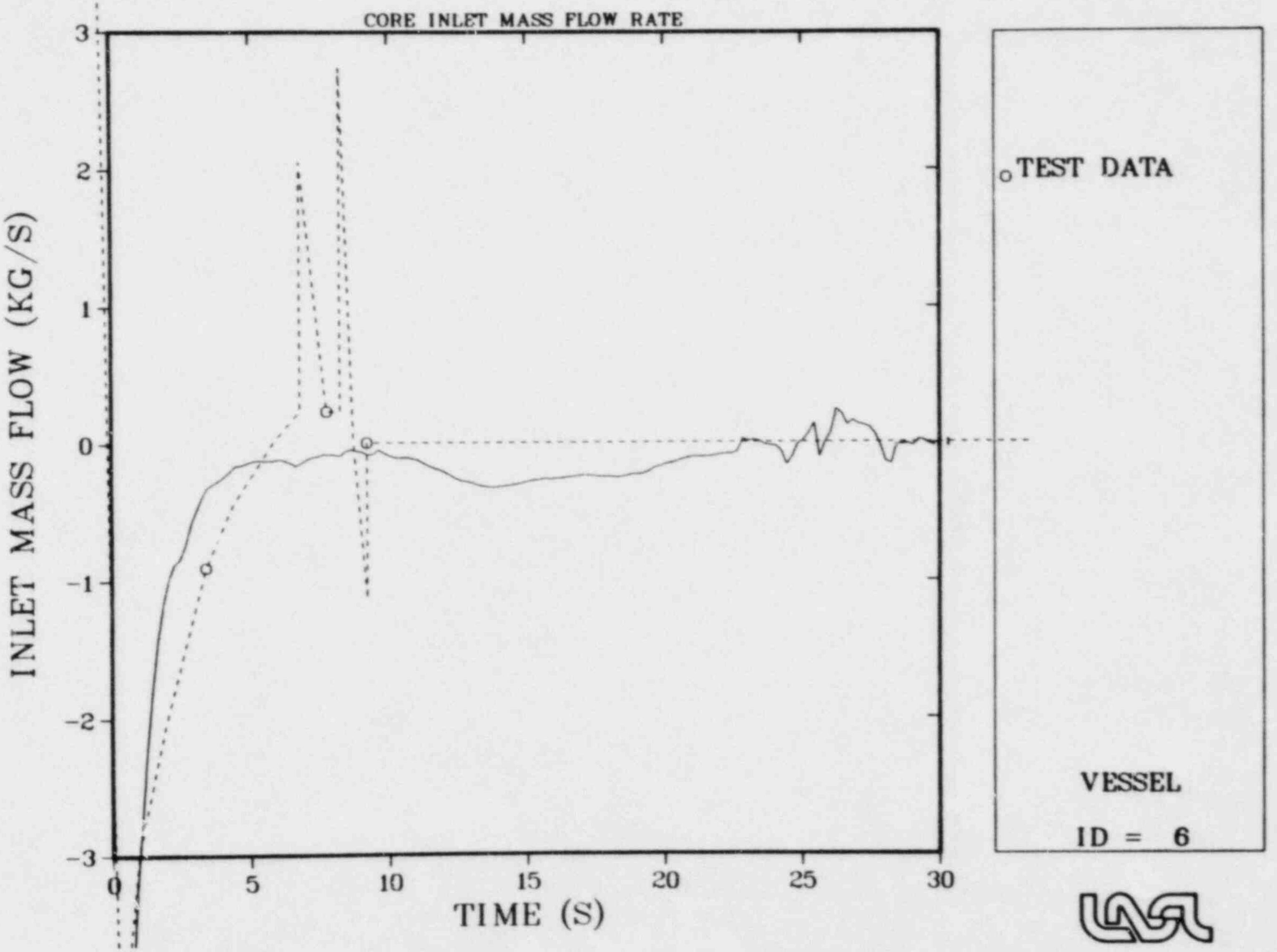
LOFT TEST L2-3 BASE CASE
TRAC-PD2/MOD1



FRAME 9

13

A COMPARISON OF TRAC PD2 RESULTS WITH SEMISCALE TEST S-06-3



Cladding Temperature Comparisons

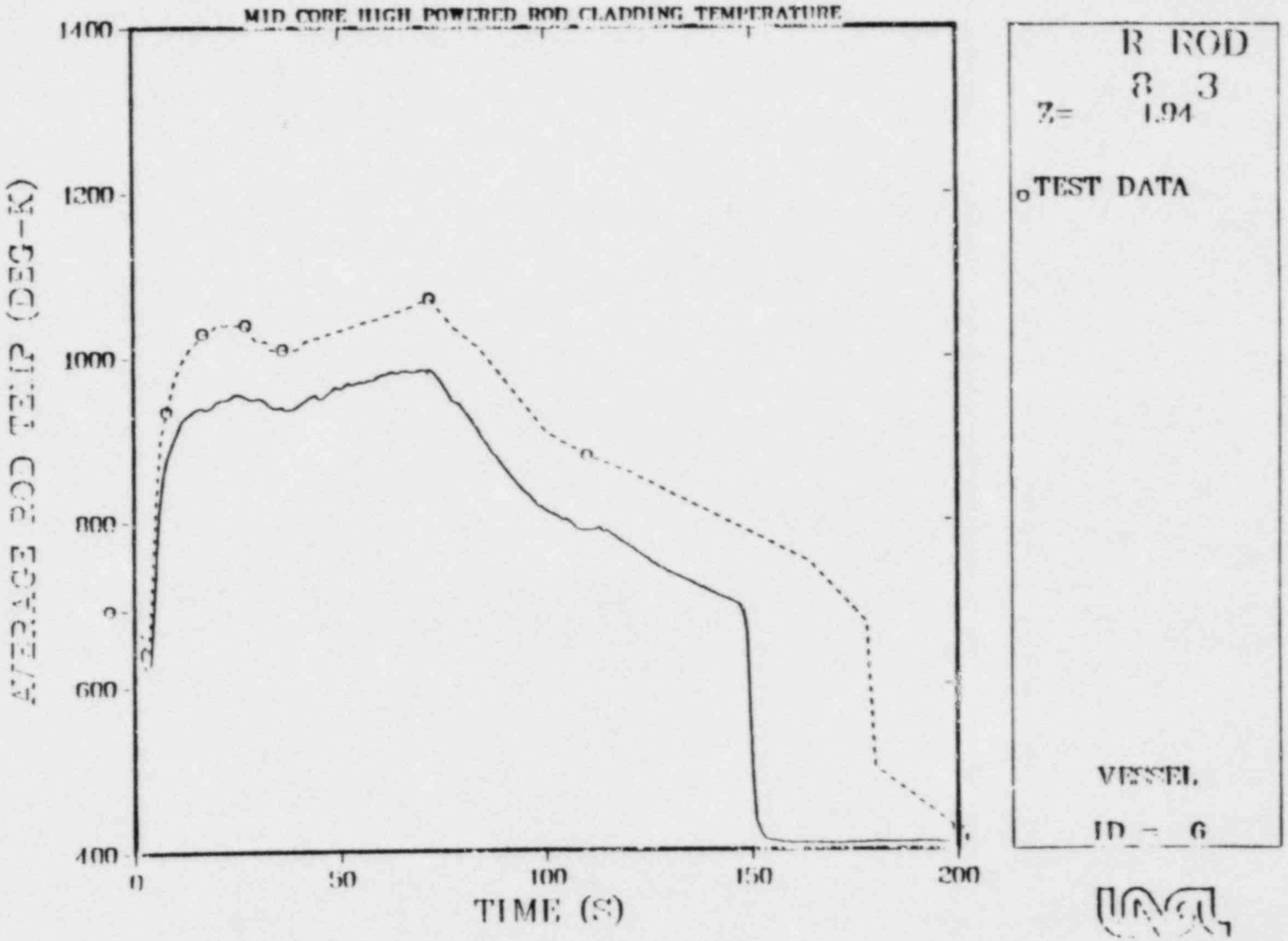
LOFT Test L2-2 - large-break LOCA at 8 kW/ft
200% double-ended cold-leg break

LOFT Test L2-3 - same as L2-2 at 12 kW/ft

Semiscale Test S-06-3 - counterpart test to L2-3

18

A COMPARISON OF TRAC PD2 RESULTS WITH SEMISCALE TEST S-06-3



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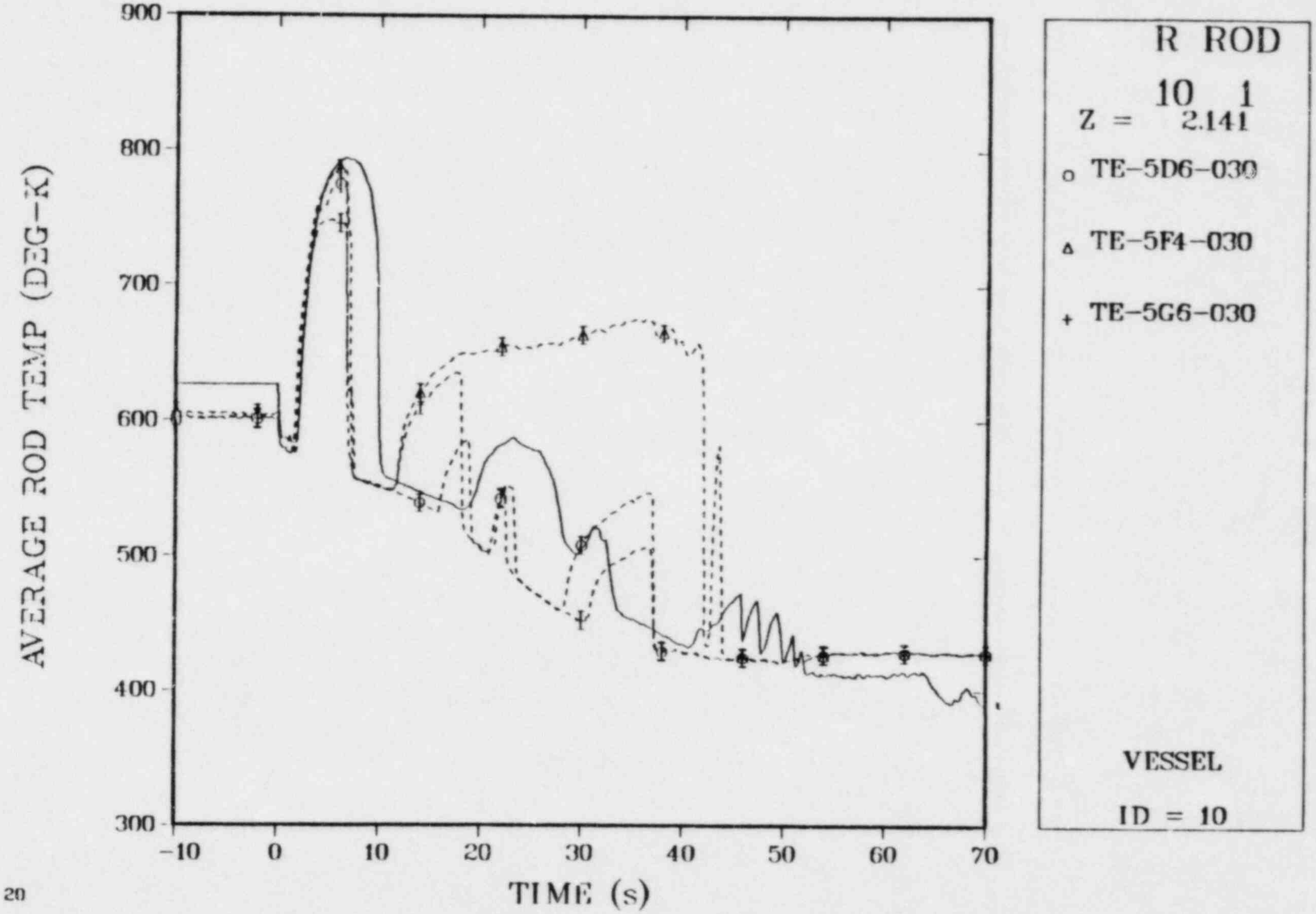
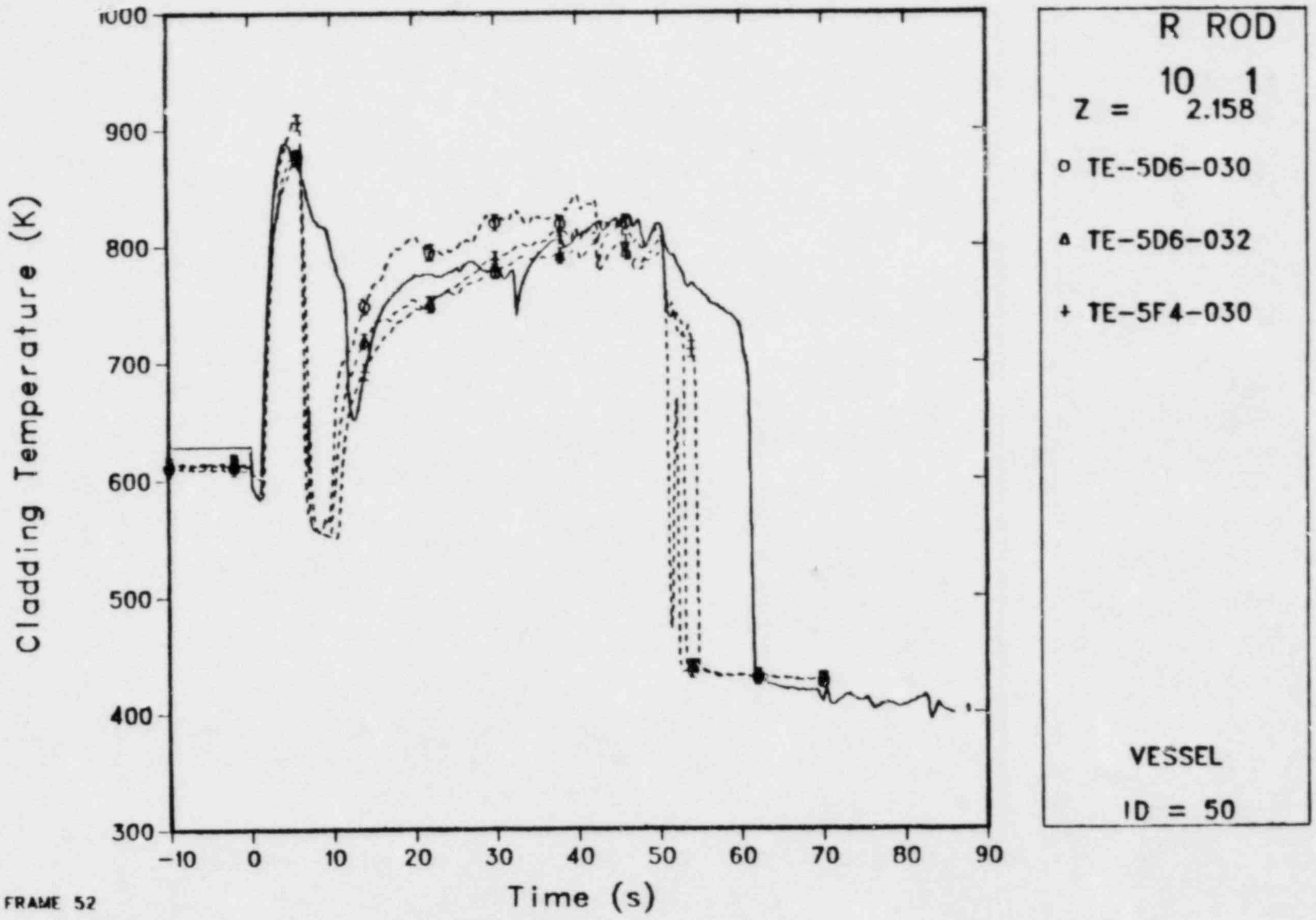
LOFT STEADY STATE TEST (INITIAL INPUT)
L2-2 INCLUDING UPPER PLENUM BLOCKAGE



