NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

ADVANCED REACTORS

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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Wednesday, July 11, 1979

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

ADVANCED REACTORS

Room 1167
1717 H Street, N. W.
Washington, D. C.

Wednesday, 11 July 1979

The ACRS Subcommittee on Advanced Reactors met, pursuant to notice, at 8:30 a.m., Professor William Kerr, chairman of the subcommittee presiding.

PRESENT:

PROF. WILLIAM KERR

DR. J. CARSON MARK, Member

P R O C E E D I N G S

PROF. KERR: The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Advanced Reactors.

My name is William Kerr. On my left is Carson Mark. And we expect at various stages of the meeting Mr. Plesset and Mr. Carbon, I think.

We are here to review the NRC research program on advanced reactors. The meeting is being conducted in accordance with the provisions of the Federal Advisory ommittee Act and the government a he Sunshine Act.

Mr. McCreless is the designated federal employee. Rules for participation were announced in the Federal Register Tuesday, June 26th.

A transcript of the meeting is being kept.

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

We will proceed with the meeting, and I call on Mr. K ber to begin the presentation.

DR. MARK: Could I ask, Mr. Chairman, what is the expectation with respect to the duration of the meeting?

PROF. KERR: The scheduled meeting is to end at 11:30, but I would expect that we should be finished by then.

MR. KELBER: Ready for me now?

PROF. KERR: Yes, sir.

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DR. MARK: Charlie, you are going to tell us about the future. Are you going to tell us what advanced reactors means?

MR. KELBER: Advanced reactors in the context of the U.S. program means anything other than a conventional land-based light water reactor, light-water cooled and moderated reactor.

DR. MARK: So an FNP is advanced.

MR. KELBER: To the extent that it poses unusual problems because of its site and the nature c [illegible] site.

DR. MARK: Some mixed oxides.

MR. KELBER: A plutonium recycled -- if thermal reactor plutonium recycled were to be entered into, certain aspects of fuel behavior would presumably be shared between us and the fuel behavior branch and light water reactors.

DR. MARK: But primarily we are thinking of LMFBRs?

MR. KELBER: That's correct. That is the primary focus.

(Slide.)

The budget that we have submitted this year is responsive to the recommendations of the ACRS. And it was the ACRS recommendations that were the foundation of our planning for '81.

Is that a curve of budgetary prospects, going high up and then down to zero, then?

(Laughter.)

MR. KELBER: No, let me explain.

I interpreted the thrust of the ACRS recommendations to be that we should have a certain amount of program balance that was not perceived to be present in our program.

I interpreted the thrust of the ACRS recommendations last year to be that there was too great an emphasis, relatively speaking, on the core melt accident, and rather too little on accident initiation and prevention, and somewhat less than desirable on the containment, the traditional licensing concerns.

In response to that, we had hoped to be able to address part of that issue during the fiscal '80 budget, and found that we face perhaps an effective budget cut; we don't know yet. But certainly, the budget in '80 that we were talking about when we dealt with you last year was decreased by OMB and has been further decreased as part of a general budget decrease in the House.

We do not know wha the Senate action will be, but it is quite likely that we will face actual budget cuts, rather than even keeping up with inflation.

DR. MARK: Could you just in one sentence say -- last year you had suggested you wanted X million, and in fact --

MR. KELBER: Yes. At the time we were discussing with you last year, we were discussing a budget at the level of $16 million: $15 million in operational support and $1 million in equipment.

DR. MARK: For '80?

MR. KELBER: For FY '80. We are now facing a budget of approximately $12.5 million in operating, which is what came out of the House Appropriations, and we don't know how much in equipment, but it will be well under $1 million.

We do not know what the Senate action will be.

Part of the decrease, I might say, was that though the Commission had requested a minimum level from the President for support of gas-cooled reactors, OMB made a decision to terminate the domestic program and zeroed that item.

The House and the Authorization Committees in the Senate have authorized and appropriated $3.7 for the gas-cooled reactor program, and have made a substantial increase in the DOE program, as well.

We have no reason to suspect that the Senate Appropriation Committee will do otherwise, and so part of the problem is that some of our funds are being diverted to the gas-cooled program.

PROF. KERR: Now, when you talk about $12.5 plus $1, which is what you said you now faced --

MR. KELBER: $12.5 plus something less than $1.

PROF. KERR: Okay. Does that include the $3.7?

MR. KELBER: No, that would be additional. It would be approximately continuing the gas-cooled reactor program at roughly its current level.

PROF. KERR: So if that held, you would have something like $16.2 and something less than $1 in addition.

MR. KELBER: That's correct, for equipment.

PROF. KERR: Okay.

DR. MARK: Now, in your judgment, could you tell Congress what they should do, how much should go into gas-cooled?

MR. KELBER: We have asked that if they wish to continue the gas-cooled program at a sustaining level, they appropriate separately $3.7 million.

DR. MARK: So the amount you are talking of is an amount they can understand, see how to use.

MR. KELBER: It is a minimum program, and we will address that in some detail later.

I might introduce now, my guess is, with respect to the gas program and then return to that.

The Department of Energy has been taking a second look at the gas-cooled reactor program, having initially become disillusioned with it last year, and at least in the minds of key individuals, thinking much more favorably of the direct cycle HTGR as an advanced thermal reactor, on both the grounds of its inherently better conversion of fuel, and inherent safety features.

In particular, the direct cycle HTGR does not face the problem of moisture ingress from a steam generator leak.

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It has other problems, but it doesn't have that one.

DR. MARK: What is a rough measure of the advantages in fuel use?

MR. KELBER: The conversion ratio, depending upon what type of fuel, whether it is highly enriched uranium or medium enriched uranium, the conversion ratio might be anywhere from the low to the high 80 percent, as compared to 55 percent in an LWR.

Now, the amount of good that does you depends upon how much burn-up. Since they are low density machines, you really have to go to very high burn-up to get a great deal of advantage from conversion.

But I always -- for years I have been an advocate of high conversion plus burning the plutonium in situ, rather than reprocessing it.

DR. MARK: That is a really impressive and useful number, 80 percent instead of 50 or something of that sort.

MR. KELBER: It is possible to go into the 90 percent region in special cases.

DR. MARK: But you can get up into numbers of that sort?

MR. KELBER: That's correct.

DR. MARK: Then you also mentioned inherent safety advantages. What are they?

MR. KELBER: The primary safety advantage which has

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impressed DOE, and I think impresses everybody, is the large amount of thermal capacity in the core, coupled with the fact that the core is basically a low power density core.

And that means that in the event of a low of cooling accident, you do have a considerable amount of time before you face massive amounts of fuel release with the prismatic block and the dispersed fuel structure that the only vendor in the U.S. is considering.

There would be some release. There is always a little bit of release, and as the temperature goes up, that release becomes more rapid. But it doesn't get to be really significant for about four hours.

This implies that you can use a variety of techniques to put into place emergency core cooling systems, which they like to call core auxiliary coolant systems.

I don't know what would be proposed for the direct cycl HTGR, but I think it might resemble the conclusions that General Atomics reached in connection with their gas-cooled fast reactor studies, and that is, that the most reliable source would be a 48-hour battery storage device, that is a battery room good for 46 hours of driving the circulators.

I don't want to make any commitments with that.

DR. MARK: I'm not asking for any details, either.

We are talking of circulating helium, and you are saying they could fend off bad consequences for 48 hours.

MR. KELDER: Before 4 hours, they would have 4 hours before they really got into any serious release within the containment, roughly speaking. And so they would have essentially 4 hours to put emergency core cooling into place.

I am not sure of the length of time that would be needed to continue the emergency core cooling, but if they can maintain roughly a 2 atmosphere back pressure within the containment, they can after some fairly short lenght of time, and I don't happen to remember what it is, but I think it is on the order of 2 or 3 days, go on to natural circulation with the helium-and-air mixture that would be within the secondary containment.

This is based on the idea that the loss of cooling accident would arise primarily from an opening in the primary containment. If it arose from a loss of power to the circulators, they would probably be in somewhat a better position, because the helium by itself is a better heat transfer agent than the mixture of helium and air. They would have to have emergency core cooling of some sort, however.

DR. MARK: But the advantages, then, from the safety point of view, we don't have to discuss or worry about this miserable business of boiling and steam versus water and supplies of water.

MR. KELBER: That's right. It is a single-phase coolant.

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DR. MARK: And the handling of the material is very consistent with what has been going on in the helium.

MR. KELBER: That's right.

PROF. KERR: Please continue.

MR. KELBER: The thrust, as I say, of the recommendations made in Chapter 6 of your report, was to restore some balance to our program.

We anticipated being able to move in that direction in fiscal '80; it now looks unlikely that we will be able to do very much, and in fact, we may have to make some cuts.

We will try, however, in doing that, to see whether there isn't work that can be carried over from the gas program, assuming that it is carried on, so as to help us in '81.

In constructing the '81 budget, we first took a look at the current balance that exists in our program, and as you can see as is reflected in the report, the majority of the effort is here under core melt accidents. There is a substantial contribution in the area of containment, and rather little comparatively speaking under accident initiation and prevention.

To some extent, this reflects the corresponding distribution of funds within DOE, where there is a very significant concentration of effort in this particular area, and of course, their well-known efforts at Argonne and HEDL aimed at core-melt accidents -- which, however, are not the

RMC 10

major proportion of their effort.

DR. MARK: Could I ask -- the fact that there is $3.7 or $3.9 million mandated by Congress for work in this field --

MR. KELBER: The gas-cooled reactor field, yes.

DR. MARK: Even though you hadn't originally necessarily proposed, it results from what? Effective lobbying on the part of some vendor, or great understanding on their part that this is a good thing?

MR. KELBER: I'm not a political commentator, and I really don't know what drives Congress to make certain decisions.

PROF. KERR: That's a good answer.

MR. KELBER: But the nature of the questions that we have received --

PROF. KERR: I thought you were going to say you didn't know.

(Laughter.)

MR. KELBER: I was going to say that he had uncovered at least one of the strain of questions in Congress. And all I can add is, there have been several questions related to the question of the relative safety of different systems.

DR. MARK: That has caught some enthusiasm, I presume.

MR. KELBER: As to what drives Congress to do some things and then not others, I don't know. Few people do, I

RMG 11

believe.

We hope in '81, at the projected level of $22.1 million, which is a number constructed through this process of meeting the recommendations -- we hope to raise this figure to the order of $7 million, raise this figure slightly, reflecting the construction of a flowing sodium loop to about $8 million, and raise this figure to about $7 million, reflecting a greater emphasis on containment strategies and the actual sources in containment concerned with core melt, with retention of melt, and core within the vessel, as opposed to retention in the secondary containment.

The question of possible vented filter containment, as I was mentioning informally earlier, we think that this should be done as a part of a systems-type analysis, rather than study each term individually.

And to aid in that, our prime tool will be the new CONTAINER code, which I am happy to report is coming along in a steady, very satisfactory fashion not tied to a particular concept of containment.

PROF. KERR: Now, when you say this represents the $22 million that you asked for for fiscal '80 --

MR. KELBER: That would be the division. We then come up with a program which is pretty closely in balance.

end #1

MR. MC CRELESS: Charlie, you said 80, but do you mean 81?

MR. KELBER: I do mean 81. There's no question that in '80 we are going to have severe financial problems. That's been reflected in a slowdown in the last half of '79.

If there are no further questions about where we're going fiscally, I would like to turn to, first --

(Slide.)

PROF. KERR: Tell me what an LOA is?

MR. KELBER: LOA stands of "line of assurance." It reflects t. e way the Department of Energy has chosen to manage its safety program.

DR. MARK: It's Jerry Griffiths buzz word.

PROF. KERR: They got away from defense and into assurance. Okay.

MR. KELBER: During the preparation of the proposed supplemental budget for '80, we received a memorandum from Roger Boyd to Roger Mattson in NRR, giving the summary of their views on our fiscal '80 budget for advanced reactor research, which essentially summarized their views of our program. And I will say that this was a completely unsolicited testimonial.

It, in fact, came as somewhat of a surprise to us, but a gratifying surprise. The NRR Division of Project Management endorsed our program and encouraged it.

This is in line with Mr. Denton's comments at the time of program endorsement last winter;or early this year, they stated they felt continuation was important in resolving key safety issues. And it was consistent with the Administration's desire -- it's been repeated on a number of occasions -- to maintain a sufficient R&D program in this country, not to close out the fast breeder.

PROF. KERR: Try and let me see if I understand Roger's comments. His first comment seems to say that if you didn't have any money, you couldn't do any work. Is that profundity of statement, or is there something missing?

MR. KELBER: That by the amendment that had been proposed -- and which is not, by the way, going forward in that form -- I suggested a number of alternatives to the Commission for funding the supplemental budget.

One of the alternatives was to zero advanced reactors in fiscal '80.

PROF. KERR: It seems to say that if you zeroed advanced reactors, you wouldn't do any work on advanced reactors.

MR. KELBER: That's absolutely correct, of course.

PROF. KERR: That does not strike me as completely profound, so I decided I must be missing something. There must be a subtlety there.

MR. KELBER: The only context is that what is being objected to here is an alternative referred to Commission -- NRR is being urged in that memorandum not to concur with that alternative because of the need for contining this program. They do not consider that alternative there as one that should be placed before the Commission.

I don't know that I need to comment on this more, except to say that it is noted in the report -- and it has been noted on the report earlier, and it's no secret -- that we have had our problems over the years of communication with NRR.

We have tried very hard to improve th t. We have very good technical discussions at the working level. I have -- and Bill Gamble on his side have worked at it hard, and I think that this letter is gratifying evidence of our increased ability to communicate.

DR. MARK: Charlie, could I ask, in this letter it says the cut from $13.7 million to zero, that they think is a bad idea, that's in Roger's letter -- the 13.7.

In the graph to which you referred us first, you have a number which read 3.9. In the comments you made on that, you also used the numbers 22.-something or other. What are these different numbers?

MR. KELBER: Let me review the budted structure.

In --

DR. MARK: Well, we're talking, I believe, of the research budget.

MR. KELBER: The research budget for fiscal '80.

