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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

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

Wednesday, December 8, 1982

The Advisory Committee on Reactor Safeguards Subcommittee on Advanced Reactors met, pursuant to notice, at 2:10 p.m., Max Carbon, Chairman, presiding.

ACRS MEMBERS PRESENT:

PAUL G. SHEWMON
CARSON MARK
HAROLD ETHERINGTON

ACRS CONSULTANTS:

DESIGNATED FEDERAL EMPLOYEE:

PAUL BOEHNERT

ALSO PRESENT:

A. BICE
ROBERT CURTIS
C. W. KELBER
P. M. WOOD
R. WRIGHT

P R O C E E D I N G S

MR. CARBON: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee On Advanced Reactor. My name is Carbon, subcommittee chairman. The other member present today is Mr. Etherington. Mr. Bender and Mr. Mark are expected to join us soon.

The purpose of the meeting today is to review the NRC Advance Reactor Program for the ACRS FY-84/85 report to Congress on NRC research.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act, and the Government in the Sunshine Act. Paul Boehnert is the Designated Federal Employee for the meeting.

The rules for participation in toda-'s meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Tuesday, November 23, 1982.

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

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

We will pause momentarily.

(Pause.)

MR. CARBON: Let's proceed with the meeting, and I will call upon Charles Kelber, Deputy Director, Division of Accident Evaluation, Office of Research.

MR. KELBER: Thank you.

We have, both in accordance with the Commission's policy and planning guidance and with the recommendations of the Advisory Committee, been formulating some plans for the period beyond the proposed time of the SER for Clinch River. And what I am going to discuss today is largely independent of Clinch River.

MR. CARBON: Let me get some time figures down here. The SER for CRBR is due by April '83.

MR. KELBER: Yes. It may slip.

MR. CARBON: Nominally April 1983, and we are talking here about fiscal year '84, which starts in October of '83.

MR. KELBER: That is correct. Now it is conceivable, of course, that there will be issues that remain and that we will have to address in the framework of the Clinch River SER, and I am not going to discuss those today because it is highly speculative.

But the guidance that we have received and are developing as potentially a long-range plan is to maintain the four objectives listed in this first vugraph. That is to categorize and set priorities among LMFBR safety issues, to maintain the capability to analyze core melt accidents, system analysis capability, and make a more concerted effort to continue to extract information from abroad.

The budget level has not yet been set. It will certainly be somewhat less than we now have.

MR. CARBON: Excuse me. You said guidance that you had received.

MR. KELBER: The policy and planning guidance from the Commission, as well as the remarks of the ACRS themselves.

MR. CARBON: I thought that the policy guidance from the Commission was essentially do not do anything else but CRBR.

MR. KELBER: There have been significant redrafts in the past several months and the latest redrafts indicate the Commission's thinking is along the lines of maintaining a capability in accordance with the plans of the Executive branch and Congress.

Now if a decision were to be made, for example, to abandon or greatly stretch out the breeder development program, of course, we would drop work.

MR. CARBON: You are saying there have been discussions or rethinking or something?

MR. KELBER: That is correct.

MR. CARBON: Is there anything in writing?

MR. KELBER: The Commission has circulated a draft lately -- and I am sure a copy is sitting in Mr. Fraley's office -- of their latest thinking on policy and planning guidance.

MR. WOOD: A first draft was prepared. It was discussed at the Commission meeting. It was very significantly marked up and I believe a second draft is back in the Commission and that is where it stands.

MR. CARBON: So it has not come to us?

MR. KELBER: No, only as a draft document.

MR. CARBON: Those meetings were quite recent?

MR. KELBER: That is correct.

MR. WOOD: The draft was returned to the Commission, I think, just within days.

MR. CARBON: Within days?

MR. KELBER: That is correct.

MR. CARBON: Am I correct in believing that the official written statements at this point are do not do anything except CRBR, but this other is perhaps --

MR. KELBER: In view of the discussion of the Commission meeting, which was an open meeting, I would say that the statements that are, as you put it, in the mill are the ones that are in effect.

MR. CARBON: They are not in writing now?

MR. KELBER: Only in draft form. There has been no reason to suppose that the Commission would suddenly change its mind on this document.

MR. CARBON: You have been basing your presentation on the draft?

MR. KELBER: That is correct.

The purpose here is to prepare us for a follow-on action by the Executive branch, as authorized by Congress, and that is we would expect that following Clinch River there would be another plant somewhere down the line that would require licensing action. And, of course, there is always the possibility that a utility or a group of utilities might decide to take action. However, I think is significantly off into the future.

The first objective clearly is to enable us both to advise the Department of Energy on the direction of their own safety and plant reliability work, to develop some insights into areas where plant reliability and safety can be improved -- again for the purpose of advising DOE -- and also to establish some tentative priority rankings for our own research program whenever the opportunity comes -- the need, rather, let me correct that -- the need arises to expand and to meet our own organizational needs and requirements.

Now we believe that the next two items -- the capability to analyze core melt accidents and system analysis capability -- are required for any safety review and we believe that in addition there is a significant amount of information being generated abroad, particularly in the international EPRI program to which we are junior partners, and in the French program, in particular, to a lesser extent the German work, and, of course, in the Japanese program connected with Monju.

MR. CARBON: Do we have full access? You said we are junior partners.

MR. KELBER: We have full access to the CABRI data. We are negotiating an agreement with France. We have an agreement with the PNC that covers the Monju reactor.

MR. CARBON: Do we have total access to the CABRI data?

MR. KELBER: Yes.

Now what we have not done under the pressure of assisting the regulatory effort for CRBR is spend a lot of time on the analysis of the CABRI data. They are, however, extremely pertinent and we will, if the regulatory needs permit, devote effort to that in the future.

MR. CARBON: For the record here, indicate in what areas the CABRI data are particularly helpful and particularly significant.

MR. KELBER: First, we have developed a rather extensive data base covering a wide range of parameters associated with the serious accidents -- loss of flow and transient over-power high reactivity insertion rates, on fuel failure -- both the timing, the location and the extent to which the clad fails.

We have very good data now on the motion of the fuel through the failure point and its interaction with the sodium in the channel. And, in fact, we are getting better data than we have ever seen from any other tests on the rate of heat transfer from the fuel to the sodium, which is an important parameter in the Simmer analyses, as well as in the SAS code analyses.

We have lesser quality data on the motion of the fuel inside the pin before failure, but we are getting it. So we have some of that, and this is, again, the only source of such data. We have very good data on the axial expansion of the fuel, the function of irradiation during a transient. That is, do you get full axial expansion? Does it expand up to a point and freeze against the clad, or what type of expansion model is needed?

All of these are parameters of great interest for the initial phase of the accident and the CABRI data, which are now focusing on the more violent accidents, was associated with high ramp rates and with the loss of flow accident, are proving to be a source of very high quality data.

In later years, the CABRI program is discussing a program of work at lower ramp rates to cover a range of considerable interest which is not covered by any experimental program yet, and we have in fact been instrumental in giving them a technical basis for such a program, and it is under active consideration and during the next year or year and a half the decision should be taken as to whether or not they will proceed along those lines.

MR. CARBON: Could you also for the record comment on how CABRI data complements, supplements and so on ACRR data?

MR. KEBLER: The CABRI loop is a flowing sodium loop using prototypic fast fuel and a fast reactor, either PFR or Phenix, and is -- roughly it is

80 centimeters long, not quite the prototypic length, which is 107 meters long. But it is a long sample, and is monitored from the point of steady state to the point of failure.

ACRR is strictly a pulse machine. We have entertained ideas of doing a simulated loss of flow experiment, but we are unable to get enough energy out of the core to do a full-scale loss of flow type of test.

The visualization of the fuel is done through a hodoscope in CABRI, and the ACRR does not have such equipment, although we have been developing a similar scheme called the coded aperture imaging system. The difference between the two is that the ACRR is highly suitable for small, very special experiments where you desire full visualization -- for example, the so-called fuel disruption experiments, where we can look in great detail at what happens in a specific process, such as the extent to which fission gas or fuel vapor causes the fuel to disperse.

Now we have just completed the series on fuel dispersion -- both for ourselves and the Germans -- and the test B-5 in CABRI, which is now entering its final planning stage, will put enough energy into a loss of flow accompanied by transient overpower to cause a significant amount of fuel dispersal by vaporization.

The gross effects should be consistent with the type of details that we have observed in ACRR, and to the extent they are not, we can attribute it to the difference between cladding properties, when it has been cooled by exposure to the sodium, as opposed to the ACRR case, where there is no cooling.

MR. CARBON: Another question. Do you expect or are the Japanese running tests that will be of --

MR. KELBER: The Japanese are running tests largely related to the balance of plant. They have done, as you undoubtedly recall, extensive work on steam generator safety and, in particular, on the interaction between the water in the steam generator and the secondary sodium. And we have that information from them.

They are doing extensive work on the structural capabilities of the plant, particularly in relation to seismic work. They have done some work on elevated temperature design, although we have limited access to that, and it is, I believe, of limited utility to us, although we are in the process of negotiating an agreement with the MITI -- the Ministry of International Trade and Industry -- and they may have data which will be of use to us. We have not explored that in detail.

MR. CARBON: Let me ask another general question applicable to ACRR. Do you have to extrapolate a very considerable distance in going from experimental data from either of those pieces of apparatus to the actual case?

MR. KELBER: Yes.

MR. CARBON: You have no choice.

Are you able to sit down on the back of the envelope, so to speak, and not spend two hours but maybe a few days or something, and take pretty much basic first physical principles and show in these rough approximations and calculations -- of course, approximations are all they can be -- that you can come out with results that are, oh, I don't know, maybe within a factor of two or something of what you come out with in one of the experiments?

Of course, I have to define what I mean by a factor of two, but I mean in a general sort of way, can you predict on paper from first principles something that is within the same ballpark as you get from the experimental data apparatus?

MR. KELBER: The experience varies. Our own experience is very limited because we have not been able to spend the resources on comparison -- prediction and examination of CABRI data that we would like to.

There is, as far as I know, no strictly first principles code. There is almost always at least some quasi-empirical model of clad failure and, frequently, a quasi-empirical model of fission gas release from the fuel.

Given that, the experience of the partners in the CABRI program has been that the newly-developed British code, TRAFFIC, does very well in comparison of the prediction to the findings, particularly with respect to the time and location of fuel failure and the motion of the sodium. That is a pretty good test.

MR. CARBON: The TRAFFIC code would basically start with first principles, then?

MR. KELBER: Well, it embodies quasi-empirical models of clad failure and fission gas release that have indeed been adjusted to account for past experience. So it is not competely, by no means, a first principles code. But it is probably the best we can do at the present time.

The German experience and the Japanese experience with their own variations of the US SAS code has been less favorable. The French code PHYSURA is used in a sense as an evaluation model as well as a best estimate model and is used for CABRI loop safety analyses. So it is probably not appropriate at this stage to look at it as -- look at the CABRI tests as a fair test of that code's capabilities.

For example, they consistently overestimate the pressure from the sodium fuel interaction. But again that is because they want to have a margin of safety in the analysis of the experiment prior to its conduct. But PHYSURA is, I believe, a collection of empirical models rather than a first principles code. TRAFFIC comes closest to being the type of code you described, and its performance these last few tests, in the analysis of the last several tests, has been remarkably good.

I have not seen a detailed prediction of the motion of the fuel as expelled from the pin. That would be a final test, it would seem to me, of the capabilities of these codes.

We ourselves would propose to use two codes, one, EXPAND, that has been developed at Sandia, and the other one is LACOBRA, which is again a variation on the SAS code and includes some fuel cladding tests of our own to analyze the CABRI data.

MR. CARSON: Let me make one more comment and ask for your response to it. It has been my own experience with big codes -- nose cone design and that sort of thing -- that always there were innumerable mistakes seen in the code -- silly little things, in some cases; bigger things in others. The only way we were able to get these bugs out was to be able to go through approximations and back-of-the-envelope kinds of calculations and find that gee, there is something that does not look right here, and so we go back to the code.

Where do you stand in evaluating the accuracy of one of these codes, when you can, if you can compare it to some big experiment?

MR. KELBER: The biggest difficulty is in the modeling of the fuel behavior in these transients. If the fuel is heating up relatively slowly, then there is an ample basis of well-established theory to check against. In fact, Mr. Kreismeier, who was here and is now at Sandia, has done some of the test work in that area.

MR. WOOD: Mr. Chairman, one of the things which is done and has been done is that in most cases if you simply the equations in the code sufficiently, you may be able to backtrack to the point where an analytic solution exists, and certainly you should match the analytic solution.

However, in backtracking and simplifying the problem you have probably left out the things that are the basis for having written the code rather than having been satisfied with the simpler solution.

MR. CARBON: But if you have no way to get some sort of approximate check, how do you know what you are talking about?

