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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

PUBLIC MEETING

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

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Nuclear Regulatory Commission
Jockey Room
Best West Airport Park Hotel
600 Avenue of Champions,
Los Angeles, California

Monday, June 30, 1980

The Committee met, pursuant to notice, at 8:30 a.m.

BEFORE:

DR. CARBON, Presiding
DR. KERR
DR. MARK
DR. PLESSET
DR. SHEWMON

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ALSO PRESENT:

DR. CATTON

DR. SIEGEL

DR. SAVIC

PROCEEDINGS

DR. CARBON: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Advanced Reactors.

I am Dr. Carbon, Subcommittee Chairman.

The other ACRS members present today are Dr. Kerr, Dr. Mark, Dr. Plesset, and Dr. Shewmon.

Also in attendance are the ACRS consultants, Dr. Catton and Dr. Siegel, and Dr. Savio of the ACRS staff.

The purpose of this meeting is to discuss the NRC-sponsored research on advanced reactors at LASL and Sandia.

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

Dr. R. Savio is the designated Federal employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice for this meeting previously published in the Federal Register on June 13 and June 26, 1980.

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

We have received no written statements from members of the public.

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

We will now proceed with the meeting, and I call upon Dr. Kelber of the NRC staff.

(The recorded proceedings begin at this point.)

MR. SCOTT: I hope this afternoon I will have time to show you some new results from this effort. There is a considerable amount of neutronics verification. I will just tell you one of the things that Ron Smith will tell you later. One of the things that we really find out is the tight coupling between the fluid dynamics and neutronics. It has a very large influence on the course of the accident, so it is quite necessary to know how well we are doing neutronically.

We are also exploring at a very low level of effort advanced fluid dynamics models and advanced neutronics models, in an effort not only to improve the accuracy but to improve the efficiency of the code, and we have over the last year explored the feasibility of a 3-D capability.

There are a variety of other activities in Q7, but really, they are mostly LWR, related to either LWR accident delineation or accident analysis which is not of any concern here.

Finally, I would like to summarize users of the SIMMER code. Of course, not everyone is using the thing the way that perhaps we are at Sandia. Sandia, Hetaline, ourselves, are probably the largest users. Some people put it to very strange use. I think Brigham Young is using it to model fluidized coal combustion. I have no idea what the University of Connecticut is using it for.

Among the foreign users, in Germany, it is operational at -- is being used to analyze accident transients in S&R 300 by the people at Carlsra. It is also being used by the people at Co ogne, GRS, the licensing agency, to analyze accident transients and S&R 300 also. Yes?

VOICE: It is also being used by the Bream Ticket Party at the University of Hamburg.

MR. SCOTT: The intervenors also have SIMMER up and running, I understand, and at the University of Hamburg. It is being used somewhat in the UK, not widely. It is being used in Ispra to investigate some accidents in the Common Market in the core, the European core, and I should point out that although not on this list, and although it is not currently operational in Japan, through the attache program with B&C, the Japanese are doing licensing calculations for Monjue using SIMMER at Los Alamos.

They expect to have it operational within three months, if I read Kironabi's last letter properly. Trying to interpret his letters sometimes is a little difficult.

That concludes what I have to say, if there are any questions.

DR. PLESSET: I would like to ask a question. Have you verified whether the code satisfies certain very elementary but basic principles, like the laws of conservation, mass, momentum, energy?

MR. SCOTT: Yes, we have attempted to verify that a number of times. It is not as straightforward as saying that it will always conserve mass, momentum, and energy. It is a olarian code. There are difficulties sometimes if the time step gets too large with the disappearance of energy from the code, but that can usually be corrected by adjusting the time stop.

DR. PLESSET: Well, the reason I ask it is that another very important code from your laboratory, they find that if you had a small break in the system, that the mass in the primary part of the system continually increases, which is somewhat disturbing. You might say that this code flunks Physics I.

(General laughter.)

DR. PLESSET: Now, we would have said your code passes Physics I.

MR. SCOTT: Yes. That is because we discovered this problem --

DR. PLESSET: What did you say?

MR. SCOTT: Yes. That is primarily because we discovered this problem somewhat earlier than the track people did, and we have worked on it over the past at least two years.

DR. PLESSET: And?

MR. SCOTT: And we usually -- I will tell you how we do it. There is a technique developed at Los Alamos for looking at the influence of time step, various convergence criteria, so forth, on the energy balance, the mass balance, and momentum balance.

When we set out to do a large calculation such as a transition phase calculation, we typically will do a small sensitivity study, same geometry, to determine what the optimum parameters are for most of these things, so that we do in fact satisfy conservation equations.

DR. PLESSET: What parameters would they be, for instance?

MR. SCOTT: I need help from someone helpful.

VOICE: I am with Los Alamos. Part of the numerical technique involves an implicit pressure --

DR. PLESSET: I am sorry, I don't hear it.

VOICE: It involves an implicit pressure iteration, and this requires an iteration to some convergence criteria, and the combination of the convergence criteria and the time step determines the amount of mass or energy conservation, and by setting up optimal parameters for time step and for the convergence criteria, you can guarantee that your mass is conserved to whatever percent or energy is conserved to whatever percent you would like.

Obviously, if you took such a criteria too small, your calculation is going to take forever to run, so if you want to conserve energy at say, a tenth of a percent, then you've got to tighten your criteria somehow.

DR. PLESSET: What about the effect of nodalization? Have you studied that?

MR. SCOTT: Yes, we have. We have studied it with regard to post-disassembly expansion problem. Once again, it seems to be problem dependent. Any time that -- well, it is primarily controlled, I believe, by the momentum equation. Any time we get into an accident environment in which we are likely to generate strong shocks, we have to use more and more nodes to get a good answer.

I have a few words to say this afternoon on the verification about nodalization in the presence of strong shocks.

As it turns out -- I will just tell you the punchline now -- somewhere between about 250 and 1,000 atmospheres SIMMER as it is currently constructed will not treat strong shocks well at all for pressure sources in there. We don't know yet where it really becomes bad, because we have a very limited number of experiments against which to test it.

It seems to perform extremely well from one atmosphere up to about 250. We do know from doing Cova experiment analysis that it performs rather poorly at 1,000 atmospheres unless you have a terrific number of nodes, a tremendous number.

DR. PLESSET: There is often, I think, I don't know much about these things, a kind of purely artificial or synthetic diffusion introduced into codes which you might say crudely is a product of the node size times the field -- velocity field. Now, this kind of spearing out is purely artificial. Now, you have studied this?

MR. SCOTT: We have looked at the influence, and I believe it is reported in the -- Who was that? Charlie Bell did the calculations. I forget where it was reported, but I will find out for you. We looked at the influence of olarian smearing on pressure decay in the core region following a disassembly burst, and looked at the influence of the various node sizes, and how that affects the artificial decay of pressure.

Pressure decays artificially because, as you point out, rightly, you have numerical diffusion, essentially, of mass into other cells, and since in this scheme any fuel which enters a cell is instantaneously equilibrated with all the other fuel, that tends to lower the pressures, but there is a reference for that. We have studied it. It is a problem. It tends to be a problem particularly in situations where you have large pressure gradients, as you would expect, and are trying to treat it in large nodes.

The only alternative if you want to prevent numerical diffusion in the presence of large pressure gradients is to use a larger number of nodes.

It does become asymptotic to no diffusion at all with the proper number of nodes. This study was done by Charlie Bell. I forget where it was reported, to tell the truth. Do either of you know?

VOICE: It was either in the sensitivity --

DR. SCOTT: Yes, it was in the sensitivity. I thin it was a document called "Impact of Model Uncertainties on SIMMER II Accident Analysis," I believe. I will find out for sure.

DR. PLESSET: I appreciate your telling me, and I am going to hold you to that, that this code passes Physics I.

DR SCOTT: All right. Mike Stevenson, I believe, has --

DR. STEVENSON: Dr. Carbon, Mike Stevenson, Los Alamos. Just to respond quickly to Dr. Plesset's concern over the TRAC code and its conservation of mass problems, the original version of the code that was released some year and a half ago did have mass conservation problems for long-term transients. It was designed to look at large break locas, not long-term transients.

As Jim says about SIMMER, this conservation problem can be controlled by small time steps. Unfortunately, in using those small time steps in analysis of long-term transients, the running times were excessive.

The version of TRAC called TRAC PD-II, which, coincidentally, is being released within the U.S. today, has solved those conservation problems, still allowing long time steps, by using some improved numerical techniques.

The point is that the first code would do a good job at mass conservation but only with small time steps for long-term transients. The new version of the code will allow you to take long time steps and has much shorter running times. In fact, we are now using it to run loss of feedwater transients much faster than real time in the accident -- two hours -- in the accident. So it is a much improved version.

DR. PLESSET: I think this is an elementary requirement that I am not going to give you a lot of credit for.

(Laughter.)

DR. PLESSET: I am a little upset that a code was out with that feature because you can't tell who is going to use it for what.

DR. STEVENSON: Yes, Dr. Plesset, but that is something you always pay the price of with numerical methods. A good numerical analysis must always take into account that there is numerical error. The analyst must be careful in his running of the code. Certainly any code can be used to give bad answers. It is the responsibility of the analyst to make sure it is not being used in that way.

When you put out a code, you can't guarantee that a user is not going to use it in a wrong way. There's no way.

DR. PLESSET: That bothers me a little bit, too, because a code is not just to be used by the person who developed it.

DR. STEVENSON: That's correct.

DR. PLESSET: It still leaves open, even if you pass Physics I, whether this code or the SIMMER code gives results that relate to the physical world.

DR. STEVENSON: The only way to tell is to do analyses of real experiments.

DR. PLESSET: Right.

DR. STEVENSON: I think in both cases there is a tremendous effort to try to see to it that the code will give good answers in comparison with real experiments.

DR. CARBON: Along that line, I would like to ask a question aimed at asking how much confidence you have in the results that you get when you run the SIMMER calculation. I'm trying to add this in a broadened sense. I appreciate that you can run SIMMER with various aims in mind, maybe t understand something, maybe to compare something with another calculation, maybe to come up with a finite answer to a particular thing.

I am sure you have given a lot of thought to the confidence you can put in the different uses. Can you state those fairly broadly here?

DR. SCOTT: Yes, I think I can. I would say that with regard to accident environments that are fairly energetic, where in fact there are large pressure gradients, that we have a fair amount of confidence in the SIMMER CODE. By fair amount, I believe that although the may be residual uncertainties in the result, they are of the order of perhaps 10 or 15 percent.

In any application where flow regime is likely to make a difference, then I guess I have less confidence. I will tell you why a little this afternoon. When it comes to analyses, investigating and influencing flow regime, what we currently are most busy with is implementing some -- we have already implemented the bubbly flow regime, and we are currently working on the film flow.

DR. SIEGEL: Excuse me. I didn't hear the conditions under which your calculations are poor.

DR. SCOTT: Well, I don't know if they are poor. I just don't know, so I don't have much confidence, I guess. In situations where flow regimes are likely to be important, we do see some substantial differences between, say, what SIMMER would predict and, say, what SAS-3-D would predict. Now, that doesn't necessarily mean anything, because SAS-3-D, if anything, is less verified than SIMMER is.

But it does give us an intermediary standard. It has been sanctified, if not verified, by use over a number of years. I guess what I am saying is that in the late initiating phase I would have difficulty quantifying uncertainties that we are getting out of SIMMER.

As far as disassembly, the post-disassembly expansion and the pure fluid dynamics in SIMMER where there is vigorous motion, large pressure gradients, I feel very comfortable with it.

DR. CARBON: What does that mean? If you carry out a calculation, are you 75 percent confident that it is close?

DR. SCOTT: Oh, I would say more confident than that. For the post-disassembly expansion sort of calculation, I am 85 percent confident, if you want a number. Within 10 or 15 percent, I think SIMMER does a pretty good job. I think we have done analysis and enough sensitivity study and enough comparison to real experiments to make a fairly good case that it is doing a good job.

DR. CARBON: There is a last point I would like to ask. You feel that you have in part of that, at least, compared it with real experiments.

DR. SCOTT: Yes, we have. We have spent three years comparing it with real experiments. Now, there is one deficiency in the experiments.

DR. CARBON: Are these real experiments ones that are representative of those that would take place in a fast reactor?

DR. SCOTT: Sort of. To the extent that it is possible. You have to realize that the transition phase, especially, is an incredibly difficult experimental regime for the experimentalist, but these are the most prototypic experiments which we can identify.

DR. CARBON: Sure. That is the point I am trying to get at, that you don't have a prototype.

DR. SCOTT: No, we don't. We are always extrapolating, unfortunately, to the real system from whatever experiment.

DR. CARBON: And that, then, is what I am trying to get at in terms of your confidence and your --

DR. SCOTT: It may be that my confidence is so high just because I have lived with SIMMER and done these calculations for so long, but I will show you some of the results this afternoon when we talk about fluid dynamics verification. There is a very large hole, in my estimation, in what we are doing simply because there is no experimental data or very little experimental data when it comes to energy transfer experiments, heat transfer from one material to another, and then to the surrounding structures. That is an incredibly difficult experimental regime, and I am glad I don't have to make my living experimenting in that. But it does impede verification somewhat.

DR. CARBON: Time is flying. Unless --

DR. MARK: Two simple questions. We were told that you could fix up the energy by proper attention to the convergence techniques and time step. Do you, simultaneously with the same attention, then get momentum and mass conservation for free, if you like, for the same adjustment of convergence criteria and time step?

DR. SCOTT: The broad answer to that is yes. You might not get the same degree, but you do get --

DR. MARK: You have a degree, and you can pick and choose -- I wish to conserve mass to one percent and take the associated departures and --

DR. SCOTT: Yes, which may be better or worse than one percent.

DR. MARK: But I say you can set this and the others then have values which you at least know.

DR. SCOTT: Right, and we continually monitor this in SIMMER. There is a printout at the end of each so-called long print, I believe, that tells you how well you are doing with conservation, mass and energy.

DR. MARK: Okay. Another question. You said that in the long-range development, you are exploring advanced fluid dynamic models, improved ones, and I am sure that two-phase treatments are a part of that.

DR. SCOTT: Yes.

DR. MARK: What did you refer to when you said you were also improving the neutronics? What needs improvement?

DR. SCOTT: What needs improvement in the neutronics is primarily efficiency, I believe.

DR. MARK: Okay. It is not that you get the wrong answers.

DR. SCOTT: No, I don't think we get the wrong answers. I think Ron Smith will show you the quality of the neutronic answers we get. On the other hand, once again I have to point out that neutronics verification is also not very simple, because it is difficult to get a --

DR. MARK: No. And here it is improvements in techniques of handling the problem in two dimensions with --

DR. SCOTT: With transport theory, yes.

DR. MARK: Rather than the two phase problem where you really have to go after the proper physics.

DR. SCOTT: That's correct.

DR. MARK: Thank you.

DR. CARBON: Jim, later would you go back and repeat your introduction for the recorder?

DR. SCOTT: Sure.

DR. CARBON: I think it would be worthwhile putting it in the transcript, the first 15 minutes or whatever.

DR. SCOTT: Okay.

DR. CARBON: Let's proceed.

DR. SCOTT: All right. Do you have a question?

DR. CAMP: We have a question. Bill Camp from Sandia.

Jim, with respect to this question of numerical diffusion, I would like to relate some experience we have had at Sandia and ask you if you agree with it. We have used several hydropoes (?) recently for experiment analysis. They have all had the property they are all eulerian hydropoes, so they have diffusion problems.

One of my staff members happens to be a good physicist so he wouldn't take the solution that, well, if I do it with end cells, that increases the two end cells and the answer isn't much different. In fact, he went back and did a lot of studies of it and found that it doesn't define a good coshi (phonetic) sequence, as mathematicians say. In fact, you have to go to a very huge number of cells with the problems with normal eulerian codes in order to get convergence to the correct answer.

If you went back to the literature and studied what has been done successfully around us, NRL particularly has done a lot of good work with flux corrector techniques, which we are trying to implement. Sam Thompson at Sandia has done some work with just building in arbitrary diffusion barriers into his equations.

So our conclusion that we are coming to is if you have problems, particularly with energy, that the only reasonable solution, if energy diffusion is a problem for you, seems to be to at least build in these flux corrector techniques rather than to say if I did it with 100 cells and I went to 200 cells and I got the same answer, I am not going to worry.

DR. SCOTT: I guess I don't totally agree. We don't have to go to a huge number of cells, typically. You said huge. I don't know what that means.

DR. CAMP: We found numbers like at 10,000 cells you were starting to preserve the shapes of pulses and things like that.

DR. SCOTT: Heavens, no. We get by with far fewer with that and get pretty good agreement, decent agreement with experiment. But I do agree that the best way to do it is to have some sort of numerical technique which reduces diffusion. Bob Steinke, who up until recently was in our group, was working on a three-dimensional formulation of the equations with reduced diffusion characteristics. I will have to ask Ron whether that was a flux in shape or what, what he was using to reduce diffusion. But it apparently worked quite well, at least in three-dimensional formulation.

DR. KERR: Isn't it characteristic of the corrector mechanisms that they work very well if you know what the answer is ahead of time?

DR. SCOTT: Yes.

DR. CATTON: On your conservation of energy to within one percent, that is one percent per what? Is it an interation, ten iterations, one hundred iterations, or total problem?

DR. SCOTT: What is it, Ron? Total problem, I think.

MR. SMITH: Total problem.

DR. CATTON: The total?

DR. SCOTT: What it amounts to is one percent of the total problem.

DR. SMITH: My name is Ron Smith. I am the alternate leader of Group Q-7, Los Alamos Scientific Laboratory. The work that I will summarize is mostly work done by people other than myself. These people have invested much time and effort in these analyses and I would like to acknowledge their efforts.

Bill Bohl, who will speak later about steam explosion analysis with SIMMER-II, was the first to demonstrate that transition phase analyses are feasible. His work on the Clinch River Breeder Reactor, transition phase, was augmented by later calculations done by Charles Bell, presented to the ACRS, I believe, last October in Albuquerque.

DR. KERR: Excuse me. Mr. Smith, since in some recent discussions I have discovered that I am not always certain what the speaker means by transition phase, could you give me briefly what it is you mean by the transition phase?

DR. SMITH: If I could acknowledge the work with these people first, then I will as part of my presentation do that.

DR. KERR: Okay.

DR. SMITH: The 1000-megawatt study ongoing at Los Alamos and detailed later in this presentation is being carried out by Larry Luck, Hunter DeVault, Marge Asprey and Plat Blewett. The computer codes used in these studies require considerable support to keep them operational and to improve their performance in models.

Fred Parker, Victor Martinez, Pat Hodson, Mac Forehand and Bob Steinke have helped maintain these code capabilities. The SIMMER verification program, which Jim Scott will speak about later, and the laffin (phonetic) fuel pin model development, which will not be discussed today, was successful through the efforts of Tom Weaner, Jim Tompkins, Michelle Schirru and J. Chapyak.

I hope that in my presentation today I can indicate to you the fine job these people have been doing and that the credit for that fine job goes to the people doing the work.

This morning I wish to cover four items: the status of transition phase analysis understanding at Los Alamos, verification of the SIMMER-II neutronics models for transition phase analysis, an example of transition phase calculation from the 1000-megawatt studies, and future efforts we expect to undertake in transition phase work.

I plan to speak for about an hour and a half, until about 10:30, but feel that the first and last subjects, transition phase status and future work, are the most important. Therefore, please inform me when I have about 20 minutes left to speak if I have not gotten to the future effort.

I will cover in the transition phase analysis status, first our approach to the transition phase analysis, and secondly, the results we have obtained from our mechanistic accident progression studies.

The first thing I will cover is in answer to Dr. Kerr's question. The transition phase came about from the inability of calculational tools to follow mechanistically the incoherent meltdown of an LMFBR core following loss of flow accident. The initiating phase, the phase when the sub-assembly phenomena lends itself to 1-D treatment, does not lead to large energetics and a subsequent quick transition to multi-dimension fluid dynamics.

Instead, it is necessary to model both intact sub-assemblies and disrupted sub-assemblies in a transition to treatment of the entire core as a fluid. A major concern during this incoherent meltdown is a potential for the mobile moltent fuel to move rapidly into a prompt critical configuration and produce fuel vapor pressure which can threaten the containment and possibly release radioactive material into the public environment.

Does that answer your question?

DR. CARBON: Yes, sir.

DR. CATTON: Isn't it also inherent in the method that when you say transition phase, you are talking about a particular fluid configuration, namely, droplets and vapor?

DR. SMITH: You are talking about the methods. I am talking about what happens in the reactor.

DR. CATTON: Fine.

VOICE: But geometrically is he right?

DR. SMITH: I don't know if we have ever seen one, so we don't know for sure.

DR. CATTON: You must have some idea what goes on in your model, though.

DR. SMITH: In our model model, it does model droplets.

DR. KERR: You guys are confusing me. I had thought that what he said was the transition phase is what you have between the time when the assemblies are intact and the time when you have a fluid, and whatever happens in between is a transition phase. It is how you treat it.

DR. CATTON: That is what Dr. Catton is addressing, how you treat it.

DR. KERR: But my question was what is it, not how do you treat it.

DR. CATTON: And I think I stated what it is. It is droplets and vapor.

DR. KERR: Well, it is whatever you have in between those two situations: droplets, vapor, pieces of stub, whatever. I think you might even have some solids.

DR. CATTON: That's true.

DR. SMITH: In my mind -- not in my model, but in my mind, which may be represented in the model -- it is a very complicated situation. It may be a combination of film flow, bubbly flow, droplets, solid particles, chunks still hanging around, chunks falling through, partially molten can walls, some still intact. The flow of material between can walls, between cans is very complicated, and the biggest problem is how do you model it and try to understand what is going on.

DR. CATTON: But in SIMMER you have a particular method of doing this.

DR. SMITH: Yes, we do.

DR. CATTON: And I think what would be good is if you could clearly state what that method is.

DR. SMITH: I was going to address sort of the philosophical approach that we are taking to incorporate those methods but not address the specific methods in detail. The details of SIMMER we have presented in the past ACRS meetings. I could go into them if necessary.

DR. CATTON: I guess if everybody here knows what the approach is, it is okay. I was just asking for clarification. I think I understand.

DR. CARBON: I think you better stick fairly closely with the presentation plan, simply because we are down to where your period is about an hour left. I think you have more planned.

DR. SIEGEL: Before you leave that one, one question about your last line, energetics caused by recriticalities. Is there some implicit assumption there that that is the only origin of large energetics?

DR. SMITH: That, in our mind, is the major one but it is not the only one. There is also the question of energetics from fuel coolant interactions and whether or not you could get sodium into this molten core and have a sudden vapor generation that could produce pressures large enough to threaten a containment.

DR. SIEGEL: You treat that question.

DR. SMITH: Yes, we do.

Our approach to analyzing the transition phase problem and other accident phases, by the way, is to attempt to predict mechanistically the course of the accident. As I have indicated earlier, this is a tough problem because the accident involved a lot of different phenomena, many controlled by microscopic detail.

What we do instead of attacking the microscopic detail is attempt to surround the problem by including the major controlling phenonema, as we see it, in an integrated analysis tool. The major pieces are certainly subject to dispute, but we have to start somewhere.

Then we perform best estimate calculations, trying to base assumptions on physical intuition and experimental results. Where we can, we evaluate the influence of modeling uncertainties on the predicted outcome of the accident.

Finally, if the uncertainties in the accident outcome are too large, the controlling models are refined through further analysis and experimental verification.

The major pieces in SIMMER-II are included in the handout and I will not attempt to go through those. They may or may not answer Dr. Catton's question.

DR. CATTON: Is the liquid field still a droplet field?

DR. SMITH: The liquid field is a droplet field except in cases of large liquid volume fractions, and then we switch over to a bubbly field. We are in addition working on putting in a film flow model.

DR. CATTON: And you have a criterion for doing that.

DR. SMITH: No, we do not. As I indicated earlier, the transition phase is a very complicated situation.

DR. CATTON: I don't disagree with that.

DR. SMITH: The problem is if we were in a single component situation like you have in a light water reactor when you are dealing with water and steam, we might be able to come up with a fairly decent flow regime treatment, although I am sure there would be quite a bit of discussion as to whether it was right or not.

In the transition phase problem where you have many components moving around, defining those flow regimes is very difficult and I think you have to take it one step at a time to get it there, and even at that, it is going to be a long problem, a long time before you could do it.

DR. CATTON: Does the heat transfer include radiation yet?

DR. SMITH: No.

The mechanistic accident progression studies that we have done to date have involved loss of flow accidents in the Clinch River Breeder Reactor design and a 1000-megawatt design similar to the conceptual design study reactor of the Department of Energy.

The CRBR study analyzed the original homogeneous core in which the driver sub-assembly region is surrounded radially by blanket sub-assemblies. In the 1000-megawatt study, blanket sub-assemblies are placed between driver regions as well as radially around the core.

Both cores have low sodium void reactivity coefficients.

DR. PLESSET: Let me ask a philosophical question because you mentioned philosophy. You just said that you studied a homogeneous core, you studied a heterogeneous core. Do you think you can tell the difference between them in this kind of a serious accident by your procedures and rely on the answer? Do you get my question?

DR. SMITH: I do not understand your question.

DR. PLESSET: Well, you talk about the mechanistic accident progression study, CRBR, with a homogeneous core, and then you talk about a 1000-megawatt electric study with a heterogeneous core. Can you tell the difference in the results whether you have homogeneous core or heterogeneous core, in a meaningful way? Regardless of size. They have the same size.

DR. SMITH: Oh, yes, definitely. I am going to have to rephrase your question in my own words just to make sure I understand you. You are asking, if I had a 1000-megawatt reactor and I had a homogeneous core for one and a heterogeneous core for the other, could I tell a difference in the accident scenario? Is that right.

DR. PLESSET: I'm sure you can, but do you feel it is meaningful, the differences you find?

DR. SMITH: I think yes. In the homogeneous core, you would get into prompt criticality before you would get to fuel motion, in the homogeneous core. In the heterogeneous core -- and this is what I am going to get into later on -- you will get into this transition phase. In the homegeneous core, you do not get into the transition phase.

DR. SHEWMON: Does heterogeneous mean to you alternately mixing blanket and fuel in the fuel or varying the enrichment, or what does heterogeneous mean?

DR. SMITH: Alternately mixing blanket and fuel.

DR. SHEWMON: Thank you.

DR. SIEGEL: In that particular discussion, your statement that both cores have relatively low sodium void worths wouldn't apply. They would have different sodium void worths.

DR. SMITH: For the CRBR design, which is a relatively --

DR. SIEGEL: Well, the 2000-megawatt core is heterogeneous.

DR. SMITH: That's true.

DR. CARBON: Let me follow up Dr. Plesset's question just briefly. You gave an indication that in these two cases, the mechanistic progression differed, but when you come to a final end result that you are working on, you want some answer at the end, perhaps the energy release or something.

Will you have confidence in the difference in the two end results that you come out with? Will you be able to see --

DR. SMITH: Qualitatively, yes. I'm not sure exactly what you are searching for.

DR. CARBON: Well, presumably the purpose in running --

DR. KERR: May I try to rephrase it? I think he is saying suppose you find out the results are significantly different? Do you believe it?

DR. CATTON: Yes. Between the homogeneous and the heterogeneous, calculations with the --

DR. SMITH: I guess I will have to answer yes again. Qualitatively, yes, I would. I think there are a lot of uncertainties in analyzing both cores. I think right now that those uncertainties are fairly large and the outcome, the uncertainty in the outcome is also somewhat large. I think that reduction of the modeling uncertainties is necessary to reduce the uncertainty in the outcome.

DR. CARBON: I guess I don't understand what you said. I thought you --

DR. PLESSET: May I just make a short comment? The kind of thrust of my question was this: that you are presumably following a rather microscopic description of an accident, and my wonder is if you will ever live to get anywhere with this; that you should perhaps take a more global approach where you don't have to answer all these questions, which are legitimate, about droplet field, bubbly field, various components and so on.

I think that some kind of averaging or integral methods or something I don't know about -- you should -- might get you somewhere in a finite time.

DR. SMITH: I disagree.

DR. PLESSET: Okay. That is what I wondered.

DR. CARBON: I want to go back to the question as Dr. Kerr expressed it. Carrying through these two calculations, is it meaningful to you when you come out with the end result? Your answer seemed to be yes, it was, and then you said in a qualitative way, and then you went ahead and put some conditions on this, and I ended up concluding that you really felt it was sort of meaningless. Is that true or not?

DR. SMITH: No. I think the problem is what we are trying to do is to analyze the potential for threat to containment. The uncertainties in analyses, in particular the transition phase analyses, indicate that we are not yet to a point whether we can assess a probability of getting into a sufficiently high reactivity ramp rate and energetics to determine whether or not we threaten that containment.

I think that we need more analysis to, in the first place, understand what our level of uncertainty is, because the analyses we have done so far are fairly preliminary, and to reduce that uncertainty in the outcome because we do not know whether or not the recriticalities we see in transition phase analysis are sufficiently small not to threaten it.

DR. CARBON: The answer I am interpreting in what you are saying here is that you run these two calculations, and really you can't place much confidence on the difference in the results of the two; that you would not recommend one core over the other.

DR. SMITH: This we do know. If you go ahead with a homogeneous core as with current oxide designs, that you will have a large positive sodium void coefficient.

DR. CARBON: You will have what?

DR. SMITH: A large positive sodium void coefficient. And when you get into sodium voiding during the initiating phase, you will go immediately into it, depending upon the size of its coefficient, into a reactivity excursion, power excursion, which will most likely throw molten fuel into sodium right in the middle of the core, and there you will be subject to some of these other questions about fuel interactions and what happens there and how much energy gets into sodium, and what happens with that being the working fluid instead of fuel being the working fluid. And again, do you penetrate or threaten the containment.

DR. SIEGEL: It is that question and that uncertainty that we were facing ten years ago. What has come out of SIMMER that helps the designer make a decision?

DR. SMITH: That is what we had hoped would happen.

DR. SIEGEL: Let's assume for a moment there is a penalty in doubling time to be paid from one or the other of the two approaches, homogeneous versus heterogeneous. Can you tell the designer now that he doesn't have to face that penalty or he has to face it because of differences in the safety behavior of the cores?

DR. SMITH: In a way your question is coming too soon. We do not understand the transition phase that well, for one thing.

DR. SIEGEL: So you are saying not now, but perhaps later.

DR. SMITH: Perhaps later, yes.

DR. SIEGEL: On the other hand, we have begun to look at some possible design changes with regard to getting fuel away from the core during the transition phase, but again, that is preliminary also, and certainly some interaction would have to be done with the vendors.

DR. CARBON: Dr. Kelber.

DR. KELBER: From the point of view that I bring to this problem, the principal contribution that has come from the studies to date has been to reduce the potential for damage to the containment a great deal, to reduce the potential for damage to the primary system to some extent. The result is --

DR. CARBON: Excuse me. How do you reduce the potential by these studies? I don't understand.

DR. KELBER: In all the calculations we have done, we estimate the potential for damage to the containment.

DR. CARBON: Did you say reduce the potential?

DR. KELBER: I am sorry; reduce the estimates of the potential for damage to the containment or damage to the primary system. Our estimates now show much lower potential for damage to the containment, and quite possibly much lower estimate of potential for damage to the primary system. You are now talking about differences of degree between heterogeneous and homogeneous cores where everyone involved, both the designers and the safety organizations, have to do a considerable amount of study of the tradeoffs.

If, for example, a homegeneous core should still have a very low estimated potential for damage to the primary system even though it is higher than the estimated potential for damage from a heterogeneous core, one must ask what is the nature of the tradeoff obtained if the heterogeneous core from a neutron economy point of view is less beneficial.

We now get into the question of risk analysis, and that is further down the road. I think this is the proper end point of the use of such tools as this.

DR. KERR: Excuse me, Charlie. I think what we were asking was, if one found that the heterogeneous core is better, would one believe it, not what would one do. Would it permit one with confidence to assume that such an indicated result was something that a designer would take and say, now I have got to look at other facets of this, but I believe that from this viewpoint, a heterogeneous core is better.

DR. KELBER: From an NRC point of view, I don't see how we could make such a conclusion, regardless of the estimated precision of the SIMMER results, without a significantly larger experimental test program.

With respect to Dr. Plesset's question earlier as to could we take a more integral view, I think that is a desirable goal further down the road. In many cases you have to take a very detailed view of things until you understand what are the gross properties that dominate the problem and how do you represent them in an integral way.

I think that is a reasonable goal somewhat down the road. As we complete our studies on the transition phase over the next few years, we should, in fact, tend in that direction.

DR. CARBON: In the interest of time, let me stop you there but go back and ask an additional question on the broader aspect.

You have said, if I understand you correctly, that the major contribution of SIMMER to date is to reduce the estimated energy release and so on, the damage to the containment. You have reduced that an appreciable amount. How much confidence do you have in that?

DR. KELBER: In that I have a very high level of confidence because it does follow from the systematic application of the laws of physics to the problem, so that it is a synergistic effect of a highly interactive process.

DR. CARBON: Do you have enough confidence that you would recommend that NRC approve a design with half the energy release if SIMMER came out showing half the energy release?

DR. KELBER: The question is what is the factor of conservatism that we have to put on in relationship to the fact that we have very little direct experimental confirmation. I would argue for a considerably lower factor of safety in the future than was used with respect to CRBR, where the factor of safety was something of the order of 100 or so. I would argue for lowering that considerably, perhaps by as much as an order of magnitude, but not more than that, at the present time.

DR. CARBON: I don't know your factor of safety of 100. Can you equate that to the 1200 megajoules?

DR. KELBER: Yes, precisely. I think the 1200 megajoules for CRBR was a figure -- by the way, as far as I can tell, arbitrarily derived -- but it was a figure which was guaranteed by the choice to be large by perhaps as much as a factor of 100 over the best test tools. I would argue at the present time that we could reduce that by perhaps as much as a factor of 10.

DR. CARBON: From 1200 of this, a factor of 10 down to 120, on the basis of SIMMER calculations.

DR. KELBER: That is correct, simply because of the transparent relationship of what is happening in SIMMER, where does the 1200 come from, how does it relate to the corresponding figure in SIMMER, which is of the order of 81 megajoules. I think on that basis we could make a strong argument. I would not go further than that at the present time.

DR. CARBON: If you did that -- you cited the use of SIMMER here -- how much of the same thing could you calculate with equal confidence on the so-called back-of-the envelope type of calculations?

DR. KELBER: Well the 1200 megajoules corresponded not so much to a "back of the envelope," but something not very much more complex than that. It was a systematic isentropic expansion of different volume elements of the fluid, which is a back-of-the-envelope calculation. The housing becomes more difficult and you have to go to a code.

DR. CARBON: What I am trying to say, though, is you have used SIMMER to come up with a reaction. You could replace SIMMER with a back-of-the-envelope --

DR. KELBER: No. I mean the 1200 megajoules is what you get with essentially back-of-the-envelope type --

DR. CARBON: But I can't believe, though, that you couldn't, with back-of-the-envelope type calculation, put in some of the in-transferred implications which which I believe are the major things in SIMMER that are reducing --

DR. KELBER: Well, there were two major sources of reduction. One was the heat transfer, yes. It was known from back-of-the-envelope type of calculations that this would be a major reduction. It was not known how much. The second, which I believe is very difficult to do by any approximate or simple-minded method, is the difference between the dynamic expansion and the isentropic expansion.

Another question is what are the thermodynamic losses involved in the actual expansion. That, I believe, is very difficult to do in any simplified method, and that, I believe, is something like a factor of 2 all by itself.

DR. CARBON: And the heat transfer is a factor of what?

DR. KELBER: I'm not sure what that --

DR. SCOTT: That reduces it from about 20 megajoules to 8 megajoules --

DR. KELBER: So it is a factor of --

DR. SCOTT: Of 2.7 or --

DR. KELBER: Something of that order.

DR. SCOTT: I am talking about this in some detail this afternoon in connection with verification.

DR. CARBON: You will? Find.

Go ahead, Dr. Smith.

DR. SMITH: I will get back to the transition phase. What you have been talking about has been closer to assembly expansion. The treatment of that and the treatment of transition phase are somewhat different.

Of the two cores I was talking about -- that is the CRBR homogeneous core and the 1000-megawatt heterogeneous core, the low sodium void coefficient of these cores means that significant positive reactivity feedbacks are not induced in the core during sodium voiding, cladding melting and motion.

This extends the time frame of the accident, and by the time fuel motion begins, substantial cladding blockages have formed at both top and bottom of the subassemblies. Incoherencies in fuel motion among the subassemblies due to burnup, power and power flow yield initial fuel motion activity ramps that are relatively small.

They produce three things. In unblocked subassemblies, the moving fuel, because it does not have a lot of pressure behind it, blocks at the ends of the subassemblies. In blocked subassemblies, the moving fuel is retained near the core, and the power increase coming about from these fuel motion and reactivity increases, although the power increase is small, causes more fuel to become molten.

Thus, more fuel becomes mobile, and as can walls begin to fail and motions become more coherent, the reactivity ramp rate becomes larger. These coherent motions may develop anyhow due to the driver reactivity and small power bursts. That is, even in subassemblies where the cans have not failed, the existence or the occurrence of reactivity ramps causes fuel to disperse, but if it can't get out, it is going to come back and it is going to start to become more coherent.

The limit to the energetics appears to be controlled by the strength of the blockages retaining the fuel. That is, in essence, the types of accident progression we are seeing in the transition phase for both the CRBR homogeneous core and the 1000-megawatt heterogeneous core.

What do we learn in these analyses? First, the extent and strength of blockage development appears to be determining in the potential for recriticalities and probably the magnitude of recriticalities. Second, the tight coupling between the fluid motions and the near-critical neutronics does not permit the development of a steady state boiled-up pool, at least in the early stages.

For the system to become far subcritical, substantial fuel must be removed from the core or diluted with poison.

DR. SHEWMON: Could you tell me what a blockage is, physically?

DR. SMITH: The blockages that I am referring to mainly are those which exist when the molten cladding or molten fuel in the core region is expelled upward or downward into the axial blankets and freezes and plugs. Those are the blockages I am talking about.

DR. SHEWMON: Okay, thank you.

DR. KERR: Is this ever rebound, or does it stop up there and blanket and still give you trouble?

DR. SMITH: It gives us trouble in that it retains fuel in the core, although I think in some instances I have seen some rebound in some of the calculations we have been through.

These two points basically summarize the accident progression, but they are still important. Incoherencies appear to help early on, but the involvement of more and more fuel movement leads to larger energetics.

DR. SIEGEL: Larger than what?

DR. SMITH: Larger than earlier.

DR. SIEGEL: These cores are essentially dried of sodium in this phase?

DR. SMITH: In the CRBR calculation I believe it was mostly dry, yes. In the heterogeneous core, the 1000 megawatt, we still have a fair amount of sodium in some of the inner channels, some of the inner subassemblies. I don't believe I am going to have time to get into a detailed description.

DR. CATTON: What is coherent multidimensional motion? Or is that a silly question?

DR. SMITH: No. In general, when we first go into SIMMER, when we make the transition from the SAS calculation to the SIMMER calculation, we are still in the one-dimensional, multichanneled mode of doing the analysis. SIMMER has models of can walls and fluid motions are basicallyk one dimensional.

We we get to can wall melting, that allows the fuel to start moving radially as well as axially. We see, in particular in the CRBR calculation, in the can well melt you don't have this can separating the motions of the fuel, and the motions are somewhat independent in the 1-D calculation. Once you get into two-dimensional, then the fuel kind of conglomerates and moves together. It has a chance to mix together.

DR. KERR: That seems perfectly clear to me, Mr. Catton.

MR. CATTON: I don't think I will ask another question about that.

(Laughter.)

DR. SMITH: The key to the whole problem in the transition phase appears to be to get the fuel out before you get to high energetics. The analyses to date have not yielded reactivity ramp rates much greater than $100 per second. Again, I would like to point out that the analyses to date have also been fairly preliminary, and we are still assessing the uncertainties.

DR. KERR: Mr. Smith, would you be willing to put that last slide on?

DR. SMITH: The previous one?

DR. KERR: Yes, sir. Would you have been quite surprised had you not learned those two things from SIMMER? Intuitively, it seems to me, one would have expected this, so in that sense SIMMER is corroborative. Or am I missing something?

DR. SMITH: No. I guess I am not surprised in retrospect. I guess the one thing that surprises me are the repeated recriticalities, that there is not one big bang but a bunch of --

DR. KERR: I don't see anything about repeated criticalities on the slide. I was asking about the things on the slide.

DR. SMITH: Okay. Well, maybe I didn't put enough on the slide. What we see are going into prompt criticality with low ramps early on, from small amounts of fuel motion. That small power burst leads to more fuel melting and more fuel becoming involved in the subsequent fuel motions. And because now we have more fuel moving, reactivity ramp rates become larger next time, the next time you go through prompt criticality.

We see -- and I'm sure I'm not going to have time to show it, but we see, like in the beginning life calculation for the 1000-megawatt core, we see a ramp rate going into prompt critical of about $10 per second, followed by dispersal, the reactivity falling far subcritical, and then the fuel motion coming back together again, or coming into more critical configuration again, followed by a reactivity ramp of about $30 a second, and again dispersal followed by one that is $100 a second.

DR. STEVENSON: Dr. Kerr, if I may, the thing that we learned that we did not necessarily expect before we began the calculations was that there were relatively large reactivity ramp rates at all. We didn't know without running the calculations but what in the transition phase there might be no recriticalities.

What we found were recriticalities, and I think that is what that says.

DR. SIEGEL: Could you talk about the geometry of these recriticalities a bit? Are they slumped cores? Are the regions radial regions which become fueled enough to become critical?

DR. SMITH: In the 1000-megawatt study, the separation of the driver regions by the blankets keeps the driver fuel basically in the region, and it is basically fuel slumping, at least in the studies we have done so far. In the long term it may melt into the blankets. It probably will.

In the CRBR calculation this was not the case. You got more into what I referred to earlier as coherent multidimensional fluid dynamics, fluid motions; and in addition to slumping, you saw a small burst causing fuel to move out radially and outward away from the core center, impacting on the surrounding structure, coming back into a pool and coming together in the center of the reactor. So they are somewhat different.

We have talked quite a bit about uncertainties, and I indicated some of our modeling uncertainties on this slide. These are what we consider to be controlling. Mostly it has to do with blockage formation, fuel removal, and those things that drive us into prompt criticalities, such as fuel pin breakup and slumping, how that affects bringing fuel together.