DR. MARK: And DOE is spending money separately.

MR. KELBER: For fiscal '80, the President's budget requested $13.7 million for fast breeder reactor safety research in program support and zero dollars for gas-cooled reactors. It requested $800,000, I believe, roughghly for equipment, but I'm not sure whether that was broken out as a separate item. I think it was part of a larger total.

The budget that came up to the House and is now before the Senate Appropriations Committee contains a total of 16.2 for adv nced reactors, divided up as follows:

3.7 for gas-cooled thermal reactors; and

12.5 for fast reactors.

The budget submitted to the Commission by RES this year for fiscal '81, and the budget that you are now reviewing carries 22.1 for fast breeder reactors, and 3.9 for gas-cooled reactors.

The BRG, the Budget Review Group, in reviewing the budget, for reasons that appear to me to be completely inexplicable, decided that the fast breeder reactor issue represents a policy issue, and decided to set it aside.

Now, why they view it as being a policy issue, I do not know. The NRR and the Commission endorsed our program in February and again in this letter.

The President, on a number of occasions, has stated that it is national policy to maintain an active research and development program in fast breeder reactors so as not to forelose that option to this country.

The Department of Energy has obviously gone along with this.

The Congress agrees, and the principal difference with the President is that the Congress, or certain elements of the Congress, wish to take a more aggressive role. And that is being debated now in the Congress, and the debate may await the President's energy speech before it's concluded.

The GAO, the Controller General, has endorsed a healthy program, including the NRC's program; and, in fact, had had several discussions with us before they formulated their report on the breeder reactor.

DR. MARK: Now, in using these terms -- "fast breeder reactor" -- what comes to my mind is a sodium-cooled thing.

MR. KELBER: That is correct. That is the major focus.

DR. MARK: A gas-cooled reactor is not a breeder.

MR. KELBER: A gas-cooled, fast reactor will be a breeder, of course, and it is not ruled out in any of the discussions to date.

DR. MARK: To what extent ar thees things separated in the conversations of Congress and the Commission?

MR. KELBER: The primary focus is on the

sodium-cooled breeder.

DR. MARK: That is what is commonly referred as to the fast breeder.

MR. KELBER: I thing that had it been possible to show a marked nonproliferation advantage to the gas-cooled fast breeder, it might have received very considerable support. But for reasons which I think we discussed to some extent last year, I believe it is now the feeling that that was a chimera; and they are, I think, abandoning that aspect.

DR. MARK: In general terms, the gas-cooled reactor is not tied in with the word "breeder." It's a gas-cooled reactor.

MR. KELBER: Well, the current support in Congress is aimed at the gas-cooled thermal reactor quite explicitly. I do not know how the Senate feels about that.

In the House, the word "thermal reactor" appears, and that is not a breeder, obviously. It is a good converter, but it is not a breeder.

DR. MARK: Thank you.

MR. KELBER: So as I say, I do not know why the two individuals on the BRG panel who were instrumental in the review of our program decided that this should be a policy issue.

Neither one of them has ever spent any time discussing it with us, nor did they discuss with us during their review.

On the other hand, we must concede that the full BRG didn't go along with their views. They must see something here that neither the President, the Commission, the Controller General, or the Congress see.

And I must say I, too, am completely mystified.

This means that in reviewing our budget the only source of advice to the Commission will be RES, on the one hand, and your Committee, on the other.

And in that respect, I would say two items -- one is that I feel you should not shrink from putting in numerical recommendations if that's the way you feel. Number 2 is I think you should avoid the view that money not spent in this effort will necessarily be spent elsewhere. The budget process does not work that way at this level.

It is sometimes true, at a late stage in budget formulation, for example, in the Appropriation Committees, that once a total is fixed, individuals will then attempt to reapportion the division of that of that total. But that is not the process that happens at this stage.

Each line item is reviewed individually and on its own, as well as the total of the budget, so that, for example, cutting out the entire advanced reactor,safety research program would not make any $22 million available for research connected with Three Mile Island, as an example.

Well, I told you that we took your recommendations to heart.

(Slide.)

As you recall, I asked for them very specifically because of the problems our program faced. So I would like now to address, in roughly the order in your report -- not precisely that order -- what we would like to do with the $22 million to meet your recommendations.

First, the major recommendation was to make a comprehensive study of safety issues for the commercial LMFBR. We have a small event tree program in place. It has problems staffing up.

We have had the advantage of some superb loan employees from England and from the Air Force. I must say that I have recently received a report from the U.K. on their progress in this area. And they have come across some of the same stumbling blocks we have, which are how to organize the event trees associated with core melt accidents because so much of the phenomenology is uncertain.

Nevertheless, we are developing slowing -- more slowly than we would like -- a cadre of people to do this, and in connection with the conceptual design studies to be released by DOE in fiscal '81, we would, at that point, focus on the safety issues that we think address the consensus of U.S. vendors as to the direction for commercial LMFBRs in this country.

There has been some preliminary work by EPRI in this area, and they have come up with a small by impressive study on a pool-type design. I believe that closes out their effort, but it has addressed the question of accident initiation rather well, reflecting some of our own concerns. And we believe there is an intention with DOE to follow up on some of their work.

And we have established a working relationship with those groups. We would staff up considerably in this area and initiate such a study on a grander scale. We at least have the techniques in place. We are carrying out cooperative studies now with the U.K.

Again, there is some cooperation from DOE in this area, largely in using their advanced codes.

PROF. KERR: Charlie, tell me a little more about what is meant by carrying out a cooperative program with the U.K. How many people are involved, and how are they involved, and what are they doing?

MR. KELBER: There is a team of roughly three people at Argonne, Harry Hummel, Phil Pitzika and one other -- plus an unknown number from DOE on our side of the fence.

The British have a significant number of people -- I believe, about six -- involved in the study of accident initiation in a commercial-sized LMFBR. According to the British conception of a commercial plant, the area of concentration right now is on the reactivity feedback coefficients, the Doppler coefficient and the sodium void coefficient, which would lead to "benign shutdown," -- to use a buzz word phrase -- of an event which might otherwise go into a whole core accident -- in other words, stop an event from autocatalytic propagation, an event which involved melting of fuel in some subassembly.

It is premature now to discuss numbers, but Harry Hummel has been coming to the feeling that there is a band of reactivity feedback coefficients involving both the Doppler coefficient and the sodium void coefficient, where it is likely to be correct to say that an accident, even on the subassembly scale, will be terminated by inherent reactivity feedback even if there is no scram before there is an autocatalytic propagation of melting through the core.

PROF. KERR: Let me read -- and I'm reading from the ACRS recommendation, but I'm actually taking it from an NRC memo -- "The NRC should undertake a comprehensive study of the safety questions that are likely to arise from commercial LMFBRs.

"The ACRS believes that there is a high priority need to review all possible sources of serious accidents -- for example, loss of shutdown, heat removal capability, their probabilities and their level of seriousness in plants of commercial size.

"Considerable use of probabilistic analysis techniques should be made. Preliminary conceptual designs should be utilized in the studies as a means for focusing on an integrated approach to the solution of problems such as post-accident heat removal." Are you telling me that this cooperative program that you just described is in response to that?

MR. KELBER: It's in response to a part of that. but that is a massive effort. The skeleton of that effort will be the event tree stuff.

PROF. KERR: I'm not trying to put words in your mouth; but I had thought you were saying that there was a cooperative program between the U.S. and the British, and that it was in response to this.

MR. KELBER: Let me repeat that there are three actions that are in direct response to this. There are actions that address shutdown cooling directly, because that is so significant in its own right. And I'll come to that.

The vent tree program furnishes the skeleton, the analytical skeleton, as it did in WASH-1400. And we intend to emulate their process.

PROF. KERR: It seems to me, if I understood what the ACRS had in mind, and if I understand what I read here -- and I may be reading things into it -- that what was being suggested was survey of all -- field of accidents, rather than a concentration, for example, in feedback coefficients in the core.

Now, certainly, that's one of the things one would look at. But the emphasis here is on a comprehensive study.

end t2

MR. KELBER: I am coming to another part of that. But let me go back over these grounds. The events tree study forms the skeleton and directs the comprehensive study. We want to emulate WASH-1400 because of its success in doing this.

PROF. KERR: Now, again, what has preceded the event tree? It seems to me, before you draw an event tree, you need to have defined the system. Is it assumed that the system is defined and that one is at a point at which one can start drawing event trees? I mean, for example, the language here talks about preliminary conceptual designs. Now, the implication here is that one is still in the stage of exploring various alternatives. When you get to the event tree stage, unless you're doing a lot of alternative event trees, you have, in a sense, already defined the system, and now you're looking at a specific system. So, I am not quite sure where we are in this stage of things.

MR. KELBER: We have started the event tree work by focusing on the only design we have, which is the CRBR. Primarily, this is to isolate certain issues, such as the phenomenological difficulties I have referred to, the issue of whether or not you go into whole core accidents or not, which is what is being addressed in the cooperative study, and issues associated with shutdown heat removal. And I am going to treat those separately.

Now, it has been known for some time that the CRBR design, if it were to start from scratch, would treat shutdown heat removal somewhat differently. But our desire here has been to build up a cadre of people who are skilled in the art and knowledgeable about the general characteristics of these plants and to focus on the product of the conceptual design study that DOE is carrying out, as representing the consensus of U.S. vendors' thoughts on a commercial plant.

Now, I think this is going to be an iterative process, both on their part and ours. But we have to get started, and we will use that conceptual design.

PROF. KERR: That's not CRVR?

MR. KELBER: No, that's a conceptual design study which will be aimed at a plant with at least the characteristics of a commercial plant.

PROF. KERR. You had mentioned CRBR earlier.

MR. KELBER: CRVP is the tool we are using to build up a cadre of people. They're doing event trees for that CRBR because it's the only design we have.

Now, if DOE were not going to develop the conceptual design study, we would then use the EPRI pool system. But with our limited resources, I feel we have to focus on something that has a chance of being a licensed machine at some stage, or at least which has the major characteristics of a machine that will be proposed for licensing.

PROF. KERR: I would guess, again, trying to reconstruct, that one of the things that ACRS might have had in mind was that at this stage the licensibility and safety of the final design perhaps could be influenced by studies in safety research which would indicate that one or another design might be safer or more nearly licensed.

MR. KELBER: You are getting a little bit ahead of my story here.

PROF. KERR: I don't wish to do that, so I apologize.

MR. KELBER: It's no problem. We are very conscious of that, and I separated out shutdown heat removal and natural circulation because I wanted to address that separately, where we are making just such considerations.

Let me just close out. We have, as you know, been carrying out some joint studies with the FRG because it's by no means clear to us that mixed oxide fuels are necessarily the fuels of commercial plants.

I am, however, informed by Wolfgang Barthold, now with Science Applications, Inc. that it is possible to make an optimized mixed oxide fuel design using large pins with a doubling time of approximately 10 years, and that probably would qualify as a full-fledged commercial design.

Whether the DOE conceptual design study will focus on such a design or not, I do not know. But I think it should have the element of commercial viability to qualify it.

Now, let me explain a little bit here about the question of the recommendation to initiate scoping studies on GCRs, gas-cooled reactors.

The statement is: Funding not available. That assumes that the administration decision not to fund the gas-cooled reactor is maintained. If the decision is otherwise, if in fact there is an active gas-cooled reactor program as it now looks and if that continues within '81, then we would seek funding under the gas program and carry out this work under the gas program.

Another recommendation was: Initiate studies which place emphasis on CDA prevention.

PROF. KERR: That would presumably, from what you said earlier, be confined to the thermal gas-cooled reactors, if the congressional language --

MR. KELBER: It depends on the congressional language, which is not complete yet.

PROF. KERR: Okay.

MR. KELBER: And I think we have to be responsive to Congress in their desires. If they direct the gas reactor program to focus on the thermal reactor, then I think we have to be responsive to that.

As part of this rather broader comprehensive study, the recommendation was made that we initiate studies which place emphasis on CDA prevention. I have already mentioned the work that Hummel is doing on the accident initiation: Are there inherent characteristics of a design? And, as you know, by a variety of techniques, one can, to some extent, tailor the feed-back coefficients in a fast reactor. Are there inherent characteristics of a design which make whole core accidents involved progressions a great deal less probable, particularly those which might be accompanied by very serious excursions.

PROF. KERR: This again is Hummel?

MR. KELBER: This is primarily Hummel plus some DOE asssitants, as well.

PROF. KERR: We're depending pretty heavily on Hummel, then?

MR. KELBER: He's a very good man. Otherwise, I think we would have to cover our bets. Yes. If Harry Hummel didn't exist, I guess we'd have to invent him. But he's a key man in this respect.

I have, by the way, recently talked with Harry about comments in this area. He indicated that he thought perhaps towards the end of the year he might be prepared to give a seminar on this general topic. And when we arrange that, we will be sure and get invitations to the ACRS.

PROF. KERR: This general topic is?

MR. KELBER: The question of the role of reactivity feedback coefficients in decreasing the likelihood of whole core accidents.

Now, we have been planning some work as a follow-on, the so-called "Phase 2" of the event tree studies we have been carrying on. And the emphasis there is two-fold.

And the emphasis there is two-fold: What are the causes of whole core accidents? Of course, we're familiar with the generic topics of loss of flow or some arbitrary reactivity insertion. But these are not the causes in terms of event trees. So, the question is: What are the causes in terms of systems that you find in any reactor plant that might lead to these accidents, and what are the routes of greatest payoff to rendering those causes much less probable.

Now, in that same respect, DOE has a very significant effort mounted. They are addressing two parts of this topic: One is component reliability, and the second is ensuring SCRAM reliability through a redundant independent system of control rods. And today and tomorrow, we are having an extensive information meeting with them, with DOE and their contractors, to understand the status of work.

Where we would attempt to fill in, I do not yet know. I think a great deal will depend on the information we receive. We don't want to duplicate what they are doing, but I think we want to use this follow-on work from the event tree study in conjunction with the work that DOE is doing, either to provide them with our insights as to where they might be weakest in their approach or may have omitted something or if it is obvious that there is a crucial area to do some confirmatory work of our own. But it is much too early at this stage to try and identify anything specific.