MR. KELBER: Well, as I said, you can get some approximate checks by the process I mentioned. Again, when processes are very rapid, then essentially the only thing that matters is the system inertia -- the processes are very rapid and portions are large -- and that is a relatively simple problem to check.

Our great interest in the intermediate range is that that is precisely the area that is the hardest to check by the sort of method that you propose. Now here I am talking principally about models of cladding failure. The models that have been introduced of fuel-to-sodium heat transfer, of course, relate to simple models of heat transfer, and there it is relatively easy to check that at least your coding algebra is correct. That has been done.

To some extent you can check that whatever model you have made of fuel motion at least looks under the right conditions as the motion of spheres through fluids because that is a relatively well known problem. But there are relatively few simple analytical checks of the whole problem. That is very difficult to do.

So we look for a variety of experiments and, for example, the clad failure models that we have developed came out of the early experiments at ACRR, as well as the experiments at Treat. These were sort of separate effects experiments that really addressed just one problem.

Going on, the ingredients of the program, as you see, are four. First we would like to do a PRA -- that is, a probabilistic risk analysis. The problem here is which plants. We would be happy to collaborate with DOE in merely a plant reliability rather than a risk analysis of their large plant design.

MR. CARBON: The 1,000 megawatt electric?

MR. KELBER: Yes. If they are in fact going to do that. We have talked about it very informally but not at any level where any action could be taken productively. Nor do I know that they have any idea what their own schedule is.

Monju in Japan will have a PRA done and they have in fact hired a firm -- Energy Incorporated -- to do this. The possibility exists of fruitful collaboration there.

Through the type of work I mentioned on CABRI and other applications of Simmer we would like to investigate whether there is a safety advantage to flowering cores or other designs with a large negative temperature coefficient. As you recall, the French claim a very significant safety advantage because their temperature coefficient is large and negative so that under conditions where they lose cooling and do not have broad scram they get enough negative reactivity simply from the expansion of the core that they do not approach sodium boiling for a period of 20 to 30 minutes, and that is a very significant safety advantage, at least to my mind.

I believe that the latest thinking within the Department of Energy agrees with that. Now I do not know that the flowering core that the French use is the only way to achieve this, but it is an advantage that I think bears a significant amount of investigation, because it affects basically the design of the core and its restraints. So it goes to the heart of the nuclear design.

MR. CARBON: What this is basically is that they have a positive void coefficient, but then when it heats up the top of it can expand out?

MR. KELBER: I believe it actually flowers from the top -- is it the top that flowers out? Yes, I think that is right. The bottom is cold and the top is hoc.

MR. CARBON: It leads to, then, a negative coefficient.

MR. KELBER: That is correct.

MR. CARBON: Is number 2, then, really an exploration of ways to --

MR. KELBER: Well, it is an exploration of --

MR. CARBON: Shutdown rather than -- it has nothing to do with development at Simmer?

MR. KELBER: No. There is code application here and the question is if you have a design which promotes this, what is the effect further on on safety. Does it in effect continue to enhance safety, or are there negative safety aspects that you should also be aware of?

MR. CARBON: And not just flowering cores?

MR. KELBER: That is correct. There are other design options that one would want to look at. The question is are there negative aspects from the point of view of safety to these design options, or they are in general of positive safety.

MR. CARBON: The British, I think, and I ask are you looking at such things as the rods, control rods, the shutdown rods, expanding when they heat up and giving negative --

MR. KELBER: Well, this is always a problem. It is an operational problem as well as a safety problem because during operation, of course, there is a reactivity shift that accompanies the ascension to power because of differential heating which may arise in the core. We are not making any special effort in looking at that because, as I say, it is an operational problem. There is a significant amount of work to be done there.

The next item, the COMIX and the SSC modeling application, again is not code development. It is code application and the question we raise here is -- low flux boiling an acceptable limit is typical of the kinds of questions you looked at. The claimed advantage -- and I think it is a considerable one for an LMFBR -- is that with a direct reactor auxiliary cooling system the reactor can go onto natural circulation without any external source of power.

In the transition to that condition, some components may get quite hot. It is a significant design problem to make sure that the components do not get overheated and yet you have sufficient cooling capability. And the question is what is an appropriate limit for that overheating and what are the safety aspects of the design options that do the best.

MR. CARBON: Well, part of this is saying that you will be exploring whether it is all right to let boiling take place at a relatively low flux.

MR. KELBER: That is correct.

I have already mentioned analysis of the CABRI tests --

MR. CARBON: Before you leave the last one, you were citing that for a large size plant or a smaller size plant?

MR. KELBER: Yes. We have just completed in CABRI recently some studies of a proposed 1,000-megawatt design. We will be looking at large plants.

MR. CARBON: On CRBR, with its direct heat removal service, you do not reach anything like --

MR. KELBER: I do not know what the answers are in the CRBR analysis. They have a different type of heat removal system than the one that is envisaged here. This would be an auxiliary cooling system directly coupled to the primary core and operating without power.

MR. CARBON: Well, what I am trying to get clear is, if you had a CRBR-size plant and you shifted to natural circulation cooling at shutdown, would you get boiling?

MR. KELBER: There limit, I believe, is 200 degrees below boiling.

MR. CARBON: Okay. And are you then saying that on a 1,000-megawatt unit you probably would get to boiling?

MR. KELBER: No. I think you can make a design that will maintain that limit. The question is that in doing that you impose certain limitations on your design which may have by themselves negative aspects in that they expose certain components to undue thermal stress.

Now the question is, can we relax these limits and still be safe, or can we in fact be safer in the sense that stable, low flux boiling may in fact enhance circulation.

MR. CARBON: Then an additional question is, is or are those problems that you speak of more severe in the large plant than in the small.

MR. KELBER: Well, I think that is so design sensitive I would be very hard put to make a blanket statement.

MR. CARBON: So then the item 3 might apply just as well to the CRBR?

MR. KELBER: Yes.

I wanted to mention one negotiation that I did not cover earlier. There is a distinct possibility that we will able to do some analysis of Phenix data on the transition's natural convection, and we are in the midst of negotiations with both the Germans and the French regarding that and similar data from the large loop at the Intratum plant near Cologne.

We have a tentative agreement and it is being discussed at the high levels in France and Germany, and we would try to do analysis of the loop at the Intratum loop, on their vertical injection tests. When we are convinced that we had a good model from the loop we would then do blind predictions of their horizontal injection tests and then compare them with the results afterwards as a means of code verification.

With Phenix we would, of course, have to do essentially post-test analysis. However, there has been some very tentative discussion that if the results contain some unusual features they might be willing to run additional tests to check the various parts of the code prediction.

This essentially would be the range of our activities.

MR. CARBON: Do the British do any testing like CABRI and ACRR?

MR. KELBER: The British program has decreased to a size which is, I suspect, less than our own at the present time. Almost all their resources are being focused on the Sizewell-D plant. There is a small program that is largely focused on the core retention problem.

The tests at the PFR are not a good source of data because of the rather poor quality of the -- poor extent, I should say, of the instruments in the plant. We have been negotiating for years to get some of the actual test data nevertheless, figuring that something was better than nothing in this regard, and we keep running into unaccountable delays. Every time we question about it, we are told that within six weeks we will get the data. This has been going on for, how long? Three years -- at least three years.

We have invested a significant amount of money and we have essentially no return on it.

MR. CARBON: A different question, going back to your bullet number 1. Monju is almost EBR-2 size, isn't it?

MR. KELBER: Monju is relatively comparable with CRBR. It is Shoyo which is EBR-2 size.

MR. CARBON: Okay. Monju, then, is CRBR size. Are your PRA results going to be near as significant for a plant like that, and particularly one that really does not exist yet?

MR. KELBER: This is why this is a problem. There is no obvious choice. Ideally, one would like to have a plant which is constructed and which is reasonably typical, but I have absolutely no hope, for example, of getting the kind of data we would need for PRA out of the Superphenix problem.

Even if a number of people wanted to do it, there are so many different organizations involved in those at Superphenix that you would have to have a whole new crop of bureaucrats negotiating the agreement. Even people in the project over there who are in favor of this type of thing believe that it is just not possible to negotiate.

MR. CARBON: I can believe that.

MR. KELBER: So we have a significant problem here of choosing what plant and my own feeling is we may be best off working with DOE on an analysis, and it would be more toward the type of reliability analysis that has sometimes been described than risk analysis. But we might be better off working with them on the large plant design than anything else.

MR. CARBON: Offhand it seems to me that you unquestionably would be aided, partly because it ought to be easier to cooperate with DOE than with the Japanese, but, more importantly, it is a big plant and ought to be more meaningful. So what is wrong with my analysis?

MR. KELBER: I think there is nothing wrong with that. I agree with you and I think that is probably the direction we will have to go. But first the DOE project has to be made firm.

MR. CARBON: But they have designed a CBS. Of course, it is in preliminary design state.

MR. KELBER: They have not, to my knowledge, fixed on a single design yet and I have been told that in the May to June time period the project review staff will be seeking an audience with the Commission -- whether that will be with Mr. Dircks or the Commission itself, I do not know -- to discuss their plans, and at that time we should have a much better idea where we are going and we would be prepared at that time to initiate these discussions.

MR. MARK: Max, I was wondering about your suggestion that it might be easier to discuss with DOE than the Japanese. I was not quite certain that that was the case.

MR. CARBON: No comment.

Your number 1, then, it says possible DOE cooperation -- Monju and so on. There is a possibility that you will do this on your own; it would be a total

NRC study?

MR. KELBER: I do not think that that is very likely for two reasons. A, the access to the information is, at best, difficult, and that is when the plant is a licensee and you have a certain power of coercion and can still get access to the data. Under these conditions, it would be almost impossible without some form of cooperation.

Secondly, the costs. Our experience is that a well done PRA on a large plant runs on the order of $1.5 million, and we simply are not going to have that type of money in this program. So we are going to have to look for some opportunity to share costs.

Now knowing the way DOE has gone in the past and the type of work they do, I am sure they will be devoting a substantial effort to reliability analysis and that is a significant portion of the cost of a PRA.

MR. CARBON: Another general question. The four bullet things here all look like real worthwhile, important things. But my question is how did you decide on these four and how do you know you are not overlooking others?

MR. KELBER: Well, undoubtedly we are overlooking others, and one of the reasons why we put the PRA first is to try and give us some assurance because we know that are going to, in view of the way the Commission is going on lightwater reactors, we are going to have to look at severe accidents. We know we are going to have to look at the transition to natural circulation.

So we know that we are going to do these things, and in a minimal program do those things which at least you know you are going to have to be prepared to do. But the main reason for putting the PRA first and assigning the priority to it is for insurance against just the type of thing you mentioned.

MR. CARBON: I accept that. How about the other three?

MR. KELBER: Well, as I say, we know we are going to have to look at serious accidents. We know that the transition to natural circulation represents a design feature of LMFBRs that is of great safety significance and that in my view the analysis of safety plays much the same role as the ECCS does in lightwater reactors. It is the primary defense against a loss of cooling type of accident.

The foreign data, this is the best source -- this is, to us, a remarkably cheap way of getting data to make our codes better and we would be foolish not to take advantage of the opportunity.

Now we may be missing a lot. I think the PRA should help us and DOE to focus on the areas where DOE is focusing, the so-called front end of the accident -- the problems that affect --

MR. CARBON: Excuse me. Let me interrupt. I would agree on number 1 and number 4. We ought to get cheap data, and the PRA is important. How do you settle on 2 and 3?

MR. KELBER: Again, the Commission is moving in the next two years to formulate a policy on severe accidents. To extend that to LMFBRs, we are going to have to know something about the extent to which severe accidents in LMFBRs can be treated in accordance with Commission policy. Does it have to be extended? Does it have to be changed? This is not code development; it is application.

And in particular we are looking here at the extent to which the design changes affect the way you look at severe accidents. With COMIX we are looking at a similar question. That is, what are the technical specifications for the plant in the design specification, the design criteria, that are appropriate for the transition to natural circulation.

Do they have any negative effects on safety? We have noted, for example, that there are components which, much to everybody's surprise, are subject to significant thermal gradients under these conditions, although the plant was designed in accordance with everybody's best idea of criteria that allow safe transition to natural circulation. To our mind, that exposes the plant to undesirable levels of stress.

If the design features can be inserted which make it better by some change of technical specifications, we want to know about that.

MR. CARBON: Let me interrupt you again. On number 3 there, is that intended to say -- maybe you said it -- that Item number 3 consists of applications of codes in a systems analysis?

MR. KELBER: That is correct.

MR. CARBON: The example you give is simply one of a dozen of examples?

MR. KELBER: That is correct.

MR. CARBON: So really you are not going to pay much attention to low flux boiling. It happens to be an example.

