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In the Matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON ADVANCED REACTORS

Rodeway Inn
Room 215
Chicago, Illinois

Tuesday, July 14, 1981

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

BEFORE:

M. CARBON, Subcommittee Chairman

J. MARK

DESIGNATED FEDERAL EMPLOYEE:

E. IGNE

ACRS CONSULTANTS:

R. AVERY

G. GOLDEN

J. HARTUNG

W. LIPINSKI

S. SIEGEL

MR. CARBON: The meeting will come to order. This is an open meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Advanced Reactors.

My name is Carbon, the Subcommittee chairman. The other ACRS member today is Mr. Mark, and Mr. Bender will be joining us tomorrow.

We also have present ACRS consultants Messrs. Avery, Golden, Hartung, Lipinski and Siegel.

Len Koch, for your information, called a little while ago and said that his wife has a herniated disc and is in excruciating pain, and he was on the way to the hospital with her.

So I don't know whether he'll be here tomorrow or not.

The purpose of this meeting is to discuss matters relating to the LMFBR safety design criteria.

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

Mr. E. Igne is a designated federal employee for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on June 30, 1981.

A transcript of the meeting is being kept; and since we have no PA system, it's requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have not received either written statements or requests for time to make oral statements from any members of the public.

The agenda first calls for an opening statement.

And I'd like to make a comment or two myself and invite other comments.

With regard to the agenda, it indicates the day ending at 4:30 p.m.

But unless this works a hardship on someone because you weren't aware of it, I would propose that we continue till 5:30.

If the DOE people are available and have presentations, we'll continue with them.

If not, we can hold executive discussions and then knock off around 5:30.

For tomorrow, the agenda as originally printed indicated quitting at 4 o'clock. I don't know why.

It's the intention there, also, that we'll probably continue our discussions till about 5:30.

I guess I have no other comments at the moment.

Parson, do you have anything to --

MR. MARK: I think not,unless agenda -- at this moment.

I expect that through the day we'll be talking about some of the things which were in this set of thoughts and opinions put together.

And maybe I would have some comments and points there.

But I don't have anything to say about the planning or layout of the meeting.

MR. CARBON: With regard to that point, each of you, I hope, has received a copy of what everyone put together on those -- our individual statements.

And then also, Mike Bender sent me a letter, and I think each of you have a copy of it.

And that general material will be a starting point for our discussions either late this afternoon or tomorrow.

Bob, any comments?

MR. AVERY: No.

MR. CARBON: Jerry?

MR. GOLDEN: No.

MR. CARBON: Jim?

MR. HARTUNG: No.

MR. CARBON: Sid?

MR. SIEGEL: I have a question of sorts, mostly addressed, I suppose, to DOE people.

In the Kemeny report on Three Mile Island, they very pointedly addressed most of their concerns to non-technical issues.

They hardly dwelt on technical aspects of the plant which they felt were at fault.

And my thoughts -- so my question or observation to DOE is, have they given any consideration to the non-technical studies in the area of fast reactor safety which might have to be done in addition to or as a new tack in the whole philosophy of reactor safety which has not been done before?

We're also familiar with the technical studies, investigations, designs, considerations that go into the matter.

And yet, the Kemeny report doesn't dwell on these but emphasizes other areas.

Who will look at these other areas? I sort of ask it to begin with because maybe the DOE people have thought about it and will have some comments to make as we proceed.

MR. CARBON: I'll be turning the meeting over to Frank in just a moment here, and perhaps he can address that question.

Any other things anyone would like to bring up now?

(No response.)

MR. CARBON: Anything we need to discuss?

MR. IGNE: No.

MR. CARBON: We'll now proceed with the meeting.

And I call on Mr. Frank Gavigan of DOE to begin discussion. Good morning, Frank.

MR. GAVIGAN: Good morning. Sid, let me just answer your question to begin with.

The Kemeny Commission Report triggered the beginning of a review inside DOE of all our operating reactors.

And one of the major purposes of the review was the very thing that you mentioned.

If you're talking particularly about organizational strength and operator capabilities, a review was conducted all through DOE in Savannah River, in Idaho, in Oregon East, Oregon West, San Francisco, Albuquerque, all the operating reactors.

A report was prepared as a result of this.

It took maybe eight months, with a multiple number of reviews.

The report was prepared that was issued recently to Congress describing the results of that review.

And it's caused quite a stir inside DOE because it tended to be somewhat negative about our own operations.

There are a lot of compensatory activities going on inside DOE.

And the questions are generally those considering the seven or eight areas inside the Kemeny report, including those of operator qualifications and organizational strength.

So an internal review has occurred. It's not part of our R and D program, however.

MR. CARBON: Frank, can I follow up Sid's question, perhaps to help him a bit.

He may not be aware, but we've had some presentations to ACRS in which it's my understanding that the recommendations of your review committee aren't really being supported in the whole, at least, and perhaps in considerable chunks aren't being supported by some management in DOE.

I don't know whether this is right or wrong.

I don't wish to take any sides.

The question I would get at, though, is, has that review led to any changes in your philosophy; for example, your approach to the safety on LMFBR?

MR. GAVIGAN: Normally, it has not to date in the R and D program.

Our program, and Don might want to spend a little time on it later, emphasizes the idea of man-machine interface and developing design criteria for operating systems that have to do with safety events that might occur, as well as operator interactions and the extent to which operators can interfere with the operation of safety systems.

But that's not new for us. It's just that we wrote it down before the TMI accident occurred.

So it has -- the major change in our program as a result of TMI has been the heavier emphasis on man-machine interface activities.

We've always had a fairly accurate risk program, as you know.

And the shutdown heat removal always has been something that we've been very interested in in the R and D program.

And we've strengthened it somewhat as a result of TMI.

But we haven't initiated any new work because of it.

We've always had a strong program there.

MR. LIPINSKI: Let me ask a question. NRC issued two documents.

One is NUREG 696, and the other one was 727, which effectively are being applied to all the plants that are seeking their operating licenses right now.

Have you looked at those documents? Some of them are very specific in terms of the technical fixes on light water reactors.

But supposedly, those documents summarized the Kemeny Commission's recommendations as well as NRC's own findings.

MR. GAVIGAN: They've been looked at inside projects rather than inside the R and D program that I know of, though Don may have done it.

And both CBR and the large developmental plant have looked at those specifically to assure that whatever we think needs to be done for breeders is being done.

This is a good time to do it in the new project.

So I think from project viewpoint, yes, they've been looked at.

This -- the presentation we've put together has been aimed at trying to give you an understanding of how we pick our R and D work, why it is that we do it.

As you recollect, at our last meeting in April, we spent maybe a morning on this.

I think at the last time I spent an hour, I think, and Don spent two hours or so giving you a general introduction.

And that's all in your meeting minutes in the hand-outs we covered last time.

Today's meeting is aimed at a lower-level description of why we do the things we do.

With the individual managers from the TMC to explain the scope and objectives of each packet of work at the LOA-1, 2, 3 and 4 level, and then individual researchers from the laboratories and the contractors will come in and describe some of the highlights of that work.

We -- as the last time, we prefer an informal approach and are ready to answer any questions that you wish.

And Al told me earlier that you'd like to start off with an hour or so of free discussion.

And we're happy to do that, if you wish.

It's not in your agenda, but do you want to do such a thing?

MR. CARBON: No.

MR. GAVIGAN: You don't want to do that. Okay.

I'll just give some general view-graphs, which are repeats of what we did before -- what I did before but to bring people up to speed.

Commercialization is the goal, of course, of LMFBR's.

And to do that, partly, we have to meet requisite levels of safety or risk depending on where risk stands versus safety.

There is a difference in approach for many people.

And from our viewpoint, there have to be unacceptable economic penalties.

There will be no commercialization unless you have reactors that people will buy.

Our purpose, then, is to provide data base to assess the risk to the public of LMFBR's.

We provide this directly to, if you wish, our customers, which is the project.

The people themselves are designing the reactors and have to go through the regulatory reviews in the future.

MR. MARK: Frank --

MR. GAVIGAN: Yes?

MR. MARK: There is a nice question raised in that first box.

It's been addressed by some of the people here who wrote what occurred to them on the general question of, what should be the real boundary conditions when you think about LMFBR's?

Okay. So they'll require the demonstration that they meet requisite levels of safety.

Now, one can ask one's self about that. Presumably, one means by that, does one, the safety.

And by "safety," I think one has to go back to something measured in person rem per year.

MR. GAVIGAN: Right.

MR. MARK: That certainly may be the most easily picked-out term, person rem per year probable for the public at large.

That clearly cannot afford to be higher than the analogous number for light water reactors.

MR. GAVIGAN: Correct.

MR. MARK: And in fact, it can't be higher than the number that will come up for light water reactors in three years or five years or something.

So while that number doesn't move a lot, it is -- there are great efforts to try to move it down.

And consequently, the requisite level of safety includes, if it isn't equal to, that kind of prospect.

MR. GAVIGAN: I agree.

MR. MARK: It would be attractive if it could be demonstrated that it were lower.

But it's essential that it be demonstrated that it's not higher.

MR. GAVIGAN: Correct.

MR. MARK: Now, in addition, of course, [illegible]BR's suffer from the perception that the fuel might be easier for terrorists or foreign nationals to use malevolently.

This really isn't very true, but it's felt to be true.

So they have to have that kind of thought in mind, at least.

But they absolutely must meet some criterion, which I've tried to put my own terms on.

It doesn't have to be ten times safer than LWR's, for myself.

But it has to be at least as safe as LWR's are perceived to be by the time LMFBR's come on the scene.

In other words, that line of questioning, which, as I say, has been raised by some of the people here -- a fair description of DOE's thoughts on this general subject --

MR. GAVIGAN: I think the phrase that we always use is comparable to LWR's.

Now, that's a fairly subjective term and allows us to be less than LWR's if we so choose.

The whole area, you know, of application -- of risk analysis itself and application of that to a design and in making decisions based on the risk analysis is rudimentary, to say the least.

We are trying it in the -- have tried it, as we showed you last time, in the large developmental plant.

And the criterion we used to judge was, given that there is no publicly published requisite level by NRC, we use the WASH-1400 number, the risk value from that, as a way of determining whether we are comparable to LWR.

Now, we have a problem in trying to determine that we're equal to or less than LWR's, which is the uncertainty in the risk values that we come up with because of our data base.

LWR's have the same problem, of course. And when you end up trying to compare means of risk curves, you end up with a broad uncertainty band around either risk curve.

And then you're trying to make judgments within that.

So what I'm trying to say is, yes, we agree with what you're talking about. We're trying to do it.

The tools that we have to use are not all that sharp or all that well-honed.

We have in addition this constraint put on us, unacceptable economic penalties.

MR. MARK: Of course.

MR. GAVIGAN: At the same time, when we work close with a project, they're leaning on us not to have too costly systems.

They're leaning on themselves not to have too costly systems.

So you're constantly searching of ways to meet this criterion at lower cost.

So I guess in summary I'm saying, yes, we have that in mind. We're trying to make it work.

We can't say that we've been totally

successful yet.

But there is promise in the approach that we're taking.

MR. MARK: I guess I would -- well, to say only in connection with what you just said, and I'm not disagreeing in the least, there is a tendency to regard WASH-1400 as having numbers in it.

And in my own belief, it doesn't deserve to be thought of that way.

You have to make a comparison. The numbers in WASH-1400 aren't known with respect to LWR's. They aren't applicable today, anyway.

And so all you can do is get a feeling for the fact that we're no worse off than the other one is if judged by the attempts that WASH-1400 set own rather than the results and probablistic risk assessment, which is a great profession.

I think in Mike Bender's letter he separates the "A" from the "PR."

He thinks that PR deserves to stand by itself in its usual connotation, and the "A" is an assessment or something.

And as a quantitative thing, it's a good effort. It's good sense. It should not be believed.

MR. GAVIGAN: Well, there are cases under which you can believe it.

MR. MARK: Only in the comparative.

MR. GAVIGAN: Yes, only in the comparative sense.

As we use it within a plant where we take a fixed data base, then comparing systems and system substitutions and component substitutions, it helps you make up your mind of what --

MR. MARK: This one is more in need of attention than that one.

MR. GAVIGAN: Right, exactly.

MR. MARK: Things like that. Well, I didn't want to go off separately except to ascertain that requisite levels is something of the sort we've been trying to phrase.

MR. GAVIGAN: Right.

MR. SIEGEL: I'm not denying your decision not to have general discussion.

But I want to raise a question about how constraining that unacceptable economic penalty really is.

Do -- we read nowadays that the nuclear steam supply costs maybe ten or fifteen percent of the total plant cost by the time it's built.

And since most of the decisions with respect to the inclusion or the omission of some particular element of the design which influences safety probably relates to the cost of the -- of some part of the nuclear steam supply, it seems to me that these decisions have a small -- a very small influence on the final total cost of the plant.

And if a more risky decision is made, one of the consequences of which is delaying the schedule of the licensing process by some time, that cost could swamp -- the costs associated with the additional licensing process time, could swamp the cost of the piece of equipment you omitted.

How is all that recognized in making a cost-benefit analysis of some particular safety feature?

MR. GAVIGAN: We haven't gotten to the point yet of making a cost-benefit analysis in a true --

MR. SIEGEL: That's what I'm asking. How does that constraint of unacceptable economic penalties really apply?

MR. GAVIGAN: Both of those elements of cost that you're talking about figure all the time in our arguments with the project.

If we as independent safety people are pushing a particular safety system, they will come back with us in the arguments of capital costs because they're controlling at a lower level than you were just talking about.

When they control plant costs, they control down the components.

If we're talking about an additional component, they'll try to keep that component out because their job is to protect cost.

On the other hand, we'll tell them that it is our considered judgment, let us say, that without this component you may add some amount of additional licensing time, which at that time in the schedule can cost you so much money.

Then, if we try to quantify it, we find it's very difficult to put any estimates on the length of delay we might have introduced in the licensing schedule by not putting the component in.

Now, we're better able to quantify the component.

So it figures subjectively but not quantitatively in our arguments, both those elements that you mentioned.

MR. SIEGEL: Subjectively.

MR. GAVIGAN: Now, how constraining is this? It is constraining because --

MR. SIEGEL: You say it is?

MR. GAVIGAN: Yes, it is constraining because I could take two cases.

For example, we are developing the safety program, something called a SASS, self-actuated shutdown system.

And there are a number of uses one can think of it -- for it.

It could replace an existing secondary suutdown system because it's activated in a different way and may have certain desirable features.

Or it could be considered as a tertiary system, though that's not a very popular idea.

In either case, they're elements of cost. People in the project tend to be happy with things the way they are.

Yet, on the other hand, they see certain features that are useful.

So cost then becomes an important element in whether you have this particular item entered into the reactor system or not.

We're not at the point now of having developed it yet that they want to take it over.

But cost is a significant element in that decision.

The second constraint is that when we discuss plant costs, and I and Jim Hartung particularly have done a lot of work in this area, they talk about costs of particular safety features that one might want to consider at this time in a conceptual design process.

And there are a lot of features that we talk about.

There are internal core catchers and external passive core catchers and different containment designs.

And all of these have cost elements involved in them.

Depending on our philosophy towards safety of the plant, you can either see those as potentially adding to the safety of the plant or not or potentially improving your schedule or not.

So cost figures in prominently into whether we introduce those features.

And they become very viable things in a project.

And every time we tend to add something to the plant, we have to go through a cost argument.

Now, it's not a very quantitative thing. Sometimes it's quite quantitative, but generally it's not quantitative.

But it's an element when you add something of safety in the plant.

And it's often -- it has to be traded off against any safety advantage, I think, you might get from it.

MR. LIPINSKI: Question?

MR. GAVIGAN: In the plant now, we're reducing the cost of this large developmental plant right now; and safety played prominently in that.

Some of the safety systems were modified because of the cost of the safety systems.

MR. CARBON: Walt?

MR. LIPINSKI: In reviewing some of the LWR systems, it's become obvious that the single-figure criteria is totally inadequate in duplicating a bad system, still doesn't give you adequate performance.

In looking at your numerical assessment, you look at the reliability of whether duplication,when you meet the single-failure criteria,or triplication, if necessary, to give you the higher reliability, is considered?

Because you get to higher cost levels each time you --

MR. GAVIGAN: Right. As you mentioned last time, not only -- it depends on what you mean by a reliability and how you achieve the reliability, of course.

If you have to look at diversity and redundancy and common-cost failure considerations in addition to just the simple number grinding out what's a failure-rated difficulties component, given this sort of omission, we do look at that in great detail.

MR. LIPINSKI: You can certainly see some numerical advantages.

MR. GAVIGAN: Right. Requisite safety levels.

Our R and D -- to achieve the goal of safe operability of LMFBR's, our R and D plan, when we put it together, considers to what extent R and D can influence a design, the operation in all modes; that is, cold shutdown, hot shutdown and normal operation as well as maintenance actions.

Licensability of LMFBR's is an ultimate goal.

That's a definition of requisite safety levels in addition to our own criteria that we use in DOE; namely, the dose criteria mentioned earlier.

We must ultimately meet NRC safety criteria, if they get established for LMFBR's.

Right now they're in an evolutionary stage.

And NRC possible utilization of quantitative risk goals must be followed also because this would give us a different kind of criterion to meet, from our viewpoint a more rational approach, perhaps.

Safety R and D program is broad. I'm sure you -- Don will show you later a work breakdown structure that shows that we have work in all areas.

We call them our lines of assurance, 1, 2, 3 and 4.

It's broad in that we address both prevention and consequence mitigation.

One of our controlling philosophies is to exploit inherent plant characteristics to resolve safety issues.

That is, we won't depend only on mechanical components.

We also depend on, let's say, Mother Nature to help us out of certain situations.

And we utilize sound management tools and structure the work and set the goals.

Don can give you some details in those, if you wish.

The broad technology base within cost constraints mentioned earlier, we arrange our programs so that we supply information during the design and evaluation process.

We aid during regulatory review by bringing experts from the national laboratories to help the project in the discussions with NRC.

And we are concerned with this question of economics optimization, as I mentioned earlier.

Now at a subjective level, eventually we hope at a more quantitative level.

We have to develop capabilities, then, to integrate these safety program elements into an overall risk assessment structure.

We do have risk assessment studies going on at the same side.

And we try to assure that our work falls in that kind of structure.

Later today, perhaps tomorrow, when Mark Temme is here, if you want to ask him, he can show you the relationship of event trees to lines of assurance, some preliminary work we've done to see what the relationship is of risk to the program planning.

And then we have to disseminate the results for use in design and licensing of future LMFBR's.

And we have elements of work aimed at that. Taking those in a little more detail, design and evaluation gives the analytical tools, the large codes that were developed in Oregon, the codes we helped support for whole-core accidents.

We evaluate cost benefit for certain design option.

And we develop new and improved safety features.

I mentioned one of those earlier, the self-actuated shutdown system.

For regulatory reviews we provide project support, SHR analysis, R and D for resolution of safety issues and expert witnesses.

We provide new and improved design features for achieving requisite safety with reduced cost.

And the last thing is risk methods development.

The development is ongoing and has been for a number of years.

We're forming a reliability data base at Oak Ridge National Laboratory using our own breeder reactor program information as well as information from the U.K.

We're trying to get the information from U.K. It has demonstrated its usefulness in the design process during the large developmental plant study.

It highlights the need for reliability testing.

It points out the shortage in the data base. It points where you think it is that you need more information.

And a quantitative acceptable risk criterion will be useful, yet the methods are useful in a relative way without such a criterion within a project.

However, overall, we're going to need such a criterion if we want to make the risk business work..

If there aren't any more -- any questions on that, I think Don can --

MR. MARK: I guess I have a kind of a question, which I'm not sure I've formulated well enough and I'm not sure that I understand well enough.

Let me hypothecate. You could concentrate on the study of reliability of control rods and the mechanisms that drive them.

And you can say we need some more assurance on the certainty that the control rods will act at some given speed when we want them.

If you had in the LWR an absolutely guaranteed decay heat removal system, if you had one, you could put all your reliance on that and say, control rod drives, it's up to you, Mr. Plant Builder, whether they're this or that reliable.

And you may study it; and you have a damn good reason to do so because, if they don't work, maybe you'll destroy your fuel.

But we don't care if you destroy your fuel because even if you do, the plant is safe.

For a limited accident, something of that sort could be said.

And, therefore, the only thing we, DOE or NRC, cares about is that you assure the latter.

And you, for your stockholders, may want to do further work on the former, on the mechanics of avoiding the need of using the decay heat removal system because you have a big interest in that; but it's not our interest, it's yours.

One could find instances, at least, and whether I've given a good one I'm not sure, where you would not need Oak Ridge to study the reliability of this or that component, providing you were insisting upon a thing which would guarantee the outside risk from the plant, even if the thing burned up.

MR. GAVIGAN: Your hypothesis depends on a system -- shutdown heat removal system that operates with a reliability of one, correct?

MR. MARK: Of course, yes.

MR. GAVIGAN: Well --

MR. MARK: But, you see, we're led into saying each thing has to work with reliability of one minus ten to the minus 3.

And if we had an absolute block at some point, we could say, you don't have to study that or do it except for your own interest.

MR. GAVIGAN: I'm not quite sure how --

MR. MARK: Would there be a line of approach in this problem which had such features?

I mean, if you knew the containment was really guaranteed --

MR. GAVIGAN: I think what you're describing is somewhat close to what our philosophy is.

MR. MARK: That's really -- it was really a question rather than an assertion.

MR. GAVIGAN: We -- I would like to phrase the answer in terms of prevention versus consequence limitation.

We could -- we would like to be able to convince people, the public and NRC, that our prevention methods are so well in hand that one doesn't have to consider the large accidents that people are very concerned about and, therefore, the large off-site doses that are normally associated with big accidents and with LMFBR's.

If we had a perfectly reliable shutdown system and a shutdown heat removal system, you could easily convince people; just fall back and say, you're right, the accident can't happen.

We are moving toward that overall, however, in the real world, with aiming at reliabilities that are very, very high.

We can calculate reliabilities by adding additional diverse systems, considering common cause failures, that can make reliability extremely high and close enough to one that one would think that human action would say, that's good enough; that's one.

But they don't. They choose instead to emphasize that one chance in a million or a billion, whatever it might be, of getting that big accident.

We would like the industry and regulatory people in general to be convinced that the biggest benefit in safety research comes from preventing accidents rather than letting the accidents happen and then bottling up in a containment building and telling the public, don't worry; we've got that thing in a building. You shouldn't fear.

I think that TMI showed that that resulted in the wrong psychological response downwind, that people wished that hadn't happened.

And from our viewpoint, the better approach by far is to tell people that the accident won't happen and demonstrate with operating history and real data that the accident does indeed not happen.

MR. MARK: I wish you would have just reversed the order of the phrases in that last sentence.

You said tell them and then demonstrate.

MR. GAVIGAN: Well, of course -- right.

MR. MARK: I would say it the other way around.

MR. GAVIGAN: I agree.

MR. LIPINSKI: You get a conflict because with probability one, if you guaranteed that any accident is contained, it doesn't affect the public, you've accomplished one thing.

But then also, if I say with probability one the first time you run that plant, you're going to destroy it, you're defeating your economic evaluations in designing that plant.

MR. GAVIGAN: Right.

MR. LIPINSKI: So you have to consider both ends of it.

One, you want to protect the public --

MR. MARK: But the economic thing is up to the builder and the utility, and it's their proper concern.

MR. LIPINSKI: Yes, but it's part of an R and D program. I think that's appropriately included.

MR. GAVIGAN: Right. I guess you have to think of -- the economics of the plan itself with respect to what it costs to make electricity and will people buy it or not is a legitimate goal of DOE.

Otherwise, we're developing the wrong system.

The viewpoint of property damage, though, from an accident is not legitimately in our area, even though we have a lot of tools that could help people make those kinds of studies.

That is, considering the accident where everything is bottled up, no one's hurt downwind, and yet the plant is in the situation that TMI is.

That kind of study we do not do in our R and D program.

We're prohibited from doing that because it simply is not in our area of responsibility.

MR. SIEGEL: I'm not sure I understand the real meaning of your last comment.

But I certainly want to concur in your position that mitigation is by no means the solution compared to prevention.

Prevention of the accident is by far the most important thing in all sorts of ways.

The economic consequences can be translated into hazards to the health and safety of the public, if you want to.

And then there are the indirect consequences that you mentioned of psychological damage.

So mitigating the accident, as was done in TMI, is by no means the solution.

Prevention has to get most of the emphasis. If that's your approach, I certainly second it.

MR. GAVIGAN: That is our approach.

MR. CARBON: Fine.

MR. GAVIGAN: Don?

MR. FERGUSON: Watching previous presentations to committees like this and others where each individual speaker gets up and throws out his view-graphs, we decided it would be more coherent and organized if we produce all of these in a single packet.

And so I've taken the liberty to do that.

That contains the copies of the view-graphs that will be presented by the speakers throughout the balance of this presentation.

I've appeared before this subcommittee or at least the same people now -- this is the third time, once last summer, June of '80, in which we tried in about a four-hour stretch to give an overview of that the DOE LMFBR safety program was about.

And then in April of this year over at the Royal Court Inn we tried to relate how the various elements of the May safety program -- how it relates to the large developmental plant project effort.

To come at this a bit of a different way today and tomorrow, what we've tried to do is to provide a brief overview of the program by stepping through it.

And in each of the major program areas identifying the strategy and approach being employed, to briefly identify each significant R and D task and then to provide more detailed presentations on R and D work in a number of the major tasks.

And about twelve or thirteen areas we'll be discussing in more detail now.

The program managers from the technology management center will be leading off the discussion in each of their respective areas of responsibility.

And except for the more detailed

presentations to follow, they'll be covering their areas fairly briefly.

If you want to ask questions and discuss tasks that they identified rather briefly in some more detail, feel free to do so.

They may not have view-graphs prepared, but certainly can discuss with you the work that's going on and so on.

We'd like to stimulate a discussion on the viability of the current strategy and approach in each of these areas.

That's the kind of feed-back that we've received at earlier presentations, has been very useful to us.

We find our program tends to be rather insulated and isolated these days in the absence of meaningful LMFBR licensing activities.

So to have a group like this look at it and comment on it is very useful.

Now, from the technology management center staff, Mr. Amar, who is on assignment to the TMC from Atomics International, Lou Baker, myself, Ralph Singer and Jussi Vaurio will be heading off the presentations in the various LOA areas.

And then the contractor staff members, as they are identified on the agenda, will be providing more detailed discussions in a number of program areas.

The next view-graph is really a capsule summary of the goals and objective statements that Frank made.

We're about providing a technology base. We're not about designing plants, designing safety systems for plants specifically but rather providing a technology base, supportive safety considerations, in the range of areas of concern; namely, design, licensing and economic optimization.

And in that latter case, I guess we always find ourselves at opposite ends with the plant design people.

And I guess that's been the case throughout history and will probably continue to be the case.

We have to provide strong justification for the addition of any safety systems because they cost money.

And that drives the overall cost of the plant up.

So a major part of our effort has to go into making certain that we can justify the inclusion of additional safety systems, both for ourselves and for plant designers.

And this third view-graph and the last in my brief introductory remarks you've seen a number of times.

It merely indicates how the program is organized.

Our reactor reliability work; core damage limitation work in terms of looking at ways in which we can accommodate, with minimal core damage, faults in the major safety systems; the work that focuses on accommodating the consequences of ACDA's; and finally, on attenuation of radiological consequences.

And we'll be covering, then, the major program tasks as they fall under these various second-level products in the work breakdown structure.

And that's a dozen or so presentations scattered throughout these major program areas.

Are there any questions up front?

(No response.)

MR. FERGUSON: That's all I'd intended to say.

I'd like to get right to the meat of the meeting as quickly as possible.

If there are none, then as indicated on the genda, Jussi Vaurio is going to go over the work in

LOA-1 and then provide more detailed discussion in the area of common cause failures.

And he'll introduce the additional speakers that are talking about tasks.

MR. VAURIO: The objective in LOA-1 is to prevent accidents and demonstrate that LMFBR's can be designed, constructed and operated so that they have extremely low probability of accidents.

By "accidents" in this context we mean multiple fuel pin failures.

There are basically two means to accomplish accident prevention.

One is to use sound, conservative, intrinsically safe design features for normal operation and adequate margins for operational transients.

The second is to use reliable, dedicated safety systems to assure that off-normal events can be prevented or accommodated safely.

These principles are used in the LMFBR program to accomplish three major second-level products.

These are reactor system reliability, reactor shutdown reliability, and shutdown heat removal reliability.

There are three major considerations in this LOA-1 area.

First we have to establish a strong designer and safety analyst interface to assure that appropriate safety design criteria and reliability requirements are reflected in the plant.

To accomplish this, we have to advance the state of the art in design, engineering, quality assurance, operating and maintenance practices.

And we have to develop proper specifications for redundancy, separation, diversity, human engineering and fail-safe features.

Consequently, reactor systems can be made very reliable so that the probability of occurrence of off-normal events that would necessitate the use of shutdown systems or shutdown heat removal systems is very low.

And these shutdown and heat removal systems can be made reliable enough so that the core disruptive events can be excluded from the design basis.

MR. SIEGEL: Excuse me. Is this last statement a goal or an accomplished fact?

MR. VAURIO: It is the goal at this time, and the best of our -- to the best of our knowledge, we are able to demonstrate that.

There is no evidence that this cannot be accomplished.

I cannot say that we have definitely proved that it need not be considered anymore, but -- because the work is continuing.

MR. GAVIGAN: It's been shown -- there is a letter from NRC COT (phonetic) project that says CDA's are not a design basis event.

MR. CARBON: That is only for CRBR, though.

MR. GAVIGAN: Well, yes, it wasn't broader.

MR. VAURIO: The first second-level product is reactor system reliability.

There the objective is to demonstrate the reliability of reactor systems resulting in low probability of occurrence of off-normal events.

The third-level products are reactor core system, heat transport systems, auxiliary systems and monitoring and control systems.

The reactor core system reliability is accomplished by providing mechanical and electronic stability, minimal fuel failures in design basis transients.

And heat transport system reliability is accomplished by mechanical integrity and providing performance capability for all design basis transients.

Reliable auxiliary systems means reliable supply, auxiliary sodium systems, component cooling systems and so forth.

And reliable monitoring and control systems mean reliable instrumentation, control systems, procedures and operational aids.

Major program tasks that --

MR. CARBON: Excuse me. What's the significance of the words "third-level product"?

What do you mean, third-level product?

MR. VAURIO: We have a safety program plan that is a structured -- structured so that the first level is this line of assurance, one line of assurance, 2, 4 or 5.

And then each of these are again divided in the sub-tasks, second-level products.

And then under those to the third-level products and so forth.

It's a structured program that Don Ferguson just started with.

MR. GAVIGAN: Third-level detail, more definition on the work structures.

MR. VAURIO: So the first level was line of assurance.

One, prevent accident. Second-level, reactors -- first item in the second level, reactor system reliability.

Under this, the third-level products are these.

I didn't number these any more, but they are numbered, of course.

And major program tasks that are ongoing or have been completed is the FFTF operational safety program at Hanford

EBR-II operational safety program at ANL; also Westinghouse is participating in that.

Reliability analysis methodology by General Electric, Atomics International and Los Alamos National Laboratory.

And seismic and fracture mecahnics studies are being conducted by ANL and Westinghouse.

MR. CARBON: You divided between ongoing and completed.

Which do you consider completed?

MR. VAURIO: Well, some of the reliability methods tasks have been completed.

But basically the work is ongoing in all these areas.

If you go into needs in each of these, there are completed and ongoing [illegible]

This is basically plant status control system development and some diagnostic/prognostic techniques development.

This is basically natural circulation experiments and local fault experiments.

And there are a number of reliabilities being developed and defect flaw analysis, operational experience and so forth.

Seismic work at ANL is concentrating on developing the most comprehensive models and metals for seismic analysis.

And those are the ways to test simpler methods that are currently used, so to test the adequacy of those goals.

And Westinghouse has concentrated on structural reliability studies of the core support structure and sodium piping.

So our strategy in reactor system reliability is to advertise comprehensive nature of the safety-related design and reliability criteria.

We want to be sure that we include everything that affects safety when analyzing any of these systems.

We want to make sure that we don't forget support systems from any essential safety systems.

We want to demonstrate that plant and reactor systems are indeed reliable and demonstrate the effectiveness of man-machine interface in enhancing reliability of plant operation.

The approach is to work with the designers to establish design and reliability criteria, carry out reliability studies of system and component designs to confirm reliability, follow extensive man-machine interaction program in LWR industry, and establish man-machine interface requirements unique to LMFBR's and continue these programs at EBR-II and FFTF.