The last two addressed the problem of essentially liquid-liquid heat transfer and also loss-of-flow driven transient overpower, because we do see situations where we still have sodium in some channels, in both the CRBR and in the 1000-megawatt.

That concludes my discussion on the transition phase status. Would you indicate how much time I have?

DR. CARBON: About a half-hour, total half-hour.

DR. KERR: May I ask a brief question? Do you and your colleagues who work on this very difficult problem, and I agree it is an extremely complicated and difficult problem, sit down maube about once every six months having made progress during that period and ask yourself if what you are trying to do is possible?

It may sound like a facetious question. I don't mean it to. In the sense that it is so large and it is so complex, and a particular sequence of events depends very much on what has happened before and the uncertainties in fabrication of new fuel, and the uncertainties of what happens to fuel after it has been in operation, and the uncertainties in motion, eventually it gets you in a situation in which it would seem to me if you started off with 100 reactors and tried to do an experiment beginning with exactly the same initial conditions, you can probably get 100 different results.

Now, if your code is clever enough to have taken into account everything that could possibly physically happen to those reactors during the time of operation, perhaps it can predict those 100 different results. But it seems to me it is worthwhile for you to continually ask yourself this question.

Mr. Kelber's response to Mr. Plesset was that once you learn all you can microscopically, then you go to a global picture. It seems to me it is possible that one might find that the microscopic approach is impossible, and that one has to, in some sense, attempt to lump some of the uncertainties in a global picture.

I don't propose to know the answer to the question I am asking, but do you ask yourself this question periodically?

DR. SMITH: You have raised several issues in asking the question. First of all -- and I don't like to contradict Dr. Kelber -- our approach is not quite trying to include all microphysics in the calculations. As I indicated earlier, we are trying to surround the problem, trying to include the major phenomena as we see it, trying to see how that controls the accident outcome.

Where we find that the uncertainty in particular phenomena does control the accident outcome, we try to reduce that uncertainty. It may turn out that even by reducing it, we are still in trouble. We don't know. At present it appears that the major thing that is getting us into recriticality is the blockage question.

In answer to your question regarding feasibility, yes, I believe it is feasible to do this, but it takes a lot more work than what we have done. If you ask the question with respect to is it feasible for so many dollars, I don't know. If you were funded for ten years for $10 million a year, would we be able to solve the problem? That is a difficult question to answer. I do feel it is feasible to come up.

The other thing is you are addressing predicting exactly -- at least this is the feeling I got -- the accident sequence: if you had 100 reactors, would you be able to predict exactly the accident sequence. I don't think that is our goal. I think our goal is to gain some understanding of the transition phase and to understand what happens, and is there potential for getting into a situation where you have very large energetics and you do threaten containment, and is the probability of that happening very large.

If we can somehow assess that probability, I think we will be there. I think right now we don't know.

DR. KERR: I think we both agree that one does not predict exactly, and indeed, one may predict with considerable inprecision. But if the prediction is to be useful, one needs to have some idea of what the inprecision is.

DR. SMITH: That's right.

DR. KERR: In that sense, I would say if the uncertainty cannot be quantified, then one has an uneasy feeling about the results, it would seem to me.

DR. SMITH: Yes.

DR. STEVENSON: Dr. Kerr, that is why we do fairly extensive sensitivity studies on both the analysis of experiments and the accident analysis, to try to get an idea of what the uncertainties mean in terms of the spread of the answers, whatever the answers might be. You are asking the question, or you have asked it, earlier, of what confidence do we have in the answers. It depends on what the uncertainty band is in these sensitivity studies. We have simply not done the sensitivity studies for the transition phase.

They were done for host disassembly expansion problems. We had an idea there of what the sensitivities were and what the uncertainty band was. But in answer to your question are we introspective and do we worry about how we are doing things, is this the best way, yes, we do, a great deal.

DR. PLESSET: I think sensitivity studies is a very good word, but I picture this thing, if I can use a geometrical analogy, as a surface of 10,000 dimensions, roughly, whatever. You are studying how things change if you wander around on one of these surfaces a little bit. I say there are surfaces all over the place, and you are not any way near them.

DR. STEVENSON: You are perhaps right in that we are always subject to the question of have we missed something, and it is always a valid question to ask. Of course, we may always have missed something. But in terms of doing sensitivity studies, the techniques that have been presented to most of you before by Dr. Bob Burns take advantage of using statistical methods to take into account or allow you to look at the whole surface. They are not linear sensitivity studies but allow you to look at very nonlinear systems.

DR. PLESSET: Is this relating to that Latin cube I heard about at Los Alamos but never could figure out --

DR. STEVENSON: That's right. We have methods that are more improved than that now. I am sure you would like to hear about those.

DR. KELBER: I would once again like to emphasize from my point of view that regardless of this method, which I think has got high value, if we are going to go into the licensing of fast breeder reactors, we need a significant experiment program which tells us that we do have a reasonable understanding and that we have not missed significant phenomena and significant interactions.

If that is the thrust behind your questions, I could not agree more.

DR. CARBON: Go ahead, Dr. Smith.

DR. SIEGEL: Could I ask a question? You place a great deal of importance on blockages as having a strong influence on the subsequent recriticality and energetics. Is a blockage by definition material which has fallen below the melting point? Is it thereafter a rigid, immobile barrier?

DR. SMITH: Part of the uncertainty is in to what extent are things blocked; how strong is that blockage and what level of pressure or whatever is it going to take to get rid of that. It may be very strong and it may not be. If it contains the core where originally it was located, then we are predicting that we get into recriticalities.

DR. SIEGEL: How does the code identify what is a blockage and what isn't?

DR. SMITH: The code will freeze up fuel, and also it has an automatic jamming model where if it gets too jammed up with solid particles, it will block off the channel. Part of what we have done in some of our analyses has been to artificially set up the code so that it will produce these blockages, because the blockages are seen in treetin (phonetic) experiments, and the code at present as currently constituted does not predict the formation of blockages. So we have turned the dial to get --

DR. KERR: Excuse me. Do you mean it is incapable of predicting them or just doesn't predict them?

DR. SMITH: It is capable of doing it but in a parametric fashion. What we would like would be to incorporate a more physical model into the code, and it is one of the things we will be working on this coming year, to put in the proper physics in order to do a blockage. As you probably heard, in the physics of blockage formation, there are all sorts of theories coming out of Argon, Brookhaven, wherever, on how freezing and melting of molten fuel and cladding occurs.

Most of the theories do not seem to be able to predict what happens in the experiments.

I think my time is fast drawing to a close. Let me indicate what we are doing in the 1000-megawatt study. First, we are trying to gain an understanding of HCDA phenomena in what we would consider commercial reactors. The direction seems to be the heterogeneous core.

Our primary emphasis is looking at the transition phase and recriticality potential. Also we are attempting to support the Sandia accident delineation study, which we will hear about this afternoon. Another part of our objective is to assess the accuracy of our analysis tools and the data base.

In the 1000-megawatt study, what we intend to do this summer is to finish preliminary calculations through transition phase and document those analyses, and then in the next year investigate the effects of the uncertainties I indicated earlier through sensitivity analysis.

I would like to address for about 15 minutes future SIMMER development. There are three areas: near-term SIMMER-II modifications; near-term modifications for LWR core disruption, and longer-term considerations.

The near-term SIMMER-II modifications are being done primarily to support the 1000-megawatt study and the uncertainty analysis. We will try to use a current framework and improve both the phenomenological models and improve the efficiency. The 1000-megawatt study really provides us with an excellent testbed for trying to improve our capabilities.

We found in the analyses that there are all sorts of strange situations that our models had not been developed for and that we are getting into, and we have to improve the models to make them run more efficiently and to handle the strange physical situations we get into.

Model improvements. We mentioned freezing and plugging. Vaporization and condensation plays a role in the transition phase calculations, particularly in terms of fuel pool interactions or any sodium that might be remaining in the core during the transition phase.

We have already talked about additional flow regimes. We are concerned about the relative movement between liquid steel and liquid fuel and the separation of those two components because of the potential for getting the fuel region bounded by a steel region and having a much more critical situation, prompt critical situation, than what we are predicting with SIMMER as it now stands.

In particular, it looks as though the transition phase may go on, may extend the accident out in time quite a bit, and that allows,you time to get this separation between fuel and steel. It is not clear that SIMMER-II is the tool in which to put another molded fuel treatment into, but we are examining the feasibility of doing this.

A fuel-pin model to handle the breakup of fuel pins and the loss of flow-driven transit overpower (phonetic) situations you might see during the transition phase is also being considered. The design of the SIMMER-II code may not allow that. There may be some things we can do there. I think we certainly could put a fuel-pin model in SIMMER-II as it exists. The question would be whether it would be efficient.

Efficiency improvements are primarily in a vaporization-condensation model, which we find takes up most of the computer time in our flow dynamics methods, and neutronics overall takes quite a bit of computer time and we would like to reduce that.

We are prepared to modify SIMMER-II to handle LWR core disruption in the Class 9 type of accidents that I believe you will be hearing the program plan for on next Wednesday. These represent some of the modifications that would be made to the code to handle LWR core disruption analyses.

The types of analyses that would be done with this code would be examination of blockage formation and melting, the effect of chemical reactions, looking at steam starvation and two-dimensional effects, multi-dimensional effects that occur during an LWR core disruption.

DR. CARBON: How extensive a problem is it to modify SIMMER to use for LWR?

DR. SMITH: I forget what we estimated. I believe it was either -- was it two man years? Two man years.

DR. SCOTT: For the entire list. You can further break that list down into things that have to be done immediately and things that can wait a little bit. For immediate modifications I believe we estimated something like nine man months, with the balance in about 24 man months.

DR. SMITH: In the long term, I think both for the LMFBR applications and the LWR applications, I think there are some major pieces missing. The three-dimensional incoherencies in the transition phase may play a very large role in extending the transition phase in terms of how much fuel becomes mobile, in terms of how quickly or how long, or to the extent that blockages develop and their strength and whether or not they melt out before you get the large fuel motions.

We feel we need a detailed field model, and there are some others that are listed there, and I am sure we could come up with a few more.

The SIMMER-II framework, as I alluded to earlier, is limited, and we probably need to develop capabilities beyond what we have right now. It turns out that much of the methods that we were talking about in terms of calculations, that we would probably end up extending the time frame and we would have to have much more efficient numerical methods than what we have presently.

I think most of what would be needed is beyond the current state of the art, and we need further research in developing numerical methods that allow you to do long-time skill problems, conserve energy, conserve mass, before we could ever attempt to start to integrate a lot of these bigger pieces that I have indicated at the top of the slide.

That concludes what I have to say.

DR. SHEWMON: I'm not very clear what a megajoule consists of, or how many of them 1200 of them is, but I have the impression it will cause a fair amount of anguish to fairly substantial hunks of stainless steel. I am confused as to how one gets enough energy to distort all that stainless steel out of something which will be held up by a little bit of mushy stainless steel and a dispersion of uranium oxide in your blockages.

Could you tell me why I am looking at the wrong part of the elephant here, or is that sort of the stutters that allow it to really go off in a big bang?

MR. SMITH: Yes, that's it right there. The initial ramp rates we are seeing, as I indicated earlier, were in the tens of dollars of range. That gives you fuel pressures at 10, 20 atmospheres. The blockages we are talking about probably would retain those. When you start getting up into 100, 150, 200 dollars a second, then you are starting to talk about fuel vapor pressures that will move that, if not move the blockages out of the way, then move the can itself out of the way.

DR. SHEWMON: Okay. It cycles a few times, and then you get enough fuel in one spot to do yourself some real damage.

DR. SMITH: That's right. And the real question is at what point do those blockages or whatever, what fuel removal paths, at what point do they open up so you can get the fuel away from there. If they open when you are in this $100 per second window, which I am not sure I would count on, then you might be all right. If they delay later, it takes more than that.

DR. SHEWMON: In this $10 per second window, what fraction of the core do you see taking part? Is this something I can hold in my hands or is it something as big as a bare fraction of the core, or what?

DR. SMITH: A few subassemblies in the initial part.

DR. SIEGEL: I am losing the thread of thought here. Your question was if you are down in the lower range of $10 a second ramp rates, what fraction of the core will be retained by the supposed blockages associated with that?

DR. SMITH: I think he was --

DR. SHEWMON: I was asking what was involved, but since it is contained, your is a perfect corollary. To me it would be the same question.

DR. SMITH: The initial fuel motions involve just a few subassemblies. Now, other subassemblies may be in some stage of disruption, cladding motions, sodium voiding; but for these low sodium void worth cores, the subassemblies that are involved in the initial fuel motion are just a few.

DR. SMITH: Let me bring up a different question that I am sure was covered someplace else and so I hesitated to ask, but tell me again what are the most common one or two events that give rise to these HCDA's or initiate them?

DR. SMITH: From the beginning? The HCDA that we are looking at or have been looking at is the loss of flow accident. There is a loss of power to the site with failures of the scrams.

DR. SHEWMON: So the sodium stops cooling, convection isn't enough to do it, and something bursts. Is that it?

DR. KERR: In addition, you have to have lost all the shutdown system.

DR. KELBER: Could I suggest deferring that till we hear the accident delineation talk this afternoon, because it does address this question?

VOICE: I'm game.

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DR. MARK: I thought the answer would be obvious. Would there be any difference in the description course of, and the results of the problem that you are dealing with pool type sodium reactor, as compared to the sorts of things you are looking at?

DR. SMITH: I think if the coast down to the pumps remain the same, then the answer to your question is no there would be no difference.

DR. STEVENS: Dr. Mark, I think the differences might well be in the response of the system to the energetics, rather than the energetics itself. The pool and the pipe system may respond somewhat differently, structurally.

DR. MARK: The sodium could keep flowing from all directions, not just up and down some alleged channel.

DR. STEVENS: The core is still contained in a core barrel in either case. I am not sure it really matters that much.

DR. MARK: Well, I wasn't sure that it mattered very much, either.

DR. KERR: I think you said the key parameter in determining the energy releases of the aid of the blockages. At the present time SIMMER does not handle the blockages very well from first principal. You are depending on intuition and some experiments to parametrically, I believe is the word you used.

DR. STEVENS: That's right.

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DR. KERR: Thank you.

DR. SIEGEL: With respect to your work on LWRs, what do you foresee you will change from SIMMER, which is a significant extension beyond what's available from other codes, perhaps March?

DR. SMITH: I think what we would be aiming for in the long run would be a mechanistic, again, description of the meltdown of an LWR core. I think initially analyses done with SIMMER-2 would be more to gaining an understanding of the phenomena involved in that mechanistic approach, and the interactions between them.

My understanding of the codes now is that you do not have this heat transfer of chemical kinetics, all of this interaction and the two-dimensionality that you see -- that we could model with SIMMER.

VOICE (9): Could I comment on that? The impetus is that we know from TMI-2 that you can pool a severely damaged core. The question is, are there limits to the extent to which you can cool a severely damaged core?

The assumption in March, which is the only code -- well, there is a German code family called KESS(?) which deals with some of these questions too -- in all of these codes the characteristic is the assumption that once you pass some point of damage, you proceed in exerbling (phonetic) the melt. This is a key question in accident limitation prevention. We seek a bf-3

better answer.

Whether we will get it from SIMMER or not is another question, but we do seek a better answer.

DR. CARBON: Let's take a fifteen minute break.

(Recess.)

DR. CARBON: Let's move on with the program. You're up next, Jim?

DR. SCOTT: Yes, I am. I am Jim Scott, same address as before.

DR. CARBON: Do you have some hand-outs?

DR. SCOTT: Yes, I do. I'm sorry. I'm going to spend about a half hour talking about SIMMER verification, or to answer the question: "Why do we believe any of this, or is there any reason to believe any of this at all?"

Before I get started, I would like to say that a very important part of SIMMER verification should have been presented this morning by Dr. Smith, because of time constratints, he could not do so. I would just like to point out that we have spent a lot of effort. I would like to take two minutes to say that we have gone to a lot of trouble to verify neutronics in the SIMMER code.

That verification effort is outlined in the hand-outs which you have been given. I will just pause to conclude that in looking at annular critical assemblies of mocking up distorted cores, the major thing that we found in examining reactivity bgm4

changes from nominal to distorted geometry is that it is probably necessary to use a transport theory treatment for these highly distorted geometries.

The diffusion theory has an unpleasant habit of underpredicting positive reactivity insertions and overpredicting negative reactivity insertions. We feel that it is reasonably important, therefore, to take the approach of using transport theory.

Should you care to hear any more about this, I'm sure Ron Smith at the end of Bill Bohl's presentation can take eight or ten minutes to summarize these calculations.

Okay. This morning, I would like to talk about why should we believe any of this and how do we go about verifying SIMMER.

Well, a long time ago, and most of you have heard this before. We adoped a four-fold approach. What we would like to do, of course, is compare results of SIMMER prediction to the results of an experiment and relatively large-scale, using real materials.

That's very expensive, and it's hard to pursuade people they should do that. So, we are stuck with dealing with other than the real world and are left to extrapolate to a certain extent.

Parts of the SIMMER verification program are basic physics studies, in which we tried to compare SIMMER against analytic solutions for a variety of things. I will put up a partial list in a minute -- to see how well it does and to see if it can, in fact, pass physics one; and at least come reasonably close to analytic solutions.

We have compared on a model by model basis the models in SIMMER-2 with other models in the literature, constantly updating that. That has lead to change of several models in the SIMMER code.

In addition to the extent that we are able to, we compared the results of SIMMER calculations with other codes. I say to the extent that we're able because there are a few other codes that will treat extended material motion the way that SIMMER will, so we have to compare in region overlap. Yes?

DR. CARBON: There must be several places where have to insert models for which none exist. Is that correct?

DR. SCOTT: That is correct. That is why we have the section U-7, called "Model Development." Finally, we attempt to verify SIMMER by comparing SIMMER to experiments and certain thermo-physical regimes that are associated with specific accident sequences.

As an example of the types of things we do, here is a partial list of calculations we've performed over the last two or so years, including comparison with shock tube problems, steady-state pressure drops, an array of things for which there are analytic solutions.

In every case, SIMMER does reasonably well, down to about one deep fluid hammer. The last three items on this list are basically experiments which have been performed. All except one have been performed; others at Los Alamos, SRM, Purdue University primarily in which we look at the ability of SIMMER to do fluid dynamics and interactive geometries.

I will go into that later. What we're talking about now, as Ron stated, we haven't yet gotten in to a verification of the transition phase calculations. We are still just looking at fundamental verifications of fluid dynamics in the national regime. It looks very much like both these assembly expansion (?)

Let me show you what the strategy here is. If you look at CRBR post-disassembly expansion calculations for SIMMER, you will see that there is a difference between the isotropic expansion cover gas volume, which uses about 105 megajoules and which nominally used, I believe, during the CRBR safety review and what SIMMER's best estimate was which was about 8 megajoules.

Unfortunately, it is difficult to ascertain that this is correct because we can experiment on CRBR or even systems remotely close to being as large and having proper materials. It is fortunate that the effect, the reduction from 105 to 8 megajoules came about for two reasons.

One was purely fluid dynamic. Just the presence of structures, themselves, presence of altered flow fields and introduction pressure drops caused the kinetic energy of systems to go from about a factor of (inaudible) from 100 to 20 megajoules.

Further reduction is due to heat transfer and energy transfer, at least to SIMMER calculations. What we are trying to do in this fluid dynamics verification is to verify that SIMMER is, in fact, treating this part correctly.

Later, we will attempt to verify that it is treating this reduction correctly. This is a substantial improvement, a factor of five from what was previously assumed was substantial improvement. I think we could demonstrate fluid dynamics a reasonable advance in technology.

DR. CARBON: A question on this. I think I understand heat trensfer effect, which I would presume is simply the fact that you have some structural material. Some of the heat in the fluid is transferred to it. It is a sync. It's all very straight forward.

What, again, is the fluid dynamics of the fact in a simple physical --

DR. SCOTT: Historically, what we did with SIMMER, we turned it on and turned off all the heat transfer. What we would get in national tropic (ph) and isotropic case. What was rather naive of us is that we didn't even get close until we came up with this number.

Basically, what we're seeing is that presence of structures, fuel pin bundles, fuel pins themselves, so modify the flow field as to cause changes in pressure drops, which are not ideal, which cause you to depart from isotropic expansion.

Furthermore, the flashing of the core itself is not an ideal process in that all the parts of the core don't participate equally, as it turns out in the calculation, at least, as we will se a little later in reality. There is a flashing front that progresses through the core, which is very non-ideal, which instead of infultessing (ph) expansion of every packet of the core, every piece of fluid in the core.

A lot of the core doesn't even participate until the top part of the core is essentially gone. You see a flahing front proceeding through the core. These are basically fluid dynamic effects. Those are major effects which we have set out to try to verify.

DR. CARBON: Then one is simply more or less a pressure drop?

DR. SCOTT: Yes.

DR. CARBON: One is -- the other major one is an expanding fuel pin?

DR. SCOTT: Yes.

DR. PLESSET: Would you explain what this non-uniform expansion is? What does that mean?

DR. SCOTT: Well, that everything is not expanding simultaneously, as Dr. Carbon says, in the core.

DR. PLESSET: Are there pieces, each one expanding isotropically, or because of --

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DR. SCOTT: No, there are not even pieces that are expanding.

DR. PLESSET: That is not the same.

DR. SCOTT: Are not expanding really.

DR. PLESSET: Okay. So, that's the point. How is the expansion?

DR. SCOTT: The expansion essentially proceeds -- I can show you a schematic of the SRI apparatus, or better yet the PURDUE apparatus -- a couple of pages over reputing to be clear.

DR. PLESSET: Why it expands this way or to be clear as to what is happening, not why it does it.

DR. SCOTT: The core is represented by pressure that's in that apparatus. Type zero diaphrams which are located here and here are ruptured. This high pressure core is essentially seized atmospheric pressure against the flash.

This is an approximation what really happens in a reactor. In the reactor, of course, your (inaudible) --

So, that it is not quite instantaneous. It is for practical purposes virtually constrained over the period of burst. So, this part of the fluid -- and this came as quite a surprise to the experimenters, as a matter of fact -- starts to flash first on the top, and the instrument remains single phase for a very long time. (?)

There is boiling weight that dresses backwards from this core. (?)

This came as quite a surprise to Dr. Theofanos. In fact, he reinstrumented his core in order to ascertain that this was really happening. It really is.

DR. PLESSET: That is a little different thing, actually. What you are saying, let me see if I understand it, is that you are getting a lot of energy going into lightened heat. Is that what you are saying?

DR. SCOTT: Yes. No.

DR. PLESSET: No? Not saying that?

DR. STEVENS: That is true but --

DR. PLESSET: That is true.

DR. STEVENS: That is not the effect.

DR. PLESSET: What is the effect?

DR. STEVENS: That pressure gradient is established in the core.

DR. PLESSET: Yes.

DR. STEVENS: During the flashing process, whether in the reactor phase or during the experiment phase. It simply does not follow an isotropic expansion.

DR. PLESSET: You're suing words again.

DR. STEVENS: I know, I know. I was just telling you -- I was saying: Yes, you're right. It is not doing it. I'm not giving you a right reason why.

DR. MARK: Am I at all close in mentioning that the isotropic model mentioned, that red box down there is full of gas at constant temperature and pressure. You then removed the disphram and the gas expands, basically.

What is rea-ly true is that it is a hot liquid, zero pressure -- well, no. It's a hot liquid anyway. It has got to convert its phase before it can really go forward and start to move.

DR. SCOTT: That is correct. Also, the isotropic expansion assumes that you're expanding essentially in the absence of pressure gradient in this vessel, which you really aren't.

DR. PLESSET: How fast does that take place?

DR. SCOTT: The boiling weight, or -- this particular case, about 6 milliseconds. Pressure gradient in this core persists to well after the head impact.

DR. PLESSET: There is also a mechanism that tends to uniformize the pressure and in the rought form of hte speed of sound.

DR. SCOTT: Yes, sir.

DR. PLESSET: Has that been checked that that is okay?

DR. SCOTT: Yes. During the shock tube problems, we see that we can, as I pointed our, we've analyzed some million (?) two-dimensional shock tube problems; we've valued gases.

DR. PLESSET: You're talking about shock tube. There, I have strong deviation from an isotropic ray. There I get

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shocks which (illegible) -- which is a way to get around isotropic behavior.

Now, are we getting shocks here?

DR. SCOTT: No, we're not.

DR. PLESSET: You aren't; so --

DR. SCOTT: Yes, it is different. It's just the treatment of sonic velocity in SIMMER was tested by using shock tubes.

DR. PLESSET: I am still not clear, but I -- I am not questioning. I am just trying to see -- it's not -- shouldn't be so complicated at what one can't give a picture of it.

DR. SCOTT: No, it shouldn't.

DR. STEVENS: There is a variety of non-equilibrium processes going on with this. There is slip between vapor and liquid. There is a temperature difference between vapor and liquid in an non-equilibrium phase transition.

All these things are intermingled in this. If you did a single cell gas problem with SIMMER, you could get it to fall in isotropic expansion. It will give you an isotropic expansion under the right conditions.

You can make approximations such that it will do an isotropic expansion, using very large inertal masses, doing a slow expansion, and drive it toward isotropic, using only a gas, for example, would help.

DR. PLESSET: What I am a little concerned about, I

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can see really -- I wan't quite telling the whole story that in this transition -- not in the sense that you were talking before, but right here in this experiment there wasn't isotropic. Later on when you get this gas which continues to expand, it doesn't do it much here. That will be isotropic. That's what I was wondering about.

DR. SCOTT: It will be very much closer after it gets to isotropic.

DR. PLESSET: That doesn't affect your problem.

DR. SCOTT: That doesn't affect the experiment.

DR. PLESSET: Not the experiment, I mean your problem.

DR. SCOTT: Our problem is over. There are two series of fluid dynamic experiments that look very much like the reaction down trade. One series was done at Stanford Research Institute, it has been called the SRI experiements.

DR. KERR: Excuse me, I missed something. I get from the applications and the discussion that you don't really completely understand the phenomena observed at PURDUE.

DR. SCOTT: At PURDUE, Yes, I think we do understand what went on in those experiments. I will come back to talk about those in just a second.

DR. KERR: Okay.

DR. SCOTT: The SRI experiements, we presented to you before, but I would like to just review these because they are very central to the verification of SIMMER.

The apparatus basically looks like this, .3 meter high; it's an acrylic vessel. All of the expansions are photographed by high speed photography. There is a chamber down here which contains 100 atmosphere, nitrogen gas or, I believe, it is 80 par-saturated water; the atmosphere is saturated water.

This lower passage -- this lower container is separated from what is a 1/25th scale of CRBR structures and internals, by explosively driven sliding doors which open in something like 200 microseconds.

So, that into the nitrogen gas or saturated water then sees the pool and begins to expand. There are many configurations that can run this experiment in, and have run them at all water. This flow can be empty, completely empty, or it can have an upright funnel structure which represents the empty subassemblies scaled model of the empty subassemblies of CRBR through which either the gas or the flashing liquid is discharged.

Furthermore, it can put an upper internal structure above the core structure, which is a scale model of the CRBR upper internal structure. Now, the structures do play an active role in litigating the consequence of this accident.

We should see longer tanks to head impact in these experiments, and reduction of kinetic energy pools, which is what SIMMER would have predicted and did predict for CRBR. Let me show you something that I showed you before, I believe; that is, the comparison of the SIMMER calculation and experimental

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results for the national experiments.(?)

With no structures present; with the other four structures present only, the upper internal structures only, and both structures present. The top numbers are the SIMMER computed numbers. The bottom numbers are the experimental values.

I believe the inpact times are very very good. These SIMMER pins to extend artificially the time to head impact with all structures present. SIMMER also tends to underpredict -- or over-predict, excuse me -- tends to over-predict the impact pressures at the head.

Now, there are a variety of reasons for this. One of the major reasons is the real head, of course, is elastic. The calculational model assumes that the head is methematically rigid, under formable.

Another reason is that real experiment can get tailor (?) instabilities developing as the pool is accelerated toward the head and it reached to break out into a sort of a spray which increased the vaporization area in the experiment, which causes expansion (inaudible).

The consumer always treats it as 80 bag.(?)

DR. PLESSET: You don't put tailor instability -- you don't put it at all?

DR. SCOTT: We can't. There is a weight of appriximating tailor instabilities in SIMMER, but we don't really treat it that way.

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DR. STEVENS: Dr. Plesset, some people will argue, and I don't accept it necessarily that the numerical diffusion process in SIMMER gives you something that looks very much like -- if it did, it would be fortuitous.

DR. PLESSET: We would be well off. I don't think it does. I think it should be not too difficult to put it in. I don't know. There is a lot of approximation to the ten parts to quality.(?) They are hard to handle.

DR. STEVENS: I think the numerical problems -- well, one could put any model that would account for the effect of tailor instabilities in terms of break-up of an interface.

It would have to be benchmarked against some good experiments that were relatively prototypic of the geometry when we're looking at it. I don't argue with you. You can put in a model.

You would not want to put in a model that follows the instability process itself, I think; but something that merely accounted for it in a parametric way.

DR. SCOTT: You can see instabilities. SIMMER will grow instabilities if we start with the variation, for example, in height of the pool, jsut a small variation.

Or if you pass the fluid through some structure which flows down one part of the fluid relative to another, you can see the stabilities form and grow. As far as verifying that SIMMER is doing that correctly, I wouldn't want to say that now.

DR. STEVENS: Los Alamos has a lot of knowledge about instabilities.

(Laughter.)

DR. SCOTT: That is true.

DR. STEVENS: More than anybody else I know.

DR. SCOTT: I think Dr. Mark may have more than we do.

DR. MARK: I n't think that the knowledge that annular wear-out includes (inaudible) would have to be taken account of here, heat transfer as effected by the interface pattern and phase changes as affected by the heat transfer while you're following the known --

(Laughter.)

DR. MARK: TKe percent of istoropic impact energy, the percentage is less than one because of heat loss or do you take account of heat loss?

DR. SCOTT: This is the nitrogen expanstion. There is, of course, some expansion cooling in this. In general, your following foom temperature nitrogen and room temperature water. This loss is primarily due to just the throttle above the core area of change.

That was the point that I really wanted to make with this vu-graph, that the percent of isotropical impact energy is being reduced by the structures.

DR. SHEWMON: Let me point out one other thing, that in addition to being elastic in structures, you are likely to apply this to, or even plastic. That would absolutely, not perhaps.

That would absorb a lot more energy if it got down to using it.

DR. SIEGEL: I guess I had a similar question. Is the upper core and upper internal structures -- are they rigid in the experiment and the calculation?

DR. SCOTT: Well, they're rigid in the calculations. Unfortunately, I don't believe they can possibly be rigid in the real world.

The -- Dominic Caliostro (ph) likes to claim that for practical purposes they are rigid, but I am not sure. As you can see, the upper internal structure is suspended from the top cover. That is set in this lucite container. It essentially has a strong back across the top of it.

It has to move. I think in some of the films you can see some slight motions on the upper internal structure, which we don't calculate, of course.

This, I am convinced, is reasonably ready. The upper core structure screws into the top of this lower container, and is probably for practical purposes is rigid. Rolls in this apparently don't flex much.

They put string gauges on the walls, there is not much wall flexure because they're about that thick, lucite.

All right. Having said that about the nitrogen experiments, let me go on to show you the complete set of experiments we have now done with SIMMER; all the nitrogen experiments; and all the falshing water experiments. We see here the head impact times compared for the nitrogen experiments, various nitrogen experiments, flashing water experiments, and the core pressure; that's down at the bottom; below the sliding doors which is the only place it is convenient to measure with all those structures in place as a function of time.

It must be out to about 4 milliseconds. The (inaudible) looks quite good, even with flashing water. I have to confess that it didn't get that good in the dream(?) that using dispersed flow regime that comes at the off the shell version of SIMMER.

In order to get results that were this good, we had to implement a bubbly flow model, which is one of the things you learned from experiment analysis, I suppose, while you do it.

There is a complementary set of experiments going on at Purdue University. It looks like this. It looks very much the same. The difference is the whole thing is still 1/25th of 1/7th of the scale of CRBR. The pressures are in the range of, instead of 100 atmospheres, 100 and 300 psi. So, this gives us an opportunity to look at a larger scale and a different driving pressure.

This apparatus does not have fast opening doors. It has two diaphrams which were ruptured. This lower pressure vessel then blows down into this throw-bridge, then finally up fm20

in the pool.

We have completed this year the first three analyses, the first three experiments. I might say that Dr. Theofanos at Purdue refused to give us the answers that we done in the analysis.

Stangely enough, we did not know what the answers were until we submitted our analysis. What we can measure best is impact time; show you a few cases for nitrogen expansion. These are our predicted impact time. These are the ones that are measured by Dr. Theofanos, estimated to within a millisecond. That's the impact time.

Over that range of pressures, I think that is very very good agreement, to tell you the truth. This gives me considerable confidence of fluid dynamics treatments of SIMMER, although I will have to say there is an awful lot of difference between flashing water and just room temperature water, and fuel and sodium. That's a very large extrapolation, to me, but this certainly improves, at least my confidence.

Once again, we were unable --

DR. PLESSET: I don't think you should get carried away by that.

(Laughter.)

I'm not as impressed as you are over that agreement. I think that one could calculate this without using SIMMER.

DR. SCOTT: As a matter of fact, you can.

DR. PLESSET: Yes, and get as good a result.

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DR. SCOTT: Up to head impact, you can calculate it just as well analytically. What you can't do, though, is when Theo gets around to running these with flashing water, it will be real tough to do it by hand.

This is in your pack. Joe commented on it. It is just further indications you really can't get by without a bubbling flow model from SIMMER.

DR. SHEWMON: What is RP Min on your slide?

DR. SCOTT: RP Min is a parameter that one uses in SIMMER to control this. As you know, SIMMER will break up droplets by a variety of mechanisms. I can't remember them all now, rubber break-up, flashing, some other -- any others?

Those are the two main ones. That is a constraint that is the lower bound of what we will let drop down. Sometimes SIMMER will stop before it gets there. Sometimes it will just keep going.

This was done just to show that for any bubble size, the off the shelf version of SIMMER can't possibly compare to the experiment. With the bubbly flow model, we get reasonably decent gradient.

DR. CATTON: In retrospect, do you understand why that is so, or is it just fortuitous?

DR.SCOTT: No, I think in retrospect, we understand why it is so. Especially in the slow expansions, like the PURDUE experiment, and in the absence of pressure gradient.

As in both of these experiments, you can see that the flow regime does make a difference. Now, if we had a large pressure gradient across that lower core like we're like to have with CRBR about 300 atmospheres per litre, I am not convinced that the flow rating would make all that much difference.

Certainly, the momentum transfer, that's the vaporization of condensation. What we're seeing, I think in the longer Purdue expansion and in the expansions perhaps of a pressure gradient, that we really do have to treat the details of flow.

DR. CATTON: When you chose to use a bubble flow model, did you do this based on flow regime maps, were you looking at flow refractions and flow rates and so forth; or did you just try it?

DR. SCOTT: No, I think this model was really developed by Oddisue Antilla (ph). He did look at the literature for quite a while before he decided. He actually tried three of four bubbly flow models before he found one that worked well in the SIMMER context.

He believe that you would expect bubbly flow in these experiments.

DR. CATTON: If you have a boiling way of travelling down this part of high pressure -- or highly superheated fluid, I am not sure it owuld be bubbly flow. I believe -- didn't Mike Rolz(ph) at Argonne take some high speed photographs of that phenomena?

DR. SCOTT: Yes, he did. It looked kind of like --

DR. CATTON: A mess.

DR. SCOTT: A mess, yes. It looked kind of like bubbly at first, as the bubbles nucleated. There was a region of bubbly flow as it progressed down and followed by churn turbulent and dispersed as it woke up, I think, in the churn turbulent regime.

DR. CATTON: So, there is good comparison that bubble was fortuitous?

DR. SCOTT: Either that or a matter of whether you use bubbly or churn turbulent.

DR. STEVENS: In a lot of calculations, there seems to be a tremendous insensitivity to the flow regime. In some cases there is a sensitivity.

DR. SCOTT: Yes, the ones that of course approach where they can get flow transitions appears to be sensitive, the ones where it is a highly dynamic situation, we don't have time to develop any particular issue, it appears to be insensitive.

All right. Let me draw some conclusions of -- fluid dynamics verification did not mention that they also did some coolant experiments at 1000 - 2000 atmosphere range, which came out considerably less well, until we decreased the (inaudible) size and the timesteps.

I would say that SIMMER ploy dynamics performs very well compared to the experiment. Also, compared to analytic solutions to, say, velocity profiles or melt points, in the 1 - 100 atmosphere range. the 1 - 100 atmosphere pressure range

In the 1000 - 2000 atmosphere range, you can't assume that it is going to perform well without making some adjustments for shocks. The most important work, I think, the stated effects of the structures on kinetic energies that were taken by SIMMER have been pretty much substantiated by experiment, both that they exist and that the magnitude that this calculation is correct.

DR. SIEGEL: How does this occur physically if the structures are rigid? What is it that causes the dissipative effect?

DR. SCOTT: Well, it is essentially modifying the flow paths you have going from a very large opening in the core regions itself, as it comes up; it engages the, say, with the upper core structure gone, the upper internal structures. Then, there are pipes, essentially above that; just tubes.

You have entrance losses, and pressure drops across that that modifies the flow field to the point that you get substantial departure from the ideal flow.

DR. SHEWMON: Now, this fluid that is flowing is some sort of a mixture of gas particulates, which flows through the pipes with the melting point well below that and the temperature without heat loss or something?

DR. SCOTT: In the experiment? In the experiment it

bfm25

is with gas, yes.

DR. SIEGEL: You are talking aboutwhat rises --

DR. SCOTT: Yes. It's room temperature nitrogen going through --

DR. SHEWMON: I don't know.

DR. SCOTT: Pardon me, go ahead.

DR. SIEGEL: I was etalking about what Scott was describing.

DR. SCOTT: We've got five minutes or so. It's been a long time since I have shown you a film comparison between SIMMER and SAS-3D or single -- subsingle disruption.

DR. CATTON: Are you trying to make any comparisons with something as simplistic as a debris bed of some kind? That seems to me to be closer to your SIMMER modelling of a reactor core than the experiments that you looked at.

DR. SCOTT: We did some momentum transfer experiments, you may recall, a few years in various fluid size beads, fluidized beads in a glass tube to look at fluidized height and the period of oscillation.

That was reported somewhere -- oh, Paul Rexroth and Oddisue Antilla (ph) reported that at the specialists meeting on predictive techniques and experiment analysis at Los Alamos.

As a matter of fact, I have, I believe, the results of those if you would like to see them.

DR. CATTON: I don't think I want to delay this. What about heat transfer?

DR. SCOTT: No heat transfer in fluidized beds. Basically what we have here is an analytic model of single CRBR subassembly (inaudible) and SAS-3D. We put this through a loss flow as they would --

Here is the comparison of temperatures, fuel cladding, just prior to void initiation. It is gratifying to me to see that relatively crude heat transfer treatment in SIMMER still doesn't look that bad compared to SAS-3D, SAS being the solid line -- is that right? No, SIMMER being the solid line and SAS being the dashed line.

The practical purposes, up to void initiation the results are identical. However, if you look 1.9 seconds after void initiation, you see the lower fuel temperature predictions are still pretty good.

As far as the cladding temperatures go, SAS is now predicting considerably lower temperatures in the bottom of the fuel assembly, higher temperatures in the lower actual blanket. That is because SAS will use more chugging at the bottom of the subassembly than SIMMER typically does. That removes energy from this region and deposits it here.

SIMMER, the oscillatory nature of the sodium trying to reenter from the bottom is most pronounced in SIMMER. However, in spite of this, you will see that with interface location versus time for SIMMER -- SAS-3D, you will see that SIMMER bf

generally exaggerates somewhat, but not greatly, the upward expulsion, the sodium. It tends to be pretty much right on, except for the aforementioned oscillations.

DR. SIEGEL: What is this you are plotting now?

DR. SCOTT: That is interface locations, sodium interface as it is voiding from the core.

DR. CARBON: Do you mean between liquid and vapor, sodium liquid and sodium vapor?

DR. SCOTT: Well, this is essentially sodium liquid above these lines, and below the lines with vapor in between. That is the position of the upper moreso than the liquid interface.

It's a reasonably good agreement, except that the difference in the cladding temperatures, we noticed in the previous slide will, of course, influence cladding relocation to perhaps (inaudible) to some extent. It was largely these studies that led us to try to implement an annular flow model for cladding relocation SIMMER which is currently under way.

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It's hard to say that the SASS treatment has been more verified than similar treatments. But there is a logical reason it's widely accepted.

And finally, current status of SIMMER verification. And this is --

DR. STEVENS: And you have checked to make sure that SASS is not changing their treatment, so that they're going to non-annular flow at the same --

(Laughter)

DR. SCOTT: That's right. They are not going to non-annular.

We would hate to chase them around all over the map; that's right.

I believe, as far as SIMMER verification goes, we have generated considerable confidence in (WORD UNINTELLIGIBLE) code to our (WORD UNINTELLIGIBLE) framework; the accuracy of the coding, I think we believe the coding is reasonably accurate and probably very accurate. And as far as functionality goes, what I mean is that other people besides ourselves can get it to run and --

(Laughter)

-- and essentially reproduce the test cases that we send.

I believe that we should have reasonable confidence in the ability to calculate two-dimensional fluid dynamics, but verification of the energy transfer leaves something to be desired. And the reason it leaves something to be desired is, there are few relevant experiments available against which to test (WORDS UNINTELLIGIBLE) highly interactive (WORDS UNINTELLIGIBLE) suggesting a fluidized bed might be good with energy transfer.

But we are very fortunate in having the and SRI and Purdue experiments, as well as (WORDS UNINTELLIGIBLE) and a few others, to check the fluid dynamics. We're not quite so fortunate when it comes to interactive heat transfer.

What all this says is that when you combine the two, heat transfer and fluid dynamics, the results are not outrageous: they're reasonable. But there's very little way to combine the effect of the two experimentally so we can look at them simultaneously. There's very few experiments of that nature.

And I might point out, as I have before, the flashing water and water are the greatest part of a real reactor situation but these are quite an extrapolation. That simply says, what that means, we've just barely begun to start thinking on how to verify SIMMER through the transitional phase -- which may be very difficult, because it's an extremely hard regime to experiment on.

And that concludes my remarks.

DR. SHEWMON: Sir, if I can take you afield once more, as I understand it, if we were going to apply this thing to a reactor, what you have done is to show that the isentropic energy calculations were quite conservative. If I can come back to the plasticity of a hunk of stainless steel, if we had a control rod and internal structure up there, the calculations that have been done so far have taken no credit for the energy that would be required to collapse this?