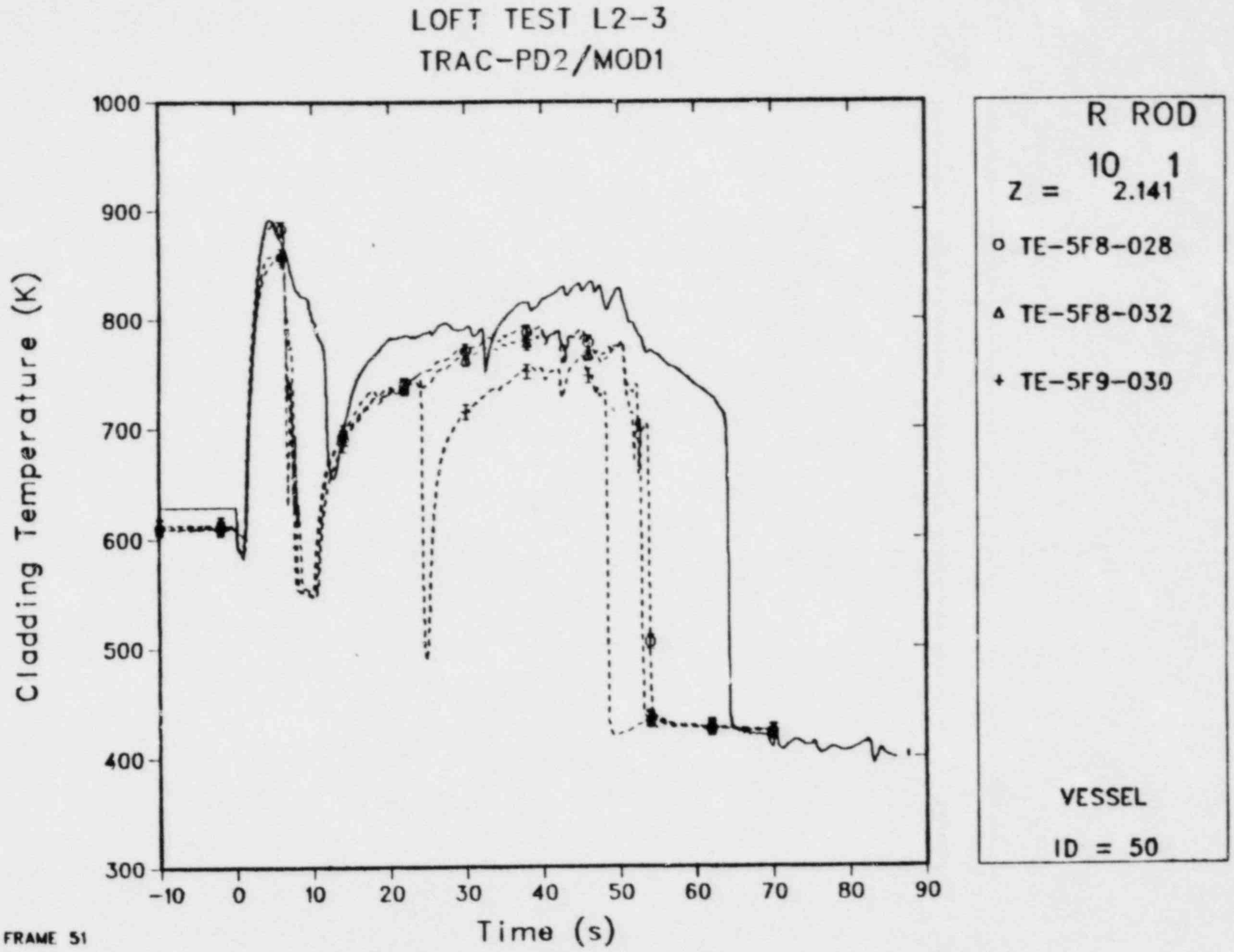
17

LOFT TEST L2-3
TRAC-PD2/MOD1



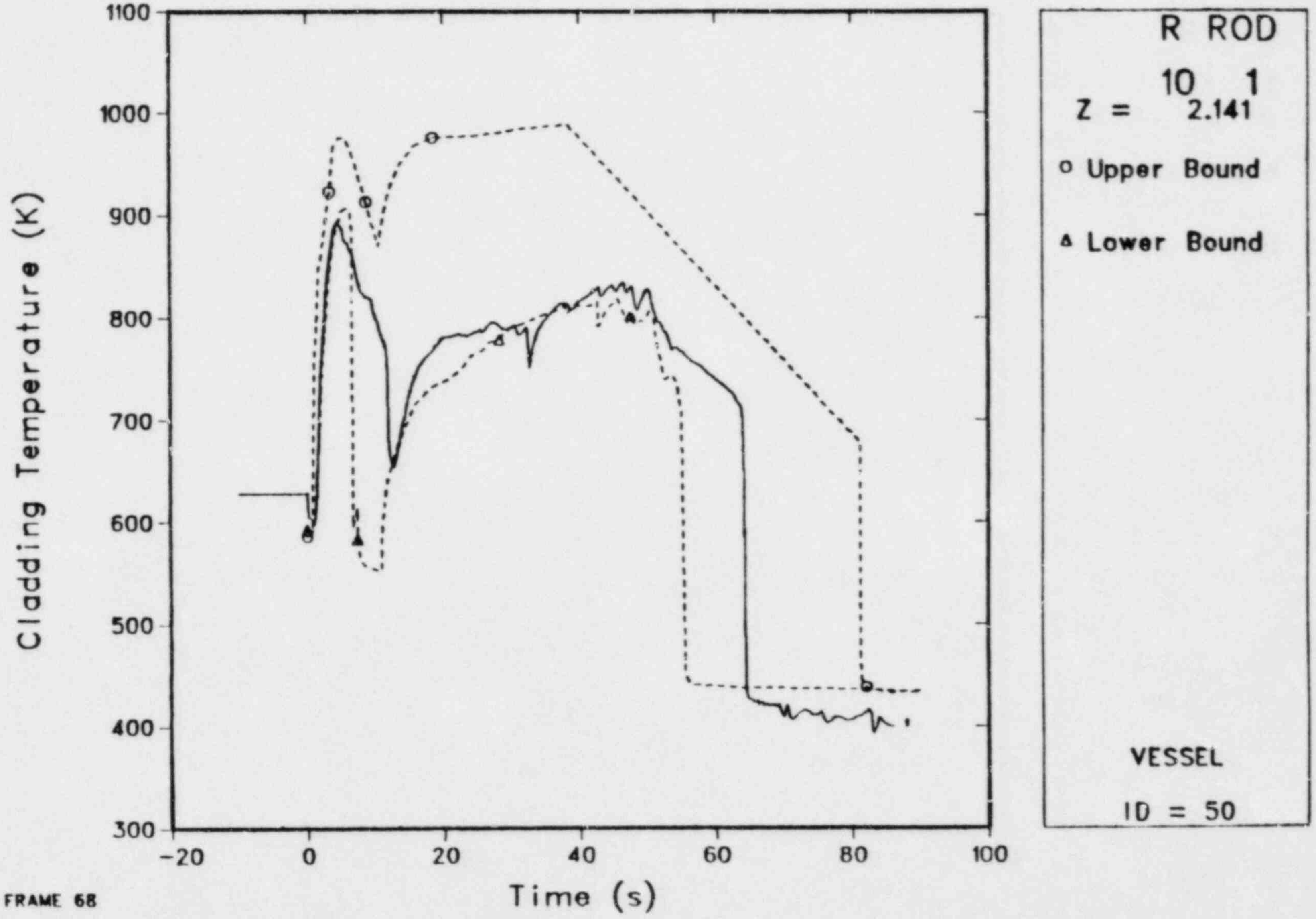
FRAME 52

⑱


LOFT TEST L2-3
TRAC-PD2/MOD1
Cladding Temperature (K)
1000
900
800
700
600
500
400
300
-10
0
10
20
30
40
50
60
70
80
90
Time (s)
R ROD
Z = 10 1 2.141
o TE-5F8-028
▵ TE-5F8-032
+ TE-5F9-030
VESSEL
ID = 50

FRAME 51

LOFT TEST L2-3
TRAC-PD2/MOD1



FRAME 68

Conclusions

We think that any problems associated with nuclear fuel rods can be resolved by adding a more detailed fuel-rod model to our code and by running a steady-state fuel-rod code to establish initial conditions. Separate-effects fuels tests should be sufficient to support code development and assessment in this area.

For the LOFT tests to date the models currently in the code are sufficient.

G. A. Pc #5 ①

GESTR - LOCA

- BACKGROUND
- DESCRIPTION OF MODEL ELEMENTS
- EXPERIMENTAL QUALIFICATION

11/82

①

GESTR - LOCA

BACKGROUND

- FUNCTION
  - INITIAL CONDITIONS AT ONSET OF EVENT
    - FUEL STORED ENERGY
    - FUEL ROD FISSION GAS INVENTORY
- APPLICATION
  - UO_2 AND UO_2 - Gd_2O_3 FUEL
  - ZIRCALOY AND ZIRCONIUM - LINED CLADDING
  - GESTR - LOCA/SAFER REPLACES GEGAP/SAFE , REFLOOD
- STATUS
  - MODEL DEVELOPMENT, QUALIFICATION COMPLETE
  - LTR SUBMITTED DECEMBER 1981
  - SECOND ROUND NRC REVIEW QUESTIONS

GAP
11/82

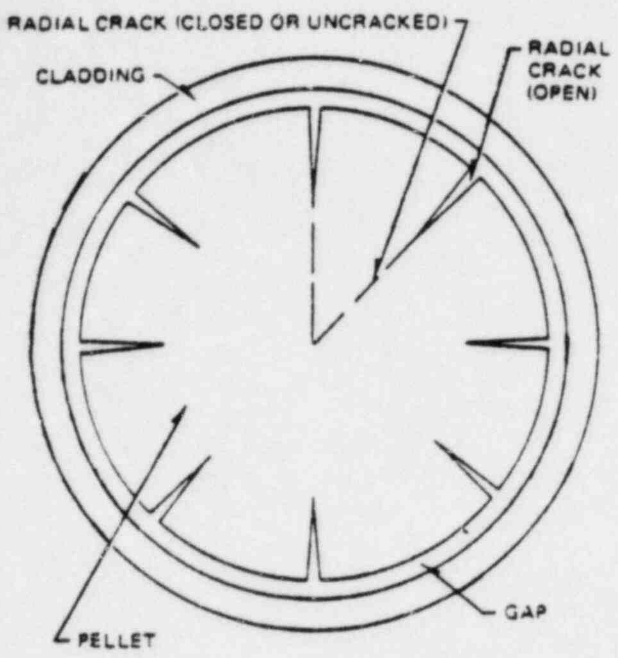
(3)

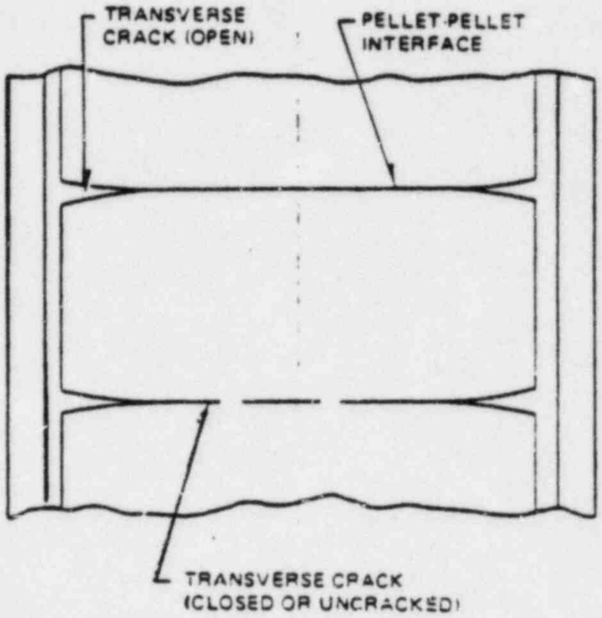
DESCRIPTION OF MODEL ELEMENTS

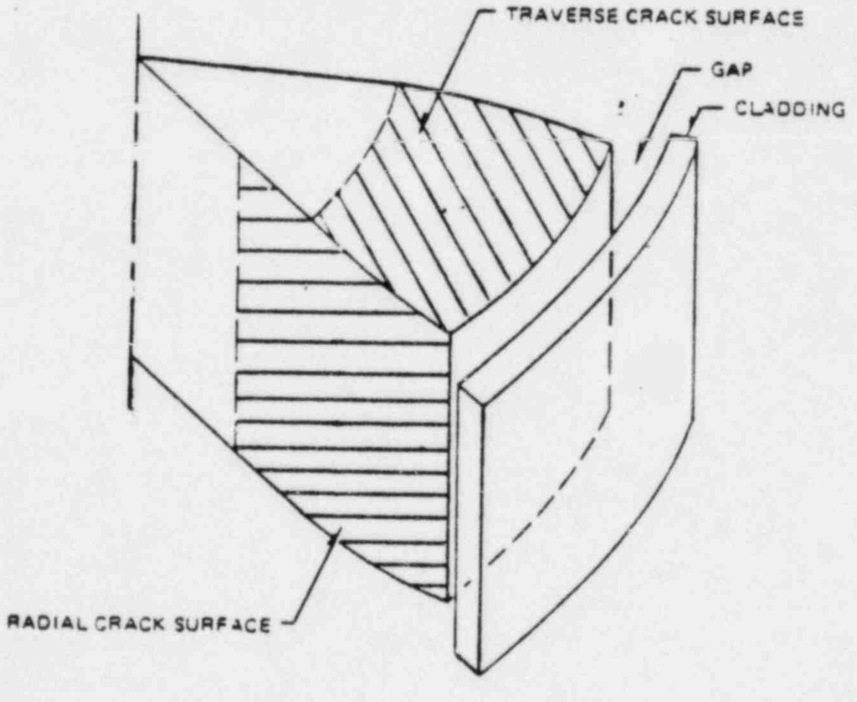
- FUEL ROD THERMAL MODEL
  - CLADDING TEMPERATURES
  - PELLET-CLADDING GAP CONDUCTANCE
  - FUEL TEMPERATURES
- FUEL ROD MECHANICAL MODEL
  - MATERIAL PROPERTIES
    - FUEL ELASTIC/PLASTIC
    - CLADDING ELASTIC/PLASTIC
  - FUEL AND CLADDING EXPANSION/DISPLACEMENT
    - FUEL AND CLADDING THERMAL EXPANSION
    - CLADDING IRRADIATION GROWTH
    - FUEL IRRADIATION SWELLING
    - FUEL DENSIFICATION
    - FUEL RELOCATION
    - FUEL AND CLADDING CREEP
    - FUEL HOT PRESSING
    - FUEL - CLADDING AXIAL INTERACTION
  - FINITE - ELEMENT MECHANICS MODEL
- FISSION GAS RELEASE
- FUEL ROD INTERNAL PRESSURE

