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NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON CLINCH RIVER BREEDER REACTOR

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Nuclear Regulatory Commission
1717 H Street, N.W.
Washington, D.C.

Friday, November 19, 1982

The meeting of the subcommittee was convened at 8:30 a.m.

PRESENT FOR THE ACRS:

M.W. CARBON, Chairman
R. AXTMANN, Member
J.C. MARK, Member
D. OKRENT, Member

DESIGNATED FEDERAL EMPLOYEE:

P. BOEHNERT

ACRS CONSULTANTS:

W. KASTENBERG
W. LIPINSKI

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# PROCEEDINGS

(8:30 a.m.)

MR. CARBON: The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on CRBR. My name is Carbon. I am the subcommittee chairman. The other ACRS members present today are Drs. Axtmann, Mark and Okrent. We have in attendance ACRS consultants Drs. Kastenberg and Lipinski.

The purpose of the meeting today is to continue review of the ACDA energetics issues for CRBR. This meeting is being conducted in accordance with provisions of the Federal Advisory Committee Act and the government in the Sunshine Act.

Paul Boehnert, on my right, is the designated federal employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on October 28, 1982.

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

It is requested that each speaker identify himself and speak clearly and loudly so that he or she can be readily heard.

We have received no written statements from members of the public, and we have no requests for time to make oral statements from members of the public.

In terms of my own comments to start the meeting I guess I have not very many. I would mention a couple of things. I believe you all have a copy of the letter we wrote to Theo on October 12th asking for coverage of certain points today. I won't go through that further.

I am sure everyone has also received a package of material, the three-part set which came from Theo depending upon where you were last weekend or early this week.

Does anyone have any point to bring up before we start? If not, I believe we will charge on into the meeting, and I guess I will call on Mr. Cardis Allen.

MR. ALLEN: I have very little to say other than to introduce Dr. Theofanous and his team. As you know, the group was formed back in July at a point after which we had been reviewing information the applicant provided on their core disruptive accident analysis. They were given the charter and technical activities to develop a position on CDA energetics, and they have been working diligently since that time, and they are here to convey the fruits of their labor to you.

And with those remarks I will turn the floor over to Dr. Theofanous.

MR. THEOFANOUS: If I remember correctly, at the last meeting of this subcommittee Dr. Mark thought that I was very quiet, so today I will do most of the talking to make up for that.

We are going to cover today the results of our assessments for core disruptive accidents of CRBR energetics. Based upon the wishes of the subcommittee we would also like to spend quite a bit of time on the organizational aspects of our effort.

The whole presentation is broken down into four parts. The first one is discussing the management and the organization and the philosophical approach of the review group. The second one goes into some more detail into the overall structure of our technical efforts. And the technical discussions are concentrated here in Part 3, and that in turn is concentrated on the loss of flow accidents energetics. Then finally we will close with conclusions. That is a pretty long presentation based upon our trials the day before yesterday, and it depends upon the number of questions we will be getting. It could be anywhere between three and six hours.

We would like to propose, Mr. Chairman, that each one of those, especially this one, the large section is broken down into five units; so I would like to propose if you have any questions or qualifications that you interrupt at the moment the question arises, either questions relating to the subject. At the end of each unit I will ask for questions, and then we can go into more depth into the questions.

Is this agreeable?

MR. OKRENT: Before you start, can I ask the Chairman why we need to go deeply into the management group since we have had something to read. Is there something special you feel needs to be covered now that it's in writing?

MR. CARBON: If it's all right with the subcommittee, we can skip the management group and simply ask questions.

MR. MARK: I have a general question. It seemed from reading the material you sent out, Theo, as if the management group and the project entirely had set up with a somewhat preconceived objective of attempting to back up the position taken by the project and the staff.

I hope that was not truly the case, namely that your study was to find out what is true rather than confirm what has been said.

MR. THEOFANOUS: This is absolutely correct. I don't know what you are specifically referring to as a preconceived notion.

MR. MARK: The way things were presented as written, one could have taken this other conclusion.

MR. THEOFANOUS: It could be that you are referring to Appendix B in which we have the tasks outlined in terms of of objectives, scope and outputs. Some of those say so that. Maybe that is what you are referring to when you say it is preconceived.

I think the reason for this verbiage there is because we wanted to really focus the efforts in a given direction so people would know what they are looking for. However, it was very clear through our interaction with everyone that certainly we were looking for what was the truth.

MR. MARK: I'm sure I knew that was the case, and my criticism, if there is one, is only in some details of the presentation.

MR. THEOFANOUS: Thank you.

MR. CARBON: I have a question with regard to the synopsis here. On page 2 there is a statement near the end that qualitative probabilistic framework for quantifying the consequences of the accident given. I stumble a little on "qualitative" and "quantitative." But I wonder if you would say just a word on philosophically why you are putting the emphasis on this particular thing.

MR. THEOFANOUS: I think that this is under Section 3; in fact, it is the first vu-graph, I think, on Section 3. And if you would like not to go over the first two, we can go right into the presentation and start exactly from this question. If you intend to skip the first two parts, we can go straight into your question by giving you what we have.

MR. CARBON: That would be fine to handle it then, but I think there will be some questions some of us will have on the first two parts. So to give us a chance to ask questions.

MR. OKRENT: If I could follow up Dr. Mark's question, was there a specific segment of the group that was given the responsibility of trying to find out what was wrong and what the other group was doing, what the project was saying, and to find possible weak spots, and that was their only function.

MR. THEOFANOUS: Yes. I think I now understand the thrust of your question as well as Dr. Mark's, and I think I will be able to explain better our position with respect to that if you will allow me to show one vu-graph in the first section. This is the review path.

I think what you are saying would have been correct if the management plan and everything that went with it was formulated at the beginning of the review. However, it's important to point out that the management plan was formulated sometime following the review. So I would like to go over this. I think it would be helpful in these two questions.

The review started by the updating of the CRBR PSAR by GEFR 523. We spent a lot of time over the first few months -- in fact, it was from December until May, sometime in May that exactly we were looking for problems and issues in reviewing and going very carefully over the applicant's case, as was documented in GEFR 523.

Over this period of time also there was a very extensive discussion going between the reviewers, which was the NRC staff, and their consultants, as well as the project and their consultants on the other hand. There were a number of meetings taking place during this time frame in which we were hashing out those issues. You saw the results of this kind of interaction in the previous CRBR subcommittee meeting here.

Based upon this extensive review in which our main emphasis was to find out what was wrong with the Applicant's case, we formulated these eight fundamental questions, eight issues, and those are given as Appendix A in the handout I gave. And by the way, we have a large number of copies of everything that will be discussed today, and if anyone does not have one, you are welcome to get some.

Those issues were formally transmitted. The applicants were aware of our narrowing down into these issues over a period of time, but they were formally transmitted to the applicant somewhere around June 1982. The applicant then responded two or three months later, and as a result of this response we had a meeting to discuss this response. We had a meeting at Argonne on 9-22 in which we agreed with the applicant on remaining issues.

There were still issues we had problems with after their presentation. As a result of that, we came up with a number of action items for the applicant to give us information. In the interim between the identification of those eight issues and this date over here (Indicating) the group was formed, and the management plan and everything that goes with it, everything you see in Appendix B was developed in this time frame. So that already we had gotten a major part of the review under way. Already we knew what we were looking for.

Maybe that is exactly the thing confusing to you. It sounds like we know what we are looking for. Certainly we had better know what we were looking for at this point. If we didn't know at that point, we would have been in bad shape later on. That is why the tasks are described more in the imperative rather than looking for something we don't know what it is.

If I may continue that a little bit further, in addition to these eight issues, we had input, other inputs in the formulation of this plan, and these inputs had the form of other letters specifically asking the consultants for another round of letters with any remaining problems they might have, and this was factored into the management plan.

After the plan was formulated we sent it out again to all of the consultants, as well as to the project, as well as to the NRC, and asked for additional feedback to make sure it was complete and sound. And following all of these interactions then, the management plan was finally assigned to individuals, and we continued going on with this, what we called further independent assessment.

At this point we changed the mode of operation. Up to this point we were formulating questions and looking for problems. From that point on we factored these problems as well as what were our perceptions of how the accident is going into more or less you might call it a positive effort in which we really tried to do our own independent assessment. And then, in addition, we got further input from the meeting, further responses from these action items. And at this point we are in this meeting here (Indicating), and you are going to here from us, primarily zeroing again into our independent assessments, which although they will not explicitly state at every point of the way the applicant's positions, of course, since we had a long interaction with the applicant it is factored into the picture.

MR. OKRENT: That was a long answer, and if I read it one way, I think it was no to my question. because my question was do you have within your management structure a group whose only function it is to see whether they can punch holes into your conclusions?

MR. THEOFANOUS: Into our conclusions? I thought you --

MR. OKRENT: Into your conclusions. Into your conclusions.

MR. THEOFANOUS: Of course we have. It was a yes because I said --

MR. OKRENT: Who are the people whose only function it is to try to puncture your conclusions?

MR. THEOFANOUS: Well, we have, Dr. Okrent, a finite number of resources. I don't think it would be wise to allocate one or two or three or any number of people with the only job to do looking at what we are doing and trying to punch holes.

I think we have a very strong interaction within the review team, and we are all alert at any given moment to check the status and find problems inside these positions. So you might say that the whole team has this function, although I don't think we can afford to take any section of the team and say that's all your job, look what you are doing and find a mistake.

MR. OKRENT: That's all for now. I will come back to it later.

MR. THEOFANOUS: And if I may take that one step further then, it seems to me we are almost there. As a result of this meeting we, as well as the applicant, expect to have your comments and criticisms. I believe that the applicant will take that into consideration together with the further assessments to give us a final response to these action items. And then we will take all of this information and integrate that into a report that will contain -- it will be in great detail so I can follow it on a technical basis.

We are scheduled to begin the writing at the beginning of December. We hope to have the first draft by the end of January and the final draft by the end of February. From that point of view, therefore, this meeting is very timely because it is almost like our last interaction with the outside world before we get into the business of writing.

MR. CARBON: Let me go to a question on page 3 of your writeup where you talk about review and evaluation of the applicant's arguments, and then you lead into the point that some of your work has involved new studies that were original in various ways, one of these ways being new phenomena or new effects being taken into account.

MR. THEOFANOUS: Yes.

MR. CARBON: How many new things in the way of new phenomena, new effects and new scenarios and so on have you brought in here that weren't considered earlier by the project? I would just like to have some feeling.

MR. THEOFANOUS: Yes. Let me first say what we consider to be the major accomplishments of the review process.

MR. CARBON: Let me make clear I am asking for what was original on your part.

MR. THEOFANOUS: Okay. All right.

This aspect here, the incorporation of the fission gas effects, was original. This problem was initiated by us. Fission gases are supposed to be in the plenum when the reactor is run, but this was the first time these effects were incorporated into the safety analysis as far as we know, and we believe they are very important effects.

There is a reason probably why they were not brought up in the FFTF review, and the reason is, of course, that the accident develops in this kind of reactor, the FFTF, in such a way that the plenum gases are most likely to get out of the plenum by the time this pressure there might become relevant. So that is something that was not included in the GEFR 523.

We thought it was very important at this point. The applicant thinks it is very important, and they consider it to be not the kind of end spectrum situation but almost like a base case or reference case, so we think that is extremely important. It changes our perceptions completely as far as a good part that might involve irradiated fuel.

MR. LIPINSKI: When you say that was original over here, it has always been a consideration as to whether the fission gas plenum should be opened and not pressurized or pressurized. It may not have been in the applicant's consideration, but it always has been in the LMFBR program.

MR. THEOFANOUS: It might have been, but as far as I know, I was the first one to actually document the concern, the safety concern, with respect to plenum gases in loss of flow accidents; and that was about five years ago. And we had as recently as just a few months ago, I guess when 523 was written about a year ago, that was not taken into account.

MR. LIPINSKI: I am saying it may not have been in the applicant's case, but it has been in the program.

MR. THEOFANOUS: And neither was it taken into account in any of the safety reviews. For example, in the homogeneous core of the CRBR it was not taken into account. And I believe -- I guess I brought up the problem in connection with that core. And I believe that in fact with respect to that core it would have been a much more serious problem than it is here.

Another item, Dr. Carbon, that I think we made original contributions as a part of this review process is to quantify the origin and severity of recriticalities. Again, recriticalities are something new. People have been struggling with them over many, many years; and we made, I believe, the first real effort, and I believe the project also is making a real effort, because they agree with us that recriticalities are possible, not something completely impossible as it was thought some years ago. And we made a very serious effort to look at the origin, look at the likelihood, as well as to try to quantify them in a way that is useful in assessing the energetics potential from the CRBR.

Another aspect is the possible revision of the energetics relief path. I think that will become more clear after I go through some of the technical discussion. But very briefly for now, this refers to the classical process of disassembly and the relief of the high pressure of the core. It is one in which high pressures develop in the core, and they push up the upper internal structure. This is the structure hanging from the head of the reactor vessel. When the pressures are high enough, they push it upward and allow an upward expansion.

We looked very carefully at the structures around this high pressure region, and we have a suspicion that -- and now we are trying to quanity it better -- that maybe the core barrel feels a little higher pressure because it is even closer to this high pressure region. Maybe that one might go first. And if that were to happen, that would result in a more isometric kind of expansion upwards and off to the side.

It's not that it will make a very big difference. In fact, in my opinion if anything it will make it a little more clear, the assessment of that part of the accident. Nevertheless, we feel it's important that we really know clearly how disassembly might evolve in a realistic sense.

And in any case, however, I need to point out that this is not complete yet. I believe that the assessment of the structural aspects of this program is probably the most straightforward and easy ones and we can count most, and therefore we cannot afford to leave alone or to not look carefully into this. We can do very well.

But I think I will have more to say about that. There are other aspects we have considered that are not here. As a result of having to consider these plenum fission gases we had to do a lot of new developments in collateral occasion dynamics, for example. I will be talking about that also.

We have another set of tasks. As you may remember, we call them I series tasks. They have to do with the initiators. There are a number of aspects of those tasks that put us again into some new phenomena, as I say in the document there, because the I-E tasks we had some difficulty identifying the appropriate individuals, and they are somewhat delayed. So we don't have the final word on those yet, but we don't expect to have a very significant impact to the schedule. But depending upon what some of those I-E tests will yield, we believe there might be some new aspects developing from there also.

Yes?

MR. OKRENT: If I can get back to the thrust of the question I was raising earlier sometime before the end of the presentation, the NRC presentation. If it is possible, I would like to hear from each of the members of the team who are here whether they have any reservations with regard to the conclusions of this work individually.

Secondly, if there is a possible one or two weak spots in what you are going to tell us, where they might be.

Thirdly, whether the range of postulated sequences is sufficiently comprehensive to have covered the things of interest to the general question being examined, namely could one somehow lose integrity early.

And then in some way I would like to hear whether this concern alluded to in a memorandum by Dr. Kelber about something at Los Alamos, I was interested in how that has been addressed.

It's not that I am suggesting that the conclusions here are not reasonable, but I would like to hear whether people, as I say, have any questions and where lurking, let's say, in the back of their minds or where they would look if they had any and so forth.

And so the members -- and I am interestec in your own reaction, obviously.

MR. THEOFANOUS: I am very happy that you made that addition, because for a moment there I thought you were doubting I would be giving you the correct picture here.

MR. OKRENT: No, no, no. But I think it is important to understand why, if this is reasonable, why it is. Also, where there might be something that's a surprise.

Now, in LWRs all too often you run into a transient that wasn't on the previous event trees which one can see if it occurred would lead to a rather different sequence and perhaps more severe conditions than in fact had been analyzed in those members of the set used in examining a problem. And I think it is therefore relevant here to make sure that we don't have an equivalent situation that just hasn't entered into this examination.

MR. THEOFANOUS: At least, Dr. Okrent, I am not clear as to what you are asking. I don't know if the other consultants are. But if you are asking me if we are considering other things than a loss of flow, oxygen -- you mentioned transients. That would prompt one kind of answer from me, and I can give you that. If you are asking the consultants whether I am going to be giving you the correct conveyance, so to speak, of their thoughts on the subject, that will prompt another kind of an answer. If you are asking what, I will tell you what.

Can you tell me, are you looking for both or one at a time?

MR. OKRENT: Well, I assume you will give a summary of the team's work.

MR. THEOFANOUS: That's right.

MR. OKRENT: But I would like to hear, as I said, from the various people where, if any, where they think there may be weak points, what are the number first and second candidates for possible weak points. In effect, if they were to take the role of devil's advocates looking at what we are going to hear, where they would pose what they consider to be hard questions or however, okay? They may say we have nothing to add, and that's okay, too, if that's what they want to tell me.

MR. THEOFANOUS: You asked also me, and I will try to give you some idea where the holes are, if there are any holes. However, I thought you were also referring to other accidents or other transients. That's why I asked you. We have a special effort in that we are intending to talk about it here. That's the sequence of I-E tasks. We think it is a very important sequence. We feel those I-E tasks will demonstrate that what we are doing within looking at great, great detail in the loss of flow accident, by looking at this information we will be able to put numbers also on those other I-E initiators.

If, however -- and I have examples I can tell you -- a new kind of thing develops that we have not anticipated or something we are not suspecting now develops in a direction that is not really covered by our technical assessment and the loss of flow accident studies, then we will focus on that also in great technical detail and try to answer that one also.

MR. OKRENT: You see, part of the reason for the question is I can't tell whether you think you are bound to the situation with the loss of flow accident or whether you still have a series of initiators that are open and we're just not hearing about them today.

I have the impression you felt strongly you had bounded it from what I read.

MR. THEOFANOUS: Perhaps it would have been preferable if I had gone through some of this earlier stuff, but indeed we believe and have no reason to doubt, and no one in the team has raised any doubt whatsoever that our first premise here, which is to show that the loss of flow -- to show that the loss of flow accidents span the ranges of phenomenology of interest is a true one and a correct one.

In particular, we have looked at the transient overpower accident in some detail. We have a whole team working on that -- and as you know, that is one of the classical other initiators -- and there seems to be no problem in that.

On the other hand, we have initiated a number of additional tasks which are not in the classical domain. As an example is the loss of heat sink accidents in which the temperature rises uncontrollably in the primary system. Then we are looking at the possibility of the structural components start creeping under high temperature and stress in such a way that there are some structural failures early on that might lead us into a phenomenology such as, for example, the whole core dropping out.

What was there to learn if this was to happen? The secondary control rods will go with it also because they are unlatched. So we are exploring accidents and sequences up to that extreme, things which to my knowledge have not been considered before.

All of this is part of the I series, and these are the activities described over here. We feel although there are some questions there and we are looking at them, we basically have -- we are not sure at this point to devote a measure of technical effort in that. However, again I emphasize if something were to come out as a result of the scoping analysis, we then, of course, will put the whole emphasis here.

But classically and over the past umpteen years this has been the problem. People have had a great difficulty resolving the loss of flow accident. So quite honestly, we felt as a starting point and a real substantial point we have to put a lot of technical emphasis in the loss of flow accident. It makes no sense to go there and distribute thinly and spread thinly over a very wide number of things and at the end come here and not be able to give you a complete story about anything.

So we are trying to look at the whole thing in perspective as well as in great depth in the problem that has been historically up to today and will be, I think, for sometime to come, and that is the loss of flow accident. And we believe by doing that we will be able to assess energetic behavior through all of the other indices. And no one in the team -- I can categorically say that -- no one in the team disagrees with this approach.

MR. CARBON: On that chart, on page 4, would you refer to page 4 in the second paragraph. There's a sentence there that says, "Recriticality is used as a short," and something is missing and I would like to know what it is.

MR. THEOFANOUS: Where are you, Dr. Carbon?

MR. CARBON: Page 4.

MR. THEOFANOUS: Yes, recriticality is a nomenclature. It is a name. We are referring to recriticality as the process that produces supercritical configurations from disrupted fuel. And we emphasize this because we also have another process that is driven by fuel, another process that is supercritical that is driven by fuel. But this fuel is a fuel that has not been disrupted yet, and here I'm referring to plenum fission gas compaction. So we need to distinguish between those three modes of obtaining superprompt or prompt bursts.

So your recriticality is really a short, a terminology for describing something.

MR. CARBON: Okay. Then on down on the same page, the paragraph on down, "We rely heavily on special purpose analytical methods and experimental evidence to scrutinize and guide system code calculations," the point being there that you are saying SAS and SIMMER calculations do not represent the essence of your efforts, but rather that special purpose analytical methods and system code calculations will be the things that you rely more heavily on.

I don't know what you mean by "special purpose analytical methods." Will this be coming out later?

MR. THECFANOUS: This will be coming out, and if by the end of the technical discussion this point is not here or if you don't have enough examples, because obviously we did not put all of the examples of these type of things in the presentation, then please tell me and I will give you some more examples.

But this means usually a homemade, a quick computer code or a back of the envelope calculation or analysis of a model other than analytical activities that come in to help interpret this information and therefore guide the system. And we think that is a very important point I want to emphasize.

Of course, as all of us know, it's a very controversial one. But we have the philosophy that those codes here really do nothing but represent our understanding of a given situation. We don't expect the codes to give us a new understanding, but only to integrate for us, basically to bookkeep reactivities. That is really the name of the game here.

That is something you cannot do in your head. You have to integrate all of these reactivities and power histories, but the phenomenology, what is really happening, we expect fully that we will get guidance from the code to give us that. I think we have examples here.

MR. KASTENBERG: Theo, I have a question. In the material you gave us, you listed the eight areas of concern.

MR. THEOFANOUS: Yes.

MR. KASTENBERG: And in reading your document I couldn't tell whether you had resolved some of them yet. For example, the first one I have in front of me has to do with the TOP, T-O-P, accident. And I couldn't tell from reading this that the Applicant has addressed that first issue.

MR. THEOFANOUS: Yes, the Applicant has addressed it, Bill, and we have addressed it. And there is another whole part of documentation that goes with that. And as you will see, our presentation is very tight, so we thought we should focus into one aspect, the one that is historically the most difficult one.

But the TOP is being addressed from the point of view of driving ramps, what are the appropriate ramps to drive the TOP, and the probabilities associated with that. That is being addressed right now as part IE 1 tasks. Given what the Applicant tells us -- and we hope we will be able to confirm that through the IE 1 tasks -- but if we assume something on the order of less than ten cents per second, our team and particularly the team at Argonne, Carey, Helmer, Physic and Olsen, tell us there's no problem with worrying about autocatalytic behavior under TOP conditions.

So in a way we have resolved it. But there is one thing to confirm, and that is taking this ramp rate, the driving ramp that is given from the Applicant to us.

MR. KASTENBERG: I recall at the May meeting there was some question raised on the TOP for some of the cases where they ended at these intermediate powers. The consultants and the Subcommittee raised that issue, and I didn't see it addressed as one of your areas of concern. Do you feel that is not an area of concern?

MR. THEOFANOUS: Well, no. This again is part of the -- we are going to address that and we are going to come out with a technical judgment on this particular problem after we have put a reasonable bound or number on the driving ramp, because that is a very strong fraction, what it is driving it, and that is currently under review under the IE 1 task.

However, it will be considered as part of what you saw in the previous slide, scoping out the disruption phenomenology. We will go through and scope it out to see whether that falls into some mode of recriticality, and if it does we will use some of the recriticality results we will hear today to assess that.

MR. KASTENBERG: Could I ask you to do this. As you run through this, of those eight areas of concern that you will address today, could you tell us which ones they are, because it wasn't clear in reading the document that you were actually responding to some of those areas. I had to keep reading back and forth to see.

MR. THEOFANOUS: First of all, the Applicant is supposed to be responding to those areas. We examined the questions, but we will discuss today everything in those eight questions that relates to the loss of flow accident, and I think if I remember it's all of the next seven. The first is TOP and all of the rest are loss of flow. All of the rest should be coming out of here, and if still something doesn't come from here please let me know and we can discuss it.

MR. CARBON: Would you flip over to number 7, I-7, 1-7.

MR. THEOFANOUS: Yes.

MR. CARBON: This has to do with the sodium void worth values. Would you comment here on, why are there different values being used here now than have been used previously?

MR. THEOFANOUS: I think the answer is, because the old ones were not correct. And why they were not correct I think we don't know. We have to ask the people who developed them.

MR. CARBON: Because why?

MR. THEOFANOUS: We don't know. We would have to ask the people who gave the first figures. All I can say from our point of view is that this first sodium worth came up in one of the eight questions. We were asking the Applicant for the uncertainty in sodium void worth. We were interested in this uncertainty and the correct value of the boundary around it, because as you know it has a great influence on the potential for loss of flow-driven transient overpower.

Following this question for the Applicant, we went back and recalculated, basically, the numbers and came up with a larger best estimate volume, as well as with an uncertainty bound around it. However, it so happened that the uncertainty was reduced by more careful scrutiny of that.

So what we ended up with was a larger number for the sodium with a smaller uncertainty. Now, this had a very measured effect, we believe, in our perspective on the whole evolution of the loss of flow accident. And we tend to agree with the current values. There is a relatively small area of controversy, and I would like to take a couple of minutes to explain that.

The project calculates a better volume now that is somewhere around 1.9 dollars. Then there are experimental data that show criticals, that show a sodium worth of about 1.4. Then the project is using the experiments to bias the calculated results. There is a systematic bias in the results. You will hear more about that in the afternoon from the project.

Therefore, we have to reduce our calculated values down to some value consistent with the experiments. We seem to have a little bit of a problem with this bias. It's not a real serious problem, but a little bit of a problem of interpretation. And we haven't talked to a lot of neutronics experts. Even they cannot quite agree with the detail of it. But I want to emphasize, it really is a detail.

What you will see us using here is a number that is a somewhat larger value of sodium worth than the project, but it is not significantly larger. We will be using something like 1.7, and I think you will have a whole presentation on the subject by the project in the afternoon.

But the interesting thing to point out here, as a result of this increase this problem also was somewhat aggravated, because, as you will see later, this problem -- the faster, so to speak, the core is the more severe this problem is. And of course, the higher the sodium worth the faster the core becomes.

Are there any other questions?

MR. LIPINSKI: Clarify your statement, the faster the core becomes. Are you talking about ramp rates or spectra?

MR. THEOFANOUS: Timing between events, power.

Are there any more questions up to Section 2, including Section 2?

MR. OKRENT: I only have one question. Is there work being done on ex-vessel containment and in-vessel containment as a part of this task group?

MR. THEOFANOUS: Oh, yes. Now, I can quantify this here. The ex-vessel containment means failure of the primary through a disassembly, as is shown here.

Everything else coming from here or there is ex-vessel containment or in-vessel containment, which means we have a permanent subcriticality within the vessel.

Now, from that point on it is the job of the TMB to look at it. So there is another -- out of here, it continues on through another group, another technical effort looking into thermal margins. We are not looking into that.

MR. CARBON: I have a question on that slide. I have a problem with your center red arrow, the dispersal task. The task, what is it that is significant there in terms of a straight line from the interruption down to the complete, other than going through the mild termination or the energetic termination?

MR. THEOFANOUS: Of course, the significant part is that this represents a continuum of disruption states, and what we want to portray by this picture here is that the disruption begins to localize at some place, and we'd find that any place in the core where the first cladding becomes molten, where the structure begins to change, from that until the complete disruption. That is the one in which somehow no material moved out of the core and all of the material is molten within that cylindrical confine. This is sometimes known as the whole core pool.

The core then, the core undergoing a loss of flow accident is going to experience a continuum of disruptive states as it proceeds from here to here. What we like to portray here is, there are paths, exit paths from this porcess, and this exit path can either be energetic or mild, normally referred to as the special.

If you enter the exit path, basically your energetic problem has finished. Especially if you enter it this way, you are at the end of energetic concern. If you enter this way, you still have to examine whether the primary system fails or not.

The important point, however, is, and you will see later more clearly, the potential for energetic disassembly is different throughout these core disruption states, and the severity should one energetic member occur, the severity would be different also because of fundamental physical phenomena I hope will become more clear later.

Therefore, one needs to be aware, at what point does one exit and terminate the accident all along this continuum of disruption stages.

MR. CARBON: Okay, thank you.

MR. KASTENBERG: Theo, where does the vessel melt-through come in?

MR. THEOFANOUS: That comes beyond that point, from this point up. Our review is only for the energetic events, so after we heat this box or this box we are finished.

MR. KASTENBERG: It is a little misleading if you don't have a little arrow coming in there.

MR. THEOFANOUS: All right, we will remember that.

MR. KASTENBERG: It seems you are making a supposition you can hold it in the vessel if you take that, and that's not true.

MR. THEOFANOUS: Yes, that's right.

MR. CARBON: I don't get your answer to that question, though. The ex-vessel containment is --

MR. THEOFANOUS: I think what Bill is saying is, this is misleading in the sense that it leads one to believe that the whole accident is all finished and the material is inside the vessel forever. And what I am saying is, in this team we are concerned with the energetics and therefore if we hit this for us the accident is finished, because all we are worrying about is energetics.

But there is another team in the NRC that worries what happens beyond that point, whether it will penetrate the vessel, when, how, and what will be the consequence to the containment.

MR. CARBON: By the same token, the right block there, ex-vessel containment, indicates you have already gone out of the vessel.

MR. THEOFANOUS: That is true also, and that also does not, I hope, give the implication everything is finished, because after you get out of the vessel you have to worry about whether the containment holds and for how long. So up to here is our range or area of interest, and there are other steps beyond that point that I guess other people have to worry about.

Really, I think I have talked to this slide as long as I want or need to, and I only show this because the next one, as we go to section 3 now, will show you --

MR. CARBON: Wait a minute. Would you go to slide 2-2 and comment upon initiator 3?

MR. THEOFANOUS: 2-2 is this one. Initiator 3 is the seismic events and the loss of piping integrity. Here we are looking for -- again, that is really shooting out in the dark. We kind of believe that maybe people in general have not looked as thoroughly as they should into what an earthquake beyond safe shutdown can do to a reactor, and we have a number of structural people looking into several aspects of the system to tell us what we can possibly expect.

And if there is a phenomenology, again, if there is a set of physical phenomena that is too widely different from what we are already considering, we would like to address that. It doesn't mean necassarily that they will become a very significant risk contributor. But we like to be as complete as we can.

And this also, piping integrity, is one of the accidents that have been looked at a little bit in the past, again I don't think in as great detail as they should, and somehow this is connected to structures and seismic events and that is why it is part of it.

MR. CARBON: On to number 3, I guess.

MR. THEOFANOUS: Number 3 is just an example of a sample task definition. Really, there is nothing to say there. It's self-explanatory. But to give you an idea, again, we knew what we were looking for, so that when the probability was unlikely we knew already because we had done it already.

At the time this was written, we knew the autocriticality behavior was demonstrated to us to be unlikely, and the project had done their independent analysis already and they knew what to expect. But we had a few loose ends to tie, so to speak, and that is why the imperative nature of this objective. And as it turns out, in fact, it is correct.

And I guess maybe it would be worthwhile to highlight the current status. We have reached a consensus within the team on the approach, the monitoring plan and the tasks. We have reached consensus, we believe, with the project on crucial points of assessment. We have essentially completed the loss of flow accident, and remaining is to consider the I tasks.

We are working on them now and plan to complete them very soon, and then document all of the details. Now we are going on with the 3-1, and this looks exactly the same as the framework that I gave you for the management plan, except for having discretized this continuum.

We have discretized it into two. If one is confronted with a continuum, there's an infinite number of points and combinations, and analysis can never be done. We believe we can identify certain stages of core disruption that are significantly unique in their structure, that can be addressed generically.

As we go there from the pin disruption, which is addressed over here, the next stage is subassembly disruption, and this process continues on with pins melting and disrupting, but with the subassembly walls being more or less intact. Now, obviously not all subassembly walls are going to melt in exactly the same time, so the transition from this stage over here to the next one, which I will explain in a minute what it is, will be somewhat confused.

However, we have a highly discretized core disruption stage over here and we believe the next significant state to be addressed is this annular pool, which I am going to show you, is I guess dictated by the structure, the heterogeneous structure of the CRBR core. What we have here is the driver, three driver rings, the inner blanket region, the outer blanket region, and we have driver fuel interdispersed into the internal blanket.

Obviously, there is a very great difference in power between the driver and the blanket, and we expect the driver assemblies will go first and their walls will go first, in fact. And if it was only for thermal effects -- in other words, if we let the blanket melt by its own power -- this would take somewhere upwards of ten seconds.

In fact, we believe that in reality it wouldn't take that long, because these blankets would be attacked from inside and outside through those driver fuels and will be then made into disrupting pellets, mixing and melting. That will happen in some time less than ten seconds.

But in any case, there is some delay between the formation of an annular pool and what is known as the whole core pool, which would involve this whole region, molten and mixed up. That would be then a full cylindrical pool.

MR. CARBON: Hold up a minute. I am having trouble relating to your sketch, which is different than ours. What is the blue?

MR. THEOFANOUS: The driver fuel.

MR. CARBON: But that's not right, is it?

MR. THEOFANOUS: Excuse me?

MR. CARBON: It doesn't seem right compared to -- oh, the blue and the white together.

MR. THEOFANOUS: Of course, not the white in here. But I want to emphasize here the annular structure. This white here is driver, inside the inner blanket.

MR. CARBON: I was trying to separate the white from the blue and you lost me. Would you go back and start again on these pools?

MR. THEOFANOUS: Yes. These are the three outer driving rings, and this is the driver fuel contained between the outer blanket and the inner blanket. The inner blanket itself is interdispersed by driver fuel also. So when the -- because of the difference in power between the blanket and the driver, the driver is going to melt first; and therefore, furthermore, the blanket will delay. If it were to melt only by its own power, it would delay by something on the order of ten seconds. We believe in fact the disruption of the blanket would be faster than that.

But nevertheless, there is a lot of thermal inertia here and it will take time before all of this becomes one big cylindrical pool. That is what is classically known as the whole core transition phase or a number of different names, and that would be the case if you had a homogeneous core, for example, this case, you go first through the annular pool and the last step is the whole core pool, as is shown over here.

Now, we believe that it is legitimate to do that. It is not only practical from the point of view of having to deal with discrete states; we believe also it is legitimate because there is a weak memory in the system in going from one state to another. What I am saying here is, through the initial phase disruption, the initial stages of fuel disruption, the system is more or less deterministic. Still, it can have some probabilistic behavior, but it is pretty well deterministic.

By the time the fuel begins to move, the fuel motion so strongly affects the power of the system, the power history, that from that point on we believe that the time from one state to another is somewhat disconnected, with a short memory. This is not to say, however, that if one were to lose, as I am going to argue for later on, if one were to lose five percent of the fuel over here, this is not to be remembered later on, because there is less fuel to go around and that has significant impact on the reactivity potential and the recriticality potential of the system.

MR. CARBON: In your concept, an annular pool would be essentially sort of a pool within the core.

MR. THEOFANOUS: It would be really a part of the core. The core is defined as -- I guess I would define it as this whole thing.

MR. CARBON: Yes.

MR. THEOFANOUS: That is all generatng power and it has a certain amount of fuel in it. However, we separate the core into two parts. One is the driver and that produces most of the power, and the other is the blanket. So the annular pool then would be an annular space there that is within that core region. It will be part of the core.

MR. CARBON: Being molten over some distance, some height, such as the fluid or --

MR. CARBON: Typically, that would be really almost the whole height of the reactor fuel, about three feet about the middle. So this pool has the dimensions of about a meter by four subassembly wheels, something like that.

Okay. Now, I want to get into the real controversial --

MR. CARBON: Before you leave that, what is the significance of the small A, B, C, D, E? Does it have any?

MR. THEOFANOUS: This is just a key, a key with what is written in the written part. Section 3.A, for example, refers to this one, B and C refer to this one, and D refers to this one.

Maybe also I would like to point out here that this is a continuum of states all of the way from here to here, while those two are processes. So those two processes are allowing to bypass a number of states. If you enter those axes you go straight from whatever state you enter all the way to the end, and that is permanent subcriticality.

This with the CRBR core means removal of upward of 40 percent of the active fuel in the core. We feel really it is closer to reality to think in terms of 30 percent, because if you consider also the timing and what the blanket is doing in this period of time and what the still is doing, it's more like 30 percent for the range of interest. But something like 30 or 40 percent leaving out of the original core confine would mean that is the termination of the accident as far as energetics are concerned.

Also, maybe I should point out that these little letters here are just to identify the paths and to identify that we have one path going into disassembly from the initiating phase, the initial disruption, and then there is a path over here showing that some portion, a proportion of those disassemblies, are going to lead to failure of the primary system, and all of the rest of them of course will go this way (Indicating).

So if you want to be exactly precise, I didn't plot it here but this part here should also be four, alpha, beta, gamma and delta. So here we wrote, alpha, beta, plus gamma, plus delta. So this hour here is complementary to those four hours over here.

And we are showing -- another point to make here is, we are showing four hours, because, as I said before, the potential for doing damage to the vessel is a function of where you are disassembling from. So we will take a separate look at these assemblies from each one of those different states.

The next is getting us into the real controversial aspect, I expect, of this presentation, and that is talking about probabilities. The way that we view our task is to come here in this very general framework, which is pretty generic to core disruptive accidents, and put numbers on those arcs, and by doing all of the multiplications and some measures to come up with a vessel failure probability. And that is a conditional probability, given a loss of flow accident.

Now, we know this is a very difficult thing to do and I know there are people who might in fact doubt our ability to do that. In our presentations of this -- and we have a couple of presentations up to now -- we got mixed reactions. There were people who wanted to see whole distributions, not only frequencies, not only single numbers in each of those, but they wanted to see whole distributions. They said, if you don't know the whole distributions you can't put a number there; that means you know nothing. There were other people who said, you cannot put numbers or distributions because you don't know enough.

We understand those limitations and we try to explain some of that in the written summary here. All I want to say is, we understand it is a very difficult job. However, we also believe very, very strongly that someone has to start throwing some numbers around. We have to put numbers, leave them up for discussion and criticism, and if someone has a better number to put there we are willing to discuss it.

However, unless you have numbers you cannot have a quantitative idea of what's going on here. The problem is that you have more than one path and you have more than one step that gets you into the vessel failure. So if you go qualitatively describe each one of those steps and say, I believe this is very unlikely and that is strongly unlikely and that is possible, then there is no way you can multiply all of those words and come up with numbers at the end.

So we had to have a way of coming to the bottom line, and in fact I believe that some of the ACRS Subcommittee members through telephone conversations have specifically asked me for such numbers. So we've made an effort to do that here, and we would of course greatly appreciate your comments and criticisms.

In doing that, we tried to keep a certain degree of consistency. I think that the choices we made might not be agreeable with everyone, but I would like to emphasize, first of all, that as you look at the numbers we are going to give you in the conclusion section, it is very, very important to remember that we assigned those numbers on the basis of the following definitions here.

We have defined, first of all, a set of probability splits, so to speak, so we attach some meaning to those numbers. Therefore, the end results should be interpreted in terms of those meanings. Again, this became necessary because we have to follow sequences or steps which involve more than one in those probabilities. If it was only one step we were worrying about, it wouldn't be enough to make any of those statements and we would be finished. But here we involve more than one step, so we have to deal with multipliers.

Therefore, as we look at those meanings it is important to look at consistency. For example, if you had one event that was one in ten because the behavior was known within known trends, but was obtainable only at the edge of spectrum choice of the parameter, and if this process was followed by one whose behavior was reasonably known, but you could only get this particular behavior by making choices of the parameter values outside the spectrum you considered reasonable, then the sequence of those two processes together qualitatively I think should be something close to being incredible.

Therefore, this seams to be consistent. That's why we chose the number one in a thousand here. I think that is the main point. Someone might really disagree with that and say, how can you assign such a high probability to an incredible behavior. If it's incredible it will never happen and you should give it zero.

Of course, obviously zero never exists. However, for the purposes of being consistent here we thought we wanted to stay at this level. So one way of looking at that is, maybe one in one thousand is a very pessimistic way of looking at the credible things, maybe. But one thing I want to caution you: Incredible -- when you do a PRA and you look at the front end of the spectrum of those accidents, you have a different data base. You worry about machine failures and you have a good data base for that. The meaning of probability is something much more quantitative there.

In this general framework, an incredibility, a phenomena incredible deserves to be given a phenomena much less than one and in fact less than 1 in 1000. So you want to emphasize you have to be careful between taking numbers in the front end and multiplying with numbers over here to get a whole perspective.

On the other hand, this is not to say that this is not meant to give you a quantitative feeling of what you believe the bottom line will be. Rather, what we are doing is cautioning you and saying that you look at the bottom line and look at the number and go back and reinterpret that in terms of this, and now carry that interpretation over to your PRA as you look at the front end and the tail end that has to do with the containment failures to put a number for this step going from loss of flow accident or CDA to vessel failure.

MR. KASTENBERG: Theo, the only thing I would like to comment on that is the one-half. Do you really mean what you say, or do you mean that I have two choices, and I really have no evidence to support one choice or the other; therefore, I give it a half?

MR. THEOFANOUS: That is exactly what I mean. Thank you, Bill.

MR. KASTENBERG: It doesn't say that.

MR. THEOFANOUS: Thank you, Bill. That is also a controversial aspect. In fact, one can make the argument here that if you have enough steps in the path and if you don't know anything along the way, by putting enough one-halfs there you can convert total ignorance into something that you think you know something.

And, of course, I have been for a long time a real opponent of this kind of approach, so we have been very careful in that respect by making an event tree that has a discrete and small number of steps so we don't fall into this.

And furthermore, as you will see, we only had this one-half -- it appears only at one line near the end, and that is the whole core pool. By the time you got there already the probability is so low really you don't care what you put there anyway. But we'll come back to that.

The important point is that the only place where these numbers show up in the end, in the conclusion is in a state that we don't believe, and the numbers support, we will ever get to. And probably that is the reason we know so little about it. And so the one goes with the other, and that is why we say we will give it an even chance to go either way.

MR. CARBON: That is the only place the one and two comes in?

MR. THEOFANOUS: That is the only place it shows. It shows going from the cylindrical pool to termination or disassembly. And there were some recent questions arising from similar calculations in whole core pools having to do with sloshing and coherence; that we feel there's enough uncertainty there that we don't want to weigh more the dispersal as opposed to disassembly, so we give it an even chance.

MR. OKRENT: Is it important that there be a sodium pool above the core region in order to damage the primary containment?

MR. THEOFANOUS: Well, the sodium slack is the means by which you focus the thermal energy into mechanical work. You focus it all in the head. It is very important, and you can see that in experiments. If you do one of those experiments, if you get half bigger than the other one, you get much less impact. It's very clear.

Someone might say why don't you fill it up completely, but of course you can't do that. The slag of sodium is important in focusing the energy.

MR. OKRENT: But if there were no sodium above the core --

MR. THEOFANOUS: Yes.

MR. OKRENT: -- Do you have an estimate as to how severe a transient, how much energy release you would have to have in order to threaten a loss of containment from the primary containment?

MR. THEOFANOUS: This aspect we in fact intend to go into. Before the vessel you have another enclosure. It is almost like a cage. You can almost view the core as being enclosed in a cage with very strong structural components.

So really your question, and as you will see, we are going on the path now that really the energy-absorbing and really the one giving us most of the margin is that inner bag, to so speak, rather than the whole bag surrounding it.

So from that point of view what is happening outside is not really all that important, although if you were going to exceed a certain level of energetics that we think is pretty high, then of course my tendency would be to say that in that eventuality of course you would be violating the bags. And in that event if you didn't have the sodium on the top, I think the effects from the head would be less pronounced.

On the other hand, you could be -- I can conceive of piercing holes through the side wall by direct impact of the molten expanding stuff and hitting directly on the side walls. But if you look at the time scales involved here, I think even that would be doubtful.

So, in general, in regard to your question, I personally feel we didn't address it in the group, that eventuality, because it's part of the loss of heat sink failure. And if that were to happen, I think the impact on the head would be less pronounced. There would be no real, direct mechanical damage on the head.

Of course, if you go to extremes of getting very, very high energy limits outside the realm of possibility, you could be generating the whole UIS itself moving up with such great force.

Now, this goes and hits up the top of the vessel, but this can happen just as well when the sodium is there.

MR. OKRENT: Well, has the group developed an assessment of what is the limiting reactivity insertion rate or whatever criterion it wishes to use for accidents where you no longer have sodium above the core? And I'm not sure I would use only limiting reactivity insertion rate, in fact, for that event.

What I have seen in here is a number like $100 a second as sort of a threshold or the event where you do have sodium, if I understand what I read.

MR. THEOFANOUS: Yes, that is correct. And the answer to the question is we have not completed -- this is part of the I series. We don't know how a loss of heat sink accident, if it were to lead us into energetic behavior, we don't know what the thing would look like. We are now scoping it out.

Now, if we find out that we are in a situation of having the core highly disrupted and potentially becoming supercritical with no sodium involved, we will certainly look into that. I think, however, that you have no mechanism of transferring the energies. The whole thing going with the water reactors, if you don't have the slack there you might get some limited steam explosion which does nothing to produce missiles for you. You don't have the energy coupling.

I think in that sense if we were to evaluate this case, the result would be allowing greater energetics, so to speak, by the primary system. You have no mechanism to get that energy converted back to impact.

MR. OKRENT: You might have some weakened structures temperature-wise. I don't know how important that would be.

MR. THEOFANOUS: That certainly would be the case, and that is exactly what we were concerned with, in fact, that the structures weaken so much they run away from you.

We think a loss of heat sink accident is possible. The whole vessel might be creeping under high temperatures, and you might get some structural failures before even you have sodium boiling, much less after you vaporize all of the sodium. But we need to look into that.

That is again part of the I series. Today we are focusing only on the loss of flow accidents.

MR. OKRENT: And just one last question. Are there any mechanisms physically possible whereby you can drop down in sodium level a la TMI and then get sodium back in at a reasonable rate?

MR. THEOFANOUS: Not that we have identified. Even the pipe break will get you a limited inventory loss.

And now then we begin Unit A which is addressing some of those questions of disassemblies and energetics. We thought we would start from that point because as you look for an energetics assessment you need to have an idea of what you are looking for, what kind of level of energetics would be of consequence to the primary system.

We define as an energetic termination one dominated by a ramp rate of greater than $30 per second, and that is in a two-phase fluid. And why I make a special effort to qualify that you will see in the next few slides.

If it is a two-phase fluid involving fuel you will need something more than $30 per second to produce the few bars of pressure in a short time scale, a few milliseconds; therefore to categorize it or identify it as an energetic event. And to obtain this kind of a ramp rate you have to have rapid material relocations. That is what will change the reactivity of the systems.

There are three materials in the core: sodium, cladding and fuel. And they, of course, they have worths; they have reactivity worths. And their rapid relocation could give rise potentially to these kinds of events.

For the sodium, for example, reactivity, the whole core sodium reactivity is something less than $2. In order to produce an energetic event by removing the sodium from the core you would have to remove it less than .07 seconds. This is a very good example of what we consider an incredible event. We know positively, we can argue today, that this can't happen, although there was at one time -- I remember when I was still going to school that this was in fact the way LMFBRs were supposed to disassemble.

The idea there is the sodium is heating the core. It is heating, heating, heating and superheating without being able to boil. It reaches a high heat of superheat, and it produces a voiding. And, of course, these kinds of rates are not out of the question.

However, we've had extensive experience since then that indicates that such voiding is impossible. The same thing with cladding here.

MR. CARBON: Before you leave that --

MR. THEOFANOUS: Yes.

MR. CARBON: -- I appreciate that the consensus is that that kind of voiding would be essentially impossible, but I would still ask is there a residue of opinion like in the fuel coolant interaction case where some people feel it is possible, or is there no residue of opinion?

MR. THEOFANOUS: Are you thinking in terms of getting sodium voiding through a fuel cooling interaction?

MR. CARBON: I am saying are there some people who believe --

MR. THEOFANOUS: The question of FCI, yes, I think certainly there are some people, and I think in fact there will always be some people who will always feel you could have a fuel coolant interaction.

MR. CARBON: No, no, not fuel coolant interaction -- superheating triggering a vaporization.

MR. THEOFANOUS: Oh, I haven't heard of any of these people since the last time I was in Europe a few years ago.

MR. CARBON: So there are no people?

MR. THEOFANOUS: I don't think there is anyone. But if you do an experiment in a laboratory and take a special precaution that involves pressurizing the vessels --

MR. CARBON: Yes, I know.

MR. THEOFANOUS: -- Then of course we can get it. You can get very high superheats.

MR. CARBON: But in a practical case like this --

MR. THEOFANOUS: No.

MR. CARBON: -- There's no significant --

MR. THEOFANOUS: No, because you can point to a very large number of data in pile, out of pile, anything that looks remotely like an LMFBR bundle has never given them superheat.

MR. MARK: Theo, is there essentially universal agreement on that first statement, that the sodium worth is no more than $2?

MR. THEOFANOUS: For the CRBR, yes. I'm

referring to CRBR now, everything I say today.

MR. MARK: Oh, of course. For the present design?

MR. THEOFANOUS: For the present design, yes.

MR. MARK: Everyone feels this applies?

MR. THEOFANOUS: Yes.

MR. MARK: What fraction of that $2 is carried by just heating the sodium from its nominal running temperature up to boiling temperature?

MR. THEOFANOUS: I would suspect a very small fraction because that is total void. That is actually taking it out.

MR. MARK: I realize it. If you just heat it, however, you take some out.

MR. THEOFANOUS: That's right. I would expect it would be on the order of 10 percent. Just looking at the density variation I would suspect 10 percent.

MR. MARK: Okay.

MR. THEOFANOUS: But you remind me of an interesting question. I think someone asked me on the telephone, one of the subcommittee members, what if the core is different. Of course I want to emphasize everything we are going to say refers to the core that is before us for review; and if someone at a future time wants to put up another core, it would have to be reviewed, I feel, exactly the same way we do for water reactors. And if there are some benefits there, of course, go ahead.

MR. CARBON: The wording in the report says that the maximum sodium void worth is well below $2. Is the .07 seconds based on $2?

MR. THEOFANOUS: That is based on $2, yes.

MR. CARBON: Okay.

MR. THEOFANOUS: Now, the cladding is around $5 total reactivity. It would take a time of removal of the cladding of two-tenths of a second, and we believe that is also truly incredible. We have no problem assigning to that the probability of $10^{-3}$ or even less. However, why this is improbable I think you can really appreciate that. I will speak about cladding later on.

The fuel worth is $1 per centimeter. If you take the whole core of the CRBR and compact it by one core all the way across, you would increase the value by $1. Therefore, if you were to make this compaction at the rate of 30 centimeters a second, that is what you would need to produce $30 a second.

Now, this kind of velocity is not something very dramatic. It's quite a bit less than you would be obtaining if you let the fuel melt and just slump under its own weight. Therefore, from the point of view of energetics, this is the primary reactor. This is the material which through its relocation can give us an energetic event.

MR. LIPINSKI: What is the total worth?

MR. THEOFANOUS: The total worth?

MR. LIPINSKI: Yes. You have it only per centimeter. What's the total upper limit?

MR. THEOFANOUS: Oh, it's a lot. I think the whole core is $140.

MR. LIPINSKI: Completely compacted.

MR. THEOFANOUS: Completely, yes. So I think this is taking that into consideration, because well before that the process would be disassembly. But you can see why this process can be.

Charlie.

MR. BELL: I think there might have been a misunderstanding on that last answer. The total compaction would be $30 to $40. The total fuel worth is like $140 to $150 if you removed it.

MR. LIPINSKI: That's why I wondered, because he had upper limit numbers in the case of sodium and cladding where he didn't put an upper limit in terms of the fuel worth.

MR. THEOFANOUS: Because here I couldn't take

37 minutes per second. I know I cannot compact it.

MR. LIPINSKI: If it were less than $1, it would not be a concern, but if it's greater than a dollar, it is,

MR. THEOFANOUS: No, no. It's a dollar per centimeter.

MR. LIPINSKI: I understand that, but if you had only less than a centimeter of motion it would be of concern.

MR. THEOFANOUS: Well --

MR. LIPINSKI: You are assuming it's already greater than $1.

MR. THEOFANOUS: No, no, no. Even if it were 50 cents per centimeter it would be of concern.

MR. LIPINSKI: Total worth, not incremental. If you could only move it and get half a dollar you would not be concerned with the phenomena.

MR. THEOFANOUS: All of the fuel?

MR. LIPINSKI: Yes.

MR. THEOFANOUS: Slump it all of the way down? Of course.

MR. LIPINSKI: That's why we need to know what the total number is as well.

MR. THEOFANOUS: Yes. We wouldn't have any problem if that was the case. That is the only actor, therefore, that can give us energetics. However, this is not to say that those two material relocations are unimportant because they set the stage in which the fuel motions take place. And what is the power level at which time, for example, the fuel begins to move is very important on the direction as well as the intensity of fuel motion.

In addition to those material relocations one also needs to take into account significant negative impacts which help set the stage. That's of course the Doppler, the axial expansion. This is axial expansion of the fuel pins why they are still integral. As they are heated they want to expand. That is a very significant negative feedback. And of course, finally, the vapor and fission gas pressures that induce fuel motion. And typically this fuel motion is dispersing which leads to less reactivity.

That is why I classify this as negative feedback. We have to say more about that later.

In addition to that, to put these worths here, and this philosophy requires as a minimum -- to get energetic events into a proper perspective I think we should mention that the whole core is made out of 156 drivers, 156 subassemblies containing fuel, and their power distributions, and their flow distributions. And each of the subassemblies behave differently than its neighbor. The timing of events is different.

Therefore, when you look at the figure of $1 per centimeter, you need to remember that not all subassemblies are going to be moving at the same time. This we refer to as intersubassembly incoherence.

In addition, we think within each subassembly we have 219 pins, and each one of those pins within the subassembly will behave differently than the next one. This is true even in homogeneous cores in which there is no mass power toting within the subassembly. Because of the wall you get cooling near the end.

For this core over here you have up to maybe a 30 percent power slump across the subassembly, so you will have a significant timing or delay difference, timing delays within a subassembly. We refer to that as intersubassembly incoherence.

When it comes down then to each of those processes, we need to worry about both of those aspects; and we will see some examples of that.

Now, here we have a little illustration of an energetic event. We had some questions from subcommittee members as far as how rapidly the power rises, how rapidly the pressures develop and so on. Here is a typical two-phase assembly from a driving reactivity of $50 a second. You can see the power rises quickly to 4,000 times nominal, and very quickly also, within the matter of a few milliseconds, goes back down to zero.

We say that the core disassembles in this case. This was done for the CRBR core, and the disassembly comes about by negative feedback from Doppler and by negative feedback from the primary one of shutting down fuel motion. Fuel must move outwards from a high worth to a low worth in order to produce the disassembly. The pressures rise, in this case to a 100 bars in the center. But this is a high flux region. This is a very localized place. And very quickly they drop as that expands, as it pushes fuel out. And it comes out eventually within a few tenths of a millisecond to something referred to as a quasistatic pressure.

As far as doing damage to the structure, this is what is significant. Here we have shown the variation of the peak pressure and the average or quasistatic pressure as a function of the ramp rate. What is interesting to note here is this scale is ten times more than that, and there is roughly one order of magnitude difference between the high pressure and the peak pressure and this quasistatic pressure.

What you see here and the thought I want to leave you with from this slide is it takes about $100 a second to produce about a hundred barss, a hundred atmospheres. I round it out because it's easy to remember 100 for 100. In reality, it's more like 75. And that is also a function of how can wall and steel mixing with the fuel and heat transfer between the two materials, how that can bring the pressure to even lower limits.

Was there a question?

MR. CARBON: I have a question on the second slide there. It indicates there are one or two calculations. I would like to inquire how closely can you come to coming out with the same general results if you did this on sort of a back-of-the-envelope kind of calculation.

Isn't it possible to carry out an estimate that you know would be in considerable error but maybe within a factor of two or some such thing, the result you get there, or are you flying strictly on the basis of the code calculation result?

MR. THEOFANOUS: I understand the thrust of your question, and I think we have made some efforts in this direction -- how much confidence do we have in these numbers being produced by SIMMER -- and I want to address that. In fact, it's the next vu-graph. Let me put this up.

One effect, in fact, that is interesting and that is the one in the letter Dr. Okrent referred to before is if you have a single-phase fluid the fluid upon heating in a fluid disassembly expands very rapidly because of thermal expansion and gives you very rapid disassembly. And people who have been doing calculations using various codes -- and we have a number of them around -- they have been accustomed -- they almost came to believe you can get any energy out of the fuel no matter what ramp rate you impose on it.

On the other hand, there are people doing similar calculations with two-phase fluids, basically a two-phase core, and they found out they are able to produce enough energy in the core by something like $50 or $100 per second. So for some years there was a discrepancy.

We wanted to investigate that further and give you an illustration of what is controlling and why there are such differences of opinion. After that I will come to another interesting result that came out as a result of this exploration here that I think you will like.

First of all, then, there to illustrate the effects first and to illustrate the basically bottom line, the bottom line is there's no difference in codes because basically the process is so fundamental. It is as fundamental as F = MA. You have such and such pressure. You will produce such and such more as long as the code does not do drastically something wrong; and one certainly knows that if the code is as wrong as not being able to calculate motions.

The real difference is because people were doing calculations using different material configurations, and we wanted to illustrate that here. We have two test cases, one-dimensional disassemblies, one-dimensional disassemblies. In this case we allow the whole core to be compacted so the whole thing is liquid.

Now, we know we can't do that neutronically because it would be, well, supercritical before that. But this is a calculation purely hydrodynamic. We impose a power pulse which would resemble one of disassembly. It goes up to 8,000 normal power within one millisecond and then goes back down to zero within another millisecond. And then we tried to see how the two one-dimensional systems respond to this to tower pulse.

From this and from the knowledge of how much fuel removal we must have in order to produce shutdown, we can identify any real differences in behavior between those two systems as far as energy-absorbing capability. So here then we are indicating that it will take about two kilograms per subassembly to move from the central half of the subassemblies in order to reduce neutronic shutdown.

We have 150 subassemblies taking out 2 kilograms each, so that means 300 kilograms over the whole core. That means the whole core -- the half of the core has a volume of about 1.5 cubic meters. That means we have to have a reduction by 200 kilograms per cubic meter over the whole core similar density in order to achieve shutdown.

Let's see in the single and two-phase calculations how much time it took to produce this kind of reduction in density. Here is the single phase. This is the initial density distribution. It is almost 77 or 7800. Within 24 milliseconds already the density has dropped by 100 kilograms per cubic minutes. Within .8 milliseconds it's well beyond. You see, it goes from 77 down to below 74; so that within .8 milliseconds if this were an actual core undergoing disassembly it would have long been shut down.

Of course, the reason for this is the whole thing is single phase. The pressures are very peaked and very quickly develop to 400 bars, 600 bars, and of course push the material up. I also need to indicate here there's a change of scale; so whatever mass has come down from here has to show up over there. And the reason it doesn't show as big is because there's a change in scale.

Now, looking at the two-phase disassembly, you start out from a smear density about half as much as the single phase, about 4,000 and, look, 1.6 milliseconds later the density has hardly decreased by 100 kilograms per cubic meter. That means this core now, if this were a core disassembling, would be still absorbing energy. It would still be before shutdown. So that is a fundamental behavior.

You see here that in .4 milliseconds here we have 400 bars and here we have almost nothing, so there is no pressure to push it because here is the pressure has to come from vapor pressures, and of course you have to heat it up before it gets there; while in this other case the pressures come just because of thermal expansion which is present right from the beginning.

That is better illustrated I think here on this slide where you saw for a single phase the slide shows mass expelled from a single half by cross-plotting results like this. It gives the energy absorbed, how much energy will you put to the central half in order to produce so much mass to come out.

And what you see here for a single phase you only need about 5 megajoules per subassembly, while for a two-phase you need almost an order of magnitude higher, almost 25 megajoules. That means a two-phase system will absorb more and more energy before it can be self-heating enough or self-pressurizing enough to shut down.

So basically, then, another interesting thing we did here, because for a moment we suspected that we might be getting such delays in the two-phase disassembly because our two-phase modeling in the calculation was such that allowing vapor to slip through and not carry with it fuel, and of course the liquid fuel removal is the one that carries the reactivity void.

So we did two calculations: one in which we allowed the nominal slip we are allowing in the core; and in the other one we made the slip essentially zero. We made them behave homogeneously, and we got exactly the same result. So basically the net result here is that this is as fundamental as F = MA, and we don't really have any great doubts about this behavior here. As long as you can calculate correctly the mass displacement because of forces -- and I think that that is again very fundamental -- the next missing part of the link -- not quite missing but the next thing you need to know is how much reactivity change will follow a given displacement of mass. And, again, neutronically we have very high confidence that we can do it quite well, and in fact, symmetry is the state-of-the-art tool in doing the job.

The bottom line then is probably those are as good a disassembly calculation as you can get today.

MR. CARBON: I guess I'm still left with questions. It may be as good as you could get.

MR. THEOFANOUS: And sufficiently good.

MR. CARBON: I would still like to ask, going back to Chart 4, could you without using SIMMER come up with some numbers that would be in the same ballpark?

MR. THEOFANOUS: Yes, we could come up with numbers, for example, using other codes, using VENUS or other disassembly codes.

MR. CARBON: Totally independent, totally separate?

MR. THEOFANOUS: Totally independent, yes. Those were done in the early days of developing SIMMER. As I remember, they were done independently of the people who developed SIMMER, because I remember in those days I was involved with the NRC staff in reviewing the

FFTV and the early CRBR, and they were doing independent assessments and calculations, and it was pretty good.

Now, you can't really do that, Max, back of the envelope, because if you could we would have done it already. You probably can do it if you were patient enough, plot some numbers for the single phase, but it's more difficult for the two-phase to do that by hand.

MR. CARBON: I appreciate that back of the envelope is misleading, but could a person sit down and in a week or so --

MR. THEOFANOUS: Yes. I feel if you sit down and you are willing to punch a few numbers in a computer, you could do it yourself.

MR. CARBON: And come out with good numbers?

MR. THEOFANOUS: Now we need to relate this pressure element to the potential for doing damage to the vessel. And here illustrates dramatically the vessel configuration. And this is the box I was referring to before. This is like a cage. The core support structure is a very sturdy one. Everyone tells us, all the structural experts tell us, that is the last thing that will fail.