DR. SCOTT: That's correct. There's no strain energy calculations in SIMMER.

DR. SHEWMON: Well, and SIMMER is the most sophisticated of the codes that have been applied to the problem so far, is that it?

DR. SCOTT: Well, yes.

DR. SHEWMON: At least with regard to that particular --

DR. SCOTT: Well, there are other codes, such as REXCO ISCO (phonetic), that are developed at Argonne National Laboratory, which are designed specifically to look at the structural dynamics under these situations. What those codes cannot do, on the other hand, is calculate the pressure versus time for the loading pressure.

What we can do is calculate a loading history. But we do not calculate the structural dynamics.

What we have sometimes thought of doing is providing these codes with a PV curve that looks like what we calculate for disassembl (WORDS UNINTELLIGIBLE), whether there are large changes into strain energy.

DR. SHEWMON: Thank you.

(Pause)

DR. BOHL: Bill Bohl, from Los Alamos. This discussion is somewhat of a digression. The problem addressed here concerns a hypothetic core meltdown accident, pressurized in all the reactor, similar to that existing at the Zion site in Illinois. The question is whether the downward melting core material contacting water could generate a steam explosion such that the resulting upwarding directed fluid kinetic energy would not only fill the pressure vessel but also generate a missile which would be sufficient to fill the containment.

The approach and scope are shown on this first Vu-graph. Sandia vapor explosion experiments were used to calibrate a two-dimensional version of SIMMER and to analyze the resulting steam explosion expansion. The reactor calculations used the same heat transfer assumptions which we used in the experimental calibration. And various steam explosion expansions (WORDS UNINTELLIGIBLE) loading patterns would then follow, assuming the pre-mixed interactive configurations.

The experimental geometry is shown here. The experiment to be simulated was the explosion resulting from dropping about 10 kilograms of iron aluminum oxide thermite into a tank of water.

Recent tests had been done with a lucite container. And here one can see the thermite falling through the water in a film boiling mode, the detonation wave passing through the mixture, and the resulting explosion.

The approximation used in SIMMER was to assume a (WORD UNINTELLIGIBLE) pre-mixed region down at the bottom of the vessel, or tank, and a two-phase liquid vapor chimney above this pre-mixed region.

The key assumptions were that the pre-mixed region was one-tenth of a meter in radius, the vapor chimney above this region was one-tenth meter in radius, and to obtain agreement with both the kinetic energy produced by the explosion and the rapid pressure pulse rise time, a water-(WORD UNINTELLIGIBLE) mixture had to be assumed. About a 300-micron-particle fuel diameter was assumed.

The fuel particle diameter is somewhat consistent with what was found from the finds after the experiment. And overall they have reasonable agreement with our Test 43 pressure history: a rapid rise to a near-critical pressure within the interaction zone and then followed by a rapid decay.

I guess the claim is that the calibration is reasonable but not necessarily unique. I have a comparison here, which unfortunately did not get in the handout, of the pressure pulse rise time -- let's see, I'm not sure how to put this thing on -- in the calculation versus that in the experiment.

DR. KERR: You don't have a pair of scissors on you, do you?

DR. BOHL: Not really. It fits on --

DR. KERR: If you did, you could just put one right on top of the other.

DR. BOHL: Well, unfortunately, the scale is a little bit different, too.

(Pause)

The rapid rise of pressure pulse is terminated by the (WORD UNINTELLIGIBLE) around the interaction region, in other words, the vapor chimney that's immediately adjacent to the interaction region. And then the rapid decay is due to WORD UNINTELLIGIBLE) of cold water quenching of the interaction zone.

These pressures are pressures that are observed at the side of the tank, where the pressure transducer was flipped on to kind of bracket.

The scale here is one-half the scale of the calculation.

The important point, I guess, to observe is that these pressures go up to about 6 to 7 megapascals, and the decay is observed over a period of 5 to 10 milliseconds.

DR. PLESSET: Do you have any way of telling how your experiment would have gone if you had a different initial ambient pressure, say, you were quite a bit higher? Because that's what we're interested in in LWR.

DR. BOHL: The current experimental data suggests a definite pressure effect in terms of reducing to the fragmentation potential for a vapor explosion. There is some question yet as to whether there is a definite cutoff such that at operating pressures one would observe no potential for a steam explosion or whether, say, the so-called pressure effect is simply due to a higher degree of difficulty in collapsing to vapor film around a fuel part.

And I believe Sandia has an experimental program to more definitively resolve this issue.

DR. PLESSET: But my problem is the initial water temperature also.

DR. BOHL: Okay.

DR. PLESSET: As well as the ambient pressure.

DR. BOHL: The experimental series considered here considered room-temperature water and heated water up to saturated conditions. And they observed essentially no difference in the results, given the scatter of the data.

DR. PLESSET: That's not quite --

DR. BOHL: That's not? All right, can you explicitly elucidate your question?

DR. PLESSET: Well, all right. I have water at a thousand psi and the water is not boiling but it's heating, it's hot water, so that it's near the boiling point at that pressure. Now I drop this stuff into it. Okay?

DR. BOHL: I would suspect that the magnitude of the resulting pressure pulse would probably be reduced. However, if you had the conditions which we assumed of an entirely molten core, you have so much molten fuel, like a hundred tons of it, that the water virtually gets overwhelmed with energy, and one can still easily see significantly upward directed fluid kinetic energies.

I will get to that point, I guess, a little later.

We are not really trying here to model the mechanism whereby a film boiling pre-mixed region fragments now and it produces a steam explosion. Basically, what we're trying to analyze is the resulting expansion.

DR. SHEWMON: You say we've got a hundred tons of molten fuel. To what extent would your results depend on the stream shape, or the geometry of this, as it comes into the water?

DR. BOHL: We attempted -- well, why don't I go on, because --

DR. SHEWMON: All right.

DR. BOHL: -- that's part of it.

DR. SHEWMON: Okay.

DR. BOHL: We looked at a couple of configurations.

(Pause)

The first configuration assumed that a downward progression of the molten material (WORD UNINTELLIGIBLE) which will (WORD UNINTELLIGIBLE) heat capacity effects, and hence one gets a puddle of molten fuel. And at some point this puddle breaks through and you establish a region over which the core mixes with water. Essentially you have a pulling mode in its center. The water is displaced up the downcomer.

A single mode of interaction was to assume that the heat transfer goes radially preferential -- preferentially to axially, such that the core breaks through the downcomer and essentially mixes on the side, such that the expansion will, one, force steam up the downcomer and, two, tends to force water into the molten core.

DR. SHEWMON: Did it happen to break out on all sides at the same instant?

DR. BOHL: You mean in terms of establishing an initial mixing configuration?

DR. SHEWMON: I mean is this coming out on one side of the core or did you, because it was so convenient, assume it a one-dimensional problem or something and bring it out on all sides, all the way around the circumference at the same time?

DR. BOHL: Because SIMMER is a two-dimensional code, we have to assume azimuthal symmetry. And so in this particular case we had to assume that it was all the way around.

DR. SHEWMON: Well, that kind of a piston effect, I would think, would give you a lot more something, a lot more oomph, than --

DR. BOHL: It would tend to exaggerate the interaction.

DR. SHEWMON: Yeah. I see.

DR. BOHL: I guess, to summarize the assumptions in the analysis, we used the same heat transfer as in the experimental calibration; in both our geometries we assumed a pour-in mode of mixing with 10 to 20 percent of the molten core materials pre-mixed with the water and steam. However, the overlying molten core precluded formation of a vapor (WORD UNINTELLIGIBLE) and provided a much more significant inertial constraint.

Also, in this calculation we ignored the internal structures.

DR. SHEWMON: Now, does that inertial constraint raise the pressure and slow down the reaction? Is that implicitly tiring them out? And does your model bring that in?

DR. BOHL: Yes. That is one of the primary results that one gets out of these assumptions. The pressure observed in the reactor calculation when you pre-mix 10 percent of the molten fuel with the water in the mode where it's down the center is shown on this Vu-graph. And instead of pressures that are 6 to 7 megapascals, one gets pressures that are on the order of, say, 200 megapascals, ignoring the single-phase pressure spike. And these pressures tend to be maintained for a significant amount of time.

As you expand, now you're entraining fuel into the interaction, rather than entraining cold water punching the interaction.

So it's important, I guess, to point out that the inertial constraint in this situation lengthens the expansion time and increases the efficiency relative to experimental configuration or simulation.

Given this assumed initial configuration, upward directed fluid fuel kinetic energies of a thousand to two thousand megajoules seem likely. We did a case where we decreased the heat transfer by more than an order of magnitude through increasing the particle size to millimeter-size particle sizes rather than 300 microns. In the reactor configuration, this decreases the kinetic energy only by a factor of two, due to the time available for heat transfer. In the experimental simulation, it decreased the kinetic energy by a factor of 18.

And better quantification of containment failure likelihood should consider that core melt sequence, the incoherence of fuel dynamic loading and structural accommodation, not the nonexistence of steam explosions, if you have situations where the core could melt down under atmospheric pressure and fall into water that's, say, saturated under those conditions.

DR. SHEWMON: Sir, you for convenience assumed that 10 percent of the core was -- the fuel was immediately mixed with water. If that had been 1 percent, or 1/10th, of a percent, would it have made any difference?

DR. BOHL: The 10 to 20 percent was chosen on the basis of the historical development of the problem and because it seemed intuitively plausible.

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DR. SHEWMON: It seems to me it's intuitively plausible that everything has to start from zero, instead of sort of full-grown from the head of Zeus, as the old saying goes.

(Laughter)

So can you answer, or will you answer, my question if I shut up?

DR. BOHL: It probably will make a difference with respect to the details. However, given, if one assumes, that the entire core is molten at the time this interaction occurs, it may be difficult to, say, avoid the result of significantly generated fluid kinetic energy, simply because of the constraints on the system.

DR. SHEWMON: Well, nobody believes that all of the core is going to be molten. But it's a nice bounding convenience for physicists. So.

But if we forgo that for a minute, what would happen if this dribbled out over a period of time? Would it be you wouldn't get that piston effect that you have, or you would take longer to get to that initial condition, or what?

DR. BOHL: I think it's plausible that you could get the water type of system in benign fashion. And --

DR. SHEWMON: What does that mean? You mean you dry out the bottom and then --

DR. BOHL: No. I -- well, I think a further program which addressed the initial phases of the accident in a more mechanistic fashion may show that the configurations I have assumed here are overly conservative and that if it dribbled down that the water would leave without an appreciable interaction or an appreciable generation of pressure.

That's certainly not an unreasonable sequence to conceive of.

DR. SHEWMON: I guess I wasn't trying to push into that though it's heartening to hear, but that if I take a ladle and pour it into a wet mold, there's a lot of activity, I grant, but I don't -- what I was trying to get at was why you had to assume that as your nucleating event. Is it, the model can't treat the (WORDS UNINTELLIGIBLE) event, or what?

DR. STEVENS Dr. Shewmon and Bill, could I interrupt just one second?

One reason for assuming the particular configuration here that has some physical basis, even though it may not be perfectly correct, is that the experiments seemed to indicate that when you have a core of the molten thermite material into water, that the interaction -- the rapid interaction -- is triggered when the front hits the bottom of the container. So that --

DR. SHEWMON: That means the molten material --

DR. STEVENS: The molten material falls through the water, and when it hits the bottom it tends to trigger the interaction. Now, this isn't observed in every case, but in most of the cases that is observed.

DR. SHEWMON: You mean before it gets to the bottom
there's a steam blanket and then it disperses when it hits the bottom and that starts things, that --

DR. STEVENS: That's --

DR. SHEWMON: -- enhances the heat transfer and --

DR. STEVENS: That's the speculation. You see a very
-- when the molten front hits the bottom of the container, for whatever reasons, pressure pulse generated by entrapment as it hits the bottom or whatever, there seems to be a very rapid fragmentation wave moving back up through the molten material that causes extremely rapid heat transfer.

Now, that was the particular reason in this case for assuming that that intermixed region extended from the grid plate to the bottom.

Now, the radius of it is, obviously, open to question. But the idea that it can be triggered very quickly is something that is physically plausible, based on the experiments.

DR. KELBER: I would like to add a comment, Phil. We're getting to some extent into the area of the Class 9 Accident Committee. And of course there's a great deal of overlap here because of the work. This is an interesting application of SIMMER to problems that are somewhat outside the scope of fast reactors but are of considerable interest to the safety community.

We did not attempt in this case to make an entire model, for example, of the steam explosion working. There will be considerable work between the group at Los Alamos and the group at Sandia.

It does, to me, this work illustrates the need for very careful consideration of how one is going to extrapolate the work done at Sandia in the FITS experiments to the reactor case.

Also, I think there is a point that has been brought up that the inertial constraint by the massive core gives you a considerable lengthening of the time scale, so that the particular details of how the mixing occurs may not be so important as they are in a smaller-scale experiment. That I think is important.

Another, another point that is important is that even if one doesn't have a significant steam explosion in the sense that Ernie Gilby (phonetic) and others who have followed him have discussed steam explosions, you may have so much steam generated, just because there is a large surface area for treatment of -- or transfer of steam and there is a high enough inertia that the steam accumulates in a constrained volume for quite a while, that considerable damage might be done.

For example, one might develop pressures sufficient to rupture steam generator tubes.

These are important considerations and illustrate, I believe, what we all know to be the case, that it is important to have a useful tool to extrapolate to the case of interest, unless you are doing a fully prototypic experiment. And we, of course, we are not.

DR. KERR: Did I understand you to use the term "FIT tests"?

DR. KELBER: Those are the tests being done at Sandia.

DR. KERR: What does the acronym mean?

DR. KELBER: Fully instrumented tests.

(Laughter, quips)

As opposed to the partially instrumented tests that were done earlier.

(Laughter)

I can't help it -- I didn't do it. I'm only reporting the past.

(Laughter)

DR. BOHL: Okay, to conclude. We have found that two-dimensional behavior strongly influences the loading dynamics. If one is attempting to accelerate a shallow pool over a considerable difference, you do not get a piston interaction.

The loadings tend to be biased more towards the apex. And that increases the likelihood of large missiles, such as presented in WASH-1400 where the entire upper head became a missile.

Further, lower head failures appear to be likely prior to any upper head failures, particularly after pressures that are calculated for the more severe interactions.

A model of a single -- of a one-degree-of-freedom rigid plate system under dynamic loading was analyzed by the structural people at Los Alamos, and the lower head was found to fail with -- at four to five milliseconds for a 100 megapascal loading.

Finally, eventual verification of lower probability for containment pressure and steam explosions is likely, although this probably cannot be technically supported conclusively under current boundaries.

DR. CARBON: Any questions?

DR. KERR: If you were someone responsible for making decisions about reactor or containment design, how seriously would you take these results?

They're interesting. But from what you know about SIMMER and its adaptation to this problem --

DR. BOHL: I would think -- .

DR. KERR: -- do you think they should be used in the decision-making process?

DR. BOHL: I would think the judgment on containment failure from steam explosions made in WASH-1400 is still the most appropriate basis to use: ten to the minus two plus one minus two on the exponent.

DR. CARBON: Any other questions?

DR. SHEWMON: Yeah. Just at the end you got to talking about missile generations. I assume this wasn't an elastic calculation when they got to the failure of the bottom head. Could you tell me anything about what they did assume about energy absorption approaches or what they were doing?

DR. BOHL: You're raising two possible questions. One is the generation of missiles from the upper head.

DR. SHEWMON: I was always --

DR. BOHL: And two is just the failure, the dynamics failure --

DR. SHEWMON: No, as I understand, the failure of the top one was, they evaporated the retaining bolts on the head, or made -- shipped them someplace else, and then they found that the head could, indeed, pick up a fair amount of velocity before it left for the containment. And that's quite plausible if you make a silly assumption to begin with.

So what I'm trying to get at here is what assumption was made with regard to the ability of this plate to absorb any energy.

Now, you may still blow it out like a balloon, but if it was -- okay, was it treated like a balloon, namely, a plastic material, or was it treated like something else?

DR. STEVENS: The calculations -- excuse me -- the calculations were finite element, elastic plastic calculations of the head dynamics.

DR. SHEWMON: I'm sorry -- of the bottom foundation.

DR. STEVENS: The bottom also.

DR. SHEWMON: Okay. Thank you.

With the retaining bolts in place?

DR. STEVENS: Where appropriate.

DR. SHEWMON: Yeah.

DR. CARBON: Let's break at this time for lunch and reconvene at one o'clock.

(Whereupon, at 12:00 p.m., the meeting was recessed, to reconvene at 1:00 p.m. this same day.)

AFTERNOON SESSION

(1:01 p.m.)

DR. CARBON: Let us reconvene and move ahead.

Dr. Walker, will you take over?

DR. WALKER: My name is Jack Walker, and I'm manager of Sandia's advanced reactors research programs.

During the rest of the day, we will be presenting a status report of our ARSR activities. Presentations will be given by those division supervisors responsible for the technical direction of our work.

Before we get into these more detailed presentations, I will try to give a short summary of some program highlights.

First let me say that during the past year we have made considerable progress in both test technology capabilities and test results. A number of facilities are just becoming operational which considerably expand our capabilities. These include the large melt facility capable of UO2 melts in the hundreds of kilogram range; the installation of the microprocessor into the ACRR control system, to produce prototypic LOF and TOP heating rates; the completion of major hot cell facilities; a new computer-based central data acquisition system; and a facility for sodium purification and test hardware filling and experimentation.

In the diagnostics area, both the coded aperture imaging system and the in-core fuel motion system have been successfully developed and are now being integrated into the program.

As you will see later, we view a significant improvement in diagnostics as essential for adequate understanding of loss of flow and transient overpower phenomenology. Other diagnostic-related items include major improvement in ultrasonic thermometry, aerosol sampling, and optical fuel motion detection.

Lastly, we are designing the ACRR flowing sodium loop and have constructed an out-of-pile prototype. As part of the multinational post-accident heat removal program on the ACRR, we have designed a bottom cool capsule and have designed and constructed hardware for first-of-a-kind in-pile transition phase tests, which will start later this summer.

To the degree that numbers of tests are, at least, one indicator of progress, one can conclude that the past year has been a very productive one. Of course, test quality is a better indicator -- and hopefully, that will come out in the later talks. We have completed some 30 to 40 major tests in the past year, with approximately half of them being in-pile. These in-pile tests are not the traditional large, expensive, long lead time proof tests which are the classical in-pile test stereotype but are, instead, closer in cost and time scheduling to any laboratory physics experiment.

The results of these experiments are used to support the development of models and to understand the basic safety related phenomenology.

In the next few minutes, I will highlight some programmatic milestones and summarize where we are and where we are going in several projects. You will hear detail later, details of each of these later on in today.

First, we have now finished phase one of the accident delineation program. Several things have come out of this work.

First, we now have a cadre of staff who are experienced in probabililistic and risk analysis and who are intimately familiar with LMFBR safety issues. Our experience here has established a good framework to review the safety of any specific design which may be considered for licensing and, perhaps more important in the current of national FBR development, will allow us to begin to define those areas where work will best contribute to improved safety.

Furthermore, we have reached several tentative conclusions regarding the increased importance of protected accidents in regard to total risk, as well as low lamp rate unprotected TOPs. You will hear more about this in the next talk by Dr. Clauser.

In the accident energetics area, our studies focusing on primary vessel damage due to prompt core disruption have led us to conclude, contrary to previous assumptions, that coolant vapor, not fuel vapor, would probably be the dominant working fluid in a CDA.

We are wrapping up our fresh fuel PBE -- or prompt burst energetics -- program with experiments designed to determine if a large-scale propagating FCI can occur in an oxide system, and if so, under what conditions.

We will also be concentrating on extrapolating the results of these small-scale experiments to reactor scale.

In the EOS area, we are now reasonably confident that the order-of-magnitude-higher fresh UO2 vapor pressure observed for tests conducted in-pile and with electron beams over the open-system tests done by laser heating and over existing theories is representative of an actual reactor core, but probably of only secondary safety significance.

We are wrapping this work up now and are moving on to irradiated fuels.

DR. KERR: Excuse me. Would --

DR. WALKER: Yes?

DR. KERR: Would you go through that ten times --

DR. WALKER: Yes.

DR. KERR: -- segment?

DR. WALKER: Well, Dr. Camp will be covering this in detail, Dr. Kerr. I can comment very briefly on it if you would like.

DR. KERR: No, if he's going to cover it in detail, that's enough.

DR. WALKER: Okay.

The results of the ACRR fuel disruption experiments to date indicate fuel response under LOF and LOF-driven TOP conditions is a complex function of several parameters and differs with heating rate and, hence, accident scenario. The current program should soon provide sufficient data to adequately model fuel disruption for use in the predictive codes. This phenomena remains of high importance for systems with sufficiently high positive void coefficient to potentially get into a LOF-driven TOP.

In the post-accident containment area, the coolability of damaged core debris remains of major importance for FBRs and has become a primary issue for LWRS in the post-TMI environment. The ACRR V series par (?) experiments continue to provide data to support phenomenological understanding and modeling of the cooling process. D4 was completed last year. D5 and D6 will be conducted within the next few months.

Phenomenological models describing debris coolability are currently being sophisticated as a result of an ever-increasing data base and have seen considerable use lately in LWR Class 9 issues.

Very briefly and simplistically, we now know that coolant dryout is probably not the ultimate coolability limit for rubblized cores. Also, we have seen, contrary to what your intuition would tell you, that debris bed overlaid by hotter sodium is probably more coolable than those beds with the cooler sodium.

The D series program is currently being --

DR. PLESSET: If it violates our intuition you have to explain it.

DR. WALKER: Okay. If you would, Dr. Plesset, I would like to wait and let Dr. Coats discuss this --

(Laughter)

-- in conjunction with the model. If I start doing it, it may take five minutes, and that would be his five minutes. He's prepared to go into that.

DR. PLESSET: All right.

DR. WALKER: This is to whet your appetite.

The D series program is currently being expanded, under almost certain multinational sponsorship, to allow tests covering a much expanded parameter space.

As I said, Dr. Coats will discuss planning for this expanded program along with the D4 results, which are very much along the lines of this sodium temperature; and we have made some recent developments in the phenomenological models, recent improvements, and he will cover these.

Another area of current high profile is the interaction of hot core debris with containment and core-retention materials. Here we have a rather extensive program and are beginning now to develop an adequate data base in a rather limited temperature regime around the melting point. And we have done this for all common FBR and LWR concrete types. We now have a reasonable handle on basemat penetration rates and gas and aerosol productions for this temperature range.

Data is still severely limited for temperatures above melt, significantly above melt, and for temperatures below the solidus. We are also only now beginning to get core melt interaction data for the common core-retention materials. But we believe with the program now in place that this data base will be reasonably complete within the next couple of years.

These data are essential for assessment both of basemat attack and penetration as well as the determination of the loading for the containment building or to mitigation systems such as vented filters, which arise from the copious gases and aerosols which are being produced by the interaction process.

So we have two things to concern ourselves with: first of all, the basemat penetration; but secondly, the products of that attack being the products which define the load to the containment.

Dr. Powers will bring you up to date on this subject in his talk.

Finally, in the containment area, the first version of the CONTAIN code is now operating, and we are starting to use it for studies of containment response under various accident scenarios.

Let me close my introduction now with a status report on some of our interactions with the foreign reactor safety community.

Following the direction of the NRC and the encouragement from the ACRS, we are working hard to develop collaborations and integrate foreign work into our program. Here I list some of the more active programs in which we are now involved.

The ACRR debris exchange has been in existence for a couple of years now. Currently we have on Los Alamos and one Sandia staff member assigned on-site at Caterrash. The program is moving very slowly, and to date there have been no major results to report.

We have been very successful, however, in developing an active collaboration with Germany and the U.K. around the ACRR. We currently have four separate ACRR experiment activities jointly supported by NRC and KFK or UKAEA. These include the carbide fuel PVE series just being completed this month with the final fuel PIE.

Incidentally, a German staffer now at Sandia today participating in these examinations.

The high ramp rate disassembly test series is now halfway completed. And the equation of state and the U.K. fuel disruption tests should be conducted sometime before the end of the year.

We believe we are benefiting from these joint programs through the participation of foreign staff assigned both on-site to the program and to those that participate in Europe on the program. They are conducting test planning, interpretation, modeling, and -- of equal importance -- funding, since this seems to always be our biggest need.

I mentioned earlier the pending tripartite agreement between NRC, Euratom and PNC. This program represents the focus of the U.S, Europe, and Japan studies on in-pile rubblized core debris coolability. The program involves a number of first-of-a-kind, difficult tests, but we are enthusiastic that it can be accomplished and meet all of its objectives. And again, Dr. Coats will cover this a little later.

Lastly, shown here, we are currently discussing the possibility of a joint program with KFK on transition phase studies, transition phase phenomenology, using their recently developed large melt facility. This facility, as you know, is unique in the world, and it is just now becoming operational.

I think that will conclude my prepared presentation, Mr. Chairman. And we can go into the detailed discussions.

I'm sorry if I only whetted your appetite, but that was the intention of it, my introduction.

ND TAPE 4

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DR. CARBON: Fine. Let's move on.

DR. CLAUSER: What I'd like to describe is the accident delineation study, which has been under way for the past few years at Sandia. The intention is to have a comprehensive and systematic delineation of LMFBR accident sequences. And, as

Dr. Walker just described, we have just recently completed phase one, which has largely been a qualitative delineation, and we're proceeding into the more quantitative part, phase two.

Can you pick this up over there?

THE REPORTER: Not too well.

DR. CLAUSER: Not too well. Okay.

To go into a little bit more detail by way of overview, again, this is a -- this is intended to be a comprehensive delineation which covers the entire sequence of an LMFBR accident. And we have divided it into three phases, which are somewhat complete in itself: the accident initiation phase, including the engineering systems response; the accident phenomenology phase, which is basically the in-core events; and finally, the post-accident phenomenology, or containment events. And we have treated all three of these areas in some detail.

The first thing that was undertaken was to investigate the applicability of the event trees and fault trees that were developed, for example, in WASH-1400, and try to determine how well they would apply, particularly to the latter part of the accident sequence, for LMFBRs.

The initial conclusion is that they work out rather well for qualitative delineation and for the quantitative delineation in the engineering systems response area. Quantitative use of the event trees for the latter part of the accident, where we're presently dominated by phenomenological uncertainties, the application is still rather uncertain at this point.

The study has been initially based on CRBR, largely because we needed details, fairly detailed information, to make progress beyond rather superficial considerations. However, we are now proceeding to examine alternatives both in designs and various other options. On that, I'll mention one or two of these activities later on.

At this point, the event trees and some fault trees for the engineering systems have been constructed, and in some cases branch-point likelihoods where you have trees have been estimated. The purpose there is, first of all, to develop the methodology and to delineate the plausible accident sequences. This gets back to the comprehensive, fairly comprehensive, set of sequences have been delineated.

The estimates of likelihood have allowed us to determine the dominant sequences and to identify the key phenomena and uncertainties in these sequences.

The eventual outcomes of this study are, initially, to provide the basis for prioritizing the research, design, and development efforts that are ongoing, subsequently to provide a basis for assessing the relative safety of different components and designs, and ultimately we may be able to help establish some of the licensing criteria that are on LMFBRs.

Let me summarize some of the -- the present status in current activities, where we've been and where we're going.

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As has been mentioned, phase one has been concluded, and a report, a final report, of this activity is in the -- has been drafted. It's just now concluding a peer review, technical review at Sandia.

I might mention that the preliminary version of this, which was -- of this report, which was put out about a year or so ago, was extensively reviewed by almost all elements of the breeder reactor community, and the comments from that have been incorporated in the present version.

This, this study, will be available, hopefully, in about a month or two, in terms of a printed version.

Okay, as I mentioned earlier, it has been delineated -- the accidents have been delineated in three areas. And to my knowledge, I might add, in the last two areas this is the first time these have been dealt with in the detail that they are here.

Okay, the next couple of points I have covered. Fault trees have been established and are presently being quantified for the engineered systems. These, I might add, are for CRBR, because at this point that's the only system that we have enough detail on to provide reasonable answers, reasonable estimates of the probabilities and failure frequencies. That is ongoing.

In the accident phenomenology and post-accident phenomenology areas, work is beginning to use mechanistic systems codes such as those mentioned here -- SAS, SIMMER, BRENDA, SSC, and CONTAIN -- to study in more detail the progression of the accidents and try to get a better handle on how the various branches of the event trees are followed.

The work with SAS and SIMMER has been, is ongoing at Los Alamos. A little bit of that was mentioned this morning. We're beginning to get into that area with Sandia staff members, but at this point it is just beginning.

BRENDA is a code that the University of Arizona has put together, and contracts between the NRC, University of Arizona, and Sandia have been established or are being established to permit their work in this area.

CONTAIN -- well, SSC is only in the thinking stages as far as the study is concerned -- CONTAIN is at the point where it can begin to be used. And I'll cover CONTAIN in a separate talk at the end of this session.

Finally, we are at this point starting on a review of alternative containment designs, basically, to review some of the various possibilities and how they compare in terms of their safety aspects. I won't say anything further on that particular effort.

DR. CARBON: Let me mention to you that the Germans are initiating a year-long probabilistic analysis study for accidents on SNR 300, that you might wish to be in contact with.

DR. CLAUSER: Yes. As I understand it, one of their people is in this country, at SAI, I believe. Well, we're in process of establishing contact there, but thank you.

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Okay, first of all, the principal result of the study to date has been the establishment of a comprehensive and systematic delineation, a qualitative delineation, of the entire sequence of an LMFBR accident. It has largely been a organizational, information-gathering task. And the result of this study, as I mentioned, is the final report. That final report is approximately 800 pages' typewritten material.

Being qualitative and comprehensive, there's no way that I can try to summarize much of the detail there, and so I won't, won't try to go into that to any extent. I was tempted to bring along a copy of it, but I didn't quite have room enough in my briefcase, and so I'll have to ask you to wait for another couple of months. I don't think anybody is going to lose any sleep over not being able to read it.

But let me give you a little flavor of the -- of what we have done here.

This shows schematically how the system is organized. Basically, there are three areas: accident initiation, accident phenomenology, post-accident phenomenology. We start -- we start with a series of subsystem accident initiators; and for our purposes the reactor was divided into -- okay, there were 16 subsystems -- 15 is mentioned here, that's because this is a little bit of an old Vu-graph. Since then the operator was added as another subsystem. A generic event tree is used to delineate the engineered systems' response; and I'll show you that in a little bit. The outcome of this is the establishment of 26 different accident categories. One of these, for example, is the number that could cause a full accident; another is a number that could be a transient overpower.

I'll go into a little bit more detail later on.

Then, in the accident phenomenology area, these are basically treated in four separate groups, one of which is the protected accident -- well, set of accidents; another is the ULOF, unprotected-loss-of-flow accident, plus about five or six other accidents which are similar in nature, have similar phenomenology, such as the unprotected loss of heat, say. UTOP and related accidents: they differ primarily in the shape of the reactivity curve. And finally, the local fault propagation accidents, initiated by such features as a single pin failure and such like.

Okay, as a result of the delineation in these areas -- well, for each of these groups an event tree or series of event trees were established, which were used with some modification for each of these types of accidents -- the result of these is the establishment of about six different -- excuse me, four different damage categories, differing primarily in their severity, degree of energetics.

And then these are delineated in the post-accident phenomenology area in terms of three sets of trees, the first of which is the primary containment event tree, considers what happens within the primary vessel; the secondary containment event trees -- there are two, one for the reactor cavity area, such as below the operating floor, the second is for the reactor containment building, the upper atmosphere.

DR. SHEWMON: What does "unprotected" mean in "unprotected-loss-of-flow accident"?

DR. CLAUSER: Okay, the -- a protected accident is one in which SCRAM succeeds; unprotected is one in which it fails.

A loss-of-flow accident is where there is loss of coolant flow to the core.

DR. SHEWMON: I have some idea what that means. But "unprotected" in both of those means the control rods don't go in?

DR. CLAUSER: That's right.

DR. SHEWMON: Thank you.

DR. CLAUSER: Okay, the next Vu-graph shows the engineered systems' event tree, the response of the engineered safety systems to the actual initiators. And there is one point that I'd like to make here, if I can. It's a point that has been made before, but one of the things that stares at you in the face once you've gone through trying to establish these event trees and tried to understand what -- what the -- when you're trying to optimize the event trees you come across this conclusion.

It's the following.

Well, let me back up a minute. The five questions that we're asking here are whether detection succeeds, whether SCRAM succeeds, whether pump trip occurs, whether the -- whether SHRS is available, the shutdown heat removal system, and, as part of that but as a separate question, whether forced flow is available to cool the core.

DR. CARBON: Your pump trip fits in that category from the standpoint of preventing thermal shock? Is that why it's there?

DR. CLAUSER: Yes. Pump trip -- well, detection, when detection occurs, it causes the reactor to SCRAM and the pumps to trip; and the reason the pumps should trip is to prevent the thermal shock, yes.

DR. CARBON: There are other ways of handling thermal shock besides pumping -- triping the pump?

DR. CLAUSER: In CRBR that's the way it is now.

DR. CARBON: This is specifically CRBR now?

DR. CLAUSER: This is specifically CRBR. I'll leave you to judge how generic it's really.

The point I wish to make is that there are, basically, two routes to an unprotected-loss-of-flow accident. One is in which you have an initiator which causes the loss of flow and detection fails, giving you an unprotected accident, in this case an unprotected loss of flow. The other way in which it can occur -- well, it can also occur if SCRAM fails. However, if you have another initiator which does not cause an unprotected loss of -- which does not cause a loss of flow and you have a situation in which SCRAM fails but the pump trip succeeds, then you can get an unprotected loss of flow possibly combined with some other accident, giving you a combined accident which may be worse than if you were -- if there had been a simple accident.

And so one of the things that comes out of this is the suggestion that you can somewhat reduce the consequences of an accident. the risk associated with an accident, and certainly reduce the complexity of the types of accidents that we need to study as part of the research program, by having some sort of an interlock mechanism to prevent the pump trip unless the SCRAM itself succeeds.

DR. CARBON: Once again on that pump trip question and the thermal shock, if you scrammed, a thermal shock, is it something that would have a harmful effect in a single SCRAM, or does it not take many of them, such that you could almost delete pump trip from any serious accident sequence here?

DR. CLAUSER: It is my understanding that it may well be serious after a number of such --

DR. CARBON: Yeah, but how about after a single?

DR. CLAUSER: That I'm not sure of.

DR. CARBON: Because if it's not serious after a single one, you can delete it from most simple plotting.

DR. CLAUSER: Well, the point is that SCRAM -- or you have a reactor SCRAM not all that infrequently, and if you could tell in advance that it was going to be a serious accident, then you might arrange for the pump trip not to occur, but --

DR. CARBON: Oh, oh, sure, but -- but in terms -- obviously, that's not the correct thing, but -- but in terms of trying to predict serious accident?

DR. CLAUSER: I -- I guess I don't -- don't know where you're coming from.

DR. CARBON: Maybe we'd better skip it. Go ahead.

DR. KELBER: I think I can answer that. The CRBR control screen -- scheme is -- and this is fairly common in many systems that I have seen designed, that when the detection system orders a SCRAM, it also orders a pump trip. There are independent signals which will also trip the pump, but that one, it -- the logic is that SCRAM signal also implies pump trip. Now, a SCRAM signal is not synonymous with success of SCRAM; in other words, you can order the rods to drop but they may not drop.

DR. CARBON: I'm trying to say something else and it's not getting across. Let's forget it and go ahead.

DR. KELBER: Okay.

DR. CLAUSER: Okay, at risk of giving rise to a whole series of other questions, this morning the question was asked what sorts of accidents can occur, and these are basically a summary of the various accident categories. As I mentioned earlier, there are about 23 separate ones, of which these summarize. There's about five, five or six, in each of these.

The point is, for different initiators you can have different accident categories at the end of each of these branch points.

One of the interesting points is that even if everything succeeds, if it works -- goes away as it should, you can still have a possible CDA, possible core disruption accident, if the initiator was core damage. That's mentioned; you can read the separate part.

(Pause)

Let me -- there's a couple of points that I'd like to try to make with this next Vu-graph, to sort of give you a flavor of the way in which the study has progressed, as well as to lead into one of the conclusions which we have come to.

Initially, the study of the unprotected-loss-of-flow accident was done in a homogeneous core with a fairly high void coefficient. At that point, the initiation phase was deemed to lead, with more or less equal probabilities, qualitatively equal, into either a transition phase or an LOF-driven transient overpower, LOF'd'TOP accident, which would produce an energetic disassembly. The transition phase at that point was considered to be more likely to result in a non-energetic meltdown. Consequently, this would be the dominant risk contribution from this type of an accident.

With the advent of a low-void-coefficient heterogeneous core, one of the purposes of which, as I understood it, was to prevent the development of a overpower and consequent disassembly,

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was the result that you now lead more likely into a transition phase. And the recent results, for example, from SIMMER, which were mentioned earlier this morning, now indicate that that's about as likely or perhaps more likely to go into an energetic disassembly, so that this has resulted in a reorientation, partly as a result of the different designs, in terms of what are the dominant accident pathways. And this has some effect on how we organize the event trees and so forth.

DR. CARBON: Are you saying that you're as likely to have a disassembly in the heterogeneous core as in the homogeneous one?

DR. CLAUSER: That's the way it seems to occur. I think that's basically the statement that Los Alamos made earlier: you pay now or you pay later, but you pay in one of --

DR. WALKER: I think relative probabilities, we're not to the point where we can assign relative probabilities. But certainly that does hit you in the face, that you may not be improving your situation, because you're getting into trouble in another path that has been least -- not so well studied.

DR. KERR: Doesn't this depend rather strongly on how far you get into the transition phase and how far you go? It is true that you don't have this void coefficient and, therefore, if anything happens there's not going to be a rapid insertion but there is going to be a slower insertion. But that doesn't mean that you go along exactly the same pathway, does it?

DR. KELBER: Well, it may be a -- it may take lor er to get there. But I think the point that was made this morning is that the -- that assume that there is no removal of fuel via melt-out of the blockages: then the endpoint of the transition phase is an energetic disassembly initiated by large-scale coherent motions, and at that point the fuel doesn't have any memory of whether it was originally in a heterogeneous array or a homogeneous array.

Now, I agree that large-scale design differences, which might, for example, involve the dilution of the material by large amounts of blanket material, may make a significant difference. We don't know as yet. And that may make a difference in the energy scale that's involved. But I think that's beyond the scope of this study.

They're addressing the likelihood of flowing down a certain event tree.

DR. KERR: Well, whatever. I have not, at least, understood -- I won't say "heard," haven't understood -- anything today that would make me -- would lead me to believe that the disassembly is just as likely to occur for one core as the other.

DR. WALKER: That's correct. You should not.

DR. KERR: Yeah. Okay.

DR. CLAUSER: We are not trying to make at this point any particular claims as far as probabilities.

DR. KERR: Okay.

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DR. CLAUSER: Rather, we've tried to establish dominant pathways as a qualitative or semi-quantitative probability, if not a strict probability.

DR. CARBON: I thought that was just the opposite of what I thought you said a moment ago.

DR. CLAUSER: I'm not saying detailed probability --

DR. CARBON: There's no probability to this, then?

DR. CLAUSER: Only -- only guestimates, if you will, of what is, what appears to us to be, more likely.

That's far from a detailed quantitative probable -- probabilities study.

DR. WALKER: What I had meant to say was that this is an illustration of what can happen when one does a design change to remove some path: it may, in fact, open up a design -- another path as the more dominant path.

And in this case, if the heterogeneous core design has been successful in removing the LOF-driven TOP, then, in fact, you will most certainly have opened this other path as the most dominant path.

DR. CARBON: But, to be completely clear, there is no probability aspect to this, is that correct?

DR. CLAUSER: Order of magnitude estimates.

DR. CARBON: What do you mean "order of magnitude"? That there is an order of magnitude estimate, probability, here?

DR. CLAUSER: The uncertainties are at least an order of magnitude in the probabilities.

DR. CARBON: Are you saying that as far as you can tell one is as probable as the other but there are order of magnitude uncertainties?

DR. CLAUSER: Yeah.

DR. CARBON: That, then, is different, I think, than what you --

DR. KERR: I was not saying what they thought. I was talking about what I had heard -- which might be quite different than what they had thought. And I was trying to understand if what I heard was representative of what they thought.

At this point I don't know.

(Laughter)

DR. CLAUSER: The final point on this Vu-graph -- and here there are even larger uncertainties -- is that there are some qualitative similarities to what happens in the transition phase that leads to a disassembly and to what happens in a protected core-disruptive accident which goes through meltdown and with considerable uncertainty, then you go into a recritical pool at that point.

DR. CARBON: Finish your paragraph.

DR. CLAUSER: Okay. Yes. The point I wish to make is that because of these qualitative similarities in these two areas, we feel --

DR. SIEGEL: What's a protected CDA, a loss of all coolant?

DR. CLAUSER: Well, again, a protected accident is one in which SCRAM succeeds. A core-disruptive accident is one which goes far enough, usually due to coolant loss or, in general, some loss of cooling, such that the core can melt down, can disrupt.

DR. SHEWMON: Fermi 1 a protected CDA?

DR. CLAUSER: I'm afraid I'm not familiar with that.

It wasn't a CDA but it was along that path, yes.

DR. WALKER: It wasn't a core disruption if it lost its geometry.

DR. SHEWMON: I mean, you guys always generalize things to 100 percent core melt. But that --

DR. KELBER: No, Fermi 1 was an example of an accident initiator which, had there been damage propagation, could have gone to this. But, as we all know, it was far from that sink. They did not lose cooling, et cetera. And it was a very small locality. It was controlled very quickly.

DR. KERR: The elements would lead me to say it was an unprotected accident because --

DR. KELBER: No, this was scrammed.

DR. KERR: The SCRAM system worked after the damage had been done. But the period in which the damage was done was one in which the reactor was operating at power.

DR. SHEWMON: That's right in that respect. And so it's an unprotected loss of flow in a homogeneous core. You ought to read about it sometime.

DR. CLAUSER: Basically, all of the accidents in here are protected accidents. And some of them, core disruption may occur. That I think defines to some extent the character of a CDA.

Okay, again, the bottom line here is that as a consequence of the similarity, we feel that protected accidents may have the possibility, have the potential for having as severe consequences as unprotected accidents. There's considerable uncertainty there. But that leads --

DR. CARBON: Would you repeat that statement?