GAP
11/82

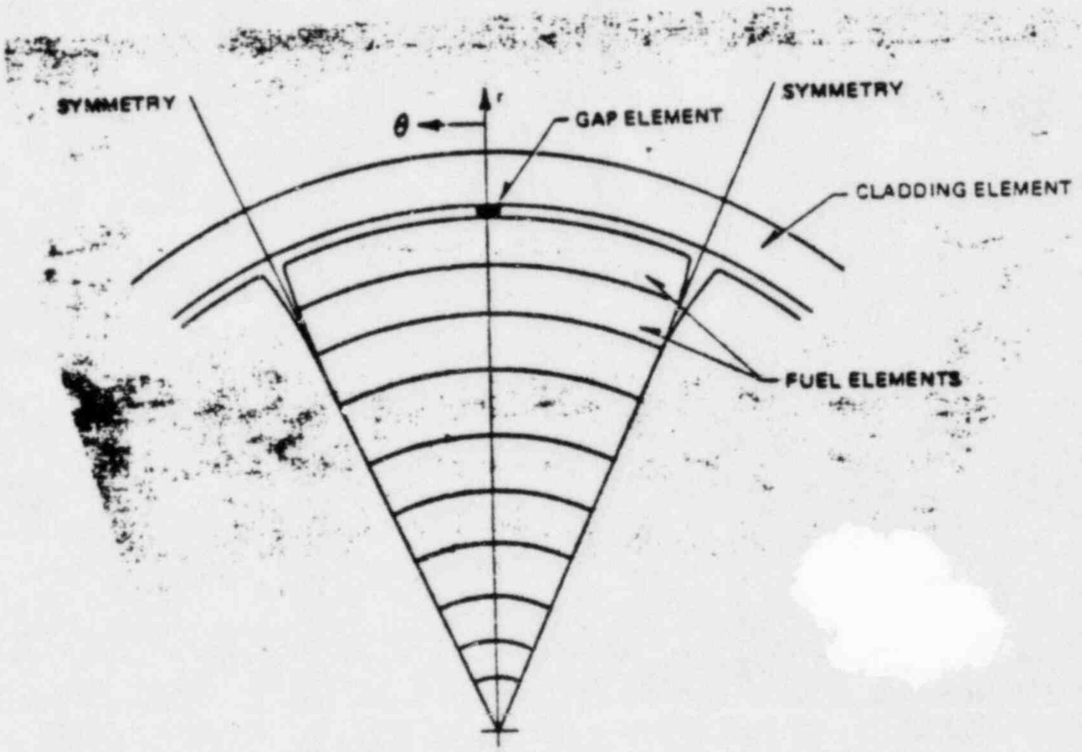
(4)


RADIAL CRACK (CLOSED OR UNCRACKED)
CLADDING
RADIAL CRACK (OPEN)
PELLET
GAP

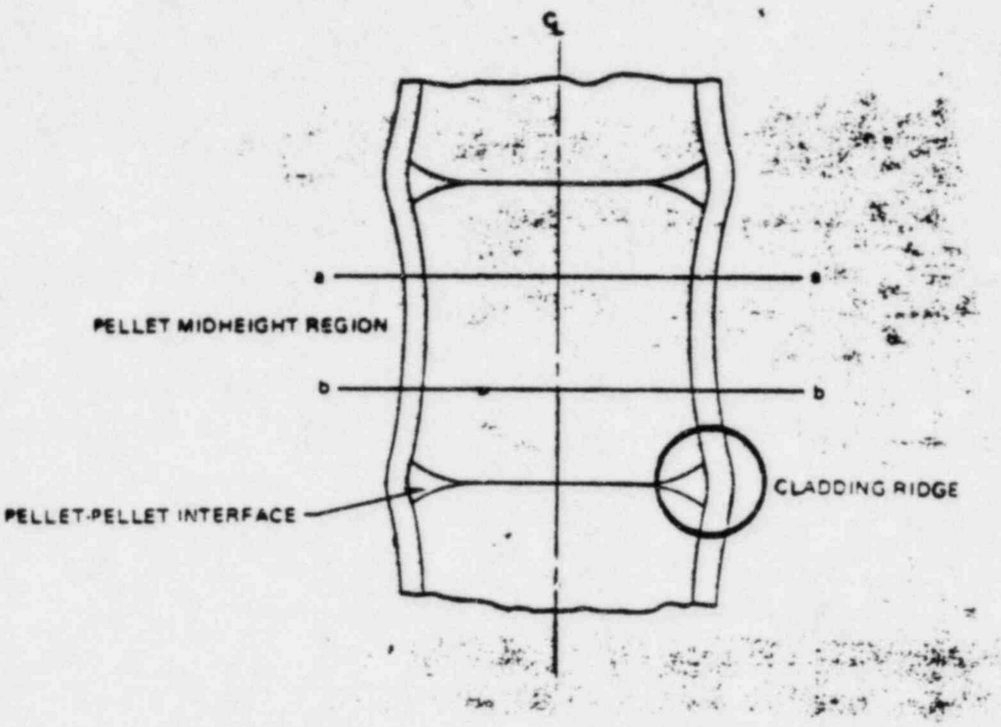

TRANSVERSE CRACK (OPEN)
PELLET-PELLET INTERFACE
TRANSVERSE CRACK (CLOSED OR UNCRACKED)


TRAVERSE CRACK SURFACE
GAP
CLADDING
RADIAL CRACK SURFACE

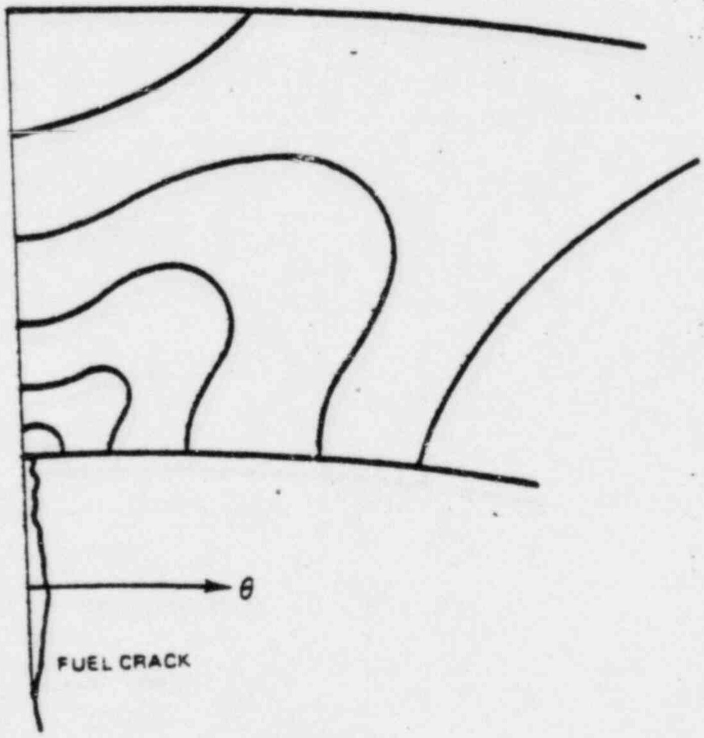
5



GESTR-LOCA Model for Fuel Rod Cross Section



Cladding Ridging (Schematic)



Cladding Hoop Strain Contours (Schematic)

(1)

EXPERIMENTAL QUALIFICATION

- THERMAL MODEL

  CONTINUOUS IN-REACTOR CENTRAL FUEL THERMOCOUPLE MEASUREMENTS

- MECHANICAL MODEL

  DIAMETER CHANGE MEASUREMENTS (MIDPLANE AND LOCAL)
  LENGTH CHANGE MEASUREMENTS

- FISSION GAS RELEASE

  FUEL ROD PUNCTURE MEASUREMENTS

- FUEL ROD INTERNAL PRESSURE

  CONTINUOUS IN-REACTOR PRESSURE TRANSDUCER MEASUREMENTS

B.S. Shiralkar ⑧

IMPROVED EVALUATION MODEL - SAFER

OVERALL DIRECTION:

Development of Physically Realistic Qualified Models.

OBJECTIVES:

- Quantify true BWR Safety Margins
- Establish Real Event Scenarios for Design/Operation Guidance.

METHODS:

TRAC-BWR: Best Estimate Model

SAFER : Realistic Evaluation Model.

BS Shiralkar
12/2/82

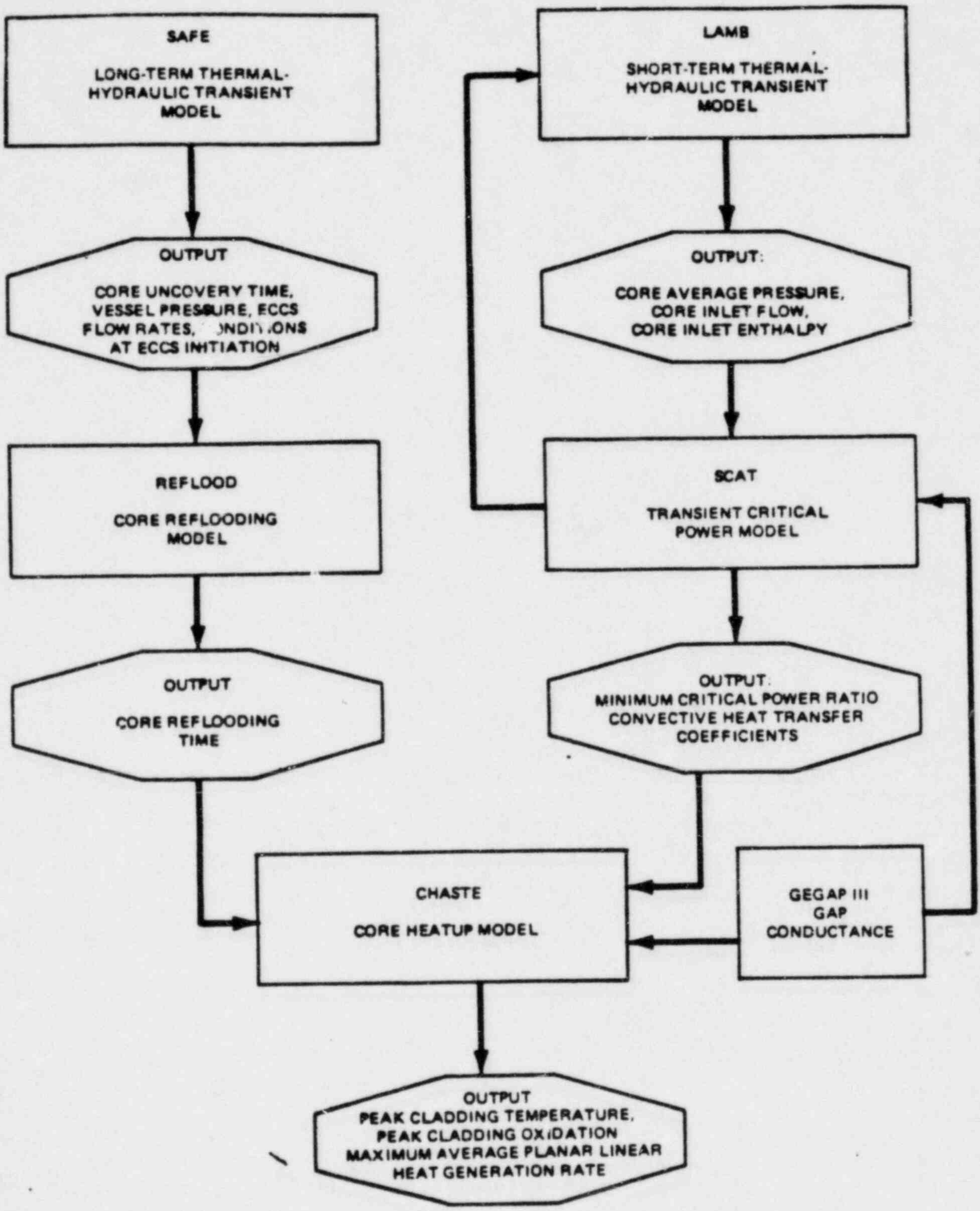
BACKGROUND

- Current LOCA EM Conservatisms
    - Inventory Distribution
    - Heat Transfer
- Excessive Conservatisms Addressed by SAFER
    - Improved Nodalization
    - Hydraulic Model Improvements
    - Realistic Heat Transfer.

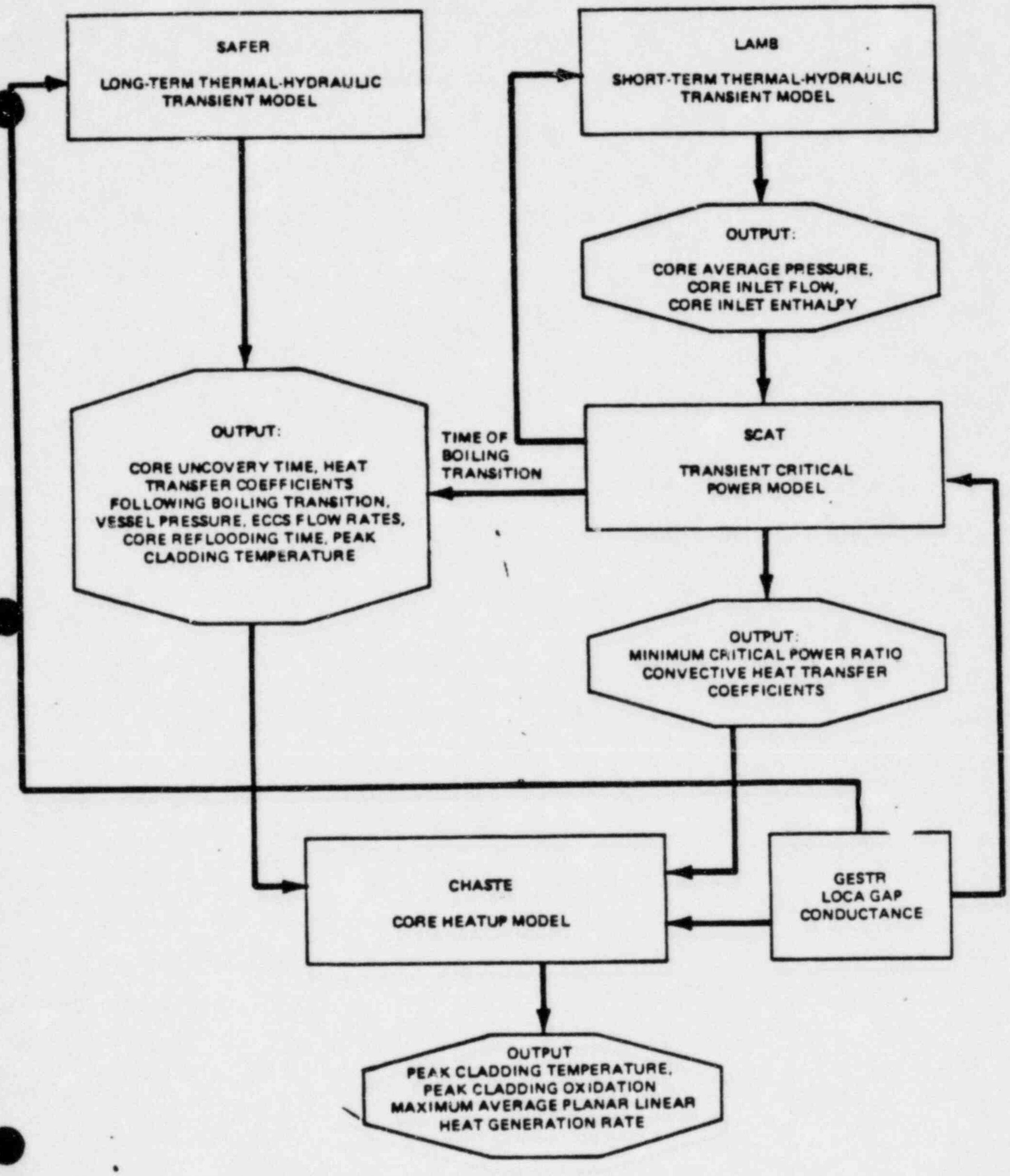
LOCA ANALYSIS EVALUATION

Application	Current Design Method	Improved Evaluation Method	Benchmark Analysis
Short Term System Blowdown	LAMB	LAMB	TRAC
Short Term Hot Channel Heat Transfer	SCAT	SCAT	
Long Term System Inventory (Refill)	SAFE REFLOOD	SAFER	
Fuel Rod Heatup	CHASTE	CHASTE (if needed)	
Fuel Rod Model	GEGAP	GESTR	

CURRENT APPLICATION



APPLICATION WITH SAFER/GESTR - LOCA



IMPROVED EVALUATION MODEL (SAFER)

- Fast Running Model for Design Application.
- Realistic Calculations
  - Includes Significant Physical Phenomena
  - Mean Value Correlations/Submodels
  - Actual Event Scenarios
- Application
  - Design/Operator Guidance
  - Appendix K Calculations with Uncertainty Adder on PCT.