MR. SIEGEL: You use the word "demonstrate effectively," demonstrate a plant is reliable.

To whom and how?

MR. VAURIO: First, of course, we have to demonstrate it for ourselves.

And then, of course, we want to demonstrate it to NRC and all regulatory authorities that have the final say of approving these plants.

The method to demonstrate is both -- it includes both experiments, real plant experience evaluations and, of course, theoretical studies with established data bases.

And I will talk about that later.

We want to highlight two areas, two topics, in the reactor system reliability that deserve most attention.

These are common-cause failures and man-machine interface.

Operational experience, including the TMI incident, indicate that if we want to improve the safety record of nuclear power plants, these are the areas we have to work on.

First, I will talk about the common-cause failures, a little bit about the definitions so we know what we are talking about.

Something about the background. What has been done before? What is our strategy and plan concerning common-cause failures? And some highlights of recent results.

By the way, these programs are in the early stage so that part of it's still in planning stage, but some work has been started.

But we don't claim that we can present very impressive results at this point.

First about definitions, IEEE Standard 352, published in 1975, defines common-cause failures as "multiple failures attributable to a common cause."

This is a very general statement. For example, it doesn't require that the failures are simultaneously present.

It doesn't require -- specify how these

failures should be attributable to the common cause.

It could be a common external event, like fire or flood, for example, that fails several components.

Or it could be a failure of one of these components in such a way that it fails one or two others.

Or it could simply be an event that changes the environment without failing any of these, but changing environment in such a way that the failure rates of all these components or more than one component is increased so that it becomes more likely that they fail simultaneously later on.

There are many other definitions, of course, this one from United Kingdom:

> "Common-cause failure is an event which, because of dependencies, causes a coincidence of failure states of components in two or more separate channels of a redundancy system, leading to the defined system failing to perform its intended function."

More specifically, it requires a coincidence of failure states.

For example, it also requires that the system fails.

This one only requires that there is more than one failure, not that the system fails.

The conclusion from all this is that common-cause failure, that term, means different things to different people.

And that is one of the reasons why more has not been done before.

This chart illustrates background if a common-cause failure problem was recognized early on in the '50's and '60's.

Data evaluation started late '60's. These evaluations all suffer from a variety of definitions.

Everybody chooses his own definition, and for that reason the results are not directly applicable to any other system or study.

You always have to go back to the original data base to determine the common-cause failure contribution in your system.

Prevention methods have been identified, also starting late in the '60's, and reliability methods.

Many of the standard reliability methods can be applied to common-cause failures, as well.

But some methods have been developed specifically for common-cause failures starting early '70's.

This doesn't include the relevant publications but some of them.

The conclusion is that operating experience indicates high unavailability contribution due to common-cause failures.

And it almost doesn't matter what definition is used and what system is being analyzed.

In spite of that, we have evaluated the current LMFBR systems using pessimistic assumptions and found that they are adequate.

There is no reason to expect common-cause failures to cause higher risk in LMFBR's than in best light reactors.

MR. CARBON: Excuse me. You just made two different statements, one that they're adequate, and one that they're no worse than in LWR's.

How can you know that they're adequate unless you've carried through a very detailed design of a specific --

MR. VAURIO: I'm saying that at this point, with the level of details that we have about the designs, the best experts we have been able to find have evaluated these systems and have found that even with the most pessimistic assumptions, using data base from currently operating plants that, of course, have different -- have used different design criteria and principles than what we have now, still the reliability of these systems is as good or better than in light water reactors.

It's because we are in -- current concepts have more diversity and redundancy, for example, than most light water reactors.

We can come back to these specific design features later on.

But that's the current status. We still have to, of course, ask why, then, pay attention to common-cause failures?

We want to demonstrate quantitatively that the designs are adequate with more specific information about the designs than we have now.

And also, we want to justify flexible, less conservative design criteria.

For the reasons I mentioned, there is considerable doubt in light water reactors -- LMFBR industry that -- or opinions that some of the current criteria and practice may be overly conservative.

We want to evaluate that more carefully.

Also, there is this public acceptance of nuclear power in general.

We don't know if we can accomplish this ever, but we ought to try.

And if we can show that we can control common-cause failures, that certainly should help.

Why do we believe that common-cause failures can be eliminated or at least reduced considerably?

One reason is that current plants have been designed in the '60's and '70's.

And all the data base we have comes from these plants.

All the design criteria or most of them have been changed. Operational criteria have changed since then.

There is a good example from aircraft industry.

They have two orders of magnitude difference from common-cause failures for systems with same degree of complexity.

MR. LIPINSKY: On that subject, the recent incident at Hawaii where all the engines went off on a Jumbo Jet simulateneously, causing it to lose altitude, is going to be interesting when they find out what the common cause was on all those independent systems.

MR. CARBON: In all seriousness, do you look into something like that?

MR. GAVIGAN: Have you tried to follow up on that 774 event and try to get the information on that?

MR. VAURIO: Not yet. I have been on vacation for a week now.

MR. CARBON: I don't mean yet, but I mean in the long run.

Do you look into probabilities, risk assessment, common-cause failures in other fields, other --

MR. VAURIO: Certainly, we try to follow that as widely as possible, both in-house and, I'm sure, the contractors are doing that all the time.

MR. CARBON: Are you sure, Frank?

MR. GAVIGAN: No, I'm not sure it's being done. That's why I asked him to answer.

MR. CARBON: I just wondered if you were sure, too.

MR. GAVIGAN: No, I'm not sure. That's his job.

MR. VAURIO: Also, studies indicate that many of the common-cause failures could be eliminated by improved testing and inspection methods, both pre-operational and periodically during plant operation.

MR. SIEGEL: At first sight, that last statement appears surprising.

I would have assumed that common-cause failures are more likely to be associated with subtle deficiencies in a design which weren't really foreseen and then they crop up and it's a design deficiency rather than an individual equipment item deficiency which improved testing or inspection might reveal.

Or it might be even some phenomenon that is occurring at a different rate in the operating reactor system than was originally believed to be true and equipment fails earlier than it should have.

MR. VAURIO: There are mostly -- most of the common-cause failures are human-related errors in one way or the other.

They may be human errors, mistakes made in the design phase or in operation and maintenance and testing and in operation.

But I'm saying that if we evaluate this pre-operational testing practices and the periodical testing practices and learn from the current -- past experience, we can see how they should be improved, what should have been done in those to have -- so that these mistakes had been found earlier and prevented.

MR. SIEGEL: Before you leave this chart, I have a relative comment or question.

I find the first two statements kind of contradictory.

Operating experience seems to -- if I understand what I'm reading, operating experience reveals what I interpret to be a substantial amount of common-cause failure.

Yet the second statement says that LMFBR design concepts today are free of these common-cause failures?

Which -- am I reading it right or what?

MR. VAURIO: This operating experience, of course, most of it comes from light water reactors.

Based on those, evaluating that experience and seeing how much more redundancy, separation, diversity and so forth we have to build for these safety systems to eliminate, reduce those common-cause failures.

And in LMFBR design concept that we have now, we have more diversity, separation, redundancy, all these features that are believed to eliminate common-cause failures.

So that we don't expect the current designs here to have as much as --

MR. SIEGEL: That operating experience relates principally, then, to the current light water reactors?

MR. VAURIO: Right, yes.

MR. LIPINSKI: On the subject of redundancy and diversity, have you found that diversity helps to improve the sensitivity to common-cause failures?

MR. VAURIO: Yes, what --

MR. LIPINSKI: It's been applied in terms of a general feeling that it should.

But have you been able to verify that it has?

MR. VAURIO: I have evaluated this operating experience, some of it, personally.

And I'm -- what I have seen convinced me that yes, it indeed helps.

If you have steam-operated pumps and electric pumps, for example, instead of all electric, it clearly helps.

I don't know how much the contractors have done this evaluation work.

But I think that that kind of work is going on at Atomics International, for example.

Can you comment on that question, Jim?

MR. HARTUNG: Yes, in our work we think that diversity buys you a considerable amount.

We're trying to determine exactly how much and trying to make the most of it.

MR. VAURIO: So what is our strategy concerning common-cause failures?

What do you want to do when you have a strong enemy and small troops of your own?

You want to do what the small armies have done throughout history.

You want to first determine the boundaries of the enemy.

You stop it and isolate it from all other troops, from other events in this case.

Then you divide your enemy into small pieces, smaller groups, so that you can then destroy these parts one-by-one.

To accomplish this we first have to develop a structure of definitions and categories for common-cause failures to improve communication and management so that everybody knows what everybody else is talking about and what he is including in his definition and under what title he handles all the other things.

It also is necessary for data collection and evaluation so that every piece of evidence has a definite place in the structure to go to and where it can be found when needed to evaluate the systems.

We have to evaluate operational experience to verify the feasibility of the categorizations, assess the likelihood under current design criteria and guides, assess the applicability of light water reactor data to LMFBR's, and estimate parameters from the experience failure rates and so forth.

And then we have to identify potential defenses against common-cause failures, evaluate the efficiency of these and identify and develop methods for quantification.

Now, this figure illustrates one of the difficulties we have in demonstrating that we can control common-cause failures.

If we take current average light water reactor plant, the unavailability of one of its systems, whatever system, is somewhere here due to common-cause failures.

If you use the most recent design and operational criteria today, if we had that kind of plant, we would have unavailability considerably lower than what we have from the current plants.

Now, before we can have this plant in operation, it takes perhaps ten years.

And then we only have one plant. One plant doesn't prove anything statistically.

We have to wait another ten years, perhaps, before the average plant and the real field experience proves that yes, indeed, in 1981 we were here.

So if we have to wait twenty years before we can prove anything, we might as well forget about it. It doesn't help us in licensing.

We have to use some other means to prove that although the current plants show this, we are actually here.

And to do that, we have to evaluate what has happened in these plants and what have we -- what are we able to do to eliminate most of those cases so we can show that this is the real situation?

This figure indicates some of our initial thoughts about structure cause failures.

If we start from any informational experience licensing report, for example, we can first determine whether that was a discreet event in time, a sudden occurrence that failed several components, or whether it was an event that simply increased failure rates instead of failing the components.

The next step is to evaluate whether these multiple failures were in the same system, in different systems or whether it was simultaneously a demand or challenge for the safety system that is being analyzed.

The next step is to look, when did the common-cause failure intrude the system?

It could be pre-operational, before the commercial operation, due to periodic activities like testing and maintenance, or it entered randomly in time during plant operation.

And these all have different causes.

Next step is to look the causes before the operation.

There could be a design error; there could be a common component, a common link between two otherwise redundant systems.

There could be a common material manufacturing or installation error.

Periodic errors usually unrelated to testing or maintenance, randomly-occurring failures, could be due to events beyond design basis.

They could be due to common operator, repeated human errors, or events either external or internal.

And all these can be divided into subcategories, of course.

Then we have to look, when did common-cause failure exit the system?

When did we find it and eliminate it?

The worst case is, of course, if we only find it because there is a challenge for the safety system. That is already too late.

Other possibilities are during annual shutdown because then more components can be inspected and more systems.

The discovery could be due to random operational circumstances due to periodic testing, or the failure could have discovered itself when it entered the system.

Then, in developing defenses against this, we have to march backwards through this chart.

These -- all these cases, of course, had one or more models and metals that could be used to exactly describe the process of entering and exiting failures.

But developing defenses, first you want to look, how could you have discovered it earlier than you did?

Then you look the causes. What should be done?

How should design criteria be changed so that you can eliminate this cause and entry?

A number of models have been developed for common-cause failures.

In this case I have divided them into two categories.

Of course, the occurrence of failures and their removal is a slow process.

And one set of models intends to model exactly the process of each different type of common-cause failures.

And these kind of models were used, for example, in WASH-1400.

They had an explicit modeling of energetic events, fires, floods and so forth.

They used conditional probabilities for repeated human errors.

After one error, the likelihood of another one similar is higher.

And they evaluated the coupling between failure rates.

Event tree methods can be used to take into account functional dependencies so that, if one system failes, it doesn't really matter whether this other system fails or not because the consequences are the same and so forth.

Many fault tree methods can be used for

common-cause failures.

Reliability and failure diagrams can be used with computer cores, such as PROBCALC, FRANTIC, et cetera.

Bionomial common-cause rate models can be used by NRC and conditional probability models by SANDIA.

Then there is another family of models I call here summary models that try to lump together all these common-cause failure types and describe them by a single model.

One is geometric mean method that was used in WASH-1400.

For these common-cause failures that were not explicitly treated, they tried to estimate to get some numbers for unidentified common-cause failures.

There is a beta-factor model that was developed by General Atomics and a correlated or coupled parameters model by Atomics International.

I feel that the summary models do not really support our strategy as well as these explicit models because these try to draw all the common-cause failures into one model, lump them together, when we really want to split common-cause failure into pieces.

And explicit model is one of them. And then find defenses against them.

MR. LIPINSKI: Earlier you stated that human errors were the biggest contributor.

Out of this total list, none of these analytical methods allow you to put a handle on the human error contribution.

MR. VAURIO: I think you can put human error contributions in any of these

MR. LIPINSKI: Yes, but where do I get the data?

It's a statistical number that I need in order to factor it in, in order to come out with a number.

MR. VAURIO: Well, of course, there are basic human error or data banks being developed like the one at SANDIA.

But also, all the nuclear event experience in licensing event reports, you can identify there what -- how human errors contributed to the event and estimate parameters for whatever model you want to use.

MR. LIPINSKI: Okay. I can look at a particular system that an LER is written for.

I can compare it to the system that I have.

If I see a relationship I can try to change the design of my system.

After I get through, what do I conclude about the reliability of my system with respect to a common-cause failure due to human error?

MR. VAURIO: Well, I don't know if I understood the question.

But if you include both for existing systems that you take your data from and evaluate where the human erorrs were in there compared to hardware errors, and you include in your new system model similar type of human error possibilities that have bee found, I think in principle you are able to compare these properly.

Of course, this human -- the emphasis in human errors is more recent than the work on hardware failures.

So that I think we need to work more on human error areas.

MR. LIPINSKI: My only comment, I think, is, there's an awful lot of judgment involved in evaluating common-load failure due to human error, that it's not amenable to hard numerical treatment.

MR. VAURIO: That may be the case, yes. The project that is going on at Atomics International has two objectives.

One is to develop understanding of common-cause failure mechanisms and apply this knowledge to improve LMFBR safety.

The technical approach is to develop a common-cause failure model, in this case the coupled parameters model, evaluate operating experience, evaluate common-cause events, design and operation criteria that were used for those plants when those events occurred, and the relevance of those events to current LMFBR.

Then identify common-cause failure prevention and accommodation techniques and evaluate the efficiency of those techniques.

MR. CARBON: In operating experience, is that intended to be all reactors, LWR's, LMFBR's?

MR. VAURIO: Since there are much more light water reactors than LMFBR's, I think we have to, to some extent, use light water reactor experience.

And I think the justification for that is that many of these common-cause failures are human-error related.

And when they are human related, it doesn't matter whether it's water or sodium in the pipes.

You can do very much the same kind of errors.

Of course, there is not complete correspondence there.

MR. CARBON: Can you cite an example or two of common-cause failures from people, experience, actual experience?

MR. VAURIO: Actual experience?

MR. CARBON: What you would consider an example that fits in that category there.

MR. VAURIO: Well, there are cases, for example, when the valves were -- that were supposed to be open were closed because after the testing, the maintenance people didn't -- forgot to open them.

And that is certainly something that could happen in any kind of plant.

Just one example, the basic assumption in this coupled parameters model is the assumption that component unavailabilities have a distribution.

There is certain probability density for the component unavailability

In this c [illegible] the distribution is such that the median value is ten to minus three, mean value five times ten to minus three, and variance seven times ten to minus four.

This is, of course, not -- nothing very new because the same kind of thinking was used in WASH-1400.

When they evaluated systems, for example, they used Monticello techniques to obtain a distribution for the system unavailability.

In one case they used completely independent sampling of the failure rates for pumps.

One pump had this value; another pump had this value.

And this kind of sampling gives the distribution of this kind.

Then they also used failure rate coupling so that they used the same failure rate.

Whatever is sampled for one pump, the same number is used for all the other pumps in the same system. And then this sampling is repeated.

And that gives another -- other distribution that has -- usually has a wider uncertainty, and also the mean value is moving.

WASH-1400 did not emphasize this effect, and they used Monticello techniques because the systems were complex and the Monticello code was available.

Now, it is possible for series parallel systems to do this analytically, and that's what this product is doing.

Here are some examples now. We have the average unavailability for one component.

And here is the number of components, parallel components, 1, 2, 3, 4, and the system unavailability.

The bottom line is for completely independent components.

Of course, then the -- with two components, the unavailabilities, the square of the mean value for one component.

And for three components, it's the third power of the mean value.

For completely correlated, "R" is correlation coefficient from zero to one.

Completely correlated case, of course,the unavailability of two component system is the mean square value of the unavailability of one component.

And for three components, it is the mean third power of the unavailability.

And for other decrease of correlation, the results are between these curves.

I don't want to go into more details with this model, just to illustrate some of the results at this point.

I want to go ahead and introduce this other important area in LOA-1 reactor system reliability; that is, man-machine interface.

The objective there is to improve operational safety and proper -- using proper application of human factors engineering to the design of systems, facilities, operational aids, procedures and environments.

The second-level products in this program include information on control systems.

And that is, control rams and facilities, control panels and data displays.

Diagnostic and prognostic systems are basically operational aids that help the operators to determine what is actually the status of the plant, what is the prediction for future and how to optimize the decision-making process to respond to the transient.

Operations personnel includes operators, maintenance personnel, management and so forth.

All the procedures associated with operations.

Systems integration and analysis evaluates all the circumstances in which this man-machine interface could be important and determines the control strategies to the optimum allocation of tasks for machines and men.

Ongoing activities include LMFBR man-machine interface program planning where all are participating, plant status control system development at HEDL, transition control concept development at HEDL --

MR. SIEGEL: What does that mean, transition control?

MR. VAURIO: Transition control is that you define safe and different degrees of unsafe states for the system, for the plant.

And then using computerized aids, you determine optimum bare to get from unsafe situations to safe situations.

And Steve Seeman, after my presentation, will talk more about these and other words at HEDL.

Critical safety parameters are being evaluated by General Electric.

And they also develop -- are developing dynamic data monitoring concepts.

Now, the second major -- second-level product in this LMFBR program is the reactor shutdown system reliability.

Objective is to demonstrate --

MR. SIEGEL: Do we have time to dwell a little bit on that previous?

MR. CARBON: Yes.

MR. SIEGEL: I'm curious, using the example of TMI that you quoted before about the valves that were inadvertently left closed, now, there are a number of ways that particular situation might -- there might have been an information system which revealed to the control room that that particular set of valves plus any others throughout the plant, which there are hundreds, are in a go or no-go state.

There might be an interlock which, if any one of those valves was in a no-go state, you couldn't start up.

You might have a philosophy that no interlocks can be bypassed, or you can have bypasses.

You might have redundancy, that if that valve is closed, there is another one in parallel with it which is open.

What kind of philosophy is sort of used in what you're doing now?

What are you going to -- is there any one identified approach that -- information, denial of operability, warning, prevention, duplication, what?

MR. VAURIO: All the means you mentioned, I think, are reasonably well established and known in nuclear industry.

MR. SIEGEL: Yes. I mean, that's where I learned them.

MR. VAURIO: But what we are looking here for -- 'cause those are the remedies.

But I think in this program we want to emphasize,how would you best discover those cases?

And, of course, there are both electronic and administrative means to discover those.

And I think -- we don't have specific program ongoing in that particular area, although I think it's very important area.

But the means to discover those errors is where this program should work.

MR. LIPINSKI: Yes, but when you talk about administrative versus electronic means, if you don't, say,enforce the line-up of your safety system, say through a plant status control system by hardware, and you fall back on administrative control, then you introduce the possibility of common-mode errors due to operator failures.

MR. VAURIO: Yes, certainly that is one aspect that has to be taken into account.

The more hardware or computerized aids we can develop rather than things that rely on human, the better.

That's why we have the advanced technologies emphasized.

MR. LIPINSKI: Will these, the plant status control system, be discussed in detail later?

MR. VAURIO: I think Seeman is going to talk about them.

MR. LIPINSKI: Is he? Okay.

MR. VAURIO: So under the reactor shutdown system reliability, we have the following third-level products: The primary shutdown system, the secondary shutdown system, shutdown system instrumentation and monitoring and control systems.

And the major program tasks that are going on are the primary control rod testing by Westinghouse, secondary control rod testing by General Electric, plant production system testing by Westinghouse, digital plant production system design and reliability analysis by Westinghouse.

This will be discussed by Rico Simonelli later on.

Operating experience evaluation, defect flow analysis associated with control systems is being done by General Electric.

So the strategy is to demonstrate the extremely high reliability of LMFBR shutdown systems.

We have two completely separate, redundant shutdown systems, for example.

The approach is to conduct extensive

out-of-reactor tests of primary and secondary control systems; evaluate the operations and failure experience and feed back the results to the designers; conduct failure modes and effect analysis, common-cause failure analysis and reliability studies of shutdown systems and feed the results back to the designers.

A lot of this testing has been completed, and we have priority criteria for tests that still, perhaps, are needed.

Shutdown heat removal system reliability is the third second-level product under LOA-1.

There we have to demonstrate that shutdown heat removal will reliably remove decay heat following shutdown.

And the third-level products here include main heat transport system, auxiliary heat transport systems and monitoring and control systems.

The major program tasks that are ongoing include reliability analyses of main heat transport systems by General Electric; reliability analyses of auxiliary heat transport systems by Westinghouse, General Electric and Atomics International.

Testing shutdown heat removal system components and systems is done mainly under LOA-2 because there is -- much of it is natural circulation experiments.

So the strategy here is to demonstrate extremely high reliability of shutdown heat removal system as a means of reducing focus on core melt events.

Approach is to conduct reliability studies for systems and components, conduct tests on hardware, tests for system and components and evaluate operating experience and work with designers to enhance reliability of shutdown heat removal systems.

Unless there are more questions, I guess I can -- I would like to introduce Steve Seeman from HEDL to talk more about man-machine interface program.

MR. LIPINSKI: I have a question. Later on, I assume, the digital PPS is going to be discussed.

But your philosophy is to have a primary rod system and a secondary rod system.

What do you conclude in terms of reliability on those systems with respect to common-cause failures due to maintenance?

The primary system has identical components; the secondary system has identical components, in terms of the mechanisms.

MR. GAVIGAN: Not true.

MR. LIPINSKI: The mechanisms on the primary system --

MR. GAVIGAN: Are different.

MR. LIPINSKI: I know the primary mechanism is different from the second, but all primary mechanisms are the same.

MR. GAVIGAN: As all the primaries, right, yes.

MR. LIPINSKI: So my question is, given a reliability analysis of those primary mechanisms, how do you factor in a common-cause failure due to maintenance error?

MR. VAURIO: There are two aspects. One is that our emphasis is now moving towards system testing rather than individual rod testing.

And this complete system testing would also include genuine operational and maintenance activities to the extent possible.

That is one approach in the long term. In the shorter term I think we have to again rely on much of the experience in earlier LMFBR's and light water reactors to see how much those maintenance activities have really contributed to the unavailability of shutdown systems, get some number for you.

MR. LIPINSKI: Okay. But the fact that they have not occurred in the past does not mean that they will not occur in the future. There is a correlation.

But the fact that you can't get a statistical base in the past doesn't give me complete confidence that I can't have it occur in the future.

MR. VAURIO: No, there must be also this qualitative evaluation of all the maintenance and operational practices and criteria and guides that were used before and what are used now.

Are the current practices better or worse than what was used before?

You can determine on that basis whether you are improving or not.

MR. CARBON: Do you have any exchange of data like LER's with the British or French or anyone?

Do you get any operational safety information?

MR. VAURIO: We are -- have started to contact U.K. in trying to formulate an agreement by which we would receive data from their plants and also work on this common-cause failure problem so that we have a wider base for demonstrating this.

MR. GAVIGAN: With France we have nothing, zero.

MR. VAURIO: Steve?

MR. SEEMAN: I would think that some of you in the aircraft industry, although you don't have exact situations, you have a number of complexities, degrees of tasks, that the operators have to do.

And if you don't have those kinds of numbers, you can pick up those from handbooks from aircraft history.

There's been a lot of work done on that in the past.

You mentioned that it wasn't -- wouldn't come across.

But if you don't have anything else, you use what you have.

MR. LIPINSKI: Well, you've done numerical reliability evaluations for the Atlas event; namely, a primary system failure given that you got a secondary system.

And you conclude that you've got a total failure probability of primary-secondary system simultaneously, which is a very small number, right?

But in order to come out with that number, you have to make certain assumptions with respect to the common mode failures that can be introduced to each primary system separately and secondary system separately in order for me to take those two numbers, put them together as a product and conclude that I've got an overall performance number.

And all I'm questioning is, how do you factor in your common mode failure, say on a primary system and common mode failure on a secondary system and in term of what you conclude their individual reliabilities are?

MR. SEEMAN: I would think that you could use some of the data from aerospace because you've got systems where a man has to make or doesn't have to make decisions and can foul something up. I'm not sure.

MR. VAURIO: One comment on that last question.

Our common-cause failure models include common-cause failures within each individual system and then common-cause failures that cross this -- between these two different systems.

MR. SEEMAN: My name is Steve Seeman. I'm from Hanford Engineering Development Laboratory, HEDL, in Richmond.

And I've been asked to talk about some of HEDL's programs or one of their programs in the man-machine interface area.

It's something that we started about a year ago.

The new TMC recognized that, that we needed some work done in this area; so we have sat down and developed a small program.

We're also involved with the TMC in developing a national program plan that brings together a lot of the elements.

It will be of interest, I think, just to some of you to see how we're set up here in the safety department.

I'm in the safety department. I work under Mr. Pedersen and Dr. Al Waltar, who I think some of you know. My group is called analysis and integration.

The objective of our work is closely related to what Jussi was talking about previously.

And that is, we're trying to enhance operational safety.

That's the key, operational safety through prevention and accommodation of plant accidents by evaluating and optimizing the man-machine interface.

The man-machine interface is a term that's kind of thrown around.

And it's worth taking just a moment to see what I mean by it since it may be different than what other people mean by it.

We look at it basically as being the boundary between man operations and machine operations, but there are two kinds.

You can say it's the space between you and the video screen or it's the physical place that man touches.

But there's also what we feel is a more important one or as important; and that is the data flow.

What data do you have the machine manipulate as opposed to what data you have the man manipulate in his mind or whatever tools he has.

And that's the primary thrust of our area, and that is to look at this functional man-machine interface.

An example of this, just to reinforce that a little bit, is when you look at the -- a control system, quote-unquote, in a power plant where you get data in, you verify the data, you diagnose the data, you decide on a course of action, you do some action.

These aren't, the man walks over to the panel or anything. These are functional dependencies.

These show the data flow within a plant. And what we're trying to find out is where within these functions you put an interface so that the man can do part of it and the machine can do part of it.

Right now it's fairly well defined for light water plants; at least it has been.

I think it's moving more in the direction of more machine operations now.

So the question is, where do you optimize this man-machine interface?

It's very complicated. It's dependent upon the state of your hardware and a lot of different other things, how you're going to run the plant.

This is my septipus here. It's intended to show the various complicated things that go into this.

In the attempt that we had to try to find out what we really need to be doing, we've got lessons learned from TMI.

We have program plan from the fast reactor technical management center.

Light water people are working on plans of action to try to improve their man-machine interface.

We've got miscellaneous advice, a number of different things.

And the question is, what really do we need, especially in my case where we're looking at breeder reactors?

And what can we do for future breeder

reactors?

What we feel is needed is a development of systems to optimize this man-machine interface, followed by assessment of the relative benefit.

This is very important because you can put one of these more automated, if you will, systems on plant, but how do you know it's going to be better?

How do you know that you haven't introduced more risk?

And so we feel a very important part of the program is to -- once we've developed some of these different systems, to do assessments of the reduction in risk, if you will, from these programs.

The way we do this, the way we plan on doing this, we have not gone into this in detail yet; but it's to look at cost benefit where the benefit is actually a reduction in consequences, where these are probabilities times -- of occurrence times the consequence of the occurrence.

So you look at what you thought it was before and what you think it's going to be with the new system, and that is a reduction in risk.

And you do some kind of quantification of your benefit from the system.

The cost will be the usual methods as to what it will cost to implement the system.

Our approach at HEDL is, since we have a real machine and it's a test machine, is to use this as a test bed where possible, to develop some of these operator aids, test them off-line, perhaps, or on-line if it's justified, and verify its reliability of things like this, determine cost benefit and try to extrapolate it to future reactors.

This is important because we're not just doing it for FFTF.

We're doing it so that we can give the designers of the large breeder reactors, future breeder reactors, a data base to work from.

As Mr. Gavigan said, we're in the business of developing this data base; and I think this is a very important part of it.

The scope of the work that we have at HEDL now is, we've laid out a fairly large program.

We're not doing a HEDL program right now. We're basically involved in planning.

We have a system that is called plant status control.

We're working on a transition control concept, and we're getting -- we're developing a testing methodology as an important part of this.

Later we feel we'll be getting into more detailed specific diagnostics or simulation concepts.

But we are not into them right now. So I'll briefly talk about the plant status control, transition control and testing methodology.

These work packages are broken up into MIDAS, which is an acronym for master information and data acquisition system, which is a plant status control system that's used -- a management system being used for testing and use on FFTF.

And I'll go into it in more detail, but it basically just releases -- it tracks the work released to the plant.

It allows the operator to make decisions to release work to the plant based on what the system has in it.

The testing methodology which we've coined our data display system, this is our tool that we're using to do the testing.

We also are developing a transition control system that is a diagnostic operational system.

And this is a system that we're hoping will give -- enable the operator to have guidance during reactor transience or equipment problems so that it tells him not necessarily what the overall problem is but how to -- where to get to a safe state.

And I'll go into that in a little more detail. I'll go through each one of these.

MR. LIPINSKI: Your plant status and control is not a hard-wired system; it's a paper system?

MR. SEEMAN: It's a paper system. It's called MIDAS.

And the situation, we feel, at present the operators have to rely on long lists.

You've all seen them. The computer prints out that, shows all the equipment in the plant, the valves, the breakers, the load lifts, this kind of thing.

They rely an awful lot on their memory; that is, their model of what the plant looks like, drawings, leg-work.

And they needed to do this to determine if it's really appropriate to release work to a plant.

If someone comes to them, and they do very often, and say they need to do maintenance on a particular valve or a particular piece of equipment, they need to take that particular piece of equipment out, taking it out, that person that's making that decision is put on the spot.

He can, by making the wrong decision, put the plant in a place where it shouldn't be.

And so we feel it's very important. And the problems, from the way they're doing it now, is that it takes time.

And again, it takes time and it's very, very complicated.

It increases -- there is a large potential for mistakes, we feel.

And this really places the operator under a lot of stress to do it right.

The solution that we're looking at is to -- is this MIDAS system, which will maintain a work document control log, maintain the status log, provide all sorts of querries and sort capabilities.

It will provide the component safety and technical information.

But most importantly, we feel, is that it integrates the plant components functionally.

That is, it doesn't just allow single searches or flat searches, as they're called, through computer files.

It allows the operator to ask, for example, if I dig out this valve, what other components in the plant will be affected?

What other components am I directly

affecting?

What is within the hierarchy of that particular component or the cross-hierarchies, if you will?

And the system that we've come up with is a MIDAS system, called master information and data acquisition system.

It was coined by one of the operators. The operators at FFTF have had a very strong hand in the design of this system.

And anybody that tries to design a system like this that doesn't get the operators in it is headed for a bad fall.

The operators know how that plant works. They know what's needed.

They know what they will use, and they know where their problems are.

They've been really valuable in helping design the system.

I've got a schematic, a rough schematic, of the system, which I'll show here. This is kind of conceptual.

I've got a couple others that are following. They get a little more detailed, which I will skim through because they are involved.

Basically, the operator works through screens, video screens; and he operates on this MIDAS data base.

The data base has access to equipment groups, and presently in these groups now we have on the order of fifty-one thousand components.

There are equipment in the lists, valves, electrical load lists and dampers. It's a very large list.

We also have included in data base document information; that is, the reproduction of work control logs and generation of the work control logs.

It provides information for these kinds of documents.

There are a number of documents in the plant that we work with.

And then it has the special lists, tech. spec, critical system and some of the shop lists where they have maintenance requirements.