MR. KELBER: It is one of a number of questions, and the reason it is cited is that current designs emphasize the need to stay more than 200 degrees from the boiling limit. The question is, is that a good criterion? Does it promote safety or does it promote design compromises which are desirable?

MR. CARBON: It is a general consensus of lots of people that these four areas are the ones that are most important, or is this largely your own view?

MR. KELBER: I have exposed this to comment and we have done a lot of discussion ourselves. I would not claim that there is a consensus except I think there is some very substantial number of people who feel this way. Where we have more money, you do more work. Work will expand to fill the budget allowed for.

We picked what we think are the things that we have to do to keep the capability alive in the future.

MR. CARBON: I am just trying to understand better the basis for number 2 and 3 being on there instead of something else.

MR. KELBER: We have to have those capabilities alive for future plants. The other capabilities will be here, but we must have these.

MR. CARBON: Do you feel that 2 and 3 are the most important things that you can do?

MR. KELBER: Given that the DOE program goes along the current pathway, yes. If DOE were suddenly to drop its safety program, we might change our views.

MR. CARBON: It is a consensus sort of thing that 2 and 3 are the most important things that you can be doing?

MR. KELBER: Among a very limited audience. This is the first time that we have discussed it outside of our own shop, and perhaps to a limited extent with NRR, but to a very limited extent, because their focus is really on CRBR.

That really completes my part of the presentation. As you know, I have to go and respond to questions from Mr. Siess and others downstairs. I will try to get back here later on for your conclusions.

MR. CARBON: Let me ask the other members if they have any questions.

You indicated that you don't have very much money and you are severely limited in what you can do. What are your views at this point of how much money you will have compared to what you need?

MR. KELBER: What you really need depends upon the schedule. To maintain this limited amount, we need more than $5 million a year, I would say, and less than $10 in my view. If you were to ask Dr. Ross or Mr. Minogue, their views might be different and they might, indeed prevail.

MR. MARK: Could you get by on $1.5 million?

MR. KELBER: I don't think anybody has mentioned a figure that low.

MR. MARK: I am asking the direction in which Ross and Minogue would go, it would not be five or ten, but less than five.

MR. KELBER: Perhaps. Let me explain the basis for my reasoning. A good PRA effort to be viable requires a minimal expenditure in the range of $750,000 a year to $1 million a year.

MR. CARBON: For a couple of years?

MR. KELBER: Depending on the extent, yes. Yes, for a couple of years.

MR. CARBON: Could it be done more slowly and be good?

MR. KELBER: The experience is that you tend to lose information. There is a certain critical mass because PRA is a kind of system analysis that involves an extensive examination at the direction of systems. If you have too few people, you simply tend to lose information. You have to concentrate on too few of the many topics. With he $750,000 CRBR. How it would work on a new design, I really don't know.

MR. CARBON: When you say $750,000 to $1 million, are you essentially saying seven to ten people actually working?

MR. KELBER: The price per man has gone up somewhat, but, yes, on that order.

MR. CARBON: Six to eight people?

MR. KELBER: Yes.

MR. CARBON: If you cut it in half, three or four people?

MR. KELBER: I don't think that it would work.

MR. ETHERINGTON: The entire amount would be in house?

MR. KELBER: No, we would probably contract out significant portions. Again, this depends entirely on the type of arrangment we make with DOE. If, indeed, DOE were doing a substantial amount of the data gathering, we might simply support in-house staff, yes, if that were the way we were to go.

SIMMER and SAS modeling and application, here we have had a significant amount of experience on what it takes to keep a minimal crew together.

MR. CARBON: Here again this is application.

MR. KELBER: Yes.

MR. CARBON: Most of your experience has been development.

MR. KELBER: In the past couple of years, there has been much more application than there has been development. As you know, with a code like this, there is always a small amount of development, but we really spend very little money on development as such.

If an unusual problem should arise which says that we must do more code development, then of course that would change the picture. Our experience is, and we have had similar experience in Germany and we will find out more from the MANJU experience, that out need to figure on spending somewhere between $1 to $1.5 million in this general area. Depending on how carefully you want to look at the iniating things, that could get very expensive because of the influence of design details.

MR. CARBON: Bullet 2, you guess $1 to $1.5 million.

MR. KELBER: Yes.

COMMIX application, COMMIX is now one of the world's most widely used codes, not because fast breeder reactors are so popularly, but because fluent mixing problems are no pervasive. We have a lot of experience on COMMIX, and there we expect to expend somewhere in the range of $.75 million to $1.5 million, depending upon the range of the systems analysis we get into, because we spend currently on the order of several hundred thousand dollars a year in SSC, but a significant amount of that has been code development for the pool type reactor and others. That has been delayed for CRBR, and we would want to finish that up and we would have better experience after an initial year or so of what it takes.

The analysis of the other data, it is hard to guess at this time, but let me say that if we were to do a full-fledged analysis of PHENIX alone, I would estimate that that would run us at least $250,000 a year.

MR. CATTON: That is the natural recirculation.

MR. KELBER: Yes.

I would budget a similar amount for the analysis of INTRATOM tests. So how much we would spend there depends to a large extent on the type of data we get access to.

MR. CARBON: So you are saying somewhere between a half million and something higher.

MR. KELBER: $1.5 million.

This what I would call a barebones activity. There is no experiments in this. To the extent that people want to do experiments, experimental programs, I have yet to see one that doesn't generate demands for enormous amounts of money. But the experimental programs that we have been running, the deep-bed cooling, the so-called FD series, these have been running on the order of $750,000 to $1 million a year. Which of those we would do depends on the types of decisions that have to be made.

MR. CARBON: You have deliberately not put them on there.

MR. KELBER: I have deliberately them in. I said, what is the minimal program that will keep certain skills alive.

MR. CARBON: And it is this that you have which happened to come out $3 to $4 million a year.

MR. KELBER: I would say that if you had a $7 million program, then you could have a program that was essentially half experiment and half analysis. The scope of experimental programs would be considerably more restricted because of the higher cost.

MR. CARBON: If you had more than $7 or $8 million, would it really not be well spent?

MR. KELBER: It would depend upon the DOE schedule and timing. I can't answer that at this time.

MR. CARBON: If you had $3.5 million for experimental work, what work would you do?

MR. KELBER: First, I would try to close out the issue that has been raised in connection with CRBR, and that is the extent to which the fuel in a coremelt accident does drain from the active region and makes this system sub-critical. But I would delay doing it until we had a better idea of the design conditions, because apparently it is significantly dependent upon some design parameters.

The debris bedwork, I believe, is finishing up. How much we would spend there depends on the timing. Sodium concrete is finishing up now, we believe, unless there is some unusual material proposed, we will need no further work in that area.

Other problems that arise in connection with CABRI analyses and their extension to the whole core may be attacked by DOE. They might be attacked by us. We would have to sit down and go over their program in detail and decide what the interfaces are. Again, this depends upon plans which I believe we will not have until later in 1983.

MR. CARBON: If I followed you, you really didn't point out where you would spend any money for experimental work at this time.

MR. KELBER: Other than the transition phase.

MR. CARBON: That is analytical work.

MR. KELBER: I am talking about the type of experimental work we are doing now in support of CRBR.

MR. CARBON: In PHENIX?

MR. KELBER: In ACRR.

MR. CARBON: The experimental work on coremelt and drainage.

MR. KELBER: That is what I call the transition phase.

MR. CARBON: You said that you would not do that now.

MR. KELBER: I would delay until we have more design detail.

MR. CARBON: How long?

MR. KELBER: Until we had those details and had a chance to do some analysis of what the problems are.

MR. CARBON: Would you have those by Fiscal Year 1984?

MR. KELBER: Yes, I would hope so, if I

understand DOE's plans.

MR. CARBON: So you really would not delay in the context that we are talking about.

MR. KELBER: In 1984, we would do essentially experiment planning and design.

MR. CARBON: How much does that cost?

MR. KELBER: Without knowing more the the details, I don't know, but I would assume that it might cost on the order of $500,000, no more than that. It might be anywhere from $250,000 to $500,000.

MR. CARBON: The second thing you said, which I did not follow you well on --

MR. KELBER: The debris bed cooling experiments will be finished, but if they are not, we will have to budget some money to clear that up.

MR. CARBON: Maybe a quarter of a million?

MR. KELBER: No, a quarter of a million can't do one test. It is more on the order of $750,000 if we have to do it.

Beyond that, I think I can't get very specific at this time. I would have to see what the DOE plant looks like, and what their own plans are.

MR. CARBON: When I added up your figures a while ago, it was $3 to $4.5 million. Now we have added maybe as much as $1.25 million, and anything above that, then, is simply contingency.

MR. KELBER: Yes, and this assumes that there is no work for CRBR. If there is work for CRBR that is a different matter.

MR. CARBON: Is that a good assumption?

MR. KELBER: I don't want to predict the CRBR vote one way or the other.

MR. CARBON: I didn't mean in terms of a vote, but suppose that CRBR continues.

MR. KELBER: If CRBR continues, there will be a continuing load associated with CRBR.

MR. CARBON: A continuing research load?

MR. KELBER: That is right. Then I would assume that that would take the highest priority, and then I would put the PRA as the next highest priority, and after that the analysis of the foreign data.

MR. CARBON: The cost of continuing CRBR work is in addition to that.

MR. KELBER: That is right.

MR. CARBON: What would it cost?

MR. CURTIS: The number I got for the project was $6.5 million.

MR. CARBON: In fiscal Year 1984?

MR. CURTIS: Yes. Research needs would be $6.5 million, and for our planning purposes, I rounded that to $7 million because of the unexpected contingencies that they have not thought of.

MR. KELBER: To the extent that funds were available, I would have as priorities the PRA analysis, the foreign data. The other items, I think, would have to be superfluous if the work on CRBR were continuing, simply because we would not have the people to do the work. They would be fully occupied with CRBR.

MR. CARBON: So if CRBR continues, you have $7 million there, one in the PRA, and one in the foreign data, so you have $9 million.

MR. KELBER: Yes.

MR. CARBON: Nine million if the CRBR continues, and that is all you really can use for fiscal year 1984.

MR. KELBER: That is correct.

MR. CARBON: If CRBR doesn't continue.

MR. KELBER: Then the number drops, but I am not quite sure to what level until we get the word from DOE as to what their design looks like.

MR. CARBON: But nominally, it would be $3 to $4.5 million.

MR. KELBER: We might round it off to $5 million as the upper limit. Perhaps $7 million might be.

MR. CARBON: Another couple for the experimental work. So it would be $7 million if CRBR stopped.

MR. KELBER: Right. This is not necessarily an OMB or Congressional view. It is simply a forecast of what it would take to do this work.

MR. CARBON: I guess one last question I want to ask you, I think that DOE's safety budget is around $35 or $40 million.

MR. KELBER: It has been at that level.

MR. CARBON: The development budget is on the order of $300 million. I know Joe Hendrie once, when he was chairman, told Congress that in his view, his judgment was that NRC ought to be spending perhaps $20 million a year. Does that make sense to you?

MR. KELBER: In the context of the national program in which the breeder reactor was at the highest priority energy development source, it made sense to me and a few other people. It never made sense, however, to the people who authorized or appropriated the money.

MR. CARBON: True.

MR. KELBER: I do not believe that the program in the next few years will be given the same sense of priority. I, therefore, think that the context changes so much that Joe Hendrie would change his remarks today.

MR. CARBON: Harold do you have questions?

MR. ETHERINGTON: I have not questions.

MR. MARK: I haven't either.

MR. MARK: I have one question. Let me make sure that I have the basis correctly. The NRC Appropriation Act ends up with stated number of dollars, I believe $6 million, for research for CRBR.

MR. KELBER: The authorization does.

MR. MARK: It also says that if the CRBR is cancelled, you may do what you like with this money as long as it is something.

MR. KELBER: It says that we may use it for the remainder of the research program.

MR. MARK: Yes, in effect, it allows you that.

MR. KELBER: The reason for that is --

MR. MARK: I can think of very good reasons.

MR. KELBER: There is a legal reason for th wording, because otherwise we are required to notify the Congressional Committees and go through a rather complex process to change our funds from one budget line item to another.

MR. MARK: That is very good. It gives you more freedom.

MR. KELBER: That is right.

MR. MARK: DOE is coming up for authorization or appropriation, because they may only be there for another week and a half.

MR. KELBER: They may be in the continuing.

MR. MARK: Anyway, it is going to have some statement about CRBR, which has an awfully good chance to be voted down, I think.

MR. KELBER: It is always possible.

MR. MARK: If it is voted down, what will the NRC decide to do with you guys? I hope they will continue this work, at least at some level.

MR. KELBER: In view of the discussions lately in connection with the redraft of the policy and planning guidance, and the discussions within the Office of Research, there will be a minimal program, I believe, of the type which I have just described. The extent of that program will depend upon various budget pressures that we do face, and also on the type of schedule put forward by DOE for the national development program, together with the type of priorities assigned to it by Congress.