PROF. KERR: Let me make sure I understand your use of that chart. The UK-USNRC sort of refers to what Hummel is doing.

MR. KELBER: And Paul Moorhead in England.

PROF. KERR: The added work to follow event tree is what?

MR. KELBER: This is work that we will do at Sandia.

PROF. KERR: It's not yet under way?

MR. KELBER: It's currently being planned.

PORF. KERR: And it will follow what is referred to above as the "event tree program"?

MR. KELBER: It is pa_ _ of the overall event tree program. And that's a specific activity to follow-on.

PROF. KERR: It's planned for Sandia at sometime?

MR. KELBER: We would hope to get it started next fiscal year at a low level.

PROF. KERR: And then information meeting with DOE and contractors.

MR. KELBER: They're going on today and tomorrow.

PROF. KERR: That's mostly information development, and after you develop the information you will decide what to do?

MR. KELBER: We will then follow with some in-house planning over the next several months and depending upon our fiscal '80 resources attempt to do something. If it is obvious what we ought to do in this area in fiscal '80, I think that it will become much clearer as we get a more detailed conceptual design and as DOE focuses its own programs and the conceptual design study.

PROF. KERR: Now, suppose -- this presumably is based on the assumption that you would get in fiscal '80 your proposed budget.

MR. KELBER: That's correct.

PROF. KERR: Does this have associated with it any amount of money? Or you're going to get to that?

MR. KELBER: I am coming to that. I have some delta charts.

(Slide.)

Another recommendation was to study the pros and cons of alternate containment designs. We have three programs in place, and in conjunction with these three programs and planned work associated with the floating nuclear plant, some work that is being done on the probabilistic staff on containment systems. We would like to make a systems analysis of the problems here involving not only the alternate containment designs as filtered and vented versus closed, but also the questions of core retention in case of core melt either partial or total. Is it more advantageous to retain the core in the primary system, even at the threat of adding a great deal of complexity to the lower plenum structure, as opposed to a secondary containment; if you use a secondary containment, ought it be a refractory or a sacrificial material. What is the extent of threat in sodium-cooled systems from sodium on concrete, sodium reactions on concrete.

Let me digress a moment. I know that for Superphenix 2, which is to be a more economical plant than Superphenix, there is a purposeful effort to move the lining of the concrete cells outside of the primary sodium system, and there is correspondingly now a reawakening in France of the need to study the problem of the sodium interaction with concrete.

As you know, we have found that there is a fairly complex chemistry to this problem, and there is apparently an intermediate band of temperatures where the sodium is hot enough to start a serious gas-producing reaction with the concrete, but not hot enough to produce a protective layer that would inhibit further reaction. When the sodium gets very hot, it apparently may indeed produce such a layer; and when it's very cold, of course, the chemical kinetics themselves are too slow to produce much reaction.

Now, a question which is tied in --

PROF. KERR: The comment you referred to earlier, "three programs," which three, where?

MR. KELBER: CONTAIN is the code being developed at

Sandia. It is the --

PROF. KERR: So, it doesn't exist yet?

MR. KELBER: It exists in major parts. It is not yet available in anything like a test version. But major parts do exist. The basic code structure is defined.

PROF. KERR: What is meant, then, by "use CONTAIN code," if it's in a developmental stage?

MR. KELBER: By the time fiscal '81 comes around, the version will be ready, and we can address the schedule for containment whenever we're ready to look at that analytical program.

PROF. KERR: How is it going to be used in this particular study?

MR. KELBER: This is the basic code for systems analysis. Having assumed there is a given threat, we will be able, by means of CONTAIN, to model the entire containment system, including the degree of reaction of the sodium with concrete, the question of where the fuel is, what heat loads there are, what radiological sources there are, how they are transported within the containment, and what ultimately is the threat ex-containment. So, it is the basic tool.

The structural integrity program is developing the data related to the failure of liners, the interaction of the sodium with concrete; and the ART program is developing the data related to radiological transport.

PROF. KERR: Who's doing the structural integrity work?

MR. KELBER: Sandia, largely. And the ART program is at Oak Ridge, largely.

PROF. KERR: ART program to define containment threat, and that means what?

MR. KELBER: Radiological source term.

Now, another recommendation which is tied, in our minds at least, to the first recommendation -- that is, a comprehensive survey of the safety problems of commercial types -- is the question: Are new experimental facilities or programs needed to demonstrate the validity of natural convection cooling?

SSC and COMMIX are codes which are now in use --

PROF. KERR: SSC?

MR. KELBER: Is a super-system code developed at Brookhaven and now in use in Germany. Japan is starting to use it. And it's being used by the vendors and DOE as well as ourselves in the U.S. It is being used by us to model the FFTF natural convection tests.

PROF. KERR: How does one use that to determine whether new experimental facilities are needed?

MR. KELBER: We believe that it has substantial capability of modeling scale effects.

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I. c i ot clear. We will attempt to model range of te s in EBR-2, which is of course a very small system; FFTF a roughly 10 times the size -- if we can get data from PFR.

And this has been proposed in some detail to us. But there has been no follow-up. If we can get detail from PFR, the English tests, then we will have very useful data from a very sizable demonstration scale plant, and of a pool-type rather than a loop-type.

We would propose to do some tests of sensitivity to scale, components design and whether you can do satisfactory work by testing individual loops, as opposed to an entire system.

The point is that SSC can model the entire system, or it can be used to model an isolated portion of the system.

PROF. KERR: I would guess that SSC is a very powerful code. You are, it seems to me, expecting a lot of it if it will handle not only the operating regime but the natural convection regime as well. Do you think it will?

MR. KELBER: Yes.

Let me say that we will have a version of SSC for we have a version for loop-type now; a pool-type is underway. A long-term program, that is, for studying long-term natural convection problems is also underway, and the guiding principal there has been John Meyer at MIT.

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And we are preparing a water-cooled version for Three Mile Island support.

PROF. KERR: I am less concerned about the versions than I am the flow regime. This is a tremendous departure from the flow that you get in the operating reactor.

MR. KELBER: It is the transition from fully developed turbulence down to laminar; yes, that is correct. And we had a lot of controvery initially over modeling that transition, particularly in the subassembly. And I don't think that that is entirely settled as yet.

But we do believe that by analyzing the rather well-instrumented tests in FFTF, we should be able to tell what type of errors are being introduced in modeling that transition.

PROF. KERR: Thereby you can determine whether you need the experimental facilities?

MR. KELBER: I don't know whether it is going to be a clear-cut answer. I think that there will have to be some engineering judgment. We are not alone in this, of course, and to this end we are sponsoring a meeting at Brookhaven next February on natural convection, the experience with natural convection in demonstration plants, and whether these facilities are needed.

So that between the analytical effort and the reflection of worldwide judgment we hope to get from that meeting, we do hope to formulate a view on this question.

And I would hope that by next year at this time when we discuss our fiscal '82 budget, or at least I hope we discuss our fiscal '82 budget, we will be able to summarize our views on this question for you.

In this connection, we would hope, we would plan in '81 to reactivate at the level of approximately $700,000 a year the old safety test facility studies. And while there would be some need for attention to facilities aimed at the whole core accident, we anticipate the need to centralize this work in that area, particularly if we do find that some new facilities are need, or that we would, for example, like to take some existing facilities at the Engineering Technology Center at AI and revamp that.

PROF. KERR: The Experimental Test Facility, as I remember, was not aimed at experimental convection cooling.

MR. KELBER: No, it was not aimed previously.

PROF. KERR: What could they tell you about the validity of natural convection cooling?

MR. KELBER: We would reactivate this programmatically. That is, we have a line item in our program.

PROF. KERR: No, I am trying to relate the material on the right to the material on the left.

MR. KELBER: No, no, no. This would be a new STF study, not aimed necessarily at the whole core accident, but aimed specifically at this question:

If there is a facility needed, what should its characteristics be, and how might existing facilit'es be used.

PROF. KERR: Okay. You are saying you would piggy-back this on the other STF studies.

MR. KELBER: The other STF studies are now zero; they are nonexistent.

PROF. KERR: Well, I read up there: "Reactivate STF studies." That means to me that something has been going on and you are going to restart.

MR. KELBER: We would get the group of people involved.

PROF. KERR: The something that was going on was not aimed at convection cooling. That is what I mean by piggyback.

MR. KELBER: Okay. We would get the people involved. We would not necessarily get the same focus.

PROF. KERR: Why would you pick out STF to demonstrate convection cooling?

MR. KELBER: STF is not a facility. STF .. our acronym for this effort, this programmatic effort. It is a generic term.

PROF. KERR: It means anything that has to do with safety?

MR. KELBER: That's correct.

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PROF. KERR: Okay. So reactivate might also read, redirect.

MR. KELBER: Except that it is not yet, there is nothing to redirect. That is why we chose the term reactivate.

Again, there is a recommendation that we evaluate on a continuing basis the need for new, large-scale experimental apparatus. And that would be a charge to the same group. That primarily is what this group does.

PROF. KERR: I would have thought that STF was based on the assumption that the facilities were needed, and one was trying to decide what they should be, or design them.

MR. KELBER: If you will recall --

PROF. KERR: That sort of says to me, let's look and see if we really need them.

MR. KELBER: If you recall the thrust of our old STF effort, which was aimed at whole core accidents, was to define the needs and to define the facility requirements.

I am, by the way, still of a mind that such facilities are needed. I don't see the need to rechew that fat unless there are dramatic advances in our understanding.

PROF. KERR: It seems to me the response for that should be, I don't see the need for answering that question since I already have the answer to it in my mind.

MR. KELBER: That's right. T..is activity, here, then would be to say: Have there been developments in the past

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years which change our understanding that should be reflected in our views.

PROF. KERR: So your response to that would really be, there is no need to reevaluate or evaluate on a continuing basis, I am already convinced.

MR. KELBER: I am convinced personally. I also concede the wisdom of having a group which makes a systematic review to see whether the existing judgment should be changed.

(Slide.)

PROF. KERR: If you will permit me --

I come from an academic background in which lectures generally don't last longer than 50 minutes. I therefore would like to declare a 10-minute break before the next lecture.

MR. KELBER: Could I complete this, and then we will be ready to take a break, and then go into the delta charts, which will be the budget?

PROF. KERR: Okay, but don't take more than 5 minutes.

MR. KELBER: One of the recommendations was continued studies of the CDA and the resolution of problems associated with its postaccident heat removal. This is largely a continuation of the current work reflected in the SIMMER code, the ACRR tests.

We are adding something new, and this is what I wanted to -- this ties back in with the first recommendation.

We have asked Dave Hetrick and his group, which is essentially an international reactor modeling systems modeling group, to look at the question of are there generic differences between pool-type and loop-type systems, with respect to the reliability of shut-down heat removal.

At least one vendor group in this country has identified loss of shut-down heat removal as the greatest likely source of a whole core accident.

I have, in a private communication from the United Kingdom, an estimate that the reliability of shut-down heat removal systems must be extremely high, roughly as high as that associated with SCRAM systems.

PROF. KERR: How high is that?

MR. KELBER: I hesitate to use the numerical value, because it is so bad, that they assume that their unreliability should be no greater than 10^{-7} per year.

And assuming then an average of 10 demands per year, they assert that the unreliability per demand should be no greater than 10^{-8}.

Now, I remind you that the only, that the most reliable system now in place in any high-technology area is the U.K. automated landing system for airplanes. And the actual test of that system allows one to place an unreliability of being somewhat less than 10^{-5}.

And the ability --

PROF. KERR: 10^{-5} of what?

MR. KELBER: Per demand. Now, that's projected to have an unreliability by estimate of less than 10^{-7} per demand. But the gap between what is demonstrated and what is estimated is, of course, 2 orders of magnitude.

And I don't see any ability on our part to demonstrate by test such low figures.

Neither does, I say, one of the vendors in the U.S.

Westinghouse has come to somewhat similar suggestions.

So that we think that this type of approach may be very valuable in tieing this type of study, which is the traditional focus of fast reactor work, to the first recommendation that the Committee made, which was a much more comprehensive study of problems associated with conceptual designs.

Finally, a recommendation was made that there be an emphasis on developing a planned, methodological program of international work. That is in place. We do have some problems with the negotiation of the broad scope agreement with the CEA.

Briefly, in April of this year, we were entering into negotiations leading to a final draft of a broad scope agreement with the French CEA when a decision was made between the DOE and the development side of the CEA to suspend their negotiations in the area of safety.

Frankly, they agreed to disagree on the scope of

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their exchange. When that happened, the CEA suspended negotiations with us.

I was informed that DOE is reopening those negotiations this summer, and assuming that there is some satisfactory resolution, then we anticipate being able to complete that.

We are involved now in detailed negoatiations for an extensive involvement of Euroatom in a joint program of work with us on cooling of debris beds. The same work, the D series program, which was used, by the way, to project cooling under natural convection circumstances in TMI-2.

Well, that ends my review of your recommendations. And after the break, I would like to go rapidly through our actual number charts, the so-called delta charts. And that would conclude my presentation.

PROF. KERR: Fine. I now declare a 10-minute break.

(Brief recess.)

PROF. KERR: Charlie, please continue.

MR. KELBER: Well, let me continue now with a very rapid run-through of our proposed fiscal '81 budget.

(Slide.)

DR. MARK: It won't take long, because it is so small.

(Laughter.)

MR. KELBER: As against our presendential submission for fiscal '80, you can see that the increases come in three

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places:

In analysis, these are the major increases;

In materials interaction, which reflects the sodium loop;

And in system integrity, which reflects the emphasis on containment, and this corresponds to the balancing action I referred to.

The codes that we are discussing are a family of codes that Dr. Curtis will discuss with you at your pleasure, related to details of accident analysis and system analysis.

SIMMER is at this stage in a kind of steady state. That is to say, we aren't going to do a great deal more code development other than the sort of technical code development involved with making it run more efficiently, or exporting it to different machines.

We do have to make some serious decisions as to whether to attempt to make a special version of the codes to handle heterogeneous cores, and this depends to a large extent on DOE and vendor decisions, as well as to some of the insights that we have developed. Right now we do not think that we will have to do that.