And the data base actually ties these together.

It will say, for example, for a particular valve, what other pieces of equipment in these other data bases apply to that, are functionally related to that piece of equipment.

It will then go into this part and say, what tech. specs are related to that particular valve that I'm taking out?

During this mode of operation of the plant, what tech. spec am I going to be possibly jeopardizing?

Critical systems, applicability and shop are the same way.

All these things are tied together, and that's what we feel is the really unique part of this.

MR. CARBON: Steve, this sounds very good to me.

Are you deep enough in it to know -- does the operator still get so many answers?

He asks about a valve. Can there be so many answers that it takes him forever to look through them all?

MR. SEEMAN: That's a real good question, and what we tried to do in the beginning in the design of this is to give him enough querry capability so that he could limit his answers.

For example, he doesn't ask, tell me all the valves. That would be suicidal in a tremendous output.

But he said instead, tell me all of the valves that are related to this tech. spec, that are in this particular cell of the reactor.

And so that automatically cuts down the list to, say, one or two or a very small number.

So he has that capability. And that's the capability of one of the standard large data base management systems that are available now through the soft core vendors.

It's very powerful that way. The querry capability -- they enjoy using it, too, because they don't have to sort through.

An operation that takes sixty seconds,for example, with this machine would take a man sorting at the reactor -- would take him on the order of eighteen hours just to get the same kind of information out, related information.

It's tremendous, the querry capability.

MR. CARBON: Is this used elsewhere at any of the LMFBR installations?

MR. SEEMAN: Not to my knowledge. I think that some of the LMFBR people have the flat list.

That is, they have this portion of it that they can go in and querry.

They can ask for a particular name of a damper and give me all the information on that; an electrical load list, give me all of the electrical equipment associated with this breaker box; give me this particular valve.

They can go through these sort lists. I'm sure they can do that.

MR. LIPINSKI: What did FFTF do to establish the status of all of the safety components in the plant? Is there a hard-wired system?

Let me give you a case at hand. TMI-1 has a turbine-driven feed-water pump, and it's got a pneumatic operator on it.

Did you mean to say that pneumatic operator can wind on the hand wheel, shove the diaphragm down all the way and that guarantees that the valve will not lift?

When you're through with maintenance on that operation, you rely on the operator to restore that hand wheel back so the diaphragm can lift if that valve's to be called on.

There is no indicator or anything. It's strictly an administrative control, that when he's through he restores that valve to service.

Now, are you in a similar condition, or do you have your systems reinforced through electrical interlocking?

MR. SEEMAN: I can't speak for the FFTF part of the house.

But I can say I know that a lot of the maintenance procedures is done by procedures administratively where you have re-tests, where you go back in and you assign re-tests.

I do not know which safety systems or which safety-related components, if you will, are hard-wired so that you can tell which way they are.

MR. LIPINSKI: Because your data base is going to be depended upon somebody that's done a maintenance operation and said, yes, I've restored it to service, and then he goes down to your data base and then you believe it.

MR. SEEMAN: That's correct. I would like to have taken a bigger step in designing this.

However, we were limited in scope. We limited ourselves in the context that we didn't want to get a specific job done to a demonstration of this functional relationship concept.

So in order to accomplish that in the time that we had, we did limit our scope.

You can expand on this system just immensely.

Everybody that's seen it has said, gee, why don't you do this? Why don't you have this particular aspect of it?

Why don't you throw it up so that it tells you when to do a maintenance?

And you can. The answer is, you can. Not very easily. It takes time and money. Software costs money.

MR. SIEGEL: Does the system so organized have a pattern which, in a sense, prevents the operator from asking foolish questions which will dump, you know, sixty thousand pieces of information on him?

MR. SEEMAN: No, he can make large dumps. He's limited from making large dumps in front of him.

He can make large dumps down at the main host computer, which is down in the Federal Building in Richmond, for those of you who have been there.

We're connected to a large UNIVAC 1100 downtown.

He can ask silly questions to it, and it won't give him the answer.

It will tell him, no such component exists, for example, if he's asking for a nonsensical component.

MR. SIEGEL: The system advises him on what's a more sensible question?

MR. SEEMAN: No, it says that I don't have the answer to that question.

Or it says that that component does not exist or that -- I don't understand.

If you ask him something that -- if you ask it the wrong valve number, for example, it will give you the wrong valve.

It can only reproduce that, that which you give it.

MR. SIEGEL: Well, you gave an example of how the operator gets a very limited and, thereby, useful set of output data when he asks the right question.

I'm still troubled whether -- how smart does the operator have to be in order to always ask right questions? Can he ask foolish questions?

MR. SEEMAN: Okay. I understand. Yes, he can, and I've seen it. I've done it myself playing with it.

You ask for something that's too broad in scope.

I asked for the components -- to list the equipment in System 81, that's the primary heat transport system; and I got tremendous dumps coming back out.

You just cancel it, and you say well, I did a dumb thing there. Let me see if I can limit that.

Let me see the valves in this particular loop, which is System 81-A, for example.

And you start to get another large one, and then you start thinking well, maybe I shouldn't do that. I need to ask for a particular valve in a cell.

You can cancel those. Those are pretty much detailed.

MR. LIPINSKI: Does he have a thesaurus that he can work with?

Because the computer rejects a request if the comma, the dash or the plural isn't in the right place because it won't be in its vocabulary.

So how does the operator know precisely how to put in statements with precision that the computer recognizes?

MR. SEEMAN: It can get around that by saying, for example, give me all of the equipment in Cell 51 that starts with "V", valve numbers. I don't have to know that.

And he then prints it out, and he moves a pointer to the particular one he wants, pushes a button; and it goes and pulls that one out.

That's a physical man-machine interface problem right there.

MR. LIPINSKI: Because one of the biggest problems with computers is, the thing is very precise.

And if you don't get to it precisely the way it has it stored, it will say, I don't have it, just because you didn't put an "S" behind it.

MR. SEEMAN: We had an operator that did not like the system.

And that's not uncommon, for operators not to like that kind of thing, mainly if they can't type.

And his comment after we brought it up -- we brought up about half of this system.

His comment was, gee, I used it this morning and it didn't even have the valve in it that I wanted. Now, what good is it for me?

And so the guy that was demonstrating said, what was the valve number? And the guy said V-2224.

And they put it up and sure enough, it said, not in file.

And the guy said gee, what cell was it in? And he said 151 on the lower level.

He said, give me all the valves on 151, and sure enough, it's not there.

And the guy was kind of looking over his shoulder, and he said there it is; it's 442, not 224.

And that's the kind of thing that we're after.

That's the kind of mistake that does lead to large problems.

We think this is going to do it.

MR. SIEGEL: Can the operator ask a question, are all valves required for operation in the right position?

MR. SEEMAN: No. It does not develop line-ups.

That's something that would be really nice to do on a system like that, but it's expensive.

You can do it. You can do all kinds of things like that.

MR. LIPINSKI: If the operators are making entries into this system after maintenance operations, is [illegible] storing the information on the current condition of the plant or not?

MR. SEEMAN: It's storing conditions as to its readiness.

It can be operable; it's not down. It's a one or a zero kind of thing.

It's not saying, the valve is lined up this particular way.

MR. LIPINSKI: You're not using the operator input to accumulate and store that information?

MR. SEEMAN: No. It could be.

MR. CARBON: I may be asking the same question Walt just did.

As soon as you undertake some action based on the operator having gotten information, is that action immediately fed into this system so that if a second operator comes along and asks about a valve in the same cell, will his answer reflect the fact that ten minutes ago they took some other --

MR. SEEMAN: That's correct. And where that happens is over in this portion right here.

In the work release form when the person -- the craftsman that is allowed -- when he's allowed to go work on that valve, he's given this form from the computer.

And he goes out then. And as it's fed into the computer, it's automatically -- the data base is updated.

And there's a -- in this particular valve, it will -- it's a taking-out status. So it's on line, if you will. It knows where it is.

The next three view-graphs I don't think I'll go through in detail.

Those of you that want to look at them that like software will enjoy them.

Basically, it just shows that the software is built in three parts, a transactions portion and the two data banks; a work control log, which is your paperwork, the documentation for what you're getting; and the MIDAS index, which is the fifty-one thousand pieces of equipment.

The one important part of that that I would like to point out, and that is within the index -- it gets more complicated as you go down into lower levels.

But within this MIDAS index, which was one of those three, you have all of these pieces of equipment, the instruments, the electrical load lists, the dampers.

But you also have this thing called FEG, and this is a system called functional equipment groups.

And that is the hierarchy. This is where that's stored and is brought together.

And that's something that's fairly tedious where you have to go down to the P and ID level and say, where does this piece of equipment belong on my overall hierarchy of the plant?

And that also gives you some kind of

standardization for your plant, too.

So everybody starts saying that's part of this system; that's part of this system.

And they're starting to use now the same nomenclature.

Right now we are set up with a keyboard in the plant.

There are two keyboards in the plant. One is right at the desk of the person that releases the work to the plant, hooked up to a CRT.

He's got a printer available to him, it's an intelligent terminal, so it has -- it's this controller type arrangement.

And it does feed with a hard line down to our controller building down in downtown Richmond where we're hooked up to our 1100 UNIVAC.

That's worked out pretty well. We've had twenty-four-hour-a-day service, and they've kept good availability for us.

Just a quicky on our milestones. We have, I would say, over half of the system in place right now and operational.

The operators can now use the flat lists and some of the functional relationships of the equipment.

They're getting used to the system. They're essentially, if you will, playing with it, getting over their initial fears of it, seeing what it can do, developing their skills with the system.

And it is helping them. It's helping them do their equipment searches.

We do not have the work release portion to it now, the documentation portion of it.

And that -- our software is on schedule, and that will be brought up before the end of the year.

That system has gone very well. I'm very pleased with it.

Some future items, depending on funding, we're going to be going in and determining some of the safety enhancements.

And this is very difficult because FFTF is a plant whose status is changing.

It's going from a cold plant to a start-up plant and then to a full-power plant.

So we're getting different ranges of information.

But there are ways we can do this, and mainly they're going to be done with operator interviews and trying to catch some of the mistakes that they are making and maybe have made in the past that have been bought.

In the future we're going to be looking at enlargements to this.

We really want to be looking at what would this plant look like on a very large plant with a balance of plants, steam generators and the whole works and perhaps working at developing some artificial intelligence for such a system.

This would be a system that would say to the operator, no, you can't do that; your valve alignment is wrong, or some of the things that Walt's been talking about.

I'm going to briefly touch on this data display system that we're setting up.

It's separate from MIDAS. MIDAS is being put up on the plant. There are tools in place for it.

But basically we're using this thing, this particular tool, to develop and test the advance systems that we're going to be looking at.

It is our testing methodology, if you will. We need to provide an interface for evaluating some of the man-machine interface methodologies that we're going to be developing and also for providing some of the advance projects, such as diagnostics, simulations and transition control system, that I'll be talking about. We've configured that system.

I'll skip over the next one and go on to this figure here that shows what we have now.

We have a color graphic system that's hooked up to a DEC VAX, which is a fairly potent CPU.

We're going to be running tapes on this VAX in the future.

This is something we're trying to develop from the operator training simulator.

It's the FFTF training simulator, and what we envision is that we will have a methodology that uses reactor data.

For example, a diagnostic system that looks at some kind of reactor situation and gives the operator a prompt and then says, that's what's happening and diagnoses something.

And what we want to do is to run a situation with the simulator and get the operator response to that condition and then run the same situation with the operator watching the video screen to see if he gets better prompts, see if he can better understand what's going on, see if it would actually help him and reduce the risk in that particular type of accident.

Early recognition, if you will, so that we can demonstrate some kind of risk reduction.

A little status information. Basically we've developed the methods, and the machine is in place and we're bringing it up.

Final item of development work that I want to talk about is this diagnostic/operational system that we've called our transition control system.

This is kind of complicated. It runs a little counter to what some of the present thinking is.

And we think it's got some merit, and we're testing it on a smaller system.

We'd like to define and demonstrate a system for normal and off-normal transition reactor control.

And by that I mean it does not necessarily tell the operator or go into high-level diagnostics as to what's going on.

It determines if a piece of equipment has failed.

And then it says, based on that, here is a state that you can go to that's safe.

That means you don't have to describe all of the unsafe states or all of the possible things that can go wrong.

All you have to decide is, what's left to go to?

Where can you go within the matrix of acceptable states?

And we think that's quite a bit different. The premise, then, is that the status of the plant equipment directly determines the possible acceptable states.

This is what operators go through in their head.

When they see something happening, they say, well, what's down? What happened? What piece of equipment failed? Where do I have to go to bypass that problem?

They need to know what the acceptable state is to put the plant in.

Our approach is that we're going to formalize the hierarchy of acceptable states.

Now, this is kind of done already with procedures.

If you go through the way the operator actually runs the plant, he has a hierarchy, whether it's written explicitly or not.

He's got one in his mind of which state is safer than which state is safer than which state.

So he can go down these lists. It also can be pulled out of the procedures as to -- you know, and laid out in some kind of hierarchy.

MR. LIPINSKI: How will the system sense the state of the plant, through direct measurements or operator entries?

MR. SEEMAN: It would have to be through direct measurements in this case.

Now, you can get grandiose where you can look at the whole plant scale.

Or you can say, well, I'm going to do it on a small piece of the plant, maybe an important piece, if you will, maybe a shutdown system or something like that where it knows the state of the plant based on software redundancy, for example.

It doesn't necessarily have a micro switch that tells you a valve is shut or opened.

But it looks at the flow downstream and looks at the temperature.

It does calculations. Software redundant type things.

MR. LIPINSKI: A lot of safety systems are standing in standby status, waiting to be called on.

You won't necessarily have flows?

MR. SEEMAN: That's correct, but then you use something where it has power to it or it's not the proper position, line-up or what have you.

MR. LIPINSKI: Where it's in standby, you'd have to go there and measure the status of all the breakers, the valve, whatever is involved that says that system will operate when you want it to.

If you don't, then you can't get that --

MR. SEEMAN: That's correct. And that -- you look ahead to an awesome task if that indeed is going to be done plant-wide because it would have to be done for all pieces of equipment.

Once the off-normal state is realized, which is no trivial matter, is what you're saying too, then you can find a transition matrix which is -- which describes the changes required to return to an acceptable state.

I can tell you briefly where we are on this.

We've gone through quite a review of work being done both internationally and nationally and came up with the methodology for doing this, the mathematics, if you will, for determining what acceptable state to go to based on equipment failure.

And it gets fairly involved. And as a demonstration, we've chosen a system on FFTF, turns out to be a system on one of the main transport pumps.

And this system is small, but it's very complicated and has all the features we want in it to be testing this kind of methodology.

It's got alarms to the control room. It's got things that require that the operator send a watch stander down to see what's wrong.

It's got valves in it. It's got all of the features that we need to be testing, and this is the system we'll test it on.

We're mocking this up. We're not running it at the plant.

We don't intend to do that until it's been demonstrated.

That's true in all these methodologies, is that we want to show that indeed it will lead to reduction risk before putting it on the plant.

MR. MARK: You said you had explored what you could find in other countries or in other situations.

Did you find anything much?

MR. SEEMAN: Sure. The people in other countries are generally right now looking at cost consequence diagrams, falt trees.

And you have a high-level plant symptom when you have several things going wrong or several symptoms happening at a high point level, temperature flows, power, whatever.

And these go back to these fault trees and down to a root cause.

And they say, aha, I know what's wrong; but they don't tell us where to go.

It's a very complicated way of -- I feel, of finding out what's wrong.

And that implies, you know, all of the accidents, all of the possible sequences.

MR. MARK: Still you must include a certain amount of that same line of thought in this system.

MR. SEEMAN: Well, I don't think we'd even use that because building those fault trees implies, you know, everything that can go wrong with the plant.

MR. MARK: True.

MR. SEEMAN: And that's a pretty impossible job.

MR. MARK: But you say your system will enunciate Pipe 117-F or a pump has just broken down.

However, what may have broken down is the flow in some line.

And it could be because the pump isn't working or there is a piece of bric-a-brac in the line.

And you have to be prepared to observe or comment on such a situation, which is not too different from pump failure or the symptoms that you say the other people are starting with.

MR. SEEMAN: That's correct. This methodology is based more on equipment status, more directly on equipment status.

And this is not to say that this is the only aid that the operator would have.

Some of the future plans that we're looking forward to, with MIDAS, I think I mentioned those at the end of the MIDAS presentation.

The data display system, we're just looking forward to getting some of the operations system put onto it, some of this transition control system put onto it so that we can start at least verifying to ourselves that we're getting the kind of results that we want.

We're then going to try to do some advance systems where we perhaps look at this transition control system on multiple systems, start increasing the complexity and making sure that it extrapolates.

In the end, then, we're -- later we'll be looking at advance techniques, such as adaptive learning and pattern recognition.

But the software field has gone so fast and there are so many things going on that it's very difficult to even keep up with it.

This is a good example, adaptive learning, where you have programmed your machine to recognize patterns or signatures or events and to tell you if anything is out of the normal range.

And when it -- when something, when an event, happens that it feels is out of the normal range but you feel is, because you have just re-loaded a new assembly in this particular area, you tell it that.

And it re-programs itself to accept that within it. It's essentially learning. They have them now for playing chess.

That's all I have for my formal presentation. I'd be happy to answer any questions that you people might have.

MR. CARBON: Frank, I have a question that doesn't directly apply.

Are Jerry Griffith and the DOE -- did they express interest in this?

MR. GAVIGAN: Right, we work with -- at headquarters, our man who follows this works directly with those people.

And the work is being done at DAS at Combustion Engineering.

They're very interested in this. In fact, we've had two contacts from utilities who are interested in the MIDAS system in particular because they say it's a real down-to-earth way of helping the operator, give him an extension to his memory so he doesn't have to go through all that process that he mentioned earlier with all the lists, plant status and so on.

MR. CARBON: Are any of the people sufficiently interested that it looks like they might apply?

MR. GAVIGAN: Yes. I don't -- did you ever get a contact from a utility who was interested in getting that information from you?

MR. SEEMAN: Pete's talked to me, but mainly our contact so far has been the CRBR. CRBR people are interested.

I haven't had a utility talk to us directly.

MR. GAVIGAN: There seems to be a reluctance when they do talk about changing their plant, is what it is, even though they see this is a useful thing.

They don't like to go through the process of changing what they have and going through an NRC review. It always gives them the shivers.

MR. MARK: Which do they find more frightening, changing the plant or talking to the NRC?

MR. GAVIGAN: It's not in the talking to the NRC.

It's the lack of progress when they talk to the NRC. That's what concerns them.

MR. CARBON: We're a little behind schedule, but why don't we take a short break, about ten minutes or something?

(Brief recess had in proceedings.)

MR. FERGUSON: The next speaker this morning is Rico Simonelli.

He's going to be talking about two subjects, one, the national digital reactor system that ARD did for the base system; and secondly, talking about the reliability analysis that was done for the shutdown heat removal system for the CDS study that was completed recently.

MR. SIMONELLI: I'm going to cover the digital reactor shutdown system right now.

The basis for going to digital is, if you remember back -- I guess it's RATC-16 which originally started some discussion on diversity, IEEE-603.

And it's been a principle now, the FFTF; and I'm not quite sure whether we do have diverse shutdown systems.

In design improvements, inherently a digital is a design improvement, as you know working with your calculators at your desks.

And design improvements based on findings started way back in 1973 in the air force document where things like that were recommended.

TMI findings, the same; we should make improvements in state of the art.

And certainly, a digital system is a state of the art. No question about that.

It enhances reliability, availabilty. It uses less power. It has built-in test capabilities.

We can self-diagnose very fast on line with it.

So, therefore, it will enhance the plant protection system because we can make very quick calculations and very complex functions; and it's very exact.

If anybody wants to interrupt with any particular questions, feel free to do so.

MR. IGNE: Are these slides in this book?

MR. LIPINSKI: Yes, they're not in sequence.

MR. SIMONELLI: At the present time we're attempting to establish performance requirements.

The big problem at this time is response time.

We'll probably do that, and we are doing it based on the duty cycle events of the plant.

So we end up with realistic numbers that would satisfy both the core people with reactivity events and how quickly the micro-processor can respond to anything

Now, in the past, all the marginals, even in light water plants and I guess Clinch River FFTF, also, we have logic comparator units, instrumentation and various things.

And we're all accustomed to parameters like fold-over, common-mold rejection, fall time, rise time, repeatability problems.

This entire lingo does not apply to a digital system.

So we have to specify new words, probably random access time, things of this nature.

And we have to get very exact in this. So we will have to specify exactly what the digital system can do.

And I think it will end up being a lot of computer jargon, things that you -- protocol, for example, access time, as I mentioned.

And that's our big problem right now or our activity at this present time.

MR. CARBON: Excuse me. This is completely out of my field.

Would you mind taking a second or two and express what the main thrust of -- what's the significance of the digital system? I'm not with you yet.

MR. SIMONELLI: Okay. In order to have two, what we call, really diverse systems, shutdown systems, it would be nice if we could have one analogue and one digital system. That's the real reason for it.

Basically, digital modules are easier to buy right now.

Probably in ten years you won't find a lot of things in our analogue.

If you have a calculator, you can see that. And that's the real reason.

It is more reliable. It's faster testing. Many things I can say for it.

It has an excellent success record in the defense department and aerospace.

So it's not that we're just using it, you know, just right now.

I'll probably get into it later, but the water reactor division are almost close to licensing the digital shutdown system.

MR. SIEGEL: This is only for the information flow from the instruments to whatever decision-making --

MR. SIMONELLI: Also, we'd like to have it for the control system.

We would like to have it initiate the control system, although that's not plant protection.

We see that we can enhance the reliability of the control system itself, notwithstanding what we were doing.

MR. SIEGEL: In addition to the shutdown system.

MR. SIMONELLI: In addition to it.

MR. LIPINSKI: Let's review FFTF and Clinch River.

Doesn't FFTF have a relay and a solid state system for diversity?

MR. SIMONELLI: Good grief, I don't think they have relays. I don't know.

MR. LIPINSKI: I thought that both FFTF and Clinch River had a --

MR. SIMONELLI: Clinch River has. Clinch River has discreet components on one system and integrated circuits on the other.

That's as diverse as we get. Relays will take you backwards.

At one time it was discussed, if I remember my Clinch River days, that we should have possibly relays on one side and solid state on the other.

Relays are so unreliable as compared to solid state it seemed like a backward attempt.

And I'm sure FFTF is all solid state. I'd be surprised if they had -- in the PPS now, understand.

MR. LIPINSKI: Arkansas has been licensed to have two channels of information, the DNBR and the kilowatts per foot handled by computers, and it's now operational.

MR. SIMONELLI: I was thinking more of a plant protection system.

MR. LIPINSKI: Westinghouse's integrated protection system is a computerized total system.

MR. GAVIGAN: Doesn't Arkansas have a CE-80 system?

MR. LIPINSKI: No, just -- there are about eight channels that still go through the comparator tripping.

But then to do the calculations for the kilowatts per foot and DNBR, they use a four-computer system and digest the input information and then come out with the trip circuits.

So effectively, it replaced two channels that they had, I believe, on St. Louis with analogue equipment with digital equipment.

And that did get licensed.

MR. SIMONELLI: The transient analysis, which would be a starting point to determine our response time based on these two key events, of course, loss of flow events and the reactivity, we're thinking somewhere around one hundred micro-seconds right now.

It seems to be a reasonable number we can meet at this time.

MR. SIEGEL: Seems to be a reasonable what?

MR. SIMONELLI: Number we can meet with a digital system.

And that seems to be in tune with -- in time to pick up all the responses.

MR. SIEGEL: Are these dedicated channels or are they multiplex?

MR. SIMONELLI: I'll get into that problem. I'm trying to follow these slides the way my boss had them presented.

This is light water situation we just talked about.

We don't have the resources to re-design an entire new system.

So if we can find or even evaluate an entire new system.

So what we try and do is get some benefit from existing designs.

And as I said, the Westinghouse water reactors division has one with a lot of -- an awful lot of information which we are looking at.

There's a lot of design information. And our job next will be to determine how we go from that to the large breeder reactor that seem to be the most cost-effective or which way to take a look at this thing.

So we don't want to re-invent -- really re-invent the wheel.

What we'd like to do is just take what's applicable to a large breeder reactor from the light water and work from that point on.

There might be some modifications and et cetera.

A little caution involved here because this is a system which Westinghouse, of course, wants to sell, has put a lot of development time into.

We have to be delicate in how we review their data and apply for our breeder reactor.

That's -- you know, it's another division.

MR. LIPINSKI: That was my question because you can get the published reports where all the proprietary pages are planning, and then you can't tell how the system works.

If you want to know what's in the system, then you have to get the proprietary version.

MR. SIMONELLI: We're working with them on that. There's a delicate balance.

We can sort of see how it works. We'd have to re-design for our channels, notwithstanding them, anyway.

This is the problem he just mentioned. If it is applicable to a large breeder reactor.

We have search issues to concern ourselves with.

Response time because of the -- there's a lot of software in the system.

Of course, that eats up time. And separation is going to be a problem.

We have testing-type problems, and here is our problem of multiplexing.

I don't know how we'll go along with that. I don't know NRC's feelings.

And I guess we will learn that from the

water reactors.

There is also problems with a single chip having many functions on it.

And you're concerned again about a single point of failure of activity.

So these are all the things we have to look at as we adapt from whatever system they have to one that's applicable to a large breeder.

And multiplex is going to be a -- I see it as difficulty selling NRC's, but that's a personal opinion.

MR. SIEGEL: It seems so closely coupled to the possibility of common-mode failure.

MR. SIMONELLI: Plus it's shared -- it's shared circuitry on a chip point of view.

So the benefit of multiplexing, which saves a lot of weight power and et cetera, we would still take more and more chips, I guess.

We could still get multi-redundant enough, but, you know, within some optimum design.

So basically, our task the rest of the time will be to establish a design basis for the large breeder.

We'll have to write programs adaptable to the large breeder.

We feel we will have enhanced protection functions.

There are many calculations that cannot be done on analogue circuitry that we infer from the data, really by inference, which will be calculated exactly on the new system.

Reliability considerations. Problems we mentioned.

We certainly can enhance reliability, but we have to be careful on separation, single-point chips and this sort of stuff.

Hardware calculations with a digital simply are really a piece of cake. There's no big difficulty.

Defense and aerospace do it all the time. We have scads of data on everything and microprocessors at this time.

Software reliability is a state of the art. It basically comes down to operator errors.

And it's a -- or programmer error, whatever we'd like to call it, or someone did not consider the right function as he coded his little program.

So software is a son of a gun. I think there are measures now in state of the art being considered, let's say, by TRW on software errors per instruction.

I think there's some ways you can play with that.

So there's a lot of work to do in this area.

Has anybody got any particular questions on the more general?

I'm going to move on to the heat removal.

MR. MARK: I believe you said that techniques of the sort you're looking at have been applied in the space program routinely.

Are the systems which they use there comparable in size to the one that you envisage here or not?

MR. SIMONELLI: Well, first of all, they don't have the so-called common-mode-type problem per se as we discussed it in the nuclear business. Their consequences aren't so serious.

They're working mostly in the computer area. They need a lot of computers.

They have more tightly packaged modules, more crowded.

So comparable-wise, we should have an easy time in a nice, mild, benign control room versus a typical aerospace environment.

However, they have more highly technical people running all phases of maintenance and operation that we may not have in the nuclear business.

MR. MARK: They don't worry about multiplexing and using a single chip for sixteen different --

MR. SIMONELLI: That's right, they don't, and they keep extra computers around.

Of course, multiprocessing is in a sense multi-redundancy.

And they have such a severe weight-type things, they can't go as wild in redundancy as we're able to do.

MR. SIEGEL: This may not be a question for you, but I'm curious.

On reactor shutdown system reliability, is this all we're going to hear, only the -- sort of the information flow rather than the actual mechanics of the shutdown system?

MR. GAVIGAN: Right, this is it. We can schedule more later, if you wish.

MR. SIEGEL: Did I hear that you were going to -- you are going to talk about self-actuated?

MR. GAVIGAN: That's single-system we're talking about.

MR. SIMONELLI: Let me say that we had -- on the PLBR before the CDS base --

MR. SIEGEL: I mean, this certainly isn't -- doesn't cover the total reliability issue on reactor shutdown systems.

MR. GAVIGAN: Right. We need more time, a couple more days, I would presume, to cover the whole program.

MR. LIPINSKI: You have a reliability goal that you're designing, too, do you not, for the overall system function?

This only represents the input information to the drives plus the function of the drives.

MR. GAVIGAN: Right, this doesn't cover the testing program or the program as run by CRBR.

MR. SIMONELLI: During the PLBR phase prior to CDS, we have gone through about six different architectures of typical digital systems.

And preliminary findings were with very small reliability differences between them.

And it came down to selecting which ones would have better separation, which ones were more suitable to your particular design and acceptance.

And there's a lot of problems involved, not to mention software.

But we've been doing this for quite some time now.

On the shutdown heat removal system, the purpose of having reliability assessments is obvious here.

We need some kind of an a priori method to see that our design will meet safety goals, if possible.

Reliable design, optimum, with various independents and diverse criteria, single failure criteria.

So these are the things we sort of look at. There is a criteria to develop a heat removal system.

The safety working group -- I guess this is an obvious statement.

The safety working group has determined that we need a highly reliable decay heat removal system.

And that, of course, is, you know, rather obvious.

They further recommended that we achieve this with two independent safety grade systems, each one of which is redundant within itself.

And furthermore, if we could get one of these not to depend on the primary heat transports.

And that's sort of our requirements on this particular thing.

On CDS, unfortunately, this design has

changed; but we'll give you what we have.

What evolved out of the system was two direct systems from the vessel, not requiring any power,sort of a natural draft system, and two systems off the intermediate.

Each one of the four systems can remove thirty-three percent of their decay heat.

And, of course, we have our normal system. These two would be the dedicated heat removal systems at the time.

This is in the process of change right now.

MR. SIEGEL: Which is in the process of change?

MR. SIMONELLI: I think we're going to two, which is the normal heat removal system all through the condenser; and the air HX, I think, will disappear.

Evaluating this particular system, basically we have two methods, I guess qualitative and quantitative.

Our qualitative approach to the reliability, of course, is to get the designers involved, thinking in reliability terms.

So that, I guess, involves working with them.

In this particular case here, this common-cause failure, the designers have checklists and, you know, many meetings we have with the design people to determine what kind of potential common-cause events should we design around?

And of course, we're reviewing the light water industry, various sodium loops, EBR-II and et cetera.

I'd like to stop here and emphasize from what I've heard previously that it looks like, from data -- a data need, everybody here has mentioned, where are you getting data?

You know, light waters, sort of lack of sodium

We're going to have to push for foreign data exchange, it looks like. That would be my recommendation right now.

Continuing on, we do failure modes and effects analysis.

We do have design engineers working together. We interface with the designs through the top document system design specifications.

We work with them in writing operational procedures.

We even do availability studies, not just reliability.

That is the plant up time, non-safety issues, make trade-offs, evaluate maintenance.

This control system here, management control, design reviews -- I wish I had a piece of chalk.

But if you have someone on a line function design, it would be nice if you had a parallel path beside them which is sort of the checks and balance against it.

This is very nicely pointed out in the Haig and Watson report out of the U.K.

And it shows some ways of minimizing single-point problems, whether it's a design error, operator error, et cetera.

It doesn't get rid of it, just minimizes it.

Our quantitative approach which we have a report on, we're to model the system.

In order to do that, you have to establish some sort of an assumption of what the hardware can do before some math guy gets there in his own world and models it.

So we assume -- we work with design, make assumptions on what the design can or cannot do, start a model, try to gather failure data, random, whatever we can find on common-cause, model the system.

We assess it various prediction methods, run computer codes, and eventually it yields us some kind of a system reliability.

On CDS we did it this particular way. We established fail-safe block diagrams, calculated the probability of what hardware contributes to each block necessary to get to a failure state.

Then we generated event trees. I'll show you some of these after.

Which in turn gives us an idea of how many times things will happen.

For instance, a re-fueling shutdown once a year, how many days and et cetera.

We pick those kinds of things up from the event trees.

Then multiplying them together, we end up with the system norm on reliability.

This is our design, which is no longer, I think, in force.

But from the decay heat removal curves, we establish these peak heat times, you know, zero to forty hours, et cetera.

And in each particular case we need certain heat sinks for success.

In this particular case we need three out of eight, in the rest of the plant, and for next phase, two out of eight, one out of eight, et cetera.