Above the core, that is illustrated here by this red mark, is a whole heavy, big structure referred to as the upper internal structure. I am going to abbreviate that by "UIS." This is supported by four big steel cones that come from the head. Surrounding the core, as you remember, was the core, the drivers, the blankets, and then we had three rings of reflectors, which are basically subassemblies essentially filled with stainless steel.

So immediately after that, we have the core barrel, and this core barrel is 2 inches thick steel, and any high pressure developed in this small region you see here will have to push out of the way either the upper internal structure or the core barrel or both before it can do any damage to any other part of the system.

Now, I don't want you to have the impression this is a completely closed case, that things will stay there forever, even if there were high pressure. This is a leaky cage. But the point is there is enough pulse up here that it is sufficent for throttling high pressure.

So any volumetric flow coming out of here under high pressure will be throttled, and it's not happening over a tenth of a millisecond, it will be happening over hundredths of milliseconds, therefore, quenching the expansion, not being able to accelerate the slack to do work on the head.

So the only way you can get a real focusing of this thermal energy to the head is by violating catastrophically one or both of those strucutures.

Now, the project has estimated in one of our questions, Bill, of the eight questions, what it will take to push it up. At that time we were not smart enough to ask about the core barrel, we only asked for this. And they figured out 100 bar, and our initial evaluation in facts suggests this is reasonable. But then we started looking into the pressure traces coming out of the disassembly calculations, and they are of this type. And this is approximate here just to illustrate the effect.

There are $100-per-second ramp. The core barrel is in immediate proximity to the high-pressure region, so it's going to see a pressure that is more representative of the peak pressure developed in the subassembly, which as you know is short-lived. It is a highly dynamic one. It goes up and goes down.

We illustrate here a range of behavior because what will happen in the longer term is a function of the heat transfer between the fuel and the steel that might be involved. On the other hand, before the UIS can become engaged in this process, it takes some time, something on the order of a few tenths of milliseconds. By the time it becomes engaged, in fact, the pressures driving the whole thing have been reduced by quite a bit.

Now, this goes about that because although it takes into account the impact of these two things coming in and hitting it, again we will experience a transient and it will come back down again. So this UIS will experience something more typical of the quasi-static pressure while the core barrel will experience something more representative of the peak pressures.

Because of this behavior, we thought that -- this again Dr. Okrent, another terminology -- we searched and discovered we should worry about the core barrel. We have an analysis done on the core barrel because of the highly dynamic nature of this pressure.

We don't think it's fair to put this pressure here statically on the top of there. It certainly wouldn't be able to take it. But there is a lot of inertia massed between the blanket actually the core as illustrated here.

This is all very heavy steel, and these three or four rows of subassemblies are all filled. So as soon as the core barrel begins to yield, I guess the pressure will drop unless all of this mass can keep up with it. So we feel there is significant inertia effects there.

We are evaluating also, we take into account the stiffening effect because of braces here, because of the vessel, and because even of the sodium between. So currently, we're not sure exactly how this will end. But we suspect maybe because of this (indicating), this might fail first.

And if this were to happen, the relief would be over on one side. It would be like a bubble growing under this liquid sodium pool, and it would be growing so rapidly because of the catastrophic failure of this that it would be able to accelerate the slag to go hit the head and produce energy.

We don't think that this kind of isometric behavior is going to be anything detrimental. In fact, we feel because of two-dimensional effects, we might even get a little less energy conversion than one gets from the classical situation where one allows this expansion to go directly into the pool and start accelerating sodium in pretty much ideal fashion.

From the point of view then of doing work to the head -- and that is the real concern here because that is where the containment is and that is how one gets concern about fires and what have you -- it is important that one is concerned with the integrity of these bags here.

And if those bags were to fail catastrophically, if one wanted to do a calculation that is almost back of the envelope, it would be one in which one quickly removes all of the obstalces out of the way, and one can do adiabatic or asymptotic calculation as the process is done to find out how much energy can be released.

And the way this is done is shown in the next slide. Basically, you take the pressure, and that would be standing out from the quasi-static pressure and you span it out as improbably against volume up to the volume of the covered gas origin. And that is 21 cubic meters. And you take the integral under this curve, and that will be an upper limit of the kinetic energy that you can expect. And I really mean the true upper limit because there are a lot of other mitigating factors between that the slag would have as it goes and hits on the head.

Therefore, remembering back the importance of the bags and the estimates of pressures and energies it will take to fill it, we feel the level of energetics is zero up to $100 per second. By that zero, we mean there is no significant acceleration of the sodium slag to really do work on the head.

However, at some place around here, around this neighborhood, the pressures developed will be sufficient to violate the integrity of these bags in a catastrophic way. And if this were to happen, if we did this process here, we end up with numbers that will be very close to the structural margin. And furthermore, we will state here that as you go beyond that point, the slope is pretty steep. And furthermore, we will state the uncertainty is pretty high because you are going out to very high ramps and you have a lot of other different questions.

Therefore, the real margin, however, is not from here to there or from here to there. Obviously, there would be some kind of a trajectory going from here to there. The real margin is from this level to this level, and we obtained this margin by claiming that for the cases of interest we were going to be concerned with reactivity ramp rates well below this Category 1.

At this point then, the conclusion is that we are looking for events of this order as the kind of event that would be of concern to the failure of the primary system.

And with that, I think we complete Unit A. And if you have any questions?

MR. CARBON: This would be a good point to stop, I think, but I do have a question. On page 13 it talks about essentially -- I think it is saying -- there cannot be a fuel-coolant interaction in effect there because the physical situation is such that it won't take place.

The thing I wonder about is could the vapor bubble in its expansion be disrupting the fuel and the surrounding subassembly such that you truly could get breakup of the fuel in the surrounding assemblies which would lead to some sort of heat transfer from the fuel particles to the sodium, which would enhance this?

MR. THEOFANOUS: That would be within the core region. That would be only possible if you enter in an energetic situation with the core still in the core -- I am sorry, excuse me -- with sodium still in the core.

If you have sodium in the core as you enter a burst situation, then very rapidly fuel will be molten while the sodium has not had a chance yet to see the high temperatures and powers and therefore will be in almost a pre-mix situation. Is that what you are referring to?

MR. CARBON: Perhaps. Or in any way, could the vapor bubble in the center of the core in undergoing its expansion cause the fuel in the exterior to be broken up such that it would somehow or another come into contact with the sodium either in the core or outside the core?

MR. THEOFANOUS: I think we have to better put this in the picture. If there is no sodium in this general area and in many cases, for example, under recriticality conditions, which is the major pathway through which one can get some energetics, we believe, of course you are not concerned with getting fuel and sodium mixed.

The first contact of fuel with the sodium will happen up here in the pool. That is where the fuel bubble expands and comes into contact with the sodium.

MR. CARBON: Could the fuel bubble be preceded by unvaporized fuel, is what I am saying, with the unvaporized fuel coming in contact with the sodium?

MR. THEOFANOUS: Well, in fact, the material that will be coming out in an expansion of this type will be a very high quality but still containing some liquid with it, fuel material. It will not be pure vapor, it will also be liquid involved with it. But it will be high-quality material. That means a high percent of this volume will be occupied by vapor and a small percent will be occupied by delivery. Therefore, if there were any potential for interaction, steel is there because the liquid could be moving.

One can postulate the liquid moves faster, makes it through the bubble, and goes and hits the sodium. However, we don't believe we are concerned with getting augmentation because of this process, because the two materials, fuel and sodium, are initially separated. They are coming in contact in the manner which does not promote mixing. And even if some mixing were to take place -- we believe mixing does not take place during those conditions -- but even if mixing were to take place, that mixing would involve small quantities of fuel and sodium before the two interact and push themselves apart again.

And in fact, we had some experiments just recently -- we have not published them yet -- in which we are blowing two-phase saturated water into a freon level under very high pressure conditions. It's very interesting to see if you did this experiment blowing two-phase high-pressure water into water, you find you are very far from isentropic expansion, of course, because you have a lot of condensation going on.

What you do when you do it with freon is you observe a smoother interface indicating a loss of mixing. And I think the reason for that is there is a natural repulsion between the hot material and the volatile material, and it's indicated that this behavior is very close to adiabatic behavior.

That's why I think when we have this kind of expansions, I believe the isentropic evaluations might not be too far from reality. I thought your original question was with some sodium staying in here. Now, it gets a little more tricky because if that were the case, you already have a pre-mix situation here, fuel and sodium within a subassembly, and that could be a low-power subassembly. And you will see some maps later on that show you this picture. You do not have an opportunity yet for the sodium to void out.

Now, suppose another part of the core is undergoing a super pump burst. Now, all of this fuel that was nicely distributed there becomes molten and, of course, naturally possible to mix with the surrounding sodium. In that case, whether you have an augmentation or not, I think it is possible to have some augmentation. However, I think we need to wait until we go into the next section to see under what conditions we can develop this and how much sodium can be around under those conditions.

Typically, most of the sodium is out. Only a very few subassemblies will have sodium in them. And even there, if something were to happen, we are more concerned with the LOF-driven TOP to the potential in this situation rather than the energy conversion potential.

(Brief recess.)

MR. THEOFANOUS: I think I would like to occasionally put this slide on the board to show you where we are in this overall picture.

We have discussed this part now (Indicating), the technical basis for making these kinds of judgments, and now we want to address this part here (Indicating), looking at what happens in the initiating phase, the initial disruption, and how we can get into energetic behavior using the initial status of disruption.

And this is really centered around this problem of the plenum fission gas compaction. If this was not the case, we would not have any problems about stating that we don't expect the initiating phase energetics or energetics during the initial stages of the CRBR core period.

Now, this becomes a problem because if the core is irradiated and a fission gas accumulation in the plenum, these pressures can get as high as 30 bars near the end of life. Initially, the pin is supported at both ends. It is really free on the top, but one can say it is supported in the sense that if the cladding is to be cut off here, it would not be allowed to be ejected upwards because the subassembly exist moves down and does not allow it to move upward. So this pressure is balanced by the axial integrity of the pin. When the pin disrupts suddenly, you have an imbalance.

Typically, the disruption of the pin will take place somewhere below the top of the core. That means there will be some fuel, shown here in blue, in the blanket that will be experiencing this downward force because of the plenum gas; and you will have then this physical ejection almost like a gun barrel geometry that would introduce reactivity because the fuel will be moving from a lower worth position to a higher worth.

Yes?

MR. CARBON: I need to inquire in here. Your words are you have a high pressure in the upper plenum there, and that is putting the fuel column in compaction. But isn't the gas pressure pretty much uniform throughout the column?

MR. THEOFANOUS: That is true. It would be uniform under static conditions. If you go suddenly and disrupt the cladding in the fuel here, what you have in the neighborhood of the disruption is the fission gases that are evolving from the intergranular spaces is a finite, small quantity. As quickly as this pressure is released, the only way you can balance this pressure release is by having a very high flow of this gas out. But this is restricted by the very small gap between the pellets and the cladding.

These are the mechanisms. What other key parameters affect the behavior? I think the potential for autocatalysis here is quite obvious, and that is the primary reason we brought it up to start. But one can easily envision that if one pin were in this compaction process, because of the inherence of the core there would be another pin close to it in terms of disruption characteristics, that it will be disrupting, and it will be compacting. This will increase the power, further accelerating more and more pins to undergo this kind of a process. So the natural question is can this become autocatalytic?

By the way, we are concerned about autocatalytic behavior, and that is the first level we are looking at things.

MR. CARBON: The first level at what?

MR. THEOFANOUS: The first level at which we are looking at this energetics question. First, we are looking at the level of autocatalysis, and the reason we are concerned is it is very difficult to bound autocatalytic behavior.

In this case, for example, fortunately we are able to demonstrate that we are not close to autocatalytic behavior; and I think the purpose of this section of the discussion, Section A, is to look into this aspect of the problem.

The key parameters that affect this process first of all, of course, is the stored plenum pressure. If you have a beginning of life fuel, you have no pressure there, so the core pressure becomes mute, and throughout the life then the pressures will build up there. So you must have that.

Furthermore, you must have good timing between the clad failure and the fuel melting or fuel heating disruption properties. And what I mean by that, the timing should be short enough so that the gas does not blow up. If there was enough separation in time between the clad failure -- and I mean clad failure, not melting, but because of internal pressures and heating and fuel disruption -- there will be enough time for this gas to come out by the time the fuel is disrupted and the fuel column became imbalanced.

Typically, the time constant for blowing down this gas is from a quarter of a second to maybe one second, and depending on how pessimistic you want to be, maybe slightly more than that. So you are talking about a very short time it takes to vent this gas, and that's why this problem is not really relevant to a core like FFTF that is very, very slow.

However, if a core because it has a good sodium volume reactivity undergoes an acceleration because of the increasing power, you can shorten this time scale between these two processes enough that at the time the fuel is disrupting there is significant pressure up here.

In turn, then, what effects -- I put more clearly here what effect is typing, is the sodium worth and the voiding rates, is the clad failures and the location rates and the initial trends of the fuel motion upon disruption.

The reason this is important for the CRBR is because the first fuel to be disrupted -- in fact, this occurs at relatively low powers. And in the first group of subassemblies to be disrupted there already is enough timing from the moment their cladding failed until voiding that for them the gas is not there; the gas has blown off.

Now, if that initial disruption of the fuel is going to end reactivity, that will increase the power, and that will bring closer all of the remaining subassemblies which either have just failed or are about to fail very soon.

If, on the other hand, this initial fuel disruption is highly negative, that means subtracting reactivity. That will, on the other hand, buy a lot of time so the remaining subassemblies can continue to void and fail clad and blowing the gases out before the fuel in them has an opportunity to dissolve. That is quite crucial.

To the extent that pressures up here mean irradiated fuel, we know this automatically implies that this fuel here has to have some intergranular gas.

And finally, of course, one must have known friction between the pellet of the cladding if this column was to be accelerated downward. Some people believe it is virtually impossible no matter what the pressure behind those pellets, it's virtually impossible to shoot a bunch of pellets through such a small clearing.

We did not feel we had enough justification to exclude that. That is the reason we go into this whole story. In fact, some early interactions with the project indicated to us that maybe some of the fission products might be even vaporized and go back there into these regions here where they recondense, but because of their melting properties and so on might even provide a lubricating layer for this stuff to go out.

In any case, we think we are approaching that conservatively in the sense that for bounding this problem we are assuming there is no interaction between the cladding and these pellets; that they are free to accelerate. However, not completely free. They have to obey the basic laws of nature, and that is inertia. So we put all of the force on those pellets and let them accelerate based on their free inertia without any negating forces.

MR. KASTENBERG: Theo?

MR. THEOFANOUS: Yes.

MR. KASTENBERG: Is the channel pressurized at this point?

MR. THEOFANOUS: The channel here is not pressurized, no. Whatever pressure can come out from these fission gases. But this is short-lived. As long as it goes through, there is no remaining pressure to oppose this compaction.

Now, let us take a look and see how the pressures build up with time in this plenum. We are showing here the plenum pressure versus burnup in full power days, and you see that we have a gradual and steady monotonic increase. Near the end, typical pressures are 30 bars, and because of heating in the early phases of this loss of flow accident, in fact these pressures develop up to 40 bars.

What you see from here is the significant fraction of the lifetime of the core is relevant to this question; I would say maybe the second half. Similarly, and because it is also important, it is quite close to this, we have the variation of the gas pressure in the fuel now in grams of fission gas, grams of fuel times $10^4$. And you see this gas building clearly.

And what you see here is that very quickly within maybe something like 50 days, from then on the fuel can be categorized as pretty gassy. This is very important because a gassy fuel tends to be dispersive and disruption. When the fuel is very fresh it has no motor forces inside it, so upon melting basically it slumps under gravity. We know that is very important on the timing of subsequent events.

So it's very crucial here to remember for consistency we are going to let the fuel plena have pressures, but at the same time we are going to allow the fuel itself to have fission gases in the structure itself. So the initial tendency for the fuel will be to be disruptive and dispersive instead of being compacted.

Also, I want to say something about what is our basis for this. We have experiments, a number of experiments with prototypic materials that in fact indicate that when we have irradiated fuel at some reasonable powers, you will get in general disruptive behavior.

Now, there are probably a lot of people, maybe even including myself, who will say that we do not know everything there is to know about the rates and timing of fuel dispersal because of fission gases. That was and in fact I think still is or can be a serious problem if one is looking at fuel dispersal to mitigate a process, for example, for the LF-driven TOP. In that case the timing is very crucial, the timing and the extent. And I don't think we know enough to be able to make this kind of judgment.

Here what is important to know is the general trend, whether we will get compaction or some kind of dispersal. And I think we know enough based on experimental analysis and total knowledge to allow us to make a pretty reasonable judgment as far as that.

MR. CARBON: Is there considerable uncertainty in that, more than you are perhaps indicating?

MR. THEOFANOUS: I think the uncertainty of that, Max, has to do again with how precise you want your answer. If you are asking me do you know the time of fuel disruptions within a few milliseconds, I think I would tell you no, I don't know it, because in fact we don't have that.

Now, maybe there are some people who disagree with me, but you asked me will the fuel disrupt at some point as it is being heated, I would say sure, I know one hundred percent it will disrupt. If you ask me is the fuel upon disruption going to collapse under gravity, or will it just stay there or disperse, I will tell you yes, with a very high degree of confidence I can tell you that the fuel in general will be dispersive. If you ask me do you know it's going to be moving with a velocity of 100 centimeters per second upon dispersal upwards and downwards, the moving activity, plus or minus 20 centimeters per second, I will tell you no, I don't think I know that. But if you ask me in general will it be a general upward direction or general downward direction, I will say yes. I think you will find very few people disagreeing with that, and if they do, they will have a hard time justifying it.

MR. OKRENT: I am trying to understand the dispersal picture that you have for the fuel. Could you indicate a little bit better for me what you think the fission gas is doing and what you think the fuel is doing and what the state of this fuel is, is it solid or molten, and how this changes as it moves from the fuel into the channel or what was the channel and so forth.

MR. THEOFANOUS: Right. In some cases you might not even have a channel because we know as the fuel heats up it swells. At least in a number of cases there is experimental data that shows as the fuel heats up at the beginning of melting, maybe 10 percent radio mill fraction, it begins to swell. It swells because of the internal pressures, because of the gases. And by the time it is ready to disrupt, which is typically 50 percent radio mill fraction, it is essentially hydrocooling; so there is nothing much flowing through them.

Now, beyond that point at some point the fuel will disrupt, and the pellets will actually disintegrate. This point will be sometime before all of it is molten. We think 50 percent is a good middle range value, and we are not very sensitive to the number we choose for that.

Upon disruption there will be further gases being released, and again, depending upon the power level, you will have multi-forces. There will be pressures inside that disruptive zone that have liquids, solids, carbons and gases; so it is like a frothing region that is some place in the middle of the core that now experiences these local forces which are high, but high with very low driving potential because the amount of gas there is not a hell of a lot.

The result of that is just like the zone experiences, a pulse in both axial directions. Fuel then is being thrown up and down in this frothy region. However, we believe that it won't go very far in that early stage of disruption, because as the fuel above and below it is swelling, maybe the passages are so small that this intends to go up, but maybe after the gas has dissipated itself, maybe start again, coming back together.

So that is the picture we have with respect to this phenomena, and we think because of this, the nature of this, especially what I have to say about this slide, we are not very sensitive to that.

MR. OKRENT: I guess listening to you it's not completely clear to me -- at least I have a picture of what the passages are through which this disruptive fuel is moving, and just how I know when the gas gets out and when it is in and just what it is that is doing the motion, and how in fact I can be sure that I know even in what direction the motion is since I don't really know what the passages are.

MR. THEOFANOUS: Well, I think that many of us share your general uncertainty of knowing exactly what is happening there. But what I was trying to indicate before is you have a region that is disrupted. It has liquids, solids and gases, high pressure gases.

MR. OKRENT: I agree it is disruptive.

MR. THEOFANOUS: It goes without saying.

MR. OKRENT: In the sense that you no longer have your original pellet.

MR. THEOFANOUS: Right, right. It is a mixture, a frothy mixture of liquid, solid and gas. That is high pressure. That goes without saying. Because you have gases there, they have to exhibit themselves. They come out.

What can happen? One thing that can happen, you might ask me which way can this fuel go. One way would be upon melting and disruption somehow magically all the gases disappear from there and the thing comes down under gravity. I will claim that this is a highly impossible situation, because we all know that first of all you at least have to get the gas out. If you are not allowing for any dispersal behavior from the gas, at least you have to get the gas out before the fuels can come back together.

MR. OKRENT: But the gas might be able to move through past the fuel, can't move through.

MR. THEOFANOUS: Of course. That is what I am saying. Eventually what will happen is the gas will be dissipated. I said that already. Eventually the gas will be dissipated, and the fuel will then come back down, and we are taking that into account in our analysis.

MR. OKRENT: I thought you indicated it was clearly dispersive.

MR. THEOFANOUS: But the timing here is very important. It will be clearly dispersive to start with for some duration of time, but then we trended the gas, and we have ways of doing that, and we let the fuel come back. So it is neither permanently nor monotonically dispersive.

We think the initial tendency of that will tend to be dispersive, but the finite amount of gas and this behavior, as well as I mentioned before what we expect to be occluded channels above and before. No matter what pressure you have, you will have a hard time getting the fuel out. We don't disagree.

MR. OKRENT: Let me put it this way. Using your probabilistic terms at the moment, I put this in the category of one-half.

MR. THEOFANOUS: That's a good judgment, and I will say we are not sensitive to that.

MR. OKRENT: If that's the case, you are better off.

MR. THEOFANOUS: That is the point we are making here. If we had to make the case for LF-driven TOP, as I indicated before, we would have been in a different predicament than the case we would have to do here. That is exactly the purpose of this figure over here.

To illustrate how we go about looking at this continuum, we have lots of variables here, and it would be forever to do a very detailed, comprehensive, parametric, statistical evaluation to obtain true statistical distributions of probabilities for plenum fission gas compaction and severity. But a few very fundamental ideas here can be useful to clarify what we are looking for.

First of all, if we -- of course, what is really important here is the reactivity feedbacks. And the question one is confronted with as one is trying to do this kind of analysis is what should I use. Should I use higher ranges of uncertainties or lower ranges of my uncertainties?

Let's see what happens here if you use the high and the low. If we were to bias all of the reactivity feedbacks in a downward direction, that means takes the sodium worth all of the way down to nominal value minus 2 sigma Doppler, everything gives them as slow as we possibly could justify, we would have such a slow initial behavior of the accident that it will provide enough time between the clad failure and the fuel disruption that this whole question of plenum fission gas reaction would be moot in the same way it is moot for FFTV. That means in this level of positive reactivity feedbacks, the severity is essentially zero.

Now, you might have expected that if we push everything to the higher limit, we might make things bad. In fact, intuitively that was behind my mind when I was thinking about this problem a few years ago. Well, it doesn't work that way.

As you make the reactivity feedbacks to be high, they show a more nominal plus 2 sigma uncertainty. Take Doppler, much more than what you expect it to be, everything to be in the positive side. You would come so close to pump critical that by the time you are ready to move the fuel, you don't have enough time to accelerate the pellet downwards and give it a good ramp at the time it comes. This is the time that it gives the energy yield.

So as a result of that, the severity again goes down, and there is no monotonic increase of the feedback; and I think that is very important. So there is some range in between, a pretty broad range, and that is why I say we are very sensitive to that.

That gives us this maximum in consignals. And the way to look at this problem is to try to put boundaries rather than coming out with a very detailed granting numbers, a lot of calculations and probabilistic assessments of all of those parameters.

MR. AXTMANN: What are the units of severity?

MR. THEOFANOUS: That would be, for example, pressure ramp rates, to start with -- that would be a unit -- and then the ramp rate could be converted through a previous slides in Section A into pressures. And that could be converted into filling backs, and after you fill back, that can be converted into megajoules of kinetic energy of the slide.

So really, the starting point of the unit of severity is ramp rate in a superprompt discussion. That is what we are looking for. If we have that, we can make all of the other steps.

MR. CARBON: Would you summarize once again why you have confidence that you are on the ends of the curve rather than in the center?

MR. THEOFANOUS: I will say we are going to be trying to stay at the center of this curve.

MR. CARBON: You're trying to what?

MR. THEOFANOUS: To assess and put a severity number to this broad maximum. We are looking for this bound that tries to put everything below it.

MR. CARBON: Okay.

MR. THEOFANOUS: And we have done a very large number of sensitivity studies, and of course, we obtained some additional insights from the sensitivity studies that the project has done. And with those insights we think that we can define the bounds. And this is shown here. This is the case where we use the sodium worth of about $1.7, which is maybe a little bit more than what the project man considers nominal. I think it is 20 percent more. We use a 50 percent axial expansion, and we use a fuel that is generally disruptive but is not extremely, exaggeratedly disruptive. And the power transient is shown here, and it goes up to several thousand times nominal. And the reactivity history is shown here.

And this bar period, the slope of this line, is on the order of $50 per second. And we think that it is a bound, an upper bound.

MR. OKRENT: How much would it be if you had zero contribution reactivity from what you called the disruptive behavior of the fuel in the middle?

MR. THEOFANOUS: If we made basically the fuel to stay motionless, right?

MR. OKRENT: Effectively motionless.

MR. THEOFANOUS: Effectively motionless. If that were the case, it would be a number below that. I can't tell you exactly what. We have done a lot of calculations, and I don't have in my head now all of the results. But what you do with the fuel, it affects how much gas also blows down.

Probably I have a feeling that if you did just that, you would not be too far maybe from that because you are in that general range of broad maximum, but you will not be more than that.

MR. OKRENT: Well, I am trying to understand. Your first answer was it would be less.

MR. THEOFANOUS: Yes, but in that general broad umbrella of maximum.

MR. OKRENT: And I guess when you say less, you mean the energy developed in the burst would be less.

MR. THEOFANOUS: Right. The dollars per second.

MR. OKRENT: And why was your answer that it would be less if instead of being dispersive it was neutral?

MR. THEOFANOUS: It's a number of things that play a role here. I don't have them all at my fingertips here, but we have done a lot of calculations around that general area. In fact, we've done calculations in which we let the fuel compact also, and that brings us over to the other side.

What we intend to do in the report, in order to give you this perspective we are going to give a range of assumptions. And by looking one against the other, it would be easier to understand how this can be really a reasonable limiting value. But it is a total integration between how much time is allowed between the clad failure, and therefore the beginning of the blowdown, and fuel disruption. And that is a function of the power, and the power, of course, is affected very strongly by the fuel motion, and also by what is the reactivity level at the time at which the pellets begin to accelerate.

As I said before, if you leave the fuel motionless, you might have been closer to being prompt, so that the time allowed for the pellets to be compacted would be less. Therefore, you would be going through this with a lower ramp.

MR. OKRENT: I think that could be a reason, but it leads me to a related question. What do you get as the largest reactivity insertion rate from the plenum pressure pushing fuel toward the middle if it's not terminated by a burst and you are not setting just below prompt critical when it occurs?

MR. THEOFANOUS: I think that I can answer better if I show you the next vu-graph, because exactly

I was intending to cover that. I knew it would be a burning question in your minds.

MR. AXTMANN: What do you assume for the thermal conductivity of the pellets during --

MR. THEOFANOUS: The thermal conductivity of the pellet?

MR. AXTMANN: Yes. Which is if the effective thermoconductivity of the pellet -- do you use the bulk conductivity of the fuel?

MR. THEOFANOUS: Yes, yes.

MR. AXTMANN: But it's pellets.

MR. THEOFANOUS: Yes.

MR. AXTMANN: And do you think in these timeframes the effective thermoconductivity will be the same?

MR. THEOFANOUS: I think so. I think if there are any questions there, the more important question is what do you use for the gap conductants.

MR. AXTMANN: Right.

MR. THEOFANOUS: I don't know if that's what you're referring to, but that can affect the time of clad failure. And we try again to expand that as well as we can within reason. But we have more of a question there.

MR. AXTMANN: That's what I meant by effective thermo --

MR. THEOFANOUS: Okay. In order to understand the answer that Dr. Okrent asked, we need to take a look at what are the material patterns as we approach the burst. And here we are showing for this case for which I showed the power and reactivity just before the prompt burst, here is the sodium void pattern. What you see is essentially 60 percent of the core is voided, and of course the reason is because of this voiding we brought up the activity, and we have been able to shorten the time so that we have pressures now, and these pressures are indicated at the top here. Those are the pressures in the plenum.

And I should say something about these plots here. These numbers below each one of those plots represent SAS channels. The width of those is proportional to the number of subassemblies grouped in that channel. So roughly then this channel is a big one representing something on the order of 10 to 12 percent of the core, 15 percent maybe. While this tunnel 6 is the one that has the higher power fuel in it, it is very small and represents something like 6 percent.

MR. OKRENT: Where is the inner blanket?

MR. THEOFANOUS: I am not showing the blanket here because typically for this kind of a problem, for example, our primary concern is the state of the driver fuel. So to make things fitting in one page -- this was three times as big before -- I only show the driver fuel.

So what you see here is by looking at this -- this is linear scale -- by looking at that up to 11 is 60 percent of the core roughly; that is voided. The next two channels are just beginning well on their way to voiding, while very little voiding happened in channels 14 and 15.

The pressures are shown on the top here, for example, and what you see is channel 6 is the first one to undergo voiding. Therefore, that is the first one to undergo pin failure and all of the subsequent events. And you sea because of that, because the power was lowest early on, it bought enough time that the pressure is only 3 bars at this point; so this channel is of no consequence whatsoever as far as the compaction.

However, you see the next channel that comes in, channel two, still has a pressure rate bar, and there are 40 bars, around 40 bars in channels 7, 8, 7, 9, and 10. Now remember, at 50 percent radial melt fraction, and that is shown over here, we allow the fuel to disrupt. So at this point, then, channel 2 and channel 7 is disrupting. Channel 4 is very, very close to being disrupted. And upon disruption those tunnels will be the ones that will be compacting, and it will give us a burst.

What is important here is, all of the other channels because of core-wide incoherency have significantly lower melt fractions. It takes some time to bring this melt fraction from here over to there, and by that time the burst is essentially over. However, at the end of the burst, I think the point here is we cannot arbitrarily take all of these channels here, all of the core, and let it compact independently of what the melt fraction of the fuel happens to be.

There are natural incoherencies that have to be acknowledged. You have to pick a number for fuel disruption, and you can pick any one you like. But after that you have to make all of the fuel disruptions at the same melt fraction, and always you will see an incoherence in entering this prompt burst behavior.

Therefore, the driving reactivity will be taking place by a small fraction of the whole core.

I think it is quite clear if you let all of the core do this you will reach an intolerable situation. That is for sure.

MR. MARK: Theo, in the large, what you have been telling us, you have not said to what extent what you have been saying here would differ if we were discussing the old original core design, homogeneous core. In what degree and in what ways does the core design affect what it is proper to say here?

MR. THEOFANOUS: I think it would be different, the homogeneous core would be different, in a very significant way, and that is because of the higher reactivity worth of this core, because of the absence of internal blankets. The accident escalates in power much quicker. It does not take, for example -- you could not have that situation. You would have only one or two channels, maybe, there to voiding, and already you would be in a high power condition, maybe already failing fuel, and already therefore initiating compaction.

I don't know if that would have been better or worse from the point of view of total energetics resulting from this compaction problem, because again we could be so close to prompt critical that it didn't allow enough time for acceleration of the pellets to go in.

However, I know for sure it would have been worse off from the point of view of having a picture like this here. This is a picture showing the molten fuel as a function of the tunnel, and you see essentially the whole core is molten. So the difference here is, in a homogeneous core you will be entering that condition post-burst with a lot of sodium in the core. You will be back to the question asked before. You have already a lot of interspersed fuel and sodium, and we are really a little shaky there as far as what happens.

So from that point of view, personally I feel, and I think the rest of the team feels, much more comfortable that we are achieving this kind of condition here with only a very minute amount of the core, denoted here by stars, which still, as you see here, is unvoided, therefore is experiencing this condition of high power with strong cladding, with sodium around, and with molten fuel inside the cladding.

This is a typical condition that we call LOF-driven TOP. And to respond at least in part to an earlier question Dr. Okrent asked about any remaining uncertainness, this is an area where we are working a little more, because although it isn't marginally the situation, because it is in typical LCF-driven TOP, we like to obtain some more confidence that that will not yield additional difficulties.

In fact, I understand yesterday in the ANS meeting there was a W-2 test addressing the failure of the fuel pins under these conditions that was discussed at ANS. We did not have an opportunity to go there, but we are looking very anxiously for an opportunity to have time to look at that and evaluate the information.

I think there are two questions there: One, number one, what is the likelihood to have pin failures in the centerline? And those are the cases where you worry about fuel coming, moving inside the pin, and getting out in the channel. And the second one is: If this were to happen, what would be the consequences? There are two questions. And from that point of view, whether this core is much better than the other one, to that extent we don't know very well those things.

The other thing I want to point out here is, these numbers here indicating fuel vapor pressure within those subassemblies that have been -- again, the post-burst period. And if you mix up with this fuel steel, which is over here (Indicating) -- this is in the pre-burst condition. You see only a small amount of steel has melted. The core exits and both axial ends are open, and because of all of this fuel here you have a lot of molten steel intermixed with the molten fuel.

So these pressures you see here will be augmented by roughly one order of magnitude to take into account the presence of steel in the vapor pressures. So you see, this point is -- you have reversed the pressure gradient to take your 140, 130, 80, 70 -- the smallest would be 50 -- while on the top it is pushing this number. So one way of looking at that is that this number times ten pushes upwards, while these numbers (Indicating) push downwards.

So there's no question about additional compaction following that on the remaining subassemblies. So clearly, then, we have seen -- we see a way out of this, a way to termination, because of all of the openings here and because of all of the high internal pressures in the core because of this fuel vapor pressure and steel vapor pressure.

David?

MR. OKRENT: From your analyses, is it clear that 11 cents a second is worse than 10 cents a second reactivity insertion ratio, and that 10 cents is worse than 10 and so forth?

MR. THEOFANOUS: I think if you put it into cents my answer is no, but that is a separate question, that refers back to initiating phase TOP in posing a ramp and then looking at what potentially can happen, and it is not necessarily monotonic, the consequence. But from the point of view of energetics, from the point of view of how much mechanical energy can do to the system, as I said before you can only develop that if you have a prompt burst, and you can only develop pressures of that -- of course, the higher the pressure the higher the energy, and from that point of view the higher the ramp the higher the pressure and the higher the energy.

So in that sense, if you go into what we have defined as energetic events, that is more than 30 dollars per second, which develops pressures within a few milliseconds, the trend is monotonic.

MR. OKRENT: I am still interested in the initiating event part, and I am trying to understand for the moment whether anything you have told us is sensitive to the assumption on the -- well, let me word it this way. Suppose one rod, instead of all of the rods staying out in the loss of flow, one rod partly got in. Would that always help?

MR. THEOFANOUS: Could I suggest, Dave, if you have questions outside of what we are covering here, for that purpose we left the last half-hour of the day to discuss other things. Again I will repeat, what I am giving you today is for the loss of flow accident, which is historically the one that gave us the problems.

We are addressing all of the others, and what you are saying here is part of the other initiators. There are I tasks. If you like we can discuss it. We are not finished on that one yet, and part of the question we are addressing is exactly that.

One other one we are addressing is, what if we have a TOP, a classic TOP that is followed immediately after that by a loss of flow because the pumps have to trip also? That is another one that has not been exampled well before and we are examining, but it's not a part of my story here. It is a different story.

MR. OKRENT: Okay.

MR. THEOFANOUS: To summarize Unit B, then, we are pretty comfortable there's no autocatalytic behavior shown. This is because of the incoherency across the core, because of the inertia of the pellets, and because the driving force forces for compaction are finite. If they were infinite, you remember this inversion of the pressure grading wouldn't be present and you would keep compacting. But here we have a finite number of compaction forces. Therefore, we do not enter this behavior.

MR. OKRENT: Excuse me. I'm not sure what you mean by non-autocatalytic behavior. I thought you said you could gain some reactivity from fuel moving inward?

MR. THEOFANOUS: Right.

MR. OKRENT: That usually is interpreted as an autocatalytic effect.

MR. THEOFANOUS: There are many ways to move fuel inward without being autocatalytic. Autocatalytic is if one event leads to the next and allows spreading. This terminology comes from chemical reactions, where you start off an autocatalytic one, there's no end until it's all reacted. If you have a chemical reaction and it goes autocatalytic, you've got to react all of it.

Over here we have shown that because of incoherencies the thing was limited to only two channels. It would have been autocatalytic of the process were such that there was no inertia, for example, the timing between the different subassemblies are causal.

MR. OKRENT: I understand how you are using the word. Thank you.

MR. THEOFANOUS: We feel we can bound it to 50 dollars per second, and in fact we believe because of intra-subassembly incoherencies not all pins within a subassembly will fail at the same time, most likely 50 percent of them. And of that were the case it would be -- more a figure of merit here would be like 35 dollars per second.

We can see the termination of this because of the openings above and below the core. We are re-evaluating the LOF-driven TOP behavior in this marginal amount of the core with sodium in it, and we will reevaluate the consequences. Especially, we will look at this new information that was presented yesterday on W-2.

MR. CARBON: Before you leave that, your bounding reactivity ramp rate, 50 dollars, maybe 35 dollars per second, if you had 100 dollars per second maybe you are on the verge. It is a factor of two or three. But this is all very nebulous. Do you have a lot of confidence in it?

MR. THEOFANOUS: We have a lot of confidence in that. I will say later on when we assign numbers, because we will try to convey from this qualitative meanings, later on to probabilities, to actual numbers. And again, in the absence of giving you a complete distribution, because that is another way of conveying to you our confidence, instead of doing that, because we don't feel it is fair at this point to do that, we will tell you the numbers we are going to give you will be high confidence, 95 percent high confidence numbers.

So we feel we are there with a very high confidence level, because basically, to put it bluntly, we tried to do the best we could to make this number go up and we couldn't. Remember that we brought this problem up, so we had all good reasons to try to make it as severe as we possibly could.

Are there any other questions on Unit B?

(No response.)

All right. On Unit C we are looking at the initiating phase behavior, loss of flow. And this is back in the -- to remind you of the general structure, we are back in here now, and we examined the path that can lead us to here. But we said there are ways by which this will not happen, for example if it were a fresh core. 50 percent of the time the core would not be going through this.

Furthermore, the pellets maybe were too -- really lodged inside the cladding and couldn't move, either freely or not move at all. Or maybe the gas could blow down faster than what we thought because instead of assigning to the gap a time constant size and therefore a time constant for blowdown of the gas of half a second as we have done, maybe we use the value of a quarter of a second like the project is doing.

So there's a number of reasons by which we could have avoided that situation. And then we need to be concerned about, suppose we avoid that and move into here and into here? We would like to know how we enter this further disruption state, so we need to get the general stage, the general framework, in which the fuel now starts melting and coming in.

And in addition, we would like to know what is the potential for developing blockages in the inlet and the exit of the core during that stage, because obviously that has an impact on dispersal. So therefore at this stage we are looking in some more detail. We are going to look in more detail in the initiating disruption phases of the core for the purpose of scoping out a range of behavior, again from the point of view of uncertainties of feedback, so that we know where we are in general concerning power and blockages.

And that is, what I said with many words, it is summarized here. The objectives are to look for fuel removal paths, for driving forces for fuel dispersal, and to scope the entry into recriticality-prone conditions. That is how we defined it.

Disruptive fuel is not going to be monotonically disruptive. It might come down again. We need to know under what conditions we will be doing that. The key process here, we feel, is something we call co-disruption. Co-disruption refers to the simultaneous melting of fuel and cladding in a channel, and by that I mean melting such that it is so close in time it has not given enough time for the cladding to separate out, so fuel and clad coexist in this frothy, solid-liquid, fuel and clad, maybe mostly molten and vaporized.

This is important because it provides us with steel pressures which are typically an order of magnitude greater than fuel pressures. It provides us with an increased penetration potential. In any kind of escape path, if you try to put this kind of material here you go a little bit further. If you put fuel-steel mixture right on steel, you go further.

And finally, even if it were to block or freeze someplace at the exits, you will have blockages that contain fuel, self-heating, therefore can melt those blockages. This process of core disruption is favored by increased sodium worth, making the whole accident faster, and by the plenum fission gas compaction.

If, for example -- and that might be the truth -- maybe 30 percent or 40 percent of those pins that meet the criteria for compaction are lost and the pellets cannot move and the other 60 percent is free to move, then we will not have a strong burst, but it will increase the power, bringing closer the melting and disruption of the fuel to the clad melting. So that the two things mix together, so that is why both of them are important in promoting core disruption.

MR. MARK: You said, and I wasn't quite clear on this, the pressures of the -- I guess it would be -- vaporized steel were an order of magnitude larger than the pressures of fuel. Is that correct?

MR. THEOFANOUS: That is for the range we are looking at. Right after melting and for another maybe 100 bar or so, this is very roughly correct. I give you a rough order of magnitude.

MR. MARK: If they were both vaporized, then that would no longer be true?

MR. THEOFANOUS: That would depend really on the mixture we have, but the vapor pressure is appropriate to the material. So for the same temperature if the two materials were to mix and be the same temperature, you would be getting most of the pressure from the vapor of the steel, which means the fuel would have condensed.

MR. MARK: Okay.

MR. THEOFANOUS: But just to give you an idea, if you have, for example, fuel that is by 300 degrees above its melting point, you would be having not very high vapor pressures, fuel vapor pressures. But if you get steel mixed up with that, you would have very high steel pressures.

MR. OKRENT: Would you again say or say for the first time, as the case may be, just what is the situation when co-disruption occurs and when does it not occur, and what does it take for it to occur?

MR. THEOFANOUS: You are anticipating the next few vugraphs. That is exactly what we want to explain here.

MR. OKRENT: Okay.

MR. THEOFANOUS: However, let me say that sodium voiding is important. That is what aids reactivity. So the extent of co-disruption is a function of the power history, and the power history is a function of the reactivity history, which in turn is really reflecting the material relocation history. So before I give you our actual results showing the extent of co-disruption for different assumptions, I want to show you, if you like, a little bit of our basis for using the voiding that we use and the co-disruption that we used and so on.

MR. OKRENT: You defined a term,

"co-disruption." I want to have a better term in my mind.

MR. THEOFANOUS: I can give you a visual picture of that. That is what we define as co-disruption.

(Laughter.)

One picture is worth a thousand words, they say, and this one is. This is a plot that comes out from a SAS calculation. On this axis here we plot percent volume fractions, and the key is shown over here. So this is all molten fuel. Over here is molten cladding. At the exit, this is the upper axial blanket and lower axial blanket. This is the cladding. Of course, that is still cold, so it's still integral over there. And this is the structure.

Now, co-disruption is a case in which, because of the short timing between clad melting and fuel melting, there is insufficient time for the cladding to move up there and produce a blockage, and most of the cladding is mixed up with the fuel. From that point of view, this graph is misleading because it shows you only the volume fractions.

But the way to really conceptualize this is, you have all of the pins, 270 pins, all the fuel and the cladding in these proportions mixed up. That is what we call co-disruption, again because of the power history, the power being high and not allowing enough time for the cladding to move out at the rate it moves and then down to produce blockages and therefore separate out.

And what we see is, in very, very slow cases -- you can have cases where the power stayed below nominal for the duration of the accident. If you have a lot of time and the initial cladding moves up, blocks up at the top because then the steaming comes down, the rest of the cladding moves down, blocks up the bottom, a nice big plug at the bottom. The top block is maybe only fractional in a cross-sectional area, and then throughout this time the fuel is still solid, and then eventually the fuel melts in a completely segregated system. That is then a typical behavior, this one here, of a high-powered situation where the timing is so short there is no time for separation.

MR. OKRENT: And how short is short?

MR. THEOFANOUS: I think you will understand that after you have an idea of how rapidly the cladding moves, and I will go into that. If the cladding was to move at 50 minutes per second, then short would be a millisecond. If the cladding moves at 50 centimeters only, in our view short is something on the order of half a second or .2 to .3 seconds, around there.

Basically, short is defined by the time it takes for the cladding to move from here to there before the fuel disrupts. That is a function of power, again. So to really better see that, I think we need to go into, what do we use for voiding and what do we use for clad relocation, because both of those processes are important, number one, in dictating the power history through the reactivity consequences, but also in dictating clad failures, clad inception of melting, and of course subsequently the clad relocation process.

So the next few slides will be into those fundamental processes. First we take a look at the voiding process. As you know, SAS is a one-dimensional code. It treats this whole subassembly as the same thing across, no variation in the rate or direction. So the moment it predicts a boiling occurring, this boiling spreads out over the whole cross-section. Of course, because of the increased fiction the process becomes highly instable and quickly leads to flow reversal.

Typically, then, the SAS will give you flow reversal from the moment of boiling inception as something on the order of .6 seconds. That's very short. If the power was higher, this number would be even lower.

For some time people have looked into the problem of inter-subassembly incoherence in this boiling process, because really, if you look at the detail of it, what you expect to happen here, near the top of the core and in the center part of the bundle, that is where the first boiling develops. It is like a boiling zone. And then as the time goes on this boiling zone spreads up, down, and in lateral directions, like illustrated here (Indicating).

Therefore, because of this localized boiling, the liquid sodium can be diverted around and can continue to cool the outside regions, which are already colder, and therefore prolong the time to flow instability and inception of flow reversal, which is crucial, because after flow reversal you go into voiding and clad overheating.

We don't have a lot of experimental information on that, although very likely -- just recently there was a test completed at Argonne and we got hold of an advanced draft report, and this is the 15-pin OPERA test, 15 pins put together in such a way that this small bundle simulates a 67-pin bundle. The purpose of this test was to study how the boiling zone propagates and how boiling takes place all the way out of boiling. So this is an out of pile experiment.

In fact, a few months ago they sent out a report of all of the tech specifications and asked people to pre-predict the test before it was done. We had a calculation that was done some time ago, but was for typical CRBR condition. This was done with an example of a little analysis we did separately from the SAS code.

This treats this boiling zone here as a two-dimensional zone with a very simple homogeneous equilibrium model, which is just the opposite extreme of SAS. SAS uses a perfectly annular -- we use homogeneous equilibrium.

As you can see here, we were very happy with the agreement, especially remembering this was a pretest prediction. It predicted very well the time and the whole trend to slow reversal. What is more, the time that it took for these -- these are calculations, again, from this model here -- the time it took for spreading to the radial walls this was 1.6 seconds according to the test and about 1.6 seconds according to the calculation.

I think we had only time to look at some few things. I think we did a reasonable job in predicting the rate of propagation and both reactions of the void. The important thing to gather from that, not so much the agreement between this and that -- I think that is the trivial part of it -- the important thing is, one code grossly underpredicts the time to boiling inception -- I mean, the time to flow reversal, compared to the actual reality obtained from the test.

That is important because it shows that an LMFBR bundle is highly two-dimensional, maybe three-dimensional. In general, it is far from being one-dimensional.

As a result of that, if you look at collateral disruption, which means melting in location, you would expect that those pins over here that have experienced diminished cooling for a longer period of time than those pins over there, they will melt first. Therefore, when a melt of the cladding takes place you would not expect it to be over the whole subassembly, but to be over a single part of it. And that is important in the rate of relocation of the cladding.

In a way, all of these results only point to the direction that in order to correctly assess collateral disruption you must take into account intra-subassembly incoherencies and effects. That is where I want to go next before -- another thing to show you, because you might have a natural question, is how good are these HEV-2D with respect to SAS.

We have done the HEV-2D model into a

configuration so it becomes one-dimensional and compare it with SAS, and you see very good agreement, although the two models are widely different, just as far as you can be from the point of view of two-phased behavior. The reason is, the whole process from here to here is really governed by heat capacity. So it's more a thermal effect that is important, and of course we know it much better than the details of the two dimensional.

Another interesting thing, incidentally, to point out is this timing. This delay, of course, decreases as the power increases, and what you see here is between 85 percent and 100 percent power. This kind of variation of 50 percent in power is showing much more in a two-dimensional calculation rather than in a one-dimensional calculation.

MR. THEOFANOUS: Taking a look at the collateral location problem with this in mind, we are confronted with a set of results. This is one case in pile results which at first glance are inconsistent. What you see here is the R series tests. Those were three tests, 17 bundles. That gives, going through a typical LF transient, that gave millimeter scale blockages. We have the R8, one part of these series. I single it out because it is the only test in which we have pre-pressurized pins. So there was some gas in the fission gas plenum at high pressure.

In fact, we have normalized our blowdown models against the results of these tests. So it was pre-pressurized and gave no blockage. The P3A, which is an SF, LSF test, 37 pin, gave two centimeters blockage. All of those blockages are above, and the P3 test, 37 pin, gave ten centimeters of blockage. This was a little bit longer. This was run. This was the same test. This was run one or two seconds longer than this one.

In all cases, the blockages were incomplete, not 100 percent of the flow area. So to do something about explaining these discrepancies, first, we take a look at the experimental data that were obtained from again a few years ago. We obtained these data at school, similar material, woods, metal, and air, and looking at the relocation characteristics of this metal as it was subjected to the shear forces of the air going by.

We are showing here how this data can be correlated with the polaminator with a parameter GAG star. It is a dimensional philosophy made dimensional by a buoyancy effect. It is like the square root of Z, the acceleration of gravity, the diameter and the delta overall.

On this band over here, this one shows the portion. This is a fraction in this axis. The fraction of the cladding or in this case of the woods metal that remained in the original position. So you see that as you go from GAG star from one increasing less and less cladding, less and less woods metal remained in the original position.

This band over here shows you the proportion that moved upwards, and this lower band here shows you the proportion that was either in train or moved onward. Basically, this is the difference of the sum of those two from the hole. What comes out of this is, and that is not really so shaky, this polameter has been used before for such configurations. I think what is important is, we demonstrated that also for a metal with a high self-extension and for geometries of extension.

For example, there were no data as far as I know in the very small clearances of the type we were interested in here, but the idea is for a range of GAG stars around one, the cladding is basically really undecided. Some might call it limitation. Some might call is sloshing, but it doesn't want to go in any clear way up and down. If the GAG star is less than .9, the cladding would like to drain, and if it is more than 1.5, we have a clear, sustained upward relocation trend.

So, these are the criteria for clad relocation. To obtain an idea of the timing of such motions, we measured the film thickness, and here is the fraction of film thickness as a function of time. You see, when GAG stars about one, there is not much variation. Again, the idea is, it wants to sit there and slosh. If it is 2.16, it goes out with the time constant of about one second. If it is 3.29, it goes out even faster, but not dramatically faster.

With this in mind, we want to take a look at the INPO test parameters vis-a-vis the reactor parameters. Here is the RC series. It is a seven pin test. There is one central pin with six around it. The behavior is highly two-dimensional, and in fact this is supported by the discrepancy between the SAS prediction of the time to flow reversal commpared to the test.

Basically, the test delayed somewhat the trend I showed you before as compared to SAS. In a given flow reversal, that indicates a central boiling zone with the sodium flow going around it. Furthermore, because of the proximity of the six pins to the wall, which of course is a very good sink, radial heat sink, it is very difficult to melt those radial pins.

Therefore, we expect a strongly two dimensional temperature distribution, and this parameter of A tilda which represents the fraction of the molten pins to the total number of molten pins is a very slow one, and we will put something on the order of .4. I refer to this as the radial incoherency of the bundle, representing the degree of radial incoherence.

Another two parameters that are important as far as the collateral location is concerned is the delta P, and it was measured at 11.5 psi. Also, another parameter that is important is the chugging intensity. After the bundle is voided, and that is the time frame at which you will look at collateral location. Sodium, because of this delta P that exists, wants to enter back into this heated zone, but as soon as it enters, it evaporates, produces local pressures, and goes back out again.

So, this leads to an even out mode of motion of the sodium at both ends, known as chugging, and very confidently with high accuracy we know this philosophy of chugging because it is measured with electromagnetic flow meters. In this case it was plus or minus .9 meters per second.

Looking at the P series, a bigger bundle, 37 pins, the behavior was one-dimensional. Looking at the thermal principles, all the predictions and all the comparisons, predictions, and tests, we see very clearly a one-dimensional behavior here. Of course, again, the pins that are right next to the wall, again, they will delay melting, because they lose heat to the can, but now, because of the 37 pins, the fraction of those pins compared to the total cross-sectional area is much smaller than compared to the R series.

Therefore, the incoherency factor here for radial incoherencies will be one. The delta P across the quarter was 16 psi, and the delta P, the chugging velocity was 1.61 meters per second, somewhere higher than the delta V over here. For the reactor, based upon the voiding profiles I was showing you before, we estimate radial incoherency of .6 or 60 percent. The delta P across the core in the reactor would be on the order of 15 psi, and the chugging velocity, this is coming out of SAS, 1.3 to 2.4 meters per second.

What we see is that the R series is closer to the reactor as far as incoherency but the P series is closer to the reactor in terms of delta P. Let's see if we can use this together with the previous information to understand the difference in blockage formation between those test series.

Our mental picture is in the middle of the bundle somewhere near the top a certain fractional portion both in the radial as well as the axial direction of the cladding becoming molten. That is the initial incoherency point. So we take this ratio to the total. We take this ratio to the total, and that is the L tilda. When I say delta P across the core, that is the delta P across the lower sodium interface to the upper sodium interface. That is the delta P. And when I talk about chugging, that is that interface goes in and out and this interface goes in and out.

The delta P across the core is convenient to make dimensionless by the liquid sodium static head as you saw over here, so this number M then is the number of liquid sodium static heads representative of delta P across the core, and the important point here is that somehow this pressure that is available to move the cladding has to be redistributed both across the dry parts as well as across the wet part, the part that is moving.

And of course what is important if we are going to use the correlation I was showing you before, we have to know what is the sodium vapor velocity in this two-phase region, and that is what we are after. Well, you can do all of this, and you end up with a graph like this, which shows that for any degree of axial and radial incoherency, it shows you trajectories of values of JG star over the square root of M going through, so let me explain this.

The way you use this is, you pick out a point here that represents your degree of incoherency radially and axially, and then you look at which trajectory goes through that point, and you read off here the value of JG star over the square root of M. Then, if you knew what M was, the square root of M, you multiply and find the volume of JG star, and compare this volume against the criteria I gave you before.

If it is something more than 1.5, it would be moving upward. If it is below .9, it will drain. If it is in between, it will stay suspended. So, with this background, we like to put the points for the P series, the R series, in this plot and see what they tell us. If I put this R series, that will be, remember, a radial incoherency of .4, and a small axial incoherency. That would be a trajectory of around 0.7, and I multiply that times the M, square root of M of R series based on this 11.5 psi which is 2.2 and I get a JG star of 1.5.

That means for this test the cladding was at the threshold of really feeling that steady upward relocation, but was not really very determined, and that is why it just barely made it into the reflector region and froze there, ending up with a very thin blockage. Also, this undecided behavior then, all it needed was a little bit of gas like happened in the R series and you could completely reverse this motion, have complete draining, and you won't have any upward location, as was again found experimentally in the R series, in the R8.

Now, then, let us put the P test here. As you remember P was very one-dimensional. That is an A tilda of one. The L tilda would be a small number. That puts it right around here. And you read a value here of about 1.3. You multiply that times the delta P of 2.5, and you end up with a JG star of 3.2. Clearly, in the upward locating region, and certainly it was no surprise that these two tests actually moved enough cladding up to plug by two centimeters, and in the other test more like ten centimeters.

Now, the other interesting thing to visualize here is, as you move in, as the time goes on, more and more cladding, more and more axial portions of the cladding are molten, and as this happens, you move in this incoherency plane upwards. Now, in the R series, because always the radial pins are so close to the wall, and always they irradiate and lose energy, they don't come into the melting for some time. Therefore, the tendency would be to move straight up, and as you see, you are crossing still smaller and smaller JG values.

So, if the initial tendency was barely upward, right at the threshold, as the thing melted more and more, it clearly became sloshing, and in fact later on maybe into draining. The same with the P series. As you move up this way, you see the numbers get smaller and smaller. So, again, although the initial tendency was to move up, the numbers became smaller, and eventually this got to the point where it just got into sloshing.

The reactor is somewhere between to start with and moves also a little bit in between, because you are going to get more melting in the radial direction as well as the axial direction. The reactor starts off with a JG star of 1.9, which is slightly over that (indicating). In my opinion, it still has a tendency to move up, but not a very pronounced, and certainly not at very high velocities. As the time goes on, and this trajectory here is written very rapidly in time, last night we were trying to figure out if we could remember how long it takes to go from here to here, and we were hoping to call this morning to look at the outputs, but we didn't manage to do that, but I will venture a guess of something on the order of maybe .2 to .3 seconds under appropriately high power conditions to go from here to here.

So, as you see, quickly you go from 1.9, 1.6, and beyond that you go below the flooding point.

Now, one more thing that needs to be said here to complete the story I think is important, and it has not been considered before. I think it is crucial. This chugging, there is enough of a delta, enough of a velocity intensity variation in the chugging that really -- it takes quite a bit of pressure to move liquid slugs moving into 1.5 or 2 meters per second, to take that, reverse it, and push it back out again.

This localized vapor source of sodium could give another pulse to this whole thing here, and that pulse would be limited in time, but could be intense. And the frequency of those pulses is of the order of, if I remember correctly, between two and five hertz. Therefore, you can see that if this whole process of going from here to here is something on the order -- that it takes something on the order of .2 or .3 seconds, and if the frequency itself, the period of the chugging is something of the same order of magnitude, you see that at the time you hit this point, depending where you were with respect to this oscillation of the chugging, you could have a certain randomness element that maybe looking from the outside, from the instrumentation experimentally, might show as a blockage formation between two centimeter and ten centimeter.

And you say, well, what does that mean? I want to point out there is a certain random element here which is real, not just imaginary. However, one thing we haven't said here, and we have to be consistent, if we are going to take into account the plenum fission gases for compacting fuel, we must also take a look at what is the possible effect clad motion. This is the nominal value in the absence of any additional flows into the channel. This volume JG will change.

MR. OKRENT: Excuse me a minute.

MR. THEOFALOUS: Yes.

MR. OKRENT: Were you suggesting that the chugging occurred in such a way as to give the difference between the two and the ten centimeters of plugging?

MR. THEOFALOUS: I think that is a real possibility in my opinion.

MR. OKRENT: Well --

MR. THEOFALOUS: It is the same chugging, but because of the shortness of going from here to here as compared to the period of those chugs, if you hit this point, for example, in the P series at the moment when a chug had just reversed, that would give you a different instantaneous movement than if you hit it at the time the liquid sodium was just coming in and there was some melting before the pressure developed, so there is a bit of randomness there.

MR. OKRENT: It seems to me if you are going to do that you should look at the possibility that chugging would have done something different, and not just use it to explain in one direction the experimental results. I have a little bit of --

MR. THEOFALOUS: What do you mean, one direction? I don't think I put any directions.

MR. OKRENT: What is the chance it would have occurred at just the right time, and what is the chance that the chugging would have done something different in some of the other experiments and so forth? Let me just leave it as a thought for now.

MR. THEOFALOUS: Well, I think your thought maybe is a little disturbing. The picture I am trying to paint here, and I don't want to leave it there, I think I make an important point. First of all, the JG star here is 1.5, and here it is 3.2, and in my opinion that is a hell of a big difference in JG star. Independently of chugging, I have no problem at all seeing an undecided collateral location here and a clearly forceful upward relocation there.

Now, we have said in the past and we still have the same opinion, based on the time it takes to move cladding under those conditions, that typically cladding would move with 50 centimeters per second. Now, if you look at that, I guess I am saying that I have no problem reconciling the difference between the R series and the P series.

Now, you might ask me, why in the P series two, and in the other non-experimenting P series two centimenters and in the other ten. There is some difference there. The important point is, and we soon lose sight of that is, in both cases we have blockages. As I said before, independent of what is going on with the chugging, I expect blockages. So at least we have been able to accomplish that.

To actually quantify the difference, I don't think anyone has done it, and I think it is difficult to do to precisely tell you the blockage, but I think it is important first of all to know under what conditions you have blockages and under what conditions you don't, and what I am showing to you here is that in the reactor conditions the JG is so marginal I don't expect to have blockages.

What I am going to do more, I will come back and tell you this volume JG will have to be brought down even more, because when I take into account fission gas coming out from the plenum into the channel, it will be an effective reduction on this M, and this is how this works. The gas comes into the channel from the plenum through the gap between the cladding and the blanket. Typically the gap will be here. And then joins the vapor flow to go out.

Initially and intuitively, when we started thinking about this, and also when the project started thinking about this, people expected this gas coming out would be so forceful that it will reverse the pressure gradient, basically producing high pressure here, moving both directions, and pushing all of the cladding downwards this way.

Well, after careful examination and analysis, and in fact that is one case where we in fact went into SAS and made some changes with new modeling, we decided that for the range of interests, in fact, this would not happen. Here is what happens. The overall delta P is like this as long as this channel is single phase. You have a pressure grading like that. When you start injecting gas, if this gas is not enough to reverse the pressure gradient, it will have in effect a higher pressure drop over here because more volume was flowing through. Therefore, it will be a readjusting of the pressure gradient.

So now we go into something like this (indicating). Now, this readjustment of the pressure gradient will be upwards. At this point it will be moving up. At the beginning, we have its maximum position when the gas flow is maximum, and as the gas flow reduces in time, this point will move back down to this line.

Therefore, if you are interested to know this pressure gradient, because that is the one that will now be concentrated to move the cladding, redistribute or move the cladding, this one is characterized now with a new effective M which we are plotting here at M is a function of time, and that M now will be changing, taking into account the movement of this point downward, because the rate of release of gas is going down with time.

What you see here, the effect of M is essentially zero at the beginning. What that means is, we are allowing so much gas there that all of the delta P can be accommodated from here to here basically blocking off any flow. So M is zero now. If I went back and put up my previous plot, and I was concerned about clad motion this time I would tell you it would be draining, because again I will be here. I will be reading 0.9 times zero is zero. JG star zero. It means draining.

Okay. Well, however, as the gas flow is reduced, the M goes up, and in fact in this particular example, and we found it to be the case in many of the examples we have run, by the time the cladding melts, and that is about .6 seconds after the rupture of the cladding, the failure of the cladding M already has reached almost its nominal value of about two. M is already 1.6.