If CRBR actually is restarted, and licensing procedures for CRBR reactivated, then we might indeed have to for that particular heterogeneous core have to have a special version of SIMMER.

PROF. KERR: Okay. FY '81 is based on the assumption of $2.4 million increase over FY '80 for analysis, and that's code development.

MR. KELBER: That's code development in the area of accident details.

PROF. KERR: The codes involved are SIMMER --

MR. KELBER No other codes than SIMMER.

PROF. KERR: SIMMER is at the same level of support?

MR. KELBER: The level of effort on SIMMER has been decreasing, and will decrease to about 75 percent of its '78 level, I think. That is about as low as it should go.

PROF. KERR: What about the percent of its FY '80?

MR. KELBER: In FY '80, SIMMER is about 25 or 30 percent of this, and it will be at roughly the same dollar level, plus some allowance for inflation, in '81.

PROF. KERR: Okay. What is the $2.4 increase portion?

MR. KELBER: It is a family of codes called BIFLOW, FRAM, and some others. Bob Curtis can address that question in detail. He is much more knowledgeable than I am. We can do it now or we can wait.

PROF. KERR: Are there 25 codes or three?

MR. KELBER: About 7.

PROF. KERR: Okay. I don't need a lot of detail. I was just curious as to what -- is there some new start-up?

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(Slide.)

MR. KELBER: These are all in various degrees of progress right now.

BODYFIT is a special code for the detailed analysis of sodium boiling experiments involving 7 and 9 fits. It is an extremely accurate code which enables us to actually track the experiment in detail so that we can predict the response of a given thermocouple.

PROF. KERR: Is this already developed?

MR. KELBER: It is in development, and some initial tests have been made.

PROF. KERR: They will do things that various versions of SASS won't do?

DR. CURTIS: Yes. It is metadimensional, a very exciting breakthrough.

Bill Shaw has found a way to map irregular boundaries into a rectargular Cartesian form to solve the continuity equations, and then map the answers back to the irregular geometry. And it looks very exciting.

PROF. KERR: It will run in real time?

DR. CURTIS: If the problem is of experimental size, it certainly would not work for reactors.

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MR. KELBER: Let me add one thing here. Our primary use here is to help on experiments being conducted by Kepler at KFK. In testing the body fit version, we have in some tests intended to match his results, but we have a rather good working relationship beteen Bill Shaw and Poeppler's group at KFK.

COMMIX is the 3-D, time-dependent,sodium-mising code. that enables us to study the mixing of sodium in the various components -- either a subassembly, all buy plenum of pipe elbow, pipe transport run during the transition from turbulence to laminar flow. And it's being used in conjunction with some of the FFTF tests to determine the types of temperature gradients that exist in these compoenents.

Particularly, there are components where mixing may be poor and temperature thermal stresses may be significant.

We do information that the water modeling is not as reliable as people had earlier supposed, and that in conjunction with Superphenix we understand that the French have put in place at Catareche full full-sized sodium test rigs to study this problem.

CONTAIN is, as I have mentioned, a master containment code, not tied to any particular concept, but capable of handling a wide range of containment concepts, including all the effects necessary for systems study.

FRAM is the tentative name for fast reactor accident models. It's to be a code linked to SASS, the new version of SASS, SASS 4-A. However, FRAM is designed -- we use a lot of SASS 4-A in it, but it is designed to do two things:

A, contain specific models, developed either by Licensing or ourselves in response to their concerns of what happens in a subassembly. An example of that is BIFLOW, which is an outgrowth of Theofanc ' work, for licensing on two-dimensional voiding patterns in the CRBR.

And an example of our own work is EPIC, which is the modeling of the fuel motion when it becomes molten. These will be linked to SASS within the framework of FRAM. But, more important, we intend to be able to do a wider range of parametric investigations of the type that Licensing Staff tends to run to understnad the sensitivity of codes like SASS.

We experienced great difficulties in doing problems with SASS with way because of its focus on a particular model. And so we attempt to incorporate in FRAM the ability to test sensitivities of specific models more readily.

SSC we have discussed. SIMMER is well known to you. The cooperative studies we have discussed. We will be extending these we hope in several areas related to fuel failure through the CABRI program.

If we get a broad program going with the French, we intend to enlarge our cooperative studies with r pect to sodium fires, with respect to transport of sodium vapor and fuel gases through the sodium pool, the so-called EXCOPUL experiments.

PROF. KEER: Does "cooperaitve studies" imply analytical -- development of analytical tools or the use of analytical tools to study problems?

MR. KELBER: Either one. It depends upon the particular case. Mostly up to now it's been comparison of the analytical tools applied to a particular problem in an attempt to understand why there are differences.

And an expansion of the probabilistic analysis --

PROF. KERR: And who's doing the cooperative studies?

MR. KELBER: Argonne in particular for us. That's the Hummel work. Sandia is doing work on fuel failure.

PROF. KERR: I hope Hummel knows how much of this depends on him.

(Laughter.)

MR. KELBER: Sandia is doing the work on fuel failure, and LASL -- Jim Scott at LASL, and Bill Kemp at Sandia. We have done some work in the aerosol field, largely at Oak Ridge and Battelle -- and there, largely, with the U.K., and to a lesser extent with the Germans.

MR. SILBERBERG: We're just starting to get into dealing with the Germans.

MR. KELBER: We do hope to explore the sodium fire area with the French, who are probably the leaders in this area.

(Slide.)

Coming back to the remainder of the Delta charts, materials interaction -- we badly need a flowing sodium loop. If we could put flowing sodium loop into the ACRR, we can do a range of tests that go well beyond the kinetics of TREAT by an order of magnitude -- cover roughly the same energe deposition range in TREAT, and significantly extend the CABRI work. And we can accommodate the same size pin as in CABRI.

Now, I don't want to, in saying this, indicate that in any way the work in TREAT is not good work. I think it is necessary work.

DOE is doing a substantial program there. I wish it were moving faster, but it does not cover a range of energy input and rates of energy input that commonly are the focus of attention in the licensing reviews. And in particular, for example, last year, if you may recall, Jerry Griffiths spent some time discussing the potential for an element with a hole in the fuel whereby if that fuel were driven to melting, the molten fuel would then go upward through this hole and freeze in the upper blanket.

This has considerable potential for shutting down accidents by fuel removal without getting ever getting outside the clad.

An English analysis, which by the way was carried out in the scope of our cooperative calculations, shows that this would probably work best in the very high ramp rate excursions. These are precisely the excursions which cannot be tested in TREAT, which can be tested in ACRR and nowhere else, and which, if it can be demonstrated, would indeed be a forward step in our view of safety problems for LMFBRs; so we need that loop.

The other big increment --

PROF. KERR: Now, what dictates whether you should be doing this or DOE should be doing it?

MR. KELBER: First of all, the machine is our machine. We view this as being responsive to licensing concerns directly. Licensing has, since at least 1970, together with the ACRS, made these concerns known to DOE. They have not responded to them.

PROF. KERR: In effect, you're saying that since DOE won't do it, you should.

MR. KELBER: We feel that we have to to do it, yes, because it is a key licensing concerning and I [illegible] no way to resolve it other than by doing the tests.

I don't think anyone believes an argument in which the primary technical tool is waving your hands.

DR. MARK: I resent that as theorist.

MR. KELBER: I'm a theorist, too, Carson.

(Laughter.)

DR. MARK: Charlie, you -- in your first phrasing of this, you made a comparison with CABRI. It did not come clearly through to me in what way this goes beyond what could have been done or could still be done after CABRI.

MR. KELBER: CABRI is limited, in its energy deposition, to approximately 1.3 kiljoules per gram. This will carry the fuel to melting and perhaps to the brink of clad failure. It is doubtful that it will carry it any further than that.

The rates are comparable to those in the ACRP. By putting in a new loop, a zircaloy loop instead of a stainless steel loop, they may be able to raise their energy deposition to something like 1.9 kilojoules per gram.

MR. SILBERBERG: 40 percent more.

MR. KELBER: And that's about the maximum, 1.8 to 1.9.

PROF. MARK: On this scale, CABRI is limited in what respect?

MR. KELBER: CABRI is limited by the nature of their driver fuel, which is uranium dioxide. And they have a temperature limit on it which is, I believe, of 2300 degrees Centigrade. And that means that the energe deposited in the tests made with the stainless steel loop is limited to the 1.3 kilojoules per gram.

PROF. MARK: So you're going 50 percent beyond that?

MR. KELBER: CABRI can extend that by about 40 percent by going to a zircaloy loop, according to their current calculations. We don't know how it will turn out in practic.

We can go approximately four times that, which enables us then to model the type of scenarios that have proved of interest to licensing, because in those scenarios a interest is in duplicating a large amount of fuel motion, fuel melting and fuel motion, and expulsion into the coolant stream prior to the peak of the power curve.

PROF. MARK. Mr. Chairman, I think Mr. Silverberg had a remark.

MR. SILVERBERG: Excuse me, Dr. Kelber, I wanted to add one point, CABRI is also limited only to a single pin, whereas the projections for the ACRR with the loop is seven pins are assured and 19 appears to be possible at this point.

PROF. MARK: That helps make the comparison.

MR. KELBER: I might say that I think CABRI is also a valuable tool.

PROF. MARK: I was merely asking, having made clear the extents in the ways you which beyond it.

MR. KELBER: I'm glad I had the opportunity.

We've mentioned the CONTAIN code, and we do want to test some of its vital issues. We will, of course, be doing the sensitivity studies as part of the systems analysis of containment alternatives, and we will want to be able to test some of the key parts of that. We also will be doing large core melt retention tests with a new facility, and these atoms then correspond to the three major shifts in our program.

We have mentioned the need to reactivate a group to look at various safety test facilities, and we antitipate changing the direction of the art program from its current focus on the radiological source term from the whole core accident.

We believe that we will be closing most of that work.

And now, looking at the radiological source from other types of accidents involving core melting or fuel failure — we hope, by the way, although they're obviously different systems, to get some insight in that from the TMI to recovery.

There clearly are very significant technical differences between the systems. Nevertheless, some of the data should be indicative of the magnitudes we should be looking for.

DR. MARK: There are five tagged items on this list.

MR. KELBER: This corresponds the plans in our program.

DR. MARK: Each goes up. Is there any prospect that some of them might sometime go down?

MR. KELBER: The art program, as I say, is really changing. The short answer to that is no, not in the near term.

(Laughter)

MR. KELBER: Give us five years; yes, I would anticipate that the art program which is now in the next few years, if we can get sufficient funding, going to close out most of the work on the ACDA-related source term -- should, in a relatively short time -- I'm not at this time going to hazard how short that might be.

Mel Silberberg can give you a better view of that, but that program probably will be among the earlier ones to terminate.

So far as fuel failure tests go, if our current judgment that we need large-scale tests does, in fact, change as the result of developments, either as the result of devlopments in SIMMER and tests of special types of fuels and other studies or as the combined result of a wide range of probabilistic studies, that should change.

Then I would guess that the fuel failure studies terminate after perhaps several years work with a loop, and there the pace is the question of handling irradiated fuel -- obtaining and handling of irradiated fuel -- because experiments with irradiated fuel bundles are time-consuming.

DR. MARK: Perhaps it's clear to you, Bill. It's not fully clear to me. These numbers for FY '81 are ones which you recommend, or which have been accepted.

MR. KELBER: We recommend them. RES recommends them as the minimum to maintain a program which is, A, viable, and. B, responsive to the ACRS recommendations.

DR. MARK: So as far as you know, these are also things one expects?

MR. KELBER: No, sir, I don't know what to expect until we get through with the Commissioner and then the Chairman gets through with the OMB.

PROF. KERR: The Budget Review Group has put the 22.1 as a setaside.

MR. KELBER: That's right.

PROF. KERR: But it has not looked -- it has not made recommendations on any individual items.

MR. KELBER: No, sir. It has set the number as either zero or 22.1. And that is the view of RES.

Now --

PROF. KERR: I don't understand that last statement. You mean you're unwilling to take anything inbetween zero and 22.1?

MR. KELBER: If we are to make a program which is responsive to your recommendations and which retains viability -- and I will return to what I mean by "viability" -- we think 22.1 is the minimum.

Now, if you were to choose to weaken the force of some of your recommendations, we could come back and say, well, if we want to deemphasize, let's say, the system studies, we could then foresee removing $2 million worth of work.

However, I don't see any large readjustments. Our program has lost viability. It is a dying program. Key people are leaving the contractor staffs at all of our contractors now.

DR. MARK: The 13.7 for FY '80 is a solid and established number?

MR. KELBER: No, sir. The House Appropriations allocated 12.5. We have appealed to the Senate to restore --

DR. MAKR: But you're at least not tangling with the idea of having zero instead?

MR. KELBER: No, except as a possible recommendation to the Commission on support of the supplement. I don't think that the Commission will take that alternative. That was the focus of Mr. Boyd's letter.

PROF. KERR: Now the FY '79 was 16-something.

MR. KELBER: By '79 was -

PROF. KERR: You got to the 22.1 according to your lstter by adding 25 percent roughly to, as you put it, respond to ACRS recommendations and then put in some escalations.

MR. KELBER: That's right. The FY '79 was 12.5. We had, at the time we discussed our program with you last year, our budget -- our proposed budget was $16 million. That got successfully cut back, because it went up a lot.

end t5

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Your recommendations were based on your understanding of our FY '80 proposal. And we took that as the basis, and went through the various elements of our program.

Now, would the program retain viability at a lesser number? Possibly.

One way of retaining viability is to remove particular areas of concentration and simply say that we are going to concentrate all our forces elsewhere, or simply ignore a question. That is, for example, why we removed the safety test facility work early on. We just didn't have the resources to keep it going, there was no real focus for it, and it represented a drag on the program.

But we have now reached the stage where key people are leaving at all of our contractors. Morale of our own staff is abysmal. I might say that morale among the entire Reactor Safety Research staff is very, very low.

I think this is generally known.

The rest of the agency seems to view us as gluttons, and we kind of resent this.