And this is from -- right from a four-loop hundred-percent power operation coming down.

This is a little more blow-up of some of the assumptions.

This is the re-fueling outage, one a year, thirty days.

These might be spurious scrams, various things like that.

Now, the forced outages, those where failures actually occur, we've come up with three different types.

Some take thirty days, seven days, one day and et cetera.

And this was determined based on the actual hardware involved to make these things fail.

And as we calculated those probabilities of failures, we've come up with this frequency per year.

Now, anything that's common-cause, which of course is a big problem over here that we know about, would be identified.

Those we don't, of course, are not in there. This is simple.

This shows four DRACS in parallel. This is four IRACS in parallel.

And this primary heat transport in the balance of the plant, really there's four heat transport loops here, four primaries with the balance of the plant.

This is to show you the paths from a very simplistic point of view.

So these are all the plants to remove the decay heat.

But the DRACS and the IRACS in the present design are capable of removing sufficient decay heat with one unit down, so it's an M minus one set-up on each of those.

Of course, one major system takes everything out.

I won't go through this whole thing but try to give you an example.

This is the -- if we need three out of eight for failure, this is six out of eight.

Three out of eight for success, this is six out of eight would be a failure situation.

Here is a simple one here. If we lose the reactor vessel and the guard vessel, that's one failure path.

If we lose four DRACS and two IRACS, of

course, we have a failure problem.

Or, you know, various events like that. Would anybody like me to go through another couple?

We have a report we can reference to show these paths.

Happens to be the way our computer calculation runs this thing.

That was the block diagram for failures. Here is the event type flow.

Eighty-five percent of the time we assume we'll be in a four-loop operation, fifteen percent of the time in a three-loop operation.

Amortizing these numbers times the probability of being in each one of these, here is the planned outage.

Here is a condenser failure; here is a primary heat transport failure, et cetera.

This multiplied times that gives us the frequency per year of a particular event involved.

If you'll notice down here, you know, the steam generator path, although this number is not particularly high, it dominates many of these things besides the -- of course, thirty-day shutdown, which is obvious.

And that was the results of the Phase 2 CDS study at the time.

Overall results, unreliability of the overall case, ten to the minus ten.

That's an enormous number, depending which side you look at it.

With sequential operation, we end up with a little lower number.

And, of course, if there is no natural circulation available, that means more force circulation. Hardware is involved.

Of course, the number lowers a bit to accommodate that.

We also had two sensitivity cases evaluated. Restricted DRACS operation, one where the two DRACS outside, opposite sides of the vessel, have to work and not any two.

And in another case where we could not lose the power to the diesel generator of the power flow on the same side as we lose a primary loop.

Or I may have that reversed.

So we've run many, many cases and come up with this series of numbers here.

And as far as the numbers, the interpretation of the numbers go, you know, we're not trying to push for exact numbers.

But apparently we've exceeded our goal on CDS with a considerable margin.

And we think we have some sufficient data in there, let's call it, to handle future design modification or maybe unforeseen things that may happen, whether error-wise and et cetera.

MR. GOLDEN: Do you think you have enough design margin so you can get rid of one of the two diverse systems?

MR. SIMONELLI: That's also being looked at. That's probably one of the reasons we've gone to the SIG ACS (phonetics).

MR. SIEGEL: Did you say that the IRACS has been removed?

MR. SIMONELLI: The next phase of design. It's about to disappear

And we will have the normal heat transport systems with the SIG ACS, very similar to Clinch River except they don't have the similar DRACS that we have.

This is the domination -- dominating number, as I mentioned before on the leaks in the steam generator.

I suppose the water people have the same problem.

Loss of off-site power really is a big

contributor.

It doesn't stay down long, but we end up with losing power.

We go to a diesel; they fail a lot, also. So this is one of the big paths of unreliability.

The reason I think we got such a good number is, the DRACS is very close to the heat source.

And by natural circulation and et cetera, not depending on a lot of active components, is the reason why I think we got one of the higher numbers or the high numbers we've gotten.

Well, we're not done, by any means; so I should say, we need a lot more work.

One is the constant review of the data base, including light water reactors.

But, you know, the unfortunate thing of that is, we'd really like to have a little more sodium experience.

And, you know, we have EBR-II. We have test loops running all the time.

But it's not really a full-scale plant; and, of course, FFTF is going to help us with this tremendously.

We're collecting data from them now and dumping it into a reliability data base at Oak Ridge.

And we're going to try to close the gap here between some of the predictions in the demonstrator.

I don't know how to do that because we don't have a plant to demonstrate, at least at this time.

But maybe from small hardware points of view. We're trying to narrow the efforts we have.

MR. SIEGEL: Is the DRAC system always on line?

MR. SIMONELLI: Only comes on when required. Is that right, Jim? Only comes on based on certain --

MR. HARTUNG: It normally operates at a very small flow to keep the circulation and temperatures in position.

And when they want to come on, they open the dampers, and it increases the flow.

MR. SIEGEL: Dampers have to be opened.

MR. HARTUNG: Yes.

MR. GOLDEN: That's easy to do, by the way. We have a similar system in EBR-II.

And it's a spring-loaded affair where, if we lose site power, the dampers open automatically.

MR. SIMONELLI: We need to get into some kind of testing, develop a test program.

Of course, that's such an enormous task, we would have to prioritize the many things involved, which I think we've done with TMC.

We need to verify and get better analytical methods because of uncertainty in data.

Common-cause failure, you heard it's a real problem on its own.

We'd like to expand the shutdown heat removal model to include maintenance and repair time.

I don't know what the advantage would be at this time, how much gain or loss we'd have on it; but we'd certainly have to look at it.

And, of course, this is the real problem here, evaluating, you know, the human factors, the man-machine problem.

And, of course, accommodating common-cause failures.

Right now, if we can identify one, we design it out or make the machines, of course, less vulnerable to them.

But this is going to be a big problem as far as perfections.

Since we're in a probabilistic field, maybe that's not too bad.

We have reports that we can reference to, if anybody needs any. Any particular questions?

MR. CARBON: Once again, what sort of changes are taking place?

MR. SIMONELLI: CDS design will go to a SIG ACS, which is the hardening of the normal heat removal path with an air blower and et cetera.

Probably have to get protected water storage in the back, and the IRACS will be gone. DRACS will be maintained.

That is the Phase 3 we're into now, and I don't know if all the reports are out. But that's what we're looking at right now.

MR. SIEGEL: Is that regarded as a safer system or a cheaper system?

MR. SIMONELLI: I don't know if it's cheaper, at least right now.

It would seem that we were trying to get two completely different heat removal paths strictly from a diversity point, for one thing.

And I don't know all the reasons. Some people think it's cheaper, some not.

Until we get all the reports on it --

MR. GAVIGAN: It's a system that's cheaper and still meets the reliability requirements of the safety system.

It's part of the cost reduction effort.

MR. SIMONELLI: See, it's also testable. It's a normal heat path.

So one can say, there's advantages, and I suppose others might say disadvantages.

It's the most different system from DRACS, but it's also furthest away from the heat source.

Well, gentlemen, that's all I have.

MR. SINGER: We've now finished a discussion of all the efforts we planned to discuss under LOA-1.

I'll now introduce the work which we defined as falling under LOA-2.

And in this area, basically looking at fault operation of a reactor plant system, sub-systems in the plant.

And we're really trying to understand the faulted behavior of the system and understand it sufficiently so that we can end up with a design or at least understanding so that the plant will respond in a benign way, in other words, with limited damage, to various faulted conditions in the plant.

The basic objective of this work is to understand the inherent phenomena which occur in the plant as well as the -- understand what types of engineered systems can also operate in the self-actuated mode or also inherently to limit damage which will occur, which could occur, if an accident actually is initiated.

The -- we've established for this particular activity the success criteria which we are aiming for.

And we would like to be able to end up with a design in which, if accidents are initiated, the accidents will be terminated with limited core damage, as defined by -- for different types of accidents.

For example, if there is any whole-core initiator, such as a seismic event or a loss of electrical power or some event similar to that, we would like to have no clad melting at all.

We want to prohibit any contact, physical contact, between molten fuel and coolant.

And for any local faults which may occur, such as perhaps internal sub-assembly blockages or the like, we want to make sure that event remains a local event and does not lead to gross coolant boiling in a reactor.

So these are our success criteria we've established.

They may change as we get into the -- understanding some of the inherent behaviors of this system, as well as some of the limitations and capabilities of the self-actuated systems we're looking at.

But at present, those are our goals and are indeed directed toward satisfying those success criteria.

The three areas where the research is directed toward are faulted behavior of the shutdown system, faulted behavior of the shutdown heat removal system, and looking at local faults, which essentially is faulted behavior of the fuel system.

So by faulted events, we're really talking about events which occur somewhat beyond a design basis; in other words, something that has actually failed in the plant or in the rack and core.

And we'd like to be able to confirm that either the inherent response of the reactor system is such that the damage is limited or that we can design inherently activated systems which can reach the same goal.

We have a number of capabilities existing in an LMFBR which are indicated here.

We can -- we have basically margins which are available in the design.

These plants aren't designed to operate right up to the limits of behavior.

So they can take certain transient

capabilities without getting us into difficulty.

There are a number of inherent capabilities of the plant, such as natural circulation, cooling, various types of inherent activity, feedback mechanisms, which also limit damage and also terminate accidents.

And, of course, we can design certain types of self-actuated systems, engineered safeguards in the plant, such as a self-actuated reactor shutdown system, which will be discussed in more detail ter,to terminate accidents.

If we meet our success criteria that I described in this area, we essentially, then, eliminate core coolability as an issue completely in this regard.

What I'd like to do is just very briefly go over the scope of the program in these three areas, reactor shutdown system, fault accommodation, shutdown heat removal system and local faults.

And then we'll have somewhat more detailed presentations after lunch on individual areas within each of these three areas.

The general objective of the first task in reactor shutdown system fault accommodation is, again, a motherhood statement.

We like to demonstrate that we can accommodate shutdown system faults with a high probability.

The main approach we're taking here, now we're assuming that there's been some sort of fault in the normal reactor shutdown systems, whether it's single system or primary plus a secondary system.

It is by the use of either a self-actuated system or by degraded mode operation of their -- your normal shutdown system.

Perhaps it shows an insertion of not the full number of control rods.

The reactor also can be shut down by inherent fuel and absorber motions.

As an example, core flowering during accidents, essentially mechanism which tend to expand the core, to expand the non-activated control rod systems into the core to add negative reactivity, things of this sort, as well as there has been some work done on annular fuel systems, also.

And, of course, a very obvious important part of this is that the basic structures are -- have necessary integrity to withstand all types of accident initiators.

The main program task in this area is the design and study of self-actuated shutdown system.

And that will be discussed in some detail later today.

I'll just hit on some of the general objectives of the system that we've directed the research toward.

And basically, we'd like to come up with a -- again, the word is cost-effective self-actuated shutdown system.

And we'd like it to be sufficiently diverse from the normal shutdown systems of a plant so that we could at least claim that common-mode failures won't take out both our self-actuated system at the same time it takes out the normal shutdown systems.

The work is really not -- is directed toward providing the technology and the demonstration of the technology's capabilities so that designers will have this option available.

The probjects, then, of course, are free to use or reject it as they decide, from their prominent point of view.

The areas we've gone to, we've looked at a number of different types of shutdown systems, many different types of concepts.

And we've basically focused the program on several types of -- on several limited types of latching systems at this point.

They both have in common electromagnetic latches.

The difference is in how these electromagnetic latches are triggered.

The first one is a Curie-point system in which the electromagnetic latch is essentially de-latched simply by heating of the magnet itself up to Curie-point temperature, where it loses the magnetic flux and then drops the control rod or safety rod, as you wish, into the reactor core.

The other switch, de-latching device, is a thermionic switch in which we have an electrical circuit in which basically a sensor increases in temperature as exposed to the primary coolant, simply changes electrical characteristics of the circuit, which then causes a loss of power to the electromagnetic latch and then causes the de-latch.

We're going through a number of testing programs right now to examine these concepts.

I've indicated here both in-pile and out-of-pile testing.

The electromagnetic coil is obviously a very key item in this whole development.

And I'll just leave it at that and let the discussion or the detail of that R and D discussion on the self-actuated system later.

In shutdown heat removal fault accommodation--

MR. CARBON: Excuse me. Can I raise a couple questions?

MR. SINGER: Yes. Should I go back to the other slide?

MR. CARBON: Please. The Curie-point electromagnet and the thermionic switch electromagnet, are they from the same system?

MR. SINGER: I'm not sure I know what you mean.

These would be envisioned as a completely separate shutdown system.

Completely separate from the normal primary/secondary or normal shutdown system.

MR. CARBON: And you would have two latch systems on this shutdown system --

MR. SINGER: There would be a completely separate shutdown system, who could be triggered or de-latched either by -- again it's a designer option.

Could be de-latched by PPS actions, since it is an electromagnetic device.

But it also could be de-latched simply by increasing the temperature in the reactor core, the primary sodium temperature.

And the question is how to convert that increasing sodium temperature to something which inherently de-latches the safety rods.

MR. CARBON: And so you have two latch systems for -- they being parallel.

MR. SINGER: Well, again, we're just developing the technology; and a designer would choose one or the other.

And we're not sure -- we can't make a final decision right now which one of these two systems would be the best.

I mean, it's -- most of the work right now is in this program here, the Curie point.

But there is a certain obvious risk that we may find out may not be the best.

And so there's efforts going on in alternate types of de-latching systems.

MR. CARBON: How far along are you on that system?

MR. SINGER: The Curie-point system? I'd just as soon defer that question to Bob Tupper from Westinghouse, actually doing the work.

There's been -- the work's been going on for several years.

I don't know if you want to answer now or --

MR. TUPPER: I'd rather do that later.

MR. SIEGEL: Do the French have one?

MR. SINGER: Yes, they have one they're testing right now in Phoenix that's a Curie -- it's -- they're developing a Curie-point de-latching device.

They're testing the whole system in Phoenix but not the Curie-point de-latching aspect of it.

They have the electromagnetic latch, the whole system all set up.

They just have not designed the magnet. They have a Curie point low enough to give the trigger.

They're just looking at basically the performance of this system in their plant.

And they are, at this moment in time, definitely planning to have a -- design the super-Phoenix 1.

They have three shutdown systems, a primary, a secondary and a self-actuated system.

And in super-Phoenix 2, their design shows a primary system plus only a self-actuated secondary system.

MR. GAVIGAN: They intend to take out their existing secondary system that they have in super-Phoenix 1 and replace it totally with the SASS for super-Phoenix 2.

MR. SIEGEL: Is our program closely aware of theirs?

MR. GAVIGAN: Bob, are we -- it's difficult to get information.

MR. TUPPER: It's very difficult. We know they're working on Curie point material. We don't know where.

As I said, it's hard to get information.

MR. FOX: We talked to them several years ago, and we knew they were going with an all-iron alloy, for example, for their Curie point, which is a much higher temperature than ours.

But we're doing the best we can.

MR. SINGER: It's a concept the French have obviously chosen, and they're going with it in their plants.

But they still are in the process of testing its -- really, its reliability and performance other than the Curie-point performance in Phoenix right now.

MR. LIPINSKI: You've listed electromagnet up there.

At one time it was thought that this could be done with permanent magnets but you could not find permanent magnet material with the right temperature for the Curie point.

MR. SINGER: I'm not sure -- was that the problem, Bob?

MR. TUPPER: It's a problem of holding strength and operator flexibility.

MR. SINGER: It also permits testability and also use in the PPS-actuated mode, using electromagnetic.

MR. LIPINSKI: One of the objectives at one time was not to have leads go into the assembly and have the freedom of loading the assembly anywhere in the core.

The electromagnetic feature, you have to have leads connected.

MR. SINGER: That's right. Of course, if anything happens to the leads, the magnet is de-sensitized; and you shut the plant down.

It's primarily an availability problem.

MR. LIPINSKI: Of course, it's a fuel-handling problem.

MR. SINGER: Yes, right.

MR. CARBON: A question back here on the inherent shutdown by fuel and absorber motion.

Are you actively pursuing something there?

Does it look promising?

MR. SINGER: It's a fairly low-level effort. What we are doing, really looking at inherent response of reactor system in term of movements of materials.

We're including these types of mechanisms in the code.

The system which A and L is developing will include these types of feedbacks in the modeling.

So at this point, we're including the mechanisms in the analytical code development.

We'll get some information out of the EBR-II operational safety test program to look at some of these mechanisms.

Other information we're going to have to get from whatever we can generate out of FFTF or from some of the PFR testing, which has also looked at inherent feedbacks or inherent shutdown.

Otherwise, it's all calculation internationally.

MR. CARBON: From PFR, I think they're finding they can demonstrate this experimentally, can they not?

MR. SINGER: That's right.

MR. CARBON: But if I understand correctly, that comes from the fact that it's a hood-type rather than a loop-type reactor.

MR. SINGER: It helps. It gives the system a much higher thermal inertia.

So they have much more time for some of the long-term feedbacks to come into play.

It's not a foregone conclusion that you can't get the same type of behavior out of a loop plant.

But it -- what I've seen so far, it appears just to be easier in a loop.

But that doesn't preclude the -- both systems are going to do that.

The other general faulted area we're looking at, essentially faulted behavior of our heat removal system.

And here we like -- the objective is to demonstrate that the normal heat transport circuits or the normal decay heat removal system of the plant can operate in a faulted mode.

And here I'm talking about the total faulted heat removal system, not only heat transport system but any auxiliary or dedicated systems which can be included as part of the plant design.

We're looking at ways of designing and predicting the performance of inherent shutdown heat removal systems.

And as a part of that, we're looking at what type of limits there are on core coolability, what type of structural integrity is needed to ensure heat decay removal, and what type of monitoring and control is necessary to -- again, to ensure that these systems are operating properly when called upon.

The main emphasis of the task we're now working on in the safety area are doing some much more extensive natural circulation testing in EBR-II, looking at simulation of some significantly faulted heat removal situations under natural circulation cooling.

We're obviously going to utilize as much as we can the acceptance test program conducted at FFTF where they had done some loss of electric power in natural circulation testing.

We're doing some work in the THORS facility at Oak Ridge, which is a sodium heat transport loop, which we're looking primarily at the sodium boiling under low flow and natural circulation conditions to find dynamic and the coolability conditions under that mode.

There is work going on under scale model testing, using various fluids at this point, water and sodium, to try and understand some of the fundamentals of primary system, natural circulation behavior.

And hopefully, we'll incorporate all this information in the SASSYS code, S-A-S-S-Y-S, which is being developed at ANL.

And it's a system code based on methodology and the SAS codes, which have been used quite widely in safety analysis.

The general strategy of this effort is to develop sufficient understanding of the inherent capabilities in an LMFBR during decay heat removal.

MR. CARBON: Excuse me. Can I stop just a second?

Frank, in line with our discussion a week or so ago, is the SASSYS code -- is it the same as the NRC SSE code?

MR. GAVIGAN: No.

MR. SINGER: SASSYS is based on methodology, and I forgot which version of SAS. Is it SAS-4-A? SAS-4-A.

And what is done is, a primary heat transport circuit and other modules involving heat transport system are being added to that methodology, which really describes core behavior in SAS-4-A.

And so what we're doing is building on the necessary amount of detail onto a very sophisticated core code, which is necessary to describe the natural circulation, among other phenomena, in a reactor system.

SSE started out from a different basis in which they developed a system code from scratch.

And they have sufficient modeling in all aspects of the plant as necessary to describe system behavior.

Is that right, Jim?

MR. GUPPY: Well, our basic intent or the basic intent of NRC in the development of SSE, which has been under -- which has been funded for about five years now, was first of all to have a generic tool that they could -- that was not plant-specific.

This was back in the days when FFTF was being analyzed.

And it was Westinghouse-proprietary. There were problems with having a computational tool for NRC that was generic in nature.

So they started funding the SSE development effort to look at -- the initial transients to be analyzed were natural circulation and pipe-break analyses.

Then once this capability was there, then additional scope was broadened to include also operational transients, which we have since done by adding control system rep representations.

As Ralph is saying here, SAS-4-A has a lot of capabilities in the core that we do not model because SAS was coming from a different angle, from an HCDA type analysis.

I see a lot of potential overlap in some of these areas.

But, you know, it's -- you know, there is overlap in the whole development area.

But their code is coming from the HCDA area. Our code is more -- it encompasses the whole system.

It started out encompassing the whole system.

Our in-vessel modeling is not -- cannot go as far as SAS-4-A models, by no stretch of the imagination.

So they are two different entities.

MR. CARBON: Fine. Thanks

MR. SINGER: Our basic product in this whole area really is the development of an experimentally validated code, which right now is essentially the SASSYS code, although there may be other codes which have very specific applications.

But the type of codes that will be used will be used hopefully not only for assessment of an existing system in licensing and safety analyses but also be a real aid for plant designers to come up with optimum type of shutdown heat removal systems and analysis of their faulted behavior.

The -- we'd like in this regard to be able to provide, with such a tool and experiments supporting it, information at an early stage to the designers and identify what type of options are available in terms of shutdown heat removal as opposed to, perhaps, only being in a position of analyzing existing system performance.

So we are hoping to provide this type of early input to designers in the next plant which is going to be built in the U.S. as well as -- if Clinch River will be the next plant, to give them support in their licensing arguments in terms of shutdown decay heat removal.

I think we already touched on this, but the basic objectives in SASSYS development are to utilize the capabilities which are already built into the SAS-4-A, primarily obviously, to describe ACDA analysis.

The code already includes a whole initiating phase of an accident.

And so we already have modeled in it the entire single-phase transient behavior as well as two-phase behavior.

A number of things which are being added to the code to make it more applicable to shutdown heat removal systems approach.

We're adding in-core real-heat transfer. This is heat transfer between sub-assemblies.

We already have modifications in the code flow re-distribution between sub-assemblies.

The main things that really have to be added, as I indicated before, are the reversal and the plenum onto the core.

It's a 3-D plenum here, but it's really a heat circuit outside of the core which includes the -- whatever type of outlet -- upper plenums, lower plenums heat transport circuits or independent auxiliary circuits for heat decay removal that are necessary.

So that work is all under way right now at N and AL.

The testing at EBR-II is not -- actual testin is not under way.

The modification facility is under way, and the modification consists merely of adding instrumtation to the plant.

The instrumentation consists of two instrumented sub-assemblies, one sub-assembly which will have the characteristics of a driver fuel sub-assembly, the other of a radial blanket sub-assembly.

And both assemblies will be fully instrumented with flow meters, thermal couples and so forth.

The test plan is currently under development and should be ready by the end of this year or very shortly thereafter and will involve a fairly wide series of natural circulation events representing various types of fault and mode operation of a shut-down heat removal system.

Again, the other two programs, one at -- THORS program, is thermal hydraulic out-of-reactor sodium testing, is being done at Oak Ridge.

Their sodium loop facility is being modified so that natural circulation testing can be conducted in it.

And they are building at the moment two fully instrumented, electrically heated sub-assembly simulators which will be hooked up in parallel with bypass channels so that a very wide range of experimental tests can be run, with main emphasis on sodium boiling in one or more of the sub-assemblies, looking at the type of interaction, hydraulic interaction, between these sub-assemblies under very severely degraded type cooling conditions.

As I indicated before, we're doing scale-model testing, both work in simulation with water at General Electric and planning sodium testing at Atomics International, both programs of which will be discussed in some detail later.

I think I'll stop there. That sort of gives a very brief over-view of the work in this particular area.

If you have any other questions, I'll be glad to talk about them now.

The details in these areas will be discussed after lunch.

MR. CARBON: Any questions anyone would like to raise?

(No response.)

MR. CARBON: With the timing of the lunch room downstairs, perhaps it's best we do break now for lunch. Let's come back about 1:20.

(Proceedings adjourned until 1:20 p.m.)

AFTERNOON SESSION

MR. TUPPER: I am Don Tupper from Westinghouse Electric Corporation, principal investigator at Westinghouse for the Self-Actuated Shutdown System Development Program.

This program was started basically with two goals in mind, that is, to develop a shutdown system that was foolproof, one that didn't require an operator-initiated signal or a PPS-initiated signal.

The other thing we tried to do is eliminate several of the most common cause failure, both mechanical and electrical, including top-head rotation, plant protection system failure and severe core disproportion.

MR. MARK: You don't mean -- Oh, you must have it on the list also that it doesn't do anything unless it is necessary.

MR. TUPPER: It can respond to plant protection system, and it can respond to overtemperature conditions.

One of our design goals is to make sure that it does operate in response to an abnormal condition and it doesn't drop inadvertly.

The program started in 1975 as an inherently safe core design program, as Ralph mentioned, we looked at things like flarring cores, other mechanisms that would shut the reactor down.

Eventually we settled on a mechanism that would do it, and toward the end -- the middle of 1976 we started looking at a temperature-sensitive electromagnet as the most viable concept.

We completed conceptual design toward the end of 1977 and started our test program. This consisted of Argonne testing essentially on a bench at elevated temperatures. With a fair degree of success, we moved into our sodium test loop at ARD. This is still on the temperature-sensitive electromagnet.

From there we went to another test program at DOE's Energy Technology Engineering Center out in Los Angeles. We have run more or less a full-scale absorber assembly electromagnet prototype.

We have finished testing our original unit, found several things that had to be changed.

We went and redesigned it and came up with a stronger magnet, and we just started initiating, we started Phase II testing, of the revised design at ARD, and at ETEC we will be starting the Phase II test program sometime in October.

In addition to those tests, on the slide we have material specimens in EBR2 irradiated, and we also have a coil reliability program aimed at establishing that we can build an electromagnet that will survive in a reactor environment at least 10 years.

The self-actuated shutdown system we have developed consists of a drive line with electromagnetic coil, a nickeline insert and the magnetic circuit that serves as our temperature-sensitive fuse and articulated control assembly, in this case consisting of three bundles, and outside of the control assembly, sic fuel pins, which provide a signal to the temperature-sensitive alloy.

I will go into that in a little bit more detail.

MR. LIPINSKI: What is the length of the articulated section, 48 inches?

MR. TUPPER: Approximately, they total 48. They cover a 48-inch core region.

The assemblies themselves were a little bit longer.

A unique feature of this assembly in comparison to the normal control assembly is the fact that it does have fuel assemblies in it in the corners. This puts burden on the inlet orifice to provide adequate flow for both the fuel and the absorber bundle.

MR. LIPINSKI: What is the total mass that the magnet has to support?

MR. TUPPER: This is the absorber assembly that we are testing at ETEC, the three-bundle assembly and the lifting socket at the top. It weighs approximately 160 pounds, and the sodium is 140.

This unit has been out at ETEC now for almost two years.

MR. MARK: Is that guy holding 160 pounds or 140 in that left hand?

MR. TUPPER: Actually, there is a crane up there with a lifting tool.

CHAIRMAN CARBON: Each of those sections are 140 inches, are they?

MR. TUPPER: They are approximately 24 from here to here.

The absorber material on the first assembly starts a fair distance up.

There is a glass burner, and it is underneath, so it would be parked underneath the car here.

We do have a B4C interruption where we have the articulated joint, and there is a much smaller glass in the top of this middle assembly, a very

5 tiny one in the top assembly. These will be parked outside of the core.

The only significant helium generation we have is in the first set of assemblies.

The articulation feature being used now by Japanese, French and the German reactors.

CHAIRMAN CARBON: Do they all have just, like, one articulated joint, or are some of them more in the form of chains, or how do you decide?

MR. TUPPER: The 48-inch core divided nicely into three sections. A smaller core, like the FFTF reactor, probably would be better off with two.

It is a trade-off between clearance, between the absorber and the guide tube that goes around it and what their expected distortion is.

When you have a specific plant, you will probably go through that trade-off.

MR. LIPINSKI: What is the clearance around this assembly into the fouler tube?

Is it quarter inch gaps or less?

MR. TUPPER: The tight clearance up at the top loop where we have to maintain a certain amount of magnetic contact, and that is a minimum of 68 thousandths of an inch; down in the absorber sections it is almost a hundred, a hundred mills.

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MR. LIPINSKI: If the distortion occurs at the top, then the top may not move, the 68 mills disappears?

MR. TUPPER: We are up in the region where the above core load plane pad, it is the stiffest part of the reactor. It is less likely to be blocked than, say, other configurations used today.

MR. LIPINSKI: But this has to travel the whole length of the core, that top piece, for insertion.

MR. TUPPER: This travels 48 inches.

MR. LIPINSKI: So, if the clearance disappears anywhere in the path, it stops.

MR. TUPPER: That is true. We haven't come up with a mechanism that will do that. We are protected, I will show you a little bit later, from the outer assembly walls; two different tubes.

MR. SIEGEL: Just the six fuel pins that are sensitive to normal power condition?

MR. TUPPER: Yes, and that can also be adjusted according to the plant requirements in multiples of six. You get a significant improvement and response time when you go to 18 or 24. We have looked at a design with 24. It makes it a lot less sensitive to variations in the fuel pin outlet

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temperature.

The temperature-sensitive electromagnet, as I said before, is located in the top load pad region. The magnetic circuit is outlined in green, the magnageal field is provided by the coil, down in the coil. There is a gap here showing just for clarity. In actuality, there is contact around nonmagnetic inset forces for magnetic flux out into the nickel iron, which is part of the guide tube assembly, and then it goes back into the absorber lifting socket, back down and into the coil.

That completes the circuit.

There is a latch configuration rod to show that you have indeed picked up the absorber as you are withdrawing.

If you are going down to pick up the absorber bundle, there is a guide tube that is made out of a foritic steel that will complete the magnetic circuit when you are down in the core. Only in a parked position are you adjacent to the nickel iron.

A VOICE: Where is the parting plate, Bob?

MR. TUPPER: This is the upper, and this is the lower. At the top surface where we do have contact, we have put a hard coat of chromium aluminide to make sure that there isn't any diffusion bonding.

This is one of the things that we are testing now at ARD.

CHAIRMAN CARBON: What kind of a change in neutron flux is necessary to trigger?

MR. TUPPER: We have responded to changes in temperature from the fuel pin outlets. Normally we would run at a 950 degree outlet plus or minus a certain temperature, depending on the uncertainties.

We have picked our curing point so that at 1020 it starts to lose power.

CHAIRMAN CARBON: What kind of change in core power does that represent?

MR. TUPPER: I think it depends on more the rate of change than the --

MR. AVERY: Are you responding to the sodium temperature so loss of flow would trigger you --

MR. TUPPER: Yes, a loss of flow would do the same thing as would a transient overpower.

CHAIRMAN CARBON: I missed something somewhere. Loss of flow?

MR. TUPPER: If you have no change in power level and you cut down on the sodium flow, the sodium temperature at the fuel outlet would go from 950 up to some elevated temperature.

At 1020 we start losing our magnetic holding force.

CHAIRMAN CARBON: If you lost the flow completely, then nothing happens, I guess?

MR. TUPPER: It is unlikely that we would get a complete step change in the flow. We have looked at single pin flow blockages to see if we can respond to that, and we can.

CHAIRMAN CARBON: But basically you are still depending on the heat generation in those fuel pins to heat the sodium to the 1020 degrees?

MR. TUPPER: Right, all the self-actuated shutdown systems that we are considering at AI&G and Westinghouse all respond to temperature.

CHAIRMAN CARBON: Yes, but temperature in turn responds to --

MR. TUPPER: Power level.

CHAIRMAN CARBON: -- power level and pins.

MR. TUPPER: And flow.

CHAIRMAN CARBON: If the flow stays constant, what kind of a flux level will change? What is the Delta T across there?

MR. TUPPER: We have a normal inlet of 650 in and 950 outlet, so it is 300 degrees Delta T.

We have analyzed it for accident conditions that are on the order of a 10-cent-step insertion up to 30 cents.

CHAIRMAN CARBON: But it sounds like if you had a slow change, you would need, like, a 20, 25 percent change in neutron flux to get up to the temperature of 1022.

MR. TUPPER: I am not sure what flux levels the temperatures correspond to right now.

I can check for you.

CHAIRMAN CARBON: I just wondered roughly what sort of sensitivity it had.

Is it quite sensitive to the 1022?

MR. TUPPER: Yes, I will show you in a minute.

MR. LIPINSKI: Where is your 16 mil clearance on this drawing?

MR. TUPPER: This pad right here, and the added factor of strength that we have is we are not connected structurally to the outer duct, so if there is deformation out there, it has to be very severe before it affects the absorber guide tube.

MR. LIPINSKI: Now, you have gone into this articulated design, and that assumed you are going to see some kind of a vertical bowing in the tube it travels to, but you are assuming it maintains a circular cross section?