Therefore now if I went back to the previous plot and read off JG star over the square root of M and multiplied it times this, I would get a JG star of 1.4. That puts me into a very low value JG star as far as upward relocation is concerned. Therefore, I come back and say there would be no upward relocation. Basically, we expect the reactor, a location more typical of the R series than the P series, and that is really the bottom line.

MR. OKRENT: What is your probability on this argument? Is it one in ten, or one in 100, or one in two?

MR. THEOFALOUS: You have to define first what you mean. If you were to run so many experiments in how many experiments you would not find the blockage? You have to tell me first what you are looking for.

MR. OKRENT: It seems to me the question is a little broader, because it is not so much experiments, but given the variety of possible situations that might exist in the reactor, not just those that pertain to this experiment, and then given how much one knows here, what is the likelihood that you will or will not get blocking above?

MR. THEOFALOUS: You keep coming back with a variety of conditions. I think if you told me one specific instead of a wide variety of conditions -- pick out one you are interested in knowing if cladding will move up, then I can answer you. We address here what we think are nominal conditions for loss of flow accident. If you think there is an accident in which you think there is a delta P across the core, that is one psi with no sodium flowing and cladding melting, then I would be able to address it, but if you tell me the delta P across the core is zero, fine, I will be able to answer you.

Based upon a loss of flow accident, we pretty much know what the delta P is across the core, and that is the fundamental parameter. Now, that is the one thing that of course you might like to put another number on it, but you have to tell me why you want to choose another number. If you have a very high pressure you wouldn't be voiding to start with. You wouldn't have a loss of flow. So I am not sure what you are looking for.

MR. OKRENT: I think there is somewhat of a deterministic picture of this situation. The boundary conditions for the physical situation you are describing.

MR. THEOFALOUS: Sure.

MR. OKRENT: And it is not at all clear to me that there isn't rather a considerable set of boundary conditions given the title Unprotected Loss of Flow.

MR. THEOFALOUS: I think, Dave, as far as collateral location, we are almost as deterministic as we can get, because it is one condition that leads to that, and that is loss of the pumping power in the pumps. Now, again, as I said before, if you postulate you have the pumping power, you don't have to worry about that. If you lost it, you lost it, and pretty much you know how much is your delta P across the core.

Now, if anything, it could be something happens to the pump and they arrest. Well, if they arrest and they can't pump anything, then you will have even less delta P and it will be moving out in the direction of even less collateral location, but I don't think it stands to reason now to arbitrarily think of this problem.

This is probably as clear as we can get to put a lot of undefined probabilistic ambiguous things on it, because then what will we do in real problems where we have reasons to be ambiguous? So I don't really want to take that ambiguity picture. I don't buy it for this kind of a problem here.

I will give you a chance to raise ambiguity questions later when I talk about recriticalities. Okay?

Therefore, we believe that plenum gas blowdown slightly interferes with upward relocation. I say slightly here because it is not very pronounced to start with, and then it is kind of tipping it off a little bit. As you see, by the time collateral locates the M value is close to nominal. That is why I say slightly.

Nevertheless, we use an upward relocation of 50 centimeters per second. We believe by making this choice we somewhat overestimate the real tendency for upward collateral location, and the way we do that is going in and putting models in SAS that will give us this kind of behavior.

We expect then incomplete blockages. We say they cannot be excluded. Maybe to some extent we expect them if there is enough time. For example in the case I showed you before, having to do with the plenum gas compaction, in that case there wasn't enough time to start with even if the cladding wanted to move upward; there wasn't enough time.

Some of the cases I will show you next, maybe there is enough time, and maybe there is some limited cladding blockages.

The other thing I want to emphasize on those blockages is, as cladding is relocated upward and freezes again it causes a lot of concentration of pressure grading, and that pulls out again, reduces the effect of M in the core. As a result of that, JG star goes down and the streaming is cut off and you can't carry any more. Therefore, these blockages with a very high degree of confidence, almost as high as you can ever get, I think, for myself, I don't expect to see completely blocked up LMFBR bundles under those conditions.

Here, then, we want to say also that we cannot significantly plug up the core, although we might allow some limited blockage because the upward location of plugging produces reactivity feedbacks, increasing power, then leading us into co-disruption, so it is almost like you cannot escape. It is almost like the accident cannot escape.

MR. OKRENT: Excuse me. You just made a statement that isn't completely clear to me, because I could envisage a situation where you had upward motion of cladding which of course includes a reactivity effect, but that other things were going on at the same time so the net effect was no change in reactivity or even a decrease in reactivity, so I don't really understand.

MR. THEOFALOUS: I can answer this question, yes. I kind of expected you to bring it up. The other things that could be happening that would be significant from the point of view of intensity of reactivity to be negative would be fuel disruptions. We went through that before, and I think you were telling me there that maybe the fuel might be more compacting rather than disturbing. Therefore, the tendency there if anything would be to accelerate the accident.

So the only way you have to postulate, and you have to be consistent in this game, that you really stretch out the accident so far that you allow a lot of time, is by assuming monatonic fuel dispersal, and I think first of all I don't agree with this concept of monatonic dispersal, and I don't think you would agree with that.

MR. OKRENT: But we are talking about not very large amounts of reactivity associated with the amount of cladding.

MR. THEOFALOUS: Thirty-five dollars.

MR. OKRENT: For all of the cladding?

MR. THEOFALOUS: Yes.

MR. OKRENT: I maintain you are not talking about very large levels of reactivity, and I could envisage fuel moving around to balance, so I must say at the moment I remain unconvinced about your statement about not signficantly plugging the core without reactivity and power increases. It is not at all clear to me that is a one to one conclusion.

MR. THEOFALOUS: I think you might be more convinced after you see some of the next vu-graphs. I think that again in order to understand those things, one has to make an effort to really look at the physics, what is going on in there. If you postulate a monatonic fuel dispersal, I grant you that is one of the ways you can stretch out the accident, and you will be blocking and moving a lot of cladding, but as you will see now, and I think as you already agree, the fuel cannot go very far. What is moving the fuel to start with is fission gases not in very great quantities, and we just a few moments ago agreed that gas will slip through.

In fact, you also agreed with that. If the gas is going to slip through, what is going to keep -- that fuel dispersed it is going to come back down. Now, you will see this picture very clearly.

MR. OKRENT: I guess in effect part of what bothers me, Theo, is what I tend to be hearing is something that takes me down a road from the beginning to the end without sufficient statement of the fact that there are other roads and there are other combinations of roads, and I can't tell which of these you think might be important, and so forth, but again, at the moment, I just don't see the basis for the statement that you seem to have made unequivocally on that previous vu-graph.

MR. THEOFALOUS: If there are any other routes, Dave, that we have not considered that support your concept of how the accident progresses, we would be happy to look at those routes and see if we have not in fact considered them. I think what you are missing here is, there is a certain time constant or time scale associated with fuel motion. There is a time scale associated with clad motion, and there is a time scale associated with incoherencies across the core.

All of those are relevant times, and they are not necessarily as ambiguous as you might like to think. Now, we have looked at all of those time constants, and we have looked at them from both sides. As you will see here, we have looked at them from the point of view of putting not only normal but higher than normal reactivity worths. We have looked from the point of view of putting as low as an activity worth as you could. We tried to look at the whole range, and I don't buy the concept that we are leading you down one road here, a one path road. I think in fact the opposite, and I really want to emphasize that here we are trying to show you in this section exactly the whole spectrum of routes or the ranges of conditions we can encounter here, and we think that is very crucial.

On the other hand, if we have some inherent processes that always lead us down after going through some intermediate path down the same road, I don't think I should try to jump out of this road just because I want to jump out of it. I should have a good reason for doing so. Basically, what we are saying here is, the reactivity stage of the materials is such that when you start disrupting them, you cannot stay at very low power. That is really the statement of fact we are saying. I think you have to produce antigravity. You have to put things up on the moon if you want to achieve that.

We are saying, as long as the reactor is sitting on this earth, you can't keep it at very low power for a long time. I think that is an important statement. We believe it, and we hope it comes true. It is really part of the story.

Now, if you don't believe that, you have to come back with some real argument to tell us how magically you are going to disperse the fuel and keep it there. But what decision you make at this point has an impact on fuel dispersal, criticality, and all of those things. I don't think you will be able to get a recriticality on the moon where there is no gravity.

So here, then, is Case B, and this one is what do you consider reference LF case, and here we put all of the reactivity worths to nominal or slightly above nominal numbers, and this is exactly the same case with the previous case of fission gas compaction I showed you, with the only difference being that at this point where the fuel disrupted, instead of letting the pellets coming in, rushing in under the pressure of the plenum gas, we let the fuel disrupt.

Now, we know what would have happened if the pellets were free to compact. Now we would like to follow a different path to see what happens if the pellets are somehow lost in there and they cannot compact or for some reason the gas gets blown out and is not there to push them down.

You see, what happens is, the fuel introduces negative reactivity. The power goes down, but it doesn't stay very long there. One tenth of a second, and boom, there it goes again. That is a fundamental behavior, and we are going to be really centering a lot of our thinking and arguments on that fundamental behavior, that you can't stay at very low powers after you start disrupting. It is very fundamental, and I think we can make some very good arguments for that.

MR. OKRENT: Again, why is it you can't stay at very low powers for times that are a quarter of a second or half a second?

MR. THEOFALOUS: Because the core is incoherent. The core is now different channels undergoing voiding. Some others undergoing steel melting. In other channels, fuel is disrupting. In the ones where the fuel has disrupted, it goes up. It cannot go very far. Then it will come back down again. The fuel cannot stay in a fully suspended condition, because there is nothing to keep it there. We have to have a way. It is totally an artificial situation. If we assume the fuel is totally dispersed and stays there in the absence of any driving forces, the steel is cold so it can't provide its own vapor pressures to stay boiled up, so it is an impossibility.

This is all very clear. You will see it even more clearly in the next slide. At this time, then, at this burst. We have shown the material patterns for cladding. Most of the core is molten, as you see, and now, because this was slower, because of this additional little time here that we bought by allowing the initial fuel to disrupt, we moved some cladding up into the top, but you see it is limited, because these, this cladding here melted so close to this time they didn't have time to move up there.

Therefore, again, most of the core is unblocked above and below and in fact we believe that even those blockages here are very thin for one thing and not fully solidified either. The fuel pattern is shown over here. A lot of the core molten. Vapor pressure you see here. Again, remember, in those channels where you have four and five bar and 12-bar, all of those channels are ready to give you also steel vapor pressures because there is a lot of steel around. So that is again the concept of co-disruption.

On the other extreme of another case in which we made reactivity worth as low as we could, to a very low sodium worth, 100 percent axial expansion, increased the doppler and all of those things, we have a prolonged transient maybe by about four seconds compared to the previous case.

So what you see here again, you don't see a monatonic low power. I think it is a very essential character of this system. It goes up and down. It goes up and down. I want you also to remember that the time scale of these things is of the order of .5 seconds. That is typically the time it takes for the fuel to come up and come down. Again, reaching a condition of burst here, because again some fuel is coming back down again, and here is the clad pattern, here is the fuel pattern, here are the fuel pressures. We give a 40 bar on this channel 6, and because we prolong it a little longer, we have more cladding moving upward, and maybe a little more blockage, but again, the core is largely unblocked, and again, remember that with this 50 centimeters a second we do, we overemphasize the clad motion upwards.

Also, I think I want to say in all of those cases we took into account this M modification, the effect of M, the cause of the plenum gas coming out.

So, to summarize on this case C, we expect extensive co-disruption. We expect extensive axial relief paths, and we expect neutronic activity. That is fundamentl in the initial disrupting stages of the core, and pressures to be present.

This is the conclusion of section B, and we will be making use of those fundamental behaviors as we believe represent a broad range of calculations. And it is not only those two I am showing here, but we have done many more calculations. We will be using those then as we go to examine the potential for recriticalities and dispersal.

Are there any questions on this Unit C?

(No response.)

MR. THEOFANOUS: Okay. Now we can take a look at recriticality. We have defined recriticality as --

MR. OREKNT: Excuse me. Would you say it is vital to your argument that you don't block a large part of the upper blanket region?

MR. THEOFANOUS: I don't think it is vital in the sense of that I don't believe if you went there and you arbitrarily blocked out the upper and lower core, I wouldn't particularly worry about getting energetics more than $100 a second. But I think it would lead you in the wrong direction from what is to be considered as a best estimate of behavior.

MR. OKRENT: But you don't consider it vital from the point of view of threatening the --

MR. THEOFANOUS: The primary system.

MR. OKRENT: The early release.

MR. THEOFANOUS: Right. Dave, I think that will become a little more clear after we go through recriticality. You will see what is really the emergence from the recriticality treatment. If you will go through this and feel pretty comfortable that even with the whole material there we can do different things and not really violate the primary system, I think really the concept of blocked-up core really leads you to recriticality.

So if recriticality can be bounded in a reasonable way, and it's not all that critically dependent upon having 80 percent of the core or 90 percent of the core inventory in the core, then I think you look at this question differently.

If, on the other hand, I came up here and said, it is very vital that we lose 20 percent of the core in the initiating phase and lose it only because we not able to plug, and if we had 90 percent we would have a real big problem from recriticality, then it would have been vital. So I don't think it is vital.

MR. OKRENT: Okay.

MR. THEOFANOUS: The definition is then -- and we will characterize here only those fuel motions that we get from a disrupted fuel. And where we are in our general picture here is shown here. We are concerned with al of these red paths. Looking at the successively higher core-disruption stages and looking at how we could end up with an energetic event from those and how large such an energetic event could be. The requirement to have such an energetic event is that we have not removed more than 40 percent of the core inventory. And I think as I said before, this number should be more like 30 percent, especially for the early parts of the disruption.

Now, compaction can happen two ways. One is through pressure, and the other is through gravity, or any combination thereof. We will take a look at each one of those items.

Pressure-driven recriticality involves open channels. If the core was already plugged up, we could not have the ability to interact fuel with sodium and produce pressures and therefore compaction.

Therefore, it has to happen at the subassembly pool stage if such an interaction were to take place, because if you are in the annular pool stage, by definition you are already plugged up. The annular pool stage wouldn't be there if the exits of the core weren't plugged up. So you couldn't stay there. There's a few seconds it takes between the end of the initial core disruption and going into the annular pools. It takes a few seconds. You have to melt all of the subassembly cam walls.

Through this time then, if there were any openings, the fuel in the cladding inside those subassemblies would like to get out. Therefore, that is fundamental. Because of this then we expect any such direction, should they occur, to be highly incoherent. They just happen independently in subassemblies. And I remember a number once, some calculations requiring something like a 24-meter-per-second in 24 subassemblies, I think, to give a number of the order of $100 per second. Is that right, Charlie?

MR. BELL: 12 subassemblies.

MR. THEOFANOUS: 12 subassemblies, 24 meters per second. That's a lot of velocity. And from that you can get an idea of how much pressure you must have to produce the kinds of reactivity ramp rates that are of interest here.

And finally, last but not least, no pressure events were noted in a lot of experiments that were made for the purpose of addressing this problem. Therefore, because of all of these reasons, we decided we would like to ignore that, to ignore these mechanisms.

MR. OKRENT: Excuse me. Before you go on, is there complete agreement among the various participants concerning the pressure-driven recriticality question?

MR. THEOFANOUS: Yes.

MR. OKRENT: Are there any of the participants who hold it as something that needs more attention?

MR. THEOFANOUS: Yes. There is complete agreement. It's unfortunate we didn't go through the other part, because following our plan, following the definition of all of the tests -- and none of them is addressing these pressure-driven recriticality -- we asked all of the consultants and all of the participants for comments And we have not seen any qualms about not considering this pressure-driven recriticality any more than what we have already.

On the other hand, gravity-driven recriticality is not only possible but is even likely, we believe. First of all, we always have the gravitational acceleration. There will be substantial heat sinks which promote condensation. And vapor pressures, of course, are the pressures that promote dispersion. If you lose the vapor, you lose the dispersal potential and, therefore, you go into recriticality.

And furthermore, there are two processes here that we have suspected for some time. And finally, we are coming close to being able to quantify. And those are the processes of triggering and tuning. And again they --

MR. CARSON: What are those processes again?

MR. THEOFANOUS: Triggering and tuning. I will go into that. Therefore, because of those reasons, we decided that recriticality, ggravity-driven recriticalities, need to be considered.

First of all, an illustration of the triggering. It has to do with the high neutronic activity we expect in the initial stages of disruption. There is a lot of steel around. There are can walls, basically, everything one needs to fight fuel dispersal. As a result of that, you see it here going up and down.

Therefore, because of the very highly nonlinear fuel position versus reactivity of the core state and power state of the core high nonlinearity, you don't expect that all of the motions will be just about right, just to keep you always at the optimum level. There is enough condensation, there is enough gravity going on, we believe there will be overshoots and power bursts very much like what you see here, maybe as much as a few hundred nominal.

That would not qualify for an energetic event, but it is a power burst that is felt throughout the core. And that is what is fundamental. The core is highly incoherent to start with because of the flux shape, and it is highly incoherent also because of the flow differences across the core.

But it has a coherence imposed by the neutronic activity. In other words, if there is a reactivity surge someplace, the whole core feels that because the power goes up in all of the core. If there is an activity reduction someplace, the whole core feels that again because the power goes down through the whole core.

Therefore, this neutronic coupling across the core is what leads to this comment of tuning. When you enter the early disruption stages, you get subassemblies fully dispersed and others that haven't even dispersed yet or even disrupted. Now, then, the pressures most likely in the ones dispersed will be high pressures. The pressures here will be low pressures. This is pressure against subassembly number.

And here is a pictorial of the kind of different states in which the core finds itself from one place to the other. However, as this fuel is dispersed here, if it can't stay monotonically in this state, if it comes down, it will introduce a reactivity, and it's possible we might see power a few hundred times nominal.

If that is to happen, this subassembly here will feel that. Whatever pressures it was undergoing, it is going to see that power pulse and it's going to accelerate it for more melting, more vapor disruption and more dispersal maybe.

So even if this were coming down at this moment, if this had already come down, that power pulse will produce vapor pressures that might even reverse the motion for this one. So you see there are mechanisms then for the subassemblies gradually coming in tune. And that is what we are referring to as "tuning."

And that is shown over here, that after a few cycles of those, we expect to have much less variation across the core of pressures, and we expect there will be more closely related motions and material configurations in the individual subassemblies within the core.

MR. KASTENBERG: Theo, what are the internal blanket elements doing during this tuning?

MR. THEOFANOUS: That depends on the sequence. In most of those cases, already they are well on their way to voiding, already voiding.

MR. KASTENBERG: And towards the end of life, their power profile is pretty good because you have bred in fissionable material?

MR. THEOFANOUS: That's right.

Now, this obviously leads us to the question of how incoherent can we get, and what if we achieve complete incoherence. And I think from the point of view of feeling the fuel motions coming back down from the point of view of an energetic event, coherence means a few milliseconds around that prompt burst period.

If there were some material motion that was much higher just before that, it doesn't count. If it is only material meant to be accelerated downwards at a later time, it doesn't count.

Within that narrow window of a recriticality event, that is what covers all of the material motion distributions and all their contributions to reactivity. That is what counts in getting it through the prompt burst, and that is what counts in yielding energy. So that is very important.

It could be, for example, two subassembles are pretty close but still not totally coherent. One has to be careful not to overinterpret that coherence.

So with this in mind, we need to talk about the time scales. We need to worry about the relevant time scales. We expect activity, typically a time constant of about half a second.

You might argue with me, .7 seconds or 1 second or maybe .3 seconds, but it is typically of a few 1/100ths of a second. That is the period between the pulses. It takes on the order of 1 to 2 seconds to melt the driver walls. That is roughly how long it takes, and you won't find very big variations of opinion on that.

Therefore, with this kind of a pulsing period here, you can see that you only have time enough for something on the order of 2 to 4 pulses before you lose all of the subassembly wall structure.

Now, it is possible that these pulses become amplified. It's possible they become more coherent because of more coherency motion. However, we believe -- and that is a qualitative argument -- it is rather unlikely that we have a highly incoherent core to start with getting to this. And now within only 2 or 3 pulses to ask a perfect coherence before we lose the subassembly walls. And that is, I think, also important to remember.

Those qualitative considerations then lead us to the identification of certain trends. We call those the principal trends. The initial motions are highly incoherent. Oscillatory motions are gradually tuned and amplifying due to highly nonlinear compaction states versus power relationship. Heat sinks influence, but do not control, the dynamics of fallback.

With this introduction, we are ready to discuss the extent of recriticalities and the ramp rates associated with that. And before we do that, I would like to say a few words about the reactivity configuration. The reactivity state of different configurations of the core.

As we look at the core, we can identify the inner ring, the inner two rings, and then the outer annular region that we separate into two rings. And to get a perspective against which to judge fuel motions and the reactivity ramp rates associated with those motions, we have looked at different configurations of compaction of those different rings, different combinations of them.

And here I will go only to show, because of time limitations, only one part of that. And that is the case in which the two inner rings are compacted while the two outer rings remain in full height. What we are showing here is if you are to compact those two inner rings, you must remove some fuel, you must reduce the whole core inventory because otherwise you will be supercritical.

And here is the trajectory. For example, for a given compaction state of these two inner rings, you find out how much core inventory removal you must have before that condition is possible. What this tells you is that even with a relatively minute quantity of core inventory being removed, you can accommodate pretty large amounts of puddlings on the order of 20 or 30 centimeters, almost a full puddling of the two inner rings.

And this is important for two reasons, and one of those reasons is something more on the matter of curiosity, but I think it is very interesting, and we decided to discuss it here. And this came up as, I guess, a result of us looking into the details of the disassembly phenomenology that I mentioned under A.

Especially here is an interplay between the rapid response you obtain from single-phase fuel when it is heated because of a high pulse against the slow response you get from a two-phase medium as it is heated through a high pulse.

If you were to do that, as you saw under A, if you were to do that over the whole core when the fuel core was molten and compacted, you could put probably $1000 a second and you wouldn't have felt anything because it disassembles very quickly.

But if you were to do this with a small amount of puddling at the bottom and all of the rest of the core being dispersed, the same effect could come in to give you a positive reactivity feedback during disassembly. We will call it an extra kick. In other words, it tries to go through this assembly; it can't make it because the single-phase puddle expands into the high-worth region and that gives it another, it hesitates a little bit and gives it another big pulse in activity and comes down.

This is one of the new things, for example, we found in this whole process, and I think it is quite interesting. First of all, let us take a look again at the configuration. I mentioned before the two inner rings puddle at different levels while the two outer rings stay up. And let's look at the flux profile against the puddle height. So the puddle is to the left of this here.

So this point is we start out with a fully dispersed two-phase region in these two inner rings. The next one over here, we let it puddle by something like 10 centimeters at the bottom. The whole thing is coming down and puddles by 10 centimeters. What you notice is that the height of the puddle is below the peak of the flux and the magnitude of the flux, of course, itself reflects the worth of the materials.

And now if you artificially -- and I say artificially because we are not really sure about that -- but if you postulate that this puddle cannot expand downwards. And to really assess that -- we haven't done that -- to really assess, you need to have a coupled structural hydrodynamic neutronic quality where you do a whole structural response to that to see if you can push anything downwards.

But if you assume you can fix the bottom of this puddle and you only let it disassemble upwards in one dimension as it goes through the heating in the early part of the disassembly, what you are observing is this puddle is going to be moving very rapidly, much more rapidly than the two-phase fluid out here is going to be moving out. It will be moving into high void regions, giving high reactivity rates.

Then what happens is, as the degree of puddling decreases, and that is shown successively from here to here to here to here to here, this distance between the maximum and the puddle height decreases and at some level of puddling, in fact, coincide and from then on the maximum is inside the puddle.

In this level here, of course, the puddling effect is beneficial because it expands quickly, it sets itself out quickly. In this range over here it is detrimental. So we have plotted then this difference.

And when it is positive, it is detrimental; when it is negative, it is beneficial.

We have plotted that against the degree of puddling. And what you see is the point of breaking even here is 20 centimeters of puddling. If the puddle is greater than that the effect is beneficial. Throughout this range then, if we have a disassembly, we expect we will get again, if the bottom is what expects some rotation.

From that point of view then, it is very interesting to know how much of puddling is possible in these two inner states. And I want to put out the previous plot to show you that in order to get a 20-centimeter puddle, you must remove just 1.5 percent of the whole core. And we think there is no doubt at all we will be able to do that.

If you remove any more than that, you need puddling higher than 20 centimeters, and therefore, we feel from the point of view of the CRBR assessment, the present assessment, that this point becomes moot.

Again, in more detail, to obtain this yardstick, I was saying before, we have here plotted the reactivity of the core as a function of the puddle depth in those two inner rings. And then taking the derivative of this, we are showing the differential worth per centimeter of fuel.

What this number shows is, and reading it over here by how many cents will the reactivity increase per each centimeter of fuel compaction that we have, what's interesting to see is there is a maximum right around the 25-centimeters and as the degree of puddling increases, this differential worth is going down.

Again, relating this back to the amounts to be removed and the high degree of puddling expected here to get recriticality, you see that the rates will be low. So now having this number, and we have, and we are producing even more plots of this type for different configurations of rings, having these numbers, one can relate a velocity of puddling or a velocity of settling to a ramp rate. You just basically multiply.

What you are seeing here is 30 cents per centimeter. There are other cases we have seen of 40 cents, other cases of 80 cents. We think, however, we can bound everything we have seen, all of these different combinations, very crudely but quite adequately by $1 per centimeter of reactivity worth, differential worth.

Now, then, looking into going back into the subassemblies and looking at the oscillatory bahavior of the subassemblies, we can postulate a power pulse of the type we saw in triggering or tuning that will produce a high-pressure region someplace in the center of the mass, leaving half of the mass on one side and the other half on the other side, and therefore, this high-pressure region causing an explusion of this mass upwards.

Now, the dimensions here are such that the distance traveled by the slug divided by the slug thickness is 2. And the aspect ratio of the slug is also 2. And we have some experimental analytical information that indicates that under these conditions the slug will in fact break up on its way up. Basically, the bubble will vent through. It can't just push up the slug in a one-dimensional configuration, make it go up and let it come back down. In fact there will be a venting, a vent through.

So it will be a pressure equilibration. And if this is about half as it is in the reactor and if it's one-half of the half, or 25 percent, and this is another two of those, you see that if you took that liquid and disperse it over this volume, you will produce a void fraction, a typical void fraction or an average void fraction, of 66 percent.

Now, if you took all of that fuel material and let it calm down under gravity, you have lost the maximum velocity of 3 meters per second. If you multiply that times the liquid fraction up here, that would be something like 0.34 percent, and that gives you a puddling rate or an accumulation of the liquid in the puddle of 1 meter per second.

So you have to multiply this now times the $1 per centimeter for the whole core, arbitrarily assuming that all of the subassemblies are doing this together, exactly together, and then you get $100 per second.

So that is the way that we have bounded this kind of recriticality from a subassembly pool stage. We think this is an extremely pessimistic number because we believe that the coherence of this going up and down is extremely, extremely -- this coherent behavior is extremely unlikely.

I also want to mention just for completeness here, other breakup mechanisms. There is a breakup mechanism because there will be steel interdispersed in that fuel. When the power pulse comes, the fuel hits it very quickly, and the steel becomes very hard, and that acts like local nucleation centers that throw out the rest of the liquid. So it's a kind of randomness of the breakup process introduced because of this process here.

In addition, we have stabilities near the top. When that slug, whatever little was left of it as it tries to go in the top, compresses whatever vapors are there, you will experience a deceleration and therefore another breakup.

Furthermore -- and we have seen this in calculations also -- initially, because of the very, very thin power beating there is a tremendous temperature gradient over this whole liquid slug. So the first material to flush produces vapor, pushes the bubble around, and then more and more cold material, as shown over here, becomes exposed to that vapor.

So you have a lot of condensation going over here, a very high vapor flow which maybe even might produce some instabilities here. But in addition, because of the condensation, you are left with the other material down here that itself now is providing vapor and flushing. So that it is almost like an erosion process going on.

So this highly coherent behavior then is characteristic more of the annular rather than the subassembly pool. So that is what is shown over here.

And this is a SIMMER calculation that shows that the breakup of those puddles and slugs is not only a result of detailed instability behavior but also the gross hydrodynamics, as reflected by SIMMER, shows you a similar venting.

And again we have seen that in some experiments, specifically some of the MARK-III experiments, which are quite relevant here. You have an annular pool there also. I can go over that in a couple of minutes.

This is an annular pool. This is the cross-section of the annular pool. We assume it's all compacted and put a power pulse here, something on the order of 300 nominal. The flux peaking is such that the peak occurs to the inner edges of the pool and somewhere around the lower parts. So it's somewhere there (indicating).

This is at 20-meter segments beginning to nucleate. Those contours indicate liquid fraction contours. 40 milliseconds later you see a bubble developing and pushing upwards, and that induces motion also into that licuid upwards.

At 60 milliseconds later you see the bubble making it all the way up to the top, and you see the bubble hitting the top and wanting to rain back in. And at 140 milliseconds, in fact, you see the whole process like a circular thing coming in.

At some point along this process, this upward-moving film loses its momentum and reverses motion and starts slumping back down. And this one is coming back in. This is only pictorial to illustrate the gross fluid mechanics. We believe there are a lot of other instabilities present that will tend to destroy to some extent this general well-behaved pattern.

However, from the point of view of accumulation at the bottom here, this is the mass inventory. This is kilograms versus time. It is going down quickly during this process, and then it starts accumulating back in again and is shown back like that. We take the slope of this and that is 13 kilograms per second. That's how much mass accumulates at the bottom of this pool.

Multiply that times a factor to convert the kilograms per second into a velocity of puddling based upon the cross-section of the pool, and you end up with 34 centimeters per second times $100 per centimeter. That's $35 a second. So it is fairly qualifying for an energetic event.

MR. KASTENBERG: Theo, again what are the blanket elements doing at this point, the ones inside? Have they all melted also?

MR. THEOFANOUS: No. This is addressing the annular pool phase. So that is before the inner blanket assemblies have melted. If they were molten, then we would have a full core pool, a whole core pool. We are going to address this whole core pool directly when we come to the conclusions.

We have not done very detailed specific for the CRBR calculations. Well, there were some done earlier, but we believe that the likelihood of the whole core pool as you will see is extremely small.

On the other hand, if such a core pool were to take place, I think at this point we are all somewhat undecided as far as the energetic behavior. We think there is a possibility of having higher energetics there because it's more coherent and because it allows for this oscillation and sloshing that SIMMER predicts.

So from that point of view, we are going to take a penalty there in our probabilities. When we look at the whole core pool and look at the chances for that getting us into trouble, failing the system basically, we will assign a much greater number to that versus assigning a number for failure of the vessel for this kind of configuration.

MR. KASTENBERG: Is this ramp rate you have a maximum ramp rate?

MR. THEOFANOUS: Yes. As you see, it's the maximum ramp rate for this process here. And that is typical of the kinds of things we see. I think there are ways by which you can make that somewhat higher, but we have never seen anything more because of this reassembly, so to speak. We have never seen anything more than the kinds of numbers I showed in the previous one, which was about $100. And in fact, not even approaching that.

MR. KASTENBERG: Let me ask one more question. That ramp rate is based upon a flux distribution that was calculated statically; right?

MR. THEOFANOUS: Right.

MR. KASTENBERG: This is not a coupled neutronics?

MR. THEOFANOUS: No. That's right.

MR. KASTENBERG: So you just imposed this motion on those curves you showed us before?

MR. THEOFANOUS: That's right; except for those curves I showed you before were obtained for a configuration in which the two inner rings were slumping and we have all kinds of sets of graphs to do that because it is much easier and simpler to do this kind of calculation, and it's not meaningful to spend a lot of money doing the coupled.

MR. OKRENT: The concern that Los Alamos and Sandia may have expressed about the potential for high ramp rates during a transition phase, does this arise from this phenomena in a fully molten core?

MR. THEOFANOUS: Yes.

MR. OKRENT: That is the only concern?

MR. THEOFANOUS: Yes. And by the way, this concern is interesting to me. I think it is a valid concern. I don't think we have scrutinized it enough yet to know for sure how much of a real concern it is or should be. But having not done that, I think we should view it now as a real concern.

But it's not definite that if you have a whole core pool, in my opinion, you will have a very, very energetic situation. I don't think anyone has demonstrated that yet.

In summary then of this recriticality business, we feel that the gravity-driven recriticalities are important. High neutronic activities and pressures dominate the subassembly wall disruption period, and subassembly and annular pool recriticalities are bounded by $100 per second; in fact, bounded by well below $100 per second.

And I have one more section to go, and that is on the dispersal. We are scheduled to go to lunch at 1:00 o'clock. Mr. Carbon, what is your pleasure?

MR. CARBON: I am being lobbied to eat at this time. Would anyone prefer to continue?

MR. OKRENT: I prefer to eat.

MR. CARBON: Let's break until about 5 after.

(Whereupon, at 1:05 p.m., the subcommittee recessed, to reconvene at 2:05 p.m., this same day.)

AFTERNOON SESSION

(2:15 p.m.)

MR. THEOFANOUS: Well, we just completed looking into these paths, and the last unit of presentation will be looking at these green paths.

Dispersal. When one is concerned about dispersal, one needs to assess freezing mechanisms, one needs to know what areas are available for fuel to get out nonenergetically, and also, one needs to know what pressures are behind the fuel pushing it into all of the spaces that lead out. And we will try to look into these matters.

It appears that I have lost one vu-graph, and I don't see it here. I have an empty one with me. I am afraid I threw away the good one and kept this one. I will refer you to the Figure 3.E.2. That should be in the handouts that were given out today. This is one of the vu-graphs we made yesterday.

And what you see there is a process through which we have gone in order to benchmark SIMMER for the purpose of predicting fuel removal through rod bundle geometries and through gap geometries.

In the rod bundle geometries we have tests that have been developed or run at Argonne National Laboratory over a period of years, and those are called injection tests. A thermite anywhere from .5 to 2 kilograms was injected into a bundle geometry, and the penetration and plugging was measured. And these were done over a range of 273 to 1173 Kelvin clad temperature range with driving pressures anywhere from 25 bar to 26 bar. They, in general, gave penetrations of the order of one foot to one and a half feet.

What was shown on this slide was these five Argonne tests at different pressure levels, the SIMMER predictions on the data. And you can't see any real trends with pressure. I think the general idea from this is indeed if you inject under this kind of pressure thermite materials, and by inference, material that has been core disruptive in the actual reactor, it would tend to penetrate the bundle upwards of one foot.

The other thing to point out here is because the cladding material is entering the blanket area, the cladding of course melts, so that the actual space occupied by this 30 centimeters, say, of fuel material is more than what would have been occupied if this came out of the core.

And the main purpose of doing this benchmarking is because we wanted to use this data as a way, together with SIMMER, as a way of scoping out what kinds of penetration we would expect at lower pressures. So we are using the code as a tool, first benchmarking against that with some reasonable physics of the melting and freezing processes. We now take the same thing applied to low pressures.

MR. OKRENT: Is that a prediction by SIMMER without the benefit of adjustment to the data?

MR. THEOFANOUS: No, I wouldn't go as far as that. That is why instead of calling it prediction I call it benchmarking. We really look at that as a benchmarking procedure to allow us to go from this 25 bar. For example, you might ask if the driving pressure was only 10 bar would it be a significantly lower penetration. It's like a way of doing an extrapolation of the data. And similarly, for the purpose of following the fuel injection in between the subassembly wall gaps. Here when we compare the crust thickness versus time, this is the theory, this is what SIMMER would give, again as a way of benchmarking, to know that the code more or less gives us the right growth of the crust.

MR. CARBON: Do you have confidence the thermite data would resemble the actual core quite closely?

MR. THEOFANOUS: I think it is reasonably representative. I think how you do these thermite experiments really has an effect on how well it represents the fuel material. For example, in some recent tests they've done over there where there was a desire to let this melt come into the gap geometry, a melt that resulted from the thermite, there was a desire to let this come not under pressure but almost under gravity or low pressure.

In order to do that you must allow thermite for some time to let the gases that develop from the reaction to get out. As they did that, they found out there was also a stratification between the molybdenum and the EOC 3 present in thermite, and that tends to give an erroneous result.

On the other hand, the tests that represent injections of thermite material into rod bundles, they were under pressure. The pressures were provided by those gases. They were not left for any length of time. I think they stratified. And as a result of that, I would expect them to be quite reasonable.

Then we have not really completed this evaluation. This is probably the part of the story that is under active evaluation, and I would like to give you here what we have up to this point.

This use of SIMMER to really go back and extrapolate to low pressures has not been completed yet. As of last night I think we had only one point. And as you know also, this figure was missing from your vu-graph.

Here we tried to indicate to you that even if we took this 40 centimeters that we normally expect for penetration at high pressures and this 50 bar, for example, and if we took this and put on the top of it a conduction limited theory, obviously this is somewhat of a mismatch here because if this bundle was to freeze according to conduction limitation at 50 bar, you would have gotten much greater penetrations. But if you did that just for the purpose of exercise, and you backed out in terms of pressure -- in other words, if you rationed the trend according to the pressure to be that which is obtained from the conduction theory, you get this kind of behavior. And what you see here is even at something like 5 bar you have significant penetration in the rod levels.

MR. MARK: Theo, what is it that SIMMER actually calculates? Does it assume some pressure at the bottom of the fuel assembly, some fuel straight into the assembly, and then calculate the rate at which it moves upwards, or what does it calculate?

MR. THEOFANOUS: It calculates the motion as well as the freezing and plugging dynamics of the gap.

MR. MARK: Yes, but what are the starting assumptions?

MR. THEOFANOUS: The starting assumption is a configuration of the subassembly. That could be again for the freezing part of the calculations here. In order to fill up, for example, the points here with SIMMER, we start out from a certain mixture that would be representative of the core disruption. We expect a certain percent of fuel, a certain percent of fuel mixed up in the subassembly under a certain vertical pressure.

We will let this thing go through in this case the upper rod bundle here on the top of the blanket area, and we will move in, this material will move, and as it moves in it will interact with the cladding that surrounds the blankets. As it interacts with this cladding, initially there will be a tendency for the cladding to start growing a fuel crust, but because the blanket provides an insulator behind the cladding and because of the limited heat capacity of the cladding, so to speak, very quickly the cladding reaches melting point and will be melted and probably re-entrained.

So similar to these processes of cladding, heating, melting, re-entrainment and the temperatures of the steam -- that is, a multi-phase steamn -- as it goes down the path in this bundle until eventually -- and by the way, we also follow reasonably well, I think, the frictional characteristics of that, so that if there is a tendency for the slurry to want to slow down, it will slow down. SIMMER will allow those momentum characterizations to let it slow down, and if it wants to plug, it will stop it there. Otherwise, it will flow.

What you will see in the next slide will be a case like this where it goes, then it slows down because of the slurry formation, and it takes some time until the slurry in this case was pushed out, and then you see the flow really bouncing back up again.

MR. MARK: But it must, as a starting assumption, have something like a piston action at the bottom of the subassembly. And all of the confusion you were just describing to us are the state of affairs somewhere along these channels; that is, it has a 5 psi overpressure or a 50 psi overpressure or something at the bottom.

MR. THEOFANOUS: Right. It has some pressure.

MR. MARK: And then it discusses what happens along the channel while this cladding is evaporating.

MR. THEOFANOUS: Or melting.

MR. MARK: And moving itself along with the fuel also.

MR. THEOFANOUS: Right.

MR. MARK: And what sort of a sweeping action occurs.

MR. THEOFANOUS: Right, right. And from that -- in fact, applying this to this kind of a concept, this SPENSIS test from Argonne for this kind of conditions, it turned out by the time it reached 45 centimeters into the blanket, it was stopped, the flow was frozen and the process was stopped.

In some cases, however, it doesn't do that. It doesn't quite freeze forever. And that will be shown, I guess, in the slide beyond that.

First I want to talk about these pressures pushing this fuel and cladding. That is needed to give an idea of what is the general pressure state of this disrupted fuel-steel mixture.

There are numbers of ways of thinking of that. One of the ways is we will go from a SAS calculation that has gone through this core disruption, and you can look at the end of those power bursts I showed you before. And you say okay, I have 10 bar fuel pressure; this may be a result of such and such steel pressure. You can see the 10 bar, if you allow enough steel to mix, it might be quite a bit higher pressure.

Depending upon what you do with that mixture, depending on the substance you use at that point, this pressure will decay faster or slower depending upon the heat transfer mechanisms. And, again, one would like here to scope out the range of possible phenomenology, the range of what can possibly be expected there.

If it was only fuel vapor, if the steel was not allowed to be heated by the fuel and there was some nominal heat loss only to the walls of the can, you would find a relatively small decay of pressure. That means for a period of 1 to 2 seconds the pressure remains above 5 bar levels.

If, on the other hand, you put in the cladding that was likely to be around and you allow the heat transfer or the thermal contact with the fuel, and if you did that in a best estimate kind of way, what you considered to be a nominal wall entrainment -- that means the wall begins to melt slowly -- and if that comes in in a nominal way, the pressures decay but not very rapidly.

On the other hand, if you consider some extreme ranges -- for example, as soon as any fracture of the wall of the subassembly is melted and came in instantaneously, as soon as it melted came into the subassembly and equilibrated thermally instantaneously whatever was inside, obviously this would be an extreme of extreme heat loss and would be characterized by more rapid pressure decay.

But even in this case you see that the pressures are high, and they decay over a time of a second in order to give you an idea of the time scale of the pressure of the drop, again, dropping in time.

Using this perspective of pressures we are showing the rate at which fuel is escaping the core in between the gaps, in between the gaps that exist, in between the subassembly can walls. And because a driver assembly is expected as it heats and is trying to disrupt that it will balance, we do not allow any fuel escape paths from one driver to the next or from one driver even to the next blanket, because we feel that maybe the wall of the driver pushes up against the blanket and therefore allows no gaps. We allow for fuel escape in gaps between blanket and blanket, internal blankets.

So the course here is that this is a driver. Right in association with that the driver has the fuel boiling up and steel interacting and producing temperatures and pressures and gradually melt attacking the wall.

At some point then this corner will become molten or will crack open. At this point then this gap will become available for whatever is inside here to come out and come down, all of the way down to the lower axial blanket. This process then of the flow through this gap, which is typically 5 millimeters in gap here, was simulated on the SIMMER with a back-driving pressure of 3.5 bar and with a wall temperature of 860 degrees K.

And here is the mass into this lower portion versus time. It makes a very big difference if the fuel that enters, if the mixture that enters is just at the melting point or has some higher degree of superheat. In fact, we expect because of the neutronic activity that it will not be just at the melting point but will have some superheat with it.

What you see, however, here is the flow is starting out at about 13 kilograms per second per gap and stays like that for some time, almost like a second, and then it is slowing down. The reason for this slowing down is because there is some slurry formation in this flow, and therefore there's a slowing down. It takes almost three seconds for the slurry to be pushed out again. At that point you are left with basically a liquid flow in a gap that is now wider because you melted some, so the flow shoots back up again at very high rates.

However, if the fuel was to enter this gap at the melting temperature, you will see that it scales down to zero quite a bit faster than in this case. So if we use this value then of 13 kilograms per second per gap times 90 gaps that are available between blanket to blanket combinations like this, we can remove 1200 kilograms of fuel-steel mixtures in one second, and that involves about 20 percent of the core. And there's just about enough time of this down in the lower part of the blanket to accommodate this kind of number.

On the other hand, if the material would also spread out readily, of course now we have much more space to accommodate much larger quantities.

MR. KASTENBERG: Theo, do these gaps close with radiation?

MR. THEOFANOUS: There will be some closure, that is true, and we are also evaluating that. I think, however, closure will be a small fraction of that. If you want more specific numbers, maybe I can ask.

Does anyone know by how much those gaps might be closed with a fully radiated core?

Charlie, do you want to?

MR. BELL: I believe that near the midplane the swelling is highest and then tapers off toward either end. So near the midplane, if I remember right, they are approximately half closed, and then as you go axially up and down, particularly down, they open up to something very much closer to their nominal size. So they might be constricted near the midplane, but since you can open into those gaps anywhere along the axial height of the core, that doesn't necessarily represent a complete constriction.

MR. THECFANOUS: I think from that I want to leave you with the impression that there are pressures, and typically there are pressures higher than 3.5 bars. This was a modest volume of pressure. There are paths both axially as well as radially into the gaps, and there are removals of these order on a matter of one or two seconds. And, therefore, we conclude -- and again, I emphasize we are in the middle of these evaluations -- that both area and pressures are available and that we expect that a fuel mass in excess of 40 percent of the fuel inventory will be removed in the time frame of the first four seconds.

And we believe that this is important, and that is what allows us to consider that, number one, any recriticality situation most likely would involve less than full core inventory and certainly less than this one or two percent removal required to avoid the extra kicker I was saying before point number one and point number two. That determination would occur earlier rather than later in the disruption phase, therefore again with a lower likelihood of getting into a whole core pool.

This is an area in plotting and freezing mechanics. It is an area where we could stand more understanding, a little more improvement in our understanding. And there are some important tests currently in progress. Argonne National Laboratory is running some gap tests, injection of thermite into gaps, and also some tests are run at Sandia with annular geometries. And we expect both of those tests will even further help us justify this conclusion.

Are there any questions on this Unit E?

MR. CARBON: Yes. I think there has been concern at times in the past that you could get some kind of molten pool below the core somewhere with a crust around it, and this might go through a series of oscillations given perhaps some recriticality.

Is that covered here? Is that still a question?

MR. THEOFANOUS: Below the core or within the original core configuration?

MR. CARBON: I thought below the core but maybe not.

MR. THEOFANOUS: I would say that the probability of getting into a significant inventory --

and from the point of view of recriticality again, that's 60 percent of your core -- all of this molten and eating its way through the lower big structures, I would consider that to be extremely limited. I think that energetically you wouldn't be able to do that.

I believe that if you had 60 percent of the core in this configuration, it would either have to terminate one way or the other, but it couldn't last very long. And that very long, I would like to put a time frame to that, maybe something like 10 to 20 seconds. It couldn't last any longer than that. And I think this time frame is short compared to what it would take to melt significantly below and move to the lower area. So that is the reason we confine our attention of termination one way or another within the original core confine, because we believe that determination will go from there.

Any other questions?

(No response.)

All right. Now we come to the punchline; that is, our conclusions, one slide of conclusions. Basically the same figure I showed before with numbers showing on each one of these branches.

Again, I would like to reiterate that this has to be taken together with our definition of probabilities as given in Figure 3.3. And also, I would like to reiterate that what we consider to be incredible and thus a probability of 1 in 1000 was done for the purposes of dealing with numbers with such small magnitude considering their involvement here, as well as the desire to stay internally consistent.

So if we have 1 in 10, 1 in 100, we wanted this product to give us the 1 in 1000 which we will classify qualitatively as an incredible situation. That is the reason we call this whole thing a qualitative probabilistic result. We don't even pretend this to be a full-blown PRA. We think that in fact it may be somewhat premature at this point to come in and have a whole distribution of those parts or even more detailed parts than that.

On the other hand, we also feel quite strongly that it is important that we make an effort, make an attempt to quantify our thinking, our qualitative thinking that resulted from all of the previous five sections in terms of numbers. So we can see in order to come up here, taking into account the likelihood of getting like this as well as the likelihood of going from here to here. That is the purpose here. That is the intent.

I would like to work through with you if you like and explain to you how we assign the different numbers and why. First of all -- and again, this is focusing on the loss of flow accident -- first of all, looking at the initial disruption of the core, we believe that there is a significant potential for direct disassembly.

In order to obtain this potential in a way that's truly a disassembly -- that is, giving more than $30 to $40 per second -- we feel we must make some end-of-spectrum assumptions. It doesn't come, in other words, every time one would do a loss of flow calculation one will end up with disassembly. In order to reflect that then, we have put this 1 in 10, which represents end-of-spectrum assumptions, but within the range of expected behavior.

On the other hand, we feel that we have bounded the energetics that may result from that reasonably well. We have tried to do almost as much as we could to augment and obtain a perspective on the upper boundaries. Because we have done that and because we see this to be something on the order of $30 to $50 per second instead of $100 to $150 per second, we feel we would like to put this probability of failing the vessel from this event as 1 in 100, 1 in 100 meaning we would require out-of-spectrum conditions to achieve that.

Now, you see, I am pointing out 1 in 100. I am pointing to 3 in 100. The reason for that 3 is not such fine-tuning or that we're cutting it short, but to show you or remind me to tell you we have still a question for that -- that is, the potential of LCF-driven TOP. I mentioned that to you before. Because we find there's about 25 percent of the core that has still sodium, and because that part of the core has molten fuel, we would like to analyze that more carefully before we can say it is truly out-of-spectrum to fail the vessel through this path. And to do that we have to take into account the recent interpretations, W-2, as well as other sensitivity studies, to be able to say that also we have adequately bound this LCF-driven TOP.

However, I do want to point out that the LCF-driven TOP behavior itself being a rather uncertain phenomenon, and this aggravated fuel compaction situation we are doing over here being itself somewhat out of spectrum is almost like compounding the level of pessimism, if you wish, so we have to look at that. It's not only marginal, but it is also pushing the limits of realism. Nevertheless, we are really going to take a good look at that, and for this purpose we allow this not to be quite out of spectrum but somewhat a little less than that.

Now, this is 1 in 10. We also believe it is rather unlikely that we obtain final dispersal and complete elimination from the initial disruption phase by going basically the only place available at this point would be through the upper and lower blankets. And again, the reason for this is because we have experiments that show that these blankets will block sooner or later, and at most we have penetrations of about a foot.

So because of this we don't feel we can remove 40 percent of the core in this initial stage like that. Therefore, again we put an out-of-ordinary, end-of-spectrum kind of condition. So the difference between those two and one will lead us to -- and that will be the majority of cases leading us to a subassembly-scale pool.

Now, at this point we need to remember the high neutronic activity. We need to remember core disruption. Although we have removed some the fuel out here -- we are quite sure about that -- this stage here is highly incoherent. It has a lot of pressures up and down. Maybe it shows a little tuning, but there's a limited amount of timing available for those motions to become coherent.

We feel there is a potential for recriticality. We have discussed that before. We think again it is not quite out of spectrum, but also it is not really something that is to be expected out of the time. And again, we give it the number of 1 in 10 here. However, at this point we are seeing additional removal, basically the same process that is moving you from here to here (Indicating). That same process is the one that opens the gaps and moves you from here to here. So the same process that generates the annular pool, that very same process also opens up the paths and allows for fuel removal.

Now, if you like to roughly estimate that looking at the previous numbers the loss may be even 5 or 10 percent up here, all you need to lose is another 20 percent to come up with this 30 percent value that I said is more appropriate for the initial stages, and you have a termination to this point, to this point, although we've not completed our evaluation of this because we still have to do the low pressure SIMMER calculations.

We feel we want to show a more realistic path going out this way with a more heavy bias rather than either of those two ways. So that's the reason there for one-tenth, one-tenth and eight-tenths.

Furthermore, because we feel we have bounded reasonably the recriticalities by doing these arbitrarily coherent fallbacks of all of the subassemblies at maximum speed under gravity and still being well within the design margin, we assigned to this branch over here a probability that is out of spectrum, not quite incredible but out of spectrum, 1 in 100.

Now, we move into the annular pool. This would be another here. And from the annular pool, again end-of-spectrum conditions, but not quite out to get us into disassembly. Again, at this point that is another two, three or four seconds, whatever it takes to really begin to attack and disrupt the internal blankets -- again, more time for fuel removal.

So another way of looking at that is if you lost 10 percent here, 10 percent here and 10 percent here, and if based upon the numbers I said before it takes one second to lose 30 percent, you see, you already have some numbers from margin there. You can come out at this point and have lost essentially 30 percent. So because of this again we put more bias to the dispersal rather than the disassembly or going into the final stage which is the whole pool.

This again is one in ten. Again we feel we have bounded, from this annular pool configuration we have reasonably bounded the expected criticalities, and therefore, based upon what we are seeing today, we cannot exclude and we cannot say that the vessel failure would be incredible, postulating a disassembly from the annular pool.

But based upon our understanding of the structural capability, as well as the trends we see with the recriticalities, we do not expect anything within the spectrum of conditions that are realistic. Therefore, it is out of spectrum, again one in 100.

And finally, if we did not obtain termination at this stage, we would end up with a whole core pool. There are a lot of things that one can say here and I don't think it is probably worth it to take the time. It takes time to get the blankets in and mixed up. The blankets dilute. There are a lot of factors, and throughout this process still you would be losing fuel.

So before the whole core pool became coherent and became fluid enough to obtain the kind of sloshing behavior that is pretty coherent, the kind of thing you see with SIMMER, still you will be losing fuel, and already we have lost quite a bit. I think one should weigh that very heavily in judging this whole structure here.

But in any case, having postulated it, until we get there, and recognizing some of the uncertainty because this pool is a very big pool with very strange materials and we really don't have a lot of direct information about those things, based again on some of the recent SIMMER calculations, we feel that is the only place we would like to take an even chance of going from here to going over there, which is almost like an equivalent to joining this thing straight like this and straight like that.

And furthermore, again because of the highly coherent state of this whole core pool as compared to the annular or previous stages, we also give it a little higher weight here. We don't think it takes such an incredible set of assumptions to postulate to end up with the vessel failure from this stage over here. That is why from this one over here we put one in ten. That is in the spectrum, but not quite out of spectrum.

Based upon these kinds of logic, then, and these numbers, what we have to do at this point is somehow going through all of those paths. The bottom line is the vessel failure probability from a loss of flow accident is 4 in 1,000. The contribution of the different paths to that is primarily from this. This is the one that gives us 3 in 1,000 and all of the rest of them together give is one in 1,000.

I think when we qualify it better, this LOF-driven TOP, this will become one in 100 as it should be and the whole thing might be more like 20 in 1,000. The general idea is that somewhere now at this moment in our thinking we are somewhere between incredible as far as considering this stage and out of spectrum.

And with that I think I will close this part of the discussion. I would like to make a couple of remarks concerning this vugraph. This as well as the document that accompanied the vugraphs we saw in this meeting were put together really kind of running against the clock for the past two weeks. We lost the difference between day and night and evening and morning. And therefore, because of this hectic schedule -- and we only completed it just the other day, as you know -- at the time you received it in the mail we did not have the opportunity yet to have all of the consultants review and all the members of the team to review each number in great detail.

What I think it is fair to say is, Charlie Bell, with whom we integrated all of this information provided from all the different tasks -- we put these numbers together, the two of us, and I think it is also fair to say that the group at Los Alamos, the whole group at Los Alamos, had an opportunity to look at those numbers somewhat peripherally, because they were running after completing some of their tasks. But I think they had a look at those numbers, more, let's say, than some of our consultants at Sandia or Brookhaven or some of the universities.

But what we intend at this point to do here is to communicate all of this information to the whole consulting team, and we are going to ask them for their inputs, their criticisms, suggestions, or what have you, again as an input to our beginnings of putting together the report.

Also, I want to say that this is, as I mentioned already, not completely resolved, not completely finished. We are still maybe in the trailing edges of this evaluation, but still we are tightening loose ends, especially in the areas of LCF-driven TOP and in the area of getting this kind of dispersal. We have to do some more things with plugging, freezing, and the timing of those processes.

So this represents the best way we could put together our best judgment of how we see things together, and we fully expect this will be the case. Also, a few months later when you have the report and you have everything to be fully documented, I think if you ask me, if anything, you will see this number going back and these numbers over here going up.

In fact, it is interesting because about three or four weeks ago we put together a similar chart for the NRC Staff because they had to us some other questions and they wanted to have from us our thinking at that time. And at that time these numbers were here, almost an order of magnitude bigger, and those numbers were an order of magnitude lower. So you see, the tendency is for these things to become smaller and those things to become bigger as the time goes on.

With that I would like to ask you if you have any questions on this, first.

MR. OKRENT: I assume everything you've presented is up for discussion now, or is there still another presentation?

MR. THEOFANOUS: I think what I would like to suggest is, first of all see if we have any questions with respect to that. Secondly, I would like to open the discussion with respect to everything I have said up to now. And following that I would like to go back to your request to go back to the other members of the team and having them stand up and give some of their feelings.

So at this point I'm asking you, are there any questions on this slide.

MR. CARBON: I'm not clear yet on where you got the 4,000.

MR. THEOFANOUS: The 4,000 comes from multiplying all the arrows, this times that, plus this times this times this, plus this times this times this times this, plus this, this, this, this, this, all of these summed up. And the major contributor is over here (Indicating).

MR. LIPINSKI: The one arrow leaving disassembly into in-vessel containment is not labeled, but I assume that's 85 and 100?

MR. THEOFANOUS: That's right. That would be the difference.

MR. KASTENBERG: I want to make a comment on that vugraph. Something you said this morning bothers me a little bit.

MR. THEOFANOUS: All right.

MR. KASTENBERG: I guess first let me preface that by the following comment. If I truly wanted that to be qualitative, if I were doing it, I guess I would have put category A for what you have one in ten, category B, category C, category D, and I would have left it at that. And the reason is because of this concern with something you said this morning. You said at some time in the future when someone does a PRA you would be calculating some of the probabilities and frequencies for component failures and so on and so forth, and then he could combine it with some numbers and come out with some risks. And to me --

MR. THEOFANOUS: That would be wrong.

MR. KASTENBERG: That would be wrong. You would be mixing apples and oranges.

MR. THEOFANOUS: Right, right. And that is exactly why we highlight this here, and that is why I gave this extensive qualification this morning about the importance of not mixing things up like that. But Bill, if you were to do that then you would have to tell me how you were going to multiple category 1 times category 2 and what would be your result.

MR. KASTENBERG: That's the point. I wouldn't.

MR. THEOFANOUS: You couldn't do it. No, no, I'm talking about, you put here a category 1, here a category 2; now what is the bottom line here? Is it category 1 times category 2? What does it mean?

We feel very much like you do, but we had to come to grips with giving you a bottom line here. If we gave 1's, 2's and 3's here all along, that is the equivalent to only making qualitative verbal statements. But you can't multiply words.

We thought then we would like to take that as a first step in this process of quantifying it, trying to be very cautious and putting all of the red flags in that we could. At the same time, we needed something we could multiply, and that is the reason for defining very carefully the levels of probabilities and then trying to make them at least internally consistent, so one in 10 times one in 100 is one in 1,000. And what this represents is an outside of spectrum situation times a situation obtained at the end of spectrum, and that is our definition of very highly unlikely or incredible. And if you don't do that you really cannot come down with a bottom line here.

MR. KASTENBERG: Theo, I'm not convinced that an end of spectrum times an end of spectrum gives me something incredible.

MR. THEOFANOUS: I didn't say end of spectrum times end of spectrum. I said end of spectrum times outside of spectrum. One in 100 is outside of spectrum, something you can't see how you can get, and one in 10 is an end of spectrum.

Now, the two together, if they have to happen one after the other, it seems to me it should be something more than end of spectrum. And still you have to have a way of conveying that information. Now, as I talked with you, I'm sure we wouldn't have any problem even to go through and say, category A times category 2 will give us a category 2-prime or whatever.

But when you try to convey this information to some other people who are not really living with those things as much as we have, you have a problem unless you can actually come up with a bottom line, and that is an effort in that direction. It is not really an easy problem, Bill. It is really very difficult to do.

MR. LIPINSKI: What is the probability per year for your LOFA, the top event?

MR. THEOFANOUS: For that I think you will have to ask somebody else, like Mr. Morris back there.

MR. LIPINSKI: I assume the initiator --

MR. THEOFANOUS: It's a very low possibility.

MR. LIPINSKI: I assume it's an ATWS.

MR. THEOFANOUS: It's a very low probability, right.

MR. LIPINSKI: So given what was a numerical design for a goal for the shutdown systems, you then have that as an opportunity to say it's an applicable number, plus the challenging events.

MR. THEOFANOUS: That's right, sure. In fact, it turns out, I don't even know if it's wise to say that now, but if you took these numbers times those numbers here maybe you are not very far off.

MR. LIPINSKI: You are moving the decimal over two more places, three more places.

MR. THEOFANOUS: If it was easy to do we would have done it.

MR. LIPINSKI: The question is, can you get someone to believe you.

MR. OKRENT: Since you have put numbers on, I have to ask you what the uncertainties are in the numbers in your opinion. You came up with three or four in 1,000 or whatever it is.

MR. THEOFANOUS: I tried to say that this morning, and only to convey, to truly convey our feelings concerning the uncertainties, we should have given you in each one of those, instead of being just frequencies, we should give you whole distributions. We feel, however, that we are not able to do that now. We don't know enough to do that now in any reasonable way. And however, also we feel that should not be an impediment for us trying to give you an end of spectrum kind of situation.

The way we look at these numbers is high confidence, high confidence level numbers for those occurrences. I can't very well give you an uncertainty around this already, end of spectrum. The way to interpret these numbers is, our best judgment is an upper limit of those cases that can give us that. What that is is, again our best judgment, the upper limit of those cases that can give us that (Indicating).

MR. OKRENT: Are you saying 4 in 1,000 is a 99 percent confidence level?

MR. THEOFANOUS: Something like that, yes, a very high level of confidence. Now, whether it's 99 or 95, Dave, I don't know.

MR. OKRENT: I'm just taking your words. You said it's a very high level of confidence.

MR. THEOFANOUS: I think the important point -- and I think it's an important question you are asking -- is those are not best estimates. We do not estimate these that 50 percent of the time that will be coming this way (Indicating). We intend it to be an upper limit of the frequency.

MR. OKRENT: You didn't say 50 percent; you said 10 percent.

MR. THEOFANOUS: I say that is why it is one-tenth. If I wanted to give you a best estimate --

MR. OKRENT: No. My question is, is the one-tenth intended to be a best estimate or an upper limit in your opinion? From what you have just said now, I really don't know.

MR. THEOFANOUS: I was trying to explain a concept, but I don't think I got through. Let me try it again.

In order to really give you my feeling of uncertainty on this number, instead of giving you a number I should be giving you a probability distribution. Now, I am not doing that because I don't think I know enough how to do that. If I were going to give you a best estimate for taking this path, I would try to hit that probability distribution under the maximum, and that would give you that frequency. I don't want to do that also, because then you will ask me what is the breadth.

Rather, what I would like to do is take the end limit of the distribution and give you this number, this frequency, as representing the end of spectrum of the probability distribution. So that is a high confidence level number for the frequency of those events. Is that clear?