The safe, bureaucratic course would be to come in with a budget which doesn't change from year to year, and doesn't really accomplish anything, that doesn't represent anything very negative, either.

That would be a very safe, bureaucratic course. That is not the one that is favored by either the research

RMG2

management nor, as far as I know, any of the lower level management within Reactor Safety Research.

I think that in your report to the Commisssion you should feel free to indicate, if you so desire, alternative numerical levels. But I must say, that at the current levels of $13.7 -- if we get that, or even the $16 million that we originally proposed in the light of escalating costs and what is actually happening -- the program will simply continue to die. It will just continue to die -- a little bit more slowly.

But it is extremely disheartening to me to see a well-conceived series of tests endlessly delayed simply because we cannot afford the staff to keep them going, and this is what we face in all aspects of our program.

DR. MARK: What is the breakdown, roughly, because work on codes, pencil and paper and stuff like that, and expenses for tests?

MR. KELBER: Roughly it is as represented here, the $7.8 in analysis, as opposed to $22 in the total. In other words, about 1/3 is pencil and paper, and the rest is tests.

PROF. KERR: Now, wait a minute, Charlie.

Systems integrity says CONTAIN qualification.

MR. KELBER: Well, the CONTAIN code itself is developed. The CONTAIN qualification is special tests. This is to pay for tests done.

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DR. MARK: Like half is on mechanical exercise.

MR. KELBER: Somewhat more than half.

DR. MARK: You said 2/3, really. But as Bill points out, some of the other numbers are really also pencil and paper.

MR. KELBER: Aerosol release and transport bill about $300,000 as code development. It is really a small part of the total. It is the tests that are expensive.

DR. MARK: So 60 percent, then.

MR. KELBER: One can choose a number, but it is a very significant portion that goes for tests. And it is my view, I was brought up as a theorist, and I have done as much as anyone, I believe, in the nuclear community to promote the use of large codes, and am a firm believer in their employment.

I am also a firm believer in licensing on the basis of actual knowledge. And having had plenty of opportunities, as I know everyone else, to match code predictions with facts, I think this split is, if anything, too heavily pencil and paper oriented.

I would like to see more money in tests and less in code work, proportionately. But I think that the code work is necessary to provide guidance as to what tests are crucial, what tests aid our understanding most, and where we should go.

These codes are key management tools, as well as tools for developing insight, and eventually for predicting the

RMG 4

outcome of accidents.

DR. MARK: Okay. So if you don't get $22 million -- I'm not suggesting that is my recommendation -- but only $14, which is 2/3 of that, which is also a number a little similar to the 1980 number, there is $7 million missing. Do they come out of tests?

MR. KELBER: We would start to terminate the program. That is effectively what we are doing now, and we would just terminate it with a certain amount of grace.

I can give you the bottom line. With $8 million we terminate in one year. With $14 million, we terminate in two years.

PROF. KERR: Well, Charlie, you mentioned the need for analysis in order to do experiments. And what little I know about would certainly lead me to believe that one does need to both think and analyze before doing experiments.

On the other hand, I am not altogether convinced that the only way to do analysis is through the development of large codes. One can sometimes do analysis from small codes, and it is even possible to do some analysis with very small codes. One can even think about things --

MR. KELBER: You know, Bill --

PROF. KERR: You know, too, you didn't quite imply it, but one could get that impression, that you have to develop all these large codes in order to do experiments.

It seems to me --

MR. KELBER: Let's look at this. What are the large codes? Really, the largest code that we are developing is SIMMER. That is truly a large code by anybody's standards, with the possible exception of the Weather Bureau and the weapons codes. But it is truly a large code, anyhow.

It is providing us with some valuable insights as to what is important, insights that we do not have through any of these small, analytical efforts at modeling, freezing, and plugging -- any of these efforts to say that if you have a melted down core it will boil up.

Finally, we have found out this year with SIMMER that it won't do that. That type of argument is not really tenable.

What we are finding is that if you do have a so-called transition phase developed, a likely behavior is very similar to that observed in reprocessing plant accidents.

Now, let me go back in my own history. As you may recall, in the late fifties -- I think it was '57 -- I published a report called The Physics of the Argonaut Reactor. That report was used, as a matter of fact, as a text in a large number of nuclear engineering courses in this country and the world. It went through, I think, three printings at Argonne, and something like 7500 copies were sent out.

Almost all of that was rather simple, hand methods;

two group calculations on various configurations of the Argonaut reactor.

A great deal of judgment went into how one approximated the actual shape by what could be calculated by hand.

There was a new method developed by Bob Avery, a modification of the old Nordheim method for calculating one of the configurations. A key part of that was deciding how to represent a trapezoidal fuel box by a circular element.

Now, when we wanted a really accurate answer, however, what we did was we used the best tool available in those days, and that was the MUG-2 code. That at that time could only be run at the Courant Institute in New York.

It has been my experience that when you really want the right answer, you use the best computational tool available.

Now, I don't think that we are doing wrong by continuing the development of SIMMER. It is a key tool in our management of the program.

For example, are all these various experiments that we and others are doing on all these various models on fuel freezing and plugging really that necessary? I am coming to the conclusion, as the result of SIMMER calculations, detailed SIMMER calculations, that to the extent that the fuel freezes within a short distance, freezes and plugs within a short distance of its entrance into the plenum, lower or upper, the details of the model make no difference. A simple energy balance model is good enough.

That only under those cases where the fuel really penetrates a significant distance over the order of 30 centimeters or more is it important.

Now, without SIMMER, I think we could spend endless years discussing the details of different models.

Now, if my judgment is backed up by further work with SIMMER, and by the technical community, I think we will be able to save money far in excess of the cost of development of SIMMER. SIMMER itself costs only about $1 million a year. The analytical program is very cheap for the amount of guidance it gives you.

It gives you far more guidance than you would get from an equivalent amount of work involving simulant fluids boiling in microwave ovens, which we are doing.

PROF. KERR: I recognize your viewpoint on this, and I did not mean to case aspersions generally on SIMMER; it is a very powerful code developed by an extremely competent group of people. And I don't need to reread necessarily, although I do note that in response to ACRS comments, which among other things indicate a view that it may be doubtful that the code can ever be validated in the sense of precise calculations. Somebody writes: "It is premature to place limits on the degree that SIMMER can be validated."

And we agree that primary values and increased understanding -- in fact, I'm not quite sure what the paragraph says, except that it seems to say that it is the view of Advanced Reactors that SIMMER probably can be validated and that this is a desirable long-range goal.

MR. KELBER: Well, let me amplify your remark a a little bit.

It is our view that it can be validated to some extent. The extent to which it needs to be validated and can be validated is yet to be decided. We don't want to close the question that it can never be validated.

PROF. KERR: I go back to my probably misunderstanding of the history of this, which is that the original motivation for SIMMER was that one needed a better representation than one had of the transition phase. And in an effort to develop this, it was discovered that indeed it is difficult to model the transition phase, but that one can model other parts of at least the beginning of CDA rather better by SIMMER than one can by other methods.

It still seems to me that a key part of the process is the transition phase. You said something earlier which seems to imply that SIMMER now permits one to make rather unequivocal predictions about the transition phase.

That's interesting, and I would be interested in seeing some of the details of such a prediction.

MR. KELBER: It is a little premature to say that

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it is unequivocal, but is can make some predictions.

PROF. KERR: What I heard was that we now know that this can't happen. To me that's pretty unequivocal.

MR. KELBER: We now know that the boiling up is not a technically feasible representation of the transition phase; that's correct.

You could force such a representation if you so chose, but it is not one which corresponds to reality.

PROF. KERR: You see, I think the feeling on the part of some of the ACRS people and its consultants was perhaps to go even as far as wondering if even in experiments, the transition phase is reproducible?

Now, if you have something which is not reproducible from one experiment to the other, to have a code which reproduces those experiments means that somehow you have to build into the code a lack of determinism. Maybe SIMMER has this. I haven't seen the most recent version.

MR. KELBER: SIMMER is clearly a deterministic code. But I think --

PROF. KERR: You see, it seems to me that this is an extremely important part of the behavior of a melting core: what happens when it melts?

MR. KELBER: Now, the problem is, Bill --

PROF. KERR: It isn't altogether certain that that is deterministic from one experiment to the next.

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MR. KELBER: Well, the question is, what are you sensitive to when you are looking at the question of where does the core end up, and what is its final state?

PROF. KERR: That's right. If you can do bounding calculations, go ahead.

MR. KELBER: If SIMMER tells you, as I believe it probably will -- we haven't done very many calculations, you understand; we haven't really scoped the entire space -- but if as my judgment now indicates SIMMER tells you that you really don't have to worry too much about the details of whether Subassembly A plugs 5 centimeters in and Subassembly B plugs 3 centimeters in and so on, that doesn't really matter.

If SIMMER tells you that, no, you are not going to get frothing, you are going to get behavior similar to the behavior of aqueous solutions in a barrel, as was observed in reprocessing plant accidents, then these gross details are what are important and the fine details become unimportant.

The reason for all the confusion in organizing or thinking about the transition phase is that there are an innumerable of fine details which, if there is no way of organizing your knowledge and setting priorities on the order of magnitudes of effects, all have to be weighted equally.

If we know that we don't have to worry about liquid-liquid ablation as opposed to dissolution of steel in UO2, then we don't have to worry about that whole class of

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individual variations, and that is what I hope will be the outcome of SIMMER.

So far what we have been finding out about the transition phase bears out that judgment.

I agree that if SIMMER says that if every detail is as important as every other detail, that we are immensely sensitive to these individual variations, then it is going to be very hard to handle the problem other than by the very grossest bounding methods. But so far things aren't going that way.

DR. CURTIS: We have done some early transition phase studies, and I have some very preliminary -- and you will probably regard them as not detailed results.

(Slide.)

But so far the lessons that I see from the early transition phase is here, is blockages are not complete. There is neutronic shutdown of rather small energies.

Complete blockage assures that you are going to melt to a recritical configuration.

The secondary excursions we see are nontrivial, but generally seem to be within the capability of the system to contain.

But if you are nonmechanistic and insist on doing it, you could build an arbitrarily large excursion with your nonreal initial state.

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PROF. KERR: I would very much like to see more detail on this, but I don't think this is the time for it.

I was simply trying to get over to Charlie what I think is the flavor of some of the ACRS concerns about the ultimate capability of SIMMER or any other code to describe something that is as difficult to describe as I believe we all agree the transition phase is likely to be.

MR. KELBER: Bill, I think there is a question of relative optimism on how we approach this problem.

And I guess I have to put myself in the camp of being relatively optimistic that with the use of a too like SIMMER, we can gain sufficient organization in our thinking and understanding of what phenomena are significant and which are not, that we can decide whether this can be handled.

PROF. KERR: I'm with Carson. I believe in being optimistic and doing hand-waving. And if it takes SIMMER to make that valid, okay.

end #6

DR. MARK: You said, Charley, that SIMMER had been costing roughly a million a year.

MR. KELBER: A million a year for the code development and testing and roughly the same amount on some supporting experimental work to get some basic parameters and checks, some basic models.

DR. MARK: So then we'll say two to three million was in this.

MR. KELBER: About two million total was in this package, and keeping it at this dollar level is what accounts for that attrition.

DR. MARK: Being both a believer in codes and skeptical about codes, I rather think that that is one of the last things on which you should hang -- you know, if they strip you of everything else, you should hang onto that.

MR. KELBER: If it comes down to that, and this is what I mean by program viability, we have an integrated program, and eliminating a small part like the STF hurt, but not too bad. But we're getting to the point now where we eliminate one part and everything else suffers. That's why the program is just no longer viable at this level. It is an integrated, systematic program, and we would like to follow your recommendations and focus such a program on a plant that's likely to be typical of commercial designs and make a systematic attack. And we can't do that, crippled as we are.

cah 2

Bill, I would like with your indulgence to skip the detailed slides that are in your package on the individual program elements and, so that there will be time within your time frame for the branch chiefs to discuss these matters with you. They're much more knowledgeable than I am anyhow.

PROF. KERR: Are we going to be anywhere within the projected 11:30 --

MR. KELBER: If I stop talking, yes.

PROF. KERR: If that's the condition, then I agree with you that we should skip the details.

MR. KELBER: I would like to, however, spend a little time with the advanced converters and then introduce the branch chiefs and let you discuss the specific program elements with them.

(Slide.)

The advanced converter line element looks like this. We expect $3.7 million to be mandated by Congress. It's not clear. We don't know what the Senate Appropriations will do. I understand that they're marking up the DOE appropriation this afternoon. Again, the final decision may yet depend upon the President's energy speech. But, we expect to se a $3.7 figure here. If we do not, then we will simply terminate the program with carryover funds in fiscal '80.

If -- we have requested $3.9 million to continue a minimum program in fiscal '81. We fully expect that a decision on the timing of the program and the nature of the plant to be built will be made in the fiscal '81 - '82 time frame. Unlike the LMFBR, the issues here are more related to economics than they are to technology, although there are some technological questions on gas turbines and high temperature technology. They are at least conceptually more directly addressed in the R issues of proliferation and plutonium use and so on.

(Slide.)

Now, the way we would handle this is as follows: In '79 we had a program addressing a variety of generic issues, graphites as structural materials, the core seismic response. These are roughly in the order of the priority we gave them, but that's really not the intention of listing it this way.

In '79 we have concentrated on just these three issues largely because of the nature of the programs in place. And we will be terminating this program in '79, and we will be using carryover funds to terminate these programs in fiscal '80 if there is no further support. If we get support, we will keep these programs going, and we will reactivate small programs in inservice inspection and in containment requirements. Both of these are directed at Fort St. Vrain support. We believe, by the way, that Fort St. Vrain support requires, by itself, of the order of a million dollars a year. And it really doesn't matter very much whether that's technical assistance or research, but NRR has requested nothing in this area. We do not think -- we think there should be some level of support, and that is within the scope of the program request we have made.

PROF. KERR: Excuse me. A million dollars a year, inevitably?