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MR. TUPPER: Yes, well, within the 16 mil tolerance that we have. We have not considered any local distortion of the guide tube, and it is not any different than what has been considered for Clinch River right now. The clearances and flow rates would have to be adjusted to plant expected conditions.

This is typical of the coils that we have been using. They started out strictly an R&D item. Over the past three years, we have got it to the point where we have an equipment spec that defines how it should be made, and we have a contract with a commercial Westinghouse division that produced 30 of these for long-term reliability testing.

This is essentially an 800-turn coil. You can see some of the wires. They are insulated with a glass bonded alumina, and we have had this operating at temperatures up to 1 00 degrees.

This particular coil was the one that was used at ETEC for 18 months. It is close to 11,000 hours of elevated temperatures. The leads will be protected by aluminus ramic pins taken out of the tube to the upper end of the reactor where we can make a transition to conventional electrical power supplies at reasonable temperatures.

name

This is the temperature-sensitive alloy that was sent to ETEC for our Phase II testing. It is separated from the guide tube by about a 10 mil clearance, which is maintained by buttons and would groove the inside and outside diameters to increase its thermal response.

CHAIRMAN CARBON: That is the nickel-iron alloy?

MR. TUPPER: Yes, 62-1/2 percent nickel.

CHAIRMAN CARBON: Does the curing point vary with age or anything like that?

MR. TUPPER: Not yet; we have not found anything that varied a curing point, other than in a chemical composition. You can vary magnetic properties like permeability and saturation point by heat treating it, a method that you work. As far as we can tell, chemistry is the only thing that determines curing point, but we do have samples in EBR2 to confirm that.

MR. LIPINSKI: What about irradiation? What happens to nickel and iron in time?

MR. TUPPER: These are curing point currents made for different nickel-iron alloys that we have tested all with the same chemical composition. You can see there is some variation in the flux density to saturation. The curing point is pretty nearly always constant. At 1050 there is almost no magnetic strength left.

MR. LIPINSKI: This set of numbers on the left there, the 62-1/2 percent, that applies to all those?

MR. TUPPER: All three of these test samples were the same chemical composition, right.

MR. LIPINSKI: What happens to nickel when it is irradiated? What happens to iron when it is irradiated? The cross sections versus what they change to?

MR. TUPPER: We have seen test data not as far as we go in terms of dosage that shows there is some change in the flux curing capability. It may go down 10 percent. Some tests have even shown it being elevated, not for this specific chemical composition, but for nickel-iron alloys.

The thing that has remained constant in all irradiation test data is the curing point where it drops off.

MR. LIPINSKI: That has not been seconded?

MR. TUPPER: Right.

MR. LIPINSKI: This is over a 10-year extended lifetime?

MR. TUPPER: This is equivalent to approximately an eight-year lifetime from what we have seen so far, and we are running tests in EPR2 that would carry it to the 10-year life.

MR. SIEGEL: What happens if the reactor -- if you have this system, the real reactor, and the reactor is operated at part load, or for some system reason, instead of 950 outlet temperature, they decide to go to 875 or whatever? Does that vitiate the effectiveness of this whole thing?

MR. TUPPER: It increases the time we have to respond. Our curing point would be preset to respond before any damage occurred in the core. So, if they are running at part load and at lower parallels, the time it takes to heat up to the curing point will be a little bit longer but will still respond the same in that we limit more damage.

MR. SIEGEL: I am saying, all operations have some temperature which is relatively close to this curing point. Then you will fire a 20 percent overpower, but if you are operating considerably lower than that selected temperature, you may fire at 100 percent overpower. You have nowhere near the same degree of safety that you had.

MR. TUPPER: We are assuming that the power level and the outlet temperatures are pretty much kept in a linear proportion.

MR. SIEGEL: So am I. I am saying that in one case you have a Delta T of maybe 20 degrees of normal operations from the firing point, and the other case you might have 120 degrees.

A VOICE: The temperatures are actually lower, so you actually will be safer.

MR. TUPPER: Right, the temperatures in the remainder of the core won't do anything different than what is happening to our trigger fuel pins, unless there is a local fault.

MR. AVERY: I guess another way to follow the question: Can you orifice the flow in this test before you go into the reactor?

MR. TUPPER: Before you go into the reactor, you can orifice it to your operating conditions. You will do that from plant to plant.

I think for part load, though, we have the orifice in the fuel pins so that they respond as average fuel pins. As the temperature goes up in the remainder of the core, due to an overpower condition, before it gets to the critical level, we will have inserted our rods.

I will try to get some more on that.

I am a little out of order, but this is basically a cross section of the absorber assembly.

It is a circular arrangement of absorber pins with fuel pins located in the corners of the hex, as is what I mentioned before about providing protection from local distortion and allowing plenty of clearance to allow the absorber to be inserted.

This particular pattern without the six fuel pins is being used by Clinch River on their secondary fuel systems.

From the testing that we have done at ARD, we have got an example of how the fuel pins or how the magnetic strength varies as a function of temperature. Between 70 degrees and a thousand degrees Fahrenheit.

We maintain over a 300 pound holding force.

At 1020 we start approaching the curing point, and at 1050 we have got almost no magnetic strength at all.

The weight of the absorber is up in this region, so in the area of 1020 to 1050 we have dropped the absorber, and it's down in the core.

Our original temperature-sensitive electromagnet didn't have nearly the holding strength when we wanted it, and it had quite a bit more when we didn't want it, so our redesign made significant improvements.

Another set of tests demonstrate that the temperature at which the absorber breaks away will be a function of the coil current or the AMP turns and the weight of the absorber.

We ran several sets with a 100-pound weight on the end of it, and breakaway temperatures were between 1035 and 1050.

Heavy absorbers, such as a thousand megawatts plants would break away between -- 1020 was the lowest point we measured and 1040.

We are always in this range between 1020 and 1050, though.

The ultimate would be to determine the effect of these variables on our response time, and we hope to do that in the next phase of testing out at ETEC.

Does that partially answer your question on the sensitivity?

CHAIRMAN CARBON: Yes.

MR. TUPPER: To summarize our testing out at ETEC, we have accumulated almost 11,000 hours of operating time.

We ran over 200 tests just to characterize the magnet, and we went on to run inherent release tests where we inject hot sodium around the nickel iron and measure its performance characteristics.

We also did a series of tests where we just interrupted the current, and this would be a normal PPS initiated SCRAM to see what our drop characteristics were, and we ran a six-month dwell to see if being up in a parked position for a long period of time would affect our performance.

Before the dwell, we ran an interruption-release time test, and we had absorber motion within 150 milliseconds of cutting the current. The same occurred after our six-month dwell.

Inherent release time had an insignificant variation before and after a six-month dwell, as did the insertion time.

CHAIRMAN CARBON: What was the time for inherent release again?

I didn't understand that.

MR. TUPPER: These are tests we ran by injecting hot sodium into the test loop. The inherent release time is a basis of comparison from one test to the other. It is not prototypic of an actual reactor transient.

We are heating up a much bigger mass, and we have a different temperature input, but we do use that data to calibrate our computer base back at ARD.

In response to an PPS Trip, the coil will hold onto the magnet until there has been enough current decay so that we are down on the order of .4 AMPS. This is determined by the time constant of the coil.

Within 150 milliseconds, we have initiated insertion, which is comparable to electromechanical releases being looked at in current permeated secondary mechanisms. So, we haven't sacrificed any features by using an electromagnet as a holding mechanism in terms of our PPS response capability.

T2 MR. LIPINSKI: Given AMP turn doesn't appear on here [illegible]t release time would be a function of the alloy or coil?

MR. TUPPER: This was an 800-turn coil.

MR. LIPINSKI: 800-turn?

MR. TUPPER: Yes. Our drop characteristics are delinear until the point of dash pot impact. The dash pot in this case would have to be located in the control assembly. These are very dependent on the absorber flow rate and the clearances that you allow for accommodating distortion.

MR. LIPINSKI: What is the external circuit? Do you just open the circuit and fire across that, or do you have a special circuit?

MR. TUPPER: It can be an on-off switch with protection in there so you don't get a voltage spike.

Tests that we have run out at ETEC, we have experienced negative voltages on the order of 100 volts as soon as you open that circuit because of the input of diodes, primarily to protect the coil. There is basically an on-off switch connected somehow at PPS system.

In addition to the testing out at ETEC, I mentioned that we had samples of EBR2. This is a coilette. We hope to have it out of EBR2 in December and examine it for damage, either damage to the insulation or swelling.

It is primarily a glass, and if we see anything at all, it's expected to be a small change in color.

We do have a chromemoli and a nickel-iron sample at EBR2, and after exposure, we will measure them in a hot cell to see if the curing point has changed under the effect of the irradiation and other magnetic properties.

These were put in EBR2 last August.

To summarize where we are, we have got test programs, Sweater II, that are underway, ARD, test program out at ETEC, which will be entering Phase II pretty soon.

Phase II program out at ETEC will also include some bowing tests to measure the effect of bow on the articulated joints that we have on the system.

We have in the area of system qualification tests breakdown voltage tests going on twisted wire pairs to see if there is any effect of time on the way that the coil insulation behaves. It turns out instead of deteriorating, it seems to get better with age.

Our breakdown voltage has gone from 700 volts up to the area of 1,000 on some of our test samples, and none of them have decreased.

We have 30 coils on order, full-sized coils that we will run a series of tests on, simulate their reactor environment, establish a 10-year life cycle, and the irradiation tests that I mentioned before.

We are considering future tests to test out various features of our concept. They include transient tests, which would use simulated fuel pins, actually put us through plant transients and measure our performance.

We would like to do inlet orificing tests, establish what hot channel factors we have to work with, what uncertainties. when we set our curing point temperature.

We would like a CDS prototype.

I keep saying I would like. This is a wish list.

We would like CDS prototype tests similar to what has been done to the primaries and secondaries at Clinch River to test the LOA-1 performance of a self-actuated shutdown system, our reliability program, similar to what they have done.

We also plan to do seismic tests at the Advanced Reactor Division for Water Facilities, and this will make sure we can get the absorber assembly down and the magnet has enough support margin so that we don't have any inadvertent drops.

Another test we consider necessary is a test up at the FFTF facility where we put dedicated fuel pins into the reactor and measure that performance and compare it to what our predictions are.

MR. LIPINSKI: Back to the seismic tests, at this point, do you have a feeling for the fact that if you have a safe shutdown earthquake that the magnet will still hold on, or will it release?

MR. TUPPER: We have got approximately 100 percent margin in our holding power, and according to the loads on Clinch River, that should be adequate. That is certainly much higher than some of the other seismic-initiated trips around the plants.

MR. HARTUNG: The reason for the articulated portion is to give the rod sensitivity to bowing and deformation, is that it?

MR. TUPPER: Yes.

MR. HARTUNG: Have you done any tests, or do you plan any tests to explore how good that current design is to maybe optimize it for different kinds of conditions for bowing, or is that just a judgment that particular absorber is good in that respect?

MR. TUPPER: Right now it is more of a judgment.

When you trade off flow area leakage, clearance, all things that turn into inefficiencies, when you design your absorber, perhaps you may not wind up with a single piston run that we have now, which will take away from your clearance or your ability to accommodate the distortion, but it also increases the overall plant performance to do that. So, there is a tradeoff that a plant designer would do.

MR. HARTUNG: I guess I am picking up on what Walt said before.

It is going to remain perfectly round. It seems like you might very well have overreacted in providing this great articulation for this thing. Without some tests, it may be difficult to determine whether, in fact, that is the case, or not.

MR. TUPPER: It does provide you a level of protection, though, that you don't get with a straight rod, and over a large demonstration plant, it gets to be a kind of a long assembly.

MR. LIPINSKI: But the thing I am not convinced about, if you are telling me I can get vertical distortions, I can't get vertical distortions without ruining the circular going to eliptical distortion if I make the thing change vertically. If I make the circle go to the elipse, 60 mils disappears.

The question is: How much can you take vertically before the 60 mils goes?

MR. TUPPER: That falls more in the category of an absorber development program, which is being carried out by HEDL. I am not trying to avoid answering your question. I am just not prepared to answer it.

We will take whatever technology is available in absorber development and incorporate it into our

25 design.

MR. LIPINSKI: It seems like the articulatio may not accommodate the vertica distortion, but your clearance on the top will not.

MR. TUPPER: It should provide you an increase capability to get your absorber in for any specific event over a normal separate rod of an equivalent length. It buys you something.

MR. LIPINSKI: 60 mils disappears.

MR. TUPPER: 60 mils can be adjusted. It can be 90 mils.

MR. HARTUNG: That is what I was thinking. I have seen some people at SASS that have chosen that. Instead of buying an articulated absorber, they have a collapsible absorber. What they have is the thing could collapse about one inch if something was not in the way. It is a totally separate question. Your absorber is totally separate from your mechanism and can be optimized, I would thing somehow, separately.

MR. TUPPER: We considered that. It is kind of on the back burner, but Tandehem Balls has a mechanism. They are still looking at them.

MR. LIPINSKI: They are supported by the flow, and if the flow disappears, the bulging came down.

MR. TUPPER: Right, and they had an electromagnet in the circuit which acted as a valve to increase the response time of the ball insertion.

MR. LIPINSKI: Is that still under development?

MR. TUPPER: Jim, do you want to comment on that -- Joe, rather, Joe?

MR. MILLS: It's not really under development. We are still looking at some materials associated with the balls at that low level. It's considered sort of a backup alternative option to the articulated control absorber right now.

MR. LIPINSKI: I am going to pose the following question. Maybe you don't have the answer, but Pete does.

He mentioned that the articulated design is someone else's responsibility.

Is that part of the program? And they are just looking at the magnet release.

How is this being coordinated?

A VOICE: I think what he was saying -- I am Paul Fox.

The articulated design was put in three or four years ago to give us three times distortion capability of the existing rods at that time.

That is all it was for.

At the time we didn't know whether it was going to slow the rod because we have more contact points or anything else. It was an option that we didn't give a tremendous amount of attention to. We knew that we could get three times existing distortion.

I am sorry, I don't remember those numbers because they were generated, but it was an inch and a half.

The heavy tube was put in so that we didn't get any load in the core that would crush that tube. We looked at very high loads in the core.

The articulation has not been our major concern.

The reactor designer can put it in or take it out, depending on how he feels, his distortion, and that sort of thing.

The exact clearance, we have had 90, we have had 100 mils. We have been testing the height of clearance to see if we got any buildup or any problems that are that way that could give us any problems in that area.

MR. LIPINSKI: You have fabricated one that is articulated that you are testing, and I am assuming there is some basis for having fabricated that particular articulated design.

MR. FOX: That is right, three times the distortion --

MR. LIPINSKI: In a 48-inch length?

MR. FOX: We can take about an inch and a half or something like that. Don't quote me on those numbers. That is approximate. It was about three times the distortion.

MR. TUPPER: It was anticipated that a normal bow would be 300 thousandths of an inch.

We are testing the one out at ETEC up to a bow of three-quarters of an inch.

MR. FOX: I couldn't remember the numbers. Okay.

MR. TUPPER: That hasn't been tested yet.

That will be in early '82.

MR. LIPINSKI: Okay.

CHAIRMAN CARBON: How serious is the terminal shock problem if you did get an inadvertent release in seismic or what have you?

MR. TUPPER: Our emphasis right now has been on putting enough holding power into the reactor so that we don't get the inadvertent release.

MR. FOX: Excuse me. Again, I am Paul Fox.

We design for this now. We don't like to get a lot of cycles of inadvertent release.

We already designed, for example, SFTF has designed for that case so that we do an orderly shutdown.

Our major emphasis now is to have these not released, as you can see. Everything is in the direction of releasing, if something goes wrong, or just about.

So, it doesn't create a serious problem if you get one, but we have to shut the plant down in an orderly, appropriate way.

It is not a serious problem.

MR. TUPPER: To answer your question before, where do we stand now, initial sodium testing has been completed, and concept viability has been demonstrated.

The self-actuated shutdown systems supported by a temperature-sensitive electromagnet will increase the reliability of a plant shutdown system, and we say that because it does have a self-actuation feature, and it goes through a plant protection system initiated separate without the movement of any mechanical parts in either the drive line or the drive mechanism. It is just a straight current reduction.

And we think that development of a plant prototype should be initiated and completed, tested, on a fairly high priority basis, and that's consistent with the development of the next generation of breeder reactors, a thousand megawatt.

MR. LIPINSKI: Now, do you have a computer program that simulates your assembly so you can run an overpower transient and an underflow transient?

MR. TUPPER: We have right now. It is two different programs. We are combining it into one; one of the 10 percent of electromagnets and one of the fuel pin area; and that is in the process of being combined into one.

MR. LIPINSKI: So, you have not run any analysis of the performance of the system for transients?

MR. TUPPER: We have done very preliminary analysis with Clinch River data, two or three years ago. It has not been updated to a thousand megawatt plan, but based on what we looked at for Clinch River, we have a fair margin in our response time to insert the absorber assembly.

MR. LIPINSKI: When you get your code development, will you have bench mark experimental results from your tests that can be correlated against your analytical calculations in order to adjust the parameters that go into that calculation?

MR. TUPPER: We are doing that now with the data that we get from our ETEC test to update the model of the temperature-sensitive electromagnet and the model of the fuel pin when it hits transient response we hope to get by using the simulated fuel pins in a separate test. Transient response is very sensitive to the mass of metal involved.

MR. LIPINSKI: And the heat transfer coefficient in terms of what you pumped into the calculation and when you get a correlation to your experimental result, you are confident in your analytical results?

MR. TUPPER: Right, it turns out we have looked at a lot of variables, and the big one right now is nickel-iron, and that is responsible for most of our lag time.

All we have to do, all we have to do, is cut down the ligaments in the nickel-iron.

Working against us is the fact that the nickel-iron increases as it goes to a clearing point so much, and then it goes back down after it passes us.

MR. LIPINSKI: What is the flow coastdown through the assembly on a total loss of flow?

MR. TUPPER: Loss of flow we have done on our Clinch River coastdowns.

MR. LIPINSKI: Total loss of flow?

MR. TUPPER: Normal pump coastdown initiated by a pump trip at 10 percent overpower.

MR. GAVIGAN: Walt, do you want some more on the transient analysis? Paul is a little more familiar with it.

MR. LIPINSKI: Okay. Do you run into cases where your plant protection system fails and you got total loss of flow?

MR. FOX: Yes, we did it for a large plant. We did it for Clinch River.

We used Clinch River because it has the fastest coastdown than any plant we have seen. It doesn't have the big inertia that some of the largest plant studies had, so it gave us a factor, and it was the worst case.

MR. TUPPER: We did reactivity insertions, two cents a second, five cents a second, on up to three cents a second. No problem. We did 30 cents a second. That is our upper limit.

We did all the flow coastdowns, and this is three or four years ago, as reported in the literature, and then we did the seismic, which is the coastdown, but the reactivity, which is the limiting case.

They were typical Clinch River coastdowns. They weren't artificially flow-interrupted type things.

MR. LIPINSKI: With the inertia of the pumps?

MR. TUPPER: Pumps, right, loops and the pumps.

MR. AVERY: The full-scale reactor, how many such assemblies would you anticipate, and how much reactivity would be in them?

MR. TUPPER: We don't have a plant right now that we are working on.

MR. AVERY: I mean, is it more than one?

MR. TUPPER: In CDS, it probably would be close to nine, six or nine, depending on --

MR. AVERY: How much reactivity you have?

MR. GLUEKLER: As a minimum, you would need three assemblies.

CHAIRMAN CARBON: We can't hear you.

MR. GLUEKLER: Emil Gluekler. One would need at least three absorbers. From a reliability standpoint, one may choose four assemblies.

This is the minimum that would be required.

MR. LIPINSKI: What is the worst of the single assembly?

MR. GLUEKLER: On the order of five.

MR. LIPINSKI: You have the Boron Rods all selected in their numbers, so it has some design basis in the number of new rods?

MR. TUPPER: That was a very old design, a large plant design that existed in 1975.

MR. LIPINSKI: He has got another design in mind, other than the one we are looking at.

MR. TUPPER: Right, and his comment that three or four assemblies would be required, that is full assemblies. We lose some absorber material because of the fuel pins in the corners, so it may be up to six, possibly nine.

CHAIRMAN CARBON: Thank you.

MR. GAVIGAN: The next speaker is Emil Gluekler.

MR. GLUEKLER: I am Emil Gluekler from General Electric Company.

The second area on our line of assurance is the recommendation of the Shutdown Heat Removal Faults.

I would like to speak about water-scale experiments that are being conducted at GE to support code development validation to generate the data base for natural convection data and to evaluate conditions in the test facility, to evaluate the phenomena of flow stratification, low mixing and their effect on the

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flow redistribution in the reactor core.

The objective of our program in the near time is to provide a well-defined data base for evaluation of natural circulation codes that are used for demonstrating core coolability under abnormal conditions.

The scope of our work is to perform water-scale tests.

First, I will make some general remarks about the validity of using water tests to simulate sodium systems. Then I will describe to you some of the results of our first experiments.

Here is a schematic of a reactor system. The problems we are concerned about is the coolability of a reactor core under natural circulation conditions.

We consider abnormal conditions, for instance, where the flow outlet is blocked or where no primary system flow exists.

In this situation it would be only flow within the reactor vessel, and there will be coupling effects between the upper plenum and the core.

For some transients, low flow transients, there will be effects of the piping flow on the core flow instability, and the piping may affect the coolability of the core schematically, and this is shown here.

We have to consider several systems, and the reactor core is affected by the conditions in the upper plenum, the lower plenum, the primary system, the interaction with the direct auxiliary cooling system and any auxiliary devices that may exist in the core to operate the direct auxiliary -- cooling system.

The phenomena of interests are listed here.

We have all used water tests to look at the flow stratification in mixing in the upper plenum of the reactor vessel. We have all looked at the flow redistribution in the reactor core under natural circulation conditions. We have looked at the effect of a direct auxiliary cooling system on core coolability, and we have all investigated, checked valve operations, and flow diodes on effect of core coolability, also flow instabilities in pipes, core coolability.

CHAIRMAN CARBON: Do you have a problem relating close stratification and mixing in the upper plenum when you are using water for sodium?

MR. GLUEKLER: I will give you some examples on how we treat this problem and how we have done the scaling to make sure that we have total similarity.

CHAIRMAN CARBON: Which of those different phenomena give you most difficulty?

MR. GLUEKLER: Obviously, the flow stratification with mixing the flow redistribution in the reactor core is predictable, a very significant flow stratification.

The redistribution is less predictable.

In order to obtain a valid simulation for a large sodium-cooled reactor, we looked at the scaling loss for any system under mixed convection. There are three dimensionalized numbers that characterize these conditions. These are the Richardson number, the Peclet number and the Euler number. We have demonstrated that if these numbers are selected for false flow conditions, the similarity would hold under natural convection systems.

We also found that with any scale system it is not possible to match all parameters simultaneously. There have to be some approximations.

So, with the experiments, as with any analytical solution, some approximations have to be introduced.

We selected a priority system to establish the parameters for the test.

First, I want to describe the scaling based on the transient momentum equation.

There are two dimensionless parameters: The Richardson number and the Euler number; the friction term. In order to obtain an equivalent water model, we would need similar geometry, the same Richardson number and the same Euler number. The Richardson number can be split into two parameters: One is a material properties, depending on metal, and one is a parameter, just dependent on the heat generation rate, the flow rate and the length.

T3

So, we can use the second parameter to design and scale a test that would give us Richardson number similarity with a soldium system.

There is a card that would show Richardson number scaling, how the water flow rate would have to be selected for various scales. You can see that for increasing a scale, large flow rates would be required. Larger flow rates would also be required for increased power in the reactor core.

The temperature difference across the core is a function of the scale that is shown in this view graph. You can see that for decreasing scale, the Delta P across the core would increase.

Based on these evaluations, we have selected a scale for a water experiment, which is one-eighth scale test with a power of one-half megawatt and a

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Delta T across the core of 12.8 degrees F.

The basis for this selection was to include sufficient geometric detail and also to choose a power density that can be obtained without boiling in the core region and to have a measurable power or have a measurable flow rate in Delta T.

Let me describe some of the other parameters that are important for the scale.

We have to match the fraction coefficient as well. For a typical water scale test, the first term which represents the viscous friction is much smaller in the water test than it would be for the sodium system. So, we have to compensate with orificing losses to make up for the overall friction coefficient.

For a typical large breeder reactor, we have a Delta P approximately of 84.4 PSI. For the selected test, we would have a Delta T of 0.57 PSI.

One problem with matching the Euler number, of course, is that we cannot exactly -- We will have have a much lower Venus number for the test.

In a typical sodium cooler reactor we operate at Venus numbers of approximately 60 thousandths, and in water tests we would have a Venus number of approximately 6,000

That means for any flow transitions, for full-flow range to natural convection, we would enter into the lamina machine much earlier. At 33 percent flow for the sodium reactor, it would be at three percent of the flow.

So, there are some conditions that we can only approximate, that we didn't match completely.

A few words on the heat transfer similitude.

From the energy equation we can divide by one dimensionless parameter, which is the Peclet number. To match this parameter, we have to pose some additional additions on the sign. For matching the ratio of the numbers, we need a ratio of one.

We can see that for a water-scale model, we come pretty close -- We can come pretty close to one. It is possible to match the Peclet number and the Richardson number simultaneously for any given scale, and we made a compromise here in order to maintain both a measurable temperature and a measurable flow rate.

We arbitrarily decreased the Peclet number and increased the Delta T in the test section.

So, it is relatively small.

To address Dr. Carbon's question about the important problems, I have a figure here that shows the ratio between the conductive and the convective heat transfer in the upper plenum as a function of the Peclet number, and this would correspond to a flow stratification problem in the upper plenum .

You can see here the value for the large breeder reactor. We can match these conditions exactly with a water-scale test. With the water-scale test we selected, we are using a slightly smaller Peclet number. With a sodium test of the same scale, the conductive heat transfer is too large, as you can see here, by approximately a factor of 100.

So, a water-scale test actually can provide a better simulation of the upper plenum of the sodium test.

Now, this applies only for certain conditions. and for any transients, one has to consider conditions slightly different.

A few words on the coupling between the momentum and the energy equation.

We have the two time scales for the momentum and for the energy equation. Ideally the time scales should be the same, and for the selected test, we come very close.

Actually you can see that this ratio that characterizes the coupling between the two time scales is exactly the Peclet number.

So, if the Peclet number is exactly matched, there is synchronization of the time scales of the momentum and the energy equation, and that can be accomplished with the water-scale test.

Here is a schematic of our test design. It consists of a core ratio of octagon shape to facilitate the flow measurements that we would like to perform using lazer velocimetry. It also includes an upper plenum and it will include a lower plenum region, and we will be able to add additional sections to the percent of the entire reactor vessel.

Initially we will operate just with the reactor core and an upper plenum attached.

The main emphasis in our water test is to generate adequate flow and temperature data.

The flow measurements are very important for validating our computer codes, so we had some efforts to develop adequate flow measurement techniques.

We will be using turbine wheel flow meters for the characterization of general flows. We will be using a two and three D lazer velocimetry.

This lazer functions like this: An interference pattern is created in the fluid, and small particles traveling through this interference pattern will change from light to dark, and this frequency will be picked up by a photo multiplier tube. So, this is the way the velocity is measured.

We will be using flow-visualization techniques, and here we have evaluated three different techniques: dye tracers, hydrogen bubble techniques, some Electrolytic Ph change methods.

We selected the Electrolytic Ph Change method because it's most reliable and provides adequate results.

Here is an example on the flow-visualization based on Electrolytic methods or thymol blue methods.

The flow inlet pipe is at the bottom of the container here; the outlet is at the top, and a vidar has been installed across the inlet section. This vidar generates a color change in the fluid that passes by the vidar, and you can see here the stream line traveling up and exiting through the outlet nozzle.

This picture is taken for a very low flow rate.

MR. LIPINSKI: What does that color change to the left mean?

MR. GLUEKLER: This is just the vidar, the extent to the vidar. That's nothing that has to do with the test.

MR. LIPINSKI: So, this indicates there is no mixing?

MR. GLUEKLER: Yes, that is right.

The advantage of this method is these vidars can be placed in any location within the test section, and we are presently trying to develop a qualitative method of --

CHAIRMAN CARBON: A what?

MR. GLUEKLER: -- this technique so there won't be continuous stream lines to all these sections.

MR. LIPINSKI: Was this analytic?

MR. GLUEKLER: We have not attempted to attack this particular problem, but we will be using 3D codes like commix, to correlate experimental evaluation here with flow redistribution.

MR. LIPINSKI: This doesn't even indicate any diffusion mixing. It is just a straight line.

MR. GLUEKLER: Yes, there is no mixing. We have not expected that to happen.

MR. LIPINSKI: I wouldn't have expected that either.

A VOICE: What was your flow rate?

MR. GLUEKLER: The flow rate was on the order of one centimeter per minute, very small.

We have performed one test with the objective to determine the flow rate redistribution in the reactor core under natural circulation condition. We represented the core with six power load channels, representing the fuel assemblies, the inner blanket, the ratio blanket assembly, the building assembly.

We had one bypass flow.

These channels were connected to an upper plenum and a lower plenum. In the upper plenum we included an upper internal structure. For the test we used full-length channels, and the other scaling consideration included here was the matching of the flow areas between the various core assemblies.

The scaling of the upper plenum was performed based on the continuity equation and assuming equal convection time for the channel and the plenum.

MR. LIPINSKI: What is UIS, upper internal structure?

MR. GLUEKLER: That is the upper internal structure.

The objective of the test was to provide a data base for evaluating the existing analytical models, mainly the one-dimensional models.

I will give you some comparisons later.

Let me first describe the test some more.

Here is an illustration of the test facility. You can see here the 12-feet high core assemblies, six power load channels, the upper plenum, the lower plenum; and here is the data acquisition system.

We developed some software that allowed direct interpretation of the test results for the data acquisition.

Here is a view of the upper plenum. You can see the upper internal structure with the flow outlets, and three of the core channels were heated channels. The instrumentation we used is summarized on this chart.

We measured the channel flow with turbine flow meters. We also used flow-visualization in these channels. We used flow-visualization to characterize the flow in the plenum.

We had some thermocouples installed, five for each channel, 10 for the upper plenum and some for the channel mode to determine the measure of the heat losses to the environment.

We also measured the pressure and the power input for the test.

Here is a summary of the transient that we simulated.

In general we started from decay power conditions with sodium overflow, and we evaluated the transition to natural convection conditions.

Here are some typical results: This figure shows the flow rate as a function of time. You can see the rapid decay that would be typical of a large breeder reactor and then the flow redistribution in the core channels under natural convection conditions.

The channels with the high power would turn out natural convection flow.

In the bypass channel, we absorbed less.

MR. LIPINSKI: In that bypass channel, is that open when the flow comes down, or are you doing that experimentally?

MR. GLUEKLER: We had to do that manually.

MR. LIPINSKI: Okay.

MR. GLUEKLER: The next figure shows the temperature history for the tests. I have an additional figure that is not included in the handout that shows

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the effect a little more clearly.

We have three channels plotted here; two high-power channels, Channel 1 and 2, representing the fuel assemblies in the core, and a Channel 3, which ?? is a radial blanket channel, which has an intermediate power ratio; and what we observed here, there is an extended period of flow stagnation in this channel initially, and during this period, the temperature in the channel increases significantly, and it exceeds the temperature of the channels that have the maximum power.

Of course, after the temperature is increased, large forces are generated, and the flow increases, and the temperature comes back down.

This is a summary of these evaluations that isn't included in the handout.

Here is the temperature rise in the channel, which is a function of the power. This is normalized to the maximum power assembly, and you can see that for intermediate power assemblies the maximum temperature increases itself.

I do not want to extrapolate beyond the measure of range.

Obviously, this core shown here would have to come down, and for a foreseeable power, the temperature increase should be one.

We used the various one-dimensional models to evaluate the test data. There are several one-dimensional codes available, including SSC, Demo, Gencon, Commix and so on.

We used generic features of these codes and developed a code called BIFR, which stands for Buoyancy Induced Flow Redistribution, which this code includes a flow redistribution model similar to the one used in SSC.

The assumptions are perfect mixing and uniform pressure in the upper plenum.

The BIFR code also includes pump coastdown characteristics, heat losses to the environment and so on.

I will show you a comparison of the model with some test data.

The flow redistribution could be predicted very well for both the fuel assemblies and the blanket assemblies.

The temperature distribution was predicted very well also.