MAJOR SAFER MODEL IMPROVEMENTS

- Improved Inventory Distribution
    - CCFL considered at all restrictions (including bottom of core)
    - Subcooled CCFL breakdown calculation
    - Drift flux model for sweep flow.
- Realistic Heat Transfer Coefficients
    - Steam cooling, transition boiling, etc.
- Increased Flexibility for Water Makeup System Simulation, Operator Action.
- Hot Fuel Assembly
    - Core ΔP imposed on hot channel
    - Inlet flows consistent with plenum conditions
    - Separate calculation of inventory and heatup.

SAFER MODEL IMPROVEMENTS AND IMPACT

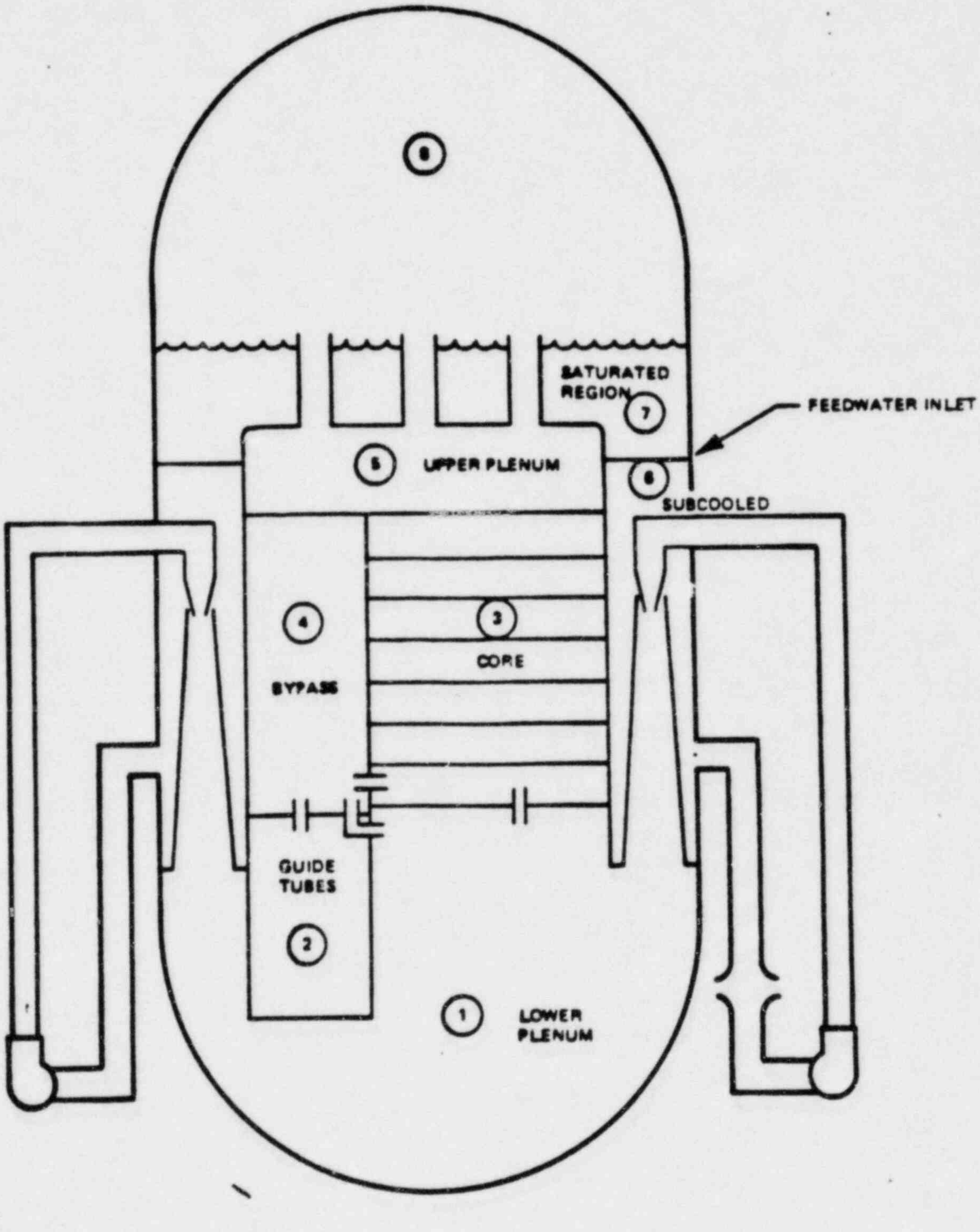
MODEL	IMPACT ON PCT
• Greater Detail and Accuracy	
- All major regions modeled	Minor
- Core axial subdivision	Minor
- Hot channel calculation	---
GESTR fuel model	Minor
- SAFE/REFLOOD interface eliminated	Minor
• Improved Inventory Distribution	
- CCFL considered at all restrictions (including bottom of core)	$\boxed{\text{Major}}$
- Subcooled CCFL breakdown calculated	$\boxed{\text{Major}}$
- Internal flow splits based on pressure differences	Minor
- Drift flux model for sweep flow	Minor
• Realistic Heat Transfer Coefficients	
- Steam cooling, transition boiling, etc.	$\boxed{\text{Major}}$
- Increased flexibility for water makeup system simulation.	---

OVERVIEW OF MODELS

- Hydraulic Models
  - Nodalization
  - Mass and Energy Balances
  - Steam Slip Models
  - Counter Current Flow Limiting
  - Break Flow
  - Overall Momentum Equations.
- Special Regional Models
  - Bypass Leakage
  - Upper Plenum
  - Hot Fuel Assembly
- External Flows
- Heat Transfer Models
  - Nodalization
  - Heat Transfer Coefficients
- Fuel Rod Models
  - Gap Conductance
  - Cladding Stress and Perforation.

HYDRAULIC MODELS

- Nodalization
  - All major regions of vessel modeled
  - Nodalization based on naturally separated regions
  - Core region axially subdivided
  - High power assembly in parallel to average core (not shown).


8
SATURATED REGION
7
FEEDWATER INLET
5
UPPER PLENUM
6
SUBCOOLED
4
BYPASS
3
CORE
GUIDE TUBES
2
1
LOWER PLENUM

HYDRAULIC MODELS

- Mass and Energy Balances
  - Separately formulated for subcooled and saturated regions
  - Thermal equilibrium between phases
  - Vapor slip with respect to liquid
  - Steam dome maintained saturated through heat transfer to liquid in downcomer and upper plenum regions. (Superheating calculated in degraded situations).
  - Average pressure rate.

$$\dot{P} = - [\dot{M}_g \; v_g + \dot{M}_f \; v_f + \dot{M}_\ell \; v_\ell + h_\ell \frac{\partial v\ell}{\partial h\ell} \; M_\ell] \; / \; [M_g \frac{dvg}{dp} + M_f \frac{dv_f}{dp} + M_\ell \frac{\partial v\ell}{\partial P}]$$

HYDRAULIC MODELS

- Steam Slip Flow Models
  - Conventional Drift Flux Model

    $$\langle j_g \rangle = \langle \alpha \rangle C_o \langle j \rangle + \langle \alpha \rangle \bar{V}_{gj}$$

  - Drift Flux Parameters C_o, $\bar{V}_{gj}$ correlated from steady state data.
  - At low flow rate, transition made to Wilson bubble rise correlation.
  - Void profile in large regions

    $$\frac{\alpha e}{\langle \alpha \rangle} = f\left(\frac{W_{gin}}{\dot{m}_{fg}}, \frac{W_{fin}}{\dot{m}_{fg}}, \frac{A \bar{V}_{gj}/C_o}{\dot{m}_{fg} V_{fg}} \right)$$

  - Exit fluxes of vapor and liquid based on exit void fraction.
  - Entrainment from level

    $$W_{ent} = f(\rho_g v_g^2)$$

COUNTER CURRENT FLOW LIMITING (CCFL)

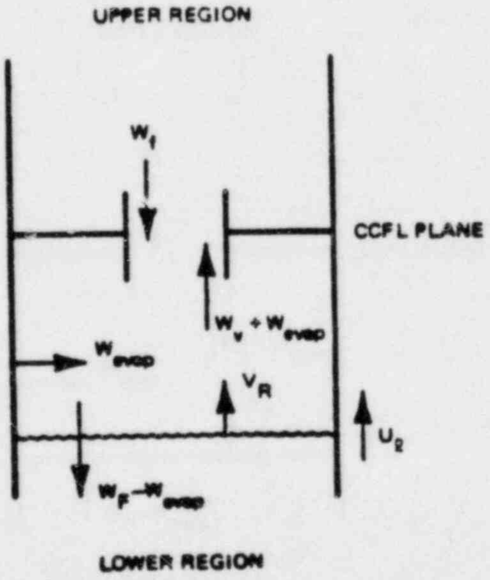
- Physical Phenomenon
- Modified Wallis Correlation

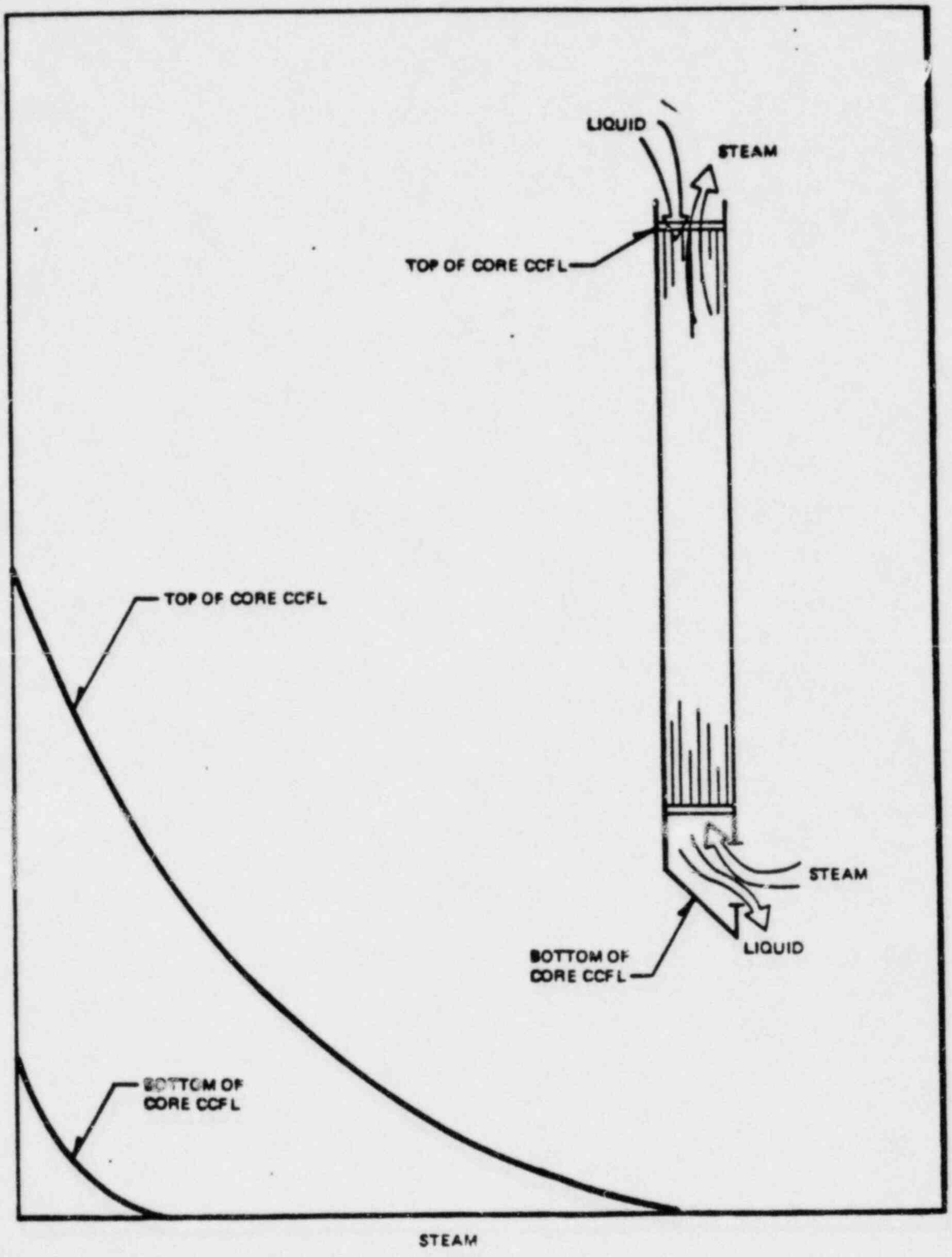
$$(j_g^*)^{1/2} D^{1/4} + K_1 (j_f^*)^{1/2} D^{1/4} = K_2 D^{1/4}$$

$$j_g^* = j_g \left[\frac{\rho_g}{g_c\ D(\rho_f - \rho_g)}\right]^{1/2}$$

$$j_f^* = j_f \left[\frac{\rho_f}{g_c\ D\ (\rho_f - \rho_g)}\right]^{1/2}$$

- Vapor flow adjusted for level movement, evaporation and condensation.
- Application

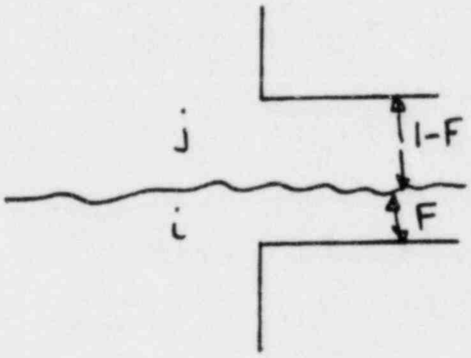




COUNTER CURRENT FLOW LIMITING

BREAK FLOW MODEL

- Moody Choked Flow Model
  - Homogeneous for realistic calculations
  - Slip for sensitivity studies.
- Break Flow Enthalpy



$$h_{break} = \frac{\frac{h_i F}{v_i} + \frac{h_j (1-F)}{v_j}}{\frac{F}{v_i} + \frac{(1-F)}{v_j}}$$