MR. OKRENT: No.

MR. LIPINSKI: 95 percent confidence.

MR. THEOFANOUS: Is the limit of the probability distribution. If this was going to be a best estimate number, it should be at the 50 percent level. I am putting it out to a very high level so that I can make it an end of spectrum, as I want to. If I had given you a best estimate, you see --

MR. OKRENT: I'm sorry. My understanding is, because it is end of spectrum you assign it one-tenth. Am I correct?

MR. THEOFANOUS: That is right.

MR. OKRENT: Then it is best estimate end of spectrum; is that what you are saying?

MR. THEOFANOUS: Right, right.

MR. OKRENT: It's not -- all right, let me leave it at that.

MR. THEOFANOUS: If that is what you think best estimate end of spectrum --

MR. OKRENT: Okay. So the one-tenth is your best estimate of the value?

MR. THEOFANOUS: Of the end of spectrum. That is important.

MR. OKRENT: But you said it's end of spectrum.

MR. THEOFANOUS: Yes.

MR. OKRENT: Okay. A small point. If you have dispersal, but a limited amount, I thought you said it is possible you can get blockage. Is that right?

MR. THEOFANOUS: Yes.

MR. OKRENT: But you don't expect to have blockage over the whole flow region from which more dispersal could occur, is that it?

MR. THEOFANOUS: No. It is possible to have that, and in fact that is why this number is so small. If we felt we could have dispersal that is monotonic and going out both exits of the core, this number would be a very big number.

It is small because we expect some fuel will get into the upper and lower blanket regions. However, it is going to plug there. It is going to occupy somewhere maybe between 10 and 30 centimeters. That means a significant fraction of the core is being removed. But it is not now -- the core is not unblocked. It is blocked at this point. So from then on we have to look at other paths for getting fuel out.

I don't think I want to discount the possibility that because of the co-disruption for those blockages to be in fact reheatable, because they contain fuel and as you go through these power pulses you might get a remelting of those blockages. However, we feel that this is again in the periphery of uncertainty and we don't really want to count on that very heavily. And that is why from here on we take into account only intra

-- inter, blanket to blanket depths.

But in fact, it's very likely we will have a remelting and a reopening of those spots and additional fuel coming out from there. The interesting part is also, if those blockages were to become remolten blanket material would be coming into the core with them and would provide further dilution and would make the criticality even more difficult to achieve, and that is also important.

MR. OKRENT: That seems to me to be a sort of slow thing. But I don't want to get into it.

MR. THEOFANOUS: A slow what?

MR. OKRENT: The rate at which the plug material leads to melting of the blanket material. Based upon all of the time periods you have been talking about, that strikes me as being something slow.

MR. THEOFANOUS: It's relatively slow. That's why you're not counting for it. But slow in this case depends on what is the power level. If you're going to have a high power pulse, it will accelerate. If you let it at decay heat levels, it may take 30 seconds, for all I know 40.

MR. OKRENT: I am going to have to cut out about 20 to 4:00, and some time before then if there are comments that other participants who are here wish to make, I would like to hear them in that time period. Is that --

MR. THEOFANOUS: I was planning to do that. Are there more questions on this one before I take it off?

(No response.)

All right. If there are no more questions on that, I would like to call for any questions of a more general nature over the whole presentation today first.

MR. OKRENT: I have questions that relate to the accident you didn't cover. Now, is that a part of today or is that not part of today?

MR. THEOFANOUS: I think accidents we did not cover, we will be happy to hear the questions because we are covering them now. And maybe if you want substantial answers for those, you might have to wait until we meet again, where we finish that part.

MR. OKRENT: On a general question, which, if any, do you think may be significant?

MR. THEOFANOUS: From those other ones?

MR. OKRENT: (Nods affirmatively.)

MR. THEOFANOUS: I like those leading questions.

My personal bias -- and here I guess our personal biases do come into the picture -- my personal bias says that the loss of flow accident really should be possible to envelope energetically everything. On the other hand, I think we are looking very carefully at the earthquake, beyond safe shutdown earthquake. But what core variations can that introduce?

And we are also looking carefully at the post-accident heat removal structural failures, because of the high temperature creep, again because you don't know what kind of situations we can get into, because these cases have not been looked at in any great detail up to now.

Therefore, if you ask me, those are the main areas I am looking for, if anything, for some unexpected behavior. But I doubt I will find one, but that is where I am looking.

On the transient overpower, at this point the state of development is that the project has promised as one of the action items to give us the end of cycle free core neutronic data and as of today we have not received it yet. We had a telephone call from Fauske & Associates three weeks ago when I was out at Los Alamos trying to wrestle with all of those things, and we have heard that the data is in the mail and we have not received it.

The reason we are interested in this data is because we want to validate from the point of view of TCP, we want to evaluate as coherent a core as possible, and that USE-3 data and USE-3 behavior would be the most coherent of the cores. We have done, however, evaluations in between by taking other neutronic data and putting in coherencies that we expect for the USE-3. But we should have the data to address that.

So from that point of view, that is one item we are expecting to obtain for transient overpower. And when we have that, as soon as we receive it, we will be ready to get on and do the analysis.

And in addition to that, the other element that is needed is the initiating driving ramp, and for that one we have a special task and we have people talking to the instrumentation people of Westinghouse to get us with a good bound on what can be expected. The project position is 10 cents per second is the maximum, and if you believe that it doesn't appear there's a big problem there. But we would like to confirm this.

So those are some of the ongoing activities.

MR. MARK: Could I ask -- and I am afraid you will be hideously scornful of my question -- is it then correct for me to be assuming that you have studied and concluded that the time scales for sodium to get away from the reactor and realize the sodium void coefficient, for other things such as that to happen, for fuel to accumulate, those time scales are at certain ranges which you have attempted to explicate. But the ramp rates that may result from whatever might go on are limited; that the energy which is available for high number work is consequently limited; and that a total disruption of the core so far as the in-vessel features are concerned is not threatening, and the HECDA may be viewed somewhat complacently because of those lines of argument, which you have worked through and tried to pin down.

Is that at all a summary of the position you have been bringing us?

MR. THEOFANOUS: This is a very excellent summary of my whole presentation.

MR. MARK: This does not discuss, then, what might happen if the fuel is at high temperature and begins to burn through the bottom of the pot.

MR. THEOFANOUS: Right.

MR. MARK: That is separate. But it does say that anything resembling an explosive reaction, something that will lift the lid sort of to the roof, is not really sensibly to be considered?

MR. THEOFANOUS: This is a very good way to put it.

MR. MARK: Thank you.

MR. OKRENT: Could I ask, has anyone made a list of the circumstances which, if they could occur physically, would be sufficient to lead to an early failure of the inner containment?

MR. THEOFANOUS: Of the containment, now, or of the vessel?

MR. OKRENT: I said the inner.

MR. THEOFANOUS: The primary?

MR. OKRENT: Yes.

MR. THEOFANOUS: That is what we have attempted to do here.

MR. OKRENT: No, I'm sorry. That's somewhat different. What you have done here is taken a certain initiating event and tried to analyze it through with some branch points. The question I was asking was whether someone has tried to say what the, I will say, physically possible situations are that could in fact jeopardize this containment if they could occur?

MR. THEOFANOUS: Oh, I think you can find a whole list of that over the literature over the years. We have nothing, yes, but one more hypothetical situation that someone postulates.

The important point, Dave, is do you want to disjoin yourself from all physical reality and say, I have all of this fuel now, what can I imagine this fuel doing to give me a large energetic? Or do you want to start from someplace that physically makes sense and take it all the way through, not one path but all possible paths that you see at every point of your way, and you see what kind of a physical reality you are confronted with?

I think what we have done here is exactly that, and we believe it is reasonably complete for the loss of flow accident, that is, given an initiator or failure to scram with loss of pumping power. And we intend to do the same thing for TOP, not to the same level of detail, but TOP, seismic, loss of heat sink, and all those other things.

MR. OKRENT: Well, your answer or your current tentative conclusion may be in the end generally accepted or even valid.

(Laughter.)

MR. THEOFANOUS: I like that.

MR. OKRENT: However, I am trying to ascertain something different. A moment ago you mentioned, maybe in the loss of heat sink postulated accident you might lead to a condition where the vessel itself begins straining too much, as I recall. What is your concern in that sequence?

MR. THEOFANOUS: My concern is not only necessarily from the point of view of the vessel, direct vessel failure, but I'm also interested in what happens to the coolant in the core during that time. And again, there has been time in the past, again, that people have looked at loss of heat sink accidents and they said we could obtain recriticalities and they were looking at these recriticalities in a framework postulating everything intact.

I am concerned that perhaps the whole framework in which this analysis was done is wrong. I think we have to follow -- it's very important to follow it from someplace, and you follow it reasonably realistically to see what is the real situation we are confronted with. I think it's very dangerous if you ad hoc a lot and take out, and not necessarily conservative. You pull out a case and say, I will examine the energetic potential of this case.

MR. OKRENT: Well, again let me pursue the matter a little bit. Are you able to rule out that an energetic disassembly could threaten the early failure of the inner containment if there is no sodium above the core at all?

MR. THEOFANOUS: I answered you that question. If there's no sodium to focus the energy, I expect for a given ramp rate to have less of a consequence than we had.

MR. OKRENT: Less I understand.

MR. THEOFANOUS: Less than 100 dollars per second. And again, I am saying what is the potentiality for actually this event happening that you are postulating? It will depend upon what is following a loss of heat sink accident. If that whole vessel was going to start flowing under gravity, it would be one thing. If the vessel will be setting there and the sodium boiling off, that will be another thing.

The proper way to look at it is to see what the different structural components are going to do as they are slowly heating. That again was not the topic of my discussion today. We have another task that is this specifically, and an emphasis looking at this phenomenon.

MR. OKRENT: Well, let me say why I asked the question, and I was not suggesting that you look at all possible physical phenomena. Those were your words, not mine. Sometimes if you can envisage a situation which is troublesome and you don't know how initially it might arise, when you set your mind to it you see ways in which it might arise.

Let me give you an example from the light water reactor area. If people ten years ago had said to themselves, if I got into a situation where I had an irradiated vessel and if I subjected it to a sharp transient in which it was rapidly cooled and if I then repressurized it, I could possibly lead to rupture of the vessel --

MR. THEOFANOUS: Right.

MR. OKRENT: At the time they would have had no obvious candidates for this event. Maybe if they sat and thought about it for a while -- I am talking about ten years ago.

MR. THEOFANOUS: I don't think -- they would have no candidates for that. They are designed for that, not necessarily all of the transients we are looking at now, but it's my understanding all the vessels are designed to take a certain thermal shock transient.

MR. OKRENT: They had some design basis transients that were in the FSAR, indeed.

MR. THEOFANOUS: Sure.

MR. OKRENT: Which in fact did not produce severe conditions. And my point is, if one then had said, given a sufficiently severe transient with a repressurization, this might lead to failure, one might in fact have said, gee, I don't have any candidates then, but it might have triggered somebody's thinking and he might have arrived earlier than let's say the community did to the general feeling that there were some transients that at least could reach that category possibly.

What I am getting at is, it can be helpful to understand situations that would give you trouble if they could occur, even if at the moment you don't see the transient yourself that gets you there. Now, that is different than saying, I am going to postulate, as people did back in the Enrico Fermi reactor days, I will assume the core is somehow being moved together so you get 100 dollars per second or even 1,000 dollars per second or whatever. That is not the same thing.

MR. THEOFANOUS: I understand.

MR. OKRENT: Okay?

MR. THEOFANOUS: Yes, I understand the thrust of your question, Dave, and let me respond in the following fashion. I believe -- and I think again within our team we have had extensive discussions, and I think we are pretty much in tune here -- that one should be mindful of variation of behavior. I think if there is one group of behavior very mindful of all kinds of strange possibilities happening, this is the group of people you are looking at.

Not only ourselves, but a lot of people in this business have looked at that over a long period of time. So we don't want to set with one path, we don't want to pursue from the point of view of worrying only about one eventuality. However, I want to point out a number of things.

First of all, the situation as far as the LMFBR core-disruptive accidents is, I think, from the point of view of assessing the general spectrum of conditions that can get you into that, I think is much easier to handle from the point of view of actual analysis and looking at that than water reactors simply on the basis of the kinds of transients that have the potential of getting you into the core-disruption in LMFBR versus the very wide variety of things, some of them which can be going on for hours having to do with ECC coming on, coming off, pumps coming on and off and the pump sitting there and the operator responding to that.

So if you would like to think of it in terms of a set of conditions, the way I see it it is almost like a very big set of conditions in water reactors that may involve a number of different things that can get you in a bad situation as opposed to the LMFBR. You really have to have a gross power to cooling mismatch.

MR. OKRENT: I will stop you there. You can postulate a loss-of-coolant accident, but you have to rupture two vessels.

MR. THEOFANOUS: That's right. So within limit, within reason, as you said before, you didn't want to take the Fermi reactor jumping up and down. I don't think you want to take rupturing three vessel walls one after the other as well as the cavity.

MR. OKRENT: You were talking about an earthquake more severe than design basis a moment ago that might be a candidate, I don't know.

MR. THEOFANOUS: We have to look at that, and we also have to look at some reasonable probability limits for this.

MR. OKRENT: The kind of question I am curious about, and the answer may be very straightforward, if you got into a situation I will call a slow heatup --

MR. THEOFANOUS: Yes.

MR. OKRENT: -- for example.

MR. THEOFANOUS: For example.

MR. OKRENT: For example, you lose the coolant above the nozzles and then you slowly -- you are shut down but you boil off.

MR. THEOFANOUS: So it's a loss of heat sink situation.

MR. OKRENT: A loss of heat sink accompanied by a slow loss of sodium so that the upper part is heated.

MR. THEOFANOUS: Yes.

MR. OKRENT: Then you were to get some kind of reactivity excursion with no sodium or no significant sodium above. Maybe some little bit of sodium around. Could things above be weakened significantly so that this lesser force, because you don't have a sodium slug, still be enough to threaten integrity? I have no reason to think it would be, but I don't know if anyone has looked at it.

MR. THEOFANOUS: Again, I am coming back saying, we are looking at that as a part of our I.4 task. We are looking exactly at this set of phenomena. Loss of heat sink, uncontrolled and unprotected. It carries on. We will see how the core might be responding. It could very well be that as part of the general spectrum of conditions we will consider reasonable for that will be one like this where you boil off the sodium and then at the time you have to worry about it there's no sodium there.

If that's within reason, if we can claim that situation to be within reason, we will do the calculation and tell you. If you like, we can even do the calculation nevertheless and tell you next time what would happen. It's a simple thing to do. It is a simple calculation.

MR. MARK: I wonder if I could come back to the thing I was really fishing for before. You have first satisfied yourself that the most violent kind of disturbance would be a TOP.

MR. THEOFANOUS: Excuse me, I didn't follow that.

MR. MARK: Perhaps you didn't say that, but the most violent disturbance you were prepared to discuss or even suppose could occur was something of that sort. And in that context, you have given thought to the rates at which various things could proceed.

MR. THEOFANOUS: Right.

MR. MARK: And that has brought you to conclude that they would not lead to an explosive or vessel-disruptive result. And that applies, and in detail applies, to the present heterogeneous pattern of the CRBR.

MR. THEOFANOUS: Right.

MR. MARK: It does not necessarily apply to the CDS or the previous CRBR homoegenous fuel design. So it does not apply to LMFBR generally but just to this particular arrangement.

MR. THEOFANOUS: Correct.

MR. MARK: Thank you.

MR. THEOFANOUS: And that is why, by the way, another question came up as part of my discussion with some of the subcommittee members as far as what kinds of things we should be covering. I think someone asked me at if the core was to change a few years later, and I ery emphatically stated we are reviewing this core and ny other core will have to be reviewed separately. ese reactors are very much core-dependent, those ccidents are very much core-dependent in these reactors.

MR. CARBON: Something was said on this this morning, and I am not sure exactly what, but Charlie Kelber's letter of July 7 talked about deep concern of Los Alamos and Sandia over the ability to resolve the question of containment failures in CRBR and so on. Are you aware of that letter?

MR. THEOFANOUS: I am aware of the letter. The letter was mentioned before. I thought Dr. Okrent was talking about another letter because Dr. Kelber likes to write a lot of letters, and there was another letter that said a similar disassembly calculation may be overly pessimistic because there were some early calculations done by somebody that gave a very early disassembly.

MR. CARBON: I believe that was the same letter.

MR. THEOFANOUS: Is that the one you're referring to? That's another letter I think than this one.

MR. CARBON: It said maybe face some

unpalatable truths with regard to the heterogeneous core and so on. Are you aware of what he is talking about?

MR. THEOFANOUS: We are aware of the letter. We have seen it.

MR. CARBON: Do you feel there is substance to it, or is it something that --

MR. THEOFANOUS: I think that is maybe a very timely question because I was just ready to ask some of the Los Alamos and Sandia people to respond to Dr. Okrent. And if they do have a problem, now would be the time to say it.

As far as I am concerned, we have kept in very close contact all along. We read together, in fact, the letter with Charlie Bell. And as far as we can tell, we could not really identify what gave rise to this verbiage that you see in the letter. As best as I can say, speaking for the team here, we are of one mind as far as which way we are going and how we are approaching the problem. There is not the slighest question about that.

I think that maybe each one, depending upon his own background and individual temperament might be more or less willing to assign numbers to a given qualitative feeling. But I think it's fair to say we all have a similar qualitative feeling. And also, as I said before, not all the participants had an opportunity to look how Bell and I have transcribed our qualitative feelings to numbers. And it still could be seen whether all of them would agree, although really I have no reason to believe there would be any big difference more than an order of magnitude there.

And with that, in fact, I want to go one step further in saying we even share, as I mentioned here, the feeling as far as what areas need additional attention, and we see them exactly eye to eye. And I mentioned already some of them. Let me just enumerate. The LOF-driven TOP marginal situation, we will look at it. The fuel dispersal in going through the paths, the freezing, plugging, and getting out, the timing involved there. Again, we need to do more on that.

And with that, I would like to ask now the individual members of the team to stand up and address Dr. Okrent's question. In particular, I will ask them to highlight their feelings about any potential problems that they see that we have not covered here as such.

MR. MARK: Could I ask, Max, do you know whether Bob Avery agrees with the qualitative conclusions that have been conveyed to us?

MR. THEOFANOUS: No, Dr. Mark. I don't think Dr. Avery even knows of that. The only people who have seen this summary we sent you with the numbers, of course, yourselves, if you had the Federal Express package sent to you Tuesday morning, hopefully. Also, I sent one package to Dr. Fauske, and he tells me he made copies and gave to the other members of the project. And I talked briefly on the telephone with Dr. Fauske yesterday, and as far as I can tell, they seem to have no big problems with the numbers we have here.

MR. MARK: I say Avery because I suspect among all of us here he has spent as much time as anyone on questions of this sort and has as deep a comprehension as one could ask anyone to have. And if he is in concurrence with the rather encouraging report you have on the CRBR design, it would be helpful to know that indeed he has that feeling.

MR. THEOFANOUS: Right. In fact, what we are planning to do and what we would like to do now that we have a little bit more time after this meeting, I will send to all of the members of the team this final copy you have in your hands. I will send also a copy to Dr. Avery and ask for his comments as well as many other people we think can take the time to help us as we embark upon documenting the final report for this. It would be very helpful to have this information.

MR. MARK: Are you going to call on some of your people?

MR. THEOFANOUS: Yes. Maybe you can start from Charlie Bell.

MR. BELL: I think Theo has enumerated some of the areas that still concern many of us. It's interesting in this group of people the different levels of skepticism that exist. I think to perhaps put Dr. Okrent somewhat at rest, if we can get ourselves all equally convinced, I will feel pretty comfortable. I think we have skeptics that will match the best. And that's good.

I think purposefully people were selected because there were a number of points of view represented. Theo and I certainly don't share all of the same perspectives and points of view, but I think we have found in the working relationship it's a very healthy thing to have to convince each other of different points of view, and out of that comes a stronger understanding of the overall problem.

I think in our last meeting we endeavored to show you a status at that time in which we had reviewed the applicant's situation and then some preliminary scoping analysis of our own in an attempt to highlight problems and also an attempt to develop where the thresholds to severe difficulty might be, under what assumptions one might have to put together in a series to get certain thresholds of whether it be autocatalytic behavior or whatever.

And what we find is except for the situation of the LCS-driven TOP being brought on us by this free-slip axial compaction of the columns, that appears to be the only candidate left that has this potential for this kind of behavior and difficulty in trying to resolve it.

So I think that is why we are putting a lot of emphasis on that right now. As Theo says, it doesn't exist with the preponderance of likelihoods as it has in the previous core design, and it does tend to be out there on the end of spectrum because of the number of assumptions we have to make.

Nevertheless, because of its potential to provide a difficult situation, we have to look at it in detail. There are a lot of cleanup items we have to do yet. Like, you mentioned the end of cycle core, that is a case in point where we have tried not to bias ourselves into a particular way of thinking. One has to trade off a number of things in looking at these various accidents.

The very ideas of cores manifesting more or less coherence, more or less sodium void, more or less fission gas pressure above; those various things will act in different ways, and one should not presume he knows how coherence will always manifest itself, and that is why we want to be sure we look at a number of points along the fuel cycle so that we know and can identify some major trends and behaviors, and that's the thing we are looking for. We are looking for trends in behavior that are relatively immune to a lot of detail.

We discussed that in our previous meeting. If everything we do is extremely dependent on every detail and there is not a fundamental identifiable degree of forgiveness in this accident response, we have trouble. We have been fortunate in the last few months to see the system portray a lot of forgiveness to details.

I don't think anybody necessarily can take credit for that. That's the way it is, and what we are doing is trying to sort it out and find a response of the system. So now if I might just comment on Dr. Kelber's letter, I am not exactly sure of the route of his interpretation either.

I suspect, however, since a number of us were away, I think at this last ACRS meeting the last time when he happened to be visiting out there, I suspect that the idea that he came away with was that if we did not achieve a significant amount of fuel removal in these early stages that Theo was talking about and we progress as a major accident trend to the whole-core pool with a high inventory, that we would have a serious situation. And I am not sure I am unprepared to back away from that sort of interpretation of the situation at this point in time.

We have seen some trends, however, where because of the flux-shaped changes and disassemblies taking place in puddled systems, that one does have a fair amount of that fuel removal, not even up to the 30 percent necessarily, these systems may in fact not manifest as much energetics as we might have otherwise suspected.

But that whole-core pool, because of its 2 degrees freedom and its ability to move material from the outside toward the center is a very differant situation. I think that's all I need to say at this point.

MR. MARK: I am afraid, Max, I have to leave before we get to Fauske's, et al., presentation. Has the Staff had time, Mr. Morris, to go through the kinds of things you have been hearing today and be able to give their considered opinion?

MR. MORRIS: Bill Morris, NRC Staff. We have been exposed to this information only within the last week or so, and we anticipate that the extra work to be done will help us evaluate this analysis and come to a conclusion. We have not reached that conclusion.

MR. MARK: Right. But supposing you should come to the position that what we have been hearing is indeed creditable and acceptable as a proper discussion of the state of affairs. Then am I right that you would be able to lay the HCDA aside as a specific concern?

MR. MORRIS: Given that this specific part of the analysis and the other part of the activities that are going on follow this same trend, I believe that would be the same case with regard to energetics.

MR. MARK: The melt-through is still the simpler item.

MR. MORRIS: If this kind of trend holds up, and subject to further analysis, if we believe this is a significant step in understanding the accident, we are looking forward to seeing that continue.

Theo, I would like to call on Bill Bohl, also from Los Alamos.

MR. BOHL: I am Bill Bohl, Los Alamos. I am not really prepared for a formal listing of all of the technical issues where I have reservations or where weak spots exist or whether the range of postulated sequences is sufficiently comprehensive to cover the range of possibilities.

I was not at Los Alamos during Charlie Kelber's visit, which seems to have led to the letter in question. However, since apparently I must make a statement, let me offer the following. The possibility of a ramp rate exceeding $100 a second must be quite low. However, I cannot with my present knowledge make a personal judgment to assign a specific numerical probability to this issue, particularly one made with high confidence.

My basic concern on these issues is that there may be developing too much of a reliance on engineering judgment where the facts and the experimental data base are incomplete.

Some possible dangers here seem to be, first, that an engineering judgment principle can be proven wrong and credibility can be lost. Second, even when the proof is unclear, a preacher of engineering judgment can acquire a reputation for nonobjectivity.

Third, a confusion of judgments with reality can arise, for example, what interpretation should be given to code calculations or numerical results based on engineering judgment or how does one maintain sufficient objectivity to fairly consider all of the evidence after widely publicizing an engineering judgment on a given topic?

Fourth, confusing engineering judgment with facts can lead to a false sense of security. Unfortunately, I see no real solution to these problems in the short term, and you the members of the ACRS must make a judgment on the incomplete data base such as exists.

That's all I really have to say.

MR. OKRENT: Well, by the way, if after he gets back to Los Alamos or if he has nothing to do on the plane, if Mr. Bohl has any specific points that he thinks the ACRS should hear more about, we would appreciate his telling us.

MR. THEOFANOUS: So would we.

MR. OKRENT: And I would have to give my regrets also to the chairman and Mr. Fauske, et al.

MR. FAUSKE: That's all right, Dave.

(Laughter.)

MR. OKRENT: It's either you or my family.

MR. LIPINSKI: Mr. Chairman, one of my observations after these comments is, had some other members of the team given the presentation, I don't think it would have come out the same way.

MR. THEOFANOUS: I think I would like to temper this observation by looking at the whole spectrum of comments you received as well as by the specificity of the comments. I think it's easy enough for somebody to sit around and say, I don't know this, I don't know that, and there's a lot of handwaving.

But I think in this business at some point we have to learn to be specific and say, there is a concern because of this, this, and this, and our approach, we took the approach here that if we are going to have a problem with some behavior in the CRBR, we ought to be able to pinpoint exactly what is our source of concern.

Bill has been invited all along, and he has been a member of the team. And I think what you hear there is not so much of a difference, really drastic difference of opinion, as much as a different temperament or willingness to put numbers, although we have defined the numbers very clearly here in this general feeling of energetics.

So I don't think you are seeing here as big a discrepancy as I think I hear your comment to say. And that is coming from one member of the team which probably really is on the extreme of skepticism.

MR. LIPINSKI: I think where the numbers are known I don't think you're arguing it's where judgment is required there is a difference of opinion, you are more optimistic with your judgments.

MR. THEOFANOUS: I don't think so. I think if you take that into total perspective, when you say that I don't know to the final detail, is it 1 gram of cladding going up or 2 grams, for some people it causes a big mental block. For me it doesn't. That doesn't mean I am optimistic about it nor that Mr. Bohl is pessimistic. It's a matter of perspective. You have got to know what you are looking for. I started out here by telling you --

MR. CARBON: Let me stop you, if I may. I think you have each made your point, and considering the time, let's go on.

MR. THEOFANOUS: Let's have Pete Maste from Sandia.

MR. MASTE: Pete Maste from Sandia. I will make a quick statement because I can't talk. I won't make any general comments about the approach or anything. I guess my feeling is I have little difficulty with the conclusions that have been drawn in general, especially with regard to obtaining larger ramp rates than $100 a second from anything other than the large-scale pool phase.

I think the key of the analysis is the avoidance of that whole-core boiling pool phase. Consequently, I think one of the key things is the fuel dispersal aspect of the problem. And as Theo has pointed out, this is an area of ongoing research with us. We have done a number of preliminary calculations. We are continuing to do more in that area. We have gotten some positive preliminary results recently which lead us to the conclusions that have been made. Certainly, further calculations are needed to verify those results.

Further, I might add just from a personal standpoint, our background at Sandia is experiments. So one of the things we would like to see more of is additional experimental confirmation of the types of behavior we are calculating. And hopefully, we will get some of that within the next year between the Argonne gap experiances and some of the ongoing work at Sandia.

I thought I would highlight that as what I consider to be the key area in the presentation.

MR. THEOFANOUS: Okay. And that, I think, covers all of the present consultants. We have three more memebers of the team in universities, and we have another maybe five or six members of the team in national laboratories who are not present.

But we are intending to send to all of them the document, and we are going to ask, as we have done in the past, comments in writing to the group and then we factor all of these comments into our going on. And if those comments necessitate us getting together and discussing this, we will do that.

And by the way, we have done that at every step of the way up till now, and we have received 99 percent good response in terms of timely response from all of the consultants sending us all their concerns.

MR. CARBON: I have one more general question of you before we shift to the second part. There has been a lot of experimental work going on now, some at Argonne, the experiments just mentioned, some out at Idaho at TREAT and so on. Will this have any bearing on your results here?

MR. THEOFANOUS: I think we are particularly anxious to obtain experiemental information on the kinds of freezing and plugging and fuel dispersal mechanisms that Pete Maste mentioned a minute ago. We are particularly interested in that. We feel that is the area probably very sensitive, very important, and maybe not really substantially supported by experiemental data as we would like to see it.

This is one set of experiments going on at Argonne as well as Sandia that we are anxiously waiting to see the results. If you ask me personally if there is any other area that I would like to see experimental information that I would think would have a bearing on all of this business, not necessarily on this particular quarter because it doesn't exhibit this potential so much.

But I think at some point in the development of the LMFBR we have to come to grips with the LOF-driven TOP, and from that point of view we have to have some experimental information with good diagnostics to know what's going on. If you have cladding with full sodium going by and if you suddenly melt the fuel under high-power conditions, that is generically one area we know little about and we need to know. But it's not really directly relevant, although it has a little bit, as you saw, relevance here. But it is not directly relevant to the present report.

MR. CARBON: Do you have any other questions?

(No response.)

MR. CARBON: Does that end your presentation?

MR. THEOFANOUS: Yes, that ends my presentation. Thank you.

MR. CARBON: Any other comments to make?

(No response.)

MR. CARBON: Our committee here has dwindled, but I would like very much to have your material on the record. So I would propose that we take a very short break and than continue right on.

(Brief recess.)

MR. CARBON: Mr. Dixon, I guess we are calling on you.

MR. DIXON: It's actually Fauske who is supposed to give the introduction. But if he doesn't get here, I am going to do it.

MR. FAUSKE: My name is Hans Fauske. It seems in order to take the opportunity to thank Max Carbon, Walt Lipinski, and the rest of the NRC Staff for staying to listen to the project.

MR. CARBON: Should I say anything about how lucky you are that you recognized Dr. Okrent has left?

(Laughter.)

MR. CARBON: Let me mention to everyone that we are definitely going to aim to conclude this portion by 5:30 at the latest. Go ahead.

MR. FAUSKE: I think before I get into introducing the speakers, I would like to make a few general comments. The last time I had an opportunity to address this committee was back in late 1976 and early '77. At that time the applicant asked for licensing for a homogeneous core design. I think it is important to restate, as it was done many times by Dr. Theofanous, that this time we are asking for a licensing of a heterogeneous core.

The change in this core design has a number of important implications from an energetics point of view. Number one, the LOF-driven TOP we feel from a project point of view has significantly decreased as being a potential for energetics. We heard the same thing from the NRC side this morning and this afternoon.

Secondly, we believe that the compaction problem by fission gas, again to some extent because of the core design, has decreased in its potential for causing autocatalytic effects.

Perhaps more importantly, the potential for getting into a large-scale pool phase -- namely, where the concerns of potential escalating recriticality may indeed occur -- has indeed essentially been removed by this core design.

I would like to emphasize this point because the difference in the two core designs leads to an increased time of vendor for fuel removal and hence being able to escape the large-scale pool phase. Of course, with the change in the hetereogenous core design, one of the important neutronics parameters, of course, is the sodium void worth and its uncertainties.

This was a question brought up by the NRC Staff, and it's been a question that has been addressed in response to the eight questions. This afternoon Herb Henryson from Argonne National Laboratory will be addressing that question specifically asked by Max Carbon in his letter to Dr. Theofanous.

This is an important parameter to establish. I think it is important that we all agree to this parameter because, as Dr. Theofanous has illustrated through his presentation, it can have a profound effect in setting the stage, particularly for fuel motion and its behavior.

We also had planned to give you a more detailed rundown of the project calculation as it rel[illegible] the LOF initiating phase. And again, I would like to point out that the NRC Staff and its consultants in their eight questions pointed out the need for looking at the fuel compaction problem by fission gas.

This was not a thing or a problem we have looked at in reporting our project decision. We have since that time explored this problem in some detail. The folks at Argonne, Dr. Avery's people, have been involved in this process. And Dave Weber is here this afternoon to report on that project status. Unfortunately, because of time, I have asked Dave to limit his presentation, but all of the vuegraphs we had planned to present to you will be sent to you as a part of this docket.

Next, as I mentioned to you earlier, the potential for getting into a large-scale pool phase has been significantly decreased because of the heterogeneous core design. We would like to take the opportunity this afternoon to tell you about some of the detailed considerations we have made in considering the various potential fuel escape paths that exist for early fuel removal, hence mitigating the potential for getting to a large-scale pool phase.

Mike Epstein will be giving this presentation, and we would like to give you some reasonable amount of detail in this area because it is also an area of great interest to the NRC Staff and its consultants.

Finally, we would like to make some concluding remarks as to the project decision to date by Danny Switick.

So with these few introductory remarks, I would like to introduce Herb Henryson to give the presentation on sodium void worth.

MR. HENRYSON: Thank you. As Hans said, my name is Herb Henryson. I am with the applied physics division at Argonne National Laboratory. And I have been asked to make a brief presentation with something of a change of pace here in the sense that we will be taking a short diversion into the world of physics and away a bit from engineering-type considerations.

The question I have been asked to address is how well do we know the sodium void worth in the CRBR reactor, how well can we calculate it, and what are the uncertainties on it.

There are two ways of addressing this problem. One is to use state-of-the-art methods and cross-section data and come out with the best possible calculation one can come out with. If we do this, we have something of a problem in that your judgment of what the uncertainty on that is might be different from mine and we would end up in almost an infinite argument.

We have chosen not to take that path. Instead, we have chosen to use a rather substantial integral data base to derive a "experimental" value of the CRBR sodium void worth. From this work, uncertainties will fall out of the analyses, and most of the talk will be dedicated to what do I mean by "experimental" value in this respect.

The experimental value is based upon experiments which have been done on the ZPPR, the Z-P-P-R, assembly out at Idaho Falls at Argonne National Laboratory. ZPPR is a 14-foot-square matrix critical assembly. It is a moving table device whereby one loads fuel in horizontal direction in each of the halves and then the halves are driven together to come up to criticality.

These little tubes here, one -- we call them drawers -- and one loads fuel into those drawers, and one is limited primarily by inventories and in what you can put into this machine. To provide an example of the type of analysis we have been doing, I have here in this insert a picture, a schematic of a ZPPR-11 assembly which was the so-called CRBR engineering market critical assembly.

If you try to keep that picture in your mind to compare it against what the true CRBR assembly looks like with its internal blankets, adial blanket and the red driver zones or core zones, if you could keep that picture in mind, you would see that we mock up the assembly design extremely well. Not only do we get the design extremely well, we also get volume fractions and mock up the materials within the reactor extremely well.

The way we can do that is through these 2-inch drawers which go into the halves of the reactor, and we have slab fuel and we simply use our inventory to get together the mixed oxide or the iron or a good representation of the control rods.

So the point is we have an extremely good experimental arrangement to mock up the specific reactor. Not only do we have that, we also have done, of course, many experiments for other assemblies. So I am referring specifically to the CRBR engineering mockups that we have at least a 10- or 15-year data base on sodium void experiments.

What do we do with these data as we get them? Clearly, one thing we do is try to calculate the experiment. From our calculation of the experiment, using the experimental values, we can derive bias factors effectively statistically analyzing these data to the point where using these bias factors we can come up with instead of calculated values of the sodium worth, we come up with what I call predicted value of the sodium worth. That is biased values based upon our calculation and our experiment.

What we tend to do is mathematically determine bias factors for both the nonleakage and the leakage terms. Because of time, I won't go into the physics of these differences. But effectively, what we are trying to do is come up with our best fit to the bias factors and some measure of the uncertainty on those fits.

The methodology we use -- and the reason I put this up -- is not to illustrate necessarily how well we can do our computations but to try to indicate to you that it's important that when we are calculating CRBR we want to use the same methodology as has been used in the critical experiment analysis because by doing that we are now tying the two together. And this is one of the integral parts of making sure that we are talking about an experimental value.

I must use the same methodology as was being used in the critical analysis and then the bias factors apply. And then I can do my power reactor calculation and make use of it. I won't go into detail through this work. I do want to mention that we are working in diffusion theory. This is in contradiction to that first way of doing it, in which case I might use exotic transport methods or whatever.

I also point out that we do correct for streaming using the so-called Benoit directional diffusion coefficients. The reason I mention that is in the ZPPR drawers you saw, there are several streaming paths that exist where neutrons can go down, vacuums, essentially, and the fact is in the power reactor where we have pin geometry, that geometry is quite different. So one has to worry a bit about how do you extrapolate from the slab lattices to those pin designs. And I will consider that.

But the use of the Benoit direction diffusion coefficients, introducing that into our methodology makes it a lot easier to make that extrapolation. It is just a minor point.

MR. LIPINSKI: Sodium is not in your drawers.

MR. HENRYSON: Excuse me?

MR. LIPINSKI: Sodium is not in your drawers. You are using the substitute material and making corrections.

MR. HENRYSON: No, no. We have sodium drawers.

MR. LIPINSKI: There are sodium in those drawers?

MR. HENRYSON: Yes. We have sodium cans.

MR. LIPINSKI: I am sorry, I didn't understand that. Thank you.

MR. HENRYSON: We have sodium cans, and we also have sodium right within the drawers.

MR. LIPINSKI: Okay. I stand corrected.

MR. CARBON: Around each pin, so to speak?

MR. HENRYSON: We get the proper volume fraction of the materials, and that includes the structure, the heavy metal and the coolant, the types. Here is an example of the types.

MR. CARBON: Excuse me. One thing is different then is you have air gaps.

MR. HENRYSON: That's correct. There are

other things which are different as well, and I will address them. The geometry, as you mentioned; temperature is different. Ours are done at room temperatures. The power reactor is 1500 degrees K. We don't have fission products, for example. And I guess that is pretty much it. But I will mention thamn.

In this vuegraph I have indicated a number of the types of experiments we have do and something which physicists or people who just came from the ANS meeting get a little bit sick of seeing C over E. That is our shorthand for saying: calculation to experiment. How well do we calculate these experiments.

The first two rows are the most important ones for the analysis we have just completed. Clearly, if we have an experiment which mocks up the power reactor extremely well, we ca make great use of our ability to bias those results and feel a great deal of confidence in them when we go to the power reactor. That is the situation in these first two rows.

ZPPR 11 F was a mockup of the beginning of cycle 1 core. ZPPR 11 E was a mockup of the end of cycle 4 core. We did extensive voiding in those assemblies. Let me show you quickly a picture of our voiding patterns.

What we did was we voided in 13 radial steps.

We voided all of the core in which the sodium void coefficient was inherently positive; that is, all of these clear zones. We started with sodium in them, and we gradually took out sodium, made a measurement, took out more sodium, and so on.

But you can see we voided a major part of the core. That gives you an example of how extensive these measurements are. That is what is represented in these first two rows. And as you see, before biasing, using the methodology which I outlined on a previous vuegraph for the beginning of cycle 1 core, we calculated almost exactly. We calculate slightly low, a calculation to experiment is .98. For the end of cycle core 4, we calculate sodium void rather high, a C over E of 1.23.

This is the sort of thing that tends to disturb physicists. One doesn't care so much what the number is, but one likes it to be the same number to indicate we understand what's going on. Well, in point of fact, if I had used the best possible methods to do the BOC 1 analysis -- that is, if I had made use of transport theory -- I would have ended up with a number considerably closer to the EOC 4 calculation.

But that is not the point of this analysis. The point of this analysis is to do our analysis of the experiment the same as we do the analysis of the power reactor. So we have these different, if you will, bias factors between the BOC 1 and the EOC 4.

Yes?

MR. CARBON: How do you get the E for the EOC 4?

MR. HENRYSON: We have measured -- oh, this is not -- this is the CRBR engineering mockup critical experiment for EOC 4. This was the so-called ZPPR 11 E experiment. But we actually voided and we made void measuurements.

MR. CARBON: But does it represent the end of cycle core?

MR. HENRYSON: It represents it from the sense of plutonium buildup in the blankets.

MR. CARBON: But no fission?

MR. HENRYSON: No fission products.

MR. CARBON: So it's not really --

MR. HENRYSON: We are missing within this -- and my next vuegraph will address that -- but let me say that what that does, the place where -- the things which are not considered within the ZPPR facility we claim do not affect the bias factor. It affects the uncertainty on our calculation because our calculation does account for the fission products when we do it.

So that what we are saying then is, how well are we accounting for it? So the bias factor we will end up using is the bias factor from the experiment, the experimental uncertainty and then we will have to add additional uncertainties to account for those things which are not included in teh critical experiment.

I mentioned the EMC. The other four rows represent effectively our last 10 years of doing sodium void experiments, 101 mixed zones, axial blankets with and without control rods.

The reason I mention this is these, the axial blankets, for example, tend to have very low uncertainties. When we did our analysis, becauase we had not voided the zones in our engineering mockup critical, we significantly increased the uncertainty as far as they were concerned. We didn't take these (indicating) as gospel when we extrapolated to the new reactor.

MR. LIPINSKI: Another question. When you put in the sodium voids, effectively you added about $5 worth of reactivity; correct?

MR. HENRYSON: All we did was void the -- all we did was void the driver assemblies. So that's less; that's $1.20 or thereabouts.

MR. LIPINSKI: Then you control that by control rod positions?

MR. HENRYSON: Outside, yes. Shim rods, effectively.

MR. LIPINSKI: I wanted to make sure you didn't readjust the core someplace else.

MR. HENRYSON: No. No, no, no, no, no. We have done that in some of the larger experiments and that is troubling. What it does is increase your experimental uncertainty. What we ended up doing for this CRBR power reactor analysis was using these bias factors and the final bias factors we used, based upon that previous vuegraph. We didn't bias anything except the positive voiding zone of the end of cycle 4 core.

What I am saying effectively is, if we forget the part of the core where if you take out the sodium you get a positive worth signal, forgetting that, we are using the calculation as it comes off the computer as our nominal value of sodium void for all of BOC 1 and indeed for those zones such as the zone near the radial blanket interface.

We are using those without any bias. The reason we have done that is the biases that one obtains, looking from those earlier experiments, tend to be within the uncertainties, and it tends that if we go this way, we are a bit conservative.

So we have chosen the route of being somewhat conservative. Here are the uncertainties which one gets, again, from this experimental analysis. It is important to note that this first row, the central core, or the positive zoning part, is the important part. This is the part that enters into your analysis. The other parts tend to be negative and tend not to get involved in any kind of an LOF-driven TOP, for example.

I have already alluded to my next slide, which is the additional uncertainties which result because of the fact that we are basing this on a ZPPR critical facility and not a power reactor. The power reactor has fuel pins. ZPPR has plates.

Within the ZPPR 11 program we did extensive voiding of the pin zones. We even have pin colandria, and we look at how well do we calculate them relative to how well do we calculate things with our plate lattices. And we found there was effectively no difference between the two.

So what that tells us is that we do not have to add any additional uncertainty because of the change of geometry. The only reasons that is true is because we took account of the streaming within the plate lattices, as I mentioned earlier. Had we not done so, there would have been an additional uncertainty here.

Similarly, the sequence of voiding. When we do our ZPPR experiment, we void a zone. I mentioned we did 13 different zoning experiments, and we calculate each and every one of those experiments. When we do the CRBR analysis, we simply void all of the sodium flowing from the core and calculate it. We then use that right through the transient where part of the core is voided and another part is not.

The uncertainty which we feel is introduced because of not taking explicit account of the voiding sequence within the power reactor calculation, we feel is less than 3.5 percent. Similarly, we do our experiments at room temperature,and we do our power reactor calculations at 1500 degrees K.

The additional uncertainty which is introduced we can relate to our uncertainty in the doppler coefficient. Effectively, we are looking at the change of the U238 capture cross-section with temperature, and we have looked at that analytically, and we feel a 2.5 percent number is quite good. It is also very consistent with what the British, for example, come up with.

Our end of cycle 4 assembly did not have any fission products in it. What additional uncertainty is introduced because of fission products, what we have done is looked at how well do we know our fission product cross-sections. And we have compared that primarily against the French, who have used the Phoenix experience to adjust their fission product cross-sections.

And what we found is a 3 percent uncertainty is extremely conservative. If one goes by the French data or, indeed, by most international data, one finds that we have a rather high fission product capture cross-section. A high capture cross-section leads to a higher sodium void coefficient. So putting a plus or minus 3 percent on our nominal calculation tends to be extremely conservative in view of what we know about fission product cross-sections.

The bottom line, once we have performed our calculations, applied this bias factor to the calculation for our end of cycle 4 power reactor core, and what we now call our best estimate, our nominal values are $1.44, about $1.50. If we look at just the driver assemblies in that 36-inch core zone, about $1.44. And the number in parentheses is, if you will, the old number. This is the number which is reported in the GFER document Dr. Theofanous alluded to.

On the other hand, this number has an uncertainty which, if you add up the experimental unceratinty and the fission product and the rest I have just alluded to, this number has about a 10 percent uncertainty on it. I think it's 8 percent actually. This number had a 60 percent uncertainty attached to it. So you can see we have, in fct, made a great deal of progress.

We also must see that our nominal numbers indeed are nominal plus two sigma numbers are, I believe, within the error band of your own numbers. So things are not bad. The analysis that has been done, one would argue, is one can rely on to some degree, particularly if you account for the uncertainties. And the other numbers are given, and I won't dwell on them.

Let me say that the analysis which has been done within the reactor analysis and safety division, using our latest void coefficient data, we use our nominal value as quoted and then took the most conservative two sigma vartiation on that.

So that was a 10 percent, 8 percent I guess, variation on the positive part of the driver, something like a 20 percent on the axial blanket and so on. So our analysis has tended to use extremely conservative assumptions and not these nominal values.

That is my story, and I will be glad to answer any questions.

MR. CARBON: Has this been culminating, what brings out the change at this particular time?

MR. HENRYSON: Let me answer that quickly, and then I would like to mention Paul. But let me say these experiments were completed just last year, the ZPPR experiments on which we base most of our biasing procedure. But I would prefer to have Paul Dixon answer that question, since he is more familiar with what happened.

MR. DIXON: Max, as I indicated last time, we don't view it as a change. We had before a number of $1.10 and a 60-cent uncertainty on top of that. And the reason we had such a large uncertainty was ZPPR 11 wasn't done. So we had an unceratinty in our calculations. When we use the $1.10 plus the uncertainty, we are actually using a number that is a little less than what you would use today using the $1.44 plus uncertainty.

Now that ZPPR 11 is done, and it was always planned to get our sodium void accurately done in ZPPR 11, we knew our uncertainty would come down, and we didn't know in which direction our nominal value would go. But the two numbers are really consistent if you consider the fact that we knew we had a fairly large uncertainty then and applied it.

MR. CARBON: The numbers that Theo was talking about this morning, this maximum void worth, wasn't it over $2 or something?

MR. THEOFANOUS: No, Max, just for illustration I said something less than $2. It was only just to give you a rough idea of the order of magnitude of void needed to get your energetic events. In our calculations, we used, for example, in our plenum fission gas compaction calculation, we used the sodium worth of 4.7, and that is the nominal plus 20 percent. We wanted to be a little bit on the high side and so on. So we are not having any big problems with this.

MR. HENRYSON: I may be a bit too much of a purist on this, but when I speak about maximum positive, I tried effectively to count every piece of the core which is positive.

What we used when we looked at the maximum positive for the LOF-driven TOP analysis was in fact $2.19 was our maximum positive based on this analysis, which looks as if it would be more like a, well, here it is, here is a maximum positive of $1.97, but that is a nominal.

MR. CARBON: The ZPPR 11 results came out 1 year ago?

MR. HENRYSON: How long has it been, Paul? It ceratinly was last year we did the experiments.

MR. DIXON: The experiments were supposed to wrap up at the end of fiscal year '81, which would have been September of a year ago. They actually were a little late, so it was about a year ago the experiments were concluded. And the nominal data reduction and report time is on the order of 1 year or a little longer.

In this particular case, last summer I asked Argonne if they couldn't accelerate their work in this area to have this work done in time for these discussions, and they did. In fact, in time for the September meeting, and they did. And they actually accelerated their work. They had the nominals. They didn't have all of the uncertainties. Normally, they would be wrapping it up about now.

MR. CARBON: Fine.

MR. HENRYSON: Thank you.

MR. LIPINSKI: Onemore. Could you go back and state how you got this $1.97 max positive given you had $1.44?

MR. HENRYSON: The $1.44 includes parts of the driver zone near the radial blanket and near the axial blanket, which actually have a larger leakage component, the leakage being negative. So that the net worth, although still in the driver zone, is negative. We get the $1.97 by summing only those positive parts of the driver zone.

MR. LIPINSKI: Okay. That doesn't show up on this tabulation.

MR. HENRYSON: What do you mean? Which tabulation?

MR. LIPINSKI: The ECC 4. If I look at all of your others where they've got maximum positive, they are the sum of the positive components in those columns only. The one below. You take the $1.50 plus the lower axial extension, and you come out with max positive of $1.52. But I can't do that in the column above.

MR. HENRYSON: That's right, because within this core zone --

MR. LIPINSKI: There's a negative component.

MR. HENRYSON: There's a negative component. Not so in the internal blanket assemblies. You are quite right.

MR. FAUSKE: Max, in view of the dewailed presentation by Dr. Theofanous and the initiating phase, we have also asked Dave to provide a summary at this point of the Argonne calculations.

MR. WEBER: I am Dave Weber from Argonne National Laboratory. I am one of Bob Avery's gang. I would like to summarize a few things about what has recently gone on in terms of loss-of-flow analysis.

There are several vuegraphs contained in the package which highlight many of the technical activities which have been going on over the last several months at Argonne and for the project.

For the purpose of this meeting, I would like to only cover two of those vuegraphs. In fact, I would summarize rather specific activities. If you would, the first vuegraph then in the package refers to the CRBR best estimate of the EOC 4 loss-of-flow assessment.

You may recall the last time we gave a presentation to this committee, we were examining the experimental data that was relevant for fuel dispersal under loss-of-flow conditions and indicated at that time that we would be incorporating that assessment into the analysis of their effect on the CRBR heterogenous core.

That assessment of the experimental information as well as the whole-core implications in fact was concluded in response to the set of NRC questions Dr. Theofanous mentioned. The specific elements contained in that analysis are indicated in here and briefly mentioned at our last meeting. But let me point out a couple of things.

First of all, the motivation for this was, in part, based upon a consideration of autocatalytic fuel behavior due to the plenum fission gas. Our technical approach, though, in this assessment was to utilize the neutronics assessment originally contained in GEFR 5.23. But as we mentioned in our last meeting, we went to in-pile experiment, principally TREAT experiments, to determine the appropriate modeling of fuel disruption under these particular conditions and then incorporate them in the whole-core analysis code, SASS-3D and the fuel motion model referred to as SLUMPY.

The second important aspect of that is we looked at the problem of fission gas availability and looked at additional experimental information, principally the Hanford fission gas release test and the analytical modeling we have within the code referred to as FRAS at Argonne to give an assessment of both fission gas availability and its potential for dispersing fuel.

These aspects were put into the whole-core analysis code, and the significant conclusions that were reported in response to the NRC are contained on this vuegraph.

First of all, the time scale for the accident sequence was in fact increased. This had an important factor in allowing more time for fission gas flowdown within the sequence. We also noticed we had a much milder excursion. In fact, our lead channel, our maximum power was only approximately 5 times nominal.

And our final conclusion was there should be a mild entry to the melt-out phase.

Now, there have been several things happen since then. The most important one was the one discussed by Dr. Henryson just previously; that is, a new assessment of the sodium void worth was conducted, and that information became available late in the time frame for analysis for answering the initial set of NRC questions.

Consequently, at the meeting held between the Clinch River project and the NRC Staff, I believe, on September 21 of this year, a specific action item was requested that we incorporate the new void worths into our analysis and assess the potential for energetics within this area.

The next several vuegraphs in your package talk somewhat about what our approach was in this particular area. When we first became involved, we had taken a technical approach and the philosophy that we would tie our analysis and the modeling we have within the whole-core codes as closely as we could to actual experimental information. The first illustration of that was our modeling of the TREAT loss-of-flow overpower experiments.

There have been several other areas since then we have also looked at. The principal ones are ones Dr. Theofanous also mentioned, and that is, an assessment of cladding, molten cladding relocation under loss-of-flow conditions and the secondary aspect as to what was the potential for the actual clad failure during one of these loss-of-flow excursions.

e have come to some conclusions in that area, and rather than go through some of the details, although I would be willing to talk about and answer any questions you may have, I will go simply to the last vuegraph that highlights our current assessment, and I will make a comment as to where we actually stand in this assessment.

The first point I have illustrated also in previous vuegraphs, I would like to again point out, is the potential for autocatalysis. This particular scenario is, to some extent, a hypothetical event. We do believe there are incoherency mechanisms within the subassembly and inter-subassembly incoherences that would in fact mitigate some of our concern.

Nevertheless, the whole-core analysis did not include those particular effects. As I mentioned, our initial assessment did show time for a complete blowdown in the early assemblies, but we did have this potential for compaction in the later assemblies; that is, when we were still using fuel dispersal arguments that Argonne felt were extremely conservative.

And that is really the key to this third point. And I have expanded this point from our recent discussion with the NRC. The assessment of this autocatalytic compaction problem is certainly affected by conservative modeling of many phenomena. The first we were to point out was the modeling of early fuel motion, but it has become obvious that the additional phenomenological aspects involving clad motion and plenum gas effects on vapor dynamics are also important.

So the approach we have taken are contained in the next several elements. In order to, we think, experimentally consistently model fuel motion, we have based our analysis on TREAT overpower loss-of-flow tests L6 and L7, although there are other loss-of-flow tests we believe consistent with this modeling.

MR. LIPINSKI: Were those seven-pin tests or higher?

MR. WEBER: Three-pin tests.

MR. LIPINSKI: Three-pin.

MR. WEBER: Our fission gas availability was determined by relating analytical considerations to experimental observations in the Hanford fission gas release tests through the Argonne code FRAS 3. Beyond these two points, which were contained in our original assessment, we have extended consideration for looking at cladding motion and the way it is described in the context of the SASS 3D code.

We are in the current process of examining what we consider the most relevant experiments in this area; that is, the TREAT experiments, including -- and this vuegraph does not contain the R4 tests -- but we are also looking at the R4 test, which is contained in the earlier vuegraphs.

But we are looking at the TREAT fresh fuel tests R4 and R5 as well as TREAT test R8, which was one Dr. Theofanous referred to, as an experiment that had in fact pressurized plenum and SLSF experiments.

I mentioned specifically P3A. At the present time, we are considering P3A and not P3, because we in fact had SASS analysis of P3A as well to give us a better appreciation of how well or poorly cladding relocation was being modeled by the code. That experimental information was reflected in our SASS 3D clad motion model.

Our initial assessment at this point, a point mentioned in one of the earlier vuegraphs, we thought there was a limited potential for any clad relocation very early in the scenario, and that meant about the time of the first fuel pin disruption.

Later on in the scenario there does appear to be some time for cladding motion, and we are going through the process of looking at the experiments to, in effect, calibrate the clad motion model to be consistent with those experiments.

The specific calculations, the whole-core calculations we have performed to date, do not take specific account of that phenomena and, in fact, hold the clad in place during these early stages of disruption. And as a result, we do not get any positive reactivity from clad motion, but we also do not get any negative reactivity from clad motion.

As we conclude this assessment, we will in fact have consistent clad motion models that is consistent with experimental models that will be utilized in the whole-core analysis.

A secondary point then also mentioned in previous vuegraphs was we have also taken a closer look at the failure of irradiated cladding to establish a clad failure criteria. We do believe the original assumptions on clad failure were, in fact, conservative in this area as well.

With all of these assumptions put back together using the SASS Code, whole-core analyses were performed, and the conclusion, as I have indicated there, is literally the bottom line. Our best estimate whole-core analyses using experimentally consistent modeling show a mild entry into the melt-out phase and the problem of autocatalytic fuel compaction driven by plenum fission gas compaction seems unlikely.

I have indicated the word "unlikely," and not that it is not predicted, because we have not concluded the analysis, but we believe the analysis, based upon our cladding relocation assessment, will in fact show it will not be predicted.

MR. LIPINSKI: Is TREAT R5 and R8 the irradiated?

MR. WEBER: No. Those are fresh fuel.

MR. LIPINSKI: How many pins did they have?

MR. WEBER: Seven pins.

MR. LIPINSKI: And the SLF 3DA was pre-irradiated? How many pins?

MR. WEBER: 37 pins pre-irradiated to .6 percent atom.

MR. LIPINSKI: So that is getting closer to being protypical for a partial loading of an assembly as opposed to seven pins?

MR. WEBER: Yes, that's correct. One thing we are interested in specifically is for these tests and in fact the reason that tests such as R5, R8 and P3A, in particular, were one was to assess voiding dynamics and clad relocation, and that was the experiment purpose and that is the information we are using in our assessments.

As far as fuel disruption is concerned for irradiated fuel, we are using other more relevant experimental information.

MR. CARBON: What kind of time schedule are you on in you analysis?

MR. WEBER: It is a short time schedule consistent with the NRC Schedule. We expect to conclude this activity within a matter of weeks.

MR. CARBON: Thank you.

MR. FAUSKE: Next we would like to give you a reasonably detailed presentation on evaluation related to fuel removal. That presentation will be presented by Dr. Epstein from Fauske & Associates.

MR. EPSTEIN: Actually, in the handout you will find some information there not only on fuel dispersal by extended fuel motion but also on recriticalities. In view of the time, we are just going to present the most recent results obtained by the project and its consultants on fuel removal by extended fuel motion.

I might add that all of the information on recriticalities and instability was presented, at least most of the information, at the last ACRS meeting and discussed at that time.

To set the stage for my conversation on fuel removal, let me talk to a core map. This wasn't pointed out, but there is already a large difference in the approach of the project and the NRC in that we are dealing with red driver fuel assemblies, and if I recall, the NRC was dealing with blue ones.

(Laughter.)

MR. EPSTEIN: I would like to talk about first and concentrate on the red regions you see here on the core map, which are the driver fuel assemblies and also the inner blanket assemblies, indicated by these shaded assemblies.

Following the initiation phase, core disruption, of course, is beginning. Fuel is mixing with clad steel, and regions of boiled-up fuel are beginning to develop in some or maybe all of the driver fuel assemblies. These regions are quite hot, 3000 degrees C perhaps plus. And, of course, they begin to attack their subassembly can walls so that they can penetrate the subassembly can wall and one driver fuel assembly can turn into two and perhaps two can turn into three. In other words, very early after the disruption or the initiation phase, I should say, the driver fuel assemblies are beginning to merge.

Now, the driver fuel just as well as merging can easily move into the blanket assemblies, too. There is no reason in the world why the driver fuel cannot work its way through the subassemblies protecting the blanket fuel and move into the blanket fuel assemblies.

When the driver fuel moves into the blanket fuel assemblies, however, it sees a very tightly packed region of blanket pins. These pins are at 2000 degrees or below; they are very cold. And that is also a tightly packed region as well. There are only about 20 percent free volume for driver fuel to flow into those assmblies.

So driver fuel sees a tight cold region, and according to our estimates, the driver fuel will move into the blanket fuel upon penetrating the subassembly can walls of the blanket assemblies and freeze, forming a composite, if you will, a temporary composite of frozen driver fuel and blanket fuel, a composite cylinder of these two materials.

Of course, the cylinder will not stick around forever. It is subject to internal heat generation and also to ablation by fuel on the outside. But our estimates are such that these blanket fuel composites will remain around for a period of at least 10 seconds in the midplane or perhaps as long as 30 or 40 seconds, for periods at least of the order of 30 or 40 seconds, in regions away from the midplane, say, below the midplane. And we call the period at which the blanket assemblies are live and still standing the melt-out phase.

The importance or the implication, I should say, of the blanket assemblies surviving is that the fuel motion in the plane of the core mat becomes difficult because of these islands of assemblies that present barriers to radial fuel motion within the core and during that period of time, during the melt-out phase when the blanket assemblies are surviving and still standing, may promote simply one-dimensional motion in and out of the plane of the core rather than in the plane of the core.

This one-dimensional motion, as I mentioned at the last ACRS meeting, is subject to Taylor instabilities; that is, following a possible collection of molten fuel in a specific region of the core leading to a mild burst and fuel acceleration, this liquid slug will quickly be brought to an end by these fluid mechanicals inspatial instability. That is the instability known as the Taylor instability.

The Taylor instability will break the slug up and render it neutronically harmless. So during the course of the melt-out phase, while the blanket assemblies are still intact, we anticipate if a mild recriticality does occur, it cannot escalate into a major one simply because the fuel slugs accelerated by the mild recriticality will not survive long. They will be broken apart before they can act or recompact again and cause a major recriticality.

As far as large compaction rates due to FCIs, we simppy rule them out -- not simply rule them out, but rule them out based on some of the physical principles discussed this morning as well as additional physical principles we feel are also active to prevent core compactions by FCIs.

So just to summarize again, the neutronics of the melt-out phase are such that mild recriticalities are possible due to slow gravitation settling and collection of fuel. These mild recriticalities cannot amplify into major ones.

Okay. In addition to merging driver fuel during the melt-out phase, in addition to driver fuel entering into the blanket assemblies and cementing them together, the driver fuel can also melt its way out into the radial assemblies. And in fact, once the driver fuel anywhere in the core melts its way out of its subassembly, it sees the inter-assembly gaps that were discussed this morning.

For example, if a driver fuel located at this point (indicating) were to escape or melt through a subassembly boundary, it would see the gap which is represented by a line in this diagram. That would remind you that the lines you see here separating assemblies really consist of three things: two adjacent disassembly walls as well as the 4- or 5-millimeter diamter gap between those assemblies.