You can see here some temperature oscillation in the internal blanket. Of course, it is very important for these comparisons to characterize

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the pressure drop in the flow channels very well, and we performed a series of tests to determine the pressure drop as a function of the Venus number.

Our preliminary conclusions are that the flow redistribution in the reactor core can be predicted reasonably well with one-dimensional models on the average basis. Some modifications would be required in the one-dimensional models to improve the predictions. That would include using several control volts in the upper plenum, rather than just one uniformly mixed or completely mixed plenum.

We observed that in intermediate power for channels, temperature peaks may occur during the transition from forced flow to natural convection flow.

The first series of experiments has been completed. We plan to continue the tests to apply some lazer velocimetry to determine more detailed flow fields in the upper plenum region.

Our effort includes the evaluation of both one-dimensional and three-dimensional models. The three-dimensional codes that we evaluated are the Commix Code and the Tempest Code, not the Thermit Code that is listed here on the chart.

Here are the results of a prediction with the three-dimensional Commix Code, the flow redistribution

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in the lower plenum. We perceive that in a combination of one-dimensional and three-dimensional models will be used to characterize the flow conditions in the reactor vessels under low flow conditions.

Any questions?

(No response.)

MR. GLUEKLER: Thank you.

CHAIRMAN CARBON: Thank you.

MR. GAVIGAN: The next speaker is Joe Mills from Atomix International.

CHAIRMAN CARBON: Frank, can I ask a general question?

MR. GAVIGAN: Yes.

CHAIRMAN CARBON: How strong a role is this going to play in your analyses?

MR. GAVIGAN: Very strong; the reason for doing this work is that the computer codes that have been developed didn't look at the off-normal conditions that we are interested in, safety people. Therefore, you see that the tone running through Emil's presentation and Joe Mills and the other natural circulation work will be looking at conditions that are outside the design basis that a designer normally designs for and people like Jim Guppe produce codes for.

We are adding models onto codes under much more serious situations, and they do two things for us: One is that they allow us to capitalize on what we described as earlier, the inherent capabilities of sodium systems to cool cores under adverse situations beyond those of the designers, and, secondly, we would hope that would allow us to help make the case that we don't have to have all the large CDA's and there is sufficient cooling capability, not only on normal design, but under adverse situations, and there is a large reliable margin, if you will, in the plant.

We are trying to see where the margin is and quantify it in these code developers.

CHAIRMAN CARBON: Does the water word on LWR's of any value to you?

MR. GAVIGAN: I didn't know people were doing that.

CHAIRMAN CARBON: Just natural circulation shutdowns, for example.

MR. GAVIGAN: I would have to ask Emil or Ralph on that one.

A VOICE: It is obviously a value, but to make it useful, we would have to convert to doing systems tests on the reactors themselves. We would have to take our codes, simulate their systems, and to see how well those codes are doing.

It is quite a significant effort.

CHAIRMAN CARBON: Too big an effort for now?

A VOICE: It is a very big effort for a limited program.

MR. MILLS: At Atomix International, we are in the process of doing a research program that compliments the efforts of General Electric in that we have been focusing on scale-model sodium testing.

Now, where we are at the present time is in the planning stages, so what I have to tell you today are going to be plans and approaches for doing things, and at the same time I would like to present some results of some scaling studies we have done that tend to reach slightly different conclusions than general electric, and we have kind of approached the problem from a slightly different perspective, and I am not sure that they are totally inconsistent, but we haven't completed all our studies to date.

I would like to give you a flavor of how we at AI envision the role of scale-model sodium testing.

In the near term, which I would characterize as the next few years, we would see doing small-scale phenomenological tests that would be done in essentially static sodium. This would be in small diameter pots, which would be in well-designed geometries; and the concept would be to initiate transients by turning on a heat source and/or activating a heat sink.

Subsequently, these would be followed by an increase in system complexity by introducing a flowing sodium system, which by its nature, implies a facility requirement, an increase in facility requirements, and there we would be looking at the interaction of the primary in-tank circulation with the external system, sort of like the dracs interacting with the primary heat transport system, and this was really the impetus for this initial effort.

We were looking at trying to do scale-model testing of fully prototypic systems.

We were looking at this effort initially in support of the large development plan and the dracs concept.

Just in terms of, I think Emil has kind of laid out the key issues as to the kind of issues we would be addressing. Here again, the focus is on a drac-type heating removal system that involved in-vessel heat exchangers and looks at in-vessel natural circulation phenomena as the focus, and our primary

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emphasis for this mere-term testing would be the generation of data for computer code validation.

We see it also as a basis for us to sort out these differences that may result from sodium versus water systems by coordinating this program with the program that is in place at GE.

A secondary objective, and one that we are also in the process of working on at AI, is since we want to use these tests for computer code validation, we would like to be able to have temperature data and flow data, and we would like to, in the near term, have this sodium testing provide a test base for the development of some sodium-flow instrumentation, which we are in the process of doing at Atomix International.

As I mentioned, all these efforts are in the planning stages, and what I would like to focus the rest of this presentation on is on natural convection and scaling requirements and to give you the benefit of some of the work we have done in that area, somewhat in contrast to the work done at GE.

Similar to the GE effort, you start with the conventional differential equations from momentum energy and structural heat transfer that we have included in trying to develop our similarity requirements.

You recast those equations in an non-dimensionalized form, and you generate a whole set of similarity requirements that have to be met in the scale model system and the prototype system if you are going to reproduce the transient behavior, and this lists about seven of the similarity requirements that fall out of that nondimensionalization of the differential equations, and essentially if you look at these and rearrange those equations, you end up with a system that has five equations and six unknowns on the assumption that you can match the flow-loss term by adjusting orifices and numbers of contractions in the piping and things like that.

So, given this situation, if I specify one of my unknowns, and, let's say, I pick the length ratio, which is a convenient thing to do when we are talking about scale model testing, say I want to test something on a scale model of one-fifth. Then theoretically I should be able to solve for the other five unknowns, which relate to the thickness of the piping, the diameter of the piping, heat transfer coefficient, the velocity and the temperature difference.

And what we have done is essentially done

57 that and looked at two sets of situations.

The first is summarized here on this chart, which is done for prototypic fluids, in other words, scale model, looking at scale models that would use the same fluid as the prototypic system, and what we see is two specific attributes that are required if you wanted to meet exact scaling, exactly match those equations.

One is that they tend to scale, the systems tend to scale uniformly with respect to diameter, thickness and length, and, two, it is something that Emil appropriately pointed out that if we want to do practical tests, things we can reasonably do because the Delta T ratio gets so large, that we have to go to large scale if we wanted to exactly match the scaling loss, and we have went through a similar exercise with water, and again we see the results -- First, we see two different aspects with water.

T4 The first thing we see is that things tend to scale nonuniformly. If given a length ratio of one-fifth, you can see the diameter and the thickness scale differently from the length if I want to match all those equations.

Now, so that is the first attribute.

The second attribute we see here again is we see the impracticability of doing tests, and that was something Emil pointed out. That just sort of clarifies that from a different perspective.

Here again, the key problem areas are the temperature ratio and also the small size of the test prototype that would be required.

Given that, I think you could conclude exactly what Emil said, that in both cases I reached a case where water models appear to be impracticable.

This is for exact scaling now, not approximate scaling, which I am going to get into in a minute.

Water models are impractical for any size range. If I went to prototypic models, they would be impractical, unless I went to the very large-sized range.

Obviously, the next logical conclusion says well, let's examine approximate scale models.

At GE they have been looking at water models on an approximate basis. At AI we have been looking at sodium models, and what we have done, is we have looked through a series of models.

I just want to go over a couple of them here.

The first one we have looked at is a single-node-wall approximation. It is one that's commonly used in system analysis codes. It has the disadvantage that it has so many inaccuracies for very slow times, but it should be pretty good for good, medium and slow transients and for reasonably long-time solutions, and it has the advantage that you could generate a theoretical solution from it, and we will get back to that.

Another one that we have looked at is a semi-infinite wall model, one that would be accurate for very short times, but obviously would not be accurate for long times, and by long times you could characterize it, if you will, as sort of viewing it as the time it would take for a temperature wave to travel from this side of the wall and back.

Here again, the advantage of looking at these approximate models is that they are theoretical solutions available that you can play around with and try to get some understanding here.

This is looking at a theoretical solution for the semi-infinite wall approximation. The thing I want to point out is the solution contains a combination of a product of a BO-type number, which is one of our similarities in parameters and the time, which is another parameter which must match, and the thing this equation tells us is we can match this solution two ways in the scale model by either matching the product or by matching the two individually, and we looked at both methods, and it turns out they lead to different sets of scaling laws, and that is one of the things you will see on the next chart.

What I have tried to depict here is those -- We looked at the semi-infinite wall model, which has these two methods of satisfying similarity, single-node lumped wall model and this was our theoretical solution, which we started off with, and that is the uniform scaling.

I just want to point out, without going through this whole thing in a lot of detail, I just want to point out several things.

First of all, as I relaxed my requirements, almost all cases, there are a couple of cases where I duplicated uniform characteristics, I get this nonuniform scaling attribute for sodium systems also, and the nice thing about it is not only when I go get this nonuniform scaling characteristics, I also improve considerably this temperature requirement that was posing a problem for me before.

What this tells us when we go through and look at all of these approximations, we think this single-node-lumped-wall model is particularly appealing, one, because it is one that it is used commonly in the analytical tools, two, that because it should be good for reasonably slow transients of the type that we might be concerned with on a natural convection basis, and it is reason for long-term solutions, that this is one that we think is a reasonable basis around which you would construct a scale model, and that is the premise on which we have been proceeding and are proceeding at AI on our test scaling plan.

The conclusion from that effort is that just what I sort of already reiterated is that reasonable scaling ratios for the dracs loop are possible with the nonuniform scale factors derived from the single-node wall model.

Now, we have gone through just an exercise, which is just to check the mathematics really of what we have done to verify that indeed going through this exercise that the scaling laws we developed are indeed accurate, and we conducted a sample problem where we take a single node loop pipe and fluid at the same initial temperature and introduced a step change of temperature of entering fluid that has a theoretical solution.

If you take that sample problem and constructed two models, one a prototype full-scale model, if you will, and another reduced scale model, a length ratio of one-fifth and the other parameter is determined by the nonuniform scaling laws, and you put that onto a little computer model, using that known theoretical solution, you can generate a family of curves that represents the temperature rise of the fluid versus dimensional times, and this is at relative places down the pipe, and you will find that the model in the prototype falls exactly on top of each other, as they well should.

We took another case, which unfortunately I don't have with me, but I took a case which was based on a uniform scaling, using both sodium and water, and in both instances it resulted in cases that were up in this regime for the model relative to the prototype, which shows that you would expect differences in the temperature behavior in the scale model versus the prototype.

What we don't know significantly is how important those differences really may be, and that is one of the things we are trying to sort out, we would expect to sort out, in the experimental program.

This is a similar chart that talks about the pipe wall temperature and its temperature rise, and here again, the model and the prototype results were identical.

What has this told us at AI?

From our perspective, if we want to look at practical, natural convection models, in our view, they require nonuniform structured scaling. In our view, when we look at what we want to from water, from our perspective it looks doubtful.

We agree that the scaling always is going to require some compromise in dimensional and thermodynamic similarity, and, therefore, you want to make it as large and as practical, and practical being set by whatever facilities we have in place in the country, and we have been looking at that and have actually developed a preliminary plan of [illegible] we would utilize an available facility at ETEC to do some of these tests.

That is it.

Any question, comments?

CHAIRMAN CARBON: What sort of testing do you expect down the road to follow from this?

MR. GAVIGAN: I think I answered a question like this the other day, Max, Tuesday, but it was differently phrased. At that time I mentioned about different roles that I have versus other people in our office back in Washington.

There are people who are responsible for development who are interested in this overall problem of natural circulation heat removal, especially for the DERACS reactor performance.

Now, that is going to be a system which will fall under the design purview within the project.

The people who do component design work over the years have developed a lot of codes similar to -- at Argonne National Laboratory similar to the work they are doing at SFC.

The view they hold at the present time is that they developed enough code work that they ought to be able to predict the performance of DERACS at large-scale facilities based on the code development work they have done to date.

However, they are holding their options open until we actually get a project, if we do, a large developmental plan, until they do some more detailed study at Argonne National Laboratories to see whether they have the full competence of the codes they would need for that range of events that they are interested in.

So, it looks like this: There may be a large-scale facility built sometime in the future. It will probably be built, if it is, by the components

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people on the grounds that they don't have sufficient knowledge and competence to predict normal performance of the DERACS system.

If they build such a facility, we will be happy to utilize it also as part of the extrapolation test that Emil Gluekler mentioned and that Joe Mills mentioned that they are going to run later in sodium.

So, the future is hazy, but we know the problem is there.

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MR. SINGER: That basically completes the presentations on the combination of local faults and shutdowns, reactor shutdown systems and shutdown heat removal, and I would like to just spend a few minutes describing the program we have on combination of local faults, which has a number of definitions, but basically involving local events which may occur in reactors, as opposed to whole core events.

The basic objective here means that we would like to be able to demonstrate that the local phenomena which may occur in the reactor core do not propagate and eventually evolve into a whole core event.

Specifically, the two criterion, actually in this particular case there is only a criteria, is simply that we don't reach gross boiling in the reactor

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66 core is caused by some local fault in the reactor core.

So, what we are after here is looking at types of local faults which may occur and trying to understand them sufficiently so that we can demonstrate there is very limited core damage which may result, that these local faults, therefore, are coolable naturally, and where these local faults are defected or defectable.

The major program task in this effort right now is the SLSF, Experiment P4, which I will very briefly describe.

Our strategy in this particular area here is that implication that the local faults are primarily caused by failure of fuel-cladding or breach-fuel elements.

Simply what we are demonstrating is that any breach in the fuel element can be detected and that suitable protective action can be taken prior to any significant damage occurring in the plant.

The international reactor project operating the plant, the operator at present has three potential options operating his plant when he is posed with the question of what to do when he has indication of a breached element:

One is simply the first indication of any failure and any element to shut the plant down and remove the offending element. This is when the first gas signal is detected.

This is a present FFTF philosophy.

However, there is clearly going to be economic incentive not to shut the plant down on the first indication of a gas leaker, and there certainly will be pressure on the operators of the plant to continue operation until such time that there is, in fact, some concern.

I may have already indicated that it may be a safety problem. Their options at present can conceivably be: Remove the offending element at the next scheduled refueling, even though that particular subassembly was not scheduled to be removed, or perhaps ideally to perform the extended operation and not remove the offending subassembly until it was normally scheduled to be removed in the first place.

In order to justify these last two operating options, there are safety implications in these two options, and there is, therefore, an R&D program which is necessary to support these options, if at all possible. That is where part of our program is directed: Specifically, to establish the feasibility of these two options and try to establish what are the safe operational limits in terms of what can be measured from defective fuel elements in an operating plant.

The approach we have taken to this is relying on basically three sources of information at the present time:

One is to essentially utilize as much information as we possibly can gather from the foreign plants which are operating on behavior of breached elements in their plants, what type of signals they give, what difficulties they have in detection and methods they have in detection.

At the other extreme we are conducting experiment P4, which I will describe in the next view graph, which is designed to be a nonmechanistic endospectrom-type test, which hopefully, in fact, is designed to encompass beyond the worst conceivable type of local faults we could possibly have.

Intermediate to these is the EBR2 local fault testing program which is part of the EBR2 operational reliability testing program, and this program has three basic objectives involved:

One is the characterization of the signals we are going to get from effective fuel elements, and what will be done is we will have various types of faults, in other words, various types of breached elements, whether they are artificially or naturally occurring, what type of signals, as well as the various stages of deterioration and operating history those breached elements are subjected to.

Related to this is a very important phenomena, which dictates to a large extent the deterioration these elements undergoe during operation and are the kinetics of sodium-fuel reaction. There is a chemical reaction simply between the sodium and the oxygen-bearing material in the fuel, which has a density less than that of the existing material, so it tends to expand and perhaps enlarge the breach.

Very little information is available on this, and we are hoping to gain some information from our tests in the fully-prototypic environment in terms of oxygen content and things like that.

And finally the other particular area which is hoped to be gained from the EBR2 testing is a measure and characterization of the type of extent of fuel loss, which can occur from breached fuel elements during realistic operating cycles, not only normal start-ups and shutdowns, but also operational transients, mild slow overpower transients and normal other events which will occur during the operating life of a plant, and, of course, connected with that is the spread of contamination through the system, potential spread of contamination, and in order to give us some idea of what type of maintenance problems might be associated with operation of breached-fuel.

That program was just underway, and we won't have results for you for a few years.

The endospectromy-type or bounding experiments, which is well under way in fabrication in the SLSF experiment P4, this particular experiment is designed so that there are 37 pins in the bundle. It has a number of heat-generating blockages, in other words, fuel blockages within the bundle, which are designed so that as the ETR reactor is brought up to power, the fuel in the blockages will melt and the blockages disrupt, so you will have a significant amount of model fuel within the fuel bundle itself.

The tests on this type of condition, and there may be some up and down ramps in power, but the basic objective is to run at this condition where you have grossly defective fuel with significant amounts of fuel outside of the fuel-cladding that is in the bundle for a fairly long period of time. 30 days is basically the fuel, one cycle of the ETR plans, and hopefully to verify under these conditions, which are way beyond any mechanistic estimate of what might happen is there is still no possibility of propagation outside of that particular subassembly.

Now, execution scheduled for late fiscal year 1981, and I think that's still pretty much on schedule.

CHAIRMAN CARBON: What was that?

MR. SINGER: Late this fiscal year, I think it is August.

MR. GAVIGAN: August 12th.

MR. SINGER: So, this particular experiment is designed to bound any type of local fault switch, which could happen, and then the next level would try to get the information out of EER2 test and foreign reactors which would be directly related to the type of information designers would need to try to accommodate these local faults and operations with breach fuel.

That is all I have to say about local faults.

CHAIRMAN CARBON: Thank you.

This would seem like it might be a good time for a break. We will take 10 minutes.

(Short recess.)

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MR. FERGUSON: We are going to spend the rest of the afternoon then on the LOA-3 work that is going on in the program, specifically, LOA-3.1, Energetics Accommodation, and to place this in perspective, the LOA-1 related activities in the safety program deal with the safety related issues associated with all concerns that lie within desig basis, and in the LOA-2 work we have done, looking at beyond design basis events, focusing on identification of margins in the way of inherent capability as well as engineered systems, providing margins for accommodation of events beyond the design basis, while limiting core damage to essentially negligible extents.

In moving into the LOA-3 area, we are now dealing with the area that is commonly known as core disruptive accidents, so we are talking about severe accidents, events well beyond the design basis.

In the LOA-3 area, we deal with separately the issues of energetics accommodation and re-accommodation, which in both cases the idea being that we went to focus on what has to be done within the design to provide for maintaining containment integrity for various periods of time in the sense that

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particularly with respect to debris accommodation, we don't have a good feel at the present time what NRC might require in the way of containment, of how long one would have to maintain absolute containment integrity.

So, the safety program focuses on saying if you want to maintain it for 24 hours, here is what you would have to do for a 48-hour period. Here is what you would have to do, that sort of thing.

In the area of energetics accommodation, we are taking a narrower approach, saying that we simply want to demonstrate the very low probability of containment failure for CDA, essentially saying that we want to demonstrate that we can keep the energetics threat bottled up within the primary vessel itself with some relatively small amounts of attention being devoted to showing that in addition, if to the extent that, let's say, a significant amount of sodium were released to the containment building itself, that that could be accommodated without short-term containment building failure occurring as well.

So, the focus is on demonstrating that the energetics-related threats, in fact, can be adequately accommodated within the primary vessel, while at the same time not significantly modifying the design of that vessel.

So, that puts a reasonably sharp focus on our work then, namely, that we don't believe that designers should have to significantly interrupt the design of the head and vessel and piping and so on to accommodate these energetic threats from CDA's.

So, the third-level product is under energetics accommodation within the primary system boundary, and accommodation within containment.

I have summarized the major programmatic tasks that are currently in place in the program field behavior analysis under these severe accident conditions and related code development for the severe accidents themselves, initiated phase analysis and code development, transition phase analysis and code development, structural response analysis and code development, the end reactor experiments program in TREAT, and then a broad range of out-of-reactor, out-of-pile experiments.

MR. MARK: On that first item, accommodation within primary system boundary, is there any clear picture of a difference in that respect between a pool- and a loop-type system?

MR. FERGUSON: Well, a couple of perspectives: One is that it is generally possible, if you were

75 concerned about very large sodium, it is very possible to provide some kind of an energy absorption capability outside of the secondary tank in the pool-type vessel that might give it the ability to withstand larger loads than a loop-type system, but we are talking about loadings that are really beyond the level that we are concerned with here.

So, let's forget about that.

MR. MARK: Okay.

T5 MR. FERGUSON: In terms of head design, it is hard to say, it is a rather complex issue, and I don't think there is any clear-cut differentiation when you start talking about the large diameter heads that you wind up with for these large pool systems. They begin to look an awful lot like the large diameter heads -- the large diameter heads in loop systems begin to look very much like the large diameter heads in pool systems, so that again there is not a big difference there.

MR. MARK: But we are talking about differences at a level far above the one which you feel need be talked about.

MR. FERGUSON: Well, in the case of head design, yes, from a realistic point of view in terms of what one can actually realize from these systems.

No, from the standpoint of the kind of requirements we are likely to recommend placing on the designs.

I want to emphasize that in my own personal opinion, I can't see any way of getting significant loadings on the head of one of these LMSBR's.

On the other hand, I clearly lack the ability today and probably will lack the ability five or 10 years from now to demonstrate that conclusively.

We are still going to be talking about having to deal with, within the range of uncertainties, loadings that are reasonably significant on the head, talking about 1,000 PSI or two pressure loadings being sustained for a significant period of time on the head.

MR. MARK: Fine. Thank you.

MR. FERGUSON: Well, our strategy in this energetic accommodation area is to demonstrate the inherent likelihood of a core-disruptive accident resulting in significant loads on the reactor vessel and head, and as we will show later on, we have a number of steps, each of which we want to show the inherent likelihood of significant energetics.

At the same time, to develop energetics

accommodation enhancement capabilities through cost-effective design options, this is primarily with respect to containment design, that is, to explore containment design options that would allow one to accommodate higher pressures resulting from sodium spray fires in the head. We really have nothing going on in this area in the way of vessel and head design itself.

Well, the approach then is to start sequentially through.

In the case of the initiating phase, the idea is to show there that the inherent processes that work are such that it is very difficult for fuel to be compacted at a significant rate, and, furthermore, that if the elevated power conditions should arise from rapid sodium voiding, that this fuel melts and disrupts under those conditions and motion will be sufficiently disbursive, that the reactor would not experience sustained super-prompt critical bursts of the type required to generate large amounts of fuel vapor.

Now, with respect to the loss-of-flow accidents, one of the two generic core-disruptive accident types, there we have focused on the heterogeneous designs as a way of limiting the amount of total sodium void worth that the system could have and, therefore, effectively reducing of the amount and rate of fuel disbursiveness that would be required to avoid the sustained super-prompt critical conditions.

MR. MARK: Can you put any numbers to that, the void coefficiency in some case or another might be plus $10 ,be?

MR. FERGUSON: I think I can give you some good guidelines there with the Clinch River homogeneous design, which was studied in the mid-'70s, had something on the order of $3.90 maximum positive reactivity and on the order of $3.50 or $.60 in terms of positive void reactivity when the core and upper axial blanket was avoided. That was a sufficient amount of positive void worth, that when you model that system with the current system analysis codes, the codes predicted a substantial amount of sensitivity in the accident energetics to variations and input parameters and this, of course, was what was argued out back in the previous Clinch River licensing.

MR. MARK: I don't remember the numbers.
I remember the arguments.

MR. FERGUSON: So that it turns out that that range, that same range, in the order of $3 to $3.50, turns out to be sort of a transition range, as

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you go toward larger systems as well. That is, so lon as you stay at the roughly $3.00 level or below in driver fuel, as in the fiscal elements in the bundle reactor, you can see a lessened amount of sensitivity in your analysis of these things.

When you go above the $3.50 to $4.00 range, then significant sensitivity begins to come into play, and the reason for that is that roughly for the range of temperatures you are likely to get to in this initiating phase, you can pick up something like a dollar to a dollar and a half negative DOPRA feedback and the same sort of amount or perhaps slightly less, maybe a dollar of fuel axial expansion reactivity feedback, and if the difference between those two and the amount of void worth you have in a system is greater than a dollar, as a rule of thumb, you are going to find that you have a lot of sensitivity to the predicted consequences of the accident with variations in the modeling of the codes.

MR. MARK: That was for a homogeneous?

MR. FERGUSON: Yes, homogeneous, the same kind of concept obtained for a large heterogeneous system in the sense that if you keep the driver fuel, the positive void worth in the driver fuel elements, include the upper axial blankets in those elements below $3, you tend to be in a regime there with less sensitivity.

If you take it above $3.50, you begin to get in a more sensitive regime.

It turns out that you can design, if you are strictly concerned about sodium void worth, you can design a large heterogeneous core with about any void worth you want from on up to the nominally $5 or $6 you can find in any homogeneous system, and you achieve that simply by going to lower and lower void worths by just decoupling the core more and more.

That is, you can put enough, you can keep the rings of core fuel thin enough and the rings of blanket fuel separating adjacent annular regions of core fuel thick enough that you can couple those, and from a sodium void core point you have a very, very small positive void worth unfortunately below about $2.50. You have a core you can't control.

That is, there used to be a great deal of decoupling between these core regimes to the point that you begin to have extraordinary large power swings over the lifetime of the plant. You will effectively have separate critical reactor systems in each of these.

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So, it is in this margin between $2.50 that sort of represents the lowest you can go to and $3.00 plus where you get into the sensitive region that designers in this country are focusing on, and that is roughly where the current large development plant core that GE has designed comes in and so on.

I think that's --

MR. MARK: Okay. You got a rather narrow window, and this heterogenity might help you?

MR. FERGUSON: That is right, and it only helps you in the case of the ability to make the argument bout the maniogenetics associated with the core-disruptive accident, and there is no reason in the world why you go to a heterogenic core, and that is something where you see, for example, that the French and the English are saying, "Why do you want to do that? Why do you want to complicate the design?"

You do somewhat complicate the design when you go to a heterogenic core because they have found other ways of finessing this issue of severe accidents in their confrontation with their licensing authorities.

The French, for example, have simply defined the probable way by agreeing that the accidents they will focus on is the milkdown of a limited amount of subassemblies. In that case, you have no concern about sodium void worth. You never get into this regime of sodium void-related accidents and so on.

The English have essentially said that for their public inquiry and their subsequent licensing activities on CDFR, they will focus on only events which do not result in core disruption. They are concerned, again, about the limiting case of a limited number of subassemblies building down.

No concern about accident energetics.

No requirements on the vessel on the primary system on the head in terms of accident-related loadings and so on.

MR. MARK: It sounds the way you said that that the English and French are taking a somewhat similar approach.

MR. FERGUSON: They wind up at the same point.

The argument they take to get there is a somewhat different path.

They wind up with the same thing, and that is a point which largely ignores the traditional focus that tends to be in play in this country on core-disruptive accidents.

They go through the same course of

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arguments about reliability of reactor systems, the shutdown heat removal systems.

The French, as we heard this morning, are looking at self-actuated shutdown systems as a way of enhancing their reliability of the diversity of the shutdown systems and so on, and from our perspective then, we have looked at heterogenic cores strictly because of past experience and current understanding of these severe core-disruptive accidents, which are such that we find it difficult to deal with the sensitivity that you find in examining the loss-of-flow accidents, particularly in a core that has $5 or $6 of void worth.

MR. MARK: That was very helpful. Thank you.

CHAIRMAN CARBON: One more question on that: The British are ruling out the CDA as design-basis accidents.

Are they going ahead and putting mitigating features similar to what NRC --

MR. FERGUSON: In their CDFR plant design, they have a -- what do they call it, not a core catcher; it is a trade. They have a rather sophisticated design, in fact, for a trade below the core support structure, which they intend to show is capable of maintaining in a coolable condition the debris from a limited number of subassemblies, limited, maybe perhaps are half a dozen or a dozen, something like that.

They have gone to great lengths, for example, to put individual subassembly instrumentation in clusters of six thermocouples per subassembly, which are hooked into their plant-protective systems, to assure themselves that they will detect any abnormal conditions in subassemblies which if left undetected, could possibly lead to large amounts of molten fuel being present in the subassembly, and from there they speculate that they could get a large energetic fuel cool intersection, given their focus on that, that that could lead to a core compaction and so on.

So, to get around that, because they cannot absolutely argue that this couldn't happen if left undetected, they have gone to the extremety of extreme length of placing this subassembly instrumentation on every subassembly and hooked it into the plant-protection system, which we think goes far beyond what is reasonable.

We believe we can detect failed fuel well before the point it reached where that existence of that failed fuel leads to safety concern by whole core detection techniques.

CHAIRMAN CARBON: What will they have in the way of containment?

What will their containment be?

MR. FERGUSON: I believe they have a containment confinement system, but the design-basis accident for that is like a single-fuel assembly being dropped in the containment.

It is something that has nothing to do with core-disruptive accidents.

CHAIRMAN CARBON: Thank you.

MR. FERGUSON: Well, as we progress through the severe accident sequence, we get into the core disruption or transition phase of the accidents.

Again, the focus is on demonstrating low energetics in this phase by showing that this phase of the accidents developing in a very incoherent manner, particularly in these large heterogenic cores, and that as molten regions develop and begin to grow, that these tend to be stable and fuel tends to be disbursive under these conditions, that if an energetic burst were to develop, which would lead to a significant amount of fuel vapor being generated and the subsequent expulsion of fuel vapor and liquid through the upper internal structure, that the Simmer predictions that this would result in very low loadings on the head; we went to firm those up with experiments and further analysis.

We want to be able to confidently predict the structural response to whatever loadings might be experienced up there by refining the analytical capability for predicting vessel, head and piping response, including considering three dimensional effects in situations where we have assymetrically located components and so on.

And finally to show that if there were a sodium fire, spray fire resulting from the injection of large amounts of sodium in the form of spray into the containment building, that there would be an inherent limitation in terms of the pressures that could be achieved in the building itself through the burning of this sodium, and furthermore, to show that, in fact, for any realistic range of energetics, that there would be at least a very limited amount of sodium injection in the containment building.

So, the focus all the way throughout this is to show that at each step, once you have admitted that you are going to consider the accident in some way, each step, in fact, expected consequences would tend to lead to mitigation of energetics.

MR. MARK: You had on there "Suggestion of firming up the Simmer predictions".

Now, as I understand it, the Simmer predictions were that if you had a certain amount of calories, whatever, released in a neutron chain reaction, something of the order of 10 percent of that or perhaps a smaller factor could be translated into kinetic energy, five percent, I believe.

MR. FERGUSON: If you take --

MR. MARK: Are there people who continue to doubt that?

MR. FERGUSON: That particular bit of analyses has not been subjected to the scrutiny of a licensing interaction.

There are people who would prudently say that certain aspects of that analysis has not been firmed up through experiments and so on and so forth.

MR. MARK: So, it is to --

MR. FERGUSON: It is to firm them up.

MR. MARK: -- firm them up.

And if you were able to persuade these remaining doubters that there was a factor of 10 or 20 or 30 or something at that point, then you think you would have at least your second item made?

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MR. FERGUSON: Yes, I think that the combination of thermal and fluid dynamics processes that Simmer models, which tend to show that there would be a large amount of energy reduction combined with other analyses that look at the ability of materials going through an elastic deformation to absorb energy, the combination of those when firmed up would say you could take a very healthy super-prompt critical burst and result reasonably low loadings on the vessel and head.

MR. MARK: I have another question: I just don't have a number in my mind. I suppose I could go upstairs and develop it, but if I mix enough sodium and air to consume all the oxygen at atmospheric pressure, initially what pressure do I get?

MR. FERGUSON: This is an area where it is a bit out of my sphere of familiarity.

If I remember correctly, there was some work done at Argonne, Argonne National Laboratories, sometime ago that looked at something on that order.

MR. MARK: Maybe someone else knows.

MR. FERGUSON: Lou back there is a better man to talk about this.

A VOICE: About 100 PSI.

MR. FERGUSON: 100 PSI; I was going to say about 100 PSI.

MR. MARK: So, if I could contain 100 PSI, then I could let sodium burn until I was blue in the face?

MR. FERGUSON: I think there has been more recent work done at Atomix International, which deals with some codes which are still in the exploratory stages, which would say more realistically something on the order of 40 PSI is the maximum you could contain.