MR. MARK: You are touching on just the question I had in mind. As long as DOE is, in this particular line of endeavor, wiped out, it would seem to me quite reasonable for you to carry on.

MR. KELBER: I know of no move to cancel the entire breeder reactor program. It well known that there is a strong move to deauthorize Clinch River, but I know of no move to cancel the entire breeder program.

MR. MARK: In that case, you would be reasonably in a well-defined for supporting the kind of work you have mentioned.

MR. KELBER: Our guidance would be to keep pace with the national program as such.

MR. MARK: Thank you.

MR. WOOD: Bob Wright is planning to be here later.

I will make the presentation on the experimental program at Sandia. I am making this presentation in the context the CRBR is continuing, and I will tell about what it is we are doing to help NRR in the licensing activities for CRBR.

Sometime about July of 1981, we were directed by the Executive Director to form a joint program with NRR people to help with CRBR licensing issues, and our activities over the last have been pretty much directed to that job.

Our code development programs have been shifted to emphasize confirmatory calculations for technical assistance, to help people evaluate. Our experimental program has been directed at direct CRBR issues.

This is what we are doing now. We have a fairly good size sodium concrete interaction program. We had some disagreement between what we thought would happen to dolomite concrete and what the other people thought would happen to dolomite concrete.

MR. MARK: Harold, how do you spell dolomite?

MR. ETHERINGTON: D-o-l-o-m-i-t-e.

MR. MARK: I thought so.

MR. ETHERINGTON: You want to know if I was aware, is that it?

MR. CARBON: It is misspelled there.

MR. ETHERINGTON: I see.

MR. WOOD: It turns out that the equilibrium partial pressure CO2 over the two materials has very little to do with the rate of reaction, and that the tests are practically identical. So that problem has largely gone away.

MR. CARBON: I can't remember who said which.

MR. WOOD: Sandia was saying that dolomite would be very bad.

MR. CARBON: Who was correct, and who was wrong?

MR. WOOD: Sandia was wrong, and Cornell never said. They said that if it was bad, they would go ahead and use calcite.

MR. CARBON: In any case, the data were quite separate.

MR. WOOD: Now, since they have run the same tests, they are in reasnoable agreement.

MR. CARBON: So the data has not changed.

MR. WOOD: No.

MR. CARBON: The Sandia data did change?

MR. WOOD: The interpretation. One was sodium monitoring test and the other was a sodium deficient test, that was the real basis. The applicant, and by that I mean the Clinch River Project, has now taken the position that if they do indeed get a coremelt and have a combination of coremelt and sodium reaction, they will get something like five feet of penetration into the basemat, but they can stand it.

That has caused NRR to shift its concerns about areosols plugging up the big ten-inch pipes betweeen the reactor cavity and the containment, and whether the filtered vent system was reliable. So we are probably going to start a new program on the reliability of the safety systems.

MR. MARK: I should know this, but I don't.

You said it has changed NRR's thoughts, in what direction, to become a little more reasonable or more concerned?

MR. WOOD: I would say more concerned. Instead of arguing about whether we are going to get two inches of penetration under the concrete or five inches.

MR. MARK: They realize that we might get five feet.

MR. WOOD: We are not concerned with whether the filtered vent system will be able to handle that load, and will it be reliable.

MR. MARK: So it is asking for more provision to handle the gases outward.

MR. WOOD: Do we have sufficient capability.

MR. MARK: It is the direction of saying, we have to require more provision than we used to do.

MR. WOOD: I would not p t it that way. We have to be convinced that those pipes won't plug. With the smaller amount of aerosols, you probably would not worry that much about it.

MR. CARBON: Isn't it that you no longer have to worry about arguing with the project on how much reaction there is. You can shift your attention to the second, and you are shiftin it, and you don't feel that it is worse than you did before?

MR. WOOD: No.

MR. CARBON: You are simply looking to see.

MR. WOOD: The battle has gone to a different place.

MR. MARK: What are these gases, mainly, carbon dioxide?

MR. WOOD: Carbon dioxide and hydrogen, and sodium and sodium hydroxide, and sodium oxide.

In the systems code area, SSC is --

MR. CARBON: Before you leave the last one, sodium concrete interaction, how much money will you be spending on that, or or will you be answering this later?

MR. WOOD: The budget was $900K, and in our new budget it is $10.5 million. I think we have taken that down considerably.

MR. CARBON: For Fiscal Year 1984?

MR. WOOD: I think that it is $200,000.

MR. CURTIS: The budget said $900,000 for 1983. We are looking right now to the potential to recover a significant fraction of that money for 1983. The 1984 budget is not set because we are convinced that we want to keep the operational capability to reopen this and reuse this test facility if necessary during the period of the hearings, which will be basically in 1984, if something surfaces during the hearings that we don't anticipate, but we don't know how much that will cost.

MR. MARK: You are going to put all the end feeders into this crucible?

MR. WOOD: Let me explain the order of my presentation. I was going to talk about what we are doing right now on all these programs, and then go back and say what I think we should be doing in 1984 and 1985.

Now I am having Brookhaven spend most of their time doing calculations on the thermal hydraulic capability of the plant in normal operation, decay heat removal and natural circulation. They are also doing work on certain accidents for SSC, like pipebreak accidents, station blackout problems, all of these operational things that the system is slow to analyze.

MR. MARK: Do they have anything from the calculations?

MR. WOOD: The calculations with both SSC and COMMIX on the FFTF experience with different power levels. It has been very good. We are doing the same calculations for CRBR. The SSC calculations show that everything is probably all right. The COMMIX calculations won't be done until January.

We are still working on some development of the direct heat removal system, The air heat exchangers that have been tacked on to the CRBR plant, and we are trying to complete the development of the feedwater chain and balance of plant, so we can do initiator accidents.

MR. CARBON: I notice that the heat removal system, is that on DHRS?

MR. WOOD: That goes to steel drums on each one of the loops for the air heat exchangers.

MR. CARBON: But you also have air heat exchangers.

MR. WOOD: Yes.

MR. ETHERINGTON: With all the money that is goingg sodium concrete reactions and the problems with the dolomite, why don't we evade the issue and use those other aggregates?

MR. WOOD: NRR's position is that they don't design plants. The applicant designs plants. My feeling is that they are being stupid.

MR. ETHERINGTON: We might argue, why should the Commission spend a lot of money on licensee's wishes.

MR. MARK: What other aggregates would you have in mind, Harold?

MR. ETHERINGTON: Granite.

MR. MARK: Sodium dioxide based?

MR. ETHERINGTON: That is right.

MR. WOOD: We have recently run a test on some concrete, alumina-concrete, basically high aluminum oxide, together with aluminum cement, and the test failed because the sodium leaked out of it, but it withstood a half-hour of hot sodium without any sign of potash.

MR. ETHERINGTON: That makes sense.

MR. WOOD: If we could convince the DOD to go that way, I would be happy.

MR. MARK: What kind of a problem would be involved in bringing in such a mixture? Would they have to move to Arkansas.

MR. ETHERINGTON: They would ship it, of course.

MR. WOOD: It is used commercially.

MR. MARK: The problem they are facing is that they just want to pick a local rock and use it. Where would you have to go in order to substitute?

MR. WOOD: I am not sure if there a natural aluminum oxide ore, or not.

MR. ETHERINGTON: Down in Florida, they are shipping the granite rocks from Georgia for the seawalls.

MR. MARK: That is from Georgia, which is not really all that far away.

MR. ETHERINGTON: In building highways, you pick the most available aggregate. When they are talking about a fast reactor with all the money that is going into it, I would think that they can spend a little more on shipping.

MR. MARK: I am really with you. I was merely curious, geographically where would you have to go. You are not going to dig it out of the soil in Tennessee. You might have to go as far as Georgia, which is not very far.

MR. WOOD: The COMMIX program, which is at Argonne, we have analyzed the FFTF reactor transients to see if we can calculate the temperatures in the upper-plenum in places. With the code with three dimensions, we run an 80 second transient, 80 seconds real time, and we have good agreement or information of what is in the reactor.

We find in that calculation that there is very serious flow stratification in the upper-plenum and a fairly short temperature gradient next to the outlet nozzles. So DOE is looking into that. We are going to do similar calculations for CRBR.

The other calculation we are going to do for CRBR is an in-vessel calculation of what happens when we have the direct heat removal system taking the heat away from the reactor, does it really work? We take hot sodium out of the top of the plenum, and put it back in the top of the plenum and still cool the core. The only way I know to do that is through three dimensional calculations. So we are planning to have that done by the 1st of February.

With SIMMER, you had a fairly long presentation under the NRR position, so I didn't intend to go into that today. What I learned from that discussion is that the heat that will be leaving is a large amount of fuel removal to prevent criticality.

Bob Wright is supposed to be here to discuss the Sandia program and the ACRR on the fuel removal experiments.

Do you have any questions on that?

MR. MARK: You referred to the substitution. The things that he said two days ago, were they ones that you would give credit to?

MR. WOOD: It all comes down to this question of the real necessity for fuel removal is needed, and I don't have the evidence to convince me yet that it is there.

MR. MARK: That is the kind of question I wanted you to comment on. Mainly, you are not absolutely, solidly with Dale?

MR. WOOD: The concern about this mechanism of fuel removal is one that does not go away that easily for me.

MR. CARBON: one question back here on the DHRS using COMMIX. The DHRS, presumably, will work. What happens if it doesn't?

MR. WOOD: If that had been in the design, and it has been the story, I am not convinced that a designer today would put it there because he has three loops, each one has air heat removal. It seems to me that this is enough redundancy, even though it is all the same kind. It is there because they want a different heat removal system, and three redundancies.

MR. CARBON: In contrast with what you are saying, I think the DHRS is because the NRC required it.

MR. CURTIS: Historically, however, the DHRS was the first.

MR. CARBON: As a sodium purification system.

MR. CURTIS: Then the NRC wanted some form of decay heat removal, and this was added. Nobody had enough confidence in it, and this was evident in the first cut PRA that was produced by the project directors themselves. They were assigning a very large fraction of the potential unreliability to that function.

MR. WOOD: There is another historic factor, and that is that the steam generator heat removal systems were not safety grade at that time. They are now.

MR. CURTIS: My final point is, because the system did not seem to be a convincing system, they went to air dump system, which is substantially better. but, of course, they already had the previously fixed design.

MR. CARBON: You are saying, basically, if the DHRS doesn't work, you don't care.

MR. WOOD: I personally would think that the three independent heat removal systems would be enough.

MR. CARBON: I think many people would disagree with you.

MR. WOOD: People would argue for philosophically different systems.

MR. MARK: Doesn't DHRS supply that?

MR. WOOD: If it works, it does.

MR. MARK: It doesn't have to work if the others work. If the others don't work, then it is there.

MR. WOOD: Yes.

MR. CARBON: My question was, suppose it doesn't work?

MR. MARK: We have to put an end to this somewhere. Suppose none of them work, nothing works, then you are in trouble, but sobeit.

MR. WOOD: I have here a list of things that I would like to do in Fiscal-83.

We have developed a CONTAIN code, first under LMFBR sponsorship, then under light water reactor. We would like to do an analysis of the CRBR in a coremelt accident in the CONTAIN code.

MR. MARK: What stops you?

MR. WOOD: Right now we have the money to do it, and we are proceeding.

MR. MARK: The money involved is like what, $100,000?

MR. WOOD: It is $400,000.

The NRR people do not feel comfortable with the LMFBR source term. The position they have is making assumptions in the PRA with respect to the project. So we are planning to do the equivalent of NUREG-0772, which is technical basis for the light water source term, and we will have Battelle-Columbus do the same thing for the LMFBR source term.

MR. MARK: In what way are they uncomfortable? I thought they had already allowed they had everything they possibly could, plus a little more? Do you mean that they have gotten embarrassed by taking that position?

MR. WOOD: They feel that when they go to public hearing, that they are going to be asked if they have looked at the differences of fission product chemistry between light water reactors and LMFBRs, and they would like to be in a position to be they have and to be able to discuss it.

MR. MARK: The fact that they need to do more study, I can understand. But the fact that they have that much radioactivity coming out, they are really allowing for everything possible.

MR. WOOD: As I said, I think the applicant's position is so conservative, perhaps really, to justify doing that work, and that is why it got put off for so long. I think that it is prudent to be prepared for public hearings, too, and that is why we are doing it.

MR. CURTIS: I might add that this is a topic that I might well have added to Dr. Kelber's list of things that ought to be done in the long range, on a generic basis, and that is a more realistic evaluation of the source term in fast reactor accidents.

MR. MARK: But a more realistic one would certainly involve lower numbers.

MR. CURTIS: I would like to add that to the list.