DR. CLAUSER: Well, let me -- let me repeat it as part of the next Vu-graph, if I may, because that's the point that I wish to come to.

One of the conclusions of this part of the study, of the study to date, regards protected accidents. First of all, we observe that protected CDAs are considerably more frequent than unprotected CDAs, at least, in CRBR. And this is a result that comes out of the CRBR safety studies.

As I just observed, and let me repeat it now, protected accident consequences may be as severe as those from unprotected accidents if you have an energetic recriticality in a protected accident.

We're now at a point where we can estimate the likelihood of that occurring in the protected accidents. There's very considerable uncertainty to that area.

Therefore, protected accidents may constitute a greater risk to the public, a higher probability and possibly equal consequences. As I have emphasized, there are considerable uncertainties in protected accident phenomenology -- quite large. They have been relatively poorly studied.

Therefore, one of the recommendations, along the lines of the charter of the study, the recommendation is to devote considerably more research and development efforts to understanding protected accidents, for the reasons outlined above.

DR. KERR: That's encouraging, because, it seems to me, it represents clear evidence that somebody has finally read an ACRS report.

DR. CLAUSER: This is not the first time we have stated this -- this conclusion, I might add. It was discussed last fall, as I recall.

DR. WALKER: Dr. Kerr, I think --

DR. CARBON: The remark was given two minutes and then ignored.

DR. WALKER: We're saying you're right.

DR. CLAUSER: That we agree, yes.

DR. CARBON: A protected CDA in your first line there is what, a loss of heat sequence?

DR. CLAUSER: By definition, here it is any protected accidents which result from core disruption.

DR. CARBON: And basically --

DR. CLAUSER: There are a variety of things which can lead to that, some of which are mentioned a couple of Vu-graphs back.

DR. CARBON: But you're saying that they're more frequent and that means you've got some numbers on them -- and where do they come from primarily?

DR. CLAUSER: CRBR (WORDS UNINTELLIGIBLE).

DR. CARBON: I mean what's going wrong: losing the heat sink or what?

DR. CLAUSER: The -- basically, you lose core coolability, in part from loss of heat sink and in part -- it depends -- there are a variety of accident initiators, and the details of what goes on depends on the accident initiator. I don't recall the details of which particular initiators are more probable and so forth.

But, look, in general, anything that causes core disruption, as far as we know, has got to result from an imbalance in the production of power versus -- or production of heat -- versus the taking away of heat by the cooling system.

DR. CARBON: Yeah, well, if you're protected you're not going to increase the heat generation, are you?

DR. CLAUSER: Certainly not compared to full-power operation, no. But there's of course the --

DR. CARBON: So you're going to shut down. And if you're going to shut down, does it not imply that you're simply losing your heat removal capability?

DR. CLAUSER: One way or another. What -- the reason I was being vague is because accidents occur in a number of different ways.

For example, you may have the pumps continuing to go, continuing to provide full circulation, you may have no loss of ultimate heat sink, but if your initiator was some form of core damage, then one of the questions -- which we don't know the answer to with certainty -- is how coolable this damaged core is.

Alternatively, you may lose the pumps and consequently be stuck with natural circulation.

DR. CARBON: If you have the pumps running and you SCRAM, how do you lose core coolant (WORD UNINTELLIGIBLE)?

DR. CLAUSER: In a damaged core I don't think it's obvious that -- well, if the core damage causes blockages, for example, of the coolant channels, that's one means of losing coolability.

(Pause)

DR. CARBON: Go ahead.

DR. CLAUSER: Well, okay, the principal questions as we see it at this point are the questions of core coolability in a general sense, questions of natural convection being able to cool an undamaged core or a partially damaged one, or the coolability of the damaged core, and second of all, the question of whether recriticality is achieved. There are several other questions involved. These are what we consider to be the principal ones at this point.

Going on, one of the -- another conclusion which has been reached at this point regards the -- regards low ramp-rate unprotected transient overpower accidents, which are here defined as being less than or approximately equal to 30 cents per second.

The situation is rather analogous to the protected accident case in that the low ramp-rate UTOPs are rather more frequent than high ramp ones. It's just a question of what is likely to go wrong in the control rod area.

And again, low ramp-rate UTOPs may have consequences comparable to high ramp UTOPs, though these have been studied rather less and so there's considerable uncertainty in this area.

DR. CARBON: Would you have to say, then, to be a little more precise, that they might but it's highly unlikely?

DR. CLAUSER: I guess we don't feel that we can make the statement that it's highly unlikely.

DR. CARBON: Well, it would seem that if you've got a lot less excess reactivity, that it just --

DR. CLAUSER: Well, it's, as I understand the situation, there is a certain analogy with this sequence here: that is to say, the high ramp-rate UTOPs are likely to go into a disassembly, the low ramp-rate ones are likely to go into something resembling a transition phase (WORDS UNINTELLIGIBLE), which, if you take this seriously, are fairly likely to go into --

DR. CARBON: I'd make the same statement here as back here, too. You said that they may -- but wouldn't you also, if you were trying to be as precise as possible, say they may but it's highly unlikely?

DR. CLAUSER: I guess we don't feel that we can be pinned down that much at this point, to make that precise a statement.

DR. KELBER: If I could interject -- I think that this, this particular problem that is that basis for a good deal of the DOE-UK combined program treating the PFR. They are focusing on this type of problem. And I think this study says that that's an appropriate focus.

I don't think that at the present time people are prepared to make the statement on the degree of likelihood that one would like to be able to make.

DR. KERR: I was about to say that if one has an area of ignorance, then research is indicated.

DR. KELBER: Well, I think the research has indicated to a point where we know that there is some payoff.

DR. CLAUSER: Okay, let me continue here. We therefore reach a similar conclusion that, again, low ramp-rate UTOPS may constitute a greater risk to the public than high ramp-rate UTOPs. And that qualifier should be definitely added there.

Again, the uncertainties in the low ramp-rate UTOP phenomenology going to a transition phase are -- you know, it's a rather large --

Parenthetically I would comment that UTOPs as a whole constitute a relatively small, well, I really should say, probability, because we aren't ready to go into the risks, relatively small part of the overall accident probability.

And again we recommend that more research effort be devoted to the low ramp-rate UTOP area. And I would also comment that as part of the experimental sequence on ACRR, some experiments in this area are in the final planning stages or have been planned.

I would have to say that I feel that this has lower priority overall than the protected accidents, but I think the cost of doing these experiments is also lower than the rather large amount of research that needs to be done for protected accidents.

Okay, a third conclusion -- I think I'm running a bit over time, so I going to try to speed up a little bit here -- in the area of local fault propagation accidents, some time was spent delineating this area; again one noticed that the initiators for these kinds of accidents -- single pin failures, for example, coolant channel blockages -- occur quite frequently; propagation does appear rather unlikely, though again fairly large uncertainties. The conclusion at this point is rather weak; that is to say, we do not feel that risk from these, from this area, from the local fault propagation accidents, do not feel they can yet be disregarded, though they're very likely to be a small contribution.

Finally, then, the area of containment. Partly as a result of a somewhat more quantitative estimate of branch-point probabilities, we can begin to be a little bit more quantitative.

First of all we note, again based on estimates that have a fairly large amount of uncertainty, perhaps as much as an order of magnitude uncertainty, we notice that containment reduces the probability of atmospheric release by, roughly, one or two orders of magnitude, that is to say, every -- what is roughly that? -- you know, one of ten or one of two, one out of a hundred core disruptive accidents might produce some atmospheric release.

Containment also reduces the consequences. But we haven't studied that part of the problem.

However, we note that the (WORD UNINTELLIGIBLE) path, give or take an order of magnitude, of all LMFBR CDAs may result in a basemat failure.

We note --

DR. SHEWMON: When you have that sort of a CDA, then CDAs always lead to large core melt, which then starts sinking down, leaving their sodium behind and working on the core at the mat, is that it?

DR. CLAUSER: Basically yes, because once you get a core meltdown, a core disruptive accident, you're fairly to breach primary containment, you're fairly certain to melt through to the basemat.

DR. WALKER: I don't understand how you leave the sodium behind, though.

DR. SHEWMON: That always happens. Every time we were out at your place last year we always had this darn core soaking through the concrete and the sodium would disappear. I never did learn where it went.

DR. WALKER: I don't think you listened to what we were sayi --

DR. SHEWMON: I listened as hard as I could. And I asked the question three different times. And you never answered it.

DR. WALKER: Yeah. Dana Powers will cover this. If you recall, we said we were studying sodium-concrete interactions and molten core-concrete interactions as separate subsets to get the phenomenology and then those are being incorporated to get a single phenomenology. You can come up with scenarios where you had concrete and sodium interacting without core melt. You can come up with scenarios where you have core melt and concrete interacting without sodium. You can also come up with scenarios where you have the three.

We have to deal with all three of those.

DR. SHEWMON: Okay. Now, in this particular situation do I have any coolers left in my containment? Am I refluxing the sodium?

DR. CLAUSER: I believe not.

DR. SHEWMON: Okay.

DR. KERR: About half of all the CDAs result in basemat failure.

DR. CLAUSER: This conclusion, as the previous ones are, based, basically, on CRBR, reactor containment of CRBR plants.

DR. SHEWMON: Now, does it have core cooling or not -- sorry, containment cooling or not?

DR. CLAUSER: It has venting. I don't believe it has coolant. This is taken from the CRBR documentation. And it does have the venting, which gives reason that the likelihood of breach of the containment building is --

DR. SHEWMON: A vent is called a breach, is that it?

DR. CLAUSER: No. What I referred to as an atmospheric release can consist of a, you know, gross failure of the containment building or it can consist of as small a thing as dirty venting, failure, or partial failure or failure of the

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venting system.

Well, to continue, then.

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The consequences of a basemat failure are far less severe than the failure of the ARCBs, and as a consequence of this conclusion and this one, I think that we would be fairly safe to say that containment itself substantially mitigates or reduces the probability of a release to the environment, and substantially mitigates the effects of a core destructive accident.

With that, I will conclude.

MR. CARBON: Does your study depend on the CRVR probabilistic analysis study. I am under the impression that some people who have reviewed that do not regard it very highly.

MR. CLAUSER: At this stage, we are, as I mentioned earlier, going through the fault-tree analysis of CRVS. We are taking the data that they have provided in the reports, examining to the extent that time allows. We are going to a rather more systematic evaluation of the various initiators, various fault-trees and so forth.

I should mention that our purposes are rather different from theirs. Theirs were basically a risk assessment. Ours are somewhat general delineations. So our purposes are somewhat different. The net result, depending a bit on how available some of the data is, should be I hope a more accurate assessments of some of the probabilities.

MR. KELBER: If I may interpolate here a remark that was engendered by Dr. Schuman's question earlier about where the sodium has gone, and what fuels a containment. I think that depending on what we do with LWR containments, the question of how you would protect fast reactor containments, if for example you want to have water cooling systems in the containment, may be a rather interesting combination of design and risk value impact study.

I would look forward to such system analyses in the next several years as the conceptual design study matures.

MR. PICKARD: Mr. Chairman, my name is Paul Pickard. I am with the Advanced Reactor Accident Energetics Division of Sandia Labs. My division is responsible for the performance of the pilot experiments in the ACRR dealing with accident energetics, and I would like to give you a very brief overview of the recent activities in our overall program, and describe a couple of the programs that we had mentioned to you the last time very briefly.

Dr. Camp will be discussing, right after my initial remarks, some of the recent results, conclusions and analyses that we have been doing in the accident energetics programs.

The purpose of the advance reactor accidents energetics programs at Sandia, of course, is to provide input for the resolution of some key issues in accident phenomenology relative to the progression of CDAs. The ultimate aim, of course, being to try to provide a data base to assist the potential threat from CDA to the containment.

Our ogram has been divided into really several phases here. Our fuel dynamics programs deals with the initiation phase issue, and there are two programs in this area. One is the visual fuel disruption program, and the other one is initial accented motion fuel program that is currently in the planning stage.

The work potential task within the energetics deals with disassembly phase phenomenology. This is comprising the prompt burst energetics capsule test. The effective equation of the pressure cell test, and the new core predispersed mixture FCI tests that are coming out.

In the transition phase in the past have done some simulant oblation heat transfer experiments, and we mentioned to you the last time but did not describe in any details some transition phase experiments in pile which deal with fuel freezing and streaming effects which I would like to mention a little bit more about today.

In addition to these major areas of phenomenology, we also supported this program with diagnostic development effort. It is obvious to us that one of the key deficiencies in the existing test is the diagnostic, particularly in the area of fuel motion that is available.

We have been working on for the past several years now a core adaptor heating system which we call CASE, that I will try to describe a little later on in a little more detail also.

We also have a much simpler scheme that has been under development using in-core detectors, vision gamma couples, vision chambers located in the pilot required core modifications, which we essentially to back out time dependent source location information with, and that is a program which has a great amount of potential for macroscopic fuel motion, but not high resolution fuel motion.

In addition to the diagnostics development, we have also been working, as Dr. Raft mentioned earlier on some facilities that support this program, the first of which was the ACRR operative modes. We were doing experiments in fuel disruption work, simulated and other rough scenarios. Also, we are looking forward to fuel motion fuel tests, and the ACCR operative modes are required to perform these tests.

We also are now in the process of doing many irradiated fuel tests, and the hot cell facilities are finally completed enough so that we can begin to perform irradiated at Sandia. In addition, we are also completing work on the sodium support facility for the IFM and PBE test.

I am only going to give a 30-second kind of summary of the activities in these past--. I am going to talk about the initial extended fuel program, and the transition phase program. The work potential task, and the fuel disruption program will be discussed by Dr. Camp.

Our work potential task, proper test of energetics, is our primary activity and here we are attempting to look at the combined mechanical energy source due to coolant and fuel vapor under prompt burst conditions. These are our single plane geometry capsule tests. We use both dry and stagnant sodium capsules. Up to this time we have performed about 20 of these tests, 17 of them have been done with stagnant sodium.

We have looked at both UO_2 and uranium carbide in these tests. Seventeen of these have been with fresh UO_2. The uranium carbide tests are now the subject of a collaborative program at Sandia Labs with the Germans from Karlsruhe looking at the post-irradiation examination of these pellets. That is currently in progress.

Since we talked to you last, we have also performed one additional experiment, Experiment 14-S in February which is the most energetic of the PBA tests to date. This is one program where we have at this stage been able to define kind of an "in-state" to this program. We have now defined five experiments which we believe will suffice to wrap up our capsule test in proper test of energetics area. These tests will examine the effects of temperature distribution, heat losses by using hot wall surroundings. There will be two tests in F-81 that will look at irradiated fuels.

As part of this program, we also do fuel motion detection tests. These are actually capsule tests with fuel pins in the reactor at a shakedown test core reactor energy system. This is coming out in August of this year.

In addition to the PBE part of the work potential test, we have also initiated some efforts in the fuel-coolant interaction area. Essentially these are going to be phenomenological experiments trying to address the question of whether UO$_2$ sodium systems can support a large scale propagating FCI.

These experiments will use a predispersed UO$_2$ sodium mixture, and these will be in pile testing reactors.

Since Dr. Camp is going to say more about these programs later, I will not say more about them. But they are scheduled to begin in October.

DR. KERR: Describe what is meant by a propagating interaction?

DR. PICKARD: A propagating interaction, the fuel-coolant interaction due to some triggering event in the transfer between the molten or very hot material and the coolant can propagate more or less explosively at vapor explosion.

We have done several of these with some fresh UO_2 in the past. We have now defined the collaborative program with the Germans at Karlsruhe to look at the pressure source from fuel temperature of interest. These will be, again, ECR experiments using pulse fission heating.

There is a nominal matrix of 12 tests set up here using UO_2, uranium carbide and mixed on-site fuels. There will be three irradiated mixed on-site fuel tests in this initial series. This on-site test, again, will be scheduled to begin in October of 1980.

In the fuel disruption program, which has been an extremely active program, the purpose of the program, of course, is to give visual observation of the disruption of the fission-heated fresh and irradiated fuels to try to evaluate the correctness of analytic models to describe this disruption process

These use high-speed cinematography. In the past, the first series of tests, the FD1 test which was completed on the old ACPR, was done with multiple pulses. Our FD2 series, which is now being done on the current reactor uses the advance mode capabilities of the ACR to more closely simulate all rough kinds of conditions. We have now initiated as of this past month the HRR series of experiments, which are a collaborative series of tests with the UK.

We [illegible] just completed four tests in the HRR series. Three of those were irradiated. The five tests in FD2, only one of which was irradiated. These tests will continue next year. The HRR series is schedule to be finished this year with six additional tests, five of which will be irradiated tests. FD2 will be finished in 1981. We are now looking forward to a collaborative program with the Germans in a series which we call FD4, again a LOFR rated high-power square rooted test.

I would like to spend a little more time on the fuel dynamics program, and the initiation phase fuel dynamics. This is a program we discussed at some length in the November meeting in Albuquerque.

The motivation for us looking at the initiation phase fuels dynamics program was a fruitful area because we felt that many key issues in the accident phenomenology, such as failure location and TOP mode time. So we thought we would place our questions in TOP's dispersal rates. All these questions seemed to require very high resolution compared to what is currently available to answer the questions.

We felt like a program that evaluated initial and extended fuel motion in LOF, and TOP, and LOFR areas was essential. In our review of this process, we essentially concluded that failure locations, times, and modes at the current time generally have to be based on inference. We have pressure flow tension data, and you tend to correlate that with some fairly low resolution fuel motion data, and you end up with a consensus sort of an answer to questions of failure location.

You end up with statements like the top third of the pin, or near the middle of the pin, as opposed to a fairly quantitative set of information that could be used as model verification and development for a code.

We decided that we would look at what was required to develop a program that focused on these key initiation phase issues which determined the accident progression. Of course, the overriding consideration here by far was the fuel motion diagnostics. Without better fuel motion diagnostics than currently exist, the IFFM program simply would not provide significant new data.

We absolutely require to have sufficient resolution of fuel motion diagnostics to address the questions of failure of locations, failure of modes and times. This is the reason that the core reactor energy system is so key to the IEFM program, and I will describe the progress of the core reactor energy system in just a little bit.

As an adjunct to that, the in-core detectors are also going to be included in these tests. In fact, the next PBE test will incorporate the in-core detectors as a first time trial of the fuel motion detection system. The requirements for the IEFM program also include an in-pile sodium loop. This is not a difficult requirement. There is a development program, and we are starting to work on the design of an in-pile sodium loop.

At this stage, and this is primarily due to trying to plan the program well as opposed to jumping into it very quickly, we are going to take the route of developing essentially a test loop that we will be able to use to develop the technology for the in-pile sodium loop.

We have also just completed a review on the ACRR operating modes, what kind of power histories, power levels are required to be able to correctly simulate the LOFTOP AND LOFRTOP Kinds of scenarios.

Let me give you a very brief sketch of the current activities.

As far as the accident delineation study, we went through a fairly extensive review of initiation phase experiments. This was a review that attempted to identify what issues were going to be the most important in the phenomenology, and try to identify tests and test procedures that could address those questions most fruitfully.

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We have also tried to review the test requirements of facility capabilities that exist to perform those tests. That has been one major activity of the IEFM program.

Another aspect of this program is, of course, a recollaboration. As Dr. Walker mentioned, there were two staff members in Caderish, one from Sandia and one from Los Alamos, that provided very close contact. As of yet, that program has not provided a high amount of information for us to digest.

The IEFM program, of course is closely coupled with diagnostic development, and the sodium loop that is under development is the other primary activity which I will mention a little bit later also.

In terms of our review of the existing base, we have gone through the three HE&R top tests, the R&L LOF tests. We have looked at the Hedly/Hut tests. We have also tried to keep on top of what was in the planning for the A&B series tests.

We have tried to use these failure data to correlate what we needed to do in our program in terms of facility requirements. We looked at power levels. We found that the more rapid LOP and TOP tests could be correlated quite well with transient energy definition. The slower tests could be correlated quite well with peak power at failure. We essentially used that to define what kind of requirements we were going to have in the ACRR if we are going to be able to do these tests.

The conclusions were a little bit mixed. We concluded easily that the uranium oxide fueled ACR can provide the energy definition requirements, and that the power history is adequate for higher rate LOF TOP. This is an area that has probably been the most heavily worked.

It does appear, however, that there will be some control systems, some transient modifications required to be able to do the higher rem LOF-D-TOP test, which are somewhat quicker than the high rem TOPs, and the very low rem TOP and LOF tests.

I would also like to mention briefly the status of our core reactor energy system, and just a brief reminder of how this differs from the holascope concept. Here we are attempting to image gamma rays coming from the fuel in a centrally located test in the reactor. The gamma rays from that are columnated through a co-adapter(?). This co-adapter is, by the way, not a fixed design. It is a series of slots, but it can be anything from pin hole to a uniformly redundant array, which is a series of pin holes. They are columnated image on a simulater, which is now a calcium translate (?) device, reflected and taken a picture with high speed cameras, which will eventually frame to 500 frames per second, one looking at the top half of the pin, and one looking at the bottom half of the pin.

This is the concept for the device. This was first put together, and we made a trial run with this system last July. The data from that very first trial was reported on briefly at the last meeting at Albuquerque. All I want to do is to give you a run down on where we have been since then.

From this first test in July, we have since done a second series of full scale tests in the reactor. These were done in April. We call these nuclear system design tests because they were basically designed to look at shielding problems, and similar problems, which seem to be the chief limitation in our resolution that we saw in the first field test.

We have also now scheduled for August of this year our first full-scale PBE test. This will be the new test using the co-adapter system in a full active mode. We are calling this one FD-2. It will be a full capsule PBE test with co-adaptor system working.

The fuel motion diagnostics effort has been coordinated all along with the LASL and Oregon peole. There was a series of meetings several years ago, the last one being in the first part of 1978. Officially, we hope to start up again now that our co-adaptor system is finally running. These are basically information exchanges. That will be a spring activity.

To kind of summarize the current status of RO-2 requirements, what we can do with this system. Right now we are looking at about two millimeters radial resolution, two-and-a-half, with two centimeters of axial resolution -- This should be a joules per square centimeter, and not a joules per gram figure here -- with a sensitivity of approximately 10 joules per square centimeter.

Looking at the image of the pin, you require a fission rate during that frame to register on the film of approximately 10 joules per square centimeter with this kind of frame of 500 pictures per second.

Going to what we have defined as requirements for the IEFM program, it really depends on the test and kind of phenomenology you are looking for. In the IEFM, for instance, for simply axial fuel motion this looks to be very adequate at the current stage for looking at that. Those requirements look more like 50 joules per square centimeter with that kind of resolution being fully adequate. On the other hand, for things such as a precise measurement of the failure location, it looks more like you have to get down into a range of maybe one or one-and-a-half joules per square centimeter.

So in the range of things as we are, we see ourselves able to do some of the more easily image requirements right now, but we are not yet there in terms of the final requirements. However, this sensitivity number, at least from the reconstructions that were done in just the past couple of weeks, seems to be a fairly optimistic number.

This is not loaded by sensitivity in terms of protons at the plane. This is simply a matter of reconstruction algorithm, and I am told that this now can be extrapolated from where we are with additional shielding and reconstruction algorithm with two by one joules per square centimeter per frame, which would put us in the range of the requirements that we are after.

The improvements in radial/axial resolution that we hope to attain here, from what we see are not solely due to reduction in signal of the noise ratios, or improvements in shielding design. The co-adaptor can be designed to obtain better resolution in one direction or the other one depending on how you configure the co-adaptor. So part of this improvement in resolution here, we really could not obtain both of these items at the same time. We would get one or the other, but we would not get both. It would look something like this.

The other major activity in the IEFM program has been the design of our in-pile sodium loop. This is a prerequisite, of course, to doing the IEFM test. I have included all three of the early sketches of our gas driven sodium loop, but I will just go through one of them.

Essentially, we decided for flexibility reasons that we would design sort of an induction pump loop. This is a gas driven loop extending something on the order of 17 feet high, consisting of an initial gas driving system up here, a sodium reservoir here, receiver and supply tank in the test frame.

We did this because it is a self-contained system. It can be easily done and contained. It also provides a considerable amount of flexibility for thermo-hydraulics that one wants to simulate. With this size loop, we simulate about 70 seconds of full power flow for a single pin, which is more than adequate. With the fact action cellonoid belts which are now under test, it looks like you can easily simulate the kind of flow coast downs that are required for the LOF test.

The other pictures are in the room. I will not go through them in the interest of time.

There is one other program that I would like to mention here. We did mention this briefly last year, but we did not talk about this particular set of experiments in any detail, and I would like to just inform you that these are coming out here very shortly .

These are our transition phase experiments.

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Obviously, in conjunction with the other work that is going that is sponsored by NRC at Brookhaven National Labs, and discussions with the Germans in terms of the transition phase, it is fairly well accepted that two major phenomenological issues are fuel/steel freezing and streaming behavior, the question of whether you do actually get to the bottom of that core, and in the boiling pool behavior in terms of the hydrodynamics of this pool with defined ramp(?) rates once you are in it.

We had defined last year, and are now in the process of getting ready to do some fuel freezing and streaming experiments in the ACR. These are going to have the purpose of examining the melt penetration and destruction as a function of the variable parameters -- the parameters being basically the driving pressure of driving this molten material through the structure, the melt temperature, the sensible heat that is actually in the molten fuel, the composition of that molten steel in combination with fuel. All temperatures, or destruction temperatures in the geometry, whether you are going to two geometries, or 10 geometries.

You are doing this in-piles. These are going to be small scale experiments in the ACR. The advantage of doing them in pile is that it gives a fairly good handle on controlling the variables, and you do not have to deal high fraction that would be typical in the thermo test. You can also very quickly reach a melt temperature of virtually anything you would like.

By doing this very quickly in the ACR, we do not have the problems of very long term containment of up to about 4000 degrees in melt.

MR. SHEWMON: Do you know anybody who teaches foundry?

DR. PICKARD: Teaches foundry?

MR. SHEWMON: What describes the validity test in casting metal in the mold, you might look into the analysis. What they do is they have a driving pressure, a melt temperature, a wall temperature, a melt composition, and then they decided how far it runs. Then they define is as validity.

DR. PICKARD: Of course in these tests we are trying to use the decoder tipping materials as well in this case.

MR. SHEWMON: In universities we usually deal with the movement device.

(Laughter.)

DR. PICKARD: This is a very poor sketch of what this test looks like. This is ACR core. What this test consists of, essentially, about a 10 centimeter high, about one centimeter in diameter fuel stack of fresh, nominally 10 percent original material. Triggered with that is a very high pressure gas system capable of about 1600 PSI. They can go through this very fast action cellonoid belt. The sequence will be a pulse reactor free of melt here, and provide pressure pulse before that, and look at the penetration as a function of the test conditions.

These initial tests are going to be limited just from safety considerations, since these are the first scoping kinds of tests that we have done of this type, to all temperature that go from about 400 to 800 --- We are going to limit so that we can make sure that we have a handle on the driving pressures and the amount of fuel pressure will allow here. So we are going to limit this first series of pilot tests to 3700 or 3800 C. There will b pressure limits of something like 1000 PSI in the first series of tests.

DR. KERR: You have a fairly good idea of where you are going, so that when you get there you will know you are there?

DR. PICKARD: The data, as I hear, is penetration in the structure as a function of the basic parameters of the test.

DR. KERR: I am not asking you to give me enough information so that I will know the answer to the question I am asking. But do you feel at this point that you have the

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problem well enough defined so that you know what you need to do, and you will know that when you have done it you are finished?

DR. PICKARD: I think in the fuel freezing and stream test, I think we do know what we are after. We are trying to arrive at analytically model experiments that we can use in things like SIMMER, or heat transfer for dynamic in-transfer maintenance.

DR. KERR: Do you look at this as a one-year program, or a two-year program?

DR. PICKARD: Obviously, since we have not done the first test yet --

DR. KERR: Experiments don't always work, but how do you view it at this point?

DR. PICKARD: What we have planned at this time, of course, is a series of 10 tests which will run through FY-81. This will encompass the range of parameters we feel are relevant from the UO_2. But there is a series of tests that have to be done as a function of melt composition with steel in the mixture. We want to look at pin geometries. These are very small scale tests.

If you look at the situation, you may find reasons to think that your boundary conditions are sufficiently poor that you find that you need the larger scale tests, which become much more difficult.

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DR. KERR: You are giving me the answer to a much more sophisticated question than I have asked.

You said that you had some idea of the kinds of things that were needed for some verification. I am trying to find out whether you look on this as a one-year program, a five-year program, or a 10-program.

DR. PICKARD: As part of the transition phase, I think that you have to look on this as a long-term effort.

DR. KERR: Is the long-term 10 years?

DR. PICKARD: No, five years for the program.

But these experiments in this form I don't think will run anything like that. I think they will be running two or so years.

DR. KERR: So you picked out those that you think are most vitally needed to do first.

DR. PICKARD: Absolutely.

DR. KERR: But if you really answer the questions that you think some people would like answers to, it will be more than that, maybe five years.

DR. PICKARD: Somebody else may have a better number, but that would be my guess. The transition phase is not the easiest area to do experiments in.

DR. KERR: If someone is trying to model transition phases, and they need some data, you are turning out the data for them, I think.

DR. PICKARD: That is right.

DR. CATTON: What is the diameter; is it one fuel pin?

DR. PICKARD: The initial experiments will be done by three millimeter. The length of this, not being to scale here, is about a meter and a quarter, I believe.

DR. SHEWMON: Three millimeters, did you say?

DR. PICKARD: Three millimeter diameter freezing tube, yes.

DR. CATTON: I don't think that that would represent your foundries very well.

DR. SHEWMON: Sixteen hundred PSI does not normally represent the head of the cast iron core. I think a different question is what three millimeters is like when it is simulated inside a real fast reactor core. It is not clear to me just what dimension that is.

DR. PICKARD: Bill has been more involved in this than I, but this looks like a pinched handled.

DR. CAMP: I believe that that is a little bit narrow, which we did for the first three tests on purpose. But that is not grossly different from the kind of hydraulic diameter you are talking about. The reactor is composed of a series of very small hydraulic diameters. It is not until you get the complete sub-assembly construction that you have much bigger diameters.

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In the upper-core structure, which is what we are addressing here, you want to look at the single channel experiments, or making a few channel experiments with very narrow channels. It is the very narrowness of these channels that makes the plugging likely in the structure.

DR. SHEWMON: This is a channel such as where the sodium flows in an FFTF sub-assembly.

DR. CAMP: In the actual pin structure in the reactor that one worries about. It was not clear from the talk by Bill Bohl. But the fact that the plugging question arises, is that the fuel stream is at the break location in the fuel pin, it is driven up by gas pressure to the upper core region.

DR. SHEWMON: Let me stop you. I have one simple question about that, and then you can let him talk again.

The fuel rods in the FFTF sub-assembly are on the order of not quite a centimeter, is my guess. Now you are saying that the channels are appreciably less than the fuel pin diameter.

DR. CAMP: Yes.

DR. SHEWMON: Thank you.

DR. PICKARD: These experiments, by the way, are scheduled. The first one will be on the first of September nominally this year. We would hope to get the first two experiments off in this FY, and the second three experiments in this series off in the fall of 1981.

DR. CATTON: What is the maximum diameter that you can look at?

DR. PICKARD: The flow channel -- the freezing channel?

DR. CATTON: Yes.

DR. PICKARD: Three millimeters. Obviously, we are limited by the amount of fuel that we can put in the reactor.

DR. CATTON: I would think that you would want to look at several fuel pins.

DR. PICKARD: Several fuel pins in terms of the amount of fuel?

DR. CAMP: That is probably beyond the scope of the budget.

DR. CATTON: It was not really a question. I was just curious.

DR. SHEWMON: As long as they have got a sub-assembly up there, then you have got more fuel, but it all goes down to a channel of this dimension. So until you have blown the head off of this assembly, why can't you talk about what happens when it gets into the channels between the solid?

DR. CAMP: Just very briefly. I agree with you. The single channel effect is the most important. We did plan to look at the question of flow bypass, to look at seven mid-channels (inaudible).

DR. PICKARD: Let me just finish this by summarizing.

This is kind of a summary of what we have been doing here in terms of the in-pile test up until this current time, the remaining part of this FY, and what we have at least planned for the first half for '81.

We look at these tests, and we see there is one (unintelligible) in terms of the first irradiated tests for the PBE. There is something like -- at the current time, including the fuel motion detection development, the HRR, including the UK collaborative efforts, and preliminary tests for equation of signal, there are like a dozen tests here, about half irradiated tests, and there are about dozen tests here in the second half of the year, about half of them being irradiated tests.

If this test series can come off, this is like 25 tests, two thirds of which are irradiated test. This is a very ambitious program, and we feel that at the stage of the freezing lie most of the relevant questions.

I think that this concludes my presentation. Bill Camp will be talking about some of the results and analyses from these programs.

DR. KERR: Do you think that some of the people understand what you are doing?

DR. PICKARD: That is a question for them.

DR. KERR: It is not a question for them altogether. It is a question for us. You are developing data to be used by them, I thought you said. Therefore, it would be nice if they understood what you were doing there, and agree that, indeed, the results of these experiments might be useful to them.

DR. PICKARD: There is certainly a sufficient amount of discussion between those two labs.

DR. KERR: I don't know how labs discuss things. I am talking about the people.

(Laughter.)

DR. PICKARD: People on the telephone.

DR. KERR: You do talk to them, and they agree that they understand what you are doing, and they think that it makes sense.

DR. PICKARD: I hate to answer for them. I believe they do, but that is because I do. I am not sure if everybody at Los Alamos agrees with everything we do.

DR. MARK: To put the question slightly differently. Are they interested in your results?

DR. PICKARD: I think certainly they are.

DR. MARK: Do they come down and ask to see them, and talk about them?

DR. PICKARD: They have not in the last two days. But they are interested in these results. They have to be. These are the only places where these kinds of results are being generated at the moment.

DR. KELBER: I might say that in addition to that there is also communication with DOE, because they are planning some extensive tests, and some that will follow on to the type of transition phase experiments that are being done here. So there is a fair exchange of test technology as well as results.

DR. KERR: I guess I have one additional question. Are you familiar with the aluminum validity test being done at Ohio State?

(Laughter.)

DR. SHEWMON: The device will possibly tell you the principle of heat transfer is universal.

DR. CATTON: Since SIMMER doesn't use radiation, the temperature really does not matter.

(Laughter.)

MR. CAMP: My name is Bill Camp, and I am supervisor of Reactor Safety Physics Division at Sandia. I will be talking in a little more detail about three sub-sets of the accident energetics program which Paul Pickard has described for you.

The first one is the prompt burst energetics and work potential program. The second one, very briefly, is the effective equation of state of fuel and core materials program. The third one is the fuel disruption experiment in model development programs at Sandia.

I have a short film, which I will show at the end if time permits, it is five minutes. If we don't have time, I am perfectly willing to skip that film, although Mr. Walker may kill me for doing so.

DR. CARBON: Dr. Camp, what kind of time are you talking about for your presentation here? We have let this earlier part here run way over.

DR. CAMP: I am prepared to be as brief as you would like me to be, 15 to 20 minutes. I have been known to speak faster than most people can listen.

DR. CARBON: Why don't you go with 15 to 20 minutes in mind, and see how you come out.

DR. CAMP: Very good.

The first area that I am going to talk about is the prompt burst energetics area of our program, known as the PBE area. Paul has already talked a little bit about that.

The reason for doing this has been the need to characterize what are the pressure sources that can derive potential failure of the primary vessel -- first of all, destruction of the core itself, and misgeneration failure of the primary vessel through either pressure generation or misgeneration. The two candidates are fuel vapor, and fuel/coolant interaction leading to coolant vapor, and a very small burn which is at the core region, but not elsewhere, of fission products.

We have done something like 20 prompt burst experiments or PBE experiments which are aimed at investigating this work potential within the core barrel region itself, not following explosion within the core barrel region.

In addition to looking at the work potential itself during prompt burst, these are pin experiments which allow us to look at reactivity effects. That is, for example, axial fuel motion for sloping within the channel, which you need to be negative or positive reactivity in the fission rate, and the pin failure mechanisms which are operant under high ramp rate TOPs for the prompt burst situation.

We have developed analytical models in this area that helped to verify other models. The pin failure model experiment had been developed for high ramp rate transient power failures at Sandia. The LAPFIN (?) model has been developed at Los Alamos. LAPFIN has been specialized to irradiated fuel in pin failures, and expanded fresh fuel pin failures.

The failure situation being sufficiently non-universal that it was profitable to follow two routes for pin failure in this situation, one for fresh fuel and the other for irradiated fuel.

The FCI modeling efforts, to date we have developed phenomenological models based on mainly fragmentation experimentation in the fast reactor safety program in England and in the United States, and in programs involving fragmentation of liquid droplets by gases, which has been on the program at Sandia and elsewhere.

Finally, another area that we have been looking at is the fuel equation of state area, and I will get into that in a little bit, both fresh oxide fuels and irradiated oxide fuels. We have not yet looked at the question of what is the effective equation of state of pressure source due to intimate mixtures of fuel and stainless which is clearly a very important question for reactor materials.

Very briefly because Paul has been over this. The recent results for the PBE series are PBE-13S and 14S, both of which have been done in the last few months. PBE-13S led to a late failure. This is a failure well after the prompt burst occurred. We can characterize this experiment by saying that we saw a series of small scale FCI following initial failure.

The reason for the late failure was that in going from the beginning of PBE series, we switched from a doubly constrained fuel pin to a singly constrained fuel pin to be more prophetific of what occurs in most real reactors. It turns out that has a significant effect on how fuel can spill. It changes the Von Meeses(?) effects of stress, and beclouds these, and affects the time by as much as several milliseconds. Experiments which are usually failing during the pulse now are failing later.

In 14S we verified this effect by going up to a much higher ramp rate, leading to about 4000 joules per gram delivered to the pin. This led to failure during the prompt transient. We saw a rather large initial transient pressure, followed by a second massive transient which was due to fuel/coolant interaction.

Again, this is just a point that pin failure is strongly influenced by the axial restraint on the pin, and generally speaking singly restrained pins, those that are only restrained at the bottom, lead to significantly greater failures than doubly restrained pins.

The conclusion that one can draw from the whole oxide and carbine series of PBE program is that FCIs are very important geometries during prompt burst situations.

The energy conversion from fuel to sodium vapors is comparable for oxide and carbide fuels, which may come as a surprise since carbide fuels have generally appeared to

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give FCIs much more spectacularly. But when you actually look at the conversion efficiencies, they turn out to be roughly comparable for the two systems.

DR. CARBON: Which ones are the more spectacular?

DR. CAMP: The carbide FCIs.

Future experiments in this program -- We are now winding down the prompt burst energetics program. This is an example of the program where we feel that we are reaching an end, that it is not going to go on forever. We have three experiments for fresh fuels left, which we think will finish the program off, and then two irradiated experiments.

The 15S and 16S are two experiments which are aimed at getting us very good fuel temperature profiles by using double pulses: a pre-heat pulse which will give us a mid-parabolic temperature profile on the fuel, and then a second pulse which will the raise the surface because we have a epithermal driver core, and we always tend to put more energy at the surface than the interior.

The effect of the first pulse is to prevent fuel vapor generation at the surface in preference to the interior. With the double pulse we will get fuel vapor pressure at the interior, driving the fuel outward the way we would like to see it, and duplicating an accident situation.

End 2

We are going to have reduced heat losses because we are using nuclear heated walls. What we're doing is using thin molybdenum tubing, two pieces of it, which are filled with low enrichment density uranium. And this is being done in such a manner that during the prompt burst the nuclear heated walls essentially follow the temperature of the clad due to normal heat transfer between the hot fuel in the clad. So that we no longer have cold walls; we have prototypic wall temperatures in these experiments.

It's a rather tricky thing, and I'm impressed that the manufacturing people have been able to put these things together for us, but they have been.

The other thing will do between 15S and 16S is look at the effect of subcooling. 15S will be done with a normal 500 degree C. sodium; 16S will use 700 degree sodium, which means that we have to use standoffs for the pressure transducers to avoid ruining since they can't stand up to temperatures much in excess of 500 degrees. These are both fresh oxide tests.

17S, which is the final fresh test, fresh oxide test, is aimed at putting the absolute sign of confidence on our claims that most of the energy that we're seeing in these experiments is due to sodium vapor pressure. We're going to do it with a tin coolant, which most of you will know that tin is very hard to boil. I think it boils at 2300 degrees C. or something like that. The point being that it's a good metal with a high thermal conductivity, properties very similar to sodium except it's much more corrosive, obviously; but it doesn't boil, and therefore you'll not get any coolant vapor pressure, and we can verify exactly what portion of the work potential in these experiments is due to fuel vapor pressure, because we just won't be seeing it in coolant vapor pressure.

And finally, the two irradiated experiments to be done early in '81 are to examine the effect of fission products, and particularly xenon and crypton gases and cesium, which are expected to be released in copious quantities during the prompt burst, on the results of the fresh test. And again, they'll look very much like 15 and 16 as to the fresh profiles. And that will be with P&L fuel -- I think it's P&L through to fairly gassy fuel.

This is a quick rundown on the expand pen model which we developed. I think I told you about it before, so I'll just skip it.

Let me just sum up the PDE program data for you with, I think, the bottom lines that are coming out of the program are. We've done something like 20 tests involving oxide fuels, fresh oxide fuels, and fresh carbide fuels. We have done it with voided channels and the sodium in it.

In terms of the work potential, in a voided core the work potential is very low generally, and that's basically because hot fuel vapor will condense out on nearly anything, okay; and you just don't get much work potential out of a voided core. With sodium in you now have a much smaller surface area for the fuel vapor, so the fuel vapor can act as a working fluid. Nonetheless, we believe that -- our instrumentation tells us that fuel vapor is being dominated by sodium vapor in this case, and that most of the work potential, even in that case, does not do the fuel vapor, do the sodium vapor.

In addition, under some conditions we've seen evidence that massive heat transfer can occur from the fuel to the sodium, leading to very energetic fuel-coolant interactions or vapor explosions involving rapid vaporization of the sodium.

Furthermore, we've seen evidence in a number of experiments that these interactions appear to be triggered by some external event; for example, the piston that we used for diagnosing the amount of energy, kinetic energy developed by the experiment will come slamming to a stop against its stop in the channel. That will create a minor shock wave, let's say 10 atmospheres to 30 atmospheres, into the system, will collapse the bubbles; we think it collapses the vapor bubbles around the fuel, whatever. The shock wave is enough to set off a vapor explosion in the sodium.

These conclusions hold for both fresh uranium carbide and uranium dioxide fuels.