So molten fuel can, in addition to getting into the blanket regions, and in addition to the driver fuel merging with other driver fuel assemblies, it can move out radially through these gap paths. It can also, as mentioned by Dr. Theofanous this morning, work its way between inner blanket assemblies and move axially downward out of the core.

Later on in time, the driver fuel can also penetrate through the hex can walls that surround the control rod assemblies, and it turns out a lot of fuel can be moved very rapidly, very quickly into these control rod assemblies.

In addition to an initial quick removal of such fuel, continuous flow of molten fuels into those control rods can be demonstrated, and I will talk a little more about this in a few minutes.

Not easily depicted in this core map is the fact that in some cases, depending upon whether we are dealing with an EOC or BOC core, driver fuel can we also believe move up into the axial blankets. So there are several avenues for fuel escape. And I will now go into a little more detail about the escape paths.

MR. EPSTEIN: Let me put up this vu-graph.

We have also covered point one, fuel removal paths are available.

Point two says fuel removal is sufficient to ensure permanent subcriticality even when assessed with conservative models.

Let me just go back for one minute, less than one minute, before I discuss what I mean by "conservative models."

I mentioned that we deal with the meltout phase, and the meltout phase is simply defined as the lifetime of the blankets. The meltout phase lasts as long as the blankets last. Once the blankets are melted away, we have entered the whole core pool phase. We don't consider that phase in our analyses because we're pretty much convinced that during the meltout phase, while the blanket assemblies are still intact, enough, more than enough fuel will be removed from the core from through these flow paths to render this core subcritical. And more than that 40 percent figure mentioned this morning will be removed from the core during the time the blankets are still intact, and that is true regardless of which freezing model we resort to.

There are two models, as you well know, I think. One is the conduction limited freezing model, and that says that the fuel penetration rate into any specific channel or between gaps into the control rods or what have you is controlled by the growth of a frozen fuel layer on the walls of that channel. And when that fuel layer completely fills the channel up the flow will stop.

With that model you can predict fairly long fuel penetration rates. The model was developed about 100 years ago, and over the past ten decades there has been enough papers that substantiate that model to fill this room.

On the other hand, we have a model that people also like to use in the fast reactor business known as the bulk freezing model; and this model has never been verified, to my knowledge, and it's based upon turbulent heat losses through the walls of the channel that the fuel is flowing through that channel. Specifically, it says freezing is controlled at a rate given simply by the turbulent heat transport from the flowing fuel to the channel wall.

As you might anticipate, this is a rather rapid freezing mechanism, and I guess that's why the model is popular, because it gives a lower bound to the freezing rate.

And what we have done is to assess fuel

removal with both of these models, and regardless of which model we use we come to the same conclusion: the core will be rendered subcritical during the course of the meltout phase while the blanket composites are still intact.

Let me go ahead and demonstrate what I have just said. Let me just say that we also looked at the requirements for permanent subcriticality. One does this by looking at various core configurations. I will not go through the details of these things, but essentially one comes to the same conclusion no matter which configuration you look at. If you can remove about 40 percent of the driver fuel from the core, the core will be subcritical.

In order to be able to convince ourselves that we can remove enough fuel from the core to render it subcritical during the meltout phase, we have to get some handle on the time scale for the meltout phase, and to do this we picked a specific example. We think it is a realistic example because we think that the core is going to, in the loss of flow accident, seek the conditions assumed -- a moderate power burst of 6 to 10 full seconds at the end of the initiating phase and then a power level of 50 percent following that. The power level of 50 percent is based upon what we know about heat losses on a subassembly scale from the molten fuel disrupted regions to tho hexcan boundaries. It tells us that 50 percent is just enough power to keep the core subcritical; so we picked a power level of about 50 percent.

We also have the following conditions: that the inner blankets have an average fuel temperature of 2100 degrees C. -- I should say clad temperature, I think, that is, of 2100 degrees C. -- and the lower clad segments at 1600 degrees C. Those are the conditions the driver fuel sees when it enters the inner blanket conditions.

Based upon this power level (Indicating) and what we think are reasonable ablation rates acting on the outside of this frozen driver fuel blanket composite, we find we will probably melt away the middle section of the composites in 12 to 16 seconds, but a lower stalagtite deposit will still be left sticking up from the bottom of the active core region into the core. That will last for a period of 35 to 40 seconds.

One might say well, suppose I double the power level. That will knock these values down by perhaps close to a factor of two. One would reduce the lifetime of the meltout phase -- that is, the lifetime of the blankets -- if one were to decide to take a higher power level. But you have to remember with a higher power level the pressure in the core is larger. It probably goes up something exponentially with power level, the relationship between pressure and temperature, and that increases the driving force for fuel removal.

So while you are reducing the time scale for the meltout phase, you are also at the same time reducing the time scale for fuel removal. And that's the purpose of this footnote here which is not really very sensitive to the choice of the power level. We picked this as an example, but we think it's a realistic example as far as the numbers we've picked are concerned.

Let's look in more detail at the fuel removal paths we are dealing with, and let's talk first about the gaps. Before I showed you a cut through the cross-section of the core. This is an actual cut through the core. This is the core region.

If fuel is going to move into the gaps, it will move out and displace the sodium in the radial blanket region and the radial shield region. It can also pass through the blanket/blanket gaps of the inner blankets and move downward as well below the core in escaping.

I might mention that when the fuel moves out of the core it has to push away the surrounding liquid sodium as well, and it turns out that the liquid sodium has to pass through the upper core load pad, and this load pad has sufficiently small holes to present a sizable impedance to the flow of sodium. That was also taken into account in our analysis of fuel removal from the core into the gaps.

And I think the results of that analysis on the effect of sodium impedance is shown in this vu-graph. We find that the penetration length is reduced by at most 40 percent. This 40 percent figure comes from assuming that all of the driver fuel is suddenly molten at a specific time, and it all escapes from the core at the same time. This produces the greatest sodium displacement rate, the greatest pressure, right, in the above-core load pad, and that's how we get the 40 percent figure.

While 40 percent may seem large, the conduction model predicts such large penetration distances to begin with that this reduction is relatively trivial and does not affect any of our conclusions. As far as the bulk freezing model is concerned, there's no effect on penetration length, and that is a result of the fact that in the bulk freezing the penetration length is independent of the pressure drop, and therefore, sodium impedance does not affect bulk freezing length predictions.

Okay. Let's take a closer look now at the control assemblies versus secondary control assembly. The molten fuel will be at the exterior of the control assembly and will probably melt through the control assembly in this region, and it has to melt through the hexcan first. Then once it does that it finds a large space, this 56 centimeter space, below the active core region. That space fills up very quickly and can remove actually about 5 percent of the core inventory in a time scale much less than 1 second.

That's not all, though. The fuel will continue to drain through this vent that you see and move out or move below to regions well below the active core zone.

We will summarize soon the time scale for the fuel removal processes and how much fuel can actually be removed as a percentage of all the driver fuel in the core.

A similar story here for the primary control assembly. In this case the molten fuel, driver fuel, only has to penetrate a single subassembly wall. It works its way down to a rather large volume below the active core region. This large volume in a time scale of less than one second can remove four to five percent of the inventory of driver fuel.

Below this large volume is an array of orifice plates which present some resistance to the fuel flow over a period of time of a few seconds. But calculations show it won't be long before the orifice plates are melted away by the draining fuel, and that speeds up the removal process through the primary control assembly.

MR. CARBON: Where are those orifice plates with respect to the bottom of the pool?

MR. EPSTEIN: The dimensions are not on there, but I imagine they are something like 40 or 50 centimeters below the active core region.

MR. THEOFANOUS: We think it is about a meter.

MR. FAUSKE: It is close to a meter.

MR. CARBON: You wouldn't have very much heat generation in the fuel at that point, would you? I guess I'm surprised it works its way through so rapidly.

MR. FAUSKE: Basically decayed heat.

MR. CARBON: Still, it works through rapidly?

MR. EPSTEIN: Are you talking about the melting away of the orifice plates? This is done by convective heat transport. The orifice plates are staggered such that once the fuel goes through one hole it sees a solid plate below it, and it has to move to the side.

But you have a geometry very similar to a jet impinging on a plate with a very high heat transfer coefficient associated with that. It rapidly melts its way through those plates by this jetting mechanism. The actual path of the plate is down, hits another plate, moves some distance, goes through another hole and does that kind of thing (Indicating) until the plates are melted away, and then it just drains straight down.

Okay. Let's summarize exactly how much fuel we can move through each one of these flow paths, and let's get back to the interassembly gaps.

Let's discuss the thermal conduction limited model. Let's assume first you believe in conducted limited freezing. After all, there's a hundred years of research behind it. I think it's a reasonable assumption to make. And, in fact, we think this is our best estimate case as far as fuel removal through the core is concerned; that fuel removal in through the gaps will be conduction-controlled.

There are so many gaps, and a cross-sectional area of the gaps is so large that within the time scale of one second if all of the driver fuel were molten, all of the driver fuel would be removed from the core through those gaps alone that I mentioned, the radial gaps and the gaps that carry the fuel downward between blanket gaps.

We are only considering here, by the way, as Dr. Theofanous mentioned this morning in his talk, the gaps between blanket/blanket assemblies.

MR. CARBON: What kind of pressure drop is there to push that out there?

MR. EPSTEIN: Gravity alone would just about do the job.

MR. CARBON: Gravity alone.

MR. EPSTEIN: I think this points to what a leaky situation you have. Once the fuel melts it's not a matter of being limited by freezing. In this circumstance the gaps are just waiting for the fuel to melt. As soon as the fuel melts it gives a little burp and moves right out of the core, according to the conduction limited model. That's why we say fuel melt is limited here. We just have to wait around for the driver fuel assemblies to melt, and that controls their motion.

And as I mentioned, this mechanism of flow into the gaps, thermal conduction, limited flow into the gaps, is sufficient to move all of the fuel. If you believed in thermal conduction, you wouldn't have to go further; the problem is over. If the core is subcritical, we don't even have to consider any of the other flow paths. But being a reactor safety analyst, we would like to put a little conservatism into the analysis.

Take a look at what happens if we make a conservative assumption. We pick out the bulk freezing model, and the bulk freezing model has very short penetration lengths associated with it. The time scale for the process to occur is about the same as the conduction limited process. The fuel will move into the gaps and freeze in a time scale of about one second, but because the penetration lengths are so short, it would just remove 10 to 15 percent of the core inventory of the driver fuel depending upon what kind of core you are dealing with, a BCC core or an ECC core. So if you postulate bulk freezing in the gaps, one has to look at other gaps as well.

The other paths I might mention are not open as soon as the gaps are open. The gaps are open as soon as the driver fuel works its way through the subassembly wall. In the case of the control rod assemblies specifically, the primary control -- no, the secondary control assembly, there is a six-second meltthrough time because the fuel has to go through two barriers. One is the subassembly wall, and then there is the guide, too.

But here are some of the time scales associated with this control rod assembly.

It takes four seconds for the fuel to enter the primary control assembly. As I mentioned before, that large volume just below the core will suck up about six percent of the core inventory, and the time scale is less than one second, and fuel will continue to flow at the rate of about one percent a second for four seconds. This four-second period is the time in which the orifice plates are still intact. The orifice plates melt away. That flow rate increases to two to three percent of the core inventory per second.

The secondary control assembly has a six-second meltthrough time. The longer time here is associated with going through the guide tube as well as the hexcan wall.

The volume just below the core in the secondary control assemblies will accept about four percent of the fuel in short order, and then the fuel will continue to be removed at a rate of five percent per second.

Remember, now, we are dealing with a time scale on the order of at least 12 seconds for the lifetime of the meltdown phase or the lifetime of the blankets -- most likely much longer, but that is one of the lower bounds.

Fuel can also move into the upper axial blanket, we feel, in the case of the EOC core, because in this situation, as a result of initiating phase analysis, there are limited clad blockages. We don't take credit for fuel removal into the upper axial blanket for the BOC core. We assume that the clad plugs prevent that from occurring.

There are also radial blankets. In addition to fuel penetrating the inner blankets, it will penetrate into the radial blankets as well, and that can remove 20 percent of the core fuel.

So when one sums up, let me go back and say again, let me repeat, if we base our analysis on conduction limited freezing in the gaps, one does not have to go beyond item number one here. The problem is over. That process will occur within one second of the driver fuel coming out of its assembly cans.

If you want to base fuel removal from the gaps on bulk freezing, then we have to consider these other paths, and this is how we sum up as far as the contributions during a ten-second period after fuel penetration into the inner blankets.

And you can see there are more than sufficient paths to remove more than all of the fuel we have in the core, indicating that during the lifetime of the meltout phase while the blankets are still intact and recriticality is not a problem, the core will be rendered subcritical.

I guess that about summarizes our new results on fuel removal. I might say that most of the new results have to do with taking a very careful look on flow into the secondary control assemblies. Prior to a month or two ago we were not taking credit for that, and after looking at the reactor plans, we found that considerable fuel can flow into those secondary control assemblies, so that is really the new feature of the fuel removal analysis since the last time we presented results at the ACRS meeting.

MR. CARBON: In terms of the fuel flowing out past the cold surface or relatively cold, what predominates the continued heat generation so the fuel stays hot or the fact there has been an excursion to raise the temperature quite high, and it has a long way before it freezes?

MR. EPSTEIN: It is reasonable to assume the fuel is slighly superheated, at least 50 or 100 degrees C. above its melting point. That helps somewhat. But let me go back again and repeat, for the gaps there are two possible freezing mechanisms you can postulate. One is the conduction limited freezing mechanism, and using that mechanism it's not too important what the superheat is or what the neutronic state of the core is. If you are dealing with molten driver fuel and you believe conduction freezing, that molten driver fuel will go a long way into those gaps because it is limited only by the relatively slow process of growth of fuel inward from the gap walls.

MR. CARBON: But even so, in the extreme case of one degree above melting it wouldn't take long.

MR. EPSTEIN: No. We still go quite a long distance even with one degree obove melting -- several hundred centimeters. It's like lava coming out of a volcano. Of course, you are dealing with a much wider flow tube in that situation, but those things go for miles.

MR. CARBON: They are not being cooled off very rapidly.

MR. EPSTEIN: Well, they can cool pretty rapidly by radiation.

MR. CARBON: That is debatable.

MR. EPSTEIN: In fact, radiation cooling could be almost as effective at these temperature as heat transfer to a solid steel structure.

I should mention in the control rod

assemblies, I should point out the differences between the gaps and the control rod assemblies as far as hydraulic diameter is concerned. In the control rod assemblies we have much less cross-sectional area for flow, and that is why it takes much longer than one second to get the driver fuel from the control rod assemblies, because we are dealing with relatively small cross-sectional areas compared to the gaps.

On the other hand, you're dealing with very large hydraulic diameters so you're not influenced by any model you pick. The hydraulic diameters are so large you can assess fuel escape by bulk freezing or conduction freezing.

MR. FAUSKE: Mike, you may want to mention the key thing here is the low thermal conductivity of the fuel. Hence, it takes a long time to form a fuel crust of sufficient thickness.

MR. EPSTEIN: Yes. They have a relatively low conductivity, and it takes quite a while for a crust to form, although it doesn't seem like it. It takes one second for the crust to close the gap, but in one second the fuel will flow a long distance through those gaps. But it doesn't take a tenth of a second or a hundredth of a second. One second is a fairly long time in terms of removing fuel even under gravitational driving forces.

MR. CARBON: I would believe your numbers, but just intuitively it doesn't seem it would flow that rapidly. I'm not arguing, but it just doesn't.

MR. EPSTEIN: I guess you have to sit down and make the calculation for yourself. It's not a difficult calculation. It can be done in a few minutes.

MR. LIPINSKI: I have a question. On the secondary control assemblies you said there's a six-second meltthrough and the removal was five percent a second. Yet, if I go to your summary table based on ten seconds, you have a total of 44 percent for the secondary control assemblies.

Is that consistent if I have five percent per second?

MR. EPSTEIN: I think you have to take into account or recognize we are talking about ten seconds after fuel penetration into the inner blanket assemblies.

MR. LIPINSKI: All right. Where is that biased six-second meltthrough time on a secondary control assembly?

MR. EPSTEIN: It's actually 14 seconds because while we are waiting around for the fuel to penetrate the inner blankets, at the same time the driver fuel is also working its way on the control assemblies for that period of time.

MR. LIPINSKI: All right. So time zero we start, and in six seconds we melt through the secondary control assembly.

MR. EPSTEIN: Then you have about eight seconds after that I think to fill up the control assembly, eight seconds of flow time. The ten seconds really refers to the lifetime of the inner blankets after the driver fuel penetrates into the inner blankets. While the driver fuel is acting on the inner blankets, it's also acting on the control rod assemblies.

MR. LIPINSKI: I guess the question is what is the time when it penetrates the inner blankets after time zero.

MR. FAUSKE: Four seconds.

MR. EPSTEIN: Four seconds.

MR. LIPINSKI: Four seconds. Okay.

MR. CARBON: Are you finished?

MR. FAUSKE: If there are no further questions for Dr. Epstein, we would like to bring on Dennis Switick to provide some summary remarks.

MR. SWITICK: I am Dennis Switick with the General Electric Company in Sunnyvale, California, and I just wanted to take a few minutes after such a long day to try to summarize a few comments and some conclusions as to where the project is relative to HCDA energetics.

and in particular where we have come since we talked to you, since we had the opportunity to talk to you last May.

First of all, at that time we presented the evaluation we had performed, as documented in GEFR-523, covering all phases of the accident. And basically, we have tried to leave you with three conclusions.

The first conclusion was that the progression of an uncontaminated event in the CRBR core led to non-energetic termintion, either partial or whole core involvement.

The second conclusion we made was that it took significant deviation from our best estimate understanding of phenomenology to generate an energetic-type termination to the accident.

And that thirdly we concluded that the change in the design to a heterogeneous core was beneficial in that that core design was less sensitive and less likely to get into an energetics scenario than the homogeneous core was.

That is basically the three main conclusions we had last May. Since that time we have done a considerable amount of work both on our own initiative and in responding to the areas identified by the NRC staff and their consultants. You have heard some of that in a rather brief form by the last couple of speakers abbreviating their discussions, of course, to try to save time.

What I would like to do with one simple vu-graph is try to summarize some of the points of where we are today based upon where we were in May relative to those conclusions.

The first one simply is that we did complete a package of work and submit it to the NRC staff, I think in probably early September, responding to the various issues that they had identified relative to energetics, many of which were discussed today by the NRC and ourselves.

One of the things that has occurred, in our opinion, since last May and has been confirmed both by the staff and in our own thinking is that the TOP initiator in and of itself is not really an energetic type event that we need to be concerned with. In particular, we would say that the focus on the loss of flow accident is the appropriate one.

Secondly, we have gone and are still ramping up at this point through a detailed re-evaluation of the loss of flow event, including a re-evaluation of the sodium void, as Dr. Henryson discussed with you, a re-evaluation of fuel motion and the effect of plenum and fuel fission gases and cladding relocation.

And putting all of the phenomenology together and taking another hard look at it with the help of Argonne laboratories, as Dr. Weber presented to you, we still conclude that the best estimate progression of an unterminated event in this core would be a benign event energetically.

Going on from there into the meltout and annular pool phases where we have done some additional work that Dr. Epstein was just referring to, we think we are in a lot better shape than we were last May. That's because taking a detailed look at the life of the internal blankets relative to the time frame for fuel removal for permanent subcriticality, we think we are a lot less sensitive to the exact timing and details of these fuel removal processes.

We have also identified for ourselves and feel comfortable with additional fuel removal paths that we had not taken credit for that last time, those being specifically the continued flow of materials out the secondary control assemblies, as well as fuel entrance into the first row of radial blankets which can significantly hold something like 20 percent of the core materials. Those two flow paths would be available on the same time frame that the internal blankets still exist, and this is in the conservative aspect of our understanding where we are going to assume that we did not get sufficient fuel out through the gaps early on.

So even in that conservative scenario we've identified several major new fuel loss paths active during that time interval before you get to a corewide pool situation.

There was one point asked explicitly by the consultants that has not been discussed in any real detail today, and that is this next to the last conclusion. It had to do with a question relative to if you had an open boiling pool system where stainless steel was leaving that system as a vapor, could you get an effective event where the rapid condensation of that steel up in the sodium pool above it could generate a reverse pressure gradient and suck liquid sodium down into the pool. That was one of the response areas in the list of NRC issues that we completed in early September.

Now, our basic conclusion on basic physical principles was that you could rule out any such re-entry of liquid sodium into the pool. I don't know if anything is in the package.

MR. FAUSKE: Yes.

MR. SWITICK: There was something in the

package Dr. Epstein presented that he did not discuss, but the details were in there and the physical principles. It's basically due to the volatility of the sodium itself. And if you're going to condense the steel, you've got to be vaporizing sodium in such a manner that it cannot come back into that pool.

Putting all of these things together as to where we were and what we have been looking at over the last half a year, we still feel the basic conclusion that the 661 type megajoule expansion designed to judge the margin in the structural evaluation for the plant is quite adequate in terms of evaluating its energetics.

MR. CARBON: I'm not sure what that last sentence says. Are you saying there is a capability of it to withstand 661 megajoules? It's a question of what you mean.

MR. SWITICK: This statement doesn't directly say this. This statement directly says that the level of energetics one should consider in terms of judging the structural evaluation was chosen originally some long time ago at a number like 661 megajoule expansion at one atmosphere. That energetic type event, if you will, or that level of loads on the system is what I am saying is totally adequate for judging HCDA energetics.

The project is, of course, going to --

MR. CARBON: Excuse me. I'm still not with you on what those words say. The 661 megajoule expansion provides margin, adequate margin. Would you go over it once more?

MR. SWITICK: Okay. What we are basically saying is we don't see any way you can generate an energetic event in the CRBR core that would approach that type of level of expansion.

MR. CARBON: Okay. That I understand. The words I guess I don't.

MR. LIPINSKI: But the 661 is dictating the design of the containment, is it not?

MR. FAUSKE: Not the containment. The primary system.

MR. SWITICK: Not the containment. The primary heat transport system is being designed relative to the loads that would be calculated for such an expansion on the primary system.

MR. LIPINSKI: This is head lift.

MR. SWITICK: The head, the vessels, the piping, the whole system.

MR. LIPINSKI: How many megajoules are involved in the containment? Is that 1200? I have forgotten the number.

MR. SWITICK: In the containment building?

MR. LIPINSKI: Yes. There was another number where NRC had given you the specification.

MR. SWITICK: Sometime ago the NRC had judged a 1200 megajoule expansion would be appropriate. That is generically the same number that would be used to evaluate the primary heat transport system response, so that is comparable.

MR. CARBON: I have one other general question I have asked you before, and you have answered me before. The French, Germans and UK people tend to think that they don't gain anything from a heterogenous core. When I was in Lyons in July I again asked the question of people there, and I keep getting the answer they have looked at a heterogeneous core, and they finally end up saying so what, why bother.

Do you have any new thoughts to shed on that?

MR. FAUSKE: Maybe I could address that, Max. I am not sure whether you refer to the safety of the reactor or the operational aspects.

MR. CARBON: The safety of the reactor.

MR. FAUSKE: I think you heard today very well from Dr. Theofanous' presentation as well as our own presentation that we think there is certain mitigating aspects brought out by the heterogeneous core design. Certainly the fact that the effect of the sodium void worth is reduced is a positive thing from the reactor safety point of view.

I don't think we should debate that. I think that clearly moves us away from the typical area of the LCF-driven TOP, particularly from the area where the phenomenology is not that validly established based on experimental facts. So that is a very gratifying thing, I think, in terms of the reactor design.

Furthermore -- and I think this is something you wouldn't expect in the European community, because I think we are quite a bit ahead of them in terms of recriticality analysis -- and that is that the heterogeneous design basically provides increased time, as indicated by Dr. Theofanous as well as Dr. Epstein's presentation -- leads to more time to remove fuel during the stage in the accident progression whereby we can confidently rule out escalating recriticality events.

This to us is a very significant aspect of the accident analysis in the sense that we can comfortably reduce the possibility of getting into a large-scale, full core pool phase where a large-scale motion becomes more difficult to rule out.

I would like to say in this case from my own personal point of view I am not that ready yet to accept even in a whole core large-scale pool phase that we would have extreme difficulties, but I do appreciate some of the concerns the folks have.

But again, to summarize, I think there is a definite aspect to the heterogeneous core design that points to a more benign accident progression dealing with a hypothetical core disruptive accident.

MR. CARBON: I know that you do, and I guess you are saying basically that you think we are ahead of them in the analysis.

MR. FAUSKE: No question about it.

MR. CARBON: Do you have more?

MR. LIPINSKI: No.

MR. CARBON: Does anyone have anything else to add before we adjourn?

(No response.)

MR. CARBON: Let me thank Mr. Switick and let me thank everyone for coming today. I think it has been very good, and we can adjourn now.

(Whereupon, at 5:32 p.m., the meeting was adjourned.)

## NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

in the matter of: ACRS/Subcommittee on Clinch River Breeder Reactor

Date of Proceeding: November 19, 1982

Docket Number:

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcrip thereof for the file of the Commission.

Sharon Filipour

Official Reporter (Typed)

Sharon Filipour

Official Reporter (Signature)

# SYNOPSIS

I. MANAGEMENT GROUP
Purpose, Organization, Approach.

II. MANAGEMENT PLAN
Structure, Tasks, Status

III. LOFA ENERGETICS RESULTS
Probabilistic Framework, Discussion of Components

IV CONCLUSIONS

Fig. 1.

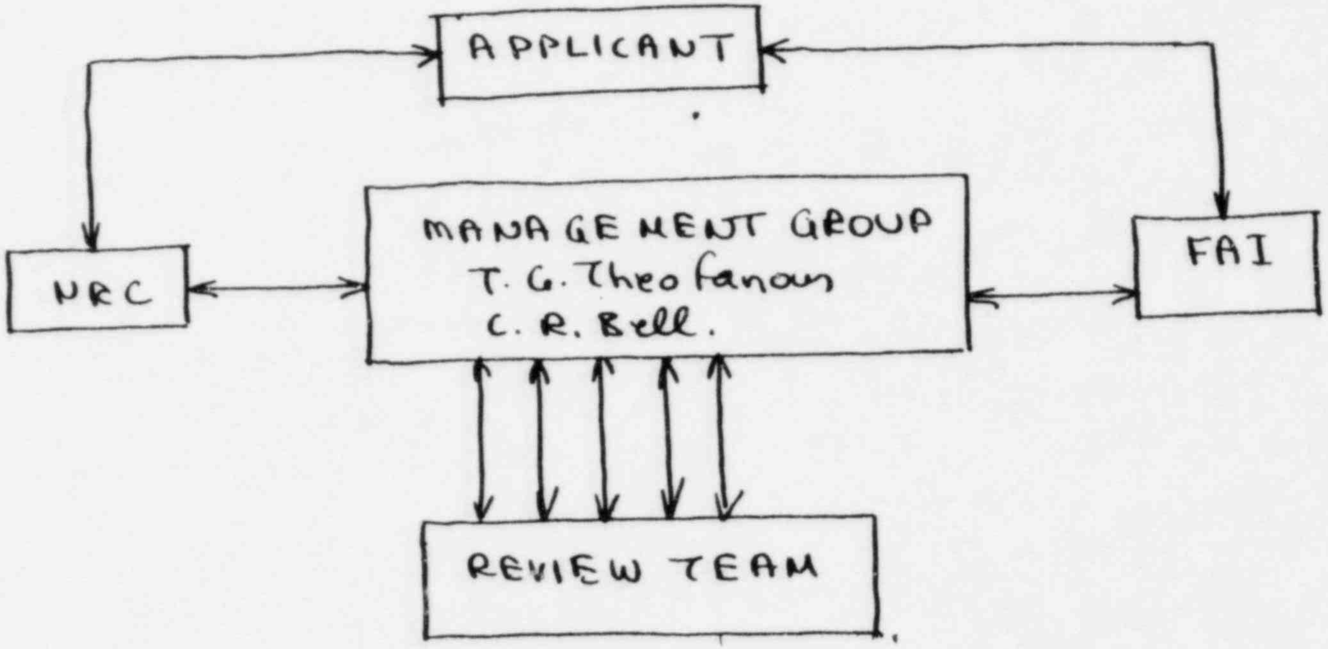
## I.1 PURPOSE OF THE MANAGEMENT GROUP

PLAN AND IMPLEMENT THE DEVELOPMENT OF AN INDEPENDENT POSITION ON CRBR ENERGETICS.

- MANAGE & FOCUS ACTIVITIES OF NRC CONSULTANTS
- ELICIT ADDITIONAL CONTRIBUTIONS FROM THE APPLICANT
- PROVIDE TECHNICAL SUPPORT TO NRC STAFF'S ONGOING LICENSING ACTIVITIES.

"Review" Process

## I.2 ORGANIZATIONAL INTERFACES.



# I.3 APPROACH.

- SHOW THAT LOFA SPANS RANGES OF PHENOMENOLOGY OF INTEREST
- PROVIDE A RANGE OF LOFA OUTCOMES (ENERGY YIELDS AND PARTITIONS) AND CORRESPONDING PROBABILITIES
    - OUTLINE COMPREHENSIVE RANGE OF SEQUENCES
    - SCRUTINIZE FOR ENERGETICS-FAVORABLE PHENOMENA
    - SCRUTINIZE FOR TERMINATION-FAVORABLE PHENOMENA
    - SCREEN-OUT PHYSICALLY UNREASONABLE SITUATIONS
    - QUANTIFY EACH OUTCOME
    - SYNTHESIZE SEQUENCES AND LIKELYHOODS
- SCOPE ALL OTHER CDA INITIATORS AS CONTRIBUTORS TO RISK FROM ENERGETIC BEHAVIOR.
    - RANGE OF DRIVING CONDITIONS, ESCALATION, RECOVERY MARGINS.

# I.4 IN PARTICULAR

**WE ARE NOT TO CLAIM**

- DETERMINISTIC PREDICTION OF CDAs IN ALL THEIR DETAIL.
- IMPOSSIBILITY OF RECRITICALITY

**RATHER THAT WE CAN**

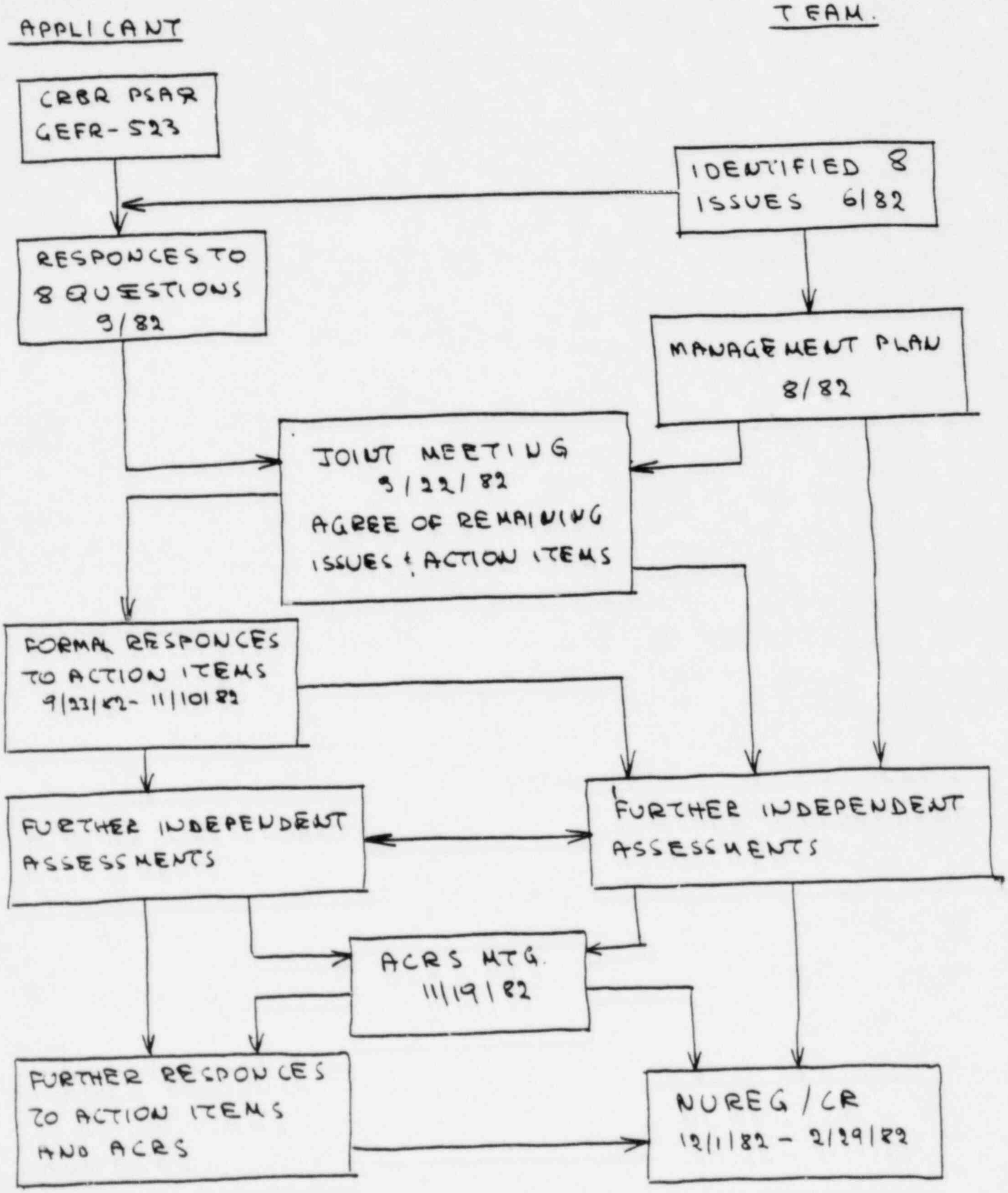
- ASSURE ABSENCE OF AUTOCATALYTIC BEHAVIOR
- REASONABLY BOUND SEVERITY OF ALL PHYISICALLY MEANINGFULL EXCURSIONS
- IDENTIFY PATHS FOR TERMINATION.

# I.5 TOOLS

- SYSTEM CODES
  SAS3D & SIMMER-II
- SPECIAL PURPOSE
  ANALYTIC METHODS
- IN-PILE EXPERIMENTAL
  DATA
- OUT-OF-PILE EXPERIMENTAL
  DATA

\+ ENGINEERING
JUDGEMENT.

# I.6 THE REVIEW PATH.



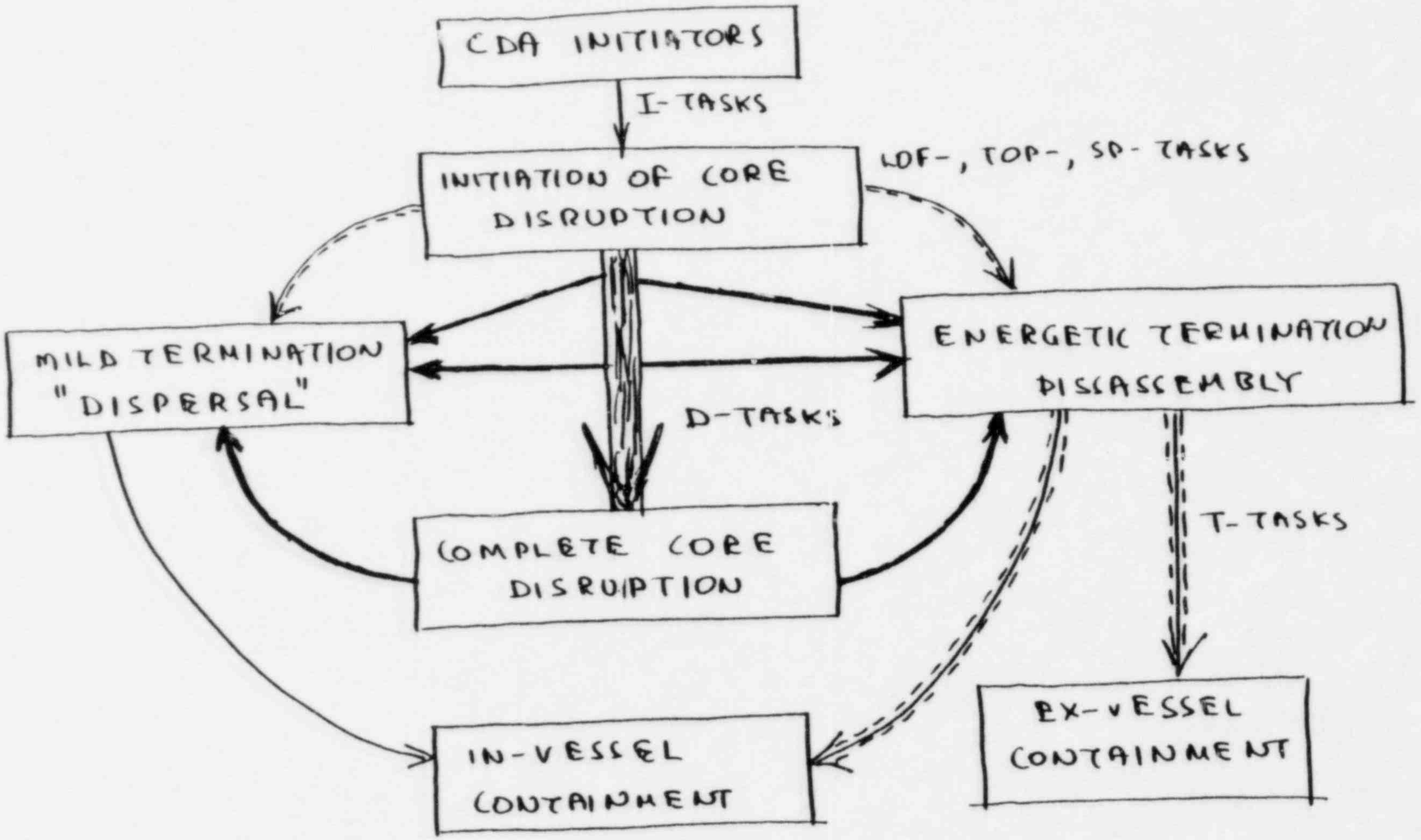
## I.7 MAJOR REVIEW ACCOMPLISHMENTS TODATE

- REVISION OF SODIUM VOID WORTH VALUES
- INCORPORATION OF PLENUM FISSION GAS EFFECTS
- QUANTIFICATION OF ORIGIN & SEVERITY OF RECRITICALITIES
- REVISION OF ENERGETIC RELIEF PATH.

<u>THEREFORE</u>

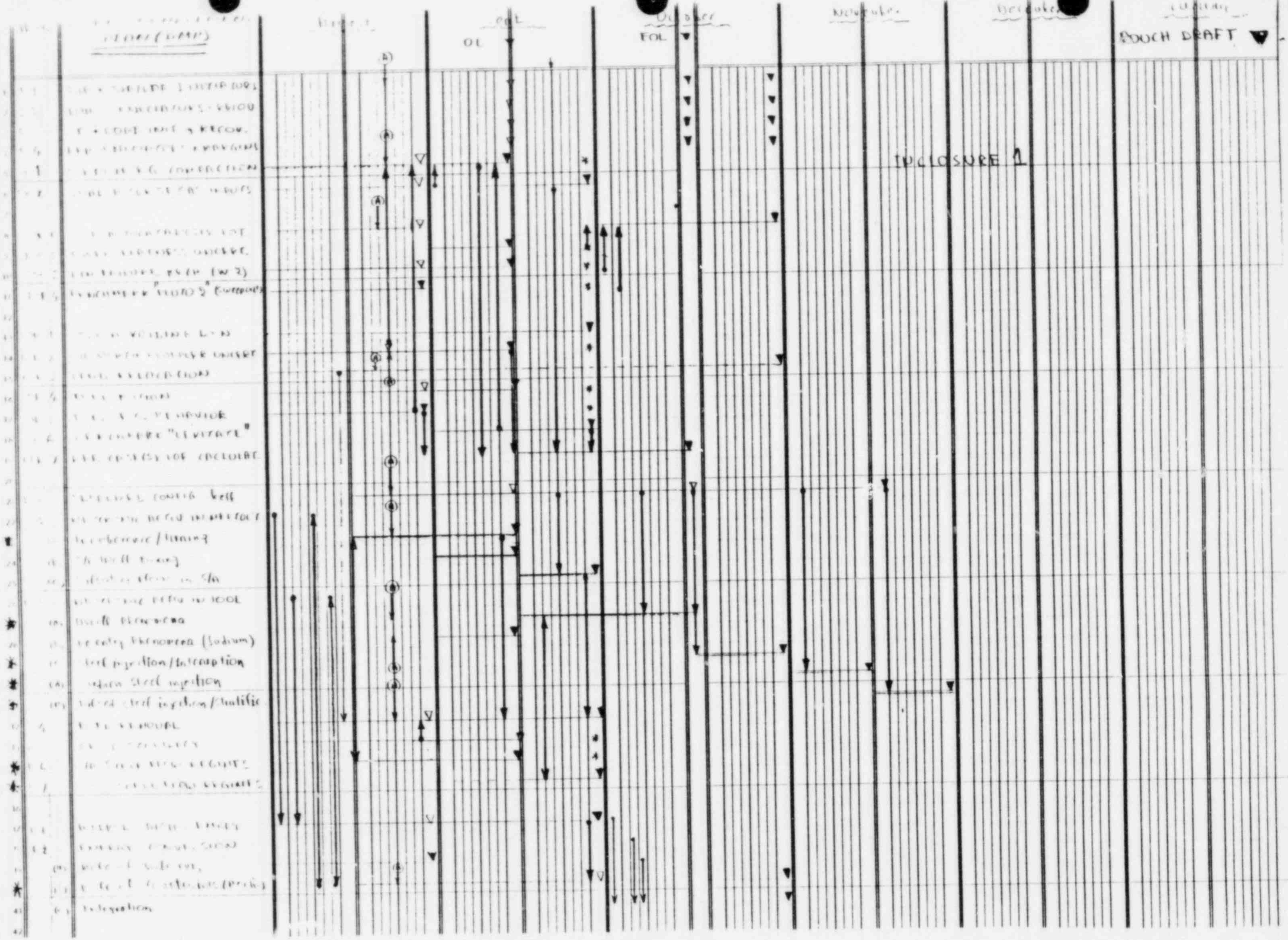
WE CAN CLAIM TO HAVE SIGNIFICANTLY IMPROVED THE REALISTIC UNDERSTANDING OF CRBR CDA'S.

## II.1. STRUCTURE OF MANAGEMENT PLAN.



ENCLOSURE 1

ROUGH DRAFT ▼



# II.2 SAMPLE TASK DEFINITION

## SP-1 PLENUM F.G. COMPACTION

### Objectives

Show that autocatalytic behavior is extremely unlikely. Establish a range of realistic LOFA initiating-phase power history outcomes.

### Scope

Consider in detail fission gas inventories, blowdown constraints and accident timing margins. Consider incoherent core behavior. Consider the effect of fuel motion history (early). Take into account Na worth uncertainties. Consider R8 experimental information.

### Output

Provide initiating phase power histories and enthalphy distributions for a range of conditions. Document one or two cases in detail adequate to visualize the scenario and sequence of processes. Highlight remaining areas of uncertainty.

### Schedule

Preliminary assessment August 30. Final report September 15.

### Resources

SAS3D, LEVITATE (SAS4A)

### Inputs

LOF-2, LOF-5, SP-2, LOF-6

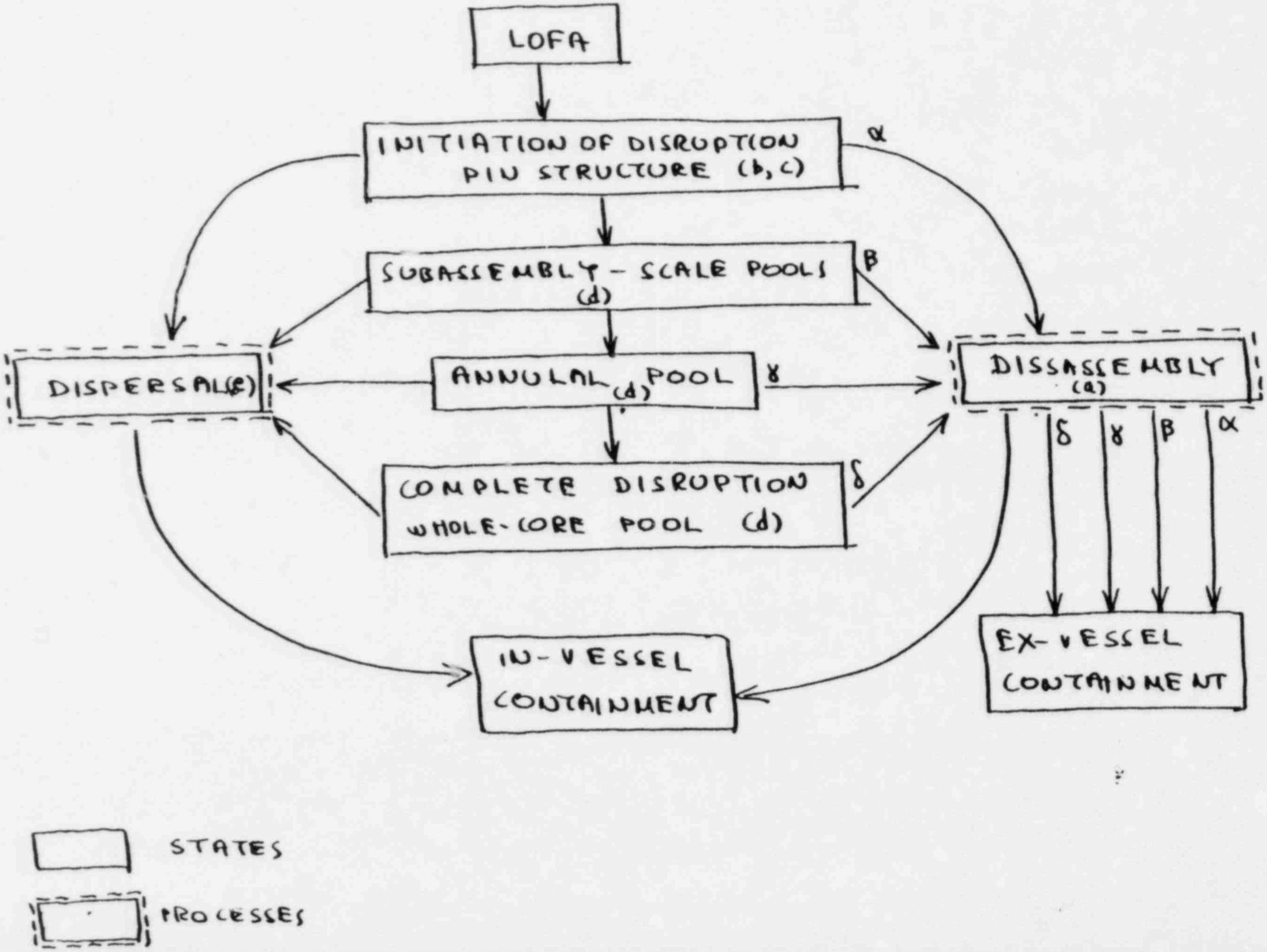
## II.4 CURRENT STATUS

- REACHED CONSENSUS WITHIN TEAM ON APPROACH MANAGEMENT PLAN AND TASKS
- REACHED CONSENSUS WITH PROJECT ON CRUCIAL POINTS OF ASSESSMENT
- ESSENTIALLY COMPLETED ASSESSMENT OF LOFA.

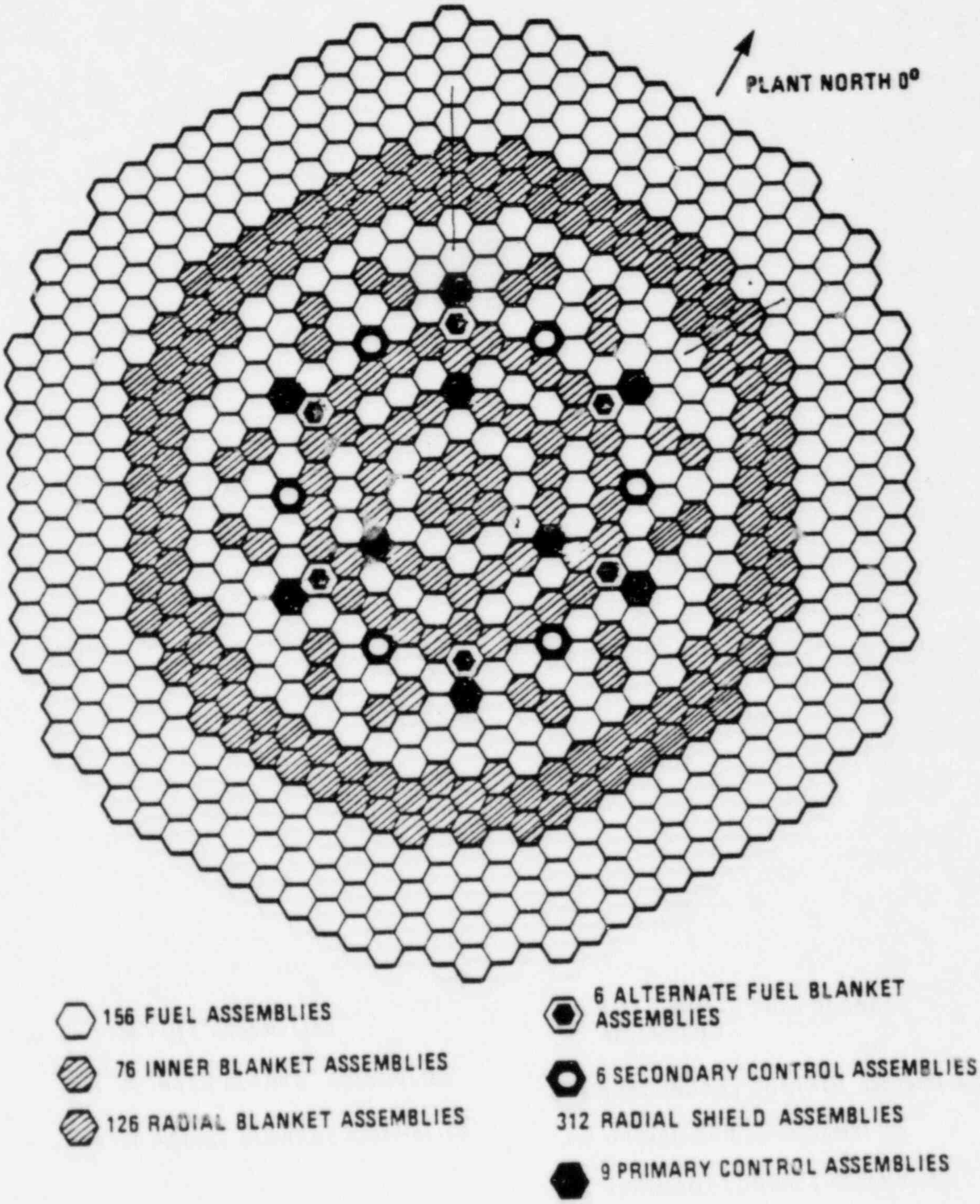
### REMAINING

- COMPLETE I-TASKS
- DOCUMENT ALL DETAILS.

# III.1. QUALITATIVE PROBABILISTIC FRAMEWORK



# III.2 CRBR CORE CONFIGURATION



## III.3 DEFINITION OF PROBABILITY SPLIT LEVELS

1/2 NO PREVAILING EVIDENCE AVAILABLE

∼1/10 BEHAVIOR WITHIN KNOWN TRENDS BUT OBTAINABLE AT THE EDGE-OF-SPECTRUM PARAMETER VALUES.

∼1/100 BEHAVIOR CANNOT POSITIVELY EXCLUDED BUT OUTSIDE THE SPECTRUM OF REASON

∼1/1000 INCREDIBLE BEHAVIOR VIOLATING WELL-KNOWN REALITY CAN POSITIVELY ARGUE AGAINST ITS OCCURANCE.

# III (a) 1. ENERGETIC TERMINATION REQUIREMENTS

RAPID REACTIVITY INSERTION $\dot{\rho} > 30$ \$/s

∴ RAPID MATERIALS RELOCATIONS

- Sodium worth $< \$2.00$ ⇒ must void in $t < 0.07$ s
- Cladding worth $< \$5.00$ ⇒ must remove in $t < 0.2$ s
- Fuel worth $\sim 1$ \$/cm ⇒ must compact with $v \sim 30$ cm/s

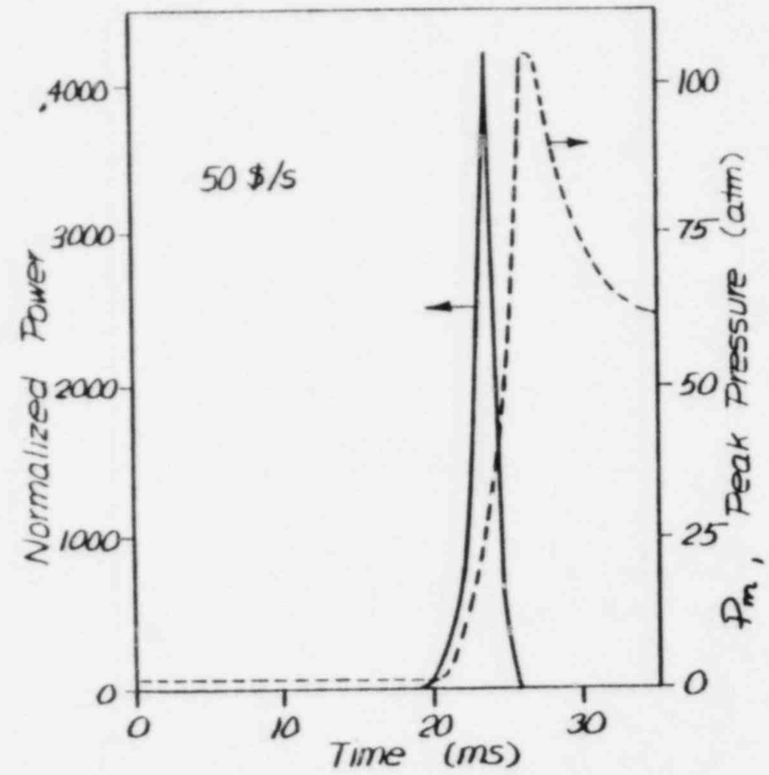
ALSO

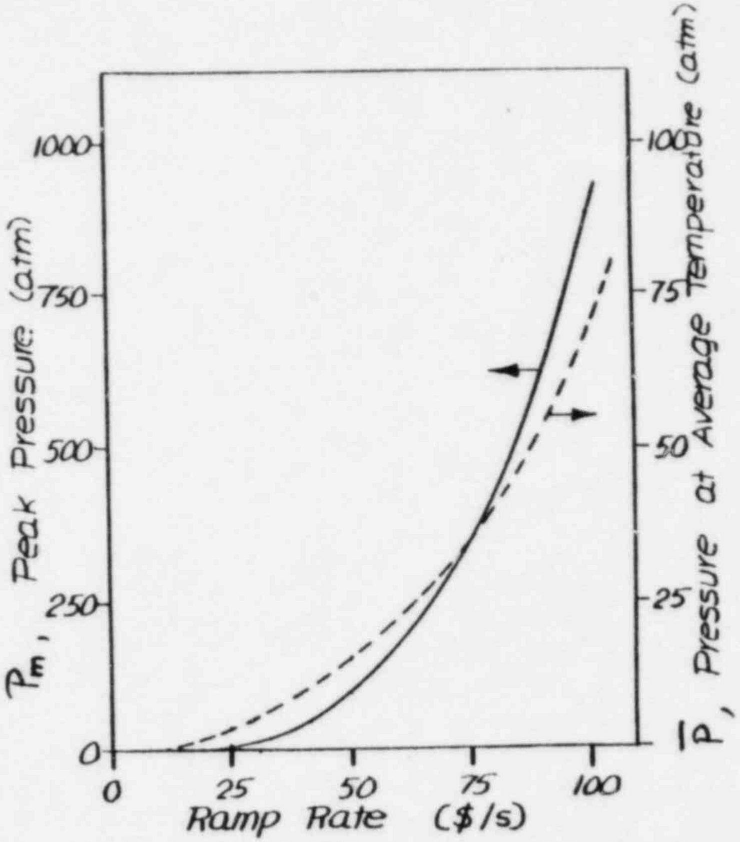
156 Drivers / Core ∴ Inter S/A Incoherence
219 Pins / Driver ∴ Intra S/A Incoherence.

ALSO NEGATIVE REACTIVITY FEEDBACKS DUE TO

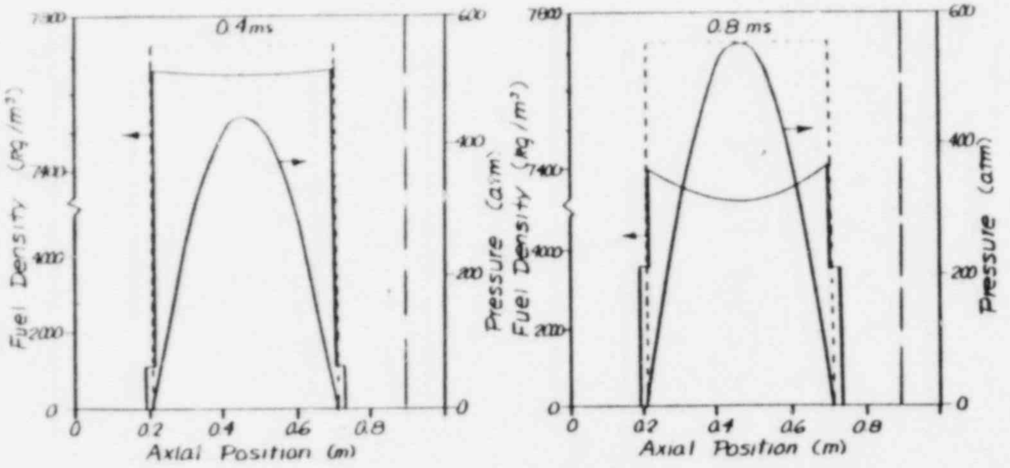
- Doppler
- Axial Expansion
- Vapor and fission gas pressures.

## III (a) 2 ENERGETIC TERMINATION TRENDS

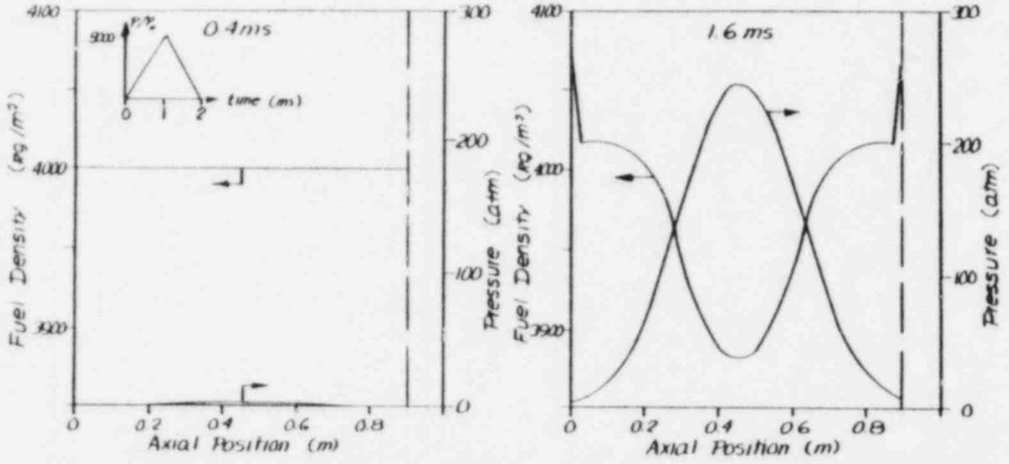




III (a).3 SINGLE vs TWO-PHASE DISSASSEMBLIES.