MR. CARBON: I just can't help but say that I will be glad to support work leading to a smaller source term.

MR. MARK: It will take work in order to justify some other numbers. On the other hand, one needs to be careful to put in the right plutonium isotopes.

MR. CARBON: Would you put it above in priority, before what Dr. Kelber has indicated, in your own personal view?

MR. CURTIS: No, but certainly equal to. As a matter of fact, I would consider it a prerequisite to having a final defensible PRA, to have a reasonably realistic source term to use in getting the consequences that are essentially the bottom line of a risk assessment, if you catch my drift.

If you have done your reliability work and you have the probabilities, but you have overstated the consequences very significantly, you have probably biased your risk assessment. Being able to have a reasonable radiological source term is an integral part of a defensible PRA.

MR. MARK: What items in the source term do you think deserve to be scaled down -- the iodine, the plutonium, or what?

MR. CURTIS: I believe the plutonium dominate the risk almost completely -- not completely, but very large.

MR. WOOD: Sodium iodine is pretty stable.

MR. MARK: The source term allows you to take all the iodine there is and puff it out.

MR. WOOD: That is the assumed source term.

MR. MARK: Yes, and that one, I think, deserves to be reconsidered. The plutonium is a little bit harder to lay your hands on. You are only allowing 1 percent on plutonium.

MR. CURTIS: Yes.

MR. MARK: That means that you only have twice as much plutonium, which is as much as a PWR anyway. It is almost that much for plutonium in LWRs.

MR. CURTIS: This fuel is 25 percent plutonium.

MR. MARK: The LWR is quite a bit smaller.

MR. CURTIS: It is 1 percent.

MR. MARK: Under these conditions, you say that plutonium dominates.

MR. CURTIS: At least, that is the work I have seen.

MR. MARK: And you would like to be able to look at it more carefully and get a better number.

MR. CURTIS: Yes.

MR. CARBON: One of the things there, the enrichment of the LMFBR is 25 percent, but in an LWR there is a lot more of it. So Carson's comment is correct, I think, that you have only got two or three times as much total plutonium in the CRBR as you have in the LWR.

MR. CURTIS: Yes, in CRBR.

MR. WOOD: If I may comment. In fast reactor buildings, you have a vaporization to get it out, which you do not have in light water.

MR. CARBON: If you have an ATWS.

MR. WOOD: It is hard to get them up to the fuel vaporization.

MR. CURTIS: Let me make the point that it is potentially more of a risk for both sides.

MR. WOOD: Fuel vaporization in accidents, the way we look at them in fast reactors is considered more than in water reactors. That is the way it has been.

MR. MARK: Your point in reassessing the source term, we have in mind expectation that the plutonium term deserves to be changed.

MR. CURTIS: It is 1 percent out of the air on a satisfactory basis. We need to evaluate what the basis ought to be.

MR. MARK: That is because you believe some allowance ought to be made for aerosol deposit in the containment before it comes out, or it ought to be made for failing to leave the fuel, or where?

MR. CURTIS: The way plutonium can get out, as I see it, we have a serious accident and you can't get the head of the vessel in the containment. You have plutonium aerosol leaked into the vessel in that accident.

MR. MARK: I can understand that you have plutonium aerosol in the vessel, but that plutonium aerosol will deposit itself rather rapidly.

MR. WOOD: Then you are to the point where you have to consider what is the efficiency of the filters, what is the contamination factor, and what are the radiological consequences of the plutonium and other active aerosols.

MR. CURTIS: The underlying reason for wanting to do the work is that there has been very considerable effort in coming up with a revised source term for the LWR. None one has given any serious thought to the source term which was rather arbitrarily imposed on fast reactors.

We think we ought to do something comparable for two reasons. One is because we are predicting that we are going to be asked that question, and it would be embarrassing not to have done it at the hearing. Two, I think it would improve the quality of any on-going risk assessment.

MR. MARK: It needs more thought than it has had.

MR. CURTIS: We have not through the problem well enough to predict what the benefits might be in detail.

MR. WOOD: Which fission products.

MR. MARK: You feel, I judge, certainly I feel, if one did it sensibly, one would come out with something smaller than we are talking about.

MR. CURTIS: I am not going to try to prejudge what species would be the beneficiaries. The fast aerosol program, we have reactivated that.

MR. CARBON: Harold, you have a question?

MR. ETHERINGTON: I have a general question. Most of these activities, I assume, are intended to confirm the good judgment of the designers. Are there any cases where we feel the judgment is sufficiently in doubt that there might be a major revision of design arising from any such activities?

MR. WOOD: The only one, I think, that might fall into that category is the filter and vent system reliability. It is an active system, and it really has to work in a serious accident or you are in trouble.

MR. ETHERINGTON: Is that being given a priority, if it is possible?

MR. WOOD: I would say that it is probably the only major program we have opened up as a result of the design review.

MR. ETHERINGTON: Thank you.

What is the question on dolomite?

MR. WOOD: The long range program for the hearing and the operating license, we may continue to do testing on concrete materials. We have discussed that already. SSCL will not have time before the SER to complete all the sensitivity studies.

After we are through with CRBR, I think we need to continue looking at SSCS for long term heat removal, and I mean hours. In discussions with DOE, it seemed prudent to go ahead and complete the SSC pot version of the SSC in the fuel chamber.

COMMIX is kind of a hard code to use. In discussions with GE and some other users, they may greater improvements by using computer graphics. I think we need further code assessment validation and we have a fair amount of participation from the Japanese in supporting the development of two-phase version. They are putting in something like $350,000 without any strings attached. In fact, they are giving us a lot of good data as well.

MR. MARK: Getting away from CRBR, there is the design study, DES -- I have forgotten the initials --

MR. CURTIS: the last time we talked about it, it was conceptual.

MR. WOOD: We are one past that. It was CDS.

MR. CURTIS: I think it has another name now.

MR. MARK: Fine.

Some of the work you have been showing could be applied to that as well.

MR. CURTIS: We were working on CDS two years ago. As we understood it, when we shifted the gears.

MR. MARK: You started working on Clinch River.

MR. WOOD: I think, in summary, that we need to improve the fuel. I think that rather than going off and developing a 3D version to handle the heterogeneous * we should explore the feasibility of such a computer code and the need for it. So I envision a fairly small effort in exploring that part of the SIMMER.

Now we come to aerosols and source term programs.

MR. CARBON: You almost can leave SIMMER, can't you?

MR. WOOD: Yes, it is just a code name.

MR. CARBON: What I mean is, the improvement of fuel removal models, knowledge or whatever, is not really SIMMER that we especially concerned about. Isn't more an understanding of the fuel.

MR. WOOD: It is a combination of fuel and Bob

Wright's experimental program. So, yes, you could remove it.

MR. CARBON: But the emphasis is on obtaining the data.

MR. CURTIS: The expensive part would be in obtaining the data.

MR. CARBON: That would give us the confidence, and we won't get the confidence through SIMMER.

MR. WOOD: I think SIMMER will incorporate the models as they are developed and verified, essentially.

MR. CARBON: But we will still get our confidence from the data and experiments, and SIMMER is not going to answer questions.

MR. WOOD: We have a very small metallurgy program which will along at the same level, if the Commission allows it. I expect the operating license review to bring some other people in the action, namely, the operator training, the human factors people, the radiological safety people, and more reliability assessment from the PRA people.

That finishes that part of my presentation. Are there any questions?

MR. CARBON: When will we get into the dollar figures.

MR. CURTIS: My instructions are the same as those people who are down on the tenth floor. Our '84 budget is under negotiation with OMB, and I am not at liberty, nor am I certain where those negotiations lie. I can't talk about '84 until it is released.

MR. MARK: Why don't you come to us, then, with a chart like that, and on the side you write the percentage. This is 10 percent, 6 percent, 7 percent of a number which we just don't mention.

MR. CARBON: It would even be extremely helpful if that chart showed us what you are spending this year.

MR. CURTIS: That we can do.

MR. CARBON: I forgot about the fact that you can't discuss some of these things in public.

MR. MARK: You can discuss the percent of an unknown number that Mr. Stockman is keeping under wrap.

MR. CURTIS: Probably that would not help as much as you might think, because if the cuts are significant, the weight loss will be amputation rather than a general weight loss.

MR. CARBON: I didn't attend the session downstairs this morning, but how are we supposed to respond to Congress on the safety research budget if we don't know what it is.

MR. MARK: I don't think it matters whether we do or not.

MR. BOEHNERT: It is a question of timing, and we will have the answer to the question in January, or something like that. The report doesn't go out until February, so we will get the final figures then.

MR. CARBON: Would you give us a chart soon that shown where all the money is going for this fiscal year.

Is there anything you can say about your own personal opinion as to how much you would like to see for fiscal year 1984 as contrasted with what OMB is considering?

MR. CURTIS: In preparing for the submission to OMB, Phil and I went into very detailed negotiations with the NRR review team. It was out of that that we came up with the number $7 million which I gave you as being necessary in support of the licensing activities for fiscal year 1984.

MR. MARK: Does it go up or down for 1985, or does it stay the same?

MR. CURTIS: It is at the same level. Then we would add to that any generic work which was done in response to the ACRS and to others in the development of regulatory methods or regulatory criteria.

MR. CARBON: It would be helpful, to me personally at least, if we had a chart showing what you are spending this year, how much and what for. Then if you can say anything on this open to the public piece of paper on trends or anything. I presume that we can get the information that went to OMB, is that okay, we just simply can't discuss it in public meetings?

MR. CURTIS: Yes.

MR. CARBON: What you are spending, we can get in public. What you put in for would have to be private.

Thank you.

MR. WRIGHT: My name is Bob Wright, and I am in the Fuel Behavior Branch under Mel Silberberg. I would like to make some brief remarks on CDA energetics and core debris coolability, and some thought on future work. I had another meeting across here with the Subcommittee on Fuel Damage Program, so I was not here earlier, and I am not familiar with earlier discussions or anything that came up. I am sorry about that.

This is a brief summary on CDA energetic. The problem or question, of course, is the potential threat to the integrity of the primary system, the sodium release and burn and threat to the containment, and fission product releases, and such.

A key issue in the CRBR review has been the question of fuel removal during the transition phase in the CDA. Let me say with confidence that it eliminates the potential for energetic recriticality and energetic considerations. That is a major part of the effort that is going on in the in the energetics area in our research program.

The current program is structured to be completely in support of the CRBR program, as I am sure Bob has told you.

This is a very brief listing or run through of the major energetics issues in the initiation phase. We are not working on all of this, this is just the issues, and we are working on part of it.

The initiation phase, the questions of fuel and clad, and sodium reactivity rates; blockage formation, and fuel and clad inventory at the start of a transition phase both for LOF and TOP. We are doing a little bit on the last, and plan for more on the last.

MR. MARK: Is that the neutronic.

MR. WRIGHT: Yes, on reactivity of sodium removal, clad removal, and fuel motion.

MR. MARK: Not the fuel/clad interaction.

MR. WRIGHT: It is the effect on the subsequent course of the accident. This has been the historical area of interest in the initiation phase work, and that has rather shifted toward these questions of blockage formation, and what happens in the transition phase. In part that is because the CRBR would be heterogeneous core and sodium boiling coefficient is down. The interest in that has changed.

I think, also, the awareness of the fuel removal problem has come on particularly strong recently in this country.

MR. CARBON: In the initiation change, the only thing you are doing anything on is item three.

MR. WRIGHT: We are planning some two and three. The clad removal, then the blockage above the core.

MR. MARK: You used the words "in this country," a few minutes ago. Has the concern you have referred to been active in other countries, or do we know what their conclusions were?

MR. WRIGHT: I think that the realization of the importance of the transition phase and the question of fuel removal, blockage formation has been more strong here than in other country. I was not at Lyons, but that is feedback I get.

There is another part of it, of course, is that we have been focusing strongly on the heterogenous core CRBR where the initiation phase burst has been very much downgraded as an item of importance, and that leads you right into the transition phase issues. So we had to face them more strongly. So there are good reasons why we are more sensitive, but I think others may be somewhat less aware than we are, or maybe they are behind.

In the transition phase, the issues of fuel removal processes, questions of blockage formation and removal, and boiling pool dynamics, and the recriticality energetics. We are doing a great deal of work in the fuel removal process, and I think our experiments at Sandia are unique. They are furnishing an important source of data in this area, and we will talk about that a little bit more.

The blockage formation is also at the initiation phase. I would like to comment that boiling pool dynamics is potentially an important area that has not been addressed adequately. My own view is that some fundamental thermal hydraulic experimental data on some of those questions could be obtained without great difficulty, and currently we are not really doing that. That has been under budget pressures.

MR. MARK: If you are not persuaded that fuel removal was defective in certain mechanisms, then you would not have to worry about the recriticality.

MR. WRIGHT: That is the argument.