OVERALL MOMENTUM EQUATION SOLUTION

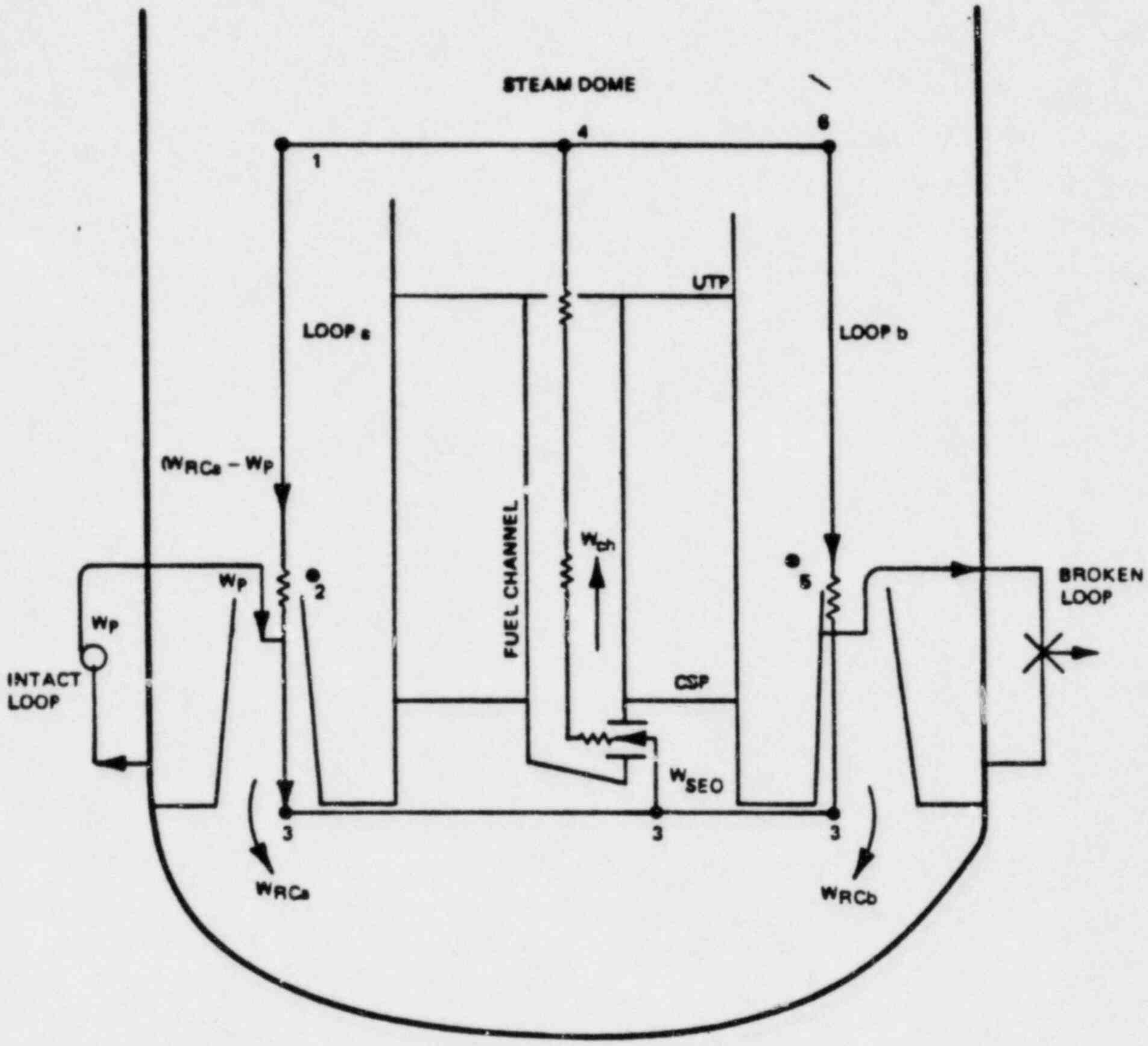
- Loop Momentum Equations for each bank of jet pumps

  $$\Sigma \Delta P_{i-j} = 0$$

- Initial pump coastdown transient approximated

- Solve simultaneously with lower plenum mass/volume balance for lower plenum flow splits between core and jet pumps.

- Flow conditions determined by mixture conditions in donor region. (Downcomer, lower plenum, or core).

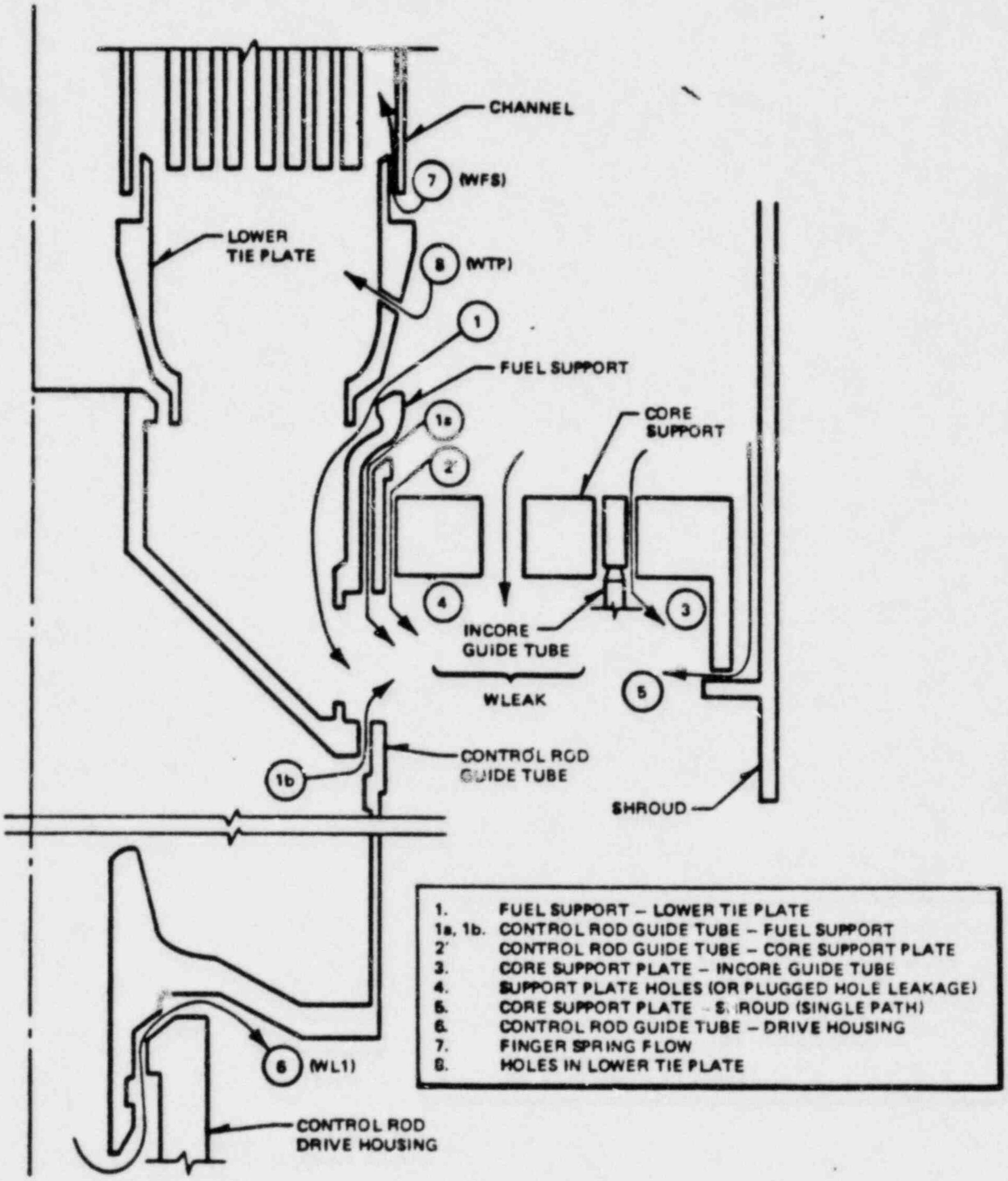


RECIRCULATION AND CORE FLOW LOOPS

SPECIAL REGIONAL MODELS

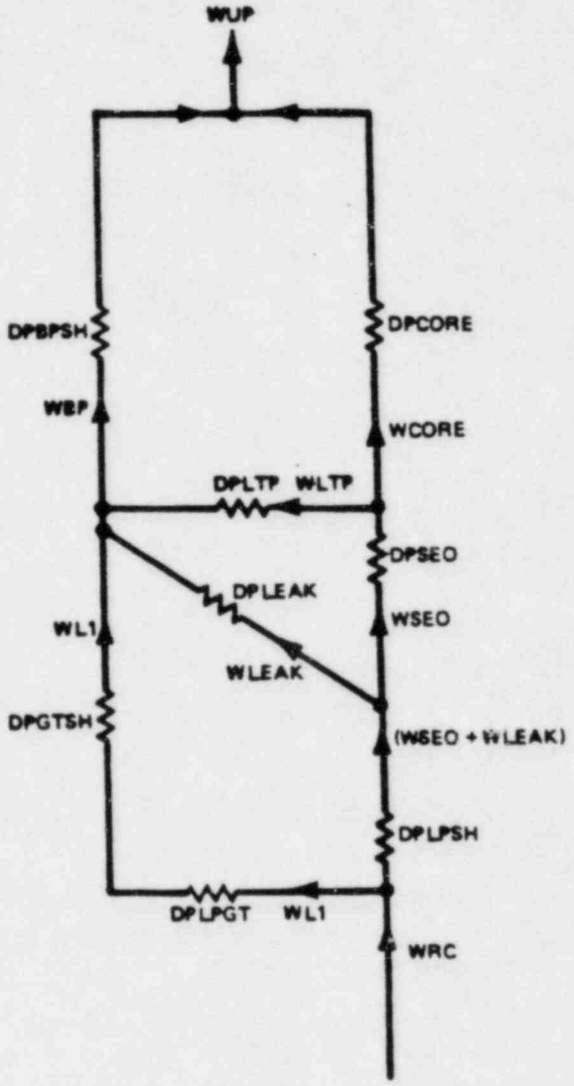
Bypass Leakage Flow

- Important BWR Feature
  - Bypass Inventory Hastens Core Cooling
- Leakage Flow Paths
- Flow Diagram and Pressure Differentials.



LEAKAGE FLOW PATHS

78



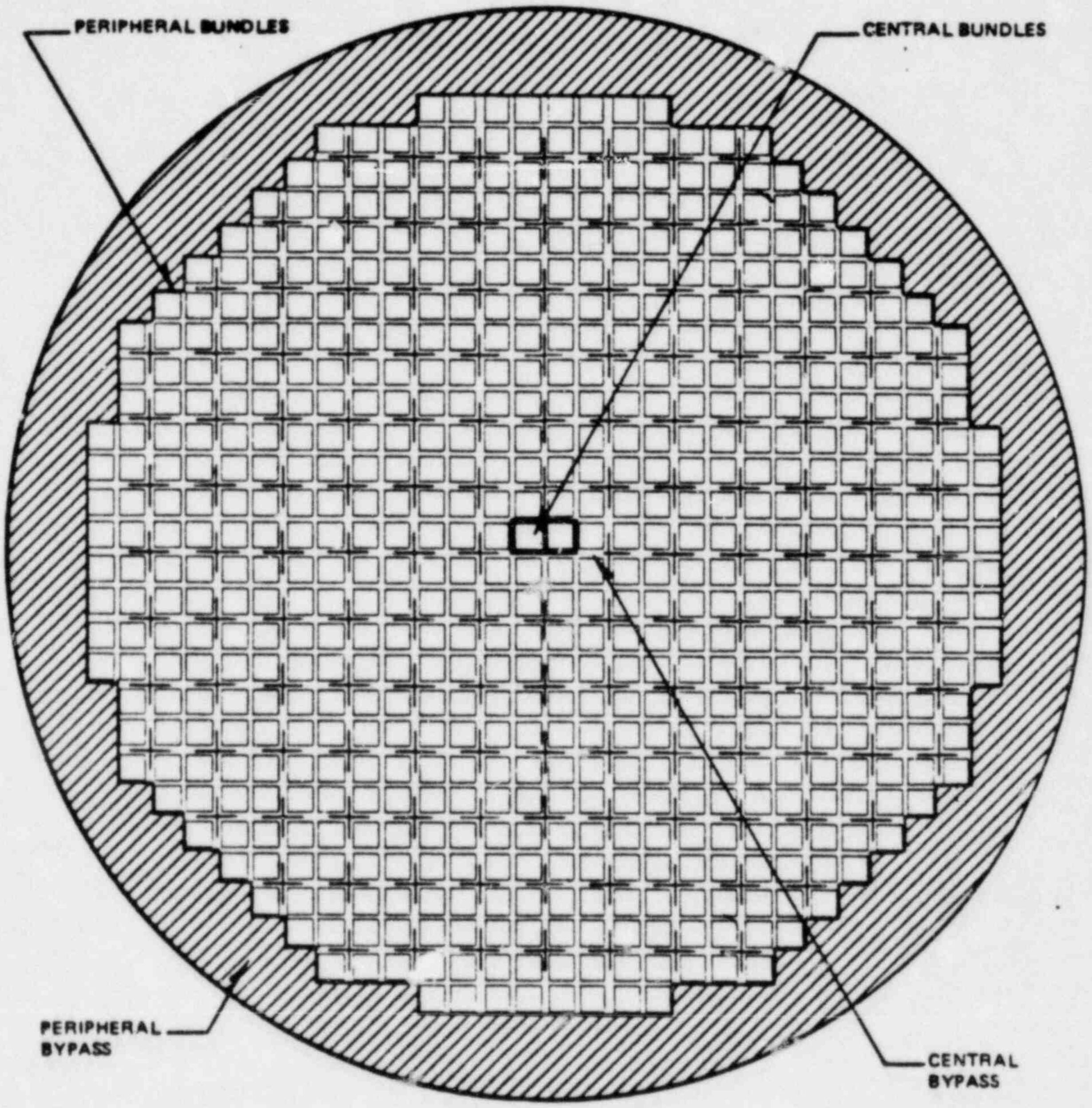
FLOW DIAGRAM FOR LEAKAGE FLOW

SPECIAL REGIONAL MODELS

UPPER PLENUM

- Subdivided into Core, Peripheral Bypass, and Central Bypass for CCFL Calculations.
- Available Flow to Each Region Based On:
  - Static head in plenum when two-phase mixture is present.
  - Core spray inflow.
- Based on Data No CCFL Assumed at Top of Bypass.
- Condensation Efficiency of Core Spray
  - Mixing length defined (~1 ft.) below sparger.
  - Mixture level below mixing length:
    Condensation efficiency 100%.
  - Mixture level within mixing length:
    Condensation efficiency ramped to zero.
  - Mixture level above sparger:
    Subcooled spray mixed with two-phase mixture.

(30)



Subdivision of Core and Bypass Regions

HOT FUEL ASSEMBLY CALCULATION

- Core ΔP Imposed on Hot Channel
  - However, no feedback from hot channel on system conditions.
- Inlet Flows Consistent with Lower Plenum Conditions.
- Separate Calculation of Inventory and Heatup.
- Liquid Downflow to Hot Channel Based on Upper Plenum Inventory and Spray Distribution.

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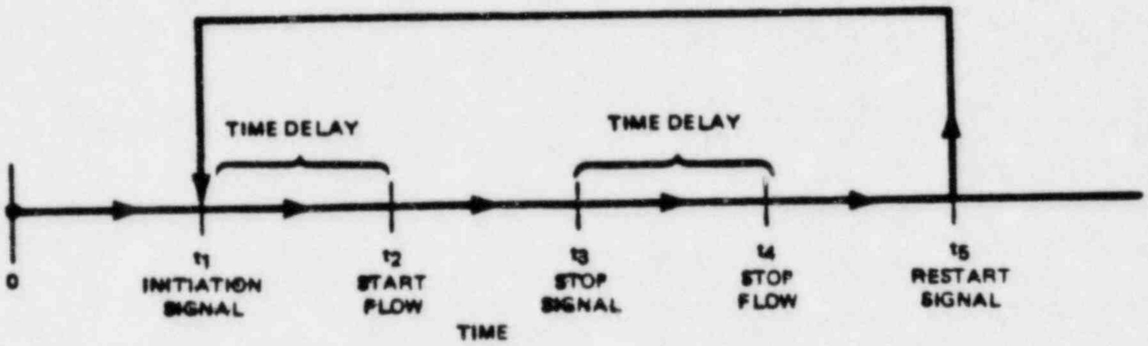
EXTERNAL FLOWS

- Water Makeup Systems
  - Flexibility of Location and Restarts
- Safety Relief Valves and ADS Function
  - Multiple Groups for Realistic Simulation
- Main Steam Line Flow
  - Turbine Admission Valve Simulation
  - MSIV Closure.