I should point out that there is one conclusion missing here, and that's the conclusion on the reactivity effects of axial fuel motion. That conclusion is being held up because while we have some thoughts on it, we have no real time diagnostics, and the last three tests in the series will provide real time diagnostics on axial fuel motion and help us to come up with more conclusive answers.

DR. CARBON: Do your FCI results confirm or in any way -- are they affected by Bousky's theories? How do your results differ with his theories?

DR. CAMP: As you know, that is a very controversial area. Most people in the world today, with the exception of Bob Henry and Hans Bousky, believe there is -- although that Bousky was correct, that there is a threshold interface temperature that has nothing to do with spontaneous nucleation, rather has to do with the ability to support a thin vapor film around a hot fuel particle, and then the collapse of that, the subsequent collapse of that vapor film.

I would say that our experiments confirm that model. I would hesitate to speak about Bousky's model. I think he still holds to it. But ours are certainly consistent with the film collapse model which is accepted by the majority of the people in the world.

Incidentally, the minimum film boiling temperature is very close to the spontaneous nucleation temperature; so as a matter of fact, maybe the answer is yes.

Because we found this potential for energetic vapor explosion in both reactor fuels of interest, we have decided that this is an area where we really need to delineate the kind of energetics that one can get under worst possible conditions for reactor fuels, and started a series of experiments called the coarse predispersed mixture experiments, which basically are to look at the fragmentation and propagation of an FCI once you have already mixed it. There are three stages in a vapor explosion -- the premixing, the fragmentation, and the propagation stage.

This experiment assumes the premixing, which incidentally, would be quite difficult to attain, I think, with reactor materials, but granted that you attained it, what kind of an FCI can you get under ideal conditions.

What we're looking at is a series of particles which have been dropped into a sodium-filled tube in the ACRR core. It's a one-dimensional geometry deliberately designed to be that way with shock wave codes so we really know what we're doing, with a series of pressure transducers axially along the tubes that watch the propagation of pressure waves, and either their amplification or deamplification with distance, and involving an external mechanical trigger, probably an explosure trigger, which we can either use or not use depending on what we want to do with that experiment. And we have a parameter matrix which we've worked out there.

The original experiment, the one we'll do this year, has 100 grams of oxide fuel which leads to roughly equal volume ratios -- I'm sorry -- a 10 to 1 volume ratio -- no. Ten to 1 mass ratio, equal volume ratios, fuel to sodium. I'm sorry about that.

And again, the things that one would want to vary in this are the particle size, the volume ratios of fuel to coolant, the initial fuel temperature that you achieve during your burst, the coolant temperature, the degree of inertial constraint, the degree of system overpressure; because it's clear to a thermodynamicist that if the film boiling model is correct, the system overpressure is the key parameter. With a high enough pressure you'd never see it in a CI. Any pressure will tend to stiffen the films up and make FCI's more difficult to occur. Clearly, if the pressure is high enough, you can suppress all film boiling and probably never lead to significant vapor explosion. So the effect of significant overpressure is certainly something one wants to look at, and again, the question of external trigger.

That experiment was meant to go off during September of '80. I think that's overly optimistic. We have only now ordered the pressure transducers for the experiment. The fuel release mechanism is under design now. I think we've come up with a final choice of designs. We haven't finalized the design.

The piston diagnostic, we have not done enough pretest calculations to make sure of exactly what kind of piston diagnostic we need in this system. In other words, we haven't done the upper bound calculations to make sure we have enough travel in our piston that we can measure all the work that's done during the experiment.

And the types of external trigger mechanisms that we want to use, exploding wire or other things explosive --

DR. KERR: You do this in order to heat the particles?

DR. CAMP: Yes. One wants to heat the particles and heat them rapidly, and in that regard the ACRR is a unique tool because it can get almost all the heat in about a 7 millisecond period. And we've done quite detailed film boiling calculations so that we now that, a) we will have liquid fuel; b) that we will have thin films that will not boil all the sodium away, but yet you will have a vapor film around it. So that it appears that we are producing what Board, and Hall, and Theothonus and just about everybody in the world concedes to be the correct set of initial conditions to get an FCI, if one's ever going to go with these.

With regard to that, I'd like to say that I consider it somewhat ironic that the fast reactor field were really worried about FCI's because they might fail the primary vessel, but in the water reactor field we've just decided that FCI's aren't a terribly important problem, because even though they'll probably fill the primary vessel, they're not going to blow a hole in the containment.

I think there's quite a difference in the degree of conservatism between the two types of reactors.

Yes.

DR. KERR: I didn't know that one was concerned about the FCI because it would fail the primary vessel, but --

DR. CAMP: I think that -- well, there are two things it can do. One of them, it can fail the primary vessel directly. The second is it can lead to high ramp rate reactivity insertions.

DR. KERR: That's what I thought was the problem.

DR. CAMP: Either of those. But my point is that it's a moot question as to whether it can even fail the primary vessel.

DR. KELBER: But if you do fail the primary vessel and start a spray sodium problem, you've got a problem.

DR. CAMP: Yes, correct. You could fail the containment, yes.

Question?

DR. SHEWMON: Yes. I guess maybe I'm getting into class 9 again, because I'm not sure what my coolant is.

(Laughter.)

DR. SHEWMON: But if we've got particles that we dump in a fluid, you just got done saying again that if you've got an overpressure, you're not so likely to develop a lot of steam pressure or vapor pressure or something. And that seems to be accepted because several people say it.

On the other hand, the problem at the end of the morning where the gentleman had a piston behind this stuff and he inhibited the expansion of the water-particulate steam mixture, he got the higher steady pressures because his piston couldn't get out of the way.

DR. CAMP: That's inertial confinement. That's a different thing from an ambient pressure, and I guess the point there, Dr. Shewmon, is when the pressure combines. Inertial confinement with no hydrostatic pressure does not prevent you from growing thin films slowly around the particles.

What I'm saying is that thermodynamically one can look at the phase diagram, look at the spinodals and various other things, and decide that there's no damn way in hell I'm ever going to get a thin film if I, you know, exceed a certain pressure, which I can calculate based, say, on your favorite equation of state, whether it be Vanderbalt or something more sophisticated, and tell you the regions under which I'm going to get a thin film and not going to get a thin film, or also the fact, which has been noticed by other people, that if you do up the pressure, even if you have the film, your films become more rigid or stiff mechanically and therefore harder to collapse.

DR. SHEWMON: I don't know what the critical pressure of sodium is, but at least the critical --

DR. CAMP: It's fairly low.

DR. SHEWMON: -- The critical temperature of water is 370 degrees Centigrade or something.

DR. CAMP: That's right.

DR. SHEWMON: And your fuel is hotter than 370 degrees Centigrade; otherwise you'd model --

DR. CAMP: But the interface temperature is what's important.

DR. SHEWMON: Well, but if you've just got water there, don't care what the interface temperature is, you've got a large amount of heat, and it'll ultimately heat it above the critical temperature.

DR. CAMP: Oh, you're right.

DR. SHEWMON: So why is it that sometimes we ignore the pressure expanding? In your case it can't do any work; this morning it could do a lot of work.

DR. CAMP: No, no, no. I think we're talking at cross purposes. This will generate a hell of a lot of vapor pressure, if you've got enough heat in the fuel, by, you know, just slow heating, overpressuring, whether you want to call it boiling because it's two phase or just heatup and expansion because you're above the critical temperature and you never go through the two-phase dome, you can still generate a hell of a lot of pressure rather gradually. And by the standards I'm talking about, those SIMMER calculations were rather gradual.

But if one is talking about a classic FCI of the type that Board and Hall had worried about and the type that other people in this field have worried about that occurs in the millisecond time scale, then one needs to generate those pressures very rapidly, and the classic mechanism for doing that is to grow a vapor film around each little particle of fuel and collapse that very rapidly, leading to an exponential increase in area over a very short time. And that's what I'm saying can occur if you've got a high enough system pressure and can never grow that vapor film.

DR. SHEWMON: I'm sorry. What collapsed in a millisecond?

DR. CAMP: The coolant vapor film.

DR. SHEWMON: And how does that -- that gives an exponential increase?

DR. CAMP: Oh, yes, because it shatters the particle. Either asymmetric collapse, which is known as a jet collapse, or a symmetric collapse of the type proposed by Brumheller, which then leads to very high rebound pressures which drive the particle apart. You basically get water hammer down on the particles. That has a one over hour inversion, and when it comes back, the relief wave just tears the particle apart and creates a heck of a lot more surface area.

DR. SHEWMON: Okay. And then when it comes in the second time, have you got enough pressure to collapse it again or what?

DR. CAMP: Well, at that stage you've generated so much surface that, you know, just the boiling calculations you'll do then give you an enormous amount of pressure. And that will propagate down to the next series of particles as a shock wave. You know, it's very much a chemical detonation wave.

DR. SHEWMON: Okay. Well, that's enough. We're raising heck with our Chairman's schedule.

DR. CAMP: I'd like to go on now to vapor pressure and show you the current situation with the equation of state, and tell you what we're up to in this area. This is a plot of vapor pressure versus 1/T, and basically this is the world data based on -- these are German experiments at KFK based on laser heating of open systems. These are extrapolations from load temperatures, and this is a series of several experiments which are done using either electron beam heating or closed capsule reactor heating. And these error vanes on the reactor experiments have been narrowed somewhat so that they look much closer.

The thing to notice is that there's roughly an order of magnitude difference between the closed system-high temperature experiments --

DR. PLESSET: What's this the vapor pressure of?

DR. CAMP: Uranium dioxide, fresh uranium dioxide.

We are currently designing a series of experiments in collaboration with KFK, half paid for by NRC and half paid for by KFK, to try to do a series of reactor experiments with very narrow uncertainty bands to try to understand the cause of this difference.

Part of the difference is clearly due to the fact that in the closed system one samples the pressure due to contaminant gases that are built into the fuel when you make it, that are prototypic parts of the fuel, whereas in these open system laser experiments, those contaminant gases get out of your way and you never measure that. So that's one difference.

And incidentally, from the point of view of reactor safety, it is then the higher pressure which is of interest, because for a reactor, the containment gases don't get out of your way. They are in the core belt region as a working fluid.

The test matrix that we're -- well, we're doing a series of, I think, a dozen tests -- fresh UO_2; Sandia has sponsored one test, KFK has sponsored two, one will be done in the ACRR, four are sponsored by us on fresh uranium carbide, three by us on irradiated uranium sodium dioxide, and two on fresh mixed oxides by the KFK people.

And with this series of tests we plan to close out our fuel testing program for equation of state with the possible exception we may go back and do some fuel-steel mixtures to try to understand effective pressure sources from those systems.

DR. KERR: I don't understand your comment about the one you're interested in is the one with the constituent gases. I agree that there are going to be some constituent gases in the fuel, but it seems to me you would want to know the equation of state for the fuel separately.

DR. CAMP: Oh, yes. What I'm saying is that in -- our experiments are designed to have roughly the same amount of void fraction in them as one would have in a reactor core during accident conditions, roughly 30 percent void fraction for the contaminant gases to expand into. There is no way in the world that we can get away from those contaminant gases if we use reactor grade fuels. It isn't clear that we can obtain enough uranium dioxide by any other means to do these experiments. And since we do closed experiments -- that is, we're actually measuring pressurization of a closed volume -- we cannot avoid measuring the pressure contribution from the accompanying contaminants in the manufacturing process.

My point simply was that that's roughly the same order of magnitude that one would expect the contribution to be during an accident in a reactor core, because it's pumping up roughly the same volume. It's an ideal gas for all practical purposes.

I'd like to switch to my final topic which is the series of fuel disruption experiments which have been carried out and are currently being carried out at Sandia. FD-1 I reported on last year. That's a completed set. It was a multi-pulse set of scoping experiments.

The key thing that came out of that was an unexpected potential for gross fuel swelling during loss of flow conditions. You will recall -- some of you will; I'm not sure if all of you have heard this -- what we're looking at with the fuel disruption experiments is visually observing the meltoff of clad or other wise failure clad, and the disruption of fuel under loss of flow conditions in a reactor channel.

And the way we're doing that is to take segments of fuel pins in gaps and keep them up in the reactor under LOF type conditions.

FD-2, which is now in progress, involves slow rate heating at LOF typical power levels. Well, for FD-2 we're using three or four kinds of nominal power and CRVR as being typical of LOF heating levels. We're using ultraclean fuel in the sense that there is no contaminants in that fuel other than those that were put in during manufacture. It will be going into an ultra-clean glove box, Argonne glove box, with, I think, a few parts per million reactive gases in there for cutting and handling of this fuel prior to the experiment.

We're looking at the variation of burnup and linear power rating, and I have a film of an energetic dispersal that was seen during FD-2.4, which was an LOF type heating at about ten times nominal power roughly equivalent, rather than three or four.

FD-3, which is also known as high ramp rate, is visual observation of fuel disruption, irradiated fuel disruption under prompt burst conditions. The idea here being that if any fission gases and fuel are strongly coupled to the fuel itself, they might be able to disrupt the fuel in the solid state or liquid state well before fuel pressure would disrupt it. This would have the effect of turning a major transient into a rather mild transient.

And we've done a series of four of those experiments. I have films of the significant parts of three of them. The total film for FD-2.4 and this one will be about five minutes, if you want to see it.

Finally, FD-4, we have a finalized test matrix with the West Germans, and this series of tests is being paid for by the West Germans, and its use of advanced modes to really simulate LOF conditions and LOF-driven TOP conditions in the SNR-300 or an advanced American core.

And FD-5 is currently just in the planning stages, and that will be a lightwater reactor series of tests.

Model development that's come out of this, because the bottom line of all these things is to come up with models that we can use in acts of analysis, we've developed two fission gas behavior models which -- running out of time?

DR. CARBON: Right.

DR. CAMP: This gas simply handles gas release and swelling. TIGRES(?) handles the same models of fiss gas in a similar manner and also treats fuel dispersal by crack linkup and breakup.

And I'll skip the FD-2 test matrix, and I think I can skip that. For the HRR series which we are currently doing with the British I want to briefly tell you about the crack model that is in this. This is a generalization. It's a qualitative idea that DeMelti at Argonne had a number of years ago. Dave Werlich from the UK, who spent two years with us as an attache for the TIGRES as a quantitative model.

Basically you're looking at a growing fission gas bubble within a grainer, on a grain boundary in irradiated fuel, and you are looking at the growth penny-shaped cracks, which is what post-test analysis of some of these things tell us are the kind of the things that grow to link up bubbles. What you are trying to see is whether you can get these penny-shaped cracks or circumferential cracks around the bubbles to grow out till they link up with each other, and therefore provide for a means of basically separating the fuel into well-defined small chunks, to turn it into a dust cloud of whatever.

Basically, the mechanism -- you are looking at a ductile fracture, plastic situation, and vacancy motion and creep are the main things that are opposing the cracking and surface energies. There are a number of criteria, two of which are always met and therefore trivial, a stress criteria and a differential or another force criteria, which is the gas pressure has to exceed the forces imposed by surface energy under the crack.

Two two that are non-trivial are total energy criteria, which is basically do you have to have enough energy in that gas within the bubble to create the new surface required to crack the fuel, and another one is that you outrun crack healing. That is, that the time rate of change of the gas pressure is always greater than equal to zero, so you don't start to shrink your bubbles; and that the crack propagation velocity exceeds the ability of vacancies to flow from the crack tip.

We did a series of calculations with the model and came up with predictions for how experiments would behave. Basically, Green says that the fuel wil disrupt by dust cloud breakup. This is time and fuel temperature. You do a heatup, a run at constant temperature. It will look like a loss of fuel situation, and another steep heatup representing a prompt burst.

The thing that you discover is that the temperature of creep at heatup is what is important, not the length of time at heatup. So we chose that to melt off the clad to get it out of our way. As you can see, you get above a certain preheat temperature and everything will disrupt. Below that, nothing will, and it doesn't depend on how long you hold. It is true for short holdups, shown by this, or long holdups, shown by that.

Finally, this is the result of the calculation with a very preliminary model. In fact, I think it has plenty of untrustworthy features. I don't want you to get the feeling that this is what we say is the way the fuel really behaves. The experiments are designed to test the models.

This is the actual histories that we used in the test that you are going to see, HRB 1 and 2, and HPP 3 and 4.

The FD-4 test matrix, as I said, is a test matrix with the Germans and is really not of too much interest. If you would like to see the film, I would be glad to show it right now if you think you have five minutes to see it.

DR. CARBON: Let's hold off on seeing the film right now. Perhaps at the end --

DR. CAMP: Well, I won't be here at the end, so I will just take it off now.

VOICE: That's fine, Bill. Leave it on, and I will bring it back.

DR. CAMP: Okay.

DR. CARBON: This apparently is the opportunity you spoke of this morning to talk about accident (unintelligible word), is that correct? I would like to go back to Dr. Kelber's comment this morning that he had a lot of confidence in the SIMMER calculation for CRBR instead of 108, 105 megajoules, that it was truly only about 8. Another order of magnitude in there, of course, would be extremely important, I would believe, in many fast reactor safety considerations.

I don't think that most of the safety community would put too much into or give too much credence to use of SIMMER in relying on a lower energy release at this time. Charlie might since has confidence.

What can one do, if anything, to really get confidence that the entire safety community would join here.

DR. KELBER: Let's divide the problem in two parts. You recall from one of this morning's graphs on SIMMER that we simply take that particular inde: in which the 105 megajoule loading on a head corresponds, roughly speaking, to the regulatory figure of 1200 megajoules to complete expansion to atmosphere, for example.

The statement was that what were essentially effects of fluid dynamics -- and this is non-isotropic nature of the expansion, and the effect of expanding through the upper internal structure accounted for a reduction from the nominal 105 down to, if I recall correctly, about 20 --

DR. CARBON: Fluid dynamics took it down to 20.

DR. KELBER: To 20 megajoules.

DR. CARBON: Heat transfer took it down to 80.

DR. KELBER: Now, that type of factor of 5 is, so far as we can tell, well verified by the small scale and even the large-scale but low pressure experiments done at Purdue, with one concern, and that is that the interaction between the expanding gas and the structure has not been followed through in detail. Let me expand on that a little bit.

DR. KERR: Let me see if I understand what you mean by verified by the experiment at Purdue. Do you mean SIMMER apparently will describe the Purdue experiment --

DR. KELBER: Yes.

DR. KERR: And hence that gives you confidence that it can describe --

DR. KELBER: It gives me confidence that it handles the fluid dynamics and also the SRI experiments. It handles those correctly.

DR. KERR: I understand.

DR. KELBER: Now, one problem that was raised is that as these gasses expand and do work on the structure, they may cause the structure to deform and perhaps deform plastically, in which case, as the process goes on you have a changing flow resistance and a changing load on the top structure. This is correct and it is not clear at the present time what the net balance is. Rather, the energy absorbed in the structure in producing that deformation compensates for any increased load on the top structure that might cause failure of the top plug.

That is an incomplete analysis, but I do believe that the folks at Argon who are doing a considerable amount of work in this area have begun now to take a look at this problem. They are, of course, aware of the results.

DR. SHEWMON: That plastic deformation cools the gas.

DR. KELBER: Yes. The question is does it also result in a higher mechanical loading of the restraints on motion of the top plug. Does it, for example, cause greater elongation of the hold-down bolts. That is a very complex mechanical problem yet to be analyzed, so there is a reservation on that.

Now, there is no question in my mind that there is effective heat transfer to the structure, and that has, we know for many years in principal, that that should have, and SIMMER gives us some idea of the quantity of energy that might be removed in that way. That has a very considerable effect of removing another 12 megajoules from the total, getting you down into the 8 megajoule region.

There are some problems here. If the excursion is a very rapid one, then it is correct the flow regime doesn't make too much difference. You are concerned with heat tranfer that occurs over a very short length of time, and probably an integral experiment is all you are going to be able to use. I don't mean integral in the sense of a large number of pins. A small well-designed experiment may do well. But I mean a type of ballistic experiment where you simply look at the total amount of energy removed.

However, as we can see both from the SIMMER analysis of the transition phase, the accident delineation study and the rest, there is considerable interest in excursions that occur at lower ramp rates and that involve much longer periods. Even though the total energy release in that particular excursion may be less, the accounting for these effects may be more complicated and we may, in fact, have to consider differences in flow regimes.

(Noise of loud telephone ringing.)

REPORTER: Can you repeat that, please?

DR. KELBER: Let me try and summarize it. I don't know that I can repeat the exact words.

There is considerable interest in excursions characterized by lower ramp rates that require a longer time scale. In these cases, the heat transfer may involve different flow regimes and we may have to consider in more detail the nature of the flow regime and its influence on heat transfer.

Now, a difficult subject to consider is the one that Bill Camp has alluded to before. That is the role of the conversion of heat to sodium vapor. The problem is highly interactive. It used to be thought, for example, that you were treating the problem in a conservative way if, indeed, you assumed that most of the energy was transformed into sodium vapor.

When we first started calculations for SIMMER, we thought that would be the case. The answer seems to be that it depends. If, in fact, there is considerable conversion at the top of the core, the core blanket interface, for example, the nature of the interaction appears to be such as to delay the rapid expansion of the gases, allowing for more heat transfer and more cooling en route. But on the other hand, the sodium vapor is a somewhat more efficient working fluid, and the main effect seems to be, therefore, that you can raise the loading from a nominal 8 megajoules, or perhaps the base loading was 4 megajoules for that case -- I don't remember the exact numbers -- to some number considerably higher, say 12 megajoules.

But you could also cases where the reverse is true. I think that is the problem that I see, requiring a fair amount of sophisticated in-pile tests to make sure that this part of the scenario is correctly treated, that we have not missed the point somewhere, that we have not missed a key phenomenon which reverses the tendency of these various effects to compensate one another and perhaps takes us all in one direction.

DR. CARBON: Charlie, I have got several questions yet and I suspect the other people do, too, but maybe we better just stop here and discuss them separately.

DR. KELBER: Whenever you please. I am at your disposal.

DR. CARBON: Unless the committee has another urgent question at the moment, let's take a 15-minute break.

(Brief recess.)

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DR. CARBON: Let us begin.

And will you stick within your allotted time?

DR. COATS: Yes, sir, I'll try to do that.

DR. CARBON: Appreciate it very much.

DR. COATS: Richard Coats, Sandia Labs.

The program that I'll be talking about is our core debris behavior program, which encompasses debris bed cooling.

On your handouts you have a statement of the basic objectives of the program, which is to develop the technology that permit an assessment (WORDS UNINTELLIGIBLE) of core debris following a core-disruptive accident or a general core degradation.

The program is comprehensive in the sense that it then considers the formation of coolable -- or cool -- to be cool geometries all the way through remelt. It has experiment work, analysis, and leading to models. It's international in the sense that we have a pretty heavy involvement with our partners through the exchange agreements, and perhaps in the near future with a joint program with ISPRA. The work with Euratom and Japan would actually involve contribution of staff and money. Our exchange programs are already in place and are active. We have Michelle Schwartz (phonetic) from France present at Sandia working with us in the analysis of the results. Soon we should have Dr. Peak (phonetic) from KFK, who is an experimentalist, will be working in the experimental community.

And we also have related activities in progress at KFK in terms of analysis and out-of-pile experimentation. And we have a relatively decent correspondence with the people in the U.K.

To move on, the regimes that we're interested in in core debris behavior are, of course, the formation, the sub-dryout behavior of debris beds, the dryout behavior, what happens after dryout has started, the post-dryout regime, in beds of steel and UO2 when steel starts to melt how does it migrate, how does this affect the subsequent behavior of the bed, and then UO2 melt and its migration.

If you recall from previous talks, the parameters controlling the behavior of beds are as follows: particle size, shape; particle size distribution; bed geometry; stratification -- we actually observe and experience that we have the larger particles at the bottom of the bed, smaller particles at the top; the presence of steel; and of course the bed depth is a very important parameter; and bed packing.

The cooling that you must concern yourself with that might be available: through flow, meaning you can have entry of coolant from the bottom of the bed, or a degraded section of the core, if you wish to look at it that way; it can either be by natural convection, it can be pump flow, or if you have a bed setting on concrete in the presence of sodium, you have gas coming up from the concrete and assisting or hindering the cooling.

U-flow is the case where you have a restrictive boundary and you only can allow your coolant to come in from above and circulate to the bottom, be generated as sodium vapor, and then up again to the top of the bed.

And in this case you can have a adiabatic lower boundary, or in the case of perhaps in a super Phoenix design for an in-vessel core catcher, have sodium below that steel, which can provide cooling downward in addition to going upward.

If you recall, the debris bed experiments that we performed at Sandia, in-pile, were something like this: roughly four inches in diameter, we have fuel debris being heated by fission heating, simulating decay product heating, giving the energy up to sodium, which, in turn, transfers it to a helium cooling system.

DR. PLESSET: Let me ask, why do these experiments have to be in-pile?

DR. COATS: Well, that's --

DR. PLESSET: It makes it more complicated.

DR. COATS: I have had a lot of practice on that question.

DR. PLESSET: Oh, you have.

DR. COATS: Yes.

DR. PLESSET: Okay. Well, maybe you can answer it very effectively.

DR. COATS: Well, I hope so. The primary reason is

O-4

because I think it's the only mechanism by which you can get the intrinsic selected heating in the real materials. And I think that's very important.

DR. PLESSET: Could you explain that?

DR. COATS: Well, most of your other heating techniques to study experimentally a bed provide either heating, say, at the lower boundary, in the case of bottom heating, or heating of the coolant rather than the debris particulate. Also, if you have a mixture of UO2, steel, and sodium, you have no mechanism other than fission heating to heat only the UO2 and not the steel or the sodium.

And these processes tell -- dictate the way that the heat is transferred from the bed to the coolant and subsequently out of the system.

DR. PLESSET: Well, I think what one is -- there are two things of thinking on it. One is to try to more or less simulate reactor conditions, so you do something in a pile.

DR. COATS: Yes.

DR. PLESSET: The other is to try to do a more basic scientific investigation, and that it might be easier to do more experiments, cheaper, and so on.

And this is what I was trying to get at: why you choose to do it the first way rather than the second way.

DR. COATS: Well, in answer to your question, we do not do it exclusively the first way. We do have out-of-pile experiments that have been performed in the past and are being performed now that complement the experimental work in-pile. Dr. Catton has done quite a bit of, a considerable amount of experimentation. The KFK people now are performing out-of-pile experiments complementing the in-pile experiments. The in-pile experiments in a sense can represent the first testing, the use of the real materials in the real environment.

DR. PLESSET: Okay.

DR. COATS: Our current program is based on our collaboration with Euratom and Japan. And our long-term planning -- and I want to point out that this is a living plan, it changes as our technology improves -- includes work through 1984, calendar 1984. These are the series of experiments that we hope to perform, and these are the parameters we intend to investigate -- the size distribution, bed stratification, the effects of adding steel, and the looks at bottom cooling for potential applications for in-vessel core retention. The regimes that we would lool at are indicated at the left -- I'm sorry, the right. And you may note that the difficulty of the experiments increases with time.

Now, recently we performed the D4 experiment. And I'll go through that very briefly before I announce the significant observations.

One, we discovered that in a situation where the sodium covering the bed was near saturation and rubbed the bed, near saturation, the bed was much more coolable than in a situation where the overhead sodium was subcoolant, in other words, below saturation by some significant amount. The reason for that is that in the saturated case there was more room for agitation of the surface, or ability to agitate the surface, because the vapor bubbles that were produced at the bottom of the bed could penetrate all the way to the surface before they condense. This would cause little volcanoes to open up, if you will, and channels to form at the top of the bed. Once the channels were formed, those, it's much easier for the sodium to re-enter the bed and for the vapor to escape. And so you had a much better heat removal scheme.

Now, you were asking me a question before?

DR. PLESSET: Yes. I still don't quite follow your argument, I must say. What is the geometry that you're thinking of?

DR. COATS: Okay. We have, roughly, a four-inch-diameter bed. It's about 11 -- the last experiment was like 11 centimeters high. Okay? In one case we have run the experiment with the overhead sodium, the kept sodium, being in subcool state, two or three --

DR. PLESSET: Above this bed?

DR. COATS: -- yes, above the bed -- two or three hundred degrees below saturation. But of course that subcooling extended down some depth into the bed.

0-7

DR. PLESSET: Which has a rather low conductivity, right?

DR. COATS: That's right.

DR. PLESSET: So how could it extend very far?

DR. COATS: I wish I had a blackboard.

DR. PLESSET: Yeah, well, but tell me.

DR. COATS: Well, it's just the -- the UO2 has a low thermal conductivity, but it is saturated thoroughly with sodium, and so while the sodium is present it has a relatively high thermal conductivity. And so the temperature gradient was such that at the top of the bed the sodium, for some layer, was below saturation, because the overhead sodium was below saturation.

In this case, the bubble that was produced at the bottom of the bed, your hottest spot, would have been condensed before it could reach the surface.

DR. PLESSET: You feel, then, that there is a significant amount of sodium trapped in the bed?

DR. COATS: Oh, yes. It's --

DR. PLESSET: And do you have any idea --

DR. COATS: -- permeating those beds.

DR. PLESSET: Beg pardon?

DR. COATS: It's permeated with sodium. At least 50 to 60 percent of the bed is sodium.

DR. PLESSET: Well, it depends what the bed -- how the bed starts, I guess. If it's big chunks that are pretty dense, you wouldn't get much vapor into those pieces; in between you'd get some of the vapor. So it depends --

DR. COATS: Well, yes, if you were not providing enough heat removal in time, you would create a vapor bubble in the bed itself. But it will initially start off as a bed totally surrounded by sodium, and then if the heat removal capability is not sufficient, then it will dry out and then you will get the vapor in the bed.

And we're looking at those conditions first, to determine what are the conditions of coolability. And then soon we're launching into the area of looking at dried out beds.

DR. PLESSET: So you assume the bed is not dried out and then take it from there, is that right?

DR. COATS: To establish the dryout limits, that's right.

DR. PLESSET: It seems to me it would depend a great deal on the initial condition of the bed.

DR. COATS: Absolutely.

DR. PLESSET: Yeah.

DR. COATS: At the risk of taking too much time, let me just point out the controlling parameters again. They are these: particle size and shape, to determine the permeability of the bed; particle size distribution, again for the porosity and permeability of the bed; the bed geometry, whether it's a cone, cylinder, what have you; stratification, whether you have your large particles on the bottom or on the top; presence of steel; the depth of the bed; and packing.

DR. PLESSET: Well, I guess my only question is, are the initial conditions the same when you compare a bed with saturated sodium and one that's below --

DR. COATS: Yes. They are.

DR. PLESSET: -- saturation? The same initial conditions?

DR. COATS: The same initial conditions. The same bed in this experiment, because we can control the subcooling conditions of the sodium as the experiment is in progress, so we would run the experiment for one case -- not disturbing the bed, just taking it to dryout -- and then step back, change the sodium conditions, rerun the experiment.

DR. PLESSET: Well, when you step back you're not starting with the same initial conditions, because as you use the bed you change the particulate conditions in the bed.

DR. COATS: No, I do not think that we do in these experiments. If we extended -- if we went to dryout and extended that dryout --

DR. PLESSET: Oh. Well, I guess, then, I am missing something. You say that the bed does not change particularly?

DR. COATS: Until channel formation occurs. And we run the saturated case after we run the subcool case.

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DR. PLESSET: Well, that's why I think the saturated one cools better -- because it's done last. Suppose you did it in the other order.

DR. COATS: It does cool better.

DR. PLESSET: Yes.

DR. COATS: If we did it in the other order, then the subcool bed might behave much like the saturated case --

DR. PLESSET: Okay.

DR. COATS: -- because the channels have been formed.

DR. PLESSET: So, then, it isn't something that violates my intuition.

DR. COATS: Okay.

DR. PLESSET: But you didn't say that first. In other words, it depends how you start. If you run the saturated one first and then the subsaturated one, the subsaturated one would cool better.

DR. COATS: That's true. But in the accident case --

DR. PLESSET: And if you did it in the reverse, it's the one that you do last that cools better, right?

DR. COATS: That's true. But in the accident --

DR. PLESSET: Okay. That's all I wanted to hear you say. Don't say any more.

DR. COATS: All right.

(Laughter)

In D4 we did see this, this effect; and I won't say any more about it. A disturbance was seen in this experiment much like we saw in D2, which we feel is the creation of these channels in the experiment bed. It is a irreversible state in the sense that the bed is much more coolable after the disturbance than before.

I would like to point out that the models that we have agree quite well with the behavior of that bed prior to these disturbances. It went from a, what we call a deep-bed model to something that Dr. Catton has worked on before, what we call a shallow bed, with the advent of these disturbances.

Our next experiment -- and I'll just run through it very briefly -- is to take a look at the effect of stratification. We will perform an experiment near the end of September, September the 45th (sic), something like that. And we will look at --

(Laughter)

-- we will look at a stratified bed that we have carefully built up to have the larger particles on the bottom, medium-sized particles in the middle, and small particles on the top, corresponding to observations that we made in the fragmentation program, which I'll talk to you about in a moment. All other characteristics are like the previous experiment we have performed and the only change would be the particle size distribution.

A following experiment, which I will not describe, will just simply take a look at the region in the bed beyond dryout. We will try to carry the bed as far as we dare after dryout has occurred.

In the modeling activity we have -- we have done considerable. I won't go into detail in the interest of time. Most of the committee members have seen the modeling activities as far as Class 9 activity.

If you have any questions I will be glad to answer them for you.

The one thing that we have done -- and this partially is the result of some of the Class 9 work, in that we did extend the model of debris bed behavior into work of testing, or, indeed, generalized and included the capillary effects that Shires and Stevens had postulated, as well as a reflood term that Ostensen had come up with, to give us an expression which has an implication primarily for light water work where you have larger particles. This particular model would tell you that your power that you could tolerate before dryout is less than you might have otherwise thought prior to this model. The interesting thing about this curve is the fact that almost all of the experimental data lies in this band. And we certainly need some experimental data for the very small particles and some for the larger particles to verify this particular model.

Supplementing -- or not supplmenting, but complementing the debris bed program we have what we now refer to as our dry capsule experiments, formerly we called this molten pool program, where we're taking quantities of UO2 from simulation of a dried out bed all the way to melt. And coupled with this program are furnaced (?) experiments out-of-pile. We also have ultrasonic thermometry development going on. Recently we did some looks at UO2/MgO. And of course the important part of the experiment -- of the program is to look at steel relocation, vaporization, condensation, and so forth as it may affect the bed behavior.

These experiments are performed in-pile, again. This is a typical experiment, where a certain amount of UO2 is placed on top of steel and then fission heated, and then primarily to look at the migration of steel, melted steel, through the bed.

The upcoming experiment -- and I'll just describe one -- is again similar -- I'm sorry, let me go back and just mention MP4 (?), which was recently performed, where we put 600 grams of UO2 on top of a disc of magnesia and performed a heating up to about 3000 degrees K in the UO2, slightly below melt, and the primary purpose of this was to look at the diffusion of the UO2 into the magnesia.

Our upcoming molten pool experiment, we're taking a homogeneous mix of stainless steel and UO2 in temperature, oven temperature, to above steel melt, to look at the agglomeration of steel and its migration. And I think you can see the implications in terms of core debris coolability from the migration of steel.

DR. SHEWMON: The migration in what direction, or what way, or what --

DR. COATS: Okay, it depends on the temperature. In the lower temperature regime, such as this one at 2000, (WORDS UNINTELLIGIBLE) driven, the steel will try and go to the outsides of the container, the cooler surfaces. At a subsequent we'll go up to temperatures in excess of 2500 C, in which case then we should have steel vapor driven migration.

DR. SHEWMON: Do you have any evidence that it migrates?

DR. COATS: Yes. We have already performed furnace experiments and one in-pile experiment where we actually did see some migration. Mostly agglomeration in the in-pile experiment, because we did not carry that one very far. And we have not examined it yet post-mortem. But in the furnace experiments we did see agglomeration and some limited amount of migration. Certainly the agglomeration. I think, if I recall, I showed you a picture of these tests.

Let me speak just very briefly on the final subject, and that being fragmentation. This is a program that we have completed, at least, as currently planned. We have performed roughly eight 20 kilogram experiments, where we have created a melt (WORDS UNINTELLIBLE) processes, conducted into the sodium, or in the reverse case put sodium onto the melt, and one last experiment where we poured sodium onto the melt in the presence of concrete. The result is, we have excellent data on particle size, particle size distribution, bed stratification, porosity, and bed formation.

One significant observation is the fact that the particle size distribution is very much like what Argonne had measured in their experiments some time ago, some smaller-scale experiments. This is one of the purposes of this program, was to --

DR. SHEWMON: That tends to be a state function? Or, you know, to what extent does it depend on what you add to what, what proportions you add it in, how suddenly?

DR. COATS: Okay. We -- of course we have not examined all parameters. One thing that we did examine was scale. We performed these experiments at 20 kilograms. The other parameters we varied was one that we put the sodium onto the melt or we put the melt into the sodium; and then we did the experiment in the presence of gases being evolved from the concrete. For those cases we saw no significant variation in the particle size distribution from that which had been determined in smaller-scale experiments.

DR. SHEWMON: Well, I assume you can call it a state function, though a further analysis might argue --

(Several speak at once)

DR. COATS: This gives you an example of the apparatus

I won't bother to go through. We simply create the melt and allowed it to go in the sodium in what we called the forward experiment.

The apparatus looks something like this. This is the experiment where we have a large tank of sodium and dump the sodium onto a melt, which is created in the lower train.

I might point out that this is the particle size distribution that we have obtained. In the lower corner are some of the in-series experiments performed by Argonne. They all lie within the same band, all the data.

We're seeing samples of the material itself. Here's an addition. This is a -- samples from a bed. This was at the bottom of the bed, half an inch up to an inch up, or one to one and a half inches up, and then near the top of the bed. And you can notice a very definite stratification.

One last point I'd like to make, this is the other observation from the fragmentation experiments, is the fact that we have a higher porosity than we have ever been using in any of our in-pile experiments or any of our debris bed cooling analyses. It's a much higher sodium fraction than we have been assuming in all of our studies. Which means that the bed is probably much more coolable.

DR. SHEWMON: Now will you leave that there and explain it to me again.

DR. COATS: Okay. I was hoping to get it off of that and move to the information.

What we have here is an example -- of a bed.

DR. SHEWMON: Which way is up?

DR. COATS: Well, I guess if I turn it this way it would help some.

(Laughter)

VOICE: You're right, Dick.

DR. COATS: How's that?

DR. SHEWMON: Well, I don't know. You (WORDS UNINTELLIGIBLE).

(Confusion of voices)

DR. COATS: This is the bottom of the bed.

DR. SHEWMON: Okay. And --

DR. COATS: Okay, this is the top of the bed.

(Laughter)

This is a sodium fraction. The sodium fraction going this way is 70 percent at the bottom of the bed. And then it actually decreases somewhat to around 40 percent near the center of the bed. And as you get to the top of the bed the sodium fraction increases, obviously, all the way to 100 percent, as you get above the bed.

DR. SHEWMON: Now, you dumped one into the other, you let it solidify, and then you sectioned it?

DR. COATS: That's right. We core it and we get sections at the top, bottom and so forth, and also a function of the radius, so we looked at it radially within the bed.

DR. SHEWMON: Okay. And that's a long ways from -- it's not bad close packing, I guess -- no, it's a long ways from it.

DR. COATS: It's a long ways from close packing. The bed is more porous than we've been assuming -- at least, we have. And we're hoping to -- we certainly will, obviously, use these results in establishing conditions of our beds for future study purposes.

DR. KERR: Those were the results from how many experiments?

DR. COATS: There were -- I think it was a total of eight; there were about five sodium -- I'm sorry, melt into sodium, two with the melt -- I'm sorry, with the sodium onto the melt, and then one with the sodium onto the melt in the presence of concrete and in gases.

DR. KELBER: If that same type of observation were to hold true for the types of light water cases that are being investigated, that would be very favorable for cooling in-vessel a la TMI-2.

DR. KERR: I don't understand they all are TMI-2 unless you think of --

DR. KELBER: Well, in TMI-2 there was, could have been -- pumps were operating, it was possible to cool the core while it was in the vessel. The question comes up: could you, had the damage progressed further could you, have cooled it in the vessel?

DR. CARBON: Any questions?

Fine. Thank you, sir.

DR. SHEWMON: Let me stay with it for a minute, or ask in general, is there -- if your particles are heated, I could imagine at least some thermal motion which would make the packing even less dense than they would be after you had it stone cold and solidified in the laboratory -- is there any analysis or argument of that given? Or is that at least -- is this a limiting case, then, what you get after the thermal energy is all dissipated?

DR. COATS: You're discussing the fragmentation experiment?

DR. SHEWMON: No, I'm discussing the agitation that might occur in a bed due to the fact that particles are generating heat, and conceivably, at least, in the water case, they're generating bubbles. You know, it's like my home-brewing operation: the particles come up in a bubble of CO2 and go down again or something.

DR. COATS: Lou Baker has a qualitative argument along those lines. I'm not thoroughly familiar with it, but I have heard him talk about it. It's qualitative at this point; he'd be the first to admit that.

DR. SHEWMON: Okay, but this is, at least, a limiting case and on that more dense than what one might expect, then, from Baker's argument, in an actually -- if the particle is generating heat?

DR. COATS: Are you saying that the real case where you'd include those arguments, that the bed would be more coolable?

DR. SHEWMON: Yes. This is more conservative.

DR. COATS: I think that's true, yeah.

DR. WALKER: To the degree that permeability of the bed is a parameter, one would hope to have a model that would handle that range of permeabilities. And depending on what the accident scenario is, then one could predict what the coolability of that particular jump tree would be.

DR. SHEWMON: Your coolability, this has to do with convection of warm fluid. And mine has to do with the rising of fluid to the particles, through the potential bubbles or --

DR. WALKER: Redistribution of this.

DR. SHEWMON: Yes.

DR. WALKER: That would, then, change the bed.

DR. COATS: Our modeling has not progressed to that point yet.

One generality I would like to offer is that the more that we learn about beds, the more coolable they appear to become.

DR. POWERS: I'm Dana Powers, and I'll be talking about accidents that -- accident events that occur when this cooling you've just heard about cannot be maintained.

When you can't keep the core debris cool, it will penetrate the primary containment system and will allow both molten sodium and molten core debris to begin to attack the reactor containment structure.

You heard earlier, I think, about the sequence of events that lead to these ex-vessel interactions when the accident delineation study was discussed. This accident delineation study identified four modes of ex-vessel material interaction.