Also, I should mention that the big codes, SIMMER in particular, have been calculating the boiling pool dynamics. I, for one, would feel more comfortable if we had some confirmatory experimental data on some of these.

MR. MARK: You said, boiling pool. We have a loop reactor, and we don't have a pool.

MR. WRIGHT: It is the boiling fuel in the steel pool in the core transition phase.

MR. CARBON: Would you summarize again how much work you are doing on blockage formation and removal.

MR. WRIGHT: It would be more relevant in the next viewgraph where I go into the programs. We are doing almost none currently, but we plan to be doing a significant amount starting in 1983, and more substantially in 1984 and 1985, depending in part on foreign support. The Japanese in particular are interested, and the Germans, too.

The last item is the disassembly phase issues. The work potential includes question of sodium, augmentation or diminution, the are the primary input questions, and I have also listed the plutonium source term in this disassembly phase because here we have the potential for vaporized fuel getting out.

Reversing the order and starting with the transition phase, because that is where most of our effort is, I am just listing things here, and I will be happy to go into any sort of descriptive detail that you might be interested here on the experimental work that we are doing.

The primary effort has been these transition phase experiments in ACRR, in which we proceed to melting the fuel sample, and put a known delta P on it, and drive it into a structure. By thermal couple measurements, we follow leading edges, but primarily post-morten examination tells us what the fuel streaming, freezing, plugging is, and we compare that with the models. This is turning out to be, as I said, probably the key issue of the CRBR analysis.

We have finished the TRAN-1. We had five experiments that used infinitely thick steel walls, and we have some results that have been reported partially or are being written up.

Briefly, no current model adequately describes the results of the experiments. Clearly, the bulk freezing model is not applicable. We see much greater penetration than that, but on the other hand, we see blockages, partial blockages, penetrations less than the very large ones that are predicted by the straight conduction model.

The post-test examination of the tubes shows fuel crust, and it is obvious that the behavior is in large part a conduction model sort of behavior where you freeze the liquid on the surface of the tube, and build an isolating crust, but that is not the whole picture. In particular, there seems to be some leading edge effects that are forming partial blockages, some heat removal sorts of things more like the freezing model.

In the two experiments that have been done with the steel walls hot enough that the molten fuel produced molten steel on contact, the behavior is rather complex. We are getting films of interleaved fuel and steel, where steel has been melted, but we are not getting the mix up and heat transfer which would produce the very short penetration which has been hypothesized in the bulk freezing type of behavior.

Our current status here is that the very large penetration lengths of the conduction model do not seem to be occurring in our experiments. The conduction is a major part of the observed behavior, but it is not the whole story.

As you know there has been other work done, particularly with thermite mixtures by Bruce Spencer at Argonne and others, and their results in this area tend to scatter. I personally have not been too fond of thermite because of the question of what you have in the metal content and how it is made, and things like that.

In FY-82 and FY-83, we are going into the TRAN-2 and TRAN-3 experiments. TRAN-2 is an improvement on the basic TRAN-1, the structure that I described, in which, for one thing, we are going to do some work with walls such that we don't have the huge sink of the infinitely thick wall, so we can get more ablation in the fuel mixing.

Secondly, we will be looking at fuel/steel mixture, which have potentially large power for melting through things, and in addition doing a little more parameter investigation of the pressure range and larger fuel masses than we have used previously.

We will also be going in from the straight tube geometry into a pin geometry with convex outward surfaces, looking at the space between an array of pins.

I should mention that the first TRAN-2 experiments that should be done by the end of the year will involve a co-axial arrangement with a center rod, and then an outer tube. So that in the same experiment we will have the convex and concave crust, and we can test the stability argument that Mike Epstein has been raising.

If you have the stable crust, you have the long penetration. If you have an unstable, it breaks up, and you have the potential for mixing and short penetration. We will have the experiment in the same geometry which clearly we expect definitive results on.

The TRAN-3 experiments have settled into a plain gap geometry as opposed to the pin geometry. There have been some discussions of doing something in integral experiments, looking at melt-in as well the fuel removal and gap geometries. That has been deferred for the present, in part because of the development of CRBR licensing.

Six months ago, it looked as if the timing of the melt through into the blanket gas was critical compared to the development of whole core pool, and the arguments going more toward the annual pool first. That is not so critical. We had some financial limitation, we could get gap geometry data sooner, and that is the way we are going.

The third part here is the PLUGUM model, which is the Sandia modeling following along with these experiments. It has flexible geometry. Currently, it has conduction modeling in it, straightforward conduction model. They are working on the leading edge effects, and they are working on various ablation situations, but it is not in the finished state. The experimental data base is still so thin in this area.

I have put down the number of experiments, that is what those numbers are, and that is with the budget of two weeks ago. If we are not cut too much, we should still follow that. We are doing some of the TRAN-2 experiments, and the TRAN-3 would follow later on in the year.

For the longer term work, what we are thinking about is completing the TRAN-2 and TRAN-3 experiments, particularly with some fuel/steel mixtures and a broade parameter range, and then going into this integral melt-out and fuel removal experiments.

It is not certain that we will do these. These involve over a kilogram of fuel, and to be meaningful you have to have the correct heat transfer with a thermal attack on the corner of the assembly wall, adjacent to a blanket gap. That means the problem of natural convection. If you don't do that, there is no point in doing this complicated and expensive experiment. It is not trivial.

So depending on the need, we will or will not proceed in trying to do this integral sort of experiment.

The feeling at Los Alamos and Sandia is that some sort of data on the integral or link process is needed. In the modeling, we plan to work on ablation effects on the gap geometry as opposed to the tube geometry they are working on now to see if there is anything strange. I personally don't foresee anything, but it doesn't mean that there won't be. This will be added to PLUGUM, and the melt-in wouldbe added in PLUGUM in some way, whether we do the experiments or not.

So that is what we are planning in the immediate future on the transition phase work, and this might be a good time if there are any questions on that area.

I will go ahead, then. Now we are back to the initiation phase. The current program, we had finished in FY-82, the joint program with KfK on fuel disruption of irradiated fuel pellets under LOF conditions. In the joint program with KfK, the FD-2 are the supported experiments for the power histories appropriate to the heteregoneous CRBR core, and for the more spiked power histories appropriate to SNR-300 with its homogeneous core.

The Sanpin model of fuel swelling and fuel disruption, and the border on which the get the break out from swelling into disruption, and lead into liquid phase, has been developed and will be published soon. I think this is a substantial contribution.

Also I should comment that these experiments show the power of the optical diagnostic mode of experimentation that we are using and have developed, and not uniquely, at Sandia.

With these ACRR experiments, when you really follow in detail in real time what is going on and can measure, we have a lot more knowledge of the system than a more direct instrumentation can give you. It gives you the ability to develop, with some confidence model, the behavior. You can really look and watch and see if this happens or not.

In FY-83, we will be, depending on the budget situation and foreign support, initiating follow-on experiments which they are now called STAR. We are getting smarter in the PR. They used to be called CFR, which stood for clad and fuel relocation. Now it is Sandia Transient Axial Relocation.

What these experiments are is single pin, single annulus, and multi-pin experiments on primarily upward clad relocation and flowing sodium vapor. The simulation is that we use Argonne vapor -- Argonne gas, instead of sodium. We use the optical diagnostic. We have been working modeling of the effects.

The question here is the development -- A lesser question is the reactivity effects from the actual clad and fuel motion. Actually, you are interested in the development blockages in the upper structure, and being able to develop models and can understand the parameter range in which these would occur. That would limit your fuel removal, and sets the condition for the transition phase situation, and the necessary fuel removal later on.

I didn't say that quite right. The question of the upward axial fuel removal path is important, and if you block early that is missing, and then you are forced to deal only with the in-structure, the gap fuel removal or the fuel removal to the gap between the assembly or fuel removal to the control rod, melting in the control rod assembly. This axial fuel removal is important when we look at details as to what reactivity you have left in the transition phase.

As I said, we were just getting started with these experiments in 1983, and then in 1984 and 1985 we would be carrying them, exploring the relevant parameter space, the blockage formation, and model development. I should say that these are exceedingly cost effective experiments, very inexpensive. You can do a good number of them, and as you see here, we are talking about 12 in the 1984 to 1986 period.

I think Dr. Carbon asked me a question about these experiments, how many or when. If the financial picture remains reasonable, we intend to move strongly in this area.

MR. CARBON: My question was, are you doing them now, or when do you plan to do them?

MR. WRIGHT: Right now, we are really in low level planning. The Japanese have shown a great deal of interest in these particular experiments. If they come in April, then this would go from the low level planning to movig out and starting to do experiments rapidly.

Steve Wright has been doing the FD experiments. We had two Germans working on the FD, skilled experimenters and analysts, and they want very much to come back and continue in this area. Of course, two skilled experimenters and analysts are very valuaable. We hope that the Germans will want to do that, they are very interested in it. How fast we go here is very uncertain.

The third area is the disassembly phase. This is the lower priority level. We have started work on this predispersed molten UO_2 sodium, FCI propagration experiment to determine whether or not propagation is possible in the UO_2 sodium system. There are some hypotheses that say it is not.

We think that this experiment has potential, if propagation is not possible, in demonstrated in this, and then FCI augmentation work can be ignored, because for any substantial work in FCI, you have to get massive involvement in the propagation process. If you just get individual non-propagating explosions or detonations, you will do no work.

The Texas thermal detonation model has been developed at Sandia. It will be published as a base for analysis of these experiments. The other thing that is happening is that there will be a report on the fuel equation, the EOS experiments and analysis which is completely funded by FfK, and they have been doing it in ACRR.

As of now, in FY-84 and 85, we are not planning any further work on this disassembly energetics.

MR. CARBON: For Fiscal Year 1983, right now, the first bullet there, FD-2/FD-4, how much money are you spending this year?

MR. WRIGHT: Are we supposed to be talking about this?

MR. CURTIS: This year is fine.

MR. WRIGHT: This year is fine. To do these two experiments, and finish it up takes around $400K. This is one of the soft areas, if we get cut. The CFR/STAR initiation, that might or might not get to the experiment, is at about that same level, about $400K. The majority of the work is in the transition phase.

MR. CARBON: What are these two experiments being done?

MR. WRIGHT: These are all in the ACRR at Sandia.

MR. CARBON: I am not clear why you are doing the FCI work.

MR. WRIGHT: This started some time ago. The question is, with molten fuel around, whether you can get, by the FCI process, sodium augmentation of the energetics such that it will threaten two primary systems.

MR. CARBON: Work like this has been going on at Argonne.

MR. WRIGHT: Right, but let me get specific.

MR. CARBON: And in England. nd in England.

MR. WRIGHT: Yes, but this experiment has the potential of really definitively settling this question. I have been in this field for about 20 years, and it is clear now that the thermal detonation modeling, the general idea of Borden Hall, is correct, whereas the Baski-Hendrie business of ten years ago is not. We have found in the INPILE experiments that have been done up to now sharp local fuel/coolant interactions, but no large high work potential interaction but these have always been very small systems.

One of the major hypotheses is that the UO_2 sodium system, because the contact innerface temperature between molten fuel and sodium is so low that propagation may not be possible. This experiments sets up the pre-mix detonable mixture, and then puts a detonating pressure pulse in a define geometry such that one can say, does it build up or does it go away. It is clean cut enough that once you analyze it, you reach the conclusion about this. This is what this is all about also, and this comes up in the later stages, can you get any energetics FCIs on molten fuel dropping into the sodium, things like that.

The thing of importance or interest is that we have reached a point where we think a definitive experiment will give the answer of whether this really can be ignored.

MR. CARBON: You have been in the field for 20 years, so I can't very well argue with you, but you say that this experiment is going to give the definitive answer.

MR. WRIGHT: For UO_2 sodium.

MR. CARBON: From what little I know about it, I find that hard to believe. There have been so many experiments.

MR. WRIGHT: I think we focused on the key situation for the UO_2 sodium system. Until Borden Hall came along with the thermal detonation model, we didn't know what the basic mechanism was. We were all looking at the wrong things.

We know in many systems you can get moderately energetic fuel cooling interaction. With UO_2 sodium system, we have not seen it, but we cannot confidently say that you cannot. Here, this experiment has the potential of giving what I think is essentially a definitive answer on the propagation questions.

You might want to talk to Mike Cordini on this.

MR. CARBON: The British, have they not, have concluded that even though Borden Hall models are generally correct --

MR. WRIGHT: They are not precisely correct.

MR. CARBON: You can get some of the

conditions such that you can write this off.

MR. WRIGHT: Not to my knowledge, but I haven't talked in the last year or so with Simon Borde.

MR. CARBON: There was a paper at the Lyons Conference.

MR. WRIGHT: I was not at Lyons, but I will look that up.