TIME DELAY
TIME DELAY
0
t1
INITIATION SIGNAL
t2
START FLOW
t3
STOP SIGNAL
t4
STOP FLOW
t5
RESTART SIGNAL
TIME

HEAT TRANSFER MODELS

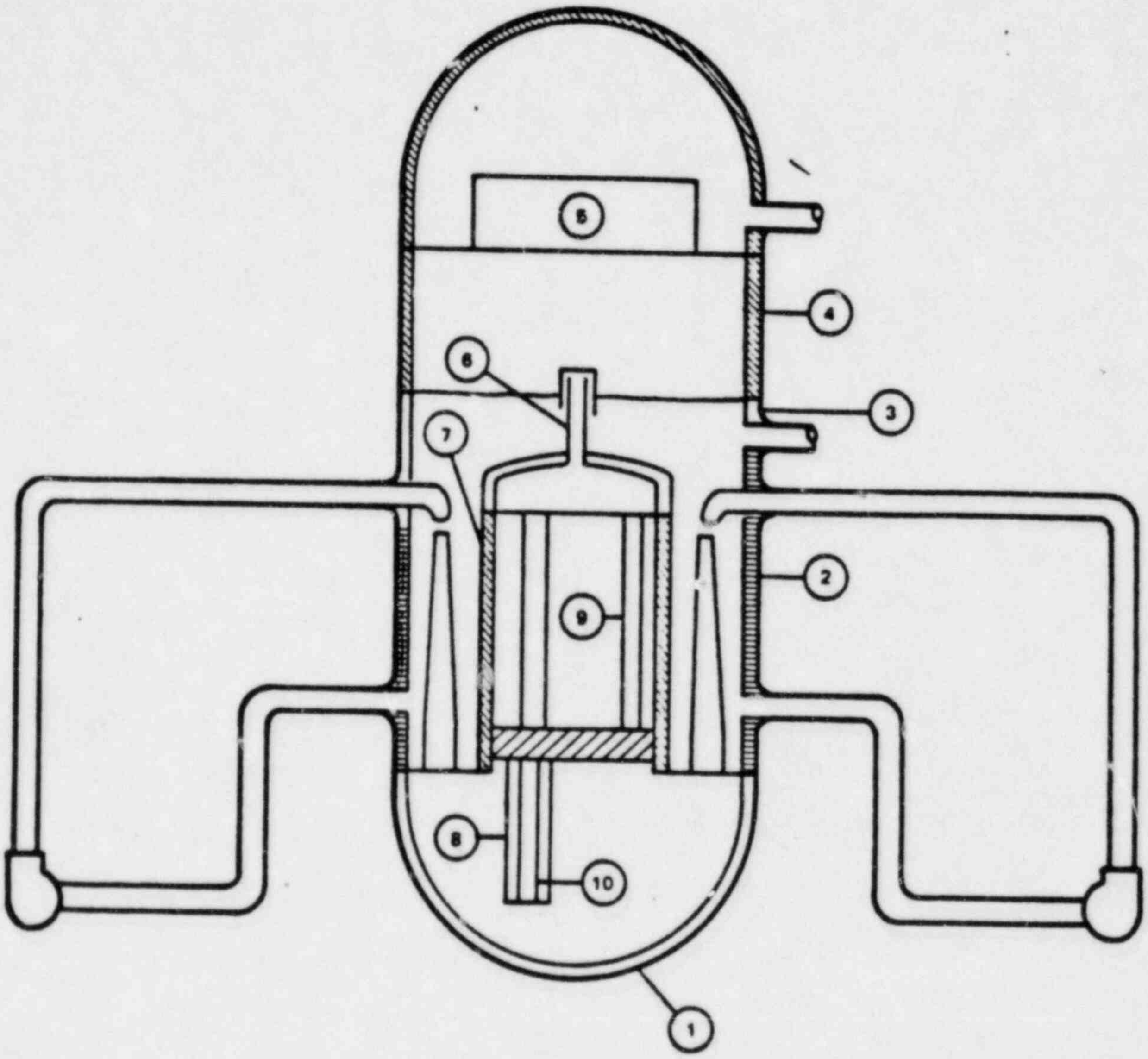
- Fuel and Cladding
  - Accounts for Peaking Factors, Radial Distribution, Gap Conductance, etc.
- Vessel and Internals - Heat Slabs
- Heat Source Distribtuion
  - Decay Heat
  - Bundle/Rod Peaking
  - Gamma Smearing
  - Metal-Water Reaction
- Gap Conductance
  - Initialized by Detailed Steady State Model

    (Fission Gas Quantities, Gap Conductance, Gap Size)
  - Dynamic Gap Conductance Calculation

    Accounts for Changes in Internal Pressure, Gap and Fuel/Clad Temperatures.

CORE HEAT SLAB REPRESENTATION

- Two Rods Modeled in Both High Power and Average Bundle
  - Average power rod for heat flux to the fluid.
  - Highest power rod for calculation of Peak Cladding Temperature.
- Heated Region of Each Rod Divided into 5 Axial Segments.
- Up to 10 Radial Fuel and Cladding Nodes with 1-D Radial Conduction Calculations.



HEAT SLAB NODALIZATION

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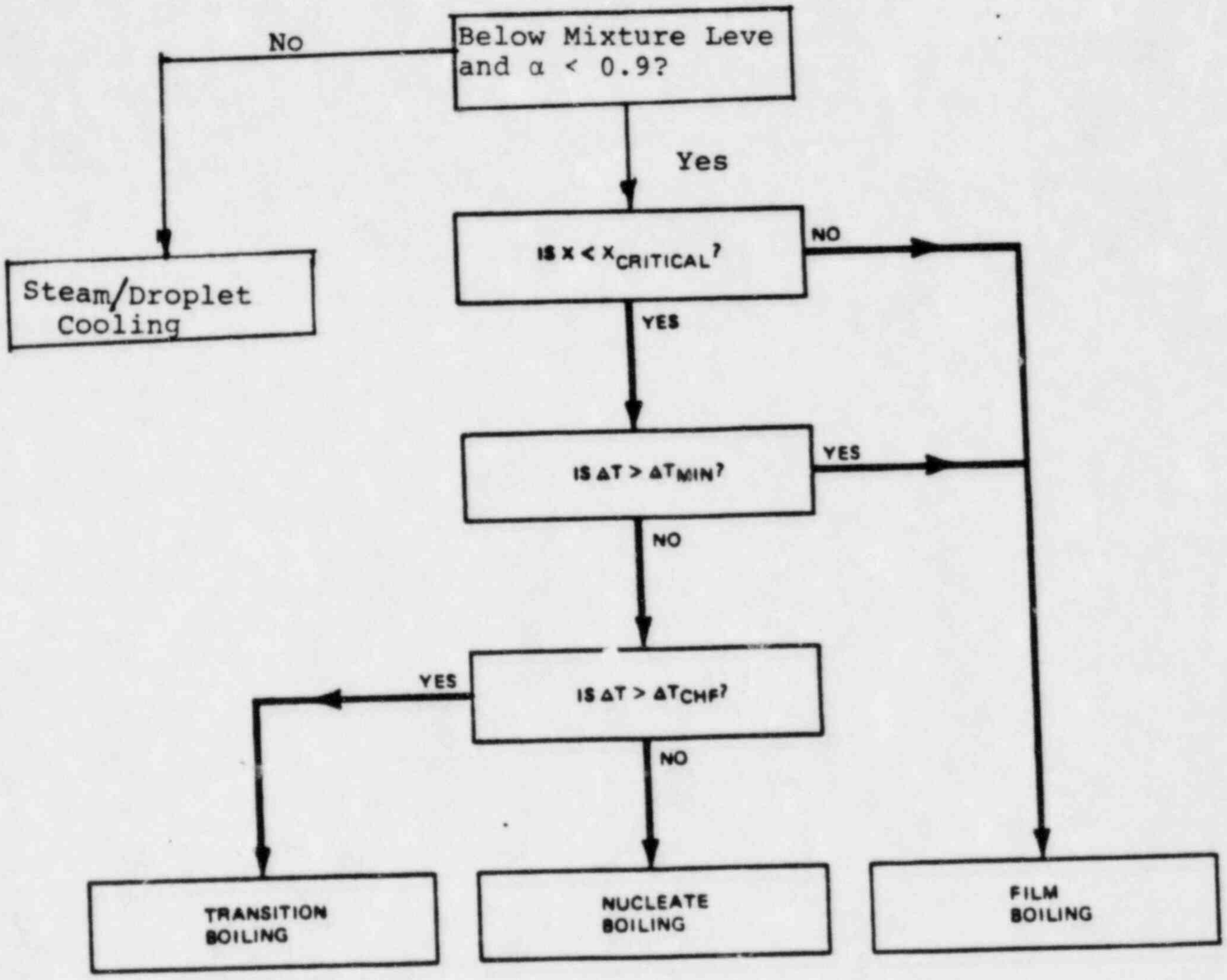
HEAT TRANSFER COEFFICIENTS

- Nucleate Boiling
  - Simple Ramp Model
- Film Boiling
  - Dougall-Rohsenow/Modified Bromley
- Transition Boiling
  - Interpolation
- Steam Cooling
  - Dittus-Boelter (Considers Steam Superheat)
- Droplet Heat Transfer
  - Function of Liquid Downflow, Steam Inflow and pressure
- Radiative Heat Transfer
  - Simplified Conservative Model.



HEAT TRANSFER LOGIC

FUEL ROD MODELS

GAP CONDUCTANCE MODEL

- Initialized by GESTR
  - Fission Gas Quantities, Gap Conductance, Gap Size.
- Dynamic Gap Conductance Calculation
  - Accounts for changes in Internal Pressure, Gap and Fuel/Clad Temperatures.
- Cladding Stress
  - Similar to 'CHASTE' Model

$$\sigma_c = \frac{r_{ci}}{r_{co} - r_{ci}} (P_g - P)$$

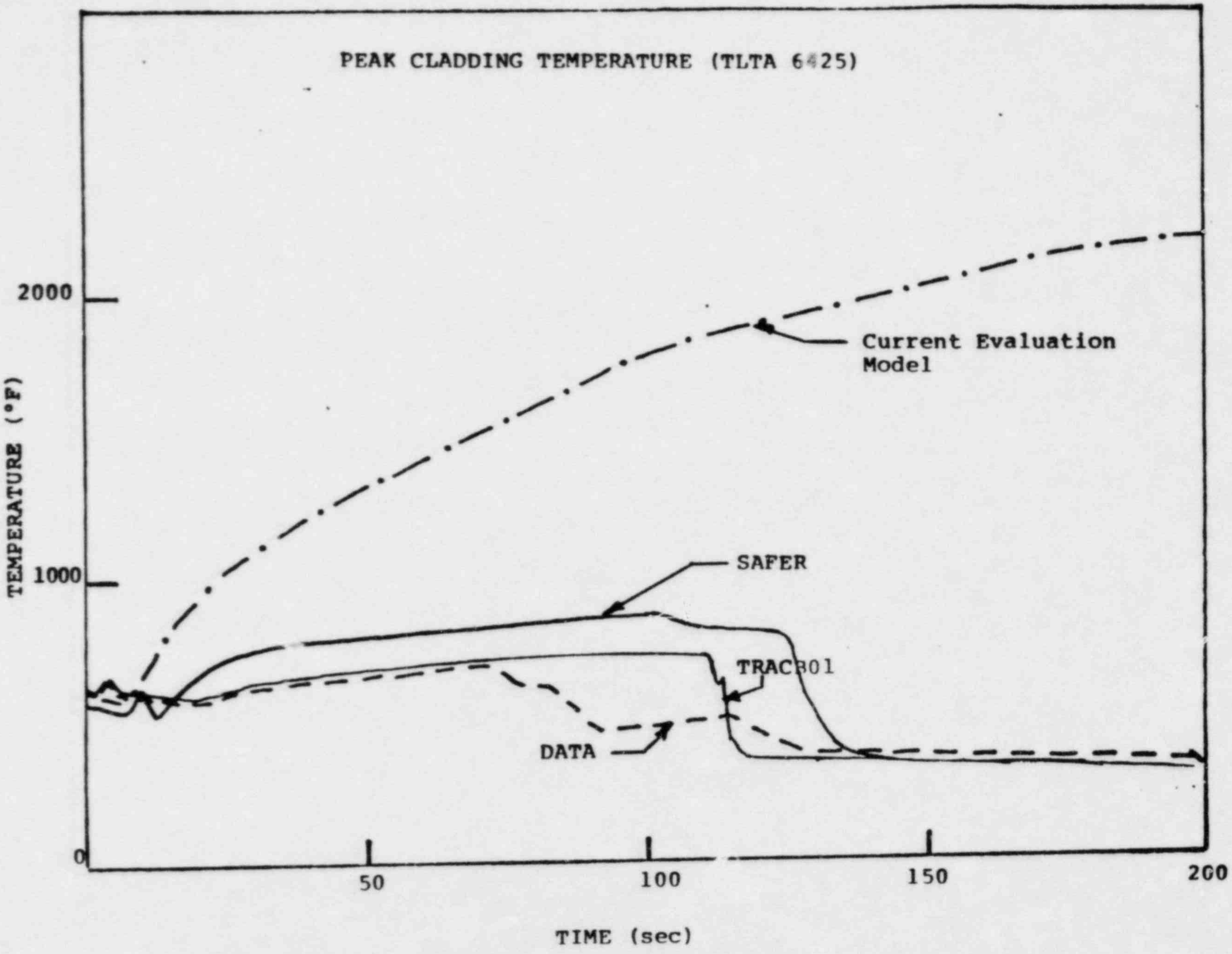
- Cladding Perforation

$$\sigma_c > \sigma_P (T_c)$$

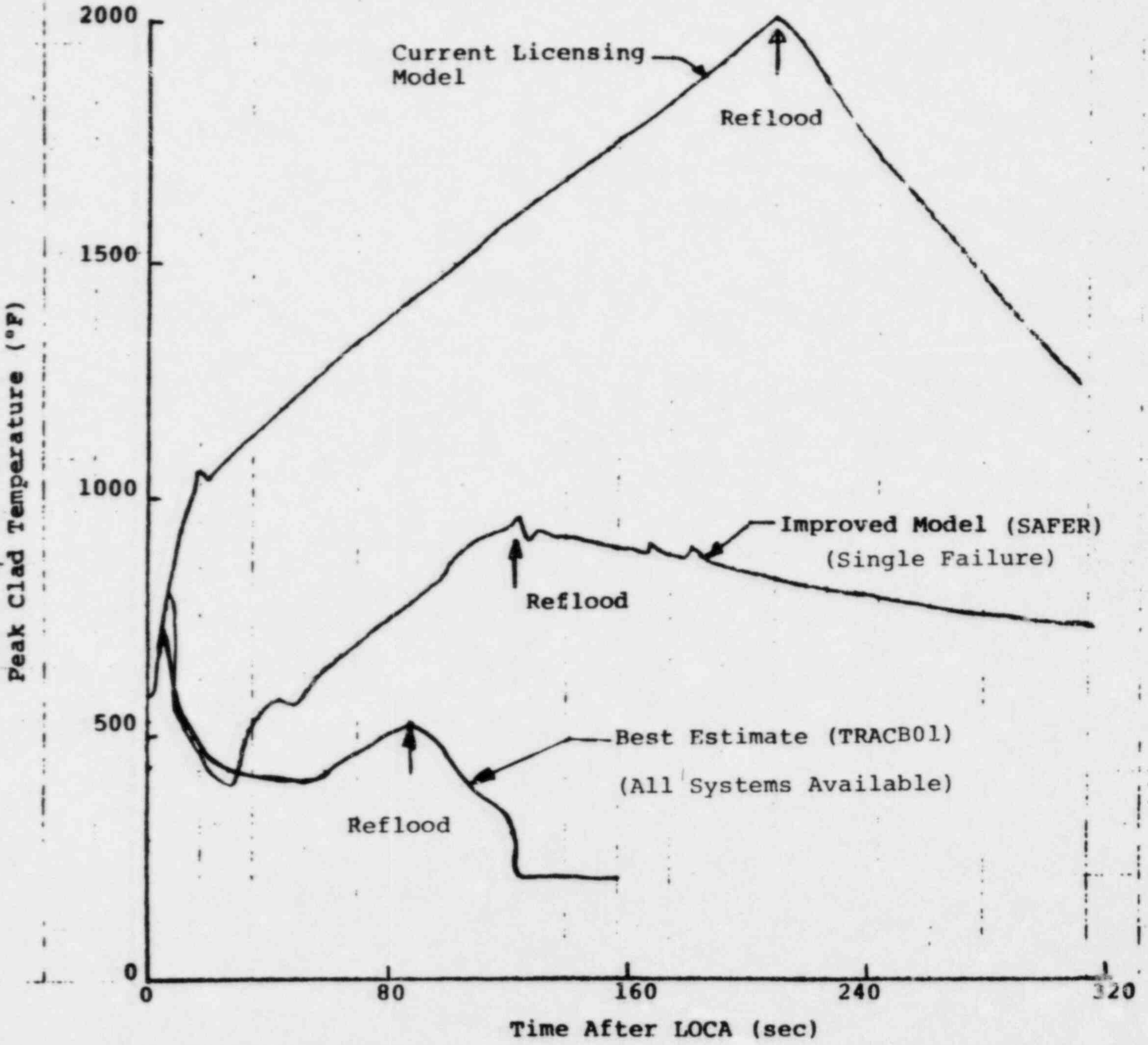
40

TYPICAL RESULT COMPARISONS

- Two Loop Test Apparatus (TLTA).
- BWR Large Recirculation Line Break.