MR. KELBER: No. But then again the type of progress we make, we would hope that in '80 or '81 these issues should be resolved. It's a little difficult to project, because we really have not done a lot of work here, so as to be able to project more definitely when the end is in sight.

In '81, we would add experiments on fuel transient response and we would do some work related to the emergency core cooling provision.

Now, the branch chiefs are here.

DR. MARK: Excuse me, Charley. You referred to technical support. Zero, which has been put down for '80, applies to research.

MR. KELBER. That's correct.

DR. MARK: Technical support is not necessarily zero.

MR. KELBER: I believe it is.

DR. MARK: That's also.

MR. KELBER: I believe so.

PROF. KERR: What you said was technical assistance?

MR. KELBER: Which is the term NRR uses. Yes.

MR. FOULDS: In the Congressional markup where they added $3.7 for '80 in research, they also added $1 million --

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they didn't add it -- they identified $1 million out of NRR funds for licensing work on preapplication review of HTGRs, which would tend to eliminate incidentally spending that fund on Fort St. Vrain. But there is a $1 million figure there as well in that same bill.

MR. KELBER: As I say, we have no idea what the fate of that will be.

PROF. KERR: Any further questions?

(No response.)

PROF. KERR: Thank you, Charley.

MR. KELBER: I don't know what order you would like to go.

PROF. KERR: I'll leave it up to you.

MR. KELBER: Let me suggest then, because we have already discussed some parts of it, that you continue with Dr. Curtis on the analysis program, on the safety test facilities program. And then we might spend a little time with Mel Silberberg on the experimental programs within the fast reactor area and close with Ron Foulds on the gas.

PROF. KERR: Let's do that.

DR. CURTIS: I have an awful lot more material here than I'll obviously have a chance to address, and so, I would like, if there are areas of specific interest, to identify them now, and I'll concentrate on the things that you'd like to talk about.

PROF. KERR: Why don't you pick out the things that you think we should give particular attention to? And if we have --

DR. CURTIS: Well, one idea that I think we should suggest first of all --

(Slide.)

DR. CURTIS: -- there has always been a considerable interest in validation, verification, or testing of codes. And, here is my understanding of some of the concerns of the committee, and I wanted to reaffirm that we are pursuing this line of work with our validation.

The first is fairly straightforward. It means that when you are formulating your models, that you base them on physical laws and that the parameters which are used have a relationship to the physical aspects of the system. In trying to convince external users that the code is worthwhile -- that we identify new experiments and do analysis of these experiments, and these experiments are not ones that are used during the development -- that with each best estimate it is an obligation to try to characterize the uncertainties that are associated with such an estimate, and finally, that we solicit your continued input into this process as we go along.

DR. MARK: Some of these items cost money. Does the last one?

(Laughter.)

MR. KELBER: I think this year we'll find out whether it costs us money or makes money.

(Laughter.)

DR. CURTIS: We were hoping perhaps it would make us money.

(Slide.)

DR. CURTIS: You've already seen this, but this is a list of the major activities in the analysis branch.

PROF. KERR: In code development, how do you decide when you're finished, especially a large code like SSC or SIMMER or whatever.

DR. CURTIS: The code can be finished -- it needs finishing in two different ways. One is in the quality of the predictions that it makes. And secondly in terms of the convenience and cost to the user. As long as there are improvements, particularly in the latter, we will probably continue to do what we call maintain a code, which is to update it, to take advantage of machines and --

PROF. KERR: So the answer is that other things being equal you don't like to decide the codes are finished. A code should be a living thing.

DR. MARK: I would add a comment here. You've got a code which took X manyears to put together, and it was two years per man, and so it's X/2 men who are involved, or something of this sort. In order to keep it alive, that thing requires some, perhaps small fraction, but not a terribly small fraction, of a group of that size, maybe it's only half as many or a quarter as many, to simply keep the miserable thing viable. The cross-sections change, machines change, errors get perceived, or neat alternatives show up. And you've got a code that takes 10 hours to run. You've got to have at least one man, better two, living indefinitely, looking at that code, and seeing that it should be used right.

DR. CURTIS: It has been my experience also that new applications seem to suggest themselves, and that each successive user finds some modification to better fit his application.

DR. MARK: Even without modifications, the thing needs maintenance. Somebody has to know pretty much everything that's in there and not be asked to do something else, but to do that.

MR. KELBER: An example that comes to my mind is PDQ, the wellknown diffusion theory code, which has now come out in its eighth version, a PDQ-8. I guess the answer is, if it's economically justified to continue development, as evidentally the naval reactors division felt in the case of PDQ-8, people will do it. Not necessarily always with the same group, but that has been true.

DR. MARK: I was trying to speak to a different thing. I know that people will do it, and they'll improve things and work at it, as long as you like. What I'm saying is if you've got a code, you've got to have a maintenance man who has got to

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be not some nobody in order to keep the code on the top of the table; otherwise it becomes a disaster.

DR. CURTIS: If a code is not getting regular use, maybe it deserves to die.

DR. MARK: True.

DR. CURTIS: And a part of this regular use is having a very well qualified computer system expert as part of the user group to do just what you say.

DR. MARK: So expenses will continue indefinitely as long as the code is good?

DR. CURTIS: Very good.

(Slide.)

PROF. KERR: SIMMER is a little bit different, it seems to me. The purpose of building SIMMER is to demonstrate that something can't happen. If you demonstrate that, then you don't need it anymore.

DR. MARK: I agree if you come to the point where we don't need it anymore. On the other hand, SIMMER is also a means of discussing what might happen.

PROF. KERR: That's a point.

DR. CURTIS: Since we last talked to you, these are the principal achievements in the SIMMER program. I think we've made some significant progress. To verify the energetics part of the problem, we have run transition phase analyses and certain generic features have been defined.

cah 10

PROF. KERR: Successful means that it ran?

DR. CURTIS: Successful means that it ran and appears to be reasonable, at least as we perceive the problem. And, as a means to identify future code development requirements, we've been doing some accident analysis on a 1,000-megawatt electric sized plant to see if there are futures in some of the conceptual designs which are different than Clinch River, which would negate some of the understanding we think we have based on our Clinch River studies.

(Slide.)

DR. CURTIS: I thought we might take a look at the 1,000-megawatt electric study. Obviously this is our goal. We admit the possibility of significantly different behavior in the conceptual designs and in the plants that we have studied in great detail.

The relationship between detailed modeling and code development is pretty clearly established in our minds. They have to proceed in parallel, and we're going to be looking at both homogeneous and heterogeneous designs

DR. MARK: What's a homogeneous design?

DR. CURTIS: This is one in which you have the core surrounded by blanket. The heterogeneous design is one in which core elements and blanket elements are interspersed.

DR. MARK: This core still has rods. It's not fuel mixed in the coolant?

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(Laughter.)

DR. CURTIS: It is not. Homogeneous is only relative.

DR. MARK. All right.

DR. CURTIS: Unless there are some questions, I'll pass from the SIMMER program and take a look at some of the other things we've done.

PROF. KERR: Please do.

DR. CURTIS: Here are a few of the ideas on SSC.

(Slide.)

DR. CURTIS: This is the status of the SSC code. The loop version is complete. We are now using it. We have detailed plant models for Clinch River plant and FFTF. We have a visiting staff member from the GRS visiting at Brookhaven.

PROF. KERR: What is the GRS?

DR. CURTIS: Gesellschaft für Reaktor Sicherheit. It's the technical support --

DR. MARK: I'm glad you asked that.

(Laughter.)

DR. CURTIS: It's the technical support for the German licensing authority. It's located in Cologne. He's modeling SNR3 and the German licensing authorities plan to use SSC in their review of the interatom application. The SSC-L has been exported to the groups that are indicated there. I was talking yesterday to the representative from B and W, and they have been using SSC on a regular basis in support of the DOE

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conceptual design study that they're doing for DOE.

A pot version under development and we're looking at the term of longterm heat removal and what modifications need to be made to treat that problem properly.

end tape #7

CR 5697
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mns 1

PROFESSOR KERR: "L" means loop, and "P" means pot, and "C" means -- I thought I saw an SSS-C in the very last one. I'm sorry, SSC-S.

And what does the "S" mean?

DR. CURTIS: Shutdown.

(Slide.)

Some results that we are projecting that we are going to be depending heavily -- well, let me give you the results before I tell you the other one.

We see that the Coal's River design, at least if we can believe our results, provides natural convective flow for core coolability.

One of the particular parametric studies that we did was to investigate the importance of the timing of pony motor failure. The thought was if the pony motor had come into operation at shutdown, and it operated until the system became on its isothermal, that it might be more dangerous to lose the pony motor at that time than it would be when significant temperature gradients exists right after shutdown. And it does not appear to be a problem.

Now, I'm a little puzzled by your item 1, in the bottom paragraph. How can you think of anything running at 100 percent power and still be just about at 80 degrees ambient?

DR. CURTIS: It's the air temperature. The FFTF

ros 2

rejects its heat through air-blast heat exchangers to the atmosphere.

DR. MARK: Okay. It's not that all that's sodium?

DR. CURTIS: No, that pottery, at that temperature.

(Laughter.)

DR. CURTIS: At least if we have what we believe are the performance data for the testing of that system, it looks to us like they're going to have problems on a hot day.

PROFESSOR KERR: Nobody else had calculated this before?

DR. CURTIS: I suspect there are others who are aware of it. But as part of our preparation for pre-calculating the natural circulation tests for FFTF which are coming up this winter, we have modeled the system and are running it. This just happens to be one of our conclusions.

(Slide.)

Some of our plans -- we are trying to build a matrix of operational transients and calculate them. We concur that we have not looked at a good many interesting upset conditions, and we need to do that.

As I said, the startup tests on FFTF will be pre-calculated, and we are looking at the multi-dimension effects on natural circulation.

PROFESSOR KERR: What is the purpose of the first one? Is that to simply see if SSC can do it, or to get some information about CRBR?

DR. CURTIS: The only plant models that we have in sufficient detail to excercise SSC are Clinch River and FFTF. They have been modeled.

The answer to your question is: the primary purpose is to excercise the code.

PROFESSOR KERR: What is meant by "excercising the code"? To see what it predicts?

DR. CURTIS: Partly, to study the system performance and to identify potential problem areas. Secondly, to insure that the many paths in this transient performance are excercised, if we are going to do calculations which make different requirements on the codes so that it follows different paths.

PROFESSOR KERR: Do you have anything to compare the results with by the methods of calculation?

DR. CURTIS: The best we can do here is there is a simpler model, which was called DEMO, which was developed by the applicant, and which was used in preparing the SAR that was submitted. And what we intend to do is to prepare our more detailed calculations with the cases that were presented in the SAR.

DR. MARK: You could learn something, Bill, with respect running a code even when you don't know the physics of the answer at all, just by observing whether the miserable arithmetic is stable. But it isn't -- you know, you have some problems.

DR. CURTIS: Perhaps that's enough of SSC; unless there are some questions?

There was some discussion of our accident deliniation study.

(Slide.)

The event tree work. I thought I would briefly give you the status of the work.

An interim report was issued for review in September of '78. There was an extensive review group meeting, and we have comments that are being considered on that now.

The accident deliniation trees are to be issued in October of '79, and we want a status report from the people who are working on it, on the problems that they are reading in attempting to find a basis for quantification of some of these. We really don't expect to get any quantification, but we would like to know how it stands.

PROFESSOR KERR: Remind me who's doing that work.

DR. CURTIS: Sandia.

Another Sandia project --

(Slide.)

-- is the containment code. Here's why we're doing it. It is a fairly low-level effort only because the CONTAIN 189 project is doing the systems integration aspect

ros 5

of building this code. The detailed model-building and correlations are coming out of the rather more extensive Sandia experimental program.

PROFESSOR KERR: I thought I had a budget number somewhere that contained that.

Okay, this is part of the analytical package?

DR. CURTIS: Yes.

PROFESSOR KERR: The 7.8?

DR. CURTIS: It's one of the line items in this report.

PROFESSOR KERR: Okay. Thank you.

DR. CURTIS: It's in the neighborhood of about four and a half man-years.

The other codes that we're working on include the work of Bill Shaw at Argonne; and his work that we've briefly discussed -- the component mixing code FLOWMIX , and the small experimental model, which is BODYFIT. And it's a very interesting application, and we are quite excited about it.

Harry Hummel's group is conducting our comparative studies; and we have heard quite a bit about that already. And they are also working of improvements to models which are to be integrated into the new SASS code, which RAS and Argonne will soon be coming out with.

I think that I've pretty well covered our major activities.

ros 6

PROFESSOR KERR: Okay.

DR. CURTIS: Unless there are specific questions?

(No response.)

PROFESSOR KERR: Thank you.

Who is next?

MR. SILBERBERG: RSR. Mr. Chairman, are we working towards the 11:30?

PROFESSOR KERR: As Bureaucratese puts it, that's a goal.

MR. SILBERBERG: Okay.

PROFESSOR KERR: I'm willing to run over some, but not indefinitely.

MR. SILBERBERG: I understand.

Let me help you achieve that goal by trying to focus my presentation on som of the experimental highlights in our program, let's say, through '79, certainly in Fiscal '79; and some of the key facilities that have come on since we last met with you; and use that as a point of departure.

To do that I think it would be simpler to refer you to page 2 of the updated program status that you all have. And you don't have to look at it now; but I am, in effect, going to highlight from that. And I think that does a fairly good job of focussing in these areas.

For example, during Fiscal '79 the performance upgrading of any ACRR at Sandia was completed; and now the test program has just been initiated in the upgraded reactor. And the highlights there are that the pulse fluents and the steady state flux capability of the reactor has been increased by more than a factor of 3. And, along with that, we have installed the coated aperture imaging system, which has been developed over the past several years.

(Slide.)

And having been installed in the system here -- I think the key point we want to make, which really addresses what Dr. Kelber said, was that any combination of the upgraded performance of the reactor, as well as the availability of the coated aperature imaging system, together I think gives the machine a capability that should be expedited, and can be used a lot faster.

PROFESSOR KERR: What answers do you expect to get from this machine?