MR. MARK: It was just that scale I wanted a feeling for.

CHAIRMAN CARBON: You're Item No. 2, what magnitude of experiments are you anticipating? Do you need something big, small?

MR. FERGUSON: No, basically simulant, the kind of things that have gone on and will continue to go on will be simulant material experiments on the scale models on the order of one-tenth.

The Purdue experiments have been one-seventh scale plus, I guess, in that particular case very little actual interreactor tests because of the difficulty of taking conditions in the reactor, taking test materials in the reactor, the kind of conditions Simmer is looking at there in terms of the DEO program.

Well, briefly I want to run through the tasks that we have in place, very briefly here, and then turn the rest of the presentation in this area over to Dave Weber, John Kramer and Al Klickman, who are going to discuss specific aspects.

In the initiating phase fuel behavior work at Argonne National Laboratory, we are developing the FPIN fuel behavior code and the associated FRAS 3 code, which looks at the details of fission gas, behavior within grains and on grain boundaries and on grain edges.

There are studies going on on cladding failure mechanisms and fuel disruption modes, both of which supplement this code development update on here.

At Hanford we are focusing on the development of a second fuel behavior code, namely, the DSTRESS code, and at General Electric the people there will maintain their behavior SST code and look at cladding failure correlation development work, and this work is all coordinated around a couple of efforts: One, a continuing code comparison activity that is aimed at focusing on similarities and differences and the predictions of these codes and which is using this ongoing PFR TREAT and cooperative program with the UK, the tests there as one of the principal means of comparing the predictive capability of these codes.

In the initiating phase accident analysis and code development efforts, the primary focus here is the development of the SASS4A code at Argonne, which Dave Weber is going to be talking about.

HEDL is looking at developing a multi-dimensional blockage model that would enable us to explore this question of post shutdown coolability in TOP accidents in large reactors, and also at MIT there is work going on in the Thermit Code development area, Thermitting a three-dimensional sodium boiling code that would allow us to explore sodium boiling under a variety of conditions.

In the transition phase area, Dave Weber will discuss the transitions of transit hyrdo code development efforts at Argonne.

In the structural response area, there is continued effort at Argonne to maintain the RESCO and ISCO two-dimensional structural response code; RESCO being a Legrangian code for short-term transients, and ISCO being an Olorian code that deals with larger material relocations during these disassembly calculations.

The NEPTUNE Code then replaces a three-dimensional coupled fluid structure model for analysis of the response of assymetrically-mounted components in large reactors.

A SAFE/TASS code development effort there focuses on a fluid and structural model for looking at the response of the above core internals, and that compliments the Simmer to work in terms of looking at the deformation of the internal.

Simmer assumes these internals are rigid. SAFE/RASS looks at their deformation under the fluid dynamic loadings that they would experience and also calculates now much energy would be pulled out of this expanded fluid through an elastic deformation of the structures.

Finally, the SHAFT code development efforts which supports both LOA-2 and LOA-3 activities, SHAFT being a three-dimensional piping structural and thermal response code used to look at the response of the piping systems, both to longer-term thermal transients under degraded shutdown heat removal conditions as well as response of piping under energetics loadings resulting from core disruptive accidents.

The TREAT in-reactor experiment program is going to be discussed by Al Klickman from Argonne National Laboratories, and ANL and HEDL are two TREAT experimenters for the safety program.

In the area of out-of-type experiments dealing with initiating phase issues, we have four different tasks going on at Hanford in the area of fuel and cladding behavior of the fuel-cladding transient tests.

These are tests where short-cladding segments are pressurized with gas-loading and heated, and one looks at the failure characteristics of the cladding segments.

The fuel cladding mechanical interaction tests were in this case where short cladding segments are subjected to that would more nearly simulate what they would experience when the loadings are being provided by expansion fuel. This is a manual-loading rig that is used, again looking at cladding failure characteristics.

The cladding propagating tests are aimed at looking at how initial cladding failure might expand under a continued loading, might propagate under continued loading, as the accident continues.

Fuel behavior tests here, using the short-fuel segments, single pellets, heated radially,

Direct electric heating experiments at Argonne National Laboratories, where short fuel segments are heated with jule heating and associated behavior as the fuel disrupts monitors.

The camel loop at Argonne, this is a loop that will now support up to 37 full length fuel pin simulators, simply being steel rods and sodium flowing through at prototypic flow rates and thermite generated UOT being injected up through the hollow lower segments of these rods and out into the flowing sodium through machine defects in the rods, and this enables us to look at various kinds of cladding failure sequences and the affect of that on pressurization within the test section and sodium response and so on under overpower conditions.

The OPRA 15 pin boiling test is coming up. This will be 15 electric fuel pin simulators in a triangular-shaped bundle with, again, prototypic sodium flow rates, which will be subjected to a flow coastdown and will simulate flow coastdown in FFTF for Clinch River and looking at the multi-dimension aspects of the sodium voiding that will be obtained under those conditions.

In the transition phase we are doing at Argonne National Laboratories simulate experiments on volumetric boiling. This is using air and inert gas, bubbled through water, looking at flow regimes as a function of flow rate of gas, flow rate of gas simulating various superficial velocities.

Transient boiling flow regime testing is a closed-tool boiling test, a continuation of these fuel-freezing tests where thermite generated UO_2 is injected into simulated fuel bundles and the behavior of the fuel as it freezes and oblates the cladding monitor.

In the termination phase, we are finishing up the Purdue energy convection test. This was a one-seventh scale plexiglass model of the CRBR upper plenum.

There were two tests which were done where the hot fluid was expanding into a cold fluid, trying to measure that expansion process and look at loadings on various components there.

The FCI upper plenum injection tests are continuing looking at somewhat more realistic simulations at the response of the thermite generated UO_2 being injected into sodium-filled bundles.

The simulant fluid upper plenum injection tests, these are tests using water and tend to simulate an explosive pair, similar to what one would see with the carbide-sodium system in a carbide-fuel bundle, attempting to show here that in the confining geometry

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of a pen bundle that the mixing lengths are sufficiently small, that one would not see energetic interaction between a pair that would ordinarily -- Two liquids would be ordinarily explosive if mixed under different conditions.

For example, if you dropped hot tin in the cold water, we know that would yield to a vapor explosion. The question is: Would that pair expand in the containing confines of a pin bundling geometry.

There are some small scale sodium-uranium-carbide interaction tests taking place at Argonne, looking at the fundamental nature of fuel interaction with that pair, large-scale on UO_2 dropping tests and some other simulant fluid FCI tests.

Finally, in the area of energetics accommodation and containment, there is some analysis being done with state of the art codes at Argonne, looking at how much sodium might actually be injected, even the kind of pressures that are calculated in kinds of cracks in head seals and so on that are calculated under severe core disruptive accident conditions.

At AI there are experiments going on in single and multiple drop burning of sodium looking at the basic physics of that, and that supports their development of the Somex 2 straight-fire code, this being a two-dimensional spray fire model with a rather sophisticated treatment of the sodium spray and the subsequent combustion products.

And finally at Argonne there is analytical work going on to model the concrete structural response to the sort of short-term, high-pressure, high-temperature loading transients that containment building would experience if a very large-scale spray fire were occurring within it.

So that in a nutshell is the range of activities we have going on in this LOA-3 area at the present time.

MR. MARK: Could you say just another word about the simulant fluid, FCI?

What are the simuland fluids?

MR. FERGUSON: Again, I may want to defer to Lou Baker here, but they have used various things, like mineral oil, and freon, for example, attempting to get at some of the fundamental aspects of fuel-coolant interactions here in terms of various kinds of variables, like temperatures of the two materials, how they affected pressures and so on and so forth.

Lou, do you want to say a bit more about that particular bit of work?

MR. BAKER: I think they are also looking at some pin water experiments to look at the propagation of the so-called detination theory.

MR. MARK: This is really very analogous then to attempts to studying steam explosion with fluids a little more convenient than alta, duranium, oxide and sodium?

MR. FERGUSON: Yes.

MR. MARK: Okay.

CHAIRMAN CARBON: Don, is it possible to summarize briefly sort of the world outlook on FCI interaction possibility?

(Laughter.)

MR. FERGUSON: Well, I think it best be described a mixed bag with many against few.

CHAIRMAN CARBON: With what?

MR. FERGUSON: Many against few.

Let me start by saying that we believe, we being many people in the United States, we believe that there is a large body of evidence which supports the concept, first of all, that for the oxide-fuel sodium system, that the kind of temperatures that we calculate might exist through the full range of accident transients, that energetics fuel cooler interactions are simply precluded by fundamental physical principles, so that for in-reactor conditions, we do not use the kind of pressures one might calculate as arising from energetic fuel interactions to in any way affect the course of our prediction in the transients, while admitting at the same time that one can, under the right kind of conditions, get energetic interactions between liquid, sodium and molten uranium and dioxide, for example, by dropping a small drop of sodium into a pot of molten UO_2, the drop is resident for a while, it is captured by that super heating and then vaporized ather explosively.

The primary evidence that is offered there is this homogeneous spontaneous nucleation theory, which looks at the interface temperature that would be established, and it says that if that interface temperature is below the homogeneous nucleation temperature of the cold fluid, then that energetic FCI is precluded, and from there that theory needs some more sophistication added to it to be able to explain the full range of experimental results.

The other side of the argument, of course, is provided primarily by our British friends who claim that this detination concept, the key one, that it is possible for a self-sustaining interaction to occur if you have a large-scale coursely pre-mixed mixture of the hot and cold material, and you get the right kind of trigger mechanism, you then get a detination occurring where this shock wave propagates and it self-sustains the interaction.

Our comment on that is that the jury is still out on whether or not that is a viable theory. We are doing some very fundamental work now on the hydrodynamic and thermodynamic aspects of that which seems to be suggesting that it may be physically impossible for that to occur, but that is still very preliminary.

At the same time, again, through additional experiments trying to refine our understanding of the limits of this homogeneous spontaneous nucleation theory.

And, as you know, NRC is sponsoring, along with some other countries, is sponsoring a series of tests at ACRR where they intend to start with a coursely pre-mixed mixture of UO_2 and sodium, and they are going to hit it with a fairly healthy bang.

I suspect they will get some reasonable pressures out of that.

I don't know what applicability that has to the interaction situation, but it may be useful in shedding some light on the question of whether or not

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this detonation theory is a viable one, or not.

Dave, you are on. We have reversed the order of presentations here. It made a little bit more sense to go with Dave first and then John Kramer.

MR. WEBER: I am going to focus on a couple of the things that Don mentioned, and in particular I want to look at a couple of the larger code development efforts that are going on presently at Argonne.

I guess I want to make a general comment about the activities at Argonne. There are a range of activities, including experimental and phenomenological investigations as well as large-scale code development.

In the code development area, we, in fact, break that up into a couple of areas where we have phenomenological modeling as well as integrated core analysis, and my particular discussion here is going to be on the integrated core analysis.

There is an approach here that we have tried to put together in the development of these large-scale codes that I think I have illustrated right here.

As I go through and talk about a couple of the examples, in particular, the SAS4A and transit codes, I would like to come back to these concepts to illustrate how we have actually put them into effect.

There are a couple of elements here that in the development of these type of codes that I would like to point out. A lot of these have evolved over the years and in particular have evolved from the analysis of core disruptive accidents. I believe the strategies that I have indicated here now have become generic characteristics for a large-core development.

The first is a development of a mathematical model to simulate particular aspects of the system. In these large codes we are talking about specific models to handle the thermohydraulic of the code or a fuel disruption process and such things.

Once we have put these mathematical and computational models together, we entered the second step, which is a verification of the methods and the models and then an interaction with the experimentalist to determine the validity of the particular models and then in a whole core context to identify the sensitivities of the whole core results to those particular parameters, and finally, the last, to perform whole core analyses.

This performance of the whole core analyses leads in a feedback-type of a process to a redefinition of a problem and to hopefully some identification of critical phenomenology and needs, as far as data base is concerned, and hopefully some implications on the design process, and that becomes our second step, that is, the application strategy.

We have been particularly involved in two activities. The first was in the Clinch River breeder reactor and the conceptual design study to support the design licensing efforts in an assessment of the energetics of the core design as given to us, and then, secondly, the second step is the closed-loop interaction, and that is an examination of the design alternatives to help in the reduction of the energetics potential.

The perspective that we have for the code that I would like to talk about, I would like to focus on the code, such as SAS4A and transients that are indicated right here.

Over the last several years since the examination of the Clinch River breeder reactor, particularly for the homogeneous core, several areas were identified as being important, and I should say in the application of a code, the previous version of SAS, were identified several deficiencies in the model, as well as the experimental data base.

But looking at these types of cores, I think these key perspectives have to be noted. First of all, the idea of some sort of thermohydraulic phenomena associated with an overheating of the cooling can lead to sodium voiding of the introduction of this voiding activity.

The melting of cladding can, in fact, enhance this particular process, and finally with this mismatching and cooling capability, we get to the point of fuel disruption and fuel relocation.

Depending upon the reactor design and the timing of these particular events, it is conceivable that we could get significant reactivity feedback.

So, the idea is to assess these particular scenarios to see if in some integrated fashion we can, in fact, get to a scenario where we have a sustained prompt critical burst.

If we do not reach a sustained prompt critical burst, then the initiating phase of the accident may, in fact, be resolved positively, but we still have not resolved the ultimate question of core energetics, and that is in a code, such as SAS4A must determine the initial provisions for the subsequent analysis.

A code at the last point here, these are

105 large-scale integrated codes. That is, although we have specific phenomenological model development taking place throughout the program, it is the integration of these activities and in particular the integration of thermohydraulic phenomena with electronic-related phenomena that brings us to the point of understanding the energetics potential.

Several of the key phenomena that were identified in our most recent analysis in the licensing context was for the Clinch River breeder reactor.

These same type of phenomena have been identified in even more recent analysis of the loss-of-flow scenario in the conception design study, both Phases I and II, and these were the phenomena:

First of all, sodium voiding led to an increase in the power of the particular system, in general led to an overheating of the clad.

Clad relocation then further enhanced this particular scenario, generally having positive reactivity in Clinch River as well as in CDS, and it was the combination of these two events in here that led the reactor to the point of prompt criticality.

Then we reached the point of what happens when the fuel itself starts to disrupt, and this became a branch point in the Clinch River analysis, particularly with the homogeneous void and the void activity that was associated with it, and at that particular branch point, we determined whether or not we went prompt critical and sustained a prompt critical burst.

In that same type of a scenario, however, if we had reached prompt critical reactivity, and power increased rapidly, enough that we had the possibility of overheating fuel pins that, in fact, still had coolant in the subassembly channels, and the possibility then existed for rupture of those particular pins and failure of the pins into channels with sodium.

This was a phenomena that was identified as the loss-of-flow driven transients overpower events.

These types of events here were the motivation for developed and refined models for the SAS-type of a system, the previous analysis was performed with SAS3D, and I will discuss several of these models or at least summarize several of these models this afternoon.

It also motivated the development of an experimental data base to better identify the phenomena and also to calibrate the experimental information, and I think that is the critical point that I want to make here.

There has been an iteration in the model development where there is integrated analyses and the experimental data base that can be used.

The phenomenological aspects and the experimental aspects will be covered by John Kramer and Al Klickman subsequently.

Again, to refresh your memory, the SAS4A code is similar in design to the SAS3B code in that several thermohydraulic and neutronic aspects are considered, and I have summarized all of them here.

The most interesting ones that involve model developments because of the implication for the Clinch River reactor were identified in our cladding motion model clad, our fuel motion and sodium boiling during the TOP and TUC OUT, which is referred to as PLUTO and the fuel motion model during the loss of flow and loss of driven TOP.

These are the principal data base that justified this particular model will be briefly referred to here.

There are other aspects, too, though, to this particular code that were also refined to provide us with a better understanding of the scenario.

I mentioned before that when we reached the point of fuel disruption, we reached a branch point in the energetics evaluation, and it was the particular branch point that motivated the development of phenomenological modeling for the description for the fuel disruption process itself.

Now, there is experimental information that is being generated along this line, and there are detailed phenomenological models that John Kramer will refer to that will account for this as well, but in this particular case we need integrated aversion of this analysis to incorporate in the integrated analysis, and this is SS Fuel Deform 3.

I specifically want to point out the view graphs that are indicated there. A couple of the models, DEFORM, Levitate and PLUTO, to illustrate what general capabilities we have and how we go about evolving these particular models and verifying their capabilities.

I think that as we go through this, you will see that we, in fact, have a closed-loop type of interration here where in terms of validation of our model we look toward experimental information, both in- and out-of-pile, but then heavily rely on either analytic verification or intercode comparisons to validate the concepts.

In particular, let me use the Deform module to illustrate how we have gone about developing a model and performing some of the intercode comparisons.

As I mentioned, we need a model in the integrating code concept to describe the initial fuel disruptions or the cladding failure and then the subsequent disruption of the fuel within the channel.

SS Fuel Deform, in fact, provides us with that type of model. It will go through the fuel characterization aspect of it. It will effectively define where the fission gas is and its disruption capability.

Then in the Deform 3 module we effectively do the fuel mechanics, including the stress, strain calculations for the fuel and the cladding to identify where our failure may take place.

That was the safety issue and the objectives.

The principal characteristics that I have indicated there are, in fact, boiled down to generic -- two models that exist within the computer code itself.

This type of a model, as all of the models in SAS4A, tend to be a couple sets of partial differential equations.

There are parameters, though, that exist in these particular codes, and identification of a data base for specification of the parameters is necessary, and, as a check on the validity of this particular model, we need extended comparison.

The next view graph briefly summarizes what, in fact, we have been doing. I guess I should also mention at this time, I should have mentioned earlier, SAS4A is the most recent version of SAS and has only recently been completed in its integrated form in the spring of 1981.

In particular, though, we look for, in SS Fuel Deform, we are looking for both experimental and analytic verification, and in the analytic verification area, we are one of the participants in a code comparison exercise that Don mentioned earlier to identify several aspects of these types of models, including distribution within the fuel pins and the stress and strain history of the fuel pins themselves.

In addition, though, to these particular codes that are developed generally by independent organizations within the U.S., we are participants as a DEO representative to the European community whole core comparative calculation program.

One brief comment on that, because I will come back to it, this is an exercise where several organizations from several different countries are utilizing both their phenomenological models and their integrated models to assess whole core energetics and as part of this, we will look at particular phenomenological aspects to form some comparisons and then also to form whole core comparisons.

This was done in particular with the Commith 3 JFBR code and the other ones that were initiated there.

The Commith code is the one that we have taken as something as a benchmark in a fairly recent study.

The Commith 3 code that is used for light-water reactors was identified as the best in the state of the art as field performance code, and the FBR code is considered to be of similar capability, and, in fact, has been calibrated against fuel pins, I believe, out of the reactor.

We have done such comparisons, as I have indicated here, in fission gas release, gas distribution, and stress and strains, and these types of exercises have identified some deficiencies in our code, but then also have provided us with a data

112 base for calibration of the model itself.

The same types of things come out in the U.S. U.K. intercode comparison.

The next view graph indicates one of those comparisons with Commith and SAS4A, and although detailed discussion of this was really necessary, I have tried to identify how, in fact, we have accomplished such comparisons of these integrated-type phenomena.

The straight lines in here are the Commith calculated strain -- positions of strain, and the dots and the squares are the SAS4A calculated results.

It is this type of information that is necessary, and in particular, other information, such as fission gas distribution, that effectively defines what our initial conditions are at the initiating transient.

That was an example.

T7 In the near term we will go on, in effect, in the evaluation of this particular model, and in comparison experiments, and in particular those experiments, such as those mentioned by Don, the EEH, FGR and Transient Intercode comparisons, things of that nature.

There are a couple of other [illegible] in here that are also identified for future [illegible]. Some of the anticipated and experimental information we will be generating, in fact, will be U.S. U.K.-related information with annular fuel pins, and that will serve as another check on the analytical modeling of a particular code, but will also necessitate improved modeling capabilities as well.

That was an example.

As I mentioned before, when we reached the point of fuel disruption, we reach a critical branch point. If the fuel motion at the point of disruption is such as to increase reactivity, we have a potential, at any rate, for a prompt-critical burst, and the prompt-critical burst, in effect, is when you vaporize the sufficient fuel to give us energetics.

Initiation of this type of a model we tend to be in the range of roughly 90 to 95 cents critical and a very slight positive increase in reactivity in reactors, of high void worth reactors, that is, a reactor of something greater than $3 has this potential for pushing us over that particular cliff.

So, this was identified in the case of Clinch River, and the safety issue that I have indicated right here was the one that was intended to be resolved.

At that time, limited, this was in the mid-'70s time frame, there was limited experimental information particularly on high power conditions, and the only data point that became available in the '76-'77 time frame was the L-5 TREAT experiment.

Subsequent to that, several other experiences, including L-6 and L-7 were conducted and provided something of an experimental data base for the development of this particular model.

There were deficiencies used in the SAS3D model.

The objectives that I have indicated here were basically the deficiencies that we hope to relieve, and the characteristics that I have identified effectively address these issues.

This became -- our model became a two-fluid calculation, which was consistent with the state of the art, these types of calculations, because we were anticipating a thermal interaction either within the initiated phase or potentially within the transition phase.

Thermal models were included, and an integrated analysis of our fuel and clad motions was included.

Let me point out, skip one view graph, and

115 point out one of the comparisons.

As DEFORM indicated, Dave gave a partial indication of our intercode comparison effort, this will be an indication of our experimental comparison. This was a calculation that was performed of the fuel reactivity history and the L-7 TREAT test, which was designed as a Clinch River homogeneous core loss-of-flow test where we ran through a constant power phase and then went to approximately 20 times nominal in a burst phase.

The information that was generated on -- The fuel motion information that was generated was then weighted by a typical Clinch River reactivity worth curve, and the results are generated in a dotted fashion on curve.

The model that describes this particular scenario, in fact, as the SLUMPY model, and it is the one that was used in the SAS analysis for several years.

That model, with our best estimate of the parameters that are necessary in that code, gave us a disruption indicated by the dashed line.

The Levitate model is indicated in the solid line, and showed a somewhat greater, something of an improvement in the master experimental data.

There were several factors that accounted for this. The particular ones were: The dynamic modeling of the two-phase flow.

Well, that was an example of our experimental comparison, and that, in fact, is the direction that we continue to go on development of this type of model.

We are participating right now in an inner code experimental comparison, again, of the L-7 experiment. This is the one that has been completed.

We are presently in the process of analyzing other relevant loss-of-flow tests for disruption, including the L-6 loss-of-flow test which was similar in design to the L-7 experiment, however, the maximum power level was 10 times nominal, rather than 20.

A similar experiment in the transition phase, which I will come back to, is the RX-1 experiment, which is basically an analysis of the lower power boiling of a fuel steel mixture. Mathematical models have some similarity to that scenario as they do to the Levitate model, and we are using Levitate to pretest analysis of that particular experiment.

Finally, we are also involved in whole core code comparisons with the SAS4A Levitate model, again, in the context of the EEC WAC whole core. This particular exercise has only recently been initiated.

A transient overpower test that I will refer to in a minute has been completed, and irradiation core loss-of-flow exercise will be conducted over the next two years.

Our near term tasks effectively are to re-examine these types of scenarios for generally heterogeneous and to complete or extend the validation of the model.

The last example that I had chosen was an example from the PLUTO II code.

Let me very briefly summarize that and not go into much detail.

The difficulty that we had with the high void worth cores have been in the area of the loss-of-flow-driven transient overpower phenomenology, and the model that we had in SAS3D, in effect, did not have a dynamic calculation of the motion of fuel within the pin or outside of the pin during the failure process.

There was some criticism of the SAS3D model and its experimental data base, and over the last several years, another model called PLUTO was developed and implemented in the SAS4A code. This particular model underwent extensive stand-alone code verification, as it was originally designed for analysis of transient overpower events, and, in fact, the code and the parameters existing in there were calibrated against the H6 and EA TREAT experiments, which were 50 cents and $3 a second perspective in the transient overpower tests.

The code was then subsequently used in the L-8 TREAT experiment, which was, in fact, a loss-of-flow-driven transient overpower TREAT experiment, and the results which I have some of the results which we are going to discuss, if necessary, showed reasonably an agreement.

This type of a model, though, was put together into the SAS4A code, and as our three elements, the comparison of integrated whole core analyses that I mentioned before.

We have used SAS4A PLUTO within the EEC-WACs whole core analysis comparison, and these are the various European codes that have been used for the analysis of this fairly well-defined event.

One thing that I will point out is that the SAS4A code in this area tends to be one of the more sophisticated codes in this area and, in fact, has capabilities of running time and mathematical modeling that exceed most of the other codes.

SAS3D that I have indicated here is the version of SAS3D that was used in 1977 in the assessment

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of the Clinch River homogeneous core, but, in fact, is being exercised by KFK in the assessment of energetics to 300 SNR.

SURDYN is the French full-core analysis code.

FRAX is the English full-core analysis code, and SAS EPIC is a combination of SAS3D and a fuel motion model called EPIC, developed by the Physics Division at Argonne National Laboratories, and, in fact, was the NRC, U.S. NRC's contribution to this particular set of calculations.

This is another element of this code verification phase that has helped us, not only in the qualification of particular models, such as Deform, but also helped us in the qualification of whole core results for these types of analysis.

MR. MARK: You said an odd thing, which I don't suppose you actually meant.

SAS4A exceeds most of the other codes in running time.

MR. WEBER: What I went to say was that that is true, but what I meant to say is that it exceeds the capability of phenomenological modeling.

MR. MARK: It does run longer?

MR. WEBER: It does run longer, yes. Generally speaking, the models are more sophisticated.

MR. MARK: What kind of time does SAS4A run?

MR. WEBER: Typically will run for a loss-of-flow, perhaps two hours of computing time on an IBM 370.

MR. MARK: Thank you. So, it is really not horrendous?

MR. WEBER: No, it is not horrendous.

MR. MARK: But for a research program budget, it is a lot.

MR. WEBER: Yes, it is. In fact, we have dominated the computing costs both in the division and I would suspect across the laboratory.

MR. SIEGEL: What were you intending to convey on this chart?

MR. WEBER: Only that there are certain elements to our code validation phase --

MR. SIEGEL: These are all calculations --

MR. WEBER: Yes, these are all calculations.

MR. SIEGEL: -- by codes that to begin with people have equal confidence in them?

MR. WEBER: That is the proposal for this.

MR. SIEGEL: At the end when you get this mirage of results, does it suggest anything, other than that there are these strange differences?

MR. WEBER: Well, that is one of the purposes of these particular calculations, to identify where the differences exist and why they exist.

MR. SIEGEL: Well, take SAS3D and SAS4A, I suppose one is an evolution of the other?

MR. WEBER: That's correct.

MR. SIEGEL: What have you done to get rid of the peak?

Is that an improvement, or does that now ignore the very important factors, which were apparent before, but somehow have been wiped out?

MR. WEBER: Generally speaking, we do a dynamic calculation in SAS4A that we did not do within the context of SAS3D, in that SAS4A has reached a higher level of sophistication in terms of its analysis and picks up elements that were effectively stepped over in the previous version of the code.

In this particular case, we had the SAS3D calculation calculate a rather rapid expulsion of the sodium slud, and we have done a more detailed dynamic calculation on SAS4A.

MR. MARK: Total energy up to a 10th of a second is different as those graphs would make one think.

MR. WEBER: No, that is correct, but again the purpose was not to determine energetics of this type of a reactor, but rather was to identify the modeling capabilities of the various codes and to identify the differences, and I put this up not as an assessment of this particular reactor assembly, but simply as an indication of another method that we have for assessing the capabilities of the codes, such as SAS4A.

MR. LIPINSKI: Given the code, SAS4A, you calculate the reactivity feedback, so as the fuel motion contributes to the reactivity, this contributes to the power versus time history.

MR. WEBER: That's correct.

MR. LIPINSKI: Now, when you compare this to a treated experiment, the power versus time is TREAT as prescribed. The experiment does not alter the power time.

MR. WEBER: That is correct.

MR. LIPINSKI: And when you show this comparison, it is only with respect to relative fuel worth, but what role does the power time period play --

CHAIRMAN CARBON: One minute, the court reporter needs to change her paper.

MR. WEBER: Generally, the iteration process that exists is that a calculation of a particular scenario with a whole core integrated code is performed months, if not years, in advance of the conduction of a particular TREAT experiment, and the attempt on the TREAT size is, in fact, to measure the power time history of a calculated sequence with the integrated code.

There is no feedback, as you point out, in the TREAT experiment.

MR. LIPINSKI: Well, without seeing the power time history, it is hard to see if the TREAT response can match the --

MR. WEBER: I'm sorry, as far as that particular curve is concerned, we have the capability within SAS of prescribing the powertime history, and that calculation in that was prescribed, and, in fact, the radial power distribution, of course, is different than a TREAT reactor than you would see in a normal fuel element, and we also prescribed that.

So, we match the TREAT conditions in the analysis with SAS.

However, the generation of the original TREAT requirements for power time history are prescribed by a whole core of integrated calculation.

Is that clear?

MR. LIPINSKI: But somehow I don't think that

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the TREAT power time cure matches your prescription exactly, does it?

MR. WEBER: Yes, because it is red.

MR. LIPINSKI: All you can do is do a flattop and put in a nominal version that gives you certain energy, but the power time will not overlay your prescription.

MR. WEBER: Yes, it does. The power time history that we have used in the analysis of the TREAT experiments --

MR. LIPINSKI: But in doing a closed-loop calculation, if you do get a loss of flow, do you come out with a power time curve? That power time curve, as you provided as the prescription, would not be duplicated in TREAT.

MR. WEBER: That is deceivable, and that is one of the difficulties in the iteration.

MR. GAVIGAN: Do you want to add something, Don?

MR. FERGUSON: Well, certainly we would not attempt to precisely duplicate all the little squiggles you would find coming out of a whole core code, but for loss of flow accidents, most of the phenomena of interest can be simulated reasonably well in TREAT with an extended flattop of some height and duration followed by a burst that one could characterize as having some period and some flow with of half maximum and some power, and that allows us to track through the phenomena of interest in perhaps not the same power time history that would be calculated in a reactor, but would certainly take us through the phenomena of interest that a proper time of power level conditions and period of conditions would be.

MR. L PINSKI: My point is: You do a calculation, you come out with a curve, you are predicting that is going to be the behavior of a full-sized reactor.

When you go into TREAT, you are only looking at a particular part of the phenomena with respect to the fuel motion. You are not getting the same power time cureve you can see in a true reactor accident.

So, you are doing an approximation in terms of energy over some time ago.

A VOICE: The question is: What is the response of the various reactor materials to a given energy input sequence and can you predict that with responses with the code, you then assume that having predicted them appropriately and well-intrigued, that your description of the reactor as a whole is, in fact, correct.

Now, that is a big step, but the only way to do that is through a code.

Let me just mention very briefly transition phase code development at Argonne, which was initiated several years ago, but approximately a year and a half ago, we became considerably more interested in this area.

During the Clinch River licensing days, there was still considerable concern about the initiating phase, and some of the arguments were used for the analysis of the transition phase.

I think it has become clear that in the evolution of the heterogeneous growing contention that it will probably shift from the initiating phase to the transition phase. They will at least appear equally important.

In particular, these are our perspectives for the development of such a whole core code at Argonne, and in particular in the heterogeneous core, I will point out that our preliminary calculations indicate that the blanket assemblies will remain under void as the driver assemblies are going through the voiding processes themselves.

And so, to perform an integrated assessment on an extended time scale, we wanted to have capability for both in-type geometry and disruptive geometry type calculations.

There is a brief indication of motivation and some of the directions for our particular computer code. The initial version of this code without all of these models has recently been completed, and we are entering a phse of code validation and improvement of the mathematical models.

We do have the elements of intra and intra subassembly incoherence by using variations of some of the models that we used on the SAS4A code, and in the area where we have disruption of the driver range, and generally speaking in the scenarios, we are using basically a two-fluid model of the fuel steel vapor hydrodynamics similar to what people had considered in the homogeneous core considerations, and the inter assembly communication is both a normal thermal communication between these elements as well as potentially a mass redistribution developed out of the subassembly walls.

Finally, the multi-components pool thermal characteristics are really where a lot of the issues of the transition phase are tied up, and we have individual elements in the program to both analytically model these particular phenomena, as well as some experimental efforts.

In-pile the RX series and out-of-pile is down in several areas to address issues and provide us with guidance for the model of these scenarios.

Let me cut it off there and answer any questions.