PEAK CLADDING TEMPERATURE (TLTA 6425)
TEMPERATURE (°F)
2000
1000
0
50
100
150
200
TIME (sec)
Current Evaluation Model
SAFER
TRACB01
DATA

TYPICAL COMPARISON OF PREDICTIONS FOR LARGE BREAKS



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CONCLUSIONS

- Realistic Models Implemented in SAFER/GESTR.
- Large Reduction in BWR-LOCA PCTs
  - Improved inventory modeling
  - Realistic heat transfer models.

(1)

INVESTIGATIONS INTO THE DIFFERENCES IN THERMAL BEHAVIOR OF ELECTRIC AND NUCLEAR FUEL RODS

by

W. G. Craddick
C. B. Mullins
Oak Ridge National Laboratory
Oak Ridge, Tennessee 37830

December 2, 1982

2

PWR-BDHT project began analytical investigations of differences in electric and nuclear rods in 1977.

- Documented in International Symposium on Fuel Rod Simulators in 1980 (CONF-801091)

- Documented in Several ORNL reports
  - ORNL/NUREG/TM-291
  - ORNL/NUREG/TM-400
  - ORNL/NUREG/TM-431
  - ORNL-5886

The PWR-BDHT project addressed two objectives:

- Determine how power should be varied through time in an electric rod to best simulate nuclear rod behavior
- Analyze post-test electric rod behavior to determine what could be inferred about nuclear rod behavior

PINSIM

In post-test analysis, specific question addressed was:

> How would time-to-DNB's of electric and nuclear rods compare if exposed to same hydrodynamic environment?

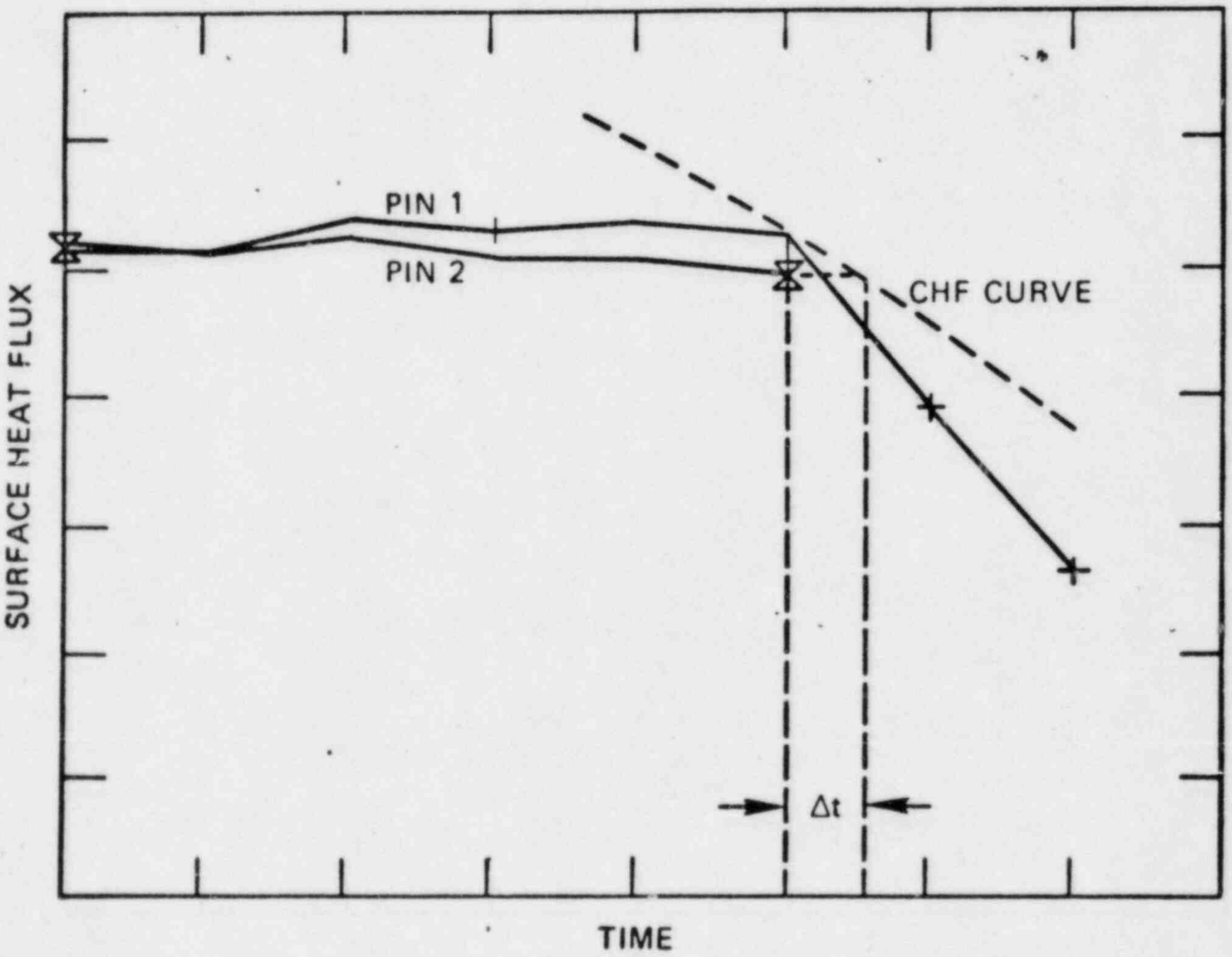
The following type of calculation proved the most informative:

- Calculate q''_E, T_{surf} and T_{sink} for electric rod from experimental data
- Compute $h = \dfrac{q}{T_{surf} - T_{sink}}$
- Assume that nuclear rod in same hydrodynamic environment sees same h and T_{sink}, and compute nuclear rod q''_N
- Assume q''_E = CHF at moment of DNB
- Compare q''_N to CHF

⑥

If $q''_N <$ CHF at time of electric DNB then later nuclear DNB

If $q''_N >$ CHF at time of electric DNB then earlier nuclear DNB



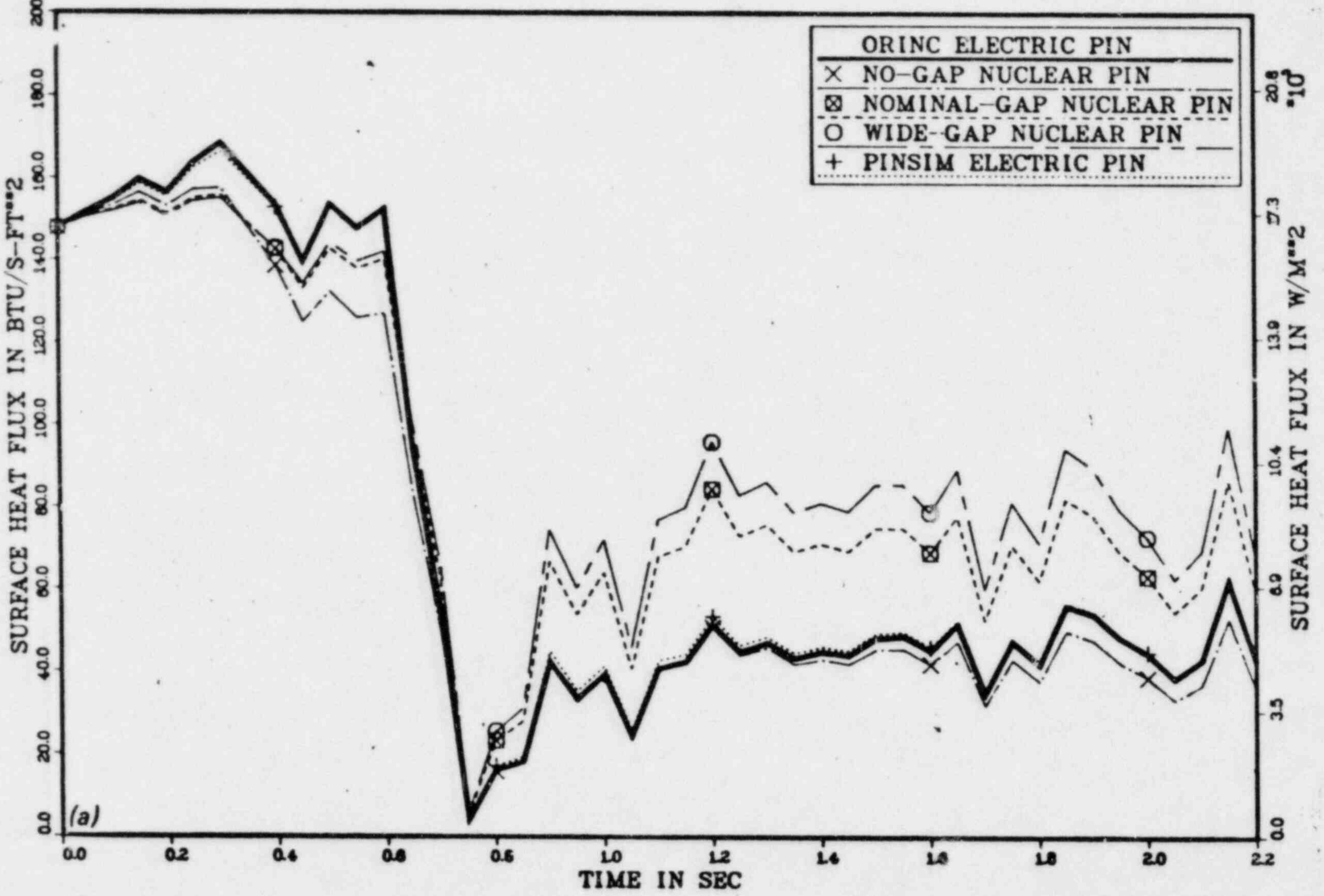
The two tests analyzed by PINSIM were conducted in the THTF.

- High pressure and high temperature water heat transfer loop
- Large bundle of full-length rods
- Both tests simulations of DECLB

THTF test 105 was conducted in 1976 with bundle 1 in the test section.

- 7 x 7 array of rods
- Pitch and OD of 15 x 15 PWR array
- Stepped, chopped cosine power profile

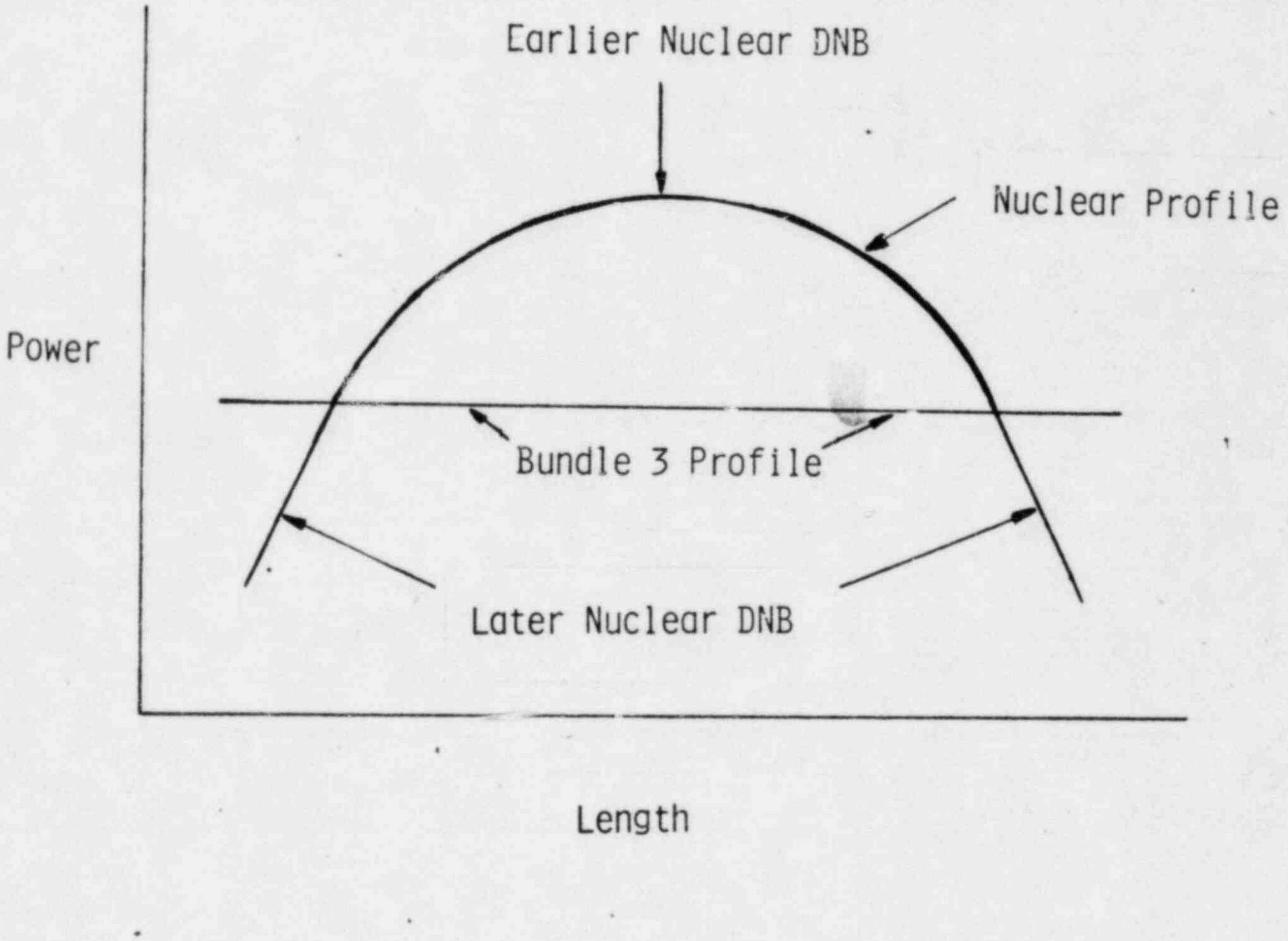
Test 105 results indicate a later time to DNB for a nuclear rod (all gap sizes).