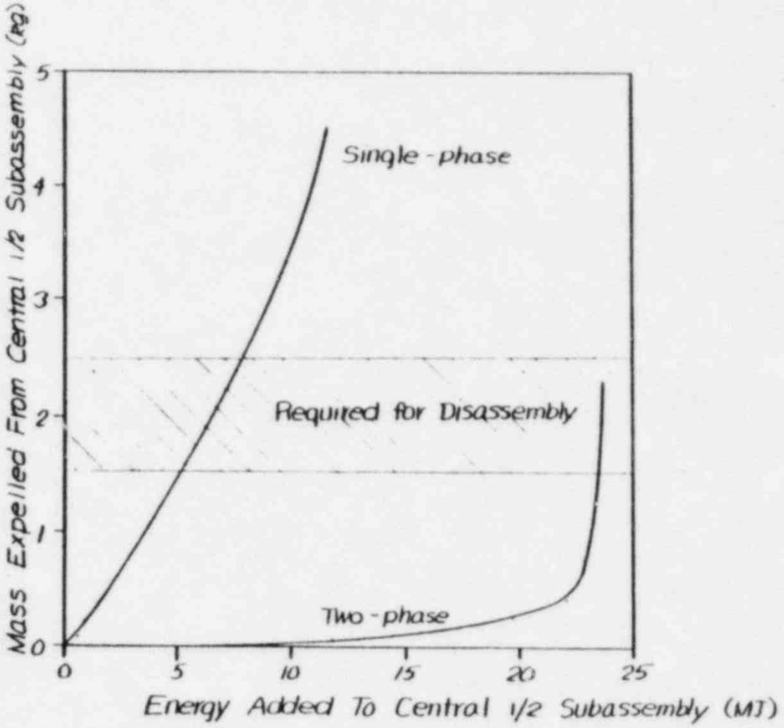


Single-phase Liquid Disassembly

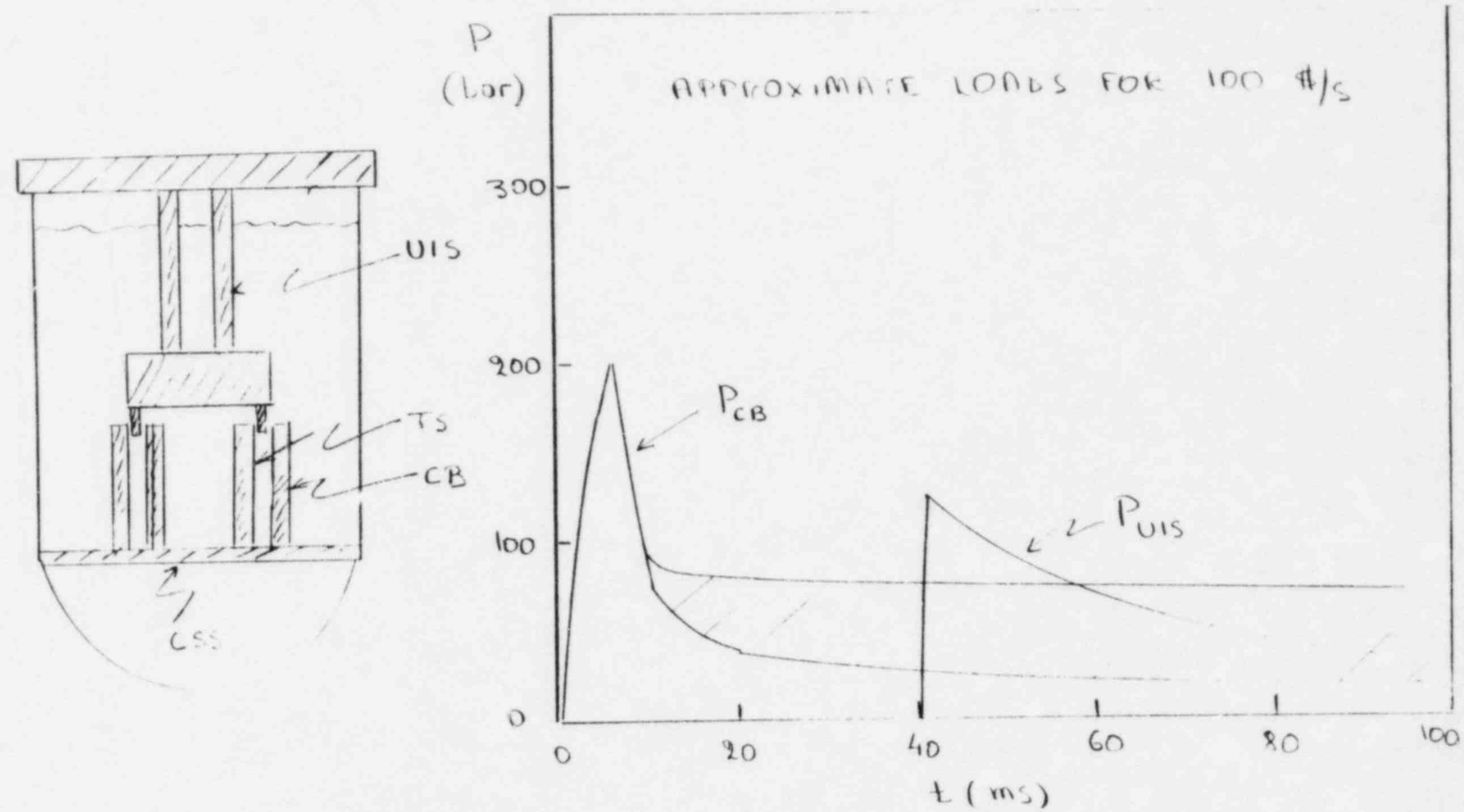


Two-phase Disassembly

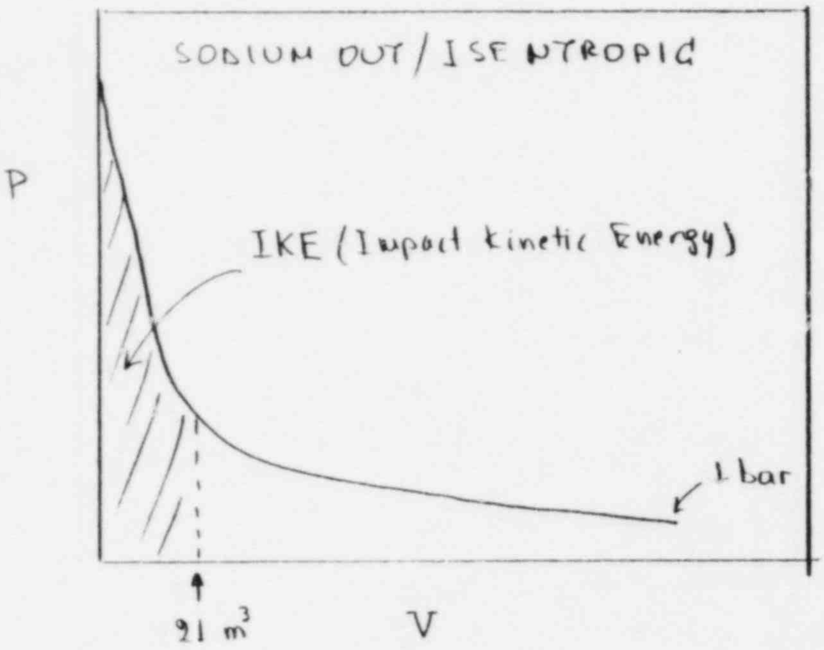
~ 60 $/s

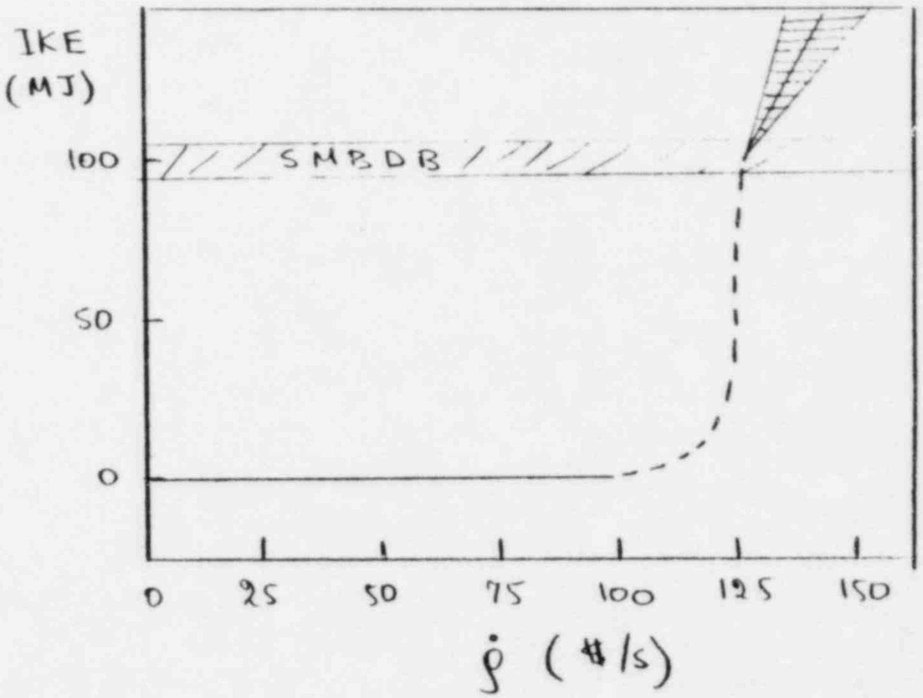


# III (a).4. DAMAGE POTENTIAL



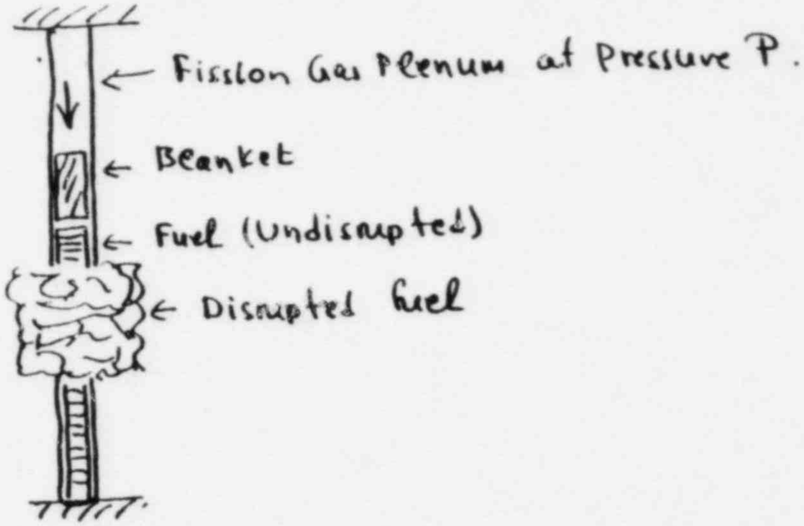
# III(a).5 DAMAGE THRESHOLDS





# III(b) 1 PLENUM FISSION GAS INDUCED FUEL COMPACTION

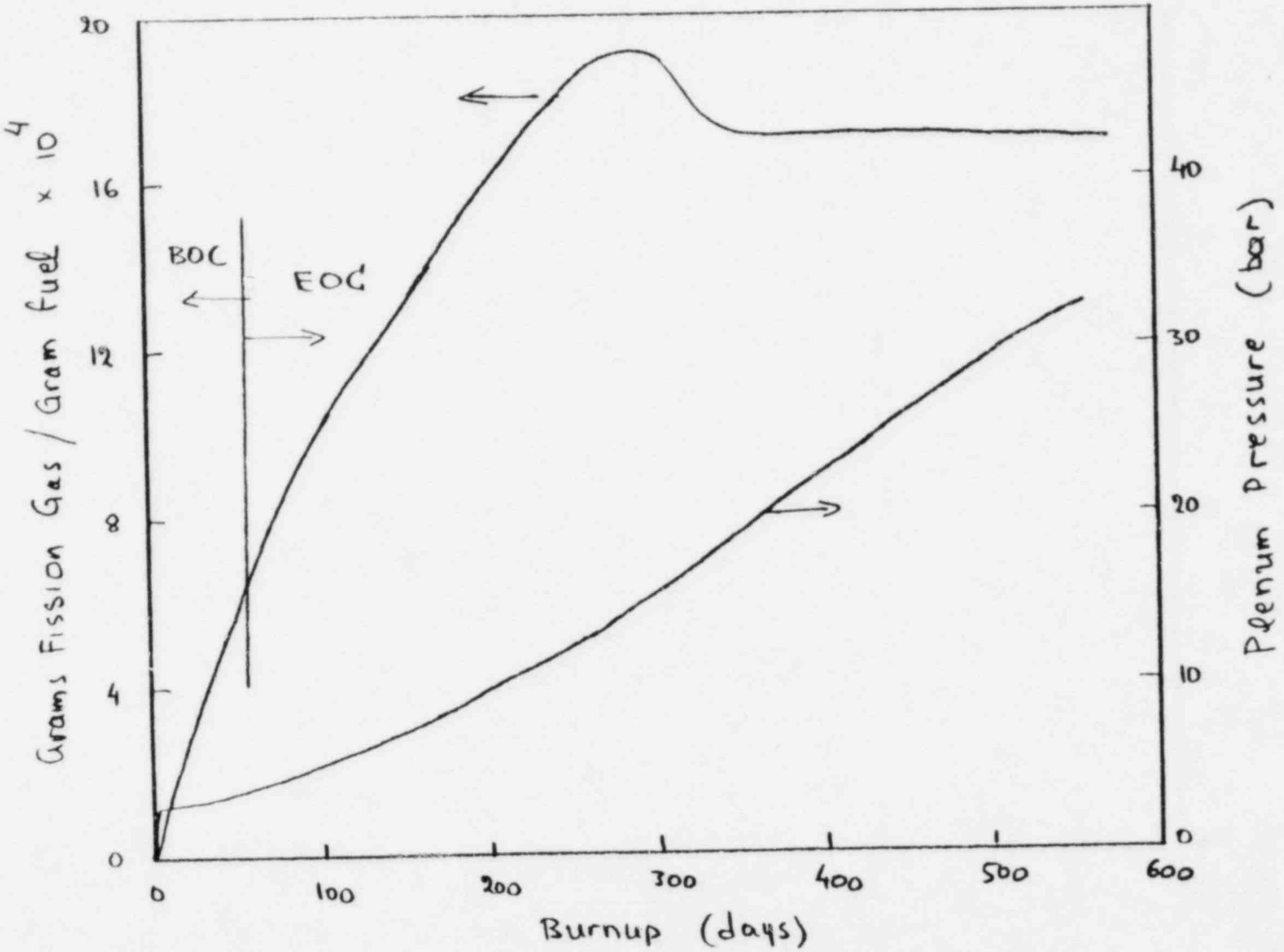
THE MECHANISM



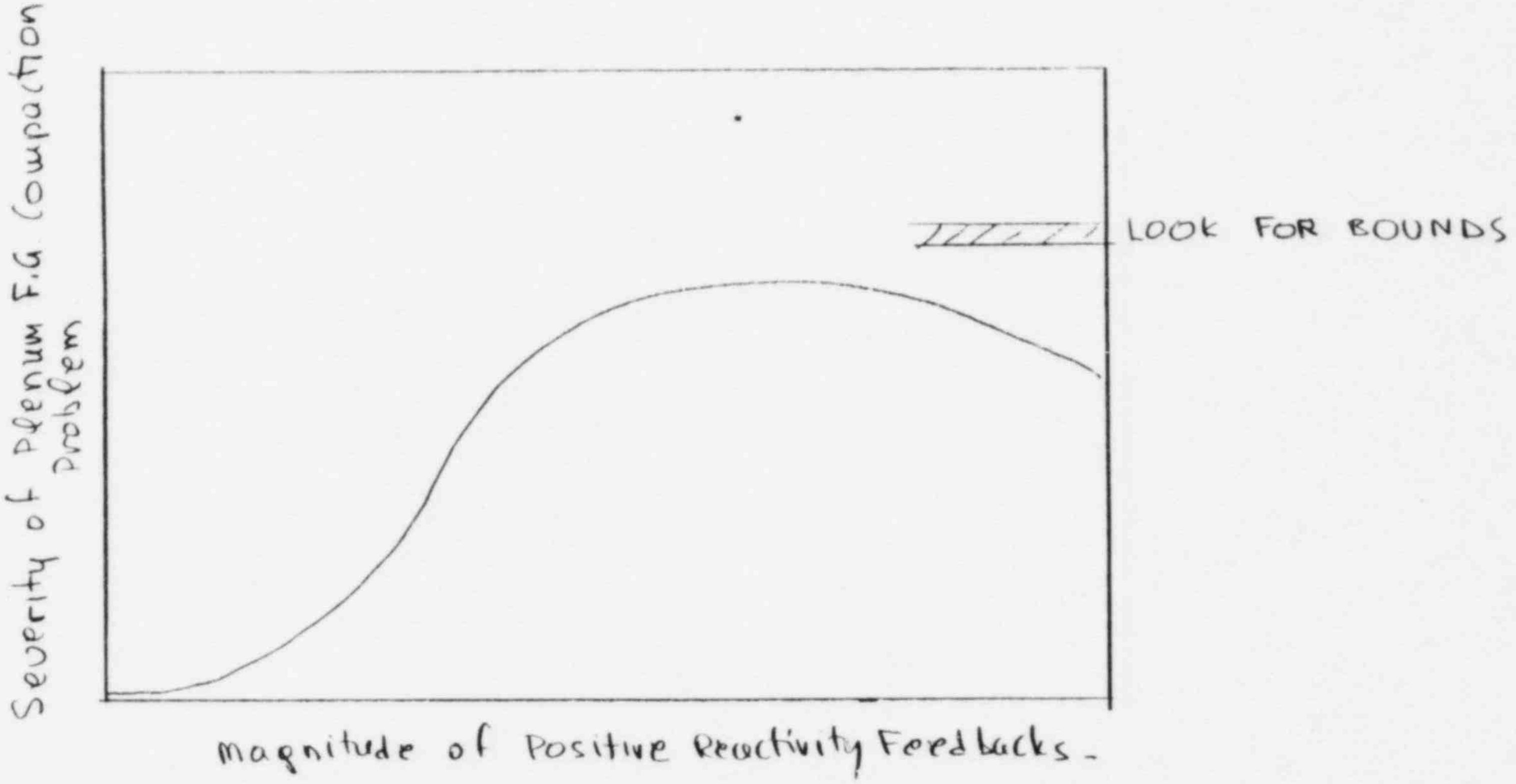
KEY PARAMETERS

- STORED PLENUM F.G. PRESSURE
- TIMING BETWEEN CLAD FAILURE & FUEL MELTING
    - Sodium worths and voiding rates
    - Clad failures and relocation rates
    - Relocation trends of disrupted fuel
- PELLET / CLADDING FRICTION.

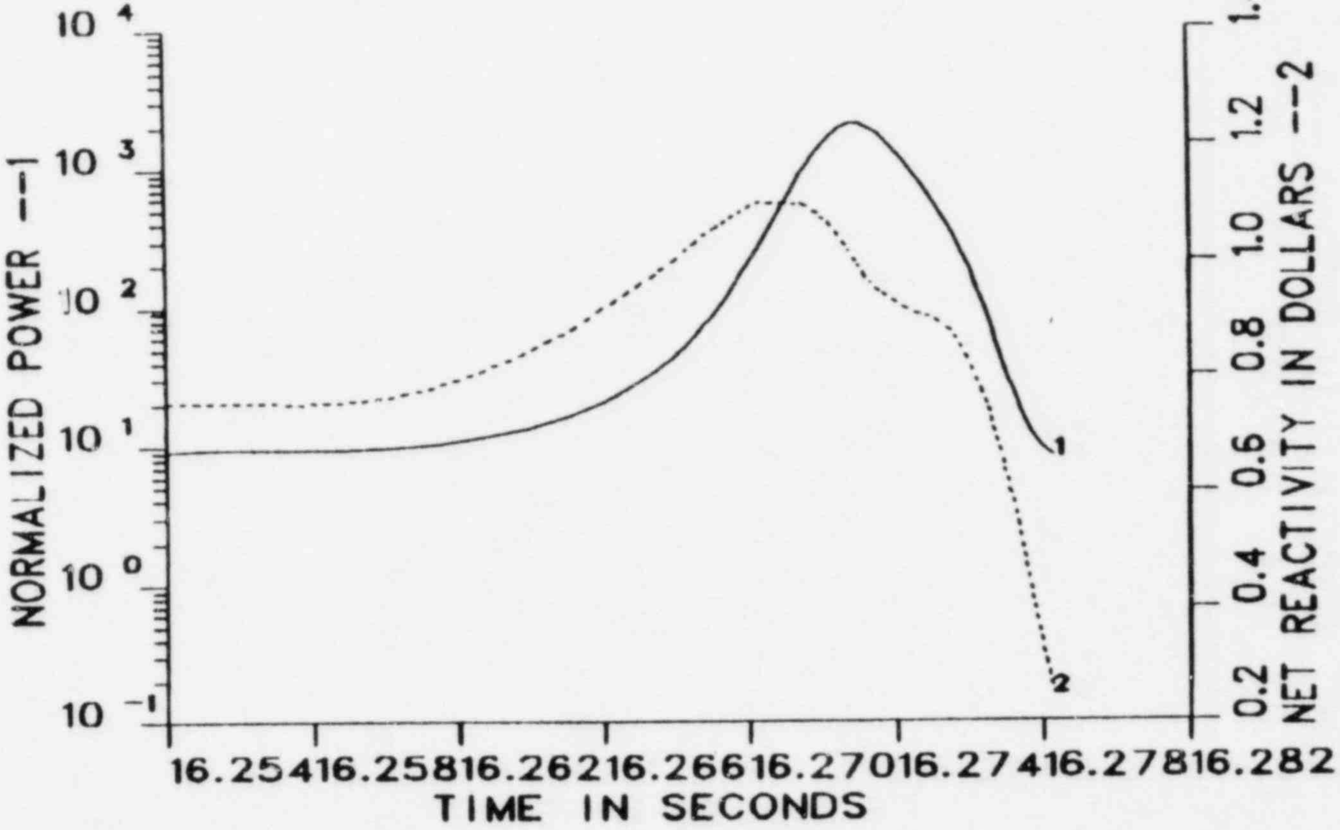
# III (b).2 KEY PARAMETERS (CONT'D).



## III (b).3 KEY PARAMETERS (CONT'D).



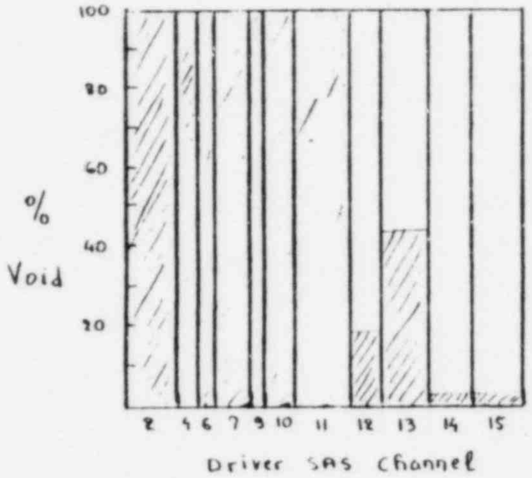
III (b)4 SAS3D PREDICTED BOUNDS OF POWER AND REACTIVITY TRANSIENTS FOR PLENUM F.G. COMPACTION.



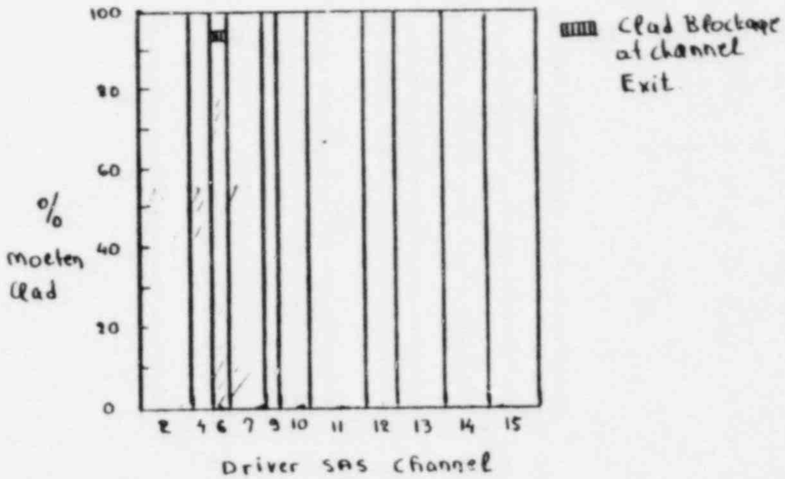
REFERENCE CASE WITH NEW CLADDING RELOCATION MODEL BURST PHASE
POWER + REACTIVITY VS TIME

# III(b).5 SAS3D MATERIAL PATTERNS

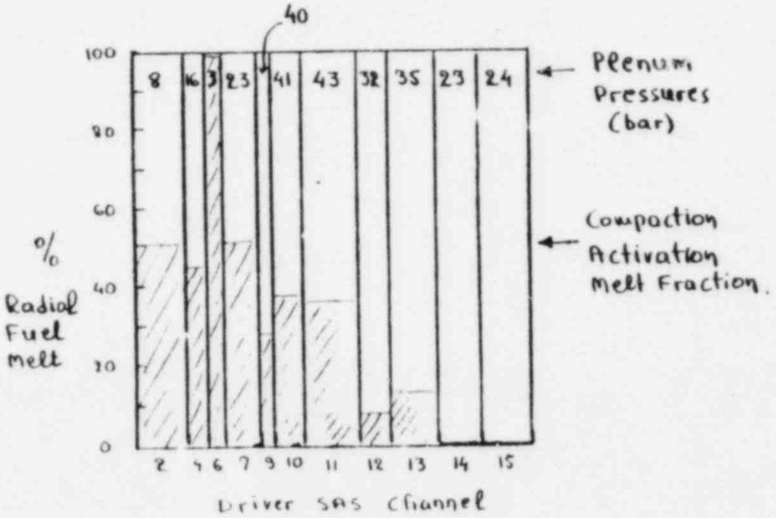
Sodium Void Pattern.



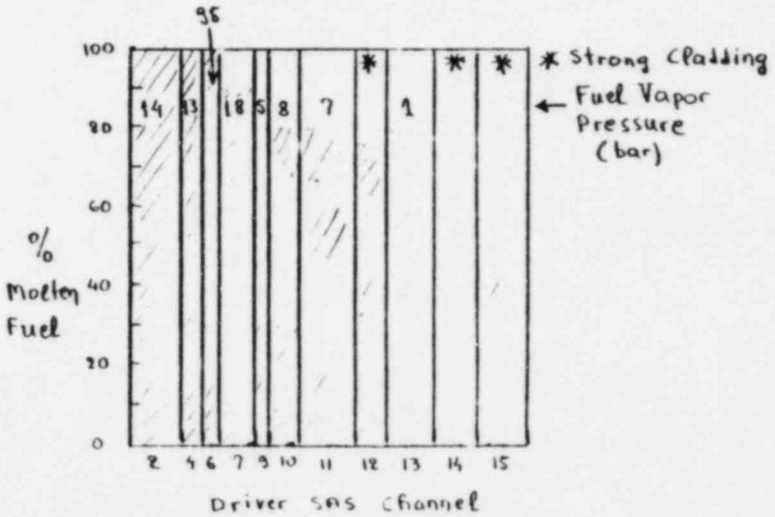
Cladding Melt Pattern.



Max. Radial Fuel Melt Fraction



Post-Burst Fuel Melt Pattern.



## III (b).6 SUMMARY.

- NON-AUTOCATALYTIC BEHAVIOR DUE TO INERTIA AND FINITE DRIVING FORCES FOR COMPACTION.
- BOUNDING REACTIVITY RAMP RATE $\sim 50\ \$/s$
- INCOHERENCY EFFECTS COULD REDUCE RATE TO $\sim 35\ \$/s$
- TERMINATION EASILY SEEN
- RE-EVALUATING (INTRA S/A INCOHERENCIES) MARGINS FOR LOF-d-TOP BEHAVIOR.
- WILL RE-EVALUATE LOF-d-TOP CONSEQUENCES (W-2).

# III (C).1. LOF REFERENCE INITIATING PHASE BEHAVIOR

OBJECTIVES

- AVAILABILITY OF FUEL REMOVAL PATHS
- DRIVING FORCES FOR FUEL DISPERSAL
- SCOPE ENTRY TO RECRITICALITY-PRONE CONDITIONS

KEY PROCESS IS CO-DISRUPTION

BECAUSE

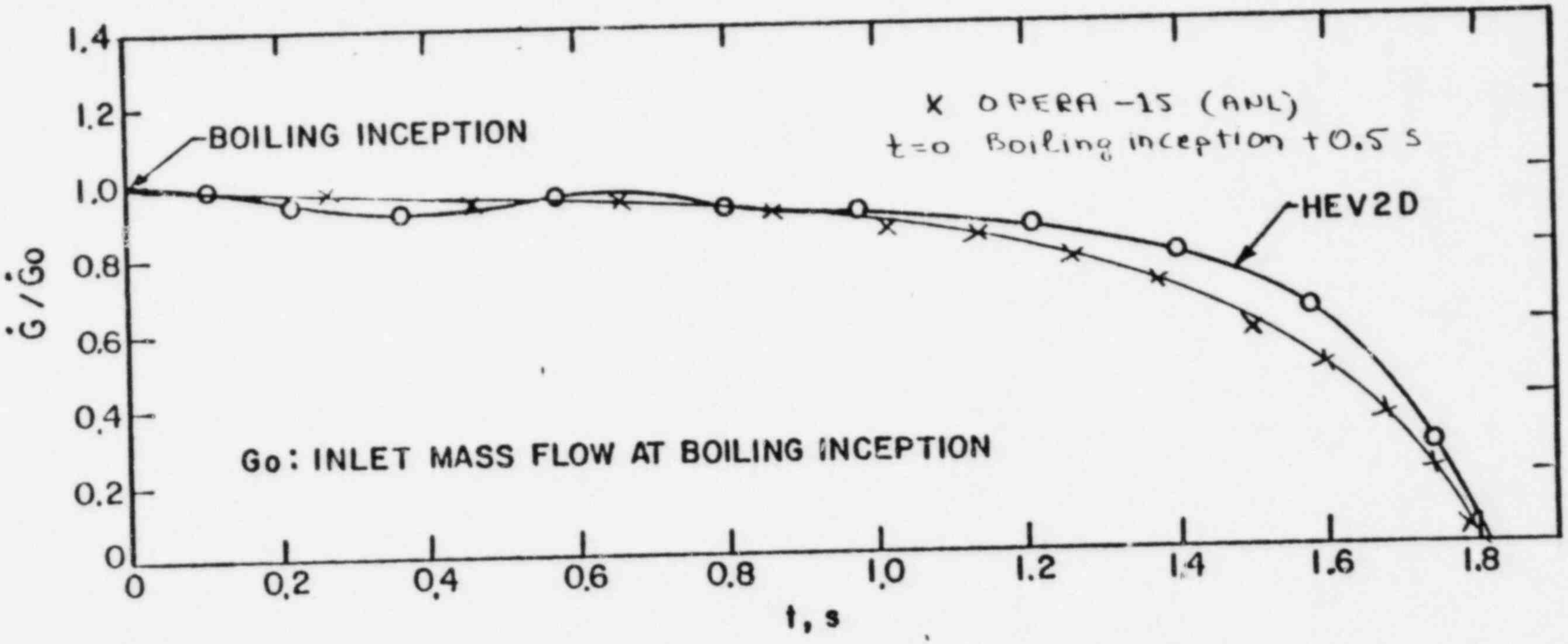
- STEEL VAPOR PRESSURES
- INCREASED PENETRATION POTENTIAL
- REMELTABLE BLOCKAGES

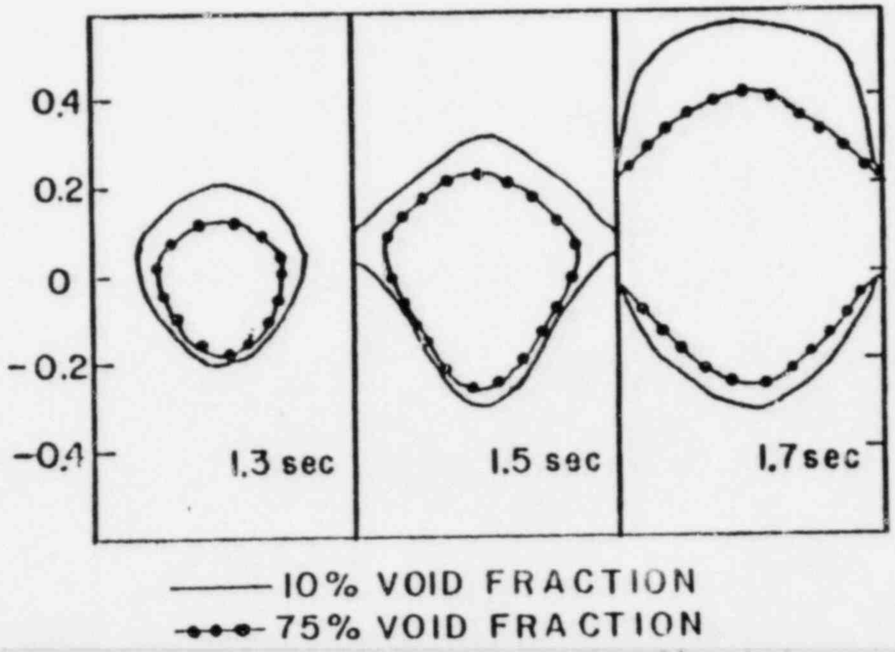
FAVORED BY

- INCREASED SODIUM WORTH
- PLENUM FISSION GAS COMPACTION

III (C).2 TIMING & RATE OF VOIDING

HEV-2D Pretest Predictions of OPERA-15 Inlet Flow Transient.



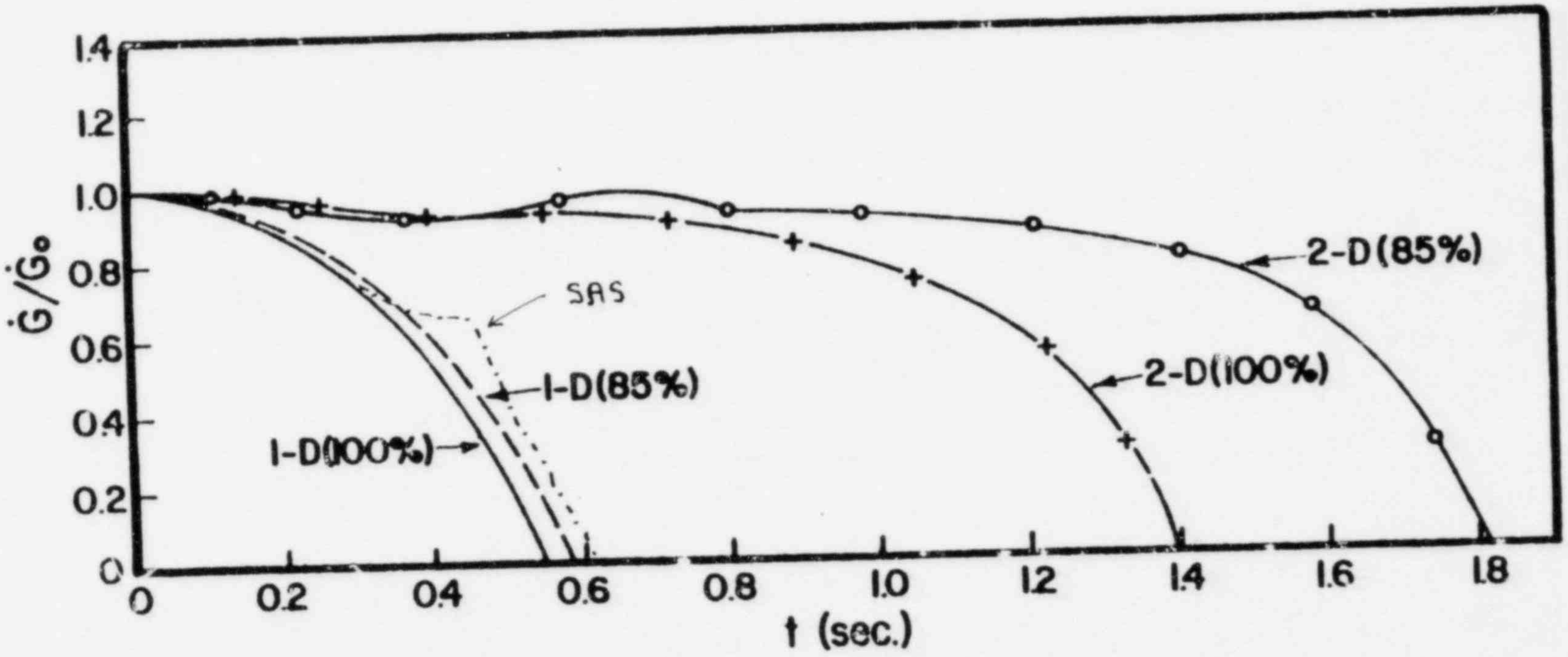


OPERA-15

1.6 s

III (c).2 (CONT'D)

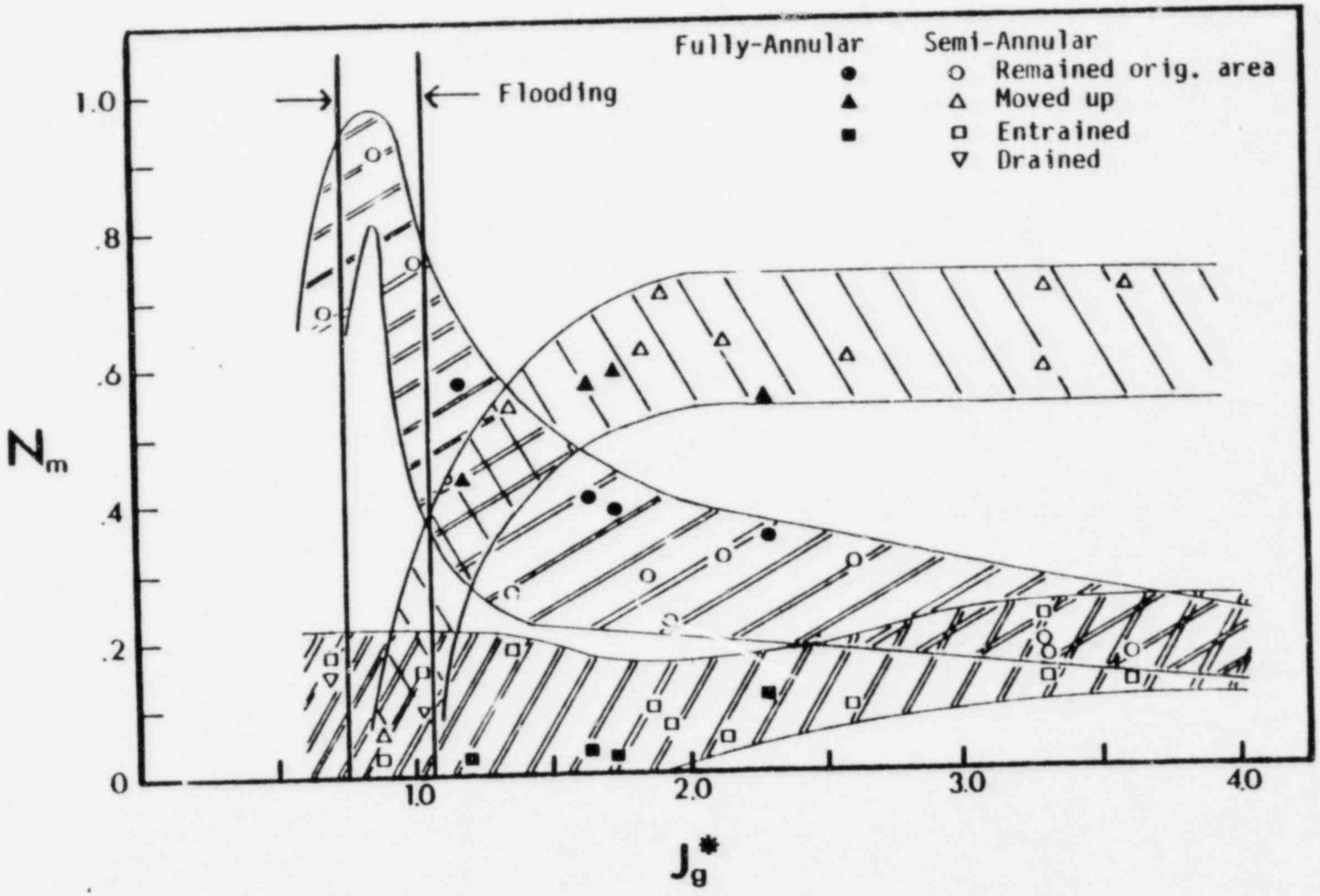
COMPARISONS OF HEV-1D & -2D at 100% and 85% Power with SAS



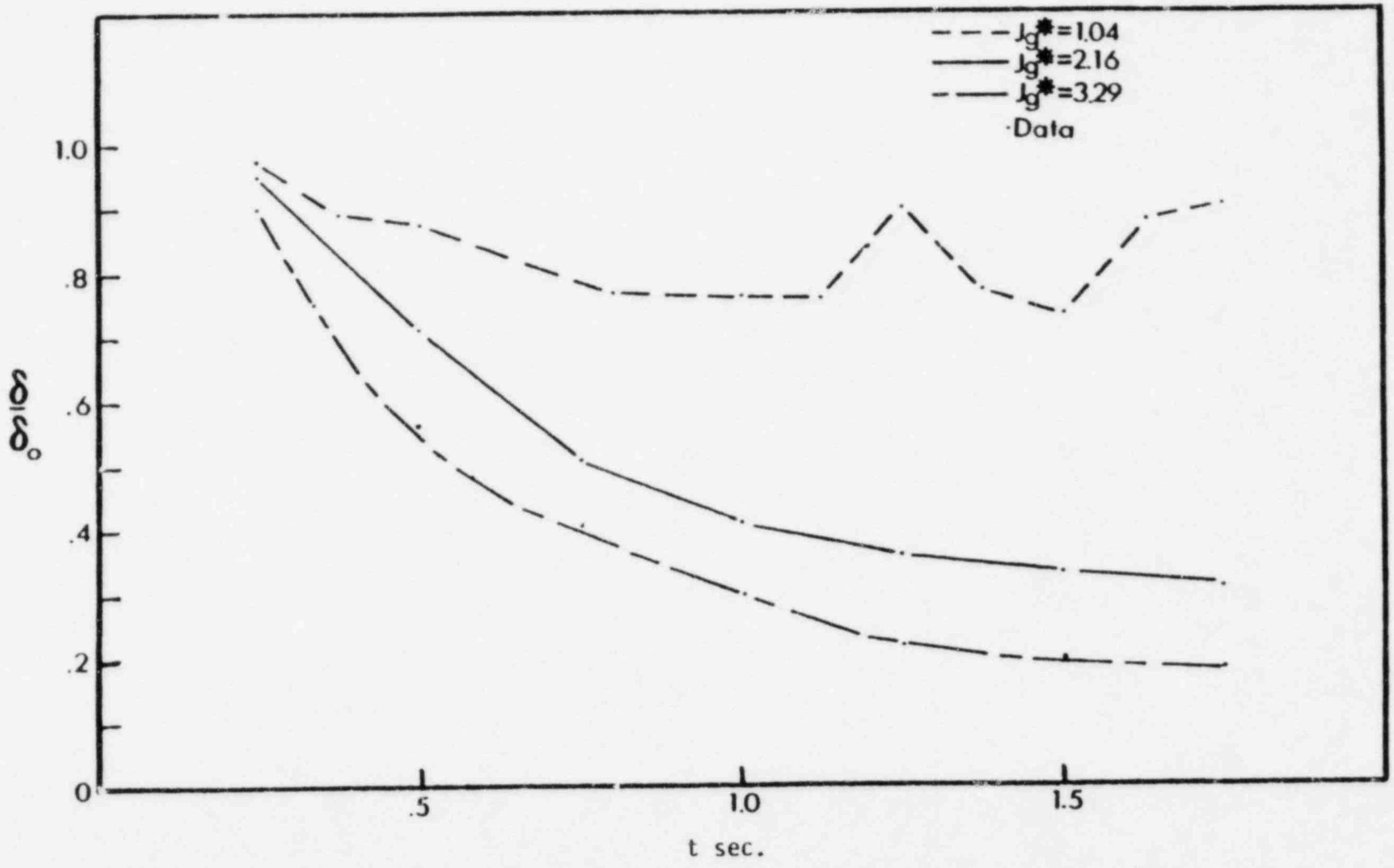
## III (c).3 IN-PILE CLAD RELOCATION DATA

| | | |
|---|---|---|
| • R-series | 7-pin | mm-SCALE BLOCKAGES |
| • R-8 | pressurized | NO BLOCKAGE |
| • P3A | 37-pin | 2 cm BLOCKAGE |
| • P3 | 37-pin | 10 cm BLOCKAGE. |

## III (c).4 QUANTIFICATION OF RELOCATION TRENDS

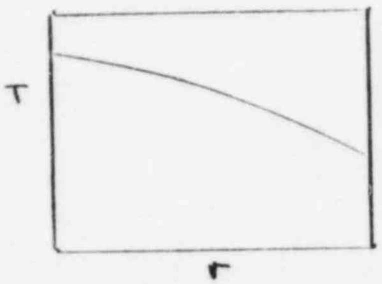


## III (c). 4 (CONT'D) QUANTIFY RELOCATION RATES



# III (c).5 IN-PILE TEST PARAMETERS

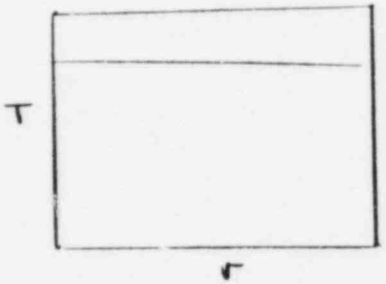
R-SERIES
(TREAT)



$\tilde{A} \sim 0.4$

$\Delta P \sim 11.5$ psi
$\Delta V \sim \pm 0.9$ m/s

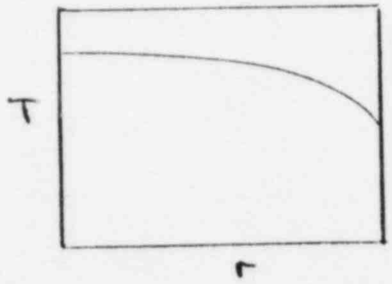
P-SERIES
(SLSF)



$\tilde{A} \sim 1$

$\Delta P \sim 16$ psi
$\Delta V \sim \pm 1.61$ m/s

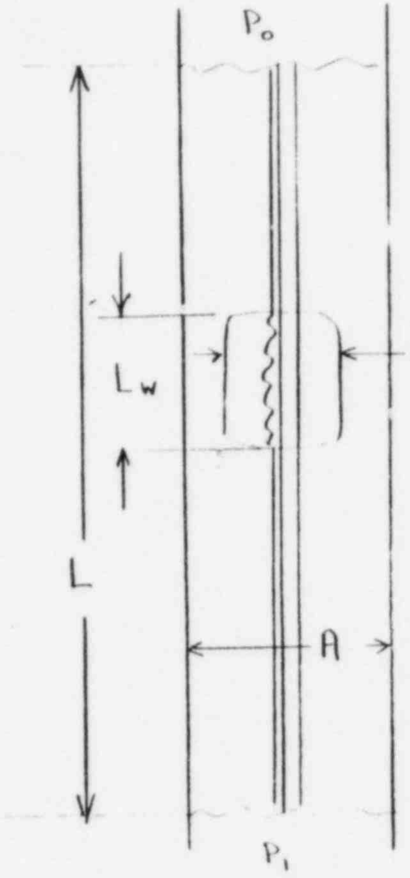
REACTOR
(CRBR)



$\tilde{A} \sim 0.6$

$\Delta P \sim 15$ psi
$\Delta V \sim \pm 1.3 - 2.4$ m/s

# III (c).6 QUANTIFY INCOHERENCY EFFECTS

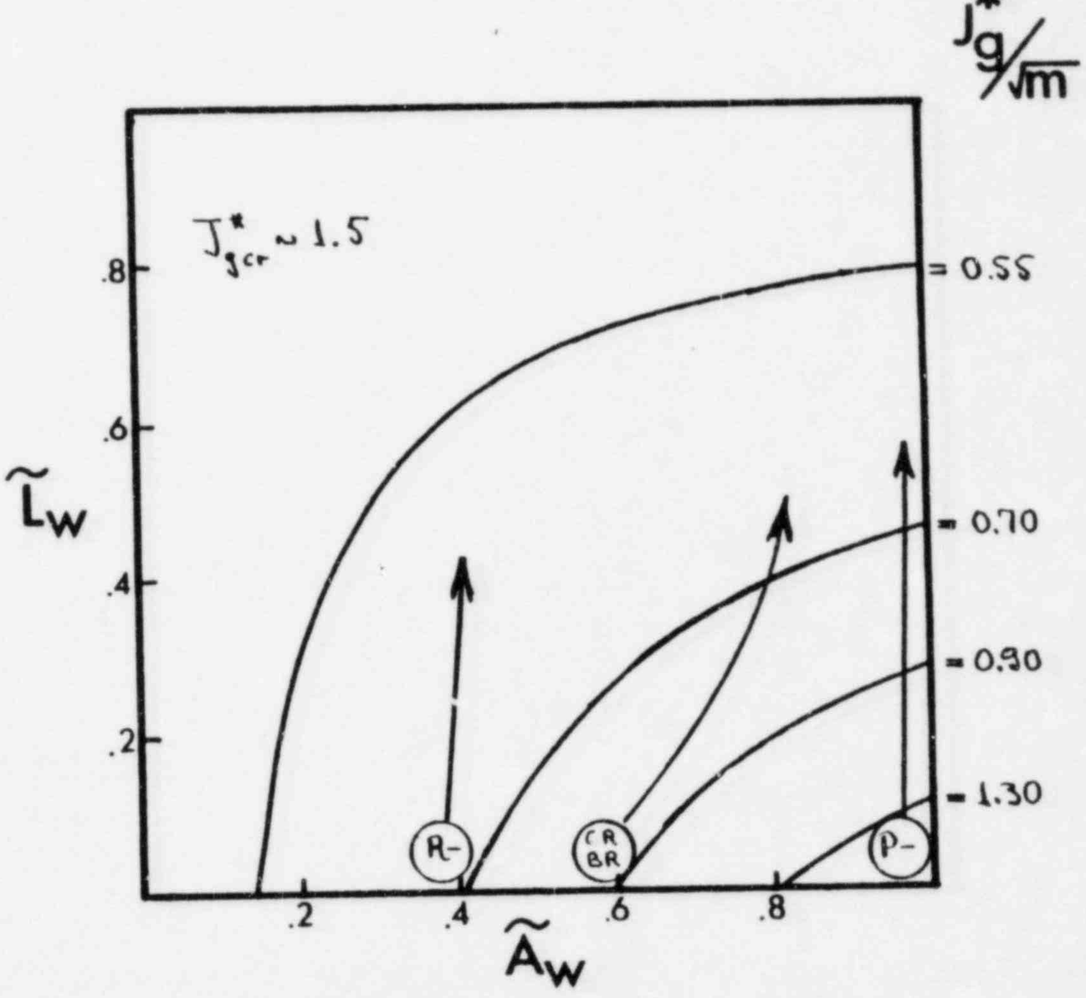


$\Delta P = P_1 - P_o$

$m = \dfrac{\Delta P}{\rho g L}$

$\tilde{A}_w = \dfrac{A_w}{A}$

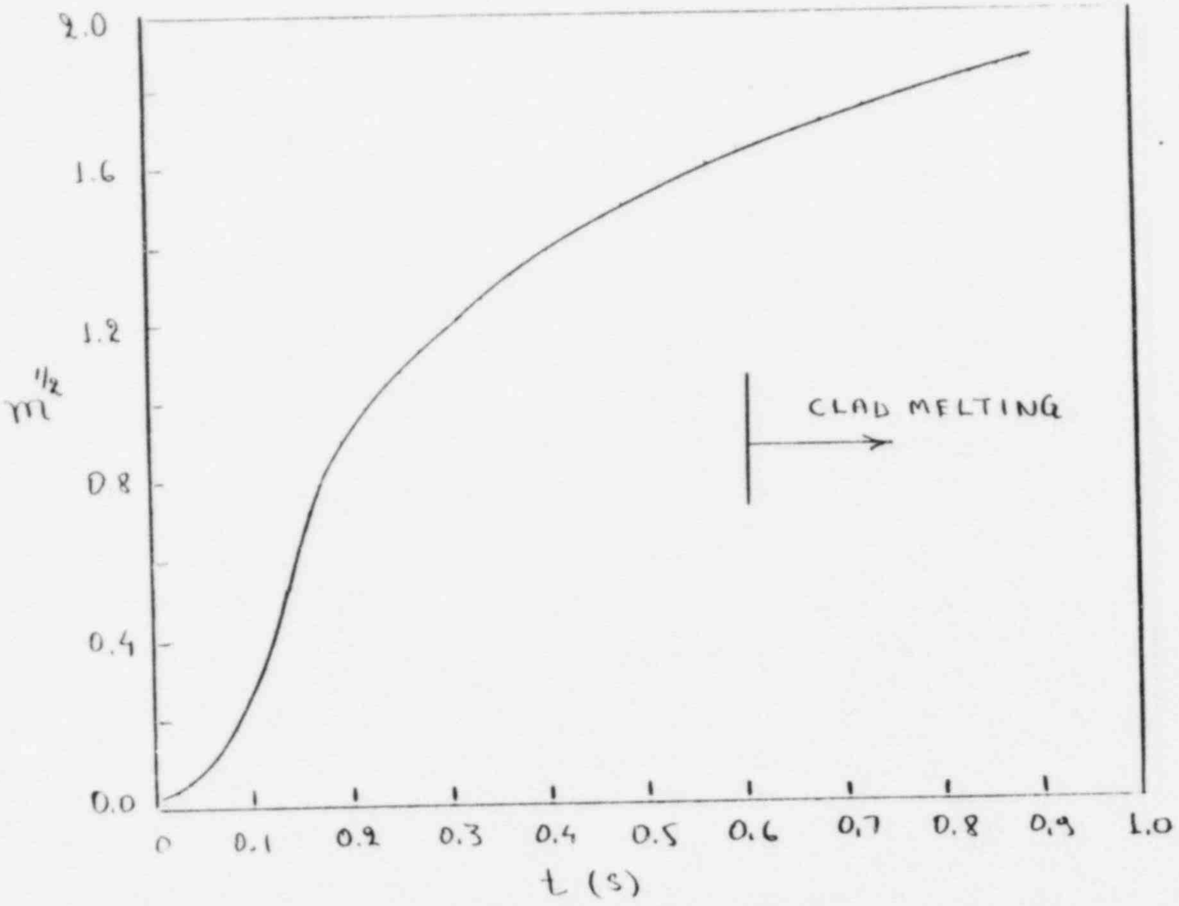
$\tilde{L}_w = \dfrac{L_w}{L}$

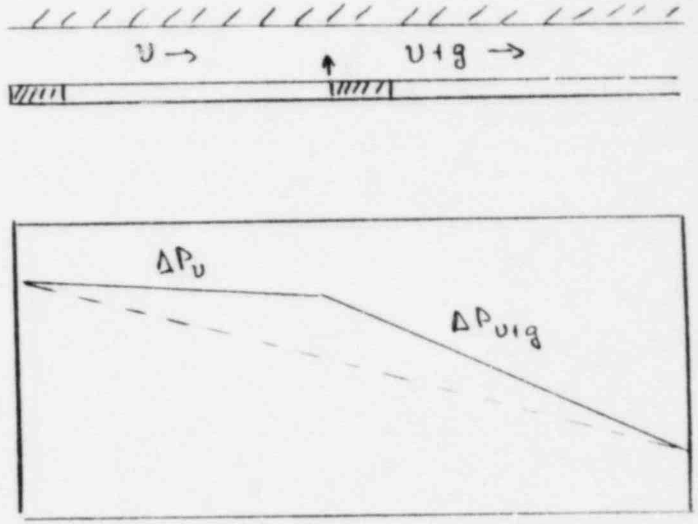


$m_R^{1/2} = 2.2$
$m_P^{1/2} = 2.5$
$m_{Rea}^{1/2} = 1.9$

PLUS : CHUGGING RANDOMNESS.