For relatively mild accidents, not necessarily involving core debris, you can have ex-vessel interactions in which only the sodium is attacking the containment structure.

In somewhat more severe accidents, you could have fragmented but still quite cool core debris intermixed into the molten sodium. The coolability could not be maintained in this situation. It would evolve to the point you would have both molten core debris and molten sodium attacking the structure.

You can also get into accidents in which only the molten core debris is attacking the containment structure.

What I'll be talking about is our experimental research program to look at these four modes of materials interactions.

First I think we should understand exactly why ex-vessel material interactions are so much a source of concern. The ex-vessel material interactions threaten the structural integrity of the containment and they also provide an additional radioactive source term that enhances the consequences of failure to maintain the integrity of the containment.

We look at the threats to containment on this slide.

DR. KERR: Excuse me. I missed something. Why do those interactions prevent additional radioactivity?

DR. POWERS: I'll go right into that --

DR. KERR: Okay.

DR. POWERS: -- right after I discuss the threats.

When you have high-temperature materials attacking concrete in the containment structure, tremendous quantities of gas are generated. These gases, of course, (WORDS UNINTELLIGIBLE) containment just by repressurizing it. The gases that are released are primarily carbon dioxide and steam. When they interact with the molten core debris or the sodium they are converted into hydrogen and carbon monoxide. Either combustion or detonation of these gases could also threaten containment integrity.

The gases as they blow through the melt transport energy up into the contaiment, either by convective heat transport, the transport of fission products, or combustion of flammable gases enhances the heat loads that must be sustained by the containment.

These three processes all threaten an above-ground damage to the containment and consequently an airborne release of radioactivity.

The fourth mechanism -- or fourth threat to reactor containment is basemat penetration. And this mechanism of failure has been much more (WORDS UNINTELLIGIBLE), but it really is less consequence than the other three, because failure of containment through the basemat, if you recall, releases radioactive material into the (WORDS UNINTELLIGIBLE) gives somewhat slower and less severe consequence than an airborne release that the other three mechanisms provide.

DR. SHEWMON: Sir, the gas that's being generated is a non-condensable gas from this interaction?

DR. POWERS: You can get out of the concrete both carbon dioxide and steam. Now, steam you could argue is condensable; but it does react with sodium or metallic traces of core melt to create hydrogen, which is definitely not condensable. So in those situations there is non-condensable gas.

DR. CARBON: The penetration through the basemat would lead to much, much slower effects on (WORDS UNINTELLIGIBLE).

DR. POWERS. I -- in many, many plants that is actually true; it's very, very slow. There are a few -- and I guess the most detailed analysis has been for light water reactors -- where you can have reasonably prompt entry into the water supply, but you're still talking about an order of magnitude slower than airborne release. "Prompt" would be in a day or two, whereas "slow" would be months, maybe years. So yes, there's a large time difference in the release.

How much release occurs is not a well-known item when you come to ground water release right now.

Now, the additional source of radioactivity that comes from the materials interaction is in the form of aerosols. These aerosols are generated by the gases sparking through the melt, by temperatures leading to vaporization of radioactive materials, and also because of chemical reactions between the gases and the radioactive materials that allows chemical transport of radioactive (WORDS UNINTELLIGIBLE) melt enough into containment. The aerosols also aggravate accidents, because they threaten some of the mitigation systems; large quantities of aerosols will come in exchangers and reduce the efficiency of air coolers; they can also clog filters and plug vents. This entire subject of aerosol is a very complicated situation to have (WORDS UNINTELLIGIBLE). And the ex-vessel material interactions are a very large source of aerosols.

DR. SHEWMON: Now, they can also trade out on any other surface around, which could get --

DR. POWERS: That's right.

DR. SHEWMON: -- (WORDS UNINTELLIGIBLE) there.

DR. POWERS: That's right. The entire subject of these is, as I said, very complicated to analyze. Clearly, if your containment has been breached, a very high aerosol generation rate is a very undesirable thing. If, on the other hand, you have maintained the containment and you have a high aerosol generation rate, that also enhances the rate of sedimentation and agglomeration of the aerosols and consequently reduces the amount of radioactivity you could release when the containment is eventually breached.

It's very complicated. And timing as well as the actual amounts is important in analyzing aerosol releases.

It is also important to recognize that ex-vessel material interactions are a generic subject. The technical issues that arise for conventional plants I've just outlined -- gas generation, flammable production, outward heat transfer, aerosol generation, and basematter motion.

Recently there has been a lot of discussion abou incorporating advanced mitigation pictures into a plant to handle where more of these technical issues. An example might be a filtered vent system. However, even to -- to design such a system or evaluate the design, you still have to understand gas source (WORD UNINTELLIGIBLE), the production of flammable species, and aerosol generation.

Another advanced mitigation system is the core catcher. Again, to evaluate a design or to design one, you have to understand the energy coefficienting from the melt, the aerosol generation, and the rate at which this core catcher is eroded.

It's clear that you cannot avoid the subject of ex-vessel interactions simply by going to some of these advanced mitigation systems.

To discuss our experimental program I would like to begin by discussing our experiments on those situations in which sodium alone is attacking the concrete of the containment.

The central questions in this field begin with what is the overall magnitude of the phenomena: what is the gas generation rate, what is the aerosol generation rate, and the erosion rate. Recently some controversy has been generated about whether the sodium-concrete interaction, because of its unique chemistry, will continue to completion or is there some effect that will allow it to be self-limiting in some way. And for very mild accidents that don't involve the core debris, there are also questions concerning how carefully do liners to prevent the sodium-concrete interaction have to be designed and inspected.

I believe in your previous -- the earlier meeting, that Mel Silberberg went over some of the accomplishments of our sodium-concrete program. It is -- consists of large-scale interaction experiments, supported by laboratory investigations and some modeling. I don't intend to discuss these specific aspects of our program in detail, but rather to relate them to this question of self-limitation, because it's very important to understand the self-limitation of the reaction (WORDS UNINTELLIGIBLE) has been controversially.

DR. SHEWMON: Before you leave that, it seems to me, a

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year or two ago, I remember hearing that there was some disagreement between us and them with regard to whether molten material in concrete would spread out or go down, or that the degree of lateral transport or lateral velocity through concrete and vertical -- "them" being the Germans, as I recall -- if we get into USINT, or OSSIENT (phonetics) or whatever, which is a model for state-of-the-art things, does that sort of comment on that? Or --

DR. POWERS: No, it does not. USINT itself comments on how concrete behaves to a heat load placed on it, how it decomposes and how heat transfer -- how heat is transferred into the concrete.

The question of whether we have downward or lateral erosion developed in magnitudes of those really arises from core debris interactions. And I can touch upon it. It is a question that is still not well resolved, to my mind.

DR. CARBON: Will you also be discussing this difference in data of a year ago between you and Edel (?)?

DR. POWERS: Yes. That's exactly what I would like to go into.

The possible mechanisms that would limit the sodium-concrete interaction, there are three -- the first two are trivial. You completely deplete the sodium. Or you completely deplete the available concrete. This is a fairly important one, because sodium itself can attack a great deal of concrete in the basemat and you might conceivably get failure of a basemat just by the sodium interaction. The controversial mechanism of limitation is one that was proposed by Edel based on some small-scale tests that he had done, in which they found a product barrier was formed that inhibited reaction before all the sodium or all the concrete was consumed.

Now, this particular observation was not confirmed in our research program with fairly large-scale tests. We found, instead, that the reaction seemed to take place in two phases -- first there was a fairly mild interaction, then, after a delay period, it suddenly went into a very energetic reaction that did not stop until all the sodium was consumed.

And if we look for a difference in the experiments, one of the differences has been the scale of the experiments. And here I have plotted the amount of sodium used in the experiments in a logarithmic scale versus whether the interaction proved to be self-limiting or proceeded until all the sodium was consumed. And I have plotted data for experiments with limestone concrete, basalt concrete, magnetite concrete, and the subscripts indicate whether the test was run at Sandia or at HEDL.

If you neglect one rather questionable test run at Sandia, you notice that all the unlimited interactions take place up in the large scales, from something over 50 to 100 kilograms of melt was used. The limit -- self-limited interactions seem to be confined to fairly small-scale tests. There appears to be a scale effect on this interaction.

In fact, I think that's something we fundamentally believe in in our experimental program, that there are scale effects, that surface-to-volume ratios do influence the nature of interactions, and that's why our program tends to emphasize conduct of large-scale tests. It serves two purposes: it assures that you don't miss any important phenomena; and it also assures us that we don't include in our analysis of accidents phenomena that later prove to be trivial in the large scale.

The scale effect of that, of the sodium-concrete interaction here, has now been confirmed with a test conducted (WORDS UNINTELLIGIBLE) with only the energetic interactions. In what we have done at Sandia there seemed to be a laboratory effect. It now appears that this energetic interaction prevents solid permutation, has something to do with large -- large scale.

We have begun to formulate a model based on our chemical interpretation of the interaction, which I think has been described to you --

DR. SHEWMON: Sir --

DR. POWERS: -- in the past. Yes?

DR. SHEWMON: -- you know, I -- I grant that if you gave that to a statistician and said, "What's the best line through it?" he'd give you something from southwest to northeast.

But --

(Laughter)

DR. POWERS: I don't propose to interpret this as the fundamental variable is scale. I only offer this as an empirical observation that small-scale tests do not --

DR. SHEWMON: Let me finish the question, will you, please. What about -- do you want to try to get rid of limestone with your operation, too, with your explanation? Or do you want to argue that limestone is basically different from basalt?

DR. POWERS: The two are different. Limestone tests have been energetic in the large scale and nonenergetic on the small scale.

DR. SHEWMON: Well, I'm looking at the two points which don't fit your correlation least. And you haven't argued that they should be thrown out. And I'm trying to give you an opportunity, but you don't seem to --

DR. POWERS: Well, I have included examples of all the tests. Sometimes it's like comparing apples and oranges. If a heater failed that was not a criteria for leaving it off this, this plot.

DR. SHEWMON: Well, you point energetic up at the top. On the other hand, there are those two L-points which were big and were limited.

DR. POWERS: That's right. I have no explanation for

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them right now.

DR. SHEWMON: Okay. Go ahead.

DR. POWERS: We are trying, in the process of trying to formulate a model that does successfully predict some of our tests, to look at each one of the tests in detail and try to explain it into intuitively pleasing parameters, such as concrete porosity or its chemical composition, to explain the different results.

DR. MARK: You don't have on there the depth of the sodium for unit area.

DR. POWERS: No, I don't. And --

DR. MARK: Are these all on a square meter or something or other?

DR. POWERS: No, they're not. They're all over the map. There are a lot of different things. If you try to correlate it that way, you don't come up with a -- any more explanation than this offers. What you do find is a changing in the time when you put it on -- in shallow pools or deep pools or pools with structuring, and it changes the timing.

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DR. POWERS: I do not have an explanation for why so many interactions don't go. I have simply observed that there does seem to be a skill.

I would like to go on and discuss our work on suggested molten core debris attacking. I would like to do that by showing you the various stages such an interaction can go through.

When the core debris first begins to attack the concrete -- well, there is a plot here in temperature versus time, and I will assure you time is an extremely nonlinear scale, perhaps a parabolic scale. When the core debris first begins to attack the concrete you are in a very high temperature situation. The erosion rate is quite high; gas generation rate is quite high. The erosion rate is like 100 centimeters per hour.

As the material cools you get down to the point where solidification begins to occur and transfer, and erosion rate is proceeding at a significantly different rate. It eventually reaches the point where the core debris begins to solidify and you enter what we call the low temperature region where you have not solid core debris still eroding the concrete.

Eventually you reach regions where the debris is so cool that no longer is there erosion, but there is still gas generation taking place. This higher ultra-high temperature phase, we call it, sets the stage for all the subsequent interaction. These two low temperature phases dictate the ultimate extent of gas generation.

If we look now where the experiments to date have been performed, both at Sandia and other places around the world, we find that predominantly experimentation is taking place in this high temperature phase not too far away from the melting point of the core debris itself.

There has been a limited amount of investigation in the ultra-high temperature phase, and now only a few experiments in the regions where the solidified but still quite hot core debris are eroding the concrete. The very low temperature phase has only been investigated in heat flux tests.

DR. SHEWMON: Where is the boiling point of sodium on your arbitrary temperature scale there?

DR. POWERS: The boiling point of sodium would be about right here.

DR. SHEWMON: All right.

DR. POWERS: I think this experimental situation is somewhat clearer in a plot of the maximum melt temperature versus the duration of the test. You can see that at the very highest temperature only steel experiments at, somewhat long steel experiments is fairly low temperatures that they run. For very high temperature experiments the experiments have not been very long. There is a significant gap.

DR. SHEWMON: Now where is the boiling point of sodium on that one?

DR. POWERS: The boiling point of sodium would be about right here.

DR. SHEWMON: Sorry, a minute ago all of the concrete work has been done below the boiling point of sodium, and all of those experiments were done above the boiling point of sodium?

DR. POWERS: No, I haven't --

DR. WALKER: I think you showed the wrong temperature on the other one, Stan.

DR. SHEWMON: All of those were below the boiling point of sodium, you said?

DR. POWERS: The temperature plotted in this way you are asking where the boiling point of sodium is, it would be down in here or -- okay, so

DR. SHEWMON: Well (simultaneous voices) will be done at temperatures where you would have violently boiling sodium, is that right?

DR. POWERS: Yes, if you were working at room temperature. What I am talking about now is just temperatures where there is just core debris interacting with a concrete item.

DR. SHEWMON: Okay, and the sodium is all boiled away and --

DR. POWERS: Yes.

DR. SHEWMON: -- someplace else. You have got things to contain it and keep it away from the core?

DR. POWERS: That is right.

DR. WALKER: Or you could have a dried out debris where it is over-covered with sodium but the local temperature is much higher and the debris is interacting with the concrete.

DR. POWERS: The combined interaction where -- -- core debris interacting with the sodium.

Again, I believe that work was done on just core debris attacking concrete, we summarized for you at the previous meeting.

I would like to comment particularly on our work with code comparison tests and the sustained hot solid debris interacting with concrete. As I indicated, our experimental work both at Sandia and Oakland was concentrated on the very high temperature phase and interaction. Our work has been both experimental and analytic, and the two have reached a sufficient stage of sophistication, both here and in the United States and in Europe that it seems reasonable to try to evaluate and begin to verify the models we have developed.

We reached an agreement with the investigators in Germany in a model comparison jointly. The model comparison effort consisted of running two sustained interaction tests involving about 200 kilograms of molten steel sustained in contact with the CRBR concrete.

In asking the developers of the various computer models to predict these tests without benefit of the end result. Currently there is analytic predictions of the tests going on at Sandia with the Corcon Code. At KFK in Germany with the Wechsl Code, KBU with the Kavren Code. The Argon Growth Code, those people have asked to participate in this, but we have not sent them the data package yet. They will soon get that.

The basis of comparison is to be on a concrete erosion rate and on temperatures within the concrete. The codes currently cannot predict gas generation well, and only recently has there been sufficient data to predict aerosol generation. So these two subjects are not included in the code comparison effort.

We are beginning to see some of the failings of the computer models of this high temperature phase of the interaction. As I indicated, gas generation seems to be significantly underestimated because of the way concrete is treated. Because we have been able to develop this -- model of concrete independently, by including that in the model I think we can improve the ability to predict gas generation very easily with the codes.

Recently data necessary to predict aerosol generation will be available. A very disturbing feature of the codes is they seem to be predicting freezing of the core much sooner than it occurs in the experiments. This may be a consequence of the way the codes are handling material properties in the mixtures.

The final point is the codes seem to be very sensitive to concrete properties. This is not really a failing of the codes. This is a failing of the experimental effort. We are obviously going to characterize concrete very well.

DR. KERR: I am sorry, I can't understand there is necessarily a failing of anything, if a physical phenomenon is sensitive to concrete properties.

DR. POWERS: No, it is not a failing of the codes. I just illustrate that it appears that we happen to have a very good characterization of concrete to get the codes to work well in predictive tests.

Ultimately, all of these interactions, high temperature melt, came down to the point to end up with a hot solid attacking the concrete. And this hot solid will determine the ultimate extent of gas generation in a basemat erosion.

We have just done a few experiments in this area, enough to outline a program, and it is clear that the scale of these tests is very critical in order to extrapolate them onto the accident situation. This means because the scaling is so difficult to do that very large scale tests will probably be necessary in this area.

It is also clear that the tests need to be run for long periods of time to establish some steady state configuration. No tests to date have been run that long.

This -- concrete interactions are, these as expressed in containment, one of the obvious recourses gives you some place concrete in reactor cavity with the core retention material, and a logical extension of our work has been to look at material interactions with core retention devices.

We have completed some survey work with high temperature steel interacting with a sacrificial retention material and some refractory retention material.

These efforts to look at retention material are continuing. One of the observations from our program is that -- -- such as high aluminum cement, seem to be particularly attractive. They are attractive not only because they retain the melt well but they are fairly inexpensive and easy to incorporate in a plant design.

The issues that you are involved with in these sacrificial material interactions are exactly the same as we had for concrete; that is, erosion, gas generation, aerosol generation, and high levels (cough) have many an attractive feature in addition to the more refractory and esoteric materials that are offered.

We have also done a considerable amount of work looking at chemical interactions with UO2-MGO systems; and some of our early model development suggests that these chemical interactions dominate the erosion of magnesium oxide by UO2.

We have also recently attempted to use our large-scale fuel melt facility to do a large-scale UO2-MGO interaction. Unfortunately, that test was not successful.

I would like to conclude by discussing our work on the combined interaction of molten sodium and molten core debris together. When one thinks about this combined interaction, you see that there are three possible modes of combined coolant, core debris, and concrete interaction.

One possible configuration is to have the core debris present as a debris bed underlying a sodium pool. This of course is just the debris bed -- situation that you have heard about earlier.

Another situation would be to have the core debris streaming into a sodium pool. This again is the pragmatician effort that you have heard about.

The area that we are trying to address in the materials interaction study is the situation where you have molten core debris attacking the concrete overlaid by a layer of sodium -- -- the concrete.

The only experiments we have in this area have come from the fragmentation, a program that you heard about earlier and some scoping work done with water. They have been enough to line out where our experimental plan will be in this area.

I would conclude by just going through this plan. It will begin with some work where we are doing water injected onto a sustained melt in contact with magnesium oxide.

Following this test we will go to one where water will be injected onto a sustained melt in contact with concrete. Unless we can handle and instrument these tests, we will then go to sodium injected onto a sustained melt.

We have chosen this rather careful approach because it is a real serious problem, the necessary experimentation to do. You can do tests that will yield nothing because its interactions are so complicated and difficult to instrument. Or even worse, you can do tests that will be misleading.

To avoid this we have been very careful about identifying some critical parameters and it appears the melt depth is undoubtedly one of the most critical parameters. It is not prototypic to do a test of coolant coming into contact with a very thin melt.

Melt temperature of the core is another critical parameter, as well as the condition at which you apply the coolant on top of the interacting melt.

That concludes what I have to say.

DR. SHEWMON: What is the aim of this stage of the experiment?

DR. POWERS: This is to look at the combined interaction of coolant, core debris, and concrete. Primarily what you would like to find out is does the coolant cause the fully developed core debris to fragment and stop the high temperature interaction and go back to just the sodium interaction with the concrete.

The fragmentation has been advocated by a large number of people.

DR. SHEWMON: And so if you inject water on a melt contained in a magnesia crucible, that will tell you about the fragmentation of this melt?

DR. POWERS: That is what --

DR. SHEWMON: You are saying?

DR. POWERS: That is what we would like to find out about. The experimentation is so complicated we would like to take it a step at a time. That has been our approach in the past.

DR. SHEWMON: There you wouldn't have gas evolution that you would in the next one?

DR. POWERS: That is right. The gas evolution is very formidable, experimentally difficult to handle.

DR. SHEWMON: Okay, and since I have missed the discussion of temperature in here, let me go back then. The sodium at its boiling point reacts with concrete rather narrowly, is that right?

DR. POWERS: That is right. You are quite right, yes.

DR. SHEWMON: And so at least as fast as your hot, your very hot core materials?

DR. POWERS: Certainly it interacts as well as a hot solid core debris does. And if you get to molten core debris, then molten core debris is a little more aggressive.

DR. MARK: You spoke of all the sodium being consumed. What is the final state of sodium when it stops doing things?

DR. POWERS: It depends of course on the kind of --

DR. MARK: On concrete.

DR. POWERS: -- concrete that you have. If you have a basalted concrete like the FFTF, you are in a sodium silicate system.

DR. MARK: Yes.

DR. POWERS: If you have concrete like the CRBR you have primarily -- -- neither one of which is a very efficient coolant medium.

DR. MARK: But they are also inert?

DR. POWERS: Yes.

DR. MARK: Tentatively inert.

DR. SHEWMON: Compared to sodium?

DR. POWERS: Yes, compared to sodium.

DR. MARK: Does it develop more gas?

DR. POWERS: Well, it presumably could if you got it heated to a very high temperature with the core debris, begin to get soft..... ..

DR. WALKER: I sense that an earlier question that Dr. Shewmon asked, that he was not satisfied of our description of the scenario whereby you could have melt temperatures at considerably higher than the sodium saturation temperature in a pool of sodium; in other words, the dried-out debris bed case -- was that -- my feeling was that that was not satisfactorly answered.

DR. SHEWMON: Yes. I wasn't sure how with those temperatures you were getting the sodium to stay up in the air someplace for you.

DR. WALKER: And that is in fact the models that are being developed as part of the debris bed program for the dried-out case. You in fact can dry the bed out and that is what causes the bed to progress into melt.

DR. SHEWMON: Okay, and if you -- -- that, then you have got enough pressure to keep it out there; that is, anything that happens to start in boils off and goes away again. Is that it?

DR. WALKER: That is right. There is a continued reflooding, and then as the bed melts you start forming crusts which further inhibits the sodium path, and then you can get into where we call the transition from the debris bed into the molten pool. All of this still under sodium, under the bed of sodium. And so that can take you in fact up through the melting point of UO2, and then you have to concern yourself with the attack on the concrete.

DR. SHEWMON: Is that still true a week after the event?

DR. WALKER: Well, it obviously depends on the decay levels. Dick, maybe you can comment on that?

VOICE: (inaudible)

DR. WALKER: In general, it is a much higher concern very early in the accident.

DR. SHEWMON: Yes. I didn't mean whether it was coolable, but I have the impression when we get into Class 9, if you can keep the containment from rupturing for the first week, life is an awful lot sweeter in explaining things to the surrounding population. And I just wondered with something was here which was a different kind of fission, and certainly also a much higher specific density of a week watching that much.

DR. WALKER: I think we have done some analysis, in Allen's and Dr. Kent's group. I don't know whether you can comment on that or not. These are detailed calculations. I don't know that we have the data at hand.

DR. KERR: You could look it up.

DR. KELBER: I would like to make a programmatic comment here, that aside from the emotional impact I do not regard from a question of risk the penetration of the basemat itself; that is, being a high priority question. The value to these tests lies in the question of what are the conditions within the containment and what are the loads on the containment, in particular what are the aerosol properties.

And there are two questions there: first, if you for some reason are so unfortunate as to have an early release, then these loads probably, if they are severe, exacerbate that. It would cause the release to occur still earlier and make things worse. On the other hand, if --

DR. SHEWMON: Your releases are not out of the pressure vessel but the containment.

DR. KELBER: The containment.

On the other hand, if the containment can be kept intact for a matter of several hours -- and as you recall, for CRBR the design criterion was 24 hours, I believe -- then there is very substantial reduction of potential dose just through the aerosol settlement, and I think that this is going to be part of the topic that is going to be covered under the contain code, and there is a very considerable incentive for that. You have mentioned a period of a week. Actually periods of several hours are significant.

DR. CARBON: Go ahead, Dr. Clauser.

DR. CLAUSER: I am reminded that during the accident delineation study part I neglected to introduce myself for the record. My name is Milton Clauser. I am the supervisor of the Advanced Reactor Safety Analysis Division of Sandia.

I should mention --

VOICE: Are you new?

DR. CLAUSER: Well, I have been supervisor for six months and prior to that time Paul Pickard was supervisor of this group, and as a consequence has played a leading role in both the accident delineation study effort and in the contained development project.

I might also explain a little bit of my ignorance in certain areas. Prior to that time I was in the -- -- fusion effort at Sandia rather than the reactor safety effort.

Going on to contain, this is a project to develop the containment analysis codes and integrated reactor containment systems code. At present we have three staff members working in this area, as you can see listed there. But as I mentioned before, Paul Pickard has also played a leading role in this development.

The code is intended to analyze post-accident sequences, starting basically with, if you will, the core on the floor situation. That is to say, it does not treat at this point the melt-through of the primary vessel but starts with the situation that we have, the molten controller or core material, debris material and sodium sitting in the reactor cavity.

And it follows the progression of the accidents through the release, or through breach of containment atoms. It is intended to cover all types of accidents basically. It is intended to apply to useable, for all types of reactors. Initially being built, has been built for LMFBR containment. We have recently added features in some parts of it that allows us to consider wide water reactor accidents.

It should cover all types of containment presently being considered. However, not all of these features have been implemented. The structure is such that they should be readily implementable. Basically it uses models for mass energy generation and transport processes, and it determines among other things the pressure, temperature, location, and state of fission products within the containment building.

I should add further that it does not do any structural analysis calculations. It provides the pressures that are seen for example by the containment building, but does not go through the mechanistic analysis, the detailed analysis of the structure strengths and so forth.

VOICE: -- -- about chemistry?

DR. CLAUSER: Let me come to some of the details in the models shortly.

DR. CATTON: When you reach the proper point, could you point out the differences between this and something like the beacon code -- supposedly for water reactors.

DR. CLAUSER: Well, yes, I will try to.

DR. KERR: Excuse me, I judge from the first slide that this was developed primarily by SAI?

DR. CLAUSER: No, not at all.

DR. KERR: No one from SAI --

DR. CLAUSER: I am sorry, J. Odom is an SAI employee legally. In practice he works as -- he is in effect a Sandia staff member in terms of how he operates, how he works with us. He works on site. Except for the color of his badge you wouldn't know he is not a Sandia staff member --

DR. SHEWMON: That is true of Rudeen also?

DR. CLAUSER: Dave Rudeen is a relatively new member. He has worked primarily on things like input and output and so forth. Operationally, yes, I expect him to be in the same category.

The reasons have to do with Sandia policy and ceilings and so forth, as well as availability of trained people and so forth.

The bottomline is operation, and I don't consider that to be a significant feature from an operational standpoint.

Okay, some of the features of contain. Well, first of all it is intended to be an integrated code, as I indicated before; that is to say, it is intended to cover all of the essential phenomenology in the containment problem as opposed to pieces of the action such as many other codes handle.

It incorporates or will incorporate state of the art physics model, and let me cite three special features here, which are either unique or which contain, led the way in the development.

First is the general cavity debris pool model called Center, which treats in some detail all of the interactions in the sodium debris pool situation on top of the concrete. And I will come in a little bit, in a few minutes, to some of the details of that interaction. I don't have time to go through the full details there.

A multicomponent sectional aerosol model, the stand-alone version of which is called MAEROS and which was developed for CONTAIN has been incorporated in CONTAIN. It is my understanding that other codes are incorporating similar models. A general and rather detailed fission product decay and transport scheme, which will follow individual isotopes, is being developed. It is not yet implemented.

I think those are the three principal features which I would like to cite at this point. And as other models become developed, the modular structure, which is the second feature I would like to cite, will allow them, will allow new models to be readily updated.

The code is modular. Pieces of the code can be put in and taken out with relative ease.

In that vein, let me point out that the intention of the code is to be a rather user-oriented code. We intend that users cannot only bring the code up to the point that it will operate on -- -- problems applied with the code, but the user can readily alter the code as he wishes to put in different, perhaps better models. I think it can be altered by, or should be alterable by people other than the few people who have put together the code in the first place.

Finally, the first version is presently operational. That is not to say that it is fully operational, but several sample problems or problems have been run with some of the model being in operation.

DR. KERR: That operation, have you been using it to calculate something for design purposes?

DR. CLAUSER: For design purposes? I wouldn't go so far as to say that it has been verified to a satisfactory level that I would want to use it as a design code yet. Very few problems have yet been run. It is not what I would call a tested code at this point.l

This is a slide you saw last fall, I believe. In any case, this indicates the extent of the physics models, which CONTAIN has or will have. This is not a complete list of all the models. The point is rather to indicate that it covers ground covered by a number of other codes, and it is more comprehensive than these other codes. .

The questions asked about BEACON, I am not familiar in detail with the code, but it is my understanding that that is primarily a fluid flow code for light water reactors. I don't believe it has aerosol treatments, for example; I don't believe it has fission product treatments.

DR. WALKER: It does not handle interactions either, with basemats and chemical -- you know, in this regime.

DR. CLAUSER: Paul, I think has looked into that in some detail and can perhaps answer your question more fully.

DR. PICKARD: We can basically handle loading phase, below the compartment (inaudible)

DR. CATTON: So your model is more appropriate once the initial stages of the accident have passed and things are happening slowly. And things like the pressure spike MTLMB-prime will probably not be predicted accurately.

DR. PICKARD: I think that is right. This is a very simple model.

DR. CATTON: Okay.

DR. CLAUSER: As I understand it, we are supposed to finish up in about six minutes. So I won't go through the next few lists in any detail. Rather I want to indicate in a pretty broad way the status of the codes.

I have also listed the very specific models within the code. The code is divided basically into two parts. One covers the cavity (inaudible). The other model covers the atmosphere above it. This covers the model within the cavity debris pool. The fission products models are involved there -- the other models are unique to this part of the code.

As you can see, most of the models are

operational. Three are still under development, and one is yet to be worked on.

So as you heard previously, the model (whispering - inaudible).

DR. KERR: What is the schedule for completion?

DR. CLAUSEN: I hadn't planned to go into that in any detail. I would say that if we expend our efforts primarily on completing the model, then we could finish up probably in another few months, maybe half a year, to the extent of the LMNCR-related models.

I feel we should turn our emphasis towards testing and using of the codes to check any of the features. As a consequence, some of these models may be delayed. As far as having a fully checked-out version with all the models, that could be about a year, I would guess. It could be pushed somewhat, with a corresponding reduction in the (inaudible) for that to happen.

Okay, this summarizes the status of the atmosphere specific models. Again, most of them are operational. A couple of parts remain under development. There is one model (inaudible) with the hydrogen burning. That has not yet been started, but it should be (inaudible) to implement any model that is desired there.

Okay, finally a few general features, the input-output related matters. All of these are essentially operational.

Let me spend a few minutes on an example, which was the problem we worked on by request from NRC. As you may recall, it was the Zion/Indian Point study starting last winter. We were requested to look at some of the aspects of the March -- -- calculation regarding the TLMB-prime scenario. And in particular we looked at some aspects of the steam spike that was generated.

Well, let me describe that in a little bit of detail here. We took the steam and aerosol generation rates as being specified; that is to say, we did not calculate the generation of steam or aerosol in a mechanistic fashion, but simply took that as an input and tried to follow that reasonably closely but not exactly. What we could determine from the March calculation.

In this particular calculation it used the atmosphere model, not the cavity debris pool model, and the relevant physics here were the multi species aerosol model, the surface condensation; that is to say, on the containment wall surface, heat transfer. It had multi-cell, two-phase flow though the particular results I will show are for a single cell.

Okay, followup fission product transport and decay of fission product. This was viewed by us as in part a test of CONTAIN of the atmospheric calculations prepared through a relatively simple model. It was also a test for a check on the March code calculations. In addition, we investigated to a limited extent, did a limited sensitivity study. In particular, we investigated the effects of the gas conductivity. This is the gas between the -- I believe it was steel wire entered in the concrete containment building.

One result I will show -- we won't have the time, I won't go into further detail -- these show the pressure in the containment building, due to the steam -- -- due to the steam generation. The solid line you can see is the March result and the two other lines are CONTAIN results with two different values of the gas conductivity.

First of all, you notice that all of these are in the same ballpark. There is perhaps a one atmosphere pressure difference between top and bottom on these calculations. So that our first conclusion is that both March and the CONTAIN are in the same ballpark.

The differences between March and CONTAIN are most likely due to differences in the input steam generation rather than detailed physics, or at least we can't assert one way or the other at this point.

The second point concerns the effect of the ga conductivity. As you can see, between the top and the middle line of the early part of the calculation, or at the peak of the pressure, the top of the bottom line, there is about a one atmosphere difference in pressure, depending upon whether you use a high or a low productivity for the gas.

First of all, there is some uncertainty as to what this conductivity is. But more relevantly, there is also some uncertainty in what the gas find is, for example, for the overpressure of the containment building. The net result is you can have a significant difference in pressure depending upon what the conductivity is.

The third point I would make related to what was said earlier, and this is an inference that one can make with a little bit of hesitation; that is, the decay of the pressure following the steam spike takes place with a time constant of order of hours, not minutes.

What this suggests is that there has been some suggestions that if the steam is generated over a longer period -- I believe the suggestion is 10 or tens of minutes rather than a shorter period -- that that will mitigate the steam spike. I would suggest -- I would not -- but I would suggest that with this long decay time, which is due to the condensation of the steam on the wall, that it matters little whether the steam is generated over seconds, minutes, or tens of minutes. It becomes to become important, as you would guess, if the steam is generated over a long period of time, meaning hours or longer.

With that I believe I will --

DR. SHEWMON: RCB stands for what?

DR. CLAUSER: Reactor Containment Building.

DR. SHEWMON: And this is done with the sprays off?

DR. CLAUSER: This is done with the sprays off, that is correct.

DR. KERR: You compare the results of -- excuse me, did I interrupt you? -- of March, and at a meeting in Washington in the 20th of May, at which March, those calculations with this was discussed, and we had some discussion -- -- I think it is, commented that the March code was designed as a risk tool and it was used to predict probability of containment failure, for example, and that it would do that reasonably well, because this was sort of an integral phenomenon, but that he did it, would not advocate its use in calculating rate -- phenomena, for example.

But you seem to be comparing March's time behavior against CONTAIN's time behavior, when I would have anticipated that the people who developed March don't trust its time behavior particularly.

DR. CLAUSER: Well, let me see. If you are referring to the time at which these various events occur, this time for example is (inaudible). As I say, that was input. So there is no surprise that these occurred at the same time. Is that what you are referring to?

DR. KERR: I am simply asking if you recognize the limitations of the design of the March put on its use, and as your comparison of the results or behavior of CONTAIN with the behaviors predicted by March.

DR. CLAUSER: I think I am in agreement with you. I am not making any big claims that this verifies CONTAIN forever and anon, not at all.

DR. KERR: I am not trying to be critical. I don't know enough to be critical. I am just trying to then understand what I should conclude from that graph, what should it tell me.

DR. CLAUSER: Okay. The magnitude of the pressures and to some extent the time -- -- is determined fairly simply by things like equations -- of the two-phase constituent in the atmospheric by the condensation rate on the wall -- that is what produces the decay the pressure -- and the heat transfer on the wall goes along with that.

I believe those are the two main features of the physics that control it here. So what we would claim here is that CONTAIN is in reasonable agreement with relatively simple models for a relatively simple problem. What you would expect.

DR. KERR: How different would those three curves be if we agreed it was unreasonable, and they certainly don't lie one on top of the other?

DR. CLAUSER: Well, there is perhaps a 15 percent disagreement if indeed there is a disagreement. As I indicated, I think the differences are primarily due to differences -- well, the difference between March and CONTAIN in the two cases, they should be similar, which I believe is the dotted line and solid line. I think those differences are largely due to differences of steam.

We were in a hurry and we didn't take a lot of care in making those example comparisons. Rather we were looking for ballpark type comparisons.

To answer your question more directly, if there is an error in the equation -- -- the pressure as a function of temperature, I would guess that the pressure would be proportional to that error. There is some compensating factors perhaps.

Likewise, an error in heat transfer and/or condensation rate would affect -- the proportional -- -- being the decay rate of that pressure.

DR. WALKER: I am not sure I can improve on this much, but let me try to comment. When we got into this zip study there was concern with March, since it was not developed for this particular purpose. There was a lot of questions about the simplicity of the model, particularly the condensation model, the pressure in late times, and the aerosol term. And that is the part of the problem that we said well, let's gin up a code here that has a much more sophisticated mechanistic treatment of that part of the problem and use the input term which was generated by March.

DR. KERR: You are doing me a favor by assuming that I asked a much more sophisticated question than I asked. I really am looking at the graph and it is supposed to tell me something, not what it tells me, that the agreement is very good, the agreement is lousy, that the codes agree, disagree --

DR. WALKER: Okay, let me tell you what we concluded.

DR. KERR: -- but they are in reasonable agreement with each other and with what we now --

DR. WALKER: Yes, and let me say, I think what we would conclude is that using a much more mechanistic treatment for the condensation and the aerosol part of the term, which he has not talked about, but the aerosols are also in general agreement, that the results that had been used for the zip study, that being the March result -- let me say it this way: it gave us more confidence in those particular results.

Addressing the pressure spike and the generation term we have no basis on which to make that comparison because they both use the same basic source term. So, yes, we were able to get some confirmation using a much more detailed mechanistic code, unverified nevertheless, with a much more simpler model, and the fact that those agreed gave us some confidence that we were working with approximately the right predictions for the zip study.

DR. SHEWMON: Could you tell me what is happening from two to five hours and then what happens at the fifth hour?

DR. CLAUSER: I don't recall the details, but basically there is some steam generated during this period and there is a much stronger steam poin -- --

DR. SHEWMON: There is a much faster pressure drop in what I guess is the lower thermal conductivity there for some reason, which is interesting.

DR. PICKARD: Dr. Shewmon, this is a very simplistic model that (inaudible)

DR. SHEWMON: Between what and what?

DR. PICKARD: (inaudible)

DR. SHEWMON: Yes, that is the only heat sink. You got an -- battery system. You slowly boil off water and you -- on the heat sink.

DR. KELBER: That is the meaning of it to me, and it actually has less to do with containment than it does with the Zion/Indian Point problem. And that is that this heat sink might under conditions of high pressure and the gap gets closed because of liners deforming, be somewhat more effective. It gives you a little bit more margin and one thing that we are concluding is that these large dry containments may have enough strength to stand this spike, particularly with this little bit of margin that you are afforded so that while it is clearly marginal against failure there may be a safety factor which is actually larger than one.

DR. KERR: Well, I guess I am a little bit reluctant to draw any conclusions about the containment from a model which has all the deficiencies I have heard about.

DR. KELBER: Yes.

DR. KERR: I like Mr. Pickard's explanation, which is that it demonstrates that you are using the same steam table. That one I can understand.

DR. KELBER: Yes, but I think the question of the gas conductivity is also pretty straightforward.d

DR. CARBON: Jack, I think we are to the elevated temperature design point. Did you intend to pass out a sheet of paper to us there?

DR. WALKER: Yes. Let me just repeat what I told you earlier. Dr. Torrance, who is the director of this work, could not be here. He prepared a statement. I was prepared to read it to you verbatim. I think that due to the lateness of the hour you could read it to yourself as well.

If you have some questions or comments I would be happy to respond to them in writing if that is okay.

DR. KERR: I agree with Mr. Walker, even though I haven't heard him read before, I am disappointed. I won't get to hear him again.

DR. WALKER: If you would like to stick around at the bar --

(Laughter.)

VOICE: Mr. Chairman, I wouldn't mind seeing five minutes of movie as long as it was carried this far.

DR. CARBON: Fine.

(A film was shown.)

DR. CARBON: That is all we need from the reporter at this moment.

(Whereupon, at 5:25 p.m., the committee meeting was adjourned.)

Doc# 0d PDC

CERTIFICATE OF REPORTER

This is to certify that the attached proceedings

before: NUCLEAR REGULATORY COMMITTEE

In the matter of: Public Meeting Advisory Committee on Reactor Safeguards - Subcommittee on Advanced Reactors -

Name of Proceeding: NRC-Sponsored Research on Advanced Reactors at LASL and Sandia

Docket No.:

Place: Los Angeles, California

Date: June 30, 1980

were held as herein appears, and that this is the ORIGINAL transcript thereof for the files of the Department.

Reporter

CAmp

PROMPT BURST ENERGETICS
OBJECTIVES

WITHIN THE PROMPT BURST REGIME:

* IDENTIFY AND CHARACTERIZE THE PRESSURE SOURCES
  - FUEL VAPOR
  - FUEL COOLANT INTERACTIONS (FCI)
  - FISSION PRODUCTS

* INVESTIGATE WORK POTENTIAL

* INVESTIGATE OTHER DISASSEMBLY PHENOMENA
  - REACTIVITY EFFECTS
  - PIN FAILURE MECHANISMS

* DEVELOP ANALYTIC MODELS TO UNIQUELY DESCRIBE THE OBSERVED PHENOMENA
  - PIN FAILURE MODEL
  - FCI MODEL
  - FUEL EOS MODEL

PROMPT BURST ENERGETICS
RECENT RESULTS

PBE-13S

- 2760 J/G
- LATE FAILURE
- MULTIPLE SMALL SCALE FCI

PBE-14S

- 4000 J/G
- PIN FAILURE DURING PROMPT TRANSIENT
  - INITIAL TRANSIENT - FUEL VAPOR ONLY?
  - SECOND MASSIVE TRANSIENT - FCI

PIN FAILURE STRONGLY INFLUENCED BY

AXIAL RESTRAINT ON PIN

SINGLY RESTRAINED PIN ⟹ LATER FAILURE

FCI IMPORTANT IN DISRUPTED GEOMETRY

IN OXIDE-SODIUM SYSTEM

ENERGY CONVERSION COMPARABLE BETWEEN

OXIDE + CARBIDE FUELS

EXPAND PIN FAILURE MODEL ACCURATE WITHIN

UNCERTAINTIES IN NECESSARY DATA BASES

-- MOLTEN FUEL TO CLAD HEAT TRANSFER

-- CLAD RUPTURE DATA

Future Single Pin Experiments

PBE-15S -- Sept. 1980

PBE-16S -- Oct. 1980

- Correct fuel temperature profiles
  - -- double pulse
- Reduced heat losses
  - -- nuclear heated wall
- Examine effects of coolant
  - -- subcooling (500°C, 700°C)
- Fresh oxide in sodium

PBE-17S -- 1-31-81

- Determine source of initial pressure transient in oxide experiments

Like 14S except tin coolant
-fresh oxide fuel

PBE-1I -- 4-30-81

PBE-2I -- 6-30-81

- Examine fission product effects on energetics
- Preirradiated mixed oxide - PNL-3
- Correct fuel temperature profiles
  - -- pulse from power
  - -- or double pulse
- Different energy deposition

SUMMATION/PBE PROGRAM

IN $\simeq 20$ EXPERIMENTS TO DATE WE HAVE EVALUATED THE WORK POTENTIAL OF CORE MATERIALS UNDER DISASSEMBLY CONDITIONS.