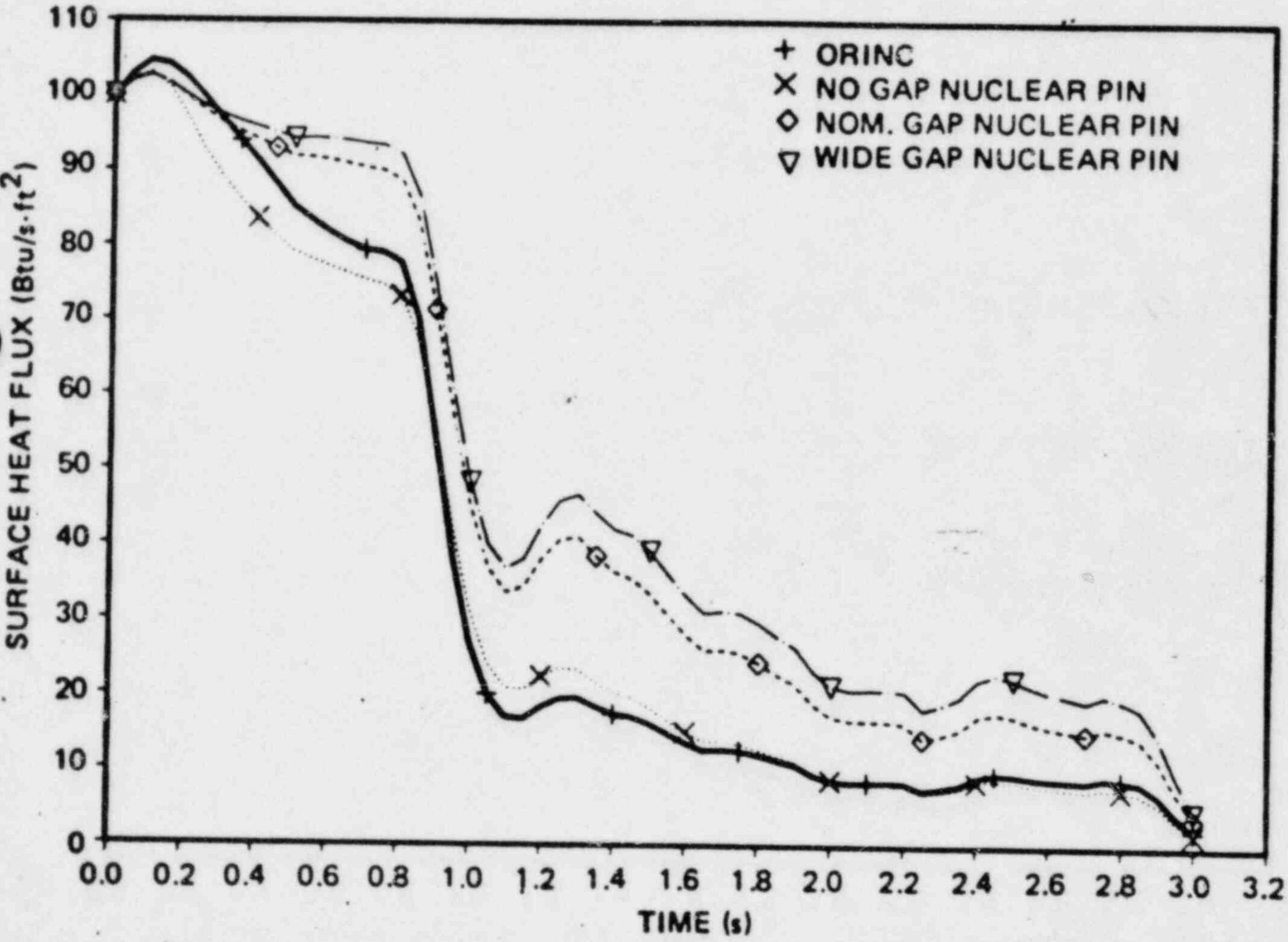
THTF test 3.05.5B was run in 1980 with bundle 3 in the test section.

- 8 x 8 array of rods
- Pitch and OD of a 17 x 17 PWR array
- Flat axial power profile
  (Bundle's primary purpose was to measure heat transfer coefficients)

The major difference in time-to-DNB with bundle 3 and nuclear rods is due to power profile.



THTF test 3.05.5B showed a nuclear rod would undergo DNB both sooner and later depending on gap size.



We are doubtful the benefits of constructing electric rods that closely simulate nuclear rod behavior is worth cost.

- Current electric rods don't, so new rods would have to be designed and built
- New electric rods designed to simulate nuclear behavior would be expensive/ difficult to implement
- Current electric rods can bound nuclear behavior

Current Electric Rods Cannot Match
Nuclear Fuel Rod Behavior

- At most, one axial level could be matched
    - axial electric power distribution is fixed
    - needed power distribution changes with time
- Even at one level, matching through time is difficult
    - generators must have infinitesimal response time
    - ability to remove energy internally

Two designs were considered with regard to matching electric and nuclear behavior:

- Sheath heated pin (very little thermal inertia)
- UO_2 filled pin heated with a Platinum-Tungsten alloy (stored energy and conductivity closer to that of nuclear pin)

There are several practical problems with the sheath heated design:

- Slow response time of T/C
- Perturbation of heat generation for imbedded T/C's
- Ill-posed conduction problem for internal or imbedded T/C's
- Fin cooling of externally mounted T/C's

The UO_2 filled rod responds more like a nuclear rod than existing THTF heaters.

- Closer match of internal thermal properties would cause needed power variation to be more attainable.
- Expensive

Electric rod behavior can be made to bound nuclear rod behavior in some respects.

- Time-to-DNB
- Quench rate?

ORNL's PWR-BDHT program has investigated differences in electric and nuclear rods.

- PINSIM
- Time-to-DNB can be bounded by electric rods
- Doubtful that cost of "more realistic" electric rods is justified by benefit

29 Nov 82

A SUMMARY OF INVESTIGATIONS INTO THE DIFFERENCES IN THERMAL BEHAVIOR OF ELECTRIC AND NUCLEAR FUEL RODS BY THE PWR-BDHT SEPARATE EFFECTS PROJECT

W. G. Craddick, ORNL
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SUMMARY

The NRC-sponsored Pressurized Water Reactor (PWR) Blowdown Heat Transfer (BDHT) Separate Effects Project began an analytical investigation into the behavioral differences of electric fuel rod simulators (FRS) and nuclear fuel rods in 1977. Analytical results were first reported to NRC in 1979 and reported publicly in the Proceedings of the International Symposium on Fuel Rod Simulators in 1980. The analytical techniques used and the results of the analyses have been documented. The investigations had two objectives: (1) to determine how the electric power supplied to the FRS during an experiment should be varied through time to best simulate nuclear fuel rod behavior (these results are both facility-specific and test-specific); and (2) to analyze the FRS behavior after the experiment to determine what could be inferred about nuclear fuel rod behavior from the experimental data on FRS behavior. The investigations described here were limited to the blowdown phase and addressed the specific question, how would time-to-departure from nucleate boiling (DNB) for a nuclear fuel rod compare to time-to-DNB for an FRS if both were exposed to the same local hydrodynamic conditions? Since identical hydrodynamic conditions were postulated, and since the effects of surface roughness and surface contaminants were not considered, the differences investigated were due to differences in the internal thermal properties of nuclear fuel rods and FRSs.

The PINSIM computer program was created to investigate these differences, and was used to analyze in detail two tests (test 105 and test 3.05.5B) conducted in the Thermal Hydraulic Test Facility (THTF). The analyses concluded that the earliest times-to-DNB experienced by the FRSs in test 105 were a lower bound on the times-to-DNB that would have been experienced by nuclear fuel rods exposed to the same hydrodynamic conditions, while for test 3.05.5B, the FRS times-to-DNB were not lower bounds. Both tests were simulations of double-ended cold leg breaks. While it appears possible to bound nuclear fuel rod behavior with FRS behavior, it appears difficult or impossible to match nuclear fuel rod behavior. Improved FRS designs might be able to improve the extent of the simulation, but it is not clear that the results would justify the expense of their development.

1. INTRODUCTION

The NRC-sponsored Pressurized Water Reactor (PWR) Blowdown Heat Transfer (BDHT) Separate Effects Project began an analytical investigation into the behavioral differences of electric fuel rod simulators (FRS) and nuclear fuel rods in 1977. Analytical results were first reported to NRC in 1979 and reported publicly in the Proceedings of the International Symposium on Fuel Rod Simulators in 1980 (Ref. 1). The analytical techniques used and the results of the analyses have been documented (Ref. 2-5). The investigations had two objectives: (1) to determine how the electric power supplied to the FRS during an experiment should be varied through time to best simulate nuclear fuel rod behavior (these results are both facility-specific and test-specific); and (2) to analyze the FRS behavior after the experiment to determine what could be inferred about nuclear fuel rod behavior from the experimental data on FRS behavior. The investigations described here were limited to the blowdown phase and addressed the specific question, how would time-to-departure from nucleate boiling (DNB) for a nuclear fuel rod compare to time-to-DNB for an FRS if both were exposed to the same local hydrodynamic conditions? Since identical hydrodynamic conditions were postulated, and since the effects of surface roughness and surface contaminants were not considered, the differences investigated were due to differences in the internal thermal properties of nuclear fuel rods and FRSs.

2. METHOD

The tool developed to conduct these investigations was the computer program, PINSIM (Ref. 2). The program was used for both pre-test and post-test analysis of experiments performed in the Thermal Hydraulic Test Facility (THTF) (Ref. 6), and was able to perform various types of calculations. The type of calculation which proved most informative (the results of which are described in the remainder of this paper) was performed in the following manner: (1) using individual FRS thermocouple and amperage data and in-situ thermal property calibrations, the "actual" FRS surface temperatures and surface heat fluxes for a THTF experiment were calculated (Ref. 7); (2) a "sink" temperature was determined by using the saturation temperature corresponding to the measured pressure; (3) the preceeding data was used to calculate the experimental heat transfer coefficient; (4) it was assumed that a nuclear fuel rod exposed to the same hydrodynamic environment as the FRS in the experiment would experience the same heat transfer coefficient and sink temperature up to the point of DNB; (5) therefore, the experimental heat transfer coefficient and sink temperature were used as boundary conditions for a calculational model of a nuclear fuel rod; (6) at the moment when the FRS experiences DNB, the predicted nuclear fuel rod surface heat flux was compared to the FRS surface heat flux (taken to be equal to the critical heat flux [CHF] at that moment). If the nuclear flux is lower than the CHF, the nuclear fuel rod would experience DNB later than the FRS; if the nuclear flux is higher, the nuclear fuel rod would experience DNB sooner than the FRS. Implicit in these conclusions is the assumption that the CHF is falling at the time when DNB occurs. The comparisons were made with nuclear fuel rod models using a range of pellet-to-clad gap sizes and various initial power levels.

3. RESULTS

An extensive analysis of electric FRS and nuclear fuel rod behavior for THTF test 105 was documented in January 1981 (Ref. 4). Test 105 was conducted in August 1976 with bundle 1 in the THTF test section. Bundle 1 was a 7 x 7 array of FRSs with dimensions equivalent to a 15 x 15 fuel rod array in a PWR. Bundle 1 had a chopped cosine axial power profile. Test 105 was a simulation of the core conditions of a PWR double-ended cold leg break (DECLB). The results of the analysis indicated that a nuclear fuel rod subjected to the test 105 coolant conditions would experience DNB later than the electric FRS regardless of the gap size assumed for the nuclear rod.

A similar analysis was performed on THTF test 3.05.5B more recently (Ref. 5). Test 3.05.5B was conducted during July 1980 with bundle 3 in the THTF test section, and, like test 105, test 3.05.5B was designed to simulate a PWR DECLB. The primary purpose of bundle 3 was to experimentally determine heat transfer coefficients. Toward that end bundle 3 was fabricated with a flat axial power profile and a design which allowed more accurate determination of surface heat fluxes and temperatures. The FRSs had the outer dimensions of PWR 17 x 17 fuel rods. Since bundle 3 has a flat power profile, its center power is much less than that of a nuclear bundle, but the power at the ends of an FRS is greater than that of a nuclear rod. Naturally, because of this profile difference, a nuclear rod would have undergone DNB earlier at the center and later at the ends. At the two locations where the power was the same the results depended on the size of the gas gap assumed for the nuclear rod. The analysis showed that a nuclear rod with no gas gap exposed to test 3.05.5B coolant conditions would experience DNB later than the electric rod, while nuclear rod models with nominal or wide gas gaps would experience DNB earlier than the electric FRS.

The implications of these conclusions for reactor accident scenarios is uncertain at the moment due to the dependency of CHF on local fluid conditions and the uncertainties associated with defining the proximity of THTF coolant conditions with those extant in a PWR DECLB. Another complicating factor in such an analysis is the accuracy with which one can measure surface conditions on FRSs. Because the FRSs used in test 3.05.5B were superior to those used in test 105, the test 3.05.5B results are probably more accurate.

The preceeding discussion has addressed a very specific question, namely, how would times-to-DNB compare between electric FRSs and nuclear fuel rods? Some comments can be made about the more general question, how well can electric FRSs match the thermal behavior of nuclear fuel rods? First, it is not clear that matching nuclear fuel rod behavior should be the objective of an FRS. An easier-to-obtain and, perhaps, just as useful objective is to try to have FRS behavior <u>bound</u> the nuclear fuel rod's behavior. It appears that lower bounds on time-to-DNB are probably obtainable using existing FRSs if appropriate temporal and axial power variations are used. On the other hand, upper bounds on peak clad temperature could not be obtained with FRSs such as those that have thus far been used in the THTF, due to their having an upper limit of 1600 degrees F on the stainless steel sheath to maintain structural integrity. This could probably be remedied by an improved FRS design using different sheath materials. If it is deemed necessary to try to match rather than bound nuclear fuel rod behavior, a variety of problems must be confronted. First, in our current FRSs, the relative axial power distribution is determined by the physical design of the FRS and thus cannot be varied between or during experiments. Since the temporal power variation required to cause the FRS to match nuclear fuel rod behavior will vary from axial location to axial location, <u>at most</u> one axial location can be caused to match nuclear fuel rod behavior. Furthermore, the ability to match nuclear fuel rod behavior through time, even at one axial location, is quite limited. This is due to the differences in thermal conductivity, stored energy and heat capacity between the nuclear fuel rod and the FRS. Our calculations have indicated that one would need electric generators with infinitely fast response times and some means to extract energy from the FRS's interior (negative power) to be able to match nuclear fuel rod behavior exactly with our current FRSs. The extent to which one could match nuclear fuel rod behavior in the absence of such engineering "magic" could be improved with improved FRS designs.

Some brief consideration was given by PWR-BDHT staff (specifically, R. C. Hagar and R. W. McCulloch) to the possibility of designing FRSs which could better simulate nuclear fuel rod behavior. Two concepts were considered. Since a primary inhibitor to controlling the FRS's surface conditions through temporal variation of the electric power supplied is the heat capacity of the material between the heating element and the sheath, using the sheath itself as the heating element would facilitate such control. However, enhanced control of the FRS's surface heat flux only helps the simulation if one already knows how the nuclear fuel rod would behave, and there are a variety of practical problems in attempting to use such an FRS. A second approach is to construct an FRS with internal thermal properties that more closely match those of a nuclear fuel rod. Toward that end some studies were made of an FRS design which used uranium dioxide filler (made from tails of the enrichment process) and a platinum-tungsten heating element. Analytical calculations indicated that the uranium dioxide-platinum FRS would respond much more like a nuclear fuel rod than an existing FRS when exposed to the same boundary conditions (heat transfer coefficient and sink temperature).

4. CONCLUSIONS

To summarize, ORNL's PWR-BDHT Separate Effects Project began investigations of the differences in the behavior of nuclear fuel rods and electric fuel rod simulators (FRS) in 1977. The PINSIM computer program was created to investigate these differences, and was used to analyze in detail two tests conducted in the THTF. The analyses concluded that the earliest times-to-DNB experienced by the FRSs in test 105 were a lower bound on the times-to-DNB that would have been experienced by nuclear fuel rods exposed to the same hydrodynamic conditions, while for test 3.05.5B, the FRS times-to-DNB were not lower bounds. Both tests were simulations of double-ended cold leg breaks. While it appears possible to bound nuclear fuel rod behavior with FRS behavior, it appears difficult or impossible to match nuclear fuel rod behavior. Improved FRS designs might be able to improve the extent of the simulation, but it is not clear that the results would justify the expense of their development.

5. REFERENCES

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