# III (C).7 EFFECT OF GAS BLOWDOWN ON $\dot{m}$.





## III (C). 8 THEREFORE

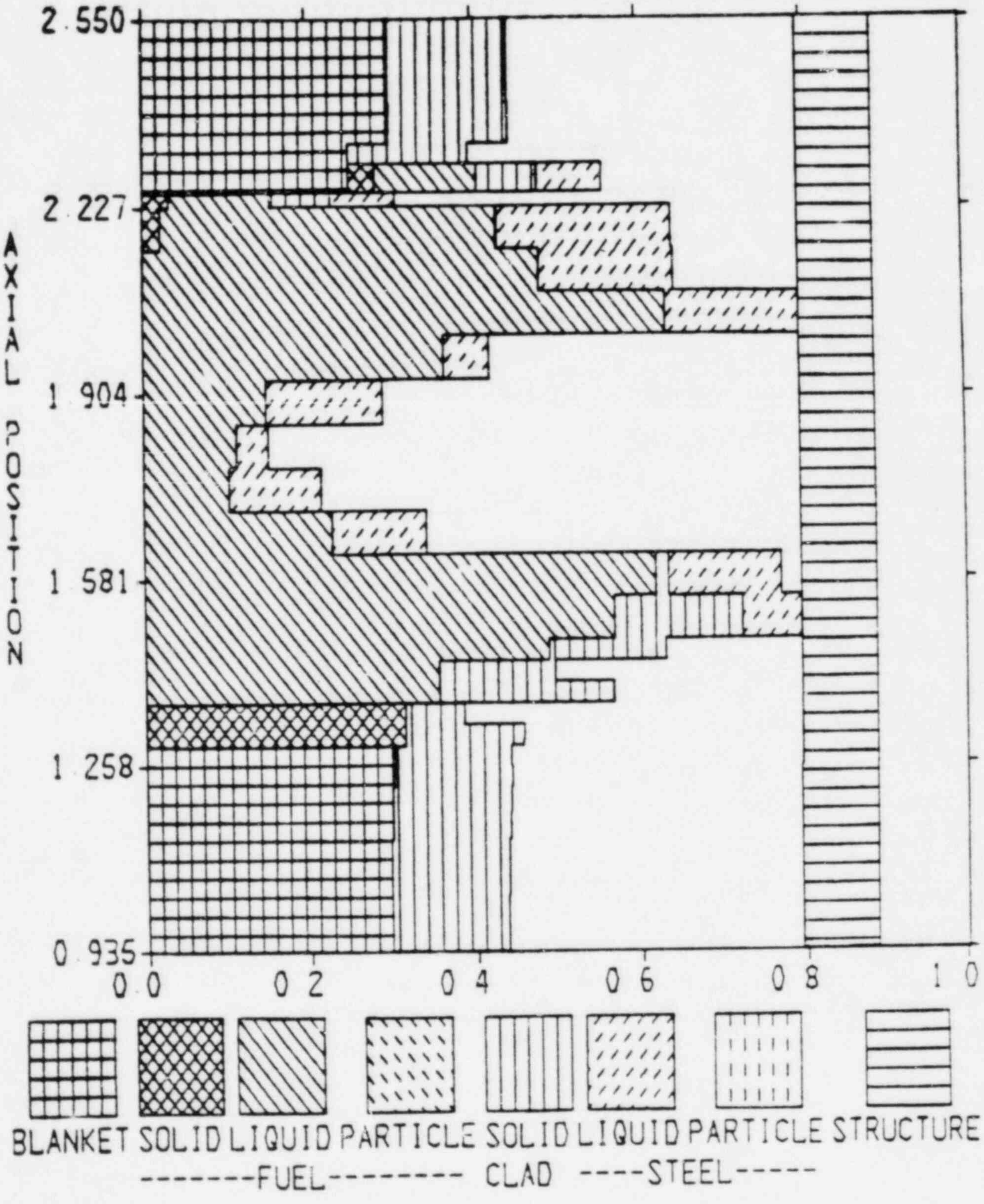
- PLENUM GAS BLOWDOWN SLIGHTLY INTERFERES WITH UPWARD RELOCATION
- RELOCATION VELOCITIES $\dot{\sim} 50$ cm/s
- THIN INCOMPLETE BLOCKAGES CANNOT BE EXCLUDED

BUT

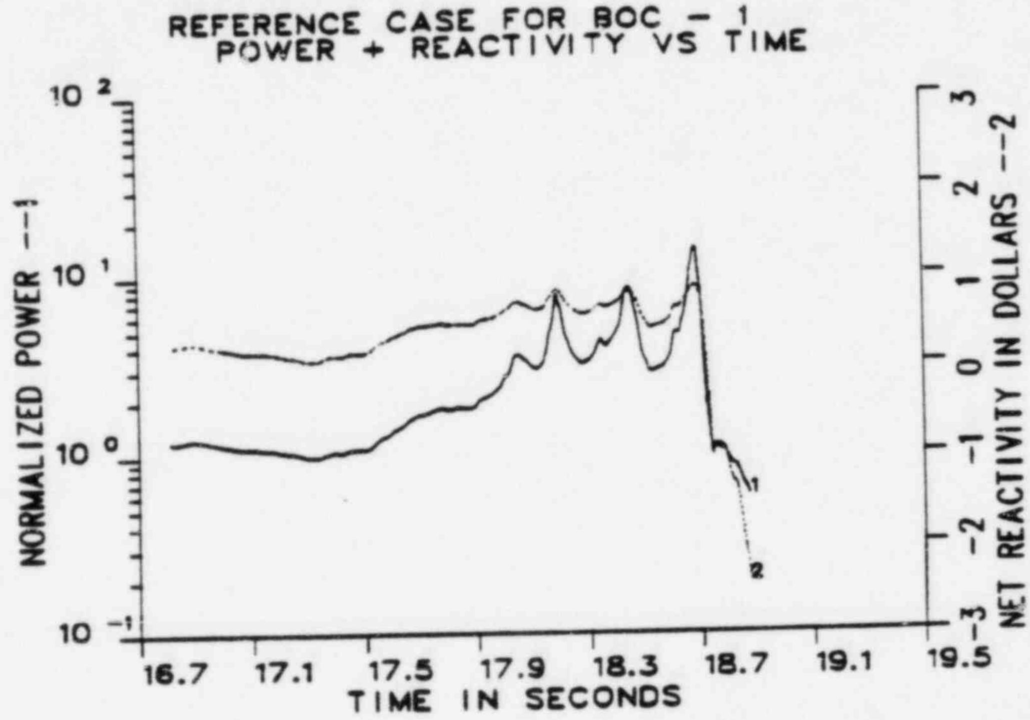
CANNOT SIGNIFICANTLY PLUG THE CRBR CORE WITH STEEL WITHOUT REACTIVITY & POWER INCREASES

∴ CO-DISRUPTION

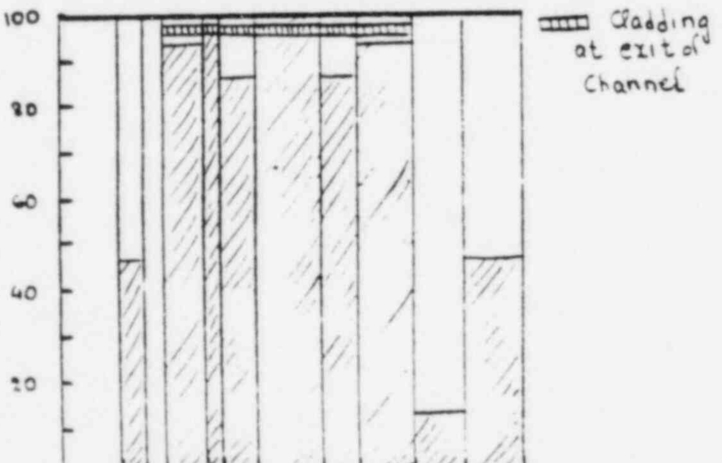
III (C).9 ILLUSTRATION OF CO-DISRUPTION



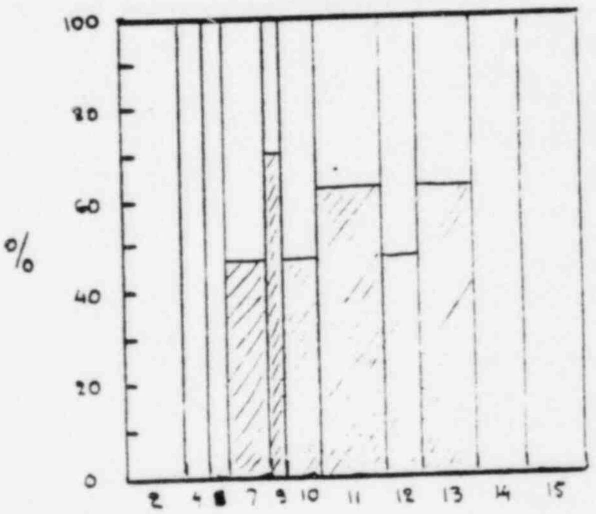
# III (C).10 CO-DISRUPTION FOR CASE A



Mobile Cladding Pattern

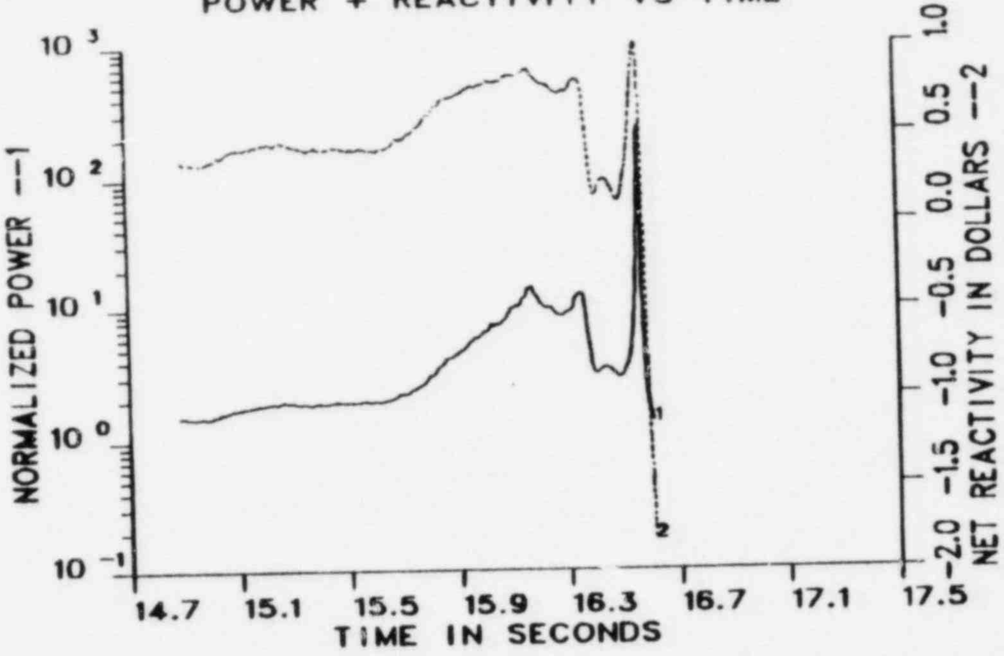


Mobile Fuel Pattern

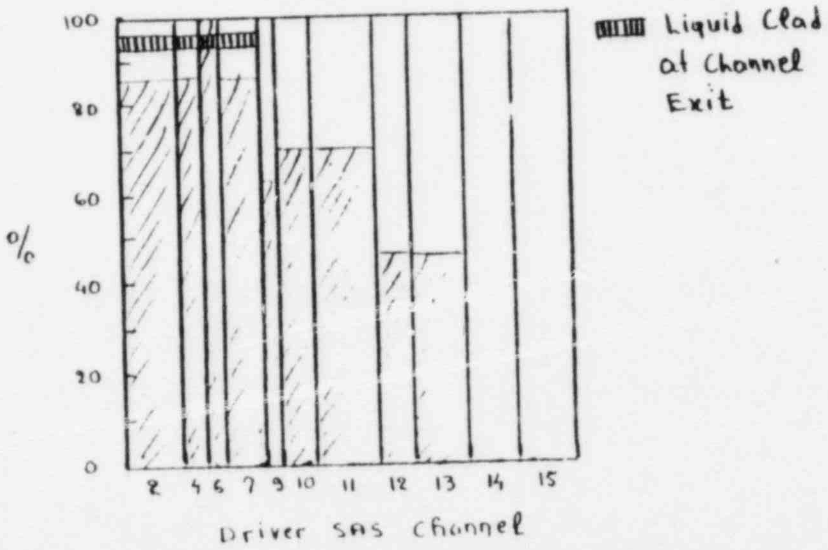


## III(C).10 CO-DISRUPTION FOR CASE B

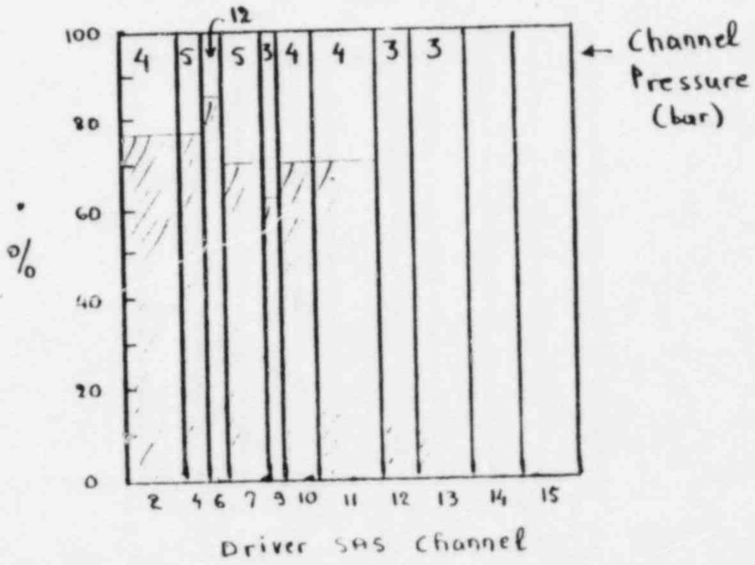
POWER + REACTIVITY VS TIME



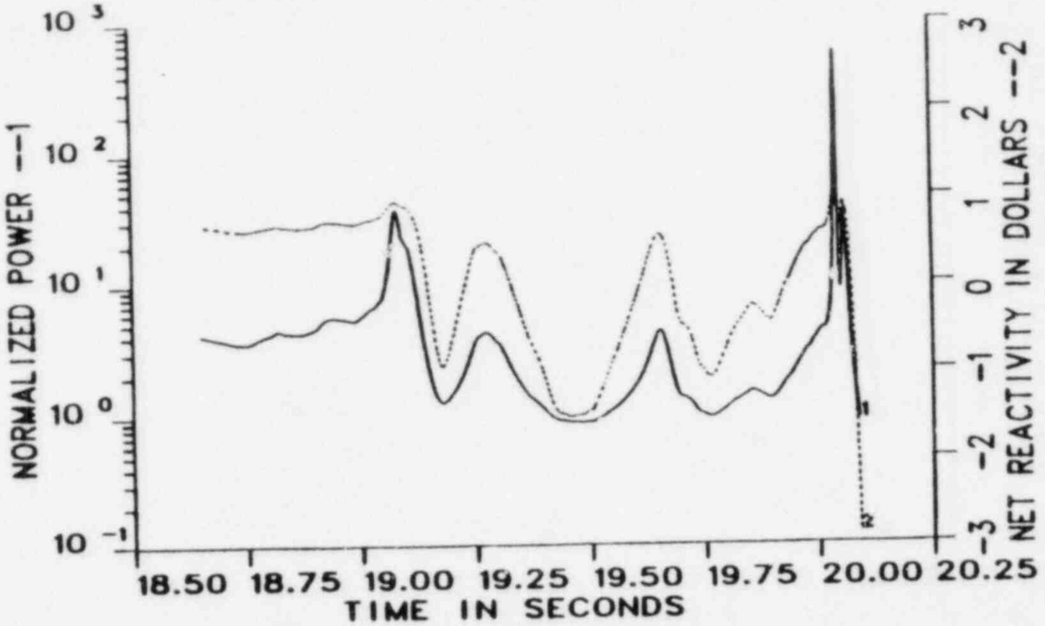
Mobile Cladding Pattern

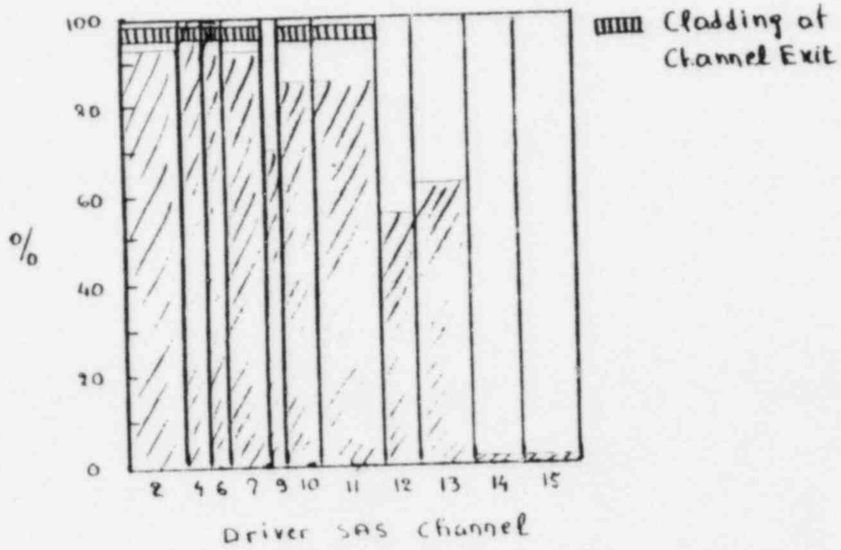


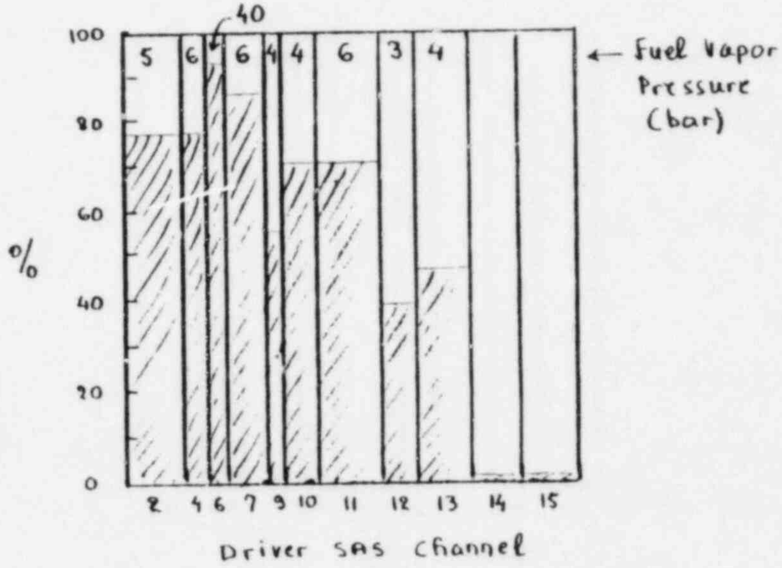
Mobile Fuel Pattern



III (c).10 CO-DISRUPTION FOR CASE G


POWER + REACTIVITY VS TIME


Mobile Cladding Pattern


Mobile Fuel Pattern

## III (c).11 SUMMARY

- EXTENSIVE CO-DISRUPTION EXPECTED
- EXTENSIVE AXIAL RELIEF PATHS
- NEUTRONIC ACTIVITY AND PRESSURES PRESENT

## III (d).1 RECRITICALITY

DISRUPTED FUEL COMPACTION TO SUPERCRITICAL CONFIGURATION

REQUIREMENT: FUEL MASS AVAILABLE IN EXCESS OF 60% OF CORE INVENTORY

- PRESSURE DRIVEN
- GRAVITY DRIVEN.

## III (d).2 PRESSURE DRIVEN RECRITICALITY

- IMPOSSIBLE IN BLOCKED CHANNELS
- IF IT WERE TO OCCUR IT WOULD AT THE S/A POOL STAGE
- INCOHERENCE ; LOCAL EVENTS ONLY
- NO PRESSURE EVENTS KNOWN IN FCIs WITH LMFBR MATERIALS IN RELEVANT GEOMETRIES AND CONTACT MODES

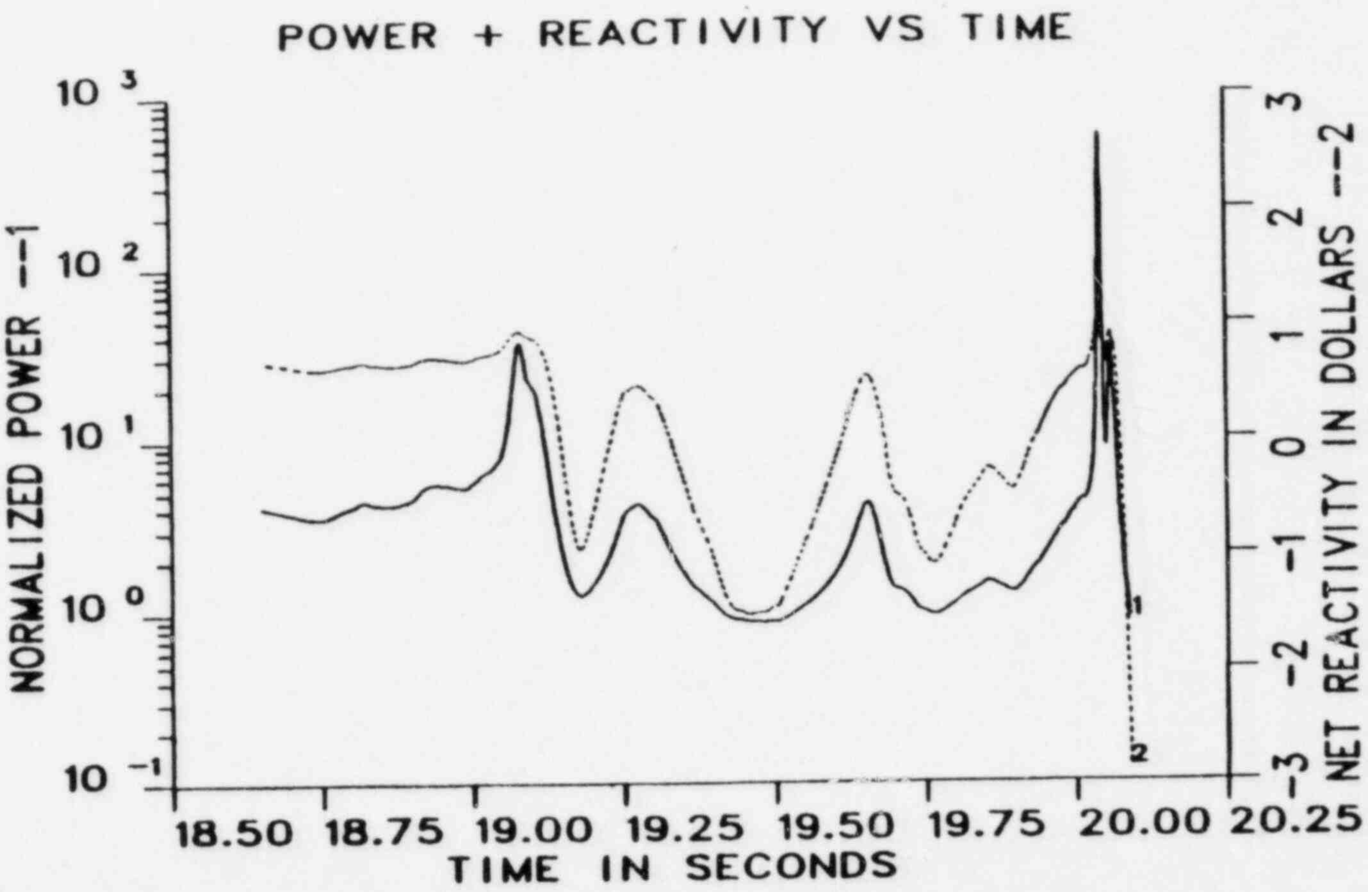
∴ IGNORE.

## III (d).3 GRAVITY DRIVEN RECRITICALITY

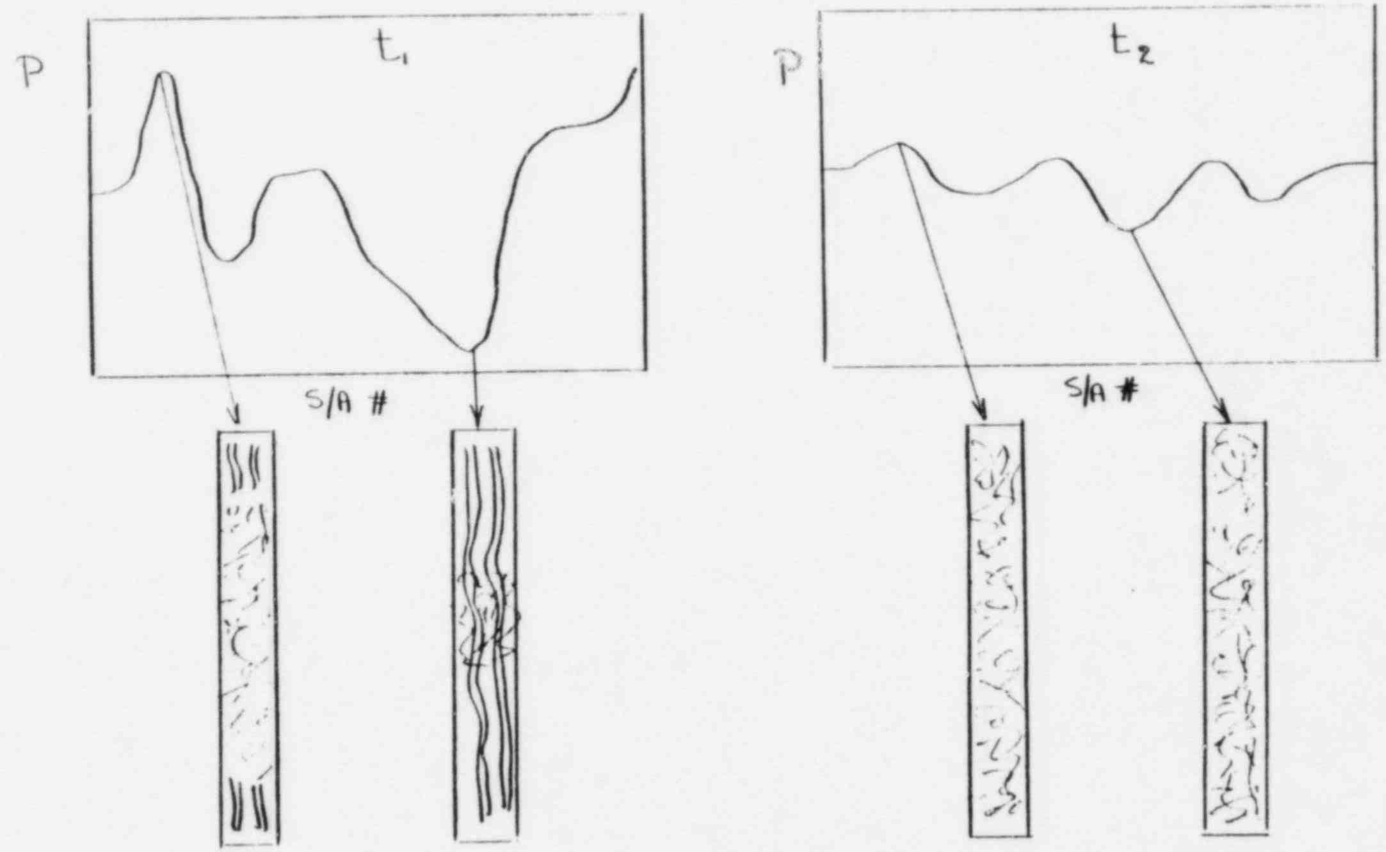
- GRAVITATIONAL ACCELERATION
- SUBSTANTIAL HEAT SINKS FOR CONDENSATION
- NEUTRONIC TRIGGERING & TUNING.

∴ CONSIDER .

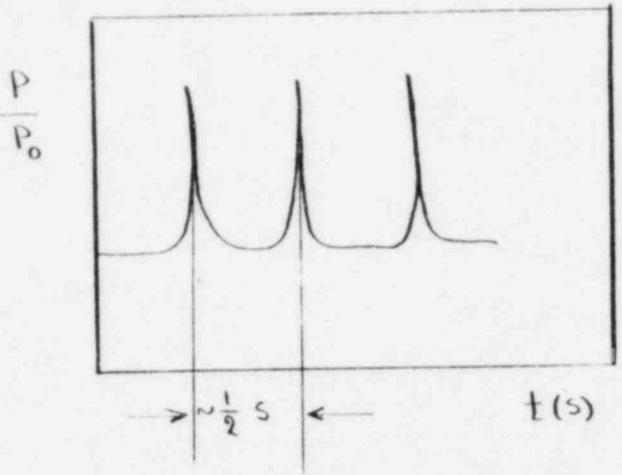
III (d).4 ILLUSTRATION OF "TRIGGERING"



# III(d).5 ILLUSTRATION OF TUNING (SCHEMATIC)



## III (d).6 RELEVANT TIME SCALES



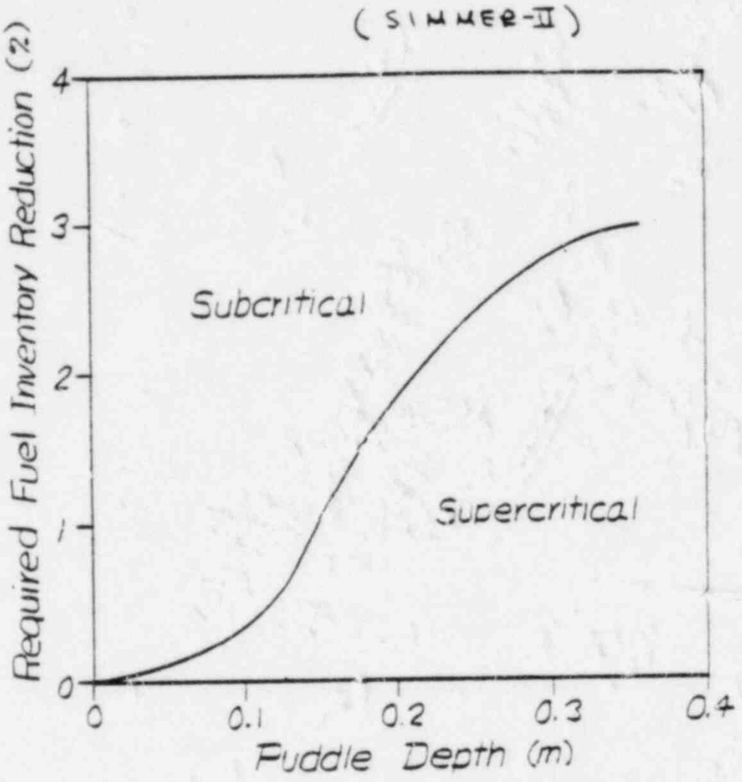
TIME TO MELT DRIVER S/A
CAN WALLS ~1 to 2 s

∴ ~2 to 4 oscillations in
power (recriticalities) in
the S/A pool phase.

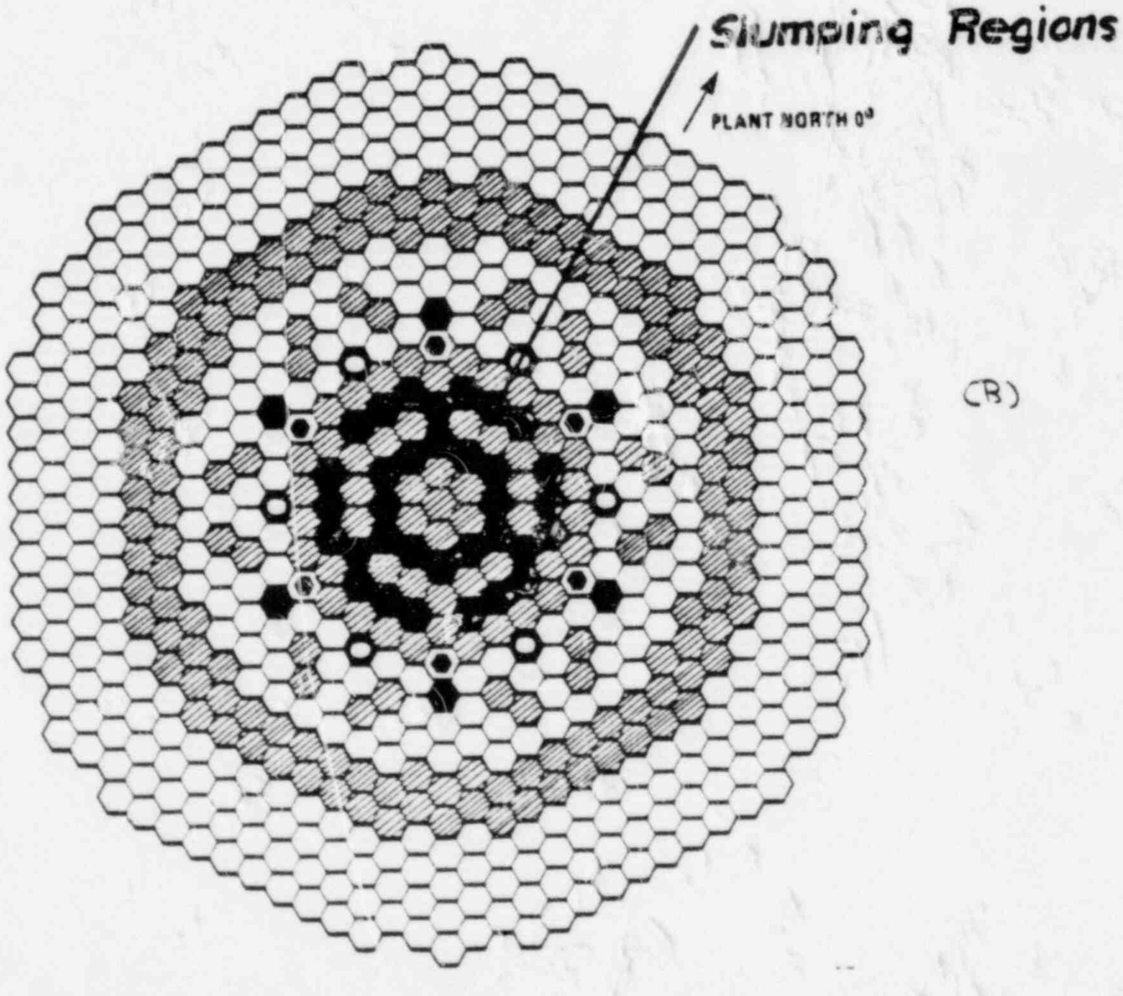
## III (d).7 PRINCIPAL TRENDS

- INITIAL MOTIONS HIGHLY INCOHERENT
- OSCILLATORY MOTIONS GRADUALLY TUNED AND AMPLIFYING DUE TO HIGHLY NON-LINEAR COMPACTION STATE vs POWER RELATIONSHIP
- HEAT SINKS INFLUENCE BUT DO NOT CONTROL THE DYNAMICS OF FALLBACK.

III(d).8 REACTIVITY CONFIGURATION STATES
( SIMMER-II )



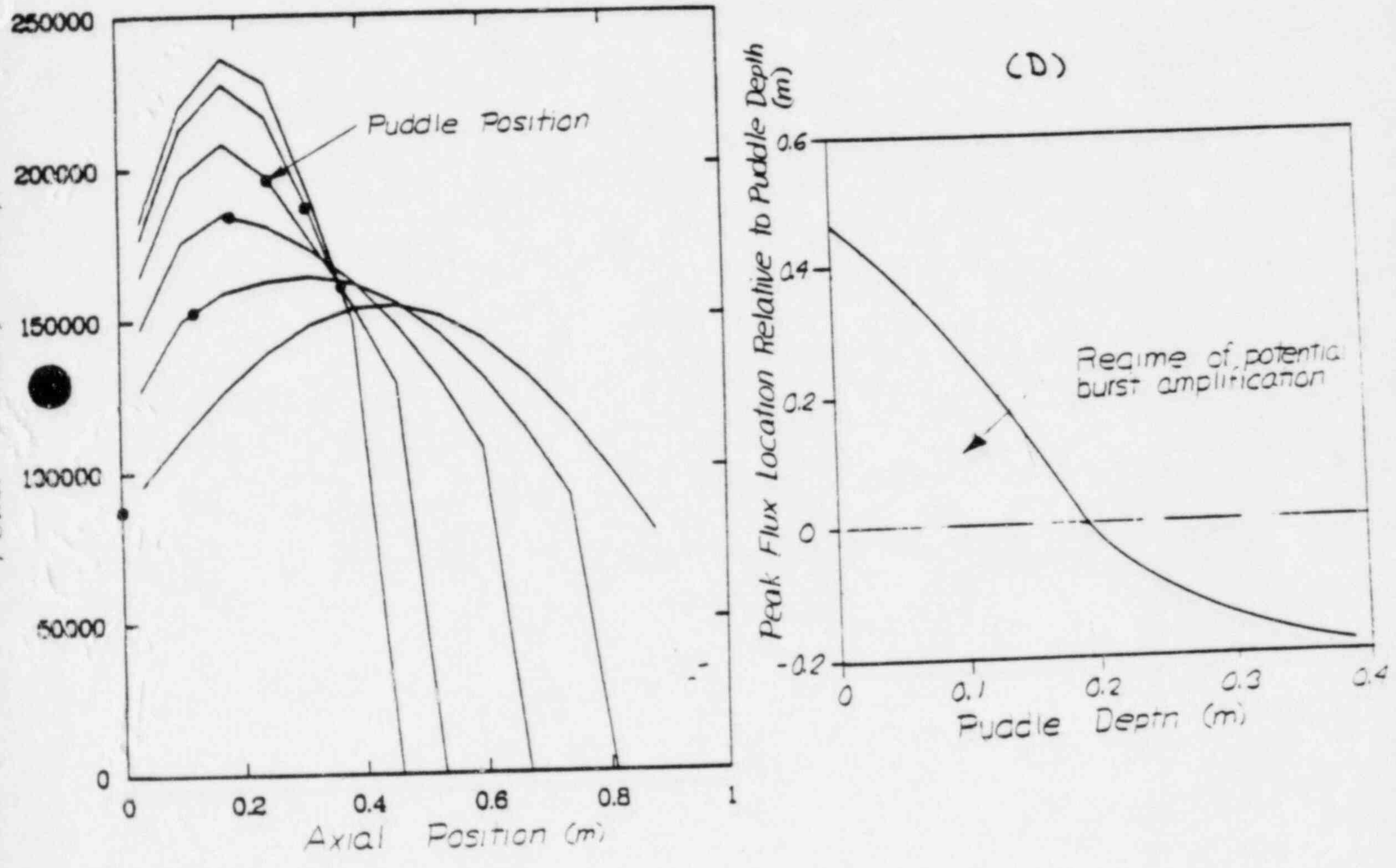
(A)



(B)

III (d). 8 (CONT'D)

BOUNDARIES OF AUGMENTATION DURING DISSASSEMBLY

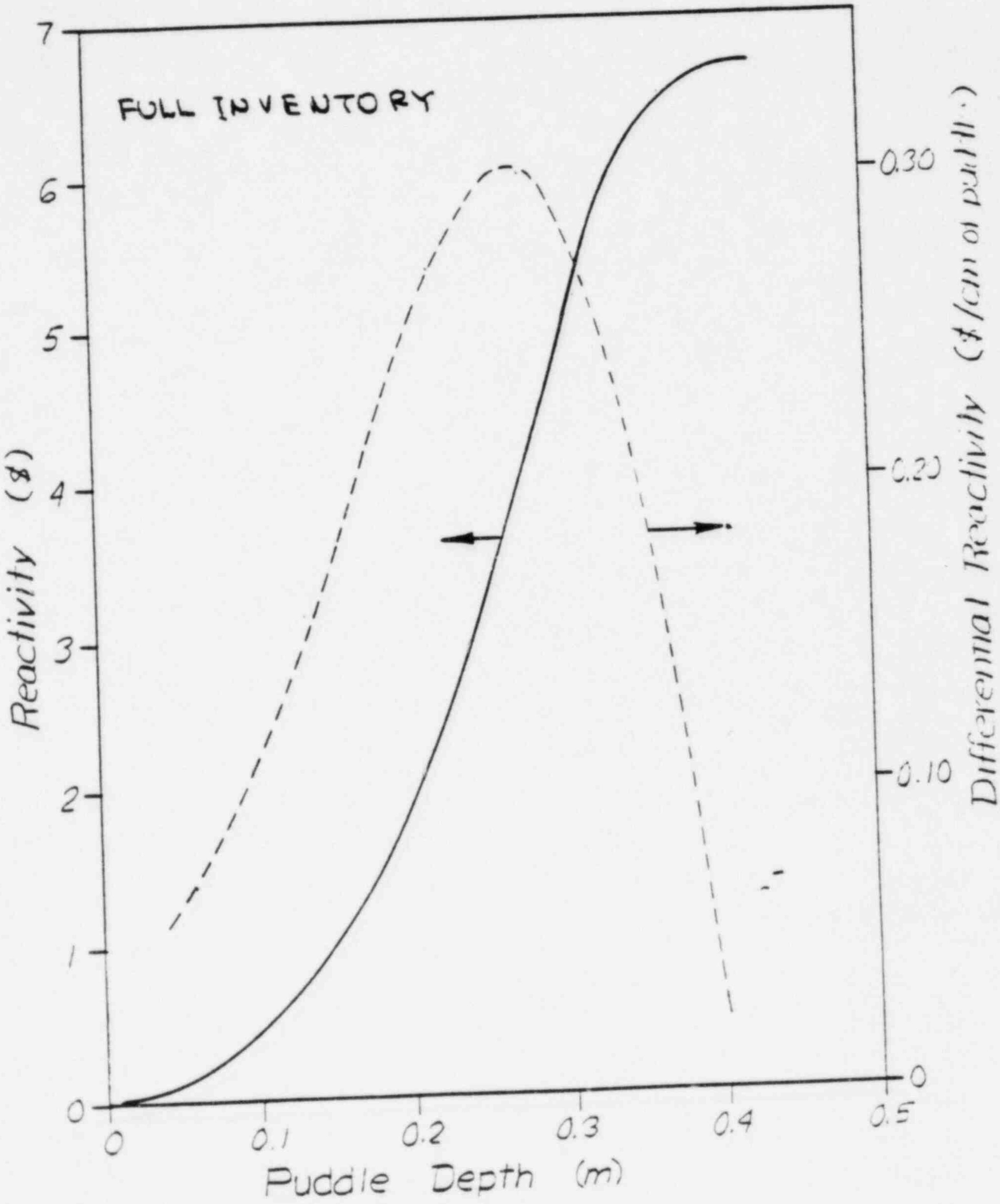


(C)

(D)

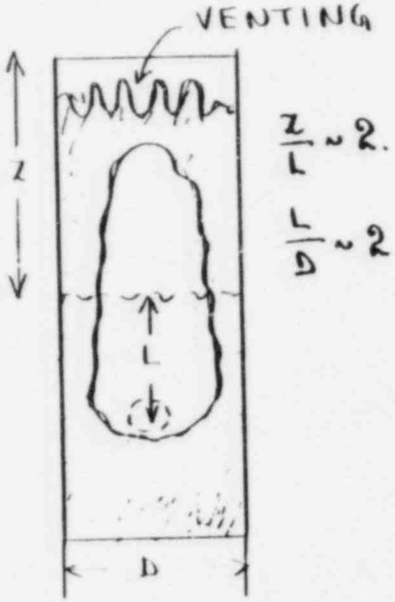
III(d).(8) (CONT'D)

FUEL REACTIVITY DIFFERENTIAL WORTH



(E)

## III (d). 9 OSCILLATORY FLOWS IN S/A.



$\frac{Z}{L} \sim 2.$

$\frac{L}{D} \sim 2$

ARBITRARILY COHERENT

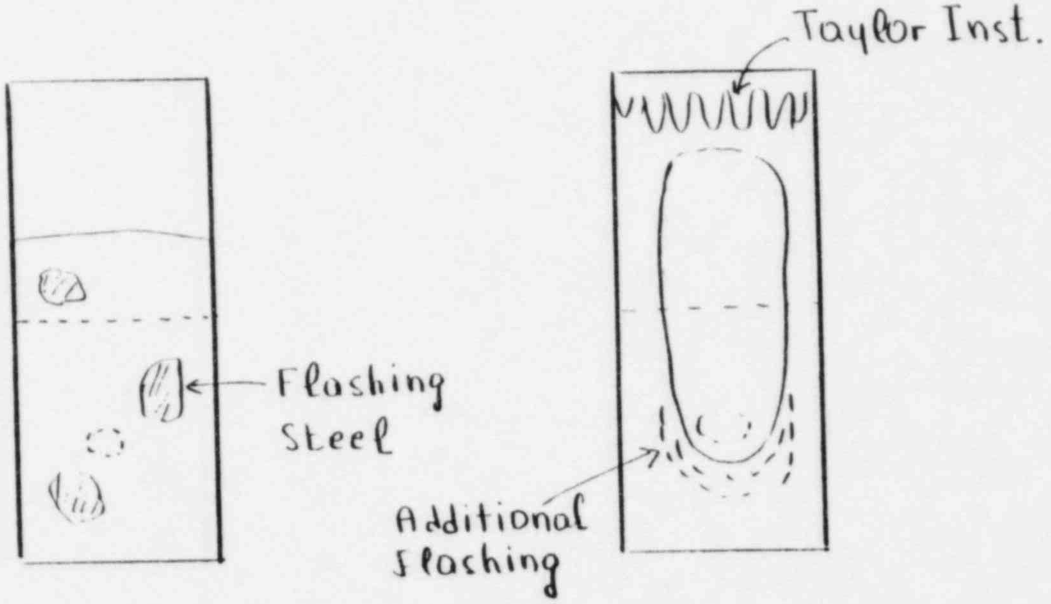
$\alpha \sim 66\,\%$

$V_{max} \sim 3\ m/s$

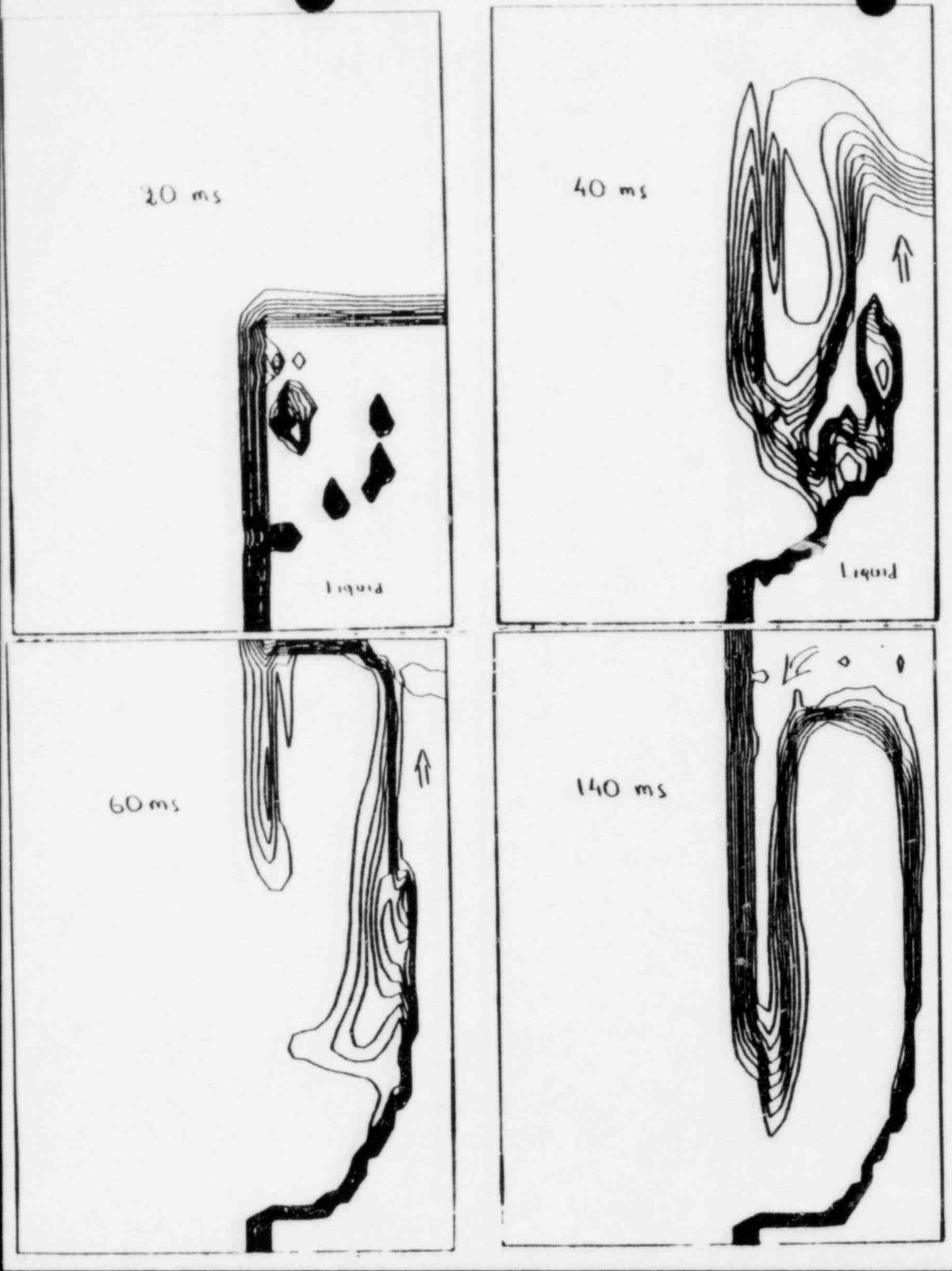
$V_{puddling} \sim 1\ m/s \qquad \frac{d\rho}{dx} \sim 1\ \$/cm$

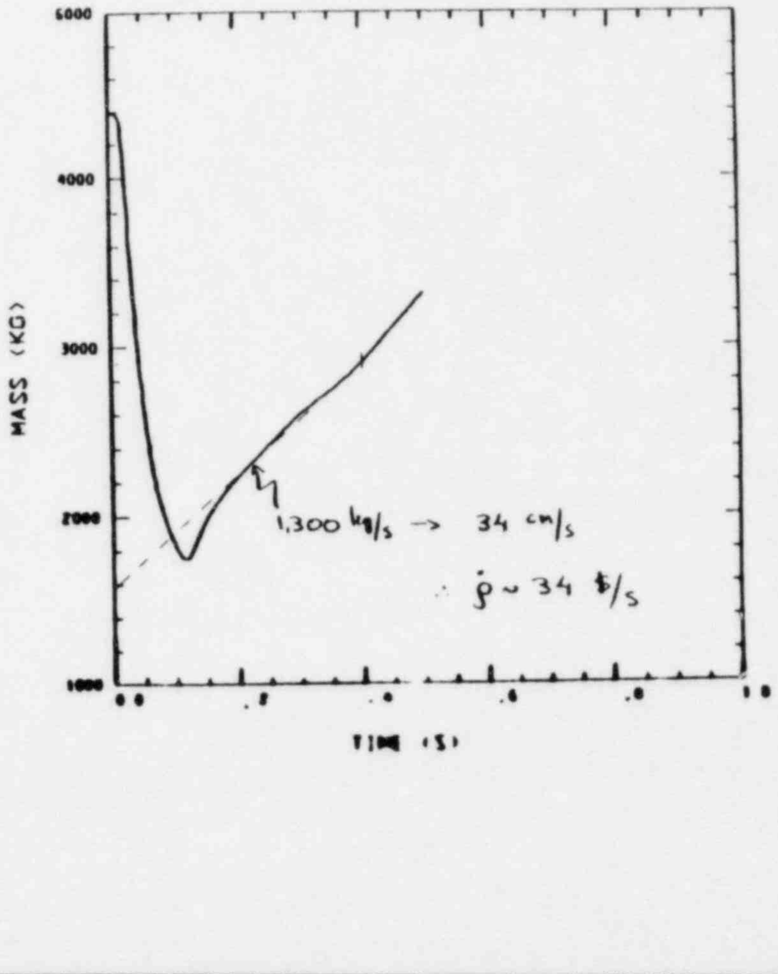
so $\dot{\rho} \sim 100\ \$/s$

## III(d).10 OTHER BREAKUP MECHANISMS.



## III(d).11 ANNULAR POOL OSCILLATION
## (SIMMER-II)





## III (d).12 SUMMARY

- GRAVITY DRIVEN RECRITICALITIES IMPORTANT
- HIGH NEUTRONIC ACTIVITY AND PRESSURES DOMINATE THE S/A WALL DISRUPTION PERIOD
- S/A AND ANNULAR POOL RECRITICALITIES ARE BOUNDED BY

$$\dot{\rho} \ll 100 \ \$/s$$

## III (e) 1 DISPERSAL

- FREEZING MECHANISMS
- ESCAPE AREAS
- PRESSURE HISTORY.

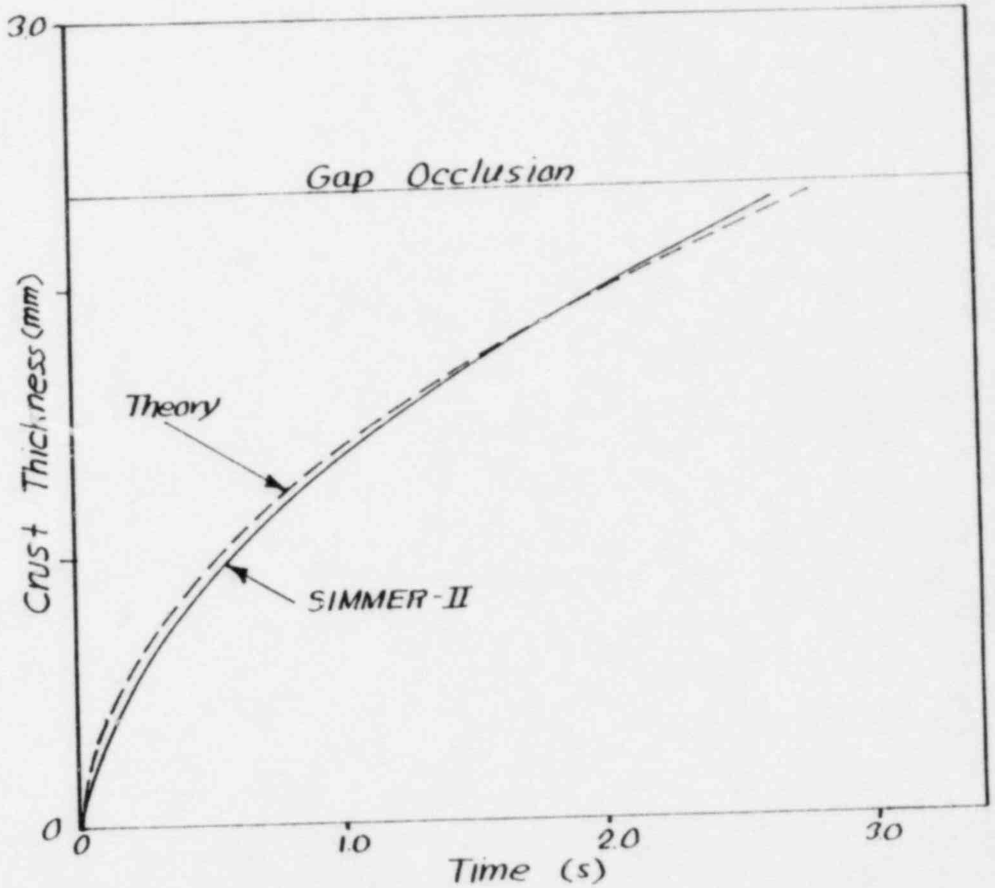
# III (e).2 FREEZING DYNAMICS (SIMMER BENCHMARKING)

ROD BUNDLES

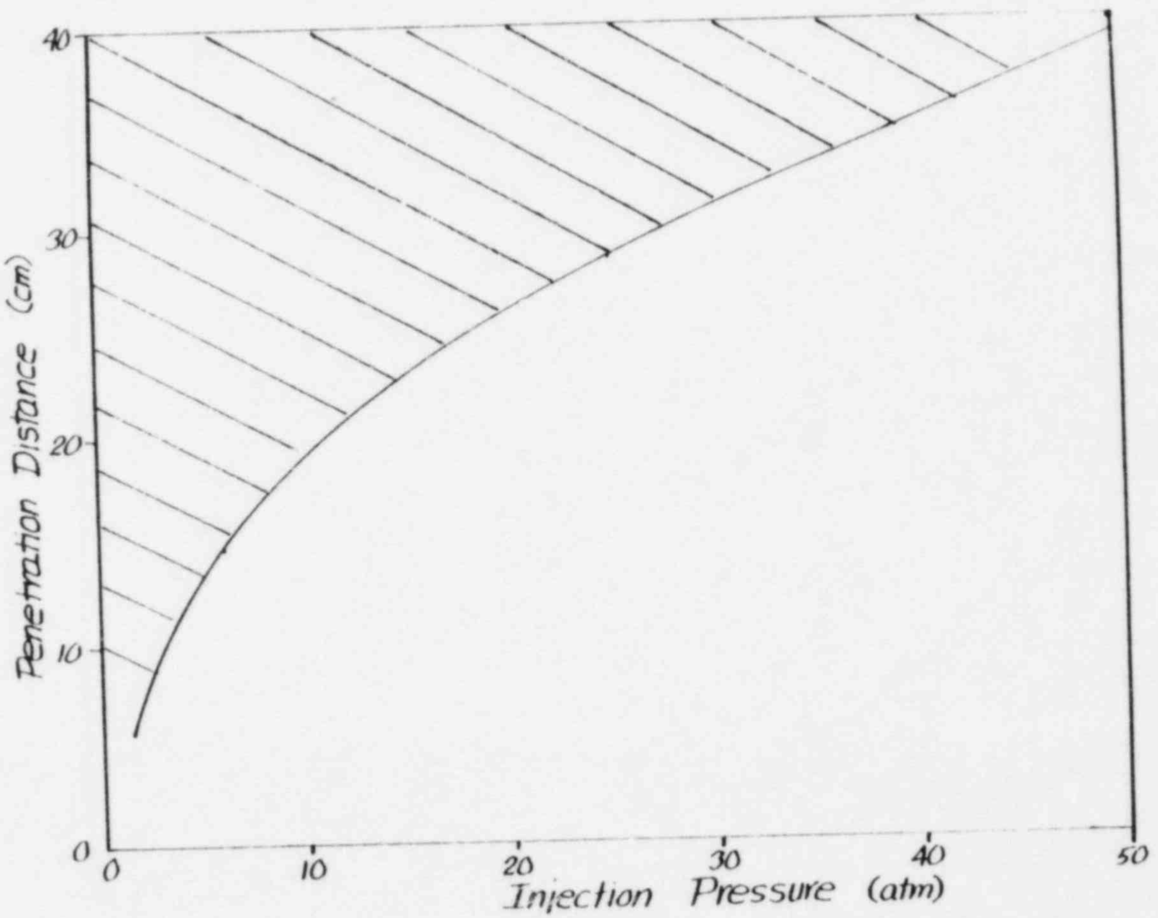
0.5 - 2.0 kg Thermite
573 - 1173 °K Clad Temperature

| P (bar) | Penetration (cm) | |
|---|---|---|
| | SIMMER | DATA |
| 66 | 45 | 43 |
| 55 | 30 | 43 |
| 25 | 35 | 34 |
| 37 | 37 | 41 |
| 50 | 40 | 40 |

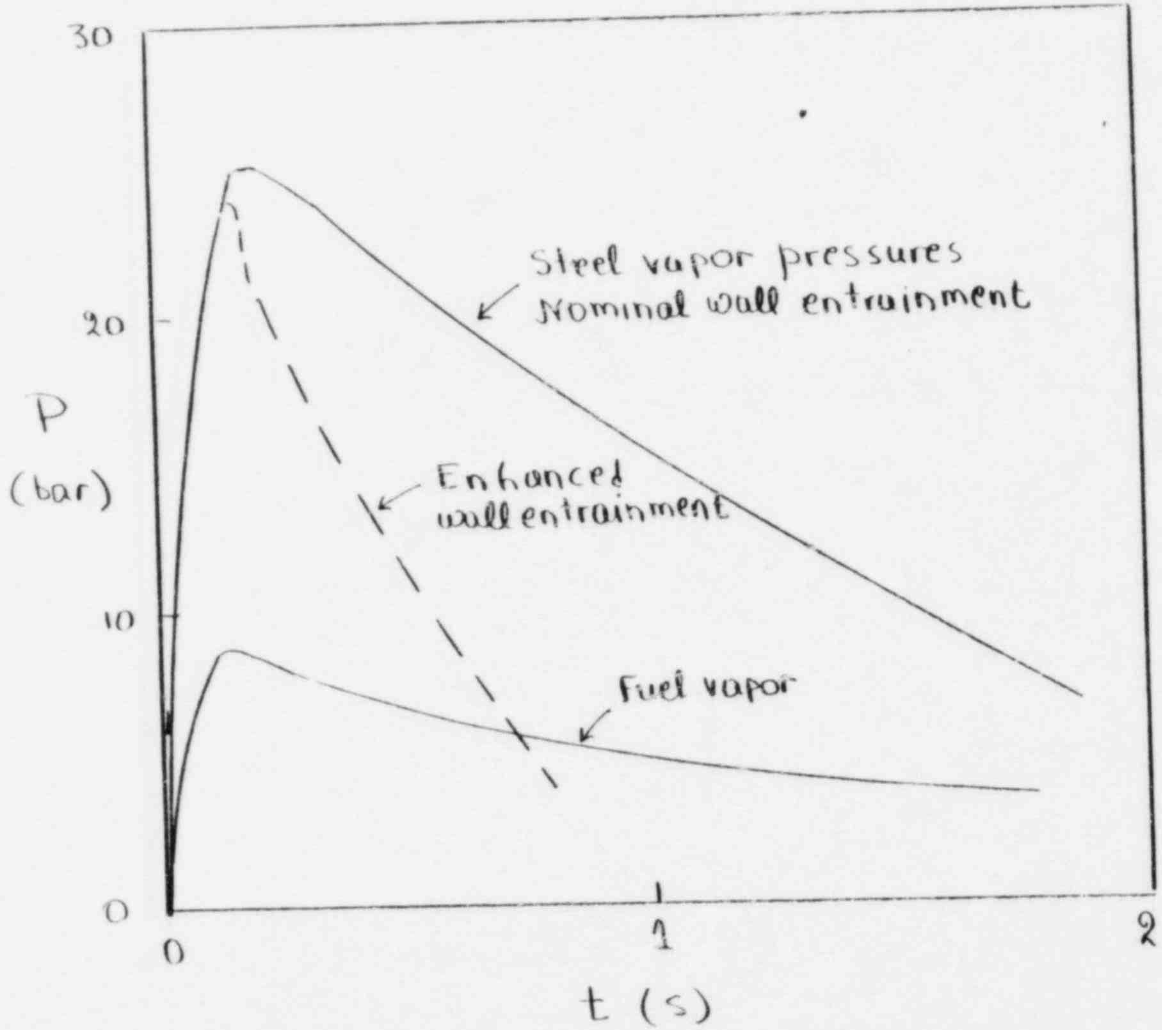
S/A WALL GAPS



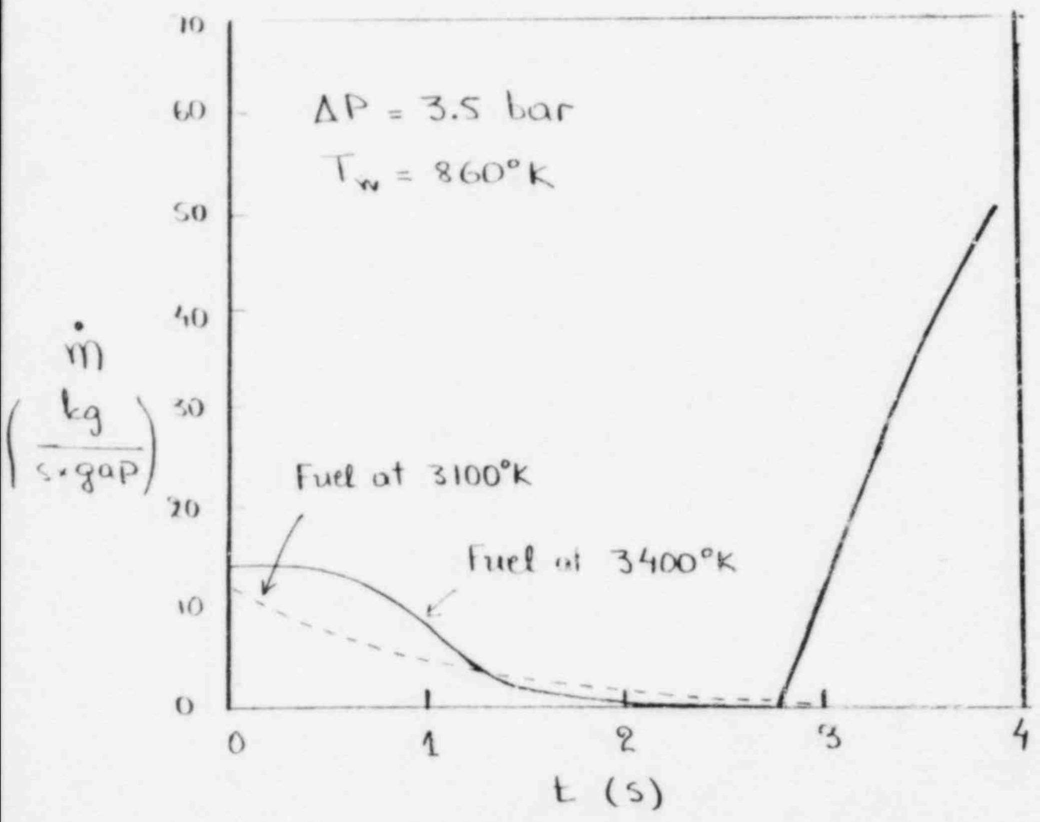
## III (e).3 PENETRATION IN ROD BUNDES AT LOW P

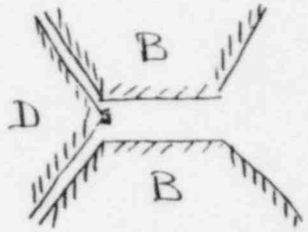


# III(e)4 SCOPING OF P DECAY RANGE (SIMMER-II).



# III (e).5 S/A GAP DISPERSAL





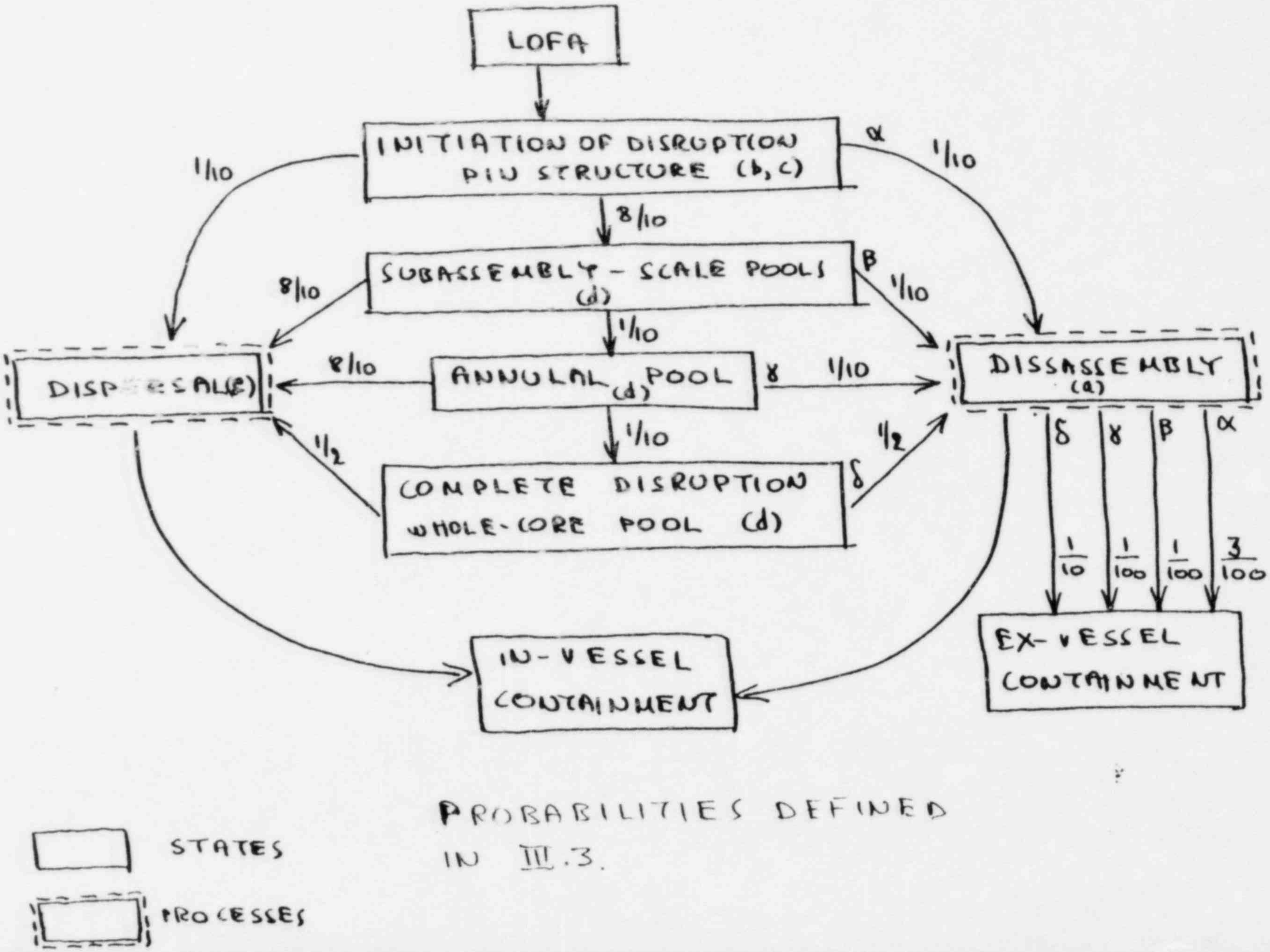
90 gaps

In 1 s remove ≈ 1200 kg
or ≈ 20 % of core

## III (e).6 SUMMARY

- AREA & PRESSURES AVAILABLE
- MASS OF FUEL REMOVED > 40% OF CORE INVENTORY IN 4 S VERY LIKELY.