MR. CARBON: There was a paper there, and it was not by Borden Hall, it was by Byrd and somebody, it confirms the kind of thing that you are saying. It does support the Borden Hall model, I guess, but it indicates that they have been unable to get any sort of propagation with the UO_2 sodium in large quantities.

MR. WRIGHT: I should look that up. Were they talking about in-body experiments?

MR. CARBON: No. I started studying this a week ago.

MR. WRIGHT: This experiment arose in an attempt to get the answer to this question. We knew we had sharp local interactions, these were observed. We had not seen large interactions, but we never had a system in which you could clearly say that you have expected a large interaction. It was not clearly enough defined, and this is how this experiment arose.

MR. CARBON: Let me send you a copy of the paper. Paul might have a copy, it is one of those five that you got from me.

MR. WRIGHT: There is another path of preventing an energetic interaction in which you cannot get the premixing of the detonable mixtures, and you can argue that one, too.

I had been unaware of any data that was in any way definitive on that part.

MR. CARBON: I will get you a copy of that.

MR. WRIGHT: The last item here, I would like to bring you up to date on the work on core debris behavior. This is in the format of a need.

The question of debris formation and characterization in the CDA, what the debris looks like, and how much. Then the debris-bed dry-out limits, including bed dynamics.

I might digress here. One thing we have learned from the experimental program is that if you open up the channels in a bed, you can have super-heat release that open the bed, that increases the coolability limits by large factors. In fact, one super-heat irruption in the D9 experiment at Sandia increased the coolability limit by a factor of eleven. Particularly for the fast reactor situation, bed dynamics is turning out probably to be more important in the practical sense than the coolability limits of a defined bed.

The next item here is pose-dry-out behavior and melt progression for non-coolable geometries, and then the question of ex-vessel long term debris coolability.

We have had this long-term program going at Sandia, the INPILE coolability experiments. It is has been a joint program with NRC, EURATOM, and PNC in Japan.

This is the final year of the program. In this year, there will be three sodium cooled experiments. They will be new in that they have bottom cooling of the bed. This is the geometry of an in-vessel core retention device, or core catcher, where you pool the heat out from the bottom as well as from over the line pool. You increase the bed specific power, dry out occurs depending on the conditions, but by substantial factors like three or four.

There are also some effects in boiling of downward vapor flux. There are predictions that some things like this could occur, and there have been no experimental data on them. The European people in particular have been interested in the program, and that has been the major focus of their efforts, and we will be getting into that in the final year.

We will be getting further into the extended dry-out and some more work on bed dynamics and stratification.

Two dry-capsule experiments will also be done. Our view is that if you are looking at extended dry-out, the melting of particular fuel in a dried-out bed and relocation of the fuel centering, crust formation, you do a separate effects type of approach. You take the sodium out and put it in the dry bed. That is what these experiments are.

Then we will continue model development for bed stratification, channeling, disruption, and also post dry-out behavior.

I think you probably know that in the area of dry-out coolability limit, the modeling that Ron Lipinski has been doing particularly at Sandia, is pretty much now the world standard. For packed beds, with sophisticated modeling and a critical data base, you can really do quite well. For fast reactors, we are finding that this question of the bed dynamics and opening up which increased the coolability is probably of more practical importance than the packed bed limit.

Another thing I have not yet mentioned is the bad effect on coolability is the stratification in the bed. the fines at the top and the coarse debris at the bottom, and this is what you get in the settling of debris through a pool, if you have an initiating event and things settle and the fines stay at the top. The Lipinski model predicts decreases in the coolability of a stratified bed by factors like three and four, significant amounts.

The reason for it is not what you might expect. A major part of the reason for this is the chocking by the fine debris at the top, pulling the liquid in and the vapor out. Another major effect is the capillary forces in fine debris pull the liquid away from the bottom, the coarse debris are dried out. For fine debris, the capillary forces are significant.

So stratification is a major problem in the LMFBR debris coolability. The way you can get around that is if you do have channeling or irruptions to open it up. We have been working on this array of processes, and we have made a good deal of progress.

In particular, if you have significant subcooling in the over-line pool, you get a stagnent conduction band at the top of the bed with no convection. In a stratified bed that makes it very, very hard to open up the channels to increase the coolability.

This is the general in which the work is focusing in this later stage.

Whether there will be follow-on work is an open question. The Japanese have indicated an interest. There are obviously some areas that would be fruitful for further work, but there are strong limitations and we don't know how this will go. I have listed here some of the things.

Actually we do need some analysis of the experimental results because we are going to be running very hard to finish up in this period with these experiments and there will have been an analysis of them.

Then, further work on the bed dyanmics process, post dry-out behavior and melt progression, and then something that we have not addressed well enough, the question of the ex-vessel long-term debris coolability which involves things like the concrete effects and the gas effects. There has been some work done at Sandia by Dana Powers on concrete melt effect selectively heated. There are probably some things in this area that could use fission heating, which is more versatile. But, as I said, these are areas of possible continued work, and no decisions have been made.

I thank you, and I will be happy to answer any questions.

MR. CARBON: One more comment on the two pool interaction mode. You told me to see to Mike, but Mike is essentially a consultant of yours, is he not.

MR. WRIGHT: That is correct. I had not thought of that.

MR. CARBON: All I am saying is that I gave Mike a copy of that paper, the British paper, and I have intended to ask him to tell me what it says in another week or two. So you can call him yourself and get the information.

MR. WRIGHT: I try to stay in contact with Mike. We are so busy that we are not in as frequent contact as we would like. I was not aware of this particular paper.

MR. WOOD: We have one more item on the agenda. How much time do you want to spend on it, I only have two viewgraphs.

You asked us to comment directly on your report in one letter. We thought that we should try to respond to it, but it is somewhat difficult to respond to it. In your report, you recommend that we earmark roughly $1 million to aid development of a regulatory position for post-CRBR LMFBRs. I think some words in that paragraph had to do with design criteria and standards.

In September of 1981, we were directed by the Executive Director to put all of our efforts on CRBR. We did prepare a plan. A little later on in FY-83, Bernero's Division of Risk Assessment made a proposal to prepare PRAs to aid in developing a regulatory position and what our design criteria should be. That was rejected by the NRR Project Office, and we had quite a hassle about it. Eventually, funds got transferred to 2D-3D program, so that money went down the drain.

Since then the NRR Projects Office has developed a set -- they call principal -- of design criteria as opposed to general design criteria for CRBR. Effectively what they did is to take the light water principal design criteria, added some, modified some, added some, to come up with a new set. It was a long meeting, and I don't think they were completely accepted wholeheartedly by some of the members. The impression was that a lot more work needed to be done.

MR. CARBON: Can I check something here. The first bullet says that we recommended $1 million to develop the regulatory position post-CRBR. The second bullet says that the EDO said, "Don't spend any money, except for CRBR." So the second bullet says that our recommendation is tossed out.

MR. WOOD: That is right.

MR. CARBON: The third bullet says that the CRBR Project Office that it was not needed for the CRBR. I would say, in response to the fourth bullet here, that I don't think the principal design criteria are going to do you any good at all in developing a regulatory position. It is waste. I see you smile, and you are shaking your head, so you agree with that.

MR. WOOD: I was in Germantown, talking to the technology people, and it seems they are leaning toward a low specific power, pot type reactor with safe reactivity coefficients, and the issues may become very different from CRBR.

MR. CARBON: This is the first I have heard of this.

MR. CURTIS: It was the afternoon before Thanksgiving.

MR. WOOD: This is nothing official.

MR. CARBON: I was going to ask if it was the afternoon before New Year.

MR. WOOD: I don't think that that is official.

MR. CURTIS: It is intelligence as gathered. We don't have anything accepted there. It is Phil's reading of the tea leaves after that afternoon.

MR. CARBON: What he seems to be saying, and I want to ask you, are these tea leaves saying that for the large prototype breeder reactor, they are thinking of a pool-type instead of a loop-type.

MR. WOOD: The quotation I can use was "a reactor with large thermal inertia."

MR. MARK: Who are the people you are referring to here?

MR. WOOD: Base technology people.

MR. MARK: Are these in NRC?

MR. WOOD: They are in DOE.

MR. CARBON: You can get large thermal inertia by going to a pool type, obviously, or you can get large thermal inertia with a loop-type if you just go to a big vessel.

MR. WOOD: I hate to quote the DOE people on the record on things that are not written down. But the tendency is toward very safe reactors, that is what I am saying.

MR. CURTIS: I think he would just as soon not name any names for the transcript.

MR. MARK: It is a safe reactor of the pool type, rather than a large thermal inertia with a big loop vessel, I guess.

MR. CARBON: It could be different from the pool type, that is all I was trying to say.

MR. WOOD: To close, I wanted to respond to your recommendation in the report last year, and ask if there is anything we can do to respond more than we have, because our hands have been tied this year.

MR. CARBON: In terms of that $1 million.

MR. CURTIS: We have hopes that the PPG, which will probably come out in January, will offer a little more latitude than last year's did.

MR. WOOD: That is all we have.

MR. MARK: If you can't respond any more to the suggestion, I suppose that we will make it again, and it may be $2 million. The principle is what we care about, and we don't know why NRR ought to be in a position to overrule items of this sort anyway.

MR. CARBON: That was really EDO, but I totally agree.

You have in your bullet number one $1 million for research on the regulatory position for post-CRBR LMFBR. But did not we also separately recommend something like $1 million, or recommend that a PRA be performed by NRC for CRBR? I am almost sure we did and that is different from this bullet one here.

MR. CURTIS: Paul is looking it up, but I do not remember it that way.

MR. CARBON: I am sure that we recommended something.

MR. BOEHNERT: It is in the report to the Commission.

MR. CARBON: We recommended a PRA for the CRBR program. What happened to that?

MR. CURTIS: That is bullet number three.

MR. CARBON: I thought that bullet number three was a follow on for bullets one and two.

MR. WOOD: No, they are independent. We were trying to figure out a way to respond to that recommendation.

MR. CARBON: So there are really two things here. The first is the recommendation we made for post-CRBR, and the EDI said, no, don't spend any money, except for CRBR.

MR. CURTIS: We thought we might be able to make a contribution toward contributing to bullet number one by means of a PRA which used CRBR as the model as a byproduct, in spite of other instructions.

MR. CARBON: But there are two things that we recommended. One is bullet one, and one is bullet three. The EDO killed one, and the CRBR Project Office of NRR killed bullet three.

MR. CURTIS: That is correct.

MR. CARBON: I didn't understand that.

MR. CURTIS: Bullet one was long since dead, and we thought we might be able to make a contribution toward that objective as a byproduct of the PRA.

MR. MARK: Are you prepared to use words that are as devastating as possible. At some time, somehow, we want to say something about what we think the NRC should be doing. I believe we still think they should be doing what we suggested before.

MR. CARBON: I am not prepared to change my mind.

MR. MARK: I think what you need are some more outrageous words.

MR. CARBON: I have no disagreement with that.

MR. CURTIS: I take it you don't think the method of the principal design criteria will lead to the required solution.

MR. CARBON: In no way.

One thing that would be real helpful to me, and I repeat, if you would make out a list in reasonable detail of where you are spending money in Fiscal Year 1983.

MR. CURTIS: We can give you that.

MR. CARBON: If you make it a closed paper, it would be helpful.

MR. MARK: They don't have to make that a closed paper. They are spending that money in 1983. They can't put down numbers because they don't know them anyway.

MR. CARBON: But you do have numbers.

MR. MARK: They may have them in mind.

MR. CARBON: They have gone to OMB.

MR. MARK: I think you should make a chart that says, we think we should increase this, and decrease that, and then add the following.

MR. CURTIS: I have no difficulty whatsoever in giving you 1983 budget numbers in great detail.

MR. CARBON: But you also can give us what you proposed for 1984 if we keep them out of the public domain, which we could do, could we not?

MR. BOEHNERT: Yes.

MR. CARBON: I would like to get those, too, and see what you are of doing. I would be, obviously, willing to consider them. If you would go a step further, it would be helpful to know your priorities. If you break your priorities and say, here is an item and it is high priority to spend something at least this much. Then it would be nice to go further. If you want to do something like that, it is all right, too, for

1984.

MR. CURTIS: This year, we can give you without any doubt. I will probably have to check for instructions. I will have to clear this and use the general procedures that have been used downstairs in terms of the release of 1984 numbers. I know right now Mr. Gillespie is talking to the balance of the committee on this very subject.

MR. MARK: Would you have a legal or institutional problem in doing what I said; you write down this year's numbers, and you say, for next year we should continue this and increase that.

MR. CURTIS: That we might be able to give you, we think, in a couple of weeks.

MR. MARK: I think he wants it in a couple of hours.

MR. CARBON: I was thinking of tomorrow. Don't we have to give this to Chet Siess at his meeting.

MR. BOEHNERT: Not at this meeting, but in January.