MR. MARK: I have two questions, three questions actually.

There is a graph you didn't get to. It is the next graph, and I am wondering if the ordinance is correct, and maybe you can tell me after the meeting.

It was also that graph that you did show in the structural radii slide that I am wondering if the ordinance is correctly labeled on that one.

My last question is: One of the codes you referred to and didn't discuss is related to the cladding motion, and I wondered if the name of that code was an acronym for a four-letter word.

MR. WEBER: The original modeler thought it was an acronum, Cladding Action Program.

Can I answer the other two questions after this meeting?

MR. MARK: By all means.

MR. GAVIGAN: The next speaker is John Kramer from Argonne National Laboratories.

MR. KRAMER: Let me put up the first graph to identify myself again.

My name is John Kramer. I would like to discuss today with you some of the aspects of the work being done by the Fuel Behavior Section, Reactor Analysis Safety Division at Argonne National Laboratories.

In particular the work I will be discussing falls under LOA-3, maintaining core integrity, and the bottom line is we are interested in the initiating phase of the accident, in particular, looking at fuel motion and how that fuel motion may affect the subsequent course of the accident through its affected reactivity.

The next two slides were designed to provide some focus on what I want to talk about. Actually Dave Weber did a good deal of this work for me. So, I will only have to spend a couple of minutes, I think, refocusing some of our attention.

In particular, after identifying the accident initiators, following through the accident scenario, one can identify certain issues; and I will be discussing those in the next view graph.

Some of the issues are related to fuel behavior as a subset, and, of course, that is where our interest comes in.

The issues that are identified, such as fuel disbursal, we would like to resolve them through a combination of both experiments and analysis. In fact, of course, there is a good deal of feedback between the experiment and the analysis.

In-reactor experiments can identify the phenomenology that we should address. Usually these are rather complex phenomenas that involve interaction of various physical elements. It is necessary to model the physical phenomena. Invariably you end up incorporating these models into codes to look at interaction of various models.

Once you identify phenomena and the models you are interested in, then you can go to out-of-reactor experiments, and I will be discussing a couple of those today, to try and look at the physical parameters that go into your models, go back to the codes and then do calculations, looking back at the in-reactor experiments and eventually, of course, trying to use these codes to predict the behavior of full-sized reactors under accident conditions.

This next view graph I borrowed from Dave Weber. In fact, it is very similar to what he had listed in slightly different form. It is an analysis by SAS3D of the CRBR loss-of-flow accident, and I think it

131 points up very nicely some of the issues I would like to talk about that are involved with fuel behavior.

If you look at the loss-of-flow accident following a coastdown, you get initial voiding. Boiling occurs, of course, with the voiding. You get cladding relocation, and the cladding tends to melt before the fuel melts with the CRBR core, at least.

T8 Eventually the fuel starts to melt, and you get to a branch point. It depends what happens to the fuel. If it is strongly dispersive and tends to move out of the core, then you can get a substantial decrease in the reactivity, due to the fuel motion. The power levels tend to go down, and you enter the transition phase.

On the other hand, if you get only limited dispersal at this stage, then you go on to this point where you are looking at possible pin failures in other subassemblies. These subassemblies initially are also voided, and so you are looking at more pin failures in other voided subassemblies.

Again, the fuel motion after you get fuel failure can be dispersive. The fuel can move out of the core, or you can only get limited dispersal or perhaps fuel compaction, in which case you are talking about rather high powers now.

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So, if you don't get fuel dispersal, then you enter a stage where you are looking at the possibility of fuel failures into unvoided subassemblies, very much like the classical TOP accidents, where now you have subassemblies with sodium, with strong cladding. You are looking at the fuel failure into these unvoided subassemblies.

Well, I think all this points out these various branches points which have to do with two basic phenomena, one of which is fuel dispersal in a voided subassembly, and the other one is fuel failure into unvoided subassemblies, and those are the two main issues that we address in the fuel behavior program under LOA-3.

The next view graph just summarizes what I just said, that is, the two main issues are looking at fuel disruption and dispersal with minimal cladding constraints.

The second major issue is looking at the response of clad pins to power transients, either in a TOP accident or in this LOF accident in a core with a large sodium void worth where you are interested in the possibility of LOF driven TOPs.

Typical questions include:

How and when does fuel disrupt?

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Is fuel basically ductile?

Does it tend to swell, or is it brittle?

Does it break up?

Must fuel melt before it can move?

What disruption criteria should be applied, and what is the character of disruptive fuel?

Is it broken into little pieces, big chunks?

Also for the other main issue we are interested in the behavior of clad fuel pins undergoing power transients. The basic issue there is when and where does the cladding fail.

Depending on the failure site, whether it is high in the core or toward the core midplane.

As I get fuel moving toward the failure site, I can get either increases or decreases in the net reactivity, due to the fuel worth.

Of course, all these phenomena are influenced by such questions as how does burnup influence fuel disruption or cladding failure and how does the clad fluids influence it, thermal history, et cetera.

The fuel behavior program activities are listed on the next view graph.

I think our effort is broken basically into two major categories:

Modeling and model development, looking at

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physical phenomena, and we are also involved about halftime in code development.

Our most recent effort in modeling has concentrated on fuel disruption modeling and in particular on fission gas behavior.

The other area of modeling we have been involved with heavily recently is looking at the mechanisms that lead to cladding failure, looking at cladding failure timing, location and care.

Under codes Don Ferguson mentioned a couple of these codes.

The F Pin code is an integrated RZ treatment of a fuel pin that does the thermal mechanics all the way from the point of actual initiation up through coolant boiling, as long as the fuel pin maintains its geometry.

There is sort of a sister code, the F State code, that looks at nonaxisymmetric phenomena that takes a single axial slice off the fuel pin.

Our primary use for this code is looking at TREAT tests, where there tend to be severe gradients across the core and across the fuel pins, causing the power to be skewed around the pin.

The FRAS code Don Ferguson mentioned. That is a code that looks at fission gas behavior, fission gas release and swelling of fission gas, and lastly I have listed the porous code, which has been available for some time that looks at the way in which fission gas may move through the interconnected poracity in fuel and be released to the fuel boundaries.

Now, what I would like to do is give some examples under each of these main headings, rather than try to discuss in detail everything we are doing.

Under the fuel disruption issue, our analysis is concentrated on looking at fission gas bubble behavior and looking at fuel breakup.

The codes that have evolved from our modeling are the FRAS code. The F Pin code is a mechanical TREAT, but we are now involved in including the feedback between the fission gas behavior and the mechanics so that these two codes are being coupled together, and we also needed to couple these two along with the porous code because once the gas is released from the drains, it is of interest to know where it goes to, does it go to the central cavity and pressureize the cavity of the pins, or is it released to the plenum drain of transients?

CHAIRMAN CARBON: How accurately does something like the porous code duplicate experimental data?

MR. KRAMER: Well, there is very little in-pile experimental data that you are going to find.

So, what is necessary to do is to look at out-of-pile data.

What we have done in developing the code is to use some gas permeabilities that were done by Grahm in Florida State, where he measured the permeability of UO_2 fuel. What we hope to be able to do is to use this code in analyzing some of the DEH Tests that are going to be coming up where they have a gas collection chamber.

I will be talking about that later.

CHAIRMAN CARBON: Which tests?

MR. KRAMER: DEH tests, Direct Electrical Heating tests at Argonne.

Other than that and other than having confidence that you understand the physics and that there are some measurements of permeability, I am afraid that you are flying a bit in the dark by using these calculations until we get the DEH test results.

CHAIRMAN CARBON: Even there, won't you still have some questions left in your mind?

MR. KRAMER: Well, we expect so because all you will ever end up with are end results, and as we all know, there are lots of different ways to get to the same end point.

That is really true of a lot of the calculathat we do. We are always extrapolating our physical model out into regimes where there is very little experimental data.

So, what we have to do is build confidence in the models where the data exists and then have confidence that we understand the phenomena that can be extrapolated.

As another example, I guess, looking at fuel disruption from the in-reactor experiments, down in the bottom we have available the TREAT F series, a SANDIA FD series and a TREAT L series test, but again these are end-result tests, so you are looking at the end results of a lot of physical phenomena.

It is very difficult from these tests to go back toward the other direction and try and sort out all physics. You are only going to get limited amount of data from it.

CHAIRMAN CARBON: What are the SANDIA FD tests?

MR. KRAMER: They are in ACRR, if that is what you mean.

I am afraid that this view graph did not reproduce very well in your notes. I hope it looks better in here.

This is an example of some of the most recent

DEH tests.

The DEH tests are Direct Electrical Heating tests done in Oregon, where they pass an electrical current through a fuel pin stack. They have the capability now of taking slices of actual fuel pins with the cladding left on them. They didn't have the capability before because the cladding tends to short out the electrical current.

What they can do now is with experimental heaters they can melt off the cladding before they apply the voltage to the UO_2 stack, and by melting off the cladding, then they apply the power through the fuel pin and simulate the second phase of fuel disruption and where now the power and the reactor could be going up.

What they found in these most recent tests is rather interesting and something they haven't seen before, and that is for very high-burnup fuel, you end up with fuel dispersal before fuel melting occurs.

If you look at the burnups of these sequences, low burnup, medium burnup and high burnup, the low-burnup pins, the cladding melts off.

Again, it is a little difficult to see here, but here a little puddle of the cladding running down the fuel pellet stack is expanding the fuel pellet stack somewhat, but nothing much else is happening.

On the other hand, with the medium-burnup pins, which were at 4.7 at a percent burnup, they contain a lot more fission gas. The cladding melts off as it did here, but now you start ending up with a fuel stalling off as the cladding runs down the stack.

If you go to even higher burnup, you get even more fuel stalling as the cladding runs down the stack, and this is probably the best picture because the cladding has run down about to the bottom here.

These two lines that are shown show the dimensions of the fuel pellet stack, and with their numbers they estimate that the fuel volume has been reduced to about 25 percent of its original volume.

So, even before fuel melting with very high-burnup pins, you can get significant amount of fuel disruption.

We have analyzed this behavior with our codes and models. [illegible] simplified analysis is just to look at the fission gas on the grain boundaries, and it is probably not surprising, given the amount of fission gas to the fuel pin, that if I heat up the pin rapidly enough, that that gas doesn't have time to incorporate, it pressurizes the grain boundaries in connection with the fractures of the grain boundaries.

If you do a simple calculation assuming that, make an assumption of the size of the particles that might be spalled off, you can match pretty well the velocities that are measured for the fuel spawling in the DEH tests.

The second slide that I show here emphasizes what I was just saying even better, I think.

This is the result of some recent FRAS calculations. The original FRAS code only treated gas within the grains. Now we have the capability of looking at gas within the grains on grain faces on grain edges, both releases of the gas and swelling.

What has been done here is to look at the rate of which fission gas on grain boundaries can coolabrate. Starting out with a hundred manometer bubbles, which is about the size of grain boundaries, if you postulate certain temperature transients of various heating rates from 10 degrees K per second to 10,000 degrees K per second, obviously if I increase the temperature very rapidly, I don't have time for these bubbles to coolabrate. That is, they can't collect up vacancies fast enough so that they can come to an equalibrium volume, and what happens then is you pressurize the bubbles, and there is a chance that you can fracture the grain boundaries, given this early fuel disruption.

141 On the other hand, as you reach higher and higher temperatures, the vacancy diffusion becomes more and more rapid, the bubbles can coolabrate and eventually at highest temperatures than I get up to this straight line, which is the curve showing the equalibrium radius of 100 manometer bubbles, as a function of temperature.

This sort of phenomena has been shown in both the DEH tests and the FGR tests down at HEDL where it seems that the fuel just sits there. You heat it up.

Depending on the heating rate, you reach the threashhold temperature in which the fuel will swell on you, and this shows why you reach the threashhold temperature.

It does occur at about 2500 degrees K, which has been observed experimentally.

The other major focus of our work has been on cladding failure.

Here we are looking again at either TOP accidents where you have full-sodium flow, or we are looking at loss-of-flow accidents where you have a flow coastdown with power increasing so rapidly that you are having fuel pin failures into subassemblies that have not yet voided.

We have gone back to look at models for fuel cladding failure. It is our feeling that it is rather senseless to do a complicated thermal mechanical calculation with a fuel pin code if at the end of that calculation you don't know what to do for failure criteria, and in order to understand what sort of failure criteria you should apply, we think it is necessary that you understand actually what is causing failure.

I will be saying a little bit more about that in just a second.

The out-of-reactor experiments that we rely on to calibrate our codes or look at the details or models are the mechanical property tests being done at HEDL and the FCTT tests. Don mentioned these tests, fuel cladding and transient test or tests.

The in-reactor tests that we have analyzed are basically the TREAT E and H series tests, which are transient overpower tests.

Now, I think I can illustrate the work we have been doing in looking at cladding failure by using this slide. It shows a segment of a piece of cladding has been testing out-of-pile by pressurizing a cladding tube, heating it at a fixed heating rate and pressuring the temperature at which failure occurs.

This piece of cladding was taken away from the fuel failure site, but what you see if you look at cladding that is presumably undergoing an incipient failure is that you get a number of multiple cracks tending to penetrate through the cladding. These cracks, if you read the report where this picture came from, the HEDL report, these cracks, they feel did propagate during the transient tests, presumably at the failure site; they propagated all the way through.

In looking at a picture like this or even the higher magnification, it is obvious these are cracks on the grain boundaries. It is also obvious that the cracks depend very much on the corrosive environments of the fuel pin.

It is very interesting that when HEDL has looked at mechanical tests of cladding, you get very different results if you look at unfueled irradiated cladding, say, from the plenum region of the pin, and compare that with results of -- and here they plotted strength ratio, which is the strength for the irradiated cladding versus the strength for unradiated cladding.

It is very interesting that the strength ratio for unfueled plenum cladding seems to saturate out at maybe 80 percent of the irradiated strength.

144 On the other hand, if you take cladding that is from the fuel section of the pin that has been subjected to corrosive fission products, you get a substantial decrease down to, say, four-tenths of the strength of unradiated cladding, and, again, it saturates out.

CHAIRMAN CARBON: Excuse me, what is unfueled and fueled again?

I am not quite sure.

MR. KRAMER: What they have done in the unfueled tests is to take irradiated cladding, but it is from the plenum region of the pin.

So, it hasn't had fuel adjacent to it.

On the other hand, the fuel cladding is cladding that has had fuel adjacent to it.

CHAIRMAN CARBON: The unfueled could be at a higher temperature, couldn't it?

I am just trying to understand it. It doesn't make any difference.

MR. KRAMER: It turns out that, say, for instance, if you look at the fuel cladding, it doesn't seem to make much difference in the fuel region where the cladding came from. So, I don't think the temperature effect during irradiation history is important.

Some of the unfueled cladding also came from the bottom reflectors, so whether it is unfueled cladding from the plenum region above the pin or below the fuel region of the pin doesn't seem to make too much difference in the results.

It is evident to us that what you are seeing here is stress corrosion cracking. Interestingly enough, HEDL has done some tests where they have cleaned up fuel cladding chemically, and you can restore this to the unfueled strength.

All this, I guess the bottom line is: I think it is important if you are going to do calculations where you are determining cladding failure, it is going to be important to understand the mechanisms that lead to cladding failure.

We still stress corroding cracking is important.

The last set of slides indicate how we have integrated some of the models into the F pin code.

These are calculations now showing one parameter, namely, the midwall hoop stress in the cladding as a function of transient time for two different transients.

I do not particularly want to distinguise between Case 1 and Case 5. All I really want to say is

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both of these were slow transients, and they both show the same qualitative behavior.

Early on in the transient you have loading, due to solid fuel cladding mechanical interaction, just due to thermal expansion.

The hoop stress in the cladding increases up to a level where the cladding begins to yield. Since the yield accident decreases with the temperature and the temperatures are increasing during this slow TOP transient, the hoop stress in the cladding begins to decrease up to a point where now both the fuel and the cladding start to soften substantially, and, I guess, significant creep in both of the materials.

The stress decreases even further would continue to decrease, except now I am getting fuel melting.

There is an expansion of fuel on melting that wants to pressurize the pin, so, then, you get an increase in the stress in the cladding up to a point where the cladding stress buildup, due to the fuel expansion, occurs less rapidly than the expansion of the cladding itself, and since I cannot load the cladding any more than it's flow stress and the flow stress decreases with temperature, then the hoop stress once again decreases up to a point where I start getting fuel vapor pressure, and now I can start deforming the cladding very rapidly, as the stress goes up once again.

All this is by way of illustration, I guess, that the mechanical loadings and the mechanical analysis of fuel pins is rather complicated involving a mini-phenomena.

Now, we have used the F pin code to analyze some of the TREAT tests. These results were presented at the Seattle Fast Reactor Safety meeting where we analyzed three tests: H4, H5, H6 and E6, 7 and 8. These are rapid transient overpower tests.

There is very little direct information or measurements that we can get from fuel behavior for these in-pile tests. In fact, about the only number that we can really verify our calculations with is the time of failure, and that is what you see listed here comparing the experimental results with time of failure to F pin calculations.

We did some F STATE calculations, and also there are a couple of empirical correlations that purport to be able to coolabrate fuel pin failure time.

Well, we were recently happy with these results, but when we started looking at slow TOP transients, we began to have less confidence in our calculations. These slow TOP transients were tending to predict failure much earlier than -- well, the only slow TOP transient that has been so far is in TREAT, which is J1, but suffice it to say, I guess, as we go on to more credible slower TOP transients, to TREAT experiments, I think that we need to go back and refine our analysis because I don't think using the same models that generated these calculations are going to do nearly so well for the slow TOP transients.

And that is basically the state we are at.

We are upgrading the F pin calculation, hopefully so we will be able to do a better job with slow TOP transients.

MR. HARTUNG: I have a question, listening to your presentation and Dave's before yours, hopefully, to see if I understood, hopefully, the work that you are doing has given you better confidence that a low void worth core, like $3 to $3.50 will behave with low energetics or no energetics.

Can you make a statement as to whether or not there is any hope that this work will allow one in the reasonably near future to make a similar statement about a higher worth core like a $6 worth core, or is that just out of the realm that you re investigating cores?

MR. KRAMER: Dave would be better to answer that question because at the level we look at things, we don't get into calculating the reactivity feedback. We are looking at the phenomenology of fuel motion.

MR. WEBER: Generally speaking, I would say that that is true. If you look at TREAT experiment information that we used to calibrate these particular codes, in particular, the L6 and L7 experiments, there appears to be sufficient negative reactivity that one could, in fact, argue for low energetics in the homogeneous core as well.

But there is a sensitivity there, if you look at the data that is coming out of there, there does seem to be a fuel motion regime over a time span of roughly 50 to 100 milliseconds where it is conceivable that one could see that there was some slight positive motion taking place there, and if one is going back into a licensing-type of concept, it is conceivable that one could use that part of the experimental data to suggest that the initial motion is compactable, and that is where we really need greater experimental information. We need more than two data points.

MR. MARK: Do you have a question, Bill?

MR. KAMP: Yes, Bill Kamp.

How happy are you, I guess from the

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presentation, not terribly happy, with our ability to predict time and location of failure for flow, i.e., realistic gravity?

MR. KRAMER: Well, my enthusiasm and happiness sort of comes and goes. You would think that with the number of people that have been working on this problem, the amount of time they have been working on it, that we would have done better, but it seems that there is always something that comes up, like when you start working on slow TOP transients, then various people start calculating early failure.

Whether these are real, or not, I don't know. But we need more experimental information in order to decide whether the people who are doing calculations and early failure are doing it right, or whether there are other people who don't predict these as failures are doing the calculations right.

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MR. KAMP: Do you think we can get that experimental information?

MR. KRAMER: I think that is definitely true. I don't think it is necessary to simulate the entire transient.

Again, it is only based on analytical calculation, so what I say is only as good as my belief in the computer codes that I have, but it would seem that you only need to simulate the last, I don't know, three or four seconds of a slow TOP transient, that the models would be insensitive to any previous history as long as you eventually brought it up to the right temperatures and you kept it to those temperatures through the failure time.

Previous history is not that important.

For instance, fission gas, it doesn't make too much difference at lower temperatures what you do to it, whether you keep it down at lower powers and then you bring it very quickly up to power and then smoothly move on your transient code, or whether you actually run it through these low temperatures over long periods of time.

MR. KAMP: That had certainly been my opinion. However, Jim Scott argued that W2 brought that into some question.

So, would you comment on that?

MR. KRAMER: I guess I would really like to look into a lot more detail than I have on the W2 experiment. I know very little about it.

I have seen a preliminary data report, and I have heard people discuss W2, but I guess I haven't really read the results of W2 in a lot of detail.

So, I wouldn't know how to comment on it.

MR. FERGUSON: I would just like to comment that when we get the post test examination, perhaps we can get a little clearer picture of what may have happened. All the evidence may have been largely destroyed by subsequent transients as well.

So, it is very difficult to tell.

MR. KRAMER: People who want to talk about W2 should also talk about J1.

MR. MARK: Thank you.

MR. GAVIGAN: Now we will hear from Al Klickman.

MR. KLICKMAN: The hour is getting late, and so what I would like to give to you today is a brief presentation on the TREAT program, what we are doing, how we do business, where we have been and where we are headed.

The program is one which we try to coordinate very closely with the other activities at Argonne and at the other laboratories across the country.

Working very closely with modeling and code development people, John Kramer, and with accident analysis.

As an example, yesterday afternoon the Reactor Experiment Steering Committee at Argonne had a meeting to discuss the development of one of the forthcoming experiments. Seated across the table from me was John Kramer, and seated across at my right was one of Dave Weber's key people.

So, we tried to develop experiments, both integral types and phenomenological experiments, which are related to the needs of the modelers and the accident analysis people.

Integral tests: These are tests which lump together many phenomena making it difficult to sort out.

Phenomenological experiments are one in which we try to look at a situation which is not prototypic but which will perhaps provide results which are related to one specific phenomena.

We are looking at both TOP accidents and LOS accidents.

They are pre-event issues, post-event issues that have to be considered.

Cladding failure time and location is one of the pre-events. Post-event issue is basically molten fuel injection, how does the fuel disperse, how is it swept out.

The multi-pin bundle can tell us something about incoherency effect. We can perhaps also see something about FCI and coolability on the LOS.

There is the voided channel issue. There are also the in-voided channel issues that have to be considered.

Some of these up in there you can look at with single pin tests. Others require multi-pin tests.

A number of these down here require multi-pin tests to provide meaningful answers.

What it boils down to is on the next slide, the objectives: Time and location of failure, fuel relocation and bundle size effects.

To do that we do single pin tests, seven pin tests and 37 pin tests, which will be done in ATL-TU to look at bundle size effects when you go to even larger bundles.

Some of the recent tests that we have done in, say, the past five years or so have been single pin capsule tests and static coolants. We have used both EBR-II pins, 13-1/2 inches long and PFR pins, 36 inches long.

There has been one capsule test which was done last fall using a fresh PFR pin.

Multi-pin integral tests used Mark II and III loops and R-Series loops. They used seven EBR-II pins in some of the tests.

We have done one test in which we used seven

PFR pins, 36 inches long, and we have done four tests in which we have used three pins, GETR pins.

Those are the L5, L6, L7 and L8 tests which Dave Weber and John Kramer mentioned earlier.

Phenomenological tests have been done in recent years.

AX-1, which was a Carbide MFCI test, and F1, F2, F3 and 4 were tested design to look at fuel rupture and ejection into a voided channel situation.

On vehicles perhaps it would be best to stop here and look at a few vehicles.

This is the R series apparatus schematic which has been used on several tests. That is how it is inserted into the reactor. As you can see, it has got equipment up on top of the reactor, and in cross section, in fact, it looks like that.

This is a gas-driven system. Sodium goes from one side down through a down, back up through a fuel zone hodoscope and into a collection tank. Because this has connections which have to be broken in order to take the loop out of the reactor, this type of an apparatus is restricted to fresh nonplutonium-bearing fuel.

The other vehicle which has been our work for multi-pin tests has been the Mark II loop. I don't think there is a Mark II picture in there. There is a Mark II picture, but this one is a recirculating system containing a total of about one leiter of sodium, which is recirculated down and back up through the fuel section.

This was designed to accommodate 13-1/2 inch long pins basically. The L5, 6, 7 and 8 tests were done in this loop. They use 36-inch long pins, but those pins had to be specifically modified to fit into this loop.

MR. MARK: Cut in half?

MR. KLICKMAN: Had to cut off the plenum and put new pins on them.

We came up with the Mark III. The Mark III happens to be a stretched Mark II with a reactor modified so that you can sink the loop down further into the reactor down below the base so that you can accommodate a full 36-inch long pin in here and still have a bottom plenum on it.

This loop is also designed so that if you elevate it into the reactor a bit, you can have your plenum up on top and use FFTF-type pins, and it will accommodate not only the 230 mil diameter pins that we were getting from the UK, but it will also accommodate 259 mil pins.

It is limited to a seven pin bundle. You can't go ary larger than a seven pin bundle in this particular loop.

The tests we have conducted in the past -- Let me chop off the coolant velocities from here so we can get the listing on. In recent years we did RTE eight F6, which were rather fast TOP, J1, which John Kramer mentioned a few minutes ago.

They were fresh pin tests, three feet long, 50 cents, TOP, L6, 7 and 8, which went anywhere from 10 to 75 times nominal power, used jet pins, three pins in the bundle.

LOF was a $5 per second.

The present and the future program at TREAT has four parts to it: The PFR TREAT program, a follow-up program after that using PFR irradiated pins in TU.

The PFR treat program that we are currently into uses two kinds of pins, UK annular pins and PFR driver pins.

All work in PFR would currently have in this country 40 irradiated pins from PFR. They arrived this spring, and we are currently assembling four experiments, two single pin capsule tests and two seven-pill bundle-flowing sodium tests, which will use some of those PFR irradiated pins.

We also are currently in the UK ready to go into the reactor. Now, some US pins, they will be inserted into PFR this month, yet some of them will come out a year from now, others two years and others three years from now so that in two to four years we should have back here US manufactured pins, these, which have been irradiated in PFR.

The single pin test will be done in a HEDL version of a Mark III loop. They have put some special features into it and label it the SPTL.

The multi-pin test will all be done in Mark III loops.

As part of this program we intend to do both TOP tests and LOF tests, as I have down here a breakdown of the single and seven-pin for TOP and LOF.

As you can see, UK currently has a somewhat even distribution between the TOP and the LOF. When you look at the TOP, the UK seems to be interested in the higher ramp rates, rather than the lower ramp rates, and if you would look at our listing, you would find that on TOP, we intend to concentrate our tests on the lower ramp rate test quite a bit.

On the LOF, the UK has more single pin than the US does because we certainly believe that the LOF test really does not provide much information. We would like to concentrate our activity onto the seven pin LOF test.

Now, in March of '83 we anticipate that there will be 17 pin -- 17 tests done with UK pins, and at that time TREAT will shut down for modification under TREAT upgrade and will not come up until 3-84.

After 3-84, the balance of the tests in this list would be done, not only would the balance of the UK tests be done, but all of the tests with the US pins would be done.

After we finished that program, we would intend to go on with PFR irradiated pins doing several TOP tests in the single pin test loop, no LOF test, about three Mark III seven pin tests on TOPs and hitting the LOF tests quite hard with approximately 10 tests.

One thing we would like to concentrate on in the LOF test is in the area of using near fresh fuel. That is fuel that has been in the reactor just a short period of time. From PFR, the fuel that we can get, the fuel will tend to have as a minimum something along the order of three to four percent burnup on it.

We would like to look at the situation when there is no fission gas content, and we anticipate we will be able to get some FTR pins with low fission gas content to pull off this set of tests. Also with FTR pins sometime after late '85, we would hope to do approximately four LOF tests and four TOP tests. These are just to examine the bundle size effect, and they would be tied quite closely with respect to the transients that were discussed to seven pin tests.

Phenomenological tests is the last series that I mentioned.

There are basically two phenomenological types of tests that we are currently interested in:

One is the F series tests, F3 and 4, and we had a small fuel segment which was subjected to a large burst and studied both photographically and with a hodoscope to determine how does fuel break up.

The results were quite significant. Fuel broke up quite a bit faster than what we had anticipated.

We are now planning some further tests with improved photography so that we can go back and learn more about how does fuel break up. In fact, these tests were the tests we were discussing yesterday afternoon in the Reactor Experiment Steering Committee.

Transition Phase: Dave Weber mentioned quite

161 a bit about the Transition Phase. We are planning a series of tests, the RX series, to look at the transition phase. We want to start out with nonprototypic situations, a fuel steel slug about five inches high, about one inch in diameter, in a capsule, which has a nuclear heated wall.

It would be a closed situation for the first test to look at how does the fuel steel mixture boil up.

After that we will look at structure interactions, cost formations.

If these five tests would be successful and provide significant information, then we would go on to three tests in a prototypic geometry to study the development into this transition phase so that we could then tie together the results from this test with the results from the prior seven pin bundle tests and 37 pin bundle tests, which would lead into transition phase.

MR. MARK: What is referred to as the bottle effect?

MR. KLICKMAN: When we look at seven pins with a wall around it, we realize that that is not 217 pins. 37 pins is really not 217 pins. You have an appreciable --

MR. AVERY: The bottle effect, not the bubble effect.

MR. KLICKMAN: Pardon me, the bottle effect would be because this is a bottled-up situation When a blockage forms, you could have this mixture restricted and bottled up like the cork on a bottle, and that could give enough pressure developed, which would blow out the cork on the bottle.

MR. MARK: Okay.

MR. KLICKMAN: Okay. I think that is basically all that I would want to accomplish today.

There are a few other view graphs in there on AATL, on the cross section of AATL, and in comparison cross section between the seven pins, Mark III and the single pin test loop, but they are basically for reference purposes and filling gaps.

Are there any questions?

CHAIRMAN CARBON: I have a general question which maybe ought to be addressed to Frank.

Six, seven, eight, nine years ago, there was a feeling, I believe, that we needed a lot more experimental apparatus, and three, four and five hundred million was spoken of, I believe. Some of the apparatus in that program, I think, TREAT upgrade was part of it, have been accomplished.

Is there still a feeling that a lot more experimental apparatus are needed?

MR. GAVIGAN: There has been a change over the years. One of the first changes was Jimmy Carter.

CHAIRMAN CARBON: The availability of money, rather than the need for it?

MR. GAVIGAN: No. It changed because of the action he took. He put the brakes on the breeder program, which at that time was moving fairly rapidly on what we call a 1986 commercialization decision. The whole ERDA program at that time was supposed to make a decision whether we could or could not go commercialized on a breeder reactor, so a lot of our program planning was aimed at supporting that decision; safety program, fuel, et cetera.

When Jimmy Carter came in '77, he took over the program so that for four years we marked time, in effect, and you didn't go anywhere.

However, this safety program did go somewhere in four years. We have learned a lot, as you have been hearing the last day and a half or so.

It is now beginning to appear that a lot of the needs that one saw at that time to move fairly rapidly to support a 1986 commercialization date doesn't exist anymore, because we don't now have a commercialization date except some hypothetical one, 2,010 or 20, something like that.

We recently conducted a study with all the effected people, and they have just finished a draft report that says pretty much the thing that I am saying, that we have learned quite a bit in the interim.

It appears now that we don't have the impressions that we used to have, and there is reason to believe that we can continue on kind of a slower pace program that we presently are embarking on, but, nevertheless, we will certainly remember in the back of our minds that eventually we may have to go to some fairly larger facility. It is possible, but it is not as severe as it was previously.

If we were to go, it would develop fairly large international involvement, I would guess. It would imply that we were going to commercialization decisions, at least. It would imply a large breeder committee, a large distribution in analysis from a number of breeder reactors. Those things are certainly far off in the distance, so the point I am trying to make is that need doesn't appear to be near as pressing now as it used to be.

CHAIRMAN CARBON: I guess almost to conclude what you have said, it is quite unlikely that you will ever ask for anything like that.

MR. GAVIGAN: Maybe not in our lifetime, maybe that is true.

CHAIRMAN CARBON: Any other questions?

(No response.)

CHAIRMAN CARBON: We can adjourn for the day and meet tomorrow morning.

(Whereupon, the meeting was adjourned at 5:50 p.m.)

This is to certify that the attached proceedings before the

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

in the matter of: ADVANCED REACTORS

Date of Proceeding: JULY 14, 1981

Docket Number:

Place of Proceeding: DES PLAINES, ILL

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

BARBARA ZUCKER
FRANCINE SALERNO

Official Reporter (Typed)

Barbara Zucker MAC
Francine Salerno MAC

Official Reporter (Signature)