MR. SILBERBERG: The question in the area of initial and extended fuel motion -- that is, under proper conditions, the failure threshold, failure location, and the movement of the fuel out into the channel beyond after failure, in terms of what its dynamics are -- its fuel dynamics are -- within the channel, and certain other conditions -- fuel disruption, such as the higher reactivity ramp rate regime that Dr. Kelber referred to, in terms of rapid inherent shutdown mechanisms, if indeed they exist -- this type of information put in the

ros 8

direction of seven pins, or more.

Now, to get to the full scope of what I just mentioned, that, of course, assumes a flowing sodium loop. In the meantime, we'll continue the capsule experiments of the type that we have done going from single tin oxides to irradiated oxide shield, prompt burst energetics, and then on to seven pin stagnate capsule; to look at the effects of seven pins in terms of all heat losses.

A VOICE: I think the uniqueness of this program is instead of waiting for the capsules to come out and then interpret what happened, they are looking for the capability of seeing in place at the time of failure where the movements are occurring.

PROFESSOR KERR: Do you look at this as sort of a general exploratory expedition of fuel behavior under accident conditions, or are there specific questions for whose answers you look?

MR. SILBERBERG: Yes. It's both. It's certainly phenomenological, in terms of the supporting development of phenomenological models. But there are specific questions like --

PROFESSOR KERR: I don't understand what you mean by "phenomenological."

MR. SILBERBERG: In other words, there are various models that people are depicting if you will, that describe

ros 9

how fuel might move in the channel and how fuel fails.

So, one would try to observe experimentally whether some of the notions in some of these processes that have been described and stated are, indeed, that way.

I think the other point is that there are some specific questions like, let's say, the question of fuel coolant interaction under the dynamic conditions of short periods.

PROF. KERR: Do you feel that you know which regimes are critical in exploring fuel coolant interaction? Are there specific areas of temperature, radiation flow, whatever, is crucial?

MR. SILBERBERG: Some of these have been identified. For example, the question of what is the contribution. In other words, is it only fuel vapor pressure that gives you the pressure flow, in terms of work potential in the system?

And how does one separate the separate effects experiments?

MR. KELBER: Could I get directly to the heart of the matter?

A breif answer to your question is no, we thought we did. We've changed our minds as a result of the PBE tests. Before we went into the PBE tests it was our judgment that we would observe no fuel coolant interaction. The first few PBE tests seems to verify that judgment.

Then, as we looked at the fine scale -- it's the PBE

ros 10

tests -- the fine time scale the PBE tests are apparently capable of. We discerned that indeed, there were fuel coolant interactions occurring later on it time, that never have been seen in the TREAT tests.

I think the answer is, the issue is open as to where fuel coolant interactions will be observed and what their efficiency is. We are learning a great deal more about them and we are developing a better way to model things phenomenologically.

PROFESSOR KERR: Do you think you know the right questions to ask, in order to get answers?

You have to design the experiments on some basis. How do you know what to look for?

MR. KELBER: One of the PBE tests -- we were going to separate out the sodium contribution by using tin instead of sodium, for example, to help us do this.

The answer to your question, Bill, no. We don't know all the right answers yet. Nor all the right questions yet. We're still groping.

PROFESSOR KERR: How do you design your experiments under those circumstances, if you don't know what questions to ask?

MR. KELBER: One of the things that disappointed me is the absence of a good analytic theory in this field. Theofanous is coming close, and if some of the work that Theo

ros 11

has done at Purdue -- we are trying to develop some experiments at Sandia.

PRCFESSOR KERR: Should you be doing experiments if you don't know what questions to ask?

MR. KELBER: I have profound difficulties with this. Bill Camp, who is the theorist at Sandia --

PROFESSOR KERR: Does that mean yes or no?

MR. KELBER: The answer is we are going very slow, until we know better what we are doing. Where we know that we have a unique effect that we want to test, we'll do an experiment. But I am very unhappy with our present state of knowledge, and we hope to develop a better approach in this coming year.

PROFESSOR KERR: Thank you.

MR. KELBER: I might say that I was not nearly so unhappy before we started the PBE tests. They have upset our view as to how things go.

MR. SILBERBERG: Let me just add to what Dr. Kelber has added, that there has been this preponderance of uncertainty in the international technical community on FCI. And, over the past several years, there has been some of this refocussing that Dr. Kelber has mentioned, in the work that's been going on not only in this country but in other countries as to what are the key questions, in terms of fragmentation breakup complication.

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PROFESSOR KERR: The PBE results are reproducable?

MR. KELBER: Yes, sir. You can take 13-S and 12-S. 12-S was the last test run on the old reactor. 13-S was run on the new reactor. And put the results right on top of one another.

PROFESSOR KERR: So they have been reproduced once?

MR. SILBERBERG: Once, yes.

MR. KELBER: Unfortunately, they contradict 5-S, which was the same test, but the fuel was different. So there are a host of questions here.

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PROFESSOR KERR: So it is two out of three.

MR. KELBER: But there were differences between 12 and 13 on the one hand, and 5 on the other. But they shouldn't.have mattered.

end #8

The differences should not have mattered, and they do. We don't understand that. We do not have an adequate theory to guide the experiments. We are going to have to develop it. Until we develop it we are going very slow.

DR. MARK: Two questions. PBE is what I used to know as PBF?

MR. SILBERBERG: No, it's not. It's the prompt burst energetics, and it's one of a series of tests that are run.

DR. MARK: PBF?

MR. SILBERBERG: Is the prompt burst --

DR. MARK: An entirely different thing.

MR. SILBERBERG: Different thing, yes. In the machine, in the experiment, in a capsule.

DR. MARK: The other question is, they used to sound like marvelous experiments, but it would surprise the heck out of you if you don't know how to explain them. They are obviously very good.

MR. SILBERBERG: I understand what you are saying.

Let me go to another item, in terms of where research information has been releases in what we call -- and let me refer to two items there, which have been released in this fiscal year. One on the results of the first three tests, on in-pile tests, in the old ACPR on core debris retention.

If you recall, this is the capsule experiment.

(Slide.)

Fuel debris with sodium, cooled by an external helium coolant loop. We are now resuming the series into the next test in the test matrix. And we are also under detailed discussions, at this point, technical and otherwise, with the European Economic Community Laboratory Joint Research Center, on how they may wish to participate in the program in a collaborative effort to augment the test matrix; and participate financially, as well as with people, which will allow us to move this program along much faster and get us into areas -- some of the more difficult questions that one has to address on core debris behavior. In other words, coolability, and the various conditions that surround coolability of debris following post-accident.

Now, let me also refer to one of the items; namely, a generic program and scoping experiments that we initiated in the core retention -- core melt retention area.

As you know, we have described for the committee in the past, our core melt technology program which was focused on concrete, interactions of core-melt with concrete. In '79 we initiated the program to look at alternate materials to concrete, such as might be used in retention system concepts. We have been looking at materials like magnisium-oxide, of crucible type sacrificial materials of a concept such as borax.

This work is just getting started. This work is now of much interest in the Floating nuclear plant evaluation. And our expertise here, in terms of the state of the art, is now being factored into the NRR review of the Floating nuclear plant.

We are, in fact, looking at ways of augmenting the program in response to a user request from NRR for support that goes first on Floating Nuclear Plant, and second on a more generic matter. I believe Dr. Kelber mentioned this.

A key aspect of that program, which is the core melt technology program, which includes not only the sodium-concrete interaction and the fuel-concrete interaction program --

(Slide.)

-- is part of the contained qualification.

This facility -- in '79, the facility was started under construction, in earnest, and with somewhere like 50 percent through it, the construction will be completed in '79. And initial ckeck-out and the first test will be conducted in '80.

DR. MARK: Does the red indicate what isn't done yet?

MR. SILBERBEG: No. The red happens to be the furnace area, the furnace proper, the induction furnace -- cabability of up to 500 kilograms of UO_2. And the low portion

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is the -- where the experiment itself is contained in the experiment chamber. And with the ability to separate and remove the experiment chamber out along on rails, and reload it, and put another experiment underneath.

But this is the type of facility and the type of technology and program that's very demanding on the program in terms of funding, in terms of its budgetary constraints.

PROFESSOR KERR: That's at Sandia?

MR. SILBERBERG: That's at Sandia, yes. I believe you have seen the area where this work is going on. We have a complex which also includes the sodium-concrete interaction work, which is also noted as a highlight in the information paper that we sent to you.

The highlight on the sodium-concrete interactions work is that in a rather short period of time, the program was briefly redirected to help NRR in their review of the FFTF SER.

(Slide.)

And their preparation of the SER, and in the information from the series of special tests that was run for them was quite benificial to the NRR staff. In it it confirmed the position that they took in another SER, relative to containment margins; and the types of assumptions that they were making in their analyses were not inconsistent with the kinds of results that we were getting.

As Dr. Kelber noted before, this is an area where we are not -- we understand some of the mechanisms, but many of them are not clear. In support of this, at the same facility we have a separate effects intermediate scale work going on looking specifically at thermal effects, chemical effects where you can turn parameters around rather quickly, a lot more quickly here. And a lot cheaper, well, in the large scale tests like this, which involves 200 kilograms of sodium in crucibles of the size shown here.

Another area that I'd like to point out is the FAST facility at Oakridge, which stands for Fuel Aerosol Simulance Test, which, if I could recall for you, is this facility --

(Slide.)

-- which has been under design for several years of development. Previously the work involves the condenser discharge vaporization of UO_2 samples ultimately under sodium.

And, up 'til now, the program has moved in the direction in a different facility before this, just looking at vaporization in argon. Understanding that, and also the development of the CDVD -- Condenser Discharge Vaporization Device.

The facility has now been constructed and tests are now being performed in a water pool. And what we are doing is we are calibrating the apparatus. We are looking at the instrumentation, comparing instruments with some of the preliminary models that we have on ACDA bubble phenomena right up to the source term. That is, how much aerosol might one expect from an ACDA after having come through a pool of sodium, with or without a structure being there.

We will carefully look at the calibrations and the data and how the facility operates before, and set criteria before we go on to sodium. We will very carefully want to understand what we have done with water; and to see that everything is working as best as we can, before moving on to sodium.

And, in the case of water, of course, one is using, or has the advantage of looking in at bubble sizes that thing like that, which is a little more difficult than sodium.

DR. MARK: One point. Maybe I don't remember this correctly.

One is interested, of course, in the number of particles in aerosol.

MR. SILBERBERG: Certainly a concentration on number, sure -- and size.

DR. MARK: And their size, how much any measurements are made. One is also necessarily interested in the mass that is carried in those, and at one time there was no clear relationship between the size and the mass, because the shapes were little hollow bubbles or solid chunks. It had to be

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assumed. Is that getting cleared up?

MR. SILBERBERG: Okay. I think I know what you are referring to.

In this area we are talking about initial sizes. There you are pretty much down to spherical sizes. If you look under the microscope, the primary particles are spherical and they have not grown, they have not had a time to age.

What you are referring to, Dr. Mark, is when the aerosol particles have aged, and their shape departs seriously from spherical. In those cases --

DR. MARK: They look like genes, or something.

MR. SILBERBERG: In those cases, what Dr. Giese King at Battelle-Columbus has been doing is separate effects measurements on the aerodynamic properties of aerosol particles to characterize what shape corrections and what density correction you put into to get at the correlation of mass and shape.

That work is proceeding along, and we expect to complete that work in Fiscal '80.

DR. MARK: This, I am sure, is of some importance, because you have to correlate all of this with radioactivity carried around. And the mass isn't there. And then the agglomeration, and the effect on the shape and size --

MR. SILBERBERG: That's right.

I think, in the interest of time, that gives you a

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capsule version of what I would like to tell you.

PROFESSOR KERR: Thank you, sir. Thank you for capsulizing also.

Who is next?

MR. FOULDS: I'm Ron Foulds, and I'm Assistant Chief of the Gas Reactor Safety Research Branch. You may recall that Bob Schamberger has been handling that in the past. He is on a special task force, now associated with Three Mile Island.

I have very little to say, maybe mostly because the budget -- I guess Reactor Safety Research is zero in 1980, Fiscal '80. And a lot of the effort that we've been carrying on in this program has been directed, most lately, to how do we terminate the program orderly and with meaningful results.

(Slide.)

For example, this is what we're doing now in Los Alomos Scientific Laboratory. During the first half of '79 we concentrated on what you see there. In the second half of '79 cut it way back to only those items that you see listed, so that we could stretch some of the work into Fiscal '80, in the event that there might somehow be some funds added back into the program for '80 by Congress.

As it appears now, as you know, there is funding identified. Of course, we won't know whether we will be doing any work in '80, until later on this year. But we have,

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by this route, been able to stretch out the program a bit, in spite of some of the cancellations of work being done in '79, and maintain that cadre of dedicated people. And we are quite fortunate to have people who feel that the Gas Reactor Business is really a sleeping giant, I guess, for the nation; and are dedicated to it, and they want to stay with it.

Luckily we haven't lost everyone in the laboratories. We have some very good, key people.

And so, that's whats happened in LASL. And in Brookhaven --

(Slide.)

we have been working on this kind of a program at the beginning of the year. This is the first half of '79, and we've cut that drastically.

DR. MARK: You didn't have any numbers on those tables. You've cut back from what to what, and you are carrying on with what? Roughly.

MR. FOULDS: All right.

We've cut back, okay, from roughly $2 million to say, about a third of that, so that we can stretch another quarter into say, the following year.

DR. MARK: So you are able to carry on with something in the neighborhood of half a million, which indeed amounts to possibly supporting five people?

MR. FOULDS: That's right. And, of course, what

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happens if once you terminate you have to use some of those dollars to terminate the program.

MR. KEBLER: Part of this stretch-out was dictated by balancing the question of termination costs, as opposed to allowing the laboratory time to place them elsewhere. Part of it was dictated by the utility of the equiptment, when some of the experiments are very long-term.

And we could, by running a few more months, we could have an identifiable block of data.

(Slide.)

MR. FOULDS: Again, the other two laboratories that are principally involved, which are a much smaller part of the program, are Oak Ridge and Battelle-Northwest.