VOIDED CORE -- WORK POTENTIAL OF FUEL VAPOR IS VERY LOW

SODIUM IN -- WORK POTENTIAL OF FUEL VAPOR IS DOMINATED BY THAT DUE TO SODIUM VAPOR.

-- UNDER SOME CONDITIONS HEAT TRANSFER TO SODIUM OCCURS EXTREMELY RAPIDLY. IN THESE CASES SODIUM VAPOR WORK POTENTIAL CAN BE VERY HIGH (FCI's).

-- TRIGGERING OF FCI'S APPEARS TO BE IMPORTANT.

-- THESE CONCLUSIONS HOLD FOR BOTH FRESH UO_2 AND FRESH UC.

EXPAND PIN MODEL

PURPOSE

TEMPERATURE, PRESSURE, AND FAILURE ANALYSIS FOR PBE TESTS

FEATURES

* TRANSIENT HEAT TRANSFER CALC
* ACCURATE GAP MODEL
* FUEL EOS
* LMP-LIFE FRACTION FAILURE CRITERION

APPLICATION

* PBE (5-10ms FWHM) FAILURE PREDICTED WITHIN A FEW ms.
* TREAT AX1 (220ms FWHM) WITHIN 40ms.
* CABRI A SERIES - NO FAILURE OR LATE FAILURE AT LOW POWER (OUTSIDE OF PREDICTIVE RANGE)

AREAS FOR IMPROVEMENT

* RECORRELATE HEDL FAILURE DATA VS EFFECTIVE STRESS
* MEASURE MOLTEN FUEL-CLAD HEAT TRANSFER COEFFICIENT

COARSE DISPERSED MIXTURE (CDM)
FCI EXPERIMENTS
STATUS

PRESSURE TRANSDUCERS ORDERED

FUEL RELEASE MECHANISM UNDER DEVELOPMENT

PISTON DIAGNOSTIC - 2 M TRAVEL - UNDER EVALUATION

EXTERNAL TRIGGER MECHANISMS UNDER EVALUATION

CDM-1 -- SEPTEMBER 80

FD Test Matrices

FD-1 (completed) -- multi-pulse scoping series, demonstrated unexpected potential for gross fuel swelling (SAND79-0940).

FD-2 (in progress) -- square wave heating at LOF-typical power levels. Ultra-clean fuel handling. Variation with burnup and Linear Power Rating. --Energetic dispersal seen in FD 2.4.

FD-3/HRR (in progress) -- cooperative program with UKAEA. Purpose - investigate the early disassembly potential due to fission gas dispersal of irradiated fuels.

FD-4 (Funding by West Germans) -- use of advanced modes to closely simulate LOF conditions. Higher power levels vis-á-vis FD-2.

FD-5 -- LWR fuels

FD MODEL DEVELOPMENT

-- FISGAS STATE-OF-ART FISSION GAS SWELLING AND RELEASE CODE. MULTI-NODE, FUEL MECHANICAL RESTRAINT THROUGH CREEP, INTRA AND INTER-GRANULAR BUBBLE MODELING.

SAND78-1790

-- TIGRES EXPLORATORY FUEL DISRUPTION MODEL--INCLUDES SIMILAR MODELS TO FISGAS, BUT SIMPLIFIED; IN ADDITION TREATS FUEL DISPERSAL BY CRACK LINK-UP AND CHUNK BREAKUP.

SAND80-0328

-- LOF, LOF-TOP AND TOP FUEL FAILURE AND DISRUPTION CODE--UNDER DEVELOPMENT. BUILDS ON LAFM, TIGRES, FISGAS, A CHANNEL BOILING HYDRAULICS PACKAGE AND A SIMPLE CLAD MOTION MODEL.

FD-2 TEST MATRIX

Expt.	Fuel	Degree of Disruption
FD2.1	UO_2/F	COMPLETE
2.2	"	THRU CLAD MELT, FUEL INTACT*
2.3	"	" " " " "
2.4	MO/I, PNL 10-12**	COMPLETE
2.5	" 10-60	COMPLETE
2.6	" 10-60	THRU CLAD MELT AND FUEL SWELLING
2.7	" 10-60	COMPLETE
2.8	" 11-57	COMPLETE
2.9	" 11-57	THRU CLAD MELT AND FUEL SWELLING
2.10	" 11-57	COMPLETE

* 40% U-235; All Others 68% Enriched

**Contaminated with Atmospheric Gases

PNL	Burnup %	Power Rating, Kw/Ft
10-12	5.4	8.0
10-60	5.4	7.0
11-57	11.0	8.5

BASED ON THE HRR SCOPING CALCULATIONS

FOUR INITIAL HRR TRANSIENTS WERE PLANNED

TEST DESIGNATION	FUEL TYPE	PREHEAT TIME (S)	PREHEAT TEMP** (K)	THERMAL RAMP (K/MS)
HRR-1	FRESH	2.0	2240	~100-110
HRR-2	PNL-11	2.0/2.3*	2240	~100-110
HRR-3	PNL-11	2.0/2.3*	2520	~ 40-50
HRR-4	PNL-9	2.0/2.3*	2520	~ 40-50

*PREHEAT TIME CHANGED FROM 2.0 S TO 2.3 S FOLLOWING HRR-1 TO ENSURE CLADDING DRAINING

**AT $R/R_0 \simeq .84$ AT START OF RAMP

FOUR TESTS DEMONSTRATE DIFFERENCE IN BEHAVIOR RESULTING FROM USE OF:

HRR-1 VS HRR-2 - FRESH FUEL VS IRRADIATED FUEL

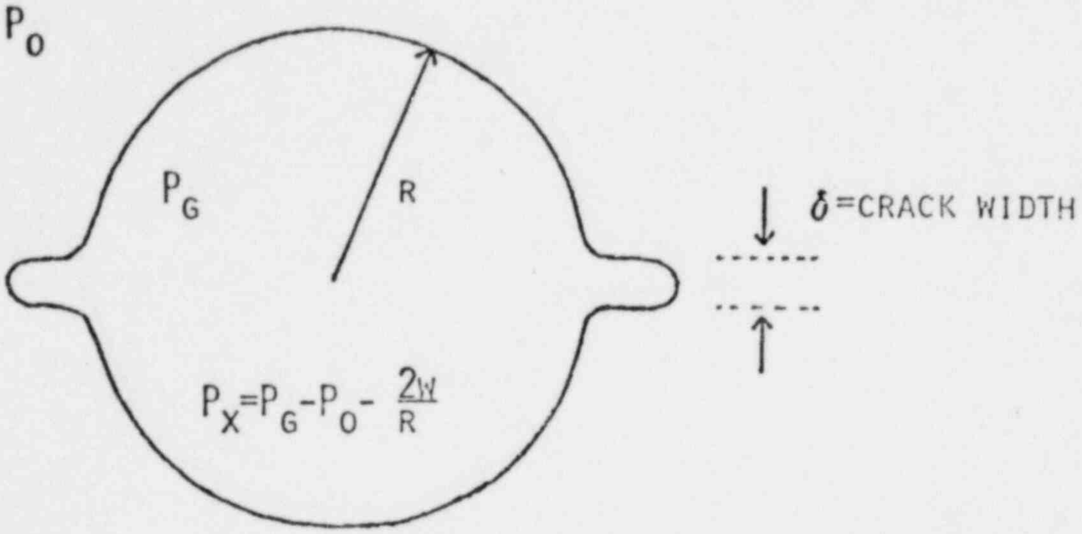
HRR-2 VS HRR-3 - 2240 K PREHEAT VS 2520 PREHEAT

HRR-3 VS HRR-4 - PNL-11 MICROSTRUCTURE VS PNL-9 MICROSTRUCTURE

PNL-9	PNL-11
4.9% B.U.	4.7% B.U.
16.3 KW/M	33.1 KW/M
NO CENTRAL VOID	CENTRAL VOID WELL DEVELOPED

HRR/FD

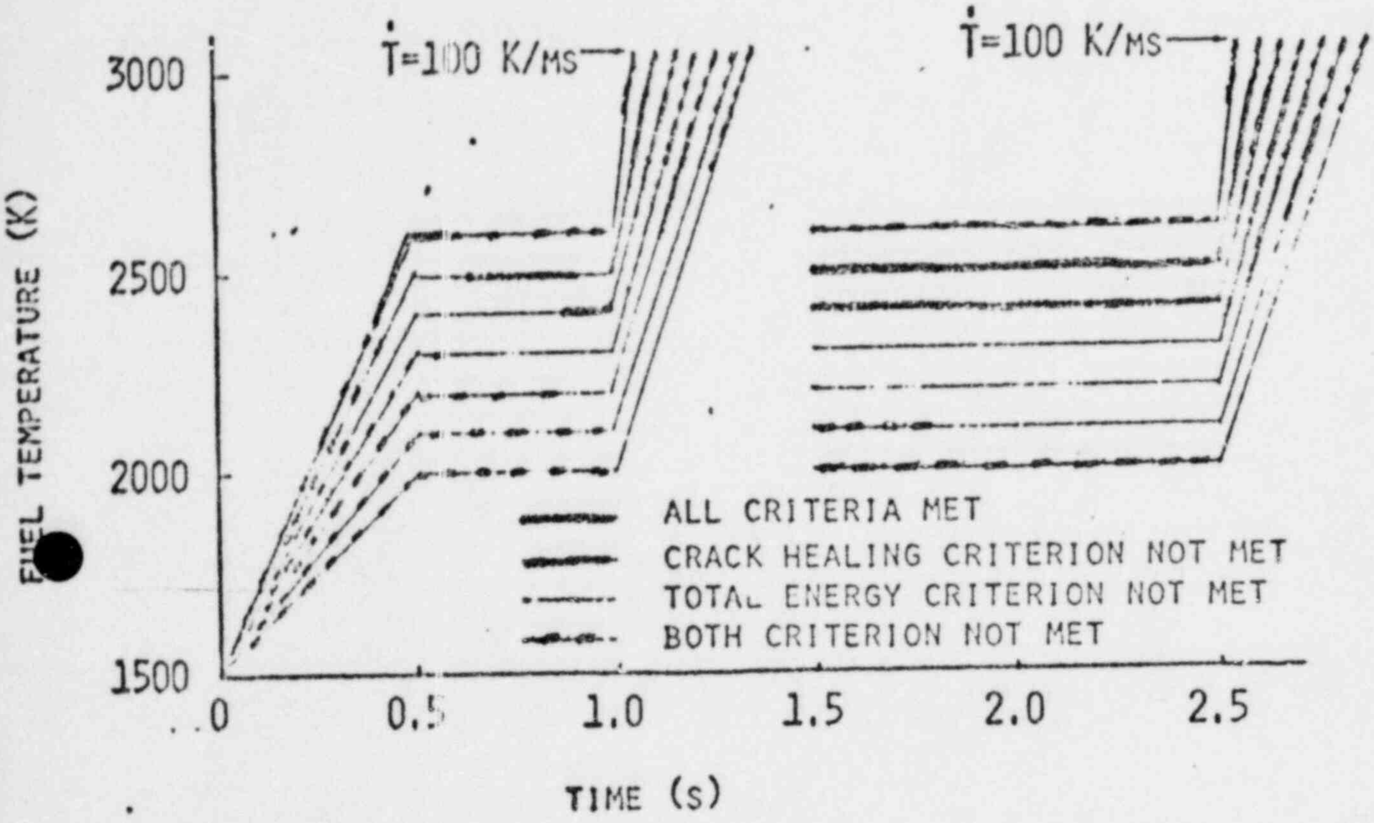
The Potential For Solid Fuel Disruption Has Been Investigated By Looking At 5 Crack Propagation Criteria



Stress Criterion	$P_X \geq \sigma_{Y/F}$
Differential Energy Criterion	$P_G \geq \frac{2\gamma}{\delta}$
*Total Energy Criterion	$E_{GAS} \geq E_{SURFACE}$
*Crack Healing	$\dot{P}_G \geq 0$ $\dot{V}_{CRACK}$ > VACANCY FLOW FROM CRACK TIP

*The Important Limiting Criteria

IDEALIZED HRF TRANSIENTS WERE ANALYZED FOR FUEL DISRUPTION POTENTIAL



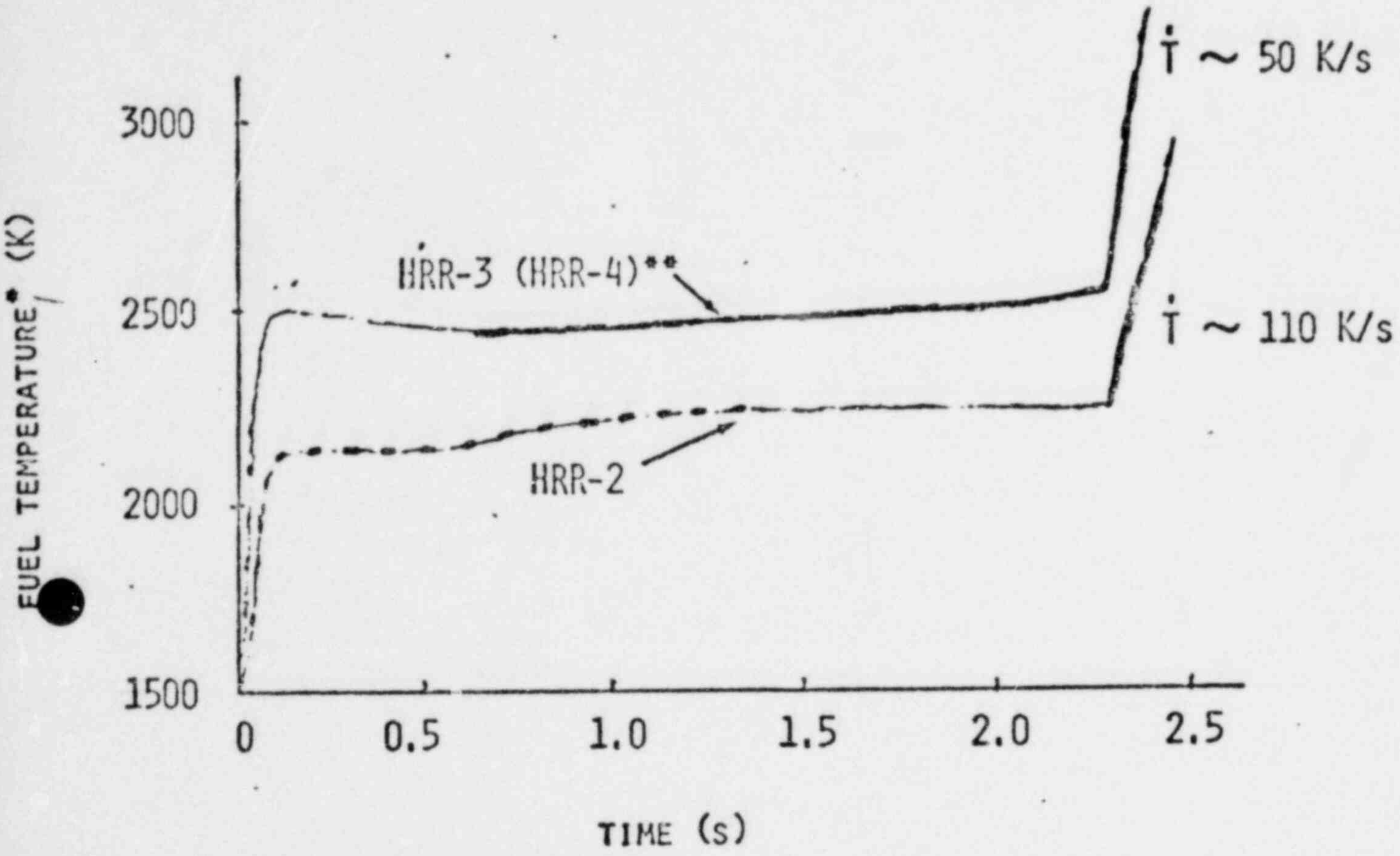
CONCLUSIONS:

-- POTENTIAL FOR DISRUPTION NOT DEPENDENT ON THERMAL RAMP RATE FOR 50 K/ms ≤ $\dot{T}$ ≤ 200 K/ms

-- POTENTIAL FOR DISRUPTION NOT DEPENDENT ON LENGTH OF PREHEAT FOR 0.5 s ≤ PREHEAT ≤ 2.0 s

-- POTENTIAL FOR DISRUPTION MOST STRONGLY A FUNCTION OF PREHEAT TEMPERATURE. DISRUPTION MOST LIKELY FOR 2300 K ≤ PREHEAT TEMP ≤ 2500 K

Pretest Analysis of HRR-2 and HRR-3 Showed Fuel Disruption in Both



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*AT $R/R_0 \simeq .84$

**BECAUSE THE FISSION-GAS CONCENTRATION IS ~ THE SAME IN THE PNL-9 AND PNL-11 PINS, THE THEORY CANNOT DISTINGUISH BETWEEN THEM.

FD-4 TEST MATRIX

Conduct of the Experiment: Preheating of the fuel to a center temperature of 2000-2300 K, then transient heating with 10^3 or 10^5 K/s up to near the temperature of fuel meltdown (Type 1). In two experiments, this temperature should be exceeded (Type 2).

Fuel Rod Tests (Designation, Con$\overset{\text{B.U.}}{\text{sum}}$ption, Enrichment, Rod Power, Transient)

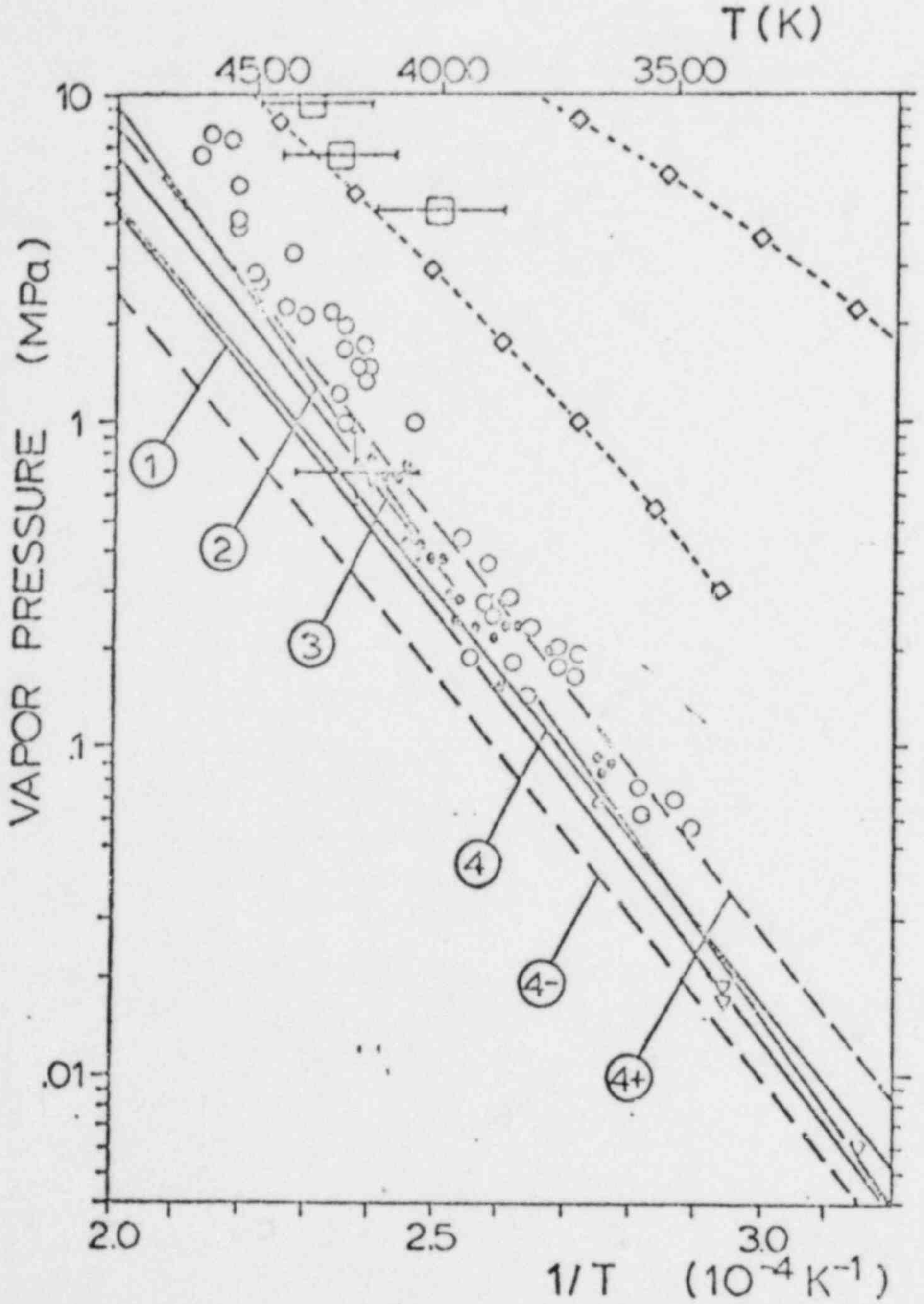
Exp. Type I:	PNL 9	4.9%	40%	170 w/cm	10^3 K/s
	PNL 9	8	40	170	10^3 (and 10^5?)
7 tests up	PNL 11	4.7	67	350	" " " "
to melt	PNL 11	11	67	"	"
temp only	PNL 15	2.5	22	150	" " "
Exp. Type 2:	PNL 9	8	40	170	10^3
exceed melt	PNL 11	11	67	350	"
temp 2 tests					

Total 9 tests typical for the LOF is 10^3 K/s, for the LOF/TOP 10^5 K/s.

FD-4 TEST MATRIX (CON'T)

Objective: Study the Response of Fuel Pins under SNR 300 Typical Conditions, for Hypothetical LOF Accident Sequences, and Investigate the Thermokinetics and Dynamics of Fuel Disruption Processes which might result.

			SNR-300 Fuel			
			300 W/cm (reference fuel)		440 W/cm (advanced fuel)	
Fuel Dispersal	Preheat	Accident Ramp	2% Burnup	5% BU	2% BU	5% BU
yes	~2300K	Reference ~1500 K/s	X	X	X	X
"	"	large 4000 K/s		X		
"	modify pre-heat from 2300 K	Reference ~1500 K/s		X		
No	?	Reference		X		X
No	?	Large		X		


T(K)
4500
4000
3500
10
1
.1
.01
VAPOR PRESSURE (MPa)
①
②
③
④
4-
4+
2.0
2.5
3.0
1/T (10^{-4} K^{-1})

EOS TEST MATRIX

	Fresh	Fresh	Irradiated	Fresh
Sponsor	UO_2	UC	$(U,Pu)O_2$	
Sandia (NRC)	1	4	3	---
KfK (PSB)	2	---	---	2

PICKARD

ADVANCED REACTOR ACCIDENT ENERGETICS PROGRAM

P. S. PICKARD
ADVANCED REACTOR ACCIDENT ENERGETICS DIVISION

W. J. CAMP
ADVANCED REACTOR SAFETY PHYSICS DIVISION

ARSR ACCIDENT ENERGETICS PROGRAM

- PROVIDE A DATA BASE FOR THE RESOLUTION OF KEY ISSUES IN CDA PROGRESSION.

ASSESS CDA THREAT TO CONTAINMENT INTEGRITY.

INITIATION PHASE -- FUEL DYNAMICS

- FUEL DISRUPTION
- FUEL MOTION (INITIAL AND EXTENDED)

DISASSEMBLY PHASE -- WORK POTENTIAL

- PROMPT BURST ENERGETICS
- EFFECTIVE EQUATION OF STATE
- FUEL COOLANT INTERACTIONS

TRANSITION PHASE -- POTENTIAL FOR "BOTTLED" CORE

- FUEL STREAMING AND BLOCKAGE
- ABLATION HEAT TRANSFER EXPERIMENTS

DIAGNOSTICS DEVELOPMENT -- CODED APERTURE IMAGING SYSTEM (CAIS)
-- IN-CORE FUEL MOTION DETECTION

FACILITIES -- ACRR OPERATING MODES
-- HOT CELL FACILITIES
-- NA SUPPORT FACILITY

WORK POTENTIAL TASK

PROMPT BURST ENERGETICS

DETERMINE THE COMBINED MECHANICAL ENERGY SOURCE DUE TO FUEL AND COOLANT VAPOR UNDER PROMPT BURST CONDITIONS.

PBE EXPERIMENTS

- SINGLE PIN GEOMETRY
- DRY AND STAGNANT SODIUM CAPSULE
- UO_2 AND UC
- 20 EXPERIMENTS PERFORMED TO DATE
- 14S EXPERIMENT 2/80

FUTURE PBE EXPERIMENTS

- FIVE EXPERIMENTS PLANNED (9/80 - 6/81)
  - TEMPERATURE DISTRIBUTION
  - HEAT LOSS
  - IRRADIATED FUELS
- FMD-2 (8/80)

WORK POTENTIAL TASK

FUEL COOLANT INTERACTION EXPERIMENTS

DETERMINE IF UO_2/NA SYSTEM CAN SUPPORT
LARGE-SCALE PROPAGATING INTERACTION

ACRR FCI EXPERIMENT

- PREDISPERSED UO_2/NA MIXTURE
- EXTERNAL TRIGGER
- PRESSURE, TEMPERATURE, PISTON DISPLACEMENT VS. TIME
- OCTOBER '80

EFFECTIVE EQUATION-OF-STATE EXPERIMENTS

CHARACTERIZE PRESSURE SOURCE FROM FUEL AT
TEMPERATURES OF INTEREST FOR CDA ANALYSIS
(FUEL VAPOR PRESSURE AND FISSION GAS EFFECTS)

EEOS EXPERIMENTS

- PRESSURE CELL WITH PULSED FISSION HEATING
- NOMINAL MATRIX OF 12 TESTS
- COLLABORATION WITH FRG
- UO_2, UC, MO FUELS
- OCTOBER '80

FUEL DYNAMICS PROGRAM

FUEL DISRUPTION EXPERIMENTS

EVALUATE INITIAL DISRUPTION MODES OF PRE-IRRADIATED LMFBR FUEL UNDER SIMULATED LOF CONDITIONS

VISUAL OBSERVATION OF DISRUPTION OF FISSION HEATED FRESH AND IRRADIATED FUELS

- HIGH SPEED CINEMATOGRAPHY
- ACRR -- MULTIPLE PULSE
  -- HIGH POWER SQUARE WAVE
  -- PROGRAMMED TRANSIENT

TO DATE

FD-1	12 TESTS	MULTIPLE PULSE
FD-2	5 TESTS	SQUARE WAVE
HRR (UK)	4 TESTS	DOUBLE PULSE

FUTURE

FD-2	6 TESTS	8/80 - 12/80
HRR (UK)	6 TESTS	7/80 - 9/80
FD-4 (KFK)	8 TESTS	→ FY81

FUEL DYNAMICS PROGRAM

Initiation Phase Fuel Dynamics (IEFM)

- Many Key Issues in LOF, TOP, and LOF-d-TOP Phenomenology Require High Resolution Fuel Motion Information to Resolve (Failure Location, Sweepout, Dispersal Rates)
- Determine Initial and Extended Fuel Motion under CDA Conditions
- Focus on Key Initiation Phase Issues which Determine Accident Progression

IEFM Program Requirements

- Fuel Motion Diagnostics
  - -- Coded Aperture Imaging System (CAIS)
  - -- In-Core Detectors
- In-Pile Sodium Loop
- ACRR Operating Modes

INITIATION PHASE FUEL DYNAMICS

CURRENT ACTIVITIES

- Review I. P. Experiments, Test Requirements, and Facility Capabilities
- CABRI Collaboration
- Diagnostic Development
- Sodium Loop Design and Development

- Review of Existing Data Base (TREAT H, E, R, L series, HEDL, HUT Tests, CABRI A and B series) used to Correlate Fuel Failure Data to Define Facility Requirements

- ACRR Capabilities
  - -- Energy Deposition Available
  - -- Power History Adequate for Higher Ramp LOF/TOP
  - -- Some Control System Modifications Required for LOF-d-TOP, Slower LOF/TOP

CAIS Development Schedule

• FMD-1 Full-Scale System Test	7/79
• In-Core System Development Tests	4/80
• FMD-2 (PBE) Imaging Test	8/80
• Fuel Motion Diagnostics Meeting	Spring 1981

Current Status

- 2.5 mm Radial Resolution
- 2 cm Axial Resolution
- 10 J/gm Required per Frame Imaged
- 20-500 Pictures/Sec Frame Rate

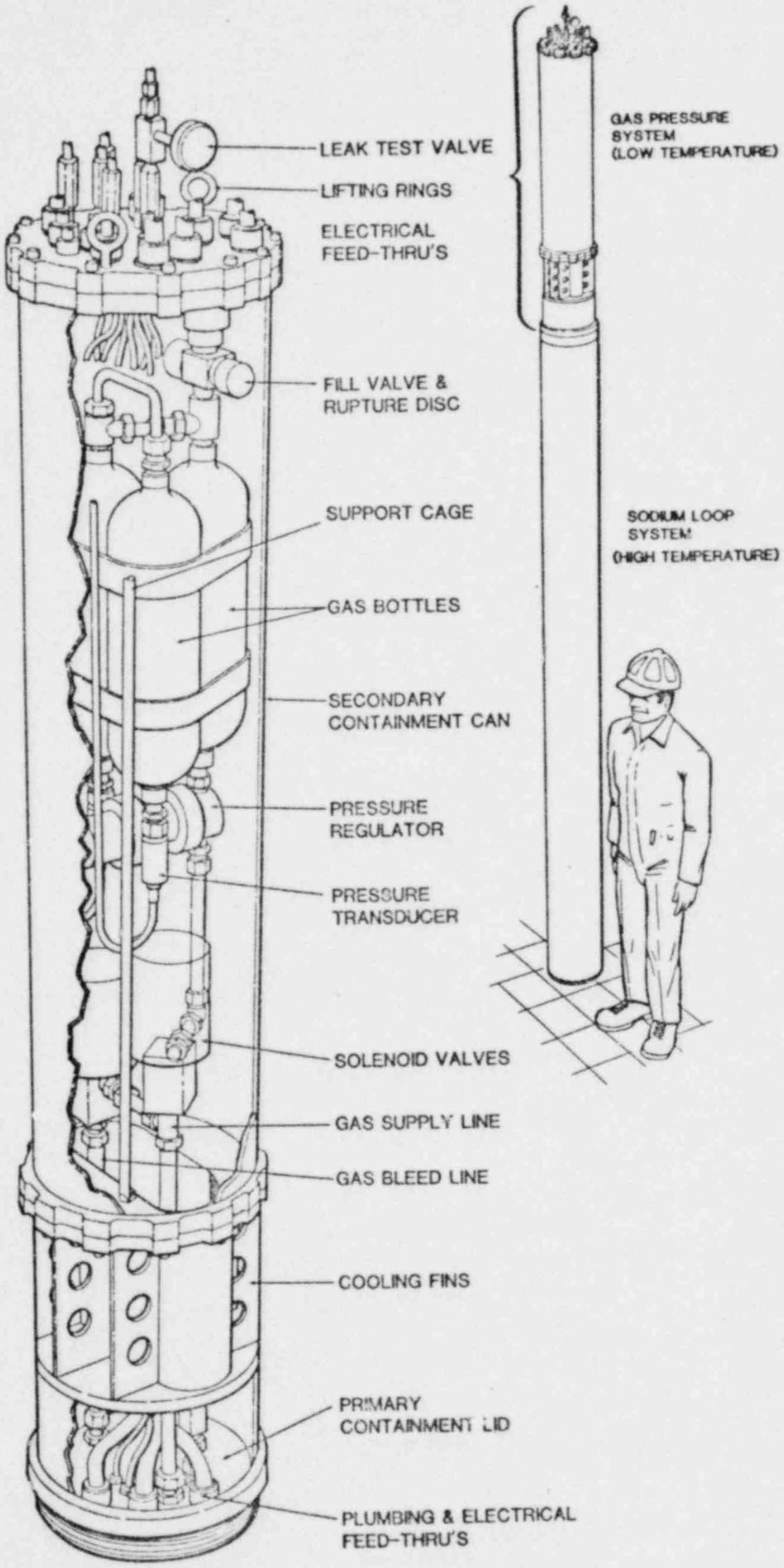
System Improvements

- Further Shielding Improvements
- Improved Reconstruction Techniques
- Advanced Aperture Designs

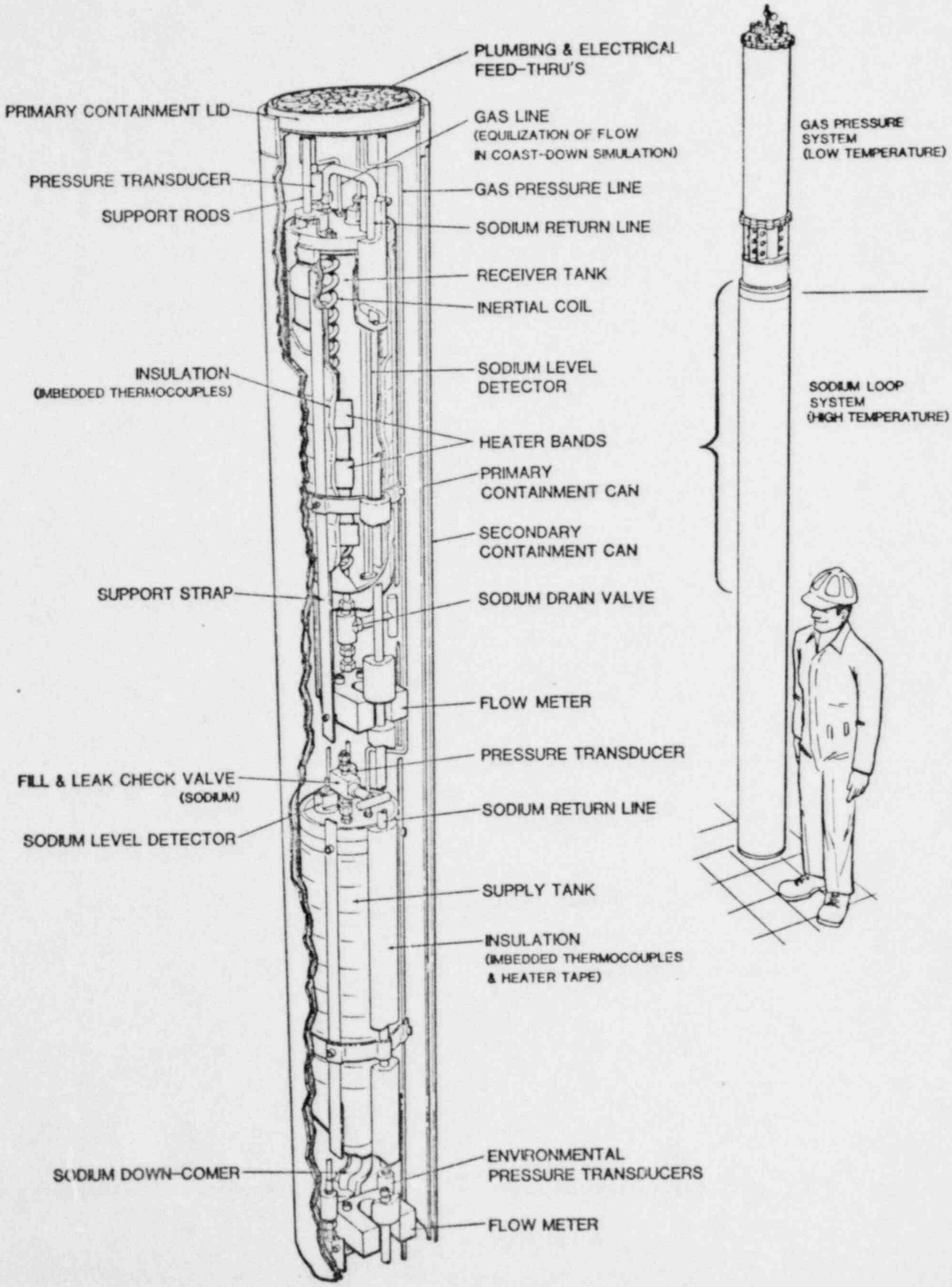
Anticipated Performance

- 1 mm Radial Resolution
- 5 mm Axial Resolution
- 1 J/gm Required per Frame Imaged
- 20-1000 Pictures/Sec Frame Rate

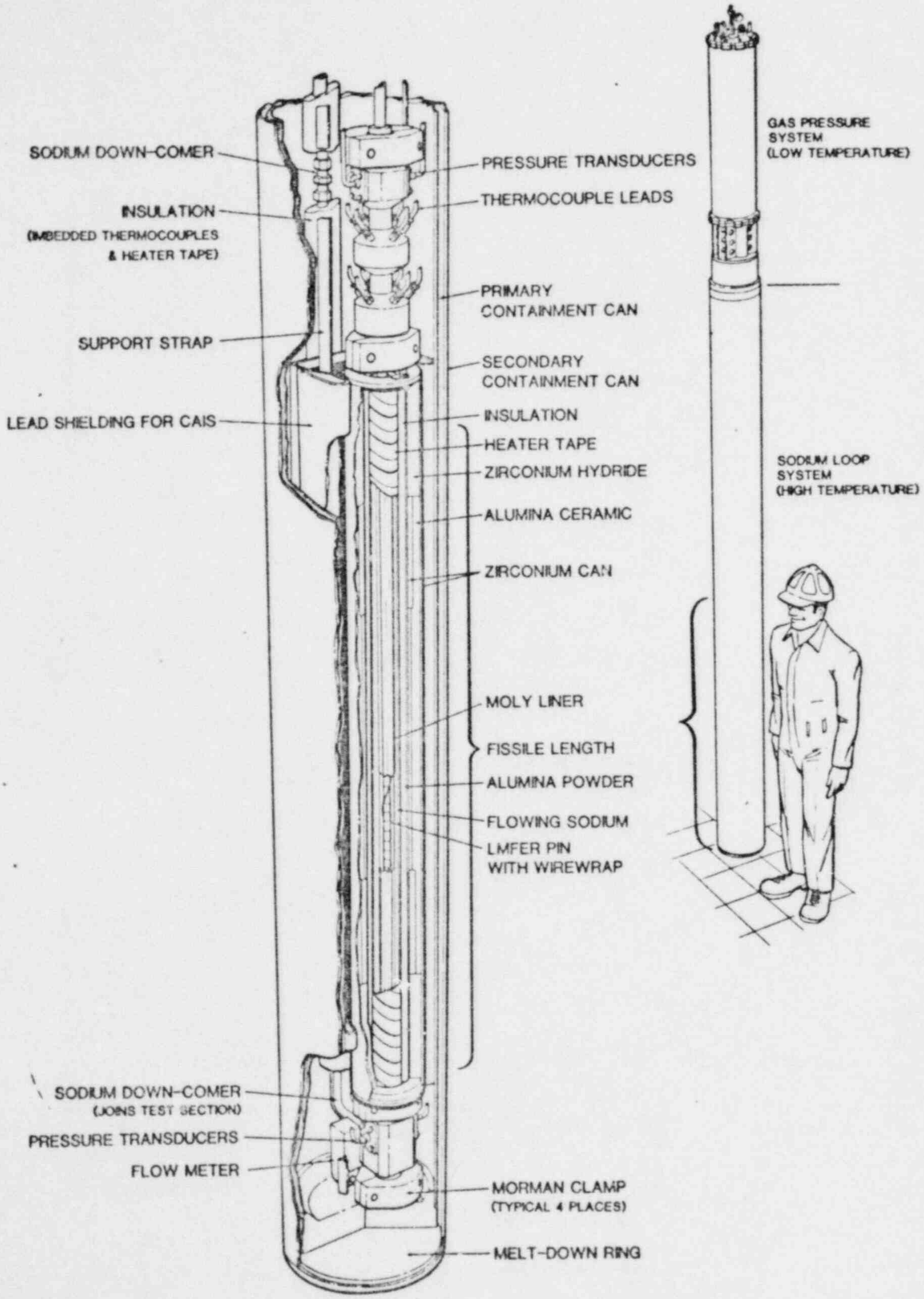
GAS PRESSURE SYSTEM FOR IEFM IN-PILE LOOP



SUPPLY & RECEIVER TANK SECTION FOR IEFM IN-PILE LOOP



TEST SECTION FOR IEFM IN-PILE LOOP



TRANSITION PHASE EXPERIMENT PROGRAM

PRIMARY PHENOMENOLOGICAL AREAS

- FUEL/STEEL FREEZING AND STREAMING
- BOILING POOL BEHAVIOR

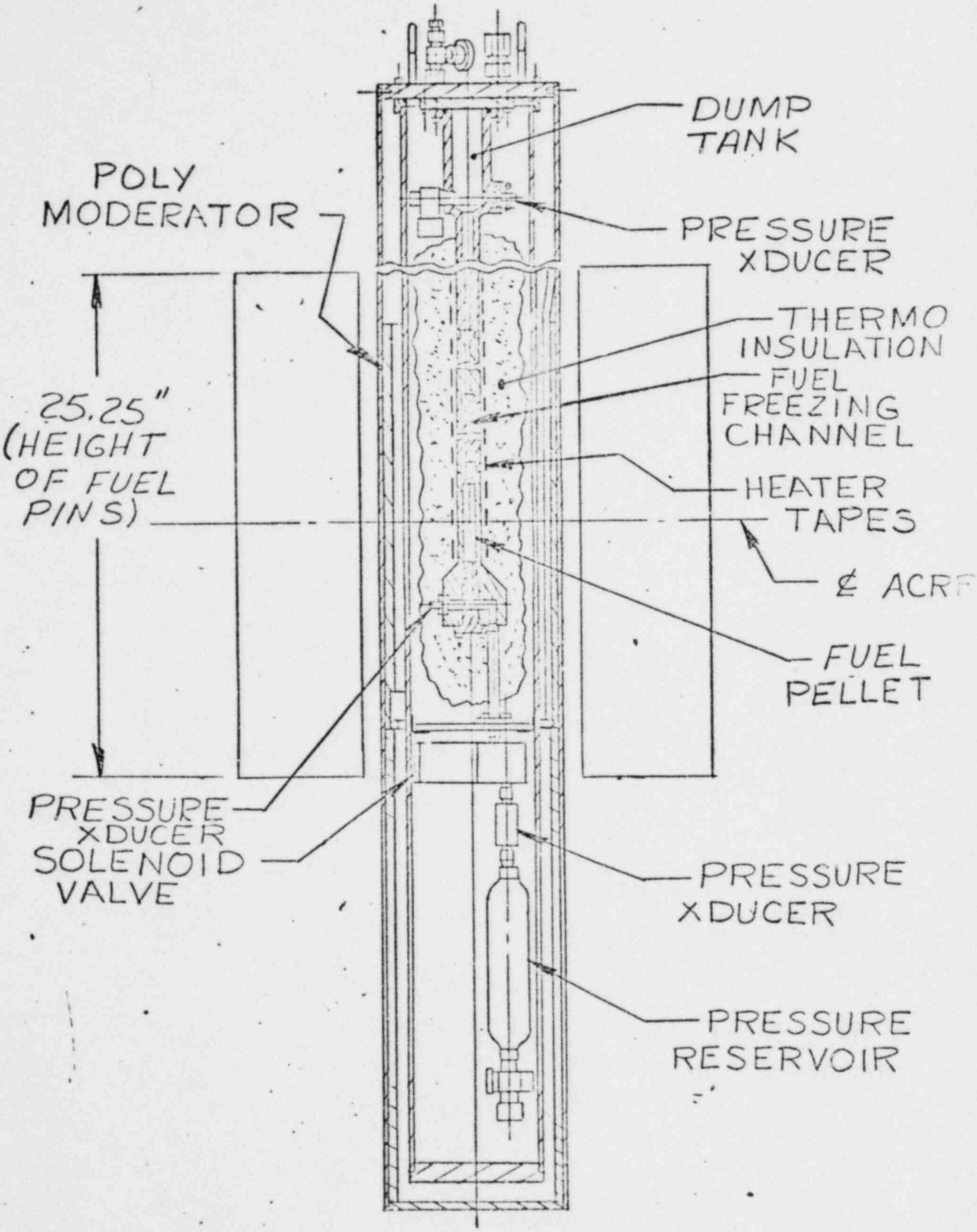
ACRR FUEL FREEZING AND STREAMING EXPERIMENTS

PURPOSE: EXAMINE FUEL/STEEL MELT PENETRATION INTO STRUCTURE AS A FUNCTION OF:

- DRIVING PRESSURE
- MELT TEMPERATURE
- WALL (STRUCTURE) TEMPERATURE
- MELT COMPOSITION
- GEOMETRY

PHENOMENOLOGICAL EXPERIMENTS TO EXAMINE DYNAMIC HEAT TRANSFER MECHANISMS

UTILIZE NEUTRONIC HEATING FROM AN ACRR TRANSIENT TO PRODUCE FUEL/STEEL MELT



ACRR TRANSITION PHASE EXPERIMENT

QUANTITIES OBSERVED

-- PRIMARY VARIABLE:

- DISTANCE OF PENETRATION

-- ALSO OBSERVE:

- STRUCTURE OF BLOCKAGE
- WALL ABLATION
- STEEL ENTRAINMENT
- FUEL CRUST EFFECTS
- MASS OF FUEL IN FREEZING TUBE AND WHICH PENETRATES, IF ANY

SERIES I TEST PARAMETER RANGES:

T_{WALL}: 400-800°C

P_D: 10-100 ATM

T_{FUEL}: 3000-4000°C

GEOMETRY: $\left\{ \begin{array}{l} \text{VARY } D_H \text{ OF TUBE} \\ \text{PIN STRUCTURE} \end{array} \right.$

MELT COMPOSITION

ACRR FUEL STREAMING AND FREEZING EXPERIMENTS

SERIES I (FY80/FY81)

(TUBE GEOMETRY, PURE UO_2)

FY80 PROGRAM

EXPT. NO.	FF-1	FF-2	FF-3	FF-4	FF-5
T_{WALL}(C)	400	400	400	400	600
P_D (ATM)	10	100	10	100	55
T_{FUEL}(C)	3000	3700	3700	3700	3350

(D_H = 3.2 MM)

ACRR FUEL STREAMING AND FREEZING EXPERIMENTS

SCHEDULE	COMPLETION DATE
FEASIBILITY CALCULATIONS	4/80
SAFETY ANALYSIS REPORT	5/80
EXPERIMENT PLAN APPROVAL	6/80
COMPATIBILITY EXPERIMENT	7/80
SERIES I (FY80 PROGRAM)	
FF-1,2	9/80
FF-3,4,5	12/80
SERIES II	
FF-6 - 10	3/81

TEST SUMMARY FY80, 81

ACCIDENT ENERGETICS EXPERIMENT PROGRAM TESTS

PROGRAM	FY 80 (- JUNE)	REMAINING FY 80	FY 81 (1ST HALF)
PBE	14S	(PIE) 15S	16S, 17S
FCI	--	--	CDM-1,2,3
EEOS	VEOS-1	EEOS-D VEOS-2	KFK - SLA 12 EEOS
FD	FD2-1,4 HRR-1,4	HRR, 5-10 FD2-5→6	FD2, 7-10 FD4
TP	--	FF-1,2	FF 2,3,4 SERIES II
IEFM	--	OOP LOOP	IP LOOP
CAIS	FULL SCALE TEST	FMD-2	PBE-11
FMD	7 PIN 19 PIN	15S, FF-1 37 PIN	

TRANSITION PHASE ANALYSIS STATUS

- APPROACH
- MECHANISTIC ACCIDENT PROGRESSION STUDIES

LOS ALAMOS SCIENTIFIC LABORATORY

THE TRANSITION PHASE PROBLEM

- * TRANSITION FROM MULTI CHANNEL MODELLING TOWARD TREATMENT OF THE ENTIRE CORE AS A FLUID.
- * RESULTS FROM FAILURE TO RE-ESTABLISH CORE COOLING AND NEUTRONIC SHUTDOWN.
- * MAJOR OBJECTIVE IS TO ACCESS THE POTENTIAL FOR LARGE ENERGETICS CAUSED BY RECRITICALITIES.