# IV.1. QUALITATIVE PROBABILISTIC RESULTS.



PROBABILITIES DEFINED IN III.3.

CLINCH RIVER BREEDER
REACTOR PLANT

CRBRP PROJECT

# ACCIDENT ENERGETICS

BRIEFING FOR

ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS (ACRS)
CRBRP SUBCOMMITTEE

NOVEMBER 19, 1982

# CRBRP PROJECT PRESENTATION AGENDA

| | | |
|---|---|---|
| I. | Introduction | H. K. Fauske (FAI) |
| II. | CRBRP Sodium Void Worth and Uncertainties | H. H. Henryson (ANL) |
| III. | LOF Reference Initiating Phase Behavior | D. Weber (ANL) |
| IV. | Recriticality by Extended Fuel Motion | M. Epstein (FAI) |
| V. | Dispersal by Extended Fuel Motion | M. Epstein (FAI) |
| VI. | Conclusion | D. Switick (G.E.) |

# CRBRP SODIUM VOID WORTH
# AND UNCERTAINTIES

HERB HENRYSON

(ANL)

# SODIUM VOID WORTH

## ALTERNATIVE METHODOLOGIES

- USE STATE-OF-THE-ART METHODS AND CROSS SECTION DATA FOR CALCULATIONS. DETERMINE UNCERTAINTIES FROM "KNOWLEDGE" OF UNCERTAINTIES IN METHODS/DATA.
- USE INTEGRAL DATA BASE TO DERIVE AN "EXPERIMENTAL" VALUE. UNCERTAINTIES FALL OUT OF ANALYSIS

# BIAS FACTOR METHOD

- SINGLE BIAS FACTOR

$$P = \alpha C$$

- TWO FACTORS (LEAKAGE AND NON-LEAKAGE)

$$P = \beta N + \gamma L$$

$$\text{MIN} \sum_{I} \left( \frac{P_I - E_I}{\sigma_I} \right)^2$$

## COMPUTATIONAL MODEL

- ENDF/B-IV DATA
- $MC^2$-2/SDX PROCESSING TO 20 ENERGY GROUPS
- THREE-DIMENSIONAL DIFFUSION THEORY
- CORRECT FOR STREAMING USING BENOIST DIRECTIONAL DIFFUSION COEFFICIENTS
- EXACT PERTURBATION THEORY

Ratios of Calculated to Measured Reactivities for Sodium Voiding

| Cases | C/E Before Biasing | % Standard Deviation After Biasing[a] |
|---|---|---|
| CRBR-EMC[b] BOC-1, positive part of core | 0.98 | 10 |
| CRBR-EMC EOC-4, positive part of core | 1.23 | 6 |
| 101 mixed zones | 1.08 | 12 |
| Axial blankets without control rods | 0.91 | 1 |
| Axial blankets with control rods | 1.23 | 2 |
| Core zones with negative reactivity signals | 1.02 | 9 |

[a] Separate bias factors applied to positive and negative components of reactivity. For any subset, the average C/E is 1.0 after biasing.

[b] Engineering mockup critical experiments for sodium-void reactivity in CRBR; reactor geometry and composition closely matched.

### Bias Factors and Uncertainties for Sodium-void Reactivity in CRBR

| Zone | Bias Factor[a] | | Calculational Uncertainty,[b] % | |
|---|---|---|---|---|
| | BOC-1 | EOC-4 | BOC-1 | EOC-4 |
| Central core | 1.0 | 0.82 | 10 | 6 |
| External core | 1.0 | 1.0 | 10 | 10 |
| Axial blankets | 1.0 | 1.0 | 20 | 20 |
| Internal blankets | 1.0 | 1.0 | 20 | 20 |

[a] to be multiplied times the calculated value.

[b] to be added in quadrature with uncertainties from other sources.

Additional Uncertainties
in CRBR Sodium-void Reactivity

| Source | Uncertainty[a], % of Total Reactivity | |
|---|---|---|
| | BOC-1 | EOC-4 |
| Fuel pins instead of plates | 0 | 0 |
| Sequence of voiding | 3.5 | 3.5 |
| Temperature distribution | 2.5 | 2.5 |
| Fission products | 0 | 3.0 |

[a] To be added in quadrature with the values of "experimental" uncertainty.

## BEST ESTIMATE SODIUM VOID[a] REACTIVITY WORTHS ($)[b]

| | BOC-1 | | EOC-4 | |
|---|---|---|---|---|
| DRIVER ASSEMBLIES | | | | |
| CORE | 0.25 | (-0.35) | 1.44 | (1.10) |
| LOWER AXIAL BLANKET | -0.22 | | -0.15 | |
| UPPER AXIAL BLANKET | -0.18 | | -0.17 | |
| TOTAL | -0.15 | | 1.12 | |
| MAXIMUM POSITIVE | | | 1.97 | (1.67) |
| INTERNAL BLANKET ASSEMBLIES | | | | |
| CORE | 1.37 | (0.95) | 1.50 | (1.11) |
| LOWER AXIAL EXTENSION | 0.01 | | 0.02 | |
| UPPER AXIAL EXTENSION | -0.01 | | -0.01 | |
| TOTAL | 1.37 | | 1.51 | |
| MAXIMUM POSITIVE | | | 1.52 | (1.12) |

[a] VOID FLOWING SODIUM (81.8% DRIVER, 72.6% BLANKET)

[b] $\beta$ = .0034

# LOF REFERENCE INITIATING
# PHASE BEHAVIOR

DAVE WEBER

(ANL)

# CRBRP BEST ESTIMATE EOC-4 LOF Assessment

- Motivation
  - Reassessment motivated by consideration of potential for autocatalytic fuel behavior due to pressurized plenum fission gas compaction of disrupting fuel pins.
- Technical Approach
  - Core conditions (powers, flows, material worths, etc.) based on CRBRP-GEFR-523 EOC-4 conditions.
  - Fuel motion at pin disruption based on ANL TREAT LOF tests under overpower conditions (principally, tests L6 and L7) within context of SAS3D/SLUMPY code.
  - Fission gas availability for dispersal based on HEDL fission gas release (FGR) tests and analytical modeling in ANL/RAS developed FRAS code.
- Significant Conclusions
  - Time scale of accident sequence increased compared to GEFR-523.
  - Early fuel dispersal in lead channel led to mild ($5P_0$) overpower condition.
  - Mild entry to meltout phase is predicted.

# POTENTIAL FOR AUTOCATALYSIS BY PLENUM FISSION GAS DRIVEN PIN COMPACTION

- PHENOMENOLOGICAL CONCEPT
  - AT FUEL PIN DISRUPTION, PLENUM FISSION GAS PRESSURES MAY BE TENS OF ATMOSPHERES, POTENTIALLY PROVIDING A MECHANISM FOR FORCED FUEL SLUMPING AND AUGMENTED POSITIVE REACTIVITY INSERTION (NUREG-0122,NUREG/CR-0224)
- MITIGATING FACTORS
  - EARLY PIN DEPRESSURIZATION BY CLAD FAILURE.
  - FRICTION AND MECHANICAL INTERFERENCE BETWEEN FUEL AND CLADDING.
  - UPWARD CLADDING STUB RELOCATION.
  - INTRA/INTER SUBASSEMBLY INCOHERENCE.
- TECHNICAL APPROACH
  - NEUTRONICS BASED ON GEFR-523.
  - FUEL COMPACTION DRIVEN BY PRESSURE DIFFERENTIAL AND GRAVITY.
  - CLAD FAILURE AT STEEL MELTING POINT, DESPITE IRRADIATED CLADDING.
  - FAILURE LOCATION AT CORE-UPPER AXIAL BLANKET INTERFACE.
  - TYPICAL SAS3D/CLAZAS MOLTEN CLADDING RELOCATION DYNAMICS.
- CONCLUSIONS
  - SIGNIFICANT MITIGATING FACTORS EXIST THAT WOULD PREVENT COHERENT PLENUM GAS DRIVEN COMPACTION.
  - ANALYSES WHICH DO NOT INCLUDE FACTORS CAUSING INCOHERENCY SHOW:
    - SUFFICIENT TIME FOR PLENUM FISSION GAS BLOWDOWN PRIOR TO PIN DISRUPTION WAS OBSERVED.
    - PLENUM FISSION GAS NOT SEEN TO AFFECT FUEL MOTION.

## Updated Material Worth and Uncertainty Evaluation

- Material Worth Assessments
    - Driver assembly void worth increase from $1.10 to $1.43 (30%).
    - Driver assembly steel worths (clad & wire wrap) increase from $4.31 to $5.16 (20%).
- Increased Material Worth Implications
    - Shortened time scale for power rise and initiation of fuel disruption.
    - Limited time available for plenum fission gas blowdown to eliminate potential for autocatalytic fuel compaction.
    - Higher potential for retaining in-pin fission gas to drive fuel dispersal.
    - Increased sensitivity to phenomenological modeling (e.g., pin failure, clad motion).
- Motivation for and elements of refined analysis.
    - Analysis philosophy of basing results on modeling that is consistent with observed experimental behavior.
    - Expand analysis to details of fission gas blowdown and clad relocation.

## DIRECT COMPARISONS OF MATERIAL WORTH

## IMPLICATIONS WITH CONSERVATIVE MODELING ASSUMPTIONS

- CASE STUDIES
  - CASE 1 - CRBRP-GEFR-523 MATERIAL WORTHS.
  - CASE 2 - ANL/AP MATERIAL WORTHS.
- KEY MODELING ASSUMPTION
  - FUEL MOTION AT PIN DISRUPTION BASED ON TREAT L6-L7/SAS3D-SLUMPY/FRAS-3.
  - CLAD MOTION BASED ON SAS3D/CLAZAS MODELING WITH STRONG VAPOR COUPLING TO (FLOODED) MOLTEN CLAD FILM.
  - PIN FAILURE/PLENUM DEPRESSURIZATION TIMING AT CORE/UAB INTERFACE AT 1400°C (LOCAL CLAD MELTING) NOTED, BUT NOT USED IN COOLANT DYNAMICS OR CLAD FUEL MOTION.
- NOTABLE RESULTS (CASE 2 RELATIVE TO CASE 1)
  - TIME TO REACH INITIAL BOILING REDUCED BY 1 SEC.
  - TIME TO INITIATE PIN DISRUPTION REDUCED BY 2 SEC.
  - MAXIMUM REACTIVITY INCREASED BY 26 CENTS.
  - MAXIMUM POWER INCREASED BY FACTOR OF 4.
  - ACCIDENT TIME SCALE REDUCED BY 2-3 SECONDS.
- OBSERVATIONS
  - POTENTIAL FOR PLENUM GAS COMPACTION INCREASED.
  - SIGNIFICANT CLAD RELOCATION (POSITIVE REACTIVITY) CALCULATED.

# OBSERVATIONS ON CLAD RELOCATION

## EXPERIMENTAL EVIDENCE AND SAS3D/CLAZAS PREDICTIONS

- CRBR EOC-4 CALCULATED RESULTS.
  - POSITIVE REACTIVITY EFFECT (50¢ TO $1.00) RAISES POWER AND COMPRESSES TIME SCALE, IMPLYING GREATER SENSITIVITY TO SUBSEQUENT MOTIONS.
  - MOLTEN CLAD VELOCITIES AS HIGH AS 200 CM/SEC, WITH ASSUMED CLADDING FLOODING.
  - NET UPWARD RELOCATION DESPITE PLENUM GAS EJECTION AND CHANNEL PRESSURIZATION.
- EXPERIMENTAL OBSERVATIONS
  - TREAT R4/R5 7-PIN FRESH FUEL LOSS-OF-FLOW TEST TO STUDY VOIDING DYNAMICS AND CLADDING RELOCATION.
    - THERMOCOUPLE RESPONSE SUGGEST CLAD VELOCITIES OF 50-70 CM/SEC.
    - THIN (0.3 CM) UPPER BLOCKAGE OBSERVED.
    - SAS3D/CLAZAS PREDICTIONS:
      - UPWARD CLADDING VELOCITIES OF 200 CM/SEC.
      - THICK (5 CM) BLOCKAGE PREDICTED.
      - CALCULATED RESULTS SOMEWHAT INDEPENDENT OF VAPOR FRICTIONAL COUPLING.
  - SLSF P3A LOW BURNUP FUEL 37-PIN LOSS-OF-FLOW TEST.
    - THERMOCOUPLE RESPONSE SUGGEST CLAD VELOCITIES OF 20 CM/SEC.
    - THIN (2 CM) BLOCKAGE OBSERVED.
    - SAS3D/CLAZAS PREDICTIONS.
      - UPWARD CLADDING VELOCITIES OF ~ 180 CM/SEC.
      - THICK (8 CM) BLOCKAGE PREDICTED.

- TREAT R8 7-PIN PRESSURIZED (3 PINS) FRESH FUEL LOSS-OF-FLOW TEST TO STUDY PLENUM FISSION GAS EFFECT ON VOIDING DYNAMICS AND CLADDING RELOCATION.
  - CLADDING FAILURE OBSERVED BELOW TOP OF ACTIVE FUEL, WITH A CLADDING STUB EJECTED UPWARDS.
  - CHANNEL PRESSURIZATION AND COOLANT SLUG EJECTION OBSERVED.
  - NO PLANAR CLADDING BLOCKAGE FORMED AT TOP OF FUEL.
  - SAS3D/CLAZAS PREDICTIONS.
    - FISSION GAS PLENUM DEPRESSURIZATION TIMING WELL REPRESENTED.
    - TYPICAL "FLOODED" PRESSURE DROP PREDICTS UPWARD CLADDING RELOCATION.
    - SMOOTH TUBE FRICTION RESULTS IN CLAD DRAINING AND NO UPPER CLADDING BLOCKAGE.
- CONCLUSIONS
  - SAS3D/CLAZAS SIGNIFICANTLY OVERPREDICTS UPWARD CLADDING RELOCATION.
  - CLAZAS MODELING, BASED ON R5 RESULTS (SMOOTH TUBE FRICTION) IS A CONSERVATIVE ESTIMATE OF THE POSITIVE REACTIVITY EFFECT.
  - PLENUM GAS EJECTION WITH SMOOTH TUBE FRICTION RESULTS IN NET DOWNWARD MOTION AND NEGATIVE REACTIVITY EFFECT.
  - A REALISTIC CLAD MOTION ASSUMPTION, THOUGH STILL CONSERVATIVE IN THE IMPORTANT EARLY TIMES, IS NO NET RELOCATION IN THE PRESENCE OF PLENUM FISSION GAS BLOWDOWN.

## ASSESSMENT OF CLADDING FAILURE AT THE FUEL-BLANKET INTERFACE UNDER PLENUM PRESSURE LOADING CONDITIONS

- PREVIOUS PLENUM BLOWDOWN CALCULATIONS ASSUMED CLADDING FAILURE AT MELTING ~1400C.
- REVIEW OF SAS3D ANALYSES OF EOC4 LOF BEHAVIOR SHOWS THAT:
  - FLUENCES ~ 3.5 - 7.5 $\times 10^{22}$ N/CM$^2$.
  - PLENUM PRESSURE ~ 20 - 45 ATM.
  - HEATING RATES NEAR MELTING ~ 100 - 500 C°/S
- RELEVANT DATA WAS REVIEWED.
  - HEDL FCTT TESTS.
  - HEDL FCTT/TUCOP TESTS.
- CALCULATIONS OF CLADDING FAILURE WERE PERFORMED USING
  - DATA CORRELATIONS.
  - THEORETICAL MODELS.
- THE ASSESSMENT OF THE DATA AND THE CALCULATIONS HAS LED TO A BEST ESTIMATE OF 1300°C FOR THE CLADDING FAILURE CRITERION IN THE SAS3D PLENUM BLOWDOWN CALCULATIONS.

# BEST ESTIMATE EOC-4 LOF SCENARIO WITH UPDATED WORTHS

- KEY ASSUMPTIONS
  - FUEL MOTION AT PIN DISRUPTION MODELED CONSISTENTLY WITH TREAT LOF OVERPOWER EXPERIMENTS L6 AND L7.
  - CLADDING RELOCATION MODELED CONSISTENTLY WITH TREAT LOF EXPERIMENTS R4/R5 AND R8 AND SLSF EXPERIMENT P3A.
  - CLADDING FAILURE PREDICTIONS MODELED CONSISTENTLY WITH HEDL BURST DATA AND APPROPRIATE THEORY.

RESULTS

- PEAK POWER OF $5P_0$ AND PEAK REACTIVITY OF 65¢.
- TIME SCALE TO REACH COOLANT BOILING AND INITIATE FUEL MOTION SIMILAR TO PREVIOUS (CASE 2) ASSESSMENT AND SHORTER THAN LOWER VOID WORTH CASE (CASE 1).
- PLENUM FISSION GAS DEPRESSURIZATION IS EXPECTED IN ALL CHANNELS PRIOR TO PIN DISRUPTION.

CONCLUSION

- MILD ENTRY TO MELTOUT PHASE IS PREDICTED.
- AUTOCATALYTIC BEHAVIOR IS NOT PREDICTED.

# BEST ESTIMATE EOC-4 LOF SCENARIO WITH UPDATED WORTHS

## SUMMARY

- THE POTENTIAL FOR AUTOCATALYSIS BASED ON AN IDEAL CONCEPT OF PLENUM FISSION GAS DRIVEN FUEL COMPACTION HAS BEEN EXAMINED. MITIGATING FACTORS THAT WOULD PREVENT COHERENT COMPACTION HAVE NOT BEEN INCLUDED.
- INITIAL ASSESSMENT (BASED ON GEFR-523 EOC-4 LOF BASE CASE 1A) SHOWED COMPLETE BLOWDOWN IN EARLY ASSEMBLIES BUT POTENTIAL FOR COMPACTION IN LATER ASSEMBLIES.
- ASSESSMENT OF AUTOCATALYTIC COMPACTION POTENTIAL IS AFFECTED BY CONSERVATIVE MODELING OF EARLY FUEL MOTION, CLAD MOTION, AND PLENUM GAS EFFECTS ON VAPOR DYNAMICS.
- TREAT LOF TESTS, ESPECIALLY L6 AND L7, WERE IDENTIFIED AS THE MOST RELEVANT DATABASE FOR FUEL MOTION AND EXTENSIVE SAS3D/SLUMPY ANALYSES WERE PERFORMED.
- FISSION GAS AVAILABILITY AND DISTRIBUTION WERE DETERMINED WITH FRAS3 CODE.
- CLADDING MOTION UNDER LOF CONDITIONS WITH AND WITHOUT THE EFFECT OF PLENUM FISSION GAS EJECTION WAS ASSESSED USING TREAT (R5 AND R8) AND SLSF(P3A) DATA AND WAS REFLECTED IN THE SAS3D CLAD MOTION MODEL CLAZAS.
- FAILURE OF IRRADIATED CLADDING WAS REVIEWED TO ESTABLISH A CLAD FAILURE CRITERION.
- WHOLE CORE ANALYSES WERE PERFORMED WITH EXPERIMENTALLY CONSISTENT MODELING.

## CONCLUSION

- BEST ESTIMATE WHOLE CORE ANALYSES, USING EXPERIMENTALLY CONSISTENT MODELING, SHOW A MILD ENTRY TO THE MELTOUT PHASE IS PREDICTED AND AUTOCATALYTIC FUEL COMPACTION DRIVEN BY PLENUM FISSION GAS IS UNLIKELY.

# RECRITICALITY BY EXTENDED FUEL MOTION

MICHAEL EPSTEIN

(FAI)

# GENERIC ISSUES COVERED

1. Definition of melt-out phase.

2. Duration of melt-out phase and sensitivity to initial conditions (power level).

3. Recriticality and related phenomena.

4. Fuel freezing mechanisms and removal paths.

5. Fuel removal requirements for permanent subcriticality.

6. Sodium re-entry via steel vapor condensation.

# POOL DEFINITIONS

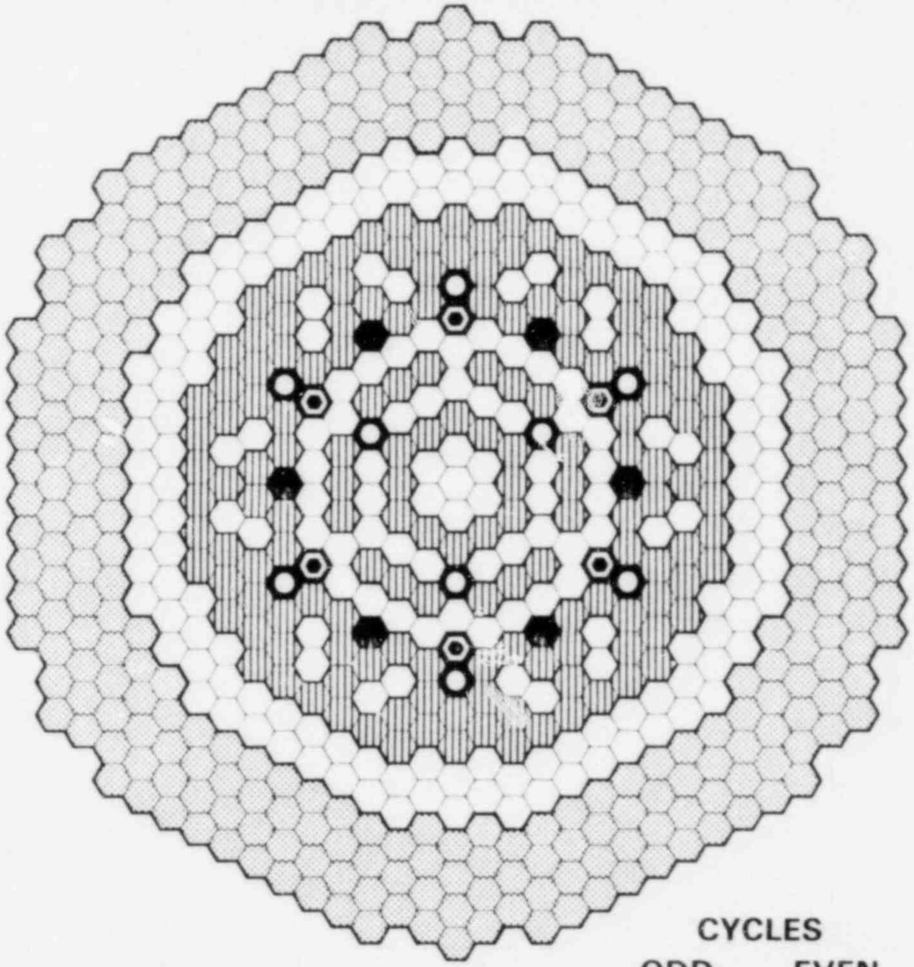
1. MELT-OUT/ANNULAR POOL PHASE - MERGING OF MOLTEN DRIVER FUEL ASSEMBLIES WHILE THE INNER BLANKET FUEL ASSEMBLIES REMAIN INTACT.

2. LARGE SCALE POOL - CONFIGURATION AFTER THE MELTING OF THE INNER BLANKET ASSEMBLIES.

# BASIS FOR CONSIDERATION OF RECRITICALITY BY EXTENDED FUEL MOTION IN MELT-OUT ANNULAR POOL PHASE

- PRESSURE DRIVEN FUEL COMPACTION FROM FUEL COOLANT INTERACTIONS CAN BE RULED OUT.
- RAPID MIXING OF COLD STEEL PRECLUDED BY FUEL CRUST FORMATION.
- CONSIDERATION OF GEOMETRY AND POWER LEVEL IN EARLY MELT-OUT PHASE INDICATE THAT MILD RECRITICALITY EVENTS MAY BE POSSIBLE.
- THE MAGNITUDE OF SUCH EVENTS CAN BE BOUNDED AND THEIR RECURRENCE LIMITED BY PHYSICAL LAWS.

# CRBRP HETEROGENEOUS CORE DESIGN



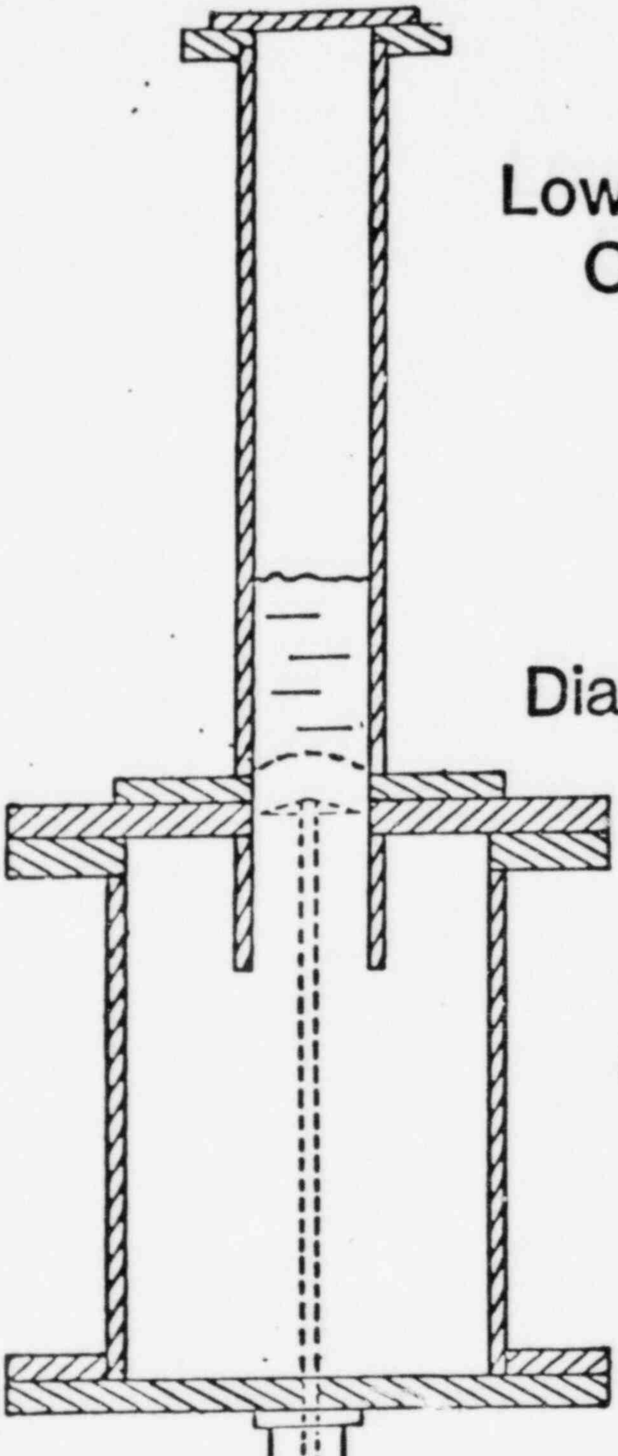
| | | CYCLES | |
|---|---|---|---|
| | | ODD | EVEN |
| ▥ | FUEL | 156 | 162 |
| ⬡ | BLANKET | 208 | 202 |
| ▨ | RADIAL SHIELD | 312 | |
| ◎ | PRIMARY CONTROL | 9 | |
| ⬢ | SECONDARY CONTROL | 6 | |
| ⊙ | ALTERNATE FUEL/BLANKET | 6 | |

1 82 1296 18 8(OV1-82 1431 40 3)

# FLOW REGIME AND RECRITICALITY CONSIDERATIONS DURING THE MELT-OUT/ANNULAR POOL PHASE

- Experiment
- Analytical Considerations

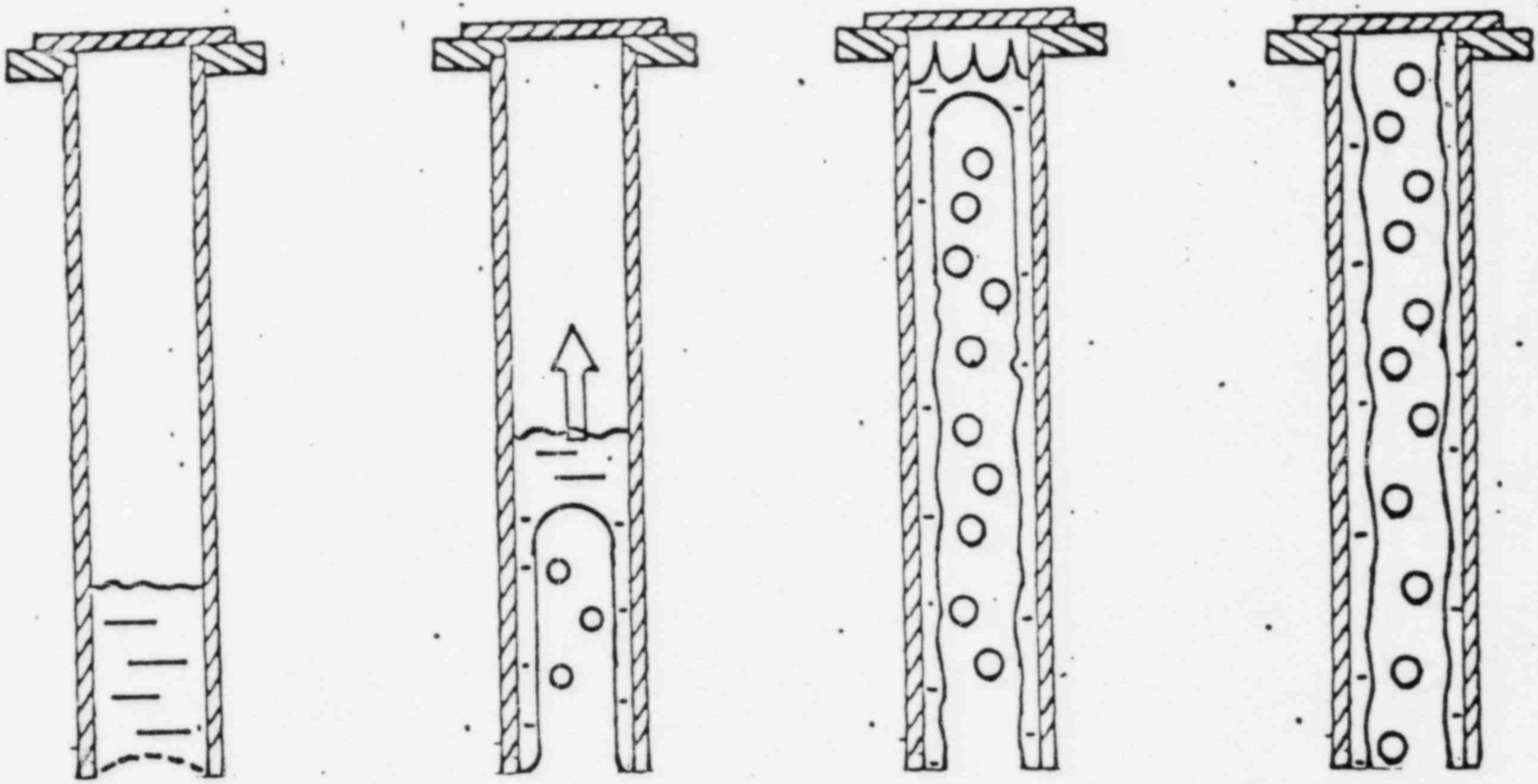
# Liquid Slug Instability Experiment



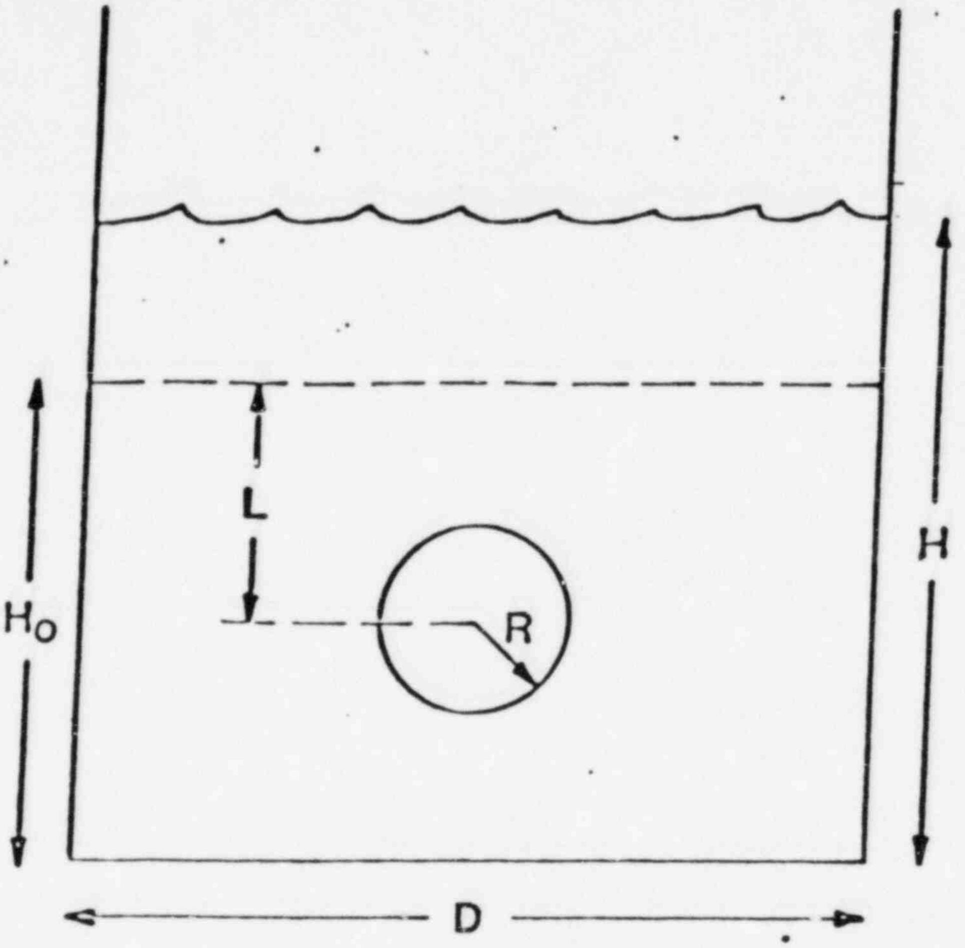
Low Pressure
Channel

Diaphragm

High Pressure
Gas Reservoi



EXPERIMENTAL OBSERVATION



$$R > \frac{D^2}{16L}$$

ANALYTICAL CONSIDERATIONS; 1D vs. 3D

# NEUTRONIC EVENTS DURING THE MELT-OUT/ANNULAR POOL PHASE

1. IF RECRITICALITIES SHOULD OCCUR THEY ARE MILD AND DO NOT AMPLIFY.

2. ASSEMBLY WALL/FUEL MIXING IS MINIMAL DUE TO FUEL CRUSTING AND MELT LAYER STABILITY.

3. FCI RULED OUT AS SOURCE OF RECRITICALITY EVENTS.

# DISPERSAL BY EXTENDED FUEL MOTION

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(FAI)

# TERMINATION OF MELT-OUT/ANNULAR POOL PHASE

1. Fuel removal paths are available.

2. Fuel removal is sufficient to assure permanent subcriticality even when assessed with conservative models.

# REQUIREMENTS FOR PERMANENT SUBCRITICALITY

## REACTIVITY LEVELS FOR VARIOUS DISRUPTED CORE CONFIGURATIONS AT BOC-1

| Case | Description of Core Configuration | Reactivity ($) |
|---|---|---|
| 1 | 43% of total fuel inventory removed from the core. The remaining fuel in the annular regions is homogenized in the core and full compacted with IB and CR assemblies intact. | -1.4 |
| 2 | Same as Case 1 except that only 33% of total fuel inventory is removed. | +10.2 |
| 3 | Same as 2 except fuel boils up with a linear/uniform void fraction. | -6/-37 |
| 4 | 41% of total inventory removed from core. The remaining fuel, the IB and CR (except $B_4C$) assemblies are homogenized and fully compact. | -10.5 |

CONCLUSION:

Removal of ≈ 40% of driver fuel inventory is sufficient to assure permanent subcriticality.

# LIFETIME OF INNER BLANKET ASSEMBLIES

ASSUMPTION: MODERATE POWER BURST OF 6 TO 10 FULL POWER SECONDS AT END OF EOC-4 INITIATING PHASE. SUBSEQUENT POWER LEVEL BOUNDED BY 50% OF NOMINAL TO PRECLUDE RE-CRITICALITY ON AN ASSEMBLY SCALE.*

CONDITIONS: IB - LIMITED CLAD MELTING.

- AVERAGE FUEL TEMPERATURE FOR, UNCLAD SEGMENT 2100°C, LOWER CLAD SEGMENTS 1600°C.

RB - BOILING INITIATED.

LIFETIME OF IB BARRIER FOR EOC-4 CORE AFTER FUEL PENETRATION**

MIDDLE SECTION - 12 TO 17 SEC

LOWER SECTION - 30 TO 45 SEC

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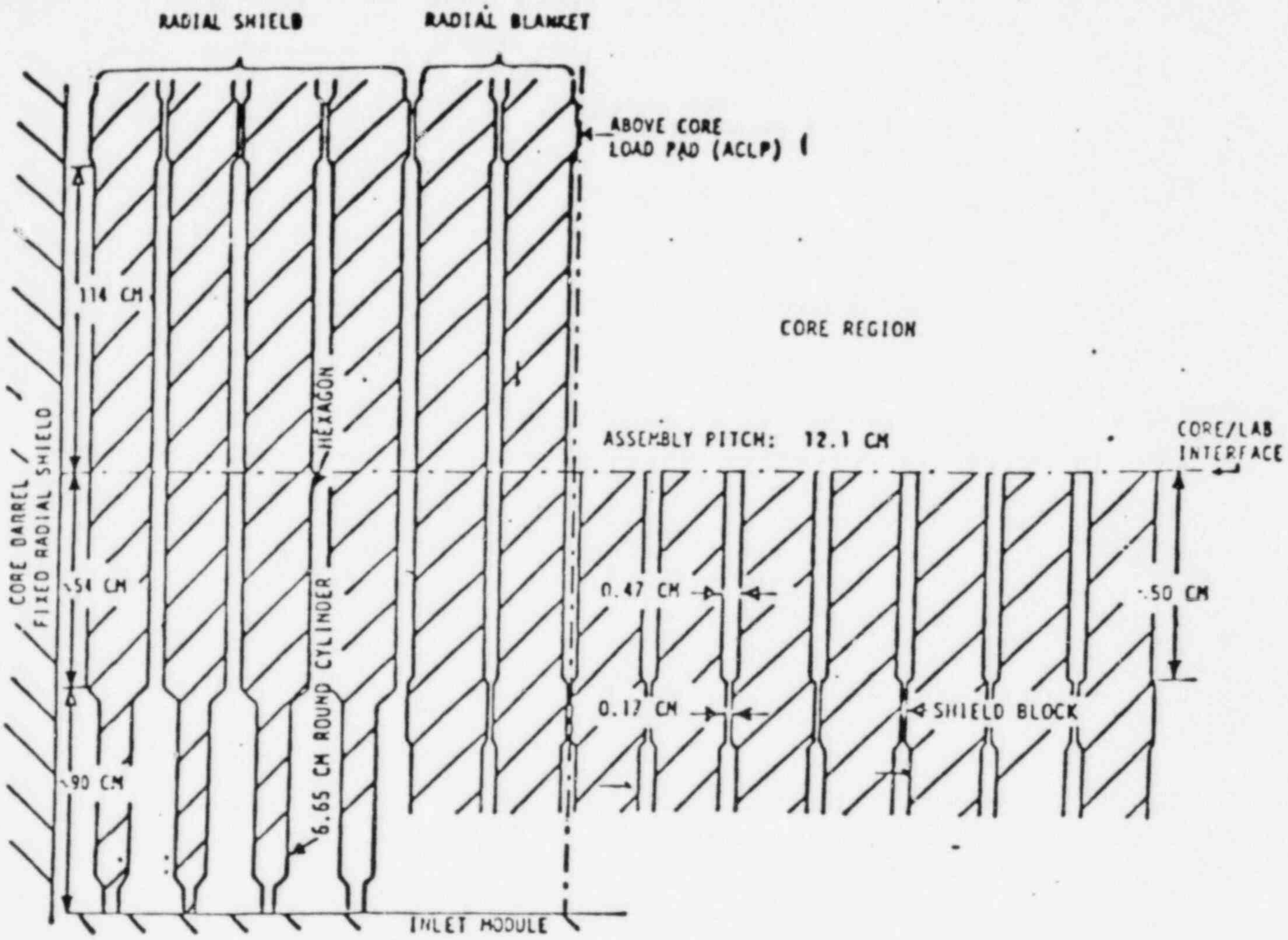
*CHOICE OF THE POWER LEVEL IS NOT IMPORTANT SO LONG AS LARGE RAMP RATE RECRITICALITIES CAN BE PRECLUDED.

**TIME SCALE IS APPROXIMATELY A FACTOR OF 2 TO 3 LONGER FOR BOC-1 CONDITIONS.

# FUEL REMOVAL PATHS

# INTERASSEMBLY GAPS



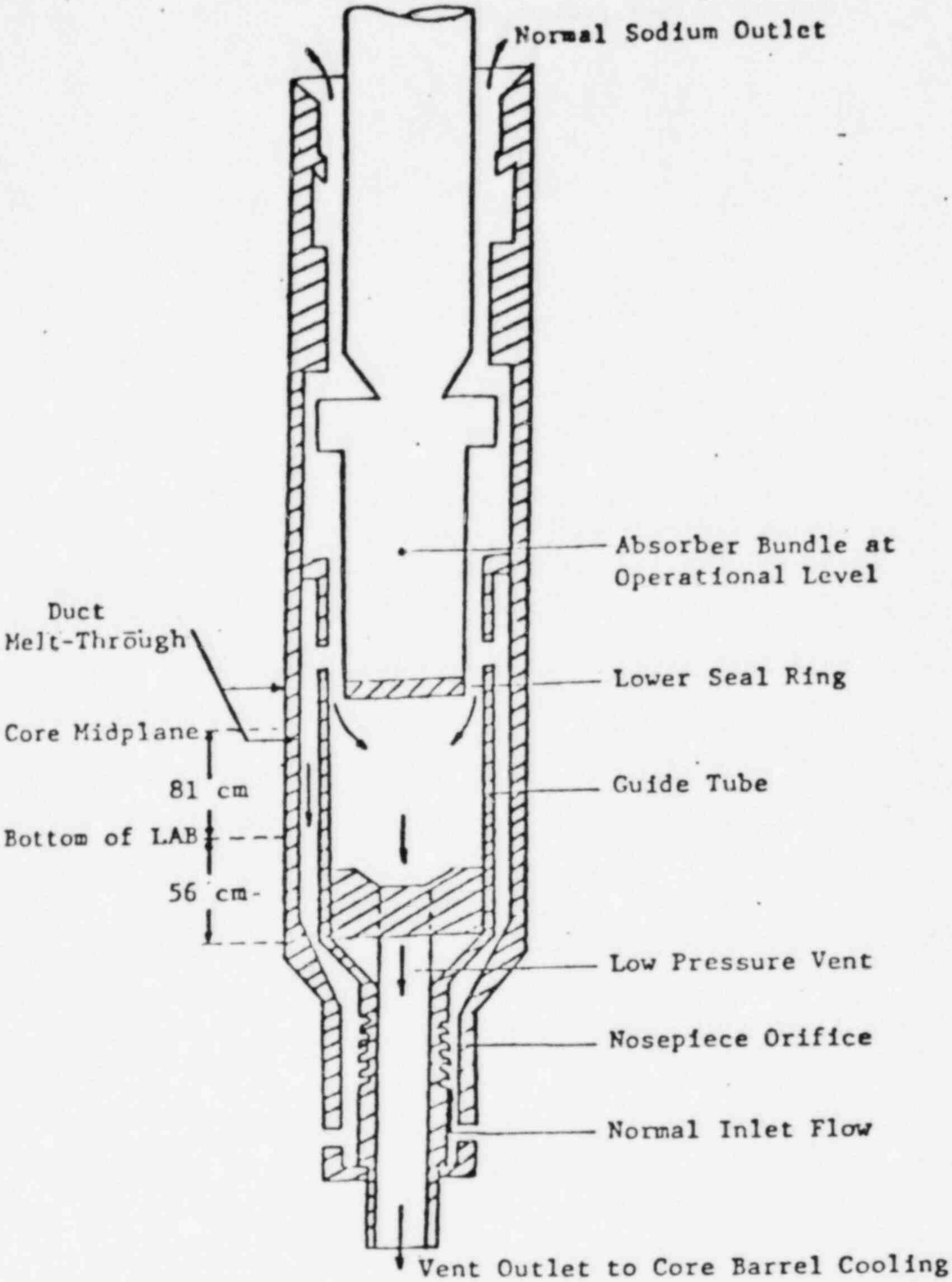
*Sketch Showing the Interstitial Gaps Outside and Below the Core Region.*

# EFFECT OF SODIUM IMPEDANCE ON FUEL PENETRATION INTO GAPS

1. Conduction Model: Penetration length is reduced by at most 40%. This reduction does not alter the fuel removal inventory.

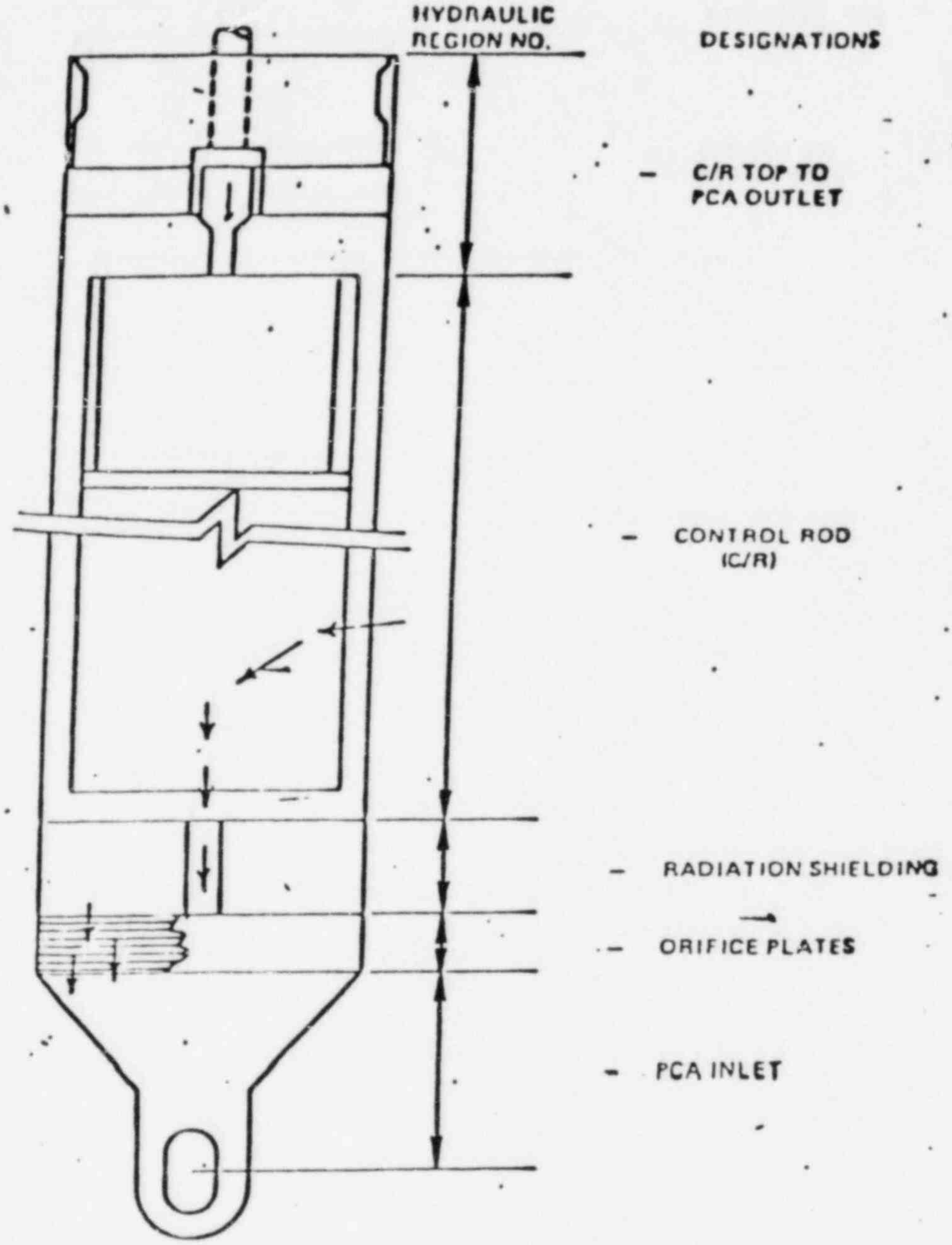
2. Bulk Freezing Model: No effect on penetration length.

# SECONDARY CONTROL ASSEMBLY



*Schematic of SCA Flow Paths for Fuel Removal (not to scale).*

# PRIMARY CONTROL ASSEMBLY



*Schematic of Primary Control Assembly.*
*⟶ Indicates Fuel Melt Path.*

# SUMMARY OF MELT-OUT
# PHASE FUEL REMOVAL PATHS

## 1. INTERASSEMBLY GAPS (IG)

| MODEL | RATE | CAPACITY |
|---|---|---|
| THERMAL CONDUCTION LIMITED | FUEL MELT LIMITED | > 40% OF FUEL |
| BULK FREEZING MODEL | FUEL MELT LIMITED | ≈ 15% BASED ON BOC GAPS<br>≈ 10% BASED ON EOC GAPS |

## 2. CONTROL ROD ASSEMBLIES

- Primary Control Assembly (PCA):

  4 sec melt-through time.

  6% inventory to fill PCA volume below core/lower axial blanket interface.

  Fuel removal at 1%/sec for 4 sec then increase to 2.3%/sec.

- Secondary Control Assembly (SCA)

  6 sec melt-through time.

  4% inventory to fill SCA volume below core/lower axial blanket interface.

  Fuel removal at 5%/sec.

## 3. UPPER AXIAL BLANKET (UAB)

| | | |
|---|---|---|
| BOC Core | ≈ 0 | Conservative Assessment |
| EOC Core | > 25% | Based on Limited Clad Blockage |

# 4. RADIAL BLANKETS (RB)

First Row Radial Blankets: ≈ 20% of Core Fuel

(ON SAME TIME SCALE AS THE INNER BLANKET LIFETIME)

# POSSIBLE FUEL REMOVAL SCENARIOS

I. Early Fuel Removal Through Interassembly Gaps

> 40% Fuel Removal Prior to Melt-Out of IB

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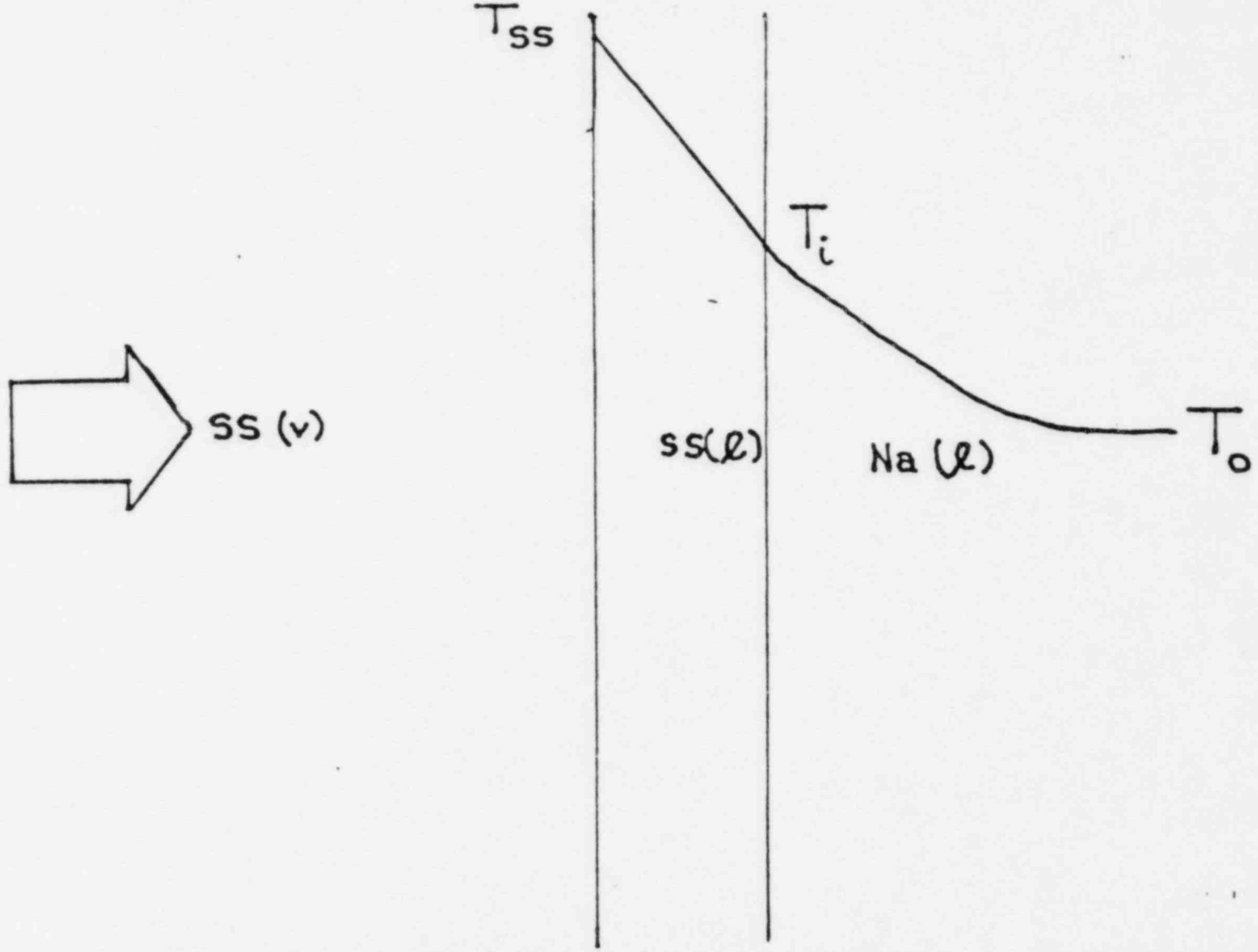
II. Limited Fuel Removal Through Interassembly Gaps

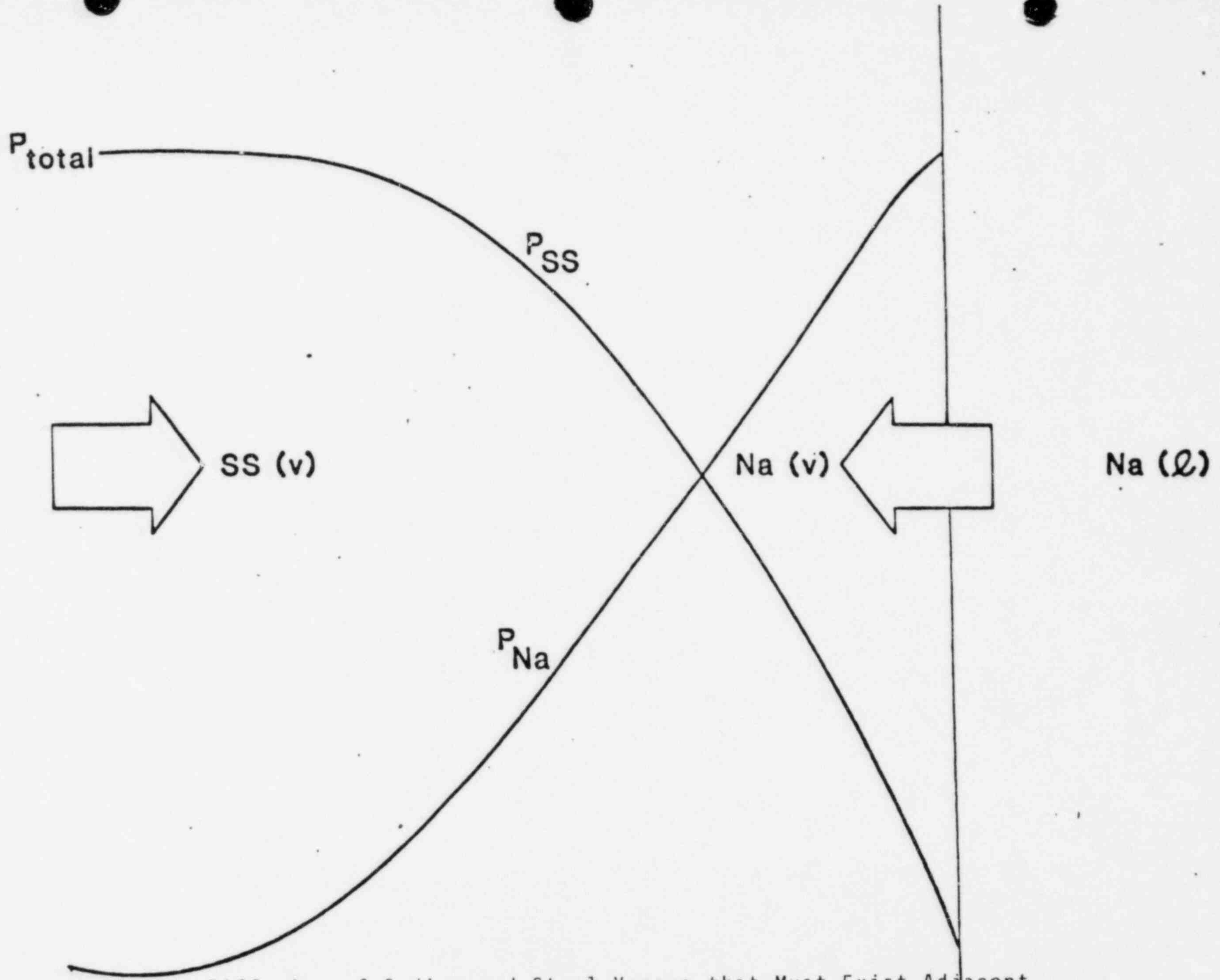
% of Driver Fuel Inventory Removal Within 10 sec After Fuel Penetration into IB Assemblies (Potential)

| Path | BOC | EOC |
|---|---|---|
| IG | 15% | 10% |
| PCA | 24% | 24% |
| SCA | 44% | 44% |
| UAB | 0 | 25% |
| RB | 20% | 20% |
| TOTAL | > 100% | > 100% |

# CONSIDERATION OF POOL SODIUM ENTRY VIA RAPID CONDENSATION OF STEEL VAPOR PRESSURE



Physical Model for Calculation of Liquid Sodium-Steel Condensate Interface Temperature <u>Assuming</u> Rapid Steel Vapor Condensation on Liquid Sodium



Counter Diffusion of Sodium and Steel Vapors that Must Exist Adjacent to the Liquid Sodium Surface and Prevents Sodium Re-Entry (Via Rapid Steel Condensation)

# ACCIDENT TERMINATION SUMMARY

1. ONCE MOLTEN FUEL BECOMES AVAILABLE ON AN ASSEMBLY BASIS, MILD RECRITICALITY EVENTS MAY BE POSSIBLE BUT THEY ARE LIMITED IN AMPLITUDE AND DO NOT AMPLIFY.

2. MULTIPLE PATHS FOR FUEL REMOVAL ARE AVAILABLE ON A SHORT TIME SCALE, RELATIVE TO THE MELT-OUT OF INTERNAL BLANKET ASSEMBLIES. CORRESPONDINGLY, FUEL REMOVAL IS NOT OVERLY SENSITIVE TO FUEL PENETRATION MODEL ASSUMPTIONS AND FUEL ESCAPE IMPEDANCES.

3. THERE IS ALWAYS TIME FOR SUFFICIENT FUEL REMOVAL, I.E., ABOUT 40% OF THE DRIVER FUEL, TO ACHIEVE PERMANENT SUBCRITICALITY PRIOR TO LOSS OF THE ANNULAR INNER BLANKET BARRIER.

4. THE ACCIDENT SEQUENCE WILL TERMINATE BENIGNLY WITHOUT THE DEVELOPMENT OF A HOMOGENEOUS LARGE SCALE CONFINED POOL PHASE AS DEFINED IN GEFR 00523.

5. SODIUM RE-ENTRY VIA STEEL VAPOR CONDENSATION CAN BE RULED OUT ON THE BASIS OF EXCESSIVE SODIUM VAPORIZATION WHEN LIQUID SODIUM COMES INTO CONTACT WITH STEEL VAPOR.

# SUMMARY AND CONCLUSIONS ON HCDA ENERGETICS

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# SUMMARY & CONCLUSIONS ON HCDA ENERGETICS

- RESPONSES TO SPECIFIC NRC QUESTIONS (760.178) ON HCDA ENERGETICS HAVE BEEN SUBMITTED.
- TOP EVENT IS NOT AN ENERGETICS CONTRIBUTOR OF SIGNIFICANCE EVEN UNDER CONSERVATIVE ASSUMPTIONS.
- THE LOF EVENT HAS BEEN REEVALUATED IN A CONSISTENT, INTEGRATED MANNER RELATIVE TO THE EFFECTS OF SODIUM VOID WORTH, PLENUM AND FUEL FISSION GAS EFFECTS, AND STEEL RELOCATION.
  - BEST-ESTIMATE RESULT IS A MILD POWER BURST WITH NONENERGETIC ENTRANCE TO MELTOUT PHASE
- ADDITIONAL ASSESSMENT OF THE MELTOUT/ANNULAR POOL PHASE SUPPORTS THE CONCLUSION THAT A NONENERGETIC TERMINATION IS EXPECTED EVEN FOR LESS PROBABLE SCENARIOS.
- THE ABOVE CONCLUSION IS LESS SENSITIVE TO TIMING AND DETAILS OF FUEL LOSS THAN ORIGINALLY PERCEIVED.
- SODIUM RE-ENTRY INTO A FUEL POOL VIA STEEL VAPOR CONDENSATION CAN BE RULED OUT.
- THE 661MJ EXPANSION PROVIDES ADEQUATE MARGIN FOR HCDA ENERGETICS.