Again, we have cut down, but here you notice in what is written on the viewgraphs here, that a lot of the work that's being done and that what we have been continuing is assciated with Fort St. Vrain. Principally because that is an operating reactor, and we have a high degree of interest on the part of the NRR in doing this work; although they haven't been able to give us the coveted user need requests that we'd like to have.

This is an advanced reactor area, and they don't have that much focus on advanced reactors I am afraid, at the top of the organizational structure.

(Slide.)

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This is what we would do to phase back up again at the four laboratories.

I'm sorry, the fourth one is off on the right. This expands slightly on what we had on another visual.

What you see at the top is the primary work that would be going on in those laboratories, carried over from the beginning of Fiscal '80; and then assuming we do get the funds that Congress has now identified, which would have to come somewhere out of the budget. They haven't increased the budget, you know. They just said, "Take this somewhere from your reactor safety research budget."

Then we would do these jobs that are indicated there in Fiscal '80. And in Fiscal '81 we have proposed the additional work that's shown below, and that would be with the $3.9 million which has been identified.

PROFESSOR KERR: These questions, these programs are designed to answer questions raised by whom? By RES? By the licensing? By NRR? Py whom?

MR. FOULDS: Let me just say yes to all of those, okay.

PROFESSOR KERR: I prefer you not say yes to all of those.

MR. FOULDS: What I would like to say is that this covers the program which was defined. Well, we had two five-year plans, all right? It's a continuation of the first

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five-year plan, which was further developed into a second five-year plan that really wasn't ever bought by the Commission, because of the termination of the program. But we have a fairly well thought out program that follows work that was developed in connection with NRR.

PROFESSOR KERR: The questions were raised internally?

MR. FOULDS: Absolutely.

For example, we have --

(Slide.)

-- a program that looks like this, that was explained to the ACRS last year; where we went through some detailed explanation of the programs, all of the programs in advanced reactor safety research. This is the kind of thing we would do per year, say in the materials interaction area. Graphite and primary system containment metals, and so on.

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This is just one of four principal areas.

end #9

start #10

So, the plan on what to do has been laid out and endorsed before.

MR. KELBER: The major source of questions is twofold. Of the greatest urgency are questions related specifically at Fort St. Vrain. These are primarily in-service inspection of the support graphite. That's probably peculiar to Fort St. Vrain, and may not, in fact, be general.

We have had a great deal of cooperation on that from General Atomics, I might say.

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Secondly, there have been questions raised, and these go back quite some time to a more sizable response. The remainder of the work has grown out of the reviews which were carried very close to the CP stage on the large commercial sized HTGR plants for Delmarva.

I think that was primarily the summit and the fulcrum plants. And those reviews did, as you may recall, roll fairly far along. So we have a body of licensing concerns, that I hear they do not include specifically the direct-cycle plants. They include all the issues we think are generic to teh HTGR concept as it has been developed here.

PROFESSOR KERR: Thank you.

MR. FOULDS: I have been advised by people in NRR that they are developing user need requests, that we would like to see for background for the gas program. But it hasn't gotten here yet.

MR. KELBER: We've been promised that for two ears.

MR. FOULDS: Are there any specific questions?

(No response.)

PROFESSOR KERR: Thank you.

MR. KELBER: That concludes out part of the morning. If there are any leftover questions, I'd be happy to respond now, or I think there is a tentative date on the calendar for next month.

PROFESSOR KERR: There is a tentative date for me

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to decide fairly surely that we want to hold another meeting. I think we will let you know, certainly shortly after the meeting of the full committee, whether we do need an additional meeting.

MR. KELBER: When is the meeting of the full committee?

PROFESSOR KERR: Tomorrow.

MR. KELBER: Is that tomorrow?

PROFESSOR KERR: Yes.

MR. KELBER: So we could expect, perhaps, by the end of this week?

PROFESSOR KERR: Yes.

MR. KELBER: I appreciate that very much.

PROFESSOR KERR: Thank you again.

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The meeting is adjourned.

(Whereupon, at 11:55 a.m., the meeting was adjourned.)

end #10

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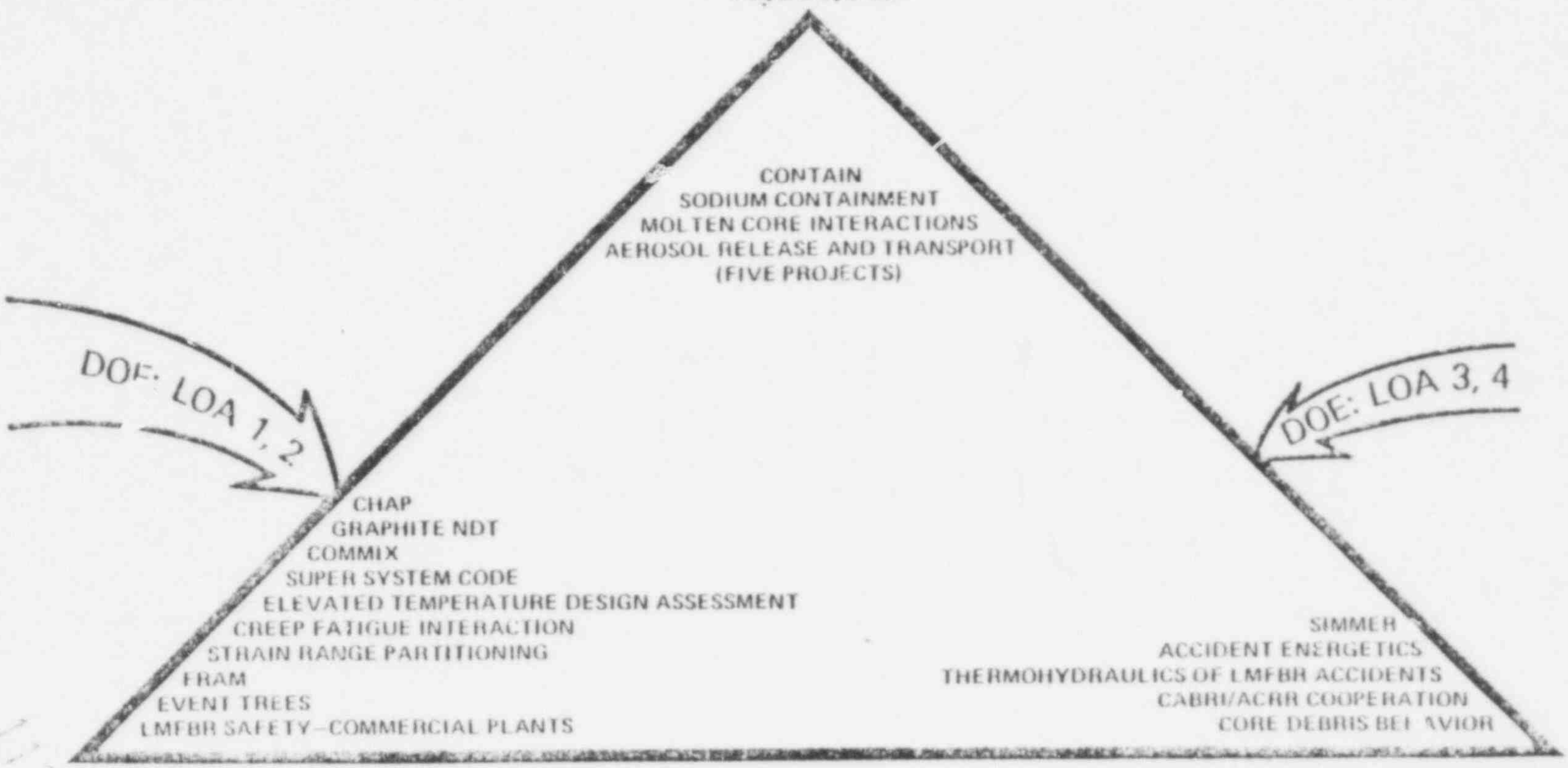
PROGRAM BALANCE
ADVANCED REACTOR SAFETY RESEARCH

ACCIDENT CONTAINMENT, MITIGATION

FY 79:
$3,900,000



ACCIDENT INITIATION, PREVENTION
FY 79:
$2,900,000

CORE MELT ACCIDENTS
FY 79:
$5,700,000

NRR COMMENTS ON ARSR FY. 80 PROGRAM*

- PROGRAM ENDORSED AND ENCOURAGED
- CONTINUATION IMPORTANT IN RESOLVING KEY SAFETY ISSUES
- CONTINUATION CONSISTANT WITH ADMINISTRATION'S DESIRE
- IDENTIFIED 4 USER REQUESTS BEING ADDRESSED
- LACK OF THIS PROGRAM WOULD BE MAJOR SETBACK FOR EVENTUAL LICENSING OF ADVANCED REACTORS.

*LETTER, R. S. BOYD TO R. J. MATTSON, "COMMENTS ON FY 1980 BUDGET AMENDMENT, 6/7/79.

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ACRS RECOMMENDATIONS - ARSR PROGRAM (NUREG-0496)

Recommendation	Comment
• Comprehensive study of safety issues for commercial LMFBR--use probabilistic analysis techniques	• Event Tree program develops cadre for study of DOE's - CDS effort (1981). - cooperative studies with UK. - Joint fuel tests with FRG.
• Initiate scoping studies on GCRs similar to LMFBRs (Direct cycle HTGR & GCFR).	• Funding not available.
• Initiate studies which place emphasis on CDA prevention.	• UK-USNRC Accident Initiation studies. • Added work to follow event tree study. • Information meetings with DOE & Contractors (Next meeting on LOA-1, 7/11 & 12).

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ACRS RECOMMENDATIONS - ARSR PROGRAM (NUREG-0496)

Recommendation	Comment
• Study pro and con of alternate containment designs (e.g., filtered & vented designs).	• Use CONTAIN code, structural integrity tests and ART program to define containment threat.
• Determine whether new experimental facilities or programs are needed to demonstrate validity of natural convection cooling for commercial-sized LMFBRs (pool and loop types).	• Reactivate STF studies and use SSC, COMMIX.
• Evaluate on continuing basis need for new large-scale experimental apparatus.	• Reactivate STF studies.

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ACRS RECOMMENDATIONS - ARSR PROGRAM (NUREG-0496)

Recommendation	Comment
• Continued study of the CDA and the resolution of problems associated with it, e.g., post-accident heat removal.	• Approximately 40% of the budget devoted to support work in this area. The SIMMER code and ACRR tests make major contributions.
• Greater emphasis be placed on developing a planned, methodical program to keep abreast and profit from safety research performed in other nations.	• Specific collaborations in progress under existing exchange agreements with FRG, Japan, UK & CEA-KfK (CABRI); Broad agreement with CEA (late 1979) will permit full exchange with Int'l leaders in FBR Technology.

584 116

LMFBR PROGRAM
(IN MILLIONS)

ACTIVITY	FY 80	FY 81	Δ	COMMENT
ANALYSIS	5.4	7.8	2.4	• COMPLETE & RELEASE ACCIDENT CODES
SAFETY TEST FACILITY STUDIES	0	0.7	0.7	• REACTIVATE ARSR PROGRAM
MATERIALS INTERACTION	2.8	4.6	1.8	• DESIGN/FABRICATE ACRR SODIUM LOOP
AEROSOL RELEASE & TRANSPORT	2.2	3.0	0.8	• CORE MELT AEROSOL SOURCE AND TRANSPORT
SYSTEM INTEGRITY	3.3	6.0	2.7	• CONTAIN QUALIFICATION • LARGE CORE MELT RETENTION TESTS
TOTAL	13.7	22.1	8.4	

584 117

6

FY 81 LMFBR PROGRAM

ANALYSIS $ 7.8 M

- ISSUE CONTAIN-II, BIFLO AND SSC-S CODES
- COMPLETE 2-PHASE COMMIX-2 AND BODYFIT CODES
- COMPLETE PHASE-2 OF ACCIDENT DELINEATION STUDY
- CONTINUE CODE QUALIFICATION PROGRAMS

SAFETY TEST FACILITY STUDIES $ 0.7 M

- REACTIVATE NRC PROGRAM

594 118

FY 81 LMFBR PROGRAM

MATERIALS INTERACTION $ 4.6 M

- ACRR LOOP DESIGN / FABRICATION
- ACRR 7-PIN ACCIDENT ENERGETICS CAPSULE TESTS
- ACRR FUEL DISPERSAL TESTS -- IRRADIATED FUEL
- ACRR TRANSITION PHASE TESTS

584 119

FY 81 LMFBR PROGRAM

AEROSOL RELEASE & TRANSPORT

- CORE MELT AEROSOL SOURCE TERM
- NSPP CORE MELT AEROSOL TRANSPORT
- FAST NA TESTS -- HCDA SOURCE
- HAARM-3 EXTENSION TO CORE MELT

584 120

FY 81 LMFBR PROGRAM

SYSTEM INTEGRITY $ 6.0 M

- CONTAIN QUALIFICATION
- LARGE CORE MELT RETENTION TESTS
- ACRR CORE DEBRIS COOLABILITY TESTS
- TESTS ON CELL LINER RESPONSE TO ACCIDENT LOADS

584 121

ADVANCED CONVERTERS

ACTIVITY	FY 80 (PRES.)	FY 81 (REQ.)	COMMENT
GCR	0*	3.9	CONTINUE MIN. MAINTENANCE PROGRAM

*EXPECT $3.7M TO BE MANDATED BY CONGRESS

584 122

11

FY 81 ADVANCED CONVERTERS PROGRAM

GCR $3.9 M

- COMPLETE CORE SUPPORT BLOCK (PGX) TESTS
- QUALIFY FSV TRANSIENT ANALYSIS CODES
- COMPLETE FSV CONVECTIVE PLUME HEAT TRANSFER TESTS

584 123

12

HTGR SAFETY ISSUES

81 ↓	80 ↓	79 ↓	
•	x	+	● Graphites as Structural Materials
•	x	+	● Core Seismic Response
•			● Fuel Transient Response
•	x		● In-Service Inspection
			● Low Probability Accidents
•	x		● Containment Requirements
•	x	+	● Primary System Integrity
•			● Emergency Core Cooling Provisions

584 123