APPROACH

- ATTEMPT TO ENCOMPASS THE MAJOR ACCIDENT PHENOMENA
- INCLUDE THESE MAJOR PIECES IN AN INTEGRATED ANALYSIS TOOL.
- TRY TO BALANCE MODELLING DETAILS, CALCULATIONAL FEASIBILITY, AND COMPLETENESS.
- PREFORM BEST-ESTIMATE ACCIDENT CALCULATIONS AND ASSESS UNCERTAINTIES.
- REDUCE UNCERTAINTIES BY FURTHER ANALYSIS AND EXPERIMENTAL VERIFICATION.

MAJOR PIECES IN SIMMER-II

- MULTIFIELD MULTICOMPONENT TWO-DIMENSIONAL FLUID DYNAMICS
  - STRUCTURE FIELD - 5 COMPONENTS
  - LIQUID FIELD - 6 COMPONENTS
  - VAPOR FIELD - 5 COMPONENTS
- INTERACTIONS BETWEEN COMPONENTS
  - HEAT TRANSFER
  - MOMENTUM EXCHANGE
  - MASS TRANSFER
- SIMPLE FUEL PIN MODEL
- NEUTRONIC COUPLING
  - TIME-DEPENDENT NEUTRON TRANSPORT
  - TIME-DEPENDENT NEUTRON DIFFUSION
  - SHIELDED CROSS SECTIONS

MECHANISTIC ACCIDENT PROGRESSION STUDIES

- CRBR
    - Homogeneous Core
    - End of Equilibrium Cycle
- 1000 MWe Study
    - Heterogeneous Core (CDS)
    - Beginning of Life Core
    - Beginning of Equilibrium Cycle
    - End of Equilibrium Cycle
- BOTH CORES HAVE RELATIVELY LOW SODIUM VOID WORTHS
- LOSS OF FLOW ACCIDENTS ANALYZED FOR BOTH REACTORS

TRANSITION PHASE ACCIDENT PROGRESSION

- CURRENT LMFBR DESIGNS PRECLUDE MAJOR ENERGETICS PRIOR TO FUEL MOTION
- BLOCKAGE FORMATION BEFORE AND AFTER INITIAL FUEL MOTION RETAINS THE FUEL NEAR ITS INITIAL POSITION
- PROGRESSIVELY MORE COHERENT FUEL MOTIONS LEAD TO SUCCESSIVELY LARGER ENERGETIC RECRITICALITIES
- FINAL ENERGETICS DEPENDS ON THE ABILITY OF BLOCKAGES TO RETAIN THE FUEL LOCALLY

THINGS LEARNED

- * BLOCKAGES - RECRITICALITIES
  NO BLOCKAGES - NO RECRITICALITIES
- * IN A NEAR CRITICAL SYSTEM, THE TIGHT COUPLING BETWEEN NEUTRONICS AND FLUID DYNAMICS PRECLUDES THE DEVELOPMENT OF A STEADY-STATE BOILED-UP POOL
- * MASSIVE FUEL REMOVAL IS REQUIRED TO OBTAIN A FAR SUBCRITICAL SYSTEM

THINGS LEARNED

* INITIALLY, SUBASSEMBLY INCOHERENCIES YIELD LOW REACTIVITY RAMP RATES, BUT GRADUAL INVOLVEMENT OF MORE FUEL INTO COHERENT MULTIDIMENSIONAL MOTIONS LEADS TO LARGER REACTIVITY RAMP RATES

* THE ENERGETICS MAGNITUDE IS DETERMINED BY THE OCCURENCE OF PATHS FOR MASSIVE FUEL REMOVAL

CONTROLLING UNCERTAINTIES IN TRANSITION PHASE ENERGETICS

- INITIAL CONDITIONS FROM INITIALING PHASE ANALYSIS
- BLOCKAGE FORMATION (FREEZING AND PLUGGING)
- OTHER FUEL REMOVAL PATHS
- FUEL PIN BREAKUP AND SLUMPING
- INCOHERENCIES
- LOSS - OF - FLOW - DRIVEN TRANSIENT OVERPOWER MODELLING
- LIQUID - LIQUID HEAT TRANSFER

LOS ALAMOS SCIENTIFIC LABORATORY

POSTULATED RECRITICALITY CALCULATIONS
SUMMARY

- POSTULATED CALCULATIONS CAN ONLY BE USED FOR GENERAL GUIDANCE
- INCOHERENCE APPEARS TO PLAY AN IMPORTANT ROLE IN MODERATING THE REACTIVITY RAMPS
- 2-D POOLS HAVE THE UNPLEASANT FEATURE OF INHERENT COHERENCY
    1. Are More Likely to Generate Higher Ramps
    2. Are Subject to Higher Reactivity Swings
- KNOWLEDGE OF THE INITIAL NEUTRONIC STATE OF POSTULATED CALCULATIONS IS ABSOLUTELY NECESSARY

THE USE OF BENCHMARK CRITICALS IN FAST REACTOR CODE VALIDATION

1. CRITICAL EXPERIMENTS ARE PREFORMED IN ZPR-9 FOR TRANSITION PHASE DISTORTED GEOMETRIES.
2. MONTE CARLO CALCULATION USING PRECISE GEOMETRIC REPRESENTATION TO IDENTIFY RESIDUAL ERROR IN BASIC CROSS SECTION DATA.
3. CALCULATION COMPARISIONS BETWEEN MONTE CARLO AND SIMMER NEUTRONICS FURTHER IDENTIFY ERRORS FROM MULTIGROUP CROSS-SECTION PROCESSING AND MODELING ASSUMPTIONS.

LOS ALAMOS SCIENTIFIC LABORATORY

COMPARISON OF SIMMER CALCULATIONS WITH EXPERIMENTAL RESULTS

1. COMPARE THE REACTIVITY CHANGE BETWEEN CONFIGURATIONS.

2. SODIUM VOIDED CONFIGURATION USED AS REFERENCE STATE TO SEPARATE FUEL MOTION FROM SODIUM VOID.

3. SIMMER MODIFIED TO MODEL PLATELET HETEROGENEITIES AND ANISOTROPIC SCATTERING.

4. SIMMER CALCULATIONS COMPARE:
   A) TRANSPORT VS. DIFFUSION METHODS
   B) 9 ENERGY GROUPS VS. 50 ENERGY GROUPS.
   C) HOMOGENEOUS VS. HETEROGENEOUS TREATMENT
   D) ISOTROPIC VS. ANISOTROPIC TREATMENT.

SUMMARY OF RESULTS

1. UNCERTAINTY IN NEUTRON LEAKAGE MODELING APPEARS DOMINANT IN PREDICTING THE EXPERIMENTAL REACTIVITY CHANGES.

2. DIFFUSION THEORY, WITHOUT STEAMING EFFECTS, FORTUITOUSLY PREDICTS THE SODIUM-VOID REACTIVITY WELL.

3. DIFFUSION THEORY IS UNABLE TO PREDICT FUEL MOTION REACTIVITY CHANGES IN THE PRESENCE OF LARGE NEAR-VOIDED REGIONS.

4. TRANSPORT THEORY UNDERPREDICTS SLUMP-OUT AND OVERPREDICTS SLUMP-IN REACTIVITIES.

5. INCLUSION OF PLATELET AND MATRIX STREAMING EFFECTS WOULD MOVE ALL TRANSPORT THEORY RESULTS TOWARD THE EXPERIMENTAL VALUES.

CONCLUSION

1. BECAUSE THE SIMMER-II TRANSPORT RESULTS PREDICT APPROXIMATELY THE EXPERIMENTAL FUEL MOTION REACTIVITY CHANGES, THE SIMMER MODELS ARE ADEQUATE FOR CURRENT INVESTIGATIVE ACCIDENT ANALYSIS.

2. LICENSING SUPPORT WOULD REQUIRE FURTHER ANALYSES AND EXPERIMENTS TO QUANTIFY AND REDUCE UNCERTAINTIES.

LASL 1000 MWe STUDY

OBJECTIVES

- GAIN UNDERSTANDING OF HCDA PHENOMENA IN COMMERCIAL LMFBRs
  - LASL PRIMARY EMPHASIS – TRANSITION PHASE AND RECRITICALITY POTENTIAL
- SUPPORT SANDIA ACCIDENT DELINEATION STUDY
- ASSESS ADEQUACY OF ANALYSIS TOOLS AND DATA BASE

1000 MWe REACTOR (CDS)

I ALTERNATE DRIVER AND BLANKET SUBASSEMBLIES

A) 36 + 72 + 162 DRIVER SUBASSEMBLIES

B) 19 + 30 + 66 BLANKET SUBASSEMBLIES

C) 24 CONTROL SUBASSEMBLIES

II REACTIVITY COEFFICIENT CHARACTERISTICS

A) LESS THAN $2 Na VOID AT EOEC

B) APPROX. -.005 (Tdk/dt) driver

APPROX. -.009 TOTAL

C) SOMEWHAT LOOSELY COUPLED DRIVER REGIONS

III SUBASSEMBLY DESIGN

A) 1.168 (m) ACTIVE CORE

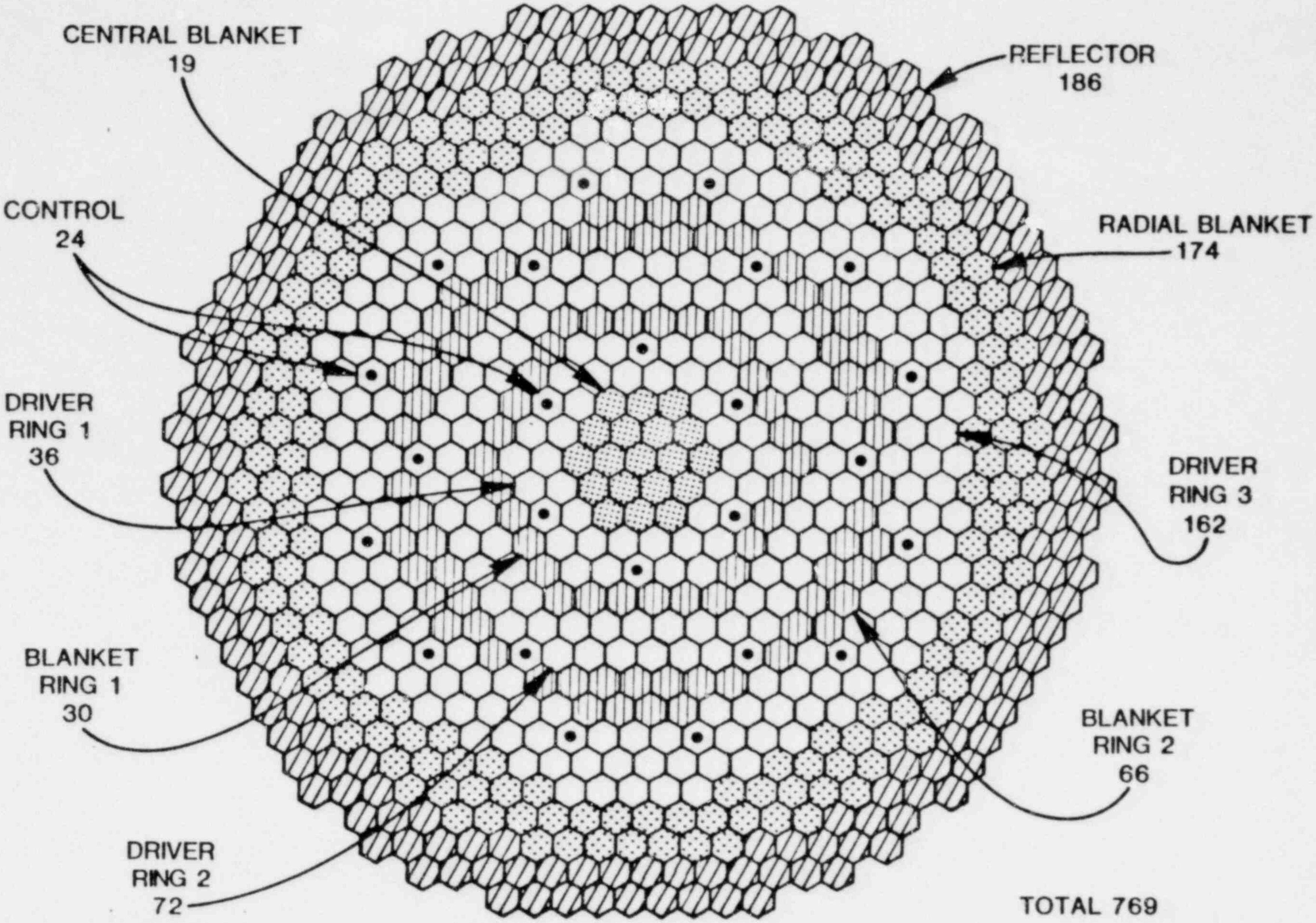
B) 271 PINS/DRIVER

C) 91 PINS/BLANKET

D) FUEL/STEEL/SODIUM VOLUME FRACTIONS

DRIVER : 40/19/39

BLANKET : 57/14/28


CENTRAL BLANKET
19
REFLECTOR
186
CONTROL
24
RADIAL BLANKET
174
DRIVER
RING 1
36
DRIVER
RING 3
162
BLANKET
RING 1
30
BLANKET
RING 2
66
DRIVER
RING 2
72
TOTAL 769

ACCIDENT ANALYSIS

I INITIAL CONDITIONS

- A) UNPROTECTED LOF
- B) BOL, EOEC, BOEC

II INITIATING PHASE MULTICHANNEL CALCULATIONS

- A) USE SAS3D CODE
- B) PROVIDE INITIAL CONDITIONS FOR TRANSITION PHASE

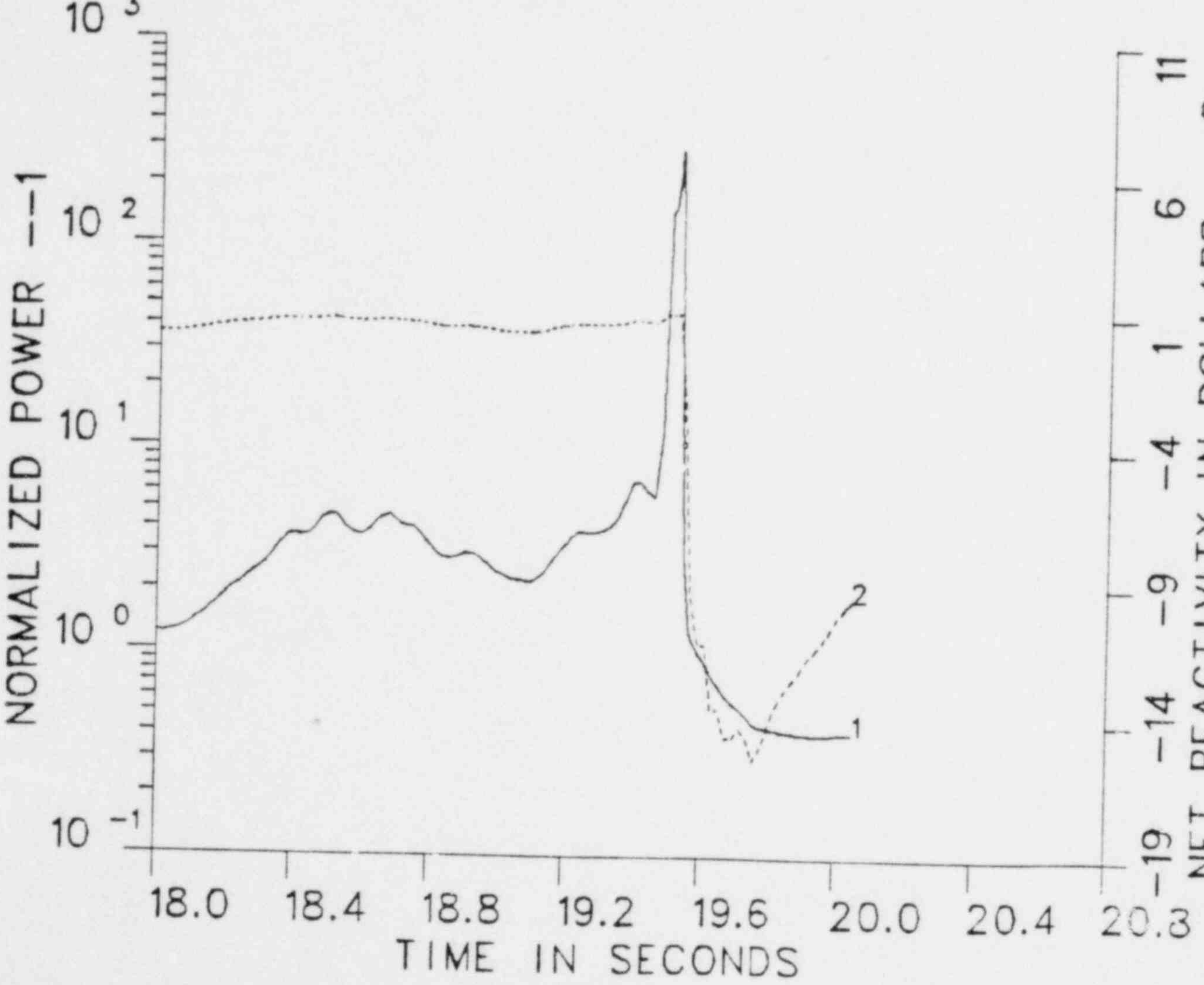
III TRANSITION PHASE CALCULATIONS

- A) USE SIMMER-II CODE
- B) ASSESS ENERGETICS, FINAL CONFIGURATION
- C) SENSITIVITY CALCULATIONS

INITIATING PHASE RESULTS

I LOW VOID WORTH LEADS TO SUBSTANTIAL CLADDING RELOCATION AND SUFFICIENT TIME FOR MASSIVE BLOCKAGE FORMATION

II NO PROMPT CRITICALITY BEFORE FUEL MOTION



BOL - 14 CHANNEL CASE
POWER + REACTIVITY VS TIME

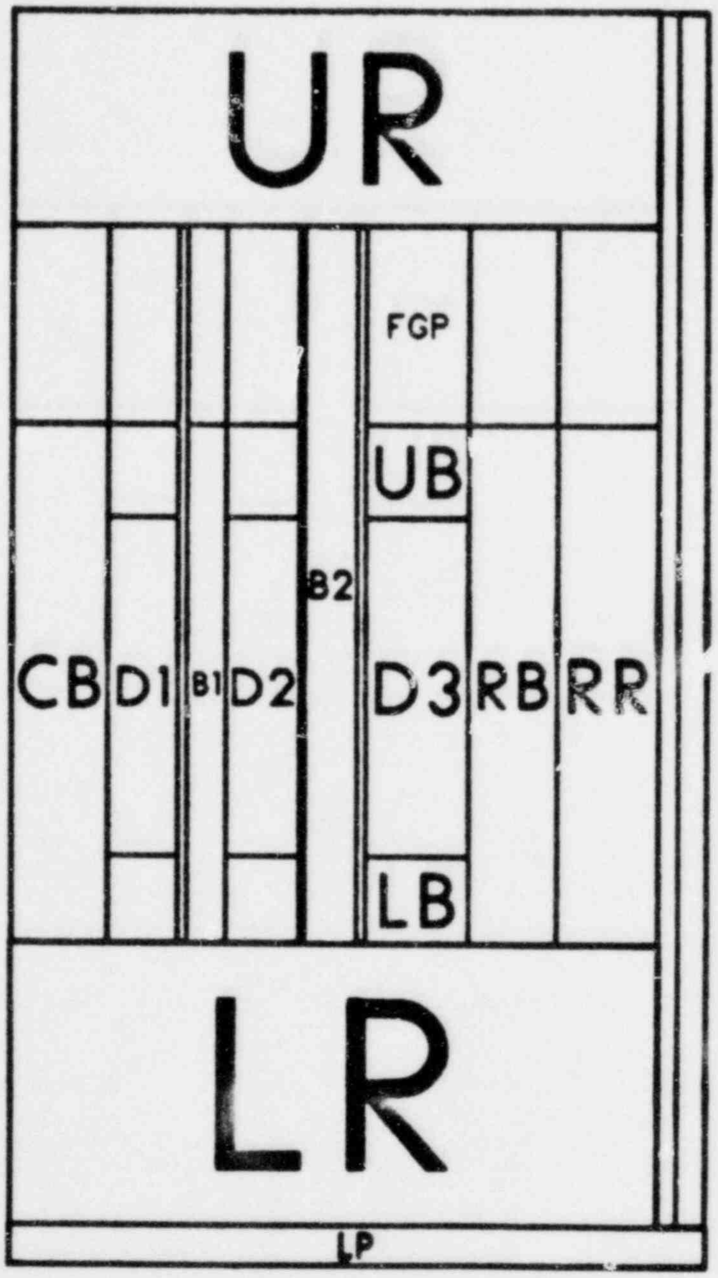
SAS-SIMMER TRANSLATION

I LACK OF DETAILED FUEL PIN MODEL IN SIMMER ENCOURAGES LATE TRANSLATION

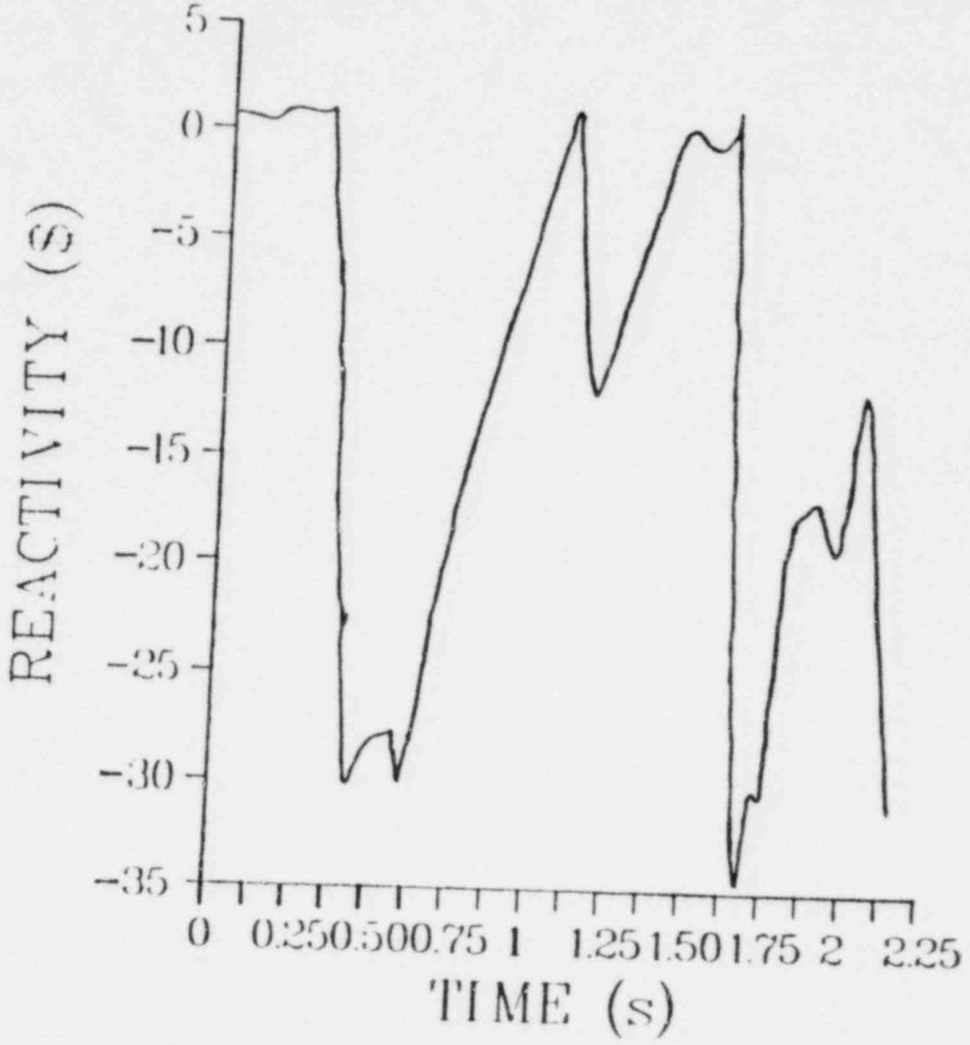
II NEUTRONIC EFFECT OF CLADDING AND FUEL MOTION ENCOURAGE EARLY TRANSLATION

III MECHANISTIC CALCULATION OF BLOCKAGE DEEMED TO BE MOST IMPORTANT

IV THE MANY MODELING DIFFERENCES BETWEEN SAS AND SIMMER REQUIRE A CAREFUL TRANSLATION


UR
FGP
UB
B2
CB
D1
B1
D2
D3
RB
RR
LB
LR
LP

REACTIVITY ($) vs TIME



LASL 1000 MWe STUDY

FUTURE EFFORT

- * FINISH PRELIMINARY CALCULATIONS THROUGH TRANSITION PHASE AND DOCUMENT
- * INVESTIGATE EFFECT OF UNCERTAINTIES THROUGH SENSITIVITY ANALYSES

FUTURE SIMMER DEVELOPMENT

- NEAR-TERM SIMMER-II MODIFICATIONS
- NEAR-TERM MODIFICATIONS FOR LWR CORE DISRUPTION
- LONG-TERM CONSIDERATIONS

NEAR-TERM SIMMER-II MODIFICATIONS

- PROVIDE SUPPORT FOR 1000 MWe STUDY AND UNCERTAINTY ANALYSIS
- USE CURRENT FRAMEWORK
- IMPROVE PHENOMENOLOGICAL MODELS
- IMPROVE EFFICIENCY
- 1000 MWe CASES PROVIDE AN EXCELLENT TESTBED

SIMMER-II MODEL IMPROVEMENTS

- FREEZING AND PLUGGING
- VAPORIZATION – CONDENSATION
- ADDITIONAL FLOW REGIMES
- MULTIFIELD TREATMENT ?
- FUEL – PIN MODEL ?

SIMMER-II EFFICIENCY IMPROVEMENTS

- VAPORIZATION – CONDENSATION
- NEUTRONICS

POTENTIAL NEAR-TERM MODIFICATIONS FOR LWR CORE DISRUPTION

- ISOLATE FLUID DYNAMICS AND DECAY HEAT MODELS
- REDEFINE COMPONENTS AND EXCHANGE FUNCTION PATHS
- MODIFY FUEL PIN MODEL FOR ZrO_2 LAYER
- ADD $Zr-H_2O$ REACTION MODEL
- SIMPLIFY PHASE TRANSITION MODEL
- ADD INTER-CELL THERMAL CONDUCTION

LOS ALAMOS SCIENTIFIC LABORATORY

POTENTIAL LWR CORE DISRUPTION ANALYSIS

- BLOCKAGE FORMATION AND MELTING
- CHEMICAL REACTIONS
- STEAM STARVATION
- TWO-DIMENSIONAL EFFECTS

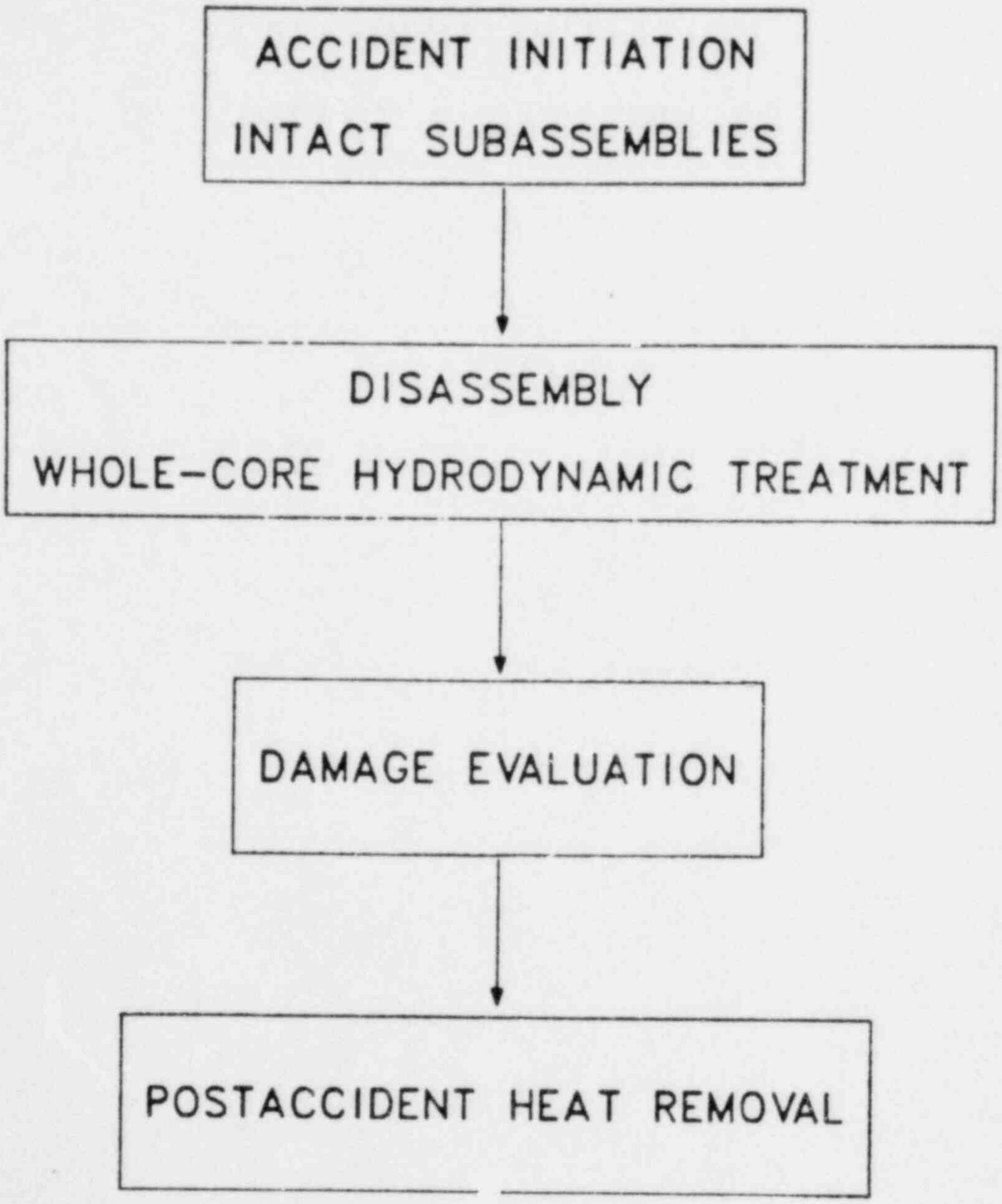
LONG TERM CONSIDERATIONS

- SOME MAJOR PIECES MISSING
  - THREE-DIMENSIONAL INCOHERENCIES
  - DETAILED FUEL PIN MODEL
  - INTERSUBASSEMBLY FUEL REMOVAL PATHS
  - MULTIFIELD FLUID DYNAMICS
  - OTHERS
- SIMMER-II FRAMEWORK LIMITED
- ADDITIONAL MAJOR PIECES REQUIRE METHODS BEYOND THE CURRENT STATE OF THE ART

STATUS OF SIMMER - II ANALYSIS AND VERIFICATION PROGRAMS

Q-7 : REACTOR SAFETY ANALYSIS

TRADITIONAL APPROACH TO MECHANISTIC ACCIDENT ANALYSIS



ACCIDENT INITIATION

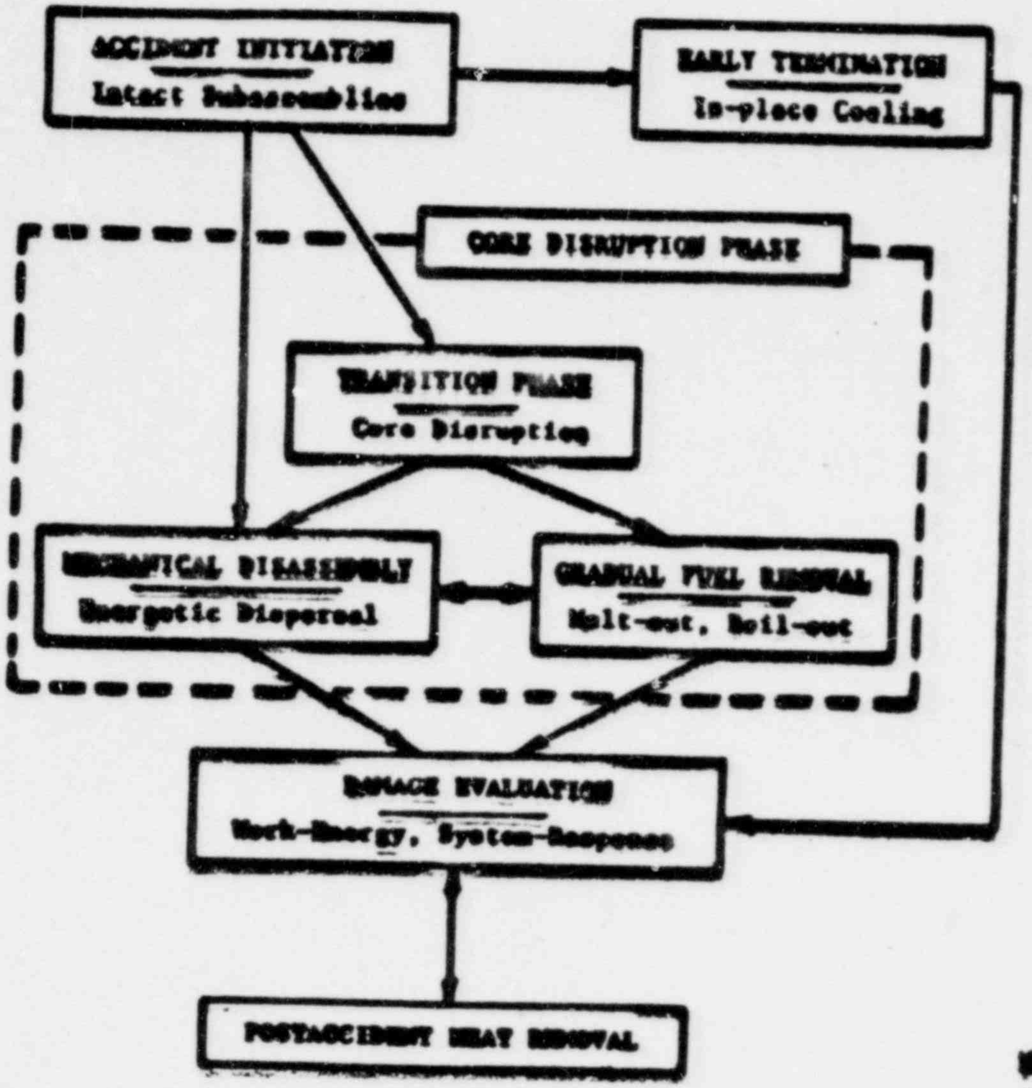
INTACT SUBASSEMBLIES

DISASSEMBLY

WHOLE-CORE HYDRODYNAMIC TREATMENT

DAMAGE EVALUATION

POSTACCIDENT HEAT REMOVAL



ACCIDENT INITIATION

Intact Subassemblies

EARLY TERMINATION

In-place Cooling

CORE DISRUPTION PHASE

TRANSITION PHASE

Core Disruption

MECHANICAL DISASSEMBLY

Energetic Dispersal

GRADUAL FUEL REMOVAL

Melt-out, Boil-out

DAMAGE EVALUATION

Work-Energy, System-Response

POSTACCIDENT HEAT REMOVAL

1. OVERVIEW OF HCDA ACCIDENT SEQUENCES

RESEARCH AREAS

SIMMER ANALYSIS

POST - DISASSEMBLY EXPANSION DYNAMICS

- LONG - TERM MATERIAL DISPOSITION

PLUG EJECTION AND SPRAY CHARACTERIZATION STUDIES

- TRANSITION PHASE ANALYSIS

SIMMER VERIFICATION

- POST - DISASSEMBLY FLUID DYNAMICS VERIFICATION

POST - DISASSEMBLY HEAT TRANSFER VERIFICATION

- NEUTRONICS VERIFICATION

RESEARCH AREAS

SIMMER DEVELOPMENT

3 - D CAPABILITY

ADVANCED FLUID DYNAMIC MODELS

OTHER

FUEL PIN FAILURE (LAFM)

LWR ACCIDENT DELINEATION

LWR PIN FAILURE DYNAMICS AND PROPAGATION STUDIES

SIMMER - II USERS

UNITED STATES	FOREIGN
ANL	KfK
SANDIA LAB.	UKAEA (WINFRITH)
BNL	JRC (ISPRA)
HEDL	
ORNL	
EG&G IDAHO	
WARD	
GE	
AI	
U OF ARIZONA	
UCLA	
BRIGHAM YOUNG	
U OF CONNECTICUT	