MR. MARK: By that time, we can even have the numbers.

MR. BOEHNERT: By that time they will be publicly available.

MR. CARBON: If we don't need them at this meeting, then I will not ask for them, but certainly by the next meeting. At the January meeting, if we had, at a minimum, a layout of what is being spent and something about priorities.

MR. CURTIS: We will give that to you.

MR. CARBON: That is then.

(Whereupon, at 4:50 p.m., the meeting was closed.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: ACRS/Subcommittee on Advanced Reactors

Date of Proceeding: December 8, 1982

Docket Number: ______________________

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Patricia A. Minson

Official Reporter (Typed)

Patricia A. Minson

Official Reporter (Signature)

PRESENTATION TO ACRS SUBCOMMITTEE ON ADVANCED REACTORS

DECEMBER 8, 1982 - WASHINGTON, D.C.

BY

PHILLIP M. WOOD

SEVERE ACCIDENT ASSESSMENT BRANCH

RESEARCH SUPPORT FOR CRBR LICENSING

- CURRENTLY OPERATING UNDER A JOINT RES-NRR TECHNICAL ASSISTANCE-RESEARCH PLAN DIRECTED TOWARD CRBR LICENSING ISSUES.
- CODE DEVELOPMENT PROGRAMS (SCC, SIMMER, ETC.) EMPHASIZING CONFIRMATORY CALCULATIONS AND TECHNICAL ASSISTANCE.
- EXPERIMENTAL PROGRAMS DIRECTED AT SPECIFIC CRBR ISSUES.

RESEARCH PROGRAM ACTIVITIES

- SODIUM CONCRETE INTERACTIONS

  1. SNL - HEDL TESTS NOW IN AGREEMENT.
  2. DOLEMITE COMPARABLE TO CALCITE.
  3. APPLICANT NOW ACCEPTING LARGE INTERACTIONS.
  4. NRR CONCERN HAS SHIFTED TO AEROSOL PLUGGING AND FILTER-VENT RELIABILITY.

RESEARCH PROGRAM ACTIVITIES (CONT.)

- SUPER SYSTEMS CODE - (SSC-L) PROJECT

  1. CONFIRMATORY CALCULATIONS FOR THERMAL HYDRAULIC CAPABILITY, DECAY HEAT REMOVAL, NATURAL CIRCULATION OF CRBR.

  2. CONFIRMATORY CALCULATION OF PIPE BREAK ACCIDENT, STATION BLACKOUT, ETC.

  3. PARTICIPATING IN REVIEW OF PSAR CHAPTERS 4, 5, 9, & 15.

  4. IMPROVED MODELS OF DHRS, AIR HEAT REMOVAL SYSTEM, AND BALANCE OF PLANT ARE BEING DEVELOPED.

RESEARCH PROGRAM ACTIVITIES (CONT.)

- COMMIX-1A 3-D THERMAL-HYDRAULICS

  1. EVALUATING IN-VESSEL STRATIFICATION DURING NATURAL CIRCULATION.

  2. EVALUATING FFTF STRATIFICATION WITH PONY MOTORS ON.

  3. EVALUATING DHRS PERFORMANCE.

- SIMMER, ACCIDENT ENERGETICS

  1. PRESENTATION OF NRR POSITION MADE TO ACRS 11/19/82.

  2. SIMMER PROVIDING BEST ESTIMATE CALCULATIONS.

  3. FUEL REMOVAL KEY TO LOW ENERGETICS RECRITICALITY POSITION.

     R. WRIGHT WILL DISCUSS SNL PROGRAM

RESEARCH PROGRAM ACTIVITIES (CONT.)

- NEW PROGRAMS IN FY 1983 IF FUNDING AVAILABLE

  1. ANALYSIS OF CRBR ACCIDENTS WITH CONTAIN.

  2. LMFBR SOURCE TERM EVALUATION AT BCL.

  3. ORNL FAST AEROSOL PROGRAM REACTIVATED.

  4. FILTER-VENT RELIABILITY ASSESSMENT NEEDED.

  5. RADIOLOGICAL CONSEQUENCES FROM ACTINUDES BEING CONSIDERED.

LONGER RANGE PROGRAM - HEARINGS AND O. L.

- SODIUM CONCRETE INTERACTIONS:

  TESTING OF ALTERNATE MATERIALS (HAC) MAY CONTINUE AT LOW LEVEL.

- SSC-L:

  1. SENSITIVITY STUDIES SHOULD CONTINUE.
  2. COMPLETION OF SSC-S FOR LONG TERM HEAT REMOVAL NEEDED.
  3. COMPLETION AND VALIDATION FOR SSC-P FOR POT-TYPE REACTORS NEEDED.

- COMMIX: 3-D THERMAL-HYDRAULICS

  1. IMPROVEMENT OF INPUT PREPARATION USING COMPUTER GRAPHICS.
  2. FURTHER CODE ASSESSMENT AND VALIDATION.
  3. IMPROVEMENT AND VALIDATION OF 2-PHASE VERSION.

LONGER RANGE PROGRAM - HEARINGS AND O. L. (CONT.)

- SIMMER - ACCIDENT ENERGETICS

  1. IMPROVEMENT OF FUEL REMOVAL MODELS WHEN DATA IS AVAILABLE.

  2. FURTHER SENSITIVITY STUDIES.

  3. METHODS DEVELOPMENT TO EXPLORE FEASIBILITY AND NEED OF A 3-D VERSION.

- AEROSOL AND SOURCE TERM PROGRAMS:

  SHOULD BE COMPLETE IN ABOUT 2 YEARS.

- HIGH TEMPERATURE METALURGY PROGRAM IS LONG TERM

- OL REVIEW WILL BEGIN NEW REQUIREMENTS

  1. HUMAN FACTORS - OPERATOR TRAINING

  2. RADIOLOGICAL SAFETY (HEALTH PHYSICS)

  3. RELIABILITY ASSESSMENT

ACRS RECOMMENDATIONS IN NUREG 0864

- o PG. 27 RECOMMENDED THAT $1 MILLION "BE EARMARKED SPECIFICALLY FOR RESEARCH TO AID DEVELOPMENT OF A REGULATORY POSITION FOR POST-CRBR LMFBR's"

- o SEPTEMBER 24, 1981 EDO DIRECTED RES & NRR TO DEVELOP A JOINT TECHNICAL ASSISTANCE - RESEARCH PLAN DIRECTED ONLY AT CRBR LICENSING ISSUES.

- o DRA PROPOSAL TO PERFORM A PRA IN FY 83 TO DEVELOP A REGULATORY POSITION ON LMFBR's REJECTED AS NOT NEED FOR CRBR. FUNDS TRANSFERRED TO 2D/3D.

- o NRR-CRBRPO HAS DEVELOPED PRELIMINARY PRINCIPAL DESIGN CRITERIA FOR CRBR. AFTER ACRS REVIEW THESE MAY PROVIDE A START TO THE PROCESS OF DEVELOPING A REGULATORY POSITION.

ACRS RECOMMENDATIONS IN NUREG 0864 (CONT.)

- DOE BASE LMFBR TECHNOLOGY PROGRAM LEANING TOWARD LOW SPECIFIC POWER POT-TYPE REACTOR. ISSUES MAY BE DIFFERENT THAN CRBR.

LONG RANGE FAST REACTOR SAFETY RESEARCH

- PRA - PROBLEM: OF WHICH PLANT? POSSIBLE DOE COOPERATION (MONJU IS IS HAVING PRA DONE.)

- SIMMER, SAS MODELLING, APPLICATION. IS THERE A SAFETY ADVANTAGE TO FLOWERING CORES?

- COMMIX, SSC MODELING APPLICATION. IS LOW-FLUX BOILING AN ACCEPTABLE LIMIT?

- ANALYSE CABRI TESTS; PHENIX DATA: POSSIBLE MONJU DATA.

OBJECTIVES OF LONG RANGE FAST BREEDER SAFETY RESEARCH

- CATEGORIZE AND SET PRIORITIES AMONG LMFBR SAFETY ISSUES
- MAINTAIN CAPABILITY TO ANALYSE CORE-MELT ACCIDENTS
- MAINTAIN SYSTEM ANALYSIS CAPABILITY
- MAINTAIN EFFORT TO EXTRACT INFORMATION FROM ABROAD

CDA ENERGETICS AND CORE DEBRIS COOLABILITY EXPERIMENTS AND MODELS

PRESENTATION TO ACRS SUBCOMMITTEE ON ADVANCED REACTORS

WASHINGTON, D. C.

DECEMBER 8, 1982

R. W. WRIGHT
FUEL BEHAVIOR BRANCH

CDA ENERGETICS

- POTENTIAL THREAT TO INTEGRITY OF PRIMARY SYSTEM.
- KEY ISSUE IN CRBR REVIEW IS:
  - TRANSITION-PHASE FUEL REMOVAL PROCESSES (ENERGETIC RECRITICALITY).
- CRBR SUPPORT IS BASIS OF CURRENT PROGRAM.
- INITIATION-PHASE ISSUES:
  - FUEL, CLAD, SODIUM REACTIVITY RATES.
  - BLOCKAGE FORMATION.
  - FUEL AND CLAD INVENTORY AT START OF TRANSITION PHASE - LOF AND TOP.
- TRANSITION-PHASE ISSUES:
  - FUEL REMOVAL PROCESSES.
  - BLOCKAGE FORMATION AND REMOVAL.
  - BOILING POOL DYNAMICS (RECRITICALITY ENERGETICS).
- DISASSEMBLY PHASE ISSUES:
  - WORK POTENTIAL.
  - PU SOURCE TERM

TRANSITION PHASE - PROGRAM CONTENT

FY 83:

- TRAN-2, TUBE AND PIN GEOMETRY EXPERIMENTS (5)
- TRAN-3, GAP GEOMETRY (2)
- PLUGM MODEL

FY 84/85:

- COMPLETE TRAN-2 EXPERIMENTS (6)
- COMPLETE TRAN-3 EXPERIMENTS (4)
- PERFORM INTEGRAL MELT-OUT AND FUEL-REMOVAL EXPERIMENTS (4)
- ABLATION, GAP GEOMETRY EFFECTS ADDED TO PLUGM
- MELT-IN ADDED TO PLUGM

INITIATION PHASE - PROGRAM CONTENT

FY 83:

- RESULTS AND ANALYSIS OF JOINT FD-2/FD-4 EXPERIMENTS WITH KfK
- SANDPIN MODEL
- PREPARE AND INITIATE STAR(CFR) EXPERIMENTS
  - MULTI-PIN, ARGON FLOW, OPTICAL DIAGNOSTICS

FY 84/45:

- STAR (CFR) EXPERIMENTS (12)
  - EXPLORE RELEVANT PARAMETER SPACE
  - BLOCKAGE FORMATION AND MELTOUT
  - MODEL DEVELOPMENT

DISASSEMBLY PHASE - PROGRAM CONTENT

FY 83:

- PRE-DISPERSED, MOLTEN UO_2-SODIUM FCI PROPAGATION EXPERIMENTS (2)
  - IF NO PROPAGATION, THEN NO FCI WORK AUGMENTATION
- TEXAS THERMAL DETONATION MODEL
- REPORT ON KfK FUNDED FUEL E.O.S. EXPERIMENTS AND ANALYSIS

FY 84/85:

- NO FURTHER WORK PLANNED

CORE DEBRIS BEHAVIOR

NEED:

- DEBRIS FORMATION AND CHARACTERIZATION
- DEBRIS-BED DRY-OUT LIMITS, INCLUDING BED DYNAMICS
- POST-DRY-OUT BEHAVIOR AND MELT PROGRESSION
- EX-VESSEL LONG TERM DEBRIS COOLABILITY

FY 83:

- FINAL YEAR OF JOINT PROGRAM: NRC (45%), EURATOM (35%), PNC (20%)
- FINAL THREE SODIUM-COOLED ACRR EXPERIMENTS, INCLUDING: BOTTOM COOLING, EXTENDED DRY OUT, BED DYNAMICS, AND STRATIFICATION
- TWO DRY-CAPSULE ACRR EXPERIMENTS ON EXTENDED DRY-OUT TO STEEL AND FUEL MELTING
- MODEL DEVELOPMENT FOR BED STRATIFICATION, CHANNELING, DISRUPTION, AND POST DRY-OUT BEHAVIOR

FY 84/85:

- POSSIBLE FOLLOW-ON WORK BY NRC AND PNC
  - ANALYSIS OF PREVIOUS EXPERIMENTAL RESULTS
  - BED DYNAMICS PROCESSES
  - POST DRY-OUT BEHAVIOR AND MELT PROGRESSION
  - IMPROVED MODELING OF BED DYNAMICS AND POST-DRY-OUT BEHAVIOR
  - EX-VESSEL LONG-TERM DEBRIS COOLABILITY